The Cyclical Behavior of Job Quality and Real Wage Growth*

Andriana Bellou
Université de Montréal
CIRANO, CIREQ and IZA

Barış Kaymak†
Université de Montréal
CIREQ

Abstract

We propose new tests for implicit insurance contracts that are robust to changes in the cyclical composition of unobserved job quality, a concern recently raised in critique of the existing evidence in the literature. The proposed tests can detect more general contractual arrangements than studied in the literature, e.g. those that are not binding on the employer or the worker. Using these tests, we document new evidence supporting the contractual view of the labor market. The results also suggest that endogenous separations do *not* lead to procyclical selection in average match quality for existing matches as cleansing effects of recessions are somewhat stronger than its sullying effects.

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†Corresponding Author. Department of Economics, Université de Montréal, C.P. 6128 succursale Centre-ville, Montréal, QC H3C 3J7. E-mail: baris.kaymak@umontreal.ca

There is a large body of empirical work documenting a strong correlation between past labor market conditions experienced by workers and their wages, even after accounting for current economic environment.[1] This appears to be at odds with a static labor market where the exchange of labor services with compensation is contemporaneous, and, therefore, where wages depend solely on current conditions. The pattern of history dependence in the data was thus thought to corroborate the prevalence of dynamic contractual arrangements in the labor market. In a seminal paper, Beaudry and DiNardo (1991), showed that wages are best explained by the lowest unemployment rate since the start of a job, and that contemporaneous conditions do not matter. This finding is consistent with implicit insurance contracts with costless worker mobility. In such a setting, the optimal insurance contract features downward wage rigidity, which insures the worker against drops in productivity, but allows for raises to prevent quits when the worker's outside option sufficiently improves (Harris and Holmstrom, 1982). As a result, a worker's wage rate reflects the best outside option they ever had, approximated by the lowest unemployment rate in Beaudry and DiNardo (1991).[2]

Nevertheless, one may be too quick to dismiss the static market hypothesis based on this evidence. In recent work, Hagedorn and Manovskii (2013) argue that the observed history-dependence in wages is an artifact of a simple omitted variable problem, caused by cyclical movements in average job quality, an unobserved component of productivity that is specific to the employer-employee match. In particular, they show that a search model with on-the-job search and heterogenous match quality generates a pro-cyclical selection effect in job quality, i.e. bad matches dissolve during expansions and, therefore, matches that survive expansions must be of better quality. Then the lowest unemployment rate since the start of a job is an indicator of its quality, and, hence of the wage rate associated with that job. Consequently, they construct proxies for average match quality based on the model and show that when these proxies are included in regressions of the Beaudry and DiNardo (1991) type, past economic conditions cease to matter, and wages are explained *solely* by contemporaneous conditions. This raises the question of whether the empirical support for the contractual view can be reinstated with different tests, or the hypothesis of implicit contracts should instead be abandoned all together.

In this paper, we propose new tests of implicit insurance contracts that are robust to the presence of heterogeneity in match quality in the data, and that can detect a broader class of contractual arrangements than studied in the empirical literature. We use these tests to provide new evidence

[1]The empirical literature finds significant cohort effects (see, for instance, Baker, Gibbs and Holmstrom (1994)) and persistent negative effects of being hired in a recession (e.g. Kahn (2010) among others). Similarly, a large literature initiated by Bils (1985) finds that wages of newly hired workers are more pro-cyclical than job stayers.

[2]While we focus on history-dependence in wages, others have used alternative methods to test for dynamic contractual arrangements. Guiso, Pistaferri and Schivardi (2005) uses matched employee-employer data to test if workers are insured against idiosyncratic firm risk. Guiso, Pistaferri and Schivardi (2013) finds evidence for dynamic arrangements where workers help alleviate credit constraints on firms by accepting backloaded wage profiles.

supporting the long-term contracts view of wage setting. Furthermore, we challenge the conclusion in Hagedorn and Manovskii (2013) regarding the cyclicality of selection in match quality. In particular, we argue that their proxy for match quality combines the non-cyclical element of selection in match quality, which results in good matches to last longer at all times (Abraham and Farber, 1987), with a potentially cyclical component that operates through dissolution of poor matches. The latter is the key for interpreting the impact of the lowest unemployment rate on current wages documented in Beaudry and DiNardo (1991). We show that when analyzed separately, selection in average quality of existing matches is acyclical, if not countercyclical, contrary to the claims in Hagedorn and Manovskii (2013). This is because poor matches dissolve not only during expansions when workers quit for better matches, but also during recessions when they become unprofitable (Davis and Haltiwanger, 1992). The latter appears to be at least as important, resulting in slightly countercyclical selection effects in match quality.[3] This is consistent with Bils (1985), who finds the composition of worker quality to be similarly countercyclical.

The key identification strategy to test for implicit insurance contract draws on the behavior of wage growth in response to a change in economic conditions for workers who *do not* change jobs. By focusing exclusively on job stayers, we are able to control for the confounding effect of match quality under the assumption that the latter is time invariant for a given employer-employee pair. This approach eliminates the need to rely on proxies for job quality.

To see the essence of our argument consider two identical workers: worker B who was hired during a boom, and worker R who was hired in the *subsequent* recession. If the employment relationship is characterized by insurance contracts, worker B enjoys a higher wage rate than worker R over the recession, because he was insured against a possible downturn prior to the recession. Nonetheless, his advantage is temporary. As the economy recovers from the recession, outside opportunities improve. Since R is paid less for the same level of productivity, he's the first to try to leave given a set of offers.[4] Consequently, to prevent severance, the employer is more likely to offer a raise to worker R, or, to offer him a larger wage raise relative to worker B. Thus R's expected wage gain is larger than B's. If, on the other hand, the labor market is static, both workers should be paid equally at all times since they are equally productive. Hence, in static models there is no reason to believe that the wage adjustments over the business cycle should depend on past economic conditions.

We apply this argument to two classes of contractual arrangements. The benchmark test is based on self-enforcing wage contracts binding on the employer, but not on the worker. In this

[3] In recent work, da Fonseca, Gallipoli and Yedid-Levi (2016) apply a similar decomposition to study differences in wage setting across occupations and find that average match quality to be countercyclical in manual jobs and acyclical in cognitive jobs. This is consistent with our results.

[4] We assume that some degree of worker mobility is allowed, and that workers cannot sign a contract that ties them to an employer regardless of their outside options.

case we show that wage growth depends solely on the change in the lowest unemployment rate. By contrast, the contemporaneous change in the unemployment rate is not a significant determinant of wage growth for job stayers. This finding is robust to non-random selection of stayers over the business cycle, to variations in the role of training and human capital, and it can be generalized to a larger class of models, where job quality is not time-invariant.

Next, we study a contractual market where neither the worker nor the employer can credibly commit to the contract (Thomas and Worrall, 1988). Empirical tests for this case are not readily available in the literature. In this more general case, the optimal contract cannot be summarized by extremum moments, such as the best or the worst unemployment rate. We propose a novel framework for testing for a broader class of contractual arrangements, without relying on the typically used indicators. In contractual markets, workers with identical productivity may receive different wages due to differences in the history of economic conditions. Those who started in good times, or experienced better economic conditions on the job command higher wages. They are, therefore, subject to larger wage cuts during downturns when the employer's outside option binds. Moreover, since their wage is high relative to their outside option, they are likely to receive smaller wage raises during recoveries. Hence, conditional on productivity, there is a negative relationship between wage levels and subsequent wage growth.

To test this prediction, a two-stage procedure is developed. In the first stage, the contractual variation in wages that is due to differences in the history of economic conditions is captured by projecting wages on a set of indicators of job history. In the second stage, wage growth is regressed on the predicted wage from the first stage. The findings indicate a negative correlation between wage levels and subsequent wage growth conditional on productivity. This is consistent with the contractual view of the labor market. If the labor market were static, then the differences in wages due to history-dependence would instead be driven by differences in current productivity. There is, however, no reason to expect a lower wage growth for more productive matches.

In the next section, we contrast the implications of contractual and static markets for wages in an economy with heterogeneous job quality, and develop our testing strategy. Section 3 presents the empirical results for contracts that are binding on the employer, but not on the worker. Section 4 presents tests for contracts without commitment. A discussion is provided in Section 5.

1 Market Structure and the Cyclical Behavior of Wages

In this section we develop a test that empirically distinguishes the contractual from the static view of the labor market. The benchmark version of the test draws on the existing theory of self-insurance contracts, which mainly focuses on contracts that are binding on the employer, but not on the worker. Therefore, we begin by reviewing this literature, and discuss the challenges that the current empirical tests face when there is cyclical variation in average job quality. The benchmark test addresses these concerns within this particular class of contractual models. Next, we consider more general wage contracts that are not binding on either part. We develop a test of the implicit contracts theory for this broader class of contracts, which is also robust to unobserved heterogeneity in job quality.

1.1 Self-Enforcing Insurance Contracts and the Distribution of Wages

In a static market, wage equals productivity at all times. By contrast, in markets with contracts, the tight link between wage and productivity link can be looser in the short-run. The theory of implicit insurance contracts, in particular, deals with arrangements where employers insure workers against temporary fluctuations in productivity. It is assumed that firms have better (or less costly) means of diversifying risk, and that, when alternative means of insurance are available to the worker, they do not completely crowd out the possibility of insurance through the employer.

In this setup, workers and employers sign a state-dependent wage contract that insures the worker against unforeseen fluctuations in his productivity. The extent of the insurance and the implications of the contractual arrangement for the empirical distribution of wages depend crucially on the levels of commitment to the contract by the worker and the employer. Full commitment by both parties, for instance, implies that both the worker and the employer are required to honor the contract at all times irrespective of the economic conditions. In this case, the optimal contract features full insurance, i.e. a constant wage rate regardless of changes in the worker's productivity. When there is free entry, the wage reflects the expected productivity of the worker at the time the contract is signed. Therefore, the economic conditions at the start of the job are sufficient to capture the cross-sectional variation in wages.

When the contract is binding only on the employer, and the worker is allowed to quit at any time, the optimal contract features a downward rigid wage, which increases only if the worker's outside option has sufficiently improved. The employer partially insures the worker by committing to never lower the wages in the future. Therefore, at any time, a worker's current wage reflects the highest wage he could command on the job. The best economic conditions since the start of the job exhaust all the variation in wages.

In a seminal paper, Beaudry and DiNardo (1991) test for the empirical validity of implicit contract theory by estimating the following wage regression, which encompasses the three different models described above.

$$w_{ijt} = \theta_1 u_t + \theta_2 u_{t_0} + \theta_3 u_{t_0,t}^{min} + X_{ijt}\Lambda + \epsilon_{ijt}. \tag{1}$$

The economic conditions are approximated by the unemployment rate: u_t is the current unemployment rate, u_{t_0} is the unemployment rate at the start of a worker's job and $u^{min}_{t_0,t}$ is the minimum unemployment rate since the start of the job. If the labor market is static, then wages are explained *only* by the current conditions ($\theta_2 = \theta_3 = 0$). If the labor market is better described by full commitment contracts, then the initial unemployment rate is the only relevant variable ($\theta_1 = \theta_3 = 0$). If, on the other hand, contracts are not binding on the worker, but are on the employer, then the minimum unemployment rate is a sufficient statistic for wages ($\theta_1 = \theta_2 = 0$). Beaudry and DiNardo (1991) reject the spot market hypothesis in favor of contractual markets with one-sided commitment.

