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ABSTRACT

Wage Rigidity and Job Creation*

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

JEL Classification: E24, E32, J31, J41 and J64
Keywords: business cycle, search and matching models and wage rigidity

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1 Introduction

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

Sticky wage setting seems to be supported by the observation that wages are less volatile than most business cycle models predict. However, the volatility of the aggregate wage is neither a sufficient nor a particularly informative statistic to measure the kind of wage rigidity that is required to amplify unemployment fluctuations. In a frictional labor market, job creation is a forward-looking decision and the amount of jobs that are created depends on the expected net present value of wages over the entire duration of the newly created jobs (Boldrin and Horvath 1995, Shimer 2004, Pissarides 2009, Kudlyak 2009). Under long-term wage contracting, the cyclical behavior of this present value may be very different from the cyclical behavior of the aggregate wage.[2] In this paper, we explore whether there is any evidence for rigidity in the present value of wages of newly hired workers.

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

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

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

[2] It is not important for this paper whether the long-term wage contracts are explicit, as in Thomas and Worrall (1988) or Macleod and Malcomsom (1993), or implicit as in Beaudry and DiNardo (1991).

with such contracts under limited commitment on the part of both worker and firm.[3]

What do our findings imply for the unemployment volatility puzzle? Long-term wage contracts with a very cyclical starting wage generate strong cyclicality in the expected net present value of wages as well. In that sense, we find very little evidence for wage rigidity in the data. However, we also show that very little wage rigidity is needed to match the observed response of job creation to changes in productivity. Thus, while our estimates rule out explanations of the unemployment volatility puzzle that rely on a high degree of stickiness in the net present value of wages, they are consistent with a moderate degree of wage stickiness, like the bargaining setup in Hall and Milgrom (2008), which reduces the influence of the value of unemployment on the outcome of the wage bargain, as well as with calibrations of a model with flexible wage setting that generates some wage rigidity through low bargaining power of workers, as in Hagedorn and Manovskii (2008).

Previous empirical studies of wage rigidity by macroeconomists have been concerned with *aggregate* wages (Dunlop 1938, Tarshis 1939, Cooley 1995). If the importance of wages of new hires has been recognized at all, then a careful empirical study has been considered infeasible because of lack of data.[4] Labor economists who have studied wages at the micro-level have mostly been concerned with wage changes of individual employees (Bils 1985). Thus, the analysis has naturally been restricted to wages in *ongoing* employment relationships, which have been found to be strongly rigid. Notable exceptions are Devereux and Hart (2006) and Barlevy (2001) who study job changers and find their wages to be much more flexible than wages of workers in ongoing jobs.

The main difference between these studies and ours, is that we focus on newly hired workers, i.e. workers coming from non-employment, which is the relevant wage series for comparison to standard search models, rather than job changers.[5] Since wages of non-employed workers are not observed, we need to use a different estimation procedure, which does not require individual-level panel data. Our procedure has the additional advantage that we can use the CPS, which gives us a much larger number of observations than the earlier studies, which use the PSID or NLSY datasets.[6]

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

[4] 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).

[5] 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 the cyclicality of their wages.

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

Like previous research, we find strong evidence for cyclical shifts in the composition of employed workers. Solon, Barsky and Parker (1994) show that failing to control for (potentially unobservable) heterogeneity across workers leads to a substantial downward bias in the cyclicality of wages. We document the cyclical patterns in the differences between new hires and the average worker in demographics, experience and particularly in the schooling level that cause this bias. Controlling for fluctuations in the skill level of the workforce is particularly important for our purposes since we study newly hired workers and at least some of the composition bias is likely to be driven by selection in the hiring process. This constitutes a potential weakness of our approach, because we cannot take individual-specific first differences and thus cannot control for unobservable components of skill as Solon, Barsky and Parker do. However, we use the PSID to demonstrate that controlling for observable skill is sufficient to control for composition bias. While unobservable components of skill might be important, they seem to be sufficiently strongly correlated with education to be captured by our controls.[7]

The two studies most closely related to ours are Pissarides (2009) and Kudlyak (2009). Both of these papers argue, like we do, that wage stickiness in old matches does not matter for job creation as long as the net present value of wages for newly created matches responds to changes in aggregate conditions. Pissarides (2009) surveys the empirical literature on the cyclicality on wages discussed briefly above and concludes that the evidence is not consistent with explanations for the unemployment volatility puzzle that are based on wage stickiness. Kudlyak (2009), like this paper, aims to provide direct evidence on the cyclicality of the net present value of wages in new matches, which she calls the wage component of the user cost of labor. Kudlyak uses panel data from the NLSY and, as a result, there are methodological differences between her paper and ours, see Section 4.3 for a discussion. Despite these differences, the estimates in Kudlyak's paper and in ours are very similar.

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

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of the pay of newly hired workers.

[7] In addition, one may be worried about job heterogeneity. If the average job that is filled in a boom is of higher quality than in a recession, the wage of new hires may look more cyclical than the average wage for an occupation. One could argue, however, that for job creation it is irrelevant whether the wage of new hires is cyclical because the wage for each occupation changes or because there are cyclical shifts in the composition of occupations. To control for job heterogeneity and worker heterogeneity simultaneously, one needs matched employer-employee data. Carneiro, Guimarães and Portugal (2012) use such data for Portugal 1986-2005 and find that, controlling for composition bias due to both sources, entry wages are much more procyclical than wages in ongoing jobs, consistent with our results.

2 Data

A commonly held view in the macro literature is that no data are available to test the hypothesis that the wage of new hires might be much more flexible than the aggregate wage (Bewley 1999, Hall 2005). Some anecdotal evidence seems to point against it.[8] 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 Current Population Survey (CPS) outgoing rotation groups (BLS 2000), a survey that has been administered every month since 1979, allowing us to construct quarterly wage series for the period 1979–2006.[9] In most of the paper we focus on the period after the Great-Moderation, 1984–2006. Wages are hourly earnings (weekly earnings divided by usual weekly hours for weekly workers) corrected for top-coding and outliers and deflated using the deflator for aggregate compensation in the private non-farm business sector.

We match workers in our survey to the same individuals in three preceding basic monthly datafiles. This allows us to identify newly hired workers as those workers that were not employed for at least one of the three months before we observe their wage.[10] In addition, we have information on worker characteristics (gender, age, education, race, ethnicity and marital status), industry and occupation.

[8] 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.

[9] 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.

[10] 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.

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 B.

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

Table 1 reports summary statistics for some observable characteristics of all workers and of new hires (the evolution of some of these characteristics over time may be found in Figure 2 in Appendix E). Clearly, newly hired workers are not representative for the labor force. New hires are slightly more likely to be female,[12] and much more likely to be African-American or hispanic. They are also slightly younger and therefore have less labor market experience.[13] Finally, new hires have a year less schooling than the average for all workers. It is not surprising therefore, that new hires on average earn much lower wages. These numbers suggest that workers with lower wages also tend to work in higher turnover jobs, which makes them more likely to have recently started a new job in any given quarter.

