The Unexpected Compression: Competition at Work in the Low Wage Labor Market*

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Abstract

Labor market tightness following the height of the Covid-19 pandemic led to an unexpected compression in the US wage distribution that reflects, in part, an increase in labor market competition. Rapid relative wage growth at the bottom of the distribution reduced the college wage premium and counteracted approximately one-quarter of the four-decade increase in aggregate 90-10 log wage inequality. Wage compression was accompanied by rapid nominal wage growth and rising job-to-job separations—especially among young non-college (high school or less) workers. At the state-level, post-pandemic labor market tightness became strongly predictive of price increases (price-Phillips curve), real wage growth among low-wage workers (wage-Phillips curve), and aggregate wage compression. Simultaneously, the wage-separation elasticity—a key measure of labor market competition—rose among young non-college workers, with wage gains concentrated among workers who changed employers and industries. Seen through the lens of a canonical job ladder model, the pandemic increased the elasticity of labor supply to firms in the low-wage labor market, reducing employer market power and spurring rapid relative wage growth among young non-college workers who disproportionately moved from lower-paying to higher-paying and potentially more-productive jobs.

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Introduction

A vast economic and sociological literature studies the contributions of technology, trade, and institutions to four decades of rising inequality in the United States (Katz and Murphy, 1992a; Katz et al., 1999; DiNardo et al., 1996; Autor et al., 2008, 2016). The role played by the competitive structure of the labor market, and its empirical manifestation in worker reallocation across jobs, has received comparatively less attention. Yet, there is reason to suspect a connection. A growing literature documents the importance of imperfect labor market competition in US workers' pay determination: facing labor supply curves that are far from perfectly elastic, many firms are able to mark down wages below competitive levels (Manning, 2021; Bassier et al., 2022; Datta, 2022; Lamadon et al., 2022; Yeh et al., 2022). The secular decline in job-to-job transitions in the United States, especially since 2000 (Bjelland et al., 2011; Hyatt and Spletzer, 2013), may be one symptom of this phenomenon: workers whose wages are set infra-marginally will be less likely to separate from their current job in response to wage fluctuations. A corollary of this observation is that if the firm-level labor supply elasticity—or its doppelgänger, the 'quit elasticity'—were to rise, the magnitude of wage markdowns would fall and the worker reallocation rate from lower- to higher-paying employers would increase.[1]

This paper studies the rapid evolution of the US labor market in the years immediately before and after the Covid-19 pandemic to understand the role of rising labor market tightness in driving wage compression, boosting the quit elasticity, and augmenting competition in the low-wage labor market. We begin with an overview of how the Covid-19 pandemic and subsequent recovery have disrupted longstanding trends in aggregate wage inequality. Following the pandemic, substantial nominal wage growth at the bottom of the distribution reversed approximately one-quarter of the rise in 90-10 wage inequality since 1980 and led to a fall in the college/high school wage premium. This wage compression was accompanied by a rise in the rate of job-to-job transitions—especially among young non-college workers.

To understand the role of market competition in these wage patterns, we exploit cross-state variation in post-pandemic labor market tightness, where tightness is measured using variation in state-level unemployment and employment-to-employment (EE) separation rates. In tighter labor markets, wage compression was greater, local price increases larger, and real wage growth among low-wage workers faster, particularly among young non-college workers. Job-shopping appears to play a key role in wage growth: alongside unemployment, job-to-job transition rates exhibit independent power for predicting cross-state wage growth,

[1] We use the term 'quit elasticity' throughout the paper to refer to what is formally the employment-to-employment (EE) separation elasticity. We also use the terms EE separations and job-to-job separations interchangeably.

reflecting movements along state-level wage-Phillips curves (Moscarini and Postel-Vinay, 2017; Cerrato and Gitti, 2022).

We next explore the potential role of labor market competition in driving these trends by assessing whether the EE separation elasticity, a key indicator of employer market power, has risen in the post- versus pre-pandemic labor market. The EE separation elasticity is informative because it reflects the ability of employers to retain workers who, according to observable characteristics, are paid relatively low wages (for a recent discussion of the theory and a review of evidence, see Langella and Manning (2021)). Canonical job ladder models, discussed in Section 2, predict that the EE separation elasticity will increase when the unemployment rate falls, and when the 'contact rate' between currently employed workers and external firms rises (Burdett and Mortensen, 1998). In both cases, the transition rate of employed workers from lower- to higher-wage jobs will grow. We also discuss non-model-based reasons for why the separation elasticity may have grown following the pandemic, including: decreased worker-firm attachment spurred by mass job loss during the pandemic; increased household liquidity deriving from pandemic transfer programs; changes in the perceived quality of low-wage work; and shifting perceptions about the availability of higher-wage jobs.

Empirically, we show that the quit elasticity for young workers and for non-college workers has risen. We further document that around half of the wage increase at the bottom of the wage distribution is associated with job change rather than same-job wage growth. This pattern of wage growth suggests that increased competition has led to a reallocation of jobs from low-wage to higher-wage employers. It is also consistent with evidence from payroll data that the employment share of larger firms has recently risen.[2] We interpret this collage of evidence as indication that the Covid-19 pandemic increased the elasticity of labor supply to firms in the low-wage labor market, reducing employer market power and spurring rapid relative wage growth among young non-college workers who disproportionately moved from lower-paying to higher-paying and potentially more-productive jobs.

A number of recent papers explore the relationship between labor market tightness and employer market power. Hirsch et al. (2018), Webber (2022), and Bassier et al. (2022) document the countercyclicality of employer labor market power, as measured by the elasticity of quits to wages. These papers do not, however, link the change in market power

[2]For example, according to the ADP National Employment Report, between January and September of 2022 the monthly employment growth at establishments with fewer than 50 workers was 0.14%, while it was 0.32% for establishments with 50 workers or more. The contrast was even starker between establishments with fewer than 20 workers (-0.05%) and those with 500 or more (0.47%). In 2019, prior to the pandemic, employment growth was close to uniform across large and small establishments. This analysis is based on data available here: https://adp-ri-nrip-static.adp.com/artifacts/us_ner/20230105/ADP_NER_history.zip?_ga=2.200523629.2113869569.1674944927-96224259.1674944926

to reallocation or wage inequality. Similarly, focusing on the pre-pandemic labor market, Bivens and Zipperer (2018) and Baker and Bernstein (2013) show that higher employment rates are associated with greater wage compression but do not consider the role of labor market competition and job change in mediating that role.[3] This rapid change in labor market conditions provides a fertile ground for exploring the tightness-competition-reallocation mechanism. Our work is also related to Cerrato and Gitti (2022), who show that a steepening post-pandemic price-Phillips curve contributes to rising inflation. While our focus is on wage inequality rather than price levels, we affirm the key Cerrato and Gitti (2022) results for the price-Phillips curve. Our primary empirical analysis draws on the Current Population Survey (CPS), which offers representative, timely survey data on employment and wages. Because precision is limited by the relatively small sample sizes available in the CPS, the evidence here is a first step toward a more detailed analysis.

The remainder of the paper is structured as follows. Section 1 discusses the data and methodology. Section 2 provides a formal framework for understanding the link between employer market power, worker reallocation, and labor market tightness. Section 3 presents evidence on employment trends by education and occupational characteristics, as well as complementary evidence on wage trends. Section 4 explores the mediating role of job-to-job transitions and provides estimates of the quit elasticity, wage-Phillips curve (in levels and in relative quantiles for different demographic groups), and possible occupational shifts. Section 5 considers the relationship between labor market tightness, price growth, and real wage growth. Section 6 offers conclusions and next steps.

1 Data sources

Our primary data source is monthly Current Population Survey (CPS) data from January 2015 through September 2022, sourced from IPUMS (Flood et al., 2021). To measure within-person wage changes, we match individuals across CPS samples observed one year apart. We drop all observations with imputed wages for wage calculations and winsorize hourly earnings at $5 and $100. We include individuals aged 16-64 who report both consistent gender throughout the panel and no more than a two-year age change between matched observations. All wages are deflated using the national CPI-U data from the US Bureau of Labor Statistics. We construct a state-level CPI-U measure using a combination of regional and metro-level information to assess geographic variation in price growth; the details are explained in Section 5. While the CPS can also be used to estimate state-level unemployment

[3]These papers were written before post-pandemic labor market conditions provided an opportunity to illuminate these mechanisms.

rates, we instead use state unemployment rate measures from the Bureau of Labor Statistics' Local Area Unemployment Statistics (LAUS), which combine CPS estimates with other data sources to increase precision (https://bls.gov/lau).

The CPS interviews sample members eight times. Respondents are interviewed for four consecutive months, are rotated out for eight months, and then are included in the sample for another four months. In the 2^{nd} through 4^{th} and 6^{th} though 8^{th} months in sample (MIS), respondents are asked if they are working for the same employer as the previous month. In MIS 4 and 8, respondents are asked to report their wage. EE transitions are directly observable using self-reported job change among previously employed CPS respondents during interview months MIS $\in \{2, 3, 4, 6, 7, 8\}$. Because job changes are neither observable in MIS $\in \{1, 5\}$ nor during the eight months when respondents are out of rotation, additional assumptions are required to identify all job changes during the full 12 months between wage observations. We will use information about changes in occupation and industry to infer job changes during the 8 month out-of-sample period, with adjustments for measurement error.

While month-to-month and year-to-year changes in CPS respondents' industry and occupation codes are frequently used to proxy for job changes, this technique introduces significant measurement error.[4] Measurement error arises because the Census Bureau assigns industry codes to each CPS respondent based on their self-reported ('write-in') job description.[5] If a worker is not entirely consistent in describing their job across successive surveys, or if the Census Bureau re-tunes its textual coding algorithm, this introduces spurious industry transitions that overstate the true frequency of job changes.

In contrast, the CPS captures *actual* job change with high precision using the following query, which is coded in the IPUMS variable *EMPSAME*: "Last month, it was reported that [name/you] worked for [company name]. [Do/Does] [you/he/she] still work for [company name]?"[6] Evidence for the reliability of this measure, provided in Section 4, is that it closely accords with published job change frequencies from the Bureau of Labor Statistics' Job Opening and Labor Turnover Survey (JOLTS). JOLTS employs a large monthly sample of establishments to precisely capture job openings, hires, and separations. Unfortunately, the CPS measure is limited by the missing data problem noted above: because the job change question is asked in only six of the eight months in which a respondent is in the survey—and, moreover, is not asked during the eight months when the respondent is out of

[4] For example, the Federal Reserve Bank of Atlanta's Wage Growth Tracker (https://www.atlantafed.org/chcs/wage-growth-tracker) codes a worker as having changed jobs if she is in a different occupation or industry than a year ago or has changed employers or job duties in the past three months.

[5] The exact CPS survey question for a worker's industry is: "What kind of business or industry is this?" Census follows a parallel procedure for assigning occupation codes.

[6] https://www.census.gov/programs-surveys/cps/technical-documentation/questionnaires.html

rotation—it cannot be used to reliably track job changes occurring between a worker's two wage observations, which are recorded one year apart.

To address these data limitations, our analysis exploits the self-reported job change measure, when available, in conjunction with information on industry and occupation change to identify job changes. We account for measurement error using two closely related corrections, which are discussed in Sections 4.2 and 4.4.[7]

There are compositional changes (which are potentially sizeable) in the employed US workforce during and after the pandemic. To account for these changes, we reweight each month's sample by using inverse probability weighting to match the characteristics of the workforce in the first quarter of 2020. Variables used for reweighting include six categories of age, five categories of education, five categories of race, Hispanic ethnicity, gender, nativity (US- vs. foreign-born), citizenship, and region. In computing wage quantiles, we account for the non-classical measurement error in reported wages that stems from bunching at round numbers. Round-number bunching occurs both because because wages are, in reality, frequently bunched at round numbers and because survey respondents further round their wages when reporting (Dube et al., 2018). This rounding creates substantial flat spots in measured nominal wage quantiles, which may exhibit no changes for many months when bunched on a round number (e.g., $15/hr), followed by a sudden, substantial change when moving to a new round number (e.g., $16/hr). To uncover the underlying wage distribution, we smooth wage quantiles, first by calculating national wage quantiles by month, and then by predicting wage quantile by rank using a lowess regression. The resulting, smoothed-wage quantiles are far more stable, as Appendix Figures A1 and A2 illustrate by presenting smoothed- and raw-wage trends. For cross-group analyses, we construct smoothed-wage quantiles by group and time period.

2 Conceptual framework: Labor market tightness, competition, and reallocation

To structure the analysis, we offer a conceptual model that links labor market tightness, employer competition, and worker reallocation in an imperfectly competitive labor market setting. We contrast this setting with the fully-competitive and market-clearing canonical model. In both perfectly and imperfectly competitive settings, rising labor market

[7]The bias that Fujita et al. (2020) identify in the CPS measure appears likely to affect the *level* of the measure after 2014 but does not obviously affect the changes over time in self-reported separations, which is the object of interest here.

tightness—stemming from either greater demand for labor or fewer workers looking for a job—generates higher wages. However, the imperfectly competitive model makes three additional predictions about the market-level impacts of rising tightness: 1) greater responsiveness of worker separations to wage levels, i.e., a higher quit elasticity; 2) increased worker reallocation from low-paid to higher-paid employers; and 3) concentration of wage gains among job-movers rather than job-stayers.

We begin with the benchmark, static model of labor market competition in Figure 1, viewed from the market level (panel A) and the firm level (panel B). Consider an inward shift of the market labor supply curve from LS to LS'. In panel A, the market-clearing wage rises and the equilibrium quantity of employed labor falls. Viewed from the perspective of price-taking firms in panel B, the labor supply curve to each firm shifts upward and employment falls accordingly. Because perfect competition enforces a law of one price for labor (of given skill level), the wage increase is identical across all firms employing workers of that skill level.

Contrast this with a monopsonistically competitive setting that involves a large number of firms, J, where workers have idiosyncratic preference shocks over jobs, ν_j, that have a Type I Extreme Value distribution (Card et al., 2018). When worker preferences are $U_j = \epsilon^L \ln(w_j) + \nu_j$, the residual (firm-level) labor supply has a constant elasticity:

$$\ln l_j(w_j) = \ln L - \ln\left(\sum_{k=1}^{J} \epsilon^L \ln w_k\right) + \epsilon^L \ln(w_j) = \ln(L) - \ln(\Phi) + \epsilon^L \ln(w_j),$$

where L is the total number of workers in the market, ϵ^L is the labor supply elasticity, and $\Phi = \ln\left(\sum_{k=1}^{J} \epsilon^L \ln w_k\right)$ is the elasticity-weighted sum of wages set by all other firms. Due to the large-J assumption, Φ is taken as a fixed constant at each firm. The inverse labor supply function of each firm can then be written as:

$$\ln(w_j) = \frac{\ln(\Phi) - \ln(L)}{\epsilon^L} + \frac{1}{\epsilon^L} \ln(l_j). \quad (1)$$

In the case of a rise in labor market competition, which raises the labor supply elasticity ϵ^L, the labor supply curve becomes flatter as the slope $\frac{1}{\epsilon^L}$ falls. The intercept Φ simultaneously rises: directly, due to a higher ϵ^L, and indirectly, due to the induced wage increases at competitor firms.[8]

Figure 2 depicts this case. Panels A and B plot the changes in employment and wages at two firms that face identical firm-specific labor supply curves but differ in productivity;

[8]There is no strategic interaction in this model since we have assumed a large J, meaning that firms do not consider how their own wage levels affects Φ. However, this Φ *is* affected by equilibrium wages w_j.

specifically, the panel-B firm has higher marginal labor productivity (at given employment) than the panel-A firm. Profit maximization leads to employment being set at the point where $MRPL = MFC$; in turn, the wage is set by the inverse labor supply curve at that employment value. This leads to wages being greater at the high-productivity firm, though wages are marked-down below marginal product at both firms.

While the increasing elasticity (flattening) of the labor supply curve, spurred by rising competition, leads to a reduction in the wage markdown at all firms, the induced wage increase is far larger at the low- than the high-productivity firm because the high-productivity firm's wage is initially closer to the competitive equilibrium level.[9] This compression in the wage distribution—a contraction in the pay gap between firms of differing productivity levels—is a distinct prediction of the monopsonistic model.

A second distinct prediction of the monopsonistic model is that rising competition induces labor reallocation from low- to high-productivity firms. At the level of an individual monopsonistically competitive firm, a rising labor supply elasticity (i.e., an increase in ϵ^L in equation (1)) reduces the firm's marginal factor cost, spurring an unambiguous increase in employment and wages. This partial equilibrium logic does not carry over to the market level, however. When all employers attempt to raise employment simultaneously, the *market-wide* wage increase yields a pure inward supply shift at each firm (seen as an increase in Φ in equation (1)). At the new market equilibrium that resolves these countervailing forces, the marginal factor cost will be lower than in the original equilibrium *only for firms with a sufficiently high level of productivity*. These firms will accordingly increase hiring. Conversely, firms below this productivity cutoff will face a higher marginal factor cost and will reduce hiring. Thus, the static monopsony model predicts that a rise in the market-level labor supply elasticity induces labor reallocation from low- to high-productivity firms. Notice further that while both firm-stayers and firm-movers experience a wage gain, the increase is largest among movers, since they gain by both the wage increase at the incumbent firm and the delta between the high- and low-wage firm. This concentration of wage gains among job-switchers is a third prediction of the monopsony model.

This reallocation can be shown analytically, as per Dustmann et al. (2022). Take the case where the firm's revenue function is $Y = p_j \ln(l_j)$. Here p_j is a productivity term that varies across firms and ϵ_L is the labor supply elasticity, as above. Profit maximization yields the following equilibrium employment choice by firms:

[9] Consider if both firms suddenly faced a perfectly elastic labor supply curve: each would raise its wage to the competitive level, generating a larger wage increase at the low-productivity firm. A similar logic applies to the introduction of a binding minimum wage if set neither "too high" nor "too low": it induces a perfectly elastic labor supply curve at each firm, a law of one price for labor, and reallocation of labor from low- to high-wage firms, as shown theoretically and empirically in Dustmann et al. (2022).

$$\ln l_j = \frac{\epsilon^L}{1+\epsilon^L}\left[\ln\left(p_j \frac{\epsilon^L}{1+\epsilon^L}\right)\right] + \frac{1}{1+\epsilon^L}\left[\ln L + \ln \Phi\right]. \quad (2)$$

Differentiating (2) with respect to productivity, p_j, yields:

$$\frac{d \ln l_j}{d \ln p_j} = \frac{\epsilon^L}{1+\epsilon^L}.$$

This derivative is positive: firm employment rises with firm productivity. Additionally, the slope of the employment-productivity locus steepens as the elasticity ϵ^L rises, meaning that a given level of productivity dispersion translates into greater employment dispersion as competition rises. Thus, in the monopsonistic competition setting, a tightening of competitive conditions causes more-productive, higher-paying employers to become relatively larger.

2.1 Why labor market tightness increases the elasticity of labor supply

The critical ingredient in the simple framework above is that the firm-specific labor supply curves become *more elastic* as the labor market tightens. Why would this occur? The static model is silent on the matter but this implication follows directly from a dynamic job ladder model (Burdett and Mortensen, 1998; Bontemps et al., 1999; Moscarini and Postel-Vinay, 2018). Consider a dynamic wage posting model with search frictions where workers engage in on-the-job search. In this model, the rate of job separations at wage w can be written as $S(w) = \delta + \rho + \lambda_e (1 - F(w))$, where δ is the exogenous separation rate to non-employment, ρ is the exogenous separation rate into another (possibly worse-paying) job (sometimes called a "Godfather shock", as in, a job offer you cannot refuse), λ_e is the outside offer arrival rate for current employees (the contact rate), and $F(w)$ is the wage offer distribution. Due to frictional wage dispersion, this distribution is non-degenerate.

In this setting, the rate at which current employees separate to better-paying jobs, $\lambda_e (1 - F(w))$, is an endogenous function of the wage at the current employer, w, holding fixed λ_e and $F(w)$. Taking logs and differentiating, the overall quit (or EE separation) elasticity is $\epsilon^{EE} = -\lambda_e f(w) w / (\rho + \lambda_e (1 - F(w)))$, which depends only on the employer's own wage *rank* in the aggregate distribution, $F(w)$. One implication of this observation, used below, is that the elasticity of EE separations, with respect to a variable that is monotone in wage rank, w, is also a function only of the rank.

This model makes clear predictions for how the quit elasticity responds to market conditions, represented by λ_e and $F(w)$. Zooming out from the firm- to the market-level, employers post V vacancies while workers exert total job-seeking effort of $JS = u + \phi(1-\delta)(1-u)$.

Here, $\phi > 0$ is the relative efficiency of on-the-job search, so that the contact rate of the employed relative to the unemployed is: $\lambda_e = \phi\lambda_u$. We represent the total number of contacts in the market with a constant-returns matching function, $m(JS, V) = m(1, \theta)$, where $\theta = \frac{V}{JS}$ corresponds to labor market tightness. Total job-search effort is equal to $JS = (1 + \phi(1 - \delta))u + \phi(1 - \delta))$. Since $\frac{\partial JS}{\partial u} > 0$, θ is also monotonically rising in the conventional tightness measure $\tilde{\theta} = u/V$.

Using these expressions, we can write the offer arrival rate to employed workers as $\lambda_e = \frac{m(JS,V)}{JS} = m(1, \theta)$, which is rising in market tightness. By implication, for a given equilibrium wage distribution, the quit elasticity ϵ^{EE} increases as either θ or $\tilde{\theta}$ rises. Hence, market tightness may increase due to either a positive demand shock, raising V, or a fall in u—perhaps reflecting a contraction in the labor force. Both factors are plausible candidates for the increase in labor market tightness following the pandemic. These comparative statics highlight how rising tightness, by increasing competition for new hires, raises the quit elasticity.

We have so far ignored the changes in the wage offer distribution $F(w)$. Endogenizing the wage offer distribution does not change the model's key features. Consider the case where firms vary in productivity, p_j, which is distributed as $H(p)$. Assume further that wages are monotonic in productivity, $w = k(p)$ with $k'(p) > 0$, which will hold in a wide class of models, including wage posting models with heterogeneous employers (Bontemps et al., 1999).[10] The wage offer distribution then simply inherits the productivity distribution of active firms, with $F(w) = H(k^{-1}(w))$.

A key prediction of the model, which we verify empirically below, is that a rise in the contact rate affects EE separations more at low- than at high-wage employers. To see this, note that the EE separation rate is equal to $\rho + \lambda_e(1 - F(w)) = \rho + \lambda_e(1 - H(p))$. Since $(1 - F(w))$ is monotonically increasing in w, an increase in the contact rate, λ_e, differentially raises separations at low-wage, low-productivity firms (i.e., low-ranked firms with small $H(.)$) as compared to high-wage, high-productivity firms. Figure 3 illustrates this point by plotting the EE separation by firm wage-rank locus for λ_e equal to 0.03 and 0.04. The 'rotation' of the equilibrium separations-wage locus, as the contact rate rises, reflects the reallocation of lower-wage workers toward higher rungs of the job ladder.

The elasticity ϵ^{EE}, with respect to the wage, depends on the wage level—whose distribution is, of course, an endogenous object. However, the effect of an increase in the contact rate, λ_e, on the EE elasticity depends only on the wage rank, $r = F(w) = H(k^{-1}(w))$, which

[10] Under wage posting, employers set wages based on the labor supply elasticity, which is the sum of the quit and the recruit elasticities. In steady state, this can be approximated as twice the absolute value of the quit elasticity (Manning, 2021).

is, in turn, a primitive of the model. Specifically, the EE elasticity as a function of r can be written as:

$$\tilde{\epsilon}^{EE} = -\frac{r\lambda_e}{\rho + (1-r)\lambda_e}.$$

The key observation is that a higher contact rate, due to either rising vacancies or falling labor supply, makes EE separations more sensitive to firm wage rank, as seen in the 'twisting' of the EE separations-firm wage locus shown in Figure 3. Empirically, rising tightness should spur a relatively larger increase in separations at low-ranked firms. In turn, this fuels a relative increase in wages, concentrated at low-wage firms, as in the static model.[11] In short, rising tightness reduces frictional wage inequality through two channels: wages rise more at lower-ranked firms, and workers move disproportionately from lower- to higher-ranked firms.

This model also shows how a tighter labor market yields, in steady state, a larger fraction of the workforce employed at more-productive firms.[12] Define $L(p)$ as the cumulative share of potential workers (normalized at 1) who are employed by employers with productivity of p or less. Further denoting $1 - F_t(p)$ as $\overline{F}_t(p)$, the law of motion for this share can be written as:

$$L_{t+1}(p) = (1-\delta)(1-\rho)\left[1 - \lambda_e \overline{F}_t(p)\right] L_t(p) + \left[(1-\delta)\rho(1-u_t) + \lambda_u u_t\right] F_t(p).$$

In steady state, we have:

$$L(p) = \frac{F(p)\left[(1-\delta)\rho + \delta\right]}{(1-\delta)(1-\rho)\left[1 - \lambda_e \overline{F}(p)\right]} \times \frac{\lambda_u}{\lambda_u + \delta},$$

$$\tilde{L}(r) \approx \frac{r\left[\rho + \delta\right]}{\left[\delta + \rho - \phi\lambda_u(1-r)\right]} \times \frac{\lambda_u}{\lambda_u + \delta}.$$

Taking logs,

$$\ln \tilde{L}(r) = \ln\left[r\left(\rho + \delta\right)\right] - \ln(\delta + \rho - \phi\lambda_u(1-r)) + \ln(\lambda_u) - \ln(\lambda_u + \delta),$$

and differentiating with respect to $\ln \lambda$, we get the following condition:

$$\frac{d \ln \tilde{L}(r)}{d \ln \lambda_u} = \frac{\phi\lambda_u(1-r)}{\delta + \rho - \phi\lambda_u(1-r)} + \frac{\delta}{\lambda_u + \delta}. \tag{3}$$

This expression reveals how the steady state allocation of labor evolves with tightness, as measured by λ_u, as well as $\lambda_e = \phi\lambda_u$. At the top of the distribution, where $r = F(w) = 1$, the

[11] The dynamic model in this section does not explicitly describe the wage setting process, where wages are marked down based on the labor supply elasticity. However, the impact of increasing contact rate on the rise in offered wages comes out of standard wage posting models, as in Bontemps et al. (1999).

[12] Moscarini and Postel-Vinay (2018) develop this point in their discussion of the job ladder model.

derivative in equation 3 is positive: greater tightness unambiguously raises employment at the highest-ranked firm. Tightness may not raise employment at firms with low productivity rank, r, however. Evaluating this expression at $r = 0$ and substituting $\phi\lambda_u$ for λ_e, we see that there will be a relative reduction in employment at the bottom of the distribution when the following condition holds:

$$\frac{\phi\lambda_u}{\delta + \rho - \phi\lambda_u} > \frac{\delta}{\lambda_u + \delta}.$$

Increased tightness is more likely to reallocate labor upward from lower- to higher-ranked firms when (1) on-the-job search is more efficient (ϕ is large), and (2) endogenous job-to-job changes are a large fraction of all separations ($\phi\lambda_u$ is large relative to δ and ρ).

Figure 4 illustrates this point for an increase in λ_e from 0.02 to 0.04, with $\delta = \rho = 0.01, \phi = 0.5$. Using these parameter values, the overall employment rate rises with market tightness, driven by upward reallocation to higher-ranked firms. As shown in the figure, cumulative employment *below* the 90^{th} percentile of firm productivity is lower in a tighter market while cumulative employment *above* the 90^{th} percentile of firm productivity is higher. Tightness also boosts the overall employment rate (by over 10 percentage points, from 0.65 to 0.76), meaning that it raises employment at high-ranked firms, in absolute terms, while decreasing it at low-ranked firms.

2.2 Empirical implications

We will empirically assess these predictions, first by estimating EE separation elasticities with respect to residual wages in the pre- and post-pandemic periods, and then by assessing whether a rise in separations is associated with a reallocation of workers from lower- to higher-paid jobs. The job ladder model also provides guidance on how to empirically measure labor market tightness for this exercise. While empirical work often measures tightness by the unemployment rate, $u = \frac{\delta}{\delta+\lambda_u}$, this measure does not capture movements in job-finding rates among the employed, $\lambda_e = \phi\lambda_u$.[13] The job ladder model implies that, to capture both λ_u and λ_e, an empirical measure of labor market tightness should include both the unemployment rate and the job-to-job transition rate.

We note that, aside from the role of labor market tightness, other pandemic-specific factors may have contributed to an increase in the separation elasticity. For example, while most industrialized countries used public payments to employers to retain workers during lock-downs (Giupponi et al., 2022), the US, instead, vastly increased the scope and gen-

[13] As shown in Moscarini and Postel-Vinay (2017), conditional on the outside option, greater ease of job finding among the unemployed that does not *also* raise job-finding rate among those at work has no impact on wages.

erosity of unemployment benefits programs for millions of workers who were temporarily or permanently laid off during the pandemic.[14] This dissolution of worker-firm ties may have increased workers' 'footlessness' following the initial shock, especially in low-wage sectors such as hospitality, which experienced the deepest pandemic-related contraction. These pandemic dislocations may have also altered workers' perceptions of the availability of better-paying jobs. Literature suggests that workers at particularly low-wage jobs may systematically underestimate the true availability of outside options (Jäger et al., 2022). Plausibly, a temporary shutdown could enable workers to discover better outside options: both directly, by increasing workers' own search activity, and indirectly, by watching their coworkers find new jobs. Finally, by substantially increasing household liquidity—particularly among low-income households (Cox et al., 2020)—the transfer payments made by supplementary unemployment benefits (Federal Pandemic Unemployment Compensation) and household stimulus benefits (Economic Impact Payments) may have raised reservation wages and enabled more job shopping.[15]

Next, we turn to evidence, first describing the evolution of the US labor market during and after the pandemic, and then testing the distinct predictions of the competitive and monopsonistic models in the face of tightening aggregate and regional labor markets.

3 The unexpected compression

3.1 Employment drop and rebound

The onset of the pandemic saw the sharpest drop in US employment of the post-WWII era. Figure 5 plots the 3-month rolling average Employment-to-Population (EPOP) ratio and the labor force participation rate from January 2015 through September 2022. EPOP fell sharply at the onset of the pandemic, plunging by an unprecedented 9.2 percentage points from 71.6% in January 2020 to 62.4% in May 2020. The labor force participation rate (employment plus unemployment) saw a smaller, but still stark, fall over the same period, from 74.5% in January 2020 to 71.7% in May 2020.[16]

[14] Simultaneously, the US Paycheck Protection Program kept a comparatively modest number of workers, 1–2 million, employed during the pandemic (Autor et al., 2022).

[15] Generous unemployment benefit replacement rates appear to have had only a modest impact on job finding rates among the unemployed, however, including through the liquidity channel (Ganong et al., 2022; Coombs et al., 2022).

[16] Figure 5 displays percentage changes in employment and labor force participation, normalized by their January 2020 values, while in the text, we discuss percentage point changes (since EPOP is already a percentage). As shown in the figure, EPOP fell by 12.8% between January and May 2020, while labor force participation fell by 3.6%.

