Working Papers

FEDERAL RESERVE BANK OF BOSTON™

No. 10-11

Some Evidence on the Importance of Sticky Wages

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Abstract:
Nominal wage stickiness is an important component of recent medium-scale macroeconomic models, but to date there has been little microeconomic evidence supporting the assumption of sluggish nominal wage adjustment. We present evidence on the frequency of nominal wage adjustment using data from the Survey of Income and Program Participation (SIPP) for the period 1996–1999. The SIPP provides high-frequency information on wages, employment, and demographic characteristics for a large and representative sample of the U.S. population.

The main results of the analysis are as follows: (1) After correcting for measurement error, wages appear to be very sticky. In the average quarter, the probability that an individual will experience a nominal wage change is between 5 and 18 percent, depending on the samples and assumptions used. (2) The frequency of wage adjustment does not display significant seasonal patterns. (3) There is little heterogeneity in the frequency of wage adjustment across industries and occupations. (4) The hazard of a nominal wage change first increases and then decreases, with a peak at 12 months. (5) The probability of a wage change is positively correlated with the unemployment rate and with the consumer price inflation rate.

JEL Classifications: E24, E32, J30

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This paper, which may be revised, is available on the web site of the Federal Reserve Bank of Boston at http://www.bos.frb.org/economic/wp/index.htm.

We thank Mark Bils, Mattgeo Iacoviello, Peter Klenow, Peter Ireland, Emil Nakamura, Fabio Schiantarelli, Jon Steinsson, and seminar participants at Boston College, CREI, the European Economic Association Annual Meetings, the Federal Reserve Bank of Boston, the Federal Reserve Bank of San Francisco, and the NBER Summer Institute for useful suggestions and comments. The usual disclaimer applies.

This version: June 8, 2010

1 Introduction

It is difficult to explain the estimated real effects of monetary policy shocks without assuming that some nominal variables adjust sluggishly. In the *General Theory*, Keynes (1936) assumed that nominal wages were rigid, and thus that expansionary monetary policy would reduce real wages and increase employment and output. Fischer (1977) and Taylor (1980) showed that nominal wage contracts would have similar effects even in explicitly dynamic models with rational expectations. Recent macro-econometric models have typically followed the important contribution of Erceg, Henderson, and Levin (2000), and assumed that both prices and nominal wages are slow to adjust.

The large number of recent models with such features has inspired researchers to examine micro data on the frequency of price changes for individual products, with notable papers by Bils and Klenow (2004) and Nakamura and Steinsson (2008). However, to date there has been little research using micro data to estimate the rigidity of nominal wages — even though Christiano, Eichenbaum, and Evans (CEE 2005) find that nominal wage rigidity is more important than nominal price rigidity for explaining the dynamic effects of monetary policy shocks.

Our paper attempts to address this gap in the literature. The lack of previous work on the business cycle implications of nominal wage rigidity using micro data may be due in part to a lack of suitable datasets. We provide evidence about the frequency of wage adjustment in the United States using data from the Survey of Income and Program Participation (SIPP). The SIPP is a survey run by the Bureau of Labor Statistics (BLS). It provides individual wage histories for a large and representative sample that is followed for a period of 24 to 48 months. Importantly, the individuals are interviewed every four months. These data allow us to examine wage changes using high-frequency data. (Most previous work on nominal wage rigidity using U.S. micro data has used the PSID, which is an annual survey and thus less useful for high-frequency analysis. Other well-known sources of micro wage data, the CPS and the Employment Cost Index (ECI), do not provide sufficiently long time-series data on individual wages and thus cannot be used for our purpose.) We use the longest panel of the SIPP for which complete data are available: the 1996 panel (run from March 1996 to February 2000).

In our main results, we focus on the frequency of nominal wage adjustments disregarding employment history. This is arguably the concept that is most relevant for macro models with nominal wage rigidities, particularly medium-scale DSGE models *a la* CEE (2005). The reason is that most business-cycle models with nominal wage rigidity follow Blanchard and Kiyotaki (1987) and assume that all workers are monopolistically competitive suppliers of differentiated labor services. In this framework, the worker sets the wage, and revises it occasionally on his/her own schedule, thus making the sequence of wages the relevant series to examine, regardless of employment history.

We use as our baseline the results for hourly workers (or wage earners) who reported their hourly wages to the SIPP interviewer. The reason is that computing wages as hourly earnings increases measurement error. For the baseline results we chose to focus on the statistic measured with least error, the hourly wage, at the cost of making the sample less representative. However, we also present results for the sample of salaried workers, using their monthly earnings as their "wage" measure. By reporting the results both for hourly workers and for salaried workers, we leave the decision of the "right number" for macroeconomics to individual researchers who may be interested in calibrating their models using our estimates.

Regardless of the sample used, it is clear that the data are contaminated with a significant amount of measurement error. This is a disadvantage of working with data on individual wages, which in U.S. survey data are always self-reported.[1] We deal with this problem by applying to the reported wage and earnings series the correction for measurement error introduced by Gottschalk (2005), who built upon the work of Bai and Perron (1998 and 2003). The application uses the identifying assumption that wages are not adjusted continuously but are changed by a discrete amount when an adjustment takes place, which corresponds to our usual intuition about labor market institutions. The implied statistical model says that the true wage (or earnings) is constant for an unspecified period of time and then changes discretely at unspecified breakpoints. Thus, true wage changes in a noisy series can be estimated as one would estimate structural break dates in a standard time series. The Bai-Perron-Gottschalk method is to test for a structural break at all possible dates in a series. If one can reject the null hypothesis of no break for the most likely break date, then assume that there is a break at that point in time. Then examine the remaining sub-periods for evidence of structural breaks, and continue until one cannot reject the hypothesis of no break for all remaining dates. The adjusted series have wage (earnings) changes at all dates where we can reject the no-break hypothesis, and are constant otherwise. This is a systematic way of excluding many instances of transitory wage changes that look very much like measurement error.

[1]Surveys in some other countries have access to administrative data from payroll or tax records, which reduces measurement error significantly.

We present some examples in the paper where we apply this method to SIPP data for individuals in our sample.

We find the following main results. First, after correcting for measurement error, wages appear to be very sticky. In our baseline result with hourly workers, we find that the probability of a wage change is about 18 percent per quarter, thus implying an expected duration of wage contracts of 5.6 quarters. For salaried workers, the probability of an earnings change is only 5 percent per quarter. By comparison, several key papers estimating DSGE models using macro data estimate this probability to be about 30 percent per quarter, so we are finding significantly more nominal wage stickiness than macroeconomists typically assume. Second, the frequency of wage adjustment does not display any significant monthly or seasonal pattern. Third, we find little heterogeneity in the frequency of wage adjustment across industries. Wages in manufacturing appear to be stickier than wages in services. Similarly, we find little heterogeneity across occupations, although the frequency of wage adjustment is somewhat larger for service-related occupations. Fourth, we find that wage changes are significantly right-skewed, in keeping with preceding papers (for example, Gottschalk, 2005) that have found evidence of downward nominal wage rigidity in microdata. Fifth, the hazard of a nominal wage change first increases and then decreases, with a peak at 12 months. Thus, at a micro level, the pattern of wage changes appears somewhat more in keeping with the staggered contracting model of Taylor (1980) than with the constant-hazard model of Calvo (1983). However, our second result suggests that the timing of wage contracts is uniformly staggered throughout the year, which is the pattern that gives maximum persistence of nominal wages following a shock. Sixth, the probability of a wage change is positively correlated with the unemployment rate and with the rate of consumer price inflation. Finally, we show that higher wage stickiness makes it easier for macroeconomic models to match the stylized fact that monetary shocks cause persistent changes in real output and small but relatively persistent changes in prices.

This paper is connected to several strands of the literature. The first is the literature assessing wage rigidity using micro data. Much of this previous literature has concentrated on the different issue of downward nominal wage rigidity, rather than the frequency of wage adjustment *per se*. For example, Kahn (1997) reports evidence of a substantial downward nominal wage flexibility using U.S. data from the PSID. Gottschalk (2005) analyses data from the SIPP and, after correcting for measurement error, finds much greater downward wage rigidity. Lebow et al. (1999) use the

Employment Cost Index and also find a substantial amount of downward wage rigidity. Dickens et al. (2007a) summarize the results coming from the International Wage Flexibility Project (IWFP).[2] The IWFP has collected data on wages and analyzed wage dynamics using data from a large number of countries. The main focus of this project is to perform analysis of wage dynamics that are comparable across different countries.[3] Perhaps unsurprisingly, one of the main findings of the project is that wage rigidity varies substantially across the different countries studied. This finding suggests that one should be careful in extrapolating our results to different countries and perhaps even to different time periods. Finally, in a very recent contribution, Heckel et al. (2008) analyze the frequency of wage adjustment for a large sample of French firms for the period 1998–2005.

