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Downward Nominal Wage Rigidity in the United States during and after the Great Recession

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Abstract

Rigidity in wages has long been thought to impede the functioning of labor markets. In this paper, we investigate the extent of downward nominal wage rigidity in US labor markets using job-level data from a nationally representative establishment-based compensation survey collected by the Bureau of Labor Statistics. We use several distinct methods to test for downward nominal wage rigidity and to assess whether such rigidity is less or more severe in the presence of negative economic shocks than in more normal economic times. We find a significant amount of downward nominal wage rigidity in the United States and no evidence that the high degree of labor market distress during the Great Recession reduced downward nominal wage rigidity. We further find a lower degree of nominal rigidity at multi-year horizons.

I. Introduction

Rigidity in wages has long been thought to affect the functioning of labor markets. Such rigidity can take different forms, with differing implications for unemployment and other aspects of labor market or economic performance. Rigidities could arise from explicit or implicit contracting, efficiency wages, fixed-length nominal wage agreements, menu costs in the wage-setting process, government regulations, or informational or behavioral factors.

One strand of the research on wage flexibility has focused on asymmetric rigidities in the nominal wage-setting process—most notably the downward nominal wage rigidity posited by Keynes (1936) and, later, Tobin (1972)—and what implications such rigidity might have for the macroeconomy at low levels of inflation.

With consumer price inflation often running well over 5 percent in the 1970s and early 1980s, such concerns were seen as mostly immaterial: The costs of inflation were viewed as clearly exceeding any potential benefits. However, as inflation declined to 2 percent or less by the mid-1990s, the question of whether workers or firms resist nominal wage cuts—and the consequences for labor market performance—became increasingly relevant. The Great Recession of 2008-09, during which the unemployment rate reached 10 percent and price deflation was at times seen as a distinct possibility, added to the relevance of this line of inquiry. Indeed, some researchers have argued that downward nominal wage rigidity had an important influence on the behavior of wage and price inflation in subsequent recent years (Daly and Hobijn, 2014).

In this paper, we use establishment-level data from a nationally representative compensation survey collected by the Bureau of Labor Statistics (BLS) to investigate the extent

of downward nominal wage rigidity in US labor markets. Rather than restricting our analysis to a single method for estimating downward nominal wage rigidity, we use four distinct methods proposed in the literature to examine both the level of rigidity and how it has changed over time. With a particular focus on the Great Recession and subsequent slow recovery, we examine whether downward nominal wage rigidity is less or more common in the presence of negative economic shocks than in more normal economic times.

As is the case with many earlier studies, we find a significant amount of downward nominal wage rigidity in the United States, both in terms of the proportion of wage changes that were actually constrained by downward nominal wage rigidity (which we will refer to as “operative rigidity”), and the proportion potentially subject to such rigidity should the notional wage change fall below zero. Although one may have expected the costs of wage rigidity to become too burdensome to maintain in periods of great economic distress, the estimators we examine do not indicate that nominal wage rigidity became less common during and after the Great Recession despite the low rates of inflation and deep economic contraction.

Despite the overwhelming evidence for downward nominal wage rigidity in the United States, many researchers find little evidence that this form of rigidity has had material consequences for the performance of the labor market. Various hypotheses have been put forward to explain this finding, some of which we can test with our data. We find that a likely reason for the lack of macroeconomic consequences from downward nominal wage rigidity is that firms take a multi-year view of labor costs when implementing their compensation practices.

II. Background

Although clearly evident in the earlier writings of Keynes, the notion that downward nominal wage rigidity might lead to undesirable macroeconomic consequences at low levels of

inflation resurfaced with Tobin (1972), who argued that to “grease the wheels of the labor market,” it would be optimal to target an inflation rate somewhat above zero.¹ As noted above, given the high levels of inflation during the 1970s and early 1980s, such an argument did not seem particularly germane to policymakers at the time. However, as inflation continued to trend down in the early 1990s, the question of the macroeconomic effects of downward nominal wage rigidity regained some prominence. The possibility that nominal wages were downwardly rigid also accorded with indications from a variety of surveys (Blinder and Choi, 1990; Bewley, 2002; Smith, 2015; Du Caju et al., 2015) that declines in real wages caused by price increases are more acceptable to workers than nominal wage cuts and that employers are often reluctant to cut nominal wages because, among other factors, they believe such cuts would damage worker morale.

This survey evidence has been accompanied by empirical research, the first wave of which focused on micro-level data from household surveys, to assess the extent of downward nominal wage rigidity in actual wages. Research by McLaughlin (1994), Lebow, Stockton, and Wascher (1995), and Kahn (1997) used individual-level wage changes constructed from the Panel Survey of Income Dynamics (PSID) to test for the presence of downward nominal rigidity, with McLaughlin and Lebow, Stockton, and Wascher finding limited supporting evidence and Kahn finding somewhat more, especially for hourly wage workers. However, concerns about the prevalence of measurement error in the wage changes constructed from these surveys (for example, Bound and Krueger, 1991; Gottschalk, 2005) led many to question the reliability of the findings from this research. One response to these concerns has been to try to correct for

¹ A similar argument can be seen in Lipsey (1960) and Summers (1991). For recent examples of studies of the adverse economic effects of nominal wage rigidity, see Christiano, Eichenbaum, and Trabandt (2020) and Murray (2019).

measurement error. For example, Altonji and Devereux (2000) estimated a model that explicitly allows for measurement error in reported wages from the PSID and found that nominal wage cuts are over-reported in that data set, while nominal wage freezes are under-reported. Similarly, Gottschalk (2005) applied methods to test for structural breaks to wage histories from the Survey of Income and Program Participation (SIPP) and found that the frequency of nominal wage cuts was overstated in the raw data.² And Dickens et al. (2007) found that the auto-covariance of individual wage changes was correlated with measures of real and nominal wage rigidity in the household-level data sets they examined, leading them to conclude that those rigidity measures are biased downward by measurement error in the data.

Other researchers turned to employer surveys, which are thought to suffer less from misreported wages. These studies generally found a large role for downward nominal wage rigidity.³ However, much of the research that has used firm-level data has been based on case studies or on samples of only a small number of firms, and thus seems of limited applicability to the US economy as a whole. That said, Lebow, Saks, and Wilson (2003) used the same nationally representative data set that we employ in this paper and found that downward nominal wage rigidity reduced the number of nominal wage cuts by about half over their sample period.

More recently, researchers have turned to payroll records to assess the presence of downward nominal wage rigidity, using either administrative data from the unemployment insurance tax system or data from payroll processing firms.⁴ For example, Kurmann and McEntarfer (2019) and Jardim et al. (2019) used payroll records from Washington state, where

² See also Barattieri, Basu, and Gottschalk (2014), although their focus is more on nominal wage rigidity in general than on downward nominal rigidity.

³ See, for example, Wilson (1999) and Altonji and Devereux (2000).

⁴ Elsby and Solon (2019) provide a review of this literature covering a number of countries. Here, we focus on results for the United States.

the state unemployment insurance system requires employers to report both quarterly earnings and quarterly hours for each employee (thereby enabling the researchers to construct an hourly wage). Both sets of researchers found that roughly 20 percent of job stayers experienced nominal wage cuts over the sample period they study, while less than 10 percent had their earnings frozen.

One caveat with these studies is that the earnings measures include all forms of cash payments—including commissions, nonproduction bonuses, and overtime pay—and not just base wage rates. The authors argue that this is an advantage of their data, as employers can (and often do) use these forms of variable pay to adjust their labor costs without changing their wage structures. However, whether or not base pay is downwardly rigid may also be of interest. In this regard, Grigsby et al. (2019) used administrative data from the payroll processing firm ADP to examine the extent of downward nominal rigidity among job stayers in base wages and compared that with the extent of downward nominal rigidity in hourly earnings. They report that only 2.5 percent of workers experienced a reduction in their base wage in a given year, while 34 percent experienced a wage freeze. At the same time, less than 10 percent saw no change in their hourly earnings (base pay plus bonuses) and more than 15 percent experienced a reduction.

With the research tending toward finding an identifiable presence of downward nominal rigidity, a second question was whether the magnitude of that rigidity was large enough to entail important macroeconomic consequences. Some researchers, including Akerlof, Dickens, and Perry (1996), argued that it did.⁵ In particular, these authors used a simulation model and found that reducing inflation from 3 percent to zero would lead to a significant and inefficient reduction in employment and raise the sustainable rate of unemployment by 1 to 2 percentage points. They

⁵ See also Groshen and Schweitzer (1999).

supplemented this finding with evidence suggesting that the Phillips curve is flatter at low rates of inflation.

Other researchers tended not to find sizable macroeconomic effects from downward nominal wage rigidity, suggesting that employers can often find ways to adjust their labor costs without having to cut nominal wages. For example, Card and Hyslop (1997) found little evidence that downward nominal wage rigidity affects real wage growth, while Lebow, Saks, and Wilson (2003) found only weak evidence of an effect of downward nominal wage rigidity on aggregate wages in a wage-price Phillips curve model. Lebow, Saks, and Wilson attributed this partly to a tendency for employers to use benefits to offset the rigidity in nominal wages and found, in particular, that downward nominal rigidity in compensation was about one-third less than in wages and salaries. Meanwhile, Elsby (2009) and Stüber and Beissinger (2012) provided evidence that firms respond to downward nominal wage rigidity by compressing the wage structure for other employees, effectively offsetting the effects of rigidity on aggregate wage growth. And, more recently, Kurmann and McEntarfer (2019) reported that the downward nominal wage rigidity apparent in nominal hourly wage rates is not evident in annual earnings, suggesting that employers respond to the downward rigidity in wages by reducing the hours worked by their employees.

The commenters on Akerlof, Dickens, and Perry (1996) also noted that the presence of important downward nominal rigidity observed in an environment of rapid inflation need not imply that such rigidity would remain important in an environment of low inflation or severe economic distress.⁶ Such a finding would be consistent with the surveys conducted by

⁶ See especially the discussions by Robert Gordon (1996) and Greg Mankiw (1996). See also Groshen and Schweitzer (1999) and Hanes and James (2003).

Kahneman, Knetsch, and Thaler (1986) and Bewley (1995); both studies suggested that employees find nominal wage cuts mostly unobjectionable if a firm is losing money.

