Downward Wage Rigidity in the United States: New Evidence from Administrative Data*

André Kurmann
Drexel University

Erika McEntarfer
U.S. Census Bureau

May 12, 2017

Abstract

This paper examines the extent and consequences of downward nominal wage rigidity using administrative worker-firm linked data for a large U.S. state. The distribution of nominal hourly wages changes of job stayers exhibits substantially less asymmetry and a smaller spike at zero than has been previously documented based on survey data. During the Great Recession, the proportion of job stayers experiencing a wage cut rose markedly, followed by a sustained increase in the proportion of wage freezes as the economy recovered. We also document a fairly symmetric distribution of hours changes in the data, with cyclical cuts in hours for job stayers. As a result, the distribution of nominal earnings changes of job stayers is more symmetric, meaning workers face higher annual earnings risk than is reflected in the wage change distribution. We rationalize these findings with a model of downward wage rigidity that features selection effects from both hiring and separations. We then exploit the worker-firm link of the data and find that during the Great Recession, firms with indicators of downward nominal wage rigidity had systematically higher job destruction and separation rates and lower job creation and hiring rates.

*Kurmann: LeBow College of Business, Drexel University (email: kurmann.andre@gmail.com); Erika McEntarfer: Center for Economic Studies, U.S. Census Bureau (email: erika.mcentarfer@census.gov). We are grateful to Cynthia Forland and the Washington Employment Security Department for permission to use the Washington data for this project and feedback on our work. We also thank Jim Spletzer for earlier involvement in the project, and Gary Solon for insightful comments.

1 Introduction

There is a long-standing argument in macroeconomics dating back to Keynes (1936) that nominal wages are difficult to adjust downward and that, as a result, firms lay off more workers in response to adverse shocks than they would otherwise. Further, as posited by Tobin (1972), if this downward rigidity pertains to nominal wages, moderate levels of inflation may "grease the wheels of the labor market" by making the constraint bind less often, thereby exerting a positive effect on economic activity.

The Great Recession of 2008-09 with its large rise in unemployment and concurrent decline in inflation close to zero brought renewed interest in this Downward Nominal Wage Rigidity (DNWR) hypothesis. Empirical studies based on survey data by Daly and Hobijn (2014), Elsby et al. (2016), and Fallick et al. (2016) indicate that the proportion of wage freezes increased noticeably between 2006 and 2011 while the proportion of job stayers with wage cuts remained relatively stable. This evidence led some researchers to conclude that DNWR has played an important role for both the large decline in employment during the Great Recession and the subsequent slow recovery (e.g. Daly et al., 2012). In parallel, a growing number of papers incorporate DNWR as a constraint into modern macro models to investigate its consequences.[1]

In this paper, we use administrative worker-firm linked data to provide new evidence about the extent and consequences of DNWR for the United States. We focus on three key questions:

1. What are the characteristics of the wage change distribution of job stayers and is the evidence for the Great Recession consistent with DNWR?

2. How do hours and earnings adjust relative to wage rates and how does this affect the implications of DNWR?

3. Do firms with indicators of DNWR exhibit different employment dynamics during the Great Recession than unconstrained firms?

The data we use comes from the Longitudinal Employer Household Dynamics (LEHD) program of the U.S. Census Bureau, which is based on worker-specific records that employers submit every

[1] A list of recent papers include Kim and Ruge-Murcia (2009); Benigno and Ricci (2011); Abbritti and Fahr (2013), Eliaz and Spiegler (2013), Schmitt-Grohe and Uribe (2013; 2016; 2017), Daly and Hobijn (2014), Eggertsson and Mehrotra (2015), Auclert and Rognlie (2016) and Dupraz et al. (2016).

quarter to state unemployment insurance (UI) offices. We concentrate our analysis on one large, nationally representative state – Washington – because unlike most other states, Washington's UI office requires employers to provide information not only on worker earnings but also on hours, allowing us to calculate average hourly wage changes for each job stayer. The sample period available extends from 1998 to 2014 and therefore includes the Great Recession and its aftermath as well as the 2001 recession.

Our data offers several advantages over household survey data that are commonly used to assess the wage dynamics of job stayers in the U.S.[2] First, the administrative nature of the UI records means that our data, while not entirely free from error, should not be subject to the type of rounding and reporting errors that have been shown to affect wage data from household surveys. Concerns about these issues have led many to question the reliability of results based on household surveys, with several prominent studies arguing that the incidence of wage cuts is substantially overstated if the data are not corrected for measurement error whereas other studies report exactly the opposite. Our data allows us to shed new light on this important debate.[3]

Second, the earnings records of our data include all forms of monetary compensation paid to workers. This is crucial when estimating the extent of DNWR since firms may use irregular payments such as bonuses and overtime pay to incentivize workers and adjust labor costs. In contrast, the wage data from household surveys typically apply to a more limited earnings concept (i.e. base pay or usual earnings) and are in certain cases affected by top-coding.

Third and perhaps most importantly, the worker-firm link of our data combined with the fact that Washington's UI records cover about 95 percent of private employment in the state allows us to test in a large sample whether firms with indicators of DNWR have systematically different employment policies than unconstrained firms. Assessing the consequences of DNWR has proven largely elusive so far since survey data for the U.S. do not contain information about

[2]Prominent studies based on either the Current Population Survey (CPS), the Panel Study of Income Dynamics (PSID), or the Survey of Income and Program Participation (SIPP) include McLaughlin (1994); Card and Hyslop (1997); Kahn (1997); Altonji and Devereux (2000); Gottschalk (2005); Dickens et al. (2007); Elsby (2009); Daly et al. (2012); Barratieri et al. (2014); Daly and Hobijn (2014); and Elsby et al. (2016).

[3]A similar point is made by Lebow et al. (2003) and Fallick et al. (2016) who use firm-based survey data from the Employment Cost Index (ECI) instead. While measurement issues are much less of a concern for the ECI, its unit of observation is the job instead of the worker, which makes it less informative for theories of wage rigidities, and the absence of firm-level employment counts does not allow the type of empirical analysis we carry out with our data.

firm employment. Yet, this is the central question since spot wages may not be allocative and therefore, nominal wage rigidities may not matter for labor market outcomes.[4] More specifically, for DNWR to have played an important role during the Great Recession, it has to impact not only layoffs, as typically implied by the literature, but also hiring. Indeed, as documented for example by Elsby et al. (2010), the distinctive feature of the Great Recession is not the increase in layoffs, which rose sharply in the beginning of the downturn but was about similar in magnitude to previous severe recessions, but the unusually large and persistent decline in hiring. Our data allows us to assess this question as we can measure not only net employment growth at the firm level but also gross hiring and separation rates. Moreover, the LEHD data infrastructure makes it possible to link the UI records to other important worker and firm-specific control variables, in particular firm revenue from the Business Register, which should help inference.

The results with regards to our first question are as follows. Consistent with the notion that firms are reluctant to reduce wages, the wage change distribution of job stayers features a noticeable spike at zero averaging about 10 percent per year and missing mass to the left of zero. At the same time, the distribution contains a substantial fraction of wage cuts of about 25 percent per year, rejecting the hypothesis of perfect (or even near-perfect) DNWR imposed in a number of recent macroeconomic models. During the Great Recession, the proportion of job stayers experiencing wage cuts increased to about 30 percent, followed by an increase in the zero spike that peaked at 16 percent in 2010 as private-sector employment in Washington started to recover.[5] The delayed increase in wage freezes concords with the survey-based results in Daly and Hobijn (2014), Elsby et al. (2016), and Fallick et al. (2016) although the magnitude of the increase is substantially larger according to our data.

At first glance, the results provide evidence both for and against the view that DNWR was operative during the Great Recession. On one hand, the increased incidence of wage cuts during the downturn suggests that DNWR may not be a binding constraint in times of large negative shocks. On the other hand, the large rise in the proportion of wage freezes as the economy started to stabilize goes against this interpretation. We try to reconcile these findings through a dynamic model of firm labor demand subject to DNWR that features long-lived employment relationships

[4] See Barro (1977) and Hall (1980) for their well-known critique of the importance of nominal wage contracts, and discussions of labor search models for a modern incarnation.

[5] A similarly timed but substantially smaller increase in the zero spike occured during the 2001 recession.

and selection effects from hiring and separations. As in Elsby (2009), DNWR is introduced through an efficiency wage assumption according to which, workers' effort reacts negatively to nominal wage cuts. The model implies that on average, DNWR-constrained firms do not necessarily have higher separation rates as they hire on average more productive workers. In response to a large negative productivity shock, however, DNWR-constrained firms account for a disproportionate fraction of separations. This implies that even though the proportion of both wage cuts and wage freezes would go up if all workers remained employed, the *observed* incidence of wage freezes among job stayers remains about unchanged at first. As the economy recovers, DNWR-constrained firms delay wage increases as their employees' past wages are high relative to productivity. This "echo effect" provides an explanation for the gradual increase in wage freezes that peaks as the recovery is already underway. Lastly, the model implies that DNWR exacerbates the adverse effects on hiring of negative productivity shocks, especially in an environment of low inflation expectations. At least qualitatively, the model can therefore rationalize the observed variations in the wage change distribution during the Great Recession, thus illustrating that the effects of DNWR may be more subtle than typically implied.

With regards to our second line of inquiry, the DNWR literature has focused almost exclusively on labor adjustments at the extensive margin. However, firms also adjust the intensive margin in response to shocks. This margin, which has been largely neglected by the literature, provides another channel through which DNWR may affect labor market outcomes. We use our data to analyze the distribution of hours changes and earnings changes of job stayers and find that both are much more symmetric with greater mass below zero than the wage change distribution.[6] We also find evidence of cyclical declines in hours, with hours cuts happening more frequently during both the Great Recession and the 2001 recession. In a formal decomposition, we estimate that while wage and hours increases contribute on average about equally to earnings increases, hours cuts account on average for about 70 percent of earnings declines. This suggests that hours are more flexible downward than wage rates, consistent with the notion that the firm's wage decision also affects the intensive margin. By reducing hours in response to adverse shocks, firms may therefore be able to reduce labor costs even if they are reluctant to cut wages, thereby mitigating

[6] While part-time workers experience the largest fluctuations in hours, the frequency of hours changes for full-time workers is also quite large.

some of the negative effects of DNWR on employment.

Lastly we exploit the worker-firm link of our data to estimate the effects of DNWR on employment dynamics at the firm level. Informed by our model, we construct wage change distributions at the firm-level and estimate measures of DNWR during the mid-2000 recovery when selection effects were presumably less important. We then test whether these DNWR measures are systematically related to firm employment dynamics. Consistent with the model, we find that DNWR-constrained firms do *on average* not exhibit different employment growth than unconstrained firms. During the Great Recession, however, firms with indicators of DNWR have systematically higher job destruction and separation rates and lower job creation and hiring rates. These effects are highly significant, even after controlling for firm fixed effects, year controls and firm-specific revenue changes. Across the different specifications, a firm subject to the average degree of DNWR experiences about 1 percent lower annual employment growth in 2008 and 2009 relative to a firm with no evidence of DNWR. To put this number in perspective, total non-farm employment fell about 6 percent in Washington during the Great Recession.

The remainder of the paper is structured as follows. Section 2 describes the linked employer-employee data for Washington state and how the wage change distribution of job stayers is constructed. Section 3 reports distributional features of the aggregate wage, hours, and earnings change distributions. Section 4 outlines the model that we use to analyze the implications of DNWR. Section 5 describes the firm-level analysis of how meausres of DNWR relate to employment change during the Great Recession. Section 6 concludes with a brief review of the results and directions for future research.

2 Data

The core of our data comes from the Longitudinal Employer Household Dynamics (LEHD) program for one large U.S. state, Washington. The LEHD consists of worker-specific earnings records that employers submit every quarter to state Unemployment Insurance (UI) offices. States submit the UI records to the LEHD as part of the Local Employment Dynamics (LED) federal-state partnership, along with establishment-level information on industry and location that are collected as part of the Quarterly Census of Employment and Wages (QCEW). The LEHD program then further augments this data with information on worker age, gender, and place of residence using census, survey, and other administrative records to produce the U.S. Census Bureau's Quarterly Workforce Indicators, LEHD Origin-Destination Employment Statistics, and Job-to-Job Flows.[7] In this paper we will further enhance the firm-level LEHD micro-data with Census Business Register data (linked via the federal employer tax identifiers) to incorporate information on revenue changes at the firm level. Washington is one of a handful of states that collects information on hours paid (as well as earnings) from employers in their unemployment insurance wage data.[8] This allows us to compute average hourly wage rates per quarter for each workers.

As highlighted in the introduction, our linked employer-employee data has several key advantages over survey-based sources that are typically used to compute wage dynamics for the U.S. First, the LEHD data is based on administrative earnings records which, while not entirely free from error or noise, are not subject to rounding and recall errors that plague survey-based measures and are likely to bias wage change statistics. Second, by definition of UI reporting requirements, UI earnings include all forms of monetary compensation received throughout a quarter and not just the base wage (i.e. gross wages and salaries, bonuses, stock options, tips and other gratuities, and the value of meals and lodging, where supplied).[9] Aside from employer-covered benefits, LEHD earnings therefore capture the total labor cost of a worker to the firm. Third, the worker-firm link in the LEHD data allows us relate employment dynamics to measures of DNWR at the firm level, controlling for firm-specific characteristics such as total number of workers, average earnings, average tenure, or gender, race and age composition. Importantly, the employer level data from the LEHD can also be linked to firm-specific data from other Census datasets, such as revenue data from the Business Register. This will be important for the inference conducted in Section 5 of the paper. Lastly, the LEHD data covers over 95 percent of private-sector workers in the participating

[7] For a full description of the LEHD data, see Abowd et al. (2009).

