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Wage Adjustment in the Great Recession and Other Downturns:
Evidence from the United States and Great Britain

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Abstract

Using 1979-2012 Current Population Survey data for the United States and 1975-2012 New Earnings Survey data for Great Britain, we study wage behavior in both countries, with particular attention to the Great Recession. Real wages are procyclical in both countries, but the procyclicality of real wages varies across recessions, and does so differently between the two countries, in ways that defy simple explanations. For example, the two countries display differential trends in wage cyclicality despite declining unionization and inflation in both countries. We devote particular attention to the hypothesis that downward nominal wage rigidity plays an important role in cyclical employment and unemployment fluctuations. We conclude that downward wage rigidity may be less binding and have lesser allocative consequences than is often supposed.

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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.[1] Since then, however, numerous empirical studies based on microdata for workers have found that real wages are substantially procyclical.[2] 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 et al. (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, especially during the Great Recession. This article addresses this question with data for both the United States and Great Britain.

Section I uses March Current Population Survey (CPS) data to trace U.S. real wage behavior over the 1979-2012 period. This confirms the usual microdata-based finding that real wages are procyclical. A conspicuous feature of recent data for the Great Recession, however, is the relatively sluggish adjustment of real wages for men. The coincidence of this observation with low rates of inflation and a historic surge in unemployment suggests an initial working hypothesis that wage adjustment in the Great Recession might have been impeded by downward nominal wage rigidity, which in turn amplified the rise in unemployment.

Further investigation, however, reveals that several aspects of the available data on wage adjustment dovetail poorly with this hypothesis. A first, tentative challenge is posed by parallel analyses of March CPS data on women's real wages in Section I. While the presence of significant secular trends in female wages makes it harder to discern cyclical patterns, the data

[1] The classic macroeconomics textbook by Blanchard and Fischer (1989, p. 19), for example, declared, "The correlation between changes in real wages and changes in output or employment is usually slightly positive but often statistically insignificant," and then it devoted much of Chapters 7-9 to discussing theories designed to accord with weak wage cyclicality and its consequences. These included efficiency wage models, implicit contract models in which employers insure workers against wage fluctuations, and insider-outsider models.
[2] Examples from the U.S. literature are Stockman (1983), Bils (1985), Solon et al. (1994), Bowlus et al. (2002), and Shin and Solon (2007). Similar studies for other countries include Devereux and Hart (2006) for Great Britain; Carneiro et al. (2012) and Martins et al. (2012) for Portugal; and Shin (2012) for Korea.

suggest that the recent sluggishness in men's real wages has not been mirrored in the recent experiences of women, for whom real wage growth stagnated in the Great Recession.

A second set of challenges is presented by the results of Section II, which uses additional CPS data to update the empirical literature on year-to-year nominal wage changes of job stayers. That literature posited that a deficit of nominal wage cuts and a surfeit of nominal wage freezes would signify the presence of pervasive downward nominal wage rigidity. Like earlier studies, our analysis finds both a substantial minority of workers reporting the same nominal wage in adjacent years (suggesting nominal wage stickiness), but also a substantial minority reporting nominal wage cuts (suggesting nominal wage flexibility). In addition, recent data spanning the Great Recession suggest only a modest rise in the incidence of nominal wage freezes. We emphasize that these findings could be distorted by reporting error, an issue we return to later in the paper. Nevertheless, we note several theoretical and empirical reasons to question whether the observed degree of wage stickiness has large allocative consequences for employment and unemployment. As underscored since the work of Becker (1962), short-run wage stickiness need not induce economically inefficient layoffs among workers engaged in long-term employment relationships (such as the job stayers we and others consider). Consistent with this, although layoffs did surge at the beginning of the Great Recession, when inflation was uncommonly low, they surged similarly in the recession of the early 1980s, when inflation was much higher. Instead, the ramp-up in unemployment during the Great Recession was marked by a prominent rise in the duration of unemployment spells, a trait we argue is difficult to reconcile with simple theories of downward nominal wage rigidity.

A final note of caution is struck in Section III, which presents parallel analyses of wage adjustment in Great Britain based on the New Earnings Survey (NES). Two aspects of our analysis of the British data enrich our inquiry into the potential role of downward wage rigidity in shaping the Great Recession. First, we find that British real wages have become *increasingly* procyclical, with a particularly large wage response to the Great Recession. This British trend is more or less opposite to what we find for U.S. men. The disparate wage cyclicality trends between Great Britain and the United States are all the more striking because the two countries share downward trends in both inflation and unionization. Second, the NES wage data, which come from payroll records, presumably are more accurate than U.S. wage measures from the CPS and other household surveys. Using the NES data to measure year-to-year nominal wage changes of job stayers, we find that the more accurate payroll-based British data feature much *lesser* frequency of zero nominal wage changes compared to U.S. household survey data, but still show strikingly many nominal wage cuts. Preliminary results from payroll-based U.S. data from the Longitudinal Employer-Household Dynamics project are similar to these British results. We conclude that downward nominal wage rigidity may be less binding than is often supposed.

In Section IV, we summarize our constellation of findings by emphasizing two themes. First, real wages are procyclical in both the United States and Great Britain, but the degree of procyclicality has evolved differently in the two countries, and in ways that defy simple explanations. Second, downward rigidity in nominal wages may be less binding and have lesser allocative consequences than assumed by some influential macroeconomic theories.

I. Real Wages in the United States, 1979-2012

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 2013 CPS data for 2012 (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 et al. (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 an hourly wage variable for every worker employed 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, and we also explore controlling for worker fixed effects by using the longitudinal aspect of the CPS. 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, we use the March supplement sampling weights. Our real wage analyses include the imputed wage measures provided for the substantial number of cases with non-response for earnings.[3] We present separate results for men and women, for two reasons. First, the secular wage trends differ between the genders, so the separation of cycle and trend operates differently for the two groups. Second, although previous analyses of U.S. wage cyclicality have found little evidence of heterogeneity with respect to education or union status, they have found indications of variation between the genders.[4]

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 (the version available in May 2014) and the CPI-U-RS version of the consumer price

[3] Unweighted estimates turn out to be similar. Excluding observations with imputed wages results in higher mean and median wages, but has almost no effect on measured cyclical variation.

[4] See Solon et al. (1992) for detailed results and a summary of the literature.

index.[5] Both are scaled to express real wages in 2009 dollars. Figure 1 adds a visual display of the real wage series based on the PCE deflator. The table and figure also show the annual unemployment rate, to emphasize which years are recession years and which are expansion years.

The first thing to note in Table 1 and Figure 1 is the stagnation of men's real wages over the 1979-2012 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.

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 2.906 to 2.862, a reduction of 0.044 (as also shown in Table 2, which provides a 0.005 standard error for the estimated change between 1979 and 1983).[6] 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 8.1 in 2012. 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 and gradual. The mean log real wage based on the PCE deflator was slightly above 3.00 in 2006 and 2007, had dropped only to 2.995 by 2010, and declined to 2.982 by 2012. Compared to the 0.044 reduction from 1979 to 1983, the 0.020 reduction from 2006 to 2012 was significantly smaller (in both the statistical and substantive senses). Similarly, with wages deflated instead by the CPI-U-RS, the 2006-12 reduction in the mean log real wage was 0.038, as compared to the 0.057 reduction of 1979-83.

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

[5] Other deflators, such as the GDP deflator, deliver qualitatively similar results.

[6] If we mimic the composition bias in aggregate wage statistics by hours-weighting the CPS data, real wages then appear considerably less procyclical. For example, the 1979-83 drop in the log of the hours-weighted mean real wage based on the PCE deflator is only 0.010.

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 2012, the one based on the PCE deflator decreased by only 0.032, and the one based on the CPI-U-RS fell by only 0.050. The medians, like the means, indicate that real wages are considerably procyclical, but the procyclicality of men's real wages has been somewhat 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 1979-83 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 (age minus years of education minus 6), and race dummies.[7] 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 2006-12 period. We estimate each period's regression separately because it is implausible that the coefficients of the control variables would

[7] 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.

come close to holding still over the entire 1979-2012 period. For example, our estimated coefficient of education is 0.072 (with standard error 0.0006) for the 1979-1983 period, but it is 0.111 (0.0005) for 2006-2012. Whereas the unadjusted means indicate that log real wages were 0.020 lower in 2012 than in 2006, the adjusted 2012 year effect is 0.037 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 is to control for worker fixed effects by tracking the same workers over time in a 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 far from ideal for longitudinal analysis. The sample sizes for March-to-March matches are almost always less than one-third of the sample sizes for the cross-sections. Worse yet, one of the sources of the sample loss is that the CPS 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 have analyzed year-to-year real wage growth for the subsamples of workers we can match between adjacent March Current Population Surveys. As reported in detail in the working paper version of our study (Elsby et al., 2013), the longitudinal results corroborate our finding from repeated cross-sections that U.S. men's real wages are procyclical, but somewhat less so in the Great Recession than one might have expected from earlier recessions.

Table 3 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 3 and Figure 2 corroborate the well-known rise in women's wages during the 1979-2012 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 3 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 4 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. And again, as detailed in Elsby et al. (2013), we have replicated these findings in a longitudinal analysis of workers matched between adjacent March surveys.

