The Quality-Adjusted Cyclical Price of Labor

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We estimate cyclicality in labor's user cost allowing for cyclical fluctuations in the quality of worker-firm matches and wages that are smoothed within employment matches. To do so, we exploit a match's long-run wage to control for its quality. Using 1980–2019 National Longitudinal Survey of Youth data, we identify three channels by which recessions affect user cost: they lower the new-hire wage and wages going forward in the match, but they also result in higher subsequent separations. We find that labor's user cost is highly procyclical, increasing by more than 4% for a 1 percentage point decline in unemployment.

I. Introduction

Going back to Pigou's *Industrial Fluctuations* (1927), economists have examined the cyclicality of real wages to disentangle the sources of employment

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fluctuations. Business cycle models can be stratified between those that generate fluctuations along a stable labor demand schedule, with a countercyclical real wage, and those that assign a primary role to procyclical shifts in the schedule and a procyclical real wage. The former include Keynes (1936) and many after who postulate sticky nominal wages, with nominal shocks driving employment. The latter include models with productivity shocks, financial shocks affecting factor demands (Arellano, Bai, and Kehoe 2019), or countercyclical markups (Rotemberg and Woodford 1999), possibly reflecting pricing frictions.

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

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

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

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[1] Pigou (1927, 217) charted real wages for Britain from 1850 to 1910 and found that "the upper halves of trade cycles have, on the whole, been associated with higher real wages than the lower halves."

[2] Hall (1980, 92) states this as follows: "Wages are insensitive to current economic conditions because they are effectively installment payments on the employer's obligation." Consider an analogy to mortgage rates. Basing the cyclicality of real wages on all matches parallels measuring mortgage rates based on the average across all existing mortgages, including those initiated 5 or even 25 years earlier. Such a series (see Berger et al. 2021, fig. 9) is extremely smooth relative to a series reflecting mortgage rates on newly initiated loans.

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

wage smoothing. First, the wage of new 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. Second, with wage smoothing, the new-hire wage can still be a poor proxy for measuring cyclicality in the price of labor. Intuitively, if hiring in the depth of a recession locks in, to some extent, a lower wage going forward in the match, then the effective price of labor, which reflects expected future wages, will be more cyclical than the new-hire wage (Kudlyak 2014).

We estimate the cyclicality of the price of labor addressing (1) cyclicality in firm-worker match quality and (2) wage smoothing within matches. By the cyclical price of labor, we mean the cyclicality of its user cost, where, as in Kudlyak (2014), user cost is defined as the impact on a firm's present discounted costs of adding a worker today while adjusting future hiring to hold constant future employments. Cyclicality of that user cost reflects not only the cycle's impact on the new-hire wage but also any impact on future match rents to the employer, in particular via an impact on the future wage path for a match that starts now versus later. Given these distinct components, we first estimate cyclicality of the quality-adjusted new-hire wage and then proceed to estimate the cyclicality 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 during 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 during 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 during 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 8 years. If this assumption is violated, our results give a conservative estimate for cyclicality of quality-adjusted wages because wage effects that persist will be treated as quality, reducing the cyclicality of quality-adjusted wages.

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

Our estimates are based on two long worker panels from the National Longitudinal Survey of Youth, the NLSY1979 and NLSY1997, spanning from 1980 to 2019. From these, we can estimate the cyclicality of the new-hire wage and user cost for 1980–2011. The quality-adjusted new-hire wage is highly procyclical, decreasing by 2.3% for a 1 percentage point increase in the unemployment rate. It is nearly as cyclical for hires from nonemployment as for those moving from 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 during a recession. Combining the cycle's impacts on the new-hire wage and future wages, 1 percentage point higher unemployment reduces the wage component of user cost by 5.3%. At the same time, the lower wages from hiring during 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 offsets only 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 percentage point increase in unemployment. This represents an elasticity with respect to real gross domestic product (GDP) of about 2.5.

In terms of the literature, our approach to match quality is most closely related to that of 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 the cyclicality of match quality include Devereux (2004) and Figueiredo (2022).

Our focus on labor's user cost follows Kudlyak (2014). The strong cyclicality 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), as well as 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

[4] Kudlyak, 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.

(Beaudry and DiNardo 1991; Baker, Gibbs, and Holmstrom 1994; Bellou and Kaymak 2021). It relates closely to studies that examine the cyclicality of wages for new hires versus incumbent workers (Bils 1985; Carneiro, Guimaraes, and Portugal 2012; Martins, Solon, and Thomas 2012; Haefke, Sonntag, and van Rens 2013; Gertler, Huckfeldt, and Trigari 2020; Grigsby, Hurst, and Yildirmaz 2021).[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, the implied measures for the cyclicality of wages for new hires, and the cyclicality of labor's user cost. We describe our data and empirical implementation in section III. Results are presented in section IV, including a number of robustness exercises with respect to our key assumptions. Section V compares our estimates for cyclicality 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.

II. Estimating Labor's User Cost

A. 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

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

where $q^{ij}_{t,t+\tau}$ is the idiosyncratic component of productivity (i.e., match quality). It reflects worker i, firm j, and worker-firm ij match effects. The 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, $q^{ij}_{t,t+\tau}$, 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. The term $\phi_{t,t+\tau}$ is an aggregate wage in that it does not reflect the characteristics of the particular match other than its start date. To explain the implications of wage smoothing for measures of cyclicality in the price of labor, consider, through the next subsection, that one can measure or control for $q^{ij}_{t,t+\tau}$.

[5] Carneiro, Guimaraes, and Portugal (2012) and Martins, Solon, and Thomas (2012) each find greater cyclicality 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.

If the labor market functioned like a spot market, with no history dependence in wages, then we could drop the subscript reflecting the starting date: $\phi_{t,t+\tau} = \phi_{t+\tau}$. One could then consistently estimate cyclicality in $\phi_{t+\tau}$ on the basis of the 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

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

where the ω_k's are employment shares by duration of tenure, k. So the common finding of greater wage cyclicality 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 cyclicality in the user cost of labor as labor's cyclical price.

B. User Cost of Labor

1. Valuation of a Match

Consider the firm's expected present discounted value of creating a match in t of quality q_t:

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

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

Firm and worker subscripts on q_t, $y_{t,t+\tau}$, and $w_{t,t+\tau}$ are kept implicit. For convenience, we assume here that match quality, q_t, is fixed during the match, although below we allow for match surplus to increase with match tenure. The term $\kappa_t \cdot q_t$ is the cost of finding and training a worker, which scales by the match quality. The term $y_{t,t+\tau}$ denotes the marginal revenue product of the match in $t + \tau$. Note that $y_{t,t+\tau} = z_{t+\tau} q_t$; 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 1. The term 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 match start date.

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 1 while $z_{t+\tau}$ fluctuates around path z, then $z > 1$ allows firms to recoup up-front costs κ_t, consistent with free entry.

2. *The Cost and Benefit of Starting a Position in t versus t + 1*

Consider the valuation of starting a continuing position. Each time the position is interrupted by a separation, this requires spending sufficient resources to create a new match to maintain the position.[6] The expected discounted value, per unit of quality, of starting a position in t is

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

The term $\pi_{t,t+\tau}$ is the probability that a new match is required in $t + \tau$, given that the position starts 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.

Consider the perturbation of starting a position in t versus one at $t + 1$. That leaves the expected labor input unaffected in $t + 1$ and beyond. The expected value of starting a continuing position in t rather than in $t + 1$ is

$$\begin{aligned} &E_t(\mathcal{P}_t - \beta_t \mathcal{P}_{t+1}) = \\ &E_t\left[V_t - \beta_t(1 - \delta_{t,t})V_{t+1} + \sum_{\tau=2}^{\infty} \mathfrak{B}_{t,t+\tau}(\pi_{t,t+\tau} - \pi_{t+1,t+\tau})V_{t+\tau}\right]. \end{aligned} \tag{4}$$

Potential future matches starting in $t + \tau$ get weighted in equation (4) by $\pi_{t,t+\tau} - \pi_{t+1,t+\tau}$, the differential probability of actually starting those matches due to beginning the position in t rather than $t + 1$.[7]

[6] Given the normalization of the quality of t-started matches to 1, we can treat any future matches as being of the normalized unit of quality.

