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Some Evidence on the Importance of Sticky Wages[†]

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We present evidence on the frequency of nominal wage adjustment using SIPP data adjusted for measurement error. The SIPP is a representative sample of the US population. Our main results are: (i) The average quarterly probability of a nominal wage change is between 21.1 and 26.6 percent, depending on the assumptions used. (ii) Wage changes are much more likely when workers change jobs. (iii) The frequency of wage adjustment does not display significant seasonal patterns. (iv) The hazard of a nominal wage change first increases and then decreases, with a peak at 12 months. (*JEL* E24, E32, E52, J31)

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 macroeconometric 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). Even though Christiano, Eichenbaum, and Evans (2005, henceforth CEE) find that nominal wage rigidity is more important than nominal price rigidity for explaining the dynamic effects of monetary policy shocks, to date there has been little research using micro data to estimate the average probability of nominal wage changes. Most of the previous literature has focused on different aspects of nominal

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wage dynamics, such as the extent of downward nominal wage rigidity.[1] Notable exceptions include the recent papers by Le Bihan, Mortornes, and Heckel (2012); Lunnermann and Wintr (2009); and Sigurdsson and Sigurdardottir (2011), all of which use European data.[2]

Our paper contributes to this recent literature by providing evidence from a large sample of the US population. The scarcity of previous work on the business cycle implications of nominal wage rigidity using US micro data may be due in part to a lack of suitable datasets. We provide evidence on the frequency of wage adjustment using data from the Survey of Income and Program Participation (SIPP). The SIPP is a survey run by the US Census Bureau. 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 US 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). This sample period covers a long boom, during which the US employment-to-population ratio reached its all-time high. Most macroeconomic models suggest that such a large rise in employment rates should be accompanied by substantial increases in wages, so from this perspective our period is a good time to estimate the frequency of wage adjustment. Unfortunately our data do not contain a recession, but our methods can be used on future panels of the SIPP to compare wage rigidity in booms and recessions.

Our research is intended to inform macroeconomic models, but as usual we find rich patterns in micro data that are not easy to fit into standard macro models. In particular, we confirm previous studies[3] that have shown that workers who switch jobs have a much higher probability of changing wages than workers who continue to be employed at the same firm.[4] This fact is not easily incorporated into the vast majority of macro models, which either do not have the concept of a "job" at all (since all workers are assumed to be continuously employed), or do not allow for job-to-job transitions without an intervening spell of unemployment. Thus, we present results separately for within- and between-job wage changes, as well as an average, allowing macroeconomists to decide which estimates best fit the concept they are trying to calibrate.[5]

[1] See, for example, McLaughlin (1994); Lebow, Saks, and Wilson (1999); Kahn (1997); Card and Hyslop (1997); Altonji and Devereux (1999); Nickell and Quintini (2003); Gottschalk (2005); Fehr and Goette (2005); and Dickens et al. (2007).

[2] Druant et al. (2012) report qualitative survey evidence on the speed of price and wage adjustment for several European countries. However, their method does not allow one to calculate the quarterly frequency of nominal wage adjustment.

[3] See, for instance, Topel and Ward (1992).

[4] We thank Steve Davis and an anonymous referee for leading us to explore this issue.

[5] This issue is reminiscent of the controversy over whether price changes due to temporary sales should be included in calculating the frequency of price changes relevant for macroeconomics. We believe that such debates are most convincingly addressed by producing theory where the data concept is modeled explicitly. For example, in

Our results for within-job wage changes are easier to compare with the recent European literature previously discussed, since these papers use firm-level data and, hence, consider only wage changes that occur under the same employer. However, we also report the frequency of nominal wage adjustment including within-job and between-job wage changes. This is arguably the concept that comes closest to the assumptions of 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 frequency of wage changes regardless of employment history the relevant statistic to examine.

Our sample consists of hourly workers who reported their hourly wages to the SIPP interviewer. We restrict our sample to hourly paid workers because, for reasons discussed below, we fear that including salaried workers and calculating their wages as hourly earnings would increase the incidence of measurement error in our data. Furthermore, it is also reasonable to treat the wage of hourly workers as their remuneration for an extra hour of work, which is the relevant concept in economic models. We chose to focus on the statistic measured with least error, the hourly wage, at the potential cost of making the sample less generalizable.

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 US survey data are typically self-reported.[6] We deal with this problem by applying to the reported wage 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, within the same job, the true wage 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 this is evidence 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 changes at all dates where we can reject the no-break hypothesis, and are constant otherwise. This results in the exclusion of many instances of transitory wage changes that look very much like measurement error. We apply a conceptually

the price change literature, the analysis of sales was greatly advanced by the model of Kehoe and Midrigan (2010). We hope that our results inspire similar theoretical work to advance the interpretation of our findings.

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

similar procedure to between-job wage changes. The only difference is that in this case we know the date at which we need to test for a change in wages.

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, because Type I and Type II errors will typically lead us to over- or underestimate the true population frequency of wage changes. We make a methodological contribution by extending the Bai-Perron-Gottschalk method to create a consistent estimator of the frequency of wage changes, taking both Type I and Type II errors into account. We show that our method generates a consistent estimator of the frequency of nominal wage changes under the assumption that we have accurate information on the signal-to-noise ratio in our observed wage data. Furthermore, we use Monte Carlo simulations to show that our estimator has good small sample properties. In these simulations, we run our procedure on simulated wage histories with the sample lengths found in our data, calibrating the signal-to-noise ratio in wages by using replication studies from the SIPP and the CPS. Reassuringly, we find that our procedure converges on estimates that are close to the population frequency of true wage changes. If anything, they slightly overestimate the frequency of wage adjustment.

We find the following main results. First, after correcting for measurement error, within-job wages at the microeconomic level appear to be sticky. We find that the probability of a within-job wage change is between 16.3 and 21.6 percent per quarter. However, the probability of a wage change conditional on a job change is much larger, between 69.1 and 77 percent. Multiplying the two sets of probabilities by the frequency of each type of observation in the SIPP data, the unconditional probability of a wage change is between 21.1 and 26.6 percent per quarter. Assuming constant hazards, as in the model of Calvo (1983), the unconditional probabilities imply that, on average, wages are unchanged in nominal terms for a duration between 3.8 and 4.7 quarters. By comparison, several key papers estimating DSGE models using macro data find this probability to be about 30 percent per quarter. Our estimated unconditional probability of a wage change is a bit lower, but in the same ballpark. On the other hand, our within-job wage change frequency is significantly lower than typical macro estimates. Using the well-known model of Smets and Wouters (2007; henceforth SW), we show that the level of wage stickiness that we estimate 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 without assuming that preset wages are automatically indexed to past inflation.

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. Similarly, we find little heterogeneity across occupations. Fourth, we find that wage changes are significantly right-skewed, in keeping with the preceding cited papers 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 more in keeping with the staggered contracting model of Taylor (1980) than the constant-hazard model of Calvo (1983).

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.* Dickens et al. (2007) survey this literature in their summary of the results coming from the International Wage Flexibility Project (IWFP). The IWFP analyzed wages using data from a large number of countries. 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 such results across countries and perhaps even across time periods. As noted above, several recent papers use European data to examine wage rigidity in France, Luxembourg, and Iceland; we discuss the relationship of our work to these other papers after reporting our results. Given the substantial heterogeneity in wage rigidity across countries found by the IWFP, we believe it is important to estimate the frequency of wage adjustment for the United States specifically. This is particularly true because the benchmark macro studies of CEE (2005) and SW (2007) are based on US macro data. Thus, a micro-to-macro comparison of US wage rigidity is in order, to match the important micro-to-macro comparisons of price rigidity made possible by the work of Bils and Klenow (2004) and Nakamura and Steinsson (2008).

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 conclusion, Del Negro and Schorfheide advocate more empirical analysis of microdata, along the lines of the literature on the frequency of price adjustment at the product level.[7] We view our paper as a first step toward 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.[8] 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 preset wage paid to current employees is also applied to new hires.[9] Haefke, Sonntag, and van Rens (2008); Martins, Solon, and Thomas (2012); and Hall and Krueger (2012) examine micro evidence more related to the key predictions of the search literature.

