

Wage Cyclicalities and Labor Market Sorting

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Abstract

This paper uses a measure of skill mismatch to separate wage flexibility from confounding variation in wages driven by differences in job quality over the business cycle. I first show that the high cyclicalities of job switchers wages goes beyond cyclical movements in skill mismatch. Then, I uncover large differences in wage cyclicalities across the skill mismatch distribution. Among incumbent workers, wages are acyclical in good matches but procyclical in poor matches, in particular for overqualified workers.

Wage rigidity has become a leading hypothesis in macroeconomic models to rationalize business cycle dynamics observed in the data. For instance, Shimer (2005) and Hall (2005) show that replacing period-by-period Nash bargaining with wage rigidity improves the ability of the standard Diamond-Mortensen-Pissarides model to account for unemployment fluctuations. Whether wage stickiness is the answer to amplify the impact of productivity shocks depends on its consistency with the data.

An extensive literature using individual level data finds little cyclical variation in wages of incumbent workers but large movements in wages of new hires. Pissarides (2009) shows that the latter is the relevant measure to empirically assess wage rigidity. However, higher wages among new hires during expansions may capture selection into better jobs (Gertler, Huckfeldt and Trigari, 2020).

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At the same time, recessions have a cleansing effect in the labor market, whereby match quality improves for job stayers (Baley, Figueiredo and Ulbricht, 2020), countering the small response of their wages to macroeconomic shocks. Disentangling wage flexibility from worker reallocation over the cycle is a well recognized challenge when measuring wage cyclicality.

In this paper, I use a direct measure of skill mismatch—the misalignment between workers’ abilities and job requirements—to account for compositional effects due to sorting dynamics in the labor market. The analysis makes two contributions. First, I show that only a small part of the large variation of job switchers wages are driven by skill mismatch. Second, I uncover large differences in wage cyclicality along the skill mismatch distribution. I find that, for incumbent workers and new hires from unemployment, the degree of wage rigidity depends on how misaligned skills are relative to job skill requirements: wages are acyclical for well-matched workers, but procyclical for those at the top of the skill mismatch distribution. The increase in wage cyclicality along the skill mismatch distribution is driven by overqualification.

I establish these facts using a worker-level panel that has been used before in the literature, the 1979 National Longitudinal Survey of Youth (NLSY79) supplemented with O*NET data. Using this dataset allows me to compute a skill mismatch index (Baley, Figueiredo and Ulbricht, 2020; Guvenen et al., 2020), that I use to account for cyclical movements in match quality. This index is defined as the difference between workers abilities in a set of skills and how intensive these skills are required by their job. Importantly, I provide new evidence showing that skill mismatch is negatively associated with job duration, a measure used in the literature as a proxy for job quality (Bowlus, 1995). Specifically, I discover that the worst-matched workers face a separation probability that is, on average, 1.24 times higher than the best-matched workers. To study wage cyclicality, I depart from the typical specification in the literature that relies on within-individual variation in wages and the unemployment rate across months individuals are employed. While the individual fixed effect accounts for selection based on unobserved characteristics with a time-invariant effect on earnings, I address selection bias due to sorting into lower paying occupation/industries during recessions by controlling for industry-year and occupation-year effects.

I start by revisiting the results in the literature. In line with Bils (1985), among others, I find

that new hires' wages are more cyclical than those of job stayers: an increase in the unemployment rate by 1pp is associated with a fall in the real wage of 0.67% for job stayers and 2.2% for new hires. Moreover, when I distinguish between new hires out of unemployment and those coming from other jobs, I show that new hires' excess cyclicalities are driven by the latter, consistent with Gertler, Huckfeldt and Trigari (2020). Next, I use the skill mismatch index to account for worker reallocation over the cycle. In doing so, I find that previous findings in the literature remain unchanged: cyclical variation in job stayers' wages remains small while wages of job switchers remain more cyclical than those of job stayers, implying that wage changes at the time of entry go beyond cyclical movements in skill mismatch, in line with Bellou and Kaymak (2021).

Finally, I document large differences in wage cyclicalities along the skill mismatch distribution. Specifically, for incumbent workers and new hires from unemployment, I discover that wage cyclicalities are increasing in skill mismatch. For well-matched workers, the wage semi-elasticity is not statistically different from zero. In contrast, wages of job stayers and new hires from unemployment at the top of the skill mismatch distribution reduce by about 2% and 3% for, respectively, following a 1pp increase in the unemployment rate. Results suggest that the increase in wage cyclicalities along the mismatch distribution is driven primarily by overqualification: the higher are workers' abilities relative to what the job requires, the more sensitive wages are to economic conditions.

Related Literature The documented evidence primarily relates to a large literature measuring wage cyclicalities. Previous work, starting with Bils (1985), has found strongly procyclical wages for new hires (Shin, 1994; Solon, Barsky and Parke, 1994; Barlevy, 2001; Shin and Solon, 2007; Carneiro, Guimarães and Portugal., 2012). More recently, Haefke, Sonntag and van Rens (2013) and Gertler, Huckfeldt and Trigari (2020) focus on individuals hired from the pool of non-employed, the key hiring flow to generate unemployment volatility in search and matching models with sticky wages. While the former suggest that wages of new hires from non-employment adjust almost one-to-one with economic conditions, the latter find no evidence of wage cyclicalities. Differences in match composition between recessions and booms, however, hinder the interpretation of these results. Building on this work, I use a skill mismatch index to separate wage flexibility from

movements in match quality over the business cycle.