To summarize, our evidence for 1979-2012 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

Some recent discussions have suggested that the high U.S. unemployment during the Great Recession may have been amplified by sluggish wage adjustment due to a combination of low inflation and downward nominal wage rigidity.[8] A preliminary basis for this suggestion is illustrated in Figure 3, which decomposes the men's mean log real wage series already shown in Figure 1 into the difference between the mean log nominal wage and the log of the price level. As discussed in the previous section, men's real wages declined considerably during the recessions of the early 1980s and early 1990s. Figure 3 shows that nominal wages grew during those recessions, but more slowly than the price level. In the Great Recession, when men's real wages declined 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%, 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% a year, and the even smaller growth in nominal wages meant that real wages underwent a modest decline.

Thus, at least for men (though apparently not for women), that the Great Recession reduced 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%. 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,

[8] For example, in a brief Federal Reserve Bank of San Francisco note entitled "Why Has Wage Growth Stayed Strong?" Daly et al. (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."

McLaughlin, 1994; Card and Hyslop, 1996; Kahn, 1997; Altonji and Devereux, 1999; Dickens et al., 2007; Elsby, 2009; and Daly et al., 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. 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 methods for longitudinal matching recommended by Madrian and Lefgren (2000), we match these workers to their data in the CPS one year earlier, when these workers were in their fourth month in sample.[9] 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.[10]

[9] In particular, we verify that longitudinal matches on identification numbers are true matches by requiring that gender and race also match and that year-to-year change in reported age is between -1 and 3.

[10] Card and Hyslop (1996) and Daly et al. (2012) also use longitudinally matched CPS data to measure nominal wage change distributions, Card and Hyslop for 1979-93 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.

Like many previous studies, we construct histograms of the empirical distribution of year-to-year nominal wage changes. In the interest of boosting sample size, each of our year-to-year matched samples 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. We use the outgoing-rotation-groups 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 have constructed two histograms – one for workers paid by the hour in both years and one 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." The typical sample size for each of our histograms is about 1,000 workers. Accordingly, the typical standard error for the estimated percentage of workers with exactly zero nominal wage change from one January or February to the next is about one percentage point.

Each of our histograms features a thin spike at zero, which shows the percentage of the workers that reported the exact same wage in both 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.

Our working paper (Elsby et al., 2013) displays the entire set of histograms. For brevity, we summarize the results here in two ways. In Figure 4, we show the histograms for hourly workers in the four most recent pairs of years. In Table 5, we present some summary statistics for both hourly and non-hourly workers in all twelve pairs of years.

In general, our histograms display several features noted by previous authors. First, a non-trivial fraction of workers reports nominal wage reductions in every pair of years. As shown in Table 5, this fraction always exceeds 10% for hourly workers and 20% for non-hourly workers. While this pattern is initially suggestive of downward flexibility in nominal wages, it is theoretically possible that the pattern is at least partly an artifact of reporting error. Workers whose true nominal wages stayed the same or increased could be measured as experiencing decreases if the second year's reporting error is sufficiently negative relative to the first year's. Later in this paper, we will return to that possibility in light of evidence from other data sources.

Second, there is always a substantial spike at zero, which is suggestive of a degree of nominal wage stickiness. As shown in Table 5, for each type of worker in each pair of years, a non-trivial minority of workers ranging between 6 and 20% reports the exact same nominal wage in both years. It is unclear *a priori* 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. When we return to the measurement error issue in light of other evidence, we will conclude that the latter bias dominates, so that the zero spikes in our histograms exaggerate the extent of wage stickiness.

Third, cuts and freezes in nominal wages are more prevalent when inflation is low and labor demand is weak. Cuts and freezes were least common in 1980-81, when the inflation rate was about 10% and unemployment was rising but had not reached the high level of 1982-83. They were most common during the Great Recession, when unusually weak demand coincided with low inflation. Figure 4 and Table 5, however, show that the spikes at zero wage change were only moderately higher in the Great Recession than in the immediately preceding years. In addition, there appears to be somewhat of an upward secular trend in the frequency of freezes, which might be a gradually evolving response to a prolonged stretch without high inflation.[11]

Although we will argue that rounding error exaggerates the height of the zero spikes in survey-based histograms, there undoubtedly is *some* stickiness in nominal wages. Some workers really experience nominal wage freezes from one year to the next; many workers, including

[11] These patterns are broadly consistent with those reported in the CPS analyses by Card and Hyslop (1996) and Daly et al. (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 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 5 do not show a drop-off after 2003-04.

ourselves, have their nominal wages reset only once a year; and almost no workers have their nominal wages reset every nanosecond. The question is not whether there is *any* wage stickiness. The questions are: First, how would the distribution of wage growth differ in the absence of nominal wage stickiness? Second, whatever that difference is, what are the effects on quantity variables like employment and unemployment? In particular, has downward nominal wage rigidity been a major cause of the Great Recession's unusually high unemployment?

On the first question, we reiterate that 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. Beyond that, it is remarkably difficult to identify how nominal wage growth distributions are affected by nominal wage stickiness. As noted earlier in this section, many excellent researchers have tackled this question before, but we are struck by how unsuccessful they have been in reaching definitive conclusions. A particularly sophisticated effort is the well-known study by Kahn (1997), which used substantial changes in inflation over the 1970-88 period to try to identify how nominal wage stickiness affected the distribution of wage growth across 12 one-percentage-point bins on either side of each year's median wage growth. Although the approach seems conceptually promising, it delivers only two robust findings – that there is a noticeable spike in the bin for zero nominal wage growth, and that there appears to be a dip in the bins for 1 or 2% above zero (which, as we will discuss in the next section, also could be a reporting effect, not a real phenomenon). All the other patterns of interest turn out to be sensitive to functional form specification or sample selection. For example, Kahn's results (nicely summarized in her Table 2) indicate a tendency away from nominal wage reductions for hourly workers, but show the opposite tendency in some specifications for non-hourly workers. Her estimates also indicate a dip in the bin for 1% below zero for non-hourly workers, but show no such dip for hourly workers. In the end, as is so often the case, inferring convincingly clear-cut counterfactual distributions from observational data turns out to be beyond the reach of even the most skillful researchers.

The second question is even harder: However much wage inertia there is, what are its effects on employment and unemployment? Here it is helpful to make a distinction between workers in the "primary" and "secondary" sectors of the labor market. Our histograms pertain to workers that stayed with the same employer for at least a year. These tend to be the primary-sector workers, for whom various types of specific human capital foster long-term employment relationships. The histograms tend to exclude workers in the secondary sector, 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 the zero spike's quantitative suggestion 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. In particular, an employer's decision about whether to continue to employ the marginal incumbent worker should depend on the employer's beliefs about the present discounted value

$$(1) \qquad V = \sum_{t=1}^{T} \{(m_t - w_t)/[(1+r)^{t-1}]\}$$

where m denotes the real value of the worker's marginal product, w is the worker's real wage, t indexes time period with the current period denoted as $t = 1$, and T is the worker's remaining tenure with the employer if not laid off this period. Of course, the latter is not only uncertain, but also endogenously determined by the employer's wage policy and future retention decisions. For simplicity, the real interest rate r is assumed to be constant over time, and the worker's employment with the firm is assumed to be binary with no hours variation at the intensive margin. Now suppose that downward rigidity in the nominal wage causes the employee's current real wage w_1 to exceed her current real value of marginal product m_1. Even so, it is in the employer's interest to retain the employee 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.

This theoretical point is buttressed by at least two pieces of empirical evidence. First is the anecdotal evidence from Bewley's interviews. On the question of why employers did not save laid-off workers' jobs by cutting their wages instead, here is Bewley's summary (pp. 180-1): "I was surprised to learn that most managers did not believe that pay cuts would prevent many layoffs…. A common reaction to the question was puzzlement. Pay cuts would create little or no extra work and so would barely reduce the number of excess workers." The direct quotations from owners and managers include these (p. 185): "If I cut pay instead of laying people off, I would have lots of people with nothing to do." "What do pay cuts have to do with layoffs? A layoff is used when you don't have sufficient work for certain skills. What would you do with the extra help?" "Wage cuts are not an alternative to layoffs. You can't have a lot of people standing around doing nothing." Whatever else one makes of these statements, they are consistent with the proposition that downward stickiness in current nominal wages need not be a major source of economically inefficient layoffs.

Second, returning to the Great Recession in particular, if current wages are indeed allocative and downward nominal wage rigidity was especially binding in the Great Recession's low-inflation environment, one would expect the Great Recession to be characterized by an extraordinary burst of layoffs. The behavior of the quantity side of the U.S. labor market during the Great Recession has been documented in detail by Elsby et al. (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 high-inflation environment of the early 1980s (see, for example, their Figure 9).

Instead, the most distinctive feature of the Great Recession with respect to labor quantities has been the extraordinarily long duration of unemployment spells. Therefore, understanding the high unemployment of the Great Recession requires understanding not only why some employers laid off so many workers into unemployment, but also why other employers have been so slow to hire the unemployed. Again, we find it instructive to consider the present-value expression in equation (1), except that now we regard it as the present value of a prospective new hire.[12] Presumably, one major reason for employers' reluctance to hire the unemployed during a recession is that depressed product-market demand reduces prospective hires' current and near-term values of m. Even so, if current and future values of w fell sufficiently, hiring the unemployed could become attractive to employers.

