Downward Nominal Wage Rigidity and Job Destruction*

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Abstract

This paper provides quasi-experimental evidence that downward nominal wage rigidity causes firms to destroy jobs and that this effect is empirically relevant for the macroeconomy. Given the unanticipated nature of the financial collapse in Q3 of 2008, differences across firms in their patterns of seasonal nominal wage adjustment generated heterogeneity in firms' exposure to downward nominal wage rigidity in Q4 of 2008. To identify these seasonal patterns, I develop a set of machine learning tools that I apply to longitudinal data on individual U.S. firms. I find that exposure to downward nominal wage rigidity generated by firms' seasonal wage adjustment patterns accounts for 23% of the spike in aggregate job destruction that occurred in Q4 of 2008. Since this empirical finding runs counter to the assumption in many macro models that downward wage rigidity does not cause job destruction, I develop a model wherein downward nominal wage rigidity causes inefficient job destruction, while ensuring, à la Barro (1977), that workers and firms realize mutually beneficial nominal wage cuts.

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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 that within a firm, employees' nominal wage raises tend to be synchronized to occur in the same calendar quarter year-over-year, but the calendar timing of these synchronized wage raises differs across firms. The quasi-experiment then identifies exogenous variation in firms' exposure to DNWR from 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 September 2008 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 wages to

[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.

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 the 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 developing a set of machine learning methods that extract 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). 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. This estimate is only a lower-bound of DNWR's effect on job destruction in 2008:Q4 since it does not capture the fact that the Q4-raising firms had less, but not no exposure to DNWR.

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.

I present a model in which DNWR can cause firms to destroy positive surplus jobs, despite i) firms being able to pay workers different wages, and ii) workers and firms not forgoing mutually advantageous nominal wage cuts. When a firm experiences a large enough negative shock (to either demand or productivity), fixed labor costs combine with a financial constraint to force the firm to choose between laying off a positive-surplus worker or cutting the nominal wages of both the worker and a coworker who generates a larger firm surplus.

The model incorporates DNWR by assuming that workers respond to nominal wage cuts with a

[3] Labor search-and-matching models incorporating wage rigidity 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 since workers and firms must ignore mutually beneficial nominal wage cuts.

large and persistent increase in their quit rates, which I show is consistent with workers' empirical quit response to nominal wage cuts. This quit response means that after a nominal wage cut, the expected future surplus generated by a worker falls significantly due to the worker's shorter expected job duration. If the gap in workers' surpluses is large enough, then the fall in expected future surplus from cutting the wages of both the low-surplus worker and the high-surplus coworker may be larger than the certain loss of future surplus from laying off the low-surplus worker. In such cases, the quit response of a high-surplus coworker to a nominal wage cut causes the firm optimally to lay off a low, but positive-surplus worker.

1.1 Contributions to the Literature

This paper contributes to both the empirical and theoretical literature on the link between DNWR and employment. It also makes a methodological contribution to wage measurement in administrative employer-employee linked data sets.

The largest 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 nominal 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 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

[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 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 the 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.

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 a different set of identifying assumptions than the Kurmann and McEntarfer (2019) and Ehrlich and Montes (2019) studies. The 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 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 findings 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

[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 similarly construct 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 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 they 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.

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)).[9]

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 wages are rigid but they adjust to preserve positive-surplus jobs in a search-and-matching model, then the wage rigidity affects employment without violating Barro's critique. In such a case, the wage rigidity affects the job creation margin through its effect on firm surplus, but the wage rigidity does not affect job destruction. Importantly, Hall 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.[10] 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)).[11] 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

[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)

[9]I also find evidence counter to a key implication of the wage adjustment cost model that the adjustment costs should suppress small nominal wage changes. See Appendix F for the full details.

[10]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)).

[11]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.

job destruction margin.[12]

This paper makes a first step towards reconciling my empirical finding with the theoretical literature by presenting a model wherein downward nominal wage rigidity causes firms to destroy positive-surplus jobs, while ensuring that workers and firms do not forgo mutually beneficial nominal wage cuts (and thus the model is robust to the Barro critique). I show empirical evidence consistent with my models two key assumptions: i) that workers persistently increase their quit rates after a nominal wage cut, and ii) the job destruction rate responds more strongly to DNWR exposure at firms that are more constrained by short-term falls in revenue (and thus profits). Additionally, I use the quasi-experimental evidence from the Great Recession to empirically test one of the model predictions. Specifically, I find, as predicted by the model, that exposure to DNWR generates more job destruction at firms with greater dispersion in worker productivity.

In addition to empirical and theoretical contributions, this paper also makes a methodological contribution. The quasi-experiment requires a measure of persistent changes to workers' nominal compensation. Unfortunately, the LEHD only reports workers' quarterly nominal earnings, for which transitory changes account for approximately 85% of the observed quarter-over-quarter fluctuations in quarterly earnings. To overcome this limitation of the LEHD, I develop a set of machine learning methods that extract persistent changes in workers' unobserved base wages from the workers' observed quarterly earnings.

1.2 Outline of the Paper

This paper proceeds as follows. Section 2 describes the LEHD data set and the machine learning methods I develop for extracting workers' unobserved persistent nominal wage changes from their observed quarterly nominal earnings. Section 3 presents evidence regarding the three wage adjustment patterns essential to the quasi-experiment: that i) the estimated persistent wage changes exhibit downward nominal wage rigidity; ii) workers receive nominal raises according to an annual schedule; and iii) workers' annual raise schedules are synchronized within firms. Section 4 presents the quasi-experiment's identification strategy and empirical results demonstrating that downward nominal wage rigidity causes job destruction. Section 5 lays out a multi-worker firm model in

[12] 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 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.

which DNWR affects employment through the job destruction margin while ensuring that workers and firms realize mutually advantageous nominal wage cuts. Lastly, Section 6 concludes with a summary of the paper and a discussion of policy implications.

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 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.[13] I chose these thirty states because there are no gaps in reported quarterly earnings for any of these states over the sample period. I also employ a secondary sample that is a 10% random sample of SEINs from the four states that also reported quarterly hours paid during the period from 2011:Q1 to 2018:Q1 (MN, OR, RI, and WA). I use this secondary sample when examining patterns of wage adjustment because the hours-paid data helps quantify the degree of measurement error in my estimates of workers' nominal wages.

For both the primary and secondary samples, when I examine labor market outcomes for workers,

[13] 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.

such as employment-to-nonemployment (EN) or employer-to-employer (EE) transitions, I identify these transitions using 100% of the firms in the 30 states. I then restrict my subsequent analysis to workers at the 10% random sample of firms. Using 100% of firms ensures that these labor market transitions are accurately identified, since otherwise some EE transitions would incorrectly be categorized as EN transitions.

2.2 Post-Lasso Estimation of Persistent Nominal Wage Changes

The key drawback of the LEHD data for my proposed quasi-experiment is that the LEHD only reports workers' quarterly earnings - which can vary not only due to changes in the base wage but also due to fluctuations in overtime pay, bonuses, payday weeks, and average weekly hours paid. Many of these components of quarterly earnings are quite transitory. The identification strategy of the quasi-experiment, however, relies on persistent changes in workers' compensation. Specifically, the quasi-experiment identifies a firm as having greater exposure to DNWR if, when an unanticipated negative shock occurs, the firm has a higher start-of-quarter wage bill because the firm historically tended to increase its employees' compensation in the calendar quarter immediately before the shock. This assumes that some changes in workers' compensation persist into subsequent periods. If transitory components (such as overtime pay, payday weeks, hours paid, and bonuses) generate most of the fluctuations in the compensation measure, then the historical fluctuations in workers' compensation will be a weak signal for firms' exposure to DNWR in future periods. I estimate that the transitory components account for approximately 86% of the fluctuations in the quarter-over-quarter log hourly earnings (see Appendix A).

To overcome this limitation of the LEHD, I develop a set of machine learning methods that identify unobserved persistent changes in each worker's base wages from their observed quarterly earnings. First, I note that variation in the number of paydays due to a worker's payday schedule can generate fluctuations of ±15% in the worker's quarter-over-quarter earnings. Although earnings fluctuations generated by these changes in the number of paydays appear as noise at an individual level, these payday-related earnings changes can be identified because they are large and common to many workers at the firm. Appendix B describes the set of clustering algorithms that I use to identify each firm's payroll schedule(s) and each worker's number of payday weeks in any given period. This estimate of each worker's number of payday weeks in any given period allows me to control for the transitory fluctuations in quarterly earnings generated by the worker's payday schedule.

Although I am interested in identifying persistent changes in each worker's base wage, I reframe this problem as one of identifying structural breaks in the worker's observed earnings. Each persistent wage change is equivalent to a structural break in the time series of a worker's log earnings over the worker's job spell at a firm. By reframing the problem, I can use the methods developed by the extensive literature on identifying structural breaks in time-series data.[14]

Because I must separately identify the persistent wage changes for tens of millions of workers, the scalability of the post-Lasso procedure makes it a particularly attractive method for identifying structural breaks in the worker's log base wage (i.e. persistent wage changes).[15] The post-Lasso estimation procedure allows for persistent wage changes to occur in any period of a worker's employment history. The procedure minimizes an objective function that has two components. The first component optimizes the model fit by choosing the wage change estimates that minimize the Euclidean distance between the predicted persistent log wage history and the observed log earnings history (this is the same as standard OLS minimization of the sum of squared residuals). The second component addresses model over-fitting by including a penalty parameter for the sum of the absolute value of the estimated log wage changes. This penalty parameter causes the post-Lasso procedure to set the persistent wage change estimates to zero for many periods. This is consistent with workers not receiving base wage changes every quarter. Appendix C lays out the full details of the post-Lasso estimation procedure and evaluates the quality of the persistent wage change estimates.

The resulting post-Lasso estimated persistent base wages exhibit patterns very similar to the base wage change patterns identified by Grigsby, Hurst, and Yildirmaz ("GHY" 2019) using the ADP administrative payroll records. Table 1 shows that the post-Lasso procedure estimates that 84.9% of workers experience no quarter-over-quarter persistent wage change, 13.6% receive a nominal raise, and 1.6% receive a nominal cut. Examining workers at firms with 50+ employees (which tend to raise workers' wages more often than smaller firms), GHY determine that 80.6% of workers experience quarter-over-quarter nominal wage freezes, 18.5% receive a nominal raise, and 0.9% receive a nominal cut.

[14] See Casini and Perron (forthcoming) for a review of recent advances in the literature on structural break identification. In the DNWR literature, Gottschalk (2005) addressed measurement error in survey respondents' reported base wages by applying structural break identification procedures proposed by Bai and Perron (1998). Barattieri, Basu and Gottschalk (2014) further extended Gottschalk's method to account for Type I and Type II error in the identification of nominal wage changes.

[15] Notable studies that use Lasso estimation for structural break identification include Harchaoui and Lévy-Leduc (2010) and Ciuperca (2014).

Table 1: Comparison of Measures of Nominal Compensation Changes

Quarter-over-Quarter Change in Log Nominal Compensation Measure					
Compensation Measure	Data Source	Period	Raise	Freeze	Cut
Hourly Earnings					
Raw Data	LEHD 4 States	2011-2018	55.5%	5.0%	39.5%
No Hours Rounding	LEHD 4 States	2011-2018	46.4%	22.2%	31.4%
No Bonus + Rounding	LEHD 4 States	2011-2018	36.9%	45.3%	17.8%
Persistent Base Wage	Post-Lasso LEHD 4 States	2011-2018	13.6%	84.9%	1.6%
Base Wage	ADP Payroll (GHY 2019)	2008-2016	18.5%	80.6%	0.9%
Base Wage	SIPP Survey (BBG 2014)	1996-2000		78.4-84.8%	

Note: The Hourly Earnings measures are the quarter-over-quarter change in log hourly earnings. The *Raw Data* is calculated from the LEHD quarterly earnings and hours-paid data with no adjustments. The *No Hours Rounding* sets to zero any change that could be accounted for by hours rounding. The *No Bonus + Rounding* also smoothes single-period bonuses in addition to correcting for hours rounding errors. See Appendix D for details on how each hourly earnings measure is constructed. *Persistent Base Wage* refers to the post-Lasso estimation results when controlling for the log estimated payday weeks using the secondary sample. Freezes are periods where the Lasso estimation procedure sets the wage change estimate to zero. The *Base Wage* estimates from ADP Payroll Records are from Grigsby, Hurst and Yildirmaz (2019). GHY examine payroll records from 2008-2017 for firms with 50+ workers who use ADP Payroll Services, reweighting the observations to match the firm characteristics of 50+ worker firms in the Census Bureau's Longitudinal Business Database. The *Base Wage* estimates from the SIPP Survey are from Barattieri, Basu and Gottschalk (2014), which use linked SIPP records from 1996-2000. To address measurement error in self-reported wages, the BBG study employs the structural break identification procedure proposed by Gottschalk (2005) and then further corrects for the frequency of Type I and Type II errors. U.S. Census Bureau Disclosure Review Board bypass numbers DRB-B0037-CED-20190327 and CBDRB-2018-CDAR-061.

