The Quality-Adjusted Cyclical Price of Labor*

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Abstract

Studies of wage cyclicality typically examine the behavior of average hourly earnings or, in some cases, the wages of new hires. But both measures fail to capture cyclicality in the effective cost of labor in the presence of (i) cyclical fluctuations in the quality of worker-firm matches being compared, and (ii) wages being smoothed within employment matches. To address both concerns, we estimate cyclicality in labor's user cost, that is the impact on present-discounted wages of an added worker, exploiting the long-run wage in a match to control for its quality. Using NLSY data for 1980 to 2019, we identify three distinct channels by which the cycle affects user cost. Hiring during recessions results in a lower new-hire wage *and* lower wages going forward in the match, but also higher subsequent separations reducing future surplus to the employer. All totaled, we find that labor's user cost is highly procyclical, increasing by about 4% for a 1 pp decline in the unemployment rate, with at least half of this reflecting a procyclical quality-adjusted new-hire wage.

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1 Introduction

Going back to Pigou's *Industrial Fluctuations* (1927), economists have examined the cyclicality of real wages to disentangle the sources of employment fluctuations. Business cycle models can be stratified between those that generate fluctuations along a stable labor demand schedule, with a countercyclical real wage, versus those that assign a primary role to procyclical shifts in the schedule and a procyclical real wage. The former includes Keynes (1936) and many after who postulate sticky nominal wages, with nominal shocks driving employment. The latter includes models with productivity shocks, financial shocks affecting factor demands (Arellano, Bai, and Kehoe, 2019), or countercyclical markups (Rotemberg and Woodford, 1999), possibly reflecting pricing frictions.

There have been many efforts since Pigou's to estimate the cyclicality of real wages.[1] For many countries and most periods, average hourly earnings appear acyclical or modestly procyclical. But cyclicality in average hourly earnings is potentially a poor proxy for that in the effective price of labor. For one, average hourly earnings fail to control for cyclicality in the quality of workers or worker-firm matches. Secondly, it treats the wages of all workers, even those in long-term employment relations, as if their wages are determined in a spot market. The implicit-contracting literature, e.g., Azariadis (1975), stresses that employers have an incentive to smooth wages to insure workers. Therefore, even if wages within matches are rigid, this does not imply a rigid effective price for firms in hiring workers or for workers in deciding whether to seek jobs.[2]

To hold the quality composition of workers fixed, many authors have examined wage cyclicality excluding workers entering or exiting the workforce or even those changing employers.[3] But this exacerbates the second measurement problem by restricting attention to those workers whose wages are especially likely to be smoothed within longer-term employment relations.

Out of concern that wages are smoothed within employment matches, a number of authors focus on wage cyclicality for new hires. But this approach to measuring labor's price still suffers from the issues of composition and wage smoothing. Firstly, the wage of new

[1] Pigou (1927) charted real wages for Britain from 1850 to 1910 and found that "the upper halves of trade cycles have, on the whole, been associated with higher real wages than the lower halves."

[2] Hall (1980) states this as: "Wages are insensitive to current economic conditions because they are effectively installment payments on the employer's obligation." Consider an analogy to mortgage rates. Basing the cyclicality of real wages on all matches parallels measuring mortgage rates based on the average across all existing mortgages, including those initiated five or even twenty-five years earlier. Such a series (see Berger, Milbradt, Tourre, and Vavra, 2021, Figure 9) is extremely smooth relative to a series reflecting mortgage rates on newly initiated loans.

[3] See, for example, Stockman (1983), Bils (1985), Solon, Barsky, and Parker (1994), Devereux (2001), and Grigsby (2021).

hires at a given time reflects the particular workers and firms that compose those hires. That composition is distinct from the workers and firms forming hires in adjoining periods that provide a basis for cyclical comparisons. Therefore, focusing on new hires exacerbates any concerns that estimated cyclicality reflects variation in the quality of workers, firms, or matches. Secondly, with wage smoothing, the new-hire wage can still be a poor proxy for measuring cyclicality in the price of labor. Intuitively, if hiring in the depth of a recession locks in, to some extent, a lower wage going forward in the match, then the effective price of labor, which captures the expected future wages, will be even lower than the new-hire wage and, therefore, more cyclical than reflected by the new-hire wage (Kudlyak, 2014).

We estimate the cyclicality of the price of labor addressing (1) potential cyclicality in firm-worker match quality, and (2) wage smoothing within matches. By the cyclical price of labor we mean cyclicality of its user cost, where, as in Kudlyak (2014), user cost is defined as the impact on a firm's present-discounted costs of adding a worker today while adjusting future hiring to hold constant future employments. Cyclicality of that user cost reflects, not only the cycle's impact on the new-hire wage, but also any impact on future match rents to the employer, in particular via an impact on the future wage path for a match that starts now versus later. Given these distinct components, we first estimate cyclicality of the quality-adjusted new-hire wage, then proceed to estimate the cyclicality in labor's user cost.

We consider two elements of match quality. First and foremost, we allow for cyclical variation in the productivity of new matches. Secondly, we allow that matches formed in recessions may differ in their durability from those formed in booms. If matches started during recessions are less durable, for which we show some evidence, then ceteris paribus these matches yield lower future surplus to the employer.

We treat the expected long-run wage in a match as an estimate for its productivity. Intuitively, if workers predictably exhibit faster subsequent wage growth if hired in recessions, then we infer that recessions act to depress wages relative to match productivity. Or, in other words, that the quality-adjusted wage is procyclical. Our approach to adjust for quality relies on two assumptions. While the approach differences away any fixed heterogeneity in match qualities, it does not eliminate quality changes that may occur within matches. Therefore, our first assumption is that any quality change *within* matches is independent of whether a job begins in recessions or booms. We provide empirical support for this assumption based on proxies for quality change. Our approach allows for wage smoothing; that is, conditions at the start of a match can influence the wage going forward. Our second assumption is that the impact of wage smoothing dissipates in the long run, which we treat as eight years. If this assumption is violated, our results give a conservative estimate for cyclicality of quality-adjusted wages because wage effects that persist will be treated as quality, reducing cyclicality of quality-adjusted wages.

To account for the second component of match quality, match durability, we estimate separation hazards as a function of both match duration and the state of the business cycle at the start of a match. We quantify the cycle's impact via turnover on expected surplus for the employer, given reasonable costs of worker hiring and training costs. To incorporate the role of match duration on cyclicality of user cost, we ask what compensating differential in wages would offset any reduction in future match surplus due to higher expected turnover.

Our estimates are based on two long worker panels from the National Longitudinal Surveys, the NLSY1979 and NLSY1997 spanning years 1980 to 2019. From these, we can estimate cyclicality of the new-hire wage and user cost for 1980 to 2012. We find that the quality-adjusted new-hire wage is highly procyclical, decreasing by 2.3% for a 1 pp increase in the unemployment rate. It is nearly as cyclical for those hires transiting via non-employment as for those moving job-to-job.

We find that the user cost of labor is considerably more cyclical than the new-hire wage, decreasing about 4% for a 1 pp increase in unemployment, so a little less than twice that in the new-hire wage. This represents an elasticity with respect to real GDP of about 2. We find that cyclicality of future wage paths, the "lock-in effect" on wages from hiring in a recession, is at least as cyclical as the new-hire component. But, for reasonable hiring/training costs, those lower future wages from hiring in a recession are partly offset by future expected losses from higher expected turnover.

The strong cyclicality for user cost we find is fairly in line with that estimated, using different methods, by Kudlyak (2014) and Basu and House (2016), and by Doniger (2021) for college-trained workers. (Doniger estimates an even more cyclical user cost for college-trained workers.) Ours is modestly less cyclical, reflecting our adjustment for the cycle affecting future surplus via separation rates.

Our approach to match productivity is most closely related to Bellou and Kaymak (2012, 2021). They demonstrate history dependence in wages by showing that wage growth within matches reflects, not only current economic conditions, but also conditions earlier in the match. Our focus on labor's user cost follows Kudlyak (2014). Kudlyak (2014), Basu and House (2016), and Doniger (2021), as mentioned, each find highly cyclical user cost. We find that this remains true after controlling for quality of matches. Doniger (2021) takes a control function approach to capture quality of new matches based on observables (e.g., match duration). One of our robustness exercises marries our approach with Doniger's control-function approach for jobs with shorter durations. Other papers studying cyclicality of match quality include Devereux (2004) and Figueiredo (Forthcoming).

Our user-cost approach to the price of labor is motivated by a long list of works documenting history dependence or wage smoothing in wages (Beaudry and DiNardo, 1991; Baker and Gibbs, 1994; Bellou and Kaymak, 2021). It is closely related to studies that examine cyclicality of wages for new hires versus incumbent workers (Bils, 1985; Carneiro, Guimaraes, and Portugal, 2012; Martins, Solon, and Thomas, 2012; Haefke, Sonntag, and van Rens, 2013; Gertler, Huckfeldt, and Trigari, 2020; Grigsby, Hurst, and Yildirmaz, 2021).[4] That includes studies that show a large, fairly persistent negative impact on wages from exiting school into a weak economy (Kahn, 2010; Oreopoulos, von Wachter, and Heisz, 2012).

The balance of the paper proceeds as follows. The next section outlines our framework to control for quality and the implied measures for cyclicality of wages for new hires and cyclicality of labor's user cost. We describe our data and empirical implementation in Section 3. Results are presented in Section 4, these include a number of robustness exercises to relax our key identifying assumption. Section 5 compares our estimates for cyclicality of new-hire wages to estimates taking prior approaches from the literature. We sum up in the last section, then discuss the implications of our results for understanding employment fluctuations.

2 Estimating Labor's User Cost

2.1 Allowing for Wage Smoothing

We can express the wage, gross of match quality, for worker i in firm j in period $t+\tau$ for a match that started in t as:

$$w^{ij}_{t,t+\tau} = \phi_{t,t+\tau} q^{ij}_{t,t+\tau}, \tag{1}$$

where $q^{ij}_{t,t+\tau}$ is the idiosyncratic component of productivity, i.e., match quality. It reflects worker i, firm j, and worker-firm ij match effects. The $t+\tau$ subscripts allow match quality to potentially change over the the course of the match.

We interpret match quality $q^{ij}_{t,t+\tau}$ in terms of productivity. Therefore, netting it out produces a quality-adjusted wage from the firm's perspective. A quality-adjusted wage from the worker's perspective would instead net out amenity values of the match.

$\phi_{t,t+\tau}$ denotes that quality-adjusted wage. For instance, the quality-adjusted new-hire wage is $\phi_{t,t}$. For this subsection, in order to focus on the impact of wage smoothing for measures of cyclicality in the price of labor, we assume one can measure or control for $q^{ij}_{t,t+\tau}$.

[4] Carneiro et al. (2012) and Martins et al. (2012) each find greater cyclicality of wages for new hires in Portugal even controlling for measures of quality. Carneiro et al. (2012) employ firm fixed-effects as quality controls, while Martins et al. (2012) restrict attention to entry-level jobs in order to reduce variation in quality.

If the labor market functioned like a spot market, with no history dependence in wages, then we could drop the subscript reflecting starting date ($\phi_{t,t+\tau} = \phi_{t+\tau}$). One could then consistently estimate cyclicality in the price of labor based on behavior of average wages, new-hire wages, or any other subset. But if wages within matches are insulated from market fluctuations then it becomes necessary to net that insurance component from wages.

As a result, many papers have looked at wage rates for new hires. The differential in the *ln* wage for new hires versus the average *ln* wage in time t, again, controlling for quality is:

$$\ln \phi_{t,t} - \sum_{i=0}^{\infty} \omega_i \ln \phi_{t-i,t} = \sum_{i=1}^{\infty} \omega_i \Big(\ln \phi_{t,t} - \ln \phi_{t-i,t} \Big),$$

where the ω_i's are employment shares by duration of tenure, i. So the common finding of greater wage cyclicality for new-hires is typically interpreted to show that the effective cyclical price of labor is more cyclical than average wage rates, with more senior workers' wages "smoothed" or insured.

But if $\phi_{t,t}$ differs from $\phi_{t-1,t}$, then one should logically expect that $\phi_{t,t+1}$ can differ from $\phi_{t+1,t+1}$, $\phi_{t,t+2}$ from $\phi_{t+1,t+2}$, and so forth. That is, the future wage *path* on a job can depend on the state of the labor market as of its start date. This leads Kudlyak (2014) to examine cyclicality in the user cost of labor as labor's cyclical price.

2.2 Labor's User Cost

Consider the expected present-discounted valuation to a firm of creating a match in t of quality q_t. For convenience, we assume that match quality remains fixed during the match, though we allow below for match surplus to increase with match tenure. For concreteness, write that match value as:

$$\begin{aligned} q_t V_t &= q_t \left[-\kappa_t + E_t \sum_{\tau=0}^{\infty} \Lambda_{t,t+\tau} \Big(\frac{y_{t,t+\tau}}{q_t} - \frac{w_{t,t+\tau}}{q_t} \Big) \right] \\ &= q_t \left[-\kappa_t + E_t \sum_{\tau=0}^{\infty} \Lambda_{t,t+\tau} \Big(z_{t+\tau} - \phi_{t,t+\tau} \Big) \right], \\ \text{where:} \quad \Lambda_{t+\tau} &= \prod_{j=0}^{\tau-1} \beta_{t+j}(1 - \delta_{t,t+j}). \end{aligned}$$

The discounting factor, $\Lambda_{t+\tau}$, allows both the time discount factor β and the separation rate δ to vary with time. It also allows δ to potentially depend on t, the start date of the match. $\kappa_t \cdot q_t$ is the cost of finding and training a worker, which we assume scales by their quality. $y_{t,t+\tau} = z_{t+\tau} q_t$ denotes the marginal revenue product of the worker in $t + \tau$, which reflects both worker quality, q_t and a cyclical term, $z_{t+\tau}$. Let the quality-adjusted wage $\phi_{t,t+\tau}$, fluctuate around a path normalized to one. $z_{t+\tau}$ fluctuates around a path, call it z, with $z \geq 1$. z sufficiently larger than 1 allows the firm to recoup its upfront cost κ_t. (Ignoring fluctuations, $V = 0$ requires $z = 1 + (1 - \beta(1 - \delta))\kappa_t$.)

Next consider the payoff, per unit of quality q_t, of starting a permanent position in t. Doing so requires refilling that position with a new match each time a match ends. For instance, if a separation occurs at $t + \tau - 1$, then $\frac{q_t}{q_{t+\tau}}$ matches are formed at $t + \tau$, where q_t and $q_{t+\tau}$ are the respective quality of the matches started at t and $t + \tau$. The expected discounted value, per unit of quality, from maintaining that position is:

$$Z_t = E_t \sum_{\tau=0}^{\infty} \mathfrak{B}_{t,t+\tau} \pi_{t,t+\tau} V_{t+\tau}, \quad \text{where } \; \mathfrak{B}_{t,t+\tau} = \prod_{j=0}^{\tau-1} \beta_{t+j}.$$

$\pi_{t,t+\tau}$ is the probability that a new match will be required in $t + \tau$, given that the original position began in t. For instance, $\pi_{t,t} = 1$, $\pi_{t,t+1} = \delta_{t,t}$, $\pi_{t,t+1} = (1 - \delta_{t,t})\delta_{t,t+1} + \delta_{t,t}\delta_{t+1,t+1}$, and so forth.