Others have controlled for the cyclical job up and downgrading using firm-occupation, job title and/or firm fixed effects (Carneiro, Guimarães and Portugal., 2012; Stüber, 2017). This work complements theirs in two dimensions. First, I leverage on a skill mismatch index to correct for job composition over the cycle. Second, I present new evidence that the wage response to business cycle conditions is increasing in skill mismatch. Close to this paper, Bellou and Kaymak (2021) also show that job switchers' wage cyclicalities go beyond match quality cyclicalities. They rely on the best and worst economic conditions experienced during the employment spell to control for match quality. Using the skill mismatch index allows to overcome potential multicollinearity issues that arise in their wage regression and account for movements in match quality driven both by the separation and hiring margins. More recently, Grigsby, Hurst and Yildirmaz (2021) account for job composition using a matching estimator and find that new hires' wages are no more cyclical than those of job stayers. This result looks at odds with the evidence in this paper suggesting that job switchers' wages are more cyclical than wages for job stayers. However, Grigsby, Hurst and Yildirmaz (2021) study excess cyclicalities in the nominal base wage, i.e. the rate specified in the labor contract. I focus instead on flexibility in realized compensation, that includes other forms of payment such as tips, overtime and bonuses. To the extent that the high cyclicalities in new hires' wages is driven by forms of compensation other than the base wage, a similar flexibility of the nominal base wage between new hires and job stayers can be reconciled with excess cyclicalities in hourly earnings.¹

The results also speak to a large literature on wage rigidity and unemployment fluctuations. The small variation of aggregate real wages over the business cycle has motivated the introduction of wage rigidity into quantitative macroeconomic models. Following Shimer (2005) and Hall (2005), others have shown that, in labor search models in the tradition of Diamond-Mortensen-Pissarides, wage rigidity for new hires can explain unemployment fluctuations (for example, Gertler and Trigari, 2009; Blanchard and Galí, 2010). Wage stickiness plays also an important role in amp-

¹Although it would be interesting to study how relevant cyclicalities in each form of compensation are to job switchers' excess cyclicalities, NLSY does not provide a clear wage decomposition. Grigsby, Hurst and Yildirmaz (2021) document small variations in overtime and bonus compensation for a sample of job stayers, providing no evidence for new hires.

lifying unemployment fluctuations in New Keynesian models. Key contributions include Christiano, Eichenbaum and Evans (2005) and Smets and Wouters (2007). This paper shows that the degree of rigidity in wages differs along the skill mismatch distribution. To the extent that the outcomes of macroeconomic models with exogenously imposed rigidities can differ substantially from those in models where rigidities are driven by micro-foundations, this paper emphasizes the importance of heterogeneity in wage cyclicalities that these models should account for.

Outline The remainder of this paper proceeds as follows. The next section describes the data. Section II and III introduce the measure of skill mismatch and the empirical methodology, respectively. Section IV presents the results and Section V concludes.

I. Data

The analysis relies on a worker-level panel from the NLSY79 supplemented with O*NET data. The dataset covers the period from 1979 to 2016. NLSY79 is a nationally representative longitudinal survey whose respondents were between 14 to 21 years old at the time of the first interview. Although others have used the Current Population Survey (CPS) (Haefke, Sonntag and van Rens, 2013) or the Panel Study of Income Dynamics (PSID) (Barlevy, 2001) to estimate wage cyclicalities, I opt for the NLSY79 for three reasons. First, by tracking individuals over time, I can isolate unobserved permanent heterogeneity at the individual level. This is not possible using CPS, a repeated cross-section. Second, if a worker holds more than one job, NLSY79 keeps a separate record for each job, as opposed to PSID that reports the average wage. Third, NLSY79 contains unique information on individual's abilities.

Sample I use the NLSY79 core sample, designed to be nationally representative, excluding thereby the military sample and the supplemental sample designed to over-represent minorities.² For com-

²From the core sample, I exclude individuals that served in the military more than two years because their military experience is not representative of the standard civilian experience. Moreover, albeit military is a career, it is difficult to translate the relevant years of experience that may apply to non-military jobs. I further exclude respondents weakly attached to the labor market (spending more than 10 years without a job) and those working more than 1200 hours in 1979, for whom I cannot observe the full labor market experience, a key determinant of wages and match

parability with the existing literature, I restrict my focus to males older than 20 years old working more than 75 hours in a month. Panel A of Table 1 characterizes the sample: individuals are, on average, 36 years old, around 83% are white and almost 40% have a college degree.

Wages and employment Using data on individuals’ weekly labor status, I build a monthly panel at the worker level as in Baley, Figueiredo and Ulbricht (2020). To do so, I focus on respondents’ main job, defined as the job in which they spend most working hours within a given month. I then identify *job stayers*, individuals employed at time t and $t - 1$ with the same employer; *job switchers*, individuals employed at time t and $t - 1$, but with a different employer; and *new hires from unemployment*, non-employed workers at $t - 1$ (i.e. reported to be not working, unemployed or out of the labor force) but employed at t . Panel B of Table 1 shows that, on average, individuals transition to a new job around 10 times, out of each 5 correspond to a job switch and the remaining to transitions from unemployment to employment.

For each employment spell, I have information on the hourly wage, which includes tips, overtime and bonuses. This means that the empirical exercise studies compensation flexibility rather than flexibility in the base wage specified in the employment contract as in Grigsby, Hurst and Yildirmaz (2021). Wages are trimmed at the top and bottom 0.1% of observations and measured in 2000 dollars using the consumer price index from the Bureau of Labor Statistics (BLS). I also observe three-digit occupation and industry codes. Occupation codes are converted into a time consistent classification system (Dorn, 2009) and industry codes to the Census 1970 one-digit codes using Guvenen et al. (2020)’s crosswalk. To mitigate measurement error, for each job, I assign the occupation/industry code that are most often observed during a job spell. This approach follows Kambourov and Manovskii (2008), in which an occupation/industry switch is only considered genuine if it is accompanied with a job transition. Therefore, even though a worker can switch jobs without changing occupation, whenever she switches 3-digit occupation, she is simultaneously transitions to a new job. In the data, 64% of all new hires (job switchers and new hires from unemployment) are observed to switch occupation.

quality. Finally, I drop respondents with missing ASVAB test scores. The Online Appendix shows that these criteria have no bearing on the results.

Economic conditions I use the national unemployment rate for individuals older than 16 from the BLS. From 1979 to 2016, the unemployment rate was 6.4%, on average, varying from 3.8% to 10.8%.

