Downward Wage Rigidity in the U.S.: New Evidence from Worker-Firm Linked Data†

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June 5, 2014

Preliminary and incomplete.
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Abstract

We analyze the extent and consequences of downward wage rigidity in the U.S. using administrative worker-firm linked data from the Longitudinal Employer Household Dynamics (LEHD) program of the U.S. Census Bureau. We start by documenting that firms systematically achieve reductions in labor costs of job stayers by adjusting hours worked downward rather than cutting hourly wages. Changes in total annual pay to the same worker therefore provide a more relevant measure of the extent to which firms can adjust labor costs in lieu of layoffs. Building on this insight, we analyze annual earnings distributions for job stayers. We find that the earnings change distributions exhibit a spike at zero and missing mass to the left of zero. Nevertheless, more than 25% of job stayers experience an earnings cut in any given year. During the Great Recession, this proportion of earnings cuts increases markedly and the earnings change distribution becomes more symmetric. We also show that the distribution of earnings changes varies substantially across firms, with a substantial fraction of firms exhibiting none of the asymmetries typically associated with downward rigidity. This suggests that downward rigidity in labor cost is not a general feature of U.S. firms. At the same time, we cannot infer from these results that downward rigidity is irrelevant. Firms may have disproportionally laid off workers whose labor cost were constrained downward. As we illustrate by means of a simple model, this introduces a potentially important survival bias problem that considerably complicates inference about the employment effects of downward rigidity in labor costs.

† We thank participants at different conferences and seminar presentations for valuable comments. The views expressed in this paper do not necessarily represent the views of the U.S. Census Bureau. All results have been reviewed to ensure that no confidential information is disclosed.
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1 Introduction

There is a long-standing argument in macroeconomics that wages are difficult to adjust downward and that, as a result, firms shed more workers in response to adverse shocks than they would otherwise. Moreover, 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 DWR bind more often.[1]

Motivated by this argument, a large empirical literature has developed that uses micro-data to examine the nature of wage changes of workers remaining with the same firm.[2] While there are some differences in results, studies for the U.S. typically find that there is a noticeable spike at zero in the nominal wage change distribution and missing mass to the left of zero. Moreover, this asymmetry has been found to increase during downturns and low-inflation periods, including the recent Great Recession.[3] These 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 binding constraint into modern macro models to investigate its consequences.[4]

In this paper, we use confidential data from the Longitudinal Employer Household Dynamics (LEHD) program of the U.S. Census Bureau to take a fresh look at the extent and consequences of DWR in the U.S. The LEHD data has several important advantages over other micro datasets previously used to analyze DWR in the U.S., allowing us to address outstanding key issues that have hampered progress of the literature.

First and most crucially, the LEHD data consists of worker-firm linked wage records that employers submit to the unemployment insurance (UI) office of their state. This allows us to analyze wage change distributions at the firm level and to investigate whether firms reduce employment more in response to negative shocks when they are subject to DWR than when they can flexibly adjust wages downward.[5] By contrast, the existing literature investigating DWR for the U.S. is based almost

[1] See for example Tobin (1972) and Akerlof, Dickens and Perry (1996).
[2] Studies for the U.S. include among others 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).
[3] See Fallick, Lettau and Wascher (2011); Daly, Hobjin and Lucking (2012); and Elsby, Shin and Solon (2013).
[4] 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).
[5] While we are to our knowledge the first to use worker-firm linked data to study the importance of DWR in the U.S., recent studies have used similar administrative data with a worker-firm link feature for other countries. The paper closest to us in terms of focus on DWR is Montes and Ehrlich (2014) for German data. Other papers using worker-firm linked data to study somewhat different questions are Castellanos, Gracia-Verdu and Kaplan (2004) for Mexico; and Carneiro, Guimares and Portugal (2011) and Martins, Solon and Thomas (2010) for Portugal.

exclusively on individual survey data from the CPS, the PSID and the SIPP.[6] By definition, this has prevented the literature from studying the extent of DWR at the firm level and whether DWR, if present, actually affects the firm's hiring and firing decisions. Yet, this is the key empirical question that needs answering since from a theoretical point of view, it is unclear whether current wages in long-term relationships should have any effect on employment dynamics.[7]

Second, the administrative nature of the UI wage records means that the LEHD data, while not entirely free from error or noise, is not subject to rounding and recall errors that may severely bias survey-based measures of DWR. Our measures of zero wage changes and wage cuts should therefore be considerably more accurate.[8]

Third, wages reported in the LEHD include all forms of monetary compensation received by workers throughout the year. Aside from employer-covered benefits, the LEHD therefore captures the total cost of a worker to the firm. This is important because different studies find that firms actively use irregular payments such as bonuses to adjust labor costs.[9] In contrast, the earnings concept of the individual survey-based data used by the existing literature is more limited and in certain cases affected by topcoding and missing overtime pay.

Fourth, the LEHD data covers the quasi-totality of private-sector workers in the participating U.S. states. The sheer size of the dataset – millions and millions of observations – means that we can decompose the data in many dimensions without compromising its representativeness. This is especially important when examining DWR during the recent Great Recession, which has affected different states and industries in the U.S. to varying degrees.

To organize our analysis, we begin by presenting a simple illustrative model of optimal employment in an environment with idiosyncratic worker productivity and wage setting that is constrained by DWR. The model highlights two conceptual issues that the literature on DWR has mostly ignored. First, if firms can reduce labor costs of an employee by cutting hours worked, then downward rigidity in the hourly wage may not be much of a binding constraint since the firm's employment decision ultimately depends on labor costs and not the hourly wage. But then, the *distribution of annual earnings changes* provides a better metric than the *distribution of hourly wage changes* to study the employment consequences of DWR.

The model also demonstrates that firms are more likely to lay off workers experiencing a negative productivity shock and workers who are subject to DWR. This implies a "survival bias" that affects in non-trivial ways the wage change

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[6] Notable exceptions are the studies by Lebow, Saks and Wilson (2009) and Fallick, Lettau and Wascher (2011) who use firm-based jobs data from the ECI. As for the individual survey-based data, the ECI is not worker-firm linked, however, and does not allow establishing a link between firm employment changes and measures of DWR.

[7] This point has been made famously by Barro (1977) with regards to nominal wage contracts. The same point applies to modern search models of the labor market. See Pissarides (2009) for a discussion.

[8] See Nickell and Quintini (2003) making a similar point for U.K. administrative data.

[9] See for example Lebow, Saks and Wilson (2009) for the U.S. ; or Babecky et al. (2012) for Europe.

distribution of workers remaining with the same firm; i.e. job stayers. Consider, for example, an environment with no DWR in which the wage change distribution would be symmetric if all workers remained with the firm. Survival bias implies that the wage change distribution *observed in the data* is asymmetric with missing mass left of zero. Alternatively, consider an environment with DWR. Survival bias implies that a potentially large fraction of workers are laid off when the DWR constraint binds and are therefore not observed in the wage change distribution. The issue is that both of these effects become more pronounced during downturns when the firm separates from a larger fraction of its workers. This considerably complicates inference about the employment effects of DWR.

The empirical part of the paper is still work in progress. The results so far can be summarized as follows. First, we examine the hourly wage change distribution and the annual earnings change distribution for job stayers in a subsample of LEHD data where we can observe hours worked. Consistent with the existing literature based on individual survey-data, we find that the distribution of nominal hourly wage changes is characterized by a noticeable spike at zero and missing mass to the left of zero. In comparison, the distribution of annual earnings changes has a smaller spike at zero and a substantially larger fraction of workers experiencing an earnings cut. We then investigate whether firms systematically adjust hours worked as a function of workers' annual earnings changes. Decomposing annual earnings changes into changes in the hourly wage and changes in hours worked, we find that firms on average cut labor earnings primarily through reductions in hours worked whereas earnings increases are on average accounted for more evenly by increases in hours worked and increases in hourly wages. This explains why the distribution of annual earnings changes is more symmetric, displaying a smaller spike at zero and a higher proportion of negative observations than the distribution of hourly wage changes. It also suggests that DWR is indeed a constraint for hourly wage adjustments but that firms react to this constraint by using other means of reducing labor costs – namely adjusting hours. Hence, to analyze the employment effects of DWR, the earnings change distribution is the more relevant metric to consider.

We then examine how the distribution of annual earnings changes over the business cycle and across firms. We document that during the recent Great Recession, the proportion of workers experiencing an earnings cut increased substantially; and the earnings change distributions become on average more symmetric, with fewer workers observed with earnings freezes at the height of the recession (although this share peaks in the weak labor market recovery that followed). 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. This suggests that downward rigidity in labor cost is not a general feature of U.S. firms and that firms were on average able to substantially reduce labor costs of remaining workers during the Great Recession. 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. This would explain why, despite DWR, the distribution of earnings changes observed in the data exhibits a smaller spike at zero and less missing mass left of zero during the Great Recession.

In future versions of the paper, we will 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 illustrated through our simple model, inference about this relationship is complicated by the presence of a potentially important survival bias problem.

The remainder of the paper proceeds as follows. Section 2 develops the illustrative model used to highlight conceptual issues with the empirical literature on DWR. Section 3 describes the LEHD data, the construction of hourly wage changes and earnings changes, and the statistics used to describe asymmetry in the different distributions. Section 4 presents aggregate results. Finally, Section 5 computes firm-specific earnings change distributions.

2 Model

We develop a stylized dynamic model of a firm's optimal employment decision in the presence of DWR to make two points:

1. Survivor bias affects the firm's earnings change distribution that we observe as econometricians in non-trivial ways, thus complicating inference about the employment effects of DWR.
2. The earnings change distribution is a better metric to predict a firm's employment decision than the hourly wage change distribution.