In turn, the expected value of starting a continuing position at t, rather than $t + 1$, is:

$$\begin{aligned} E_t\big(Z_t - \beta_t Z_{t+1}\big) &= z_t - E_t \sum_{\tau=0}^{\infty} \mathfrak{B}_{t,t+\tau} \big(\pi_{t,t+\tau} - \pi_{t+1,t+\tau}\big)\big(\kappa_{t+\tau} + \Phi_{t+\tau}\big), \\ \text{where } \; \Phi_{t+\tau} &= \sum_{i=0}^{\infty} \Lambda_{t+\tau,t+\tau+i} \phi_{t+\tau,t+\tau+i} . \end{aligned} \tag{2}$$

The difference in revenue streams in (2) reduces to z_t, since starting the position at t versus $t + 1$ holds effective labor constant from $t + 1$ forward. The terms $\kappa_{t+\tau}$ and $\Phi_{t+\tau}$ reflect, respectively, the hiring costs and stream of wage rates from starting a match at $t + \tau$, discounted to the start of that match at $t + \tau$. The discounting factor reflects the time discount factor and the match's survival probability.

The costs $\kappa_{t+\tau}$ and $\Phi_{t+\tau}$ get reflected in $E_t\big(Z_t - \beta_t Z_{t+1}\big)$ only to the extent that beginning the position in t, rather than $t + 1$, affects the probability of later starting a match at $t + \tau$. That is, each future potential match gets weighted in (2) by $\pi_{t,t+\tau} - \pi_{t+1,t+\tau}$. Clearly, $\pi_{t,t+\tau} - \pi_{t+1,t+\tau}$ will differ for t and $t+1$, with $\pi_{t,t} - \pi_{t+1,t} = 1$ and $\pi_{t,t+1} - \pi_{t+1,t+1} = -(1 - \delta_{t,t})$. In general, $\pi_{t,t+\tau} - \pi_{t+1,t+\tau}$ can also differ for $\tau > 1$. This difference can be expressed in recursive form for $\tau > 1$:

$$\pi_{t,t+\tau} - \pi_{t+1,t+\tau} = \sum_{j=0}^{\tau-1} \Psi_{t+j,t+\tau-1} \delta_{t+j,t+\tau-1} \Big(\pi_{t,t+j} - \pi_{t+1,t+j}\Big), \quad \text{for } \tau \geq 2,$$

$$\text{where} \quad \Psi_{t+j,t+\tau-1} = \prod_{i=j}^{\tau-2} \Big(1 - \delta_{t+j,t+i}\Big),$$

where $\Psi_{t+j,t+\tau-1}$ is the probability that a match started in $t+j$ survives to $t+\tau-1$.

We refer to the impact on wage payments of beginning the position in t, rather than $t+1$, as the wage component of labor's user cost:

$$UC_t^W = E_t \sum_{\tau=0}^{\infty} \mathfrak{B}_{t,t+\tau} \big(\pi_{t,t+\tau} - \pi_{t+1,t+\tau}\big) \Phi_{t+\tau} \ . \tag{3}$$

Absent wage smoothing, UC_t^W reduces to simply $\phi_{t,t} = \phi_t$. Even with history dependence, if there is no expected differential in wage paths hiring at t and $t+1$, perhaps because expected labor market conditions are not expected to change, then UC_t would reduce to $\phi_{t,t}$. But, more generally, beginning the position in t, instead of $t+1$, can lead to a different expected sequence of wages from $t+1$ forward.

Consider the case of $\delta_{t,t+\tau} = \delta_{t+\tau}$, that is, the separation rate is time-varying, but not specific to a match's start date. We treat this as our benchmark specification in the empirical section. This implies: $\pi_{t,t+\tau} = \delta_{t+\tau-1}$, and in turn:

$$\begin{gathered} E_t\big(Z_t - \beta_t Z_{t+1}\big) = E_t\big(V_t - \beta_t(1-\delta_t)V_{t+1}\big) = z_t - \Big(\kappa_t - E_t\beta_t(1-\delta_t)\kappa_{t+1}\Big) - UC_t^W, \\ \text{where} \quad UC_t^W = \phi_{t,t} + E_t \sum_{\tau=1}^{\infty} \Lambda_{t,t+\tau} \Big(\phi_{t,t+\tau} - \phi_{t+1,t+\tau}\Big), \quad \text{for } \delta_{t,t+\tau} = \delta_{t+\tau}. \end{gathered} \tag{4}$$

The wage component of labor's user cost, UC_t^W reflects the quality-adjusted new hire wage, $\phi_{t,t}$, and the impact of hiring at t versus $t+1$ on the future wage paths, where points on the paths are discounted to reflect time and the possibilities of separating. The other components of $E_t\big(Z_t - \beta_t Z_{t+1}\big)$ reflect marginal revenue at t and the cost of moving the hiring cost up to t. Because these are neither part of wages nor specific to workers hired at t, we do not include them in the *wage component* of labor's user cost, UC_t^W. Just below, we consider if matches started at cohort t exhibit a differential separation rate. We view that as as added component of match quality, as it has ramifications for future hiring costs.

For intuition for (4), contemplate a firm hiring one additional worker in t, then reducing hiring by $1-\delta_t$ workers in $t+1$. This perturbation leaves total labor input unaffected from $t+1$ forward. It also does not affect the probability of starting matches in $t+2$ and beyond. Because the t match replaces $1-\delta_t$ matches in $t+1$, the user cost of labor will reflect any differential in wage paths starting at t versus $t+1$. For instance, suppose that high unemployment reduces the new-hire wage. If that lower wage persists in the match, and labor conditions are anticipated to improve, then labor cost in (4) is pushed below the new hire wage because hiring in a bust allows the firm to partially lock in a lower wage rate. If discounting, from $\Lambda_{t,t+\tau}$, is not too extreme and the lock-in effect on wages not too transitory, then equation (4) implies labor's user cost can be much more cyclical than the new-hire wage.

For the empirics, we will consider the *ln* of user cost. Taking a first-order approximation to equation (4) in the neighborhood of $\phi_{t+1,t+\tau} = \phi_{t,t+\tau}$, that is, in the neighborhood of no wage history dependence, yields:[5]

$$\ln UC_t^W \approx E_t \left[\ln \phi_{t,t} + \sum_{\tau=1}^{\infty} \Lambda_{t,t+\tau} \frac{\phi_{t,t+\tau}}{\phi_{t,t}} \big(\ln \phi_{t,t+\tau} - \ln \phi_{t+1,t+\tau} \big) \right].$$

If we evaluate this approximation for reasonably small business cycle movements in wages (near $\frac{\phi_{t,t+\tau}}{\phi_{t,t}} = 1$) it further reduces to:

$$\ln UC_t^W \approx E_t \left[\ln \phi_{t,t} + \sum_{\tau=1}^{\infty} \Lambda_{t,t+\tau} \big(\ln \phi_{t,t+\tau} - \ln \phi_{t+1,t+\tau} \big) \right]. \tag{5}$$

Up to here, we have focused on the wage component of labor's user cost, which reflects the quality-adjusted new hire wage and the impact of hiring at t versus $t+1$ on discounted future wage paths. However, starting the position at t, rather than $t+1$ will also affects its sequence of hiring costs, $\kappa_{t+\tau}$'s. Most obviously, it adds κ_t while, with probability $1-\delta_t$, subtracting κ_{t+1}. More generally, starting the position in t versus $t+1$ adds net expected hiring costs of $\big(\pi_{t,t+\tau} - \pi_{t+1,t+\tau}\big)\kappa_{t+\tau}$ at $t+\tau$. Suppose match survival rates exhibit cohort effects. As an example, suppose matches started in a downturn exhibit higher subsequent separation rates. (Below we report evidence for such an effect in our NLSY data.) Then, apart from the match productivity q_t, matches started in recessions can be viewed as lower quality because those hires entail larger future hiring costs. That is, ceteris paribus, matches that start in recessions should exhibit lower wages as a compensating differential to employers for the higher future costs. Ignoring this added component of quality, our user cost estimate would then be procyclically biased.

In Section 4, we augment our estimates of cyclicality in the wage component of user cost by estimating the impact of such cohort effects on retention. To do so, we combine estimates of separation rates specific to each match-year cohort with calibrated costs of hiring. Moreover, we generalize the specification in equation (2) to allow for the possibility that the flow of match rents to the firm grow with its duration.[6]

[5] To see this approximation, rewrite eq. (4), taking into account that $\phi_{t,t}$ is in the information set at t, as

$$UC_t = \phi_{t,t} E_t \left[1 + \sum_{\tau=1}^{\infty} \Lambda_{t,t+\tau} \frac{\phi_{t,t+\tau}}{\phi_{t,t}} \left(\frac{\phi_{t,t+\tau} - \phi_{t+1,t+\tau}}{\phi_{t,t+\tau}} \right) \right].$$

[6] We that Mike Elsby for encouraging us to quantify the impact on labor's user cost of cohort effects on retention, as well as his suggestions for doing so.

2.3 Identifying match quality by its expected long-run wage

From our benchmark specification in equation (5), the wage component of labor's user cost can be broken into the new-hire wage plus any differential in the wage path for hires at t versus $t+1$. Accordingly, our empirical work begins by estimating cyclicality in the new-hire wage, while controlling for match quality, then proceeds to examine cyclicality in labor's user cost. But first we lay out our approach to control for match quality based on a match's expected long-run wage. In turn, that allows to estimate cyclicality of new hire wages and user cost controlling for quality.

As discussed above, we can write the (ln of the) new-hire wage as:

$$\ln w_{t,t}^{ij} = \ln \phi_{t,t} + \ln q_{t,t}^{ij},$$

where $\phi_{t,t}$ is the quality-adjusted new-hire wage. So, cyclicality of the quality-adjusted new-hire wage is:

$$\begin{aligned} \mathrm{Cov}(Cycle_t, \ln \phi_{t,t}) &= \mathrm{Cov}(Cycle_t, \ln w_{t,t}^{ij}) - \mathrm{Cov}(Cycle_t, \ln q_{t,t}^{ij}) \\ &= \mathrm{Cov}(Cycle_t, \ln w_{t,t}) - \mathrm{Cov}(Cycle_t, \ln q_{t,t}), \end{aligned} \tag{6}$$

where $Cycle_t$ is a measure of the business cycle, such as the unemployment rate. $\ln w_{t,t}$ and $\ln q_{t,t}$, without ij superscripts, denote the population means of $\ln w_{t,t}^{ij}$ and $\ln q_{t,t}^{ij}$ for jobs starting at t. For example, $\ln w_{t,t} = \int_{ij} \ln w_{t,t}^{ij}$. The transition to the second line of (6) reflects that the variable $Cycle_t$, being purely time-varying, cannot covary with deviations of $\ln w_{t,t}^{ij}$ and $\ln q_{t,t}^{ij}$ from their means for t. We see immediately from equation (6) that cyclicality of new-hire wage provides a biased estimate of the cyclicality of the quality-adjusted new-hire wage unless $\mathrm{Cov}(Cycle_t, \ln q_{t,t}) = 0$.

The quality of new-hire matches will be cyclical if there is cyclical selection into new jobs in terms of worker quality, firm quality, or match-specific quality. The direction of overall bias is hard to sign a priori. In terms of worker quality, Mueller (2017) finds, based on the 1962-2012 Current Population Surveys, that during recessions the unemployed pool shifts toward workers with relatively high wages in their prior job, suggesting that the quality of hires will be countercyclical. Uncontrolled for, this will act as a countercyclical bias in new-hire wages. At the same time, several papers estimate a sullying effect of recessions, with "good jobs" not hiring (Barlevy, 2002; Carneiro et al., 2012; Haltiwanger, Hyatt, McEntarfer, and Staiger, 2021). This implies procyclical firm quality, which will lead to a procyclical bias. Finally, the theories of a cleansing effect of recessions (Mortensen and Pissarides, 1994; Caballero and Hammour, 1994) imply that matches created in recessions are of a higher quality (higher threshold for q_t^{ij}). That cleansing effect implies countercyclical match quality creating a countercyclical bias in new-hire wages.

We can write the quality-adjusted new-hire wage as follows:

$$\begin{aligned}\ln \phi_{t,t} &= \ln w_{t,t}^{ij} - \ln q_{t,t}^{ij} \\ &= \ln w_{t,t}^{ij} - \ln w_{t,t+\tau}^{ij} + \left(\ln q_{t,t+\tau}^{ij} - \ln q_{t,t}^{ij}\right) + \ln \phi_{t,t+\tau},\end{aligned}$$

where the last equality obtains from adding and subtracting $\ln w_{t,t+\tau}^{ij}$.

Therefore, cyclicality of the quality-adjusted new-hire wage can be expressed as:

$$\begin{aligned}\mathrm{Cov}(Cycle_t, \ln \phi_{t,t}) = {} & \mathrm{Cov}(Cycle_t, \ln w_{t,t} - \ln w_{t,t+\tau}) \\ & + \mathrm{Cov}(Cycle_t, \ln q_{t,t+\tau} - \ln q_{t,t}) + \mathrm{Cov}(Cycle_t, \ln \phi_{t,t+\tau}),\end{aligned} \tag{7}$$

where $\left(\ln w_{t,t} - \ln w_{t,t+\tau}\right)$ and $\left(\ln q_{t,t+\tau} - \ln q_{t,t}\right)$ denote the population means of $\left(\ln w_{t,t}^{ij} - \ln w_{t,t+\tau}^{ij}\right)$ and $\left(\ln q_{t,t+\tau}^{ij} - \ln q_{t,t}^{ij}\right)$ for jobs starting at t. For example, $\ln w_{t,t} - \ln w_{t,t+a} = \int_{ij}(\ln w_{t,t}^{ij} - \ln w_{t,t+a}^{ij})$.[7] We take the population for means $\left(\ln w_{t,t} - \ln w_{t,t+\tau}\right)$ and $\left(\ln q_{t,t+\tau} - \ln q_{t,t}\right)$ as all matches starting at time t. For exposition, we set aside here the important question of whether matches survive from t to $t+\tau$. We return explicitly to this matter in Section 3.2.

We now state two assumptions sufficient for the covariances in the second row to be zero.

Assumption 1:

$$\mathrm{Cov}(Cycle_t,\ \ln q_{t,t+\tau} - \ln q_{t,t}) = 0 \tag{8}$$

Assumption 1 states that the mean change in match quality for matches started at t is orthogonal to cycle at t. We provide empirical support for this assumption in Section 4.2.2.

Assumption 2:

$$\mathrm{Cov}(Cycle_t,\ \ln \phi_{t,t+a}) = 0,\ \text{for } a \text{ sufficiently large.} \tag{9}$$

Assumption 2 can be viewed more intuitively as implied by a pair of conditions. The first being $\mathrm{Cov}(Cycle_t,\ \ln \phi_{t+a,t+a}) = 0$, and the second $\mathrm{Cov}(Cycle_t,\ \ln \phi_{t,t+a} - \ln \phi_{t+a,t+a}) = 0$.

The first condition imposes that the current stage of the business cycle does not predict the new-hire wage a periods ahead. We see this as a natural assumption if a is chosen large enough so that the current cyclical state does not predict $Cycle_{t+a}$, that is, the stage of cycle a periods ahead. We test this assumption in the data for the a we choose in practice, given measures of the cycle at t and $t+a$.

