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ABSTRACT

How Prevalent Is Downward Rigidity in Nominal Wages? Evidence from Payroll Records in Washington State*

For more than 80 years, many macroeconomic analyses have been premised on the assumption that workers' nominal wage rates cannot be cut. The U.S. evidence on this assumption has been inconclusive because of distortions from reporting error in household surveys. Following a British literature, we reconsider the issue with more accurate wage data from the payroll records of most employers in the State of Washington over the period 2005-2015. For every one of the 40 four-quarters-apart periods for which we observe year-to-year wage changes, we find that at least 20 percent of job stayers experience nominal wage reductions.

JEL Classification: J3, E24
Keywords: nominal wage rigidity, payroll records

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How Prevalent Is Downward Rigidity in Nominal Wages?
Evidence from Payroll Records in Washington State

"If, when we investigate something, we find there is reason and proof for it, we must acknowledge that as reality – even if it is in contradiction with a literal scriptural explanation that has held sway for many centuries or with a deeply held opinion or view. So one fundamental attitude shared by Buddhism and science is the commitment to keep searching for reality by empirical means and to be willing to disregard accepted or long-held positions if our search finds the truth is different."

– The Dalai Lama (2006, pp. 24-5)

1 .Introduction

In chapter 2 of *The General Theory of Employment, Interest, and Money* (1936), John Maynard Keynes set out the labor-market premise of his macroeconomic model – that the reason the labor market does not clear in a recession, but instead exhibits high unemployment, is that workers refuse to accept reductions in their nominal wages. In the 80-plus years since publication of *The General Theory*, Keynes's premise of downward nominal wage rigidity has continued to be highly influential in macroeconomic analysis. A couple of prominent examples from decades ago are Tobin's (1972) presidential address to the American Economic Association and the 1996 *Brookings Papers* article by Akerlof, Dickens, and Perry. Both of these much-cited articles restated and extended Keynes's analysis and advocated for positive inflation as a device to "grease the wheels of the labor market."

Quite understandably, attention to Keynes's analysis increased during the Great Recession. For example, according to Schmitt-Grohe and Uribe's (2013) article "Downward Nominal Wage Rigidity and the Case for Temporary Inflation in the Eurozone" in the *Journal of Economic Perspectives*, "downward nominal wage rigidity played an important role in the current unemployment crisis in the euro area." A widely noticed San Francisco Fed note by Daly, Hobijn, and Lucking (2012) reached a similar conclusion for the United States. Based partly on the work of Daly et al., Paul Krugman repeatedly blogged about the crucial role of downward nominal wage rigidity. His July 22, 2012, entry ("Sticky Wages and the Macro Story") argued that "downward nominal wage rigidity … is a glaringly obvious feature of the real world…. It's simply a fact that actual cuts in nominal wages happen only rarely and under great pressure." In the aftermath of the

Great Recession, prominent macroeconomic analyses have continued to rely on the assumption of downward nominal wage rigidity. A couple of particularly recent examples are Schmitt-Grohe and Uribe (2016) and Dupraz, Nakamura, and Steinsson (2018).

After more than 80 years of such analyses, the question we wish to pose is: What is the evidence for the proposition that downward nominal wage rigidity is a binding constraint, so much so that it can account for major allocative inefficiencies in real quantities such as employment and unemployment? To be clear, we do not mean to deny the existence of any nominal wage stickiness. We expect that most of our readers, like ourselves, have their salaries set in nominal terms and typically see them adjusted only once a year. But does it follow that nominal wages *cannot* be cut, even when layoffs into unemployment are the alternative?

Given the long history of downward nominal wage rigidity as a premise of macroeconomic theorizing, it is surprising that the evidence on this question is as weak as it is. As we will discuss in the next section, until recently most of the evidence came from U.S. studies based on wage reports in longitudinal household surveys. Two key findings in these studies have been (a) that many workers staying with the same employer report the same nominal base wage in successive years, but (b) that many other job stayers report nominal wage cuts. The first finding is suggestive of wage rigidity; the second is suggestive of wage flexibility. Both findings reasonably have been questioned on the ground that they could be artifacts of the considerable reporting error in household surveys.

In sections 3 and 4, we will revisit the question with the benefit of more accurate and comprehensive data drawn from the payroll records of most employers in the State of Washington over the period 2005-2015. Like a British literature we will review in section 2, the new Washington evidence shows that nominal wage freezes are much less common than they appear in household survey reports, but nominal wage cuts occur with strikingly high frequency. Section 5 will provide a summary and discussion.

2. The Existing Empirical Literature

Most of the existing U.S. evidence on nominal wage rigidity has come from longitudinal analyses of workers' year-to-year wage changes as measured in household surveys, mainly the Panel Study of Income Dynamics and the Current Population Survey (CPS). Some of the most influential early examples are McLaughlin (1994), Kahn (1997), and Card and Hyslop (1996).

Because it is obvious that job changers typically experience wage changes, these studies have focused on the more interesting question of whether workers staying with the same employer exhibit sticky nominal wages.

A sense of the main findings is provided in figure 1, which reproduces figure 4 from the CPS-based replication and update of the literature in Elsby, Shin, and Solon (2016). The figure shows histograms of January-to-January changes in log nominal base wages for workers paid by the hour in 2005-2006, 2007-2008, 2009-2010, and 2011-2012.[1] The thin spike at zero tells the percentage of workers who reported the exact same wage in both years. The next bin to the right contains workers whose change in log nominal wage was positive but no greater than 0.02; the next bin contains those whose change in log nominal wage was greater than 0.02 and less than or equal to 0.04; and so forth. The bins to the left of zero are constructed symmetrically. For the sake of readability, workers with changes in log nominal wage greater than 0.64 are piled up in the rightmost bin and those with changes less than -0.34 in the leftmost bin.

Many of the exhibited patterns will be discussed in section 3, when we present histograms from our payroll-based Washington State data. For now, we emphasize three patterns commonly observed in the literature based on household surveys. First, as expected, most job stayers show positive nominal wage growth. This is necessary just to keep up with positive inflation, and nominal wage growth greater than the inflation rate frequently occurs to deliver the real wage growth associated with human capital enhancement and other factors. Second, the spikes at zero nominal wage growth are substantial. In the years shown in figure 1, the percentage of hourly job stayers reporting zero wage change is regularly between 15 and 20 percent, a finding that seems to suggest nominal wage stickiness. On the other hand, a third pattern is that many cases are located to the left of zero. In the years shown in the figure, the percentage of hourly job stayers reporting nominal wage cuts ranges from 17.0 to 25.5 percent. Taken at face value, this finding seems to contradict the assumption that nominal wages cannot be cut.