2.1 Environment

Consider a firm that operates for two periods and does not discount the future. In the beginning of the first period, the firm is matched with a continuum of workers. The firm's revenue from employing a given worker net of fixed costs is given by

$$y = zh^{\alpha} - c \quad with \;\; 0 < \alpha < 1$$

where $z > 0$ is the productivity level of the firm-worker match; h the number of hours worked by the worker; and c the fixed cost. The productivity level z is heterogeneous across workers but as we show below, the form of heterogeneity is not important for our results under relatively general assumptions.

Given productivity level z, the worker and the firm negotiate over the real wage rate w. The outcome of this negotiation is described by the following reduced-form wage-setting equation

$$logw = \varphi logz + (1-\varphi)logb$$

where b measures worker-specific and aggregate factors that influence wage setting (e.g. the worker's option of finding another job; unemployment benefits).

At the beginning of the second period, each worker-firm match draws a new productivity level $z'|z$ from a distribution with cumulative density function G. Firms and workers then negotiate a new real wage, denoted by w'. For a fraction $\lambda \in [0,1]$ of the workers – the unconstrained workers – the outcome of this negotiation is described by the same wage setting equation as above; i.e.

$$logw' = \varphi logz' + (1-\varphi)logb$$

For the remaining fraction $1-\lambda$ of the workers – the DNWR constrained workers – negotiations occur over the nominal wage instead of the real wage and are constrained by the restriction that the second-period nominal wage cannot be below the first-period nominal wage. This constraint implies the following wage negotiation outcome[10]

$$logw' = max[\varphi logz' + (1-\varphi)logb, logw - log\pi]$$

where π is the inflation rate between the first and the second period.

In what follows we assume that both b and π are constant and known in advance. This assumption could be easily changed without affecting the points we want to make with the model.

Firms may either flexibly adjust hours of their workers in each period; or hours are fixed at some level $\bar{h}$ (due, for example, to some contractual obligation). Upon observing productivity and wage of a worker, the firm therefore makes following decisions:

- if hours are fixed, the firm decides whether to employ the worker or lay the worker off;

[10] In more detail, the wage setting equation for the DWR-constrained workers is

$$logW' = max[\varphi logP'z' + (1-\varphi)logP'b, logW]$$

where P and P' are price levels and W and W' are the nominal wages in the two periods. Subtracting $logP'$ on both sides and making use of the fact that $logw' = logW' - logP'$ and $logw = logW - logP + logP' - logP'$ with $logP' - logP = log\pi$, we obtain the equation in the text.

- if hours are flexible, the firm decides conditional on the optimal hours decision whether to employ the worker or lay the worker off.

The firm therefore has the right-to-manage both in terms of hours worked and employment. The outside option of not employing a worker is assumed to be zero.

2.2 Wage change distribution and employment decision if hours are fixed

If hours are fixed, the firm's employment decision in the first period is

$$V(z) = max\left[z\bar{h}^{\alpha} - w\bar{h} - c + E(z'|z)\bar{h}^{\alpha} - E(w'|z)\bar{h} - c, 0\right]$$

subject to above wage setting equations. The firm's employment decision in the second period is

$$V'(z', w) = max\left[z'\bar{h}^{\alpha} - w'\bar{h} - c, 0\right]$$

subject to the above wage setting equations. The second-period value *V'(z',w)* depends on w in case the worker is DWR-constrained because then, w' depends on w.

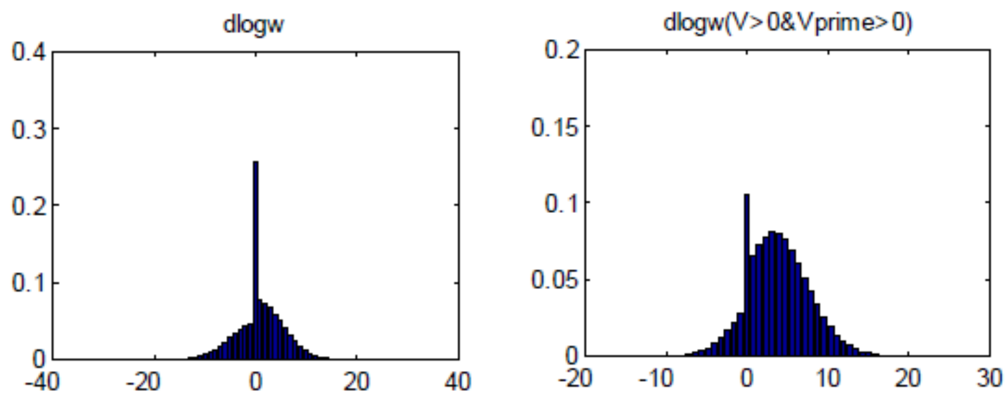
For expository purposes, define a notional wage change distribution $\widetilde{dlogw}$, which would obtain if the firm did not lay off any worker. Absent DWR, it is straightforward to see from the two wage equations that $\widetilde{dlogw}$ inherits the distribution of *dlogz*. As long as the density G from which z' is drawn is independent of the *distribution* of z, the distribution of $\widetilde{dlogw}$ is independent of the assumed first-period heterogeneity of z. With DWR, it should be equally straightforward to see that the notional wage change distribution $\widetilde{dlogw}$ features a spike a zero and missing mass to the left of zero. Moreover, as emphasized by Elsby (2009), if firms are forward-looking (in our model, if firms operated for more than two periods), then the notional wage change distribution under DWR also features compression in wage increases because a higher wage today makes it more likely that the DWR constraint is binding in the future. The larger is the fraction of DNWR-constrained workers, the more important are the these three features.

Now, consider the firm's employment decision. As long as c>0, there exists a threshold $\underline{z}'(\bar{h})$ such that if $z' < \underline{z}'(\bar{h})$, the firm lays off the worker in the second period.[11] Moreover, it is easy to show that this threshold is higher for DWR-constrained workers. Since we do not observe wages of workers who are laid off, this implies that the wage change distribution in the data *dlogw* differs from the notional wage change distribution $\widetilde{dlogw}$. In particular, even absent DWR and assuming that the density G from which z' is drawn is such that *dlogz* is symmetric, the observed wage change distribution will feature missing mass to the left of zero.

[11] A similar threshold exists for the first period but this is not important for us since the distribution of z for job-stayers is not relevant for the distribution of *dlogw'*.

Moreover, with DWR-constrained workers, the observed spike at zero will be smaller than the spike in the notional distribution because some of the workers for which the constraint is binding are laid off. We call this difference between the observed and the notional wage change distribution "survival bias".

To illustrate the survival bias, we assume that firm-worker specific match productivity in the second period is drawn from a lognormal distribution, implying that *dlogz* is symmetric. The following graphs show the results, on the left for the notional wage change distribution and on the right for the actual wage change distribution observed in the data.



Clearly, the observed wage change distribution on the right has a smaller spike at zero than the notional wage change distribution on the left (notice the difference scale of the two graphs).

For this particular example, the observed wage change distribution has the features that the literature typically associates with DWR. Moreover, for this particular example, these features become more pronounced the larger the fraction of DWR-constrained workers. Zero spike and missing mass to the left of zero would therefore predict higher layoffs, as implied by the literature.

The problem is that these predictions do not necessarily hold in general. For example, in a situation of large unexpected negative aggregate shocks, firms subject to a large degree of DWR may lay off so many of the DWR-constrained workers that the observed wage change distribution for these firms has a smaller spike at zero than the distribution of firms with little DWR. In such a situation, the relation between zero spike and employment changes would be inverse.

2.3 Wage change distribution and employment if hours are flexible

If hours are flexible, the firm first solves the following problem $max_h[zh^{\alpha} - wh]$. The resulting optimal hours choice is

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

and the firm's per period profit can be expressed as

$$zh^\alpha - wh - c = \tilde{\alpha}\left(\frac{z}{w}\right)^{1/(1-\alpha)} - c$$

with $\tilde{\alpha} = (\alpha^{-1} - 1)\alpha^{1/(1-\alpha)} > 0$. The firm's employment decision problem in the first period therefore becomes

$$V(z) = max\left[\tilde{\alpha}\left(\frac{z}{w^\alpha}\right)^{1/(1-\alpha)} - c + \tilde{\alpha}E\left(\left(\frac{z'}{w'^\alpha}\right)^{1/(1-\alpha)} |z\right) - c, 0\right]$$

and in the second period

$$V'(z', w) = max\left[\tilde{\alpha}\left(\frac{z'}{w'^\alpha}\right)^{1/(1-\alpha)} - c, 0\right]$$

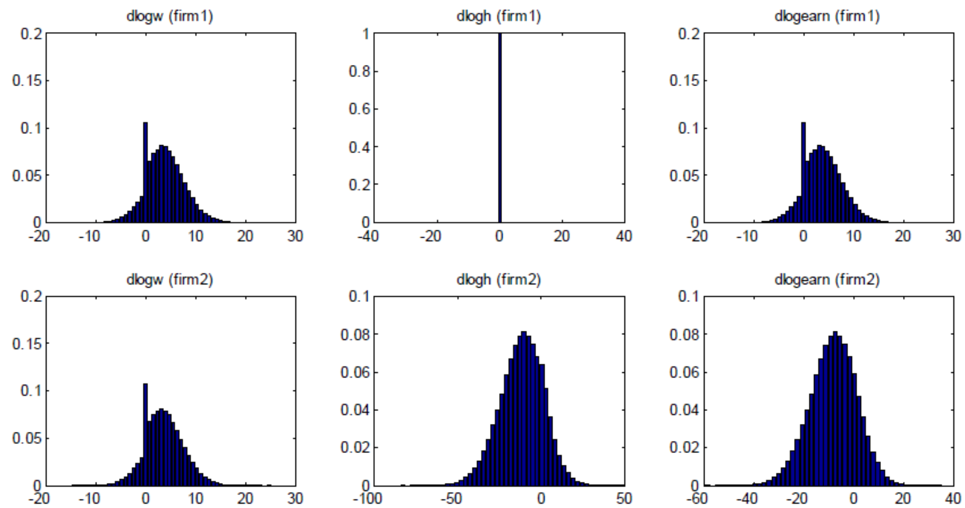
subject to above wage setting equations.