[7] Those differences can be expressed in recursive form for $\tau > 1$:

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

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

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

Equation (4) captures the two key trade-offs that enter the firm's decision of opening and maintaining a position starting in t versus in $t+1$. The first trade-off involves the value of creating a match in t versus creating $(1-\delta_{t,t})$ fewer matches in $t+1$. The second trade-off stems from maintaining a filled position from period $t+2$ onward if the position starts in t versus in $t+1$. Maintaining either position after $t+2$ requires creating a new match if the existing match separates. The value of the new match does not depend on when the position started, but the probability of separation, and therefore of creating a new match, does. Clearly, if the separation rate does not depend on when the match starts (e.g., $\delta_{t,t+\tau}=\delta_{t+\tau}$), then the second trade-off disappears and the last term in equation (4) is zero.

Substituting the path of productivity, wages, and hiring costs from equations (2)–(4) yields

$$
\begin{aligned}
E_t(\mathcal{P}_t-\beta_t\mathcal{P}_{t+1}) = {} & \underbrace{z_t}_{\text{payoff}} \\
& \underbrace{-E_t\left[\Phi_t-\beta_t(1-\delta_{t,t})\Phi_{t+1}+\sum_{\tau=2}^{\infty}\mathfrak{B}_{t,t+\tau}(\pi_{t,t+\tau}-\pi_{t+1,t+\tau})\Phi_{t+\tau}\right]}_{\text{wage component of the user cost of labor, UC}^{W}} \\
& \underbrace{-E_t\left[\kappa_t-\beta_t(1-\delta_{t,t})\kappa_{t+1}+\sum_{\tau=2}^{\infty}\mathfrak{B}_{t,t+\tau}(\pi_{t,t+\tau}-\pi_{t+1,t+\tau})\kappa_{t+\tau}\right]}_{\text{hiring cost component of the user cost of labor, UC}^{\kappa}} \qquad (5) \\
= {} & z_t-\mathrm{UC}_t^W-\mathrm{UC}_t^{\kappa}, \\
\text{where } \Phi_{t+\tau} = {} & \sum_{k=0}^{\infty}\Lambda_{t+\tau,t+\tau+k}\phi_{t+\tau,t+\tau+k}.
\end{aligned}
$$

Equation (5) shows that the benefit of starting a position in t versus $t+1$ equals its output in t net of the user cost of labor. The terms $\kappa_{t+\tau}$ and $\phi_{t+\tau}$ reflect, respectively, the hiring costs and stream of wage rates from starting a match in $t+\tau$, discounted to the start of that match in $t+\tau$. Discounting reflects the time-discount factor and the match's survival probability. The costs $\kappa_{t+\tau}$ and $\Phi_{t+\tau}$ get reflected in $E_t(\mathcal{P}_t-\beta_t\mathcal{P}_{t+1})$ only to the extent that beginning the position in t, rather than $t+1$, affects the probability of later starting a match in $t+\tau$.

3. *The Wage Component of the User Cost of Labor*

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

$$\mathrm{UC}_t^W = E_t\left[\Phi_t-\beta_t(1-\delta_{t,t})\Phi_{t+1}+\sum_{\tau=2}^{\infty}\mathfrak{B}_{t,t+\tau}(\pi_{t,t+\tau}-\pi_{t+1,t+\tau})\Phi_{t+\tau}\right].$$

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. It is instructive to consider a simple case with a constant separation rate, δ, and a constant discount factor, β. Using equations (4) and (5), the net gain from the perturbation is then

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

The wage component of labor cost with constant separation and discount rates is

$$\mathrm{UC}_t^W = \phi_{t,t} + E_t \sum_{\tau=1}^{\infty} \beta^{\tau}(1-\delta)^{\tau}(\phi_{t,t+\tau} - \phi_{t+1,t+\tau})$$
$$\text{for } \delta_{t,t+\tau} = \delta, \beta_{t+\tau} = \beta.$$

The first component is the new-hire wage, $\phi_{t,t}$, while the latter reflects the impact of hiring in 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 the 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 discounting is not too extreme and the lock-in effect on wages is not too transitory, then the wage component of labor's user cost can be much more cyclical than the new-hire wage.

Most business cycle models abstract from history dependence in wages, $\phi_{t,t+\tau} = \phi_{t+\tau}$, and in separation rates. In that simplified setting, the perturbation of starting one more match in t while starting $(1-\delta)$ fewer in $t+1$ yields the expected net gain of

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

The perturbation's net gain is independent of future productivities and wages because it does not affect future labor input. Assuming an interior solution with nonzero match creation in $t+1$, this perturbation should yield zero expected gain. In turn, that implies the 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 simplified setting context, ϕ_t is the wage component of that user cost. Given no history dependence in wages, it is simply the (quality-adjusted) wage in t, common across match durations.

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 that of Thomas and Worrall (1988). But the relevance of user cost as a measure of wage cyclicality does not hinge on the source of history dependence. In particular, models of sticky wages (e.g., Calvo 1983) imply that hiring in 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 cyclicality by labor's user cost nests the case of no history dependence. Thus, it provides a robust measure of wage cyclicality 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.

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—equations (4) and (5) reduce to

$$E_t(\mathcal{P}_t - \beta_t\mathcal{P}_{t+1}) = z_t - (\kappa_t - E_t\beta_t(1-\delta_t)\kappa_{t+1}) - \mathrm{UC}_t^W \quad \text{for } \delta_{t,t+\tau} = \delta_{t+\tau},$$

where the wage component of the user cost of labor is

$$\mathrm{UC}_t^W = \phi_{t,t} + E_t\sum_{\tau=1}^{\infty}\Lambda_{t,t+\tau}(\phi_{t,t+\tau} - \phi_{t+1,t+\tau}) \quad \text{for } \delta_{t,t+\tau} = \delta_{t+\tau}. \tag{6}$$

We treat equation (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 in t versus $t+1$ on future wage paths, discounted to reflect time and probability of separating.

For the empirics, we will consider the natural logarithm 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]

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

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

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

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

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

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 in t versus $t + 1$ on future wage paths. However, starting the position in 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 $(\pi_{t,t+\tau} - \pi_{t+1,t+\tau})\kappa_{t+\tau}$ in $t + \tau$. Suppose that matches that start during 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 starting during 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 more procyclical.

In section IV, we augment our estimates of cyclicality in the wage component of user cost by estimating the impact of such cohort effects on retention. To do so, we combine estimates of separation rates specific to each match-year cohort with calibrated costs of hiring. That is, we estimate the cyclicality of the wage component of the user cost of labor compensating for the hiring cost component of the user cost of labor in equation (5), using plausible quantification of the hiring costs. 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]

C. 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 in t versus $t + 1$. Accordingly, our empirical work begins by estimating cyclicality in the new-hire wage while controlling for match quality and then proceeds to examine cyclicality in labor's user cost. But first we lay out our approach to control for match quality based on a match's expected long-run wage.

As discussed above, we can write the (natural logarithm 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 cyclicality is

$$\begin{aligned} \mathrm{Cov}(Cycle_t, \ln \phi_{t,t}) &= \mathrm{Cov}(Cycle_t, \ln w_{t,t}^{ij}) - \mathrm{Cov}(Cycle_t, \ln q_{t,t}^{ij}) \\ &= \mathrm{Cov}(Cycle_t, \ln w_{t,t}) - \mathrm{Cov}(Cycle_t, \ln q_{t,t}), \end{aligned} \tag{8}$$

[9] We 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. The terms $\ln w_{t,t}$ and $\ln q_{t,t}$, without ij superscripts, denote the population means of $\ln w^{ij}_{t,t}$ and $\ln q^{ij}_{t,t}$ for jobs starting in t. For example, $\ln w_{t,t} = \int_{ij} \ln w^{ij}_{t,t}$. The transition to the second line of equation (8) reflects that the variable $Cycle_t$, being purely time varying, cannot covary with deviations of $\ln w^{ij}_{t,t}$ and $\ln q^{ij}_{t,t}$ from their means for t. We see immediately from equation (8) that the cyclicality of the new-hire wage provides a biased estimate of the cyclicality of the quality-adjusted new-hire wage unless $\mathrm{Cov}(Cycle_t, \ln q_{t,t}) = 0$.