Since pandemic business shutdowns disproportionately affected low-paid, front-line service workers, the employment drop was particularly pronounced among less-educated workers, as shown in Figure 6. Between January and May of 2020, EPOP fell by 7 percentage points among those with a college (Bachelor's) degree or more, versus 10.2 points among those with a high school diploma or less (from 84.5% to 77.4% in the former group, and from 60.1% to 49.9% in the latter).[17]

Although the employment fall among less-educated workers was dramatic, the subsequent rebound was equally-and-oppositely pronounced. By early 2022, EPOP among adults with only a high-school education had already attained 100% of its early-2020 level, which it has since slightly surpassed.[18] The college-degree EPOP rate did not have nearly as much ground to make up after the pandemic, but as of the third quarter of 2022, it had nearly returned to its pre-pandemic level.

The disproportionate fall and rebound of less-educated workers is mirrored in the pattern of employment change by occupation. Ranking occupations by their average wage level in January 2020, Figure 7 shows that employment in the (population-weighted) bottom earnings tercile occupations decreased by more than 18% between January 2020 and May 2020. Those occupations in the top tercile lost a mere 4% of employment, while those in the middle tercile lost 8%. By early 2022, occupations in all three terciles had essentially reattained their immediate pre-pandemic employment levels. The rapidity of this rebound stands in contrast with the protracted recovery from the 2007 financial crisis, particularly for low-wage workers (Hoynes et al., 2012; Carnevale et al., 2016).

3.2 Wage distribution changes

The occupational asymmetry of the pandemic labor demand shock, focused disproportionately on less-educated workers performing low-wage occupations, would normally augur further wage divergence between the top and bottom of the distribution—compounding four decades of rising US wage inequality (Hoffmann et al., 2020). Indeed, in the early days of the pandemic, one of this paper's authors predicted a slack post-pandemic labor market for non-college workers, with accompanying wage stagnation at the bottom of the distribution (Autor and Reynolds, 2020). We thus feel comfortable labeling as *unexpected* the remarkable wage *compression* underway in the post-pandemic labor market.

Since 2020, both real and relative wages have grown substantially more at the bottom

[17] Thus, the proportional fall in EPOP was over twice as large – 17% vs. 8.3% – for the less-educated group.

[18] Except where otherwise noted, the term 'high-school workers' refers to those with a high school diploma or less (i.e., less than high-school or high-school educated). This group does *not* include workers with some college (i.e., more than high school but less than a four-year college degree).

of the distribution (10^{th} percentile) than at the median or top (90^{th} percentile), as shown in Figure 8.[19] Despite substantial post-pandemic inflation (which we account for using the benchmark Consumer Price Index for all Urban Consumers (CPI-U)), real hourly earnings at the 10^{th} percentile of the wage distribution rose by 6.4% between January 2020 and September 2022. Real wages *also* spiked at the 50^{th} and 90^{th} percentiles of the distribution in late 2020, but these gains were subsequently eroded fully by inflation. The median real wage in September 2022 was 5.6% below its May 2020 level, while the 90^{th} percentile wage had fallen by 6.7%. To benchmark the magnitude of the wage compression, observe that the nine-log-point drop in the 90/10 wage ratio since 2020 reverses approximately 27% of the 33-log-point *rise* in that measure over the previous four decades.[20]

One noteworthy pattern in Figure 8 is that the 10^{th} percentile of the wage distribution was rising strongly in the five years prior to the pandemic, a phenomenon discussed in depth by Aeppli and Wilmers (2022), Dey et al. (2022), and Shambaugh and Strain (2021). This pattern might indicate that wage compression following the pandemic primarily reflects an acceleration of pre-pandemic wage trends. An alternative (or complementary) explanation is that wage compression prior to the pandemic was substantially driven by state minimum wage laws, many of which were adopted in the preceding decade (Cengiz et al., 2019). Figure 9 explores these possibilities by contrasting trends in 10^{th}, 50^{th}, and 90^{th} percentiles in the 31 states that set minimum wages above the federal level, 'minimum-wage states' (panel A), with analogous trends in the 20 states that did not, 'non-minimum-wage states' (panel B). Prior to the pandemic, the rise in the 10^{th} percentile was limited to minimum-wage states; no such trend was visible in the non-minimum-wage states. Yet, in both groups, the 10^{th} percentile jumped sharply in late 2020 and remained at its elevated level through late 2022. Trends in the 50^{th} and 90^{th} percentiles were, however, comparable between minimum- and non-minimum-wage states. While institutional forces likely drove wage compression in a subset of states prior to the pandemic, the evidence in Figure 9 suggests that the sharp post-pandemic wage compression in both minimum- and non-minimum-wage states is not, primarily, a continuation of this trend.[21] This conclusion is reinforced by comparing Figures 10 and A2, which show that the overall compression in wages between 2019 and 2022 was

[19] We also separately report trends in average wages, along with wages by percentile and metropolitan status, in Appendix Figures A3 and A4. These show broadly similar trends in inequality between metro and non-metro areas.

[20] Analysis of CPS data in Economic Policy Institute (2022) finds that the log 90/50 wage ratio rose from 0.69 to 0.91 between 1979 and 2019, while the log 50/10 wage ratio rose from 0.54 to 0.65, implying a 33-log-point rise in the 90/10 ratio.

[21] Examining wage quantiles by sex reveals that the pre-pandemic wage compression was sharper among men than women (Appendix Figure A5). Since women comprise a disproportionate share of the workers paid at or below the minimum wage (U.S. Bureau of Labor Statistics, 2021), it is equally clear that the minimum wage was not the only force driving lower-tail wage compression prior to the pandemic.

quantitatively much larger than the compression between 2015 and 2019.

To offer a more comprehensive picture of real wage changes across the full wage distribution, the left-hand panel of Figure 10 plots nominal wage changes by percentile between August 2019 and August 2022. The figure overlays the contemporaneous change in the CPI-U, thus delineating real wage gains from losses. Over these three years, real wage gains were positive for approximately the bottom 65% of the wage distribution and were negative for the remainder. Real wage declines were steepest at higher percentiles, implying a large compression of wage inequality. Real wage gains primarily accrued in late 2020, though these have been substantially eroded by inflation in the interim. The middle and right-side panels of Figure 10 show that only the bottom 20% of the distribution has gained in real terms during the last two years, and only the bottom 5% of the distribution has gained in the past year (between August 2021 and August 2022).

A limitation of the CPS real earnings measure is that it may not capture changes in non-wage compensation that coincided with the pandemic. Barrero et al. (2022) estimate that the recent, rapid rise of remote work arrangements has improved the amenity value, and hence raised the real compensation, of the jobs held by highly educated workers. Simultaneously, the disamenities associated with low-paid, in-person jobs have arguably intensified: greater disease exposure risk, thinner staffing levels, and a seeming epidemic of irate customers. It appears plausible that trends in real wage compression modestly overstate trends in real compensation.[22]

3.3 Between-group inequality

We next document how wage compression is reshaping wage inequality among age-by-education skill groups. Thirty years of literature has analyzed the expansion of these differentials (Katz and Murphy (1992b); Katz et al. (1999); Card and Lemieux (2001); Acemoglu and Autor (2011); Autor (2014); Hoffmann et al. (2020)). Figure 11 documents the pronounced wage growth among high-school and some-college workers overtaking that of college-educated workers. Akin to the pre-pandemic compression of the lower tail of the distribution, the college wage differential was also contracting prior to the pandemic (Aeppli and Wilmers, 2022), particularly between 2015 and 2017.[23] But the post-pandemic compression is faster and more abrupt than the preceding trend. As shown in Figure 12, this

[22] Larrimore et al. (2022) analyze changes in earnings levels during the pandemic—inclusive of fiscal relief provided by the array of Covid-19 policy responses enacted in that period. They find that the real median incomes of the bottom quintile of earners rose by over 60% in 2020, and that these earnings increases offset relief reductions during the 2021 recovery.

[23] Appendix Figure A6 shows real-wage trends for more-detailed educational categories—less than a high school degree, high school degree, some college, Bachelor's degree, and greater than Bachelor's.

post-pandemic jump in the real wages of high-school workers was equally rapid in states with and without minimum wages set above the federal level.[24] The wage gap between some-college and four-year college-graduate workers has closed even more rapidly since the pandemic than the pure college/high school wage gap (as seen in Figure 11).[25]

Figure 13 plots wage trends by age, documenting that younger workers have seen the largest wage gains since the onset of the pandemic. Further breaking down the data by age and education in Figure 14 makes clear that wage compression among young non-college workers drives these trends.[26] Of the four groups depicted—high-school vs. college-educated $\times$ above vs. below age 40—young high-school workers constitute the only group that has *not* seen its post-pandemic earnings gains entirely eroded by inflation. Our subsequent analysis of mechanisms focuses primarily, though not exclusively, on the contrast between young high-school workers and the balance of the workforce.

This pattern of wage compression is not limited to educational wage differentials. Grouping occupations into terciles based on wage ranks in 2019, Figure 15 shows that wage growth since early 2020 has been strongest in the lowest occupational wage tercile and weakest in the top occupational wage tercile. Paralleling the patterns above, pre-pandemic wage growth in low-wage occupations relatively exceeded that of mid- and high-wage occupations. This trend sharply accelerated with the pandemic: since mid-2020, average earners in the lowest tercile of occupations have outpaced those in high-wage occupations by almost 10%, and by 6% in the case of mid-wage occupations. Low-wage occupations tend to be those that are intensive in non-routine manual tasks—occupations which have also experienced fast wage growth since 2020, as documented in Appendix Figure A7. This category is dominated by in-person service occupations, such as food preparation, building and grounds, cleaning, personal care, and personal service, many of which suffered a disproportionate share of layoffs early in the pandemic.

Finally, Figures 16 and 17 report real wage trends by sex and race, examining the gains of white, non-Hispanic workers relative to Black and Hispanic workers. There is no appreciable difference in real-wage trends between men and women, either before or after the pandemic. By contrast, the earnings gap between Black/Hispanic and white workers closed by approximately four percentage points after the onset of the pandemic. Distinct from the quantile, education, and occupational wage differentials above, there was no trend in the racial earn-

[24] Figure 12 also documents that the jump in the wage of college workers starting in 2020 is initially several points larger in states with supra-federal minimum wage laws. But this differential falls to less than 1.5 points by late 2022.

[25] We focus primarily on the pure college/high school gap because the some-college category is an unstable amalgam of workers with two-year degrees or incomplete two- and four-year college enrollments.

[26] A comparison of earnings trends between non-college workers and college or more is reported in Appendix Figure A8.

ings gap prior to the pandemic. A question for future investigation is whether the earnings trends among education, age, and occupational groups fully account for the post-pandemic compression of the racial gap, or if a distinct, race-specific component remains.

4 Competition at work? Testing the role of intensifying labor market competition

Given the sharp drop in demand for low-wage workers during the pandemic, what explains the substantial real earnings growth at the bottom of the distribution since 2020? We focus on mechanisms in this section. Guided by the theoretical model in Section 2, we consider five margins of adjustment that inform whether the recent wage compression primarily reflects a shift between two competitive equilibria or, instead, a *tightening* of competitive conditions within an imperfectly competitive labor market. Specifically, the imperfectly competitive model predicts:

1. A rise in employment-to-employment (EE) transitions among low-wage workers
2. A rise in wages among groups with rising separation rates
3. An increase in the elasticity of quits with respect to (low) wages
4. A reallocation of employment from low-wage to high-wage firms and sectors
5. A concentration of wage gains among job-movers versus job-stayers

4.1 Tightness and wage growth

We begin with EE transitions. Using CPS data, we classify workers as having made an EE transition if they are employed in two consecutive survey months and report having changed employers or primary jobs between those months. The upper panel of Figure 18 plots three-month moving averages of EE separation rates for the full sample of employed working-age adults, comparing years 2021 and 2022 to a pre-pandemic baseline average of 2017 through 2019. EE transitions rose modestly in the second half of 2021 and remained elevated relative to their pre-pandemic level. The lower panel of Figure 18 presents complementary evidence on voluntary job separation rates (‘quit rates’) from the Bureau of Labor Statistics’ Job Openings and Labor Turnover Survey (JOLTS). Reassuringly, there is a tight correspondence between the CPS-based and JOLTS-based measures of separations. In the CPS data, the monthly separation rate rose from approximately 2.1% during 2017–2019 to approximately 2.6% in the third quarter of 2022; in the JOLTS data, which exhibits smaller monthly fluctuations due to its larger sample frame, the corresponding rise is from 2.3% in 2017–2019 to 2.7% in the third quarter of 2022.

Figure 19 reports analogous separation rates for education-by-age subgroups. The four panels of this figure reveal that the aggregate increase in EE transitions in Figure 18 is driven by young high-school workers. Monthly EE transitions among this group rose from approximately 3.2% in 2017 through 2019 to approximately 3.6% during 2021 and 2022. Among the other three demographic subgroups (old high-school workers, young and old college workers), there is almost no visible rise in separations following the onset of the pandemic.[27] Figure 20 rounds out this evidence by using JOLTS data to track voluntary separation rates, by industry, for years 2015–2022. The sharp post-pandemic rise in job separations is most pronounced in the hospitality and service industries, which employ a disproportionate share of young and less-educated workers (we calculate that young high-school workers make up 41% of the hospitality industry, compared to 21% of the overall sample).

In the job-ladder model, rising job-to-job transition rates signal an overall tightening of the labor market, which enables workers to move from from lower- to higher- (residual) wage employers, leading to aggregate wage gains. We explore this prediction by estimating the state-level relationship between tightness and wage growth. Following the theoretical treatment above, we measure state-level labor market tightness using both state-level EE transition rates and state unemployment rates. To efficiently combine these measures, we standardize both, reverse the sign of the unemployment rate, and take the average of the two.

As documented in Figure 21, labor market tightness in late 2021 through early 2022 varied substantially across states, with a cross-state standard deviation of 0.71 percentage points. The labor market was generally tighter in low-density states, such as Maine and Montana, and was relatively slack in states that maintained prolonged pandemic lockdowns, including Massachusetts, New York, and California. Figure 22 shows the time path of our composite tightness measure (panel A) and its two components (panel B). Tightness fell by over two standard deviations at the outset of the pandemic. It then rebounded quickly and, by 2022, rose above pre-pandemic levels. The evolution of tightness reflects movements in both unemployment and EE transitions: the national unemployment rate returned to its low pre-pandemic level by the end of the sample window, while the EE separation rate remained substantially elevated relative to the pre-pandemic baseline.

We estimate the relationship between state-level tightness and wage changes—the wage-

[27] Appendix Figure A9 reports EE separation rates separately for high-school and some-college workers. The rise in transitions is concentrated among workers without a college education.

Phillips curve—by focusing on wage growth between the first half of 2021 (2021_{q1q2}) and the 2^{nd} and 3^{rd} quarters of 2022 (2022_{q2q3}), which is the close of our sample. Using pooled worker-level CPS wage observations for each six-month period, we fit the following equation:

$$\ln W_{ist} = \alpha_t + \beta \text{Tightness}_{s,2021_{q3}-2022_{q1}} \times [t = 2022_{q2q3}] + X_i^{'}\gamma + \delta_s + e_{ist}, \tag{4}$$

where state-level tightness is measured at the mid-point of the start- and end-period wage observations ($2021_{q3} - 2022_{q1}$). Due to the inclusion of state (δ_s) and time (α_t) fixed effects, with an interaction between the tightness measure and a dummy for the second period (2022_{q2q3}), the β coefficient estimates the relationship between the *change* in state-level log wages from 2021_{q1q2} to 2022_{q2q3} and the *level* of labor market tightness in the middle period ($2021_{q3} - 2022_{q1}$).[28] Some additional specifications control for education, age, sex, race, sector (manufacturing, finance, business services, or professional services), union coverage, and state-specific Covid-19 death rates (from Goda and Soltas (2022)). Standard errors are clustered at the state level.

Estimates of equation (4) for all working-age adults and for demographic subgroups are reported in Table 1 and Figure 23. The first panel of the figure corresponds to the estimated cross-state wage-Phillips curve for all working-age adults. In the most demanding specification, reported in column 5 of Table 1, the well-determined slope of 0.011 (se = 0.005) implies that a one-standard deviation increase in tightness predicts additional wage growth of 1.1% between the first half of 2021 and the 2^{nd} and 3^{rd} quarters of 2022. Given the considerable cross-state dispersion of the tightness measure depicted in Figure 21, this is an economically sizable relationship.

Panel B of Figure 23 extends this exercise by separately reporting wage-Phillips curves estimates for the 1^{st} quartile of the wage distribution and for the remaining three quartiles combined, with corresponding regression estimates reported in Appendix Table A1. The slope estimate for the bottom-quartile wage-Phillips curve is around eight times as steep as the corresponding estimate for the combined upper-three quartiles: 0.044 (se = 0.011) versus 0.005 (se = 0.007). This pattern constitutes a first piece of evidence that the wage compression documented above is associated with labor market tightness.

Panel C presents a second piece of evidence: the cross-state wage-Phillips curve for high-school workers under age 40 has a slope of 0.038 (se = 0.010); for the complementary set of all other working-age adults, however, this curve is shallow and statistically insignificant at 0.004 (se = 0.006). Thus, labor market tightness is strongly predictive of wage gains at the bottom of the distribution.

[28] Wage observations for $2021_{q3} - 2022_{q1}$, the time interval used for estimating the EE separation measure, are excluded from the estimating equation.

Table 1 further documents that these relationships are concentrated in the bottom half of the wage distribution, particularly among workers without four-year college degrees, and are highly robust to the inclusion of demographic control variables. By contrast, estimates of the wage-Phillips curve for the upper half of the wage distribution and for workers with four-year degrees are generally imprecise and opposite-signed. Appendix Tables A2 and A3 replicate these estimates using each of the two standardized components of the tightness measure—unemployment and EE separations—as standalone predictors. The wage-Phillips curve relationship for unemployment closely replicates the pattern of results for labor market tightness in Table 1, though with slightly smaller magnitudes. The wage-Phillips curve for EE separations is robustly concentrated among bottom-quartile and young high-school workers but is not significant for other groups.

These steep wage-Phillips curves were not evident in the pre-pandemic era. Appendix Table A4 reports analogous estimates for the 2015–2019 period and finds no significant relationship between labor market tightness and wage growth—for the full sample of working-age adults or for the subgroups exhibiting the steepest wage-Phillips curves after 2020 (1^{st} wage-quartile earners and high-school workers under age 40). This contrast underscores how profoundly the low-wage labor market has changed since the onset of the pandemic.

Above, we showed that, prior to the pandemic, state minimum wage regulations likely contributed to lower-tail wage compression (Figure 9). To abstract from the likely effects of the minimum wage, Appendix Table A5 repeats the main wage-Phillips curve estimations using a trimmed sample that omits the bottom 15 percentiles of earners in each state and period—workers whose wages are the most likely to be affected by the minimum wage. Results for the trimmed sample are highly comparable to those in Table 1, with slightly *larger* point estimates for low-earnings groups. This pattern suggests that the state-level correlation between wage compression and labor market tightness-wage compression is unlikely to be driven by state minimum wage policies.

Our approach to estimating wage-Phillips curves echoes that of Katz and Krueger (1999), who studied the evolution of wages and prices in the high-pressure labor market of the 1990s, with two key differences. First, the tightness measure applied here incorporates *both* state-level EE transition rates and state-level unemployment rates (as opposed to solely the unemployment rate). Second, whereas the wage-Phillips curve estimated in Katz and Krueger (1999) is expectations-augmented by imposing an estimated *price*-coefficient on the wage-Phillips curve, our estimate directly regresses wage changes on labor market tightness. This direct approach proves important because, as predicted by theory, tightness appears to disproportionately affect wage growth among low-wage workers.

4.2 Separation elasticities

The job-ladder model predicts that the tightening labor market will spur a rise in the quit elasticity—the sensitivity of job-to-job separations to wage levels—and differentially so for low-paid workers. In the theoretical model, low-paid workers are those who earn less than comparable workers employed by other employers. In the data, it is difficult to distinguish workers who receive low wages *despite* their skills from workers who receive low wages *because* of their skills. Bassier et al. (2022) surmount this problem by using matched worker-firm data to estimate the elasticity of quits to the firm-specific component of wages (distinct from the worker skill component). This is not feasible in Current Population Survey data, however, which are based on household surveys. We use two alternative approaches. The first estimates how worker separations respond to workers' own wage levels purged of the influence of standard Mincerian covariates (in effect, their wage residuals). A second approach uses industry wage premia to proxy for rents paid to workers, motivated by the evidence that (normally) unobserved worker skill differentials do not fully account for industry wage premia; instead, these premia represent an aggregation of firm-specific wage components that differ systematically by industry (Katz and Summers, 1989; Card et al., 2022).

As discussed below, there are challenges to linking job-to-job separations with the wage being paid to a worker in the CPS data, owing to the disparate timing of when these questions are asked. This affects the estimation of the the own-wage separation elasticity, which requires us to connect the reported wage at a given time to separations occurring many months later.[29] We employ two different (imperfect) approaches to overcome the challenge. In each case, we estimate a model for worker separations as follows:

$$\Delta J_{it}^k = \alpha_{T(t)}^k + \beta_{T(t)}^k \ln w_{i,t-1} + X_{it}' \lambda_{T(t)}^k + \epsilon_{it}, \tag{5}$$

Here, the dependent variable, ΔJ^k, is either a 3-month ($k = 3$) or an annual ($k = 12$) measure of job-to-job separations. The key independent variable, $\ln w_{i,t-1}$, is the log of hourly earnings reported by the respondent in MIS 4; this is prior to any possible employment transition.

For the case of 3-month separations, the outcome is equal to one if worker i reported a job-to-job transition in MIS 6 through 8, when such job-to-job transitions are directly reported by the respondent. Unfortunately, given the CPS sampling frame, we do not directly observe EE transitions during the 8-month period between MIS 4 and 5, nor in MIS 5. This creates a missing data problem since, if a worker *did* change their job during this interval, the wage reported in MIS 4 would no longer be the relevant one to explain their quit behavior. For this

[29]The industry wage premium approach surmounts this problem, since the premium is only a function of one's industry, which is reported in each period.

reason, we restrict the sample to those who were *unlikely* to have changed their jobs between MIS 4 and 5: namely we exclude workers who report having a change in their industry *and* their occupation between those periods. Therefore, the estimate β^3 represents a hazard of quitting a job in MIS 6, 7, or 8, *conditional* on being unlikely to have done so between MIS 4 and 5—as best we are able to infer based on industry and occupation information.

In a second approach to estimating the own-wage separation elasticity, instead of considering the conditional job-to-job hazard in MIS 6, 7, and 8, we construct a proxy measure for annual (12-month) job-to-job transition. We do so by combining (1) the direct observation of EE transitions in MIS 6, 7, and 8 with (2) a proxy for such a transition between MIS 4 and 5 using changes in industry and occupation. Again, we use a joint change in both industry *and* occupation between MIS 4 and 5 as a proxy for a true EE separation during that interval. The use of this proxy introduces measurement error into our separation elasticity estimates, which we account for using a procedure described in Appendix A1.

Both the 3-month and the annual separation models use a covariate vector X_i that includes indicators for gender, race, ethnicity, citizenship, state, metro area status, five education categories (less than HS, HS, some college, BA, and greater than BA), and four age groups (under 25, 25-39, 40-54, 55+). We anticipate a *rise* in the magnitude of the separation elasticity from a tightening labor market. We allow for this by fitting equation (5) separately for two time intervals: first for pre-pandemic job changes in 2015–2019, and second for post-pandemic job changes in 2021–2022. These time intervals are subscripted as $T(t) = 1$ for $t \in [2015, 2019]$ and $T(t) = 2$ for $t \in [2021, 2022]$.

The results in columns 1 and 2 of Table 2 provide separation elasticity estimates for the 3-month and the adjusted annual switch definition, respectively. The various panels report estimates for the workforce overall, as well as for age-education subgroups. These estimates find that only for the high-school under-40 group did the magnitude of the separation elasticity consistently increase from 2015–2019 to 2021–2022. For the 3-month switch measure, this elasticity doubles, increasing from an absolute value of 0.333 (se = 0.137) to 0.667 (se = 0.326) from 2015–2019 to 2021–2022, while for the annual switch measure the corresponding increase is from 0.175 (se = 0.044) to 0.267 (se = 0.104). Meanwhile, the elasticity for the overall workforce decreased slightly from 2015–2019 to 2021–2022 from (an absolute value of) 0.276 (se = 0.044) to 0.216 (se = 0.115) for the 3-month switch measure and from 0.183 (se = 0.015) to 0.156 (se = 0.039) for the annual switch measure. We see a qualitatively similar pattern when we turn to the annual separation elasticity estimates in column 2. We detect a sizable rise in the magnitude of the separation elasticity only for the high-school under-40 group, where the elasticity rises from (an absolute value of) 0.175 to 0.269.

The person-level wage residual calculated from cross-sectional data incorporates both firm-specific rents (the explanatory variable of interest) and unobserved worker quality, which is a confound that will attenuate separation elasticity estimates based on equation (5). As an alternative measure of on-the-job rents, we use the estimated wage premium $\tilde{w}_j$ in each worker's industry j of current employment. Industry wage premia (IWP) are estimated from cross-sectional regressions of log hourly wages on gender, age, age squared, and age cubed, as well as dummy variables for race, ethnicity, citizenship, education, metro area status, and 3-digit industry, where the coefficients on the industry dummies capture the estimated premia. These IWP models are fit separately by education-age groups using pooled CPS data for years 2015–2019.[30]

We fit the following model for separations as a function of industry wage premia:

$$\Delta J_{it}^k = \alpha_{T(t)} + \beta_{1,T(t)} \ln \tilde{w}_{j(i,t-1)} + X_{it}^{'} \lambda_{T(t)} + \epsilon_{it}. \tag{6}$$

This model differs from equation (5) in several respects. First, it uses $\tilde{w}_{j(i)}$ in place of $\tilde{w}_i$. Second, standard errors are clustered at the industry level. Third, since we are using industry wage premia instead of own-wage, we can use a monthly EE separation rate ($k = 1$) as the dependent variable, as opposed to the 3-month ($k = 3$) or annual one ($k = 12$) used above. Elasticities from these specifications are obtained by dividing $\beta_{1,T(t)}$ by $E(\Delta J_{it}^1)_{T(t)}$. Columns 3 and 4 of Table 2 report these elasticities with and without controls. The results in these columns show that the elasticities from 2015–2019 and 2021–2022 increased for both high-school age groups, although there is a larger increase for the under-40 age group.

Following the job ladder model's prediction that the separation elasticity is larger at lower wage levels, we also fit equation (6) using a quadratic specification for the main explanatory variable, $\tilde{w}_{j(i,t-1)}$. Elasticities are then estimated by dividing the derivative of ΔJ_{it}^1 with respect to $\tilde{w}_{j(i,t-1)}$ by the predicted value of ΔJ_{it}^1 at several values of $\ln \tilde{w}$. Appendix Tables A6 and A7 report the coefficients on $\ln \tilde{w}_{j(i,t-1)}$ and its square from equation (6). Appendix Table A8 reports the same coefficients using a Poisson regression specification.

Key results from this analysis are summarized in Figures 24 and 25 and in Table 3, which reports estimated elasticities at $\ln \tilde{w} \in \{-0.3, 0.0, 0.3\}$ as well as their pre- versus post-pandemic contrasts. As shown in Figure 24, the aggregate EE separation elasticity at its mid-point (normalized to zero) is largely comparable before and after the pandemic. Below the mean, however, the elasticity is steeper in the post period, rising from (an absolute value

[30] Table 4 of Card et al. (2022) reports that approximately 20% of the variance ($0.122^2/0.240^2$) in cross-sectional industry wage premia reflects differences in employer pay across industries, with most of the remainder due to skill sorting. These unobserved skill differences contribute to variation in $\tilde{w}_j$ and hence will attenuate estimates of the quit elasticity—though this attenuation can be calculated using estimates reported in Card et al. (2022).

of) 0.740 (se = 0.270) to 1.094 (se = 0.293). Thus, the quit elasticity rose at lower (more negative) wage premia.

Most critically, Figure 25 shows that the quit elasticity has risen most among the lowest paid worker groups and least among the highest paid, as predicted by the conceptual framework. This is most visible for non-college workers (high-school or less) under age 40 (panel A): the quit elasticity rose from (an absolute value of) 0.539 (se = 0.149) to 0.984 (se = 0.186) at the mean wage premium; below the mean, the magnitude rose nearly three-fold, with the elasticity increasing from 0.354 (se = 0.252) to 1.372 (se = 0.411). We see a similar but less pronounced pattern for non-college workers over age 40 (panel C), again showing a particularly sharp rise in magnitude of the elasticity at the bottom (from 0.663 (se = 0.349) to 1.075 (se = 0.389). Among high-school workers under 40, the increase in the (absolute value of) elasticity at $\ln \tilde{w} = 0.0$ and $\ln \tilde{w} = -0.3$ are both significant, as reported in Table 3. The elasticity at the mean rose by 0.445 (se = 0.237, p-value = .062), while the elasticity at $\ln \tilde{w} = -0.3$ increased by 1.018 (se = 0.481, p-value = .035). In contrast, there is no evident change in the quit elasticity for old high-school workers or for four-year college grads, young or old. Thus, much of the rise in job-to-job transitions is occurring among low-wage workers employed in 'low-rent' jobs, i.e. jobs that pay particularly low wages, conditional on observable worker characteristics.

4.3 Job changes and wage changes

In the job-ladder model, job separations spike as the labor market tightens and workers transition to higher-paid jobs. Accordingly, wage gains stemming from a tightening labor market should be concentrated among job movers. We test that prediction here.

Let $\Delta w_T = \Delta w_T^M \Delta J_T + \Delta w_T^S (1 - \Delta J_T)$ denote the mean wage change for a demographic group in time interval T, equal to the wage change among job-movers, Δw_T^M, times the estimated (12-month) job switch rate[31], ΔJ_T, plus the wage change among job-stayers, Δw_T^S, multiplied by the complement of the switch rate. Using this identity, we can decompose the change in wage growth between two periods, 2015–2019 ($T(t) = 1$) and 2021–2022 ($T(t) = 2$), using the following equation:

[31] Note that the 12-month job-to-job separation rate is not directly observed, but is instead estimated based on the monthly job-to-job separation rate under the assumption of a constant hazard rate.

$$\Delta w_2 - \Delta w_1 = \underbrace{\left(\Delta w_2^M - \Delta w_1^M\right)\Delta J_1}_{Movers} + \underbrace{\left(\Delta w_2^S - \Delta w_1^S\right)(1-\Delta J_1)}_{Stayers} + \underbrace{(\Delta J_1 - \Delta J_2)\left(\Delta w_2^M - \Delta w_2^S\right)}_{Switch\ rate}. \quad (7)$$

Given estimates of $\left\{\Delta w^M, \Delta w^S, \Delta J\right\}$, equation (7) apportions the observed change in wage growth across time periods for a given demographic group into three components: the change in the job-mover premium, scaled by the switch rate; the change in the job-stayer premium, scaled by the complement of the switch rate; and the change in the switch rate, scaled by the difference between the mover and stayer premia.