Our paper is also related to the macro literature on nominal wage rigidity. Recent medium-scale macroeconomic models have used the sticky-wage assumption extensively. Most of these models, estimated through Bayesian techniques using aggregate data, suggest that nominal wages are quite sticky. However, as recently pointed out by Del Negro and Schorfheide (2008), this approach to estimation often delivers estimates that mirror the priors. In their conclusions, Del Negro and Schorfheide advocate more empirical analysis of microdata, along the lines of the work by Bils and Klenow (2004) and Nakamura and Steinsson (2008) on the frequency of price adjustment.[4] We view our paper as a first step towards providing similar micro estimates for wage dynamics.

A prominent strand of the literature on wage and employment dynamics over the business cycle has focused on search and matching models of the labor market.[5] Our paper is not directly related to this line of work. First, these papers are formulated in purely real terms, so the relevant concept is real wage rigidity, rather than the nominal rigidity we examine. Second, the search and matching framework indicates that the issue that matters for macroeconomic purposes is whether a pre-set wage paid to current employees is also applied to new hires.[6] We estimate the wage stickiness that matters in a monopolistically competitive labor market setting, which is the frequency of wage changes for an individual regardless of employment history. Haefke et al. (2008) and Hall and Krueger (2008) examine micro evidence more related to the key predictions of the search literature.

[2]Other contributions are Dickens et al. (2007b) and Druant et al. (2008).

[3]The United States was included among the countries studied for assessing downward wage rigidity but not among those used to analyze wage stickiness. The reason, again, is that the IWFP data on U.S. wages comes from the PSID, which provides only annual data.

[4]Although they warn that aggregation is a key issue when inferring macro behavior from micro evidence.

[5]For example, Shimer (2005) and Hall (2005).

[6]See Gertler and Trigari (2009).

Finally, our results shed some light on a small but interesting literature on the seasonal effects of monetary policy shocks. Recently, Olivei and Tenreyro (2008) have found that monetary policy shocks that occur in the first half of the year have larger real effects than those that occur later in the year. They explain this result by positing a model where wage changes are more likely to occur in the second half of the year. We find that while the frequency of wage changes is indeed slightly higher in the second half of the year, the magnitude of the difference is much smaller than assumed in the calibrated model of Olivei and Tenreyro, suggesting that a different model might be needed to explain their very interesting empirical finding.

The structure of the paper is as follows. The next section discusses the SIPP sample and the data definitions that we use. Section 3 summarizes the methodology we use to correct the wage series for unobserved measurement error. Section 4 contains the main results of the analysis obtained using the sample of hourly workers, while Section 5 presents the results for salaried workers. Section 6 explores the implications of the results obtained for the characteristics of a standard macroeconomic model. Section 7 contains the discussion of the hazard estimates and Section 8 concludes, and suggests some directions for future research.

2 Data

The data source for this paper is the Survey of Income and Program Participation (SIPP). The SIPP data have been collected by the Bureau of Labor Statistics (BLS) since 1983, with a major revision in 1996. The SIPP sample is a multi-stage, stratified, representative sample of the U.S. population. A large number of individuals are interviewed in order to collect detailed data regarding the source and amount of their income, a variety of demographic characteristics, and their eligibility for different federal programs. Each individual is followed for a period ranging from 24 to 48 months, with interviews taking place every four months.

The SIPP has at least two advantages compared to the other two large surveys used for this kind of analysis, namely the Outgoing Rotation Group (ORG) data from linked Current Population Surveys, and the Panel Study of Income Dynamics (PSID). First, unlike the PSID, the SIPP provides us with high-frequency information about wage changes. The near-quarterly frequency of the SIPP data makes it much more relevant for analyzing business cycles. Second, unlike the ORG, where an individual is interviewed for four consecutive months, not interviewed for the next eight months, and then interviewed for another four months before being dropped from the sample, the 1996 panel of the SIPP, which we use, follows each individual for up to 48 months, thus creating the proper panel data essential for our analysis.[7] Finally, the SIPP reports more-reliable information on wages and hours than the ORG or the PSID. In both these surveys, respondents are asked about their income only once a year, and must recall the amount and type of their income from various sources over the preceding calendar year – a daunting prospect for most people. By contrast, in the SIPP workers paid by the hour are asked specifically for their hourly wage rate in each interview.

We focus on the longest panel of this survey for which complete data are available: the 1996 panel (run from March 1996 to February 2000). For each person in the panel, we have time-series information about their wage rate (if they get paid by the hour) and monthly earnings as well as their industry and occupation. Table 1 reports basic descriptive statistics for our sample. The 1996 Panel follows 39,095 people, 49.4 percent of whom are women. We restrict our sample to workers between 15 and 64 years of age. The average person in our sample is around 38 years old.

Our first step aimed at minimizing measurement error is to focus on the smaller sample of those people who directly reported their hourly wage to the SIPP interviewer (because they are paid by the hour). Focusing only on hourly workers, however, has the drawback of reducing the size and representativeness of the sample for the full population. Thus, we also report separately the results for salaried workers. Of course, the latter do not report their wage rates. For salaried workers, we use monthly earnings as opposed to hourly earnings. The reason for this is that using hourly earnings would introduce much more noise into the wage history for salaried workers (since the earnings are divided by the worker's recollection of hours worked, which is also likely to suffer from measurement error).

Our smaller sample of hourly workers includes 17,148 people. Table 1 gives basic descriptive statistics of reported wages and earnings. The average wage rate in our sample for hourly workers is \$10.03. There is, however, a great deal of heterogeneity. The 5^{th} percentile of the distribution of wages is \$5 and the 95^{th} is \$20. The average monthly earnings for salaried workers is about \$3,000 dollars. The 5^{th} percentile of the distribution of earnings is \$440 and the 95^{th} is \$6,800.

Tables 2 and 3 report the breakdowns by industry and occupational category at the one-digit

[7] In fact, the CPS is even less suitable than this summary indicates, because the sampling unit is the household and not the individual. An individual leaving the housing unit is not followed; instead, new residents become survey members.

level. As Table 1 shows, services is the most highly represented industry (33.3 percent of total hourly workers), followed by trade (26 percent) and manufacturing (21 percent). Agriculture and mining, on the other hand, have very few observations. As for occupational categories, among hourly workers technical sales and support is the most highly represented in our sample (30 percent) followed by machine operators (24 percent) and services (19 percent). On the other hand, professionals and managers account for only 13 percent of the total in the hourly workers sample, while they represent almost 30 percent of the entire survey. Not surprisingly, our smaller sample under-represents occupational categories where workers are less likely to report receiving hourly wages.

3 Method

A key to our results is the need to limit the impact of measurement error in assessing the frequency of wage adjustment. Our first way of achieving this objective is to reduce the sample to the people who are hourly workers and reported their base wage rates to the SIPP interviewer. We prefer to concentrate on the hourly wage, since earnings may vary at the same wage if people change their hours worked. We also believe that people paid by the hour are likely to remember their hourly wage rates, but few people recall their monthly earnings down to the last dollar.

A second step is to apply to the reported data the procedure introduced by Gottschalk (2005), which is intended to purge the wage series of unobserved measurement error.[8]

The procedure relies upon the Bai and Perron (1998 and 2003) method to test for structural breaks in the time-series context. The key identifying assumption is that wage changes take place in discrete steps. Assume that an individual works for T periods experiencing s wage changes at times $T_1 ... T_s$. The observed wage at time t, w_t, is equal to a constant α_t plus the unobserved measurement error ϵ_t:

[8]We also apply this procedure to earnings. We apply the same procedure to the reported wage series for hourly workers and to the reported earnings series for salaried workers. We refer only to wages in this section for expositional simplicity.

$$
\begin{aligned}
w_t &= \alpha_1 + \epsilon_t \qquad t = 1...T_1 \\
&= \alpha_2 + \epsilon_t \qquad t = T_1...T_2 \\
&= ... \\
&= \alpha_{s+1} + \epsilon_t \qquad t = T_s...T.
\end{aligned} \tag{1}
$$

The objective is to estimate the s break dates and the $s + 1$ constant wages. The method proposed by Bai and Perron proceeds sequentially. First, using the whole sample of T observations, assume that there is one structural break, and pick the break date that minimizes the sum of squared residuals (SSR). Then test to see whether one can reject the null hypothesis of no break over the entire sample against the alternative that there is a break at the point that minimizes the SSR.[9] If one cannot reject the null, then the procedure is finished, and one concludes that there are no structural breaks in the sample (that is, the wage is constant over the whole sample). If one can reject, then test for structural breaks in each of the subperiods identified by the break test. Again, pick the date that minimizes the SSR in each subperiod, and then test whether a significant break is detected at that point. Continue until no significant structural break is detected in any of the remaining subintervals of data.

One might object that this procedure is biased towards finding wages that are sticky, since the identifying assumption is that wages are set in nominal terms for a certain period of time! However, we do not constrain the procedure to assume any minimum number of periods between true wage changes. So, for instance, if an individual is followed for 48 months, corresponding to 12 interviews, the procedure can detect up to 10 wage changes.[10] One might then ask whether the procedure would be able to estimate a large number of breaks in a short time series. This is the important issue of the power of the test, which we discuss at length below.

