Some Evidence on the Importance of Sticky Wages *

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Abstract

Nominal wage stickiness is an important component of recent medium-scale structural 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 last two complete panels of the Survey of Income and Program Participation (SIPP). The SIPP provides high-frequency information on wages, employment and demographic characteristics for a large and representative sample of the US 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 6 and 20 percent, depending on the samples and assumptions used. 2) There is no significant heterogeneity in the frequency of wage adjustment across industries, but there is heterogeneity across occupations when salaried workers are included in the analysis. 3) The frequency of wage adjustment does not display any significant seasonal pattern. 4) The hazard of a nominal wage change first increases and then decreases, with a peak at 12 months.

These results are robust to examining wage changes for either hourly or salaried workers, controlling for attrition by using a balanced panel, and varying the degree of correction for measurement error.

JEL classification: E24, E32, J30

Keywords: Wage stickiness, micro-level evidence, measurement error

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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), which provides detailed information about 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 and are asked to specify their wages for the current and three preceding months, thus creating monthly wage series at the individual level. 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. The other well-known source of micro wage data, the CPS, does not provide continuous time-series data on individual wages, and thus cannot be used for our purpose.) We use the two most recent panels of the SIPP for which complete data are available: the 1996 panel (run from December 1995 to January 2000) and the 2001 panel (run from October 2000 to December 2003). Thus, our data span periods of both rapid and slow economic growth, and include one NBER recession.

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 regardless of employment history the relevant series to examine.

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 the results for the total sample, including salaried workers. By reporting the results for both hourly workers and for all 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 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 a,b). The application uses the intuition that wages are relatively persistent, and actual wage changes are unlikely to be purely transitory. This leads to the identifying assumption that true wages are constant for an unspecified period of time and then change 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 wage series has wage changes at all dates where we can reject the no-break hypothesis, and

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

is constant at the previous value of wages otherwise. This is a systematic way of excluding many instances of transitory wage changes that look very much like measurement error. 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 11 percent per quarter. When salaried workers are included in the sample, the probability falls to 6 percent. We experiment with varying the significance level of the measurement error correction. Even lowering the significance level to 50 percent, the probability of a wage change rises only to 20 percent for hourly workers. By comparison, several key paper 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, we do not find significant heterogeneity in the frequency of wage adjustment across industries, but do find that there is heterogeneity across occupations when salaried workers are included in the analysis. Third, the frequency of wage adjustment does not display any significant monthly or seasonal pattern. Fourth, we find that wage changes are significantly right-skewed, in keeping with preceding papers (e.g., Gottschalk, 2005) that have found evidence of downward nominal wage rigidity in microdata. Finally, 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 the constant-hazard model of Calvo (1983). However, our third 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.

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 wages 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 at 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 the rigidity in the wage of new hires. Our paper tries to estimate the wage stickiness that matters in a monopolistically competitive 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 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 (2008) have found that monetary policy

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

[3] The US 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 US 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).

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 first half of the year. We find that the frequency of wage changes does not display a significant seasonal pattern, suggesting that a different model is needed to explain Olivei's and Tenreyro's 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, while section 5 presents robustness analysis. Section 6 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 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 and asking questions about the previous three. 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 monthly frequency of the SIPP data and near-quarterly frequency of the interviews make 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 SIPP follows each individual for 24 to 48 months, thus creating the proper panel data essential for our analysis.[6] Finally, the SIPP reports more reliable

[6]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.

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.

We focus on the two most recent panels of this survey for which complete data are available: the 1996 panel (run from December 1995 to January 2000) and the 2001 panel (run from October 2000 to December 2003).[7] For each person in the panel, we have time series information about their wage rate, as well as their industry and occupation. The 1996 Panel follows 39,095 people, 49.4% of whom are women. The 2001 Panel surveys 38,692 people (49.2% female). We restrict our sample to workers between 15 and 64 years of age.

Our first step aimed at minimizing measurement error is to focus on the smaller sample of those people who directly reported their base wage to the SIPP interviewer. Focusing only on hourly workers, however, has the drawback of reducing the size and representativeness of the sample. Thus, as a robustness check we also report the results for the total sample, comprising both hourly workers and salaried workers. Of course, the latter do not report their wage rates. For salaried workers, we impute the wage rate as the ratio of reported monthly earnings over reported monthly hours.[8] Defining wages in this manner clearly introduces much more noise into the wage history.

Our smaller sample of hourly workers includes 17,148 people for the 1996 panel and 18,705 people for the 2001 panel. We tried to assess the representativeness of the smaller sample by checking the correlation of the hourly worker sub-sample and the total sample along three different dimensions: the total number at each point in time, the number in each industry, and the number employed in each occupation. The correlation between the total number of people in each sample at each survey date is high, as we would expect: 0.99. We next check the correlation between the number of people in each industry in the two samples, and find it is 0.95. Finally, we check the correlation between the numbers of people in the two samples in each occupation category, and find that the correlation is 0.78. The fact that this last correlation is lower than the other two is not surprising, as we discuss below. Tables 1 and 2 report the breakdowns for industry and occupational category at the more aggregate one digit level. As Table 1 shows, Services is the best represented industry (35% of total hourly workers in the 2001 panel), followed by Trade (25%) and Manufacturing (18%). Agriculture and Mining, on the other hand, have very few observations. As

[7]The complete 2004 Panel is still not available.

