Sticky Wages on the Layoff Margin*

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Abstract

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 such discussions don't happen, nearly four-in-ten UI recipients don't know. 18 percent say cuts would undermine morale or lead the best workers to quit, and 38 percent doubt wage cuts would save their jobs. For lost union jobs, 45 percent say contractual restrictions prevent wage cuts. In aggregate, our results suggest that wage cuts acceptable to workers could prevent about 30 percent of layoffs that result in UI benefits. 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. We draw on our findings and other evidence to assess received theories of wage stickiness and the role of sticky wages in layoff outcomes.

JEL codes: E24, E31, J31, J64.
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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. When asked why, nearly four-in-ten UI recipients do not know. 38 percent doubt wage cuts would save their jobs, and 18 percent say pay cuts would undermine morale or lead the best workers to quit. For those who lost union jobs, 45 percent say contractual restrictions prevent wage cuts. Adjusting for jobs that cannot be salvaged according to respondents, our results suggest that wage cuts acceptable to workers could prevent about 30 percent of layoffs that result in UI benefits.

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 cuts 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 an 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 pervasive unwillingness of employers to even broach the subject.

[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).

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 compensation cuts don't happen. Moreover, only four percent report an hourly pay rate on their lost job less than 1.05 times the legal minimum. 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 seldom experience pay cuts in the months leading up to layoffs. That is, we see little evidence 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.

The rarity of employer-worker discussions about pay cuts in lieu of layoffs sits uneasily with theories that stress private information about outside options as a source of wage stickiness and privately inefficient layoffs. These theories lead us to anticipate efforts to overcome informational impediments to the preservation of valuable matches. That we see so few instances of such efforts raises a challenge for these theories, which we put in the form of a question: If private information sometimes leads to the dissolution of valuable matches, why don't employers (and workers) make greater efforts to overcome informational asymmetries? To our knowledge, theoretical work has not addressed this challenge.

The next section expands on the motivation for our study. Section 3 describes our survey, reports summary statistics, and provides evidence on the quality of the survey data. Section 4 documents the willingness of new UI recipients to accept hypothetical wage cuts to prevent layoffs and the dearth of discussions about compensation cuts prior to layoff. Section 5 presents workers' explanations for this wage stickiness and why some would decline wage cuts. Section 6 considers potential explanations for our findings, and Section 7 discusses some directions for future research.

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 marginal product of labor to the marginal 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, closely related, motivation is the longstanding question of whether downward wage stickiness 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), MacLeod and Malcomson (1993) and Malcomson (1997). 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 et al. (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 (2018), Ehrlich and Montes (2019), and Murray (2021).

We are further motivated by this question: Is there scope for policies or third-party interventions to overcome the communication, coordination, contractual or other frictions that inhibit wage adjustments on the layoff margin, and to thereby reduce the frequency of layoffs and UI claims? Our evidence puts this question squarely on the table. In Section 6, we review documented instances of compensation cuts to save jobs. In some cases, they appear to have worked well in preserving valuable employment relationships. In other cases, they led to effort reductions or even deliberate sabotage.

[2]Leading examples in a vast literature include Keynes (1936), Gray (1976), Fischer (1977), Taylor (1980), Erceg et al. (2000), Smets and Wouters (2003), Woodford (2003), Christiano et al. (2005), Gali (2011), Michaillat (2012) and Basu and House (2016). A related literature considers the benefits of inflation for ''greasing the wheels of the labor market'' (e.g., Tobin, 1972; Akerlof et al., 1996; Schmitt-Grohé and Uribe, 2013, 2016).

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

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.[4] Studies that rely on payroll records find a higher incidence of outright nominal wage cuts, especially when looking beyond base pay.[5] Taken as a 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 reciprocity and motivation can lead to wage rigidity on the layoff margin and inefficient separations.

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

[4]See Card and Hyslop (1997), Kahn (1997), Barwell and Schweitzer (2007), Bauer et al. (2007), Devicienti et al. (2007), and Dickens et al. (2007). However, Blinder and Choi (1990), McLaughlin (1994), Smith (2000), and Fallick et al. (2020) find less downward rigidity using a variety of survey data.

[5]See, for example, Nickell and Quintini (2003), Elsby et al. (2016), Kurmann and McEntarfer (2018), Ehrlich and Montes (2019), Elsby and Solon (2019), Jardim et al. (2019), and Grigsby et al. (2021).

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.[6]

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.3). 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.[7] Respondents received a $5 Amazon gift 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 Appendix Table 1.[8] 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 74 percent for waves fielded 12 weeks afterward.

All told, we received 5,484 completed responses, and 1,203 persons completed all three

[6] 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.4.

[7] 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.

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

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.7 covers the coding of responses, 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 (about two percent) 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 the weights to match the Current Population Survey (CPS) distribution of persons unemployed less than five weeks in cells defined by the cross product of two age groups (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 persons 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 rates in manufacturing, given 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 et al., 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 (Appendix Table 2). This pattern is to be expected because separation rates fall with tenure, but it should be kept in mind when thinking about the character of our sample. Appendix Figure 1 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).[9] Thus, reservation wages shortly after job loss co-vary strongly and positively with realized wages on new jobs. We see this finding as clear evidence that 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 (1.2)). 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)).[10]

[9] 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 wage on the log reservation wage, we obtain an elasticity estimate of 0.84 (0.04). See Appendix Figure 2 for the corresponding scatter plots.

[10] Those who say yes to larger proposed pay cuts experience larger pay cuts upon re-employment (Panel A,

In summary, respondents who accept hypothetical pay cuts have lower reservation wage ratios, and they experience lower re-employment wage ratios than those who reject pay cuts. Moreover, those who accept larger pay cuts have lower reservation and re-employment wage ratios. Finally, reservation wages are highly predictive of re-employment wages. These results indicate that our survey yields meaningful data on reservation wages and the willingness to accept pay cuts to save lost jobs.

4 The Willingness to Accept Pay Cuts to Save Jobs

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, 53 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 appeal to 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 continuing to work (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 worker 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 says 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

Column (2.2)), but these differences are not statistically significant.

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. For both temporary and permanent layoffs, persons with less than six months of tenure on the lost job are more open to pay cuts.

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 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.[11] This variable is motivated by a large body of evidence that union jobs often pay wages that exceed the outside options of union members. See, for example, Freeman and Medoff (1984) and Lewis (1986).

Our second rent variable follows a large literature that interprets industry wage differentials for 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 to this literature include Akerlof (1982), Bulow and Summers (1986) and Krueger and Summers (1988). To quantify industry-level worker rents, we use the wage premia for nine one-digit industries that Stansbury and Summers (2020) estimate from CPS micro data.

If these wage differentials do indeed reflects worker rents, we would expect to see more willingness to accept pay cuts to save jobs in "high-wage" industries and larger firms. We do not find evidence for that pattern.

These patterns are consistent with some investments or rents in the employment relationship. Higher wage residuals could represent larger investments in the employment relationship, via human capital or search, or economic rents. These larger investments or rents suggest a larger scope for wage reductions to save the job when demand declines. This

[11] Based on survey responses to "Was the job that ended on [date] covered by a union contract?

view suggests that individuals with higher wage residuals should be more likely to accept a pay cut in lieu of layoff. This evidence also supports the interpretation that workers with larger wage residuals were more ''overpaid'' at their previous jobs relative to their outside option than workers with smaller wage residuals, and these overpaid workers are more likely to accept hypothetical pay cuts. But, in results not show, we find evidence that is more consistent with employment investments or rents. In particular, workers in the highest wage quintile are more likely to report discussions about compensation cuts prior to layoff than workers with lower residual wages. And, these increased discussions are initiated by the *employer* not the worker, in contrast with our expectation that overpaid workers would be more likely to initiate discussions than their employers.

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

Our results about the willingness of workers to accept wage cuts are 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, and this lack of discussion is pervasive. Overall, less than 3 percent of our sample report having any discussion with their previous employer about cuts in pay, benefits or hours as an alternative to layoff, as shown in table 5. These discussions occur infrequently across the entire economy, with no more than about 5 percent of workers reporting discussions when we disaggregate the data by observable characteristics, such as industry, tenure, union status, and firm size. Moreover, laid-off workers in our sample rarely have adjustments to their pay, hours, or benefits in the months leading up to their job loss, as discussed below.