2.2 Construction of the wage index

Workers are heterogeneous and newly hired workers are not a representative subsample of the labor force. If the composition of newly hired workers varies over the business cycle, then this heterogeneity will bias our estimate of wage cyclicality. Solon, Barsky

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

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

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

and Parker (1994) show that this composition bias is substantial and that failing to control for changes in the composition of employed workers over the cycle makes wages seem less cyclical than they really are.

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

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

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

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

To obtain composition-bias corrected wages, we regress log wages on observable worker characteristics and take the residuals. Since we are interested in the comovement of wages with aggregate labor market conditions, we then aggregate by averaging these residuals by quarter for different subgroups of workers (e.g newly hired workers or workers in ongoing jobs).[14] Thus, the wage index for subgroup j, $\hat{w}_{jt}$, relates to the average wage of that group of workers, w_{jt}, as follows,

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

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

2.3 Volatility of wages

Table 2 presents standard statistics for the volatility and persistence of various wage series. We present these statistics for detrended data using the bandpass filter and the

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

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 C.

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

3 Response of wages to productivity

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

3.1 Estimation

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

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

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

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

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

where w_{ijt} is the uncorrected wage of individual i, belonging to subgroup j, at time t, as in (1). However, since the wage last quarter is unobserved for newly hired workers (because they were not employed then), this approach is not feasible for our purpose.

Therefore, we implement our procedure as a two-step estimator and estimate (3) from aggregate wage series.

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

The first column of Table 3 replicates Devereux's (2001) estimate of the response of the wage of workers in ongoing relationships to changes in the unemployment rate.[16] This response is estimated as in Devereux, from equation (4) using a two-step procedure. First, we take first differences for the wage of individual workers and average those by year. In the second step, we regress the annual averages of the change in the wage on the first difference of the unemployment rate.[17] The second column presents the same elasticity, estimated directly from the micro-data in a 1-step procedure, clustering the standard errors by year. As expected, this leaves both the point estimate and the standard error virtually unaltered.

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

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

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

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

3.2 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 individual and 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 one-to-one to changes in labor productivity, with an elasticity of 0.79 in our baseline estimates. The point estimates are never significantly different from one and often significantly different from zero.

If hours per worker cannot be freely adjusted, one may argue that output per person and earnings per person provide better measures of wages and labor productivity. Results for these measures are also presented in Table 4 and provide a very similar picture as the hourly data. The results are also similar or even strengthened if we use median instead of mean wages or if we weight the regression by the inverse of the variance of the first step estimates to obtain the efficient second step estimator and to different sample selection criteria for constructing average wages from the CPS, see Tables 11 and 12 in Appendix E.

3.2.1 Composition bias

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

We find that education is 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 6 we explore how this choice affects our results. The response of wages to productivity is substantially higher for men, although the difference is never significant. The differences are particularly large for newly hired workers. Thus, focusing on male workers only would further strengthen our evidence that wages of new hires are flexible.

Table 6 also presents some estimates including workers from a larger age range in the sample. In our baseline results, we focus on workers between 25 and 60 years old in order to exclude workers on their first job as well as workers close to retirement. Particularly excluding the young workers is important for our result. Adding workers between 20 and 25 years old to the sample, the elasticity of the wage of new hires decreases substantially, although not significantly. The result seems more robust to including older workers between 60 and 65 years old, with the elasticity remaining virtually unaltered. We argue that the behavior of both young and old workers is not described well by a simple model of labor supply and the correct comparison between model and data is to limit the analysis to workers that are in the middle of their career. To make sure we have set our age limits 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. If the production function is Cobb Douglas, the average and marginal product of labor are proportional to each other and output per hour is the appropriate measure of productivity to calculate elasticities (Hall 2007). For our purposes, it is irrelevant what drives changes in productivity. The estimates have the same interpretation for any shock that does not affect wages directly, but only through changes in productivity. However, if labor productivity is endogenous, then the causal interpretation of the effect of productivity on wages is lost.

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

[18]Suppose production requires capital and labor and is of the Cobb-Douglas form with diminishing returns to total hours, $Y_t = A_t K_t^{\alpha} L_t^{1-\alpha}$, where A_t is total factor productivity, K_t is capital and L_t is total hours. Log total factor productivity equals $\log A_t = \log Y_t - \alpha \log K_t - (1-\alpha)\log L_t$, whereas log labor productivity is given by $\log y_t = \log Y_t - \log L_t = \log A_t + \alpha \log K_t - \alpha \log L_t$. This illustrates the problem

quarterly version of the Basu, Fernald and Kimball (2006) series, constructed by Fernald (2007).

Since total factor productivity is arguably an exogenous source of fluctuations in labor productivity, we use these measure of TFP to instrument output per hour in our regressions. The results are presented in Table 7. For all instruments, our results become stronger and the elasticity of the wage of newly hired workers is now 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 a standard search and matching model to. However, as argued by Pissarides (2009), job changers, although not strictly comparable to a model without on-the-job search, may also be informative about wage flexibility of new hires. Some previous studies explored the cyclicality of wages of this group of workers (Bils 1985, Devereux and Hart 2006, Barlevy 2001, see also Pissarides 2009 for a survey of these and other papers).

To compare our results to those studies, we replicate and extend some of the results in Devereux (2001). Using annual panel data from the PSID, 1970-1991, Devereux finds an elasticity of the wage of all workers to changes in the unemployment rate of about -1 and for job stayers of about -0.8. These estimates are replicated in Table 8. Devereux does not report the cyclicality of job changers, but this elasticity can readily be estimated using his data and is also reported in the Table.[19] With an elasticity of -2.4, the wages of job changers are much more cyclical than those of all workers.

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

Finally, we check whether there might be systematic differences between the PSID and the CPS by estimating the cyclicality in the wage of job changers from our CPS data. After 1994, the CPS asks respondents whether they still work in the same job

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

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

[20] 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.

as at the time of the last interview one month earlier. We use this question to identify job changers and find the estimates in the bottom panel of Table 8. Since we can only use data since 1994, the standard errors of these estimates are very large. The point estimates however, are well in line with the estimates from the PSID.

3.4 Great moderation and pre-1984 wage rigidity

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

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

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

4 Implications for models of wage setting and job creation

What kind of models of wage setting and labor market fluctuations are consistent with the observed behavior of wages? First of all, our estimates provide evidence for long-term wage contracts, e.g. as in Rudanko (2009). The difference in the response of wages of workers in ongoing matches versus newly hired workers to changes in productivity

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

indicates stickiness in the wage over the duration of the relation between worker and firm. Further, to make it possible to implement these long-term wage contracts, it must be that the labor market is subject to frictions.[22] In a frictionless labor market, workers can be costlessly replaced so that each worker is 'marginal' and differences in the wage of newly hired workers and workers in ongoing jobs cannot be sustained as an equilibrium (Barro 1977).

