



The Quality-Adjusted Cyclical Price of Labor *

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Typical measures of wages, such as average hourly earnings, fail to capture cyclicality in the effective cost of labor in the presence of (i) cyclical fluctuations in the quality of worker-firm matches, or (ii) wages being smoothed within employment matches. To address both concerns, we estimate cyclicality in labor's user cost exploiting the longrun wage in a match to control for match quality. Using NLSY data for 1980 to 2019, we identify three channels by which hiring in a recession affects user cost: It lowers the new-hire wage; it lowers wages going forward in the match; but it also results in higher subsequent separations. All totaled, we find that labor's user cost is highly procyclical, increasing by more than 4% for a 1 pp decline in the unemployment rate. For large recessions, like the Great Recession, that implies a decline in the price of labor of about 15%.

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1 Introduction

Going back to Pigou’s *Industrial Fluctuations* (1927), economists have examined the cyclicity 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 cyclicity of real wages.¹ For many countries and most periods, average hourly earnings appear acyclical or modestly procyclical. But cyclicity 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 cyclicity 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 cyclicity 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 cyclicity 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

¹Pigou (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.”

²Hall (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 cyclicity 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.

³See, for example, Stockman (1983), Bills (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 concerns of cyclical bias from 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 cyclicalities 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 reflects expected future wages, will be more cyclical than the new-hire wage (Kudlyak, 2014).

We estimate the cyclicalities of the price of labor addressing (1) cyclicalities in firm-worker match quality, and (2) wage smoothing within matches. By the cyclical price of labor, we mean cyclicalities 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. Cyclicalities of that user cost reflect 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 cyclicalities of the quality-adjusted new-hire wage, then proceed to estimate the cyclicalities in labor's user cost.

We consider two components of match quality. First, we allow for cyclical variation in the productivity of new matches. Second, 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 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, being lower in recessions than booms, 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 within a match. 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 cyclicalities of quality-adjusted wages because wage effects that persist will be treated as quality, reducing cyclicalities 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 cyclicalities 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 1980 to 2019. From these, we can estimate cyclicalities of the new-hire wage and user cost for 1980 to 2012. 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 hires from non-employment as for those moving job-to-job.

We find that the user cost of labor is considerably more cyclical. The cycle has a large impact on future wage paths, the “lock-in effect” on wages from hiring in a recession. Combining the cycle’s impacts on the new-hire wage and future wages, 1 pp higher unemployment reduces the wage component of user cost by 5.3%. At the same time, the lower wages from hiring in a recession are partly offset by costs of higher expected turnover. But, even generously calibrating both hiring costs and growth in rents to firms within matches, the cycle’s impact on future turnover only offsets about one-fifth of its impact on wages. Accounting for all three effects, we estimate that labor’s user cost decreases by 4.2% for a 1 pp increase in unemployment. This represents an elasticity with respect to real GDP of about 2.5.

In terms of the literature, our approach to match quality 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. Other papers studying cyclicalities of match quality include Devereux (2004) and Figueiredo (2022).

Our focus on labor’s user cost follows Kudlyak (2014). The strong cyclicalities for user cost we find is in line with that estimated, using different methods to deal with match quality, by Kudlyak (2014) and Basu and House (2016), and by Doniger (2021) for non-college-trained workers. (Doniger estimates an even more cyclical user cost for college-trained workers.)⁴

In turn, the 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 relates closely studies that examine cyclicalities of wages for new hires versus incumbent workers (Bils, 1985; Carneiro, Guimaraes,

⁴Kudlyak, as well as Basu and House, primarily employs worker fixed-effects to control for quality. Doniger 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.

and Portugal, 2012; Martins, Solon, and Thomas, 2012; Haefke, Sonntag, and van Rens, 2013; Gertler, Huckfeldt, and Trigari, 2020; Grigsby, Hurst, and Yildirmaz, 2021).⁵ 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 cyclicity of wages for new hires and cyclicity of labor’s user cost. We describe our data and empirical implementation in Section 3. Results are presented in Section 4, including a number of robustness exercises with respect to our key assumptions. Section 5 compares our estimates for cyclicity of new-hire wages to those using prior approaches to control for quality. 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 history dependence in wages

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

$$w_{t,t+\tau}^{ij} = \phi_{t,t+\tau} q_{t,t+\tau}^{ij}, \quad (1)$$

where $q_{t,t+\tau}^{ij}$ is the idiosyncratic component of productivity, i.e., match quality. It reflects worker i , firm j , and worker-firm ij match effects. The two time subscripts allow match quality to depend on its start date and to potentially change over the course of the match.

A match’s quality, $q_{t,t+\tau}^{ij}$, reflects its idiosyncratic productivity. Therefore, netting it out of the wage yields a quality-adjusted wage, $\phi_{t,t+\tau}$. For instance, the quality-adjusted new-hire wage is $\phi_{t,t}$. Being relative to match productivity, $\phi_{t,t+\tau}$ is quality-adjusted from the firm’s perspective. A quality-adjusted wage from the worker’s perspective would instead net out amenity values of the match. $\phi_{t,t+\tau}$ is an aggregate wage, in that it does not reflect the characteristics of the particular match other than its start date. In order to explain the implications of wage smoothing for measures of cyclicity in the price of labor, consider, through the next subsection, that one can measure or control for $q_{t,t+\tau}^{ij}$.

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

⁵Carneiro, Guimaraes, and Portugal (2012) and Martins, Solon, and Thomas (2012) each find greater cyclicity of wages for new hires in Portugal even controlling for measures of quality. Carneiro, Guimaraes, and Portugal (2012) employ firm fixed-effects as quality controls, while Martins, Solon, and Thomas (2012) restrict attention to entry-level jobs in order to reduce variation in quality.

consistently estimate cyclical behavior in $\phi_{t+\tau}$ based on behavior of average wages, new-hire wages, or any other subset. But, out of concern that wages within matches are insulated from market fluctuations, many papers look at wages 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:

$$\ln \phi_{t,t} - \sum_{k=0}^{\infty} \omega_k \ln \phi_{t-k,t} = \sum_{k=1}^{\infty} \omega_k \left(\ln \phi_{t,t} - \ln \phi_{t-k,t} \right),$$

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

But, if $\phi_{t,t}$ differs from $\phi_{t-1,t}$, then one should logically expect that $\phi_{t+1,t}$ can differ from $\phi_{t,t+1}$, $\phi_{t+1,t+2}$ from $\phi_{t,t+2}$, 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 cyclical behavior 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 concreteness, write that match value as:

$$\begin{aligned} q_t V_t &= q_t \left[-\kappa_t + E_t \sum_{\tau=0}^{\infty} \Lambda_{t,t+\tau} \left(\frac{y_{t,t+\tau}}{q_t} - \frac{w_{t,t+\tau}}{q_t} \right) \right] \\ &= q_t \left[-\kappa_t + E_t \sum_{\tau=0}^{\infty} \Lambda_{t,t+\tau} \left(z_{t+\tau} - \phi_{t,t+\tau} \right) \right], \end{aligned} \tag{2}$$

$$\text{where: } \Lambda_{t,t+\tau} = \prod_{k=0}^{\tau-1} \beta_{t+k} (1 - \delta_{t,t+k}), \text{ with } \Lambda_{t,t} = 1.$$

Firm and worker subscripts are kept implicit. For convenience, we assume here that match quality, q_t , is fixed during the match, though we allow below for match surplus to increase with match tenure. $\kappa_t \cdot q_t$ is the cost of finding and training a worker, which we assume scales by the match quality. $y_{t,t+\tau}$ denotes the marginal revenue product of the match in $t + \tau$. $y_{t,t+\tau} = z_{t+\tau} q_t$; that is, it reflects both match quality, q_t , and an aggregate cyclical term, $z_{t+\tau}$. Note that all costs and benefits scale by match quality, q_t . So, for this subsection, we normalize q_t to one. E_t is the expectations operator conditional on t information. The discounting factor, $\Lambda_{t,t+\tau}$, allows both the time-discount factor β and the separation rate δ to vary with time. It also allows δ to depend on t , the start date of the match.

Equation (2) maps to models of vacancy creation with our cost of match creation, κ_t , corresponding to a vacancy's posting cost relative to its probability of yielding a match. In that literature a free-entry condition is typically imposed: $V_t = 0$. If the quality-adjusted wage $\phi_{t,t+\tau}$, fluctuates around a path normalized to one, while $z_{t+\tau}$ fluctuates around path z , then $z > 1$ allows firms to recoup upfront costs κ_t , consistent with free entry.

Our goal is to measure the cyclical price of labor allowing for history dependence in wages (wage-smoothing) as well as possibly in separation rates. However, given most business-cycle models abstract from these factors, we first define the user cost of labor in that simplified setting. Without history-dependence in wages and separation rates, we can write, respectively, $\phi_{t,t+\tau} = \phi_{t+\tau}$ and $\delta_{t,t+\tau} = \delta_{t+\tau}$. To further simplify the exposition, we start by assuming a constant separation rate, δ , and a constant discount factor, β .