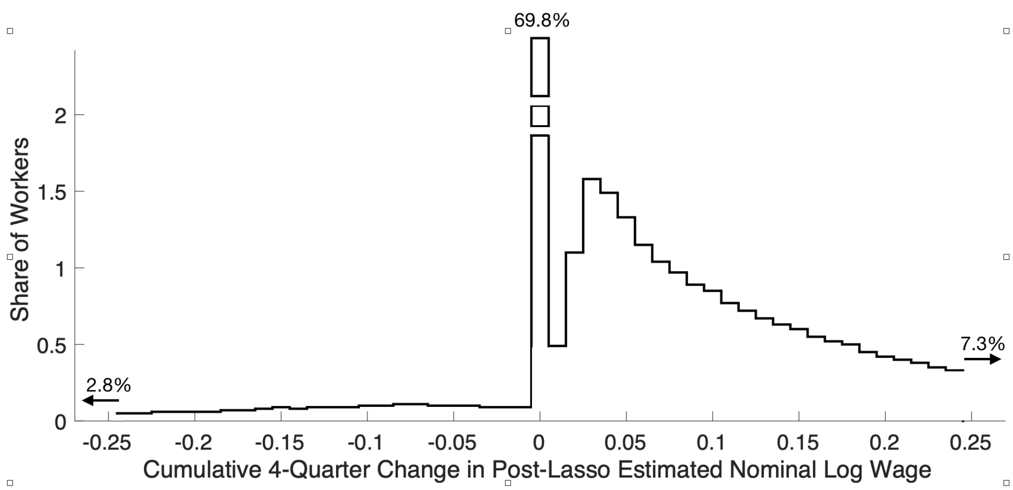
3 Patterns of Nominal Wage Adjustment

This section provides evidence regarding the three patterns of nominal wage adjustment critical for the quasi-experiment described in Section 4: i) the post-Lasso estimated persistent wage changes exhibit downward nominal wage rigidity, ii) workers' nominal raises follow a Taylor-like pattern, with the probability of a wage raise spiking every four quarters, and iii) the timing of workers' annual raises are synchronized within the firm.

3.1 Wages Exhibit Downward Nominal Rigidity

Consistent with previous studies,[16] 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



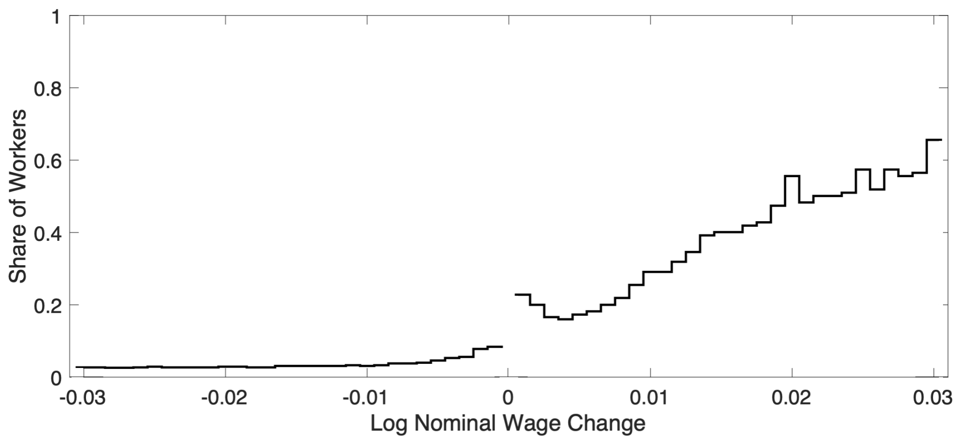
Notes: Histogram of workers' four-quarter cumulative change in their post-Lasso estimated persistent log nominal wage. Estimated from the Primary Sample after restricting to workers with at least five full quarters of non-zero earnings. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0037-CED-20190327.

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 nominal change. I more formally test for the presence of DNWR in two ways. First, in Appendix E, I estimate a series of regression discontinuity models that check for a discontinuity in the dis-

[16] 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.

tribution 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 F, 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

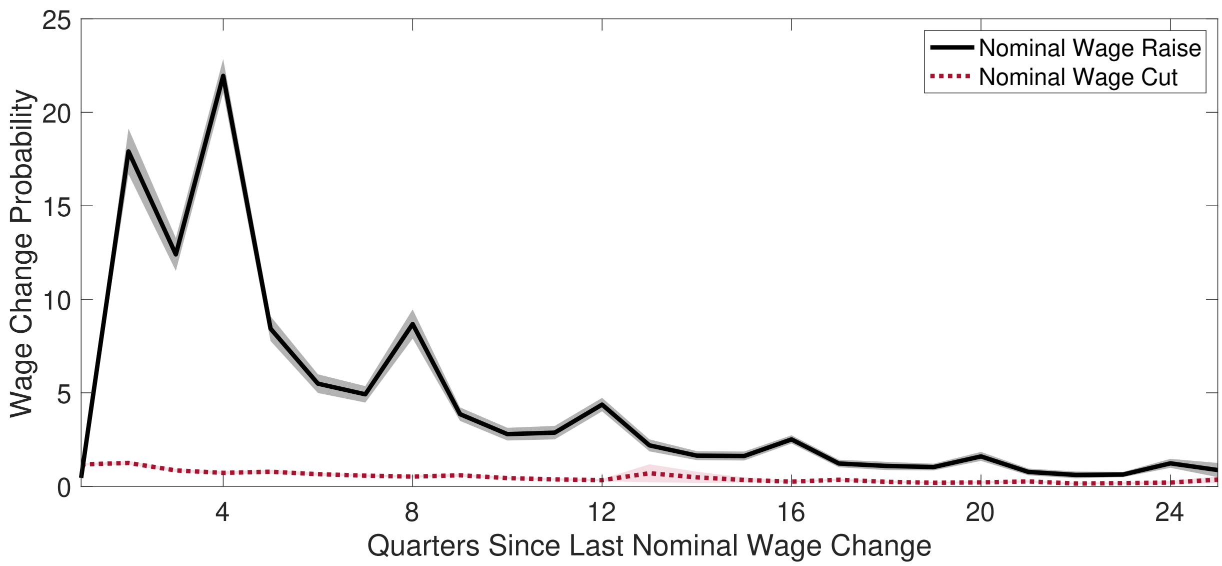


Notes: Frequency of four-quarter cumulative change in workers' post-Lasso estimated persistent log nominal wage grouped into 0.1 log point change bins. Sample is restricted to workers with at least five full-quarters of non-zero earnings. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0037-CED-20190327.

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.[17] 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, with the plurality of workers having Q3 as their typical raise quarter (26%), the fewest having Q2 (10%), approximately the same share in Q1 and Q4 (16% and 15% respectively), and 12.6% of workers having 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.[18]

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 the worker receives a nominal wage raise; 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 2 indicate that the probability that a worker receives a 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

[17] 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 $p = 0.25$).

[18] 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.

implies that workers' annual nominal raise schedules are strongly coordinated within the firm.

Table 2: 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. **Bold** and *Italics* indicate statistical significance at the 0.1% and 1.0% levels respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

4 Quasi-Experimental Evidence DNWR Causes Job Destruction

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 productivity shock, I require variation in the firms' exposure to DNWR that is exogenous 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 will 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 each 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 firm's 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 which 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 the rate of job destruction at firms which 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. Otherwise, if firms can anticipate the upcoming negative shock, then 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 negative shock relative to the firms' typical raise quarters.

Second, the quasi-experiment requires that absent these differences in their typical raise quarters, the job destruction rates at the 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. Otherwise, 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 the question whether exposure to DNWR causes firms to destroy jobs at higher rates, I use the identification strategy described above, focusing on the job destruction rate of firms in

2008:Q4, immediately following Lehman Brothers' bankruptcy in September 2008 and the ensuing financial collapse. I look to the Federal Reserve Bank of Philadelphia's Federal Reserve Bank of Philadelphia (2008:Q4) for evidence that the financial collapse in September 2008 qualifies as a large, unanticipated negative aggregate shock. Between August 8th and November 10th of 2008, professional economic forecasters' predictions for the annualized real GDP growth rate in 2008:Q4 fell 3.8 percentage points, from +0.7% to -2.9%. Their newfound pessimism also extended into longer-term forecasts, as their predictions for real GDP growth in 2009 fell from +1.5% to -0.2%. Given the dramatic downward revisions of professional forecasters' predictions, the financial collapse in 2008:Q3 arguably qualifies as a large, unanticipated negative aggregate shock.

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 seasonality and industry-by-time fixed effects. Table 3 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 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

Table 3: 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 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 would have been 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 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,[19] $D_{kt}^{2008:Q4}$ 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 4 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 employment weighting). 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.

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

[19] 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 4: 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. **Bold** and *Italics* indicate statistical significance at the 0.1% and 1.0% levels respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

firm's exposure to the financial collapse. This concern is reinforced by the typical raise quarters of firms in certain industries appearing 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 3 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-raiser firms responded more strongly than Q4-raiser firms to similar magnitude revenue changes. 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 real revenue DHS change if it is positive (and zero otherwise), and the second column is the absolute value of the revenue DHS 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 the negative revenue change, since this indicates whether Q2-raising firms had stronger responses than Q4-raising firms to similar magnitude 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 5 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 4 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 (rising approximately 0.16 percentage points for every 1% fall in revenue).

Table 5: 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. **Bold** and *Italics* indicate statistical significance at the 0.1% and 1.0% levels respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

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 if the firms experienced positive year-over-year revenue change (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, in 2008:Q4, differences in the calendar quarter in which firms historically tended to raise their workers' wages caused the firms' job destruction rates to respond differently to similar magnitude shocks. See Appendix G.1 for a detailed discussion of the endogeneity issues related to the use of year-over-year revenue changes in this difference-in-differences framework and the expected sign of the bias.

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 when an unanticipated negative productivity shock occurs 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 firms' 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)[20] and w_{ikt-1}^r is the real wage of the worker in period $t-1$ (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

[20] 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.

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 G.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). Thus, in 2008:Q4, I construct three instrumental variables for each firm: r^{Q}_{kt-1}, r^{Q}_{kt-2}, and r^{Q}_{kt-3} that correspond to the historical employment-weighted share of nominal raises 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^{q}_{k} where q indicates the calendar quarter). Then I calculate the historical employment-weighted share of nominal raises for each calendar quarter q as:

$$s^{q}_{k} = \frac{p^{q}_{k}}{\sum_{a=1}^{4} p^{a}_{k}} \tag{6}$$

These historical raise share measures for the four calendar quarters sum to one for every firm.[21] 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: $r^{Q}_{kt-1} = s^{3}_{k}$, $r^{Q}_{kt-2} = s^{2}_{k}$, $r^{Q}_{kt-3} = s^{1}_{k}$.

Using these instrumental variables, I estimate the following first-stage relationships for the en-

[21] 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.

dogenous 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 6 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.[22] There does not appear to be a weak instruments problem for either of the endogenous explanatory variables.[23]

Table 7 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 percentage points in 2008:Q4 for every 1% that firms' start-of-quarter real wage bill were above their four-quarter average real wage bill due to the firms' historical raise schedules.[24]

To give a sense of the aggregate magnitude of this estimate, I consider the counterfactual scenario where all firms had the nominal wage flexibility of Q4-raising firms in 2008:Q4. This is equivalent to using the coefficient estimate from the second-stage of the IV regression to calculate the job destruction rate at firms if all the historical raise share values were set to zero (and thus the firm was a Q4 raiser). When I do this simple adjustment for the exposure to DNWR generated by the annual staggering of firm's nominal raise schedules, I find the job destruction rate would have risen 23% less in 2008:Q4 (which was also the quarter with the most job destruction since 1993, when job destruction was first measured by the Quarterly Workforce Indicators). I should note that this estimate serves as a lower bound of the effect of DNWR on job destruction in 2008:Q4 since even

[22]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.