Finally, our results shed some light on a small but interesting literature on the seasonal effects of monetary policy shocks. Recently, Olivei and Tenreyro (2007) 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

[7] Although they warn that aggregation is a key issue when inferring macro behavior from micro evidence.
[8] For example, Shimer (2005) and Hall (2005).
[9] See Haefke, Sonntag, and van Rens (2008) and Gertler and Trigari (2009).

positing a model where wage changes are more likely to occur in the second half of the year. We do indeed find that the frequency of wage changes is slightly higher in the second half of the year. Whether these differences can also explain the differential seasonal effects of monetary policy shocks is an open question.

The structure of the paper is as follows. The next section discusses the SIPP sample and the data definitions that we use. Section II summarizes the methodology we use to correct the wage series for unobserved measurement error. Section III contains the main results of the analysis. Section IV explores the implications of the results obtained for the characteristics of a standard macroeconomic model. Section V contains the discussion of the hazard estimates. Section VI concludes, and suggests some directions for future research.

I. Data

The data source for this paper is the *Survey of Income and Program Participation* (SIPP). The SIPP data have been collected by the US Census Bureau since 1983, with a major revision in 1996. The SIPP sample is a multi-stage, stratified, representative sample of the US 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 on a rotating basis.[10]

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 data from linked Current Population Surveys, and the Panel Study of Income Dynamics. 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.[11]

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 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.

[10] Every month 25 percent of the sample is interviewed. See the online Appendix for the distribution of the number of interviews per individual and a discussion on attrition.

[11] 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.

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)	2,942

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 get paid by the hour).[12] 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 dollars. There is, however, a lot of heterogeneity. The fifth percentile of the distribution of wages is \$5 and the ninety-fifth is \$20. The average monthly earnings for salaried workers is about \$3,000. The fifth percentile of the distribution of earnings is \$440 and the ninety-fifth is \$6,800.

Tables 2 and 3 report the breakdowns for industry and occupational categories at the one digit level. As Table 2 shows, services is the most highly represented industry (33.3 percent of total hourly workers), followed by trade (26 percent) and manufacturing (21.5 percent). Agriculture and mining, on the other hand, have very few observations. As for occupational categories, technical sales and support is the most highly represented in our sample (29.8 percent) followed by machine operators (24.7 percent) and services (19.6 percent). On the other hand, professionals and managers account for only 9.7 percent of the total in the hourly workers sample, while they represent 27.1 percent of the entire survey.

II. Method

A key challenge is trying to limit the impact of measurement error in assessing the frequency of wage adjustment. Our first way of achieving this objective is to limit 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.

Our 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. 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 time $T_1 \ldots T_s$. The

[12] We consider hourly workers only those individuals that were paid by the hour over the entire period.

TABLE 2—INDUSTRY COMPOSITION OF THE SAMPLE

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

TABLE 3—OCCUPATIONAL COMPOSITION OF THE SAMPLE

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

observed wage at time t, w_t, is equal to a constant α_t plus the unobserved measurement error ϵ_t:

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

The objective is to estimate the s break dates and the $s + 1$ constant wages. The method proposed by Gottschalk (2005) proceeds sequentially. First, using each individual's whole wage history within the same job of T observations, pick the break date that minimizes the sum of squared residuals (SSR) between reported wages and constant wages in the two sub-periods.[13] Then test to see if one can reject the null hypothesis of no break over the entire T periods against the alternative that there is

[13] We need to exclude instances where $T < 3$ because it would be impossible to run a break test with two observations.

a break at the point that minimizes the SSR.[14] If one cannot reject the null, then the procedure is finished since all other points have higher SSR, and one would conclude that there are no structural breaks in the wage history for the individual (i.e., the wage is constant for all observations of that individual in that job spell).[15] If one can reject, then test for structural breaks in each of the sub-periods identified by the break test. Again, pick the date that minimizes the SSR in each sub-period, and then test if a significant break is detected at that point. Continue until no significant structural break is detected in any of the remaining subintervals of data.[16]

After having detected the significant breaks within a job, we can turn to consider the issue of the wage changes between jobs. Here we use a conceptually analogous procedure, with the difference that now we know the break date (the date of the job change).

One might object that this procedure is biased toward finding wages that are sticky, since the identifying assumption is that wages are set in nominal terms and change discretely! However, the Bai-Perron method does 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.[17] One might then ask whether the procedure would be able to estimate a large number of statistically significant breaks in a short time series. This is the important issue of the power of the test, which we discuss at length below.

An example illustrates how this procedure works.[18] Figure 1 shows the reported and the adjusted wage series for "Linda," a 40-year-old secretary with a high school degree. The reported series[19] shown by the dashed line, is characterized by five wage increases and four wage decreases over the period considered (no job change occurred for the case of "Linda"). 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.[20]

Our third step is to use the nominal wage series we obtain by applying the Bai-Perron-Gottschalk correction to obtain a consistent estimate of the quarterly probability of a wage change. Following the procedure sketched above gives us a

[14] Given the short time periods of the wage histories, the critical values for the structural break tests are obtained through Monte Carlo simulations. See Section IIA for details.

[15] This is formally equivalent to a Bai-Perron test for a structural break in a model with only a constant. Such a point process is a special case of the Bai Perron procedure that is applicable to more general processes as well.

[16] Bai and Perron formally show that this process, which tests sequentially for break points in each segment, is sufficient to identify multiple break points in the full series. No assumption is made about the existence of breaks prior to the start of the period or after the end of the period.

[17] Given that some people are observed for fewer than 48 months, we calculate a maximum quarterly frequency of true wage changes potentially obtainable. For people interviewed 12 times, for example, such probability is 83 percent (=10/12). Computing a weighted average of these probabilities over the distribution of the number of interviews per person in our sample, we get a maximum detectable quarterly probability of a true wage change of 56 percent, which is much higher than we actually estimate.

[18] We made up the name of the individual in the example.

[19] (\$12.57, \$12.53, \$12.55, \$12.53, \$12.83, \$12.83, \$12.83, \$13.50, \$13.61, \$13.40, \$13.70, \$13.61).

[20] This example illustrates how measurement error can arise in our data due to misreporting. Indeed there are several possible stories of how an error in the measurement could arise in the data. Also the misreporting, for example, could be due to a lack of recollection, as well as to an actual mistyping of the figure by the interviewer. Another possible source of measurement error is rounding. We do not take a strong stance on any of these possibilities, nor do we think we have to do so. The reason is that we can get from replication studies some hints on the structure of the measurement error process present in our data. This allows us to correct for measurement error independently from how this measurement error arose.

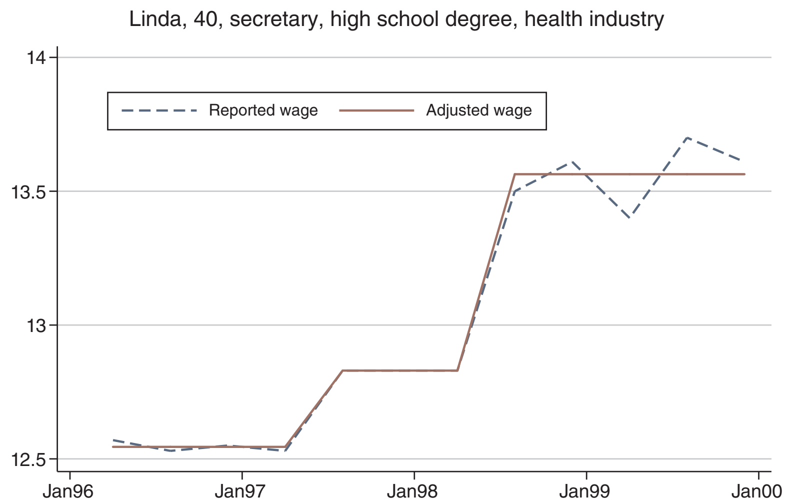

Figure 1. Adjusted Wage Series, an Hourly Worker

consistent estimator of the break dates for each individual wage history. 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, because there can be either a Type I or Type II error in each test for a structural break. We explain below how we scale the frequency of breaks in the adjusted wage series to get a consistent estimator of the probability of a wage change.

