Preliminary and incomplete

Wage Adjustment in the Great Recession

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As of a quarter-century ago, the conventional wisdom among macroeconomists was that real wage rates are more or less non-cyclical, and many macroeconomic models described wage inflexibility as a key contributor to cyclical unemployment. Since then, however, numerous empirical studies based on microdata for workers have found that real wages are substantially procyclical.[1] This procyclicality had been obscured in aggregate wage statistics, which tend to give more weight to low-skill workers during expansions than during recessions. As summarized by Martins, Solon, and Thomas (2012), the microdata-based literature has found that the cyclical elasticity of real wages is similar to that of employment.

Most of the U.S. microdata-based literature, however, is based on data extending no later than the early 1990s. An obvious question is what the cyclical wage patterns have been more recently. Most importantly, how have wages behaved during the Great Recession? Is there reason to think that wages responded especially sluggishly during this downturn and that stickiness of wages contributed to the Great Recession's unusually high unemployment?[2]

This article addresses these questions with data for both the United States and Great Britain. Section I uses March Current Population Survey data to trace U.S. real wage behavior over the 1979-2011 period. Section II uses additional Current Population Survey data to explore the role of inflation and nominal wage stickiness. Section III previews parallel analyses we are conducting for Great Britain with data from the New Earnings Survey. Section IV summarizes our findings.

[1] Examples from the U.S. literature are Stockman (1983), Bils (1985), Solon, Barsky, and Parker (1994), Devereux (2001), Bowlus, Liu, and Robinson (2002), and Shin and Solon (2007). Similar studies for other countries include Devereux and Hart (2006) for Great Britain; Carneiro, Guimarães, and Portugal (2012) and Martins, Solon, and Thomas (2012) for Portugal; and Shin (2012) for Korea.

[2] For example, in a brief Federal Reserve Bank of San Francisco note entitled "Why Has Wage Growth Stayed Strong?" Daly, Hobijn, and Lucking (2012) concluded from aggregate wage data that "Real wage growth ... has held up surprisingly well in the recent recession and recovery," and also found that, "During the recent recession and recovery, the run-up in the fraction of workers subject to downward nominal wage rigidity has been especially large…. This may partly explain why real wage growth has not significantly declined since the onset of the recession in December 2007 and why hiring has been slow since the start of the recovery in mid-2009."

I. Real Wages in the United States, 1979-2011

Our U.S. analyses of real wages are based on the annual March Current Population Surveys (CPS). Every March CPS asks sample members about their annual earnings and employment in the preceding calendar year, so we can measure each worker's hourly wage in the preceding year as the ratio of annual earnings to annual hours of work. Relative to alternative U.S. data sets, the March CPS has three advantages. First, because the Bureau of Labor Statistics (BLS) generates the public use files quickly, we have access to recent data, up through the March 2012 CPS data for 2011 (and dating back in a comparable way to 1979). We therefore have good data for the Great Recession and its immediate aftermath, as well as for earlier recessions, including the similarly severe recession of the early 1980s. Second, the CPS provides large nationally representative samples. Our main analyses of real wages are based on well over 20,000 workers of each gender every year.

Third, access to the microdata goes a considerable way towards reducing the composition-bias issues associated with aggregate data such as the average hourly earnings series from the BLS employer survey. As discussed by Solon, Barsky, and Parker (1994) and others, such aggregate series are constructed as hours-weighted averages of workers' wages, so workers with more employment get greater weight in the statistics. It is well documented that low-skill workers' employment is especially sensitive to cyclical fluctuations, so low-skill workers get less weight in aggregate wage statistics during recessions than they do during expansions. This imparts a countercyclical bias in aggregate wage statistics, making workers' real wage opportunities appear less procyclical than they really are. Thanks to access to the CPS microdata, we can obtain a reliable hourly wage variable for every worker with substantial employment sometime during the calendar year, and we can weight those workers equally instead of weighting them by their annual hours. Unfortunately, though, we cannot avoid composition bias entirely. We cannot measure the wage opportunities of individuals with no work during the calendar year (and, as we will discuss in a moment, for data reliability reasons we also will exclude individuals with very few work hours over the calendar year). We will achieve a partial correction of the resulting composition bias by regression-adjusting our annual wage measures for some observable characteristics (education, potential work experience, and race) of the worker samples. Finally, for workers employed for only part of the year, we measure their wages when they were employed, but we do not observe their wage opportunities during the time they were not employed. Of course, this is an insoluble problem in every data set.

To focus on worker groups with substantial attachment to the labor force, we restrict our real wage analyses to workers between the ages of 25 and 59. Because of extreme outliers (such as the man recorded as having over $400,000 of earnings but only one hour of work in 2008!), we require at least 100 annual hours of work, and we also exclude the cases with the top 1% and bottom 1% of average hourly earnings. The CPS oversamples in less populous states, so for the sake of national representativeness, all our analyses use the provided sampling weights. Our real wage analyses include the imputed wage measures provided for the substantial number of cases with non-response for earnings.

Table 1 displays men's mean and median log real wages by year. Results are shown for two deflators, the personal consumption expenditures (PCE) deflator from the national income accounts and the CPI-U-RS version of the consumer price index. Both are scaled to express real wages in 2009 dollars. The table also displays the annual unemployment rate, to emphasize which years are recession years and which are expansion years. Figure 1 adds a visual display of the real wage series based on the PCE deflator, with shading of the periods of the NBER-dated recessions. In interpreting the latter, it is important to remember that the labor market usually is slack not only during the recessions, but for some time afterwards.

The first thing to note in Table 1 and Figure 1 is the stagnation of men's real wages over the 1979-2011 period. Real wages at the end of the period are much the same as at the beginning. Although this is bad news for us as male workers, it is convenient for us as researchers. With almost no secular trend, it becomes easy to discern cyclical patterns. (The subsequent analysis for women will be a little more nuanced.)

Like previous microdata-based studies, Table 1 and Figure 1 indicate that men's real wages are substantially procyclical. For example, from 1979 to 1983, when the unemployment rate went from 5.8 to 9.6, the mean log real wage based on the PCE deflator fell from 2.933 to 2.889, a reduction of 0.044 (as also shown in Table 2, which provides a 0.005 estimated standard error for the estimated change between 1979 and 1983). With the CPI-U-RS used as an alternative deflator, the estimated wage reduction of 0.057 is even larger. The recession of the early 1990s was much less severe, but still was associated with a large reduction in the mean log real wage. The recession of the early 2000s showed relatively little impact on the labor market, as reflected in either the unemployment rate or the mean log wage.

What interests us most, though, is the experience of the Great Recession. The unemployment rate, which was 4.6 in 2006 and 2007, reached 9.6 in 2010 and was still at 8.9 in 2011. Even though this run-up in the unemployment rate was even greater than that of the early 1980s, the reduction in men's real wages was comparatively modest. The mean log real wage based on the PCE deflator was 3.007 in 2006 and 2007, had dropped to 2.993 by 2010, and declined slightly further to 2.989 in 2011. Compared to the 0.044 reduction from 1979 to 1983, the 0.018 reduction from 2006-2007 to 2011 was significantly smaller (in both the statistical and substantive senses). Similarly, with wages deflated instead by the CPI-U-RS, the 2006-2011 reduction in the mean log real wage was 0.026, as compared to the 0.057 reduction of 1979-1983.

So far we have discussed means, but there is considerable merit in looking at medians as well. For one thing, medians are more robust to outliers. In fact, the medians stay the same regardless of whether we do or do not trim the top and bottom 1% of wage observations. Relatedly, medians sidestep the problem of earnings top-codes in the CPS. Before 1996, top-coded earnings observations were simply assigned the top-code threshold. Since 1996, a top-coded individual has been assigned the sample mean value among all cases above the threshold that share the individual's gender, race, and status vis-à-vis full-time/full-year work. As a result, our means of log real wages before and after 1996 are not altogether comparable. The medians, however, are comparable over time because they are unaffected by the treatment of top-codes.

As can be seen in Tables 1 and 2, our medians tell much the same story as the means. From 1979 to 1983, the median log real wage based on the PCE deflator decreased by 0.056, and the one based on the CPI-U-RS decreased by 0.069. In contrast, from 2006 to 2011, the one based on the PCE deflator decreased by only 0.026, and the one based on the CPI-U-RS fell by only 0.032. The medians, like the means, indicate that real wages are considerably procyclical, but the procyclicality of men's real wages has been significantly milder in the Great Recession than in the recession of the early 1980s.

The cyclical patterns in these mean and median wage series are subject to a countercyclical composition bias because our sample selection criterion requiring at least 100 annual hours of work disproportionately screens out low-wage workers during recessions. We can partially correct for that bias by controlling for year-to-year changes in the demographic composition of our samples. For example, as shown in Table 2, in addition to showing mean log wages for each year in the 1979-1983 period, we also estimate "regression-adjusted" year effects by applying least squares (again weighting by the provided CPS sampling weights) to a regression of individual workers' log real wages on year dummies for 1980, 1981, 1982, and 1983 (with 1979 as the omitted reference category) and controls for years of education, a quartic in potential work experience (ages minus years of education minus 6), and race dummies.[3] As expected, the regression-adjusted year effects show even more wage procyclicality. Whereas the unadjusted means indicate that log real wages were 0.044 lower in 1983 than in 1979, the adjusted 1983 year effect is 0.059 less than the 1979 effect.

We perform the same exercise for the 2006-2011 period. We estimate each period's regression separately because it is implausible that the coefficients of the control variables would come close to holding still over the entire 1979-2011 period. For example, our estimated coefficient of education is 0.072 (with estimated standard error 0.0005) for the 1979-1983 period, but it is 0.111 (0.0004) for 2006-2011. Whereas the unadjusted means indicate that log real wages were 0.018 lower in 2011 than in 2006, the adjusted 2011 year effect is 0.032 less than the 2006 effect. Again, however, this wage drop in the Great Recession is significantly smaller than the wage decrease during the early 1980s recession.

For the same reasons it was worthwhile to calculate medians along with means, it makes sense to estimate median regressions as well as mean regressions. In the last column of Table 2, we report the estimated year effects from applying weighted least absolute deviations to the regression of log real wages on year dummies and control variables. The results indicate again that men's real wages decreased during the Great Recession, but not as much as in the recession of the early 1980s.