[23]The HAC-robust F statistics for W_{kt} and $d_{kt}^{2008:Q4}W_{kt}$ are 4444 and 33.5 respectively. Given that there are two endogenous regressors, the heteroskedasticity robust Kleibergen-Paap statistic of of 33.4, for the joint weak instrument test, is also relevant.

[24]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 G.3 for the alternative second-stage regression results.

Table 6: 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_{kt-x}^{Q})				
1 Quarter Lag	**4.06**	2x10^{-4}	**3.91**	*2.2x*10^{-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. **Bold** and *Italics* indicate statistical significance at the 0.1% and 1.0% levels respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0073-CED-20190910.

the less-exposed Q4-raising firms (which serve as the baseline comparison group) had exposure to DNWR.[25]

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

[25]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.

Table 7: 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. **Bold** and *Italics* indicate statistical significance at the 0.1% and 1.0% levels, respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0073-CED-20190910.

more readily available. Similarly, the aggregate dynamics resulting from DNWR may be very different 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: Greater 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 layoffs by the firm and slowdowns in the hiring of replacements for retiring and quitting workers. Thus, I evaluate whether one or both channels are driving 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 but change the outcome variable to be the firms' hiring rates or layoff rates.[26] The results in Table 8 show that the layoff rate at

[26]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)).

the Q2-raising firms rose 1.44 percentage points. This estimate is statistically significant and very similar in magnitude to the 1.30 percentage point rise in the job destruction rate at Q2-raising firms. The estimate for the hiring rate at Q2-raising firms, however, is not statistically significant (and 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.

Table 8: 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. **Bold**, *Italics*, and * 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.

4.5 Little Heterogeneity in Firms Response to DNWR Exposure

I explore heterogeneity in firms' responsiveness to DNWR exposure along the dimensions of firm age, size, average labor productivity, and variability in worker productivity. The regression specification in Equation 8 explores whether Q2-raising firms with a particular firm characteristic had more job destruction in 2008:Q4 relative to otherwise similar Q4-raising firms, thereby identifying firm characteristics that are associated with firms' sensitivity to downward nominal wage rigidity.

$$JD_{kt} = \boldsymbol{D}_{\boldsymbol{kt}}^{\boldsymbol{2008{:}Q4}} \left(\beta^{Q4} + \boldsymbol{C}_{\boldsymbol{kt}}\gamma^{Q4} + \boldsymbol{D}_{\boldsymbol{kt}}^{\text{Q2 raiser}}\left(\beta^{Q2} + \boldsymbol{C}_{\boldsymbol{kt}}\gamma^{Q2}\right)\right) + \boldsymbol{X}_{kt}\beta_t + \epsilon_{kt} \tag{8}$$

where $\boldsymbol{D}_{\boldsymbol{kt}}^{\mathbf{2008:Q4}}$ is an nxn diagonal matrix where each diagonal element equals one if t=2008:Q4; $\boldsymbol{C}_{\boldsymbol{kt}}$ is a matrix of the dummy variables corresponding to the 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 a given firm characteristic. 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. Also included in the set of control variables are time-varying firm characteristics (firm age, firm size, and average labor productivity) interacted with the firm's typical raise quarter.[27]

I find little heterogeneity in the effect of DNWR on firms' job destruction rates. Table 9 reports the results of various difference-in-difference models that interact the firm's typical raise quarter and 2008:Q4 dummy variables with firm age, firm size (as of March 13th), the one-year lag of firm log labor productivity (in units of one standard deviation), and the standard deviation of employee wage growth at the firm (in units of one standard deviation). Young firms with only 2-3 years of employment history have larger increases in their rates of job destruction because of exposure to DNWR, but there was little variation in the job destruction rates among firms that were 4+ years old. More interestingly, greater historical variation in employees' average annualized log real wage change since hiring is associated with larger increases in firms' job destruction rates in response to exposure to DNWR. This finding is consistent with firms laying off more workers when there is greater dispersion in worker productivity (assuming that firms adjust workers' wages as they learn about worker productivity).

4.6 Heterogeneous Response of Worker Layoff Rates to 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

[27] The time-invariant firm characteristics are absorbed by firm-specific seasonality fixed-effects.

Table 9: Differential Q2 vs Q4-Raiser Job Destruction Rate in 2008:Q4 by Firm Characteristic

	Firm Age		Firm Size	Log Labor Productivity	Std Dev of Log Employee Wage Growth
	(1)		(2)	(3)	(4)
0-1	-1.27	1-19	**1.12**	-0.12	*0.70*
	(0.90)		(0.25)	(0.29)	(0.23)
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	7.7%		7.7%	7.4%	6.5%
Observations	4.6M		5.6M	1.7M	1.8M
R-Squared	0.317 M		0.290	0.340	0.335

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. **Bold** and *Italics* indicate statistical significance at the 0.1% and 1.0% levels respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

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, 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:

$$L_{ikt} = \boldsymbol{C_{ikt}} \left[\alpha^4 + \boldsymbol{D}_{kt}^{2008:Q4} \theta^4 + \sum_{q=1}^{3} \left(\boldsymbol{D}_{kt}^{\text{Qq-raiser}} \alpha^q + \boldsymbol{D}_{kt}^{2008:Q4} \boldsymbol{D}_{kt}^{\text{Qq-raiser}} \theta^q \right) \right] + \boldsymbol{X_{kt}} \beta_t + \epsilon_{kt} \tag{9}$$

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 10 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 10: 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. **Bold** and *italics* indicate statistically significant at the 0.1% and 1.0% levels respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

5 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.[28] This section presents 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.

I model a multi-worker firm with 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 the workers' quit decisions are influenced by both their real wage levels and nominal wage changes.

The model incorporates downward nominal wage rigidity by assuming that 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.[29] 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.[30] 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, a worker is still 95% more likely to quit relative to a worker with no exposure to a nominal wage

[28] Labor search-and-matching models incorporating wage rigidity 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 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.

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

[30] The regression sample includes only individuals age 18-61 with at least seven quarters of tenure. To better isolate persistent earnings changes that are due to changes in base wages rather than hours paid, I use the secondary sample of four states with quarterly hours paid for this regression. I include as control variables the firm's period-specific no-gap employer-to-employer transition rate and other separation rate,[31] 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 variables indicating whether the worker received a raise in each of the last six periods.

cut.

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). 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. Quarterly LEHD data from 2011:Q1 to 2017:Q4 from the four states with hours paid. Sample of 12.5 million observations 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 model also incorporates a current-period minimum profit constraint that prohibits the firms' current-period profit from falling below some threshold. My specification of the current-period profit 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. The current-period minimum profit constraint is critical for an effect of DNWR on job destruction since it forces the firm to consider second-best alternatives to surplus-maximizing layoff and wage setting decisions. Although alternative financial frictions could similarly force the firm to consider second-best alternatives, the current-period minimum profit constraint is attractive because it is consistent with my empirical finding from Section 4.2.1 that firms' job destruction rates were 48% more responsive to negative revenue changes when firms had greater exposure to DNWR. Thus, short-term fluctuations in revenue (and presumably current-period profits) are strongly associated with greater job destruction at firms with more exposure to DNWR.[32]

[32]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

The inefficient job destruction by DNWR in the model results from the interaction of: i) the firm's current-period profit 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, this current-period minimum profit constraint forces 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 will optimally choose to lay off the worker in order to avoid cutting the nominal wage of a coworker with a larger firm surplus. Thus, DNWR generates job destruction not because the worker and firm fail to agree on a mutually advantageous nominal wage cut, but 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.

5.1 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 $\Theta_t = (A_t, \pi_t, w_t^n, \theta_t)$.

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 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 $\Omega_t = (z_{kt}, E_{kt})$.

5.2 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

margins.

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:

$$f\left(A_t, z_{kt}, \{x_{it}, h_{it}\}_{\forall i \in E_{kt}}\right) = A_t z_{kt} \left(\prod_{i \in E_{kt}} (x_{it})^{h_{it}/n_{kt}} n_{kt} \right) \tag{10}$$

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 $n_{kt} = \sum_{i \in E_{kt}} h_{it}$.

5.3 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:

$$\Delta^-_{it} = \begin{cases} 1 & \text{if } \Delta^-_{it-1} = 1 \text{ or } w_{it} < w_{it-1}/\pi_t \\ 0 & \text{otherwise} \end{cases} \tag{11}$$

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:

$$S\left(\Theta_t, \Omega_{kt}\right) = \max_{v_{kt}, \{h_{it}, w_{it}\}_{\forall i \in E_{kt}}} f_{kt} - \sum_i w_{it} h_{it} - c^n n_{kt} - \frac{c^v}{g(\theta_t})v_{kt} + \beta \mathbb{E}_t\left[S_{kt+1}\right] \tag{12}$$

subject to:

1. A current period profit 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 $g' < 0$.

5.4 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.[33] 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 ($\Delta_{it}^- = 1$).

$$q_{it} = \delta^{\Delta_{it}^-} q\left(\frac{w_{it}}{w_t^n}\right) \tag{13}$$

5.5 The Firm's Optimal Layoff and Wage Setting Decisions

5.5.1 Unconstrained Optimal Retained Employee Set and Wage 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

[33] 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.

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,

$$S^{+i}\left(w_{it};\Theta_t, z_{kt}, E^r_{kt}\right) = S\left(\{w^s_{jt}\}_{\forall j\in E^r_{kt}};\Theta_t, z_{kt}, E^r_{kt}+i, w_{it}, \Delta^-_{it}\right) - S\left(\{w^s_{jt}\}_{\forall j\in E^r_{kt}};\Theta_t, z_{kt}, E^r_{kt}\right) \tag{14}$$

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

The worker's surplus-maximizing wage, w^s_{it}, is the wage that maximizes the worker's marginal surplus as defined in Equation 14, ignoring the current-period profit 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 ($E^{r^*}_{kt}$) as the employee set that maximizes the firm's total surplus if the current-period profit 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 profit 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 $E_{kt}^{r^*}$.

5.5.2 Profit Constrained Optimal Retained Employee Set and Wage Decisions

To determine how the firm's optimal layoff and wage setting decisions change when the current profit 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 profit at the employees' surplus-maximizing wages falls below the constraint threshold a. In such a scenario, the current-period profit 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 profit constraint binds, it is useful to define a worker i's current-period marginal profit, $p^{+i}(w_{it}; \Theta_t, z_{kt}, E_{kt}^r, x_{it})$ as the change in the firm's current-period profit 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:

$$p^{+i}\left(w_{it}; \Theta_t, z_{kt}, E_{kt}^r, x_{it}\right) = A_t z_{kt} \bar{x}_{kt} \left(\left(\left(\frac{x_{it}}{\bar{x}_{kt}} \right)^{\frac{1}{n_{kt}+1}} - 1 \right) n_{kt} + \left(\frac{x_{it}}{\bar{x}_{kt}} \right)^{\frac{1}{n_{kt}+1}} \right) - w_{it} - c^n \tag{15}$$

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 profit 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 profit at her surplus-maximizing wage ($p^{+i}(w_{it}^s) > 0$) 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 profit constraint and lower the firm surplus.

Second, a firm will never consider laying off workers who generate positive current-period marginal profits since laying them off would actually tighten the current-period profit 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, $\underline{w}$,[34] and the surplus-maximizing wage, w_{it}^s.[35] The firm considers only wages within the range of $[\underline{w}, w_{it}^s]$ because any wage outside of this range simultaneously tightens the current-period profit constraint and lowers the firm surplus.

These three restrictions imply that the firm considers the following set of potential second-best

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

[35] A sufficient condition for the surplus-maximizing wage to be greater than the current-period profit-maximizing wage is that, at the worker's current-period profit-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,

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

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 profit-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).

policies $(\tilde{H}_{kt}, \tilde{W}_{kt})$ when the current-period constraint binds:

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

and

$$w^c_{kt} \in \begin{cases} [\underline{w}, w^s_{it}] & \text{if } i \in E^{r^*}_{kt} \end{cases}$$

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 profit 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 profit for any given wage rate or layoff decision. Specifically, for a layoff, this ratio metric is:

$$\ell(h_{it} = 0) = \frac{\text{Surplus Loss from Layoff}}{\text{Profit Gain from Layoff}} = \frac{0 - S^{+i}_{kt}(w^s_{it})}{0 - p^{+i}_{kt}\left(w^s_{it}\right)} \tag{16}$$

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

$$\ell(h_{it} = 1, w_{it}) = \frac{\text{Surplus Loss at Wage } w_{it}}{\text{Profit Gain at Wage } w_{it}} = \frac{S^{+i}_{kt}(w_{it}) - S^{+i}_{kt}(w^s_{it})}{w_{it} - w^s_{it}} \tag{17}$$

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 profit.