In the following subsections, we discuss the three key statistical issues central to our methodology, namely (i) how to get the critical values for our tests of significant changes in wages, (ii) how to get the power of our test, and (iii) how to construct a consistent estimator for the probability of a wage change by correcting the estimate obtained from the iterative procedure used by Gottschalk (2005).

A. *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 wages is not classical.[21] The critical value must, therefore, be adjusted to take account of this violation of the assumptions. Second, the Bai-Perron test for structural breaks used in this paper to detect within-job wage changes 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 the fact that the test is based on the maximum of l-test statistics, where l is the number of possible break points in 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 than the critical value for a single F-statistic.[22]

[21] See the large literature reviewed in Bound, Brown, and Mathiowetz (2001).
[22] Andrews (1993) was the first to investigate the distribution of the maximum of a set of F-statistics.

We address this problem by using Monte Carlo simulations that simulate data with the signal-to-noise ratio found in two replication studies. The first such study is Gottschalk and Huynh (GH 2010). 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. They defined measurement error as the difference between DER earnings and reported earnings in the SIPP. These matched records are used to estimate the autocorrelation of measurement error (0.482) and the ratio between the variance of the true earnings and the variance of the difference between the true earnings and the reported earning (the signal-to-noise ratio, found to be 2.64).[23] Given that GH's study focused on earnings rather than wages, we test for robustness using a different signal-to-noise ratio. Our second figure for the signal-to-noise ratio comes from Angrist and Kruger (1999)—henceforth, AK—who matched hourly wages from the CPS with employer records and calculated the signal-to-noise ratio at 1.80, which is almost 40 percent smaller than the value found by GH. The AK study measured hourly wages as earnings divided by hours, while we use a direct measure of reported hourly wages, which is likely to suffer less from measurement error than hourly earnings. Thus, the AK value may overstate the importance of measurement error. To the best of our knowledge, there has been no replication study using administrative data on hourly wages matched to survey data on reported wage rates. This is not too surprising, given the difficulty of access to administrative records on hourly wages. We are aware that this is an important point to bear in mind while considering our results.[24]

In order to obtain critical values to test the null hypothesis of no change in wages, we generate 10,000 wage profiles of length l with no change in wages. We add to these wage profiles with no wage changes a measurement error process featuring zero mean and whose autocorrelation is consistent with what was found by Gottschalk and Huynh (0.482). We then apply the method described above 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 maximum F-value for each wage series, ranking these F-values, and finding the critical F 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 $l = 3, 4 \ldots L$. A similar procedure is adopted to obtain the critical values of the test for the between-jobs wage changes. The only difference is that, since we know the date of the break we need to test, we do not search for a maximum F in our simulations. In this case, we simply rank the F-values obtained in our simulated series, and find the critical F-value where we falsely reject the null of no change in wages α percent of the time.

[23] Gottschalk and Huynh (2010) also report a negative correlation between measurement error and DER earnings of -0.339. Whether this mean reversion should be included in the analysis of individual wage profiles depends 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 wage rises). 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.

[24] AK's results and other validation studies are summarized in Bound, Brown, and Mathiowets (2001, Appendix A).

B. *Power*

It is clear that if our tests for structural breaks have low power, we will underestimate the frequency of wage changes. We address the issue of power and its implications for our estimated wage change frequency using simulations. We generate 10,000 wage series of length l. In this case each wage series has a wage change of Δw after $\tilde{l} < l$ periods, where $\tilde{l}$ is randomly assigned.[25] The Δw are allowed to vary, since smaller Δw will also lead to low power.[26] The observed wage series has measurement error around this nonconstant wage series. The variance of the measurement error is set to deliver a signal-to-noise ratio for the median Δw consistent with the figure of 1.80 found by AK or 2.64 found by GH.[27] The autocorrelation of the measurement error is set to 0.482 (from GH). We run our iterative procedure on these observed wage series and calculate the fraction of times our tests correctly reject the null of no wage change, using the critical values computed as in Section IIA. This yields the power of the test for a wage history of length l using a significance level of α and a wage change of Δw. The procedure is repeated for each length $l = 3, 4 \ldots L$.

Table 4 reports Monte Carlo results for the power of the break test for a wage change using a significance level of $\alpha = 0.05$, for the two values of the signal-to-noise ratio found by GH and AK. Not surprisingly, the power of our test is very high for large Δw and long sample lengths, while power is much lower for short lengths and small wage changes. Power is also consistently higher with a higher signal-to-noise ratio. A completely analogous procedure is run to retrieve the power of our test for between-job wage changes.

C. *A Consistent Estimator of the Average Probability of a Wage Change*

Applying Gottschalk's (2005) procedure described above to the reported data gives us consistent estimates of the break dates for each individual wage history. 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, for two reasons. The first is the power of the test. Clearly, we will fail to detect every break in the wage series. Low power will lead us to underestimate the frequency of wage changes, and conclude that wages are stickier than they truly are. But there is also the issue of size—even if all wages were constant forever, the fact that we pick the critical values to ensure a specified probability of Type I error means we would falsely conclude that α percent of wages change in any given period, where α is the size of the test. By itself, Type I error would lead us to conclude that wages are more flexible than they really are. In order to get an unbiased estimator of the probability of a wage

[25] 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.

[26] We set Δw to the median values of the five quintiles of the distribution of within-job wage changes in the SIPP data that we test for a structural break. For hourly workers, these are approximately (in absolute values) 2.9. percent, 5.8 percent, 9.5 percent, 15.5 percent, and 32.2 percent.

[27] Note that fixing the median signal-to-noise ratio lets power vary with the size of the break.

Table 4—Power of the Break Test by Significance Level, Periods and Size of the Break

Size of the break	≈0.029	≈0.058	≈0.095	≈0.155	≈0.322
		Median signal to noise: 2.64 (*GH*), α: 0.05			
Periods					
3	0.0531	0.0692	0.1025	0.1627	0.3283
4	0.0623	0.1054	0.217	0.4517	0.9133
5	0.0688	0.1379	0.3189	0.6737	0.994
6	0.0818	0.1842	0.4254	0.8306	0.9999
7	0.0881	0.217	0.5055	0.8997	≈1
8	0.1014	0.2644	0.598	0.9519	≈1
9	0.1211	0.3207	0.6866	0.9789	≈1
10	0.131	0.3593	0.7556	0.9914	≈1
11	0.1352	0.3960604	0.8082	0.9969	≈1
12	0.1625	0.4701	0.8752	0.999	≈1
		Median signal to noise: 1.80 (*AK*), α: 0.05			
Periods					
3	0.0507	0.0627	0.086	0.1358	0.2713
4	0.0602	0.0843	0.1618	0.3399	0.8145
5	0.064	0.1069	0.224	0.5281	0.9706
6	0.0712	0.143	0.2999	0.6809	0.9965
7	0.0734	0.1592	0.3618	0.7625	0.9997
8	0.0856	0.1927	0.4411	0.8494	0.9998
9	0.0988	0.2366	0.5268	0.9125	≈1
10	0.1051	0.2585	0.5846	0.9493	≈1
11	0.109	0.2914	0.6323	0.9708	≈1
12	0.1287	0.3396	0.7228	0.9885	≈1

change, we need a method to correct for Type I and Type II errors. The method we propose is a novel contribution of our paper, and may be of interest in its own right.

