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Wage Rigidity and Job Creation

by Christian Haefke, Marcus Sonntag and Thijs van Rens

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Keywords: Wage Rigidity, Search and Matching Model, Business Cycle

JEL Codes: E24, E32, J31, J41, J64

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Wage Rigidity and Job Creation*

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Abstract

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1 Introduction

This paper documents that wages of newly hired workers out of non-employment strongly respond to aggregate labor market conditions. In the context of a labor market that is characterized by search frictions, the wage of newly hired workers is important because new hires are the 'marginal' workers that affect firms' decisions to create jobs. The wage of workers in ongoing jobs on the other hand, does not fluctuate much. Since there are many more workers in ongoing jobs than new hires, this makes the aggregate wage rigid. To document these facts, we construct time series for the wage of various subgroups of workers from the CPS, the largest publicly available US micro-dataset that allows to make this distinction.

Shimer (2005) and Costain and Reiter (2008) showed that a business cycle version of the search and matching model falls severely short of replicating labor market dynamics. In particular, for commonly used calibrations of the model, the predicted volatility of labor market tightness and unemployment is much lower than observed in the data. Shimer argued that period-by-period Nash bargaining over the wage leads wages to respond strongly to technology shocks, dampening the effect of these shocks on expected profits and therefore on vacancy creation. He suggested wage rigidity as a mechanism worth exploring to amplify the response of vacancy creation and unemployment to technology shocks.

Hall (2003) proposed a model of unemployment fluctuations with equilibrium wage stickiness, in which wages are completely rigid when possible and rebargaining takes place only when necessary to avoid match destruction (either through a layoff or a quit). In Hall's model there is a unique market wage, which implicitly extends this rigidity of wages on the job to wages of newly hired workers. A large number of more recent papers have appealed to some form of wage rigidity to improve the performance of labor market models with search frictions to match the business cycle facts in the data (Costain and Reiter 2008; Menzio 2005; Rudanko 2008; Farmer 2006; Moen and Rosen 2006; Braun 2006; Gertler and Trigari 2006; Blanchard and Galí 2008; Kennan 2008; Hall and Milgrom 2008; Shimer 2009).

Few economists would doubt the intuitive appeal of this solution. A simple supply and demand intuition immediately reveals that technology shocks lead to larger fluctuations in the demand for labor if wages are rigid. Furthermore, it is a well documented fact that wages are less volatile than most models of the business cycle predict.[1] Using individual-level panel data on wages, several studies document evidence for wage rigidity (Bils 1985, Solon, Barsky and Parker 1994, Beaudry and DiNardo 1991).

We argue, however, that the empirically observed form of wage rigidity does not

[1] Like the observation that employment (or total hours) is more volatile than predicted by the model, this is true for Real Business Cycle models, search and matching models as well as new Keynesian models.

generate additional volatility in employment and vacancies. The argument goes in two steps. First, we present new evidence that wages of newly hired workers are volatile and respond one-to-one to changes in productivity. We also find that wages for *ongoing* job relationships are indeed rigid over the business cycle, as in previous studies. Second, we show that in order to replicate these findings in a search model, we need to assume that wages in ongoing jobs are rigid but at the start of a job are set in a perfectly flexible manner. This kind of wage rigidity does not affect job creation. Thus, there is evidence for wage rigidity, but not of the kind that leads to more volatility in unemployment fluctuations.

The first contribution of this paper is to to construct a large, representative dataset of wages for newly hired workers out of non-employment. We use data on earnings and hours worked from the Current Population Survey (CPS) outgoing rotation groups to calculate wages. We match the outgoing rotation groups to the basic monthly data files and construct four months employment history for each individual worker. We use these micro-data to construct quarterly time series for a wage index of new hires and workers in ongoing jobs and explore the cyclical properties of each series. After controlling for composition bias, we find an elasticity of the wage with respect to productivity of 0.8 for new hires and 0.2 for all workers.

Previous empirical studies on 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.[2] This practice has given rise to the conventional wisdom that wages fluctuate less than most models predict and that the data would therefore support modeling some form of wage rigidity.

Labor economists who have studied wages at the micro-level have mostly been concerned with wage changes of individual employees. 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 both study job changers and find their wages to be much more flexible than wages of workers in ongoing jobs. Pissarides (2007) surveys these and other empirical micro-labor studies and concludes that wages of job changers respond much stronger to unemployment than wages of workers in continuing employment relationships.

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.[3] Since wages of

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

[3] 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 very different groups of workers, although we show in section 3.3 that there is no large difference in

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.

Like previous research, we find strong evidence for composition bias because of worker heterogeneity. 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 are sufficiently strongly correlated with education to be captured by our controls.

A final difference between this paper and the existing literature is that we focus on the response of wages to changes in labor productivity, whereas previous studies have typically considered the correlation between wages and the unemployment rate. With a search model, in which fluctuations are driven by exogenous changes in labor productivity but unemployment fluctuations are endogenous, our statistic is the more interesting one.[4] The elasticity of the wage to labor productivity has a natural interpretation in a wide range of models. It is not necessary for example, that changes in labor productivity are driven by technology shocks. Our estimates have the same interpretation for any shock that does not affect wages directly, but only through changes in productivity, e.g. government expenditure shocks or monetary policy shocks. We explore the robustness of our estimates to alternative measure of productivity and find very similar results. If we use unemployment rather than productivity as our regressor, we find similar estimates to those of Barlevy (2001) and Devereux and Hart (2006) for job changers. This indicates that the wage of new hires out of non-employment behaves similar to that of job-to-job movers and lends additional credibility to our estimates.

Our second contribution is to point out the implications of our findings for the unemployment volatility puzzle. In the standard stochastic search and matching model as in Shimer (2005), the elasticity of the wage with respect to productivity is close to

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the cyclicality of their wages.

[4]Moreover, as pointed out by Hagedorn and Manovskii (2008), since we are ultimately interested in the predictions of the model for unemployment fluctuations, the calibration targets, including the cyclicality of wages, should not depend on unemployment. We discuss this issue further in section 3.

one. We refer to this model, in which wages are set period-by-period through Nash bargaining, as the flexible wage model.[5] In order to match our estimate for the average wage elasticity of all workers, we need to assume that wages are rigid in ongoing job relationships. By rigidity we mean any kind of constraint on the wage bargaining process that implies that the division of match surplus between worker and firm shifts in favor of workers in periods when the surplus is small.

Theory suggests several reasons why wages of newly hired workers should vary more strongly with productivity than wages of workers in ongoing employment relationships. Beaudry and DiNardo's (1991) model of implicit wage contracts is a good illustration of the type of wage rigidity that we believe to be plausible. Upon the start of a work-relationship the bargaining parties are relatively free in their wage determination. However, once the contract has been signed, wages can no longer be changed very much, in order to insure the worker against fluctuations in her income. In addition, internal labor markets can give rise to almost deterministic wage increases for continuing workers (Baker, Gibbs and Holmstrom, 1994). Many other theories of wage rigidity, because of efficiency wages (Yellen 1984), unions (Oswald 1985), motivational concerns (Bewley 1999) or simply because rebargaining is costly, all provide plausible explanations for why wages are not changed very often during the relationship, but do not seem to apply to newly hired workers.