The second condition imposes that wage smoothing is transitory. This is consistent with models with limited commitment, e.g., Thomas and Worrall (1988), and is supported in the data, e.g., Beaudry and DiNardo (1991), Bellou and Kaymak (2021). It is important to note that, to the extent this assumption is violated in practice, it will cause us to understate

[7] As with equation (6), match-specific deviations from $\left(\ln w_{t,t} - \ln w_{t,t+\tau}\right)$ are orthogonal to $Cycle_t$.

procyclicality of new-hire wages. For instance, suppose that wages for workers hired during a recession are lowered indefinitely, as predicted by models with perfect commitment. Then our assumption will understate the quality of matches that begin in recessions, thereby understating the procyclicality of wages.

Under these two assumptions, we immediately obtain the following from equation (7):

Implication 1. *Given Assumptions 1 and 2, the cyclicality of the quality-adjusted new-hire wage is*

$$\mathrm{Cov}(Cycle_t,\ \ln \phi_{t,t}) = \mathrm{Cov}\left(Cycle_t,\ \ln w_{t,t} - \ln w_{t,t+a}\right) \text{ for } a \gg 1. \tag{10}$$

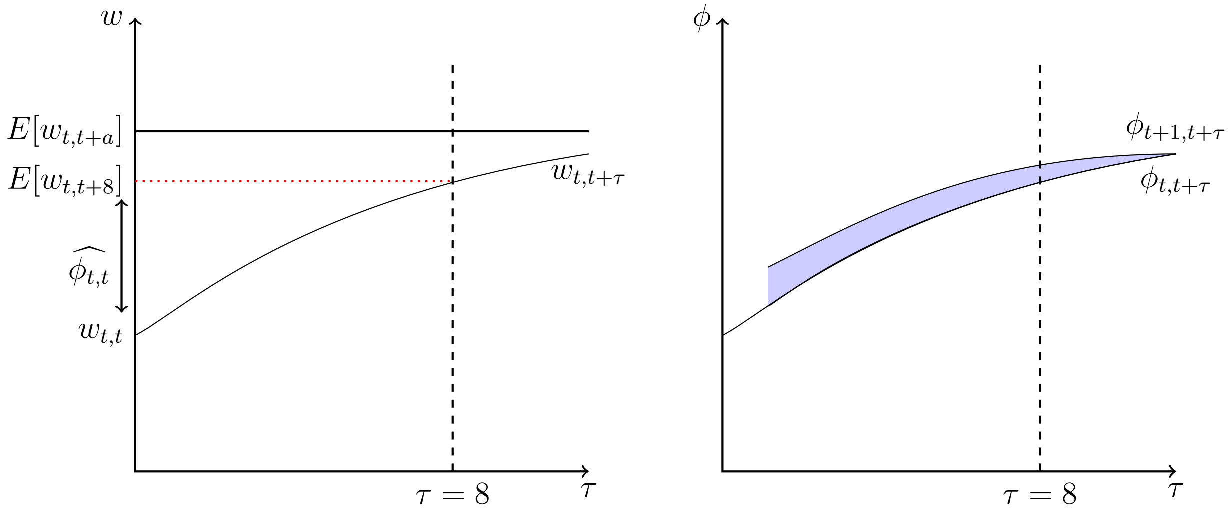
That is, cyclicality of the quality-adjusted new-hire wage equals the *negative* of cyclicality of the match's cumulative wage growth as it moves to its long-term expected wage. Note that Assumptions 1 and 2, and their Implication 1, do not require that $q_{t,t}^{ij} = w_{t,t+a}^{ij}$, only that deviations between $w_{t,t+a}^{ij}$ and $q_{t,t}^{ij}$ not be correlated with the stage of the cycle at t.

We illustrate Implication 1 in Figure 1a for a match that starts in a recession. Match quality is captured by the expected wage at $t+a$. To the extent the match wage predictably grows faster starting in a recession, this implies that $\phi_{t,t}$ is depressed. Our estimate $\widehat{\phi}_{t,t}$ reflects that predictable cumulative wage growth from t to $t+8$. Figure 1a is drawn such that Assumption 2 is not completely satisfied as of $t+8$, as $w_{t,t+8}$ still remains below q_t, equal to the expected wage at $t+a$. This illustrates the conservative nature of Assumption 2 – to the extent $w_{t,t+8}$ remains below the expected wage at $t+a$, we underestimate how much the recession at t depresses $\phi_{t,t}$.

Next consider the quality-adjusted wage component of user cost. For exposition, we focus on our benchmark specification that assumes the separation rate varies only with time, $\delta_{t,t+\tau} = \delta_{t+\tau}$. From equation (5), its cyclicality reflects not only the new-hire wage, but also any impact on future wages by hiring at t versus $t+1$. For $t+\tau$, as an example, that means any impact on $\ln \phi_{t,t+\tau} - \ln \phi_{t+1,t+\tau}$. But, similarly to Implication 1, Assumptions 1 and 2 imply that cyclicality of the quality-adjusted wage τ periods into the match, $\mathrm{Cov}\left(Cycle_t,\ \ln \phi_{t,t+\tau}\right)$, is given by $\mathrm{Cov}\left(Cycle_t,\ \ln w_{t,t+\tau} - \ln w_{t,t+a}\right)$. Substituting in equation (5), we obtain:

Implication 2: *Given Assumptions 1 and 2, for $a \gg 1$*

$$\begin{aligned}\mathrm{Cov}(Cycle_t, \ln UC_t^W) = \ & \mathrm{Cov}\Bigg(Cycle_t,\ \ln w_{t,t} - \ln w_{t,t+a} \\ & + \sum_{\tau=1}^{a} \Lambda_{t,t+\tau}\Big[\Big(\ln w_{t,t+\tau} - \ln w_{t,t+a}\Big) - \Big(\ln w_{t+1,t+\tau} - \ln w_{t+1,t+a+1}\Big)\Big]\Bigg),\end{aligned} \tag{11}$$



(a) New-Hire Wage (b) User Cost of Labor

Figure 1: Illustration of Our Approach

where component $\ln w_{t,t} - \ln w_{t,t+a}$ reflects the quality-adjusted new-hire wage and the remainder reflects future wage paths.[8] As before, $\Lambda_{t+\tau} = \prod_{j=0}^{\tau-1} \beta_{t+j}(1 - \delta_{t,t+j})$.

There are two key observations from eq. (11). First, for a match started in t, the higher is cumulative wage growth to $t + a$, the lower is the new-hire wage at t, and so the lower is user cost. Intuitively, predictably rapid wage growth indicates the wage is below match quality (again see Figure 1a). Second, the higher is wage growth from $t + 1$ forward for matches started at t, compared to those started in $t + 1$, the lower is the user cost in t. The impact of future wage paths on user cost for a match starting in a recession at t is illustrated in Figure 1b. Faster cumulative wage growth from $t + 1$ to $t + a + 1$ for a match starting at t versus $t + 1$ indicates that the t-start match continues to exhibit a lower wage relative to its quality than if started at $t + 1$.

3 Empirical Implementation

3.1 Data, sample selection and variable definitions

We combine data from the two National Longitudinal of Youth Surveys: the NLSY79 and the NLSY97. The NLSY79 cohort consists of 12,686 young men and women born from 1957 to 1964. Respondents were interviewed annually from 1979 until 1994, then biannually since.

[8]Comparing with equation (5), note that the summation in (11) can be truncated at a. Given Assumption 2, there is no predicted discrepancy between $\ln w_{t,t+\tau}$ and $\ln w_{t,t+a}$ for $\tau \geq a$. Secondly, while user cost reflects the expectations of the future wage paths, not realized, we drop the expectations operator in equation (11). This assumes the realized deviations from expectations at t are orthogonal to the cyclical stage at t.

The NLSY97 cohort consists of 8,984 young men and women born between 1980 and 1984, with respondents interviewed annually from 1997 until 2010 and biannually since. Our last NLSY79 and NLSY97 surveys are, respectively, 2018 and 2019.

An important advantage of the National Longitudinal Surveys for our purposes is that they track respondents' work history over the panel, with identifiers for each distinct employer. In particular, at each survey, the NLSY79 provides data on up to five jobs held since the prior survey, while the NLSY97 does so for all jobs held. We use these data to identify starting dates for worker-employment matches and to construct the durations and wage growth within those matches.

Our sample reflects the NLSY79 and NLSY97's nationally-representative samples.[9] We further restrict to respondents who are at least 21 years old and who are not enrolled in school. The oldest respondents in our NLSY79 sample are 62, while in our NLSY97 sample 39. We exclude respondents who are self-employed or employed in the government or armed forces. We also exclude jobs with less than 25 usual hours worked per week.

We define a job as a period of working for the same employer.[10] We allow jobs to experience interruptions, provided they last less than a year. That is, we treat any separation of 52 weeks or longer as a break to a new job. From the NLSY surveys, we can identify the calendar week a job starts and ends. Of course, we do not observe the end date for a job held by a respondent at their last survey. We can record the start date for a job held at a respondent's first interview, but only based on a retrospective question.[11] We define a match as a new hire if it represents the first wage observed for the worker at that job and it has match tenure of less than one year. We distinguish new hires that occur via non-employment versus job-to-job. We classify a transition as via non-employment if the worker was non-employed during the month before the start of the new job.

Our wage measure is the hourly wage constructed by the BLS: It is the reported hourly wage for those paid hourly; for others, it is computed based on reported earnings per pay period and hours worked. The wage reflects any tips, overtime, and bonuses.[12] For ongoing jobs, we assign the observed wage to the interview date; for jobs that have ended, we assign it

[9] We do still employ the NLSY sampling weights in all empirical work. These weights estimate how many U.S. individuals are represented by each respondent.

[10] If the respondent simultaneously works multiple jobs, we consider all jobs that satisfy our sample restrictions, including working at least 25 hours per week.

[11] We exclude jobs that the respondent started at age less than 16. In actuality, only 9.5 percent of jobs have starts prior to age 21, when we begin measuring their wages, and only 0.8 percent have starts prior to age 18. We also exclude jobs that: (i) have no valid ending date, despite ending (ii) report starting later than ending, or (iii) start before 1980.

[12] For the NLSY79, our wage measure is the hourly wage variable HRP#, where # references each job for which wage information was collected. For NLSY97, the compatible measure is the variable HRLY_COMPENSATION#, since it includes overtime, tips, and bonuses. NLS User Services responded to a request for clarification by stating that for the NLSY79: i) pay rate questions do ask respondents to

to the job's ending date. When available, we use a question asking the wage retrospectively at the start of the job, which allows us to observe what happens in its beginning years.[13] We compute a real wage using the CPI deflator. We drop observations with a reported wage less than half the federal minimum hourly wage for nonfarm workers or above the 99th percentile of the wage distribution for that survey year.

Using our wage data, we construct an individual's wage growth as the log difference of their wage rates across two consecutive surveys. Note that the length of the time between two successive wage observations in our data varies. In particular, in the early years of the NLSY79 and NLSY97, observations are at an annual frequency, while, in later years, they are only biannual. Therefore, for calculating wage growth, we restrict the interval between the wage observations to 0.5 to 1.5 years across annual surveys and 1.5 to 2.5 years across biannual. In addition, to deal with possibly extreme values, we trim wage growth rates below the 1st and above the 99th percentiles of the growth distribution for that survey year.

We additionally use information on gender, race, educational attainment, and age as control variables. These are dummies for male/female, white/black, and schooling categories. We specify age effects as a cubic polynomial for any wage-level specifications and a quadratic for those specified in changes.

Our resulting sample consists of 144,367 wage observations from 11,769 unique individuals (90,176 observations from 5,773 NLSY79 individuals and 54,191 from 5,996 NLSY97 individuals). When working with growth rates, our sample consists of 63,986 observations from 10,152 distinct individuals (42,836 observations from 5,199 NLSY79 individuals and 21,150 from 4,953 NLSY97 individuals). In addition, in our sample, some jobs have a really short duration. We restrict our sample to jobs whose maximum observed tenure is more than 18 months. Lastly, because our approach uses expected future wages to control for quality, we restrict our sample to jobs starting up to 2011 for some exercises. In these cases, the observation number of each sample is described in table notes. Table 14 in the data appendix provide statistics on the key variables for our sample.

We employ two alternative measures of the business cycle — the unemployment rate and real GDP — and several different de-trending methods for defining the cycle. Unemployment rate and real GDP data are from the BLS and BEA, respectively.

include tips, overtime, and bonuses; and ii) there is no way to create a pay rate that does not include this information.

[13]In practice, the BLS did not construct a starting wage rate as they did for the wage at the time of the interview. However, all variables necessary to create it are available in the NLSY public release (the reported pay rate, the time unit to interpret the pay rate, and the usual weekly hours). We construct the starting wage rate following the procedure used by the NLSY to create the available hourly pay rate measure and described in the NLSY documentation appendix. The starting wage question has been available since 1986 in the NLSY79 (some years have strangely few respondents) and for all years in the NLSY97.

3.2 Estimation approach

We estimate the cyclicality of the new-hire wage and user cost from the following regression:

$$\ln y_t = \chi * Cycle_t + trend_t + \epsilon_t,$$

in which y_t reflects, in turn, the quality-adjusted new-hire wage or user cost, $Cycle_t$ is a measure of the cycle, and $trend_t$ is chosen to remove slower-moving time trends. Our benchmark specification controls for a cubic trend. For robustness, we also consider a quadratic trend, one and two-sided Hodrick–Prescott filters, and a Hamilton filter. In this section, we describe how we employ wage growth within job matches to construct the dependent variables, $\ln y_t$, to estimate the cyclicality of quality-adjusted new-hire wages and user cost.

First, consider the choice of a, which is the horizon for Assumption 2 to apply. That is, it is the duration for a match such that the match wage, conditional on quality, no longer reflects labor-market conditions at its start. Guided by the models of limited commitment, as Thomas and Worrall (1988), we set a benchmark value for a of eight years, a period more than sufficient to cover the duration of business cycles. Models of limited commitment, with workers not committed, suggest that the discrepancy between inherited contract wages and new-hire wages dissipates with the arrival of a cyclical peak. We also consider shorter cutoffs for a — six or four years. An advantage of a shorter cutoff for a is that more matches will reach that duration. The downside is that it biases downward the cyclicality of our estimates to the extent that the impact of wage smoothing remains intact.

This leads to the question of how to deal with matches that do not reach duration a. Estimating based only on matches that last a full a years would clearly throw out a lot of information from those lasting up to $a-1$ years. Our approach is to use all matches starting at t, except those lasting less than one year and a half, to construct wage growth for matches starting at t. Relative to considering only matches lasting eight years, this greatly reduces, though does not eliminate, concerns with selection bias. We discuss the selection issues at length at the end of this subsection.