Consider the perturbation of starting one more match at t , while starting $(1 - \delta)$ fewer at $t + 1$. That leaves expected labor input unaffected at $t + 1$ and beyond.⁶ The expected net gain is:

$$V_t - (1 - \delta)\beta E_t V_{t+1} = z_t - (\kappa_t - (1 - \delta)\beta E_t[\kappa_{t+1}]) - \phi_t.$$

The perturbation's net gain is independent of future productivities and wage rates because it does not affect future labor input. Assuming an interior solution, with non-zero match creation at $t + 1$, this perturbation should yield zero expected gain. In turn, that implies marginal revenue product, z_t , is equated to labor's user cost, $\kappa_t - (1 - \delta)\beta E_t[\kappa_{t+1}] + \phi_t$. In this context, ϕ_t is the wage component of that user cost. But, given no history-dependence in wages, it is simply the (quality-adjusted) wage at t , common across match durations.

With history dependence, a match's wage also reflects its start date. The net gain from the perturbation is then:

$$V_t - (1 - \delta)\beta E_t V_{t+1} = z_t - (\kappa_t - (1 - \delta)\beta E_t[\kappa_{t+1}]) - \phi_{t,t} - E_t \sum_{\tau=1}^{\infty} \beta^\tau (1 - \delta)^\tau (\phi_{t,t+\tau} - \phi_{t+1,t+\tau}).$$

The wage component of labor cost is now: $\phi_{t,t} + E_t \sum_{\tau=1}^{\infty} \beta^\tau (1 - \delta)^\tau (\phi_{t,t+\tau} - \phi_{t+1,t+\tau})$. The first component is the new-hire wage, $\phi_{t,t}$, while the latter reflects the impact of hiring at t versus $t + 1$ on future wages. Kudlyak (2014), Basu and House (2016), and Doniger (2021) each find that the wage component of user cost, as just defined, is considerably more cyclical than either the average or new hire wage. Those findings reflect the following. Empirically, high unemployment reduces the new-hire wage, with that lower wage persisting into the match. Therefore, hiring in a bust allows the firm to partially lock in a lower wage rate. If

⁶Given we normalized quality of t -started matches to one, we can view this more explicitly as starting $(1 - \delta)/q_{t+1}$ fewer physical matches at $t + 1$. But, as both the costs and benefits of those matches gets scaled by q_{t+1} , we can simply treat any future matches as being of the normalized unit of quality.

discounting is not too extreme, and the lock-in effect on wages not too transitory, then the wage component of labor's user cost can be much more cyclical than the new-hire wage.

Before moving on, we make three additional comments. One is that history-dependence in wages is often motivated from models of risk sharing, such as Thomas and Worrall (1988). But the relevance of user cost as a measure of wage cyclicalities does not hinge on the source of history dependence. In particular, models of sticky wages, e.g., Calvo (1983), imply that hiring at t , versus $t + 1$ affects future match wages, unless one adds a strong assumption that wages of new hires are literally bound to that of existing workers. The second point is that measuring wage cyclicalities by labor's user cost nests the case of no history dependence. Thus, it provides a robust measure of wage cyclicalities regardless of the presence of history dependence, whereas average or new-hire wages do not. The final point is that, if one disciplines a model by its cyclical price of labor, the appropriate corresponding data moment is the wage component of labor's user cost, regardless of whether the particular model in question generates such history dependence in wages.

Now return to the general case: Separation and discount rates vary over time, while the separation rate can also be a function of a match's start date. For this case, it is useful to consider the payoff of starting and maintaining a continuing position. That requires creating a new match each time one ends.⁷ The expected discounted value, per unit of quality, of starting a position in t is:

$$\mathcal{P}_t = E_t \sum_{\tau=0}^{\infty} \mathfrak{B}_{t,t+\tau} \pi_{t,t+\tau} V_{t+\tau}, \quad \text{where } \mathfrak{B}_{t,t+\tau} = \prod_{k=0}^{\tau-1} \beta_{t+k} \text{ with } \mathfrak{B}_{t,t} = 1. \quad (3)$$

$\pi_{t,t+\tau}$ is the probability that a new match will be required in $t + \tau$, given that the original position began in t . For instance, $\pi_{t,t} = 1$, $\pi_{t,t+1} = \delta_{t,t}$, $\pi_{t,t+2} = (1 - \delta_{t,t})\delta_{t,t+1} + \delta_{t,t}\delta_{t+1,t+1}$, and so forth.

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

$$E_t(\mathcal{P}_t - \beta_t \mathcal{P}_{t+1}) = z_t - E_t \sum_{\tau=0}^{\infty} \mathfrak{B}_{t,t+\tau} (\pi_{t,t+\tau} - \pi_{t+1,t+\tau}) (\kappa_{t+\tau} + \Phi_{t+\tau}), \quad (4)$$

where $\Phi_{t+\tau} = \sum_{k=0}^{\infty} \Lambda_{t+\tau,t+\tau+k} \phi_{t+\tau,t+\tau+k}$.

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 $t + \tau$. Discounting reflects the time-discount factor and the match's survival probability. The impact on wage payments

⁷More exactly, the number of positions is maintained in expectation. So, conditional on a separation, sufficient resources are spent on hiring to add back the departed labor.

of beginning the position in t , rather than $t + 1$, is the wage component of labor's user cost:

$$UC_t^W = E_t \sum_{\tau=0}^{\infty} \mathfrak{B}_{t,t+\tau} (\pi_{t,t+\tau} - \pi_{t+1,t+\tau}) \Phi_{t+\tau}. \quad (5)$$

The costs $\kappa_{t+\tau}$ and $\Phi_{t+\tau}$ get reflected in $E_t(\mathcal{P}_t - \beta_t \mathcal{P}_{t+1})$ 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 (4) by $\pi_{t,t+\tau} - \pi_{t+1,t+\tau}$. Clearly, $\pi_{t,t+\tau} - \pi_{t+1,t+\tau}$ will differ for $\tau = 0$ and $\tau = 1$, 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 $\tau > 1$:

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

where $\Psi_{t+k,t+\tau-1} = \prod_{\ell=k}^{\tau-2} (1 - \delta_{t+k,t+\ell})$,

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

If $\delta_{t,t+\tau} = \delta_{t+\tau}$, that is, the separation rate is time-varying, but not specific to a match's start date, then $\pi_{t,t+\tau} = \delta_{t+\tau-1}$. In turn, equations (4) and (5) reduce to:

$$E_t(\mathcal{P}_t - \beta_t \mathcal{P}_{t+1}) = z_t - (\kappa_t - E_t \beta_t (1 - \delta_t) \kappa_{t+1}) - UC_t^W,$$

where $UC_t^W = \phi_{t,t} + E_t \sum_{\tau=1}^{\infty} \Lambda_{t,t+\tau} (\phi_{t,t+\tau} - \phi_{t+1,t+\tau})$, for $\delta_{t,t+\tau} = \delta_{t+\tau}$. (6)

We treat (6) as our baseline specification in the empirical section. Again, the wage component of user cost, UC_t^W , reflects the new hire wage, $\phi_{t,t}$, and the impact of hiring at t versus $t + 1$ on future wage paths, discounted to reflect time and probability of separating.

For the empirics, we will consider the \ln of user cost. Taking a first-order approximation to equation (6) in the neighborhood of $\phi_{t+1,t+\tau} = \phi_{t,t+\tau}$, that is, in the neighborhood of no wage history dependence, yields:⁸

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

⁸To see this, rewrite eq. (6), taking into account that $\phi_{t,t}$ is in the information set at t , as:

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

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

$$\ln UC_t^W \approx E_t \left[\ln \phi_{t,t} + \sum_{\tau=1}^{\infty} \Lambda_{t,t+\tau} (\ln \phi_{t,t+\tau} - \ln \phi_{t+1,t+\tau}) \right]. \quad (7)$$

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 future wage paths. However, starting the position at t , rather than $t + 1$ will also affect 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 $(\pi_{t,t+\tau} - \pi_{t+1,t+\tau})\kappa_{t+\tau}$ at $t + \tau$. Suppose matches started in recessions 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 cyclicity 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 (4) to allow for the possibility that the flow of match rents to the firm grow with its duration.⁹

2.3 Identifying match quality by its expected long-run wage

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 cyclicity in the new-hire wage, while controlling for match quality, then proceeds to examine cyclicity 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.

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. Its cyclicity is:

$$\begin{aligned} \text{Cov}(\text{Cycle}_t, \ln \phi_{t,t}) &= \text{Cov}(\text{Cycle}_t, \ln w_{t,t}^{ij}) - \text{Cov}(\text{Cycle}_t, \ln q_{t,t}^{ij}) \\ &= \text{Cov}(\text{Cycle}_t, \ln w_{t,t}) - \text{Cov}(\text{Cycle}_t, \ln q_{t,t}), \end{aligned} \quad (8)$$

⁹We thank 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.

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 (8) 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 (8) that cyclical-ity of the new-hire wage provides a biased estimate of the cyclical-ity of the quality-adjusted new-hire wage unless $\text{Cov}(Cycle_t, \ln q_{t,t}) = 0$.