The quality of new-hire matches will be cyclical if there is cyclical selection into new jobs in terms of worker quality, firm quality, or match-specific quality. The direction of overall bias is hard to sign a priori. In terms of worker quality, Mueller (2017) finds, on the basis of the 1962–2012 Current Population Survey (CPS), that the average predisplacement 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 sullying effect of recessions, with "good jobs" not hiring (Barlevy 2002; Carneiro, Guimaraes, and Portugal 2012; Haltiwanger et al. 2021). This implies procyclical firm quality, which will lead to a procyclical bias. Finally, the theories of a cleansing effect of recessions (Caballero and Hammour 1994; Mortensen and Pissarides 1994) imply that matches created in recessions are of a higher quality (higher threshold for q^{ij}_t). 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:

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

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

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

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

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 $(\ln w^{ij}_{t,t} - \ln w^{ij}_{t,t+\tau})$ and $(\ln q^{ij}_{t,t+\tau} - \ln q^{ij}_{t,t})$ for jobs starting in t. For example, $\ln w_{t,t} - \ln w_{t,t+a} = \int_{ij}(\ln w^{ij}_{t,t} - \ln w^{ij}_{t,t+a})$. For exposition, we set aside here the important question of whether matches survive from t to $t + \tau$, taking all matches started in 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 III.B.

We now state two assumptions sufficient for the covariances in the second and third rows to be zero.

Assumption 1.

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

Assumption 1 states that the mean change in quality for matches started in t is orthogonal to the cycle in t. We provide empirical support for this assumption in section IV.B.3. For instance, we show there that occupational upgrading within matches, measured as reporting a new occupational code associated with higher averages wages, is not stronger within matches starting during recessions than during booms.[10]

Assumption 2.

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

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

The first condition imposes that the current stage of the business cycle does not predict the new-hire wage a periods ahead. We see this as a natural assumption if a is chosen large enough so that the current cyclical state does not predict $Cycle_{t+a}$, that is, the stage of cycle a periods ahead. We test this assumption in the data for the a we choose in practice, $a = 8$ years, given measures of the cycle in 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

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

is violated in practice, it will cause us to understate the 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 during 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 cyclicality of the quality-adjusted new-hire wage is

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

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

We illustrate implication 1 in figure 1*A* for a match that starts in a recession. Match quality is captured by the expected wage in $t + a$. (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 during a recession, this implies that $\phi_{t,t}$ is depressed. Our estimate $\hat{\phi}_{t,t}$ reflects that predictable cumulative wage growth from t to $t + 8$. Figure 1*A* is drawn such that assumption 2 is not completely satisfied as of $t + 8$, as $w_{t,t+8}$ still remains below q_t, equal to the expected wage in $t + a$. This

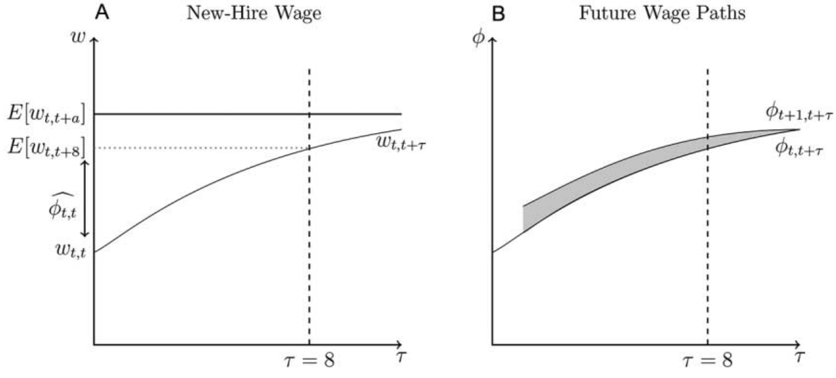

FIG. 1.—Illustration of our approach. A color version of this figure is available online.

illustrates the conservative nature of assumption 2—to the extent that $w_{t,t+8}$ remains below the expected wage in $t + a$, we underestimate how much the recession in t depresses $\phi_{t,t}$.

Next consider the quality-adjusted wage component of user cost. For exposition, we focus on a specification that assumes that the separation rate varies only with time, $\delta_{t,t+\tau} = \delta_{t+\tau}$. From equation (7), its cyclicality reflects not only the new-hire wage but also any impact on future wages by hiring in t versus $t + 1$. For $t + \tau$, as an example, that means any impact on $\ln \phi_{t,t+\tau} - \ln \phi_{t+1,t+\tau}$. But similarly to implication 1, assumptions 1 and 2 imply that cyclicality of the quality-adjusted wage τ periods into the match, $\mathrm{Cov}(Cycle_t, \ln \phi_{t,t+\tau})$, is given by $\mathrm{Cov}(Cycle_t, \ln w_{t,t+\tau} - \ln w_{t,t+a})$. Substituting in equation (7), we obtain the following.

Implication 2. Given assumptions 1 and 2, for $a \gg 1$

$$\begin{aligned}\mathrm{Cov}(Cycle_t, \ln \mathrm{UC}_t^W) = \mathrm{Cov}(&Cycle_t, \ln w_{t,t} - \ln w_{t,t+a}\\ &+ \sum_{\tau=1}^{a} \Lambda_{t,t+\tau}[(\ln w_{t,t+\tau} - \ln w_{t,t+a}) - (\ln w_{t+1,t+\tau} - \ln w_{t+1,t+a+1})]),\end{aligned} \tag{13}$$

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 equation (13). First, for a match started in t, the higher cumulative wage growth to $t + a$ is, the lower the new-hire wage in t is, and so the lower the user cost is. Intuitively, predictably rapid wage growth indicates that the wage is below match quality (again, see fig. 1*A*). Second, the higher wage growth from $t + 1$ forward for matches started in t is, compared with those started in $t + 1$, the lower the user cost in t is. The impact of future wage paths on user cost for a match starting during a recession in t is illustrated in figure 1*B*. The shaded area reflects the differentials in future wages hiring during a recession in t rather than delaying one period. Faster cumulative wage growth from $t + 1$ to $t + a + 1$ for a match starting in t versus $t + 1$ indicates that the t-start match continues to exhibit a lower wage relative to its quality than if started in $t + 1$.

[11] Comparing with eq. (7), note that the summation in eq. (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 $\tau \geq a$. Second, while user cost reflects the expectations of the future wage paths not realized, we drop the expectations operator in eq. (13). This assumes that the realized deviations from expectations at t are orthogonal to the cyclical stage at t.

III. Empirical Implementation

A. Data, Sample Selection, and Variable Definitions

We combine data from the two National Longitudinal Survey of Youth panels: 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 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 the oldest in our NLSY97 sample are 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 his or her 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 1 year. We distinguish new hires that occur via nonemployment versus job to job. We classify a

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

[13] If 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% of jobs have starts prior to age 21, when we begin measuring their wages, and only 0.8% 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.

transition as via nonemployment if the worker was nonemployed during the month before the start of the new job.

Our wage measure is the hourly wage constructed by the Bureau of Labor Statistics (BLS). It is the reported hourly wage for those paid hourly; for others, it is computed on the basis of 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 consumer price index 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.

