# Downward Wage Ridigidity in the United States: New Evidence from Worker-Firm Linked Data[†]

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## Abstract

Using administrative worker-firm linked data for the United States, we examine the incidence of nominal wage and earnings rigidities for U.S. firms. We find less evidence of downward wage rigidity in the administrative data than has been documented in previous studies using self-reported earnings from surveys. In our data, only 13 percent of workers who remain with the same firm (job stayers) experience zero change in their nominal hourly wage within a year, and over 20 percent of job stayers experience a reduction in their nominal hourly wage. The lower incidence of wage stickiness in the administrative data is likely partly a function of our earnings concept, which includes all monetary compensation paid to the worker (e.g. overtime pay, bonuses), whereas the previous literature has almost exclusively focused on the base rate of pay. When we examine firm labor cost adjustments on both the hours and wage margins, we find firms have substantially more flexibility in adjusting hours downward than wages. As a result, the distribution of changes in nominal earnings is less asymmetric than the wage change distribution, with only about 6 percent of job stayers experiencing no change in nominal annual earnings, and over 25 percent of workers experiencing a reduction in nominal annual earnings. During the recent Great Recession, this earnings change distribution became almost completely symmetric and the proportion of job stayers experiencing a decline in annual earnings rose markedly to about 40 percent.

# 1 Introduction

There is a long-standing argument in macroeconomics that 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, if this downward wage rigidity (DWR) is in nominal terms, then inflation levels close to zero have negative long-run effects on employment since this makes the DWR constraint bind more often.[1]

A large empirical literature has studied the question of DWR by looking at the distribution of wage changes of workers who remain with the same firm, called job stayers from hereon.[2] While there are some differences in results, studies for the U.S. typically find that the distribution of job stayers' nominal wage changes exhibits a noticeable spike at zero and missing mass to the left of zero.[3] Moreover, this asymmetry has been found to increase during downturns and low-inflation periods, including the recent Great Recession.[4] The results are frequently interpreted as evidence of nominal DWR that contributed to the sharp decline in employment during the Great Recession; and a growing number of researchers are incorporating nominal DWR as a constraint into modern macro models to investigate its consequences.[5]

In this paper, we use linked employer-employee microdata from the Longitudinal Employer Household Dynamics (LEHD) program of the U.S. Census Bureau to take a new look at the extent and consequences of DWR in the U.S. The LEHD data consists of employee wage records that firms submit to the unemployment insurance (UI) office of their state. These data have several advantages over household survey data from the Current Population Survey (CPS), the Panel Study of Income Dynamics (PSID), and the Survey of Income and Program Participation (SIPP) – the main data sources previously used to analyze wage dynamics for the U.S.[6]

First, the administrative nature of the UI records means that the LEHD data, while not entirely free from error, are not subject to the type of rounding and recall error that contaminates wage data from household surveys.[7] These measurement issues have

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[1] See for example Tobin (1972) and Akerlof, Dickens and Perry (1996).

[2] The literature focuses on job stayers because the theories motivating DWR typically apply to wage setting within the firm. Wages of workers changing firms are substantially more flexible. Hence, the degree of rigidity in job-stayers' wages provides an upper bound for wage rigidity in the economy.

[3] See for example Baker, Gibbons and Holmstrom (1994); McLaughlin (1994); Card and Hyslop (1997); Kahn (1997), Altonji and Devereux (1999); Lebow, Saks and Wilson (2003); Gottschalk (2005); Elsby (2009); and Barratieri, Basu and Gottschalk (2014). For a summary of the international evidence, see Dickens et al. (2007).

[4] See Fallick, Lettau and Wascher (2011); Daly, Hobjin and Lucking (2012); and Elsby, Shin and Solon (2013).

[5] See for example The Economist (2014). For studies on the effects of DWR in modern macro models, see Kim and Ruge-Murcia (2009), Benigno and Ricci (2011), Schmitt-Grohe and Uribe (2013a,b).

[6] Prominent studies on wage dynamics using data from the CPS, the PSID and the SIPP include McLaughlin (1994); Card and Hyslop (1997); Kahn (1997), Altonji and Devereux (1999); Gottschalk (2005); Dickens et al. (2007); Elsby (2009); and Barratieri, Basu and Gottschalk (2014).

[7] See Nickell and Quintini (2003) for details on this point with respect to U.K. data. Also see Lebow, Saks and Wilson (2003) and Fallick, Lettau and Wascher (2011) who instead use firm-based data from the Employment Cost Index to analyze wage dynamics in the U.S. While the data from the ECI is of

seriously hampered progress of the literature, with several prominent papers arguing that measurement error leads to substantially overstating the probability of wage cuts at the expense of underestimating the incidence of wage rigidity.[8] The LEHD data allows us to shed new light on this important debate without having to make assumptions about the nature of measurement error.

Second, the LEHD data includes all forms of monetary compensation paid to workers. This is crucial when estimating the incidence of wage rigidity since firms can use irregular payments such as bonuses and overtime pay to incentivize workers and to adjust labor costs.[9] In contrast, the wage data from household surveys typically applies to a more limited earnings concept (i.e. base pay or usual earnings) and is in certain cases affected by topcoding and missing overtime pay.

Third, the worker-firm link of the LEHD data combined with the fact that the LEHD data contains wage records of all workers employed by a firm allows us to relate job stayers' wage changes to a rich set of firm-level variables including size, employment growth, excess turnover and different characteristics of the firm's workforce. This is impossible with other datasets for the U.S. because they contain at most general information about the firm such as industry affiliation.[10] Moreover, since the LEHD data covers the quasi-totality of private-sector workers in the participating U.S. states – i.e. millions and millions of observations – we can drill down into firm-specific wage dynamics without compromising the sample size of the data.

To organize our analysis, we begin with a simple dynamic model of a firm's optimal labor demand in an environment with idiosyncratic worker productivity, hiring costs, and wage setting subject to DWR. The model highlights several conceptual issues ignored so far by the literature that complicate inference about the extent and consequences of DWR. First, when workers have heterogeneous productivity, employees with DWR-constrained wage setting are on average more productive since for the same productivity, the expected value of hiring a worker with a downward rigid wage is lower than hiring an unconstrained employee. This selection effect implies that employees at firms with DWR policies are on average not necessarily more likely to get laid off as is typically implied by the literature. Firms instead incorporate the DWR constraint in their hiring decision.

Second, the model implies that by itself, asymmetries in the wage change distribution of job stayers are not necessarily indicative of the extent of DWR, as

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considerably higher quality, it shares several of the other drawbacks of the household survey data and does not allow the type of analysis we carry out with the LEHD data.

[8] See Akerlof, Dickens and Perry (1996); Altonji and Devereux (1999); Gottschalk (2005); and Barratieri, Basu and Gottschalk (2014).

[9] For example, Babecky et al. (2012) 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.

[10] While we are to our knowledge the first to use administrative worker-firm linked data to analyze the issue of DWR in U.S. firms, recent studies have used similar administrative data with a worker-firm link feature for other countries. The papers closest to us in terms of focus on wage dynamics are Castellanos, Gracia-Verdu and Kaplan (2004) for Mexico; Martins, Solon and Thomas (2010) and Carneiro, Guimares and Portugal (2011) for Portugal; Le Bihan, Montornes and Heckel (2012) for France; and Ehrlich and Montes (2014) for Germany.

frequently assumed in the literature. This is because the observed wage change distribution for job stayers reflects not only the wage policies of the firm, but also its employment decisions, which are affected by productivity growth and other firm-internal and external factors. Instead, it is the *difference* in asymmetries across otherwise similar firms that is a useful measure to quantify the extent to which firms are subject to DWR.

Third, economists are largely interested in wage rigidity because of its presumed effect on employment. However, if a firm has greater flexibility in adjusting hours worked downward than wages, then it may reduce its labor cost by cutting hours rather than conducting layoffs. As a result, downward rigidity in the hourly wage may not be as much of a binding constraint for the firm's *employment* decision. This means that *earnings changes* provide a more relevant metric to study the employment consequences of DWR than hourly wage changes.

The empirical part of the paper is still work in progress. The results so far can be summarized as follows. We begin by examining the distribution of hourly wage changes for the subset of the LEHD data for which we can observe hours paid. Consistent with the existing literature based on household survey-data, we find that the distribution of hourly wage changes is asymmetric, with a spike at zero and missing mass to the left of zero. About 13 percent of job stayers experience no change in their nominal hourly wage within a year and over 20 percent of job stayers experience a reduction in their nominal hourly wage. These results imply substantially less nominal wage rigidity than recently reported for the U.S. by the literature (e.g. Card and Hyslop, 1997; Lebow, Saks and Wilson, 2003; Daly, Hobjin and Lucking, 2012; or Elsby, Shin and Solon, 2013). In particular, Gottschalk (2005) and Barratieri, Basu and Gottschalk (2014) use wage data from the SIPP and find, after correcting for measurement error with econometric methods, a degree of wage rigidity that is more than twice as high and a much lower incidence of wage cuts.

Next, we compare the distribution of job stayers' hourly wage changes to the corresponding distribution of annual earnings changes. We find that this earnings change distribution is less asymmetric, with only about 6 percent of workers experiencing no change in nominal annual earnings within a year, and over 25 percent of workers experiencing a reduction in nominal annual earnings. Moreover, there is important heterogeneity across firms, with many firms showing no evidence (or the opposite) of a zero spike and missing mass to the left of zero in their earnings change distribution. In particular, large firms and contracting firms exhibit on average substantially less earnings rigidity and have more symmetric earnings change distributions.

Decomposing annual earnings changes into hourly wage changes and hours changes, we document that firms systematically achieve reductions in labor costs by decreasing hours rather than cutting hourly wages, as implied by our model. This explains why the distribution of annual earnings changes is less asymmetric. It suggests that DWR may indeed be a constraint for hourly wage adjustments but that firms react to this constraint by adjusting hours worked. Hence, to analyze the *employment* effects of DWR, we believe the earnings change distribution is the more relevant metric to

consider, as it reflect adjustments in both hours and wages that the firm can make in lieu of layoffs.[11]

We then examine how the distribution of annual earnings changes varies over the business cycle and across firms. We document that during the recent Great Recession, the annual earnings change distribution becomes almost completely symmetric and the proportion of job stayers experiencing a reduction in annual earnings increases markedly to about 40 percent. This contrasts with recent studies that report an increase in asymmetry in wage change distributions during the Great Recession (e.g. Fallick, Lettau and Wascher, 2011 ; and Daly, Hobjin and Lucking, 2012).

These results suggest that that U.S. firms are able to substantially reduce labor costs of job stayers when in distress – especially during the Great Recession – either by adjusting hourly wage rates or hours worked or both. At the same time, because of survival bias, we cannot infer from these results that DWR is irrelevant. Firms may have disproportionally laid off workers whose earnings were constrained by DWR.

In ongoing work, we exploit the worker-firm link of the LEHD to relate employment changes at the firm level to different measures of asymmetry in the earnings change distribution typically associated with DWR. As highlighted by our model, inference about this relationship is complicated by the presence of potentially important selection problems. We attempt to address these problems by measuring firm-level DWR with asymmetry measures of the firms' pooled earnings change distribution during 2003-2007 and relating them to job destruction during the Great Recession – an unexpected negative shock that our model implies should affect DWR-constrained firms more severely than unconstrained firms.  While this approach does not completely do away with the problems of identifying DWR from asymmetries in the earnings change distribution of job stayers, at least it avoids measuring rigidity contemporaneously with the recession.

The remainder of the paper proceeds as follows. Section 2 develops the illustrative model to highlight conceptual issues. Section 3 describes the LEHD data, the construction of hourly wage changes and earnings changes, and the statistics used to summarize hourly wage change and earnings change distributions. Section 4 presents results for the subset of the LEHD data for which we have information on hours and hourly wages. Section 5 presents results on earnings changes for the full dataset. Section 6 reports firm-specific wage change distributions and relates them to the firm's employment dynamics. Section 7 concludes.

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[11] Obviously the ability of employers to cut hours rather than pay has implications for *underemployment,* even if it implies more employment than there would be in absence of the ability of employers to adjust hours downward.  Indeed, the number of workers in the CPS who respond they are working 'part-time for economic reasons' spiked in the Great Recession and has remained high throughout the recovery.

# 2    Model

We develop a stylized dynamic model of a firm's optimal labor demand with DWR-constrained wage setting to make three points that inform our empirical strategy:

1. DWR-constrained firms are on average more productive, which implies that DWR-constrained firms do not necessarily lay off a larger fraction of employees.

2. The shape of the observed wage change distribution of job stayers is not only affected by the firm's wage policies but also by other firm-internal or external factors such as expected productivity growth. Asymmetries in the wage change distribution of job stayers in and of themselves are therefore not necessarily indicative of the extent of DWR.

3. The earnings change distribution is a more relevant measure to examine the firm's *employment* decision than the hourly wage change distribution.

## 2.1    Environment

Consider an environment with labor market frictions that prevent firms from replacing workers immediately and at no cost. Once matched, firms and workers therefore enjoy a surplus that justifies wage dispersion and continued employment relationships even in the presence of wage rigidities.[12]

Firms operate for two periods and do not discount the future. In the beginning of the first period, firms match with a mass $m$ of workers. Upon matching, the firm learns each worker's productivity $z \geq 0$, which is stochastic and distributed according to cumulative density function $F(z)$ for the first period and $F(z'|z)$ with $dE(z'|z)/dz > 0$ for the second period (i.e. worker productivity is persistent). After learning z, the firm decides whether to pay hiring cost c and employ the worker at a negotiated wage rate (as described below). In the beginning of the second period, after learning new productivity z' for its employed workers, the firm decides on which workers to continue employing and which workers to lay off.

The firm's per period output from employing a worker with productivity level z is:

$$y = zh^\alpha \ \ with \ 0 < \alpha < 1 \tag{1}$$

where $h$ denotes the number of hours worked. We will consider two cases. In the first case, hours are fixed at some level $\bar{h}$ (due, for example, to contractual obligations) and the firm's decision is simply about employment in each of the two periods. In the second case, the firm can adjust hours flexibly as a function of observed productivity and the wage rate. The firm's employment decision in this case is then made conditional on the optimal hours decision. In all of these decisions, the firm's outside option of not employing a worker or laying off a worker is assumed to be zero.