1.2 Endogenous separations and cyclical selection of job quality

In contractual markets, the current wage distribution displays a certain type of history-dependence, and current economic conditions do not matter. A superficial history dependence can nevertheless arise in static labor markets, if unobserved components of productivity are correlated with past economic conditions. To see this, assume a static market ($\theta_2 = \theta_3 = 0$), set $\Lambda = 0$ in (1) for simplicity, and decompose the error term into three terms:

$$\epsilon_{ijt} = a_i + m_{ij} + \nu_{ijt}, \tag{2}$$

where a_i is a worker-specific productivity component, m_{ij} is a time-invariant match-specific component reflecting job quality and ν_{ijt} is a random error term.

Let us begin with quit behavior associated with on-the-job search. Every period workers draw offers from a stationary distribution. The offers are drawn before ν_{ijt} is realized. Let $\tilde{w}_{it}$ denote the best wage offer that a worker, employed or unemployed, can obtain in the market. This offer depends only on the current economic conditions. A worker decides to quit his job if $\tilde{w}_{it} > w_{ijt}$. Since the cyclical and the worker-specific components are equally valuable in all jobs, and since $E[\nu_{ijt}] = 0$, a better wage offer must come from a better match as otherwise the current employer could retain the worker by outbidding the outside offer.

The endogenous quit decision leads to destruction of poor matches over time, leading to the survival of only the best ones. Denoting the match quality corresponding to the best wage offer in year t by $\tilde{m}_{it}$, the average match quality conditional on seniority is

$$E[m_{ij} | m_{ij} > \max\{\tilde{m}_{it_0},\ ...,\ \tilde{m}_{it}\}]. \tag{3}$$

The number of arguments in the max operator above increases with tenure, raising the average quality of surviving matches.[5] If the wage offer $\tilde{w}_{it}$ is pro-cyclical, the selection in (3) applies

[5] When the match quality is not observed, this confounds the estimates of the return to seniority (Topel, 1991;

more stringently to those workers who experienced better economic conditions. This would lead to a *negative* relation between the wage rate and the minimum unemployment rate experienced over a worker's tenure as observed by Beaudry and DiNardo (1991) in the data.

One way to address pro-cyclical quit behavior could be to include proxies for match quality in wage regressions. To that end, let $\tilde{m}_{it} = f(y_t)$ with $f' > 0$ represent the procyclical nature of outside offers. Then, the average match quality of workers who stayed with their employers for T periods depends on the best offer they ever received: $\max\{\tilde{m}_{it_0},\ \ldots,\ \tilde{m}_{it_0+T}\} = f(\max\{y_\tau\}_{\tau=t_0}^{\tau=t_0+T})$. Therefore, the best economic conditions experienced *for the entire duration of the job* is a natural proxy for match quality. Note that this proxy is similar but not the same as the variable used to test for implicit contracts, which aim to approximate the best market conditions between the time of the hire and the time when wage is observed, or $f(\max\{y_\tau\}_{\tau=t_0}^{\tau=t})$. When both are included in a wage regression, the identification of the proxy for match quality comes from the changes in aggregate conditions between t and $t_0 + T$. This could lead to multicollinearity issues in the data and limit the power of wage level regressions for testing the implicit contracts model empirically.

Consider now a fall in aggregate productivity which deems certain matches unprofitable to sustain. Whether a match dissolves or not depends critically on the match surplus, m_{ij}. Let $\underline{m}(y_t)$ with $\underline{m}' < 0$ be the minimum match quality sustainable given the aggregate conditions y_t. The average match quality of workers who stayed with their employers for T periods is $E[m_{ij}|m_{ij} > \max\{\underline{m}(y_{t_0}),\ \ldots,\ \underline{m}(y_{t_0+T})\}]$, which can be approximated by $\min\{y_\tau\}_{\tau=t_0}^{\tau=t_0+T}$ since $\underline{m}$ is strictly decreasing in y_t. Therefore, the *worst* economic conditions experienced for the entire duration of the job is a natural proxy for selection in match quality caused by downturns.

These proxies for match quality are not the same as those used in Hagedorn and Manovskii (2013), who adopt an alternative formulation, which begins by positing that a worker receives a total of $N_t = g(y_t)$ offers each period with $g' > 0$. Let w_{nt} denote a single offer in period t with $n = 1, .., N_t$. In this case, average match quality of workers who stayed with their employers for T periods depends on the best offer ever received out of $\sum_{\tau=t_0}^{\tau=T} N_\tau$ offers in total. If $g()$ is additive, then sum of aggregate conditions for the entire duration of the job, $\sum_{\tau=t_0}^{\tau=t_0+T} y_\tau$ could serve as a proxy for the total number of offers received by a worker, which is a sufficient statistic for the best wage offer, and, hence, for match quality.

This proxy, however, is not desirable in our view for two reasons. First, it does not distinguish between separations caused by workers leaving to better matches during expansions and separations caused by downturns, e.g. into unemployment, as discussed above. To see this, suppose each firm sampled $N_t = \tilde{g}(y_t)$ (with $\tilde{g}' > 0$) perishable production technologies every period and picked the most productive technology to operate with. Those who sample fewer technologies to choose from experience a decline in productivity on average. This will cause separation with low-quality

Altonji and Shakotko, 1987).

matches, raising the average match quality of those who stay. Since $\tilde{g}' > 0$, this selection will happen more frequently during recessions. Following the same argument as in the previous paragraph, the strength of this selection in match quality after T periods depends on the total number of technologies sampled, which can be proxied by, once again, the sum of labor market conditions $\sum_{\tau=t_0}^{\tau=t_0+T} y_\tau$. This complicates the interpretation of the proxy as it is possible that selection of match quality during upswings offsets selection during downturns, causing average match quality to be acyclical.

Our second concern is that this proxy constrains the estimated cyclicality of selection in average match quality by combining cyclical and non-cyclical components of selection. To see this, note that this proxy can be expressed as $T\bar{y}_{t_0,T}$: the total duration of the job times the average economic conditions between t_0 and $t_0 + T$. This distinction is important. The duration of the match, T, captures the notion that good matches survive longer on average (Abraham and Farber, 1987), regardless of the cyclical conditions in the economy that may or may not lead to further selection through separations. This implies a positive coefficient on job duration.[6] The coefficient on $\bar{y}_{t_0,T}$ could be either positive or negative depending on the relative strengths of selection during recessions versus expansions as in our first point. However, when estimated in combination with T, it is more likely to have a positive coefficient. Since the cyclicality of selection is crucial for explaining the patterns of history-dependence documented by Beaudry and DiNardo (1991), one needs to include duration and average market conditions separately in wage regressions. If average match quality in existing matches is indeed pro-cyclical, as claimed by Hagedorn and Manovskii (2013), then the coefficient on $\bar{y}_{t_0,T}$ should be positive.

In the empirical section, we estimate wage regressions with different combinations of the proxies for match quality described above and find no evidence for procyclical selection through separations. At the very least, one has to conclude that the particular choice of the proxy and the associated results obtained from the estimation of equation (1) are sensitive to the specification of the outside offer process and prone to multicollinearity problems. This leaves open the important question of whether new and more reliable tests can be developed to distinguish between the two different views of the labor market, which we turn to next.

1.3 Using wage growth of job stayers to distinguish between models

An important difference between the two models lies in the behavior of individual wages over time. In static markets with cyclical selection, individual workers' wages do not respond to the minimum unemployment rate. It is the change in the composition of jobs that makes wages seemingly de-

[6]Even though the significance of job duration in a wage regression does not imply the cyclicality of average job quality, by controlling for match quality, it helps address any omitted variable bias present in the estimates of other regressors.

pendent on past conditions. By contrast, in a contractual market with one-sided commitment, each worker's wage rate is adjusted upward when there is a reduction in the minimum unemployment rate since he started the job. The two models can, therefore, be distinguished by studying wage growth for workers who do not switch jobs.

To see the key idea, take the difference of equation (1) for stayers.

$$\Delta_k w_{ijt} = \theta_1 \Delta_k u_t + \Delta_k X_{ijt}\Lambda + \theta_3 \Delta_k u_{t_0,t}^{min} + \Delta\nu_{ijt}, \tag{4}$$

where k is the number of periods between two consecutive wage observations for the same worker on the same job. Since the unobserved worker and match quality effects are time-invariant, they disappear in the wage growth equation. Therefore, they can no longer cause a selection bias. Equation (4) encompasses all three models: the static labor markets ($\theta_3 = 0,\ \theta_1 < 0$), a contractual market with full-commitment ($\theta_1 = \theta_3 = 0$), and a contractual market with one-sided commitment binding on the employer ($\theta_1 = 0,\ \theta_3 < 0$).

The intuition is simple. Consider two workers with identical productivity: worker B who was hired when the unemployment rate was 4%, and worker R who was hired in the *subsequent* recession when the unemployment rate was 8%. In contractual markets with one-sided commitment, worker B maintains his wage rate over the recession, because he was insured against a possible downturn prior to the recession. As the economy recovers, the unemployment rate decreases to 6%. This reduces the minimum unemployment rate for R, but not for B. Consequently, R's wage rate increases while B's stays the same. If the labor market were static instead, both B and R would have the same wage rate, which decreases during the recession and increases during recovery following the current unemployment rate.

A potential problem with estimating wage growth regressions with job stayers is that the error term $\Delta_k\nu_{ijt}$ may be correlated with the variables of interest conditional on staying on the job. If matches with lower realizations of ν_{ijt} were discontinued, then observed wage growth of stayers would be biased upwards, presumably more so for low wage workers. If, for instance, there is a threshold productivity level, below which the match is dissolved, then workers with low productivity are more likely to hit this threshold. Given the correlation between starting wages and the unemployment rate, this type of non-random selection could lead to spurious history-dependence in wage growth even in a static labor market. This concern can be addressed by the Heckman correction procedure, which requires an exclusion restriction for robust identification. Under our assumptions, match quality does not enter equation (4), and hence, measures of match quality act as a valid exclusion restriction. We use total job duration. Those with a low match quality are more likely to switch jobs, therefore, have a shorter job duration, which, by construction, predicts the likelihood of staying on the same job.