The firm's optimal set of second-best layoff and wage setting decisions (H^c_{kt}, W^c_{kt}) will minimize the loss of firm surplus (relative to the unconstrained firm surplus) subject to the current-period profit 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^s_{kt}, W^s_{kt}). 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.[36] I continue selecting second-best wage and layoff decisions until the current-period profit constraint no longer binds. This final set of original optimal decisions and

[36] As this iterative process continues, it is possible that the gain in the current-period profit from laying off a positive-surplus worker exceeds the remaining profit gap between the firm's current-period profit, 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 profit gap rather than the total realized profit gain from the lay off. This is because greater current-period profits are only useful (relative to the surplus loss) insofar as they help relax the current-period profit constraint.

the least costly second-best decisions becomes the firm's constrained policy decision set (H^c_{kt}, W^c_{kt}).

5.5.3 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 profit 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 profit constraint binds at the employees' surplus-maximizing wages. In such a scenario, the current-period profit 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 profit constraint binds. If the $A_t z_{kt}$ value is so low that it falls below the T1 threshold, then the current-period profit constraint forces the firm to lay off both workers since not even the higher productivity worker (Worker A) generates sufficient profit at her current-period profit-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 profit-maximizing wages. The current-period profit 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 $(\sqrt{x_{At}}\sqrt{x_{At}})$.

$A_t z_{kt}$

T1: $\frac{a+\underline{w}+c^n}{x_{At}}$ **T2**: $\frac{a+2\underline{w}+2c^n}{2\bar{x}^{AB}_{kt}}$ **T3**: $\frac{a+\frac{w_{At-1}}{\pi_t}+\underline{w}+2c^n}{2\bar{x}^{AB}_{kt}}$ **T4**: $\frac{a+\frac{w_{At-1}}{\pi_t}+\frac{w_{Bt-1}}{\pi_t}+2c^n}{2\bar{x}^{AB}_{kt}}$ **T5**: $\frac{a+w^s_{At}+w^s_{Bt}+2c^n}{2\bar{x}^{AB}_{kt}}$

5.5.3.1 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 profit 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 profit-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:

$$\underbrace{S_{kt}\left(w_{At}^{c|A}, \Delta_{At}^{-}=0, h_{Bt}=0\right)}_{\text{Surplus From Only Worker A - No Cut}} > \underbrace{S_{kt}\left(w_{At}^{c|AB}, \Delta_{At}^{-}=1, w_{Bt}^{c|AB}, \Delta_{Bt}^{-}=1\right)}_{\text{Surplus From Both Worker A \& B - Both Cut}} \tag{18}$$

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:

$$\overbrace{\frac{1}{\beta}\Pi\left(w_{At}^{c|A},-\right)}^{\text{E1: Current Profit from A}} + \overbrace{\left(q\left(w_{At}^{c|AB}, cut\right) - q\left(w_{At}^{c|A}, no\ cut\right)\right)}^{\text{E2: }\triangle\text{Quit rate from wage cut}} \overbrace{\mathbb{E}\left[S_{t+1}\left(\left(w_{At}^{c|A}, no\ cut\right),-\right)\right]}^{\text{E3: Future surplus: A(no-cut)}}$$

$$+ \underbrace{\left(1-q\left(w_{At}^{c|AB}, cut\right)\right)}_{\text{E4: Prob A stays after cut}} \underbrace{\left(\mathbb{E}\left[S_{t+1}\left(w_{At}^{c|A}, no\ cut\right),-\right] - \mathbb{E}\left[S_{t+1}\left(w_{At}^{c|AB}, cut\right),-\right]\right)}_{\text{E5: }\triangle\text{ Future surplus: A(no-cut) - A(cut)}}$$

$$>$$

$$\overbrace{\frac{1}{\beta}\Pi\left(w_{At}^{c|AB}, w_{Bt}^{c|AB}\right)}^{\text{E6: Current Profit from A\&B}} + \overbrace{\left(1-q\left(w_{Bt}^{c|AB}, cut\right)\right)q\left(w_{At}^{c|AB}, cut\right)}^{\text{E7: Prob only B stays after cut}} \overbrace{\mathbb{E}\left[S_{t+1}\left(-,\left(w_{Bt}^{c|AB}, cut\right)\right)\right]}^{\text{E8: Future surplus: B(cut)}}$$

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

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 profit 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 profit 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.

5.5.3.2 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 the job would have a positive surplus at a lower nominal wage. 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 profit 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:

$$\underbrace{S_{kt}\left(w_{At}^{c|A}, \Delta_{At}^{-}=0, h_{Bt}=0\right)}_{\text{Surplus From Only Worker A - No Cut}} > \underbrace{S_{kt}\left(w_{At}^{c|AB}, \Delta_{At}^{-}=0, w_{Bt}^{c|AB}, \Delta_{Bt}^{-}=1\right)}_{\text{Surplus From Both Worker A \& B - Only B Cut}} \tag{19}$$

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

$$S_{kt}\left(w_{At}^{c|AB}, \Delta_{At}^{-}=0, w_{Bt}^{c|AB}, \Delta_{Bt}^{-}=1\right) = S_{kt}\left(w_{At}^{c|A}, \Delta_{At}^{-}=0, h_{Bt}=0\right) + S_{kt}^{+B}\left(w_{Bt}^{c|AB}\right) \tag{20}$$

Combining the condition in Equation 19 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.

$$0 > S^{+B}\left(w_{Bt}^{c|AB}; \Theta_t, \Omega_{kt}\right) \tag{21}$$

If the marginal current-period profit 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 profit generated by the worker at his constrained surplus-maximizing wage remains negative (which could occur if the coworker is generating sufficient positive profits to offset these negative marginal profits). 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 profits 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.

5.5.4 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 profit constraint binds is also relevant for a multi-worker firm. When the current-period profit 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 5.5.2. 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).

5.6 Testable Implications of Mechanism for DNWR to Cause Job Destruction

Using the quasi-experiment described in Section 4, I empirically test one implication of the model. Namely, when firms experience a negative aggregate productivity shock, the job destruction rate should rise more at firms with greater within-firm dispersion in workers' marginal firm surpluses.

Although I do not observe the firm surplus generated by individual workers, I construct the following proxy for the dispersion in the firm surplus generated by workers at each firm. I first estimate the average annualized growth rate of each worker's real wage at a given firm. When the firm hires a worker, it presumably 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 growth since hiring should exhibit greater dispersion in their workers' observable real wage changes since hiring. Thus, 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 standard deviation of these growth rates serves as a proxy for dispersion in worker surplus 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).

The model would predict that the job destruction rate should have risen more in 2008:Q4 at Q2-raising firms with greater dispersion in their workers' real wage growth. I use the difference-in-differences estimation specification described in Section 4.5. Column (4) of Table 9 shows that in 2008:Q4, Q2-raising firms with 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 dispersion of the firm's workers' annualized real wage growth (the within-firm dispersion of annualized wage growth is measured as the standard deviation of real wage growth across all workers at the firm). This effect is statistically significant at the 1% level, supporting the model's mechanism.

6 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. First, 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 simply because of the timing of their historical raise schedules relative to the unanticipated financial collapse in September 2008. I find 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 capture the fact that the Q4-raising firms had less, but not no exposure to downward nominal wage rigidity, it is only a lower-bound estimate of the effect of downward nominal wage rigidity on job destruction in 2008:Q4, .

My quasi-experimental results conflict with a common assumption of labor search-and-matching models that wage rigidity should not cause job destruction (which is a common interpretation of Barro's argument that workers and firms should not forgo mutually beneficial nominal wage cuts). As a step towards addressing this conflict, I present a model in which downward nominal wage rigidity causes firms to destroy positive surplus jobs. DNWR has this effect despite firms being able to pay workers different wages and workers and firms not forgoing mutually advantageous nominal wage cuts. When a large enough negative shock occurs, fixed labor costs combine with a current-period minimum profit constraint to force the firm to either i) lay off a low but positive-surplus worker, or ii) cut the nominal wages of both the worker and a coworker who generates a larger firm surplus. I show empirically that workers respond to nominal wage cuts with a large and persistent increase in their quit rates. This quit response means that the expected future surplus generated by a worker falls significantly after a nominal wage cut due to the worker's shorter expected job duration. The implication of this quit response for firms' job destruction decisions is that a worker with a low but positive marginal surplus may be laid off in response to a negative shock in order to avoid cutting the nominal wage of a coworker who generates a larger marginal surplus. Thus, DNWR causes a positive-surplus worker to be laid off not because the worker and firm fail to realize a mutually advantageous nominal wage cut, but because the firm prefers laying off a positive-surplus worker to cutting the nominal wages of both the worker and a higher-surplus coworker. Specifically, the certain loss in firm surplus from laying off the worker is less than the expected loss in firm surplus from a higher-surplus worker being more likely to quit after a nominal wage cut.

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 effect for other recessions in which the negative shocks are unlikely to be as large and short-term financing may be more readily available. Consequently, it is unclear what role DNWR plays more generally in firms' job destruction decisions. Lastly, the estimates and model presented in this paper are all partial equilibrium results that ignore both the impact of the availability of more unemployed workers on firms' job destruction decisions (presumably making it easier to hire high-quality workers) and the negative effect on consumer demand of greater unemployment (from greater job destruction) potentially exacerbating negative shocks hitting the firms.

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.[37] 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 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-job has implications for the asymmetric response of employment and output to contractionary versus expansionary aggregate shocks. Many of the DSGE and labor search-and-

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

matching models that explore these asymmetric responses tend to ignore the possibility that downward nominal wage rigidity would affect 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 downward nominal rigidity affecting job destruction would also have implications for the effectiveness of contractionary versus expansionary monetary policy shocks.

A final set of policy implications concerns the mechanism by which downward nominal wage rigidity affects job destruction. In this paper, I propose a model that relies on a minimum profit constraint binding when a large negative shock drives a firm's revenues below the firm's labor expenses. If this model accurately captures a mechanism by which downward nominal wage rigidity affects job destruction, then the negative impact of downward nominal wage rigidity could be ameliorated by policies such as work sharing. For example, in response to a negative shock, a firm could use the unemployment insurance system to reduce workers' hours worked while maintaining much or all of their original pay (Abraham and Houseman (2014)). Additionally, policies that increase the fixed costs of labor (such as employer mandates for health insurance or other fixed cost benefits) may increase the likelihood that firms will respond to negative shocks by destroying jobs. These fixed labor costs increase the degree to which laying off the low-surplus worker relaxes the firm's profit constraint, thus making the layoff more attractive relative to nominal wage cuts for a broader set of workers.

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A Relative Contribution of Permanent vs. Transitory Earnings Changes

If the quasi-experiment were to measure workers' compensation changes using changes in log earnings, then the transitory components of quarterly earnings would present two problems. First, the frequency of earnings changes makes it difficult to identify a firm's typical raise quarter. As shown in Table 1, even when controlling for quarterly hours paid, workers experience a change in log earnings almost every quarter. Only 5.0% of workers have no quarter-over-quarter change in log hourly earnings, whereas 55.5% receive a nominal raise and 39.5% receive a nominal cut.[38] In contrast, Grigsby, Hurst and Yildirmaz (2019) use ADP payroll data to examine workers' base wages and they find that 80.6% of workers have no quarter-over-quarter change in their base nominal wage, 18.5% receive a nominal base wage raise, and only 0.9% receive a nominal cut. They further show both that the bonus component of hourly earnings drives much of this difference between hourly earnings and base wage changes and that bonuses exhibit little persistence. That workers' quarterly earnings change so often makes it difficult to use quarterly earnings changes to identify a dominant calendar quarter in which a firm tends to raise its workers' compensation.