It is convenient to rewrite cumulative wage changes in terms of annual growth rates, in particular: $\ln w^{ij}_{t,t} - \ln w^{ij}_{t,t+a} = -\sum_{\tau=1}^{a} \Delta \ln w^{ij}_{t,t+\tau}$. $\Delta \ln w^{ij}_{t,t+\tau}$ denotes the wage growth between years $t+\tau-1$ and $t+\tau$ of worker i on job j, which we can construct from the individual wage data within a match. Implication 2 can then be rewritten as:

$$\begin{aligned}\mathrm{Cov}(Cycle_t, \ln UC^W_t) = \mathrm{Cov}\Bigg(&Cycle_t, \\ -\sum_{\tau=1}^{a} \Delta \ln w_{t,t+\tau} - \sum_{\tau=2}^{a} \Omega_{t,t+\tau}\big(\Delta \ln w_{t,t+\tau} - \Delta \ln w_{t+1,t+\tau}\big) &+ \Omega_{t,a+1}\Delta \ln w_{t+1,t+a+1}\Bigg),\end{aligned} \tag{12}$$

where $\Omega_{t,t+\tau} = \sum_{i=0}^{\tau-2} \left(\prod_{j=0}^{i} \beta_{t+j}(1-\delta_{t+j}) \right)$; $\Omega_{t,a+1}$ is equal to $\Omega_{t,t+\tau}$ at $\tau = a+1$; and $\Delta \ln w_{t,t+\tau} = \int_{ij} \Delta \ln w_{t,t+\tau}^{ij}$. Cyclicality of the new-hire wage is captured by the covariance of the cycle with the first term, $-\sum_{\tau=1}^{a} \Delta \ln w_{t,t+\tau}$, with cyclicality of the future wage path captured by the balance. That difference in wage paths is reflected in whether matches starting in t exhibit faster wage growth from $t+1$ to $t+a$ than matches starting at $t+1$. Note that, with the wage paths expressed in terms of growth rates, the weight $\Omega_{t,t+\tau}$ is actually increasing in τ. This reflects that predictably faster wage growth further out, for instance from $t+a-1$ to $t+a$, implies a lower $\phi_{t+\tau}$, not just at $t+a-1$, but all the way back to $t+1$.

To estimate match wage growth as a function of its start date, $\Delta \ln w_{t,t+\tau}$, we employ the NLSY data to regress $\Delta \ln w_{t,t+\tau}^{ij}$ on dummies to capture the full set of interactions between the calendar year a match started (the t's) and all subsequent periods observed in the data (the $t+\tau$'s).[14] Specifically, we estimate the $\psi_{t,t+\tau}$'s from the following regression for workers' rates of wage growth within matches:

$$\Delta \ln w_{t,t+\tau}^{ij} = \Psi x_{t+\tau}^{ij} + \sum_{d_0=1980}^{2010} \sum_{d=d_0+1}^{2018} \psi_{d_0,d} D_{d_0,d}^{ij} + \epsilon_{t+\tau}^{ij}. \tag{13}$$

in which gummy variables $D_{d_0,d}^{ij}$ equal 1 if $d_0 = t$ and $d = t+\tau$, equaling 0 otherwise, and $x_{t+\tau}^{ij}$ reflects additional controls for individual characteristics that could affect measured wage growth. These are dummies capturing the respondent's sex, race, educational attainment, survey instrument (NLSY79 or NLSY97), and a quadratic in their age. Because we set a to eight years, we estimate the regression on the sample of jobs that start between 1980 and 2011, i.e., eight years before our sample ends in 2019.[15]

Given estimates for $\psi_{t,t+\tau}$, we then substitute in equation (12) to obtain:

$$\begin{aligned} &\mathrm{Cov}(Cycle_t, \ln UC_t^W) \\ &= \ \mathrm{Cov}\left(Cycle_t,\ -\sum_{\tau=1}^{a} \widehat{\psi}_{t,t+\tau} - \sum_{\tau=2}^{a} \Omega_\tau \left(\widehat{\psi}_{t,t+\tau} - \widehat{\psi}_{t+1,t+\tau} \right) + \Omega_{a+1} \widehat{\psi}_{t+1,t+a+1} \right), \end{aligned} \tag{14}$$

[14]Both NLSY started annually and became biannually after some time, so the wage growth between survey years could be annual or biannual. We annualize two-year growth rates by assigning half to the first year and half to the second. In practice, we annualize the growth rate between $t+\tau$ and $t+\tau+2$ by creating two observations and assigning half of the growth to $t+\tau+1$ and half to $t+\tau+1$. We assign half of the original sampling weight for these two new observations.

[15]When estimating $\psi_{t,t+\tau}$, we require each combination of starting and current year, (t,t+τ), to have more than 20 wage change observations. This restriction is binding for some first wage growth rates, i.e., those between t and $t+1$. For example, in our baseline specification, we cannot estimate the $\psi_{t,t+1}$ for the following combinations of starting year and current year: 1980-1981, 1995-1996, and 1997-1998. In these cases, we impute the first growth rate using the growth between $t+1$ and $t+2$ as proxy.

where $a = 8$ years. This yields 32 annual observations – for each year from 1980 to 2011 – to estimate the cyclicality of the quality-adjusted new-hire wage and labor's user cost.[16]

Substituting $\widehat{\psi}_{t,t+\tau}$'s in Equation (14) implicitly imputes the average wage change from $t+\tau-1$ to $t+\tau$ for matches that survive to $t+\tau$ for the hypothetical wage growth for those matches that end before $t+\tau$. Due to this selection, the expected value of $\widehat{\psi}_{t,t+\tau}$ is: $E\left[\ln w^{ij}_{t,t+\tau} - \ln w^{ij}_{t,t+\tau-1} | \; \Gamma^{ij}_{t,t+\tau-1} = 1, \Gamma^{ij}_{t,t+\tau} = 1\right]$, where $\Gamma^{ij}_{t,t+\tau-1}$ and $\Gamma^{ij}_{t,t+\tau}$ are 0/1 variables, equal to 1 if match ij survives to $t+\tau-1$ and $t+\tau$, respectively. If there are idiosyncratic shocks to match quality, then this is potentially biased from $E\left[\ln w^{ij}_{t,t+\tau} - \ln w^{ij}_{t,t+\tau-1}\right]$ by selection on which matches survive. But the direction of that bias is difficult to predict as it reflects selection on the wage at $t+\tau-1$ as well as at $t+\tau$.[17]

For our purposes, what matters is whether the magnitude of any selection effect varies systematically with the state of the business cycle at t. That is, the contribution to the covariance terms in Equation (12) based on surviving matches is $Cov\Big(Cycle_t, E\left[\Delta \ln w^{ij}_{t,t+\tau} \; | \; \Gamma^{ij}_{t,t+\tau-1} = 1, \Gamma^{ij}_{t,t+\tau} = 1\right]\Big)$ rather than the covariance of $Cycle_t$ with $\Delta \ln w_{t,t+\tau}$, expected wage growth for all matches that start at t. One possible reason for concern is that there is evidence, e.g., Mustre-Del-Rio (2019), that matches formed in recessions have shorter average duration. Below we document such an effect, though small, for our data. So the set of matches surviving τ periods, starting from a recession, is potentially more selected.

For this reason, in Section 4.2, we conduct a number of extensions to test the robustness of our findings for wage cyclicality. These include varying the threshold duration a as well as employing a selection correction for whether a match at $t+\tau-1$ survives to $t+\tau$. We also include all workers in constructing cumulative eight-year wage growth, including those who change matches. In doing so, we control for subsequent changes in match quality based on the new match's relative hours worked and realized duration.

We should also highlight that our estimates are *not* biased by differences in match quality that are fixed within a match. Any such differences in match quality, which have been the focus of the literature, e.g., Hagedorn and Manovskii (2013) or Gertler et al. (2020), are differenced away by our first step estimation of equation (13) as it is based on wage growth *within* a match. A corollary is that our estimates are unaffected by any selection on match

[16] For term $\Omega_a \widehat{\psi}_{t+1,t+a+1}$, we set $\widehat{\psi}_{2012,2020}$ equal to the mean of matches' eighth-year wage change. Otherwise we would lose the last observation we treat as t, 2011, for estimating cyclicality of user cost.

[17] Selection would be for positive match shocks at both $t+\tau-1$ and at $t+\tau$; so selection on their difference, which the wage change reflects, is ambiguous. If the variance of match shocks is greater at $t+\tau$ than at $t+\tau-1$, then selection would presumably bias upwards realized wage changes, with the converse holding if the variance is greater at $t+\tau-1$.

duration driven by the fixed quality of a match because, again, the first-step estimates of wage growth removes those differences.

Our presentation has assumed constant separation and discount rates, e.g., as in equation (14). But, in the estimation, we allow both for time-varying separation and time-discount rates. We estimate fluctuations in the separation rate from the NLSY data, and we estimate time variations in the discount factor β based on variations in the growth rate of consumption. Details for both are provided in Section 4.3. The computation of user cost is described in Appendix A.

4 Cyclicality of the New-hire Wage and User Cost

4.1 Preliminaries

Labor's user cost reflects the new-hire wage and the impact of hiring now, versus later, on future match wages. For this reason, we first estimate cyclicality of the new-hire wage in Section 4.2, then cyclicality of user cost in 4.3. Because most estimates of wage cyclicality are based on average hourly earnings, we first examine cyclicality for this measure in our NLSY data. Table 1 gives results from the NLSY data for 1980 to 2011, reflecting 115,795 observations from 11,467 distinct individuals. We stop the sample in 2011 so that the period is comparable to that for our estimates of the quality-adjusted new-hire wage and user cost reported below. (We report results for 1980 to 2019 in the notes to Table 1). The cycle is measured by the national unemployment rate, controlling for a cubic trend.

Table 1: Cyclicality of Average Hourly Earnings

Dependent Variable is log of real wage: $\ln(\frac{w}{p})$

	(1) Age Control	(2) Individual FE	(3) Matching FE
Unemp Rate	-0.25	-0.76	-0.41
	(0.48)	(0.35)	(0.32)

Notes: Our sample – NLSY79 and NYSY97 panels – has 115,795 observations for 1980 to 2011. Additional controls are a cubic trend and dummies for sex, race and education groups and cubics in age and tenure. All coefficients are specific to the NLSY79 and NLSY97 samples except those for the unemployment rate and cubic trend. Standard errors are clustered by survey year. All regressions reflect survey sampling weights. For the full period of 1980-2019 the estimated coefficients are -0.00 (0.33), -0.82 (0.29), and -0.43 (0.24), respectively.

Table 1, Column 1 presents results without any individual controls except age, as a cubic, which we include because each NLSY panel ages through time. Real average hourly earnings are nearly acyclical, decreasing by 0.25 percent for a 1 pp increase in unemployment rate with a standard error of 0.48.[18] This estimate will reflect any cyclical changes in the composition of the workforce – and many papers have noted that employment is more cyclical for lower-wage workers. We correct for that compositional effect in Column 2 by including individual fixed effects in the regression while also controlling for cubics in the worker's age and match tenure. The estimated impact of 1pp higher unemployment goes from -0.25 to -0.76 and is now statistically significant (standard error 0.35).[19]

Lastly, Column 3 includes a full set of match-specific fixed effects. The estimate now captures the response of the real wage *relative* to its match average to a 1 pp increase in unemployment *relative* to the average over the match. The estimate is reduced back to -0.41 with a standard error of 0.32. Echoing our discussion above, there are two clear competing explanations for why match controls reduce cyclicality. One is that job turnover produces strongly procyclical firm and match quality. The second is that wages are largely insulated within matches, as predicted by many contracting models, so including match effects misses much of the cycle's impact on wages. To progress past this perceived impasse, we turn to our quality-adjusted estimates for the new-hire wage and user cost.

4.2 Cyclicality of the quality-adjusted new-hire wage

4.2.1 Benchmark estimates

We first estimate wage-growth dummies, $\psi_{t,t+\tau}$'s in equation (13), from the NLSY worker-firm match histories. Those estimates reflect 82,557 observations from 9,361 individuals across 17,794 matches. We then construct our dependent variable, $-\sum_{\tau=1}^{8}\widehat{\psi_{t,t+\tau}}$, to estimate new-hire wage cyclicality. For convenience, we refer to this variable as the new-hire wage for the balance of this section. But, more accurately, our assumptions imply it is equal to the quality-adjusted new-hire wage at t plus an error that is orthogonal to the cycle at t.

[18] For comparison, we estimate cyclicality of average hourly earnings measured from the Current Population Surveys (CPS) IPUMS microdata or by the Current Employment Surveys (CES) national series for 1980 to 2011. The CPS measure is calculated by dividing an individual's annual wage and salary income by the product of their weeks worked and usual weekly hours worked. (For the CPS regression, we control for a cubic in age as well as the cubic time trend.) The CES measure is its *average hourly earnings of production and nonsupervisory employees, total private*. So it a more restrictive sample of workers than we consider in the NLSY. Furthermore, given it reflects aggregate earnings relative to aggregate hours, in estimating cyclicality it implicitly weights individual workers by their relative earnings. Comparable to our estimates from the NLSY panels in Column 1, the average hourly earning series from the CPS is perhaps slightly procyclical (estimated impact of 1pp higher unemployment on real wages of -0.49 with standard error 0.28), while that from the CES is essentially acyclical (estimated impact of -0.13 with standard error 0.26).

[19] If we drop the individual fixed effects, but control for the worker's demographics (education, gender, race) and age and tenure, estimated cyclicality goes from -0.25 to -0.64, with a standard error of 0.39. So most of the cyclicality in worker fixed effect is captured by cyclicality in these observables.

Figure 1 presents the time series for our new-hire wage for the 32 annual observations for 1980 to 2011, with our benchmark-treatment of $a = 8$, together with the national unemployment rate. The new-hire wage is clearly highly procyclical. Most notably, it decreases by about 9.2% and 12.9% for the two large recessions in 1980-82 and 2007-2009. Table 2, Column 1 gives the estimated cyclicality of the new-hire wage: The new-hire wage decreases by 2.30% for each percentage point cyclical increase in the unemployment rate, with a standard error of 0.67%.

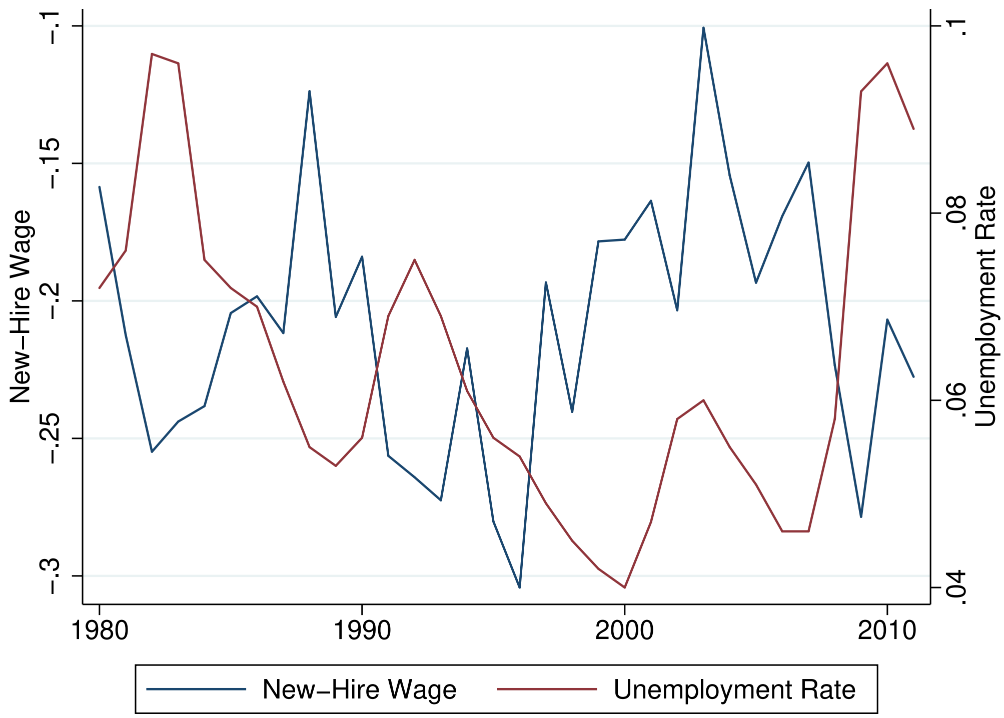


Figure 2: Time Series of the Quality-Adjusted New-Hire Wage

Our approach relies on two assumptions. The first is that the cyclical state at t does not predict quality growth within matches, either fundamentally or via selection in the matches that we can follow. We turn to a number of tests for violations of this assumption in the next section. The second is that the state of the cycle does not predict the quality-adjusted wage in the match eight years ahead. This would be violated if the cyclical state at t either: (i) is correlated with the cyclical state eight years later, or (ii) still helps to predict wages eight years into the match because of the highly persistent effects of cyclical wage smoothing. Note that the latter violation should act to bias our estimates toward zero cyclicality.