Wage rigidity in ongoing jobs has no effect on job creation and unemployment fluctuations in the standard search and matching model. What matters for employment dynamics is not the aggregate wage in the economy, but the wage of the marginal workers that are being hired. Formally, when firms decide on whether or not to post a vacancy, they face a trade-off between the search costs (vacancy posting costs) and the expected net present value of the profits they will make once they find a worker to fill the job. Thus, what matters for this decision is the expected net present value of the wage they will have to pay the worker they are about to hire. How this expected net present value is paid out over the duration of the match, is irrelevant (Boldrin and Horvath 1995, Shimer 2004, Kudlyak 2007). Previous studies that have used wage rigidity to explain the unemployment volatility puzzle, have either extended the rigidity to newly formed matches (Hall 2005, Gertler and Trigari 2006) or find very small effects (Rudanko 2008).

What do our results imply for the unemployment volatility puzzle? We show that there is no need to assume rigidity in the wage of newly hired workers in order to match the wage data. Based on our estimates, we cannot rule out a moderate degree of rigidity in the wages of these workers, like for example the bargaining setup in Hall and Milgrom (2008), which reduces the influence of the value of unemployment on the outcome of the wage bargain. Neither can we rule out a calibration as in Hagedorn and Manovskii (2008) that relies on a wage elasticity slightly smaller than one in combination with a very small

[5] The number depends on the calibration. For example, if workers' bargaining is very low, as in Hagedorn and Manovskii (2008), the elasticity is much lower, although wages in that model are flexible.

match surplus. In fact, we find some evidence that the response of wages of new hires to changes in productivity is smaller in the period before the Great Moderation. In the post 1984 period however, we find no evidence for rigidity in the wage of new hires and in the expected net present value of wage payments for newly created jobs.

The remainder of this paper is organized as follows. 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

The prevailing opinion 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.[6] 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.

2.1 Individual-level data from the CPS

We use data on earnings and hours worked from the CPS outgoing rotation groups from the BLS (2000), a survey that has been administered every month since 1979 which allows us to construct quarterly wage series for the period 1979–2006.[7] However, in most of the paper we focus on the post Great-Moderation period 1984–2006. Wages are hourly

[6] 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.

[7] 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.

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.[8] 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 Appendix A.

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 then in the later part of the sample. There is a clear cyclical pattern, with the fraction of new hires substantially higher in recessions.[9] 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 workers and the evolution of some of these characteristics over time can be found in Figure 2. Clearly, newly hired workers are not representative for the labor force. New hires are more likely to be female, and much more likely to be African-American or hispanic. They are also slightly younger and therefore have less labor market experience.[10] Finally, new hires

[8] 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.

[9] This countercyclical pattern may be surprising compared to Shimer's (2007) finding that the hiring rate is strongly procyclical. The difference is 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.

[10] 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.

have on average 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_{ijt} 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 else varies deterministically with time, like age, and $\hat{w}_{ijt}$ 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 subgroups of workers (e.g newly hired workers or workers in ongoing jobs).[11] Thus, the wage index for subgroup j, $\hat{w}_{jt}$, relates to the average wage of that group of workers, w_{jt}, as follows,

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

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

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 are time-invariant, the average characteristics for a group of workers 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 Appendix B.

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. Also, our conclusions are the same, and often even starker, if we use the median instead of the mean wage for each group. 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.

As pointed out by Hagedorn and Manovskii (2008), the elasticity of wages with respect to productivity is a better summary statistic for calibrating the search model than the correlation or elasticity of wages with other variables, like the unemployment rate, vacancies or labor market tightness. There are at least two reasons for this. First, in the model, other labor market variables are endogenous, but productivity is exogenous. Therefore, a regression of log wages on log productivity will deliver an unbiased estimate of the elasticity. Second, the coefficient of a regression of wages on unemployment or vacancies is inversely proportional to the variance of these variables. If we are evaluating the performance of the model to match these variances, then we do not want to target them in the calibration.

3.1 Estimation

In the context of this paper, there are additional advantages of using the elasticity rather than the correlation of wages with productivity. Our wage series are subject to (intertemporally uncorrelated) measurement error. This biases the volatility of wages and therefore their correlation with other variables (see Appendix B). In a regression, however, measurement error in the dependent variable does not bias the coefficient. Moreover, the coefficient has a clear causal interpretation as an elasticity, it is straightforward to calculate standard errors, and we can easily control for other factors that affect wages, if necessary.

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.

The main methodological difference between our study and previous work, which allows us to explore the cyclicality in the wage of newly hired workers, is that we use the first difference of the average wage, rather than the average first difference of the wage, as the dependent variable. 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.[12]

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.[13] 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 averages of the change in the wage on the first difference of the unemployment rate.[14] 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 and 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

Estimation results for the elasticity of the wage of new hires with respect to productivity are reported in Table 4. 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 quarterly observations.

The elasticity of the wage of new hires with respect to productivity is much higher than the elasticity of the wage of all workers. The wage of new hires responds almost

[12] 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 from 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.

[13] 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.

[14] 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.

one-to-one to changes in labor productivity, with an elasticity of 0.79 in our the baseline estimates. The point estimates are never significantly different from one and often significantly different from zero. Thus, we do not find evidence for wage rigidity in the wage of new hires.

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, see Table 5. Finally, the results are robust to different sample selection criteria for constructing average wages from the CPS, see Table 6.

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 7, 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 0.54.

We find that education is by far 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 8 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 8 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 stringent 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. As Hall (2007) has recently pointed out, the average and marginal product of labor are proportional to each other if the production function is Cobb Douglas and, under this assumption, 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).[15] 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

[15] 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 $\log \tilde{y}_t = \log Y_t - (1-\alpha)\log L_t = \log y_t + \alpha \log L_t$.

regressions. The results are presented in Table 9. For all instruments, our results become stronger and the elasticity of the wage of newly hired workers is now very 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 to a standard search and matching model. However, as argued by Pissarides (2007), 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 2007 for a survey of these and other papers). Compared to new hires out of non-employment, job changers are an attractive group to study because one can control for composition bias by taking an individual-specific first difference.

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 10. Devereux does not report the cyclicality of job changers, but this elasticity can readily be estimated using his data and is also reported in the Table.[16] 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.[17]

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 10. 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.

We find that the wage of job-to-job movers responds similarly to changes in labor

[16] 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.

[17] Notice that 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.

market conditions as the wage of newly hired workers out of non-employment and -if anything- is even more cyclical. Intuitively, this makes sense. A story of wage rigidity that is based on rigidity in ongoing job relationships would affect neither new hires out of non-employment nor job-to-job movers. To the best of our knowledge, this result was not known before. It justifies the exercise in Pissarides (2007), to use the wage of job changers as a proxy for the wage of newly hired workers out of unemployment to calibrate a search and matching model without on-the-job search.

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 (2007).

As opposed to virtually all 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 11 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.[18] Even though we add only 5 years of data to the sample, wage respond substantially less to changes in productivity over the full sample than in the post 1984 period. 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. However, now even the wage of newly hired workers responds substantially less than one for one to changes in labor productivity. Like our baseline results, these estimates are robust across different measures of productivity, different sample selection criteria and different ways to calculate the wage series or to estimate the elasticity.

These findings provide some evidence for wage rigidity prior to 1984 and a flexibilization of the labor market during the Great Moderation. And because there seems to have been rigidity in wages of newly hired workers as well as in wages of workers in ongoing jobs, this flexibilization may have affected fluctuations in employment and other macroeconomic aggregates. 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 on the labor market as the ultimate cause of the Great Moderation (Galí and Gambetti 2007) or have even attributed the Great Moderation to a reduction

[18] 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.

in wage rigidity (Gourio 2007).

