Wage Rigidity and Job Creation

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Abstract

Recent research in macroeconomics emphasizes the role of wage rigidity in accounting for the volatility of unemployment fluctuations. We use worker-level data from the CPS to measure the sensitivity of wages of newly hired workers to changes in aggregate labor market conditions. The wage of new hires, unlike the aggregate wage, is volatile and responds almost one-to-one to changes in labor productivity. We conclude that there is little evidence for wage rigidity in the data.

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1. Introduction

Recent research in macroeconomics emphasizes the role of wage rigidity in accounting for the volatility of unemployment fluctuations. Shimer (2005) and Costain and Reiter (2008) documented the failure of a search and matching model to match the volatility of job creation and unemployment. Hall (2005) argued this problem could be fixed with equilibrium wage stickiness instead of period-by-period Nash bargaining over wages. Since then, a large number of studies have appealed to some form of wage stickiness to improve the performance of their model to match the data (Menzio 2005, Farmer 2006, Moen and Rosen 2006, Braun 2006, Blanchard and Galí 2007, Hall and Milgrom 2008, Gertler and Trigari 2009, Kennan 2010 and Shimer 2010, among others).[1]

Sticky wage setting seems to be supported by the observation that wages are less volatile than most business-cycle models predict. However, the volatility of the aggregate wage is neither a sufficient nor a particularly informative statistic to measure the kind of wage rigidity that is required to amplify unemployment fluctuations. In a frictional labor market, job creation is a forward-looking decision and the amount of jobs that are created depends on the expected net present value of wages over the entire duration of the newly created jobs (Boldrin and Horvath 1995, Shimer 2004, Pissarides 2009, Kudlyak 2009). Under long-term wage contracting, the cyclical behavior of this present value may be very different from the cyclical behavior of the aggregate wage. In this paper, we explore whether there is any evidence for rigidity in the present value of wages of newly hired workers.

We use worker-level data from the Current Population Survey (CPS) to measure the sensitivity of the wages of newly hired workers to changes in aggregate labor market conditions and show that the wages of these workers are much more cyclical than the average wage. In our baseline estimates, we find an elasticity of the wage with respect to productivity of 0.8 for new hires compared to 0.2 for all workers. The difference comes from the fact that the wage of workers in existing employment relationships does not respond much to changes in aggregate conditions. Since there are many more workers in ongoing jobs than new hires, this makes the aggregate wage look rigid.

We find that wages in ongoing jobs grow largely independently of aggregate productivity while wages at the start of an employment relationship react strongly to changes in aggregate productivity, similar to what Baker, Gibbs and Holmstrom (1994) found for a single firm. This finding suggests wages are set in long-term wage contracts. Comparing our estimates with the results in Rudanko (2009), we find that the data are consistent with such contracts under limited commitment on the part of both worker and firm.[2]

[1] We use the term wage stickiness to denote an explicitly modeled friction that prevents wages from adjusting to the level that would otherwise obtain. Wage rigidity refers to the observed response of wages to changes in productivity in the data being smaller than one. Clearly, wage stickiness implies wage rigidity, but a certain amount of wage ridigity can also be generated in models with flexible wage setting.

[2] Apart from long-term contracts, which insure risk-averse workers against fluctuations in their wage, theory suggests several other reasons why wages of workers in ongoing employment relationships vary less with aggregate labor market conditions than wages of new hires, as we find in the data: efficiency wages (Yellen 1984), unions (Oswald 1985) or motivational concerns (Bewley 1999).

What do our findings imply for the unemployment volatility puzzle? Long-term wage contracts with a very cyclical starting wage generate strong cyclicality in the expected net present value of wages as well. In that sense, we find very little evidence for wage rigidity in the data.

Previous empirical studies of wage rigidity by macroeconomists have been concerned with *aggregate* wages (Dunlop 1938, Tarshis 1939, Cooley 1995). If the importance of wages of new hires has been recognized at all, then a careful empirical study has been considered infeasible because of lack of data.[3] Labor economists who have studied wages at the micro-level have mostly been concerned with wage changes of individual employees (Bils 1985). Thus, the analysis has naturally been restricted to wages in *ongoing* employment relationships, which have been found to be strongly rigid. Notable exceptions are Devereux and Hart (2006) and Barlevy (2001) who study job changers and find their wages to be much more flexible than wages of workers in ongoing jobs.

The main difference between these studies and ours, is that we focus on newly hired workers, i.e. workers coming from non-employment, which is the relevant wage series for comparison to standard search models, rather than job changers.[4] Since wages of non-employed workers are not observed, we need to use a different estimation procedure, which does not require individual-level panel data. Our procedure has the additional advantage that we can use the CPS, which gives us a much larger number of observations than the earlier studies, which use the PSID or NLSY datasets.[5]

Like previous research, we find strong evidence for cyclical shifts in the composition of employed workers. Solon, Barsky and Parker (1994) show that failing to control for (potentially unobservable) heterogeneity across workers leads to a substantial downward bias in the cyclicality of wages. We document the cyclical patterns in the differences between new hires and the average worker in demographics, experience and particularly in the schooling level that cause this bias. Controlling for fluctuations in the skill level of the workforce is particularly important for our purposes since we study newly hired workers and at least some of the composition bias is likely to be driven by selection in the hiring process. This constitutes a potential weakness of our approach, because we cannot take individual-specific first differences and thus cannot control for unobservable components of skill as Solon, Barsky and Parker do. However, we use the PSID to demonstrate that controlling for observable skill is sufficient to control for composition bias. While unobservable components of skill might be important, they seem to be sufficiently strongly correlated with education to be captured by our controls.[6]

[3] Hall (2005) writes that he does "not believe that this type of wage movement could be detected in aggregate data" (p.51). Bewley (1999) claims that "there is little statistical data on the pay of new hires" (p.150).

[4] Job changers include both workers that experience an unemployment spell and find a new job before the next interview date and workers that move directly from one job to another. Potentially, these are two different groups of workers, although we show in section 3.3. that there is no large difference in the cyclicality of their wages.

[5] More recent literature, inspired in part by this paper, recognizes the importance of wages of new hires and tries to gather more information on how these wages are set. For example, Galuščák et al. (2010) describe a firm-level survey on wage and price-setting procedures in 15 European countries in the context of the ECB's wage dynamics network, which includes specific questions about the determinants of the pay of newly hired workers.