But it is not clear that either the second or third finding should be taken at face value. As many writers have noted, either finding could be largely an artifact of reporting error. Starting with the third finding, Altonji and Devereux (1999) and Akerlof et al. (1996), among others, have speculated that nominal wage cuts really are rare and that the many reports of negative changes

[1] Some of these histograms are slightly in error because of a coding mistake that excluded never-married workers. Eck (2018) discovered the error and found that correcting it made very little difference for the results.

mostly reflect instances in which the second year's reporting error is sufficiently negative relative to the first year's. At the same time, the large spikes at zero could be partly an artifact of rounding error. For example, if a worker whose true nominal hourly base wages were $19.80 last year and $20.30 this year reports a wage of $20 in both years, she would appear in the histogram as a wage freeze case even though her nominal wage actually grew by almost 3 percent.[2] In addition, it is possible that many of the measured freezes in base pay overlook variation in other types of pay, such as bonuses and commissions.

Of course, the most compelling solution to ambiguities from measurement error is to look at more accurate data. This is exactly the approach taken in part of the British literature. In a pioneering study, Smith (2000) first used the 1991-1996 waves of the British Household Panel Study (BHPS) to verify that she got results similar to those based on U.S. household surveys. Then she exploited a remarkable feature of the BHPS: respondents were told they could consult their pay slips when answering the wage questions, and the survey recorded who did so. When Smith restricted her analysis to the sub-sample that did check their pay slips, the spike at zero nominal wage change became much smaller. Smith concluded that the spike had been exaggerated on account of rounding error. Even more interestingly, she found that the pay-slip-consulting sub-sample reported fewer wage cuts, but not by that much. Even in this group, the proportion with negative nominal wage change was 18 percent. To quote her striking summary, "Some of the results in this paper may seem difficult to believe – the quite common occurrence of nominal pay cuts, for example. It may well be that the difficulty in believing them stems not from the weight of contradictory evidence, but rather from conventional wisdom that has survived because of the previous lack of evidence either way."

Smith's study was followed by Nickell and Quintini's (2003) study based on 1975-1999 data from the New Earnings Survey (NES). The NES sample is a 1 percent sample of income tax-paying workers, but the survey is administered to employers, who are legally required to report wage information from their payroll records for a reference week each April. Nickell and Quintini's first use of these relatively accurate wage data was to verify that they produced 1991-1996 results similar to Smith's for the BHPS respondents that checked their pay slips. Nickell and

[2] Both Kahn (1997) and Card and Hyslop (1996) report that many of their measured wage freezes occur at round numbers. Both correctly observe that, with the household survey data alone, it is difficult to tell whether that pattern reflects rounding error or a genuine tendency of some employers to pay a round-number wage and stick to it until circumstances impel them to jump the wage to a different round number.

Quintini went on to an analysis for their full 1975-1999 period, which continued to show a smaller spike at zero than usually found in household surveys as well as substantial numbers of nominal wage cuts. Elsby, Shin, and Solon (2016) conducted a replication of Nickell and Quintini and an update through the year 2012. Their table 6 shows that the zero spike ranged from a low of 0.4 percent in 1979-1980, when inflation was almost 20 percent, to a high of 9.1 percent in 2011-2012, when inflation was moderate and unemployment was high. In the majority of years, the proportion with frozen nominal wages was less than 3 percent. The percentage with nominal wage cuts ranged from a low of 4.9 in 1979-1980 (when inflation was almost 20 percent) to a high of 23.5 in both 2009-2010 and 2011-2012. Most strikingly of all, over the last 20 years of the sample period, the percentage of job stayers receiving nominal wage cuts was regularly close to 20 percent.

What all these researchers said they learned from the data is that British nominal wages are more flexible than was previously realized.[3] A natural question for American readers of this research is whether relatively accurate payroll-based wage data for the United States would show similar results. The next section addresses that question with such data from Washington State.

3. Evidence from Washington State

No U.S. data set is quite like the payroll-based NES data for Great Britain. Most state unemployment insurance programs in the United States, however, do require employers to report every employee's quarterly earnings, which are needed to calculate workers' benefit entitlements if they become unemployed and file claims for unemployment benefits. The problem was that, without hours data as well, it seemed impossible to measure hourly wage rates. The key breakthrough occurred when Kurmann, McEntarfer, and Spletzer (2016) discovered that a few states do require employers to report each employee's quarterly hours. In most of these states, the hours data may not be very accurate, but Washington State is an exception. Washington's benefit entitlement rules depend on quarterly hours as well as quarterly earnings, so Washington needs accurate reporting of both variables.[4]

[3] Several studies of other countries have used payroll-based or pay-slip-based data and also have found considerable frequency of nominal wage cuts. See Elsby and Solon (2018) for a summary of studies of West Germany, Austria, Italy, Spain, Mexico, Ireland, South Korea, Portugal, and Sweden.

[4] For the purpose of obtaining accurate hours data, the State of Washington requires employers to keep a record of daily work hours for each employee for at least four years from the date when taxes were paid. In addition, the Employment Security Department conducts annual audits of selected employers and can fine employers if they fail to report hours worked or make regular mistakes in reporting. An analysis of the quality of the Washington hours

In Washington, therefore, it is possible to measure each worker's average hourly earnings each quarter as the ratio of quarterly earnings to quarterly hours.[5] Unlike the NES data, which are for only a 1 percent sample of tax-paying workers, the Washington data are nearly comprehensive. The Washington unemployment insurance system encompasses about 95 percent of private sector employment. It also covers state and local government workers, but excludes federal government employees, contractors who receive 1099 instead of W-2 tax forms, corporate officers, and elected officials. The wage measurement also is relatively comprehensive in the sense that it encompasses all monetary compensation, including bonuses, commissions, and the like. This is reasonable for our purposes because a reduction in average hourly earnings due to, say, a decreased bonus is economically interpretable as a wage reduction. A more nuanced situation, to be discussed below, is a reduction in overtime hours. In the remainder of this paper, we often will use the term "wage" as a shorthand for the ratio of quarterly earnings to quarterly hours, acknowledging that this is more accurately if verbosely described as a measure of average monetary compensation per hour.