4 Implications for job creation and unemployment

What kind of models of labor market fluctuations are consistent with the observed behavior of wages? First of all, it must be that the labor market is subject to search frictions. 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. In this section we show that, in addition to search frictions, we also need rigidity in the wages of workers of ongoing jobs in order to match the low response of those wages to changes in productivity. We also show that wages must be close to flexible at the time of creation of a match to match the response of wages of newly hired workers.

The type of wage rigidity we find to be consistent with the data (flexible at the start of a match, rigid over the duration of the job) does not affect job creation and therefore is unlikely to explain the unemployment volatility puzzle. The basic intuition for this result is that in search and matching models, 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 these wage payments in a match, but the path at which wages are paid out over the duration of the match 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 (Hall 2005). This means that wage rigidity matters only if it implies rigidity in the expected net present value of wage payment at the start of a match (Shimer 2004).

4.1 Job creation on a frictional labor market

To illustrate this point, consider a standard search and matching model with aggregate productivity shocks. Because we focus on job creation, we assume job destruction is exogenous and constant, as in Pissarides (1985). We think of fluctuations as being driven by shocks to productivity, as in Shimer (2005).[19] In this model, job creation is determined by vacancy posting. Risk-neutral firms may open a vacancy at cost $c > 0$ per period. With probability $q(\theta_t)$, a firm finds a worker to fill its vacancy, in which case a match is formed. The worker finding probability is strictly decreasing in labor market tightness $\theta_t = v_t/u_t$, where v_t is the total number of vacancies in the economy and u_t is the unemployment rate. Matches produce output y_t and the worker needs to

[19] Our empirical results do not rely on this assumption. If business cycles were driven, for example, by demand shocks, these shocks would still affect wages only through the productivity of labor. However, in more general models the effect of wage rigidity on unemployment fluctuations is less clear, because there may be interaction effects with other frictions like nominal rigidities, see e.g. Thomas (2008).

be paid a wage w_t so that profits are $y_t - w_t$ in every period. With probability $\delta \in (0,1)$, matches are exogenously separated.

The decision how many vacancies to post is a trade-off between the vacancy posting costs on the one hand and the expected net present value of profits on the other. This trade-off is summarized by the job creation condition,[20]

$$c = q(\theta_t) \frac{\bar{y}_t - \bar{w}_t}{r + \delta} \tag{5}$$

where $r > 0$ is the discount rate for future profits and $\bar{y}_t$ and $\bar{w}_t$ are the 'permanent' levels of productivity and the wage, defined as[21]

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

Notice that the firm uses an effective discount rate of $r+\delta$ because of the possibility that the match is destroyed. When expected profits go up, firms post more vacancies, which increases labor market tightness θ_t and therefore reduces the worker finding probability until in expectation profits are equal to the vacancy posting costs c again. The derivation of equation (5) is standard; details may be found in Appendix C.1.

We now turn to the question what kind of wage determination mechanism we need to assume in order to match our findings for the response of wages to changes in productivity. If wages are rigid in the sense that the permanent wage $\bar{w}_t$ does not increase in response to an increase in (permanent) productivity $\bar{y}_t$, then profits and therefore vacancy creation respond more strongly to this increase in productivity. Because we can think of the job creation equation (5) as a labor demand curve, this is the sense in which search models replicate the Walrasian intuition for why wage rigidity amplifies unemployment fluctuations. The difference with the Walrasian framework is that not current profits $y_t - w_t$ matter for vacancy creation, but the expected net present value of profits over the duration of the match.

4.2 Flexible wages

Because search frictions drive a wedge between the reservation wages of firm and worker, there is a positive surplus from a match. The standard assumption in the literature is that each period, firm and worker engage in (generalized) Nash bargaining over the wage, so that each gets a fixed proportion of the surplus. Under this assumption, we

[20] We write the model in discrete time but assume that all payments are made at the end of the period, so that the expressions look similar to the continuous time representation.

[21] 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.

can derive the following wage curve or labor supply equation,

$$\bar{w}_t = (1 - \beta)\, b + \beta \bar{y}_t + \beta c \bar{\theta}_t \tag{7}$$

where b is the value to the worker of being unemployed in each period, which includes utility from leisure as well as the unemployment benefit, and β is workers' bargaining power in the wage negotiations. The wage depends on labor market conditions because of the worker's outside option to look for another job. The derivation of equation (7) is again standard, see Appendix C.2. Combined with the job creation equation (5), the wage curve fully describes the equilibrium of the model.

If wage bargaining takes place in every period, the wage in this model is flexible in the sense that it immediately adjusts to changes in productivity and labor market conditions. To explore the quantitative predictions of the flexible wage model for the response of wages to changes in productivity, we assume that y_t follows an exogenous stochastic process that is consistent with labor productivity data, and simulate the model. The details of the calibration and simulation procedure are described in Appendix C.3. Since some of the parameters are calibrated directly to data, we show only the model predictions for different values of the unemployment benefit b and workers' bargaining power β, keeping the other calibration targets fixed at the values used by Shimer (2005).

The simulation results in Table 12 reveal several interesting patterns. First, the elasticity of the wage of newly hired workers with respect to current productivity is very close to the elasticity of the permanent wage with respect to permanent productivity for all calibrations. Since we observe the former, but the latter matters for job creation, this finding is encouraging in light of the exercise in this paper. (In section 4.3, we discuss why the two elasticities are not exactly the same.)

Second, we find that the response of the wage of newly hired workers is identical to the response of the wage of job stayers to changes in productivity. This finding is not surprising. Since all firms and all workers are identical, they have the same outside options at each point in time. And since each firm-worker pair bargains over the wage in each period, they always agree on the same wage. This prediction of the model however, is clearly at odds with our estimates.

Finally, the simulation results show that the elasticity of the wage with respect to productivity is close to one for a wide range of parameter values. In models with a frictionless labor market, this elasticity is always exactly equal to one if the expenditure share on labor in the production function is constant. In that case, the marginal product of labor is proportional to its average product, and the wage equals the marginal product. However, on a labor market with search frictions, the wage is no longer equal to the marginal product of labor. What we show here is that for a wide range of calibrations, the wage is roughly proportional to the marginal product. This provides an intuitive benchmark for the empirical results: in a model with flexible wage setting, wages should respond almost one-for-one to changes in labor productivity.[22] And this prediction is consistent with our estimate of the response of the wage of newly hired workers, suggesting that wage setting is flexible for those workers.

Summarizing, a model with search frictions on the labor market, but perfectly flexible wage setting, predicts a response of wages of newly hired workers to changes in productivity that is in line with our estimates. The model fails however, to capture the substantially lower response of wages of workers in ongoing matches. This suggests that wages in ongoing jobs are rigid. We now proceed to introduce this kind of wage rigidity into the model.

4.3 Rigid wages in ongoing jobs

We maintain the assumption that wages are determined by Nash bargaining, but only at the start of a match. Thereafter, wages are rigid so that they do not change much anymore for the duration of the match. Under this assumption the wage curve is exactly like (7). Notice that the permanent wage depends not only on current but also on expected future labor market conditions, because by accepting a job the worker forfeits the option value to find another job in the future. The fact that the period wage does not appear in the equilibrium conditions for θ_t illustrates that the path at which wages are paid is irrelevant for labor market tightness θ_t and therefore job creation. The period wage is not determined in this model, unless we explicitly model the type of wage rigidity we have in mind.

