Downward Nominal Wage Rigidity and Job Destruction*

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November 9, 2020

Abstract

Whether downward nominal wage rigidity causes firms to destroy jobs affects both optimal monetary policy and the asymmetry of employment fluctuations over the business cycle. This paper provides quasi-experimental evidence that downward nominal wage rigidity causes firms to destroy jobs. The quasi-random variation in firm's exposure to downward nominal wage rigidity stems from the combination of i) the unanticipated nature of the financial collapse in Q3 of 2008, and ii) seasonal differences in the timing of firms' annual nominal wage adjustment. As a lower bound, I find that exposure to downward nominal wage rigidity accounts for 23% of the spike in aggregate job destruction that occurred in Q4 of 2008.

*I am deeply indebted to my dissertation committee: Borağan Aruoba, John Haltiwanger, Judy Hellerstein, Henry Hyatt, and John Shea, for their support and guidance. I also want to thank Katharine Abraham, Joonkyu Choi, Leland Crane, Joanne Gaskell, Jessica Goldberg, Ethan Kaplan, Erika McEntarfer, Michael Murray, Felipe Saffie, Kristin Sandusky, Matthew Staiger, Lesley Turner, Larry Warren, the seminar participants at the University of Maryland, the Bureau of Labor Statistics, Bates College, and the U.S. Census Bureau, and the LEHD group at the Center for Economic Studies in the U.S. Census Bureau for engaging with me on my research. I am grateful for the financial support of the Center for Retirement Research and the Betancourt Fellowship. The opinions expressed herein are those of the author alone and do not necessarily reflect the view of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential data are disclosed. See U.S. Census Bureau Disclosure Review Board bypass numbers: DRB-B0073-CED-20190910, DRB-B0069-CED-20190725, DRB-B0037-CED-20190327, CBDRB-2018-CDAR-061. Email: seth.m.murray@frb.gov

1 Introduction

A prominent question in macroeconomics is the role of downward nominal wage rigidity (DNWR) in explaining employment fluctuations over the business cycle. Whether and how DNWR affects employment has important implications for a wide range of macroeconomic questions, including: why does the effectiveness of monetary policy differ for contractionary versus expansionary monetary policy shocks; what is the Federal Reserve's optimal inflation target; and why do employment and output exhibit asymmetric fluctuations over the business cycle. Despite the importance of these questions and the central role of downward nominal wage rigidity in many New Keynesian DSGE models[1] and labor search-and-matching models,[2] there is remarkably little empirical evidence whether DNWR causes firms to destroy jobs (or change their employment allocations more generally).

This paper presents quasi-experimental evidence that DNWR plays an important causal role in employment fluctuations through the job destruction margin. I begin by establishing two empirical regularities: i) that within a firm, employees' nominal wage raises tend to be synchronized to occur in the same calendar quarter year-over-year, and ii) the calendar timing of these synchronized wage raises differs across firms. The quasi-experiment then exploits exogenous variation in firms' exposure to DNWR generated by the timing of an unanticipated negative aggregate shock relative to the calendar quarter in which the firms tend to raise their workers' nominal wages.

The financial collapse precipitated by the failure of Lehman Brothers in the last month of 2008:Q3 was a large, unanticipated negative aggregate shock. In 2008:Q4, immediately after the collapse, firms that historically tended to raise their workers' wages in the fourth calendar quarter ("Q4-raising firms"), could choose to freeze workers' nominal wages, thereby lowering the firms' real wage bills. Conversely, firms that typically raised workers' wages in the second calendar quarter ("Q2-raising firms") would have raised their workers' wages in 2008:Q2, not anticipating the financial collapse in 2008:Q3. As a result, the Q2-raising firms would have had to cut their workers' nominal

[1] Kim and Ruge-Murcia (2009), Fagan and Messina (2009), and Schmitt-Grohé and Uribe (2013) use DSGE models with DNWR to explore the optimal inflation rate. Benigno and Ricci (2011) and Daly and Hobijn (2014) examine the importance of DNWR for the Phillips Curve. Abbritti and Fahr (2013) and Schmitt-Grohé and Uribe (2016) argue that DNWR may be responsible for the asymmetric distribution of unemployment and output over the business cycle. Schmitt-Grohé and Uribe (2017) examine the role of DNWR in explaining jobless recoveries. Shen and Yang (2018) demonstrate the DNWR causes the fiscal multiplier to vary over the business cycle. Eggertsson, Mehrotra and Robbins (2019) include DNWR in a New Keynesian DSGE model exhibiting secular stagnation.

[2] Elsby (2009); Carlsson and Westermark (2016); Chodorow-Reich and Wieland (forthcoming); and Dupraz, Nakamura and Steinsson (2019) embed DNWR into search-and-matching models of the labor market to examine employment fluctuations over the business cycle.

wages to achieve a decrease in the firms' real wage bills similar to that of the Q4-raising firms. If exposure to DNWR has a causal effect on job destruction, then we should expect larger increases in the rate of job destruction at Q2-raising firms relative to otherwise similar Q4-raising firms.

For the quasi-experiment, I use a 10% random sample of firms from 30 states in the U.S. Census Bureau's LEHD data set, an employer-employee linked administrative data set covering approximately 96% of employment in each state. I begin by employing the set of machine learning methods described in Murray (2020) to extract estimates of the timing and magnitude of each worker's unobserved persistent base wage changes from the worker's observed quarterly earnings. I then use these estimated persistent wage changes to identify the firm-specific seasonal adjustment patterns in workers' nominal wages.

I find that the job destruction rate at Q2-raising firms increased by 36% in 2008:Q4, nearly double that of the 19% increase in the job destruction rate of Q4-raising firms (who had less exposure to DNWR). I further find that if all firms had been Q4-raisers, then U.S. firms' real wage bills at the start of 2008:Q4 would have been 1.1% lower and the increase in the job destruction rate would have been 23% less in 2008:Q4. Since this estimate does not take into account the fact that the Q4-raising firms had less, but still some exposure to DNWR, this estimate is a lower-bound of DNWR's effect on job destruction in 2008:Q4.

My quasi-experimental empirical finding that exposure to DNWR causes a substantial increase in firms' job destruction rates is inconsistent with many common macro models of employment with wage rigidity.[3] The literature has mostly dismissed the effect of DNWR on job destruction, instead focusing on how DNWR can affect employment through either the intensive margin of hours worked or the extensive margin of job creation. My results show that the causal effect of DNWR on job destruction is sufficiently large that our models should account for DNWR's effect on employment through the job destruction margin.

The key contribution of this paper is to the empirical literature examining causal links between DNWR and firm-level employment allocation.[4] Establishing a causal link between downward nom-

[3] When search-and-matching models of the labor market incorporate wage rigidity, the models tend to assume that the wage rigidity affects job creation, but not job destruction. New Keynesian DSGE models where wage adjustment costs generate nominal wage rigidity (a la Rotemberg (1982)) oftentimes only find small effects of nominal wage rigidity on employment. DSGE models that generate nominal wage rigidity using staggered wage adjustment (Taylor (1980)) or random arrival of wage adjustment opportunities (Calvo (1983)) find larger effects of wage rigidity on employment, but violate the Barro critique. Barro (1977) argues that models should not have wage rigidity affect employment by assuming that workers and firms ignore mutually beneficial nominal wage cuts.

[4] A number of other studies have explored DNWR and its effect on employment at a more aggregate level. Bernanke and Carey (1996) examine the role of DNWR in explaining countries' differential response during the Great Depression

inal wage rigidity and job destruction specifically, or employment in general, is notoriously hard. The challenge lies in identifying variation in a firm's exposure to downward nominal wage rigidity that is exogenous with respect to the unobserved factors affecting its employment decisions. This is difficult because firms' wage setting and employment decisions are forward looking and almost inextricably intertwined.

I am aware of only three studies that examine whether nominal wage rigidity affects firm-level employment. Card (1990) examines unionized Canadian manufacturing firms. He finds that when unionized manufacturing firms are bound by fixed nominal wage contracts, firm employment moves in the opposite direction of unexpected real wage changes. While Card focuses on firms' overall employment, the studies by Kurmann and McEntarfer (2019) and Ehrlich and Montes (2019) examine the distinct effects of DNWR on the job creation / hiring and job destruction / layoff margins. Both studies use employer-employee linked administrative data to establish a measure of individual firms' historical incidence of DNWR. They then examine whether a firm's job destruction or layoffs respond more strongly to a negative shock if the firm historically exhibited more DNWR.[5] Both studies find substantial effects on layoffs or job destruction from greater exposure to downward nominal wage rigidity.

The quasi-experimental evidence presented in this paper contributes to this sparse empirical literature in two ways. First, the identification strategy employed by this paper applies to nearly all firms, not just firms in a particular industry or those firms with high historical levels of nominal wage rigidity. As a result, my empirical results can be interpreted as the average treatment effect of downward nominal wage rigidity on job destruction for the population of U.S. firms. Second, this paper demonstrates a causal effect of exposure to DNWR on job destruction by relying on

to monetary policy shocks transmitted through the gold standard. Fehr and Goette (2005) and Ridder and Pfajfar (2017) use variation in localities' exposure to wage rigidity in a monetary union (Switzerland and the United States respectively) to identify the effect of DNWR on local employment. Pischke (2018) finds that occupations with greater wage flexibility (real estate agents) had smaller employment declines in response to the post-2006 U.S. housing market collapse relative to occupations with more rigid wages (architects and construction workers). Kaur (2019) examines agricultural employment in Indian villages - finding that wages exhibit downward nominal rigidity and that this rigidity causes agricultural employment to fall if a positive rainfall shock is followed by a negative shock.

[5] In the case of Kurmann and McEntarfer (2019), they use LEHD data for Washington state to construct various firm-level measures of wage rigidity in the years prior to 2008, and then they examine the differential response of job destruction and other employment outcomes during the Great Recession at firms with more historical nominal wage rigidity. Ehrlich and Montes (2019) use administrative data from Germany to construct similar firm-specific measures of historical DNWR. Since the persistence of productivity shocks may confound the relationship between firms' historical incidence of DNWR and current productivity shocks, Ehrlich and Montes (2019) use collectively bargained wage floors that are set at a region-industry level as an instrumental variable for the firms' DNWR measure. These region-industry specific wage floors are valid instruments if, as Ehrlich and Montes (2019) argue, the business conditions of firms in a given region and industry do not affect the bargaining of the wage floor for that industry and region.

a different set of identifying assumptions than Kurmann and McEntarfer (2019) and Ehrlich and Montes (2019). Identification in these earlier studies requires that no confounding variables affect both the historical incidence of DNWR at the firm (or the industry-region for Ehrlich and Montes) and the firm's current-period employment decisions. Thus, persistent negative shocks affecting both the historical incidence of wage rigidity and current-period business conditions threaten the validity of these studies' identifying assumptions. On the other hand, the identifying assumption of the quasi-experiment presented in this paper is robust to persistent negative shocks. Specifically, my quasi-experiment assumes that no confounding variables affect both i) the calendar quarter in which a firm has historically tended to raise its workers' nominal wages, and ii) the firm's employment decisions when it experiences an unanticipated negative aggregate shock.

The broad theoretical literature that examines the macroeconomic implications of wage rigidity (for employment, output, and monetary and fiscal policy) tends to ignore any potential effect of DNWR on firms' job destruction. Consequently, my quasi-experimental finding that this effect is large in magnitude should influence the future direction of the theoretical literature. One class of these theoretical models are the New Keynesian DSGE models wherein wage rigidity affects employment through the margin of hours worked. These models typically incorporate wage rigidity through one of three mechanisms: i) time-dependent wage adjustment opportunities (à la Calvo (1983) random intervals or Taylor (1980) fixed intervals between opportunities to adjust wages),[6] or ii) restrictions on period-over-period declines in wages,[7] or iii) wage adjustment costs (à la Rotemberg (1982)).[8] The first two of these mechanisms run afoul of Robert Barro's influential critique of wage rigidity models. Namely, Barro (1977) argues that a model should not generate an effect of wage rigidity on employment by requiring that workers and firms ignore mutually advantageous wage changes. The models with wage adjustment costs are robust to the Barro critique but, counter to my empirical results, oftentimes find small effects of wage rigidity on employment (e.g. Kim and Ruge-Murcia (2009)).

A second class of models that examine the implications of wage rigidity for fluctuations in employment are the labor search-and-matching models. Hall (2005) was the first to show that if

[6]Models of DNWR with time-dependent pricing include: Daly and Hobijn (2014); Carlsson and Westermark (2016); and Mineyama (2018).

[7]Models of DNWR with restrictions on period-over-period declines in wages include: Akerlof, Dickens, Perry, Gordon and Mankiw (1996); Benigno and Ricci (2011); Schmitt-Grohé and Uribe (2016); and Chodorow-Reich and Wieland (forthcoming).

[8]Models of DNWR with wage adjustment costs include: Kim and Ruge-Murcia (2009); Fahr and Smets (2010); Abbritti and Fahr (2013); and Aruoba, Bocola and Schorfheide (2017).

wages are rigid but they adjust to preserve positive-surplus jobs, then the wage rigidity affects employment in a search-and-matching model without violating Barro's critique. In such a case, wage rigidity affects the job creation margin through its effect on firm surplus, but wage rigidity does not affect job destruction. Importantly, Hall (2005) shows that incorporating wage rigidity significantly improves the empirical fit of the model to the volatility of employment and vacancies. Subsequent search-and-matching labor market models that incorporate wage rigidity have similarly maintained a focus on the job creation margin.[9] This focus on hiring and job creation also shifted the search-and-matching literature to emphasize new hires' wage rigidity, since incumbent workers' wage rigidity is not relevant for firms' job creation decisions in most search and matching models (Pissarides (2009), Kudlyak (2014)).[10] As a result, the search-and-matching literature generally ignores both rigidity in incumbent workers' wages and any potential effect of wage rigidity on the job destruction margin.[11]

2 Data and Wage Measurement

2.1 Data

My proposed quasi-experiment requires panel data on both firms' employment levels and workers' nominal wages at a sub-annual frequency. This paper uses the U.S. Census Bureau's LEHD data set - an employer-employee linked data set with quarterly earnings for approximately 96% of all employment in a state. The quarterly earnings data in the LEHD is derived from firms' mandatory unemployment insurance filings. This earnings data is complemented with both worker characteristics (age, sex, race, and education) and firm characteristics (industry, firm age, and firm size) from other data sources. Individuals are uniquely identified by a Protected Identification Key (PIK) that allows each individual to be tracked across different employers and locations. The

[9] Examples include Hall and Milgrom (2008), Hagedorn and Manovskii (2008), Pissarides (2009), Gertler and Trigari (2009), Elsby (2009), Kudlyak (2014), Schoefer (2016), Bils, Chang and Kim (2016), Hazell and Taska (2018), Dupraz, Nakamura and Steinsson (2019), and Chodorow-Reich and Wieland (forthcoming).

[10] A number of papers have since evaluated the relative cyclicality of new hire versus incumbent wage rigidity, with conflicting results. For instance, Haefke, Sonntag and Van Rens (2013) find significantly greater cyclicality in new hires' wages relative to incumbents' wages. Gertler, Huckfeldt and Trigari (forthcoming), on the other hand, find that the greater wage cyclicality of new hires largely comes from job switchers. When they restrict their analysis to the wage cyclicality of new hires from unemployment, they find it matches the cyclicality of incumbents' wages.

[11] Four notable exceptions examine how incumbents' wage rigidity can affect employment through either efficiency wages (Elsby (2009) and Bils, Chang and Kim (2016)), or financial frictions (Schoefer (2016)), or the random arrival of opportunities to adjust workers' wages (Carlsson and Westermark (2016)). The first three papers ignore the potential effect of DNWR on the rate of job destruction. Carlsson and Westermark (2016) do consider the effect of wage rigidity on job destruction, but assume that opportunities to renegotiate incumbents' wages arrive randomly. While their model does generate inefficient job destruction, it violates the Barro critique since in periods when the opportunity to renegotiate wages does not arrive workers and firms must ignore mutually advantageous wage cuts.

LEHD identifies employers at the level of a state employer identification number (SEIN). Firm age and firm size are derived from aggregating of one or more SEINs (potentially across states) to the level of the federal employer identification number (EIN). For simplicity, I refer to each SEIN as a firm. Although revenue data is not available in the LEHD data set, each SEIN is linked to an EIN for which I obtain real revenue and real revenue per worker at an annual frequency from the Census Bureau's revenue-enhanced Longitudinal Business Database (Haltiwanger, Jarmin, Kulick and Penciakova (2019)).

This paper extracts two samples from the LEHD data set. The primary sample used for the quasi-experiment consists of a 10% random sample of SEINs from thirty states covering the period from 1998:Q1 to 2017:Q1.[12] However, when I examine labor market outcomes for individual workers, such as employment-to-nonemployment (EN) or employer-to-employer (EE) transitions, I identify these transitions using 100% of the worker-firm observations in the 30 states. I then restrict my subsequent analysis to workers at the 10% random sample of firms. Using 100% of the worker-firm observations ensures that these labor market transitions are accurately identified, since otherwise some employer-to-employer transitions would incorrectly be categorized as employment-to-nonemployment transitions.

One limitation of the LEHD dataset is that it only reports each worker's quarterly earnings, which is a function of not only the worker's base wage, but also the number of hours worked as well as any variable compensation (e.g. annual bonuses, commissions, tips, and overtime). In order to overcome this limiation, I implement the machine learning procedures described in Murray (2020) that identify workers' unobserved base wage changes from their observed quarterly earnings.

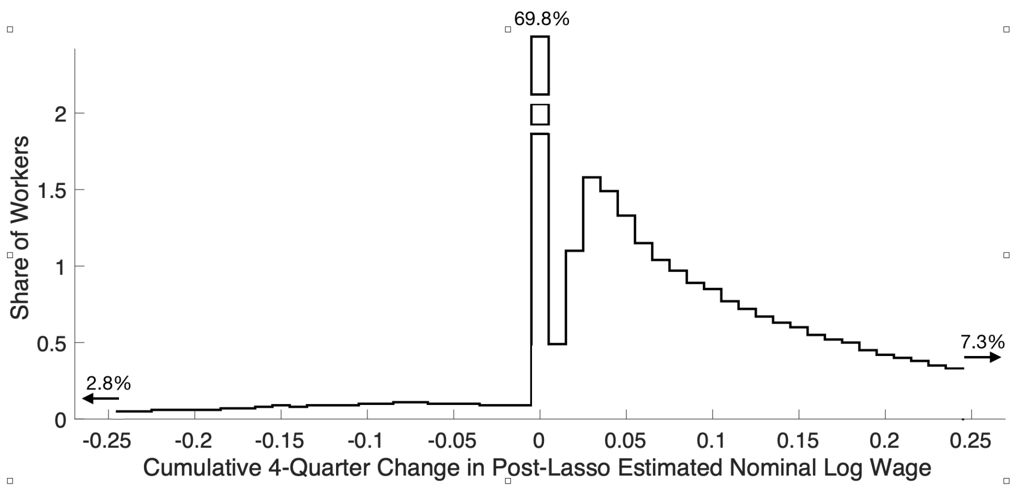
[12]The states included in the primary sample are: CA, CO, CT, FL, GA, HI, ID, IL, IN, KS, LA, MD, ME, MT, NC, ND, NJ, NM, NV, OR, PA, RI, SC, SD, TN, TX, VA, WA, WI, and WV. I chose these thirty states because there are no gaps in reported quarterly earnings for any of these states over the sample period.