Similar to the fixed-hours case, there is a threshold productivity level $\underline{z}'(h^*)$ such that workers with $z' < \underline{z}'(h^*)$ are laid off in the second period. 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 level $\underline{z}'(\bar{h})$ at which a worker with fixed hours gets laid off is equal or higher than the threshold productivity level $\underline{z}'(h^*)$ at which a worker with flexible hours is laid off.

This 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. The observed hourly wage change distribution of the two firms is exactly the same. However, the employment decision of the two firms can be quite different; in particular, everything else constant, firm 2 will lay off less workers than firm 1. This only gets revealed when looking at the earnings change distribution of the two firms. The following graphs illustrate this.



For firm 1, the earnings change distribution is exactly the same as the hourly wage change distribution since by definition, hours are fixed. For firm 2, by contrast, the earnings change distribution looks very different from the hourly wage change distribution. This is because the firm uses hours worked to systematically adjust earnings. In particular, notice how the hours change distribution of firm 2 is skewed with a larger mass to the left of the median. This reflects that the firm adjusts downward hours worked of workers for which DNWR binds.

3 Data

3.1 The LEHD data

We use data from the Longitudinal Employer-Household Dynamics (LEHD) Program at the U.S. Census Bureau to construct earnings changes and hourly wage changes for continuing workers across firms. The core of this 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.[12]

The linked worker-firm dimension of the LEHD is crucial for our investigation. Additionally, the LEHD has three other important advantages over the survey-based

[12] 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.

datasets historically used to compute wage change distributions for the U.S. First, 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. Second, the LEHD is based on administrative data which, while not entirely free from error or noise, is not subject to rounding and recall errors that plague survey-based measures and may bias statistics on changes in wages and hours worked towards zero. Third, 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.[13] Aside from benefits, the LEHD therefore captures the total cost of a worker to the firm.

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 information on hours paid for each worker. Finally, the characteristics of the LEHD allow us to infer the tenure of workers within a given firm and the total number of employees per firm.

3.2 Construction of annual earnings changes and hourly wage changes

Since the main objective of the paper is to analyze the employment effects of wage rigidity within firms, we focus on changes in annual earnings of workers who remain with the same firm; i.e. job-stayers from hereon. In order to be considered a job-stayer, a worker has to remain with the same firm for at least ten consecutive quarters: the eight quarters for which the year-to-year change in earnings is computed plus the last quarter preceding the first year and the first quarter following the second year. These latter quarters are part of our selection criteria since we need to ensure that workers worked all eight quarters of the two calendar years (we do not want to 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* as their year-to-year change in earnings is potentially affected by the duration of their employment).

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.

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

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				

Notice that our analysis focuses on year-to-year changes in annual earnings even though the LEHD data is available on a quarterly basis. This choice is motivated by two considerations. First, a substantial fraction of workers receive end-of-year bonuses and other irregular payments that are recorded in a particular quarter. While these payments are part of compensation and a potentially important component of labor cost, their timing within the year is unlikely to be relevant for firm employment decisions. Second, firms typically report to the UI system the earnings disbursed during the quarter rather than the earnings accrued. This results in potentially large spikes in the quarter-to-quarter earnings change distribution (imagine partitioning 26 bi-weekly pay periods into four quarters – two quarters will have six pay periods and two pay periods will have seven pay periods). On an annual basis, this pay-period effect disappears and does not appear to affect our non-parametric histogram approach.

For the three states with individual hours information (MN, RI and WA), we compute the average hourly wage rate (called 'hourly wage' henceforth) of a job-stayer as annual earnings divided by annual hours worked. Year-to-year changes are then obtained similarly to earnings, by computing the log difference of the hourly wage, respectively of hours worked, between years *t* and *t-1*.

3.3 Sample and descriptive statistics

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 4th quarter of 2009. Sample 1 therefore consists of annual earnings changes and hourly wage changes of job-stayers in these three states for 2010-2011.[14]

Sample 2 consists of the 30 states with earnings information from 1998:Q2 through 2012:Q2. This yields 12 years of annual earnings change observations for all job-stayers in these states (1999-2000, 2000-2001,…,2010-2011).[15]

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 in Section 5.

[14] The LEHD disclosure rules require a minimum of three states for publication. As such, having only 2010-2011 data is driven by the state of Rhode Island adding hours information to its wage records in 2009:Q4 (Minnesota and Washington have hours data for longer time periods).
[15] We will expand our analysis to include 2011-2012 in future revisions.

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 at least 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 several orders of magnitude larger than the sample size from household surveys such as the CPS or the PSID.

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.

3.4 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%.

In addition to histograms, we quantify key characteristics of the distributions through a set of statistics that capture the type of distributional asymmetry that the literature has typically associated with DWR. Specifically, we consider the following statistics, with $F(\,)$ denoting the cumulative density:

- 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, γ, 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, η, 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 zero, ζ, 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).

The different statistics are closely related to DWR measures used by Card and Hyslop (1997), Kahn (1997) and others in the sense that we compare different parts of the histogram to the left of the median with corresponding parts to the right of the median. If the distribution is symmetric, then all three statistics are zero. Contrary to most of the literature, however, our analysis does not assume that absent downward wage rigidity, the hourly wage change and earnings change distributions are symmetric. Instead, we use the symmetry statistics to compare distributions across time and firms; and relate *differences* in the statistics at the firm level to employment dynamics.

4 Aggregate distributions of hourly wage and earnings changes

4.1 Aggregate distributions for 3-state sample

We use as our starting point the literature on hourly wage change distributions for stayers. Because this is the focus of much of the empirical literature on downward nominal wage rigidity, it is interesting to see how these distributions in the LEHD data compare to those earlier papers, which principally use household data from the U.S. We also in this section contrast the hourly wage change distribution to the annual earnings change distribution.

As discussed in the last section, we only observe hours data in three LEHD states, starting in 2010 (Sample 1). So for this section, all results are for job stayers in those three states in the period 2010-2011. Despite this narrow selection criteria, this sample is still much larger than studies using U.S. household data.

Figure 2 shows the aggregate hourly wage change distribution of job stayers in Sample 1. The distribution exhibits a clear spike at zero of about 10% and pronounced missing mass to the left of zero. At the same time, about 20% of job stayers experience a cut in their hourly wage. In comparison, the distribution of earnings changes is, while similar in overall shape, more symmetric with a markedly lower spike at zero and substantially fatter tails. In particular, almost 30% of job stayers experience a cut in their annual earnings.

The distributions for the 3-state sample look overall quite similar to the ones reported in the existing literature based on U.S. household survey data from the PSID and the CPS (e.g. Kahn, 1997; Card and Hyslop, 1997; Daly, Hobjin and Lucking, 2012; or Elsby, Shin and Solon, 2013). One noticeable difference is that the spike at zero in our distributions is smaller than what is reported, for example, by Elsby, Shin and Solon (2013) for a comparable year based on CPS data (see their Figures 5 and 6). 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 indeed issues for wage change distributions derived from individual survey-based data such as the CPS.[16]

At the same time, our results suggest that the substantial fraction of job-stayers with negative hourly wage changes observed in individual survey-based studies is not the result of measurement error. This is interesting because there exists somewhat of a controversy in the literature about this issue. 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, 1999). On the other hand, the above listed studies based on individual survey data report that wage cuts are more frequent. Akerlof, Dickens and Perry (1996) argue based on a model that this difference is due to measurement error; while Altonji and Devereux (1999), Gottschalk (2005) and Barratieri, Basu and Gottschalk (2014) use econometric techniques to identify these errors and find that after error correction, the extent of wage cuts in the individual survey data is substantially reduced.

To examine this issue 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 3 shows the results.

Consider the first grey and black bar on the very left. These bars indicate that there are 31% of firms in which less than 10% of job stayers experience an hourly wage cut; while in only 11% of firms, less than 10% of job stayers experience an earnings cut. Moving to the right, the histogram shows that the number of firms with more than 20% of its job stayers experiencing an hourly wage cut is relatively small; whereas the opposite is true for earnings cuts. In other words, earnings cuts are a quite common feature across firms. Hourly wage cuts, by contrast, are less frequent but still quite common except for a small fraction of firms. Assuming that administrative data provided by employers are relatively free of measurement error, these results indicate that the econometric methods applied by Altonji and Devereux (1999), Gottschalk (2005), and Barratieri, Basu and Gottschalk (2014) may classify too many negative wage changes as measurement error, therefore exaggerating the extent of DWR that is effectively present in the data.

The marked difference between the hourly wage change distribution and the earnings change distribution in Figure 2 implies that firms adjust hours worked as well as hourly wage rates. To shed more light on the role of this hours adjustment, Figure 4 expands on Figure 2 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

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

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.[17]

Figure 5 decomposes annual earnings changes into changes in hours worked and hourly wage rates; i.e. for each job-stayer *i*, we compute

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

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 worked than by decreases in hourly wages. In contrast, increases in annual earnings are on average accounted for more evenly by increases in both hours worked 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 2a reports the results for the hours change regressions. Table 2b 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 2a and 2b 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.[18] 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 2a). 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 2b).

In sum, the results in Figures 3-5 and Table 2 suggest an interesting new fact about 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 explains why the aggregate distribution of annual earnings changes is more symmetric and displays a smaller spike at zero than the

[17] 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.
[18] We also ran regressions adding the job-stayer's first-year earnings level as a control. None of the results changed noticeably.

distribution of hourly wage changes, with more workers receiving annual earnings losses than hourly wage cuts.

4.2 Aggregate earnings change distributions for 30-state sample

To examine the question of how the earnings change distribution varies over the business cycle, we use the 30-state sample for which we have annual earnings changes from 1999-2000 to 2010-2011 (Sample 2). As we illustrate in the previous section, the earnings change distribution is fatter and more symmetric than the hourly wage change distribution, capturing the greater flexibility firms appear to have in adjusting hours in lieu of wage changes. Changes in this distribution over time capture both the hours and hourly wage adjustment margins over the business cycle.