We can test condition (i) by seeing whether the unemployment rate at t, relative to any trend movements, predicts the rate at $t + 8$. It does not. Furthermore, Column 2 of

Table 2 shows that controlling for the unemployment rate at $t + 8$ yields essentially the same cyclicality of the new-hire wage, with a response to a one percentage point higher unemployment rate at t of -2.46 percent (standard error 0.60).

Table 2: Cyclicality of the Quality-Adjusted New-Hire Wage

	(1)	(2)
URate	-2.30	-2.46
	(0.67)	(0.60)
URate 8yr Ahead		1.15
		(0.53)

Notes: The table shows the percent change in wage in response to a 1 pp increase in unemployment. 32 annual observations: 1980-2011. Regressions include a cubic trend. Robust standard errors are in parentheses.

Cyclicality of match quality

Our measure of match quality for a match started at t is its expected wage at $t+8$. We can, therefore, construct a time series for the average match quality in these matches by taking the predicted eight-year wage growth for t-start matches and adding it to the average starting wage for new hires at t. This yields 32 annual observations from which we can estimate cyclicality of match quality. In constructing the average starting wage at t, we control for the effects of the same demographic variables that are controlled for in estimating wage growth (gender, race, and education dummies and a cubic in age). So the implied measure of match quality should be viewed as net of the impact of these worker characteristics.

We regress our implied measure of match quality for hires at t on the unemployment rate at t and a cubic trend. The estimated coefficient implies that a 1 pp higher unemployment rate is associated with 0.06% lower quality of new hires (standard error 0.67%). Thus, our approach implies that the quality of new hires is acyclical. We discussed above the forces for quality of new matches to be countercyclical (recession's cleansing effect) or procyclical (recession's sullying effect). So our estimate of acylical match quality is consistent with these effects roughly canceling or neither being overly important.

4.2.2 Robustness to changing match quality during the match

We assume that quality growth within a match is not predicted by the state of the cycle when it started (our first identifying assumption). If matches that begin in recessions exhibit faster quality growth, that would bias our estimate towards a more procyclical wage. Conversely, if matches starting in recessions exhibit less quality growth, our estimate is countercyclically biased. Selection for remaining in the match can also bias our estimate if that selection acts differently for matches that start in recessions. For instance, if remaining in a match selects positively on match quality growth *and* that selection happens to be stronger for matches that begin in recessions, then our estimate would be procyclically biased.

Both Bowlus (1995) and Mustre-Del-Rio (2019) find from NLSY79 data that jobs that began in recessions exhibited somewhat shorter average duration.[20] This is suggestive that selection on shocks to match quality growth could differ by whether a match begins in a recession. For our sample, we similarly find lower match survival for matches that begin under higher unemployment rates, though the differential is quantitatively quite small. We estimate survival probabilities from a proportional Cox model as a function of the unemployment rate at the match's start, a cubic trend, and our standard controls for worker characteristics. We estimate survival for matches starting between 1980 and 2011 to be comparable to our sample for estimating match wage growth. We find that a 1 pp higher unemployment rate at the beginning of the job increases the separation hazard relative to the baseline by 1.61% (standard error 0.37%). Figure 3 presents the estimate by comparing a match that starts in a boom (blue line), evaluated at an initial unemployment rate of 4.3%, versus one that starts in a severe recession (red line), at an unemployment rate of 9.6%. While we strongly reject statistically that the survival functions are the same, the magnitude of that difference is slight.

Even though these survival rates differ little quantitatively for matches started in booms versus recessions, we perform four robustness exercises to address if match quality grows faster for matches that began in recessions: i) We examine proxies for match quality; ii) we shorten the duration we follow matches; iii) we control for cyclical selection in the estimation by controlling for a match's relative duration in its cohort of matches or based on a Heckman correction in our wage-growth estimates; iv) we follow wages for eight years from the start of job matches, even if the worker moves to a new match; but control for observable differences in match-quality between any new job at $t + 8$ versus the job started at t.

Changes in measures of match quality

We examine two measures of job quality to test whether starting in a boom or bust predicts greater within-match quality growth. Our first measure of quality change is based on any

[20] Mustre-Del-Rio (2019) further finds that those matches that *end* in a move to non-employment are the ones that exhibit longer durations if started in a boom, whereas those that end in a job-to-job transition actually exhibit shorter durations conditional on starting in a boom. He interprets this as the first set of matches showing procyclical quality, while the latter showing countercyclical quality. Repeating, because our approach is based on wage growth within matches, it is robust to such quality differences across matches.

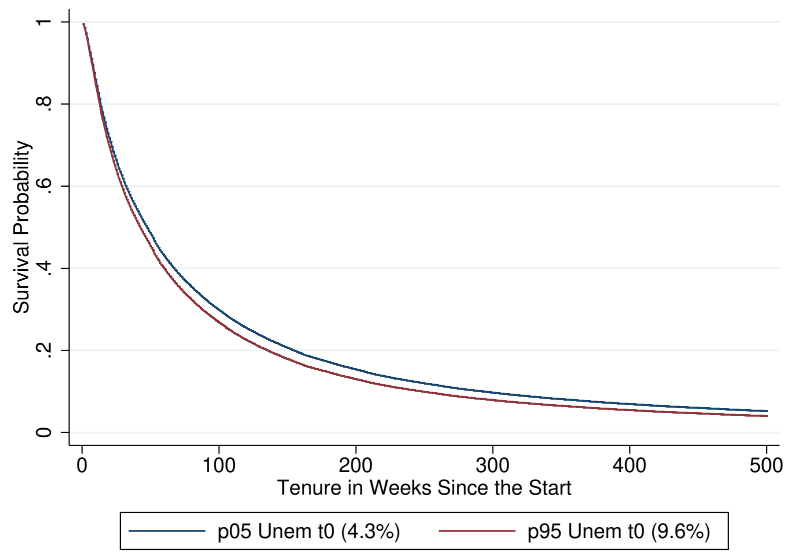


Figure 3: Survival Analysis

Notes: The figure shows the estimated survival probability from a proportional Cox model. The left-hand side in the model is the survival hazard, the right-hand side is the initial unemployment rate, cubic age polynomial, cubic time trend, and gender, education, and race dummies. We interact all variables (except the initial unemployment rate) with the dummy for the NLSY97 sample.

occupational upgrading within matches. The second is the growth in weekly hours worked during matches. Hours worked should positively reflect predictable increases in quality within matches because, being predictable, the quality change should not affect permanent income.[21]

To measure occupational upgrading, we construct an occupation quality index by regressing the log of hourly wage on a set of occupational dummies.[22] We then use the estimated coefficients on the dummies as our measure of occupation quality. Finally, we associate a quality index value for each wage observation and construct its change using any changes in occupational codes within matches across surveys.

Table 3, Column 2 presents the results from regressing annualized growth in the occupational wage index on the unemployment rate at the start of the match, the concurrent change

[21] More precisely, if predictable quality changes do not affect the marginal utility of consumption, then an efficient contract should yield a change in hours equal to the change in match quality times the Frisch elasticity of labor supply relevant for weekly hours.

[22] We use the crosswalk of David Autor and David Dorn to create a consistent occupation code between survey years. Unfortunately, the regular 3-digits codes are too fine for our exercise, having several occupations with only a few wage observations. We aggregate occupations to 2-digits, which gives 81 occupations. For example, occupation 166 – economists, market and survey researchers – is classified as group 16, together with i) Vocational and educational counselors, ii) Librarians, archivists, and curators, iii) Psychologists, and iv) Social scientists and sociologists. In the regression, we control for a worker's sex, race, and education, cubics in age and tenure, and survey-year fixed effects.

in the unemployment rate, and a cubic trend. We include all survey changes that fall within the first eight years of match tenure to be consistent with our estimates for the new-hire wage and user cost. Because these regressions, unlike those in Table 2, are estimated on the micro NLSY data, we cluster standard errors by survey year. We see no evidence that within-match occupational upgrading depends on the state of the economy when a match starts. From column 2, 1 pp. higher unemployment at match start predicts upgrading that would increase the wage by one-hundredth of one percent per year. High unemployment at the start also predicts declining unemployment during the match. But the impact of a decline in the unemployment rate on upgrading during the match is also extremely small and insignificant.

For comparison, the first column of the table gives results from estimating the same specification for annualized wage growth within the first eight years of matches. Consistent with our results for cyclicality of the new-hire wage from Table 2, matches display significantly faster growth of 0.32% per year for each additional percentage point of unemployment at their start (standard error 0.10%). If one were to deduct the estimated impact of occupational upgrading on wage growth from column 2, this would leave this magnitude essentially unaffected. We also see from column 1 that, consistent with wage smoothing, wage growth within matches is not significantly related to concurrent changes in the unemployment rate.

Table 2, Column 3 gives results for the growth of the workweek within matches. We again see no evidence that quality growth is greater for matches starting in recessions. The coefficient on the initial unemployment rate, -0.041 with standard error 0.046, actually suggests less quality growth within matches that start at higher unemployment rates, implying that within-match quality changes actually bias our results by making the new-hire wage appear less procyclical. But the implied bias is not especially large, nor statistically significant.

Robustness to following matches less than 8 years

Assumption 2 states that, for sufficiently large a, the $t+a$ quality-adjusted wage of a match started at t is uncorrelated with the cycle at t. Hence, any path-dependence of the initial match conditions on wages should have vanished after a years. Our benchmark estimates treat a to be eight years. We now consider reducing the threshold for a to six or even four years. Doing so presumably lessens the impact of any selection on idiosyncratic shocks to growth in match quality on our first-stage estimates of wage growth. The downside of shortening a is that it will also bias our estimates toward an acyclical new-hire wage to the extent that the impact of the cycle at t is still exhibited in wages at $t+6$ or $t+4$.

Table 4, Column 1 shows that the estimates of the cyclicality of the quality-adjusted new-hire wage are little affected by shortening a to six years. The impact of a one percentage

Table 3: Cyclicality of Quality Measures within Matches

	(1) Δ ln (wage)	(2) Δ (occ index)	(3) Δ ln (wk week)
Δ Unrate	-0.002	-0.001	-0.004
	(0.002)	(0.001)	(0.001)
Unrate at t_0	0.317	-0.011	-0.041
	(0.099)	(0.057)	(0.046)

Notes: Sample reflects 47,531 observations from 1980 to 2019 for matches started between 1980-2011 and have 8 years of tenure or less. Changes in wage, weekly hours, the occupation index, and the unemployment rate can reflect a one or two-year change across consecutive surveys. Changes are annualized, by dividing by the time in years between the observations, with observations also weighted by the time spanned by the change as well as the NLSY survey weight. Regressions also include a cubic trend, defined by the match's start year, controls for sex, race, education, and quadratics in age and tenure. We allow all coefficients to differ between the NLSY79 and NLSY97 samples except those on the cubic trend, initial unemployment rate, and change in unemployment rate. Standard errors are clustered by survey year.

point higher unemployment rate is now to reduce the new-hire wage by 2.07 percent, with standard error 0.51 percent, rather than by 2.30 percent. Cutting a to four years further reduces the cyclicality of the new-hire wage, with 1 pp in the unemployment rate predicting a 1.50 percent lower wage, standard error 0.57 percent. This could reflect that selection on wage change modestly increases our estimated cyclicality. It could alternatively reflect that the impact of the unemployment rate at t on the wage at $t+\tau$ subsides only two-thirds as much at $\tau = 4$ as at $\tau = 8$. Regardless, the estimated new-hire wage, even setting $a = 4$, is highly procyclical.

Table 4: Cyclicality of New-Hire Wage: Robustness to Cutoff Horizon

	(1) Cut off after 8 years	(2) Cut off after 6 years	(3) Cut off after 4 years
URate	-2.30	-2.07	-1.50
	(0.67)	(0.51)	(0.57)

Notes: 32 annual observations: 1980-2011. Coefficients are percent responses to the unemployment rate. Regressions include cubic trend. Robust standard errors in parentheses.

Robustness to controls for selection

Repeating, selection to remain in matches that display higher quality growth *can* bias procyclically our estimate *if* that selection acts more strongly for matches that begin in recessions. In Table 5, we extend our benchmark results by including controls for such selection in our first step that estimates wage growth as a function of year and match start date.

We first control for a match's relative realized duration, relative to its cohort, in predicting its wage growth in equation (13). Match cohort refers to the set of matches starting in the same year. Relative duration is measured by the ventile of a match's realized duration in its cohort. The logic of controlling for relative duration is as follows. Assume that longer duration within a cohort proxies for better shocks to match quality. If so, controlling for relative duration in our wage-growth equations controls, at least partially, for the impact of match quality shocks. Because matches that start in recessions have lower average realized duration, the observed wage changes at any specific duration τ, e.g., from $t+\tau-1$ to $t+\tau$, will be systematically associated with higher *relative* within-cohort duration for cohorts starting in a recession. For this reason, controlling for realized duration's effect on wage growth will, by extension, assume and control for better shocks to match quality τ periods into a match starting in a recession rather than a boom.

We find that a ventile increase in relative duration in a cohort does predict 0.48% higher annual wage growth, with standard error 0.04%. (We assume the impact of a ventile increase in relative duration on wage growth is the same across cohorts.) But, comparing Columns 1 and 2 from Table 5, controlling for this effect in our first stage has little effect on the estimated cyclicality of the new-hire wage: A one percentage point higher unemployment rate predicts a 2.39% lower wage with a standard error of 0.72%.

We next treat cyclical selection by employing a Heckman correction in wage-growth equation (13). So now our exercise is composed of three steps. The first step is a probit regression modeling whether a match that survives to $t + \tau - 1$ further survives to $t + \tau$, that is, whether we observe the match's rate of wage growth for $t + \tau$.[23] To help capture turnover the probit includes, in addition to all variables from the wage-growth regression, variables for marital status, residence in an urban area, and the number of children (ages less than 18) in the household.[24] In the second stage, our wage growth-regression controls for the inverse Mills ratio. Its coefficient is positive (1.29%) but not statistically significant (standard error of 1.02%), meaning that the average observed rate of wage growth is slightly higher for those that have a lower probability of selecting into the sample.