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[12] See Hall (2005), Shimer (2005), or Gertler and Trigari (2009) among many others for an elaboration of this argument in the context of a search model of the labor market.

The wage rate that splits the match surplus is the outcome of a negotiation between the firm and a worker. In the first period, the outcome of this negotiation is assumed to be the same for all workers and described by the following reduced-form wage-setting equation:

$$w(z) = \varphi z + (1 - \varphi)b \qquad (2)$$

where *w(z)* is the real wage rate; *b≥0* measures a combination of worker-specific, firm-specific and aggregate factors that are assumed independent of productivity level *z*; and $\varphi \in [0,1]$ measures the importance of the worker's productivity for wage setting.[13]

In the second period, wage setting depends on whether the negotiation is DWR-constrained or not. In particular, we assume that for a fraction $1 - \lambda \in [0,1]$ of the firm's workers, wage setting is unconstrained and described as above:

$$w'(z') = \varphi z' + (1 - \varphi)b \qquad (3)$$

For the remaining fraction $\lambda$ of workers, negotiations occur instead over the nominal wage rate and are constrained by the restriction that the second-period nominal wage cannot be below the first-period nominal wage; i.e.

$$w'_{DWR}(z', w) = max[\varphi z' + (1 - \varphi)b, w/\pi] \qquad (4)$$

where $\pi$ is the gross inflation rate between the first and the second period.[14] The reasons for this DWR constraint are not made explicit here but could capture, for example, wage norms that enter into negotiations as a result of fairness considerations.[15] Moreover, we assume that firms know in advance (i.e. at the hiring stage) which workers are subject to DWR-constrained wage setting in the second period and that this is independent of productivity *z*.

In much of what follows, we keep b and $\pi$ constant (and normalize $\pi$ =1 without loss of generality). While we recognize that both variables are equilibrium objects, this considerably simplifies the analysis. In future versions of the paper, we will discuss the importance of taking into account variations in b and $\pi$.

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[13] In a baseline labor search model, for example, this form of wage setting equation would, under certain assumptions, result from Nash-bargaining, with *b* being a combination of the worker's reservation wage; the firm's hiring and vacancy posting costs; and aggregate labor market tightness.

[14] In more detail, the wage setting equation for the DWR-constrained workers is $W' = max[\varphi P'z' + (1 - \varphi)P'b, W]$, where *P* and *P'* are price levels and *W* and *W'* are the nominal wages in the two periods. By dividing both sides with *P'* and defining *π= P'/P*, we obtain the equation in the text. Notice that the downward rigidity can easily be formulated in real terms by setting *π=1.*

[15] See Bewley (1999) for an empirical account and, for example, Elsby (2009) for a formal treatment of this idea.

## 2.2    Employment decision if hours are fixed

Suppose hours are fixed at $\bar{h} = 1$, without loss of generality. We start by considering the firm's employment decision for firm-worker match under unconstrained wage setting. Given initial productivity z, the period 1 value of the firm for this worker is

$$V(z) = max[-c + (z - w(z)) + E(max\,[z' - w', 0]|z), 0] \tag{5}$$

subject to wage setting equations (2) and (3); and, given new productivity z', the period 2 value of the firm is

$$V'(z') = max[z' - w'(z'), 0] \tag{6}$$

subject to wage setting equation (3). Substituting for the wage, the two values can be expressed as

$$V(z) = max[-c + (1 - \varphi)(z - b) + (1 - \varphi)E(max\,[z' - b, 0]|z), 0] \tag{7}$$

and

$$V'(z') = (1 - \varphi)max[(z' - b), 0]. \tag{8}$$

Since V(z) is increasing in z and V(0)<0, the firm hires the worker if period 1 productivity z ≥ $\underline{z}$, where $\underline{z}$ is the employment threshold level of productivity defined by V($\underline{z}$) = 0. This threshold level is increasing in c and b. Likewise, since V'(z') is increasing in z' and V'(0) < 0, the firm continues to employ a worker in period 2 if period 2 productivity z' ≥ $\underline{z}'$ = b since V'(b) = 0.

Now, consider the firm's employment decision under DWR-constrained wage setting. The firm's period 1 and period 2 values are as above, but now subject to DWR-constrained wage setting in period 2, equation (4), instead of equation (3). Substituting for the wage, we obtain

$$V_{DWR}(z) = max\left[\left(\begin{array}{c} -c + (1 - \varphi)(z - b) + \\ E(max[z' - \varphi\max(z', z) - (1 - \varphi)b, 0]\,|z) \end{array}\right), 0\right] \tag{9}$$

and

$$V'_{DWR}(z', z) = max[z' - \varphi\max(z', z) - (1 - \varphi)b, 0], \tag{10}$$

where the DWR subscript indicates that period 2 wage setting is DWR-constrained. As for the unconstrained case, V_DWR(z) is increasing in z and V_DWR(0) < 0. Hence, the firm hires the workers in period 1 if period 1 productivity z ≥ $\underline{z}$_DWR, where $\underline{z}$_DWR is defined by V_DWR($\underline{z}$_DWR )=0. Likewise since V'_DWR(z',z) is weakly increasing in z' and V'_DWR(0) < 0, the firm continues to employ a worker in period 2 if period 2 productivity z' ≥ $\underline{z}'$_DWR defined by V'_DWR($\underline{z}'$_DWR) = 0.

To analyze the employment threshold levels under DWR-constrained wage setting, it is useful to distinguish between workers with period 1 productivity z≤b and workers with z>b. For z≤b, $z' - \varphi\max(z', z) < (1 - \varphi)b$ for any z' < b and thus $\underline{z}'$_DWR = b, as in

8

the unconstrained case (this can be easily seen from equation 10). This means that for workers with z ≤ b, the firm's expected period 2 value is the same independent of whether period 2 wage setting is DWR-constrained or not; i.e. $E(V'_{DWR}(z')| z \le b) = E(V'(z')| z \le b)$. But then, for these workers, the firm's period 1 value is also independent of DWR; i.e.

$$V_{DWR}(z) = V(z) \ for \ z \le b \qquad (11)$$

For z>b, in contrast, the period 2 threshold is $\underline{z}'_{DWR}= \varphi z + (1 - \varphi)b = w(z) > b$ (this can again can be seen from equation 10). Hence, for workers with z > b, the firm's expected period 2 value is lower under DWR-constrained wage setting – i.e. $E(V'_{DWR}(z)|z>b) < E(V'(z')|z>b)$. As a result, the firm's period 1 value under DWR-constrained wage setting is lower as well; i.e.

$$V_{DWR}(z) < V(z) \ for \ z > b \qquad (12)$$

Result (12) implies that as long as hiring cost c are sufficiently high such that the period 1 threshold for an unconstrained workers is $\underline{z}$>b, then it will be the case that $\underline{z}_{DWR} > \underline{z}$. Firms with DWR-constrained wage setting therefore 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 period 2 productivity level z'*, the threshold for layoff is higher under DWR-constrained wage setting; i.e. $\underline{z}'_{DWR} > \underline{z}'$. On the other hand, employees in DWR-constrained firms have on average higher period 2 productivity, 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.

Instead, one needs to look to other empirical strategies. One is to compare measures of worker productivity across employees or firms with different extents of DWR. Another is to look at the response of layoff or job destruction rates to large unexpected negative shocks. We will explore both of these possibilities in the last part of the paper.

## 2.3    Wage change distribution if hours are fixed

The wage change distribution, which is the object of interest in much of the existing wage rigidity literature, is defined by equations (2)-(4). Remaining with the assumption that hours are fixed at $\bar{h} = 1$, consider a firm, or more generally the economy, with a fraction of workers that are subject to DWR-constrained wage setting. Define a notional wage change distribution, which would obtain if the firm hired all workers and did not lay off any worker in the second period.  Absent DWR and keeping b constant, equations (2) and (3) imply that wage growth *dlogw = logw' – logw* is approximately proportional to productivity growth *dlogz = logz' – logz*. Hence, the notional wage change distribution absent DWR is approximately the same as the distribution of productivity growth. With DWR-constrained wage setting, equations (2) and (4) imply that wage growth is 0 for

z′ ≤ z and approximately proportional to dlogz for z′ > z. Hence, for a firm with a fraction of workers subject to DWR-constrained wage setting, the notional wage change distribution is asymmetric, with a spike a zero and missing mass to the left of zero. The larger the fraction of workers with DWR-constrained wage setting, the more important these asymmetries. This is why the literature on downward wage rigidity commonly interprets missing mass and zero spike in the wage change distribution as indicators of DWR.

The problem with this interpretation is that, as analyzed above, firms only employ workers with productivity above a certain threshold level. This *selection effect,* which is different from the selection effect discussed above, implies that the *observed* wage change distribution of job stayers differs from the *notional* wage change distribution. This complicates inference about downward wage rigidity in non-trivial ways.

To illustrate, we simulate our model for the case where z and z′ are drawn from a lognormal distribution, with the mean of z′ increasing in z (i.e. productivity is persistent as assumed in the model). Specifically,
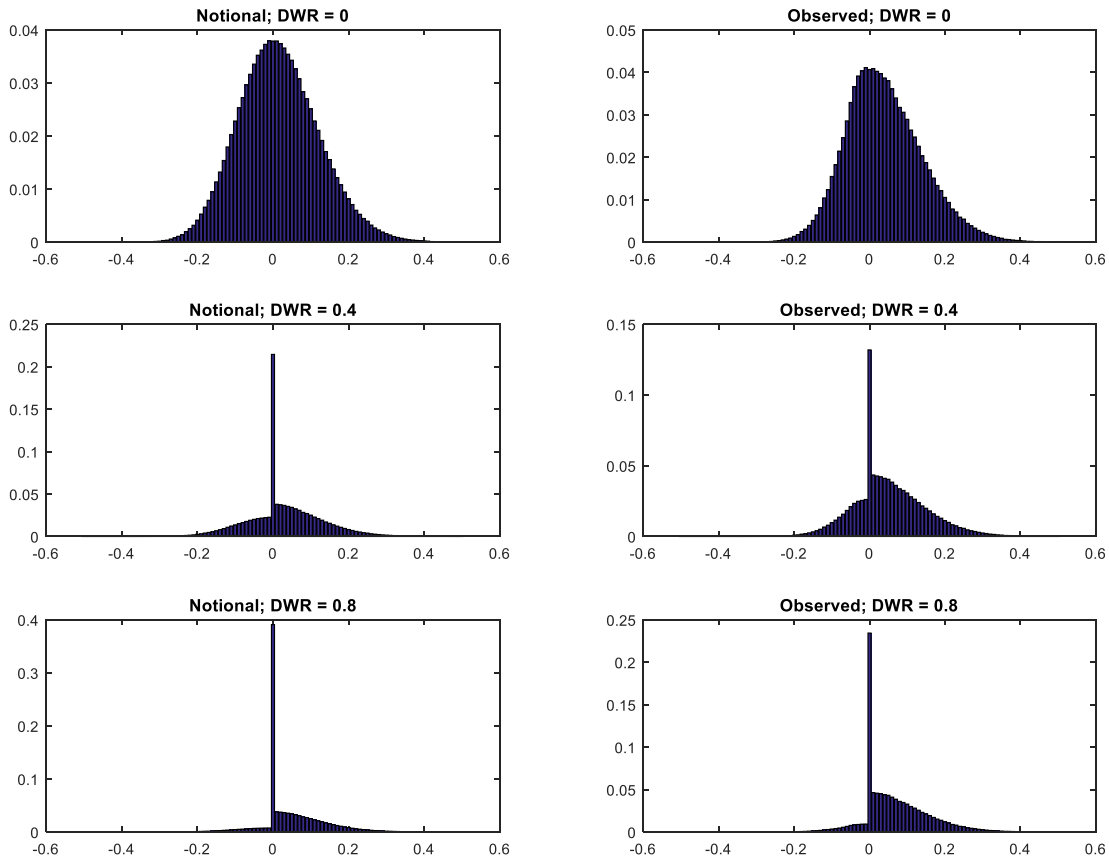
$$log(z) = \sigma \varepsilon \tag{13}$$

and

$$log(z') = \mu + log(z) + \sigma \varepsilon \tag{14}$$

where μ denotes the average growth rate of productivity and ε is distributed N(0,1). The resulting distribution of productivity growth is therefore symmetric.

The following figure compares the notional wage change distribution with the observed wage change distribution for three different degrees of DWR, $\lambda$.

As shown in the first row of the figure, the notional wage change distribution absent DWR is approximately symmetric, due to the assumed symmetry of the productivity growth distribution. The observed wage change distribution, in contrast, is skewed to the right, with missing mass to the left of zero. Intuitively, the skewness arises because some of the negative wage change observations included in the notional distribution are associated with workers who are laid off in the second period because their productivity declines below the zero profit threshold. As a result, these workers' wage changes do not appear in the observed distribution.

The second and third rows of the figure show the notional and the observed wage change distribution for a firm with 40%, respectively 80% of workers subject to DWR-constrained wage setting. There is a zero spike and missing mass to the left of zero in both the notional and the observed distribution. The selection effect for these two cases is even more apparent. In particular, the proportion of job stayers experiencing a zero spike in the observed distribution is significantly smaller than the one of all workers – employed or not – in the observed distribution (notice the difference in scale between

11

the left- and the right-hand side graphs). This is because many of the DWR-constrained workers who experience a decline in productivity (and would therefore have zero wage change) are laid off.

By itself, the difference between notional and observed wage change distribution would not be a problem as the asymmetry and zero spike are increasing in the degree of DWR (e.g. at the extreme where wage setting of all workers is DWR-constrained, both the notional and the observed wage change distribution feature a large spike at zero with no mass to the left of 0). A problem arises, however, once we take into account that the extent of the selection effect depends not only on the degree of DWR but also on other factors influencing the firm's employment decision, in particular the average growth rate of productivity.

Specifically, the lower the expected average productivity growth μ, the higher the period 1 thresholds below which workers are not hired. This affects the composition of the workers and therefore the shape of the observed wage change distribution of job stayers. Similarly, if a firm (or the economy) gets hit by an unexpected negative shock, layoffs will increase and disproportionately so for workers whose wage setting is DWR-constrained. The resulting decline in the proportion of job-stayers whose wage setting is DWR-constrained again affects the shape of the observed wage change distribution. For a range of parameterizations of the model, we find that such expected or unexpected declines in average productivity growth lead to *more* symmetry in the observed wage change distribution and a reduction in the spike at zero – features typically associated with less wage rigidity – even though the extent of DWR in the model is left unchanged.

