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

David Autor
Arindrajit Dube
Annie McGrew

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MIT Department of Economics
77 Massachusetts Avenue, Bldg. E53-390
Cambridge, MA 02139

National Bureau of Economic Research
1050 Massachusetts Avenue, 3rd Floor
Cambridge, MA 02138

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David Autor
MIT & NBER

Arindrajit Dube
UMass Amherst & NBER

Annie McGrew
UMass Amherst

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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 around one-third 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. Comparing across states, post-pandemic labor market tightness became strongly predictive of 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. Seen through the lens of a canonical job ladder model, the pandemic reduced employer market power by increasing the elasticity of labor supply to firms in the low-wage labor market. This spurred 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, 1992; Katz et al., 1999; DiNardo et al., 1996; Autor et al., 2008a, 2016a). 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 separations 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-paying to higher-paying employers would increase.

This paper studies the role of imperfect competition in shaping wage setting, worker separations, and worker reallocation in the rapid tightening of the US labor market in the years immediately before and after the Covid-19 pandemic. A key fact motivating our inquiry is that both real and relative wages have grown substantially more at the bottom of the distribution (10^{th} percentile) than at the median or top (90^{th} percentile) since the onset of the pandemic. The top panel of Figure 1 documents this pattern by tracing the evolution of the 10^{th}, 50^{th} and 90^{th} percentiles of the real US hourly wage distribution between 2015 and 2023, with each series normalized to one in January of 2020.[1] Real US hourly wages rose by approximately 10 percentage points at all percentiles during the first quarter of the Covid-19 pandemic, from March through June of 2020. (As we show below, much of this spike reflected a change in composition of the workforce as low-wage workers disproportionately lost their jobs.) Thereafter, these quantiles diverged. The 10^{th} wage percentile held its real value over the next three years, while the 50^{th} and 90^{th} real wage percentiles *fell* by around 6 and 8 percentage points, respectively. In net, the 90/10 ratio declined by about 8 percentage points over these three years, with almost three-fourths of this compression explained by the decline in the 50/10 ratio. Moreover, despite substantial post-pandemic inflation–measured with 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 7.8% between January 2020 and June 2023.

Was this rapid wage compression unexpected? A notable pattern in panel A of Figure 1 is that the 10^{th} percentile of the wage distribution was also rising modestly relative to the median in the five years prior to the pandemic, as discussed in depth by Aeppli and Wilmers (2022), Dey et al. (2022), and Shambaugh and Strain (2021). This might indicate that wage compression following

[1] Details on data construction are provided in in Section 1.

the pandemic primarily reflects an acceleration of pre-pandemic wage trends. An alternative (or complementary) explanation is that pre-pandemic wage compression was substantially driven by state minimum wage laws, many of which were adopted in the preceding decade (Cengiz et al., 2019). The bottom two panels of Figure 1 explore these possibilities by contrasting wage trends in the states that set minimum wages above the federal level (29 states + Washington D.C.) with analogous trends in the 21 states that did not.[2] Prior to the pandemic, compression of the 50/10 wage ratio evident in panel A was entirely confined to states with minimum wages above the federal level (panel C). No such compression is evident in non-minimum wage states (panel B). Starting in 2021, however, the 10^{th} percentile rose steeply in both sets of states—minimum wage and non-minimum—relative to the median and 90^{th} percentiles, leading to a sharp wage compression that was in fact slightly more pronounced in non minimum-wage states. These patterns suggest that, while institutional forces likely drove wage compression in a subset of states prior to the pandemic, the sharp wage compression in both minimum- and non-minimum-wage states after 2020 was not, primarily, a continuation of this trend.

How economically significant is the post-2020 wage compression? Figure 2 compares it with two other well-known episodes of major wage structure change. Using data on hourly wages from the decennial Census, the first bar in Figure 2 shows that during the 'Great Compression' of 1940–1950, the 90/10 log wage ratio closed by 23.7 log points (Goldin and Margo, 1992). The late 1970s mark the beginning of the Great Divergence (Krugman, 2009; Noah, 2012), a four-decade interval of nearly continuously rising inequality: the 90/10 hourly earnings ratio increased by 28.6 log points between 1979 and 2019. In comparison, the 8.6 log point 'Unexpected Compression' of 2019 to present (third bar) was only about one-third as large in absolute magnitude as these two prior episodes. However, it also unfolded in a matter of only three and a half years. Moreover, this compression was primarily accounted for by rising earnings levels and falling inequality in the lower half of the wage distribution. As documented in Table A1, this pattern is distinct from the two prior episodes, where reductions in upper half inequality played the dominant role.[3]

This sharp change in wage structure offers an unusual opportunity to analyze the competitive and frictional forces that shape low-wage earnings in the US labor market. Leveraging changes in labor market tightness accompanying the Covid-19 pandemic, we explore whether the 'Unexpected Compression' is adequately accounted for by the benchmark competitive labor market model in which the law of one price for skill prevails, or whether more is needed. The focal alternative we consider is a search setting where labor market frictions provide each employer with limited monopsony power. Although the competitive and frictional models have much in common, they make distinct predictions for how tightening labor market conditions—due to rising labor demand or falling labor supply—impact job separations, wage growth, and wage inequality. In both settings, rising labor market tightness generates higher wages. What distinguishes these settings is the role

[2]Virginia was the only state that raised its minimum wage above the federal level in the post-pandemic period. Though we base our minimum wage classifications on 2019, we verify that classifying Virginia as a "minimum wage" state has a negligible impact on our results.

[3]Also, see Table I of Goldin and Margo (1992) and Figure 7 of Autor et al. (2008b).

played by the sensitivity of worker separations to wage levels, conventionally known as the 'quit elasticity' (Manning, 2013; Langella and Manning, 2021). In the canonical competitive setting, the quit elasticity is infinite, giving rise to a law of one price for workers of the same skill level. In the frictional model, this elasticity is finite, enabling different firms to pay identical workers different wages. As the market tightens, however, the frictions that support these pay differentials attenuate. Quits rise differentially at low-wage employers, low-wage workers reallocate from lower-wage to higher-wage firms and sectors, wage inequality falls among workers with similar productive characteristics, and wage gains among low-wage workers accrue primarily to job-changers rather than job-stayers.

We formalize these implications in a simple dynamic wage-posting model (Burdett and Mortensen, 1998) that links labor market tightness, employer competition, and worker reallocation. We test this model's implications by exploiting cross-state variation in post-pandemic labor market tightness. We show that the aggregate wage compression seen in Figure 1 is significantly more pronounced in states with tighter post-pandemic labor market conditions, and it is concentrated in the lowest quartile of the wage distribution, among young, non-college workers, and among Black/Hispanic workers. These patterns are consistent with both competitive and frictional labor market settings. We adjudicate between these interpretations by testing whether the quit elasticity rose as the labor market tightened, as predicted by the frictional model, and whether this rise was more pronounced at lower wage quantiles, as also predicted. We confirm both predictions. Moreover, we estimate that wage compression is entirely accounted for by wage growth associated with job changes rather than same-job wage growth, suggesting that increased competition has led to a reallocation of jobs from low-wage to higher-wage employers. Consistent with a decline in labor market frictions, we document that falling aggregate inequality between 2015 and 2023 primarily reflects a reduction in wage differences among workers of the same age-by-education groups, rather than a fall in earnings gaps among different skill groups.

Our primary interpretation of this body of evidence is that rapid labor market tightening accompanying the pandemic attenuated employer market power, thus spurring rapid wage growth among young non-college workers, who disproportionately moved from lower-paying to higher-paying and potentially more-productive jobs. We also discuss non-traditional reasons for why employer market power may have declined, including: decreased worker-firm attachment spurred by mass job loss during the pandemic; increased household liquidity deriving from pandemic transfer programs; and shifting perceptions about the availability of higher-wage jobs (Jäger et al., 2022). Each of these narratives implies a fall in job stickiness, which is the central mechanism of our search-theoretic interpretation.

While the specific labor market conditions surrounding the pandemic were unique, the economic mechanisms at work are, we believe, quantitatively but not qualitatively different from those operating in conventional circumstances. Consistent with this interpretation, we find that state-level wage-Phillips curves steepened and slackened on multiple occasions in the last four-decades. Applying consistent data and methods back to 1980, we find that the wage-Phillips curve steepened in the mid-1980s during the rebound from the deep early-1980s recession; slackened from the early 2000s recession through the slow recovery from the Great Recession; steepened again (modestly) during the improving economy of 2015-2019; and then became far steeper as Covid-era lockdowns were lifted. While our data do not permit analogous time-series estimates of the quit elasticity prior to the year 2015, our evidence suggests that this elasticity reached a pre-pandemic high in 2019, fell sharply in 2020 and early 2021, and then attained a post-pandemic peak in 2022. We further estimate that labor market tightness has declined since its peak in 2022 and that wage compression has largely plateaued. Nothing in our data suggests that the 'Unexpected Compression' has begun to unwind, however.

We also consider whether our inferences about wage compression are skewed by the rapid diffusion of work-from-home (WFH) arrangements after 2020 among (primarily) high-wage workers, who are increasingly able to conduct their work remotely. Survey evidence from Barrero et al. (2021), reported in Barrero et al. (2023), finds that workers value WFH at approximately 8 percent of pay, and that employers offered the WFH benefit after 2020 in part to moderate wage growth (Barrero et al., 2022a). Using data from Hansen et al. (2023), we confirm that WFH rose by significantly more in states with tighter post-pandemic labor markets. Yet, because cross-state differences in tightness explain only a small part of the overall rise, incorporating the amenity value of WFH into wages implies only a very small downward adjustment to tightness-induced wage compression. Similarly, we show that adjusting for secular and cyclical shifts in the demographic composition of the employed workforce has only a modest impact on our estimates of wage compression.

Several 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, though these papers do not link changes in market power to reallocation or wage inequality. Similarly, focusing on the pre-pandemic labor market, Bivens and Zipperer (2018), Baker and Bernstein (2013), and Freeman (1990) 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. Our work also complements findings from Haltiwanger et al. (2018), who document that the job ladder and reallocation of workers to high-wage firms broke down following the Great Recession. Faberman et al. (2022) find that conventional unemployment measures understated the extent of labor market tightness in 2020 and 2021 due to a decline in desired work hours. 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 compression rather than inflation, we confirm their finding that faster nominal wage growth in tighter markets was tempered by faster local price growth. Nevertheless, tightness-driven wage growth at lower wage quantiles substantially exceeded tightness-driven inflation, meaning that low-wage workers gained in real terms.

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 overall and by education, as well as complementary evidence on wage trends by demographic characteristics. Section 4 explores the mediating role of job-to-job separations. This section decomposes wage growth by job change status, and provides estimates of the quit elasticity, and the wage-Phillips curve (overall, as well for different quantiles and demographic groups). It further documents the contribution of falling inequality within demographic groups to aggregate wage compression, and analyzes the contributions of industry-switching and job-switching to aggregate wage growth. 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 sourced from IPUMS (Flood et al., 2021), covering the the period January 2015 through June 2023. Some historical analyses use CPS Outgoing Rotation Group (ORG) data from 1979 forward. Because the IPUMS CPS ORG data are not available prior to 1982, we use NBER's CPS ORG extracts for the years 1979 through 1981. To estimate wage compression between 1940 and 1950, we use IPUMS 1% samples of the decennial Census of Populations.

The CPS provides a consistent, representative sample of the US workforce, captured at high (monthly) frequency and publicly released with minimal lag. The primary disadvantage of the CPS is that its monthly sample size of 60,000 households limits state and metro-level analysis, particularly for earnings outcomes, which are collected from only one-fourth of sampled households each month (i.e., the ORG sample). We supplement the CPS data with representative state data series from the BLS Local Area Unemployment Statistics (LAUS), publicly available tabulations of worker quits and job-to-job movements from the Longitudinal Household and Employment Dynamics database (LEHD) via the J2J data portal, industry-level job quit data from the Job Opening and Labor Turnover Statistics (JOLTS), and inflation data from the Consumer Price Index (CPI). We also draw on Goda and Soltas (2022) for estimates of Covid-19 deaths by state during the pandemic, on Barrero et al. (2021) for representative data on the evolution of Working from Home (WFH) during and after the pandemic, on Vaghul and Zipperer (2019) for data on state minimum wages, and on Hazell et al. (2022) for data on historical state-level inflation.

The CPS interviews each sampled household eight times over 16 months. 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 months in sample (MIS) 4 and 8, respondents are asked to report their wage. For wage calculations, we drop all observations with imputed wages. (We show below how our sample adjustments affect our primary inferences.) Our analytic samples include all individuals ages 16-64. To measure within-person changes, we match individuals across CPS samples observed one year apart. In forming matches, we include only individuals who report a consistent gender throughout the panel and report no more than a two-year age change between matched observations.

Our analysis focuses on hourly wages of workers in paid employment excluding the self-employed. For hourly-paid workers, we use self-reported hourly wages. For salaried workers, we calculate hourly wages as the quotient of weekly earnings and usual weekly hours at their main job. In most of our analysis, wages are deflated using the national CPI-U data from the US Bureau of Labor Statistics. We also construct a state-level CPI-U measure using a combination of regional, state and metro-level information to assess geographic variation in price growth, and to construct regional price deflated wages; the details of which are explained in Section 5.

We take several steps to create a consistent wage series in the CPS. We process extremely low hourly wage observations in the CPS (either reported or calculated) by winsorizing all wage observations that are below the minimum wage for tipped workers to \$2.13 per hour, which has been the tipped minimum wage since 1991. There were multiple changes in the Census' topcoding procedure during our study window. Between January 2003 and March 2023, the Census Bureau winsorized *weekly* earnings in excess of \$2884.61 to \$2884.61 (nominal), equivalent to annualized earnings of \$150,000. Prior to 2003, the Census winsorized annualized earnings at \$100,000. Beginning in April 2023, the Census Bureau imposed a "dynamic" topcode in which the hourly wages of the top 3% of earners are replaced with the weighted average of their reported earnings.

We account for these changes as follows. For wage data from January 2003 forward, we topcode hourly earnings at \$75 for both hourly and salaried workers, as this translates into annualized earnings of \$150,000 at 40 weekly hours over 50 weeks. For pre-2003 data, we topcode all nominal hourly earnings greater than \$50 to exactly \$50, i.e., the hourly equivalent of \$100,000 in annual earnings. Following convention, we replace topcoded wage values in these years with the estimated conditional mean above the topcode. We assume that the censored upper tail of the wage distribution has a Pareto distribution with shape parameter $\alpha = 3$, implying that $E[w|w > z] = \frac{\alpha}{1-\alpha} \times z = 1.5z$ (Acemoglu and Autor, 2011). We finally circumvent the "dynamic" 2023 top-coding change in 2023 by excluding wage observations for workers who entered the CPS after 2022.[4] This procedure requires us to drop 50% of CPS wage observations after March 2023. We accordingly double the sample weights on retained wage observations.

Our analyses of wage quantiles account for non-classical measurement error in reported wages stemming from round number bunching. Bunching occurs both because in reality, wages are frequently bunched at round numbers, and because survey respondents further round their wages when reporting (Dube et al., 2018). This creates substantial flat spots in measured nominal wage quantiles, which may exhibit no changes for many years when bunched at a round number (e.g., \$10/hr), followed by a sudden, substantial change when moving to a new round number (e.g., \$11/hr). For example, Figure A1 documents that the 10$^{\text{th}}$ percentile nominal wage nationally in the CPS data was 'stuck' at \$10/hour for two years, from the 3$^{\text{rd}}$ quarter of 2017 through the

[4]Recall that respondents are included in the survey for 4 months (MIS 1-4), are out for 8 months, and are then back in for another four (MIS 5-8), with wage data collected in MIS 4 and 8. Respondents who entered the CPS sample between September 2021 and December 2022 will contribute wage observations under the old topcoding scheme through April of 2024.

3rd quarter of 2019. It is likely that the estimated fall in the measured *real* 10th percentile over those two years was an artifact of nominal bunching. To uncover changes in the underlying distribution, we smooth wage quantiles by calculating national wage quantiles by month and then predicting wage quantiles by rank using a lowess regression. For cross-demographic and earnings group comparisons, we construct smoothed-wage quantiles by group and time period. The wage quantiles resulting from this procedure are far more stable, as illustrated by the top panel of Figure 8—which is the smoothed analog to Figure A2. Figure A3 documents how these data construction steps affect measured inequality in the data by plotting in successive panels un-smoothed wage quantiles (A); smoothed wage quantiles (B); smoothed wage quantiles with imputed wages excluded (C); and smoothed wage quantiles with imputed wages excluded and further adjusted for changes in demographic composition across sample years (D). Our primary descriptive figures implement steps (A) – (C), with several sensitivity analyses reporting step (D).

Some analyses track worker-level job changes using the panel component of the CPS. The CPS captures job change with high precision using IPUMS *EMPSAME* question: "Last month, it was reported that [name/you] worked for [company name]. [Do/Does] [you/he/she] still work for [company name]?"[5] We define job-movers as those who respond "no" to this question. 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 surveys a large sample of establishments each month to precisely capture job openings, hires, and separations. The *EMPSAME* question is unfortunately asked only during interview months MIS $\in \{2, 3, 4, 6, 7, 8\}$. Job changes are accordingly not observable in MIS $\in \{1, 5\}$ nor during the eight months when respondents are out of rotation. We therefore cannot reliably track job changes occurring between a worker's two wage observations, which are recorded one year apart. To address this limitation, we implement a measurement correction procedure in Section 4.3 that apportions observed annual wage gains into those accruing to switchers versus stayers.

A frequently used alternative approach for measuring job change in the CPS codes is to code month-to-month and year-to-year changes in respondents' reported industry and occupation as job changes.[6] This technique introduces significant measurement error, however, because the Census Bureau assigns industry and occupation codes to each CPS respondent each month based on their self-reported ('write-in') job description. If a worker is not entirely consistent in describing her job across successive surveys, or if the Census Bureau re-tunes its textual coding algorithm, spurious industry and occupation transitions result.[7]

[5]The questionnaire is available at `https://www.census.gov/programs-surveys/cps/technical-documentation/questionnaires.html`. Fujita et al. (2020) detect an increase in the incidence of missing answers between 2007-2009 to the CPS question corresponding to the IPUMS variable EMPSAME, which they ascribe to changing Census technology and policies. This increase in missing answers levels out around 2015, meaning the bias in the CPS variable primarily affects the *level* of the measure after 2014. The effect on changes in self-reported job separations after 2014, which is the object of interest here, should be relatively modest.

[6]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.

[7]The exact CPS survey question for a worker's industry is: "What kind of business or industry is this?" The Census

Where worker-level job change data is not required, we instead use publicly available state-level Job-to-Job (J2J) Flows data calculated from Longitudinal Employer-Household Dynamics (LEHD), a matched employer-employee dataset covering 95% of employment in the U.S.[8] These data provide more precise estimates of job-to-job transition rates than the CPS. While the CPS can be used to estimate state-level unemployment 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).

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

This section provides a conceptual model that links labor market tightness, employer competition, and worker reallocation in an imperfectly competitive labor market setting, and contrasts that setting with the textbook fully-competitive, market-clearing model. In both perfectly and imperfectly competitive settings, rising labor market tightness—stemming from either greater demand for labor or fewer workers looking for a job—generates higher wages. The imperfectly competitive model makes four additional predictions about the market-level impacts of rising tightness that are absent from the textbook model: 1) a compression in wages paid to similar workers; 2) increased reallocation of workers from low-paid to higher-paid employers; 3) greater responsiveness of worker separations to wage levels, i.e., a higher quit elasticity; and 4) concentration of wage gains among job-movers rather than job-stayers. We develop those implications here and test them in subsequent empirical sections.

We begin with the benchmark, static model of labor market competition depicted in Figure 3. This competitive market setting is portrayed at the market level in panel A and at the firm level in panel B. Consider an inward shift in the market labor supply curve from LS to LS', leading to a rise in the market-clearing wage and a fall in the equilibrium quantity of employed labor (panel A). Viewed from the perspective of a price-taking firm (panel B), the labor supply curve firm shifts upward and employment falls. Perfect competition enforces a law of one price for skill, so the wage increase is identical across all firms employing workers of that skill level.

We contrast this case with a monopsonistically competitive setting where the wage and employment impacts of a uniform increase in the labor supply elasticity may differ between high and low productivity firms. Specifically, consider a market with a large number of firms, J, and a mass of workers who have idiosyncratic preferences over jobs, ν_j, drawn from a Type I Extreme Value distribution, with $U_{ij} = \epsilon^L \ln(w_j) + \nu_{ij}$. As shown in Card et al. (2018), these preferences give rise

follows a parallel procedure for assigning occupation codes.

[8] https://lehd.ces.census.gov/data/#j2j

to residual firm-level labor supply function with a constant elasticity:

$$l_j(w_j) = L \times \frac{w_j^{\epsilon^L}}{\sum_{k=1}^{J} w_k^{\epsilon^L}}$$

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

where L is the total number of workers in the market, and ϵ^L is the labor supply elasticity. Due to the large-J assumption, the term $\sum_{k=1}^{J} w_k^{\epsilon^L}$ is taken as a constant by any given firm. The inverse labor supply function of each firm can therefore be written as:

$$\ln(w_j) = \left[\frac{\ln(\sum_{k=1}^{J} w_k^{\epsilon^L}) - \ln(L)}{\epsilon^L}\right] + \frac{1}{\epsilon^L}\ln(l_j). \tag{1}$$

Next, consider an increase in labor market competition that raises the labor supply elasticity, ϵ^L, thus increasing the stringency of labor market competition. Figure 4 depicts the effect of this change on employment and wages at two firms that differ in productivity but face the same inverse labor supply elasticity $1/\epsilon^L$. As illustrated in the graph and demonstrated formally next, the increase in market competition compresses the wage differential between the two firms and causes a partial reallocation of labor from the low-productivity firm (panel A) to the high-productivity firm (panel B).

To illustrate these results formally, we parameterize each firm's production function as $y_j = p_j \times \ln(l_j)$, where p_j is a firm-specific productivity shifter. The MRPL is p_j/l_j, so a profit-maximizing firm sets wages at $w_j = \frac{\epsilon^L}{1+\epsilon^L} \times \frac{p_j}{l_j}$. If we take two firms $j \in \{H, L\}$ where $p_H > p_L$, relative wages can be written as:

$$\frac{w_L}{w_H} = \frac{l_H}{l_L} \cdot \frac{p_L}{p_H}$$

Further substituting in the expression for the labor supply function $l_j(w_j)$ and rearranging terms yields the following equilibrium relative wages:

$$\frac{w_L^*}{w_H^*} = \left(\frac{p_L}{p_H}\right)^{\frac{1}{\epsilon^L+1}} \tag{2}$$

Taking logs and differentiating with respect to ϵ^L yields:

$$\frac{\partial\left(\ln(w_L^*) - \ln(w_H^*)\right)}{\partial \epsilon^L} = \frac{\ln(p_H) - \ln(p_L)}{(\epsilon^L+1)^2} > 0.$$

The positive sign of this derivative indicates that as labor supply becomes more elastic (higher ϵ^L), the wage gap between low- and high-productivity firms declines (becomes less negative), meaning that the wage distribution compresses. This result is seen in Figure 4, where an increase in ϵ^L spurs an increase in wages at both low and high-productivity firms, but more so at the former than the latter. This is the first empirical prediction of the imperfectly competitive model: rising competition leads to wage compression due to a reduction in the dispersion of wages among workers of the same skill level.

A second implication of the model is that rising competition spurs labor reallocation from low- to high-productivity firms. At the level of an individual monopsonistically competitive firm (where the term $\sum_{k=1}^{J} w_k^{\epsilon^L}$ is held constant), an increase in the labor supply elasticity (ϵ^L in equation (1) spurs a fall in the firm's marginal factor cost and hence an unambiguous increase in employment. Since not all employers can raise employment simultaneously, this partial equilibrium logic does not carry over to the market level. The labor supply curve facing each firm must shift inward in order to ensure that aggregate firm employment is consistent with market labor supply. Rising competition therefore decreases the slope and increases the intercept of the inverse labor supply curve, as shown in Figure 4. At the new market equilibrium, the marginal factor cost is lower than in the original equilibrium *only* for a subset of firms with sufficiently high productivity. Firms at or above this threshold increase hiring (Figure 4B) while those below it reduce hiring (Figure 4A).

This reallocation is seen in our model as follows. Rearranging equation (2) to obtain relative employment at the low versus high-productivity firm yields:

$$\frac{l_L^*}{l_H^*} = \left(\frac{p_L}{p_H}\right)^{\frac{\epsilon^L}{\epsilon^L+1}}. \tag{3}$$

Taking logs and differentiating (3) with respect to ϵ^L gives:

$$\frac{\partial \left(\ln(l_L^*) - \ln\left(l_H^*\right)\right)}{\partial \epsilon^L} = \frac{\ln\left(p_L\right) - \ln\left(p_H\right)}{\left(\epsilon^L+1\right)^2} < 0.$$

The negative sign of this derivative indicates that when the labor supply elasticity rises, relative employment falls at the lower-productivity firm. Since market labor supply is fixed at L, an equilibrium reduction in *relative* employment at lower productivity firms implies that there is labor reallocation from low- to high-productivity firms. Thus, a second prediction of the monopsonistic competition model is that a tightening of competitive conditions causes more-productive, higher-paying employers to become relatively larger.

A third prediction of this model is that the wage gains stemming from a tightening labor market are particularly concentrated among movers. The reason, also seen in Figure 4, is that while both firm-stayers and firm-movers experience a wage gain as the labor supply elasticity rises, movers benefit both from rising market wages at all firms and from a boost to their marginal products as they move from low- to high-productivity firms.

The final prediction of the monopsony model is that the elasticity of worker separations with respect to (low) wages rises as the labor market tightens. This prediction emerges when we embed our static labor supply model in a search framework, which we do next.

2.1 Why labor market tightness increases the elasticity of labor supply

The critical ingredient in the 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 canonical search framework (e.g., Burdett and Mortensen (1998); Bontemps et al. (1999); Moscarini and Postel-Vinay (2018)). Consider a dynamic wage posting model where workers engage in on-the-job search. The rate of job separations at wage w in this model can be written as $S(w) = \delta + \chi + \lambda_e (1 - F(w))$, where δ is the exogenous separation rate to non-employment, χ is the exogenous separation rate into another—possibly worse-paying—job, sometimes called a 'Godfather shock' (i.e., a job offer you cannot refuse), λ_e is the outside offer arrival rate for current employees, and $F(w)$ is the cumulative wage offer distribution. Due to frictional wage dispersion, this distribution is non-degenerate.

In this setting, workers separate to better-paying jobs if they receive a superior wage offer. This occurs at rate $\lambda_e (1 - F(w))$, which is a function of the wage at the current employer, w, holding fixed λ_e and $F(w)$. Taking logs and differentiating, the overall quit (EE separation) elasticity is $\epsilon^{EE} = -\lambda_e f(w)w/(\chi + \lambda_e (1 - F(w)))$, which depends on the employer's own wage *rank* in the aggregate distribution, $F(w)$. One implication of this observation, which we use 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 offer arrival 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, $m(JS, V)$, and the offer arrival rate to employed workers as $\lambda_e = \frac{m(JS,V)}{JS} = m(1, \theta)$, where $\theta = \frac{V}{JS}$ corresponds to labor market tightness. Rearranging the total job-search effort equations as $JS = (1 - \phi(1-\delta))u + \phi(1-\delta)$ makes it clear that $\frac{\partial JS}{\partial u} > 0$.[9] Thus, θ is also monotonically rising in the conventional tightness measure $\tilde{\theta} = V/u$.

The offer arrival rate to employed workers, $\lambda_e = \frac{m(JS,V)}{JS} = m(1, \theta)$, is also rising in market tightness. By implication, for a given equilibrium wage distribution, the quit elasticity ϵ^{EE} increases in absolute value (becomes more negative) as market tightness (θ or $\tilde{\theta}$) rises. These comparative statics highlight how rising tightness, by increasing the offer arrival rate, raises the magnitude of the quit elasticity, which is the fourth implication of our conceptual framework. We note that market tightness may increase due either to 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.

We have so far ignored the changes in the wage offer distribution $F(w)$. Endogenizing this

[9]The derivative, $\frac{\partial JS}{\partial u} = 1 - \phi(1-\delta)$, is positive if $\phi < \frac{1}{1-\delta}$, which for small enough δ means that $\phi <= 1$. The term ϕ is assumed to be strictly greater than zero, since a zero value would imply an absence of on-the-job search and result in the Diamond paradox—where even an infinitesimal amount of search friction results in all firms setting wages at the single monopsonist level (Diamond, 1971). Thus, $0 < \phi \leq 1$ and $\frac{\partial JS}{\partial u} > 0$.

distribution and allowing it to depend on firm-level productivity does not change the model's key features, however, as shown in Appendix A2. The reason is that, while the separation elasticity, ϵ^{EE}, depends on the wage distribution (an endogenous object), the effect of an increase in the offer arrival rate, λ_e, on the separation elasticity depends only on the rank of the firm's wage (or productivity, p), $r = F(w) = H(\kappa^{-1}(w))$, which is a primitive of the model (here $w = \kappa(p)$ is the mapping between productivity and wage, and is assumed to be monotonic).

Consistent with the logic of the static monopsony model above, a rise in the offer arrival rate affects EE separations more at low- than at high-productivity employers, reallocating workers from the former to the latter. To see this formally, observe that the EE separation rate is equal to $\chi + \lambda_e(1 - F(w)) = \chi + \lambda_e(1 - H(p))$. Since $(1 - F(w))$ is monotonically decreasing in w, an increase in the offer arrival 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. Intuitively, for workers who are already at the highest-paid quantile of the wage distribution, an increase in the offer arrival rate never yields a better offer and hence never triggers a separation; by contrast, this same increase boosts the odds that a low-paid worker gets a better offer. Figure 5 illustrates this logic by plotting the EE separation rate by firm wage-rank locus for λ_e equal to 0.02 and 0.04. The 'rotation' of the equilibrium separations-wage locus, as the offer arrival rate rises, reflects the reallocation of lower-wage workers toward higher rungs of the job ladder. Figure 6 further shows how, in steady state, a tighter labor market, represented by a higher offer arrival rate, yields a larger fraction of the workforce employed at more-productive (i.e., high-ranked) firms.