[9]Given the short length of the wage histories, the critical values for the structural break tests are obtained through Monte Carlo simulations. The appendix contains a more extensive explanation of the procedure adopted to compute the correct critical values.

[10]Given that some people are observed for less than 48 months, we calculate a maximum quarterly frequency of true wage changes potentially obtainable. For people interviewed 12 times, for example, the probability is 83 percent (=10/12). Computing a weighted average of these probabilities across different numbers of interviews, we get a maximum detectable quarterly probability of a true wage change of 56 percent, which is much higher than we actually estimate

Individual examples illustrate how this procedure works.[11] Figure 1 shows the reported and the adjusted wage series for "Linda," a 40-year-old secretary with high school degree. The reported series,[12] shown by the dashed line, is characterized by five wage increases and three wage decreases over the period considered. By contrast, the adjusted series, shown by the solid line, shows only two breaks, from $12.54 to $12.83 and from $12.83 to $13.56 (the last figure being the average of the subsequent reported wages).

Figure 2 reports the distribution of the measurement error as implied by our correction procedure. As the figure shows, the distribution of the measurement error appears symmetric, with a big spike at zero.

Figure 3 shows the reported and the adjusted series for the hourly earnings of "Christina," a 40 year-old health care worker. Figure 3 highlights a potential problem with our methodology: the low power of the test for structural breaks (especially in a short time series) might lead us to underestimate the probability of a wage change. If people in our sample with true wage changes have sufficiently noisy reported wages (because of measurement error), then our test might fail to reject the null of no wage change.

Our application of trend-break tests to estimating the average frequency of wage changes raises several statistical issues. We present an in-depth discussion of these issues and our solutions in the appendix. Here we sketch an overview of this material.

The first problem is that standard F-tests are not the appropriate method to test for a structural break in wage series, for two conceptually different reasons. First, even in a setting where the error term is i.i.d. and Gaussian, a standard F-test can only be used to test a hypothesis about a structural change at a given date. By contrast, we perform tests for structural breaks at every possible date and then test for a structural break at the date where such a break appears most likely, and the distribution of this maximum F-statistic is different from a standard F distribution. This problem was addressed by Bai and Perron (1998). Second, the error term is not i.i.d., because the measurement error in wages is not a classical white noise error. In order to address this second problem, we need to know the structure of the measurement error. Fortunately, Gottschalk and Huynh (forthcoming) provide the required statistics. They compare reported SIPP wages with wages for the same people in an administrative data set, where wages are based on tax records and

[11] We made up the names of the individuals in these examples.

[12] ($12.53, $12.55, $12.53, $12.83,$12.83,$12.83, $13.5 $13.61 $13.4 $13.7 $13.61)

thus measured essentially without error. The difference between true and reported wages allows them to estimate the measurement error process for the SIPP. Armed with this information, we address the first and second problems together via Monte Carlo simulations (microsimulations). We simulate wage histories for individuals whose wages are constant in the simulations, but are assumed to be observed with measurement error of the type found by Gottschalk and Huynh. Based on these simulations, we calculate the critical value for the test of a structural break that will give us the required size of the test (the probability of Type I error).

Following this procedure ensures that we have a consistent estimator of the break dates for individual wage histories. However, just tabulating the frequency of these breaks does not give us a consistent estimator of the frequency of nominal wage changes in the population. Again, there are two reasons. The first is the size of the test. Even if all wages were constant, the fact that we pick the critical values to ensure a certain probability of Type I error means we would falsely conclude that α percent of wages change each period, where α is the size of the test. By itself, this force would lead us to conclude that wages are more flexible than is really the case. But of course there is also the issue of power — we do not necessarily detect every true break in the wage series. Low power will lead us to make the opposite mistake, and conclude that wages are more sticky than is really the case. We again address the issue of power and its implications for our estimated wage change frequency via simulation. Using simulations where true wages change in the sample, we can calculate the power of the test. Once the power of the test is known (of course, the size is also known, as it is a parameter we specify), we can adjust the raw frequency count of estimated breaks for both Type I and Type II error, and get a consistent estimate of the actual frequency of nominal wage changes. (See the appendix for details.) It is the frequency adjusted for Type I and Type II errors that we report in the next section.

4 Main Results

As noted in the introduction, we face the difficult task of mapping the large set of outcomes in micro data into simple macro models. To guide our exercise, we stick as closely as possible to estimating key parameters for the labor market institutions assumed in macro models with nominal wage stickiness, although these institutions surely characterize only a subset of the rich heterogeneity of employer-employee relationships present in our micro data. In macro models of this type, each worker is assumed to be a monopolistically competitive entrepreneur, supplying a unique variety of labor and setting his or her own wage. An example is the behavior of an independent contractor, such as a plumber or electrician, who charges according to a "rate sheet" specifying the wage charged per hour. Such a worker may work at a number of different residences over the course of a day, thus being paid by several different "employers" in quick succession and experiencing a number of very short "employment spells." Or the contractor might work on a single, large project for several weeks or even months, which would show up in the data as a long employment spell. But the rigidity of the contractor's nominal wage depends on the frequency with which she or he revises the rate sheet. In this framework, the right statistic to examine is the frequency of nominal wage changes (rate sheet revisions) over the entire sample for which we have data, disregarding any job transitions as irrelevant. For this reason, all the results presented in this section refer to the entire wage history of each individual, regardless of his or her employment history.

While our data and analysis are at conducted with interviews taking place every four months, we report the results at a quarterly frequency for ease of comparison with the previous literature.[13] Table 4 reports the frequency of wage adjustment for hourly workers. The quarterly frequency of wage adjustment for reported wages is very high. In the typical quarter for the 1996 panel, 48.1 percent of people report a different wage than they reported in the previous interview. The situation changes radically when considering the adjusted series for wages. In the 1996 panel, in the typical quarter only 17.8 percent of hourly workers experience a change in their wage.

Under the assumption that the correction for measurement error is appropriate, we therefore find evidence of greater wage stickiness than previously found in estimated DSGE models using aggregate data for the U.S. economy. CEE (2005), for example, estimated the quarterly Calvo probability of wage adjustment to be 36 percent in their benchmark model. In SW (2007) the benchmark estimate of the same parameter is 26.2 percent. Our results are more consistent with the findings of Gottschalk (2005), who used the same method but analyzed a previous wave of SIPP data. Gottschalk does not report exactly the parameter we estimate, but computing the analogous statistic from his adjusted wage series gives a figure closer to ours (11 percent). The difference can be partly explained by the fact that Gottschalk analyzes a different time period (1986–1993), and that arguably the U.S. labor market became more flexible over time. Finally, in

[13] We transform the results into quarterly results by multiplying by $\frac{3}{4}$.

a recent contribution, Heckel et al. (HLM, 2008) found the average quarterly frequency of wage adjustment to be 35 percent for a large sample of French firms. However, HML have access only to firm-occupation data; therefore, they cannot test the frequency of wage change at the individual level nor correct for measurement error in the reported wages.[14]

We should note that macro estimates of the nominal wage parameter are not always estimated to be around 0.30. In fact, CEE (2005) estimate variants of their baseline model, in two of which (no habit formation, and low investment adjustment costs) the estimated degree of wage stickiness is substantially higher, and close to our micro results. Perhaps future work on estimated DSGE models should simply follow CEE's policy and use micro evidence like ours as a way of disciplining macro estimates based on aggregate data.[15]

4.1 Seasonality

A second question we explore regards the seasonality of the pattern of wage adjustment. Olivei and Tenreyro (2008) find that monetary shocks have much larger effects on output if they occur in the first half of the year than if they occur in the last two quarters. They explain their findings by proposing a model where wage adjustment is seasonal, and is much more likely to take place in the second half of the year. Their calibrated model assumes that 24 percent of annual wage changes occur in the first quarter, 2 percent in the second quarter, 32 percent in the third quarter, and 42 percent in the fourth quarter. However, they explain that this calibration is based on a small sample of New England firms because "there is no systematic empirical evidence pointing to particular values for the [quarterly wage change frequencies]." We can supply this evidence using direct observation for a representative sample of the U.S. economy.

Figure 4 illustrates the frequency of wage adjustment by month. The frequency of wage adjustment for both the reported series and the adjusted series does not display the kind of sizable seasonal pattern assumed in Olivei and Tenreyro's calibration.

In order to investigate more formally the seasonality in the frequency of wage adjustment, we regress the probability of wage adjustment for both the reported and the adjusted wage series on a set of quarterly dummies, where the excluded category is the frequency of wage changes in the

[14] Moreover, the mean wage in a firm-occupation cell can change even if there is no change in the wage for any individual worker, if the composition of the individuals in the cell varies over time

[15] CEE (2005, p. 40) write "Our position is that a reasonable contract length is one that matches the duration of contracts found in survey evidence. In this respect, we follow the empirical literature on wage and price frictions."

first quarter.