1.4 Anticipated wage growth and cyclical selection of job stayers

The benchmark specification assumes the predictable component of match quality to be time-invariant. However, differences in the training contents of various career paths, the role of firm specific human capital, or varying degrees of moral hazard problems could lead to predictable variations in wage growth across jobs. Similarly, differences in the ability to accumulate human capital could lead to persistent differences in wage growth across workers (Haider, 2001; Guvenen, 2007). One might be concerned that such variation in wage growth, if anticipated, could affect workers' career decisions and lead to potential endogeneity in estimation of (4).

To study the implications in a more general model, assume the following specification for wages in a static market:

$$w_{ijt} = \theta_1 u_t + \beta_{ij} T_{ijt} + \phi_i X_{it} + a_i + m_{ij} + \nu_{ijt}, \tag{5}$$

where T_{ij} is job tenure and X_{it} is years of market experience.[7] The return to job seniority is specific to the match, and the return to experience differs across workers. A job is characterized by a match quality level, m_{ij}, and an anticipated return to tenure β_{ij}. Using equation (5), the wage growth in the general model is:

$$\Delta w_{ijt} = \theta_1 \Delta u_t + \phi_i + \beta_{ij} + \Delta \nu_{ijt}. \tag{6}$$

Equation (6) could be estimated directly. A fixed effects panel estimation would identify ϕ_i at the worker level and β_{ij} at the job level. This requires at least three wage observations per job. The estimation, thus, necessarily leaves out jobs with very short durations and workers who have just started their career.

Note that when job and worker specific components are ignored, the benchmark test would be biased towards the implicit contracts model only if ϕ_i and β_{ij} are correlated *negatively* with the change in the minimum unemployment rate. However, endogenous job search behavior does not suggest such correlation. In fact, the theory predicts that the average value of ϕ_i for job stayers is orthogonal to quit behavior, because the decision to switch jobs does not depend on worker-specific characteristics that are valued equally at all jobs. Moreover, anticipated wage growth β_{ij} is likely *positively* correlated with $\Delta u^{min}_{t_0,t}$, contrary to what one would expect in contractual markets. To see this, note that workers will quit their jobs if either the flow pay component, m_{ij}, or the anticipated growth component, β_{ij}, at their current job is low. Consequently, job stayers will have higher wages m_{ij}, and faster wage growth, β_{ij}, on average. Since workers who experienced favorable economic conditions faced more stringent selection constraints, the endogenous quit behavior thus implies that these workers must be working at jobs with higher β_{ij}'s on average. Moreover, they

[7] We abstract from higher order terms in experience and tenure in this section for simplicity. These terms are included in the empirical implementation of the tests below.

have a higher (less negative) change in the minimum unemployment rate, suggesting a *positive* correlation with average wage growth of stayers (β_{ij}). This is contrary to the predictions of the implicit contracts model: if a worker experienced more favorable conditions, he already receives a larger wage relative to productivity and, therefore, experiences a *lower* wage growth during expansions, suggesting a *negative* correlation with the change in the minimum unemployment rate.

1.5 Contracts with Two-Sided Lack of Commitment

The contractual models considered in Beaudry and DiNardo (1991) require the contract to be binding on the firm. Empirical tests for the more general case, when neither party can credibly commit, are not available in the literature. In this section we develop a new test that fills this gap.

In the more general case the contracted wage rate moves with productivity, only when the latter is altered substantially, and remains constant otherwise. The incidence and the extent of an adjustment depends on the wage of the worker relative to his productivity, and thereby, on the history of economic conditions. The form of this dependence, however, cannot be represented by extremum moments as done above with the minimum unemployment rate.[8] Nevertheless, an indicator for the start year and the current year pair, (t_0, t), summarizes all possible histories of economic conditions between t_0 and t, and, hence, is a sufficient statistic for the entire cross-sectional distribution of wages conditional on productivity. This observation allows for a more parsimonious test for the presence of general contractual arrangements.

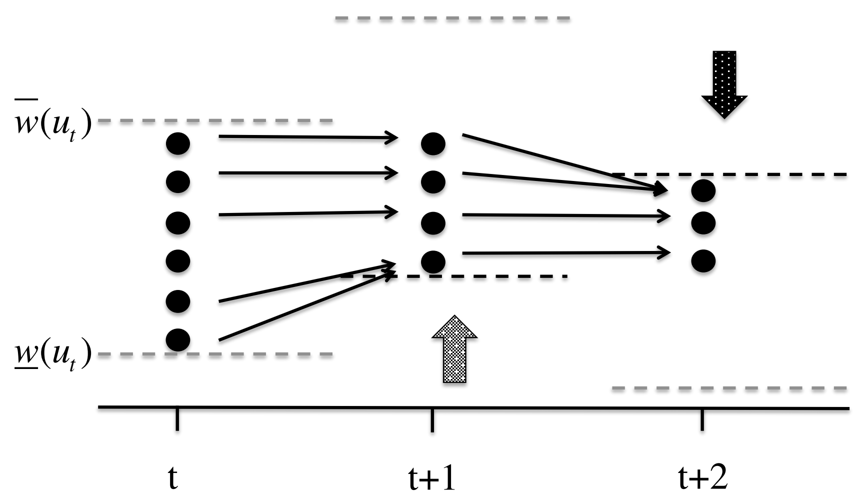
The nature of the optimal wage contract without commitment is better seen in Figure 1, which shows the wage behavior over a boom-bust cycle. The economy goes from an average state (a) to a boom (b) followed by a recession (r). The dashed lines show the worker's and the employer's reservation wages. Both are functions of current economic conditions. Wages lie between these two values at all times. The cross-sectional variation of wages conditional on productivity reflects different insurance premiums paid by workers with different labor market histories.[9]

When the economy expands from t to $t+1$, the participation constraint binds workers at the low end of the premium distribution, leading to an increase in their wages. Wages of workers with larger premiums remain unchanged. Similarly, when the economy enters a recession at $t+2$, the employer's participation constraint binds, especially for high wage workers. These workers receive wage cuts, while others are spared. Notice that, in both cases, the boom and the recession, the opti-

[8] One might be tempted to include the maximum unemployment rate since the start of the job in the regression, but this specification corresponds to the case where workers can be enslaved by employers who cannot credibly commit to keeping them, not to contracts with two-sided lack of commitment.

[9] Technically, an insured worker receives indemnity whenever his wage is higher than his productivity, and pays a premium otherwise. We use the term premium more generally to denote the gap between wage and productivity in contractual markets. It is understood that the premium can be negative.

Figure 1: Wages over a boom cycle in a contractual market without commitment



mal contract calls for a smaller wage increase for workers who receive larger insurance premiums over their productivity. Each worker's position in the cross-sectional distribution of premiums in turn depends on the past labor market conditions. Workers who were hired during expansions, or those who experienced more favorable conditions since they were hired, find themselves receiving higher wages conditional on productivity, and are, therefore, subject to lower wage increases in general. The wage growth, therefore, depends negatively on the last period's wage *conditional on productivity*. The test developed here exploits precisely this prediction of contractual markets.

To test whether wage differences are consistent with this prediction, the following equation is estimated:

$$\Delta_k w_{ijt} = \theta_1 \Delta_k u_t + \gamma W^p_{t-k} + \Delta_k X_{ijt}\Lambda + \Delta_k \nu_{ijt}. \tag{7}$$

W^p_{t-k} denotes the insurance component of wages. To identify this component, we first regress wages on the interactions of job-start year and lagged year indicators, $I(t_0 \times t-k)$, controlling for other variables in (7). Given the contemporaneous changes in economic conditions, the combination of start year and current year captures the entire history of economic conditions. This procedure is identical to estimating (7) by replacing W^p_{t-k} with the actual wage, W_{t-k}, and using a full set of indicators for all possible $\{t_0, t-k\}$ pairs as instruments for W_{t-k}. We use the TSLS estimate since it is more efficient.

The main advantage of this method is that it is robust to any contractual arrangement that can be defined over the history of economic conditions. Therefore, it encompasses the case where neither the employer nor the worker can commit to honoring the contract.[10] Another advantage is

[10]A similar strategy was employed in Beaudry and DiNardo (1995) to estimate the intertemporal elasticity of labor

that we do not need to rely on proxies, such as the unemployment rate or the market tightness, to capture economic conditions.

Essentially, W^p_{t-k} is the average wage in year $t-k$ of all workers hired in t_0 conditional on control variables. If the labor market were static, W^p_{t-k} would have to capture the cyclical differences in match-specific productivity. There is, however, no reason to believe that workers with higher match qualities should be subject to larger cuts in downswings, and small raises during upswings. If anything, one would expect the opposite. A positive correlation between match quality and subsequent wage growth is more likely, as indicated by observed measures of productivity, such as education. Therefore, the static model implies a non-negative value for γ, while a strictly negative value for γ would be consistent with contractual markets.

2 Data

The data come from the 1979 cohort of the National Longitudinal Survey of Youth (NLSY) for the years 1979 - 2008. NLSY is a panel that closely tracks workers' jobs, their start dates and end dates, making it ideal for our purposes.[11] We use the nationally representative cross-sectional sample for our estimations, and restrict the sample to workers of 21 years of age or older, working at least 15 hours a week and to jobs in the private sector. We exclude jobs that started before 1976, or before the respondent was 16 years old or if they were enrolled in school.[12] The resulting sample is fairly broad, including part-time jobs, workers with multiple jobs etc., and constitutes a conservative choice for testing for implicit contracts. In Appendix B, we show that the results are stronger for subsamples where contracts are more likely to be prevalent, such as workers with a single, full-time job.

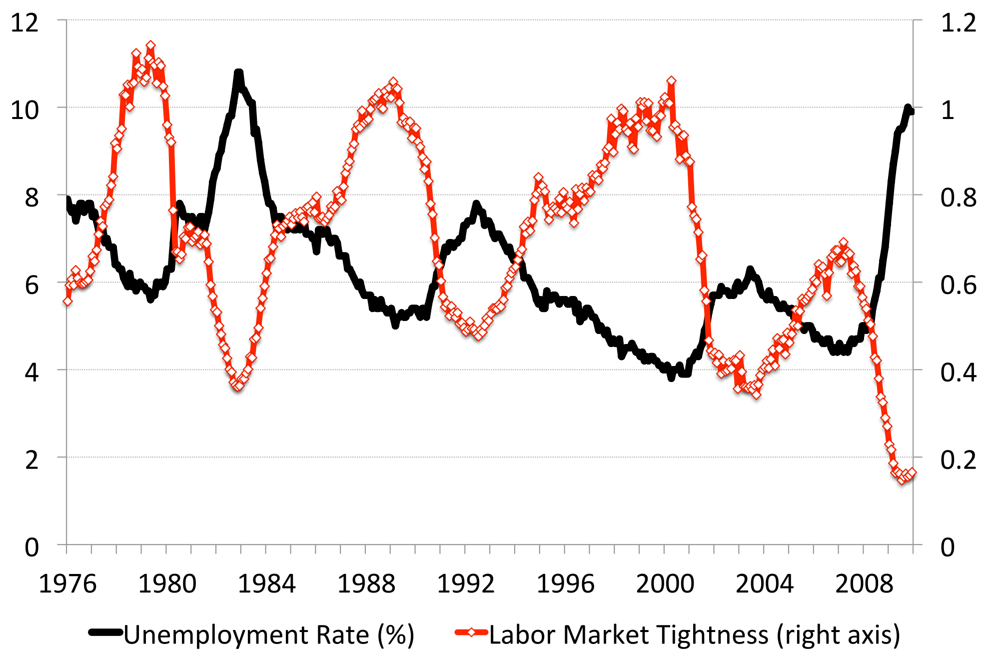
To measure the cyclical fluctuations in workers' outside option in the labor market, Bils (1985) and Beaudry and DiNardo (1991) rely on the unemployment rate. Hagedorn and Manovskii (2013) follow except in constructing their proxies for match quality, where they use the more recently available labor market tightness measures. To be able to compare our results, we too use the unemployment rate to construct variables pertaining to the implicit contracts theory, but use the labor market tightness to construct our proxies for match quality. This choice has no bearing on our results (see Appendix B.1). The statistics for the monthly unemployment rate come directly from the BLS. The monthly labor market tightness series combines the direct measures from the JOLTS data with the Help Wanted Index from the Conference Board (see Appendix A for details). Figure 2 shows the unemployment rate and the labor market tightness during the sample period,

supply in contractual markets.