[23]More exactly, the dependent variable is equal to one if a match from one survey remains intact, at 25 hours per week or more, at the following survey so that wage growth for the match is observed across the surveys. We treat an observation as missing for our first step if the respondent departs from the NLSY sample between the two surveys.

[24]We allow coefficients for these variables to differ by NLSY survey. Economically and statistically significant effects in the probit include: Married or never-married respondents have a higher probability of staying in a match than those separated, divorced, or widowed; rural respondents have a higher probability of staying than urban; and having more children increases the probability of staying.

The third column of Table 5 reports the resulting cyclicality of the new-hire wage with predicted match wage growth augmented for the Heckman correction. Estimated cyclicality is smaller than our benchmark estimate, with a 1 pp. higher unemployment rate associated with a decrease in the new-hire wage of 2.03%, with standard error 0.64%. But the estimate still implies a new-hire wage that is economically and statistically highly procyclical.

Table 5: Cyclicality of New-Hire Wage: Robustness to Selection Controls

	(1) Benchmark	(2) Control Relative Duration	(3) Heckman Correction
URate	-2.30	-2.39	-2.03
	(0.67)	(0.72)	(0.64)

Notes: 32 annual observations: 1980-2011. Coefficients are percent responses to the unemployment rate. Regressions include cubic trend. Robust standard errors in parentheses. Because we add new regressors in these two specifications, our first-stage sample size is not the sample for all specifications. The baseline has 82,557 wage-growth observations, while the relative duration and the Heckman ones have 81,923 and 82,285, respectively. When estimating our baseline regressions again with the more restrictive samples, we obtain coefficients of -2.40 (0.67) and -2.30 (0.67).

Robustness to following all workers for 8 years

Finally, we check the robustness of our results to following wage growth for all workers starting matches at t until $t+8$, even those that have moved from the t-match by then.[25] The advantage of this alternative is that it removes any issue of cyclical selection on whom we can follow for eight years. The downside is that it violates the spirit of our approach by looking across matches for some workers. To limit that downside, in estimating wage growth from t to $t+8$ we include controls for match quality for the match observed at $t+8$ versus that started at t. These are average working hours in the match and dummies for the realized duration of the match (less than 2 years, 2 to 4 years, or more than 4 years). We presume that matches that generate higher working hours or last longer are of better quality on average. Of course, for matches that last to $t+8$, these variables take the same values at t and $t+8$. Including these controls is kindred to the approach to match quality in Doniger

[25] We construct the sample by associating the main matches for the following survey years with an initial set of matches. For example, for a match in 1980, we associate the respondent's main match in 1981, 1982, and so on. The main match is defined as the match that is active at survey time. If the respondent has more than one job active, we select the one with a higher working week. We then compute an 8-years wage growth. Given that the sample became bi-annual after some years, we also compute a 7-years change for those that we cannot compute the 8-years one. For each match, we select the first long change we observe that happened within the first three years of the match.

(2021), who includes such controls to control for the quality of new matches versus past and future matches in the worker's wage panel.

Table 5 reports estimated cyclicality of the new-hire wage constructed from wage growth for workers fully 8 years from match start, including those who leave the match. We restrict the sample to matches that last at least 18 months to be consistent with our previous results. Columns 1 and 2 give results respectively without and with the controls for match quality. A 1 pp. higher unemployment rate is associated with a 2.70% lower new-hire wage (standard error 0.62%). When we add the match-quality controls, the new-hire wage is slightly more cyclical, with coefficient −2.76% (standard error 0.58%). Both estimates imply modestly greater cyclicality than our benchmark estimate, −2.30.[26]

Our primary approach to control for quality exploits wage growth *within* matches. That requires us to impose a minimal match duration, which we set at 18 months, in order to calculate those wage changes. But the approach in Table 5, following all workers eight years, does not require that restriction. Columns (3) and (4) of the table repeats the estimations for all the matches in our sample, including those that last less than 18 months. Without match-quality controls, column 3, a 1 pp. higher unemployment rate is associated with a 3.10% lower new-hire wage (standard error 0.59%). Adding the match-quality controls, column 4 has little affect: the coefficient becomes −3.07% (standard error 0.55%). The finding in Table 5 of a *more* cyclical new-hire wage when all matches are included reassures us somewhat that our general finding of a highly cyclical new-hire wage is not driven by excluding matches shorter than 18 months.

4.3 Cyclicality of the user cost of labor

We now move to estimates of the cyclicality of labor's user cost. In the next subsection we examine the impact on the "pure" wage component of user cost, that is, the impact of the cycle on the quality-adjusted wage paths from starting a position at t rather than $t+1$. We then turn to consider the impact of the cycle on user cost if match quality reflects, not only match productivity, but also the survival rate of the match.

[26] The average workweeks enter positively in the cumulative wage-growth regression but are not statistically significant, with respective coefficients of 0.046% (standard error 0.078%) and 0.035% (standard error 0.063%) for the current and the 8-years ahead matches. The dummies for realized match duration (2 to 4, and more than 4 years) have respective coefficients of −3.47% (standard error 0.97%) and −0.94% (standard error 1.04%) for the current match and 3.93% (standard error 1.42%) and 8.56% (standard error 1.12%) for the match 8-years ahead. But differences in these measures at t versus $t+8$ are little predicted by the unemployment rate at t.

Table 6: Cumulative Wage Growth 8 Years Ahead Even if Change Jobs

	(1)	(2)	(4)	(4)
	$\geq$ 18 mo. duration		All Matches	
	Quality Controls		Quality Controls	
	No	Yes	No	Yes
URate	-2.70	-2.76	-3.10	-3.07
	(0.62)	(0.58)	(0.59)	(0.55)

Notes: 32 annual observations: 1980-2011. Coefficients are percent responses to the unemployment rate. Regressions include cubic trend. Robust standard errors in parentheses. Quality controls reflect workweeks and realized duration in jobs started at t and working at $t+8$.

4.3.1 Cyclicality of Wage Paths

The wage-component of user cost will reflect cyclicality in the new-hire wage, just reported, as well as any cyclical differential in the wage path from $t+1$ forward for matches starting at t versus $t+1$. This latter effect is discounted to reflect match separation rates as well as for time discounting. To illustrate directly the role of future wage paths, we first consider a constant discount factor and separation rate, setting $\beta = 0.989$ and $\delta = 0.228$. The separation rate of 0.228 is estimated from the first eight years of the matches we construct from our NLSY samples.[27] We then move to our benchmark specification that constructs user cost allowing for time-varying separation rates and time-varying discount rates. We estimate the separation rate, δ_t, from year dummies in a linear probability model for staying in a match. This is described in Appendix B.2. We estimate a time-varying discount factor, β_t, based on movements in real consumption of nondurables and services as, for instance, in Bansal, Kiku, Shaliastovich, and Yaron (2014).[28]

Table 7 reports the cyclicality of labor's user cost. Assuming a constant separation rate and discount factor, we find that a 1 pp higher unemployment rate is associated with a 5.74% decline in the wage component of user cost, with a standard error of 2.04%. The high cyclicality of the user cost of labor is robust to allowing for cyclical discount and separation rates. Allowing only for cyclical β_t, row 3 of Table 7, a 1 pp higher unemployment rate reduces labor's cost by 5.97% (standard error 2.06%). That is slightly more procyclical than under our a constant β. Although the effective discount factor, $\beta_t(1-\delta)$, is highly procyclical, this

[27]The sample restrictions mirror those for our sample for estimating match wage-growth, except to estimate separation rates we do not require that a match lasts one year and a half.

[28]We restrict attention to CRRA preferences with an intertemporal elasticity of substitution equal to 0.5. More detail is provided in Appendix B.3.

has little influence on the cyclicality of user cost.[29] While higher discounting in recessions acts to lower the impact of future wage paths on user cost, the decline in discounting in booms acts in the opposite direction. In row 4 of Table 7, we allow for time variation in the separation rate as well as the discount rate. User cost is now estimated to have almost the same response to 1 pp of unemployment, -5.78% (standard error 2.13%), as under constant δ and β.[30]

Table 7: Cyclicality of Quality-Adjusted New-Hire Wage and User Cost

	Unemployment
New-Hire Wage	-2.30
	(0.67)
Wage Component of Labor's User Cost	
User Cost w/ constant δ and β	-5.74
	(2.04)
User Cost w/ time-varying discount rates	-5.97
	(2.06)
User Cost w/ time-varying separation & discount rates	-5.78
	(2.13)
User Cost w/ time-varying sep. & disc. rates, sep. rate start-date specific	-5.07
	(1.69)

Notes: 32 annual observations: 1980-2011. Regressions include cubic trend. Robust standard errors are in parentheses.

We next allow for the separation rate to vary both with the current year and the match's starting year, while continuing to allow β to time vary. (See equation 3 for the definition of user cost for this general case.) To implement, we estimate the separation rate as a function of a full set of dummies interacting match start-year with the current year. Allowing the separation rate to vary freely with current and start dates alters the discounting of future wage paths in two ways: Directly by affecting the values for $\delta_{t,t+\tau}$, and less directly by altering the probability of starting any future wage paths at date $t+\tau$. In terms of discounting, this specification allows separation rates to systematically decline with tenure, as seen in Figure

[29]For instance, regressing $\prod_{\tau=0}^{i} \beta_{t+\tau}(1-\delta)$ on the unemployment rate at t and a cubic trend yields respective coefficients for a pp of unemployment of -0.55 (0.24), -1.05 (0.25), and -0.56 (0.11) for $i = 0, 2, 6$.

[30]Our estimated combined discount factor, $\beta_t(1-\delta_t)$, is not clearly procyclical or countercyclical, and that cyclicality is not statistically significant. Regressing $\prod_{\tau=0}^{i} \beta_{t+\tau}(1-\delta_{t+\tau})$ on the unemployment rate (and trend) yields respective coefficients for 1 pp of unemployment of -0.18 (0.82), 0.18 (0.59), and -0.21 (0.27) for $i = 0, 2, 6$.

3. That acts to reduce the effective discounting, so we can anticipate some reduction in cyclicality of user cost.

The impact of allowing the general separation rate $\delta_{t,t+\tau}$ on discounted wages – the pure wage-component of labor's user cost – is presented in row 5 of Table 7. The estimated response to 1 pp of unemployment is -5.07 (standard error 1.69%). While this is less cyclical than our benchmark estimate that assumes $\delta_{t,t+\tau} = \delta_t$, it is still economically and statistically highly procyclical.

To put that impact in perspective, consider the 2007-09 recession: between 2007 and 2009, the unemployment rate went up by 3.5 pp, controlling for a cubic trend. Our estimates associate a decline in labor's user cost of more than 17 percent with such a large recession, implying a substantial decline of the price of labor. The user cost of labor is more than twice as cyclical as the quality-adjusted new-hire wage. Intuitively, consider a firm hiring a worker in a recession, with the unemployment rate high and the new-hire wage low. As the economy recovers, wages of these workers respond less to business cycle conditions than subsequent hires. Therefore, their present-discounted wages from $t+1$ forward are lower. We can isolate cyclicality of the discounted future wage path by simply subtracting the impact of the cycle on the new-hire wage from its impact on user cost: 1 pp higher unemployment reduces discounted future wages by -2.77%, with a standard error of 1.33%.

4.3.2 Adjusting for Heterogeneous Match Quality from Survival Rates

If cohorts of new hires display systematically different separation rates then, as discussed in Section 2.2, starting a position at t, rather than $t+1$ will affect future hiring costs. Here we explore the potential importance of that heterogeneity for cyclicality of labor's user cost.

To gauge the impact of cohort-specific separation rates on future hiring costs, we proceed as follows. We first construct what we label the hiring cost component of user cost: $UC_t^{\kappa} = E_t \sum_{\tau=0}^{\infty} \mathfrak{B}_{t,t+\tau}\big(\pi_{t,t+\tau} - \pi_{t+1,t+\tau}\big)\kappa_{t+\tau}$ using our estimates for the $\delta_{t,t+\tau}$'s and β_t's discussed just above. But note, variations UC_t^{κ} will reflect fluctuations in β_t's and separation rates that do not reflect any cohort effect from hiring at at t. To address the impact of hiring in a recession, we recalculate UC^{κ} suppressing the role of the business cycle at a match's start on its subsequent separation rates. More exactly, we take our estimated series for separation rates, $\widehat{\delta}_{t,t+\tau}$'s and calculate a hypothetical set of separation rates, $\tilde{\delta}_{t,t+\tau}$, that removes the estimated impact of the unemployment rate at match start. We base that adjustment on our estimates of the hazard function from Section 4.2.2, where we found that the separation rate is increased by 1.61%, relative to its baseline hazard, for a 1 pp increase in unemployment at match start. We next estimate cyclicality in $\ln UC_t^{\kappa}$ under these counterfactual separation rates. Finally, we contrast cyclicality of $\ln UC_t^{\kappa}$ without and with controlling for the impact of unemployment rate at a match's start on its subsequent separation rate.

For hiring costs we consider two scenarios. We first follow the presentation in Section 2.2, by assuming that a hiring cost is only incurred in the starting period. We set that cost, κ, equal to one fourth, that is, the equivalent of one quarter of wages. This is fairly large relative to typical values in the literature. For instance it is a bit larger than costs calculated by Silva and Toledo (2013) for hiring and training. It is roughly the size of fees that head hunters typically charge to fill positions, which are presumably positions that are relatively difficult to fill.[31]

Alternatively, we allow for a hiring/training costs that persist at a declining level over a number of years into a match. These declining costs imply that rents to the employer grow over time. (Growth in match productivity would act similarly.) This adds to the user cost of matching with a cohort that is more likely to separate. We introduce this growth by extending κ_t to take the more general form $\kappa_{t,\tau} = (1+\alpha)^{N-\tau} - 1$ for $\tau \leq N$, and 0 for $\tau > N$. In the first period the cost is $[(1+\alpha)^N - 1]\%$ of the long-run wage; it then falls gradually, generating rents to the firm that rise at a rate of α percent of wages per year for N years. We choose $\alpha = 0.035$ and $N = 8$. These imply a first-period cost $\kappa_{t,0} = 0.32$, so somewhat higher than our prior choice of a one-time cost $\kappa = 0.25$. Given the discounting implied by our estimates for β_t's and $\delta_{t,t+\tau}$'s, the present-discounted value of these hiring/training costs average 0.72, so nearly three times that of our calibrated case of a one-time cost. The 3.5% rate for α corresponds to the average rate of wage growth we observe within matches in our sample.[32] Thus we are implicitly assuming that firm rents grow fully as much during the eight years as do the wages received by workers. We view this as a generous calibration for growth in firm rents since a sizable portion of wage growth presumably reflects growth in the worker's general human capital, which will not be mirrored firm rents.

Table 8 presents our results for cyclicality of labor's user cost, augmented to adjust for match quality in terms of both productivity and separation rates. For comparison, the first two rows repeat the results from Table 7 for cyclicality in the new-hire wage and the pure wage-component of user cost. For the case of $\kappa_{t,\tau} = \kappa_t = 0.25$, we find that 1 pp of unemployment has a sizable effect on future hiring costs, UC_t^{κ}, of 3.04% (standard error of 0.62%). In order to put this impact into terms comparable to the estimates of the wage

[31] The *Indeed* Editorial Team reports that headhunter fees are typically 20-25% of a position's annual pay (https://www.indeed.com/career-advice/finding-a-job/headhunters-fee).