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 wage payments over the duration of a match, but the path at which wages are paid out is irrelevant for job creation as long as the wage remains within the bargaining set and does not violate the worker's or firm's participation constraint (MacLeod and Malcomson 1993, Hall 2005). 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). In Section 4.3, we explore what our estimates imply for the cyclicality of the present value of wages and job creation.

4.1 Evidence for long-term wage contracting

It is tempting to interpret our estimates for the cyclicality of the wages of newly hired workers and workers in ongoing matches as the cyclicality of wages at the start and over the duration of individual wage contracts. This interpretation would be incorrect however, because of compositional changes in our dataset. The pool of new hires in a given quarter does not include the same workers as new hires in the quarter before. And the pool of workers in ongoing matches includes workers that were newly hired only last quarter as well as workers that have been in their current job for a long time. Nevertheless, our estimates are of course informative about the cyclicality of individual wage contracts. Here, we formalize that link.

The wage w_{it}^{a} of a worker i in a match of age a at time t consists of four components: the initial wage this worker received at the time of hiring $w_{i,t-a}^{0}$, wage growth with job tenure, revisions to the wage in response to changes in aggregate economic conditions, and changes in the wage because of idiosyncratic circumstances. For simplicity, we assume the functional form of the wage contract is log-linear, like our estimation equation (3), so that the wage is given by,

$$\log w_{it}^{a} = \log w_{i,t-1}^{a-1} + \phi_{0,\text{stay}} + \phi_{1,\text{stay}} \left(\log y_t - \log y_{t-1}\right) + v_{it} \tag{5}$$

where $\phi_{0,\text{stay}}$ is average wage growth per period of tenure, $\phi_{1,\text{stay}}$ is the response of the wages in ongoing matches to aggregate productivity, and v_{it} is idiosyncratic wage growth, which averages zero over the cross-section in each period. The question is what

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

values for $\phi_{0,\text{stay}}$ and $\phi_{1,\text{stay}}$ are consistent with our estimates.

We simulate wages using wage contract (5) for 1.5 million workers over 158 periods, dropping the first 70 periods to initialize the wage distribution so that our sample of simulated data, like the actual data, consists of 88 quarters. In these simulations, we assume the idiosyncratic component of wage growth v_{it} is normally distributed, so that cross-sectional distribution of wages is log normal, although this assumption does not matter for the result because invididual heterogeneity is averaged out. In order to replicate the compositional changes in the actual data, we also need to model when contracts start and end. To this end, we match the number of separations and new hires in each period. Notice that this strategy yields an employment rate that is consistent with the data as well. Finally, we assume stochastic processes for productivity and wages of new hires so that we can forecast both variables to compute expected values and backcast wages in order to initialize the wage distribution. We assume wages of new hires depend log-linearly on productivity, $\log w_{it}^0 = \phi_{0,\text{newh}} + \phi_{1,\text{newh}} \log y_t + v_{it}^0$, setting $\phi_{0,\text{newh}}$ and $\phi_{1,\text{newh}}$ to match the average wage of newly hired workers and the elasticity of the wage of new hires with respect to productivity. For productivity we assume a simple ARIMA$(1,1,0)$ process, $\log y_t = \log y_{t-1} + \psi_0 + \psi_1 (\log y_{t-1} - \log y_t) + v_t$, where ψ_0 and ψ_1 are estimated directly from the data.[23] Then, we vary $\phi_{0,\text{stay}}$ and $\phi_{1,\text{stay}}$ so that average wage growth α_{allw} and the elasticity of wages with respect to productivity η_{allw}, as in equation (3), estimated from the simulated data are the same as in the actual data. Details of this simulation exercise are in Appendix D.1.

Table 10 shows the results of the simulations for different values of the cyclicality of the wage of new hires $\phi_{1,\text{newh}}$ and the cyclicality of the contract wage $\phi_{1,\text{stay}}$. As expected, a wage contract like (5) drives a wedge between the cyclicality of wages of new hires and all workers, the former responding more to changes in productivity than the latter if $\phi_{1,\text{stay}} < \phi_{1,\text{newh}}$. The measured elasticity of wages of new hires with respect to productivity by construction equals $\phi_{1,\text{newh}}$. The measured elasticity of wages of all workers with respect to productivity increases with the contract elasticity $\phi_{1,\text{stay}}$, but there is a substantial difference. The reason for this difference is that the group of job stayers changes over time: this period's job stayers include last period's new hires. The larger is the difference between the cyclicality of the wage in ongoing matches $\phi_{1,\text{stay}}$ and at the start of a job $\phi_{1,\text{newh}}$, the larger is the gap between the measured elasticity for all workers and the contract elasticity for job stayers.[24] The implied wage contract that matches our estimates in Table 4 has an average wage growth with tenure of 2% per

[23] Within the class of ARIMA$(p,1,q)$ processes, the ARIMA$(1,1,0)$ specification fits the data best according to the Bayesian Information Criterion. Moreover, the estimate for ψ_1 is small, so that productivity is close to a random walk. As a robustness check, we repeat the exercise with actual data for the wages of new hires and productivity for the period these data are available, using simulated data only for the backcasting, and find the results are very similar.

[24] If $\phi_{1,\text{newh}}$ is smaller or not much larger than $\phi_{1,\text{stay}}$ then the estimated elasticity for all workers can be smaller than the contract elasticity for job stayers because of the exogenous wage growth in wages in continuing job relationships. This pattern disappears when we set $\phi_{0,\text{stay}} = 0$.

year, $\phi_{0,\text{stay}} = 0.02$, and an elasticity of the wage with respect to aggregate productivity of $\phi_{1,\text{stay}} = 0.25$.

How do these estimates compare to the type of wage contracts that have been used in the literature? A micro-founded theory of long-term wage contracting is provided by Rudanko (2009). In Rudanko's model, wages in ongoing matches are rigid because risk-neutral firms use long-term wage contracts to insure risk-averse workers. The amount of wage rigidity generated this way is limited by the participation constraints of firms and workers. If both the worker and the firm can commit to staying in the match, even if their reservation wage falls below or rises above the rigid wage, then a constant wage is feasible and optimal, i.e. $\phi_{1,\text{stay}} = 0$ in our notation. If the worker may walk out but the firm can commit to retaining the worker (one-sided commitment), then the wage needs to be more responsive to changes in productivity in order to prevent the worker from leaving, and if neither worker nor firm can commit (two-sided limited commitment) the contract wage needs to be even more cyclical. The elasticity of the average wage with respect to productivity generated by this model is consistent with our estimates if the replacement ratio is around 0.95 under one-sided commitment or around 0.7 under two-sided limited commitment (Rudanko 2009, Figure 4). Reiter shows that, with a replacement ratio of 0.7, the model with long-term wage contracting (under two-sided limited commitment) also correctly predicts the difference in the cyclicality of wages of new hires versus average wages of all workers (Reiter 2007, Table 5).[25] Since the true replacement ratio is probably close to 0.7 (Mortensen and Nagypal 2007), we conclude that our estimates support long-term wage contracting under two-sided limited commitment.