These considerations suggest that we might have expected to experience a decline in the degree of downward nominal wage rigidity in the United States during and after the Great Recession. The evidence on this from previous research is mixed. Using data from the Current Population Survey (CPS), Daly, Hobijn, and Lucking (2012) and Daly and Hobijn (2014) found a noticeable rise between 2007 and 2011 in the percentage of workers in the same job who reported no change in their wage relative to a year earlier, with the proportion of workers with no change in 2011 higher than in any period since the beginning of their sample period in 1983. The increase in nominal wage rigidity was widespread by education level and across industries, suggesting that a broad range of employers were reluctant to cut wages despite the adverse demand shocks associated with the financial crisis. They also argued that downward nominal wage rigidity restrained the pace of aggregate wage growth during the recovery as employers refrained from raising wages even as the economy improved.

Elsby, Shinn, and Solon (2016) also used the CPS data to test for the presence of downward nominal wage rigidity in the United States. In contrast to Daly and Hobijn, these researchers used the January tenure supplement to the CPS to identify individuals who had been with the same employer for at least a year. Overall, their results showed roughly similarly sized spikes at zero as those reported by Daly and Hobijn. However, their results also indicated that there was only a modest increase in the size of the zero spike during and shortly after the Great Recession, which was accompanied by a more pronounced rise in nominal wage cuts, suggesting an increase in the degree of wage flexibility as well.

Kurmann and McEntarfer (2019) also report a noticeable increase in the prevalence of wage cuts during the Great Recession using their measure of hourly earnings. However, their results suggest that the increased prevalence of wage cuts was not sufficient in the aggregate to offset the magnitude of the adverse macroeconomic shock. Firms where wages were downwardly rigid reduced their employment during the recession by more than firms that cut wages, while the incidence of wage freezes doubled during the ensuing recovery, consistent with the results from Daly and Hobijn.

In this paper, we make two primary contributions to the question of the evolution of downward nominal wage rigidity in recent years.

First, we employ several estimators of the extent of downward rigidity that go beyond simple tallies of the size of the spike at zero wage change and the frequency of nominal wage reductions. This strategy has two advantages. First, we can assess whether our conclusions are robust to alternative methods of detecting rigidities. Second, we can, under some assumptions, use our methods to assess both the extent to which *actual* wage changes were constrained by downward nominal wage rigidity, and whether there were behavioral changes on the part of employers and workers that led to a change in the *potential* for downward nominal wage rigidity to bind during or after the Great Recession – for example, whether individual or employer norms toward nominal wage cuts responded to the prolonged period of low inflation and economic stress.

Second, we use data from a nationally representative survey of employers, which, like payroll records, are likely subject to less measurement error than data from household surveys. Unlike payroll records, however, our data refer to wage rates for specific jobs rather than specific individuals, which may be more relevant to employers' decisions. (See below.)

III. Data

Our study uses employer-based data that extend from the early 1980s through the Great Recession and subsequent recovery. In particular, we rely on a large, nationally representative sample of specific jobs within specific establishments from the BLS' National Compensation Survey, which is used to produce the Employment Cost Index (ECI). The ECI is intended to measure how much an employer must pay to employ the same labor input in the current period as in a base period. Each quarter, the survey collects data on various components of compensation per hour for, currently, about 34,000 jobs representing specific occupations within about 8,000 establishments throughout the United States.⁷ In addition to wages and salaries, the survey asks employers about the costs of various benefits, including paid leave, supplemental pay, insurance, pensions, and legally required benefits.⁸ Costs are converted to an annual rate and divided by annual hours worked to arrive at hourly compensation costs.

For each establishment, data are collected for a sample of jobs, with a median of five jobs per establishment. The jobs, which refer to the most detailed occupation and work level recognized by the firm, are selected by randomly sampling from a list of employees in the establishment such that the probability that a job is selected is proportional to the number of workers in that job at the establishment.⁹ Accordingly, sampled jobs are likely to be those in which the establishment's workers are concentrated, but they do not represent a census of jobs in

⁷ Private and state and local government establishments are included in the survey, while the federal government is not included. The sample size has increased substantially from the beginning of the program in the early 1980s.

⁸ Training and in-kind benefits are excluded, as are employee stock options.

⁹ Workers are included in the data for a job at an establishment only if they are actually employees of the establishment. Outside contractors or employees of a temporary help agency, for example, would not be included in the data for a job at an establishment even if the work is performed at the establishment. (They would be included in data for the temp agency.)

the establishment. The unit of observation is the job rather than the worker, and each observation represents the average wage or compensation costs for the job as of the pay period that includes the twelfth day of the third month of the quarter. Each establishment is assigned a fixed weight that is proportional to its size when it enters the sample. The sample is stratified to ensure sufficient coverage of industries, regions, and establishment-size classes.¹⁰

For our analysis of wage rigidity, we use the micro-level data from the National Compensation survey as an unbalanced panel of observations on jobs within establishments, and concentrate on wages and salaries. Starting with the same sample of observations as is used to calculate the published ECI (which, for example, excludes observations without positive wages and salaries per hour), we then restricted our sample to private-sector establishments in which at least three jobs were sampled in a given year; this reduces the number of observations by 15 to 20 percent in each year. In order to avoid complications arising from the seasonality of pay-setting, we also restrict our analysis to 12-month changes, from March to March in particular. We exclude imputed data from our analysis.¹¹ In addition, we trimmed the data set by setting any log changes in wages and salaries per hour that were below the first percentile to the first percentile value by year and any that were above the 99th percentile to the 99th percentile value by year. We date the wage changes by the end year of the change; that is, the change from March T-1 to March T is dated as year T.

As indicated in Table 1, the micro-data on wage changes are available from 1983 to 2019. Our sample consists of wage changes for an average of 18,156 jobs within 3,947 establishments per year.

¹⁰ For more details, see <https://www.bls.gov/opub/hom/ncs/home.htm>.

¹¹ The BLS imputed values for wages and salaries per hour for the roughly 8 percent of the observations that were missing data due to nonresponse or other reasons. In these cases, an initial value was collected, but values were imputed for quarters for which changes from the previous quarter were not reported.

As noted above, the unit of observation in our data is the job – a specific job within a specific establishment – not the worker. As we see it, there are conceptual advantages to each type of observation. The wage rate for the job may be more relevant to firms’ employment, production, and pricing decisions, while wages for specific individuals may say more about the behavioral roots of wage rigidity and the implications for earnings. At a practical level, the large sample size and accuracy of measurement from this source of data make it a valuable object of study regardless of which unit of observation one considers conceptually superior.

Table 1 also presents some simple summary statistics from our data set. On average over the years in our sample, the mean annual log wage change was 0.031 and the median log wage change was 0.030. As indicated in Figure 1, however, there has been considerable variation in the magnitude of wage changes over time, with the median ranging from 0.05 points in 1983 to just 0.014 points in 2010. On average over our sample, 16 percent of jobs see a decline in the average hourly wage rate, while 15 percent see no change.

IV. Preliminary Data Analysis

Figure 2 presents histograms of the wage change distribution for four selected years. The top two panels— wage changes ending in 1983 and 2000—represent relatively high-inflation years, while the bottom two panels—2010 and 2017—represent low-inflation years; additionally, 1983 and 2010 were years when the unemployment rate was relatively high, while 2000 and 2017 were years with low unemployment. In each panel, the horizontal axis shows the log wage

change. The bars show the proportion of wage change observations in bins of width 0.01 (except at zero, where it shows the proportion of observations with no wage change).¹²

Consistent with the idea that nominal wage rigidity is prevalent in US labor markets, the most notable feature of the histograms is that all of them exhibit a sizable spike at zero. Apart from the spike at zero, the log wage change distributions appear to be unimodal and more concentrated around the mode than a normal distribution. There is no clear evidence of systematic asymmetry near the mode of the distribution.

V. Tests of Rigidity Using Properties of the Wage Change Distribution

In this section, we describe the various tests we use to examine properties of the wage change distribution for signs of downward nominal wage rigidity. We focus both on estimating the actual proportion of the wage change distribution affected by downward nominal wage rigidity (operative rigidity) in each year and the proportion of jobs that are potentially subject to downward nominal wage rigidity in the event that their notional wage changes are negative.

A simple measure of operative downward nominal wage rigidity is the size of the spike at zero, already noted in Figure 2. This is essentially the measure employed by Daly, Hobijn, and Lucking (2012) and Daly and Hobijn (2014) using household-level data from the Current Population Survey; by Kurmann and McEntarfer (2019) and Jardim et al. (2019) using data from Washington; and by Grigsby et al. (2019) using data from ADP. A related test for potential rigidity, described in Dickens et al. (2007), assumes that all reported nominal wage freezes would have instead been nominal wage cuts in the absence of downward nominal wage rigidity.

¹² Also, the bins at the extremes of the distribution are wider because of the sparsity of the data in those regions.

Under this assumption, the ratio of nominal wage freezes to the sum of nominal wage freezes and nominal wage cuts provides an estimate of the proportion of jobs that are potentially subject to downward nominal wage rigidity. Because wage changes could be set to zero for other reasons (for example, menu costs), this measure should be viewed as an upper bound. However, it provides a good starting point for our analysis.

The second test, which was developed by Lebow, Stockton, and Wascher (1995), assumes that the wage change distribution that would obtain in the absence of rigidities (the “notional” distribution) is symmetric and that the upper half of the distribution is largely unaffected by rigidity. This procedure (which we subsequently refer to as the LSW statistic) then uses deviations in the shapes of the upper half and lower half of the distribution as an indication of operative downward nominal wage rigidity. In particular, the difference between the mass above twice the median and the mass below zero provides an estimate of the fraction of wage observations that were constrained by downward nominal wage rigidity. The related test for potential downward nominal wage rigidity uses the ratio of this LSW difference statistic to the mass above twice the median. Under certain assumptions, this ratio can be viewed as a measure of the proportion of observations potentially subject to downward nominal wage rigidity if their notional wage changes were to fall below zero.¹³

The third test is based on a procedure developed by Kahn (1997) that assumes the distribution of notional wage changes is fixed over time except for a variable median. The idea underlying this test is to examine the extent to which the mass at various points in the wage

¹³ These assumptions include that (1) the shape of the notional distribution does not vary as the average wage change falls or rises; (2) the extent of potential rigidity is invariant to the average wage change; and (3) the degree of potential rigidity is the same at all relevant points in the wage change distribution (that is, a job at the Xth quantile of the wage change distribution is as potentially rigid as a job in the Yth quantile of the distribution, for any X and Y that have a realistic chance of falling below zero). Dickens et al. (2007) use a broadly similar measure to test for potential real downward wage rigidity.

change distribution differs from what would be expected in the absence of wage rigidity, where the mass points of that notional distribution in each percentile of the histogram are based on years in which a particular type of rigidity would not be expected to affect the proportion of observations in that bin. In this case, the estimate of downward nominal rigidity is based on comparisons of the size of the histogram bars below zero in low and high inflation years, while the estimate of rigidity associated with menu costs is based on comparisons of the bars in the neighborhood of zero in different years. Thus, for example, the estimator asks how much the mass of the N th percentile bin of the distribution increases when wage inflation is such that the wage change at that N th bin is zero. The rigidity estimates are estimated simultaneously by regressing the fraction of observations in each bin in each year on a set of dummy variables representing each bin of the distribution and a second set of dummy variables that indicate the type of rigidity expected to affect each bin in each year.