Figure 6 plots the annual earnings change distribution of the 30-state sample for each 2-year period, with the superimposed 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, with the mass at zero increasing somewhat in 2009-2010 and 2010-2011.

Table 3 (second column) reports the different asymmetry statistics for the earnings change distribution of the 30-state sample pooled over all years. There is some missing mass right of zero and excess zero spike. Excess mass right of zero is very small. In comparison to the earnings change distribution of the 3-state sample exhibits larger missing mass left of zero and larger excess mass right of zero (Table 3, first column). Most of this difference is due to the fact that the asymmetry statistics of the 30-state-sample pertain to the pooled sample. For 2010-2011, the statistics look more similar.

Figure 7 summarizes the time series of the annual earnings change distribution over time through different statistics. As Panel A shows, the mean earnings change and the proportion of job-stayers experiencing earnings increases drops precipitously in 2007-08 and 2008-09 before recovering, while the proportion of job-stayers experiencing earnings decreases rises markedly during the same period. This confirms the marked left-ward shift of the distribution during the Great Recession seen in Figure 6. 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 again in 2009-2010 and 2010-2011. This confirms that the distribution of annual earnings changes has become more symmetric during the height of the recession and only turned more asymmetric again as the economy emerged from the recession.

The different observations in Figure 7 suggest that firms were on average able to substantially reduce labor costs of job-stayers during the Great Recession and that as a result, DWR was not a generally binding constraint. From this, we cannot conclude, however, that DWR was irrelevant for U.S. labor markets during the Great Recession. In particular, the highlighted changes in the asymmetry of the aggregate earnings change distribution may be driven at least in part by compositional changes. For example, firms that are not subject to DWR may have reduced their workforce by less than firms subject to DWR, thus becoming a more important contributor to the aggregate wage change distribution. Alternatively, firms themselves may have disproportionally laid off workers whose earnings were constrained by DWR. Both of these possibilities would make the observed aggregate earnings change distribution stayers more symmetric. To shed further light on these issues, we need to consider firm-specific earnings change distributions.

5 Firm-specific earnings change distributions

As described above, the linked worker-firm characteristics of the LEHD data allows us to construct firm-specific earnings change distributions. Specifically, for each firm *j* with job-stayers in a given two-year period *t-1* to *t*, we can compute the asymmetry statistics γ_{jt}, η_{jt}, ζ_{jt} associated with the firm's earnings change distribution and use the statistics to address the following questions:

1. Are there large differences in distributional asymmetry across firms?
2. Are the asymmetry statistics systematically related to firm characteristics and business cycle conditions?
3. Are the different asymmetry statistics systematically related to each other at the firm level?

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 less meaningful the proposed asymmetry statistics.[19] 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 3 (third column) reports the average asymmetry statistics of the resulting annual earnings change distributions pooled over all years. Interestingly, the 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. These averages are important to keep in mind when considering the results reported below.

[19] Consider, for example, the zero spike measure η_{jt}. In our 30-state sample, 40% of all firm-year observations simply have no zero earnings changes because the number of job-stayers in these firms is very small.

5.1 Differences in distributional asymmetry across firms

Figures 8 and 9 display the distribution of zero spikes η_j and missing mass γ_j across the firms in Sample 3. The most striking observation from the two figures is the dispersion of these distributions. In particular, only about one third of all firms have a spike at 0 ($\eta_j > 0$); and only about one half of firms have missing mass left of 0 ($\gamma_j > 0$) in their earnings distribution. Moreover, for many of the firms with positive zero spike and missing mass, the asymmetry in their wage change distribution is relatively small. Only about 10% of all firms in the sample have zero spikes at zero in excess of 5% and missing mass in excess of 10%, respectively.

These observations lead to an important conclusion: there are large differences in earnings dynamics across firms with 50 job-stayers or more. A majority of these firms display earnings change distributions that look very different from the ones typically reported in the literature based on aggregate data, and that one would hardly interpret as evidence of DWR.

5.2 Distributional asymmetry across firms and over the business cycle

To examine whether the different asymmetry statistics are systematically related to firm characteristics and business cycle conditions, we regress γ_{jt}, η_{jt} and ζ_{jt} on firm-specific variables and time-dummies.

Table 4 reports the results, for now only for zero spike statistics η_{jt} (regressions for γ_{jt} and ζ_{jt} are to follow). There is a strong negative relationship with firm size, measured by the number of job-stayers. This result is equally present in the pooled sample across all firms where we do not control for firm-specific variables and time-dummies (see Table 2). In sum: the larger the firm, the smaller the spike at zero.

Table 4 also shows that there are strong time effects, with the Great Recession years of 2008 and 2009 being associated with lower (or negative) zero spikes and 2011 being associated with a larger positive zero spikes. This confirms the findings from Figure 6 for the pooled sample above. To confirm, Figure 10 plots the mean zero spike and missing mass across the different firms in Sample 3. The results look quite similar. Hence, the drop in the zero spike during the height of the recession is, at least for larger firms, not driven by compositional changes but instead is a general result across firms.

5.3 Relation between distributional asymmetry statistics

Table 5 reports correlation coefficients between the three distributional asymmetry statistics across firms in Sample 3. Interestingly, there is a strong negative correlation between γ_{jt} and ζ_{jt}; but only a weak positive correlation between γ_{jt} and η_{jt}; and no correlation between η_{jt} and ζ_{jt}.

On the one hand, the negative correlation between missing mass left of zero and excess mass right of zero is consistent with DWR. On the other hand, the absence of strong correlation of these two statistics with zero spike is not what the literature would associate with DWR.

Of course, the absence of strong correlations between η_{jt} and γ_{jt}, respectively ζ_{jt} could be driven at least in part by survival bias. In particular, if firms in distress lay off disproportionally workers whose earnings are constrained by DWR, then this makes the observed distribution of earnings changes more symmetric, thus weakening the link between the different asymmetry statistics. We plan to investigate this possibility in future versions of the paper.

6 Earnings change distributions and employment dynamics

In future versions of the paper, we will relate the different asymmetry statistics of the earnings change distribution to employment dynamics at the firm level. As described above, this task is complicated by the presence of a potentially important survival bias problem.

7 Conclusion

To be added.

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Figures and Tables

Figure 1: Distributional statistics

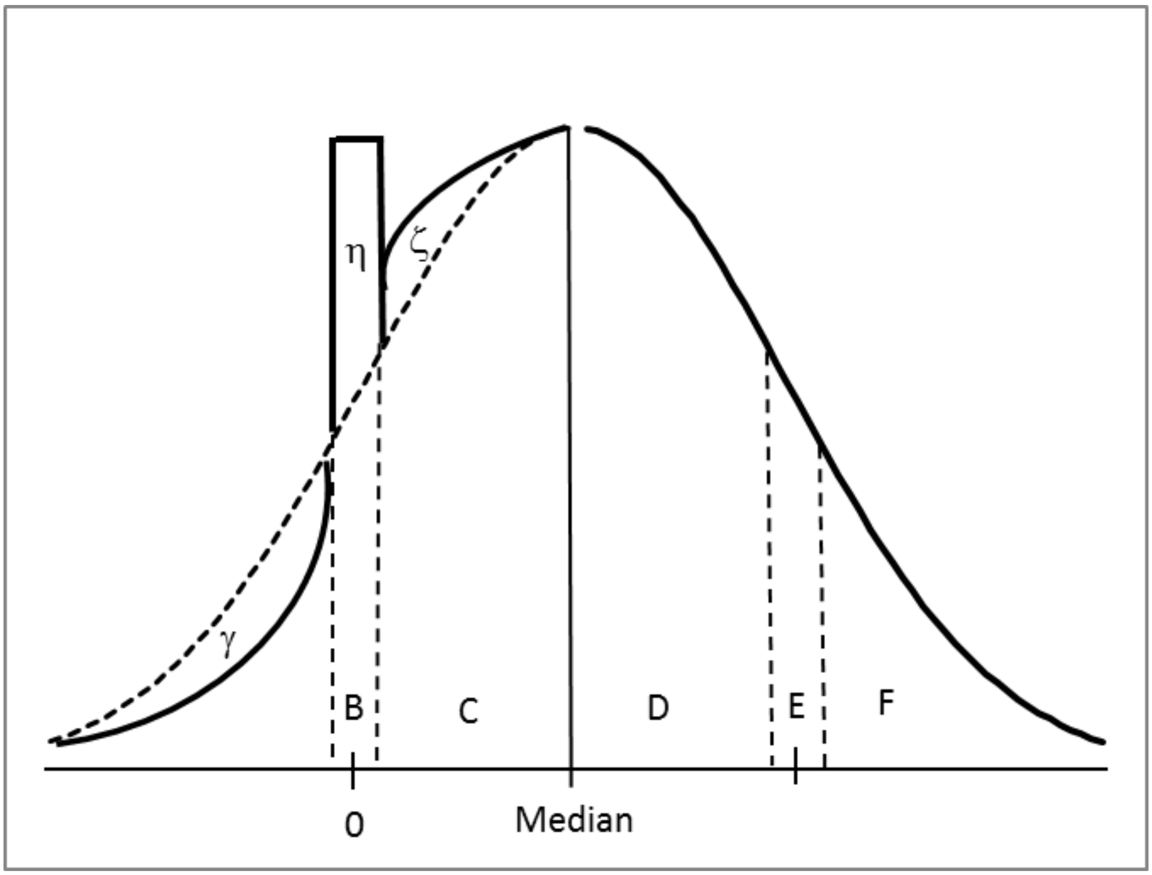
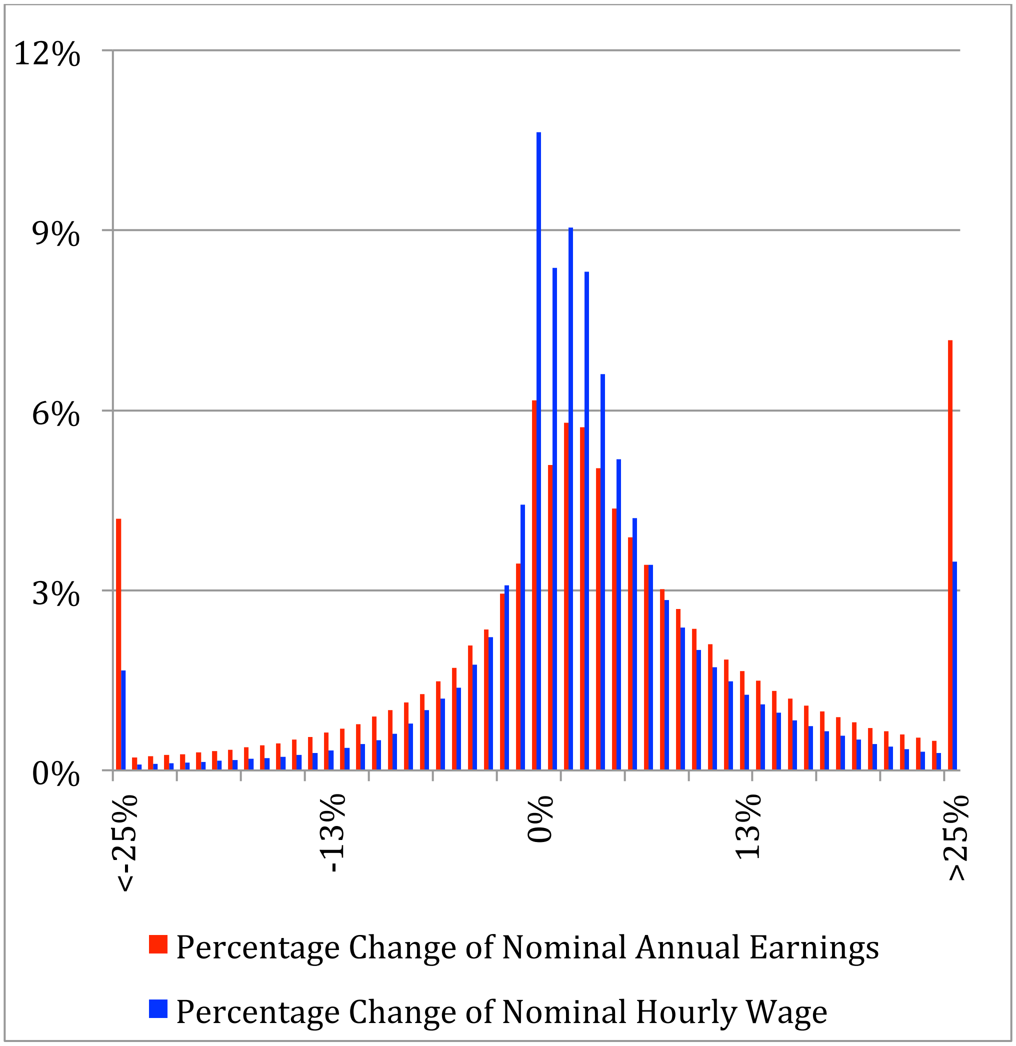