Because it is infeasible to accurately measure year-apart *job* changes in the Current Population Survey, as detailed below, we instead focus on an error-corrected *industry* change measure. Prior to elaborating this methodology, we demonstrate the value of industry change as a measure of earnings mobility. For this exercise, we explore whether the rate of net worker mobility from lower-paid to higher-paid industries increased following the pandemic. As in Section 4.2, industry wage premia, $\tilde{w}_j$, are measured as industry fixed effects, obtained from a cross-sectional Mincerian wage regression that is estimated separately by subgroup. For this exercise, we subdivide industries into four groups based on ranked wage premia, $Q(\tilde{w}_j)$, with each containing 25% of employment in 2015–2019.[32] We fit the following linear probability model to characterize the probability that a worker who is employed in the lower half of the industry wage premium distribution in year $t-1$ is employed in the upper half of the distribution in the following year t:

$$E\left[\mathbb{1}\left[Q(\tilde{w}_{j(i),t}) = 3 \vee 4\right] \middle| Q(\tilde{w}_{j(i),t-1}) = 1 \vee 2\right] = \alpha_{T(t)=1} + \alpha_{T(t)=2}. \quad (8)$$

The coefficients $\alpha_{T(t)=1}$ and $\alpha_{T(t)=2}$ in equation (8) capture the probability of upward industry mobility during years 2015–2019 and 2021–2022, respectively. Figure 26 and Table 4 report estimates for both the full sample of employed workers and the subsample of non-college workers under age 40.

Movements across the two halves of the industry premium distribution in a given month are relatively rare: we estimate this probability at 0.49% prior to 2020 for the full sample of workers. Following the pandemic, this probability rose to 0.52%, an increase that is not statistically or economically significant. Mobility among young non-college workers is

[32] Quartile groupings are kept constant after 2019, allowing industries at various wage levels to grow or contract as a share of employment.

75% higher than across the full sample of workers: 0.85% versus 0.49% during 2015–2019. Moreover, as our conceptual framework anticipates, and as shown in the third set of bars in Figure 26, upward industry wage mobility among young non-college workers rose significantly after the pandemic, from 0.85% to 1.05% (compared with 0.49% to 0.52% for the overall sample).[33] If this rise in upward industry mobility were accompanied by a comparable increase in downward industry mobility, the net effect would be a wash. This is not the case, however. Figure 26 finds no increase in downward mobility, either overall or among young non-college workers (this is also confirmed in panel C of Table 4).[34]

Figure 27 and Table 5 refine this exercise by analyzing movements into and out of the bottom quartile of industries. The probability of upward mobility from the bottom quartile is roughly twice that of the entire bottom half of the distribution and it has increased by substantially more. The overall bottom-quartile mobility rate among the full population rose modestly from pre- to post-2020: 0.98% before and 1.08% after. Among young non-college workers, the gain was quantitatively larger (and highly precise): a 40% increase, from 1.41% to 2.00%. There was no corresponding increase in downward mobility. These findings indicate that the rise in the EE separation elasticity documented above reflects a net reallocation of young non-college workers away from particularly low-premium (i.e., low-rent) sectors.

In comparing upward and downward mobility rates across sectors, it is critical to account for size differences among the underlying risk sets. Since, by definition, there are three times as many workers in the top-three quartiles than in the bottom quartile, the exit rate from the top-three quartiles of the distribution would need to exceed the exit rate from the bottom quartile by a factor of three in order to maintain the initial, steady-state distribution. Thus, in constructing up-down comparisons in Table 5 and Figure 27, we adjust for relative sizes of the risk sets by multiplying downward movements by the factor $(1-p)/p$, where p is the fraction of workers in the bottom quartile ($p = .25$).

Figure 28 and Table 6 provide a third perspective on sectoral mobility by charting movements from the typically low-paid hospitality sector. According to our IWP estimates, the hospitality sector is slightly below the 10^{th} percentile of the residual industry wage distribution. The share of hospitality workers in the overall sample fell from 8.0% in 2015–2019, to 7.4% in 2021–2022. Among high-school workers under age 40, the share of hospitality

[33] These probabilities likely overstate the frequency of upward movements because measurement error in industry assignments will generate false transitions. But the estimated *change* in this probability should be unbiased unless measurement error changes.

[34] Downward mobility is not simply the mirror of upward mobility for two reasons. First, on average, workers tend to move upward in the wage distribution as they age—particularly young workers—so we expect some upward mobility at the person level. Second, high-premium industries may expand and low-premium industries contract, leading to a rise in aggregate upward industry mobility.

workers fell from 18.7% before 2020, to 18.3% after. Among the overall working population, the flows out of and into hospitality both rose modestly—though the former experienced a larger rise, yielding a small increase in the net exit rate. Among young non-college workers, however, the exit rate rose sharply from 1.46% to 1.98%, with a smaller increase in the adjusted entry rate. Movements into the hospitality sector in Figure 28 and Table 6 are also scaled by $(1-p)/p$, where p is the fraction of workers in the hospitality industry in 2015–2019 ($p = 0.080$ for the overall sample and $p = 0.187$ for HS under 40).

Summarily, evidence from all three measures of industry mobility suggests that the unexpected, post-pandemic compression of the US wage distribution reflects—at least in part—rising net flows of young non-college workers into higher-wage industries. This pattern motivates a formal examination of the role of industry-change versus on-the-job wage growth to account for the rising relative wages of young non-college workers.

4.4 The contribution of cross-industry mobility to wage growth

A key challenge in decomposing the role of job-movers and job-stayers in overall wage growth, using equation (7) above, is that the Current Population Survey does not provide reliable measures of job change or industry change at annual frequencies. As noted above, the first outcome, job change, is observed in only a subset of months over the course of a year. The latter outcome, industry change, is always available—but with substantial measurement error, which leads to spuriously high rates of measured industry-moving. We address both limitations by leveraging a period of overlap for the self-reported job change and measured industry change measures, which are contemporaneously available during the final two months that each respective worker is present in the CPS sample (MIS 7 and 8). This overlapping period enables us to error-correct the CPS industry-change measure and to calculate the wage changes associated with true industry changes versus true industry stays (i.e., non-changes).

We proceed, first calculating an error-corrected measure of 'true' industry changes, then estimating the wage changes associated with industry-moves versus industry-stays. Table 7 summarizes the inputs into this procedure. Panel A cross-tabulates the frequencies of industry changes and job changes using the final two months of each worker's time in the CPS sample, as explained above. We define a true industry move, $\Delta I^* = 1$, as the joint event that the worker reports a job change and that the Census assigns the worker a new industry code. Panel A of Table 7 reports that the unconditional monthly probability of a true industry move (row A1), $E[\Delta I^*] = \Pr[\Delta I = 1 \wedge \Delta J = 1]$, among young high-school workers is 1.9% during 2015–2019 and rises to 2.2% after 2020. Row A2 reveals that a non-trivial fraction of industry changes are spurious, meaning that they overstate the true frequency of industry changes. Among young high-school educated workers, 79% of industry changes coincide with a self-reported job change (in both the pre- and post-pandemic periods) while the other 21% do not. Among the complementary set of all other workers, around 65% of industry changes coincide with a job change while 35% do not.[35] In rows A3–A5, we use the monthly industry move probability (and a constant-hazard assumption) to derive the probability that there was no industry move for a given worker in the past year—including the 8-month period between MIS 4 and 5 when individuals are out of the CPS sample. The estimated probability of 'no industry move in the past year' ranges from 76% to 86% across samples and periods (row A5).

The second step in our procedure requires that we estimate annual wage changes among industry-stayers and industry-movers respectively, denoted as $w^S \equiv E[\,\Delta w|\sum_{m=1}^{12}\Delta I_m^* > 0]$ and $w^M \equiv E[\,\Delta w|\sum_{m=1}^{12}\Delta I_m^* = 0]$. Panel B of Table 7 provides relevant information for this calculation, summarizing mean *annual* wage changes among workers—both overall (row B1) and by true industry-change status (rows B2 and B3)—in a worker's final month in the sample. By definition, any worker who has experienced a true industry move in the past month has experienced a true industry move in the past year: $\widehat{w}^M = E\left[\Delta w \middle| \sum_{m=1}^{12}\Delta I_m^* > 0\right] = E\left[\Delta w\,|\Delta I^* = 1\right]$. Hence, we can read $\widehat{w}^M$, the annual wage change among true industry-movers, directly from the table (row B2). Among the complementary set of workers who do not experience a true industry move in the final sample month, some will have experienced a true industry move in the preceding 11 months. We estimate this share by assuming that the monthly true industry-move hazard is constant at $E[\,\Delta I^*]$. This implies that the probability of a true industry move in the prior 11 months for a worker who did not experience an industry move in their 12^{th} month (row A3) is $\Pr\left[\left(\sum_{m=1}^{11}\Delta I_m^*\right) > 0 \middle| \Delta I_{12}^* = 0\right] = 1 - \Pi_{m=1}^{11}\left(1 - \Pr\left(\Delta I_m^* = 1\right)\right)$. Hence, we can estimate the (annual) probability of a true industry move in the past year (row A4) as $\Pr\left(\sum_{m=1}^{12}\Delta I_m^* > 0\right) = \Pr\left[\left(\Delta I_{12}^* = 1\right) \cup \left[\left(\sum_{m=1}^{11}\Delta I_m^*\right) > 0 \middle| \Delta I_{12}^* = 0\right]\right] = 1 - \Pi_{m=1}^{12}\left(1 - \Pr\left(\Delta I_m^* = 1\right)\right)$. Likewise, the probability of no industry move in the past year (row A5) is estimated as $\Pr\left(\sum_{m=1}^{12}\Delta I_m^* = 0\right) = \Pi_{m=1}^{12}\left(1 - \Pr\left(\Delta I_m^* = 1\right)\right)$.

We can then use the observed annual wage change among all workers (row B1), $\Delta\widehat{w}$, the estimated annual wage change among true industry movers (row B2), $\Delta\widehat{w}^M$, and the estimated annual probability of a true industry move (row A4), $\Pr\left(\sum_{m=1}^{12}\Delta I_m^* > 0\right)$ to derive the annual wage change among true industry-stayers (row B3) according to the following identity:

[35] While a worker can change jobs without changing industry, the reverse should not occur. Accordingly, we are comfortable labeling those industry changes that do not coincide with a job change as spurious.

$$\Delta\widehat{w} \;=\; \Delta\widehat{w}^{M} \times \Pr\left(\sum_{m=1}^{12} \Delta I_m^* > 0\right) + \Delta\widehat{w}^{S} \times \left(1 - \Pr\left(\sum_{m=1}^{12} \Delta I_m^* > 0\right)\right). \tag{9}$$

Except for $\Delta\widehat{w}^S$, all terms in this equation are observed or estimated. Hence, $\Delta\widehat{w}^S$ can be calculated as the solution to this equation. Estimates of all input terms for equation (9) are reported in panels A and B of Table 7.

Finally, we bring these estimates to the wage decomposition in equation (7) to determine the roles of industry moving and industry staying in overall wage change. Figure 29 summarizes the results. The first set of bars in panel A of this figure shows the delta in average year-over-year wage change among CPS respondents between the 2015–2019 and 2021–2022 periods. For the high-school under-40 group, this rate was 0.7 log points lower in 2021–2022, reflecting the impact of rising inflation. This 0.7 log point slowdown is more than accounted for by a 2.5 log point fall in the industry-stayer premium for young high-school workers (the 2^{nd} set of bars). By contrast, and consistent with the predictions of the job ladder model, both the mover premium and the industry move rate increased substantially among young high-school workers as the labor market tightened (the 3^{rd} and 4^{th} sets of bars). Specifically, the rising industry-mover premium contributed 1.3 log points to wage growth among young high-school workers while a rising move rate contributed another 0.40 log points.

The lighter color bars in panel A of Figure 29 replicate the same exercise for the complementary set of workers—those who are not young high-school workers. Among this group, the stayer premium became substantially more negative after the pandemic. Conversely, both the mover premium and the move rate increased, partly offsetting the effect of the falling mover premium. Nevertheless, these positive impacts of a rising mover premium and rising move rate were far smaller than among young high-school workers. In net, the increasing gains from industry moving—a higher move premium, a higher move rate—were concentrated among young non-college workers.

The second panel of Figure 29 formalizes this comparison by contrasting the components of wage change among young high-school workers with those of all other workers. Comparing the changes for each group between the 2015–2019 and 2021–2022 periods, the first bar in this panel shows that wage growth was 2.6 log points greater among young high-school workers than among other workers. Wage changes among both industry-stayers and industry-movers, as well as changes in the industry move rate, differentially benefited young high-school workers. Of the 2.6 log point differential increase in wage growth among young high-school workers, 1.4 log points is explained by greater wage growth among young high-school stayers (scaled by the initial stay rate), 1.1 log points by greater wage growth among young high-school movers (scaled by the initial move rate), and 0.1 log points by a differential increase in the move rate among young high-school workers (scaled by the difference in wage growth among industry-movers versus industry-stayers). Note further that the pre- versus post-2020 increase in wage growth was far more positive among industry-movers than industry-stayers—and differentially so among young high-school workers—as predicted by the job ladder model: 6.7 versus -3.1 log points among young high-school workers, and 2.4 versus -3.4 log points among all other workers. Although industry moves are less than one-fourth as frequent as industry stays (panel A of Table 7), they nevertheless account for 40% of the differential post-2020 wage growth among young high-school worker versus other workers (Figure 29).

These findings shed light on how tight labor markets lead to higher wages. Standard macroeconomic models of the labor market (Blanchard and Galí, 2010; Mortensen and Pissarides, 1994) typically assume that the higher labor demand associated with increasing tightness improves workers' bargaining positions, which leads them to renegotiate for higher wages. Strikingly, a large part of the wage growth we have seen is not of this variety. Wage growth among stayers has risen much less than wage growth among movers—in particular, among those movers who exit industries that pay low wages conditional on worker observables. This type of adjustment highlights the importance of changes in competition—especially through on-the-job search and frictional wage dispersion—as important building blocks to understand how labor market tightness affects wages.

5 Nominal wage, tightness, and inflation

The tightening labor market during 2021–2022 was central to the increase in labor market competition—and the resulting compression in wages—following the pandemic. The same 2021–2022 period *also* saw a rapid rise in prices, at a rate unprecedented since the Great Inflation of the 1970s and early 1980s, with the headline CPI-U inflation reaching a 12-month peak of 9.0 in June 2022. Was the labor market tightness that produced the unexpected wage compression also responsible for this surge in inflation? An answer in the affirmative would suggest an unappealing trade-off: a highly competitive labor market that strongly raises lower-tail wages but requires an unsustainable rate of inflation. If, however, the impact of the unexpected compression on price levels was more modest, this would suggest a less-severe trade-off between fostering labor market competition and stoking inflation.

We estimate the relationship between state-level tightness and price inflation using a regional price-Phillips curve in parallel to our previous analysis of the wage-Phillips curve. We consider the growth in prices between 2021_{q1q2} and 2022_{q2q3}. Annualized headline CPI inflation over this period was 8.4%. We use the headline CPI-U, which includes the full basket of goods and services, for the purpose of quantifying real wage trends while allowing for regional variation in prices. However, it is widely recognized that the headline inflation surge was, in part, driven by volatile global energy prices, which were exacerbated by the Russia-Ukraine conflict of 2022. Therefore, we focus on CPI-less-energy as the key price index for our Phillips curve analysis (similarly to Cerrato and Gitti (2022)). Over the same period, (annualized) CPI-less-energy inflation was 6.7% nationally. We then assess how much our measure of labor market tightness is associated with regional differences in price inflation using this index.

We construct state-level price changes as follows: we apply the 21 CBSA-level CPI-U deflators to the main metro areas in each state, the state average of CBSA-level CPI-U deflators to other (non-main) metro areas within each state, and census division-level CPI-U deflators to the remainder of areas. Using pooled worker-level CPS observations for each of these six-month periods, we fit the following equation, analogous to the specification for estimating the wage-Phillips curve:

$$\ln P_{st} = \alpha_t + \beta \text{Tightness}_{s,2021_{q3}-2022_{q1}} \times [t = 2022_{q2q3}] + X_i^{'}\gamma + \delta_s + e_{ist}, \qquad (10)$$

where state-level tightness is measured at the mid-point between the start- and end-period price observations ($2021_{q3} - 2022_{q1}$). Recall that due to the inclusion of state δ_s and time α_t fixed effects, with an interaction between the tightness measure and a dummy for the second period (2022_{q2q3}), the β coefficient estimates the relationship between the *change* in state-level log prices from 2021_{q1q2} to 2022_{q2q3} and the *level* of labor market tightness in the middle period ($2021_{q3} - 2022_{q1}$).[36]

Figure 30 plots the price-Phillips curve, as well as the wage-Phillips curves for high-school workers under 40 and for the workforce overall, superimposed on the binscatters associated with each curve. The 'overall' wage-Phillips curve and the price-Phillips curve are nearly identical, with matching slope estimates of 0.011 (se = 0.005). This equality is consistent with a simple (two-equation) wage and price determination model, as in Katz and Krueger (1999). At the same time, these estimates of 0.011 are less than one-third of the wage-Phillips curve slope for young workers without a college education, estimated to be 0.038 (se = 0.010). This contrast highlights the fact that the tightness-fueled wage gains are accruing disproportionately to workers whose low earnings levels exert relatively little leverage on the overall wage and price levels.

Panel A of Table 8 reports price-Phillips curve estimates using a variety of specifications. These estimates range between 0.009 and 0.011 depending on the exact set of controls. If we

[36] As with the wage-Phillips estimation, price observations for $2021_{q3} - 2022_{q1}$, the time interval used for estimating the EE separation measure, are excluded from the estimating equation.

consider the period $2021_{q3} - 2022_{q1}$, national-level tightness rose by 1.76 standard deviations (using our combined unemployment and EE-separation index). Taking the preferred estimate of 0.011 (column 5) suggests an increase in non-energy prices of 0.0193 over this period, which translates into a $((1 + 0.0193)^{\frac{12}{16}} \times 100) = 1.5$ percentage point contribution to inflation in annualized terms. Over this same period, annualized non-energy inflation rose by 6.7 percentage points, which suggests that the additional tightness can explain only slightly more than one-fifth of the increase in non-energy price inflation between early 2021 and late 2022. Furthermore, the (annualized) 3-month CPI inflation rate *excluding energy* was around 0.8% in January 2021. Putting these together, the cross-sectional estimates suggest that the rise in labor market tightness between quarters 1 and 2 of 2021 and quarters 2 and 3 of 2022 would have led to a (1.5 + 0.8 =) 2.3 percent annualized inflation in late 2022, absent other disruptions or price dynamics. In reality, the inflation rate stood at a much higher (6.7 + 0.8 =) 7.5 percent.

Panel B of Table 8 reports price-Phillips curve estimates when we use the (non-standardized) unemployment rate alone. The implied price increases associated with these estimates and the change in the unemployment rate, when multiplied by the standard deviation of unemployment (0.0103), are comparable to the estimates with the composite tightness measures. Using EE separation rates alone (instead of the composite tightness measure) in Panel C yields imprecise estimates. This is similar to the wage-Phillips curve estimates in Appendix Table A3, which show that the EE separation rate alone is strongly predictive of wage growth primarily for low-wage workers, i.e., the group for whom EE separations have risen the most. Overall, our conclusions about the relative contribution of labor market tightness on the increase in inflation are similar to the findings in Cerrato and Gitti (2022). In Appendix Table A9, we provide a bridge between various specifications and sample periods used in the two papers.

How does this regional variation in inflation (some of it associated with regional variation in tightness) affect real wage trends? Figure 31 provides a regional perspective on real wage gains across the distribution by deflating wage levels using our constructed *state-level* headline CPI, distinct from the national deflator used in Figure 10. Conclusions about the level and distribution of real wage changes during the two- and three-year intervals ending in August 2022 are essentially unaffected when using regional rather than national price indices. However, this adjustment does matter for the twelve months immediately preceding August 2022. We detect essentially no real wage growth at any point in the wage distribution between August 2021 and August 2022, implying that areas with the largest nominal wage gains were also those with the steepest inflation. Thus, real wage growth in the most recent period is modestly overstated when using the national price deflator.

Finally, to sharpen our focus on the relationship among tightness, inflation, and real wage growth, we estimate real wage-Phillips curves where wages are deflated using our regional, state-level, headline CPI. These estimates, reported in Table 9, reveal how labor market tightness has affected real wage growth for different wage quantiles and demographic groups, net of regional price changes.[37] For the workforce overall, regionally-deflated real wages were largely unaffected by tightness. This is expected, given the nearly identical slopes of the underlying wage- and price-Phillips curves.

However, this aggregate relationship masks important heterogeneity along the wage distribution, as suggested by Figure 30. The bottom two quartiles of the distribution experienced real wage gains, corresponding to precisely estimated increases of 0.048 (1^{st} quartile) and 0.033 (2^{nd} quartile) log points for each one-standard deviation increase in state-level tightness. In contrast, the upper two quartiles experienced real wages losses, corresponding to decreases of -0.015 log points (both quartiles) for each one-standard deviation increase in state-level tightness (though this estimation is imprecise for the fourth quartile). When we look across educational groups, workers under 40 with less than a Bachelor's degree saw pronounced real wage gains from increased tightness, even after accounting for regional inflation. In contrast, additional inflation from market tightness significantly eroded the wages of workers with a college degree. Summarily, the tightening labor market following the height of the pandemic is strongly associated with wage compression—relative and real wage gains at the bottom of the distribution that reduce inequality and increase the purchasing power of low-paid workers.

6 Conclusion

Labor market tightness following the height of the Covid-19 pandemic led to an unexpected compression in the US wage distribution that reflects, in part, an increase in labor market competition. Disproportionate wage growth at the bottom of the distribution reduced the college wage premium and reversed the rise in aggregate wage inequality since 1980 by approximately one quarter, as measured by the 90-10 ratio. The rise in wages was particularly strong among workers under 40 years of age and without a college degree.

Wage compression, associated with rapid nominal wage growth, was accompanied by rising job-to-job separations—especially among young non-college workers. These two phenomena are closely linked. State-level, post-pandemic labor market tightness became strongly

[37] To discern the impact of regional price adjustment, one can compare these wage-Phillips curve estimates with analogous, national price-deflated estimates presented in Table 1.

predictive of aggregate wage compression, substantial real wage growth among low-wage workers, and re-allocation toward higher-paying sectors. While tightness was also associated with higher local area price inflation, the increase in tightness between 2021 and 2022 explains only slightly more than one-fifth of the increase in price inflation during that period. Importantly, even accounting for its impact on inflation, labor market tightness spurred real wage growth for the lowest-earning quartile of workers.

The post-pandemic rise in labor market tightness—and the consequent wage compression—represent a profound shift in US labor market conditions, seen most clearly in the rise of the wage-separation elasticity among young non-college workers. The collage of evidence above leads us to tentatively conclude that the pandemic increased the elasticity of labor supply to firms in the low-wage labor market, reducing employer market power and spurring rapid relative wage growth among young non-college workers who disproportionately moved from lower-paying to higher-paying and potentially more-productive jobs.

This evidence has several limitations. One is that the relatively small size of the monthly CPS sample provides, at most, adequate precision for testing some of the key empirical implications of the imperfectly competitive model. Additionally, the infeasibility of accurately tracking workers' job changes over the course of a year requires us to focus on industry change rather than job change as a measure of worker mobility, though job change is the object of interest underscored by theory. Third, and most critically, our evidence on the rise of the quit elasticity relies on using either own-wage residuals or estimated industry premia to proxy for rents—that is, the wage premia (or deficits) that workers receive, relative to their competitive wage level. A stronger test of the evolution of the quit elasticity would employ establishment-level measures of wage premia that are purged of workers' own skill levels (or fixed effects). Our ongoing work seeks to overcome these limitations by using large samples of matched worker-establishment data to perform fine-grained analyses of worker separations across individual establishments, both in the cross-section and in the years before and after the onset of the pandemic.

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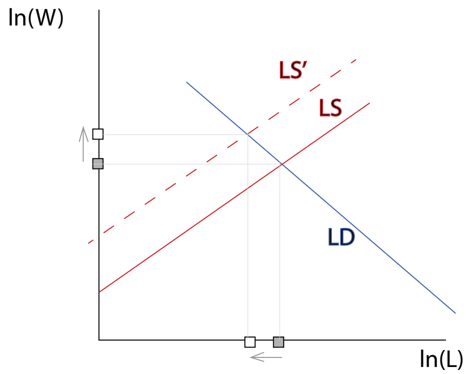
Figure 1: Effect of Inward Labor Shift in Competitive Labor Market

A. Market-level impact



B. Price-taking firm response

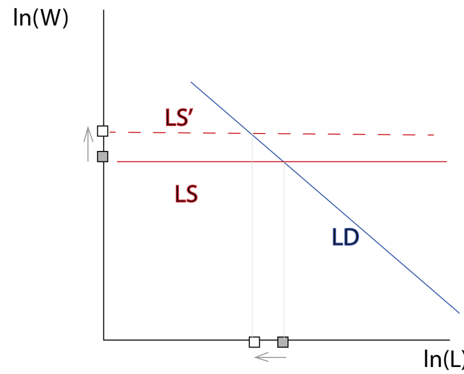
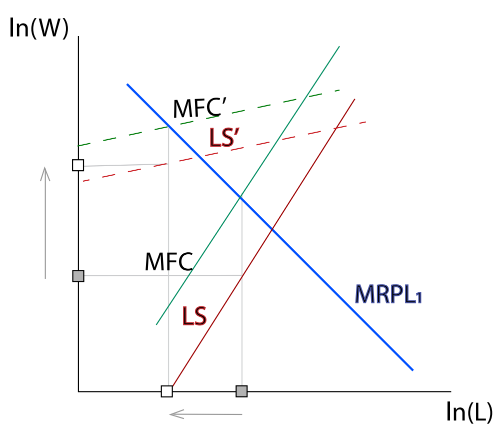


Figure 2: Effect of Rotation of Labor Supply Curve in Monopsonistic Labor Market

A. Low-productivity monopsonistic firm



B. High-productivity monopsonistic firm

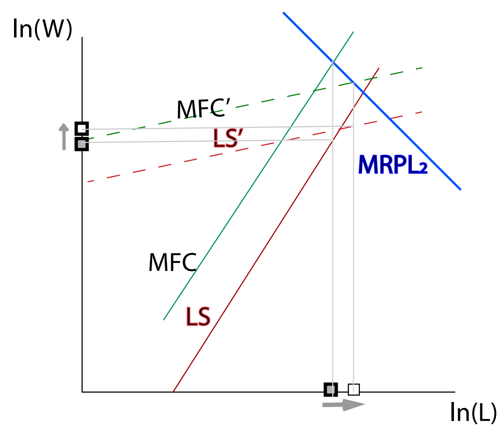


Figure 3: Shift in Job-to-Job Separations and Firm Wage Rank Locus in Response to a Higher Contact Rate

Figure 4: Reallocation of Steady-State Employment by Firm Wage Rank in Response to a Higher Contact Rate

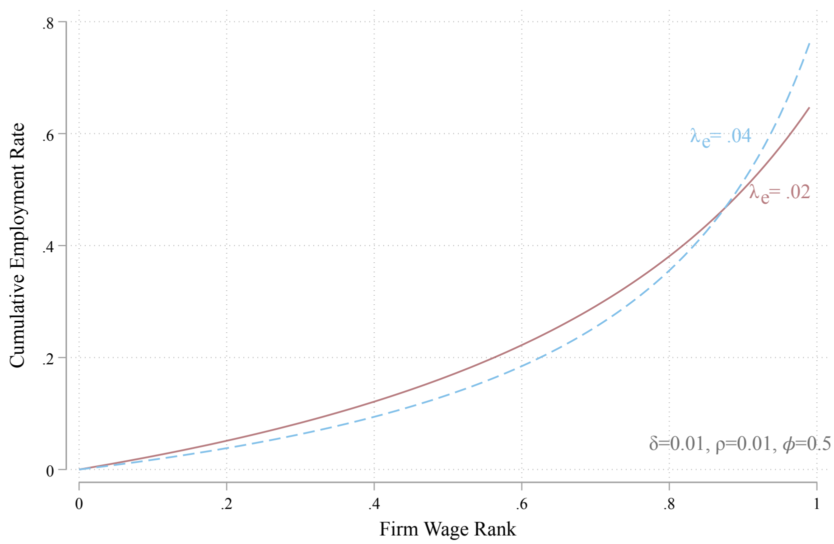
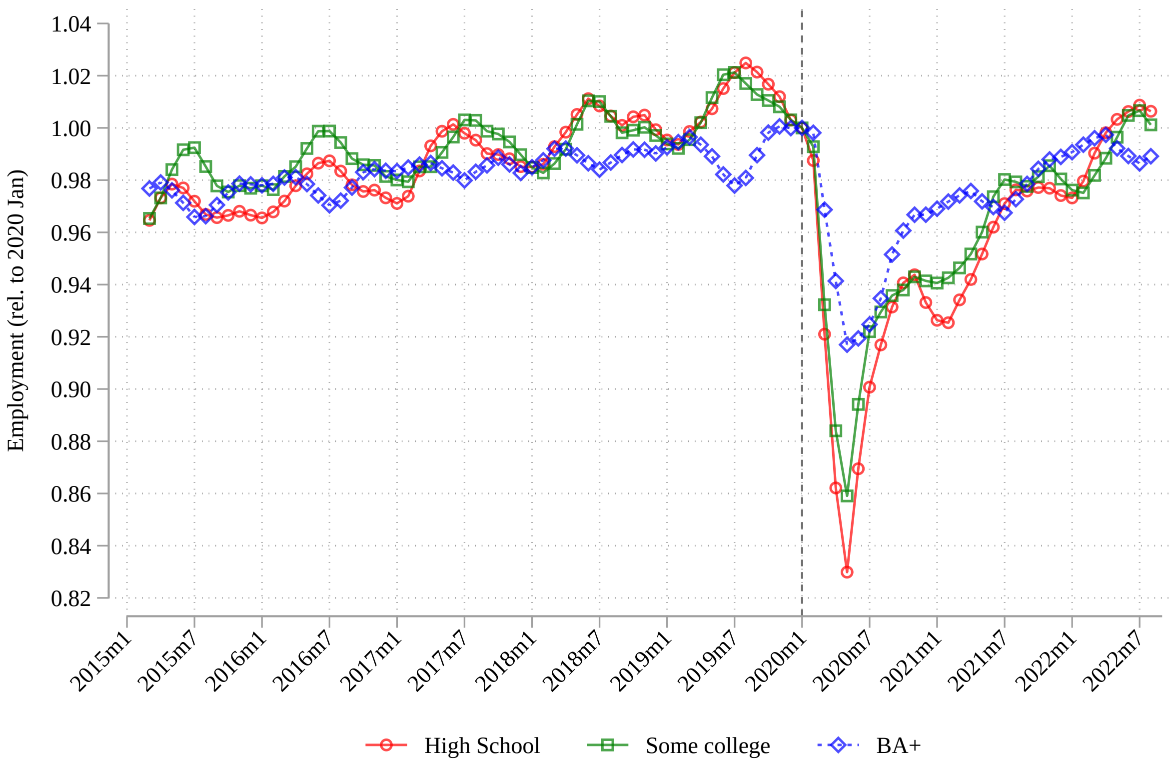


Figure 5: Employment and Labor Force Participation Rates, Relative to January 2020

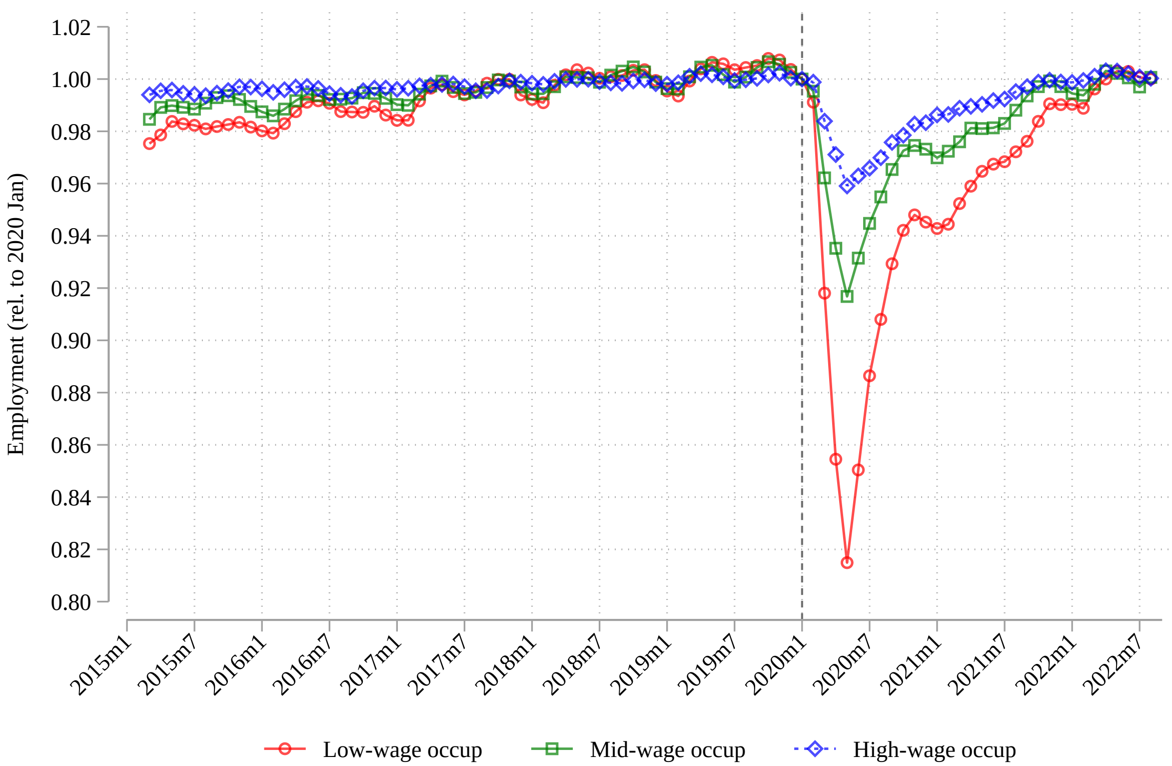
Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Employment is smoothed with a 3-month moving average.

