Education and the Margins of Cyclical Adjustment in the Labor Market

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Abstract

Allocative wages—the labor costs considered when deciding to form or dissolve a long-term employment relationship—are more sensitive to cyclical conditions for more educated workers. Specifically, college-educated workers' allocative wages are highly pro-cyclical, while high school dropouts' wages exhibit only moderate cyclicality. Further, as education increases, an increasing share of the sensitivity of allocative wages is driven by the persistent scarring effects of the cyclical position at the time of hiring on the wages associated with higher levels of tenure, amounting to more than a third of the overall sensitivity for the college educated. The greater job stability of the more educated—and therefore the exposure to scarring—contributes to these differences. In addition, more significant scarring at each horizon of tenure amplifies the effect. In service of documenting these facts, I develop new methods for inferring the sensitivity of labor costs to shocks when agents are forward-looking and wages may be intertemporally smoothed.

JEL Classifications:

E24:	Employment • Unemployment • Wages
J31:	Wage Differentials • Education Based • Tenure Earning
J63 & M51:	Turnover • Employee Retention
J41:	Labor Contracts • Implicit Contracts
M52:	Compensation Methods and Their Effects

Keywords: User Cost of Labor, Implicit Contracts, Education and Wage Differentials, Tenure and Turnover, Wage Rigidity.

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1 Introduction

Economists and policy makers are concerned with heterogeneity. From a macroeconomic perspective, the differences in how agents' wages respond to shocks may help explain their amplitude and propagation. From a microeconomic perspective, heterogeneity in the pass-through of shocks to wages may be informative about the nature of labor contracting. From a policy perspective, acknowledging heterogeneity facilitates addressing the needs of those who are most vulnerable to shocks. If workers and firms are forward-looking, allocative wages include both wages paid to new hires and also the present discounted value of any persistent deviations between wages paid to workers hired today and their equivalent replacements who could be hired tomorrow. Thus, we should measure wages in a user cost framework (Kudlyak, 2014; Basu and House, 2016).[1] This paper documents that the cyclical sensitivity of both components of the allocative wage rises with education as does the contribution of the persistent-deviations component.

In documenting these facts, I develop a new method for estimating the sensitivity of the user cost of labor (UCL) to shocks, such as the business cycle. The intuition for the method lies in recognizing that the sensitivity in the UCL to a shock is a function of the sensitivity of the full wage-tenure profile of a worker hired contemporaneously to that shock as compared to the sensitivity of the full wage-tenure profile of an equivalent replacement hired a period later. With this intuition in mind, I derive the mapping between the sensitivity of the UCL and the sensitivities of wages to 1) tenure, 2) tenure interacted with the conditions at the time of hiring for contemporaneously hired workers, and 3) tenure interacted with those same conditions for equivalent replacements. I show how these sensitivities and their covariance can be estimated via a Mincer (1974)-style wage regression. Further, I document that the novel method produces results comparable to existing studies.[2]

Deriving the estimator in this way enables investigating the contributions of separation

[1]Evidence of implicit contracts that yield persistent deviations has been documented as early as Beaudry and DiNardo (1991).

[2]See Appendix A.2 for details.

rates, returns to tenure, and the variation in each of those with respect to the shock and with the respect to an additional covariates to the overall sensitivity of the UCL. Specifically, the sensitivity of the UCL is the sum of three components: 1) the sensitivity of wages paid in the first period, the new hire's wage (NHW); 2) the present discounted value of the persistent difference in wages between a new hire and an equivalent replacement that is attributable to conditions at the time of the shock, the expected wage wedge (EWW); and 3) the sensitivity of the weight placed on wedges between wages paid at future dates due to the dependence of match survival probabilities of a worker hired at the time of the shock as compared to their equivalent replacement. While the contributions of all three sources of variation have the same sign, the third is smaller by an order of magnitude. This relative insignificance stems from the limited sensitivity of separation rates to the cyclical position at the time of hiring.[3]

In contrast, the cross-sectional variation in separation rates with respect to education is *large*. College-educated workers are 25 to 30 percentage points less likely to separate from an employer within a year than those without a high school degree, depending on the data source. These differences lead to differing importance of EWW, since more-educated worker's employment matches are more likely to survive to high tenures. The cyclical sensitivity of the EWW component accounts for less than a quarter of the cyclical sensitivity of the UCL for the least educated but more than a third for the most educated. The greater contribution of persistent deviations stems from a combination of greater job stability—which makes more-educated workers more exposed to persistent differences—and larger and more-persistent period-by-period differences.

I document that the aforementioned facts hold in the National Longitudinal Survey of Youth (NLSY) 1979 cohort—the workhorse data employed in previous studies of the UCL—and in the 1996-2008 panels of the Survey of Income and Program Participation (SIPP). Studying the UCL using the SIPP is unique to this paper and is facilitated by the novel empirical strategy, which facilitates the use of moderate-length panels. An advantage

[3]This is consistent with evidence present in Kudlyak (2014) and Bils, Kudlyak and Lins (2023).

to studying the SIPP is that it represents many cohorts' experiences over two business cycles and enables me to document two additional facts, as described below.

First, for workers with a high school degree or more, the cyclical sensitivity of the UCL and each of its components is relatively stable throughout their career. In contrast, for the less educated, sensitivity emerges later in their careers. This coincides with a dramatic increase in job stability for this group. Second, the UCL is substantially more sensitive to variation in the unemployment rate during expansions than contractions.[4] The reduced sensitivity in contractions is driven by a lower sensitivity of the NHW and is modestly offset by the greater sensitivity of the EWW. This suggests that wage contracts may offset to downward rigidity in current wages by withholding subsequent wage growth, as suggested by Elsby (2009).

This paper contributes to the growing literature within macroeconomics that documents that the labor market operates differently for different workers. Specifically, Cairó and Cajner (2018); Hall and Kudlyak (2019); Gregory, Menzio and Wiczer (2021), and Ahn, Hobijn and Sahin (2023) find that a large share of unemployment and unemployment fluctuations are driven by a subset of workers who face secularly higher job displacement rates. The secular correlation between being low income and being at high risk of displacement could increase the difficulty that agents face in self-insuring against shocks and amplify the welfare effects of business cycles (Mukoyama and Sahin, 2006). While the positive correlation between secular displacement risk and the propensity to be unemployed during a recession has clear implications for earnings via the extensive margin, the intensive margin—wages—is less well understood. This paper strives to understand this less well documented dimension.

Understanding the heterogeneity in the sensitivity of wages to shocks, particularly on the dimensions of workers' skill and expected match durability, can inform our understanding of the mechanisms at play in labor contracting (Keane and Prasad, 1993). Decomposing differences across education in the UCL as compared to the NHW, offers additional insight

[4] Maruyama and Mineyama (mimeo) document similar behavior using Japanese data.

into the use and design of forward-looking labor contracts; in particular, more educated workers' UCL is particularly sensitive because, for this group, macroeconomic conditions today have particularly persistent effects on remitted wages in the future. These differences are predicted by Thomas and Worrall (1988), who show that a longer planning horizon, because of a lower discount or separation rate, makes wage revisions less sensitive to shocks, which, in turn, results in a more persistent effect of past conditions on present wages. The Elsby (2009) model of a forward-looking firm's response to downward nominal wage rigidity has similar implications.[5]

Section 2 introduces the novel method for estimating the cyclical sensitivity of the forward-looking view of the wage contract. Section 3 describes the data used in this paper. Section 4 documents the differences in employment stability by education. Section 5 documents the headline results regarding the cyclical sensitivity of the UCL and decomposes the effect into its components across education and with respect to experience at the time of hiring and the cyclical position. Section 6 concludes and discusses applications of the new method to novel contexts that are beyond the scope of this paper.

2 Estimating the Cyclical Sensitivity of Wages

Kudlyak (2014) and Basu and House (2016) demonstrate that the UCL is the appropriate measure of the allocative wage to consider in an economy populated by forward-looking agents. This measure admits, but does not impose, the possibility that labor market frictions impart a durable quality to an employment relationship and, as a result, the sequence of payments under a wage contract (or an implicit one) might diverge from the sequence of wages that would arise in a spot market.

The existing methodology for estimating the UCL and its sensitivity to business cycle conditions follows a multistep procedure (Kudlyak, 2014; Basu and House, 2016). First, one

[5]Both models derive a real-rigidity in wages remitted during each period of an employment relation; specifically, an optimal wage contract that prescribes no revision to remitted wages for an interval of macroeconomic shocks. These inaction regions are larger when planning horizons are longer.

estimates the coefficients on a set of indicators that capture the effect on wages of having been hired on a specific past date given employment on a particular current date. In this first step, controls or other methods are used to purge these estimates of composition effects and biases due to covariation between tenure and match quality. Second, using the coefficients obtained in the first step, one constructs the time series of realized present discounted values of the implicit contracts originating at each date and, from these, the time series of the implied UCL. Inference regarding cyclicality can then be obtained using standard time-series methods. Two-step strategies for obtaining composition-corrected estimates of wage elasticities from panel data have been employed as early as Solon, Barsky and Parker (1994).

A drawback of a two-step procedure is the difficulty in considering heterogeneity in the cyclical sensitivity of wages with respect to a covariate that varies in the cross-section. In the case of a continuous covariate, this is impossible, as it would necessitate an infinite-dimension block of indicators in the first step. Even in the case of a discrete covariate, such as educational attainment, if some *start-date*×*current-date* cells are sparsely observed or unobserved, the two-step procedure can require imputations, and the statistical model used for imputation typically differs from statistical model of the UCL—for example in Basu and House (2016) and Maruyama and Mineyama (mimeo).

It is also possible to obtain composition- and bias-corrected estimates of the cyclical sensitivity of wages via a single-step procedure—classic examples being Bils (1985) and Beaudry and DiNardo (1991). In this approach, one runs a regression of log wages on an indicator for the cyclical position—typically the unemployment rate—in a panel of worker-level data. As in the two-step procedure, controls or other methods are used to purge these estimates of composition effects that may bias the results. Particularly relevant for the present work, Bils (1985) illustrates how one can include interactions between the cyclical indicator and covariates to obtain inference regarding heterogeneity in the cyclical sensitivity, and Beaudry and DiNardo (1991) illustrate that one may estimate the sensitivity of wages at date t to a shock that occurred at date $t - j$.

I begin by writing down a formulation of the wage component of the UCL in an environment that admits trend returns to tenure and duration dependence in survival probabilities as well as dependence on the state at hiring for both of these objects. This formulation is given by:

$$UCL_{v,t} = \mathbb{E}_t \sum_{j=0}^{\infty} \beta^j \left[S_{v,t}(j) w_{v,t,t+j} - \beta S_{v,t}(1) S_{v,t+1}(j) \frac{S_v^*(j+1)}{S_v^*(j) S_v^*(1)} \frac{w_{v,j+1}^*}{w_{v,j}^*} w_{v,t+1,t+j+1} \right], \quad (2.1)$$

where $w_{v,t,t+j}$ is the wage paid at date $t+j$ to a worker of type v hired on date t. $S_{v,t}(j)$ is the probability of survival to the j^{th} period for a v-type worker hired at date t. $w_{v,j}^*$ and $S_v^*(j)$ are, respectively, the expected wage paid to and survival probability of a worker of type v at horizon j who was hired when the cyclical indicator was on trend. v may be categorical, as in the case of education, which is the primary focus of this paper, or continuous, as in the case of potential experience at the time of hiring.

It is illuminating to rearrange the expression:

$$\begin{aligned} UCL_{v,t} = \; & \overbrace{w_{v,t,t}}^{\substack{\text{payment at date } t \\ \text{to a new hire} \\ \text{at date } t}} + \mathbb{E}_t \sum_{j=1}^{\infty} \beta^j \Bigg[\overbrace{S_{v,t}(j) w_{v,t,t+j}}^{\substack{\text{payment at date } t+j \text{ to a} \\ \text{worker hired at date } t}} \\ & - \underbrace{S_{v,t}(1) S_{v,t+1}(j-1) \frac{S_v^*(j)}{S_v^*(j-1) S_v^*(1)} \frac{w_{v,j}^*}{w_{v,j-1}^*} w_{v,t+1,t+j}}_{\text{payment at date } t+j \text{ to an } \textbf{\textit{equivalent replacement}} \text{ hired at date } t+1} \Bigg]. \end{aligned} \quad (2.2)$$

The first term is the new hire's wage (NHW). The first terms in the summation are the expected payments at dates $t+j$ to a worker hired at date t, while the second terms in the summation are the expected payments at dates $t+j$ to an *equivalent worker* hired at date $t+1$. Note, at horizon j, this equivalent replacement has $j-1$ periods of tenure. The terms on the second line account for potential trend returns to tenure and duration dependence in survival probabilities that may cause a worker with j years of tenure to systematically differ from a worker with $j-1$ years of tenure.[6] Specifically, the $\frac{w_{v,j}^*}{w_{v,j-1}^*}$ terms account for

[6] Examples pertaining to the return to tenure include the following: the growth in general human capital

the changed price of the labor input due to having accrued a period of tenure. If there is no trend return to tenure, then the term is equal to one. Similarly, the $\frac{S_v^*(j)}{S_v^*(j-1)S_v^*(1)}$ accounts for the change in the probability of match survival due to having accrued a period of tenure. If there is no duration dependence in survival probabilities, then the term is equal to one. The terms in summation represent the scarring effect of conditions at time t on a worker hired at time t as compared to on an equivalent replacement hired one period later at time $t+1$—an expected wage wedge (EWW).

Let $\tilde{x}_t$ be a deviation (resp. log-deviation) from trend of macroeconomic indicator x.

Assumption 1. *There exists a steady state in which the macro indicator is expected to be on trend next period if it is on trend now:* $\mathbb{E}_t[\tilde{x}_{t+1}|\tilde{x}_t = 0] = 0$.

Assumption 1 implies that $\mathbb{E}_t[w_{v,t,t+j}|\tilde{x}_t = 0] = \mathbb{E}_t[w_{v,t+1,t+j+1}|\tilde{x}_t = 0] = w^*_{v,j}$ and $\mathbb{E}_t[S_{v,t}(j)|\tilde{x}_t = 0] = \mathbb{E}_t[S_{v,t+1}(j)|\tilde{x}_t = 0] = S^*_v(j)$. Plugging these into equation 2.1 yields the following:

$$UCL^*_{v,t}\big|_{\tilde{x}_t=0} = w^*_{v,0} + \mathbb{E}_t \sum_{j=1}^{\infty} \beta^j \left[S^*_v(j) w^*_{v,j} - \frac{S^*_v(j)}{S^*_v(j-1)S^*_v(1)} S^*_v(1) S^*_v(j-1) \frac{w^*_{v,j}}{w^*_{v,j-1}} w^*_{v,j-1} \right] = w^*_{v,0}.$$

In words, Assumption 1 implies that the steady state UCL is equal to the steady-state NHW.

