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The Ways the Cookie Crumbles: Education and the Margins of Cyclical Adjustment in the Labor Market

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Abstract

Less educated workers experience higher and more cyclically sensitive job separation rates. Meanwhile, workers with a bachelor's degree or more exhibit pro-cyclical wages and workers without a high school degree exhibit no statistically discernible cyclical pattern. Differences in the sensitivity are most stark when measurement of labor costs accounts for the persistent effects of current macroeconomic conditions on future remitted wages. These findings suggest optimally differential implementation of self-enforcing implicit wage contracts in which educated workers and their employers leverage relative employment stability to smooth the effects of cyclical fluctuations over longer horizons. This margin of adjustment is less available to the less well educated, who have shorter expected employment durations. Furthermore, failure to account for the heterogeneities documented here leads to substantial underestimation of the welfare costs of business cycles.

JEL Classifications:
E24: Employment • Unemployment • Wages
J31: Wage Differentials • Education Based • Tenure Earning
J63: Turnover
J41: Labor Contracts • Implicit Contracts
M52: Compensation Methods and Their Effects
E52: Monetary Policy • Policy Effects

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

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1 Introduction

Increasingly, macroeconomists and policy makers are concerned with heterogeneity. From the macroeconomists' perspective, differences in how economic agents respond to shocks help explain their amplitude and propagation. From a policy perspective, acknowledging heterogeneity facilitates addressing the needs of those most vulnerable to shocks. The need to acknowledge heterogeneity is particularly salient in light of the K-shaped impact of the 2020 Covid-19 pandemic: white collar workers appear to be insulated by the ability to work from home while blue collar workers, particularly in service industries, appear to be exposed. By shedding light on education-specific differences in the response of wages to cyclical fluctuations that have pertained in the United States for decades, this work adds perspective to the employment-centric view. In particular, I find that past recessions have left scarring effects on wages that are concentrated among the educated.

There are large secular differences in employment stability across education, with workers with less than a high school degree being more twice as likely to separate from a job as workers with a Bachelors degree or more, regardless of cyclical position. In addition, greater cyclical variation in employment for the less educated is frequently and readily documented.[1] These differences, while stark, may provide an incomplete story. This paper asks whether the cyclical sensitivity of wages increases or decreases with education, what are the factors driving differences, and what are the welfare consequences.

The first contribution of this paper is to document heterogeneity in the cyclical sensitivity of wages with respect to education that is opposite to the pattern observed in employment stability. Specifically, the wages of the more educated are *more* sensitive to cyclical conditions and macroeconomic shocks than those of their less educated counterparts.[2] The results here suggest that changes in labor market composition over the business cycle are a consequence both of variation in displacements and of cost minimization that takes into account the fact that wages are differentially sensitive to shocks. Thus, in addition to the welfare costs of the sullying effect of recessions on output via match quality, further welfare loss is due to the distortion of relative wages following shocks.

Second, the heterogeneity I document is most stark in a forward-looking view of the (implicit) wage contract that allows for the possibility that macroeconomic shocks have a persistent impact on wages that exceeds the persistence of the shocks themselves. Evidence

[1] A non-exhaustive list of contributions: Mukoyama and Sahin (2006); Cairó and Cajner (2018); Aaronson et al. (2019). Mueller (2017) offers a novel counterpoint in this literature.

[2] These facts speak to the literature on cyclical sorting and the cleansing versus the sullying effects of recessions: Barlevy (2002), Hagedorn and Manovskii (2013), Kahn and McEntarfer (2014), Haltiwanger et al. (2015), Cairó et al. (2016), Abel and Deitz (2016), Haltiwanger et al. (2018), and Crane et al. (2018).

of implicit contracts has been documented as early as 1991 by Beaudry and DiNardo (1991).[3] This paper follows the formalization of these facts into a notion of labor costs—a user cost of labor (UCL)—introduced by Kudlyak (2014) and promoted by Basu and House (2016). With this evidence in mind, I note that assessing the impact of any current shock using only data on contemporaneous employment and contemporaneously remitted wages—the wages paid in a given interval of time—understates the true economic impact by omitting the long term consequences for wages, particularly for the highly educated.

In light of the facts regarding employment stability, the cause of the starker differences across education in the UCL as compared to the new hires' wages (NHW) offers additional insight into the use and design of forward-looking labor contracts. In particular, I document that more educated workers' UCL is particularly sensitive because, for this group, past macroeconomic conditions have particularly persistent effects on remitted wages. The differences in persistence suggest that relative employment stability for the more educated encourages labor contracts that smooth shocks over longer horizons, effectively rendering any near-term rigidities —even if faced in common by workers of all educational attainments —less binding for the more educated. These differences are predicted by Thomas and Worrall (1988) who show that a longer planning horizon, due to a lower discount or separation rate, increase the use of deferred payment in the optimal wage contract.[4]

Third, I consider the welfare consequences of the documented heterogeneity in a parsimonious model. I find that the welfare loss that is overlooked by ignoring heterogeneity is *substantial*. Further, my result is likely a very conservative lower bound due to a simplifying assumption: I allow households to pool consumption risk and as a result my welfare loss derives labor market missalocations alone.[5] It is well known that admitting idiosyncratic consumption risk greatly reduces tractability; however, intuition suggests that the costs associated with idiosyncratic consumption risk disproportionately burden the low skilled since, as I document here, these workers face income loss on an extensive rather than intensive margin.[6] Further, my results, which suggest increasing implementation of implicit contracting increasing education, suggest that highly educated workers differentially use such contracting

[3] A related pair of literatures document the persistent effects of the cyclical position at the time of college graduation—for example Kahn (2010); Oreopoulos et al. (2012)—and of job displacement—for example Davis and Wachter (2011).

[4] An exploration of the facts presented here in the framework of Thomas and Worrall (1988) couched in a full fledged macroeconomic model, à la Rudanko (2009), is warranted but reserve for future work.

[5] While the assumption is useful for parsimony there is compelling evidence to the contrary, for example the recent evidence documented in Coibion et al. (2017) that monetary policy shocks induce inequitable consumption responses.

[6] Indeed, in a model that includes differential employment risk and idiosyncratic consumption risk. Krusell et al. (2009) find welfare losses an order of magnitude larger than in the Lucas (1987) framework, which is employed in the present paper.

to self-insure against idiosyncratic consumption streams.

In service of this analysis, I develop a new method for estimating the sensitivity of the UCL to the business cycle and to macroeconomic shocks. The intuition for the new method lies in recognizing that cyclical sensitivity in the UCL is a function of the cyclical sensitivity of the wage-tenure profile. I show that an estimate of the cyclical sensitivity of the UCL can be recovered from the coefficients on the interaction between a flexible function of tenure and the cyclical position at the time of hiring estimated within an augmented Mincer (1974) regression. The estimator is more efficient—requiring the identification of fewer parameters—and more transparent, particularly with respect to the construction of standard error and incorporation of controls and covariates than the existing method developed in Kudlyak (2014) and implemented in Basu and House (2016). In addition, the new method admits higher frequency identification, which facilitates, among other things, the nonparametric identification of the impulse response to macroeconomic shocks via a variant of the Jordà (2005) local projection method. Finally, I am able to obtain estimates from common and previously thought to be unsuitable data types.

My baseline specification estimates a cyclical sensitivity of the UCL to a one percent deviation of the unemployment rate from trend of almost 16 percent for workers with college degrees or more, while workers who did not complete high school exhibit no statistically discernable pattern. This apparently huge effect is, in fact, in line with back of the envelope estimates obtained by compounding the effects reported in Kahn (2010) and Oreopoulos et al. (2012) (for Canadian college graduates). However, my results starkly suggest that those estimates are not externally valid to workers with less education. Indeed, I document that less educated workers' wages are acyclical and that they, instead, face labor market adjustment on the extensive margin.

I provide a lower bound for the welfare consequences that are overlooked by ignoring the heterogeneity documented in this paper. In a parsimonious model with price and wage rigidities and homogenous workers, shocks induce misallocation in the labor market because wage adjustment takes time (Galí et al., 2007). In the heterogeneous worker version, misallocation is compounded by distortions in the relative price of labor types because, as I have documented, wage adjustments are not homogenous. I find that ignoring distortions in relative labor costs leads to understating the macroeconomic costs of fluctuations in this type of model by 15 percent and that the costs are borne by the least educated, whose welfare losses due to fluctuations are 15 times larger than the most educated.

The methods introduced in this paper facilitate novel robustness checks. First, I show that cyclical variation in match quality, even when allowed to impact measurement of the cyclical sensitivity of the wage-tenure profile, accounts for only a small minority of the variation documented in this paper. Second, I document differential sensitivity to aggregate demand shocks—as measured by monetary policy shocks—using the non-parametric estimator proposed by Jordà (2005). This analysis supports the main conclusions of this paper—that more highly educated workers' wages are more sensitive—and illustrates the improvement upon the art of estimation in this literature, which previously relied on interpolation methods (Basu and House, 2016). Finally, I apply the methods proposed here to data types previously thought to be unsuitable—specifically a repeated cross-section—and document that my main findings are recoverable even with data of inferior quality.

The next section briefly introduces the data used in this paper. Section 3 documents secular and cyclical differences in employment stability by education. Section 4 introduces my new method for estimating the cyclical sensitivity of the forward-looking view of the wage contract. The section concludes with a discussion of data requirements and a broad overview of the data used. Section 5 documents my headline results regarding the cyclical sensitivity of average hourly earnings, new hires' wages, and the user cost of wages and its components. I also document an analogous pattern of heterogeneity with respect to monetary policy shocks and that the main findings are borne out in alternative data sources. Section 6 places the results in a *very* simple model in order to bound the welfare consequences of the documented heterogeneity. Even in this bounding exercise the welfare consequences are large. Finally, Section 7 concludes. Detailed discussion of the data and standard empirical techniques; additional robustness checks; and details of the welfare analysis are relegated to the appendix.

2 Data

This paper uses data from the National Longitudinal Survey of Youth (1979) (NLSY) and the Current Population Survey (CPS).[7] Each data source has pros and cons and I highlight the main features of each data source here. Appendix A provides further details of the data sources and data preparation.

2.1 NLSY

The NLSY is a nationally representative sample of individuals who were young adults in 1978. These individuals are tracked over time and their employment status and wages are recorded yearly before 1994 and biannually after. In addition, the survey collects information on employment status and employer for the intervening period. The intervening labor market

[7] Replication of main results using the Survey of Income and Program Participation available upon request.

histories enable identification of current and completed tenure with each employer as well as the most recent accession date, the date of the most recent spell of unemployment.

The NLSY data provide comparability to existing studies of the UCL (Kudlyak, 2014; Basu and House, 2016). In addition, the detailed job histories can be used to address concerns about cyclical sorting (Hagedorn and Manovskii, 2013). The largest drawback to the NLSY is that it follows a single cohort that is reaching a mature age.

2.2 CPS

The CPS is a nationally representative sample of households from which the official unemployment rate is constructed. Households in the CPS are surveyed eight times in two blocks of four consecutive months eight months apart. Individuals can be linked over time, enabling observation of flows between employment, unemployment, and non-participation in the labor force. From 1995 onward, these data can be used to infer rates of job-to-job transitions (Fallick and Fleischman, 2004; Fujita et al., 2020).[8] In the fifth and eight survey respondents report weekly earnings. Every other year, starting in 1996, the CPS Job Tenure and Occupational Mobility Supplement collects data on respondents' original start date with their current employer.[9] These data are sufficient to estimate the cyclical sensitivity of the UCL.[10]

The CPS provides comparability to existing studies of the differences in employment volatility across educational attainment. The main disadvantages are limitations on the data on job separations and job-to-job mobility and the short length of the panel component. These issues require treating the data as repeated cross-section when estimating the UCL. Since individual fixed effects can not be included in the controls estimates are more reliant on control variables. In addition to concern about cyclical selection on observable, limited information about past and future work histories make controls for cyclical variation in unobserved match quality infeasible.

[8]From 1994 to 2007 the CPS recorded weather or not each observed individual was still employed in the job observed during the previous survey. Starting 2007 the "same job" question is only asked differently depending on whether the respondent is the same household member in serial waves, leading to bias (Fujita et al., 2020). I follow the imputation method of Fujita et al. (2020) when presenting the separation rates by education. Because less educated workers tend to live in larger households the bias induced by the change in the reference based survey following 2007 is more severe for this group.

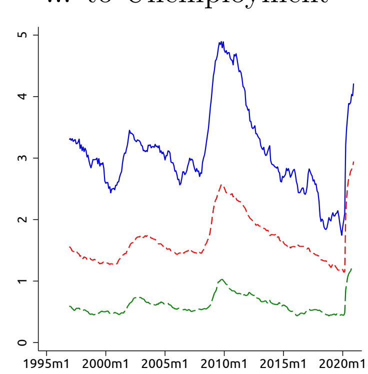
[9]Tenure data is available intermittently in the 1980s. Inclusion of these does not change the main results.