Second, most fluctuations in quarterly earnings are unlikely to persist into future periods. As a result, historical patterns of workers' quarterly earnings changes are less predictive of firms' start-of-quarter worker compensation costs. To demonstrate the lack of persistence in workers' quarterly earnings changes, I evaluate the relative importance of permanent versus transitory earnings changes using the autocorrelation of workers' four-quarter change in log hourly earnings ($\Delta^{y^H}_{ik,t,t-4}$).[39] The four-quarter log hourly earnings change can be decomposed into the sum of the persistent quarterly changes in hourly earnings in $t-3$, $t-2$, $t-1$, and t ($\Delta^P_{ik,t-a}$), plus the transitory change in hourly earnings in t (Δ^T_{ikt}).

$$\Delta^{y^H}_{ik,t,t-4} = \ln\left(\frac{y_{ikt}}{h_{ikt}}\right) - \ln\left(\frac{y_{ikt-4}}{h_{ikt-4}}\right) = \Delta^T_{ikt} + \Delta^P_{ikt} + \Delta^P_{ikt-1} + \Delta^P_{ikt-2} + \Delta^P_{ikt-3} \tag{22}$$

[38]The relatively small number of quarter-over-quarter freezes in log hourly earnings is consistent with the findings of Kurmann and McEntarfer (2019) and Jardim, Solon and Vigdor (2019) regarding the frequency of four-quarter changes in log hourly earnings. Both studies use quarterly earnings and hours-paid data from Washington state to show hourly earnings exhibit far less year-over-year rigidity relative to the frequency of nominal wage freezes in the survey literature. Grigsby, Hurst and Yildirmaz (2019) uses ADP payroll data to confirm this large difference in the relative frequency of nominal changes in base wages versus hourly earnings. I find that even when controlling for measurement error due to rounding in hours paid and annual bonuses, the share of workers with no quarter-over-quarter change in log hourly earnings remains low at 22.2% (adjusting for rounding in hours paid) and 45.3% (adjusting for annual bonuses and rounding in hours paid).

[39]This uses data for workers in the four states with hours-paid data.

Notice that the four-quarter change and its one-period lag ($\Delta^{y^H}_{ik,t-1,t-5}$) share three of these persistent components - namely Δ^P_{ikt-1}, Δ^P_{ikt-2}, and Δ^P_{ikt-3}. Assuming that the persistent change components are distributed iid, then the autocorrelation of $\Delta^{y^H}_{ik,t,t-4}$ is 0.75 if there are no transitory changes. However, I find that the autocorrelation of the four-quarter change in log hourly earnings is only 0.284. Under a set of strong assumptions,[40] I can estimate the relative magnitude of the permanent and transitory changes in hourly earnings as follows:

$$Corr\left(\Delta^{y^H}_{ik,t,t-4},\Delta^{y^H}_{ik,t-1,t-5}\right)=\frac{\mathbb{E}\left[\Delta^{y^H}_{ik,t,t-4}\Delta^{y^H}_{ik,t-1,t-5}\right]}{Var\left(\Delta^{y^H}_{ik,t,t-4}\right)Var\left(\Delta^{y^H}_{ik,t-1,t-5}\right)} \tag{23}$$

Assuming that i) the persistent change components for each quarter follow the same iid distribution, ii) similarly, the transitory change components for each quarter follow an iid distribution, and iii) the persistent and transitory components are independent then

$$Corr\left(\Delta^{y^H}_{ik,t,t-4},\Delta^{y^H}_{ik,t-1,t-5}\right)=\frac{3Var\left(\Delta^P_{ik}\right)}{4Var\left(\Delta^P_{ik}\right)+Var\left(\Delta^T_{ik}\right)} \tag{24}$$

This implies that the relative variation in quarterly earnings from the transitory versus the persistent changes is:

$$\frac{Var\left(\Delta^T_{ik}\right)}{Var\left(\Delta^P_{ik}\right)}=\frac{3-4Corr\left(\Delta^{y^H}_{ik,t,t-4},\Delta^{y^H}_{ik,t-1,t-5}\right)}{Corr\left(\Delta^{y^H}_{ik,t,t-4},\Delta^{y^H}_{ik,t-1,t-5}\right)} \tag{25}$$

Thus, the autocorrelation estimate implies that the variance of the transitory component of the quarterly measure of hourly earnings is 6.6 times greater than the variance of the permanent component. This indicates that transitory changes account for 86% of the quarter-over-quarter fluctuations in hourly earnings.

It is reassuring to note that the post-Lasso procedure identifies persistent wage changes as occurring in periods in which this autocorrelation measure indicates that log earnings changes are more persistent. Using the same metric of the autocorrelation of the four-quarter change in log hourly earnings, I find that the autocorrelation is 0.512 if the post-Lasso procedure detects a wage change in $t-1$, whereas it is only 0.123 if no wage change is detected by the post-Lasso procedure in $t-1$. The estimated wage changes from the post-Lasso procedure are much more likely to persist, and

[40] Namely the three assumptions are i) that persistent change components are distributed iid, ii) that temporary change components are distributed iid, and iii) that the temporary and permanent change components are independent within any 5-quarter window.

thus affect the firms' start-of-quarter wage bills in future periods.

B Estimation of Payday Weeks

Payroll schedules generate significant fluctuations in quarterly earnings because of variation in the number of pay periods from quarter to quarter. For instance, workers who are paid bi-weekly typically experience $\pm 15\%$ fluctuations in quarterly earnings from one quarter to the next as the number of quarterly paydays consistently switches between six and seven. Although earnings fluctuations generated by changes in the number of payday weeks appear as noise at an individual level, these payday-related changes can be identified because they are common to many workers at the firm. Knowing the changes that are induced by payroll schedules becomes a useful feature of the data because these changes are directly related to the number of weeks worked, and thus allow estimation of a worker's average weekly earnings.

The method for identifying a firm's payroll schedule(s) exploits three empirical regularities. First, there are a limited number of payday schedules: 7 weekly, 14 bi-weekly, one monthly, and one semimonthly payroll schedule. Second, each of these payday schedules has a distinct time series of payday weeks from quarter to quarter. Importantly, the time series of quarterly payday weeks for each payday schedule can be determined from the annual calendar. And third, firms tend to use a small number of payroll schedules for their employees (typically only one or two, see Burgess (2014)), so the fluctuations in quarterly earnings caused by payday schedules are common to many workers at the firm.

Estimating a worker's payday weeks first requires identifying the set of payroll schedules used by the worker's employer. This firm-specific set of potential payroll schedules is determined by iteratively selecting the payroll schedule that best fits the observed quarter-over-quarter earnings changes for the largest number of workers. For each worker at the firm and potential payroll schedule p, I construct the worker's payroll-schedule-adjusted log earnings change, Δy_{ikt}^{p}, defined as:

$$\Delta y_{ikt}^{p} = \big(\ln(y_{ikt}) - \ln(y_{ikt-1})\big) - \big(\ln(n_t^p) - \ln(n_{t-1}^p)\big) \tag{26}$$

where n_t^p is the number of payday weeks for the payroll schedule p in quarter t based on the annual calendar.[41]

[41] One complication from using the annual calendar is that January 1st, New Years Day, is a holiday that occurs at the transition between Q4 and Q1. From the data, it is apparent that some firms shift weeks paid from Q1 to Q4 when the weekday of payment falls on New Years Day.

The best fitting payroll schedule for a given worker, p^*_{ik}, is the payroll schedule that has the lowest variance of Δy^p_{ikt}. The intuition behind this rule is that for the true payroll schedule, the subtracted change in the payroll schedule's payday weeks, $\left(\ln(n^p_t) - \ln(n^p_{t-1})\right)$, is perfectly negatively correlated with the true payday weeks change component of the quarterly earnings change. This perfect negative correlation, combined with an assumption that changes in wages, weekly hours, and variable compensation are independent of the number of payroll weeks, implies that the true payday schedule minimizes the variance in Equation 26 as the duration of the job spell approaches infinity. Once each worker's best-fitting payroll schedule has been identified, I select the payroll schedule that is best for the largest number of workers, where each worker's best schedule is given a weight equal to the duration of the worker's job spell (this accounts for differences in the precision of the variance estimates caused by differences in the job spell duration).

After each iteration, I remove from the next iteration any workers for whom the selected payroll schedule was the best-fitting payday schedule. This process continues until either four payroll schedules have been selected or no remaining payroll schedule is the best-fitting payroll schedule for at least five workers. Having identified the set of potential payday schedules at the firm, I select the best-fitting payroll schedule for each individual worker by choosing from among this constrained set of payday schedules the payday schedule that minimizes the variance of the worker's payroll-schedule-adjusted log earnings (Δy^p_{ikt}).

C Post-Lasso Estimation of Persistent Nominal Wage Changes

The key drawback of the LEHD data for my proposed quasi-experiment is that the LEHD only reports workers' quarterly earnings - which can vary due to fluctuations in overtime pay, bonuses, payday weeks, average weekly hours paid, or the base wage.[42] Many of these components of quarterly earnings are quite transitory, and thus are unlikely to affect employment decisions in long-term employment relationships. To overcome this limitation of the LEHD, I develop a set of novel machine learning tools that identify persistent changes in workers' unobserved nominal base wages from their observed nominal quarterly earnings. This section describes these machine learning methods and evaluates the quality of the resulting estimated nominal wage changes.

To more formally distinguish between persistent versus transitory components of quarterly earn-

[42]The LEHD data set does have total quarterly hours paid for four states from 2011 forward. Although these hours-paid data are helpful for identifying patterns in workers' wage changes, only Washington state has hours-paid data covering the period of the quasi-experiment (prior to 2009). For more details on patterns of adjustment in the non-wage components of quarterly earnings, see Appendix D.

ings, I decompose each worker's quarterly earnings as a function of the worker's wage, the number of payday weeks in the quarter, the number of hours worked, and any variable compensation paid to the worker (e.g. annual bonuses, tips, and commissions).[43] Specifically,

$$y_{ikt} = w_{ikt}\left(n_{ikt}\bar{h}_{ikt} + \theta^{o}_{ikt} n_{ikt}\bar{h}^{o}_{ikt}\right) v_{ikt}\epsilon_{ikt} \tag{27}$$

where y_{ikt} is worker i's total nominal earnings at firm k in quarter t, w_{ikt} is the worker's base hourly wage, n_{ikt} is the number of payday weeks in the quarter, $\bar{h}_{ikt}$ is the worker's average weekly hours paid, θ^{o}_{ikt} is the overtime premium for overtime pay (typically 1/2), $\bar{h}^{o}_{ikt}$ is the worker's average weekly overtime hours paid, v_{ikt} is the worker's variable compensation as a percent of the base wage, and ϵ_{ikt} is measurement error (such as dropping or adding a decimal place in the recorded quarterly earnings).[44]

I use a post-Lasso procedure to extract persistent changes in each worker's unobserved base wage (w_{ikt}) from their observed quarterly earnings (y_{ikt}). This procedure involves four steps. First, I express each worker's base wage in any given period t as a recursive formulation of the worker's starting wage (w_{ik1}) and all base wage changes up to the current period (Δ^{w}_{iks}):

$$w_{ikt} = w_{ik1}\prod_{s=2}^{t}\left(1 + \Delta^{w}_{iks}\right) \tag{28}$$

Inserting this recursive formulation into the quarterly earnings decomposition in Equation 27 and then taking the natural log allows the observed quarterly earnings to be rewritten as:

$$\ln\left(y_{ikt}\right) = \ln\left(w_{ik1}\right) + \sum_{s=2}^{t}\ln\left(1 + \Delta^{w}_{iks}\right) + \ln\left(n_{ikt}\right) + \ln\left(\bar{h}_{ikt} + \theta^{o}_{ikt}\bar{h}^{o}_{ikt}\right) + \ln\left(v_{ikt}\right) + \ln\left(\epsilon_{ikt}\right) \tag{29}$$

Second, although I do not observe any of the right-hand side variables, I can write Equation 29 using a standard linear regression model. Specifically, if we observe T periods of employment for

[43] When measuring workers' wages, I only consider "full-quarter" earnings - where the worker has positive earnings from the same SEIN in the quarter immediately before and after the current quarter. This has the benefit of reducing fluctuations in quarterly earnings that result from new hires and job separators working only part of the quarter in which they are hired or separate.