[6] In addition, one may be worried about job heterogeneity. If the average job that is filled in a boom is of higher quality than in a recession, the wage of new hires may look more cyclical than the average wage for an occupation. One could argue, however, that for job creation it is irrelevant whether the wage of new hires is cyclical because the

The two studies most closely related to ours are Pissarides (2009) and Kudlyak (2009). Both of these papers argue, like we do, that wage stickiness in old matches does not matter for job creation as long as the net present value of wages for newly created matches responds to changes in aggregate conditions. Pissarides (2009) surveys the empirical literature on the cyclicality on wages discussed briefly above and concludes that the evidence is not consistent with explanations for the unemployment volatility puzzle that are based on wage stickiness. Kudlyak (2009), like this paper, aims to provide direct evidence on the cyclicality of the net present value of wages in new matches, which she calls the wage component of the user cost of labor. Kudlyak uses panel data from the NLSY and, as a result, there are methodological differences between her paper and ours, see Section 4. for a discussion. Despite these differences, the estimates in Kudlyak's paper and in ours are similar.

In the next section we describe our dataset and comment on some of its strengths and weaknesses. We also provide a comparison of new hires and workers in ongoing jobs in terms of observable worker characteristics. In section 3., we focus on the cyclical properties of the wage and present our estimates of the elasticity of the wage of new hires with respect to productivity. We also discuss how we control for composition bias and explore the robustness of our results. Section 4. discusses the implications of our findings for macroeconomic models of the labor market. Section 5. concludes.

2. Data

A commonly held view in the macro literature is that no data are available to test the hypothesis that the wage of new hires might be much more flexible than the aggregate wage (Bewley 1999, Hall 2005). Some anecdotal evidence seems to point against it.[7] To our knowledge, this paper is the first attempt to construct data on the aggregate wage for newly hired workers based on a large dataset that is representative for the whole US labor market.

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wage for each occupation changes or because there are cyclical shifts in the composition of occupations. To control for job heterogeneity and worker heterogeneity simultaneously, one needs matched employer-employee data. Carneiro, Guimarães and Portugal (2012) use such data for Portugal 1986-2005 and find that, controlling for composition bias due to both sources, entry wages are much more procyclical than wages in ongoing jobs, consistent with our results.

[7] According to Bewley, not only "there is little statistical data on the pay of new hires" (1999, p.150), but in addition, "the data that do exist show little downward flexibility." The data he refers to are average starting salary offers to college graduates in professional fields collected by the College Placement Council. While suggestive, these data are hardly representative for the labor force as a whole. Bewley also cites evidence in favor of wages of new hires being more flexible from Baker, Gibbs and Holmstrom (1994), who show that the average real pay of newly hired managers declined in recessions, even as the wage of existing employees continued to increase.

Some interesting additional suggestive evidence in favor of flexibility in the wage of new hires comes from Simon (2001). Simon documents that during the Great Depression, from 1929 to 1933, wages asked from situations-wanted ads for female clerical workers fell by almost 58%, much more than wages of existing female office workers (17.6%). However, Simon also argues that the wages offered to workers that were actually hired, although more flexible than wages paid to existing workers, fell by much less than wages asked and interprets his findings as evidence that employers rationed jobs. We are grateful to Emi Nakamura for drawing our attention to this paper.

2.1. Individual-level data from the CPS

We use data on earnings and hours worked from the Current Population Survey (CPS) outgoing rotation groups (BLS 2000), a survey that has been administered every month since 1979, allowing us to construct quarterly wage series for the period 1979–2006.[8] In most of the paper we focus on the period after the Great-Moderation, 1984–2006. Wages are hourly earnings (weekly earnings divided by usual weekly hours for weekly workers) corrected for top-coding and outliers and deflated using the deflator for aggregate compensation in the private non-farm business sector.

We match workers in our survey to the same individuals in three preceding basic monthly datafiles. This allows us to identify newly hired workers as those workers that were not employed for at least one of the three months before we observe their wage.[9] In addition, we have information on worker characteristics (gender, age, education, race, ethnicity and marital status), industry and occupation.

We restrict the sample to non-supervisory workers between 25 and 60 years of age in the private non-farm business sector but include both men and women in an attempt to replicate the trends and fluctuations in the aggregate wage. In an average quarter, we have wage data for about 25 000 workers, out of which about 19 000 can be classified to be in ongoing job relationships. The details on the data and the procedure to identify job stayers and new hires are in Online Appendix A.[10]

Figure 1 plots the number of new hires as a fraction of the total number of workers over time. On average, about 8% of employed workers found their job within the current quarter. This fraction seems to have been higher in the 1980s than in the later part of the sample. There is a clear cyclical pattern, with the fraction of new hires substantially higher in recessions.[11] In the quarter with the smallest fraction, we still have about 7% or 1300 newly hired workers. The only exceptions are the third and fourth quarter of 1985 and 1995. In these quarters, we cannot match individuals to the preceding four months because of changes in the sample design so that all our series that require workers' employment history in the previous quarter will have missing values in those quarters.

Table 1 reports summary statistics for some observable characteristics of all workers and of new

[8] The BLS started asking questions about earnings in the outgoing rotation group (ORG) surveys in 1979. The March supplement goes back much further (till 1963), but does not allow to construct wage series at higher frequencies than annual. The same is true for the May supplement, the predecessor of the earnings questions in the ORG survey.

[9] Abowd and Zellner (1985) show there is substantial misclassification in employment status in the CPS and provide correction factors for labor market flows. Misreporting of employment status also affects our results. A worker who, at some point during the survey period, incorrectly reports not to be employed will be classified as new hire by our procedure. Hence, such misreporting implies that some workers who are actually in ongoing relationships will appear in our sample of new hires. Given our argument that the wage of new hires reacts stronger to productivity fluctuations, such misreporting will bias the estimates against our result.