Figure 6: Employment-to-Population Rates by Education, Relative to January 2020



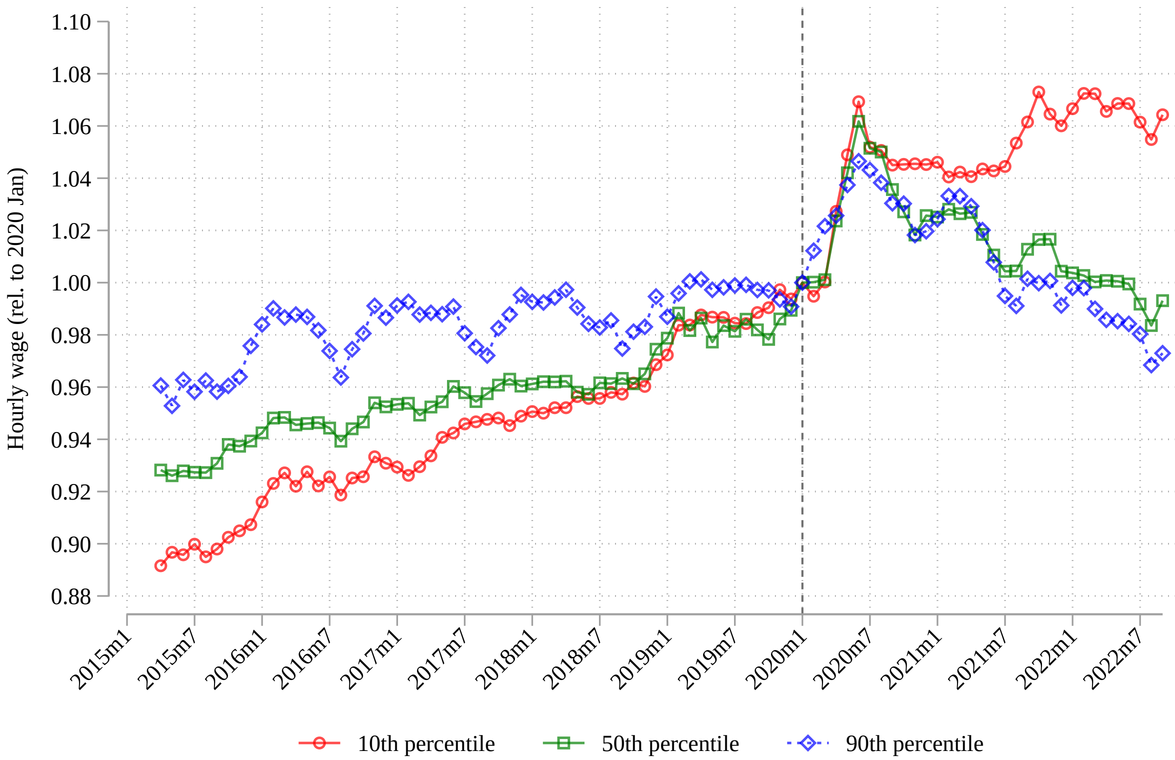
Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Employment is smoothed with a 3-month moving average.

Figure 7: Employment Trends for Low-, Mid-, and High-Wage Occupations, Relative to January 2020



Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Employment is smoothed with a 3-month moving average. Occupational wage terciles are measured pre-pandemic, in 2019.

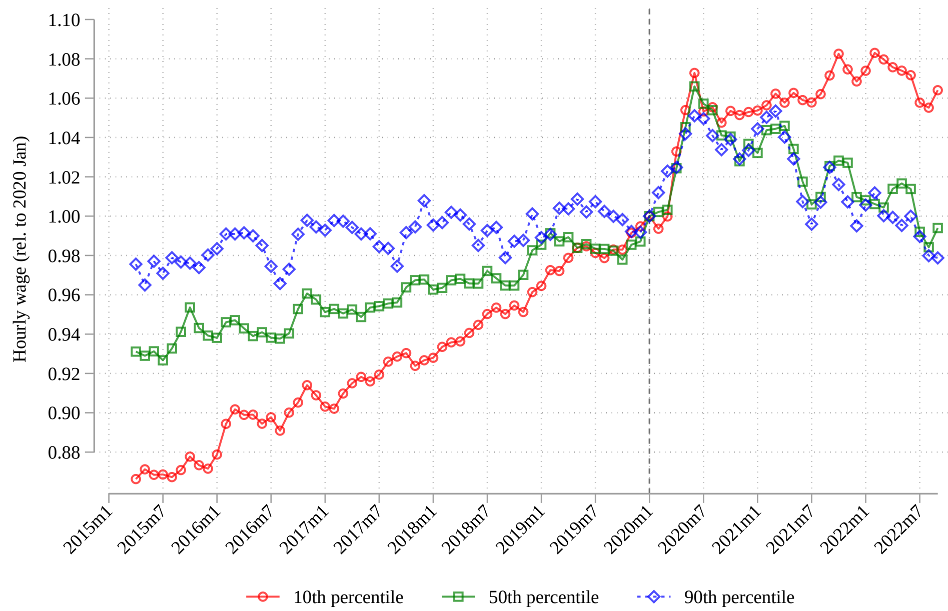
Figure 8: Real Hourly Wages by Quantile, Relative to January 2020



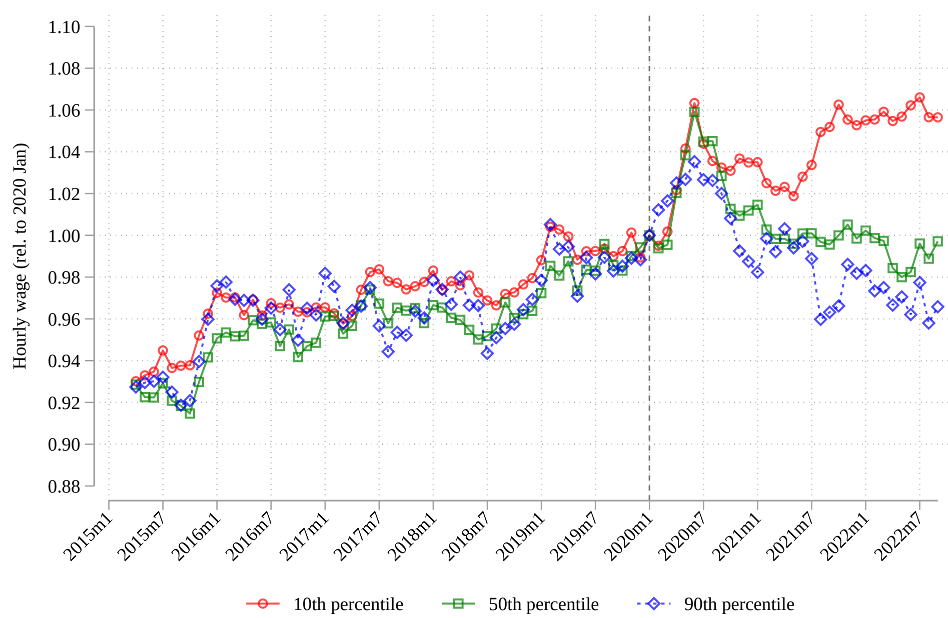
Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Wages are real (2022_{q3} USD). Wage percentiles smoothed with lowess and 3-month moving average.

Figure 9: Real Hourly Wages by Quantile and State Minimum Wage Status, Relative to January 2020

A. State minimum wage above federal level

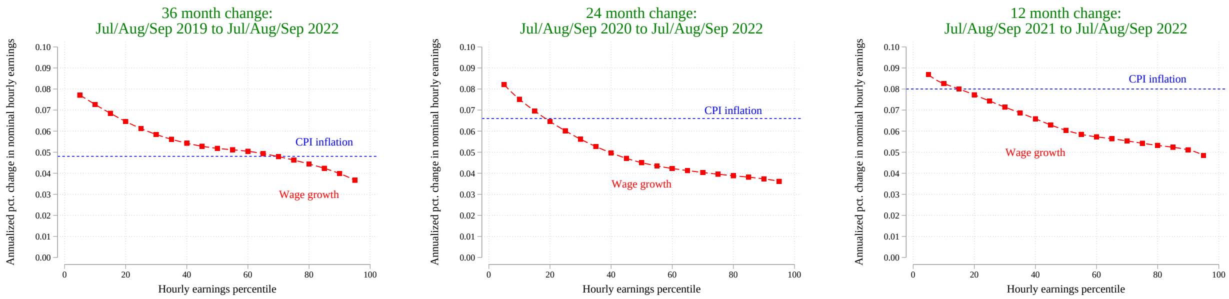


B. Federal or no minimum wage



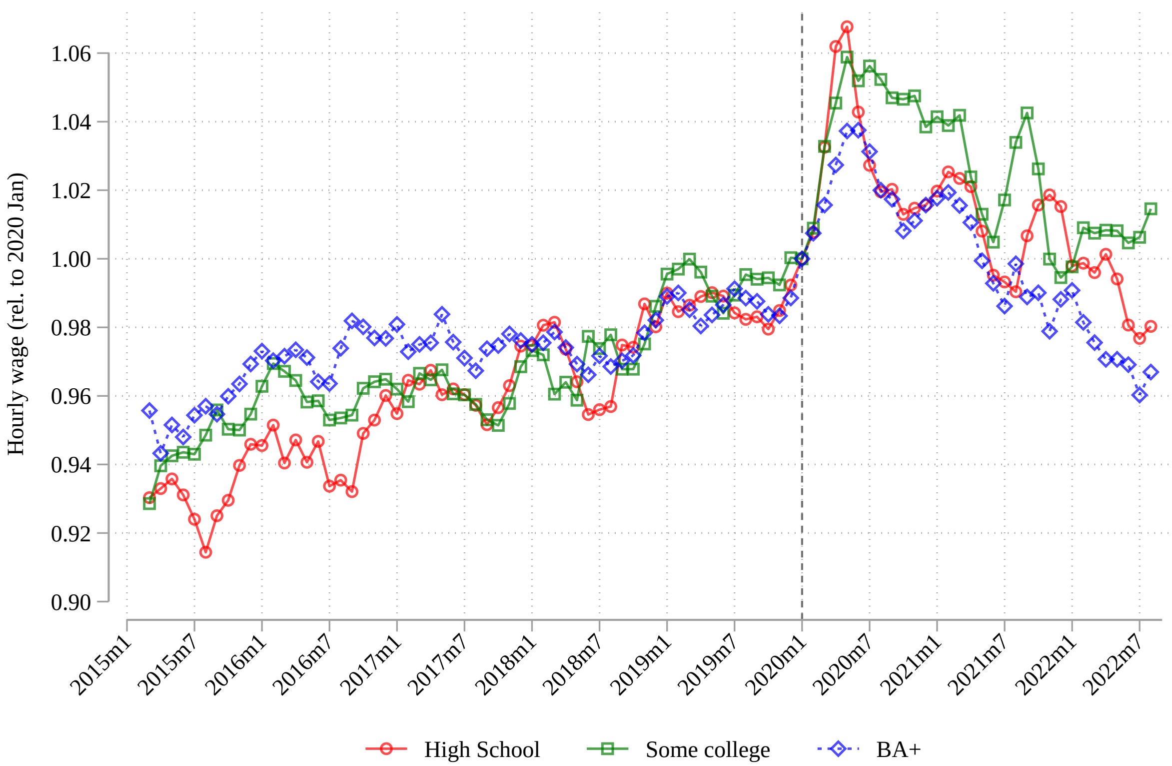
Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Wages are real (2022_{q3} USD). Wage percentiles are smoothed with lowess and a 3-month moving average. Thirty-one US states (including Washington DC) have a minimum wage above the federal level. Fifteen states have a minimum wage equal to the federal level, \$7.25, and 5 states have no minimum wage.

Figure 10: Annualized Percent Change in Nominal Hourly Earnings by Earnings Percentile Over 36, 24 and 12 Months, Adjusted for Composition



Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Wage percentiles are smoothed with lowess. CPI-U is annualized, not-seasonally adjusted, all workers.

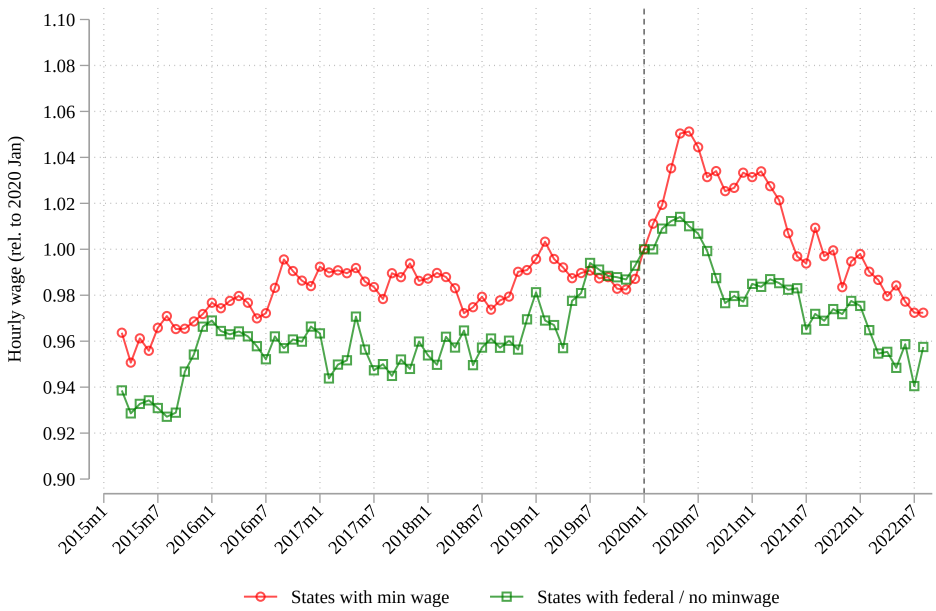
Figure 11: Real Hourly Wages by Education, Relative to January 2020



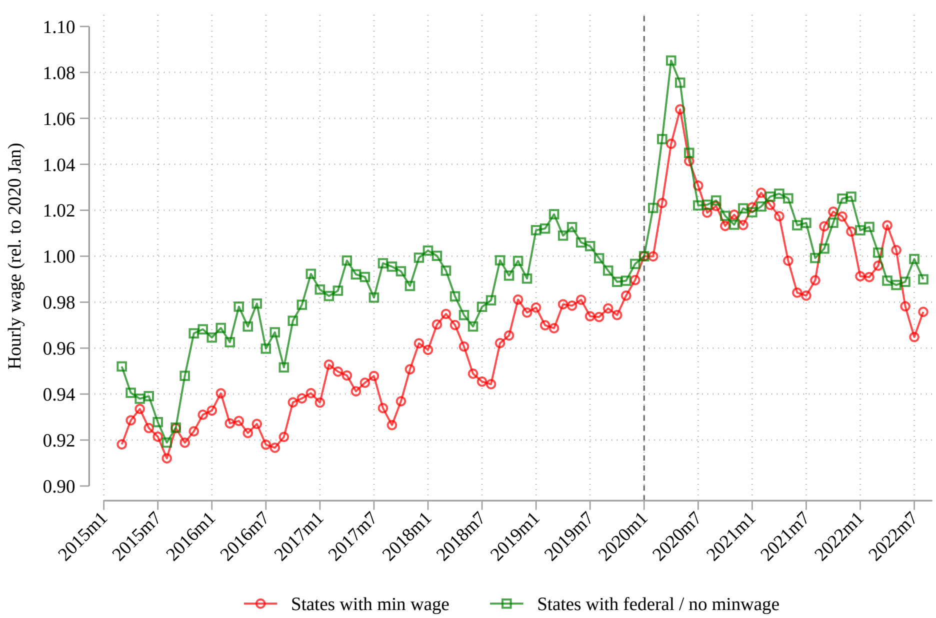
Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Wages are real (2022_{q3} USD) and smoothed with a 3-month moving average.

Figure 12: Real Hourly Wages by Education and State Minimum Wage Status, Relative to January 2020

A. BA+ educated workers

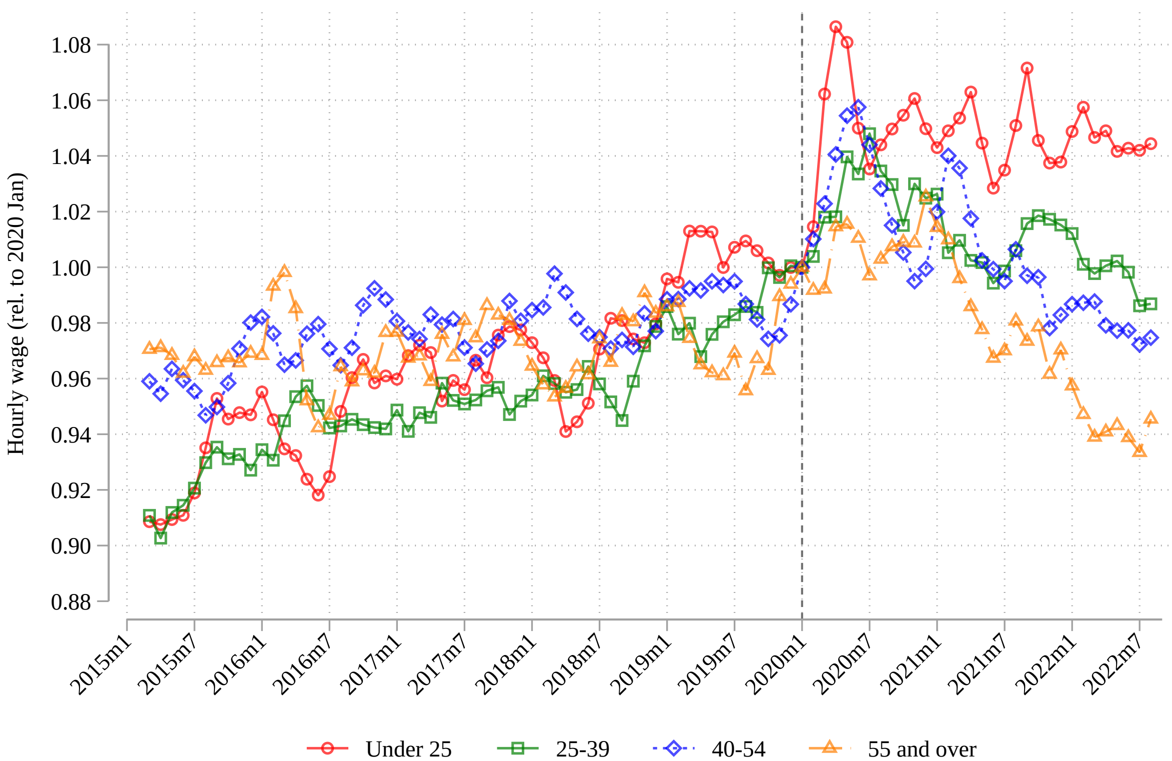


B. High-school educated workers



Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Wages are real (2022_{q3} USD) and smoothed with a 3-month moving average. Thirty-one US states (including Washington DC) have a minimum wage above the federal level. Fifteen states have a minimum wage equal to the federal level, \$7.25, and 5 states have no minimum wage.

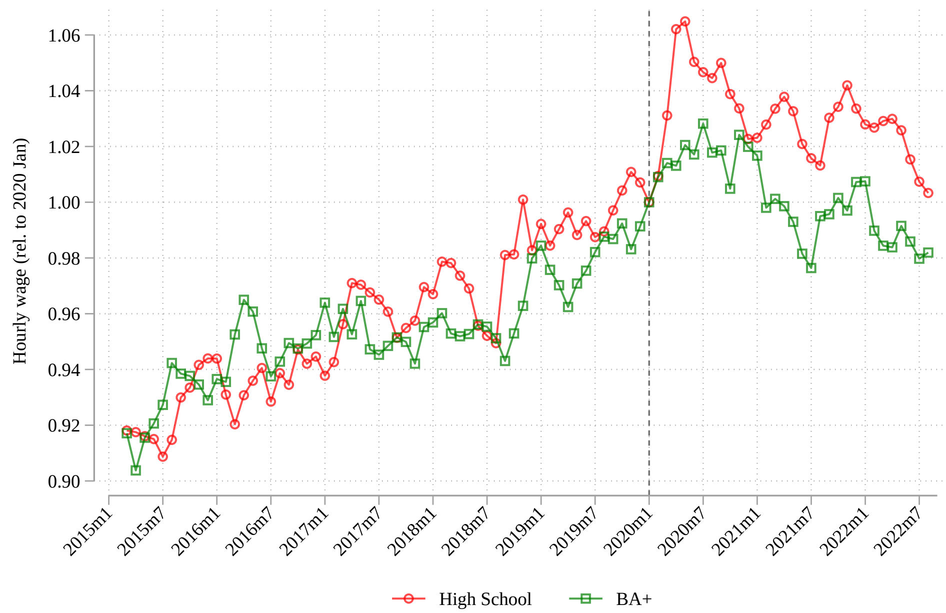
Figure 13: Real Hourly Wages by Age, Relative to January 2020



Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Wages are real (2022_{q3} USD) and smoothed with a 3-month moving average.

Figure 14: Real Hourly Wages by Age and Education, Relative to January 2020

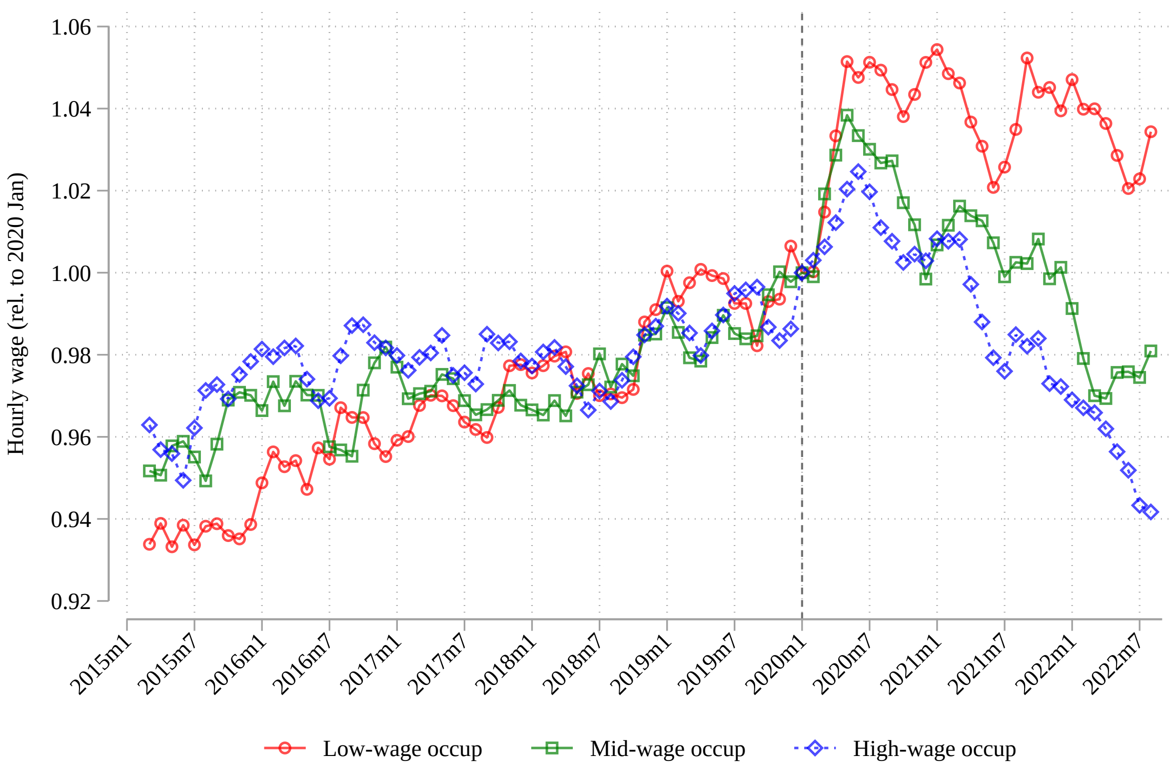
A. Workers under age 40



B. Workers age 40 and above

Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Wages are real (2022_{q3} USD) and smoothed with a 3-month moving average.

Figure 15: Real Hourly Wages by Occupational Wage Tercile, Relative to January 2020

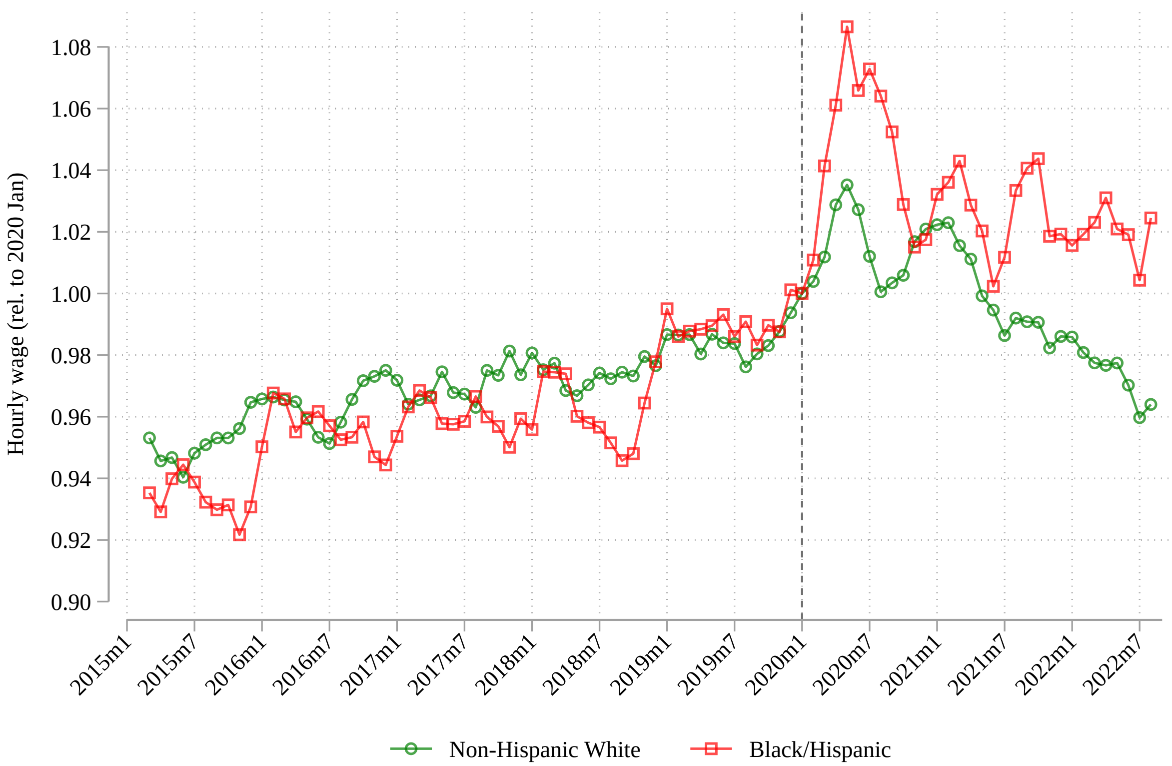


Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Wages are real (2022_{q3} USD) and smoothed with a 3-month moving average. Occupational wage is measured pre-pandemic, in 2019.

Figure 16: Real Hourly Wages by Sex, Relative to January 2020

Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Wages are real (2022_{q3} USD) and smoothed with a 3-month moving average.