Like most of the literature, our study overlooks fringe benefits, such as employer contributions to health insurance. Lebow, Saks, and Wilson (2003) have argued that fringe benefits are an additional dimension for adjustment in compensation, so overlooking them is likely to make total compensation seem less flexible than it actually is. A similar point applies to variation in work effort.

Following the literature's tradition of measuring year-to-year wage changes, we study four-quarters-apart changes in average hourly earnings. Our sample covers 40 periods, starting with the change between the first quarters of 2005 and 2006 and ending with the change between the fourth quarters of 2014 and 2015. The sample therefore includes periods before, during, and after the Great Recession. Again following the literature, we focus on wage changes of workers that stay with the same firm.[6] Thus, to contribute a wage change observation in our first sample period, a worker had to have worked positive hours for the same employer in both the first quarter of 2005

data by Lachowska, Mas, and Woodbury (2018) concludes that "the reliability of administrative hours reporting is high."

[5] Employers are instructed to report "all hours worked during the quarter." For full-time salaried and other employees whose weekly hours are not explicitly tracked, employers are instructed to report 40 hours per week.

[6] In our analysis, a worker in a multi-establishment firm who moves from one Washington establishment to another within the firm is classified as a stayer.

and the first quarter of 2006.[7] Finally, to avoid observations that seem likely to be erroneous, we exclude observations with more than 1,000 work hours in the quarter, observations with a nominal hourly wage greater than $500 but fewer than 10 work hours, and individuals with multiple records with the same employer in the same quarter. These restrictions exclude less than 1 percent of the data over our 10-year period. Still, the existence of these cases highlights that even the Washington data are imperfect, though we expect them to be a considerable improvement over household survey data.

The Washington data are such a great resource that they have attracted two research teams – ourselves and Kurmann and McEntarfer (2017). That the two concurrent projects partially overlap is good for the sake of cross-validation. But they also differ in important respects, which will be noted in further detail below. For now, a broad-brush characterization of the differences is that our study concentrates on a more detailed description of year-to-year hourly wage change, while Kurmann and McEntarfer's analysis extends to other topics, especially earnings changes due to hours changes[8] and the very challenging question of the extent to which downward nominal wage rigidity causes layoffs and other allocational changes.[9]

The heart of our analysis is the plotting of histograms for job stayers' four-quarters-apart nominal wage growth for each of our 40 sample periods. The sample size for each of the 40 periods is approximately two million job stayers. Our results are illustrated in figure 2, which displays four of our histograms, for the first quarters of 2005-2006, 2008-2009, 2011-2012, and 2014-2015. In each histogram, a thin vertical line marks the position of exactly zero nominal wage change, and the overlaid red bar shows the relative frequency of nominal wage freezes. As in figure 1, the next bin to the right of zero contains workers whose change in log nominal wage is positive but no greater than 0.02; the next contains those whose change in log nominal wage is greater than 0.02 but no greater than 0.04; and so forth, with a symmetric layout to the left of zero. In figure

[7] More precisely, our unit of analysis is a worker-employer pair. A worker who works for the same *two* employers in both of the two quarters thus contributes two wage change observations.

[8] This aspect of their work leads them to restrict their sample to workers staying with the same employer for at least 10 consecutive quarters, so for our purpose of studying nominal wage rate changes, their sample is smaller (about half the size of ours per time period) and more narrowly selected.

[9] This latter analysis follows in the footsteps of Ehrlich and Montes (2014). That study and the one by Kurmann and McEntarfer both find that firms with relatively many nominal wage freezes also tend to lay off more workers, and both studies infer that nominal wage rigidity *causes* the layoffs. The identification problem is that there is an alternative interpretation of the observed correlation – that economically distressed firms tend to have both more wage freezes and more layoffs, even if the layoffs were not caused by wage rigidity. This alternative interpretation is supported by the interviews of managers reported in section 11.3 of Bewley (1999).

2, workers with changes in log nominal wage greater than 0.40 are piled up in the rightmost bin and those with changes less than -0.40 in the leftmost bin.

All four histograms (as well as the 36 others we have not displayed here) show the three typical features already mentioned in section 2: most job stayers experience positive nominal wage growth; a noticeable spike appears at zero nominal wage change; and many stayers are measured as receiving nominal wage cuts. As in the British literature using payroll or pay slip data, though, the zero spike is much smaller than what typically is measured in household surveys, which are likely subject to considerable rounding error and often overlook variation in wages other than base pay. Also as in that British literature, the proportion receiving wage cuts is strikingly large.

These findings are spelled out in more detail in table 1, which lists key statistics for all of our 40 periods. The "wage cut" column shows that the percentage of Washington job stayers receiving nominal wage reductions exceeds 20 percent in all 40 periods, with a low of 20.4 percent between the first quarters of 2006 and 2007 and a high of 33.1 percent between the fourth quarters of 2008 and 2009. It is no surprise that the prevalence of wage cuts rose considerably during the Great Recession and returned to a normal (but still strikingly high) level afterwards.

The "wage freeze" column shows that the percentage with zero wage change ranges between a low of 2.5 percent between the fourth quarters of 2006 and 2007 and a high of 7.7 percent between the second quarters of 2009 and 2010. It is no surprise that the prevalence of wage freezes also rose considerably during the Great Recession. But it may surprise many readers that, once wages are measured more accurately with payroll-based data, the frequency of wage freezes turns out to be so low. The percentage is always below 8 percent and in the majority of our sample periods is less than 4 percent. Echoing the British literature based on payroll or pay slip data, these zero spikes are much smaller than those measured in household survey data that are subject to rounding error and exclude compensation beyond base pay.