Denote the probability of a wage change π and the frequency of statistically significant breaks in wages $\hat{\pi}$. Consider conducting P tests for structural breaks in wages. In expectation, $P(1 - \pi)$ of these tests will be in periods where the true wage does not change. 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 by itself means that $\hat{\pi}$ will overestimate π. On the other hand, Type II error (failing to reject the null of no wage change when it is false) will lead $\hat{\pi}$ to underestimate 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.[28] The net impact of Type I and Type II errors is[29]

$$p\lim(\hat{\pi}) = \frac{\alpha P(1 - \pi) + \gamma P \pi}{P} \tag{2}$$

$$= \alpha(1 - \pi) + \gamma\pi \tag{3}$$

$$= \alpha + (\gamma - \alpha)\pi, \tag{4}$$

[28] 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, the number of periods used in the estimation, and the size of the wage change.

[29] 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.

which implies

$$(5) \qquad p \lim\left[\frac{\hat{\pi} - \alpha}{(\gamma - \alpha)}\right] = \pi.$$

So if we define

$$(6) \qquad \tilde{\pi} = \frac{\hat{\pi} - \alpha}{(\gamma - \alpha)},$$

then $\tilde{\pi}$ is a consistent estimator of the probability that a tested wage change is non-zero.

Equation (6) is crucial to our effort to derive a consistent estimator of the frequency of wage changes, both within-job and between jobs. We have argued that low power will lead $\hat{\pi}$ to underestimate the true probability of a wage change. However, since we know α, if only we knew the power of our test for structural breaks, we could adjust $\hat{\pi}$ by using α and γ to get a consistent estimator. But as shown in Section IIB, we can calculate the power γ using Monte Carlo simulations calibrated with the signal-to-noise ratio found from replication studies. Finally, equation (6) shows that the choice of significance level of our tests for structural breaks, α, is basically arbitrary. A high significance level (low α) will indeed lead to a lower $\hat{\pi}$, but then we will apply a larger correction to $\hat{\pi}$ to get $\tilde{\pi}$. Operationally, we compute $\hat{\pi}$ by using our iterative procedure and then sorting the results into cells. Each cell is defined by the number of periods available to test for a break and by the size of the break we are testing (the average reported wage before and after the potential break date).[30] For each cell, we obtain the power of our test from Monte Carlo simulations. We then compute $\tilde{\pi}$ for each cell, where the significance level, α, is set to 0.05, and the power of tests in each cell, γ, is obtained from the simulations in Section IIB.[31] We then calculate an aggregate value of $\tilde{\pi}$ by taking a weighted average of the estimates across cells, where the weights are the number of observations in each cell.[32]

In order to see whether $\tilde{\pi}$ is a precise estimator of π even in relatively small samples, we report simulation results of our procedure in Table 5. The rows in Table 5 give some of the different possible lengths of individual wage histories, from three periods (the shortest we can test) to 12 periods (the most data we can have for any individual). For all sample lengths, we chose two different possible values of the probability of a wage change (15 percent and 30 percent) and three possible sizes of changes in wages, corresponding to three possible signal-to-noise ratios. The middle column for each value of π has a signal-to-noise ratio of 2.25, roughly the average of the figures calculated by AK and GH. For each cell, we generate 500 wage series of different lengths. We assign breaks (of different sizes) to a fraction π of these series.

[30] We define the cells by sorting the break sizes into quintiles.

[31] In a small number of cells our finite sample correction gives us a probability smaller than zero or larger than one. There we apply the mean correction for the other cells in that quintile of wage changes.

[32] See the online Appendix for some more details about the entire statistical procedure.

Table 5—Effects of the Correction for Type I and Type II Error in Small Sample (500 *Replications*)

	True π Break Signal-to-noise	0.15 Low $\approx$0.25	0.15 Med $\approx$2.25	0.15 High $\approx$6.25	0.3 Low $\approx$0.25	0.3 Med $\approx$2.25	0.3 High $\approx$6.25
l	Variable						
3	Estimated probability $\hat{\pi}$	0.050	0.058	0.074	0.048	0.058	0.090
	Correction $\tilde{\pi}$	0.167	0.235	0.207	0.343	0.235	0.345
	Power γ	0.064	0.084	0.166	0.064	0.084	0.166
6	Estimated probability $\hat{\pi}$	0.058	0.092	0.170	0.068	0.136	0.272
	Correction $\tilde{\pi}$	0.129	0.143	0.159	0.290	0.293	0.294
	Power γ	0.112	0.344	0.804	0.112	0.344	0.804
9	Estimated probability $\hat{\pi}$	0.066	0.154	0.192	0.084	0.254	0.332
	Correction $\tilde{\pi}$	0.178	0.158	0.152	0.378	0.309	0.302
	Power γ	0.140	0.710	0.984	0.140	0.710	0.984
12	Estimated probability $\hat{\pi}$	0.076	0.172	0.192	0.098	0.282	0.328
	Correction $\tilde{\pi}$	0.197	0.150	0.150	0.364	0.284	0.293
	Power γ	0.182	0.866	0.998	0.182	0.866	0.998

Then we run our iterative procedure to get $\hat{\pi}$, and for each cell we compute our corrected $\tilde{\pi}$.

As the table shows, our estimator generally does a good job of correcting the $\hat{\pi}$ for Type I and Type II errors. In cells where we have both small sample lengths and small break sizes, the power of the break test is naturally low, and thus the Bai-Perron-Gottschalk method finds a low $\hat{\pi}$. However, our corrected $\tilde{\pi}$, if anything, overestimates the probability of a wage change in these cells.[33] When we compute weighted averages across the cells, as we do in the actual data, our results are very close to the population parameters. For the case of $\pi = 0.15$, our estimate of $\hat{\pi}$ is 0.112, while our estimate of $\tilde{\pi}$ is equal to 0.168. For the case of $\pi = 0.30$, our $\hat{\pi} = 0.17$, while our $\tilde{\pi}$ is equal to 0.31. Note that in both cases we slightly *overestimate* the true probability of a wage change.

III. 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

[33] In the two cells where we have a sample length of three periods and a low signal-to-noise ratio, $\hat{\pi} \leq \alpha$. We attribute a value of $\tilde{\pi}$ to those cells that is the average of the other cells in their columns, just as we would if such a situation arose in the actual data.

Table 6—Quarterly Frequency of Wage Adjustment, Hourly Workers

	Within-job	Between-jobs	Overall
Reported	0.531	0.908	0.565
Adjusted	0.084	0.485	0.120
	(0.002)	(0.001)	(0.002)
Adjusted + Correction (GH)	0.163	0.691	0.211
	(0.010)	(0.007)	(0.010)
Adjusted + Correction (AK)	0.216	0.770	0.266
	(0.010)	(0.009)	(0.010)
Adjusted + Correction (GH) −40 hours	0.152		
Aggregate estimates			
CEE (2005)			0.36
SW (2008)			0.26
Evidence for other countries			
France (HLM 2008)	0.35		
Luxembourg (LW 2009)	0.36/0.19		
Iceland (SS 2011)	0.28/0.13		

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. However, there is no reason to assume that the probabilities of wage adjustment are the same within and across jobs. For this reason, while we present the estimated probability of a wage change within the same job, we also report the probability of a wage change conditional on a job change, and then an overall probability of a wage change regardless of the employment history (obtained as a weighted average of the two).

While our data and analysis are conducted with interviews taking place every four months, we report the results at a quarterly frequency for ease of comparison with the previous literature.[34] Table 6 reports the frequency of wage adjustment for hourly workers. The quarterly frequency of wage adjustment for reported wages within-job is very high (53.1 percent). As we would expect from the simulations above, the adjusted series for wages shows a much lower estimate of the quarterly frequency of wage changes when we simply apply the iterative procedure of Gottschalk (2005), 8.4 percent. However, as we stressed, this estimate needs to be corrected for the incidence of Type I and Type II errors. The result of the correction boosts the estimated probability to 16.3 percent, if we use the power obtained in the simulations assuming that the signal-to-noise ratio is the one found by GH for total earnings in the SIPP. If

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

we use the lower signal-to-noise ratio found by AK (1999) for hourly earnings, our estimated probability of a wage change rises to 21.6 percent.[35]

Unsurprisingly, we find a much higher probability of a wage change conditional on a job change. The reported figure is 90.8 percent. After we test for a significant break, we obtain an adjusted probability of 48.5 percent. After the correction for Type I and Type II errors, the estimated probability is boosted to 69.1 percent (in the case of the GH signal-to-noise ratio) or 77.0 percent (using the AK signal-to-noise ratio).

