Downward Nominal Wage Rigidity's Effect On Employment Through Job Destruction: Quasi-experimental Evidence From The Great Recession [*]

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January 21, 2021

Abstract

This paper provides quasi-experimental evidence that downward nominal wage rigidity affects firms' employment through the job destruction margin. I identify quasi-random variation in firms' exposure to downward nominal wage rigidity by combining (i) the unanticipated nature of the financial collapse in the third quarter of 2008 and (ii) seasonal differences in the timing of firms' annual nominal wage adjustment. I show that downward nominal wage rigidity is an important driver of cyclical job losses, as it can account for at least 23% of the spike in aggregate job destruction that occurred at the onset of the 2008 financial collapse.

JEL Codes: E12, E24, J23, J31, J63

Word Count: 99 (Abstract); 7,295 (Body); 4,330 (Appendix)

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 the following: Why do employment and output exhibit asymmetric fluctuations over the business cycle? Why does the effectiveness of contractionary monetary policy differ from that of expansionary monetary policy? What is the Federal Reserve's optimal inflation target? Despite the importance of these questions and the central role of DNWR in many New Keynesian dynamic stochastic general equilibrium (DSGE) models and labor search-and-matching models, there is remarkably little empirical evidence whether DNWR causes firms to destroy jobs (or, more generally, change their employment allocations).

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

More specifically, 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 financial collapse, firms that historically tended to raise their workers' wages in the fourth calendar quarter ("Q4-raising firms") could respond to the unanticipated financial collapse by freezing 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 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 job destruction rate at Q2-raising firms relative to otherwise similar Q4-raising firms.

For the quasi-experiment, I use a 10% random sample of firms from 30 states in the U.S. Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) data set — an employer-employee

linked administrative data set covering approximately 96% of employment in each state. Using the set of machine learning methods described in Murray (2020), I extract estimates of the timing and magnitude of each worker's unobserved base wage changes from the worker's observed quarterly earnings. For each firm, I then use the estimated persistent wage changes of the firm's employees to identify the calendar quarter in which the firm historically tended to raise its workers' nominal wages (a firm's "typical raise quarter").

I find that the job destruction rate at Q2-raising firms increased by 36% in 2008:Q4, nearly double the 19% increase in the job destruction rate at Q4-raising firms (which had less exposure to DNWR). I estimate that, without the exposure to DNWR generated by firms' seasonal wage raise patterns, U.S. firms' real wage bills in 2008:Q4 would have been 1.1% lower and the increase in the job destruction rate in 2008:Q4 would have been 23% smaller. Because seasonal wage raise patterns were not the sole source of firms' exposure to DNWR, this estimate serves as a lower-bound of DNWR's effect on job destruction in 2008:Q4.

The key contribution of this paper is to the empirical literature examining causal links between DNWR and employment. A number of studies have explored the relationship between DNWR and employment at more aggregate levels, including at the level of countries (Bernanke and Carey (1996)), localities within a monetary union (Fehr and Goette (2005); Ridder and Pfajfar (2017)), industries (Pischke (2018)), and villages (Kaur (2019)). Establishing a causal link between DNWR and employment, however, requires a firm-level analysis because that is the level at which the DNWR constraint may bind. Thus, this paper's empirical contribution is most closely related to Card (1990), Kurmann and McEntarfer (2019), and Ehrlich and Montes (2020) — each of which uses firm-level data to explore the link between nominal wage rigidity and employment. Even with firm-level data, the challenge remains to identify variation in firms' exposure to DNWR that is exogenous with respect to the unobserved factors affecting the firms' employment decisions. Identifying such variation is difficult because firms' wage setting and employment decisions are forward looking and almost inextricably intertwined.

The quasi-experimental evidence presented in this paper contributes in two ways to the sparse empirical literature on the causal effect of DNWR on firm-level employment. First, the identification strategy employed by this paper applies to nearly all firms, not just unionized firms (the focus of Card (1990)), firms with high historical levels of nominal wage rigidity (Kurmann and McEntarfer (2019)), or firms in industries and regions with greater incidences of DNWR (Ehrlich and Montes

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(2020)). Because I apply this strategy to a representative sample of U.S firms, my empirical results can be interpreted as the average treatment effect of DNWR on job destruction in 2008:Q4 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 set of identifying assumptions different from those of Kurmann and McEntarfer (2019) and Ehrlich and Montes (2020). A causal interpretation of the relationship between DNWR and employment documented in these earlier studies requires that no confounding variables affect both a firm's employment decisions and either: (i) the historical incidence of DNWR at the firm (Kurmann and McEntarfer (2019)) or (ii) the incidence of DNWR in the firm's industry-region (Ehrlich and Montes (2020)). Thus, negative shocks both affecting the incidence of wage rigidity (either historically or at the industry-region level) and affecting current-period business conditions threaten the validity of these studies' identifying assumptions. By focusing on the period immediately following an **unanticipated** negative shock, the identifying assumption of the quasi-experiment presented in this paper is robust to persistent negative shocks. Specifically, my quasi-experiment assumes that no confounding variables affect both: (i) the calendar quarter in which a firm has historically tended to raise its workers' nominal wages and (ii) the firm's employment decisions when it experienced the unanticipated negative aggregate shock of the financial crisis in September 2008.

This paper also relates to the literature that uses search-and-matching models to study the labor market. As first documented in Hall (2005), this literature has long recognized that incorporating wage rigidities into search-and-matching models of the labor market enables these models to better fit the magnitude of cyclical fluctuations in unemployment. However, this paper's quasi-experimental results call into question two aspects of how these models typically incorporate wage rigidity.

First, when search-and-matching models of the labor market incorporate wage rigidity, the models tend to assume that the wage rigidity affects job creation, but not job destruction (Hall (2005); 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 (2020); Dupraz, Nakamura and Steinsson (2020); Chodorow-Reich and Wieland (2020)). These models' oft-used assumption that job destruction is exogenous to cyclical shocks runs counter to this paper's evidence that DNWR can account for a large portion of the spike in aggregate job destruction that occurred at the onset of the 2008 financial crisis. This empirical finding suggests

that labor search-and-matching models should consider incorporating DNWR in such a manner that the rigidity would affect employment through the job destruction margin.

Second, as incumbent workers' wage rigidity is not relevant for firms' job creation decisions in most search-and-matching models, the literature's focus on hiring and job creation also shifted the search-and-matching literature to emphasize new hires' wage rigidity (Pissarides (2009), Kudlyak (2014)).[1] Given that this paper exploits nominal rigidities in **incumbent** workers' wages to identify an effect of DNWR on job destruction, the search-and-matching literature may benefit from allowing rigidities in incumbent workers' wages to affect employment (and, particularly, job destruction).