[8]Reported monthly hours are the product of reported weekly hours times reported monthly weeks.

for occupational categories, Technical Sales and Support is the best represented in our sample (30%) followed by Operatives (22%) and Services (20%). On the other hand, Professionals and Managers account for only 13% of the total in the hourly workers sample, while they represent almost 30% of the entire survey. Not surprisingly, our smaller sample under-represents occupational categories that are less likely to report receiving hourly wages.

Table 3 gives basic descriptive statistics of reported wages. The average person in our sample is around 38 years old. The average wage rate in our sample is \$10.03 dollars for the 1996 panel and \$11.63 for the 2001 panel. There is, however, a lot of heterogeneity. In the 1996 panel the 5^{th} percentile of the distribution of wages is \$5 and the 95^{th} is \$20. The corresponding values are 5.5 and 23.58 dollars for the 2001 panel. The annualized average wage rate growth is 5.2% for the 1996 panel and 3.9% for the 2001 panel.

3 Method

A key to our results is trying to limit the importance 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. A second step is to apply to the reported data (for both the smaller and larger samples) 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 (1998a and 1998b) 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 $T_1...T_s$. The observed wage at time t, w_t, is equal to a constant α_t plus the unobserved measurement error ϵ_t:

$$\begin{aligned} w_t &= \alpha_1 + \epsilon_t \qquad t = 1...T_1 && (1) \\ &= \alpha_2 + \epsilon_t \qquad t = T_1...T_2 && (2) \\ &= ... && (3) \\ &= \alpha_{s+1} + \epsilon_t \qquad t = T_s...T && (4) \end{aligned}$$

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 if 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 concludes that there are no structural breaks in the sample (i.e., the wage is constant over the whole sample). 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 sub-intervals of data.

Individual examples illustrate how this procedure works.[10] Figure 1 shows the reported and the adjusted wage series for "Linda", a 30-year-old health-care worker with a high school diploma. The series is characterized by four wage increases and two wage decreases over this period. By contrast, the adjusted series shows only two breaks, from \$5.35 to \$5.15 and from \$5.15 to \$8.25 (the last figure being the average of the subsequent reported wages). On the other hand, the corrections applied to the wage series of "Christina," shown in Figure 2, highlight a possible drawback of this procedure. Christina is a 40-year-old woman with a high school degree, who works as a sales officer in a grocery store. Over the 96-99 period, Christina reported three sizable wage increases and one sizable wage decrease, coupled with two small wage increases and three small wage decreases. However, our method reports no structural break, showing how the procedure might be affected by a problem of low power. In order to address this possible problem, we follow Gottschalk (2005) and use a 10% level of significance for the break tests in the baseline results, thus improving the power of the test. As a robustness check, in section 5.2 we report the results obtained using different levels of significance (5%, 15%, 20%, 30% and 50%).

[9]Given the short time periods of the wage histories, the critical values for the structural break tests are obtained through Monte Carlo simulations. For the simulations, we assume that the measurement error follows an AR(1) process.

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

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 a monthly frequency, we report the results at a quarterly frequency for ease of comparison with the previous literature.[11] Table 4 reports the frequency of wage adjustment for hourly workers for the two panels of interest. The reported quarterly frequency of wage adjustment for reported wages is quite close to the standard results estimated by macro modelers using Bayesian techniques applied to aggregate data. In the typical quarter for the 1996 panel, 35.4% of the people report a wage change. The proportion decreases to 33.8% when considering the 2001 panel. The situation changes radically when considering the adjusted series for wages. In the 1996 panel, in the typical quarter only 11.2% of people experience a change in their wage. The proportion declines to 10.2% for the period 2001-2003.

Under the assumption that the correction for measurement error is appropriate, we therefore

[11] We transform monthly results into quarterly results as follows. Suppose that x^m is the average probability of a wage change. Then the quarterly probability of a wage change x^q is obtained as $x^q = 1 - (1 - x^m)^3$.

find evidence of greater wage stickiness than previously found in estimated DSGE models with aggregate data for the US economy. CEE (2005), for example, estimated the quarterly Calvo probability of wage adjustment to be 36% in their benchmark model. In SW (2007) the benchmark estimate of the same parameter is 30%. Our results are consistent with the findings of Gottshalk (2005), who used the same method but analyzed a previous wave of SIPP data. Gottshalk does not report exactly the parameter we estimate, but computing the analogous statistic from his adjusted wage series gives a figure very close to ours (11%). Finally, in a recent contribution, Heckel at al (HLM, 2008) found the average quarterly frequency of wage adjustment to be 35% for a large sample of French firms.[12]

What accounts for the difference between the aggregate results and the greater wage stickiness that we find in micro data? 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 basically nothing to the variance of any aggregate wage series. One possibility is that the difference is due only 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 wages appear to be more sticky, this explanation appears unlikely.