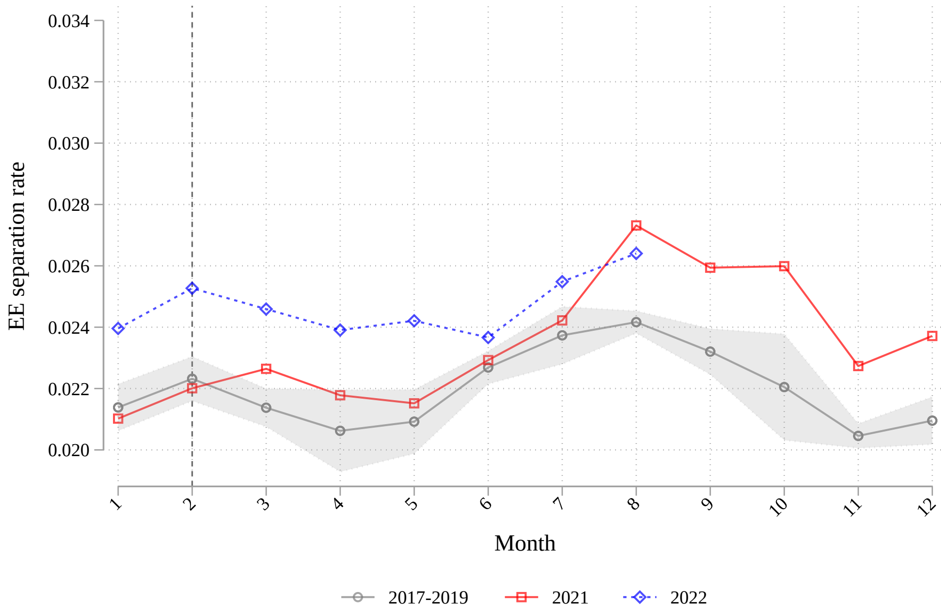
Figure 17: Real Hourly Wages by Race, Relative to January 2020



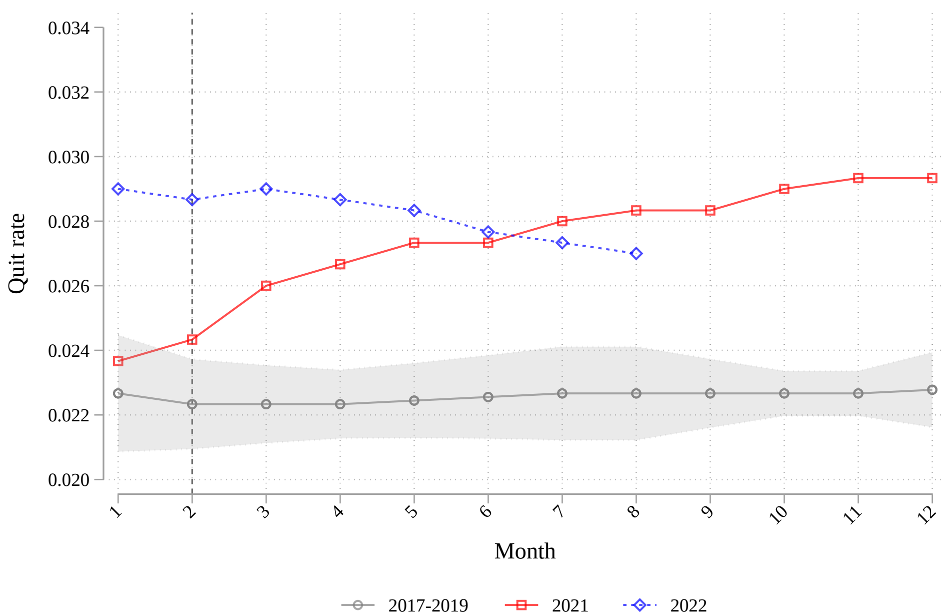
Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Wages are real (2022_{q3} USD) and smoothed with a 3-month moving average.

Figure 18: Job Transitions by Month and Year

A. EE Separation Rate

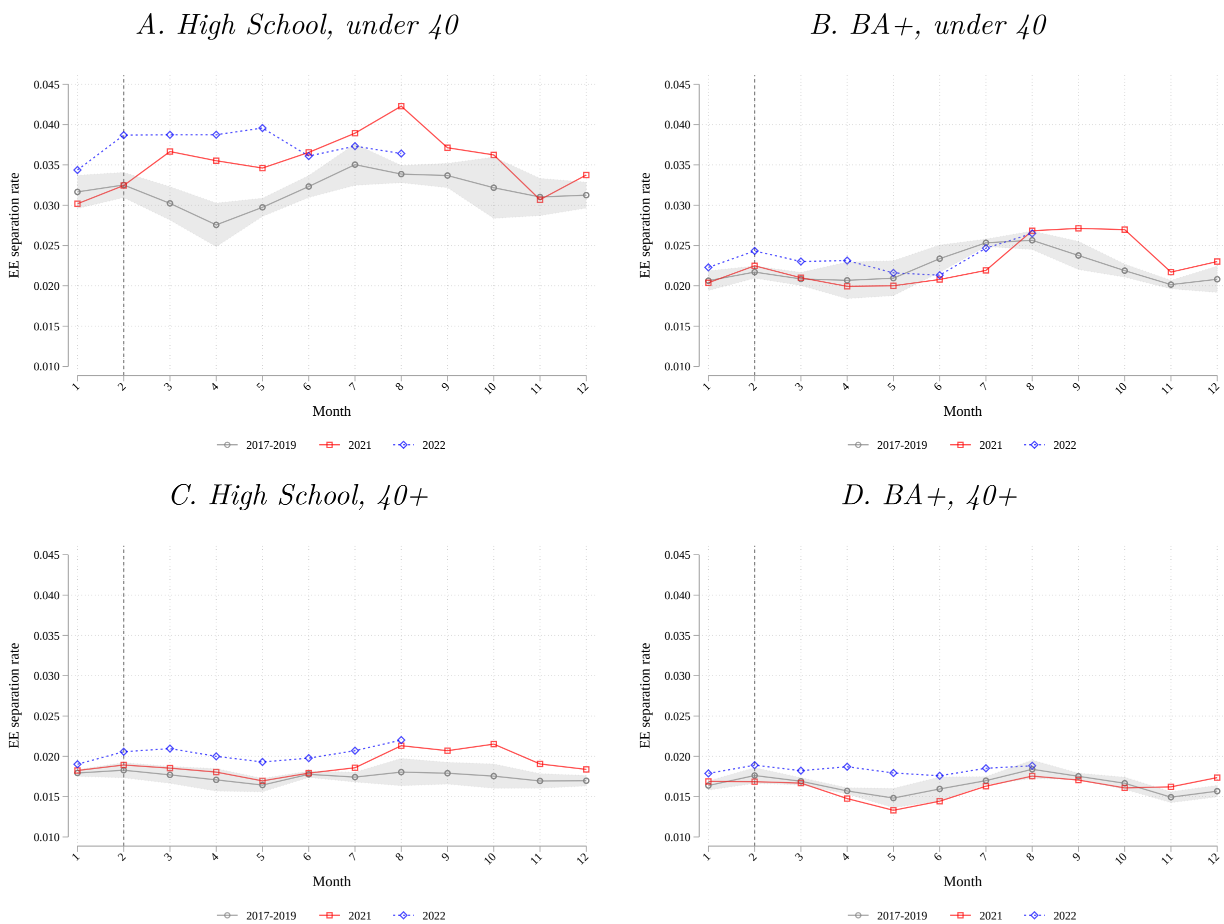


B. Quit Rate



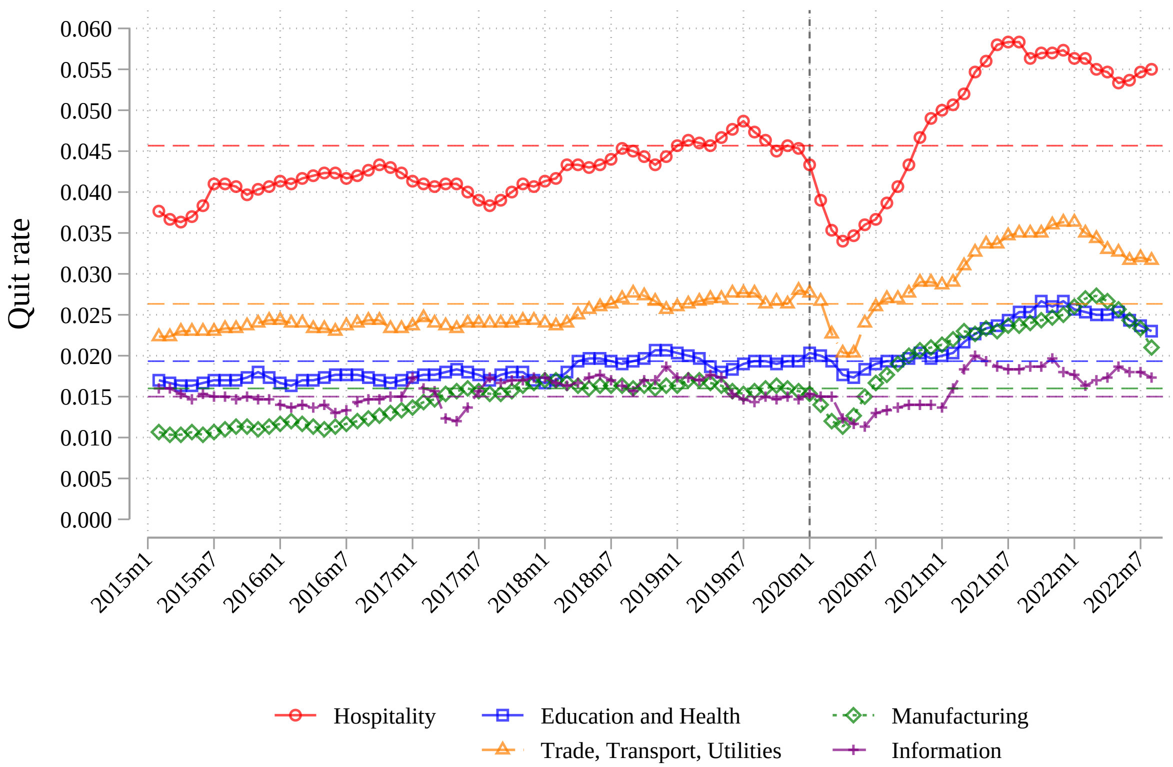
Note: Employment-to-employment (EE) separation rate obtained from CPS monthly data. Quit rate obtained from BLS Job Openings and Labor Turnover Survey (JOLTS) data. EE separation rates and quit rates are smoothed with a 3-month moving average. Shaded areas represent the 95% confidence interval for the monthly EE separation (panel A) or quit rate (panel B) during the 2017–2019 period.

Figure 19: EE Separation Rates by Month and Year: Age and Education



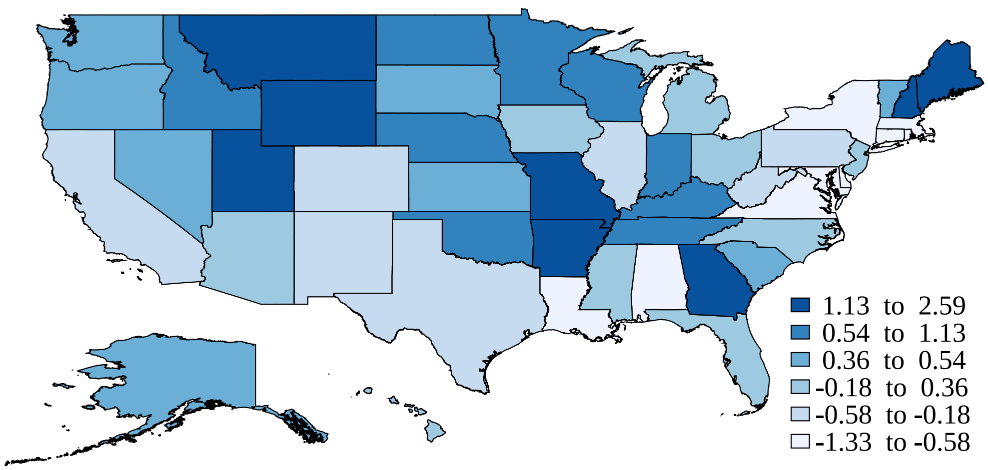
Note: CPS monthly data. Employment-to-employment (EE) separation rate is smoothed with a 3-month moving average. Shaded area represents the 95% confidence interval for the monthly EE separation rate during the 2017–2019 period.

Figure 20: Quit Rate Trends by Sector



Note: BLS Job Openings and Labor Turnover Survey (JOLTS) data. Quit rate is seasonally adjusted and smoothed with a 3-month moving average. Dashed lines represent the industry quit rate in November 2019. Manufacturing sector includes durable and non-durable goods manufacturing. Trade sector includes wholesale trade, retail trade, transportation, warehousing, and utilities. Information sector includes financial activities. Education and health sectors include education services, healthcare, and social assistance. Hospitality sector includes accommodation and food services, arts, entertainment, and recreation.

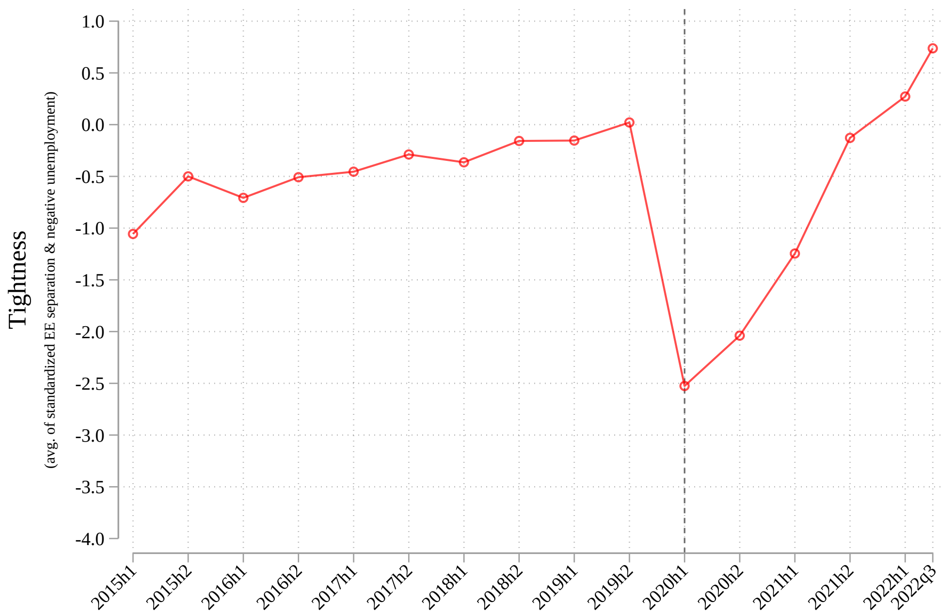
Figure 21: Cross-State Variation in Tightness, July 2021–March 2022



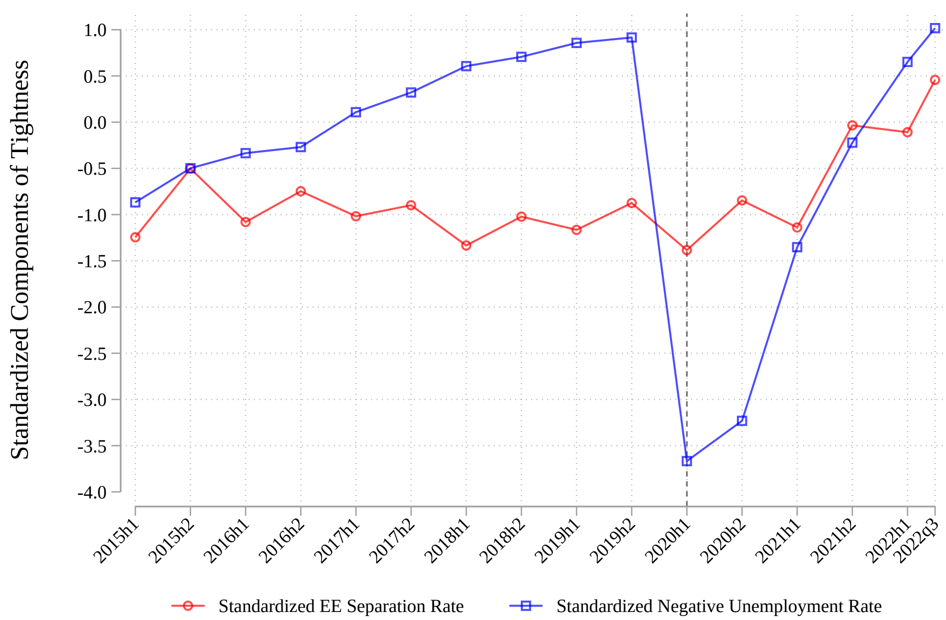
Note: Cross-state variation in tightness over the $2021_{q3} - 2022_{q1}$ period. Tightness is calculated as the state-level average of the standardized EE separation rate and the negative, standardized unemployment rate. EE separation rates obtained from CPS monthly data. Seasonally-adjusted state unemployment rates obtained from BLS LAUS.

Figure 22: Tightness Over Time

A. Tightness



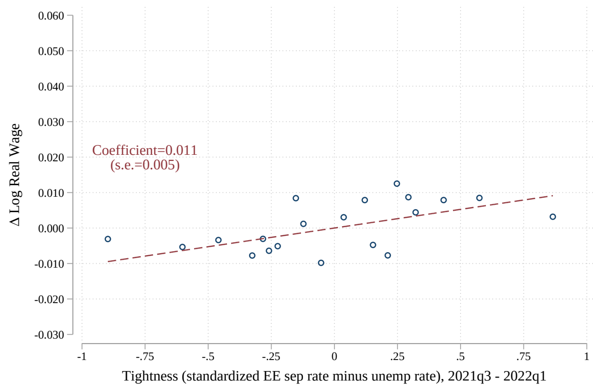
B. Components of tightness



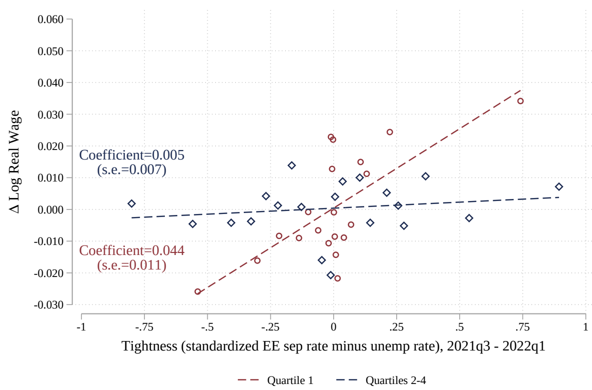
Note: Tightness measure and its components between 2015–2022. Estimates are reported at the midpoint of each six-month interval. EE separations and unemployment are standardized relative to their respective mean and standard deviation during July 2021–March 2022 period. EE separation rates obtained from CPS monthly data. Seasonally-adjusted state unemployment rates obtained from BLS LAUS.

Figure 23: Wage-Phillips Curves, Using Cross-State Variation in Tightness (Standardized EE Separation Rate and Negative Unemployment Rates)

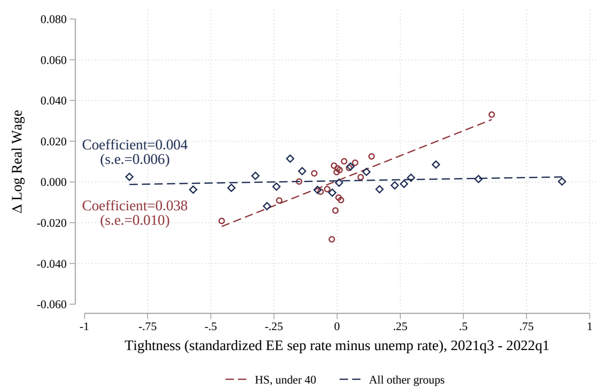
A. Overall



B. 1st quartile vs. all others

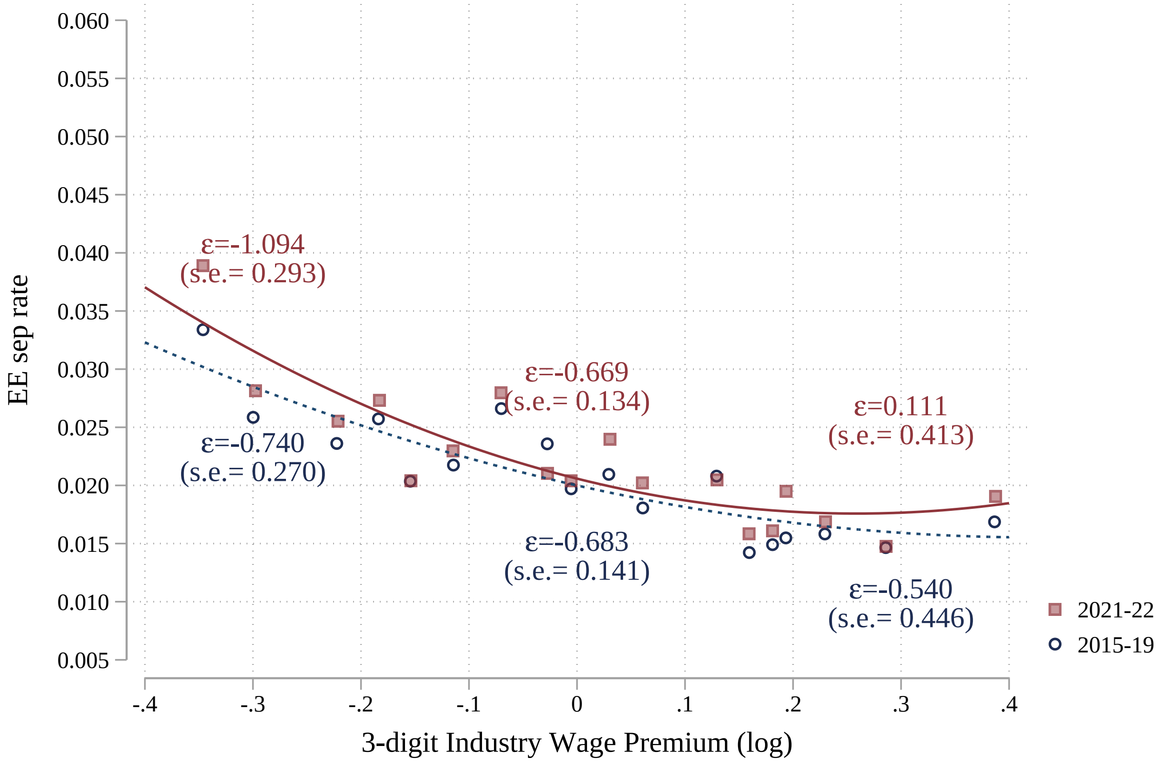


C. High School, under 40 vs. all others



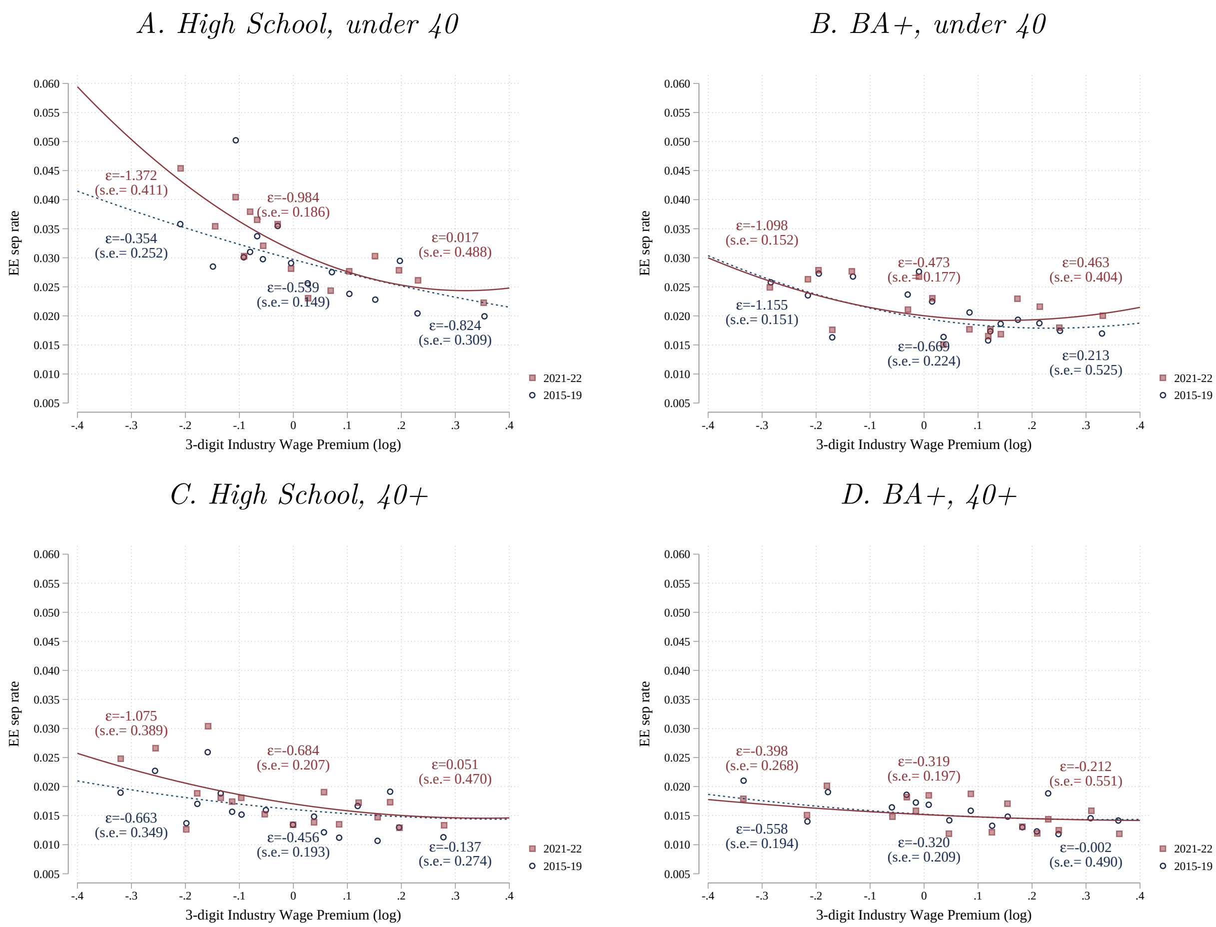
Note: Binscatters show the relationship between tightness and log real wage change. Panel A shows the overall wage change associated with tightness. Panel B contrasts wage gains for workers in the bottom wage quartile versus all other quartiles. Panel C contrasts wage gains for high-school workers under age 40, versus all other workers. The x-axis is tightness from 2021_{q3} to 2022_{q1}. Tightness is an average of the standardized EE separation rate and the negative, standardized unemployment rate, measured at the state level. EE separation rates obtained from CPS monthly data. Seasonally-adjusted state unemployment rates obtained from BLS LAUS. The y-axis is log real wage change between 2021_{q1q2} and 2022_{q2q3}. Real wages obtained from CPS monthly data. Wage quartiles estimated by state and time period. Binscatters include state and period fixed effects. Standard errors clustered at the state level. Specification corresponds to Appendix Table A1.

Figure 24: Job-to-Job Separation by Residual Log Industry Wage



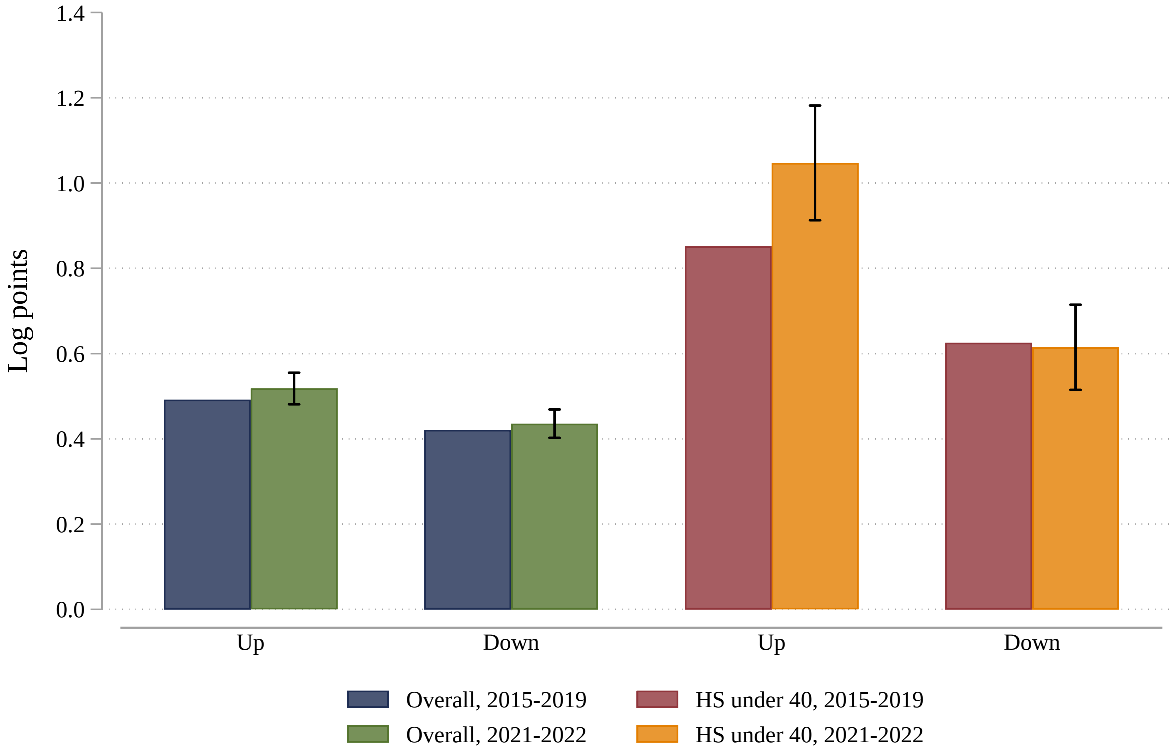
Note: Binscatter shows the quadratic relationship between industry wage premia (IWP) and EE separations, before and after the pandemic. Elasticities at $x = \{-0.3, 0, 0.3\}$ are calculated in two steps: first, we regress an indicator for EE separation at time t on IWP at time $t-1$, its square, demographic controls, and state fixed effects. Second, we evaluate the derivative of EE separation with respect to IWP at x and divide by the conditional mean of EE separation at x to get the elasticity at x. Demographic controls from the regression in step one include dummy variables for sex, race, ethnicity, age group, education, citizenship, and metro area status. The IWP are calculated from a regression of log real wage on demographic controls and 3-digit industry fixed effects for the pre-pandemic period, 2015–2019. The dependent variable, EE separation rate, is obtained from CPS monthly data. Standard errors are clustered at the industry level. Coefficients from the regression in step one are reported in Table A6 and the elasticities in this figure, as well as their difference between the 2015–2019 and 2021–2022 periods, are reported in Table 3.

Figure 25: Job-to-Job Separation by Residual Log Industry Wage: by Age and Education



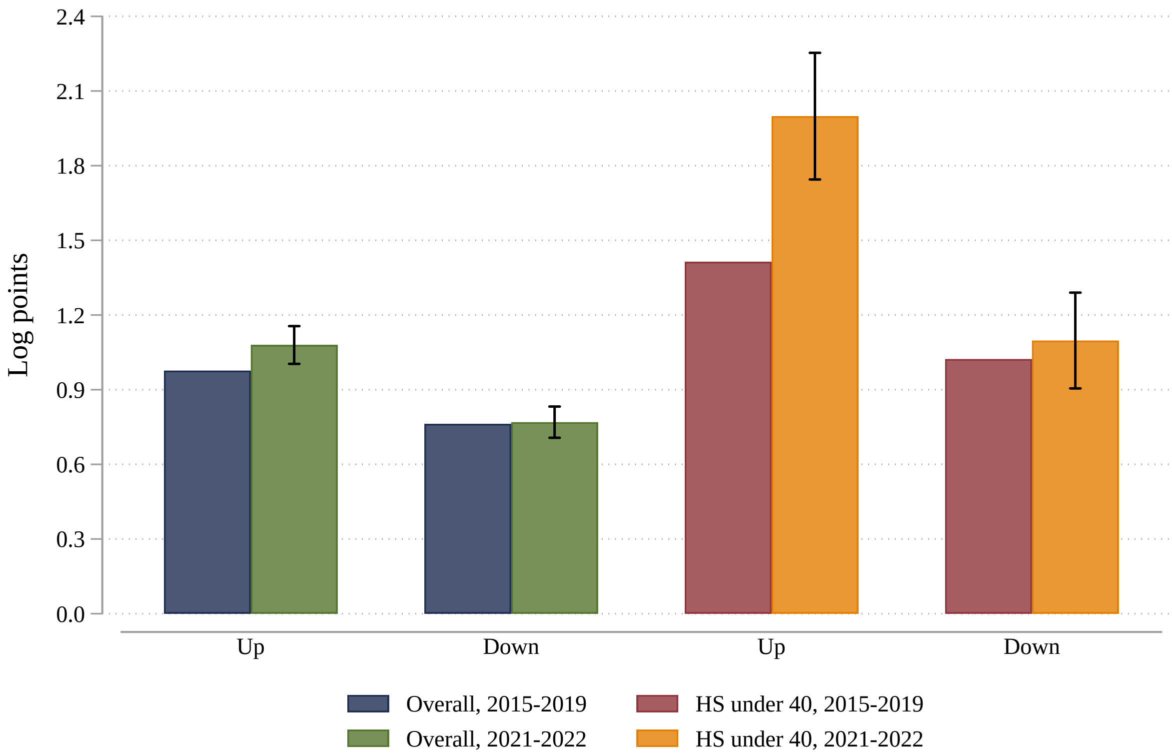
Note: Binscatters show the quadratic relationship between industry wage premia (IWP) and EE separations, before and after the pandemic, by age and education group. Elasticities at $x = \{-0.3, 0, 0.3\}$ are calculated in two steps: first, we regress an indicator for EE separation at time t on 3-digit IWP at time $t-1$, its square, demographic controls, and state fixed effects. Second, we evaluate the derivative of EE separation with respect to IWP at x and divide by the conditional mean of EE separation at x to get the elasticity at x. Demographic controls from the regression in step one include dummy variables for sex, race, ethnicity, age group, education, citizenship, and metro area status. The IWP are calculated separately for each subgroup from a regression of log real wage on demographic controls and 3-digit industry fixed effects for the pre-pandemic period, 2015–2019. The dependent variable, EE separation rate, is obtained from CPS monthly data. Standard errors are clustered at the industry level. Coefficients from the regression in step one are reported in Appendix Table A6 for panel A and in Appendix Table A7 for panels B through D. The elasticities in this figure, as well as their difference between the 2015–2019 and 2021–2022 periods for each subgroup, are reported in Table 3.