Figure 2: Distribution of Hourly Wage and Annual Earnings Changes



Pooled data for MN, RI, and WA; 2010-2011.

Figure 3: **Histogram of firms according to proportion of job-stayers with hourly wage cuts, respectively earnings cuts, in MN, RI, and WA for 2010-2011**

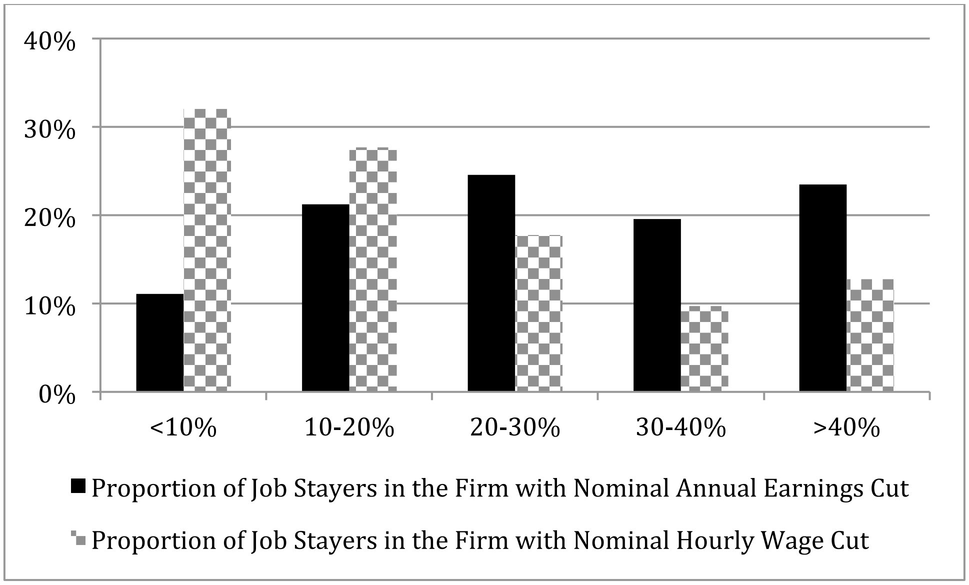


Figure 4: **Distribution of Annual Earnings Changes, Hourly wage changes and Hours Changes for job-stayers in MN, RI, and WA for 2010-2011**

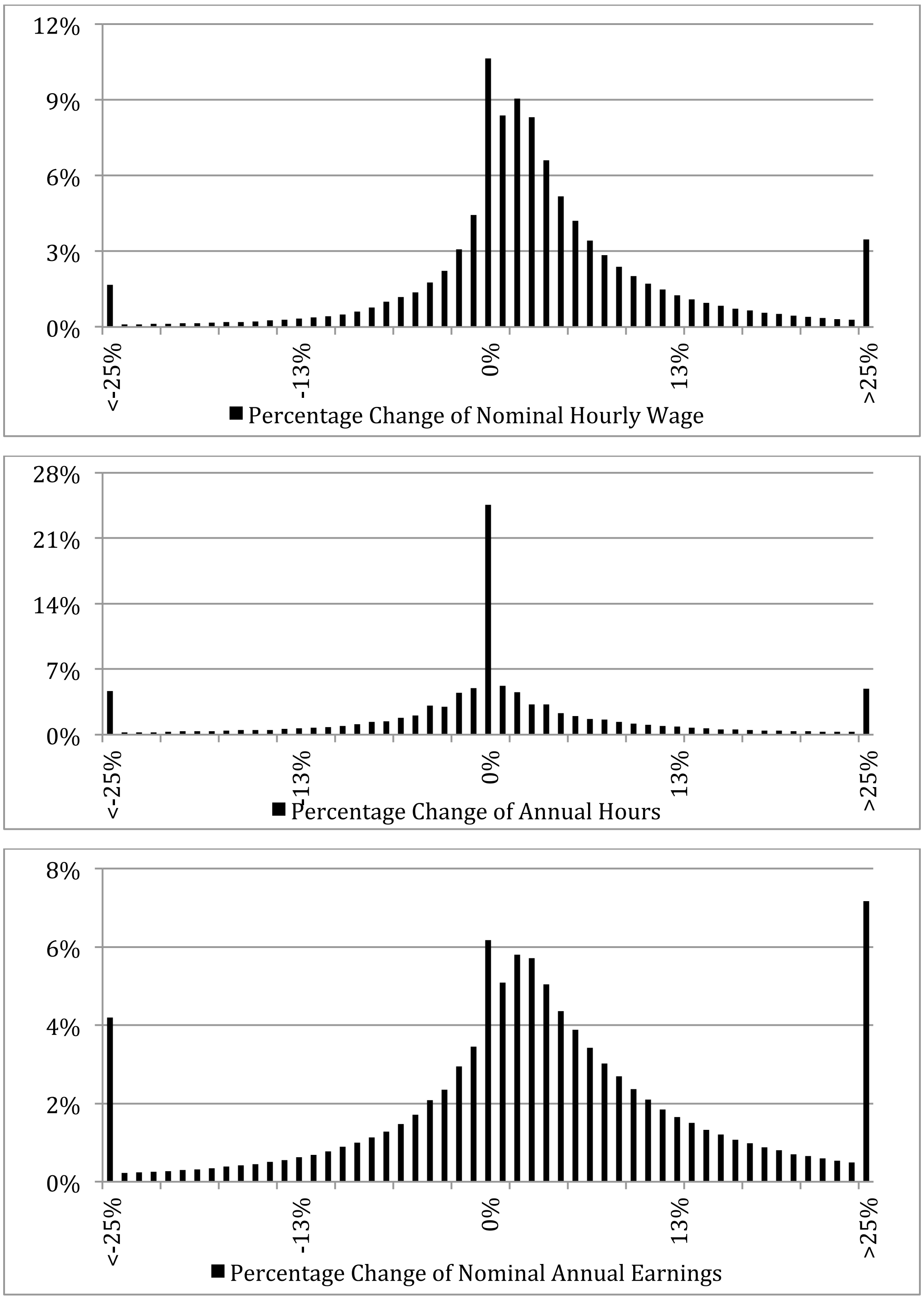


Figure 5: Decomposition of Annual Earnings Changes into Hourly wage changes and changes in hours for job-stayers in MN, RI, and WA for 2010-2011