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 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.[13]

4.1 Heterogeneity

Our access to micro data allows us to explore whether wage stickiness differs across sectors or occupations. Table 5 reports the estimated frequency of wage adjustment by industry, and thus gives an idea of the sectoral heterogeneity present in the data. Overall, the amount of wage stickiness

[12] HLM use hourly earnings as their measure of the wage. Since they have access only to firm-occupation data (as opposed to individual data), they are not able to correct for measurement error in reported wages.

[13] 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."

does not seem to vary significantly across different sectors in either the reported or the adjusted data. The "stickies" sector (Construction) is only 15% stickier than the most flexible one (Mining). However, it is worth remembering that our samples from these sectors are quite small (Table 1). The sectors where we have data for a larger number of people (Manufacturing and Services) display very similar degrees of wage stickiness.

We investigate heterogeneity across occupations in Table 6. Again, we do not find significant differences. Our finding of no significant heterogeneity in wage rigidity across industries and occupations is consistent with the earlier findings of Kahn (1997) and HLM (2008).

4.2 Seasonality

A third question we explore regards the seasonality of the wage adjustment. Olivei and Tenreyro (2008) found 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 more likely to take place at the beginning of the year. However, they work offer only anecdotal evidence that wage adjustment is seasonal. We can investigate their proposed explanation using direct observation.

Figure 3 illustrates the frequency of wage adjustment by month. The reported quarterly frequency of wage adjustment does seem to be higher in the second and third quarters and smaller in the first and fourth (which is contrary to the pattern assumed by Olivei and Tenreyro). However, the results obtained using the adjusted wage series display no significant seasonal pattern.

To investigate this issue further, Table 7 report the results of a Probit model where the dummies reporting a change in the wage (or adjusted wage) is regressed against a full set of calendar month dummies.[14] The excluded month is January, so all the other coefficients should be interpreted in relative terms. The statistically significant coefficients are in bold type. Using adjusted wages, especially for the 2001 panel, suggests that there is not much seasonal variation in the quarterly frequency of wage adjustment.

Heckel et al (2008), using French firm-level data, do 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

[14] We control also for gender, age and month of the interview.

law the minimum wage is updated each year in July. There is no such feature in the US labor market.

4.3 Downward Wage Rigidity

Finally, we provide some evidence on the importance of downward wage rigidities. Figures 4 and 5 report the histograms of the yearly non-zero adjusted wage changes for the 1996 and the 2001 panels. (The distributions exclude the observations with no change in adjusted wages.) In order to avoid including outliers in the calculations, we follow Dickens et al (2007a) and exclude wage decreases of more than 35% and wage increases of more than 60%. As the graphs show, wage reductions are much less frequent than wage increases. More precisely, they correspond to the 12% of the non-zero wage changes in the 1996 panel and to the 16% of the non-zero changes in the 2001 panel. Our results suggest that the period between 1996 and 2003 has been characterized by downward nominal wage rigidity. However, a rigorous test of this hypothesis would require a model that would establish a baseline for the expected shape of the distribution of wage changes when there is positive price inflation. With inflation, one would expect to find a right-skewed distribution even if there is no structural downward nominal wage rigidity.

5 Robustness

In order to assess the robustness of our results, we tested the impact on our results of i) increasing the representativeness of the sample ii) changing the significance level of the test for structural breaks and iii) controlling for attrition bias. To avoid presenting too many results, we report results for the 1996 panel, which we chose because it is the longer one.[15] We robustly find a high level of wage stickiness and lack of seasonality in the frequency of wage adjustment. On the other hand, there is some heterogeneity across occupations and industries when considering the total sample, including salaried workers.

5.1 Representativeness

An easy way to increase the coverage of the sample is to include workers who receive a salary, using hourly earnings as the measure of their wage rate. The reason why we chose to present these

[15] Results for the 2001 panel are available upon request.

results as robustness checks, as opposed to baseline results, is apparent from Figure 6, representing the reported and adjusted wage series for "Emi," a 42 year-old manager in the non-profit sector. Emi's reported hourly earnings change very frequently. The adjusted series, on the other hand, is completely flat. While this is an extreme case, and many other examples of salaried workers look more similar to the cases of Linda or Christina, this example suggests that we should expect to observe an higher frequency of reported wage changes and a lower frequency of adjusted wage changes once we expand the sample to include all workers. This is exactly what Table 8 reports. The average quarterly frequency of wage adjustment in the total sample is 64.8% for reported wages and 7.2% for the adjusted ones. Tables 9 and 10 report the breakdowns by occupation and by industry. Interestingly, moderate heterogeneity emerges in wage setting across occupations: Operatives (the most flexible occupation) are almost twice as flexible as Managerial positions (the stickiest). There is still not much difference in wage stickiness across industries. As for seasonality, Figure 7 shows that there is no seasonal pattern in wage changes even when considering the total sample (both in the reported and in the adjusted series).