In most instances, the regression adjustments indicate that accounting for *observed* heterogeneity reveals greater procyclicality in real wages. Presumably, accounting for *un*observed heterogeneity would move further in the same direction. The traditional approach to accounting for unobserved heterogeneity in the microdata-based literature on real wage cyclicality is to control for worker fixed effects by tracking the same workers over time in a

[3] We use the method of Jaeger (1997, last column of Table 2) to construct a consistent education variable over time. The race categorization we are able to construct for the full time period consists of three categories: white, black, and other.

panel survey. Although the rotating panel design of the CPS makes it possible to follow a portion of one March's sample to the next March, the CPS is not ideal for longitudinal analysis because it does not follow residential movers, and exclusion of movers is an endogenous sample selection in a study of wage changes.

Nevertheless, as a further check on our results from repeated cross-sections, we perform year-to-year matches from our samples of workers between adjacent March Current Population Surveys. We follow the guidance of Madrian and Lefgren (2000) in verifying that longitudinal matches on identification numbers are true matches by requiring that gender and race also match, that year-to-year change in reported age is between -1 and 3, and that the respondent reports living in the same residence as in the previous March. We are able to match between most adjacent March surveys during our sample period, but not for 1985-1986 or 1995-1996 because of changes in household identifiers, and not for 1984-1985 because the residence question was not asked in 1985. We therefore cannot obtain longitudinal wage changes for 1983-1984, 1984-1985, or 1994-1995. For all other pairs of years in our sample period, the left side of Table 3 shows weighted sample means of men's year-to-year change in log real wages.[4] We weight individuals by the simple average of their sampling weights across the two years.

The first thing to notice is that, because of normal life-cycle wage growth, the mean changes are almost always positive, and tend to be substantially positive in expansion years (for example, averaging about 0.03 from 1985 to 1989 and about 0.04 from 1995 to 2000). In stark contrast, the longitudinally matched men experienced virtually zero average real wage growth during the recessions of the early 1980s and early 1990s. Again, as we observed in our previous analyses, the mild recession of the early 2000s had little impact on the labor market. Finally, over the years 2006-2011, real wage growth averaged about 0.01, less than in typical expansion years, but more than in the recessions of the early 1980s and early 1990s. Thus, as in our analysis of repeated cross-sections, men's real wages continued to follow a procyclical pattern in the Great Recession, but to a lesser extent than one might have expected from earlier recessions.

Table 4 shows mean and median log real wages by year for women, as Table 1 did for men. And Figure 2 provides a visual display for women, as Figure 1 did for men. Where Table 1 and Figure 1 documented stagnant real wages for men, Table 4 and Figure 2 corroborate the

[4] We also have tried estimating regressions with controls for polynomials in age or potential experience, but the variation over time in the estimated year effects is virtually identical to that for the unadjusted means shown in Table 3.

well-known rise in women's wages during the 1979-2011 period. All our measures suggest that, over the period as a whole, women's real wages rose at a rate of close to 0.10 per decade.

This upward secular trend in women's wages makes it trickier to distill the cyclical patterns. Nevertheless, inspection of Table 4 and Figure 2 reveals a clear tendency for women's real wages to rise more slowly during recessions. And in the Great Recession in particular, women's real wage growth appears to have stalled out completely. The relatively large effect that the Great Recession appears to have had on women's wages stands in contrast to its effect for men, which was smaller than in the recessions of the early 1980s and early 1990s. But a conclusive judgment on this will require additional years of data because a possible reading of Figure 2 is that women's real wage growth was starting to peter out *before* the Great Recession. If the upward trend in women's wages resumes in the years to come, the cyclical impact of the Great Recession will appear large. If it does not resume, the stalling-out will be interpreted instead as a change in secular trend.

Table 5 highlights the effects of the two most severe recessions on women's wages, as Table 2 did for men's wages. In addition to showing the relative movements in mean and median log wages, the table also presents regression-adjusted series that account for variation in the samples' education, potential experience, and race. As in Table 2 for men, these adjustments suggest even greater procyclicality in real wages. After adjustment, there appears to be virtually no real wage growth for women during the recession of the early 1980s, and negative growth during the Great Recession.

The right side of Table 3 shows the mean year-to-year changes in log real wages for women longitudinally matched between adjacent March Current Population Surveys. The combination of life-cycle wage growth and the upward secular trend in women's wages causes the mean log change to be positive in every single year. In 20 of the 29 pairs of years that can be matched, the growth rate in real wages is higher for women than for men. Again, the procyclicality of women's real wages is manifested as a tendency for slower growth in recession years than in expansion years. All four of the smallest increases in women's real wages are in recession years, three of them in the Great Recession.

To summarize, our evidence for 1979-2011 from March Current Population Surveys corroborates and updates the findings from earlier microdata-based studies that real wages in the United States are substantially procyclical. For men, however, we find that real wages took a smaller hit in the Great Recession than might have been expected from the experience of earlier recessions. Our results for women are less clear-cut because of the confounding of cyclical and trend variation, but a tentative impression is that women's real wages may have taken a relatively large hit in the Great Recession.

II. Nominal Wages and Inflation in the United States

The previous section discussed the mean log real wage series plotted for men in Figure 1 and for women in Figure 2. Of course, the mean log real wage is the difference between the mean log nominal wage and the log of the price level (measured in these figures with the PCE deflator). Both of these component series are plotted in Figures 1 and 2 along with their difference, the log real wage series. The scale for the two log wage series is on the left axis, and the scale for the log deflator is on the right axis.

In both figures, nominal wages grow substantially over time. In the case of men, long-run nominal wage growth is similar to the growth in the price level, so real wages show little secular trend. For women, nominal wage growth exceeds price inflation, so real wages trend substantially upwards.

As discussed in the previous section, men's real wages declined considerably during the recessions of the early 1980s and early 1990s. Figure 1 shows that nominal wages grew during those recessions, but more slowly than the price level. In the Great Recession, men's real wages seemed to decline less and more belatedly than in the recessions of the early 1980s and early 1990s. Nominal wages grew very little, but so did the price level. In 2009, when the unemployment rate was 9.3 percent, inflation as measured by the annual PCE deflator was virtually zero (and was slightly negative according to the CPI-U-RS). With no decline in nominal wages that year, real wages did not decline either. After 2009, inflation ran at about 2 percent a year, and the even smaller growth in nominal wages meant that real wages declined a little.

As also discussed in the previous section, the upward secular trend in women's real wages makes the discernment of cyclical patterns somewhat more challenging. In the recessions of the early 1980s and early 1990s, women's real wages grew, but more slowly than in the expansions of the late 1980s and late 1990s. During the Great Recession, women's real wage growth stalled out altogether, seemingly implying a larger cyclical impact on real wages. A clear separation of the cyclical and trend movements, however, will have to await more years of data in the aftermath of the Great Recession.

At least for men, that the Great Recession appeared to reduce real wages less and more belatedly than in previous recessions suggests a possible role for the inflationary environment. At the outset of the recession of the early 1980s, inflation was unusually high, and employers could reduce real wages substantially even while granting nominal wage increases. This was still somewhat true in the recession of the early 1990s, when annual inflation was about 4 percent. But during the Great Recession, especially in 2009, the inflation rate was lower, and substantial real wage cuts would have required nominal wage cuts. Economists going back at least to Keynes (1936) have suggested that resistance to nominal wage cuts can constrain the response of real wages to slack labor demand, and that this wage stickiness might exacerbate rising unemployment during recessions.

This possibility that downward stickiness in nominal wages can impede wage adjustments to negative labor demand shocks has led numerous researchers (for example, McLaughlin, 1994; Card and Hyslop, 1996; Kahn, 1997; Altonji and Devereux, 1999; Elsby, 2009; and Daly, Hobijn, and Lucking, 2012) to examine longitudinal microdata to assess the prevalence of nominal wage stickiness in the United States. Because it is obvious that job changers typically experience wage changes, most of these researchers have focused on the more interesting question of whether workers staying with the same employer appear to experience nominal wage stickiness. What would be more interesting still would be to ascertain how many workers *lose* their jobs and become unemployed because of downward stickiness in nominal wages, but no one knows how to do that. Instead, the implicit assumption in this literature is that, if downward rigidity in nominal wages is sufficiently common to cause a lot of job losses, it also should be common among workers that stay employed with the same employer. In this section, we use longitudinally matched data from Current Population Surveys to extend this literature and update it to include the Great Recession.

Our analysis begins with the Current Population Surveys of January 1981, January 1983, January 1987, January 1991, February 1998, February 2000, January 2002, January 2004, January 2006, January 2008, January 2010, and January 2012. Each of these waves of the CPS included a job tenure supplement, which enables us to determine whether a worker had been employed for at least a year with the worker's current main employer.[5] We focus on such workers in their eighth (and last) month in the CPS because workers in that "rotation group" also were asked to report their current nominal wage rate. Using the same method for longitudinal matching described in Section I, we matched these workers to their data in the CPS one year earlier, when these workers were in their fourth month in sample. The fourth rotation group is the other "outgoing" rotation group asked to report a current nominal wage rate, so we are able to obtain an empirical distribution of year-to-year nominal wage growth of stayers for January 1980-January 1981, January 1982-January 1983, …, January 2011-January 2012. Fortunately, these matches include at least one year-to-year change from every recession from the 1980s on, as well as several expansion years.[6]

Like many previous studies, we present histograms of the empirical distribution of year-to-year nominal wage changes. Our sample for each year-to-year match pools women and men between the ages of 16 and 64 in both years. We exclude observations for which wages were imputed on account of non-response. As in our analyses of real wages, we use the provided sampling weights to adjust for the Current Population Survey's oversampling of less populous states (though, in practice, this turns out not to affect the results much).

For each year-to-year match, we display two histograms – one in Figure 3 for workers paid by the hour in both years, and one in Figure 4 for workers not paid by the hour in either year. For the former, we use the reported hourly wage rate. For the latter, we follow Card and Hyslop (1996) in using the reported usual *weekly* earnings. In Card and Hyslop's words, "In principle, we can construct an hourly wage for non-hourly-rated workers by dividing usual weekly earnings by usual weekly hours. However, any measurement error in reported hours will lead to excessive volatility in imputed hourly wages."