This illustrate that by itself, asymmetry in the wage change distribution of job stayers is not necessarily indicative of DWR, as is often implied by the existing literature. Instead, it is the *difference* in asymmetries across otherwise similar firms that represent a more useful measure to quantify the extent to which firms are subject to DWR.

### 2.4 Employment decision and wage change distribution if hours are flexible

Finally, consider the case where the firm can flexibly adjust hours worked of its employees. The firm solves the following problem $max_h[zh^\alpha - wh]$; and the resulting optimal hours choice is

$$h^* = \left(\frac{\alpha z}{w}\right)^{1/(1-\alpha)} \tag{15}$$

Hence, hours adjust inversely with the wage. Similar to the fixed-hours case, it can be shown that there exists productivity threshold levels below which a worker will not be hired in the first period, respectively laid off in the second period. But by the fact that $h^*$ is the optimal hours choice, it has to be that $z\bar{h}^\alpha - w\bar{h} \leq zh^{*\alpha} - wh^*$ for any $z$, $w$ and

any degree of DWR. Hence, the threshold productivity levels in the flexible hours case are uniformly lower than the ones in the fixed hours case.

The result implies that the distribution of hourly wage changes is not necessarily a good predictor of the firm's employment dynamics. Consider for example two identical firms except that firm 1 employs workers with fixed hours only, whereas firm 2 employs workers with flexible hours only. As above, we assume that z and z' are drawn from a lognormal distribution with the mean of z' increasing in z (i.e. productivity is persistent as assumed in the model); and that about half of the workers are DWR-constrained.
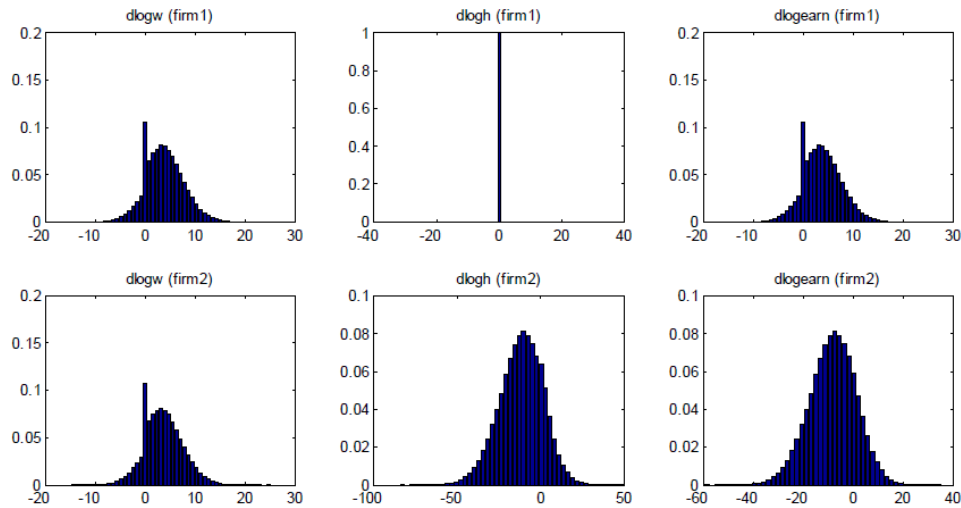


The figure shows the observed distributions of hourly wage changes, hours changes, and earnings changes of firm 1 (top row) and firm 2 (bottom row). As the two panels in the left column show, the observed hourly wage change distribution of the two firms looks by definition exactly the same. However, the hours change distribution is very different and so is the earnings change distribution. This is because firm 2 adjusts hours worked systematically so as to maximize profits. As a a result, firm 2 hires more workers in period 1 and lays off fewer workers in period 2.

The example makes clear that if one wants to use asymmetries in the wage change distributions to predict the *employment* consequences of DWR, then the earnings change distribution is a more relevant metric than the hourly wage change distribution (subject to the challenges discussed above with using asymmetry statistics to measure DWR).

# 3 Data

We start by providing a brief overview of the Longitudinal Employer-Household Dynamics (LEHD) Program at the U.S. Census Bureau. Then, we describe how we use the data to construct hourly wage and earnings changes for job stayers; and how we characterize the distribution of hourly wage and earnings changes with different non-parametric statistics.

## 3.1 The LEHD data

The core of the data consists of worker-specific earnings records that employers submit every quarter to the unemployment insurance (UI) office of their state. States, in turn, submit the UI records to the LEHD program as part of the Local Employment Dynamics federal-state partnership. The earnings record data are submitted along with establishment-level datasets collected as part of the Quarterly Census of Employment and Wages (QCEW), which provides information about employers. Overall, the LEHD data covers over 95% of employment in the private sector, as well as employment in state and local government.[16]

Aside from earnings, the LEHD contains detailed information about the location and industry of firms, as well as the age and gender of workers. Moreover, for the three states Minnesota (MN), Rhode Island (RI) and Washington (WA), the LEHD also contains the number of hours paid for each worker.[17] Finally, the characteristics of the LEHD allow us to infer tenure of workers within a given firm, the total number of employees per firm, as well as average earnings and average gender, race and age composition of the firm's workforce.

As discussed in the introduction, the LEHD data has several important advantages over survey-based datasets that are typically used to compute wage dynamics for the U.S. First, the LEHD data is based on administrative 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, the LEHD earnings concept includes all forms of monetary compensation received throughout a year and not just the base wage. Specifically, earnings include gross wages and salaries, bonuses, stock options, tips and other gratuities, and the value of meals and lodging, where supplied.[18] Aside from benefits, LEHD earnings therefore capture the total cost of a worker to the firm. Third, the worker-firm link in the LEHD allows us to relate wage dynamics of job stayers to firm-specific conditions. Fourth, the LEHD covers the quasi-totality of private-sector workers in the participating U.S. States. The size of the dataset – millions and millions of observations – allows us to decompose the data in several important dimensions without compromising its representativeness.

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[16] For a full description of the LEHD data, see Abowd et al. (2009). Our analysis considers workers employed in private-sector firms, although the analysis could in principle be extended to local and state government workers.

[17] The other states' UI offices do not require their firms to submit information on hours paid.

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

## 3.2 Construction of earnings changes and average hourly wage changes

The bulk of our analysis focuses on year-to-year changes in annual earnings and average hourly wages of job stayers. This choice is motivated by two considerations. First, a substantial fraction of workers receive bonuses and other irregular payments that are recorded in a particular quarter. These payments are part of compensation and a potentially important component of labor cost, but their exact timing may not be as relevant for the firm. Second, firms typically report to the UI system the earnings disbursed during the quarter rather than the earnings accrued. This results in potentially large spurious spikes in the quarter-to-quarter earnings change distribution.[19] On an annual basis, this pay-period problem disappears and does not seem to affect the results.

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 year *t-1* or whose employment ended *during* the fourth quarter of year *t*, thereby leading to spurious year-to-year changes in earnings.

For each of the identified job-stayers, we compute annual earnings as the sum of quarterly earnings for year t-1 and for year t; and the year-to-year change in annual earnings as the log difference in annual earnings between *t* and *t-1*. The following diagram illustrates these computations.

| Year t-2 Q4 | Year t-1 Q1 | Year t-1 Q2 | Year t-1 Q3 | Year t-1 Q4 | Year t Q1 | Year t Q2 | Year t Q3 | Year t Q4 | Year t+1 Q1 |
|---|---|---|---|---|---|---|---|---|---|
| Job X | Job X | Job X | Job X | Job X | Job X | Job X | Job X | Job X | Job X |
| | Annual Earnings Year t-1 | | | | Annual Earnings Year t | | | | |

For the three states with individual hours information (MN, RI and WA), 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. Each job stayer therefore generates

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[19] As an example, imagine partitioning 26 bi-weekly pay periods into four quarters. Two quarters will have six pay periods and two quarters will have seven pay periods.

four annual hourly wage change observations over a two-year period. We call this the *4-quarter change in the hourly wage*. Alternatively, we obtain the hourly wage for each year by dividing annual earnings by annual hours paid and then compute the year-to-year change in the hourly wage as the log difference of the annual hourly wage between two years. We call this the *year-to-year change in the hourly wage*.

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 period prior to the interview.[20] We will 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.

## 3.3   Samples

We consider three different samples. Sample 1 consists of the three states with individual hours information (MN, RI, WA).  Since the LEHD Program imposes a three-state minimum for public disclosure of any results based on micro-data, the length of this sample is restricted by the quarter the last of the three states made its hours data available, which is Rhode Island in the fourth quarter of 2009.[21] Sample 1 therefore consists of annual earnings changes and hourly wage changes of job-stayers in these three states for 2010-2011.[22]

Sample 2 consists of the 30 states with earnings information from 1998:Q2 through 2012:Q2.[23]  This yields 12 years of annual earnings change observations for all job-stayers in these states (1999-2000, 2000-2001,...,2010-2011).

Sample 3 consists of all observations in Sample 2 coming from firms with positive median earnings change and at least 50 job-stayers in a given year. The reason for considering this 'restricted large firm' sample is discussed further below.

Table 1 reports descriptive statistics for each of the three samples. One basic fact stands out: the size of our samples is very large, even if we restrict the analysis to the three states with hours data; or to large firms in the 30-state sample with 50+ stayers. The 3-state sample has over 2 million job stayers between 2010 and 2001, and the 30-state sample has over 30 million job stayers in the average year.  This is orders of

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[20] 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.

[21] Minnesota and Washington have data available for longer time periods.

[22]  We will expand our analysis to include 2011-2012 in future revisions.

[23] The other states joined the LEHD program gradually after 1998. For data consistency, we limit our study to the 30 states available in the program as of 1998.

magnitude larger than the sample size of the usual household surveys such as the PSID, the CPS or the SIPP; or the firm-based ECI sample.

One other observation of note in Table 1 is that Sample 3 – firms with 50 or more job stayers – contains considerably fewer firms (about 66,000 firms in the average year) than Sample 2 (about 2.5 million firms in the average year). This is primarily because the majority of firms have less than 10 job-stayers per year. Since the firms with 50 or more job stayers account for a large fraction of total employment, however, Sample 3 remains very large, with almost 16 million job-stayers in the average year.

A final but important observation is that the mean characteristics of Sample 1 and Sample 2 are very similar despite the fact that Sample 1 only contains data for three states for 2010-2011 whereas Sample 2 contains data for 30 states for 1999-2011. This suggests that the results from Sample 1 reported below apply more generally than just to the three states for that particular two-year period.


### 3.4    Histograms and asymmetry statistics

The large sample size allows us to analyze the distributions of hourly wage and earnings changes of job-stayers non-parametrically through histograms. All of the histograms reported below show 1% bins centered around zero; i.e. the zero interval contains all hourly wage or earnings change observations in (-0.5%, 0.5%); the adjacent intervals contain observations in (-1.5%, -0.5%] and [0.5%, 1.5); and so forth. In total, we have 51 intervals of size 1%, with two open-ended intervals for observations smaller than -25.5% and observations exceeding 25.5%.

We quantify the characteristics of the different wage change distributions through a set of asymmetry statistics. Let $F(.)$ the cumulative density of a wage change distribution. We define:

- Missing mass left of 0:

  $\gamma = 1 - F(2 \times median + 0.005) - F(-0.005)$

- Spike at 0:

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

- Excess mass right of 0:

  $\zeta = [0.5 - F(0.005)] - [F(2 \times median - 0.005) - 0.5)]$


Figure 1 provides a graphical representation of the three statistics. The missing mass left of zero, $\gamma$, is positive if the mass to the left of zero (area A) is smaller than the corresponding mass that is equidistant to the right of the median (area F). The spike at zero, $\eta$, is positive if the mass around zero (area B) is larger than the corresponding mass that is equidistant to the right of the median (area E). The excess mass right of

17

zero, $\zeta$, is positive if the mass between zero and the median (area C) is larger than the mass that is equidistant to the right of the median (area D). If the wage change distribution is symmetric, then all three statistics are zero.

The different statistics are closely related to asymmetry measures that the literature has associated with DWR; e.g. Lebow, Stockton and Wascher (1995), Card and Hyslop (1997), Kahn (1997) or Lebow, Saks and Wilson (2003). Contrary to these papers, we do not interpret our asymmetry statistics as direct measures of DWR. Even in the absence of DWR, the wage change distribution of job stayers may be asymmetric because of asymmetries in the distribution of worker productivity growth, non-linearities in the wage setting process that are unrelated to DWR, or selection bias (i.e. the fact that we only observe wage changes of job stayers who are systematically different from separated workers). We argue instead that the *difference* in asymmetry statistics across otherwise similar firms is a useful measure to quantify the extent to which firms are subject to DWR.

## 4  Hourly wage and earnings changes for the 3-state sample

Much of the existing literature on wage dynamics for the U.S. focuses on hourly wage changes. We therefore start the analysis with the 3-state sample (Sample 1) for which we can compute hourly wage changes. We then compare the hourly wage change results to annual earnings changes computed from the same 3-state sample.

### 4.1  Aggregate hourly wage change distributions

Figure 2a shows the 4-quarter hourly wage change distribution in the 3-state sample (i.e. hourly wage changes computed as the log difference in quarterly hourly wages four quarters apart). Figure 2b shows the year-to-year hourly wage change distribution (i.e. hourly wage changes computed as the log difference in annual hourly wages). Figure 2c combines the two distributions in the same graph for ease of comparison. Table 2 (first column) provides summary statistics for the two distributions.

Both hourly wage change distributions exhibit a spike at zero and missing mass to the left of zero. However, the asymmetry is more pronounced for the 4-quarter hourly wages changes (Figure 2a), with about 13% of job stayers experiencing zero change in the hourly wage within a year and a smaller proportion experiencing hourly wage cuts in the vicinity of zero. The larger mass at zero for the 4-quarter hourly wage change distribution relative to the year-to-year hourly wage change distribution should not be surprising given that average hourly wages for the distribution computed with annual data need to remain constant for two years instead of just one year in order to register as a zero.[24]

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[24] We thank Gary Solon for making this point.