Table 5 reports the results. The F-test of joint significance of the quarterly coefficients always rejects the hypothesis that they are all zero. The results qualitatively support the assumption that drives Olivei and Tenreyro's model: Wage changes, do in fact, appear to be more likely in the second half of the year, not at the beginning. However, the magnitude of the difference is much smaller than Olivei and Tenreyro's calibration assumes. In our data, the proportion of quarterly wage changes relative to the total number of wage changes in a year is 23.6 percent in the first quarter, 24.1 percent in the second quarter, 26.6 percent in the third quarter and 25.5 percent in the last quarter. Whether these small differences can explain the differential seasonal effects of monetary policy shocks is an open question, but we suspect that they cannot.[16]

Interestingly, Heckel et al. (2008), using French firm-level data, find evidence that the frequency of wage adjustment is highly seasonal, with a spike in the third quarter. As the authors emphasize, this finding might be due to a very specific institutional feature of the French labor market, where by law the minimum wage is updated each year in July. However, there is no such feature in the U.S. labor market. Anecdotal evidence, in fact, suggests that in the United States wage changes indeed take place in January in some firms, but in other firms they occur at the hiring date of the worker. In still other firms, wage changes are implemented at the beginning of the fiscal year.[17]

4.2 Heterogeneity

Our access to micro data allows us to explore whether wage stickiness differs across sectors or occupations.

Table 6 reports the results from regressing the probability of a wage change for hourly workers on a full set of industry dummies. The first two columns report the probability of a wage change obtained using, respectively, reported and adjusted wages. The third and the fourth columns report the differences relative to the manufacturing industry. While in general the hypothesis of total absence of heterogeneity is always rejected by the data, as shown by the p-value of the hypothesis that all the coefficients are zero, there is not much evidence of heterogeneity across industries in the frequency of wage changes. Services, trade and transport and communication display lower levels

[16] See Dupor and Han (2009) for further discussion and a test of the hypothesis advanced by Olivei and Tenreyro (2008).

[17] In some peculiar cases, the wage changes take place on the birthday of the company!

of wage skickiness than manufacturing, while construction, mining, and agriculture do not display significantly different coefficients.[18]

Table 7 repeats the exercise of Table 6, but now for different occupations. The coefficients in the third and fourth columns are relative to production workers. Again, we find only few significant results. One such result is that there is less wage stickiness in occupations related to services and in managerial occupation.

The result of relatively little heterogeneity across industries and occupation might seem puzzling. We explored whether this is an artifact of aggregation by exploring the data at the 2-digits level of disaggregation (for both industries and occupations). While the point estimates display significantly more dispersion using the more refined classifications, the very small number of observation available for each category prevented us from finding differences that are statistically significant.[19]

4.3 Downward Nominal Wage Rigidity

In order to address a question typically asked by the labor literature on wage stickiness, we provide some evidence on the importance of downward nominal wage rigidities. Figure 5 reports the histogram of the non-zero adjusted wage changes. In order to avoid including outliers in the calculations, we plot only the inner 98 percentiles of the distributions (that is to say, we exclude the lowest and the highest percentiles). As the graphs show, wage reductions are much less frequent than wage increases. More precisely, they correspond to 11.5 percent of the non-zero wage changes. It is important to remember that we are analyzing one of the highest-growth periods of the last several decades, when nominal wage declines were probably less likely than in normal times.

Our results show that the period between 1996 and 1999 has been characterized by infrequent nominal wage cuts, which is normally taken as evidence of "downward nominal wage rigidity" in the literature. In our view, the term "rigidity" implies a friction, in this case a barrier to wages tracking the value marginal product of labor. Many authors have advanced the hypothesis that this barrier is stronger when the marginal product declines than when it rises. In our view, the evidence that we have produced (like the similar evidence that has been offered elsewhere in the literature) does not establish the existence of a rigidity in this sense. We think that a rigorous

[18] But this is also partly due to the imprecision of these estimates resulting from the very small number of observations for these industries.

[19] The results (not shown) are available upon request.

test of the hypothesis would require a model and further empirical work that would establish a baseline for the expected shape of the wage distribution. Absent such a baseline, we can establish that nominal wage cuts are infrequent, but this finding does not have a structural interpretation.

4.4 Cyclicality

A fourth question of interest is the correlation between the frequency of wage adjustment for hourly workers and some macroeconomic variables.

Figure 6 shows a scatter–plot of the cyclical components of the hp-filtered frequency of wage adjustment and the realized monthly inflation rate, computed as the percentage change of the U.S. CPI for all goods and all cities. As the figure shows, the two variables are positively correlated. The correlation between the two variables is 0.38 and is statistically significant.

Figure 7 shows a scatter-plot of the cyclical components of the hp-filtered frequency of wage adjustment against the monthly total U.S. unemployment rate. The relationship between the two variables is positive, with a correlation coefficient of 0.28, but it is not statistically significant .

In considering these results it is important to realize that the answers we provide in this section are based on an extremely short time-period (only four years of data), and hence must be considered with great caution. Subject to this caveat, the evidence suggests that pure time-dependent models of nominal wage rigidity, such as the models of Calvo and Taylor, cannot capture the whole story of infrequent nominal wage changes. Nominal wage adjustments appear to also have a state-dependent component. However, unlike the literature on nominal price rigidity, which has explored state-dependent models of price changes, we are not aware of models of nominal wage rigidity that are state- rather than time-dependent. Our results suggest that such models may be necessary. It would be useful to redo an exercise like ours using data from periods where the inflation and/or unemployment rates were high, to see whether the evidence of state-dependent wage changes is stronger in such periods.

5 Salaried Workers

While we chose to present our baseline results using only the sample of hourly workers for whom we have hourly wage data, here we also present estimates obtained using the sample of salaried workers. In order to do that, we focus on monthly earnings (as opposed to hourly earnings) and we apply the same measurement error correction procedure to earnings that we used for wages.

Table 8 reports the basic results for salaried workers. The average quarterly frequency of earnings change is about 65 percent. The results we get with the adjusted series are much smaller, 4.9 percent. In order to gain intuition about what might drive these extreme results, it is useful to discuss two examples.

Figure 8 shows the reported and adjusted series of monthly earnings for "Mark," a 42-year-old manager at a business service firm. This example clarifies how our procedure is able to capture any kind of change in earnings, provided that it is sufficiently large or persistent. Figure 9 reports instead the earnings history of "Peter," a 39-year-old mechanic. The reported series is very noisy, with continual ups and downs in the reported earnings. Given the procedure explained in Section 3, the reader will not be surprised to see that in such a case the suggested adjusted earnings profile is totally flat. The predominance of reported earnings' histories similar to Peter's is the likely cause of the results shown in Table 8.

We repeat the same exercises we did for hourly workers on the sample of salaried workers. Figure 10 shows how the frequency of earnings changes varies by calendar month. It is hard to see any sizable seasonal effect. We investigate this hypothesis more formally, and Table 9 reports the results. While we always reject the hypothesis that wage changes are uniform over the year, the magnitudes of the differences are always very small.

The results for heterogeneity across industries and occupations are reported in Tables 10 and 11. While service and trade still exhibit higher wage flexibility than manufacturing, transport and communication industries, the group does not seem to have greater wage flexibility than manufacturing in the case of salaried workers. On the other hand, for salaried workers the only occupation category that displays a significant difference from the production workers category is that of service workers, who display more wage flexibility.

Finally, correlating the frequency of earnings adjustment for salaried worker with macro variables, we confirm a positive relationship with the unemployment and inflation rates, as in the case of hourly workers. The correlation with the monthly inflation rate is slightly lower (0.34). The correlation with the unemployment rate is 0.20 and is not statistically significant.

6 The Importance of Sticky Wages

We evaluate the significance of our findings for macroeconomics by using our parameter estimates in a benchmark medium-scale macro model. We use the DSGE model proposed by Smets and Wouters (2008; henceforth SW).[20] We take the model exactly as presented in their article, and we perform two simple exercises.

First, we estimate all the parameters of the model through Bayesian techniques after fixing the parameter for wage stickiness at 0.178 (our baseline result for hourly workers) and at 0.105.[21] Second, we compute the impulse response functions produced by the model following a monetary shock, using both the parameter estimates of the original SW paper and the estimates we obtained from our first exercise using data for hourly workers.

Table 12 reports the results obtained for some key parameters of the model. The first column reports the mode found in SW, the second column displays the posterior mode we find after fixing the wage stickiness parameter to 0.178, and the third column what we get with 0.105. As the table shows, the parameters related to price stickiness, price indexation, and wage indexation do not differ dramatically among the three cases. Also, the elasticity of intertemporal substitution appears to change very little. Interestingly, instead, the elasticity of labor supply increases a bit under our specification, going from 1.92 to 2.28 to 2.61. Finally, the capital share in the production function increases in our specification from the 0.19 obtained by SW to a more standard value of 0.3, which is also more consistent with long-run evidence from national income shares.

In terms of the dynamic responses to shocks, Figure 11 reports the impulse responses to a monetary policy shock. The solid line is the impulse response from the SW model using the parameters reported in their paper, the long-dashed line reports the results from the model using our parameter estimates for hourly workers, and the dot-dashed line the results obtained for the totality of the workers. As expected, we find that with our estimates the model produces a larger and more persistent response of output and consumption to a monetary shock. This is not particularly surprising, since the micro data indicate that wage stickiness is higher than SW estimated based on aggregate data, while our estimates of the other structural parameters are substantially unchanged. The responses of hours do not differ dramatically, while the responses of the real wage and of price

[20]We use the code of the model that is available on the *American Economic Review* website.