II. Skill Mismatch

I employ a skill mismatch index (Baley, Figueiredo and Ulbricht, 2020; Guvenen et al., 2020) to account for job differences over the cycle and obtain a composition-free estimate of wage cyclicality. Consider jobs and workers are characterized by J skill dimensions, $j = \{1, \dots, J\}$. Let a_i^j be worker i 's ability in skill j , and $r_{c_{it}}^j$ be the required level of skill j by the occupation individual i has in her job at time t , c_{it} . Skill mismatch between individual i and her occupation c_{it} is defined as the weighted average of the absolute difference between the worker's abilities and the skill requirements in each dimension:

$$(1) \quad m_{i,t} \equiv \sum_{j=1}^J \omega_j |a_i^j - r_{c_{it}}^j|, \quad \sum_{j=1}^J \omega_j = 1,$$

where ω_j is the j -skill weight. Throughout the analysis, I assume weights are equal across skills, but results are robust to using skill-specific weights as in Guvenen et al. (2020).

A worker may be mismatched ($m_{i,t} > 0$) because her ability exceeds the required skill and/or because her ability is below what is required. Importantly, a worker can be overqualified in one skill dimension and simultaneously underqualified in another dimension, implying that $m_{i,t}$ can be decomposed into *overqualification*,

$$m_{i,t}^+ \equiv \sum_{j=1}^J \omega_j \max\{a_i^j - r_{c_{it}}^j, 0\},$$

and *underqualification*

$$m_{i,t}^- \equiv \sum_{j=1}^J \omega_j |\min\{a_i^j - r_{c_{it}}^j, 0\}|.$$

In practice Four skill dimensions are considered, capturing both cognitive and non-cognitive

abilities important for labor market outcomes: math, verbal, technical and social (Heckman, Stixrud and Urzua, 2006; Lindqvist and Vestman, 2011). To obtain empirical measures of workers' abilities and skill requirements, I follow Guvenen et al. (2020). For workers' abilities, I leverage on scores in a subset of ASVAB test categories. After adjusting for differences in the test-taking age—by normalizing the mean and the variance by their age-specific values—, the scores are reduced into 3 skill dimensions, math, verbal and technical, using Principal Components. Social ability, in turn, relies on individual scores in the Rotter locus-of-control and the Rosenberg self-esteem scales. As NLSY79 respondents only took the ASVAB test once at the beginning of the survey, the measures of workers' abilities are time-invariant. The empirical measures of skill requirements rely on occupational level data from O*NET, which provides 277 descriptors characterizing occupations in terms of knowledge and skills required. I focus on a subset of 26 descriptors with a relatedness score to ASVAB test categories, that I use to create a O*NET analogue of each ASVAB test category by taking the weighted average of all the 26 descriptors. The obtained O*NET analogues of the ASVAB categories are then collapse into the same 3 skill dimensions (verbal, math and technical) by applying Principal Components. In a similar way, I use another 6 descriptors linked to social skills to obtain a measure of social requirements.³

As a final step, to make scores of abilities and requirements comparable, I normalize them in terms of percentile ranks. Thereby, $m_{i,t}$ ranges between 0 and 100, with lower values reflecting a lower mismatch. The data show a positive correlation between requirements and abilities across each dimension (Online Appendix, Table A.2), suggesting that workers tend to select themselves into jobs that fit abilities best, nonetheless sorting is not perfect: workers are, on average, mismatched, with the magnitude of overqualification being larger than underqualification (Panel C of Table 1). Over time, changes in $m_{i,t}$ occur whenever workers switch occupation.

Interpretation I interpret $m_{i,t}$ as a direct measure of the lack of match quality. This interpretation hinges on two empirical facts. First, Guvenen et al. (2020) document that skill mismatch reduces wages. Second, I discover that skill mismatch is negatively associated with job duration, a measure

³A list of ASVAB test categories and O*NET descriptors used as well as a mapping between NLSY79 and O*NET can be found in the Online Appendix.

used in the literature as a proxy for match quality (Bowlus, 1995). This finding relies on a discrete proportional hazard model using the complementary log model to compute the job separation hazard,

$$(2) \quad h_i(\tau) = \alpha_0 + \alpha_1 m_i + \alpha_2 U_0 + \alpha_3 U_\tau + \delta x_{i,\tau} + h_0(\tau) + \varepsilon_{i,\tau},$$

where $h_i(\tau)$ be the probability that individual i 's job ends at date τ given that it lasted until τ , $h_0(\tau)$ is the baseline hazard, parameterized as $\ln(\tau)$, m_i is skill mismatch on the current job, U_0 is the unemployment rate at the start of the job relationship, U_τ is the current unemployment rate, and $x_{i,\tau}$ is a set of controls including age, current wage, education, race, one-digit level occupation and industry and month fixed effects. Figure 1 plots the estimated hazard and shows that the larger is the difference between workers' skills and skill requirements, the more likely a match is to end at all tenures. Specifically, the 5% worst-matched workers face a separation probability that is, on average, 1.24 times higher than the 5% best-matched workers.

III. Measuring Wage Cyclicity

To study how wages move along the cycle, I estimate the wage semi-elasticity with respect to the aggregate unemployment rate, following the literature (Pissarides, 2009).

Baseline specification Starting with Bils (1985), the typical empirical model to assess wage cyclicity regresses the change in log wage on the change in the unemployment rate and its interaction with an indicator variable that equals one if the worker is a new hire and zero otherwise to compare wage cyclicity between job stayers and new hires. The first-differences estimator is used to account for worker time-invariant unobserved heterogeneity. This is important as Solon, Barsky and Parke (1994) show that the employment pool shifts towards high-ability workers during recessions. Others use instead a wage level equation with worker fixed effects to net out selection into employment (Carneiro, Guimarães and Portugal., 2012; Gertler, Huckfeldt and Trigari, 2020). This approach has the advantage of not restricting the exercise to workers employed for two consecutive

periods, allowing one to distinguish job switchers wage cyclicalities from that of new hires out of unemployment. I thus take this model as the baseline of the empirical exercise,

$$(3) \quad w_{i,t} = \beta_0 + (\beta_1 + \beta_2 NH_{i,t}) \times U_t + \gamma' (NH_{i,t} + controls_{i,t}) + \delta_i + \varepsilon_{i,t},$$

where $w_{i,t}$ is the log real hourly earnings of individual i at time t , U_t the unemployment rate at time t and $NH_{i,t}$ a new hire dummy. β_1 in specification (3) measures the wage semi-elasticity of job stayers and $\beta_1 + \beta_2$ corresponds to the wage semi-elasticity of new hires. The key result in the literature is that both β_1 and β_2 are significantly negative, thus new hires' wages are more sensitive to economic conditions.