[10] All online appendices as well as the data and Stata codes used for this paper are available as supplemental materials to this article from http://www.sciencedirect.com and http://www.thijsvanrens.com/wage/

[11] This countercyclical pattern may be surprising compared to Shimer's (2012) finding that the hiring rate is strongly procyclical. The difference arises because the hiring rate (or job finding rate) is the ratio of new matches over the number of unemployed workers, whereas here we plot the ratio of new matches over the number of employed workers. We could retrieve the job finding rate by multiplying the series in figure 1 by a factor $(1-u)/u$, where u is the unemployment rate, which is a strongly procyclical factor.

hires (the evolution of some of these characteristics over time may be found in Figure 2 in Online Appendix E). Clearly, newly hired workers are not representative for the labor force. New hires are slightly more likely to be female,[12] and much more likely to be African-American or hispanic. They are also slightly younger and therefore have less labor market experience.[13] Finally, new hires have a year less schooling than the average for all workers. It is not surprising therefore, that new hires on average earn much lower wages. These numbers suggest that workers with lower wages also tend to work in higher turnover jobs, which makes them more likely to have recently started a new job in any given quarter.

2.2. Construction of the wage index

Workers are heterogeneous and newly hired workers are not a representative subsample of the labor force. If the composition of newly hired workers varies over the business cycle, then this heterogeneity will bias our estimate of wage cyclicality. Solon, Barsky and Parker (1994) show that this composition bias is substantial and that failing to control for changes in the composition of employed workers over the cycle makes wages seem less cyclical than they really are.

Taking into account individual heterogeneity, the wage w_{it} of an individual worker i at time t, depends in part on worker i's individual characteristics and in part on a residual that may or may not depend on aggregate labor market conditions.

$$\log w_{it} = x_i'\beta + \log \hat{w}_{it} \tag{1}$$

Here, x_i is a vector of individual characteristics that is constant or varies deterministically with time, like age, and $\hat{w}_{it}$ is the residual wage that is orthogonal to those characteristics.

Following Bils (1985), the standard approach in the micro-literature has been to work with first differences of the wage, so that the individual heterogeneity terms drop out. However, taking first differences of individual wages limits the analysis to workers that were employed both in the current and in the previous period and thus does not allow to consider the wage of newly hired workers. Therefore, we take a different approach and proxy x_i by a vector of observables: gender, race, marital status, education and a fourth order polynomial in experience. We know from an extensive literature on the return to schooling, that these variables explain part of the idiosyncratic variation in wages, see e.g. Card (1999).

To obtain composition-bias corrected wages, we regress log wages on observable worker characteristics and take the residuals. Since we are interested in the comovement of wages with aggregate labor market conditions, we then aggregate by averaging these residuals by quarter for different

[12] The gender difference is driven by the early part of the sample and disappears in the late 1980s, see Figure 2 in Online Appendix E.

[13] If we include workers under 25 years old, the difference in experience becomes much larger. In this sample, new hires have an average experience level of 14.0 years, compared to 19.5 years for all workers because workers that find their first job are classified as new hires. For this reason, we exclude young workers from our baseline sample. The averages for the other characteristics are similar in both samples.

subgroups of workers (e.g newly hired workers or workers in ongoing jobs).[14] Thus, the wage index for subgroup j, $\hat{w}_{jt}$, relates to the average wage of that group of workers, w_{jt}, as follows,

$$\log \hat{w}_{jt} = \log w_{jt} - (x_{jt} - \bar{x}_j)' \beta \tag{2}$$

where x_{jt} is the average of the vector of observable characteristics for that subgroup of workers in each quarter and $\bar{x}_j$ denotes the sample average x_j. Notice that even if an individual worker's characteristics x_i are time-invariant, the average characteristics for a group of workers x_{jt} may vary with time because the composition of the group changes.

2.3. Volatility of wages

Table 2 presents standard statistics for the volatility and persistence of various wage series. We present these statistics for detrended data using the bandpass filter and the Hodrick-Prescott filter. We have also corrected the statistics for the sampling error in the wage series that are constructed from the CPS, which biases the second moments, see Online Appendix B. The volatility of the average wages of all workers in the CPS is lower than the volatility of the aggregate wage. Therefore, we will always compare the wages of newly hired workers to the average wages of all workes from the CPS.

The standard deviation of the wage of new hires is about 40% higher than for the wage of all workers and an F-test overwhelmingly rejects the null that the two variances are equal. The wage of new hires is also somewhat less persistent. The wage for stayers looks consistently very similar to the wage of all workers, because of the fact that in any given quarter, the vast majority of workers are in ongoing job relationships. These results are not specific to the filter used for detrending. This is our first piece of evidence that the wage for newly hired workers is less rigid than the aggregate wage.

3. Response of wages to productivity

We now focus on a particularly relevant business cycle statistic: the coefficient of a regression of the log real wage index on log real labor productivity. This statistic has a natural interpretation as a measure of wage rigidity: if wages are perfectly flexible, they respond one-for-one to changes in productivity, whereas an elasticity of zero corresponds to perfectly rigid wages.

[14] We consider average log wages to be consistent with the aforementioned micro-literature, although our results are robust for log average wages as well.

3.1. Estimation

In order to avoid a spurious estimate of the elasticity if wages and productivity are integrated, we estimate our regression in first differences.

$$\Delta \log \hat{w}_{jt} = \alpha_j + \eta_j \Delta \log y_t + \varepsilon_{jt} \tag{3}$$

where $\hat{w}_{jt}$ is a wage index that controls for changes in the skill composition of the worker pool as in (2), j denotes the subgroup of workers (e.g. new hires) and y_t is labor productivity. Estimating in first differences has the additional advantage that we do not have to detrend the data using a filter, which changes the information structure of the data and therefore makes it harder to give a causal interpretation to the coefficient.

Notice that $\hat{w}_{jt}$ in equation (3) is itself an estimate from the underlying individual level wage data. Previous studies on the cyclicality of wages, starting with Bils (1985), have collapsed the two steps of the estimation procedure into one, and directly estimated the following specification from the micro data.

$$\Delta \log w_{ijt} = \tilde{\alpha}_j + \tilde{\eta}_j \Delta \log y_t + \tilde{\varepsilon}_{ijt} \tag{4}$$

where w_{ijt} is the uncorrected wage of individual i, belonging to subgroup j, at time t, as in (1). However, since the wage last quarter is unobserved for newly hired workers (because they were not employed then), this approach is not feasible for our purpose. Therefore, we implement our procedure as a two-step estimator and estimate (3) from aggregate wage series.