3 Patterns of Wage Adjustment

3.1 Wages Exhibit Downward Nominal Rigidity

Consistent with previous studies,[13] the post-Lasso estimated persistent base wages exhibit significant downward nominal wage rigidity. Figure 1 shows the histogram of annual post-Lasso estimated nominal wage changes within ±25 log points of zero. The histogram of estimated persistent base wage changes indicates that a large mass of workers have no change in their nominal wages year-over-year, and that the distribution of nominal wage changes is missing mass to the left of zero nominal change.

Figure 1: Histogram of Cumulative 4-Quarter Change in Persistent Log Nominal Wage



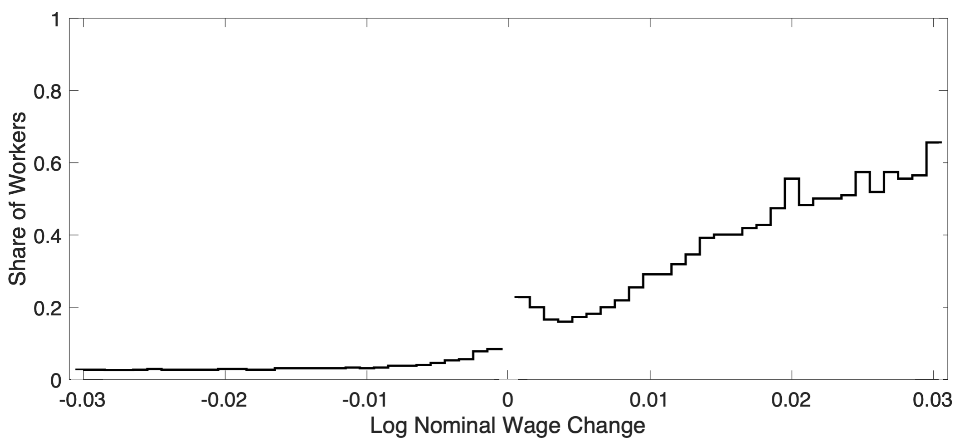
Notes: Histogram of workers' four-quarter cumulative change in their post-Lasso estimated persistent log nominal wage. Estimated from the Primary Sample after restricting to workers with at least five full quarters of non-zero earnings. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0037-CED-20190327.

DNWR would imply that there is a discontinuous drop in the frequency of nominal wage adjustment immediately below zero nominal change. Such a discontinuity is apparent in Figure 2, which shows the histogram of nominal changes in 0.1 log point bins within 3 log points of zero

[13]Studies documenting downward nominal wage rigidity using the Panel Study of Income Dynamics (PSID) include: McLaughlin (1994); Lebow, Stockton and Wascher (1995); Akerlof, Dickens, Perry, Gordon and Mankiw (1996); Card and Hyslop (1997); Kahn (1997); Altonji and Devereux (2000); and Dickens, Goette, Groshen, Holden, Messina, Schweitzer, Turunen and Ward (2007). Studies using the Current Population Survey (CPS) include: Card and Hyslop (1997); Daly and Hobijn (2014); Elsby, Shin and Solon (2016); and Jo (2019). Studies using the Survey on Income and Program Participation (SIPP) include: Gottschalk (2005); and Barattieri, Basu and Gottschalk (2014). Studies using the Employer Cost Index (ECI) survey include: Lebow, Saks and Wilson (2003); and Fallick, Lettau and Wascher (2016). Studies using unemployment insurance administrative data include: Kurmann and McEntarfer (2019) and Jardim, Solon and Vigdor (2019). Grigsby, Hurst and Yildirmaz (2019) use ADP payroll data. Hazell and Taska (2018) using job posting data from Burning Glass.

nominal change. I more formally test for the presence of DNWR in two ways. First, in Appendix **??**, I estimate a series of regression discontinuity models that check for a discontinuity in the distribution of nominal wage changes at zero nominal change. For all model specifications (changing both bandwidths and polynomials in the running variable), I find evidence of a discontinuity in the distribution at zero nominal change - implying the existence of downward nominal wage rigidity. Second, in Appendix **??**, I employ a variant of the method proposed by Kahn (1997) for measuring how much the frequency of observing a real wage change of a given size falls when that real wage change requires a nominal wage cut. I find that 55% of "optimal" real wage changes are suppressed when they require a nominal wage cut.

Figure 2: Histogram of Persistent Nominal Wage Changes at Annual Frequency Near Zero

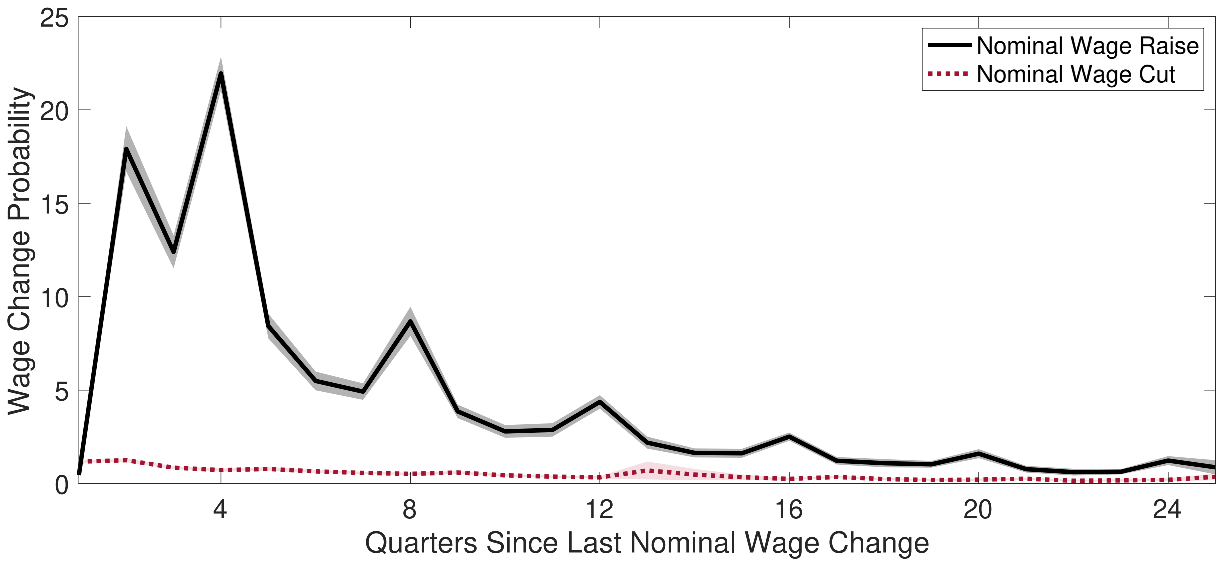


Notes: Frequency of four-quarter cumulative change in workers' post-Lasso estimated persistent log nominal wage grouped into 0.1 log point change bins. Sample is restricted to workers with at least five full-quarters of non-zero earnings. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0037-CED-20190327.

3.2 Annual Schedules of Nominal Raises

I find workers' nominal wages in the LEHD data set broadly follow an annual schedule of nominal raises. This finding is consistent with Taylor (1980) which theorizes that workers receive wage adjustments at regularly scheduled intervals. Figure 3 shows that a worker's nominal wage raise hazard rate spikes four quarters after the worker's last nominal wage change and every subsequent four-quarter anniversary. On the other hand, nominal wage cuts do not exhibit a Taylor-style annual schedule. The figure plots the coefficient estimates (and their 95% confidence intervals) from regressing an indicator variable equal to one if a worker receives a nominal wage raise on a set of dummy variables for the number of quarters since the worker's last wage change (and similarly for nominal wage cuts). The result that nominal wage raises follow an annual adjustment schedule is consistent with the finding of Barattieri, Basu and Gottschalk (2014) that the wage change hazard rate in the PSID spikes twelve months after the last wage change (although they did not find a similar pattern at subsequent annual anniversaries of the last wage change, and they did not separately examine wage cuts).

Figure 3: Probability of Wage Raise or Cut by Quarters Since Last Wage Change



Notes: Coefficient estimates from a linear regression model of the probability of an increase (raise) or decrease (cut) in the post-Lasso estimated nominal persistent base wage given the number of quarters since the worker's last wage change. Shaded areas correspond to 95% confidence intervals using robust standard errors clustered at the SEIN level. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

That nominal raises exhibit a Taylor-style annual raise schedule while nominal cuts do not may have implications for asymmetries in the effectiveness of monetary policy. Dixon and Le Bihan (2012) show that Calvo and Taylor-style wage adjustment assumptions generate different output and employment responses to monetary policy shocks, with greater persistence in the response under the Calvo-style wage adjustment. Thus, if only nominal raises follow a Taylor-style adjustment schedule, then the dynamics and persistence of responses to contractionary versus expansionary monetary policy shocks may also differ.

3.3 Annual Raise Schedules are Synchronized Within the Firm

Critically for the quasi-experiment, workers' annual nominal raise schedules are synchronized within firms. A worker is twice as likely to receive a nominal wage raise in the firm's "typical raise quarter" - the calendar quarter in which coworkers have historically tended to receive nominal wage raises at the firm.[14] I identify typical raise quarters for the firms of 79.6% of workers. This is likely to be an underestimate of the prevalence of within-firm synchronization of annual raises because the procedure for identifying typical raise quarters is underpowered for firms with relatively few observed nominal wage changes. These typical raise quarters exhibit some seasonality: the plurality of workers have Q3 as their typical raise quarter (26%), the fewest have Q2 (10%), approximately the same share have Q1 and Q4 (16% and 15% respectively), and 12.6% of workers have two typical raise quarters.

That nominal wage raises are more likely to occur in the second half of the year aligns with the hypothesis of Olivei and Tenreyro (2007), which used this fact to show that the effectiveness of monetary policy differs over the calendar year. Olivei and Tenreyro found when monetary policy shocks occur in the first half of the year, wages are slower to adjust and output responds more strongly to the shock.[15]

To determine whether workers' annual schedules of wage raises are coordinated within firms, I test whether the probability that a worker receives a nominal raise can be predicted using the historical typical raise quarter of coworkers. I focus only on a typical raise quarter measure based on data from previous years so as to avoid the concern that firm-wide shocks generate contemporaneous correlation in coworkers' raise frequencies. Thus, I estimate the following relationship using OLS:

$$\mathbb{1}\left[\Delta^{w}_{ikt} > 0\right] = \alpha d^{RQ}_{ikt} + \boldsymbol{X_{ikt}}\beta + \epsilon_{ikt} \tag{1}$$

where $\mathbb{1}\left[\Delta^{w}_{ikt} > 0\right]$ is an indicator variable equal to one if worker i at firm k receives a nominal wage raise in quarter t; d^{RQ}_{ikt} is an indicator variable equal to one if the calendar quarter in t corresponds to the firm's typical raise quarter for coworkers in previous years; and $\boldsymbol{X_{ikt}}$ is a set of control variables that includes the worker's age, tenure, quarters since their last wage change, and earnings quintile dummy variables.

The regression results shown in Table 1 indicate that the probability that a worker receives a

[14] I classify a firm as having a particular calendar quarter as its "typical raise quarter" if two criteria are met. First, at least 33% of raises in previous years occurred in the given calendar quarter. Second, given the observed number of raises in this calendar quarter and all calendar quarters, I reject the null hypothesis that raises are randomly distributed with equal probability across the four calendar quarters (I use a one-sided hypothesis test at the 5% significance level for a binomial distribution with $p = 0.25$).

[15] A more recent study by Björklund, Carlsson and Nordström Skans (2019) examines the implications of seasonal nominal wage rigidity in Sweden and finds that monetary policy was more effective in periods when the staggered timing of union contracts meant that a larger share of workers had rigid nominal wages.

nominal raise increases 9.3 percentage points in the firm's typical raise quarter, more than doubling the baseline 7.3% probability that a worker receives a nominal raise in any given quarter. This implies that workers' annual nominal raise schedules are strongly coordinated within the firm.

Table 1: Probability of Nominal Wage Change & Typical Raise Quarter

Dependent Variable:	Raise Probability			Cut Probability	
	(1)	(2)	(3)	(4)	(5)
Typical Raise Quarter	**8.2*****		**9.3*****		**-0.3*****
	(0.9)		(0.9)		(0.03)
Baseline (Calendar Quarter I)		**9.3*****	**7.3*****	**1.2*****	**1.1*****
		(0.6)	(0.8)	(0.07)	(0.10)
Calendar Quarter II		-0.24	-0.09	**-0.18*****	**-0.18*****
		(0.37)	(0.32)	(0.04)	(0.04)
Calendar Quarter III		1.94	1.15	**-0.28*****	**-0.20****
		(1.14)	(0.81)	(0.03)	(0.05)
Calendar Quarter IV		0.55	0.59	0.14	-0.09
		(0.32)	(0.28)	(0.07)	(0.10)
Observations	17.4M	10.6M	10.6M	10.6M	10.6M

Note: Outcome variables are indicator variables equal to one if a worker has a nominal wage raise (1-3) or cut (4-5) in the quarter. Models 1, 3, and 5 include an indicator if the quarter qualifies as the firm's typical raise quarter. Models 2-3 and 4-5 include a set of calendar quarter dummy variables (with the intercept representing the calendar quarter I). All models include as control variables: worker age, tenure, quarters since last wage change, and earnings quintile dummy variables. Robust standard errors clustered at the firm level. ***, **, * indicate statistical significance at the 0.1%, 1.0%, and 5.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

4 Quasi-Experimental Evidence that DNWR Destroys Jobs

To determine whether greater exposure to downward nominal wage rigidity (DNWR) causes firms to increase their rate of job destruction when faced with a negative shock, I require variation in firms' exposure to DNWR that is exogenous with respect to the unobserved factors affecting their employment decisions. For this quasi-experiment, this exogenous variation is generated by the timing of firms' annual schedule of nominal wage raises relative to an unanticipated negative aggregate shock.

In any given quarter, firms with more recent typical raise quarters tend to have real wage bills above their annual average real wage bill. This follows from the finding that, within a firm, employees' annual nominal raises are synchronized to occur in the same calendar quarter year-over-year. This synchronization of annual raises generates a stair-step pattern in the firm's quarterly nominal wage bill. Given positive inflation, the stair-step pattern of nominal wages implies that, over any given four-quarter period, the firm's real wage bill spikes in the typical raise quarter, declines over the next three quarters (as inflation eats away at the fixed nominal wage), and reaches a nadir immediately before the firm's next typical raise quarter.

When a large, unanticipated negative aggregate shock occurs, firms with upcoming typical raise quarters can choose to freeze their workers' nominal wages, resulting in quarterly real wage bills that remain below their recent annual average real wage bills. On the other hand, firms that just experienced their typical raise quarter would have to cut workers' nominal wages to achieve a similar decrease in their quarterly real wage bill. Thus, when an unanticipated negative aggregate shock occurs, firms will have differential exposure to downward nominal wage rigidity simply because of differences in their typical raise quarters. If exposure to DNWR has a causal effect on job destruction then we should expect larger increases in job destruction at firms that had their typical raise quarter immediately prior to the negative shock.

The identification strategy of the quasi-experiment relies on two assumptions. First, it requires an unanticipated negative shock. If firms anticipate the negative shock, then those firms with typical raise quarters immediately before and after the negative shock will similarly freeze their workers' nominal wages. As a result, when the negative shock is anticipated, firms' exposure to DNWR is unrelated to the timing of the shock relative to firms' typical raise quarters.

Second, the identification requires that, absent these differences in typical raise quarters, the job destruction rates at firms would have been similar (parallel trends assumption). Most critically, this requires that the magnitude of the negative shock is independent of the firms' typical raise quarters. If the negative shock is stronger for firms with a particular typical raise quarter, then this differential magnitude of the shock confounds the effect of exposure to DNWR by typical raise quarter. Any such confounding invalidates the characterization of treatment versus control groups based on their typical raise quarters.

4.1 Identification from the Onset of the Great Recession

To answer whether exposure to DNWR causes firms to destroy jobs at higher rates, I use the identification strategy described above, focusing on job destruction in 2008:Q4, immediately following Lehman Brothers' bankruptcy in September 2008 and the ensuing financial collapse. Perhaps the best evidence that the financial collapse in September 2008 was unanticipated comes from the Greenbook economic forecasts prepared by the Federal Reserve Board of Governors in support of the FOMC meetings at which U.S. monetary policy is set. On September 10th, just five days before Lehman Brothers declared bankruptcy, the Greenbook forecasted 1.1% annualized GDP growth in Q4 of 2008. Six weeks later, the Greenbook forecast for GDP growth in 2008:Q4 had fallen over two percentage points to -1.3% (between September 10th and October 22nd there was a similar two percentage point decline in the forecast for 2009 GDP growth). This substantial downward revision in the Greenbook economic forecast was mirrored among professional economic forecasters surveyed by the Federal Reserve Bank of Philadelphia. Between August 8th and November 10th of 2008, professional economic forecasters' predictions for the annualized real GDP growth rate in 2008:Q4 fell by 3.8 percentage points, from +0.7% to -2.9%. The professional forecasters' newfound pessimism also extended into their longer-term forecasts, as their predictions for real GDP growth in 2009 fell from +1.5% to -0.2%.

Although it is impossible to test the assumption that firms' job destruction rates at the onset of the Great Recession would have been similar absent their differences in typical raise quarter, I improve the plausibility of this assumption by including both firm-specific seasonal dummy variables and industry-by-time fixed effects as explanatory variables for firm-level job destruction. Table 2 reports the share of start-of-quarter employment in 2008:Q4 for Q2 and Q4-raising firms, broken down by two-digit NAICS industry, firm age, and firm size in 2008:Q4. Along all three dimensions, there are significant differences in the share of Q2 versus Q4-raisers by industry, firm size and firm age. These differences, particularly by industry, are to be expected since industry-specific seasonal demand patterns may affect the optimal timing of firms' annual nominal raise schedules. Given these differences in industry composition, I control for both firm-specific seasonality (using firm-specific calendar-quarter fixed effects) and industry-by-time fixed effects (which help absorb industry-specific shocks in 2008:Q4).