This empirical framework, however, does not deal with variation in wages driven by worker reallocation that spur cyclical movements in match quality. This is important as recent evidence by Baley, Figueiredo and Ulbricht (2020) show that recessions have both a cleansing and sully effect in the labor market: jobs cleansing, improves average quality among job stayers, while bad matches are also created (sully), decreasing the average job quality among new hires. There is thus an omitted variable that enters the error term in equation (3). Match quality is positively associated with wages for all workers, but its relationship with aggregate unemployment is negative for new hires and positive for incumbent workers. Not correcting for match composition translates into an upward bias of β_1 and a downward bias of β_2 . As such, one should be careful in interpreting the high cyclicalities of new hire wages as evidence of wage flexibility, as argued by Gertler, Huckfeldt and Trigari (2020), but also in interpreting the low variability of incumbent workers' wages as suggestive of wage rigidity.

Disentangle wage cyclicalities from sorting To separate wage flexibility from compositional effects due to sorting dynamics over the business cycle, I add the skill mismatch index to specific-

ation (3). Specifically, I estimate:

$$(4) \quad w_{i,t} = \beta_0 + (\beta_1 + \beta_2 NH_{i,t}) \times U_t + \beta_4 m_{i,t} + \gamma' (NH_{i,t} + x_{i,t}) + \delta_t t + \delta_m + \delta_i + \varepsilon_{i,t},$$

where $m_{i,t}$ is skill mismatch and $x_{i,t}$ is a vector of time-varying controls at the individual level including a quadratic polynomial in age and job tenure, education, and indicator variables for current occupation and industry (one-digit level codes) interacted with year. Specification (4) also includes a time-trend, month (δ_m) and individual (δ_i) fixed effects. $\varepsilon_{i,t}$ is the error term, which includes all unobserved determinants of wages for worker i at time t . Standard errors are clustered at the individual level.

Bellou and Kaymak (2021) account for confounding variation in wages due to match quality using two indirect measures—the best and worst economic conditions during the entire job duration. Using the skill mismatch index brings two advantages. First, it overcomes potential issues of multicollinearity in the wage regression. Second, it captures observed match quality dynamics in the separation and hiring margins. Bellou and Kaymak (2021) proxy match quality using the worst economic conditions during a job spell following the idea that in recessions, a match breaks if its quality is below a certain threshold \bar{m} . The underlying assumption is that no match is created below \bar{m} . While this holds under complete information, Baley, Figueiredo and Ulbricht (2020) show that, under imperfect information and learning, recessions are times where bad matches are destroyed but also created. Not taking this into account may inflate match quality for new hires in recessions in Bellou and Kaymak (2021)’s setting, specially in short tenure. Consider the following example. Worker a was hired in a boom, worker b in a recession; both jobs last two periods. As b ’s job started during bad times, skill mismatch is higher and economic conditions are also most likely worst during her spell. Even though mismatch is larger, Bellou and Kaymak (2021) interpret b ’s match quality to be higher.

Identification The identification strategy exploits within-individual variation in unemployment

and wages across months when individuals are employed. The individual fixed effect accounts for systematic differences in the workers who move over the business cycle. Then, as skill mismatch is added as a control, the OLS estimates will not be confounded by match quality cyclicalities. Additionally, by including indicator variables that describe one-digit level industries and occupations interacted with year, equation (4) also accounts for sorting into lower-paying occupation and/or industries during bad times. The identifying assumption is that, conditional on the included covariates, changes in unemployment are uncorrelated with unobserved determinants of wages, $\mathbb{E}[\varepsilon_{i,t} \cdot U_t | m_{i,t}, x_{i,t}, \delta_m, \delta_i] = 0$. The worker fixed effect implicitly assumes that the selection process into employment is constant over time. It also implies that the level effect of mismatch is identified using information from differences in occupations individuals have over time.

IV. Results

Main results are reported in Table 2. Coefficients on U_t are multiplied by 100 and correspond to the % wage change following a 1 percentage point (pp) increase in the unemployment rate.

Revisiting previous findings In line with Bilts (1985), among others, column 1 of Table 2 shows that the coefficient interacting the new hires dummy with unemployment is significantly negative and larger than the coefficient on unemployment, implying that new hires' wages are more cyclical than those of job stayers. Column 2 adds a separate interaction term for job switchers ($EE'_{i,t}$) and new hires from unemployment ($UE_{i,t}$) and shows that wage cyclicalities of newly hired workers is driven by job switchers, consistent with Gertler, Huckfeldt and Trigari (2020). For every percentage point increase in the unemployment rate, incumbent workers' wages decrease by about 0.67%, compared to 2.53% for job switchers. Wages of new hires from unemployment, in contrast, show a response to aggregate conditions that is not statistically significant different from job stayers.

These estimates are consistent with earlier literature. Using NLSY, Bilts (1985) finds a semi-elasticity of 0.6% for job stayers, versus 3.0% for job switchers. Barlevy (2001) recovers semi-elasticities between 2.6% and 3.0% for switchers in PSID and NLSY, and Devereux (2001) of 0.8% for stayers. In Gertler, Huckfeldt and Trigari (2020), the wage semi-elasticity of job switchers is

around 1.9pp larger than that of incumbent workers based on the Survey of Income and Program Participation.

Separating wage flexibility from sorting Gertler, Huckfeldt and Trigari (2020) argue that job switchers’ excess wage cyclicality relative to stayers captures composition effects due to differences in job composition over the course of the cycle. Thus, estimates in column 2 potentially reflect omitted variable bias driven by the cleansing and sullyng effect of recessions, whereby match quality improves among incumbent workers but deteriorates among new hires during bad times. To account for confounding variation due to worker job reallocation along the cycle, column 3 in Table 2 adds skill mismatch to the regression.