The minor differences in the propensity to have compensation adjustment discussions in our sample documented in table 5 accord with intuition and previous literature. For example, we find that workers on temporary layoff are slightly more likely to have discussions about cuts in compensation as an alternative to their temporary layoff than permanently laid-off workers. This discrepancy is intuitive because firms reveal a preference for temporarily laid-off workers over permanently laid-off workers and so firms are more likely to engage in job-saving discussions with them. We find that workers at small firms are more likely to discuss cuts in pay, benefits or hours in lieu of layoff than workers at larger firms, consistent with Bewley (1999, pg. 172-173). Workers with more tenure and those reporting layoff due to slow business conditions discuss compensation adjustment more frequently than workers with less tenure and those reporting other reasons for layoff, such as the firm going out of business or reorganization, or the worker being fired.[12] We also consider these results intuitive because

[12]Inconsistent with UI eligibility requirements, almost one-third of our respondents report being ''fired or terminated'' from their last job. We suspect that this seeming contradiction is partly because our survey response did not specify ''fired or terminated *for cause*'' and hence there likely exists substantial measurement

high-tenured workers are likely valuable to the firm and slow business conditions allow more room for discussion than when an entire plant is shutting down.

Our finding that employers and workers seldom engage in discussions about compensation in lieu of layoff is consistent with previous employer-side surveys and does not rule out that some employment separations are inefficient. For example, Bertheau et al. (2022) find that most employers do not think that pay cuts would save jobs, which theses authors suggest might explain employers' reluctance to broach compensation-related discussions. These authors also find that about one-fifth of firms could have avoided layoffs with salary cuts of less than 20 percent. The willingness of workers to accept pay cuts to save their lost job in our sample documented in section 4.1 (about one-third were willing to accept a 25 percent cut), suggests some overlap between firms' assessment of the pay cuts necessary to prevent layoffs and workers' willingness to accept them. We also find that a large fraction of workers do not know why discussions about compensation adjustments did not occur, consistent with evidence in Bertheau et al. (2022) that about three-fifths of employers did not consider the size of the necessary salary cut (figure 9). Of course, comparisons between our study and Bertheau et al. (2022) are made difficult because our Entry Survey was fielded in Illinois in 2018 during a tight labor market, whereas their survey was fielded in Denmark in the summer of 2021 during the recovery from the pandemic recession.

We acknowledge that individuals who saved their jobs by accepting cuts in wages, benefits, or hours, will not appear in our survey population, but this selection does not undercut the substance of our results for three reasons. First, our sample is highly relevant for studying wage stickiness among laid-off workers because the sample frame is UI recipients. We are interested in whether wage flexibility could have prevented the layoff of these workers, regardless of whether discussions occur among workers who are not laid off. Second, if conversations about compensation adjustments in lieu of layoff are ubiquitous, we would expect at least some of them to end unsuccessfully in layoffs, so that, even in our sample, the share of people having such discussions should be high. Because this implication is inconsistent with our findings, we infer that these discussions are rare even among individuals not in our sample. Third, table 5 suggests that these discussions are rare even when there is less scope for employers to have discussions with some workers and not others—among small firms and unionized jobs. This finding suggests that these selective conversations are likely not playing an important role in our results.[13]

error in this survey response. Nevertheless, inconsistencies between UI receipt and reason for layoff are not uncommon in survey data. Summers (1983), for example, reports that 20 percent of UI beneficiaries were classified as job leavers or reentrants in the May 1976 CPS.

[13]With small firms, for example, the maintained assumption is that if one's co-worker has a discussion about compensation adjustment with the employer to save their job, one will learn about it and likely initiate a similar discussion. As such, the lack of discussion among these types of jobs suggest that employers are likely not having these discussions with some workers and not others.

Moreover, *employed* workers also rarely report that their hourly wages were ever reduced at their current employer because of difficult business conditions. In particular, the labor market module of the Survey of Consumer Expectations (SCE, 2020) asks employed workers "At your current job, has your employer ever reduced your hourly wage, salary or hours because of slack demand or other difficult conditions for the business?" Data from 2013 to 2017 indicate that less than five percent of workers ever experienced an hourly wage cut, which is broadly consistent with our findings that job losers rarely receive pay cuts, or report discussions about compensation cuts, prior to job loss. In the SCE, about 13 percent of workers report a reduction in hours, which is more common than reports of hours adjustments prior to layoff in our data. However, the vagueness of the time frame in the SCE question makes the results difficult to compare.

We also find that laid-off workers in our sample rarely have adjustments to their pay, hours, or benefits in the months leading up to their job loss, and temporarily-laid-off workers did not discuss possible cuts after recall. Figure 4 reports the changes in hourly wages among individuals in our sample who report either the same pay and benefits throughout their previous job (black bar) or those reporting a change in their compensation due to a change in their salary or hourly wage (gray bars). We restrict to individuals with at least one year of tenure on the lost job, consistent with Elsby et al. (2016). Over 50 percent of workers in our sample had no change in pay or benefits in the year before layoff on their previous job, referred to as the "spike at 0." The mean salary or hourly wage increase was 2.3 percent. Figure 4 suggests that there were essentially no cuts in hourly wages in the year before layoff with less than 1.5 percent of our sample seeing pay reductions. Among workers who did not have a change in salary or hourly wages, only about three percent of workers had benefit adjustments and another three percent had a change in pay due to a change in hours worked in the year before layoff. Another 12 percent of workers had a change in pay that was not associated with a change in hours worked, salary, or hourly wages, but as a result of other reasons, including changes in bonuses, overtime pay, and tips. Unfortunately, our data do not identify whether the adjustments to benefits, hours, or changes in pay due to these other reasons were increases or decreases. Grigsby et al. (2021) also find that employers adjust compensation using non-wage labor costs, like bonuses, benefits, and promotions. Only two percent of workers on temporary layoff discussed possible pay or benefit cuts to take effect upon recall.

Sample and survey methodology differences make it difficult to compare the nominal wage rigidity in our sample with previous work using the CPS, but larger nominal wage rigidity in our sample is consistent with our sample frame. The spike at 0 in figure 4 (about 50 percent) is considerably larger than in previous work. For example, Elsby et al. (2016, Table 6) find that about 14 percent and 20 percent of CPS non-hourly and hourly workers, respectively,

had no annual wage changes between 2011 and 2012 and Daly et al. (2012, Figure 3) report 16 percent for all workers. Our sample reports lower tenure on their lost job than employed CPS respondents, as discussed in section 3.4. Restricting to individuals in our sample with at least three years of tenure on their lost job reduces the spike at 0 (appendix figure 3). Our nominal wage changes are based on workers' recollections about their most recent change in pay (if applicable) during the previous job, which may have occurred several months ago. This measurement approach may be subject to more recollection bias than the results based on CPS data that measure workers' earnings in the previous week. As such, the fraction of respondents reporting no wage growth in our sample may be an upper bound on the true spike at 0. Finally, our sample of laid-off workers likely faced slower than average labor demand growth at their previous jobs. If downward nominal wage rigidity is operative, then it follows that workers in our sample are more likely to experience zero wage growth than CPS respondents.

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

In this section we document workers' reasons for a lack of compensation discussions and for refusing hypothetical pay cuts to remain at their previous job. Many workers do not know why discussions did not occur and doubt that wage cuts would save their jobs. Some workers say pay cuts would undermine morale or lead the best workers to quit. Among those that refused our proposed wage cuts, about half of permanently-laid-off workers say they have better outside options, one-third regard pay cuts as insulting, and one-fifth prefer unemployment to working at the lower wage. Among those on temporary layoff, the most widely-cited reason for refusing a wage cut is fear that it might become permanent.

5.1 Reasons for Wage Stickiness at the Point of Separation

Workers provide several salient reasons for why discussions about compensation did not occur prior to layoff, as shown in table 6. First, around one-third of workers believe that adjustment to their compensation would not have prevented their layoff, consistent with efficient separations and evidence in Bewley (1999, table 11.3). Second, about eight percent of workers report that compensation cuts would lead the best workers to quit (adverse selection) and about another eight percent report that cuts would undermine worker morale. That is, among workers who believe that compensation adjustments could have saved their jobs and those that do not respond "don't know," about one-third cite adverse selection and about one-third cite morale concerns for the lack of compensation discussions. The prominence of adverse selection and morale issues in our worker-based survey is largely consistent with surveys of firms in Campbell and Kamlani (1997), Bewley (1999), and Bertheau et al. (2022),

although making comparisons between these surveys and ours is difficult because of differences in sample frames, geographies, and labor market conditions. Third, almost 45 percent of unionized workers report that compensation cuts were not allowed under union contract. This finding suggests an important role for nominal wage rigidities in the union sector, as in Card (1990), Dickens et al. (2007), and Babecký et al. (2010). Fourth, almost 40 percent of workers report that they do not know why discussions about compensation adjustments did not occur.