As an extreme case, assume that wages are perfectly rigid in ongoing jobs. This is the model analyzed in Shimer (2004). As in that paper, we need to make an assumption to avoid inefficient match destruction. Shimer assumes that search frictions are large enough that, given the stochastic process for labor productivity, the wage in ongoing matches never hits the bounds of the bargaining set. Here, we make the simpler assumption of full commitment on the part of both worker and firm, so that matches never get destroyed endogenously (as in the simple case in Rudanko 2008). This model is relatively simple to solve. The simulation results are presented in Table 13.

Three main results follow from the simulations. First, wage rigidity in ongoing jobs drives a wedge between wages of newly hired workers and of workers in ongoing jobs, the latter now responding substantially less to changes in productivity than the former. Second, some of the wage rigidity seems to 'spill over' to newly hired workers and the response of the wages of these workers to changes in productivity is now substantially less than one. Third, this type of wage rigidity does not affect the response of the

[22] The only calibrations for which the elasticity is substantially smaller than one are very small values of workers' bargaining power as, for example, in Hagedorn and Manovskii (2008), who calibrate β to a wage elasticity of 0.3. This calibration is ruled out by our estimates for the response of wages of newly hired workers. Notice however, that this it is not crucial for their result that the flexible wage model can match the volatility of vacancies and unemployment. Even with large values for β, the model can generate large amounts of volatility as long as b is close enough to 1 so that the match surplus is small.

permanent wage to changes in permanent productivity and therefore also does not affect the volatility of job creation. We discuss each of these results in turn.

Since we assumed wages of workers in ongoing jobs to be rigid, it is not surprising that the wage of this group of workers responds less to productivity than the wage of newly hired workers, which is not subject to the rigidity. The only reason that the elasticity for job stayers is not equal to zero is that the group of stayers changes over time: this period job stayers includes last period's new hires. But because the fraction of new hires is small compared to the overall size of the labor force, this effect is small. The much lower responsiveness of the wage of workers in ongoing jobs than the wage of new hires to changes in productivity is consistent with our estimates, improving the ability of the model to match the wage data compared to the model with perfectly flexible wages.

To understand why the wage of newly hired workers responds less than one-for-one to changes in productivity, despite the fact that wages setting is flexible for these workers, it is useful to consider the following identity,

$$\frac{d\log \bar{w}_t}{d\log \bar{y}_t} = \frac{d\log \bar{w}_t / d\log w_t^0}{d\log \bar{y}_t / d\log y_t} \frac{d\log w_t^0}{d\log y_t} \tag{8}$$

where w_t^0 denotes the wage of newly hired workers, so that $d\log w_t^0/d\log y_t$ is the elasticity of the wage of newly hired workers with respect to current productivity, which we observe, and $d\log \bar{w}_t/d\log \bar{y}_t$ is the elasticity of the permanent wage with respect to permanent productivity, which determines fluctuations in job creation. The elasticity of the permanent wage differs from that of the wage of new hires by a factor that reflects the relative persistence in wages and productivity in ongoing jobs.

Since in this model the permanent wage equals the wage of new hires (since the wage in a given job never changes anymore after the time of hiring), the numerator of this ratio equals one. If productivity were a random walk, then $\bar{y}_t = y_t$ and the denominator would be one as well. In that case, the observed elasticity of the wage of newly hired workers would exactly reflect the elasticity of the permanent wage. If there is mean reversion in productivity, $d\log \bar{y}_t/d\log y_t$ is smaller than one, so that the observed elasticity provides a lower bound for the elasticity of the permanent wage. This result is consistent with Kudlyak (2007), who constructs an estimate for the permanent wage, which she calls the 'wage component of the user cost of labor', and finds that "the wage component of the user cost is more cyclical than the wages of newly hired workers, which in turn are more cyclical than the wages of all workers."

Equation (8) can also be used to explain why, in the flexible wage model, the response of the wage of new hires to changes in current productivity is close, but not exactly equal, to the response of the permanent wage to changes in permanent productivity. In that model, persistence in wages is equal to the persistence of the productivity process plus any additional persistence coming from the model dynamics. But since the search and matching model exhibits virtually no endogenous propagation, the ratio of the persistence of wages over productivity is very close to one.

The model with perfectly rigid wages in ongoing jobs slightly underpredicts the response of the wage of both workers in ongoing jobs (0.16) and new hires (0.65) to changes in productivity compared to our estimates (0.25 and 0.79 respectively). There are many reasons why wages in ongoing jobs would be less than perfectly rigid. One possibility would be to relax the assumption of full commitment and assume that wages in ongoing jobs are rebargained if but only if the wage hits the bounds of the bargaining set, as in an earlier version of Hall's (2005) paper. What is important for the argument here, is that we match the response of wages to productivity, assuming that wages are rigid *only* in ongoing jobs. As we argued in the introduction, this assumption is consistent with most micro-foundations for wage rigidity.

Wage rigidity in ongoing jobs does not affect job creation. The reason is that job creation, which is completely pinned down by equations (5) and (7), is affected only by the permanent wage. And rigidity of the wage in ongoing jobs does not imply any rigidity in the permanent wage. The intuition for this result is that equilibrium tightness is determined by those firms who have not yet found a worker and are deciding whether or not to post a vacancy. These firms are trading off payment of the search cost c with the expected future profits after hiring a worker. What matters for these profits, is the expected future wage payments to be made to the worker.

For comparison, we also present simulation results for a model with rigid wages at the start of a match. Here, we think of wage rigidity as countercyclical bargaining power of workers, as suggested by Shimer (2005). We model this in the simplest possible way, by making β depend negatively on the level of productivity, and calibrate the degree of rigidity to match the response of job creation to changes in productivity. Without any additional rigidity in wages of ongoing jobs, this model roughly matches the response of the wage of workers in ongoing jobs but implies a much lower response of the wage of newly hired workers than we find in the data.

4.4 The unemployment volatility puzzle

Wage rigidity in ongoing jobs, which is consistent with the wage data, does not affect job creation and therefore does not generate more volatility in unemployment. What are the implications of our results for the unemployment volatility puzzle more generally? A useful starting point is to calculate the response of the job finding rate to changes in labor productivity from the job creation equation (5). Assume the matching function is Cobb-Douglas with constant returns to scale and let μ denote the share parameter of the unemployment rate. Then, the response of the hiring rate $p(\theta_t) = \theta_t q(\theta_t) = \theta_t^{1-\mu}$ is given by

$$\frac{d \log p(\theta_t)}{d \log y_t} = \frac{1-\mu}{\mu} \left[\frac{\bar{y}_t}{\bar{y}_t - \bar{w}_t} - \frac{\bar{w}_t}{\bar{y}_t - \bar{w}_t} \frac{d \log \bar{w}_t}{d \log \bar{y}_t} \right] \tag{9}$$

Two things matter for the volatility of the job finding rate in response to productivity shocks: the elasticity of the permanent wage with respect to permanent productivity, and the size of permanent profits $\bar{y}_t - \bar{w}_t$. Our estimates indicate that the wage elasticity $d \log \bar{w}_t / d \log \bar{y}_t$ is close to one in the data.