Each of our histograms features a thin spike at zero. This shows the percentage of the workers that reported the exact same wage in both years. For the hourly workers, in some years we display a pink segment of the zero spike, which represents workers whose wage was at the national minimum wage in both years. The relative value of the minimum wage became so low

[5] The appendix in Farber (2008) provides a valuable summary of the availability and content of CPS job tenure supplements over time.

[6] Card and Hyslop (1996) and Daly, Hobijn, and Lucking (2012) also use longitudinally matched CPS data to measure nominal wage change distributions, Card and Hyslop for 1979-1993 and Daly et al. for 1980-2011. Unlike us, they use all CPS months, not just those with job tenure supplements. As a result, they have many more observations, but their identification of stayers assumes that workers staying in the same industry and occupation also stayed with the same employer.

during our sample period, however, that there are no such cases in many years. The next bin to the right of the zero spike contains workers whose change in log nominal wage was positive but no greater than 0.02; the next bin contains those whose change in log nominal wage was greater than 0.02 and less than or equal to 0.04; etc. The bins to the left of zero are constructed symmetrically. To limit the histograms to a readable scale, we pile up workers with change in log nominal wage greater than 0.64 in the rightmost bin and those with change less than -0.34 in the leftmost bin. Some of the salient statistics from the histograms are summarized in Table 6.

In general, our histograms display several features noted by previous authors. First, there always *is* a spike at zero. As shown in Table 6, for each type of worker in each pair of years, a non-trivial minority of workers – ranging between 6 and 20 percent – reports the exact same nominal wage in both years. It is unclear in which direction these estimates are biased by reporting error. On one hand, a worker with the same true nominal wage in both years may be recorded as changing wages if the worker misreports the wage in either year. On the other hand, a worker with a modest wage change may round to the same number in both years and thus appear to have zero wage change. For example, a worker whose true nominal hourly wages were $19.80 last year and $20.30 this year may report an hourly wage of $20 in both years. As will be discussed in the next section, studies of nominal wage stickiness in Great Britain have exploited richer data to suggest that the latter bias may dominate.

Second, the distribution of nominal wage changes is always centered on positive nominal wage growth. This is to be expected both because of keeping pay raises up with price inflation and because of exceeding price inflation in order to achieve normal life-cycle growth in real wages.

Third, there is always a non-trivial fraction of workers reporting nominal wage reductions. As shown in Table 6, this fraction always exceeds 10 percent for hourly workers and always exceeds 20 percent for non-hourly workers. Again it is unclear what proportion of these cases is an artifact of reporting error, but again the next section's discussion of British evidence will be instructive.

An advantage of having these distributions for many years is the opportunity to relate the patterns to variations in the economic environment. In particular, there is good reason to expect the distributions to be affected by both business cycle conditions and the inflation rate. We can illustrate with a very simple model. Suppose that workers always stay employed with the same employer and that, in the absence of any tendency towards nominal wage stickiness, the process determining worker i's real wage growth between years $t-1$ and t would be

$$\Delta \log(W_{it} / P_t) = \beta_0 + \beta_1 C_t + \varepsilon_{it}$$

where W_{it} is the worker's nominal wage rate, P_t is the price level, C_t is a measure of business cycle conditions, $\beta_1 > 0$ if both C_t and real wage growth are procyclical, and, conditional on C_t and P_t, ε_{it} is normally distributed with mean zero and variance σ^2. Then the probability that the worker's nominal wage growth would be negative would be the probit function

$$\begin{aligned}\text{Prob}(\Delta \log W_{it} < 0) &= \text{Prob}(\varepsilon_{it} < -\beta_0 - \Delta \log P_t - \beta_1 C_t) \\ &= \Phi[-(\beta_0 / \sigma) - (1/\sigma)\Delta \log P_t - (\beta_1 / \sigma) C_t]\end{aligned}$$

where $\Phi(\ )$ is the standard normal cumulative distribution function. Now add the further extreme assumption that, because of downward rigidity of nominal wages, everyone that otherwise would have negative wage growth instead has zero nominal wage growth. Then the proportion of workers with zero nominal wage growth would be the probit function above, which clearly is negatively related to both the inflation rate and the business cycle indicator. Although the model is extreme, it does give a clear illustration of why many would expect the spike at zero in the histograms to be highest when inflation is low and the economy is in recession.

Indeed, Table 6 and the histograms show that there is something to that expectation. The highest spikes do appear during the Great Recession, when unusually weak demand coincided with a low inflation rate. The lowest spike appears in 1980-1981, when the inflation rate was about 10 percent and unemployment was rising but had not reached the high level of 1982-1983. In addition, there appears to be somewhat of an upward secular trend in the frequency of zero nominal wage change, which conceivably might be a gradually evolving response to a prolonged stretch without high inflation.[7]

[7] These patterns are broadly consistent with those reported in the CPS analyses by Card and Hyslop (1996) and Daly, Hobijn, and Lucking (2012), but two differences stand out. First, unlike Card and Hyslop and ourselves, Daly et al. divide reported weekly earnings by reported weekly hours to get their nominal wage measure for non-hourly workers. As expected, this leads to considerably smaller spikes at zero nominal wage change. Second, for hourly

Although our results, like the previous literature, do show some evidence of nominal wage stickiness, we are reluctant to leap to the conclusion that downward nominal wage rigidity has been a major *cause* of the Great Recession's extraordinarily high unemployment. First, the spikes at zero nominal wage change that we measure during the Great Recession are only moderately greater than the ones we measure for earlier in the 2000s. Second, it is important to remember that the statistics in analyses like ours are for workers that stayed with the same employer for at least a year. These tend to be the "primary labor market" workers, for whom various types of specific human capital foster long-term employment relationships. Our analysis mostly excludes workers in the "secondary labor market," where specific human capital is mostly absent and labor turnover is high. There is little theoretical reason to expect wage rigidity in the secondary sector, and Bewley's (1999) anecdotal evidence from extensive interviews with employers during the recession of the early 1990s corroborates the expectation of wage flexibility in the secondary sector.

On the other hand, Bewley's interviews also dovetail with our quantitative indications that primary-sector employers are reluctant to cut incumbent workers' nominal wages. But a long history of economic analysis, dating back at least to Becker (1962), questions whether current wages in long-term employment relationships are "allocative." Rather, current wages can be seen as installment payments within a longer-term compensation package. Even when a long-term employee's current wage exceeds her current value of marginal product, it is in the employer's interest to retain the worker as long as continuing her employment is profitable in present-value terms. Consequently, even in the face of evidence that there exists some stickiness in current nominal wages, it does not follow that such wage stickiness necessarily must generate inefficient job separations.

Finally, even if one were to assume that current wages are indeed allocative, there still are reasons for caution in ascribing a prominent role to downward wage rigidity in the Great Recession. In a world where firms are unable to cut the wages of their incumbent workers, it is natural to expect that a recession would induce many more of these employment relationships to become unprofitable, and would give rise to a commensurate burst of layoffs. The behavior of the quantity side of the U.S. labor market during the Great Recession has been documented in

workers, Daly et al. estimate a substantial dip in the frequency of zero nominal wage change in the years preceding the Great Recession. In contrast, our estimates in Table 6 do not show a drop-off after 2003-2004.

detail by Elsby, Hobijn, and Sahin (2010). They show that, while layoffs rose sharply during the recent downturn, the magnitude of the rise was comparable to that seen in prior severe recessions, notably the early 1980s episode (see, for example, their Figure 9). Thus, initial evidence appears to be weak for a simple story in which the combination of downward stickiness in nominal wages and low inflation has generated high unemployment through excessive rates of job loss.

None of this is meant to deny the obvious – that the Great Recession has been a terrible economic downturn with painful consequences for many workers. Rather, we are saying that the existence of some nominal wage stickiness is not in itself *prima facie* evidence that such wage stickiness has caused an epidemic of economically inefficient choices by employers and employees. It is conceivable that the high unemployment of the Great Recession would have been nearly as high even in a parallel universe with absolutely flexible wages.

III. Wages in Great Britain, 1975-2011

Our analyses of real and nominal wages in Great Britain are based on the New Earnings Survey (NES). These are the same data used by Devereux and Hart (2006) to study real wage cyclicality and by Nickell and Quintini (2003) to study nominal wage rigidity. Devereux and Hart's sample period ends at 2001, however, and Nickell and Quintini's ends at 1999. We are able to replicate both analyses and update them to 2011.

Besides the obvious substantive merit of studying wage adjustment in Great Britain in addition to the United States, there is a methodological bonus – the NES data are superior to available U.S. data in several ways. First, the NES sample sizes are large. The survey is based on a one-percent sample of British income taxpayers, defined by individuals whose National Insurance numbers end in a given pair of digits. The resulting sample covers about 160,000 workers each year. Second, since the sample frame consistently has been based on the same pair of National Insurance number digits, the survey naturally has a panel structure. Third, the survey's wage information is unusually accurate. The survey is administered to *employers*, which are required by law to respond to the survey. The information on earnings and work hours elicited from the employers pertains to payroll information for a reference week in April. Because the earnings and hours data come from payroll records, they are thought to be much more accurate than similar data gathered from household surveys.

It has been only a few days since we received clearance from the U.K. Data Service to disclose the results from our analyses of the NES microdata, so we are unable to complete a detailed account in time for the NBER conference's April 26 submission deadline. Instead, we now provide a brief preview of our main findings. We hope to complete a detailed version of this section before the May 17-18 conference.

By and large, our analysis of real wages corroborates Devereux and Hart's (2006) finding of substantial procyclicality. But, as in our analysis for the United States, we also find considerable variation across different recessions, and this variation for Great Britain is almost the reverse of that for the United States. In the recession of the early 1980s, real wage growth in Britain slowed only slightly even though that recession was especially severe and the inflation rate then was even higher in Britain than in the United States. The recession of the 1990s, which also was quite severe in Britain, was accompanied by a more noticeable slowing of real wage growth. And the Great Recession, which brought a smaller increase in unemployment in Britain than in the United States, also brought a remarkable reduction in real wages for both men and women.