[8] Other states that collect hours information on UI wage data are Minnesota, Rhode Island, and Oregon (Louisiana has also recently begun experimentally collecting hours data). However, Rhode Island and Oregon only began collecting hours data relatively recently. Minnesota, which has collected hours data on UI for quite some time, did not send Census hours data for several years in the middle of our time period of interest. Minnesota also appears to have a relatively high non-response rate for hours, relative to Washington, at least in the files delivered to Census. This paper is part of a larger project at Census investigating the feasibility of using administratively provided hours data in Census public use data products as more states begin to collect hours data.

[9] See http://www.bls.gov/opub/hom/pdf/homch5.pdf

U.S. states.[10] The size of the dataset allows us to cut the sample in several important dimensions while still having very large samples.

2.1 Hourly wage and earnings changes of job stayers

Our analysis focuses on annual changes in average hourly wages and earnings of job stayers. We focus on annual instead of quarterly changes because a substantial fraction of workers receive bonuses and other end-of-year payments that are recorded in a particular quarter but reflect performance over a longer period of time. These payments are a potentially important component of labor cost, but their exact timing may not be as relevant for the firm's employment decision.

We compute the change in the average hourly wage (called 'hourly wage' henceforth) in two different ways. First, we obtain the hourly wage for each quarter by dividing quarterly earnings by quarterly hours paid and then compute the change in the hourly wage as the log difference between the hourly wage for a given quarter and the hourly wage for the corresponding quarter one year later. We call this the *four-quarter change*. Alternatively, we obtain the hourly wage for each year by dividing annual earnings by annual hours paid and then compute the log difference of the average annual hourly wage between two years. We call this the *year-to-year change*.

For earnings and hours, we could in principle also compute four-quarter changes. However, firms report to the UI system earnings and hours *paid* rather than *accrued* during the quarter. Whenever the number of pay periods per quarter differs, this results in potentially large spurious changes in earnings and hours that, absent worker-firm specific knowledge of the pay modalities, are hard to correct in a precise manner.[11] In our inspection of the data we find that on an annual basis, this pay-period problem largely disappears, which is why we only consider year-to-year changes in earnings and hours paid.

The 4-quarter change in the hourly wage is close to the one employed by much of the existing literature, where the hourly wage rate (either reported directly for hourly-paid workers or computed as the ratio of reported earnings to reported hours) refers to a relatively short reference

[10] State UI data also covers certain employees in state and local government. Our analysis considers workers employed in private-sector firms, although the analysis could in principle be extended to local and state government workers.

[11] As an example, consider a worker with 26 bi-weekly pay periods per year. Two quarters will have six pay periods and two quarters will have seven pay periods.

period prior to the interview.[12] We use this calculation to compare our results to the existing literature. The year-to-year change in the hourly wage takes into account wage changes over the entire two-year period, which makes it less comparable to the existing literature but allows us to directly relate it to annual earnings changes as well as annual hours changes.

In order to be retained as a job stayer for our analysis, a worker has to remain with the same firm for at least ten consecutive quarters: the eight quarters for which we compute year-to-year changes in earnings plus the last quarter preceding the first year and the first quarter following the second year. These surrounding two quarters are part of the selection criteria so as to ensure that we consider as job stayers only workers who remain with the firm the entire eight quarters of the two calendar years. Otherwise, our selection criteria would include workers whose employment either started during the first quarter of the first year or whose employment ended during the fourth quarter of the second year, thereby leading to spurious year-to-year changes in earnings.

A potential concern for our analysis is the quality of the hours data. Washington uses hours worked in the previous year as part of the eligibility requirements for UI, so the UI office is incentivized to collect accurate and complete information on hours from employers. Hours reported are generally hours worked, with two exceptions: hours on leave with pay are recorded as hours worked, and salaried workers only have hours worked reported if they are tracked by the employer; otherwise employers are instructed to report 40 hours per week. In our inspection of the data, we find the characteristics of the hours data to be sensible. As Figure A.1 in the Appendix shows, the distribution of weekly hours for job stayers has a large peak at 40 hours, with 55% of the mass between 35 and 43 hours; and full-time workers are more likely to have high hourly wages. The peak at 40 hours suggests that many employers do not track or report hours worked of their salaried workers; so we will characterize hours as generally representing hours paid rather than hours worked.[13] The distribution of annual hours *changes,* reported in Figure 4 (discussed in further detail below), is nicely behaved with 20 to 25 percent of job-stayers experiencing a zero

[12]For example, for the CPS ORGs used by Card and Hyslop (1999), Daly et al. (2013) or Elsby et al. (2013), the reference period for the hourly wage / earnings questions is the week prior to the interview. For the SIPP data used by Gottschalk (2005) and Barratieri, Basu and Gottschalk (2014), the reference period is the month of the interview.

[13]For hourly paid workers, this distinction should not matter although one may be concerned more generally that hours reported by the employer do not reflect effective hours worked. When viewed through the lens of the model presented below, one can interpret the difference between reported hours and effective hours as worker effort per hour paid, which the firm tries to manage through its wage policy.

hours change over a two-year period, and part-time time workers being much more likely to see their hours change over time (see Figure A.1 in the Appendix). Finally, a firm-level regression of the proportion of job stayers with zero hours change can account for almost 70 percent of the variation across firms, with the main explanatory variables being firm size and industry fixed effects (e.g. firms in education, finance, and utilities employ a larger proportion of workers with zero hours change, presumably reflecting the higher share of salaried workers in these industries). These results make us fairly confident that the hours data collected by Washington UI offices is of reasonably high quality.

2.2 Sample characteristics

The main sample for the descriptive analysis of hourly wage and earnings changes in Section 3 consists of all 10-quarter job stayers employed in private-sector firms in Washington State between 1998:3 and 2014:1, as described above. For the firm-specific regressions in Section 5, we work with the subsample of employers that we can match to revenue data from the Business Register and have at least 50 job stayers over the 2004-2007 period. These sample restrictions are necessary for the firm-level regressions because we need employers with well-defined wage change distributions to identify employers with evidence of DNWR. Additionally, we link in revenue data to estimate the size of demand shocks to the employer using federal tax identifiers (EINS) on both files. However, we cannot match every employer in the LEHD data to the Business Register using the EIN.

Table 1 reports basic characteristics of employers of all job stayers (Column 1) as well as employers in the employer-level regression sample (Column 3). For comparison, the characteristics of employers in Column 1 that match the revenue data are provided in Column 2. The pooled sample of all job-stayers has a total of 11.8 million observations or about 1 million observations per year. This is orders of magnitude larger than the sample size of the household surveys with job-stayer wage information available for the United States. Even in the firm sample used for the regression analysis, there is a total of 6 million job-stayers over all years, or about 500,000 observations per year. Other than sample size, the principal difference between the stayers sample and the employer sample used in the firm-level regressions is that employers in the employer-level sample are much more likely to be mid-size and larger employers, with a small share of the employer sample representing employers less than 50 employees. As can be seen by comparing

Columns 2 and 3, this skewness towards larger employers is principally due to our need to restrict the sample to firms with enough job stayers to estimate a firm-level wage change distribution, and not from the match to the revenue data. Interestingly, the industry composition of the firm sample is quite similar as in the sample of all job stayers. Industries with large firms such as manufacturing, education, and health receive a somewhat larger weight in the firm sample but overall, these differences look small.

2.3 Histograms and distributional statistics

In the following section, we report results on the hourly wage and earnings change distribution of job-stayers non-parametrically through histograms. In line with much of the DNWR literature, all histograms show log changes grouped in 1% bins centered around zero; i.e. the zero bin contains all log changes between -0.005 and 0.005, which approximately corresponds to changes between -0.5 and 0.5 percent; the adjacent intervals contain observations in between -0.015 and -0.005, respectively 0.005 and 0.015; and so forth. In total, we have 51 intervals, with two open-ended intervals for observations smaller than -0.255 and observations exceeding 0.255.

To characterize the different distributions, we introduce a set of distributional statistics. Let $F(\cdot)$ be the cumulative density of a wage or earnings change distribution. We define *mass at zero* as

$$M_0 = F(0.005) - F(-0.005);$$

excess zero spike as

$$ES_0 = [F(0.005) - F(-0.005)] + [F(2 \times median + 0.005) - F(2 \times median - 0.005)];$$

and *missing mass left of zero* as

$$MM_{<0} = 1 - F(2 \times median + 0.005) - F(-0.005).$$

The second and third statistics are closely related to asymmetry measures that the literature has associated with DNWR; e.g. Lebow, Stockton and Wascher (1995), Card and Hyslop (1997), Kahn (1997) or Lebow, Saks and Wilson (2003). Although we will use these indicators to measure the degree of DNWR at firms and in the aggregate, we note some nuance in interpreting these asymmetry statistics as indicators of DNWR. First, even in the absence of DNWR, the wage change distribution of job stayers may be asymmetric because of asymmetries in the distribution of worker productivity growth, or non-linearities in the wage setting process that are unrelated to DWR. Second, as the model in Section 3 illustrates, selection effects from hiring and separation can affect the wage change distribution of job stayers in important and nontrivial ways.

As additional distributional statistics, we consider the *dispersion* between the 25^{th} and the 75^{th} percentile of the distributions; i.e.

$$D_{25-75} = P75 - P25;$$

and *Kelley's skewness*, which is defined as

$$KS = \frac{(P90 - P50) - (P50 - P10)}{P90 - P10} = 1 - 2 \times \frac{P50 - P10}{P90 - P10}$$

Dispersion is an inverse measure of compression that, as Elsby (2008) argued, should be associated with the extent to which DNWR binds (see Section 4). In turn, Kelley's skewness is a complementary measure of asymmetry that allows for an interesting comparison with the work by Guvenen et al. (2015, 2016) on the distribution of earnings changes in administrative data from the Social Security Administration (SSA).

3 Aggregate Wage and Earnings Change Distributions

Much of the existing DNWR literature for the U.S. focuses on hourly wage changes. We therefore start by documenting the hourly wage change distribution of job stayers in our Washington state data and compare it to estimates obtained in the literature based on survey data. We then examine the underlying hours change distributions and earnings change distributions.

3.1 Hourly wage changes

Figure 1 shows the histogram of four-quarter hourly wage changes for job stayers employed by private-sector firms in Washington, pooled over the 1998-2013 period.[14] The distribution features a noticeable spike at zero of about 10 percent and some missing mass to the left of zero. At the same time, the distribution contains a substantial fraction of wage cuts, totaling about 25 percent.

While Figure 1 shares similarities with results from U.S. survey data, closer inspection reveals that our wage change distribution contains fewer zero wage changes and is at the same time more concentrated. Our wage change distribution therefore looks relatively symmetric. In comparison, Daly and Hobijn (2014) report based on CPS data that the fraction of workers with the same wage as one year prior is about 13 percent over the same time period. Since their overall distribution is markedly more disperse, this implies an *excess* zero spike that is much larger than in our data.[15] According to the PSID and the SIPP, the proportion of zero wage changes relative to the rest of the distribution is even higher.[16]

Given the administrative nature of our data, the difference in results suggests that the survey data may be affected both by rounding error (especially for workers experiencing modest wage changes), which would exaggerate the spike at zero, and by classical reporting error, which would result in a more disperse overall distribution. Another potential explanation for the lower spike at zero in Figure 1 is that, as described above, the earnings records in our data contain all compensation paid to workers, including overtime pay, commissions and bonuses, whereas the earnings questions in the different survey datasets typically ask about regular pay only.[17] As long

[14] All results here pertain to four-quarter wage changes for the second quarter (i.e. Q2 relative to Q2 of the previous year). Alternatively, we could show results pooled over all four-quarter wage changes. This would reduce the proportion of zero wage changes as wage changes based on the fourth quarter are more variable, presumably because of irregular earnings that are paid out disproportionally towards the end of the year. See below for additional discussion.

[15] The results of Daly and Hobijn (2014) pertain to all workers who report a wage. However, simialar results obtain when the sample is restricted to job stayers. See the "Wage Rigidity Meter" website maintained by the Federal Reserve Bank of San Francisco. Elsby et al. (2016) report results that are broadly similar to Daly and Hobjin's although they report wage change statistics only for hourly paid workers, for which the proportion of zeros is substantially higher. Moreover, their histograms pertain to 0.02 log change bins, which makes their wage change distributions appear more concentrated than they would be for 0.01 log change bins, which is the bin size that Daly and Hobijn and we use to report results.

[16] For the SIPP, see for example Gottschalk (2005) who finds that about 25 percent of job stayers report zero wage changes per year for the 1986-1993 period. For the PSID, see for example Dickens et al. (2007) who, for 1987, show a similarly disperse wage change distribution as Daly and Hobijn (2014) with a zero spike of about 15 percent.