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 0.5–1.5 years across annual surveys and 1.5–2.5 years across biannual surveys. Given that 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

[14] For the NLSY79, our wage measure is the hourly wage variable HRP#, where # references each job for which wage information was collected. For NLSY97, the compatible measure is the variable HRLY_COMPENSATION#, since it includes overtime, tips, and bonuses. NLS User Services responded to a request for clarification by stating that for the NLSY79, (i) pay rate questions do ask respondents to include tips, overtime, and bonuses and (ii) there is no way to create a pay rate that does not include this information.

[15] The BLS does not construct an hourly wage for these starting date wages, as it does 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.

individuals and 52,631 NLSY97 observations from 5,978 individuals.[16] Table B1 (available online) provides 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 detrending methods for defining the cycle. Unemployment rate and real GDP data are from the BLS and Bureau of Economic Analysis, respectively.

B. Estimation Approach

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

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

where $Outcome_t$ 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 (HP) 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 cyclicality of quality-adjusted new-hire wages and user cost.

First, consider the choice of a, which is the horizon for assumption 2 to apply. That is, it is the duration for a match such that the match wage, conditional on quality, no longer reflects labor market conditions at its start. Guided by the models of limited commitment (Thomas and Worrall 1988), we set a benchmark value for a of 8 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—6 or 4 years. An advantage of a shorter cutoff for a is that more matches will reach that duration. The downside is that it biases downward the cyclicality of our estimates to the extent that the impact of wage smoothing remains intact.

This leads to the question of how to deal with matches that do not reach duration a. Estimating only on the basis of 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 in t except those

[16] When 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). Last, 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.

lasting less than 1.5 years, to construct wage growth for matches starting in t. Relative to considering only matches lasting 8 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 $\ln w^{ij}_{t,t} - \ln w^{ij}_{t,t+a} = -\Sigma^{a}_{\tau=1}\Delta\ln w^{ij}_{t,t+\tau}$. The term $\Delta\ln w^{ij}_{t,t+\tau}$ denotes the wage growth between years $t+\tau-1$ and $t+\tau$ of worker i on job j, which we can construct from the individual wage data within a match. Implication 2 can then be rewritten as

$$\begin{aligned}\mathrm{Cov}(Cycle_t, \ln \mathrm{UC}^{W}_{t}) = \mathrm{Cov}\Bigg(&Cycle_t, -\sum_{\tau=1}^{a}\Delta\ln w_{t,t+\tau} \\ &- \sum_{\tau=2}^{a}\Omega_{t,t+\tau}(\Delta\ln w_{t,t+\tau} - \Delta\ln w_{t+1,t+\tau}) + \Omega_{t,t+a+1}\Delta\ln w_{t+1,t+a+1}\Bigg),\end{aligned} \tag{14}$$

where $\Omega_{t,t+\tau} = \Sigma^{\tau-2}_{k=0}(\prod^{k}_{\ell=0}\beta_{t+\ell}(1-\delta_{t+\ell}))$, $\Omega_{t,t+a+1}$ is equal to $\Omega_{t,t+\tau}$ at $\tau = a+1$, and $\Delta\ln w_{t,t+\tau} = \int_{ij}\Delta\ln w^{ij}_{t,t+\tau}$. Cyclicality of the new-hire wage is captured by the covariance of the cycle with the first term, $-\Sigma^{a}_{\tau=1}\Delta\ln w_{t,t+\tau}$, with cyclicality of the future wage path captured by the balance. That difference in wage paths is reflected in whether matches starting in t exhibit faster wage growth from $t+1$ to $t+a$ than matches starting in $t+1$. 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 in $t+a-1$ 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 $\Delta\ln w^{ij}_{t,t+\tau}$ 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:

$$\Delta\ln w^{ij}_{t,t+\tau} = \Psi x^{ij}_{t+\tau} + \sum_{d_0=1980}^{2011}\sum_{d=d_0+1}^{2019}\psi_{d_0,d}D^{ij}_{d_0,d} + \epsilon^{ij}_{t+\tau}, \tag{15}$$

where dummy variables $D^{ij}_{d_0,d}$ equal 1 if $d_0 = t$ and $d = t+\tau$, equaling 0 otherwise, and $x^{ij}_{t+\tau}$ 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

[17] For NLSY surveys that are biannual, we annualize 2-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.

or NLSY97), and a quadratic in their age. Because we set a to 8 years, we estimate the regression on the sample of jobs that start between 1980 and 2011 (i.e., 8 years before our sample ends in 2019).[18]

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

$$\begin{aligned}\mathrm{Cov}(Cycle_t, \ln \mathrm{UC}_t^W) &= \mathrm{Cov}(Cycle_t,\\ -\sum_{\tau=1}^{a}\hat{\psi}_{t,t+\tau} - \sum_{\tau=2}^{a}\Omega_{t,t+\tau}&(\hat{\psi}_{t,t+\tau} - \hat{\psi}_{t+1,t+\tau}) + \Omega_{t,t+a+1}\hat{\psi}_{t+1,t+a+1}),\end{aligned} \tag{16}$$

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

Substituting $\hat{\psi}_{t,t+\tau}$'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 $t + \tau$. Because of this selection, the expected value of $\hat{\psi}_{t,t+\tau}$ is $E[\ln w^{ij}_{t,t+\tau} - \ln w^{ij}_{t,t+\tau-1} | \Gamma^{ij}_{t,t+\tau-1} = 1, \Gamma^{ij}_{t,t+\tau} = 1]$, where $\Gamma^{ij}_{t,t+\tau-1}$ and $\Gamma^{ij}_{t,t+\tau}$ are 0/1 variables, equal to 1 if match ij survives to $t + \tau - 1$ and $t + \tau$, respectively. If there are idiosyncratic shocks to match quality, then this is potentially biased from $E[\ln w^{ij}_{t,t+\tau} - \ln w^{ij}_{t,t+\tau-1}]$ by selection on which matches survive. But the direction of that bias is difficult to predict, as it reflects selection on the wage in $t + \tau - 1$ as well as in $t + \tau$.[19]

For our purposes, what matters is whether the magnitude of any selection effect varies systematically with the state of the business cycle in t. That is, the contribution to the covariance terms in equation (14) based on surviving matches is $\mathrm{Cov}(Cycle_t, E[\Delta \ln w^{ij}_{t,t+\tau} | \Gamma^{ij}_{t,t+\tau-1} = 1, \Gamma^{ij}_{t,t+\tau} = 1])$ rather than the covariance of $Cycle_t$ with $\Delta \ln w_{t,t+\tau}$, the expected wage growth for all matches that start in 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.

[18] When estimating $\psi_{t,t+\tau}$, we require each combination of starting and current year, $(\mathrm{t}, \mathrm{t} + \tau)$, 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: 1980–81, 1995–96, and 1997–98. In these cases, we impute the first growth rate using the growth between $t + 1$ and $t + 2$.

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

For this reason, in section IV.B 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 in $t + \tau - 1$ survives to $t + \tau$. We also include all workers in constructing cumulative 8-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; 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 cyclically in job ladders, 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 that the separation rate only depends on calendar year. But the estimation allows 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 IV.C and in sections B.2 and B.3 of appendix B (apps. A, B are available online). The computation of user cost in its general form is described in appendix A.

IV. Cyclicality of the New-Hire Wage and User Cost

A. 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 the cyclicality of the new-hire wage in section IV.B, then the cyclicality of user cost in section IV.C. Because most estimates of wage cyclicality are based on average hourly earnings, we first examine cyclicality for this measure in our NLSY data. Table 1 gives results from the NLSY data for 1980–2011, 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

[20] While 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 (2013) 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.

Table 1
Cyclicality of Average Hourly Earnings (Dependent Variable Is Log of Real Wage: ln(*w*/*p*))

	Age Control (1)	Individual Fixed Effects (2)	Match Fixed Effects (3)
Unemployment rate	−.29	−.83	−.50
	(.49)	(.34)	(.31)

NOTE.—Our sample (the NLSY79 and NLSY97 panels) has 110,047 observations for 1980–2011. 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.

estimates of the quality-adjusted new-hire wage and user cost reported below. (We report results for 1980–2019 in the note to table 1.) The cycle is measured by the national unemployment rate, controlling for a cubic trend.