[21]Roughly the weighted average of the findings obtained for hourly workers and salaried workers using the numbers of workers as weights.

inflation appear to be damped and more persistent in our estimation.

Consistent with intuition, the findings show that higher wage stickiness makes it easier for macroeconomic models to match the stylized fact that monetary shocks cause persistent changes in real output and small but relatively persistent changes in prices.

7 Hazard Functions

Thus far, we have intentionally limited ourselves to computing a statistic that can be interpreted as the constant hazard of a wage (earnings) change, which is also the statistic estimated in macroeconometric models. However, our data allow us to test whether the hazard is truly constant, by estimating hazard functions. The estimated hazards let us compare the fit of Calvo-style models of wage rigidity—which imply a constant hazard of experiencing a wage (earnings) change—*vis a vis* other alternatives, such as contract renegotiations at fixed intervals as in the Taylor (1980) model, which would imply hazard functions that peak at certain durations.

To explore this issue we first use the reported and the adjusted wage (earnings) series to estimate a discrete-time hazard model, where an exit is defined as a change of the reported (or adjusted) wage (or earnings).[22] A new spell starts each time a new wage (or level of earnings) is observed, and we include in the sample all non-left-censored spells. We control for age, gender, and educational attainment, and we include a full set of duration dummies. We use the estimated coefficients on the duration dummies to find the hazard function for wage (earnings) changes.

Figure 12 shows the estimates of the hazard obtained using the reported wage series for the hourly workers and the reported earnings series for the salaried workers. The hazards are decreasing, with more than half of the respondents experiencing a wage change in the first four months. Declining hazards imply that the highest probability of having a wage change is immediately after the previous wage change. This pattern is intuitively unreasonable, suggesting that there is indeed significant measurement error in the reported wage. Figure 13 reports the estimates of the hazard obtained using the adjusted wage and earnings series. Here, by contrast, there is a clear peak at 12 months in both series.

We conclude that Taylor-type fixed-length contracts have stronger empirical support than Calvo-type constant-hazard models. However, the fact that the wage change frequency is almost

[22] See Box-Steffensmeier and Jones (2004) p. 73.

flat over the calendar year (Figure 4 and Table 5) suggests that the starting time of the wage contracts is uniformly staggered throughout the year. This pattern is, of course, the one that gives the largest contract multiplier and creates maximum persistence of the real effects of nominal shocks. Although it gives the greatest persistence, uniform staggering is typically found to be an unstable Nash equilibrium, so it is interesting that we are finding indirect evidence of staggered, rather than synchronized, wage contracts.[23]

8 Conclusion

Since we already outlined the main results in the introduction, we conclude by suggesting directions that future research might take.

First, it is important to understand why the stickiness estimated from micro data is greater than that estimated from aggregate data using Bayesian techniques. Idiosyncratic measurement error, such a large concern in the analysis of micro data, is unlikely to be the explanation. Such errors would average out and contribute little to the variance of any aggregate wage series. One possibility is that the difference is due purely to aggregation issues: for example, if high-wage workers' wages also adjust more frequently, then the aggregate wage will appear to more flexible than the average worker's wage. We plan to investigate this possibility using our data, but since high-wage workers are likely to be salaried workers, whose adjusted earnings we find to be stickier than the wages of hourly workers, this explanation appears unlikely. The reasons for this micro-macro gap should shed light on the perplexing issues of aggregation that must concern all macroeconomists interested in "structural" models.

Second, the lack of sizable seasonality in wage changes leaves an open question: what can explain the estimated differential effects of monetary shocks occurring in different quarters? Nakamura and Steinsson's (2008) finding that price adjustment is seasonal suggests one answer.[24]

Third, the findings on the shape of the hazard functions suggest that we should explore the properties of models based on fixed-length wage contracts, as in Taylor (1980), in addition to the

[23] Our findings are consistent with the empirical studies of Taylor (1983) and Cecchetti (1984), who found staggered wage setting in union contracts. However, in the U.S. labor market, very few workers are covered by formal union contracts, so it is useful to extend their results to a representative sample of the U.S. labor force. Some notable papers show that in richer models staggering might be a stable Nash equilibrium after all. See, for example, Fethke and Policano (1984), Ball and Cecchetti (1988), and Bhaskar (2002).

[24] Of course, Dupor and Han (2009) question the robustness of the finding itself.

very tractable stochastic-length contracting models in the style of Calvo (1983).[25]

Fourth, our desire to estimate the key parameter of one particular macro-labor model led us to focus on wage histories and disregard employment histories. However, the implication that employment history is irrelevant is not shared by all macro models of the labor market. For example, in the literature on search and matching in business cycle models, the wage stickiness that matters for macro is the degree of (real) wage rigidity for new hires. We plan to explore further these issues in future research.

Finally, from an epistemological point of view, we hope that this work will increase the awareness that greater communication between economists working in different fields (in this case, macro and labor economics) can produce valuable insights at relatively low cost.

[25] See, for instance, Knell (2010).

A Technical Appendix

This appendix describes the methods we use to (1) obtain critical values to test for changes in wages when the assumptions for the standard F test are not met, (2) estimate the power of these tests, and (3) obtain unbiased estimates of the probability of a wage change by correcting for the impact of Type I and Type II errors.

A.1 Critical values

The standard F test cannot be used to test for wage changes since the necessary assumptions for the F tests are violated in two conceptually different ways. First, measurement error in earnings is not classical.[26] The critical value must, therefore, be adjusted to take account of this violation of the assumptions. Second, the test for structural breaks used in this paper is a test of the maximum of a set of F statistics rather than the test of a single F statistic. Bai and Perron (1998) show that the appropriate test for structural breaks must take into account that the test is based on the maximum of l test statistics, where l is the length of the period being analyzed. The standard critical values are no longer applicable since the critical value for the maximum of l test statistics is higher the critical value for a single F statistic.

We address these problems by using Monte Carlo simulations that simulate data with the same non-classical measurement structure found in the SIPP by Gottschalk and Huynh (forthcoming). Their estimate of the structure of measurement error in the SIPP was obtained from SIPP earnings records matched to uncapped W-2 earnings records in the Detailed Earnings Records (DER) file. Measurement error is defined as the difference between DER earnings and reported earnings in the SIPP. These matched records are used to estimate the autocorrelation of measurement error (0.54) and the signal-to-noise ratio (2.64).[27]

In order to obtain critical values to test the null hypothesis of no change in wages, we generate 2,000 wage profiles of length l with no change in wages.[28] We then apply the method described in the paper to test for structural breaks in each of these constant wage series with measurement error. The critical value for a test with a significance level of α is obtained by calculating the F value for each wage series, ranking these wage series on the basis of their F values, and finding the critical F value where we falsely reject the null of no changes in wages α percent of the time. This is repeated for simulated earnings series of length $l = 3, 4...L$

A.2 Power

We obtain the power of the test using a similar simulation procedure. In order to obtain the power of the test we similarly generate 2,000 wage series of length l. But in this case each wage series has

[26] See the large literature reviewed in Bound and Mathiowetz (2001)

[27] Gottschalk and Huynh (forthcoming) also report a negative correlation between measurement error and DER wages of -0.339. Whether this mean reversion should be included in the analysis of individual wage profiles depends crucially on whether the negative correlation is between group (the expected value of measurement error is lower for respondents with above average earnings) or within group (the expected value of measurement error declines when an individual's wages rise). We assume the correlation is between group so mean reversion affects the mean but not the variance of reported wages and has no impact on our estimate of the probability of wage change.

[28] Including multiple changes in wages over the l periods would not affect the estimates since the algorithm in the first iteration is based on the maximum F statistic over the full l periods, no matter how many wage changes are found in further iterations.

a permanent wage change of Δw after $\tilde{l} < l$ periods, where $\tilde{l}$ is randomly assigned. This is the true wage series. The observed wage series has measurement error around this non-constant wage series. The variance of the measurement error is set to be consistent with the signal-to-noise ratio found by Gottschalk and Huynh (forthcoming). These wage series are used to calculate the proportion of times our tests falsely fail to reject the null of no wage change, using the previously discussed critical values. This yields the power of the test for a wage history of length l using a significance level of α.

A.3 Adjustments to obtain consistent estimates

Estimating π, the probability that an individual will experience a wage change between waves, requires an adjustment to $\hat{\pi}$ for Type I and II errors. We start by defining an indicator variable, $I_{ip}(W_i, \alpha, \gamma|X)$, that takes the value 1 when the Bai and Perron (2003) algorithm finds a statistically significant change in person i's wages in period p, given the person's wage history, W_i, the significance level α used to test for breaks, and the power of this test, γ, given the number of observations over which the maximum F is calculated.[29]

The object of interest is the probability of a wage change across all persons and periods. Our sample analogue estimator is

$$\hat{\pi} = \frac{\sum\limits_{i=1}^{N} \sum\limits_{p=1}^{p_i^T} I_{ip}}{\sum\limits_{i} p_i^T}, \tag{2}$$

where p_i^T is the total number of periods in i's wage history. The numerator is the total number of statistically significant wage changes across the N histories, while the denominator is the total number of periods across all histories.