[44] For salaried employees who are exempt from overtime pay requirements, the average reported weekly hours paid always equals 40 (no matter how many hours are actually worked) and the average weekly overtime hours paid is always zero.

the worker at the firm, then the current period quarterly earnings can be expressed as:

$$\ln(y_{ikt}) = \beta_{ik}^{1} d_{ikt}^{1} + \sum_{s=2}^{T} \beta_{ik}^{s} d_{ikt}^{s} + \alpha_{ik} \ln(n_{ikt}) + \underbrace{\ln\left(\bar{h}_{ikt} + \theta_{ikt}^{o} \bar{h}_{ikt}^{o}\right) + \ln(v_{ikt}) + \ln(\epsilon_{ikt})}_{\tau_{ikt}} \tag{30}$$

where d_{ikt}^{s} is a set of T indicator variables that, in any given period t, take on a value of 1 only if $t \geq s$. Thus, d_{ikt}^{1} corresponds to the intercept term and its coefficient, β_{ik}^{1}, represents the log starting weekly wage of the worker: $\ln(w_{ik1})$. The coefficient on each subsequent indicator variable, β_{ik}^{s}, represents the persistent nominal wage change experienced by the worker in period s: $\ln\left(1 + \Delta_{iks}^{W}\right)$.

Although I do not observe the number of payday weeks for each worker (n_{ikt}), controlling for the number of payday weeks in the regression model specified in Equation 30 is important because the quasi-experiment aggregates the individual workers' estimated wage changes to the firm level. Since firms tend to use only one or two payday schedules for all of their workers (see Burgess (2014)), a large share of workers at a firm may exhibit similar persistent earnings changes simply because of their number of payday weeks. To address this concern, I develop a clustering method that estimates the number of payday weeks for each worker. This clustering method exploits the fact that each firm uses only a small number of payday schedules, which are themselves selected from a total universe of 23 payday schedules.[45] Critically, each of these potential payday schedules has a distinct time series of payday weeks from quarter to quarter - where this time series can be determined from the annual calendar. Thus, the clustering algorithm identifies payday schedules at the firm (and then for a worker) based on patterns of quarter-over-quarter earnings changes that are both common to many workers at the firm and align with one of the potential payday schedules. Appendix B contains a complete description of the clustering method for estimating workers' payday weeks ($\hat{n}_{ikt}$).

The number of payday weeks are common to many workers at the firm. The remaining unobserved components, representing average weekly hours paid, overtime, and variable compensation, are included in the τ_{ikt} error term. While I would ideally observe these components as well, their absence is less troubling for the quasi-experiment since persistent changes in these unobserved components of earnings are much more likely to be idiosyncratic to the worker. As a result, they are less likely to affect the firm-level wage change measures.

In the third step, I identify the quarters in which a worker received a wage change. It is impossible

[45] The 23 payday schedules include 7 weekly, 14 bi-weekly, one monthly, and one semimonthly payroll schedule.

to estimate the regression model in Equation 30 using standard methods because there are $T+1$ variables and only T observations. Instead, I exploit the fact that there are many quarters in which a worker has no change in their base wage. This implies that $\beta^s_{ik}=0$ in those quarters, making the Lasso variable selection procedure an ideal method for identifying quarters in which a worker has a persistent wage change (i.e. has a non-zero β^s_{ik} coefficient).[46]

The Lasso estimation procedure selects variables to include in a regression model by trading off the improvement in the explanatory power of the model when the variable is allowed a non-zero coefficient (the standard OLS minimization of the sum of squared residuals) against a penalty for the absolute distance of the coefficient from zero. Thus, for every worker-firm job spell, I use the Lasso estimation procedure to select the set of non-zero β^s_{ik} that solve the following minimization problem:[47]

$$\min_{\beta^1_{ik},\ldots,\beta^T_{ik},\alpha_{ik}} \left(\sum_{t=1}^{T} \ln(y_{ikt}) - \sum_{s=1}^{T} \beta^s_{ik} d^s_{ikt} - \alpha_{ik} \ln\left(\hat{n}_{ikt}\right)\right)^2 + \lambda_{ik}\left(||\alpha_{ik}|| + \sum_{s=1}^{T} ||\beta^s_{ik}||\right) \tag{31}$$

where $\hat{n}_{ikt}$ is the estimated number of payday weeks from the procedure described in Appendix B. The λ_{ik} penalty parameter is set using 10-fold cross validation for each worker-firm job spell.[48] If the job spell has fewer than five full quarters of employment, then I, instead, use leave-one-out cross validation.

The error term of this Lasso minimization problem includes any error from the estimation of payday weeks plus deviations of log weekly hours, overtime hours, variable compensation, and measurement error from the variables' averages over the job spell. Minimizing the Lasso objective function identifies the quarters for which assigning a non-zero wage change coefficient significantly improves the model fit in both the current period and all subsequent periods. Thus, the Lasso procedure identifies persistent changes in the worker's weekly earnings.[49] Although these persistent

[46] Notable studies that use Lasso estimation for variable selection in a time-series context include Harchaoui and Lévy-Leduc (2010) and Ciuperca (2014).

[47] For the 4-state sample with quarterly hours-paid data, I replace $\ln(\hat{n}_{ikt})$ with the log of the reported hours paid ($\ln(n_{ikt}\bar{h}_{ikt})$), which changes the meaning of the β^1_{ik} coefficient to be each worker's initial starting hourly wage rather than their starting weekly wage.

[48] X-fold cross validation randomly partitions the worker's wage history into X distinct subsets. Each subset then serves as a holdout group that evaluates the prediction quality of the estimated wage changes generated using the other four subsets. The optimal λ_{ik} penalty parameter is chosen to maximize the prediction quality on the five hold-out subsets.

[49] This Lasso procedure is very similar in spirit to the structural break identification procedure that Gottschalk (2005) adapted from Bai and Perron (1998) in order to correct for measurement error in self-reported wages from survey data. Barattieri, Basu and Gottschalk (2014) further improve upon Gottschalk's method by explicitly accounting for Type I and Type II errors in the error correction process.

changes in workers' weekly earnings will oftentimes come from changes in their base wages, the Lasso procedure will also pick up persistent changes in workers' average weekly hours worked (e.g. going from part to full-time) or persistent changes in their variable compensation (e.g. a permanent change in the sales commission rate), which will mistakenly be attributed to changes in a worker's persistent base wage.

By penalizing non-zero coefficients based on their absolute distance from zero, the Lasso estimation procedure generates attenuation bias in the coefficient estimates. Thus, the fourth and final step of the wage estimation process addresses this bias by estimating a standard post-Lasso OLS regression model for every worker-firm job spell that only includes the variables with non-zero coefficients selected by the Lasso procedure in Step 3. The resulting coefficient estimates from the post-Lasso OLS regression serve as my estimates of each worker's persistent nominal wage changes.

C.1 Quality Evaluation of Post-Lasso Estimated Nominal Wage Changes

Because I do not observe workers' true base wages, it is difficult to validate this post-Lasso estimation procedure. That said, I evaluate the quality of the post-Lasso estimation procedure in two ways. First, in the four states with hours-paid data from 2011:Q1 to 2018:Q1, I evaluate how often the post-Lasso procedure identifies wage changes for workers with hourly earnings at or near the old minimum wage in quarters in which the state changes its minimum wage. Second, I compare the frequency of quarterly wage raises identified by the post-Lasso procedure relative to the frequency of base wage changes that Grigsby, Hurst and Yildirmaz (2019) identify from administrative payroll records.

For the evaluation of minimum-wage workers' post-Lasso estimated wage changes, I begin by identifying quarters in which a state changed its minimum wage. After constructing each worker's average hourly earnings in a given quarter by dividing their reported nominal earnings by their hours paid, I include in the sample all workers in a state whose hourly earnings in $t-2$ (where t is the quarter of the state's minimum wage change) were between the old and the new minimum wage. The Lasso estimation procedure identifies that 55.0% of these minimum-wage workers received a wage raise in the quarter of the state's minimum wage change. An additional 33.3% of minimum wage workers are identified as receiving a wage change in the quarter immediately before the minimum wage change, but approximately half of these are workers who are also identified as receiving a wage change in the state's minimum wage change quarter.[50] Thus, I find the post-Lasso procedure

[50]The post-Lasso procedure may identify a worker as receiving wage changes in two, back-to-back quarters if the

identifies wage changes for 70.0% of minimum-wage workers in the quarter of or immediately before a state's minimum wage change. Thus, for minimum wage workers in these four states, 30.0% is a reasonable estimate of the Lasso procedure's Type II error rate (the failure to detect a wage change when there is a change). In the quarter immediately after the state's minimum wage change takes effect, the Lasso estimation procedure identifies a nominal wage change for 14.7% of workers. This provides an upper-bound estimate of the Type I error rate (detecting a wage change when there is none), since some minimum wage workers may have received both a raise in the state's minimum wage change quarter and an additional wage change in the quarter immediately following the minimum wage change.

Most studies of nominal wage rigidity examine annual changes in workers' wages. This paper, on the other hand, focuses on quarter-over-quarter wage changes because the proposed quasi-experiment requires estimates of nominal wage changes at a sub-annual frequency to identify calendar quarters in which firms tend to raise wages. Thus, for my primary sample, I compare the results of the post-Lasso procedure with the two studies that report quarterly nominal wage adjustment frequencies using U.S. data: Barattieri, Basu and Gottschalk (2014), which uses the SIPP, and Grigsby, Hurst and Yildirmaz (2019) (GHY), which uses ADP administrative payroll data. Table 1 reports the frequency of nominal wage raises, freezes, and cuts at both a quarterly and annual frequency. GHY serves as the best benchmark for my post-Lasso persistent wage change estimates because they use administrative data from a large sample of U.S. firms' payroll records for which they observe the workers' true base wages. The only downside to using their results as a benchmark is that their sample includes only firms with 50+ workers. Since smaller firms are less likely to raise workers' wages, GHY overestimates the frequency of nominal wage raises in the broader population (and vice-versa for nominal wage freezes).

GHY find that 18.5 percent of workers at 50+ employee firms receive a nominal wage raise in any given quarter. The post-Lasso procedure identifies 26.5% fewer nominal wage raises, estimating that only 13.6 percent of workers receive a nominal raise each quarter. This difference is similar to the 30% Type II error rate upper bound from the earlier analysis of minimum-wage workers. Although it is not clear how much of the difference is due to GHY's exclusion of small firms (small firms employed 28.2% of workers in 2014 according to the Census Bureau's Quarterly Workforce

true wage change occurred in the middle of the first quarter. In this case, many of these workers with back-to-back estimated wage changes would have received their true wage change in the month or two immediately before the mandated minimum wage change.

Indicators), I believe that the post-Lasso estimation procedure fails to identify a non-trivial share of nominal wage raises.

Although this significant Type II error rate would be troubling in many contexts, it is less so for my proposed quasi-experiment. The quasi-experiment relies on aggregating the post-Lasso estimated wage changes to a firm level and at a calendar quarter frequency. Given a Type II error rate of approximately 30%, this is equivalent to constructing measures of firms' historical nominal raise patterns based on a sample of 70% of the full set of workers' nominal raises. If the post-Lasso procedure fails to identify true wage changes at random, or if the non-randomness of this wage change identification is small relative to the underlying wage change patterns, then the measurement error in my estimates of firms' historical raise patterns should be approximately classical in nature. For the quasi-experiment, I either directly account for this measurement error in my estimates or examine the robustness of the results to differing levels of measurement error.

D Measures of Wage Compensation

For every worker i, firm k, and quarter t combination, the LEHD data set provides either: i) quarterly earnings (primary sample), or ii) both quarterly earnings (y^Q_{ikt}) and quarterly hours paid (h^Q_{ikt}) (secondary sample). For simplicity, I begin by describing the various measures of wage compensation that can be constructed when quarterly hours paid is observed.