Noting that $\frac{dz}{dy} = \frac{dln(z)}{dy} z$, I can write down the semi-elasticity (resp. elasticity) of the

due to tenure, an explicitly back-loaded contract designed to reduce turnover, or evolving bargaining power over the course of the match. An example pertaining to duration dependence in the survival function is the revelation over time of a taste for the match that is orthogonal to productivity.

wage component of the UCL as the following:

$$
\begin{aligned}
\left.\frac{d\ln(UCL_{v,t})}{d\tilde{x}_t}\right|_{\tilde{x}_t=0} &= \frac{1}{w^*_{v,0}}\left.\frac{dUCL_{v,t}}{d\tilde{x}_t}\right|_{\tilde{x}_t=0} \\
&= \frac{d\ln(w_{v,t,t})}{d\tilde{x}_t} + \mathbb{E}_t\sum_{j=1}^{\infty}\beta^j\frac{w^*_{v,j}}{w^*_{v,0}}\Bigg[S^*_v(j)\left(\left.\frac{d\ln(w_{v,t,t+j})}{d\tilde{x}_t}\right|_{\tilde{x}_t=0} - \left.\frac{d\ln(w_{v,t+1,t+j})}{d\tilde{x}_t}\right|_{\tilde{x}_t=0}\right) \\
&\qquad + \left.\frac{dS_{v,t}(j)}{d\tilde{x}_t}\right|_{\tilde{x}_t=0} - \left.\frac{dS_{v,t}(1)}{d\tilde{x}_t}\frac{S^*_v(j)}{S^*_v(1)}\right|_{\tilde{x}_t=0} - \left.\frac{dS_{v,t+1}(j-1)}{d\tilde{x}_t}\frac{S^*_v(j)}{S^*_v(j-1)}\right|_{\tilde{x}_t=0}\Bigg].
\end{aligned}
$$

Note, the first line applies Assumption 1. In this paper, I focus on percentage point deviations in the detrended unemployment rates as the cyclical indicator; therefore, I henceforth focus on the semi-elasticity. If deviations from trend of the cyclical driver were instead measured in percent, the following could be applied identically to measure an elasticity.

Assumption 2. *The effect of a deviation of the macro indicator from the steady state at time t on wages and survival probabilities at time $t+j$ is independent of calendar time t.*

In other words, the effect of $\tilde{x}_t$ on wages at time $t+j$ for a worker hired at time t or $t+1$ is driven by the relative timing of the wage observation, j, and hiring—0 or 1—and not the calendar timing, t, of any of these events. Thus, I can write $\left.\frac{d\ln(w_{v,t,t+j})}{d\tilde{x}_t}\right|_{\tilde{x}_t=0} = \frac{d\ln(w_{v,0,j})}{d\tilde{x}_0}^*$, where $\frac{d\ln(w_{v,0,j})}{d\tilde{x}_0}^*$ is the percentage premium over the average return to the j^{th} period of tenure for worker type v due to having been hired when the macroeconomic variable of interest was $\tilde{x}$ above trend. Similarly, $\left.\frac{d\ln(w_{v,t+1,t+j})}{d\tilde{x}_t}\right|_{\tilde{x}_t=0} = \frac{d\ln(w_{v,1,j})}{d\tilde{x}_0}^*$, where $\frac{d\ln(w_{v,1,j})}{d\tilde{x}_0}^*$ is the percentage premium over the average return to the $j-1^{th}$ period of tenure due to having been hired one period after the macroeconomic variable of interest was $\tilde{x}_0$ above trend. Also, $\frac{dS_{v,0}(j)}{d\tilde{x}_0}^*$ and $\frac{dS_{v,1}(j)}{d\tilde{x}_0}^*$ are the probabilities of survival for j periods for cohorts of v-type workers hired at the time and one period after the time the macroeconomic variable of interest was $\tilde{x}$ above trend, respectively. So, the percentage effect of a unit deviation in $\tilde{x}$ on the UCL in the neighborhood of the steady state can be written as the following:

$$\underbrace{\frac{d\ln(UCL_{v,0})}{d\tilde{x}_0}}_{\epsilon_{UCL,\tilde{x}}} = \underbrace{\frac{d\ln(w_{v,0,0})^*}{d\tilde{x}_0}}_{\epsilon_{NHW,\tilde{x}}} + \underbrace{\mathbb{E}_0\left[\sum_{j=1}^{\infty}\beta^j S_v^*(j)\frac{w_{v,j}^*}{w_{v,0}^*}\left(\frac{d\ln(w_{v,0,j})^*}{d\tilde{x}_0} - \frac{d\ln(w_{v,1,j})^*}{d\tilde{x}_0}\right) + \Upsilon_v\right]}_{\epsilon_{EWW,\tilde{x}}}. \tag{2.3}$$

The semi-elasticity of the UCL with respect to $\tilde{x}_0$—henceforth denoted $\epsilon_{UCL,\tilde{x}}$—is the sum of two components. The first is the semi-elasticity of the NHW with respect to $\tilde{x}_0$—henceforth referred to as $\epsilon_{NHW,\tilde{x}}$. The second is the sensitivity of the discounted difference between expected wages in matches formed at state $\tilde{x}_0$ and in equivalent replacement matches formed one period later conditional on the information available at the time that state $\tilde{x}_0$ is observed: the semi-elasticity of the EWW—henceforth $\epsilon_{EWW,\tilde{x}}$. $\epsilon_{EWW,\tilde{x}}$ is a function of 1) the trend survival function; 2) the trend return to tenure; 3) the difference between the semi-elasticity of wages at tenure j and $j-1$ due to having been hired at the time that the state was $\tilde{x}_0$ above trend and due to having been hired one period after that time, respectively; and 4) $\Upsilon_v = \sum_{j=1}^{\infty}\beta^j \frac{w_{v,j}^*}{w_{v,0}^*}\left(\frac{dS_{v,0}(j)}{d\tilde{x}_0}^* - \frac{dS_{v,0}(1)}{d\tilde{x}_0}^*\frac{S_v^*(j)}{S_v^*(1)} - \frac{dS_{v,1}(j-1)}{d\tilde{x}_0}^*\frac{S_v^*(j)}{S_v^*(j-1)}\right)$, the sensitivity of the weight placed on wages paid at horizon j due to the dependence of the match survival probability on $\tilde{x}_0$ in the matches formed when the state is $\tilde{x}_0$ and in the matches formed one period later, respectively, again conditional on the information available at the time state $\tilde{x}_0$ is observed.

The marginal effect of v in the neighborhood of $v = 0$ is given by the following:

$$\begin{aligned}\left.\frac{d^2\ln(UCL_t)}{d\tilde{x}_t dv}\right|_{\tilde{x}_t=0,v=0} &= \frac{d^2\ln(w_{v,0,0})^*}{d\tilde{x}_0 dv}\\ &+ \sum_{j=1}^{\infty}\beta^j\left[S_0^*(j)\frac{w_{0,j}^*}{w_{0,0}^*}\left(\frac{d\ln(w_{v,j})^*}{dv} - \frac{d\ln(w_{v,0})^*}{dv}\right)\left(\frac{d\ln(w_{0,0,j})^*}{d\tilde{x}_0} - \frac{d\ln(w_{0,1,j})^*}{d\tilde{x}_0}\right)\right.\\ &+ \frac{w_{0,j}^*}{w_{0,0}^*}\frac{dS_v(j)^*}{dv}\left(\frac{d\ln(w_{0,0,j})^*}{d\tilde{x}_0} - \frac{d\ln(w_{0,1,j})^*}{d\tilde{x}_0}\right)\\ &+ \left.\frac{w_{0,j}^*}{w_{0,0}^*}S_0^*(j)\left(\frac{d^2\ln(w_{v,0,j})^*}{d\tilde{x}_0 dv} - \frac{d^2\ln(w_{v,1,j})^*}{d\tilde{x}_0 dv}\right) + \frac{d\Upsilon_v}{dv}\right].\end{aligned} \tag{2.4}$$

The first term in equation 2.4 is the marginal effect of v on $\varepsilon_{NHW,\tilde{x}}$. The second set of terms captures the variation in $\varepsilon_{EWW,\tilde{x}}$ due to variation in the trend wage tenure profile with respect to v. The third set of terms capture the variation in $\varepsilon_{EWW,\tilde{x}}$ due to variation in the trend survival probabilities with respect to v. The fourth set of terms captures variation in $\varepsilon_{EWW,\tilde{x}}$ due to variation in the sensitivity of wages at horizon j due to having been hired when the state was at $\tilde{x}_0$ or one period after with respect to v. Finally, $\frac{d\Upsilon_v}{dv}$ captures the marginal effect of v on the contribution of the cyclicality in separation rates to the cyclical sensitivity of the UCL of the v-type.[7]

Kudlyak (2014) and Bils et al. (2023) document evidence that cyclical variation in the survival probabilities has limited mechanical impact on the aggregate UCL. In Figure 1, I document that the state at hiring has limited impact on survival probabilities for every category of educational attainment. As a result and as documented in Appendix A.1, Υ_v is an order of magnitude smaller than the sum of the other terms in equation 2.3 for each level of educational attainment. Henceforth, I assume $\epsilon_{\Upsilon_v,\tilde{x}} = 0$ for all v. In addition, Υ_v is negative, thus the contribution of the cyclical fluctuation in separation rates amplifies rather than dampens the sensitivity of the UCL to cyclical fluctuations. Therefore, the assumption that Υ_v is zero imposes a small bias that attenuates estimates toward zero.

[7]For completeness

$$\begin{aligned}\frac{d\Upsilon_v}{dv} =& \sum_{j=1}^{\infty} \beta^j \Bigg[\frac{w^*_{0,j}}{w^*_{0,0}} \left(\frac{d\ln(w_{v,j})}{dv}^* - \frac{d\ln(w_{v,0})}{dv}^* \right) \left(\frac{dS_{v,0}(j)}{d\tilde{x}_0}^* - \frac{dS_{v,0}(1)}{d\tilde{x}_0}^* \frac{S^*_v(j)}{S^*_v(1)} - \frac{dS_{v,1}(j-1)}{d\tilde{x}_0}^* \frac{S^*_v(j)}{S^*_v(j-1)} \right) \\ &+ \frac{w^*_{0,j}}{w^*_{0,0}} \left(\frac{dS^2_{v,0}(j)}{d\tilde{x}_0 dv}^* - \frac{dS^2_{v,0}(1)}{d\tilde{x}_0 dv}^* \frac{S^*_v(j)}{S^*_v(1)} + \frac{dS_{v,0}(1)}{d\tilde{x}_0}^* \frac{S^*_v(j)}{S^*_v(1)} \left(\frac{dS^*_v(j)}{dv} - \frac{dS^*_v(1)}{dv} \right) - \frac{dS^2_{v,1}(j-1)}{d\tilde{x}_0 dv}^* \frac{S^*_v(j)}{S^*_v(j-1)} \right. \\ &\left. - \frac{dS_{v,1}(j-1)}{d\tilde{x}_0}^* \frac{S^*_v(j)}{S^*_v(j-1)} \left(\frac{dS^*_v(j)}{dv} - \frac{dS^*_v(j-1)}{dv} \right) \right) \Bigg].\end{aligned}$$

Turning to estimation, consider an augmented Mincer regression of the following form:

$$
\begin{aligned}
\ln(wage_{i,\tau}) = &\sum_{j=0}^{\infty}\Big[\chi_j\big(\mathbb{I}^{tenure}_{i,\tau,j}\times\tilde{x}_{\tau-j}\big)+\chi_{v,j}\big(v_{i,\tau}\times\mathbb{I}^{tenure}_{i,\tau,j}\times\tilde{x}_{\tau-j}\big)\Big]\\
&+\sum_{j=1}^{\infty}\Big[\psi_{j-1}\big(\mathbb{I}^{tenure}_{i,\tau,j-1}\times\tilde{x}_{\tau-j}\big)+\psi_{v,j-1}\big(v_{i,\tau}\times\mathbb{I}^{tenure}_{i,\tau,j-1}\times\tilde{x}_{\tau-j}\big)\Big]\\
&+\sum_{j=0}^{\infty}\Big[\zeta_j\mathbb{I}^{tenure}_{i,\tau,j}+\zeta_{v,j}\big(v_{i,\tau}\times\mathbb{I}^{tenure}_{i,\tau,j}\big)\Big]\\
&+\xi v_{i,\tau}+controls_{i,\tau}\Xi+\varepsilon_{i,\tau},
\end{aligned}
\tag{2.5}
$$

where $\mathbb{I}^{tenure}_{i,\tau,j}$ is an indicator equal to one if i has tenure j at time τ. If *controls* is correctly specified, the $\hat{\chi}_j$ are estimates of the premium or penalty over the trend return to the j^{th} year of tenure for the $v = 0$-type worker due to conditions at the time of hiring, $\tilde{x}_{\tau-j}$.[8] That is, the $\hat{\chi}_j$ identify the $\frac{dln(w_{v=0},0,j)}{d\tilde{x}_0}$ terms in equations 2.3 and 2.4. Similarly, the $\hat{\chi}_{v,j}$ identify the marginal effect of v on this sensitivity—that is, the $\frac{d^2ln(w_v,0,j)}{d\tilde{x}_0 dv}$ terms in equation 2.4. Meanwhile, $\mathbb{I}^{tenure}_{i,\tau,j-1}$ is an indicator equal to one if i has tenure $j-1$ at time τ and the $\hat{\psi}_{j-1}$ identify the $\frac{dln(w_{v=0},1,j)}{d\tilde{x}_0}$ terms in equations 2.3 and 2.4 while the $\hat{\psi}_{v,j-1}$ identify the $\frac{d^2ln(w_v,1,j)}{d\tilde{x}_0 dv}$ terms in 2.4.[9] Finally, ζ_j and $\zeta_{v,j}$ provide estimates of 1) the wage-tenure profile of a $v = 0$-type worker hired when $\tilde{x}$ is on trend and 2) the marginal effect of v on this trend

[8] *controls* consist of broad occupation (thirteen categories), a quadratic in potential experience (age-education-6), sex, race, Hispanic origin, years of education, and calendar time, and Ξ is the corresponding vector of coefficients. *controls* also contains corrections that address bias that would derive from omitting unobserved match quality from the regression. These corrections are discussed at the end of this section and in Appendix E. All controls are fully interacted with v.