Since the parallel trends assumption is untestable, I follow the common practice of examining both the trend in the differences between Q2 and Q4-raising firms prior to 2008:Q4 and the relative magnitude of these differences across periods. I use the same difference-in-differences specification that I use for the quasi-experiment to estimate the difference in job destruction rates between Q2-raising firms versus Q4-raising firms in every other sample period. Figure 4 plots these coefficient

Table 2: Firm Characteristics by Typical Raise Quarter

	Employment Share at Start of 2008:Q4			
		Typical Raise Quarter		
	QWI	Any Quarter	Q2 Only	Q4 Only
2-Digit NAICS Industry or Cluster				
Ag & Mining & Utilities	1.9%	1.6%	1.3%	2.1%
Construction	6.5%	10.2%	18.9%	4.0%
Manufacturing	12.3%	15.2%	11.6%	18.7%
Wholesale & Retail Trade	19.0%	13.9%	16.9%	12.3%
Transportation	3.9%	3.3%	3.4%	4.6%
Information	2.4%	2.5%	0.7%	2.7%
FIRE	6.8%	7.5%	2.8%	9.0%
Professional Services	6.9%	10.8%	6.1%	14.5%
Management	1.9%	2.0%	0.7%	2.4%
Waste Management	7.0%	6.3%	9.5%	3.4%
Education	2.0%	4.0%	0.5%	5.1%
Health Care	13.5%	7.4%	4.0%	10.1%
Arts & Entertainment	1.7%	3.5%	5.8%	2.4%
Accommodation	10.5%	9.3%	15.0%	6.3%
Other Services	3.7%	2.5%	2.8%	2.4%
Firm Age				
0-1	4.0%	2.1%	2.6%	1.6%
2-3	4.6%	4.2%	5.3%	3.2%
4-5	4.1%	4.5%	6.0%	3.4%
6-10	9.5%	10.5%	13.8%	8.8%
11+	77.8%	78.7%	72.3%	83.0%
Firm Size				
0-19	19.8%	24.6%	26.8%	24.7%
20-49	10.1%	18.1%	24.4%	16.1%
50-249	15.8%	25.8%	30.8%	24.5%
250-499	5.7%	8.34%	7.0%	9.7%
500+	48.6%	23.2%	11.0%	24.9%

Note: The "QWI" column reports the share of start-of-quarter employment by industry from the U.S. Census Bureau's Quarterly Workforce Indicators data product for the same 30 states included in 10% random sample from LEHD. The "Any Quarter" column reports the share of employment in the given industries at firms for which I identify typical raise quarters. The "Q2 Only" and "Q4 Only" columns report the share of employment in the given industries with typical raise quarters in Q2 or Q4. U.S. Census Bureau Disclosure Review Board bypass number CBDRB-2018-CDAR-061.

estimates. The two key takeaways from this figure are that: i) there are no economically or statistically significant pre-trends in the differences between Q2 and Q4 raising firms, and ii) the magnitude of the difference between Q2 and Q4-raising firms in 2008:Q4 is an extreme outlier relative to the typical magnitude of differences between these two firm types. (The magnitude of the 2008:Q4 coefficient estimate is 30% larger in absolute value than the next largest coefficient estimate from any other period and 3.6 times larger than the standard deviation of the coefficient estimates from all periods.)

Figure 4: Period-Specific Q2 vs. Q4-Raiser Differential Job Destruction Coefficient Estimates

Notes: Coefficient estimates for the differential job destruction rate at firms with typical raise quarters in Q2 versus Q4 after controlling for firm-specific seasonality, as well as fixed effects for 2-digit NAICS sector by time, firm age, and firm size. The black dot indicates the coefficient estimate for 2008:Q4. The dashed grey lines represent ± 2-standard deviations from the zero mean difference. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

4.2 DiD: 2008:Q4 Job Destruction by Typical Raise Quarter

To implement this identification strategy, I begin with a difference-in-differences estimation of the job destruction rate in 2008:Q4. Firms that typically raise wages in Q4 comprise the control group. Firms that typically raise wages in Q1, Q2, and Q3 comprise the three distinct treatment groups. The Q1 and Q2-raising firms should have greater exposure to DNWR since, not anticipating the financial collapse, they are more likely to have raised their workers' nominal wages earlier in the year. Of these treatment groups, the Q2-raising firms should have the greatest exposure, since inflation would have had more time to eat away at the nominal wage bill of Q1-raising firms. The relative degree of exposure to DNWR of the treatment group composed of Q3-raising firms is more ambiguous, since some Q3-raising firms may have observed the start of the financial collapse (which began in late 2008:Q3) before deciding whether to raise their workers' nominal wages - thus giving them a degree of nominal wage flexibility similar to that of the Q4-raising control group.

The difference-in-difference specification is:

$$JD_{kt} = D_{kt}^{2008:Q4}\theta^{Q4} + \sum_{q=1}^{3} D_{kt}^{2008:Q4} D_{kt}^{\text{Qq Raiser}}\theta^{Qq} + \boldsymbol{X}_{kt}\beta_t + \epsilon_{kt} \tag{2}$$

where JD_{kt} is firm k's DHS job destruction rate in period t,[16] $D_{kt}^{2008:Q4}$ is an indicator variable equal to one if t =2008:Q4, $D_{kt}^{\text{Qq Raiser}}$ is a set of indicator variables equal to one if firm k's typical raise quarter equals calendar quarter q, and $\boldsymbol{X}_{kt}$ is a set of control variables that includes firm-specific calendar quarter fixed effects, as well as fixed effects for industry by time, firm age, and firm size. The coefficients of interest are θ^{Q1} and θ^{Q2}. These coefficient estimates indicate whether firms with greater exposure to DNWR destroyed jobs at a higher rate when confronted with a negative aggregate shock.

I estimate this difference-in-differences specification on all firms in the primary sample for which I identify the firm as having a typical raise quarter. Table 3 shows the results of this difference-in-differences estimation without industry-by-time fixed effects (Model 1), with industry-by-time fixed effects (Model 2), and with industry-by-time fixed effects and employment weighting (Model 3). First, as expected given the severity of the financial crisis, Q4-raising firms increased their rates of job destruction by 1.4 percentage points in 2008:Q4. Second, across all specifications, I find strong evidence that exposure to DNWR significantly increased firms' job destruction. Specifically, Q2-raising firms, which had the greatest exposure to DNWR because of their nominal raise schedule, increased their job destruction rates by an additional 1.3 percentage points (or 2.0 percentage points when weighting by employment). This implies that the firms most constrained by DNWR because of their typical raise quarter increased their job destruction rates by nearly twice as much as the least-constrained Q4-raising firms. The results for Q1 and Q3-raising firms are more ambiguous, with statistical significance depending on the model's control variables and employment-weighting.

[16] I compute the DHS rate of change measure proposed by Davis, Haltiwanger, Schuh et al. (1998) for a firm's quarterly job destruction rate, as defined by the Census Bureau's Quarterly Workforce Indicators. Specifically:

$$JD_{kt} = \max\left[0, \frac{\text{start of quarter employment - end of quarter employment}}{0.5 \text{ start of quarter employment} + 0.5 \text{ end of quarter employment}}\right]$$

The DHS rate of change has the advantages of both being symmetric (unlike percent changes) and capable of handling zero values (unlike logs). It is also bounded between -2.0 and 2.0.

Table 3: Difference-in-Differences Job Destruction by Typical Raise Quarter

Dependent Variable	Job Destruction Rate		
	(1)	(2)	(3)
2008:IV * Q4-Raiser	**1.48*****		
	(0.16)		
	Relative to Q4-Raiser		
2008:IV * Q1-Raiser	0.34	0.24	**1.02*****
	(0.28)	(0.25)	(0.18)
2008:IV * Q2-Raiser	**2.11*****	**1.30*****	**2.01*****
	(0.21)	(0.26)	(0.20)
2008:IV * Q3-Raiser	**0.70****	0.26	-0.22
	(0.26)	(0.24)	(0.14)
Industry*Time FE	No	Yes	Yes
Employment Weighted	No	No	Yes
Mean JD Rate	8.6%	7.7%	4.9%
Observations	5.7M	5.6M	5.6M
R-Squared	0.28	0.29	0.38

Note: The outcome variable is the SEIN-level DHS job destruction rate. This is regressed on a set of control variables, a 2008:Q4 dummy variable, and this dummy variable interacted with a set of dummy variables indicating firms with typical raise quarters in Q1, Q2, or Q3. The control variables include firm-specific seasonal fixed effects as well as dummy variables for firm age and firm size. Models (2) and (3) also include fixed effects for two-digit industry by time. Quarterly LEHD data from 1999:Q1 to 2014:Q4. Sample includes only firms with one (and no more) typical raise quarter. Robust standard errors clustered at the SEIN-level. ***, **, * indicate statistical significance at the 0.1%, 1.0%, and 5.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

4.2.1 A Further Test of the Parallel Trends Assumption

A key concern for the parallel trends assumption is the possibility that seasonal factors in firms' business conditions could be correlated with both the firm's historical typical raise quarter and the firm's exposure to the financial collapse. This concern is reinforced by the fact that nominal wage raises in certain industries appear to be clustered in particular calendar quarters (e.g. firms in professional services and FIRE tend to raise workers' wages in Q4, whereas construction and accommodation firms cluster their typical raise quarters in Q2; see Table 2 for the full breakdown). Even though the difference-in-differences specification controls for both industry-specific shocks in 2008:Q4 and firm-specific seasonality, it is still possible that unobserved factors that determine a firm's historical raise patterns also affect the magnitude of the shock that the firm experienced in 2008:Q4.

To address this concern, I extend the difference-in-difference estimation model to determine whether the job destruction rate at Q2-raising firms responded more strongly than at Q4-raising firms to a given-sized change in revenue. If exposure to DNWR is the root cause of Q2-raising firms choosing to destroy more jobs, then I should expect Q2-raising firms to respond more strongly than Q4-raising firms to similar magnitude negative revenue changes in 2008:Q4. The regression model I estimate is:

$$JD_{kt} = \boldsymbol{D_{kt}^{2008:Q4}} \left(\theta^{Q4} + \boldsymbol{R_{kt}}\gamma^{Q4} + \boldsymbol{D_{kt}^{\text{Q2 raiser}}} \left(\theta^{Q2} + \boldsymbol{R_{kt}}\gamma^{Q2}\right)\right) + \boldsymbol{X}_{kt}\beta_t + \epsilon_{kt} \tag{3}$$

where the outcome variable, JD_{kt}, is the firm's DHS job destruction rate; $\boldsymbol{D_{kt}^{2008:Q4}}$ is an nxn diagonal matrix where n is the number of observations and each diagonal element equals one if t=2008:Q4; $\boldsymbol{R_{kt}}$ is an nx2 matrix where the first column corresponds to the firm's year-over-year DHS real revenue change if it is positive (and zero otherwise), and the second column is the absolute value of the DHS revenue change if it is negative (and zero otherwise); $\boldsymbol{D_{kt}^{\text{Q2 raiser}}}$ is an nxn diagonal matrix with each diagonal element equal to one if firm k's typical raise quarter is the second calendar quarter; and $\boldsymbol{X_{kt}}$ is a set of control variables that includes $\boldsymbol{R_{kt}}$, $\boldsymbol{R_{kt}D_{kt}^{\text{Q2 raiser}}}$, firm-specific seasonal fixed effects, industry-by-time fixed effects, and dummy variables for firm age and firm size.

The coefficient of interest is the γ^{Q2} coefficient for negative revenue change, since this indicates whether Q2-raising firms had stronger responses than Q4-raising firms to a given-sized negative revenue shock. My coefficient estimate from this difference-in-differences framework is likely to be biased due to a combination of: i) measurement error resulting from the annual measure of revenue change spanning a longer time range than the quarterly measure of job destruction, and ii) simultaneity in the relationship between changes in employment levels and changes in revenue (since, even absent any demand or productivity shocks, it is standard to assume that a firm's labor inputs contemporaneously affect its revenue). In Appendix A.1 I argue that despite these sources of bias, the coefficient estimate is still informative as to whether Q2-raising firms responded more strongly than Q4-raising firms to negative revenue changes in 2008:Q4. First, I discuss how the similarity of Q4-raising firms' response to negative revenue changes in 2008:Q4 relative to other periods indicates that the bias from measurement error was similar across periods (and thus is compensated for by the difference-in-differences estimation strategy). Second, I show that the difference-in-difference framework implies that the γ^{Q2} coefficient estimate is only biased if the Q2-raising firms had a differential response to negative revenue shocks in 2008:Q4. Furthermore, I demonstrate that, given standard assumptions about the relationship between revenue and employment, the simultaneity bias attenuates the coefficient estimate, resulting in an under-estimate of the true differential response of Q2-raising firms to negative revenue shocks.

There are two significant changes in the sample used for this estimation. First, annual revenue data is only available in the U.S. Census Bureau's Revenue-Enhanced Longitudinal Business Database (rLBD) from 2002. Second, a large fraction of SEINs are not linked to EINs with revenue data in the rLBD. The mean job destruction rates of the original and more restricted samples are similar (7.7% and 7.0%, respectively).

Model (2) in Table 4 reports the results of this difference-in-differences estimation with the additional revenue change variables and industry-by-time fixed effects (I include column (2) of Table 3 as Model (1) for comparison purposes, since it uses the same specification with industry-by-time fixed effects and no employment weighting, but without the revenue change variables).

There are three important takeaways from this table. First, in periods other than 2008:Q4, the job destruction rate at both Q2 and Q4-raising firms responded similarly and strongly to negative year-over-year revenue changes. For every 1% fall in annual revenue, job destruction rose approximately 0.165 percentage points at Q4-raising firms and 0.158 percentage points at Q2-raising firms.

Second, in 2008:Q4, there was no statistically significant change in the response of job destruction to either positive or negative year-over-year revenue changes at Q4-raising firms. Similarly, in 2008:Q4, the response of the job destruction rate at Q2-raising firms did not change for firms experiencing positive year-over-year revenue changes (which is what I would expect if DNWR is what drives the differential response of Q2-raising firms).

Last, and most importantly, the responsiveness of the job destruction rate at Q2-raising firms to negative year-over-year revenue changes increased by 48%, rising from a 0.158 percentage point increase in the job destruction rate for every 1% decline in revenue to a 0.235 percentage point increase in 2008:Q4. These results are consistent with the hypothesis that differences in the calendar quarter in which firms historically tended to raise their workers' wages caused firms' job destruction rates to respond differently to similar magnitude shocks in 2008:Q4.

Table 4: Differential Q2 vs Q4-Raiser Job Destruction Rate & Annual Revenue Change

	Job Destruction Rate	
	(1)	(2)
Q4-Raiser * Δ^{+}Revenue		**2.47*****
		(0.13)
Q4-Raiser * Δ^{-}Revenue		**16.48*****
		(0.12)
2008:IV * Q4-Raiser * Δ^{+}Revenue		-0.52
		(0.82)
2008:IV * Q4-Raiser * Δ^{-}Revenue		1.02
		(0.76)
	Relative to Q4-Raiser	
2008:IV * Q2-Raiser	**1.30*****	0.06
	(0.26)	(0.36)
Q2-Raiser * Δ^{+}Revenue		**-0.66*****
		(0.17)
Q2-Raiser * Δ^{-}Revenue		**-0.68*****
		(0.16)
2008:IV * Q2-Raiser * Δ^{+}Revenue		1.15
		(1.89)
2008:IV * Q2-Raiser * Δ^{-}Revenue		**7.68*****
		(1.70)
Industry*Time FE	Yes	Yes
Employment Weighted	No	No
Mean JD Rate	7.7%	7.0%
Observations	5.6M	1.6M
R-Squared	.29	.36

Note: The outcome variable is the SEIN-level DHS job destruction rate. Controls include fixed effects for firm-specific seasonality, firm age, firm size, and industry-by-time. Specification (4) also includes measures of positive (Δ^{+}Revenue) and negative (Δ^{-}Revenue) annual revenue changes, interacted with the firm's typical raise quarter. Quarterly LEHD data from 2002:Q1 to 2014:Q4. Robust standard errors clustered at the SEIN-level. ***, **, * indicate statistical significance at the 0.1%, 1.0%, and 5.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

4.3 Exploring the Mechanism: Raise Schedules and the Real Wage Bill

According to the reasoning laid out in Section 4.1, if downward nominal wage rigidity causally affects a firm's rate of job destruction by constraining the firm's ability to cut workers' nominal wages, then the firm will have a higher job destruction rate in response to an unanticipated negative shock if the firm's real wage bill is exogenously above its optimal level. Although it is impossible to observe a firm's optimal real wage bill, the firm's average four-quarter real wage bill is a reasonable benchmark, since the annual staggering of nominal wage raises implies that firms should set the nominal wage such that the expected average real wage over the year equals its optimal level. Thus, for each firm-quarter observation, I measure the ratio of the firm's real wage bill in the previous period relative to its four-quarter moving average (W_{kt}) as:

$$W_{kt} = \frac{\sum_{i \in E_{kt}^{FY}} w_{ikt-1}^{r}}{\sum_{i \in E_{kt}^{FY}} \sum_{s=1}^{4} w_{ikt-s}^{r}/4} \tag{4}$$

where E_{kt}^{FY} is the set of full-year workers (workers who have been full-quarter employees at the firm for each of the last four quarters)[17] and w_{ikt-s}^{r} is the real wage of the worker in period $t-s$ (in 2015:Q1 dollars, computed using the Employment Cost Index (ECI) from the Bureau of Labor Statistics).

The reduced form relationship of interest explores whether a firm's job destruction rate in 2008:Q4 is higher when its start-of-quarter real wage bill ratio is higher. Specifically,

$$JD_{kt} = \gamma W_{kt} + \gamma^{2008:Q4} d_{kt}^{2008:Q4} W_{kt} + \boldsymbol{X_{kt}} \beta_t + \epsilon_{kt} \tag{5}$$

where JD_{kt} is the firm's DHS job destruction rate, $d_{kt}^{2008:Q4}$ is an indicator variable equal to one in 2008:Q4, and $\boldsymbol{X_{kt}}$ is a set of control variables that includes firm fixed-effects, as well as sets of dummy variables for firm age, firm size, and two-digit industry fixed effects for 2008:Q4.