4.2 Wage setting and job creation

Given the parameters of a specific long-term wage contract, we turn to the question how this type of contract affects the cyclicality of job creation. In a frictional labor market, job creation is a forward-looking decision, which is described by a job creation condition of the following form.

$$c\left(q_t\right) = \frac{\bar{y}_t - \bar{w}_t}{r + \delta} \tag{6}$$

Here, $c\left(q_t\right)$, with $c'\left(.\right) \leq 0$ and $c''\left(.\right) \geq 0$, is the expected net present value of the cost of opening a vacancy, given a probability q_t that the firm can fill this vacancy in a given period, which depends on the unemployment rate and the aggregate number of

[25] Reiter suggests modeling technological change as embodied in job matches, because in his calibration the model with long-term contracts underpredicts the cyclicality of the average wages. With embodied technology, the model not only matches the elasticity of wages with respect to productivity for new hires as well as for all workers, but also replicates the relative volatility of labor market variables, solving the unemployment volatility puzzle.

vacancies.[26] The right-hand side of the equation equals the expected net present value of profits the firm will make once the vacancy has been filled, which depend on the 'permanent' levels of productivity $\bar{y}_t$ and wages $\bar{w}_t$ of the marginal worker, defined as,[27]

$$\bar{x}_t = \frac{r+\delta}{1-\delta} \sum_{\tau=1}^{\infty} \left(\frac{1-\delta}{1+r} \right)^{\tau} E_t x_{t+\tau} \tag{7}$$

where $r > 0$ is the discount rate for future profits and δ the probability that the match is destroyed in a given period. A form of job creation condition (6) holds true in a wide class of labor market models.[28]

When productivity increases, expected profits $\bar{y}_t - \bar{w}_t$ go up, so that firms post more vacancies, reducing the job filling probability q_t until in expectation vacancy posting costs $c(q_t)$ are again equal to profits. How many vacancies are created depends on how much of the additional match surplus goes to the worker in the form of higher wages. This is why the wage contract matters for the volatility of job creation. To formalize this point, we assume a standard iso-elastic matching technology with constant returns to scale so that we can link the job finding probability p_t to the job filling probability q_t. Let μ denote the share parameter of unemployment in the matching function, so that $p_t = \theta_t^{1-\mu} = q_t^{-(1-\mu)/\mu}$, where θ_t is the vacancy-unemployment ratio or labor market tightness. Then, taking a total derivative with respect to permanent productivity $\bar{y}_t$ and using (6) to calculate the effect of productivity on the job filling probability q_t, we get the following expression for the response of the job finding rate to changes in permanent productivity.

$$\frac{d \log p_t}{d \log \bar{y}_t} = -\frac{c(q_t)}{q_t c'(q_t)} \frac{1-\mu}{\mu} \left(\frac{\bar{y}_t}{\bar{y}_t - \bar{w}_t} - \frac{\bar{w}_t}{\bar{y}_t - \bar{w}_t} \frac{d \log \bar{w}_t}{d \log \bar{y}_t} \right) \tag{8}$$

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

[26] In the standard version of the model, as in Pissarides (2000), there is a per-period cost of maintaining a vacancy, so that $c(q_t) = k/q_t$, but in general there may be a fixed component to vacancy posting costs as well, e.g. $c(q_t) = K + k/q_t$ as in Pissarides (2009).

[27] 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.

[28] In Appendix A.1, we derive this expression for a search and matching model as in Pissarides (1985, 2000) or Shimer (2005). For many other models, some details may be different, but the condition will still look very similar and the results that follow will go through. For example, the separation probability δ may be time-varying as in Mortensen and Pissarides (1994), productivity y_t may represent the marginal product of labor and depend on capital as in Merz (1995) and Andolfatto (1996), firms may have multiple workers as in Rotemberg (2008) or Ebell and Haefke (2009), participation may be endogenous as in Haefke and Reiter (2011) or expectations about future productivity and wages may include the option value of moving into a different job if there is on-the-job search as in Menzio and Shi (2010). An identical job creation condition can also be derived in a model without search frictions but with worker heterogeneity, as in Merkl and van Rens (2012).

Four things matter for the volatility of the job finding rate in response to productivity shocks: the degree of countercyclicality of vacancy posting costs $q_t c'(q_t)/c(q_t)$, the elasticity of the matching function μ, the level of profits as a fraction of ouput $(\bar{y}_t - \bar{w}_t)/\bar{y}_t$, and the response of the permanent wage with respect to permanent productivity. If wages are fully flexible, in the sense that the elasticity of the permanent wage with respect to permanent productivity equals one, the response of the job finding rate to changes in productivity in (8) depends only on the elasticities of the cost and matching functions. If the response of the permanent wage to permanent productivity does not equal one, then the level of permanent profits is crucial for the amount of labor market volatility the model predicts. By making profits a small share of total match output, i.e. by calibrating the surplus of a match for firms to be small, the response of the job finding rate to changes in productivity can be made arbitrarily large (Costain and Reiter 2008, Hagedorn and Manovskii 2008).

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

4.3 Response of job creation to productivity

How large is the response of the present value of wages in new jobs to changes in productivity that is implied by our estimates? For the simulated wage contracts in Section 4.1, which we calibrated to be consistent with our estimates for the response of the average wage of new hires and all workers to changes in productivity, we have all the information necessary to calculate the expected net present value of wages at the start of a match.[29] Since we assumed a stochastic process for productivity, we can calculate the expected net present value of productivity as well. Appendix D.2 describes the details of these calculations, and Table 10 shows the results.

The third number in each cell in Table 10 reports the response of the permanent wage with respect to permanent productivity, $d\log\bar{w}_t/d\log\bar{y}_t$, for a given set of parameters of the wage contract. By the argument in Section 4.2, this elasticity is a good summary statistic for the cyclicality of the wage contract that affects labor market volatility. It is clear from the table that the elasticity of the permanent wage with respect to permanent productivity is always very close to the elasticity of the wage of new hires with respect to current productivity, suggesting that the latter is a good observable proxy for the

[29] The only additional piece of information we need is a discount rate, for which we use the three-month T-bill rate or the bank prime loan rate (FRED series TB3MS or MPRIME).

cyclicality of the wage contract. For the contract that is consistent with our estimates, we find an elasticity of the permanent wage with respect to permanent productivity of 0.8.