[9] Note, if $\tilde{x}$ is autocorrelated and this autocorrelation is the sole reason why $\tilde{x}_{\tau-j}$ contains predictive power with respect to the wage-tenure profile of a worker hired at $\tau-j+1$—i.e. predictive power with respect to the $\frac{dln(w_{v=0},1,j)}{d\tilde{x}_0}$—the econometrician can obtain point estimates for the $\varepsilon_{UCL,\tilde{x}}$ and its components using only the wages of workers hired contemporaneously to the shock. Specifically, $\hat{\rho}\hat{\chi}_j$ identifies $\frac{dln(w_{v=0},1,j)}{d\tilde{x}_0}$, where $\hat{\rho}$ is an estimate of the autocorrelation of $\tilde{x}$. However, this procedure has two drawbacks. First, to construct standard errors, I would need to obtain an estimate of the covariance between $\hat{\rho}$ and all the other estimated coefficients, and $\hat{\rho}$ would derive from a separate statistical model of the autocorrelation of $\tilde{x}$; thus, I could not obtain standard errors via the Delta method, as I do in the body of this paper. In contrast, the procedure used in this paper recovers all the required coefficients in a single estimating equation and therefore enables implementation of the Delta method. Second, this procedure rules out data-generating processes in which $\tilde{x}_0$ has a direct effect on wages of hires at time 1 (that is, an effect that is not attributable to the time 1 or later realizations of $\tilde{x}$).

profile—specifically, $\mathbf{e}^{\hat{\zeta}_j-\hat{\zeta}_0}$ provides an estimate of each of the $\frac{w^*_{v=0,j}}{w^*_{v=0,0}}$ terms in 2.3 and 2.4, while the $\hat{\zeta}_{v,j}$ provide estimates for the $\frac{dln(w_{v,j})^*}{dv}$ terms in 2.4.[10]

Supposing β and $S^*_v(j)$ are known, I can plug the coefficients recovered in equation 2.5 into post-estimation equation 2.6 to construct an estimate of the semi-elasticity of the $v=0$-type worker's UCL to $\tilde{x}$ in the neighborhood of $\tilde{x}=0$:

$$\widehat{\epsilon_{UCL,\tilde{x}}} = \overbrace{\hat{\chi}_0}^{\widehat{\epsilon_{NHW,\tilde{x}}}} + \overbrace{\sum_{j=1}^{\infty} \mathbf{e}^{\hat{\zeta}_j-\hat{\zeta}_0}\beta^j S^*_0(j)\left(\hat{\chi}_j - \hat{\psi}_{j-1}\right)}^{\widehat{\epsilon_{EWW,\tilde{x}}}}. \tag{2.6}$$

Note that, like equation 2.3, the estimate of the $\epsilon_{UCL,\tilde{x}}$ implied by postestimation equation 2.6 decomposes into an estimate of the $\epsilon_{NHW,\tilde{x}}$ and the $\epsilon_{EWW,\tilde{x}}$. Also note that, nonzero χs and ψs are not themselves sufficient for nonzero $\epsilon_{EWW,\tilde{x}}$. Rather, a nonzero $\epsilon_{EWW,\tilde{x}}$ derives from nonzero *wedges* between the wages paid to time $\tau-j$ hires and to their equivalent replacements hired at time $\tau-j+1$—that is, nonzero $(\chi_j-\psi_{j-1})$.[11]

Similarly, I can plug these coefficients into equation 2.4 and obtain the marginal effect of v in the neighborhood of $v=0$ on this semi-elasticity:

$$\begin{aligned}\widehat{\frac{d\epsilon_{UCL,\tilde{x}}}{dv}} = \overbrace{\hat{\chi}_{v,0}}^{\substack{\text{marginal effect of } v\\ \text{on } \epsilon_{NHW,\tilde{x}}}} + \overbrace{\sum_{j=1}^{\infty}\beta^j\mathbf{e}^{\hat{\zeta}_j-\hat{\zeta}_0}\Big[\underbrace{S^*_0(j)\left(\hat{\chi}_{v,j}-\hat{\psi}_{v,j-1}\right)}_{\substack{\text{contribution of the}\\ \text{effect of } v \text{ on scarring}}} + \underbrace{\frac{dS^*_v(j)}{dv}\left(\hat{\chi}_j-\hat{\psi}_{j-1}\right)}_{\substack{\text{contribution of the}\\ \text{effect of } v \text{ on survival}}}}^{\substack{\text{marginal effect of } v\\ \text{on } \epsilon_{NHW,\tilde{x}}}} \\ + \underbrace{(\hat{\zeta}_{v,j}-\hat{\zeta}_{v,0})S^*_0(j)\left(\hat{\chi}_j-\hat{\psi}_{j-1}\right)}_{\substack{\text{contribution of the effect}\\ \text{of } v \text{ on trend returns to tenure}}}\Big],\end{aligned} \tag{2.7}$$

[10]Note, the ζ_j are co-linear and the $\zeta_{v,j}$ and ξ are co-linear. I normalize ζ_0 and $\zeta_{v,0}$ to zero.

[11]It is instructive to consider the case in which wages depend *only* on the contemporaneous realization of $\tilde{x}_\tau$—i.e., there are no scarring effects. In this case, $\chi_j=\psi_{j-1}$ for all j since, for each j, both χ_j and ψ_{j-1} are the same function of the realization of contemporaneous shock, $\tilde{x}_\tau$, which is in turn simply a function of $\tilde{x}_{\tau-j}$, the regressor in equation 2.5. While equation 2.5 will identify both $\hat{\chi}_j$ and $\hat{\psi}_{j-1}$, they will be *identical*, and the EWW implied in post-estimation equation 2.6 will be zero. Furthermore, the covariances between $\hat{\chi}_j$ and all the other coefficients, and those between $\hat{\psi}_{j-1}$ and all the other coefficients, will be the same, so the Delta method will yield a standard error of zero as well. In other words, the estimator will return a precisely estimated zero in the case of a data-generating process in which w depends only on contemporaneous realizations of a shock even if the shock process exhibits autocorrelation.

And, like equation 2.4, the estimated marginal effect of v on the $\varepsilon_{UCL,\tilde{x}}$ decomposes into four terms. The first is the estimated effect of v on the semi-elasticity of the NHW. The second, captured in the first block of terms in the summation, is the effect of v on the cumulative scarring effect of having been hired at the time of a shock vs one period after. The third, captured by the second block of terms in the summation, is the cumulative effect of the differences in the secular separation rate attributable to v. The fourth and final part, captured by the third block of terms in the summation, is the cumulative effect of the differences in the trend wage-tenure profile due to v. This empirical approach and decomposition extend naturally to the case where v is a categorical variable, as detailed in Appendix A.3.

Now, observe that each realization of $\tilde{x}$ can be linked to workers hired contemporaneously and to workers hired one period later. Also observe that, apart from timing conventions, this is equivalent to saying that, for every new hire, it is possible to observe $\tilde{x}$ at the time of hiring and one period before. Thus, since each wage observation in the data can be linked to the realization of $\tilde{x}$ at the time of hiring and to the realization of $\tilde{x}$ one period before the time of hiring, each observation provides both variation that can be used to infer the χ *and* variation that can be used to infer the ψ. To implement the estimation, I duplicate each observation and construct indicators $\mathbb{I}^{tenure}_{i,\tau,j}$ and $\mathbb{I}^{tenure}_{i,\tau,j-1}$ such that one of each duplicate is assigned to represent a hired-at-time-0 sample and the other to represent a hired-at-time-1 sample. That is, for the time-0 sample the indicator $\mathbb{I}^{tenure}_{i,\tau,j}$ that corresponds to the observation's tenure is one and all other $\mathbb{I}^{tenure}_{i,\tau,j}$ and all the $\mathbb{I}^{tenure}_{i,\tau,j-1}$ are zero. Conversely, for the time-1 sample the $\mathbb{I}^{tenure}_{i,\tau,j-1}$ corresponding to the observation's tenure is one and all the other $\mathbb{I}^{tenure}_{i,\tau,j-1}$ and all the $\mathbb{I}^{tenure}_{i,\tau,j}$ are zero.[12] Without further adjustment, lengthening the data in this way would artificially overstate the precision of the estimates; however, this overstatement is offset by clustering the standard errors on $i \times \tau$.

[12] The procedure for estimating equation 2.5 requires *lengthening* the data set such that it holds one observation for each individual linked to $\tilde{x}$ at the time of hiring and another observation linked to $\tilde{x}$ one period before hiring. Note, it may appear that the estimation could have been achieved using only a time-0

While standard errors are straightforward to obtain via the Delta method, the accuracy of these requires that the standard errors in the underlying Mincer regression are correct. Following the literature that estimates cyclical effects using panel data—for example, Bils (1985), Beaudry and DiNardo (1991), and Gertler, Huckfeldt and Trigari (2020)—I cluster standard errors at the person level. This clustering accounts for correlated errors within an individual across waves of the panel.[13] To address the fact that all individuals experience the same aggregate shocks at the same time, Appendix **??** reports the standard errors of all results presented herein, adjusted to cluster on individual and time of hiring. Note, clustering at the time of the shock asserts that, while we have many individuals observed for each shock, they all experience the same shock, and, thus, the unit of variation is time and not time×individual. Clustering on time returns standard errors comparable with those recovered using the two-step method. Indeed, two-step methods were proposed by Solon et al. (1994) precisely to address the correlation of time-dimension experiences across units in a panel, and, subsequently, clustering algorithms have been developed that achieve the purpose Cameron, Gelbach and Miller (2011).

Selection on worker or match quality is a threat to identification that could bias the

sample via estimating the following equation:

$$\begin{aligned}\ln(wage_{i,\tau}) = \sum_{j=0}^{\infty}\Big[&\breve{\chi}_j\big(\mathbb{I}^{tenure}_{i,\tau,j}\times\tilde{x}_{\tau-j}\big)+\breve{\chi}_{v,j}\big(v_{i,\tau}\times\mathbb{I}^{tenure}_{i,\tau,j}\times\tilde{x}_{\tau-j}\big)\\ &+\breve{\psi}_j\big(\mathbb{I}^{tenure}_{i,\tau,j}\times\tilde{x}_{\tau-j-1}\big)+\breve{\psi}_{v,j}\big(v_{i,\tau}\times\mathbb{I}^{tenure}_{i,\tau,j}\times\tilde{x}_{\tau-j-1}\big)\\ &+\breve{\zeta}_j\mathbb{I}^{tenure}_{i,\tau,j}+\breve{\zeta}_{v,j}\big(v_{i,\tau}\times\mathbb{I}^{tenure}_{i,\tau,j}\big)\Big]+\breve{\xi}v_{i,\tau}+controls_{i,\tau}\breve{\Xi}+\varepsilon_{i,\tau}.\end{aligned} \tag{2.8}$$

The difference between 2.5 and 2.8 is subtle. Regression equation 2.8 suggests *widening* the data set such that each observation is linked to the realization of $\tilde{x}$ at the time of hiring and the realization of $\tilde{x}$ one period before the time of hiring. Unlike equation 2.5, equation 2.8 conditions the $\breve{\chi}$ on the lag of $\tilde{x}_{\tau-j}$; therefore, the interpretation of these coefficients is the effect of $\tilde{x}_{\tau-j}$ on the wages of a worker with tenure j at time τ, *holding constant* the lag, $\tilde{x}_{\tau-j-1}$, at its sample mean. Similarly, the interpretation of the $\breve{\psi}$ is the effect of $\tilde{x}_{\tau-j-1}$ on the wages of a worker with tenure j at time τ, *holding constant* the lead, $\tilde{x}_{\tau-j}$, at its sample mean. The time-0 forecasting problem posited in equation 2.3 *does not* impose this conditioning. Further, if $\tilde{x}$ exhibits a high degree of autocorrelation, the restriction to hold constant a function of the lag (resp. lead) produces $\breve{\chi}s$ and $\breve{\psi}s$ that may be substantially different from the χs and ψs, resulting in a biased estimate of the differences $(\chi_j - \psi_{j-1})$ that underlie the $\varepsilon_{EWW,\tilde{x}}$. In contrast, equation 2.5 imposes the orthogonality posited in equation 2.3 and enables estimating the $\varepsilon_{EWW,\tilde{x}}$, even if the $\tilde{x}$ are highly autocorrelated.

[13]Note that the $i\times\tau$ cluster, which accounts for the lengthening of the data, is nested within, and therefore fully accounted for by, the person cluster.

estimates of the ζ, χ, and ψ and through them the semi-elasticity of the UCL and its components. To see this, note that the error term in equation 2.5 can be written as a sum of worker, match, and time components:

$$\varepsilon_{i,\tau} = \varepsilon_i + \varepsilon_m + \varepsilon_\tau, \tag{2.9}$$

where ε_i is the worker fixed effect, ε_m is a match fixed effect, and ε_τ is a transitory and mean zero shock arriving at time τ. Employed workers and workers with longer tenure may be selected on ε_i or ε_m. If, for example, higher-quality matches are selected during recessions, then the failure to account for match quality biases the estimates of cyclical sensitivity. Meanwhile, higher match quality may be more highly compensated at all lengths of tenures and may also lead to longer-lasting matches, generating dynamic selection (Altonji and Shakotko, 1987; Abraham and Farber, 1987; Topel, 1991). If dynamic selection were more pronounced in an expansion, when it is plausible that the opportunities to reallocate to matches of higher quality are more frequent, then the spurious upward slope would be more pronounced in an expansion. If workers are cyclically and dynamically selected on the same omitted variables—match quality and individual fixed effects—including suitable proxies for this variable in the *controls* addresses both margins of selection simultaneously. In other words, the proxies constitute a control function Wooldridge (2002).

Abraham and Farber (1987) suggest completed tenure with an employer as a proxy for match quality and Hagedorn and Manovskii (2013) propose a variation that weights this time, as well as the time employed prior to accession, by the labor market tightness observed during those intervals. The resulting objects are the cumulative labor market tightness during those intervals. These variables are directly observable in NLSY, they can be included in *controls* along with an individual fixed effect to implement a correction via a control function approach in that data. Further, since the variables are directly observable, no correction for generated regressors is required (Wooldridge, 2002).

Since the SIPP are short panels, the proposed proxies are not observable and, in addition, individual fixed effects may be correlated with cyclical variation that has a longer frequency than the panel length. However, if a suitable control function can be generated from the data, bias can still be purged. Altonji and Shakotko (1987) demonstrate that within-panel innovations in tenure and experience at a particular job relative to the individual within-panel means for the same job are, by construction, orthogonal to the sum of individual and match quality effects. Thus, a proxy for match quality can be constructed as the residuals of regressions of these instruments on tenure and experience, respectively. In addition, an instrumental variables approach in which the same specification used to generate this proxy is instead employed as the first stage delivers identical point estimates (Wooldridge, 2002). An added advantage is that standard errors resulting from the IV are not biased by a generated regressor and therefore need no further adjustment. The details of each approach and robustness checks can be found in Appendix A.4.