2.2 Empirical implications

We empirically assess these relationships in three steps. First, we document trends in (overall and residual) wage compression and assess the role played by tightness. We then estimate EE separation elasticities with respect to residual wages to test if they have risen in the post- relative to pre-pandemic period, as predicted. We finally explore whether the rise in separations is associated with wage compression and a reallocation of workers from lower- to higher-paid jobs—measured both by wage gains among job movers and by worker reallocation out of low-wage sectors. 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, $\lambda_e = \phi\lambda_u$.[10] 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 separation rate.

Alongside the direct role of labor market tightening in boosting the separation elasticities as implied by theory, we suspect that other pandemic-specific factors may have contributed as well. One such factor is the structure of transfer payments during the pandemic. While most industrialized countries used public payments to employers to retain workers during lock-downs (Giupponi et

[10] 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 the job-finding rate among those at work has no impact on wages.

al., 2022), the US instead vastly increased the scope and generosity of unemployment benefits programs for millions of workers who were temporarily or permanently laid off during the pandemic.[11] This dissolution of worker-firm ties may have increased workers' 'footlooseness' following the initial pandemic shock, especially in low-wage sectors such as hospitality, which experienced the deepest pandemic-related contraction. These dislocations may have weakened existing employer-employee bonds. Additionally, existing 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 might raise workers' awareness of better options, both directly, by increasing workers' own search activity, and indirectly, through observing their coworkers find new jobs (Porter and Rigby, 2021).

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.[12] Finally, if liquidity constraints prevent low-wage workers from bargaining for higher wages, pandemic stimulus payments may have facilitated job switching or wage bargaining (Caratelli, 2022). Though our analysis does not adjudicate among these narratives, each implies a fall in job stickiness, which is the central mechanism of our search-theoretic model.

3 The unexpected compression

3.1 Employment drop and rebound

The onset of the pandemic saw the sharpest drop in US employment in the post-WWII era. Between January and May of 2020, the employment-to-population (EPOP) rate dropped from 71.5% to 62.7%, while the labor force participation rate (employment plus unemployment, LFP) fell from 74.4% in January 2020 to 72%, reflecting a massive surge in unemployment. Panel A of Figure 7 plots the evolution of the employment-to-population (EPOP) and labor force participation (LFP) rates between January 2015 and June 2023, with each data point normalized relative to its January 2020 value (so January 2020 equals 1.0). Between January and May 2020, EPOP fell 12.4% while LFP fell by 3.2%.

Reflecting the fact that pandemic business shutdowns were concentrated in front-line, customer-facing services, the employment drop was particularly pronounced among less-educated workers who typically perform these jobs. As shown in panel B of Figure 7, EPOP fell by 17% among those with a high school diploma or less versus 8.2% among those with a Bachelor's degree or higher.[13]

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

[12] 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).

[13] In absolute rather than proportional terms, EPOP for those with a college degree decreased from 84.3% to 77.4%, and from 60.3% to 50.0% among those with high-school or lower education.

After reaching a trough in late 2020, the subsequent rebound of employment was equally dramatic. By mid 2022, EPOP among adults with no more than a high school education had returned to 100% of its early-2020 level, which it subsequently surpassed. Although the EPOP among adults with a BA or more education did not have nearly as much ground to make up, it also had returned to its pre-pandemic level by mid-2023 (Montes et al., 2022).

3.2 Wage distribution changes

The disproportionate exposure of less-educated workers in low-wage occupations to adverse pandemic employment shocks would normally presage a divergence between the top and bottom of the wage 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 justified in labeling as *unexpected* the wage compression that followed the Covid-19 pandemic.

To provide a picture of real wage changes across the full wage distribution, Figure 8 plots (smoothed) annualized nominal wage changes by percentile between January 2020 and June 2023. The figure overlays the contemporaneous change in the CPI-U to delineate real versus nominal wage changes: values above this line indicate real wage gains while those below indicate real wage declines. The first panel shows that, over this nearly 3.5 year period, real wage gains were positive at and below the 85^{th} percentile and were negative for the top 15% of earners. Focusing only on the most recent 24 months of data (June 2021 through June 2023)–a period with higher inflation–real wage gains accrued to the bottom 35% of the wage distribution while other percentiles lost in real terms. Finally, in the twelve months between June 2022 and June 2023, real wage gains were broadly positive, and spread relatively evenly throughout the distribution, with slower growth in the top decile. Most of the wage compression was therefore concentrated between January 2020 through June 2022. Figure A4 reports annualized wage growth across the distribution during the four years preceding the pandemic, 2015–2019. A comparison with Figure 8 underscores that wage compression was substantially greater between 2020 through 2023 than between 2015 through 2019. Although hourly wages are the standard metric of compensation, they do not capture potential changes in non-wage compensation that coincided with the pandemic. Barrero et al. (2022b) 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 arguably intensified in the years immediately after the pandemic: greater disease exposure risk, thinner staffing levels, and a seeming epidemic of irate customers. It therefore appears plausible that trends in real wage compression modestly overstate the compression of full compensation. We return to this point in Section 4.1.[14]

[14] 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. According to their estimates, real median incomes of the bottom quintile of earners rose by over 60% in 2020, more than offsetting subsequent relief reductions during the 2021 recovery.

3.3 Between-group inequality

We next document how wage compression is reshaping wage inequality between skill groups. Thirty years of literature has analyzed the expansion of these differentials (Katz and Murphy, 1992; Katz et al., 1999; Card and Lemieux, 2001; Lemieux, 2006; Autor et al., 2008a; Autor, 2014; Hoffmann et al., 2020; Vogel, 2023). Panel A of Figure 9 documents their recent contraction. Pronounced wage growth among high-school and some-college workers relative to workers with a four-year degree compressed these differentials by several percentage points between 2020 and 2023. Akin to the pre-pandemic compression of the lower tail of the distribution, the wage differential between college and high-school workers was contracting prior to the pandemic (Aeppli and Wilmers, 2022), particularly between 2015 and 2017. But the post-pandemic compression was more pronounced and occurred more rapidly.[15]

Younger workers have seen the largest wage gains since the onset of the pandemic, as shown in panel B of Figure 9. Further subdividing the data by age and education in Figure 10 reveals that these trends are driven by outsized wage growth among young non-college workers. Of the four groups depicted in the figure—high-school vs. college-educated by above vs. below age 40—young high-school workers constitute the only group that has *not* seen its post-pandemic earnings gains largely eroded by inflation. Our subsequent analysis of mechanisms focuses on the contrast between young high-school workers and the balance of the workforce.[16]

Our theoretical framework does not directly predict how rising labor market tightness should affect wage gaps between skill categories (proxied, for example, by age-by-education groups). However, to the extent that non-college worker hiring is more sensitive to overall strength of the economy—as evidenced by greater cyclicality of high school worker unemployment in most time periods—we may expect a tighter labor market to raise wage growth differentially among low-paid workers.

This pattern of wage compression is also highly apparent in comparisons across occupations. Grouping occupations into terciles based on their wage ranks in 2019, Figure 11 documents that wage growth has been strongest in the lowest occupational wage tercile and weakest in the top occupational wage tercile since early 2020. Paralleling earlier results, pre-pandemic wage growth in low-wage occupations exceeded that of mid- and high-wage occupations between 2015 and 2020, but this trend sharply accelerated with the pandemic: since early 2020, average earnings in the lowest tercile of occupations have grown relative to earnings in high-wage and mid-wage occupations by ten and four percentage points, respectively. Low-wage occupations tend to be intensive in non-routine manual tasks (Autor et al., 2003), such as food preparation, building and grounds, cleaning, personal care, and personal service. These occupations suffered a disproportionate share of layoffs

[15] As show in Figure 9, the real wage gap between some-college and four-year college-graduate workers has closed even more rapidly than the pure college/high school wage gap. We focus primarily on the pure college/high school gap since the some-college category is an unstable amalgam of workers with two-year degrees or incomplete two- and four-year college enrollments. There are other striking patterns within the college group as well: as Figure A5 shows, the greatest fall in real wages between 2020 and 2023 happened for those with a MA or PhD degree.

[16] A comparison of overall earnings trends between non-college workers and college or more is reported in Figure A6.

early in the pandemic. As shown in Figure A7, wage growth has been especially pronounced in these occupations since 2020. By comparison, occupations that specialize in non-routine cognitive tasks, which are typically highly-educated and highly-paid, saw the smallest real wage growth post-pandemic. This pattern is a striking departure from the occupational wage changes during much of the post-1980 period (Acemoglu and Autor, 2011).

Figure 12 reports real wage trends by sex and race, in panels A and B respectively. Prior to the pandemic, wage trends were closely comparable between men and women. After 2020, the gender wage gap fell modestly but persistently. Similarly, the earnings gap between Black and Hispanic workers relative to non-Hispanic white workers closed by several percentage points following the pandemic. As with the gender wage differentials, there was no clear trend in racial earnings gap prior to the pandemic. Taking a longer historical sweep, Figure 13 shows that the white/Black wage differential expanded nearly continuously between 1980 and 2018, from 17 to 26 log points, then plummeted by a remarkable 8 log points over the subsequent 5 years. The trend in the earnings differential between non-Hispanic whites and Hispanics was equally pronounced: after expanding by 15 log points between 1980 and 2018, this gap compressed by approximately 6 log points between 2018 and 2023. (We use 5-year averages prior to 2015 due to small CPS sample sizes.) 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 gaps, or if a distinct, race-specific component remains (Bayer and Charles, 2018).

3.4 The contribution of the minimum wage

As highlighted in the Introduction, pre-pandemic wage compression was almost entirely confined to minimum wage states, i.e., states with statutory minimum wages exceeding the federal level. By contrast, post-pandemic compression was equally evident in minimum wage and non-minimum wage states. Here we corroborate that observation by directly estimating the effect of minimum wage changes on state-level wage compression in the pre- and post-pandemic periods. We estimate the following specification:

$$\Delta \ln W_{p,s,t} = \beta_0 + \beta_1 \Delta \ln MW_{s,t} + \epsilon_{s,t}, \tag{4}$$

where $\Delta \ln W_{p,s,t}$ is the change in the log real wage at percentile or wage ratio p in state s from year $t-4$ to t. In the upper panel of Figure 14, we plot and report estimates from these bivariate regressions of state level changes in the log of the real 10^{th} wage percentile, the log 10/50 wage ratio, and the log 50/90 wage ratio on state level changes in log minimum wages, $\Delta \ln MW_{s,t}$. During the pre-pandemic years of 2015–2019, we detect a sizable, well-estimated elasticity of the lower-tail of state wage distributions with respect to real minimum wage changes: each log point increase in the minimum wage predicts a 0.41 log point rise in the 10^{th} percentile (SE = 0.06) and a 0.47 log point compression of the 10/50 ratio (SE = 0.07).

Repeating this exercise for the years 2020–2023, panel B of Figure 14 shows that these elasticities are substantially attenuated during and after the pandemic. The coefficient for the 10^{th} percentile is 0.13 (SE = 0.07), 32% as large as in the four prior years, while the coefficient for the log 10/50 ratio is 0.12 (SE = 0.09), one-quarter the size of the pre-pandemic coefficient.[17]

The Figure 14 estimates establish three regularities. First, the evident wage compression in the pre-pandemic period was substantially related to rising minimum wages. Second, the compression in the-post pandemic period was much broader-based and generally not strongly correlated with state minimum wage policies. (This is seen in the shallower slopes *and* higher intercepts in the regressions for the 10/50 gap in the pre- versus post-pandemic periods.) Finally, and reassuringly, there is no association between the minimum wage and the log 50/90 wage ratio in either period (rightmost panels of Figure 14), passing an important falsification test used in earlier literature on the minimum wage's impact on inequality (Autor et al., 2016b).

This rapid post-pandemic fall in low-wage employment in both minimum and non-minimum wage states is further documented in Figure 15. Here, low-wage workers are those who earn less than two-thirds of their state's median wage (as per OECD convention for country-level comparisons). The share of low-wage jobs workers rose steeply from 21% to 25% in the early 1980s. It then declined in the 1990s, likely due to a combination of federal minimum wage increases (which were particularly binding in non-minimum wage states) and tightening labor markets between 1995 and 2000. The low-wage share then rose continuously from 2000 to its 2013 peak in the aftermath of the Great Recession, then declined between 2013 and 2020. As evident from the figure, this decline was largely concentrated in minimum wage states, many of which implemented or raised minimum wage floors in these years. The onset of the pandemic marks a sharp inflection point: the low-wage share dropped from 23% to 19% in non-minimum wage states, and from 24% to 21% in minimum wage states. Overall, the low-wage worker share in 2023 was beneath its previous low 43 years earlier in 1980.

3.5 Shifting labor force composition during the pandemic

Due to sharp fluctuations in employment surrounding the Covid-19 pandemic, it is plausible that observed wage trends in this period may in part be driven by shifts in worker composition rather than changes in the underlying wage structure. The role of such compositional shifts when measuring wage pro-cyclicality has long been recognized (Solon et al., 1994). Composition bias is likely to be particularly pronounced among low-wage workers during the pandemic since they had the largest employment fall and rebound.

The fourth panel of Figure A3 reports trends in real wages at the 10^{th}, 50^{th}, and 90^{th} percentiles, where compositional shifts are accounted for by reweighting each month's sample to the character-

[17] This approach of using continuous treatment variation assumes a 'strong form' of the parallel trends assumption that places limits on the nature of heterogeneity of the treatment effect (Callaway et al., 2024). Results are similar, however, if we instead estimate the average treatment effect using only a binary treatment indicator. This 'binarized' approach avoids comparing units by treatment intensity and restricts identifying variation only between treated and untreated units. We convert this estimate into a minimum wage elasticity by using change in log minimum wage as a 'first stage' outcome and the binary treatment as an instrument. The 2SLS estimate in the binarized case is 0.46 in the pre-pandemic period and 0.04 in the post-pandemic period.

istics of the workforce in the first quarter of 2020.[18] This exercise yields three results. First, the sharp rise in real wages during the early months of the pandemic in 2020 (and the subsequent fall) is attenuated when reweighting, consistent with substantial compositional changes during the early pandemic. Second, the post-2020 wage compression is actually slightly *larger* when adjusting for composition: the 90/10 gap closes by 9.3 percentage points between 2020 and 2023 when adjusting for composition versus 7.7 percent otherwise. Third, adjusting for composition indicates somewhat greater lower-tail wage compression prior to the pandemic. But when the reweighted data are further split among minimum wage and non-minimum wage states (Figure A8), we continue to find that the pre-pandemic compression is confined to minimum wage states. Finally, composition adjustment does not substantially alter the findings on pay growth along the wage distribution, as shown by a comparison of Figures A9 and 8. These findings rule out a major role for changing demographic composition in driving wage dynamics after 2020, except for a transitory impact during the height of the pandemic. Our inferential analysis that follows uses standard CPS sampling weights.

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

What explains the substantial real earnings growth since 2020 at the bottom of the distribution? We focus on mechanisms in this section. Guided by the theoretical model in Section 2, we consider four margins of adjustment that inform whether the recent wage compression can be fully understood as a shift between two competitive equilibria or whether, instead, an imperfectly competitive interpretation is warranted, specifically, a *tightening* of competitive conditions in a frictional labor market. This imperfectly competitive model predicts:

i. A rise in employment-to-employment (EE) transitions among low-wage workers, and an accompanying rise in wages among groups with rising transition rates (section 4.1)
ii. A rise in the elasticity of quits with respect to (low) wages, and a fall in the variance of log wages among workers with observably similar skills (section 4.2)
iii. A reallocation of employment from low-wage to high-wage firms and sectors (section 4.3)
iv. A concentration of wage gains among job-movers versus job-stayers (section 4.3)

4.1 Tightness and wage growth

Measuring tightness

The conceptual model above underscores that a suitable measure of tightness should incorporate both the unemployment rate and the job-to-job (EE) transition rate. We implement such a measure

[18]Variables used for this inverse probability reweighting, estimated with a probit model, include indicators for six age groups, five education categories, five racial categories, Hispanic ethnicity, gender, nativity (US- vs. foreign-born), and region.

using state-level EE separation rates from the LEHD/J2J data—paired with state-level unemployment rates from the Local Area Unemployment Statistics (LAUS). These administrative sources offer greater precision than is attainable with CPS survey-based estimates. To efficiently combine the EE separation and unemployment rate variables, we standardize both, reverse the sign of the unemployment rate, and take the average of the two. We discuss trends in these measures before turning to wage growth estimates.

Panel A of Figure 16 reports quarterly EE separation rates from the LEHD. Between 2017 and 2019, prior to the pandemic, quarterly EE separation rates averaged 3.5 to 4.3. Exiting the pandemic in the first quarter of 2021, the EE separation rate was slightly below its pre-pandemic average. Over the next two quarters, however, it rose to a full percentage point above its pre-pandemic level and remained elevated through the third quarter of 2022. It returned to pre-pandemic levels in the fourth quarter of 2022 and first quarter of 2023.

Panel B of Figure 16 complements this evidence by using the Job Openings and Labor Turnover Survey (JOLTS) data to track voluntary monthly separation rates (rather than quarterly, as in the LEHD) by sector for years 2015–2023. Panel C additionally shows that the sharp post-pandemic rise in job separations is most pronounced in the low-wage sectors of Leisure and hospitality and Retail trade, both of which employ a disproportionate share of young and less-educated workers. (We calculate that workers under age 40 with no more than a high-school degree comprised 36% of the Leisure and hospitality industry during 2015 through 2023, as compared to 21% of the overall workforce.)

To corroborate that the CPS tracks these administrative data sources, panel D of Figure 16 plots three-month moving averages of monthly EE separation rates for the full CPS sample of employed working-age adults. In the CPS, we classify workers as having made an EE transition if they are (a) employed in two consecutive survey months and (b) report having changed employers or primary jobs between those months. The CPS data replicate the key patterns in the administrative LEHD data, showing a rise in EE separations in 2021–2022 followed by a return to pre-pandemic levels in late 2022 and early 2023.

A virtue of the CPS is that it also enables estimation of EE separation rates by education and age. Figure 17 reports these estimates. The overall increase in EE transitions in Figure 16 is driven primarily by increased transitions among young, high-school workers. The monthly EE separation rate among this group rose from an average of 3.2% between 2017 and 2019 to a high of 4.0% in 2021, before returning to its pre-pandemic level by 2023. There was also a smaller rise in the EE separation rate among older high-school workers. There is almost no visible rise in separations among college-educated workers.

Figure 18 shows the time path of the composite tightness measure (panel A) and its two components (panel B). Tightness, which is standardized to a cross-sectional variance of one for the years 2015–2019, fell by roughly 4.5 standard deviations at the outset of the pandemic but rebounded quickly, rising above pre-pandemic levels throughout 2022. The evolution of tightness reflects movements in both unemployment and EE transitions: the national unemployment rate, after spiking in 2020, returned to its low pre-pandemic level by 2022, while the EE separation rate remained substantially elevated relative to the pre-pandemic baseline. As documented in Figure 19, the labor market was generally tighter in low population density states, such as North Dakota and Idaho, and was relatively slack in states that maintained prolonged pandemic lockdowns, including Massachusetts, New York, and California.

Estimating state-level wage-Phillips curves

In the job-ladder model, rising job-to-job separation rates signal an overall tightening of the labor market, enabling workers to move from lower- to higher- (residual) wage employers. Our first step in exploring this prediction is to estimate the state-level relationship between tightness and wage growth, or the the wage-Phillips curve.

We estimate this relationship by aggregating wage observations into half-year intervals t from 2015 to the 1^{st} half of 2023 and fitting the following equation:

$$\Delta \ln W_{s,t} = \beta \left(\text{Tight}_{s,t-1}\right) + X'_{s,t}\gamma + \alpha_t + \delta_s + e_{s,t}. \tag{5}$$

Here, $\Delta \ln W_{s,t}$ is the change in the average log wage in state s between $t-1$ and t, and the vectors α_t and δ_s contain time effects (in six-month increments) and state fixed effects, respectively. The explanatory variable of interest, $\text{Tight}_{s,t-1}$, is state-level labor market tightness. The coefficient β estimates the relationship between the *level* of tightness at time $t-1$ and the *change* in state-level annualized log wages between $t-1$ and t. To translate β into an annual measure, the outcome variable, the semi-annual wage change, is doubled. Additional specifications include as controls semi-annual state-level demographic shares by education, age, race, and ethnic groups. Goda and Soltas (2022) show that long Covid-19 illness reduced labor supply, which we control for using state-specific Covid-19 death rates (from CDC (2020)). Our most demanding specification further controls for the contemporaneous change in the log state-level minimum wage between $t-1$ and t. Standard errors are clustered at the state level.[19]

Estimates of equation (5) are reported in Tables 1a and 1b. The first three specifications in each panel report pooled estimates for the full 2015–2023 period (excluding 2020). In the next three specifications, the coefficient of interest is allowed to take separate slopes in 2015–2019 and 2021–2023. The first row of panel A demonstrates that wage growth was faster in tighter local labor markets between 2015 and 2023. The slope of 0.026 (SE = 0.013) indicates that a one-standard deviation increase in tightness predicts 2.6% additional annual wage growth (column 1). Controlling for worker demographics (column 2) and contemporaneous changes in state-level minimum wages

[19] Our approach to estimating state 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. Two differences are: the tightness measure applied here incorporates both state-level EE separation rates and state-level unemployment rates; and while Katz and Krueger (1999) estimate an expectations-augmented Phillips curve by imposing an estimated *price*-coefficient in the wage-Phillips regression, our estimate directly regresses wage changes on labor market tightness. This direct approach proves important because tightness appears to disproportionately affect wage growth among low-wage workers, as our simple model predicts.

(column 3) does not affect this inference.

When the wage-Phillips coefficient is allowed to take distinct slopes during 2015–2019 and 2021–2023 in columns 4 – 6, we find that the wage-Phillips relationship is driven by post-pandemic variation. Estimates of β are positive but insignificant for 2015–2019. They are substantially larger and more precisely estimated for 2021–2023. In the most demanding specification (column 6), the well-determined slope of 0.029 (SE = 0.014) implies that a one-standard deviation increase in tightness predicts additional annual wage growth of 2.9% over the post-pandemic period. The cross-state differences in tightness depicted in Figure 19 imply economically meaningful differences in wage growth. Panel A of Figure 20 plots the post-pandemic estimates (column 5) as a binned scatterplot using our baseline specification.

Panel B of Figure 20 extends this exercise by reporting wage-Phillips curve estimates during the post-pandemic period separately for the 1^{st} (bottom) quartile of the wage distribution and for the remaining three quartiles combined. The slope estimate for the bottom-quartile wage-Phillips curve is more than twice as steep as the corresponding estimate for the combined upper-three quartiles: 0.051 (SE = 0.016) versus 0.022 (SE = 0.017), respectively. This pattern constitutes a first piece of evidence that the compression of the lower half of the wage distribution documented in Section 3 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 0.052 (SE = 0.015) while the slope for the complementary group of workers is shallow and statistically insignificant at 0.010 (SE = 0.018). The corresponding regression estimates for these figures, as well as several variants of each specification, are reported in Tables 1a and 1b.

The onset of the pandemic accelerated a compression in wages between Black and Hispanic workers relative to white workers, as shown previously in Figure 12. To gauge whether the tightening labor market directly contributes to this compression, the bottom panel of Table 1b reports state-level wage-Phillips curve models estimated separately for Black and Hispanic workers (pooled for precision) and white workers. Within the bounds of available precision, these wage-Phillips curves appear steeper for Black and Hispanic workers overall, and steeper still during the pandemic, as shown in our baseline specification plotted in panel D of Figure 20. In the final column of estimates in Table 1b, for our most saturated specification, the wage-Phillips curve for Black and Hispanic workers is estimated at 0.040 (SE = 0.015) during 2021–2023 relative to 0.014 (SE = 0.025) during 2015–2019. For non-Hispanic white workers, these coefficients are imprecisely estimated despite the larger sample size and are smaller relative to non-white workers after 2020: 0.017 (SE = 0.017) during 2021–2023, and 0.023 (SE = 0.023) during 2015–2019. The greater responsiveness of non-white workers' wages to labor market conditions is consistent with classic work by Freeman (1990), who finds that pockets of tight labor markets raised the wages of young Black workers during the 1980s.

How unusual is the steepening of the wage-Phillips curve in the post-pandemic period? Figure 21 reports the time-series of wage-Phillips curves for the 43 year period of 1980–2023, where the coefficient of interest is allowed to vary at pooled five-year intervals between 1980 and 2019, and again for the 2021–2023 interval. Unemployment serves as the sole tightness measure for this exercise since consistent EE separation data are not available for this four-decade interval. The three series in the figure correspond to Phillips curves coefficients for all workers, for first quartile earners, and for high-school educated workers under age 40. For the two low-wage groups, the wage-Phillips slope is typically at least as large or larger than the corresponding estimate for all workers—meaning tightness tends to be compressive. While the wage-tightness coefficient for the two low-wage groups is weakly pro-cyclical, there is also a secular component: this slope gradually attenuates from 1990–1994 through 2010–2014, then rebounds between 2015–2019 following the slow recovery from the Great Recession. Importantly, during the post-pandemic interval, the wage-Phillips slope more than doubles for all groups.

Table A2 fits a variant of our baseline wage-Phillips curve specification (equation 5) to these 43 years of data, where again unemployment serves as the sole tightness measure. The wage-Phillips relationship is statistically significant for all three groups (overall, quartile 1, and high-school under-40), both before and after the pandemic. The wage-Phillips curve roughly doubles for the overall workforce after the pandemic, and it roughly triples for quartile 1 and high-school under-40 wages. In the final column of estimates, the wage-Phillips slope for 1st quartile wages rises from 0.009 (SE = 0.001) during 1980–2019 to 0.029 (SE = 0.006) during 2021–2023. For high-school under-40 workers, the analogous values are 0.010 (SE = 0.001) and 0.033 (SE = 0.007). These increases are statistically significant, with p-values of 0.003 and 0.001 for bottom quartile workers and high-school under-40 workers, respectively.

This recent steepening of the wage-Phillips curve is consistent with the non-linear Phillips curve proposed by Benigno and Eggertsson (2023) and Crust et al. (2023). Similarly, Burya et al. (2023) show that the wage-Phillips curve is weaker in areas where firms have more market power, underscoring that the relationship between wage growth and tightness is stronger when the labor market is more competitive (see also Azar et al. (2019)).

Does work-from-home mask wage decompression?

The amenity value of the documented rise in work-from-home (WFH) arrangements is not reflected in our wage estimates, as noted above. Since WFH is disproportionately prevalent among technical, managerial and professional occupations that tend to pay high wages (Barrero et al., 2023), the trends we report in wage compression might potentially overstate the extent of compression in full *compensation*. Moreover, if a tightening labor market spurs employers to offer additional compensation to high-wage workers in the form of WFH, our wage-Phillips curve estimates will overstate the extent of compression.

We explore the importance of WFH to our findings in two steps. We first test whether labor market tightening raises the incidence of WFH by estimating the semi-elasticity of WFH shares with respect to labor market tightness. Harnessing data on remote work vacancies at the state-by-time level (at six month frequencies) from Hansen et al. (2023), we estimate in Figure 22 and

Table A3 that a one-standard deviation increase in tightness predicts a 0.020 (SE = 0.007) log point increase in the state WFH rate from 2020 forward. Thus, WFH responds positively to labor market tightness, as conjectured.

What does this relationship imply about compression? In a second step, we construct a measure of total compensation inclusive of the amenity value of WFH. Barrero et al. (2023) report that workers are on average willing to pay eight percent of wages for the option to work from home. We make the conservative assumption that the entire amenity value of WFH accrues to workers in the top quartile of the wage distribution, thus maximizing the potential offset that WFH makes to observed wage compression. For each worker in the top quartile of their state-by-month wage distribution, we adjust their wage upward to incorporate the WFH amenity: $\tilde{w}_{i,s,t} = w_{i,s,t} + \mathbb{1}\left[w_{i,s,t} \geq w_{s,t}^{p75}\right] \cdot (\text{WFH-Share}_{s,t} \times 4 \times w_{i,s,t})$.[20] We do not perform a similar adjustment for pre-pandemic wages since WFH was rare prior to 2020 and comparable data are unavailable.

Table 2 presents WFH-adjusted wage-Phillips curves estimates of equation (5), where $w_{i,s,t}$ is replaced with $\tilde{w}_{i,s,t}$. As compared to the unadjusted estimates in Tables 1a and 1b, this WFH adjustment only minimally increases the wage-Phillips point estimate for workers at the top of the wage distribution and for workers with a Bachelor's degree. For example, the wage-Phillips estimate for quartile 4 earnings in the WFH-adjusted series in column 1 is 0.0246 (SE = 0.0163). The comparable point estimate in Table 1a is 0.0236 (SE = 0.0161). This adjustment has such a modest impact because most of the rise in WFH is common across states and hence not directly associated with market tightness.

4.2 Rising quits from low-wage jobs and falling wage dispersion

The job-ladder model predicts that a tightening labor market will spur a rise in the the sensitivity of job-to-job separations to wage levels (i.e., a higher quit elasticity), 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. The person-level wage residual calculated from cross-sectional data, however, incorporates both firm-specific rents (the explanatory variable of interest) and unobserved worker quality—a confound that will attenuate separation elasticity estimates based on equation (6). Cognizant of this issue, we use two alternative approaches to identify workers who are low paid.[21]

The first approach estimates the response of worker separations to workers' own wage earnings, purged of the influence of standard Mincerian covariates (in effect, their wage residuals). We estimate the EE separation elasticity with the following model:

$$\Delta J_{i,t}^{k} = \alpha_t + \beta \ln w_{i,t-1} + X_{i,t-1}^{\prime}\lambda + \epsilon_{i,t}, \tag{6}$$

[20] The amenity adjustment is multiplied by four since it is imputed to only one-fourth of workers (i.e., the top quartile). We cap the WFH share at 25% in each state and time period, thus winsorizing outliers.