[10]Flaws in the "same job" question, discussed in the previous footnote, mean that it is not possible to infer tenure in the non-tenure-supplement years; thus, while earnings are observed twice per respondent, tenure can be observed reliably at most once.

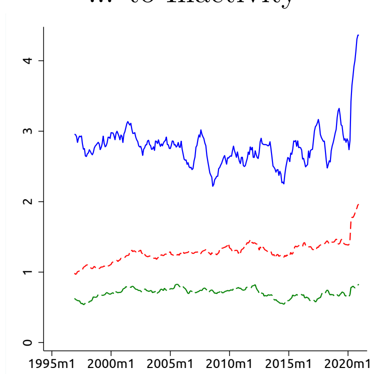
Figure 1: Job Separations by Education.

Panel A: Rate of Separations

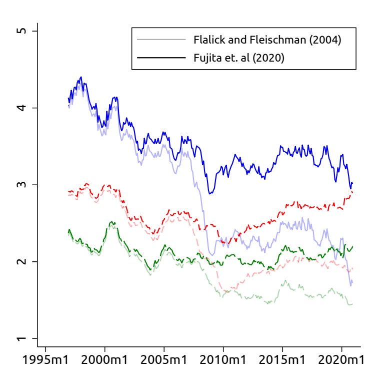
Panel B:
... to Unemployment



Panel C:
... to Inactivity



Panel D:
... to Another Job



Source: Current Population Survey Basic Monthly files; author's calculations.
Note: 12 month trailing averages. Employment to unemployment and to inactivity transitions identified as in Shimer (2012) using the individual identifiers supplied in Flood, King, Rodgers, Ruggles, and Warren (Flood et al.). Job-to-job transitions imputed as in Fujita et al. (2020) (bottom right illustrates the difference between this and Fallick and Fleischman (2004)). Sample restricted to males with 0-30 years of potential experience.

3 Inequitably Volatile Employment

Using data on employment to non-employment transitions and on job-to-job mobility inferred from the 1995 to 2020 CPS, I construct the monthly probability of job separation. Separations are the sum of transitions from employment to unemployment (EU) or inactivity (EI) and job-to-job transitions (JTJ). Monthly EU and EI rates are constructed by matching the CPS month-over-month as in Shimer (2012). JTJ transitions are inferred from answers to the "Same Job" question, which entered the survey with the 1994 reference-based survey redesign. From 2007 on , the reference-based survey changed interview protocols and as a result I impute JTJ transitions following the method proposed by Fujita et al. (2020). Because less educated workers live in larger households, on average, the bias introduced by the change in interviewing protocols studied by Fujita et al. (2020) is larger for this group. Following the literature both on the user cost of labor (e.g. Kudlyak (2014) and Basu and House (2016)) and on cyclical differences in employment volatility (e.g. Cairó and Cajner (2018)), I restrict the sample to men with 0-30 years of potential work experience.[11] Further, in order to avoid misclassifying workers' educational attainment, I exclude workers who report being in school in any of the eight months in which they are observed.

Figure 1 and Table 1 document that more-educated workers have lower and less volatile separation rates than their more educated counterparts. Figure 1 Panel A illustrates the separations series by education and Panels B through D illustrate the components EU, EI, and JTJ by education. Similarly, Table 1 records the average and absolute volatility of the separation rate and its components by education. Both the figure and table clearly document that more-educated workers have lower separation rates than do their less educated counterparts. Indeed, workers with less than a high school degree are more than twice as likely to separate from their job than workers with a Bachelors degree or more regardless of cyclical position. Declining separation rates as education rises holds for all separations combined and individually for each component of separations.[12]

Table 1 also documents that the separation rate of less educated workers is more volatile at business cycle frequencies than that of the more educated. This extends the findings of Mukoyama and Sahin (2006); Cairó and Cajner (2018) and Aaronson et al. (2019), which focus on the unemployment rates and separations into unemployment.

[11] Potential experience is defined in the usual way as $age - years\ of\ schooling - 6$. Including women yields higher separation rates and lower cyclical sensitivity of the UCL for all education groups, but maintains the main findings: separations are most common and most volatile for the least educated and cyclical sensitivity of the UCL is largest for the most educated. Including workers with greater experience also increases measured separation rates and mutes the cyclicality of the UCL but maintains the ordering of both across educational attainment.

[12] This analysis is closely related to that of Cairó and Cajner (2018). The main difference is that, with an eye toward understanding forward looking implicit wage contracts, this analysis is made inclusive of all possible separations while Cairó and Cajner (2018) focus only on EU separations.

Table 1: Job Separations by Education.

	Less than High School		High School or Some College		Bachelors or more	
	Mean	Volatility	Mean	Volatility	Mean	Volatility
Total	9.4	1.7	5.6	1.0	3.4	0.6
to Unemployment	3.1	1.3	1.7	0.8	0.6	0.3
to Inactivity	2.8	0.7	1.3	0.3	0.7	0.2
to Another Job	3.5	0.7	2.6	0.4	2.1	0.3

Source: Current Population Survey Basic Monthly files and author's calculations.
Note: Employment to unemployment and to Inactivity transitions identified as in Shimer (2012) using the individual identifiers supplied in Flood, King, Rodgers, Ruggles, and Warren (Flood et al.). Job-to-job transitions imputed as in Fujita et al. (2020). Sample restricted to males with 0-30 years of potential experience. Volatility reports the standard deviation of the time series.

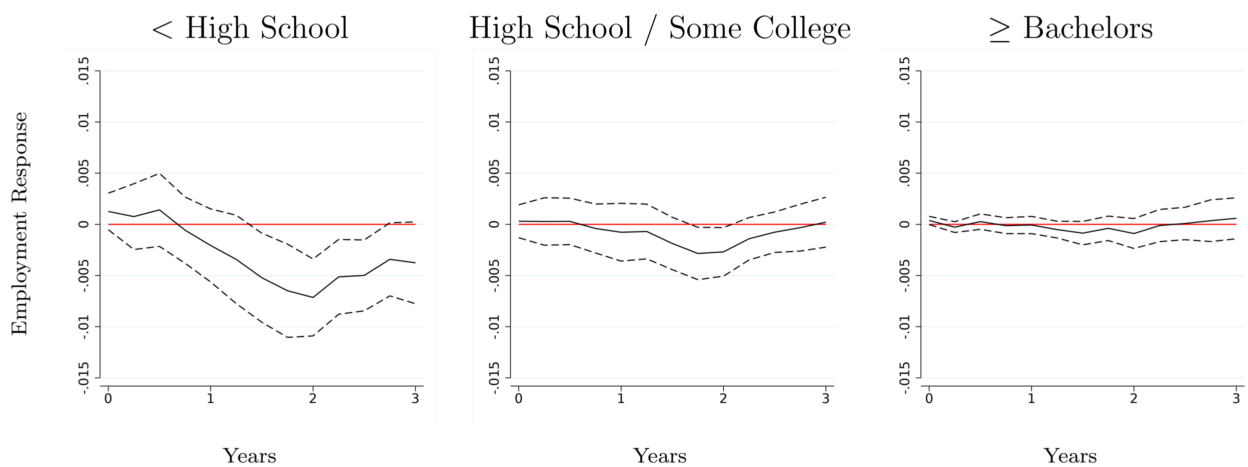
Cyclical differences could be due to differences in the cyclicality of labor demand in industries or occupations that primarily employ less versus more educated workers.[13] A way to address this hypothesis using only data employment is to expose all workers equally to an aggregate demand shock—for instance, a monetary policy shock. Using Romer and Romer (1999) shocks and the Jordà (2005) local projection method, I document in Figure 2 that the employment of workers with less than a high school education falls in response to a monetary policy shock, whereas employment of workers with a college education or more is unaffected. This outcome provides evidence that variation in the composition of employment with respect to employment stems, at least in part, from either differences in labor supply elasticities across education or distortions in relative wages.

While the differences in volatility are of clear import, these within-education differences over time and with the cycle are small in comparison to the secular between-education differences. Indeed, the increase in separations from the business cycle peak of late 2019 to the 2020 trough experienced by the least educated, while huge, is still smaller than the secular difference in separations between the least and most educated. Large secular differences in separation rates suggest a hypothesis that lower steady-state separation rates among the more highly educated make the persistence in the effect of shocks on wages a more important component in compensation and effectively render the UCL less rigid for the more highly educated.

To see the intuition clearly, it is useful to state and manipulate the basic expression for

[13]This hypothesis would suggest that the least educated workers' wages are also more cyclically sensitive. In section 5, I document that the opposite is the case.

Figure 2: Impulse Response to a 100 Basis Point Monetary Policy Contraction, Employment by Education



Source: Current Population Survey Basic Monthly files; Federal Reserve Board Greenbooks as cleaned by Coibion (2012); author's calculations.
Note: Monetary policy shocks are identified as in Romer and Romer (2004). Impulse responses control for four lags of the monetary policy shock, the left-hand-side variable, and the aggregate unemployment rate. The 95 percent confidence intervals are constructed with Newey-West standard errors.

the UCL. The UCL can be written as

$$UCL_t = \mathbb{E}_t \left[PDV_t - \beta(1-s)PDV_{t+1} \right] \tag{3.1}$$

where β is the discount factor, s is the exogenous separation rate, PDV_t is the present discounted value of wage payments in an employment relationship starting at date t, and $\mathbb{E}_t$ is the time-t expectations operator. This formulation is due to Kudlyak (2014). Manipulating yields,

$$UCL_t = \underbrace{w_{t,t}}_{New\,Hire's\,Wage} + \underbrace{\mathbb{E}_t \sum_{j=1}^{\infty} \left[\beta^j (1-s)^j (w_{t+j,t} - w_{t+j,t+1}) \right]}_{Expected\,Wage\,Wedge}, \tag{3.2}$$

where $w_{t,t+j}$ is the wage paid at date $t+j$ to a worker hired on date t. This formulation of the UCL illustrates its decomposition into the new hire's wage (NHW) and the expected wage wedge (EWW). Not only do deferred payments carry more weight because of the lower separation probabilities but also more-educated workers and their employers may strategically defer payments in the face of rigidities that bind in the near term but relax over time. Such strategic contracting would make near-term constraints functionally less binding. In addition, such strategies would make the employment relationships of the more educated more resilient and thus contribute to the observed lower volatility of employment and the lower sensitivity to aggregate shocks documented earlier.

Indeed, in Section 5, I document large differences across educational attainment in the cyclicality of wages and their sensitivity to aggregate demand shocks (as measured by monetary policy shocks). In addition, I show evidence that these differences are strategic and not simply mechanical functions of exposure to the EWW. Specifically, wage cuts are much more persistent for the more highly educated.

4 Estimating the Sensitivity of Wages

The wage is a notoriously difficult macroeconomic object to measure. Not only is there substantial qualitative and quantitative divergence between the various measures put forth, but there is also disagreement about which measure is substantively correct. Kudlyak (2014) and Basu and House (2016) argue that the appropriate measure of allocative wage to consider from the macroeconomic perspective is the UCL. This measure takes into account, but does not impose, the possibility that labor market frictions impart a durable quality to an employment relationship and that, as a result, the sequence of payments under a(n implicit) wage contract might diverge from the sequence of wages that would arise in a spot market.

A drawback is that the existing methodology for estimating the UCL and its sensitivity to business cycle conditions and monetary policy shocks (due to Kudlyak, 2014; Basu and House, 2016) follows an inefficient multistep procedure. This existing methodology for recovering the cyclicality of the UCL relies on (1) estimating coefficients on a very large set of indicators, which capture the return to having been hired on a specific past date given employment on a particular current date, and (2) using these coefficients to construct the time series of the UCL and then analyzing the properties of the resulting time-series. This strategy makes cross-sectional disaggregation and high-frequency measurement difficult, as both increase the already very large size of the block of indicators. In particular, investigating variation in the cyclical sensitivity of the UCL with respect to a continuous covariate is impossible, as this exercise would increase the necessary set of indicators infinitely. In addition, even in the case of a categorical covariate with few categories, inference is problematic, as relevant covariances are lost in the multistep procedure. Meanwhile, failure to address heterogeneity assumes that agents do not differ in the cyclical sensitivity of their return to tenure *or* their propensity to be observed at any given tenure horizon, potentially resulting in bias. Finally, low-frequency measurement is problematic when considering such questions as the effect of monetary policy shocks.[14]

[14]Without the innovations in methodology developed in this paper, analysis of monetary policy shocks

Here, I provide a more parsimonious estimation strategy for recovering the response of the (log) UCL to a deviation in macroeconomic variable x from trend. The new strategy allows higher-frequency measurement of the cyclical position at the time of hiring and inference regarding heterogeneity in the cyclical sensitivity of the UCL—including with respect to a continuous covariate. In addition, because the procedure has fewer steps, it is easier to pinpoint the relevant covariations yielding headline results and, therefore, to interrogate potential sources of bias.