Internal pay structures were not an important reason for a lack of discussion about compensation prior to layoff. Less than three percent of workers in our sample cite their employer's pay scale as a reason for no compensation discussions (table 6, column 7). This finding stands in contrast to evidence in Baker et al. (1994) and Davis and Haltiwanger (1996) about the importance of equitable internal pay structures, and evidence in Bewley (1999) that this equity may be important when considering compensation adjustments.

There is little heterogeneity in workers' reasons for the lack of compensation discussions by observable characteristics. Workers on temporary layoff are slightly less likely to report concerns about adverse selection and morale than permanently laid-off workers. Workers at large firms report adverse selection and morale with similar frequency to workers in the overall sample, and report their employer's pay scale slightly more frequently. Unionized workers are less likely to say that compensation cuts would not have prevented their layoff than workers in the overall sample.

We find only a small role for minimum wage laws and benefit laws. In particular, less than one percent of workers report that compensation cuts would violate minimum wage laws and less than two percent report that benefit cuts would violate the law.

Adjusting for jobs that cannot be salvaged according to respondents, our results suggest that wage cuts acceptable to workers could prevent about 30 percent of layoffs that result in UI benefits. In particular, for each worker in our sample we compute the product of the worker's willingness to accept the proposed wage cut and their belief that a pay cut could have saved their job. This product averages to about 30 percent in our sample.

5.2 Reasons for Refusing Hypothetical Pay Cuts

Workers on permanent layoff report several reasons for not accepting pay cuts to remain on their previous job, as shown in table 7, panel A. The most common reason, reported by about 51 (38) percent of non-union (union) workers, is that they can find another job that pays more. About one-third of workers permanently laid-off from union and non-union jobs report that the pay cut would feel like an insult, consistent with theories about effort reductions in response to compensation cuts because of fairness norms, reference wages, or

reduced morale (Solow, 1979; Akerlof, 1982; Akerlof and Yellen, 1988, 1990; Bewley, 1999).[14] This evidence and that in section 5.1 is loosely consistent with Bewley's (1999) findings that businesspeople did not consider "a possible tradeoff between productivity and pay level. . . , except in connection with pay cuts. . . This reaction speaks against almost all efficiency wage theories, not just the shirking theory" (pg. 111). Another one-fifth of non-union workers report that the outside option of not working is preferable to working at the proposed pay, and about one-third of union workers choose that response. About one-fifth of permanent layoffs report other reasons for not accepting the proposed pay cut, including not being able to afford working at that pay, being underpaid already, and commuting costs. Other reasons for permanent layoffs from union jobs include not being able to afford working at that pay, but also union agreement restrictions.

Workers on temporary layoff report several reasons for not accepting pay cuts, as shown in table 7, panel B. Temporary layoffs often express a fear that the proposed temporary pay cut would become permanent. About one-third (half) of temporary layoffs from non-union (union) jobs choose this response. Temporary layoffs often report that the pay cut would feel like an insult (about one-fourth), and that they prefer not working over working at lower pay (about one-fourth), similar to permanent layoffs. But, only about 18 percent of temporary layoffs from union jobs report that they can find another job that pays more. This evidence is consistent with our evidence in section 4.1 (table 4) that these workers are less willing to take pay cuts and Freeman and Medoff (1984) who document significant wage premiums for unionized workers.

6 Discussion

One view interprets all separations (and retentions) as efficient outcomes in the sense of maximizing the joint employer-worker surplus. Under this view, "quits" and "layoffs" tell us which party initiated the separation, but the quit-layoff distinction lacks deeper economic significance. McLaughlin (1990, 1991) provides a detailed development of this view.

As Barro (1977) (page 311) puts it: "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." He explains how rigid wages in the context of a long-term employment relationship can give rise to an "apparent nonwage rationing of jobs" when, in fact, the contractual employment rule equates the marginal product of labor to the marginal value of the worker's time in all states of nature. However, our evidence that most new UI recipients are willing to accept wage cuts to save their lost jobs undercuts

[14] Bils et al. (2022) show that effort adjustment in a Mortensen-Pissarides model with sticky wages in existing matches can affect hiring and employment, despite flexible wages for new hires.

this interpretation. In particular, our evidence about wage rigidity from laid-off workers is less easily dismissed by contentions that stickiness in measured wages is inconsequential for employment outcomes.

Hall and Lazear (1984) consider bilateral employment relationships in settings where the worker has private information about his outside option, the employer has private information about its outside option, or both have private information. When only one party has private information, they show that a suitable contract can deliver efficient separations. When both parties have private information, the second-best contract involves real wage rigidity and the excess sensitivity of layoffs (and quits) to labor demand shocks. That is, privately inefficient separations arise in some states of the world. Malcomson (1997) revisits their analysis, allowing for renegotiation and investments within the employment relationship, reaching the same conclusion about for the case of two-sided private information. However, ''If only the employee's outside option is private information, a contract in which the employee sets the wage ensures separations are efficient but at the expense of the firm making no specific investments at all. A fixed wage contract can typically on that.'' Thus, the combination of private information about the worker's outside option and the desire to provide incentives for specific investments by the employer can give rise to sticky wages that induce privately inefficient layoffs.

Other interpretations of the quit-layoff distinction stress legal and institutional constraints on compensation and separations, negative effects of wage cuts on employee morale and productivity (Bewley, 1999), adverse selection effects on quits and workforce quality induced by wage cuts (Weiss, 1990), and insider-outsider conflicts that lead to inefficient wage structures and rigid separation policies (Lindbeck and Snower, 2002).

Many of these other interpretations of the quit-layoff distinction depart, implicitly or explicitly, from a strictly bilateral perspective on compensation, separations and other aspects of the employment relationship. Collective bargaining provides an obvious example. Aside from collective bargaining and legal constraints, employers often rely on wage policies—rather than individually bargained wages—to determine compensation and influence turnover. These policies specify wages as a function of seniority, credentials, and position within an organization. Such ''multi-lateral'' compensation policies offer greater scope for a meaningful distinction between quits and layoffs. It is a challenge to incorporate ''multi-lateral'' aspects of compensation and turnover and departures from bilateral efficiency into equilibrium search models. Diamond (2011) presents an argument for rigid wages in bilateral models. He argues that consistent treatment of all workers is a real constraint in many-employee firms, consistent with evidence in Bewley (1999). As such, ''there may not be a wage that is within the bargaining range for the full set of existing employees without some layoffs. And if there is one, it may be too high for the firm when considering new hires,'' a topic studied

by Gertler and Trigari (2009) and Snell and Thomas (2010). Diamond (2011) proposes that these concerns over internal pay structures could motivate rigid wages in models with one-employee firms without violating the Barro (1977) critique that a model should not have "an inefficiency that intelligent actors could easily avoid."

To the extent that our results support the view that some layoffs are inefficient, we ask: What policies or third-party interventions could overcome the communication, coordination, contractual or other frictions that inhibit wage adjustments on the layoff margin, and thereby reduce the frequency of layoffs and UI claims?

Previous work suggests three important considerations for successful compensation negotiations and concessions. First, rents and workers' outside employment options are important during compensation adjustments. For example, Lee and Rupp (2007) find short-lived negative effects of pilot effort, as measured by on-time flight performance, at major US airlines following announcements of significant and permanent reductions in pilot wages due to financial difficulties associated with the terrorist attacks in the early 2000s. The authors suggest that pay cuts had small effects on effort because of large rents for airline pilots (Hirsch and Macpherson, 2000). The association between workers' increased willingness to accept pay cuts and larger rents is consistent with our evidence in section 4.1. The authors also suggest that pay cuts had small effects on effort because of limited pilot mobility because pilots lose their seniority if they switch carriers and they have highly specialized skills that do not easily transfer to other industries. This explanation is consistent with Sandvik et al. (2019) who find that reductions in take-home pay at an inbound sales call center—a more competitive labor market setting than the airlines industry in the early 2000s—caused a significant increase in the quit rate of the firm's most productive workers, but had little effect on effort among retained workers.

Second, employers' profitability and transparency about their financial situation are important. In their airline-pilot study, Lee and Rupp (2007) find that changes in pilot effort in response to pay cuts varied by the financial situation of the carrier. Namely, pilot effort did not decline at bankrupt carriers and declined significantly at nonbankrupt carriers. The authors argue that "bankruptcy is a clear signal of a firm's financial distress and hence it makes management's requests for pay cuts more credible." Bewley (1999) also finds 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). Krueger and Mas (2004) also find an important role for profitability. These authors find that announcements about shift increases and significant pay cuts for new hires at a Bridgestone/Firestone plant in Illinois in the mid-1990s led to a contentious strike and substantial labor strife, which increased the production of defective tyres. Importantly, the announcements occurred "just after Bridgestone/Firestone's first

profitable year since Bridgestone acquired Firestone.'' Finally, the importance of financial distress is consistent with Kahneman et al. (1986) who find that respondents consider reductions in wages acceptable if they avert firm losses but not if they arbitrarily increase profits.