[21] 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 the Current Population Survey, which is based on household surveys.

The dependent variable, ΔJ^k, is an indicator for whether worker i made a job-to-job separation during the past 3-months ($k = 3$). The key independent variable, $\ln w_{i,t-1}$, is the log of hourly earnings reported by the respondent in MIS 4, prior to any possible employment transition occurring over the next 12 months. The covariate vector $X_{i,t-1}$ 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+). Because we anticipate a *rise* in the magnitude of the separation elasticity from a tightening labor market, we fit (6) separately for 2015–2019 and 2021–2023.

The disparate timing of the job change questions in the CPS complicates estimation of own-wage separation elasticities. Specifically, job changes occurring in the 8 month interval between a worker's 4th and 5th month in the CPS rotation, as well as those occurring as they reenter the rotation in MIS 5, are not observed. We code $\Delta J_{i,t}^{k=3} = 1$ if worker i reported a job-to-job separation in MIS 6 through 8 (the months in which job-to-job separations are directly reported by the respondent) while excluding observations where workers have different industry *and* occupation affiliations between MIS 4 and 5, since these likely signal an intervening job change. Accordingly, β estimates the hazard of quitting a job in MIS 6, 7, or 8 conditional on being *unlikely* to have changed jobs between MIS 4 and 5.[22] To obtain the elasticities corresponding to equation (6), we divide $\widehat{\beta}$ by the mean separation rate in the estimation sample, $E\left[\Delta J_{i,t}^k\right]$.

Estimates for the wage separation elasticity are reported in Tables 3a and 3b. The first column of each table corresponds to estimates of equation (6) for 3-month job changes. With a p-value of 0.975, we detect no statistically meaningful change in the *overall* separation elasticity between the pre (2015–2019) and post (2021–2023) periods. The point estimate of -0.27 is almost identical in both intervals and both point estimates are precisely estimated. Echoing the pattern for the state-level wage-Phillips curves, however, the separation elasticity is generally higher among the high-school under-40 group. And it increases sizably following the pandemic, rising from -0.317 (SE = 0.129) during 2015–2019 to -0.551 (SE = 0.182)) during 2021–2023, though with a p-value of 0.293, we cannot statistically distinguish these coefficients at conventional significance levels.

Our second approach uses industry wage premia (IWP) to proxy for rents paid to workers. This approach is motivated by the classic and modern evidence that observed industry wage premia reflect in part industry rents rather than exclusively unobserved skill differentials (Katz and Summers, 1989; Card et al., 2022).[23] We estimate IWPs, $\tilde{w}_j$, by fitting cross-sectional regressions of log hourly wages on gender, age, age squared, and age cubed, as well as indicators for race, ethnicity, citizenship, education, metro area status, and 3-digit industry effects. These industry effects serve as estimates of IWPs, which we use to estimate EE separation elasticities with the

[22] Using industry and occupation changes to proxy for, and condition out, likely job changes during respondents' 8 out-of-sample months between MIS 5 and 6 is clearly an imperfect fix. We are aware of no better solutions for the CPS data.

[23] 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 estimated IWPs and hence will attenuate estimates of the quit elasticity.

following equation:

$$\Delta J^k_{i,t} = \alpha_t + \beta \ln \tilde{w}_{j(i,t-1)} + X^{'}_{i,t}\lambda + \epsilon_{i,t}, \tag{7}$$

where $\tilde{w}_{j(i,t-1)}$, corresponding to the estimated IWP for worker $i^{'}s$ industry j in period $t-1$, is used in place of $w_{i,t-1}$ in equation (6). Since workers' industry affiliations are observed in each month in sample (rather than only in MIS and 8), the dependent variable in equation (7) is monthly separations ($k = 1$) rather than 3-month ($k = 3$) separations above. We cluster standard errors at the industry level. Elasticities from these specifications are obtained by dividing $\widehat{\beta}$ by $E[\Delta J_{i,t}]$, as above.

Estimates of equation (7) are reported in column 2 and 3 of Tables 3a and 3b. The first estimate, reported in column 2, detects an increase in the magnitude of the separation elasticity among young non-college workers from -0.789 (SE $= 0.137$) in the pre-pandemic period to -1.030 (SE $= 0.140$) in the post-pandemic period. Adding demographic controls in column 3 reduces the magnitude of the separation elasticities in both the pre and post periods, but the qualitative findings are unchanged. The separation elasticity among young non-college workers rises from -0.546 (SE $= 0.133$) to $-.766$ (SE $= 0.152$) from the pre- to post-pandemic period, though the change is not statistically significant (P $= 0.277$). While we again lack the statistical power to distinguish these coefficients, we note that this pattern of a higher post-pandemic EE separation elasticity is found almost entirely among the high-school under-40 group across all specifications in Tables 3a and 3b.

A further prediction of the job ladder model is that the separation elasticity should be larger (more negative) at lower wage levels, where outside offers are more likely to generate a voluntary worker move (see Section 2.1). Harnessing this prediction, we fit a non-linear version of equation (7), using a quadratic specification for the main explanatory variable, $\tilde{w}_{j(i,t-1)}$. Implied elasticities are then calculated by dividing the derivative of ΔJ with respect to $\tilde{w}_{j(i,t-1)}$ by the predicted value of ΔJ at several values of $\ln \tilde{w}$. Results are summarized in Figures 23 and 24 and are enumerated in columns 1–3 of Table 4, which reports estimated elasticities at $\ln \tilde{w} \in \{-0.3, 0.0, 0.3\}$ in both the pre- and post-pandemic period, as well as the contrast between the two.[24] Additionally, columns 4–6 of Table 4 show that these patterns are robust to the inclusion of demographic and geographic controls.

While the aggregate EE separation elasticity at its midpoint ($\tilde{w}_j = 0$) did not meaningfully change between the pre- and post-pandemic periods, this elasticity steepened at *low* wage levels ($\tilde{w}_j = -0.3$), from -1.220 (SE $= 0.240$) in the pre-pandemic period to -1.394 (SE $= -0.276$) in the post-pandemic period, as shown in Figure 23. Consistent with the array of evidence above that post-pandemic labor market tightness has been differentially important for young, non-college workers, the rise in the EE separation elasticity is concentrated among this group. Panel A of Figure 24 shows that the quit elasticity among high-school under-40 workers at the mean wage

[24] Tables A4 and A5 report the coefficients on $\tilde{w}_{j(i,t-1)}$ and its square from equation (7). Table A6 reports these estimates using a Poisson regression specification, and Table A7 reports the implied elasticities at various values of $\tilde{w}$.

premium rose in magnitude from -0.789 (SE $= 0.135$) to -1.136 (SE $= 0.128$). At $\tilde{w}_j = -0.3$, the increase was steeper still, rising from -0.637 (SE $= 0.232$) to -1.226 (SE $= 0.279$). This change is statistically significant at $p = 0.10$, as reported in Table 4. As highlighted by Table A8, this rise in the elasticity was especially pronounced until mid-2022, after which it somewhat attenuated. In contrast, there is no evident change in the quit elasticity for older high-school workers, or for four-year college grads in either age bracket. The job-to-job separation elasticity therefore rose differentially among low-wage workers employed in 'low-rent' jobs, i.e., jobs that pay particularly low wages conditional on observable worker characteristics.

A corollary to this pattern of rising quits among workers occupying 'low-rent' jobs is that frictional wage inequality among workers with similar productive capacities should be falling. We test this prediction by regressing log wages for the years 2015–2023 on a complete set of interactions between three education categories (high school or less, some college, BA+), six age categories, and indicators for each year. The explained variation from this wage regression corresponds to the standard 'between group' component of wage inequality while the residual variation corresponds to the 'within group' component (Lemieux, 2006). These components are plotted in Figure 25, with all values normalized to zero in 2019. Consistent with the wage compression seen in Figure 1 above, the variance of log wages declined in this interval. The fall of 5.5 log points on a base of 38.4 log points is economically substantial.

Relevant to the post-pandemic wage compression, two points stand out. First, overall variance fell more between 2019 and 2023 (3.4 log points) than during the prior four years of 2015–2019 (2.1 log points). Second, the acceleration after 2019 is even more pronounced for the within- than between-group component of variance. The 2.2 log point drop in within-group variance after 2019 accounts for 65% of the overall post-2019 decline of 3.4 log points. In the four years prior to the pandemic, by contrast, the within-group fall in wage variance was only 0.8 log points and accounted for less than 40% of the overall decline. Thus, consistent with the logic of the frictional search model, we find wage differences among observationally similar workers have fallen as the labor market has tightened following the pandemic.

4.3 Job change and wage growth

In the job-ladder model, job separations spike as the labor market tightens because workers transition to higher-paid jobs. This also implies that the steepening wage-Phillips Curve relationships detected in Section 4.1 are driven by wage gains among job movers. We assess this prediction in two ways. First, we test whether the rate of net worker mobility from lower-paid to higher-paid industries increased following the pandemic. Then we decompose annual wage growth between job-movers and job-stayers to analyze whether wage gains are concentrated among movers, as predicted.

Switching out of low-wage industries

To test for upward sectoral moblity, we subdivide industries into four groups based on their ranked wage premia, $Q(\tilde{w}_j)$, with each group containing 25% of employment in 2015–2019.[25] The following linear model characterizes the probability that a worker who is employed in the lower half of the industry wage premium distribution in month $t-1$ switches employers and is employed in the upper half of the distribution the following month t:

$$E\left[\mathbb{1}\left[Q(\tilde{w}_{j(i,t)}) = 3 \vee 4\right] \middle| Q(\tilde{w}_{j(i,t-1)}) = 1 \vee 2\right] = \beta_1 \mathbb{1}\left[2015 \geq t \geq 2019\right] + \beta_2 \mathbb{1}\left[2021 \geq t \geq 2023\right]. \quad (8)$$

The coefficients β_1 and β_2 in equation (8) capture the probability of upward industry mobility during years 2015–2019 and 2021–2023, respectively. We also estimate downward mobility using a variant of equation (8), where the outcome is instead $E[\mathbb{1}[Q(\tilde{w}_{j(i,t)}) = 1 \vee 2]|Q(\tilde{w}_{j(i,t-1)}) = 3 \vee 4]$.

Panel A of Figure 26 reports estimates for both the full sample of employed workers and the subsample of non-college workers under age 40.[26] Monthly movements across the two halves of the industry premium distribution are relatively infrequent. The average monthly upward transition rate is 0.52% (SE = 0.01%) during 2015–2019. This rate rises slightly to 0.54% (SE = 0.01%) during 2021–2023, an increase that is not statistically (or economically) significant. As our conceptual framework anticipates, we see a different pattern for young non-college workers. Upward industry wage mobility among high-school under-40 workers increases after the pandemic, from 0.84% (SE = 0.03%) during 2015–2019 to 1.00% (SE = 0.05%) during 2021–2023, a difference that is highly significant. 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. Panel A of Figure 26 finds no increase in downward mobility, either overall or among young non-college workers.[27]

Panel B of Figure 26 refines this exercise by analyzing movements into and out of the bottom quartile of industries (rather than the bottom half). The upward mobility hazard for moving out of

[25] As in Section 4.2, industry wage premia, $\tilde{w}_j$, are measured as industry fixed effects, obtained from a cross-section Mincerian wage regression, estimated separately by subgroup in the pre-pandemic period, 2015-2019. Industry groupings are kept constant after 2019, allowing industries in each quartile to grow or contract as a share of employment. We would ideally test if workers moved from lower- to higher-paying employers, but this is infeasible with CPS household data. We instead proxy for industry rents using IWP, which, as noted in Card et al. (2022), are an aggregation of firm premia within industries.

[26] Point estimates corresponding to panels A, B, and C of Figure 26 are reported in Tables A9a, A9b, and A9c. Our estimates for mobility *levels* likely overstate the cross-sectional frequency of upward and downward movements since measurement error in industry assignments will generate false transitions. Measurement error should not affect the estimated *change* in this frequency, however. In constructing up-down comparisons, 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 (Figure 26 panel B) or the hospitality sector (Figure 26 panel C). For the hospitality sector, $p = 0.079$ for the overall sample and $p = 0.185$ for the high-school under-40 sample.

[27] Downward mobility is not simply the mirror of upward mobility for two reasons: 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; and high-premium industries may expand and low-premium industries contract, leading to a rise in aggregate upward industry mobility.

the bottom quartile is nearly double that of the hazard out of the bottom half of the distribution. For the full population of workers, this upward mobility hazard rises modestly from pre- to post-pandemic: 0.99% (SE = 0.02%) during 2015–2019 and 1.04% (SE = 0.03%) during 2021–2023. Among young non-college workers, the gain is larger: from 1.44% (SE = 0.05%) during 2015–2019 to 1.77% (SE = 0.09%) during 2021-2023 (a highly significant rise), with no corresponding increase in downward mobility. By implication, the jump 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.

Panel C of Figure 26 provides a third perspective on sectoral mobility by charting movements in and out of the typically low-paid hospitality sector. The hospitality sector lies just below the 10^{th} percentile of the industry wage premium distribution, $\tilde{w}_j$, as calculated above. The share of workers employed in the hospitality sector falls from 8.0% in 2015–2019, to 7.4% in 2021–2023. The corresponding fall among high-school under-40 workers is 18.5% to 18.0%. Panel C shows that among the overall working population, the exit rate from hospitality rises from 1.40% (SE = 0.03%) pre-pandemic to 1.54% (SE = 0.06%) post-pandemic with a small decrease in the adjusted entry rate. Among young non-college workers, the exit rate rises further, from 1.49% (SE = 0.06%) to 1.73% (SE = 0.10%), which is again a significant contrast. This is accompanied by a small and statistically insignificant increase in the entry rate.

In short, the totality of evidence from these 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-rent industries. If workers are, indeed, reallocating to better paid jobs in the post-pandemic period, then the wage growth of job movers should be larger than the wage growth of job stayers. We test this prediction next.

The contribution of job-moving to wage compression

We now quantify the contributions of job-moving and job-staying to overall wage growth before and after the pandemic. A key challenge for this exercise, as discussed in Section 1, is that respondent-level wage changes in the CPS are observed at annual frequencies while job changes are reliably classified over only a shorter three-month interval. We address this limitation by using a quarterly measure of job change to estimate the annual job-to-job separation rate.

In order to decompose the wage change into job-mover and job-stayer components, we write the mean real wage change for a demographic group during time interval, T, as $\Delta w_T = \Delta w_T^M \delta_T + \Delta w_T^S (1 - \delta_T)$. Here, the overall within-person wage change over period T, Δw_T, is equal to the wage change among job-movers, Δw_T^M, times the estimated (12-month) job switch rate, δ_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 ($T = 1$)

and 2021–2023 ($T = 2$), using the following equation:

$$\Delta w_2 - \Delta w_1 = \underbrace{\left(\Delta w_2^M - \Delta w_1^M\right)\delta_1}_{\text{Movers}} + \underbrace{\left(\Delta w_2^S - \Delta w_1^S\right)(1-\delta_1)}_{\text{Stayers}} + \underbrace{(\delta_1 - \delta_2)\left(\Delta w_2^M - \Delta w_2^S\right)}_{\text{Switch rate}}. \quad (9)$$

Given estimates of $\left\{\Delta w_T^M, \Delta w_T^S, \delta_T\right\}$, equation (9) apportions the observed difference in wage growth across time periods for a given demographic group into three components: the difference in the wage growth of job-movers scaled by the switch rate; the difference in the wage growth of job-stayers scaled by the complement of the switch rate; and the difference in the switch rate scaled by the difference between the mover and stayer wage growth.

Implementing equation (9) requires estimates of wage changes among movers, wage changes among stayers, and annual move rates. Only the first of these three terms is directly observed, Δw_T^M. We measure Δw_T^M as the annual real wage change among individuals who reported switching jobs in MIS 6, 7, or 8.[28] Estimating wage changes for stayers, Δw_T^S, is complicated by the fact that workers who did not move during MIS 6–8 may nevertheless have moved in the three prior quarters. We infer Δw_T^S by combining estimates of the average annual wage change among all workers (Δw_T), the observed wage change among movers (Δw_T^M), and the annual probability of moving (δ_T). While this latter term is unobserved, it can be calculated from the (observed) quarterly move rate.

Define $\delta_T^3 \in \{0, 1\}$ as an indicator equal to 1 if a worker switches jobs in MIS 6, 7 or 8, and equal to 0 otherwise. Assuming a constant quarterly hazard, the annual switch rate can then be written as $\Pr[\delta_T = 1] = \left[1 - (1 - \Pr[\delta_T^3 = 1])^4\right]$. Estimates of all the terms of equation (9) are reported in panels A and B of Table 5 for young non-college workers, as well as their complementary group. Quarterly separation rates, $\Pr\left[\delta_T^3\right] = 1$, are reported in row 1 of panel A. Estimates for the annual switch probability, $\Pr\left[\delta_T = 1\right]$ are reported in row 2 of panel A.

The annual probability of changing jobs increases by over two percentage points for young non-college workers (from 27.23% to 29.66%) between 2015–2019 and 2019–2023, while it increases by less than a percentage point (from 19.82% to 20.48%) for all other workers. Average annual wage changes in each period among workers—both overall (Δw_T) and by job-change status (Δw_T^M and Δw_T^S)—are reported in panel B of Table 5. Here we can see that young non-college workers who switched jobs are the only group to experience annual real wage growth between the 2015–2019 and 2021–2023.

Plugging in estimates from panels A and B to the wage decomposition in equation (9), panel C of Table 5 reports estimates of the components in equation (9) for young high-school workers and for all other workers. Figure 27 summarizes these results. The first set of bars in panel A shows the change in average annual wage growth among CPS respondents between 2015–2019 and 2021–2023. For high-school under-40 workers, this rate is 0.94 log points lower in 2021–2023 than 2015–2019, reflecting the toll of rising inflation. This slowdown is more than fully accounted for by the fall

[28] We focus on MIS 6, 7, 8 because these are the only months in which we observe job change in between our two wage observations in MIS 4 and 8.

in the average wages of *job-stayers* (the 2^{nd} set of bars), which accounted for -1.93 log points of the overall decline. By contrast, and consistent with the predictions of the job ladder model, the average wage change among movers and the job change rate rises among young non-college workers in a tightening labor market (the 3^{rd} and 4^{th} sets of bars). These two components contributed 0.85 log points and 0.14 log points, respectively, to net wage changes among high-school under-40 workers.

The rising gains to job mobility are concentrated among young non-college workers. Among all other workers (light colored bars), the overall post-pandemic decline in wage growth of 2.25 log points is due to a fall of among stayers of 1.80 log points, a fall among movers of 0.47 log points, and a small offsetting contribution of 0.02 log points from a rising move rate.

Panel B of Figure 27 documents the contribution of job-switching to wage *compression* by contrasting each wage decomposition component among young high-school workers with the corresponding value for all other workers. Annual wage growth was 1.31 log points greater among high-school under-40 workers than among all other workers during 2021–2023 relative to 2015–2019. This difference is fully is accounted for by greater wage gains among high-school under-40 job-movers relative to all other job movers (a contrast of 1.32 log points). The slight relative fall in wage growth of -0.13 log points among high-school under-40 stayers relative to all other workers is offset by a gain of 0.12 log points due to higher mobility. In summary, post-pandemic wage growth among high-school under-40 workers accrues exclusively to movers. Contrasting the gains of high-school under-40 workers with all other workers further underscores the centrality of job moves to post-pandemic wage growth among this young, non-college group.

In many standard macroeconomic models of wage setting (Blanchard and Galí, 2010; Mortensen and Pissarides, 1994), rising labor market tightness leads to higher wages by amplifying workers' bargaining power, enabling them to renegotiate for higher wages. Strikingly, a large part of the wage growth we see after the pandemic is not of this variety. Instead, wage growth is concentrated among movers and is generally negative (in real terms) among stayers. The dependence of wage growth on job change indicates that job search and frictional wage dispersion are key mediators between rising labor market competition and resulting wage growth.

5 Tightness, regional inflation, and real wage growth

The tightening post-pandemic labor market spurred a burst of labor market competition that boosted real wages among low-paid workers and compressed the lower half of the wage distribution. Simultaneously, price inflation spiked to a level not seen since the 1970s and early 1980s, with the headline CPI-U reaching a 12-month peak of 9.0 percent in June 2022. Section 4.1 above documented that wage growth and wage compression were especially rapid in states with tighter post-pandemic labor markets. If this same labor market tightness contributed to localized price inflation, our wage-Phillips curve analysis would overstate real wage gains in tight labor markets since we have so far assumed that price inflation is uniform nationally. We address this issue here by using regional price index data to jointly assess the contribution of tightness to price growth and (regionally adjusted) real wage growth—both for the workforce overall and for demographic subgroups.

A key challenge in this endeavour is that BLS does not provide state-level CPI measures. To address this limitation, we construct a state-level measure as follows: for the main metro areas in each state, we use the 23 Core Base Statistical Area (CBSA)-level CPI-U deflators; for other (non-main) metro areas within each state, we use the state average of CBSA-level CPI-U deflators within each state; for the remaining areas, we use Census division-level CPI-U deflators. BLS does not provide deflators for CBSAs and Census divisions prior to 2018. To extend the regional price series back to 2015, we use state-level inflation measures for years 2015–2017 from Hazell et al. (2022), who calculate inflation rate series separately for shelter, and for CPI excluding shelter. We aggregate these two series into an overall headline CPI using weights of 0.362 and 0.638 for the shelter and non-shelter components respectively.[29] The seam between the BLS and Hazell et al. (2022) series inhibits consistent inflation measurement between the second half of 2017 and the first half of 2018, and these periods are omitted accordingly. For completeness, we additionally report companion estimates for this outcome in Table A10, where we find smaller price-Phillips relationships than in our primary estimates below.

Analogous to the wage-Phillips curve estimates from equation (5), we fit the following price-Phillips curve regression for the log *price* change, $\Delta \ln P_{s,t}$, in state s during the half-year period from $t-1$ to t. As before, the outcome variable (semi-annual log price change) is doubled so that β can be interpreted as an annualized relationship.

$$\Delta \ln P_{s,t} = \beta \left(\text{Tight}_{s,t-1}\right) + X_{s,t}^{'}\gamma + \alpha_t + \delta_s + e_{s,t}. \tag{10}$$

Figure 28 plots the results of this exercise for the post-pandemic period, with detailed coefficients for the full 2015–2023 period and as well as for 2015–2019 and 2021–2023 separately are reported in Table 6. The first bar of the figure reports a regional price-Phillips curve of 0.0155 (SE = 0.003), implying that one additional standard deviation of tightness predicts 1.6% of additional inflation (see column A4 of Table 6). The second bar reports the corresponding *nominal* wage-Phillips curve for the full workforce, which is estimated using the identical sample (see panel B of Table 6).[30] This wage-Phillips slope of 0.034 (SE = 0.16) is roughly twice as large as the corresponding price-Phillips curve, indicating that tighter regional labor markets yield higher *real* wages, net of their effect on regional prices.

The final five bars of Figure 28 report nominal wage-Phillips curves estimates separately for high-school under-40 workers as well as for each quartile of the wage distribution. The wage-Phillips curve slope estimates of 0.062 (SE = 0.017) for high-school under-40 workers and 0.060 (SE = 0.014)

[29] We weigh shelter by its relative importance in the CPI as of December 2023 (https://www.bls.gov/cpi/tables/relative-importance/2023.htm)

[30] The only difference between the wage-Phillips curve estimates in Section 4.1 and the estimates in Panel B of Table 6 is that the latter estimate excludes the second half of 2017 and the first half of 2018 from the analysis in order to be consistent with our price-Phillips curve specification.

for the 1st wage quartile are about twice as steep as the slope for the full workforce, as well as for the bottom three quartiles. This contrast highlights that tightness-fueled wage gains accrue disproportionately to low-wage workers. This is confirmed in panel C of Table 6, which reports estimates of the effect of tightness on real wage growth for young, non-college workers and for each wage quantile net of regional price changes. Figure 29 shows the distributional implications of these adjustments by reporting estimates of annual real hourly wage growth by percentile for the 41, 24, and 12 months preceding May 2023, where wages are deflated by regional price indices. After accounting for the positive correlation between wage and price changes across regional labor markets, we estimate that between January 2020 and June 2023, real wage growth averages approximately 2.1 percent annually (7.2 percent cumulatively) at the 10th percentile and 1.1 percent annually (3.7 percent cumulatively) at the 25th percentile. On the other hand, average real wage growth at the 90th percentile falls negligibly by 0.1 percent annually (0.5 percent cumulatively). The cumulative fall in the 90/10 ratio between January 2020 and June 2023 using regional price deflation is 7.6 percentage points. Thus, our use of regional price deflators in Figure 29 negligibly changes the magnitude of the compression compared to using a national price deflator, as in Figure 8 above, which suggests a fall of 8.0 percentage points.

One final threat to the interpretation of these conclusions is the potential role of inflation inequality. If post-pandemic price inflation were differentially rapid for low-paid workers and low-income households—perhaps because they spend a disproportionate share of income on pandemic-afflicted goods or services—Figure 29 would still overstate real wage gains among low-earners. Two pieces of evidence suggest that this is not a major factor. First, recent calculations by Klick and Stockburger (2021) and updated by BLS find that, while *aggregate* post-pandemic inflation was substantial, inflation *inequality* was modest. BLS estimates that between January 2020 and June 2023, prices rose most for the lowest quintile of households (by 19.0%) and rose least for the highest quintile of households (by 17.4%). This cumulative inflation differential of 1.6% over three and a half years is much smaller than the estimated compression over the same period, which was around 8% (Figure 29). Second, item-level expenditure analysis by Jaravel (2024) finds that inflation was modestly *lower* for the bottom quartile of households relative to the next two higher quartiles between May 2020 and May 2022. This is because bottom quartile households spend a relatively small share of income on motor vehicles and gasoline, both of which saw rapid pandemic-era inflation. This inflation differential was however less than 2 percentage points over that two year period—and contrary to estimates from Klick and Stockburger (2021), it favored low-income households. Applying this adjustment would modestly strengthen our conclusion that low-paid workers saw differentially rapid real wage growth in the post-pandemic economy.

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 both between- and within-group differentials in wages, and reversed around one-third of the cumulative rise in 90/10 log wage inequality since 1980. Measured by the fall in the 90/10 log wage ratio, the Unexpected Compression was around one-third as large as the Great Compression of the 1940s. More than the past two episodes, the recent compression was concentrated in the bottom half of the wage distribution and was driven by particularly rapid wage growth among workers under 40 years of age and without a college degree.

The profound shift in US labor market conditions in the aftermath of the pandemic is seen most clearly in the rise of the wage-separation (quit) elasticity among young non-college workers following the pandemic. In the canonical competitive setting, this quit elasticity is infinite, giving rise to a law of one price for workers of the same skill level. In contrast, in the frictional model, this elasticity is finite, which allows between-firm wage differentials for workers with comparable productivity to persist in equilibrium. As the market tightens, however, the frictions that support these differentials attenuate. Consequently, this imperfectly competitive model predicts that in a tighter labor market, low-wage workers will reallocate from lower-wage to higher-wage firms and sectors, wage inequality will decrease among workers with similar productive characteristics, and wage gains will accrue primarily to job-changers rather than job-stayers. We test and affirm each of these predictions. Further, we show that the wage gains spurred by these reallocative forces are only modestly offset by faster price growth in regions with higher labor market tightness. Thus, the post-pandemic wage compression reduced wage inequality in substantial part by 'leveling up' the wages of low earners rather than merely eroding the wages of high earners.

Our analysis has a number of limitations. A first is that the relatively small size of the monthly CPS sample provides merely adequate precision for testing some of the key empirical implications of the imperfectly competitive model. Second, some analyses of worker mobility are constrained by our inability to reliably track job changes over the course of a year, requiring us to use industry and occupational changes as proxies. Finally, our evidence on the rise of the quit elasticity relies on using either own-wage residuals or estimated industry wage premia to proxy for the firm component of wages. A more refined measure of the quit elasticity would employ directly estimate establishment-level measures of wage premia that are purged of workers' own skill levels (or fixed effects). Future work in this vein will benefit from administrative data that can more precisely pin down the mechanisms and magnitudes that are identified by the job-ladder model and illustrated by our empirical work.

Though our analysis has focused on how macroeconomic shocks that amplify labor market tightness yield aggregate wage compression and real wage gains among low-paid workers, the mediating channel of operation—more strenuous labor market competition—has implications that extend beyond macroeconomic policies. Recent studies document that the transmission of labor market tightness to wage growth, and of minimum wage hikes to employment changes, depend upon the extent of firm market power (Burya et al., 2023; Azar et al., 2019). More ambitiously, Stansbury and Summers (2020) posit that falling worker power can explain the constellation of sluggish wage growth, a falling labor share, and rising profitability and market valuations of US businesses in the post-1980 period. The results in this paper support the inference that heightened labor market competition has both distributional and allocative effects—spurring wage gains among low-paid workers while simultaneously reallocating such workers from lower-paying, less productive employers to higher-paying, more productive employers. We infer that competition policy—alongside macroeconomic policy—may play a critical role in shaping the distributional and allocative consequences of economic growth.