Write the desired (semi-) elasticity:

$$\begin{aligned}
\frac{dln(UCL_t)}{dx_t}|_{x_t=0} =& \Big\{\frac{dUCL_t}{dx_t}\frac{1}{UCL_t}\Big\}|_{x_t=0} \\
=& \Big\{\frac{1}{UCL_t}\sum_{j=0}^{\infty}\Big[\beta^j(1-s)^j\frac{dw_{t+j,t}}{dx_t} - \beta^{j+1}(1-s)^{j+1}\frac{dw_{t+1+j,t+1}}{dx_t}\Big]\Big\}|_{x_t=0} \\
=& \Big\{\frac{1}{UCL_t}\sum_{j=0}^{\infty}\Big[\beta^j(1-s)^j\frac{dln(w_{t+j,t})}{dx_t}w_{t+j,t} \\
& - \beta^{j+1}(1-s)^{j+1}\frac{dln(w_{t+1+j,t+1})}{dx_t}w_{t+1+j,t+1}\Big]\Big\}|_{x_t=0} \qquad (4.1)
\end{aligned}$$

where x_t is a deviation of some macro indicator from trend x_t^*. Note, $\frac{dln(w_{t+j,t})}{dx_t}w_{t+j,t}$ is the percentage premium over the average in the return to the j^{th} year of tenure due to having been hired when the macroeconomic variable of interest was x_t above trend.

Identification requires three assumptions: (1) the effect of a deviation of the macro indicator from trend on wages at lead j—$\frac{dw_{t+j,t}}{dx_t}w_{t+j,t}$—is stationary for all j; (2) the macro indicator is expected to be on trend tomorrow if it is on trend today: $\mathbb{E}_t[x_{t+1}|x_t = 0] = 0$, which implies that $UCL_t(0) = w_{t,t}(0)$; and (3) x_t and x_t^* are well estimated by the detrending algorithm. Under these assumptions, equation 4.1 simplifies to

$$\frac{dln(UCL_t)}{dx_t}|_{x_t=0} = \left[\sum_{j=0}^{\infty}\beta^j(1-s)^j\frac{w_{t+j,t}}{\bar{w}_{t,t}}\left[\frac{dln(w_{t+j,t})}{dx_t} - \beta(1-s)\frac{dln(w_{t+j,t})}{dx_{t-1}}\right]\right]|_{x_t=0} \qquad (4.2)$$

Thus, under these assumptions, an estimate of the sensitivity of the UCL to x_t is then obtained as

$$\frac{d\widehat{ln(UCL)}}{dx} = \sum_{k=0}^{\infty} exp(\hat{\zeta}_j)\left[\hat{\chi}_j - \hat{\psi}_j\right], \qquad (4.3)$$

requires interpolation of data, as in Basu and House (2016).

where $\hat{\zeta}_k$, $\hat{\chi}_k$, and $\hat{\psi}_k$ are the estimated coefficients from wage regression

$$ln(wage_{\tau,i}) = \sum_{k=0}^{T}\left[\zeta_k \mathbb{I}_{k,\tau,i}^{tenure} + \chi_k \frac{\mathbb{I}_{k,\tau,i}^{tenure}}{\beta^k(1-s_i)^k} * x_{\tau-k} + \psi_k \frac{\mathbb{I}_{k,\tau,i}^{tenure}}{\beta^{k+1}(1-s_i)^{k+1}} * x_{\tau-k-1}\right] + controls_{i,k,\tau}\Xi_{k,\tau} + \varepsilon_{i,\tau}, \tag{4.4}$$

where $\mathbb{I}_{k,\tau,i}^{tenure} = 1$ if individual i has tenure k at time τ and $x_{\tau-k}$ and $x_{\tau-k-1}$ are the deviation of the macro indicator from trend at the time of hiring and one period before the time of hiring, respectively. The $\mathbb{I}_{k,\tau,i}$ are collinear, and I resolve this by restricting ζ_0 to be zero. This restriction gives the interpretation of the ζ_k as the percentage return to the k^{th} year of tenure relative to the first. Meanwhile, χ_k is the percent increase in the return to the k^{th} year of tenure due to having been hired when the macroeconomic indicator was x_t above trend, and ψ_k is the percent increase in the return to the k^{th} year of tenure due to having been hired one year after the macroeconomic indicator was x_t above trend. Following the literature, I set $T = 7$. Standard errors are recovered via a straightforward application of the delta method.[15]

With this more parsimonious estimator, considering potential heterogeneity in the cyclical sensitivity of the UCL with respect to covariates is straightforward. This calculation is achieved simply by allowing ζ, χ, and ψ (and the relevant covariates in Ξ) to depend on the covariate of interest via interaction terms. I can also allow the discount rate and/or separation rate to vary systematically with the covariate of interest, such as education, as is the focus of this paper. In addition, this formulation of the estimator for the UCL permits transparent assessment of sources of bias. A final advantage of this method, particularly in the application to monetary policy shocks, is that τ and k need not have the same frequency. In particular, while the frequency at which wages are measured, τ, is low (yearly or biyearly), while the frequency at which job-start dates are measured, k, may have a higher frequency, which more closely corresponds to the pattern of monetary policy shocks facilitating estimation of the impulse response at date $\tau - k - h$ via a local projection, as in Jordà (2005). To fix ideas, the NLSY records wages yearly or biyearly, but records start dates at the weekly frequency.

In panel data, regressions contain individual fixed effects and standard errors are clustered at the individual level. This setup allows for errors to be auto-correlated within individual (Solon et al., 1994). In addition to the variables explicitly mentioned, *controls* contains

[15] Note, following the $T = 7$ protocol suggested by the literature biases the estimated UCL toward zero. The bias is greater if the separation rate is lower, since a greater proportion of workers with low separation rates expect to be employed beyond the seven year horizon. In the present context, this implies that the cyclical sensitivity of the most educated is most severely biased toward zero.

Table 2: Wage Cyclicality by Education Group.

	User cost of labor		New hire's wage		Ave. hourly earnings[b]	
≥ College	-15.49***	(3.86)	-3.38***	(0.79)	-1.40***	(0.38)
High School / Some Coll.	-4.90***	(1.52)	-1.81***	(0.27)	-1.10***	(0.17)
< High School	1.36	(2.48)	-1.15***	(0.48)	-1.00***	(0.34)
R-squared	0.69		0.69		0.68	
Observations	54,543		54,543		54,543	

Note: Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Source: National Longitudinal Survey of Youth 1979; author's calculations.

education, a quadratic in potential experience, a linear time trend, fixed effects for the quarter of the wage observation and the quarter of hiring, industry fixed effects, and the realization of x_τ in the quarter of the wage observation. For comparability, the sensitivity of AHE and NHW are estimated via analogous methods that are outlined in Appendix B. In Section 5.2, I consider additional controls designed to assess robustness to cyclical sorting.

5 Inequitably Volatile Wages

To investigate inequalities in the sensitivity of wages to macroeconomic shocks, I augment the statistical model described in Section 4 in two ways. First, I allow the separation rate to depend on education. Specifically, in the NLSY data, yearly separation rates are 37, 30, and 24 percent for workers with less than a high school degree, workers with high school or some college, and workers with bachelors degrees or more, respectively. Second, I fully interact the model with three education categories: (1) less than high school, (2) high school or some college, and (3) bachelor's or more. Fully interacting allows for the EWW to differ by education group while ensuring that estimated differences are not spuriously being inferred from steady-state differences in, for example, the return to experience. I also estimate the sensitivity of AHE and NHW by education using the analogous panel methods described in the appendix.

Table 2 presents the results when deviations of the unemployment rate from trend are used as the cyclical indicator.[16] As in Kudlyak (2014) and Basu and House (2016), AHE and

[16]Trend is identified using a Hodrick-Prescott (HP) filter with tuning parameter 100,000 on quarterly data. In Appendix C.1, I show that results are robust to using unfiltered unemployment as well as unemployment filtered using the Hamilton (2018) filter, both of which address susceptibility of the HP filter to the end-point problem. Results are also robust to identifying the cycle using deviations of log real gross domestic product from trend.

NHW exhibit more mild cyclicality than the UCL for all but the least educated, for whom the UCL is estimated imprecisely.

Differences across education are much starker than differences across wage measures. Strikingly, the UCL exhibits no statistically discernable cyclicality for the least educated but is increasingly pro-cyclical as educational attainment increases. A 1 percent increase in the unemployment rate decreases the UCL of workers with a bachelor's degree or more by almost 16 percent! While this sensitivity appears shockingly large, it is, in fact, broadly in line with existing findings. In particular, the magnitude is inline with back of the envelope calculations of the cyclical sensitivity of the UCL based on the persistent effects of labor market entry during a recession documented in Oreopoulos et al. (2012). The ranking of cyclical sensitivity holds for the NHW and AHE, but differences are considerably less stark.

The pattern of increasing procyclicality in educational attainment is notable for two main reasons. First, the wages of the least educated are a-cyclical by all measures. A-cyclicality for this group reverses the headline finding of Kudlyak (2014) and Basu and House (2016) for this segment of the labor market and suggests that wage rigidities may, in fact, contribute to our understanding of cyclical fluctuations in employment for this group.[17]

Second, increasing pro-cyclicality as educational attainment rises suggests that employment of more-educated workers is more robust to changing business cycle conditions. This finding comports well with the dearth of sensitivity documented in the preceding section. I will discuss the causes and consequences further in Section 6.

5.1 Differential Separation Rates or Wage-Tenure Effects?

Intuition, based on examination of equation 3.2, suggested that differences in cyclical sensitivity across education derives from differentials in separation rates. The data reveal that these differentials are amplified by differentials in the cyclical sensitivity of the wage-tenure profile across educational attainment. Indeed, I document here that differential sensitivity of the wage-tenure profile accounts for the vast majority of differentials. These differentials suggest that, in addition strategic intertemporal shifting of risk is an important feature in the labor contracts of the more educated.

Figure 3 plots the NHW followed by the discounted EWW for workers with less than a high school degree, with high school or some college, and with a bachelor's or more to a 1 percent increase in the detrended unemployment (solid line). Consistent with estimates for the effect of cyclical shocks on NHWs, the effect on wages of newly hired workers is negative

[17] Specifically a-cyclicality for this submarket may restore the potential of nominal wage rigidity in generating both amplification and persistence in the Diamond-Mortensen-Pisarides class of models, criticized by Kudlyak (2014), and in the class of New Keynesian models, criticized by Basu and House (2016).

Figure 3: Expected Wage Wedge, by Education

Source: National Longitudinal Study of Youth 1979; author's calculations.

and statistically significant for the high school or some college and college or more groups but not for the least educated. Strikingly, convergence back to zero is neither immediate nor equally rapid across groups. Rather, the negative effect of having been hired at a time with an elevated unemployment rate is larger and persists significantly longer for the college or more group.

To evaluate the contributions of this persistent divergence in responses, I construct counterfactual UCL by education under two assumptions: (1) constant separation rates across education and (2) constant cyclical sensitivity of wage-tenure profiles across education. Table 3 reports both counterfactuals. This exercise clearly shows that differences in the cyclicality across education groups are mainly attributable to differences in the sensitivity of wage-tenure profiles to the aggregate state and do not merely result from compounding identical wedges over a longer expected horizon, confirming the predictions of an implicit contracting model (Thomas and Worrall, 1988). Figure 3 plots the NHW and discounted EWW under the counterfactual in which separation rates are identical (dotted line) and in which only the separation rates are allowed to vary with education (dashed line). These counterfactuals clearly illustrate that the differentials in the sensitivity of the wage-tenure profile to shocks are the main driver of divergence across education groups.

This finding helps resolve a tension between a labor economics literature that documents persistent scarring for college-educated men entering the labor market during recessions (for example, Kahn, 2010; Oreopoulos et al., 2012) and the common assumption in many macroeconomic contexts of relative acyclicality of wages for the representative agent. While Kahn (2010) and Oreopoulos et al. (2012) focus on college-educated men to ensure that

Table 3: Job Duration versus Wage-Tenure Profiles; Cyclical Regressions.