Using the first difference of the average wage rather than the average first difference of the wage means we do not control for individual-specific fixed effects. This raises the question whether our approach to control for composition bias using observable worker characteristics is sufficient to control for all worker heterogeneity. To explore this issue, we re-estimated the results in Devereux (2001), the most recent paper that is comparable to ours. For this purpose, we use annual panel data from the PSID and apply the same sample selection criteria as Devereux does.[15]

The first column of Table 3 replicates Devereux's (2001) estimate of the response of the wage of workers in ongoing relationships to changes in the unemployment rate.[16] This response is estimated as in Devereux, from equation (4) using a two-step procedure. First, we take first differences for the wage of individual workers and average those by year. In the second step, we regress the annual

[15] We are grateful to Paul Devereux for making his data available to us. To our knowledge, Devereux (2001) is the most recent paper with estimates comparable to ours that uses the PSID. Devereux and Hart (2006) use UK data. Barlevy (2001) regresses wages on state-level unemployment rates and includes interactions of the unemployment rate with unemployment insurance. Other more recent papers (Grant 2003, Shin and Solon 2007) use the NLSY. While the NLSY may be well suited to explore some interesting questions closely related to the topic of this paper (in particular, the cyclicality of the wage of job changers because of the much larger number of observations for this particular group of workers), it is not a representative sample of the US labor force.

[16] Previous studies have typically focused on the response of wages to unemployment as a cyclical indicator rather than productivity. Since here we are interested in evaluating the estimation methodology, we follow this practice for comparability.

averages of the change in the wage on the first difference of the unemployment rate.[17] The second column presents the same elasticity, estimated directly from the micro-data in a 1-step procedure, clustering the standard errors by year. As expected, this leaves both the point estimate and the standard error virtually unaltered.

We now try to re-estimate these numbers using the 2-step estimation procedure we use for the CPS, first aggregating wages in levels and then estimating the elasticity in first differences. This procedure, which fails to control for composition bias, gives a very different point estimate, making the wage look less cyclical. However, when we include controls for education and demographic characteristics in the first step, the estimate in column 4 is once again very close to that in Devereux (2001). Surprisingly – given that our procedure is less efficient than the one used by Devereux – we even get virtually the same standard error, suggesting the efficiency loss is small. We conclude that our procedure to control for individual heterogeneity using observable worker characteristics works well in practice.

3.2. Newly hired workers out of non-employment

Table 4 reports estimation results for the elasticity of the wage of new hires with respect to productivity. The regressions in this table include quarter dummies to control for seasonality but are otherwise as in equation (3). For each regression, we report the estimate for the wage elasticity η_j, its standard error and the number of individual and quarterly observations.

The elasticity of the wage of new hires with respect to productivity is higher than the elasticity of the wage of all workers. The wage of new hires responds almost one-to-one to changes in labor productivity, with an elasticity of 0.8 in our baseline estimates. The standard error of this estimate is relatively large. This is due to the fact that the number of newly hired workers in a given quarter is relatively small, so that the wage series for these workers is noisy. However, we believe that it is important to document the evidence for this important statistic even if our estimates are not very precise.

If hours per worker cannot be freely adjusted, one may argue that output per person and earnings per person provide better measures of wages and labor productivity. Results for these measures are also presented in Table 4 and provide a very similar picture as the hourly data. The results are also similar or even strengthened if we use median instead of mean wages or if we weight the regression by the inverse of the variance of the first step estimates to obtain the efficient second step estimator and to different sample selection criteria for constructing average wages from the CPS, see Tables 10 and 11 in Online Appendix E.

[17] Devereux includes a time trend, experience and tenure as additional controls in the second step. In order to exactly replicate his estimates, we do the same. However, excluding these second step controls changes the estimates very little, indicating that first differencing in the first step largely takes care of heterogeneity across workers along these dimensions.

3.2.1. Composition bias

Controlling for composition bias is crucial for our results. This is particularly true for newly hired workers, whose wage is more sensitive to changes in the composition of the unemployment pool. In Table 5, we present alternative estimates if we control only for a subset of observable components of skill. Not controlling for skill reduces the elasticity of the wage of new hires from 0.79 to about 0.67.

We find that education is the most important component of skill. Not controlling for education gives an estimate that is similar to the elasticity we get if we do not control for skill at all. Controlling for experience or demographic characteristics has a much smaller effect on the elasticity. To our knowledge, this result is new. Whereas the importance of composition bias was well known, we document that it is largely driven by education level of unemployed workers, or at least by some component of skill for which the education level is a good proxy.

3.2.2. Wage response by gender and age groups

Much of the micro-literature on wage cyclicality has focused on male workers, arguing that female workers may be more loosely attached to the labor market. While we believe that for our purposes, including both genders provides the correct comparison for the model predicted behavior of wages, in Table 6 we explore how this choice affects our results. The response of wages to productivity is substantially higher for men, although the difference is never significant. The differences are particularly large for newly hired workers. Thus, focusing on male workers only would further strengthen our evidence that wages of new hires are flexible.

Table 6 also presents some estimates including workers from a larger age range in the sample. In our baseline results, we focus on workers between 25 and 60 years old in order to exclude workers on their first job as well as workers close to retirement. Particularly excluding the young workers is important for our result. Adding workers between 20 and 25 years old to the sample, the elasticity of the wage of new hires decreases substantially, although not significantly. The result seems more robust to including older workers between 60 and 65 years old, with the elasticity remaining virtually unaltered. We argue that the behavior of both young and old workers is not described well by a simple model of labor supply and the correct comparison between model and data is to limit the analysis to workers that are in the middle of their career. To make sure we have set our age limits stringently enough, the last rows of the table present results based on workers between 30 and 45 years of age only. Since the sample size goes down substantially, the standard errors increase but the point estimates are almost identical.

3.2.3. Exogenous changes in productivity

Our baseline productivity measure is output per hour. If the production function is Cobb Douglas, the average and marginal product of labor are proportional to each other and output per hour is the appropriate measure of productivity to calculate elasticities. For our purposes, it is irrelevant what drives changes in productivity. The estimates have the same interpretation for any shock that does not affect wages directly, but only through changes in productivity. However, if labor productivity is endogenous, then the causal interpretation of the effect of productivity on wages is lost.