Visually, the two hourly wage change distributions look quite similar to the ones reported by Kahn (1997); Card and Hyslop, (1997); Daly, Hobjin and Lucking (2012); or Elsby, Shin and Solon, (2013) based on U.S. household survey data from the PSID and the CPS. However, the mass at zero in our distributions is noticeably smaller than what is reported, for comparable years, by Daly, Hobjin and Lucking (2012) and Elsby, Shin and Solon (2013) (see their Figure 3 and Figure 5, respectively). Given that the LEHD data is based on administrative records and captures total compensation to the workers, this difference suggests that measurement error and limitations in the earnings concept are important issues when computing wage change distributions derived from survey-based data, especially what the fraction of zero wage changes is concerned.[25]

Another result coming out of Figure 2 is that over 20% of job stayers experience a cut in their hourly wage. According to the LEHD, wage cuts are therefore far from a rare occurrence. This is interesting because there exists somewhat of a controversy in the literature about the extent of wage cuts for job stayers in the U.S. On the one hand, evidence from personnel records of individual firms indicates that wage cuts are rare (e.g. Baker, Gibbs and Holmstrom, 1994; or Altonji and Devereux, 2000). On the other hand, the above listed studies based on survey data report that wage cuts are more frequent. Akerlof, Dickens and Perry (1996), Altonji and Devereux (2000), Gottschalk (2005) and Barratieri, Basu and Gottschalk (2014) argue that this difference may be due to measurement error. After using different econometric models to correct for measurement errors, they find that the extent of wage cuts in the survey data is substantially reduced.

Provided that the administrative data submitted by employers to the UI offices are relatively free of measurement error, our results indicate that reductions in hourly wage and even more so earnings are quite common for job stayers across many firms. One of the reasons for this difference in result is presumably that the earnings concept in the LEHD is more complete than what is available in the survey data. This is consistent with Altonji and Devereux (1999) who find that the proportion of wage cuts in their personnel data increases considerably when bonuses are added as part of compensation. Moreover, it is possible the econometric methods applied by some of the aforementioned studies classify too many negative wage changes as measurement error.

## 4.2   Aggregate earnings change distributions

Figure 3 shows the distribution of year-to-year earnings changes for the 3-state sample together with the distribution of year-to-year hourly wage changes (i.e. the distribution from Figure 2b). In comparison to the distribution of hourly wage changes,

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[25] In particular, the CPS ORGs that Daly, Hobjin and Lucking (2012) and Elsby, Shin and Solon (2013) use do not cover irregular bonus payments, and a substantial fraction of earnings observations is topcoded. Moreover, hourly wage rates reported by hourly paid workers do not cover compensation for overtime. See Abraham, Spletzer and Stewart (1998) and Champagne, Kurmann and Stewart (2015) for details.

the distribution of earnings changes is more disperse and more symmetric with markedly lower mass at zero. Only about 6% of job stayers experience zero change in annual earnings and almost 30% of job stayers experience a cut in annual earnings.

It is again interesting to compare our results to results from household survey data. Card and Hyslop (1997) and Elsby, Shin and Solon (2013) report earnings change distributions for salaried workers in the CPS and find that the fraction of workers experiencing no earnings change is between 10 and 15 percent (see in particular Elsby et al.'s Figure 6 and Table 6). This is markedly above what we find, reinforcing the conclusion from above that measurement error and limitations in the earnings concept are indeed important issues for wage change distributions computed from survey data.

To investigate the distribution of hourly wage and earnings changes further, we take all firms in Sample 1 with at least 50 job-stayers and compute for each firm the proportion of job stayers experiencing an hourly wage cut and an annual earnings cut. We then bin the firms according to these proportions. Figure 4 shows the results.

As the grey and black bars on the very left of the figure indicate, there are only about 31% of firms in which less than 10% of job stayers experience an hourly wage cut; and there only about 11% of firms in which less than 10% of job stayers experience an earnings cut. In other words, hourly wage and earnings cuts are a relatively frequent occurrence in the majority of U.S. firms.


## 4.3   The importance of hours changes

The difference between the hourly wage change distribution and the earnings change distribution in Figure 3 suggests that firms systematically adjust hours paid. To shed more light on the role of this hours adjustment, Figure 5 expands on Figure 3 by showing in separate panels the distribution of hourly wage changes, the distribution of hours changes, and the distribution of earnings changes.  Interestingly, the distribution of changes in hours worked is roughly symmetric and concentrated between -10% and 10% with only about 25% of job-stayers working the same number of hours in both years.  This suggests that for a large fraction of job-stayers, firms have substantial flexibility in adjusting hours either upward or downward.[26]

Figure 6 decomposes year-to-year earnings changes into changes in hours worked and changes in hourly wages; i.e. for each job stayer *i*, we compute

$$\Delta\ln\left(earnings_{it}\right) = \Delta\ln\left(hourly\ wage_{it}\right) + \Delta\ln\left(hours_{it}\right)$$

and average the numbers for each 1% bin of earnings changes. As the figure shows, a much larger fraction of decreases in annual earnings is on average accounted for by decreases in hours than by decreases in hourly wages. In contrast, increases in annual

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[26] This variation in number of hours worked may be even larger than implied by the reported results because our hours measure pertains to hours paid and not hours effectively worked.

earnings are on average accounted for more evenly by increases in both hours and hourly wages.

To provide further evidence of this asymmetry in hours adjustment, we regress for each job stayer the annual hours change and the hourly wage change on annual earnings change and different control variables. Table 3a reports the results for the hours change regressions. Table 3b reports the results for the hourly wage change regression. In each table, column (1) is a linear specification across all observations (excluding the two open-ended annual earnings intervals), while columns (2)-(4) split the sample according to whether the earnings change was positive or negative. The regression results confirm the visual from Figure 5: hours changes account on average for about 75% of negative earnings changes but only for about half of positive earnings changes.

The results in Tables 3a and 3b are largely robust to adding different demographic and firm controls. This is somewhat surprising since a priori, one would think that for job stayers paid by the hour, hours can be adjusted downward more easily than for salaried workers and that this would show up in some of the demographic controls.[27] We plan to investigate this point further in the future. Interestingly, the regression R-squared of the hours change regression increases somewhat when a firm fixed effect is added (column (4) of Table 3a). This provides mild evidence that hours changes occur more frequently for some firms than for others. Conversely, adding a firm fixed effect to the hourly wage change regression does not change the regression R-squared significantly (column (4) of Table 3b).

The results in Figures 3-6 and Table 3 suggest an interesting new fact of how firms adjust labor costs downward while retaining workers, namely that in many instances, they adjust hours downward more flexibly than they cut the hourly wage rate. This empirical finding is consistent with basic firm optimizing behavior, as illustrated by our model, and explains why the aggregate distribution of annual earnings changes is more symmetric and displays a smaller spike at zero than the distribution of hourly wage changes, with a larger proportion of workers experiencing a reduction in earnings than a hourly wage cut.

The results also suggest that DWR may indeed be a relevant constraint for labor adjustments at the intensive (hours) margin. This is different from the usual interpretation in the literature that DWR mainly affects labor adjustment at the extensive (employment) margin. We plan to analyze the extent and consequences of this "intensive-margin" DWR constraint further in future versions of the paper.

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[27] We also ran regressions adding the job-stayer's first-year earnings level as a control. None of the results changed noticeably.

# 5 Earnings changes for the 30-state sample

We turn to the 30-state sample for which we can compute annual earnings changes from 1999-2000 to 2010-2011 (Sample 2). As the previous section illustrated, the earnings change distribution is more disperse and symmetric than the hourly wage change distribution, due to the greater flexibility firms appear to have in adjusting downward hours in lieu of hourly wages. Since we do not have information on hours for the 30-state sample, it is unfortunately not possible to decompose earnings changes into hourly wage changes and hours changes. Nevertheless, looking at the earnings change distribution is instructive because it is total labor cost that ultimately decides the employment decision of the firm.

## 5.1 Aggregate earnings change distributions

Table 2 (second column) reports the statistics for the earnings change distribution of the 30-state sample pooled over all years. There are just under 6% of job stayers experiencing zero change in annual earnings, and about 30% of job stayers experiencing a decrease in earnings. Further, there is about 3% missing mass right of zero; an excess spike at zero of about 2%; and excess mass right of zero of about 1%. These numbers are close to the asymmetry statistics of the earnings change distribution of the 3-state sample for 2010-2011 (see Table 2, first column), which suggests that the 3-state sample for these particular two years may be quite representative of the U.S. economy for a larger time period.

Figure 7 plots the annual earnings change distribution of the 30-state sample for each 2-year period between 1999 and 2011, with the superimposed red line showing the distribution of the annual earnings change distribution pooled over all years. As is clear from the figure, the distribution of annual earnings changes shifts markedly to the left during the Great Recession and seems to become more symmetric. The mass at zero increases slightly in 2009-2010 and 2010-2011 as the economy starts to emerge from the recession.

Figure 8 quantifies these shifts in the earnings change distribution by reporting the time series of the different distributional statistics. As Panel A shows, the mean earnings change and the proportion of job-stayers experiencing earnings increases drop precipitously in 2007-08 and 2008-09 before recovering somewhat by 2010-2011, while the proportion of job-stayers experiencing earnings decreases rises markedly from about 25% in 2005-2006 to almost 40% in 2008-2009 before decreasing to about 30% by 2010-2011. Panel B shows that missing mass left of zero and excess mass right of zero also decrease markedly in 2007-2008 and 2008-2009 before increasing again in 2009-10 and 2010-2011. Finally, as Panel C shows, the excess spike at zero also decreases in 2007-09 and 2008-09 before increasing above 3% in 2010-2011. These statistics confirm the main result from Figure 7 that the earnings changes became more symmetric during the Great Recession and only turned asymmetric again as the economy started recovering.

22

Overall, Figures 7-8 indicate that firms are on average able to substantially reduce labor costs of job stayers, especially so during the Great Recession. This result provides clear evidence against a strong form of DWR according to which almost no workers experience earnings cuts. At the same time, we cannot infer from our results that DWR is not a binding constraint for at least parts of the U.S. economy. As illustrated by our model, the earnings change distribution of job stayers may be more symmetric than the hypothetical earnings change distribution of the entire workers because DWR-constrained workers are on average less likely to be hired. Moreover, during the recent Great Recession, the earnings change distribution of job-stayers may have become more symmetric because firms disproportionally laid off DWR-constrained workers. Similarly, firms with more flexible wage policies may have reduced their workforce by less during the Great Recession, thus becoming a more important contributor to the aggregate earnings change distribution. We examine this possibility below when we consider firm-specific earnings change distributions.

## 5.2    Earnings change distributions across broad firm characteristics

As described above, the linked worker-firm feature of the LEHD data allows us to construct firm-specific earnings change distributions. We exploit this feature by first considering earnings change distributions according to broad firm characteristics; in particular, firm employment growth and firm age and size. Then we dig deeper by running predictive regressions for firm-specific earnings asymmetry statistics.

Figure 9 displays earnings change distributions in the 30-state sample pooled over all years according to whether the firm in a given two year period was in the bottom third of employment growth (i.e. contracting), the middle third of employment growth (stable), or the top third of employment growth (expanding). As the middle panel shows, the earnings change distribution of stable firms displays a noticeable spike at zero and substantial missing mass left of zero. By contrast, the earnings change distribution of contracting and expanding firms is close to symmetric.

Next, we decompose the sample into small, young firms (less than 100 employees, less than 20 years old); small, older firms (less than 100 employees, 20 years old or more); large, young firms (100 employees or more, less than 20 years old); and large, older firms (100 employees or more; 20 years old or more). As Figure 10 shows, the earnings change distribution of small firms, independent of age, exhibits a larger than average spike at zero and substantial missing mass left of zero. By contrast, the earnings change distribution for larger firms has no zero spike and is generally more symmetric. In other words, there is a marked difference in earnings change distribution according to firm size.

Figure 11 displays the time series of summary statistics for the earnings change distributions of the different firm types. During the Great Recession, mean earnings growth of small firms dropped considerably more than for larger firms while the proportion of job-stayers experiencing earnings cuts increased substantially across all firms  (although slightly more so for job-stayers in small and young firms). This

confirms that independent of age and size, firms during the Great Recession were on average able to substantially reduce labor costs of job stayers.

The results in Figures 9-11 raise a number of interesting questions. The most important take-away remains, however, that all distributions are substantially more disperse than the distributions reported by previous studies based on survey data, with 25 to 30 percent of job stayers experiencing a reduction in annual earnings in an average year – a proportion that increases substantially during the Great Recession – and never more than 10% of job stayers experiencing zero change in annual earnings.


### 5.3   Firm-specific earnings change distributions

We now dig deeper and consider firm-specific earnings change distributions. Specifically, for each firm $j$ with job-stayers in a given two-year period $t-1$ to $t$, we compute the asymmetry statistics $\gamma_{jt}$, $\eta_{jt}$, $\zeta_{jt}$ associated with the firm's earnings change distribution and use the statistics to examine whether our asymmetry statistics are systematically related to firm characteristics and business cycle conditions.

An important issue when looking at firm-specific earnings change distribution is size. The smaller a firm and the fewer job-stayers it has, the sparser its earnings change distribution and therefore, the more noisy its asymmetry statistics.[28] This issue does not vanish with number of firms. A second issue is that our asymmetry statistics are only well-defined if the median earnings change is positive – a condition that is not satisfied for about 10% of all firms in the 30-state sample. For the firm-specific earnings change distributions, we therefore restrict the sample to firms with positive median earnings changes and at least 50 job-stayers in a given year (Sample 3).

Table 2 (third column) reports the average asymmetry statistics of the resulting annual earnings change distributions pooled over all years. Consistent with our results according to firm size above, the excess spike at zero for Sample 3 is considerably smaller than in Sample 2 that includes all firms. At the same time, Sample 3 has larger missing mass left of zero and larger excess mass right of zero than Sample 2.

We regress $\gamma_{jt}$, $\eta_{jt}$ and $\zeta_{jt}$ on firm-specific variables and time-dummies. Table 4 reports the results for the excess zero spike statistic $\eta_{jt}$. Future versions of the paper will report regression results for other statistics as well. Consistent with the above results, there is a strong negative relationship with firm size as measured by the number of job-stayers of a firm; and excess zero spikes decline on average during the Great Recession years 2008 and 2009. Excess zero spikes increase on average in 2010 and 2011. These results are robust to including industry and state fixed effects and indicate that the drop in the excess zero spike in the pooled sample of all firms (Sample 2) during the Great Recession was not driven by compositional changes.