Putting together the previous results, we can report an overall probability of observing a wage change as a weighted average of the within-job and between-jobs probabilities. The point estimates for the probability of a wage change after correcting for Type I and Type II errors are 21.1 percent (obtained using a GH correction) and 26.6 percent (using the AK correction).[36] Below each estimated probability, we report standard errors obtained through a bootstrapping procedure. As the table shows, these standard errors are quite small, thus indicating that our estimates are precise.[37]

We also explore whether forms of nonwage temporary compensation relevant to hourly workers, such as overtime pay, play a significant role in determining the extent of wage flexibility. In order to do so, we restrict our sample to those individuals who consistently report that they worked for exactly 40 hours per week, meaning that they worked no overtime (we only consider within-job wage changes for this case). The results obtained using the GH signal-to-noise ratio is reported in Table 6. Consistent with intuition, restricting the sample to people who worked 40 hours per week reduces our estimate of the quarterly frequency of wage change, but only to 15.6 percent. Under the assumption that the correction for measurement error is appropriate, we therefore find evidence of somewhat greater wage stickiness than previously found in estimated DSGE models using US aggregate data for the US economy. (Our use of the "overall" wage rigidity figures is obviously tentative, pending a resolution of the question as to which estimate is the "right one" for a specific macroeconomic model.) As reference values, we also report in the table the estimated quarterly frequencies of wage changes obtained by estimating DSGE models using US aggregate data. CEE, for example, estimated the quarterly Calvo probability of wage adjustment to be 36 percent in their benchmark model. In SW, the benchmark estimate of the same parameter is 26.2 percent. This estimate is very close to our higher figure for the overall wage change probability, and not too far from the other, lower, estimate based on the GH signal-noise ratio. Thus, it appears that at least some estimated DSGE models pass the test proposed by CEE that macro estimates based on aggregate data should match micro evidence on the same parameters.[38]

[35] The important difference between the "adjusted" and the "adjusted + corrected" probabilities reveal the importance of our correction for Type I and Type II errors. The power of our test (from Table 4) averages 56 percent.

[36] The weights depend on the probability of observing a job change. In the SIPP population, this probability is 9 percent.

[37] See the online Appendix for further details on the bootstrapping procedure.

[38] CEE (2005, 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 …"

In Table 6 we also report results obtained by papers investigating the extent of wage stickiness in other countries. In a recent contribution, Le Bihan, Mortornes, and Heckel (2012) found the average quarterly frequency of wage adjustment to be 35 percent using data from a large sample of French firms.[39] However, there are major institutional differences between the US and European labor markets. The French labor market, for instance, is characterized by a high incidence of collective bargaining and by wages that are changed every year due to statutory changes in the minimum wage.[40] Unfortunately, Le Bihan, Mortornes, and Heckel's (2012) data do not allow them to distinguish these sources of wage changes from the others. Other recent European studies tried to make this distinction. Lünnermann and Wintr (2009) report evidence from monthly data from Luxembourg. After cleaning the dataset to try to account for measurement error, they find a monthly frequency of wage change of 14 percent (equivalent to a quarterly frequency of roughly 36 percent). After they control for wage changes due to institutional factors, such as indexation or a change in the minimum wage, their estimate drops to 7 percent—roughly equal to 19.5 percent at quarterly level. Sigurdsson and Sigurdardottir (2011), compute similar statistics using detailed firm-level data for Iceland. They find an unadjusted quarterly frequency of wage changes of around 28 percent. However, the frequency of wage changes drops to 13.5 percent when they remove wage changes due to union settlements.[41]Note that all of these papers use firm-level data. Thus, they provide information only on the frequency of within-job wage changes, and cannot examine the between-job wage change margin that we find is important using our worker-level data.

For macro purposes, what matters is the frequency with which wages incorporate new information on the aggregate state of the economy. Government-mandated changes in wages should probably be excluded by this criterion; statutory changes in the minimum wage are likely to be based on lagged information, possibly reaching back years. It is less clear how collective bargaining fits into this definition. If collectively bargained wages change due to an indexation clause or a "pre-programmed" escalator clause, as is often the case, then the change incorporates either no or relatively little new information.[42] On the other hand, if the new wage represents a new collective bargaining agreement, then it clearly qualifies as a "true wage change" for macro purposes.

A long line of literature in macroeconomics and labor economics, going back at least to Bruno and Sachs (1985), has viewed European labor-market institutions as sufficiently different from US institutions that similar shocks would have very different macroeconomic effects in the two areas. Given our method for obtaining a consistent frequency of wage changes from noisy US survey data, we believe we can supply an important piece of information that has been lacking in the US macroeconomic literature.

[39] Firm-level data are usually less prone to measurement error than household surveys.

[40] Over 50 percent of wage changes, according to Le Bihan, Mortornes, and Heckel (2012), are due to collective bargaining, and synchronized changes in the minimum wage cause a large spike in the seasonality of wages in France that is absent in the United States.

[41] These quarterly probabilities are the ones implied by their monthly estimates using the formula $\pi_q = 1 - (1 - \pi_m)^3$, where π_q is the quarterly estimate and π_m is the monthly estimate.

[42] In fact, it would be interesting to know how frequently wages change automatically due to indexation clauses, since this would provide evidence on a different parameter of DSGE macro models, the prevalence of "dynamic updating" as defined by CEE.

Finally, throughout the paper, we implicitly assume that nominal wages are allocative, as do the European papers we have cited and the macro literature that we are trying to inform. A proper test of this assumption is clearly outside the scope of this paper, although the early analysis of Card (1990) suggests that preset nominal wages are indeed allocative. However, we did a preliminary investigation of whether hours respond to wage changes in our SIPP sample. In the online Appendix, we provide evidence that the relation between changes in hours and changes in wages is indeed positive, provided that the change in the wage is sufficiently large.[43] Understanding the allocative nature of wages is obviously an important task for all papers that assume wage rigidity as a propagation mechanism for shocks.

A. *Seasonality*

A second question we explore regards the seasonality of the pattern of wage adjustment. Olivei and Tenreyro (2007) 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.[44]

Figure 2 illustrates the frequency of wage adjustment within-job by month. The frequency of wage adjustment does not display any sizable seasonal pattern in either the reported series or the adjusted (and corrected) series. 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 first quarter.

Table 7 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 in our dataset is smaller than the differences in their calibrated model. Whether these differences can also explain the differential seasonal effects of monetary policy shocks is an open question.[45]

Interestingly, Le Bihan, Mortornes, and Heckel (2012), 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

[43] Naturally this finding is subject to many caveats. For example, we do not have a way to correct for measurement error in hours, which will attenuate the correlation between hours and wages.

[44] 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]."

[45] A caveat here is that while Olivei and Tenreyro focus on the timing of wage setting, here we detect wage changes when they are implemented. If there are lags between the time at which the wage change decisions are taken and when they are actually implemented, then it is harder to compare our results with those of Olivei and Tenreyro.