The most prominent possibility of endogeneity in labor productivity are diminishing returns to labor. In this case, the marginal product of labor is proportional to total factor productivity, but the factor of proportionality depends on employment. And since we are not sure what drives fluctuations in employment, this might introduce a spurious correlation between productivity and wages. To explore whether this type of endogeneity is important, we construct a measure of exogenous changes in log productivity, that is given by log output minus $1-\alpha$ times log hours, where $1-\alpha$ is the labor share in a Cobb-Douglas production function. If capital is fixed, this measure is proportional to total factor productivity (TFP).[18] As a more precise measure of TFP, we also use the quarterly version of the Basu, Fernald and Kimball (2006) series, constructed by Fernald (2007).

Since total factor productivity is arguably an exogenous source of fluctuations in labor productivity, we use these measure of TFP to instrument output per hour in our regressions. The results are presented in Table 7. For all instruments, our results become stronger and the elasticity of the wage of newly hired workers is now close to unity.

3.3. Job changers

Throughout this paper, we have focused on newly hired workers out of non-employment. We argue that this is the relevant group of workers to compare a standard search and matching model to. However, as argued by Pissarides (2009), job changers, although not strictly comparable to a model without on-the-job search, may also be informative about wage flexibility of new hires. Some previous studies explored the cyclicality of wages of this group of workers (Bils 1985, Devereux and Hart 2006, Barlevy 2001, see also Pissarides 2009 for a survey of these and other papers).

To compare our results to those studies, we replicate and extend some of the results in Devereux (2001). Using annual panel data from the PSID, 1970-1991, Devereux finds an elasticity of the wage of all workers to changes in the unemployment rate of about -1 and for job stayers of about -0.8. These estimates are replicated in Table 8. Devereux does not report the cyclicality of job changers,

[18] Suppose production requires capital and labor and is of the Cobb-Douglas form with diminishing returns to total hours, $Y_t = A_t K_t^{\alpha} L_t^{1-\alpha}$, where A_t is total factor productivity, K_t is capital and L_t is total hours. Log total factor productivity equals $\log A_t = \log Y_t - \alpha \log K_t - (1-\alpha)\log L_t$, whereas log labor productivity is given by $\log y_t = \log Y_t - \log L_t = \log A_t + \alpha \log K_t - \alpha \log L_t$. This illustrates the problem of endogenous fluctuations in total hours. If what we are interested in is total factor productivity, then log labor productivity is endogenous because of the $\alpha \log L_t$ term. Ignoring fluctuations in the capital stock, which are small compared to fluctuations in labor at high frequencies, we can construct a quarterly productivity series corrected for endogenous fluctuations in total hours as $\log \tilde{y}_t = \log Y_t - (1-\alpha)\log L_t = \log y_t + \alpha \log L_t$.

but this elasticity can readily be estimated using his data and is also reported in the Table.[19] With an elasticity of -2.4, the wages of job changers are much more cyclical than those of all workers.

When we replace the right-hand side variable in these regressions with labor productivity, we find estimates that are very well in line with our baseline results. With an elasticity of about 0.96, the wage of job changers responds almost one-to-one to changes in productivity. The wage of all workers is slightly more responsive than in our baseline estimates (this may be due to the difference in the sample period), but is much less cyclical than the wage of job changers.[20]

Finally, we check whether there might be systematic differences between the PSID and the CPS by estimating the cyclicality in the wage of job changers from our CPS data. After 1994, the CPS asks respondents whether they still work in the same job as at the time of the last interview one month earlier. We use this question to identify job changers and find the estimates in the bottom panel of Table 8. Since we can only use data since 1994, the standard errors of these estimates are very large. The point estimates however, are well in line with the estimates from the PSID.

3.4. Great moderation and pre-1984 wage rigidity

Although our data starts in 1979, all estimates we presented so far were based on the 1984-2006 sample period. The reason is that around 1984 various second moments, relating to volatility but also to comovement of variables, changed in the so called Great Moderation (Stock and Watson 2003). The change in the comovement seems to be particularly relevant for labor market variables, see Galí and Gambetti (2009).

As opposed to most other macroeconomic aggregates, the volatility of wages did not decrease around the Great Moderation. This is true for the aggregate wage as well as for the wage of newly hired workers, see Table 2. We now explore whether the response of wages to productivity changed in this period.

Table 9 presents the elasticity of the wage with respect to productivity for our baseline sample 1984-2006 as well as for the full period for which data are available, 1979-2006.[21] Even though we add only 5 years of data to the sample, the estimates change substantially. The ordering of the response of the wages of the various groups of workers is unchanged: the wage of new hires responds more than the average wage, the wage of workers in ongoing jobs less. All wages, including those of newly hired workers, respond substantially less than one for one to changes in labor productivity prior to 1984.

These findings provide some evidence for wage rigidity prior to the Great Moderation and a

[19] Here we define job changers as workers that are employed in different jobs at two subsequent interview dates. This includes workers that make a job-to-job transition as well as workers that become unemployed and find a new job before the next interview date.

[20] The sample size of job changers in the PSID is very small and the standard error of the elasticity of the wage of job changers to changes in productivity is much larger than our baseline estimate for the response of new hires out of non-employment, despite the fact that the estimation procedure in the PSID is more efficient, see section 3.1..

[21] Ideally, we would like to compare the elasticities to those for the pre-1984 period, but since we have only 5 years of data prior to 1984, this is infeasible.

more flexible labor market since then. While one has to interpret these estimates with care given the short period of data before 1984, they are consistent with studies that have pointed towards changes in the labor market as the ultimate cause of the Great Moderation (Galí and Gambetti 2009) or have even attributed the Great Moderation to a reduction in wage rigidity (Galí and van Rens 2010, Champagne and Kurmann 2011, Nucci and Riggi 2011).

4. Implications for models of wage setting and job creation

What kind of models of wage setting and labor market fluctuations are consistent with the observed behavior of wages? First of all, our results can only be understood if labor markets are subject to frictions.[22] On a frictionless labor market, workers can be costlessly replaced so that each worker is 'marginal' and differences in the wage of newly hired workers and workers in ongoing jobs cannot be sustained as an equilibrium (Barro 1977).