The zero spikes reported for Washington by Kurmann and McEntarfer (2017) also are smaller than those from household surveys, but are larger than ours. The main reason for the apparent discrepancy between the two Washington studies is that Kurmann and McEntarfer's zero spikes include not only the exact zeros, but also log wage changes of no more than 0.005 in magnitude. In our view, very small wage changes are economically interesting and should be distinguished from the exact zeros. Indeed, the last two columns of our table 1 display log nominal wage reductions and log nominal wage increases of no more than 0.01. As shown for the British

NES data in table 6 of Elsby, Shin, and Solon (2016), these small wage changes occur with non-trivial frequency. The percentage of Washington stayers experiencing log wage changes no greater in magnitude than 0.01 exceeds 7 percent in every one of our 40 sample periods and usually exceeds 10 percent. Some previous studies based on household surveys (e.g., Kahn, 1997) have claimed to see "holes" in the wage growth distribution near zero and have attributed these holes to menu-cost reasons for employers to avoid small wage changes. But our histograms and the last two columns of table 1 show no such holes. Of course, the same rounding error that overstates the frequency of wage freezes in household survey data also understates the frequency of small wage changes.

Some data sets distinguish base pay from other wage components,[10] but such a separation is not possible with the Washington data. We therefore cannot determine what proportion of our measured wage cuts are due, for example, to reductions in bonuses or commissions. In any case, such cuts are properly viewed as a sort of wage flexibility. In contrast, the economic interpretation of a measured wage cut is less clear when it arises from a reduction in the share of a worker's quarterly hours that are overtime work paid at time-and-a-half. If the worker welcomed the overtime work and regrets the reduction, it makes economic sense to say that the worker's compensation was reduced. On the other hand, if the worker experienced the old overtime share as burdensome and is glad for the reduction, it would be a mistake to say that the worker's compensation was cut. What little evidence exists on this issue (see chapter 7 in Ehrenberg and Schumann, 1982) does not point clearly in one direction or the other.

Although we cannot isolate overtime pay and hours, we can analyze a sub-sample of workers for whom overtime is not a likely factor. Table 2 redoes the key analyses in table 1, but for a sub-sample of job stayers satisfying two restrictions: (a) their quarterly hours are between 480 and 560 in both of the quarters involved in the measurement of wage change, and (b) they have positive earnings or hours with the same employer in the quarters both preceding and

[10] For example, the British NES obtains earnings and hours variables that explicitly exclude overtime. Also, a new preliminary manuscript by Grigsby, Hurst, and Yildermaz (2018) uses U.S. data from the ADP payroll processing company that provide some basis for separating base pay from other wages. The authors find that base pay reductions are rare in expansion years, but they replicate our finding that reductions in overall earnings per hour are strikingly common. This finding regarding the role of compensation other than base pay in nominal wage changes echoes a familiar result from the literature on cyclicality in *real* wages. For example, the last two sentences in Shin and Solon (2007) conclude, "Even among workers staying with the same employer, though, real average hourly earnings appear to be substantially procyclical. An important part of that procyclicality probably is due to compensation beyond base wages, such as overtime pay and bonuses."

following both of the quarters involved in the measurement of wage change. This group is approximately 30 percent of our full sample. Of course, it is possible for some cases with overtime work to creep into this sub-sample, but we conjecture that the large majority of these workers worked 40 hours in every paid week of each quarter.[11] The results show that some combination of lack of overtime and the particularly stable employment of this sub-sample leads to a somewhat higher frequency of wage freezes and lower frequency of wage cuts, as compared to the full sample in table 1. Even for this sub-sample, though, the frequency of nominal wage cuts remains striking. The percentage with wage cuts ranges from a low of 14.5 percent between the third quarters of 2006 and 2007 to a high of 31.8 percent between the fourth quarters of 2008 and 2009. In most of our sample periods, the percentage with wage cuts is fairly close to 20 percent.

4. Some Intriguing Details

Footnote 16 in Elsby, Shin, and Solon (2016) reports on sectoral disaggregations of the British NES data and concludes that "nominal wage cuts are remarkably pervasive across sub-groups of workers/jobs." We similarly have disaggregated the Washington data by industry and firm size. Appendix table 1 shows each sample period's percentages of job stayers with nominal wage cuts in two industries. The utilities industry is the one with the chronically lowest percentage of wage cuts, and the mining and oil and gas extraction industry is the one with the chronically highest, so the two give a good sense of the range across industries. Even in the utilities industry, the percentage with nominal wage cuts ranges from a low of 13.8 percent between the first quarters of 2005 and 2006 to a high of 29.3 percent between the first quarters of 2007 and 2008. With only two exceptions, it is above 15 percent in every period.

We also have disaggregated our sample into six firm-size categories (measured as of the first of the two quarters used in measuring wage change): 1-9 employees; 10-49; 50-99; 100-499; 500-9,999; and 10,000 or more. Again we have found that nominal wage cuts are pervasive. In each of the first five categories, the percentage of job stayers receiving wages cuts tracks very closely with the overall numbers in table 1. The series for the firms with at least 10,000 employees

[11] We use the 480-560 range because the target population need not have been paid for exactly 13 weeks in the quarter. For example, many employers use bi-weekly pay periods, in which case the workers' quarterly earnings would be for either 12 or 14 weeks.

is more volatile because it involves fewer firms, but its central tendency is only a little below that for the full sample.

If 20 percent of all the job stayers in a particular period show wage cuts, this could happen because 20 percent of the stayers in *every* firm receive wage cuts. Or it could happen because the cuts occur universally in firms that employ 20 percent of stayers, and not at all in other firms. Where between these extremes does the reality lie? To explore this question, we create for each job stayer receiving a wage cut the following variable – the percentage of that worker's job-staying co-workers that also received a wage cut in the same period. Then, in each of our 40 sample periods, we plot the histogram of the distribution of that variable. Figure 3 shows the histograms for the first quarters of 2005-2006, 2008-2009, 2011-2012, and 2014-2015. All the histograms (including the 36 not shown in figure 3) indicate that, in every period, the majority of job stayers receiving nominal wage cuts work for firms that cut the wages of between 10 and 50 percent of their job stayers.

This finding that wage-cutting firms tend to target the cuts on a subset of their employees echoes a small anecdotal literature (Bewley, 1999; Blinder and Choi, 1990). For example, on pages 199-200 in his chapter 12 ("Experiences with Pay Reduction"), Bewley reported, "Some companies did cut or freeze the pay of groups of employees whose pay was felt to be excessive…. These cuts usually occurred in newly acquired companies or resulted from a tightening of control over local management, and they were triggered by financial problems or increased product market competition." The quotations that followed, along with some near the end of chapter 5, also noted a tendency, when responding to financial problems, to concentrate pay cuts on workers in management positions.