The greater accuracy of the payroll-based NES wage data is especially valuable in the analysis of year-to-year changes in nominal wages. It will be instructive here to begin by reviewing two previous British studies of nominal wage rigidity. Smith (2000) used the 1991-1996 waves of the British Household Panel Study (BHPS) to retrace the steps of the U.S. literature described in our previous section. Whereas most U.S. researchers have attempted to restrict their samples to workers staying with the same employer, Smith further restricted to workers who reported staying in the same job with the same employer. If anything, one would expect that difference to lead to a higher frequency of zero nominal wage change. Her initial results turned out to be fairly similar to those from U.S. household surveys – she found that 9 percent of stayers experienced zero nominal wage change from one year to the next, and that 23 percent experienced nominal wage reductions.

But then she exploited a remarkable feature of the BHPS data – respondents were told they could consult their pay slips when answering the wage questions, *and* the survey recorded who did so. When Smith restricted her sample to those who did check their pay slips in both years, the proportion with zero nominal wage change fell to 5.6 percent. As we mentioned in the previous section, it is *ex ante* unclear in which direction reporting error would bias the estimation of the spike at zero nominal wage change. Purely classical measurement error would bias the estimation downward, but rounding error could go the other way. For example, a worker whose hourly wage was $19.80 last year and is $20.30 this year might round to $20 in both years and get coded as experiencing zero change. Smith's results suggest that, on net, nominal wage change distributions from household surveys *over*estimate the proportion of stayers with zero nominal wage change. Unsurprisingly, she also found that, among the respondents who did check their pay slips, the proportion reporting nominal wage reductions was somewhat smaller. But it was still quite substantial, at almost 18 percent. In combination, Smith took these results as showing that nominal wages are considerably more flexible than economists had believed. To quote her striking summary, "Some of the results in this paper may seem difficult to believe – the quite common occurrence of nominal pay cuts, for example. It may well be that the difficulty in believing them stems not from the weight of contradictory evidence, but rather from conventional wisdom that has survived because of the previous lack of evidence either way."

Smith's study was following by Nickell and Quintini's (2003) study based on the New Earnings Survey data for 1975-1999. Like Smith, Nickell and Quintini focused on workers staying in the same job with the same employer. Nickell and Quintini began by comparing their 1991-1996 nominal wage change measures, based on employers' payroll-based reporting, to Smith's household survey measures for the respondents who consulted their pay slips. The results from the two sources line up quite closely with each other. More generally, over their full 1975-1999 sample period, Nickell and Quintini found that there was regularly a noticeable spike at zero nominal wage change, but that it was much smaller than usually found in household surveys. In most years, the proportion of stayers with zero nominal wage change was less than 3 percent, with the highest proportion being 7.1 percent in 1992-1993. And despite the presumed accuracy of the employers' wage reports, the proportion of stayers with nominal wage reductions from year to year was substantial, ranging from a low of 5 percent in 1979-1980 (when the inflation rate was about 20 percent) to a high of 22 percent in 1996-1997. Nickell and Quintini concluded, "Despite the substantial numbers of individuals whose nominal wages fall from one year to the next, we find that there is evidence of some rigidity at zero nominal wage change. While the effect is statistically significant, the macroeconomic impact of the distortion is very modest."

Our analysis of the NES data updates Nickell and Quintini's analysis to 2011. First, we also find relatively small spikes at zero nominal wage change, ranging from a low of 0.4 percent in 1979-1980 to a high of 7.4 percent in 2009-2010. In most years, the spike is less than 3 percent. As in previous research for both Britain and the United States, these spikes vary over time with respect to inflation and business cycle conditions in the ways that one would expect. Second, we replicate and update the finding of substantial proportions of job stayers experiencing negative nominal wage changes. Our estimates range from a low of 5 percent in 1979-1980 to a high of 24 percent in 2009-2010. From 1993-1994 on (when the inflation rate typically has been about 3 percent), the proportion experiencing nominal wage cuts regularly has run in the neighborhood of 20 percent.

Like previous researchers, we are struck by the surprising degree of wage flexibility revealed by the British data. In addition, we wonder whether the greater frequencies of zero year-to-year nominal wage change measured in U.S. household surveys may be partly an artifact of reporting error.

IV. Summary

Our analyses of both U.S. and British data replicate and update the finding of previous microdata-based studies that real wages are substantially procyclical. The degree of real wage cyclicality, however, varies over time and place. Our analysis of March Current Population Survey data for the United States suggests that real wages took large hits in the recessions of the early 1980s and 1990s, but that men's real wages were somewhat less affected in the Great Recession. Because of difficulty in separating cyclical effects from secular trends, the effect of the Great Recession on U.S. women's real wages is less clear. Our analysis of New Earnings Survey data for Great Britain also finds differences across recessions, with practically an opposite pattern to that of the United States. In Great Britain, real wages were not much affected by the severe recession of the early 1980s, were slowed somewhat more in the severe recession of the early 1990s, and were affected very negatively by the Great Recession.

We also have used the CPS and NES data to replicate and update the literature that documents the distribution of job stayers' year-to-year nominal wage changes. Like previous studies of U.S. household surveys, our CPS analysis finds a substantial minority of stayers reporting the exact same nominal wage from one year to the next (seemingly indicating a degree of nominal wage rigidity), but also a substantial minority reporting nominal wage reductions (seemingly indicating a degree of wage flexibility). As previous writers have noted, both findings may be distorted by reporting error. This makes the presumably more accurate NES wage data, reported by employers from payroll records, of particularly high interest. These data show a much lower frequency of zero year-to-year nominal wage change, but they show a surprisingly high frequency of nominal wage reductions.

What conclusions can we take from the analysis so far? As often is the case, the available data stubbornly refuse to conform to a simple story of the role of wages in the determination of unemployment in the Great Recession, or indeed in any other recession. For the United States, it is tempting to tell a story in which downward rigidity in nominal wages has interacted with low inflation rates both to impede reductions in average real wages and, through this channel, to cause the record levels of unemployment seen in recent years. If only the data were that simple! While we cannot definitively rule out such a channel, we have highlighted a number of potential puzzles that accompany this story. For example, why has the growth in real wages among women appeared to slow down particularly in the Great Recession? Why have layoff rates not risen even further in the recent downturn in comparison to prior recessions? And why have real wages in Britain fallen so much in the Great Recession at a time when rates of inflation are so much lower than in the past?

Table 1. U.S. Men's Mean and Median Log Real Wages (2009 Dollars) by Year

Year	Unemployment Rate	Mean Log Real Wage (PCE)	Mean Log Real Wage (CPI-U-RS)	Standard Error Estimate for Means	Median Log Real Wage (PCE)	Median Log Real Wage (CPI-U-RS)	Standard Error Estimate for Medians
1979	5.8	2.933	2.995	0.004	2.991	3.053	0.003
1980	7.1	2.908	2.968	0.004	2.984	3.043	0.005
1981	7.6	2.908	2.962	0.004	2.975	3.029	0.003
1982	9.7	2.901	2.950	0.004	2.972	3.021	0.001
1983	9.6	2.889	2.938	0.004	2.935	2.984	0.008
1984	7.5	2.891	2.937	0.004	2.960	3.006	0.008
1985	7.2	2.903	2.947	0.004	2.963	3.008	0.006
1986	7.0	2.914	2.965	0.004	2.977	3.028	0.002
1987	6.2	2.907	2.960	0.004	2.977	3.030	0.008
1988	5.5	2.906	2.962	0.004	2.974	3.030	0.008
1989	5.3	2.897	2.953	0.004	2.943	2.999	0.001
1990	5.6	2.875	2.927	0.004	2.915	2.968	0.008
1991	6.8	2.859	2.912	0.004	2.902	2.955	0.004
1992	7.5	2.851	2.907	0.004	2.907	2.964	0.006
1993	6.9	2.841	2.895	0.004	2.889	2.943	0.003
1994	6.1	2.861	2.914	0.004	2.905	2.958	0.002
1995	5.6	2.864	2.915	0.005	2.892	2.943	0.008
1996	5.4	2.884	2.930	0.004	2.918	2.964	0.010
1997	4.9	2.916	2.959	0.004	2.924	2.967	0.008
1998	4.5	2.946	2.985	0.004	2.968	3.007	0.000
1999	4.2	2.979	3.014	0.004	2.992	3.027	0.006
2000	4.0	3.007	3.033	0.005	3.017	3.043	0.002
2001	4.7	3.017	3.035	0.004	3.003	3.021	0.007
2002	5.8	3.017	3.032	0.004	3.012	3.027	0.002
2003	6.0	3.019	3.032	0.004	3.021	3.033	0.006
2004	5.5	3.002	3.014	0.004	3.018	3.030	0.007
2005	5.1	3.001	3.009	0.004	3.005	3.012	0.008
2006	4.6	3.007	3.010	0.004	3.016	3.018	0.000
2007	4.6	3.007	3.008	0.004	2.989	2.990	0.001
2008	5.8	2.999	2.995	0.004	2.982	2.977	0.010
2009	9.3	3.007	3.007	0.004	3.005	3.005	0.004
2010	9.6	2.993	2.995	0.004	2.986	2.989	0.004
2011	8.9	2.989	2.984	0.004	2.990	2.986	0.006

Notes: The standard error estimates for means are robust to heteroskedasticity. The standard error estimates for medians are bootstrap estimates. In two years, the latter estimates are absolute zero. The reason is that, because of a substantial mass point at the sample median, the estimated median came out the same in every one of the 1,000 bootstrap replications.