Second, our estimates provide evidence for long-term wage contracts. The difference in the response of wages of workers in ongoing matches versus newly hired workers to changes in productivity indicates stickiness in the wage over the duration of the relation between worker and firm. Approximately, our estimate for the cyclicality of the wages of workers in ongoing matches can be interpreted as the cyclicality of wages over the duration of individual wage contracts.[23]

Our estimates are consistent with the type of wage contracts that have been analyzed in the literature. For example, in Rudanko (2009), wages in ongoing matches are rigid because risk-neutral firms use long-term wage contracts to insure risk-averse workers. The amount of wage rigidity generated this way is limited by the participation constraints of firms and workers. If both the worker and the firm can commit to staying in the match, even if their reservation wage falls below or rises above the rigid wage, then a constant wage is feasible and optimal. If the worker may walk out but the firm can commit to retaining the worker (one-sided commitment), then the wage needs to be more responsive to changes in productivity in order to prevent the worker from leaving, and if neither worker nor firm can commit (two-sided limited commitment) the contract wage needs to be even more cyclical. The elasticity of the average wage with respect to productivity generated by this model is consistent with our estimates if the replacement ratio is around 0.95 under one-sided commitment or around 0.7 under two-sided limited commitment (Rudanko 2009, Figure 4). Reiter shows that, with a replacement ratio of 0.7, the model with long-term wage contracting (under two-sided limited commitment) also correctly predicts the difference in the cyclicality of wages of new hires versus average wages of all workers (Reiter 2007, Table 5). Since the true replacement ratio is probably close to 0.7 (Mortensen and Nagypal 2007), we conclude that our

[22] These may be search frictions, as in Mortensen and Pissarides (1994), or any other labor market frictions that drives a wedge between the reservation wages of workers and firms, see Malcomsom (1999).

[23] This interpretation is only approximate because of compositional changes: the pool of workers in ongoing matches includes workers that were newly hired only last quarter as well as workers that have been in their current job for a long time. However, simulations show that the effect of these compositional changes is negligible in the relevant parameter range, see Online Appendix D.

estimates support long-term wage contracting under two-sided limited commitment.

Third, our estimates cast doubt on the common belief that wage rigidity is the reason that unemployment is more volatile than the search and matching model of the labor market predicts. In the search and matching model, as in all models with long term employment relationships, the period wage is not allocative (Boldrin and Horvath 1995). Labor market equilibrium determines the present value of wage payments over the duration of a match, but the path at which wages are paid out is irrelevant for job creation as long as the wage remains within the bargaining set and does not violate the worker's or firm's participation constraint (MacLeod and Malcomson 1993, Hall 2005). We now consider what this argument implies for the relevance of our estimates.

In a frictional labor market, job creation is a forward-looking decision, which is described by a job creation condition of the following form.

$$c\left(q_t\right) = \frac{\bar{y}_t - \bar{w}_t}{r+\delta} \tag{5}$$

Here, $c\left(q_t\right)$, with $c'\left(.\right) \leq 0$ and $c''\left(.\right) \geq 0$, is the expected net present value of the cost of opening a vacancy, given a probability q_t that the firm can fill this vacancy in a given period, which depends on the unemployment rate and the aggregate number of vacancies. The right-hand side of the equation equals the expected net present value of profits the firm will make once the vacancy has been filled, which depend on the 'permanent' levels of productivity $\bar{y}_t$ and wages $\bar{w}_t$ of the marginal worker, defined as,[24]

$$\bar{x}_t = \frac{r+\delta}{1-\delta}\sum_{\tau=1}^{\infty}\left(\frac{1-\delta}{1+r}\right)^{\tau} E_t x_{t+\tau} \tag{6}$$

where $r > 0$ is the discount rate for future profits and δ the probability that the match is destroyed in a given period. A form of job creation condition (5) holds true in a wide class of labor market models, as we show in Online Appendix C.

When productivity increases, expected profits $\bar{y}_t - \bar{w}_t$ go up, so that firms post more vacancies, reducing the job filling probability q_t until in expectation vacancy posting costs $c\left(q_t\right)$ are again equal to profits. How many vacancies are created depends on how much of the additional match surplus goes to the worker in the form of higher wages. This is why the wage contract matters for the volatility of job creation. To formalize this point, we assume a standard iso-elastic matching technology with constant returns to scale so that we can link the job finding probability p_t to the job filling probability q_t. Let μ denote the share parameter of unemployment in the matching function, so that $p_t = \theta_t^{1-\mu} = q_t^{-(1-\mu)/\mu}$, where θ_t is the vacancy-unemployment ratio or labor market tightness. Then, taking a total derivative with respect to permanent productivity $\bar{y}_t$ and using (5) to calculate the effect of productivity on the job filling probability q_t, we get the following

[24] These are the constant levels for productivity and wages that give rise to the same expected net present value as the actual levels. We borrow the term permanent levels from the consumption literature, cf. permanent income.

expression for the response of the job finding rate to changes in permanent productivity.

$$\frac{d\log p_t}{d\log \bar{y}_t} = -\frac{c(q_t)}{q_t c'(q_t)} \frac{1-\mu}{\mu} \left(\frac{\bar{y}_t}{\bar{y}_t - \bar{w}_t} - \frac{\bar{w}_t}{\bar{y}_t - \bar{w}_t} \frac{d\log \bar{w}_t}{d\log \bar{y}_t} \right) \tag{7}$$

Note that this calculation is similar to the 'steady state elasticities' in Mortensen and Nagypal (2007) and Hornstein, Krusell and Violante (2005), but more general because we did not impose that the labor market is in steady state.

Four things matter for the volatility of the job finding rate in response to productivity shocks: the degree of countercyclicality of vacancy posting costs $q_t c'(q_t)/c(q_t)$, the elasticity of the matching function μ, the level of profits as a fraction of ouput $(\bar{y}_t - \bar{w}_t)/\bar{y}_t$, and the response of the permanent wage with respect to permanent productivity. If wages are fully flexible, in the sense that the elasticity of the permanent wage with respect to permanent productivity equals one, the response of the job finding rate to changes in productivity in (7) depends only on the elasticities of the cost and matching functions. If the response of the permanent wage to permanent productivity does not equal one, then the level of permanent profits is crucial for the amount of labor market volatility the model predicts. By making profits a small share of total match output, i.e. by calibrating the surplus of a match for firms to be small, the response of the job finding rate to changes in productivity can be made arbitrarily large (Costain and Reiter 2008, Hagedorn and Manovskii 2008).