The literature on nominal wage rigidity has tended to focus on rigidity in workers' base wage (w_{ikt}). With the LEHD data, however, I instead observe workers' quarterly-averaged hourly earnings ($\bar{y}^H_{ikt}$, hereafter hourly earnings. Two recent working papers, Kurmann and McEntarfer (2019) and Jardim, Solon and Vigdor (2019), use administrative UI records data to explore the degree of nominal rigidity in hourly earnings and find hourly earnings are much less rigid than base wages.[51] Thus, it will be useful to deconstruct the relationship between these two measures of wage compensation. First, note that the quarterly earnings measure can be decomposed as:

$$y^Q_{ikt} = w_{ikt}\left(n_{ikt}\bar{h}^W_{ikt} + \frac{1}{2}n_{ikt}\bar{h}^o_{ikt}\right)v_{ikt}\epsilon_{ikt} \tag{32}$$

where n_{ikt} is the number of payroll weeks in quarter t, $\bar{h}^W_{ikt}$ is the average number of hours worker

[51] This finding of greater nominal wage flexibility in worker earnings (relative to the findings from the survey literature and the administrative payroll records) is echoed in the survey of Elsby and Solon (2019). They consolidated the findings of many of the more recent international studies of nominal wage rigidity and argued that about 15-25% of workers receive year-over-year nominal wage cuts.

per week, $\bar{h}^{o}_{ikt}$ is the average number of overtime hours worked per week, v_{ikt} is any non-overtime variable compensation paid in period t, and ϵ_{ikt} captures measurement error (e.g. the rounding of hours worked to integer digits or order of magnitude errors in hours worked). As I show in Appendix B, including the number of payroll weeks in the quarter proves to be quite useful since, depending on the payroll schedule in effect at the firm, the number of payday weeks can fluctuate among 12, 13, or 14 weeks from one quarter to the next. This results in substantial variation in quarterly earnings (±15%) simply because of payroll schedules.

Since overtime pay only applies to hours worked in excess of 40 hours per week, I will approximate total quarterly overtime hours as:

$$n_{ikt}\bar{h}^{o}_{ikt} = \max\left[0, n_{ikt}\left(\bar{h}^{W}_{ikt} - 40\right)\right] \tag{33}$$

This approximation and the decomposition of quarterly earnings in Equation 32 implies that a worker's base wage is related to her hourly earnings as follows:

$$y^{H}_{ikt} = y^{Q}_{ikt}/h^{Q}_{ikt} \approx w_{ikt}\left(1 + \frac{1}{2}\frac{\max\left[0, h^{Q}_{ikt} - 40n_{ikt}\right]}{h^{Q}_{ikt}}\right)\frac{v_{ikt}\epsilon_{ikt}}{h^{Q}_{ikt}} \tag{34}$$

It is evident from this decomposition that the difference in the degree of rigidity between measures of workers' hourly earnings and their base wages could come from three potential sources: overtime compensation ($\max\left[0, h^{Q}_{ikt} - 40n_{ikt}\right]$), non-overtime variable compensation (v_{ikt}), or measurement error (ϵ_{ikt}). As to the relevance of these three sources of variation for measuring the true rigidity in wage compensation, I discount the importance fluctuations in hourly earnings caused by measurement error and overtime compensation. Fluctuations in hourly earnings caused by measurement error are simply spurious. Fluctuations in hourly earnings due to changes in overtime compensation are unrelated to the persistence of the worker's base wage, but instead reflect a temporary change in the worker's utilization. Thus, it will be useful to explore the relative importance of each of these three sources in the degree of measured rigidity in hourly earnings.

Table 11 shows the proportion of quarter-over-quarter ($\ln(\bar{y}^{H}_{ikt}) - \ln(\bar{y}^{H}_{ikt-1})$) and four-quarter ($\ln(\bar{y}^{H}_{ikt}) - \ln(\bar{y}^{H}_{ikt-4})$) raises / freezes/ cuts in log hourly earnings for individuals who are employed for the full-quarter in both periods (i.e. they were employed at both the start and the end of the quarter at the same firm). The log hourly earnings exhibits significant variability, with only 5.0%

of workers having the same hourly earnings from one quarter to the next.

D.1 Measurement Error: Rounding in Hours Paid

Although I cannot identify all instances of measurement error in hourly earnings changes, I can control for changes in hourly earnings that may be due to state unemployment insurance agencies' instructions that firms should report hours paid after rounding them to whole numbers. This instruction has significant implications for the frequency of wage freezes. To measure the impact of this rounding rule, I set to zero change all observed changes in log hourly earnings that could be due to the reported hours paid being misreported by ± 1 in the current and/or the lagged quarter. This provides an upper-bound on the degree to which flexibility in hourly earnings could be due to measurement error from rounding hours paid to whole numbers. I find that resetting to zero any hourly earnings change that could be due to hours-rounding causes the frequency of quarter-over-quarter wage freezes to increase more than four-fold, from 5.0% to 22.2%.

D.2 Overtime Compensation

Although the hours paid reported in the LEHD data set makes no distinction between overtime hours and regular pay hours, I use the following method to identify earnings changes that could be due to overtime pay. First, if a worker is reported as having worked 480 or fewer hours in the quarter ($h^Q_{ikt} \leq 480$), then I do not consider whether they worked any overtime because even with the fewest number of payday weeks (12), they could still have attained this number of hours without working any overtime.[52] Focusing, instead, on quarters in which a worker had 481 or more hours paid ($h^Q_{ikt} > 480$), I consider three alternative numbers of payday weeks ($n_{ikt} \in (12, 13, 14)$). Since overtime hours can be approximated as $\max \left[0, h^Q_{ikt} - 40n_{ikt}\right]$, each of these three scenarios corresponds to a different number of overtime hours worked. I conclude that overtime hours have been worked if a particular overtime adjustment would result in the hourly pay in period t being within 3 cents of the hourly earnings in at least three of the surrounding four quarters (allowing for adjustments to the hourly earnings in the surrounding quarters for overtime pay).

D.3 Variable Compensation: Annual Bonuses

While a worker's non-overtime variable compensation, v_{ikt}, can include tips, commissions, and bonuses, I describe here a method for estimating only one form of variable compensation: annual

[52] This assumption misses some overtime hours since overtime hours are calculated on a weekly basis. Thus, it is possible for a worker to work more than 40 hours in one week (thus receiving overtime pay) and fewer in another week of the same quarter, and yet still have fewer than 481 hours of pay in the quarter.

bonuses. An annual bonus that occurs in a particular quarter will appear as a single-quarter spike in hourly earnings. Thus, I identify quarters in which a worker received an annual bonus as any quarter in which $y^H_{ikt} > \max\left[y^H_{ikt-1}, y^H_{ikt+1}\right]$. For any such period, I construct an estimate of the bonus as:

$$\hat{b}_{ikt} = \frac{y^H_{ikt}}{\max\left[y^H_{ikt-1}, y^H_{ikt+1}\right]} h^Q_{ikt} = \frac{w_{ikt}\left(1 + \frac{1}{2}\frac{\bar{h}^o_{ikt}}{40}\right)\frac{\epsilon_{ikt}}{h^Q_{ikt}}}{\max\limits_{s=t-1,t+1} w_{iks}\left(1 + \frac{1}{2}\frac{\bar{h}^o_{iks}}{40}\right)\frac{\epsilon_{iks}}{h^Q_{iks}}} b_{ikt} \tag{35}$$

In cases where there is no measurement error or overtime hours worked in either the annual bonus period or the comparison period and the base wage is constant across periods, then the estimated $\hat{b}_{ikt}$ is an accurate measure of the true annual bonus, b_{ikt}. When I exclude the estimated annual bonuses from the measure of hourly earnings and adjust potential hours rounding errors to zero, the frequency of quarter-over-quarter freezes in log hourly earnings increases to 45.3% from 5% in the raw hourly earnings measure and from 22.2% in the hourly earnings that exclude potential rounding errors.

Table 11: Hourly Earnings Change Comparison

Quarter-over-Quarter Nominal Log Hourly Earnings Change	Source	Period	Raise	Freeze	Cut
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%

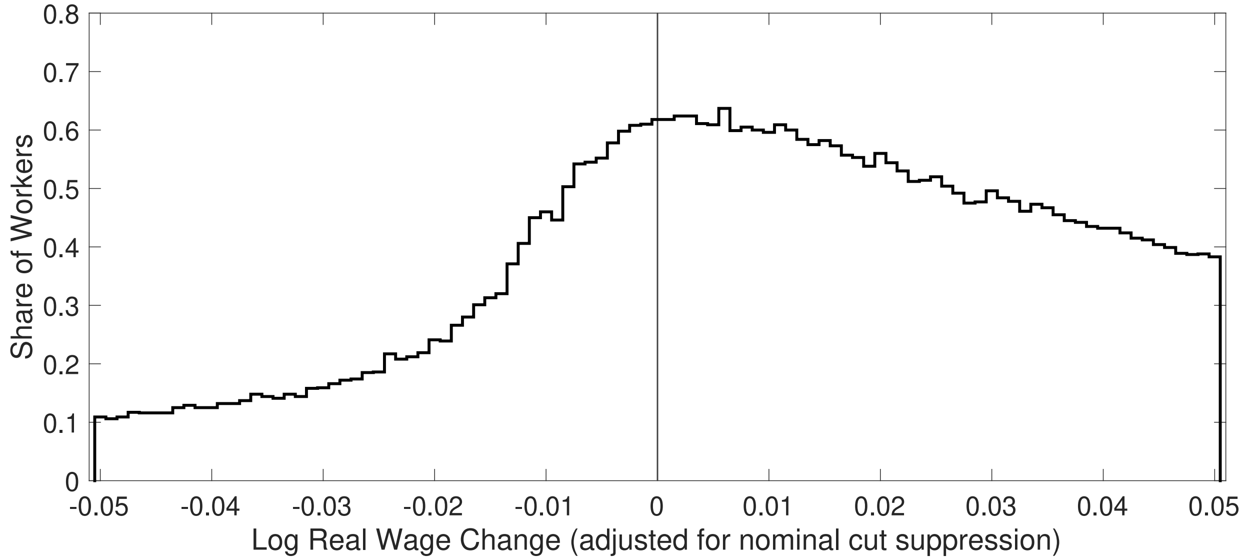
Note: "Raw" indicates log change calculated from the original quarterly earnings and hours paid. "Rounding Adj" sets to zero any nominal change that could be explained by adjusting the hours paid by ±1 hour to account for potential rounding errors. "Bonus & Rounding Adj" first smooths bonuses in the 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.

E RD Tests for Downward Real and Nominal Rigidity

To more formally test for the existence of a discontinuity at zero nominal change, I use a standard regression discontinuity specification to test for a sharp break in the proportion of workers receiving nominal wage changes immediately below versus above zero nominal change. (I exclude nominal wage freezes from this analysis because DNWR implies a discontinuity in realized nominal wage changes at zero nominal change.) As shown in Table 12, using a variety of polynomials in the running variable (nominal wage change) and bandwidths around zero nominal change, I always find a large and statistically significant break in the histogram of nominal wage changes at zero. This finding of a sharp break in the distribution of nominal wage changes at zero nominal change is consistent with the extensive literature on downward nominal wage rigidity.

I also evaluate whether there is any downward real rigidity after taking into account the downward nominal rigidity. This is of particular interest because the "fair-wage" theory of efficiency wages proposed by Akerlof and Yellen (1990) implies that workers are reluctant to accept wages that are below some reference wage. Any formulations of the "fair-wage" efficiency wage theory with the reference wage denoted in real terms would imply that there should be a discontinuity in the real wage change distribution at zero real wage change. To determine whether there is any downward real wage rigidity after taking into account the nominal wage rigidity, I identify all instances where a worker's nominal wage changes, and then I use the change in the Employment Cost Index between the current period and the period that the worker last received a wage change to calculate the real wage change. The resulting histogram of real wage changes in 0.1 log point bins is shown in Figure 6. Unlike the nominal wage change distribution, the histogram exhibits no apparent discontinuity at zero real wage change. As shown in Table 13, estimating a similar set of regression discontinuity models with various bandwidths and polynomials in the running variable delivers ambiguous results, confirming that there is no strong evidence of a discontinuity in the real wage change distribution at zero, and thus it is unlikely that there is any downward real wage rigidity after accounting for downward nominal wage rigidity.