This paper also lends support to the literature that incorporates DNWR into New Keynesian DSGE models (Kim and Ruge-Murcia (2009); Fagan and Messina (2009); Schmitt-Grohé and Uribe (2013); Benigno and Ricci (2011); Daly and Hobijn (2014); Abbritti and Fahr (2013); Schmitt-Grohé and Uribe (2016); Schmitt-Grohé and Uribe (2017); Shen and Yang (2018); Eggertsson, Mehrotra and Robbins (2019)). My finding that DNWR causally affects aggregate employment fluctuations through the job destruction margin helps justify a common assumption in these DSGE models — namely that DNWR prevents real wages from falling sufficiently in response to negative shocks — resulting in asymmetric amplification of negative shocks.

Lastly, this paper also makes a minor contribution to the extensive literature documenting the presence and character of nominal wage rigidities.[2] Barattieri, Basu and Gottschalk (2014), Grigsby, Hurst and Yildirmaz (forthcoming), and Murray (2020) show that an individual worker's nominal wage changes tend to occur at annual frequencies — which is consistent with a Taylor-style annual wage adjustment process. Grigsby, Hurst and Yildirmaz (forthcoming) take this finding a step further to show that employees' annual wage adjustments are highly synchronized within a firm. In this paper, I document that the timing of these within-firm synchronized annual wage adjustments are stable from year to year — such that workers are more than twice as likely to receive a nominal

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[1]Four notable exceptions examine how incumbents' wage rigidity can affect employment through efficiency wages (Elsby (2009); Bils, Chang and Kim (2016)), or financial frictions (Schoefer (2016)), or the random arrival of opportunities to adjust workers' wages (Carlsson and Westermark (forthcoming)).

[2]Studies documenting DNWR using the Panel Study of Income Dynamics 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 include Card and Hyslop (1997); Daly and Hobijn (2014); Elsby, Shin and Solon (2016); and Jo (2019). Studies using the Survey of Income and Program Participation include Gottschalk (2005); and Barattieri, Basu and Gottschalk (2014). Studies using the Employment Cost Index (ECI) survey include Lebow, Saks and Wilson (2003); and Fallick, Villar and Wascher (2020). Studies using unemployment insurance administrative data include Kurmann and McEntarfer (2019) and Jardim, Solon and Vigdor (2019). Grigsby, Hurst and Yildirmaz (forthcoming) use ADP payroll data. Hazell and Taska (2020) use job posting data from Burning Glass Technologies.

wage raise in the calendar quarter in which their co-workers have historically tended to receive their nominal wage raises.

2 Identification strategy: Does DNWR destroy jobs?

To determine whether greater exposure to DNWR causes firms to destroy jobs when faced with a negative shock, I require variation in firms' exposure to DNWR that is exogenous with respect to the unobserved factors affecting their employment decisions. For this paper, the exogenous variation in firms' exposure to DNWR stems from cross-firm differences in the timing of an unanticipated negative aggregate shock relative to when in the calendar year the firms have historically tended to raise their workers' nominal wages.

In Section 3.3, I present evidence that, within a firm, workers' nominal wage raises are synchronized to occur at the same time. Furthermore, firms' typical raise quarters are highly stable year over year. This synchronization of employees' annual nominal raises generates a stair-step pattern in a firm's quarterly nominal wage bill. Given positive inflation, the stair-step pattern in a firm's nominal wage bill implies that, over any given four-quarter period, the firm's real wage bill spikes in the typical raise quarter, declines over the next three quarters (as inflation eats away at the fixed nominal wage), and reaches a nadir immediately before the firm's next typical raise quarter. As a result, in any given quarter, firms with more recent typical raise quarters tend to have real wage bills above their annual average real wage bill.

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. By contrast, firms that just experienced their typical raise quarter would have to cut workers' nominal wages to achieve similarly low quarterly real wage bills (relative to these firms' annual average real wage bills). Thus, when an unanticipated negative aggregate shock occurs, firms will have differential exposure to DNWR simply because of differences in their typical raise quarters.[3] If exposure to DNWR has a causal effect on job destruction, then we should expect larger increases in job destruction (relative to each firm's historical seasonal changes in job destruction) at firms that typically raised their employees' nominal wages in the quarter immediately before the unanticipated negative shock.

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[3]If firms anticipate the negative shock, then those firms with typical raise quarters immediately before and after the negative shock will similarly freeze their workers' nominal wages. As a result, when the negative shock is anticipated, firms' exposure to DNWR is unrelated to the timing of the shock relative to the firms' typical raise quarters.

For the quasi-experiment, I assume that the financial collapse that immediately followed Lehman Brothers' bankruptcy in September 2008 qualifies as a large, unanticipated negative shock. Perhaps the best evidence that the financial collapse in September 2008 was unanticipated comes from the substantial forecast revisions of both the Federal Reserve Board's staff and professional economic forecasters. On September 10, just five days before Lehman Brothers declared bankruptcy, the Greenbook economic forecasts prepared by the Board's staff predicted +1.1% annualized gross domestic product (GDP) growth in 2008:Q4. Six weeks later, the Greenbook forecast for GDP growth in 2008:Q4 had fallen more than 2 percentage points to -1.3%. This substantial downward revision in the Greenbook economic forecast was mirrored among professional economic forecasters surveyed by the Federal Reserve Bank of Philadelphia. Between August 8 and November 10, 2008, professional economic forecasters' predictions for the annualized real GDP growth rate in 2008:Q4 fell by 3.8 percentage points, from +0.7% to -2.9%.[4] The fact that forecasts of economic activity fell dramatically immediately following the Lehman Brothers bankruptcy supports my assumption that the negative shock in 2008:Q3 was unanticipated.

3 Data and measurement

3.1 Data

The 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. Workers' quarterly earnings data in the LEHD are derived from firms' mandatory unemployment insurance filings. These earnings data are 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 that allows each individual to be tracked over time and across employers. The LEHD identifies employers at the level of a state employer identification number (SEIN). Firm age and firm size are derived from aggregating all SEINs (potentially across states) that share a common federal employer identification number (EIN). For simplicity, I refer to each SEIN as a firm. Although revenue data are not available in the LEHD data set, each SEIN is linked to an EIN

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[4]This newfound pessimism also extended into longer-term forecasts. The Board's staff lowered their forecast for real GDP growth in 2009 by 2 percentage points in the five weeks following the Lehman Brothers bankruptcy. Similarly, the professional forecasters lowered their predictions for real GDP growth in 2009 from +1.5% in August 2008 to -0.2% in November 2008.

for which I obtain real revenue and real revenue per worker at an annual frequency from the U.S. Census Bureau's revenue-enhanced Longitudinal Business Database (rLBD) (Haltiwanger, Jarmin, Kulick and Penciakova (2019)).