[11] Another panel used in the literature has been the PSID, however it is much harder to identify the job switches in the PSID.

[12] See Appendix A for further details on the construction of variables and sample selection.

Figure 2: Cyclical fluctuations in the labor market



which starts at the onset of the 1981 recession, and includes three full cycles, providing substantial variation for the estimations.

3 Cyclical behavior of wages: Testing for implicit contracts

We begin the analysis by documenting the history dependence in wage levels. The variables of interest are the contemporaneous unemployment rate, the unemployment rate when the worker started his current job, and the minimum unemployment rate since the beginning of his current job. The control variables included in the regression are individual fixed effects, cubic polynomials in tenure and experience, and indicators for region and industry. A quadratic time trend is also included to capture any long term relation between average wages and the unemployment rate.

Table 1 shows the effect of past labor market conditions on wage levels. When regressed only on the contemporaneous unemployment rate, wages appear to be strongly procyclical. An increase in the unemployment rate by 1 percentage point is associated with a 1.52% decline in wages. When the initial unemployment rate is introduced, however, the coefficient on the contemporaneous unemployment rate substantially declines, and eventually becomes statistically insignificant in column 3 when the minimum unemployment rate is included in the regression. The specification in column 4 contrasts all three models at once. The past unemployment rates are not only statistically but also quantitatively important. On average, wages decline by 0.75% in response to the unemployment rate at the time of hire, and by 2.82% in response to the minimum unemployment

Table 1: Real Wages and Unemployment History

	(1) $\log w$	(2) $\log w$	(3) $\log w$	(4) $\log w$
U_t	-1.52^{***}	-0.94^{***}	0.25	0.04
	(0.22)	(0.17)	(0.20)	(0.21)
U_{t_0}		-2.05^{***}		-0.75^{**}
		(0.18)		(0.26)
U_t^{min}			-3.70^{***}	-2.82^{***}
			(0.34)	(0.44)
N	84,131	84,131	84,131	84,131

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note.— All specifications control for individual fixed effects, cubic polynomials in experience and tenure, a quadratic time trend, and indicators for industry and region. Data comes from the 1979 cohort of the NLSY. Coefficients and standard errors are multiplied by 100. Standard errors are clustered by start year and current year interactions and shown in parentheses.

rate since then. Assuming a 6 percentage point difference in the initial unemployment rate between peak and trough, this implies a 21.4% (=6*(0.75+2.82)) gap in starting wages. The effect of contemporaneous unemployment rate, on the other hand, is virtually zero.

Overall, the estimates confirm the marked history-dependence in wages documented in earlier studies. The coefficient on the minimum unemployment rate was estimated as -2.9% in Beaudry and DiNardo (1991), and -2.5% in Grant (2003). The coefficients on other variables are also similar, with the exception that Grant (2003) finds a stronger effect for the contemporaneous unemployment rate in his sample.[13] Our findings are also consistent with Bils (1985) who finds the wages of job stayers to be acyclical, while the wages of newly hired workers to be highly procyclical.

3.1 Is there cyclical selection in match quality?

Could the patterns in Table 1 be explained by pro-cyclical selection in unobserved match quality? This section attempts to answer this question by including proxy variables for match quality in the wage regression.

The results are shown in Table 2. The first column includes the log-sum of job duration and average labor market tightness from the start to the end of the job as in Hagedorn and Manovskii

[13]The estimates in Beaudry and DiNardo (1991) come from the PSID (1976 - 1984), and those in Grant (2003) use NLSY (1979 - 1998).

(2013) and confirms their result with an elasticity of 6.04%. Second column regresses wages on these measures separately. The elasticity of wages to job duration is 5.48%. The coefficient on average tightness is also significant at 4.38% suggesting that endogeneity of separations lead to a procyclical selection in match quality. The third column replaces average tightness by minimum and maximum tightness levels ever experienced on the job. This allows for a distinction between selection in match quality during downturns, as bad matches dissolve with workers likely becoming unemployed, and during upswings, when badly matched workers quit for better jobs as argued in Hagedorn and Manovskii (2013). The estimates suggest significant selection effects at both times, with a stronger degree of selection during upswings resulting in procyclical match quality overall. All of these findings are overturned when we allow for contractual adjustments to wages.

Columns 4 to 6 include the initial unemployment rate and the minimum unemployment rate between the start of the job and the time the wage is observed in addition to the proxy variables for match quality. In column 4, the coefficient on the minimum unemployment rate is estimated as -1.35%, roughly half the estimate in the last column of Table 1. In this specification the unobserved match quality appears procyclical and partially (but not fully) accounts for the history-dependence in wages, which was originally thought to corroborate the theory of dynamic contracts in the labor market.[14] A different result emerges when job duration and average tightness are included in the regression separately. While the coefficient on job duration remains significant at 5.29%, the coefficient on average tightness becomes negative(!) at -2.55%, albeit statistically insignificant, suggesting that selection in match quality is acyclical, if not counter cyclical. Furthermore, the coefficient on the minimum unemployment rate is -2.40%, close to -2.82% reported in Table 1 where proxy variables are excluded from the regression. This shows that job duration is far more important than average tightness in understanding the selection effects in match quality . By combining the two proxies, the specification in column 1 assigns a highly positive coefficient on average tightness on account of the relevance of job duration.[15] The last column further confirms the presence of a strong countercyclical selection in match quality, driven by dissolution of bad matches during downturns.[16]

[14]In a similar regression, Hagedorn and Manovskii (2013) find the coefficient on the minimum unemployment rate to be statistically zero, and conclude against the presence of wage contracts. This difference between our results arise from differences in samples as our data includes more recent waves of the NLSY and in the construction of the minimum unemployment rate. In Appendix C, we reconcile the differences by first replicating their sample in our data and then running our regression specifications in their data. Both approaches confirm our conclusions in this section.

[15]In fact, the correlation of the combined proxy $\log(duration \times \bar{\theta}_j)$ with job duration is 98.5% whereas the correlation is 7% with average tightness.

[16]In the appendix to Hagedorn and Manovskii (2013), the authors test the sensitivity of their results to the separation of job duration and average tightness in two steps. First, they estimate $\hat{\Gamma}$ by running $\log w = \beta_1(\log duration + \log \bar{\theta}_j) + X\Gamma$, where X includes all the other variables. Then they regress $\log w - \hat{\Gamma} X$ on $\log duration$ and $\log \bar{\theta}_j$ separately. By giving the first pass to $\log duration + \log \bar{\theta}_j$, this approach favors the specification where the two proxies are combined together.

Table 2: Real Wages, Unemployment History and Proxies for Match Quality

	(1) $\log w$	(2) $\log w$	(3) $\log w$	(4) $\log w$	(5) $\log w$	(6) $\log w$
U_t	-0.89^{***}	-1.07^{***}	-1.12^{***}	-0.01	-0.43^{*}	-0.65^{**}
	(0.18)	(0.22)	(0.20)	(0.19)	(0.20)	(0.20)
U_{t_0}				-0.92^{***}	-0.93^{***}	-0.90^{**}
				(0.23)	(0.24)	(0.27)
U_t^{min}				-1.35^{***}	-2.40^{***}	-2.09^{***}
				(0.36)	(0.42)	(0.42)
$\log(duration \times \bar{\theta}_j)$	6.04^{***}			5.73^{***}		
	(0.27)			(0.26)		
$\log(duration)$		5.48^{***}	3.31^{***}		5.29^{***}	4.16^{***}
		(0.25)	(0.32)		(0.25)	(0.34)
$\log \bar{\theta}_j$		4.38^{***}			-2.55	
		(1.24)			(1.42)	
$\log \theta_j^{min}$			-6.53^{***}			-5.63^{***}
			(1.08)			(1.02)
$\log \theta_j^{max}$			13.57^{***}			3.69
			(1.53)			(2.07)
N	84,131	84,131	84,131	84,131	84,131	84,131

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes.– All specifications control for individual fixed effects, cubic polynomials in experience and tenure, a quadratic time trend, and indicators for industry and region. Data comes from the 1979 cohort of the NLSY. U_t, U_{t_0}, U_t^{min} are contemporaneous, initial and the minimum unemployment rate experienced since the start of the job until year t. $\bar{\theta}_j$, θ_j^{min}, θ_j^{max} denote the average, minimum and maximum labor market tightnesses *ever* observed during job spell j. Coefficients and standard errors are multiplied by 100. Standard errors are clustered by start year and current year interactions and shown in parentheses.

Taking all the results in Table 2 together, we conclude that average match quality in continuing jobs is somewhat countercyclical and the evidence on implicit insurance contracts are robust to selection effects in match quality. In Appendix B, we show that the latter conclusion is robust to various sampling restrictions. However, countercyclicality of unobserved match quality is sensitive to the inclusion of part-time jobs (those with 15 and 35 hours of work a week). Among full-time workers, we find no evidence for selection in match quality through separations.

3.2 Wage growth and labor market history

Next, we turn to the wage growth of job stayers to disentangle the two models of the labor market. In the benchmark test, wage growth is regressed on the change in the current unemployment rate and the change in the minimum unemployment rate over a worker's tenure. The control variables included in the regression are differences in cubic polynomials in tenure and experience, in a quadratic time trend, and indicators for industry and region.

Table 3 presents the estimation results. Wage growth does not respond to changes in current economic conditions. If the labor market were static, then wage growth would be strongly procyclical. It is instead acyclical and displays significant history dependence. The coefficient of the minimum unemployment rate in Table 1, -2.82%, remains significant at -2.02% in Table 3 for job stayers.[17] In fact, the estimate is very close to -2.09%, the estimate obtained when our proxies for match quality are included in the regression (see last column in Table 2). This implies that the dependence of wages on the minimum unemployment rate in Table 1 was mostly due to contracts.