	User cost of labor		Holding constant: Separation rate		Holding constant: Wage-tenure effects	
$\geq$ College	-15.48***	(3.86)	-12.98***	(3.15)	-6.31**	(2.59)
High school / some coll.	-4.90***	(1.52)	-4.51***	(1.41)	-5.47**	(2.59)
< High school	1.36	(2.48)	2.12	(3.04)	-4.73*	(2.59)
R-squared	0.69		0.69		0.68	
Observations	54,543		54,543		54,543	

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Source: National Longitudinal Survey of Youth 1979; author's calculations.

graduation rather than strategic considerations on the part of the worker drive the timing of labor market entry, findings for their educated samples need not extrapolate to less educated groups. Indeed, while Oreopoulos et al. (2012) show that scarring effects are larger for less technical majors, Genda et al. (2010) find little persistence in wages of the timing of labor market entry for less-educated males in the United States.

While this study does not focus on the effects of the timing of labor market entry, differences in the cyclical sensitivity of the UCL speak to this debate. In contrast to the pattern one would extrapolate from the variation across majors documented by Oreopoulos et al. (2012), I find that contemporaneous macroeconomic conditions and conditions at the time of hiring affect the wages of less-educated workers *much less* systematically.

5.2 Composition Effects of Recessions

The relatively poor employment outcomes for less-educated workers coupled with the relative insensitivity of their wages suggest a compositional shift, as more-educated workers crowd lower-skilled labor markets (Devereux, 2002). In this case, the persistently depressed wages observed for the highly educated hired during a recession may reflect poor match quality and, therefore, low productivity rather than cyclically sensitive wages for the same work. From a macroeconomic perspective, knowing if wages paid *for the same work* are cyclically sensitive is important. Thus, removing the effects of cyclical sorting from the estimates documented in the previous section is important. In other words, it would be ideal to estimate the cyclical sensitivity of the UCL for matches of identical quality if match quality were observed.

In search of a proxy for match quality, I take on a view of cyclical sorting as driven by a job ladder. In a steady-state version of this view, Altonji and Shakotko (1987) hypothesize that well-matched and consequently well-paid workers are less likely to leave their employers.

Table 4: Imputed Idiosyncratic Yearly Separation Rates

	Mean	Standard Deviation
$\geq$ College	0.25	0.18
High School / Some Coll.	0.30	0.19
$<$ High School	0.38	0.18

Source: National Longitudinal Survey of Youth 1979; author's calculations.

As a result, the coefficient on tenure obtained from a Mincer-style regression without controls for match quality is biased away from zero. With the job-ladder view in mind, Altonji and Shakotko (1987) suggest a worker's completed tenure with their incumbent as a proxy for match quality. Through revealed preference, the better match will last longer. Hagedorn and Manovskii (2013) provide the nuance necessary for a business cycle context. In particular, they argue that workers remaining with their employers during tight labor markets, during which workers have many opportunities to reallocate to better matches, reveals higher match quality than similar tenure in a slack labor market. Hagedorn and Manovskii (2013) suggest using the cumulated market tightness during the time of continuous employment leading up to the present job, m^{ctj}, and during the completed tenure on the present job, m^{job}, as proxies for match quality. Loosely, these proxies capture the opportunities that a worker climbed through to arrive at the present job and the opportunities the worker passed up to remain at the present job.

With these measures of match quality in hand, I use two steps to recover the cyclical sensitivity of the UCL for a worker of each level of educational attainment who has the average match quality. First, I recover each individual's predicted yearly separation rate as the predicted value from a fitted logistic model of separation as a function of the Hagedorn and Manovskii (2013) measures of match quality interacted with the category of educational attainment and cyclical position at the time of hiring, controlling for an education-specific linear time trend. Table 4 reports the mean and standard deviation of the predicted yearly separation rates by education.

Second, I estimate a version of the statistical model described in equation 5.1 in which all of the terms related to the present job are interacted with the match quality of that job and separation rates take on the predicted idiosyncratic value for each job. Specifically,

$$
\begin{aligned}
&ln(wage_{\tau,i}) = \\
&\quad \sum_{k=0}^{T}\Bigg[\zeta_k \mathbb{I}^{tenure}_{k,\tau,i} + \chi_k \frac{\mathbb{I}^{tenure}_{k,\tau,i}}{\beta^k(1-s_{i,t})^k} * x_{\tau-k} + \psi_k \frac{\mathbb{I}^{tenure}_{k,\tau,i}}{\beta^{k+1}(1-s_{i,t})^{k+1}} * x_{\tau-k-1} \\
&\qquad \zeta^{job}_k \mathbb{I}^{tenure}_{k,\tau,i} * m^{job}_{i,t} + \chi^{job}_k \frac{\mathbb{I}^{tenure}_{k,\tau,i}}{\beta^k(1-s_{i,t})^k} * x_{\tau-k} * m^{job}_{i,t} + \psi^{job}_k \frac{\mathbb{I}^{tenure}_{k,\tau,i}}{\beta^{k+1}(1-s_{i,t})^{k+1}} * x_{\tau-k-1} * m^{job}_{i,t} \\
&\qquad \zeta^{ctj}_k \mathbb{I}^{tenure}_{k,\tau,i} * m^{ctj}_{i,t} + \chi^{ctj}_k \frac{\mathbb{I}^{tenure}_{k,\tau,i}}{\beta^k(1-s_{i,t})^k} * x_{\tau-k} * m^{ctj}_{i,t} + \psi^{ctj}_k \frac{\mathbb{I}^{tenure}_{k,\tau,i}}{\beta^{k+1}(1-s_{i,t})^{k+1}} * x_{\tau-k-1} * m^{ctj}_{i,t}\Bigg] \\
&\qquad + \eta_{job} m_{job} + \eta_{ctj} m_{ctj} + controls_{i,k,\tau}\Xi_{k,\tau} + \varepsilon_{i,\tau}. \qquad (5.1)
\end{aligned}
$$

Again, because of co-linearity, one ζ_k, ζ^{job}_k, and ζ^{ctj}_k is not identified, and I normalize ζ_0, ζ^{job}_0, and ζ^{ctj}_0 to zero. This regression yields an estimate of the sensitivity of UCL to x_t for the worker with average match quality for her educational category:

$$\frac{dln(\widehat{UCL})}{dx} = \sum_{k=0}^{\infty} exp(\hat{\zeta}_j)\left[\hat{\chi}_j - \hat{\psi}_j\right]. \qquad (5.2)$$

It also yields estimates of the marginal effect of an increase in each of the two measures of match quality:

$$\frac{dln(\widehat{UCL})}{dxdm^{job}} = \sum_{k=0}^{\infty} exp(\hat{\zeta}^{job}_j)\left[\hat{\chi}^{job}_j - \hat{\psi}^{job}_j\right] \qquad (5.3)$$

and

$$\frac{dln(\widehat{UCL})}{dxdm^{ctj}} = \sum_{k=0}^{\infty} exp(\hat{\zeta}^{ctj}_j)\left[\hat{\chi}^{ctj}_j - \hat{\psi}^{ctj}_j\right]. \qquad (5.4)$$

For ease of interpretation, I demean m^{job} and m^{ctj} and standardize them such that a 1 percent decrease in the detrended unemployment rate triggers a one unit increase in each measure of match quality.

Figure 4 plots the NHW and EWWs recovered for the worker with average match quality for each level of education (dashed line) alongside the specification without controls for match quality (solid line). The adjustment reveals that, on average, more-educated workers hired during recessions have unusually low match quality. However, within two years, the bias fades. Further, the adjustment reveals that, for college graduates, being hired in a recession leads to lasting deficits in remuneration *for the same quality match*. Table 5 reports the cyclical sensitivity of the UCL for workers with the average match quality conditional on their education, along with the marginal effect on the cyclical sensitivity of the UCL of a

Figure 4: Adjusting for Cyclical Sorting

Source: National Longitudinal Survey of Youth 1979; author's calculations.

Table 5: Adjusting for Cyclical Sorting

	≥ College		High School/ Some Coll.		< High School	
Unemployment rate[a]	-12.39**	(5.56)	-1.38	(2.35)	1.01	(4.61)
$\times\ m_{job}$	-1.35**	(0.64)	0.40	(0.30)	0.31	(0.40)
$\times\ m_{ctj}$	-0.13	(0.41)	-0.15	(0.18)	-0.12	(0.30)
R-squared			0.68			
Observations			54,543			

[a] Deviations from trend. Trend identified using an HP filter.
Note: Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Source: National Longitudinal Study of Youth 1979; author's calculations.

Table 6: Effect of Match Quality on Contemporaneous Wages.

	≥ College		High School/ Some Coll.		< High School	
m_{job}	0.52	(0.36)	0.21	(0.16)	-0.33**	(0.15)
m_{ctj}	0.09	(0.50)	1.24***	(0.26)	0.90*	(0.51)
R-squared			0.68			
Observations			54,543			

[a] Deviations from trend. Trend identified using an HP filter.
Note: Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Source: National Longitudinal Study of Youth 1979; author's calculations.

one standard deviation increase in match quality, as measured by the sorting opportunities leading up to the present job, m^{ctj}, and on the present job, m^{job}. For more college workers, the UCL is *more* rather than less cyclically sensitive in high-quality matches as measured by sorting opportunities on the present job.

Further, Table 6 reports the sensitivity of contemporaneous wages to a one standard deviation increase in each proxy for match quality from the same regression used to infer the UCL. For the most educated, match quality is irrelevant to contemporaneous wages after controlling for its effects on the wage tenure profile. However, match quality as measured by opportunities for cyclical sorting before hiring accounts for a significant share of contemporaneous wages—after controlling for its effects on the wage tenure profile—with a one standard deviation increase in match quality as measured by opportunities for sorting prior to hiring accounting for just over 1 percent higher wages.

5.3 Current Population Survey

A criticism of the existing studies of the UCL is dependence on the NLSY data. Among the advantages of these data are that they cover the longest time horizon, but the main disadvantage is the single and aging cohort. Inference may spuriously derive from an interaction between the timing of shocks and the particular moment in the lifecycle of the NLSY cohort. To address this concern, I replicate my main findings in the CPS, which has the advantage of a sampling design representative of the population as a whole at the time of each survey. While the CPS is constructed as a rotating panel, the tenure data are bi-yearly and the intervening flows data have gaps and thus, at best, provide censored measures of tenure. Thus the data must be treated as repeat cross-section and it is impossible to control for individual fixed effects. As a result, a key drawback of the CPS estimates is that they rely more heavily on the controls in Ξ, particularly industry and occupation, to absorb cyclical variation in the composition of the labor force. With this grain of salt in mind, estimates obtained from the CPS confirm the main findings obtained from the NSLY: the UCL is procyclical for the most highly educated and acyclical for the least and the divergence stems from a larger and more persistent expected wage wedge for the more highly educated.

5.4 Monetary Policy Shocks

As in the analysis of employment, differences in the cyclicality of demand for workers with different education could drive divergence in the sensitivity of wages. While such a suspicion would suggest that wages and employment would be most sensitive for the same educa-

Table 7: Wage Cyclicality, by Education Group, (Current Population Survey).

	User cost of labor		New hire's wage		Avg. hourly earnings	
≥ College	-6.99**	(3.29)	-1.50*	(0.88)	0.79**	(0.33)
High School / Some Coll.	-2.88*	(1.74)	-1.15***	(0.59)	-0.92***	(0.25)
< High School	-0.08	(3.16)	-0.17	(1.50)	0.15	(0.69)
R-squared	0.37		0.37		0.37	
Observations	44,862		44,862		44,862	

Note: Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Source: Current Population Survey, Tenure Supplements; Current Population Survey, Earner's Studies; author's caculations.

tion level—the opposite of what I have documented in this paper—I can address the issue more rigorously by subjecting all labor markets to a uniform aggregate demand shock. One candidate is a monetary policy shock.

My approach is straightforward. First, I identify monetary policy shocks from Federal Reserve Board Greenbook forecast errors, as in Romer and Romer (2004). Second, I trace out the impulse response of unemployment and wages by educational attainment using a local projection method (Jordà, 2005). In Appendix C.2, I consider alternative strategies for the identification of monetary policy shocks.[18] All methods of identifying monetary policy shocks yield consistent results.