[32] Controlling for a quadratic in age, we estimate an average annual growth rate of 3.48% (standard error 0.35%) for the first eight years of match tenure in our sample, evaluated at the mean sample age of 34.5 years. (The average is 4.23% (0.50%) for the first four years, then 2.82% (0.25%) from years four to eight years.) Relatedly, Kehoe, Lopez, Midrigan, and Pastorino (2022) set the rate of human-capital growth in their model to 3.5%, citing estimates average wage growth from Rubinstein and Weiss (2006).

component of user cost, we need to weight this 3% by the relative importance of the hiring costs relative to the wage rate, which equals 0.25. The impact on user cost of making this adjustment is given in Row 3 of the table. The response of user cost to a pp of unemployment is reduced from -5.07% to -4.31% (standard error of 1.70%).

The last row of Table 8 shows the impact on user cost allowing for hiring/trades that decline gradually during the match. The impact of a 1 pp increase in unemployment on user cost is now reduced to -3.93% with standard error of 1.70%.[33] Thus the cyclicality of user cost is cut by nearly a fourth. Another way to view this is that the increase in labor costs associated with matches started in recessions having higher separation rates offsets about 40% of the reduction in future costs via lower wage rates.[34] Nevertheless, labor's user cost remains highly procyclical, decreasing by about 4% for each pp increase in the unemployment rate. For a very large recession, like the Great Recession, that implies a fall in the price of labor falls of upwards of 20%.

Table 8: Cyclicality of Quality-Adjusted New-Hire Wage and User Cost

	Unemployment
New-Hire Wage	-2.30
	(0.67)
User Cost (Table 7, row 5)	-5.07
	(1.69)
Heterogeneous Match Quality in Survival Rates	
User Cost w/ up-front hiring costs	-4.31
	(1.70)
User Cost w/ persistent training costs	-3.93
	(1.70)

Notes: 32 annual observations: 1980-2011. Regressions include cubic trend. Robust standard errors are in parentheses.

4.4 Robustness to measures of the business cycle

In Table 9, we report cyclicality in the new-hire wage and user cost of labor across alternative methods of detrending to define the cycle, as well as expressing the cycle in terms of (log of)

[33] In this case, 1 pp of unemployment increases future hiring costs, UC_t^{κ}, by 1.58% (standard error 0.31%). Weighting by 0.72, the ratio between hiring costs relative and the wage rate, we obtain the user cost cyclicality of -3.93% (standard error 1.70%).

[34] 1 pp of unemployment increase expected hiring costs by 1.14%, while reducing expected wages from $t+1$ forward by 2.77%.

real GDP rather than the unemployment rate. In addition to our benchmark of a cubic trend, we consider the following filters: a quadratic trend, two and one-sided Hodrick-Prescott (HP) filters (parameter 6.25), and the Hamilton Filter.

Looking at column (1) of Table 9, the cyclical response of the new-hire wage to the unemployment rate is fairly similar across the filters: It declines by a little more than 2% for a pp increase in unemployment defined relative to a quadratic or cubic trend; it declines by around 1.7% for a pp in unemployment defined by either HP filter or the Hamilton filter. So, regardless of the filter, the new-hire wage is both economically and statistically highly procyclical. Looking at column (2), the new-hire wage is highly procyclical regardless of whether the cycle is measured by unemployment or real GDP. The elasticity of the new-hire wage with respect to real GDP varies from 0.84 under the Hamilton filter to 1.44 under our benchmark of a cubic trend.

For user cost, in columns (3) and (4) we first consider the "pure" wage component of user cost, that adds the impact of the cycle on future wage paths to that for the new-hire wage. We allow the separation rate to vary both with the current year and the match's starting year and β to vary with time. The wage component of user cost varies from -4.8 to -5.6% for a pp of unemployment, across all the filters except the Hamilton. With the Hamilton filter, it declines by less, by -3.6% for a pp in unemployment (standard error 1.6%), but is still highly cyclical. The elasticity of the wage component of user cost with respect to real GDP is larger than that of the new-hire wage for all the filters: by about double for the quadratic and cubic trends and Hamilton filer, and by about triple for the two HP filters. As with the cycle measured by unemployment, the estimated standard errors for responses in user cost are uniformly larger than for the new-hire wage. But all estimated elasticities are significantly procyclical.

Lastly, columns (5) and (6) show the estimated cyclicality for the user cost augmented for the impact of cyclicality in the separation rates on future training costs — that is, the case of higher and persistent costs from Section 4.3.2. Adjusting for future training costs reduces cyclicality of user cost by about 20 to 25% across each of the filters, and regardless of measuring the cycle by the unemployment rate or real GDP.

5 Comparison with Prior Treatments of Quality

The literature has mainly used two approaches to control for quality to estimate cyclicality of new-hire wages. The first compares the new hires' wage to the worker's wage fixed-effect (e.g., Carneiro et al., 2012; Kudlyak, 2014). The second examines growth rates in wages, implicitly comparing the worker's new-hire wage to their wage at the end of the prior match (e.g., Bils,

Table 9: Robustness to Measure of Cycle

	New-Hire Wage		User Cost		Adj. User Cost	
	Unemp	log(GDP)	Unemp	log(GDP)	Unemp	log(GDP)
Quadratic trend	-2.41	1.33	-4.86	2.40	-3.66	1.94
	(0.39)	(0.18)	(1.45)	(0.62)	(1.46)	(0.63)
Cubic	-2.30	1.44	-5.07	2.71	-3.93	2.29
	(0.67)	(0.26)	(1.69)	(0.75)	(1.71)	(0.74)
HP filter	-1.73	1.11	-5.58	3.30	-4.30	2.76
	(0.70)	(0.35)	(2.41)	(1.26)	(2.42)	(1.22)
One-Sided HP filter	-1.82	1.21	-4.83	2.80	-3.61	2.27
	(0.46)	(0.26)	(2.25)	(1.32)	(2.00)	(1.14)
Hamilton Filter	-1.68	0.84	-3.58	1.63	-2.81	1.40
	(0.49)	(0.20)	(1.60)	(0.69)	(1.69)	(0.67)

Notes: All regressions have 32 annual observations from 1980-2011, except the ones using using Hamilton Filter that has 29 observations from 1983-2011. Robust standard errors are in parentheses.

1985; Gertler et al., 2020). This section discusses the biases affecting each approach. We estimate each on our NLS data, comparing the results to those from our new approach to adjust for cyclical match quality.

5.1 Individual fixed effects

Under the fixed-effect approach, the cyclicality of wages of new hires is estimated from:

$$\ln w_{t,t}^{ij} = \alpha \, Cycle_t + \ln w_{fe}^{i} + \epsilon_{t,t}^{ij}. \tag{15}$$

Here w_{fe}^{i} is a fixed effect in worker's wages; it serves as the control for worker/match quality. The fixed effect, $\widehat{\ln w_{fe}^{i}}$, is estimated using all available wage observations. Thus, the estimated quality-adjusted price of labor is:

$$\ln \widehat{\phi}_{t,t} = \ln \phi_{t,t} + (\ln q^{ij} - \widehat{\ln w_{fe}^{i}}). \tag{16}$$

This yields a biased estimate of new-hire wage cyclicality if

$$\text{Cov}\left(Cycle_t, \ln q^{ij} - \widehat{\ln w_{fe}^{i}}\right) \neq 0.$$

There are distinct reasons this might be the case. First, the worker's wage fixed effect, $\widehat{\ln w_{fe}^{i}}$, reflects match qualities in the individual's entire panel, not only on the job started at t. So, if match quality at t differs from the worker's average match quality over their sample, then this will affect estimated cyclicality. As discussed from the outset, this bias could be procyclic (sullying effect of recessions) or countercyclic (cleansing effect of recessions). By comparison, our approach is based on wage growth within matches. That eliminates the concern of using other matches' information when estimating new-hire wage cyclicality.

Second, if wages are smoothed, then the worker's wage fixed effect will reflect the impact of the cycle at t on the worker's wage in the periods subsequent to t. This is more problematic the shorter the worker panel. From equation (14), to the extent that $\widehat{\ln w^i_{fe}}$ reflects $\phi_{t,t}$, $\widehat{\phi}_{t,t}$ will understate fluctuations in $\phi_{t,t}$. Therefore, $\mathrm{Cov}(Cycle_t, \ln \widehat{\phi}_{t,t})$ will understate cyclicality of new-hire wages. By comparison, our exercise uses the expected long-run wage in a match as a proxy for its quality. More exactly, to alleviate the bias of cyclicality at t affecting the control for match quality, we look at the expected wage eight years ahead.

Table 10 gives estimates of wage cyclicality separately for stayers versus new hires controlling for a worker fixed effect on wages.[35] We find that wages for stayers are only mildly procyclical, decreasing by -0.54% for each pp increase in the unemployment rate (standard error 0.32%). New-hire wages are considerably more procyclical, decreasing by -1.86% for each pp in unemployment (standard error 0.38).[36] When estimated with fixed-effects, the new-hire wage is modestly less cyclical than based on our approach; but it is economically and statistically highly procyclical.

Table 10: Cyclicality of Wages, Fixed-effect Approach

	(1) log(wage)
Stayer × Urate	-0.54
	(0.32)
New Hires × Urate	-1.86
	(0.38)

Notes: The table shows the percent change in wages in response to a 1 pp increase in the unemployment rate. The sample is for 1980 to 2011; it reflects 78,491 observations weighted by survey sampling weights. Additional controls are a cubic trend and cubics in age and tenure. We allow all coefficients to differ for the NLSY79 and NLSY97, except the unemployment rate and cubic trend coefficients. Standard errors are clustered by survey year.

[35] We restrict our sample to matches active at the survey interview. If the respondent works multiple jobs, we select the one with higher hours per week (or longer tenure in the case of a tie).

[36] Our fixed-effects estimate of cyclicality for new-hire wages is in line with findings by Figueiredo (Forthcoming) for NLSY data and by Gertler et al. (2020) for SIPP data.

Cyclicality of new-hire wage, job-to-job versus via non-employment

Recently Gertler et al. (2020) estimate new-hire wage cyclicality from both a fixed-effects and wage-change specification stratifying new-hires by whether the match was job-to-job or preceded by a spell of non-employment. They estimate, based on Survey of Income and Program Participation (SIPP) data, that wages are more procyclical for job-to-job hires than hires transiting non-employment. Figueiredo (Forthcoming) finds a comparable pattern based on NLSY79 data. Gertler et al. (2020) interpret this differential in the context of a model that exhibits a procyclical wage bias for job-to-job hires because job-to-job movers leave particularly bad matches in booms. In terms of equation (16), they presume that $\mathrm{Cov}\left(Cycle_t, \ln q^{ij} - \widehat{\ln w^i_{fe}}\right) = 0$ for hires from non-employment while being positive for job-to-job hires. But an alternative interpretation is that $\mathrm{Cov}\left(Cycle_t, \ln q^{ij} - \widehat{\ln w^i_{fe}}\right) < 0$ for hires from non-employment, for instance, because workers entering unemployment in recessions leave particularly bad matches. Then the new-hire wage is countercyclically biased for hires that experienced non-employment. As discussed above, the literature is consistent with match quality being either pro or countercyclical. Our approach avoids the confounding effects of changes in match quality by exploiting wage changes within matches.

In Table 11, we estimate the fixed-effects specification allowing separate interactions of the unemployment rate for new hires that experienced non-employment and those hired directly from another job. Non-employment is defined by reporting any weeks not employed in the month prior to the start of the new match. Consistent with the estimates in Gertler et al. (2020) and Figueiredo (Forthcoming), with fixed effects as the implicit quality control, the estimates suggest much more procyclical wages for job-to-job hires: their coefficient for 1 pp of unemployment is -2.16% (standard error 0.49%) versus -1.20% (standard error 0.33%) for hires from non-employment.

For comparison, Row 1 of Table 12 gives estimates for our approach, but now it is estimated separately for hires from non-employment and job-to-job. We estimate greater cyclicality for job-to-job hires, but the difference is not statistically significant. The impact of 1 pp. of unemployment is -2.09% for hires from non-employment (standard error 0.97%) compared to -2.91% for those job-to-job (standard error 0.60%). Thus the new-hire wage is highly cyclical for both groups, especially compared to cyclicality in wages for all workers (See Table 1). Our approach yields greater cyclicality than using fixed effects both for hires from non-employment (-2.09 versus -1.20) and job-to-job (-2.91 versus -2.16). One interpretation is that fixed-effects estimates are biased by countercyclical match quality, especially for hires from non-employment. But, at the same time, it is not surprising that

Table 11: Fixed-effects, Splitting New Hires by whether Job-to-Job

	(1)
	log(wage)
Stayer × Urate	-0.54
	(0.32)
Via Non-Emp × Urate	-1.20
	(0.33)
Job-to-Job × Urate	-2.16
	(0.49)

Notes: The table shows the percent change in wages in response to a 1 pp increase in the unemployment rate. The sample is for 1980 to 2011; it reflects 66,423 observations weighted by survey sampling weights. Additional controls are a cubic trend and cubics in age and tenure. We allow all coefficients to differ for the NLSY79 and NLSY97, except the unemployment rate and cubic trend coefficients. Standard errors are clustered by survey year.

the fixed-effects estimate yields less cyclical wages for both types of hires, given that, if wages are smoothed, it is biased toward zero cyclicality.

We considered a number of specifications for robustness in the prior subsection. The results from the first row of Table 12 largely apply to these as well. For instance, the remaining rows of Table 12 show the breakdown for our exercises, including a Heckman correction in predicting wage growth or using a full eight years of wage growth for all workers, even those who move again before $t+8$. With the Heckman correction, Row 2, the results closely parallel our benchmark estimates in Row 1, though wages are a little less procyclical for both sets of new hires. Following wages for all new hires for eight years, Row 3, wages for new hires from non-employment and job-to-job are equally procyclical.