The Bai-Perron algorithm provides a consistent estimate of I_{ip}, the indicator of whether a wage change has occurred in period p of person $i's$ wage history. However, this does not ensure that $\hat{\pi}$ is a consistent estimator of π, since the tests for wage changes are subject to both Type I and Type II errors.

Consider conducting $P = \sum\limits_{i} p_i^T$ tests for structural breaks. In expectation, $P(1 - \pi)$ of these tests will be in periods where $\Delta w = 0$. However, as a result of Type I error, $\alpha P(1 - \pi)$ of the tested segments with no wage change will be falsely classified as having a statistically significant wage change. This error will lead us to over-estimate π. On the other hand, Type II error (failing to reject the null of no wage change when it is false), leads to an underestimate of π. The expected value of the number of wage changes that are falsely classified as having constant wages due to sampling error is $(1 - \gamma)P\pi$, where γ is the power of the test.[30]

The net impact of Type I and Type II errors is[31]

[29]The X vector includes observables that affect the critical value, such as the length of the period over which the test is being carried out. X may also include variables of substantive interest, such as occupation or cyclical variables. In practice, we condition on these probabilities by including a set of dummies in descriptive logit estimates. These include a set of dummies for the length of the period used to calculate the maximum F.

[30]Power is defined as one minus the probability of a Type II error. Power depends on the significance level of the test for a wage change and the number of periods used in the estimation.

[31]This adjustment for Type I and Type II errors would seem to be applicable to a wider set of estimators in which $\hat{\theta}$ is function of a set of estimators $\hat{\gamma}_j(x)$ from a lower level of aggregation, j, each of which is subject to Type I and Type II errors. Estimators using imputed values are one such example.

$$
\begin{aligned}
p\lim(\hat{\pi}) &= \frac{\alpha P(1-\pi)+\gamma P\pi}{P} && (3)\\
&= \alpha(1-\pi)+\gamma\pi && (4)\\
&= \alpha+(\gamma-\alpha)\,\pi, && (5)
\end{aligned}
$$

which implies

$$
p\lim\left[\frac{\hat{\pi}-\alpha}{(\gamma-\alpha)}\right]=\pi. \qquad (6)
$$

Let $\tilde{\pi}=\frac{\hat{\pi}-\alpha}{(\gamma-\alpha)}$, so $\tilde{\pi}$ is a consistent estimator of the probability that a tested wage change is non-zero. It can be computed by using our initial estimate of the probability of a wage change, $\hat{\pi}$, the significance level used in these tests, α, and the power of these tests, γ, that we obtain from the microsimulations.

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Table 1: **Descriptive Statistics**

Total People (beginning)	39,095
Females	19,321
Hourly Workers (beginning)	17,148
Females	8,931
Mean Age	38
Mean Wage (Hourly Workers)	$10.03
Mean Earnings (Salaried Workers)	$2942

Table 2: **Industry Composition of the Sample**

Sample	**Total**	**Total**	**Hourly**	**Hourly**
Agriculture	778	1.99 percent	386	2.25 percent
Mining	174	0.45 percent	73	0.43 percent
Construction	1,993	5.10 percent	1,128	6.58 percent
Manufacturing	6,785	17.36 percent	3,684	21.48 percent
Transport and Communication	2,736	7.00 percent	1,093	6.37 percent
Trade	8,168	20.89 percent	4,459	26.00 percent
Services	15,881	40.62 percent	5,721	33.36 percent
Government and Public Administration	2,377	6.08 percent	597	3.48 percent
Army and Unemployed	203	0.52 percent	7	0.04 percent
Total	**39,095**	**100 percent**	**17,148**	**100 percent**

Table 3: **Occupational Composition of the Sample**

Sample	Total	Total	Hourly	Hourly
Professional	5,660	14.48 percent	1,033	6.02 percent
Managerial	4,932	12.62 percent	638	3.72 percent
Technical Sales and Support	11,761	30.08 percent	5,109	29.79 percent
Craftsmen and production	4,048	10.35 percent	2,337	13.63 percent
Operatives	6,185	15.82 percent	4,239	24.72 percent
Service	5,504	14.08 percent	3,360	19.59 percent
Farming	807	2.06 percent	426	2.48 percent
Miscellaneous and Unemployed	198	0.51 percent	6	0.03 percent
Total	39,095	100 percent	17,148	100 percent

Table 4: **Quarterly Frequency of Wage Adjustment, Hourly Workers**

Period	Reported	Adjusted	CEE 05	SW 07	HLM 08	Gottschalk 05
96-99	0.481	0.178				
65-95			0.36			
66-04				0.26		
98-05					0.35	
86-93						0.11

Table 5: **Seasonality of the Frequency of Wage Adjustment, Hourly Workers, Results Relative to the First Quarter**

Type of Wages	Reported	Adjusted
Q_2	-0.002	0.003
	(0.003)	(0.003)
Q_3	0.020***	0.021***
	(0.003)	(0.003)
Q_4	0.012***	0.013***
	(0.003)	(0.003)
F-test	0.000	0.000
N	136043	136043

Table 6: **Heterogeneity in the Quarterly Frequency of Wage Adjustment, by Industry, Hourly Workers (excluded category: Manufacturing)**

Wages	**Reported Levels**	**Adjusted Levels**	**Reported Relative**	**Adjusted Relative**
Agriculture	0.423***	0.178***	-0.081***	0.010
	(0.008)	(0.009)	(0.008)	(0.009)
Mining	0.482***	0.148***	-0.022	-0.020
	(0.014)	(0.015)	(0.014)	(0.015)
Construction	0.456***	0.174***	-0.048***	0.007
	(0.004)	(0.004)	(0.004)	(0.005)
Manufacturing	0.504***	0.168***		
	(0.002)	(0.002)		
Transport and Communication	0.501***	0.186***	-0.003	0.018***
	(0.004)	(0.004)	(0.004)	(0.005)
Trade	0.475***	0.186***	-0.029***	0.018***
	(0.002)	(0.002)	(0.003)	(0.003)
Services	0.473***	0.180***	-0.031***	0.012***
	(0.002)	(0.002)	(0.003)	(0.003)
Govt and Pub. Admin.	0.511***	0.168***	0.007	-0.000
	(0.005)	(0.006)	(0.006)	(0.006)
F-test	0.000	0.000	0.000	0.000
N	136043	136043	136043	136043

SE in parenthesis. ***, **, * : Statistically significant at 1 percent, 5 percent and 10 percent

Table 7: **Heterogeneity in the Quarterly Frequency of Wage Adjustment, by Occupation, Hourly Workers (excluded category: Production Workers)**

Wages	**Reported Levels**	**Adjusted Levels**	**Reported Relative**	**Adjusted Relative**
Professional	0.476***	0.171***	-0.010**	-0.001
	(0.004)	(0.004)	(0.005)	(0.005)
Managerial	0.455***	0.186***	-0.031***	0.014**
	(0.005)	(0.006)	(0.006)	(0.006)
Technical Sales and Support	0.478***	0.184***	-0.008**	0.012***
	(0.002)	(0.002)	(0.003)	(0.004)
Craftsmen and production	0.486***	0.172***		
	(0.003)	(0.003)		
Operatives	0.494***	0.174***	0.008**	0.003
	(0.002)	(0.002)	(0.003)	(0.004)
Service	0.478***	0.180***	-0.008**	0.008**
	(0.002)	(0.003)	(0.003)	(0.004)
Farming	0.442***	0.182***	-0.044***	0.011
	(0.007)	(0.008)	(0.007)	(0.008)
F-test	0.000	0.000	0.000	0.000
N	136043	136043	136043	136043

SE in parenthesis. ***, **, * : Statistically significant at 1 percent, 5 percent and 10 percent

Table 8: **Quarterly Frequency of Earnings Adjustment, Salaried Workers**

Type	**Reported**	**Adjusted**
1996-1999	0.653	0.049

Table 9: **Seasonality of the Frequency of Earnings Adjustment, Salaried Workers, Results Relative to the First Quarter**

Type of Earnings	**Reported**	**Adjusted**
Q_2	-0.007***	0.001
	(0.002)	(0.001)
Q_3	0.012***	0.007***
	(0.002)	(0.001)
Q_4	0.008***	0.006***
	(0.002)	(0.001)
F-test	0.000	0.000
N	187694	185980

Table 10: **Heterogeneity in the Quarterly Frequency of Earnings Adjustment, by Industry, Salaried Workers (excluded category: Manufacturing)**