There are two ways to interpret our results. First, one might conclude that wages must be perfectly flexible and so that the wage elasticity is virtually equal to one, as in Table 13. This interpretation is certainly consistent with our estimates. In this case, the response of the job finding rate to changes in productivity in (9) reduces to $(1-\mu)/\mu$. The only parameter that matters for fluctuations in job creation is the elasticity of the matching function. Petrongolo and Pissarides survey empirical estimates of μ and find that the share of unemployment in the matching function is no greater than 0.5. Thus, the response of $p(\theta_t)$ to changes in y_t predicted by the model, is at most 1. In the data, the ratio of the standard deviation of the job finding rate $p(\theta_t)$ over the standard deviation of labor productivity y_t is about 5.9. Thus, in this interpretation, the model cannot be calibrated to match the volatility of job creation. Since (9) was derived only from the job creation equation (5), which was derived without any assumptions on wage determination or workers' behavior, the only way to fix the model would be to change modeling of labor demand side of the market.

Our estimates are consistent with an alternative interpretation as well. A value for $d \log \bar{w}_t / d \log \bar{y}_t$ that is close to, but not equal to one, cannot be rejected based on our estimates. Thus, a moderate degree of wage rigidity, for example as implied by the bargaining setup in Hall and Milgrom (2008), may help generate more volatility in job creation. In this case, an alternative calibration may also contribute to solving the puzzle. By making profits a very small share of total match output, the response of the job finding rate to changes in productivity as in equation (9) can be made arbitrarily large. This is the intuition for why the small surplus calibration of Hagedorn and Manovskii (2008) generates large fluctuations in unemployment.

Finally, a generalization of the model that allows for endogenous job destruction could contribute to the volatility of unemployment, although the contribution to fluctuations in job creation -if any- is likely to be small. Fujita and Ramey (2008), in response to Shimer (2007), show that fluctuations in the separation rate may explain up to 50% of the volatility of unemployment. In our model, the separation rate is constant, so that fluctuations in unemployment are attributed entirely to fluctuations in the job finding rate by the following accounting identity.

$$u_{t+1} = u_t + \delta (1 - u_t) - p(\theta_t) u_t \tag{10}$$

Since exogenous fluctuations in the separation rate δ_t, imply a counterfactual positive correlation between unemployment and vacancies (see e.g. Shimer 2005), the most promising way to relax this assumption seems to be to endogenize job destruction, e.g. as in Mortensen and Pissarides (1994). This raises the question whether wage rigidity may affect job creation through its effect on job destruction, for example because worker and firm take into account the effect on the probability that their match will be destroyed when they bargain over the wage at the start of the match. We argue that this effect is likely to be small. First, it seems implausible on theoretical grounds that wage rigidity would affect job destruction, since the effect would imply inefficient destruction of matches, i.e. separations that could be avoided by re-bargaining the wage when necessary, see Hall (2005). Second, as shown by Mortensen and Nagypál (2007) and Pissarides (2007), while endogenous separations may have an important impact on unemployment fluctuations, this generalization of the model does not affect the dynamics of labor market tightness. Since in this paper, we focus on the dynamics of job creation, relaxing the assumption of an exogenous separation rate will not affect our results.

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 these wages of newly hired workers react one-to-one to productivity fluctuations, whereas wages of workers in ongoing job relationships react very little to changes in productivity. 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 similar to wages of job-to-job movers.

Our results point agaist rigidity in the wage of newly hired workers as an explanation for the volatility of unemployment over the business cycle as forwarded by Hall (2005), Gertler and Trigari (2006) and Blanchard and Galí (2008). However, a moderate degree of wage rigidity or alternative calibrations as in Hagedorn and Manovskii (2008) or Hall and Milgrom (2008) are within the confidence interval of our estimates. Finally, 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 year.

A Description of the data

We use wage data for individual workers in the CPS outgoing rotation groups from 1979 to 2006. We match these workers to the three preceding basic monthly datafiles in order to construct four months (one quarter) of employment history, which we use to identify newly hired workers.

A.1 Wages from the CPS outgoing rotation groups

We consider only wage and salary workers that are not self-employed and report non-zero earnings and hours worked. Both genders and all ages are included in our baseline sample. Our wage measure is hourly earnings (on the main job) for hourly workers and weekly earnings divided by usual weekly hours for weekly workers. For weekly workers who report that their hours vary (from 1994 onwards), we use hours worked last week. Top-coded weekly earnings are imputed assuming a log-normal cross-sectional distribution for earnings, following Schmitt (2003), who finds that this method better replicates aggregate wage series than multiplying by a fixed factor or imputing using different distributions. Notice that the imputation of top-coded earnings affects the mean, but not the median wage.

Outliers introduce extra sampling variation. Therefore, we apply mild trimming to the cross-sectional distribution of hours worked (lowest and highest 0.5 percentile) and hourly wages (0.3 percentiles). These values roughly correspond to USD 1 per hour and USD 100 per hour at constant 2002 dollars, the values recommended by Schmitt (2003). We prefer trimming by quantiles rather than absolute levels because (*i*) it is symmetric and therefore does not affect the median, (*ii*) it is not affected by real wage growth and (*iii*) it is not affected by increased wage dispersion over the sample period. We also check that our results are robust to using median wages, which are less affected by outliers.

We do not correct wages for overtime, tips and commissions, because (*i*) the relevant wage for our purposes is the wage paid by employers, which includes these secondary benefits, (*ii*) the data necessary to do this are not available over the whole sample period, and (*iii*) this correction has very little effect on the average wage (Schmitt 2003). We also do not exclude allocated earnings because (*i*) doing so might bias our estimate for the average wage and (*ii*) allocation flags are not available for all years and (*iii*) even if they are only about 25% of allocated observations are flagged as such (Hirsch and Schumacher 2004).

Mean and median wages in a given month are weighted by the appropriate sampling weights (the earnings weights for the outgoing rotation groups) and by hours worked, following Abraham et al. (1999) and Schmitt (2003). We explore robustness to the weights and confirm the finding of these papers that hours weighted series better replicate the aggregate wage. Average mean or median wages in a quarter are simple averages of the monthly mean or median wages. Consistent with the literature, we consider mean log wages rather than log mean wages.

In order to correct the business cycle statistics for the wage for sampling error (see Appendix B), we calculate standard errors for mean and median wages. Standard errors for the mean are simply the standard deviation of the wage divided by the square root of the number of observations. Medians are also asymptotically normal, but their variance is downward biased in small samples. Therefore, we bootstrap these standard errors.

We seasonally adjust our wage series by regressing the log wage on quarter dummies. Nominal wages are deflated by the implicit deflator for hourly earnings in the private non-farm business sector (chain-weighted) from the BLS productivity and costs program. Using different deflators affects the results very little, but decreases the correlation of our wage series with the aggregate wage.

Our baseline sample includes non-supervisory workers in the private non-farm business sector. This subsample of workers gives the best replication of the aggregate wage in terms of its correlation with hourly compensation from the establishment survey and in terms of its volatility, persistence and comovement with other variables.[23] We identify private sector workers using reported 'class of worker'. We construct an industry classification that is consistent over the whole sample period (building on the NBER consistent industry classification but extending it for data from 2003 onwards) and use it to identify farm workers. Similarly, we identify supervisory workers using reported occupation. Because of the change in the BLS occupation classification in 2003, there is a slight jump in the fraction of supervisory workers from 2002:IV to 2003:I. It is not possible to distinguish supervisory workers in agriculture or the military, so all workers in these sectors are excluded in the wage series for non-supervisory workers.