Accordingly, several recent papers, such as Hall and Milgrom (2008) and Kennan (2010), have appended various sorts of hiring-wage stickiness to the Mortensen-Pissarides (1994) matching model in an effort to generate realistically large cyclical fluctuations in unemployment.

[12] Following Becker (1962), the version of equation (1) for new hires should subtract off the part of hiring and training costs borne by the employer (and not borne by the worker through wage reductions), but that modification does not alter our main points.

Of course, it is always possible theoretically to generate more unemployment by assuming inflexible wages, but is the assumed inflexibility in hiring wages realistic? It is surprisingly difficult to answer that question because most countries have no publicly available data that track hiring wages within particular jobs within particular firms. Martins et al. (2012) use such data from the Portuguese census of employers and find that real hiring wages in Portugal have been quite procyclical. Their conclusion, however, acknowledges that the initial hiring wage by itself is not a sufficient statistic for the relevant labor price. Referring again to equation (1), suppose that the initial hiring wage w_1 drops considerably in a recession, but this bargain price for labor vaporizes quickly because, as the recession passes, the cheaply hired workers either quit (small *T*) or are retained at the cost of substantial wage increases (high w_t for $t > 1$). In that case, even a substantial drop in initial hiring wages might not raise *V* enough to induce primary-sector employers to hire from the unemployed. On the other hand, as emphasized by Kudlyak (2009), if instead workers hired at a low wage during a recession are somehow locked into a long-term employment relationship at a persistently low wage, the labor cost relevant to employers' recruiting decisions could be as cyclical as, or even more cyclical than, the initial hiring wage. Therefore, assessing the practical relevance of the new theories based on inflexible hiring wages will require more empirical work that, in the tradition of Beaudry and DiNardo (1991), recognizes the durability of primary-sector employment relationships and studies how wage paths in those relationships depend on current, past, and anticipated business cycle conditions. By the same token, future theoretical research needs to analyze the nature of implicit contracts in long-term employment relationships and consider how contracts for new workers interact with ongoing contracts for incumbent workers. In our view, the model of Snell and Thomas (2010) is a promising step in that direction.

In any case, we are skeptical that a theory of downward rigidity in nominal wages in particular is the key to understanding depressed hiring during the Great Recession. We already have noted that whatever such rigidity there is did not cause a greater upsurge in layoffs than occurred in the high-inflation environment of the early 1980s recession. While models such as the Snell-Thomas one recognize the possibility of spillovers from wage stickiness for incumbent workers to stickiness in hiring wages, it is hard to imagine that nominal rigidity is *more* constraining for new hires than for incumbents. Therefore, given that nominal rigidity did not appear to cause an unusually large upsurge in layoffs of incumbents, it seems implausible that it could account for the Great Recession's unusually long duration of unemployment spells.

To summarize, at an early stage of our research project, we were intrigued by the possibility that the combination of downward nominal wage rigidity with low inflation might be an important part of explaining the Great Recession's high unemployment. With some disappointment, we have come to feel that much of the evidence lines up poorly with that story. Although men's real wages seem to have taken a smaller hit in the Great Recession than in earlier recessions, women's wages seem to show the opposite pattern; although the histograms show a spike at zero nominal wage change, they also show many nominal wage cuts; the zero spike increased in the Great Recession, but not dramatically; and layoffs were not dramatically more prevalent in the Great Recession than in earlier severe recessions.

None of this is 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, 1975-2012

Our analyses of real and nominal wages in Great Britain are based on the panel files from the New Earnings Survey (NES, Office for National Statistics). 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 update both analyses to 2012.[13]

Besides the 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 1% sample

[13] Concurrently with our research, Blundell et al. (2014) and Gregg et al. (2014) also have used the NES to study British wage adjustment in the Great Recession. As usual, different research teams using the same data on the same topic have made many different methodological choices. For example, compared to our study, the study by Gregg et al. focuses on a shorter sample period, weekly instead of hourly earnings, and medians instead of means; disaggregates by region; and does not study nominal wage changes or otherwise use the panel dimension of the data. Nonetheless, these two studies and ours agree that the labor market impact of the Great Recession in Great Britain fell unusually much on wages rather than employment.

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. A noteworthy limitation of the survey, however, is that it samples from taxpayers registered in the income tax system and therefore is thought to under-represent low-paid workers.

To provide context for our analysis, the left panel of Figure 5 displays the 1975-2012 U.K. series for the unemployment rate (as measured by the Labour Force Survey) and the inflation rate (as measured by the change in the logarithm of the RPIX, which is based on the Retail Price Index, but excludes mortgage interest payments). To conform to the April timing of the NES, we use April measures for the unemployment rate and the RPIX. As the unemployment rate shows, our sample period encompasses three recessions. Like the United States, the United Kingdom experienced a very severe recession in the early 1980s. The somewhat less severe episode in the early 1990s again brought the unemployment rate to double digits. By some stroke of good fortune, the United Kingdom escaped the global contraction in the early 2000s, but was not spared in the Great Recession. Interestingly, although the output fall associated with the Great Recession was relatively large, the unemployment rate did not go as high as it had in the recessions of the early 1980s and early 1990s.

As the left panel of Figure 5 also shows, the United Kingdom entered the 1980s with even higher inflation than the United States, reaching nearly 20% on an annual basis. Subsequently, inflation fell rapidly in the 1980s and, except for an aberration in the late 1980s (associated with the boom at that time), remained below 5% from 1985 through to the early stages of the current recession. Inflation did rise somewhat during the Great Recession, though, exceeding 5% in 2010.

We will analyze real wages in part A of this section and nominal wages in part B. Throughout, our nominal wage measure is the worker's gross hourly earnings (defined as earnings divided by hours in the reference week in April) excluding overtime. For the analysis of real wages, we convert the nominal wage into 2012 pounds based on the April RPIX. In our initial sample selection, we exclude individuals who reported wages for more than one job in the reference week, who lost pay due to absence in that week, or who were younger than 16 or older than 64, and then we trim the remaining sample for each year by excluding the cases with the top and bottom 1% of wages.

A. Real wages

Following our U.S. analysis of real wages, we further restrict the sample to workers between the ages of 25 and 59.[14] The resulting sample of men is typically about 60,000 per year. The women's sample starts at more than 30,000 in 1975 and exceeds 60,000 in the later years of the sample period.

Our main analysis of U.S. real wages used repeated cross-sections from the March CPS that measured wages with average hourly earnings over the entire preceding calendar year. We also referred to longitudinal results based on March-to-March matches, but with some reservation on account of imperfections in the CPS as a source of longitudinal data, especially its failure to follow residential movers. Somewhat in parallel, we will begin by analyzing repeated cross-sections from the NES, but with a caveat. Because the NES measures wages only for those working in the reference *week* in April, it seems potentially subject to more severe composition bias than the March CPS. Fortunately, however, the NES is an excellent source of longitudinal data, so we will proceed to longitudinal analyses that hold composition constant by following the same workers from one April to the next.

Starting with the repeated cross-sections, Table 7 in Elsby et al. (2013) shows mean log real wages by year separately for men and women. Here we display both series visually in the right panel of Figure 5. In Great Britain, as in the United States, the upward trend in women's wages is dramatic. But whereas men's wages stagnated in the United States, they have risen considerably in Great Britain, though not nearly as much as women's.

The cyclical patterns for men and women look similar, so we will discuss them together. The most striking cyclical setback to real wages occurred during the Great Recession. From

[14] We have verified, however, that the cyclical patterns remain much the same if we use a 16-64 age range and also if we do not trim the outliers.

2008 to 2012, as the unemployment rate went from 5.2 to 8.1, men's real wages declined by about 14 log points, and women's declined by about 8. Viewed against the backdrop of the upward secular trends in real wages, these reductions look all the more striking. The only other prominent reduction in real wages during our sample period occurred in the non-recession year of 1977. The story behind this episode appears to be related to incomes policies negotiated between the British government and the trade unions at that time. The agreement placed an upper bound on increases in nominal wages for that year, presumably in an attempt to curb inflation by stemming wage inflation. Despite this, price inflation remained very high, and so workers experienced real wage cuts.

Turning to the other recessions, during the early 1980s, when the unemployment rate rose to almost 12% and inflation was even higher than in the United States, real wage growth hardly slowed at all. The recession of the early 1990s was accompanied by a more noticeable slowing of real wage growth, but nothing like the reduction during the Great Recession. Thus, the British variation in wage cyclicality across recessions is more or less the opposite of what we measured for U.S. men in Section II. This poses still another challenge to simple stories about the interaction of downward nominal wage rigidity and the inflationary environment. In Great Britain, real wages took a much bigger hit in the Great Recession than in the early 1980s even though the early 1980s were a period of much higher inflation.

All of this, however, is based on wage measures from repeated cross-sections for a reference week in April. The U.S. evidence indicates that such measures could be subject to a substantial countercyclical composition bias, so we follow Devereux and Hart (2006) in using the longitudinal nature of the NES to hold worker composition constant by following the same workers from one April to the next. This longitudinal matching loses some workers not employed in one reference week or the other, but the sample sizes are still large. The men's sample size per year is usually over 50,000. The women's sample size starts at over 20,000 in 1975-76 and reaches over 60,000 towards the end of our sample period.