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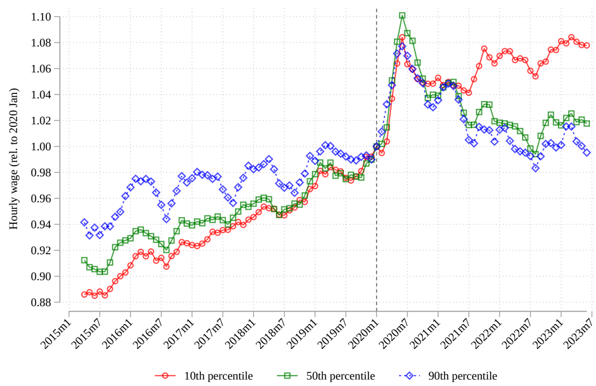
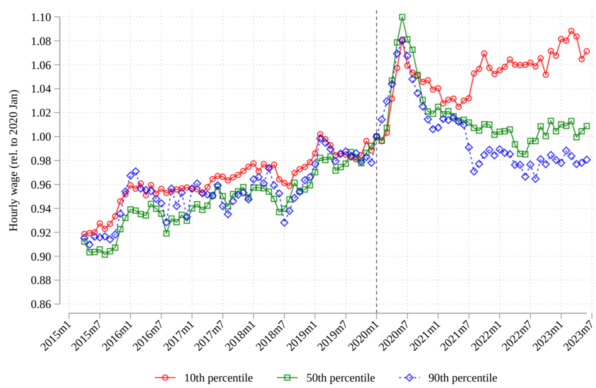
Figure 1: Trends in Real Hourly Wages by Quantile and State Minimum Wage Status 2015 – 2023, Relative to January 2020

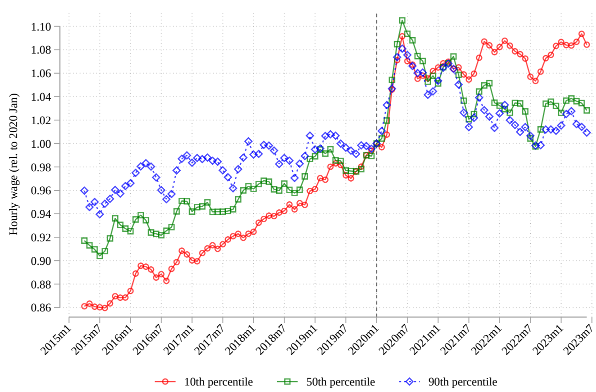
A. Overall



B. Federal or no minimum wage

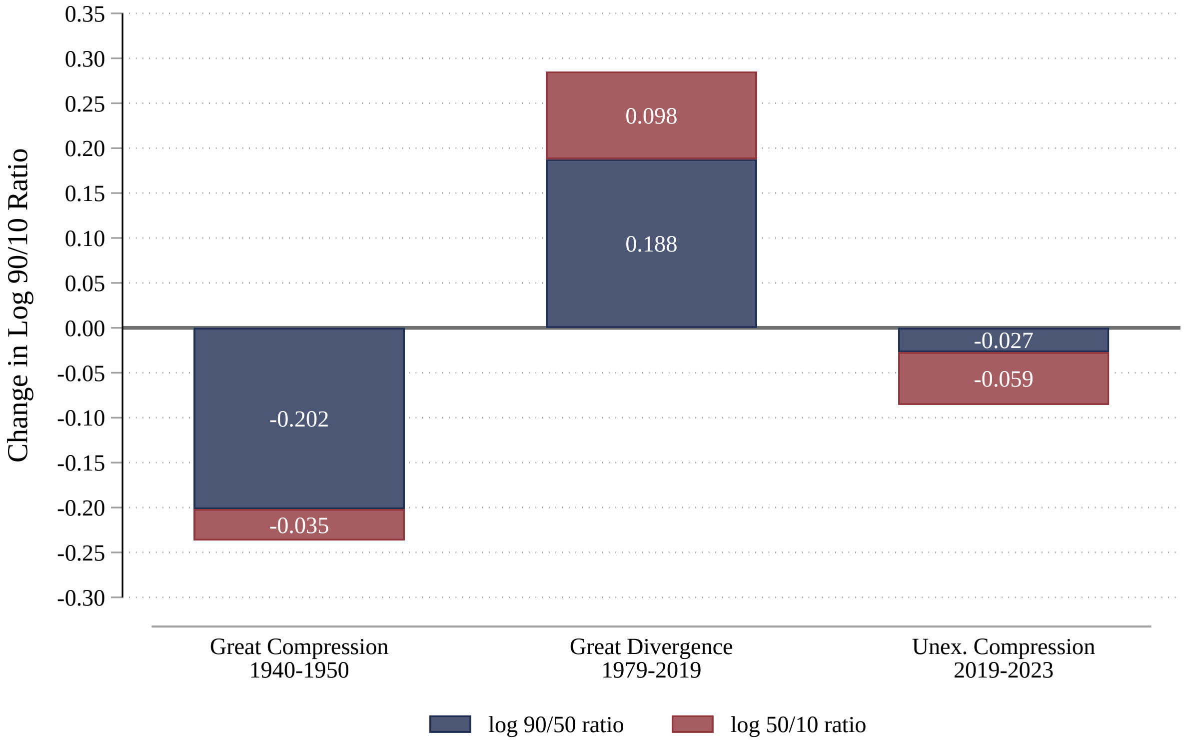


C. State minimum wage above federal level



Note: CPS monthly data. Wages are real (June 2023 USD). Wage quantiles are constructed by month and are smoothed first with lowess and then a 3-month moving average. In panels B and C, we identify thirty US states (including Washington DC) with a minimum wage above the federal level in 2019. Sixteen states have a minimum wage equal to the federal level, \$7.25, and 5 states have no minimum wage.

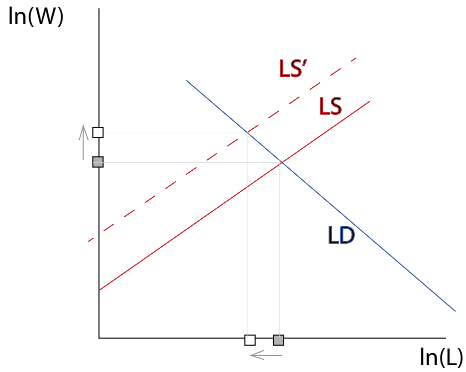
Figure 2: Change in Log 90/10 Ratio during Three Episodes of Wage Structure Change



Note: Figure displays the change in the 90/50 (blue), 50/10 (red), and 90/10 log hourly wage ratios (sum of blue and red) for three intervals: 1940–1950; 1979–2019; and 2019–2023. Data for 2023 extends from January - June of that year. For each analysis, the sample is limited to individuals between 16-64 years old. For the latter two periods, imputed wages are excluded and percentiles are smoothed using lowess. Data for 1940 and 1950 are from the 1% sample decennial Census sourced from IPUMS. CPS monthly data is obtained for 1979 from NBER and for 2019–2023 from IPUMS. Estimates in this figure correspond to Table A1.

Figure 3: Effect of Inward Labor Shift in a Competitive Labor Market

A. Market-level impact



B. Price-taking firm response

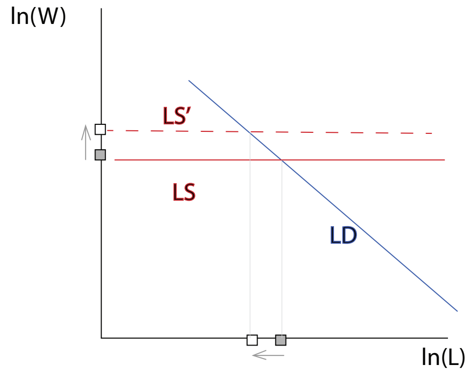
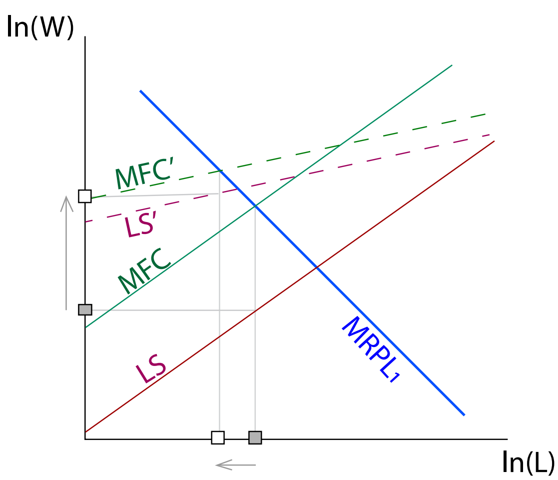


Figure 4: Effect of Rotation of Labor Supply Curve in a Monopsonistic Labor Market

A. Low-productivity monopsonistic firm



B. High-productivity monopsonistic firm

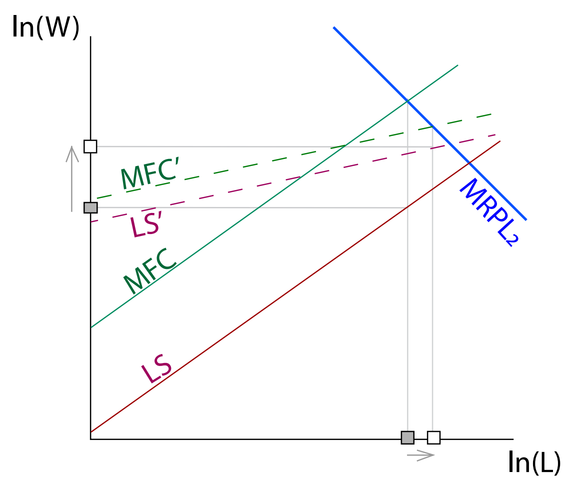
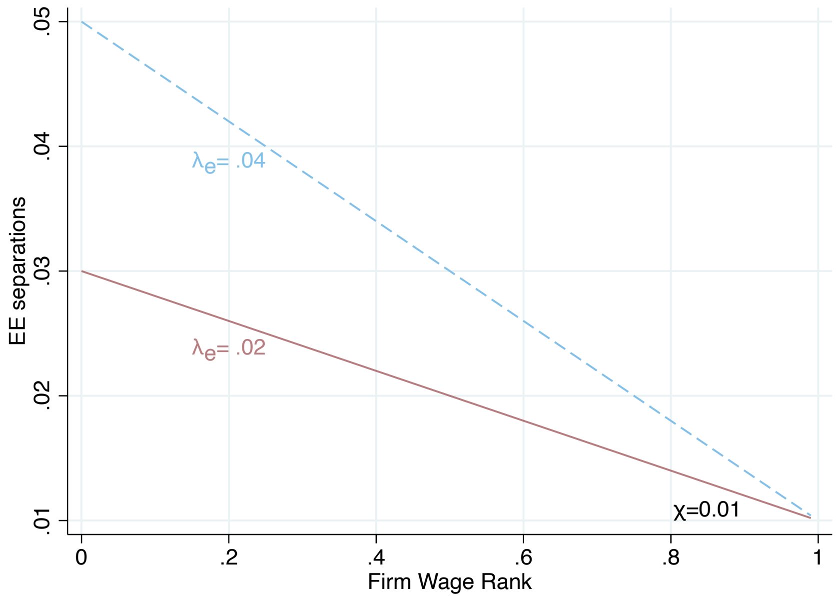
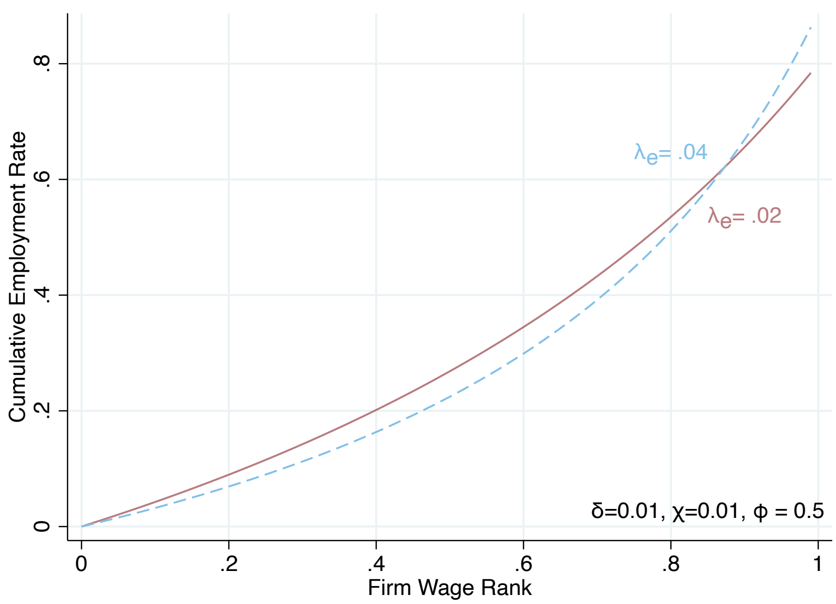


Figure 5: A Rise in the Offer Arrival Rate Primarily Increases the Job-to-Job Separation Rate at Firms with Lower Wage Ranks



Note: Figure plots the EE separation rate as a function of firm wage rank from the dynamic job ladder model presented in Section 2.1 and further elaborated in Appendix Section A2. The EE separation equation is $EE(w) = \chi + \lambda_e(1-r)$, where EE is the employment-to-employment separation rate, χ is the exogenous separation rate to another job, r is firm wage rank, and λ_e is the offer arrival rate. This equation is plotted for $\chi = 0.01$ and separately for $\lambda_e = .02$ and $\lambda_e = .04$.

Figure 6: A Higher Offer Arrival Rate Increases the Steady-State Share of Workers at Higher Wage-Rank Firms

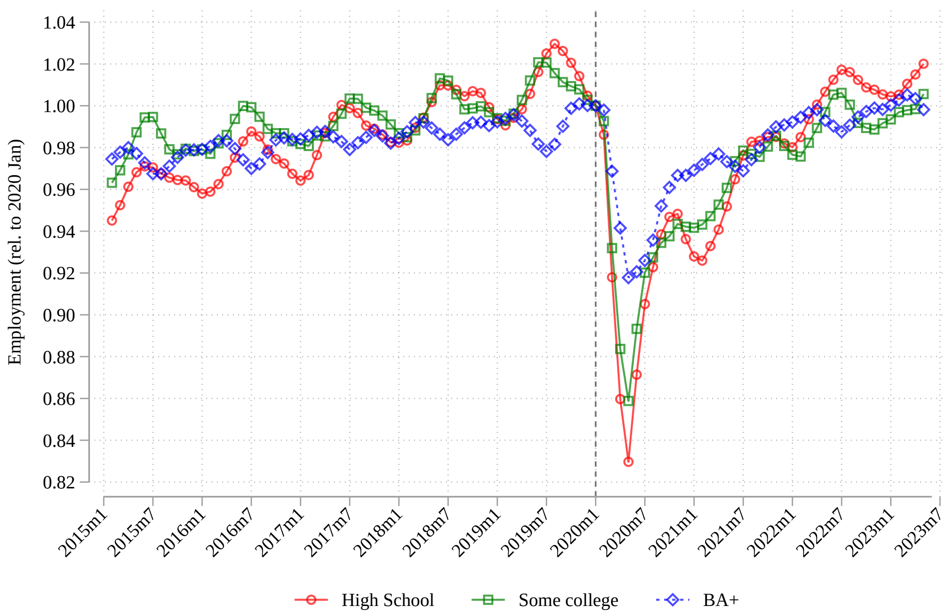


Note: Figure plots the cumulative employment rate in steady state as a function of firm wage rank from the dynamic job ladder model presented in Section 2.1 and further elaborated in Appendix Section A2. The equation estimated in this figure is equation (11) (in levels) for $\delta = 0.01$, $\chi = 0.01$, and $\phi = 0.5$. Cumulative employment is plotted separately for different offer arrival rates ($\lambda_e = .02$ and $\lambda_e = .04$).

Figure 7: Trends in Employment 2015 – 2023, Relative to January 2020

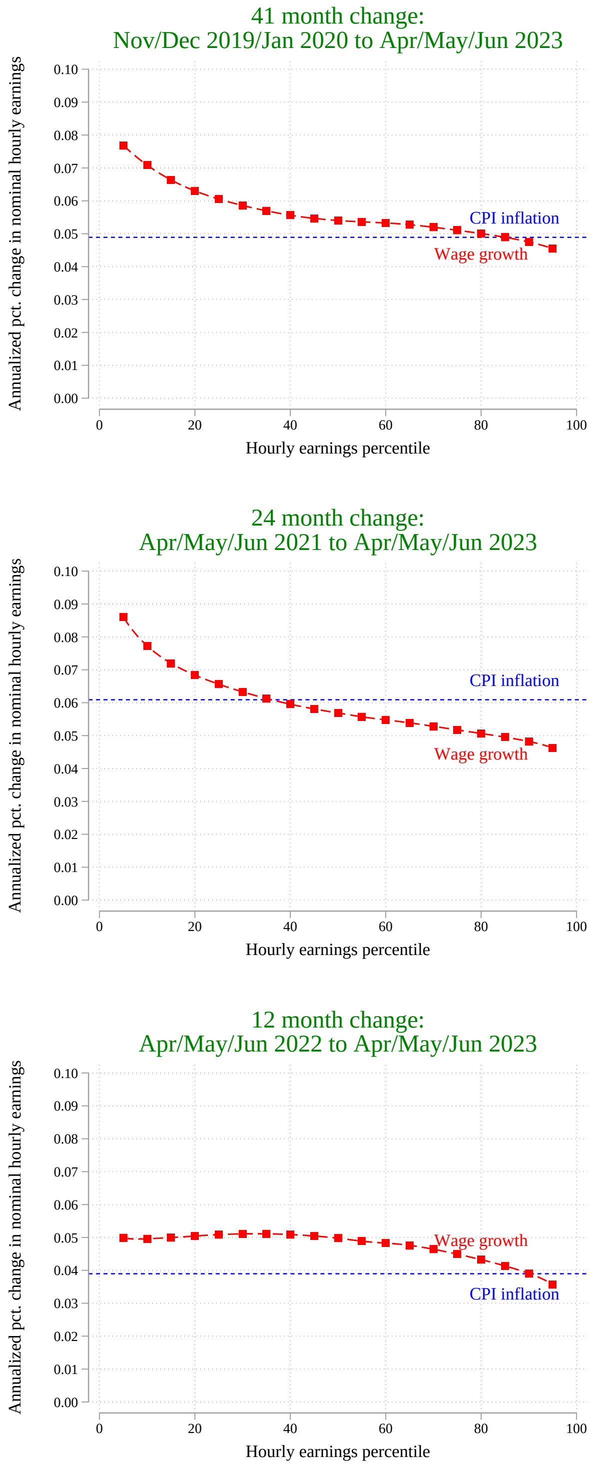
A. Employment and Labor Force Participation Rates

B. Employment Rates by Education



Note: CPS monthly data. Employment and labor force participation rates are smoothed with a 3-month moving average.

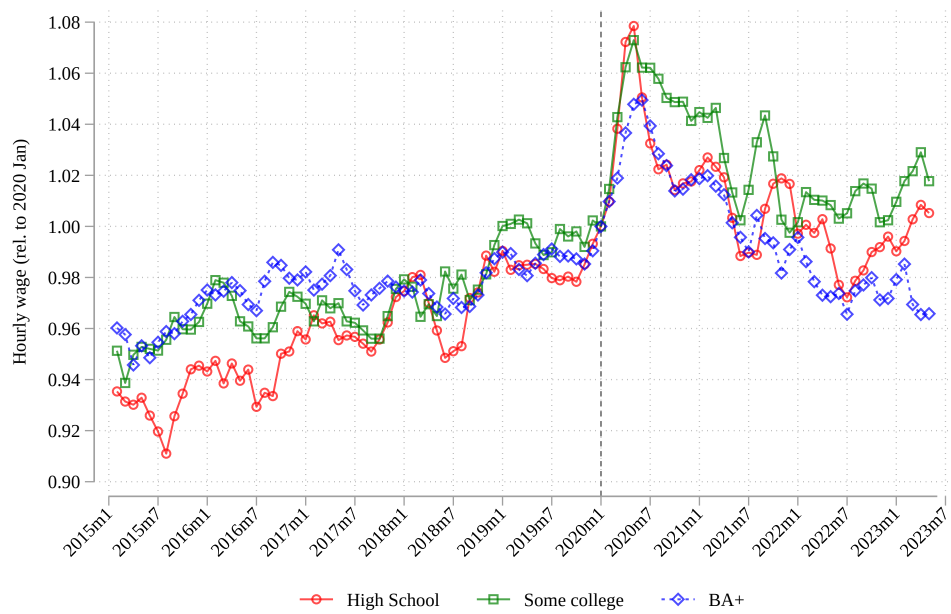
Figure 8: Annualized Percent Change in Nominal Hourly Earnings by Earnings Percentile Over 41, 24, and 12 Months



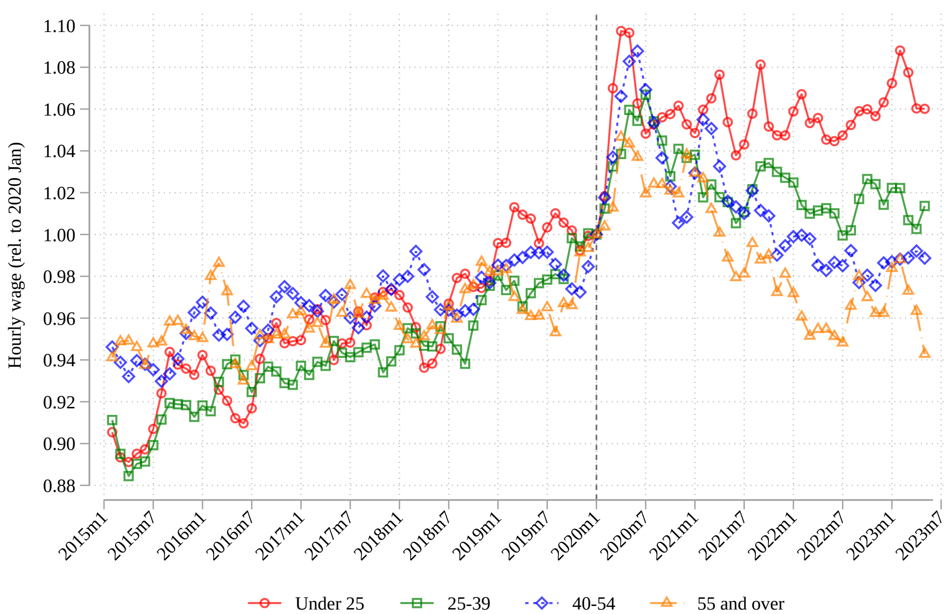
Note: CPS monthly data. Wage percentiles are smoothed with lowess. Inflation is calculated using seasonally unadjusted CPI-U.

Figure 9: Trends in Real Hourly Wages by Education and Age 2015 – 2023, Relative to January 2020

A. By Education



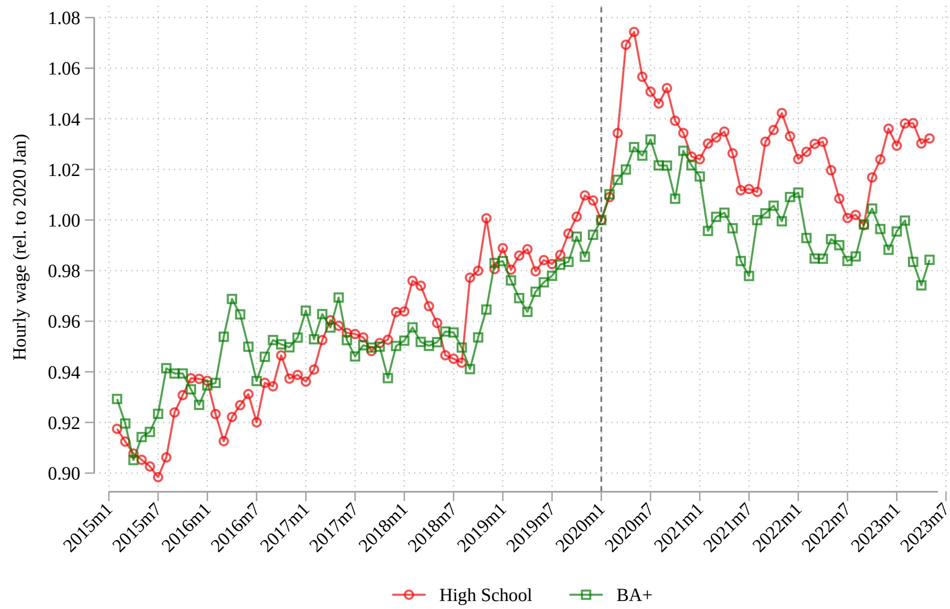
B. By Age



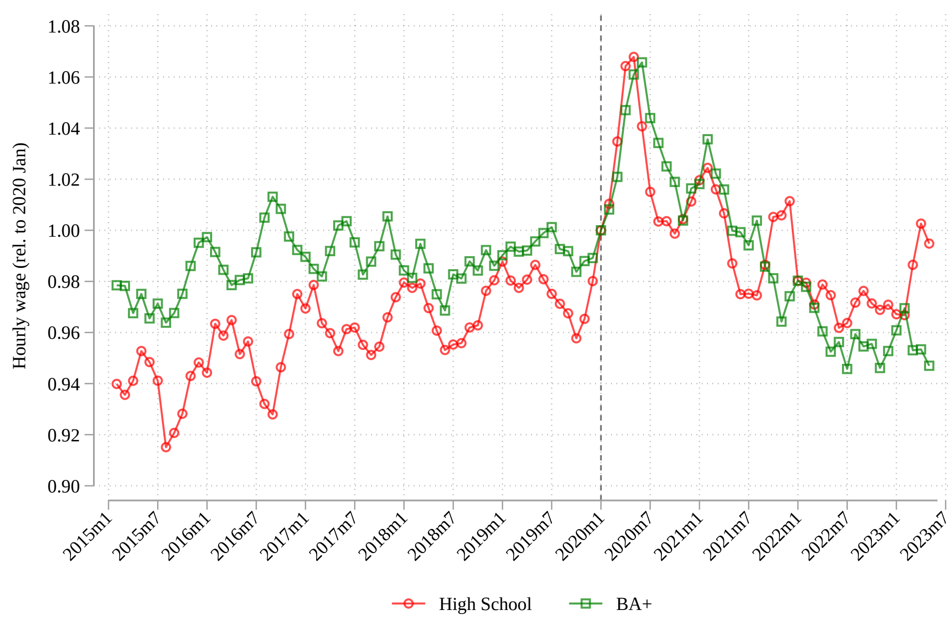
Note: CPS monthly data. Wages are real (June 2023 USD) and smoothed with a 3-month moving average.

Figure 10: Trends in Real Hourly Wages among High School and BA+ Workers by Age Group 2015 – 2023, Relative to January 2020

A. Workers under age 40

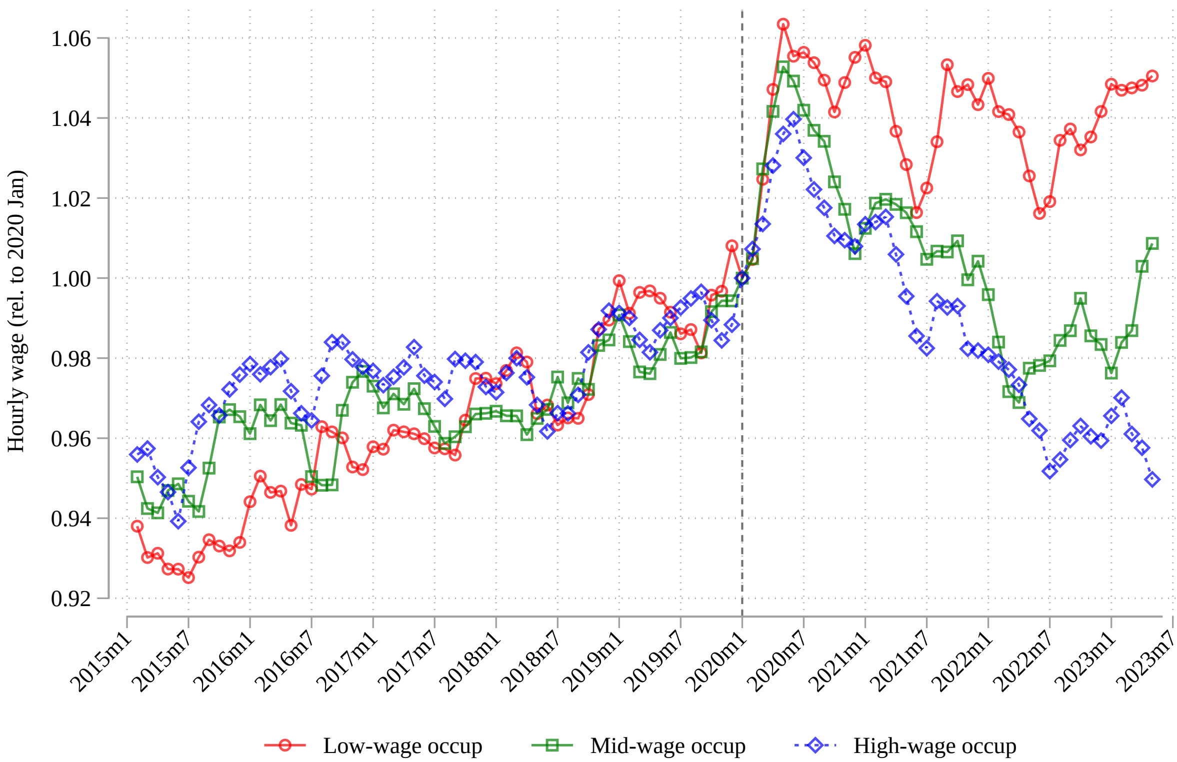


B. Workers age 40 and above



Note: CPS monthly data. Wages are real (June 2023 USD) and smoothed with a 3-month moving average. "High School" workers are those with a high school diploma or less, and "BA+" workers are those with a bachelor's degree or greater.