Table 2. U.S. Men's Log Real Wages (PCE Deflator) in Severe Recessions

Year	Mean Relative to Pre-Recession Year	Regression-Adjusted Mean	Median Relative to Pre-Recession Year	Regression-Adjusted Median
1979	0 (normalized)	0 (normalized)	0 (normalized)	0 (normalized)
1980	-0.024 (0.005)	-0.025 (0.005)	-0.007 (0.006)	-0.020 (0.005)
1981	-0.024 (0.005)	-0.030 (0.005)	-0.016 (0.004)	-0.030 (0.005)
1982	-0.031 (0.005)	-0.043 (0.005)	-0.018 (0.003)	-0.043 (0.006)
1983	-0.044 (0.005)	-0.059 (0.005)	-0.056 (0.008)	-0.057 (0.006)
2006	0 (normalized)	0 (normalized)	0 (normalized)	0 (normalized)
2007	0.000 (0.005)	-0.004 (0.005)	-0.027 (0.001)	0.001 (0.005)
2008	-0.008 (0.005)	-0.011 (0.005)	-0.034 (0.010)	-0.007 (0.005)
2009	0.000 (0.005)	-0.006 (0.005)	-0.011 (0.004)	-0.003 (0.005)
2010	-0.014 (0.005)	-0.027 (0.005)	-0.030 (0.004)	-0.023 (0.005)
2011	-0.018 (0.005)	-0.032 (0.005)	-0.026 (0.006)	-0.034 (0.005)

Notes: The numbers in parentheses are estimated standard errors. The standard error estimates for means are robust to heteroskedasticity. The standard error estimates for medians are bootstrap estimates.

Table 3. Mean Year-to-Year Changes in Log Real Wages (PCE Deflator) by Gender from Longitudinally Matched CPS Data

Year	Men's Mean Log Real Wage Change	Estimated Standard Error	Women's Mean Log Real Wage Change	Estimated Standard Error
1979-1980	-0.012	0.004	0.017	0.006
1980-1981	0.010	0.005	0.018	0.006
1981-1982	0.004	0.005	0.032	0.006
1982-1983	0.003	0.005	0.026	0.006
1983-1984	--	--	--	--
1984-1985	--	--	--	--
1985-1986	0.040	0.005	0.031	0.006
1986-1987	0.025	0.005	0.039	0.006
1987-1988	0.032	0.006	0.015	0.007
1988-1989	0.022	0.006	0.015	0.006
1989-1990	0.004	0.006	0.008	0.006
1990-1991	0.001	0.005	0.025	0.006
1991-1992	-0.001	0.006	0.012	0.007
1992-1993	0.006	0.007	0.014	0.007
1993-1994	0.010	0.007	0.028	0.008
1994-1995	--	--	--	--
1995-1996	0.025	0.007	0.047	0.007
1996-1997	0.039	0.007	0.047	0.007
1997-1998	0.046	0.007	0.059	0.007
1998-1999	0.048	0.007	0.039	0.007
1999-2000	0.049	0.007	0.043	0.007
2000-2001	0.035	0.007	0.039	0.007
2001-2002	0.023	0.007	0.038	0.007
2002-2003	0.027	0.007	0.018	0.007
2003-2004	0.012	0.008	0.014	0.007
2004-2005	0.007	0.007	0.021	0.007
2005-2006	0.025	0.007	0.016	0.007
2006-2007	0.010	0.007	0.024	0.007
2007-2008	0.006	0.007	0.002	0.007
2008-2009	0.022	0.007	0.040	0.007
2009-2010	-0.003	0.007	0.009	0.007
2010-2011	0.016	0.007	0.009	0.007

Notes: The estimated standard errors are robust to heteroskedasticity. As explained in the text, data for 1983-1984, 1984-1985, and 1994-1995 could not be matched longitudinally because of the absence of residential information in 1985, and because of changes in household identifiers in 1983-1984 and 1994-1995.

Table 4. U.S. Women's Mean and Median Log Real Wages (2009 Dollars) by Year

Year	Unemploy-ment Rate	Mean Log Real Wage (PCE)	Mean Log Real Wage (CPI-U-RS)	Standard Error Estimate for Means	Median Log Real Wage (PCE)	Median Log Real Wage (CPI-U-RS)	Standard Error Estimate for Medians
1979	5.8	2.408	2.471	0.004	2.452	2.514	0.006
1980	7.1	2.405	2.464	0.004	2.440	2.499	0.006
1981	7.6	2.407	2.461	0.004	2.438	2.492	0.007
1982	9.7	2.429	2.478	0.004	2.462	2.511	0.000
1983	9.6	2.435	2.484	0.004	2.484	2.533	0.008
1984	7.5	2.446	2.493	0.004	2.486	2.532	0.006
1985	7.2	2.463	2.508	0.004	2.504	2.549	0.006
1986	7.0	2.482	2.533	0.004	2.529	2.580	0.008
1987	6.2	2.498	2.551	0.004	2.547	2.600	0.007
1988	5.5	2.498	2.553	0.005	2.540	2.595	0.002
1989	5.3	2.516	2.572	0.004	2.558	2.614	0.003
1990	5.6	2.522	2.574	0.004	2.570	2.622	0.003
1991	6.8	2.527	2.579	0.004	2.568	2.620	0.007
1992	7.5	2.540	2.597	0.004	2.596	2.652	0.009
1993	6.9	2.540	2.594	0.004	2.589	2.643	0.000
1994	6.1	2.557	2.610	0.004	2.585	2.638	0.011
1995	5.6	2.560	2.610	0.005	2.586	2.637	0.004
1996	5.4	2.589	2.635	0.004	2.617	2.664	0.006
1997	4.9	2.614	2.657	0.004	2.642	2.685	0.006
1998	4.5	2.655	2.694	0.004	2.680	2.719	0.000
1999	4.2	2.670	2.705	0.004	2.705	2.739	0.003
2000	4.0	2.706	2.732	0.005	2.719	2.745	0.000
2001	4.7	2.737	2.754	0.004	2.749	2.766	0.008
2002	5.8	2.754	2.769	0.004	2.767	2.782	0.007
2003	6.0	2.764	2.776	0.004	2.786	2.799	0.008
2004	5.5	2.752	2.764	0.004	2.784	2.796	0.002
2005	5.1	2.758	2.766	0.004	2.755	2.763	0.002
2006	4.6	2.774	2.777	0.004	2.769	2.772	0.010
2007	4.6	2.780	2.781	0.004	2.796	2.797	0.007
2008	5.8	2.760	2.755	0.004	2.775	2.770	0.006
2009	9.3	2.783	2.783	0.004	2.794	2.794	0.005
2010	9.6	2.779	2.781	0.004	2.794	2.797	0.006
2011	8.9	2.772	2.768	0.004	2.780	2.776	0.008

Notes: The standard error estimates for means are robust to heteroskedasticity. The standard error estimates for medians are bootstrap estimates. In four years, the latter estimates are absolute zero. The reason is that, because of a substantial mass point at the sample median, the estimated median came out the same in every one of the 1,000 bootstrap replications.

Table 5. U.S. Women's Log Real Wages (PCE Deflator) in Severe Recessions

Year	Mean Relative to Pre-Recession Year	Regression-Adjusted Mean	Median Relative to Pre-Recession Year	Regression-Adjusted Median
1979	0 (normalized)	0 (normalized)	0 (normalized)	0 (normalized)
1980	-0.003 (0.006)	-0.006 (0.006)	-0.012 (0.009)	-0.012 (0.005)
1981	-0.001 (0.006)	-0.010 (0.006)	-0.014 (0.009)	-0.022 (0.005)
1982	0.021 (0.006)	0.002 (0.006)	0.010 (0.006)	-0.006 (0.006)
1983	0.027 (0.006)	0.004 (0.006)	0.032 (0.010)	0.008 (0.006)
2006	0 (normalized)	0 (normalized)	0 (normalized)	0 (normalized)
2007	0.006 (0.005)	-0.003 (0.005)	0.027 (0.012)	0.003 (0.005)
2008	-0.014 (0.005)	-0.022 (0.005)	0.006 (0.011)	-0.012 (0.005)
2009	0.009 (0.005)	-0.003 (0.005)	0.025 (0.011)	0.003 (0.005)
2010	0.005 (0.005)	-0.015 (0.005)	0.026 (0.011)	-0.012 (0.005)
2011	-0.002 (0.005)	-0.025 (0.005)	0.011 (0.010)	-0.023 (0.005)

Notes: The numbers in parentheses are estimated standard errors. The standard error estimates for means are robust to heteroskedasticity. The standard error estimates for medians are bootstrap estimates.

Table 6. Nominal Wage Rigidity in the United States

Years	Annual Unemployment Rate in Year t-1	Survey-to-Survey Change in Log PCE Deflator	Percentage of Hourly Workers with:		Percentage of Non-Hourly Workers with:	
			Zero Nominal Wage Change	Negative Nominal Wage Change	Zero Nominal Wage Change	Negative Nominal Wage Change
1980-1981	7.1	0.099	6.2	11.2	11.0	21.5
1982-1983	9.7	0.046	14.4	16.6	12.4	23.5
1986-1987	7.0	0.023	15.2	17.9	11.8	27.9
1990-1991	5.6	0.047	12.4	19.9	11.1	30.1
1997-1998	4.9	0.009	14.6	17.7	9.3	26.8
1999-2000	4.2	0.025	14.7	15.9	8.9	26.0
2001-2002	4.7	0.007	16.2	14.2	11.9	26.5
2003-2004	6.0	0.021	17.6	19.5	12.9	30.2
2005-2006	5.1	0.032	17.6	17.0	12.0	26.6
2007-2008	4.6	0.035	17.7	18.7	9.4	37.1
2009-2010	9.3	0.024	19.3	23.4	14.9	33.7
2011-2012	8.9	0.024	19.5	25.5	13.9	33.1