The only other estimate of the cyclicality of the expected net present value of wages in the literature we are aware of is by Kudlyak (2009). Kudlyak uses panel data from the NLSY and, as a result, there are methodological differences between her paper and ours. The main difference is that Kudlyak estimates wages as a function of time and age of the match using data for matches of all ages. Since the age of a match is not available in the CPS, we can only distinguish new matches from all other matches and have to assume that the cyclicality of wages in ongoing matches does not depend on the age of the match. In addition, Kudlyak can control for individual fixed effects, whereas we can only control for observable worker characteristics, see Sections 2.2 and 3.1.[30] Despite these differences, Kudlyak's estimates for the cyclicality of the expected net present value of wages are very similar to ours.

We now turn to the question how much wage ridigidy amplifies the effect of productivity shocks on job creation. As a benchmark, first consider the case of fully flexible wages that respond one-for-one to changes in productivity. In the calibration of Mortensen and Nagypal (2007), all vacancy posting costs are per-period costs, $c(q_t) = c/q_t \Rightarrow -q_t c'(q_t)/c(q_t) = 1$, and the elasticity of the matching function with respect to unemployment equals $\mu = 0.6$. Thus, with $d \log \bar{w}_t / d \log \bar{y}_t = 1$, job creation condition (6) predicts an elasticity of the job finding rate with respect to permanent productivity of $d \log p_t / d \log \bar{y}_t = (1-\mu)/\mu = 0.7$, see equation (8). Our estimates for the process for productivity imply that productivity is very close to a random walk, $d \log \bar{y}_t / d \log y_t = 1.04$, so that the elasticity of the job finding rate with respect to current productivity is roughly equal to the elasticity with respect to permanent productivity, $d \log p_t / d \log y_t = d \log p_t / d \log \bar{y}_t = 0.7$. In the data, a regression of the log of the job finding rate on the log of productivity gives a coefficient of 7.6 (Mortensen and Nagypal 2007). Thus, the model underpredicts the volatility of the job finding rate in response to technology shocks by a factor 10.

According to our estimates, the elasticity of the permanent wage with respect to permanent productivity equals 0.8. In order to assess how much this amount of wage rigidity amplifies fluctuations in job creation using equation (8), we need a value for the ratio of wages over productivity $\bar{w}_t / \bar{y}_t$. Since no direct calibration target is available, we need to close the model in order to calibrate this ratio. We solve the model in steady state and assume, without loss of generality, that in steady state a fraction ϕ of the surplus generated by a match goes to the worker, regardless of the amount of wage stickiness. Under this assumption, we show in Appendix A.2 that the wage is a weighted average of productivity y and the flow value of unemployment z, where the weight can be written

[30] The advantage of our approach, on the other hand, is that we can use the CPS, a dataset that is much larger and representative for the US labor force.

in terms of direct calibration targets only. Substituting this equation for the wage into expression (8), the elasticity of the job finding rate with respect to productivity can be written to a good approximation as,

$$\frac{d\log p_t}{d\log \bar{y}_t} \simeq -\frac{c(q)}{qc'(q)}\frac{1-\mu}{\mu}\left[1+\frac{\phi}{1-\phi}\frac{p}{r+\delta}\frac{y}{y-z}\left(1-\frac{d\log \bar{w}_t}{d\log \bar{y}_t}\right)\right] \tag{9}$$

where the approximation is valid for $\phi p >> r+\delta$.

Since the job finding rate p and the separation rate δ are observable and their average levels are typically used as calibration targets, there is no controversy about the ratio $p/(r+\delta)$ in steady state, which equals 12 in the US data.[31] This high ratio, which corresponds to a relatively low unemployment rate, strongly amplifies the effect of small surplus $y/(y-z)$ as well as wage rigidity $1-d\log \bar{w}_t/d\log \bar{y}_t$. Assuming per-period vacancy posting costs as in the standard model, $-q_t c'(q_t)/c(q_t)=1$, using a value for $\mu=0.6$ as in Mortensen and Nagypal (2007), assuming the Hosios condition is satisfied in steady state so that $\phi=\mu$ and using our estimate for wage rigidity, $d\log \bar{w}_t/d\log \bar{y}_t = 0.8$, we find that $d\log p_t/d\log \bar{y}_t = 5$ for $z/y=0.4$ as in Shimer (2005), $d\log p_t/d\log \bar{y}_t = 9$ for $z/y=0.7$ as in Mortensen and Nagypal (2007) and $d\log p_t/d\log \bar{y}_t = 49$ for $z/y=0.95$ as in Hagedorn and Manovskii (2008). Thus, given the observed response of wages to changes in productivity, the model can comfortably match the observed regression coefficient of the job finding rate on productivity of 7.6 for reasonable values of the replacement ratio.

Equation (9) can be used to understand the various solutions that have been proposed for the unemployment volatility puzzle. We find that on the one hand there is evidence for very little wage rigidity in the data, but on the other hand very little wage rigidity is needed to match the volatility of job creation. The intuition for this conclusion is that the wage as a fraction of productivity $\bar{w}/\bar{y}$ is very close to one so that even a small amount of wage rigidity generates a large amount of amplification, see equation (8). A similar argument was made by Mortensen and Nagypal (2007), although they did not have any direct evidence on the amount of wage rigidity in the data. The observed amount of wage rigidity is consistent with a modest degree of wage stickiness e.g. as in Hall and Milgrom (2008), but can also be replicated by models with flexible wage setting, for example by reducing workers' bargaining power as in Hagedorn and Manovskii (2008). By assuming less countercyclicality in vacancy posting costs, as Pissarides (2009) does, it is even possible to match the volatility of job creation without any wage rigidity, i.e. with $d\log \bar{w}_t/d\log \bar{y}_t = 1$. In this case, $-q_t c'(q_t)/c(q_t) = k/(qK+k)$ so by making the per-period component of vacancy posting costs k arbitrarily small relative to the fixed component K, one can amplify the volatility of job creation to arbitrarily high levels.

[31] There may be disagreement about the average levels of p and δ, which depend on the time period used and the aggregation method to go from monthly data to other frequencies, but not about their ratio.

The contribution of this paper is to provide an estimate of the response of the expected net present value of wages to changes in productivity, which can be used as a calibration target and rules out models with very sticky wage setting.

5 Conclusions

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

Our results point against rigidity in the wage of newly hired workers as an explanation for the volatility of unemployment over the business cycle as advocated by Hall (2005), Gertler and Trigari (2009) and Blanchard and Galí (2007). However, we also show that very little wage rigidity is needed to match the volatility of job creation, so that our results are consistent with studies that imply a moderate degree of wage stickiness, like Hall and Milgrom (2008), or studies that generate wage rigidity with flexible wage setting by reducing workers' bargaining power, as Hagedorn and Manovskii (2008). 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 Details on the derivations

A.1 Derivation of job creation condition (6)

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

$$c(q_t) = E_t J_{t+1} \tag{10}$$

The value to the firm of having a filled job J_t, is given by the following Bellman equation.[32]

$$(1+r)J_t = y_t - w_t + (1-\delta)E_t J_{t+1}. \tag{11}$$

Solving equation (11) forward gives an expression for the value of a filled job.

$$E_t J_{t+1} = \frac{\bar{y}_t - \bar{w}_t}{r+\delta} \tag{12}$$

Substituting (12) into (10) gives the job creation equation in the main text.