The fourth test takes a more parametric approach to the problem. It assumes that the notional wage change distribution can be modeled as a two-sided symmetric Weibull distribution, with location, shape, and scale parameters to be estimated.¹⁴ The model then allows for downward nominal rigidity (for example, $\Delta \log w = 0$ when the notional wage change is less than zero) and menu costs (for example, $\Delta \log w = 0$ when the notional wage change is small in magnitude) to create differences between the observed wage change distribution and that implied by the estimated notional distribution, and assumes that these potential rigidities take particular parametric forms.

¹⁴ See Gottschalk (2005) in support of the choice of the Weibull assumption. Guvenen et al (2015) find that the empirical distribution of one-year changes in earnings across individuals is decidedly more peaked than the typically assumed log-normal distribution, providing further support to the choice of a double-sided Weibull distribution. See also Deelen and Verbeek (2015). Note that although we have chosen to specify the notional distribution to be symmetric, one could allow for an asymmetric distribution.

In the specification we have implemented, the probability that a notional nominal log wage change that is less than zero will, in practice, become zero due to downward rigidity takes a maximum value, p^{DR} (to be estimated), just below $\Delta \log w = 0$ and declines linearly to zero at some negative value, $-b$ (to be estimated). That is,

$$\begin{aligned} \Pr^{DR}(\Delta \log w^a = 0) &= p^{DR} * (1 + (\Delta \log w^n / b)) \text{ if } -b < \Delta \log w^n < 0, \text{ and} \\ &= 0 \text{ if } \Delta \log w^n < -b \text{ or } \Delta \log w^n > 0 \end{aligned}$$

where $\Delta \log w^a$ is the actual change, $\Delta \log w^n$ is the notional change, and $b > 0$.

Similarly, the probability that a small notional nominal log wage change, positive or negative, will, in practice, become zero due to menu cost rigidity takes a maximum value, p^{MC} (to be estimated), close to $\Delta \log w = 0$ and declines linearly and symmetrically to zero at some distance, m (to be estimated), from $\Delta \log w = 0$. That is,

$$\begin{aligned} \Pr^{MC}(\Delta \log w^a = 0) &= p^{MC} * (1 - |\Delta \log w^n| / m) \text{ if } |\Delta \log w^n| < m, \text{ and} \\ &= 0 \text{ if } |\Delta \log w^n| > m, \end{aligned}$$

where $m > 0$.

Both downward and menu cost rigidity can apply simultaneously.

The estimated parameters of the model then provide an indication of the fraction of observations subject to each type of rigidity, both overall and at each value of notional wage change. This is a greater level of detail than is available from the other tests we employ, at the cost of making stronger assumptions about the underlying distribution and parameterization of the rigidities. In keeping with the first two tests, we estimate this model separately for each year, so the parameters may vary freely by year.

None of the tests we examine is ideal; each relies on particular assumptions about the underlying distribution of wage changes in the absence of rigidities. For this reason, we present the results from all of the tests and evaluate the preponderance of the evidence about downward nominal wage rigidity.

VI. Estimates

Estimates from the various tests are summarized in Tables 2 and 3 and in Figures 3 through 10. In the tables, we show the estimates averaged over the entire period for which the ECI is available, as well as for selected sub-periods.

A. Size-of-Spike and LSW

The size-of-spike and LSW estimator measures of operative rigidity are shown in Figure 3 and the top rows of Table 2. Looking first at the size-of-spike in row 1, over our entire sample period, an average of about 15 percent of jobs saw no change in nominal wages from one period to the next, suggesting a substantial quantity of wage rigidity. Moreover, as can be seen in the upper panel of Figure 3, there is no indication that the severe economic stress of the Great Recession caused a decline in the amount of wage rigidity. If anything, the amount of rigidity indicated by this measure increased in the aftermath of the recession, before declining in recent years to levels more consonant with the pre-recession period.

The LSW statistic, in row 2 of the table and the lower panel of Figure 3, tells a similar tale. Between 1983 and 2009, the statistic was fairly flat, suggesting little change in the extent of downward nominal wage rigidity over that period. This estimate of rigidity, like the size-of-

spike measure, moved sharply higher around the Great Recession, and has receded somewhat in the years since, albeit less so than the size-of-spike.

B. Relative Size-of-Spike and Proportional LSW

As noted above, each of these two measures has a counterpart that, under certain assumptions, measures the amount of potential as opposed to realized rigidity. These, the relative size-of-spike and proportional LSW measures, are shown in Figure 4 and the middle rows of Table 2.

The relative proportion of zero wage changes (row 3 of the table and upper panel of the figure) averages about 47 percent over our entire sample period, suggesting that the number of nominal wage cuts would have been almost twice as large in the absence of downward nominal wage rigidity. This measure, too, rose around the Great Recession, but to no higher a level than was common before a downshift that began in 2001.

The proportional version of the LSW statistic (row 4 of the table and the lower panel of the figure) has behaved similarly, with an increase following the Great Recession back to levels similar to those that prevailed before a downshift during the prior decade.

C. Correlation with Wage Inflation

The size of the spike at zero and the LSW statistic provide straightforward and simple estimates of the degree of downward nominal wage rigidity operative in each year. For a given structure of potential rigidity, we would expect this amount of observed rigidity to be negatively correlated with the overall rate of wage inflation, simply because more notional wage changes

are in negative territory.¹⁵ This negative relationship can be seen in Figure 5, which plots the size-of-spike and LSW statistics in each year against the median log wage change, along with a simple linear regression line.¹⁶

Figure 6 instead plots the two measures of potential rigidity in each year against the median log wage change in that year. One would not necessarily expect a negative correlation between potential rigidity and wage inflation, and, indeed, we see none in the figure. The relative size-of-spike measure is actually positively correlated with the median log wage change, which appears to be driven primarily by the downward shift in the measure in about 2001 that we noted in Figure 3.

D. Kahn Tests

Table 3 shows some results from the Kahn (1997) estimator, which formalizes the expectation of a negative correlation between operative nominal wage rigidity and wage inflation. We include estimates from three versions of the model that parallel those in Kahn (see there for details).

The first (labeled “Linear w/o pile-up”) allows for a spike at zero, for mass to be missing from the negative part of the distribution (as would be the case under general downward nominal wage rigidity), and for mass to be missing from two positive bins and one negative bin close to

¹⁵ Of course, a negative relationship between inflation and observed rigidity could also occur for other reasons. For example, symmetric rigidity around a zero wage change (for example, due to menu costs) would also be expected to produce a larger spike at lower rates of inflation if a decline in inflation shifted the bulk of the wage change distribution toward zero.

¹⁶ Calculating the correlation with nominal wage growth as opposed to price inflation allows for the possibility that increases in productivity limit the extent of notional nominal wage cuts by raising the prevalence of notional real wage increases.

zero (as might be the case under a menu cost type of rigidity). However, it does not explicitly relate any missing mass from these regions to any extra mass at zero. The second (“Linear with pile-up”) is similar to the first version, but enforces the restriction that the masses missing from the negative and near-zero bins contribute to the “pile-up” at zero (although they need not be the only source of the increase at zero).

Both of these linear versions assume that the amount of missing or added mass in each bin is independent of the mass that bin would have contained under the notional wage change distribution. The third version (labeled “Proportional”) instead assumes that the increase in rigidity leads to a proportional rise in the size of the zero spike and related proportional declines in the mass in the negative and near-zero bins.

The first three rows of Table 3 show the estimates using our full sample of years, 1983-2019. As can be seen in column (a) of rows 1 and 2, the linear versions of the model without and with pile-up show substantial and statistically significant increases (of 17.3 and 11.6 percentage points, respectively) in a bin’s mass when that bin corresponds to zero wage change. The model without pile-up fails to attribute that increase to a dearth of wage cuts. The model with pile-up, in contrast, attributes about half of the increase at zero to a decline in wage cuts.¹⁷

The proportional model (row 3, column a) also indicates a substantial increase in the mass of a bin when it corresponds to zero wage change, and a related decrease in mass among bins that correspond to wage reductions, consistent with a significant degree of downward nominal wage rigidity. To put the estimated coefficients in perspective, given the average proportion of wage changes of various amounts in the sample, the model sees an increase in the

¹⁷ There is an average of 8.2 bins in negative territory over the sample period, which, multiplied by the coefficient in column (b), yields a reduction in the proportion of wage cuts of about 6½ percentage points.

proportion of zero wage changes of about 1.6 percentage points, and an even larger decrease in the number of wage cuts of 3.9 percentage points.

Because the Kahn estimate relies on variation across years to identify nominal rigidities, we cannot produce year-by-year estimates like those for the size-of-spike and LSW statistics. However, we can get some sense of how the Kahn measures of nominal rigidity have varied over time by estimating the models over various sub-periods of the sample. Rows 4 to 6 of the table show results from a sub-sample ending in 2007, the year prior to the Great Recession. The results are qualitatively similar to those for the full sample. Rows 7 to 9 show results from a sub-sample for 2006-2019. Here, two of the models show large differences from the full sample. In particular, the proportional model for this sample period indicates no increase in mass at zero wage change, but large decreases in mass for negative changes (albeit tempered when those changes are near zero) and nearby positive changes. However, we are skeptical of these results, because we suspect that there is too little variation in wage inflation over this latter sample period from which to identify rigidities in the Kahn set-up. The standard deviation of the median log wage change over the full sample period is 0.0088, while over the 2006-2019 period it is only 0.0049.