Figure 1. U.S. Men's Mean Log Real and Nominal Wages

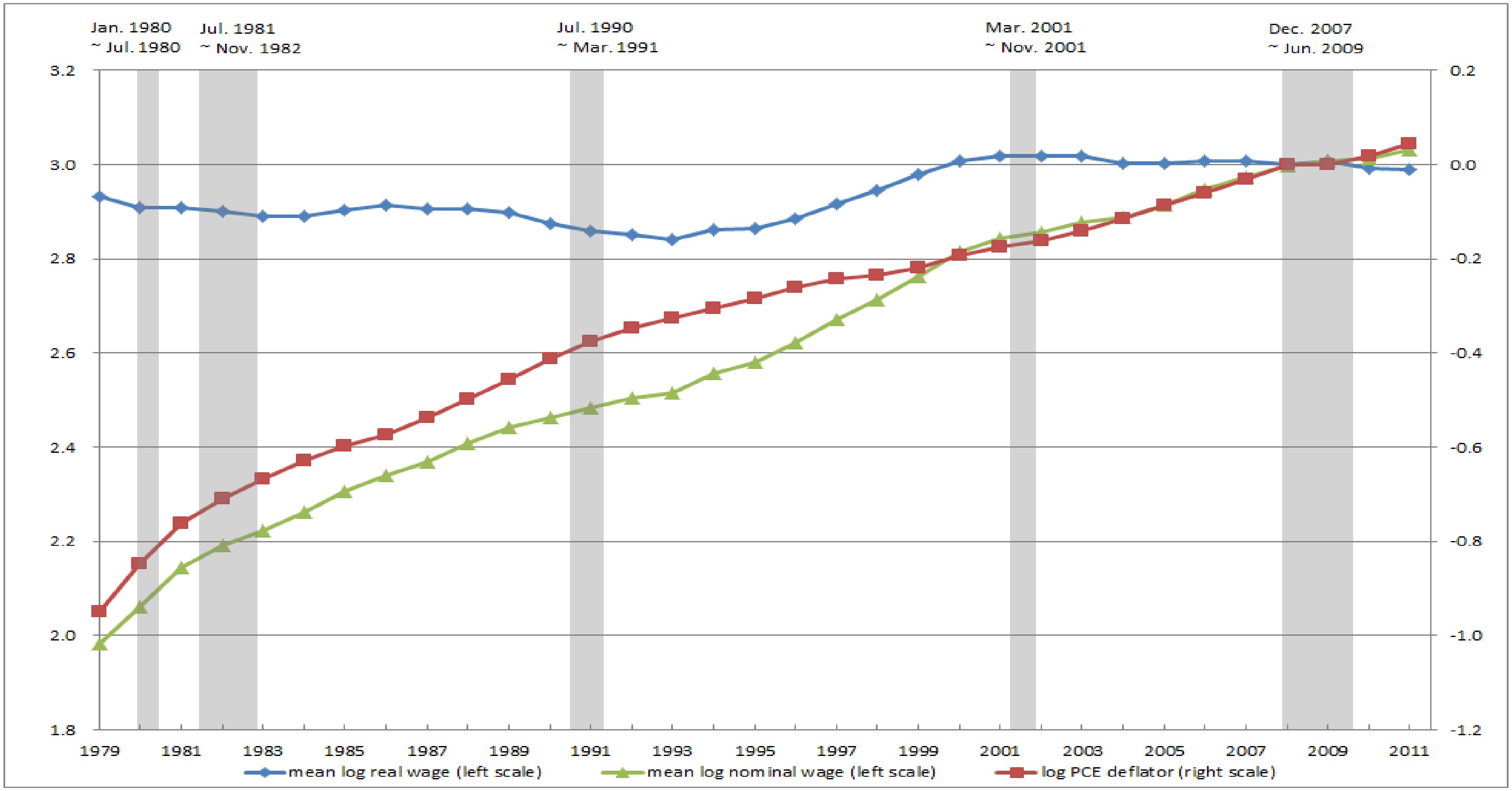


Figure 2. U.S. Women's Mean Log Real and Nominal Wages

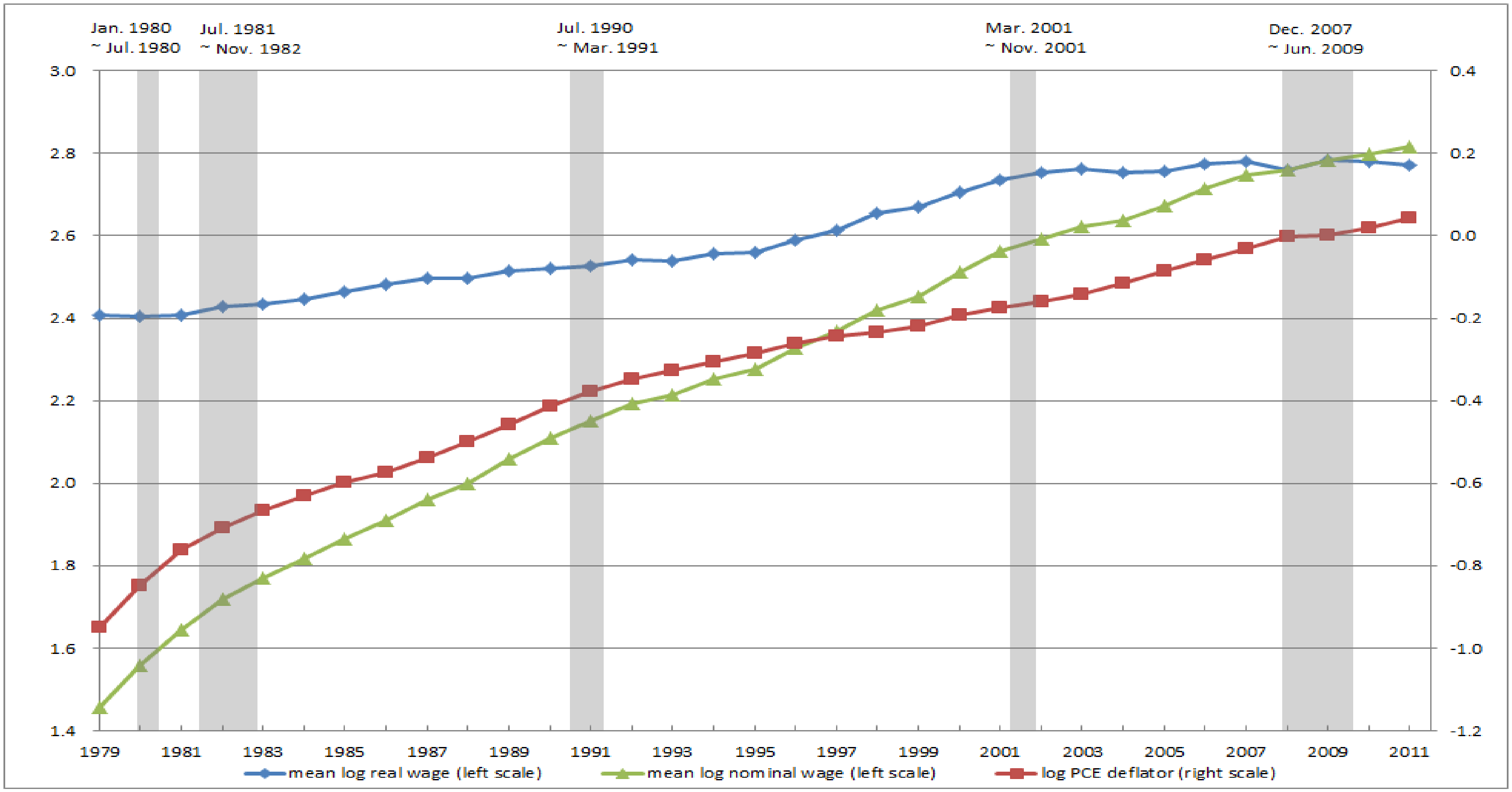


Figure 3. Distributions of Year-to-Year Change in Log Nominal Hourly Wages for U.S. Hourly Workers