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 the average pre-displacement wage of the unemployed pool is higher during recessions. This could suggest that the quality of hires is countercyclical. Uncontrolled for, this will act as a countercyclical bias in new-hire wages. At the same time, several papers estimate a sullyng effect of recessions, with “good jobs” not hiring (Barlevy, 2002; Carneiro, Guimaraes, and Portugal, 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:

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

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

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

$$\begin{aligned} \text{Cov}(Cycle_t, \ln \phi_{t,t}) &= \text{Cov}(Cycle_t, \ln w_{t,t} - \ln w_{t,t+\tau}) \\ &\quad + \text{Cov}(Cycle_t, \ln q_{t,t+\tau} - \ln q_{t,t}) + \text{Cov}(Cycle_t, \ln \phi_{t,t+\tau}), \end{aligned} \tag{9}$$

where $(\ln w_{t,t} - \ln w_{t,t+\tau})$ and $(\ln q_{t,t+\tau} - \ln q_{t,t})$ denote the population means of $(\ln w_{t,t}^{ij} - \ln w_{t,t+\tau}^{ij})$ and $(\ln q_{t,t+\tau}^{ij} - \ln q_{t,t}^{ij})$ for jobs starting at t . For example, $\ln w_{t,t} - \ln w_{t,t+a} = \int_{ij} (\ln w_{t,t}^{ij} - \ln w_{t,t+a}^{ij})$. For exposition, we set aside here the important question of whether matches survive from t to $t + \tau$, taking all matches started at t as the population for means $(\ln w_{t,t} - \ln w_{t,t+\tau})$ and $(\ln q_{t,t+\tau} - \ln q_{t,t})$. 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:

$$\text{Cov}(Cycle_t, \ln q_{t,t+\tau} - \ln q_{t,t}) = 0 \tag{10}$$

Assumption 1 states that the mean change in quality for matches started at t is orthogonal to the cycle at t . We provide empirical support for this assumption in Section 4.2.3. For instance, we show there that occupational upgrading within matches, measured as reporting a new occupational code associated with higher average wages, is not stronger within matches starting in recessions than in booms.¹⁰

Assumption 2:

$$\text{Cov}(\text{Cycle}_t, \ln \phi_{t,t+a}) = 0, \text{ for } a \text{ sufficiently large.} \quad (11)$$

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

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, $a = 8$ years, given measures of the cycle at t and $t + 8$.

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 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 (9):

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

$$\text{Cov}(\text{Cycle}_t, \ln \phi_{t,t}) = \text{Cov}(\text{Cycle}_t, \ln w_{t,t} - \ln w_{t,t+a}) \text{ for } a \gg 1. \quad (12)$$

That is, cyclicity of the quality-adjusted new-hire wage equals the *negative* of cyclicity of the match's cumulative wage growth as it moves to its long-term expected wage. Note that

¹⁰On-the-job training models are ambiguous on whether investment in should be greater in matches beginning in recessions or booms. If workers' marginal revenue products are lower in recessions, this is a force to substitute toward investment. But we estimate below that time-discount factors, β 's, are lower in recessions and that separation rates are higher for matches that begin in recessions. That works to suppress on-the-job training in matches started in recessions, especially in skills specific to the match. While it is difficult to measure informal training, the evidence from the NLSY, see Méndez and Sepúlveda (2012), is that training by those employed is, if anything, procyclical. More exactly, Méndez and Sepúlveda find that training is acyclical for less-skilled workers, while quite procyclical for higher-skilled workers.

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 $t+a$. (The figure abstracts from any life-cycle or secular trends in match productivity and wages.) 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 $q_{t,t}$, 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 a specification that assumes the separation rate varies only with time, $\delta_{t,t+\tau} = \delta_{t+\tau}$. From equation (7), its cyclical nature 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 cyclical nature of the quality-adjusted wage τ periods into the match, $\text{Cov}(Cycle_t, \ln \phi_{t,t+\tau})$, is given by $\text{Cov}(Cycle_t, \ln w_{t,t+\tau} - \ln w_{t,t+a})$. Substituting in equation (7), we obtain:

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

$$\begin{aligned} \text{Cov}(Cycle_t, \ln UC_t^W) = & \text{Cov} \left(Cycle_t, \ln w_{t,t} - \ln w_{t,t+a} \right. \\ & \left. + \sum_{\tau=1}^a \Lambda_{t,t+\tau} \left[\left(\ln w_{t,t+\tau} - \ln w_{t,t+a} \right) - \left(\ln w_{t+1,t+\tau} - \ln w_{t+1,t+a+1} \right) \right] \right), \end{aligned} \quad (13)$$

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.¹¹ Here $\Lambda_{t,t+\tau} = \prod_{k=0}^{\tau-1} \beta_{t+k}(1 - \delta_{t+k})$.

There are two key observations from eq. (13). 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. The shaded area reflects the differentials in future wages hiring in a recession

¹¹Comparing with equation (7), note that the summation in (13) 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 (13). This assumes the realized deviations from expectations at t are orthogonal to the cyclical stage at t .

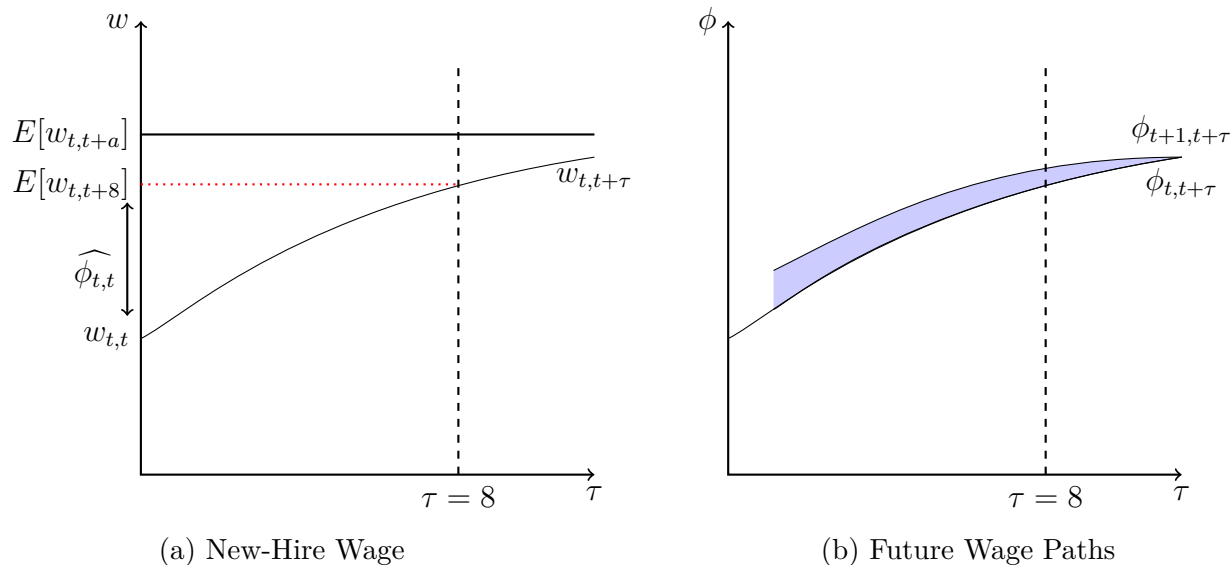


Figure 1: Illustration of Our Approach

at t rather than delaying one period. 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. 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 wage growth within those matches.

Our sample reflects the NLSY79 and NLSY97’s nationally-representative samples.¹² 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. 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.¹³ 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.¹⁴ For ongoing jobs, we assign the observed wage to the interview date; for jobs that have ended, we assign it to the job’s ending date. When available, we use a retrospective question for the wage at the start of a job.¹⁵ 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.

¹²We do still employ the NLSY sampling weights in all empirical work. These weights estimate how many U.S. individuals are represented by each respondent.

¹³If the respondent simultaneously works multiple jobs, we consider all jobs that satisfy our sample restrictions, including working at least 25 hours per week. 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.

¹⁴For 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 include tips, overtime, and bonuses; and ii) there is no way to create a pay rate that does not include this information.

¹⁵The BLS does not construct an hourly wage for these starting date wages, as they do for the survey wage. But all variables necessary to create that hourly wage are available: The pay rate, the time unit for that pay, and the usual weekly hours. Therefore, we construct a starting wage rate that corresponds to the BLS procedure for the survey-date wage. (See the NLSY documentation appendix.) The starting wage question began with the 1986 survey of the NLSY79; it was asked in all years of the NLSY97.

From the wage data, we construct an individual’s wage growth as the log difference of wage rates across 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. To calculate 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. Given many surveys are biannual, to calculate wage growth we exclude matches that do not reach 18 months duration. To deal with extreme values we exclude as missing wage growth rates outside of the 1st and 99th percentiles of the growth distribution in a 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 135,782 wage observations from 11,675 unique individuals. These reflect 83,151 NLSY79 observations from 5,697 individuals and 52,631 NLSY97 observations from 5,978 individuals.¹⁶ Table B1 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.

3.2 Estimation approach

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

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

in which $Outcome_t$ 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 lower frequency time trends. Our benchmark specification controls for a cubic trend. For robustness, we 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 to estimate the cyclicity of quality-adjusted new-hire wages and user cost.

¹⁶When working with growth rates, our sample consists of 83,367 observations from 10,832 distinct individuals (52,469 NLSY79 observations reflecting 5,296 individuals, and 30,898 NLSY97 observations from 5,536 individuals). 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.