[17] In particular, the CPS data that Daly and Hobijn (2014) and Elsby et al. (2016) use do not cover irregular

as firms adjust irregular pay components more flexibly, this contributes to the smaller proportion of zero wage changes in our data than in the survey data. In line with this point, we find that wage change distributions computed from fourth quarter records (i.e. Q4 to Q4), which contain a dispropoportionate share of irregular compensation, have a lower spike at zero and are more disperse than the distributions computed based on the other quarters.[18]

Despite these differences, an important common finding of our analysis and the aforementioned papers is that wage cuts are far from a rare occurrence. This contrasts with Akerlof et al. (1996), Altonji and Devereux (2000), Gottschalk (2005) or Barratieri et al. (2014) who argue that due to (classical) reporting error, the incidence of wage cuts in household survey data is substantially overstated and that once one corrects for these errors using econometric methods, wage cuts become the exception and the probability of a zero wage change rises as high as 50 percent per year. As the above discussion should make clear, this does not imply that measurement error in household surveys is unimportant. Our evidence simply challenges the notion that measurement error biases the incidence of wage freezes in household survey data downward. Instead, comparison between our results and the ones reported in the literature suggest that rounding error may impart at least as important of a bias in the other direction and that wage cuts are in fact quite common. Interestingly, this is consistent with results in Smith (2000), Nickell and Quintini (2003) and Elsby et al. (2016)who find in U.K. data that administrative data feature a substantially lower incicidence of zero wage changes than household survey data.[19]

Figure 2 compares the wage change distribution for 2005-2006, two years prior to the Great Recession, with 2009-2010 as the economy started to recover. Two observations stand out. First is the marked increase in the share of workers with wage freezes from 7.4 percent in 2005-2006 to 16 percent in 2009-2010. Second is that the wage change distribution shifts noticeably to the left and becomes more compressed, containing more small wage cuts and raises but fewer large wage increases in 2009-2010 than in 2005-2006. The increase in the spike at zero during the Great Recession mirrors the findings by Daly and Hobijn (2014) based on CPS data and has also been

bonus payments and overtime compensation of hourly paid workers.

[18] This finding is consistent with Babecky et al. (2012) who analyze survey data from 12 European countries and find that firms frequently use margins other than changes in the base wage to adjust labor costs. Similarly, Altonji and Devereux (2000) report in a study of personnel files of a large financial corporation that reduction in bonuses are quite common.

[19] See Elsby et al. (2016) for a nice discussion of this evidence.

noted by Elsby et al. (2016). However, the magnitude of the increase in our data – almost 9 percent – is substantially larger than the 4 percent reported in Daly and Hobijn (2014).

Figure 3 provides further evidence on the variation in the wage change distribution over time. Panel (a) shows the quarterly time series of the 25th, 50th, and 75th percentile points of the four-quarter hourly wage change distribution from 1998:3 to 2014:1.[20] There is a remarkable compression of the wage change distribution during both the Great Recession and the 2001 recession. While the 25th percentile remains steady around zero except for a temporary dip to about -3 percent in 2008-2009 when the share of workers receiving wage cuts rises to 30%, the 50th and 75th percentiles decline during both recessions and then gradually shift back up as the economy recovers.

Panels (b) and (c) of Figure 3 show that there are equally if not more important variations in the asymmetry of the wage change distribution. As employment contracts at the start of the Great Recession, the missing mass left of zero drops markedly from about 9 percent to 4 percent before recovering to its pre-recession level by 2010. Similarly, Kelley's Skewness drops to zero by the fourth quarter of 2009 before sharply increasing again. Both of these changes indicate that firms cut wages more frequently during the depths of the Great Recession. Interestingly, the proportion of wage freezes remains approximately unchanged throughout that time but then increases as the economy starts to stabilize, peaking at the previously noted higher level of 16 percent in early 2010 exactly when Washington's private-sector employment bottoms out and starts growing again. The excess zero spike remains equally steady at about 4 percent until mid-2009 before growing to 10.7 percent by early 2010. A similarly timed but substantially smaller increase in zero mass and excess zero spike occurs for the 2001 recession. The compression of wage gains during recessions and the delayed increase in wage freezes is also highlighted by Daly and Hobijn (2014) except that, as we noted above, the magnitude of the increase in zero wage changes during the Great Recession is substantially larger in our data.

Taking stock, while the substantial fraction of wage cuts observed in our data rejects the hypothesis of perfect (or even near-perfect) DNWR imposed in a number of recent macroeconomic

[20] In this figure, we display statistics for the hourly wage change distributions for each quarter of the sample relative to 4 quarters prior; i.e. the statistics pertaining to, say, 2009:3 are for wage changes between 2009:3 and 2008:3. For confidentiality purposes, all released percentile points are fuzzed by taking a 5-percentile average around the percentile point of interest, e.g. reported medians are the average of the 48th, 49th, 50th, 51st, and 52nd percentiles.

studies, the noticeable spike at zero together with missing mass left of zero in Figure 1 suggests that on average, firms are least partially reluctant to cut wages of their employees. The variations in the wage change distribution over the last decade are at first glance somewhat contradictory as to whether DNWR was operative during the Great Recession. On one hand, the sharp drop in asymmetry and the increase in the proportion of wage cuts in the beginning of the Great Recession would imply that firms are more willing to cut wages in times of large negative shocks – i.e. exactly when DNWR would have the largest bite – and that as a result, DNWR may not be as important of a constraint for business cycle fluctuations. On the other hand, the large rise in the proportion of wage freezes that started in the second half of Great Recession and extended to when private-sector employment started to recover goes against this interpretation. Similarly, the increase in compression in the wage change distribution driven primarily by smaller wage increases is consistent with the argument in Elsby (2008) that DNWR-constrained firms react to lower growth prospects by giving workers smaller raises, due to the higher risk of being downward constrained in the future. These observations suggest that the effects of DNWR may be more subtle than typically assumed in the literature, which motivates our attempt in Section 4 to explore the implications of DNWR more formally through a model.

3.2 Earnings changes and the importance of hours adjustments

Although most of the existing DNWR literature for the U.S. focuses on hourly wage changes, earnings changes of job stayers are equally relevant. First, worker resistance to wage cuts is often cited as the source of DNWR. But we might expect workers to be similarly resistant to reductions in earnings. Second, firms can adjust labor cost not only through wage cuts but also through reductions in paid hours or temporary furloughs. Hence, the firm's ability to reduce paid hours in response to a negative shock may be as important for its employment decisions as the ability to cut hourly wage rates. Finally, the properties of earnings risk of job-stayers are interesting for independent reasons as they are a key ingredient for the consumption literature (e.g. Guvenen et al., 2016).

Due to above mentioned pay-period problem, we only report distributions of year-to-year changes for earnings and hours. The corresponding distribution in year-to year changes in hourly wages has the same properties as described above for the distribution of four-quarter changes, but it is somewhat more disperse and has a lower mass at zero. This is unsurprising given that year-to-year changes consider wage changes over a two year period whereas four-quarter changes carry only over one year.

Figure 4 shows the distribution of hours changes and earnings changes for 2005-2006 and 2009-2010. As shown in Panel (a), about 23 percent of job stayers see their hours unchanged. For the rest, hours changes are relatively small and close to symmetrically distributed. For 2009-2010, the hours change distribution shifts left with a somewhat smaller proportion of job stayers with unchanged hours and a larger fraction of job stayers experiencing a reduction in hours.

As Panel (b) of Figure 4 shows, the distribution of earnings changes is much more dispersed and symmetric than the distribution of hourly wage changes for the same years in Figure 2.[21] For 2005-2006, the incidence of earnings cuts is about 27 percent and there is barely an excess spike at zero. For 2009-2010, the earnings change distribution shifts noticeably to the left, with the incidence of earnings cuts increasing to 35 percent, which implies that job stayers have higher downside earnings risk than indicated by the distribution of hourly wage rates.[22]

Figure 5 provides further evidence about the time variation in the hours change and the earnings change distribution. Panel (a) confirms that the hours change distribution shifts left during the Great Recession as well as during the 2001 recession, with both the 25th and the 75th percentile declining while the median remains steady. As opposed to the wage change distribution, the hours change distribution bounces back relatively quickly, throughout 2009, while Washington was still experiencing net job losses. This suggests that as the economy stabilized, firms first increased hours of job stayers before increasing employment.

As Panel (b) of Figure 5 shows, the left-ward shift in the earnings change distribution during the Great Recession is more important than the left-ward shift of the hourly wage distribution.

[21] Relative to a normal distribution centered around the median and with the same standard deviation, the earnings change distribution remains considerably more concentrated. This is consistent with Guvenen et al. (2016).

[22] As mentioned above, most of the literature focuses on hourly wage changes. Hence, there is only little evidence from survey data about earnings changes. One exception is Elsby et al. (2016) who report earnings change statistics for salaried job stayers in the CPS. For the period 1998 to 2012, the fraction of zero earnings changes averages about 12 percent per year and the proportion of earnings cuts averages about 30 percent per year. Both proportions increase during the Great Recession and its aftermath. Given that we cannot distinguish between hourly paid and salaried workers, these results are not directly comparable to ours. Nevertheless, it seems safe to conclude that the proportion of zeros in our sample is substantially lower while the proportion of cuts is somewhat higher, similar to the comparisons between hourly wage change distributions above.

This shift occurs about equally for the lower end and the higher end of the distribution, with proportion of job stayers experiencing an earnings loss rising to 40 percent in early 2009. Panel (c), in turn, reports Kelley's skewness of the hours and the earnings change distribution. On average, hours changes are either symmetric or slightly negatively skewed while earnings changes are on average positively skewed. During the Great Recession, hours changes become negatively skewed while earnings changes also skew slightly negatively. We therefore find similar countercyclical skewness in earnings changes of job stayers as reported in Guvenen et al. (2016). Compared to their work, our results suggest that concurrent declines in both hourly wages and hours during the Great Recession lead to an amplified decline in earnings for many job stayers, accounting for part of the counter-cyclicality in the skewness of earnings changes. Variation in hours are therefore a potentially important contributor to earnings risk, which is an interesting dimension for the life-cycle consumption literature that is the focus of Guvenen et al. (2016) analysis.

To further explore the relationship between changes in wages, hours and earnings, we decompose the earnings change for each job stayer i and quarter $t$into the corresponding hourly wage change and hours change; i.e. $\Delta ln(e_{it}) = \Delta ln(w_{it}) + \Delta ln(h_{it})$. Figure 6 represents this decomposition by averaging hourly wage and hours changes for each 1 percent earnings change bin. The result is quite striking. For job stayers experiencing an earnings cut, on average about 75% of the earnings cut is accounted for by a decrease in hours and about 25% is accounted for by cut in the hourly wage. For job stayers experiencing an increase in earnings, by contrast, the split is roughly 50-50 on average.[23]

We confirm this result in regressions with demographic and firm controls. As Table 2 shows, the results barely change when we introduce a firm fixed effect (column 4 versus column 2 and 3), indicating that this phenomenon occurs within firms. While large hours changes are concentrated among the 1/4th of our job stayers sample that are part-time workers, many full-time employees have hours that fluctuate from year to year. These hours changes are typically much smaller than those for part-time workers. But a similar relationship emerges. As column 5 shows of Table 2 shows, reductions in earnings are to a large part accounted for by cuts in hours whereas increases in earnings are primarily accounted for by increases in the hourly wage rate. The difference in

[23]Interestingly, for job stayers experiencing a zero earnings change, the average hours change is slightly negative while the average wage change is slightly positive.

coefficients between earnings increases and earnings decreases is in fact starker than for all job stayers.[24]

Together, these results indicate that due to systematic variations in hours, earnings appear less rigid than hourly wages. While we cannot say whether this reflects choices made by the workers or the firm, variations in hours are responsible for a substantially larger part of earnings declines than variations in wages. Seen through the lens of DNWR, the result suggests that hours are more flexible downward than wage rates, consistent with the idea that the firm's wage decision also affects the intensive margin. By reducing hours in response to adverse shocks, firms may therefore be able to reduce labor costs even if they are reluctant to cut wages, thereby mitigating some of the negative effects of DNWR on employment. To our knowledge, this is a point that has not yet been made by literature.

4 A Model of Downward Wage Rigidity with Selection Effects

To better understand the implications of DNWR on the wage change distribution of job stayers, we build a dynamic model with DNWR that features selection effects from both hiring and separations. As in Elsby (2009), we introduce DNWR through an efficiency wage assumption where firms set the wage so as to elicit optimal effort by workers. For now, the model abstracts from an endogenous hours margin although we plan to investigate this possibility in the future versions.

The novelty of our analysis is that we consider not only incumbent workers as Elsby (2009) does but endogenize hiring and separations. This turns out to have potentially important selection effects. We then apply the model to analyze the effects of an unexpected negative aggregate shock on the wage change distribution of job stayers and ask to what extent the model can rationalize the aggregate evidence presented in Section 3. The model also has important implications for the specification of the firm-level regressions that we perform in Section 5 to measure the extent of DNWR and to assess the consequences of DNWR.