5.2 Power

In order to address the possible problem of low power, we eased the significance level of the test for structural breaks, thus implicitly improving the power of our tests.

As an illustration, reconsider the wage history of Christina, the 40-year-old grocery-store sales officer. Figure 8 shows the reported and the adjusted wages using the 10% baseline significance level as in Figure 2. The points correspond to the series of adjusted wages using a less demanding 20% level of significance. As the picture shows, and as expected, the procedure allowing for a less demanding test for structural breaks now detects four structural breaks instead of zero, and seems to give a more accurate description of Christina's wage history.

The results obtained with different levels of significance are reported in Table 11, where we report just the baseline results for different level of significance for the 1996 panel. The average quarterly frequency of wage adjustment for the hourly workers increase monotonically with the level of significance of the test, going from 11.2% (with a significance level at 5%) to 19.5% (using a significance level of 50%). In the total sample, the figures range from 5.7% to 17.3%. Overall, the results still suggest a higher level of wage stickiness than typically found in the macro literature.

5.3 Attrition

In order to control for the potential attrition bias that characterizes results coming from survey data, we restrict our attention to the balanced panel, thus considering only the individuals that stayed in the sample for all 12 waves of interviews. These are almost half of the sample.

We expect to find more flexible wages in the balanced panel, since many of the individuals that stayed in the sample for a few interviews displayed constant wage histories.[16] Table 12 reports the results. As expected, the average quarterly frequency of wage adjustment is now higher. For the wage earners the figure suggested is 13.5%, while for the total sample the figure is 8.3%. The lack of any significant seasonal pattern continues to hold in the balanced panel (not shown).

6 Hazard Functions

In this first examination of the data, we have intentionally limited ourselves to computing a statistic that can be interpreted as the constant hazard of a wage change, which is also the statistic estimated in macroeconometric models. However, our data allows us to estimate hazard functions. This allows us to compare the fit of the Calvo-style models of wage rigidity - which imply a constant hazard of experiencing a wage change - *vis a vis* other alternatives, such as contract renogotiations 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 series to estimate a Cox proportional hazard model, where an exit is defined as a change of the reported (or adjusted) wage. A new spell starts each time a new wage is observed, and we include in the sample all the non-left-censored spells with constant wages.[17] We control for age, gender, interview month and educational attainment, and we include a full set of monthly dummies. We then recover the baseline hazard to explore the timing of wage changes.[18] We then plot the results at interval of four months starting from month four of the sample.

Figure 9 shows the estimates of the hazard obtained using the reported wage series for both

[16]intuitively, we are keeping the numerator of our averages constant while reducing the denominator.

[17]We chose to be conservative by not using spells that start after a missing value in the reported wage. Thus, we are probably examining only a subset of all the non-left-censored spells, but we are sure that we are including only spells that are truly non-left-censored.

[18]The baseline hazard at time t is computed as difference in the log of the survivor function at time $t-1$ and the log of the survivor function at time t

the hourly workers and the total sample. The hazard are decreasing, with more than half of the respondents experiencing a wage change in the first four months. Declining hazards are intuitively unappealing, suggesting that there are is indeed significant measurement error in the reported wage. Figure 10 reports the estimates of the hazard obtained using the adjusted wage series. Here, by contrast, there is a clear peak at 12 months, a second local peak at 24 months (for both hourly workers and the total sample), and an even smaller peak at 36 months.

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 flat over the calendar year (Figure 5 and 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.[19]

7 Conclusion

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

First, it would be interesting to see whether fixing the wage stickiness parameters at the levels suggested by our work would change the estimates of other parameters in representative DSGE models. Second, it is important to understand why the stickiness estimated from micro data is greater than the one estimated from aggregate data using Bayesian techniques. The reasons should shed light on the perplexing issues of aggregation that must concern all macroeconomists interested in "structural" models. Third, the lack of seasonality in wage changes leaves an open question: What can explain the estimated differential effects of monetary shocks occurring in different quarters? The results by Nakamura and Steinsson (2008) on the seasonality of price adjustment might constitute the first venue for future research. Fourth, the findings on the shape

[19] 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 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).

of the hazard functions suggest that we should explore fixed-length wage contracting models as in Taylor (1980) in addition to highly-tractable stochastic-length contracting models in the style of Calvo (1983). Fifth, 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 use our data to investigate this different concept of wage stickiness.

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.