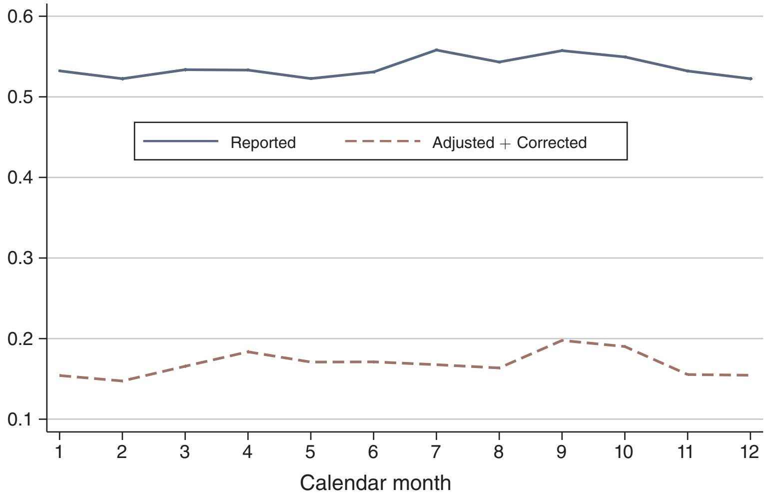

Figure 2. Seasonality in the Frequency of Wage Adjustment, within Job

Table 7—Seasonality of the Frequency of Wage Adjustment, Hourly Workers, Results Relative to the First Quarter

Type of wages	Reported	Adj + Corrected
Q2	−0.000	0.019***
	(0.005)	(0.004)
Q3	0.024***	0.021***
	(0.005)	(0.004)
Q4	0.005	0.011***
	(0.005)	(0.004)
F-test	0.000	0.000

Note: Standard errors in parentheses.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

minimum wage is updated each year in July. However, there is no such feature in the US 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.[46] Analogously, institutional factors might explain the findings for Luxembourg and Iceland (Lünnemann and Wintr 2009; and Sigurdsson and Sigurdardottir 2011) where most of the wage changes take place in January.

Note that if wage change probabilities are not approximately constant over the calendar year, one cannot directly compare the estimated wage change probabilities between micro data and macro models, since the latter assume uniform staggering of wage changes throughout the year. A model where the average wage

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

TABLE 8—HETEROGENEITY IN THE QUARTERLY FREQUENCY OF WAGE ADJUSTMENT, BY INDUSTRY, HOURLY WORKERS (*Excluded Category: Manufacturing*)

Wages	Reported levels	Adj + Corrected levels	Reported relative	Adj + Corrected relative
Agriculture	0.508***	0.206***	−0.046***	0.055***
	(0.009)	(0.007)	(0.010)	(0.008)
Mining	0.554***	0.182***	0.001	0.031***
	(0.013)	(0.010)	(0.014)	(0.011)
Construction	0.517***	0.167***	−0.037***	0.016***
	(0.006)	(0.005)	(0.007)	(0.006)
Manufacturing	0.554***	0.151***		
	(0.004)	(0.004)		
Transportation and communication	0.555***	0.174***	0.002	0.023***
	(0.005)	(0.004)	(0.007)	(0.005)
Trade	0.518***	0.168***	−0.036***	0.017***
	(0.004)	(0.003)	(0.006)	(0.005)
Services	0.528***	0.175***	−0.026***	0.024***
	(0.004)	(0.003)	(0.006)	(0.005)
Public sector	0.569***	0.160***	0.015**	0.009
	(0.006)	(0.004)	(0.007)	(0.006)
F-test	0.000	0.000	0.000	0.000

Note: Standard errors in parentheses.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

change frequency is the same as in the model with uniform staggering but all wages change in, say, January, would show much less persistence of the effects of monetary shocks, essentially for the reasons given by Olivei and Tenreyro (2007). This is another reason to believe that one needs estimates of the wage change frequency that are specific to the United States.

B. *Heterogeneity*

Our access to micro data allows us to explore whether wage stickiness differs across sectors or occupations. We restrict ourselves to within-job wage changes to avoid including individuals that switch not only job but also industry or occupation.

Table 8 reports the results from regressing the probability of a within-job wage change for hourly workers on a full set of industry dummies. The first two columns show the reported and the adjusted probability of a wage change.[47] The third and the fourth column 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. Manufacturing appears to be the industry where the wages are stickier. Agriculture, mining, and services are the industries where the wages appear more flexible.

[47] We apply here the correction for Type II error using the GH signal-to-noise ratio.

Table 9—Heterogeneity in the Quarterly Frequency of Wage Adjustment, by Occupation, Hourly Workers (*Excluded Category: Production Workers*)

Wages	Reported levels	Adj + Corrected levels	Reported relative	Adj + Corrected relative
Professional	0.555***	0.170***	0.011	−0.004
	(0.006)	(0.005)	(0.008)	(0.006)
Managerial	0.525***	0.194***	−0.018***	0.020***
	(0.006)	(0.004)	(0.007)	(0.006)
Sales and support	0.537***	0.173***	−0.006	−0.002
	(0.004)	(0.003)	(0.006)	(0.005)
Production	0.544***	0.174***		
	(0.004)	(0.003)		
Operatives	0.534***	0.157***	−0.010*	−0.017***
	(0.004)	(0.003)	(0.006)	(0.005)
Service	0.542***	0.147***	−0.002	−0.027***
	(0.005)	(0.004)	(0.007)	(0.005)
Farming	0.501***	0.178***	−0.043***	0.004
	(0.008)	(0.006)	(0.009)	(0.007)
F-test	0.000	0.000	0.000	0.000

Note: Standard errors in parentheses.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Table 9 repeats the exercise of Table 8, but now for different occupations. The coefficients in the third and fourth columns are relative to the production workers. Again, we do not find much heterogeneity. Services and operatives, though, appear to be occupations characterized by higher wage stickiness among hourly workers, while managerial occupations display slightly lower wage stickiness.

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 two-digits level of disaggregation (for both industries and occupations). While the point estimates show significantly more dispersion using the more disaggregated classifications, the differences are not statistically significant due to the small number of observations in each category. Other studies using micro-level evidence on wage stickiness also find no substantial heterogeneity across industries or occupations.[48]

C. *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 3 reports the histogram of the nonzero adjusted within-job wage changes. The changes are in percentage points and the graph reports the distribution of wage changes between interviews. In order to avoid including outliers

[48] See Le Bihan, Mortornes, and Heckel (2012).

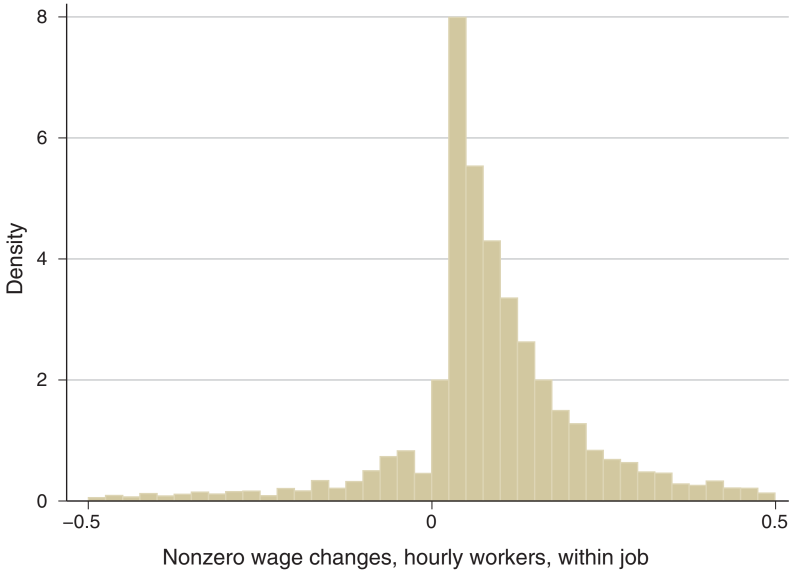

Figure 3. Distribution of Nonzero Wage Changes, within Job

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 graph shows, wage reductions are much less frequent than wage increases. More precisely, they correspond to 12.3 percent of the nonzero 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 some asymmetry that prevents wages from adjusting in response to negative shocks to the (value) marginal product of labor while allowing them to adjust to positive ones. Absent observations on the distribution of shocks to marginal products at the firm level, it is not clear whether nominal wages are not observed to decline in the data because there is a rigidity in this sense, or simply because negative shocks are rare.

IV. 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 of SW.[49] We take the model exactly as presented in their article, and perform two simple exercises.