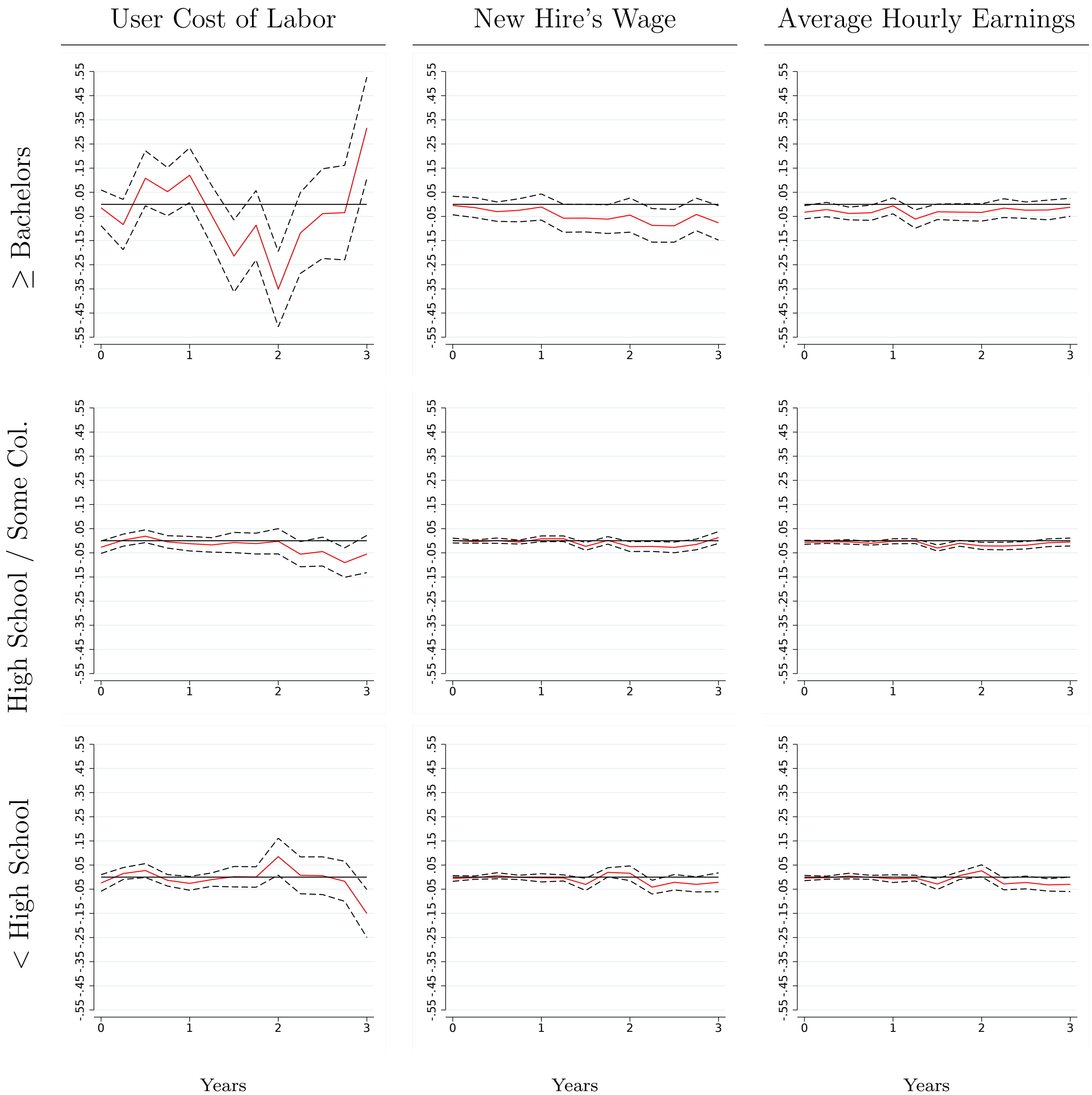
Figure 5 illustrates the impulse response for AHE, NHW, and UCL by educational attainment. As in the earlier regressions, the UCL is the most sensitive, and sensitivity is concentrated on the most educated. A 100 basis point contraction in monetary policy is associated with a 35 percent decrease in the UCL for the most educated at the two-year horizon but has no effect on the UCL of the least educated.

6 Macroeconomic Costs of Fuctuations

I turn now to assessing the potential welfare implications of differentials in the cyclicality of wages. From Galí (2013), we know that increasing wage flexibility need not increase aggregate welfare, the intuition being that rigid wages facilitate a degree of consumption smoothing that in turn stimulates activity through the aggregate demand channel. Here

[18] Figure A2 plots the impulse response when excluding the Volker Reform (1979-1982). Figure A1 checks the robustness of the identification of the underlying shocks to the exclusion of the 1969-1978 Greenbook data. Finally, Figure A3 considers identifying monetary policy shocks using a high-frequency identification strategy, as in Gertler and Karadi (2015).

Figure 5: Romer and Romer (2004) Identified Shocks.



Note: 95% confidence interval.
Source: National Longitudinal Survey of Youth 1979, Current Population Survey; Federal Reserve Board Greenbooks as cleaned by Coibion et al. (2012); author's calculations.

I consider the related question of whether the labor variety with relatively more flexibility enjoys relatively greater welfare. I begin by detailing the assumptions under which the response of aggregate consumption to shocks is equivalent to a representative agent model despite heterogeneity in wage and employment responses. I then show that, despite this equivalence, period utility is more volatile for workers with more-rigid wages and therefore that the welfare in the heterogeneous agent economy falls short of the output-gap equivalent representative agent economy.

However, before building the model, I need to clarify when the allocative wage of variety v, which I will call $W_{v,t}$, is the UCL for variety v and not some other function of remitted wages. Necessary assumptions are that both firms and workers have (1) accurate expectations regarding the evolution of future wages given the existing set of realized shocks and (2) access to financial markets through which they can smooth intertemporally.[19] I proceed under these assumptions.

6.1 Counterfactual Representative Agent Economy

To assess the welfare consequences of heterogeneous rigidities, it is useful to lay out the assumptions under which a counterfactual representative agent economy results in the same paths for the price level and output gap.

Let there be an intermediate producer who produces with the canonical Cobb-Douglass production function augmented to include two labor varieties:[20]

$$y_t(s) = z_t k_t(s)^{\alpha} \left(l_{1,t}(s)^{\gamma} l_{2,t}(s)^{(1-\gamma)} \right)^{(1-\alpha)},$$

where k_t, $l_{v,t}$, and y_t denote capital, labor varieties $v \in \{1,2\}$, and output at time t, respectively. For expositional purposes, I assume that the marginal product of each labor variety is identical up to differences in their output elasticities.[21] I want to consider the economy's response to aggregate demand shocks; therefore, it is useful to consider these producers as differentiated goods producers who have price-setting power, as in the standard New Keynesian framework. Thus, s indexes the intermediate producers' differentiated good.[22] I assume that these producers are competitive in their input markets and take the nominal input prices $W_{v,t}$, for $v \in \{1,2\}$, and R_t as given when minimizing costs.[23]

The firms choose inputs to minimize costs each period. Constant returns to scale and free flow of capital and labor across firms ensure that all firms choose the same capital-to-labor

[19] Note that this assumption does *not* preclude an implicit wage contract motivated out of differential risk aversion on the part of the worker and firm as in, for example, Rudanko (2009).

[20] Additional varieties can be added without loss of generality.

[21] Assuming heterogeneous, log-additive labor productivity—that is $l = en$ where l is the effective units of labor produced by n physical units of labor with efficiency e—does not substantively change results.

[22] As in the standard model, intermediate goods are aggregated into a final output good using a constant elasticity of subsaturation aggregation technology: $Y_t = \left(\int_0^1 y_t(s)^{\frac{\epsilon-1}{\epsilon}} \right)^{\frac{\epsilon}{\epsilon-1}}$.

[23] This assumption is typical when modeling the producer or intermediate producer in the real business cycle and New Keynesian models, respectively.

ratios and the same ratio of labor varieties.[24]

It is then straightforward to derive the factor demands and show that the elasticities of demand for each labor variety with respect to demand for final goods are

$$\varepsilon_{L_1,Y} = 1 + \Upsilon + \alpha\varepsilon_{R,Y} + (1-\alpha)\left[\gamma\varepsilon_{W_1,Y} + (1-\gamma)\varepsilon_{W_2,Y}\right] - \varepsilon_{W_1,Y}$$
$$\varepsilon_{L_2,Y} = 1 + \Upsilon + \alpha\varepsilon_{R,Y} + (1-\alpha)\left[\gamma\varepsilon_{W_1,Y} + (1-\gamma)\varepsilon_{W_2,Y}\right] - \varepsilon_{W_2,Y},$$

where $\varepsilon_{L_v,Y}$, $\varepsilon_{R,Y}$, and $\varepsilon_{W_v,Y}$ for $v \in \{1,2\}$ are the elasticities of labor demand, the rental rate, and wages with respect to demand for the final good, respectively, and Υ captures the effect of the sensitivity of the price level and the relative prices to demand for the final good. Clearly, if the elasticity of wages of variety one is greater than that of labor variety two, then the elasticity of labor of variety two is larger.

Meanwhile, it follows that the elasticities of earnings with respect to demand for the final good are

$$\varepsilon_{E_1,Y} = \varepsilon_{E_2,Y} = 1 + \Upsilon + \alpha\varepsilon_{R,Y} + (1-\alpha)\left[\gamma\varepsilon_{W_1,Y} + (1-\gamma)\varepsilon_{W_2,Y}\right],$$

where $\varepsilon_{E_v,Y}$ are the elasticities of earnings with respect to demand for the final good for varieties $v \in \{1,2\}$. The earnings elasticities are identical! This is a well-known property of the Cobb-Douglas production technology: relative expenditures (factor shares) are invariant to relative prices.

Let there also be variety-specific households that maximize the discounted value of utility flows from consumption and labor supply decisions subject to a simple budget constraint:

$$\max_{C_{v,t},L_{v,t},S_{v,t}} E_0 \sum_{t=0}^{\infty} \beta^t \left[u(C_{v,t}) - \phi v_v(L_{v,t})\right]$$
$$s.t.\ \ P_t C_{v,t} + S_{v,t+1} \leq S_{v,t}(1+i_t) + \Pi_t + W_{v,t}L_{v,t}$$

where $C_{v,t}$, $L_{v,t}$, and $S_{v,t}$ are the consumption, labor supply, and savings of the variety v household; P_t and i_t are the price level and the interest rate, and Π_t are dividends remitted from the firm. Finally, $u(\cdot)$ and $v_v(\cdot)$ are constant relative risk aversion flow utilities. Importantly, this assumption allows consumption insurance within but *not across* variety, which gives rise to the following Euler equation for each variety:

$$\frac{u'(C_{v,t})}{P_t} = \beta E_t \frac{u'(C_{v,t+1})(1+i_{t+1})}{P_{t+1}}, \tag{6.1}$$

[24]To verify note: $\frac{R_t}{W_{1,t}}\frac{(1-\alpha)\gamma}{\alpha} = \frac{l_{1,t}(s)}{k_t(s)} = \frac{L_{1,t}}{K_t}$, $\frac{R_t}{W_{2,t}}\frac{(1-\alpha)(1-\gamma)}{\alpha} = \frac{l_{2,t}(s)}{k_t(s)} = \frac{L_{2,t}}{K_t u_t}$, and $\frac{W_{1,t}}{W_{2,t}}\frac{1-\gamma}{\gamma} = \frac{l_{2,t}(s)}{l_{1,t}(s)} = \frac{L_{2,t}}{L_{1,t}}$.

In the steady state, (6.2) implies that varieties will consume proportionate to their earnings whenever dividends are also proportionate to earnings and the elasticity of intertemporal substitution is identical for all varieties. For the remainder of this section, I assume this to be true.

Because, as shown earlier, all varieties receive the same windfall income and because all the prices and preferences governing the Euler equation are invariant across varieties, each variety will choose to save and consume out of the windfall earnings identically. Thus, the elasticity of the consumption response is invariant across varieties whenever distributing dividends in proportion to earnings.

I can now consider a counterfactual representative agent economy in which (the elasticity of) the output gap is equivalent to the economy in which workers have heterogeneously flexible wages. Note that intermediate producers will produce equivalent output when they face equivalent costs and a real price level. In the heterogeneous agent economy, marginal cost is

$$mc = \frac{1}{z}\left(\frac{R}{\alpha}\right)^{\alpha}\left(\frac{W_1}{(1-\alpha)\gamma}\right)^{(1-\alpha)\gamma}\left(\frac{W_2}{(1-\alpha)(1-\gamma)}\right)^{(1-\alpha)(1-\gamma)} \tag{6.2}$$

and its elasticity is

$$\varepsilon_{mc,Y} = \alpha\varepsilon_{R,Y} + (1-\alpha)\left[\gamma\varepsilon_{W_1,Y} + (1-\gamma)\varepsilon_{W_2,Y}\right] \tag{6.3}$$

Even with heterogeneity, the elasticity of consumption is invariant across varieties: The aggregate effect on the price level is identical to that which would arise in a representative agent economy with identical consumption elasticity. Further, the elasticity of the marginal cost and, therefore, the output gap is equivalent to the output gap in a representative worker economy with

$$\varepsilon_{W_{rep},Y} = \gamma\varepsilon_{W_1,Y} + (1-\gamma)\varepsilon_{W_2,Y} \tag{6.4}$$

where $\varepsilon_{W_{rep},Y}$ is the elasticity of wages of a representative worker. Together with the equality of the earnings elasticity, (7.5) implies

$$\varepsilon_{L_{rep},Y} = \gamma\varepsilon_{L_1,Y} + (1-\gamma)\varepsilon_{L_2,Y}. \tag{6.5}$$

Therefore, the output-gap-equivalent representative agent has wage and labor supply elasticities that are a weighted average of the varieties with weights corresponding to the output elasticities.