These findings are at odds with a pure spot market model of wage determination. Instead, they are consistent with a contractual market, where wages are adjusted whenever the worker's outside option binds.

Note that the findings do not refute on-the-job search. They simply indicate that endogenous separations are not a main source of cyclical variation in average match quality for existing matches, and they can not explain the significance of the minimum unemployment rate in Table 1.

The findings do not, however, entirely rule out potential changes in the composition of job quality over the business cycle. Cyclical movements in average match quality may still arise from differences in the job quality of newly hired workers. The specification in (4) does not allow us to gauge the quantitative significance of this directly since the initial unemployment rate is differenced away. Nonetheless, the extent of cyclical variations in job quality of new hires can be gauged by examining how much and how fast the dispersion in wages due to time-of-entry effects

[17] One might be concerned that these estimates are based on different samples. When we estimate the last column in Table 1 using the sample of job stayers only, we obtain a coefficient of -3.02 (s.e. 0.47) for the minimum unemployment rate and -0.56 (s.e. 0.29) for the initial unemployment rate, which are similar to -2.82 and -0.75 respectively. This is not too surprising since job switchers do not contribute to the identification of U_{min} in Table 1.

Table 3: Real Wage Growth and Unemployment History: Job Stayers

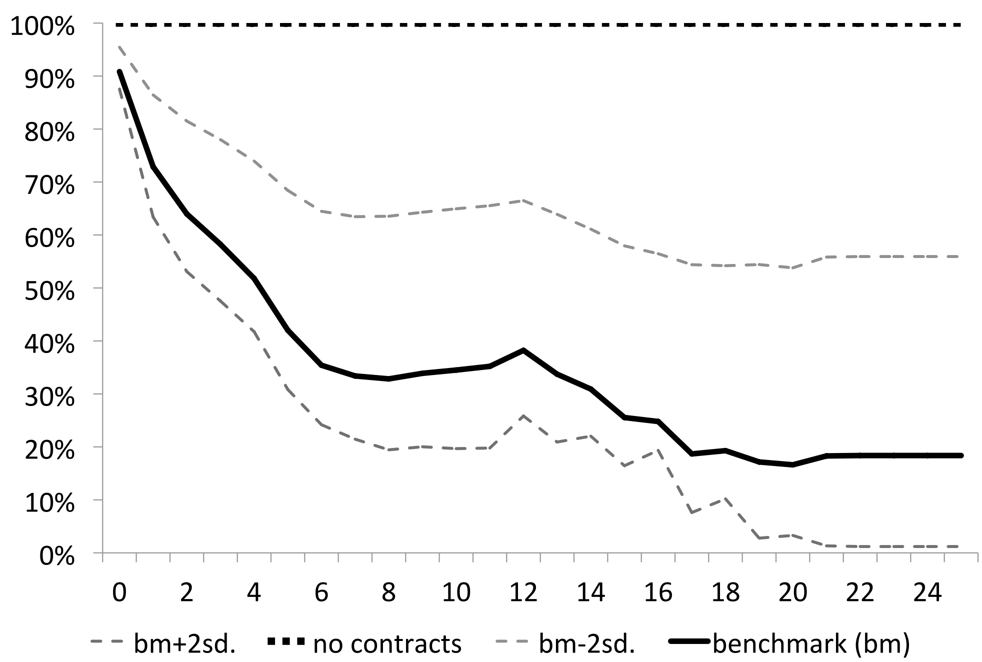
	(1) $\Delta \log w$	(2) $\Delta \log w$	(3) $\Delta \log w$	(4) $\Delta \log w$	(5) $\Delta \log w$
ΔU_t	-0.31	0.07	0.07	0.19	0.07
	(0.17)	(0.21)	(0.21)	(0.21)	(0.20)
ΔU_t^{min}		-2.02^{***}	-2.18^{***}	-2.06^{***}	-1.78^{***}
		(0.56)	(0.58)	(0.59)	(0.61)
Inverse Mills Ratio			-2.59^{**}		
			(0.90)		
		First Step Probit Estimation			
Job Duration			15.99^{***}		
			(5.05)		
Worker fixed effects	no	no	no	yes	yes
Job fixed effects	no	no	no	no	yes
Sample size	47,360	47,360	64,744	47,360	47,360

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note.– All specifications control for differences in cubic polynomials of experience and tenure, differences in a quadratic time trend, and indicators for industry and region. Estimates in the last column is corrected for non-random sample selection using the Heckman correction procedure. Data comes from the 1979 cohort of the NLSY. Sample includes job stayers only. Coefficients and standard errors are multiplied by 100. Standard errors are clustered by start year and current year interactions.

fades out with tenure. If contracts were irrelevant, then time-of-entry effects would only reflect differences in match quality, which are persistent by definition. As a result, wage dispersion at the time of entry would be permanent. With contracts, however, wages are updated as the market improves. Consequently, initial differences in wages disappear as all workers' eventually experience favorable conditions on the job.[18] This can be implemented by, first, computing the differences in wages at the time of hire using the estimates in the last column of Table 1, and then simulating an entire wage sequence for each worker by adding the predicted on-the-job wage growth estimated from the one-sided commitment model in column 2 of Table 3. Given the estimates, predicted initial wage is: $\hat{w}_{i,0} = -(0.75 + 2.82) * U_{i,t0}$ as initially $U_{t0} = U^{min}_{t0,t}$. Consequently, the predicted wage after $\tau = t - t_0$ years on the job is $\hat{w}_{i,\tau} = \hat{w}_{i,0} - 2.02 * (U^{min}_{i,t0,t} - U_{i,t0})$. Denote the variance of the predicted wages for tenure year τ by ν^{bm}_{τ}, where bm refers to the benchmark estimate. The hypothetical scenario without contracts can be computed by setting $\theta_3 = 0$ instead of the estimate of -2.02. Denote this hypothetical variance series by ν^0_τ. If the time-of-entry effects were driven *only* by changes in job quality so that contractual variations did not matter, then the relative dispersion of the simulated wage series ν^{bm}_τ/ν^0_τ would remain at 1. If, instead, the variation in starting wages entirely reflects differences in contractual terms, then it should fade away over time to 0 with tenure.

Figure 3: Persistence of time of entry effects in wages



The solid line in Figure 3 shows the relative variance of predicted wages by job tenure. The

[18]Since the stochastic process for the unemployment rate is stationary and bounded from below, the minimum unemployment rate converges for all workers.

dashed series correspond to predicted variances using estimates of the effect of the minimum unemployment rate on the wage rate that are two standard deviations below and above the benchmark estimate: -2.02$\pm$0.56 (see 3). The variance of the predicted wage series declines by 80% over time, indicating that only about a fifth of the time-of-entry effects is, in fact, due to variations in average job quality. Even with an elasticity that is two standard deviations below the benchmark estimate, roughly half the variation dies out.[19]. We conclude that the time-of-entry effects in wages reflect in large part variations in contractual terms rather than permanent differences in match quality.

3.3 Are job stayers special?

One might be worried that focusing on job stayers exposes the test to a potential sample selection bias. It is possible, for instance, that low-wage workers who experience negative productivity shocks quit their jobs, leading to higher observed wage growth among stayers. Same may not apply to high wage workers if they are further from their reservation wage. Note that this generally affects the estimated intercept in the wage growth equation but is not a concern for the estimated elasticity to U_{min}, our main variable of interest, unless selection of stayers is somehow history dependent. In particular, it would be a concern only if, given the contemporaneous conditions, selection were more stringent for people who, for historical reasons, had a larger decline in their minimum unemployment rate.

We address this concern by correcting our estimates for sample selection using a two-step Heckman selection model, where total job duration (completed tenure), a natural proxy for match quality, used as an exclusion restriction. Notice that job duration is constant within a match, and, therefore, it does not affect wage growth by construction under the null hypothesis, but it predicts the probability of staying on the job by construction. Those with longer completed job duration were less likely to switch jobs each period.

The third column in Table 3 shows the estimation results. In the first step, job duration has a significant and positive effect on the probability of staying on the job as expected. Moreover, when job duration is controlled for, the probability of staying on the job is not related to the change in the minimum unemployment rate and the change in the contemporaneous unemployment rate.[20]. The coefficient on the inverse mills ratio is -2.59 and is significant, implying that job stayers have a higher wage growth on average. However, the coefficient of the change in the minimum unemployment rate remains virtually unchanged at -2.18, implying that such selection is not related to the history of economic conditions.

[19]To address possible issues that may arise from attrition by tenure, we repeated the figure for jobs that lasted at least 10 years. The time-of-entry effects for these jobs decline faster, converging similarly to about 20% of its initial value after 15 years.

[20]The coefficient on ΔU_t in the first step is 4.2 (s.e. 7.8) and that on ΔU^{min} is 6.9 (s.e. 16.9)

3.4 Anticipated wage growth

The benchmark test abstracts from potential permanent variation in wage growth specific to a worker or to a match. Ignoring this variation would lead to an endogeneity problem if survival of jobs were dependent on anticipated wage growth. This concern can be addressed by including worker and job fixed effects in our regressions. The last two columns in Table 3 shows the results.

When fixed worker effects are included, the coefficient on the change in the minimum unemployment rate remains the same. This is consistent with our conclusion in section 1.4 on the effect of experience on selection: since market experience is rewarded equally at all jobs, endogenous survival of jobs does not lead to a selection effect in worker-specific wage growth. In the last column, where we control for job fixed effects, the coefficient on the change in the minimum unemployment rate is -1.78, indicating that the anticipated wage growth is not a major factor driving the results. Meanwhile, the coefficient on the contemporaneous change in the unemployment rate is close to zero and insignificant in all of the specifications in Table 3.

3.5 Job training and the cyclicality of human capital investment

An important component of a worker's wage is their human capital. Could a systematic variation in human capital accumulation over the business cycle explain our findings? Unlikely. Generic models of on-the-job training (e.g. Ben-Porath (1967)) predict countercyclical investment in training: since wages are generally procyclical, it is rational to invest in human capital during recessions, and work during booms. But then workers who are hired during booms, and those who experience favorable market conditions on-the-job would have accumulated less human capital, leading to *lower* wages. In addition, since there are decreasing returns to human capital investment, these workers would also experience *faster* wage growth relative to those hired in recessions. Both of these predictions are in contrast with our findings and the implications of the implicit contracts model. Nonetheless, one could argue for a model with procyclical job training. If, for instance, the employer bears the costs of training, then potential liquidity problems during recessions may lead to lower training activity.

To empirically evaluate the implications of training and human capital for our findings, we directly control for training activity using the available measures in the NLSY. The NLSY questions workers on the amount of time spent on training activities since the last time the worker was interviewed. Based on the responses, we constructed two variables: total hours of training activity between two wage observations, and the total cumulative amount of training since the worker first entered the labor market. Although the training measures are imperfect, as probably most informal training activity goes unrecorded, we think that the available measures could give us an idea about the plausibility of a human capital explanation of our results.