Figure 11: Trends in Real Hourly Wages by Occupational Wage Tercile 2015 – 2023, Relative to January 2020

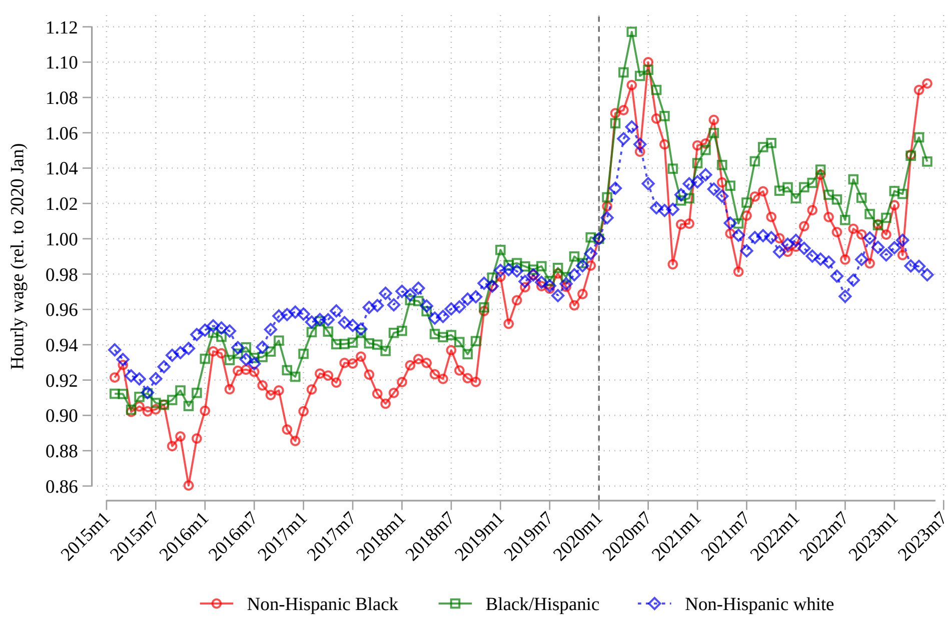


Note: CPS monthly data. Wages are real (June 2023 USD) and smoothed with a 3-month moving average. Occupational wage terciles are measured pre-pandemic, in 2019.

Figure 12: Trends in Real Hourly Wages by Sex and Race 2015 – 2023, Relative to January 2020

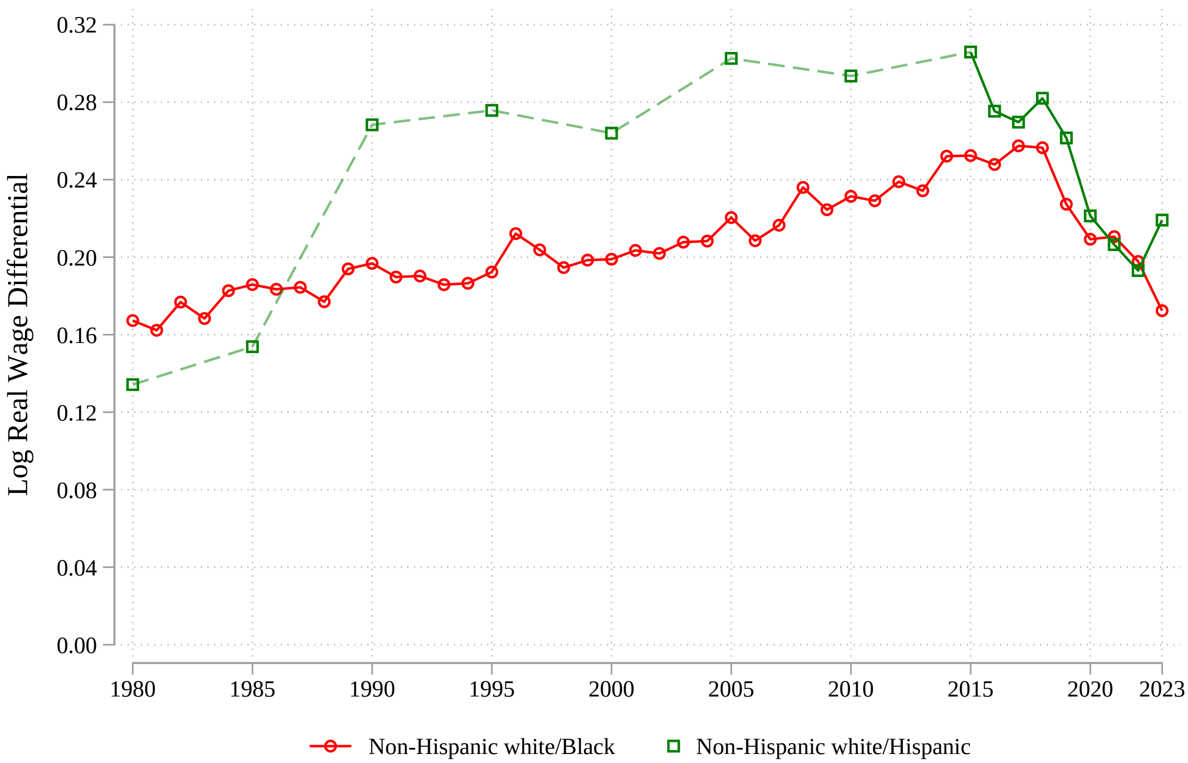
A. By Sex

B. By Race



Note: CPS monthly data. Wages are real (June 2023 USD) and smoothed with a 3-month moving average.

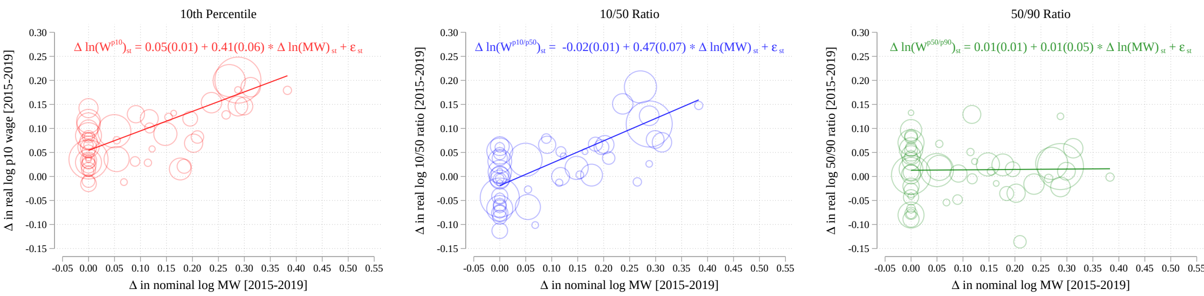
Figure 13: Estimates of Racial Wage Differentials, 1980 – 2023



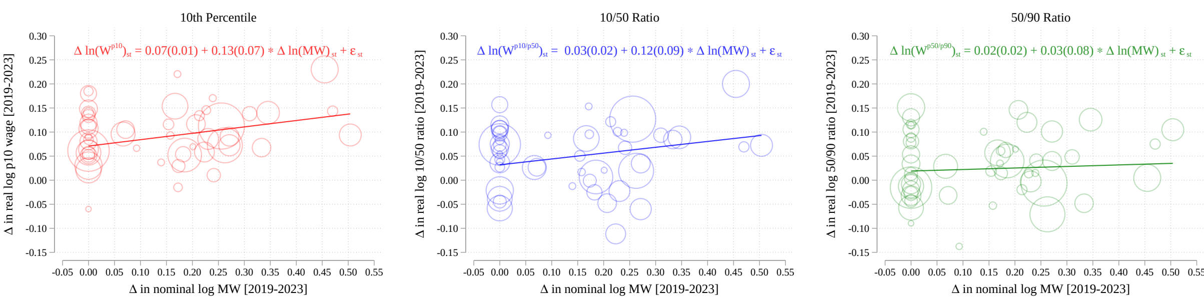
Note: CPS monthly data is sourced from NBER for 1980–1981 and from IPUMS for 1982–2023. Wage differentials estimated as the difference in the annual average log real wages of non-Hispanic white and non-Hispanic Black workers (circular markers) or the difference between non-Hispanic white and Hispanic workers (square markers). Prior to 2015, the wage differential between non-Hispanic white and Hispanic workers is noisy: hence we estimate 5-year averages, plotted at 5-year intervals and connected by the dashed line.

Figure 14: State-Level Relationship between Minimum Wage Changes and Wage Growth at Various Percentiles, 2015 – 2019 and 2019 – 2023

A. 2015–2019

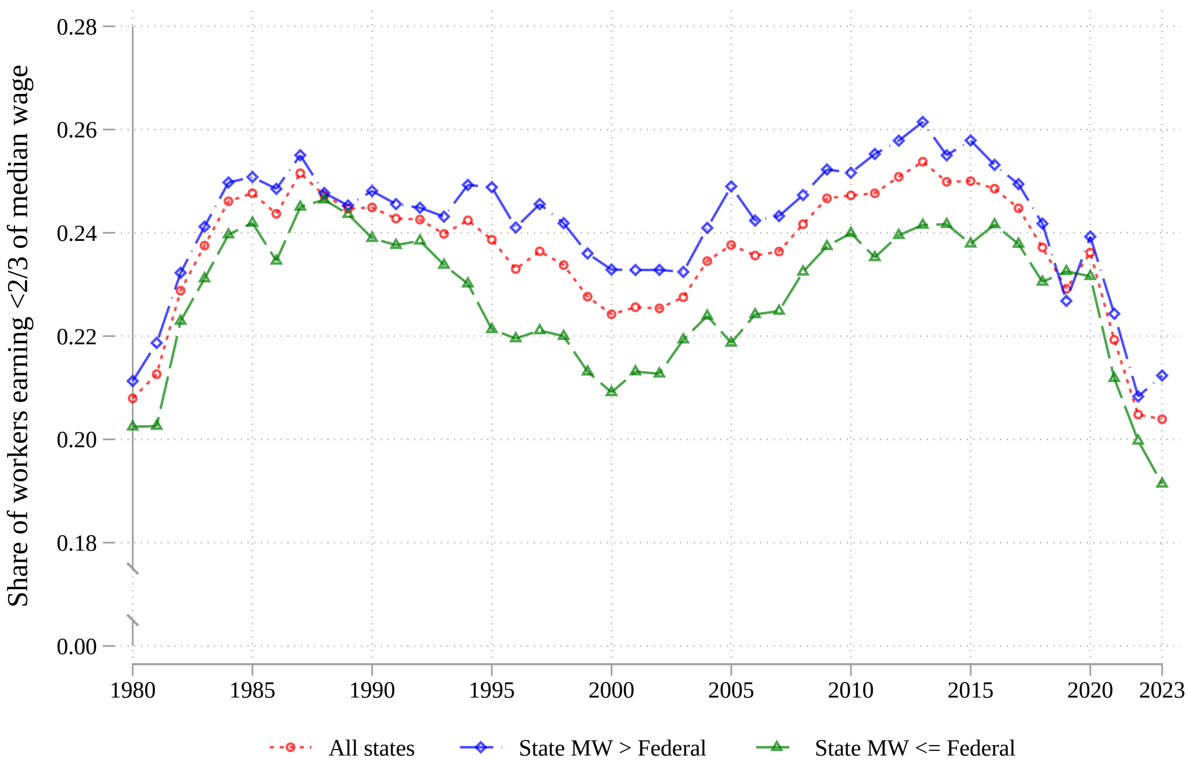


B. 2019–2023



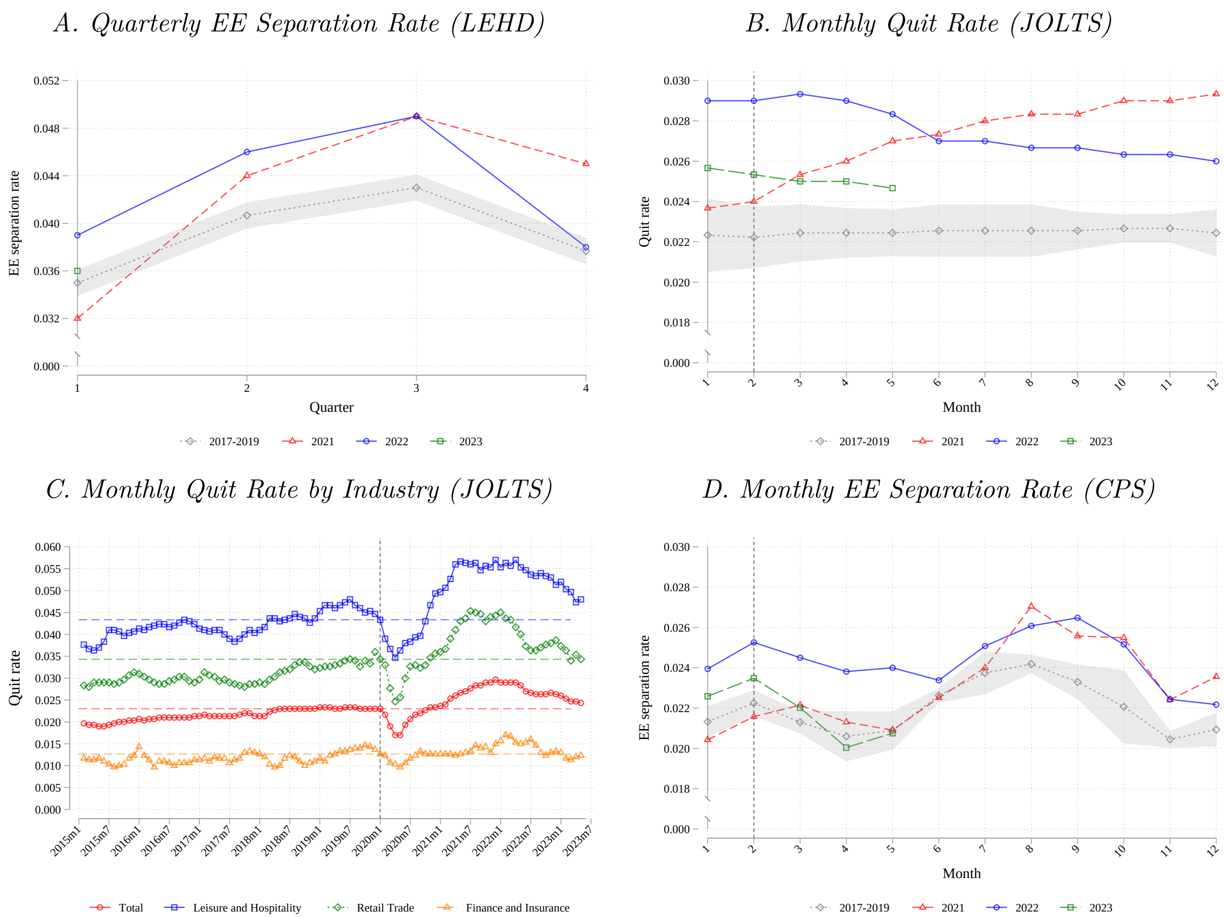
Note: CPS monthly data from IPUMS and annual state minimum wage data from Vaghul and Zipperer (2019). The figures visualize regressions of real hourly wage changes (June 2023 USD) on nominal minimum wage changes. Each circle on the figure represents a state with the size of the circle corresponding to state population size (of wage earners). Regression coefficients and intercepts are specified in the corresponding equation at the top of each panel. Standard errors are displayed in parenthesis to the right of coefficients. The dependent variable is the state-level change in the log real wage at the 10^{th} percentile of the wage distribution (first column), the change in the log ratio of the 10^{th} to 50^{th} wage percentiles (second column) and the change in the log ratio of the 50^{th} to 90^{th} wage percentiles (third column). The independent variable is the change in the log nominal minimum wage over the corresponding periods. We estimate these regressions for changes between 2015 and 2019 in Panel A and for changes between 2019 and the first half of 2023 in Panel B.

Figure 15: Share of Workers Earning Under 2/3$^{\text{rds}}$ of the State Median Wage, 1980 – 2023



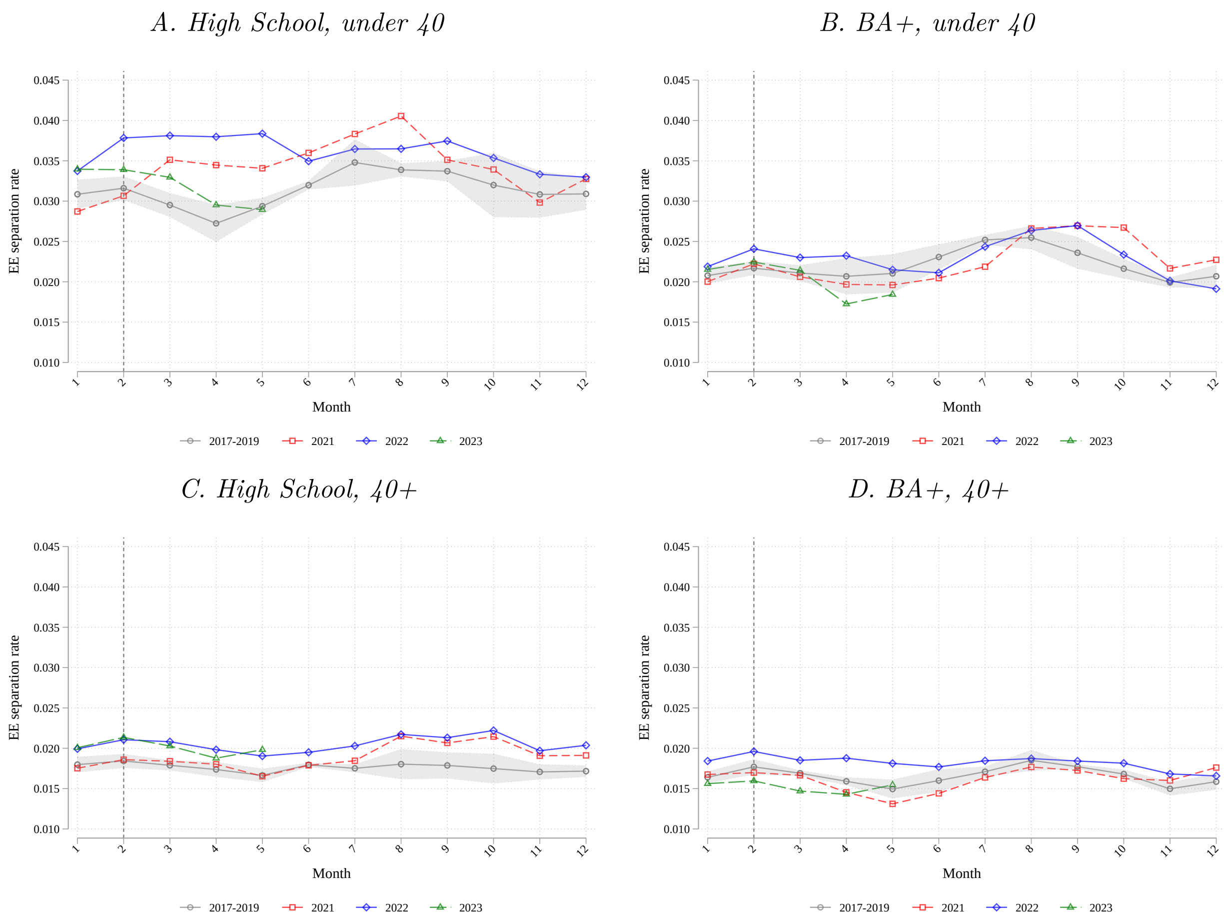
Note: CPS monthly data is sourced from NBER for 1980–1981 and from IPUMS for 1982–2023. However, we confirm that NBER data in the years following 1981 align with the IPUMS data. In this figure, we take the share of all workers earning below 2/3rds of their state and year-specific median wage. Median wages are calculated using state-by-year population weights. We identify states above the federal minimum wage based on minimum wages in 2019.

Figure 16: Monthly and Quarterly Employment-to-Employment Transition Rates, 2017 – 2023



Note: Quarterly EE separation rates are from Job-to-Job flows data aggregated from Longitudinal Employer-Household Dynamics (LEHD) data published by the Census Bureau (panel A). Monthly quit rates obtained from BLS Job Openings and Labor Turnover Survey (JOLTS) data (panels B and C). In panel C, dashed lines represent the industry quit rate in January 2020 and sectors reflect 1-digit or 2-digit NAICS codes. Monthly employment-to-employment (EE) separation rates obtained from CPS monthly data (panel D). Monthly EE separation rates and quit rates are smoothed with a 3-month moving average. Shaded areas in panels A, B and D represent the 95% confidence interval for the respective rates during the 2017–2019 period.

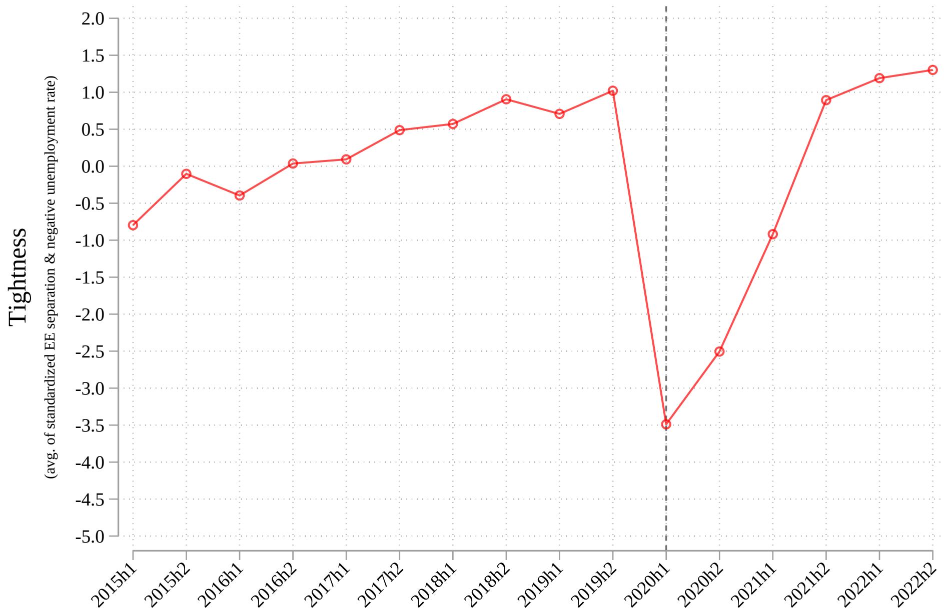
Figure 17: Monthly Employment-to-Employment Transition Rates by Age and Education Groups, 2017 – 2023



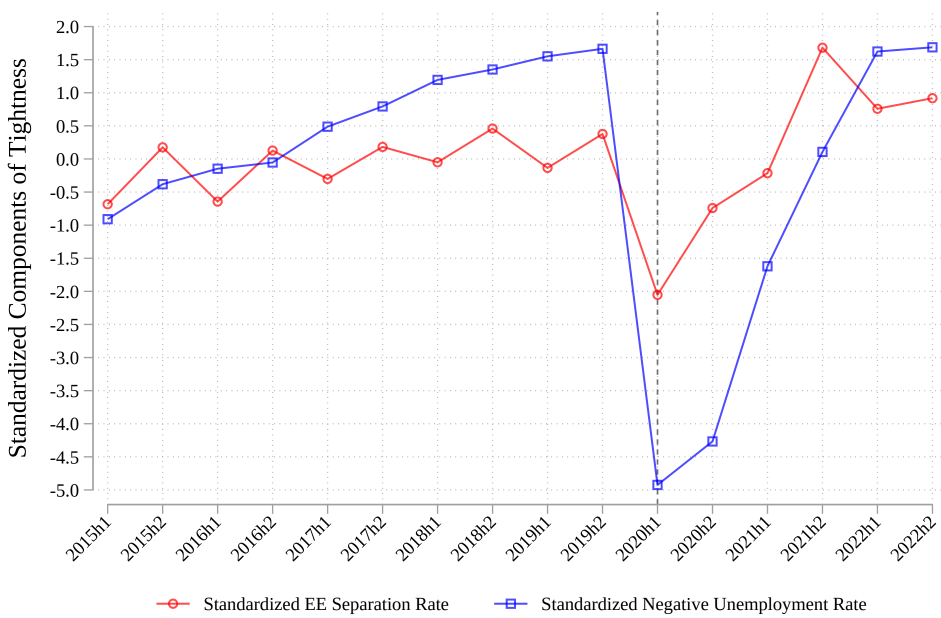
Note: CPS monthly data. Employment-to-employment (EE) separation rate is smoothed with a 3-month moving average. Shaded area represents the 95% confidence interval for the monthly EE separation rate during the 2017–2019 period.

Figure 18: Standardized Labor Market Tightness and its Components, 2015 – 2022

A. Tightness

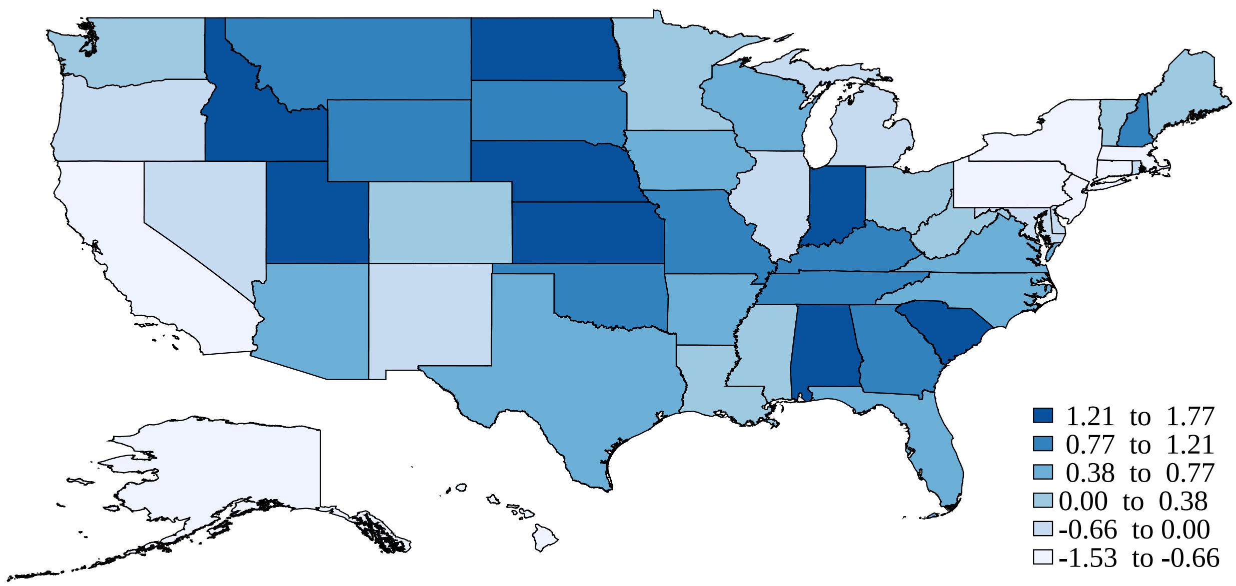


B. Components of tightness



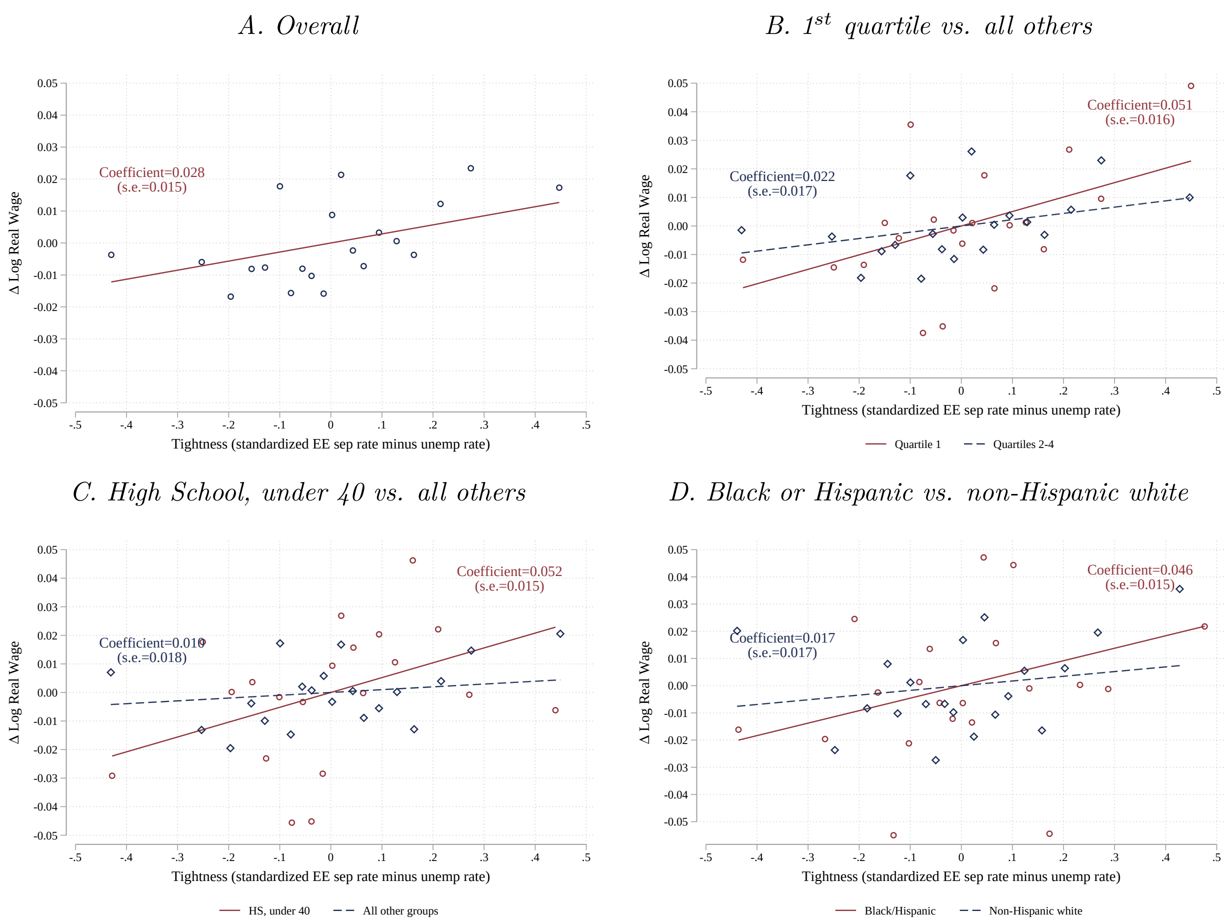
Note: Seasonally-adjusted state unemployment rates obtained from BLS LAUS. EE separation rates (not seasonally adjusted) obtained from J2J flows data aggregated from LEHD data published by the Census Bureau. Tightness is calculated as the state-level average of the standardized EE separation rate and the negative, standardized unemployment rate. EE separation and unemployment rates are standardized relative to their respective mean and standard deviation over the entire sample period.