The primary sample used for the quasi-experiment consists of a 10% random sample of SEINs from 30 states covering the period from 1998:Q1 to 2017:Q1.[5] However, when I examine labor market outcomes for individual workers, such as employment-to-nonemployment (EN) or employer-to-employer (EE) transitions, I identify these transitions using an expanded sample that includes 100% of the worker-firm observations in the 30 states. After accurately labeling these transitions between all firms, I then restrict my subsequent analysis to workers employed at the 10% of firms included in the primary sample.[6]

3.2 Measuring workers' base wages

One limitation of the LEHD dataset is that it only reports each worker's quarterly earnings — which are a function of the worker's base wage, the number of hours worked, and any variable compensation (e.g., annual bonuses, commissions, tips, and overtime). Given that I require a measure of workers' base wages, I implement the two-step procedure described in Murray (2020) to extract estimates of workers' unobserved base wage changes from their observed quarterly earnings.

The first step of the procedure identifies and adjusts for fluctuations in workers' quarterly earnings caused by changes in the number of paydays from quarter to quarter.[7] This adjustment for payday-related fluctuations exploits three characteristics of payroll schedules. First, there are a limited number of potential payroll schedules. Second, for each payroll schedule the actual number of paydays in any given quarter can be identified from the calendar. And third, any given firm typically uses the same payroll schedule for all of its employees (although some firms may use two or three different payroll schedules). Murray (2020) presents a clustering algorithm that uses these three characteristics to identify the payroll schedule(s) in operation at each firm. The clustering procedure does so by determining, for each worker at the firm, which of the potential payroll schedules would minimize the variance of the workers' quarter-over-quarter earnings changes if these earnings

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[5]The states included in the primary sample are: California, Colorado, Connecticut, Florida, Georgia, Hawaii, Idaho, Illinois, Indiana, Kansas, Louisiana, Maryland, Maine, Montana, North Carolina, North Dakota, New Jersey, New Mexico, Nevada, Oregon, Pennsylvania, Rhode Island, South Carolina, South Dakota, Tennessee, Texas, Virginia, Washington, Wisconsin, and West Virginia. I chose these 30 states because there are no gaps in reported quarterly earnings for any of these states over the sample period.

[6]Using 100% of the worker-firm observations ensures that these labor market transitions are accurately identified, as otherwise some EE transitions would incorrectly be categorized as EN transitions.

[7]These fluctuations are both large and frequent. For instance, workers who earn a constant weekly salary but are paid bi-weekly typically report fluctuations of ±15.4 log points in their quarterly earnings.

changes were adjusted to account for the number of paydays implied by the potential payroll schedule. The clustering algorithm selects the payroll schedule(s) that minimizes this variance for a large share of workers at the firm.

The second step identifies the base wage of individual workers in each quarter using a post-Lasso procedure to identify structural breaks in the worker's payday-adjusted earnings series. In spirit, this wage estimation method is very similar to the structural break estimation procedure proposed in Gottschalk (2005) to adjust for measurement error in SIPP respondents' reported wages. (Barattieri, Basu and Gottschalk (2014) further refined this procedure.) These studies employed a series of F-tests to identify the timing of structural breaks in respondents' reported wages - where these structural breaks correspond to true wage changes. Similarly, structural breaks in a worker's payday-adjusted quarterly earnings correspond to changes in the worker's base wage (although changes in hours worked or variable compensation that persist for many quarters will also appear as structural breaks). Because the F-test structural break estimation procedure would struggle (computationally) to process the tens of millions of workers from the LEHD dataset, Murray (2020) uses a post-Lasso procedure to identify the structural breaks in workers' quarterly earnings, which correspond to changes in these workers' base wages.

As documented in Murray (2020), the patterns of base wage changes estimated using this two-step procedure are broadly consistent with previous studies examining nominal rigidities in workers' wages. Most critically for the quasi-experiment, the estimated base wage changes exhibit both: (i) significant downward rigidities (less than 5% of workers receive nominal wage cuts year over year) and (ii) annual staggering of workers' wage changes (with the probability of a wage raise spiking at the one-year anniversary of the worker's most recent nominal wage change).

3.3 Measuring firms' typical raise quarters

The annual staggering of workers' wage adjustments, first proposed by Taylor (1980), is a common assumption in macro models. Barattieri, Basu and Gottschalk (2014) were the first to provide empirical evidence that U.S. worker wages followed an annual adjustment pattern — using the SIPP to show that the probability of a wage change spikes 12 months after the worker's most recent nominal wage change. Grigsby, Hurst and Yildirmaz (forthcoming) similarly document a Taylor-style annual staggering of workers' wage changes using administrative wage data from a large payroll processor. They then take this finding a step further to show that, within a firm, workers' annual wage changes tend to be synchronized to occur in the same month of the calendar

year.

The quasi-experiment in this paper, however, requires that: (i) the calendar timing of a firm's synchronized wage changes is stable from one year to the next and (ii) that firms differ as to when in the calendar year they make these synchronized wage adjustments.

I document these two facts using workers estimated base wage changes (as described in Section 3.2). For each firm and quarter, I evaluate whether there is a calendar quarter in which workers have historically tended to receive nominal wage raises (which I define as the 'typical raise quarter'). Specifically, 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 historical 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 $p = 0.25$.)

This procedure identifies typical raise quarters for the firms of 79.6% of all 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 raises.

I find that firms' typical raise quarters are spread throughout the calendar year.[8] The plurality of workers are employed at Q3-raising firms (26%), Q2-raising firms employ the fewest workers (10%), whereas Q1- and Q4-raising firms employ approximately the same share of workers (16% and 15% respectively). Firms with two typical raise quarters employ 12.6% of workers.

Critically for the quasi-experiment, firms' typical raise quarters are stable from one year to the next. I find that a worker is more than twice as likely to receive a nominal wage raise in the calendar quarter in which co-workers have historically tended to receive nominal wage raises at the firm (rising 9.3 percentage points from a baseline probability of 7.3%; see Appendix B for further details on the specification and results). This finding implies that the calendar timing of synchronized wage raises in previous years strongly predicts the timing of wage raises within the current calendar year.

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[8]I find evidence of significant variation in firms' typical raise quarters along the dimensions of industry and firm size. Appendix A provides a detailed breakdown of these differences.