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

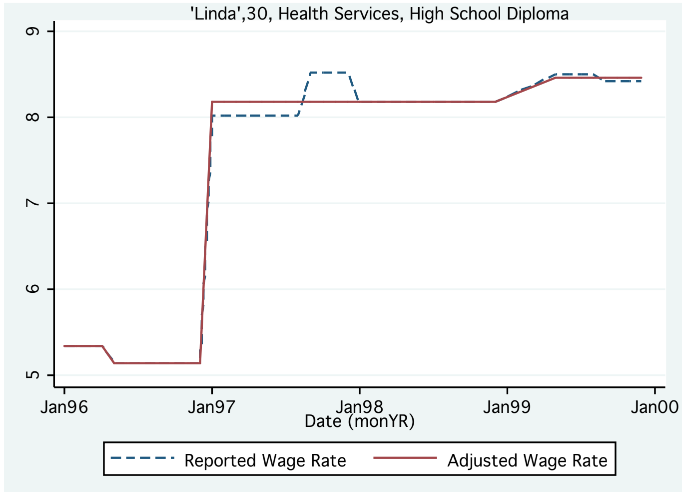

Figure 2: **Adjusted Wage Series, An Hourly Worker**

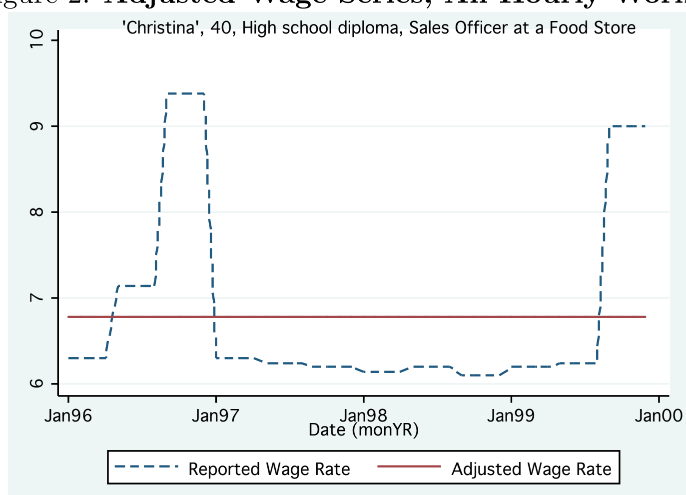

Figure 3: **Seasonality in the Frequency of Wage Adjustment, Hourly Workers**

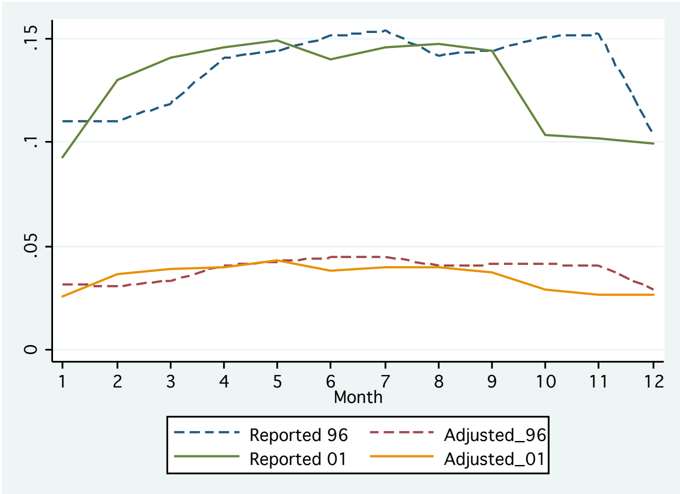

Figure 4: **Distribution of Non Zero Wage Changes, Hourly Workers, 1996 Panel**

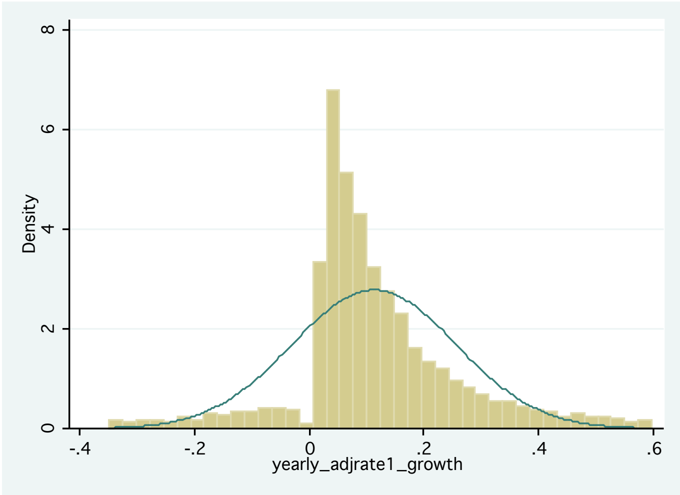

Figure 5: **Distribution of Non Zero Wage Changes, Hourly Workers, 2001 Panel**

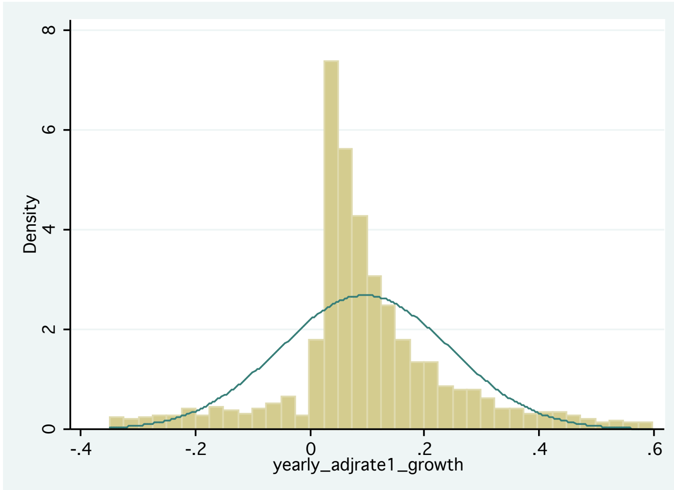

Figure 6: **Adjusted Wage Series, a Salaried Worker**

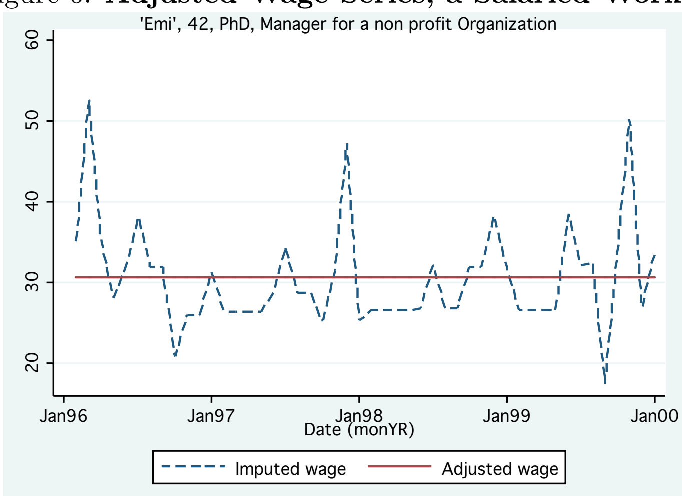

Figure 7: **Seasonality in the Frequency of Wage Adjustment, 1996 Panel**

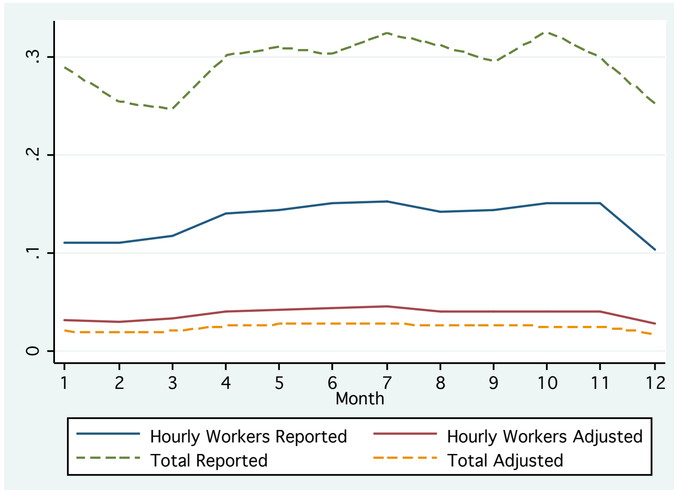

Figure 8: **Adjustment for Measurement Error, Difference Significance Levels**

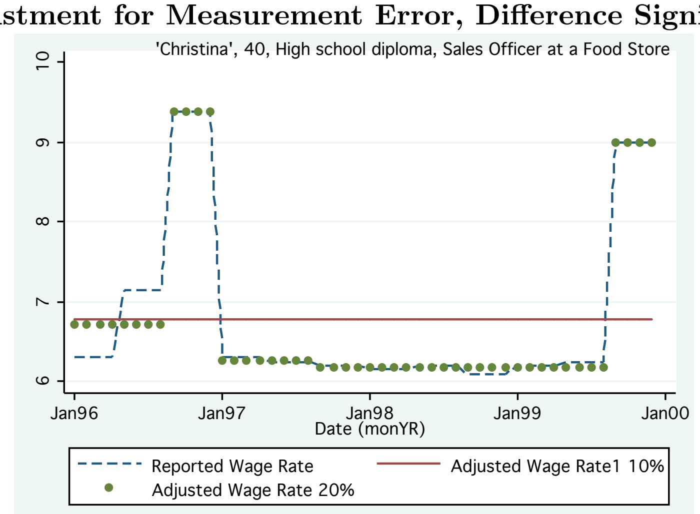

Figure 9: **Hazard of a Wage Change, Reported Wages**

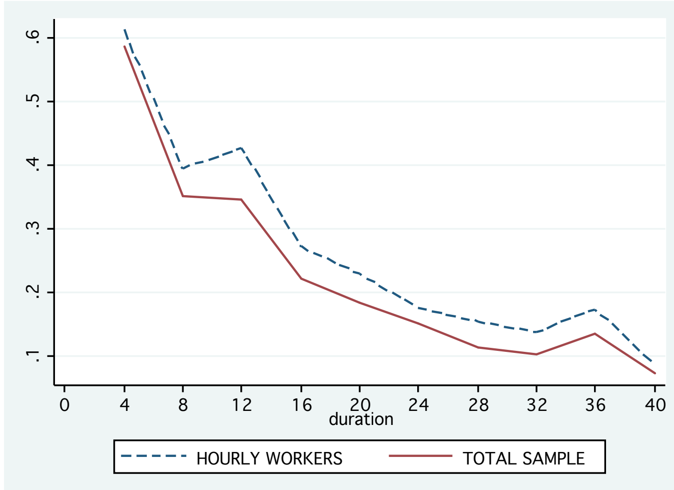

Figure 10: **Hazard of a Wage Change, Adjusted Wages**

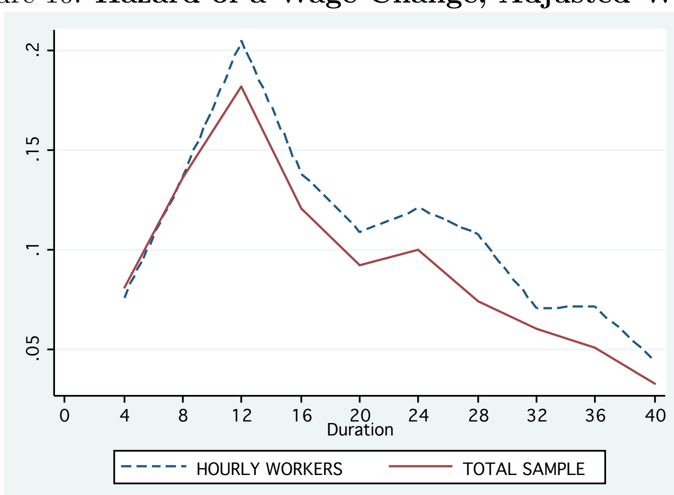