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 cyclical nature 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 how we deal with 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: $\ln w_{t,t}^{ij} - \ln w_{t,t+a}^{ij} = -\sum_{\tau=1}^a \Delta \ln w_{t,t+\tau}^{ij}$. $\Delta \ln w_{t,t+\tau}^{ij}$ 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:

$$\text{Cov}(Cycle_t, \ln UC_t^W) = \text{Cov} \left(Cycle_t, \right. \\ \left. - \sum_{\tau=1}^a \Delta \ln w_{t,t+\tau} - \sum_{\tau=2}^a \Omega_{t,t+\tau} (\Delta \ln w_{t,t+\tau} - \Delta \ln w_{t+1,t+\tau}) + \Omega_{t,t+a+1} \Delta \ln w_{t+1,t+a+1} \right), \quad (14)$$

where $\Omega_{t,t+\tau} = \sum_{k=0}^{\tau-2} \left(\prod_{\ell=0}^k \beta_{t+\ell} (1 - \delta_{t+\ell}) \right)$; $\Omega_{t,t+a+1}$ is equal to $\Omega_{t,t+\tau}$ at $\tau = a + 1$; and $\Delta \ln w_{t,t+\tau} = \int_{ij} \Delta \ln w_{t,t+\tau}^{ij}$. Cyclical nature 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 cyclical nature 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).¹⁷ Specifically, we estimate the $\psi_{t,t+\tau}$'s from the following regression for workers' rates of wage growth within matches:

$$\Delta \ln w_{t,t+\tau}^{ij} = \Psi x_{t+\tau}^{ij} + \sum_{d_0=1980}^{2011} \sum_{d=d_0+1}^{2019} \psi_{d_0,d} D_{d_0,d}^{ij} + \epsilon_{t+\tau}^{ij}. \quad (15)$$

in which dummy 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 2019.¹⁸

Given estimates for $\psi_{t,t+\tau}$, we then substitute in equation (14) to obtain:

$$\begin{aligned} & \text{Cov}(\text{Cycle}_t, \ln UC_t^W) \\ &= \text{Cov} \left(\text{Cycle}_t, - \sum_{\tau=1}^a \widehat{\psi}_{t,t+\tau} - \sum_{\tau=2}^a \Omega_{t,t+\tau} (\widehat{\psi}_{t,t+\tau} - \widehat{\psi}_{t+1,t+\tau}) + \Omega_{t,t+a+1} \widehat{\psi}_{t+1,t+a+1} \right), \end{aligned} \quad (16)$$

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

Substituting $\widehat{\psi}_{t,t+\tau}$'s in Equation (16) 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: $E [\ln w_{t,t+\tau}^{ij} - \ln w_{t,t+\tau-1}^{ij} | \Gamma_{t,t+\tau-1}^{ij} = 1, \Gamma_{t,t+\tau}^{ij} = 1]$, where $\Gamma_{t,t+\tau-1}^{ij}$ and $\Gamma_{t,t+\tau}^{ij}$ 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 $E [\ln w_{t,t+\tau}^{ij} - \ln w_{t,t+\tau-1}^{ij}]$ 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 $t + \tau$.¹⁹

¹⁷For NLSY surveys that are 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 $t + \tau + 2$. We assign half of the original sampling weight for these two new observations.

¹⁸When estimating $\psi_{t,t+\tau}$, we require each combination of starting and current year, $(t,t+\tau)$, 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 $t + 2$.

¹⁹Selection 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

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 (14) based on surviving matches is $Cov\left(Cycle_t, E\left[\Delta \ln w_{t,t+\tau}^{ij} \mid \Gamma_{t,t+\tau-1}^{ij} = 1, \Gamma_{t,t+\tau}^{ij} = 1\right]\right)$ 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 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 highlight that our estimates are *not* biased by any differences in match quality that are fixed within a match. Any such differences, which have been the focus of the literature, e.g., Hagedorn and Manovskii (2013) or Gertler, Huckfeldt, and Trigari (2020), are differenced away by our first-step estimation of wage growth *within* matches. In particular, that removes the impact of cyclicality in job-ladders, such as formalized in Moscarini and Postel-Vinay (2013).²⁰ A corollary is that our estimates are unaffected by any selection on match duration driven by the fixed quality of a match because, again, the first-step estimates of wage growth removes those differences.

Our presentation here assumed the separation rate only depends on calendar year. But the estimation allow for $\delta_{t,t+\tau}$ to depend both on the start date t and the current period $t + \tau$. We estimate these fluctuations in the separation rate from the NLSY data. We estimate discount factors β_t based on fluctuations in the growth rate of consumption. Details for both are provided in Section 4.3 and in Appendices B.2 and B.3. The computation of user cost in its general form is described in Appendix A.

$t + \tau - 1$, then selection would presumably bias upwards realized wage changes, with the converse holding if the variance is greater at $t + \tau - 1$.

²⁰While a cyclical job-ladder can generate cyclical differences in within-match wage growth, these differences are consistent with our estimation approach. For example, in Moscarini and Postel-Vinay there is one-sided commitment, with firms committing to pay state-contingent wages. So if hiring in recessions is associated with a higher rate of growth in workers' outside options, this will be mirrored by higher within match wage growth. But this is exactly what our quality measure captures: recessions are periods of high expected wage growth, with wages depressed relative to their long-run level and match productivity.

4 Cyclicalities 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 labor costs. For this reason, we first estimate cyclicalities of the new-hire wage in Section 4.2, then cyclicalities of user cost in 4.3. Because most estimates of wage cyclicalities are based on average hourly earnings, we first examine cyclicalities for this measure in our NLSY data. Table 1 gives results from the NLSY data for 1980 to 2011, reflecting 110,047 observations from 11,363 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)	(2)	(3)
	Age Control	Individual FE	Match FE
Unemp Rate	-0.29	-0.83	-0.50
	(0.49)	(0.34)	(0.31)

Notes: Our sample – NLSY79 and NYSY97 panels – has 110,047 observations for 1980 to 2011. Regressions include a 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 -0.02 (0.33), -0.90 (0.27), and -0.53 (0.22), 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 0.29 percent for a 1 pp increase in unemployment rate with a standard error of 0.49.²¹ 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-

²¹For comparison, we estimate cyclicalities 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 non-supervisory employees, total private*. So it is a more restrictive sample of workers than we consider in the NLSY. Furthermore, given it reflects aggregate earnings relative to aggregate hours, in estimating cyclicalities 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).

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 -0.29 to -0.83 and is now statistically significant (standard error 0.34).

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 -0.50 with a standard error of 0.31. Echoing our discussion above, there are two clear competing explanations for why match controls reduce cyclical. 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 Cyclical 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 (15), from the NLSY worker-firm match histories. Those estimates reflect 72,990 observations from 8,963 individuals across 16,705 matches. We then construct our dependent variable, $-\sum_{\tau=1}^8 \widehat{\psi_{t,t+\tau}}$, to estimate new-hire wage cyclical. 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 .

Figure 2 presents the time series for our new-hire wage for the 32 annual observations for 1980 to 2011 for $a = 8$, together with the national unemployment rate. The new-hire wage is clearly highly procyclical. Most notably, it decreases by about 9% and 12% for the two large recessions in 1980-82 and 2007-2009. Table 2, Column 1 gives the estimated cyclical of the new-hire wage: The new-hire wage decreases by 2.35% for each percentage point cyclical increase in the unemployment rate, with a standard error of 0.67%.

Our approach relies on two assumptions. The first is that the cycle 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 cycle eight years later, or (ii) still helps to predict wages eight years into



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

Notes: The unemployment rate (right scale) is in percentage points. The new-hire wage (left scale) is in terms of percent and is normalized to average zero for the sample period (e.g., 0.1 means 10 percent above sample-period mean).

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 cyclical.

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 cyclical of the new-hire wage, with a response to a one percentage point higher unemployment rate at t of -2.49 percent (standard error 0.62).

4.2.2 Cyclical 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 for new hires 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 cyclical 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

Table 2: CYCLICALITY OF THE QUALITY-ADJUSTED NEW-HIRE WAGE

	(1)	(2)
URate	-2.35 (0.67)	-2.49 (0.62)
URate 8yr Ahead		1.04 (0.56)

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.

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 0.05% lower quality of new hires (standard error 0.65%). 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 sullyng effect). So our estimate of acyclical match quality is consistent with these effects roughly canceling or neither being overly important.

4.2.3 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. 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. We estimate survival probabilities from a proportional Cox model for matches starting between 1980 and 2011 as a function of the unemployment rate at the match's start, a cubic trend, and our standard controls for worker characteristics.

We find that a 1 pp higher unemployment rate at the beginning of the job increases the separation hazard relative to the baseline by 2.60% (standard error 0.38%). 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%.

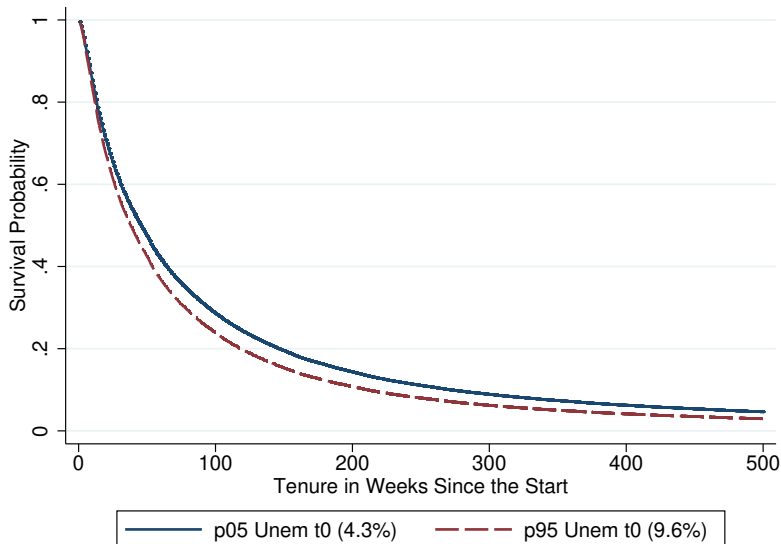


Figure 3: MATCH SURVIVAL ANALYSIS

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.