6.2 Welfare

Assuming that wages are set flexibly for all varieties implies that all varieties equate their marginal rate of substitution to their marginal productivity. In this case, the welfare consequences of the output gap are identical across varieties. However, if wage flexibility holds, the variation across education in the cyclicality and sensitivity to monetary policy shocks of wages and employment documented in the preceding sections must imply that highly educated workers' Frisch elasticity is substantially smaller than that of less-educated workers. Limited evidence supports this hypothesis, in part because education is often used as an instrument to identify the representative agent's Frisch (Peterman, 2016). Alternatively, the heterogeneity documented in the preceding sections could stem from increasing rigidity of wages as education declines. Indeed, as discussed in those sections, decreasing durability of employer-employee matches as education declines implies that employers have more limited ability to smooth through transient shocks in the presence of identical near-term wage rigidities for less-educated employees.

The result that all varieties experience identical earnings elasticities is invariant to the microfoundation of differential elasticities of wages. Thus, even in an environment with nominal wage rigidities, the preceding results imply equally sensitive consumption across varieties. However, wage rigidities drive a wedge between the marginal rate of substitution and the marginal product of labor. Still, Galí (2013) shows that increasing wage rigidity need not decrease aggregate welfare. This derives from noting that, in a model with a representative worker variety, wage rigidity mutes the consumption response to the underlying shock, potentially resulting in a smaller fluctuation in aggregate demand. Under the assumptions made here, the intuition of Galí (2013) holds and the magnitude of the offsetting effect of wage rigidity is captured by the representative agent's elasticity of wages $\varepsilon_{W_{rep},Y}$. In other words, variation in the elasticity of wages for the varieties improves aggregate welfare whenever the analogous variation in the output-gap equivalent $\varepsilon_{W_{rep},Y}$ improves welfare. However, despite this finding, it is straightforward to observe that period utility is lower and more volatile (1) for the variety with more rigid wages and (2) when aggregated across varieties in the economy with heterogeneity as compared to the output-gap equivalent representative agent economy. These results stem from straightforward applications of Jensen's inequality.

Using the framework laid out in Ball and Romer (1989) and, most closely related to this work, Galí et al. (2007), one can assess empirically the magnitude of the excess welfare loss in the heterogeneous agent economy relative to the representative agent economy. Galí et al. (2007) show that under assumptions analogous to those that govern the representative agent analogue to the model considered here, welfare costs can be evaluated using data on the price and wage markups, consumption, and employment. The earlier results show that

Table 8: Welfare Costs of Fluctuations Relative to an Output-Gap-Equivalent Representative Agent Economy (1976-2018)

	Frisch Elasticity = 1		*Frisch Elasticity* = 5	
	EIS = 1	= 5	*EIS* = 1	= 5
Heterogeneous workers economy				
Aggregate	0.0039	0.0590	0.0118	0.0669
	1	*1*	*1*	*1*
< High school	0.0100	0.0650	0.0299	0.0850
	2.52	*1.10*	*2.52*	*1.27*
High sch. / some coll.	0.0036	0.0587	0.0109	0.0659
	0.92	*0.99*	*.92*	*0.99*
≥ Bachelor's	0.0006	0.0557	0.0019	0.0570
	0.16	*0.94*	*0.16*	*0.85*
Output-gap-equivalent representative worker economy				
	0.0034	0.0584	0.0102	0.0652
	0.86	*0.98*	*0.86*	*0.98*

Note: Italics report the ratio to the aggregate welfare cost of fluctuations in the heterogeneous workers economy. EIS is elasticity of intertemporal substitution.
Source: From the USECON database, I use compensation per hour (LXNFC) and real and nominal output (LXNFO and LXNFI), which refer to the nonfarm business sector; nondurable and services consumption (CNH + GSH), drawn from the respective national income and product accounts series; and implicit price deflator (LXNFI). Unemployment and hours by educational attainment are constructed from the Current Population Survey Basic Monthly and Outgoing Rotation files, respectively. Output elasticities are recovered using the National Longitudinal Survey of Youth data. Author's calculations follow the method of Gali et al. (2007).

extending the method of Galí et al. (2007) to the heterogeneous agent economy requires only measurement of employment at the variety level. I measure employment using the CPS monthly surveys. In addition to employment by variety, I require a measure of the output elasticity of each labor variety. In Appendix D.1, I derive a second-order approximation to the welfare cost of fluctuations. In Appendix D.2, I discuss how to recover the output elasticities from the NLSY data.

Table 8 documents the welfare costs of fluctuations relative to the costs experienced in the output-gap equivalent representative agent economy. Results depend on the modeled Frisch elasticity and EIS. I allow these parameters to take on values $\{1, 5\}$ and $\{1, 5\}$, respectively. As demonstrated by Galí et al. (2007), welfare losses due to unemployment fluctuations are overall small.[25] The baseline calibration, which sets both Frisch and EIS to unity, indicates

[25]Under the baseline specification of unit elasticities, welfare costs are an order of magnitude smaller than the 0.07 benchmark of Lucas (1987). Indeed, the losses presented here, which measure fluctuations in labor

a welfare cost that exceeds the representative agent economy by more than 15 percent. Further, the welfare loss of the least educated is more than *15 times larger* than that of the most educated! The level of welfare losses is higher when the Frisch elasticity and EIS are larger, as noted by Galí et al. (2007), but larger elasticities mute the effects of heterogeneity.

6.3 Discussion

The welfare consequences of heterogeneity suggested above are *substantial*. Still, my result is likely a very conservative lower bound due to the simplifying assumption of pooled household consumption. While the assumption is useful for parsimony, there is compelling evidence to the contrary, for example Coibion et al. (2017) document that monetary policy shocks induce inequitable consumption responses. While it is well known that admitting idiosyncratic consumption risk greatly reduces tractability, intuition suggests that the costs associated with idiosyncratic consumption risk disproportionately burden the low skilled since, as I document here, these workers face income loss on an extensive rather than intensive margin. Indeed, in a model that includes differential employment risk and idiosyncratic consumption risk, Krusell et al. (2009) find welfare losses an order of magnitude larger than in the Lucas (1987) framework employed here.

Further, my results, which imply increasing implementation of implicit contracting increasing education, suggest that highly educated workers differentially use such contracting to self-insure against idiosyncratic consumption streams. This adds to the myriad of other ways that more privileged members of the economy enjoy differentially better access to credit.

7 Conclusion

This paper documents divergence in the flexibility of wages across educational attainment. This divergence is especially evident when using a measure of wages designed to capture the allocative consequences of the long-term nature of employment relationships and the history dependence of wage remittances: the UCL. I also document divergence in the response to monetary policy shocks across educational attainment. Finally, while the allocative wages of more educated workers are more sensitive, their employment is less sensitive.

In addition, I find that the wage-tenure profile of more-educated workers is differentially cyclically sensitive. In conjunction with differentially lower separation rates, this finding renders allocative wages for this group more sensitive to business cycle conditions; however,

using the employment rate, are even smaller than those of Galí et al. (2007) because the employment rate varies less over the cycle than hours. However, I use employment because it can be constructed by education from 1976 onward, whereas hours can be constructed only from 1982.

the differentially persistent effect of historical shocks on wages drives the divergence. This relationship suggests that a greater proportion of the wage response to shocks is *optimally* deferred over a longer period of time when the job's separation rate is lower, confirming the predications of Thomas and Worrall (1988). A promising avenue for future research would be to augment the business cycle implementation of Thomas and Worrall (1988), due to Rudanko (2009), to admit differential durability.

Using a parsimonious, and admittedly stylized, macroeconomic model I argue that ignoring these heterogeneities leads to underestimating the welfare costs of business cycles by 15 percent. This likely is a lower bound on the welfare consequences that would be obtained by a more realistic, but significantly more cumbersome, model incorporating financial frictions. The assumptions made here, in particular consumption pooling, stand in contrast to evidence,for example Coibion et al. (2017) that monetary policy shocks induce inequitable consumption responses. While admitting idiosyncratic consumption risk greatly reduces tractability, intuition suggests that the costs associated with idiosyncratic consumption risk disproportionately burden the low skilled since, as I document here, these workers face income loss on an extensive rather than intensive margin. Indeed, in a model that includes differential employment risk and idiosyncratic consumption risk Krusell et al. (2009) find welfare losses an order of magnitude larger than in the Lucas (1987) framework, which is employed in the present paper. Further, my results, which suggest increasing implementation of implicit contracting increasing education, suggest that highly educated workers differentially use such contracting to self-insure against idiosyncratic consumption streams. Thus, even in a model in which all education groups face identically incomplete asset markets, the highly educated, due to their differential durability, are better able to harness the asset value of their labor to negotiate smoother income streams.

Finally, the differences documented here have clear practical import for policy makers. In particular, when assessing impact of policy variation on welfare, results presented here suggest placing relatively greater weight on the experience of the least well educated. For example, re-optimizing the monetary policy rule to account for the heterogeneity documented here would entail placing greater weight on the unemployment of the least well educated when measuring the output gap.

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A Data

A.1 National Longitudinal Survey of Youth 1979

The canonical papers studying the UCL and NHW utilize the 1979 cohort of the NLSY (the NLSY79). The data are an unbalanced panel of workers surveyed yearly from 1979 to 1994 and every other year thereafter. Respondents were aged 14 to 21 at the date of the initial survey. Following Kudlyak (2014) and Basu and House (2016), I restrict the sample to males. This sample selection results in 54,543 observations of whom 16, 64, and 20 percent had less than a high school education, a high school education or some college, or a bachelors degree or more, respectively. 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.

Wage and retrospective employment date information is available for each respondent in each survey for up to five jobs. From these data, the NLSY constructs a variable "hourly rate of pay" for each job to synchronize reporting pay intervals (hour, day, week, month, year) using reported typical hours worked and earnings in a reference week. This variable includes tips, overtime pay, and bonuses before any deductions. The NLSY also constructs weeks of tenure as the sum of weeks worked with each of the five employers cumulated over the time between intervening surveys and across survey years. The original studies of Kudlyak (2014) and Basu and House (2016) use this measure of weeks of tenure; each identifies the date of hiring as the date of most recent observation of the wage less the weeks of tenure. In this study, I take a more conservative approach and date the hiring date as the first week of the current spell of employment with each employer. This approach results in more precise and, by construction, less-remote start dates for workers with temporary job separation as well as slightly higher separation rates. Estimates of cyclical sensitivity obtained from my identification of job start dates are slightly smaller than in Kudlyak (2014) and Basu and House (2016). Although this method contradicts usual notions of the effects of measurement error, note that the measurement error induced by constructing job start date from cumulated tenure is non-classical. Next, I provide details of how job interruptions are defined.

Handling the NLSY79 tenure data as in Kudlyak (2014) and Basu and House (2016) and including the same controls, the method proposed here replicates the headline findings of those papers almost exactly. For the sample considered by Kudlyak (2014), 1978-97, I obtain a cyclical sensitivity of the UCL to the unemployment rate of -5.61 (1.08) while Kudlyak reports -5.20 (0.70), standard errors are in parentheses. Kudlyak reports standard errors from a bootstrapped method that presumably resamples the NSLY microdata but *does not* block bootstrap the time-series variation. Such a strategy explains the very small standard error. Meanwhile, Basu and House (2016) report cyclical sensitivities to deviations from the HP-filtered unemployment rate and log real GDP over the horizon 1978-2006 of $-5.82\,(2.08)$ and 3.12 (1.35), respectively. Basu and House (2016) reports Newey-West standard errors. I obtain $-6.42\,(1.14)$ and 2.98 (0.53). Coding the start date of employment as I do in this paper results in slightly lower point estimates. Note, the measurement error induced by the Kudlyak (2014) and Basu and House (2016) construction of the start dates is non-classical.

Table A1: Reported Reasons for Separation.

Voluntary	Involuntary
· Quit for pregnancy, childbirth or adoption of a child	· Layoff, job eliminated
· Quit to look for another job	· Company, office or workplace closed
· Quit to take another job	· End of temporary or seasonal job
· Quit because Rs ill health, disability, or medical problems	· Discharged or fired
· Quit to spend time with or take care of children, spouse, parents, or other family members	· Government program ended
· Quit because didn't like job, boss, coworkers, pay or benefits	· Transportation problems
· Quit to attend school or training	· Retired
· Other (SPECIFY)	· No desirable assignments available
· Moved to another geographic area	· Project completed or job ended
· Dissatisfied with job matching service	· Job assigned through a temp agency or a contract firm became permanent
· Sold business to another person or firm	· Project completed or job ended
· Business temporarily inactive	· Business failed or bankruptcy
· Closed business or dissolved partnership	· Went to jail, prison, had legal problems

Note: Categorization follows Basu and House (2016) and Hagedorn and Manovskii (2013).