[24]In a set of regressions with year controls, these patterns are shown to be fairly stable over time. The share of earnings gains attributable to hours changes rises modestly (by about 0.1) in the 2009-2012 period, due to the leftward shift of the wage change distribution in those years.

4.1 Environment

There is a unit mass of atomistic workers and an infinite mass of atomistic firms. Time is discrete and discounted at rate β. The labor market is characterized by search frictions that prevent firms from replacing workers immediately and at no cost. Once matched, an employment relationship between a worker and a firm therefore enjoys a surplus that rationalizes wage dispersion and continued employment relationships even in the presence of wage rigidity.[25]

The economy enters the period with M_{-1} new and N_{-1} existing worker-firm matches, a constant fraction s of which separate. Firms then observe productivity a, which is idiosyncratic to a worker-firm match, and decide on whether the employment relationship should be continued or not. In case of continuation, the relationship generates net revenue $ae - w - f$ for the firm, with $f > 0$ a fixed cost and the worker's effort e being determined by

$$e = ln(^{w}/_{b}) + cln(^{W}/_{W_{-1}})\mathbf{1}_{\mathrm{W<W_{-1}}}, \tag{1}$$

where w is the real wage, b some exogenous reference wage level; $W \equiv wP$ the nominal wage, W_{-1} last period's nominal wage in the employment relationship (equal to zero for new hires); and $\mathbf{1}_{\mathrm{W<W_{-1}}}$ an indicator that takes the value of 1 if $W < W_{-1}$. The parameter $c \geq 0$ determines the extent to which effort reacts negatively to wage cuts and therefore the degree of DNWR. In case of separation, the firm's expected value of posting a new vacancy is 0, as implied by the free entry condition discussed below.

Defining $A \equiv aP$ and $B \equiv bP$, the firm's nominal value of an employment relationship with lagged nominal wage W_{-1} and nominal productivity A can be expressed recursively as

$$J(W_{-1}, A) = \underset{W}{max} \left\{ R(W; W_{-1}, A) + (1-s)\beta e^{-\pi} \int max(J(W, A'), 0) dG(A'|A) \right\} \tag{2}$$

where $R(W; W_{-1}, A) = A\left[ln(^{W}/_{B}) + cln(^{W}/_{W_{-1}})\mathbf{1}_{\mathrm{W<W_{-1}}}\right] - W - F$ denotes the firm's nominal net revenue; and $e^{-\pi} \equiv P'/P$ denotes gross inflation. The firm continues the relationship if $J(W_{-1}, A) \geq 0$ and severs it otherwise.

The model is closed with the definition of labor market flows and the free entry condition as in

[25] See Hall (2005), Shimer (2005), or Gertler and Trigari (2009) among many others for an elaboration of this point.

a standard search model. Employment in the current period is related to employment last period by

$$N = (1 - s)(1 - \rho)N_{-1} + hM$$

where ρ and h are the endogenous separation and hiring rates for existing and new worker-firm matches, respectively. Accordingly, unemployment in the current period is defined as

$$U = (1 - f)\, U_{-1} + (1 - (1 - s)(1 - \rho))\, N_{-1}$$

where f is the job-finding rate of unemployed workers who are all identical ex-ante. Following Pissarides (2000), the flow of new matches is governed by a matching function $M = m(U, V)$ where V denotes the mass of firms posting new vacancies in the current period. This function is homogenous of degree one, increasing in each of its arguments, concave, continuously differentiable and satisfies $m(U, V) \leq min(U, V)$. Under random search, the homogeneity implies that a vacancy matches with an unemployed worker at rate

$$q(\theta) \equiv \frac{m(U, V)}{V} = m(1, \frac{1}{\theta})$$

which is decreasing in the vacancy-unemployment ratio $\theta \equiv {}^{V}\!/_{U}$. Analogously, an unemployed worker matches with a vacancy at rate

$$\theta q(\theta) = \frac{m(U, V)}{U},$$

which is increasing in θ. Hence, the job finding rate is defined as

$$f = h\theta_{-1}q(\theta_{-1}).$$

Finally, firms post vacancies at flow cost κ. When matched, a firm-worker pair draws an idiosyncratic shock from distribution $G(A'|\overline{A})$ where $\bar{A}$ denotes average productivity this period. Under free entry, firms post vacancies until the expected value is zero; i.e.

$$\frac{\kappa}{q(\theta)} = \beta e^{-\pi} \int max(J(W, A'), 0) dG(A'|\overline{A})$$

For the purpose of the below simulations, we assume as in Elsby (2008) that idiosyncratic match productivity evolves according to a geometric random walk

$$lna' = \mu + lna - \frac{1}{2}\sigma^2 + \varepsilon', \tag{3}$$

where μ is average firm productivity growth; and $\varepsilon^{'} \sim N(0, \sigma^2)$ is the idiosyncratic productivity shock. For the model to have meaningful employment dynamics, we also assume that the exogenous reference level b grows at the same rate μ. Given that $e^{-\pi} \equiv P'/P$, nominal match-specific productivity evolves according to

$$lnA' = \mu + \pi + lnA - \frac{1}{2}\sigma^2 + \varepsilon'. \tag{4}$$

While other processes could obviously be entertained, this one has the advantage that absent DNWR and selection effects – to be discussed below – the wage change distribution would be symmetric. Moreover, the lognormal property of $A'|A$ allows for a analytical solution of the optimal wage policy under DNWR.

4.2 Optimal employment and wage setting decisions

Since the firm's problem is concave in W, continuous and increasing in A and decreasing in W_{-1} with $J(W_{-1}, 0) < 0$, there is a unique productivity threshold for existing matches $\underline{A}_N(W_{-1}) > 0$ for which $J(W_{-1}, \underline{A}_N(W_{-1})) = 0$. This threshold is increasing in W_{-1}. For productivity draws $A \geq \underline{A}_N(W_{-1})$, the employment relationship is continued; otherwise there is separation. Given a distribution of W_{-1}, this threshold determines the endogenous separation rate for existing matches ρ. Similarly, there is a unique threshold for new matches $\underline{A}_M > 0$ for which $J(0, \underline{A}_M) = 0$. For productivity draws $A \geq \underline{A}_M$, a new match leads to a hire; otherwise the worker returns to the pool of unemployed job searchers. The threshold $\underline{A}_M$ determines the endogenous hiring rate h. The difference to the separation rate for existing matches is that this hiring rate does not depend on past wages since by definition there is no wage history for new employment relationships.

To analyze the firm's optimal wage policy, we proceed as in Elsby (2009)with the added complication that the wage policy function needs to take into account the firm's employment decision.

The first-order condition of the firm's problem can therefore be expressed as

$$(1 + c1_{\mathrm{W<W_{-1}}})(A/W) - 1 + (1-s)\beta e^{-\pi} D(W, A) = 0 \ \text{ for W} \neq \mathrm{W}_{-1}, \tag{5}$$

where $D(W, A) \equiv \int\limits_{\underline{A}(W)} J_W(W, A')dF(A'|A)$ is the marginal effect of the nominal wage choice on the future discounted value of the employment relationship. This expression is the same as in Elsby (2008) except for the important difference that $D(W, A)$ is affected by the employment threshold $\underline{A}(W)$. This considerably complicates the characterization of the function $D(\cdot)$. Despite this complication, the general structure of the firm's wage setting policy is as in Elsby (2009):

Proposition 1. *Given last period's nominal wage W_{-1} and productivity A, the firm's optimal wage policy conditional on employment is*

$$W = \begin{cases} U^{-1}(A) \; if \; A > U(W_{-1}) & Raise \\ W_{-1} \quad if \; A \in [U(W_{-1}), L(W_{-1})] & Freeze \\ L^{-1}(A) \; if \; A > L(W_{-1}) & Cut \end{cases}$$

where the functions $U(\cdot)$ and $L(\cdot)$satisfy

$$(U(W)/W) - 1 + (1-s)\beta e^{-\pi} D(W, U(W)) = 0$$
$$(1+c)(L(W)/W) - 1 + (1-s)\beta e^{-\pi} D(W, L(W)) = 0$$

Of course, this wage policy depends on the employment threshold $\underline{A}_N(W_{-1})$ for existing matches; i.e. for all productivity draws $A < \underline{A}_N(W_{-1})$, the firm optimal policy is to set the wage to zero and separate. This implies the following characterization for $D(\cdot)$:

Proposition 2. *The function $D(\cdot)$ is the following form:*
For $\underline{A}_N(W) < L(W)$

$$D(W, A) = \int_{\underline{A}_N(W)}^{L(W)} \left[-c(A'/W)\right] dF + \int_{L(W)}^{U(W)} \left[(A'/W) - 1\right] dF + \beta e^{-\pi} \int_{L(W)}^{U(W)} D(W, A')dF$$

For $\underline{A}_N(W) \geq L(W)$

$$D(W, A) = \int_{\underline{A}_N(W)}^{U(W)} \left[(A'/W) - 1\right] dF + \beta e^{-\pi} \int_{\underline{A}_N(W)}^{U(W)} D(W, A')dF$$

Either case is a contraction mapping in $D(\cdot)$ *and thus has a fixed point.*

For the particular case of conditional lognormal productivity shocks assumed in (4), this allows us to derive the following employment and optimal wage policies.

Proposition 3. *Given the nominal shock process (4), the optimal employment policy for existing matches is*

$$\underline{A}_N(W_{-1}) = \underline{A}_N * W_{-1}$$

and the bounds of the optimal wage policy are

$$L(W_{-1}) = L * W_{-1} \text{ and } U(W_{-1}) = U * W_{-1}$$

where $\underline{A}_N$*,* L*, and* U are functions of the model parameters.

In other words, the optimal wage policy takes a simple piecewise linear form as in Elsby (2009) with the added condition that the productivity draw needs to be above a threshold that is proportional to the past wage.

4.3 Implications

While the model can only be solved numerically, we can nevertheless discuss several important implications.

1. Absent hiring and separations, DNWR naturally predicts an excess spike at zero and compression of both positive and negative wage changes. To see this, assume counterfactually that $A < \underline{A}_M$ for new matches and $A \geq \underline{A}_N(W_{-1})$ for all existing matches so that there is no hiring nor separations. In case of no DNWR (i.e. $c = 0$), the firm's wage policy consists of $A = W$ and the wage change distribution inherits the symmetric distribution of idiosyncratic

productivity changes. Now, impose DNWR (i.e. $c > 0$) but maintain the assumption that $A > \underline{A}(W_{-1})$ for all worker-firm pairs. As Elsby (2009) shows, in this case $U > 1 > L$, which implies that firms freeze the wage for productivity realizations $A \in [LW_{-1}, UW_{-1}]$ and adjusts wage by less than productivity for the other cases. The compression for negative wage changes follows naturally from the assumption that wage cuts entail a disproportionateley negative effort response. The compression for positive wage changes is somewhat less obvious but equally intuitive: firms are forward-looking and realize that a higher wage today will increase the possibility of wage cuts and the ensuing negative effort consequences in the future. This pushes firms towards setting wages below what is warranted by current productivity.

2. The propensity of wage freezes is increasing in the wage levels that firms "inherit" from the past. To see this, suppose a worker was subject to a negative productivity shock in the past and experienced a wage cut. Since $L < 1$, the worker's past wage was high relative to its productivity, making it less likely that the worker's current productivity level will be sufficiently high to warrant a wage increase. This echo effect, which is a direct consequence of the dynamic nature of the model, can potentially explain the delayed increase in zero mass in the wage change distribution that we observed in Section 3as the economy started emerging from the Great Recession.

3. Hiring and separations introduce selection effects that affect the wage change distribution of job stayers in potentially important ways. To illustrate the selection effect from separation, consider one more time the case of no DNWR (i.e. $c = 0$). While the *notional* distribution of wage changes (i.e. if there was no separation) in this case is symmetric, the *observed* wage change distribution of job-stayers is skewed to the right because the firm separates from workers with productivity $A < \underline{A}_N$. This selection effect carries over to the case with DNWR except that it not only reduces the mass of wage changes left of the median but also the observed zero spike. During large unexpected negative shock, DNWR-constrained firms lay off more workers; as a result, their weight in the observed wage change distribution decreases and firms administering (large) wage cuts become proportionally more important. This can potentially explain the increase in wage cuts / drop in missing mass / delayed

increase in zero spike that we observe during the Great Recession. A downturn also brings about a selection effect from hiring: new hires are systematically different from the average employee in that they have a shorter wage history that could constrain current wage setting. As economy emerges from a downturn, it therefore has a distribution of past wages that is, on average, lower. DNWR is therefore less constraining.

4. DNWR-constrained firms do not lay off more workers on average than unconstrained firms. Intuitively, firms that are more DNWR-constrained (i.e. firms with a larger parameter c) employ on average higher productivity employees than unconstrained firms. This selection effect means that DWR-constrained firms do not necessarily lay off a larger fraction of their employees. On the one hand, for a given W_{-1}, the threshold separation is higher for a firm with more DNWR-constrained workers. On the other hand, employees in DWR-constrained firms have on average higher period productivity and receive lower wages (because of the forward-looking nature of the firm) which by itself puts them further away from the layoff threshold. Hence, comparing layoff rates across firms or time as a function of some measure of DWR will not necessarily be indicative of the consequences of DWR. DNWR-constrained firms do, however, lay off more workers in response to large unexpected negative shock.