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 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.²²

To measure occupational upgrading, we construct an occupation quality index by regressing the log of hourly wage on a set of occupational dummies.²³ 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 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. 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, higher unemployment at match start has no effect on upgrading. 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 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 cyclicalities 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 adjust the estimated impact of occupational upgrading on wage growth from column 2, this would leave this magnitude 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 3, 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, -0.036 with standard error 0.048, actually suggests

²²More 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.

²³We 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.

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.

Table 3: CYCLICALITY OF QUALITY MEASURES WITHIN MATCHES

	(1)	(2)	(3)
	$\Delta \ln$ (wage)	Δ (occ index)	$\Delta \ln$ (wk week)
Δ Unrate	-0.002 (0.001)	-0.001 (0.001)	-0.004 (0.001)
Unrate at t_0	0.318 (0.102)	-0.003 (0.056)	-0.036 (0.048)

Notes: Sample reflects 45,269 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.

Robustness to a shorter cutoff for a

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 2 shows that the estimates of the cyclical quality-adjusted new-hire wage are little affected by shortening a to six years. The impact of a one percentage point higher unemployment is now to reduce the new-hire wage by 2.12 percent, with standard error 0.51 percent. Cutting a to four years further reduces the cyclical quality-adjusted new-hire wage, with 1 pp in unemployment predicting a 1.53 percent lower wage, standard error 0.58 percent. This could reflect that selection on wage changes increases our estimated cyclical quality-adjusted new-hire wage. 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)	(2)	(3)
	Cut off after 8 years	Cut off after 6 years	Cut off after 4 years
URate	-2.35	-2.12	-1.53
	(0.67)	(0.51)	(0.58)

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 (15). 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 shorter 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, 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 0.49% 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 cyclicity of the new-hire wage: A one percentage point higher unemployment rate predicts a 2.46% lower wage with a standard error of 0.73%.

We next treat cyclical selection by employing a Heckman correction in wage-growth equation (15). 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 $t + \tau$.²⁴ 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.²⁵ In the second stage, our wage growth-regression controls for the inverse Mills ratio. Its coefficient is positive (0.86%) but not statistically significant (standard error of 1.15%), meaning that the average observed rate of wage growth is slightly higher for those that have a lower probability of selecting into the sample.

The third column of Table 5 reports the resulting cyclicalities of the new-hire wage with predicted match wage growth augmented for the Heckman correction. Estimated cyclicalities are smaller than our benchmark estimate, with a 1 pp higher unemployment rate associated with a decrease in the new-hire wage of 2.17%, 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)	(2)	(3)
	Benchmark	Control for Relative Duration	Heckman Correction
URate	-2.35	-2.46	-2.17
	(0.67)	(0.73)	(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. Because we add new regressors in these two specifications, our first-stage sample size is not the same for all specifications. The baseline has 72,990 wage-growth observations, while the relative duration and the Heckman ones have 72,402 and 72,742, respectively. When estimating our baseline regressions again with the more restrictive samples, we obtain coefficients of -2.45 (0.67) and -2.35 (0.67).

²⁴More 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.

²⁵We 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.

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.²⁶ 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 (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 6 reports estimated cyclicity 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 2.90% lower new-hire wage (standard error 0.70%). When we add the match-quality controls, the new-hire wage is slightly more cyclical, with coefficient -2.88% (standard error 0.66%). Both estimates imply modestly greater cyclicity than our benchmark estimate, -2.35 .²⁷

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 6, following all workers eight years, does not require that restriction. Columns (3) and (4) repeat the estimation for all the matches in our sample, including those that last less than 18 months. Without match-quality

²⁶We construct the sample by associating the worker’s main job eight years later with the match started at t . For example, for a match starting in 1980, we associate it with the respondent’s main job in 1988. If we do not observe the match in its first year, we use its second or third year and associate it with the main job eight years later. The main job is defined as the current or most recent job. If the respondent has multiple jobs, we select the one with higher hours. We then compute the 8-years wage growth. Given that the samples became bi-annual, we also compute a 7-year change for those we cannot compute at 8 years.

²⁷Coefficients in the cumulative wage-growth regression for the average workweeks in the current and 8-year ahead matches are respectively 0.035% (standard error 0.078%) and 0.157% (standard error 0.071%). The dummies for realized match duration (2 to 4, and more than 4 years) have respective coefficients of -3.50% (standard error 1.07%) and -0.58% (standard error 1.20%) for the current match and 3.40% (standard error 1.31%) and 8.91% (standard error 1.05%) for the match 8-years ahead. But differences in these durations across the t and $t + 8$ matches are not predicted by unemployment at t .

controls, column 3, 1 pp. higher unemployment is associated with a 3.17% lower new-hire wage (standard error 0.64%). Adding match-quality controls, column 4, yields nearly the same coefficient: -3.13% (standard error 0.62%). The finding in Table 6 of a *more* cyclical new-hire wage when all matches are included implies that short-duration matches exhibit even more procyclical new-hire wages. That reassures us somewhat that our general finding of a highly cyclical new-hire wage is not driven by excluding matches shorter than 18 months.

Table 6: CUMULATIVE WAGE GROWTH 8 YEARS AHEAD EVEN IF CHANGE JOBS

	(1) ≥ 18 mo. duration		(3) All Matches	
	Quality Controls		Quality Controls	
	No	Yes	No	Yes
URate	-2.90	-2.88	-3.17	-3.13
	(0.70)	(0.66)	(0.64)	(0.62)

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 Cyclicalities of the user cost of labor

We now move to estimates of the cyclicalities 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.

4.3.1 Cyclicalities of the wage component of labor’s user cost

The wage-component of user cost will reflect cyclicalities 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 $\delta = 0.285$. The separation rate of 0.285 is estimated from the first eight years of the matches in our NLSY samples.²⁸ We then

²⁸More exactly, 0.285 is the mean value of the estimated year dummies in the linear probability model for separating described in Appendix B.2. The sample restrictions estimating separation rates mirror those for estimating match wage-growth, except we require that matches last at least 6 months, not 18.

move to our baseline specification that allows for time-varying separation and discount rates. We estimate the separation rate, δ_t , from year dummies in a linear probability model for exiting 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).²⁹

Table 7 reports the cyclical 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 4.81% decline in the wage component of user cost, with a standard error of 1.83%. The high cyclical of user cost 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 4.98% (standard error 1.85%), so slightly more procyclical than under a constant β . Although the effective discount factor, $\beta_t(1 - \delta)$, is highly procyclical, this has little influence on the cyclical of user cost.³⁰ 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 even more cyclical, responding by -5.28% (standard error 2.08%) to 1 pp of unemployment.³¹

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 vary. (See equation (5) 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 separation rates 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 $t + \tau$. Note, this specification allows separation rates to systematically decline with tenure, as seen in Figure 3.

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 -5.32 (standard error 1.87%). This is essentially unchanged from our baseline estimate just describe that assumes $\delta_{t,t+\tau} = \delta_t$.

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. The estimate

²⁹We restrict attention to CRRA preferences with an intertemporal elasticity of substitution equal to 0.5. More detail is provided in Appendix B.3.

³⁰For 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 -0.70 (0.22), -0.73 (0.21), and -0.23 (0.08) for $i = 0, 2, 6$.

³¹Our estimated combined discount factor, $\beta_t(1 - \delta_t)$, is acyclical. 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 (0.68), -1.64 (0.50), and -0.17 (0.18) for $i = 0, 2, 6$.

Table 7: CYCLICALITY OF QUALITY-ADJUSTED NEW-HIRE WAGE AND USER COST

	Unemployment
New-Hire Wage	-2.35 (0.67)
<i>Wage Component of Labor's User Cost</i>	
User Cost w/ constant β , constant δ	-4.81 (1.83)
User Cost w/ time-varying β , constant δ	-4.98 (1.85)
User Cost w/ time-varying β , time-varying δ	-5.28 (2.08)
User Cost w/ time-varying β , time-varying and start-date specific δ	-5.32 (1.87)

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

of -5.32 associates a decline in labor's user cost of 18 percent with such a large recession. That is a substantial decline of the price of labor; it 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 cyclicalities 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 -2.97% , with a standard error of 1.47% .

4.3.2 Adjusting for less-durable matches starting in recessions

If cohorts of new hires that start in recessions display systematically higher separation rates then, as discussed in Section 2.2, starting a position at t rather than at $t + 1$ will affect future hiring costs. Here we explore the potential importance of that channel for cyclicalities of labor's user cost by adjusting for the impact on future hiring costs due to recession-started matches being less durable.

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, from equation (4): $UC_t^\kappa = E_t \sum_{\tau=0}^{\infty} \mathfrak{B}_{t,t+\tau} (\pi_{t,t+\tau} - \pi_{t+1,t+\tau}) \kappa_{t+\tau}$ using our estimates for the

$\delta_{t,t+\tau}$'s and β_t 's discussed just above and using $\kappa_{t+\tau}$ described below. We then recalculate a counterfactual series, \widetilde{UC}_t^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 hypothetical separation rates, $\widetilde{\delta}_{t,t+\tau}$, that remove the estimated impact of the unemployment rate at match start. That adjustment reflects our estimated hazard function from Section 4.2.3, where we found that the separation rate is increased by 2.6% for a 1 pp increase in unemployment at match start. Finally, we estimate cyclicity in $\ln UC_t^\kappa$ both for the actual and counterfactual separation rates. The differential cyclicity of UC_t^κ under the actual versus counterfactual separation rates captures the “quality effect” that starting a match in a recession leads to greater subsequent hiring costs.