E. Parametric Model

We estimated the parametric model separately for each year in our sample period. The distributions of notional and operative wage changes implied by the model for selected years are shown by the histograms in Figure 7. In each histogram, the blue bars show the estimated density of notional log wage changes, while the red bars show the estimated density with rigidity operating. Thus, the difference between the lines shows the distortion caused by downward nominal and menu-cost rigidities. This estimator, like the others, finds a large amount of

downward nominal wage rigidity. (It also finds some menu-cost rigidity, but by and large, this element is of small importance.)

To summarize these findings and compare them to the earlier estimators, we calculated the size-of-spike and LSW statistics implied by the estimated notional wage change distributions and rigidity parameters from the parametric model; these statistics are shown in the bottom panel of Table 2. The degree of rigidity indicated by the size-of-spike and LSW statistics implied by the parametric model (rows 5 and 6) is similar to that found by the corresponding empirical statistics (rows 1 and 2). The fluctuations over time in the implied statistics, shown in Figure 8, also resemble their empirical counterparts, although the drop in the early 2000s and the jump toward the end of the Great Recession are somewhat less pronounced (and the implied LSW statistic drops markedly in 2019, which its empirical counterpart does not).

Figure 9 shows the estimated extent of rigidity two additional ways. The top panel shows the proportion of negative notional wage changes that are “swept up” to zero by nominal rigidities (of which, according to the estimated parameters, downward nominal rigidity is by far the more important factor). This sweep is a function of both the estimated rigidity parameters of the model and the estimated distribution of notional wage changes in each year, and so provides a measure of operative rigidity. Although a downtrend is evident, the estimates give no indication of a persistent decline in operative rigidity brought on by the Great Recession.¹⁸

The bottom panel shows the probability that a notional log wage change of -0.1 becomes zero due to rigidity. This is one look at potential rigidity, as it depends on the estimated rigidity parameters but not on the estimated distribution of notional wage changes. Here we see less of a

¹⁸ This sweep measure is similar in concept to the proportional LSW statistic.

downtrend and, again, no indication of a persistent decline in potential rigidity brought on by the Great Recession.

As another measure of potential rigidity, Figure 10 shows the relative size-of-spike and proportional LSW statistics implied by the parametric model. The implied relative size-of-spike in the upper panel is similar to the empirical relative size-of-spike from Figure 4. The implied proportional LSW statistic in the lower panel, however, shows a smoother decline than its empirical counterpart in the two decades before the 2001 recession, and no rebound after 2009.

VII. *Implications for Aggregate Wage Growth*

In sum, none of the three estimators of downward nominal wage rigidity from which we believe we can take reliable signals indicate that the high degree of economic distress combined with low inflation during the Great Recession reduced the proclivity toward downward nominal rigidity, as one might have expected.

Daly and Hobijn (2014) also found no indication of a decline in downward nominal wage rigidity during the Great Recession. They argue that such rigidity held up aggregate wage growth during the Great Recession and subsequently held down aggregate wage growth during the recovery as employers worked off a stockpile of pent-up wage cuts. Daly and Hobijn employed only the size-of-spike estimator, and Figure 11 compares their numbers to ours. As noted above, each data source has its advantages and disadvantages. Daly and Hobijn's data are based on repeated responses to the Current Population Survey about specific individuals, which, because wages are self- or proxy-reported, are thought to be more prone to measurement error. The ECI data are reported by establishments, which we believe to be more accurate.¹⁹ However,

¹⁹ As noted in Dickens et al (2007), if measurement errors in the level of wage rates are independent from one year to the next, then such errors should appear as negative autocorrelation in wage changes. One manifestation of this

they refer to the average wage change in a specific ongoing job in a specific establishment, which can reflect changes in the mix of employees in a job in addition to wage changes for particular employees. (As noted above, whether this is an advantage or a disadvantage may depend on the question at hand.)

The blue line in Figure 11 shows our measure; the green line the Daly and Hobijn measure, as updated.²⁰ The proportion of jobs with no wage change in our data tends to run somewhat above the proportion of job-stayers reporting no wage change in Daly and Hobijn's data, and their series exhibits a fairly steady uptrend that ours lacks.

As noted above, the Daly and Hobijn series refers to the wage change for a specific individual. While we cannot do the same with our ECI data, we can provide a potentially more consistent comparison by restricting attention to a sub-sample of jobs for which the number of employees in the job did not change from one year to the next. Although turnover can change the mix of employees while leaving the number of employees constant, this sample does eliminate some observations that surely include a change in that mix. Unfortunately, this information was not available prior to 2006. This restriction greatly raises the proportion of zeros: For the years 2006-2019, the mean spike at zero was 14.8 percent in the full sample and 29.3 percent in the restricted sample. Despite these differences in levels, the evolution of the magnitude of the spike at zero over the sample period is broadly similar in the two sets of data. In particular, to the extent that such a short sample allows any inference, the constant-

would be that a wage change on one side of the average would be more likely to be followed by a wage change on the other side of the average than would be implied by the marginal frequencies of above- and below-average wage changes. In our data, the actual proportion of such "sign flips" is smaller, not larger, than would be implied by independence.

²⁰ <https://www.frbsf.org/economic-research/indicators-data/nominal-wage-rigidity/>

employment sample also gives no indication that nominal rigidity fell because of the Great Recession.

Moreover, according to the hypothesis put forth by Daly and Hobijn, the continued presence of desired real wage adjustments that were previously prevented by downward nominal wage rigidity caused employers to mute nominal wage increases even as the economy was improving. Daly and Hobijn argue that in their data, many of these would-be wage increases were reduced to zero, with the result that observed nominal wage rigidity, as measured by the size of the spike in the wage change distribution at zero, remained elevated, while the proportion and magnitude of positive observed wage changes fell. Taken alone, the persistence of large spikes at zero in our data following the recession (indicated in Figure 3) is consistent with this hypothesis. However, the evolution of wage changes at other parts of the distribution tells a somewhat different story. In particular, the fact that the LSW statistic remained quite high in the years following the recession suggests that the additional mass observed at zero continued to reflect reluctance on the part of some employers to cut nominal wages rather than a reluctance to raise nominal wages for jobs for which the notional wage change is positive.

That said, our data do lend some support to the more general notion that employers take a longer view of wage changes that may mitigate the economic impact of downward nominal wage rigidity. The inability to reduce wage rates in one year is less important if employers can make up for it with a lower raise in a subsequent year. In addition, as suggested by Elsbey (2009) and Stüber and Beissinger (2012), an employer may provide a smaller raise in one year in anticipation of being unable to lower nominal wage rates in a subsequent year. If so, then the distribution of wage changes over a multiple-year period should be more symmetric than the distribution of wage changes over a single year.

The ECI, being a panel of establishments, allows such a comparison. In particular, we can compute the change in wages in each job over two-year and three-year periods and compare those distributions with the distributions of one-year changes.²¹ Figure 12 compares the distribution of one-year, two-year, and three-year wage changes ending in 2010 as an illustration. Although there remain noticeable spikes at zero in the two- and three-year changes, they are much smaller than in the one-year change. (In addition, apart from the spike, the multi-year changes appear less peaked around the mode of the distribution than the one-year changes.) These comparisons hold for other years as well.

Table 4 shows the size-of-spike and LSW statistics for the one-year, two-year and three-year changes. Across all of the years in our sample the size of the spike at zero averages 7 percent for the two-year changes, as opposed to 15 percent for the one-year changes, and falls to 4 percent for the three-year changes. Similarly, the LSW measure of rigidity falls from an average of about 9 percentage points for the one-year changes to 5½ percentage points for the two-year changes and 5 percentage points for the three-year changes.

Using the relative size of the spike as a metric, the mean fraction of jobs estimated to be potentially subject to downward nominal wage rigidity over our sample period declines from 48 percent for one-year wage changes, to 34 percent for two-year changes, to 27½ percent for three-year changes. Likewise, the proportional LSW statistics are 35 percent for one-year wage changes, 28 percent for two-year changes, and 24 percent for three-year changes.

²¹ Because of sample rotation and attrition, using two-year wage changes reduces the average number of jobs in the sample each year by about 30 percent, and three-year wage changes by close to another 30 percent.

Given the relative magnitudes of the various measures of rigidity at different horizons, it seems clear that nominal rigidities are less important when one takes a longer view of wage changes, suggesting that time is, indeed, an ally of wage flexibility.

Elsby (2009) further suggests examining the upper tail of the wage change distribution during periods of greater downward nominal wage rigidity to look for signs of compression. While we have not tested this proposition as such, at a quick glance the ECI data do not provide much support for this hypothesis. In particular, the proportion of nominal wage increases that were greater than twice the median is negatively correlated with the median wage change (not shown), in contrast to the predictions from Elsby's model.

Finally, our data also do not provide support for the hypothesis that rigidity in wage rates is counterbalanced by flexibility in benefits. We find similar results when we perform our tests on the ECI's concept of total compensation as we did for only wages and salaries: There is significant downward rigidity, no indication that rigidity declined during the Great Recession, and rigidity is lower from a multi-year perspective.

VIII. Conclusion

On the whole, we interpret our results as indicating that the wage-setting process in the United States is characterized by a significant degree of downward nominal wage rigidity. We find no evidence that the great labor market distress of the Great Recession reduced the degree of operative or potential downward nominal wage rigidity either during the recession or in its aftermath. We find that the degree of nominal rigidity is much smaller at two-year and three-year horizons than over one year, which may help explain the lack of macroeconomic implications of downward nominal wage rigidity.

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Table 1
Dimensions of the Data

Years	1983-2019
Average number of jobs per year	18,156
Average number of employers per year	3947
Mean log wage change	0.031
Average median log wage change	0.030
Average proportion wage change < 0	16%
Average proportion wage change = 0	15%

Notes: Wage rates refer to nominal hourly wages and salaries, and annual changes are defined as the change over the 12-month period ending in March. Data exclude imputations. The counts refer to the sample of annual wage changes. Statistics through 2019 to be added when available.