Table 8 in Elsby et al. (2013) shows mean year-to-year change in log real wages by gender for each pair of years from 1975-76 to 2011-12. Here we plot this longitudinally based series for each gender in Figure 6, along with the first difference of the cross-sectional mean log wage series previously shown in the right panel of Figure 5. Figure 6 vividly depicts two patterns. First, as one would expect, the longitudinal series runs somewhat higher because it encompasses life-cycle wage growth. But second, the cyclical patterns for the longitudinal series and the series from repeated cross-sections are remarkably similar. Although composition bias repeatedly has been found to be an important issue for measuring wage cyclicality in the United States, it appears to matter much less for Great Britain.[15] Thus, we continue to find that real wage growth in Great Britain hardly slowed at all in the early 1980s, slowed more in the early 1990s, and went negative in the Great Recession.

We already have noted that this pattern cannot be explained by changes in the inflationary environment, which go the "wrong" way. So what does account for the increasing procyclicality of British real wages? One possibility is that declining unionization has led to more flexible wages. Setting aside the puzzle of why the same trend has not led to greater wage flexibility in the United States as well, we have pursued the British evidence on this idea by redoing Figure 6 disaggregated by union status. Figure 10 in Elsby et al. (2013) shows mean year-to-year change in log real wages by gender separately for those who were in jobs covered by union agreements in both years and those who were in non-union jobs in both years. Those plots for union and non-union workers are strikingly similar to each other, and to the aggregate plots in Figure 6. Thus, although declining unionization could be part of the story, it cannot be nearly all of it. Even after controlling for union status, the procyclicality of real wages remains much stronger in the Great Recession than in earlier recessions.

B. Nominal wages

The relative 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 1991-96 waves of the British Household Panel Study (BHPS) to retrace the steps of the U.S. literature described in our Section II. 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 for the same period – she found that

[15] A similar result was reported earlier by Liu (2003).

9% of stayers experienced zero nominal wage change from one year to the next, and that 23% 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%. As we mentioned in Section II, 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 £11.93 last year and is £12.17 this year might round to £12 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%. 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 NES data for 1975-99. Like Smith, Nickell and Quintini focused on workers staying in the same job with the same employer. Nickell and Quintini began by comparing their 1991-96 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 1975-99 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%, with the highest proportion being 7.1% in 1992-93. 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% in 1979-80 (when the inflation rate was close to 20%) to a high of 22% in 1996-97. 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 2012. As in our U.S. analysis of nominal wages in Section II, we pool women and men between the ages of 16 and 64. Unlike in our U.S. analysis, we pool hourly and non-hourly workers, who are not distinguished in the NES data. Because our analysis of year-to-year nominal wage changes is restricted to workers staying in the same job with the same employer, our sample sizes become somewhat smaller than in our longitudinal analysis of real wages in the NES. With women and men combined, our sample size starts at almost 60,000 for 1975-76 and rises to over 100,000 by the end of our sample period.

In Elsby et al. (2013), we present a complete set of histograms of job stayers' year-to-year nominal wage growth for each pair of years from 1975-76 on. Here we summarize by showing the histograms for the six most recent pairs of years in Figure 7, along with summary statistics for all years in Table 6. The histograms are laid out similarly to our U.S. ones except that Figure 7 combines hourly and non-hourly workers. Because the spikes at zero are less prominent in the British data, we highlight them in dark blocks instead of thin lines.

Four patterns stand out in the histograms and table. First, like Nickell and Quintini, we find relatively small spikes at zero nominal wage change, ranging from a low of 0.4% in 1979-80 to a high of 9.1% in 2011-12. In most years, the spike is less than 3%. That the zero spikes are so much smaller in the British payroll-based data than in the U.S. household survey data suggests a possibility that the larger U.S. spikes might be at least partly an artifact of rounding error.

Second, we replicate and update the finding of substantial proportions of British job stayers experiencing negative nominal wage changes. Our estimates range from a low of 4.9% in 1979-80 to a high of 23.5% in 2009-10 and 2011-12. From 1993-94 on (when the inflation rate typically has been about 3%), the proportion experiencing nominal wage cuts regularly has run in the neighborhood of 20%.[16] Like the Smith and Nickell-Quintini studies before us, we are

[16] Furthermore, these nominal wage cuts are remarkably pervasive across sub-groups of workers/jobs. For example, in 2011-12, when the overall proportion of job stayers experiencing cuts was 23.5%, the proportions were 22% in

struck by the wage flexibility indicated in the frequency of nominal wage cuts as well as the infrequency of nominal wage freezes. Some U.S. writers, such as Altonji and Devereux (1999), have conjectured that the substantial fraction of U.S. job stayers reporting nominal wage reductions is an artifact of response error in household surveys. The payroll-based British data, however, also show many nominal wage cuts.

Third, as in previous research for both Great Britain and the United States, the fractions of job stayers with zero and negative nominal wage changes vary over time with respect to inflation and business cycle conditions in the ways that one would expect. In the first few years of our British sample period, when inflation was particularly high, the fractions with zero and negative nominal wage changes were particularly low. In the last three years of the sample period, when the Great Recession was at its worst and inflation was moderate, the fractions with zero and negative nominal wage changes were higher than usual.

Fourth, some of the household-survey-based U.S. literature, such as Kahn (1997), has noted some evidence for dips in the distribution of nominal wage change in the bins immediately surrounding zero, and has interpreted those dips as possibly reflecting menu costs in wage setting. Such dips are not particularly apparent in our histograms from the British payroll-based data. And, as shown in Table 6, the percentages of job stayers with positive and negative log nominal wage changes no larger than 0.01 are non-trivial, reaching a combined share of 11-12% in the last three years of our sample period. This leads us to wonder whether rounding error in U.S. household surveys has not only exaggerated the spike at zero nominal wage change, but has done so by reducing the reporting of small non-zero nominal wage changes, thus creating the appearance of dips around zero in the histograms.[17]

Payroll-based longitudinal hourly wage data for the United States would be invaluable for exploring our conjectures about how rounding and other measurement error may distort U.S. survey-based measures of nominal wage change. Fortuitously, Kurmann et al. (2014) recently

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the private sector and 26% in the public sector; 27% for union workers and 22% for non-union workers; at least 20% for every single-digit occupation; and 32% for workers that received incentive pay in either 2011 or 2012 and 22% for workers that did not.

[17] Following an excellent suggestion from David Green, we have checked what happens if we redo our British histograms with weekly instead of hourly earnings. The patterns are mostly the same. The percentage with nominal wage reductions is very similar. The percentage with zero nominal change goes up somewhat, but is still strikingly small compared to the U.S. estimates based on household surveys. In most years, it is less than 4%. The increase in the zero spike from using weekly instead of hourly earnings is about equal to the reduction in the percentage with positive or negative log changes no greater than 0.01. As a result, the percentage with log changes between 0.01 and -0.01 is about the same with the weekly vs. hourly wage measures.

discovered that, for three U.S. states (Minnesota, Rhode Island, and Washington), the Longitudinal Employer-Household Dynamics data include payroll-based reports of workers' quarterly hours as well as earnings. At our request, Kurmann et al. graciously have constructed histograms of year-to-year changes in nominal average hourly earnings from quarters of 2010 to the corresponding quarters of 2011. These pertain to workers who stayed with the same employer from the fourth quarter of 2009 to the first quarter of 2012. For comparison, recall that the CPS results in our Table 5 show that, in 2009-10 and 2011-12, the percentages of stayers with zero nominal wage change were over 19% for hourly workers and almost 15% for non-hourly workers, while the percentages with nominal cuts were 23-26% for hourly workers and 33-34% for non-hourly workers. The payroll-based results from Kurmann et al. show only 4% with zero nominal wage change, but 24% with nominal wage cuts of at least half of 1%. These payroll-based results, like our British results, lead us to suspect that downward nominal wage rigidity may be less binding than is often supposed.[18]

IV. Summary and Discussion

Our analyses of both U.S. and British data replicate and update the finding of previous microdata-based studies that, by and large, 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 picture for U.S. women is less clear, but a tentative impression is that the Great Recession's impact on U.S. women's real wages was particularly adverse. Our analysis of New Earnings Survey data for Great Britain also finds differences across recessions, with practically an opposite pattern to that for U.S. men. In Great Britain, real wages were not much affected by the severe recession of the early 1980s, displayed slowed growth in the severe recession of the early 1990s, and were affected very negatively by the Great Recession. The between-country

[18] Of course, the patterns for Great Britain and the United States do not span the full range across all countries. According to Doris et al. (2014), during Ireland's Great Recession, which was extraordinarily severe and involved a deflation in prices, the *majority* of job stayers experienced nominal wage cuts. At the other extreme, Carneiro et al. (2014) report virtually no nominal wage cuts for job stayers in Portugal, where nominal cuts are explicitly outlawed.

difference in the evolution of real wage cyclicality over our sample period is all the more striking given that both countries experienced reductions in inflation and unionization.