3 Data

The data used in this study come from three main sources: the National Longitudinal Survey of Youth (NLSY), the Survey of Income and Program Participation (SIPP), and the Current Population Survey (CPS). Following Basu and House (2016), all nominal-valued variables are converted to real values using the Bureau of Economic Analysis' implicit price deflator. Also following Basu and House (2016), I use a Hodrick-Prescott (HP) filter with a tuning parameter of 100,000 on quarterly data to find the deviations from trend of the national unemployment rate, which are taken as indicators of the cyclical position. Data on the official unemployment rate come from the Bureau of Labor Statistics.

I restrict all data sets to individuals with 0 to 30 years of potential experience at the time of observation whose potential experience weakly exceeds their tenure.[14] For comparability with studies by Kudlyak (2014) and Basu and House (2016), I restrict the NLSY to males

[14]The later restriction is only relevant in the cases where the start date is reported retrospectively.

only. In the other two data sets, I include both males and females.

3.1 National Longitudinal Survey of Youth: 1979 Cohort

The canonical data used in studies of the UCL is the NLSY.[15] These data are a sample of individuals who were 14 to 21 at the time of the initial survey in 1979. The panel was surveyed every year from 1979 to 1994 and every other year thereafter. Although the sample is not representative of the U.S. population, yearly cross-sectional sampling weights render the sample comparable with each year's population up to the natural aging of the sample. At the time of the survey, wage information for up to five jobs is collected, and the at wage reflects the interview date or the most recent employment date in the case of jobs no longer held. Thus, wages are observed even for short spells of employment.[16] From these data, the NLSY constructs a variable "hourly rate of pay" for each job to synchronize reporting pay intervals (hours, days, weeks, months, and years) using reported typical hours worked and earnings in a reference week. This variable includes tips, overtime pay, and bonuses before any deductions. Data also include demographics (age, education, sex, race, etc.) and job-attributes (industry, occupation, union status, etc.).

The NSLY contains a weekly diary of employment status and the reason for the transition between statuses (again, for up to five jobs per survey period) over the duration of the several decades for which respondents are observed. From these, I infer the start date of each employment relationship and use this date to match to cyclical conditions at the time of hiring.[17] Weekly employment histories and the long panel length enable addressing the

[15]Both Kudlyak (2014) and Basu and House (2016) rely on the 1979 cohort. Bils et al. (2023) expand the analysis to include the 1997 cohort.

[16]However, the shortest employment spells—ones thar result in employment in more than five jobs between survey waves—are under-observed. In the initial years of the NLSY, this means more than five jobs in a year, and in later waves more than five jobs in two years.

[17]Specifically, from the weekly diaries, I record as the job start date the date on which the worker most recently acceded into each job. This may differ from the first date of employment in said job in the case of seasonal work or if a worker returns to a previous employer after an absence. In addition, this job-start date will be distinct from current date less NLSY-constructed cumulative weeks of tenure in the match in cases where the worker has more than one spell with the same employer. If a worker holds multiple jobs, I count the worker's tenure on each job as beginning at start date of the current episode of the employment

issue of non-random selection via a control-function which includes individual fixed effects and proxies for the quality of the match in the present job, inspired by Abraham and Farber (1987). Details and robustness checks can be found in Appendix A.4.

3.2 Survey of Income and Program Participation: 1996-2008 Waves

The SIPP is an ongoing series of panels. The 1996, 2001, 2004, and 2008 panels share a common questionnaire and cover the years from 1996 to 2013. In each of these panels, the surveys are conducted once every four months and information about employment status and earnings in each of the intervening months is recorded for up to two jobs.[18] For hourly workers, the survey records the typical pay rate. For salary earners, total monthly earnings and usual weekly hours are recorded. Following Moscarini and Postel-Vinay (2017), I infer an hourly pay rate equivalent for salaried workers based on the potential weeks of work in the month, the reported number of weeks worked at all jobs in the month, and the worker's tenure for the job in question.[19] Data include demographics (age, education, sex, race, etc.) and job-attributes (industry, occupation, union status, etc.). Employment status is recorded retrospectively for each week, making it possible to observe even short spells of employment.[20] In addition, respondents are asked the start date of their current job(s).[21]

Since data are collected retrospectively, the reported start date and potential experience (age-years of education-6) are not always consistent. I restrict the sample to workers whose reported tenure is shorter than their potential experience. Studies that use SIPP wage data typically restrict samples to the observations that regard the month immediately preceding the interview, with the idea being that wages are revised infrequently and recall is best for

relationship, even if it is not the main job—so long as both jobs are continuously held.

[18]Specifically, start and end dates of employment are recorded on the calendar day, and one observation of wages per job per month is recorded. The retrospective data collection results in monthly data with varying degrees of recall bias.

[19]The number of weeks at each job is not queried.

[20]Again, as in the case of the NLSY, the shortest employment spells—ones that result in employment in more than two jobs in a four-month period—are under-observed.

[21]Respondents are prompted to respond with a calendar day but are permitted to be as vague as calendar year for jobs held longer than four months, in which case the start date is recorded as July 1.

the most recent month (Ham, Li and Shore-Sheppard, 2009). My results are insensitive to this restriction.

SIPP panels are of moderate length. In particular, panels are three to five years long and none of them spans a full business cycle. Indeed, by chance, the break points between panels nearly coincide with business cycle peaks and troughs. However, the SIPP records wage information at relatively high frequency which facilitates adjustments for match quality via the instrumental variables approach inspired by Altonji and Shakotko (1987). Details and robustness checks can be found in Appendix A.4.

3.3 Current Population Survey

The core CPS is a monthly rotating survey of approximately 40,000 households upon which official U.S. employment statistics are based. Each wave records labor force status, basic demographics, and job attributes (if an individual is employed). Following Shimer (2012) and utilizing the panel identifiers supplied by Flood, King, Rodgers, Ruggles, Warren, Backman, Chen, Cooper, Richards, Schouweiler and Westberry (2023), I link respondents over time to obtain probabilities of flow between employment, unemployment, and inactivity each month. Beginning with the 1994 survey redesign, which implemented an electronic, referenced-based questionnaire, respondents who are employed for two consecutive months may be asked if they hold the same job. Following Fallick and Fleischman (2004), I construct the monthly probability of a job-to-job transition and following Fujita, Moscarini and Postel-Vinay (2020), I adjust estimated job-to-job transition probabilities for bias due to changes in which respondents are asked the "same job" question in the later years.

While job flows can be ascertained from the core survey, it does not contain information regarding tenures of longer durations. However, in 1983, 1987, and every other year since 1996, the CPS has fielded a supplemental module of questions regarding the tenure of employed workers.[22] In the tenure supplement, workers are asked the duration during which

[22]February of 1996, 1998, and 2000 and in January in all other years. Due to the bi-yearly frequency, no

Table 1: Tenure and job continuation probabilities, by education.

	Tenure in progress			Job continuation probability (yearly)		
	NLSY	SIPP	CPS	NLSY	SIPP	CPS
Less than high school	1.80	2.82	3.30	0.50	0.40	0.33
High school or some college	3.22	4.94	5.43	0.66	0.58	0.55
College or more	4.24	6.18	6.55	0.76	0.72	0.71

Source: Current Population Survey (CPS), matched Basic, Tenure, and Earnings surveys; Survey of Income and Program Participation (SIPP); National Longitudinal Survey of Youth (NLSY); author's calculations.

Sample: Workers with 0 to 30 years of potential work experience. Males only in the NLSY (following convention). Both sexes in SIPP and CPS.

they have worked for their current main employer.[23]

4 Education and Job Stability

Here I describes on the themes in these data of particular import for the present work evaluating the magnitude of and differences in the cyclical sensitivity of the UCL and its components.[24]

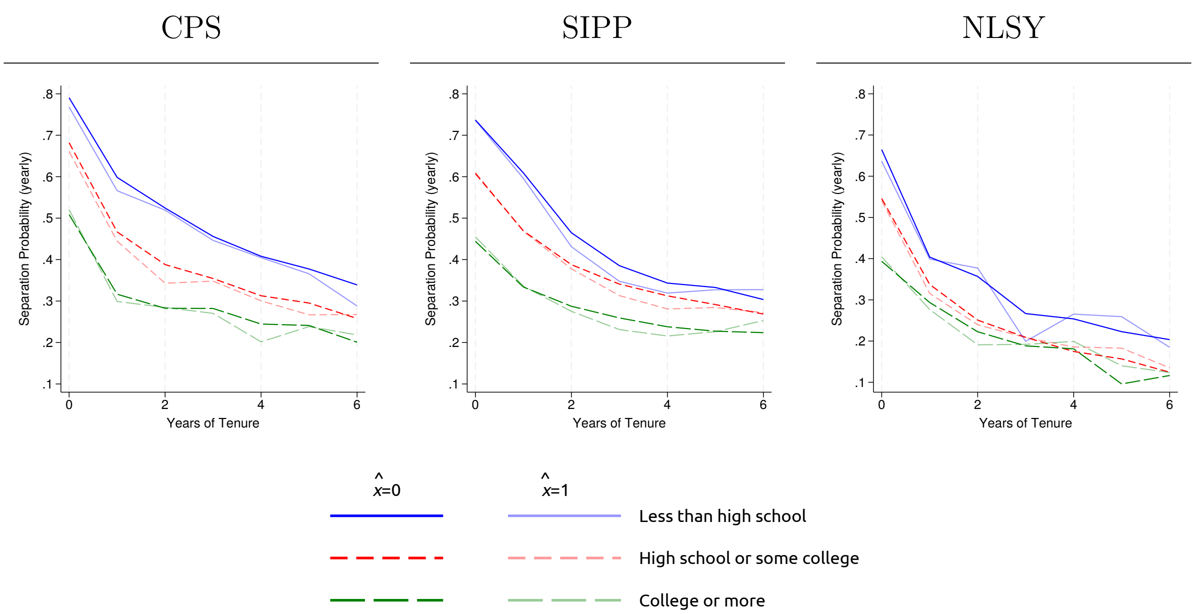
Table 1 records the average job continuation probabilities for the NLSY and the SIPP as well as the sub-sample of the CPS that can be linked to tenure information. In every data source, more highly educated workers are less likely to separate from their jobs. As a result, these workers exhibit longer tenures in progress with their current jobs. Figure 1 explores two additional margins of variation: the duration dependence and the sensitivity to the cyclical position at the time of hiring. For every level of educational attainment in each data source, the probability of separation is declining in tenure. However, conditional on tenure, separation rates remain increasing in education.

respondent responded in more than one tenure supplement.

[23]From 1996 onward, responses have been able to be reported in days, weeks, months, or years and respondents are prompted to provide job durations in months if they initially report durations shorter than three years. In 1983 and 1987, respondents reported in months for durations shorter than a year and in years thereafter.

[24]A more comprehensive evaluation of the similarities and differences across data sets is reserved for the Online Appendix.

Figure 1: Implied yearly separation probabilities by education, tenure, and cyclical position at the time of hiring.



Source: Current Population Survey (CPS), matched Basic, Tenure, and Earnings surveys; Survey of Income and Program Participation (SIPP); National Longitudinal Survey of Youth (NLSY); author's calculations.
Sample: Workers with 0-30 years of potential work experience. Males only in the NLSY (following convention). Both sexes in SIPP and CPS.
Note: Yearly probabilities for the SIPP and CPS are calculated assuming constant within-year hazards.

Finally separations and their pattern across education are largely invariant to the cyclical position at the time of hiring. Specifically, the dark shaded lines in Figure 1 record the separation probabilities for each horizon of tenure when the unemployment rate was on trend at the time of hiring, while the light shaded lines record the same when the unemployment rate is 1 percent above trend, which is approximately a one standard deviation increase.[25] Shimer (2012) documents similar insensitivity and concludes that cyclical unemployment patterns are driven by fluctuations in the job finding rate, not the separation rate.[26] In

[25]Specifically, the figures plot the coefficients from a regression of separations on indicators for educational, attainment that are fully interacted with indicators for year of tenure, and both fully interacted deviations from the HP-filtered unemployment rate at the time of hiring.

[26]The insensitivity of separations to the cyclical position, however, does not imply that workers of each level of education are equally exposed to unemployment. Rather, Cairó and Cajner (2018) document that job-finding rates are nearly identical across levels of educational attainment. This finding holds in the CPS data studied here, which extend through mid-2023. Together, these empirical regularities imply that unem-

Table 2: Semi-elasticity of the UCL, NHW, and EWW with respect to unemployment rate deviations from trend, by education.

	NLSY					SIPP				
	$\epsilon_{UCL,\tilde{x}}$	=	$\epsilon_{NHW,\tilde{x}}$	+	$\epsilon_{EWW,\tilde{x}}$	$\epsilon_{UCL,\tilde{x}}$	=	$\epsilon_{NHW,\tilde{x}}$	+	$\epsilon_{EWW,\tilde{x}}$
Less than	-0.88		-0.86		-0.02	-1.67		-1.21		-0.47
high school	*(0.87)*		*(0.74)*		*(0.69)*	*(0.69)*		*(0.52)*		*(0.25)*
High school or	-3.64		-2.27		-1.37	-2.49		-1.70		-0.79
some college	*(0.69)*		*(0.50)*		*(0.63)*	*(0.54)*		*(0.33)*		*(0.27)*
Bachelor's degree	-6.59		-3.72		-2.87	-5.98		-3.79		-2.19
or more	*(2.59)*		*(1.80)*		*(2.20)*	*(1.45)*		*(0.69)*		*(0.93)*
Observations			52,823					1,203,495		
Individuals			4,273					200,266		

Source: Survey of Income and Program Participation (SIPP); National Longitudinal Survey of Youth (NLSY); author's calculations.
Sample: Workers with 0-30 years of potential work experience with tenure not exceeding experience. Males only in the NLSY (following convention). Both sexes in SIPP.
Standard errors: Clustered on individual. Standard errors clustered at the individual and time of hiring can be found in Appendix Table D2.
F statistics: In the SIPP data, where I employ an IV strategy, the Kleibergen-Paap F statistics are 163, 2,670, and 1,792 for less than high school, high school and some college, and college or more, respectively.
Note: Appendix A.4 documents moderate variations in point estimates under alternative control functions in the NLSY and small variations when instrumenting for tenure alone in the SIPP.

Appendix A.1, I document that the bias is an order of magnitude smaller than the main effects documented in the following section and shares the same sign.