These implications have the potential to explain the observed time variation in the aggregate wage change distribution while maintaining the hypothesis that DNWR continued to at least partially constrain firms' wage setting during the Great Recession. More generally, selection effects affect the shape of the wage change distribution in non-trivial ways. Since these selection effects vary with shocks that hit the firms, asymmetry statistics of the wage change distribution are by themselves not necessarily indicative of DNWR. It is therefore important to control for these shocks (or better, the selection effects) when constructing measures of DNWR from the shape of the wage change distribution.

5 Extent and Consequences of Downward Wage Rigidity

We now exploit the worker-firm linked nature of the LEHD to assess the extent and consequences of DNWR at the firm-level. This exercise is subject to a number of challenges. First, as illustrated by the model from the preceding section, the wage change distribution of job stayers is affected not only by rigidities in the wage setting process but also by selection effects due to hires and separations, which are systematically related to average firm growth and other firm- and local labor market attributes. Second, DNWR-constrained firms should hire on average more productive workers and respond with smaller wage increases to positive productivity shocks than unconstrained firms. Hence, it is unclear whether the employment decisions of DNWR-constrained firms are on average more sensitive to shocks than the employment decisions of unconstrained firms. We address these issues by estimating measures of DNWR at the firm level for the 2004-2007 period and then assessing how these DNWR measures relate to firm employment dynamics during the Great Recession, a large unexpected negative shock to which DNWR-constrained firms are predicted to respond more negatively than unconstrained firms. To mitigate selection effects, we control for firm median wage growth during the 2004-2007 period and other firm- and worker specific variables. We also assess the robustness of our estimates to firm-fixed effects.

An additional challenge is that our analysis requires a sufficient number of job stayers to have well-defined firm-level wage change distributions. This requirement skews our firm sample towards larger firms (and smaller and mid-sized firms with relatively low turnover). To reduce this issue, we pool wage change observations of job stayers in each firm over 2004-2007 and compute the distributional statistics based on these pooled wage changes. Moreover, we only retain employers with matched revenue data from the Census Business Register as firm revenue growth is another important control variable for our regressions. As discussed in Section 2, the resulting employer sample is naturally biased towards larger firms with higher-earning workers, but the industry composition and other important attributes remain comparable to the full sample.

5.1 Descriptive regressions

We start with a set of descriptive regressions to analyze the extent to which worker and firm characteristics predict the different distributional statistics at the firm level. Table 3 reports the results of the following descriptive regression

$$z_j = \alpha + \mathbf{X}_j^{'}\beta + \epsilon_j \tag{6}$$

where z_j is a one of the distributional statistics for firm j, and $\mathbf{X}'_j$ is a matrix of firm and worker characteristics. As the first four rows show, firm size is an important and highly significant predictor for all distributional statistics: the smaller the firm size, the larger the mass at zero, the larger the asymmetry (as measured by excess zero spike and missing mass left of zero), and the more concentrated the distribution of wage changes. This result is interesting and to our knowledge new. It suggests that smaller firms are on average more DNWR-constrained than large firms, which is consistent with the predictions of our model. Since our sample for the below regressions skews towards larger firms, the result also implies that the estimates we obtain should be considered a conservative indicators of the consequences of DNWR.

The fifth row shows that median wage growth has a negative effect on the mass of zero, the excess zero spike, and the 25-75 distance, and a positive effect on missing mass. These estimates are consistent with the predictions of the model and indicate that firm growth (proxied here by median wage growth) exerts important effects on the wage change distribution. At the same time, the R^2 in all of these regressions remains low despite the fact that we control for industries and a number of other worker wage and demographic variables. So, there remains substantial variability in distributional statistics across firms even after controlling for the different variables.

5.2 Regression specifications and data

To assess the consequences of DNWR on employment, we estimate a set of equations relating changes in firm-specific employment growth to different measures of DNWR and other controls. We start with a simple specification that links net employment growth of firm j between year $t-1$ and t, Δy_{jt} , to firm-specific measures of DNWR:[26]

$$\triangle y_{jt} = \beta_1 DNWR_j + \beta_2 DNWR_j \mathbf{1}_{\text{GR}} + \delta + \mathbf{X}'_{jt}\delta_X + \varepsilon_{jt}, \tag{7}$$

where $\mathbf{1}_{\text{GR}}$ is a Great Recession indicator taking the value of 1 for the years 2008 and 2009, and $DNWR_j$ is measured as either the excess zero spike, the mass at zero, the missing mass left of zero, or the 25-75 dispersion of firm j's wage change distribution over the 2004-2007 period, i.e.

[26] All growth rates are defined in percent relative to the average over t and $t-1$.

prior to the Great Recession. For firms with negative excess zero spike and negative missing mass left of zero, we set these measures equal to zero. For now, the different DNWR measures do not directly control for selection effects that may have occurred during the 2004-2007 period. Instead, we include firm-specific median wage growth during this time period directly in the control vector $\mathbf{X}_{jt}$, which in addition contains firm-specific characteristics as well as a set of year fixed effects.[27] Under the assumption that the different measures capture the extent to which firms are constrained by DNWR and the Great Recession is the result of negative shocks that were unexpected by the firm, the coefficient β_2 can be given a causal interpretation.

In a second specification, we augment (7) with revenue growth as follows:

$$\triangle y_{jt} = \beta_1 DNWR_j + \beta_2 DNWR_j \mathbf{1}_{\mathrm{GR}} + \gamma_1 \triangle rev^+_{jt} + \gamma_2 \left|\triangle rev^-_{jt}\right| + \delta + \mathbf{X}'_{jt}\delta_X + \varepsilon_{jt}, \tag{8}$$

where $\triangle rev^+_{jt}$ denotes positive firm revenue growth of firm j between year $t-1$ and t; and $\left|\triangle rev^-_{jt}\right|$ the absolute value of negative revenue growth.[28] The two terms allow for a differential relationship of employment growth with positive and negative revenue changes. We do not attribute a causal interpretation to γ_1 and γ_2 since variations in employment and revenues are generally driven by both firm-internal shocks (e.g. productivity shocks, changes in regulation or taxation) and exogenous demand shocks. Instead, we introduce revenue growth to control for the possibility that the distributional statistics used to measure DNWR pick up variations in revenue (e.g. mitigating concerns that evidence of DNWR in 2004-2006 may be correlated with the size of the shock to the firm in 2008-2009).

The third regression specification adds interactions terms between negative revenue growth, the Great Recession indicator, and the DNWR measures:

[27] Unless otherwise noted, the firm-specific characteristics are firm size category in t (as defined in Table 3) as well as industry sector, median wage change, average share of salaried workers, average share of part-time workers, and average share of female workers over the 2004-2006 period.

[28] In addition, the vector of controls $\mathbf{X}_{jt}$ includes quadratic revenue terms. Revenue change is measured at the national level for the firm, while all other indictors are measured at the state-employer level.

$$\begin{aligned}\triangle y_{jt} = {} & \beta_1 DNWR_j + \beta_2 DNWR_j \mathbf{1}_{\text{GR}} + \beta_3 DNWR_j \left|\triangle rev^-_{jt}\right| + \beta_4 DNWR_j \left|\triangle rev^-_{jt}\right| \mathbf{1}_{\text{GR}} \\ & + \gamma_1 \triangle rev^+_{jt} + \gamma_2 \left|\triangle rev^-_{jt}\right| + \gamma_3 \left|\triangle rev^-_{jt}\right| \mathbf{1}_{\text{GR}} \\ & + \delta + \mathbf{X}'_{jt}\delta_X + \varepsilon_{jt}.\end{aligned} \tag{9}$$

The main purpose of this specification is to assess the extent to which the employment effects of DNWR differ with the decline in revenue, both during and outside of the Great Recession period.

We estimate specification (9) both without and with a firm-fixed effect. In the latter case, all time-invariant variables drop out. Aside from annual employment growth as the left-hand side variable, we also consider the job creation rate, the job destruction rate, the gross hiring rate, and the gross separation rate. As is common in the literature, job creation and destruction rates are defined as $max(\triangle y_{jt}, 0)$, respectively $max(-\triangle y_{jt}, 0))$. The gross hiring and separation rates are available on a quarterly basis as the share of new employees relative to total employment at the firm at the end of the quarter, respectively the share of separated employees relative to total employment at the firm in the beginning of the quarter. We average these quarterly hire and separation rates over the course of the year.[29]

5.3 Results

Table 4 reports results for the regressions of employment growth on the excess zero spike as the measure of DNWR. All regressions are weighted by employment, although results are robust to working with unweighted observations. Standard errors are clustered at the firm level and reported in parenthesis below the point estimates. As the estimates for specification (7) in the first column show, the relationship between employment growth and zero excess spike is overall insignificant, consistent with the prediction of the model that DNWR-constrained firms do *on average* not

[29]By definition, the net employment growth rate equals the job creation rate minus the job destruction rate. Because the denominators used to compute hiring and separation rates are different from the denominator used to compute net employment growth (average employment between year $t-1$ and t) and because we consider quarterly hiring and separation rates averaged over one year, net employment growth does not equal the hiring rate minus the separation rate. It should also be noted that separations include both layoffs and quits (in particular job-to-job transitions). As Haltiwanger et al. (2015) show, it is possible under certain assumptions to distinguish between these two types of separations in the LEHD. We plan to consider these two types of separations in future versions.

exhibit different employment growth than unconstrained firms. By contrast, the coefficient on the excess zero spike interacted with the Great Recession indicator is negative and highly significant, which again is consistent with the prediction of the model that employment of DNWR-constrained firms is more negatively affected by unexpected large negative shocks. The point estimate of -0.314 implies that a firm with an excess zero spike of 4 percent (the average over the 2004-07 period), had about 1.2 percent lower annual employment growth during the Great Recession than a typical unconstrained firm. Given that total non-farm employment in Washington declined by 6.1 percent during the Great Recession, this effect is sizable.

The second and third column of Table 4 report the results for specifications (8) and (9). The estimates indicate that the employment growth effect of the excess zero spike during the Great Recession is robust to the inclusion of revenue growth and the different interaction terms. The relationship between employment growth and positive and negative revenue growth is highly significant and of the expected sign. During the Great Recession, declines in revenue are associated with larger negative employment growth, which is consistent with the idea that the Great Recession was also a period of negative news and increased uncertainty about future business conditions. Interestingly, the estimates show that outside of the Great Recession period, DNWR-constrained firms with negative revenue growth exhibit only slightly smaller employment growth than unconstrained firms with negative revenue growth. This further confirms the notion discussed above that DNWR-constrained firm are, on average, not more sensitive to shocks than unconstrained firms. Finally, the very small and insignificant estimate on the three-way interaction term suggests that the negative impact of the excess zero spike on employment growth during the Great Recession arises independently of the revenue decline that the firm experiences.

The fourth and fifth column of Table 4 provide robustness checks for specification (9) by excluding the years 2004-07 (i.e. the years over which the excess zero spike is computed) and by imposing a firm fixed effect. The coefficient estimates remain essentially unchanged. This is encouraging – especially for the regression with fixed effects – and indicates that the excess zero spike does not pick up unobserved firm attributes that are correlated with negative employment growth during the Great Recession. For all regressions that follow, we impose a firm fixed effect as we consider this specification as the most convincing.

Table 5 reports estimation results for the relationship between net employment growth and the other measures of DNWR. For comparison, the first column displays the results for the regression with excess zero spike in column 5 of Table 4. Zero mass and missing mass left of zero are both associated with large and highly significant negative employment growth effects during the Great Recession whereas the interaction between these indicators and negative revenue changes remains very small. The 25-75 dispersion indicator also has a large and highly significant employment effect during the Great Recession although this effect is positive. This is consistent with the prediction of the model that DNWR-constrained firms have more concentrated wage change distribution than unconstrained firms. The estimates in this table therefore all afford the same conclusion: firms with indicators of DNWR had systematically more negative employment growth during the Great Recession but on average do not exhibit different employment growth dynamics than unconstrained firms.

Table 6 returns to the excess zero spike as the measure of DNWR and estimates the effect on finer firm-specific measures of employment dynamics. For reference, the first column shows the results for the net employment growth regression in column 5 of Table 4. As the second and third column show, firms with an excess zero spike during the pre-recession period had significantly higher job destruction rates and significantly lower job creation rates during the Great Recession. Since by definition, the job creation rate minus the job destruction rate sums to the net employment growth rate, these estimates illustrate nicely that the DNWR effect on net employment growth manifests itself both through higher job destruction and lower job creation. This is consistent with the predictions of the model and suggests that DNWR contributed to the large decline in job creation, which is one of the most distinctive features in terms of labor market flows of the Great Recession (see for example Elsby, Hobjin and Sahin (2010)).

To further investigate this point, the fourth and fifth column of Table 6 report the regression estimates for gross hiring and separation rates. Consistent with the above point, firms with an excess zero spike have significantly lower hiring rates during the Great Recession.[30] In turn, the excess zero spike is associated with a higher separation rate during the Great Recession although this effect is smaller and only marginally significant. This could be due to the fact separations include both layoffs and quits, which are affected in opposite directions by DNWR; i.e. while

[30] As explained above, the magnitude of the coefficient estates is substantially lower since hiring rates are computed on a quarterly basis whereas employment growth is computed on an annual basis.