4 Firm-level evidence that exposure to DNWR destroys jobs

In order to evaluate whether DNWR has a causal effect on firms' job destruction, I follow the identification strategy described in Section 2 by estimating a difference-in-differences specification. Given that the Lehman Brothers' bankruptcy and ensuing financial collapse occurred in 2008:Q3, firms that typically raise employees' nominal wages in Q4 compose the control group. Conversely, the "treatment" group consists of the firms with the greatest exposure to DNWR — namely, those firms that have historically tended to raise employees' nominal wages in Q2.[9]

I estimate the following difference-in-differences specification using all firms with identified typical raise quarters in the primary sample (covering 80% of total employment):

$$JD_{kt} = \sum_{q=1}^{4} \alpha_{kt}^{Qq} + D_{kt}^{2008:Q4} \theta^{Q4} + \sum_{q=1}^{3} D_{kt}^{2008:Q4} D_{kt}^{Qq\ \text{Raiser}} \theta^{Qq} + \boldsymbol{X}_{kt} \beta_t + \epsilon_{kt} \tag{1}$$

The outcome variable, JD_{kt}, is firm k's job destruction rate in period t.[10] α_{kt}^{Qq} is a set of firm-specific calendar quarter fixed effects that control for firm-level seasonality in job destruction rates. θ^{Q4} measures the average increase in the job destruction rate of Q4-raising firms in 2008:Q4, as $D_{kt}^{2008:Q4}$ is an indicator variable equal to one if $t =$2008:Q4. Similarly, θ^{Qq} coefficients measure the incremental change in job destruction rates in 2008:Q4 (beyond that of the Q4-raising firms) of firms with typical raise quarters in the qth calendar quarter (because $D_{kt}^{Qq\ \text{Raiser}}$ is a set of indicator variables equal to one if firm k's typical raise quarter is calendar quarter q).

The primary coefficient of interest is θ^{Q2} because this coefficient indicates whether the firms with the greatest exposure to DNWR in 2008:Q4 (Q2-raising firms) destroyed jobs at a higher rate relative to the firms with the least exposure to DNWR (Q4-raising firms). These differences in exposure to DNWR stem from these firms historically tending to raise workers' wages in the

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[9]Firms that typically raise workers' nominal wages in Q1 can also be considered as a distinct treatment group, although they should have less exposure to DNWR relative to the Q2-raising firms as inflation would have had more time to reduce Q1-raising firms' real wage bills. The relative degree of exposure to DNWR for Q3-raising firms is more ambiguous as some Q3-raising firms would have observed the start of the financial collapse in 2008:Q3 before making their decision whether to adjust workers' nominal wages in 2008, thus giving them a degree of nominal wage flexibility similar to that of the Q4-raising control group.

[10]I compute the measure of the firm-level job destruction rate as defined by the Quarterly Workforce Indicators (and first proposed by Davis, Haltiwanger and Schuh (1998)). Specifically,

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

This measure of the rate of change has the advantages of both being symmetric (unlike percent changes) and capable of handling zero values (unlike logs). The measure is bounded between 0 and 2.

calendar quarter immediately before rather than after the unanticipated negative shock of the financial collapse in 2008:Q3.

The difference-in-differences specification also includes a set of control variables, represented by $\boldsymbol{X}_{kt}$. Most critically, I control for industry-by-time fixed effects (which help absorb any industry-specific shocks in 2008:Q4). The set of control variables also includes fixed effects for firm age and firm size.

4.1 Evaluation of pre-trend differences between Q2- and Q4-raising firms

As with any difference-in-differences analysis, the identification of a causal effect hinges on the assumption that the change in Q2- and Q4-raising firms' job destruction rates in 2008:Q4 would have been similar (after accounting for the control variables) absent the differences in the timing of their typical raise quarters relative to the unanticipated financial collapse in 2008:Q3. Because 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 before 2008:Q4 and the relative magnitude of these differences across periods.

Figure 1 plots the $\hat{\theta}^{Q2}$ coefficient estimates that result from estimating the same difference-in-differences specification but examining the difference in job destruction rates between Q2- and Q4-raising firms in each quarter of the sample from 1998 to 2015.

There are two key takeaways from Figure 1. First, there are no economically or statistically significant pre-trends in the differences between Q2- and Q4-raising firms. Second, the magnitude of the difference between Q2- and Q4-raising firms in 2008:Q4 (represented by the black dot) is an extreme outlier relative to the typical magnitude of differences between these two firm types. The magnitude of the 2008:Q4 coefficient estimate is 30% larger in absolute value than the next largest coefficient estimate from any other period and 3.6 times larger than the standard deviation of the coefficient estimates from all periods.[11]

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[11]Despite the absence of pre-trend differences in job destruction rates at Q2- versus Q4-raising firms and the inclusion of industry-by-time fixed effects, it is still a concern that any difference in job destruction rates in 2008:Q4 could be due to differences in the severity of the unanticipated negative shock for Q2- versus Q4-raising firms that remain even after controlling for industry-specific Q4 shocks (rather than the differences in their real wage bills generated by the timing of their historical schedules of nominal wage raises). Appendix C presents an alternative estimation that addresses this concern by using firms' annual revenue changes to show that, in 2008:Q4, job destruction rates at Q2-raising firms rose more (relative to Q4-raising firms) in response to similar magnitude declines in year-over-year annual revenue.

4.2 Effect of DNWR on job destruction in 2008:Q4

Table I shows the results of the difference-in-differences estimation from Equation 1. Model 1 reports the θ^{Qq} estimates without any industry-by-time fixed effects. Model 2 shows these same coefficient estimates with controls for industry-by-time fixed effects. Model 3 similarly includes the industry-by-time fixed effects but uses firm-level employment weights.

First, as expected given the severity of the financial crisis, Q4-raising firms increased their job destruction rates by 1.48 percentage points in 2008:Q4. Second, across all specifications, I find strong evidence that exposure to DNWR increased firms' job destruction. Focusing on the set of firms with the greatest exposure to DNWR, I find that Q2-raising firms increased their job destruction rates by an additional 1.3 percentage points (or 2.0 percentage points when weighting by employment). This finding implies that the firms most constrained by DNWR due to 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.

4.3 Effect of DNWR on job destruction: More layoffs, not less hiring

Exposure to DNWR could generate job destruction through two potential channels. Namely, the decline in employment between the start and end of the quarter could result from more layoffs, less hiring of replacements for retiring and quitting workers, or some combination thereof. In order to evaluate which of these channels drove the large increase in job destruction at firms with greater exposure to DNWR in 2008:Q4, I re-estimate the difference-in-differences specification described in Equation 1, changing the outcome variable to be either the firms' hiring rate or layoff rate.[12]

The results in Table II show that the layoff rate at Q2-raising firms rose by 1.44 percentage points more than at Q4-raising firms. This estimate is statistically significant and similar in magnitude to the 1.30 percentage point rise in the job destruction rate at Q2- versus Q4-raising firms. The estimate for the change in the hiring rate at Q2-raising firms, however, is not statistically different from that of Q4-raising firms; it is large in magnitude, but of the wrong sign to explain a rise in job destruction. Thus, the greater increase in job destruction rates at Q2- versus Q4-raising firms in 2008:Q4 comes from Q2-raising firms laying off more workers (rather than hiring fewer workers).