Figure 6: Histogram of Persistent Real Wage Changes at Quarterly Frequency Near Zero



Notes: Frequency of post-Lasso estimated real persistent wage change grouped into 0.1 log point bins between -5.0% and 5.0%. The real magnitude of the change is calculated as the nominal change since the workers' last wage change and then deflated using the BLS Employment Cost Index (ECI). U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

Table 12: Regression Discontinuity Test at Zero Nominal Wage Change

	Nominal Wage Change Bandwidth Window		
Polynomial Order	[-1.5%, 1.5%]	[-2.0%, 2.0%]	[-3.0%, 3.0%]
First	2.3*	1.9*	**3.0**
	(0.90)	(0.77)	(0.60)
Second	**4.9**	**3.9**	1.9*
	(0.79)	(0.88)	(0.93)
Third		**5.5**	*3.8*
		(1.10)	(1.13)

Note: Outcome variable is the share of workers with a nominal wage change within each 0.1 percentile bin of the nominal wage change distribution. The percentile bins are constructed from the post-Lasso estimation using hours-paid data from the secondary sample for each quarter from 2011:III to 2017:IV. The RD specification allows for distinct polynomials in the nominal wage above and below zero nominal change. **Bold**, *Italics*, and * indicate statistical significance at the 0.1%, 1.0%, 5.0% levels respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

Table 13: Regression Discontinuity Test at Zero Real Wage Change

Polynomial Order	Real Wage Change Bandwidth Window [-1.5%, 1.5%]	[-2.0%, 2.0%]	[-3.0%, 3.0%]
First	-3.4*	-2.7*	2.4
	(1.27)	(1.01)	(1.36)
Second	-0.2	-2.9	**-5.9**
	(1.56)	(1.57)	(1.4)
Third		1.0	-2.4
		(1.88)	(1.57)

Note: Outcome variable is the share of workers with a real wage change within each 0.1 percentile bin of the real wage change distribution (conditional on a nominal wage change). The real wage change distribution includes all nominal wage changes that are then converted into the real wage change using the change in the Employment Cost Index since the worker's last wage change. The percentile bins are constructed from the post-Lasso estimation using hours-paid data from the secondary sample for each quarter from 2011:III to 2017:IV. The RD specification allows for distinct polynomials in the real wage above and below zero real change. **Bold**, *Italics*, and * indicate statistical significance at the 0.1%, 1.0%, 5.0% levels respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

F DNWR Suppresses Real Wage Changes

Given the finding that workers' persistent wage changes exhibit downward nominal wage rigidity, a natural question is how many wage changes are suppressed when the change requires a nominal wage cut. This is a useful empirical moment for calibrating models with downward nominal wage rigidity. To estimate this suppression of real wage changes caused by downward nominal wage rigidity, I employ a variant of the method proposed by Kahn (1997) for measuring the effect of downward nominal rigidity on the wage change distribution.[53]

Kahn's method compares the frequency of similar magnitude real wage changes across different periods, distinguishing between when the same real change corresponds to a positive versus negative nominal change. I begin by generating histograms of the nominal wage change distribution from different periods. The critical assumption of Kahn's method is that the modal (or in her case median) nominal wage change in each period corresponds to the same optimal modal "real" wage change. Under this assumption, absent any rigidities or frictions, the proportion of wage changes in the histogram bins that are the same distance r from the period-specific modal nominal wage change bin should be the same across all periods.

To measure the degree to which DNWR suppresses real wage changes, I calculate p_{rt} - the proportion of all wage changes observed in period t (including nominal wage freezes) that fall into the r-distance bin from the modal nominal wage change bin for period t (where the distances are in 0.1 log points). If DNWR suppresses real wage changes, then, when a given r-distance bin requires a nominal wage cut, we should expect the proportion of wage changes in that bin to fall by some percent. Thus, I estimate the following regression model:

$$\ln(p_{rt}) = \sum_{x=-5}^{5} \alpha_x d_{rt}^x + \beta_1 d^{\Delta^{w-}} + \beta_2 d^{\Delta^{w,small+}} + \epsilon_{rt} \tag{36}$$

d_{rt}^x is an indicator variable equal to one if $x = r$, which captures the assumption that, absent

[53]There are two main differences between Kahn's proposed method and what I do. First, she proposes to use the median of the full wage change distribution, whereas I use the mode of the non-zero changes. I do this because the infrequency of wage changes would mean that the median wage change is always zero (at both quarterly and annual frequencies). Using the mode of the non-zero changes provides a more consistent "real" wage change measure since the median of the non-zero changes will change significantly depending on the share of wage changes that are frozen. Second, she includes in the regression model described in Equation 36 the zero nominal change bin in each period, along with a build up from the suppressed bins with nominal wage cuts. Including the zero nominal change bin with this build up imposes a restriction that the suppressed nominal wage changes are necessarily wage freezes, whereas excluding the zero nominal change bin relaxes this restriction. By relaxing this assumption, I can estimate the relationship using OLS with a log specification as opposed to requiring a non-linear estimation procedure.

rigidities and frictions, the proportion of nominal wage changes in the r distance bin should be constant over time. $d^{\Delta^{w-}}$ is an indicator variable equal to one if the r-distance bin in period t corresponds to a nominal wage cut, which captures the effect of downward nominal wage rigidity. And $d^{\Delta^{w,small+}}$ is an indicator variable equal to one if the r-distance bin in period t corresponds to a small positive nominal raise (wage change bins between +0.1 and +0.9 log points), which identifies if small changes are suppressed. For this regression, I use the secondary LEHD sample (which enables me to use hours-paid data in the post-Lasso wage change estimation procedure). I calculate the proportion of nominal wage changes that fall into 0.1 log point bins of the quarterly nominal wage change distributions for each quarter from 2011:Q3 through 2017:Q3.

As shown in Table 14, I find that the proportion of wage changes in a given real change bin falls by 55% when the change requires a nominal wage cut versus a nominal wage raise. For comparison purposes, this estimate is slightly above Kahn's estimate using the PSID survey data that, when a given real change requires a nominal wage cut, DNWR suppresses 47.3% of hourly workers' wage changes. The estimate is even further above Kahn's estimate of 38.1% suppression of salaried workers' wage changes, but within the 95% confidence interval of her estimate.

The finding that the likelihood of observing a real wage change falls significantly when the wage change requires a nominal cut has two implications for economic models that rely on assumptions about the wage adjustment process. First, that such a large share of wage changes are suppressed by DNWR lends empirical support for the various models that examine the role of DNWR in explaining the asymmetric response of employment and output to contractionary versus expansionary shocks (Kim and Ruge-Murcia (2009); Schmitt-Grohé and Uribe (2016); Evans (2018); Mineyama (2018), Dupraz, Nakamura and Steinsson (2019), and Chodorow-Reich and Wieland (forthcoming)).

Second, Calvo (1983) proposed modeling nominal wage rigidity as the random arrival of opportunities to adjust wages. One testable implication of the Calvo wage adjustment process (and the staggered wage adjustment process proposed by Taylor (1980)) is that the frequency of wage adjustments should not respond to the aggregate state. This implication no longer holds if the wage adjustment process is modified such that the opportunity to cut a worker's nominal wage arrives less frequently than the opportunity to raise the worker's wage. With this minor modification, the frequency of wage adjustments will fall in response to negative aggregate shocks because a greater share of jobs will have an optimal wage that requires a nominal wage cut. Since nominal wage cuts are even less likely to occur relative to nominal wage raises, the frequency of wage adjustment will decrease disproportionately for a negative shock versus a similar magnitude positive shock - thus making the "time-dependent" Calvo and Taylor wage setting processes also state-dependent.

Table 14 also indicates that the likelihood of observing a given real change rises when it requires a small positive nominal change (versus a large positive nominal change). This has two implications for models of the wage adjustment process. First, this finding is consistent with Elsby (2009), which argues that DNWR would generate downward compression in the distribution of positive wage changes since DNWR makes it harder to reverse wage raises. Second, this finding runs counter to the adjustment cost model of nominal wage changes proposed by Rotemberg (1982). If nominal wage changes incur adjustment costs (which could explain the Taylor-style staggering of nominal wage changes), then small nominal wage changes that are close to zero should be suppressed. Kahn's estimation procedure allows for the testing of the adjustment cost theory of wage changes by checking whether the proportion of wage changes that fall into a particular real change bin falls when that real change bin corresponds to a small positive versus a large positive nominal change. The results of the post-Lasso estimation procedure are the exact opposite of what we would expect with an adjustment cost model - specifically there are more wage changes in a given real bin when the wage change requires a small nominal raise versus a larger nominal raise. This finding is unlikely to be an artifact of the post-Lasso estimation procedure because the post-Lasso estimation procedure penalizes small wage changes with little explanatory power for a worker's subsequent nominal wage - which implies that the post-Lasso is more likely to under-report small nominal wage changes.

Table 14: Suppression of Wage Changes Due to Downward Nominal Wage Rigidity

Log Proportion of Nominal Wage Changes in 0.1 Percentile Bin		
	(1)	(2)
Nominal Cut	**-0.56**	**-0.79**
	(0.06)	(0.07)
Small Nominal Raise [0.1,1.0]	*0.12*	*0.12*
	(0.04)	(0.04)
Small Nominal Cut [-1.0,-0.1]		**0.24**
		(0.04)
R-Squared	0.958	0.958
Observations	12,000	12,000

Note: Outcome variable is the log proportion of wage changes in a given nominal wage change bin that is r-distance from the modal nominal wage change bin (excluding wage freezes). Distinct nominal wage change distributions are generated for every quarter between 2011:Q3 and 2017:Q3 and all 0.1 percentage point bins between -5.0% and 5.0% (excluding 0.0) are included in the regression sample. Coefficient estimates correspond to the log change in the proportion of wage changes that fall within a given r-distance bin when the change requires a nominal cut, small nominal raise, or small nominal cut. **Bold** and *Italics* indicate statistical significance at the 0.1% and 1.0% levels respectively. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0069-CED-20190725.

G Empirical Analysis: Extensions and Robustness

G.1 DiD: Endogeneity of Revenue Change

Two concerns prohibit a causal interpretation of this estimate. 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 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 worse results in the first three quarters of the year. If the variation in firm's 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 somewhat by the fact that the Q4-raising firms did not exhibit 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 the fact that exogenous negative revenue shocks in 2008:Q4 forced both Q2 and Q4-raising firms to destroy jobs. This job destruction lowered employment levels at the 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 the Q2-raising firms. This weakens the estimated correlation 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 the Q2-raising firms' actual differential sensitivity to the fundamental negative revenue shocks in 2008:Q4.

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

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 a combination of measurement error in real wages and persistent unobserved confounders affecting both past wages and current-period job destruction.

The fact that I construct the firm's real wage bill from worker's nominal wages estimated using the post-Lasso procedure 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 the 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 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 the coefficient estimates.

G.3 Alternative IV Results

In the baseline instrumental variable estiamtion described in Section 4.3.1, I include a the historical raise share in calendar quarter Q3 interacted with a 2008:Q4 indicator variable as an instrumental variable to identify exogenous variation in the firms' start-of-quarter real wage bill ratios. It is is possible to argue that this raise share is 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 including 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 (4) and (5) for the employment-weighted estimation models. Columns (3) and (6) reports the IV results when I instead include the $t-1$ raise share in 2008:Q4 as a control variable.

Table 15: 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. **Bold** indicates statistical significance at the 0.1% level. U.S. Census Bureau Disclosure Review Board bypass number DRB-B0073-CED-20190910.

H Proofs and Derivations

H.1 Attenuation Bias in Revenue Change Estimation

Simultaneous equations problem. 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. Unfortunately, the revenue change I observe is not the fundamental residualized revenue shock ($\tilde{\Delta}R^{-^F}$), where:

$$\tilde{\Delta}R^{-}_{kt} = \tilde{\Delta}R^{-^F} + \alpha\tilde{J}D_{kt} + \upsilon \tag{37}$$

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

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

$$\tilde{J}D_{kt} = \beta_1\tilde{\Delta}R^{-^F} + \beta_2 d_{kt}^{Q2\ raiser}\tilde{\Delta}R^{-^F} + \epsilon \tag{38}$$

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 $\beta_2 \geq 0$, which means that 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, but instead, because of the simultaneity bias, I estimate:

$$\tilde{J}D_{kt} = \frac{\beta_1 + \beta_2 d_{kt}^{Q2\ raiser}}{1 + \beta_1\alpha + \beta_2\alpha d_{kt}^{Q2\ raiser}}\tilde{\Delta}R^{-}_{kt} + \zeta_{kt} \tag{39}$$

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

$$\hat{\beta}_1^{DiD} = \frac{\beta_1}{1 + \beta_1\alpha} \tag{40}$$

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

$$\tilde{J}D_{kt}^{Q2} = \frac{\beta_1 + \beta_2}{1 + \beta_1\alpha + \beta_2\alpha}\tilde{\Delta}R_{kt}^{-} - \hat{\beta}_1^{DiD}\tilde{\Delta}R_{kt}^{-} + \zeta_{kt} \tag{41}$$

Accordingly,

$$\hat{\beta}_2^{DiD} = \frac{\beta_2}{(1 + \beta_1\alpha + \beta_2\alpha)(1 + \beta_1\alpha)} \tag{42}$$

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