Third, managing workers' expectations can mitigate reduced performance following compensation adjustments. For example, Mas (2006) finds a decline in the performance of unionized police departments after they lose in final offer arbitration. Moreover, the shirking was larger when the pay raises were further from the police union's demand. Mas writes ''On the whole, these results highlight the importance of managing and, in particular, lowering employee expectations prior to manipulating wage policy in organizations.'' Consistent with this conclusion, we find that union workers on temporary layoff are significantly less likely to accept temporary pay cuts than non-union workers (section 4.1, table 4). These workers' unwillingness to accept pay cuts could be because these workers expect to be recalled to their previous job and they expect the same pay because contractual restrictions prevent reductions in compensation (section 5.1, table 6). In their defective-tyre study, Krueger and Mas (2004) note that Bridgestone/Firestone's proposals about shifts and pay deviated from the industry-wide ''pattern bargain,'' something that had not happened since the 1940s. This sudden change in compensation relative to expectation might explain why those negotiations were so contentious.

7 Concluding Remarks

Wage rigidity has been important for explaining labor market and aggregate fluctuations since at least Keynes (1936); however, there exists little direct evidence on the allocative effects of wage stickiness. To improve our understanding on this score, we design and field a new, innovative survey of unemployment insurance recipients in the State of Illinois.

We use our survey to document several new facts about wage stickiness on the layoff margin. First, many workers are willing to accept pay cuts, and often large ones, to stay on their previous job. But, our second finding is that discussions about possible cuts in compensation to avoid layoff are exceptionally rare and this lack of discussions is pervasive across the economy. Moreover, laid-off workers in our sample rarely have adjustments to their pay, hours, or benefits in the months leading up to their job loss. Third, many workers do not know why discussions about compensation did not occur and doubt that wage cuts would save their jobs. Some workers say pay cuts would undermine morale or lead the best workers to quit. Unionized workers often report that compensation cuts were not allowed under their union contract. Finally, among those that refused our proposed wage cuts, about half of permanently-laid-off workers say they have better outside options, one-third regard

pay cuts as insulting, and one-fifth prefer unemployment to working at the lower wage. Among those on temporary layoff, the most widely-cited reason for refusing a wage cut is fear it might become permanent. Our work is not definitive about the allocative implications of wage rigidity, but suggests the potential for reducing layoffs by encouraging a transparent dialogue between workers and employers when jobs are at risk.

We acknowledge that workers not in our sample population may have had their compensation adjusted and that laid-off workers may be *ex-post* more willing to accept pay cuts to return to their previous jobs, but we do not think these forms of selection play an important role in our results. In particular, the widespread dearth of discussions about compensation we document suggests that these discussions are not happening more broadly. Our results also suggest that bilateral discussions about the possibility of layoff could elicit workers' willingness to accept a wage cut, and often a large one, to continue employment. Also, short unemployment durations among workers in our sample leave little room for learning about new employment opportunities and revisions to the willingness to accept pay cuts at the time of separation. And, the propensity to accept wage cuts to remain at the previous job is not correlated with unemployment duration in our sample. In any event, our sample is highly relevant for studying the possibility of avoiding job loss by adjusting compensation, regardless of wage stickiness and willingness to accept pay cuts among workers who were not laid off.

In subsequent work, we aim to extend our survey in at least four important dimensions. First, we would like to design and field complementary surveys of employers to obtain a fuller account of wage stickiness at the separation margin from both sides of the employment relationship. Second, we would like to field our surveys on an ongoing basis to understand how the prevalence of wage stickiness and workers' willingness to accept pay cuts varies over the business cycle. Third, we would like to extend our surveys to other states to measure inter-state variation in wage stickiness and assess whether this variation can account for differences in layoff rates and unemployment rate fluctuations. Inter-state differences in the the take-up of work sharing or short-time compensation—prorated unemployment benefits for workers whose hours are reduced for economic reasons—may be one source of policy-relevant variation in compensation stickiness (Abraham and Houseman, 2014). Fourth, we aim to more carefully consider displaced workers and effort responses to reduced wage cuts in future surveys. The former is related to work by Krolikowski (2017), Moore and Scott-Clayton (2019), and Lachowska et al. (2020), and the latter to Bewley (1999) and Bils et al. (2022). Our simple survey methodology makes all these extensions feasible, and with them we aim to gather further direct evidence on wage stickiness and reservation wages and to determine their roles in the functioning of labor markets.

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Figure 1: Survey timing and sample design