This latter point motivated us to investigate where wage cuts are concentrated in within-firm wage distributions. For this analysis, we focus on job stayers experiencing wage cuts in firms that have at least 20 stayers that period and that cut wages for less than 100 percent of those stayers. Next we split each firm's stayers into quartiles with respect to their wages in the first of the two quarters involved in the measured wage change. Finally, for each of our 40 sample periods, we plot a histogram for where the stayers receiving wage cuts lie in that within-firm wage distribution. Appendix figure 1 shows the histograms for the first quarters of 2005-2006, 2008-2009, 2011-2012, and 2014-2015. If there were no association between receiving a wage cut and prior position in the within-firm wage distribution, each of the four quartile bins would contain 25 percent of the

cases. Instead, all the histograms (including the 36 not shown) display a tendency for the cuts to be more concentrated in higher quartiles of the within-firm wage distribution. In every one of our 40 sample periods, the percentage of stayers receiving cuts that were in the top half of their within-firm wage distribution is between 60 and 70 percent. This is quite similar to a finding for South Korea reported in Park and Shin (2017).

We think that all these detailed findings are interesting, and some warrant further research. But we wish now to step back from the trees and gaze at the forest. Our big-picture finding is that the payroll-based wage information from Washington State, like earlier evidence from Great Britain, shows that nominal wage cuts for job stayers are much more common than most of us previously believed.

5. Summary and Discussion

For over 80 years, many macroeconomists have based their models on an assumption that workers' nominal wage rates cannot be cut. Seemingly contrary evidence from household surveys reasonably has been dismissed on the ground that it could reflect rampant response error. Like a British literature that moved from household surveys to presumably more accurate information from employers' payroll records, we have reconsidered the issue with payroll-based earnings and hours data from most employers in Washington State over the period 2005-2015.

Like the British payroll-based studies, we find that nominal wage cuts are far more common that most of us had thought. In every one of the 40 four-quarters-apart periods for which we observe year-to-year wage changes, we find that at least 20 percent of job stayers experience nominal wage reductions. Like many previous studies, we find that both cuts and freezes in nominal wages become even more common during a recession. We also find that the striking frequency of nominal wage cuts is pervasive across industries and firm sizes.

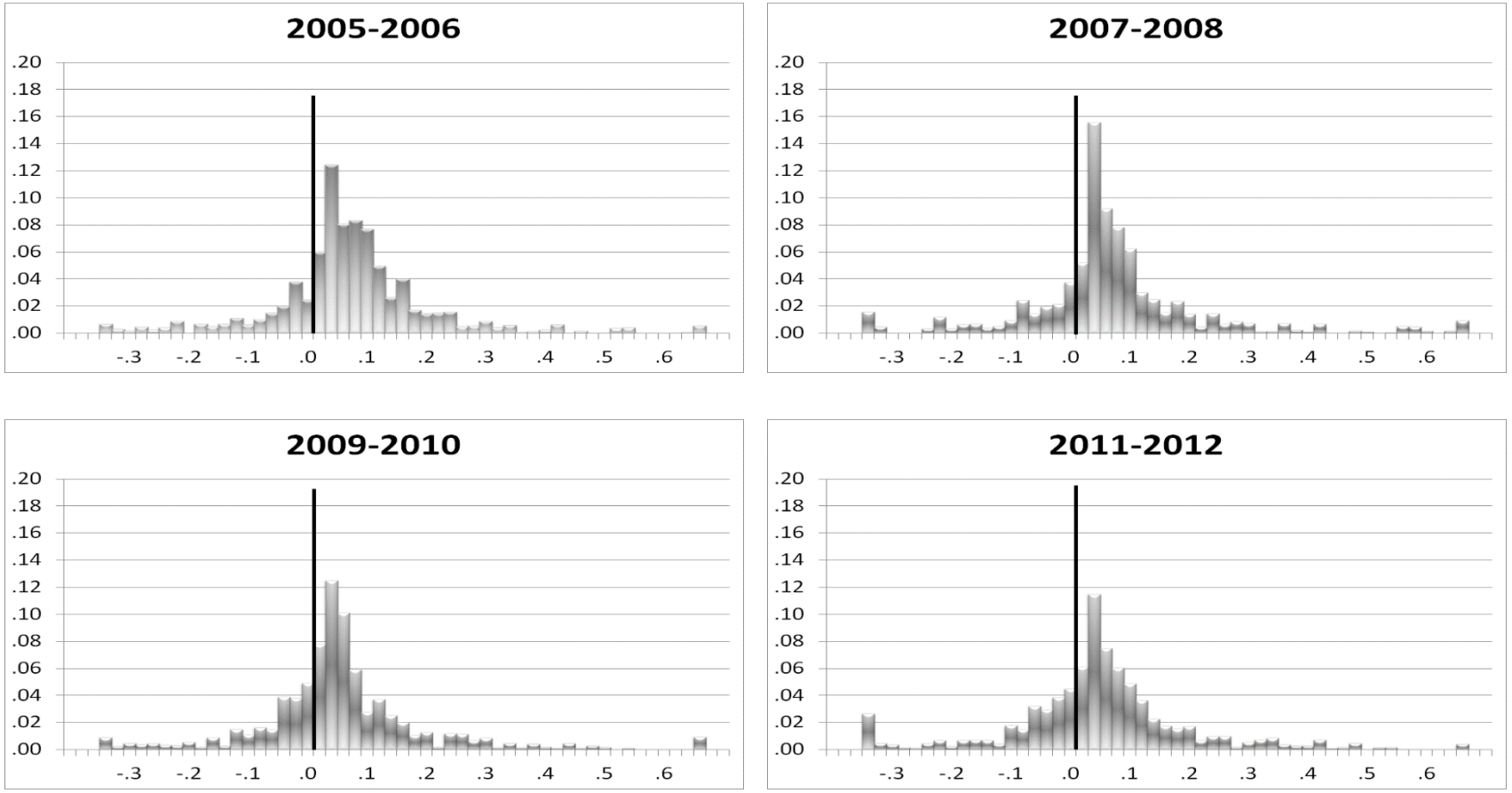
None of this is to deny that nominal wage stickiness exists in the world. After all, our own salaries are set in nominal terms and typically are adjusted only once a year. But does such wage stickiness stand in the way of efficient employment decisions? Empirically, as discussed above in our footnote 9, it is very difficult to identify causal effects of wage stickiness on employment outcomes. Theoretically, thanks to the classic analyses by Becker (1962) and Barro (1977), we have long understood that, in the large part of the labor market with long-term employment

relationships (which is the part that departs the *most* from a flexible-wage spot market), short-run wage stickiness need not prevent efficient employment outcomes.[12]