Table 1: **Industry Composition of the Sample**

Year	**1996**	**1996**	**2001**	**2001**
Sample	**Total**	**Hourly**	**Total**	**Hourly**
Agriculture	1.99%	2.25%	1.77%	2.17%
Mining	0.45%	0.43%	0.43%	0.39%
Construction	5.10%	6.58%	5.86%	7.33%
Manufacturing	17.36%	21.48%	15.34%	18.34%
Transport and Communication	7.00%	6.37%	7.09%	6.64%
Trade	20.89%	26.00%	20.45%	25.51%
Services	40.62%	33.36%	42.73%	35.65%
Government and Public Administration	6.08%	3.48%	5.95%	3.93%
Army and Unemployed	0.52%	0.04%	0.38%	0.03%
Total	**39,095**	**17,148**	**38,692**	**18,705**

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

Year	**1996**	**1996**	**2001**	**2001**
Sample	**Total**	**Hourly**	**Total**	**Hourly**
Professional	14.48%	6.02%	15.91%	7.17%
Managerial	12.62%	3.72%	13.74%	5.28%
Technical Sales and Support	30.08%	29.79%	29.27%	30.77%
Craftsmen and production	10.35%	13.63%	9.79%	12.67%
Operatives	15.82%	24.72%	14.65%	21.87%
Service	14.08%	19.59%	14.52%	20.05%
Farming	2.06%	2.48%	1.74%	2.16%
Miscellaneous and Unemployed	0.51%	0.03%	0.38%	0.03%
Total	**39,095**	**17,148**	**38,692**	**18,705**