DNWR-constrained firms are predicted to lay off more workers in response to a large unexpected negative shock, constraints on the extent to which wages can be cut provide a disincentive for job stayers to engage in on-the-job search, resulting in a decline in voluntary quits.

6 Conclusion

In this paper we make several new contributions to the literature on DNWR. First, we use linked employer-employee administrative data for a large, nationally representative U.S. state to analyze the characteristics of the wage change distribution of job stayers and how this distribution changed during the Great Recession and its aftermath. Generally we find evidence consistent with some degree of DNWR in U.S. private sector firms, although wage cuts are far from a rare occurence and the incidence of wage freezes is low relative to studies based on U.S. survey data. We believe these differences are driven by two factors: (i) less measurement error in the administrative wage data; and (ii) a more complete earnings concept that includes both bonuses and overtime pay. We also find evidence that the incidence of wage cuts has increased and the wage change distribution has become more concentrated during the Great Recession, followed by a marked increase in the incidence of wage freezes as the economy started to recover. This last finding is consistent with what other recent papers in the literature report, although the increase in the incidence of wage freezes is markedly higher according to our analysis.

Our second contribution is to examine role of hours in mitigating the effects of DNWR for firms and potentially exposing workers to more downside earnings risk than implied by rigidity in wages. Here we find evidence that hours change distributions are much more symmetric than wage changes, with corresponding larger shares of workers experiencing earnings declines. In future work, we plan to explore this intensive margin in more detail.

Our third contribution is to exploit the employer-employee link of our data to estimate the relationship between indicators of DNWR at the firm-level and their employment response during the Great Recession. While concerns that DNWR leads to higher unemployment in recessions is the principal reason why DNWR is of economic interest, there is little direct evidence of the effect of DNWR on employment growth. While our results are largely descriptive, we believe they are suggestive that while DNWR constrained firms on average do not differ from non-DNWR constrained firms with regards to employment growth, they did have higher rates of net job destruction and separations and lower rates of net job creation and hiring during the Great Recession. In future work, we plan to expand this analysis to include how indicators of DNWR relate to wage and hours dynamics at the firm level.

Our final contribution is to formalize the implications of DNWR through a dynamic model. The model builds on Elsby (2009) but extends it in important ways to include hiring and separations, and investigates how this affects the wage change distribution of job stayers in response to an unexpected negative shock. At least qualitatively, the model can rationalize the empirical patterns observed in the aggregate data. Furthermore, the model highlights the importance of selection effects in interpreting DNWR indicators, which informs our empirical regression analysis.

References

Abbritti, M. and S. Fahr (2013). Downward wage rigiditiies and business cycle asymmetries. *Journal of Monetary Economics 60*, 871–886. 1

Akerlof, G. A., W. T. Dickens, and G. L. Perry (1996). The macroeconomics of low inflation. *Brookings Papers on Economic Activity 1*, 1–59. 3.1

Altonji, J. G. and P. J. Devereux (2000). The extent and consequences of downward nominal wage rigidity. *Worker Well-Being 19*(383-431). 2, 3.1, 18

Auclert, A. and M. Rognlie (2016). Inequality and aggregate demand. 1

Babecky, J., P. D. Caju, T. Kosma, J. Messina, and T. Room (2012). How do european firms adjust their labour costs when nominal wages are rigid? *Labour Economics 19*, 792–801. 18

Barratieri, A., S. Basu, and P. Gottschalk (2014). Some evidence on the importance of sticky wages. *American Economic Journal: Macroeconomics 6*, 70–101. 2, 3.1

Benigno, P. and L. A. Ricci (2011). The inflation-output trade-off with downward wage rigidities. *American Economic Review 101*, 1436–1466. 1

Card, D. and D. Hyslop (1997). Does inflation'grease the wheels of the labor market? In C. D. Romer and D. H. Romer (Eds.), *Reducing Inflation: Motivation and Strategy*. University of Chicago Press. 2

Daly, M. and B. Hobijn (2014). Downward nominal wage rigidities bend the phillips curve. *Journal of Money, Credit, and Banking 46*(2), 51–93. 1, 1, 2, 1, 3.1, 15, 16, 17

Daly, M. C., B. Hobijn, and B. Lucking (2012). Why has wage growth stayed strong? *Federal Reserve Bank of San Francisco Economic Letter 10*. 1, 2

Dickens, W. T., L. Goette, E. L. Groshen, S. Holden, J. Messina, M. E. Schweitzer, J. Turunen, and M. Ward (2007). How wages change: Micro evidence from the international wage flexbility project. *Journal of Economic Perspectives 21*(2), 195–214. 2, 16

Dupraz, S., E. Nakamura, and J. Steinsson (2016). A plucking model of business cycles. 1

Eggertsson, G. B. and N. R. Mehrotra (2015). A model of secular stagnation. 1

Eliaz, K. and R. Spiegler (2013). Reference dependence and labor market fluctuations. *NBER Macreoconomics Annual 28*(1), 159–200. 1

Elsby, M. W. L. (2009). Evaluating the economic significance of downward nominal wage rigidity. *Journal of Monetary Economics 56*, 154–169. 2, 1, 4, 4.2, 4.2, 4.2, 1, 6

Elsby, M. W. L., B. Hobijn, and A. Sahin (2010). The labor market in the great recession. *Brookings Papers on Economic Activity 1*, 1–48. 1

Elsby, M. W. L., D. Shin, and G. Solon (2016). Wage adjustment in the great recession and other downturns: Evidence from the united states and great britain. *Journal of Labor Economics 34*(1), S249–S291. 1, 2, 1, 15, 17, 3.1, 19, 22

Fallick, B., M. Lettau, and W. Wascher (2016). Downward nominal wage rigidity in the united states during and after the great recession. Working paper 16-02, Federal Reserve Bank of Cleveland. 1, 3, 1

Gottschalk, P. (2005). Downward nominal wage flexibility: Real or measurement error? *Review of Economics and Statistics 87*(3), 556–568. 2, 16, 3.1

Guvenen, F., F. Karahan, S. Ozkan, and J. Song (2016). What do data on millions of u.s. workers reveal about life-cylce earnings dynamics? 3.2, 21

Haltiwanger, J., H. Hyatt, and E. McEntarfer (2015). Cyclical reallocation of workers across employers by firm size and wage. Working paper 21235, NBER. 29

Kahn, S. (1997). Evidence of nominal wage stickiness from microdata. *American Economic Review 87*(5), 993–1008. 2

Keynes, J. M. (1936). *The General Theory of Employment, Interest, and Money.* 1

Kim, J. and F. Ruge-Murcia (2009). How much inflation is necessary to grease the wheels? *Journal of Monetary Economics 56*, 365–377. 1

Lebow, D. E., R. E. Saks, and B. A. Wilson (2003). Downward nominal wage rigidity: Evidence from the employment cost index. *Advances in Macroeconomics 3*(1). 3

McLaughlin, K. (1994). Rigid wages? *Journal of Monetary Economics 34*, 383–414. 2

Nickell, S. and G. Quintini (2003). Nominal wage rigidity and the rate of inflation. *Economic Journal 113*(490), 762–781. 3.1

Schmitt-Grohe, S. and M. Uribe (2013). Downward nominal wage rigidity and the case for temporary inflation in the eurozone. *Journal of Economic Perspectives 27*(3), 193–212. 1

Schmitt-Grohe, S. and M. Uribe (2016). Downward nominal wage rigidity, currency pegs, and involuntary unemployment. *Journal of Political Economy 124*(5), 1466–1514. 1

Schmitt-Grohe, S. and M. Uribe (2017). Liquidity traps and jobless recoveries. *American Economic Journal: Macroeconomics 9*(1), 165–204. 1

Smith, J. C. (2000). Nominal wage rigidity in the united kingdom. *Economic Journal 110*, C176–C195. 3.1

Tobin, J. (1972). Inflation and unemployment. *American Economic Review 61*(1/2), 1–18. 1

Tables and Figures

Table 1: Sample characteristics. Washington State sample, 2004-2014

	Washington, private sector	SEIN matches to revenue data	Firm sample for regressions SEIN has 50 job stayers and matches to revenue data
		Job stayer characteristics	
Person year observations	11.8 million		6.0 million
Average age	44.1		44
Share female	46%		45%
Average hourly wage	$34.81		$36.99
Average weekly hours paid	35.6		36.9
Average annual earnings	$59,313		$67,269
Median change hourly wage	3.5%		3.8%
		Firm characteristics	
N SEIN year observations	1,168,752	751,349	53,395
- Share of SEINs	100%	64%	5%
- Share of employment	100%	73%	46%
Employment share by SEIN size			
-Less than 50 employees	33%	32%	6%
-50-249 employees	23%	23%	28%
-250-499 employees	9%	9%	12%
-500-999 employees	8%	8%	11%
-1000+ employees	26%	29%	42%
Employment share by industry			
-Natural Resources & Mining	0.1%	0.1%	0.1%
-Trade, Transportation & Utilities	25%	26%	26%
-Construction	7%	7%	4%
-Manufacturing	13%	14%	17%
-Information	5%	5%	7%
-Finance Activities	7%	6%	6%
-Professional & Business Services	14%	14%	11%
-Educational & Health Services	17%	18%	20%
-Leisure & Hospitality	12%	11%	8%

Notes: Establishment counts are rounded. Private sector establishment universe excludes sectors 81 and 11 (not covered in revenue data).

Table 2: Hours change regressions. Washington State, 1998-2013

Dependent variable: annual log change in hours of job stayers

	1	2	3	4	5
Change ln(Annual Earnings)	0.550**				
	(0.0004)				
Change ln(Annual Earnings) < 0		0.721**	0.722**	0.728**	
		(0.0008)	*(0.0009)*	*(0.0009)*	
Change ln(Annual Earnings) >= 0		0.442**	0.445**	0.451**	
		(0.0006)	*(0.0006)*	*(0.0006)*	
Change ln(Annual Earnings) < 0					0.644**
- weekly hours >=35 in initial year only					*(0.0007)*
Change ln(Annual Earnings) >= 0					0.277**
- weekly hours >=35 in initial year only					*(0.0006)*
R-squared	0.163	0.167	0.17	0.215	0.176
Demographic Controls	No	No	Yes	No	Yes
Firm Controls	No	No	Yes	No	Yes
Firm Fixed Effects	No	No	No	Yes	No

Table 3: Characteristics of distributional statistics. Washington State firm sample, 2004-2007

	Dependent variable			
	Zero mass	Excess zero spike	Missing mass left of zero	25-75 dispersion
Employer size < 50	3.716**	4.250**	1.957**	-6.775**
	(0.189)	*(0.237)*	*(0.331)*	*(1.347)*
Employer size >= 50 and < 250	2.001**	2.443**	1.358**	-6.531**
	(0.123)	*(0.154)*	*(0.215)*	*(0.878)*
Employer size >= 250 and < 500	0.85**	1.037**	0.699**	-3.392**
	(0.164)	*(0.205)*	*(0.286)*	*(1.164)*
Employer size >=500 and < 1000	0.679**	1.000**	1.101**	-1.340**
	(0.162)	*(0.202)*	*(0.282)*	*(1.15)*
Median wage change	-0.06**	-0.154**	0.078**	-0.438**
	(0.009)	*(0.011)*	*(0.015)*	*(0.062)*
Share salaried workforce	0.01**	0.009**	-0.019*	0.209**
	(0.003)	*(0.003)*	*(0.005)*	*(0.019)*
Share part-time workforce	0.009**	0.015**	-0.042**	0.186**
	(0.003)	*(0.004)*	*(0.005)*	*(0.02)*
R-squared	0.148	0.164	0.097	0.257
Firm size, industry, and worker controls	Yes	Yes	Yes	Yes
Weighted by employment	Yes	Yes	Yes	Yes

Table 4: Relationship between net employment growth and excess zero spike

Washington State firm sample, 2004-2013

	Dependent variable: net employment growth				
			Includes interaction effects	Excludes years 2004-2007	Includes firm fixed effects
	1	2	3	4	5
Spike0 (excess zero mass 2004-2007)	0.083	-0.023	0.126	0.195	
	(0.109)	*(0.134)*	*(0.099)*	*(0.155)*	
Spike0 * GR	**-0.314****	**-0.254****	**-0.259****	**-0.299****	**-0.314****
	(0.094)	*(0.078)*	*(0.085)*	*(0.115)*	(0.050)
Spike0 * Change in revenue (<0)			**-0.011****	**-0.014****	**-0.010****
			(0.003)	*(0.005)*	(0.002)
Spike0 * Change in revenue (<0) * GR			0.011*	0.013*	0.011**
			(0.005)	*(0.006)*	(0.003)
Change in revenue (>0)		0.284**	0.286**	0.277**	0.249**
		(0.022)	*(0.021)*	*(0.034)*	(0.010)
Change in revenue (<0)		-0.321**	-0.250**	-0.260**	-0.206**
		(0.037)	*(0.041)*	*(0.06)*	(0.012)
Change in revenue (<0) * GR			-0.170**	-0.168**	-0.175**
			(0.034)	*(0.046)*	(0.013)
R-squared					0.23
Includes:					
Quadratic terms for revenue change		X	X	X	X
Firm size, industry, and worker characteristics	X	X	X	X	
Year controls	X	X	X	X	X
Firm fixed effects					X