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% for a 1 percentage point 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-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 1 percentage point higher unemployment goes from −0.29 to −0.83 and is now statistically significant (standard error: 0.34).

Last, 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 percentage point 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

[21] For comparison, we estimate the cyclicality of average hourly earnings measured from the CPS IPUMS micro data or by the Current Employment Survey (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 his or her weeks worked and usual weekly hours worked. (For the CPS regression, we control for a cubic in age as well as the cubic time trend.) The CES measure is its "average hourly earnings of production and nonsupervisory employees, total private." So it is a more restrictive sample of workers than we consider in the NLSY. Furthermore, given that it reflects aggregate earnings relative to aggregate hours, in estimating cyclicality it implicitly weights individual workers by their relative earnings. Comparable to our estimates from the NLSY panels in col. 1, the average hourly earning series from the CPS is perhaps slightly procyclical (estimated impact of 1 percentage point higher unemployment on real wages of −0.49, with a standard error of 0.28), while that from the CES is essentially acyclical (estimated impact of −0.13, with a standard error of 0.26).

competing explanations for why match controls reduce cyclicality. One is that job turnover produces strongly procyclical firm and match quality. The second is that wages are largely insulated within matches, as predicted by many contracting models, so including match effects misses much of the cycle's impact on wages. To progress past this perceived impasse, we turn to our quality-adjusted estimates for the new-hire wage and user cost.

B. Cyclicality of the Quality-Adjusted New-Hire Wage

1. Benchmark Estimates

We first estimate wage growth dummies, $\psi_{t,t+\tau}$'s in equation (15), from he 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, $-\Sigma_{\tau=1}^{8}\hat{\psi}_{t,t+\tau}$, to estimate new-hire wage cyclicality. For convenience, we refer to this variable as the new-hire wage for the balance of this section. But more accurately, our assumptions imply that it is equal to the quality-adjusted new-hire wage in t plus an error that is orthogonal to the cycle in t.

Figure 2 presents the time series for our new-hire wage for the 32 annual observations for 1980–2011 for $a = 8$, together with the national unemployment rate. The new-hire wage is clearly highly procyclical. Most notably,

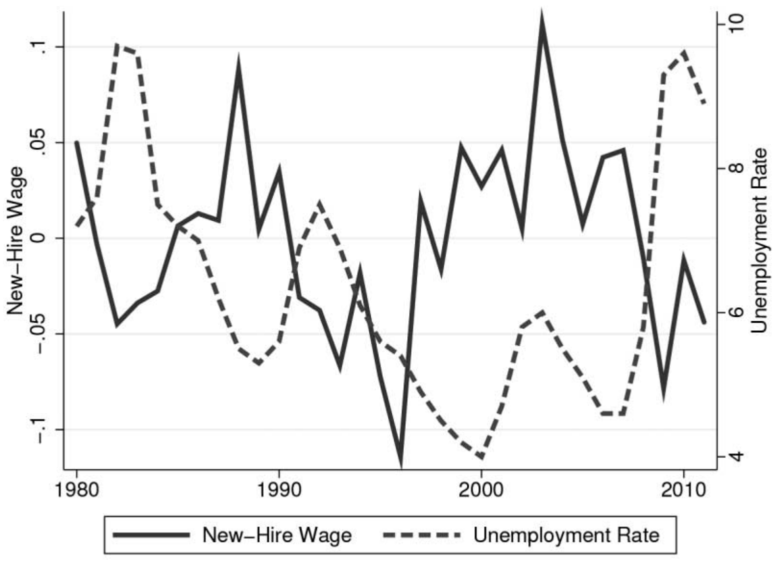

FIG. 2.—Time series of the quality-adjusted new-hire wage. The unemployment rate (right scale) is in percentage points. The new-hire wage (left scale) is in terms of percentages and is normalized to average zero for the sample period (e.g., 0.1 means 10% above the sample-period mean). A color version of this figure is available online.

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

	(1)	(2)
Unemployment rate	−2.35	−2.49
	(.67)	(.62)
Unemployment rate 8 years ahead		1.04
		(.56)

NOTE.—This table shows the percent change in wages in response to a 1 percentage point increase in unemployment. Thirty-two annual observations: 1980–2011. Regressions include a cubic trend. Robust standard errors are in parentheses.

it decreases by about 9% and 12% for the two large recessions in 1980–82 and 2007–9, respectively. Table 2, column 1, gives the estimated cyclicality 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 in 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 8 years ahead. This would be violated if the cyclical state in t either (i) is correlated with the cycle 8 years later or (ii) still helps to predict wages 8 years into the match because of the highly persistent effects of cyclical wage smoothing. Note that the latter violation should act to bias our estimates toward zero cyclicality.

We can test condition i by seeing whether the unemployment rate in t, relative to any trend movements, predicts the rate in $t + 8$. It does not. Furthermore, column 2 of table 2 shows that controlling for the unemployment rate in $t + 8$ yields essentially the same cyclicality of the new-hire wage, with a response to a 1 percentage point higher unemployment rate in t of −2.49% (standard error: 0.62%).

2. *Cyclicality of Match Quality*

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

We regress our implied measure of match quality for hires in t on the unemployment rate in t and a cubic trend. The estimated coefficient implies that a 1 percentage point 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 sullying effect). So our estimate of acyclical match quality is consistent with these effects roughly canceling or neither being overly important.

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 during recessions exhibit faster quality growth, that would bias our estimate toward a more procyclical wage. Conversely, if matches starting during 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 during 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 during recessions, then our estimate would be procyclically biased.

Both Bowlus (1995) and Mustre-Del-Rio (2019) find from NLSY79 data that jobs that began during recessions exhibited somewhat shorter average duration. This is suggestive that selection on shocks to match quality growth could differ by whether a match begins during 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 percentage point 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 during a boom (solid line), evaluated at an initial unemployment rate of 4.3%, versus one that starts during a severe recession (dashed line), at an unemployment rate of 9.6%.

We perform four robustness exercises to address whether match quality grows faster for matches that began during 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; and (iv) we follow wages for 8 years from the start of job matches, even if the worker moves to a new match, but control

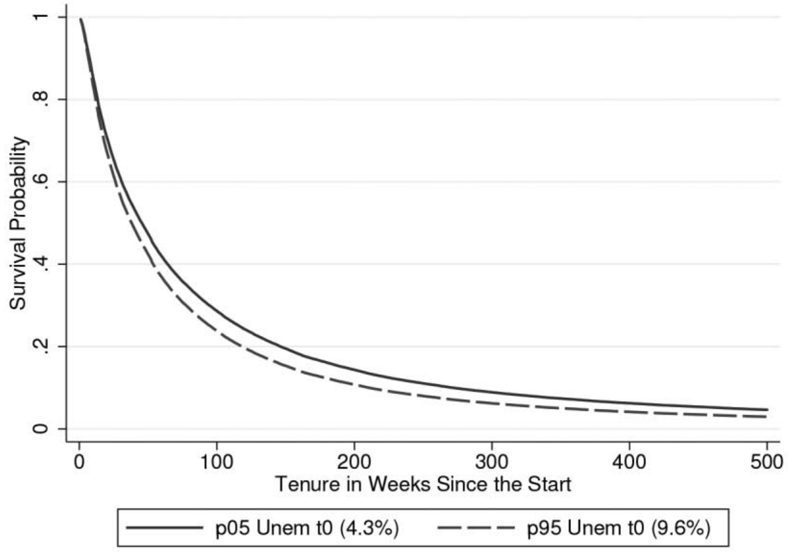

FIG. 3.—Match survival analysis. This figure shows the estimated survival probability from a proportional Cox model. The left-hand side in the model is the survival hazard; the right-hand side is the initial unemployment rate, cubic age polynomial, cubic time trend, and gender, education, and race dummies. We interact all variables (except the initial unemployment rate) with the dummy for the NLSY97 sample. A color version of this figure is available online.

for observable differences in match quality between any new job in $t + 8$ versus the job started in 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]

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

[23] We use the crosswalk of David Autor and David Dorn to create a consistent occupation code between survey years. Unfortunately, the regular three-digit codes are too fine for our exercise, having several occupations with only a few wage observations. We aggregate occupations to two digits, which gives 81 occupations. For example, occupation 166—economists, market and survey researchers—is classified

Table 3
Cyclicality of Quality Measures within Matches

	Δln(wage) (1)	Δ(occupation index) (2)	Δln(workweek) (3)
ΔUnemployment rate	−.002	−.001	−.004
	(.001)	(.001)	(.001)
Unemployment rate in t_0	.318	−.003	−.036
	(.102)	(.056)	(.048)

NOTE.—The sample reflects 45,269 observations from 1980 to 2019 for matches started between 1980 and 2011 and have 8 years of tenure or less. Changes in wage, weekly hours, the occupation index, and the unemployment rate can reflect a 1- or 2-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, and 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.