But what about those cases in which employment relationships can be preserved only if nominal wages *are* cut? Keynes's assertion in *The General Theory* (1936) was that workers are so adamant about refusing nominal wage cuts that the workers would lose their jobs and become unemployed instead. Undoubtedly, most of us workers hate to see reductions in our base pay or other types of compensation, but would we really prefer to lose our jobs, especially in the midst of an economic downturn? The new evidence from payroll records indicates that strikingly many job stayers do suffer nominal wage cuts, and this finding calls into question whether resistance to nominal wage cuts is as binding as often has been assumed. In keeping with our opening quotation from the Dalai Lama, we urge economists "to keep searching for reality by empirical means and to be willing to disregard accepted or long-held positions if our search finds the truth is different."

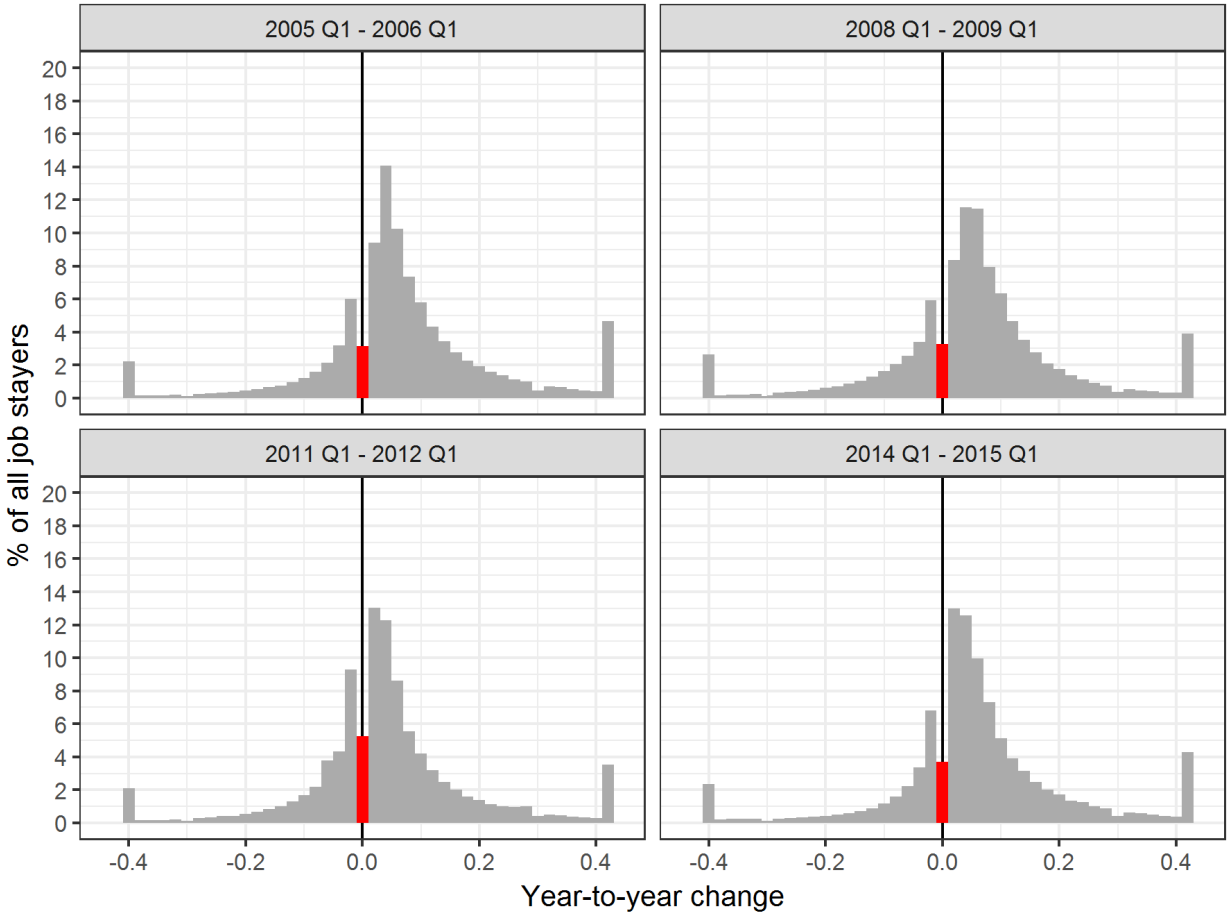
[12] For an extended discussion of this point and its implications with respect to downward nominal wage rigidity, see pages S272-6 in Elsby, Shin, and Solon (2016).

Figure 1. Distributions of Year-to-Year Change in Log Nominal Hourly Wages for Hourly Job Stayers in the Current Population Survey