Overall, controlling for training does not change the findings. In the wage level regression, the coefficient on the initial unemployment rate is -0.90 (s.e. 0.27) and the coefficient on the minimum unemployment rate is -2.19 (s.e. 0.43). The return to a year of job training is 7.6%. In the wage growth regression, the coefficient on the change in the minimum unemployment rate is -2.16 (s.e. 0.59), similar to the benchmark estimate of -2.02 reported in Table 3.[21] Based on these results, we conclude that our empirical findings are not likely to be driven by cyclical fluctuations in human capital or training activity.

4 Contracts with Two-Sided Lack of Commitment

We now generalize our test to contractual arrangements where neither the worker nor the employer can fully commit to the contract. We examine whether the the insurance premium that the worker receives (or pays) has a negative impact on subsequent wage growth. This is essentially a test of mean-reversion in the insurance premium. Since, in contractual markets, insurance premiums across workers are dispensable when the participation constraints bind, workers who start in a disadvantaged position catch up with other workers during expansions. Similarly, those with initial wage advantages lose them in severe downswings.

To implement our idea, we regress wage growth on the lagged wage rate and use the full set of history indicators as instruments for the lagged wage rate. To control for changes in the economic conditions between two consecutive interviews, we control for a full set of interactions of indicators for the current year, t, and the last interview year $t-k$.

Table 4 shows the results. As before, we also control for differences in cubic polynomials of experience and tenure, in a quadratic time trend, and indicators for industry and region. The estimates for the constant-match-quality model indicate that a worker who enjoys a 1% lower wage rate, for instance, because he was hired in a recession, enjoys a 0.09% larger wage growth on average. This confirms our earlier conclusion. If the wages were determined in a static market, the variation in wages predicted by the history indicators would correspond to real productivity differences between jobs. It is not obvious, however, why those with better jobs should have lower wage growth on average. On the contrary, one would expect larger wage raises in good matches, for instance due to increased investment in job specific capital (Becker, 1964). The positive correlation between education and wage growth usually observed in the data is supportive of this hypothesis.

The third column adds worker fixed effects in wage growth. The coefficient of lagged wages declines to -0.22. This is consistent with a positive correlation between the starting year (and the history thereafter), and the worker-specific wage growth. This might seem to be at odds with the insensitivity of the results in Table 3 to the inclusion of worker fixed effects, however this

[21] A table of estimates can be found in the appendix

Table 4: Real Wage Growth and No-Commitment Contracts

	(1) $\Delta \log w$	(2) $\Delta \log w$	(3) $\Delta \log w$
ΔU_t	-0.37^{*}	-0.14	-0.15
	(0.17)	(0.14)	(0.15)
$\log w_{-1}$	-0.09^{***}	-0.22^{***}	-0.38^{***}
	(0.02)	(0.02)	(0.03)
Worker fixed effects	no	yes	yes
Job fixed effects	no	no	yes
N	47,560	47,560	47,560

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note.— TSLS estimates where the last observed wage, $\log W_{t-k}$, is instrumented by a full interaction of start year and last year indicators, $I(t_0 \times t - k)$. All specifications also control differences in cubic polynomials of tenure and age, indicators for region and industry. Data comes from the 1979 cohort of the NLSY. Sample includes job stayers only. Standard errors are clustered by start year and current year interactions.

discrepancy simply indicates that the correlation between worker-specific wage growth and start year is not related to the changes in the cyclical conditions, as measured by the change in the minimum unemployment rate. It suggests that workers with higher (or lower) persistent wage growth have similar job histories.

When we add job fixed effects, the coefficient on lagged wage further declines to -0.38. Since the gap between two interviews varies in the data, this figure is not per annum. Given the distribution of time lags in our sample between two consecutive interviews, the annual change corresponds roughly to 80% of the estimated coefficient. A worker who was paid a one percent higher premium because of the history of economic conditions on the job, therefore, enjoys about 30% lower wage growth on average per year. This implies that 76% ($= (1 - 0.3)^4$) of the time-of-entry effects on starting wages fade out within 4 years, more than our earlier estimate of 50% in Figure 3 when we ignored persistent variations in wage growth by worker and by job. This is consistent with the hypothesis that workers that are hired during expansions not only have better jobs, but also better prospects for wage growth.[22]

These findings confirm the earlier result that the time-of-entry effects in wages fade out rel-

[22] We also estimated our model using the initial and the minimum unemployment rate as instruments for lagged wage. The coefficient on the lagged wage rate is -0.10 (s.e. 0.03) comparable to -0.09 in Table 4. When worker and job fixed effects are included sequentially, the estimates are -0.13 (s.e. 0.03) and -0.29 (s.e. 0.04), lower than -0.22 and -0.38 reported in Table 4. Therefore, when worker and job-specific wage growth profiles are taken into account, downward adjustments in the wage rate in response to adverse economic conditions (apparently in the form of lower wage raises) is an important feature of the contractual arrangements in the data.

atively quickly. In our view, the theory of self-enforcing insurance contracts provides a natural interpretation of this result. Differences in the economic conditions when a contract is signed lead to job-cohort effects in wages. As the economic conditions fluctuate, wages are updated to reflect workers' and firms' outside options so as to prevent separation. Since the outside options are forward looking, the initial differences disappear.

5 Discussion

The results show that wage adjustments over the business cycle show significant dependence on past economic conditions. By contrast, changes in contemporaneous conditions do not have a significant effect on wage growth when past labor market conditions are controlled for. This is at odds with a static model of the labor market. The particular history-dependence observed in wage growth is consistent with a contractual labor market, where employers and workers partake in an implicit agreement to shield wage payments from fluctuations in a worker's marginal product, without fully committing themselves to future payments and work.

The results also provide an insight into the cyclical workings of on-the-job search behavior. The findings imply that the relative job quality of stayers to leavers remains roughly stable over the business cycle. This suggests that the cyclical movements in average match quality occur mostly through newly hired workers. Our calculations indicate that about 20% of the history dependence in wages can be explained this way. By contrast, we find no evidence for procyclical movements in average match quality through endogenous separations. The results suggest that poor matches are equally, if not more, likely to dissolve during recessions than they are during expansions. This is also in line with Mustre del Rio (2012), who recently finds that jobs that end during recessions are of lower duration on average, an indicator of low job quality. This suggests a cyclical pattern in the on-the-job search behavior. Nonetheless, the finding disappears when fixed worker characteristics are controlled for in the analysis, implying that such cyclicality is specific to the worker and not to the match per se.

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APPENDIX

A Data

The analysis focuses on male respondents in the cross-sectional sample, who at the time of the interview were not enrolled in school and were employed.

Wages: The wage is the hourly rate of pay constructed by the NLSY. Nominal wages are deflated using the annual CPI index (All Urban Consumers, U.S City Average, All Items) from the Bureau of Labor Statistics (base period 1982-84). Wages were deflated using the CPI of the year when the worker last worked for the job as reported at the time of the interview. Observations with missing wage information or real wages below $1 and above $100 are dropped.

Hours: These are the usual weekly hours worked. Observations with missing information on hours were dropped. The sample includes only full-time workers (usual weekly hours of 35 or more).

Class of the job: The sample includes workers in the private sector only, thus dropping government employees, self-employed and those working without pay.

Industry Classification: The NLSY has employed the 3-digit 1970 and 1980 Census classification system in the 1979-2000 surveys in order to code all jobs into industry groups. Beginning 2002, the 3-digit 2000 Census codes were used to classify industries of all jobs reported by the respondents. To minimize potential inconsistencies or the effect of coding changes due to switching from the 1970/1980 to 2000 classification system for respondents who did not change jobs between consecutive interviews, 9 broader industry groups are defined based on the reported industry classification. The groups are: Agriculture, Forestry and Fisheries; Mining; Construction; Manufacturing; Utilities, Transportation and Warehousing; Wholesale and Retail Trade; Finance, Insurance, Real Estate, Rental and Leasing; Professional, Scientific, Technical Services, Management, Administrative and Waste Management Services, Educational Services, Health Services, Accommodation and Food Services, Arts, Entertainment and Recreation, Other Services; Public Administration

Job start date: The starting date of the job is identified by subtracting tenure (constructed by the NLSY and measured in weeks) from the date the worker last worked for the job as reported at the interview date. Jobs that started prior to 1976 are disregarded.

Current age: The current age corresponding to each job observation is constructed as the difference between the year the worker last worked at the job as reported at the time of the interview and the birth year. The age at the start of the job is calculated as the difference between the start year of the job and the birth year of the respondent. We only consider jobs that started when the respondent was 16 or older. Moreover, we restrict attention to workers with current age 21 years old and above.

Experience: This is actual experience measure in weeks constructed by adding for consecutive interviews the "total number of weeks the respondent worked since the last interview". This variable is constructed by the NLSY for all respondents of ages 16 years old and above. The results are very similar to the usage of current age at each job observation as a measure of potential experience.

Unemployment rate: The unemployment rate is the monthly, seasonally adjusted, civilian unemployment rate for ages 16+ obtained from the Bureau of Labor Statistics. The contemporaneous unemployment rate is the unemployment rate at the date (month, calendar year) when the respondent reported last working for the job. The initial unemployment rate corresponds to the unemployment rate at the date (month, calendar year) the job started. The minimum quarterly unemployment rate in the wage growth specifications is calculated as the historical minimum unemployment rate recorded between the date (month, calendar year) the job started and the last interview date (month, calendar year) before the contemporaneous year. All specifications are robust to the usage of quarterly instead of monthly unemployment.

Labor market tightness: The constructed series uses the number of vacancies per 1000 unemployed from the JOLTS starting in December 2000. For years prior to 2004, the labor market tightness is computed by dividing the help wanted ad index from the Conference Board by the number of unemployed. The two series overlap for years 2000 to 2003. A consistent series was constructed by projecting the tightness measure from the JOLTS on the tightness measure from the Conference Board for 2000-2003, and then by extrapolating backwards. The projection equation is $\theta_{JOLTS} = 0.12 + 58.1 \times \theta_{CB}$ with a correlation coefficient of 0.98.

Training variables: At every survey respondents were asked if they had participated in any training programs since the previous interview. Detailed information, then, were collected on the duration, intensity and the type of the training spells. The training data used in our estimations cover 1979 to 2004.The earlier surveys, 1979 to 1986, do not provide these details for training spells that lasted less than a month. For longer spells, the respondents reported the beginning and ending dates of each training spell (in month and year) and the average number of hours a week spent for training. This enables a construction of the total time investment in training in hours since the last interview. If the respondent was currently enrolled in a training program, an additional dummy variable was created. Until 1988, up to three training spells were recorded. Later this limit was raised to four. The respondents were however asked if they had fourth (fifth after 1986) training program to report. Based on this question, it is possible to calculate the number of workers for which this limit was binding. The limit was binding for a total of only 80 observations (about 0.2% of the sample) in all years.