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[28] For example, in a firm with one job-stayer, the proportion of earnings cuts is either zero or one. Or consider the excess zero measure $\eta_{jt}$ for some firm $j$. In our 30-state sample, 40% of all firm-year observations simply have no zero earnings changes simply because the number of job-stayers in these firms is very small.

## 6  The employment consequences of DWR

To be completed.


## 7  Conclusions

To be completed.

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# Figures and Tables

**Figure 1: Distributional statistics**
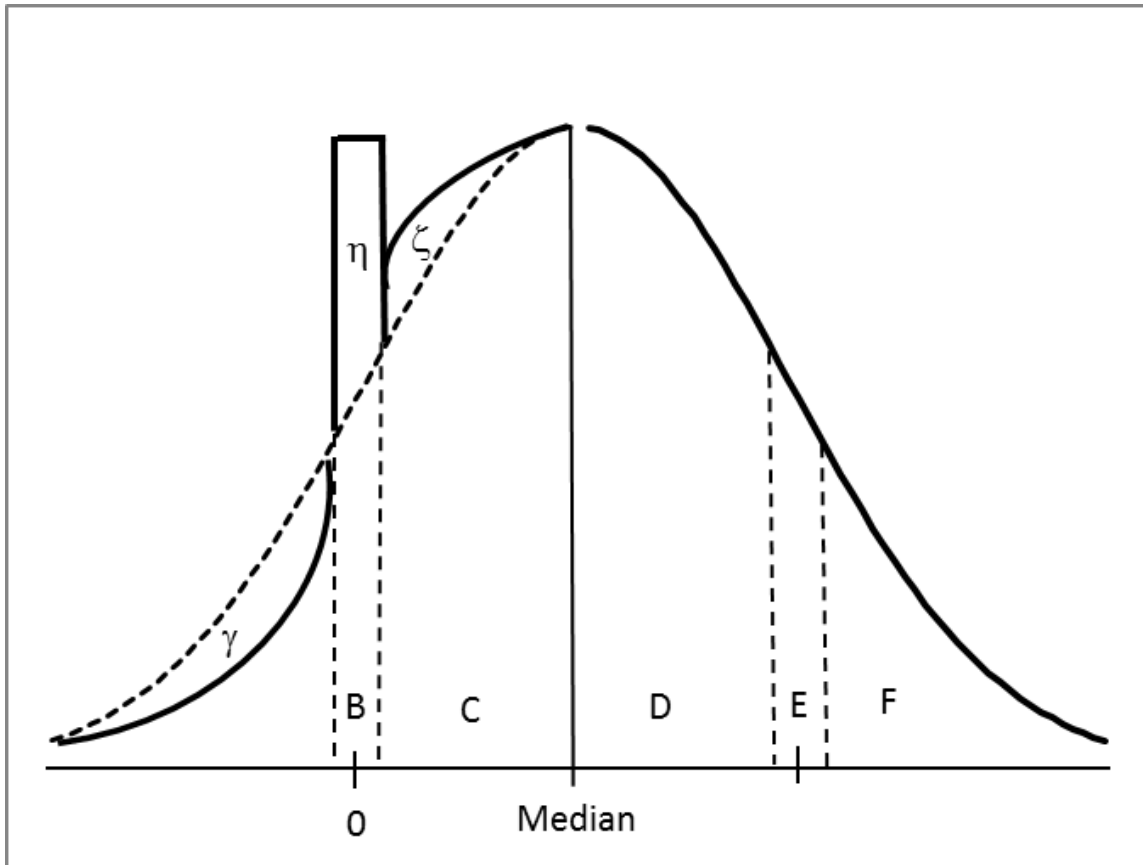
**Figure 2: Distributions of hourly wage changes, 3-state sample, 2010-2011**

**A: 4-quarter hourly wage changes**
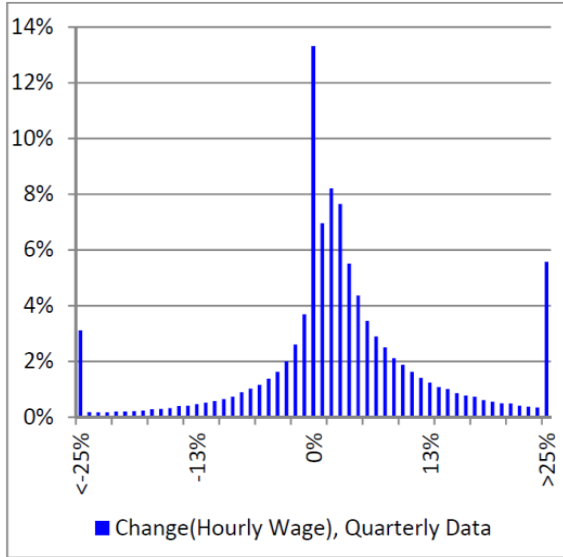


**B: Year-to-year hourly wage changes**
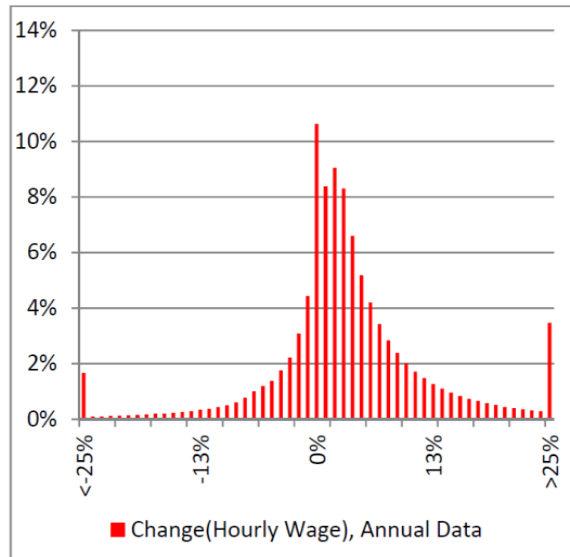


**C: Comparison of 4-quarter and year-to-year hourly wage changes**
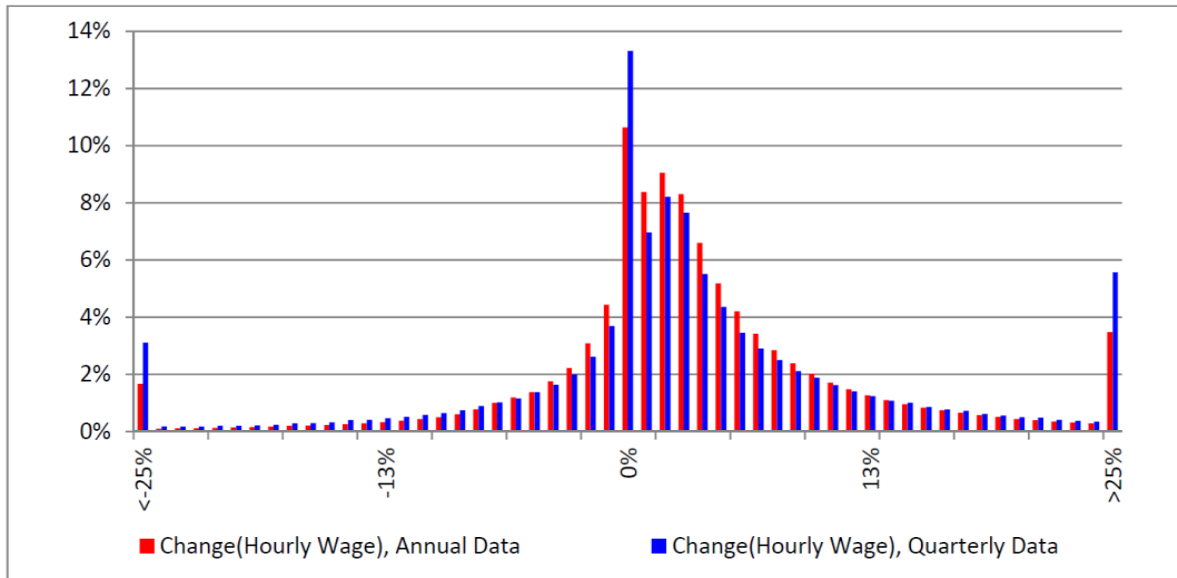
**Figure 3: Distribution of year-to-year Hourly Wage and Annual Earnings Changes 3-state sample, 2010-2011**
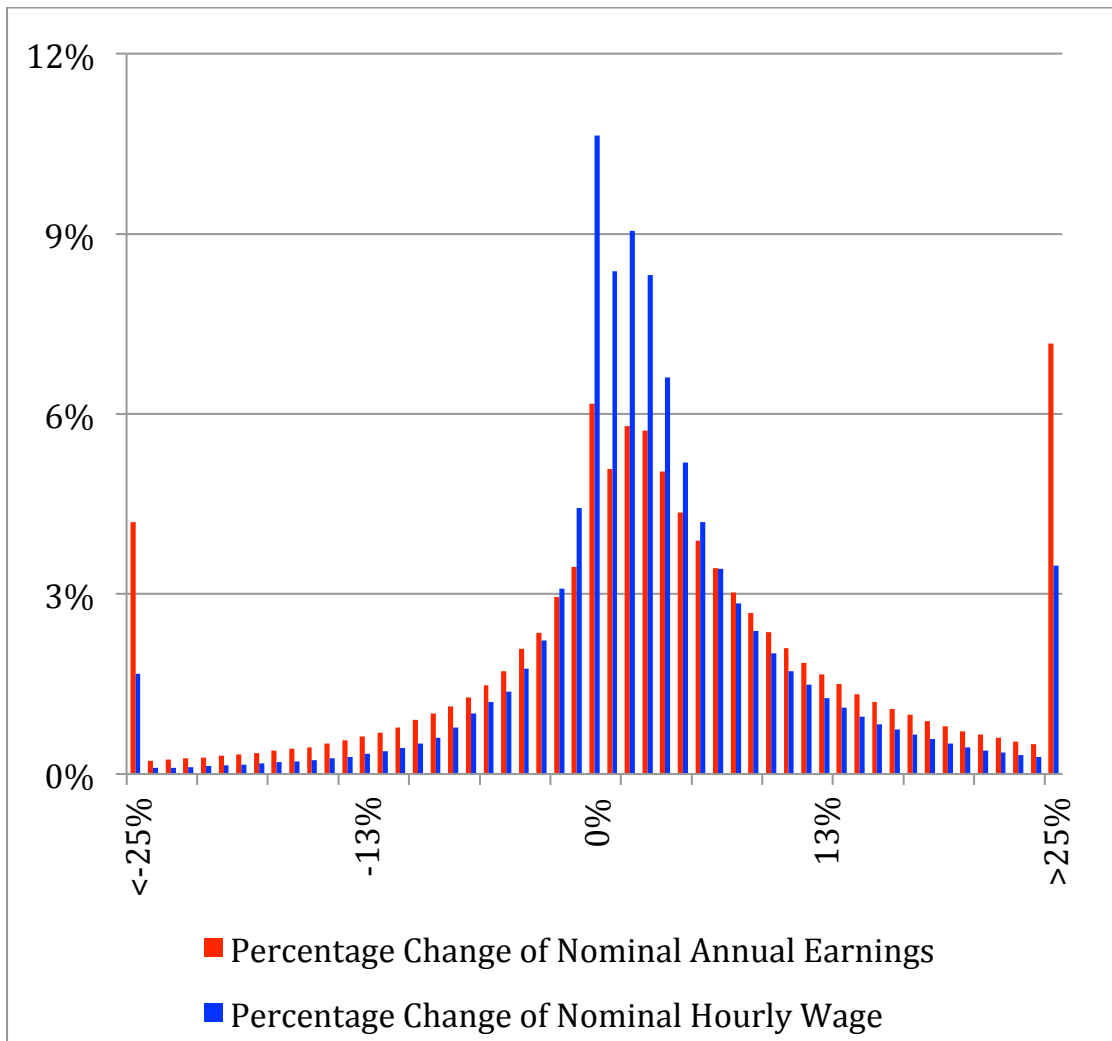
**Figure 4:** **Histogram of firms according to proportion of job-stayers with year-to-year hourly wage cuts, respectively earnings cuts, 3-state sample 2010-2011**