Table 2
Estimates of Downward Nominal Wage Rigidity
(units are percentages)

Measure	1983-2019	1983-2007	2008-2010	2011-2019
<i>Operative rigidity:</i>				
1. Size of Spike	14.8	14.3	14.8	16.0
2. LSW	8.7	7.8	9.2	10.9
<i>Potential rigidity:</i>				
3. Relative Size of Spike	47.5	48.4	44.8	45.8
4. Proportional LSW	34.1	33.4	32.4	36.5
<i>Parametric Model:</i>				
5. Implied Size of Spike	12.5	12.0	12.0	14.0
6. Implied LSW	10.6	10.6	11.6	10.2
7. Proportion swept to zero	39.6	42.0	39.9	32.8

Table 3**Kahn Estimator****(units are percentage points for linear models, percent for proportional model)**

				(a)	(b)	(c)	(d)
	Sample	Dates	Model	Δ mass when zero	Δ mass when negative	Δ mass when small positive	Δ mass when small negative
1	All	1983- 2019	Linear w/o pile-up	17.3*** (0.26)	0.40 (0.28)	0.37 (0.24)	-0.004 (0.21)
2			Linear with pile-up	11.6*** (1.03)	-0.78*** (0.12)	-0.07 (0.20)	0.28 (0.19)
3			Proportional	12.9*** (0.8)	-33.7*** (4.7)	-4.4 (6.8)	13.8 (8.9)
4	All	1983- 2007	Linear w/o pile-up	17.3*** (0.4)	0.49 (0.46)	0.19 (0.34)	-0.18 (0.29)
5			Linear with pile-up	9.8*** (1.7)	-1.2*** (0.25)	-0.2 (0.3)	0.26 (0.26)
6			Proportional	10.6*** (1.9)	-49.1*** 8.8)	-7.2 (10.4)	11.3 (10.1)
7	All	2006- 2019	Linear w/o pile-up	16.3*** (0.4)	0.9** (0.4)	-0.3 (0.3)	-0.2 (0.3)
8			Linear with pile-up	-5.8*** (1.1)	-2.4*** (0.1)	-1.4*** (0.2)	0.5*** (0.2)
9			Proportional	-0.1 (1.3)	-58.1*** (2.5)	-32.3*** (3.2)	15.2*** (4.2)

Note: Standard errors in parentheses. Each bin is 1 log point wide. “small” refers to one bin above or below zero.

Table 4
Estimates of Downward Nominal Wage Rigidity for 1, 2, and 3-Year Changes

Measure	1-year	2-year	3-year
Date range	1983-2019	1984-2019	1985-2019
<i>Operative rigidity:</i>			
Size of Spike	15.0	7.1	4.1
LSW	8.8	5.5	5.0
<i>Potential rigidity:</i>			
Relative Size of Spike	48.0	34.2	27.5
Proportional LSW	34.6	27.9	23.9

* Excludes changes ending in years 2001, 2006, and 2007.

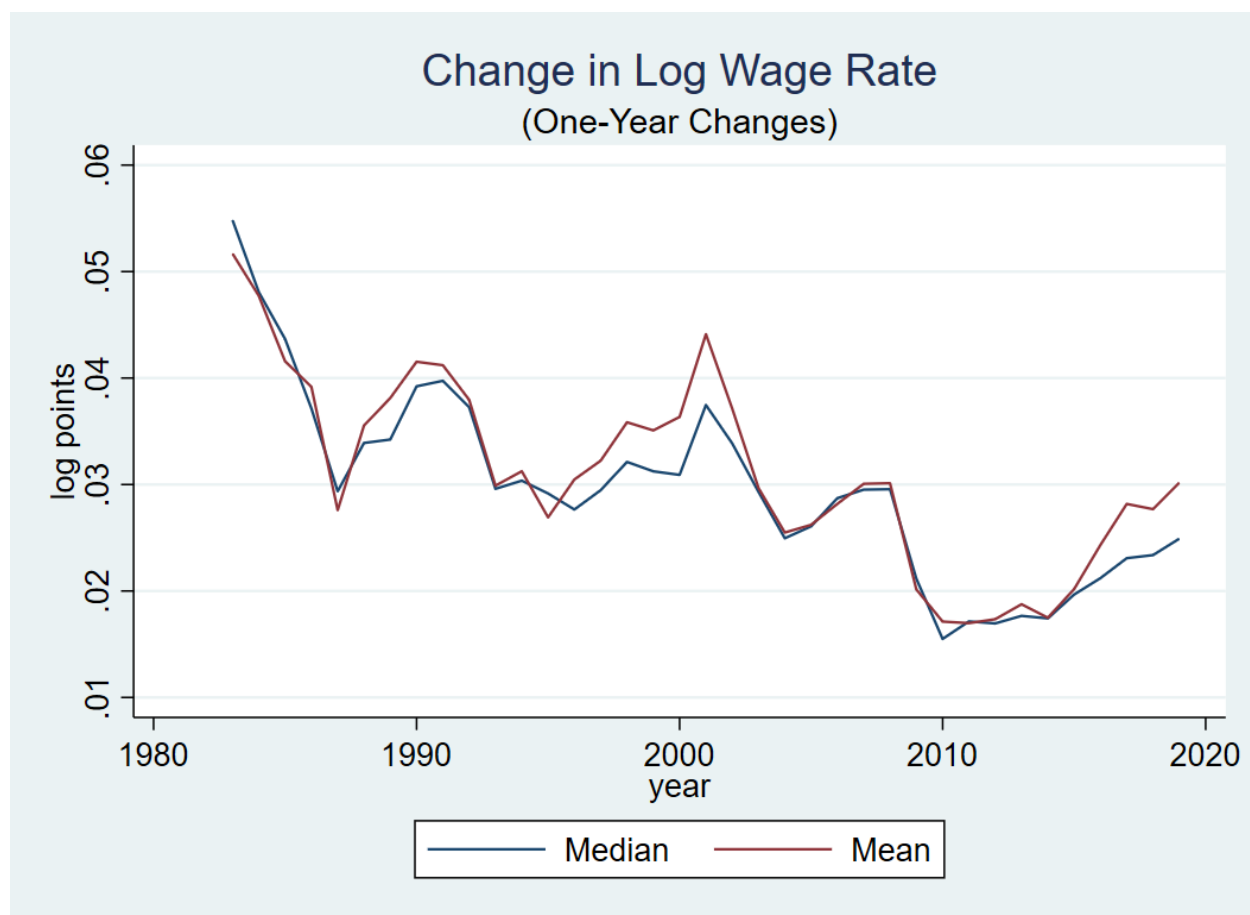
Figure 1

Figure 2
Distribution of 1-Year Wage Changes
(Selected Years)