B Robustness of the Results to Sample Selection

In this section, we investigate the sensitivity of the results to sampling restrictions. Table 5 reports the estimates from the main specification where the sum of (log) job duration and average tightness are included as a proxy for match quality along with the unemployment measures. This specification corresponds to the fourth column in Table 2. The results for the benchmark sample used in the text are reported in Column 5. Specifications to the left of Column 5 are less restrictive than the benchmark sample, and those to the right are more restrictive (see table notes for detailed sampling restrictions). The proxy for match quality and the minimum unemployment rate are significant in all of the specifications in Table 5. The estimated elasticity of wages to the minimum unemployment rate is second lowest in Column 5, indicating that the results in the main text are conservative. In more restricted samples, e.g. full time workers with only one job at the time of the interview, the evidence for contractual variation in wages is stronger. The initial unemployment rate, however, becomes insignificant when multiple job holders and part-time jobs are excluded from the sample. This suggests that the market for full-time jobs is best described by contracts where workers do not commit to employment but firms do.

Table 6 reports estimates when job duration and average tightness are included separately in the regressions. The two primary results discussed in the main text are observed in all of the samples. The coefficient on average tightness is never positive (and sometimes negative), confirming our first conclusion that separations do not lead to procyclical variation in average match quality. Second, the coefficient on the minimum unemployment rate is consequently smaller than the figure in corresponding sample in Table 5 in all of the columns.

Table 7 replaces average tightness with minimum and maximum tightness measures during the entire duration of the job. This specification is comparable to the last column in Table 2, replicated in the fifth column here. That the minimum unemployment rate is significant and the maximum tightness is not can be seen in all of the samples. The coefficient on the minimum tightness measure is significant in more general samples, but insignificant once multiple job holders and part-time jobs are excluded from the benchmark sample. This indicates that separations, be it during downturns or upswings, are not cyclical in the core full-time sample.

B.1 Tightness versus Unemployment Rate

The original paper by Beaudry and DiNardo (1991) used the unemployment rate to gauge the cyclical conditions in the labor market whereas Hagedorn and Manovskii (2013) use the concept of labor market tightness. In principle, one could redefine measures of contractual variation using labor market tightness instead of the unemployment rate. To that end, we computed the maximum tightness between the time of hire, t_0, and the time of the wage observation $t \in \{t_0, .., T\}$ to replace

Table 5: Robustness of the Results to Sampling Restrictions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\log w$	$\log w$	$\log w$	$\log w$	$\log w$	$\log w$	$\log w$	$\log w$
U_t	-0.08	-0.06	-0.01	-0.04	-0.01	-0.08	-0.15	-0.17
	(0.33)	(0.24)	(0.24)	(0.20)	(0.19)	(0.24)	(0.26)	(0.26)
$\log(duration \times \bar{\theta}_j)$	6.96^{***}	6.71^{***}	6.16^{***}	5.88^{***}	5.73^{***}	6.12^{***}	4.47^{***}	3.97^{***}
	(0.35)	(0.32)	(0.32)	(0.29)	(0.26)	(0.33)	(0.43)	(0.45)
U_{t_0}	-0.06	-0.73^{*}	-0.89^{**}	-0.93^{***}	-0.92^{***}	-0.60^{*}	-0.23	-0.23
	(0.36)	(0.29)	(0.30)	(0.25)	(0.23)	(0.31)	(0.37)	(0.37)
U_t^{min}	-2.81^{***}	-1.85^{***}	-1.67^{***}	-1.17^{**}	-1.35^{***}	-2.28^{***}	-2.73^{***}	-2.87^{***}
	(0.61)	(0.44)	(0.44)	(0.38)	(0.36)	(0.49)	(0.53)	(0.54)
N	115,314	96,197	88,257	87,417	84,131	57,563	44,832	40,810

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Samples.– All restrictions are added cumulatively. The fifth column corresponds to the benchmark sample used in the main text. (1) Basic sample: all workers over the age of 21 and all jobs that started in 1976 or later and where the respondent was at least 16 years of age at the time of hire. (2) drops jobs outside the private sector. (3) drops observations where the respondent was enrolled in school. (4) drops wages less than $1 an hour and more than $100 an hour in 1982-1984 dollars. (5) drops jobs with less than 15 hours of work per week. (6) drops workers who hold multiple jobs at the time of the interview. (7) drops workers who are not working at the time of the interview. (8) drops part-time jobs (those with 15 to 34 hours of work per week).

Table 6: Robustness of the Estimates to Sampling Restrictions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\log w$	$\log w$	$\log w$	$\log w$	$\log w$	$\log w$	$\log w$	$\log w$
U_t	−0.43	−0.58*	−0.52*	−0.47*	−0.43*	−0.38	−0.38	−0.33
	(0.34)	(0.25)	(0.25)	(0.21)	(0.20)	(0.26)	(0.27)	(0.28)
U_{t_0}	−0.05	−0.73*	−0.90**	−0.94***	−0.93***	−0.61	−0.21	−0.21
	(0.37)	(0.30)	(0.31)	(0.25)	(0.24)	(0.32)	(0.37)	(0.37)
U_t^{min}	−3.66***	−3.10***	−2.94***	−2.24***	−2.40***	−3.13***	−3.35***	−3.31***
	(0.68)	(0.51)	(0.50)	(0.42)	(0.42)	(0.56)	(0.57)	(0.58)
$\log(duration)$	6.36***	6.19***	5.72***	5.43***	5.29***	5.75***	4.34***	3.80***
	(0.33)	(0.31)	(0.30)	(0.27)	(0.25)	(0.31)	(0.40)	(0.43)
$\log \bar{\theta}_j$	0.14	−3.28*	−3.86*	−2.60	−2.55	−0.92	−2.24	−0.94
	(2.16)	(1.62)	(1.61)	(1.46)	(1.42)	(1.68)	(1.97)	(2.05)
N	115,314	96,197	88,257	87,417	84,131	57,563	44,832	40,810

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Samples.– All restrictions are added cumulatively. The fifth column corresponds to the benchmark sample used in the main text. (1) Basic sample: all workers over the age of 21 and all jobs that started in 1976 or later and where the respondent was at least 16 years of age at the time of hire. (2) drops jobs outside the private sector. (3) drops observations where the respondent was enrolled in school. (4) drops wages less than $1 an hour and more than $100 an hour in 1982-1984 dollars. (5) drops jobs with less than 15 hours of work per week. (6) drops workers who hold multiple jobs at the time of the interview. (7) drops workers who are not working at the time of the interview. (8) drops part-time jobs (those with 15 to 34 hours of work per week).

Table 7: Robustness of the Estimates to Sampling Restrictions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\log w$	$\log w$	$\log w$	$\log w$	$\log w$	$\log w$	$\log w$	$\log w$
U_t	-0.79^{*}	-0.84^{***}	-0.76^{**}	-0.69^{**}	-0.65^{**}	-0.49	-0.43	-0.39
	(0.33)	(0.25)	(0.25)	(0.21)	(0.20)	(0.26)	(0.27)	(0.28)
U_{t_0}	0.05	-0.72^{*}	-0.89^{**}	-0.89^{**}	-0.90^{**}	-0.62	-0.31	-0.27
	(0.40)	(0.33)	(0.34)	(0.29)	(0.27)	(0.33)	(0.38)	(0.37)
U_t^{min}	-3.37^{***}	-2.75^{***}	-2.64^{***}	-1.90^{***}	-2.09^{***}	-3.23^{***}	-3.54^{***}	-3.49^{***}
	(0.71)	(0.52)	(0.51)	(0.43)	(0.42)	(0.58)	(0.59)	(0.60)
$\log(duration)$	4.83^{***}	4.91^{***}	4.52^{***}	4.25^{***}	4.16^{***}	5.35^{***}	4.39^{***}	3.73^{***}
	(0.55)	(0.45)	(0.43)	(0.36)	(0.34)	(0.48)	(0.61)	(0.62)
$\log\theta_j^{min}$	-6.33^{***}	-6.57^{***}	-6.55^{***}	-5.79^{***}	-5.63^{***}	-2.44	-1.49	-1.48
	(1.51)	(1.26)	(1.26)	(1.04)	(1.02)	(1.28)	(1.48)	(1.61)
$\log\theta_j^{max}$	6.65^{*}	3.89	2.96	4.01	3.69	0.04	-3.34	-1.97
	(3.03)	(2.40)	(2.43)	(2.19)	(2.07)	(2.56)	(2.82)	(2.85)
N	115,314	96,197	88,257	87,417	84,131	57,563	44,832	40,810

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Samples.– All restrictions are added cumulatively. The fifth column corresponds to the benchmark sample used in the main text. (1) Basic sample: all workers over the age of 21 and all jobs that started in 1976 or later and where the respondent was at least 16 years of age at the time of hire. (2) drops jobs outside the private sector. (3) drops observations where the respondent was enrolled in school. (4) drops wages less than $1 an hour and more than $100 an hour in 1982-1984 dollars. (5) drops jobs with less than 15 hours of work per week. (6) drops workers who hold multiple jobs at the time of the interview. (7) drops workers who are not working at the time of the interview. (8) drops part-time jobs (those with 15 to 34 hours of work per week).

the minimum unemployment rate measure in our regressions. This is different than the maximum tightness measure that serves as a proxy for match quality. To approximate match quality, the maximum is taken over the *entire* duration of the job, t_0 to T. As a result, the proxy variable does not vary during the job spell. Therefore, the identification of the contractual variable comes from variations in wages during the job spell, whereas the identification for the match proxy comes from differences in wages across jobs. The results are reported in Table 8 and lead to the same qualitative conclusions as in Section 3.1.

B.2 Wage Growth Regressions

Next we analyze the sensitivity of wage growth regressions in Table 3 to sampling restrictions. The results for the benchmark sample used in the text are reported in Column 5. Specifications to the left of Column 5 are less restrictive than the benchmark sample, and those to the right are more restrictive (see table notes for detailed sampling restrictions). The change in the minimum unemployment rate is statistically significant in all of the specifications in Table 9. The coefficients vary approximately between -3% and -2%, slightly below the estimates obtained from wage level regressions in Tables 6 and 7.