Legend:
- ■ Proportion of Job Stayers in the Firm with Nominal Annual Earnings Cut
- ▣ Proportion of Job Stayers in the Firm with Nominal Hourly Wage Cut
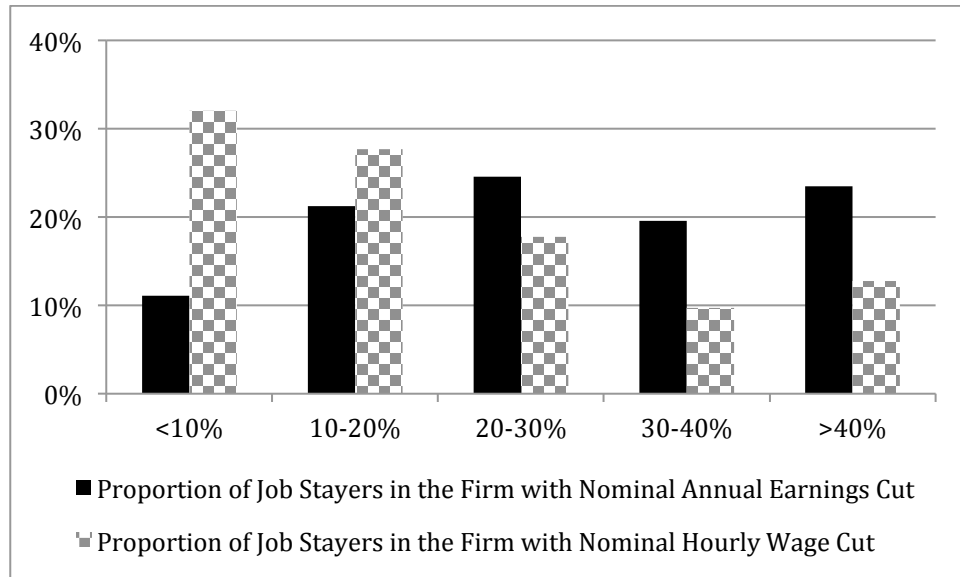
**Figure 5:** **Distribution of Annual Earnings Changes, Hourly wage changes and Hours Changes for job-stayers in MN, RI, and WA for 2010-2011**
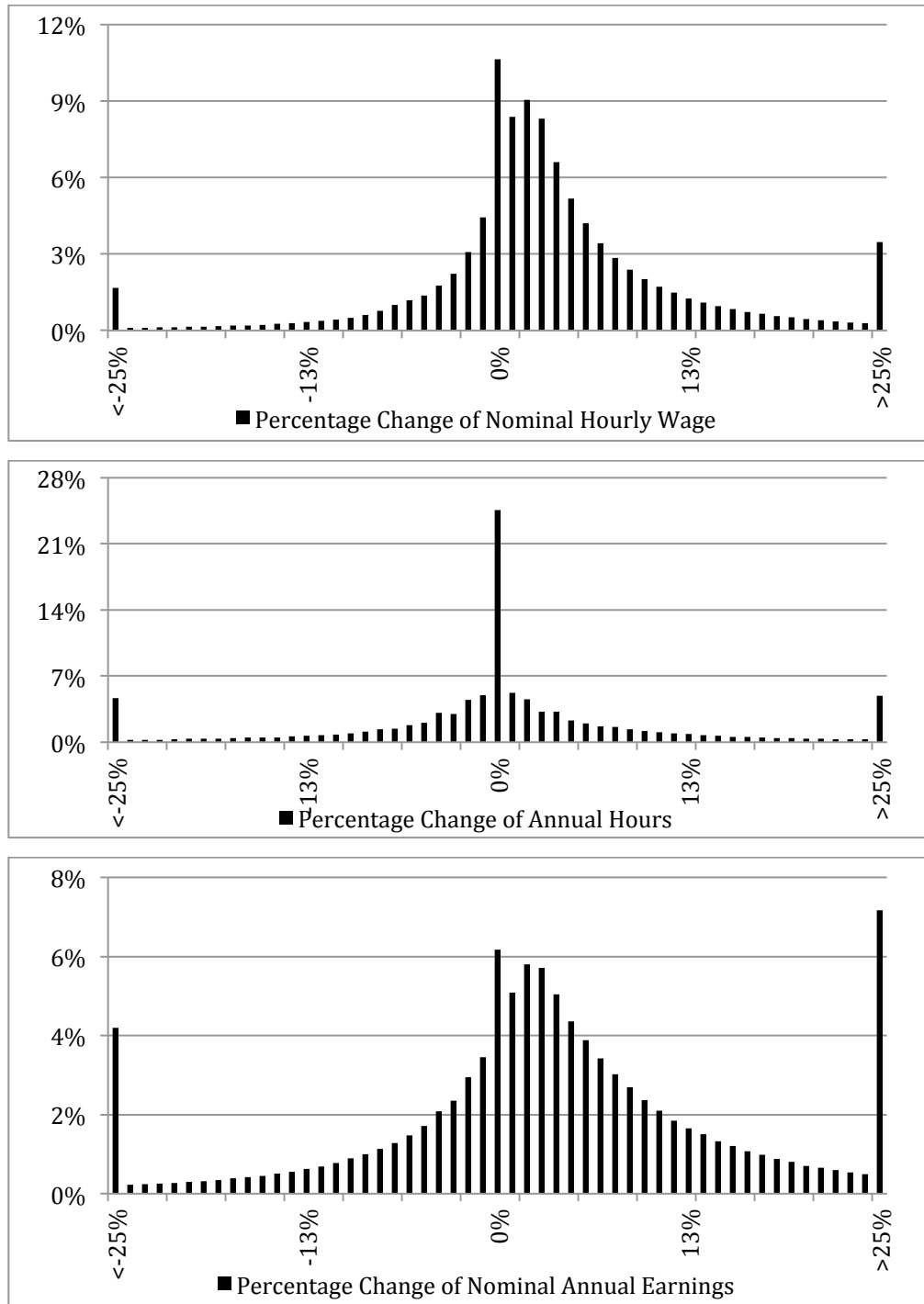
**Figure 6: Decomposition of Annual Earnings Changes into Hourly wage changes and changes in hours for job-stayers in MN, RI, and WA for 2010-2011**
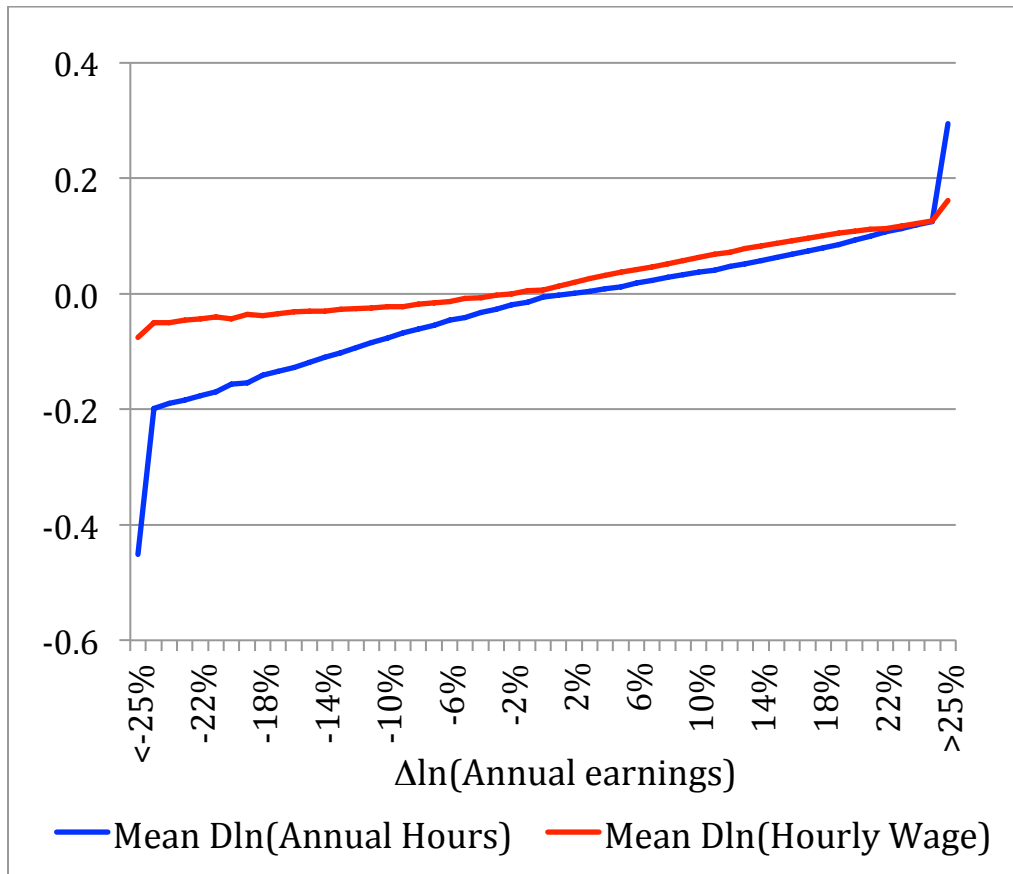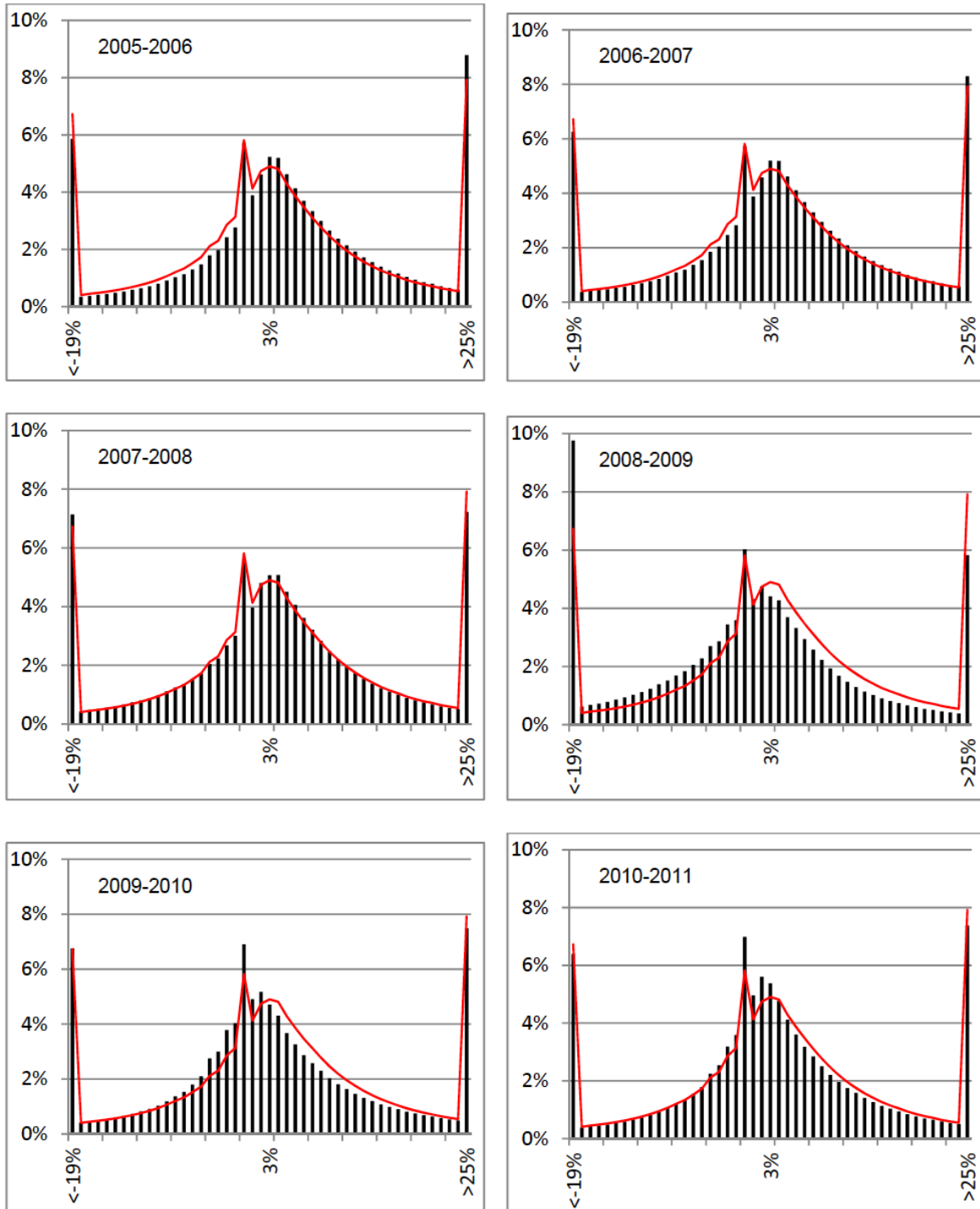
**Figure 7: Nominal earnings change distribution, all job-stayers in 30-state sample, by year**
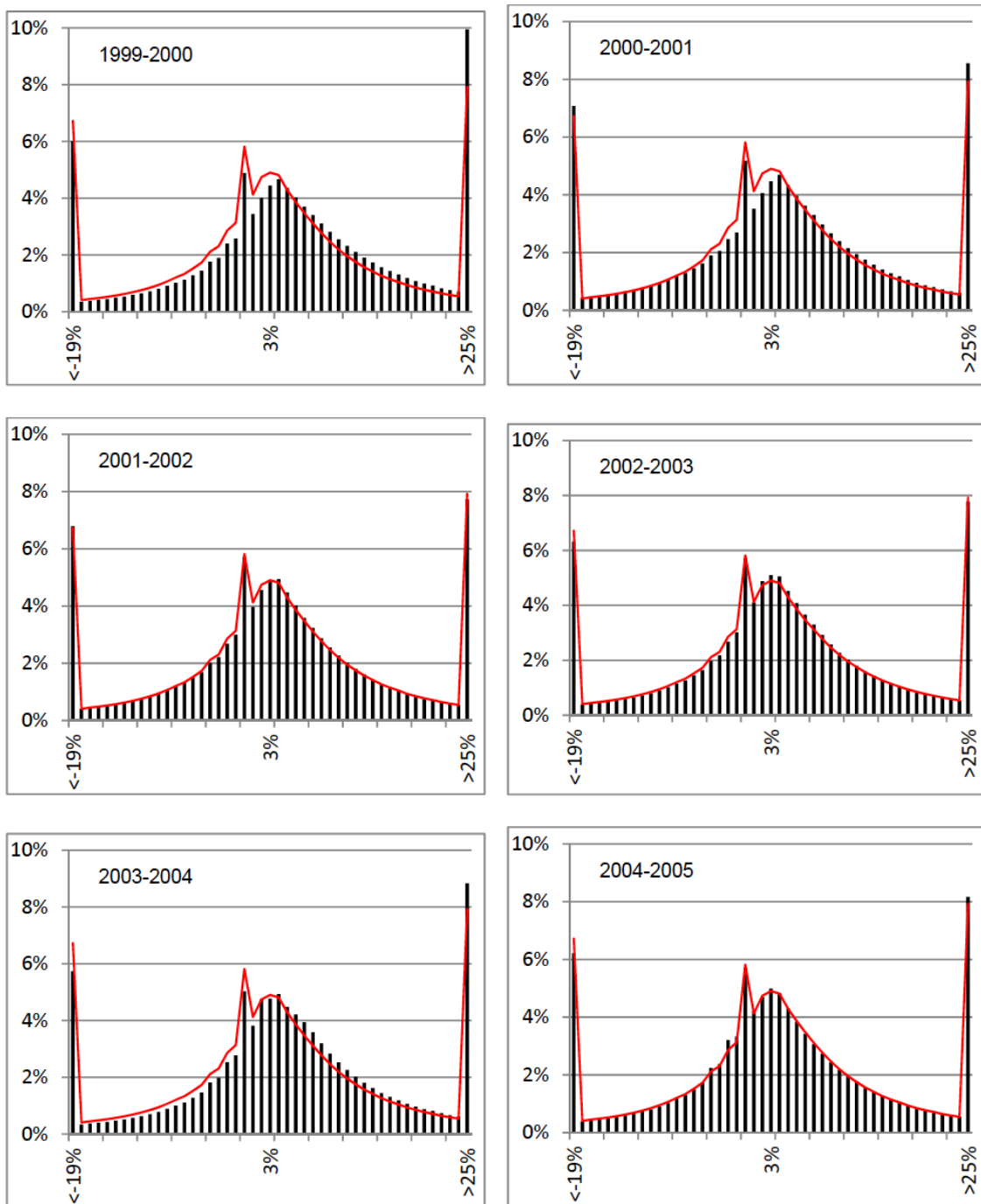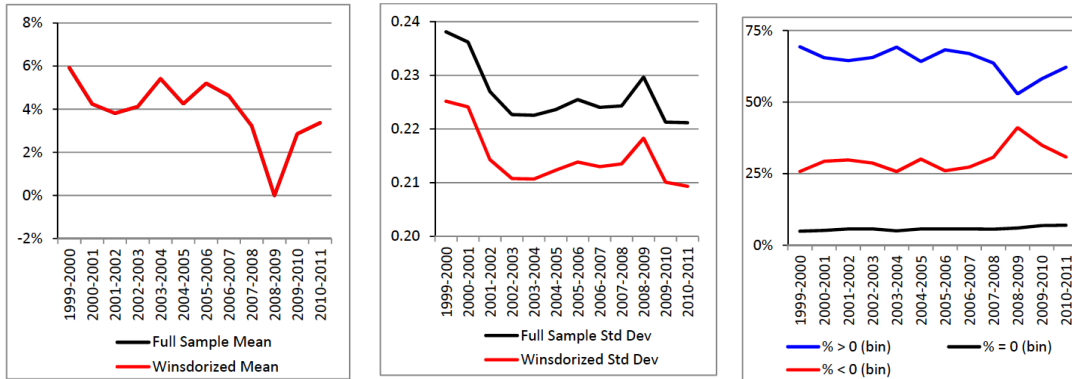