Notes: * significant at 0.05, ** significant at 0.01

Table 5: Relationship between net employment growth and other DNWR measures

Washington State firm sample, 2004-2013

Dependent variable: net employment growth

	DNWR measure			
	Spike0	ZeroMass	MMleft	25-75 dispersion
DNWR * GR	**-0.314****	**-0.168****	**-0.276****	**2.772****
	(0.050)	*(0.040)*	*(0.028)*	*(0.771)*
DNWR * Change in revenue (<0)	**-0.010****	**-0.005***	**-0.004***	**0.145****
	(0.002)	*(0.001)*	*(0.001)*	*(0.024)*
DNWR * Change in revenue (<0) * GR	**0.011****	**0.007****	**0.006****	**-0.257****
	(0.003)	*(0.002)*	*(0.002)*	*(0.052)*
Change in revenue (>0)	0.287**	0.240**	0.249**	0.248**
	(0.020)	*(0.010)*	*(0.010)*	*(0.010)*
Change in revenue (<0)	-0.310**	-0.200**	-0.196**	-0.263**
	(0.039)	*(0.013)*	*(0.013)*	*(0.013)*
Change in revenue (<0) * GR	-0.175**	-0.187**	-0.197**	-0.108**
	(0.013)	*(0.015)*	*(0.017)*	*(0.014)*
R-squared				
Includes:				
Quadratic terms for revenue change	X	X	X	X
Firm and year fixed effects	X	X	X	X

Notes: * significant at 0.05, ** significant at 0.01, † significant at 0.10

Table 6: Relationship between employment flows and excess zero spike

Washington State firm sample, 2004-2013

	Dependent variable				
DNWR measure=spike0	Net Employment Growth Rate	Job Destruction Rate	Job Creation Rate	Hiring rate	Separation rate
DNWR * GR	**-0.314****	**0.190****	**-0.123****	**-0.024****	**0.016†**
	(0.050)	*(0.037)*	*(0.026)*	*(0.008)*	*(0.008)*
DNWR * Change in revenue (<0)	**-0.010****	**0.008****	**-0.002****	**-0.002****	**0.000**
	(0.002)	*(0.001)*	*(0.001)*	*(0.000)*	*(0.000)*
DNWR * Change in revenue (<0) * GR	**0.011****	**-0.008****	**0.002†**	**0.001****	**0.000**
	(0.003)	*(0.002)*	*(0.001)*	*(0.000)*	*(0.000)*
Change in revenue (>0)	0.287**	-0.074**	0.175**	0.046**	-0.005**
	(0.020)	*(0.007)*	*(0.005)*	*(0.002)*	*(0.002)*
Change in revenue (<0)	-0.310**	0.187**	-0.020**	-0.013**	0.030**
	(0.039)	*(0.009)*	*(0.006)*	*(0.002)*	*(0.002)*
Change in revenue (<0) * GR	-0.175**	0.149**	-0.026**	-0.030**	0.005*
	(0.013)	*(0.010)*	*(0.007)*	*(0.002)*	*(0.002)*
R-squared					
Includes:					
Quadratic terms for revenue change	X	X	X	X	X
Firm and year fixed effects	X	X	X	X	X

Notes: * significant at 0.05, ** significant at 0.01, † significant at 0.10

Figure 1: Distribution of hourly wage changes of job stayers. Washington State, 1998:2-2013:2

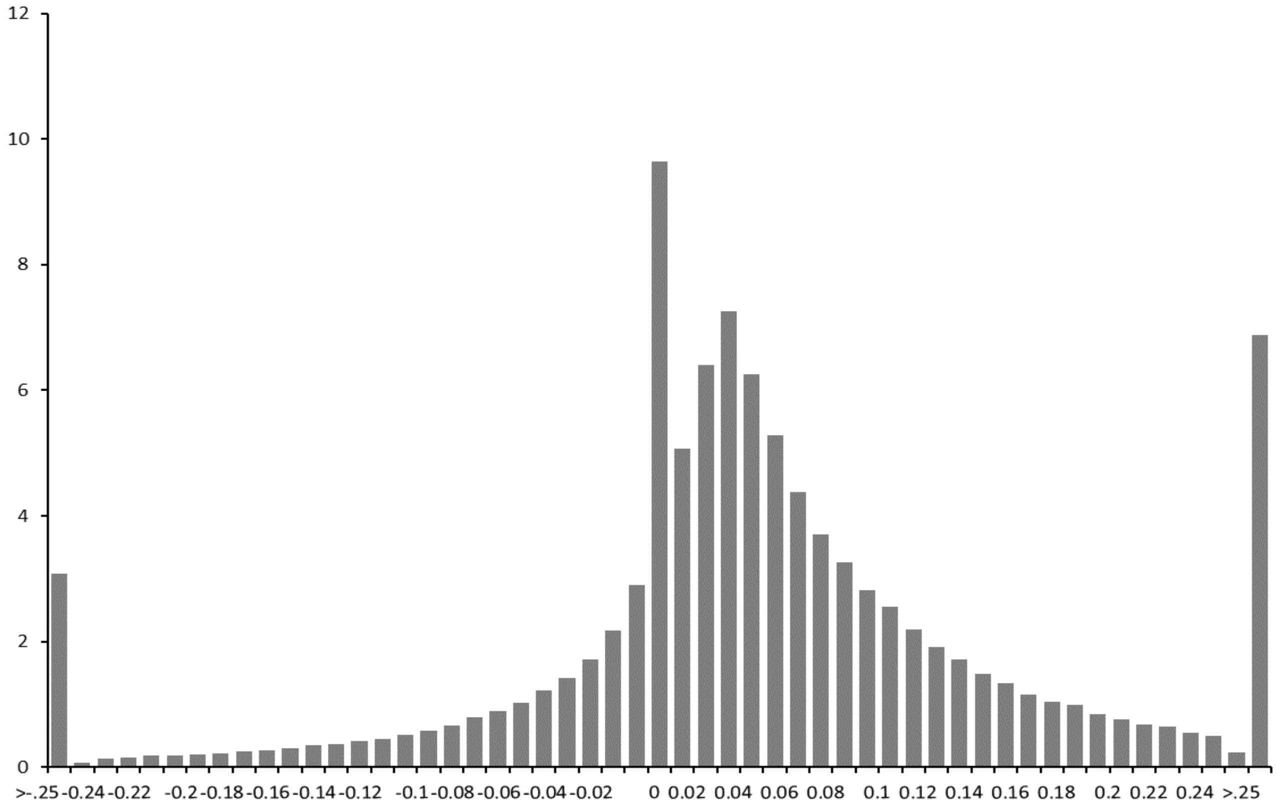


Figure 2: Distribution of hourly wage changes of job stayers. Washington State, 2005-06 vs 2009-10