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

5 Aggregate effects of DNWR on job destruction in 2008:Q4

According to the reasoning laid out in Section 2, if DNWR causally affects a firm's job destruction rate by constraining the firm's ability to cut workers' nominal wages, then the firm will have a higher job destruction rate in response to an unanticipated negative shock if the firm's real wage bill is exogenously above its optimal level. Although observing a firm's optimal real wage bill is impossible, the firm's average four-quarter real wage bill is a reasonable benchmark, as the annual staggering of nominal wage raises implies that firms should set the nominal wage such that the expected average real wage over the year equals its optimal level. Thus, for each firm-quarter observation, I measure the ratio of the firm's real wage bill in the previous period relative to its four-quarter moving average (W_{kt}) as

$$W_{kt} = \frac{\sum_{i \in E_{kt}^{FY}} w_{ikt-1}^r}{\sum_{i \in E_{kt}^{FY}} \sum_{s=1}^{4} w_{ikt-s}^r / 4} \tag{2}$$

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 past four quarters)[13] and w_{ikt-s}^r is the real wage of the workers in period $t - s$ (in 2015:Q1 dollars, computed using the ECI).

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,

$$JD_{kt} = \gamma W_{kt} + \gamma^{2008:Q4} d_{kt}^{2008:Q4} W_{kt} + \boldsymbol{X_{kt}} \beta_t + \epsilon_{kt} \tag{3}$$

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

5.1 Effect of higher real wages from DNWR on job destruction

A simple OLS regression of the model in Equation 3 is unlikely to yield a causal estimate of the effect of having a higher real wage bill in 2008:Q4 because of a combination of measurement error in real wages and persistent unobserved confounders affecting both past wages and current-period job destruction (see Appendix E 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 job destruction rate, I

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[13]Restricting the real wage ratio to include only workers employed by the firm 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.

employ an instrumental variable strategy using the firm's historical pattern of nominal wage raises. As instrumental variables, I construct the share of a firm's nominal wage raises that occurred in each of the past three calendar quarters in previous years (before 2007:Q4). For instance, in 2008:Q4, I construct three instrumental variables for each firm: r^Q_{kt-1}, r^Q_{kt-2}, and r^Q_{kt-3}, which correspond to the historical share of nominal raises occurring in the third, second, and first calendar quarters 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 in the period before 2007:Q4 (p^q_k where q indicates the calendar quarter). Then, I calculate the historical share of nominal raises for each calendar quarter q as:

$$s^q_k = \frac{p^q_k}{\sum_{a=1}^{4} p^a_k} \tag{4}$$

These historical raise share measures for the four calendar quarters sum to one for every firm.[14] Finally, I convert these 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: $r^Q_{kt-1} = s^3_k$, $r^Q_{kt-2} = s^2_k$, and $r^Q_{kt-3} = s^1_k$.

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

$$W_{kt} = \sum_{a=1}^{3} \alpha_a r^Q_{kt-a} + \sum_{a=1}^{3} \alpha_a^{2008:Q4} d^{2008:Q4}_{kt} r^Q_{kt-a} + \boldsymbol{X_{kt}}\beta_t + \nu_{kt} \tag{5}$$

and similarly for $d^{2008:Q4}_{kt} W_{kt}$. Table III reports the results of these first-stage regressions for the set of firms with at least 10 observed nominal raises before 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.[15] There does not

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[14]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.

[15]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 being only 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 recession beginning in January 2008.

appear to be a weak instruments problem for either of the endogenous explanatory variables.

Table IV shows the results of estimating the reduced form relationship of interest from Equation 3 with both OLS and two-stage least squares (2SLS). The 2SLS estimation finds that firms' job destruction rates rose 3.6 percentage points in 2008:Q4 for every 1% that firms' start-of-quarter real wage bill was above their four-quarter average real wage bill.[16]

To give a sense of the aggregate magnitude of this estimate, I consider the counterfactual scenario in which all firms had the nominal wage flexibility of Q4-raising firms in 2008:Q4. This counterfactual is equivalent to using the coefficient estimates from the second-stage of the IV regression to calculate the job destruction rate at firms if all historical raise share values were set to zero (and, thus, the firm is a Q4 raiser). When I do this simple adjustment to eliminate the exposure to DNWR generated by the annual staggering of a firm's nominal raise schedules, I find that the job destruction rate would have risen 23% less in 2008:Q4. I should note that this estimate serves as a lower bound of the effect of DNWR on job destruction in 2008:Q4, as even the less-exposed Q4-raising firms (which serve as the baseline comparison group) had exposure to DNWR.[17]

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 one of the largest unanticipated negative aggregate shock experienced by the United States in at least the past 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 will likely be smaller in magnitude and short-term financing may be 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 to smooth large, unanticipated negative shocks.

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[16]This finding is most likely an underestimate of the effect of DNWR on job destruction as it includes as an instrumental variable the share of raises that historically occurred in Q3. Because some of these Q3-raising firms would have observed the Lehman Brothers' bankruptcy that occurred on September 15, 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 F for the alternative second-stage regression results.

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

6   Summary and concluding remarks

This paper demonstrates that DNWR plays an important causal role in explaining employment fluctuations through the job destruction margin. The paper presents quasi-experimental evidence that in 2008:Q4 the job destruction rate increased nearly twice as much at firms with greater exposure to DNWR because of the timing of their historical raise schedules relative to the unanticipated financial collapse in September 2008. I find that the increase in the aggregate job destruction rate in 2008:Q4 would have been 23% smaller if all firms had had the wage flexibility of firms whose annual raise schedules occurred in the fourth calendar quarter.

This paper leaves unanswered several important questions about the relationship between DNWR and job destruction. First, although I show that greater exposure to DNWR causally increased firms' job destruction rates 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 aggregate job destruction that I identify is large, this estimate serves as only a lower bound for the true causal effect of DNWR on aggregate 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 focus brings into question the external validity of the magnitude of the aggregate effect. While firms experiencing large negative shocks in more "normal" recessions are likely to respond in a similar fashion to the firms in my instrumental variables estimation, there will be fewer such firms if the recession is less severe. As a result, I expect DNWR to generate less aggregate job destruction when the recession is less severe.