5.2 First differences

Under the wage-growth approach, the cyclicality of wages of new hires is estimated by:

$$\ln w^{ij}_{t,t} - \ln w^{ij-1}_{\cdot,t-1} = \alpha\,\Delta Cycle_t + (\epsilon^{ij}_{t,t} - \epsilon^{ij-1}_{\cdot,t-1}).$$

$w^{t-1}_{\cdot,t-1}$ is the wage for a job that began before, or at, $t-1$ *and* ended in $t-1$. As a result, a worker's wage at the end of their prior match implicitly serves as the control for match quality for the match starting at t, yielding an *estimated* change in new-hire wage:

$$\ln\left(\widehat{\frac{\phi_{t,t}}{\phi_{t-1,t-1}}}\right) = \ln\left(\frac{\phi_{t,t}}{\phi_{t-1,t-1}}\right) + \Big(\ln q^{ij} - \ln q^{ij-1}\Big) + \Big(\ln \phi_{t-1,t-1} - \ln \phi_{\cdot,t-1}\Big).$$

Table 12: Cyclicality of new-hire wage, job-to-job versus via non-employment

	All New Hires	Via Non-emp	Job-to-Job
Benchmark	-2.30	-2.09	-2.91
	(0.67)	(0.97)	(0.60)
Heckman Correction	-2.03	-1.72	-2.68
	(0.64)	(0.93)	(0.59)
8-years Change w/ Quality Controls	-2.89	-2.85	-2.68
	(0.61)	(0.66)	(0.65)

Notes: 32 annual observations: 1980-2011. Coefficients are percent responses to the unemployment rate. Regressions include cubic trend. Robust standard errors in parentheses. When estimating our baseline regressions again with the Heckman correction sample, we obtain coefficients -2.30 (0.67), -2.10 (0.96), -2.91 (0.60) for all hires, hires via non-employment, and hires via job transition, respectively.

q^{ij-1} is the actual quality for the prior job that ended in $t-1$; and $\phi_{\cdot,t-1}$ is the corresponding quality-adjusted wage. This estimate of the cyclicality of the new-hire wage is biased if:

$$\mathrm{Cov}(\Delta Cycle_t, \ln q^{ij} - \ln q^{ij-1}) + \mathrm{Cov}(\Delta Cycle_t, \ln \phi_{t-1,t-1} - \ln \phi_{\cdot,t-1}) \neq 0.$$

The first covariance is the simplest to interpret. It creates a procyclical bias if workers move to higher quality matches when the economy improves (the unemployment rate is falling), or a countercyclical bias if they move to worse matches. As discussed repeatedly above, the literature welcomes either prior.

The second covariance is zero if there is no wage smoothing, as $\phi_{\cdot,t-1} = \phi_{t-1,t-1}$. With wage smoothing its sign will reflect the autocorrelation of *changes in* the cycle. For instance, if an expansion (declining unemployment) is typically preceded by a bust (rising unemployment), then booms should produce $\phi_{\cdot,t-1} > \phi_{t-1,t-1}$. Therefore, $\mathrm{Cov}(\Delta Cycle_t, \ln \phi_{t-1,t-1} - \ln \phi_{\cdot,t-1}) < 0$, imparting a countercyclical bias to the wage-change estimate.

In the first column of Table 13, we present cyclicality of wages, separately for stayers and new hires, by regressing changes in log wages on changes in the unemployment rate for our NLSY sample as well as a quadratic trend.[37] Consistent with most earlier studies, we find that wage growth for new hires responds more to changes in the unemployment rate than that for stayers. A 1 pp higher change in the unemployment rate is associated with -1.12% lower wage growth for new hires (standard error 0.50%). Wage growth for stayers is essentially acyclical. The new-hire coefficient estimated from wage growth and changes in the unemployment rate is smaller than that estimated from our approach (-2.30%) or by fixed effects (-1.86%). But the estimates are not especially comparable as the definition

[37] As with the fixed-effects, we restrict our sample to jobs active at the survey interview and, if the respondent works multiple jobs, select the one with higher hours worked.

of the cycle here – changes in the unemployment rate – differs considerably from the cycle defined by filtering the level of the unemployment rate.

In Column 2, we distinguish job-to-job hires from those with a spell of non-employment. We find that wage changes are similarly procyclical for the two groups – with 1 pp higher growth in the unemployment rate reducing the rate of wage growth by about one percent. But the standard errors are sufficiently large that the estimate is not statistically significant for either group if viewed separately.[38]

Table 13: Cyclicality of Wages, First-Differences Approach

	(1) Δ log(wage)	(2) Δ log(wage)
Stayer ×Δ Urate	-0.16	-0.17
	(0.29)	(0.29)
New Hires ×Δ Urate	-1.12	
	(0.50)	
Via Non-Emp ×Δ Urate		-1.07
		(0.81)
Job-to-Job ×Δ Urate		-0.90
		(0.52)

Notes: The table shows the percent change in wages in response to 1 pp in the unemployment rate. The sample covers 1980 to 2011 reflecting 46,050 wage changes. Additional controls are dummies for sex, race and education groups, and quadratic trend, age and tenure polynomials. We allow all coefficients to differ for the NLSY79 and NLSY97, except the unemployment rate and quadratic trend coefficients. Standard errors are clustered by survey year. All regressions are estimated using survey sampling weights.

6 Conclusions

We estimate the cyclicality of the price of labor taking into account wage smoothing within matches and cyclical variation in match quality.

[38]From SIPP data, Gertler et al. (2020) estimate a positive relationship between changes in the unemployment rate and the rate of wage growth for new hires that is statistically significant for job-to-job hires but not for those that had a spell on non-employment. Beyond being different samples, the SIPP and NLSY data differ in their frequency of wage observation. The SIPP asks for respondents' wages at four-month intervals. Our NLSY data collect individuals' wages annually or biannually. The differences in frequencies not only affect the definition of the cycle but could also affect the importance of the alternative biases outlined in this subsection that affect estimates from the wage-growth specification.

We estimate that the new-hire wage is highly procyclical, decreasing by more than 2% for a 1 pp increase in the unemployment rate. Many prior studies have estimated highly procyclical wages for new hires. But those studies employed proxies for match quality (e.g., fixed effects) that reflect wages not only from the current match but also from past and future matches, thereby potentially biasing these estimates if match quality changes cyclically with job transitions. We construct a measure of match quality, the expected long-run match wage, to avoid any impact of quality changes across matches.

We find that the user cost of labor is even more procyclical, decreasing by about 4% for a 1 pp increase in the unemployment rate, or with an elasticity with respect to real GDP of about 2. This cyclicality in user cost reflects two effects, beyond that on the new-hire wage, that partially offset. Hiring during a recession, versus waiting, predicts a lower future path for wages. This could reflect wage smoothing, or simply wage stickiness, for ongoing matches. That impact on future match wages contributes a drop in user cost of almost 3% for a pp increase in unemployment. But hiring in a recession also predicts modestly higher match separation rates. For reasonable growth in employer surplus during matches, this impact on separation rates offset nearly half of the impact of the cycle on future wage paths.

Our results for labor's user cost require some force, or forces, for highly cyclical labor demand to explain fluctuations in employment and hours. It is common to introduce that force in models via procyclical productivity shocks. But, given that labor productivity was not procyclical for our sample period (e.g. Fernald and Wang, 2016), this suggests a key role for other drivers of procyclical labor demand. A number of explanations have been proposed in the literature. One is price stickiness that constrains sales during downturns, depressing labor demand. Countercyclical desired markups have a comparable upshot (Rotemberg and Woodford, 1999). If producing has an investment component, then tightening financial constraints will reduce production and labor demand, with no decline in labor productivity. Examples include models where, by producing more, firms expand their customer base (Gilchrist, Schoenle, Sim, and Zakrajšek, 2017), or generate a more productive future workforce (Kehoe et al., 2022). Yet another force suggested in the literature is via uncertainty. More exactly, uncertainty is modeled as reducing labor demand in combination with evidence that uncertainty is heightened during recessions Examples include Arellano et al. (2019), Jo and Lee (2022), and Wang (2022).

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A User Cost: Computation

We use a matrix notation to explain how we compute labor's user cost. We begin by defining several matrices in which i and j index rows (start date, t) and columns (current date, $t+\tau$):

$$B_{i,j} = \mathfrak{B}_{i,j} = \prod_{s=0}^{j-i-1} \beta_{i+s} \implies B = \begin{bmatrix} 1 & \beta_1 & \beta_1\beta_2 & \dots \\ 0 & 1 & \beta_2 & \dots \\ \vdots & \vdots & \vdots & \ddots \end{bmatrix}$$

$$D_{i,j} = \prod_{s=0}^{j-i-1} (1-\delta_{i,i+s}) \implies D = \begin{bmatrix} 1 & (1-\delta_{1,1}) & (1-\delta_{1,1})(1-\delta_{1,2}) & \dots \\ 0 & 1 & (1-\delta_{2,2}) & \dots \\ \vdots & \vdots & \vdots & \ddots \end{bmatrix}$$

We implement empirically these matrices as follow:

- For matrix B, we estimate β_t as a function of one-period consumption growth rate. For example, $\beta_{1980} = f(C_{1981}/C_{1980})$. See section B.3.
- For matrix D, we estimate δ_t and $\delta_{t,t+\tau}$ using a linear probability model. For example, δ_{1980} is the probability that a separation happened between 1980 and 1981. In the same logic, $\delta_{1980,1981}$ is the probability that a separation happened between 1981 and 1982 for a job that started in 1980. See section B.2.

With these two matrices on hand, we can easily compute the matrix of discounting factors, $\Lambda_{t+\tau}$, allowing both the time discount factor β and the separation rate δ to vary with time.

$$\Lambda_{i,j} = \mathfrak{B}_{i,j} \times D_{i,j} \implies \Lambda = B \circ D$$

where $\circ$ is the Hadamard product (or element-wise product).

We define a quality-adjusted wage matrix as

$$\phi_{i,j} = -\ln\left(\frac{w_{i,i+8}}{w_{i,i+j}}\right) = -\sum_{s=j}^{8} \ln\left(\frac{w_{i,s+1}}{w_{i,s}}\right) \implies \phi = -\begin{bmatrix} \ln\frac{w_{1,8}}{w_{1,1}} & \ln\frac{w_{1,8}}{w_{1,2}} & \ln\frac{w_{1,8}}{w_{1,3}} & \dots \\ 0 & \ln\frac{w_{2,9}}{w_{2,2}} & \ln\frac{w_{2,9}}{w_{2,3}} & \dots \\ \vdots & \vdots & \vdots & \ddots \end{bmatrix}.$$

We explain how we estimate the $\phi_{i,j}$ in section 3.2. The stream of wages vector, $\overrightarrow{\Phi}$, is computed as:

$$\overrightarrow{\Phi}_i = \sum_j (\Lambda \circ \phi)_{i,j} \implies \overrightarrow{\Phi} = \begin{bmatrix} \sum_j (\Lambda \circ \phi)_{1,j} \\ \sum_j (\Lambda \circ \phi)_{2,j} \\ \vdots \end{bmatrix}$$

For last, we define the matrix Ψ and Π:

$$\Psi_{i,j} = \prod_{s=0}^{j-2} \Big(1 - \delta_{i,i+s}\Big) \implies \Psi = \begin{bmatrix} 1 & 1 & (1-\delta_{1,1}) & (1-\delta_{1,1})(1-\delta_{1,2}) & \dots \\ 0 & 1 & 1 & (1-\delta_{2,2}) & \dots \\ \vdots & \vdots & \vdots & \vdots & \ddots \end{bmatrix}$$

$$\Pi_{i,j} = \pi_{t,t+\tau} - \pi_{t+1,t+\tau} \implies \Pi = \begin{bmatrix} 1 & -(1-\delta_{1,1}) & \Pi_{1,1}\Psi_{1,3}\delta_{1,2} + \Pi_{1,2}\Psi_{2,3}\delta_{2,2} & \dots \\ 0 & 1 & -(1-\delta_{2,2}) & \dots \\ \vdots & \vdots & \vdots & \ddots \end{bmatrix}$$

in which we refer to equation 2 for the definition of $\pi_{t,t+\tau} - \pi_{t+1,t+\tau}$. The user cost is then easily computed as $\overrightarrow{UC} = (C \circ \Pi) \times \overrightarrow{\Phi}$.

B Data Appendix

B.1 Sample Description

Table 14: Sample Description

	Full Sample Mean	NLSY 79 Mean	NLSY 97 Mean
Avg. Age	32.64	35.59	27.72
Fraction of Male	0.54	0.54	0.55
Avg. Years of Schooling	13.28	13.18	13.44
Avg. Hourly Wage	19.92	20.35	19.20
Avg. Weekly Hours	42.34	43.10	41.07
Avg. Tenure (in weeks)	207.76	251.67	134.67
Fraction new hires	0.29	0.27	0.32
Avg. match length	6.27	7.78	3.76
Avg. ind. length in panel	22.31	28.12	12.64
Observations	144,367	90,176	54,191

B.2 Separation rate series

We use a slightly different approach to estimate the separation rates, δ, as a function of their current dates. First, we construct a dummy that equals one if a match termination happens, $\mathbb{1}_{\delta}$. A termination event is defined as 1) the last observation in which we observe a match and the worker reports a reason why he left the job or 2) the last observation in which we observe a match and the worker appears in subsequent surveys. Second, we use all matches except those lasting less than half a year.

As with the wage regression, we employ the NLSY data to regress $\mathbb{1}_{\delta,t}^{ij}$ on dummies to capture when the job terminated (the t's). Different from the wage regression, we stratify each survey in a different set of dummies. Specifically, we estimate δ_d^s for survey s from the following regression:

$$\mathbb{1}_{\delta,t}^{ij} = \Psi x_t^{ij} + \sum_{d=1980}^{2018} \delta_d^{79}\ \mathbb{1}_{79}^{ij}\ D_d^{ij} + \sum_{d=1980}^{2018} \delta_d^{97}\ \mathbb{1}_{97}^{ij}\ D_d^{ij} + \epsilon_{t+\tau}^{ij}.$$

Dummy variables D_d^{ij} equal 1 if $d = t$ and 0 otherwise; $\mathbb{1}_{79}^{ij}$ and $\mathbb{1}_{97}^{ij}$ are dummies capturing survey instrument (NLSY79 or NLSY97); and x_t^{ij} reflects additional controls for individual characteristics that could affect the match separation. These are dummies capturing the respondent's sex, race, age, educational attainment, and survey instrument. We allow the a flexible function for age (several dummy variables) to ensure that most estimated probabilities be between 0 and 1.

When a NLSY survey runs annually, each estimated coefficient, $\widehat{\delta}_t^s$, captures the average separation that happened between period t and $t+1$ for a job that started at t. When it runs biannually, each estimated coefficient will capture the average separation that happened between t and $t+2$. In this case, we annualize the separation rate between years t and $t+1$ using the formula,

$$\tilde{\delta}_t^s = \tilde{\delta}_{t+1}^s = 1 - \sqrt{1 - \widehat{\delta}_t^s}$$

in which $\tilde{\delta}_t^s$ is the adjusted annual separation rate for the year t. When we have only one NLSY survey, we set the separation rate as the estimated coefficient for this survey. When both NLSY surveys run together, we aggregate the estimated coefficients giving each of them 1/2 as weight.

B.3 Discount rate series

We construct the time-varying discount rate series as a function of one-period consumption growth rates. Assuming a CRRA utility function, the Euler equation can be approximated by

$$\log(r_t) \approx \log(\beta) - \theta E_t \left[\Delta \log(C_t/C_{t-1})\right] \ .$$

θ is the risk aversion parameter, β is intertemporal discount factor, and $\log(C_t/C_{t-1})$ is the one-period consumption growth rate. First, we set $\beta = 0.989$, the inverse of the average real one-month T-bill rate is our sample, downloaded from Kenneth French's website. We use the personal consumption expenditures (PCE) price index to deflate the interest rate. Second, we use non-durable plus services consumption from NIPA as our measure of consumption. Third, we set $\theta = 2$, as usual in the macro literature. Finally, we adjust our time-varying discount factor series to have an average equal to β.

The user-cost of labor holds in expectation. However, we construct it using realized (or ex-post) wage growth. When we allow for a time-varying discount rate, we also use the realized discount rate and construct expectations when projecting our measure into the unemployment rate.