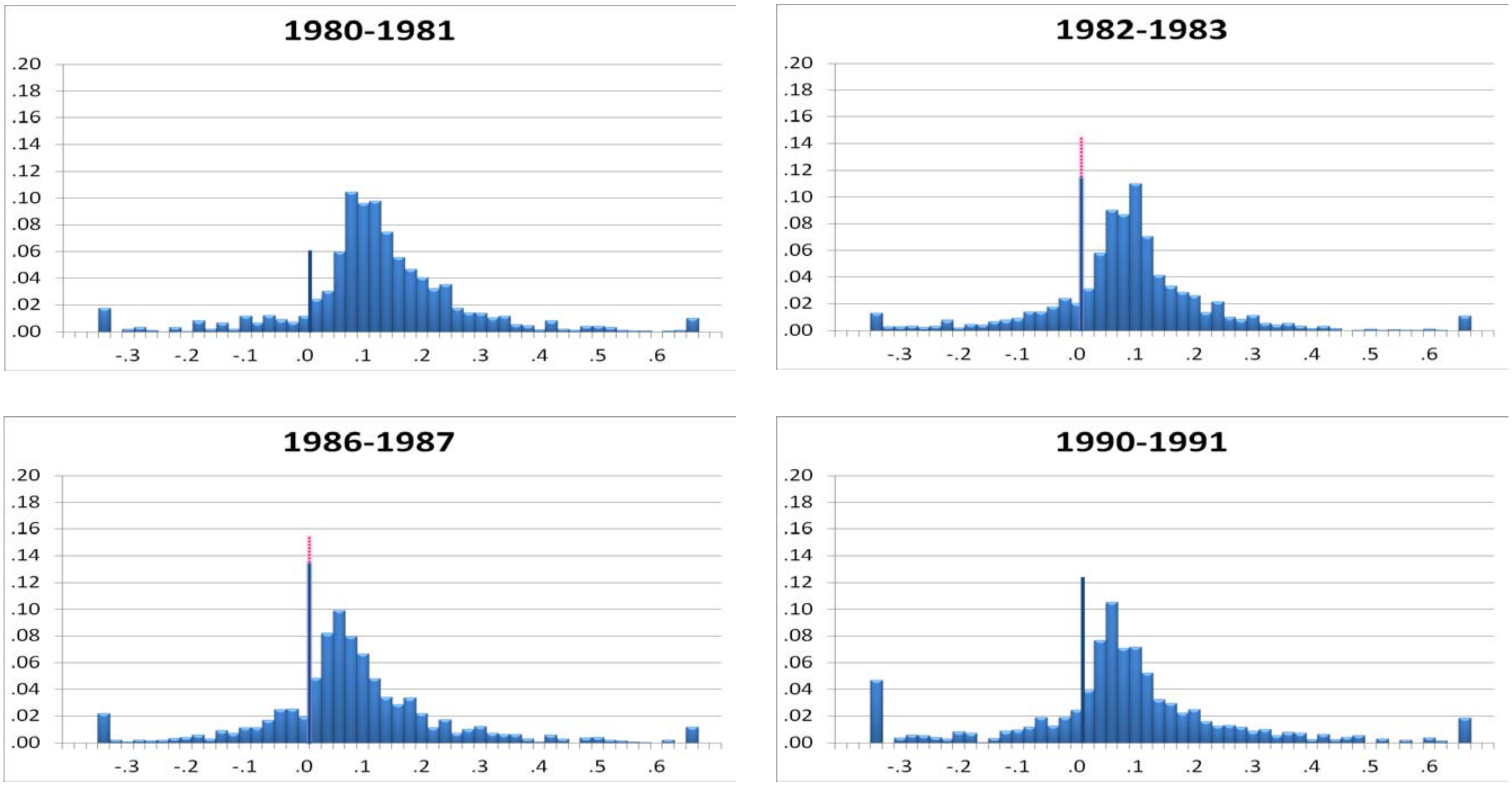
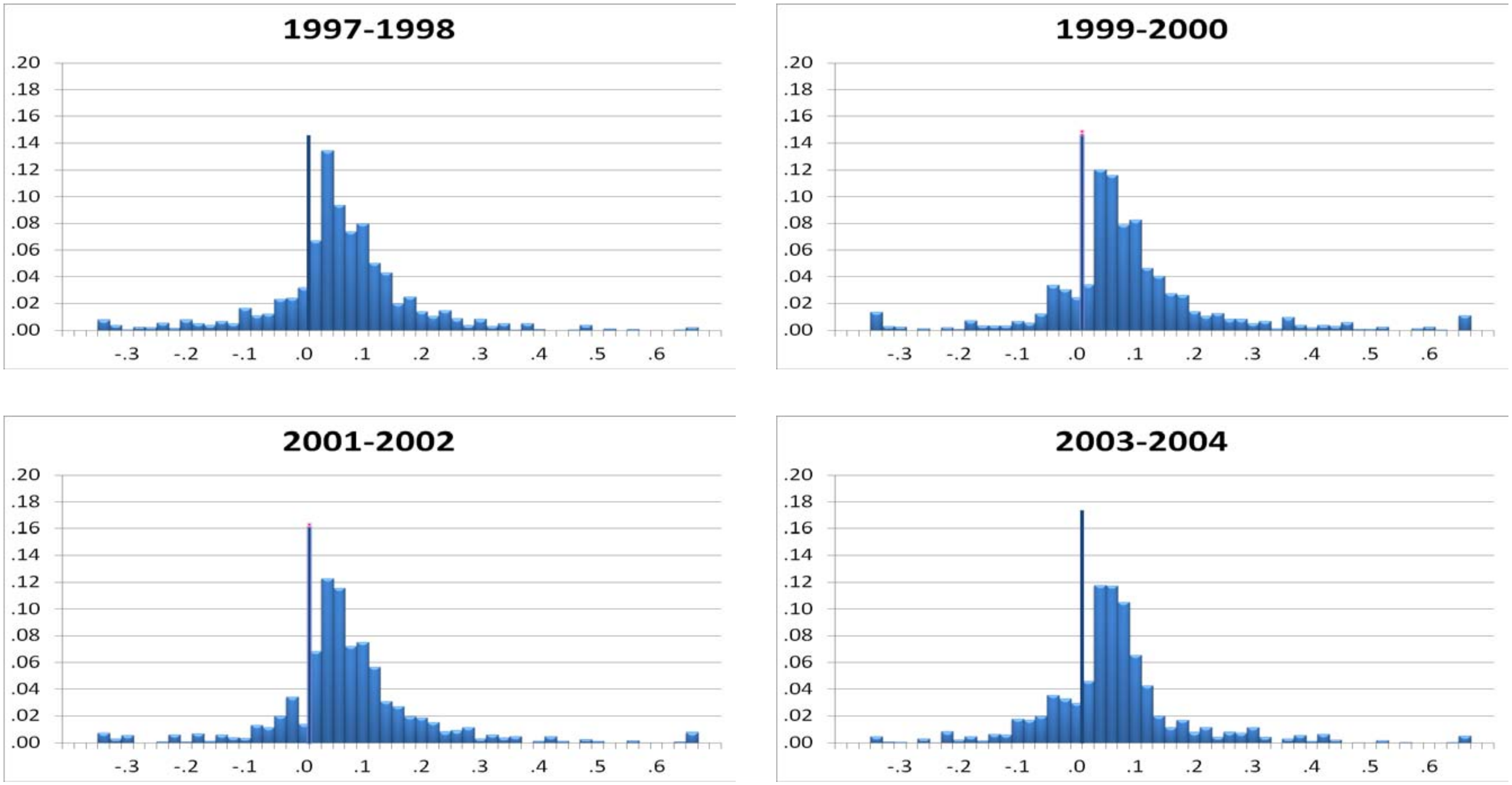


Figure 3 continued



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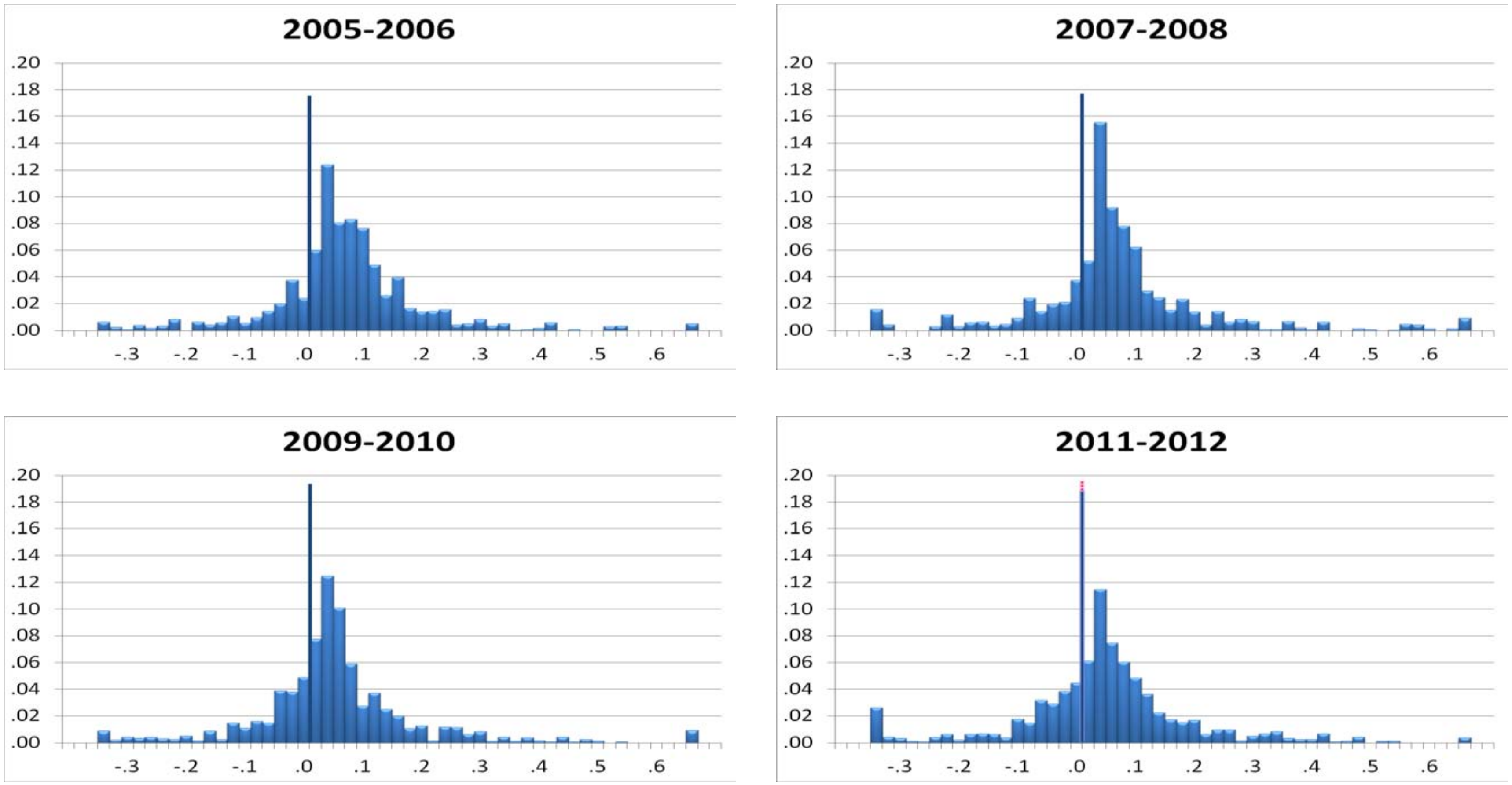
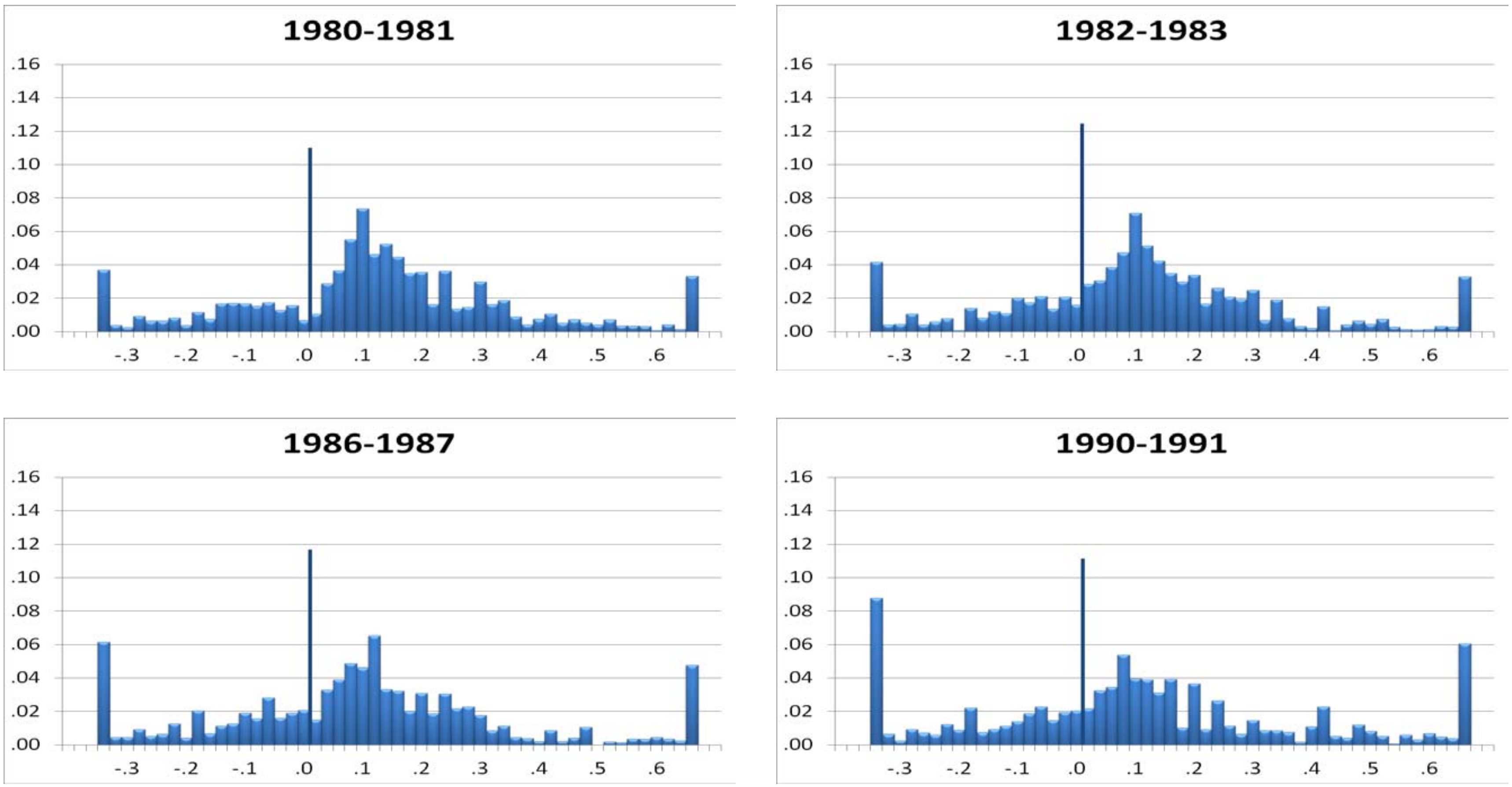