NLSY data contain weekly data on the employment situation of each individual that are collected retrospectively at the time of each (bi)yearly interview. These data record employment in up to five concurrent jobs for each week as well as the reason for termination of the employment relationship when it occurs. For each week, I record information about the *primary job*, defined as the job in which the worker reports the highest pay or as the job designated as primary if no pay is reported for any job. I also record the presence, if any, of secondary jobs.

An *employment cycle* is defined as a period during which a worker is continuously employed. I consider there to be a break in continuous employment if the respondent reports involuntary separation from her employer and there is no secondary job or if she reports greater than eight continuous weeks of unemployment regardless of the reason for separation. Reasons for severance are categorized into voluntary and involuntary in table A1.

Tenure is defined as the completed period during which the worker reports to have continuously worked for a given employer. Tenure is inclusive of time spent in multiple jobs such that tenure at job A begins with the first week that employer A is primary and ends with the last week that employer A is primary so long as job A was held continuously (potentially as a secondary job). Two or more discontinuous spells with the same employer result in two different tenure measures (corresponding to the completed length of each spell). Spells with employer A may be discontinuous if they are within different employment cycles or if they are interrupted by employment with another employer without employer A remaining as a secondary employer.

In addition to conforming with the cannon, the NLSY97 data are particularly useful for checking robustness to cyclical variation in match quality. In particular, weekly employment histories enable constructing proxies for match quality following Hagedorn and Manovskii (2013): cumulative labor market tightness from the last unemployment episode to the start date of the current job and during the completed tenure on the current job. If a worker holds multiple jobs, I define a worker's main job as the job in which they report the highest pay and I count the worker's tenure at each job as beginning at start date of the employment relationship even if it is not the main job so long as both jobs are continuously held.[26] The data design permits linking wages to tenure (measured to the week) for up to five jobs in each year that a worker is surveyed.

Educational upgrading on the job potentially biases results. Table A2 shows the limited incidence of upgrading on the job. Additionally, robustness checks (available upon request) reveal that alternative treatment of upgrading produces nearly identical results. Specifically, I consider coding education as contemporaneous education or as the highest educational attainment achieved during the job spell. The former effectively treats an educational upgrade as a new hire, and the latter assumes that employers have ex-ante knowledge of an employee's educational prospects at the time of hiring.

A.2 Current Population Survey

I also test robustness of the main findings using the CPS. The main advantage of the CPS is a nationally representative sample from which important macroeconomic indicators are constructed.

[26]The treatment of secondary jobs differs slightly from the treatment in Basu and House (2016). There, they study only the main job and as a result select jobs with longer tenure.

Table A2: Educational Upgrading while Employed

	Percentage upgrading education on the job:			
	All years	1979-1988	1989-1998	1999-2008
Attain high school equivalent	1.81	3.35	0.52	0.52
Attain college degree	2.30	5.21	1.06	0.88

Source: National Longitudinal Survey of Youth 1979; author's calculations.

The CPS records the employment status and demographic characteristics of a representative sample of U.S. workers monthly. I use the data since 1976 over which time microdata are available in the basic monthly survey that enable consistent construction of unemployment and employment rates by demographic groups. From 1982 onward the micro data collected from the outgoing rotation groups—roughly one-fourth of the sample—also contain information on usual hours worked and weekly earnings. Merging these data to create a rotating short panel is possible. Shimer (2012), among others, studies the properties of the employment and job-flows data. The properties of the wage data in the short panel have been studied in Daly and Hobijn (2016) among others.

Retrospective data on tenure are collected in 1973, 1978, 1981, 1983, 1987, and in every other year from 1996 onward. I restrict attention to the biyearly data. Of the tenure data, 86 percent can be linked to wage data in at least one outgoing rotation survey and 56 percent can be linked to two outgoing rotations. Unfortunately, job separations between outgoing rotations are not measured consistently over time. Specifically, identification of job-to-job mobility is, in concept, possible using the referenced-based survey's "same job" question from 1994 onward (Fallick and Fleischman, 2004). However, Fujita et al. (2020) document that responses to this question are non-randomly missing from 2007 onward. Meanwhile, identifying job switches using industry and occupation switches performs poorly. In particular, such switches fail to exhibit the cyclical patterns observed in other job transition data, such as the Job Openings and Labor Turnover Survey (JOLTS).

Because the return to tenure is only identified separately from the time trend and individual fixed effect if separations between outgoing rotations are measured reliably, I primarily treat the data as repeated cross sections and use only the wage data observed contemporaneously with the tenure data or in the preceding three months (if tenure exceeds three months).

The CPS records educational attainment. Before 1992, education is recorded as the highest grade of school completed. From 1992 onward, education is recorded as the highest degree or diploma attained. The difference is particularly important for consistently measuring high school graduation. I follow the crosswalk used in Elsby and Shapiro (2012).

A.3 Macroeconomic Indicators

I supplement these main data sources with data on labor market tightness constructed as in Barnichon (2010) using the publicly available data from the Conference Board and JOLTS. Following Basu and House (2016) I deflate the hourly rate of pay with the implicit price deflator for the nonfarm business sector downloaded from the Federal Reserve Economic Data (FRED) database.

Following the literature, I take deviations from trend of the unemployment rate as the indicator of the business cycle. In the appendix, I check robustness instead considering deviations from trend of log real GDP. The respective series are downloaded from FRED and detrended using an HP filter with smoothing parameter 100,000 for quarterly data. The choice of smoothing parameter follows Shimer (2005) and Basu and House (2016).

In assessing the welfare implications of heterogeneity, I use the following series drawn from the USECON database: compensation per hour (LXNFC) and real and nominal output (LXNFO and LXNFI), which refer to the nonfarm business sector; nondurable and services consumption (CNH + GSH), drawn from the respective national income and product accounts series; and implicit price deflator (LXNFI).

A.4 Monetary Policy Shocks

I construct monetary policy shocks as in Romer and Romer (2004), using data updated by Coibion (2012). In robustness tests, I consider monetary policy shocks as identified by Gertler and Karadi (2015) using data obtained from the replication files.

B Estimating Average Hourly Earnings and New Hire's Wages

For comparability with the literature, I estimate the response of average hourly earnings (AHE) and new hire's wages (NHW) to x_t via standard panel methods. Explicitly, I run the wage regression:

$$ln(wage_{\tau,i}) = \gamma x_{\tau} + +controls_{i,k,\tau}\Xi_{k,\tau} + \varepsilon_{i,\tau}, \tag{B.1}$$

to obtain an estimate of the sensitivity of AHE as γ. And the wage regression:

$$ln(wage_{\tau,i}) = \eta_1 \mathbb{I}^{new\ hire}_{\tau,i} + \eta_2 x_{\tau} + \eta_3 \mathbb{I}^{new\ hire}_{\tau,i} * x_{\tau} + +controls_{i,k,\tau}\Xi_{k,\tau} + \varepsilon_{i,\tau}, \tag{B.2}$$

where $\mathbb{I}^{new\ hire}_{\tau,i}$ is an indicator equal to 1 if individual i has tenure less than one year at time τ, as in Bils (1985), to obtain an estimate of the sensitivity of the NHW as $\eta_2 + \eta_3$. Note, because the sensitivity of the NHW is a function of tenure, it, too, is biased by failure to appropriately control for cyclical variation in match quality.

Table A3: Wage Cyclicality, by Education Group

Cyclical indicator = log real GDP (HP filter)	User cost of labor		New hire's wage		Avg. hourly earnings	
≥ College	7.58***	(1.92)	1.16***	(0.40)	0.79***	(0.20)
High school / some coll.	1.27*	(1.03)	0.71	(0.13)	0.60***	(0.08)
< High school	-0.38	(1.17)	0.41	(0.22)	-0.43	(0.16)
R-squared	0.69		0.69		0.68	
Observations	54,543		54,543		54,543	
Cyclical indicator = unemployment rate	**User cost of labor**		**New hire's wage**		**Avg. hourly earnings**	
≥ College	-15.04***	(3.56)	-2.63***	(0.69)	-1.04***	(0.37)
High school / some coll.	-4.96***	(1.15)	-1.38***	(0.25)	-0.93***	(0.17)
< High school	1.61	(2.49)	-1.19***	(0.44)	-0.94***	(0.33)
R-squared	0.69		0.69		0.69	
Observations	54,543		54,543		54,543	
Cyclical indicator = unemployment rate (Hamilton)	**User cost of labor**		**New hire's wage**		**Avg. hourly earnings**	
≥ College	-9.25***	(2.95)	-2.96***	(0.74)	-0.78**	(0.37)
High school / some coll.	-1.50	(1.19)	-0.71***	(0.03)	-0.39**	(0.16)
< High school	2.06	(1.89)	-0.04	(0.45)	-0.28	(0.33)
R-squared	0.69		0.69		0.69	
Observations	54,543		54,543		54,543	

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Source: National Longitudinal Survey of Youth 1979; author's calculations.

C Alternative Identification of Shocks

C.1 Cyclical Position

Table A3 replicates Table 2 using alternative identifications of the cyclical position: deviations of log real gross domestic product (GDP) from trend identified by a Hodrick-Prescot

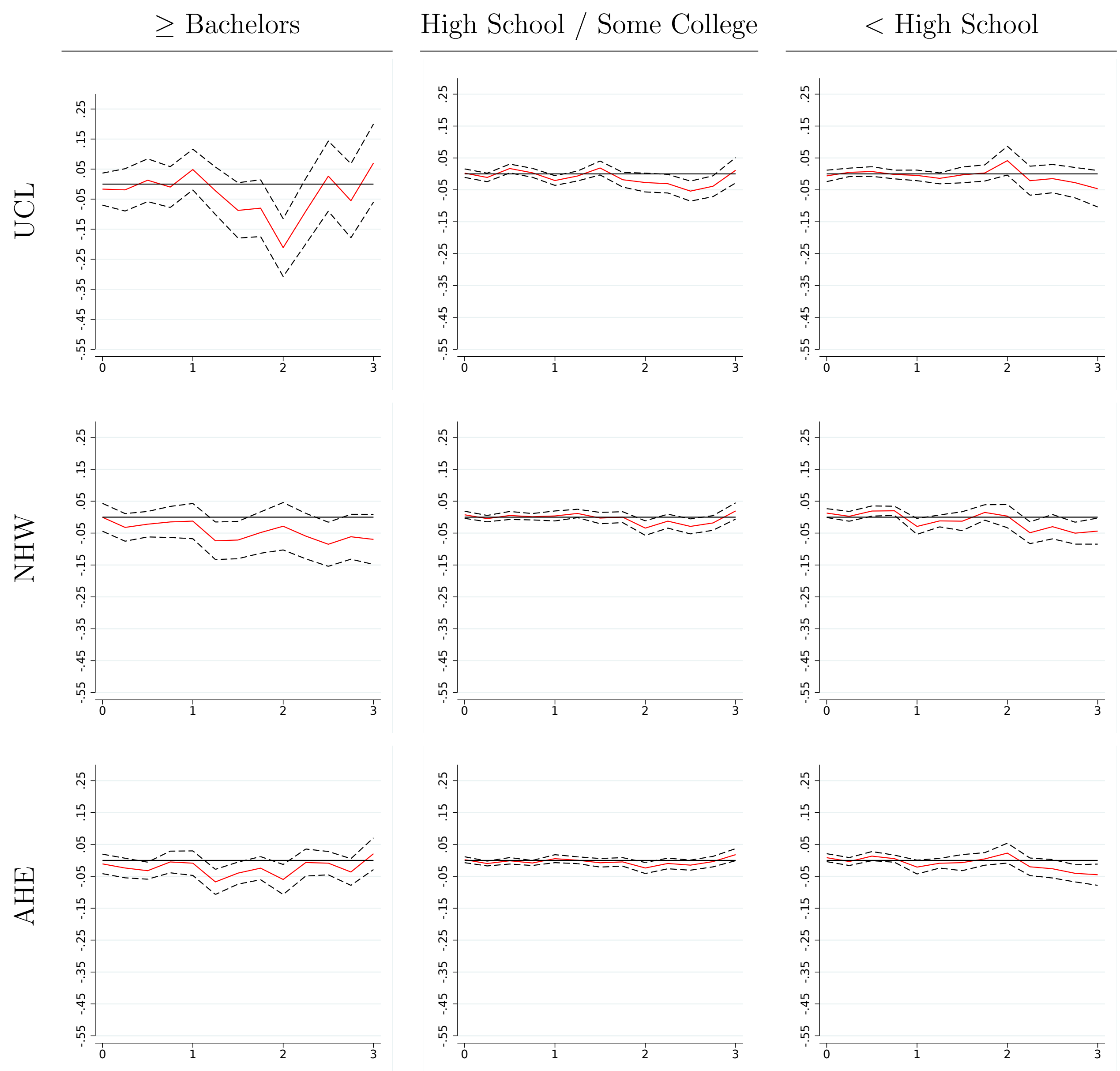
(HP) filter, the unfiltered unemployment rate, and deviations of unemployment from trend identified by the Hamilton (2018) filter. Conclusions are consistent across measures of cyclical position.[27]

C.2 Monetary Policy

I consider alternative strategies for the identification of monetary policy shocks. Figure A2 plots the impulse response of the employment rate, hours, and the user cost of labor (UCL) when excluding the Volcker reform (1979-82). In Figure A1, I check the robustness of the identification of the underlying shocks to the inclusion of the 1969-78 Federal Reserve Board Greenbook data. Finally, Figure A3 plots the impulse responses to monetary policy shocks identified using a high frequency identification strategy as in Gertler and Karadi (2015). All identification strategies yield the same conclusion.