C Reconciliation with Hagedorn and Manovskii (2013)

In this section of the appendix, we compare our findings in Section 3.1 to those obtained in Hagedorn and Manovskii (2013). We begin by making two changes to our regressions. First, we drop the waves 2006 and 2008 from our sample. Second, we change the construction of the minimum unemployment rate. Hagedorn and Manovskii (2013) interpret the wage reported by a worker to be the *average* wage since the last interview date. Consequently, they average the right hand side variables if they are continuous (e.g. experience), and take the mode if they are categorical (e.g. industry). The averaging does not affect variables that do not vary on the same job, such as proxies for match quality, but those that vary during the job spell, such as the minimum unemployment rate. In particular, they first compute the minimum unemployment rates between t_0, the start period of the job, and all the periods between t_0 and t, the period when a wage observation reported. Denote this variable by $U^{min}_{t0,j}$ for $j = t_0, t_0+1, ..t$. Suppose the previous wage observation reported by the worker is in period $t - k$.[23] Then they compute the average of the minimum unemployment rates: $U^{HM}_{min,t} = \frac{1}{k}(\sum_{j=t-k}^{t} U^{min}_{t0,j})$. They associate this variable with the wage observation reported in period t, w_t. By comparison, we interpret the wage observation reported for period t to pertain

[23]Typically, k varies between 1 and 2 years as the NLSY switched from annual interviews to biannual interviews after 1994. It can be much longer if the respondent misses interview cycles.

Table 8: Using Labor Market Tightness to Test Contracts

	(1)	(2)	(3)	(4)
	$\log w$	$\log w$	$\log w$	$\log w$
$\log \theta_t$	-0.41	-0.18	1.04	1.94^{**}
	(0.74)	(0.69)	(0.68)	(0.66)
$\log \theta_{t_0}$	0.03^{*}	0.04^{***}	0.04^{***}	0.04^{**}
	(0.01)	(0.01)	(0.01)	(0.01)
$\log \theta_t^{max}$	8.34^{***}	2.23	7.21^{***}	4.96^{*}
	(1.86)	(1.65)	(1.91)	(1.98)
$\log(duration \times \bar{\theta}_j)$		5.85^{***}		
		(0.26)		
$\log(duration)$			5.35^{***}	4.07^{***}
			(0.25)	(0.36)
$\log \bar{\theta}_j$			-2.17	
			(1.64)	
$\log \theta_j^{min}$				-5.65^{***}
				(1.06)
$\log \theta_j^{max}$				5.42^{*}
				(2.43)
N	84,131	84,131	84,131	84,131

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes.– Table replaces the initial and the minimum unemployment rates used by Beaudry and DiNardo (1991) to test for implicit contracts with the initial and the maximum labor market tightness since the start of the job.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 9: Real Wage Growth and Unemployment Rate: Sensitivity to Sample Selection

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	$\Delta \log w$	$\Delta \log w$	$\Delta \log w$	$\Delta \log w$	$\Delta \log w$	$\Delta \log w$	$\Delta \log w$	$\Delta \log w$
ΔU_t	1.00^{**}	0.41	0.43	0.00	0.09	0.10	0.12	0.15
	(0.32)	(0.25)	(0.26)	(0.21)	(0.21)	(0.21)	(0.21)	(0.20)
ΔU^{min}	-4.80^{***}	-2.89^{***}	-3.08^{***}	-2.09^{***}	-2.04^{***}	-1.98^{**}	-2.69^{***}	-2.72^{***}
	(1.06)	(0.82)	(0.84)	(0.61)	(0.57)	(0.62)	(0.70)	(0.59)
N	58,142	50,419	48,997	48,304	47,355	38,583	31,481	28,372

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes.– All restrictions are added cumulatively. The fifth column corresponds to the benchmark sample used in the main text. (1) Basic sample: all workers over the age of 21 and all jobs that started in 1976 or later and where the respondent was at least 16 years of age at the time of hire. (2) drops jobs outside the private sector. (3) drops observations where the respondent was enrolled in school. (4) drops wages less than $1 an hour and more than $100 an hour in 1982-1984 dollars. (5) drops jobs with less than 15 hours of work per week. (6) drops workers who hold multiple jobs at the time of the interview. (7) drops workers who are not working at the time of the interview. (8) drops part-time jobs (those with 15 to 34 hours of work per week).
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

to period t directly as is common in the literature. Consequently, we construct the minimum unemployment rate by simply taking the minimum between the periods t_0 and t. These differences do not change our conclusions as we show next.

Table 10 re-estimates the specifications in Table 2 by dropping the data from 2006 and 2008 waves and adding the (average) minimum unemployment rate. We do not make any other changes at the moment.[24] The first column is comparable to the first column in 2 and confirms the results reported by Hagedorn and Manovskii (2013). The second column introduces the initial unemployment rate and the *average* minimum unemployment rate as in Hagedorn and Manovskii (2013) and replicates the finding that the minimum unemployment rate is statistically zero when the log-sum of job duration and average tightness is included in the regression. The third column replaces their construction of the minimum unemployment rate with ours. This specification is the same as the fourth column in Table 2, but the sample excludes the last two waves of the NLSY, which results in a slightly smaller elasticity of the wage rate to the minimum unemployment rate (-0.91% here compared to -1.31% in Table 2). Third column includes job duration and average tightness separately, which reduces the coefficient on the minimum unemployment rate to -1.81%. The coefficients on duration and average tightness confirm our earlier conclusion that there is selection in unobserved match quality, but it is acyclical. The last column brings back the 2006 and 2008 waves of the NLSY for comparison, where the results are equal to the fifth column in Table 2.

Our next approach is to conduct our regressions using the data supplement to Hagedorn and Manovskii (2013) provided by the American Economic Review on their website. We were not able to match the data file to the original NLSY data. However, we were able to construct the relevant variables within the provided dataset.[25] The results are reported in Table 11. The first column replicates the findings in Table 1 of their paper: when the proxies for match quality are included, the initial and the minimum unemployment rate are insignificant. The second column replaces the unemployment measures with those used in this paper. The coefficient on the minimum unemployment rate is now significantly negative at -1.50%. The third column includes job duration and average tightness separately. As in Tables 2 and 10, the estimates suggests that the selection in

[24] In particular, we do not average the other explanatory variables. We also did not construct a secondary proxy for match quality based on the employment cycle, consisting of consecutive job cycles where the agent in principle makes job-to-job transitions. It will become apparent that these differences are inconsequential when we run our regressions in their data, where all of these variables are readily available.

[25] In particular, we computed the job duration by taking the maximum job tenure ever observed during the job spell. We deduced the quarter when the job started by taking the difference between the interview period and job tenure. This allowed us to merge our unemployment measures (initial and minimum unemployment) with their data. One remaining difference between our samples are the extreme wage observations due to NLSY's imputation of hourly wages. While we use wage observations between 1$ and $100 an hour in 1982 dollars, Hagedorn and Manovskii (2013) report including more extreme values: those between 0.1$ and $1,000 an hour. It is not clear to us what year's price index is used. Therefore, to make our regressions comparable, we excluded the lowest and highest 1% of the observations.

Table 10: Wages, Unemployment History and Proxies for Match Quality: Sample Restrictions and Variable Definitions of Hagedorn and Manovskii (2013)

	(1)	(2)	(3)	(4)	(5)
	$\log w$	$\log w$	$\log w$	$\log w$	$\log w$
U_t	-0.92^{***}	-0.39^{*}	-0.21	-0.55^{**}	-0.43^{*}
	(0.17)	(0.18)	(0.19)	(0.20)	(0.20)
U_{t_0}		-1.30^{***}	-1.03^{***}	-1.03^{***}	-0.93^{***}
		(0.24)	(0.24)	(0.25)	(0.24)
U_{HM}^{min}		-0.31			
		(0.29)			
U_t^{min}			-0.91^{*}	-1.81^{***}	-2.40^{***}
			(0.36)	(0.42)	(0.42)
$\log(duration \times \bar{\theta}_j)$	6.04^{***}	5.84^{***}	5.80^{***}		
	(0.27)	(0.27)	(0.27)		
$\log(duration)$				5.34^{***}	5.29^{***}
				(0.25)	(0.25)
$\log \bar{\theta}_j$				-1.21	-2.55
				(1.45)	(1.42)
N	78,056	78,056	78,056	78,056	84,131

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Notes.– Columns (1)-(4) use the NLSY 1979 waves up until 2004. $U_{HM,t}^{min}$ is the minimum unemployment rate computed with the same method as Hagedorn and Manovskii (2013): the average of the minimum unemployment rates between two interview dates.

Table 11: Wages, Unemployment History and Proxies for Match Quality: Hagedorn and Manovskii (2013) Data

	(1)	(2)	(3)
	$\log w$	$\log w$	$\log w$
$U_{HM,t}$	-0.94^{*}		
	(0.46)		
U_{HM,t_0}	-0.05		
	(0.39)		
$U_{HM,t}^{min}$	-0.35		
	(0.63)		
$\log \bar{\theta}_j \times \; duration$	7.00^{***}	6.89^{***}	
	(0.38)	(0.39)	
q_2^{HM}	2.54^{***}	2.56^{***}	2.68^{***}
	(0.40)	(0.41)	(0.40)
$U_{BD,t}$		-0.44	-1.01^{*}
		(0.43)	(0.43)
$U_{BD,t}^{min}$		-1.50^{*}	-1.88^{**}
		(0.67)	(0.69)
U_{BD,t_0}		0.46	0.38
		(0.34)	(0.34)
$\log \bar{\theta}_j$			1.89
			(1.08)
$\log duration$			6.82^{***}
			(0.39)
N	41,883	41,883	41,883

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

match quality is acyclical as the coefficient on average tightness is statistically insignificant. More importantly, the coefficients on the initial and the minimum unemployment rate are both significant and negative. Furthermore, the estimated elasticities are practically the same as those reported in the fourth column of Table 10, which are based on our data.

Overall, both Tables 10 and 11 confirm our earlier conclusion that the selection in match quality through separations is not procyclical, and, therefore including proxies for match quality has no bearing for the evidence on dynamic contracts in the wage data.

D Job Training and Human Capital Models

The first column in Table 12 shows the BD regression controlling for total hours of training investment. The initial and the minimum unemployment rate remain significantly negative, while the current unemployment rate is insignificant. The coefficient on the minimum unemployment rate is -2.14, lower than the benchmark estimate of -2.82 in Table 1. The next column reports the results from wage growth regressions using job stayers and including training variables among the covariates. The results are similar to those reported in Table 3.

Table 12: Training, Unemployment and Wage Growth

Dependent Var.	$\log w$	$\Delta \log w$
U_t	-0.24	
	(0.20)	
U_{t_0}	-0.90^{***}	
	(0.27)	
U_t^{min}	-2.19^{***}	
	(0.43)	
$\sum_t Tr_t/2000$	7.57^{***}	
	(0.69)	
ΔU_t		-0.04
		(0.22)
ΔU^{min}		-2.16^{***}
		(0.59)
$Tr_t/2000$		0.00
		(0.00)
Sample size	78,056	43,423

Note.– All specifications control for differences in cubic polynomials of experience and tenure, differences in a quadratic time trend, and indicators for industry and region. Tr_t denotes the training activity between two consecutive wage observations, and $\sum Tr_t(/2000)$ denotes the total cumulative training of a worker. Data comes from the 1979 cohort of the NLSY (1979 - 2004). Sample includes men of ages 21 and older who work full time in the private sector. Coefficients and standard errors are multiplied by 100. Standard errors are clustered by start year and current year interactions.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.