First, in line with Guvenen et al. (2020), the results show that labor market earnings are negatively associated with mismatch. This corroborates the interpretation of mismatch as the lack of match quality. Regarding cyclicality, adding skill mismatch does not substantially change the wage response to economic conditions uncovered in column 2: the wage semi-elasticity of incumbent workers remains small;⁴ wages of new hires from unemployment mimic those of job stayers, whereas wages of job switchers remain more cyclical when compared to job stayers. The latter suggests that only a small variation in wages at the time of entry is in fact coundfounding variation driven by skill mismatch cyclicality, in line with Bellou and Kaymak (2021).

Heterogeneity along the mismatch distribution The results so far show that, conditional on skill mismatch, the sensitivity of job stayers’ wages to economic conditions is small. This, however, captures an average response across all incumbent workers. To explore potential heterogeneities, I run a triple-interaction regression by interacting mismatch with unemployment and the new hire

⁴The interpretation of job stayers wage cyclicality as small/acyclical relies on an implicit reference to an Okun-type relation that converts the unemployment cyclicality to output cyclicality. Following Pissarides (2009), a wage-unemployment semi-elasticity of 0.74% (column 3 of Table 2) is converted to an elasticity w.r.t. productivity of 0.25%, similar to Haefke, Sonntag and van Rens (2013), who interpret estimates of this magnitude as wages being acyclical.

dummies:

(5)

$$w_{i,t} = \beta_0 + (\beta_1 + \beta_2 EE'_{i,t} + \beta_3 UE_{i,t}) \times U_t + (\beta_5 + \beta_6 EE'_{i,t} + \beta_7 UE_{i,t}) \times U_t \times m_{i,t} + \gamma'_1 (1 + UE_{i,t} + EE'_{i,t}) \times m_{i,t} + \gamma'_2 (UE_{i,t} + EE'_{i,t} + x_{i,t}) + \delta_t t + \delta_m + \delta_i + \varepsilon_{i,t},$$

Column 4 of Table 2 shows that the level effect of unemployment, no longer statistically significant, is replaced by a negative effect that depends on skill mismatch, significant at 1% level.⁵ Panel A in Figure 2 plots the implied wage semi-elasticities (multiplied by -1) along the skill mismatch distribution.

One important pattern stands out: the extent to which wages move over the cycle depends on the worker position in the mismatch distribution. For well-matched job stayers, the wage semi-elasticity is not statistically different from zero. In contrast, for the worst-matched job stayers, wages decrease by 1.7% in response to a 1pp increase in the unemployment rate, an estimate significant at the 1% level. The difference with respect to the bottom is around 1.6pp and is statistically different from zero. Wage cyclicity of new hires from unemployment exhibits a similar pattern. For workers at the bottom 10% of the mismatch distribution, wages do not respond to changes in business cycle conditions, while for those at the top 10% face a 3.3% decrease in wages when the unemployment rate increases by 1pp.

For job switchers, results show that, both at the bottom and at the top of the skill mismatch distribution, wages change following a 1pp decrease in unemployment. However, the wage semi-elasticity of the 10% worst-matched workers is around 1.6pp larger when compared to the 10%-best matched ones. This difference is similar in magnitude to what one observes among job stayers, but, given the large standard error (1.11), it is imprecisely estimated. Thus, for job-to-job transitions, the results provide small evidence of the increase in wage cyclicity with skill mismatch.

⁵In specification (5), the coefficient on U_t measures wage cyclicity of perfectly matched job stayers ($m_{i,t} = 0$). The estimated coefficient, albeit not statistically different from zero, is positive. Nonetheless, mismatch in the sample is always larger than 0. For instance, for workers at the 5th percentile of the distribution of mismatch, the estimates reported in column 4 imply a wage semi-elasticity of 0.07%, very close to zero.

Overqualification or underqualification?— Is the increase in wage cyclicality driven by over- or underqualification? To answer this question, I rerun regression (5) but now splitting $m_{i,t}$ into overqualification ($m_{i,t}^+$) and underqualification ($m_{i,t}^-$). Panel B and C of Figure 2 plot the wage semi-elasticity (multiplied by -1) computed from estimates displayed in column 5 of Table 2. As workers can be overqualified in some skills and simultaneously underqualified in others, to gauge the effect of overqualification, Panel B displays the wage semi-elasticity along the overqualification distribution, computed at the average level of underqualification. In a similar way, Panel C displays the wage semi-elasticity along the underqualification distribution, computed at the average level of overqualification.⁶

I discover that wage cyclicality increases with overqualification, but not with underqualification. For job stayers at the bottom of the overqualification distribution, the wage semi-elasticity is insignificantly different from zero. Moving from the bottom 10% to the top 10% of the distribution increases wage cyclicality by 2pp, a magnitude significant at the 1% level. This implies that the 10% most overqualified workers see their wages increase by 2% when the unemployment rate decreases by 1pp. In contrast, the wage-semi elasticity remains roughly unchanged along the underqualified distribution. The same pattern is observed among new hires from unemployment. Wages of workers at the bottom 10% of the overqualification distribution do not respond to economic conditions, as opposed to wages of those at the top 10%, for whom the wage semi-elasticity is of 3% and is significant at the 1% level. In turn, underqualification does not change the wage sensitivity to economic conditions.

Robustness Figure 3 shows that the documented findings are robust to alternative specifications. First, as short non-employment spells may be instead job-changers taking a short break, I recode jobless spells shorter than 3 months as job switchers. Second, I exclude workers that return to their previous employer from the pool of new hires. Third, I use skill-specific weights to compute a weighted measure of skill mismatch, in which weights correspond to the factor loadings from

⁶The Online Appendix shows that these results are independent of the distribution moment used to compute the wage semi-elasticity plotted in Panel B and C of Figure 2.

the first principal component, normalized to sum to one, as in Guvenen et al. (2020): (verbal, math, technical, social) = (0.32, 0.32, 0.31, 0.05). I also use information on the region of residence (Northeast, North Central, South and West) and exploit both cross-region and time-series variation in economic conditions. Finally, I show that results are robust to additional controls. In particular, I estimate specifications that include separately occupation skill requirements, occupational tenure (cubic polynomial), and cumulative past mismatch (Guvenen et al., 2020). The latter is defined as the average mismatch in previous occupations and its inclusion aims to account for potential long-run effects of past jobs operating through dynamic decisions such as human capital accumulation.