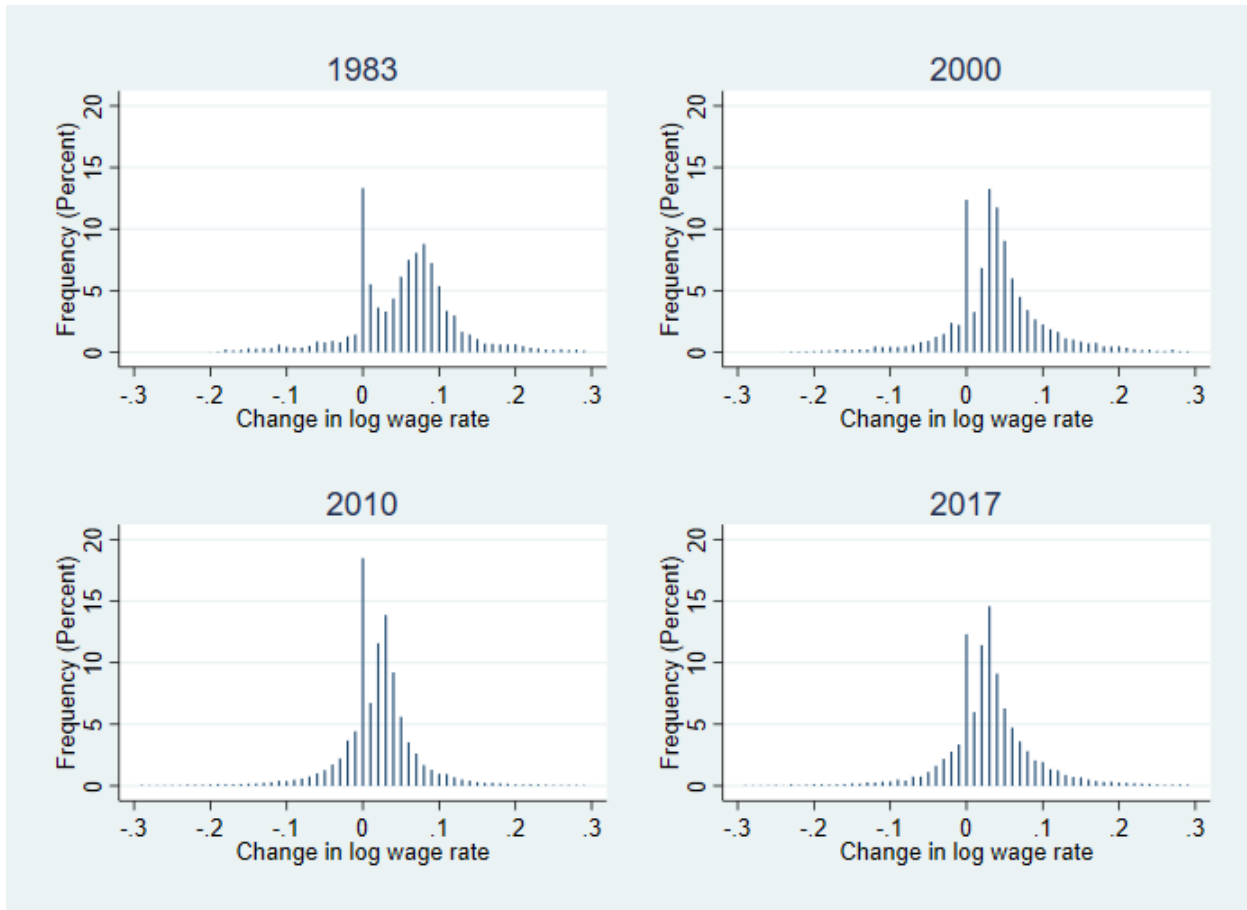


Figure 3
Size-of-Spike and LSW Estimators of Operative Rigidity

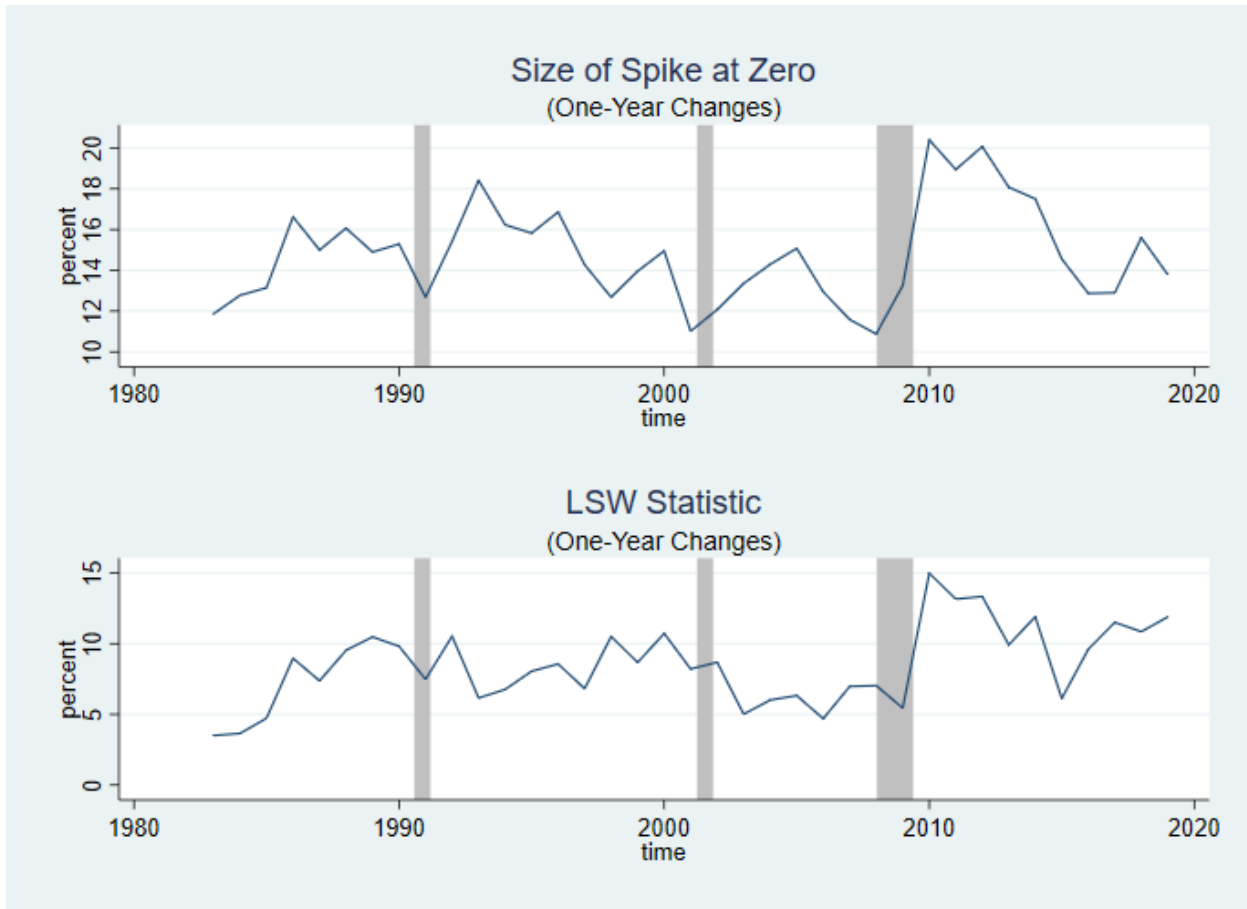


Figure 4
Relative Size-of-Spike and Proportional LSW Estimators of Potential Rigidity