Motivated by the oft-stated hypothesis that downward nominal wage rigidity is an important contributor to cyclical employment and unemployment fluctuations, 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 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. Like the authors of previous British studies of nominal wage change, we are struck by the apparent flexibility of British wages. Preliminary payroll-based evidence for the United States from the Longitudinal Employer-Household Dynamics data suggests that U.S. wages also may be less rigid than is often supposed.

However much wage stickiness there is, does it have important effects on employment and unemployment? In particular, did its interaction with an environment of very low inflation contribute to the large upsurge of U.S. unemployment in the Great Recession? At an early stage of our project, we thought this story was suggested by the sluggish response of men's real wages to that downturn. But, while there may be something to the story, it lines up poorly with many other empirical patterns. Why did the Great Recession appear to have a particularly large effect on U.S. women's real wages? Why didn't the prevalence of zero nominal wage change rise more precipitously during the Great Recession? As discussed in Section II, there are theoretical reasons to question whether wage stickiness has major "allocative" effects, and empirically, if it does, one would expect the Great Recession to have featured an extraordinarily large upsurge in layoffs. Layoffs did indeed surge upwards, but similarly to how they did in previous severe recessions, including the high-inflation environment of the early 1980s. The more distinctive aspect of the U.S. labor market's response to the Great Recession has been the extraordinary length of unemployment spells. But it is hard to see why downward nominal wage rigidity would be *more* constraining for the hiring of new workers from the unemployed than it is for the retention of incumbent workers. Finally, if downward nominal wage stickiness was important in the U.S. experience of the Great Recession, why was it so much less so in Great Britain? In the end, while wages undoubtedly are at least somewhat sticky, we doubt that a simple theory based on downward nominal rigidity can accord with the full range of empirical patterns we have documented for the United States and Great Britain.

The Great Recession has served as a reminder of how deeply important and puzzling is the behavior of labor markets over the business cycle, and we are keenly aware that our paper's results raise more questions than they answer. We are pleased that the NBER project of which our study is a part has succeeded in drawing labor economists back into this research area. Given the many important but unanswered questions, we hope that both labor economists and macroeconomists will continue to pursue both theoretical and empirical approaches to understanding how and why labor markets behave as they do over recessions and expansions.

Table 1. U.S. Men'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 for Means	Median Log Real Wage (PCE)	Median Log Real Wage (CPI-U-RS)	Standard Error for Medians
1979	5.8	2.906	2.995	0.004	2.964	3.053	0.003
1980	7.1	2.881	2.968	0.004	2.957	3.043	0.005
1981	7.6	2.881	2.962	0.004	2.948	3.029	0.003
1982	9.7	2.875	2.950	0.004	2.946	3.021	0.001
1983	9.6	2.862	2.938	0.004	2.908	2.984	0.008
1984	7.5	2.864	2.937	0.004	2.933	3.006	0.008
1985	7.2	2.873	2.947	0.004	2.933	3.008	0.006
1986	7.0	2.887	2.965	0.004	2.950	3.028	0.002
1987	6.2	2.885	2.960	0.004	2.955	3.030	0.008
1988	5.5	2.885	2.962	0.004	2.953	3.030	0.008
1989	5.3	2.877	2.953	0.004	2.923	2.999	0.001
1990	5.6	2.857	2.927	0.004	2.897	2.968	0.008
1991	6.8	2.844	2.912	0.004	2.887	2.955	0.004
1992	7.5	2.839	2.907	0.004	2.895	2.964	0.006
1993	6.9	2.826	2.895	0.004	2.874	2.943	0.003
1994	6.1	2.846	2.914	0.004	2.890	2.958	0.002
1995	5.6	2.850	2.915	0.005	2.878	2.943	0.008
1996	5.4	2.871	2.930	0.004	2.904	2.964	0.010
1997	4.9	2.904	2.959	0.004	2.911	2.967	0.008
1998	4.5	2.936	2.985	0.004	2.957	3.007	0.000
1999	4.2	2.971	3.014	0.004	2.984	3.027	0.006
2000	4.0	2.998	3.033	0.005	3.008	3.043	0.002
2001	4.7	3.008	3.035	0.004	2.994	3.021	0.007
2002	5.8	3.008	3.032	0.004	3.003	3.027	0.002
2003	6.0	3.011	3.032	0.004	3.012	3.033	0.006
2004	5.5	2.996	3.014	0.004	3.012	3.030	0.007
2005	5.1	2.995	3.009	0.004	2.999	3.012	0.008
2006	4.6	3.002	3.010	0.004	3.011	3.018	0.000
2007	4.6	3.004	3.008	0.004	2.986	2.990	0.001
2008	5.8	2.998	2.995	0.004	2.981	2.977	0.010
2009	9.3	3.007	3.007	0.004	3.005	3.005	0.004
2010	9.6	2.995	2.995	0.004	2.989	2.989	0.004
2011	8.9	2.992	2.984	0.004	2.993	2.986	0.006
2012	8.1	2.982	2.972	0.004	2.978	2.968	0.007

Notes: The standard errors for means are robust to heteroskedasticity. The standard errors 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	0.002 (0.005)	-0.002 (0.005)	-0.025 (0.001)	0.003 (0.005)
2008	-0.004 (0.005)	-0.007 (0.005)	-0.030 (0.010)	-0.003 (0.005)
2009	0.006 (0.005)	-0.001 (0.005)	-0.005 (0.004)	0.003 (0.005)
2010	-0.007 (0.005)	-0.019 (0.005)	-0.022 (0.004)	-0.016 (0.005)
2011	-0.010 (0.006)	-0.024 (0.005)	-0.018 (0.006)	-0.025 (0.005)
2012	-0.020 (0.006)	-0.037 (0.005)	-0.032 (0.007)	-0.039 (0.005)

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

Table 3. 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 for Means	Median Log Real Wage (PCE)	Median Log Real Wage (CPI-U-RS)	Standard Error for Medians
1979	5.8	2.381	2.471	0.004	2.425	2.514	0.006
1980	7.1	2.378	2.464	0.004	2.413	2.499	0.006
1981	7.6	2.380	2.461	0.004	2.411	2.492	0.007
1982	9.7	2.402	2.478	0.004	2.435	2.511	0.000
1983	9.6	2.408	2.484	0.004	2.457	2.533	0.008
1984	7.5	2.419	2.493	0.004	2.459	2.532	0.006
1985	7.2	2.434	2.508	0.004	2.475	2.549	0.006
1986	7.0	2.456	2.533	0.004	2.502	2.580	0.008
1987	6.2	2.476	2.551	0.004	2.525	2.600	0.007
1988	5.5	2.477	2.553	0.005	2.519	2.595	0.002
1989	5.3	2.495	2.572	0.004	2.537	2.614	0.003
1990	5.6	2.504	2.574	0.004	2.552	2.622	0.003
1991	6.8	2.512	2.579	0.004	2.552	2.620	0.007
1992	7.5	2.528	2.597	0.004	2.584	2.652	0.009
1993	6.9	2.525	2.594	0.004	2.574	2.643	0.000
1994	6.1	2.542	2.610	0.004	2.570	2.638	0.011
1995	5.6	2.546	2.610	0.005	2.572	2.637	0.004
1996	5.4	2.576	2.635	0.004	2.604	2.664	0.006
1997	4.9	2.602	2.657	0.004	2.629	2.685	0.006
1998	4.5	2.645	2.694	0.004	2.670	2.719	0.000
1999	4.2	2.661	2.705	0.004	2.696	2.739	0.003
2000	4.0	2.697	2.732	0.005	2.711	2.745	0.000
2001	4.7	2.728	2.754	0.004	2.740	2.766	0.008
2002	5.8	2.745	2.769	0.004	2.759	2.782	0.007
2003	6.0	2.755	2.776	0.004	2.778	2.799	0.008
2004	5.5	2.745	2.764	0.004	2.777	2.796	0.002
2005	5.1	2.752	2.766	0.004	2.749	2.763	0.002
2006	4.6	2.769	2.777	0.004	2.764	2.772	0.010
2007	4.6	2.777	2.781	0.004	2.793	2.797	0.007
2008	5.8	2.759	2.755	0.004	2.774	2.770	0.006
2009	9.3	2.783	2.783	0.004	2.794	2.794	0.005
2010	9.6	2.781	2.781	0.004	2.797	2.797	0.006
2011	8.9	2.775	2.768	0.004	2.783	2.776	0.001
2012	8.1	2.765	2.755	0.004	2.765	2.755	0.003

Notes: The standard errors for means are robust to heteroskedasticity. The standard errors 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 4. 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 (0.006)
1981	-0.001 (0.006)	-0.010 (0.006)	-0.014 (0.009)	-0.022 (0.006)
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	0.008 (0.005)	-0.002 (0.005)	0.029 (0.012)	0.006 (0.006)
2008	-0.010 (0.005)	-0.018 (0.005)	0.010 (0.011)	-0.007 (0.005)
2009	0.014 (0.005)	0.002 (0.005)	0.030 (0.011)	0.008 (0.005)
2010	0.012 (0.005)	-0.007 (0.005)	0.033 (0.011)	-0.003 (0.006)
2011	0.006 (0.005)	-0.017 (0.005)	0.019 (0.010)	-0.015 (0.005)
2012	-0.004 (0.005)	-0.034 (0.005)	0.001 (0.010)	-0.026 (0.006)