4.3.1 IV: Effect of Higher Real Wages from DNWR on Job Destruction

A simple OLS regression of the model in Equation 5 is unlikely to yield a causal estimate of the effect of having a higher real wage bill in 2008:Q4 due to a combination of measurement error in real wages and persistent unobserved confounders affecting both past wages and current-period job destruction (see Appendix A.2 for a detailed discussion of these endogeneity issues and the expected direction of the bias). To estimate the causal effect of DNWR on a firm's rate of job destruction, I employ an instrumental variables strategy using the firm's historical pattern of nominal wage raises. As instrumental variables, I construct the employment-weighted share of a firm's nominal wage raises that occurred in each of the last three calendar quarters in previous years (prior to 2007:Q4). For instance, in 2008:Q4, I construct three instrumental variables for each firm: r_{kt-1}^{Q}, r_{kt-2}^{Q}, and r_{kt-3}^{Q}, which correspond to the historical employment-weighted share of nominal raises

[17] Restricting the real wage ratio to include only workers who have been employed for the entire previous year ensures a consistent measure of relative wages but biases the measure to represent the real wage ratio of longer-tenure workers.

occurring in calendar quarters Q3, Q2, and Q1 respectively (as a share of all raises).

To construct these raise share variables, I calculate the probability that a full-quarter employee receives a nominal raise at the firm for each calendar quarter (Q1 to Q4) in the period before 2007:Q4 (p_k^q where q indicates the calendar quarter). Then I calculate the historical employment-weighted share of nominal raises for each calendar quarter q as:

$$s_k^q = \frac{p_k^q}{\sum_{a=1}^4 p_k^a} \tag{6}$$

These historical raise share measures for the four calendar quarters sum to one for every firm.[18] Finally, I convert these employment-weighted historical raise shares into three firm-quarter specific measures of the historical raise shares in quarters $t-1$, $t-2$, and $t-3$. For instance, if t falls in the fourth calendar quarter, then I define the instrumental variables: $r_{kt-1}^Q = s_k^3$, $r_{kt-2}^Q = s_k^2$, $r_{kt-3}^Q = s_k^1$.

Using these instrumental variables, I estimate the following first-stage relationships for the endogenous explanatory variables W_{kt} and $d_{kt}^{2008:Q4} W_{kt}$:

$$W_{kt} = \sum_{a=1}^{3} \alpha_a r_{kt-a}^Q + \sum_{a=1}^{3} \alpha_a^{2008:Q4} d_{kt}^{2008:Q4} r_{kt-a}^Q + \boldsymbol{X_{kt}} \beta_t + \nu_{kt} \tag{7}$$

and similarly for $d_{kt}^{2008:Q4} W_{kt}$. Table 5 reports the results of these first-stage regressions for the set of firms with at least ten observed nominal raises prior to 2007:Q4. These results are consistent with the theory that a firm's real wage bill spikes in the quarter in which it historically tends to raise workers' nominal wages and steadily declines over the next three quarters.[19] There does not appear to be a weak instruments problem for either of the endogenous explanatory variables.

Table 6 shows the results of estimating the reduced form relationship of interest from Equation 5 with both OLS and 2SLS. The 2SLS estimation finds that firms' rate of job destruction rose 3.6

[18] I use the historical share of raises instead of the historical probability of a raise in a given calendar quarter. This is because firms that were historically doing well would also have been more likely to raise workers' nominal wages. Given that business conditions persist over time, the historical probability of a raise in a given calendar quarter is likely to be correlated with the current business conditions and thus would not address the omitted variable problem.

[19] To give a sense of the magnitude of variation in firms' real wage bill over the calendar year, it is easiest to consider the special cases where 100% of a firm's historical raises occurred in the calendar quarter in $t-1$, $t-2$, or $t-3$. In such cases, the firm's real wage bill at the start of period t is higher by 4.1%, 3.2%, and 1.6% in the first, second, and third quarters after the firm's historical raise quarter. In 2008:Q4, however, the real wage bill increase was more muted, with the firm's real wage bill only being 2.1%, 1.7%, and 0.7% higher in the first, second, and third quarters after the firm's historical raise quarter. This more muted relationship in 2008:Q4 is consistent with the fact that the recession began in January 2008.

Table 5: First-Stage: Start-of-Quarter / 4-Quarter Lag Moving Average Real Wage Bill

Dependent Variable: W_{kt} = Start-of-Quarter / 4-Quarter Lag Moving Average Real Wage Bill				
	W_{kt}	$d_{kt}^{2008:Q4} W_{kt}$	W_{kt}	$d_{kt}^{2008:Q4} W_{kt}$
Model:	(2a)	(2b)	(4a)	(4b)
Historical Raise Share (r^Q_{kt-x})				
1 Quarter Lag	**4.06*****	2x10^{-4}	**3.91*****	**2.2x10**$^{-3**}$
	(0.03)	(1x10^{-4})	(0.07)	(7x10^{-4})
2 Quarter Lag	**3.19*****	-3x10^{-4}	**3.09*****	2.1x10^{-3}
	(0.03)	(2x10^{-4})	(0.09)	(1.4x10^{-3})
3 Quarter Lag	**1.56*****	2x10^{-4}	**1.60*****	4x10^{-4}
	(0.02)	(2x10^{-4})	(0.07)	(1.2x10^{-3})
2008:Q4 * 1 Quarter Lag	**-2.14*****	**2.14*****	-0.81	**3.08*****
	(0.17)	(0.18)	(0.35)	(0.39)
2008:Q4 * 2 Quarter Lag	**-1.58*****	**1.66*****	**-1.05*****	**1.94*****
	(0.17)	(0.17)	(0.31)	(0.30)
2008:Q4 * 3 Quarter Lag	**-0.86*****	**0.68*****	-0.43	**1.02*****
	(0.17)	(0.17)	(0.33)	(0.31)
Employment Weighted	N	N	Y	Y
Firm FE	Y	Y	Y	Y
F-Test (clustered SE)	4444	33.45	596	15.2
Kleibergen-Paap rk LM	33.4		14.2	
Andersen-Rubin Wald Test	342		18.6	
Observations	7.07 million			
Clusters	161,000 firm clusters			

Note: The outcome variable is the SEIN-level real wage ratio. Control variables include firm fixed-effects, as well as fixed effects for two-digit NAICS industry in 2008:Q4, firm age, and firm size. Quarterly LEHD data from 1999:Q1 to 2014:Q4. Sample includes only firms with at least ten raises observed prior to 2007:Q4. Robust standard errors clustered at the SEIN-level. ***, **, * indicate statistical significance at the 0.1%, 1.0%, and 5.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0073-CED-20190910.

percentage points in 2008:Q4 for every 1% that firms' start-of-quarter real wage bill was above their four-quarter average real wage bill due to the firms' historical raise schedules.[20]

To give a sense of the aggregate magnitude of this estimate, I consider the counterfactual scenario in which all firms had the nominal wage flexibility of Q4-raising firms in 2008:Q4. This is equivalent to using the coefficient estimates from the second-stage of the IV regression to calculate the job destruction rate at firms if all historical raise share values are set to zero (and thus the firm is a Q4 raiser). When I do this simple adjustment to eliminate the exposure to DNWR generated by

[20] This is most likely an underestimate of the effect of DNWR on job destruction since it includes as an instrumental variable the share of raises that historically occurred in Q3. Since some of these Q3-raising firms would have observed the Lehman Brothers' bankruptcy that occurred on September 15th, 2008, some Q3-raising firms may have endogenously frozen more of their workers' wages and thus been even less exposed to DNWR than the Q4-raising firms. If, instead, I use only Q1 and Q2 raise shares as instrumental variables and include the Q3 raise share as a control variable in 2008:Q4, then a 1% increase in the real wage bill ratio is estimated to increase the firm's job destruction rate by 7.8 percentage points. See Appendix A.3 for the alternative second-stage regression results.

Table 6: Second-Stage: Job Destruction Rate

Dependent Variable:	Firm DHS Job Destruction Rate			
Estimator:	OLS	IV	OLS	IV
Model:	(1)	(2)	(3)	(4)
Real Wage Bill Ratio				
W_{kt}	**-0.19*****	**1.26*****	**-0.13*****	**-0.67****
	(0.004)	(0.04)	(0.02)	(0.22)
2008:Q4 * W_{kt}	**-0.15*****	**3.56*****	0.04	**3.20*****
	(0.03)	(0.55)	(0.07)	(0.83)
Employment Weighted	N	N	Y	Y
Firm FE	Y	Y	Y	Y
R-Squared	0.050		0.054	
Observations	7.07 million			
Clusters	161,000 firm clusters			

Note: The outcome variable is the SEIN-level DHS job destruction rate. This is regressed on the predicted real wage bill ratio and the predicted real wage bill ratio in 2008:Q4, as well as a set of control variables. The control variables include firm fixed effects as well as dummy variables for firm age, firm size, and two-digit industry-specific shocks in 2008:Q4. Quarterly LEHD data from 1999:Q1 to 2014:Q4. Sample only includes firms with at least ten nominal raises prior to 2007:Q4. Robust standard errors clustered at the SEIN-level. ***, **, * indicate statistical significance at the 0.1%, 1.0%, and 5.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0073-CED-20190910.

the annual staggering of firm's nominal raise schedules, I find that the job destruction rate would have risen 23% less in 2008:Q4 (which was the quarter with the most job destruction since job destruction became available in the Quarterly Workforce Indicators in 1993). I should note that this estimate serves as a lower bound of the effect of DNWR on job destruction in 2008:Q4, since even the less-exposed Q4-raising firms (which serve as the baseline comparison group) had exposure to DNWR.[21]

An important caveat regarding the external validity of my causal estimate of the effect of DNWR on job destruction is that the quasi-experiment examines the largest unanticipated negative aggregate shock experienced by the United States in at least the last 35 years. The aggregate implications of the estimated effect of DNWR on job destruction may be very different in other recessions for which the negative shocks are likely to be smaller in magnitude and short-term financing may be more readily available. Similarly, the aggregate dynamics resulting from DNWR may be very dif-

[21] Two additional concerns are: one, this is a partial equilibrium estimate of the aggregate effect of DNWR since my firm-level IV specification does not take into account any general equilibrium effects. And two, this aggregate estimate does not fully capture the effect of DNWR on job destruction since my instruments only capture one-dimension of firms' exposure to DNWR.

ferent in more stable periods, when only a small set of firms may be without the financial resources necessary to smooth large unanticipated negative shocks.

4.4 Job Destruction: More Layoffs, Not Less Hiring

There are two potential channels by which exposure to DNWR could generate job destruction. Namely, the decline in employment between the start and end of the quarter could result from some combination of more layoffs and less hiring of replacements for retiring and quitting workers. I now evaluate which of these channels drove the large increase in job destruction at firms with greater exposure to DNWR in 2008:Q4. I do this by re-estimating the difference-in-differences specification described in Section 4.2, changing the outcome variable to be either the firms' hiring rate or layoff rate.[22] The results in Table 7 show that the layoff rate at Q2-raising firms rose by 1.44 percentage points more than at Q4-raising firms. This estimate is statistically significant and similar in magnitude to the 1.30 percentage point rise in the job destruction rate at Q2-raising firms relative to Q4-raising firms. The estimate for the change in the hiring rate at Q2-raising firms, however, is not statistically different from that of Q4-raising firms; while it is large in magnitude, it is of the wrong sign to explain a rise in job destruction. Thus, the greater increase in job destruction rates at Q2-raising firms relative to Q4-raising firms in 2008:Q4 comes from more workers being laid off at Q2-raising firms, and not from a decline in replacement hiring.

4.5 Variation in Worker Layoff Risk From DNWR Exposure

Given the substantial effect that DNWR has on firms' rates of job destruction, it is also informative to explore whether particular worker characteristics expose employees to higher layoff risk when their employer is constrained by DNWR. I use a similar difference-in-differences framework as described in Section 4.2 but now at the level of worker-firm pairs. The outcome of interest is whether a worker is laid off, defined as any instance when the worker either: i) transitions from employment in the current quarter to non-employment in the next quarter, or ii) switches from one employer in this quarter to a new employer either in this quarter or the next, but where the earnings gap between the two jobs exceeds one month of earnings. (This measure of job transitions is derived from Haltiwanger, Hyatt, Kahn and McEntarfer (2018)). I augment the difference-in-difference estimation with a large set of worker characteristics that include the worker's sex, race,

[22]Since the LEHD data set does not contain the reason for separation, I label a job separation as a "layoff" in any case where either: i) the worker experienced an employment-to-nonemployment transition (so has at least one full quarter of non-employment post separation), or ii) experienced a same-quarter or adjacent-quarter employer-to-employer transition where the earnings gap between jobs was at least one month (see Haltiwanger, Hyatt, Kahn and McEntarfer (2018)).

Table 7: Differential Q2 vs Q4-Raiser Employment Outcomes in 2008:Q4

Dependent Variable:	Job Destruction	Layoffs	Job Creation	Hiring
	(1)	(2)	(3)	(4)
	Relative to Q4-Raiser			
2008:Q4 * Q2-Raiser	**1.30*****	**1.44***	0.22	2.04
	(0.26)	(0.66)	(0.12)	(2.79)
Industry*Time FE	Yes	Yes	Yes	Yes
Employment Weighted	No	No	No	No
Mean Rate	7.7%	9.6%	5.8%	25.3%
Observations	5.6M	5.6M	5.6M	5.6M
R-Squared	0.29	0.24	0.34	0.18

Note: The outcome variable is the SEIN-level DHS job destruction, layoff, job creation, and hiring rates. These are regressed on a set of control variables, a 2008:Q4 dummy variable, and this dummy variable interacted with a set of dummy variables indicating firms with typical raise quarters in Q1, Q2, or Q3. The control variables include firm-specific calendar quarter fixed effects as well as dummy variables for firm age, firm size, and two-digit industry by time. Quarterly LEHD data from 1999:Q1 to 2014:Q4. Sample only includes firms with one (and no more) typical raise quarter. Robust standard errors clustered at the SEIN-level. Robust standard errors clustered at the SEIN-level. ***, **, * indicate statistical significance at the 0.1%, 1.0%, and 5.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

education, age group, tenure group, and log earnings. These worker characteristics are fully interacted with an indicator variable for 2008:Q4 and a set of dummy variables for the employer's typical raise quarter. Thus, I estimate:

$$L_{ikt} = \boldsymbol{C_{ikt}} \left[\alpha^4 + \boldsymbol{D}_{kt}^{2008:Q4} \theta^4 + \sum_{q=1}^{3} \left(\boldsymbol{D}_{kt}^{\text{Qq-raiser}} \alpha^q + \boldsymbol{D}_{kt}^{2008:Q4} \boldsymbol{D}_{kt}^{\text{Qq-raiser}} \theta^q \right) \right] + \boldsymbol{X_{kt}} \beta_t + \epsilon_{kt} \quad (8)$$

where L_{ikt} is an indicator variable equal to one if worker i was laid off from firm k in period t; $\boldsymbol{C_{ikt}}$ is the matrix of worker characteristics; $\boldsymbol{D}_{kt}^{2008:Q4}$ is a diagonal matrix with values equal to one if t=2008:Q4; $\boldsymbol{D}_{kt}^{\text{Qq-raiser}}$ is a diagonal matrix with values equal to one if firm k typically raises wages in quarter q; and $\boldsymbol{X_{kt}}$ is a set of firm-level control variables that includes dummy variables for firm age, firm size, and industry-by-time fixed effects, as well as firm-by-calendar-quarter fixed effects (for firm-specific seasonality).

Table 8 reports the results of this regression. This table shows that less-educated workers and workers hired prior to the start of the recession (i.e. before 2008:Q1) but within the last three years had higher layoff risk because of DNWR. These results are consistent with firms differentially laying off lower productivity workers when the firms are constrained by DNWR, since these workers have either fewer years of education or less time to accumulate firm-specific human capital. Conditional on observables, higher-paid workers are more likely to be laid off, which is consistent with firms choosing to lay off workers with lower firm surplus.

That older workers (age 61-70) and black and multi-racial workers are disproportionately exposed to layoff risk is not surprising given the greater cyclical volatility of these groups' employment rates. It is surprising, however, to find that younger workers (under age 35) have slightly lower layoff risk relative to middle-aged workers (age 36-60).

Table 8: Differential Layoff Rate in 2008:Q4 by Worker Characteristic

Differential Worker Layoff Rate at Q2-Raising Firms in 2008:Q4 (Relative to Q4-Raising Firms)		
	Coefficient	Standard Error
Log Earnings	**0.76*****	(0.05)
Sex		
Male	Baseline	
Female	**-1.67*****	(0.10)
Education		
Less than High School	**1.76*****	(0.34)
High School	Baseline	
Some College	**-1.03*****	(0.26)
College	**-1.47*****	(0.30)
Race		
White	Baseline	
Black	**1.10*****	(0.17)
American Indian	0.22	(0.47)
Asian	**-0.7*****	(0.22)
Native American	0.6	(0.89)
Two or More Races	**1.6*****	(0.37)
Age		
18 to 20	**-0.98*****	(0.25)
21 to 25	**-0.55****	(0.20)
26 to 30	**-0.75*****	(0.20)
31 to 35	**-0.88*****	(0.20)
36 to 40	**-0.06****	(0.20)
41 to 45	Baseline	
46 to 50	0.21	(0.20)
51 to 55	0.14	(0.21)
56 to 60	-0.02	(0.22)
61 to 65	**0.92****	(0.25)
66 to 70	**2.80*****	(0.33)
Tenure		
2 quarters	**-5.2*****	(0.20)
3 quarters	**0.4***	(0.20)
1 year	**0.65****	(0.22)
2 years	Baseline	
3 years	**-0.65*****	(0.16)
4 years	**-0.96*****	(0.19)
5 years	**-0.92*****	(0.21)
6-10 years	**-1.59*****	(0.16)
11+ years	**-3.07*****	(0.19)
R-Squared	0.068	
Observations	49.7M	

Outcome Variable: Worker level layoff rate (mean quarterly layoff rate among start-of-quarter workers is 5.5%), where layoff is defined as either an employment-to-nonemployment transition or an employment-to-employment transition with an earnings gap of at least one month. The set of control variables includes dummy variables for firm age, firm size, and industry-by-time fixed effects, as well as firm-by-calendar-quarter fixed effects. Standard errors clustered at the SEIN-level. Quarterly LEHD data from 1999:Q1 to 2014:Q4. ***, **, * indicate statistical significance at the 0.1%, 1.0%, and 5.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

5 Summary and Concluding Remarks

This paper argues that downward nominal wage rigidity plays an important causal role in explaining employment fluctuations through the job destruction margin. The paper presents quasi-experimental evidence that in 2008:Q4, the job destruction rate increased twice as much at firms with greater exposure to DNWR due to the timing of their historical raise schedules relative to the unanticipated financial collapse in September 2008. I find that the increase in the aggregate job destruction rate in 2008:Q4 would have been 23% smaller if all firms had had the wage flexibility of firms whose annual raise schedules occurred in the fourth calendar quarter. Since this estimate does not account for the fact that the Q4-raising firms may also have had exposure to downward nominal wage rigidity, it is a lower-bound estimate of the effect of downward nominal wage rigidity on job destruction in 2008:Q4.