Earnings	**Reported Levels**	**Adjusted Levels**	**Reported Relative**	**Adjusted Relative**
Agriculture	0.622***	0.050***	-0.036***	0.003
	(0.005)	(0.003)	(0.005)	(0.003)
Mining	0.621***	0.047***	-0.037***	-0.000
	(0.008)	(0.004)	(0.008)	(0.004)
Construction	0.669***	0.053***	0.011***	0.006***
	(0.003)	(0.002)	(0.003)	(0.002)
Manufacturing	0.658***	0.047***		
	(0.002)	(0.001)		
Transport and Communication	0.669***	0.048***	0.011***	0.000
	(0.002)	(0.001)	(0.003)	(0.001)
Trade	0.665***	0.050***	0.007***	0.003***
	(0.001)	(0.001)	(0.002)	(0.001)
Services	0.645***	0.051***	-0.012***	0.004***
	(0.001)	(0.000)	(0.002)	(0.001)
Govt and Pub. Admin.	0.658***	0.047***	-0.000	-0.000
	(0.002)	(0.001)	(0.003)	(0.001)
F-test	0.000	0.000	0.000	0.000
N	187694	185980	187694	185980

SE in parenthesis. ***, **, * : Statistically significant at 1 percent, 5 percent and 10 percent

Table 11: **Heterogeneity in the Quarterly Frequency of Earnings Adjustment, by Occupation, Salaried Workers (excluded category: Production Workers)**

Salaried	**Reported Levels**	**Adjusted Levels**	**Reported Relative**	**Adjusted Relative**
Professional	0.637***	0.050***	-0.032***	0.002
	(0.001)	(0.001)	(0.002)	(0.001)
Managerial	0.639***	0.048***	-0.030***	-0.001
	(0.001)	(0.001)	(0.002)	(0.001)
Technical Sales and Support	0.657***	0.050***	-0.012***	0.002
	(0.001)	(0.001)	(0.002)	(0.001)
Craftsmen and production	0.669***	0.048***		
	(0.002)	(0.001)		
Operatives	0.684***	0.048***	0.015***	-0.000
	(0.002)	(0.001)	(0.003)	(0.001)
Service	0.673***	0.052***	0.004	0.004***
	(0.002)	(0.001)	(0.003)	(0.001)
Farming	0.633***	0.050***	-0.036***	0.002
	(0.005)	(0.003)	(0.005)	(0.003)
F-test	0.000	0.000	0.000	0.003
N	187694	185980	187694	185980

SE in parenthesis. ***, **, * : Statistically significant at 1 percent, 5 percent and 10 percent

Table 12: **SW (2008) DSGE Model: Estimated Structural Parameter Values Fixing Wage Stickiness (Selected Parameters, Posterior Mode)**

SW Parameter	**Meaning**	**SW Original**	**BBG Hourly**	**BBG Total**
ξ_w	Wage Stickiness	**0.262**	**0.178**	**0.105**
ξ_p	Price Stickiness	0.66	0.66	0.69
ι_w	Wage Indexation	0.59	0.57	0.55
ι_p	Price Indexation	0.23	0.21	0.20
σ_c	EIS	1.40	1.37	1.40
σ_l	Elast. of Labor Supply	1.92	2.28	2.61
α	Capital Share	0.19	0.30	0.30

Figure 1: **Adjusted Wage Series, An Hourly Worker**

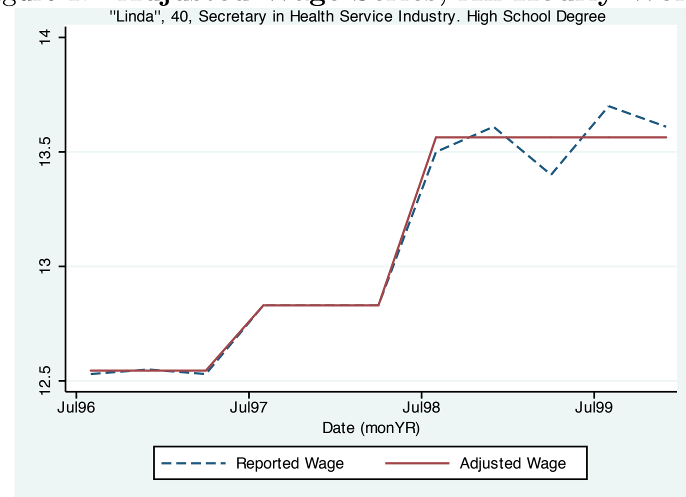

Figure 2: **Implied Measurement Error, Hourly Workers, 1996 Panel**

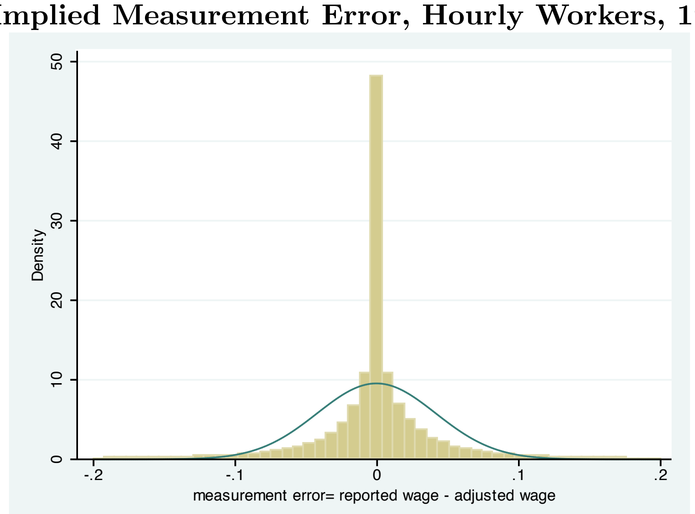

Figure 3: **Adjusted Wage Series, An Hourly Worker**

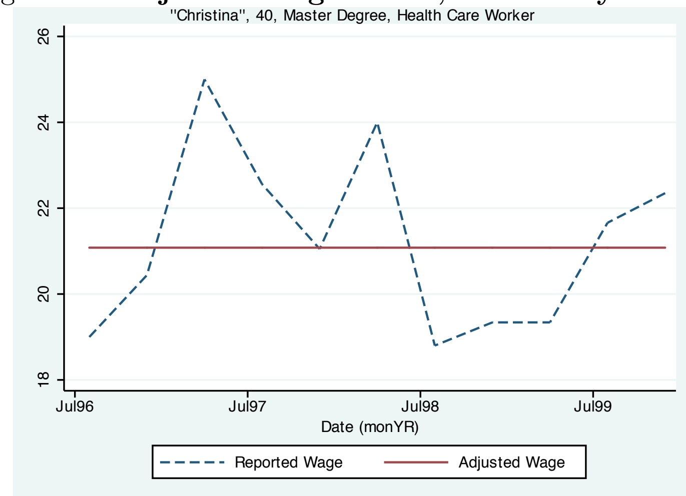

Figure 4: **Seasonality in the Frequency of Wage Adjustment, Hourly Workers**

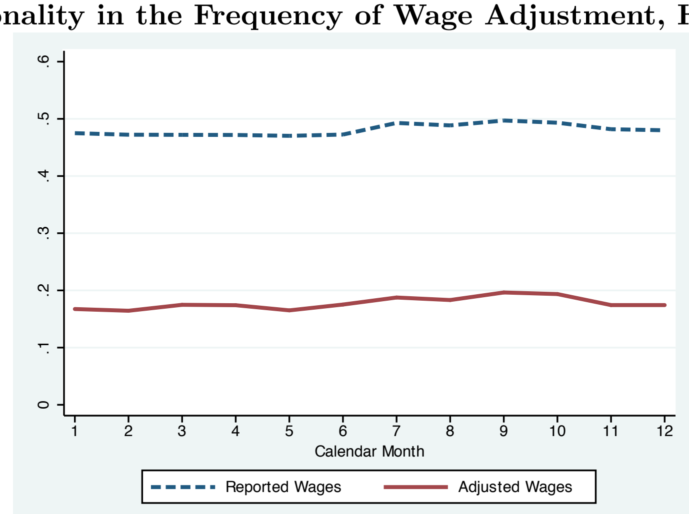

Figure 5: **Distribution of Non-Zero Wage Changes, Hourly Workers, 1996 Panel**

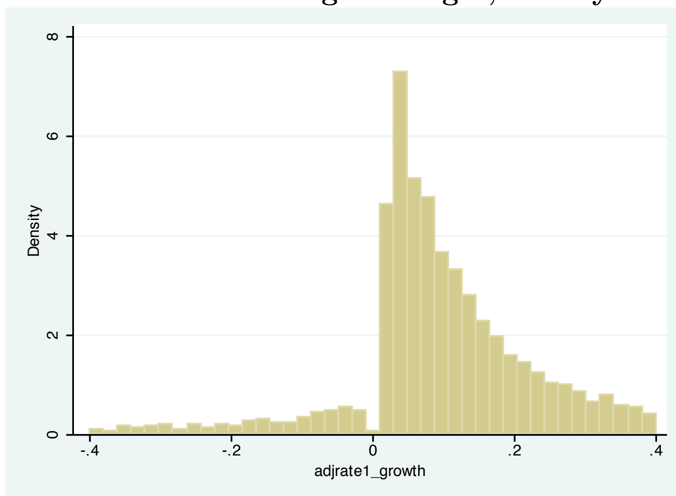

Figure 6: **Frequency of Wage Change and Monthly Inflation Rate, Hourly Workers, 1996-1999**