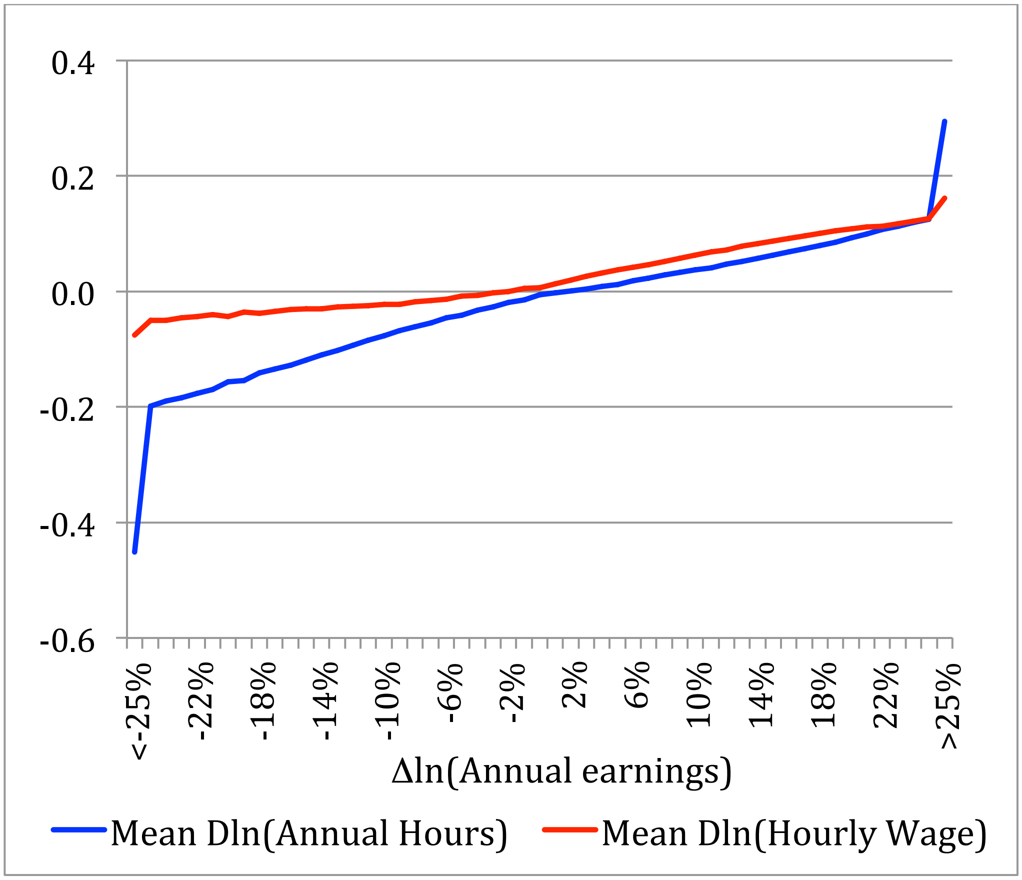
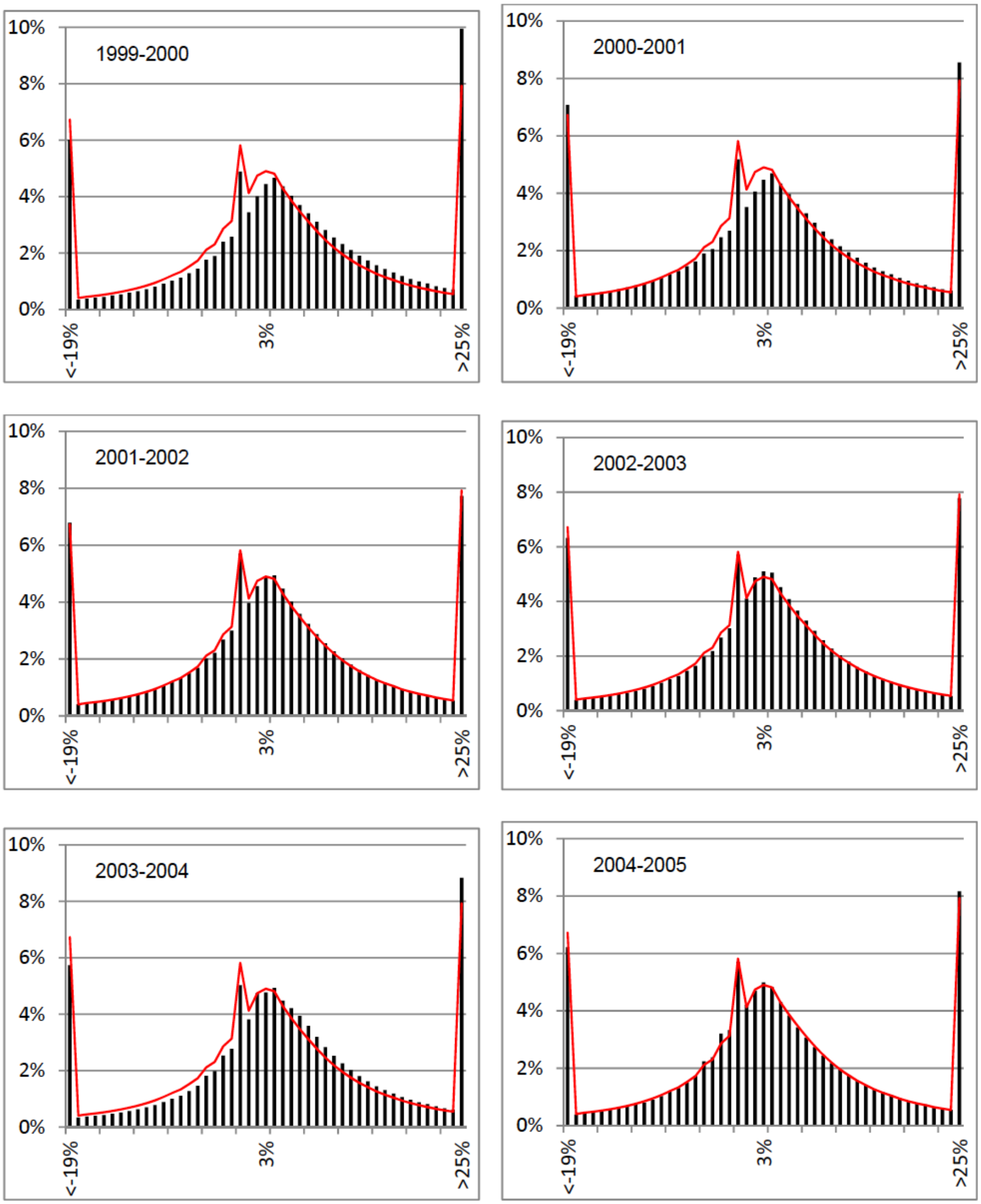


Figure 6: Nominal earnings change distribution, all job-stayers in 30-state sample, by year



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

Figure 6 continued

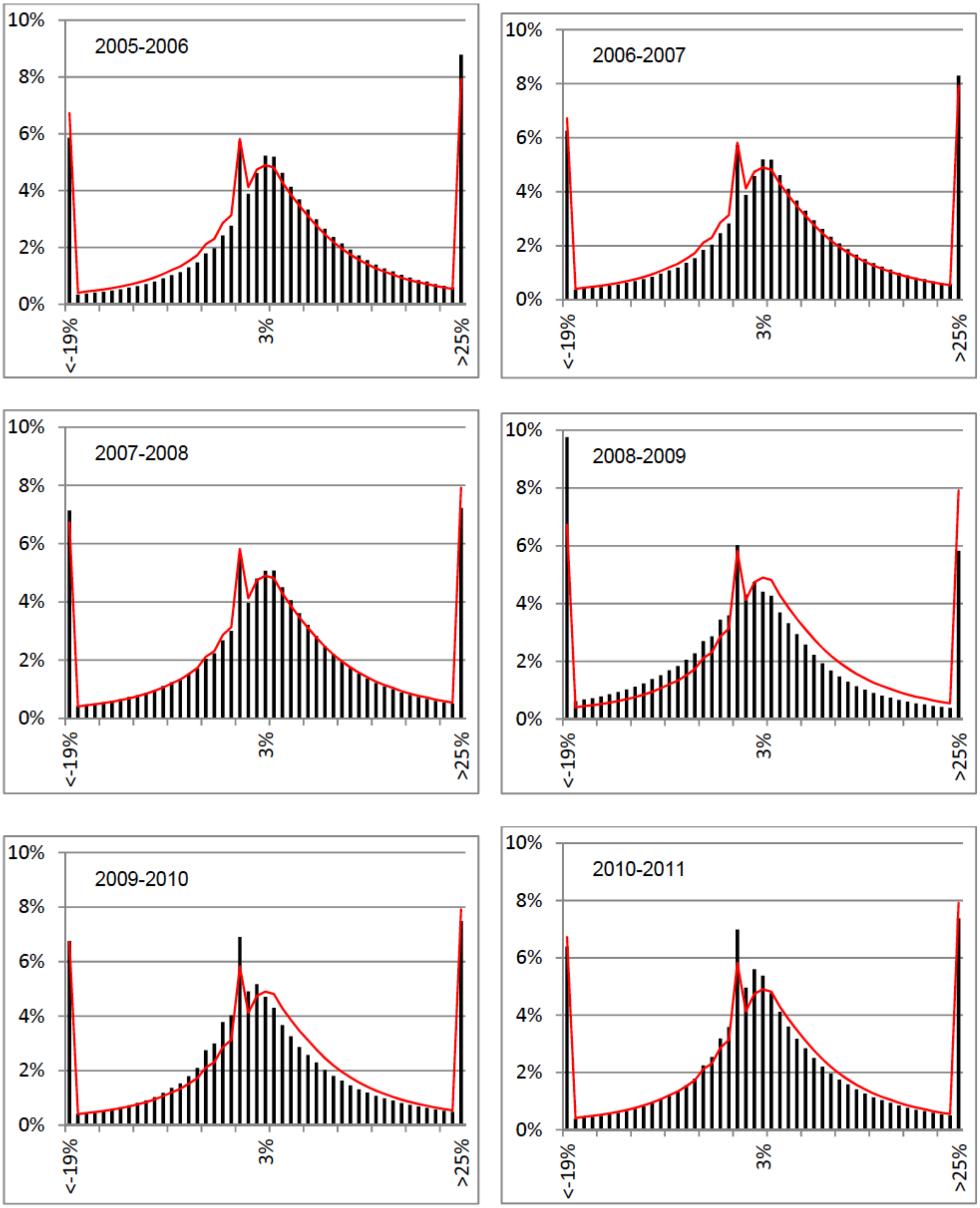
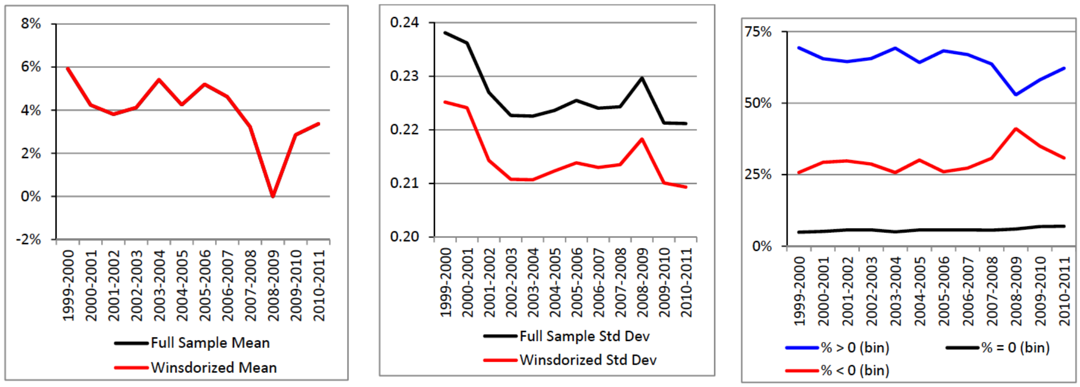
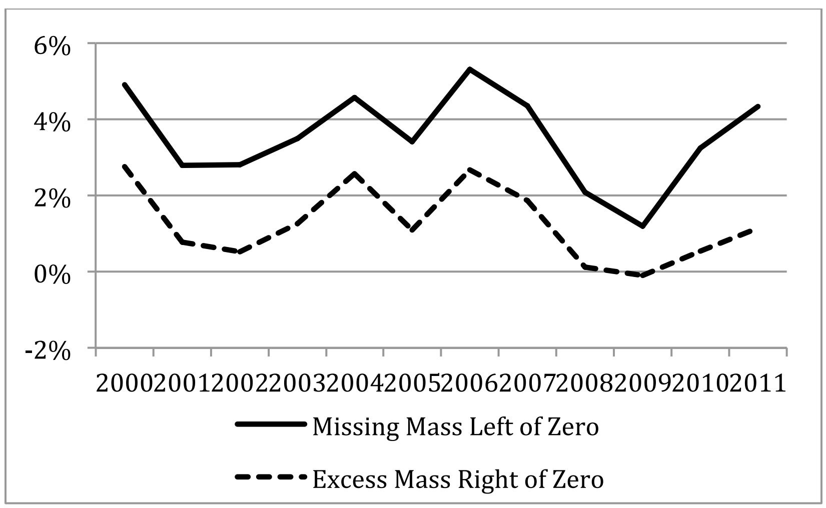