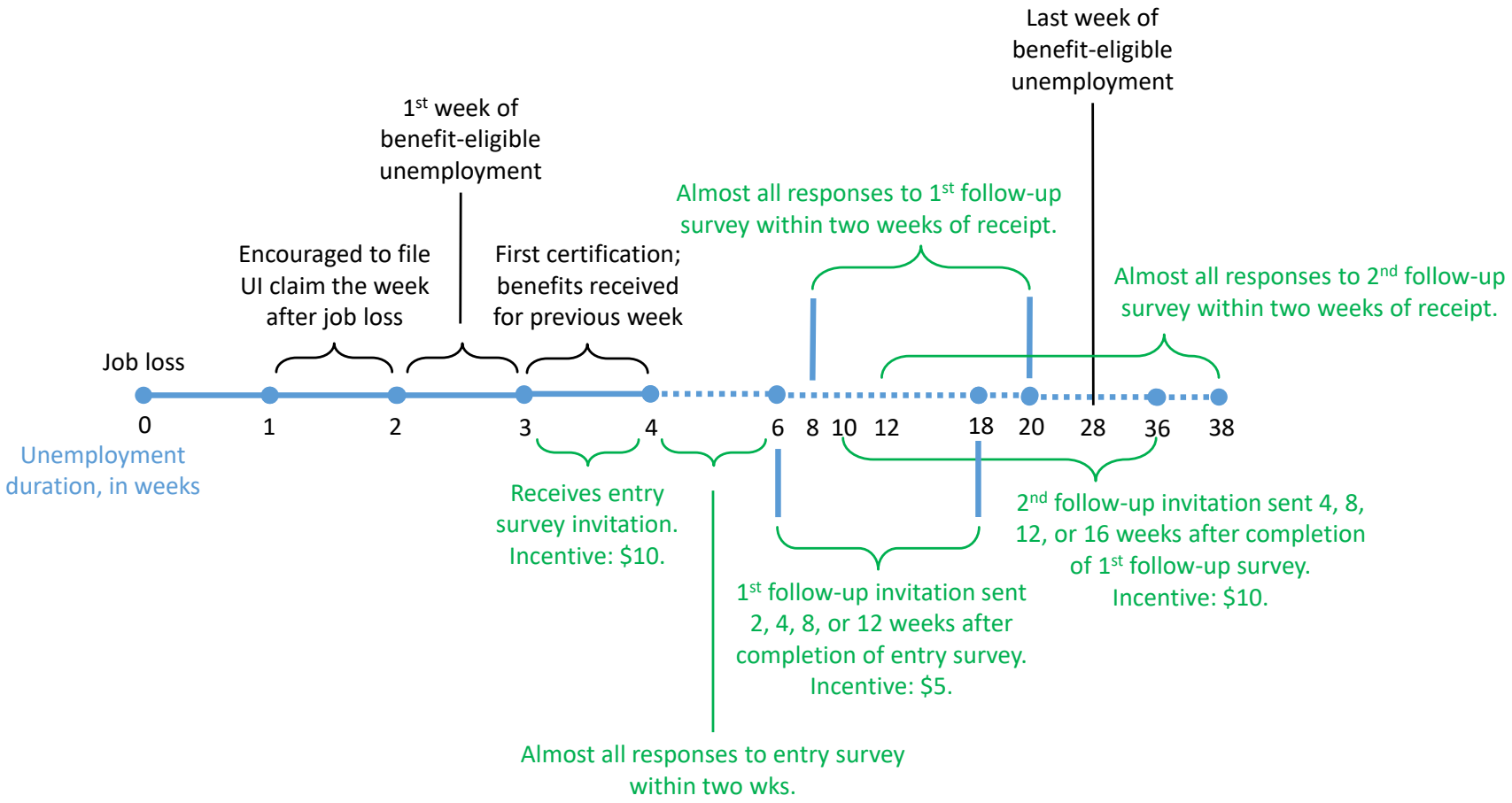
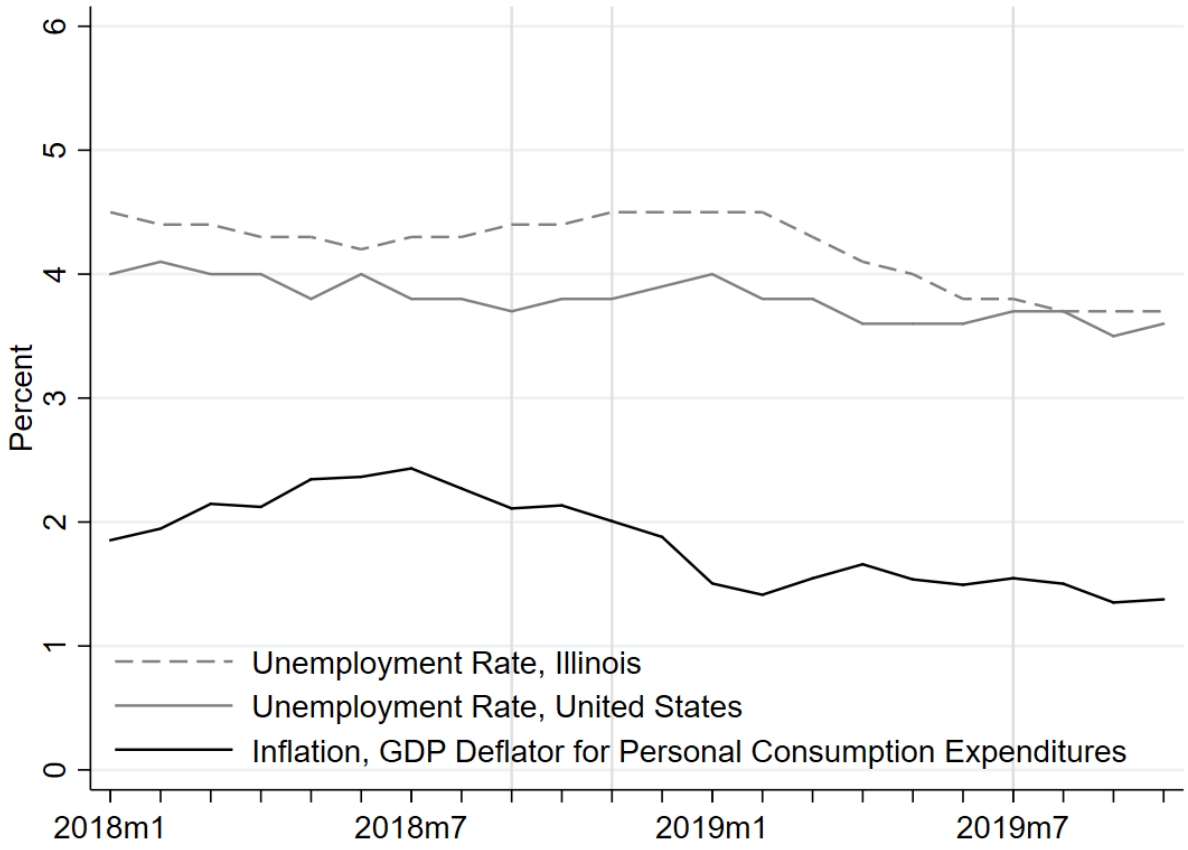
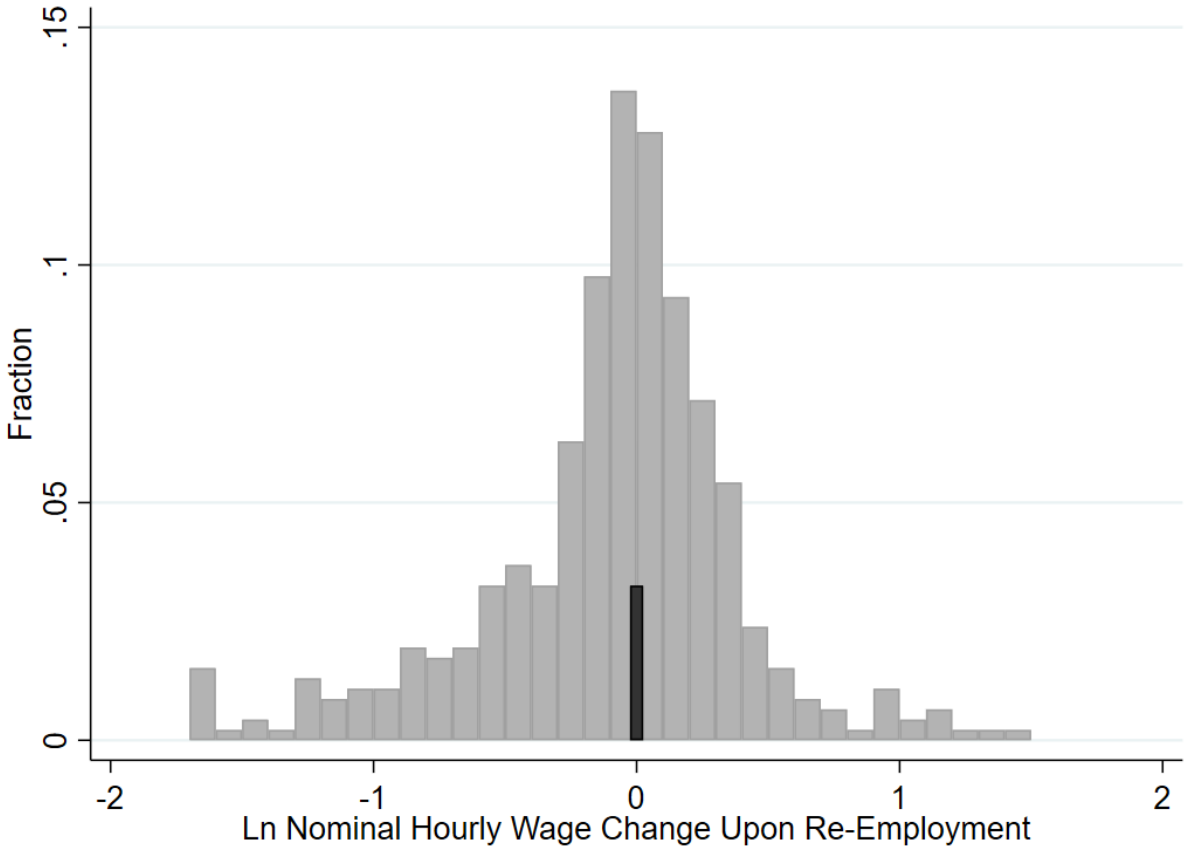


Figure 2: Survey sample period and seasonally adjusted unemployment and inflation rates



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.

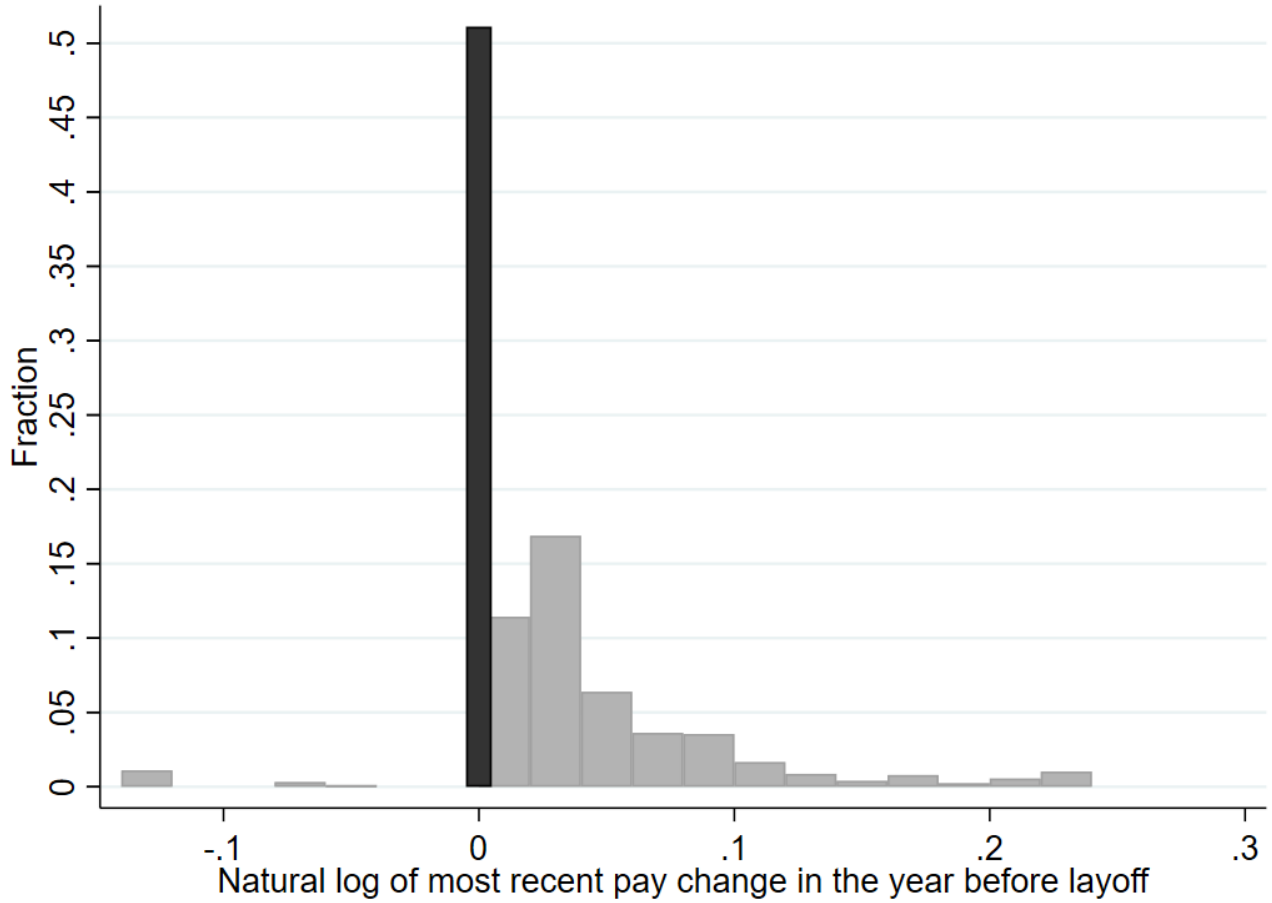
Figure 3: The Distribution of Wage Changes for Re-Employed Job Losers