The most important observation for the purpose of this paper is that wage setting only matters insofar as it affects the response of the permanent wage $\bar{w}_t$ to changes in permanent productivity $\bar{y}_t$. The fact that the actual wage w_t does not appear in the equilibrium conditions for the job finding rate p_t illustrates that the path at which wages are paid is irrelevant for job creation. This observation, which was made earlier in Shimer (2004), is crucial to the argument in this paper, as well as in the closely related studies by Pissarides (2009) and Kudlyak (2009).

How large is the response of the present value of wages in new jobs to changes in productivity that is implied by our estimates? Since we estimate wages in ongoing wage contracts to be close to a random walk, the elasticity of the present value of wages is close to the elasticity of the wages of newly hired workers,[25] i.e. $d\log \bar{w}_t / d\log \bar{y}_t = 0.8$. We propose to use this estimate as a calibration target in future research on models with long-term employment relationships.

The only other estimate of the cyclicality of the expected net present value of wages in the literature we are aware of is by Kudlyak (2009). Kudlyak uses panel data from the NLSY and, as a result, there are methodological differences between her paper and ours. The main difference is that Kudlyak estimates wages as a function of time and age of the match using data for matches of all ages. Since the age of a match is not available in the CPS, we can only distinguish new matches from all other matches and have to assume that the cyclicality of wages in ongoing matches does not depend on the age of the match. In addition, Kudlyak can control for individual fixed effects,

[25] Online Appendix D establishes this link more formally.

whereas we can only control for observable worker characteristics, see Sections 2.2. and 3.1.. The advantage of our approach, on the other hand, is that we can use the CPS, a dataset that is much larger and representative for the US labor force. Despite these differences, Kudlyak's estimates for the cyclicality of the expected net present value of wages are similar to ours.

5. Conclusions

In this paper we construct an aggregate time series for the wage of workers newly hired out of non-employment. We find that the wage of new hires reacts almost one-to-one to changes in productivity fluctuations, whereas the wage of workers in ongoing job relationships reacts very little to productivity fluctuations. Controlling for cyclical variation in the skill composition of the workforce is important for this result and we show that the average skill level of the workforce is captured well by the average number of years of education. Finally, we relate our finding to existing studies on the cyclicality of wages of job changers and show that wages of new hires out of non-employment behave similarly to wages of job-to-job movers.

Our results point against rigidity in the wage of newly hired workers as an explanation for the volatility of unemployment over the business cycle as advocated by Hall (2005), Gertler and Trigari (2009) and Blanchard and Galí (2007). Our baseline estimates are based on the post 1984 period and we find some evidence that wages of newly hired workers were more rigid prior to that.

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Table 1: Worker characteristics, sample averages

	All workers	New hires
Percentage of female workers	44.0	44.9
Percentage of African-Americans	11.5	15.2
Percentage of hispanics	9.5	15.0
Education (years of schooling)	13.4	12.2
Experience (years)	20.5	20.1

The sample includes all individuals in the CPS over the period 1984–2006 who are employed in the private non-farm business sector and are between 25 and 60 years old (men and women), excluding supervisory workers. Experience is potential labor market experience: age minus years of schooling minus 6.

Table 2: Volatility of wages at business cycle frequencies

		BP filter		HP filter	
		Relative std. dev.	Auto correl.	Relative std. dev.	Auto correl.
Aggregate wage	1951-2001	0.41	0.92	0.43	0.91
	1984-2006	0.85	0.92	0.84	0.93
CPS, all workers	1984-2006	0.44	0.91	0.67	0.92
CPS, new hires	1984-2006	0.68	0.80	1.09	0.71

The aggregate wage is hourly compensation in the private non-farm business sector from the BLS productivity and cost program. Wages from the CPS are averages for all employed workers in the private non-farm business sector between 25 and 60 years old, excluding supervisory workers, corrected for composition bias as described in the main text. All series in logs. Bandpass filtered data include fluctuations with periodicities between 6 and 32 quarters. HP filtered data use a smoothing parameter of 100,000. In the CPS wage series the moments have been corrected for sampling error as described in Online Appendix B.

Table 3: Reponse of wages of job stayers to unemployment

	2-step est. first diff.	1-step est.	2-step est. levels	2-step est. controls
Elasticity wrt unemployment	-0.81	-0.81	-0.37	-0.80
Std. error	0.20	0.19	0.62	0.20
Observations	42164			

Elasticities are estimated using annual panel data from the PSID, 1979-1991. The estimates in the first column replicate those reported in Devereux (2001), applying his 2-step procedure. In the first step, individual-specific first differences of the wage are regressed on time dummies. In the second step, the coefficients of these time dummies are regressed on the change in the national unemployment rate. This 2-step procedure can be replicated in one step, clustering the standard errors by quarter (column 2). In the third column we regress the log of the average wage on time dummies and then regress the coefficients of these dummies on the unemployment rate in first differences. The fourth column reports the results of our 2-step procedure, which includes individual characteristics (years of education, a fourth order polynomial in experience, and dummies for gender, race, marital status) as control variables in the first step.

Table 4: Response of wages to productivity

	Wage per hour		Earnings per person	
	All workers	New hires	All workers	New hires
Elasticity wrt productivity	0.24	0.79	0.37	0.83
Std. error	0.14	0.40	0.17	0.51
Observations	1566161	117243	1566161	117243
Quarters	83	83	83	83

Elasticities are estimated using the two-step method described in the text. The number of observations is the number of individual workers in the first step. Labor productivity is output per hour in the non-farm business sector from the BLS productivity and cost program. For the hourly wage we use labor productivity per hour and for regressions of earnings per person we use labor productivity per person. The second step includes seasonal dummies.