Figure 7: Distributional statistics by year; all job-stayers in 30-state sample

Panel A



Panel B



Panel C

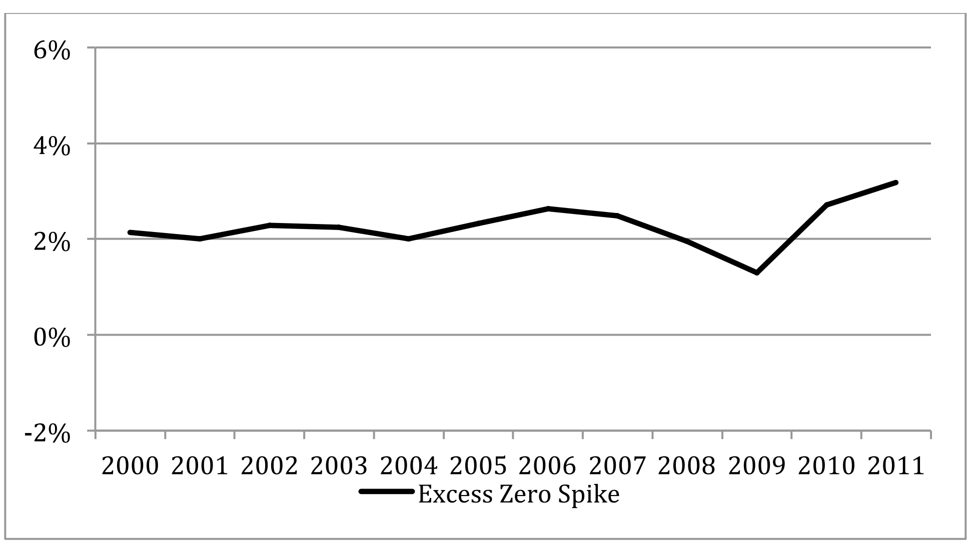


Figure 8: Distribution of zero spike statistic across firms

Firms with 50 or more job-stayers, all years

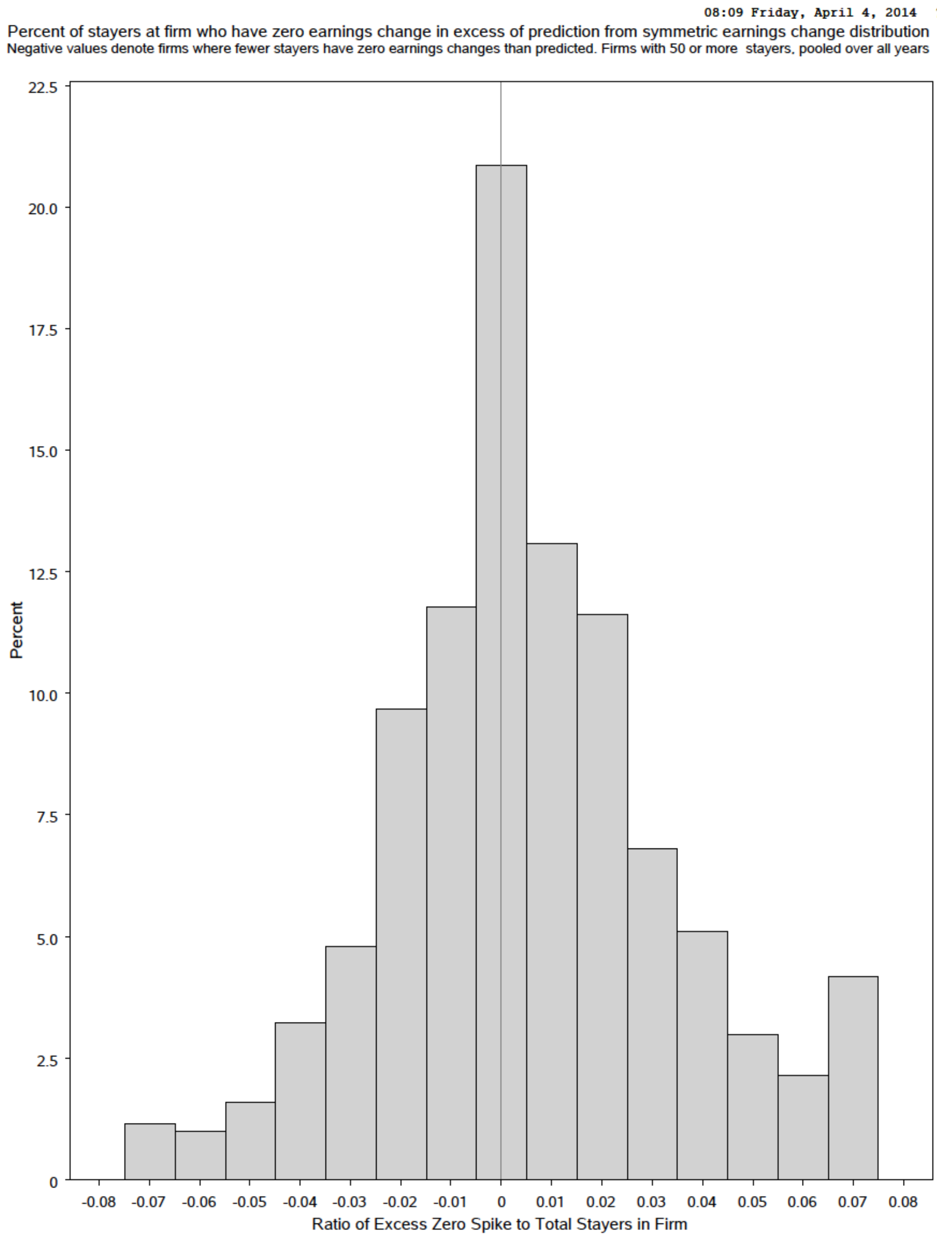


Figure 9: Distribution of missing mass statistic across firms

Firms with 50 or more job-stayers, all years

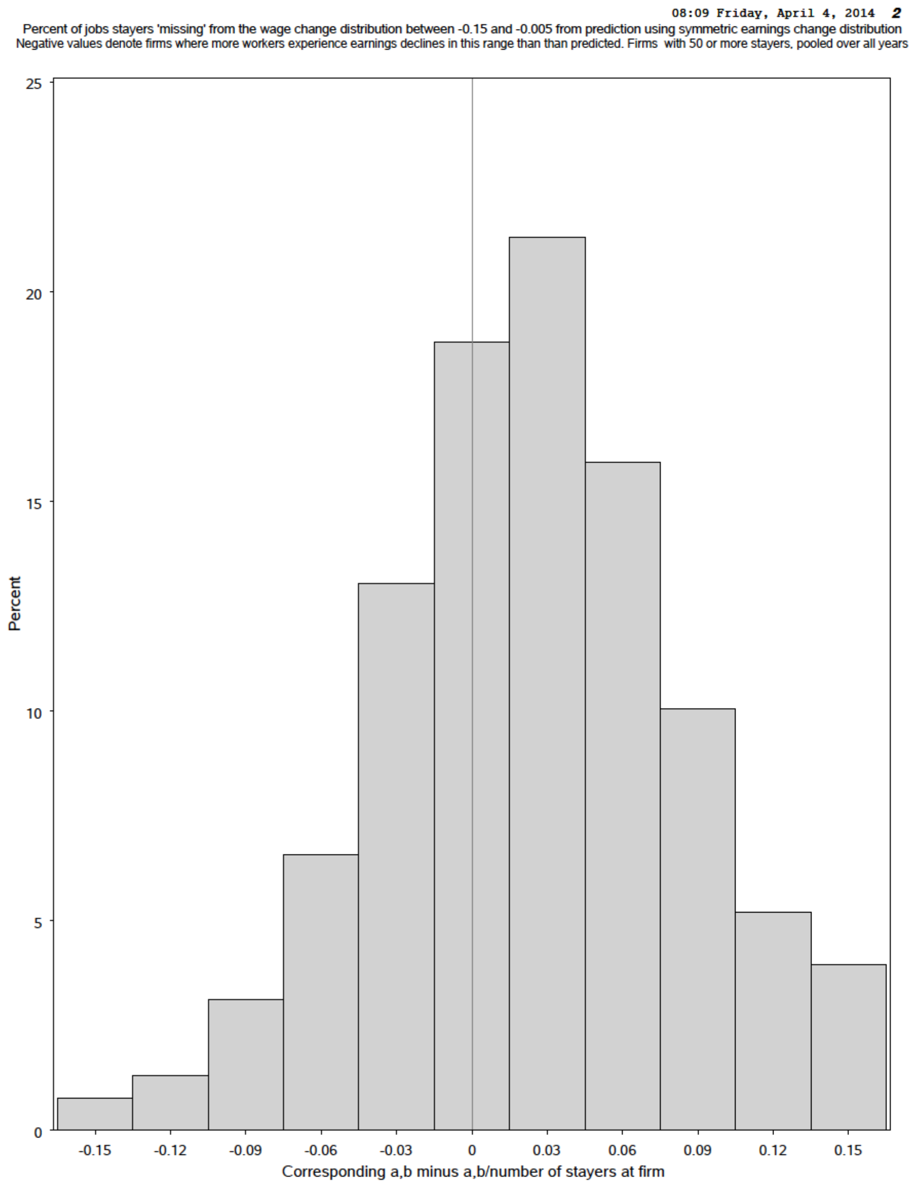
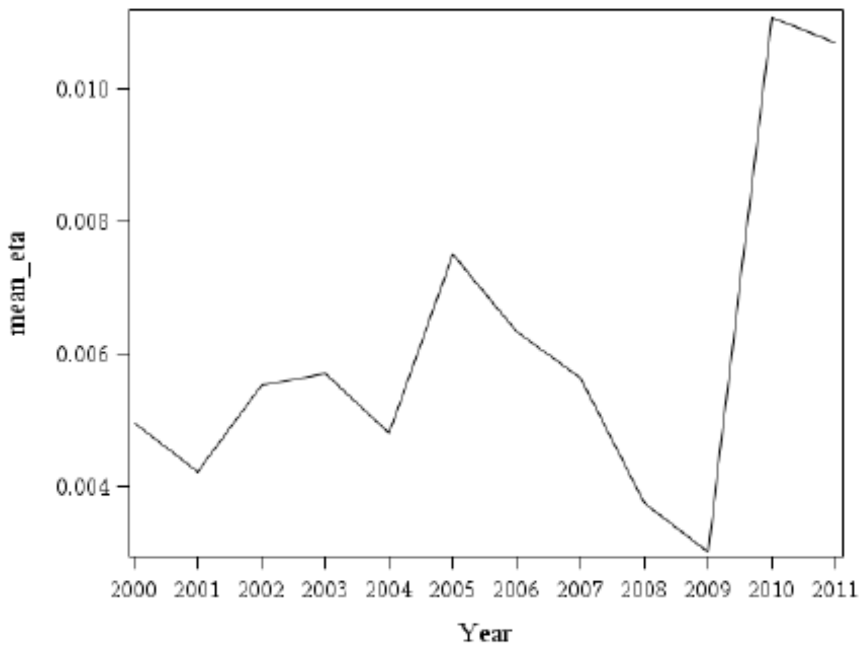


Figure 10: Zero-spike and missing mass across firms averaged by year

Firms with 50 or more job-stayers



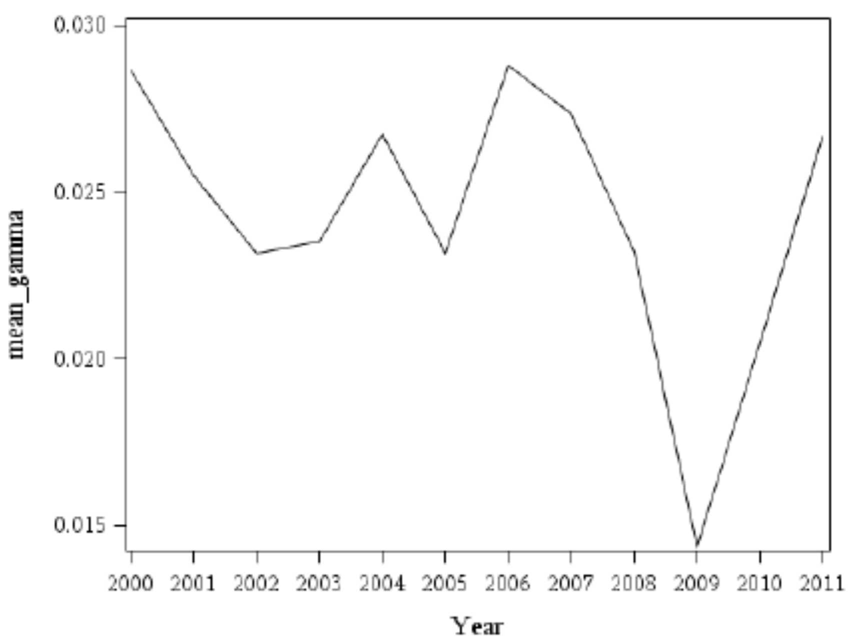


Table 1: Descriptive statistics

	Sample 1 Job Stayers 3 States	**Sample 2** Job Stayers 30 states	**Sample 3** [1] 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		
ΔLN(annual earnings)	.0383	.0381	.0536
ΔLN(annual hours)	.0037		
ΔLN(hourly wage)	.0346		
Δ(annual earnings) ≤ -.5%	.2889	.3031	.2496
Δ(annual earnings) -.5% < 0 < .5%	.0617	.0582	.0395
Δ(annual earnings) ≥ .5%	.6493	.6387	.7109
Δ(annual hours) ≤ -.5%	.3577		
Δ(annual hours) -.5% < 0 < .5%	.2456		
Δ(annual hours) ≥ .5%	.3967		
Δ(hourly wage) ≤ -.5%	.2188		
Δ(hourly wage) -.5% < 0 < .5%	.1064		
Δ(hourly wage) ≥ .5%	.6748		

[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.

Table 1: Descriptive statistics (continued)

	Sample 1 Job Stayers 3 States	**Sample 2** Job Stayers 30 states	**Sample 3** [1] Job Stayers 30 states
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 2a: Annual Hours Change Regressions
Job Stayers, {MN, RI, WA}, 2010-2011

	(1)	(2)	(3)	(4)
Δln(Annual Earnings)	.5796 *			
	(.0010)			
Δln(Annual Earnings) <0		.7262 *	.7260 *	.7602 *
		(.0020)	(.0021)	(.0018)
Δln(Annual Earnings) $\geq$0		.4831 *	.5015 *	.5043 *