Finally, in order to control for composition bias because of heterogeneous workers (see section 2.2), we need additional worker characteristics to use in a Mincerian earnings regression. Dummies for females, blacks, hispanics and married workers (with spouse present) are, or can be made, consistent over the sample period. We construct a consistent education variable in five categories as well as an almost consistent measure for years of schooling following Jaeger (1997) and calculate potential experience as age minus years of schooling minus six.

A.2 Identifying newly hired workers

We match the individuals in the outgoing rotation groups to the three preceding basic monthly data files using the household identifier, household number (for multiple households on one address), person line number (for multiple wage earners in one household), month-in-sample and state. To identify mismatches, we use the s|r|a criterion proposed by Madrian and Lefgren (2000): a worker is flagged as a mismatch if gender or race changes between two subsequent months or if the difference in age is less than 0 or greater than 2 (to allow for some measurement error in the reported age). Madrian and Lefgren show that this criterion performs well in the trade-off between false matches and false mismatches. Within the set of measures that they find to perform well, s|r|a is the strictest. We choose a strict criterion because mismatches are more likely to be classified as newly hired workers (see below) and are therefore likely to affect our results substantially.

We can credibly match about 80% of workers in the outgoing rotation group to all three preceding monthly files. Because of changes in the sample design, we cannot match sufficiently many individuals to the preceding four months in the third and fourth quarter of 1985 and in the third and fourth quarter of 1995, so that the wage series for validly matched workers, job stayers and new hires have missing values in those quarters. In our regressions, we weight quarters by the variance of the estimate for the mean or median

[23] Detailed results for this replication exercise are available in a previous version of this paper (July 2007), available from our websites.

wage so that quarters with less than average number of observations automatically get less weight.

Including the outgoing rotation group itself, the matched data include four months employment history (employed, unemployed or not-in-the-labor-force), which we obtain from the BLS labor force status recode variable. We use this employment history to identify newly hired workers and workers in ongoing job relationships. New hires are defined as workers that were either unemployed or not in the labor force for any of the preceding three months. Job stayers are identified as workers that were employed for all four months. Notice that the two groups are not comprehensive for the group of all workers, because workers that cannot be matched to all preceding months can not always be classified.

B Correcting business cycle statistics for sampling error

We estimate wages for all workers, job stayers and new hires from an underlying micro-data survey. Therefore, our wage series are subject to sampling error. Given the way we construct these series, we know three things about the sampling error. First, because there is no overlap between individuals included in the outgoing rotation groups in two subsequent quarters, the sampling error is uncorrelated over time.[24] Second, because the sampling error in each period is the error associated with estimating a mean (or median), it is asymptotically normally distributed. Third, we have an estimate for the standard deviation of the sampling error in each quarter, which is given by the standard error of the mean (or median) wage in that quarter. Notice that taking first difference exacerbates the measurement error, increasing the standard deviation by a factor $\sqrt{2}$. Because of these three properties, and because the estimated standard errors are stable over time, we can treat the sampling error as classical measurement error, which is independent and identically distributed.

Let w_t denote an estimated wage series, $w_t = w_t^* + \varepsilon_t$, where w_t^* is the true wage and ε_t is the sampling error in the wage, which is uncorrelated over time and with w_t^* and has a known variance σ^2. The business cycle statistics we consider are the standard deviation of w_t^*, the autocorrelation of w_t^* and the correlation of w_t^* with x_t, an aggregate variable that is not subject to measurement error. These statistics can be calculated from the estimated wage series w_t and the estimated standard deviation of the sampling error σ as follows.

$$var\left(w_t\right) = var\left(w_t^*\right) + \sigma^2 \Rightarrow sd\left(w_t^*\right) = \sqrt{R} \cdot sd\left(w_t\right) \tag{11}$$

$$cov\left(w_t, w_{t-1}\right) = cov\left(w_t^*, w_{t-1}^*\right) \Rightarrow corr\left(w_t^*, w_{t-1}^*\right) = \frac{corr\left(w_t, w_{t-1}\right)}{R} \tag{12}$$

$$cov\left(w_t, x_t\right) = cov\left(w_t^*, x_t\right) \Rightarrow corr\left(w_t^*, x_t\right) = \frac{corr\left(w_t, x_t\right)}{\sqrt{R}} \tag{13}$$

[24] Individuals in the CPS are interviewed four months in a row, the last one of which is an outgoing rotation group, then leave the sample for eight months, after which they are interviewed another four months, the last one of which is again an outgoing rotation group. Therefore, about half of the sample in quarter t (individuals in rotation group 8) is also included in the sample in quarter $t-4$ (when they were in rotation group 4) and the other half is included in the sample in quarter $t+4$. Thus, the sampling error may be correlated with a four quarter lag, but not between subsequent quarters. We ignore this correlation structure and treat the sampling error as uncorrelated over time.

where $R = \left(var\left(w_t\right) - \sigma^2\right)/var\left(w_t\right) \in (0,1)$ is the fraction of signal in the variance of w_t. Unless explicitly specified, we use the correction factors $\sqrt{R}$, $1/R$ and $1/\sqrt{R}$ for all reported business cycle statistics. This bias correction is small for the wages of all workers and job stayers, because sample sizes are large and therefore σ^2 is small, but substantial for the wage of new hires. Notice that the bias correction decreases the reported standard deviations towards zero but increases the reported autocovariances and correlation coefficients away from zero. For bandpass filtered series no correction is necessary because the filter removes the high-frequency fluctuations due to measurement error from the data. Regression coefficients for the wage on labor productivity are not biased in the presence of classical measurement error in the dependent variable so no correction is necessary.

C Details about the model in section 4

C.1 Derivation of the job creation equation

Free entry drives the value of a vacancy to zero, which implies that the period cost c must equal the probability that the vacancy transforms in a match times the expected value of that match.

$$c = q\left(\theta_t\right) E_t J_{t+1} \tag{14}$$

The value to the firm of having a filled job J_t, is given by the following Bellman equation.

$$(1+r)J_t = y_t - w_t + (1-\delta)E_t J_{t+1}. \tag{15}$$

Solving equation (15) forward gives an expression for the value of a filled job.

$$E_t J_{t+1} = \frac{\bar{y}_t - \bar{w}_t}{r+\delta} \tag{16}$$

Substituting (16) into (14) gives the job creation equation in the main text.

C.2 Derivation of the wage equation

The derivation of the wage curve (Equation 7) follows Pissarides (2000, section 1.4). Here the steps are slightly different because we consider a stochastic version of the search model. First of all, it is convenient to note, that Nash bargaining implies

$$W_{t+1} - U_{t+1} = \frac{\beta}{1-\beta} J_{t+1} \tag{17}$$

To derive the wage equation, start from the Bellman equation for the value of being unemployed.

$$(1+r)\,U_t = b + \theta_t q\left(\theta_t\right) E_t W_{t+1} + \left(\left(1 - \theta_t q\left(\theta_t\right)\right) E_t U_{t+1}\right. \tag{18}$$

Rearrange to obtain:

$$E_t U_{t+1} - (1+r)U_t = -b - \theta_t q\left(\theta_t\right) E_t\left(W_{t+1} - E_t U_{t+1}\right) \tag{19}$$

Now use (17) to replace the worker surplus on the right-hand side of the equation. Then use (14) to replace the value of a job.

$$E_tU_{t+1} - (1+r)\,U_t = -b - \theta_t q(\theta_t)\left(\frac{\beta}{1-\beta}\frac{c}{q(\theta_t)}\right) = -b - \frac{\beta}{1-\beta}c\theta_t \tag{20}$$

This equation will be useful momentarily.