There are two important implications of this paper for monetary policy. First, regarding the Federal Reserve's target inflation rate, studies examining the optimal inflation rate have largely ignored the potential for DNWR to inefficiently destroy jobs.[18] Thus, it would be informative to examine whether and how much the optimal rate of inflation changes after incorporating the inefficient job destruction caused by DNWR into a model designed to identify the optimal rate of inflation. One complexity in modeling the effect of DNWR in a high-inflation environment is that the variation in firms' exposure to DNWR that I use in the quasi-experiment could actually be exacerbated in a high-inflation environment because the real wage change over the calendar year

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[18]See Kim and Ruge-Murcia (2009); Coibion, Gorodnichenko and Wieland (2012); Mineyama (2018); and Dupraz, Nakamura and Steinsson (2020)

would be greater.

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

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7 Tables

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

Dependent variable	Job destruction rate		
	(1)	(2)	(3)
2008:Q4 * Q4-raiser	1.48***		
	(0.16)		
	Relative to Q4-raiser		
2008:Q4 * Q1-raiser	0.34	0.24	1.02***
	(0.28)	(0.25)	(0.18)
2008:Q4 * Q2-raiser	2.11***	1.30***	2.01***
	(0.21)	(0.26)	(0.20)
2008:Q4 * Q3-raiser	0.70**	0.26	-0.22
	(0.26)	(0.24)	(0.14)
Firm*calendar quarter fixed effects	Yes	Yes	Yes
Industry*time fixed effects	No	Yes	Yes
Employment weighted	No	No	Yes
Mean job destruction 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 job destruction rate at the SEIN-level. 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 NAICS sector by time. Quarterly LEHD data are from 1999:Q1 to 2014:Q4. Sample consists of only firms with one typical raise quarter (and no more). Robust standard errors are clustered at the SEIN-level. ***, **, 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.

Table II: Differential Q2- versus 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)
Firm*calendar quarter fixed effects	Yes	Yes	Yes	Yes
Industry*time fixed effects	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 variables are the SEIN-level 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 NAICS sector by time. Quarterly LEHD data are from 1999:Q1 to 2014:Q4. Sample consists of only firms with one typical raise quarter (and no more). Robust standard errors are clustered at the SEIN-level. Robust standard errors clustered at the SEIN-level. ***, **, 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.

Table III: 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***	$2\text{x}10^{-4}$	3.91***	$2.2\text{x}10^{-3}$**
	(0.03)	$(1\text{x}10^{-4})$	(0.07)	$(7\text{x}10^{-4})$
2-quarter lag	3.19***	$-3\text{x}10^{-4}$	3.09***	$2.1\text{x}10^{-3}$
	(0.03)	$(2\text{x}10^{-4})$	(0.09)	$(1.4\text{x}10^{-3})$
3-quarter lag	1.56***	$2\text{x}10^{-4}$	1.60***	$4\text{x}10^{-4}$
	(0.02)	$(2\text{x}10^{-4})$	(0.07)	$(1.2\text{x}10^{-3})$
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	No	No	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Industry*time fixed effects	Yes	Yes	Yes	Yes
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 bill ratio. Control variables include firm fixed-effects, as well as fixed effects for two-digit NAICS sector in 2008:Q4, firm age, and firm size. Quarterly LEHD data are from 1999:Q1 to 2014:Q4. Sample consists of only firms with at least 10 raises observed before 2007:Q4. Robust standard errors clustered at the SEIN-level. ***, **, 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-B0073-CED-20190910.

Table IV: Second-Stage: Job Destruction Rate

Dependent variable:		Firm 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	No	No	Yes	Yes
Firm fixed effects	Yes	Yes	Yes	Yes
Industry*time fixed effects	Yes	Yes	Yes	Yes
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 NAICS sector-specific shocks in 2008:Q4. Quarterly LEHD data from 1999:Q1 to 2014:Q4. Sample consists of only firms with at least 10 nominal raises before 2007:Q4. Robust standard errors are clustered at the SEIN-level. ***, **, 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-B0073-CED-20190910.
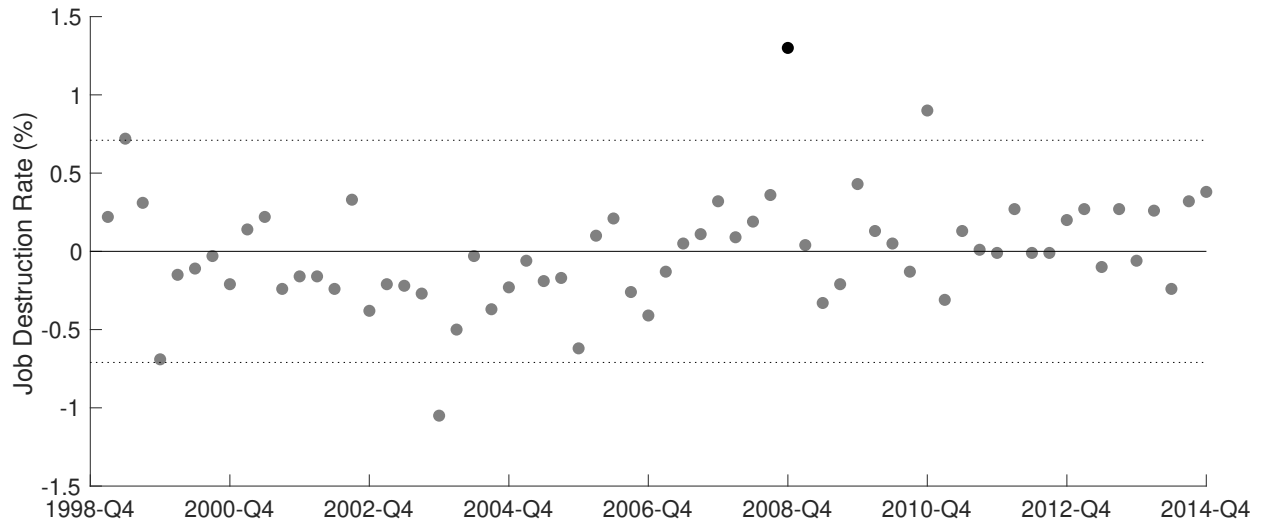
8 Figures

Figure 1: 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 two-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.

A Differences in typical raise quarters by firm characteristics

Table A1 reports the share of start-of-quarter employment in 2008:Q4 for Q2- and Q4-raising firms, broken down by two-digit North American Industry Classification System (NAICS) sector, firm age, and firm size. Along all three dimensions, there are significant differences in the share of Q2- versus Q4-raisers. These differences, particularly by industry, are to be expected because 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).