For hiring costs we consider two scenarios. We first assume that a hiring cost is only incurred in the starting period. We set that cost, κ , equal to one fourth of the steady state wage ϕ , which we normalize to one. That is, the hiring cost is equivalent to three months 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 headhunters typically charge to fill positions, which are presumably positions that are relatively difficult to fill.³²

Alternatively, we allow both for that up-front hiring cost and persistent training costs that decline over the 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 “hiring costs” to take the more general form $\kappa_\tau = \kappa + \lambda_\tau$. κ , as before, captures the upfront hiring cost. λ_τ reflects the training cost. We specify λ_τ as $(1 + \alpha)^{N-\tau} - 1$ 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 training cost of $\lambda_0 = 0.32$, which added to the hiring cost gives $\kappa_0 = 0.57$. The 3.5% rate for α corresponds to the average rate of wage growth we observe within matches in our sample.³³ The choice of N implies 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 a worker's

³²The *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>).

³³Controlling for a quadratic in age, we estimate an average annual growth rate of 3.09% (standard error 0.09%) for the first eight years of match tenure in our sample, evaluated at the mean sample age of 34.5 years. (The average is 3.51% (0.13%) for the first four years, then 2.43% (0.12%) 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).

general human capital, which will not be mirrored in firm rents. Given our estimates for separation rates and time discounting, the expected discounted value of the flow of κ_τ 's is 0.96; so nearly a full year of steady-state earnings.

Table 8 presents our results for cyclicalities of labor's user cost, augmented to adjust for match quality in terms of both productivity and separation rates. The first two rows repeat the results from Table 7 for cyclicalities in the new-hire wage and the pure wage component of user cost. The higher separation rate for workers hired in recessions implies that the hiring cost component of user cost is highly countercyclical. For $\kappa = 0.25$, 1 pp higher unemployment increases UC_t^κ by 6.21% (standard error of 1.32%). In order to put this impact into terms comparable to the estimates of the wage component of user cost, we weight this 6.21% by the relative importance of UC_t^κ to UC_t^W , which equals 8.57%.³⁴ That is, we multiply 0.0857 times the cyclicalities in UC_t^κ , 6.21%, then add the product to the estimate of user-cost cyclicalities, -5.32% , from Row 2. The result, Row 3 of the table, shows that the response of user cost to a percentage point of unemployment is reduced to -4.79% (standard error of 1.88%).³⁵

The last row of Table 8 shows the impact on user cost of also allowing for training costs that persist into the match. In this case, a 1 pp increase in unemployment increases future hiring costs, UC_t^κ , by 3.72% (standard error 0.78%). That is a smaller percent response than with only an upfront hiring costs. But accounting for match durability is now more important because UC_t^κ is larger with training costs, averaging 30.05% of UC^W .³⁶ In the last row of Table 8, we add 0.3005 times the cyclicalities in UC_t^κ to the estimate of user-cost cyclicalities

³⁴More exactly, let η^W and η^κ be respectively the semi-elasticities of the wage and hiring-cost components of user cost with respect to the unemployment rate. Our adjusted measure of cyclicalities equals: $\tilde{\eta}^W = \eta^W + \frac{UC^\kappa}{UC^W} \eta^\kappa$, where $\frac{UC^\kappa}{UC^W}$ captures the importance of hiring costs, relative to wages, in user cost. Thus the estimate for $\tilde{\eta}^W$ answers the question: How cyclical is the wage component of user cost, if one adjusted wage payments to compensate firms for any cyclicalities in future hiring costs?

The relative importance of UC^κ reflects our estimated $\delta_{t,t+\tau}$'s and $\beta_{t+\tau}$'s. But it is most easily seen for constant rates of separating and discounting. In that case, accelerating hiring by one period incurs a cost of κ at t while saving in expectation $(1 - \delta)\kappa$ at $t + 1$. So the discounted net cost, UC^κ , equals: $\kappa(1 - \beta(1 - \delta))$. For $\kappa = 0.25$ and our mean values for δ and β , this yields a UC^κ of a little over 7%. Our higher number in practice, 8.57%, reflects that our estimated separation rates are higher in the first year. See equation (4). Given the wage, ϕ is normalized to one, the steady-state wage component of user cost, UC^W , is also normalized to one. So 8.57% is the relative importance of UC^κ to UC^W .

³⁵This coefficient adjusts for 1 pp higher unemployment at match start increasing separation hazards by an estimated 2.60% (call that $\hat{\beta}$). But the standard error, 1.88%, does not reflect uncertainty in that estimate $\hat{\beta} = 2.60$. Using Gauss–Hermite quadrature, we estimate the variance of the semi-elasticity of the hiring-cost component of user cost, $\eta^\kappa(\hat{\beta})$, by integrating over the sampling distribution of the estimated coefficient $F(\hat{\beta})$. In particular, we compute $\int_{-\infty}^{+\infty} (\eta^\kappa(\hat{\beta}) - \bar{\eta}^\kappa)^2 dF(\hat{\beta}) \approx \sum_{i=1}^n w_i (\eta^\kappa(\hat{\beta}_i) - \bar{\eta}^\kappa)^2$, where w_i and β_i are the Gauss–Hermite weights and nodes, and $\bar{\eta}^\kappa$ is the semi-elasticity point estimate of -4.79% . The standard error of the estimated semi-elasticity is 0.08%, which only marginally increases the standard error in Row 3.

³⁶The calculation of the relative importance of UC^κ allowing for training costs parallels that discussed in footnote 34 with only hiring costs.

from row 2. The result is that 1 pp higher unemployment reduces user cost by -4.21% (standard error 1.90%). Comparing this estimate, -4.21% , to that ignoring any impact on future hiring and training costs, -5.32% , we see that adjusting for match durability only reduces the cyclicity of user cost by about a fifth, even generously calibrating hiring and training costs. For a very large recession, like the Great Recession, even this lower estimate implies a fall in the price of labor of about 15% .

Table 8: CYCLICALITY OF QUALITY-ADJUSTED NEW-HIRE WAGE AND USER COST

	Unemployment
New-Hire Wage	-2.35 (0.67)
User Cost (Table 7, row 5)	-5.32 (1.87)
<i>Wage Component of Labor's User Cost, adjusted for match durability</i>	
User Cost w/ hiring costs	-4.79 (1.88)
User Cost w/ hiring and persistent training costs	-4.21 (1.90)

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 cyclicity 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) 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 0.79 under the Hamilton filter to 1.51 under our benchmark of a cubic trend.

For user cost, in columns (3) and (4) we first consider the 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 -5.3% for a pp of unemployment, across all the filters except the Hamilton. With the Hamilton filter, it declines by -4.0% (standard error 1.8%), 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.

Lastly, columns (5) and (6) show the estimated cyclical in user cost allowing that hiring in a recession increases both future hiring and training costs, that is, the latter case in Section 4.3.2. Adjusting for future hiring and training costs reduces cyclical of user cost by at most a fourth across the filters, and regardless of measuring the cycle by the unemployment rate or real GDP. The elasticity of the adjusted user cost with respect to real GDP is above 1.5 for all filters and on the order of 2.5 for all but the Hamilton.

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	-2.48 (0.39)	1.40 (0.20)	-5.24 (1.59)	2.68 (0.70)	-4.10 (1.62)	2.25 (0.70)
Cubic	-2.35 (0.67)	1.51 (0.28)	-5.32 (1.87)	2.98 (0.79)	-4.21 (1.90)	2.58 (0.78)
HP filter	-1.59 (0.69)	1.05 (0.36)	-5.33 (2.76)	3.22 (1.39)	-4.08 (2.80)	2.70 (1.38)
One-Sided HP filter	-1.75 (0.43)	1.20 (0.26)	-4.83 (2.57)	2.91 (1.42)	-3.64 (2.34)	2.40 (1.24)
Hamilton Filter	-1.64 (0.48)	0.79 (0.21)	-4.02 (1.76)	1.76 (0.77)	-3.25 (1.91)	1.53 (0.78)

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.

5 Comparison with Prior Treatments of Quality

The literature has mainly used two approaches to control for quality to estimate cyclical-ity of new-hire wages. The first compares the new hires’ wage to the worker’s wage fixed-effect (e.g., Carneiro, Guimaraes, and Portugal, 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, 1985; Gertler, Huckfeldt, and Trigari, 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 cyclical-ity of wages of new hires is estimated from:

$$\ln w_{t,t}^{ij} = \alpha Cycle_t + \ln w_{fe}^i + \epsilon_{t,t}^{ij}. \quad (17)$$

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 for worker i . Thus, the estimated quality-adjusted price of labor is:

$$\ln \widehat{\phi}_{t,t} = \ln \phi_{t,t} + (\ln q_{t,t}^{ij} - \widehat{\ln w_{fe}^i}). \quad (18)$$

This yields a biased estimate of new-hire wage cyclical-ity if

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

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 cyclical-ity. As discussed from the outset, this bias could be procyclical (sully-ing effect of recessions) or countercyclical (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 cyclical-ity.

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. To the extent that $\widehat{\ln w_{fe}^i}$ reflects $\phi_{t,t}$, $\widehat{\phi}_{t,t}$ will understate fluctuations in $\phi_{t,t}$. Therefore, $\text{Cov}(Cycle_t, \ln \widehat{\phi}_{t,t})$ will understate cyclical-ity of new-hire wages. Our approach alleviates that bias by basing the control for match quality on the expected wage eight years ahead, which we assume is little influenced by the cycle at t .