Figure 19: Cross-State Variation in Standardized Labor Market Tightness, 2021 – 2022



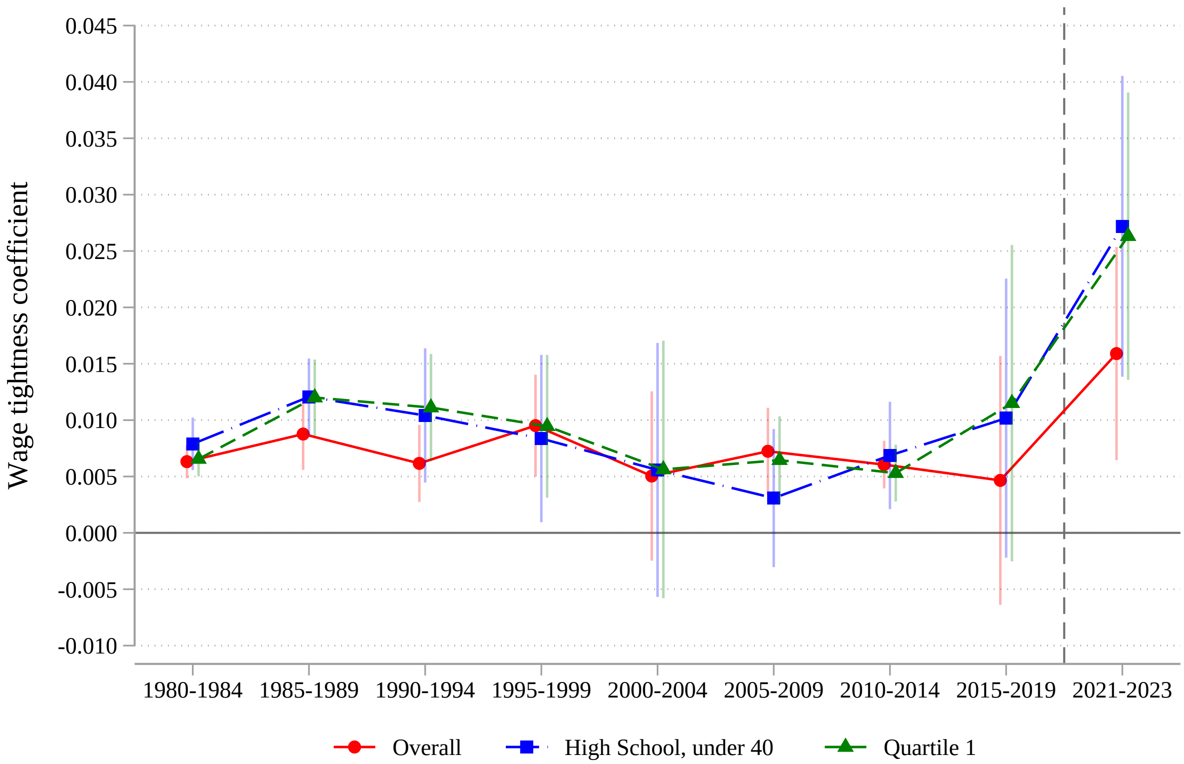
Note: Seasonally-adjusted state unemployment rates obtained from BLS LAUS. EE separation rates (not seasonally adjusted) obtained from J2J flows data aggregated from LEHD data published by the Census Bureau. Tightness is calculated as the state-level average of the standardized EE separation rate and the negative, standardized unemployment rate. EE separation and unemployment rates are standardized relative to their respective mean and standard deviation over the entire sample period.

Figure 20: Estimated Wage-Phillips Curves 2021 – 2023, Using Cross-State Variation in Tightness: Overall and by Subgroup



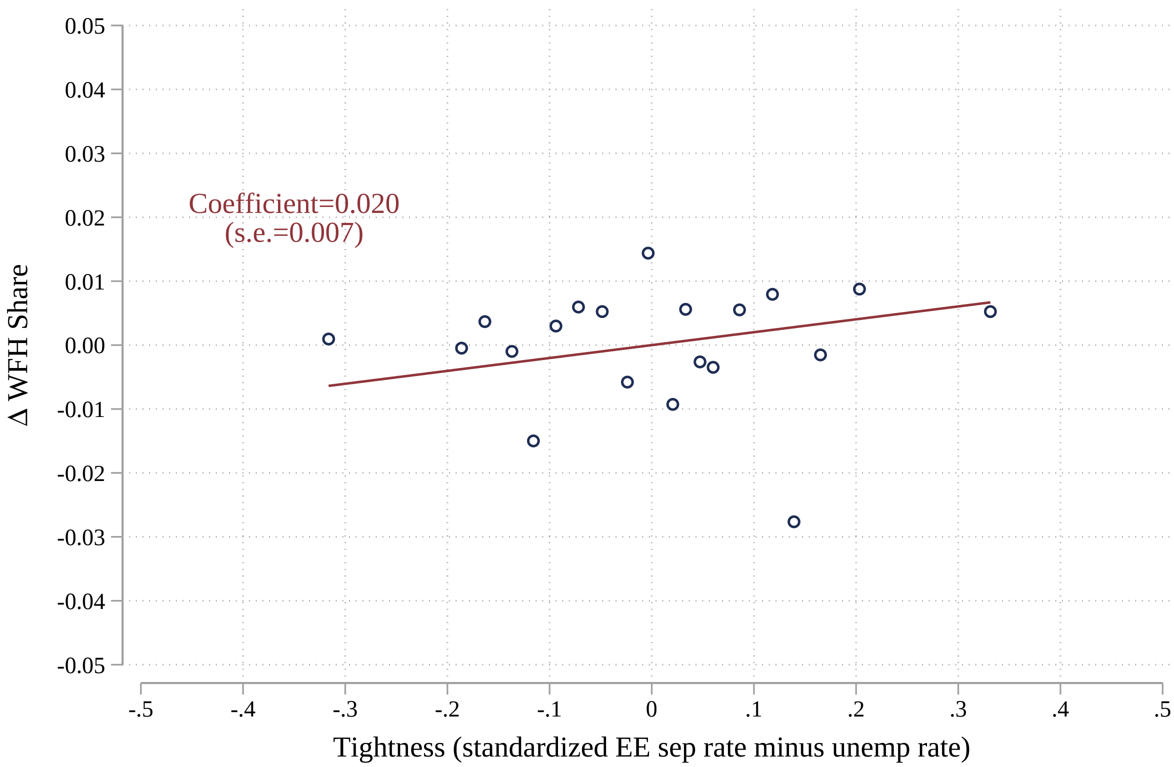
Note: Binscatters show the relationship between tightness (in half-year period $t-1$) and log real wage change (between $t-1$ and t) in 2021–2023. Tightness is calculated as the average of the standardized EE separation rate and the negative, standardized unemployment rate, measured at the state level. Panel A shows the overall wage change associated with tightness, while 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. Finally, Panel D contrasts wage gains for Black or Hispanic workers versus non-Hispanic white workers. Wage-Phillips curve coefficients estimated from the regression equation (5), are displayed on each figure and reported in Tables 1a and 1b. The wage-Phillips curve coefficients are allowed to vary by pre- and post-pandemic periods (2015–2019, 2021–2023); the latter are reported here. All regressions include state and time fixed effects. Demographic controls include state-by-period population shares of age groups, education groups, race (Black), Hispanic ethnicity and sex. We also control for the state Covid-19 death rate per 100,000 people as of June 2023. All wage and demographic data are obtained from CPS monthly files. We obtain seasonally-adjusted state unemployment rates from BLS LAUS, seasonally-unadjusted EE separation rates from LEHD J2J Flows, Covid-19 death rates from CDC (2020). The y-axis is average annualized log real wage change. Wage quartiles are estimated semi-annually by state. Standard errors clustered at the state level.

Figure 21: Estimated Wage-Phillips Curves 1980 – 2023, Using Cross-State Variation in Unemployment: Overall and by Subgroup



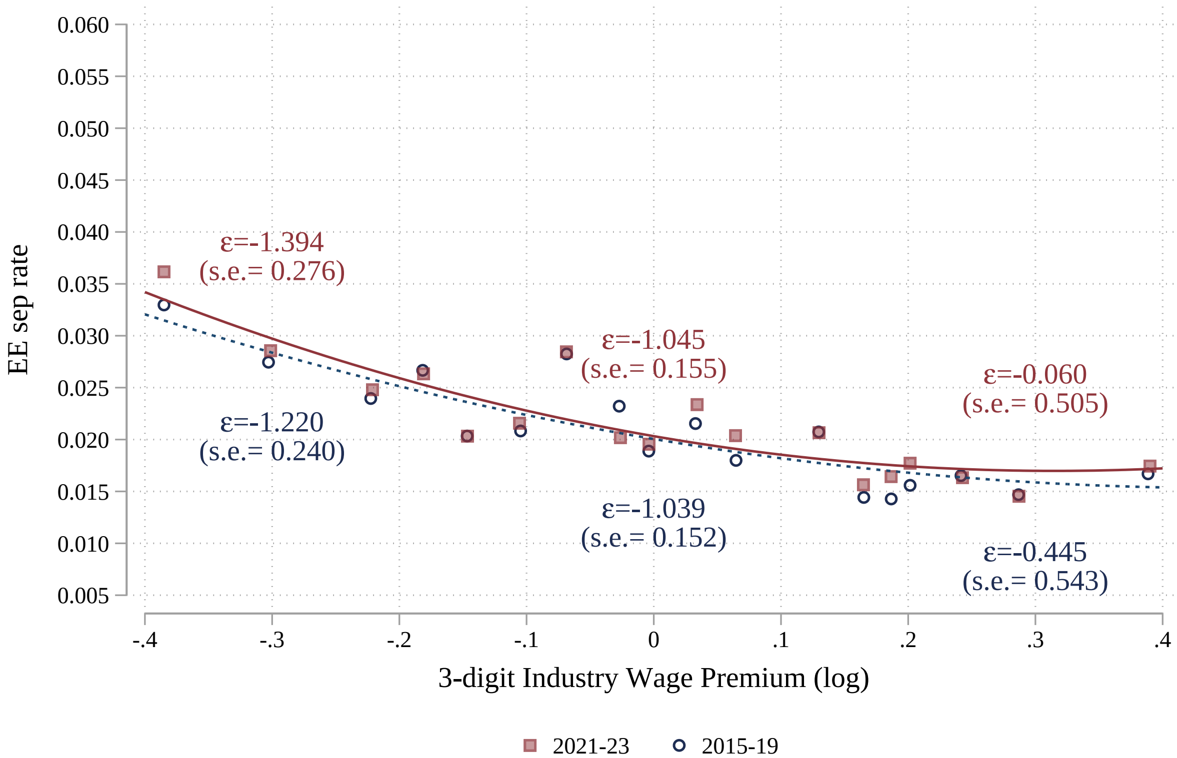
Note: Figure shows the historical evolution of the wage-Phillips curve coefficient (β from equation (5)) over negative standardized unemployment, measured at the state level. We interact this measure of unemployment with indicators for five-year periods from 1980–2019 and the three-year post-pandemic period (2021–2023), to allow the coefficients to vary dynamically. Each series represents regressions estimated separately for each group of workers: all workers, high-school educated workers under 40, and bottom quartile earners. Each point represents the average annualized log real wage change associated with a one standard deviation increase in unemployment over that time interval. Vertical lines represent 95% confidence intervals. The vertical dashed line represents the year 2020 — we exclude observations that overlap with 2020 to avoid capturing pandemic-related wage effects. We estimate our main specification, which includes state and time fixed effects, where time is half-year periods. We include as demographic controls state-by-period population shares of age groups, education groups, race (Black), Hispanic ethnicity, and sex. the state. We also control for the Covid-19 death rate per 100,000 people as of June 2023. Wage quartiles are estimated by state and half-year. Standard errors clustered at the state level. *Data sources:* All wage and demographic data are obtained from CPS monthly files. We obtain seasonally-adjusted state unemployment rates from BLS LAUS, seasonally-unadjusted EE separation rates from LEHD J2J Flows, and Covid-19 death rates from CDC (2020).

Figure 22: Estimated Relationship between Work-From-Home (WFH) Arrangements and Tightness 2021 – 2023, Using Cross-State Variation in Tightness



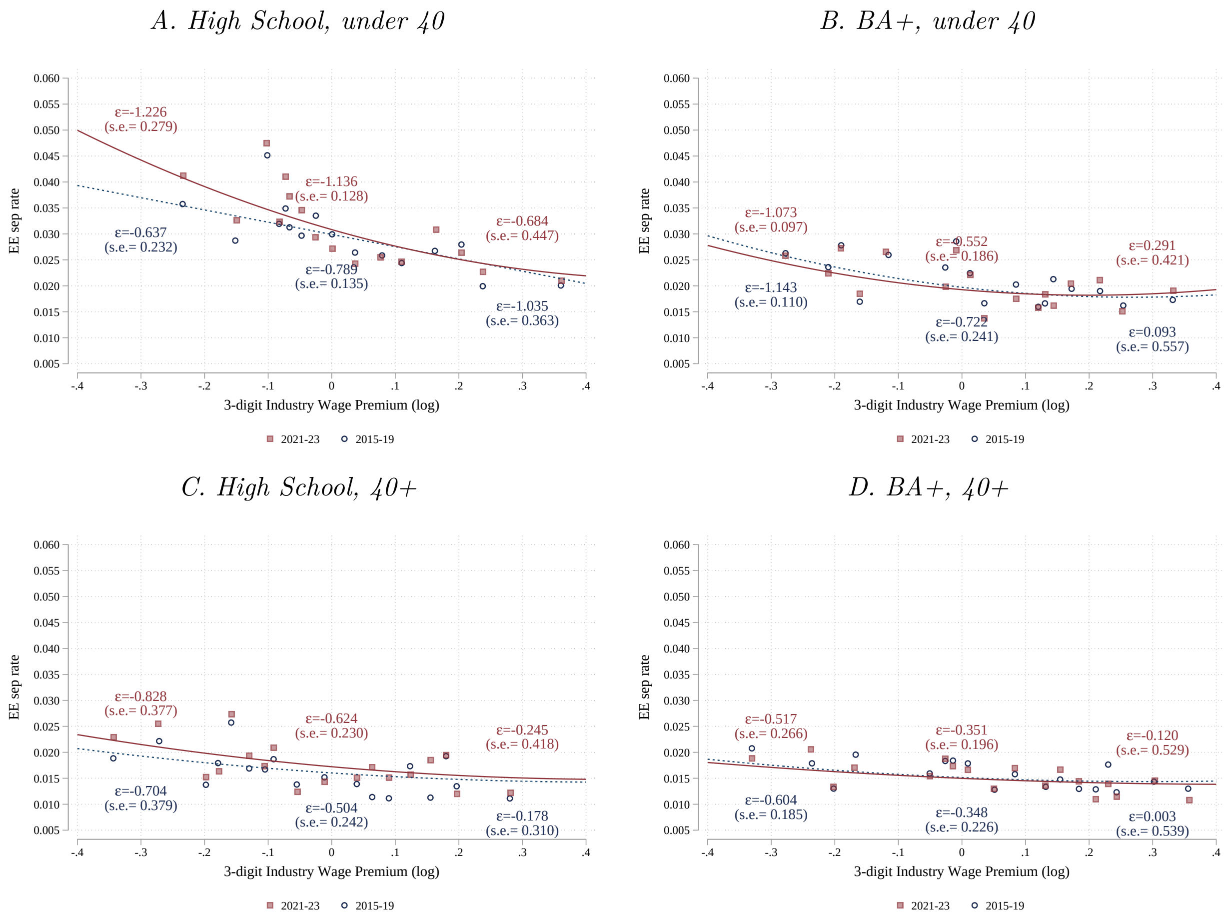
Note: Binscatter shows the relationship between tightness (in half-year period $t-1$) and the change in work-from-home shares (between $t-1$ and t) in 2021–2023, as reported in Table A3. Tightness is calculated as the average of the standardized EE separation rate and the negative, standardized unemployment rate, measured at the state level. We include state and time fixed effects. Demographic controls include state-by-period population shares of age groups, education groups, race (Black), Hispanic ethnicity and sex. We also control for the state Covid-19 death rate per 100,000 people as of June 2023. The y-axis is average annualized WFH share change. Standard errors (in parentheses) are clustered at the state level. *Data sources:* WFH share data from Hansen et al. (2023); demographic data from CPS monthly files; seasonally-adjusted state unemployment rates from BLS LAUS; seasonally-unadjusted EE separation rates from LEHD J2J Flows; and Covid-19 death rates from CDC (2020).

Figure 23: Estimated Job-to-Job Separation Elasticities, 2015 – 2019 and 2021 – 2023



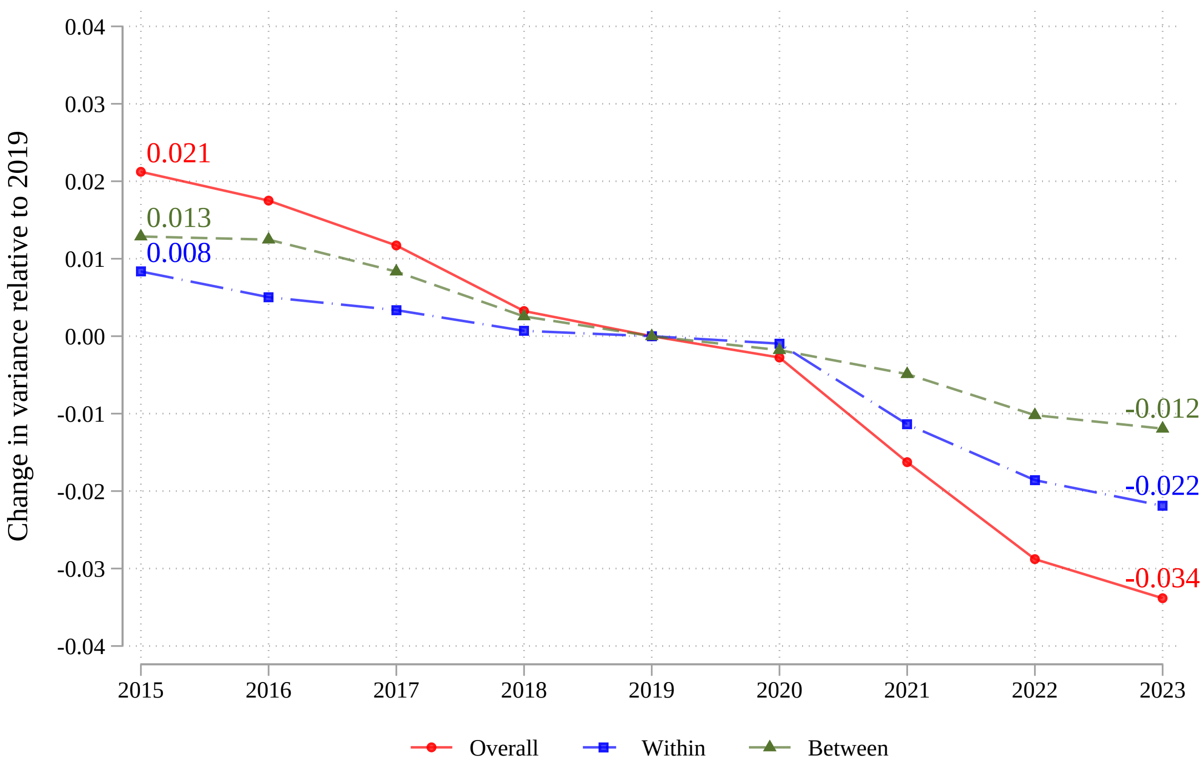
Note: Binscatter shows the quadratic relationship between industry wage premia (IWP) and EE separations, before and after the pandemic. Elasticities at $x = \{-0.3, 0, 0.3\}$ are calculated in two steps: first, we regress an indicator for EE separation at time t on IWP at time $t-1$ and its square (based on equation (7)). 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. 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. Demographic controls from this regression include age (linear, squared and cubed), education categories, race categories, Hispanic ethnicity, citizenship status, sex, and metro area status. Standard errors are clustered at the industry level. Coefficients from the regression in step one are reported in Table A4 and the elasticities in this figure, as well as their difference between the 2015–2019 and 2021–2023 periods, are reported in columns 1–3 of Table 4 and, with demographic controls, in Columns 4–6. *Data sources:* CPS monthly data.

Figure 24: Estimated Job-to-Job Separation Elasticities, 2015 – 2019 and 2021 – 2023: by Age and Education Groups



Note: Binscatters show the quadratic relationship between industry wage premia (IWP) and EE separations, before and after the pandemic, by age and education groups. 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 $t-1$ and its square (based on equation (7)). 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. 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. Demographic controls from this regression include age (linear, squared and cubed), education categories, race categories, Hispanic ethnicity, citizenship status, sex, and metro area status. Standard errors are clustered at the industry level. Coefficients from the regression in step one are reported in Table A4 for panel A and in Table A5 for panels B through D. The elasticities in this figure, as well as their difference between the 2015–2019 and 2021–2023 periods for each subgroup, are reported in columns 1–3 of Table 4 and, with controls, in columns 4–6. *Data sources:* CPS monthly data.

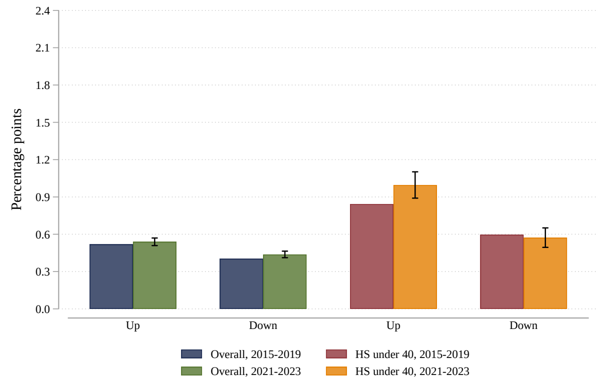
Figure 25: Trends in the Variance of Log Wages 2015 – 2023:
Within versus Between Components



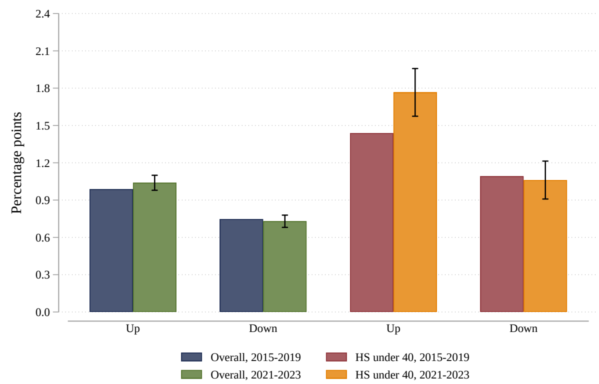
Note: CPS monthly data. The red circular markers plot the change in the overall variance of ln(wage) between year y and 2019. The blue square markers plot the change in the variance of the log residual wage with respect to 2019, which is the within-group component of the change in variance. The residual wage is calculated by regressing ln(wage) on fully interacted indicators for 3-educational categories, 6 age groups and each year (2015–2023). The short green triangle markers, representing the between-group component, are just the difference between the circular and square markers.

Figure 26: Exit and Entry Rates from Industry Wage Premium and Hospitality Sector

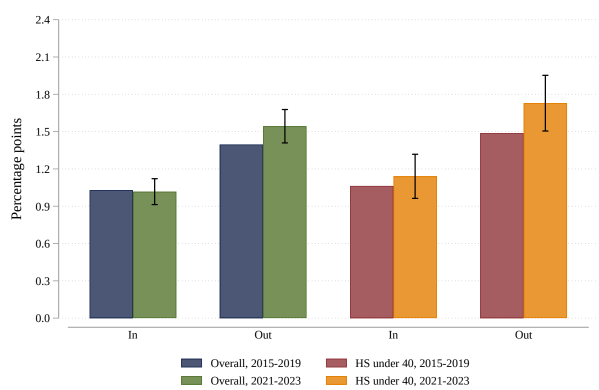
A. Bottom Half of the 3-Digit Industry Wage Premia



B. Bottom Quartile of the 3-Digit Industry Wage Premia



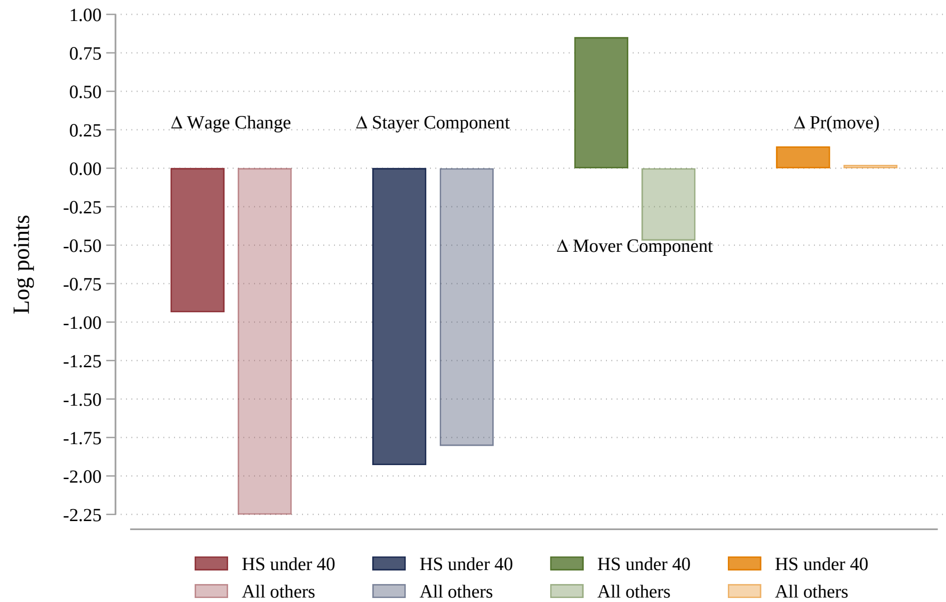
C. In and Out of the Hospitality Industry



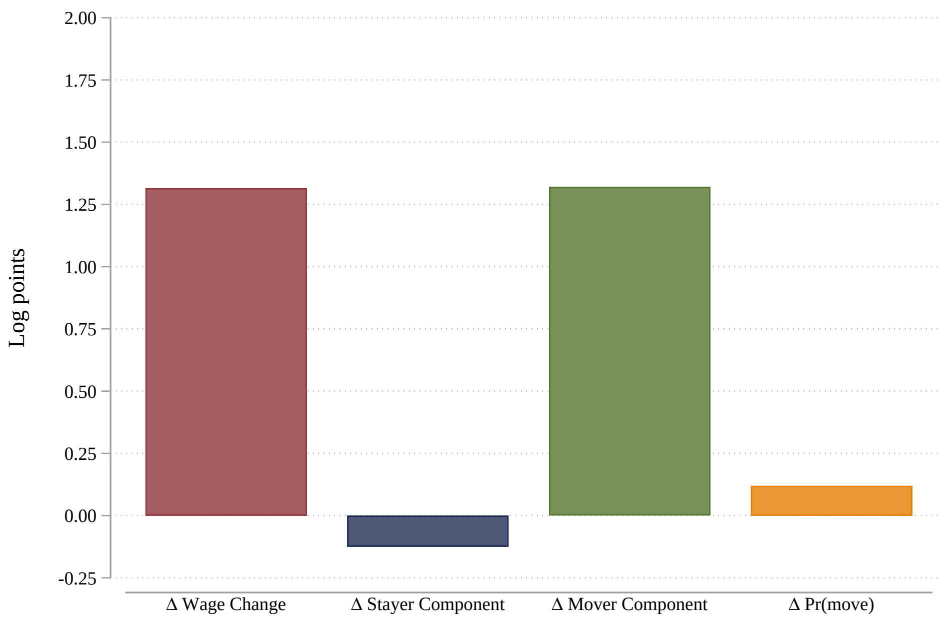
Note: Figure reports monthly hazard rates for exiting and entering industry groups based on equation (8). In panels A and B, *Up* represents the likelihood of switching from the bottom half (or bottom quartile) of the 3-digit IWP distribution to the top half (or top three quartiles). *Down* represents the likelihood of switching from the top half (or top three quartiles) of the IWP distribution to the bottom half (or bottom quartile). In panel C, *In* and *Out* represent the likelihood of entering and exiting the hospitality sector, respectively. To account for the size differentials in exit and entry rates, *Down* bars for panel B and *In* bars for panel C as well as the corresponding confidence intervals are re-scaled by $(1-p)/p$. For panel B, p is the share of workers in the bottom quartile ($p = 0.25$). For panel C, p is the share of workers in hospitality in 2015–2019, where $p = 0.079$ for the overall sample and $p = 0.185$ for the high-school under-40 sample. The error bars represent the 95% confidence intervals for the difference in movement between periods. 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, indicators variables for race, ethnicity, education, citizenship, metro area status, and industry. The sample is limited to those who were employed in the current and previous month. An individual is considered to have moved industries between time $t-1$ and t only if they also reported switching jobs since the previous month. The hospitality sector is composed of all the industries within the Bureau of Labor Statistics' sector category "Accommodation and Food Service". Estimates in panels A, B, and C correspond to Tables A9a, A9b, and A9c, respectively.