Notes: Red line is kernel estimate of aggregate earnings change distribution pooled over all years
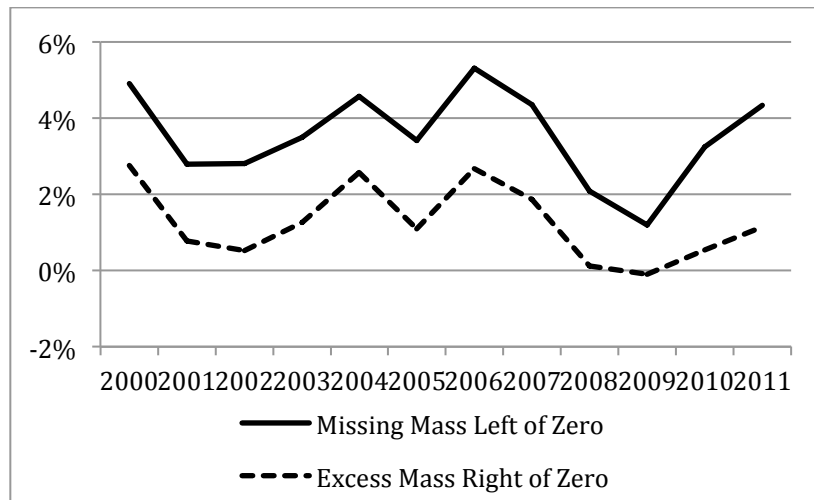
**Figure 7 continued**

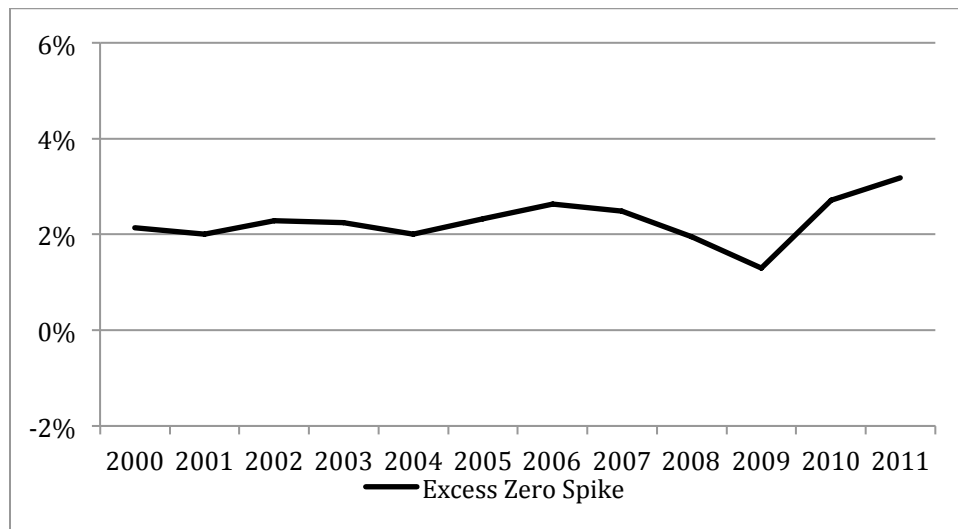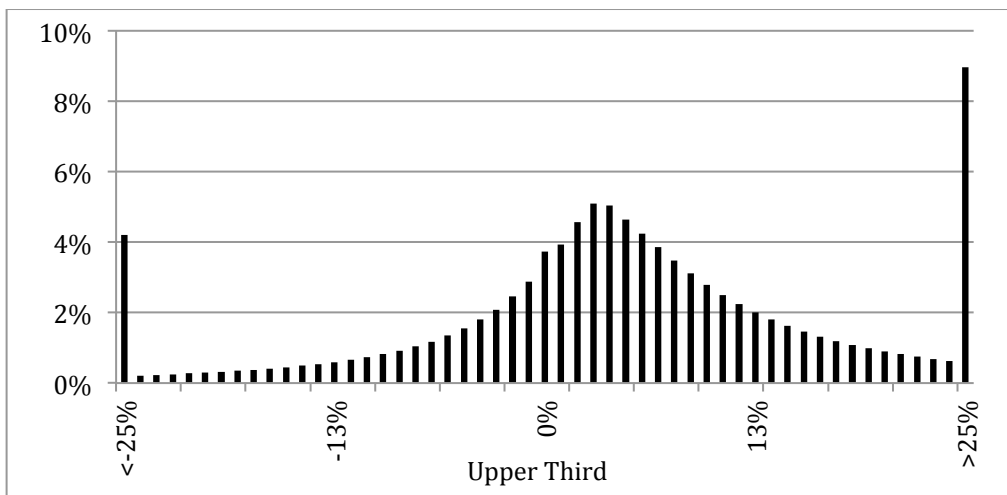**Figure 8: Distributional statistics by year; all job-stayers in 30-state sample**

Panel A
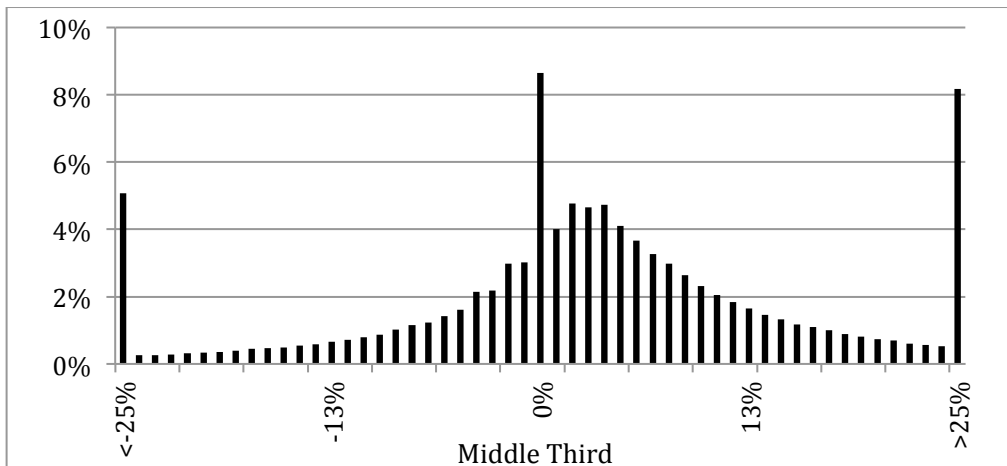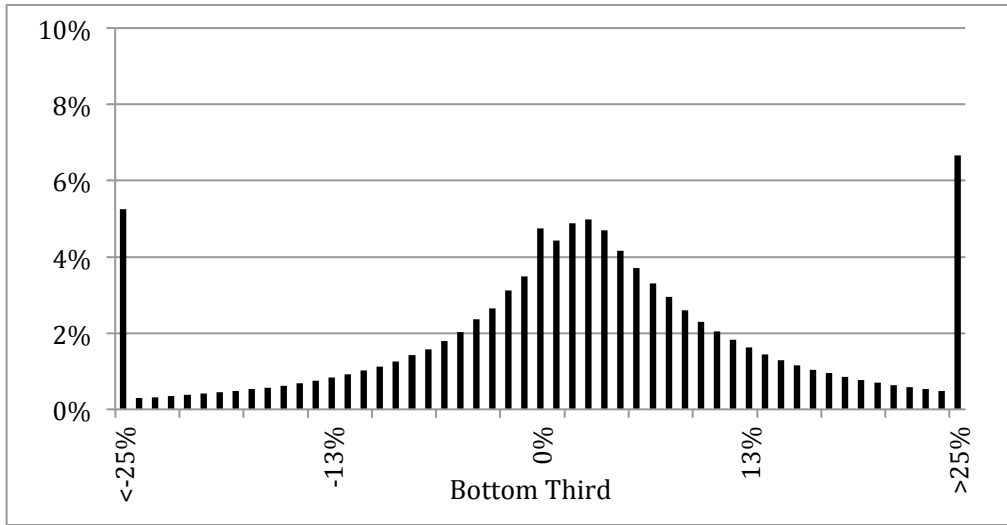


Panel B



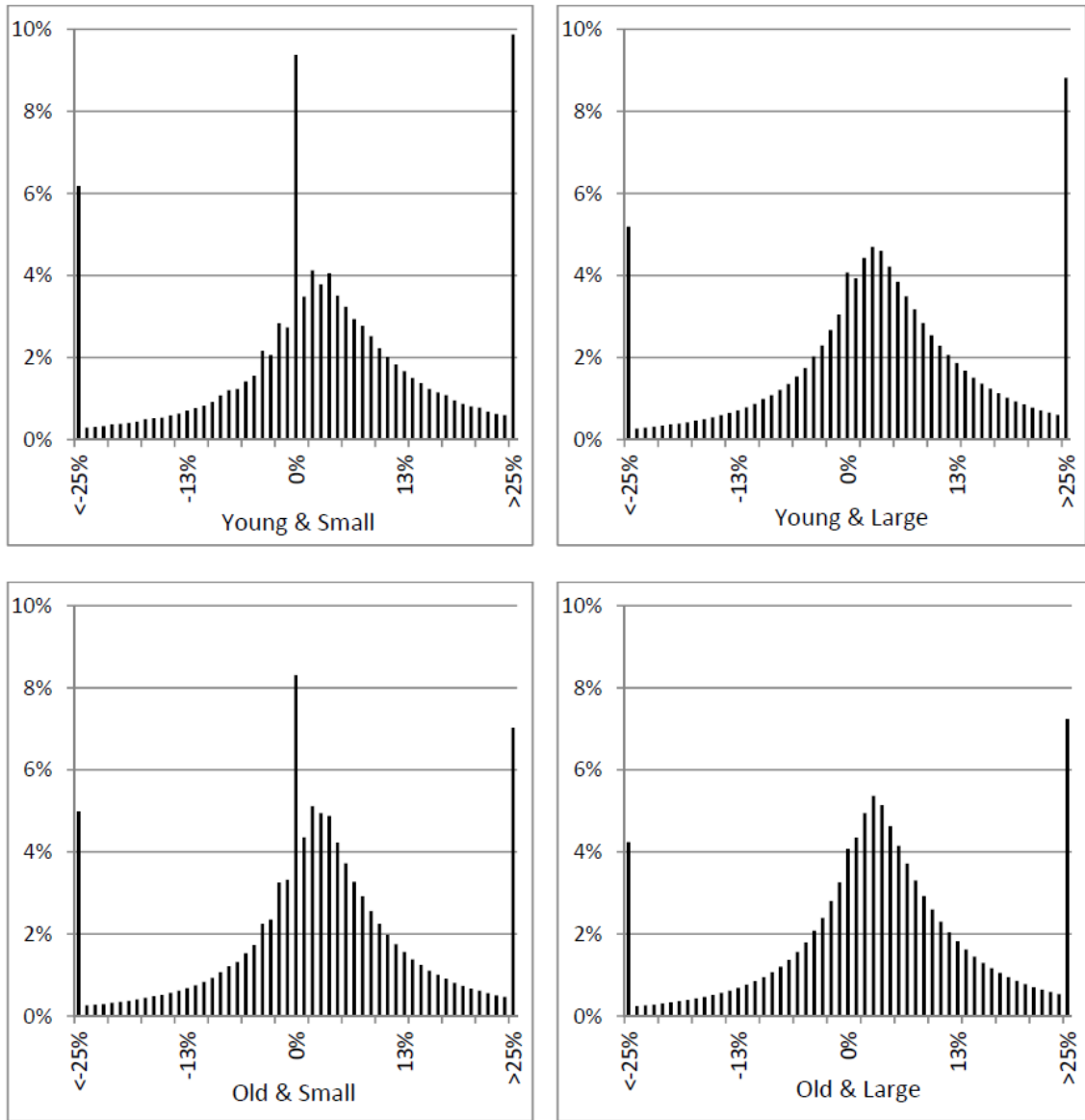Panel C

**Figure 9: Distribution of Annual Earnings Changes, by Firm Employment Growth**

**Figure 10: Distribution of Annual Earnings Changes, by Firm Age and Size**



Young firms are defined as those <20 years old; old firms are defined as those ≥20 years old.
Small firms are defined as those with <100 employees; large firms are defined as those ≥100 employees.