Figure 26: Movement Between Top Half and Bottom Half of the 3-Digit Industry Wage Premium Distribution



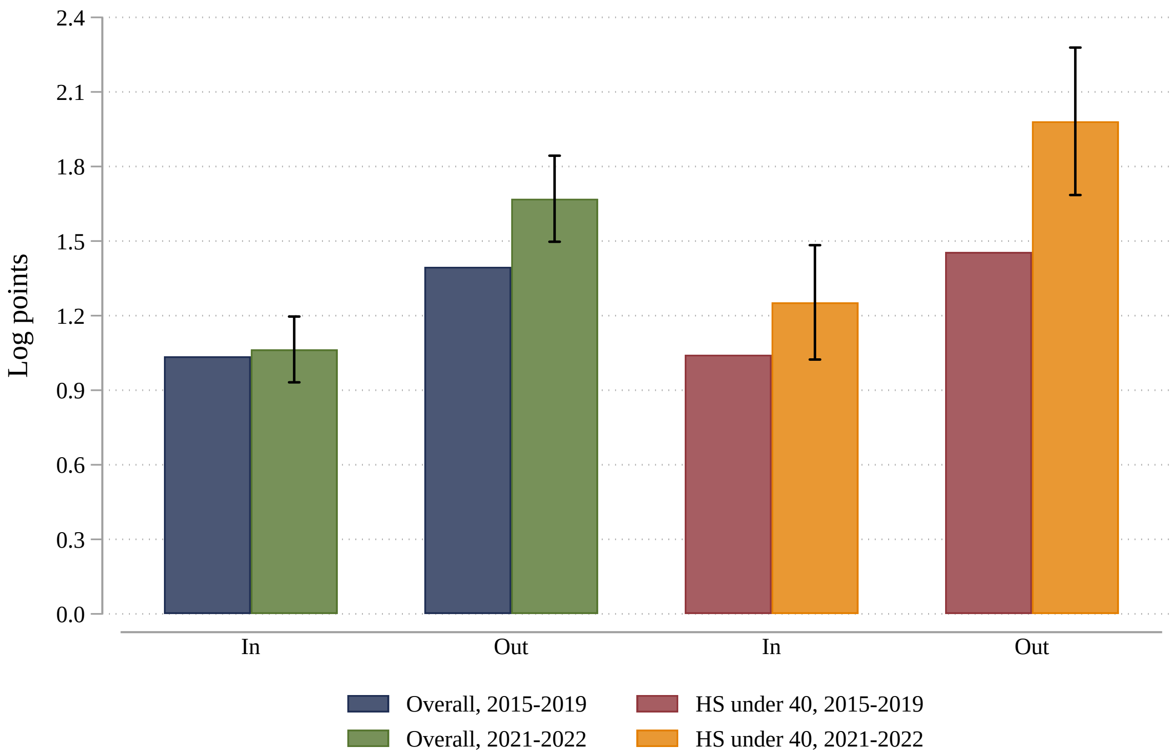
Note: Movement between the top and bottom halves of the 3-digit industry wage premium (IWP) distribution, by period, for all workers and for HS under 40 workers. The sample is limited to workers employed in both the current and the previous month. *Up* represents the likelihood of switching from the bottom half of the 3-digit IWP distribution to the top half. *Down* represents the likelihood of switching from the top half of the 3-digit IWP distribution to the bottom half. Movements in 2015–2019 correspond to column 1 of Table 4 and movements in 2021–2022 correspond to column 2 of Table 4. The error bars represent the 95% confidence intervals for the difference in movement between periods, corresponding to column 3 of Table 4.

Figure 27: Movement Into and Out of Bottom Quartile of the 3-Digit Industry Wage Premium Distribution



Note: Movement into and out of the bottom quartile of the industry wage premium (IWP) distribution, by period, for all workers and HS under 40 workers. *Up* represents the likelihood of switching from the bottom quartile of the 3-digit IWP distribution to the top three quartiles. *Down* represents the likelihood of switching from the top three quartiles of the 3-digit IWP distribution to the bottom quartile. Movements in 2015–2019 correspond to column 1 of Table 5 and movements in 2021–2022 correspond to column 2 of Table 5. The error bars represent the 95% confidence interval for the difference in movement between periods, corresponding to column 3 of Table 5. To account for the size differentials in exit and entry rates, *Down* bars and confidence intervals are re-scaled by $(1-p)/p$ where p is the share of workers in the bottom quartile ($p = 0.25$).

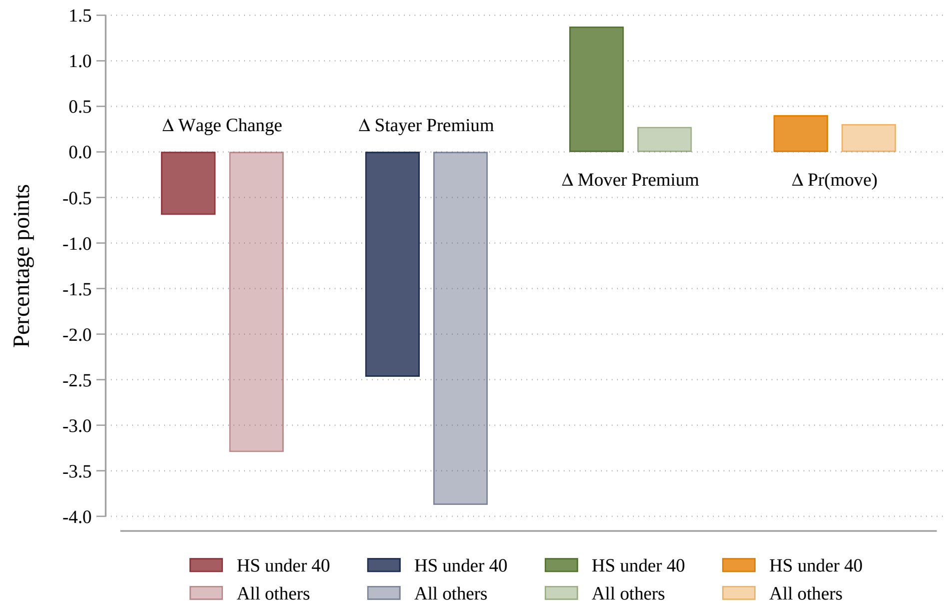
Figure 28: Movement Into and Out of the Hospitality Industry



Note: Movement into and out of the hospitality sector, by period, for all workers and HS under 40 workers. The sample is limited to workers employed in both the current and the previous month. An individual is considered to have moved into or out of the hospitality industry only if their industry changed from the previous month and if they indicated switching jobs since the previous month. *In* represents the likelihood of entering the hospitality sector. *Out* represents the likelihood of leaving the hospitality sector. Movements in 2015–2019 correspond to column 1 of Table 6 and movements in 2021–2022 correspond to column 2 of Table 6. The error bars represent the 95% confidence interval for the difference in movement between periods, corresponding to column 3 of Table 6. To account for the size differentials in exit and entry rates, *In* bars and confidence intervals are re-scaled by $(1-p)/p$ where p is the share of workers in hospitality in 2015–2019. For the overall sample, $p = 0.080$, and for HS under 40, $p = 0.187$.

Figure 29: Decomposition of the Change in Annual Wage Growth During 2021–2022 vs. 2015–2019

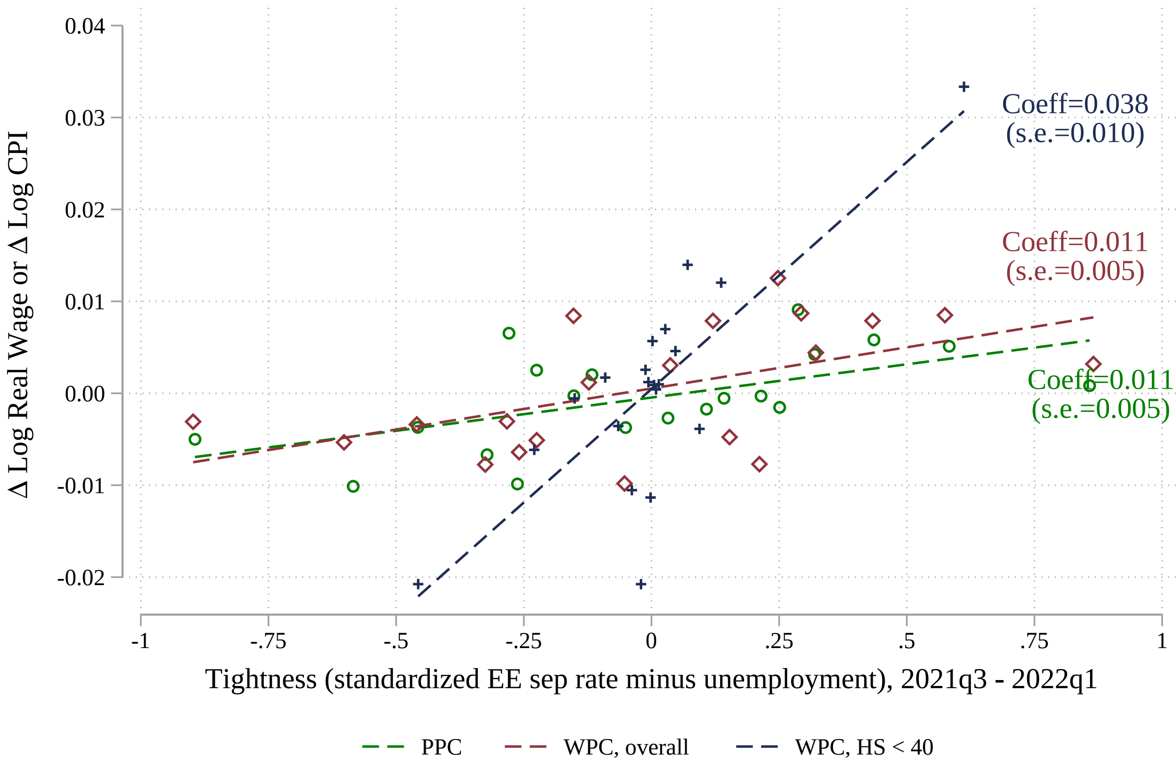
A. High-school, under-40 vs. all other workers



B. Difference: High-school, under-40 vs. all other workers

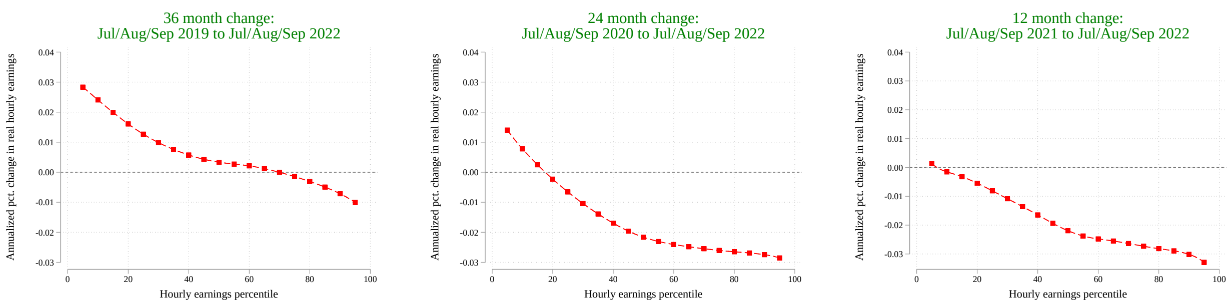
Note: Panel A decomposes the impact of industry-moving and industry-staying on overall wage changes for high-school workers under 40 (dark bars) and the complementary set of all other workers (light bars) using equation (7), with results reported in panel C of Table 7. ΔStayer Premium is the change in annual wage growth among industry-stayers between 2015–2019 and 2021–2022, scaled by the initial industry stay rate. ΔMover Premium is the change in annual wage growth among industry-movers between 2015–2019 and 2021-2022, scaled by the initial industry move rate. Δ Pr(Move) is the difference in annual wage gains among industry-movers and industry-stayer in 2021–2022, scaled by the change in the industry move rate between 2015–2019 and 2021–2022. Panel B highlights the difference in the components of wage change between high-school under-40 workers and all other workers.

Figure 30: Wage- and Price-Phillips Curves, Using Cross-State Variation in Tightness (Overall and High-School Under-40)



Note: Binscatter shows the relationship between tightness and log real wage change or price change. The x-axis is tightness from 2021_{q3} to 2022_{q1}. Tightness is an average of the standardized EE separation rate and the negative, standardized unemployment rate, measured at the state level. EE separation rates obtained from CPS monthly data. Seasonally-adjusted state unemployment rates obtained from BLS LAUS. For the wage-Phillips curve series, the y-axis is log real wage change between 2021_{q1q2} and 2022_{q2q3}. Real wages obtained from CPS monthly data. Wage quartiles estimated by state and time period. For the price-Phillips curve series, the y-axis is log CPI-U change between 2021_{q1q2} and 2022_{q2q3}. CPI-U-less-energy is obtained from BLS. We apply CBSA-level CPI-U deflators to main metro areas in each state, state average of CBSA-level CPI-U deflators in other metro areas within the state, and census division-level CPI-U deflators for the remaining non-metro areas. All models include state and period fixed effects. Coefficients correspond to column 5 of tables 1 and 8. Standard errors clustered at the state level.

Figure 31: Annualized Percent Change in Real Hourly Earnings by Earnings Percentile Over 36, 24, and 12 Months, Adjusted for Composition and Regional CPI



Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Wage percentiles smoothed with lowess. CPI-U is obtained from BLS. We apply CBSA-level CPI-U deflators to main metro areas in each state, state average of CBSA-level CPI-U deflators in other metro areas within the state, and census division-level CPI-U deflators for the remaining, non-metro areas.

Table 1: Coefficient on Tightness from Regressions of Wage Change on State Labor Market Tightness

	(1)	(2)	(3)	(4)	(5)
Overall	0.0207***	0.0189***	0.0126**	0.0109**	0.0105**
	(0.0055)	(0.0056)	(0.0049)	(0.0050)	(0.0054)
Within wage quartiles					
1st Quartile	0.0570***	0.0572***	0.0552***	0.0549***	0.0547***
	(0.0124)	(0.0123)	(0.0120)	(0.0120)	(0.0125)
2nd Quartile	0.0419***	0.0417***	0.0404***	0.0400***	0.0398***
	(0.0093)	(0.0094)	(0.0092)	(0.0092)	(0.0092)
3rd Quartile	−0.0077	−0.0075	−0.0067	−0.0073	−0.0075
	(0.0071)	(0.0072)	(0.0071)	(0.0072)	(0.0076)
4th Quartile	−0.0078	−0.0083	−0.0084	−0.0081	−0.0083
	(0.0121)	(0.0121)	(0.0115)	(0.0114)	(0.0115)
Within age and education groups					
High School, under 40	0.0401***	0.0438***	0.0436***	0.0384***	0.0381***
	(0.0083)	(0.0093)	(0.0095)	(0.0093)	(0.0100)
High School, 40+	0.0380**	0.0370**	0.0341**	0.0296**	0.0292**
	(0.0159)	(0.0152)	(0.0156)	(0.0140)	(0.0135)
Some College, under 40	0.0382***	0.0323***	0.0311***	0.0271***	0.0267***
	(0.0096)	(0.0095)	(0.0093)	(0.0090)	(0.0096)
Some College, 40+	0.0137	0.0124	0.0106	0.0124	0.0122
	(0.0103)	(0.0102)	(0.0105)	(0.0097)	(0.0100)
BA+, under 40	−0.0180	−0.0176	−0.0149	−0.0143	−0.0145
	(0.0119)	(0.0117)	(0.0119)	(0.0112)	(0.0110)
BA+, 40+	−0.0157	−0.0170	−0.0146	−0.0144	−0.0147
	(0.0122)	(0.0129)	(0.0127)	(0.0127)	(0.0132)
Controls:					
Age		X	X	X	X
Demographics			X	X	X
Sector and Union				X	X
Covid Death Rate					X

Note: N=75,199. Table reports estimates from OLS regressions of log real wage change on tightness. The independent variable is state-level tightness from 2021_{q3} to 2022_{q1}. Tightness is an average of the standardized EE separation rate and negative standardized unemployment rate, measured at the state level. EE separation rates obtained from CPS monthly data. Seasonally-adjusted state unemployment rates obtained from BLS LAUS. The dependent variable is log real wage change from 2021_{q1q2} and 2022_{q2q3}. Real wages obtained from CPS monthly data. All specifications include state and 6-month period fixed effects. Wage quartiles are estimated by state and period. Age controls include five age group dummies. Demographic controls include dummies for three education levels, 26 race categories, and sex. Sector controls include dummies for work in the manufacturing sector, professional services sector, finance sector, and business sector. Union controls are a dummy for union coverage. Covid death rate controls include a continuous variable for the state Covid-19 death rate per 100,000 people as of Sep 2022, and an interaction with an indicator for the 2nd and 3rd quarter of 2022. Within quartile estimates reported from a regression with quartile interactions. Within age and education estimates reported from a regression with age and education group interactions. Standard errors in parentheses, clustered by state. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table 2: Employment-to-Employment Separation Elasticity Estimates

	Individual-level Wage		Industry Wage Premia	
	(1)	(2)	(3)	(4)
Overall				
2015-2019	-0.2769^{***}	-0.1833^{***}	-0.9964^{***}	-0.6705^{***}
	(0.0443)	(0.0146)	(0.2039)	(0.1471)
2021-2022	-0.2160^{*}	-0.1559^{***}	-1.0067^{***}	-0.6148^{***}
	(0.1147)	(0.0386)	(0.2751)	(0.1677)
High School Educated, Under 40 Years Old				
2015-2019	-0.3332^{**}	-0.1754^{***}	-0.7995^{***}	-0.5627^{***}
	(0.1370)	(0.0438)	(0.1544)	(0.1465)
2021-2022	-0.6673^{**}	-0.2678^{***}	-1.1283^{***}	-0.7922^{***}
	(0.3260)	(0.1037)	(0.2190)	(0.1969)
High School Educated, 40 Years and Older				
2015-2019	-0.3981^{***}	-0.1446^{***}	-0.4704^{**}	-0.4163^{**}
	(0.1287)	(0.0405)	(0.2218)	(0.1727)
2021-2022	-0.2730	0.0339	-0.7324^{***}	-0.5961^{***}
	(0.4217)	(0.1087)	(0.2291)	(0.2131)
Bachelor's Degree or Higher, Under 40 Years Old				
2015-2019	-0.2485^{**}	-0.1976^{***}	-0.8713^{***}	-0.8061^{***}
	(0.1041)	(0.0336)	(0.2710)	(0.2437)
2021-2022	-0.1902	-0.2104^{**}	-0.6433^{**}	-0.5877^{***}
	(0.2283)	(0.0867)	(0.2656)	(0.2172)
Bachelor's Degree or Higher, 40 Years and Older				
2015-2019	-0.1797^{**}	-0.1339^{***}	-0.4205^{**}	-0.3926^{**}
	(0.0858)	(0.0310)	(0.1805)	(0.1714)
2021-2022	0.0256	-0.0774	-0.3335^{*}	-0.3448^{**}
	(0.2668)	(0.0828)	(0.1800)	(0.1634)
Aggregation Level	Individual	Individual	3-digit Ind.	3-digit Ind.
Time Interval	3-month	Annual (adjusted)	Monthly	Monthly
Controls	Y	Y	N	Y

Note: Separation elasticities are estimated in two steps: first by regressing an indicator for EE separation on logged real wage (columns 1 and 2) or industry wage premia (columns 3 and 4), and then by dividing the coefficient on wage from these regressions by the mean EE separation rate for the corresponding period and subgroup. Columns 1 and 2 are based on Equation 6 – the independent variable is log real wage and the dependent variable is a 3-month (col 1) or an annual (col 2) measure of EE separation. Estimates in column 2 are adjusted for measurement error as detailed in Appendix A1. Columns 3 and 4 are based on Equation 6 – the independent variable is the 3-digit industry wage premia (IWP), calculated from a regression of log real wage on demographic controls and industry fixed effects, and the dependent variable is a monthly measure of separation. Estimates are run without (col 3) and with controls (col 4). Columns 1, 2, and 4 include as controls indicators for state, age group, gender, race, ethnicity, education, citizenship, and metro area status. Standard errors in parentheses are clustered by industry in columns 3 and 4. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table 3: Employment-to-Employment Separation Elasticity Estimates at Different Values of Industry Wage Premia

	(1) IWP = -0.3	(2) IWP = 0	(3) IWP = 0.3
Overall			
2015-19	-0.7396^{***}	-0.6827^{***}	-0.5399
	(0.2702)	(0.1407)	(0.4455)
2021-22	-1.0942^{***}	-0.6693^{***}	0.1115
	(0.2932)	(0.1343)	(0.4135)
Diff	-0.3546	0.0134	0.6514
	(0.3982)	(0.1943)	(0.6071)
High School Educated, Under 40 Years Old			
2015-19	-0.3542	-0.5391^{***}	-0.8237^{***}
	(0.2515)	(0.1489)	(0.3091)
2021-22	-1.3719^{***}	-0.9840^{***}	0.0167
	(0.4112)	(0.1856)	(0.4884)
Diff	-1.0176^{**}	-0.4449^{*}	0.8404
	(0.4813)	(0.2376)	(0.5772)
High School Educated, 40 Years and Older			
2015-19	-0.6629^{*}	-0.4558^{**}	-0.1371
	(0.3493)	(0.1929)	(0.2743)
2021-22	-1.0753^{***}	-0.6835^{***}	0.0513
	(0.3888)	(0.2072)	(0.4701)
Diff	-0.4124	-0.2278	0.1885
	(0.5220)	(0.2827)	(0.5435)
Bachelor's Degree or Higher, Under 40 Years Old			
2015-19	-1.1549^{***}	-0.6694^{***}	0.2135
	(0.1511)	(0.2245)	(0.5247)
2021-22	-1.0977^{***}	-0.4726^{***}	0.4626
	(0.1516)	(0.1771)	(0.4042)
Diff	0.0573	0.1968	0.2492
	(0.2138)	(0.2856)	(0.6615)
Bachelor's Degree or Higher, 40 Years and Older			
2015-19	-0.5575^{***}	-0.3196	-0.0017
	(0.1942)	(0.2094)	(0.4903)
2021-22	-0.3976	-0.3192	-0.2116
	(0.2684)	(0.1968)	(0.5508)
Diff	0.1600	0.0005	-0.2099
	(0.3309)	(0.2870)	(0.7365)

Note: See Tables A6 and A7 and Figures 24 and 25 for notes. Standard errors in parentheses clustered by industry. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table 4: Mobility Rates from the Bottom Half of the 3-digit Industry Wage Premia Distribution, 2021–2022 v. 2015–2019

	(1) 2015-2019	(2) 2021-2022	(3) Difference
A. Exit rate from bottom half of IWP			
Overall	0.00492***	0.00518***	0.00026
N=1,046,399	(0.00009)	(0.00017)	(0.00019)
HS, under 40	0.00852***	0.01047***	0.00196***
N=156,181	(0.00031)	(0.00061)	(0.00069)
B. Exit rate from top half of IWP			
Overall	0.00421***	0.00436***	0.00014
N=1,070,446	(0.00008)	(0.00015)	(0.00017)
HS, under 40	0.00626***	0.00615***	−0.00011
N=160,992	(0.00026)	(0.00044)	(0.00051)
C. Net exit rate from bottom half of IWP			
Overall	0.00071***	0.00082***	0.00012
N=2,116,845	(0.00012)	(0.00022)	(0.00025)
HS, under 40	0.00226***	0.00432***	0.00206**
N=317,173	(0.00040)	(0.00075)	(0.00085)

Note: Table reports the likelihood of moving between the bottom and top half of the industry wage premium distribution. Industry wage premia (IWP) are calculated separately for subgroup (overall vs. HS under 40) in 2015-2019 by regressing log real wage on age, age^2, age^3, dummy variables for race, ethnicity, and industry. The sample is limited to those who were employed in the current and previous month. An individual is considered to have moved from the bottom to top (top to bottom) half of the IWP distribution if their industry at time t is in the top (bottom) half of the IWP and their industry in the previous month (time $t-1$) was in the bottom (top) half of the IWP distribution *and* they reported switching jobs since the previous month. Panel A reports the likelihood of moving from the bottom to top half of the IWP distribution, panel B reports the likelihood of moving from the top half to bottom half, and panel C represents the net movement between the two halves. Panel C is simply the difference between the first two panels. The first column presents these statistics for 2015-2019, the second for 2021-2022, and the third column is the difference between the first and second columns. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table 5: Mobility Rates from the Bottom Quartile of the 3-digit Industry Wage Premia Distribution, 2021–2022 v. 2015–2019

	(1) 2015-2019	(2) 2021-2022	(3) Difference
A. Exit rate from the bottom quartile of IWP			
Overall	0.00977***	0.01080***	0.00103***
N=524,945	(0.00018)	(0.00034)	(0.00039)
HS, under 40	0.01414***	0.01999***	0.00585***
N=80,557	(0.00055)	(0.00118)	(0.00130)
B. Exit rate from the top three quartiles of IWP			
Overall	0.00763***	0.00770***	0.00007
N=1,591,900	(0.00016)	(0.00028)	(0.00032)
HS, under 40	0.01023***	0.01098***	0.00074
N= 236,616	(0.00048)	(0.00086)	(0.00098)
C. Net exit rate from bottom quartile of IWP			
Overall	0.00214***	0.00310***	0.00096*
N=2,116,845	(0.00023)	(0.00043)	(0.00049)
HS, under 40	0.00391***	0.00901***	0.00511***
N=317,173	(0.00071)	(0.00145)	(0.00162)

Note: Table reports the likelihood of moving between the bottom quartile and top three quartiles of the industry wage premium (IWP) distribution. IWP are calculated for the period 2015-2019 separately for each subgroup (overall vs. HS under 40) by regressing log real wage on age, age^2, age^3, and indicators for race, ethnicity and industry. The sample is limited to those who were employed in the current and previous month. An individual is considered to have moved from the bottom quartile to the top three quartiles (top to bottom) of the IWP distribution if their current industry is in the top three (bottom) quartiles of the IWP and their industry in the previous month was in the bottom (top three) quartile of the IWP distribution *and* they reported switching jobs since the previous month. Panel A reports the likelihood of moving out of the bottom quartile of the IWP distribution, panel B reports the likelihood of moving into the bottom quartile and panel C represents the net movement out of the bottom quartile. Estimates in panel B are calculated by multiplying the mean exit rate from the top three quartiles by $(1-p)/p$ where p is the share of workers in the bottom quartile ($p = .25$). We do this to account for the size differentials between exit and entry rates into the bottom quartile. Panel C is then the difference between the first two panels. The first column presents these statistics for 2015-2019, the second for 2021-2022, and the third column is the difference between the first and second columns. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table 6: Mobility Rates from the Hospitality Sector, 2021–2022 v. 2015–2019

	(1) 2015-2019	(2) 2021-2022	(3) Difference
A. Exit rate from Hospitality sector			
Overall	0.01396***	0.01670***	0.00274***
N=151,640	(0.00039)	(0.00079)	(0.00088)
HS, under 40	0.01456***	0.01982***	0.00526***
N=58,770	(0.00064)	(0.00137)	(0.00151)
B. Exit rate from non-Hospitality sector			
Overall	0.01036***	0.01064***	0.00028
N=2,128,484	(0.00033)	(0.00059)	(0.00068)
HS, under 40	0.01043***	0.01253***	0.00211*
N=325,951	(0.00055)	(0.00104)	(0.00117)
C. Net exit rate from Hospitality sector			
Overall	0.00360***	0.00606***	0.00246**
N=2,128,484	(0.00050)	(0.00097)	(0.00110)
HS, under 40	0.00413***	0.00728***	0.00315*
N=325,951	(0.00083)	(0.00171)	(0.00190)

Note: Table shows the entrance and exit rates for the hospitality industry. The sample is limited to individuals working in the current and previous month. The hospitality sector is composed of all the industries within the Bureau of Labor Statistics' sector category "Accommodation and Food Service". An individual is considered a hospitality mover if their industry switched from a non-hospitality to a hospitality industry (or vice versa) from one month to the next, and they reported switching employers. Panel A reports the likelihood of exiting hospitality, panel B reports the likelihood of entering hospitality, and panel C represents the net exit rate from the hospitality sector. For panel B, the mean exit rate from non-hospitality industries is multiplied by $(1-p)/p$ to account for the size differentials between exit and entry rates, where p is the share of workers in hospitality in 2015-2019. For the overall sample, the hospitality share is $p = 0.080$, and for HS under 40, $p = 0.187$. The mean exit rate from non-hospitality overall in 2015-2019 is 0.0009, so the estimate presented in the table is $0.01036 \approx 0.0009 * 11.5$. Panel C is the difference between the first two panels. Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table 7: Decomposition of the Change in Annual Wage Growth During 2021—2022 vs. 2015—2019

	High-School under-40		All others	
	(1) 2015-2019	(2) 2021-2022	(3) 2015-2019	(4) 2021-2022
A. Job Change and Industry Change Rates (% points)				
1. Pr(Ind move = 1)	1.9	2.2	1.0	1.3
2. Pr(Ind move = 1 | Measured ind move = 1)	78.8	79.4	64.9	66.4
3. Pr(Ind move in past 11 mo)	19.1	22.0	10.8	12.9
4. Pr(Ind move in past year)	20.7	23.8	11.7	14.0
5. Pr(No ind move in past year)	79.3	76.2	88.3	86.0
B. Mean Real Wage Changes by Switcher Status (log points)				
1. E(Wage change)	4.6	3.9	3.4	0.1
2. E(Wage change | Industry move)	7.2	13.9	9.2	11.6
3. E(Wage change | No industry move)	3.9	0.8	2.7	-1.7
C. Decomposition of Wage Change: 2021-22 vs. 2015-19 (log points)				
1. Contribution of industry movers		1.37		0.27
2. Contribution of industry stayers		-2.47		-3.87
3. Contribution of move rate		0.40		0.31
4. Total		-0.69		-3.29

Note: Table presents components of the wage decomposition in equation (7). Panel A summarizes job change and industry change rates. In row A1, *True ind move* equals one for workers who experience both a job change and industry change between months in sample (MIS) 7 and 8. In row A2, *Measured ind move* equals one for workers who experience an industry move between MIS 7 and 8. The probability of an industry move in the past 11 months (row A3) is calculated using information from row A1. The probability of an industry move in the past year (row A4) and of no industry move in the past year (row A5) are calculated from rows A1 and A3. Panel B estimates average annual wage changes overall, and conditional on *true* industry move status. Wage changes for industry stayers (row B3) are calculated by solving for equation (9) using wage changes in rows B1 and B2, as well as probabilities from rows A4 and A5. Panel C uses inputs from panels A and B to estimate equation (7). See Section 4.4 for additional details.