Table 10 gives estimates of wage cyclicality separately for stayers versus new hires controlling for a worker fixed effect on wages.³⁷ We find that wages for stayers are only mildly procyclical, decreasing by -0.64% for each pp increase in the unemployment rate (standard error 0.31%). New-hire wages are considerably more procyclical, decreasing by -1.95% for each pp in unemployment (standard error 0.36).³⁸ 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 \times Urate	-0.64 (0.31)
New Hires \times Urate	-1.95 (0.36)

Notes: The table shows the percent wage response to a 1 pp increase in unemployment. Sample is for 1980 to 2011; it reflects 73,727 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.

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

Gertler, Huckfeldt, and Trigari (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 (2022) finds a comparable pattern based on NLSY79 data. Gertler, et al. 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 (18), they presume that $\text{Cov}\left(\text{Cycle}_t, \ln q_{t,t}^{ij} - \widehat{\ln w_{fe}^i}\right) = 0$ for hires from non-employment while being positive for job-to-job hires. But an alternative interpretation is that $\text{Cov}\left(\text{Cycle}_t, \ln q_{t,t}^{ij} - \widehat{\ln w_{fe}^i}\right) < 0$

³⁷We 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).

³⁸Our fixed-effects estimate of cyclicality for new-hire wages is in line with findings by Figueiredo (2022) for NLSY data and by Gertler, Huckfeldt, and Trigari (2020) for SIPP data.

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. 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 from 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, Huckfeldt, and Trigari (2020) and Figueiredo (2022), with fixed effects as the implicit quality control, the estimates suggest more procyclical wages for job-to-job hires: their coefficient for 1 pp of unemployment is -2.22% (standard error 0.47%) versus -1.30% (standard error 0.34%) for hires from non-employment.

Table 11: FIXED-EFFECTS, SPLITTING NEW HIRES BY WHETHER JOB-TO-JOB

	(1)
	log(wage)
Stayer \times Urate	-0.64 (0.31)
Via Non-Emp \times Urate	-1.30 (0.34)
Job-to-Job \times Urate	-2.22 (0.47)

Notes: The table shows the percent wage response to a 1 pp increase in unemployment. Sample is for 1980 to 2011; it reflects 73,727 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.

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 cyclicity for job-to-job hires, but the difference is not statistically significant. The impact of 1 pp of unemployment is -2.31% for hires from non-employment (standard error 1.01%) compared to -2.89% for those job-to-job (standard error 0.60%). Thus the new-hire wage is highly cyclical for both groups, especially compared to cyclicity in wages for all workers (See Table 1). Our approach yields greater cyclicity than using fixed effects both for hires from non-employment (-2.31 versus -1.31) and job-to-job (-2.89 versus -2.21). 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 the fixed-effects estimate yields less cyclical wages for both types of hires, given that, if wages are smoothed, it is biased toward zero cyclical.

The second and third rows of Table 12 again split new-hires by whether via-non-employment or job-to-job, but now treat selection by: a) employing a Heckman correction, or b) following all workers out eight years, even if they move to a new match. In no case do we see much differential in cyclicalities across the two sets of new hires. With the Heckman correction the results closely parallel our benchmark estimates in Row 1, though wages are a little less procyclical for both sets of new hires. Following all new hires for eight years, wages for new hires from non-employment and job-to-job are comparably cyclical.

Table 12: NEW-HIRE WAGE CYCLICALITY, JOB-TO-JOB VERSUS VIA NON-EMPLOYMENT

	All New Hires	Via Non-emp	Job-to-Job
Benchmark	-2.35 (0.67)	-2.31 (1.01)	-2.89 (0.60)
Heckman Correction	-2.17 (0.65)	-2.08 (0.98)	-2.69 (0.58)
8-years Change w/ Quality Controls	-2.88 (0.66)	-2.84 (0.70)	-2.73 (0.70)

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 a new regressor in the Heckman-correction specification, its first-stage sample size differs from the benchmark specification. Estimating our benchmark regression for the Heckman-correction sample, we obtain coefficients -2.35 (0.67), -2.32 (1.01), -2.89 (0.60) for all hires, hires via non-employment, and hires via job transition, respectively.

5.2 First differences

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

$$\ln w_{t,t}^{ij} - \ln w_{t-1,t-1}^{ij-1} = \alpha \Delta Cycle_t + (\epsilon_{t,t}^{ij} - \epsilon_{t-1,t-1}^{ij-1}).$$

$w_{t-1,t-1}^{ij-1}$ 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:

$$\ln \left(\frac{\widehat{\phi}_{t,t}}{\phi_{t-1,t-1}} \right) = \ln \left(\frac{\phi_{t,t}}{\phi_{t-1,t-1}} \right) + \left(\ln q_{t,t}^{ij} - \ln q_{t-1,t-1}^{ij-1} \right) + \left(\ln \phi_{t-1,t-1} - \ln \phi_{t-1,t-1} \right).$$

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

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

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 $\phi_{.,t-1} = \phi_{t-1,t-1}$. 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 $\phi_{.,t-1} > \phi_{t-1,t-1}$. Therefore, $\text{Cov}(\Delta Cycle_t, \ln \phi_{t-1,t-1} - \ln \phi_{.,t-1}) < 0$, imparting a countercyclical bias to the wage-change estimate.

In the first column of Table 13, we present cyclical 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.³⁹ 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 -0.80% lower wage growth for new hires (standard error 0.43%). 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 (-2.35%) or by fixed effects (-1.95%). But the estimates are not especially comparable as the definition 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 procyclical for job-to-job hires – with 1 pp higher growth in the unemployment rate reducing the rate of wage growth by nearly one percent – and acyclical for hires from non-employment. But the standard errors are sufficiently large that the estimate is not statistically significant for either group if viewed separately.⁴⁰

³⁹As 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.

⁴⁰From SIPP data, Gertler, Huckfeldt, and Trigari (2020) estimate a positive response of wage growth to the change in the unemployment rate that is statistically significant for job-to-job hires but not for those hired after a non-employment spell. 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 biases outlined in this subsection.

Table 13: CYCLICALITY OF WAGES, FIRST-DIFFERENCES APPROACH

	(1)	(2)
	$\Delta \log(\text{wage})$	$\Delta \log(\text{wage})$
Stayer $\times \Delta$ Urate	-0.23 (0.29)	-0.24 (0.29)
New Hires $\times \Delta$ Urate	-0.80 (0.43)	
Via Non-Emp $\times \Delta$ Urate		0.01 (0.80)
Job-to-Job $\times \Delta$ Urate		-0.90 (0.48)

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 42,293 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 cyclicity of the price of labor taking into account wage smoothing within matches and cyclical variation in match quality.

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 considerably more procyclical, decreasing by 4.2% for a 1 pp increase in unemployment and increasing with an elasticity of about 2.5 with respect to real GDP. Relative to that in the new-hire wage, cyclicity in user cost reflects two additional effects. Hiring during a recession, versus waiting, predicts a lower future path for wages. That impact on future match wages contributes a drop in user cost of about 3% for a pp increase in unemployment. Finally, hiring in a recession also predicts higher match separation rates. But even generously calibrating hiring costs and growth in employer surplus

during matches, this impact on separation rates only offsets about one-fifth the cyclicity in user cost from wages.

Our results for labor's user cost require some force, or forces, for 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). Another force suggested in the literature acts via uncertainty. Arellano, Bai, and Kehoe (2019), Jo and Lee (2022), and Wang (2022) each model uncertainty as reducing labor demand, while providing evidence that uncertainty is heightened during recessions.

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A User Cost: Computation

In this appendix, we describe the algorithm used to compute labor’s user cost. We use matrix algebra to simplify notation. We index rows and columns using t (start date) and $t + \tau$ (current date).

As a first step, we organize all our estimated time series into several matrices, B , D , and ϕ . Matrix B stacks the series of cumulative time-discount factors, $\mathfrak{B}_{t,t+\tau}$, that discount the value of creating new matches in the future, as defined in equation 3. The first row has the time-discount used to discount a match started in 1980, the second has the one used to discount a match started in 1981, and so forth. Since we define $\mathfrak{B}_{t,t} = 1$, all elements on the diagonal are 1’s.

$$B_{t,t+\tau} = \mathfrak{B}_{t,t+\tau} = \prod_{k=0}^{\tau-1} \beta_{t+k} \implies B = \begin{bmatrix} 1 & \beta_{1980} & \beta_{1981}\beta_{1982} & \dots \\ 0 & 1 & \beta_{1981} & \dots \\ \vdots & \vdots & \vdots & \ddots \end{bmatrix}$$

The matrix D stacks the cumulative survival probabilities of a match created at time t surviving until $t + \tau$. For example, the first row of the matrix contains the survival probability of a match created at time 1980 surviving until 1981 in the second column, its survival probability until 1982 in the third column, and so forth. As in the previous matrix, all elements on the diagonal are 1’s.

$$D_{t,t+\tau} = \prod_{k=0}^{\tau-1} (1 - \delta_{t,t+k}) \implies D = \begin{bmatrix} 1 & (1 - \delta_{1980,1980}) & (1 - \delta_{1980,1980})(1 - \delta_{1980,1981}) & \dots \\ 0 & 1 & (1 - \delta_{1981,1981}) & \dots \\ \vdots & \vdots & \vdots & \ddots \end{bmatrix}$$

Lastly, we construct the quality-adjusted wage matrix ϕ . For example, the first row of the matrix contains the new-hire wage for a match created in 1980 in the first column, its quality-adjusted wage $\phi_{1980,1981}$ in the second column, and so forth.

$$\phi_{t,t+\tau} = - \sum_{k=\tau}^7 \ln \left(\frac{w_{t,t+k+1}}{w_{t,t+k}} \right) \implies \phi = - \begin{bmatrix} \phi_{1980,1980} & \phi_{1980,1981} & \phi_{1980,1982} & \dots \\ 0 & \phi_{1981,1981} & \phi_{1981,1982} & \dots \\ \vdots & \vdots & \vdots & \ddots \end{bmatrix}$$

Sections B.2 and B.3 describe how we estimate the separation rates and the discount factor. In Section 2.3, we explain how we identify quality, and in Section 3.2, we explain our estimation approach.