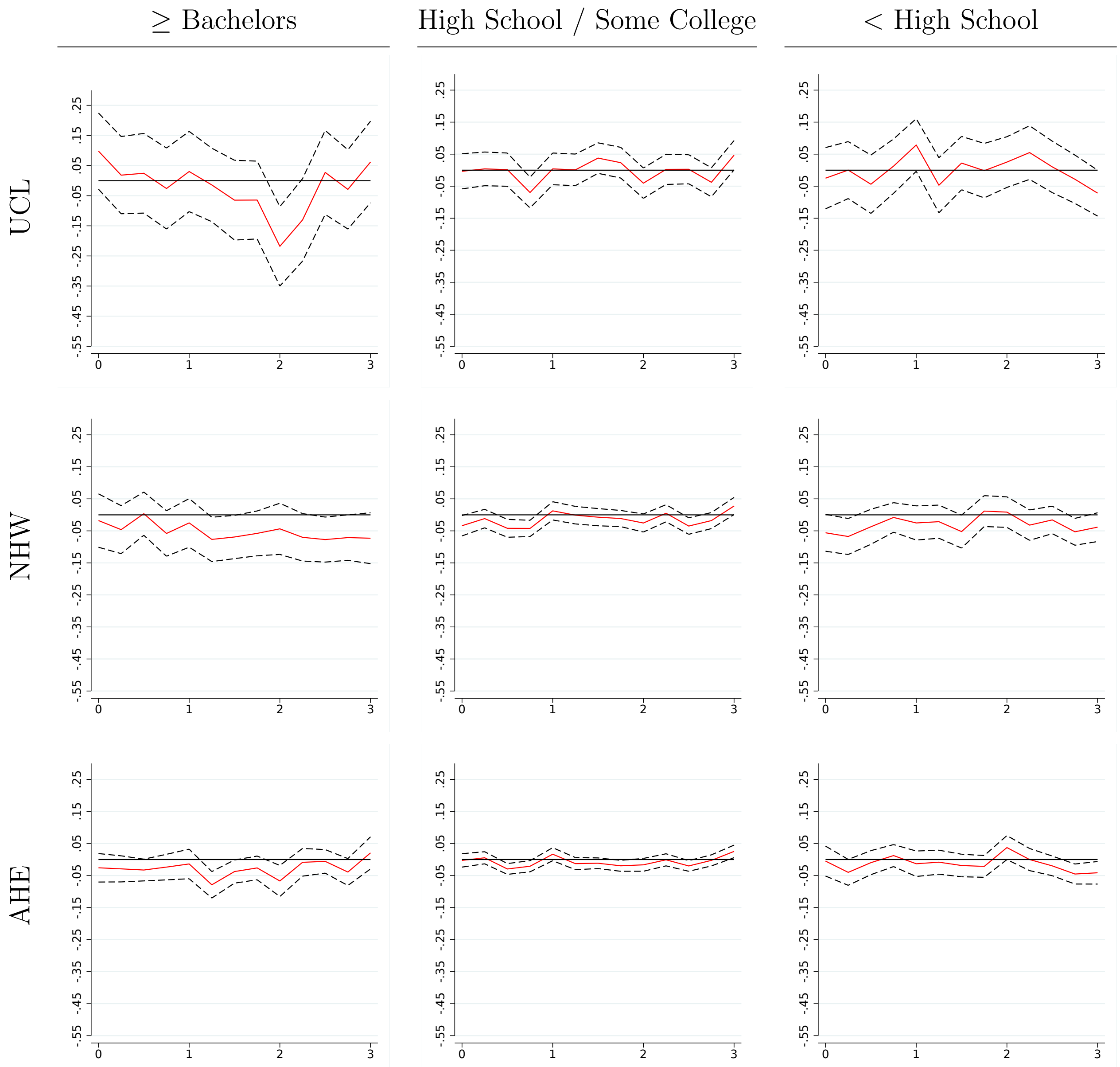
[27]The Hamilton (2018) decomposition produces a smoother cycle and more volatile trend than the HP decomposition. A consequence is that regressions that use Hamilton (2018) filtered series as the cyclical driver produce smaller and less- statistically significant coefficients.

Figure A1: Romer and Romer (2004) Shocks Excluding Pre-1979 Data



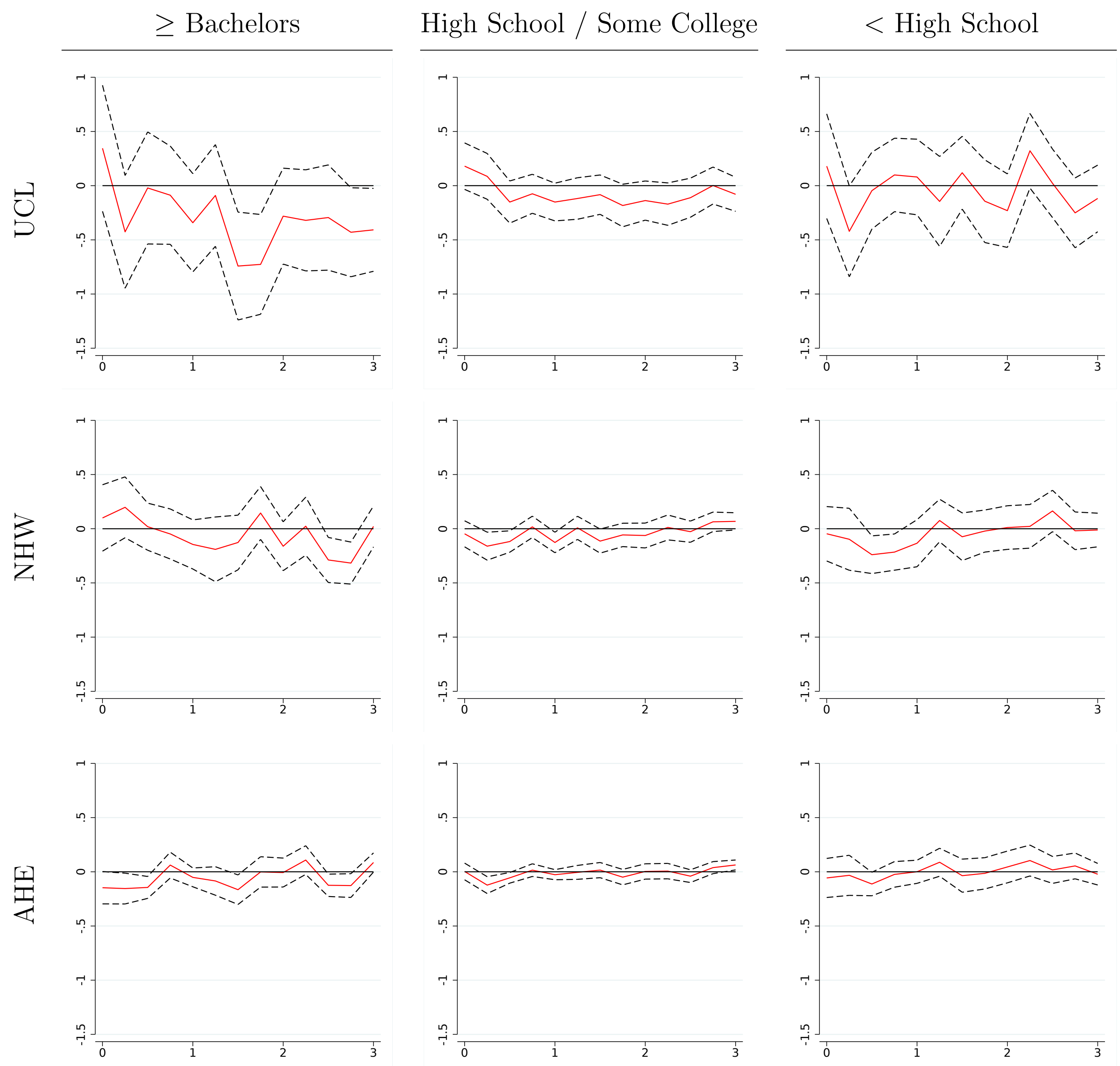
Note: 95% confidence interval.
Source: National Longitudinal Survey of Youth 1979, Current Population Survey, Greenbooks as cleaned by Coibion et al. (2012); author's calculations.

Figure A2: Romer and Romer (2004) Excluding the Volcker Reform



Note: 95% confidence interval.
Source: National Longitudinal Survey of Youth 1979; Current Population Survey; Greenbooks as cleaned by Coibion et al. (2012); author's calculations.

Figure A3: High-Frequency Identification (1990-2007)



Note: 95% confidence interval.
Source: National Longitudinal Survey of Youth 1979; Current Population Survey; monetary policy shocks as identified by Gertler and Karadi (2015); author's calculations.

D Welfare

D.1 Second-Order Approximation to the Welfare Cost

Time subscripts are suppressed for notational convenience.

$$
\begin{aligned}
Welfare\, Cost =& \mathbb{E}\left[\frac{U(C,L_v)-U(\bar{C},\bar{L}_v)}{\bar{U}_C\bar{C}}\right] && \text{(D.1)}\\
\approx& \mathbb{E}\left[\frac{\bar{U}_C\bar{C}\{\tilde{c}+\frac{1-\sigma}{2}\tilde{c}^2\}+\bar{U}_{L_v}\bar{L}_v\{\tilde{l}_v+\frac{1-\sigma}{2}\tilde{l}_v^2\}}{\bar{U}_C\bar{C}}\right] && \text{(D.2)}\\
=& \mathbb{E}\left[\tilde{c}+\frac{1-\sigma}{2}\tilde{c}^2+\frac{\bar{U}_{L_v}\bar{L}_v}{\bar{U}_C\bar{C}}\{\tilde{l}_v+\frac{1-\sigma}{2}\tilde{l}_v^2\}\right] && \text{(D.3)}\\
\approx& \left(\tfrac{1-\sigma}{2}\right)\mathbb{V}[\tilde{c}]-(1-\Phi)\left(\tfrac{1+\phi}{2}\right)\mathbb{V}[\tilde{l}_v] && \text{(D.4)}
\end{aligned}
$$

where bars denote the value on the constant-gap path, tildes denote mean-zero log-deviations from this path, and $U(C, L_v)$ denotes the period utility enjoyed by a household of variety v consuming C and supplying labor L_v. Defining

$$(1-\Phi)=\frac{M\bar{R}S_v}{M\bar{P}L_v}, \tag{D.5}$$

the final line holds with equality if all output is consumed each period. Galí et al. (2007) argue that this is approximately true. In the present context I require, in addition, that output is consumed by varieties proportionately to their earnings, which is the implication of the Cobb-Douglas production technology and a household's intertemporal optimization whenever Π_v is proportionate to earnings (as assumed in the main text). I follow Galí et al. (2007) and calibrate $(1-\Phi)=exp(-0.5)$.

D.2 Measuring the Output Elasticities of Labor Varieties

Measuring the output elasticities requires comparing the factor shares, which is complicated by the fact that allocative wages are not remitted in a given period. Thus, the factor shares must be computed from the National Longitudinal Survey of Youth (NLSY) data. I calculate these as the $\{number\, employed\} * UCL$ for each educational category divided by the sum of this figure across categories. Because the NLSY is an aging cohort these shares are unstable in the early years as workers increase their educational attainment. Therefore, I compute the shares from 1986 onward. This calculation results in shares 0.15, 0.64, and 0.21 for less than high school, high school or some college, and bachelor's degree or more, respectively. Shares are biased toward the less educated to the extent that they work fewer hours. Adjustments which account for this using hours differentials observable in the Current Population Survey (CPS) data are available upon request.