Table 8: Price-Phillips Curve Estimates:
Regressions of Δ Log CPI on Various Measures of State Labor Market Tightness

	(1)	(2)	(3)	(4)	(5)
A. Independent var: Tightness					
Tightness	0.0094^{***}	0.0093^{***}	0.0091^{**}	0.0114^{**}	0.0113^{**}
	(0.0034)	(0.0034)	(0.0036)	(0.0045)	(0.0045)
B. Independent var: 1 - Unemployment					
1-Unemp	0.6801^{***}	0.6764^{***}	0.6726^{***}	0.8404^{***}	0.8306^{***}
	(0.1981)	(0.1976)	(0.2038)	(0.2345)	(0.2277)
C. Independent var: EE Separation Rate					
EE Sep	0.8329	0.8220	0.7687	0.9569	0.9897
	(0.8911)	(0.8869)	(0.9093)	(1.1674)	(1.1821)
Controls:					
Age		X	X	X	X
Demographics			X	X	X
Sector and Union				X	X
Covid Death Rate					X

Note: N=754,352 in columns 1-3, and N=121,768 in columns 4-5. Columns 4 and 5 have fewer observations because they control for union status, which is only available in the Outgoing Rotation Groups, MIS 4 and 8. All columns report estimates from OLS regressions of change in log CPI on different measures of tightness. In panel A, the independent variable is state-level tightness from 2021_{q3} to 2022_{q1}. In panel B, the independent variable is 1 - unemployment rate at the state-level from 2021_{q3} to 2022_{q1}. In panel C, the independent variable is the state-level EE separation rate from 2021_{q3} to 2022_{q1}. Tightness is an average of the standardized EE separation rate and negative standardized unemployment rate, measured at the state level. EE separation rates obtained from CPS monthly data. Seasonally-adjusted state unemployment rates obtained from BLS LAUS. The dependent variable is log change in CPI less energy from 2021_{q1q2} and 2022_{q2q3}. CPI less energy is obtained from BLS. We apply CBSA-level CPI to main metro areas in each state, state average of CBSA-level CPI in other metro areas within the state, and census division-level CPI for remaining non-metro areas. All specifications include state and 6-month period fixed effects. Age controls include five age group dummies. Demographic controls include dummies for three education levels, 26 race categories, and sex. Sector controls include dummies for work in the manufacturing sector, professional services sector, finance sector, and business sector. Union controls are a dummy for union coverage. Covid death rate controls include a continuous variable for the state Covid-19 death rate per 100,000 people as of Sep 2022, and an interaction with an indicator for the 2nd and 3rd quarter of 2022. Standard errors in parentheses, clustered by state. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table 9: Coefficient on Tightness from Regressions of Wage Change on State Labor Market Tightness - using constructed *state-level* CPI

	(1)	(2)	(3)	(4)	(5)
Overall	0.0132**	0.0115**	0.0052	0.0034	0.0032
	(0.0055)	(0.0049)	(0.0039)	(0.0040)	(0.0043)
Within wage quantiles					
1st Quartile	0.0497***	0.0499***	0.0478***	0.0476***	0.0475***
	(0.0142)	(0.0140)	(0.0137)	(0.0137)	(0.0138)
2nd Quartile	0.0347***	0.0344***	0.0331***	0.0328***	0.0327***
	(0.0083)	(0.0083)	(0.0080)	(0.0080)	(0.0080)
3rd Quartile	−0.0151**	−0.0149**	−0.0141**	−0.0147**	−0.0148**
	(0.0059)	(0.0059)	(0.0058)	(0.0058)	(0.0060)
4th Quartile	−0.0147	−0.0153	−0.0153	−0.0150	−0.0151
	(0.0112)	(0.0112)	(0.0106)	(0.0105)	(0.0105)
Within age and education groups					
High School, under 40	0.0329***	0.0366***	0.0364***	0.0313***	0.0310***
	(0.0089)	(0.0101)	(0.0102)	(0.0102)	(0.0106)
High School, 40+	0.0303*	0.0292*	0.0264	0.0219	0.0216
	(0.0170)	(0.0162)	(0.0164)	(0.0149)	(0.0145)
Some College, under 40	0.0310***	0.0251***	0.0239***	0.0199**	0.0197**
	(0.0098)	(0.0091)	(0.0084)	(0.0082)	(0.0087)
Some College, 40+	0.0067	0.0054	0.0036	0.0054	0.0052
	(0.0099)	(0.0096)	(0.0096)	(0.0088)	(0.0090)
BA+, under 40	−0.0258**	−0.0254**	−0.0227**	−0.0221**	−0.0223**
	(0.0112)	(0.0109)	(0.0112)	(0.0105)	(0.0102)
BA+, 40+	−0.0230**	−0.0243**	−0.0218**	−0.0217**	−0.0219**
	(0.0103)	(0.0110)	(0.0109)	(0.0108)	(0.0111)
Controls:					
Age		X	X	X	X
Demographics			X	X	X
Sector and Union				X	X
Covid Death Rate					X

Note: N=75,199. Table reports estimates from OLS regressions of log real wage change on tightness. The independent variable is state-level tightness from 2021_{q3} to 2022_{q1}. Tightness is an average of the standardized EE separation rate and negative standardized unemployment rate, measured at the state level. EE separation rates obtained from CPS monthly data. Seasonally-adjusted state unemployment rates obtained from BLS LAUS. The dependent variable is log real wage change from 2021_{q1q2} and 2022_{q2q3}. Real wages obtained from CPS monthly data and deflated using CPI at the most regional level available. We deflate wages with CBSA-specific CPI-U for available main metro areas, state average of CBSA-level CPI-U for other metro areas, and census-division level CPI-U for non-metro areas. All specifications include state and 6-month period fixed effects. Wage quartiles are estimated by state and period. Age controls include five age group dummies. Demographic controls include dummies for three education levels, 26 race categories, and sex. Sector controls include dummies for work in the manufacturing sector, professional services sector, finance sector, and business sector. Union controls are a dummy for union coverage. Covid death rate controls include a continuous variable for the state Covid-19 death rate per 100,000 people as of Sep 2022, and an interaction with an indicator for the 2nd and 3rd quarter of 2022. Within quartile estimates reported from a regression with quartile interactions. Within age and education estimates reported from a regression with age and education group interactions. Standard errors in parentheses, clustered by state. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Appendix for The Unexpected Compression: Competition at Work in the Low Wage Labor Market

By: David Autor, Arindrajit Dube, and Annie McGrew

A1 Measurement Error Correction for Separation Elasticities Based on Individual Wage

In order to correct for measurement error in the annual measure of EE separation, we estimate how switching both industry and occupation relates to switching employers in months where we can observe true EE separations (MIS 6-8). As discussed in Section 1, we consider the IPUMS variable *EMPSAME* to provide a "ground truth" measure of monthly EE separations, which we can check against monthly changes in industry and occupation. True annual EE separations (ΔJ^{12}) are then related to proxied annual separations ($\tilde{\Delta} J^{12}$) as follows:

$$
\begin{aligned}
E\left(\tilde{\Delta} J^{12}\right) &= P(\tilde{\Delta} J^{12}=1|\Delta J^{12}=1)E(\Delta J^{12}) + P(\tilde{\Delta} J^{12}=1|\Delta J^{12}=0)(1-E(\Delta J^{12}))\\
E\left(\tilde{\Delta} J^{12}\right) &= \theta_{11}E(\Delta J^{12}) + \theta_{10}(1-E(\Delta J^{12}))\\
E\left(\tilde{\Delta} J^{12}\right) &= (\theta_{11}-\theta_{10})\,E(\Delta J^{12}) + \theta_{10}
\end{aligned}
$$

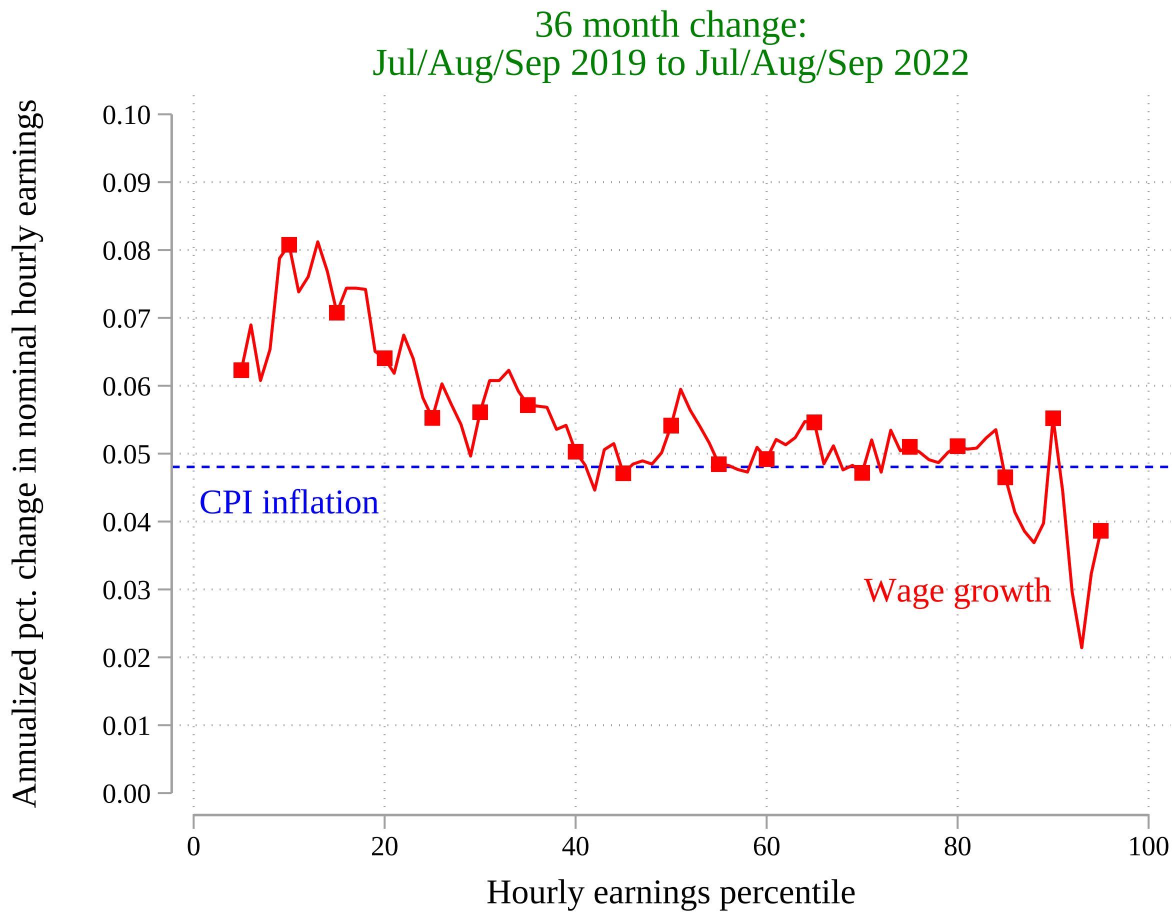
We can then solve for true annual separations as $E\left(\Delta J^{12}\right) = \frac{E(\tilde{\Delta} J^{12})-\theta_{10}}{(\theta_{11}-\theta_{10})}$. However, since we don't have a ground truth estimate of ΔJ^{12}, we also lack an estimate of $\theta_{11} = P(\tilde{\Delta} J^{12} = 1|\Delta J^{12} = 1)$ or $\theta_{10} = P(\tilde{\Delta} J^{12} = 1|\Delta J^{12} = 0)$. We can estimate proxies for θ_{11} and θ_{10} using monthly data. Define p_{11} as the share of annual separators who leave between MIS 4 and 5, so that $p_{11} = P(\Delta J^{MIS4,5} = 1|\Delta J^{12} = 1)$. If we define ΔJ^1 as monthly separations, $1-\Delta J^1$ is the probability of not switching jobs in 1 month and $1-(1-\Delta J^1)^m$ is the probability of switching jobs at least once in the span of m months. Since we observe 'true' job switching in 3 months out of the year, we need to know the proportion of true switchers that switch jobs in the 9 months between MIS 4 and 5. Thus, $p_{11} = \frac{1-(1-\Delta J^1)^9}{1-(1-\Delta J^1)^{12}}$. We can then define $\theta_{11} = p_{11}(\theta^m_{11}) + (1-p_{11})$ where $\theta^m_{11} = P(\tilde{\Delta} J^1 = 1|\Delta J^1 = 1)$, $\tilde{\Delta} J^1$ is the observed EE separation (measured as the joint occurrence of an industry and occupation switch between month $t-1$ and t), and ΔJ^1 is the monthly ground truth EE separation measured by the IPUMS variable *EMPSAME*. If $\theta^m_{10} = P(\tilde{\Delta} J^1 = 1|\Delta J^1 = 0)$, then $\theta_{10} = 1-(1-\theta^m_{10})^9$. The observed parameters used in calculating θ_{11} and θ_{10} are calculated separately for subgroup and period.

True monthly separations are a function of residual wage w as follows: $E(\Delta J^{12}) = \alpha +$

$\beta \ln(w)$, whereas the conditional expectation function for observed separations is $E(\tilde{\Delta} J^{12}) = [\alpha + \theta_{10}] + [\beta\,(\theta_{11} - \theta_{10})] \ln(w)$. (For notational simplicity, we have dropped the monthly subscript to the regression coefficient, simplifying α^{12} and β^{12} as α and β, respectively.)

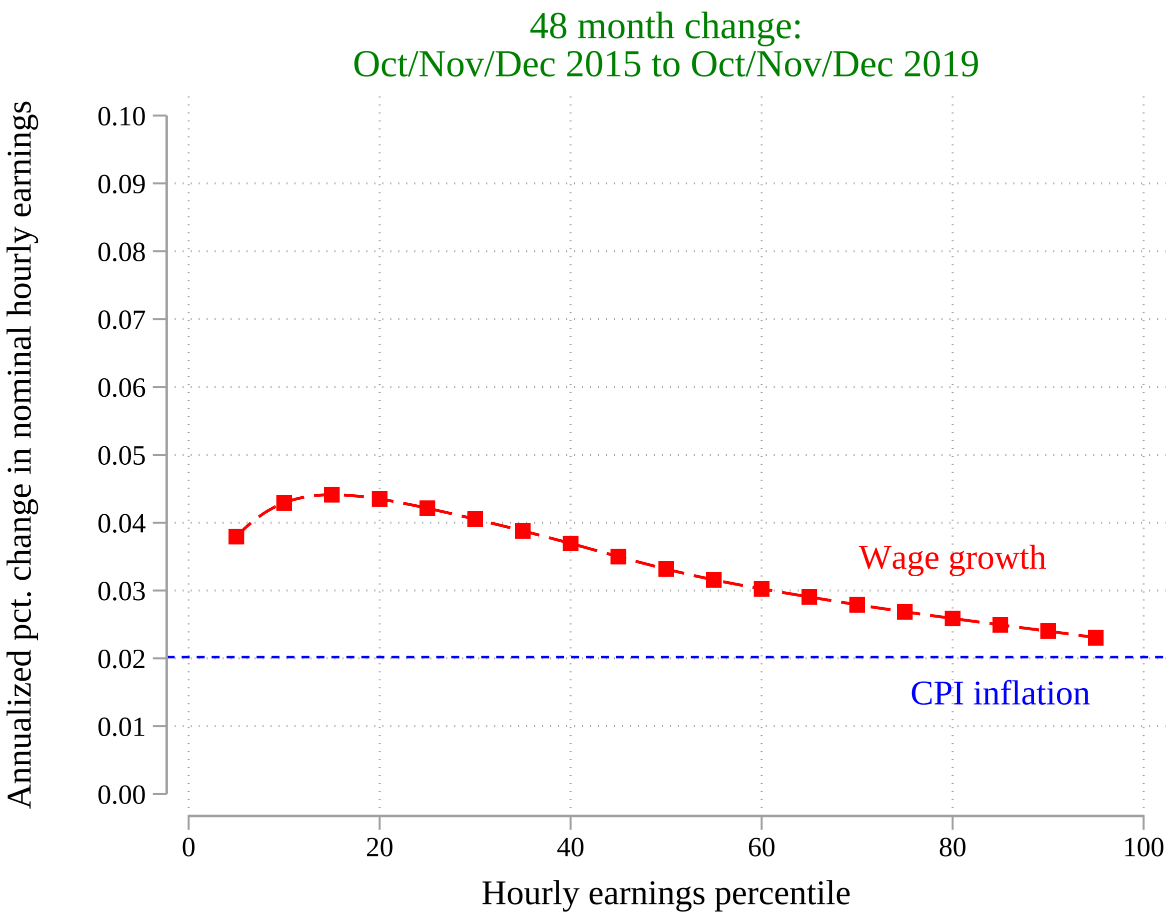
The semi-elasticities can thus be obtained by dividing $\tilde{\beta}$ by $(\theta_{11} - \theta_{10})$. Empirically, $\theta_{11} - \theta_{10}$ for the overall sample in period $T(t) = 2$ is approximately 0.57, meaning that the (semi-elasticity) coefficient adjustment factor is around 1.75. To obtain the elasticities corresponding to these estimates, we then divide the $\beta_{T(t)}$ by the true mean separation rate $E(\Delta J^{12}) = \frac{E(\tilde{\Delta} J^{12}) - \theta_{10}}{\theta_{11} - \theta_{10}}$ in period $T(t)$ by subgroup. The corrected 12-month EE separation elasticities using this method are presented in Table 2 column 2. The correction itself makes a modest difference: the elasticities with measurement error correction for the high-school under-40 group is -0.175 (se= 0.044) for period 1 and 0.268 (se = 0.104) for period 2, without the correction, these elasticities are 0.159 (se = 0.040) and 0.252 (se = .098), respectively.

Figure A1: Changes in Nominal Wage and Inflation Rate Along the (Unsmoothed) Distribution



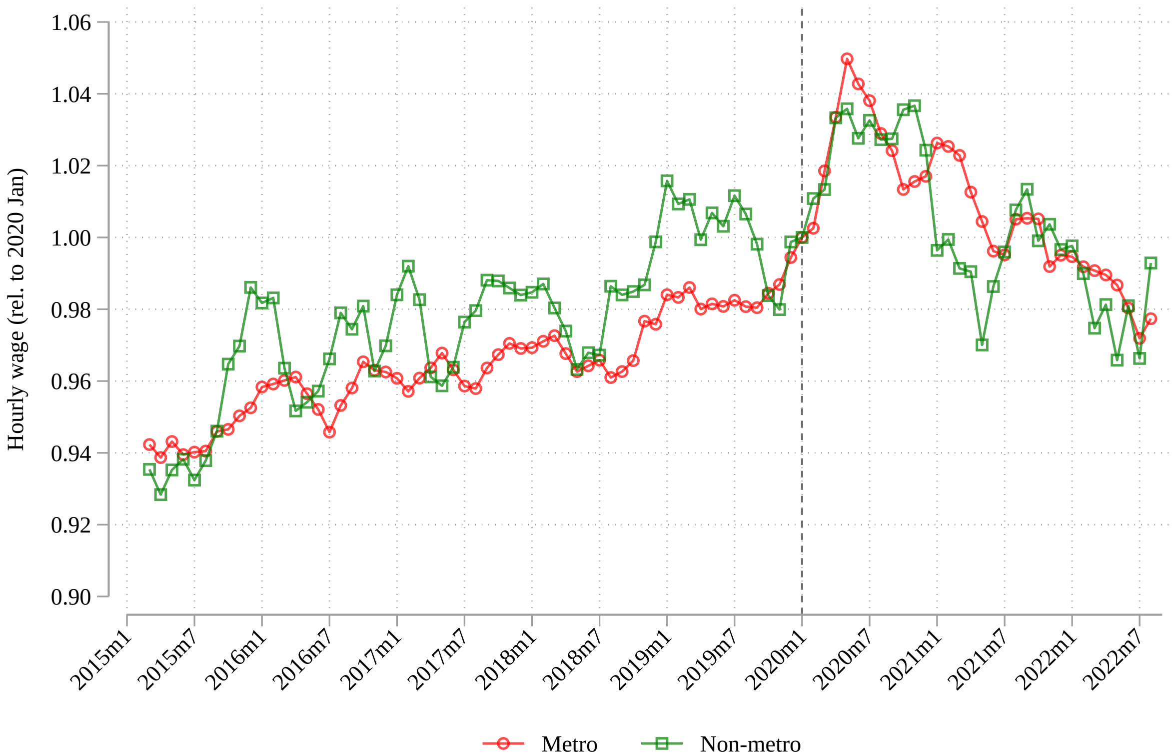
Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Use CPI-U annualized, not-seasonally adjusted, all workers.

Figure A2: 48 Month Changes in Nominal Wage and Inflation Rate Along the Distribution



Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Wage percentiles smoothed with lowess. Use CPI-U annualized, not-seasonally adjusted, all workers.

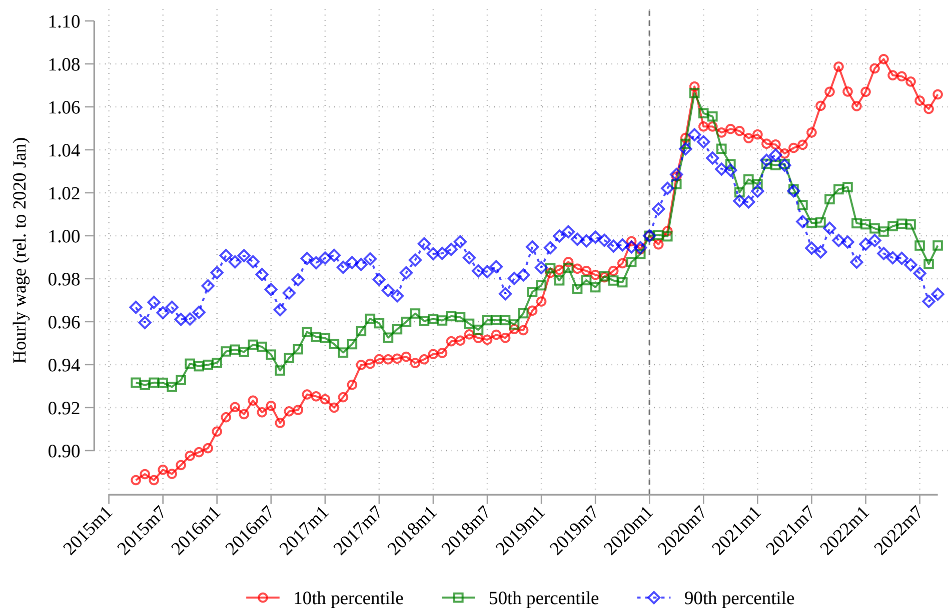
Figure A3: Real Hourly Wages by Metro Area Status, Relative to January 2020



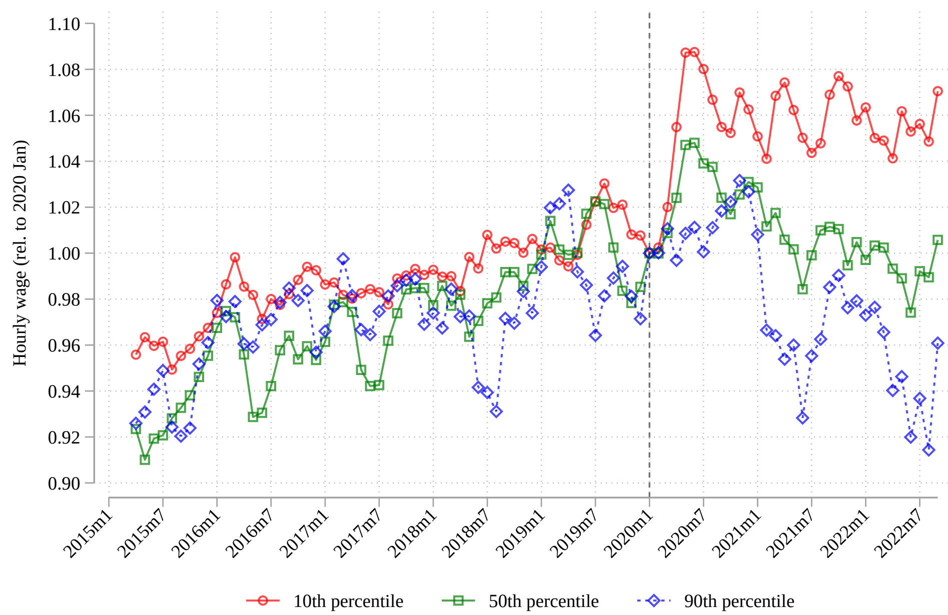
Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Wages are real (2022_{q3} USD) and smoothed with a 3-month moving average.

Figure A4: Real Hourly Wages by Quantile and Metro Area Status, Relative to January 2020

A. Metro area



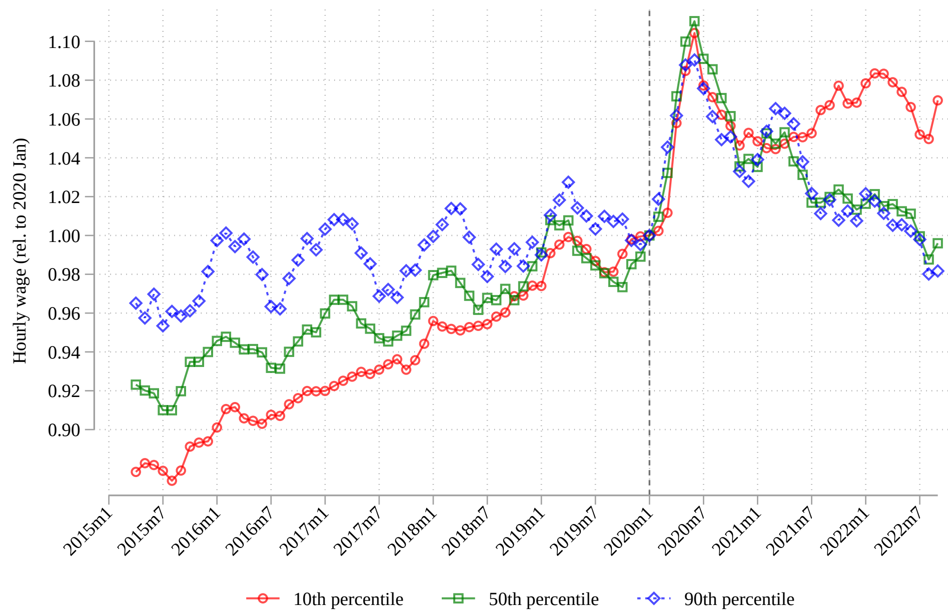
B. Non-metro area



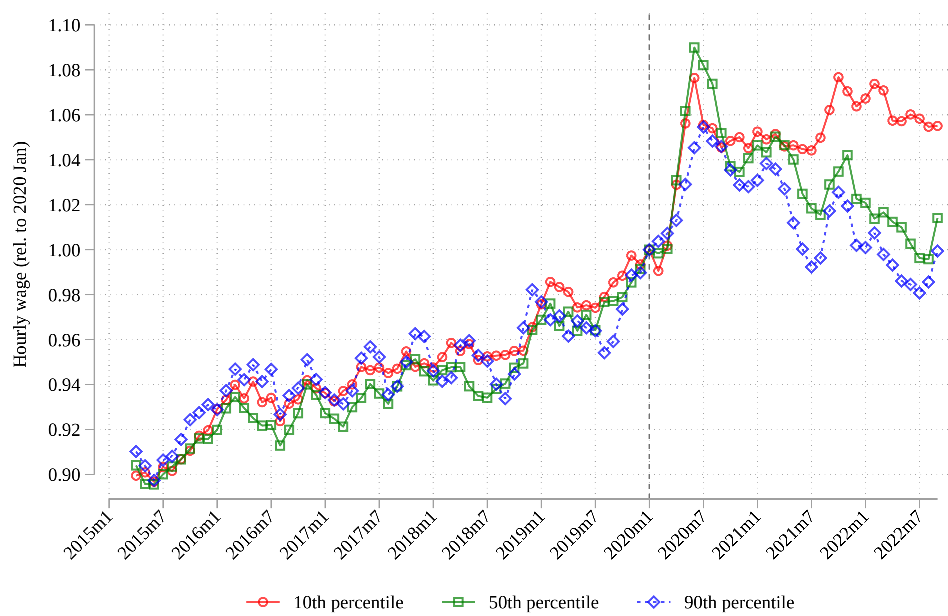
Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Wages are real (2022_{q3} USD). Wage percentiles smoothed with lowess and 3-month moving average.