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

Table 5. 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

Table 6. Nominal Wage Rigidity in Great Britain

Years	Start-of-Period Unemploy-ment Rate	April-to-April Change in Log RPIX	Percentage of Log Nominal Wage Changes by Interval:			
			Exactly 0	[-0.01,0)	(0,0.01]	Less than 0
1975-1976	4.2	0.175	0.6	0.4	0.4	5.2
1976-1977	5.4	0.160	1.1	0.8	1.0	9.1
1977-1978	5.5	0.083	1.7	0.9	1.1	8.9
1978-1979	5.6	0.087	1.8	0.7	0.9	8.4
1979-1980	5.3	0.188	0.4	0.3	0.4	4.9
1980-1981	6.1	0.117	2.0	0.7	0.8	8.6
1981-1982	9.4	0.087	2.3	0.8	1.0	9.2
1982-1983	10.5	0.048	1.8	0.9	1.2	10.5
1983-1984	11.3	0.048	4.1	1.2	1.7	12.8
1984-1985	11.9	0.052	1.4	1.0	1.3	11.9
1985-1986	11.4	0.033	1.2	1.6	1.2	12.2
1986-1987	11.3	0.035	2.1	1.2	1.3	12.2
1987-1988	10.9	0.041	1.3	0.9	1.2	11.6
1988-1989	8.9	0.057	1.8	1.0	1.1	11.0
1989-1990	7.3	0.076	2.1	1.0	1.2	11.0
1990-1991	6.9	0.066	2.3	0.9	1.0	11.4
1991-1992	8.5	0.055	4.2	1.3	1.6	13.7
1992-1993	9.8	0.029	6.0	1.8	2.8	16.7
1993-1994	10.5	0.023	5.5	2.4	3.0	19.9
1994-1995	9.7	0.026	4.9	1.9	2.6	20.9
1995-1996	8.8	0.029	1.4	1.8	4.4	19.8
1996-1997	8.3	0.025	1.6	4.8	2.8	22.8
1997-1998	7.2	0.030	3.4	1.8	2.0	20.2
1998-1999	6.3	0.024	3.8	1.6	2.0	18.1
1999-2000	6.1	0.019	3.8	2.0	2.4	18.7
2000-2001	5.6	0.020	3.4	1.6	3.4	15.2
2001-2002	4.9	0.023	1.2	3.4	3.8	19.5
2002-2003	5.2	0.030	1.3	2.6	4.2	20.7
2003-2004	5.0	0.020	1.5	4.4	4.1	22.9
2004-2005	4.8	0.023	1.3	3.1	2.4	18.4
2005-2006	4.8	0.024	1.9	3.3	3.8	21.4
2006-2007	5.4	0.035	2.3	3.5	3.8	20.2
2007-2008	5.4	0.039	2.9	2.5	2.9	18.3
2008-2009	5.2	0.017	4.6	3.3	3.3	19.4
2009-2010	7.6	0.053	7.5	4.2	7.4	23.5
2010-2011	7.9	0.052	7.3	4.2	6.7	22.8
2011-2012	7.8	0.034	9.1	5.0	6.7	23.5

Figure 1. U.S. Men's Mean Log Real Wages (PCE Deflator) over the Business Cycle

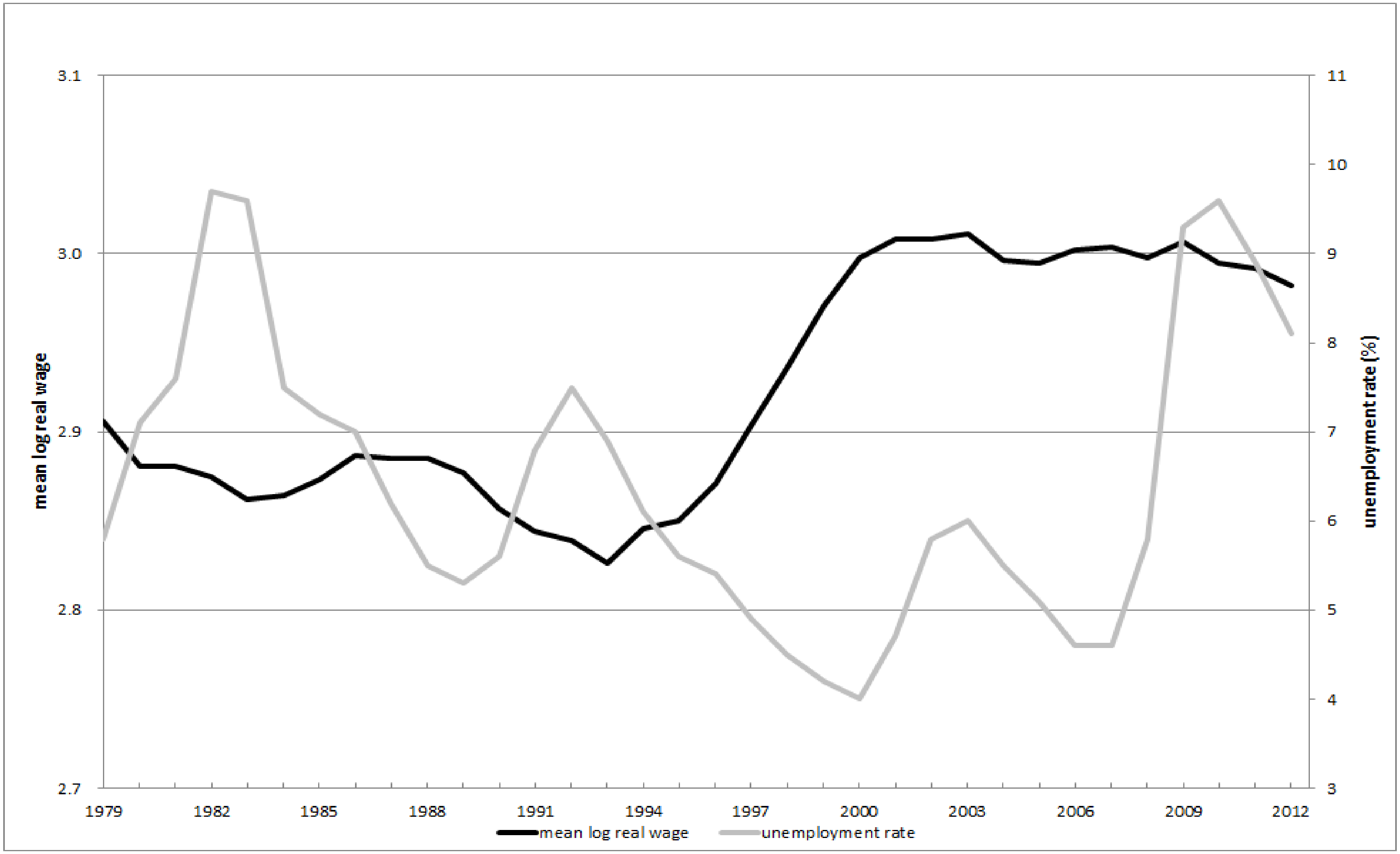


Figure 2. U.S. Women's Mean Log Real Wages (PCE Deflator) over the Business Cycle