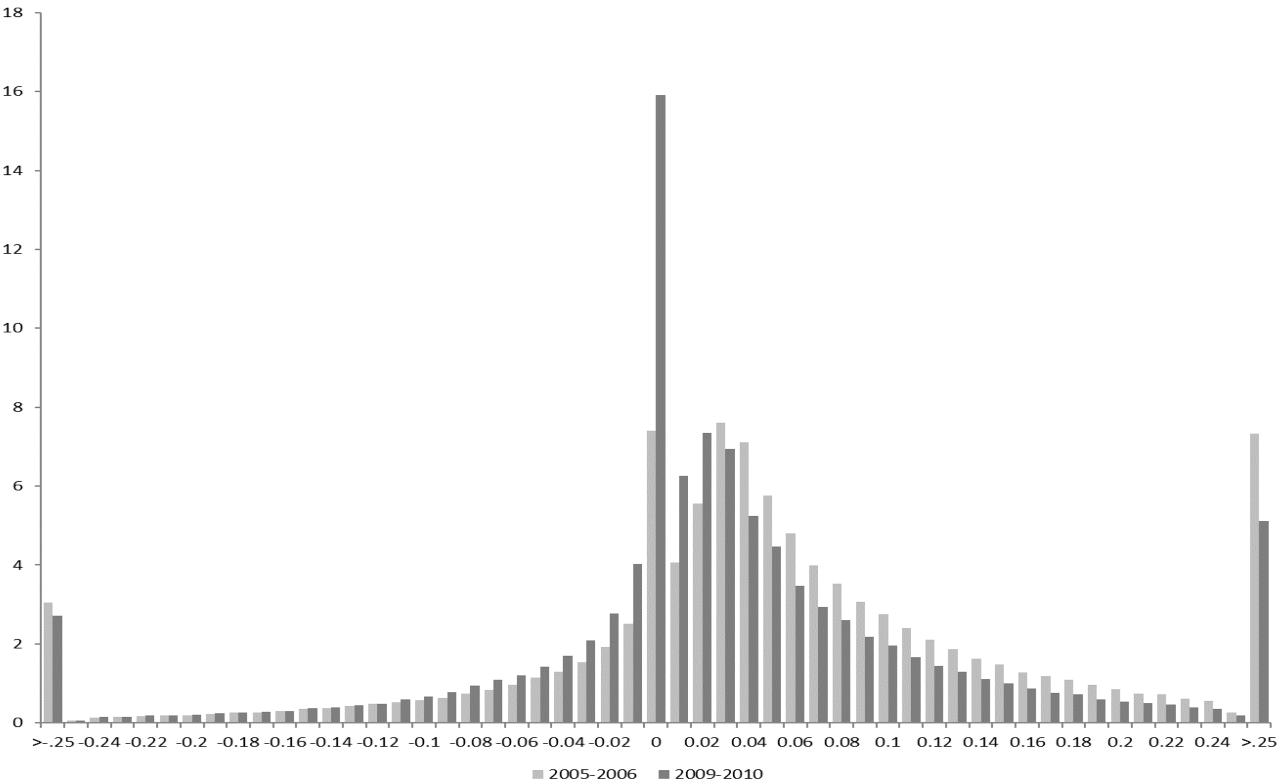


Figure 3: Variation in hourly wage change distribution over time

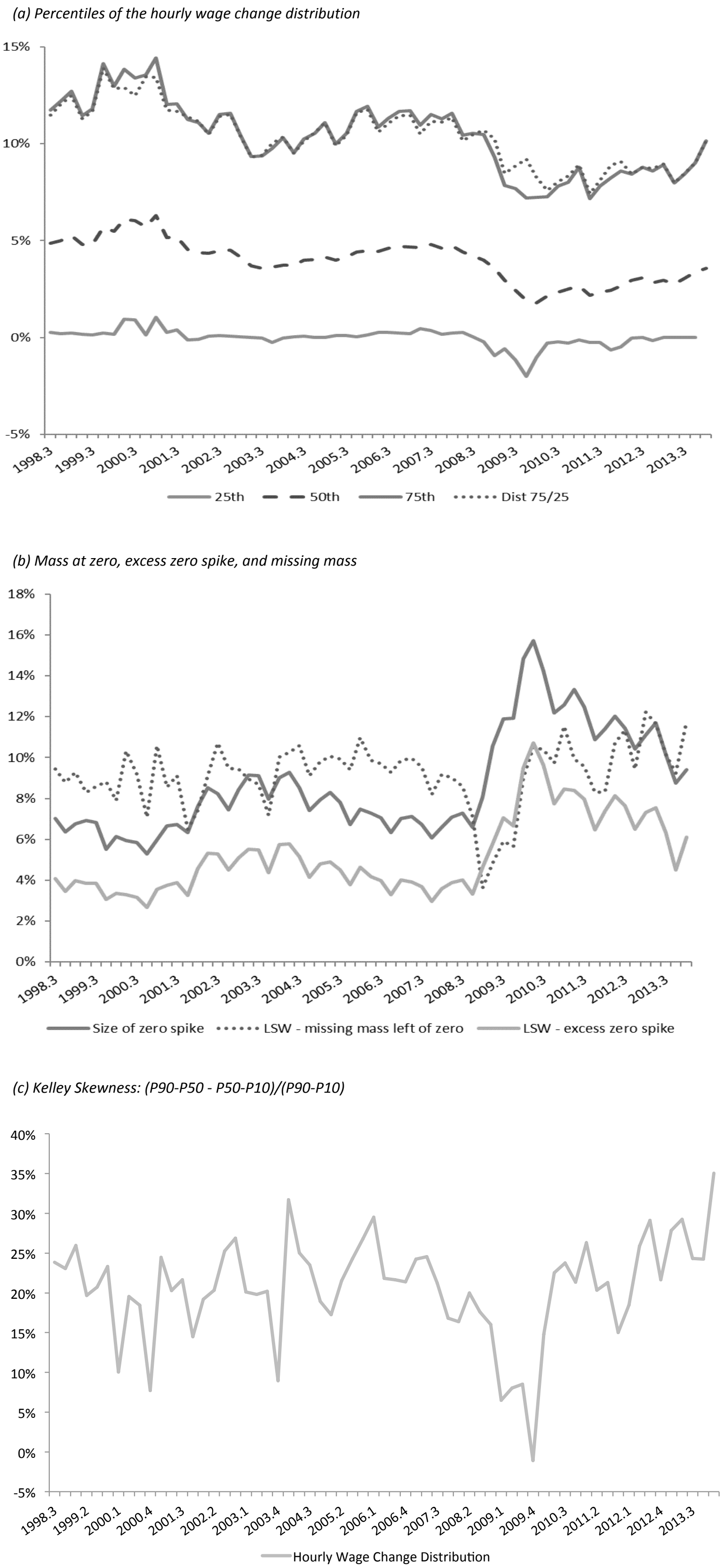


Figure 4: Distribution of hours and earnings changes of job stayers

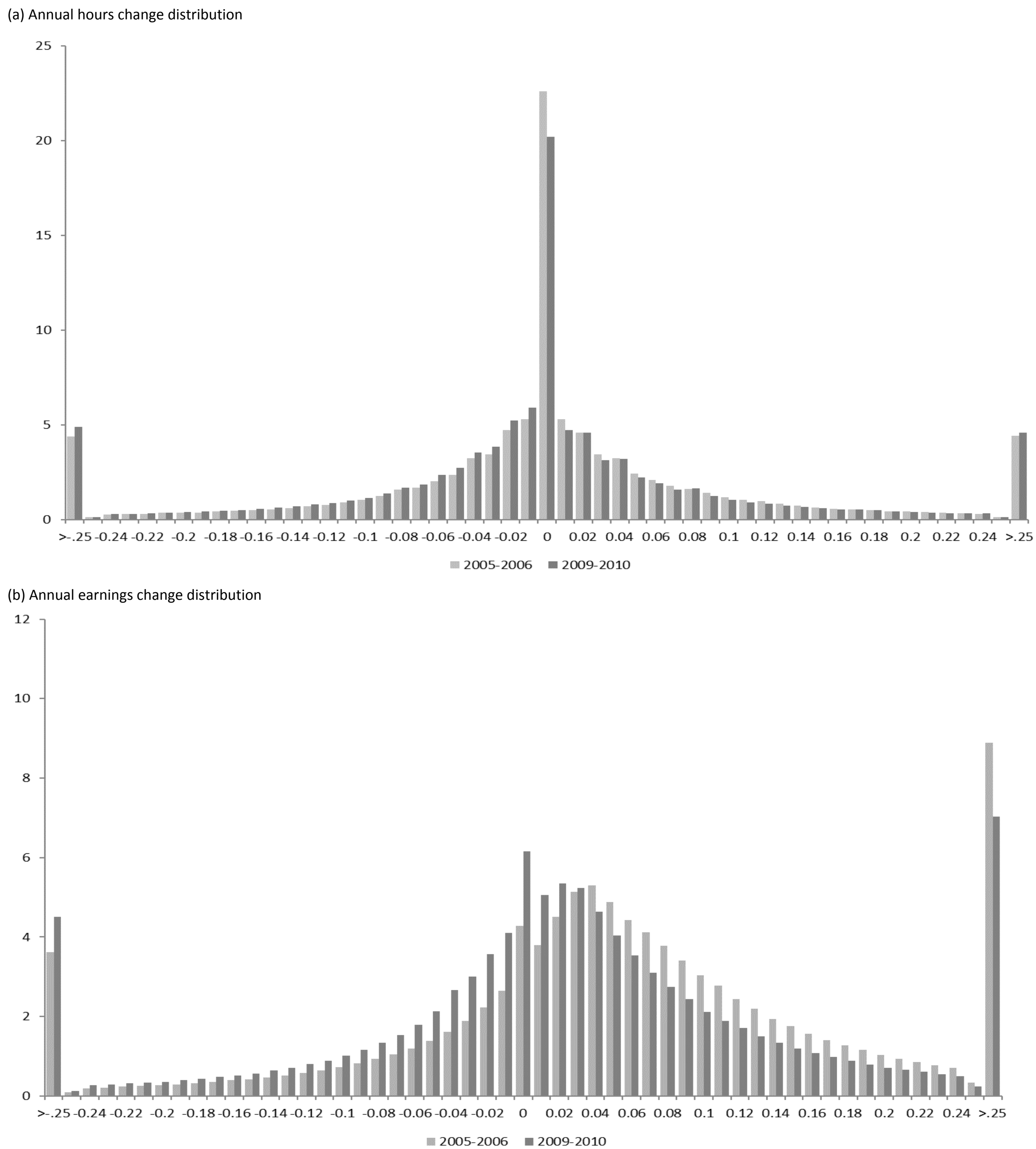
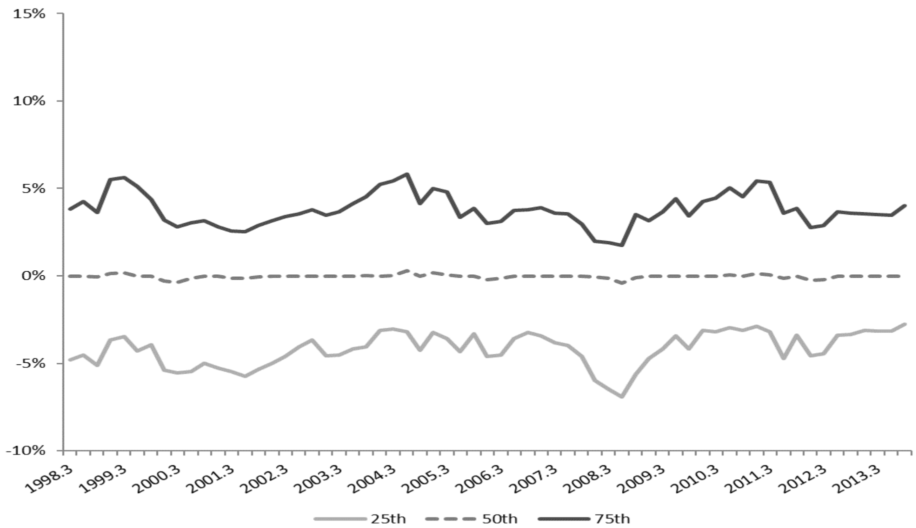


Figure 5: Variation in hours and earnings change distribution over time

(a) Percentiles of annual hours distribution



(b) Percentiles of annual earnings change distribution

(c) Kelley skewness of annual hours change and earnings change distirbutions

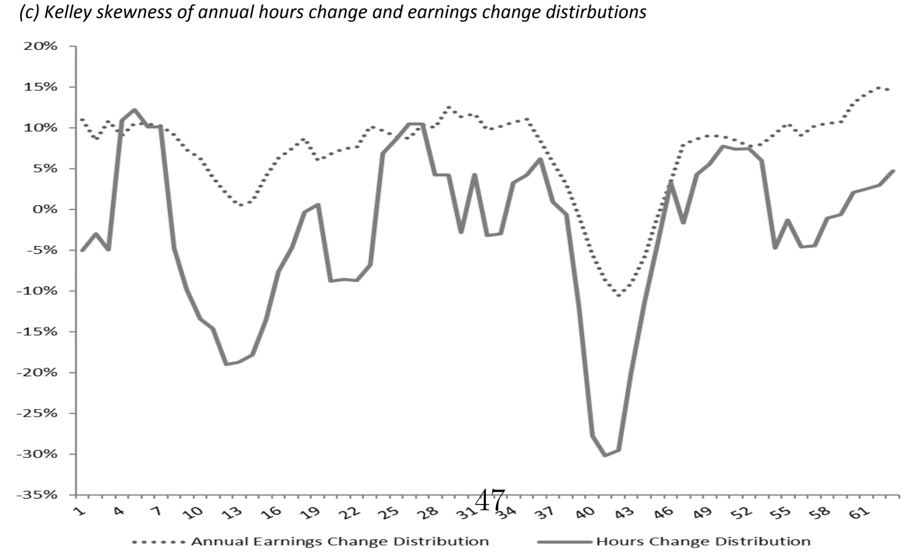


Figure 6: Earnings change decomposition. Washington State, 1998-2013

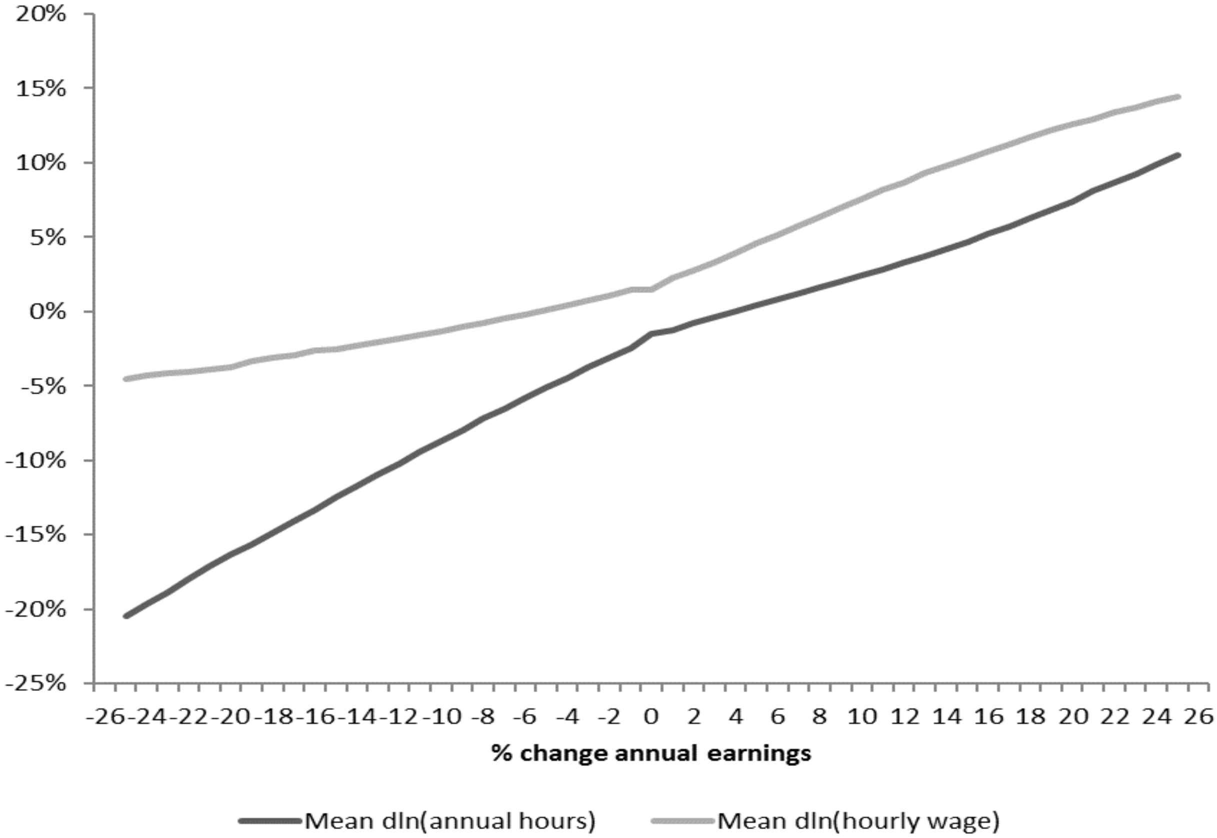
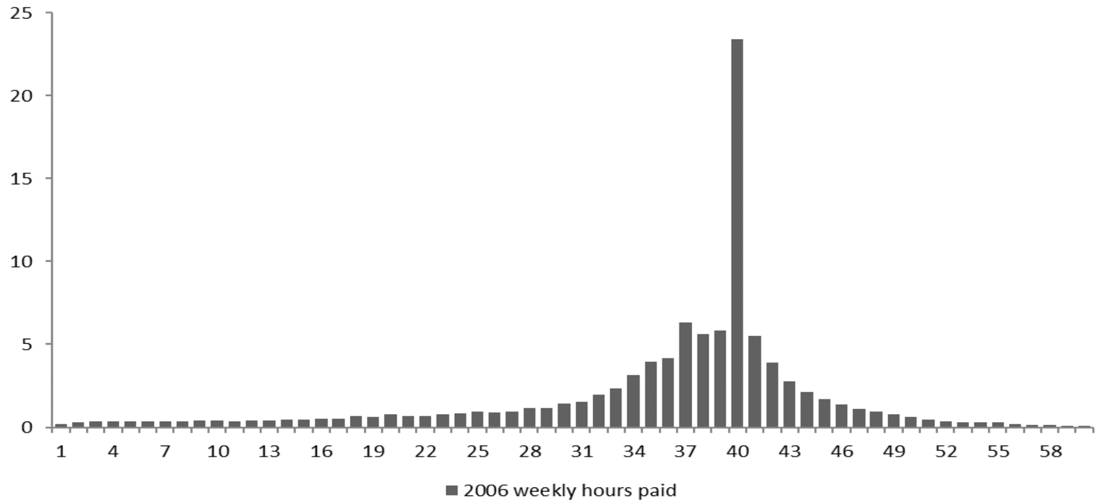


Figure A.1: Distribution of hours worked and change in hours, Washington UI data, selected years.

(a) Distribution of weekly hours paid in 2006, two-year job stayers



(b) Distribution of change in weekly hours paid in 2009, by hours paid in 2008, two-year job stayers