This paper leaves unanswered several important questions about the relationship between downward nominal wage rigidity and job destruction. First, although I show that greater exposure to downward nominal wage rigidity causally increased firms' rates of job destruction during the Great Recession, the instrument I use (variation in the historical seasonality of firms' wage raises) does not capture a firm's full exposure to DNWR. Thus, while the effect of DNWR on job destruction that I identify is large, this estimate serves as only a lower bound for the true causal effect of DNWR on job destruction. Second, the quasi-experiment in this paper focuses on the large unanticipated negative aggregate shock at the onset of the Great Recession, a period in which firms faced unusual financial exigencies. This brings into question the external validity of the magnitude of the aggregate effect. While firms experiencing large negative shocks in more "normal" recessions are likely to respond in a similar fashion to the firms in my instrumental variables estimation, there will be fewer such firms if the recession is less severe. As a result, I expect downward nominal wage rigidity to generate less aggregate job destruction when the recession is less severe.

There are two important implications of this paper for monetary policy. First, regarding the Federal Reserve's target inflation rate, studies examining the optimal inflation rate have largely ignored the potential for downward nominal wage rigidity to inefficiently destroy jobs.[23] Thus, it would be informative to examine whether and how much the optimal rate of inflation changes once the inefficient job destruction caused by downward nominal wage rigidity is incorporated into a

[23] Kim and Ruge-Murcia (2009); Coibion, Gorodnichenko and Wieland (2012); Mineyama (2018); Dupraz, Nakamura and Steinsson (2019)

model designed to identify the optimal rate of inflation. One complexity in modeling the effect of downward nominal wage rigidity in a high-inflation environment is that the variation in firms' exposure to downward nominal wage rigidity that I use in the quasi-experiment could actually be exacerbated in a high-inflation environment since the real wage change over the calendar year would be greater.

Second, the finding that exposure to downward nominal wage rigidity causes firms to destroy positive-surplus jobs has implications for the asymmetric response of employment and output to contractionary versus expansionary aggregate shocks. Many of the DSGE and labor search-and-matching models that explore these asymmetric responses tend to ignore the possibility that downward nominal wage rigidity affects employment through the job destruction margin. It would be informative to explore how the results change when the models include the effect of downward nominal wage rigidity on the job destruction margin. Importantly for monetary policy, asymmetric responses of employment and output to aggregate shocks due to the effect of downward nominal rigidity on job destruction also have implications for the effectiveness of contractionary versus expansionary monetary policy shocks.

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Table 9: Hourly Earnings Change Comparison

	Source	Period	Raise	Freeze	Cut
Quarter-over-Quarter Nominal Log Hourly Earnings Change					
Raw	LEHD 4 States	2011-2018	55.5%	5.0%	39.5%
Rounding Adj	LEHD 4 States	2011-2018	46.4%	22.2%	31.4%
Bonus & Rounding Adj	LEHD 4 States	2011-2018	36.9%	45.3%	17.8%
4-Quarter Log Nominal Hourly Earnings Change					
	Source	Period	Raise	Freeze	Cut
Rounding Adj	LEHD 4 States	2011-2018	68.1%	11.8%	20.1%
Kurmann and McEntarfer (2019)	LEHD WA State	1990-2014		8-16%	20-25%
Jardim, Solon and Vigdor (2019)	UI WA State	2005-2015		2.5-7.7%	20.4-33.1%
Grigsby, Hurst and Yildirmaz (2019)	ADP 50+ Workers	2008-2016	75.3%	9.0%	15.7%

Note: 'Raw' indicates log change calculated from the original quarterly earnings and hours paid. 'Rounding Adj' sets to zero any nominal change that could be explained by adjusting the hours paid by ± 1 hour to account for potential rounding errors. 'Bonus & Rounding Adj' first smooths bonuses in the hourly earnings and then sets to zero any nominal change that could be accounted for by a ± 1 change in hours paid. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0037-CED-20190327.

A Empirical Analysis: Extensions and Robustness

A.1 DiD: Endogeneity of Revenue Change

Two concerns prohibit a causal interpretation of the estimate Q2-raising firms' differential response to negative revenue changes in 2008:Q4. First, the revenue change measure is the year-over-year revenue change, but the job destruction measure is for only the fourth quarter of the year. Although the specification does control for firm-specific seasonality (so the comparison is within each firm's fourth-quarter observations), it is still the case that a firm's revenue in the first three quarters of the year may affect the observed job destruction rate in 2008:Q4 through the start-of-quarter employment level rather than through the firm's employment decisions in Q4. For instance, a firm with a greater negative change in revenue in the first three quarters of 2008 may have already laid off a number of workers and thus entered 2008:Q4 with a lower employment level. When I compare this firm against another firm with a similar negative change in annual revenue, I would expect to see fewer fourth-quarter layoffs at the firm with a larger fall in revenue during the first three quarters of the year. If the variation in firms' quarterly revenue is related to the timing of firms' historical typical raise quarters (for instance through seasonal effects), then this would bias the DiD estimate of Q2-raisers differential response to negative revenue changes. The direction of this bias for my coefficient estimate is ambiguous because i) I cannot observe the revenue changes experienced by firms in the first three quarters of the calendar year, and ii) I do not have a prior as to the whether Q2-raising firms had larger or smaller negative revenue changes in 2008:Q1-Q3 relative to Q4-raising firms. That said, any concern regarding bias of this sort is mitigated by Q4-raising firms not exhibiting any change in their degree of responsiveness to negative revenue changes (relative to other periods).

The second concern stems from simultaneity between the annual revenue measure and a firm's Q4 job destruction rate. It is natural to assume that a firm's labor inputs contemporaneously affect the firm's revenue. Thus, there is an endogeneity issue created by reverse causality. Exogenous negative revenue shocks generated high rates of job destruction in 2008:Q4. This job destruction, in turn, lowered employment levels, and thus further decreased revenue in 2008:Q4. I expect that this reverse causality attenuates the coefficient estimate for the differential response of Q2-raising firms' job destruction rates to negative revenue shocks (γ^{Q2}). The attenuation bias results from exogenous negative revenue shocks in 2008:Q4 forcing both Q2 and Q4-raising firms to destroy jobs. This job destruction lowered employment levels at firms, which further lowered the firms' revenue (thus generating negative year-over-year revenue changes that were larger than the exogenous revenue shocks). Critically, absent any effect of exposure to DNWR on firms‘ rates of job destruction, the effect of this simultaneity bias is the same for both Q2 and Q4-raising firms. For the same fundamental negative revenue shock, job destruction should rise similarly at both Q2 and Q4-raising firms, thus generating similar degrees of simultaneity bias. If, instead, greater exposure to DNWR forces firms to destroy more jobs, then job destruction should rise more at the Q2-raising firms in 2008:Q4. The simultaneity of revenue and employment, in conjunction with this higher job destruction, means that the observed negative revenue change is disproportionately larger than the fundamental negative revenue shock for Q2-raising firms. Thus, the same fundamental negative revenue shock generates a larger observed fall in revenue at Q2-raising firms. As a result, the estimated correlation is weaker between the job destruction rate and the interaction of the observed negative revenue change with the firm's exposure to DNWR (relative to its true correlation with the fundamental negative revenue shock), but does not reverse the sign. Thus, I interpret the result that Q2-raising firms increased their job destruction rates relative to Q4-raising firms in 2008:Q4 by an additional 0.077 percentage points for every one percent fall in year-over-year revenue as a lower bound on Q2-raising firms' actual differential sensitivity to the fundamental negative revenue shocks in 2008:Q4.

A.1.1 Attenuation Bias in Revenue Change Estimation

The estimation model includes firm-specific calendar quarter dummy variables, as well as fixed effects for industry-by-time, firm age, and firm size, so define $\tilde{J}D_{kt}$ and $\tilde{\Delta}R^{-}_{kt}$ as the within-firm-calendar-quarter residuals of job destruction and year-over-year absolute value of negative revenue changes after controlling for all of these variables. The revenue change I observe, however, is not the fundamental residualized revenue shock ($\tilde{\Delta}R^{-^F}$), where:

$$\tilde{\Delta}R^{-}_{kt} = \tilde{\Delta}R^{-^F} + \alpha\tilde{J}D_{kt} + \upsilon \tag{9}$$

where $\alpha \geq 0$ since the absolute value of the negative revenue change is weakly increasing in the number of jobs destroyed.

Among the Q2 and Q4 firms with negative revenue changes, my assumed population model is:

$$\tilde{J}D_{kt} = \beta_1\tilde{\Delta}R^{-^F} + \beta_2 d_{kt}^{Q2\ raiser}\tilde{\Delta}R^{-^F} + \epsilon \tag{10}$$

where $d_{kt}^{Q2\ raiser}$ is an indicator equal to one if the firm has a typical raise quarter in the Q2 calendar quarter. I assume that $\beta_1 > 0$ since when negative revenue shocks are larger in absolute value, I expect job destruction to rise. I assume the $\beta_2 \geq 0$, so if DNWR affects job destruction when the firm has a larger negative revenue shock, then it will increase job destruction.

When I regress the firm's job destruction rate on its observed negative revenue change, I am not estimating the population model. Instead, because of the simultaneity bias, I estimate:

$$\tilde{J}D_{kt} = \frac{\beta_1 + \beta_2 d_{kt}^{Q2\ raiser}}{1 + \beta_1\alpha + \beta_2\alpha d_{kt}^{Q2\ raiser}} \tilde{\Delta} R_{kt}^{-} + \zeta_{kt} \tag{11}$$

Given I am using a difference-in-differences estimation strategy, $\hat{\beta}_1^{DiD}$ is identified from the Q4-raisers for whom $d_{kt}^{Q2\ raiser} = 0$. Namely

$$\mathbb{E}\left[\hat{\beta}_1^{DiD}\right] = \frac{\beta_1}{1 + \beta_1\alpha} \tag{12}$$

The difference-in-differences strategy means that the $\hat{\beta}_2^{DiD}$ estimate is derived after differencing out the effect of the negative revenue change using the $\hat{\beta}_1^{DiD}$ estimate. Essentially, the Q2-raisers are used to estimate the following relationship

$$\tilde{J}D_{kt}^{Q2} = \frac{\beta_1 + \beta_2}{1 + \beta_1\alpha + \beta_2\alpha} \tilde{\Delta} R_{kt}^{-} - \hat{\beta}_1^{DiD} \tilde{\Delta} R_{kt}^{-} + \zeta_{kt} \tag{13}$$

Accordingly,

$$\mathbb{E}\left[\hat{\beta}_2^{DiD}\right] = \frac{\beta_2}{(1 + \beta_1\alpha + \beta_2\alpha)(1 + \beta_1\alpha)} \tag{14}$$

Since $\beta_1 > 0$, $\alpha > 0$, and $\beta_2 \geq 0$, it is the case that the $\mathbb{E}\left[\hat{\beta}_2^{DiD}\right]$ will always be attenuated towards zero, relative to the true value of β_2.

A.2 Biases in OLS Regression of Job Destruction on Real Wage Bill Ratio

Due to a combination of measurement error in real wages and persistent unobserved confounders affecting both past wages and current-period job destruction, a simple OLS regression of the model in Equation 5 is unlikely to yield a causal estimate of the effect of having a higher real wage bill in 2008:Q4.

The fact that I construct the firm's real wage bill from workers' estimated persistent nominal wages implies that the real wage ratio is subject to measurement error. The measurement error may be correlated with the firm's job destruction rate because the post-Lasso estimation procedure is less likely to detect wage changes towards the end of a worker's tenure at a firm (because persistent changes are truncated upon job separation). If a firm has a higher job destruction rate in 2008:Q4, then the start-of-quarter real wage estimates of laid-off full-year workers will tend to underestimate their true start-of-quarter real wages. Thus, the measurement error is likely to reinforce the omitted variable bias since it generates a negative correlation between the estimated real wage bill ratio and the firm's job destruction rate.

Any given firm will make wage change decisions by taking into account its expected future job creation and job destruction decisions. This creates a simultaneity problem whereby a firm's job destruction decision in period t could have affected the firm's wage setting behavior in period $t-1$ (from which the real wage ratio is constructed). The forward-looking nature of the firm's wage setting decisions implies that many unobserved factors could affect both the firm's start-of-quarter real wage ratio, W_{kt}, and the firm's job destruction rate - the most obvious being persistent productivity and product demand shocks. I would expect that persistent positive shocks from the recent past will increase the firm's start-of-quarter real wage ratio while decreasing its current period job destruction rate – generating a negative bias in coefficient estimates.

A.3 Alternative IV Results

In the baseline instrumental variable estimation described in Section 4.3.1, I include as an instrumental variable the firm's historical raise share in calendar quarter Q3 interacted with a 2008:Q4 indicator variable. This raise share may be endogenous since some portion of firms that historically tended to raise their workers' wages in Q3 would have done so after the Lehman Brothers bankruptcy. The ability of some of these Q3-raisers to observe the negative shock in late 2008:Q3 may have endogenously affected their real wage bill ratio at the start of 2008:Q4. To test this, I estimate the same instrumental variable model but now include the $t-1$ historical raise share in 2008:Q4 (which corresponds to the Q3 calendar quarter) as a control variable instead of as an instrumental variable. The original OLS and IV results are reported in columns (1) and (2) for the unweighted and in columns (4) and (5) for the employment-weighted estimation models. Columns (3) and (6) report the IV results when I instead include the $t-1$ raise share in 2008:Q4 as a control variable.

Table 10: Second-Stage: Job Destruction Rate

Dependent Variable:	Firm DHS Job Destruction Rate					
Estimator:	OLS	IV	IV	OLS	IV	IV
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Real Wage Bill Ratio						
W_{kt}	**-0.19*****	**1.26*****	**1.25*****	**-0.13*****	**-0.67*****	**-0.67*****
	(0.004)	(0.04)	(0.04)	(0.02)	(0.22)	(0.22)
2008:Q4 * W_{kt}	**-0.15*****	**3.56*****	**7.85*****	0.038	**3.20*****	**9.28*****
	(0.03)	(0.55)	(1.09)	(0.07)	(0.83)	(1.81)
Employment Weighted	N	N	N	Y	Y	Y
Firm FE	Y	Y	Y	Y	Y	Y
2008:Q4 Raise Share $t-1$	-	Instrument	Control	-	Instrument	Control
Kleibergen-Paap rk LM		33.4	22.5		14.2	13.3
Andersen-Rubin Wald Test		342	407		18.6	21.7
R-Squared	0.050			0.054		
Observations	7.07 million					
Clusters	161,000 firm clusters					

Note: The outcome variable is the SEIN-level DHS job destruction rate. This is regressed on the predicted endogenous explanatory variables and a set of control variables. The control variables include firm-specific fixed effects as well as dummy variables for firm age, firm size, two-digit industry, and two-digit industry-specific shocks in 2008:Q4. Quarterly LEHD data from 1999:Q1 to 2014:Q4. Sample only includes firms with at least 10 raises observed prior to 2007:Q4. Robust standard errors clustered at the SEIN-level. ***, **, * indicate statistical significance at the 0.1%, 1.0%, and 5.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0073-CED-20190910.

B Model of DNWR and Job Destruction at a Multi-Worker Firm

My quasi-experimental empirical finding that exposure to DNWR causes a substantial increase in firms' job destruction rates is inconsistent with many common macro models of employment with wage rigidity.[24] This section describes a model wherein DNWR causes positive-surplus jobs to be destroyed while ensuring that workers and firms do not forgo mutually beneficial nominal wage cuts (and thus the model is robust to the Barro critique).

Consider a multi-worker firm with time-varying, match-specific productivity and productivity spillovers between workers. The firm's production is determined by the firm's efficiency units of labor, where the firm's efficiency unit is the average of the workers' time-varying, match-specific productivity levels. The firm unilaterally sets worker-specific wages in every period, taking into account that workers' quit decisions are influenced by both their real wage levels and nominal wage changes.

Downward nominal wage rigidity affects the firm's decision-making because workers' quit rates rise persistently after a nominal wage cut. Campbell and Kamlani (1997) and Bewley (1999) conducted separate surveys of firms to investigate why firms tend not to cut workers' nominal wages. The one reason that was offered time-and-again by respondents in both surveys was that cutting workers' nominal wages encourages the best workers to quit.[25] Using the LEHD data, I confirm that workers' quit rates exhibit a persistent increase after a nominal wage cut. In Figure 5, I show estimation results from regressing an indicator variable equal to one if the worker switches employers in period t on a set of variables indicating whether the worker experienced a nominal wage cut one to six quarters ago. The figure shows that in the quarter after a nominal wage cut, workers' quit rates rise 3.2 percentage points above the baseline quit rate of 1.7%. The quit rate then slowly declines over the next five quarters, but even six quarters after the nominal wage cut, workers who have experienced a nominal wage cut are still 95% more likely to quit relative to workers with no exposure to nominal wage cuts.

The firm's decision-making is also affected by an occasionally-binding constraint that prohibits

[24] Labor search-and-matching models incorporating wage rigidity tend to assume that wage rigidity affects job creation, but not job destruction. New Keynesian DSGE models where wage adjustment costs generate nominal wage rigidity (a la Rotemberg (1982)) oftentimes find only small effects of nominal wage rigidity on employment. DSGE models that generate nominal wage rigidity using staggered wage adjustment (Taylor (1980)) or random arrival of wage adjustment opportunities (Calvo (1983)) find larger effects of wage rigidity on employment, but violate the Barro critique since workers and firms must ignore mutually beneficial nominal wage cuts.

[25] Respondents to Bewley's survey also frequently noted that cutting workers' nominal wages lowers their morale.