5 Education and the Cyclical Sensitivity of Wages

I now turn to documenting the heterogeneity in the sensitivity of labor costs with respect to education. Table 2 records the $\epsilon_{UCL,\tilde{x}}$ by educational attainment, along with the $\epsilon_{NHW,\tilde{x}}$ and $\epsilon_{EWW,\tilde{x}}$ obtained by estimating equation (2.5) and making post-estimation calculations (2.6) and (2.7) using the NLSY and SIPP data. I take the job continuation probabilities to be those recorded in Table 1 and the discount rate to be 0.97, following Basu and House (2016).

ployment is more cyclically sensitive for the less educated. Specifically, the elasticity of the unemployment rate with respect to the job-finding rate is increasing in the level of the separation rate, as is observed in the data. In other words, a decline in the job-finding rate yields a greater buildup in the unemployment rate for the group with the higher secular separation rate.

As noted in section 2, *controls* are broad occupation (thirteen categories), a quadratic in potential experience (age-education-6), sex, race, Hispanic origin, years of education, and calendar time. To control for possible cyclical variation match quality, I implement the control function approach in the NLSY data and the instrumental variables approach in the SIPP data. The details and applicability of these approaches are discussed Section 2 and Appendix A.4, which also presents the robustness to alternative specifications.

Two broad features emerge. First, the $\epsilon_{UCL,\tilde{x}}$ is more cyclically sensitive for more educated workers. Workers with less than high school have an $\epsilon_{UCL,\tilde{x}}$ of -0.88 and -1.67 in the NLSY and SIPP data, respectively. For more educated workers, the UCL is more elastic: approximately -3 for workers with high school or some college and -6 for workers with Bachelor's or more. A similar pattern is evident in the components of the $\epsilon_{UCL,\tilde{x}}$. The absolute value of the $\epsilon_{NHW,\tilde{x}}$ and $\epsilon_{EWW,\tilde{x}}$ both rise notably with education.

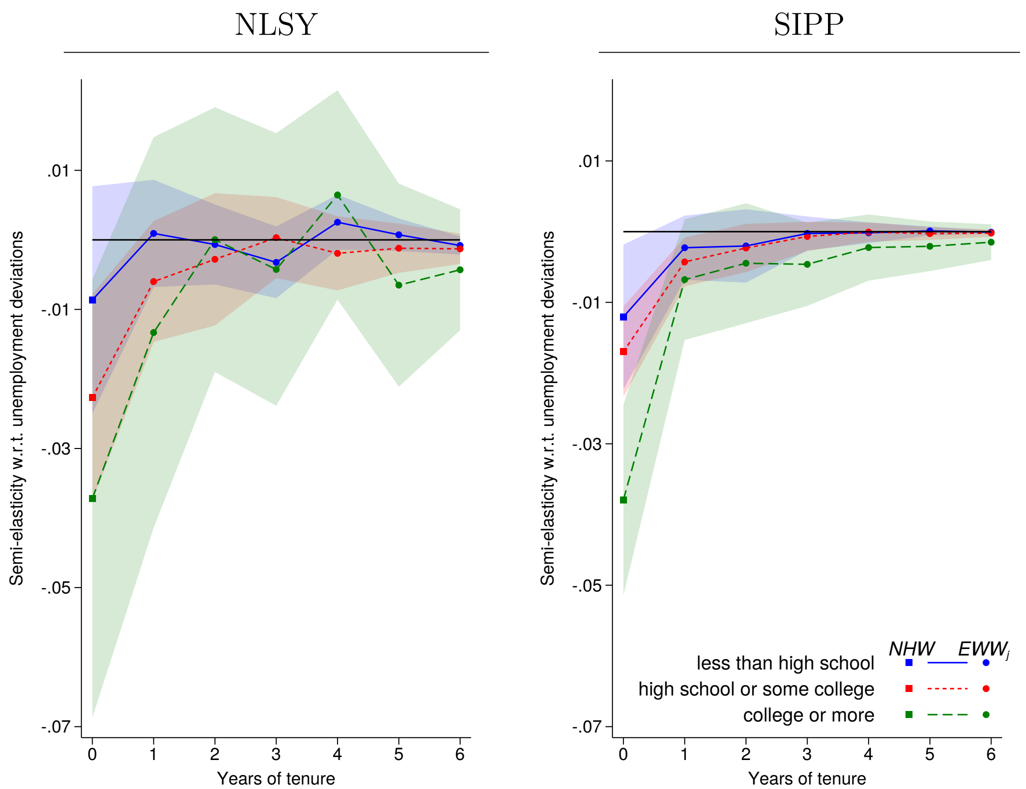
Second, the share of the $\epsilon_{UCL,\tilde{x}}$ that is accounted for by the $\epsilon_{NHW,\tilde{x}}$ is declining in education. Specifically, in the NLSY the $\epsilon_{NHW,\tilde{x}}$ accounts for 98 percent of the $\epsilon_{UCL,\tilde{x}}$ for the least educated, but this falls to 62 percent and to 56 percent as education increases. In the SIPP the corresponding values are the following: 72, 68, and 63 percent, respectively. Referring to equation (2.6), note that $\epsilon_{EWW,\tilde{x}}$ supplies the rest of the variation.

Figure 2 illustrates the semi-elasticity of NHW, marked with squares, and EWW at each horizon $j = 1...6$, marked with circles, recovered from the NLSY and SIPP data. Specifically,

$$\frac{d\ln(\widehat{NHW}_{e,j})}{d\tilde{x}} = \hat{\chi}_{e,0} \qquad \text{and} \qquad \frac{d\ln(\widehat{EWW}_{e,j})}{d\tilde{x}} = \beta^j S_e(j)\mathbf{e}^{\hat{\zeta}_{e,j}-\hat{\zeta}_{e,0}}[\hat{\chi}_{e,j} - \hat{\psi}_{e,j-1}],$$

for $e = \{$less than high school, high school or some college, Bachelor's or more$\}$ and $j = 1...6$. The shaded regions indicate 95 percent confidence intervals and are computed via the Delta method from the variance-covariance matrix of the underlying panel regressions, which are clustered on individual. Confidence intervals clustered on individual and time of hiring are illustrated in Online Appendix Figure D2. The $\epsilon_{NHW,\tilde{x}}$ corresponds to the values

Figure 2: Semi-elasticity of the NHW and EWW at each horizon j with respect to unemployment rate deviations from trend at the time of hiring, by education.



Source: Survey of Income and Program Participation; National Longitudinal Survey of Youth; author's calculations.
Sample: Workers with 0-30 years of potential work experience. Males only in the NLSY (following convention). Both sexes in SIPP.
95% confidence intervals: Clustered on individual. Confidence intervals clustered on individual and time of hiring are illustrated in Online Appendix Figure D2.

report in Table 2. The semi-elasticity of the $\epsilon_{EWW,\tilde{x}}$ reported in Table 2 is the sum of the elasticities of the EWW_j illustrated in Figure 2. Although individually statistically indistinguishable from zero, point estimates for the EWW_j are typically most negative for the most highly educated and are closest to zero for the least educated. These wedges reveal that two otherwise observationally equivalent workers hired one period apart are, on average, paid amounts that differ on account of the conditions prevailing at the time of the earlier hire. In other words, conditions at the time of the earlier hire result in a scarring effect that is different from that experienced by the later hire, and these differences persist for many subsequent time periods. Similar results have been documented elsewhere—canonically in Beaudry and DiNardo (1991)—and underlie the difference between the elasticities of the UCL and NHW documented in Kudlyak (2014) and Basu and House (2016). though they are not explicitly documented therein.

5.1 Accounting for the sources of variation

The heterogeneity in the semi-elasticities of the NHW and the EWW across educational attainment are both large. Table 3 decomposes the differences $\epsilon_{UCL,\tilde{x}}$ in the most and least educated from those with high school or some college. The differences in the $\epsilon_{EWW,\tilde{x}}$ account for roughly one-third of the differences in the $\epsilon_{UCL,\tilde{x}}$. Table 3 also documents how the differences in the $\epsilon_{EWW,\tilde{x}}$ themselves depend on differences in match survival rates, differences in the effect of shocks on wages at longer tenures, and secular differences in the return to tenure. The difference in the effect of shocks on future wages and in secular match survival rates each account for roughly one third of the excess $\epsilon_{EWW,\tilde{x}}$ experienced by those with a Bachelor's or more over those with high school or some college. By comparison, trend returns to tenure provide a very small offsetting effect, reflecting *declining* trend returns to tenure as education rises. Note that, since the $\epsilon_{EWW,\tilde{x}}$ is a non-linear function of the underlying coefficients and education is discrete, higher-order terms account for the remainder of the variation in the semi-elasticity, as detailed in equation A.7. Consistent with the small first-order effects of the differences in the return to tenure, the interaction between scarring and survival accounts for nearly all of the four remaining contributions to the difference. With all that said, estimates of the difference in the $\epsilon_{EWW,\tilde{x}}$ and its drivers are not statistically significantly different from zero.[27]

[27] The differences between those with high school or less and those with high school or some college show some similar and some different stories. Like the difference between the more educated groups, differences in secular survival probabilities are a powerful part of the story. Indeed, these differences alone over-predict the total difference in the $\epsilon_{EWW,\tilde{x}}$. Meanwhile, the first-order effect of scarring offsets—while the interaction of survival and scaring together reinforces—the effects of survival alone. The explanation is that differences in scaring at short tenures (2 to 3 years) are larger, although imprecisely estimated, for the least educated, while differences at longer tenures, also imprecisely estimated, are smaller. Note that, these estimates are

Table 3: Variation in the $\Delta\epsilon_{UCL,\tilde{x}}$, $\Delta\epsilon_{NHW,\tilde{x}}$, and $\Delta\epsilon_{EWW,\tilde{x}}$ due to differences in ...

	Less than high school − high school or some college			Bachelor's or more − high school or some college		
	$\Delta\epsilon_{UCL,\tilde{x}}$	$\Delta\epsilon_{NHW,\tilde{x}}$	$\Delta\epsilon_{EWW,\tilde{x}}$	$\Delta\epsilon_{UCL,\tilde{x}}$	$\Delta\epsilon_{NHW,\tilde{x}}$	$\Delta\epsilon_{EWW,\tilde{x}}$
	0.82	0.49	0.33	-3.49	-2.10	-1.39
	(0.88)	*(0.62)*	*(0.37)*	*(1.55)*	*(0.76)*	*(0.97)*
Contribution of:						
Scarring			-0.08			-0.54
			(0.85)			*(0.74)*
Survival			0.36			-0.44
			(0.49)			*(0.67)*
Returns to tenure			-0.00			0.02
			(0.34)			*(0.34)*
Survival and scarring jointly			0.05			-0.48
			(0.51)			*(0.53)*

Source: Survey of Income and Program Participation (SIPP); author's calculations.
Sample: Workers with 0-30 years of potential work experience.
Standard Errors: Clustered on individual. Online Appendix Table D3 documents standard errors clustered on individual and time of hiring.
F statistics: Kleibergen-Paap F statistics are 82, and 1,436 for the difference between less than high school and high school and between Bachelor's or more and high school or some college, respectively.

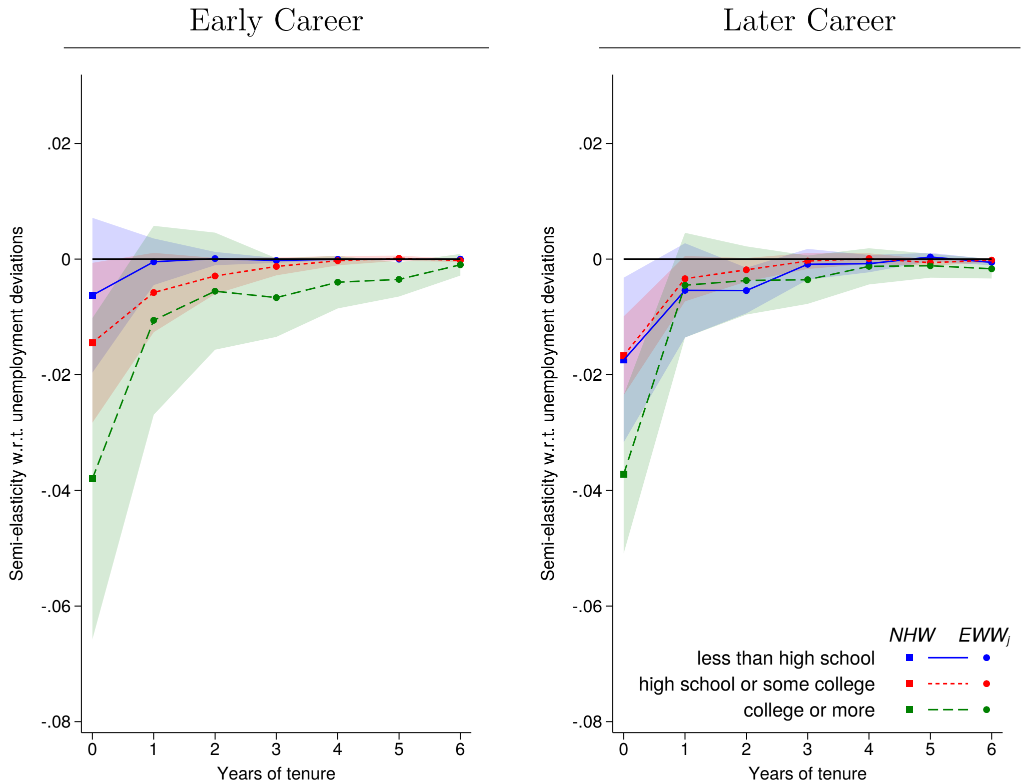
5.2 Changing effects over the career

Figure 3 plots the $\epsilon_{NHW,\tilde{x}}$ and $\epsilon_{EWW_j,\tilde{x}}$ estimated for jobs starting in the first five years after labor market entry and for jobs starting later in workers' careers.[28] The figure illustrates that the differences across education are most pronounced early in the career. Table 4 records the $\epsilon_{UCL,\tilde{x}}$ conditional on having the average potential experience of a new hire at the time of hiring and the marginal effect of experience at the time of hiring on the $\epsilon_{UCL,\tilde{x}}$ derived from a model in which all terms in equations 2.6 are interacted with the worker's experience at the time of hiring and post-estimation equation 2.7. Corroborating Figure 3, the marginal effect of potential experience at the time of hiring is negative—making the $\epsilon_{UCL,\tilde{x}}$ more negative for more-experienced workers—for the least educated but positive for the other two categories. Thus, as workers progress through their careers, the $\epsilon_{UCL,\tilde{x}}$ that are faced by each group

not statistically significantly different from zero.