A.2 Derivation of steady state elasticity (9)

In steady state, the job creation equation (10) and the Bellman equation (11) for a filled job J simplify to

$$c(q) = J \tag{13}$$
$$(1+r)J = y - w + (1-\delta)J \tag{14}$$

In order to solve for the wage, we need to complement this labor demand side of the model with Bellman equations for an employed worker W and an unemployed worker U.

$$(1+r)W = w + (1-\delta)W + \delta U \tag{15}$$
$$(1+r)U = z + pW + (1-p)U \tag{16}$$

We assume that in steady state workers receive a fraction ϕ of the surplus generate by a match, so that we get the following surplus sharing rule.

$$\frac{W-U}{\phi} = \frac{J}{1-\phi} \tag{17}$$

The steady state wage can be calculated from the Bellman equation for a filled job

[32] 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.

J, using the surplus sharing rule.

$$w = y - (r + \delta) J = y - (r + \delta)(1 - \phi) S \tag{18}$$

where total match surplus $S = W - U + J$ is given by

$$S = \frac{y - z}{r + \delta + \phi p} \tag{19}$$

Substituting and simplifying, we get

$$w = \Phi y + (1 - \Phi) z \tag{20}$$

where

$$1 - \Phi = \frac{(r + \delta)(1 - \phi)}{r + \delta + \phi p} \simeq \frac{1 - \phi}{\phi} \frac{r + \delta}{p} \tag{21}$$

The approximation is valid for $p >> r + \delta$. Notice that we have left the endogenous variable p in this expression, which is why we did not use steady state job creation equation (13) and why the wage does not depend on vacancy posting costs. We do this, because the average level of p is directly observable and typically used as a calibration target.

Substituting this expression into the expression for the elasticity of the job finding rate with respect to productivity (8) in the main text, evaluated in steady state, we get.

$$\frac{d \log p_t}{d \log \bar{y}_t} \simeq - \frac{c(q)}{q c'(q)} \frac{1 - \mu}{\mu} \left[1 + \frac{y}{(1 - \Phi)(y - z)} \left(1 - \frac{d \log \bar{w}_t}{d \log \bar{y}_t} \right) \right] \tag{22}$$

where we again used the approximation that $p >> r + \delta$ so that $1 - \Phi > 0$ is close to zero. Using equation (21) to substitute for $1 - \Phi$ gives expression (9) in the main text.

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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 C.

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: Worker heterogeneity and composition bias

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

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

Table 6: Differences across gender and age groups

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

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

Table 7: Exogenous changes in productivity

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

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

Table 8: Response of wages of job changers

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

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

Table 9: Wage rigidity before the Great Moderation

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

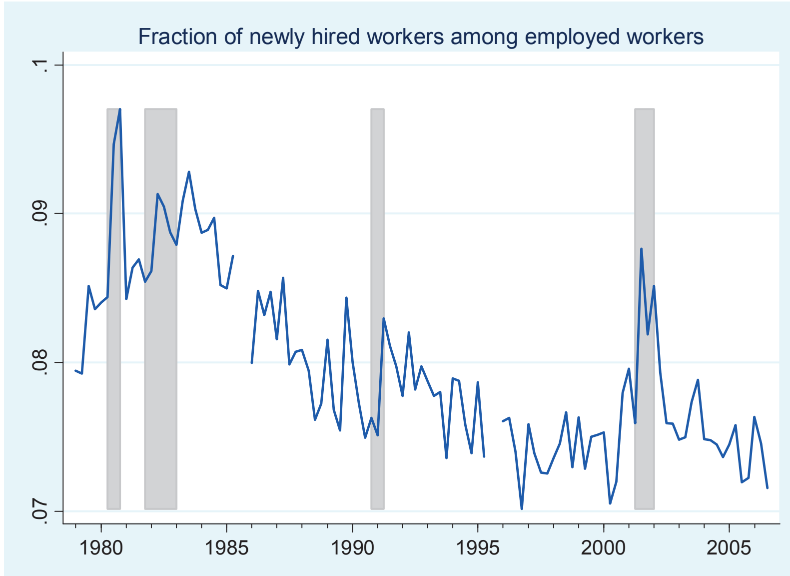
The table compares the results for our baseline sample of post 1984 data to the full sample starting in 1979. Elasticities are estimated using the two-step method described in the text.

Table 10: Simulated long-term wage contracts

$\phi_{1,\text{newh}}$	$\phi_{1,\text{stay}}$				
	0.00	**0.25**	0.50	0.75	1.00
	0.10	0.10	0.10	0.10	0.10
0.10	-0.04	0.18	0.40	0.63	0.85
	0.10	0.11	0.12	0.13	0.14
	0.30	0.30	0.30	0.30	0.30
0.30	-0.02	0.20	0.42	0.64	0.87
	0.29	0.30	0.31	0.32	0.33
	0.50	0.50	0.50	0.50	0.50
0.50	0.00	0.22	0.44	0.66	0.88
	0.48	0.49	0.50	0.51	0.52
	0.80	**0.80**	0.80	0.80	0.80
0.80	0.03	**0.25**	0.47	0.69	0.91
	0.77	**0.78**	0.79	0.80	0.81
	1.00	1.00	1.00	1.00	1.00
1.00	0.05	0.27	0.49	0.71	0.93
	0.97	0.97	0.98	0.99	1.00

The table reports three elasticities from simulated data for individual wages, assuming long-term wage contracts with parameters $\phi_{1,\text{newh}}$ and $\phi_{1,\text{stay}}$ as described in Section 4.1. The first two numbers in each cell are the elasticities for the wages of new hires and all workers, estimated from the simulated data using specification (3). Since we repeated the simulations many times and averaged the results, the standard errors of these estimates are negligible. The third number is the elasticity of the expected net present value of wages with respect to the expected net present value of productivity, calculated consistent with the stochastic processes we used for the simulations. Across rows and columns of the table we vary the parameters of the wage contracts. Different rows show results for different values for the cyclicality of the wage at the start of a contract. Different columns correspond to different values for the cyclicality of the wage in an ongoing job relationship.

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 B. The grey areas indicate NBER recessions.

Supplemental Materials

B 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.

B.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 C), 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.[33] 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.

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

B.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 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.

C 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.[34] 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.

$$var\left(w_t\right) = var\left(w_t^*\right) + \sigma^2 \Rightarrow sd\left(w_t^*\right) = \sqrt{R} \cdot sd\left(w_t\right) \tag{23}$$

$$cov\left(w_t, w_{t-1}\right) = cov\left(w_t^*, w_{t-1}^*\right) \Rightarrow corr\left(w_t^*, w_{t-1}^*\right) = \frac{corr\left(w_t, w_{t-1}\right)}{R} \tag{24}$$

$$cov\left(w_t, x_t\right) = cov\left(w_t^*, x_t\right) \Rightarrow corr\left(w_t^*, x_t\right) = \frac{corr\left(w_t, x_t\right)}{\sqrt{R}} \tag{25}$$

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.