Table A1: Firm Characteristics by Typical Raise Quarter

	Employment share at start of 2008:Q4			
		Typical raise quarter		
	QWI	Any quarter	Q2 only	Q4 only
Two-digit NAICS sector or cluster				
Agriculture, mining, and 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 and 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 and 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 30 states included in 10% random sample from the Longitudinal Employer-Household Dynamics. 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. "FIRE" indicates the finance, insurance, and real estate NAICS industries. U.S. Census Bureau Disclosure Review Board bypass number CBDRB-2018-CDAR-061.

B   Persistence of firms' typical raise quarters

To show evidence of the stability of firms' typical raise quarters, I evaluate whether a worker is more likely to receive a wage change in the calendar quarter in which co-workers at the firm have historically tended to receive wage raises. Specifically, I estimate the following specification:

$$\mathbb{1}\left[\Delta_{ikt}^w > 0\right] = \alpha d_{ikt}^{RQ} + \boldsymbol{X_{ikt}}\beta + \epsilon_{ikt} \tag{B.1}$$

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

When determining firm k's typical raise quarter for worker i in period t, I use only co-workers' historical wage raises to identify the typical raise quarter. This formulation overcomes two concerns. First, focusing on co-workers ensures that the worker's own annual wage adjustment does not affect the measured typical raise quarter of the firm. Second, constructing the firm's typical raise quarter using co-workers' wage raises from previous years avoids the issue of firm-wide shocks generating contemporaneous correlation in co-workers' raise frequencies.

The regression results shown in Table B1 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 finding implies that workers' annual nominal raise schedules are strongly coordinated within the firm and tend to occur in the same calendar quarter from year to year.

Table B1: 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 (Q1)		9.3***	7.3***	1.2***	1.1***
		(0.6)	(0.8)	(0.07)	(0.10)
Calendar Q2		-0.24	-0.09	-0.18***	-0.18***
		(0.37)	(0.32)	(0.04)	(0.04)
Calendar Q3		1.94	1.15	-0.28***	-0.20**
		(1.14)	(0.81)	(0.03)	(0.05)
Calendar Q4		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 Q1). All models include as control variables: worker age, tenure, quarters since most recent wage change, and earnings quintile dummy variables. Robust standard errors are clustered at the firm level. ***, **, 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.

C   A further test of the parallel trends assumption

A key concern for the parallel trends assumption is the possibility that seasonal factors in firms' business conditions could be correlated with both the firm's historical typical raise quarter and the firm's exposure to the financial collapse. This concern is reinforced by the fact that nominal wage raises in certain industries appear to be clustered in particular calendar quarters (e.g., firms in finance, insurance, and real estate (FIRE) and professional services tend to raise workers' wages in Q4, whereas construction and accommodation firms cluster their typical raise quarters in Q2; see Table A1 for the full breakdown). Even though the difference-in-differences specification controls for both industry-specific shocks in 2008:Q4 and firm-specific seasonality, unobserved factors that determine a firm's historical raise patterns may still also affect the magnitude of the shock that the firm experienced in 2008:Q4.

To address this concern, I extend the difference-in-differences estimation model to determine whether the job destruction rate at Q2-raising firms responded more strongly to a given-sized change in revenue (relative to the job destruction rate at Q4-raising firms). 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:

$$JD_{kt} = \sum_{q=1}^{4} \alpha_{kt}^{Qq} + \boldsymbol{D}_{\boldsymbol{kt}}^{\boldsymbol{2008:Q4}} \left(\theta^{Q4} + \boldsymbol{R_{kt}} \gamma^{Q4} + \boldsymbol{D}_{\boldsymbol{kt}}^{\text{Q2-raiser}} \left(\theta^{Q2} + \boldsymbol{R_{kt}} \gamma^{Q2} \right) \right) + \boldsymbol{X}_{kt} \beta_t + \epsilon_{kt} \quad \text{(C.1)}$$

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

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

There are two significant changes in the sample used for this estimation. First, annual revenue data are available in the U.S. Census Bureau's revenue-enhanced Longitudinal Business Database (rLBD) only 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 C1 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 I as Model (1) for comparison purposes, as it uses the same specification with industry-by-time fixed effects and no employment weighting, but without the revenue change variables.

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

Table C1: Differential Q2- Versus 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:Q4 * Q4-raiser * Δ^+revenue		-0.52
		(0.82)
2008:Q4 * Q4-raiser * Δ^-Revenue		1.02
		(0.76)
	Relative to Q4-raiser	
2008:Q4 * 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:Q4 * Q2-raiser * Δ^+revenue		1.15
		(1.89)
2008:Q4 * Q2-raiser * Δ^-revenue		7.68***
		(1.70)
Industry*time fixed effects	Yes	Yes
Employment weighted	No	No
Mean job destruction rate	7.7%	7.0%
Observations	5.6M	1.6M
R-squared	0.29	0.36

Note: The outcome variable is the job destruction rate at the state employer identification number (SEIN) level. 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 Longitudinal Employer-Household Dynamics data are from 2002:Q1 to 2014:Q4. Robust standard errors are clustered at the SEIN-level. ***, **, 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.

firms.

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

driving the differential response of Q2-raising firms).

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

C.1   Endogeneity of revenue change

Two concerns prohibit a causal interpretation of the estimated differential response of Q2-raising firms' to negative revenue changes in 2008:Q4 (relative to Q4-raising firms). 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), a firm's revenue in the first three quarters of the year may affect the observed job destruction rate in 2008:Q4 through the start-of-quarter employment level rather than through the firm's employment decisions in Q4. For instance, a firm with a greater negative change in revenue in the first three quarters of 2008 may have already laid off a number of workers and thus entered 2008:Q4 with a lower employment level. When I compare this firm against another firm with a similar negative change in annual revenue, I would expect to see fewer fourth-quarter layoffs at the firm with a larger fall in revenue during the first three quarters of the year. If the variation in firms' quarterly revenue is related to the timing of firms' historical typical raise quarters (for instance through seasonal effects), then this would bias the difference-in-differences 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 whether Q2-raising firms had larger or smaller negative revenue changes from 2008:Q1 to 2008:Q3 relative to Q4-raising firms. That said, any concern regarding bias of this sort is mitigated by Q4-raising firms not exhibiting any change in their degree of responsiveness to negative revenue changes (relative to other periods).