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 8 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 8 years of matches. Consistent with our results for cyclicality of the new-hire wage from table 2, matches display significantly faster growth of 0.32% per year for each additional percentage point of unemployment at their start (standard error: 0.10%). If one were to 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.

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.

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 during recessions. The coefficient on the initial unemployment rate, −0.036 with a standard error of 0.048, actually suggests less quality growth within matches that start at higher unemployment rates, implying that within-match quality changes actually bias our results by making the new-hire wage appear less procyclical. But the implied bias is not especially large, nor is it statistically significant.

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 in t is uncorrelated with the cycle in 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 8 years. We now consider reducing the threshold for a to 6 or even 4 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 in t is still exhibited in wages in $t + 6$ or $t + 4$.

Table 4, column 2, shows that the estimates of the cyclicality of the quality-adjusted new-hire wage are little affected by shortening a to 6 years. The impact of 1 percentage point higher unemployment is now to reduce the new-hire wage by 2.12%, with a standard error of 0.51%. Cutting a to 4 years further reduces the cyclicality of the new-hire wage, with a 1 percentage point increase in unemployment predicting a 1.53% lower wage (standard error: 0.58%). This could reflect that selection on wage changes increases our estimated cyclicality. It could alternatively reflect that the impact of the unemployment rate in t on the wage in $t + \tau$ subsides only two-thirds as much in $\tau = 4$ as in $\tau = 8$. Regardless, the estimated new-hire wage, even setting $a = 4$, is highly procyclical.

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.

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

	Cutoff after 8 Years (1)	Cutoff after 6 Years (2)	Cutoff after 4 Years (3)
Unemployment rate	−2.35	−2.12	−1.53
	(.67)	(.51)	(.58)

Note.—Thirty-two annual observations: 1980–2011. Coefficients are percent responses to the unemployment rate. Regressions include a cubic trend. Robust standard errors are in parentheses.

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

	Benchmark (1)	Control for Relative Duration (2)	Heckman Correction (3)
Unemployment rate	−2.35	−2.46	−2.17
	(.67)	(.73)	(.65)

NOTE.—Thirty-two annual observations: 1980–2011. Coefficients are percent responses to the unemployment rate. Regressions include a cubic trend. Robust standard errors are 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).

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 during 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 during 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 during 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 a standard error of 0.04%. (We assume that the impact of a ventile increase in relative duration on wage growth is the same across cohorts.) But comparing columns 1 and 2 from table 5, controlling for this effect in our first stage has little effect on the estimated cyclicality of the new-hire wage: a 1 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,

[24] More exactly, the dependent variable is equal to 1 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.

variables for marital status, residence in an urban area, and 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: 1.15%), meaning that the average observed rate of wage growth is slightly higher for those who have a lower probability of selecting into the sample.

The third column of table 5 reports the resulting cyclicality of the new-hire wage with predicted match wage growth augmented for the Heckman correction. Estimated cyclicality is smaller than our benchmark estimate, with a 1 percentage point higher unemployment rate associated with a decrease in the new-hire wage of 2.17%, with a standard error of 0.64%. But the estimate still implies a new-hire wage that is economically and statistically highly procyclical.

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 in 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 8 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 in $t + 8$ versus that started in t. These are average working hours in the match and dummies for the realized duration of the match (less than 2 years, 2–4 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 in 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.

[25] We allow coefficients for these variables to differ by NLSY survey. Economically and statistically significant effects in the probit include the following: 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 respondents; and having more children increases the probability of staying.

[26] We construct the sample by associating the worker's main job 8 years later with the match started in 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 8 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-year wage growth. Given that the samples became biannual, we also compute a 7-year change for those we cannot compute at 8 years.

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

	≥18 Month Duration		All Matches	
	Quality Control		Quality Control	
	No (1)	Yes (2)	No (3)	Yes (4)
Unemployment rate	−2.90	−2.88	−3.17	−3.13
	(.70)	(.66)	(.64)	(.62)

NOTE.—Thirty-two annual observations: 1980–2011. Coefficients are percent responses to the unemployment rate. Regressions include a cubic trend. Robust standard errors are in parentheses. Quality controls (QC) reflect workweeks and realized duration in jobs started in t and working in $t + 8$.

Table 6 reports estimated cyclicality of the new-hire wage constructed from wage growth for workers fully 8 years from match start, including those who leave the match. We restrict the sample to matches that last at least 18 months to be consistent with our previous results. Columns 1 and 2 give results respectively without and with the controls for match quality. A 1 percentage point 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 similarly cyclical, with a coefficient of −2.88% (standard error: 0.66%). Both estimates imply modestly greater cyclicality 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, to calculate those wage changes. But the approach in table 6, following all workers 8 years, does not require that restriction. Columns 3 and 4 repeat the estimation for all of the matches in our sample, including those that last less than 18 months. Without match quality controls (col. 3), 1 percentage point higher unemployment is associated with a 3.17% lower new-hire wage (standard error: 0.64%). Adding match quality controls (col. 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.

[27] Coefficients in the cumulative wage growth regression for the average workweeks in the current and 8-years-ahead matches are, respectively, 0.035% (standard error: 0.078%) and 0.157% (standard error: 0.071%). The dummies for realized match duration (2–4 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 in t.

C. Cyclicality of the User Cost of Labor

We now move to estimates of the cyclicality of labor's user cost. In the next subsection we examine the impact on the "pure" wage component of user cost, that is, the impact of the cycle on the quality-adjusted wage paths from starting a position in 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.

1. *Cyclicality of the Wage Component of Labor's User Cost*

The wage component of user cost will reflect cyclicality in the new-hire wage, just reported, as well as any cyclical differential in the wage path from $t+1$ forward for matches starting in 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 8 years of the matches in our NLSY samples.[28] We then 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 section B.2 of appendix B. We estimate a time-varying discount factor, β_t, based on movements in real consumption of nondurables and services as, for instance, in Bansal et al. (2014).[29]

Table 7 reports the cyclicality of labor's user cost. Assuming a constant separation rate and discount factor, we find that a 1 percentage point 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 cyclicality of user cost is robust to allowing for cyclical discount and separation rates. Allowing only for cyclical β_t (table 7, row 3), a 1 percentage point 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 cyclicality of user cost.[30] While higher discounting during recessions acts to lower the impact of future wage paths on user cost, the decline in discounting during booms acts in the opposite direction. In row 4 of table 7, we allow

[28] More exactly, 0.285 is the mean value of the estimated year dummies in the linear probability model for separating described in sec. B.2 of app. B. 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.

[29] We restrict attention to constant relative risk-aversion preferences with an intertemporal elasticity of substitution equal to 0.5. More detail is provided in sec. B.3 of app. B.