[28]To be precise, the sample is split into jobs that began before the worker attained five years of potential experience and after.

Figure 3: Differences in the cyclical sensitivity of the NHW and EWW_j in early- and later-career jobs.



Source: Survey of Income and Program Participation (SIPP); author's calculations.
Sample: Workers with 0-30 years of potential work experience with tenure not exceeding potential experience.
95% confidence intervals: Clustered on individual. Confidence intervals clustered on individual and time of hiring are illustrated in Online Appendix Figure D3.
F statistics: Kleibergen-Paap F statistics for early-career jobs are 41,538, 55,059, and 79,209 for less than high school, high school and some college, and Bachelor's or more, respectively. For later-career jobs, Kleibergen-Paap F statistics are 537, 2,707, and 1,418 for less than high school, high school and some college, and Bachelor's or more, respectively.

becomes more similar.

Table 5 reveals that the convergence for the least educated stems from the increasing sensitivity of wages, particularly those of new hires', and dramatically increasing survival probabilities as experience at the time of hiring increases. For this group, survival probabilities increase by nearly 1 percent per year of potential experience at the time of hiring. Meanwhile, for the most educated, convergence stems from the declining sensitivity of new hires' wages and the expected wage wedge as well as a flattening return to tenure. The

Table 4: Variation in the semi-elasticity of the UCL due to variation in v=*experience at the time of hiring.*

	$\left.\frac{dln(UCL)}{d\tilde{x}}\right\rvert_{v=\mathbb{E}[v]}$	$\frac{d^2ln(UCL)}{d\tilde{x}dv}$	$\mathbb{E}[v]$
less than	-1.843	-0.081	7.45
high school	*(0.675)*	*(0.045)*	
high school	-2.449	0.054	10.21
or some college	*(0.544)*	*(0.041)*	
Bachelor's	-5.905	0.069	11.00
or more	*(1.340)*	*(0.137)*	

Source: Survey of Income and Program Participation (SIPP); author's calculations.
Sample: Workers with 0-30 years of potential work experience with tenure not exceeding potential experience.
Standard errors: Clustered on individual. Online Appendix Table D4 documents standard errors clustered on individual and time of hiring.
F statistics: Kleibergen-Paap F statistics are 128, 695, and 1,345 for less than high school, high school and some college, and Bachelor's or more, respectively.

differences over the career partially explain the differences in estimates obtained from the NLSY and the SIPP. The former contains a single cohort that faced a particularly large contraction earlier in their careers, while the latter contains many cohorts.

The increasing durability of matches and the increasing sensitivity of wages to shocks suggest (at least) two possibilities. On the one hand, workers could be increasingly well matched to their jobs as their careers progress. On the other, the relative value of non-market activities (e.g. additional schooling) may decline. In Appendix A.4, I document that estimates of the $\epsilon_{UCL,\tilde{x}}$ are broadly insensitive to alternative approaches to controlling for match quality, suggesting that the variation in match quality over the career is not the prevailing difference. Meanwhile, in the same appendix, I document that the least educated enjoy the greatest returns to tenure.[29] This suggests that as careers progress, the value of employment rises relative to non-market activities, and that this may be most relevant to the least educated.

[29] Note that, the first-order effect of experience is not identified in the NLSY as it is co-linear with the combination of the time trend and individual fixed effect.

Table 5: Attributing variation in the semi-elasticity of the UCL due to variation in v=*experience at the time of hiring* to its sources.

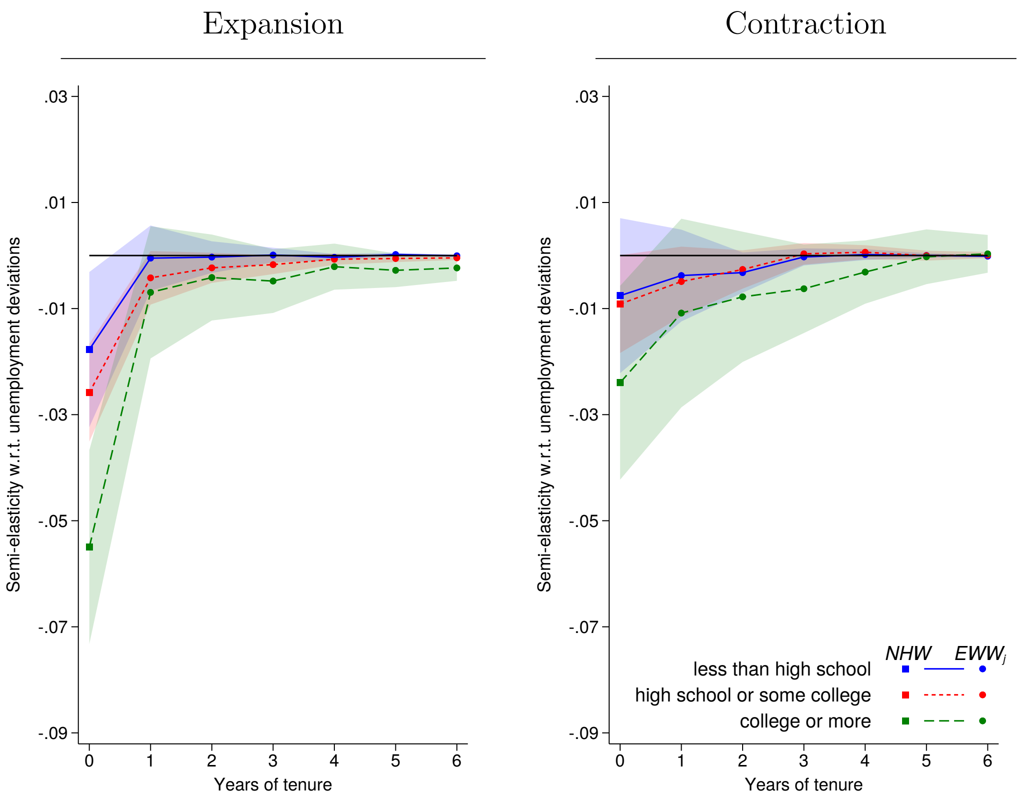
			Contributions of differences in:		
	$\frac{d^2 ln(NHW)}{d\tilde{x}dv}$	$\frac{d^2 ln(EWW)}{d\tilde{x}dv}$	Scarring	Survival	Trend return to tenure
Less than	-0.027	-0.054	-0.027	-0.027	-0.000
high school	*(0.035)*	*(0.024)*	*(0.023)*	*(0.018)*	*(0.000)*
High school	0.029	0.025	0.038	-0.013	0.000
or some college	*(0.025)*	*(0.026)*	*(0.028)*	*(0.006)*	*(0.000)*
Bachelor's	0.032	0.037	0.034	0.000	0.003
or more	*(0.059)*	*(0.103)*	*(0.103)*	*(0.000)*	*(0.002)*

Source: Survey of Income and Program Participation (SIPP); author's calculations.
Sample: Workers with 0-30 years of potential work experience with tenure not exceeding potential experience.
Standard errors: Clustered on individual. Online Appendix Table D5 documents standard errors clustered on individual and time of hiring.
F statistics: Kleibergen-Paap F statistics are 128, 695, and 1,345 for less than high school, high school and some college, and Bachelor's or more, respectively.

5.3 Asymmetry

Table 4 documents that the semi-elasticity of the UCL with respect to the unemployment rate is larger in an expansion than a contraction, substantially so for the more educated. The difference derives in large part from a much larger effect of the unemployment rate on the NHW during an expansion. For the least and most educated, this difference is partially offset by a *larger* sensitivity of the EWW in contractions than expansions, while for those with high school or some college, the sensitivity of the EWW is largely independent of the phase of the business cycle. The pattern is illustrated in Figure 4 which shows that while the NHW sensitivity is smaller in contractions, the sensitivities of the EWW_j are larger in the subsequent few years. These facts together suggest that employers may face a constraint on cutting wages for new hires but offset this constraint, in part, by withholding returns to tenure to a somewhat greater degree for employees hired during contractions. This pattern of wage behavior is suggested by the one-sided commitment implicit contract model of Thomas and Worrall (1988) and the downward nominal wage rigidity model of Elsby (2009). That

Figure 4: Differences in the cyclical sensitivity of the NHW and EWW_j in expansions and contractions.



Source: Survey of Income and Program Participation (SIPP); author's calculations.
Sample: Workers with 0-30 years of potential work experience with tenure not exceeding potential experience.
95% confidence intervals: Clustered on individual. Confidence intervals clustered on individual and time of hiring are illustrated in Online Appendix Figure D4.
F statistics: Kleibergen-Paap F statistics are 102, 1,796, and 1,193 for less than high school, high school and some college, and Bachelor's or more, respectively.

said, the differences in the UCL achieved through the EWW channel are small and do little to mitigate the asymmetry imposed by the asymmetric response of the NHW.

Is the asymmetry mechanical and indicative of a failure of Assumption 1? Specifically, if $\tilde{x}$ has been falling, it is plausible that $\mathbb{E}[\tilde{x}_1|\tilde{x}_0 = 0] < 0$, and visa versa if $\tilde{x}$ has been rising. In this case, the expansion estimates are biased away from zero relative to the true $\epsilon_{UCL,\tilde{x}}$, and the contraction estimates are biased toward zero. Interestingly, given the relative magnitudes of the $\epsilon_{NHW,\tilde{x}}$ and $\epsilon_{EWW_j,\tilde{x}}$ in each phase, this alternative assumption implies that the $\epsilon_{NHW,\tilde{x}}$ offsets *more* of the difference in the $\epsilon_{NHW,\tilde{x}}$ across the phases of the cycle.

Thus, under the alternative assumption, the behaviors of the UCL and its components fall more clearly in line with the predictions of the aforementioned contracting models.

6 Conclusion

I document heterogeneity with respect to education in job stability and the flexibility of allocative wages as measured by the UCL. These margins offset each other, with the less educated facing less-stable employment but more-stable wages, conditional on employment. The flexibility of the UCL exceeds that of the NHW because of the persistent differences between the wages of workers hired when unemployment is above trend relative to their equivalent replacements who might be hired a period later. Since more educated workers have greater job stability, their wages are more sensitive to these wedges. In addition, the differences in the wages of workers and their equivalent replacements that are attributable to the conditions at the time of hiring are larger and more persistent for the more educated, further amplifying differences in allocative wages across groups.

In service of documenting this evidence, I develop novel methods for estimating the (semi-)elasticity of labor costs that accounts for the possibility of inter-temporal distortion in remitted wages. These methods enable new approaches to address the problem of possible confounding variation. The methods also expand the types of data suitable for investigating the cyclical sensitivity of the UCL; here, I employ the SIPP in addition to the more-traditionally used NLSY.

Using the SIPP enables me to document two additional facts. First, the heterogeneity in the cyclical sensitivity of the UCL is particularly acute early on in workers' careers, when the differences in job stability are also most acute. Later on in their careers, workers of all levels of educational attainment exhibit sensitivity to the cycle. Second, the sensitivity of the UCL to the unemployment rate depends on the phase of the business cycle, with greater sensitivity occurring during expansions. In other words, the UCL appears to exhibit downward rigidity. That said, phases of the cycle impact the components of the UCL differently: the differences in the sensitivity of the EWW somewhat offset the differences in the sensitivity of the NHW. As such, documenting downward rigidity in the remitted wages of new hires may overstate its extent.

I conclude by detailing how methods derived in this paper facilitate the study of the sensitivity of the UCL to shocks that are at high frequency or that have crosssectional variation. To fix ideas, assume 1) wages are observed at a monthly frequency, 2) shocks arrive at a daily frequency, and 3) tenure is measured to the day. Assumptions 1 and 2 imply that we can associate a worker to the shock that arrived on the day of their hiring. If we further assume linearity in the marginal effect of the shock on the UCL and that a large enough sample of workers is hired each day, then one can trace out the effect of the shock by modifying the first step of the two-step procedure such that the block of indicators estimated in that step is granular to the day of hiring rather than the year. This increases the number of parameters that need to be estimated by a factor of $365 \times T$. In contrast, one can trace out the effect of the shock on wages by allowing j to take the daily frequency in equation (2.5) in the one-step method. This procedure increases the number of parameters that need to be estimated by a factor of only 365.

Turning to cross-sectional variation, suppose one wants to know the effect of unionization on the cost of labor, for example. One could apply the two-step method in a difference-in-differences setup by dividing the sample into firms with a unionization event and a match sample of control firms. One would then estimate the first step in both sub-samples, construct the change in the UCL around the unionization event (or placebo event times in the control firms) and compare them. This requires assigning firms definitively to one group or the other. Suppose instead one wanted to instrument for unionization. The first stage of one's instrumental variables strategy returns a probability that a firm unionizes, which is not a binary categorization. This makes it impossible to split the sample into two parts ex-ante. However, instrumenting for $\tilde{x} = \textit{unionization}$ within the one-step procedure is straightforward. Furthermore, the one-step procedure preserves the covariance structure in the crosssection, which are required for inference in this example.

7 Data Availability Statement

The data and code underlying this research is available on Zenodo at https://doi.org/10.5281/zenodo.17793305.

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A Appendix

A.1 Cyclical variation in separation rates

For clarity, it is useful to impose duration independence: $S^*(j) = (1 - s^*)^j$, where s^* is the per-period separation rate when the economy is on trend. In this case, the final line of equation 2.3 becomes the following:

$$\Upsilon_v = \left[\frac{ds_{v,1}}{d\tilde{x_0}}^* - \frac{ds_{v,0}}{d\tilde{x_0}}^* \right] \times \sum_{j=1}^{\infty} \beta^j \frac{w^*_{v,j}}{w^*_{v,0}} (1 - s^*_v)^{j-1} (j-1), \tag{A.1}$$

where $\frac{ds_{v,0}}{d\tilde{x_0}}^*$ and $\frac{ds_{v,1}}{d\tilde{x_0}}^*$ are the effects on the separation rate for a v-type worker of having been hired at the time $\tilde{x}$ was one percentage point above trend and having been hired one period after that, respectively.

Table A1: Bias due to cyclical variation in separation rates.

	$\frac{ds_{v,1}}{d\tilde{x}_0}^* - \frac{ds_{v,0}}{d\tilde{x}_0}^*$	$\Upsilon_v \times 100$
Less than high school	-0.0044	-0.46
	(0.0027)	(0.29)
High school or some college	-0.0030	-0.82
	(0.0862)	(23.31)
Bachelor's or more	-0.0003	-0.16
	(0.0993)	(53.41)

Source: Survey of Income and Program Participation (SIPP); author's calculations.
Standard Errors: Clustered on individual.
Note: Standard errors impose the assumption that separation rates and their dependence on $\tilde{x}$ are measured without error.