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: Most recent change in pay

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, utilites	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 produced of two age groups, two education groups, and sex. Appendix A.8 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	0.000	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 paycut	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)***	(0.04)***	(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	0.11
	(0.03)***	(0.06)*	(0.03)***	(0.06)*
Experience	0.01	-0.01	0.01	-0.00
	(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	-0.07
	(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)**	(0.08)
More than 5yrs	-0.05	-0.21	-0.05	-0.16
	(0.04)	(0.07)***	(0.03)	(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			-0.00	0.00
			(0.00)	(0.01)
Union job (Yes=1)			-0.02	-0.17
			(0.05)	(0.05)***
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 did not finish high school and who had less than six months of job tenure at layoff. Industry wage premia are from Stansbury and Summers (2020). Wage residuals are from a Mincerian wage equation, as described in the text. Marginal effects at the mean in probit models are similar.

Table 5: Percent of workers 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 (%)	SE (%)	Obs
Overall	2.9	0.3	2,391
Type of layoff (p-value equality: 0.07)			
Permanent	2.5	0.4	1,935
Temporary	4.4	1.0	456
Industry (p-value equality: 0.45)			
Leisure and hospitality	4.0	1.6	151
FIRE	2.7	1.1	224
Construction	1.6	1.1	128
Educ & Hlth care services	2.5	0.8	396
Info & other services	2.7	1.1	225
Manufacturing	4.0	0.9	480
Prof, tech, bus services	1.7	0.7	298
Retail & wholesale trade	4.5	1.4	224
Transp, warehousing, utilities	2.7	1.3	147
Union job (p-value equality: 0.76)			
No	2.9	0.4	1,945
Yes	2.6	0.9	341
Tenure (p-value equality: 0.91)			
0-6mos	2.5	0.8	434
6mos to 2rs	2.7	0.6	806
2yrs to 5yrs	3.0	0.8	498
More than 5yrs	3.2	0.7	653
Reason for layoff (p-value equality: 0.01)			
Slow business conditions	5.1	0.9	593
Going out of business	3.2	1.4	156
Reorganization/position abolished	2.3	0.6	609
Fired	1.5	0.5	604
Firm size (p-value equality: 0.05)			
1-49	4.2	0.7	857
50-499	2.1	0.5	794
500+	2.6	0.6	609

Note: Standard errors and the number of observations appear in the second and third columns, respectively. See section 4.3 for details.

Table 6: Percent of workers 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=2,322	8.4	7.8	36.9		0.9	1.3	2.4	38.5	9.4
	(0.6)	(0.6)	(1.0)		(0.2)	(0.2)	(0.3)	(1.0)	(0.6)
Permanent layoff									
Count=1,886	8.9	8.9	38.4		0.9	1.4	2.4	39.0	10.6
	(0.7)	(0.7)	(1.1)		(0.2)	(0.3)	(0.4)	(1.1)	(0.7)
Temporary layoff									
Count=436	6.4	3.0	30.5		0.7	1.1	2.5	36.5	4.1
	(1.2)	(0.8)	(2.2)		(0.4)	(0.5)	(0.8)	(2.3)	(1.0)
500+ employees									
Count=593	8.8	9.4	34.7		0.5	1.5	3.0	34.6	10.6
	(1.2)	(1.2)	(2.0)		(0.3)	(0.5)	(0.7)	(2.0)	(1.3)
Union job									
Count=332	5.1	3.3	26.5	44.6	0.6	2.4	3.3	28.6	4.5
	(1.2)	(1.0)	(2.4)	(2.7)	(0.4)	(0.8)	(1.0)	(2.5)	(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
1.1	0.3	1.7	1.0	0.5	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. See section 5.1 for details.

Table 7: Percent of workers by reason for not accepting 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	1,022	50.5 (1.6)	37.6 (1.5)	20.2 (1.3)	19.1 (1.2)	4.3 (0.6)
A.1. Permanent layoffs from non-union jobs	961	51.3 (1.6)	37.9 (1.6)	19.6 (1.3)	19.1 (1.3)	4.3 (0.7)
A.2. Permanent layoffs from union jobs	61	37.7 (6.3)	32.8 (6.1)	29.5 (5.9)	18.0 (5.0)	4.9 (2.8)
		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		7.6 (0.9)	4.3 (0.7)	3.4 (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		8.2 (3.5)	8.2 (3.5)	1.6 (1.6)		
		(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	247	27.1 (2.8)	23.5 (2.7)	24.7 (2.7)	55.1 (3.2)	4.5 (1.3)
B.1. Temporary layoffs from non-union jobs	81	46.9 (5.6)	23.5 (4.7)	27.2 (5.0)	35.8 (5.4)	3.7 (2.1)
B.2. Temporary layoffs from union jobs	166	17.5 (3.0)	23.5 (3.3)	23.5 (3.3)	64.5 (3.7)	4.8 (1.7)
		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		29.6 (5.1)	2.5 (1.7)	3.7 (2.1)		
		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		45.2 (3.9)	0.6 (0.6)	16.3 (2.9)	2.4 (1.2)	0.6 (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. 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.4, 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, 2019b).[15] This means that roughly 40 percent of initial UI claims end up 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 that 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.

[15] 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 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 seven 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 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, like when they were laid off, their tenure, their industry and occupation, the number of people that 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 that 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. This survey approach, however, misses changes in job search activity at the beginning of an unemployment spell, when they are perhaps the most dramatic (Faberman and Kudlyak, 2019). After verifying a respondent's e-mail address, they were sent their incentive payment.

A.4 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.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 UI in Illinois During our Survey

At the time our Entry Survey was in the field, job losers in Illinois were eligible for a maximum of 26 weeks of UI benefits. Replacement ratios in Illinois were about 45 percent when our survey was in the field (DOLETA, 2019a), as measured by the weighted average of the weekly benefit amount over the average weekly wage. The maximum weekly benefit amount in 2018 (2019) was $458 ($471) for individuals, $545 ($561) for individuals with spouses,

and $627 ($648) for individuals with child (IDES, 2018, 2019). The minimum qualifying earnings in 2018 ad 2019 were $1600 in the base period, with at least $440 outside of the highest quarter. The base period is defined as the first four of the last five completed calendar quarters immediately preceding the beginning of an individual's benefit year. UI benefits recipients in the state of Illinois are required to actively seek employment and to register with the Illinois Employment Service system called Illinois Job Link.

A.7 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 had the option to write in a response, like the individuals' 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.8 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.