Figure 5: Employer-to-Employer Transition Rate after a Nominal Wage Cut

Notes: The outcome variable is the worker-level indicator variable that equals one if the worker undergoes a no-earnings gap employer-to-employer transition (EE rate), which Haltiwanger, Hyatt, Kahn and McEntarfer (2018) defines as same-quarter or adjacent employer-to-employer transitions with less than a one month gap in earnings. The control variables include the firm's period-specific EE rate and non-EE separation rate, the magnitude of the raise or cut in each of the six lagged periods, dummy variables for the time-period, the firm's typical raise quarter, the firm's two-digit industry, the worker's age, sex, race, and education level, as well as indicator variables if the worker received a raise in each of the last six periods. Sample of 12.5 million observations from the secondary sample includes only workers between 18-61 years of age with at least seven quarters of tenure. R-squared of 0.147. Robust standard errors. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

the firms' current-period earnings from falling below some threshold.[26] If a negative shock lowers the firm's current-period earnings sufficiently, then the constraint becomes binding, forcing the firm to consider second-best alternatives to the surplus-maximizing layoff and wage-setting decisions.[27]

Using an occasionally binding constraint based on earnings as part of the mechanism by which DNWR affects firms' job destruction is consistent with the empirical findings from my quasi-experiment. Section 4.2.1 shows, in 2008:Q4 when the Q2-raising firms were more exposed to DNWR relative to Q4-raising firms, Q2-raising firms' job destruction rates were temporarily 48% more responsive to similar magnitude, negative revenue changes relative to the job destruction rates of Q4-raising firms. Thus, short-term declines in revenue (and presumably current-period earnings)

[26]It reasonable to assume that an earnings-based constraint is relevant for both small and large firms. Small firms have limited access to financing that could smooth temporary fluctuations in earnings. Large firms can tap into financing, but Lian and Ma (2019) show that 80% of non-financial corporations' debt contains covenants limiting borrowing based on cash flows from operations (the other 20% focus on assets). Chodorow-Reich and Falato (2017) find that one-third of non-financial corporate debt breached a loan covenant during the 2008-2009 financial crisis. Drechsel (2019) explores the macroeconomic implications of earnings-based borrowing constraints in a DSGE model with nominal wage rigidities.

[27]In the model, I specify a current-period earnings constraint is the same as the 'internal funds' constraint that Schoefer (2016) used to examine the implications of incumbent wage rigidity on hiring and job creation.

are strongly associated with greater job destruction at firms with more exposure to DNWR.[28]

In the model, DNWR causes inefficient job destruction due to the interaction of: i) the firm's current-period earnings constraint, ii) heterogeneity in the firm surplus generated by each worker, and iii) the workers' quit response to nominal wage cuts. When the firm experiences a large negative shock, fixed labor costs combine with the current-period minimum earnings constraint to force the firm to choose between laying off a worker with a positive firm surplus or cutting the nominal wages of both the worker and a coworker with a larger firm surplus.[29] If workers' quit rates respond strongly enough to a nominal wage cut and the gap in the employees' firm surpluses is wide enough, then the firm optimally chooses to lay off the worker in order to avoid cutting the nominal wage of a coworker with a larger firm surplus. Thus, DNWR destroys the job of a positive-surplus worker because the certain loss in surplus from laying off the worker is less than the expected loss in surplus from a higher-surplus coworker being more likely to quit after a nominal wage cut.

Importantly, this model is robust to the Barro critique. Specifically, DNWR never causes the firm to destroy a job when there is a nominal wage at which both i) the worker and firm receive non-negative surpluses, and ii) the current-period earnings constraint does not bind. When the negative shock is small enough that the firm could relax the current-period earnings constraint by cutting the nominal wage of a single worker, the firm will always choose to cut the worker's wage so long as the surplus generated by the worker remains positive at the lower wage.

B.1 Firm State Variables

At the start of each period, the aggregate state consists of the aggregate productivity level (A_t) and the inflation rate (π_t). Although both the starting real wage of new hires, w_t^n, and the labor market tightness, θ_t, are set in equilibrium every period, each firm is small enough that firms take the equilibrium wage and labor market tightness as given. I denote the firm's perceived aggregate state as $\Theta_t = (A_t, \pi_t, w_t^n, \theta_t)$. Both aggregate productivity and the inflation rate are stochastic processes with persistent shocks.

A firm represents the combination of a set of firm practices and a team of workers, both of which evolve over time. As a result, the firm starts each period with both a firm-specific, time-varying

[28] It is also the case that Schoefer (2016) presents empirical evidence that causally links short-term fluctuations in cash flows with employment levels. He did not, however, differentiate between the job creation and job destruction margins.

[29] More specifically, the magnitude of the negative shock must be such that the current-period earnings constraint still binds after cutting one worker's wage to the minimum wage, but becomes relaxed if the firm lays off the worker as a layoff would eliminate the fixed labor costs associated with the worker.

productivity level (z_{kt}) and a set of employees E_{kt}. Each employee i in period t is characterized by the tuple $(x_{it}, w_{it-1}, \Delta^-_{it-1})$, where x_{it} is the employee's time-varying match-specific productivity, w_{it-1} is her nominal wage in the previous period, and Δ^-_{it-1} is an indicator variable equal to one if she has ever received a nominal wage cut from this employer in the past. I denote the firm-specific state as $\Omega_t = (z_{kt}, E_{kt})$. Both the firm-specific productivity and the employee's time-varying match-specific productivity are stochastic processes with persistent shocks.

B.2 Firm Production

The firm's production is determined by the team of workers it chooses to retain. There are productivity spillovers among team members, so the total production of the team is determined by the average productivity level of its members. I model this as a firm production function with constant returns to scale in efficiency units of labor, scaled by the product of the aggregate and firm-specific productivity levels. The firm's efficiency unit is the geometric average of workers' time-varying, match-specific productivity levels. Specifically, the firm's production function is:

$$f\left(A_t, z_{kt}, \{x_{it}, h_{it}\}_{\forall i \in E_{kt}}\right) = A_t z_{kt} \left(\prod_{i \in E_{kt}} (x_{it})^{h_{it}/n_{kt}} \, n_{kt} \right) \tag{15}$$

where h_{it} is an indicator variable equal to one if the firm chooses to retain worker i in period t. Laid off workers affect neither the firm's current period production nor its wage bill, so the firm's total employment level after the layoff decision is $n_{kt} = \sum_{i \in E_{kt}} h_{it}$.

B.3 Firm Decision Set and Surplus

Each period, for every worker in the firm's employee set, the firm chooses whether to lay off the worker ($h_{it} = 0$). For any worker not laid off ($h_{it} = 1$), the firm sets a worker-specific real wage (w_{it}). If the wage requires a nominal wage cut relative to the worker's previous nominal wage, then the nominal wage cut indicator is set to one. Otherwise, it retains its value from the previous period:

$$\Delta^-_{it} = \begin{cases} 1 & \text{if } \Delta^-_{it-1} = 1 \text{ or } w_{it} < w_{it-1}/\pi_t \\ 0 & \text{otherwise} \end{cases} \tag{16}$$

The firm also chooses whether to post vacancies, v_{kt}. The firm makes these decisions to maximize the following value function where output prices are normalized to one:

$$S\left(\Theta_t, \Omega_{kt}\right) = \max_{v_{kt}, \{h_{it}, w_{it}\}_{\forall i \in E_{kt}}} f_{kt} - \sum_i w_{it} h_{it} - c^n n_{kt} - \frac{c^v}{g(\theta_t})v_{kt} + \beta \mathbb{E}_t\left[S_{kt+1}\right] \tag{17}$$

subject to:

1. A current period earnings constraint: $f_{kt} - \sum_i w_{it} h_{it} - c^n n_{kt} - c^v/g(\theta_t) \geq a$
2. Binary layoff decision sets for all workers: $h_{it} \in \{0, 1\}\ \forall i \in E_{kt}$
3. A constraint that the vacancies be a non-negative integer: $v_{kt} \in \mathbb{Z}^*$
4. A constraint that workers' real wages be at or above a minimum wage: $w_{it} \geq \underline{w}\ \forall i \in E_{kt}$

where c^n is the fixed cost associated with each worker, c^v is the cost of posting a job vacancy, β is the discount factor, and $g()$ is the firm's job filling rate. I assume that the firm always fills a posted vacancy, but that the cost varies with the labor market tightness, where $g' < 0$.

B.4 Evolution of the Firm's Employee Set

The firm's employee set evolves from t to $t+1$ as follows. The firm's employee set in $t+1$ is the union of the set of new hires $N(v_{kt})$ and the set of workers in the firm's period t employee set who neither quit nor are laid off ($\{i \in E_{kt} | i \notin (L_{kt} \cup Q_{kt})\}$). The set of new hires in $t+1$ has v_{kt} members, each of whom has a previous period wage of w_t^n, a nominal wage cut indicator equal to zero, and a productivity draw from the new hire productivity distribution (with expected value $\bar{x}^n$). The set of laid-off workers, L_{kt}, includes all workers $i \in E_{kt}$ with $h_{it} = 0$.

I denote the set of workers who chose to quit as Q_{kt}, where a worker's quit rate has two components. The first component follows a standard turnover efficiency wage model (Stiglitz (1974); Salop (1979)), where the quit rate is declining in the ratio of the worker's real wage to the market wage. The second component captures the substantial and persistent increase in a worker's quit rate that I find occurs after the worker experiences a nominal wage cut.[30] Specifically, I assume that the probability that a worker quits the firm, q_{it}, is a weakly decreasing, concave function of the ratio of the worker's real wage w_{it} to the new hire starting wage w_t^n, scaled by a factor δ (where $\delta > 1$) if the worker has ever experienced a nominal wage cut at the firm ($\Delta_{it}^- = 1$).

$$q_{it} = \delta^{\Delta_{it}^-} q\left(\frac{w_{it}}{w_t^n}\right) \tag{18}$$

[30] This representation of workers' quit behavior is similar to the fair-wage efficiency wage model proposed by Akerlof and Yellen (1990). The fair-wage efficiency wage model argues that a worker's quit rate rises (or effort level falls) when the worker's wage falls below some reference wage - in this case her previous nominal wage. In this model, however, the response of the quit rate to a nominal wage cut persists indefinitely into the future, no matter what happens subsequently to the worker's wage.

B.5 Optimal Firm Layoff and Wage Setting Decisions

In the model, DNWR causes firms to destroy positive surplus jobs due to the interaction of: i) the firm's current-period earnings constraint, ii) heterogeneity in the firm surplus generated by each worker, and iii) the workers' quit response to nominal wage cuts. When the firm experiences a large negative productivity shock, fixed labor costs combine with the current-period minimum earnings constraint to force the firm to choose between laying off a worker with a positive firm surplus versus cutting the nominal wages of both the worker and a coworker with a larger firm surplus. If workers' quit rates respond strongly enough to a nominal wage cut and the gap in the employees' firm surpluses is wide enough, then the firm optimally chooses to lay off the low-surplus worker in order to avoid cutting the nominal wage of a coworker with a larger firm surplus. Thus, DNWR generates job destruction because the certain loss in firm surplus from laying off the worker is less than the expected loss in firm surplus from a higher-surplus coworker being more likely to quit after a nominal wage cut. Here I specify four important implications of the model. Appendix **??** derives the firm's optimal layoff and wage setting decisions and presents proofs of these four propositions.

Proposition 1 (DNWR Causes Job Destruction)**.** *Given a sufficiently large negative shock to either aggregate or firm-specific productivity, DNWR (in the form of workers' quit response to nominal wage cuts) may cause a firm to optimally layoff a positive-surplus worker instead of retaining the worker at a lower nominal wage.*

Proposition 2 (Job Destruction Increasing in Quit Response to Nominal Wage Cuts)**.** *If the firm is bound by the current-period earnings constraint and cutting the nominal wage of a single worker is insufficient to relax the constraint, then the likelihood that DNWR causes a firm to layoff a positive-surplus worker is increasing in δ, the magnitude of the persistent rise in a worker's quit rate after a nominal wage cut.*

Proposition 3 (Job Destruction Caused by DNWR Increasing in Dispersion of Workers' Marginal Surplus)**.** *If the firm is bound by the current-period earnings constraint and cutting the nominal wage of a single worker is insufficient to relax the constraint, then the likelihood that DNWR causes a firm to layoff a positive-surplus worker is increasing in the difference in marginal firm surplus between the worker at risk of layoff and the coworker at risk of a nominal wage cut.*

Proposition 4 (Robustness to Barro Critique)**.** *If i) the firm is bound by the current-period earnings constraint, and ii) cutting the nominal wage of a single worker is sufficient to relax the current-*

period earnings constraint, and iii) the worker generates a positive marginal surplus at this lower nominal wage, then the firm always chooses to cut the worker's wage instead of laying off the worker.

B.6 Testable Prediction of Mechanism for DNWR to Cause Job Destruction

One prediction of the model is that when the firm experiences a negative shock, the job destruction rate should rise more at firms with greater within-firm dispersion in workers' marginal firm surpluses. I use the quasi-experiment described in Section 4 to test this prediction empirically.

Although I do not observe the firm surplus generated by individual workers, I argue that the within-firm dispersion in workers' real wage growth serves as a proxy for the within-firm dispersion in workers' marginal firm surplus. When the firm hires a worker, it sets an optimal wage for the worker based on the worker's observable characteristics. It is reasonable to assume that subsequent changes in the real wage of the worker are positively correlated with changes in either the worker's productivity (e.g. firm-specific human capital accumulation) or the firm's certainty about the worker's productivity. Thus, firms with greater dispersion in workers' productivity innovations since hiring should tend to exhibit greater dispersion in their workers' observable real wage changes since hiring. I calculate each worker's annualized real wage growth since the worker's initial hiring at the firm. I then calculate the within-firm standard deviation of workers' annualized real wage growth rates. The within-firm standard deviation of these growth rates serves as a proxy for dispersion in the surpluses generated by workers at the firm (since these productivity changes were not factored into workers' initial wages, but, rather, were incorporated over the workers' tenure as the firm periodically resets workers' wages to their surplus-maximizing levels). I then standardize this proxy measure to be in units of the across-firm standard deviation of this within-firm measure of dispersion in workers' real wage changes.

Within the framework of my quasi-experiment, the model predicts that the job destruction rate should have risen more in 2008:Q4 at Q2-raising firms with greater historical dispersion in their workers' real wage growth. Thus, to test the model prediction, I include in the difference-in-differences specification from Section 4.2 a set of 2008:Q4 interaction terms of the dummy variables indicating a firm's typical raise quarter with the historical within-firm dispersion in workers' real wage growth (as a proxy for the dispersion in the surplus generated by workers at the firm). Specifically, I estimate

$$JD_{kt} = \boldsymbol{D}_{\boldsymbol{kt}}^{\mathbf{2008:}\boldsymbol{Q4}} \left(\beta^{Q4} + \boldsymbol{C}_{\boldsymbol{kt}}\gamma^{Q4} + \boldsymbol{D}_{\boldsymbol{kt}}^{\text{Q2 raiser}} \left(\beta^{Q2} + \boldsymbol{C}_{\boldsymbol{kt}}\gamma^{Q2}\right)\right) + \boldsymbol{X}_{kt}\beta_t + \epsilon_{kt} \tag{19}$$

where $\boldsymbol{D}_{\boldsymbol{kt}}^{\boldsymbol{2008:Q4}}$ is an nxn diagonal matrix where each diagonal element equals one if t=2008:Q4 and n is the number of observations; $\boldsymbol{C_{kt}}$ is the standardized measure of the within-firm dispersion of workers' real wage growth (or other firm characteristic); and $\boldsymbol{D}_{\boldsymbol{kt}}^{\text{Q2 raiser}}$ is an nxn diagonal matrix with each diagonal element equal to one if firm k typically raises wages in the second quarter. The coefficient of interest is γ^{Q2}, since this indicates whether firms with greater exposure to downward nominal wage rigidity had a differential job destruction response to the unanticipated negative shock in 2008:Q4 based on the dispersion in workers' real wage growth. The $\boldsymbol{X}_{kt}$ matrix contains a set of control variables that include fixed effects for firm-specific seasonality, firm age, firm size, and industry-by-time. I use a similar specification to explore heterogeneity in firms' responsiveness to DNWR exposure along the dimensions of firm age, size, and average labor productivity.[31]

Table 11: Differential Q2 vs Q4-Raiser Job Destruction Rate in 2008:Q4 by Firm Characteristic

	Std Dev of Log Employee Wage Growth	Firm Age		Firm Size		Log Labor Productivity
	(1)	(2)		(3)		(4)
	0.70**	0-1	-1.27	1-19	**1.12*****	-0.12
	(0.23)		(0.90)		(0.25)	(0.29)
		2-3	**3.19*****	20-49	**1.61****	
			(0.75)		(0.43)	
		4-5	1.26	50-249	**1.55****	
			(0.78)		(0.54)	
		6-10	**1.27****	250-499	3.08	
			(0.49)		(1.54)	
		11+	**1.20*****	500+	2.73	
			(0.24)		(1.90)	
Outcome Mean	6.5%		7.7%		7.7%	7.4%
Observations	1.8M		4.6M		5.6M	1.7M
R-Squared	0.335		0.317 M		0.290	0.340

Outcome Variable: SEIN-level DHS job destruction rate. Controls include fixed effects for firm-specific seasonality, firm age, firm size and industry-by-time, as well as interactions of the firm characteristic with the firm's typical raise quarter indicator. Quarterly LEHD data from 1999:Q1 to 2014:Q4. Robust standard errors clustered at the SEIN-level. ***, **, * indicate statistical significance at the 0.1%, 1.0%, and 5.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

Table 11 reports the results of various difference-in-difference models that interact the firm's typical raise quarter and 2008:Q4 dummy variables with firm characteristics. Critically for the empirical test of the model, Column (1) of Table 11 shows that in 2008:Q4, Q2-raising firms with

[31] For these alternative firm characteristics, I also include in the set of control variables an interaction of the time-varying firm characteristics (firm age, firm size, and average labor productivity) with the firm's typical raise quarter. The time-invariant firm characteristics are absorbed by firm-specific seasonality fixed-effects.

greater dispersion in employees' annualized wage growth raised their job destruction rates by 0.70 percentage points more for every one standard deviation increase (relative to other firms) in the within-firm dispersion of the workers' annualized real wage growth. This effect is consistent with the model's prediction that exposure to DNWR generates more job destruction at firms with greater dispersion in workers' marginal firm surpluses.

This section presents a framework for determining a firm's optimal layoff and wage setting decisions and how these decisions change when the current-period earnings constraint binds. This framework is complicated by the heterogeneity in worker productivity, productivity spillovers between workers, and the firm's ability to unilaterally set a distinct wage for each worker. Further complications result from the discontinuities in the firm's surplus function introduced by both the workers' quit response to nominal wage cuts and the firm's current-period earnings constraint.