Figure 4. Distributions of Year-toYear Change in Log Nominal Weekly Earnings for U.S. Non-Hourly Workers



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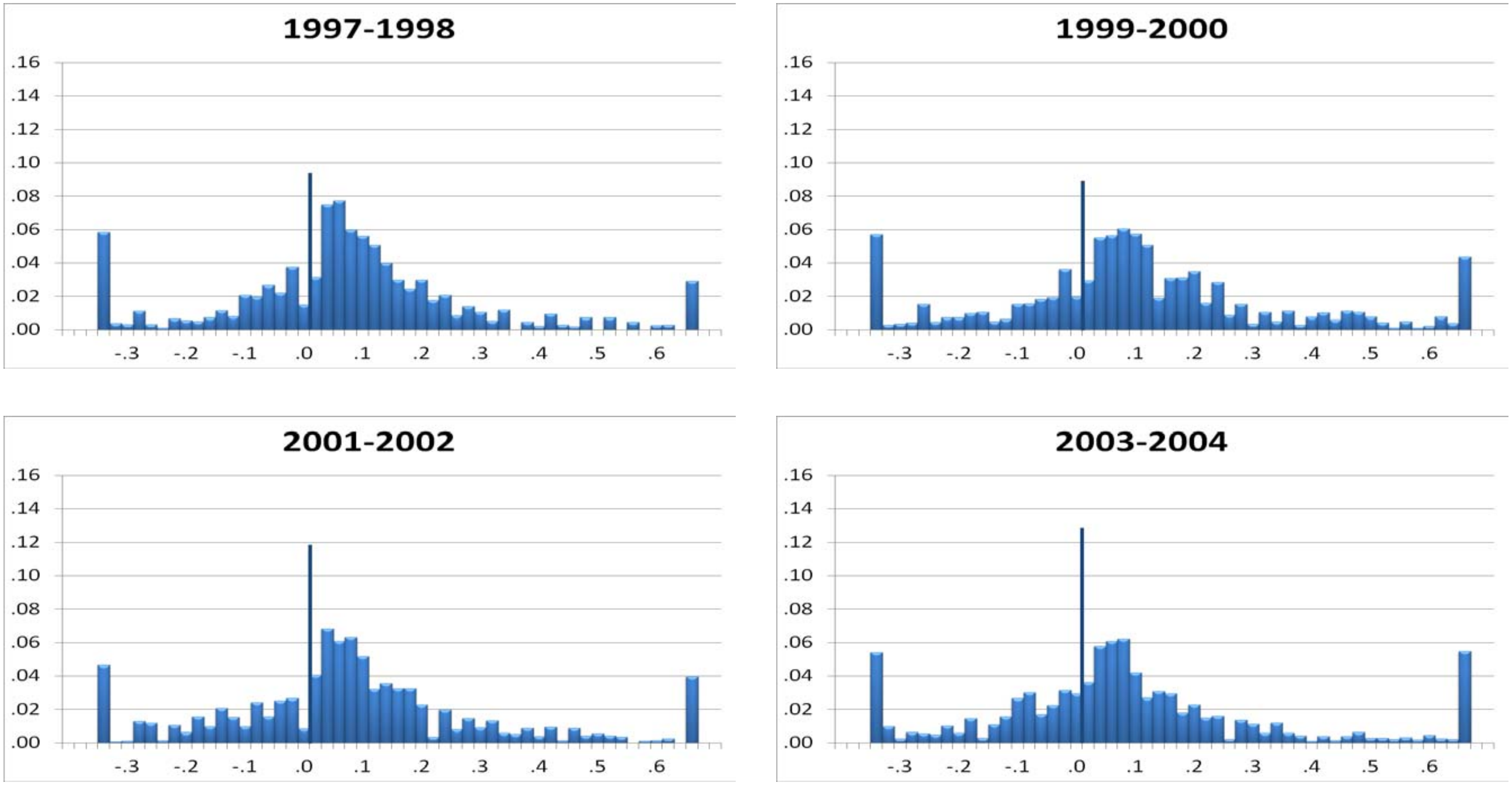

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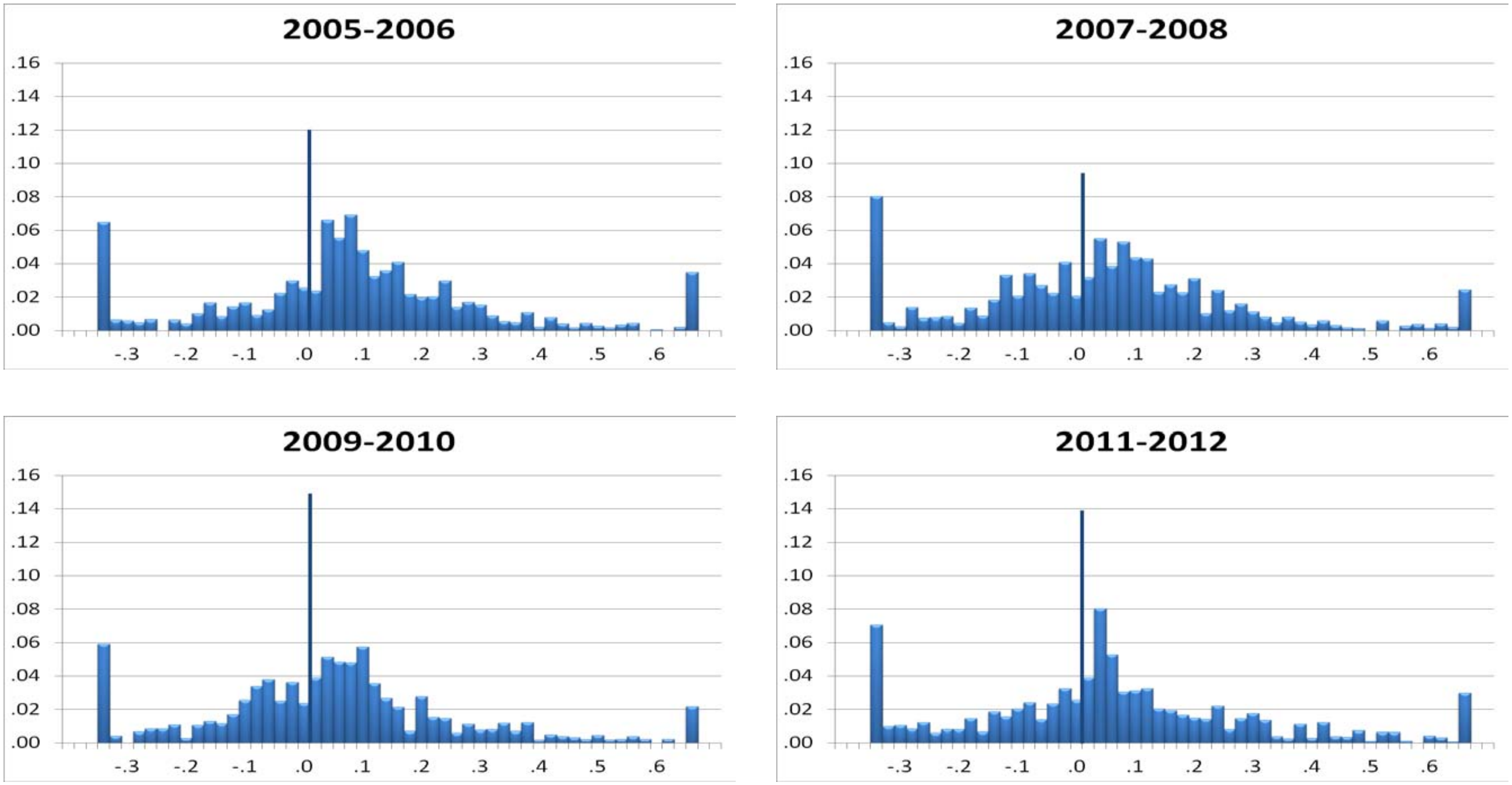

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