**Figure 11: Distribution of Annual Earnings Changes, by Firm Age and Size**
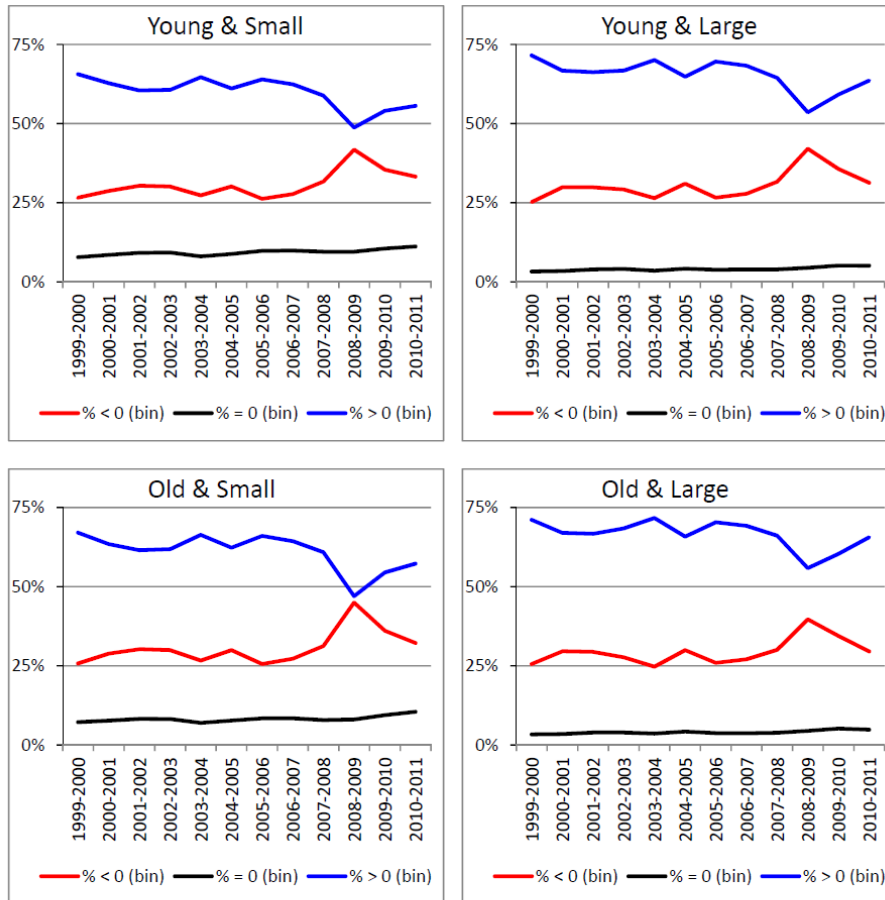
**Panel A**
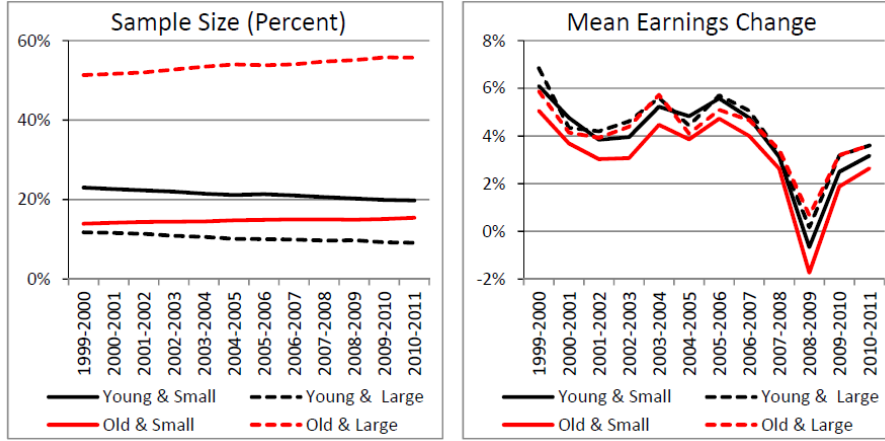


**Panel B**



Young firms are defined as those <20 years old; old firms are defined as those ≥20 years old.
Small firms are defined as those with <100 employees; large firms are defined as those ≥100 employees.

**Table 1: Descriptive statistics**

| | Sample 1 All firms with job-stayers 3 States | Sample 2 All firms with job-stayers 30 states | Sample 3 [1] Select firms with job-stayers 30 states |
|---|---|---|---|
| Years | 2010 - 2011 | 1999 – 2000 to 2010 – 2011 | 1999 – 2000 to 2010 – 2011 |
| # Job stayers (average per year) | 2.3 million | 30.5 million | 15.8 million |
| # Firms (average per year) | 185 thousand | 2,487 thousand | 66 thousand |
| # Job stayers per firm (avg per yr) | 12.4 | 12.3 | 238.3 |
| Real annual earnings last year | 53,047 | 49,079 | 51,260 |
| Real annual earnings current year | 55,685 | 51,347 | 54,722 |
| Annual hours last year | 1818 | | |
| Annual hours current year | 1828 | | |
| Nominal hourly wage last year | 28.78 | | |
| Nominal hourly wage current year | 29.80 | | |
| $\Delta$LN(annual earnings) | .0383 | .0381 | .0536 |
| $\Delta$LN(annual hours) | .0037 | | |
| $\Delta$LN(hourly wage) | .0346 | | |
| Individual-level means: | | | |
|   Worker age | 44.63 | 44.01 | 43.63 |
|   Worker gender (1=female) | 0.49 | 0.47 | 0.49 |
|   Worker tenure (# quarters) [2] | 26.19 | 25.41 | 25.54 |
| Firm-level means: | | | |
|   Avg. worker age | 45.57 | 45.35 | 43.75 |
|   Avg. worker gender (1=female) | 0.51 | 0.47 | 0.46 |
|   Avg. worker tenure (# qtrs) [2] | 24.30 | 23.94 | 25.48 |
| Firm level means: | | | |
|   Firm employment last year | 21.27 | 22.51 | 403.8 |
|   Firm employment current year | 21.66 | 22.69 | 409.9 |
|   Employment change (rate) | 0.75% | 0.19% | 1.53% |
|   Job creation (rate) [3] | 6.56% | 6.92% | 4.77% |
|   Job destruction (rate) | 5.81% | 6.73% | 3.24% |

[1] Sample 3 starts with Sample 2 (job stayers in 30 states), and restricts to firms with at least 50 job stayers and firms with a positive median earnings change.

[2] Tenure is reported for 2011 only. Job durations in 2011 that are left censored at 1996:Q2 are assigned a tenure of 54 quarters.

[3] Job creation and job destruction are defined as in Davis, Haltiwanger, and Schuh (1996), using average employment in the denominator. These rates are between -2 and 2. Note that job creation minus job destruction equals net employment change.

**Table 2:  Summary statistics of hourly wage change and earnings change distributions**

| | **Sample 1** All firms with job-stayers 3 States | **Sample 2** All firms with job-stayers 30 states | **Sample 3**[1] Select firms with job-stayers 30 states |
|---|---|---|---|
| **4-quarter hourly wage changes** | | | |
| Δ(hourly wage) ≤ -.5% | 23.54% | | |
| Δ(hourly wage) -.5% < 0 < .5% | 13.32% | | |
| Δ(hourly wage) ≥ .5% | 63.15% | | |
| Missing mass left of zero [2] | 8.19% | | |
| Spike at zero [3] | 8.48% | | |
| Excess mass right of zero [4] | 0.42% | | |
| **Year-to-year hourly wage changes** | | | |
| Δ(hourly wage) ≤ -.5% | 21.88% | | |
| Δ(hourly wage) -.5% < 0 < .5% | 10.64% | | |
| Δ(hourly wage) ≥ .5% | 67.48% | | |
| Missing mass left of zero [2] | 8.02% | | |
| Spike at zero [3] | 5.47% | | |
| Excess mass right of zero [4] | 2.56% | | |
| **Year-to-year hours changes** | | | |
| Δ(annual hours) ≤ -.5% | 35.77% | | |
| Δ(annual hours) -.5% < 0 < .5% | 24.56% | | |
| Δ(annual hours) ≥ .5% | 39.67% | | |
| **Year-to-year earnings changes** | | | |
| Δ(annual earnings) ≤ -.5% | 28.89% | 30.31% | 24.96% |
| Δ(annual earnings) -.5% < 0 < .5% | 6.17% | 5.82% | 3.95% |
| Δ(annual earnings) ≥ .5% | 64.93% | 63.87% | 71.09% |
| Missing mass left of zero [2] | 4.78% | 3.22% | 5.00% |
| Spike at zero [3] | 2.47% | 2.37% | 0.78% |
| Excess mass right of zero [4] | 2.31% | 0.85% | 4.22% |

[1]  Sample 3 starts with Sample 2 (job stayers in 30 states), and restricts to firms with at least 50 job stayers and firms with a positive median earnings change.

[2]  Missing mass left of zero is defined as $\gamma = 1 - F(2\times median + 0.005) - F(-0.005)$.  See Figure 1.

[3]  Spike at zero is defined as $\eta = [F(0.005) - F(-0.005)] - [F(2\times median + 0.005) - F(2\times median - 0.005)]$.  See Figure 1.

[4]  Excess mass right of zero is defined as $\zeta = [0.5 - F(0.005)] - [F(2\times median - 0.005) - 0.5)]$.  See Figure 1.

## Table 3a: Year-to-year Annual Hours Change Regressions
### 3-state sample, 2010-2011

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Δln(Annual Earnings) | .5796 *<br>(.0010) | | | |
| Δln(Annual Earnings) <0 | | .7262 *<br>(.0020) | .7260 *<br>(.0021) | .7602 *<br>(.0018) |
| Δln(Annual Earnings) ≥0 | | .4831 *<br>(.0015) | .5015 *<br>(.0016) | .5043 *<br>(.0014) |
| | | | | |
| Demographic Controls [2] | No | No | Yes | No |
| Firm Controls [3] | No | No | Yes | No |
| Firm Fixed Effects [4] | No | No | No | Yes |
| | | | | |
| R-Squared | .1488 | .1516 | .1549 | .1956 |

[1]  Dependent variable is Δln(Annual Hours); mean=.0016.  Standard errors in parentheses.  * implies statistically different from zero at the 5% level of significance.  Sample size is 2.0 million job stayers. Observations with Δln(Annual Earnings) <-.25 or >.25 are not included in regressions.

[2]  Demographic controls are age, gender, education, and tenure.

[3]  Firm controls are firm size, firm age, and 19 industry dummies.

[4]  There are 171,000 firms.

**Table 3b:  Year-to-Year Nominal Hourly Wage Change Regressions**
            **3-state sample, 2010-2011**

|  | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Δln(Annual Earnings) | .4204 * | | | |
|  | (.0010) | | | |
| Δln(Annual Earnings) <0 | | .2738 * | .2740 * | .2398 * |
|  | | (.0020) | (.0021) | (.0018) |
| Δln(Annual Earnings) ≥0 | | .5169 * | .4985 * | .4957 * |
|  | | (.0015) | (.0016) | (.0014) |
|  | | | | |
| Demographic Controls [2] | No | No | Yes | No |
| Firm Controls [3] | No | No | Yes | No |
| Firm Fixed Effects [4] | No | No | No | Yes |
|  | | | | |
| R-Squared | .0842 | .0872 | .0908 | .0958 |

[1]  Dependent variable is Δln(Annual Hours); mean=.0294.  Standard errors in parentheses.  * implies
   statistically different from zero at the 5% level of significance.  Sample size is 2.0 million job stayers.
   Observations with Δln(Annual Earnings) <-.25 or >.25 are not included in regressions.

[2]  Demographic controls are age, gender, education, and tenure.

[3]  Firm controls are firm size, firm age, and 19 industry dummies.

[4]  There are 171,000 firms.

**Table 4: Predictive regressions for excess zero spike for 30-state sample, all years for firms with 50 job-stayers or more and positive median earnings changes**

| Model | 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|---|
| Firm is less than | | -0.002 | -0.010 | -0.002 | -0.014 |
| 11 years old | | 0.010 | 0.011 | 0.011 | 0.011 |
| Firm has less than | | **0.523** | **0.535** | **0.572** | **0.567** |
| 500 employees | | 0.008 | 0.008 | 0.008 | 0.009 |
| Firm has less than | | **0.099** | **0.100** | **0.145** | **0.139** |
| 5000 employees | | 0.010 | 0.010 | 0.010 | 0.010 |
| Average stayer | | | **-0.001** | 0.000 | 0.000 |
| tenure | | | 0.000 | 0.000 | 0.000 |
| Average stayer | | | **-0.003** | **0.005** | **0.006** |
| age | | | 0.001 | 0.001 | 0.001 |
| Share stayers | | | **-0.333** | **-0.258** | **-0.239** |
| that are female | | | 0.012 | 0.018 | 0.018 |
| yr2000 | **-0.127** | **-0.127** | **-0.077** | **-0.114** | **-0.117** |
| (relative to 05-06) | 0.016 | 0.016 | 0.019 | 0.019 | 0.019 |
| yr2001 | **-0.184** | **-0.181** | **-0.148** | **-0.170** | **-0.172** |
| | 0.016 | 0.016 | 0.018 | 0.018 | 0.018 |
| yr2002 | **-0.064** | **-0.064** | **-0.044** | **-0.051** | **-0.050** |
| | 0.016 | 0.016 | 0.016 | 0.016 | 0.016 |
| yr2003 | **-0.049** | **-0.048** | **-0.038** | **-0.040** | **-0.040** |
| | 0.016 | 0.016 | 0.016 | 0.016 | 0.016 |
| yr2004 | **-0.145** | **-0.145** | **-0.141** | **-0.141** | **-0.141** |
| | 0.015 | 0.015 | 0.015 | 0.015 | 0.015 |
| yr2005 | **0.128** | **0.127** | **0.127** | **0.126** | **0.126** |
| | 0.015 | 0.015 | 0.015 | 0.015 | 0.015 |
| yr2007 | **-0.058** | **-0.056** | **-0.056** | **-0.054** | **-0.055** |
| | 0.015 | 0.015 | 0.015 | 0.015 | 0.015 |
| yr2008 | **-0.219** | **-0.214** | **-0.211** | **-0.203** | **-0.205** |
| | 0.015 | 0.015 | 0.015 | 0.015 | 0.015 |
| yr2009 | **-0.258** | **-0.247** | **-0.234** | **-0.225** | **-0.226** |
| | 0.016 | 0.016 | 0.016 | 0.016 | 0.016 |
| yr2010 | **0.412** | **0.414** | **0.410** | **0.412** | **0.410** |
| | 0.015 | 0.015 | 0.015 | 0.015 | 0.015 |
| yr2011 | **0.380** | **0.388** | **0.388** | **0.383** | **0.379** |
| | 0.015 | 0.015 | 0.015 | 0.015 | 0.015 |
| Includes industry | | | | X | X |
| fixed effects | | | | | |
| Includes state | | | | | X |
| fixed effects | | | | | |