[30] For instance, regressing $\prod_{\tau=0}^{i}\beta_{t+\tau}(1-\delta)$ on the unemployment rate in t and a cubic trend yields respective coefficients for a percentage point increase in unemployment of -0.70 (0.22), -0.73 (0.21), and -0.23 (0.08) for $i = 0, 2, 6$.

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

	Unemployment
New-hire wage	−2.35
	(.67)
Wage component of labor's user cost:	
User cost with constant β, constant δ	−4.81
	(1.83)
User cost with time-varying β, constant δ	−4.98
	(1.85)
User cost with time-varying β, time-varying δ	−5.28
	(2.08)
User cost with time-varying β, time-varying and start date–specific δ	−5.32
	(1.87)

NOTE.—Thirty-two annual observations: 1980–2011. Regressions include a cubic trend. Robust standard errors are in parentheses.

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 percentage point increase in unemployment.[31]

We next allow for the separation rate to vary with both the current year and the match's starting year while continuing to allow β to vary. (See sec. II.B.3 for the definition of user cost for this general case.) To implement, we estimate the separation rate as a function of a full set of dummies interacting the 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 that 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 a 1 percentage point increase in unemployment is −5.32 (standard error: 1.87%). This is essentially unchanged from our baseline estimate just described that assumes $\delta_{t,t+\tau} = \delta_t$.

To put that impact in perspective, consider the 2007–9 recession: between 2007 and 2009, the unemployment rate went up by 3.5 percentage points, controlling for a cubic trend. The estimate of −5.32 associates a decline in labor's user cost of 18% 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 during

[31] Our 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 a 1 percentage point increase in unemployment of −0.18 (0.68), −1.64 (0.50), and −0.17 (0.18) for $i = 0, 2, 6$.

a recession, with the unemployment rate high and the new-hire wage low. As the economy recovers, the 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 the cyclicality of the discounted future wage path by simply subtracting the impact of the cycle on the new-hire wage from its impact on user cost: 1 percentage point higher unemployment reduces discounted future wages by -2.97%, with a standard error of 1.47%.

2. *Adjusting for Less Durable Matches Starting during Recessions*

If cohorts of new hires who start during recessions display systematically higher separation rates, then, as discussed in section II.B, starting a position in t rather than in $t + 1$ will affect future hiring costs. Here, we explore the potential importance of that channel for the cyclicality 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, from equation (5), what we label the hiring cost component of user cost, $\mathrm{UC}_t^{\kappa} = E_t\Sigma_{\tau=0}^{\infty}\mathfrak{B}_{t,t+\tau}(\pi_{t,t+\tau} - \pi_{t+1,t+\tau})\kappa_{t+\tau}$, 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, $\widetilde{\mathrm{UC}}_t^{\kappa}$, 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, $\hat{\delta}_{t,t+\tau}$'s, and calculate hypothetical separation rates, $\tilde{\delta}_{t,t+\tau}$, that remove the estimated impact of the unemployment rate at match start. That adjustment reflects our estimated hazard function from section IV.B.3, where we found that the separation rate is increased by 2.6% for a 1 percentage point increase in unemployment at match start. Finally, we estimate cyclicality in $\ln \mathrm{UC}_t^{\kappa}$ for both the actual and the counterfactual separation rate. The differential cyclicality 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 incurred only in the starting period. We set that cost, κ, equal to one-fourth of the steady state wage ϕ, which we normalize to 1. That is, the hiring cost is equivalent to 3 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]

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

Alternatively, we allow for both 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 $\kappa_\tau = \kappa + \lambda_\tau$. As before, κ captures the up-front 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 8 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 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 cyclicality 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 cyclicality in the new-hire wage and the pure wage component of user cost. The higher separation rate for workers hired during recessions implies that the hiring cost component of user cost is highly countercyclical. For $\kappa = 0.25$, 1 percentage point higher unemployment increases UC_t^κ by 6.21% (standard error: 1.32%) compared with the counterfactual hiring user cost constructed without history dependence in separation rates. 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 cyclicality in UC_t^κ,

[33] Controlling for a quadratic in age, we estimate an average annual growth rate of 3.09% (standard error: 0.09%) for the first 8 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 4 years, then 2.43% [0.12%] from 4–8 years.) Relatedly, Kehoe et al. (2022) set the rate of human capital growth in their model to 3.5%, citing estimates of average wage growth from Rubinstein and Weiss (2006).

[34] More exactly, let η^W and η^κ be, respectively, the semielasticities of the wage and hiring cost components of user cost with respect to the unemployment rate. Our adjusted measure of cyclicality equals $\tilde{\eta}^W = \eta^W + (UC^\kappa/UC^W)\eta^\kappa$, where UC^κ/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 cyclicality in future hiring costs? The relative importance of UC^κ reflects our estimated $\delta_{t,t+\tau}$'s and

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

	Unemployment
New-hire wage	−2.35
	(.67)
User cost (table 7, row 5)	−5.32
	(1.87)
Wage component of labor's user cost, adjusted for match durability:	
User cost with hiring costs	−4.79
	(1.88)
User cost with hiring and persistent training costs	−4.21
	(1.90)

NOTE.—Thirty-two annual observations: 1980–2011. Regressions include a cubic trend. Robust standard errors are in parentheses.

6.21%, then add the product to the estimate of user cost cyclicality, −5.32%, from row 2. The result, row 3 of the table, shows that the response of user cost to a percentage point increase of unemployment is reduced to −4.79% (standard error: 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 percentage point increase in unemployment increases the hiring component of the user cost of labor, UC_t^κ, by 3.72% (standard error: 0.78%) compared with the counterfactual hiring component of the user cost. That is a smaller percent response than with only an up-front 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

$\beta_{t+\tau}$'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 in t while saving in expectation $(1-\delta)\kappa$ in $t+1$. 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. Given the wage, ϕ is normalized to 1, and the steady-state wage component of user cost, UC^W, is also normalized to 1. So 8.57% is the relative importance of UC^κ to UC^W.

[35] This coefficient adjusts for 1 percentage point 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 semielasticity 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 $\hat{\beta}_i$ are the Gauss-Hermite weights and nodes and $\bar{\eta}^\kappa$ is the semielasticity point estimate of −4.79%. The standard error of the estimated semielasticity is 0.08%, which only marginally increases the standard error in row 3.

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

the cyclicality in UC_t^κ to the estimate of user cost cyclicality from row 2. The result is that 1 percentage point 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 reduces the cyclicality of user cost only 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%.

D. Robustness to Measures of the Business Cycle

In table 9, we report cyclicality in the new-hire wage and user cost of labor across alternative methods of detrending to define the cycle, as well as expressing the cycle in terms of (the 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 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 percentage point increase in unemployment defined relative to a quadratic or cubic trend, and it declines by around 1.7% for a percentage point increase 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.

Table 9
Robustness to Measure of Cycle

	New-Hire Wage		User Cost		Adjusted User Cost	
	Unemp (1)	log(GDP) (2)	Unemp (3)	log(GDP) (4)	Unemp (5)	log(GDP) (6)
Quadratic trend	−2.48	1.40	−5.24	2.68	−4.10	2.25
	(.39)	(.20)	(1.59)	(.70)	(1.62)	(.70)
Cubic	−2.35	1.51	−5.32	2.98	−4.21	2.58
	(.67)	(.28)	(1.87)	(.79)	(1.90)	(.78)
HP filter	−1.59	1.05	−5.33	3.22	−4.08	2.70
	(.69)	(.36)	(2.76)	(1.39)	(2.80)	(1.38)
One-sided HP filter	−1.75	1.20	−4.83	2.91	−3.64	2.40
	(.43)	(.26)	(2.57)	(1.42)	(2.34)	(1.24)
Hamilton filter	−1.64	.79	−4.02	1.76	−3.25	1.53
	(.48)	(.21)	(1.76)	(.77)	(1.91)	(.78)

Note.—All regressions have 32 annual observations from 1980 to 2011 except the one using the Hamilton filter, which has 29 observations from 1983 to 2011. Robust standard errors are in parentheses.