B.7 The Firm's Unconstrained Optimal Layoff and Wage Setting Decisions

As noted by Elsby and Michaels (2013), productivity spillovers between workers in a multi-worker firm model complicates the wage setting decision (and also the layoff decision for this model) because the marginal product of a worker is different from her inframarginal product. To overcome this challenge, Elsby and Michaels employ the wage bargaining methodology proposed by Stole and Zwiebel (1996) where workers and firms determine wages using Nash bargaining over the marginal surplus of the worker. In Elsby and Michaels' multi-worker firm model, the productivity spillovers result from decreasing returns to scale in labor inputs from workers with homogeneous productivity. My model assumes constant returns to scale but with the firm's production determined by the average of its workers' heterogeneous productivity levels. I follow Elsby and Michaels in assuming that workers' wages are set based on each worker's marginal surplus. The combination of this assumption with heterogeneity in worker productivity, however, requires that the firm in my model be able to set distinct wage rates for different workers.

I define $S^{+i}(w_{it}; \Theta_t, z_{kt}, E^r_{kt})$ as the marginal surplus of worker i at wage w_{it} given the aggregate state (Θ_t), the firm productivity level z_{kt}, and the firm's retained employee set (E^r_{kt}). A worker i's marginal surplus at wage w_{it} is the change in firm surplus generated when worker i is paid w_{it} and added to the retained employee set minus the firm surplus that is generated if the firm employs only the retained worker set E^r_{kt}. Specifically,

$$S^{+i}\left(w_{it}; \Theta_t, z_{kt}, E^r_{kt}\right) = S\left(\{w^s_{jt}\}_{\forall j \in E^r_{kt}}; \Theta_t, z_{kt}, E^r_{kt} + i, w_{it}, \Delta^-_{it}\right) - S\left(\{w^s_{jt}\}_{\forall j \in E^r_{kt}}; \Theta_t, z_{kt}, E^r_{kt}\right) \qquad (20)$$

where w^s_{jt} is worker j's surplus-maximizing wage given the set of retained workers.

The worker's surplus-maximizing wage, w_{it}^s, is the wage that maximizes the worker's marginal surplus as defined in Equation 20, ignoring the current-period earnings constraint. Critically for DNWR's effect on firm layoff and wage setting behavior, the assumption that workers' quit rates persistently rise after a nominal wage cut implies that, for workers with a positive expected future marginal surplus who have not yet experienced a nominal wage cut ($\Delta_{it-1}^{-} = 0$), the worker's marginal surplus experiences a discontinuous drop when the worker's current period wage falls below the worker's previous period nominal wage (i.e. if $w_{it} < w_{it-1}/\pi_t$). This drop in marginal surplus can be quite large since much of the value of a worker derives from the expected future stream of profits. Cutting the worker's nominal wage decreases the expected job duration, and consequently lowers the future stream of profits. As a result of this discontinuity in a worker's marginal surplus function, the worker's surplus-maximizing wage is whichever of the following two wages maximizes the worker's marginal firm surplus: i) the current-period real value of the worker's previous nominal wage: w_{it-1}/π_t, or ii) the wage w_{it}^* that satisfies the following first-order condition:

Heterogeneity in worker productivity creates a further complication when combined with productivity spillovers between workers, since it is not straightforward to identify the optimal set of workers that maximizes the firm surplus. In many cases, workers who have a negative marginal contribution to the firm surplus under one employee set may generate a positive marginal contribution under an alternative employee set. I define the firm's unconstrained optimal retained worker set ($E_{kt}^{r^*}$) as the employee set that maximizes the firm's total surplus if the current-period earnings constraint is ignored.

The unconstrained optimal retained worker set, $E_{kt}^{r^*}$ has two properties of note. First, only workers with positive marginal surpluses are members of $E_{kt}^{r^*}$ (since otherwise the firm's total surplus would rise by laying off the included worker with a negative marginal surplus). Second, no worker with a positive marginal surplus, given $E_{kt}^{r^*}$, is excluded from the unconstrained optimal retained worker set (since adding such a worker to the set would necessarily increase the firm's total surplus).

I denote the firm's optimal layoff and wage setting policies when the current-period earnings constraint does not bind as (H_{kt}^s, W_{kt}^s). H_{kt}^s is the set of the firm's optimal layoff decisions, h_{it}^s, for all workers in E_{kt}. These are:

$$h_{it}^{s} = \begin{cases} 0 & \text{if } i \in E_{kt} \text{ and } i \notin E_{kt}^{r^*} \\ 1 & \text{if } i \in E_{kt} \text{ and } i \in E_{kt}^{r^*} \end{cases}$$

W_{kt}^{s} is the set of the firm's employees' surplus-maximizing wages given that the retained employee set is $E_{kt}^{r^*}$.

B.8 The Firm's Earnings-Constrained Optimal Layoff and Wage Setting Decisions

To determine how the firm's optimal layoff and wage setting decisions change when the current-period earnings constraint binds, I consider any firm with a non-empty unconstrained optimal retained worker set, $E_{kt}^{r^*}$. Despite these employees' positive marginal surpluses, it is possible that the product of aggregate productivity A_t and firm-specific productivity z_{kt} is temporarily low enough that the firm's current-period earnings at the employees' surplus-maximizing wages falls below the constraint threshold a. In such a scenario, the current-period earnings constraint forces the firm to make one or more second-best decisions. It must do some permutation of cutting employees' wages relative to their surplus-maximizing wage or laying off employees with positive marginal surpluses.

In cases when the current-period earnings constraint binds, it is useful to define a worker i's current-period marginal earnings, $p^{+i}(w_{it}; \Theta_t, z_{kt}, E_{kt}^{r}, x_{it})$ as the change in the firm's current-period earnings when the worker is added to the baseline retained worker set, E_{kt}^{r}, and paid wage w_{it} versus if she is laid off and the firm only employs the workers in E_{kt}^{r}. This is:

$$p^{+i}\left(w_{it}; \Theta_t, z_{kt}, E_{kt}^{r}, x_{it}\right) = A_t z_{kt} \bar{x}_{kt} \left(\left(\left(\frac{x_{it}}{\bar{x}_{kt}} \right)^{\frac{1}{n_{kt}+1}} - 1 \right) n_{kt} + \left(\frac{x_{it}}{\bar{x}_{kt}} \right)^{\frac{1}{n_{kt}+1}} \right) - w_{it} - c^{n} \qquad (21)$$

where $\bar{x}_{kt}$ and n_{kt} are both functions of the retained worker set E_{kt}^{r}. $\bar{x}_{kt}$ is the geometric mean of the idiosyncratic productivity levels of all workers included in the retained worker set. n_{kt} is the number of workers included in the retained worker set.

There are three important restrictions on the set of potential second-best policies that a firm considers when trying to maximize firm surplus while satisfying the current-period earnings constraint. First, a firm will never reconsider the layoff decision of any worker who is optimally laid off absent the constraint. This first restriction follows from two facts: i) only workers with negative marginal surpluses are excluded from the unconstrained optimal retained worker set; and ii) any worker who generates a positive current-period marginal earnings at her surplus-maximizing wage ($p^{+i}(w^s_{it}) > 0$) must necessarily also have a positive marginal firm surplus (since the firm can costlessly lay off the worker in the next period). Thus, retaining any workers who would optimally be laid off when the firm is unconstrained would both tighten the current-period earnings constraint and lower the firm surplus.

Second, a firm will never consider laying off workers who generate positive current-period marginal earnings since laying them off would actually tighten the current-period earnings constraint. Given that all such workers are included in the optimal retained worker set, the firm will always choose to retain these workers because they both help to relax the constraint and generate positive marginal surpluses.

And third, for any employee i in the optimal retained worker set, the firm will consider only the range of wages between the minimum wage, $\underline{w}$,[32] and the surplus-maximizing wage, w^s_{it}.[33] The firm considers only wages within the range of $[\underline{w}, w^s_{it}]$ because any wage outside of this range simultaneously tightens the current-period earnings constraint and lowers the firm surplus.

These three restrictions imply that the firm considers the following set of potential second-best policies $(\tilde{H}_{kt}, \tilde{W}_{kt})$ when the current-period constraint binds:

$$h^c_{it} \in \begin{cases} 1 & \text{if } i \in E^{r^*}_{kt} \text{ and } p^{+i}_{kt}\left(w^s_{it}\right) \geq 0 \\ \{0,1\} & \text{if } i \in E^{r^*}_{kt} \text{ and } p^{+i}_{kt}\left(w^s_{it}\right) < 0 \end{cases}$$

[32] Any wage below the minimum wage is proscribed. The minimum wage maximizes the worker's current-period marginal earnings since the current-period earnings maximizing wage ignores the effect of today's wage on employee retention and thus the future value of the employee.

[33] A sufficient condition for the surplus-maximizing wage to be greater than the current-period earnings-maximizing wage is that, at the worker's current-period earnings-maximizing wage, the expected marginal benefit from lowering the worker's quit rate (by raising his wage) is greater than the expected marginal benefit of the lower likelihood that the worker is bound by downward nominal wage rigidity in the future. More concretely,

$$\frac{1}{w^n_t}\frac{\partial \mathbb{E}\left[S^{+i}_{t+1}\right]}{\partial q_i}\frac{\partial q}{\partial w_i / w^n} \geq \frac{\partial \mathbb{E}\left[S^{+i}_{t+1}\right]}{\partial w_{it}}$$

This condition will always be met for workers with $\Delta^-_{it} = 1$ since they have already been exposed to a nominal wage cut, and thus the RHS of the condition is zero, while the LHS is strictly positive for workers with positive marginal surplus. For workers who have not been exposed to a nominal wage cut, it is possible that this condition may not hold if the earnings-maximizing wage is quite high due to a very high aggregate productivity and the likelihood of a fall in aggregate productivity is quite high (thus making it more likely that the worker would ideally have a wage cut in the future).

and

$$w_{kt}^c \in \begin{cases} [\underline{w}, w_{it}^s] & \text{if } i \in E_{kt}^{r^*} \end{cases}$$

Given this constrained set of potential second-best policies, the firm optimally chooses the set of layoff and wage setting decisions that lose the least amount of firm surplus (relative to the unconstrained firm surplus) while relaxing the current-period earnings constraint. To determine this optimal set of second-best layoff and wage setting decisions, I construct a metric, ℓ, that compares the loss in marginal surplus relative to the gain in additional earnings for any given wage rate or layoff decision. Specifically, for a layoff, this ratio metric is:

$$\ell(h_{it} = 0) = \frac{\text{Surplus Loss from Layoff}}{\text{Earnings Gain from Layoff}} = \frac{0 - S_{kt}^{+i}(w_{it}^s)}{0 - p_{kt}^{+i}(w_{it}^s)} \tag{22}$$

and for a deviation of an employee's wage from the surplus-maximizing wage, this ratio metric is:

$$\ell(h_{it} = 1, w_{it}) = \frac{\text{Surplus Loss at Wage } w_{it}}{\text{Earnings Gain at Wage } w_{it}} = \frac{S_{kt}^{+i}(w_{it}) - S_{kt}^{+i}(w_{it}^s)}{w_{it} - w_{it}^s} \tag{23}$$

For each potential second-best layoff or wage setting decision, these ratio metrics are always negative. The firm prefers layoff and wage setting decisions that have ratio metrics closer to zero since less surplus is lost for a given gain in earnings.

The firm's optimal set of second-best layoff and wage setting decisions (H_{kt}^c, W_{kt}^c) will minimize the loss of firm surplus (relative to the unconstrained firm surplus) subject to the current-period earnings constraint no longer binding. To arrive at this second best policy decision set, I begin with the set of unconstrained optimal layoff and wage setting decisions (H_{kt}^s, W_{kt}^s). I then iteratively identify which second-best decision to take next, choosing from the remaining set $(\tilde{H}_{kt}, \tilde{W}_{kt})$ the decision with the ℓ value closest to zero.[34] I continue selecting second-best wage and layoff decisions until the current-period earnings constraint no longer binds. This final set of original optimal decisions and the least costly second-best decisions becomes the firm's constrained policy decision set (H_{kt}^c, W_{kt}^c).

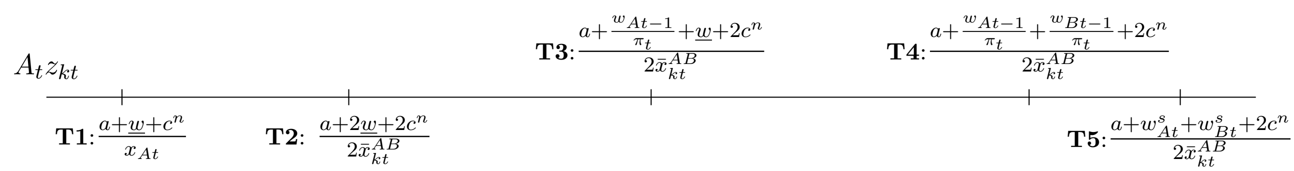
[34] As this iterative process continues, it is possible that the gain in the current-period earnings from laying off a positive-surplus worker exceeds the remaining earnings gap between the firm's current-period earnings, given all of the previous second-best decisions, and the threshold a. In such a case, the denominator in the $\ell(h_{it} = 0)$ ratio metric for a layoff is set to the remaining earnings gap rather than the total realized earnings gain from the lay off. This is because greater current-period earnings are only useful (relative to the surplus loss) insofar as they help relax the current-period earnings constraint.

B.8.1 DNWR and Constrained Layoff / Wage Setting Decisions at a 2-Worker Firm

To understand how downward nominal wage rigidity affects a firm's layoff and wage-setting decisions when the current-period earnings constraint binds, it is helpful to consider a simple example of a firm whose unconstrained optimal retained worker set, $E^{r^*}_{kt}$ includes only two employees: Worker A and Worker B. I assume that: i) Worker A generates a larger marginal firm surplus than Worker B ($S^{+A}_{At} > S^{+B}_{Bt}$); and that neither worker has ever experienced a nominal wage cut at the firm ($\Delta^-_{At-1} = 0$ and $\Delta^-_{Bt-1} = 0$). Since both workers are in $E^{r^*}_{kt}$, they both generate a positive marginal surplus at their surplus-maximizing wages: w^s_{At} and w^s_{Bt}.

Despite their positive marginal surpluses, it is possible that the product of aggregate productivity A_t and firm productivity z_{kt} is temporarily low enough that the current-period earnings constraint binds at the employees' surplus-maximizing wages. In such a scenario, the current-period earnings constraint forces the firm to make second-best decisions; it must either lay off or cut the wages of one or both of its employees. The set of second-best decisions depends on the magnitude of $A_t z_{kt}$. The diagram below shows the range of $A_t z_{kt}$ values over which the current-period earnings constraint binds. If the $A_t z_{kt}$ value is so low that it falls below the T1 threshold, then the current-period earnings constraint forces the firm to lay off both workers since not even the higher productivity worker (Worker A) generates sufficient earnings at her current-period earnings-maximizing wage. The T2 threshold indicates the $A_t z_{kt}$ value at which the firm is able to retain both workers at their current-period earnings-maximizing wages. The current-period earnings constraint continues to bind at the workers' surplus-maximizing wages until $A_t z_{kt}$ reaches the T5 threshold.

This diagram also shows two other $A_t z_{kt}$ thresholds of interest. The T4 threshold indicates the $A_t z_{kt}$ value below which the firm must choose to either cut the nominal wage of at least one worker or lay off a worker. The T3 threshold is the $A_t z_{kt}$ value below which the firm must either cut the nominal wages of both workers or lay off a worker. To ease notation, I define $\bar{x}^{AB}_{kt}$ as the geometric mean of the workers' productivity levels ($\sqrt{x_{At}}\sqrt{x_{At}}$).



B.8.1.1 Two Worker Firm: DNWR Generates Inefficient Layoffs of Positive-Surplus Workers

Downward nominal wage rigidity may generate inefficient layoffs of positive surplus workers when the $A_t z_{kt}$ value falls in the range between the T2 and T3 thresholds. When $A_t z_{kt}$ is below the T2 threshold, the current-period earnings constraint always forces the firm to lay off Worker B. When $A_t z_{kt}$ is above the T3 threshold, the firm can choose to freeze Worker A's nominal wage while retaining Worker B at his current-period earnings-maximizing wage. It is only when the $A_t z_{kt}$ shock is between T2 and T3 that the firm must choose between laying off Worker B or cutting the nominal wages of both Worker A and Worker B. The firm will choose whichever of these two options maximizes the firm surplus. Specifically, the firm lays off Worker B (who has a positive firm surplus) if, given the start-of-period aggregate and firm-specific states (Θ_t, Ω_{kt}), the following condition holds:

$$\underbrace{S_{kt}\left(w_{At}^{c|A}, \Delta_{At}^{-}=0, h_{Bt}=0\right)}_{\text{Surplus From Only Worker A - No Cut}} > \underbrace{S_{kt}\left(w_{At}^{c|AB}, \Delta_{At}^{-}=1, w_{Bt}^{c|AB}, \Delta_{Bt}^{-}=1\right)}_{\text{Surplus From Both Worker A \& B - Both Cut}} \tag{24}$$

where $w_{At}^{c|A}$ is Worker A's constrained surplus-maximizing wage when only Worker A is employed; and $w_{it}^{c|AB}$ is Worker i's constrained surplus-maximizing wage when both Worker A and Worker B are employed.