Next consider the Bellman equation for having a job.

$$(1+r)W_t = w_t + (1-\delta)E_tW_{t+1} + \delta E_tU_{t+1} \tag{21}$$

Subtract $(1+r)\,U_t$ from both sides to obtain:

$$(1+r)(W_t - U_t) = w_t + (1-\delta)\,E_t(W_{t+1} - U_{t+1}) + E_tU_{t+1} - (1+r)\,U_t \tag{22}$$

Now replace the last two terms using (20) and rearrange,

$$(1-\delta)\,E_t(W_{t+1} - U_{t+1}) - (1+r)(W_t - U_t) = -w_t + b + \frac{\beta}{1-\beta}c\theta_t \tag{23}$$

solve forward and substitute the definitions of $\bar{w}_t$ and $\bar{\theta}_t$, to get an expression for the worker's surplus of being in a match.

$$(r+\delta)\,E_t(W_{t+1} - U_{t+1}) = \bar{w}_t - b - \frac{\beta}{1-\beta}c\bar{\theta}_t \tag{24}$$

Again using Nash bargaining (17) and eliminating J_{t+1} using (16), we get,

$$\frac{\beta}{1-\beta}(\bar{y}_t - \bar{w}_t) = \bar{w}_t - b - \frac{\beta}{1-\beta}c\bar{\theta}_t \tag{25}$$

which after solving for $\bar{w}_t$ gives equation (7) in the main text.

C.3 Numerical solution and simulations

Because these more general models can no longer be solved analytically, we simulate them. We assume (as in Shimer 2005), that labor productivity follows an AR(1) type process, bounded below by the flow utility of unemployment.

$$y_t = b + e^{z_t}(1-b) \tag{26}$$

$$z_t = \rho z_{t-1} + \varepsilon_t \tag{27}$$

where productivity shocks are normally distributed, $\varepsilon_t \sim N(0, \sigma^2)$. Our calibration of the model parameters is identical to Shimer (2005). As an alternative we present results for a small surplus calibration in the spirit of Hagedorn and Manovskii (2008). The vacancy posting cost is chosen to yield steady state tightness of unity. We simulate the model at a weekly frequency and aggregate to quarterly observations. The reported elasticities are averages over 1000 simulations of length 89.

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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 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 productivity	-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 our 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: Robustness to alternative estimators

	Wage per hour		Earnings per person	
WLS	All workers	New hires	All workers	New hires
Elasticity wrt productivity	0.25	0.79	0.36	0.86
Std. error	0.14	0.40	0.17	0.50
Median	All workers	New hires	All workers	New hires
Elasticity wrt productivity	0.13	0.89	0.15	0.56
Std. error	0.20	0.45	0.24	0.70
Median, WLS	All workers	New hires	All workers	New hires
Elasticity wrt productivity	0.11	0.89	-0.05	0.57
Std. error	0.24	0.49	0.22	0.72

Elasticities are estimated using the two-step method described in the text. WLS weights the second step regression by the inverse of the variance of the first step estimates. Median uses the median wages instead of mean wages by quarter.

Table 6: Robustness to alternative sample selection criteria

	Wage per hour		Earnings per person	
Including supervisory workers	All workers	New hires	All workers	New hires
Elasticity wrt productivity	0.10	0.57	0.39	0.70
Std. error	0.13	0.40	0.18	0.49
Including public sector	All workers	New hires	All workers	New hires
Elasticity wrt productivity	0.06	0.70	0.33	0.57
Std. error	0.12	0.48	0.15	0.54
New hires out of unemployment	All workers	New hires	All workers	New hires
Elasticity wrt productivity	0.24	0.77	0.37	0.69
Std. error	0.14	0.55	0.17	0.70

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.

Table 7: 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 8: 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 9: 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 10: 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. In order to be consistent with the other estimates in this paper, job stayers include job-to-job movers.

Table 11: 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.

Table 12: Simulated elasticities for the flexible wage model

b	β	$\frac{d\log\bar{w}}{d\log\bar{y}}$	$\frac{d\log w}{d\log\bar{y}}$	$\frac{d\log w}{d\log y}$	$\frac{d\log\bar{w}}{d\log w}$	$\frac{d\log\bar{y}}{d\log y}$	$\frac{d\log\theta}{d\log y}$	$\frac{\sigma_\theta}{\sigma_y}$	$\frac{\sigma_u}{\sigma_y}$
0.200	0.050	0.727	1.126	0.732	0.646	0.650	1.171	1.171	0.240
0.200	0.100	0.843	1.300	0.845	0.648	0.650	1.191	1.192	0.243
0.200	0.300	0.951	1.464	0.951	0.649	0.650	1.221	1.221	0.250
0.200	0.500	0.978	1.505	0.978	0.650	0.650	1.231	1.231	0.251
0.200	0.700	0.990	1.524	0.990	0.650	0.650	1.236	1.236	0.252
0.200	0.900	0.997	1.535	0.997	0.650	0.650	1.239	1.239	0.254
0.400	0.050	0.592	0.920	0.598	0.643	0.650	1.561	1.561	0.319
0.400	0.100	0.751	1.160	0.754	0.647	0.650	1.588	1.588	0.324
0.400	0.300	0.919	1.415	0.920	0.649	0.650	1.627	1.627	0.333
0.400	0.500	0.963	1.483	0.964	0.650	0.650	1.641	1.642	0.335
0.400	**0.700**	**0.984**	**1.514**	**0.984**	**0.650**	**0.650**	**1.647**	**1.647**	**0.338**
0.400	0.900	0.996	1.532	0.996	0.650	0.650	1.652	1.653	0.338
0.600	0.050	0.499	0.777	0.505	0.642	0.650	2.341	2.342	0.479
0.600	0.100	0.677	1.047	0.680	0.646	0.650	2.381	2.381	0.486
0.600	0.300	0.889	1.369	0.890	0.649	0.650	2.443	2.444	0.499
0.600	0.500	0.949	1.461	0.949	0.650	0.650	2.462	2.463	0.503
0.600	0.700	0.977	1.504	0.978	0.650	0.650	2.471	2.472	0.505
0.600	0.900	0.994	1.530	0.994	0.650	0.650	2.478	2.478	0.507
0.800	0.050	0.431	0.672	0.437	0.641	0.650	4.684	4.686	0.957
0.800	0.100	0.616	0.954	0.620	0.646	0.650	4.761	4.763	0.975
0.800	0.300	0.861	1.327	0.862	0.649	0.650	4.878	4.880	0.998
0.800	0.500	0.935	1.440	0.936	0.649	0.650	4.921	4.923	1.007
0.800	0.700	0.971	1.494	0.971	0.650	0.650	4.945	4.948	1.011
0.800	0.900	0.992	1.527	0.992	0.650	0.650	4.949	4.951	1.013
0.980	**0.050**	**0.384**	**0.600**	**0.390**	**0.640**	**0.650**	**46.749**	**46.782**	**9.542**
0.980	0.100	0.570	0.884	0.574	0.645	0.649	47.518	47.554	9.721
0.980	0.300	0.837	1.291	0.839	0.648	0.650	48.772	48.811	9.979
0.980	0.500	0.923	1.422	0.924	0.649	0.649	49.103	49.144	10.046
0.980	0.700	0.966	1.487	0.966	0.649	0.650	49.352	49.392	10.107
0.980	0.900	0.991	1.525	0.991	0.650	0.650	49.486	49.528	10.129