Figure A5: Real Hourly Wages by Quantile and Sex, Relative to January 2020

A. Male workers

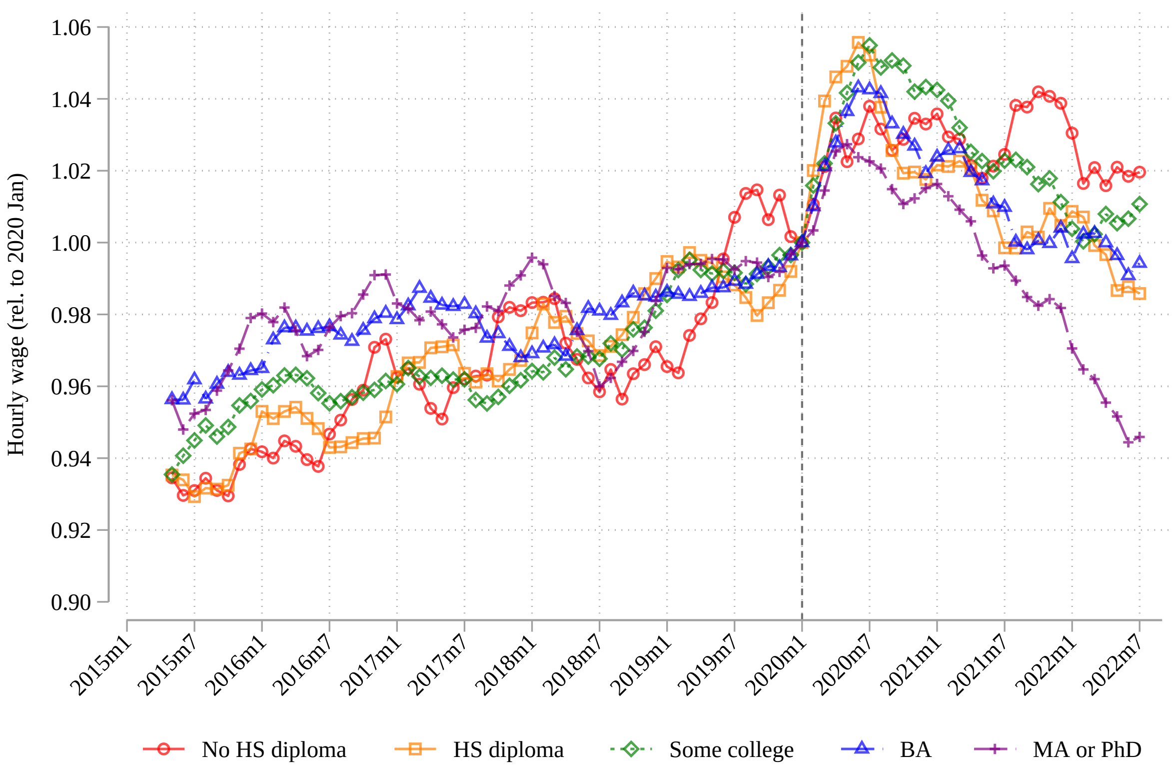


B. Female workers



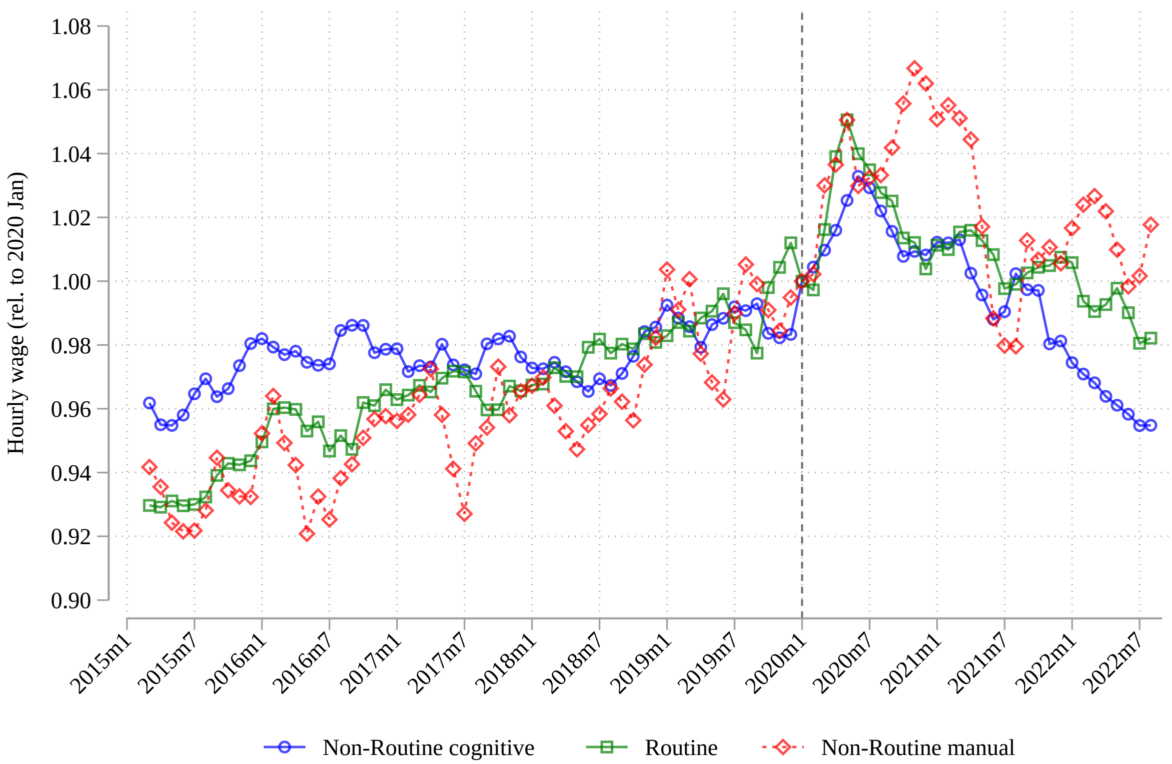
Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Wages are real (2022_{q3} USD). Wage percentiles are smoothed with lowess and a 3-month moving average.

Figure A6: Real Hourly Wages by Education Levels (5 Education Categories), Relative to January 2020



Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Wages are real (2022_{q3} USD) and smoothed with a 6-month moving average.

Figure A7: Real Hourly Wages by Types of Occupation Using Task Measures, Relative to January 2020



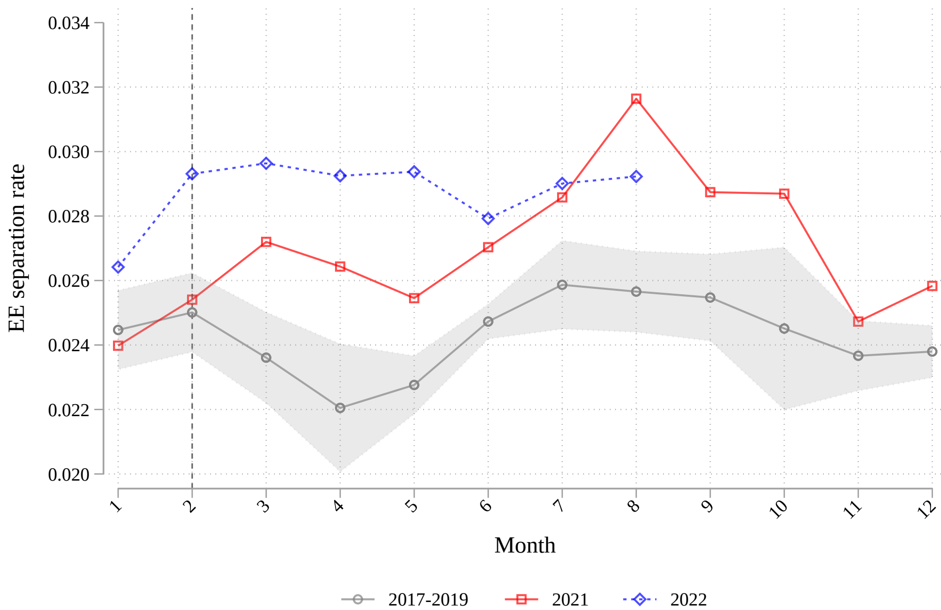
Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Wages are real (2022_{q3} USD) and smoothed with a 3-month moving average. Occupation task types were identified following Jaimovich and Siu (2020).

Figure A8: Real Hourly Wages by Education Levels (Non-BA vs. BA+), Relative to January 2020

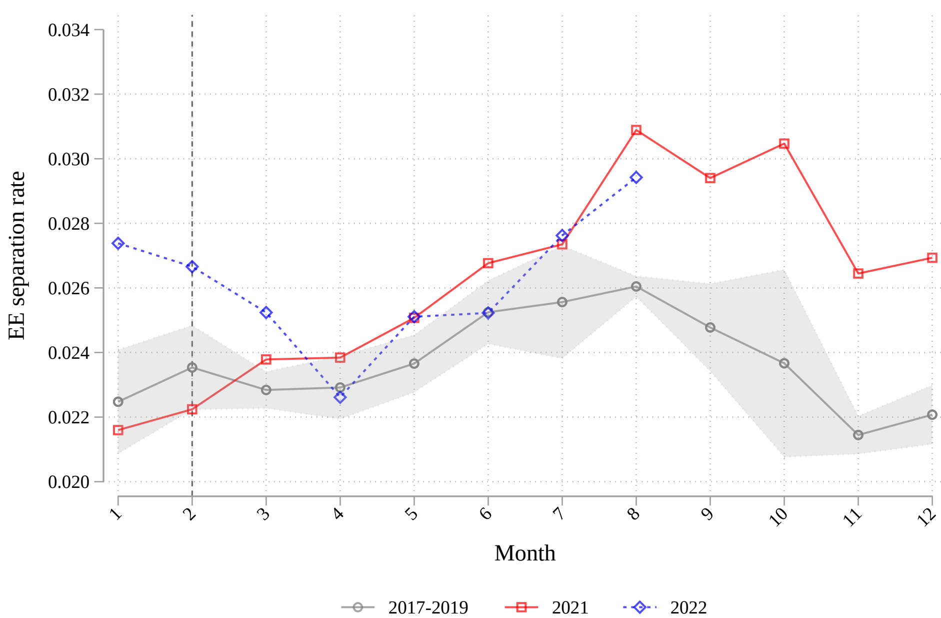
Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, citizenship, country of birth, and region. Wages are real (2022_{q3} USD) and smoothed with a 3-month moving average.

Figure A9: EE Separation Rates by Month and Year: Non-BA Workers

A. High School education



B. Some college education



Note: CPS monthly data. Employment-to-employment (EE) separation rate is smoothed with a 3-month moving average. Shaded area represents the 95% confidence interval for the monthly EE separation rate during the 2017–2019 period.

Table A1: Coefficient on Tightness from Regressions of Wage Change on State Labor Market Tightness

	(1)
Overall	0.0105**
	(0.0054)
Within wage quartiles	
1st Quartile	0.0437***
	(0.0105)
2nd-4th Quartiles	0.0052
	(0.0073)
Within age and education groups	
HS under 40	0.0384***
	(0.0101)
All other groups	0.0038
	(0.0058)
Controls:	
Age	X
Demographics	X
Sector and Union	X
Covid Death Rate	X

Note: N=75,199. Table reports estimates from OLS regressions of log real wage change on tightness corresponding to Figure 23. The independent variable is state-level tightness from 2021_{q3} to 2022_{q1}. Tightness is an average of the standardized EE separation rate and negative standardized unemployment rate, measured at the state level. EE separation rates obtained from CPS monthly data. Seasonally-adjusted state unemployment rates obtained from BLS LAUS. The dependent variable is log real wage change from 2021_{q1q2} and 2022_{q2q3}. Real wages obtained from CPS monthly data. Specifications includes state and 6-month period fixed effects. Wage quartiles are estimated by state and period. Age controls include five age group dummies. Demographic controls include dummies for three education levels, 26 race categories, and sex. Sector controls include dummies for work in the manufacturing sector, professional services sector, finance sector, and business sector. Union controls are a dummy for union coverage. Covid death rate controls include a continuous variable for the state Covid-19 death rate per 100,000 people as of Sep 2022, and an interaction with an indicator for the 2nd and 3rd quarter of 2022. Within quartile estimates reported from a regression including interactions with the bottom quartile, and all others. Within age and education estimates reported from a regression including high schools workers under 40, and all others. Standard errors in parentheses, clustered by state. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table A2: Coefficient on Unemployment from Regressions of Wage Change on State Standardized Negative Unemployment Rate

	(1)	(2)	(3)	(4)	(5)
Overall	0.0125**	0.0132**	0.0080**	0.0070*	0.0065*
	(0.0055)	(0.0054)	(0.0040)	(0.0039)	(0.0037)
Within wage quartiles					
1st Quartile	0.0310**	0.0311**	0.0296**	0.0294**	0.0292**
	(0.0133)	(0.0132)	(0.0128)	(0.0128)	(0.0133)
2nd Quartile	0.0324***	0.0325***	0.0313***	0.0311***	0.0308***
	(0.0068)	(0.0068)	(0.0066)	(0.0066)	(0.0066)
3rd Quartile	−0.0039	−0.0035	−0.0031	−0.0035	−0.0037
	(0.0043)	(0.0043)	(0.0043)	(0.0043)	(0.0044)
4th Quartile	−0.0094	−0.0097	−0.0092	−0.0088	−0.0091
	(0.0086)	(0.0087)	(0.0083)	(0.0083)	(0.0083)
Within age and education groups					
High School, under 40	0.0255***	0.0287***	0.0289***	0.0253***	0.0248***
	(0.0063)	(0.0061)	(0.0059)	(0.0058)	(0.0063)
High School, 40+	0.0310***	0.0299***	0.0293**	0.0259**	0.0253***
	(0.0119)	(0.0115)	(0.0115)	(0.0103)	(0.0095)
Some College, under 40	0.0338***	0.0317***	0.0299***	0.0270***	0.0265***
	(0.0057)	(0.0057)	(0.0054)	(0.0054)	(0.0059)
Some College, 40+	0.0082	0.0069	0.0065	0.0065	0.0060
	(0.0077)	(0.0076)	(0.0078)	(0.0078)	(0.0074)
BA+, under 40	−0.0210***	−0.0179**	−0.0162**	−0.0156**	−0.0161**
	(0.0079)	(0.0073)	(0.0075)	(0.0073)	(0.0069)
BA+, 40+	−0.0153**	−0.0167**	−0.0151**	−0.0131*	−0.0136*
	(0.0071)	(0.0072)	(0.0071)	(0.0071)	(0.0073)
Controls:					
Age		X	X	X	X
Demographics			X	X	X
Sector and Union				X	X
Covid Death Rate					X

Note: N=75,199. Table reports estimates from OLS regressions of log real wage change on the negative standardized unemployment rate. The independent variable is the negative of standardized unemployment rate at the state-level from 2021_{q3} to 2022_{q1}. Seasonally-adjusted state unemployment rates obtained from BLS LAUS. The dependent variable is log real wage change from 2021_{q1q2} and 2022_{q2q3}. Real wages obtained from CPS monthly data. All specifications include state and 6-month period fixed effects. Wage quartiles are estimated by state and period. Age controls include five age group dummies. Demographic controls include dummies for three education levels, 26 race categories, and sex. Sector controls include dummies for work in the manufacturing sector, professional services sector, finance sector, and business sector. Union controls are a dummy for union coverage. Covid death rate controls include a continuous variable for the state Covid-19 death rate per 100,000 people as of Sep 2022, and an interaction with an indicator for the 2nd and 3rd quarter of 2022. Within quartile estimates reported from a regression with quartile interactions. Within age and education estimates reported from a regression with age and education group interactions. Standard errors in parentheses, clustered by state. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table A3: Coefficient on EE Separation Rate from Regressions of Wage Change on State Standardized EE Separation Rate

	(1)	(2)	(3)	(4)	(5)
Overall	0.0086*	0.0060	0.0049	0.0041	0.0043
	(0.0049)	(0.0053)	(0.0040)	(0.0039)	(0.0037)
Within wage quartiles					
1st Quartile	0.0272***	0.0273***	0.0267***	0.0266***	0.0268***
	(0.0092)	(0.0092)	(0.0089)	(0.0088)	(0.0085)
2nd Quartile	0.0100	0.0097	0.0095	0.0094	0.0096
	(0.0070)	(0.0071)	(0.0068)	(0.0069)	(0.0067)
3rd Quartile	−0.0038	−0.0041	−0.0036	−0.0039	−0.0037
	(0.0048)	(0.0048)	(0.0047)	(0.0047)	(0.0048)
4th Quartile	0.0019	0.0016	0.0010	0.0009	0.0011
	(0.0070)	(0.0070)	(0.0067)	(0.0067)	(0.0066)
Within age and education groups					
High School, under 40	0.0154*	0.0160**	0.0155*	0.0139*	0.0142*
	(0.0080)	(0.0080)	(0.0081)	(0.0078)	(0.0076)
High School, 40+	0.0069	0.0068	0.0046	0.0035	0.0038
	(0.0114)	(0.0116)	(0.0110)	(0.0108)	(0.0103)
Some College, under 40	0.0031	−0.0006	0.0000	−0.0010	−0.0007
	(0.0073)	(0.0073)	(0.0070)	(0.0069)	(0.0068)
Some College, 40+	0.0058	0.0058	0.0044	0.0061	0.0065
	(0.0065)	(0.0066)	(0.0070)	(0.0066)	(0.0067)
BA+, under 40	0.0027	−0.0000	0.0010	0.0010	0.0013
	(0.0065)	(0.0067)	(0.0066)	(0.0064)	(0.0061)
BA+, 40+	−0.0005	−0.0004	0.0004	−0.0015	−0.0013
	(0.0072)	(0.0074)	(0.0072)	(0.0077)	(0.0081)
Controls:					
Age		X	X	X	X
Demographics			X	X	X
Sector and Union				X	X
Covid Death Rate					X

Note: N=75,199. Table reports estimates from OLS regressions of log real wage change on the standardized EE separation rate. The independent variable is the standardized EE separation rate measured at the state level from 2021_{q3} to 2022_{q1}. EE separation rates obtained from CPS monthly data. The dependent variable is log real wage change from 2021_{q1q2} and 2022_{q2q3}. Real wages obtained from CPS monthly data. All specifications include state and 6-month period fixed effects. Wage quartiles are estimated by state and period. Age controls include five age group dummies. Demographic controls include dummies for three education levels, 26 race categories, and sex. Sector controls include dummies for work in the manufacturing sector, professional services sector, finance sector, and business sector. Union controls are a dummy for union coverage. Covid death rate controls include a continuous variable for the state Covid-19 death rate per 100,000 people as of Sep 2022, and an interaction with an indicator for the 2nd and 3rd quarter of 2022. Within quartile estimates reported from a regression with quartile interactions. Within age and education estimates reported from a regression with age and education group interactions. Standard errors in parentheses, clustered by state. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table A4: Coefficient on Tightness from Pooled Regressions of Wage Change on State Labor Market Tightness

	(1)	(2)	(3)
A. Overall			
2021-2022	0.0207***	0.0189***	0.0105**
	(0.0055)	(0.0056)	(0.0054)
2015-2019	0.0010	0.0024	0.0023
	(0.0031)	(0.0025)	(0.0020)
B. 1st Quartile			
2021-2022	0.0570***	0.0572***	0.0547***
	(0.0125)	(0.0123)	(0.0125)
2015-2019	0.0013	0.0014	0.0016
	(0.0050)	(0.0048)	(0.0047)
C. High School, under 40			
2021-2022	0.0401***	0.0438***	0.0381***
	(0.0083)	(0.0093)	(0.0100)
2015-2019	0.0008	−0.0005	0.0002
	(0.0050)	(0.0038)	(0.0037)
Controls:			
Age		X	X
Demographics			X
Sector and Union			X
Covid Death Rate			X

Note: N=421,629. Table reports pooled estimates from OLS regressions of log real wage change on tightness. The dependent variable is log real wage change over 2015–19 and 2021–22 using stacked differences from year $t - 1_{q1q2}$ to year t_{q2q3}. The independent variable is state-level tightness which is an average of the standardized EE separation rate and negative standardized unemployment rate from $t - 1_{q3}$ to year t_{q1}. In order to pool the estimates, the dependent variables also include an indicator for post-pandemic time period ($period = 2021 - 22$), an indicator *post* equal to one for t_{q2q3} and zero for $t - 1_{q1q2}$, and an interaction between tightness, *period*, and *post*. The year 2020 is excluded from the analysis. All specifications include state and 6-month period fixed effects. Wage quartiles are estimated by state, year, and *post*. Age controls include five age group dummies. Demographic controls include dummies for three education levels, 26 race categories, and sex. Sector controls include dummies for work in the manufacturing sector, professional services sector, finance sector, and business sector. Union controls are a dummy for union coverage. Covid death rate controls include a continuous variable for the state Covid-19 death rate per 100,000 people as of Sep 2022 interacted with *post*. Within group estimates (quartiles or age/education groups) are reported from a regression where indicators for each group are interacted with tightness, *period*, and *post*. All covariates with the exception of our covariate of interest, tightness, are interacted with an indicator for stack (year $t - 1_{q1q2}$ to year t_{q2q3}). EE separation rates and real wages obtained from CPS monthly data. Seasonally-adjusted state unemployment rates obtained from BLS LAUS. Standard errors in parentheses, clustered by state. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table A5: Coefficient on Tightness from Regressions of Wage Change on State Labor Market Tightness - 15^{th} percentile trimmed

	(1)	(2)	(3)	(4)	(5)
Overall	0.0189***	0.0186***	0.0130***	0.0113**	0.0109**
	(0.0060)	(0.0059)	(0.0045)	(0.0045)	(0.0049)
Within wage quantiles					
1st Quartile	0.0745***	0.0746***	0.0713***	0.0707***	0.0704***
	(0.0103)	(0.0103)	(0.0097)	(0.0097)	(0.0098)
2nd Quartile	0.0455***	0.0454***	0.0435***	0.0432***	0.0429***
	(0.0086)	(0.0087)	(0.0083)	(0.0082)	(0.0083)
3rd Quartile	−0.0042	−0.0039	−0.0032	−0.0037	−0.0039
	(0.0065)	(0.0064)	(0.0062)	(0.0062)	(0.0069)
4th Quartile	−0.0042	−0.0046	−0.0046	−0.0046	−0.0048
	(0.0119)	(0.0119)	(0.0111)	(0.0110)	(0.0113)
Within age and education groups					
High School, under 40	0.0478***	0.0496***	0.0484***	0.0442***	0.0437***
	(0.0107)	(0.0109)	(0.0108)	(0.0101)	(0.0100)
High School, 40+	0.0252*	0.0242*	0.0217	0.0183	0.0179
	(0.0150)	(0.0145)	(0.0144)	(0.0135)	(0.0128)
Some College, under 40	0.0438***	0.0409***	0.0392***	0.0354***	0.0349***
	(0.0105)	(0.0105)	(0.0104)	(0.0103)	(0.0104)
Some College, 40+	0.0235**	0.0226**	0.0191**	0.0194**	0.0192**
	(0.0092)	(0.0094)	(0.0095)	(0.0089)	(0.0096)
BA+, under 40	−0.0175*	−0.0169	−0.0129	−0.0128	−0.0131
	(0.0106)	(0.0104)	(0.0103)	(0.0097)	(0.0097)
BA+, 40+	−0.0082	−0.0093	−0.0056	−0.0066	−0.0070
	(0.0117)	(0.0121)	(0.0117)	(0.0115)	(0.0122)
Controls:					
Age		X	X	X	X
Demographics			X	X	X
Sector and Union				X	X
Covid Death Rate					X

Note: N=64,318. Table reports estimates from OLS regressions of log real wage change on tightness. The independent variable is state-level tightness from 2021_{q3} to 2022_{q1}. Tightness is an average of the standardized EE separation rate and negative standardized unemployment rate, measured at the state level. EE separation rates obtained from CPS monthly data. Seasonally-adjusted state unemployment rates obtained from BLS LAUS. The dependent variable is log real wage change from 2021_{q1q2} and 2022_{q2q3}. Real wages obtained from CPS monthly data. All specifications include state and 6-month period fixed effects. Wage quartiles are estimated by state and period. The bottom 15^{th} percentile of earners in each state and period are trimmed from sample. Age controls include five age group dummies. Demographic controls include dummies for three education levels, 26 race categories, and sex. Sector controls include dummies for work in the manufacturing sector, professional services sector, finance sector, and business sector. Union controls are a dummy for union coverage. Covid death rate controls include a continuous variable for the state Covid-19 death rate per 100,000 people as of September 2022 interacted with an indicator for the 2nd and 3rd quarter of 2022. Within quartile estimates reported from a regression with quartile interactions. Within age and education estimates reported from a regression with age and education group interactions. Standard errors in parentheses, clustered by state. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table A6: Relationship Between Employment-to-Employment Separation and Industry Wage Premia

	Overall		HS, under 40		All others	
	(1)	(2)	(3)	(4)	(5)	(6)
Period 1: 2015-2019						
Ind. Wage Premium	-0.0209^{***}	-0.0141^{***}	-0.0250^{***}	-0.0162^{***}	-0.0162^{***}	-0.0130^{***}
	(0.0035)	(0.0029)	(0.0046)	(0.0045)	(0.0032)	(0.0029)
Ind. Wage Premium^2	0.0245^{*}	0.0080	0.0111	-0.0067	0.0193	0.0089
	(0.0144)	(0.0111)	(0.0121)	(0.0113)	(0.0119)	(0.0100)
Period 2: 2021-2022						
Ind. Wage Premium	-0.0232^{***}	-0.0142^{***}	-0.0433^{***}	-0.0310^{***}	-0.0153^{***}	-0.0112^{***}
	(0.0043)	(0.0028)	(0.0060)	(0.0059)	(0.0035)	(0.0028)
Ind. Wage Premium^2	0.0448^{**}	0.0272^{**}	0.0679^{***}	0.0525^{**}	0.0309^{**}	0.0211^{*}
	(0.0190)	(0.0126)	(0.0264)	(0.0252)	(0.0142)	(0.0116)
Controls		X		X		X

Note: Table reports coefficients on industry wage premium (IWP) and its square from a regression of an indicator for EE separation at time t on 3-digit industry wage premia at time $t-1$ and its square as well as demographic controls and state fixed effects. Demographic controls include dummy variables for sex, race, ethnicity, age group, education, citizenship, and metro area status. The 3-digit industry wage premia are calculated from a regression of log real wage on demographic controls and industry fixed effects for the pre-pandemic period, 2015-2019. The dependent variable, EE separation rate, is obtained from monthly CPS data. Estimates from this table are used for calculating the elasticities reported in Figure 24, panel A of Figure 25, and in the first two panels of Table 3. These elasticities are calculated by evaluating the derivative of EE separation w.r.t IWP at $x = \{-.3, 0, .3\}$ and dividing by the conditional mean of EE separation at $x = \{-.3, 0, .3\}$ to estimate the elasticity at $x = \{-.3, 0, .3\}$. The third row of each column in Table 3 reports the difference between the coefficients in rows 1 and 2. Standard errors in parentheses are clustered by industry. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table A7: Relationship Between Employment-to-Employment Separation and Industry Wage Premia: All others

	HS, 40+		BA, under 40		BA, 40+	
	(1)	(2)	(3)	(4)	(5)	(6)
Period 1: 2015-2019						
Ind. Wage Premium	-0.0082^{**}	-0.0073^{**}	-0.0145^{***}	-0.0132^{***}	-0.0054	-0.0049
	(0.0039)	(0.0031)	(0.0048)	(0.0044)	(0.0034)	(0.0032)
Ind. Wage Premium^2	0.0100	0.0089	0.0312^{***}	0.0285^{***}	0.0079	0.0081
	(0.0082)	(0.0076)	(0.0106)	(0.0098)	(0.0082)	(0.0076)
Period 2: 2021-2022						
Ind. Wage Premium	-0.0139^{***}	-0.0116^{***}	-0.0107^{***}	-0.0095^{***}	-0.0045	-0.0049
	(0.0040)	(0.0035)	(0.0040)	(0.0036)	(0.0033)	(0.0030)
Ind. Wage Premium^2	0.0196	0.0206^{*}	0.0357^{***}	0.0315^{***}	0.0049	0.0031
	(0.0124)	(0.0119)	(0.0096)	(0.0088)	(0.0099)	(0.0093)
Controls		X		X		X

Note: Table reports coefficients on industry wage premium (IWP) and its square from a regression of an indicator for EE separation at time t on 3-digit industry wage premia at time $t-1$ and its square as well as demographic controls and state fixed effects. Demographic controls include dummy variables for sex, race, ethnicity, age group, education, citizenship, and metro area status. The 3-digit industry wage premia are calculated from a regression of log real wage on demographic controls and industry fixed effects for the pre-pandemic period, 2015-2019. The dependent variable, EE separation rate, is obtained from monthly CPS data. Estimates from this table are used for calculating the elasticities reported in panels B through D of Figure 25 and in the last three panels of Table 3. These elasticities are calculated by evaluating the derivative of EE separation w.r.t IWP at $x = \{-.3, 0, .3\}$ and dividing by the conditional mean of EE separation at $x = \{-.3, 0, .3\}$ to estimate the elasticity at $x = \{-.3, 0, .3\}$. The third row of each column in Table 3 reports the difference between the coefficients in rows 1 and 2. Standard errors in parentheses are clustered by industry. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table A8: Employment-to-Employment Separation Elasticities Using Industry Wage Premia: Poisson Estimates

	Overall	HS, under 40	HS, 40 +	BA+, under 40	BA+, 40+
	(1)	(2)	(3)	(4)	(5)
Period 1: 2015-2019					
Ind. Wage Premium	-0.6718^{***}	-0.5493^{***}	-0.4417^{**}	-0.5860^{***}	-0.3061
	(0.1467)	(0.1588)	(0.1907)	(0.2100)	(0.2016)
Ind. Wage Premium^2	-0.0645	-0.5563	0.4419	0.7690^{*}	0.3735
	(0.5807)	(0.4770)	(0.4640)	(0.4614)	(0.4414)
	N=1,969,694	*N=284,602*	*N=351,275*	*N=319,995*	*N=427,576*
Period 2: 2021-2022					
Ind. Wage Premium	-0.5940^{***}	-0.9188^{***}	-0.6198^{***}	-0.3904^{**}	-0.3131
	(0.1261)	(0.1697)	(0.1830)	(0.1722)	(0.1941)
Ind. Wage Premium^2	0.7509	1.2571	0.9370	0.9751^{**}	0.1149
	(0.5756)	(0.8027)	(0.6782)	(0.3899)	(0.5516)
	N=524,684	*N=76,518*	*N=85,741*	*N=94,304*	*N=124,985*

Note: Table reports coefficients on industry wage premium and its square from a Poisson regression of an indicator for EE separation at time t on 3-digit industry wage premia at time $t-1$ and its square as well as demographic controls and state fixed effects. Demographic controls include dummy variables for sex, race, ethnicity, age group, education, citizenship, and metro area status. The 3-digit industry wage premia are calculated from a regression of log real wage on demographic controls and industry fixed effects for the pre-pandemic period, 2015-2019. The dependent variable, EE separation rate, is obtained from monthly CPS data. Standard errors in parentheses are clustered by industry. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table A9: Various Specifications of Price-Phillips Curves

	(1)	(2)	(3)	(4)	(5)	(6)
			A. Dep Var: Δ Inflation			
Δ Unemployment	-0.7924^{*}	-0.7290^{*}	-1.4588^{**}	-0.9821	-0.9578	
N=21	(0.3897)	(0.3682)	(0.6607)	(0.6375)	(0.6196)	
			B. Dep var: Δ Log CPI			
Unemployment			-1.2120^{**}	-1.1523^{**}	-1.1913^{***}	-0.8132^{**}
N=42			(0.5421)	(0.4667)	(0.4609)	(0.3580)
Pre-period	Jan/Feb '20	Jan/Feb '20	Jan-Jun '19	Sep '19-Feb '20	Jan/Feb '20	Jan-Jun '21
Post-period	Mar/Apr '22	Mar/Apr '22	Apr-Sep '22	Apr-Sep '22	Apr-Sep '22	Apr-Sep '22
LAUS adjustment		X	X	X	X	X
Imputed CPI			X	X	X	X

Note: Table reports estimates from OLS regressions of changes or levels of inflation, on changes or levels of unemployment over different periods for the 21 metro areas used in Cerrato and Gitti (2022). In panel A the dependent variable is change in inflation and the independent variable is change in unemployment, from various pre- to post-periods. In panel B the dependent variable is change in log CPI (level of inflation) from various pre- to post-periods. The independent variable is unemployment from July 2021 to March 2022. CPI-U less energy is obtained from BLS at the CBSA-level. Column 1 replicates the coefficient in Figure 2 of Cerrato and Gitti (2022). We make incremental changes to bridge their specification and time periods to ours. LAUS adjustment indicates the use of seasonally-adjusted unemployment rates obtained from BLS LAUS. CPI is reported bi-monthly at the CBSA level. In columns 3-6, we impute for missing monthly CPI assuming constant growth such that $CPI_t = \epsilon^{0.5[ln(CPI_{t-1})+ln(CPI_{t+1})]}$. Column 6 uses the same periods as our main wage- and price-Phillips curve specifications. All specifications include metro area and period fixed effects. Standard errors in parentheses, clustered at the metro-level. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$