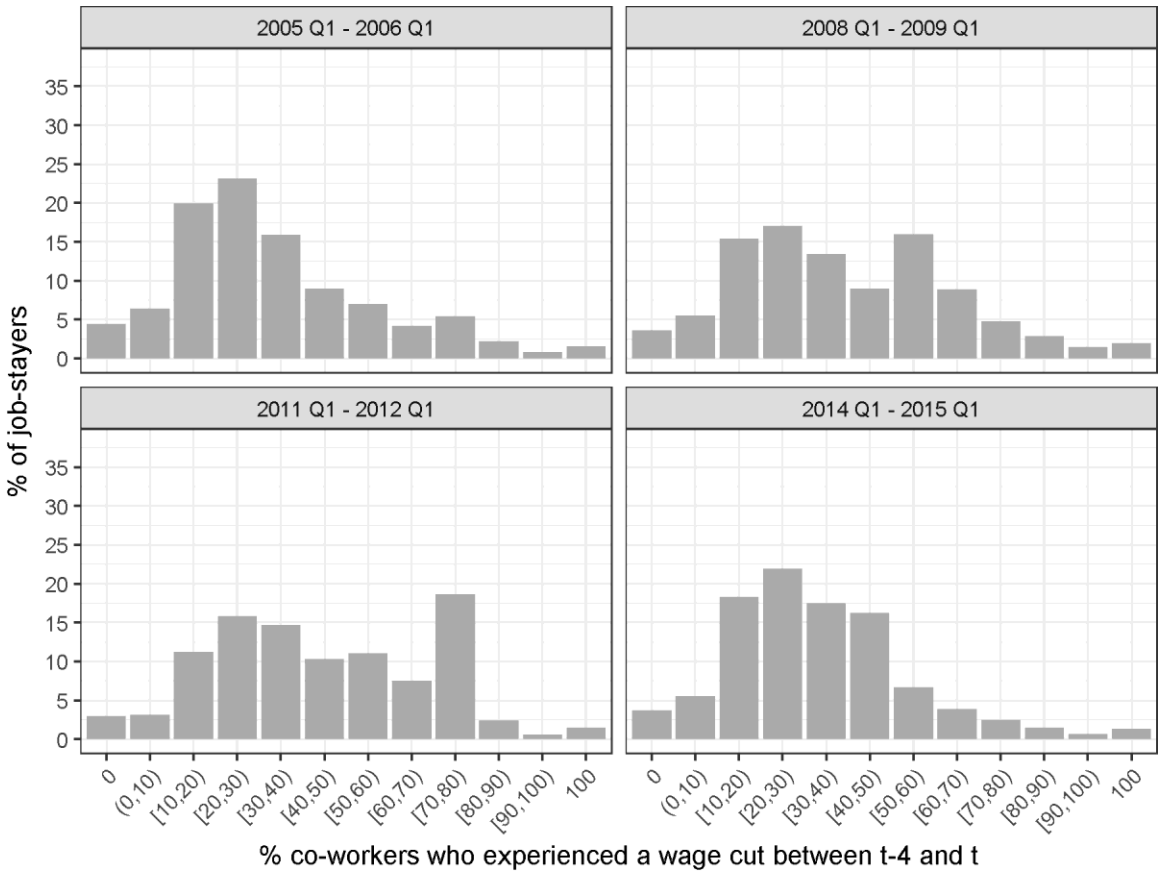
Source: Elsby, Shin, and Solon (2016).

Figure 2. Distributions of Year-to-Year Change in Log Nominal Hourly Wages for Job Stayers in Washington State



Source: Authors' calculations based on unemployment insurance records from the Washington Employment Security Department.

Figure 3. Distributions of Percentage of Co-Workers with a Wage Cut for Washington Job Stayers Who Themselves Experienced Wage Cuts



Source: Authors' calculations based on unemployment insurance records from the Washington Employment Security Department.

Table 1. Percentages of Washington State Job Stayers in Various Categories for Year-to-Year Change in Log Nominal Wages

Period	Wage Cut	Wage Freeze	[-0.01, 0)	(0, 0.01]
2005-2006 Q1	21.86	3.14	3.68	4.54
Q2	20.59	3.16	3.32	4.11
Q3	20.99	2.99	3.17	3.87
Q4	21.56	2.67	3.16	3.87
2006-2007 Q1	20.36	3.05	3.24	4.00
Q2	20.60	3.09	3.25	4.01
Q3	20.77	2.86	3.03	3.58
Q4	22.65	2.49	3.20	3.88
2007-2008 Q1	20.85	2.99	3.13	3.93
Q2	20.83	3.04	3.26	4.02
Q3	25.41	3.10	3.62	4.24
Q4	24.48	2.94	3.45	4.04
2008-2009 Q1	25.45	3.26	3.61	4.21
Q2	26.70	4.16	4.47	6.02
Q3	29.26	4.78	4.87	6.01
Q4	33.09	5.22	6.18	6.40
2009-2010 Q1	32.43	6.74	6.66	8.06
Q2	29.61	7.73	7.13	8.05
Q3	29.45	7.15	6.62	7.27
Q4	27.66	6.48	6.33	7.44
2010-2011 Q1	27.78	6.59	6.20	7.58
Q2	26.98	6.99	6.30	7.85
Q3	28.53	6.56	5.36	6.94
Q4	29.77	5.74	5.31	6.66
2011-2012 Q1	30.11	5.26	5.15	6.47
Q2	25.54	6.17	5.15	6.58
Q3	23.73	5.21	4.66	6.25
Q4	27.95	5.56	5.36	6.77
2012-2013 Q1	24.30	5.92	5.13	6.83
Q2	23.20	5.46	5.09	6.74
Q3	24.88	4.48	4.68	6.15
Q4	24.29	3.73	4.48	5.69
2013-2014 Q1	22.46	3.90	4.33	5.85
Q2	21.65	4.11	4.51	6.08
Q3	23.88	3.81	4.69	5.75
Q4	22.79	3.47	4.41	5.64
2014-2015 Q1	23.07	3.71	4.29	5.53
Q2	21.75	3.84	4.30	5.52
Q3	21.57	3.42	3.82	4.96
Q4	21.24	2.89	3.67	4.55