To better understand the factors driving this layoff decision, it is useful to expand the two sides of this inequality condition:

$$\begin{aligned}
&\overbrace{\frac{1}{\beta}\Pi\left(w_{At}^{c|A}, -\right)}^{\text{E1: Current Earnings from A}} + \overbrace{\left(q\left(w_{At}^{c|AB}, cut\right) - q\left(w_{At}^{c|A}, no\ cut\right)\right)}^{\text{E2: }\triangle\text{Quit rate from wage cut}} \overbrace{\mathbb{E}\left[S_{t+1}\left(\left(w_{At}^{c|A}, no\ cut\right), -\right)\right]}^{\text{E3: Future surplus: A(no-cut)}} \\
&+ \underbrace{\left(1 - q\left(w_{At}^{c|AB}, cut\right)\right)}_{\text{E4: Prob A stays after cut}} \underbrace{\left(\mathbb{E}\left[S_{t+1}\left(w_{At}^{c|A}, no\ cut\right), -\right] - \mathbb{E}\left[S_{t+1}\left(w_{At}^{c|AB}, cut\right), -\right]\right)}_{\text{E5: }\triangle\text{ Future surplus: A(no-cut) - A(cut)}} \\
&> \\
&\overbrace{\frac{1}{\beta}\Pi\left(w_{At}^{c|AB}, w_{Bt}^{c|AB}\right)}^{\text{E6: Current Earnings from A\&B}} + \overbrace{\left(1 - q\left(w_{Bt}^{c|AB}, cut\right)\right) q\left(w_{At}^{c|AB}, cut\right)}^{\text{E7: Prob only B stays after cut}} \overbrace{\mathbb{E}\left[S_{t+1}\left(-, \left(w_{Bt}^{c|AB}, cut\right)\right)\right]}^{\text{E8: Future surplus: B(cut)}}
\end{aligned} \tag{25}$$

$$+\underbrace{\left(1-q\left(w_{Bt}^{c|AB},cut\right)\right)\left(1-q\left(w_{At}^{c|AB},cut\right)\right)}_{\text{E9: Prob both A\&B stay after cut}}\underbrace{\mathbb{E}\left[S_{t+1}^{+B}\left(\left(w_{At}^{c|AB},cut\right),\left(w_{Bt}^{c|AB},cut\right)\right)\right]}_{\text{E10: Marginal future surplus: A(cut)—B(cut)}}$$

The left-hand side of this inequality captures the firm surplus that results from laying off Worker B and setting Worker A's wage to its constrained optimal value (which does not require a nominal wage cut). There are three components of this surplus. The first component, E1, represents the current-period earnings that is generated when only Worker A is employed at her constrained optimal wage $w_{At}^{C|A}$. The second component, the product of E2 and E3, represents the gain in expected firm surplus from the worker being less likely to quit immediately if her nominal wage is not cut. Specifically, this is the decrease in Worker A's quit rate if her nominal wage is not cut, multiplied by the expected future marginal surplus she generates if her nominal wage remains uncut. And the third component, the product of E4 and E5, represents the difference in the worker's expected future marginal firm surplus if her nominal wage is uncut (and thus she is persistently more likely to stay at the firm) versus her expected future marginal surplus after having experienced a nominal cut (in which case she is persistently more likely to quit). More formally, this is the probability that Worker A stays at the firm after her nominal wage is cut, multiplied by the difference in her expected future surplus if her wage remains uncut versus if she experiences a nominal cut in the current period. Notice that both the second and third components are largely determined by the value of δ, the worker's persistent increase in her quit rate once she has ever experienced a nominal wage cut. If δ is significantly greater than one, then the E5 component will be quite large since much of a worker's surplus comes from her expected future stream of profits. If a nominal wage cut significantly increases a worker's quit rate, then the shorter expected job duration causes a large fall in the firm's expected future surplus.

The right-hand side of this inequality represents the incremental surplus of retaining Worker B, but having to cut Worker A's nominal wage in order to do so. Again, there are three components to this surplus. The first component, E6, is the current-period earnings that is generated when both Worker A and Worker B are employed (at their constrained wages which require nominal cuts). The second component, the product of E7 and E8, is the probability that only Worker B stays at the firm into the next period after both workers' nominal wages are cut, multiplied by the expected future surplus of retaining only Worker B after a nominal wage cut. And the third component, the product of E9 and E10, is the probability that both Worker A and Worker B remain after their nominal wages are cut times Worker B's expected marginal firm surplus in the next period, conditional on both workers receiving nominal cuts today.

This decomposition shows that there are two key factors determining whether laying off Worker B is the firm's optimal decision. The first factor is the magnitude of δ, the degree to which workers increase their quit rates when they are exposed to a nominal wage cut. As δ grows, the left-hand side of the inequality condition grows. This comes from the effect of a nominal cut on both: i) workers' current period quit rates (E2), and ii) the expected future firm surplus (E5). The second factor is the gap in the workers' marginal firm surpluses. As the gap in their marginal firm surpluses grows, the incremental value of keeping Worker B falls. In particular, components E3 and E5 become large relative to E8 and E10. Thus, when aggregate productivity $A_t z_{kt}$ lies between T2 to T3, DNWR will be more likely to cause a firm to lay off a positive surplus worker when: i) workers' quit rates rise strongly in response to nominal wage cuts, and ii) there are large differences in the marginal firm surpluses generated by different workers.

B.8.1.2 2-Worker Firm: Laying Off a Worker When a Nominal Cut Would Suffice

It is also important to explore whether a firm in this model would ever choose to destroy a job when i) the job would have a positive surplus at a lower nominal wage, and ii) the firm's earnings would be sufficient to avoid the current-period earnings constraint. Consider the same two-worker firm, but now in the case when the aggregate productivity $A_t z_{kt}$ lies between T3 and T4. Within this range of aggregate shocks, the firm must either cut the nominal wage of at least one worker (since $A_t z_{kt}$ <T4) or lay off a worker. The Barro critique argues that the firm should not lay off Worker B if the job would generate a non-negative surplus for both the worker and the firm after a nominal wage cut sufficient to relax the current-period earnings constraint.

To see in which cases the firm chooses to lay off the worker, I compare the surplus from laying off the worker against the surplus from setting the worker's wage at the constrained surplus-maximizing wage. The firm optimally chooses to layoff Worker B if the following inequality condition holds:

$$\underbrace{S_{kt}\left(w_{At}^{c|A}, \Delta_{At}^{-}=0, h_{Bt}=0\right)}_{\text{Surplus From Only Worker A - No Cut}} > \underbrace{S_{kt}\left(w_{At}^{c|AB}, \Delta_{At}^{-}=0, w_{Bt}^{c|AB}, \Delta_{Bt}^{-}=1\right)}_{\text{Surplus From Both Worker A \& B - Only B Cut}} \tag{26}$$

Using the definition of a worker's marginal surplus, the right-hand side of this condition can be written as:

$$S_{kt}\left(w_{At}^{c|AB}, \Delta_{At}^{-}=0, w_{Bt}^{c|AB}, \Delta_{Bt}^{-}=1\right) = S_{kt}\left(w_{At}^{c|A}, \Delta_{At}^{-}=0, h_{Bt}=0\right) + S_{kt}^{+B}\left(w_{Bt}^{c|AB}\right) \tag{27}$$

Combining the condition in Equation 26 with this alternative definition of the right-hand side shows that the firm optimally chooses to layoff Worker B only if the worker's marginal surplus at the constrained surplus-maximizing wage, $w_{Bt}^{c|AB}$, is negative, i.e.

$$0 > S^{+B}\left(w_{Bt}^{c|AB}; \Theta_t, \Omega_{kt}\right) \tag{28}$$

If the marginal current-period earnings generated by the worker at his constrained surplus-maximizing wage is greater than zero ($p_{kt}^{+i}\left(w_{it}^{c|AB}\right) \geq 0$), then it is necessarily the case that the worker's marginal surplus is non-negative (since the firm can costlessly lay off the worker in the next period). Thus, in this case, the firm retains the worker at his constrained surplus-maximizing wage.

It is more complicated if the marginal current-period earnings generated by the worker at his constrained surplus-maximizing wage remains negative (which could occur if the coworker is generating sufficient positive earnings to offset these negative marginal earnings). Now the discounted expected value of Worker B's future surplus at the constrained surplus-maximizing wage must be greater than the negative marginal current-period earnings he generates. Although this requirement was met at the worker's surplus-maximizing wage, there are two reasons why this may no longer hold at the lower, constrained surplus-maximizing wage. First, the worker's current-period quit rate rises as the worker's real wage declines. And second, the worker's quit rate rises persistently because of the nominal wage cut. In both cases, it is possible for the increased quit rate to cause the discounted expected value of the worker's future surplus to fall sufficiently that the worker no longer generates a positive marginal surplus at the constrained surplus-maximizing wage. Note that, even though DNWR causes this layoff, the layoff would be efficient because the worker's quit response to a nominal wage cut actually flips the marginal surplus of the job from positive to negative.

B.8.2 Constrained Layoff / Wage Setting Decisions at a Many-Worker Firm

The above description of a two-worker firm's optimal layoff and wage setting decision process when the current-period earnings constraint binds is also relevant for a multi-worker firm. When the current-period earnings constraint binds a multi-worker firm, the firm will identify the set of second-best layoff and wage setting decisions using the iterative process described in Section B.8. In any given iteration, the firm is essentially acting like a two-worker firm. The two workers are i) worker i with the layoff ratio metric, $\ell(h_{it} = 0)$ closest to zero (and thus worker i is the first employee to be laid off if any employees are laid off), and ii) worker j with the wage cut ratio metric $\ell(h_{jt} = 0, w_{jt})$ closest to zero (and thus the first worker to receive a wage cut if any worker receives a wage cut). The firm chooses between these two second-best options in a similar fashion as in the two-worker firm example. After having determined this second-best decision, the firm then begins the next iteration with a new two-worker pair (one of whom it carries over from the previous iteration).

B.9 Proofs of Optimal Firm Layoff and Wage Setting Propositions

B.9.1 Proposition 1: DNWR May Cause Layoffs of Positive-Surplus Workers

Proposition 1. *Given a sufficiently large negative shock to either aggregate or firm-specific productivity, DNWR (in the form of workers' quit response to nominal wage cuts) may cause a firm to optimally layoff a positive-surplus worker instead of retaining the worker at a lower nominal wage.*

Consider two workers such that Worker A has the $\ell(h_{it} = 1, w_{it})$ closest to zero and Worker B has the $\ell(h_{it} = 0)$ closest to zero of all the workers in the firm's optimal retained worker set (and thus they are the marginal workers for a wage change and layoff, respectively).

Assumption 1. *The current-period earnings constraint binds because $A_t z_{kt}$ is in the following range:*

$$A_t z_{kt} \in \left[\max\left[\frac{a + \frac{w_{At-1}}{\pi_t} + c^n}{x^A_{kt}}, \frac{a + 2\underline{w} + 2c^n}{2\bar{x}^{AB}_{kt}} \right], \frac{a + \frac{w_{At-1}}{\pi_t} + \underline{w} + 2c^n}{2\bar{x}^{AB}_{kt}} \right)$$

Assumption 2. *The high-surplus worker's optimal constrained wage when the low-surplus worker is laid off is at or above their previous nominal wage.*

$$w^{c|A}_{At} \geq \frac{w_{At-1}}{\pi_t}$$

Assumption 3. *Both workers generate a positive marginal surplus*

$$S^{+A}_{kt}\left(w^{c|AB}_{At}\right) > S^{+B}_{kt}\left(w^{c|AB}_{Bt}\right) > 0$$

Proof:

1. Given Assumption 1, the firm can relax the current-period earnings constraint by either laying off the low-surplus worker or cutting the nominal wage of both workers, but cutting the nominal wage of just the low-surplus worker is not sufficient.

    (a) Laying off the low-surplus worker would relax the current-period earnings constraint since

$$A_t z_{kt} x_{kt}^A - \frac{w_{At-1}}{\pi_t} - c^n \geq a$$

(b) Cutting the nominal wage of both the low and high-surplus workers is sufficient to relax the current-period earnings constraint.

$$2A_t z_{kt} \bar{x}_{kt}^{AB} - 2\underline{w} - 2c^n \geq a$$

(c) Cutting the nominal wage of just the low-surplus worker is not sufficient to relax the current-period earnings constraint.

$$2A_t z_{kt} \bar{x}_{kt}^{AB} - \frac{w_{At-1}}{\pi_t} - \underline{w} - 2c^n < a$$

2. Thus, laying off the low-surplus Worker B is feasible (since it relaxes the current-period earnings constraint). Furthermore, per Equation 24, it is the optimal second-best decision for the firm if:

$$S_{kt}\left(w_{At}^{c|A}, \Delta_{At}^{-} = 0, h_{Bt} = 0\right) > S_{kt}\left(w_{At}^{c|AB}, \Delta_{At}^{-} = 1, w_{Bt}^{c|AB}, \Delta_{Bt}^{-} = 1\right) \tag{29}$$

3. Given Assumptions 1-3, if the inequality condition 29 holds, then the firm optimally chooses to layoff Worker B even though he generates a positive marginal surplus.

B.9.2 Proposition 2: Layoff Probability Increasing in Quit Response to Nominal Cut

Proposition 2. *When the firm is bound by the current-period earnings constraint, the likelihood that DNWR causes a firm to layoff a positive-surplus worker is increasing in δ, the magnitude of the persistent rise in a worker's quit rate after a nominal wage cut.*

Assumptions: Make the same three Assumptions 1-3.

Proof: Consider the expanded inequality condition in Equation B.8.1.1 in which the firm's optimal decision is to layoff Worker B. If the rise in a worker's quit rate after a nominal wage cut, δ, increases in magnitude, then this affects no components of the RHS and only two components of the LHS of the inequality condition. Specifically, on the LHS of the inequality condition a change in δ affects:

1. E2: The component representing the change in the quit rate from a nominal wage cut increases in magnitude (remaining positive). Since this is multiplied by the marginal surplus of Worker

A without Worker B, E3, which is necessarily positive, the effect of a rise in δ is a rise in the product of E2 and E3.

2. E5: The component representing the change in the future surplus generated by Worker A increases. This follows from:

   (a) The expected future surplus generated from just Worker A absent a nominal wage cut, $\mathbb{E}\left[S_{t+1}\left(w_{At}^{c|AB}, cut\right), -\right]$, is unchanged.

   (b) The expected future surplus generated from just Worker A after a nominal wage cut, $\mathbb{E}\left[S_{t+1}\left(w_{At}^{c|AB}, cut\right), -\right]$, can be represented as the profits generated in each potential future state of the world times the probability that the state occurs times the probability that the worker has not yet quit.

$$\mathbb{E}\left[S_{t+1}\left(w_{At}^{c|AB}, cut\right), -\right] = \sum_{u=1}^{\infty} \sum_{s_{t+u} \in S} Pr(s_{t+u}) \Pi^*_{At+u} \prod_{v=1}^{u} (1 - \delta q(s_{t+v})) \tag{30}$$

   (c) The fact that Worker A generates a positive marginal surplus implies that

$$\sum_{u=1}^{\infty} \sum_{s_{t+u} \in S} Pr(s_{t+u}) \Pi^*_{At+u} \prod_{v=1}^{u} q(s_{t+v}) > 0 \tag{31}$$

   (d) Given that $\delta > 1$, it will also be the case that

$$\sum_{u=1}^{\infty} \delta^u \sum_{s_{t+u} \in S} Pr(s_{t+u}) \Pi^*_{At+u} \prod_{v=1}^{u} q(s_{t+v}) > 0 \tag{32}$$

   (e) Thus, the change in Worker A's future expected surplus given an increase in δ is necessarily negative.

   (f) Given that the expected future surplus absent a nominal cut is unchanged but the expected future surplus given a nominal cut falls, the E5 component increases. Since E4 is necessarily positive, the product of E4 and E5 increases.

3. Thus, the effect of an increase in the magnitude of a worker's quit response to a nominal wage cut, δ, is to increase the magnitude of the LHS of the inequality in Equation B.8.1.1 without changing the RHS. This implies that the likelihood that the firm's optimal second-best decision is to layoff the low-surplus Worker B rises.

B.9.3 Proposition 3: Layoff Probability Increasing in Workers' Marginal Surplus Gap

Proposition 3. *When the firm is bound by the current-period earnings constraint, the likelihood that DNWR causes a firm to layoff a positive-surplus worker is increasing in the difference in productivity between the worker at risk of layoff and the coworker at risk of a nominal wage cut.*

Assumptions: Make the same three Assumptions 1-3.

Proof: Consider the expanded inequality condition in Equation B.8.1.1 which, if it holds, necessarily implies that the firm's optimal decision is to layoff Worker B. All else equal, if the productivity of Worker A remains the same, but the productivity of Worker B falls, then this results in an increase in the difference in marginal surpluses generated by the two workers. In such a case,

1. The LHS of Equation B.8.1.1 is unchanged since all values here only depend on the surplus generated when Worker A works alone, and thus is unaffected by Worker B's productivity.

2. The RHS of the inequality condition of Equation B.8.1.1 necessarily falls since E6, E8, and E10 are each increasing in worker productivity (and thus each of these components falls when Worker B's productivity falls).

3. If the LHS is unchanged and the RHS falls, then the likelihood that the firm's optimal second-best decision is to layoff Worker B rises.

B.9.4 Proposition 4: Model is Robust to Barro Critique

Proposition 4. *When the firm is bound by the current-period earnings constraint, if i) cutting the nominal wage of a single worker is sufficient to relax the current-period earnings constraint, and ii) the worker generates a positive marginal surplus at this lower nominal wage, then the firm always chooses to cut the worker's wage instead of laying off the worker.*

Consider the case where that Worker B has the $\ell(h_{it} = 1, w_{it})$ closest to zero and has the $\ell(h_{it} = 0)$ closest to zero of all the workers in the firm's optimal retained worker set (and thus Worker B is both the marginal worker for a wage change and layoff).

Assumption 4. *The current-period earnings constraint binds because $A_t z_{kt}$ is in the following range:*

$$A_t z_{kt} \in \left[\frac{a + \frac{w_{At-1}}{\pi_t} + \underline{w} + 2c^n}{2\bar{x}_{kt}^{AB}}, \frac{a + \frac{w_{At-1}}{\pi_t} + \frac{w_{Bt-1}}{\pi_t} + 2c^n}{2\bar{x}_{kt}^{AB}} \right)$$

Assumption 5. *The worker generates a positive marginal surplus at their constrained surplus-maximizing wage,* $w_{At}^{c|AB}$.

$$S_{kt}^{+B}\left(w_{Bt}^{c|AB};\Theta_t,\Omega_{kt}\right)>0$$

Proof:

1. Per Equation 26, the firm optimally chooses to layoff Worker B if

$$S_{kt}\left(w_{At}^{c|A},\Delta_{At}^{-}=0,h_{Bt}=0\right)>S_{kt}\left(w_{At}^{c|AB},\Delta_{At}^{-}=0,w_{Bt}^{c|AB},\Delta_{Bt}^{-}=1\right)$$

2. Given the definition of marginal surplus, the RHS of this equation can be rewritten as:

$$S_{kt}\left(w_{At}^{c|AB},\Delta_{At}^{-}=0,w_{Bt}^{c|AB},\Delta_{Bt}^{-}=1\right)=S_{kt}\left(w_{At}^{c|A},\Delta_{At}^{-}=0,h_{Bt}=0\right)+S_{kt}^{+B}\left(w_{Bt}^{c|AB}\right)$$

3. Subtracting $S_{kt}\left(w_{At}^{c|A},\Delta_{At}^{-}=0,h_{Bt}=0\right)$ from both sides yields:

$$0>S^{+B}\left(w_{Bt}^{c|AB};\Theta_t,\Omega_{kt}\right)$$

4. By Assumption 5, this necessarily holds. Furthermore, by Assumption 4, this nominal wage cut is sufficient to relax the firm's current-period earnings constraint. Thus, the firm always chooses to cut the worker's nominal wage.