Empirical concerns Skill mismatch measures the average difference between a worker’s abilities and the skills required by the occupation in the current job. Hence, the definition of a match in the analysis is closely tied to that of an occupation. This relates to the job concept in theoretical frameworks of multidimensional sorting in which workers differ along multiple skill dimensions and sort into jobs characterized by (fixed) heterogeneous skill requirements along those dimensions (Lise and Postel-Vinay, 2020; Lindenlaub and Postel-Vinay, 2020; Baley, Figueiredo and Ulbricht, 2020). Nonetheless, jobs may be characterized by other attributes not captured by the skill mismatch index. As such, one might be worried that cyclical variation in match quality depends on these other job attributes, biasing wage cyclicality. As wages and match quality are positively correlated, if other job attributes improve in recessions among job stayers, in line with the cleansing effect, the coefficient on U_t in regressions (4)-(5) will be biased towards zero. Therefore it provides a lower bound for the magnitude of the level effect of unemployment on wages. Importantly, this is not a concern for the coefficient on the interaction term $M_t \cdot U_t$ in regression (5). Unless, this selectivity of jobs is somehow skill mismatch dependent. To further address this concern, I compare the separation hazard into unemployment in recessions and expansions across high and low tenure jobs. Importantly, both hazards are cleaned of the effects of skill mismatch among other observables. I find that, conditional on skill mismatch, recessions increase separations into unemployment by roughly the same proportion at all tenures (Online Appendix, Figure E.1). Under the assumption that, for a given level of mismatch, high tenure jobs are high quality matches due

to other attributes missed by skill mismatch, I interpret this pattern as suggestive evidence that match quality cyclicalities are not driven by these other job attributes. In contrast, if match quality was indeed higher in recessions because matches with poor attributes, other than mismatch, are destroyed, the cleansing effect of recessions, conditional on mismatch, would be decreasing in tenure, i.e. separations at low tenure jobs would increase by a much greater proportion than at high tenure jobs. A constant ratio between the separation hazard in recessions and expansions thus suggests that results are not confounded by changes in match quality driven by other relevant aspects not captured by skill mismatch.

Another concern is that wages are lower in recessions because overall jobs and hiring shifts towards firms that pay low wages. So, even if wages do not change, average wages will fall during bad times. Due to data restrictions, I cannot control for firm heterogeneity. Nonetheless, Carneiro, Guimarães and Portugal. (2012) show that introducing firm fixed effects has a small effect on wage cyclicalities. Therefore, concerns regarding changes in firm composition over the cycle are small.

V. Conclusion

This paper revisits the issue of wage cyclicalities while accounting for the cleansing and sully effect of recessions using a skill mismatch index. I first show that the high cyclicalities of job switchers' wages go beyond skill mismatch cyclicalities. Then, I find that, for job stayers and new hires from unemployment, wage cyclicalities increase in skill mismatch: for well-matched workers wages do not adjust to macroeconomic shocks, while for the worst-matched workers wages are procyclical.

The documented findings highlight a heterogeneity that is not accounted for in models that exogenously prevent wages from adjusting. This is important to the extent that the outcomes of macroeconomic models with exogenously imposed rigidities can differ substantially from those in models where rigidities are micro-founded, as underlined by Rudanko (2009). Moreover, the increase in wage cyclicalities along the mismatch distribution suggests that propagation of positive and negative shocks may be asymmetric as the distribution of matches moves towards good or bad matches.

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TABLE 1: DESCRIPTIVE STATISTICS OF THE SAMPLE, NLSY79, 1979-2016

	Mean	Standard Deviation
Panel A: Sample characteristics		
Age (years)	35.85	9.75
Non-white	0.17	0.37
College graduate	0.37	0.48
Panel B: Labor Market Outcomes		
Job tenure (months)	54.56	65.04
Labor market experience (years)	15.88	9.23
Hourly wage (log)	7.37	0.71
Total # transitions (EE+UE)	9.66	6.27
Total # EE transitions	4.98	3.95
Panel C: Mismatch		
Skill Mismatch	27.22	14.53
Overqualification	16.27	16.32
Underqualification	10.95	13.31
Total # individuals		1515
Total # observations		381954

Note: Panel A presents main characteristics of the sample, Panel B reports summary statistics on individual labor market outcomes and Panel C on skill mismatch ($m_{i,t}$), overqualification ($m_{i,t}^+$) and underqualification ($m_{i,t}^-$). *EE* stands for job-to-job transitions, *UE* stands for transition through an unemployment spell.

TABLE 2: WAGE CYCLICALITY

Dependent variable: Log hourly wage					
	(1)	(2)	(3)	(4)	(5)
U_t	-0.665 (0.210)	-0.672 (0.210)	-0.738 (0.210)	0.467 (0.477)	0.396 (0.478)
$U_t \cdot NH_{i,t}$	-1.501 (0.318)				
$U_t \cdot EE'_{i,t}$		-1.858 (0.456)	-1.826 (0.456)	-1.809 (0.992)	-2.272 (0.973)
$U_t \cdot UE_{i,t}$		-0.747 (0.464)	-0.712 (0.462)	1.241 (1.156)	0.317 (1.173)
$m_{i,t}$			-0.0014 (0.0005)	0.002 (0.001)	
$U_t \cdot m_{i,t}$				-0.044 (0.016)	
$U_t \cdot m_{i,t} \cdot EE'_{i,t}$				0.002 (0.029)	
$U_t \cdot m_{i,t} \cdot UE_{i,t}$				-0.060 (0.033)	
$U_t \cdot m_{i,t}^+$					-0.057 (0.016)
$U_t \cdot m_{i,t}^+ \cdot EE'_{i,t}$					0.021 (0.030)
$U_t \cdot m_{i,t}^+ \cdot UE_{i,t}$					-0.037 (0.034)
$U_t \cdot m_{i,t}^-$					-0.017 (0.020)
$U_t \cdot m_{i,t}^- \cdot EE'_{i,t}$					0.034 (0.036)
$U_t \cdot m_{i,t}^- \cdot UE_{i,t}$					0.011 (0.043)
Observations	384094	384094	381394	381394	381394
Adjusted R^2	0.648	0.648	0.649	0.650	0.650

Note: $NH_{i,t}$, $EE'_{i,t}$ and $UE_{i,t}$ equal one for new hires, switchers and new hires from unemployment, respectively. $m_{i,t}$, $m_{i,t}^+$ and $m_{i,t}^-$ correspond to skill mismatch, over- and underqualification in the current job, respectively. Coefficients and standard errors on U_t multiplied by 100. All columns control for a quadratic polynomial in age and job tenure, education, time trend, month and individual fixed effects, and one-digit level occupation and industry interacted with year. Standard errors are clustered at the individual level. Sample includes all worker-job matches between 1979 and 2016 in a sub-sample of NLSY79.