I can obtain estimates $\frac{ds_{v,0}}{d\tilde{x}_0}^*$ and $\frac{ds_{v,1}}{d\tilde{x}_0}^*$, respectively, by regressing an indicator for job separation for person i at time τ on education interacted and indicator for and the state at the time hiring for the worker who now has j periods of tenure, $\mathbb{I}_{tenure=j} \times x_{\tau-j}$, and and indicator for and the state one period before hiring for the prototypical equivalent replacement who now has $j-1$ periods of tenure, $\mathbb{I}_{tenure=j-1} \times x_{\tau-j}$. Specifically,

$$\mathbb{P}(Separation_{\tau,i}) = \sum_{e=1}^{E} \omega_{v,0} \times \mathbb{I}_{v_{i,\tau}=e} \times \mathbb{I}_{tenure=j} \times \tilde{x}_{\tau-j} + \omega_{v,1} \times \mathbb{I}_{v_{i,\tau}=e} \times \mathbb{I}_{tenure=j-1} \times \tilde{x}_{\tau-j}$$
$$+ controls_{i,\tau,j}\Xi_{i,j} + \varepsilon_{i,\tau}$$

yields estimates $\hat{\omega}_{v,0}$ and $\hat{\omega}_{v,0}$ of $\frac{ds_{v,0}}{d\tilde{x}_0}^*$ and $\frac{ds_{v,1}}{d\tilde{x}_0}^*$, respectively. The coefficients, standard errors, and implied bias are summarized in Table A1.

The effects of the cyclical position at the time of hiring on the difference in the separation probabilities of the newly hired cohort, $\frac{ds_{v,0}}{d\tilde{x}_0}^*$, and its equivalent replacement, $\frac{ds_{v,1}}{d\tilde{x}_0}^*$, are two orders of magnitude smaller than the average separation probabilities, the complements of which is recorded as the job-continuation probability in Table 1. Applying these wedges over the expected course of an employment relationship implies biases in the $\epsilon_{UCL,\tilde{x}}$—i.e. magnitudes of the Υ_v—that are an order of magnitude smaller than the headline effects recorded in Table 2. Note that the implied biases are multiplied by 100 in order to be comparable with the statistics reported in Table 2. In addition, the implied bias shares the sign of the estimated $\epsilon_{UCL,\tilde{x}}$. Thus, omitting this effect attenuates all estimates toward zero.

A.2 Comparison to two-step procedure

The methods proposed herein differ from Kudlyak (2014), which I summarize here and henceforth refer to as the two-step procedure. Kudlyak (2014) proposes to first estimate the following:

$$\ln(wage^{i}_{t,t+j}) = \Phi X^{ij}_{t+j} + \sum_{d_0=1}^{T} \sum_{d=d_0+1}^{T+7} \chi_{d_0,d} D^{ij}_{d_0,d} + \varepsilon^{ij}_{t+j}, \tag{A.2}$$

where t is the time of hiring, $t+j$ is the time of the wage observation, and $D^{ij}_{d_0,d}$ is a set of indicators equal to one if the worker was hired on date d_0 and wages are observed on date d using panel data. With the obtained estimates, construct UCL_t as the following:

$$\widehat{UCL}_t = \sum_{j=0}^{7} \left[\beta^j (1-s)^j \mathbf{e}^{\widehat{\chi}_{t,t+j}}\right] - \sum_{j=1}^{6} \left[\beta^j (1-s)^j \mathbf{e}^{\widehat{\chi}_{t,t+j}}\right], \tag{A.3}$$

where $\hat{\chi}$ is the predicted value implied by the coefficients obtained in regression A.2. Note that in cases in which t is not observed every year, as in the later years of the NLSY, this procedure requires interpolating $\hat{w}_{t,t+j}$ for $t+j \in \{1995, 1997, ..., 2011\}$, typically via a polynomial spline.[30] By comparison, the one-step method implicitly interpolates via a spline constructed from the statistical model 2.5, which is simultaneously being used to infer the elasticities that compose the UCL. Finally, an estimate of the semi-elasticity of the UCL to deviations from the HP-filtered unemployment rate is obtained as γ from the following regression:

$$\ln(\widehat{UCL}_t) = \alpha + \gamma\tilde{x} + \iota t + \varepsilon_t. \tag{A.4}$$

[30]The somewhat large estimate of the UCL obtained by Basu and House (2016) is attributable to an error in the implementation of this spline. Correcting the error brings their estimate to 5.5 (1.12), with the Newey-West standard error in parentheses.

Table A2: Estimates in the literature (using two-step procedures).

	Kudlyak (2014)	Basu and House (2016)	Bils et al. (2023)
Semi-elasticity of the UCL with respect to unemployment rate deviations from trend[a]	-5.2	-5.8	-4.8
	(0.8)	(2.1)	(1.8)

[a] Trend inferred using an HP filter with a bandwidth of 100,000.

Table A3: Comparison of one-step and two-step procedures.

	One-step[b]	Two-step[c]	Synthetic[d]
Semi-elasticity of the UCL with respect to unemployment rate deviations from trend[a]	-4.8	-5.2	-5.0
	(1.3)	(1.2)	—

[a] Trend inferred using an HP filter with bandwidth 100,000 as in Basu and House (2016).
[b] Estimate obtained from equation 2.5 and post-estimation calculation 2.6. Observations weighted according to the NLSY sample weights. Standard errors clustered on year of observation and individual.
[c] Estimate obtained via the two-step procedure. Standard errors are Newey-West robust with a single lag.
[d] Point estimate obtained by estimating equation 2.5 on synthetic $\hat{w}_{t,t+\tau}$ generated from the coefficients obtained in regression A.2.
Source: National Longitudinal Survey of Youth (NLSY); author's calculations.
Sample: Males (following convention) with 0-30 years of potential work experience.

Standard errors are computed from a bootstrapping procedure in Kudlyak (2014) and are Newey-West robust in Basu and House (2016) and Bils et al. (2023). Table A2 documents the range of estimates of the $\epsilon_{UCL,\tilde{x}}$ obtained from the two-step procedure. For comparability, I record the estimates obtained without controls for selection. Following the literature, I report estimates multiplied by a factor of 100.

To validate the methods proposed herein, I compare the estimates of the semi-elasticity of the UCL to deviations of the unemployment rate from trend obtained in the single-step procedure to those obtained in the two-step procedure and to estimates from the literature. The results are present in the first two columns of Table A3. Exploiting the single- and two-step method in the NLSY sample used in this paper reveals a coefficients in line with the literature. The estimates are economically similar and statistically identical.[31] The third

[31] While the estimates obtained from the two procedures in the same data are statistically the same, they are not identical. Given that the interpolation implemented to obtain a time-series of the UCL in the two-step procedure is independent of the estimates of the cyclical sensitivity of the estimated wage-tenure profile, the single- and two-step procedures are not guaranteed to be identical even if both equations A.2 and

column of Table A3 applies the one-step procedure to a synthetic data set derived from the coefficients obtained from estimating equation A.2. Specifically, I estimate equation A.2 and then construct a synthetic data set in which all variables in X are held constant at their means (as in the two-step procedure); I then estimate 2.5 on the synthetic data. This procedure reveals a point estimate between the single- and two-step procedures.

A.3 Categorical types

This empirical model can be adapted to the case where v is categorical in the usual way:

$$\begin{aligned}
\ln(wage_{i,\tau}) = {} & \chi_0 \times \mathbb{I}^{tenure}_{i,\tau,0} \times \mathbb{I}_{d=1} \times \tilde{x}_\tau + \sum_{e=1}^{E}\Big[\chi_{0e} \times \mathbb{I}_{v_{i,\tau}=e} \times \mathbb{I}^{tenure}_{i,\tau,0}\mathbb{I}_{d=1} \times \tilde{x}_\tau\Big] \\
& + \sum_{j=1}^{\infty}\Big[\zeta_j \times \mathbb{I}^{tenure}_{i,\tau,j} + \sum_{e=1}^{E}\Big[\zeta_{e,j} \times \mathbb{I}_{v_{i,\tau}=e} \times \mathbb{I}^{tenure}_{i,\tau,j}\Big] \\
& \quad + \chi_j \times \mathbb{I}^{tenure}_{i,\tau,j} \times \mathbb{I}_{d=1} \times \tilde{x}_{\tau-j} + \sum_{e=1}^{E}\Big[\chi_{e,j} \times \mathbb{I}_{v_{i,\tau}=e} \times \mathbb{I}^{tenure}_{i,\tau,j} \times \mathbb{I}_{d=1} \times \tilde{x}_{\tau-j}\Big] \\
& \quad + \psi_{j-1} \times \mathbb{I}^{tenure}_{i,\tau,j-1} \times \mathbb{I}_{d=0} \times \tilde{x}_{\tau-j} + \sum_{e=1}^{E}\Big[\psi_{e,j-1} \times \mathbb{I}_{v_{i,\tau}=e} \times \mathbb{I}^{tenure}_{i,\tau,j-1} \times \mathbb{I}_{d=0} \times \tilde{x}_{\tau-j}\Big]\Big] \\
& \quad + \sum_{e=1}^{E}\Big[\xi_e \mathbb{I}_{v_{i,\tau}=e}\Big] + controls_{i,\tau,j}\Xi_{i,j} + \varepsilon_{i,\tau},
\end{aligned} \tag{A.5}$$

where $\mathbb{I}_{v_{i,\tau}=e}$ is an indicator equal to one if individual i is in category e at time τ, $S(j)$ is the survival probability of the (arbitrary) base category, and $S_e(j)$ is the survival probability of the e category. As before, ζ_0 is normalized to one and, given the inclusion of the ξ_e, the $\zeta_{0,e}$ are all zero.

In this case the semi-elasticity of the UCL to $\tilde{x}$ for type $e = 0$ case remains as in equation

2.5 are correctly specified and applied to identical data. As discussed above, interpolation in the two-step procedure imposes a structure on the data that is independent of the estimating equation. Separately, model-misspecification—in particular failure to account for all relevant covariates (observed and unobserved)—will impose differences between the covariance between shocks and wages recovered from the actual data and those recovered from synthetic data in which all other observed covariates are held constant at their means.

2.6. And the cyclical sensitivity of the e-type is the following:

$$\frac{d\ln(\widehat{UCL_{v=e}})}{d\tilde{x}} = \hat{\chi}_0 + \hat{\chi}_{e,0} + \sum_{j=1}^{\infty} \mathbf{e}^{\hat{\zeta}_j+\hat{\zeta}_{e,j}-\hat{\zeta}_0-\zeta_{e,0}}\beta^j S_e^*(j)\left(\hat{\chi}_j + \hat{\chi}_{e,j} - \hat{\psi}_{j-1} - \hat{\psi}_{e,j-1}\right). \quad \text{(A.6)}$$

This implies a difference in marginal effects between the 0-type and e-type worker of

$$\begin{aligned}
\frac{d\ln(\widehat{UCL_{v=e}})}{d\tilde{x}} - \frac{d\ln(\widehat{UCL_{v=0}})}{d\tilde{x}} &= \hat{\chi}_0 + \hat{\chi}_{e,0} + \sum_{j=1}^{\infty}\beta^j \mathbf{e}^{\hat{\zeta}_j+\hat{\zeta}_{e,j}-\hat{\zeta}_0-\zeta_{e,0}} S_e^*(j)\left(\hat{\chi}_j + \hat{\chi}_{e,j} - \hat{\psi}_{j-1} - \hat{\psi}_{e,j-1}\right) \\
&\quad - \hat{\chi}_0 + \sum_{j=1}^{\infty}\beta^j \mathbf{e}^{\hat{\zeta}_j-\hat{\zeta}_0} S^*(j)\left(\hat{\chi}_j - \hat{\psi}_{j-1}\right) \\
= \overbrace{\hat{\chi}_{e,0}}^{\substack{\text{difference in}\\ \epsilon_{NHW,\tilde{x}}}} &+ \overbrace{\sum_{j=1}^{\infty}\beta^j\Big[\mathbf{e}^{\hat{\zeta}_j-\hat{\zeta}_0}S^*(j)\left(\hat{\chi}_{e,j} - \hat{\psi}_{e,j-1}\right) \quad \leftarrow \substack{\text{contribution of differences}\\ \text{in scarring alone}}}^{\text{difference in } \epsilon_{EWW,\tilde{x}}} \qquad \text{(A.7)} \\
&+ \mathbf{e}^{\hat{\zeta}_j-\hat{\zeta}_0}\left(S_e^*(j) - S^*(j)\right)\left(\hat{\chi}_j - \hat{\psi}_{j-1}\right) \quad \leftarrow \substack{\text{contribution of differences}\\ \text{in survival alone}} \\
&+ \left(\mathbf{e}^{\hat{\zeta}_j+\hat{\zeta}_{e,j}-\hat{\zeta}_0-\zeta_{e,0}} - \mathbf{e}^{\hat{\zeta}_j-\hat{\zeta}_0}\right) S^*(j)\left(\hat{\chi}_j - \hat{\psi}_{j-1}\right) \quad \leftarrow \substack{\text{contribution of differences}\\ \text{in trend returns to tenure alone}} \\
&+ \mathbf{e}^{\hat{\zeta}_j-\hat{\zeta}_0}\left(S_e^*(j) - S^*(j)\right)\left(\hat{\chi}_{e,j} - \hat{\psi}_{e,j-1}\right) \quad \leftarrow \substack{\text{interaction between differences in}\\ \text{scarring and survival}} \\
&+ \left(\mathbf{e}^{\hat{\zeta}_j+\hat{\zeta}_{e,j}-\hat{\zeta}_0-\zeta_{e,0}} - \mathbf{e}^{\hat{\zeta}_j-\hat{\zeta}_0}\right)\left(S_e^*(j) - S^*(j)\right)\left(\hat{\chi}_j - \hat{\psi}_{j-1}\right) \leftarrow \substack{\text{interaction between differences in}\\ \text{returns to tenure and survival}} \\
&+ \left(\mathbf{e}^{\hat{\zeta}_j+\hat{\zeta}_{e,j}-\hat{\zeta}_0-\zeta_{e,0}} - \mathbf{e}^{\hat{\zeta}_j-\hat{\zeta}_0}\right) S_0^*(j)\left(\hat{\chi}_{e,j} - \hat{\psi}_{e,j-1}\right) \quad \leftarrow \substack{\text{interaction between differences in}\\ \text{returns to tenure and scarring}} \\
&+ \left(\mathbf{e}^{\hat{\zeta}_j+\hat{\zeta}_{e,j}-\hat{\zeta}_0-\zeta_{e,0}} - \mathbf{e}^{\hat{\zeta}_j-\hat{\zeta}_0}\right)\left(S_e^*(j) - S^*(j)\right)\left(\hat{\chi}_{e,j} - \hat{\psi}_{e,j-1}\right)\Big]. \leftarrow \substack{\text{interaction between}\\ \text{differences in all sources}}
\end{aligned}$$

The first three rows capture the first-order effects that coincide with the case of continuous types. The second three rows correspond to second-order effects and the final row to a third-order effect. These terms are present since the EWW is non-linear and therefore in the discrete case heterogeneity in the marginal effects is not fully captured by the first-order terms.