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

Table 10—SW (2007) DSGE Model: Estimated Structural Parameter Values Fixing Wage Stickiness (*Selected Parameters, Posterior Mode*)

SW	Meaning	SW	BBG within	BBG total
$1-\xi_w$	Wage stickiness	0.262	0.189	0.239
ξ_p	Price stickiness	0.66	0.69	0.64
ι_w	Wage indexation	0.59		
ι_p	Price indexation	0.23	0.23	0.24
σ_c	EIS	1.40	1.4	1.39
σ_l	Elast. of labor supply	1.92	2.02	1.76
α	Capital share	0.19	0.30	0.30

First, we estimate all the parameters of the model through Bayesian techniques after fixing the parameter for wage stickiness at the midpoint of the two estimates in Table 6 for the quarterly frequency of a wage change for hourly workers. We experiment both with a value of 0.189 (the midpoint obtained for the frequency of within-job wage changes) and with a value of 0.239 (the midpoint of the estimates for the overall frequency of a wage change). Second, we compute the impulse response functions produced by the model following a monetary policy shock using both the parameter estimates of the original SW paper and the estimates we obtain from our first exercise using data for hourly workers.

Table 10 reports the estimates of some key parameters of the model conditional on the preset parameters for wage stickiness. The first column reports the posterior mode for each parameter found by SW, while the second and third columns show the posterior mode we find after fixing the wage stickiness parameter at the two levels discussed above. Importantly, we also set the wage indexation parameter to zero when we use our results to fix the hazard of a wage change, since our estimates show the probability of a change in the wage for any reason.[50]

As the table shows, the parameters related to price stickiness and price indexation do not differ dramatically between the two cases, so the model is not switching from wage to price frictions to match the data. The elasticity of intertemporal substitution and the labor supply elasticity also appear to change very little. 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 0.30, which is also more consistent with long-run evidence from national income shares.

Figure 4 reports the impulse responses to a monetary policy shock (arise in interest rates). The dashed line is the impulse response from the SW model using the parameters reported in their paper. The solid line is the impulse response of the same model with the parameters obtained conditional on our micro estimate of the within-job wage change hazard and assuming no indexation. The dashed-dotted line reports the results from the model using our wage stickiness parameter for the overall frequency of a wage change, again assuming no indexation. As expected, we find that with our estimates the model produces a larger and more persistent response of

[50] We are indebted to an anonymous referee for urging us to consider the issue of indexation more closely. As we discuss below, our strategy implies the maximum flexibility for nominal wages consistent with our results.

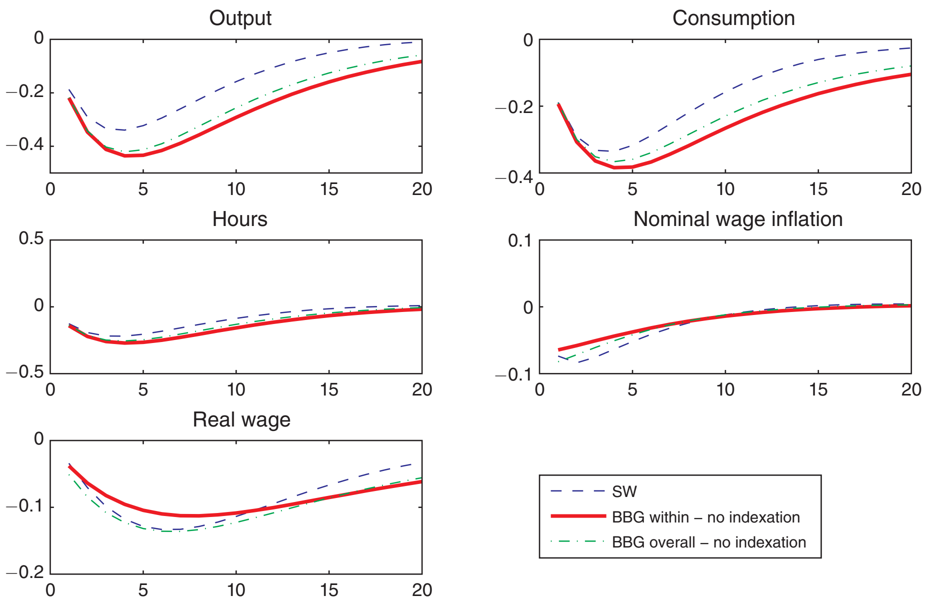

Figure 4. Dynamic Response to a Monetary Shock, SW (2007) Model, Different Levels of Wage Stickiness

output and consumption to a monetary shock. This is not 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 inflation appear to be damped and more persistent in our estimation.

We then re-estimated all the parameters of the model fixing wage stickiness at our estimated value, but allowing wage indexation. Figure 5 reports the impulses responses to a monetary policy shock under the assumptions of no indexation and indexation, using 0.189 as the parameter for wage stickiness. Interestingly, the presence or absence of wage indexation does not have a significant impact on this particular set of impulse responses.

Wage indexation is an interesting issue, since in its usual form it is clearly at odds with the micro data. The indexation assumed in the standard macro models of CEE and SW implies that every wage (and price) in the economy is changed in every period, although only a subset are changed in a fully optimal way. This of course is not what we find in micro data, even before the correction for measurement error. Medium-scale macro models are thus often accused of "hardwiring" the inflation inertia observed in the data, in a way that is at odds with micro observations. However, CEE (2005, figure 4) find that when they drop the assumption of "dynamic updating" of prices, the estimated model does almost equally well at matching the behavior of inflation.[51] Tellingly, however, CEE (2005, 32) note that

[51] CEE continue to assume that preset prices are updated using the steady-state inflation rate, but this does not affect the dynamics of the log-linearized model: compare equations (33) and (34) in CEE.

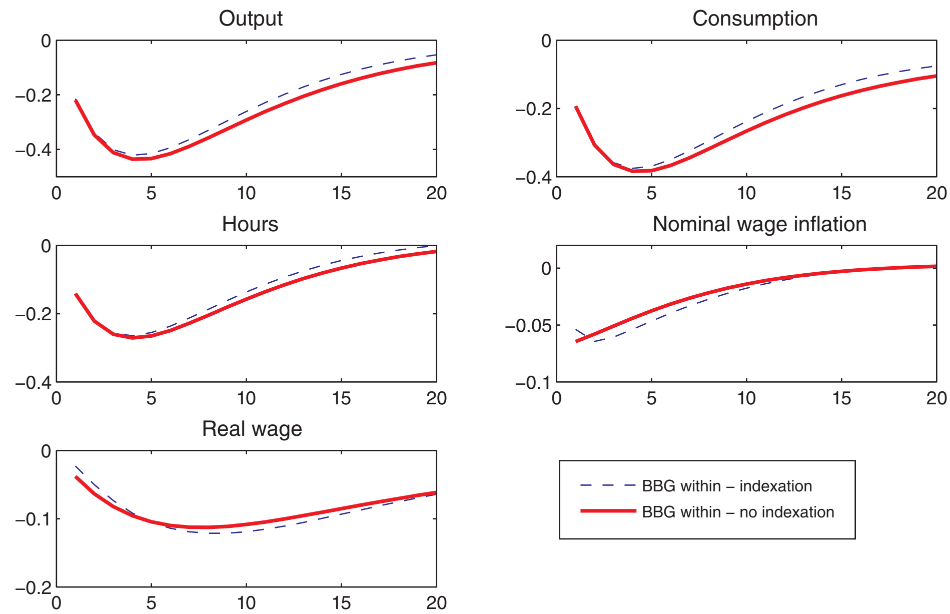

Figure 5. Dynamic Response to a Monetary Shock, SW (2007) Model, Impact of Wage Indexation

when they reestimate the model dropping the assumption of dynamic updating of prices, "the average duration of price contracts rises from 2.5 quarters to almost a year." This is quite similar to our results reported above for the SW model, especially for the "within" case where wage stickiness is much higher than SW assumed. It appears that there may be a "folk theorem" that greater stickiness in the *level* of prices and wages can substitute for the assumption of structural inertia in inflation. If so, this would be good news for macroeconomic modelers, for two reasons. First, as noted above, it would make the models more consistent with micro data. Second, dropping structural inflation inertia would allow medium-scale models to match data for historical periods when inflation did not display persistence, for example the Gold Standard period when inflation was close to white noise.