The second concern stems from simultaneity between the annual revenue measure and a firm's Q4 job destruction rate. It is common 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 job destruction rates in 2008:Q4. This job destruction, in turn, lowered employment levels, and thus further decreased revenue in 2008:Q4. I expect that this reverse causality attenuates the coefficient estimate for the differential response of Q2-raising firms' job destruction rates to negative revenue shocks (γ^{Q2}). The attenuation bias results from exogenous negative revenue shocks in 2008:Q4 forcing both Q2- and Q4-raising firms to destroy jobs. This job destruction lowered employment levels at firms, which further decreased 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' job destruction rates, the effect of this simultaneity bias is the same for both Q2- and Q4-raising firms. For the same fundamental negative revenue shock, job destruction should rise similarly at both Q2- and Q4-raising firms, thus generating similar degrees of simultaneity bias. If, instead, greater exposure to DNWR forces firms to destroy more jobs, then job destruction should rise more at the Q2-raising firms in 2008:Q4. The simultaneity of revenue and employment, in conjunction with this higher job destruction, means that the observed negative revenue change is disproportionately larger than the fundamental negative revenue shock for Q2-raising firms. Thus, the same fundamental negative revenue shock generates a larger observed fall in revenue at Q2-raising firms. As a result, the estimated correlation is weaker between the job destruction rate and the interaction of the observed negative revenue change with the firm's exposure to DNWR (relative to its true correlation with the fundamental negative revenue shock), but does not reverse the sign. Thus, I interpret the result that Q2-raising firms increased their job destruction rates relative to Q4-raising firms in 2008:Q4 by an additional 0.077 percentage point for every 1% fall in year-over-year revenue as a lower bound on Q2-raising firms' actual differential sensitivity to the fundamental negative revenue shocks in 2008:Q4.

C.1.1 Attenuation bias in revenue change estimation

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

$$\tilde{\Delta}R_{kt}^{-} = \tilde{\Delta}R^{-^F} + \alpha\tilde{J}D_{kt} + \upsilon \tag{C.2}$$

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

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

$$\tilde{JD}_{kt} = \beta_1 \tilde{\Delta} R^{-F} + \beta_2 d_{kt}^{Q2\ raiser} \tilde{\Delta} R^{-F} + \epsilon \tag{C.3}$$

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 $\beta_1 > 0$ because when negative revenue shocks are larger in absolute value, I expect job destruction to rise. I assume the $\beta_2 \geq 0$, so if DNWR affects job destruction when the firm has a larger negative revenue shock, then it will increase job destruction.

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

$$\tilde{JD}_{kt} = \frac{\beta_1 + \beta_2 d_{kt}^{Q2\ raiser}}{1 + \beta_1 \alpha + \beta_2 \alpha d_{kt}^{Q2\ raiser}} \tilde{\Delta} R_{kt}^{-} + \zeta_{kt} \tag{C.4}$$

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

$$\mathbb{E}\left[\hat{\beta}_1^{DiD}\right] = \frac{\beta_1}{1 + \beta_1 \alpha} \tag{C.5}$$

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

$$\tilde{JD}_{kt}^{Q2} = \frac{\beta_1 + \beta_2}{1 + \beta_1 \alpha + \beta_2 \alpha} \tilde{\Delta} R_{kt}^{-} - \hat{\beta}_1^{DiD} \tilde{\Delta} R_{kt}^{-} + \zeta_{kt} \tag{C.6}$$

Accordingly,

$$\mathbb{E}\left[\hat{\beta}_2^{DiD}\right] = \frac{\beta_2}{(1 + \beta_1 \alpha + \beta_2 \alpha)(1 + \beta_1 \alpha)} \tag{C.7}$$

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

D Variation in worker layoff risk from DNWR exposure

Given the substantial effect that DNWR has on firms' job destruction rates, 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 but now at the level of worker-firm pairs. The outcome of interest is whether a worker is laid off, defined as any instance when the worker either: (i) transitions from employment in the current quarter to non-employment in the next quarter, or (ii) switches from one employer in this quarter to a new employer either in this quarter or the next but where the earnings gap between the two jobs exceeds one month of earnings. (This measure of job transitions is derived from Haltiwanger, Hyatt, Kahn and McEntarfer (2018).) I augment the difference-in-differences estimation with a large set of worker characteristics that includes 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

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

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 the firm-by-calendar-quarter fixed effects (for firm-specific seasonality).

Table D1 reports the results of this regression. This table shows that less-educated workers and workers hired before 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 as these workers have either less educational attainment or 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 (aged 61 to 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. However, finding that younger workers (younger than age 35) have slightly lower layoff risk relative to middle-aged workers (aged 36 to 60) is surprising.

Table D1: 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	

Notes: 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 EN transition or an EE 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 are clustered at the SEIN-level. Quarterly LEHD data from 1999:Q1 to 2014:Q4. ***, **, 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.

E   Biases in regression of job destruction on real wage bill ratio

Because of a combination of measurement error in real wages and persistent unobserved confounders affecting both past wages and current-period job destruction, a simple OLS regression of the model in Equation 3 is unlikely to yield a causal estimate of the effect of having a higher real wage bill in 2008:Q4.

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

Any given firm will make wage change decisions by taking into account its expected future job creation and job destruction decisions. This practice 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 thereby generating a negative bias in coefficient estimates.

F   Alternative instrumental variable results

In the baseline instrumental variable estimation described in Section 5.1, I include as an instrumental variable the firm's historical raise share in calendar quarter Q3 interacted with a 2008:Q4 indicator variable. This raise share may be endogenous because 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 the robustness of the results to this potential bias, I estimate the same model but now include the $t - 1$ historical raise share in 2008:Q4 (which corresponds to the Q3 calendar quarter) as a control variable instead of as an instrumental variable. The original OLS and IV results are reported in columns (1) and (2) for the unweighted and in columns (4) and (5) for the employment-weighted estimation models. Columns (3) and (6) report the IV results when I instead include the $t - 1$ raise share in 2008:Q4 as a control variable.

In both the unweighted (models (2) and (3) and employment-weighted (models (5) and (6)) IV results, including the Q3 raising firms as a control variable instead of as an instrument more than doubles the estimated effect size. This implies that firms' job destruction rates responded more strongly to the 2008 financial shock if firms tend to raise their workers' wages in Q1 and Q2 versus Q3 and Q4 of the calendar year. This is consistent with: (i) some Q3 raising firms and all Q4 raising firms having greater flexibility in their real wage bills as a result of their lower exposure to DNWR; and (ii) this lower exposure to DNWR resulting in large and statistically significant differences in job destruction rates across firms.

Table F1: Second-Stage: Job Destruction Rate

Dependent variable:	Firm 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 fixed effects	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 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 NAICS sector, and two-digit NAICS sector-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 are clustered at the SEIN-level. ***, **, 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-B0073-CED-20190910.