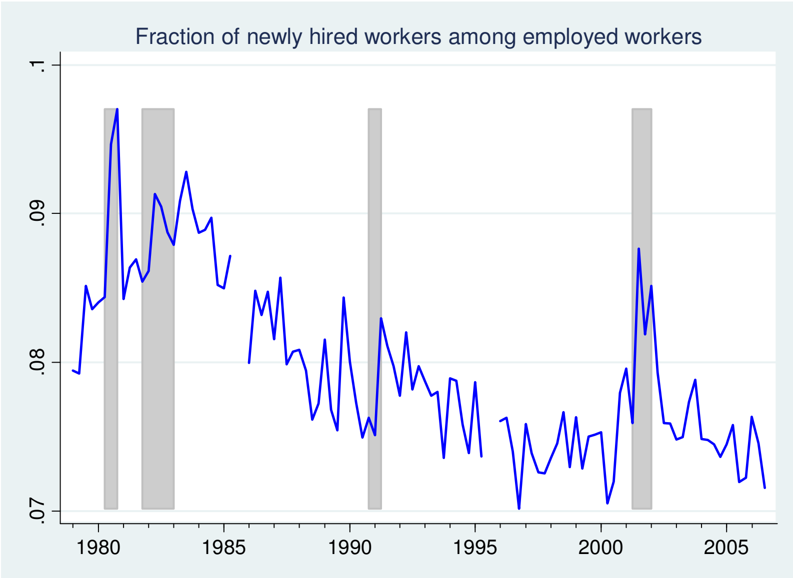
The table reports simulated elasticities for different calibrations of the model. We vary the flow value of unemployment b and workers' bargaining power β. Other parameters are calibrated as in Shimer (2005). The reported elasticities are averages of 1000 simulations of 89 quarters. All simulated data are in log first differences. The model is simulated at weekly frequency and aggregated to quarterly data before computing the statistics. In bold face the calibrations of Shimer (2005) and Hagedorn and Manovskii (2008). w is the average wage in the model, y is average productivity, $\bar{w}$ and $\bar{y}$ are the permanent values of these variables (see section 4.2), θ is labor market tightness and σ_x denotes the standard deviation of variable x.

Table 13: Simulation results for rigid wage models

Model	$\frac{d\log \bar{w}}{d\log \bar{y}}$	$\frac{d\log w^n}{d\log y}$	$\frac{d\log w^s}{d\log y}$	$\frac{d\log w^a}{d\log y}$	$\frac{d\log \theta}{d\log y}$	$\frac{\sigma_u}{\sigma_y}$
Shimer, AER calibration	0.985	0.986	0.986	0.986	1.646	0.413
Small surplus calibration	0.384	0.389	0.389	0.389	46.516	11.706
Countercyclical bargaining power	0.601	0.228	0.228	0.228	24.028	6.002
On-the-job wage rigidity	0.985	0.648	0.159	0.163	1.646	0.413

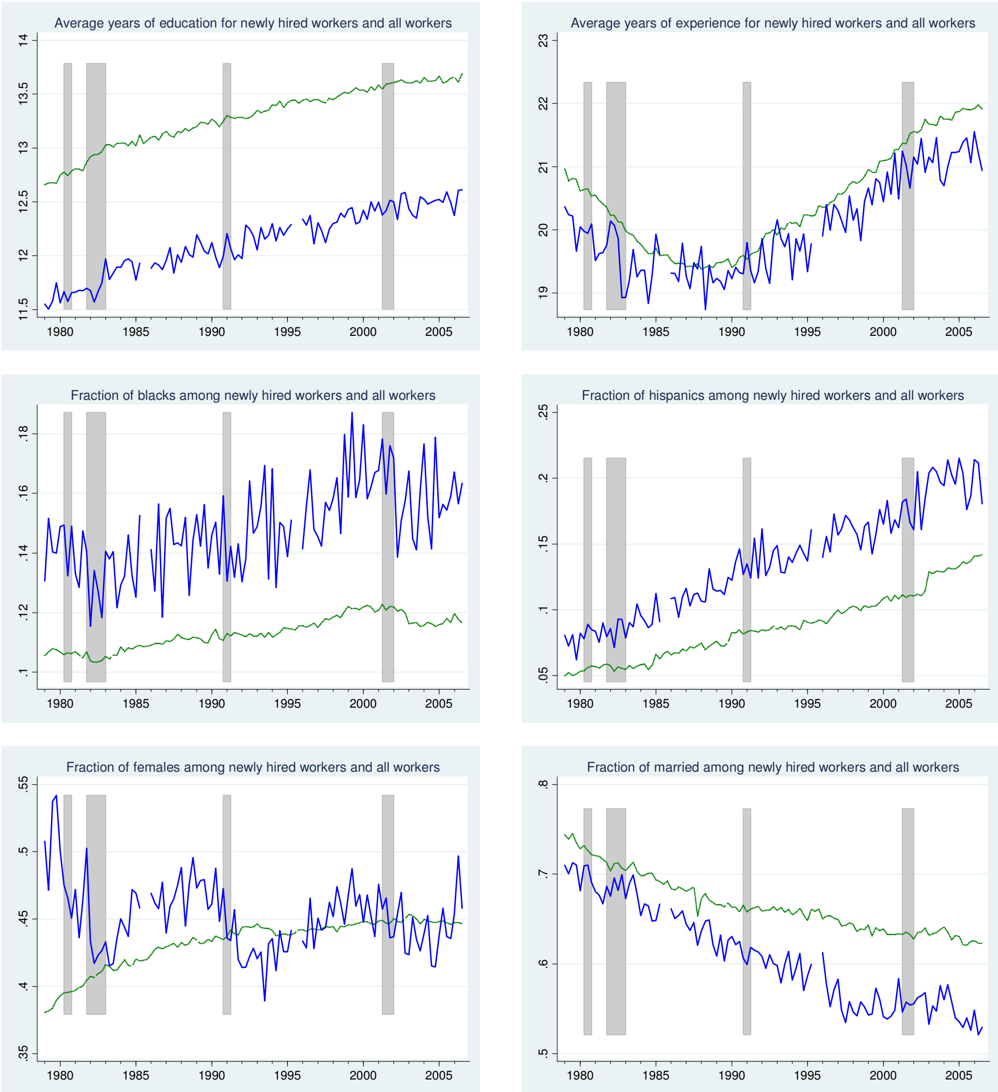
The table reports simulated elasticities for different models, varying the type of wage rigidity. Parameters are calibrated as in Shimer (2005), except for the small surplus calibration where the flow utility of unemployment is 0.98 of per period productivity and the worker bargaining power is 0.05. In each simulation the vacancy posting cost is chosen to normalize steady state labor market tightness to unity. The reported elasticities are averages of 1000 simulations of 89 quarters. All simulated data are in log first differences. The models are simulated at weekly frequency and aggregated to quarterly data before computing the statistics.

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 Appendix A. The grey areas indicate NBER recessions.

Figure 2: Characteristics of newly hired workers over time



The green dotted line is the average for all workers and the blue solid line for new hires. Education coding changes in 1992. In order not to loose that observation, we regressed the average education level in the sample on a third order polynomial in time and a post 1992 dummy and took the residuals, adding back up the polynomial but not the dummy to correct the resulting level shift. 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 Appendix A. The grey areas indicate NBER recessions.

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