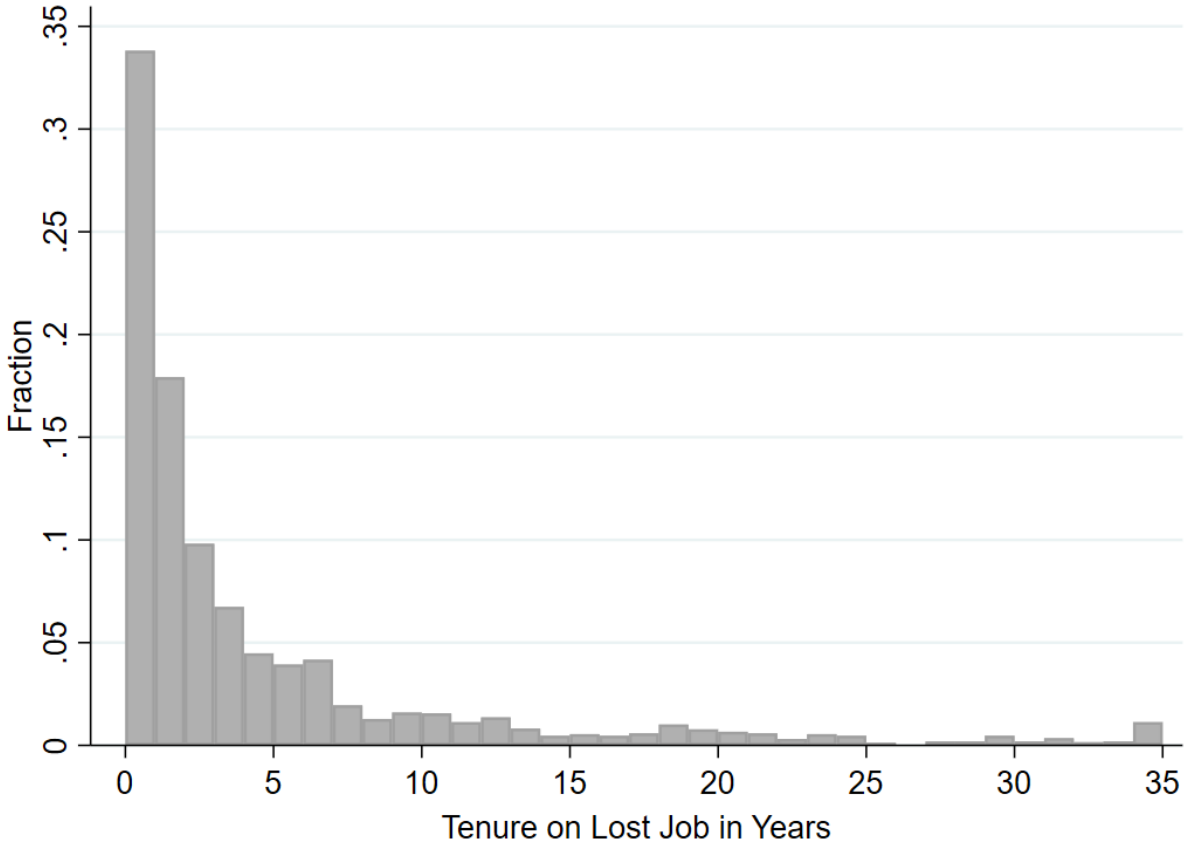
First, many workers are willing to accept pay cuts, and often large ones, to stay on their previous job, as shown in table 3. About 60 percent of workers on permanent layoff were willing to accept a 5 percent pay cut in lieu of layoff and about one-third were willing to accept a 25 percent pay cut. These 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 4. 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 are similar when using weighted and unweighted data (compare appendix table 5 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 are similar when using weighted and unweighted data (compare appendix table 6 wtih table 7). The most common reason, reported by about half of non-union 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.

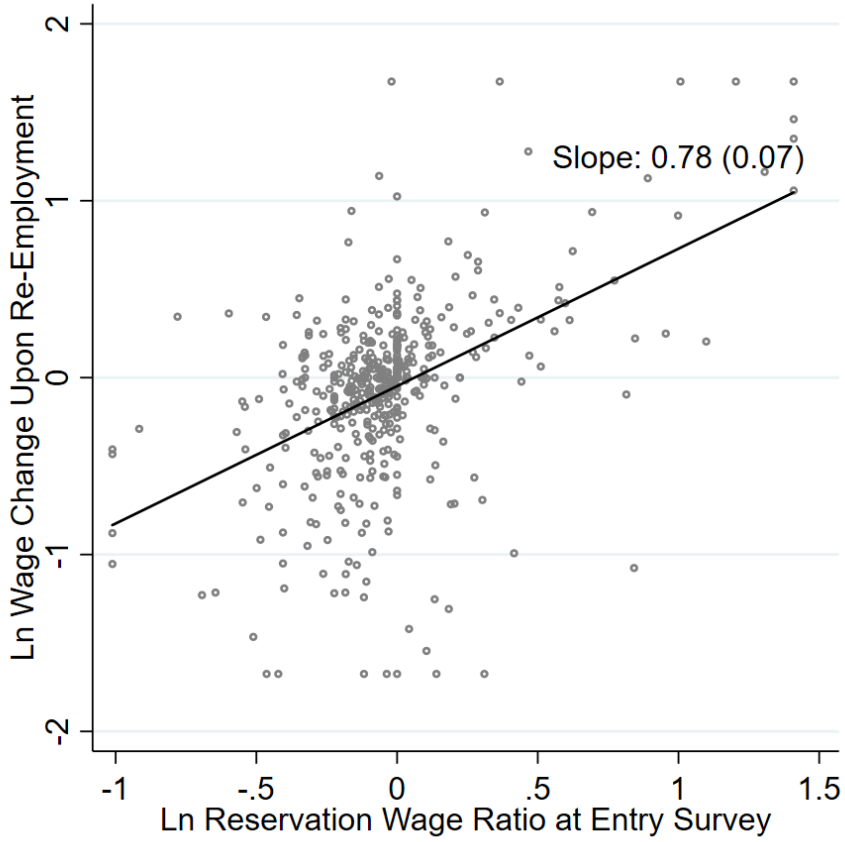
Appendix Figure 1: Tenure distribution for Entry-Survey sample



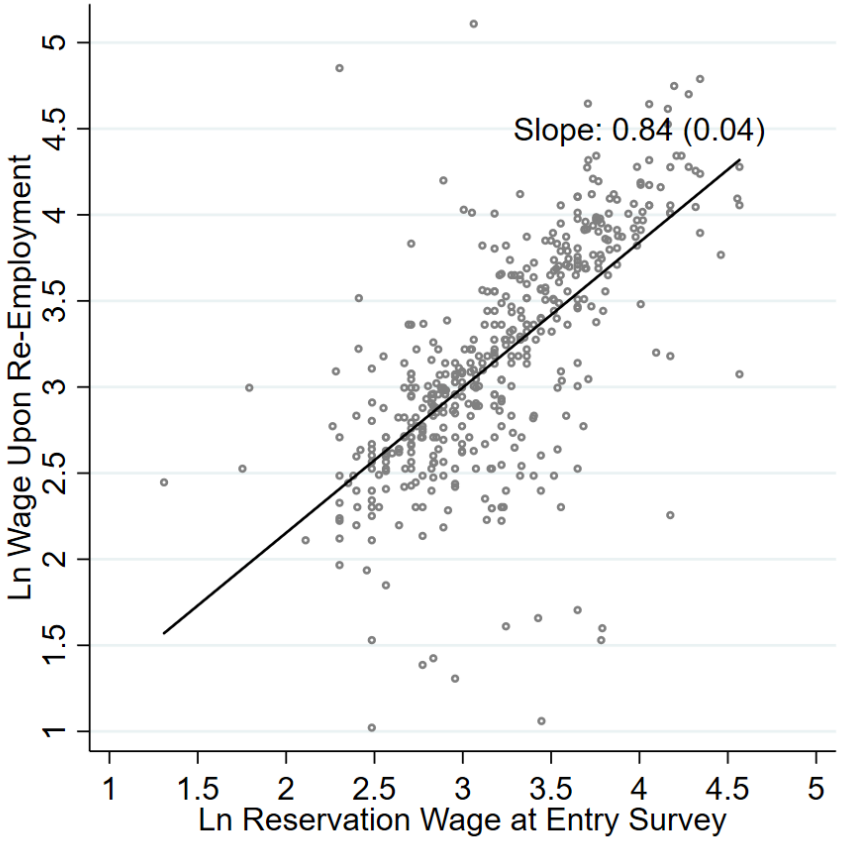
Note: We winsorize observations above the 99th percentile. See section 3.4 for details.