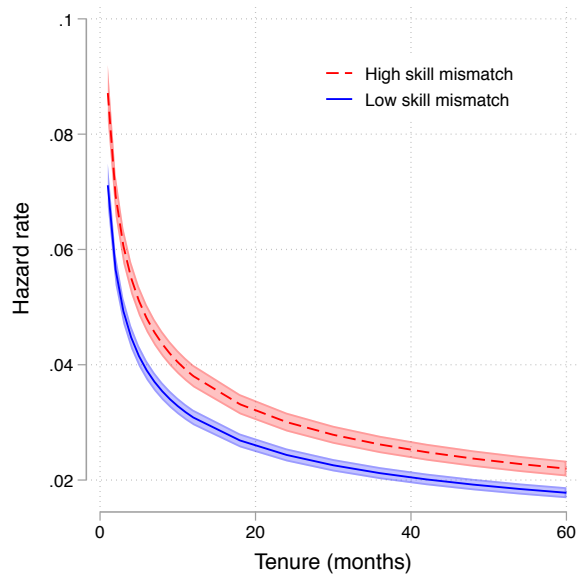
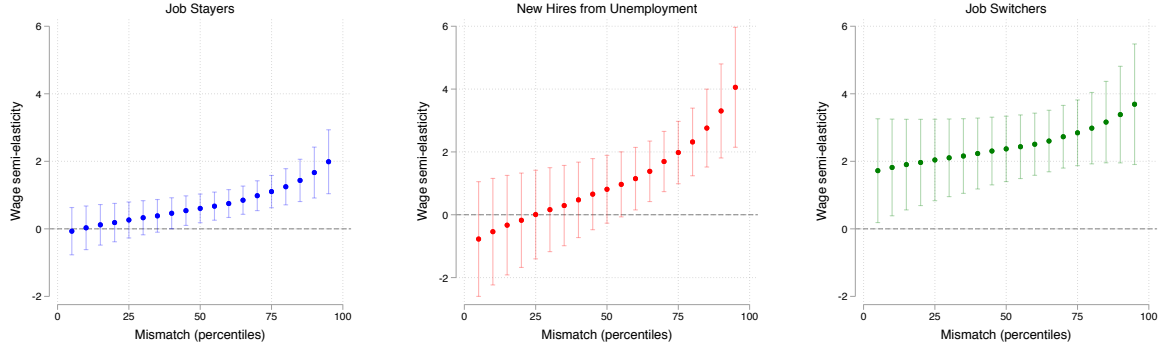


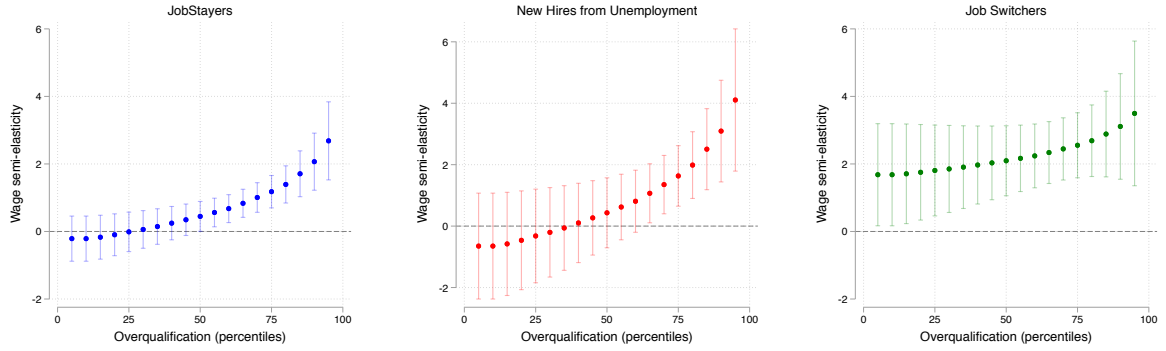
FIGURE 1: SEPARATION HAZARD AND MISMATCH

Note: The graph plots the estimated separation hazard. *Low* and *High skill mismatch* correspond to the 5th and 95th percentile of the skill mismatch distribution. Controls include current unemployment rate, unemployment rate and age (cubic) at the start of the spell, current wage, and indicator variables for education, race, one-digit industry, one-digit occupation and month. Shaded areas represent 95% confidence intervals. Sample includes all worker-job matches between 1979 and 2016 in a sub-sample of NLSY79.