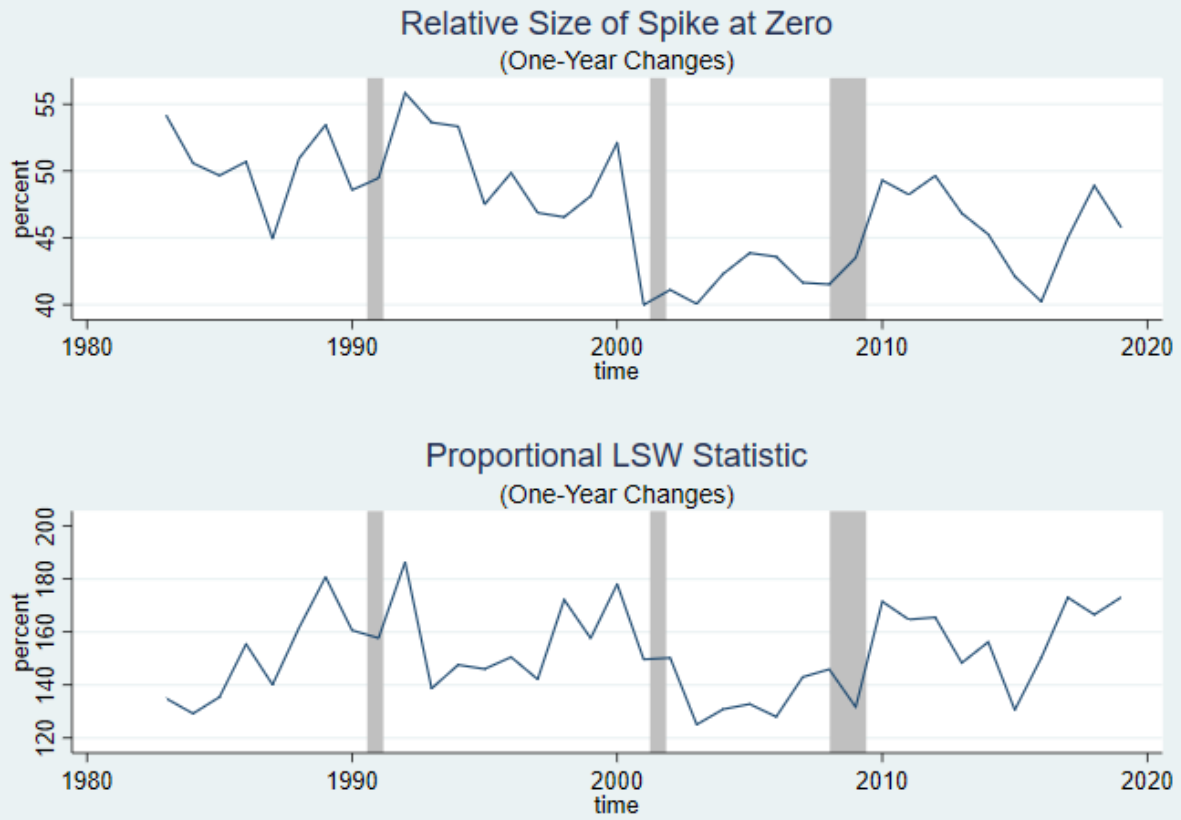


Figure 5
Size-of-Spike and LSW Estimators vs. Wage Inflation

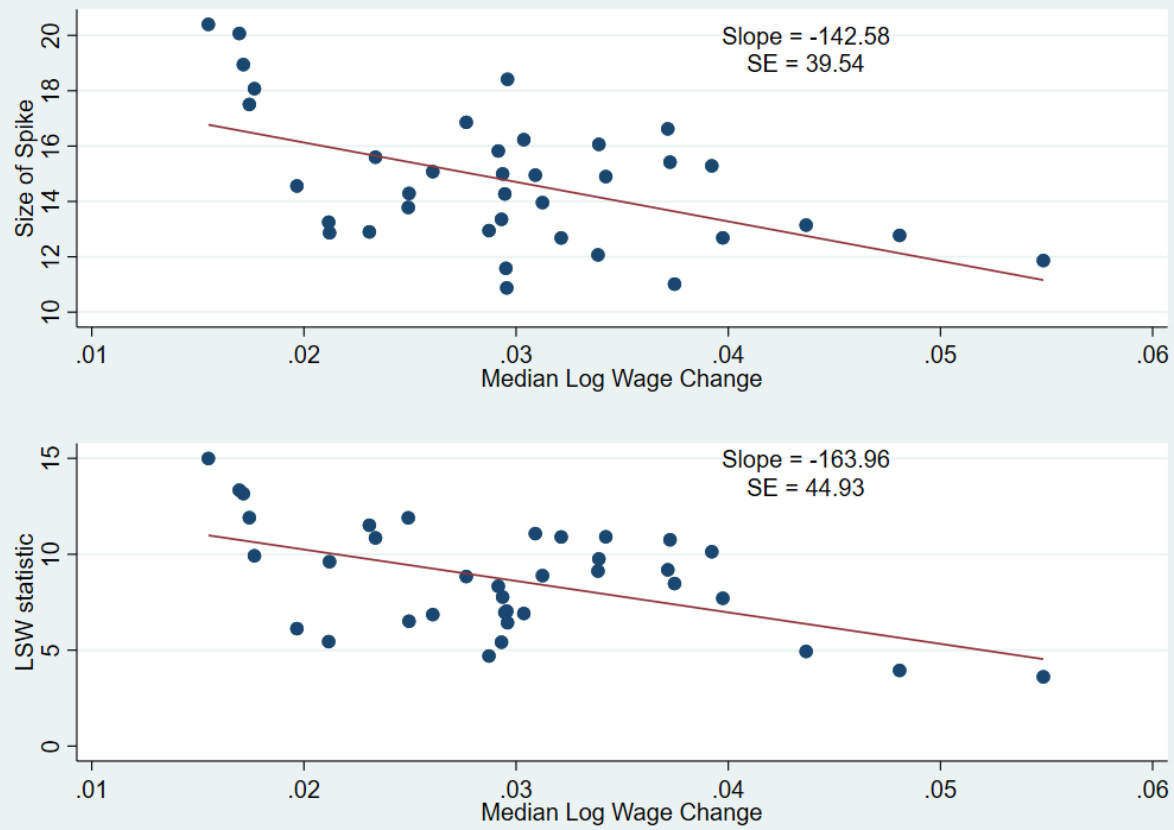


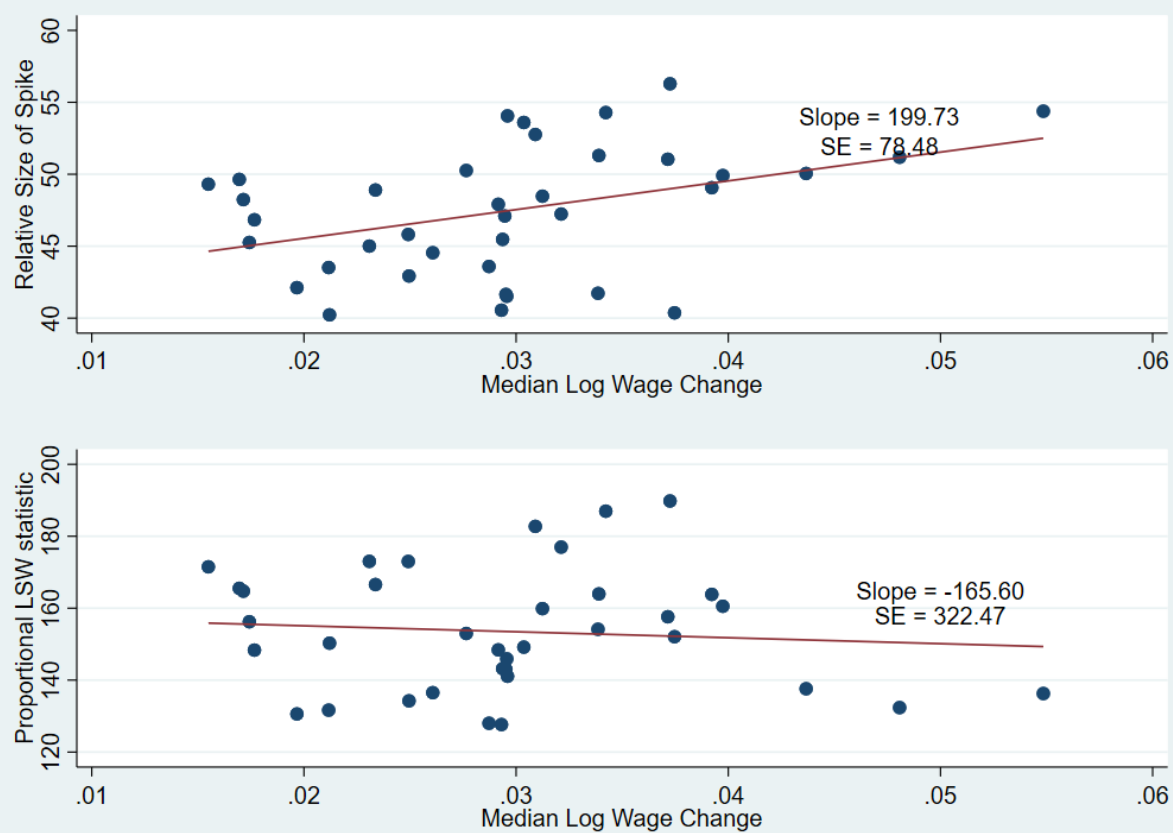
Figure 6**Relative Size-of-Spike and Proportional LSW Estimators vs. Wage Inflation**

Figure 7
Parametric Model
Estimated Notional Densities of Log Wage Changes and Densities with Rigidity