Table 3: **Descriptive Statistics**

Year	**1996**	**1996**	**2001**	**2001**
Sample	**Total**	**Hourly**	**Total**	**Hourly**
People (beginning)	39,095	17,148	38,692	18,705
Females	19,321	8,931	19,072	9,812
Age	37.6	37.1	38.3	37.3
Wage	13.53	10.03	15.6	11.63
Wage growth	5.8%	5.2%	4.2%	3.9%

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

Period	**Reported**	**Adjusted**	**CEE 05**	**SW 07**	**HLM 08**	**Gottschalk 05**
96-99	0.354	**0.112**				
01-03	0.338	**0.102**				
65-95			0.36			
66-04				0.30		
98-05					0.35	
86-93						0.11

Table 5: **Quarterly Frequency of Wage Adjustment, by Industry, Hourly Workers**

Years	**96-99**	**96-99**	**01-03**	**01-03**
Type	**Reported**	**Adjusted**	**Reported**	**Adjusted**
Agriculture	0.307	0.099	0.303	0.099
Mining	0.370	0.104	0.372	0.113
Construction	0.339	0.109	0.315	0.099
Manufacturing	0.376	0.118	0.355	0.102
Transport and Communication	0.369	0.118	0.352	0.105
Trade	0.346	0.107	0.332	0.097
Services	0.347	0.111	0.334	0.104
Government and Public Administration	0.372	0.116	0.368	0.117
Total	**0.354**	**0.112**	**0.338**	**0.102**