[34] 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.

D Simulating long-term wage contracts

D.1 Parameters of the wage contract

The goal of the simulation is to simulate individual wage histories that when aggregated, yield the same aggregate behavior as the observed all workers series. In order to simulate the individual wage histories, several data needs to be known:

1. Productivity: taken from the actual data, or otherwise based on coefficients estimated from actual data. When estimated we assume a specification as represented in equation (26).

2. Aggregate wage series of newly hired workers: taken from the actual data, or otherwise based on coefficients estimated from actual data. When estimated we assume a specification as represented in equation (27).

3. Form of the Wage Contract for Continuing Workers: We do not directly observe the individual wage path for continuing workers. There are two obvious components in the wage contract for continuing workers, one is how these wages react to productivity, the other one is a constant expected growth rate, the equation is (28). We pick $\phi_{1,\text{stay}}$ so that the regression coefficient for the simulated wage of all workers on productivity matches the one from the data. However, in the data the average wage of all workers is substantially larger than the average wage of newly hired workers. This difference in the two aggregate wage series comes from the growth of wages on the job independent of productivity. The role of $\phi_{0,\text{stay}}$ is to allow for this average wage difference.

4. Separation Probabilities, δ_t: We simulate the data at a quarterly frequency. I take the observed number of workers and number of separations from our CPS dataset to find the quarterly separation probability:

$$\begin{aligned} S_{t-1} &= E_{t-1} + N_t - E_t \\ \delta_t &= \frac{S_t}{E_t} \end{aligned}$$

5. Job Finding Probabilities: Quarterly job finding probabilities are very high. In order to avoid these probabilities to exceed one, I assume that workers who were separated can immediately search again. This is a completely innocuous assumption for our purposes because we only care about the evolution of wages on the job and not about what happens during unemployment. Picking job finding and separation probabilities in this way guarantee that the employment path in our simulations coincides with actual employment from the data.

6. Interest Rate: To compute the present value, an interest rate needs to be used, we take it from FRED, either the three month T-bill rate (TB3MS) or the bank prime loan rate (MPRIME).

We assume that productivity and wages are described by:

$$\log y_t = \log y_{t-1} + \psi_0 + \psi_1 (\log y_{t-1} - \log y_{t-2}) + \upsilon_t \tag{26}$$
$$\log w_t^0 = \phi_{0,\text{newh}} + \phi_{1,\text{newh}} \log y_t + \upsilon_t^0 \tag{27}$$
$$\log w_t^a = \log w_{t-1}^{a-1} + \phi_{0,\text{stay}} + \phi_{1,\text{stay}} (\log y_t - \log y_{t-1}) + \nu_t \tag{28}$$
$$\upsilon_t^0 \sim i.i.d\, \mathcal{N}(0, \sigma_0^2) \quad \forall t \tag{29}$$
$$\nu_t \sim i.i.d\, \mathcal{N}(0, \sigma^2) \quad \forall t \tag{30}$$

Denote by $\log w_t^a$ the wage of a worker in period t who was hired in period $t-a \leq t$. Thus the wage $\log w_t^0$ is the newly hired wage. It is easy to see that wages and productivity are cointegrated, but this is not key for the present derivation.

We also see that log-productivity follows an AR(1) in first differences or an AR(2) in levels. Wages for stayers are specified to grow for two reasons. $\phi_{0,\text{stay}}$ denotes a constant, autonomous growth rate, and $\phi_{1,\text{stay}}$ determines how strongly wages of stayers grow with productivity. The parameters of equation (28) will be determined by the simulation estimator, equations (26) and (27) are estimated directly from the data.

We simulate for the 88 quarters which we are also using for the original data analysis. All simulations are executed for 1 500 000 individuals.

In the simulation we proceed in several steps. First all the data is processed and the empirical process for aggregate newly hired wages and labor productivity are estimated. To generate a wage distribution at the beginning of 1984, we start the simulation 70 quarters earlier, so that by the time 1984 is reached, we have a nice distribution of wages for ongoing workers. In these 70 quarters preceding 1984 productivity is taken from the actual data but newly hired wages are computed based on the estimated coefficients. From 1984 onwards we use actual data for newly hired wages.

Two exercises are performed:

1. finding the wage contract for continuing workers such that the the elasticity of the wage for all workers is matched.

2. For various, exogenously given values of $\phi_{1,\text{newh}}$ and $\phi_{1,\text{stay}}$ simulate wage paths and estimate $\hat{\eta}_{\text{newh}} = \hat{\phi}_{1,\text{newh}}$, $\hat{\eta}_{\text{allw}}$ as well as the response of the expected present values.

D.2 Present value of wages and productivity

In order to later compute expectation and variance of wages and productivity it is useful to have access to a moving-average type representation for productivity.

$$\gamma_1 = \frac{1}{\psi_1 - 1} \left(\log y_t - \log y_{t-1} \right) \tag{31}$$

$$\gamma_2 = \log y_t - \psi_1 \gamma_1 = \frac{1}{\psi_1 - 1} \log y_t + \frac{\psi_1}{\psi_1 - 1} \log y_{t-1} \tag{32}$$

$$\log y_{t+k} = \gamma_2 + \gamma_1 \psi_1^{k+1} + \frac{1}{\psi_1 - 1} \left\{ \sum_{s=1}^{k} \left(\psi_0 + \upsilon_t \right) \left(1 - \psi_1^{k+1+s} \right) \right\} \tag{33}$$

Notice that we are considering an *exploding* series here. Therefore it is not possible to simply take the MA-representation of an AR(2). Based on the distributional assumptions given in (29) we can now compute conditional expectations and variances:

$$\begin{aligned} E(\log y_{t+k} | \log y_t) &= \gamma_2 + \gamma_1 \psi_1^{k+1} + \frac{\psi_0}{\psi_1 - 1} \sum_{s=1}^{k} \left(1 - \psi_1^{k+1+s} \right) \\ &= \gamma_2 + \gamma_1 \psi_1^{k+1} + \frac{k(1-\psi_1) - \psi_1(1-\psi_1^k)}{(1-\psi_1)^2} \psi_0 \\ &= \log y_t + \frac{\psi_1 \left(1 - \psi_1^k\right)}{1 - \psi_1} \left(\log y_t - \log y_{t-1} \right) + \frac{k(1-\psi_1) - \psi_1(1-\psi_1^k)}{(1-\psi_1)^2} \psi_0 \qquad (34) \\ V(\log y_{t+k} | \log y_t) &= \frac{\sigma_\upsilon^2}{(1-\psi_1)^2} \sum_{s=1}^{k} \left(1 - \psi_1^{k+1+s} \right)^2 \\ &= \left(\frac{1}{(-1+\psi_1)^2} k^2 + \frac{\psi_1 \left(-1 + \psi_1^k\right) \left(-2 + \psi_1(-1 + \psi_1^k)\right)}{(-1+\psi_1)^2 (-1+\psi_1^2)} k \right) \sigma_\upsilon^2 \qquad (35) \end{aligned}$$

Not surprisingly the conditional variance is growing at the rate of k^2, so when computing the present values we need to hope that our discounting and separations probabilities will counter this effect. Any potential divergence is going to come from this conditional variance term.