With these matrices, we can compute the other variables we define in the text. First, we compute the matrix of discounting factors, $\Lambda_{t,t+\tau}$ (e.g., equation (2)), which reflects the time-discount factors and the match survival probabilities used to discount the match’s future stream of wages. The matrix is just the element-wise product of the previously constructed

matrices B and D :

$$\Lambda_{t,t+\tau} = \mathfrak{B}_{t,t+\tau} \times D_{t,t+\tau} \implies \Lambda = B \circ D,$$

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

Next, we compute a vector for the stream of quality-adjusted discounted wage rates, $\vec{\Phi}$. Each vector element presents the stream of wage rates starting from a match at $t + \tau$, discounted to the start of that match. That is, we do an element-wise product between matrices Λ and ϕ and sum over rows.

$$\vec{\Phi}_t = \sum_{\tau} (\Lambda \circ \phi)_{t,t+\tau} \implies \vec{\Phi} = \begin{bmatrix} \sum_{\tau} (\Lambda \circ \phi)_{1980,1980+\tau} \\ \sum_{\tau} (\Lambda \circ \phi)_{1981,1981+\tau} \\ \vdots \end{bmatrix}$$

Lastly, we define the matrices Ψ and Π following the formulas in Section 2.2. The first matrix, Ψ , collects the probability that a match starts in t and survives to $t + \tau - 1$. The second matrix, Π , collects the probability differentials of later starting a match at $t + \tau$ when beginning a position in t , rather than $t + 1$.

$$\Psi_{t,t+\tau-1} = \prod_{k=0}^{\tau-2} (1 - \delta_{t,t+k}) \implies \Psi = \begin{bmatrix} 1 & 1 & (1 - \delta_{1980,1980}) & (1 - \delta_{1980,1980})(1 - \delta_{1980,1981}) & \dots \\ 0 & 1 & 1 & (1 - \delta_{1981,1981}) & \dots \\ \vdots & \vdots & \vdots & \vdots & \ddots \end{bmatrix}$$

$$\Pi_{t,t+\tau} = \pi_{t,t+\tau} - \pi_{t+1,t+\tau} \implies \Pi = \begin{bmatrix} 1 & -(1 - \delta_{1980,1980}) & \pi_{1980,1983} - \pi_{1981,1983} & \dots \\ 0 & 1 & -(1 - \delta_{1981,1981}) & \dots \\ \vdots & \vdots & \vdots & \ddots \end{bmatrix}$$

The user cost is then computed as $\vec{UC} = (B \circ \Pi) \times \vec{\Phi}$. That is, we first compute the element-wise product of the cumulative time-discount rate and the probability differentials. Second, we multiply the resultant matrix by the stream of wage rates vector.

In the case of time-varying but not start-date specific separation rates, $\delta_{t,t+\tau} = \delta_{t+\tau}$, only the first two elements of the sequence $\{\pi_{t,t+\tau} - \pi_{t+1,t+\tau}\}$ are non-zero. Therefore, to compute a time series of labor's user cost until 2011, we need the stream of wage rates for matches starting through 2012. However, when the separation rates also vary by start year, no elements of $\{\pi_{t,t+\tau} - \pi_{t+1,t+\tau}\}$ are zero. For this case, we set $\pi_{t,t+\tau} - \pi_{t+1,t+\tau} = 0$ for $\tau > 8$. By doing so, we only require the stream of quality-adjusted wages for matches starting through 2019. But this still requires wage changes beyond 2019. Given lack of those estimates, we set these values to $\phi_{t,t+\tau} = \bar{\phi}_{\tau}$, where $\bar{\phi}_{\tau}$ is the average estimated in the previous years. We note, however, that the relevant weights on these wage changes, reflecting $\pi_{t,t+\tau} - \pi_{t+1,t+\tau}$, are predicted to be quite small given estimated behavior of separation rates.

B Data Appendix

B.1 Data sample moments

Table B1: SAMPLE VARIABLE MEANS

	Full Sample	NLSY 79	NLSY 97
	Mean	Mean	Mean
Avg. Age	34.0	36.2	27.9
Fraction of Male	0.57	0.57	0.57
Avg. Years of Schooling	13.3	13.3	13.5
Avg. Real Hourly Wage	21.0	21.5	19.8
Avg. Weekly Hours	42.9	43.5	41.3
Avg. Tenure	4.2	4.8	2.7
Fraction new hires	0.27	0.27	0.30
Avg. match length	6.6	7.7	3.8
Avg. individual panel duration	23.5	27.5	12.6
Observations	135782	83151	52631

Notes: Means for all duration variables reported in years.

B.2 Separation rate series

We employ a similar approach to estimate separation rates as the one used to estimate match wage growth rates. Specifically, we estimate linear probability models where we allow the separation rate to potentially depend on a full set of interactions between the calendar year a match began and all subsequent years observed in the data. In the special case of $\delta_{t,t+\tau} = \delta_{t+\tau}$ that interaction reduces to just the current calendar year. In contrast to the wage regression, we include all matches in the estimation, except those that lasted less than half a year.

For illustrative purposes, we describe how we estimate separation rates that depend only on the current year. The case with two subscripts is a direct extension that only increases the number of estimated time dummies. We estimate δ_d 's from the following regression:

$$\mathbb{1}_{\delta,t}^{ij} = \Psi x_t^{ij} + \sum_{d=1980}^{2019} \delta_d^{79} D_d^{79,ij} + \sum_{d=1997}^{2019} \delta_d^{97} D_d^{97,ij} + \epsilon_{t+\tau}^{ij}.$$

$\mathbb{1}_{\delta,t}$ is an indicator variable that equals one if a match separation occurred between t and $t+1$. Dummy variables D_d^s equal 1 if $d = t$ and 0 otherwise, with the subscripts capturing the survey instrument (NLSY79 or NLSY97); and x_t reflects additional controls for individual characteristics that could affect the match separation. These are dummies capturing the respondent's sex, race, age, and educational attainment. We allow for a flexible function for age (multiple dummy variables) because a predicted separation rates are sometimes outside of 0 and 1 estimating with only a cubic in age.

We define a match as separating between years t and $t+1$ if the worker is employed at that match at year t , but employed in a new match, or not employed, at the $t+1$ survey. Given we take advantage of retrospective questions in the NLSY, at the $t+1$ survey respondents may also report matches that started after the t survey date, but ended prior to the $t+1$ survey. If these matches last at least six months, for estimating separation rates we treat them as a match that exhibited a separation from t to $t+1$.

When an NLSY survey is conducted annually, each estimated coefficient $\widehat{\delta}_t^s$ represents the separation probability between period t and $t+1$. When the surveys are biannual, the dependent variable captures whether any separation occurred between t and $t+2$. For this reason, we take the estimated 2-year separation coefficient, $\widehat{\delta}_t^s$ and create annualized rates for t and $t+1$ according to:

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

e.g., $\tilde{\delta}_t^s$ represents the annualized separation rate for t to $t+1$. This adjusts for the possibility of consecutive separations, imposing that the probability of a separating in $t+2$ is independent of separating in $t+1$.

We estimate the dummies separately by survey instrument (NLSY79 or NLSY97). When only one NLSY survey is being conducted, the separation rate is the estimated coefficient for that survey year. When the two NLSY surveys overlap, we aggregate their estimated coefficients, giving each equal weight.

B.3 Discount rate series

We estimate a time-varying discount factor, β_t , based on movements in real consumption of nondurables and services. We use the stochastic discount factor that emerges from the Euler equation of a representative agent as our time-varying discount factor.

The Euler equation of a representative agent with CRRA preferences pricing a risk-free bond can be approximated by:

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

where θ is the risk aversion parameter, β is the constant intertemporal discount factor, and $\log(C_{t+1}/C_t)$ is the one-period consumption growth rate. We construct the right-hand side of this equation using observable consumption growth rates and chosen values for θ and β . The exponential of the left-hand side is what we use as the time-varying discount factor.

We set $\beta = 0.989$, which is the inverse of the average real one-month T-bill rate in our sample, which is the nominal rate deflated by the personal consumption expenditures (PCE) price index. We set $\theta = 2$, a common value in the literature. Second, we construct our measure of consumption, real spending on non-durables and services, from NIPA series of its components. Finally, we adjust our time-varying discount factor series (β_t) to have the mean equal to β , which is our measure of a constant time-discount rate.

The user-cost of labor reflects agents' expectations. We construct the user cost series using realized (or ex-post) wage growth. Similarly, when we allow for time-varying discount or separation rates, we also employ the realized rates. We implicitly construct expectations by projecting on a measure of the cycle, e.g. the unemployment rate at t , that we presume is in agents' time- t information set.