		(.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	.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 2b: Year-to-Year Nominal Hourly Wage Change Regressions Job Stayers, {MN, RI, WA}, 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 3: Asymmetry statistics of aggregate earnings change distributions

	Sample 1 Job Stayers 3 States	**Sample 2** Job Stayers 30 states	**Sample 3** [1] Job Stayers 30 states
Annual Earnings:			
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%
Hourly Wage:			
Missing mass left of zero [2]	8.02%		
Spike at zero [3]	5.47%		
Excess mass right of zero [4]	2.56%		
# Firms (average per year)	185 thousand	2,487 thousand	66 thousand

[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 4: Descriptive Regressions

Dependent Variable: Excess spike at zero (eta)

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
		0.856	0.371	0.880	0.189
Firm has less than		**0.523**	**0.535**	**0.572**	**0.567**
500 employees		0.008	0.008	0.008	0.009
		0.000	0.000	0.000	0.000
Firm has less than		**0.099**	**0.100**	**0.145**	**0.139**
5000 employees		0.010	0.010	0.010	0.010
		0.000	0.000	0.000	0.000
Average stayer			**-0.001**	0.000	0.000
tenure			0.000	0.000	0.000
			0.000	0.939	0.873
Average stayer			**-0.003**	**0.005**	**0.006**
age			0.001	0.001	0.001
			0.000	0.000	0.000
Share stayers			**-0.333**	**-0.258**	**-0.239**
that are female			0.012	0.018	0.018
			0.000	0.000	0.000
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
to 2005-2006)	0.000	0.000	0.000	0.000	0.000
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
	0.000	0.000	0.000	0.000	0.000
Includes industry				X	X
fixed effects					
Includes state					X
fixed effects					

Table 5: Correlation across firm-specific asymmtry statistics

	Gamma	Eta	Zeta
Gamma (missing mass left of zero)	1	0.218	-0.695
Eta (excess zero spike)	0.218	1	-0.064
Zeta (excess mass right of zero)	-0.695	-0.064	1