Figure 27: Decomposition of the Change in Annual Wage Growth between Job-Movers and Job-Stayers, 2021 – 2023 and 2015 – 2019

A. Levels: High school under 40 workers, all other workers

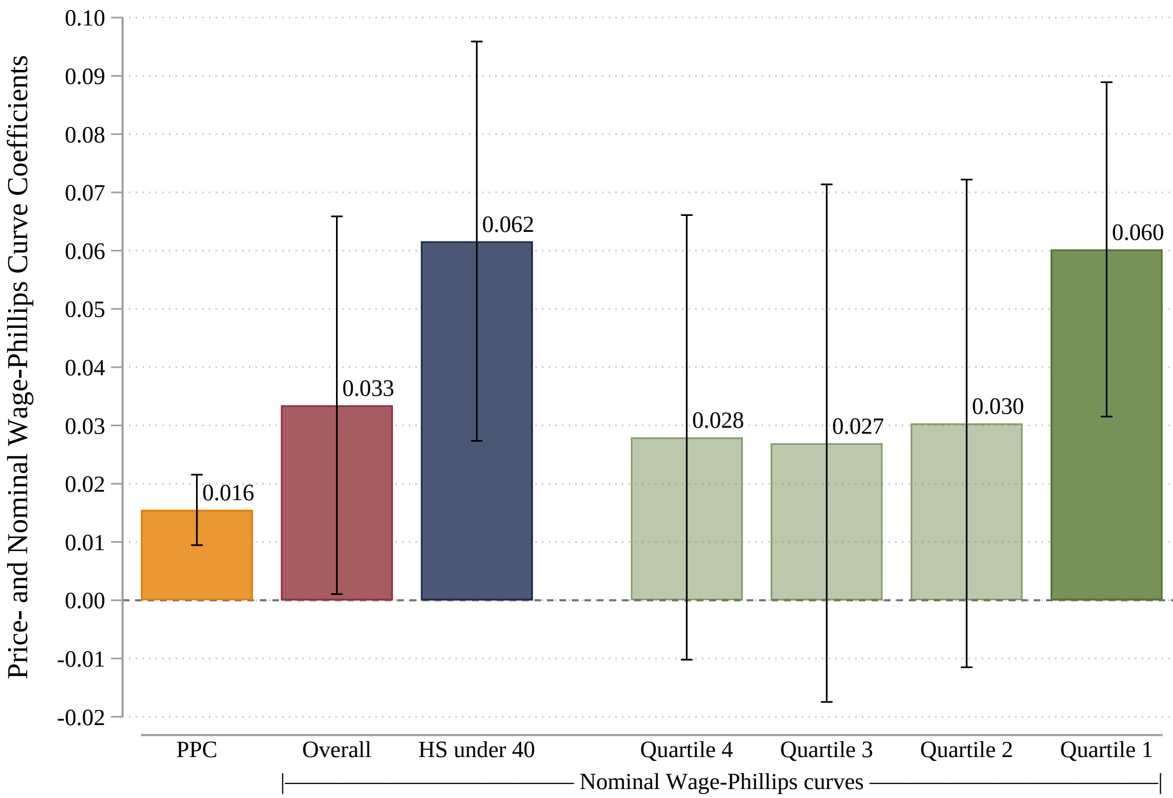


B. Difference 2021–2023 vs. 2015–2019: High school under 40 vs. all other workers



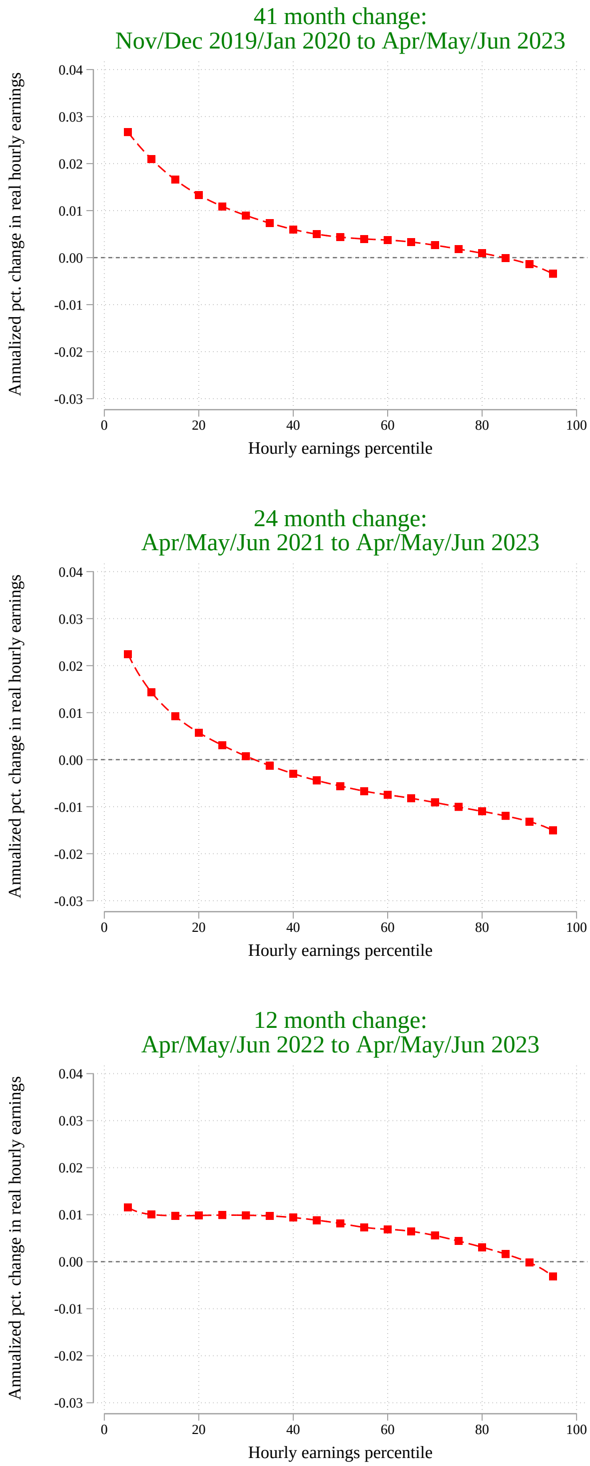
Note: CPS data. Panel A decomposes the change in annual wage growth between 2015–2019 and 2021–2023 into the contribution of job moving and job staying based on Equation (9), separately for high-school workers under age 40 and their complementary group. Wage changes are measured within individuals observed one year apart. The red bar represents the total change in wage growth between the 2015–2019 and 2021–2023 periods. The blue bars then represent the portion of the this total change due to the change in the wage growth for job-stayers, which in Equation (9) is equal to the difference in the wage growth of job-movers scaled by the complement of the switch rate in the first period (2015–2019). The green bars represent the the portion of the total change due to the change in the wage growth of switchers which is equal to the difference in the wage growth of job-switchers scaled by the switch rate in the first period. Finally, the yellow bars represent the contribution of the change in the switch rate, which is defined as the difference in the switch rate scaled by the difference between the mover and stayer wage growth in period 2 (2021-2023). Panel A corresponds to estimates in panel C of Table 5. Panel B reports the difference between the components of the wage decomposition for the two groups (simply the difference between the lighter and darker colored bars in Panel A or equivalently the two columns of panel C in Table 5).

Figure 28: Estimated State-Level Price-Phillips Curve and Wage-Phillips Curves: Overall and by Subgroup, 2021 – 2023



Note: Figure shows the estimated coefficients of the price-Phillips curve based on regression equation (10) and wage-Phillips curves for the overall workforce, high-school workers under age 40, and each wage quartile estimated separately using equation (5). The vertical bars represent 95% confidence intervals. We regress annualized log CPI change (1st bar) or real wage change (all other bars) between $t-1$ and t on tightness (in $t-1$), where t is half-year periods from 2015 through the second half of 2023. The main explanatory variable, tightness, is calculated as the average of the standardized EE separation rate and negative standardized unemployment rate, both measured at the state level. The wage-Phillips curve coefficients are allowed to vary by pre- and post-pandemic periods (2015–2019, 2021–2023), and the latter are reported here. All specifications include state and time fixed effects.Controls include state-by-period population shares of age groups, education groups, race (Black), Hispanic ethnicity and sex. We also control for the state Covid-19 death rate per 100,000 people as of June 2023. Standard errors are clustered on state. *Data sources:* Wage and demographic data from CPS monthly files; seasonally-adjusted state unemployment rates from BLS LAUS; seasonally-unadjusted EE separation rates from LEHD J2J Flows; Covid-19 death rates from CDC (2020); state-level inflation measures for 2015–2017 from Hazell et al. (2022); regional CPI measures from the BLS.

Figure 29: Annualized Percentage Changes in Real Hourly Earnings by Earnings Percentile Using State-Level Price Indices: 41, 24, and 12 Month Changes



Note: CPS monthly data. Wage percentiles are 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 1a: Wage Phillips Curve Coefficients, Overall and by Wage Quartiles

	(1)	(2)	(3)	(4)		(5)		(6)	
	Pooled *2015-2023*	Pooled *2015-2023*	Pooled *2015-2023*	Pre *2015-2019*	Post *2021-2023*	Pre *2015-2019*	Post *2021-2023*	Pre *2015-2019*	Post *2021-2023*
Overall	0.0264^{**}	0.0283^{*}	0.0254^{*}	0.0140	0.0257^{*}	0.0151	0.0284^{*}	0.0147	0.0293^{**}
	(0.0129)	(0.0149)	(0.0144)	(0.0193)	(0.0136)	(0.0186)	(0.0150)	(0.0194)	(0.0136)
Within wage quartiles									
Quartile 1	0.0463^{***}	0.0500^{***}	0.0413^{**}	0.0202	0.0455^{***}	0.0301	0.0506^{***}	0.0351	0.0435^{***}
	(0.0148)	(0.0165)	(0.0161)	(0.0203)	(0.0140)	(0.0227)	(0.0157)	(0.0232)	(0.0147)
Quartile 2	0.0242	0.0269	0.0244	0.0118	0.0238	0.0157	0.0272	0.0154	0.0277
	(0.0148)	(0.0192)	(0.0182)	(0.0208)	(0.0152)	(0.0217)	(0.0194)	(0.0235)	(0.0179)
Quartile 3	0.0211	0.0236	0.0254	0.0156	0.0209	0.0166	0.0238	0.0124	0.0301
	(0.0173)	(0.0199)	(0.0187)	(0.0233)	(0.0177)	(0.0206)	(0.0202)	(0.0207)	(0.0196)
Quartile 4	0.0235	0.0235	0.0237	0.0270	0.0236	0.0206	0.0236	0.0186	0.0256
	(0.0165)	(0.0161)	(0.0167)	(0.0226)	(0.0165)	(0.0195)	(0.0161)	(0.0197)	(0.0173)
Controls:									
Demographic		X	X			X	X	X	X
Δ Min. Wage:			X					X	X

Note: N=628. Table reports estimates of β from equation (5). We regress annualized log real wage change (between $t-1$ and t) on tightness (in $t-1$), where t is half-year periods from 2015 through the second half of 2023. The dependent variable is the change in log real wage and the main explanatory variable, tightness, is an average of the standardized EE separation rate and negative standardized unemployment rate, both measured at the state level. Regressions are estimated overall, and separately for each quartile. Columns 1–3 report estimates pooled over the full 2015–2023 period. In columns 4–6 the wage-Phillips curve coefficients are allowed to vary by pre- and post-pandemic periods (2015–2019, 2021–2023). All specifications include state and time fixed effects. Wage quartiles are estimated by state and half-year. In all specifications, we exclude observations that overlap with 2020 to avoid capturing pandemic-related wage effects. Demographic controls include state-by-period population shares of age groups, education groups, race (Black), Hispanic ethnicity, and sex. We also control for the state Covid-19 death rate per 100,000 people as of June 2023. In columns 3 and 6 we additionally control for changes in the average log state minimum wage in each period, again allowing the slope of this control to vary pre- and post-pandemic in column 6. Standard errors in parentheses are clustered on state. *Data sources:* Wage and demographic data from CPS monthly files; seasonally-adjusted state unemployment rates from BLS LAUS; seasonally-unadjusted EE separation rates from LEHD J2J Flows; Covid-19 death rates from CDC (2020); and changes in log state minimum wages from Vaghul and Zipperer (2019). * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table 1b: Wage Phillips Curve Coefficients for Demographic Subgroups

	(1)	(2)	(3)	(4)		(5)		(6)	
	Pooled *2015-2023*	Pooled *2015-2023*	Pooled *2015-2023*	Pre *2015-2019*	Post *2021-2023*	Pre *2015-2019*	Post *2021-2023*	Pre *2015-2019*	Post *2021-2023*
				A. Age-by-Education					
High School, under 40	0.0457***	0.0520***	0.0532***	0.0333*	0.0452***	0.0526**	0.0520***	0.0520**	0.0537***
	(0.0154)	(0.0156)	(0.0171)	(0.0193)	(0.0147)	(0.0208)	(0.0155)	(0.0205)	(0.0167)
High School, 40+	−0.0015	−0.0024	−0.0195	−0.0272	−0.0028	−0.0336	−0.0019	−0.0275	−0.0164
	(0.0216)	(0.0218)	(0.0230)	(0.0244)	(0.0220)	(0.0269)	(0.0221)	(0.0259)	(0.0242)
Some College, under 40	0.0217	0.0205	0.0179	0.0059	0.0212	0.0115	0.0207	0.0139	0.0196
	(0.0241)	(0.0235)	(0.0229)	(0.0277)	(0.0235)	(0.0288)	(0.0232)	(0.0311)	(0.0226)
Some College, 40+	−0.0088	−0.0182	−0.0200	−0.0078	−0.0088	−0.0104	−0.0186	−0.0055	−0.0259
	(0.0278)	(0.0308)	(0.0323)	(0.0341)	(0.0278)	(0.0379)	(0.0305)	(0.0382)	(0.0310)
Bachelor's, under 40	0.0460*	0.0467*	0.0415*	0.0262	0.0440*	0.0220	0.0455*	0.0200	0.0484*
	(0.0251)	(0.0273)	(0.0248)	(0.0232)	(0.0246)	(0.0267)	(0.0264)	(0.0261)	(0.0270)
Bachelor's, 40+	0.0067	0.0041	0.0087	−0.0096	0.0051	−0.0241	0.0029	−0.0319	0.0215
	(0.0198)	(0.0199)	(0.0254)	(0.0324)	(0.0209)	(0.0384)	(0.0209)	(0.0381)	(0.0224)
				B. Race					
Black/Hispanic	0.0385***	0.0473***	0.0329*	0.0114	0.0351***	0.0132	0.0459***	0.0136	0.0404***
	(0.0142)	(0.0168)	(0.0172)	(0.0178)	(0.0127)	(0.0240)	(0.0150)	(0.0251)	(0.0153)
White Non-Hispanic	0.0184	0.0176	0.0176	0.0173	0.0184	0.0230	0.0173	0.0230	0.0173
	(0.0166)	(0.0172)	(0.0172)	(0.0251)	(0.0166)	(0.0227)	(0.0166)	(0.0227)	(0.0166)
Controls:									
Demographic		X	X			X	X	X	X
Δ Min. Wage			X					X	X

Note: N=628. Table reports estimates of β from equation (5). We regress annualized log real wage change (between $t-1$ and t) on tightness (in $t-1$), where t is half-year periods from 2015 through the second half of 2023. The main explanatory variable, tightness, is calculated as the average of the standardized EE separation rate and negative standardized unemployment rate, both measured at the state level. Regressions are estimated separately for each age-by-education group in panel A, and for each group of race/ethnicity in panel B. Columns 1–3 report estimates pooled over the full 2015–2023 period. In columns 4–6 the wage-Phillips curve coefficients are allowed to vary by pre- and post-pandemic periods (2015–2019, 2021–2023). All specifications include state and time fixed effects, where time is half-year periods. In all specifications, we exclude observations that overlap with 2020 to avoid capturing pandemic-related wage effects. Demographic controls include state-by-period population shares of age groups, education groups, race (Black), Hispanic ethnicity and sex. We also control for the state Covid-19 death rate per 100,000 people as of June 2023. In columns 3 and 6 we additionally control for changes in the average log state minimum wage in each period, again allowing the slope of this control to vary pre- and post-pandemic in column 6. Standard errors in parentheses are clustered by state. *Data sources:* Wage and demographic data from CPS monthly files; seasonally-adjusted state unemployment rates from BLS LAUS; seasonally unadjusted EE separation rates from LEHD J2J Flows; Covid-19 death rates from CDC (2020); and changes in log state minimum wages from Vaghul and Zipperer (2019).
* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table 2: Coefficient on Tightness from Regressions of Wage Change on State Labor Market Tightness: Adjusting for Total Compensation

	(1)	(2)	(3)		(4)	
	Pooled	Pooled	Pre	Post	Pre	Post
	2015-2023	*2015-2023*	*2015-2019*	*2021-2023*	*2015-2019*	*2021-2023*
Overall	0.0265**	0.0285*	0.0144	0.0258*	0.0155	0.0286*
	(0.0129)	(0.0150)	(0.0193)	(0.0136)	(0.0185)	(0.0150)
Within wage quartiles						
Quartile 1	0.0463***	0.0500***	0.0202	0.0455***	0.0301	0.0506***
	(0.0148)	(0.0165)	(0.0203)	(0.0140)	(0.0227)	(0.0157)
Quartile 2	0.0242	0.0269	0.0118	0.0238	0.0157	0.0272
	(0.0148)	(0.0192)	(0.0208)	(0.0152)	(0.0217)	(0.0194)
Quartile 3	0.0211	0.0236	0.0156	0.0209	0.0166	0.0238
	(0.0173)	(0.0199)	(0.0233)	(0.0177)	(0.0206)	(0.0202)
Quartile 4	0.0241	0.0245	0.0283	0.0242	0.0223	0.0246
	(0.0167)	(0.0162)	(0.0226)	(0.0166)	(0.0193)	(0.0163)
Within age and education groups						
High School, under 40	0.0457***	0.0521***	0.0334*	0.0453***	0.0526**	0.0521***
	(0.0155)	(0.0157)	(0.0194)	(0.0148)	(0.0209)	(0.0156)
High School, 40+	−0.0014	−0.0024	−0.0267	−0.0027	−0.0331	−0.0019
	(0.0218)	(0.0219)	(0.0246)	(0.0222)	(0.0271)	(0.0221)
Some College, under 40	0.0219	0.0208	0.0060	0.0214	0.0117	0.0210
	(0.0243)	(0.0237)	(0.0279)	(0.0237)	(0.0289)	(0.0233)
Some College, 40+	−0.0088	−0.0185	−0.0076	−0.0088	−0.0103	−0.0189
	(0.0280)	(0.0309)	(0.0341)	(0.0280)	(0.0379)	(0.0307)
Bachelor's, under 40	0.0461*	0.0469*	0.0264	0.0440*	0.0222	0.0457*
	(0.0253)	(0.0276)	(0.0231)	(0.0247)	(0.0267)	(0.0266)
Bachelor's, 40+	0.0072	0.0047	−0.0085	0.0057	−0.0229	0.0035
	(0.0201)	(0.0201)	(0.0325)	(0.0211)	(0.0384)	(0.0210)
Controls:		X			X	X

Note: N=628. Table reports estimates of β from equation (5). We regress the change in annualized log total compensation (between $t-1$ and t) on tightness (in $t-1$), where t is half-year periods from 2015 through the second half of 2023. Compensation is the sum of log real wage and the amenity value of WFH. The main explanatory variable, tightness, is calculated as the average of the standardized EE separation rate and negative standardized unemployment rate, both measured at the state level. Regressions are estimated separately for each quartile and age-by-education group. Columns 1 and 2 report estimates pooled over the full 2015–2023 period. In columns 3 and 4, the wage-Phillips curve coefficients are allowed to vary by pre- and post-pandemic periods (2015–2019, 2021–2023). All specifications include state and time fixed effects. In all specifications, we exclude observations that overlap with 2020 to avoid capturing pandemic-related wage effects. Demographic controls include state-by-period population shares of age groups, education groups, race (Black), Hispanic ethnicity and sex. We also control for the state Covid-19 death rate per 100,000 people as of June 2023. Standard errors in parentheses are clustered on state. *Data sources:* Wage and demographic data from CPS monthly files; seasonally-adjusted state unemployment rates from BLS LAUS; seasonally unadjusted EE separation rates from LEHD J2J Flows; Covid-19 death rates from CDC (2020); and WFH share data from Hansen et al. (2023).

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table 3a: Employment-to-Employment Separation Elasticity Estimates

	Individual-level Wage	Industry Wage Premium	
	(1)	(2)	(3)
	Overall		
2015–2019	-0.2725^{***}	-1.0112^{***}	-0.6788^{***}
	(0.0437)	(0.1834)	(0.1357)
	N=80,729	*N=1,921,670*	*N=1,921,670*
2021–2023	-0.2700^{***}	-0.9803^{***}	-0.6096^{***}
	(0.0660)	(0.2221)	(0.1380)
	N=33,746	*N=735,378*	*N=735,378*
	High School Educated, Under 40 Years Old		
2015–2019	-0.3169^{**}	-0.7891^{***}	-0.5460^{***}
	(0.1290)	(0.1366)	(0.1334)
	N=10,020	*N=284,569*	*N=284,569*
2021–2023	-0.5508^{***}	-1.0302^{***}	-0.7662^{***}
	(0.1817)	(0.1395)	(0.1523)
	N=3,825	*N=110,086*	*N=110,086*
	High School Educated, 40 Years and Older		
2015–2019	-0.3742^{***}	-0.4712^{**}	-0.4246^{**}
	(0.1245)	(0.2235)	(0.1776)
	N=14,232	*N=334,735*	*N=334,735*
2021–2023	-0.1973	-0.5882^{***}	-0.4751^{**}
	(0.1906)	(0.2228)	(0.1902)
	N=5,112	*N=117,190*	*N=117,190*
Aggregation Level	Individual	3-digit Ind.	3-digit Ind.
Time Interval	3-month	Monthly	Monthly
Controls	X		X

Note: Separation elasticities are estimated in two steps: first by regressing an indicator for EE separation on logged real wage (column 1) or industry wage premia (columns 2 and 3), and then by dividing the coefficient on wage from these regressions by the mean EE separation rate for the corresponding period and subgroup. Column 1 is based on equation (6): the independent variable is log real wage and the dependent variable is a 3-month measure of EE separation. Columns 2 and 3 are based on equation (7): 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 2) and with controls (col 3). Columns 1 and 3 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 2 and 3.

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table 3b: Employment-to-Employment Separation Elasticity Estimates

	Individual-level Wage	Industry Wage Premium	
	(1)	(2)	(3)
Bachelor's Degree or Higher, Under 40 Years Old			
2015–2019	-0.2604^{**}	-0.8795^{***}	-0.8154^{***}
	(0.1051)	(0.2528)	(0.2248)
	N=14,644	*N=319,962*	*N=319,962*
2021–2023	-0.2826^{**}	-0.6788^{***}	-0.6243^{***}
	(0.1368)	(0.2388)	(0.1986)
	N=7,046	*N=135,359*	*N=135,359*
Bachelor's Degree or Higher, 40 Years and Older			
2015–2019	-0.1631^{*}	-0.4283^{**}	-0.4030^{**}
	(0.0838)	(0.1797)	(0.1707)
	N=19,346	*N=409,955*	*N=409,955*
2021–2023	0.0089	-0.3993^{**}	-0.4071^{**}
	(0.1417)	(0.1716)	(0.1587)
	N=9,330	*N=173,475*	*N=173,475*
Aggregation Level	Individual	3-digit Ind.	3-digit Ind.
Time Interval	3-month	Monthly	Monthly
Controls	X		X

Note: Separation elasticities are estimated in two steps: first by regressing an indicator for EE separation on logged real wage (column 1) or industry wage premia (columns 2 and 3), and then by dividing the coefficient on wage from these regressions by the mean EE separation rate for the corresponding period and subgroup. Column 1 is based on equation (6): the independent variable is log real wage and the dependent variable is a 3-month measure of EE separation. Columns 2 and 3 are based on equation (7): 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 2) and with controls (col 3). Columns 1 and 3 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 2 and 3.

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table 4: Employment-to-Employment Separation Elasticity Estimates at Different Values of Industry Wage Premia

	(1) IWP=-0.3	(2) IWP=0	(3) IWP=0.3	(4) IWP=-0.3	(5) IWP=0	(6) IWP=0.3
Overall						
2015-19	-1.2200^{***}	-1.0391^{***}	-0.4451	-0.7166^{***}	-0.6837^{***}	-0.5842
	(0.2403)	(0.1524)	(0.5426)	(0.2160)	(0.1322)	(0.4331)
2021-23	-1.3940^{***}	-1.0446^{***}	-0.0603	-0.8954^{***}	-0.6318^{***}	-0.1483
	(0.2763)	(0.1546)	(0.5050)	(0.2086)	(0.1135)	(0.3643)
Difference	-0.1740	-0.0055	0.3848	-0.1788	0.0518	0.4360
	(0.3658)	(0.2169)	(0.7404)	(0.2999)	(0.1741)	(0.5653)
High School Educated, Under 40 Years Old						
2015-19	-0.6372^{***}	-0.7889^{***}	-1.0351^{***}	-0.2621	-0.5151^{***}	-0.9064^{***}
	(0.2318)	(0.1350)	(0.3626)	(0.2238)	(0.1321)	(0.3245)
2021-23	-1.2257^{***}	-1.1364^{***}	-0.6845	-0.9415^{***}	-0.8328^{***}	-0.5335
	(0.2789)	(0.1283)	(0.4470)	(0.3040)	(0.1472)	(0.4086)
Difference	-0.5885	-0.3475^{*}	0.3506	-0.6794^{*}	-0.3177	0.3729
	(0.3622)	(0.1860)	(0.5749)	(0.3770)	(0.1975)	(0.5211)
High School Educated, 40 Years and Older						
2015-19	-0.7045^{*}	-0.5037^{**}	-0.1780	-0.6779^{**}	-0.4596^{**}	-0.1232
	(0.3789)	(0.2416)	(0.3096)	(0.3449)	(0.1968)	(0.2803)
2021-23	-0.8283^{**}	-0.6241^{***}	-0.2446	-0.7198^{**}	-0.5070^{***}	-0.1617
	(0.3765)	(0.2296)	(0.4184)	(0.3491)	(0.1929)	(0.3838)
Difference	-0.1239	-0.1204	-0.0666	-0.0419	-0.0474	-0.0384
	(0.5336)	(0.3329)	(0.5199)	(0.4902)	(0.2752)	(0.4746)
Bachelor's Degree or Higher, Under 40 Years Old						
2015-19	-1.1425^{***}	-0.7222^{***}	0.0926	-1.0592^{***}	-0.6572^{***}	0.0757
	(0.1096)	(0.2408)	(0.5572)	(0.1070)	(0.2211)	(0.5041)
2021-23	-1.0725^{***}	-0.5525^{***}	0.2910	-0.9687^{***}	-0.5013^{***}	0.2256
	(0.0967)	(0.1863)	(0.4209)	(0.0992)	(0.1666)	(0.3795)
Difference	0.0700	0.1697	0.1984	0.0905	0.1559	0.1500
	(0.1460)	(0.3041)	(0.6976)	(0.1457)	(0.2766)	(0.6303)
Bachelor's Degree or Higher, 40 Years and Older						
2015-19	-0.6044^{***}	-0.3480	0.0035	-0.5790^{***}	-0.3196	0.0264
	(0.1852)	(0.2259)	(0.5394)	(0.1833)	(0.2135)	(0.4964)
2021-23	-0.5166^{*}	-0.3507^{*}	-0.1202	-0.4998^{*}	-0.3624^{**}	-0.1682
	(0.2663)	(0.1959)	(0.5289)	(0.2569)	(0.1767)	(0.4774)
Difference	0.0877	-0.0027	-0.1237	0.0792	-0.0428	-0.1946
	(0.3240)	(0.2987)	(0.7546)	(0.3152)	(0.2769)	(0.6878)
Controls:				X	X	X

Note: 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 $t-1$ and its square (based on equation (7)). In columns 4-6, we also control for age groups, education categories, race categories, Hispanic ethnicity, citizenship, sex, and metro area status. In the second step, 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. 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. Demographic controls from this regression are the same as above but rather than age groups include controls for age, age^2, and age^3. Standard errors are clustered at the industry level. The third row of each panel in Table 4 reports the difference between the coefficients in rows 1 and 2. Standard errors in parentheses clustered by industry. *Data sources:* CPS monthly data.

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table 5: Decomposition of the Change in Annual Wage Growth During 2021 – 2023 vs. 2015 – 2019

	High-School under-40		All others	
	(1) 2015-2019	(2) 2021-2023	(3) 2015-2019	(4) 2021-2023
A. Job Change Rates (% points)				
Pr(Mover in last qtr)	7.64	8.42	5.37	5.57
Pr(Mover in past year)	27.23	29.66	19.82	20.48
B. Mean Log Wage Changes by Switcher Status (log points)				
E(Wage change)	4.63	3.69	3.46	1.20
E(Wage change | Job move)	4.67	7.80	6.25	3.88
E(Wage change | No job move)	4.61	1.96	2.77	0.52
C. Decomposition of Wage Change: 2021-23 vs. 2015-19 (log points)				
Contribution of job-movers		0.85		-0.47
Contribution of job-stayers		-1.93		-1.80
Contribution of move rate		0.14		0.02
Total		-0.94		-2.25

Note: The table decomposes the change in annual wage growth between 2015–2019 and 2021–2023 into the contribution of job moving and job staying based on Equation (9). The decomposition is done separately for HS under 40, and the complementary group. Panels A and B in table report inputs for calculating the components of the decomposition from equation (9). Panel A reports the quarterly probability of moving, δ_T^3, in row 1, and the annual probability of moving, δ_T, in row 2. Panel B reports annual wage growth overall, Δw_T, in row 1, for job movers, Δw_T^M, in row 2, and for job stayers Δw_T^S in row 3. Panel C reports the components of the decomposition. The total change in wage growth (row 4) of the change in wage growth associated with job moving (row 1), with job staying (row 2) and with a change in the move rate (row 3). The portion of the total change due to the change in the wage growth of switchers is equal to the difference in the wage growth of job-switchers scaled by the switch rate in the first period. Finally, the contribution of the change in the switch rate is defined as the difference in the switch rate between the two periods scaled by the difference between mover and stayer wage growth in period 2 (2021–2023). *Data sources:* CPS monthly data.