Figure 3. U.S. Men's Mean Log Real and Nominal Wages

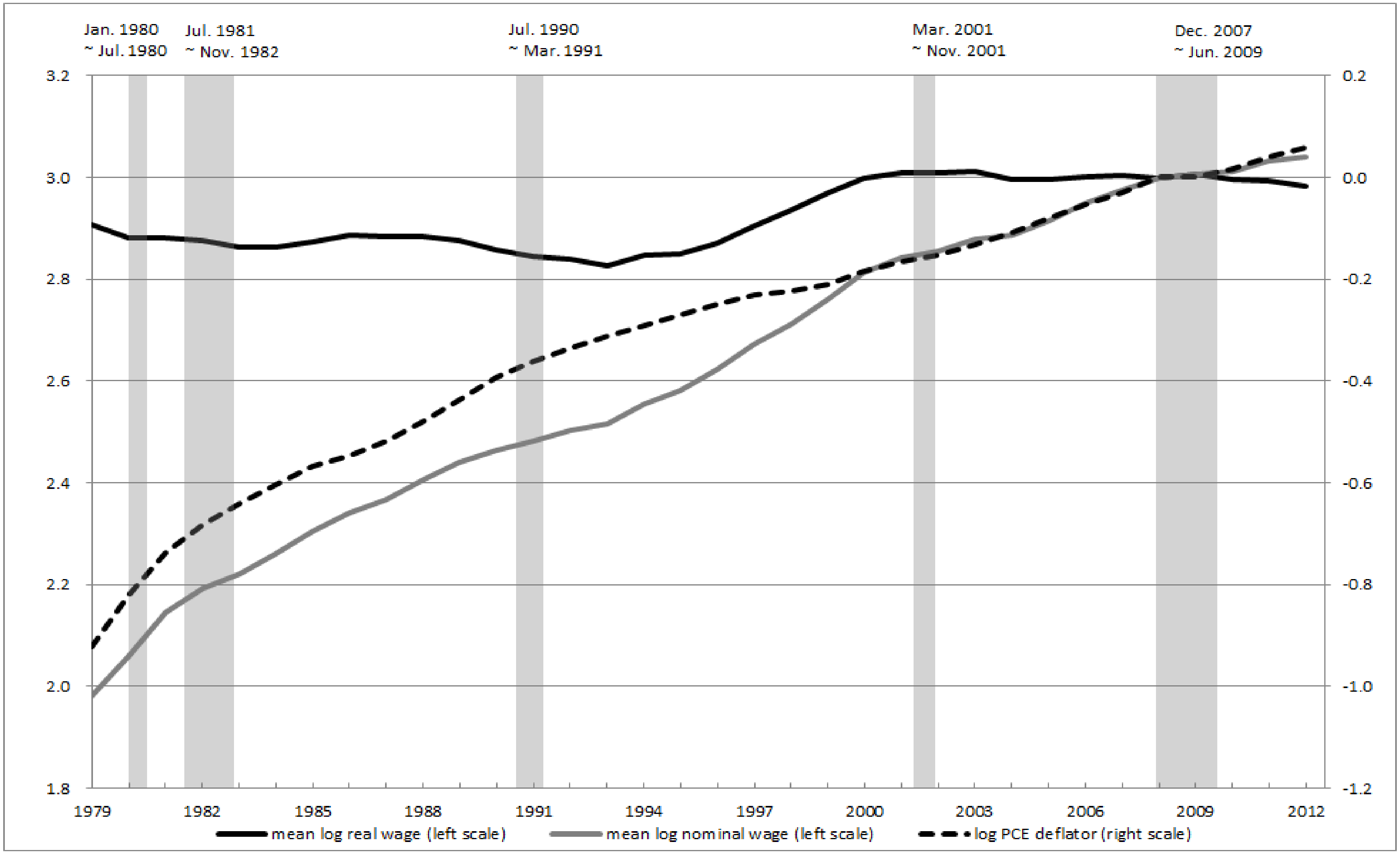


Figure 4. Distributions of Year-to-Year Change in Log Nominal Hourly Wages for U.S. Hourly Workers

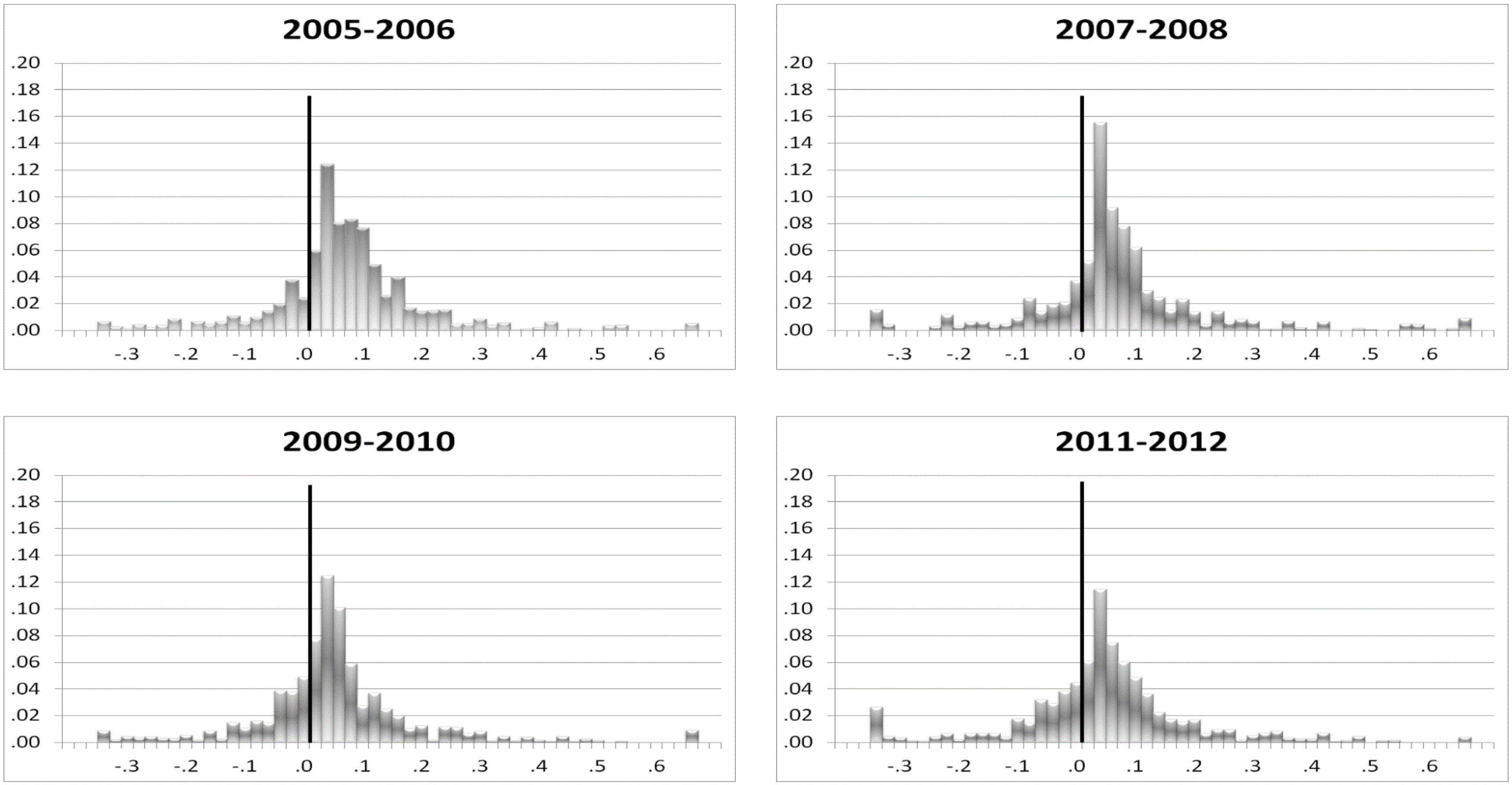
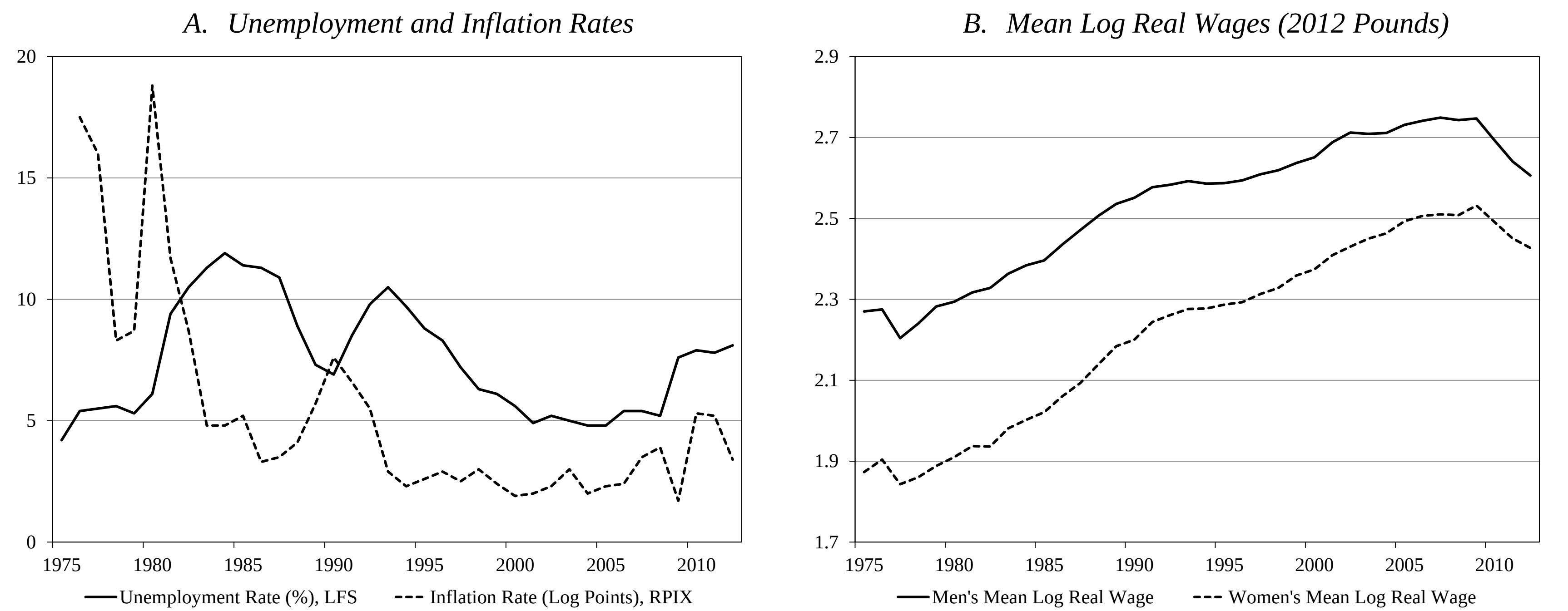