Finally, note that there is an alternative calibration strategy for macroeconomics that would be consistent with the micro data and our estimates, but imply much greater nominal wage rigidity than we have assumed in our exercises with the SW model. One could take the standard New Keynesian model of wage stickiness with indexation, but instead of assuming that all wages that are not re-optimized are automatically updated using lagged inflation, one can assume that there is a draw from a second distribution that determines whether a preset price can be updated or must be left unchanged. If both distributions have constant hazards, then we are estimating (approximately) the sum of the two Poisson parameters. Thus, our results are consistent with a very low probability of wage reoptimization. Of course, the same issue arises in the literature on estimating price, the reoptimization frequency from micro data, but it does not seem to have been discussed in that literature.

Two important stylized facts of macroeconomics are that monetary shocks cause persistent changes in real output and small but relatively persistent changes in prices. An important microeconomic observation is that wages and prices are fixed in nominal terms for discrete periods of time. We find that the relatively high level of microeconomic wage stickiness that we estimate makes it easier for macroeconomic models to match all three stylized facts simultaneously.

V. Hazard Functions

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

To explore this issue we first use the reported and the adjusted within-job wage series to estimate a discrete-time hazard model, where an exit is defined as a change of the reported (or adjusted) wage.[52] A new spell starts each time a new wage is observed, and we include in the sample all nonleft-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 changes.

Figure 6 shows the estimates of the hazard obtained using the reported wage series for hourly 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 7 reports the estimates of the hazard obtained using the adjusted wage series. Here, by contrast, there is a clear peak at 12 months in both series. Le Bihan, Mortornes, and Heckel (2012) report a similar finding for France.[53]

We conclude that Taylor-type fixed-length contracts have stronger empirical support than Calvo-type constant-hazard models. This finding is significant, since Dixon and Kara (2006) show that Calvo models produce greater persistence and more relative price variability (which reduces welfare) than do Taylor models that are calibrated to have the same average duration of preset prices.[54] However, the fact that the wage change frequency is almost flat over the calendar year (Figure 2 and

[52] See Box-Steffensmeier and Jones (2004, 73).

[53] Note that nothing in our procedure for detecting wage changes within jobs mechanically reduces the possibility of observing two consecutive breaks. In the online Appendix, we report the distribution of the wage spell lengths, where we show a substantial fraction of wage spells lasting one or two periods.

[54] For a more recent treatment of the difference between Taylor-types and Calvo-types contracts, see also Dixon and Le Bihan (2012).

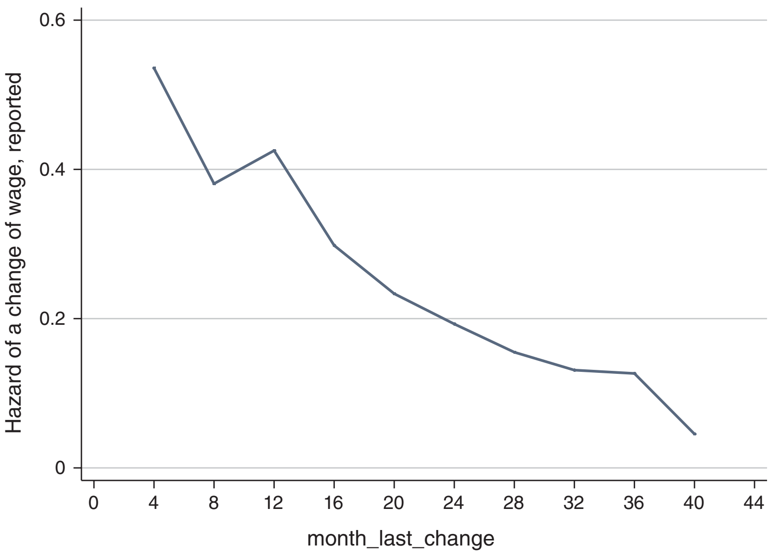

FIGURE 6. HAZARD OF A WAGE CHANGE, REPORTED WAGES, WITHIN JOB

Table 7) 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.[55]

VI. 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 develop macro models with job-to-job transitions that can be calibrated with our estimates for wage flexibility within and across jobs, in order to map our estimates of micro wage rigidity into a single "right number" for standard macroeconomic models. We have proposed two plausible summary statistics—within-job and the average of within- and between-job wage rigidity—but formal theory is needed to tell us which one is appropriate for the class of models we are trying to inform. We hope that in this respect the literature on nominal wage rigidity evolves in the same way as the literature on price rigidity. By presenting a range of estimates, we follow the example of Bils and Klenow (2004) who reported estimates for price rigidity with and without sales. It was left to Kehoe and Midrigan (2010) to model sales explicitly in a setting with nominal rigidities, and conclude that for

[55] Our findings are consistent with the empirical studies of Taylor (1983) and Cecchetti (1987), who found staggered wage setting in union contracts. However, in the US 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 US 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).

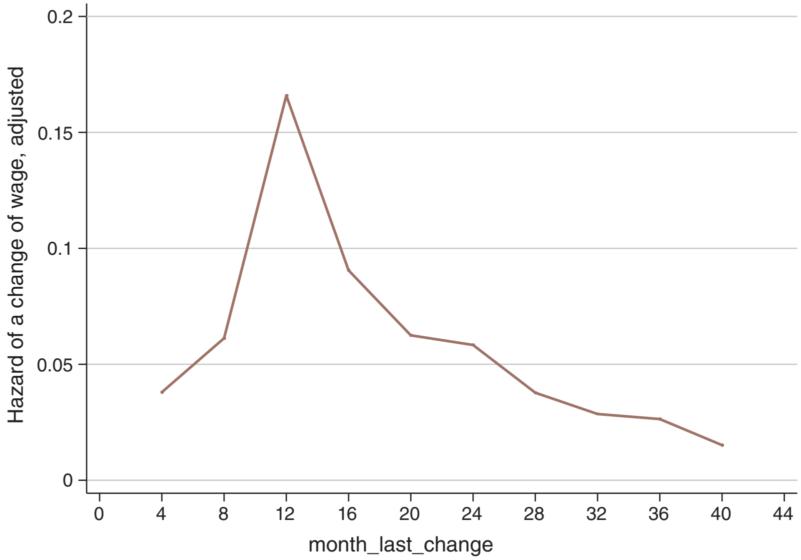

Figure 7. Hazard of a Wage Change, Adjusted Wages, within Job

macro purposes the duration of the "base price"—that is, excluding sales—is close to the right concept. We believe an analog of the work of Kehoe and Midrigan is needed for wages, in a setting that allows for job-to-job transitions.

Second, both of our measures suggest that the wage stickiness in micro data is greater than the macro estimates using aggregate data and Bayesian techniques (although some macro estimates are fairly close to our overall average). 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. 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 wages for hourly workers are more sticky than salaries; however, in earlier versions of this paper we found that salaries appear substantially more sticky than hourly wage rates. Perhaps most fundamentally, estimates of wage stickiness from macro models are conditional on the entire assumed structure of the model, some of which may be misspecified.[56] By contrast, we are able to measure the microeconomic frequency of wage change directly.

Third, the lack of sizable seasonality in wage changes leaves an open question: Do we need other mechanisms to 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.

Fourth, our findings on the shape of the hazard function for wage changes suggest that we should explore the properties of models based on fixed-length wage

[56] We thank an anonymous referee for this observation.

contracts, as in Taylor (1980), in addition to the very tractable stochastic-length contracting models in the style of Calvo (1983).[57]

We plan to explore these issues further in future research. We hope that our work inspires others to do so as well.

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[57] See for instance Knell (2010) and Dixon and Le Bihan (2012).

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