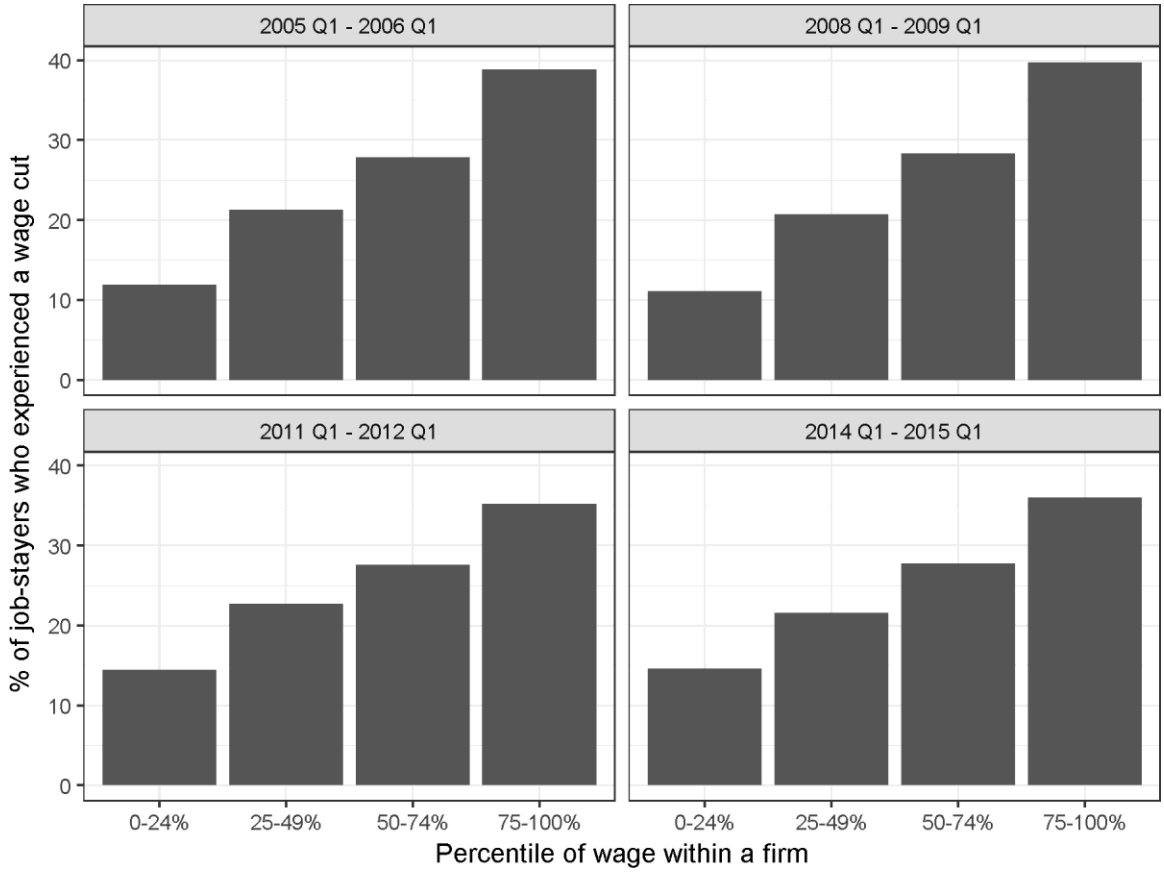
Source: Authors' calculations based on unemployment insurance records from the Washington Employment Security Department.

Table 2. Percentages Receiving Nominal Wage Cuts and Freezes among Washington State Full-Quarter Job Stayers with 480-560 Quarterly Hours of Work

Period		Wage Cut	Wage Freeze
2005-2006	Q1	16.62	3.91
	Q2	15.91	4.20
	Q3	16.11	4.17
	Q4	16.90	3.03
2006-2007	Q1	16.61	3.71
	Q2	14.69	3.82
	Q3	14.47	3.72
	Q4	17.24	2.69
2007-2008	Q1	16.85	3.51
	Q2	15.80	3.70
	Q3	21.10	3.97
	Q4	20.95	2.88
2008-2009	Q1	21.66	4.22
	Q2	24.16	6.02
	Q3	26.78	7.65
	Q4	31.77	8.66
2009-2010	Q1	29.14	11.62
	Q2	25.43	11.48
	Q3	22.79	10.73
	Q4	23.73	8.67
2010-2011	Q1	21.41	9.90
	Q2	21.99	10.09
	Q3	23.45	9.38
	Q4	25.58	7.50
2011-2012	Q1	28.74	6.21
	Q2	22.59	8.98
	Q3	18.49	6.35
	Q4	23.62	6.99
2012-2013	Q1	20.24	7.50
	Q2	18.44	6.41
	Q3	19.41	6.06
	Q4	19.35	4.64
2013-2014	Q1	18.45	4.85
	Q2	16.87	5.62
	Q3	19.00	5.12
	Q4	19.58	4.44
2014-2015	Q1	18.21	4.63
	Q2	18.79	4.80
	Q3	17.90	4.55
	Q4	21.24	2.89

Source: Authors' calculations based on unemployment insurance records from the Washington Employment Security Department.

Appendix Figure 1. Distributions of Within-Firm Wage Rank of Washington Job Stayers Who Received Wage Cuts in Firms with 20 or More Stayers (At Least One of Whom Did Not Receive a Wage Cut)



Source: Authors' calculations based on unemployment insurance records from the Washington Employment Security Department.

Appendix Table 1. Percentages Receiving Nominal Wage Cuts among Washington State Job Stayers in Selected Industries

Period		Utilities	Mining and Oil and Gas Extraction
2005-2006	Q1	13.80	16.82
	Q2	24.97	30.26
	Q3	15.79	25.97
	Q4	16.01	32.76
2006-2007	Q1	19.51	27.42
	Q2	18.14	18.86
	Q3	22.51	22.58
	Q4	27.90	26.20
2007-2008	Q1	29.32	22.48
	Q2	20.89	30.28
	Q3	14.44	33.69
	Q4	18.64	39.11
2008-2009	Q1	18.28	32.05
	Q2	19.43	39.05
	Q3	20.12	40.84
	Q4	16.80	41.80
2009-2010	Q1	28.08	38.23
	Q2	23.43	33.01
	Q3	21.95	32.05
	Q4	22.10	34.78
2010-2011	Q1	17.94	29.60
	Q2	21.30	32.97
	Q3	17.54	34.53
	Q4	22.60	38.48
2011-2012	Q1	23.10	28.62
	Q2	16.70	30.80
	Q3	18.65	28.75
	Q4	18.81	33.95
2012-2013	Q1	25.18	28.61
	Q2	15.71	28.18
	Q3	15.34	27.60
	Q4	18.23	30.05
2013-2014	Q1	26.18	26.88
	Q2	19.04	26.13
	Q3	24.52	27.12
	Q4	18.85	24.18
2014-2015	Q1	18.47	22.87
	Q2	17.24	23.21
	Q3	18.74	27.31
	Q4	16.92	28.77

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