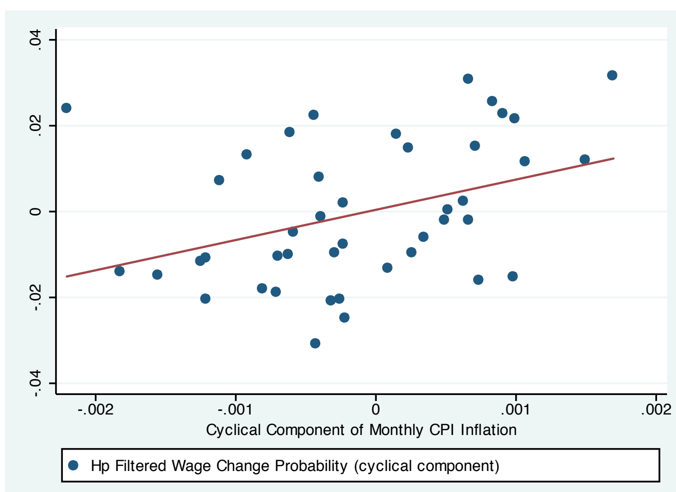

Figure 7: **Frequency of Wage Change and Unemployment Rate, Monthly, Hourly Workers, 1996-2003**

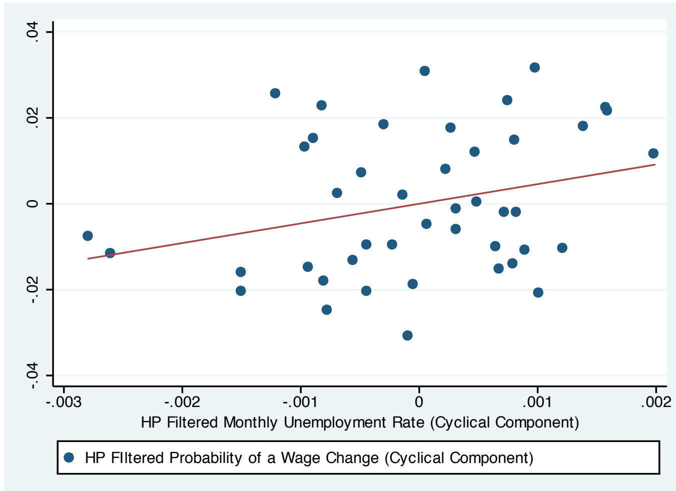

Figure 8: **Adjusted Wage Series, a Salaried Worker**

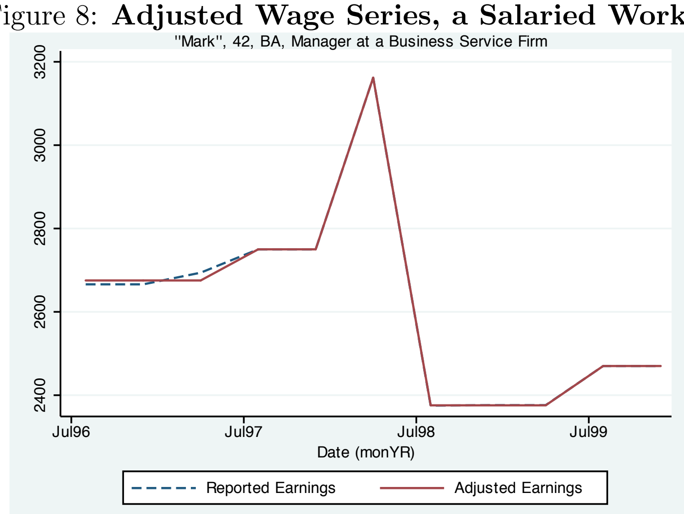

Figure 9: **Adjusted Wage Series, a Salaried Worker**

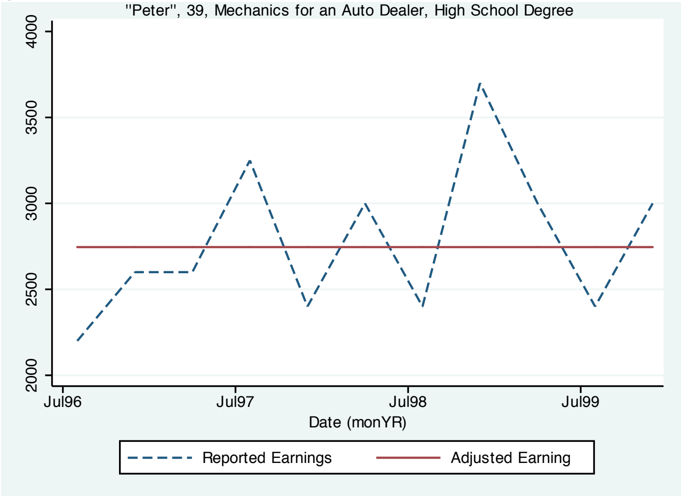

Figure 10: **Seasonality in the Frequency of Wage Adjustment, Salaried Workers**

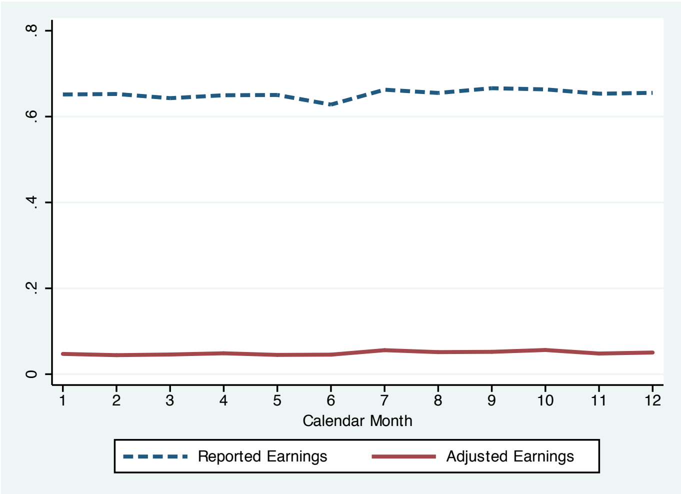

Figure 11: **Dynamic Response to a Monetary Shock, SW (2008) Model, Different Levels of Wage Stickiness**

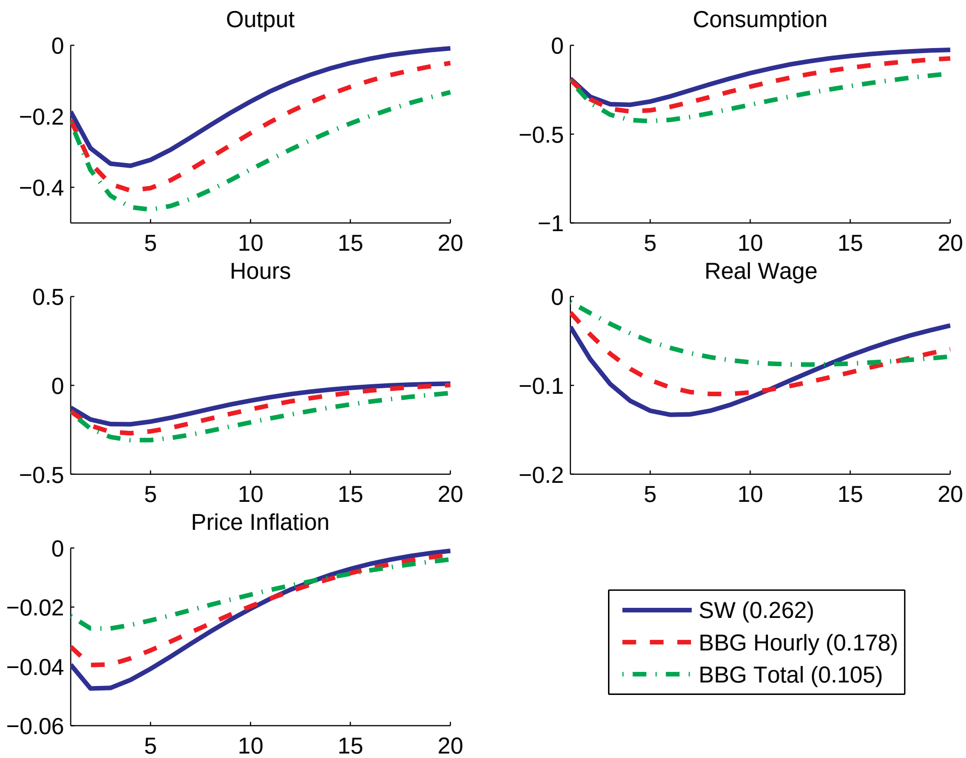

Figure 12: **Hazard of a Wage Change, Reported Wages, 1996 Panel**

Figure 13: **Hazard of a Wage Change, Adjusted Wages, 1996 Panel**