For user cost, in columns 3 and 4 we first consider the wage component of user cost, which adds the impact of the cycle on future wage paths to that for the new-hire wage. We allow the separation rate to vary with both 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 percentage point increase in unemployment, across all of 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 of the filters: by about double for the quadratic and cubic trends and the Hamilton filter, 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 those for the new-hire wage.

Last, columns 5 and 6 show the estimated cyclicality in user cost allowing that hiring during a recession increases both future hiring and training costs—that is, the latter case in section IV.C.2. Adjusting for future hiring and training costs reduces the cyclicality of user cost by at most a fourth across the filters and regardless of whether the cycle is measured 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.

V. Comparison with Prior Treatments of Quality

The literature has mainly used two approaches to control for quality to estimate the cyclicality 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 his or her 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 NLSY data, comparing the results to those from our new approach to adjust for cyclical match quality.

A. Individual Fixed Effects

Under the fixed effects approach, the cyclicality of wages of new hires is estimated from

$$\ln w^{ij}_{t,t} = \alpha Cycle_t + \ln w^{i}_{\mathrm{fe}} + \epsilon^{ij}_{t,t}. \tag{17}$$

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

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

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

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

There are distinct reasons this might be the case. First, the worker's wage fixed effect, $\widehat{\ln w_{\text{fe}}^{i}}$, reflects match qualities in the individual's entire panel, not only on the job started in t. So, if match quality in t differs from the worker's average match quality over their sample, then this will affect estimated cyclicality. As discussed from the outset, this bias could be procyclical (sullying 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 cyclicality.

Second, if wages are smoothed, then the worker's wage fixed effect will reflect the impact of the cycle in 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 $\widehat{\ln w_{\text{fe}}^{i}}$ reflects $\phi_{t,t}, \hat{\phi}_{t,t}$ will understate fluctuations in $\phi_{t,t}$. Therefore, $\mathrm{Cov}(Cycle_t, \ln \hat{\phi}_{t,t})$ will understate the cyclicality of new-hire wages. Our approach alleviates that bias by basing the control for match quality on the expected wage 8 years ahead, which we assume is little influenced by the cycle in 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 percentage point increase in the unemployment rate (standard error: 0.31%). New-hire wages are considerably more procyclical, decreasing by −1.95% for each percentage point increase 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.

1. *Cyclicality of New-Hire Wage, Job to Job versus via Nonemployment*

Gertler, Huckfeldt, and Trigari (2020) estimate new-hire wage cyclicality from both a fixed effects and a wage-change specification stratifying new hires by whether the match was job to job or preceded by a spell of nonemployment. They estimate, based on SIPP data, that wages are more

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

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

Table 10
Cyclicality of Wages, Fixed Effects Approach

	log(wage)
Stayer × unemployment rate	−.64
	(.31)
New hires × unemployment rate	−1.95
	(.36)

Note.—This table shows the percent wage response to a 1 percentage point increase in unemployment. The 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 NLSY79 and NLSY97 except the unemployment rate and cubic trend coefficients. Standard errors are clustered by survey year.

procyclical for job-to-job hires than hires transiting nonemployment. 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 $\mathrm{Cov}(Cycle_t, \ln q^{ij}_{t,t} - \widehat{\ln w^i_{\mathrm{fe}}}) = 0$ for hires from nonemployment while being positive for job-to-job hires. But an alternative interpretation is that $\mathrm{Cov}(Cycle_t, \ln q^{ij}_{t,t} - \widehat{\ln w^i_{\mathrm{fe}}}) < 0$ for hires from nonemployment, for instance, because workers entering unemployment in recessions leave particularly bad matches. Then the new-hire wage is countercyclically biased for hires that experienced nonemployment. 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 nonemployment and those hired directly from another job. Nonemployment 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

Table 11
Fixed Effects, Splitting New Hires by Whether Job to Job

	log(wage)
Stayer × unemployment rate	−.64
	(.31)
Via nonemployment × unemployment rate	−1.30
	(.34)
Job to job × unemployment rate	−2.22
	(.47)

Note.—This table shows the percent wage response to a 1 percentage point increase in unemployment. The 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 NLSY79 and NLSY97 except the unemployment rate and cubic trend coefficients. Standard errors are clustered by survey year.

Table 12
New-Hire Wage Cyclicality, Job to Job versus via Nonemployment

	All New Hires (1)	Via Nonemployment (2)	Job to Job (3)
Benchmark	−2.35	−2.31	−2.89
	(.67)	(1.01)	(.60)
Heckman correction	−2.17	−2.08	−2.69
	(.65)	(.98)	(.58)
Eight-year change with quality controls	−2.88	−2.84	−2.73
	(.66)	(.70)	(.70)

NOTE.—Thirty-two annual observations: 1980–2011. Coefficients are percent responses to the unemployment rate. Regressions include a cubic trend. Robust standard errors are 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), and −2.89 (0.60) for all hires, hires via nonemployment, and hires via job transition, respectively.

(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 a 1 percentage point increase in unemployment is −2.22% (standard error: 0.47%) versus −1.30% (standard error: 0.34%) for hires from nonemployment.

For comparison, row 1 of table 12 gives estimates for our approach, but now it is estimated separately for hires from nonemployment and job to job. We estimate greater cyclicality for job-to-job hires, but the difference is not statistically significant. The impact of a 1 percentage point increase in unemployment is −2.31% for hires from nonemployment (standard error: 1.01%) compared with −2.89% for those job to job (standard error: 0.60%). Thus, the new-hire wage is highly cyclical for both groups, especially compared with the cyclicality in wages for all workers (see table 1). Our approach yields greater cyclicality than using fixed effects both for hires from nonemployment (−2.31 vs. −1.31) and job to job (−2.89 vs. −2.21). One interpretation is that fixed effects estimates are biased by countercyclical match quality, especially for hires from nonemployment. 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 cyclicality.

The second and third rows of table 12 again split new hires by whether they are via nonemployment or job to job but now treat selection by (*a*) employing a Heckman correction or (*b*) following all workers out 8 years even if they move to a new match. In no case do we see much difference in cyclicality across the two sets of new hires. With the Heckman correction, the results closely parallel our benchmark estimates in row 1, although wages are a little less procyclical for both sets of new hires. Following all new hires for 8 years, wages for new hires from nonemployment and job to job are comparably cyclical.

B. First Differences

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

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

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

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

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

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

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

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

In the first column of table 13, we present the cyclicality of wages, separately for stayers and new hires, by regressing changes in log wages on changes in the unemployment rate for our NLSY sample as well as a quadratic trend.[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 percentage point 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

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

Table 13
Cyclicality of Wages, First-Differences Approach

	Δlog(wage) (1)	Δlog(wage) (2)
Stayer × Δunemployment rate	−.23	−.24
	(.29)	(.29)
New hires × Δunemployment rate	−.80	
	(.43)	
Via nonemployment × Δunemployment rate		.01
		(.80)
Job to job × Δunemployment rate		−.90
		(.48)

NOTE.—This table shows the percent change in wages in response to 1 percentage point increase 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 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.

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 nonemployment. We find that wage changes are procyclical for job-to-job hires—with 1 percentage point higher growth in the unemployment rate reducing the rate of wage growth by nearly 1%—and acyclical for hires from nonemployment. But the standard errors are sufficiently large that the estimate is not statistically significant for either group if viewed separately.[40]

VI. Conclusions

We estimate the cyclicality 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 percentage point 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

[40] From 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 nonemployment spell. Beyond being different samples, the SIPP and NLSY data differ in their frequency of wage observation. The SIPP asks for respondents' wages at 4-month intervals. Our NLSY data collect individuals' wages annually or biannually. The differences in frequencies not only affect the definition of the cycle but also could affect the importance of the biases outlined in this subsection.

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 percentage point 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, cyclicality 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 percentage point increase in unemployment. Finally, hiring during 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 offsets only about one-fifth the cyclicality 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 et al. 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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