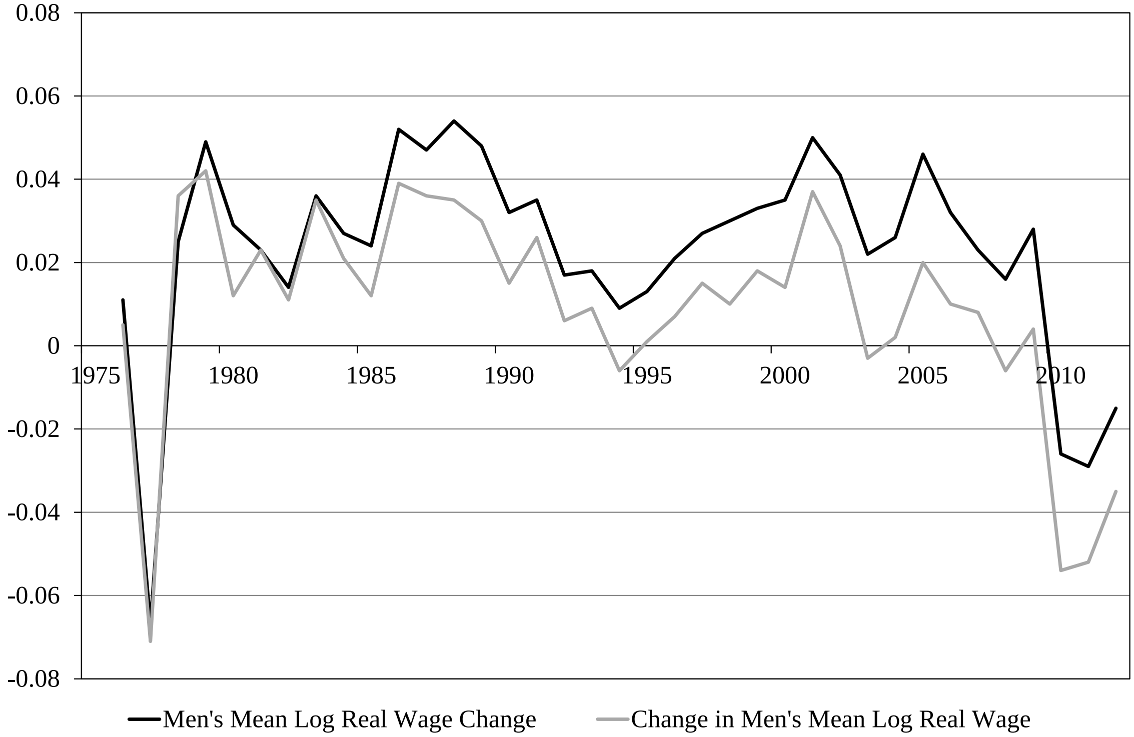
Figure 5. Unemployment, Inflation, and Mean Log Real Wages in Great Britain, 1975 to 2012



Notes: The unemployment and inflation rates are for the entire United Kingdom, i.e., Great Britain plus Northern Ireland. In line with the timing of the NES, the unemployment rate is for April, and the inflation rate is 100 times the April-to-April change in the logarithm of the RPIX. The real wage series depict mean log hourly earnings excluding overtime, deflated using the RPIX, for individuals aged 25 to 59, excluding the top and bottom 1% of hourly earnings.

Figure 6. Log Real Wage Changes in Great Britain, 1975 to 2012

A. Men



B. Women

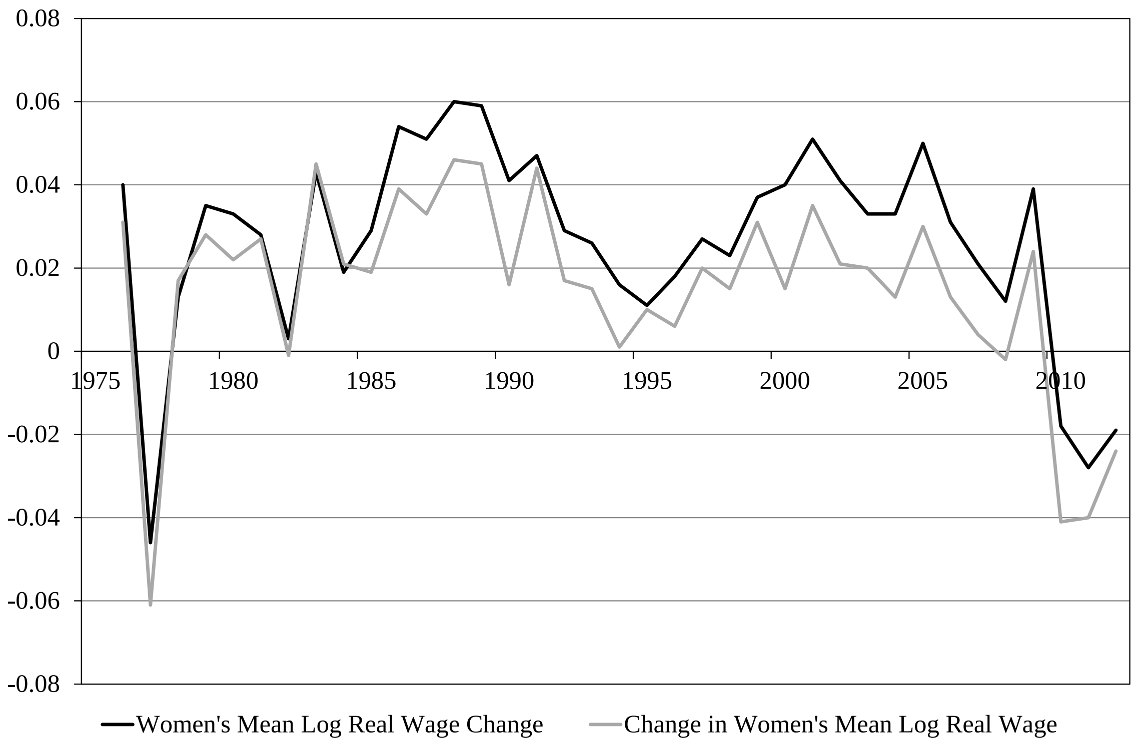
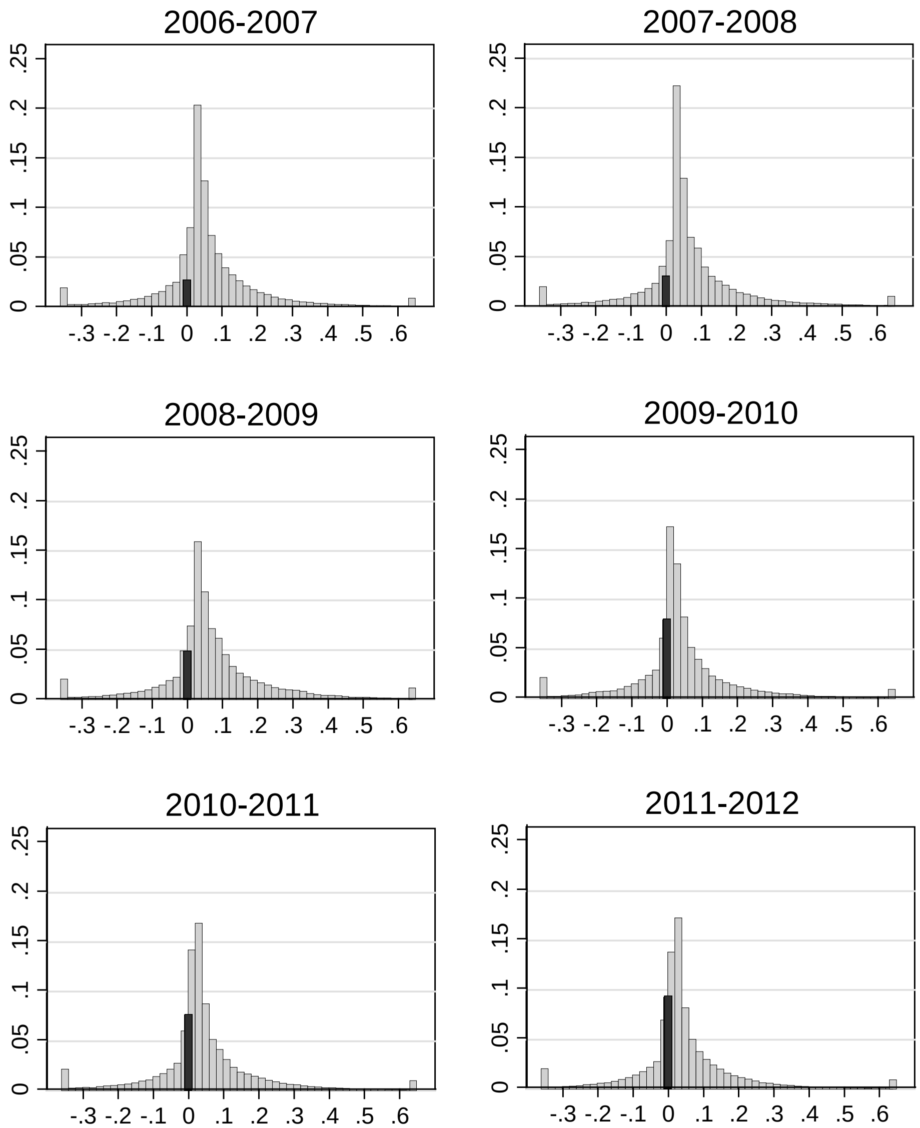


Figure 7. Distributions of Year-to-Year Change in Log Nominal Hourly Wages for British Job Stayers



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