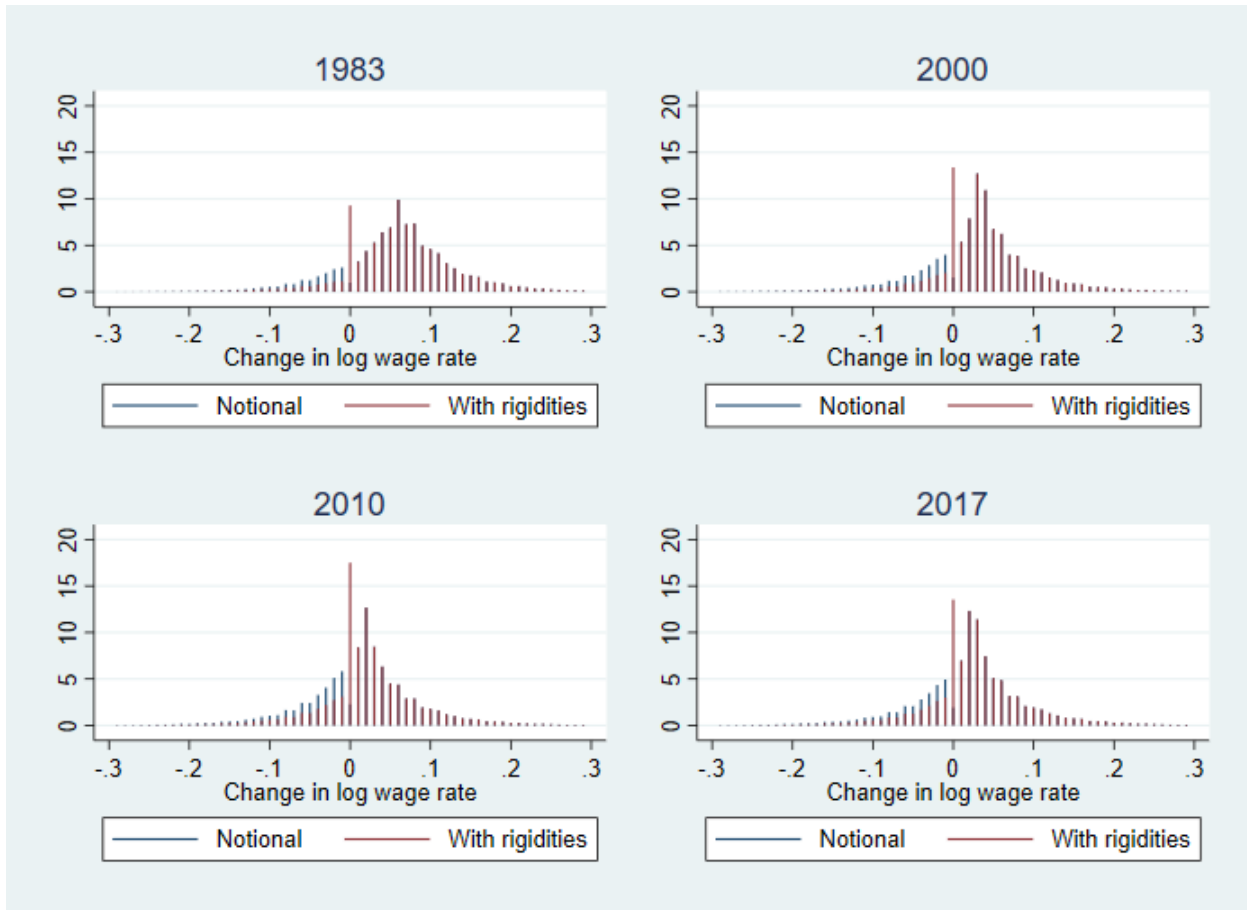


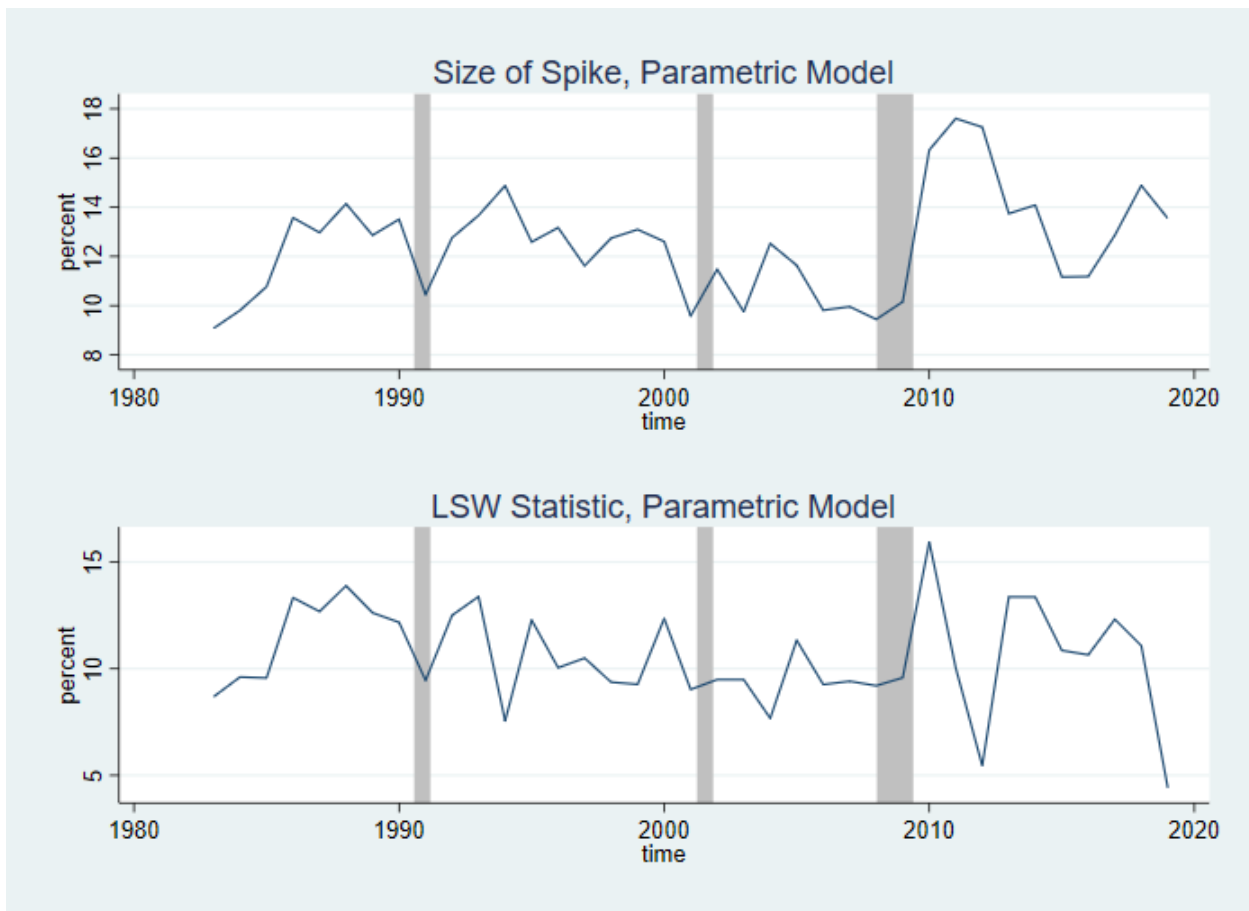
Figure 8

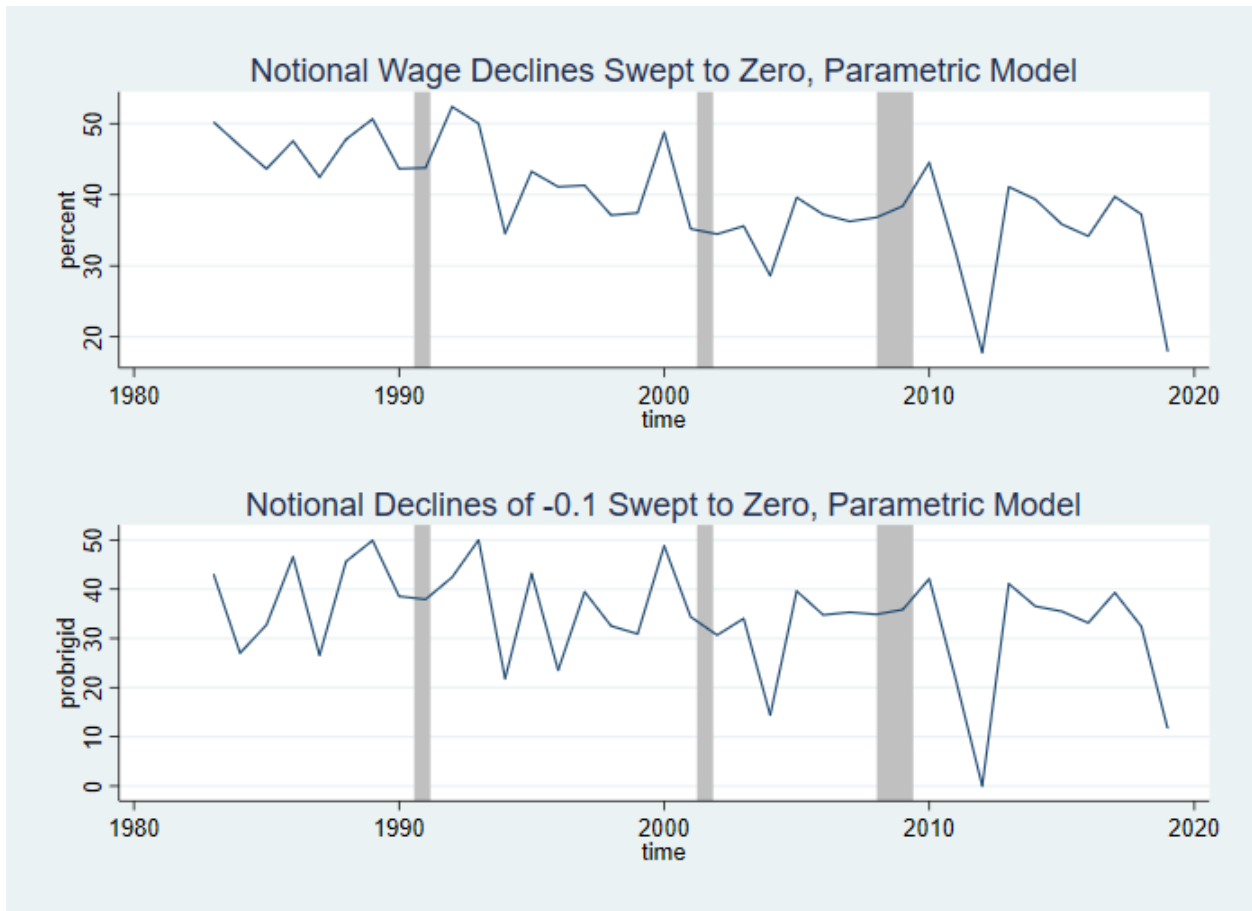
Figure 9

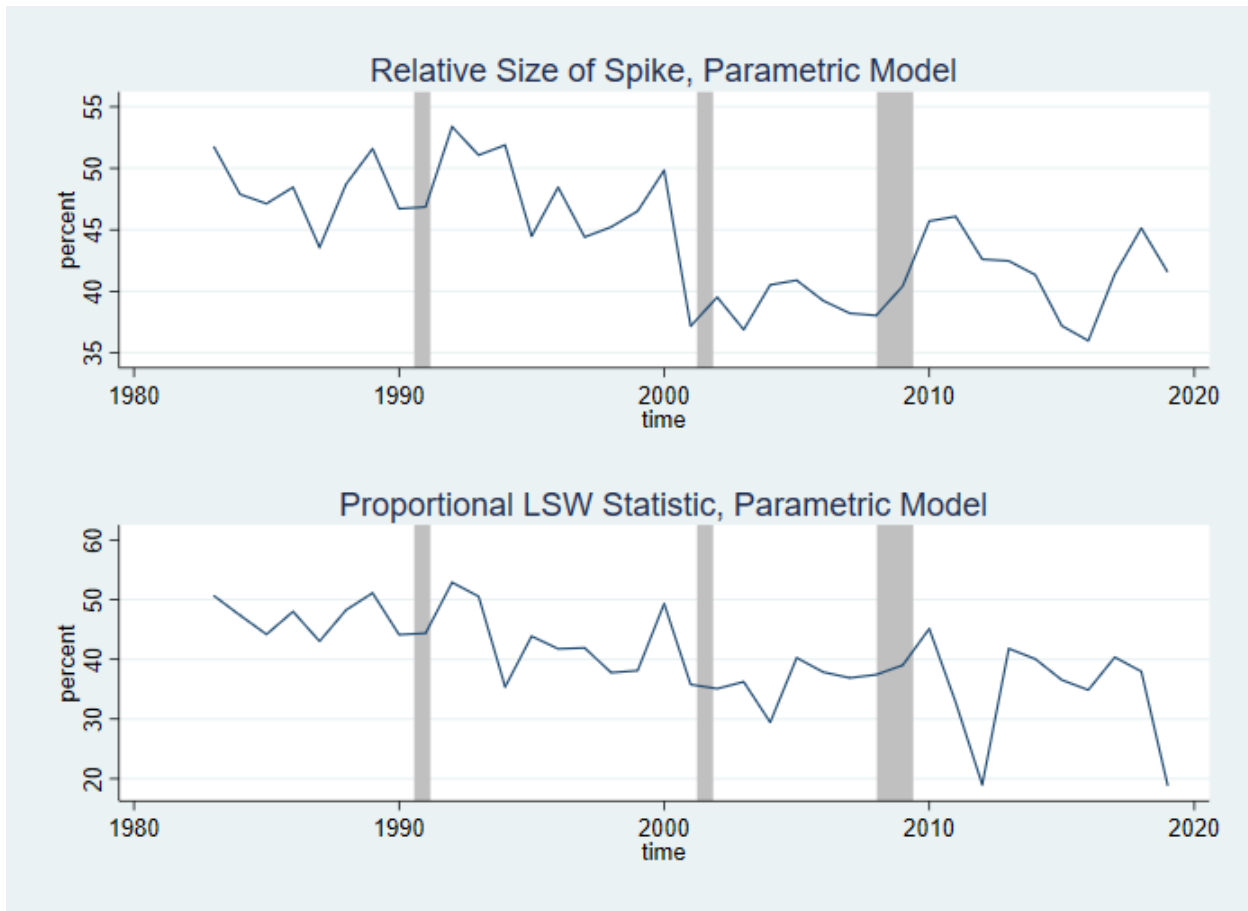
Figure 10

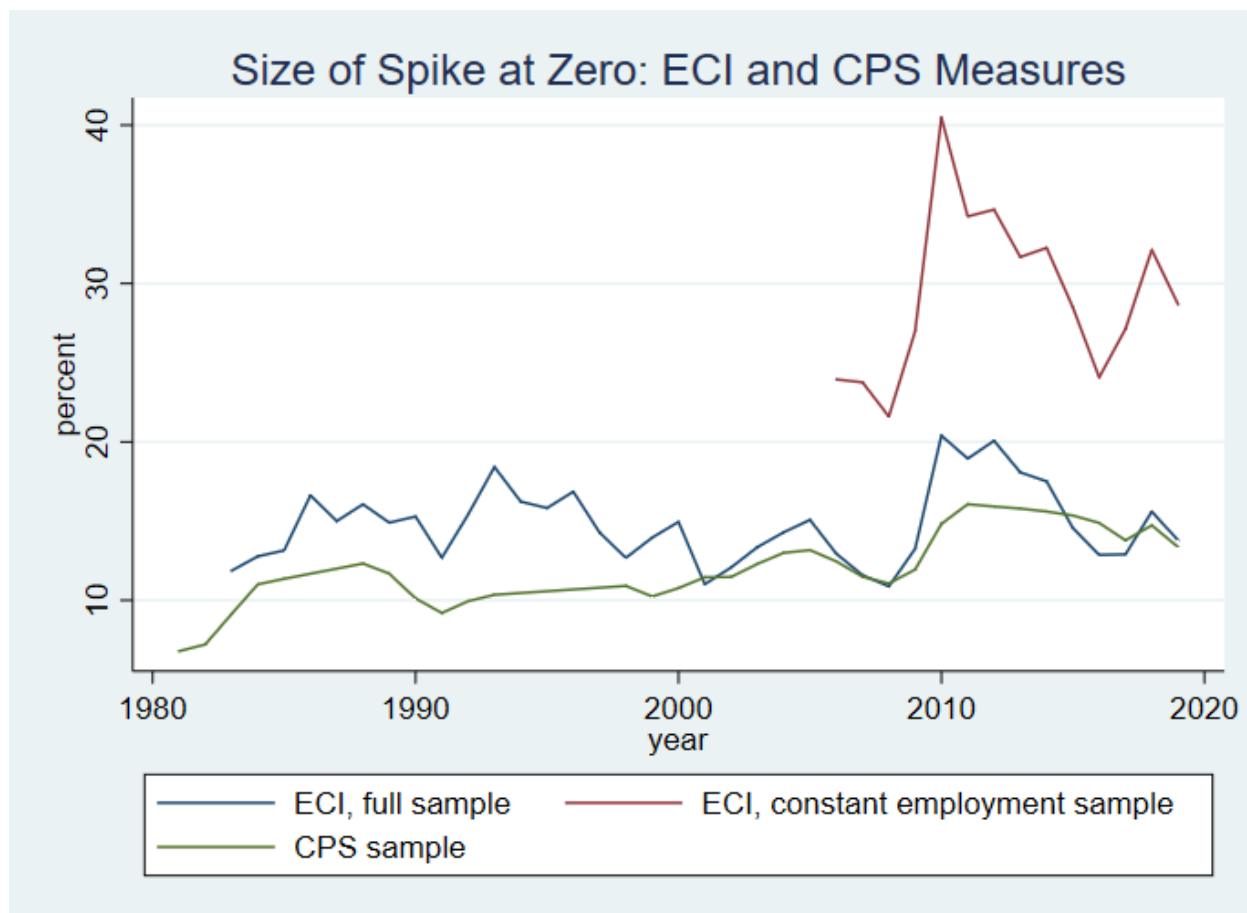
Figure 11

Figure 12
Distribution of 1-Year, 2-Year, and 3-Year Wage Changes
(2010)