Panel A. Skill Mismatch



Panel B. Overqualification



Panel C. Underqualification

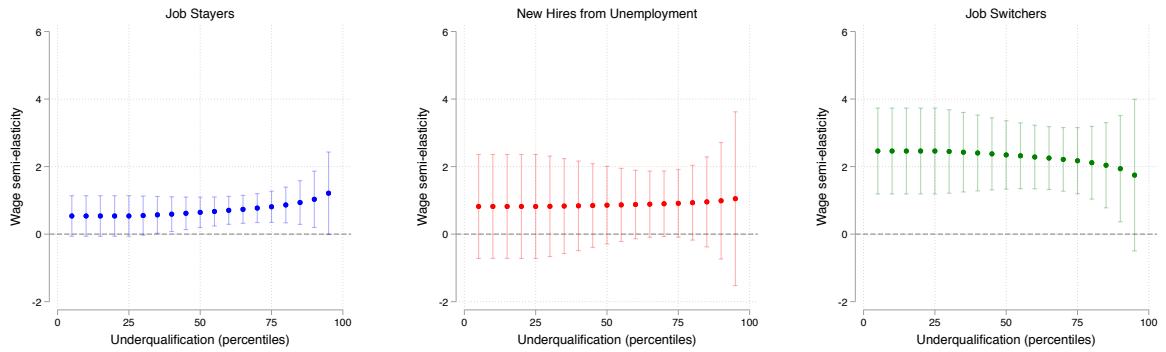
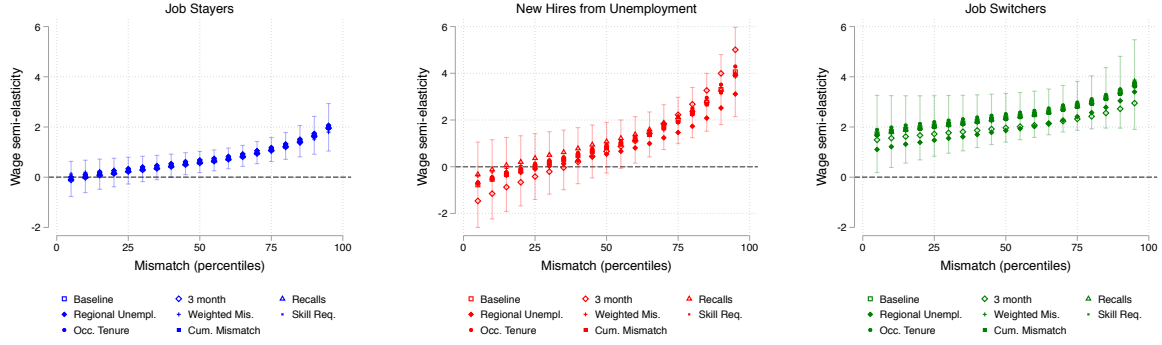


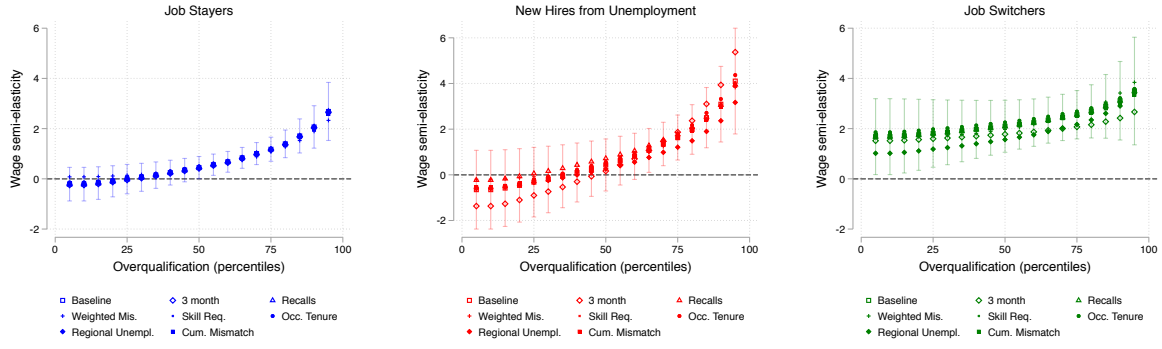
FIGURE 2: HETEROGENEITY IN WAGE CYCLICALITY

Note: Each graph plots the % wage change in response to a 1pp drop in the unemployment rate along the distribution of skill mismatch (Panel A), overqualification (Panel B) and underqualification (Panel C). Wage-unemployment semi-elasticities computed using the estimates reported in Table 2, multiplied by -1. Panel B plots the wage semi-elasticity along the overqualification distribution, computed at the average level of the underqualification. Panel C plots the wage semi-elasticity along the underqualification distribution, computed at the average level of overqualification. Solid lines are 95% confidence intervals. Sample includes all worker-job matches between 1979 and 2016 in a sub-sample of NLSY79.

Panel A. Skill Mismatch



Panel B. Overqualification



Panel C. Underqualification

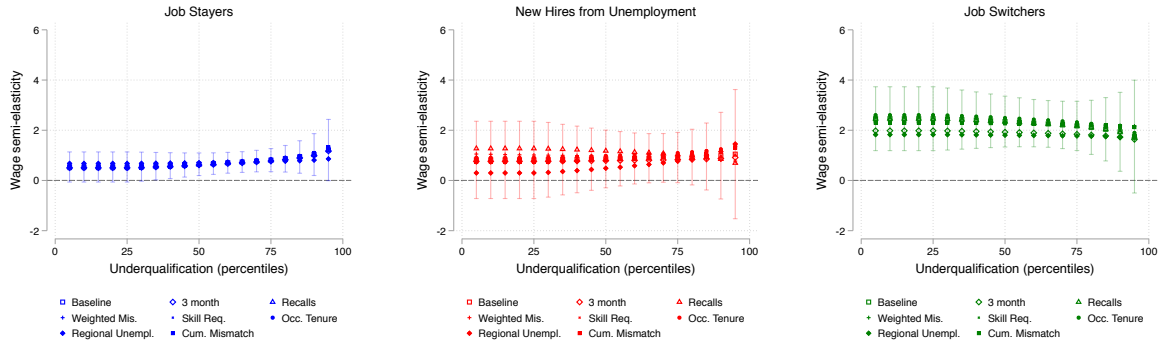


FIGURE 3: HETEROGENEITY IN WAGE CYCLICALITY: SENSITIVITY TO ALTERNATIVE SPECIFICATIONS

Note: Each graph plots the % wage change in response to a 1pp drop in the unemployment rate along the distribution of skill mismatch (Panel A), overqualification (Panel B) and underqualification (Panel C). Wage-unemployment semi-elasticities are computed using estimates on Table 2 (baseline) and Tables C.1-C.3 in the Online Appendix (all alternative specifications), multiplied by -1. Panel B plots the wage semi-elasticity along the overqualification distribution, computed at the average level of the underqualification. Panel C plots the wage semi-elasticity along the underqualification distribution, computed at the average level of overqualification. Solid lines are 95% confidence intervals for the baseline specification. Sample includes all worker-job matches between 1979 and 2016 in a sub-sample of NLSY79.