Table 6: **Quarterly Frequency of Wage Adjustment, by Occupation, Hourly Workers**

Years **Type**	**96-99** **Reported**	**96-99** **Adjusted**	**01-03** **Reported**	**01-03** **Adjusted**
Professional	0.353	0.109	0.323	0.107
Managerial	0.340	0.123	0.335	0.105
Technical Sales and Support	0.351	0.115	0.341	0.107
Craftsmen and production	0.364	0.116	0.333	0.102
Operatives	0.364	0.113	0.348	0.102
Service	0.348	0.102	0.335	0.094
Farming	0.313	0.101	0.307	0.102
Total	**0.354**	**0.112**	**0.338**	**0.102**

Table 7: **Seasonality of the frequency of Wage Adjustment, Probit Estimates**

Years **Type**	**96-99** **Reported**	**96-99** **Adjusted**	**01-03** **Reported**	**01-03** **Adjusted**
Feb	-0.016	-0.031	0.017	0.032
	(-0.016)	(-0.021)	(-0.019)	(-0.023)
Mar	0.009	-0.002	0.036*	0.02
	(-0.016)	(-0.02)	(-0.019)	(-0.023)
Apr	**0.044*****	0.023	**0.041****	0.019
	(-0.015)	(-0.019)	(-0.019)	(-0.023)
May	**0.045*****	**0.042****	**0.040****	**0.055****
	(-0.015)	(-0.019)	(-0.019)	(-0.023)
Jun	**0.089*****	**0.062*****	**0.069*****	0.038
	(-0.015)	(-0.019)	(-0.019)	(-0.024)
Jul	**0.066*****	**0.051*****	0.031*	0.024
	(-0.015)	(-0.019)	(-0.019)	(-0.024)
Aug	**0.049*****	0.025	0.044**	0.016
	(-0.015)	(-0.019)	(-0.019)	(-0.024)
Sep	**0.060*****	0.028	0.012	-0.025
	(-0.015)	(-0.019)	(-0.019)	(-0.024)
Oct	**0.045*****	-0.005	0.007	0.02
	(-0.015)	(-0.019)	(-0.02)	(-0.026)
Nov	0.018	-0.033*	0.006	-0.022
	(-0.015)	(-0.019)	(-0.02)	(-0.026)
Dec	-0.024	-0.032	0.016	0.001
	(-0.016)	(-0.021)	(-0.02)	(-0.026)
Pseudo R-sq	0.55	0.32	0.57	0.33
N	626908	626908	446574	446574

Table 8: **Quarterly Frequency of Wage Adjustment, Total Sample, 1996 Panel**

Type	**Reported**	**Adjusted**
Total	0.648	**0.072**
Hourly Workers	0.354	0.112

Table 9: **Quarterly Frequency of Wage Adjustment, by Industry, 1996 Panel**

Sample **Type**	**Hourly Workers** **Reported**	**Hourly Workers** **Adjusted**	**Total** **Reported**	**Total** **Adjusted**
Agriculture	0.307	0.099	0.623	0.063
Mining	0.370	0.104	0.594	0.075
Construction	0.339	0.109	0.575	0.082
Manufacturing	0.376	0.118	0.613	0.083
Transport and Communication	0.369	0.118	0.685	0.070
Trade	0.346	0.107	0.599	0.077
Services	0.347	0.111	0.673	0.067
Government and Public Administration	0.372	0.116	0.731	0.059
Total	**0.354**	**0.112**	**0.648**	**0.072**

Table 10: **Quarterly Frequency of Wage Adjustment, by Occupation, 1996 Panel**

Sample **Type**	**Hourly Workers** **Reported**	**Hourly Workers** **Adjusted**	**Total** **Reported**	**Total** **Adjusted**
Professional	0.353	0.109	0.764	0.050
Managerial	0.340	0.123	0.819	0.047
Technical Sales and Support	0.351	0.115	0.641	0.075
Craftsmen and production	0.364	0.116	0.562	0.086
Operatives	0.364	0.113	0.507	0.092
Service	0.348	0.102	0.518	0.081
Farming	0.313	0.101	0.580	0.068
Total	**0.354**	**0.112**	**0.648**	**0.072**

Table 11: **Quarterly Frequency of Wage Adjustment, Different Significance Levels, 1996 Panel**

Significance Level	**Hourly Workers**	**Total Sample**
5%	0.093	0.057
10%	**0.112**	**0.072**
15%	0.125	0.085
20%	0.137	0.097
30%	0.158	0.121
50%	0.195	0.173

Table 12: **Quarterly Frequency of Wage Adjustment, Balanced Sample, 1996 Panel**

	Reported	**Adjusted**
Total sample	0.677	**0.083**
Hourly Workers	0.386	0.135