Appendix Figure 2: Reservation and re-employment wages

(a) Hourly wage ratios



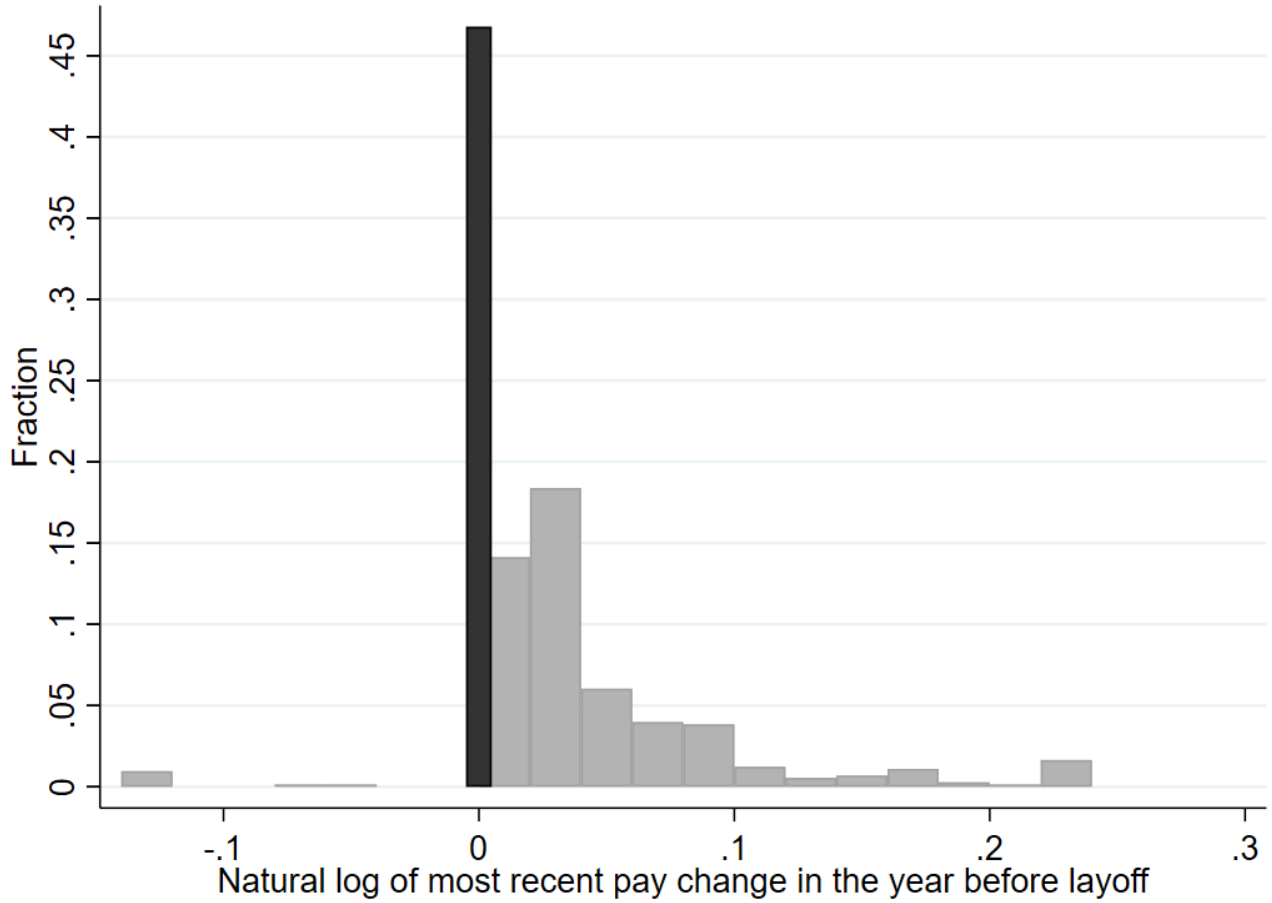
(b) Hourly wages



Note: Figure 2a 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 2b 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 2a 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 2a (Figure 2b).

Appendix Figure 3: Most recent change in pay

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 three years 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. This figure is similar to figure 4 in the main text.

Appendix Table 1: 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 in 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 that 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.4 for details about incentives and incentive experimentation.

Appendix Table 2: 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 rightmost column reports statistics for employed persons in the CPS from (Hyatt and Spletzer, 2016, Figure 1).

Appendix Table 3: 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.

Appendix Table 4: Percent of workers who discussed a cut in pay, benefits or hours in lieu of layoff (weighted)

	Mean (%)	SE (%)	Obs
Overall	3.0	0.3	2,391
Type of layoff (p-value equality: 0.20)			
Permanent	2.6	0.4	1,935
Temporary	4.0	0.9	456
Industry (p-value equality: 0.04)			
Leisure and hospitality	5.5	1.9	151
FIRE	2.9	1.1	224
Construction	1.5	1.1	128
Educ & Hlth care services	2.5	0.8	396
Info & other services	2.7	1.1	225
Manufacturing	4.1	0.9	480
Prof, tech, bus services	1.4	0.7	298
Retail & wholesale trade	3.7	1.3	224
Transp, warehousing, utilities	2.7	1.3	147
Union job (p-value equality: 0.34)			
No	3.2	0.4	1,945
Yes	2.3	0.8	341
Tenure (p-value equality: 0.78)			
0-6mos	2.7	0.8	434
6mos to 2rs	2.6	0.6	806
2yrs to 5yrs	3.8	0.9	498
More than 5yrs	3.2	0.7	653
Reason for layoff (p-value equality: 0.04)			
Slow business conditions	5.3	0.9	593
Going out of business	1.8	1.1	156
Reorganization/position abolished	2.6	0.6	609
Fired	1.6	0.5	604
Firm size (p-value equality: 0.11)			
1-49	4.4	0.7	857
50-499	2.3	0.5	794
500+	2.6	0.6	609

Note: Standard errors and the number of observations appear in the second and third columns, respectively. Similar to table 5 in the main text, but uses CPS weights. See appendix B for details.

Appendix Table 5: 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=2,322	7.9	6.9	35.1		0.9	1.3	2.6	38.7	8.6
	(0.6)	(0.5)	(1.0)		(0.2)	(0.2)	(0.3)	(1.0)	(0.6)
Permanent layoff									
Count=1,886	8.8	8.2	37.0		1.1	1.4	2.5	39.7	10.1
	(0.7)	(0.6)	(1.1)		(0.2)	(0.3)	(0.4)	(1.1)	(0.7)
Temporary layoff									
Count=436	5.7	3.3	29.8		0.5	1.1	3.1	35.8	4.2
	(1.1)	(0.9)	(2.2)		(0.3)	(0.5)	(0.8)	(2.3)	(1.0)
500+ employees									
Count=593	8.2	7.9	32.7		0.6	2.0	4.1	33.7	9.4
	(1.1)	(1.1)	(1.9)		(0.3)	(0.6)	(0.8)	(1.9)	(1.2)
Union job									
Count=332	5.3	3.6	25.2	46.3	0.6	2.2	3.7	27.9	4.6
	(1.2)	(1.0)	(2.4)	(2.7)	(0.4)	(0.8)	(1.0)	(2.5)	(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
0.7	0.3	1.7	0.9	0.4	2.2	4.0

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.

Appendix Table 6: 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	1,022	48.5 (1.6)	37.7 (1.5)	20.8 (1.3)	17.7 (1.2)	4.0 (0.6)
A.1. Permanent layoffs from non-union jobs	961	49.8 (1.6)	38.0 (1.6)	19.5 (1.3)	17.7 (1.2)	4.0 (0.6)
A.2. Permanent layoffs from union jobs	61	36.5 (6.2)	34.5 (6.1)	31.9 (6.0)	17.8 (4.9)	4.1 (2.6)
		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	184	7.6 (0.9)	3.9 (0.6)	2.3 (0.5)	0.6 (0.3)	3.5 (0.6)
		Can't afford the pay cut	Union agreement	Miscellaneous		
A.4. Other detail for permanent layoffs from union jobs	11	9.2 (3.7)	7.4 (3.4)	1.2 (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	247	27.1 (2.8)	23.5 (2.7)	24.7 (2.7)	55.1 (3.2)	4.5 (1.3)
B.1. Temporary layoffs from non-union jobs	81	46.9 (5.6)	23.5 (4.7)	27.2 (5.0)	35.8 (5.4)	3.7 (2.1)
B.2. Temporary layoffs from union jobs	166	17.5 (3.0)	23.5 (3.3)	23.5 (3.3)	64.5 (3.7)	4.8 (1.7)
		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	29	30.4 (5.1)	2.5 (1.7)	2.5 (1.8)		
		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	107	44.0 (3.9)	0.9 (0.7)	18.7 (3.0)	2.3 (1.2)	0.9 (0.7)

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.