A.4 Selection, Match Quality, and Model Specification

Selection on worker or match quality is a threat to identification that could bias estimates of the ζ, χ, and ψ and, through them, the semi-elasticity of the UCL and its components.

To see this, note that the error term in equation 2.5 can be written as a sum of the worker, match, and time components

$$\varepsilon_{i,\tau} = \varepsilon_i + \varepsilon_m + \varepsilon_\tau, \tag{A.8}$$

where ε_i is the worker fixed effect, ε_m is a match fixed effect, and ε_τ is a transitory and mean-zero shock. Two possible solutions are to control for a control function that provides a well-measured proxy for $\varepsilon_i + \varepsilon_m$ and to take an instrumental variables approach that the isolates variation in the components of the sensitivity of the UCL from $\varepsilon_i + \varepsilon_m$.

In the NLSY, I apply controls for the interaction between the business cycle, pre-job experience, and ultimately completed tenure—which proxy for ε_m—and individual fixed effects—which proxy for ε_i. Specifically, column (1) of Table A4 presents estimates that include only individual fixed effects. Following Abraham and Farber (1987), Hagedorn and Manovskii (2013), and Bellou and Kaymak (2021), columns (2) through (4) control in various ways for ultimately completed tenure on the current job and continuously employed pre-job experience.[32] Note, all of these variables can be observed directly from the data; therefore, standard errors follow directly and do not require adjustment for generated regressors (Wooldridge, 2002). Table A4 reports the semi-elasticity of the UCL and NHW under a variety of specifications of the control function, by education.

Columns (1) of Tables A4 estimate the semi-elasticity of the UCL and its components, controlling for demographics, a time trend, industry, and individual fixed effects. Columns (2) add controls for selection: completed tenure with the current employer and continuous prior employment. In other words, columns (2) assume that the relation between match quality and the proxies included as controls are cyclically invariant but allows workers to be cyclcially selected into employment and into higher tenures on the basis of their match quality/these proxies.

Columns (3) and (4) allow for cyclical variation in the covariation between the proxy and

[32]Note: Bellou and Kaymak (2021) point out that the measure proposed by Hagedorn and Manovskii (2013) is the sum of the logs of these variables. Here I apply linear forms, and in a previous draft I have demonstrated robustness to the functional form of these controls.

Table A4: Specification of the control function (NLSY).

	Less than high school				High school or some college				Bachelor's or more			
	(1)	(2)	**(3)**	(4)	(1)	(2)	**(3)**	(4)	(1)	(2)	**(3)**	(4)
Semi-elasticity	-0.011	-0.012	**-0.009**	-0.015	-0.041	-0.037	**-0.036**	-0.045	-0.087	-0.072	**-0.066**	-0.082
of the UCL	*0.009*	*0.009*	***0.009***	*0.010*	*0.007*	*0.007*	***0.007***	*0.008*	*0.027*	*0.027*	***0.026***	*0.030*
Semi-elasticity	-0.012	-0.011	**-0.009**	-0.011	-0.025	-0.023	**-0.023**	-0.022	-0.045	-0.043	**-0.037**	-0.040
of the NHW	*0.007*	*0.008*	***0.007***	*0.007*	*0.005*	*0.005*	***0.005***	*0.005*	*0.018*	*0.018*	***0.018***	*0.018*
Return to 5 years	0.181	0.170	**0.167**	0.169	0.184	0.138	**0.136**	0.137	0.142	0.137	**0.133**	0.135
of tenure	*0.026*	*0.029*	***0.028***	*0.028*	*0.013*	*0.012*	***0.013***	*0.013*	*0.024*	*0.025*	***0.024***	*0.024*
Return to 5 years	0.143	0.104	**0.105**	0.104	0.020	-0.029	**-0.028**	-0.030	0.028	-0.023	**-0.025**	-0.023
of experience	*0.139*	*0.142*	***0.141***	*0.141*	*0.031*	*0.032*	***0.031***	*0.031*	*0.067*	*0.064*	***0.065***	*0.065*
Completed tenure	.	0.009	**0.008**	0.009	.	0.029	**0.029**	0.030	.	0.041	**0.039**	0.041
	.	*0.004*	***0.004***	*0.004*	.	*0.003*	***0.003***	*0.003*	.	*0.008*	***0.008***	*0.008*
$\times\mathbb{I}_{d=0} \times \hat{x}_{\tau-j}$	.	.	.	0.002	.	.	.	0.003	.	.	.	0.011
	.	.	.	*0.003*	.	.	.	*0.002*	.	.	.	*0.005*
$\times\mathbb{I}_{d=1} \times \hat{x}_{\tau-j}$	.	.	.	0.000	.	.	.	-0.001	.	.	.	-0.008
	.	.	.	*0.001*	.	.	.	*0.001*	.	.	.	*0.004*
Continuous prior	.	0.039	**0.033**	0.039	.	0.056	**0.049**	0.056	.	0.099	**0.084**	0.099
employment	.	*0.008*	***0.008***	*0.008*	.	*0.005*	***0.005***	*0.005*	.	*0.014*	***0.016***	*0.014*
$\times\mathbb{I}_{d=0} \times \hat{x}_{\tau-j}$	.	.	.	0.001	.	.	.	0.006	.	.	.	0.008
	.	.	.	*0.006*	.	.	.	*0.004*	.	.	.	*0.007*
$\times\mathbb{I}_{d=1} \times \hat{x}_{\tau-j}$	.	.	.	-0.001	.	.	.	-0.006	.	.	.	-0.004
	.	.	.	*0.003*	.	.	.	*0.002*	.	.	.	*0.005*
Hired from	.	.	**0.022**	.	.	.	**-0.039**	.	.	.	**0.003**	.
unemployment	.	.	***0.041***	.	.	.	***0.020***	.	.	.	***0.059***	.
Average market tightness during:												
completed	.	.	**0.028**	.	.	.	**-0.005**	.	.	.	**0.029**	.
tenure	.	.	***0.019***	.	.	.	***0.010***	.	.	.	***0.029***	.
continuous prior	.	.	**-0.004**	.	.	.	**0.002**	.	.	.	**0.000**	.
employment	.	.	***0.006***	.	.	.	***0.002***	.	.	.	***0.003***	.
R-squared	0.213	0.223	**0.223**	0.225	0.384	0.401	**0.401**	0.402	0.514	0.528	**0.528**	0.529

Source: National Longitudinal Survey of Youth; author's calculations.

Sample: Males with 0-30 years of potential work experience with tenure not exceeding experience.

Standard errors: Clustered at the individual. Online Appendix Table DA4 documents standard errors clustered on individual and time of hiring.

Table A5: Specification of the first stage (SIPP).

	Less than high school				High school or some college				Bachelor's or more			
	(1)	(2)	**(3)**	(4)	(1)	(2)	**(3)**	(4)	(1)	(2)	**(3)**	(4)
Semi-elasticity	0.001	-0.018	**-0.017**	0.000	-0.010	-0.023	**-0.025**	-0.026	-0.039	-0.061	**-0.060**	-0.067
of the UCL	*0.004*	*0.007*	***0.007***	*0.014*	*0.003*	*0.005*	***0.005***	*0.007*	*0.008*	*0.014*	***0.015***	*0.017*
Semi-elasticity	0.003	-0.013	**-0.012**	0.000	-0.009	-0.016	**-0.017**	-0.018	-0.029	-0.038	**-0.038**	-0.044
of the NHW	*0.003*	*0.005*	***0.005***	*0.009*	*0.002*	*0.003*	***0.003***	*0.004*	*0.004*	*0.007*	***0.007***	*0.008*
Return to 5 years	0.144	0.073	**0.107**	0.111	0.177	0.064	**0.087**	0.091	0.172	-0.013	**0.047**	0.052
of tenure	*0.010*	*0.014*	***0.013***	*0.013*	*0.004*	*0.005*	***0.005***	*0.005*	*0.007*	*0.010*	***0.009***	*0.010*
Return to 5 years	0.062	0.075	**0.031**	0.030	0.066	0.086	**0.051**	0.053	0.068	0.107	**0.029**	0.031
of experience	*0.002*	*0.003*	***0.010***	*0.010*	*0.001*	*0.002*	***0.004***	*0.004*	*0.002*	*0.004*	***0.008***	*0.008*
Potential	0.015	0.017	**0.008**	0.008	0.017	0.021	**0.012**	0.012	0.020	0.027	**0.009**	0.009
Experience	*0.000*	*0.001*	***0.002***	*0.002*	*0.000*	*0.000*	***0.001***	*0.001*	*0.000*	*0.001*	***0.002***	*0.002*
$\times\mathbb{I}_{d=0} \times \hat{x}_{\tau-j}$	.	.	.	-0.035	.	.	.	-0.004	.	.	.	-0.032
	.	.	.	*0.029*	.	.	.	*0.011*	.	.	.	*0.016*
$\times\mathbb{I}_{d=1} \times \hat{x}_{\tau-j}$	.	.	.	0.000	.	.	.	0.000	.	.	.	0.001
	.	.	.	*0.001*	.	.	.	*0.000*	.	.	.	*0.001*
(Potential	0.000	0.000	**0.000**	0.000	-0.001	-0.001	**0.000**	0.000	-0.001	-0.001	**-0.001**	-0.001
Experience)^2	*0.000*	*0.000*	***0.000***	*0.000*	*0.000*	*0.000*	***0.000***	*0.000*	*0.000*	*0.000*	***0.000***	*0.000*
$\times\mathbb{I}_{d=0} \times \hat{x}_{\tau-j}$	.	.	.	0.023	.	.	.	-0.008	.	.	.	-0.008
	.	.	.	*0.029*	.	.	.	*0.009*	.	.	.	*0.016*
$\times\mathbb{I}_{d=1} \times \hat{x}_{\tau-j}$	.	.	.	-0.002	.	.	.	0.000	.	.	.	0.000
	.	.	.	*0.001*	.	.	.	*0.000*	.	.	.	*0.001*
R-squared:	0.255	0.105	**0.091**	0.085	0.286	0.140	**0.139**	0.136	0.204	0.077	**0.080**	0.077
Kleibergen-Paap rk												
Wald F statistic:	.	41,868	**163**	5,615	.	8,745	**2,671**	3,264	.	884	**1,792**	7,917
Instrumented:												
Tenure	.	X	**X**	X	.	X	**X**	X	.	X	**X**	X
Experience	.	.	**X**	X	.	.	**X**	X	.	.	**X**	X

Source: Survey of Income and Program Participation; author's calculations.

Sample: Workers with 0-30 years of potential work experience with tenure not exceeding experience.

Standard errors: Clustered at the individual. Online Appendix Table DA5 documents standard errors clustered on individual and time of hiring.

the omitted variable. Specifically, the specification in columns (3) controls for the average market tightness during completed tenure and during continuous prior employment. Thus, it posits that selection occurs at a different pace when the market is tighter. Columns (4) posit an alternative: that selection depends on the conditions at the time of hiring. These two specifications bracket those recovered in columns (2). This could be because the correlation between match quality and the proxies depends on the cycle; that cyclical selection, to which (4) speaks most directly is counter-cyclical; and dynamic selection, to which (3) speaks most directly is pro-cyclical. However, this interpretation is possibly confounded. Specifically, it could also be due to the heterogeneity in marginal effects with respect to potential experience documented in section 5.2.

Turning to the SIPP, I could also take a control function approach. However, in this case would have to generate the control from the data rather than observing it directly as in the NLSY. Assuming that the same omitted variable—match quality—is the source of both cyclical and dynamic selection, I appeal to the instrument for match quality studied in Altonji and Shakotko's 1987 study of the return to tenure. Altonji and Shakotko (1987) demonstrate that $\tilde{X} = X - \mathbb{E}[X]$, where $\mathbb{E}[X]$ is the average observation of X within the observed portion of an employment spell provides a valid instrument for X=*tenure* and X=*experience* or functions of these variables. Thus, I could generate proxies for match quality as the *residuals* from a regression of tenure on these instruments, controlling for the other covariates in my intended main regression. This procedure yields equivalent point estimates to two stage least squares but standard errors would need to be adjusted for the generated regressor (Wooldridge, 2002). Thus, I implement the IV approach. This approach is appropriate to the SIPP in which workers are observed for three to five years, making controls needed for the control function approach unobservable, but in which wages are observed every four months, making it possible to construct strong instruments even when jobs are relatively short.

Columns (3) of Table A5 are equivalent specifications to columns (2) of Table A4.

Columns (4) of Table A5 perform an analogue to columns (4) of Table A4 and, similar to the NLSY, return a coefficient that is larger than under the assumption of a cyclically invariant relationship between the endogenous component of tenure and match quality.[33]

Now let me return to the comparison of Table A4's columns (2), (3), and (4) to columns, in light of Table A5's columns (3) and (4). In both data, specifications (4) suggest that allowing for cyclical variation between match quality its proxy implies a more procyclical user cost. However, specification (4) is sensitive to the demeaning of prior experience and completed tenure in the NLSY and potential experience in the SIPP.[34] Further, this sensitivity is consistent with heterogeneity in the marginal effects with respect to potential experience, which is documented in Section 5.2. Therefore, I do not consider these regressions to be robust and do not present them as the headline findings.

These observations suggests that I should present columns (2) of both Table A4 and Table A5, as the headline findings, since they are analogous specifications. However, since Table A4 columns (3) recovers less cyclical sensitivity and the explanation of a cyclically varying relation between the match quality and the available proxies can not be ruled out, I take a conservative position and report Table A4 columns (3) as the headline for the NLSY. This suggests that the SIPP results may overstate the cyclical sensitivity. Note that in all specifications, the NHW and UCL remain more pro-cyclcical for the more educated, which is the punchline of this paper.

[33]Note, I include columns (1) for comparison with the CPS and columns (2) because they record the specification reported in a previous draft.

[34]Note, in the previously submitted draft I demeaned the NLSY such that these were zero for the average employee, here I demean such that they are zero for the average new higher. This explains the change in magnitude of the coefficients report in the Appendix tables included in that draft and here.