For wages, note that we can write:

$$\log w_{t+k}^k = \log w_t^0 + k\phi_{0,\text{stay}} + \phi_{1,\text{stay}} \left(\log y_{t+k} - \log y_t \right) + \sum_{s=1}^{k} \nu_{t+s} \tag{36}$$

$$E(\log w_{t+k}^k | \log w_t^0) = k\phi_{0,\text{stay}} + \phi_{1,\text{stay}} \left(E(\log y_{t+k} | \log y_t) - \log y_t \right) \tag{37}$$

$$V(\log w_{t+k}^a | \log w_t^0) = \phi_{1,\text{stay}}^2 V \left(\log y_{t+k} | \log y_t \right) + k\sigma^2 \tag{38}$$

The final step is the computation of expected present values. First consider productivity:

$$
\begin{aligned}
E_t PV(y) &= e^{\log y_t} + \sum_{k=1}^{\infty} \left(\frac{1-\delta}{1+r} \right)^k E_t e^{\log y_{t+k}} \\
&= e^{\log y_t} + \sum_{k=1}^{\infty} \left(\frac{1-\delta}{1+r} \right)^k e^{\{E_t(\log y_{t+k} | \log y_t) + 0.5 V_t(\log y_{t+k} | \log y_t)\}} \qquad (39)
\end{aligned}
$$

To compute the expected present value of wages, we can use the assumption that wages are a linear function of contemporaneous productivity.

$$
\begin{aligned}
E_t PV(w) &= w_t^0 + \sum_{k=1}^{\infty} \left(\frac{1-\delta}{1+r} \right)^k E_t e^{\log w_t^0 + k\phi_{0,\text{stay}} + \phi_{1,\text{stay}} (\log y_{t+k} - \log y_t) + \sum_{s=1}^{k} \nu_{t+s}} \\
&= w_t^0 \left(1 + \sum_{k=1}^{\infty} \left(\frac{1-\delta}{1+r} \right)^k E_t e^{k\phi_{0,\text{stay}} + \phi_{1,\text{stay}} (\log y_{t+k} - \log y_t) + \sum_{s=1}^{k} \nu_{t+s}} \right) \\
&= w_t^0 \left(1 + \sum_{k=1}^{\infty} \left(\frac{1-\delta}{1+r} \right)^k e^{k\left(\phi_{0,\text{stay}} + 0.5\sigma^2\right) + \phi_{1,\text{stay}} (E_t \log y_{t+k} - \log y_t) + 0.5 \phi_{1,\text{stay}}^2 V_t \log y_{t+k}} \right) \qquad (40)
\end{aligned}
$$

E Additional tables and figures

Table 11: 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

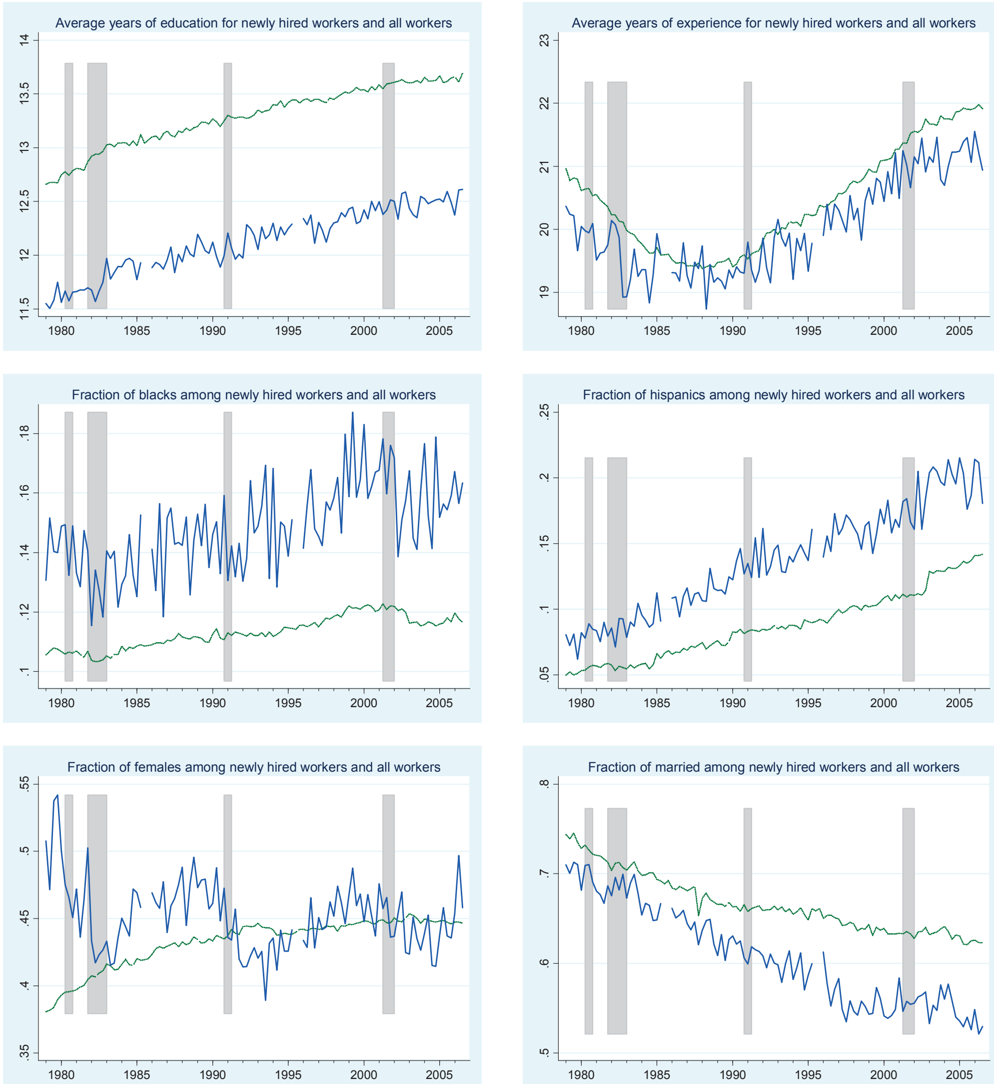
Elasticities are estimated using the two-step method described in the text. WLS weights the second step regression by the inverse of the variance of the first step estimates. Median uses the median wages instead of mean wages by quarter.

Table 12: 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.

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 B. The grey areas indicate NBER recessions.