Table 5: Worker heterogeneity and composition bias

	Wage per hour		Earnings per person	
No controls for skill	All workers	New hires	All workers	New hires
Elasticity wrt productivity	0.14	0.67	0.27	0.73
Std. error	0.15	0.41	0.18	0.50
No controls for experience	All workers	New hires	All workers	New hires
Elasticity wrt productivity	0.26	0.91	0.40	0.94
Std. error	0.14	0.42	0.17	0.53
No controls for education	All workers	New hires	All workers	New hires
Elasticity wrt productivity	0.16	0.54	0.30	0.58
Std. error	0.15	0.40	0.18	0.48
Only controls for education	All workers	New hires	All workers	New hires
Elasticity wrt productivity	0.22	0.92	0.35	0.98
Std. error	0.14	0.44	0.17	0.53

Elasticities are estimated using the two-step method described in the text. The table compares the results for varying specifications of the first step regression. The first specification excludes all controls for individual characteristics from the regression. The second and third specification omit controls for labor market experience and education, respectively. The fourth specification omits controls for both experience and demography but includes controls for education.

Table 6: Differences across gender and age groups

	Men and women		Men only	
Age: 25 – 60	All workers	New hires	All workers	New hires
Elasticity wrt productivity	0.24	0.79	0.26	1.29
Std. error	0.14	0.40	0.14	0.55
Age: 20 – 60	All workers	New hires	All workers	New hires
Elasticity wrt productivity	0.17	0.34	0.21	0.71
Std. error	0.13	0.35	0.13	0.47
Age: 25 – 65	All workers	New hires	All workers	New hires
Elasticity wrt productivity	0.23	0.70	0.25	1.15
Std. error	0.13	0.40	0.14	0.56
Age: 30 – 45	All workers	New hires	All workers	New hires
Elasticity wrt productivity	0.13	0.70	0.20	1.72
Std. error	0.17	0.62	0.19	0.71

Elasticities are estimated using the two-step method described in the text. The table compares the results for different compositions of the sample from which the CPS wages are constructed, varying gender and age ranges.

Table 7: Exogenous changes in productivity

	Wage per hour		Earnings per person	
Corrected labor productivity	All workers	New hires	All workers	New hires
Elasticity wrt productivity	0.33	1.07	0.43	1.00
Std. error	0.18	0.47	0.19	0.55
TFP	All workers	New hires	All workers	New hires
Elasticity wrt productivity	0.26	1.03	0.33	0.82
Std. error	0.19	0.48	0.20	0.55
TFP, corr. for factor utilization	All workers	New hires	All workers	New hires
Elasticity wrt productivity	0.19	1.06	0.29	1.07
Std. error	0.18	0.58	0.23	0.70

Elasticities are estimated using the two-step method described in the text. The table compares the results for varying measures of productivity in the second step regression. The first specification uses a rough measure of TFP, log output minus $1-\alpha$ times log hours worked, where $1-\alpha$ is the labor share in a Cobb-Douglas production function. The second and third specifications use the quarterly version of the Basu, Fernald and Kimball (2006) productivity series. In all cases, these productivity measures are used to instrument labor productivity.

Table 8: Response of wages of job changers

PSID, 1970-1991	All workers	New hires	Job changers
Elasticity wrt unemployment	-1.01		-2.43
Std. error	0.21		0.68
Elasticity wrt productivity	0.43		0.96
Std. error	0.21		0.74
Observations	52525		6406
Years	21		21
CPS, 1994-2006	All workers	New hires	Job changers
Elasticity wrt productivity	0.42	1.31	2.02
Std. error	0.54	1.74	2.09
Observations	863600	62753	57619
Quarters	45	45	45

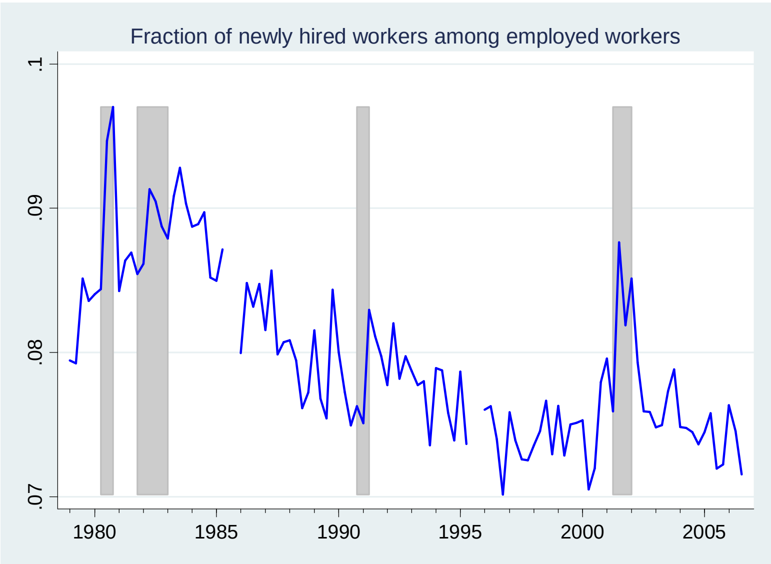
The table compares the response of the average wage of job changers to the average wage for all workers and for new hires. The estimates from the PSID use Devereux's (2001) annual data, take individual-specific first differences and include a linear time trend. The estimates from the CPS are estimated using the two-step method described in the text.

Table 9: Wage rigidity before the Great Moderation

	Wage per hour		Earnings per person	
1984-2006	All workers	New hires	All workers	New hires
Elasticity wrt productivity	0.24	0.79	0.37	0.83
Std. error	0.14	0.40	0.17	0.51
1979-2006	All workers	New hires	All workers	New hires
Elasticity wrt productivity	0.18	0.49	0.20	0.30
Std. error	0.11	0.32	0.10	0.35

The table compares the results for our baseline sample of post 1984 data to the full sample starting in 1979. Elasticities are estimated using the two-step method described in the text.

Figure 1: Fraction of new hires among employed workers



The graph presents the number of new hires as a fraction of the total number of employed workers. The sample includes all individuals in the CPS who are employed in the private non-farm business sector and are between 25 and 60 years of age (men and women), excluding supervisory workers. New hires are workers that were non-employed at least once within the previous 3 months. The gaps in the graph are quarters when it is not possible to identify newly hired workers, see Online Appendix A. The grey areas indicate NBER recessions.