Table 6: Price- and Wage-Phillips Curve Estimates:
Regressions of Δ Log CPI on Various Measures of State Labor Market Tightness

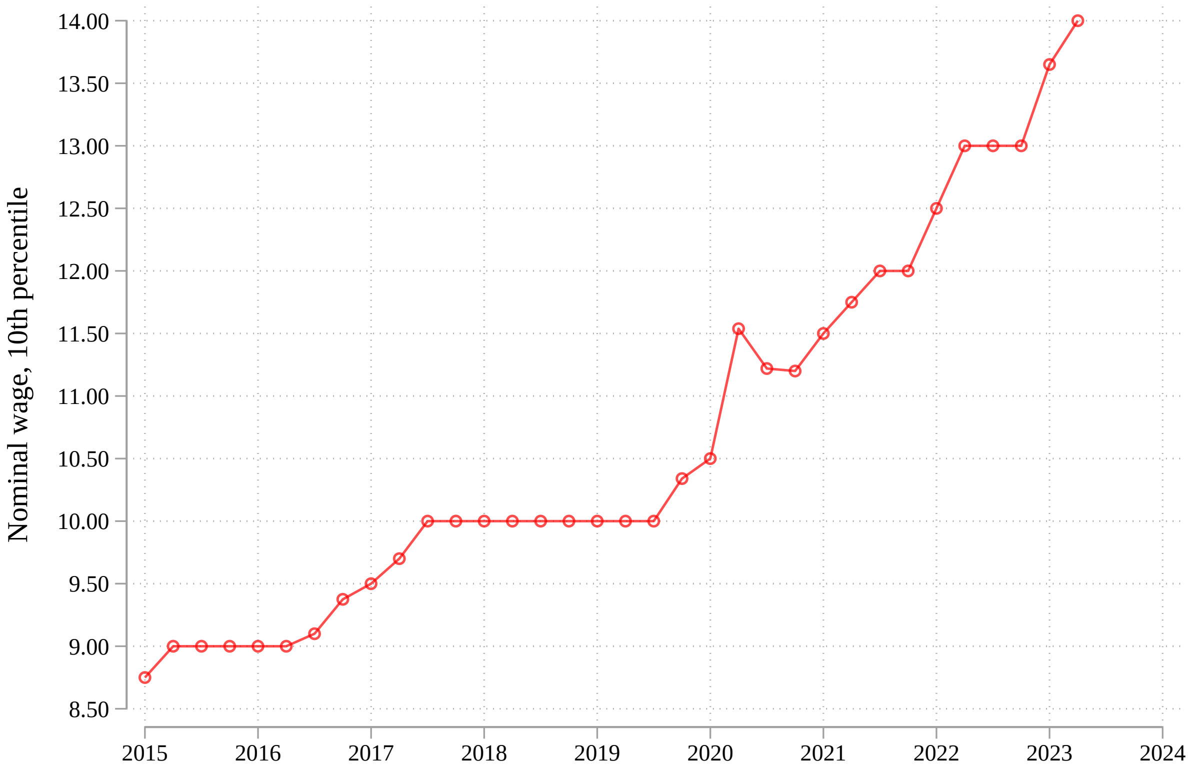
	(1)	(2)	(3)		(4)	
	Pooled *2015-2023*	Pooled *2015-2023*	Pre *2015-2019*	Post *2021-2023*	Pre *2015-2019*	Post *2021-2023*
A. Price-Phillips Curve Estimates						
Tightness	0.0161^{***}	0.0159^{***}	0.0037	0.0147^{***}	0.0054	0.0155^{***}
	(0.0042)	(0.0039)	(0.0025)	(0.0028)	(0.0032)	(0.0030)
B. Wage-Phillips Curve Estimates - National Price Adjustment						
Overall	0.0316^{**}	0.0341^{**}	0.0252	0.0309^{**}	0.0199	0.0335^{**}
	(0.0138)	(0.0161)	(0.0197)	(0.0143)	(0.0190)	(0.0159)
High School, under 40	0.0528^{***}	0.0616^{***}	0.0407^{*}	0.0517^{***}	0.0606^{**}	0.0616^{***}
	(0.0163)	(0.0167)	(0.0226)	(0.0159)	(0.0256)	(0.0168)
Quartile 1	0.0561^{***}	0.0607^{***}	0.0314^{*}	0.0540^{***}	0.0418^{*}	0.0602^{***}
	(0.0135)	(0.0148)	(0.0187)	(0.0130)	(0.0228)	(0.0141)
Quartile 2	0.0274^{*}	0.0308	0.0167	0.0264	0.0153	0.0303
	(0.0152)	(0.0203)	(0.0239)	(0.0161)	(0.0252)	(0.0206)
Quartile 3	0.0238	0.0272	0.0273	0.0241	0.0180	0.0270
	(0.0194)	(0.0219)	(0.0263)	(0.0194)	(0.0247)	(0.0218)
Quartile 4	0.0273	0.0279	0.0428^{*}	0.0286	0.0298	0.0280
	(0.0187)	(0.0189)	(0.0236)	(0.0182)	(0.0207)	(0.0188)
C. Wage-Phillips Curve Estimates - Regional Price Adjustment						
Overall	0.0155	0.0181	0.0215	0.0162	0.0145	0.0179
	(0.0138)	(0.0159)	(0.0190)	(0.0138)	(0.0194)	(0.0159)
High School, under 40	0.0373^{**}	0.0462^{***}	0.0376^{*}	0.0373^{**}	0.0560^{**}	0.0464^{***}
	(0.0157)	(0.0163)	(0.0224)	(0.0161)	(0.0258)	(0.0164)
Quartile 1	0.0402^{***}	0.0446^{***}	0.0273	0.0391^{***}	0.0358	0.0444^{***}
	(0.0124)	(0.0144)	(0.0181)	(0.0126)	(0.0220)	(0.0142)
Quartile 2	0.0115	0.0147	0.0126	0.0116	0.0092	0.0146
	(0.0162)	(0.0210)	(0.0236)	(0.0165)	(0.0258)	(0.0212)
Quartile 3	0.0080	0.0111	0.0232	0.0093	0.0119	0.0112
	(0.0203)	(0.0220)	(0.0259)	(0.0193)	(0.0258)	(0.0218)
Quartile 4	0.0114	0.0118	0.0387^{*}	0.0137	0.0238	0.0121
	(0.0190)	(0.0182)	(0.0231)	(0.0174)	(0.0217)	(0.0180)
Controls:		X			X	X

Note: N=374. Table reports estimates of β from equation (10) in panel A, and from our wage-Phillips equation (5) in panels B and C. We regress annualized log CPI change (Panel A) or real wage change (Panel B) between $t-1$ and t) on tightness (in $t-1$), where t is half-year periods from 2015 through the second half of 2023. The main explanatory variable, tightness, is calculated as the average of the standardized EE separation rate and negative standardized unemployment rate, both measured at the state level. Columns 1–2 report estimates pooled over the full 2015–2023 period. In columns 4–6 we interact our coefficient of interest with indicators for the pre- and post-pandemic periods (2015-2019, 2021-2023) in order to get the price/wage-Phillips curve coefficient for each period. All specifications include state and time fixed effects. In all specifications, we exclude observations that overlap with 2020 to avoid capturing pandemic-related wage effects. Additionally, we exclude wage and price changes from the second half of 2017 to the first half of 2018, as we cannot construct a consistent price series between these periods. Demographic controls include state-by-period population shares of age groups, education groups, race (Black), Hispanic ethnicity and sex. We also control for the state Covid-19 death rate per 100,000 people as of June 2023. Standard errors are clustered on state. *Data sources:* Wage and demographic data from CPS monthly files; seasonally-adjusted state unemployment rates from BLS LAUS; seasonally-unadjusted EE separation rates from LEHD J2J Flows; Covid-19 death rates from CDC (2020); state-level inflation measures for 2015–2017 from Hazell et al. (2022); regional CPI measures from the BLS. * $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Appendix

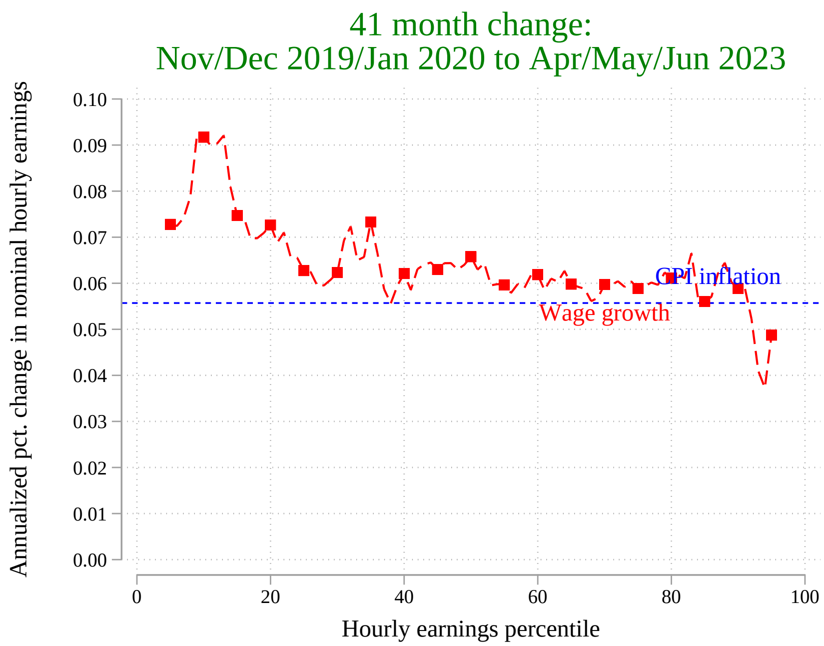
A1 Appendix Figures and Tables

Figure A1: 10^{th} Percentile of CPS Nominal Hourly Wage Distribution, 2015 – 2023



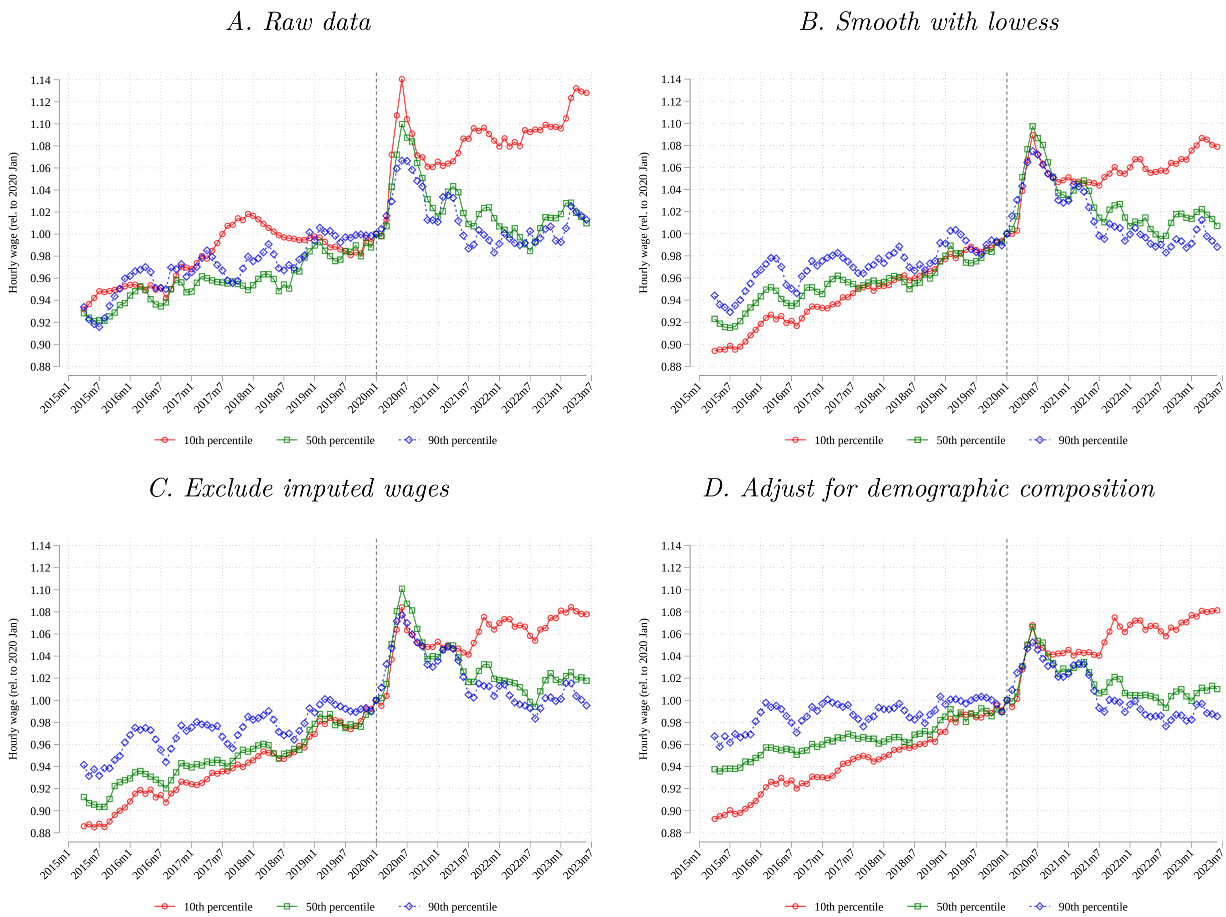
Note: Quarterly aggregation of CPS monthly data.

Figure A2: Changes in Nominal Wages by Percentile (Unsmoothed), January 2020 – June 2023



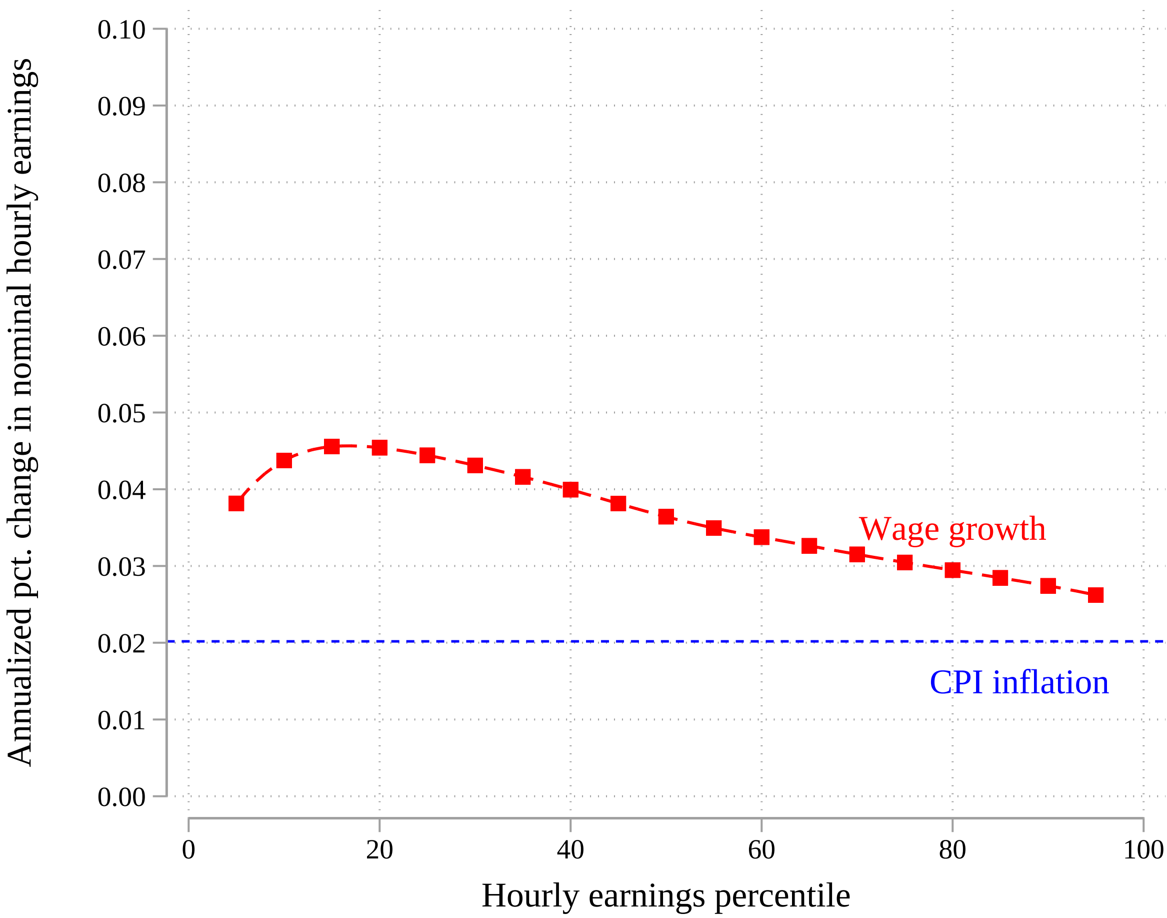
Note: CPS monthly data. Inflation is calculated using seasonally unadjusted CPI-U.

Figure A3: Trends in Real Hourly Wages at Various Percentiles 2015 – 2023:
Raw and Adjusted Data Series



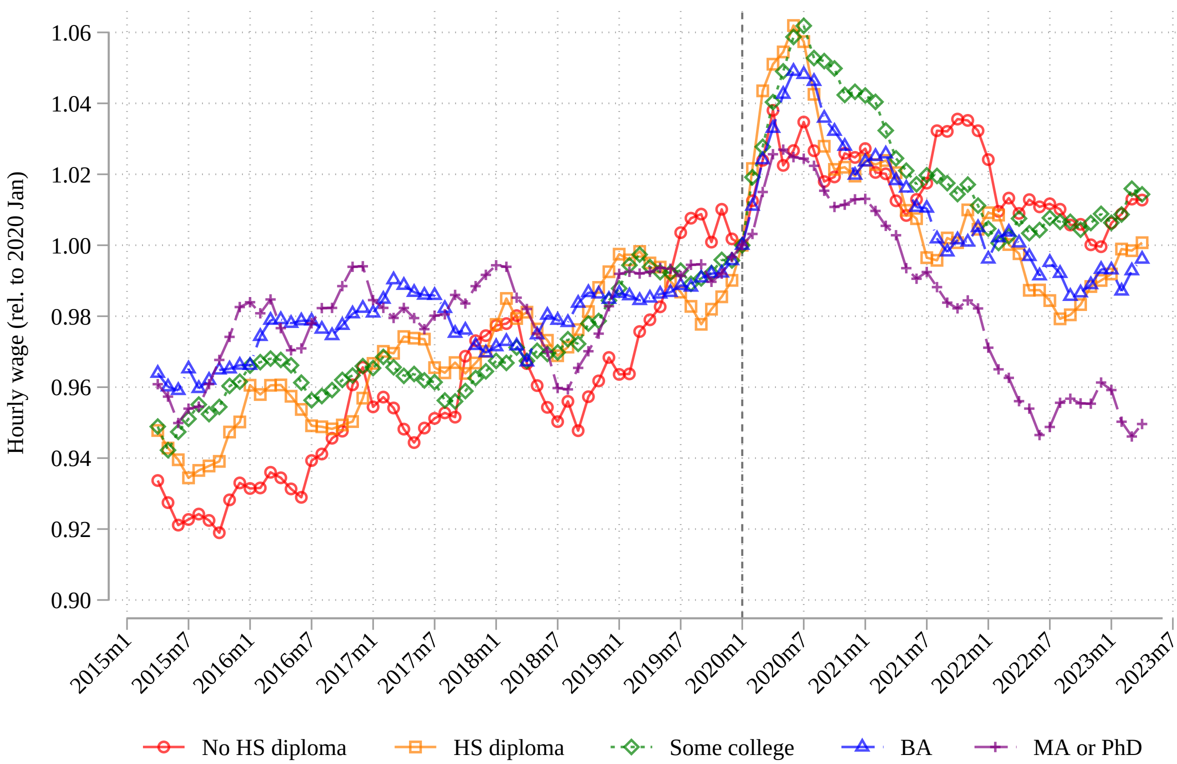
Note: CPS monthly data. Wages are real (June 2023). Wage quantiles are constructed by month and are smoothed with a 3-month moving average. In panel B, wage percentiles are also smoothed with lowess. Panel C is our preferred method of adjustment, where imputed wages are excluded before smoothing with lowess and the moving average. In panel D, in addition to smoothing and excluding imputed wages, we also adjust wages to maintain the same demographic composition in January–March 2020 using inverse probability weighing based on age, education, race/ethnicity, gender, country of birth, and region .

Figure A4: Changes in Nominal Wages by Percentile, December 2015 – December 2019



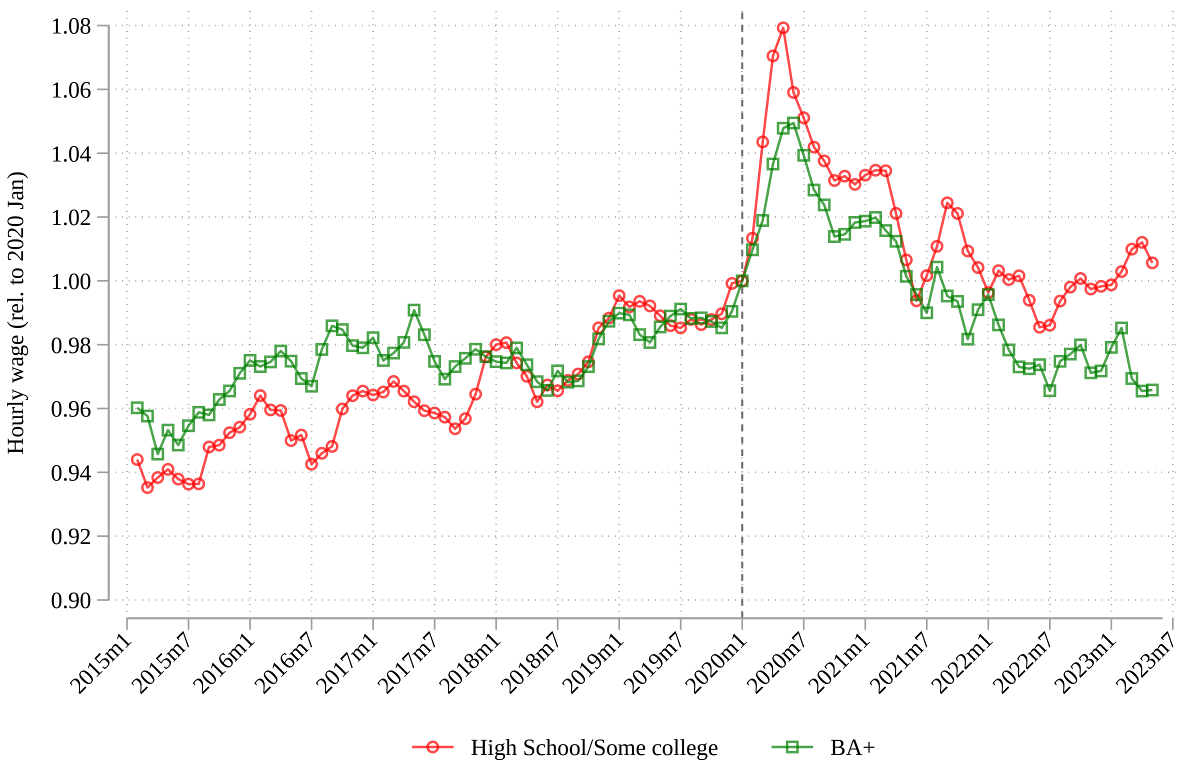
Note: CPS monthly data. Wage percentiles smoothed with lowess. Inflation is calculated using seasonally unadjusted CPI-U. Start and end point observations pool data from October through December 2015 and October through December 2019, respectively.

Figure A5: Trends in Real Hourly Wages by Detailed Education Group 2015 – 2023, Relative to January 2020



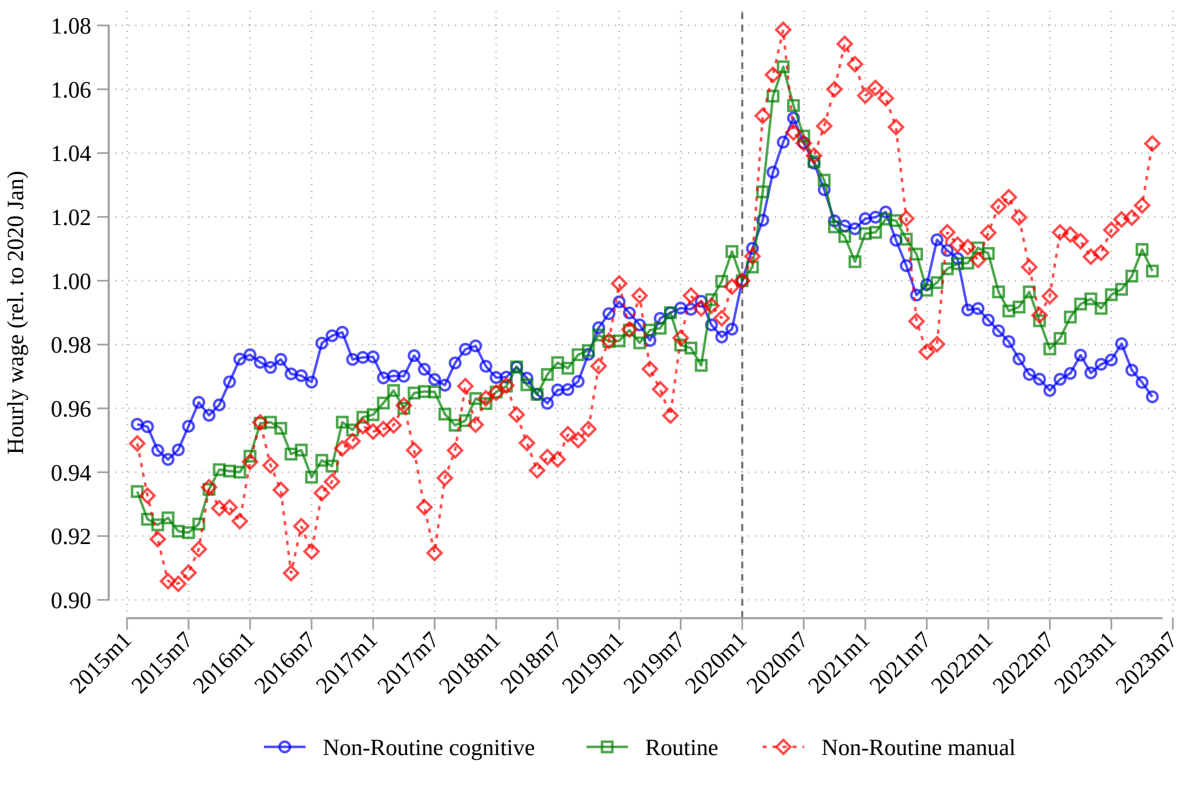
Note: CPS monthly data. Wages are real (June 2023 USD) and smoothed with a 6-month moving average.

Figure A6: Trends in Real Hourly Wages by Education Levels (Non-BA vs. BA+) 2015 – 2023, Relative to January 2020



Note: CPS monthly data. Wages are real (June 2023 USD) and smoothed with a 3-month moving average.

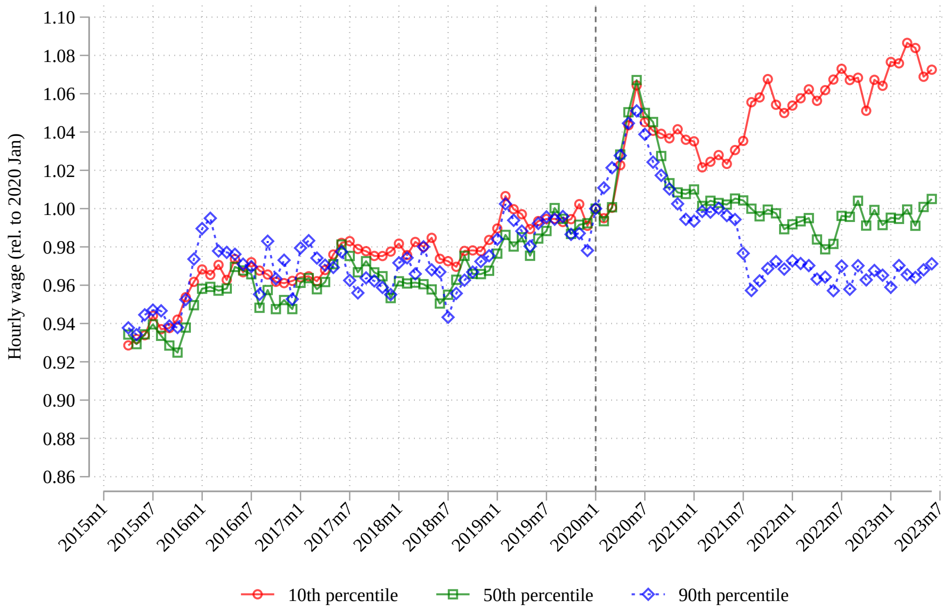
Figure A7: Trends in Real Hourly Wages by Occupational Task Group 2015 – 2023, Relative to January 2020



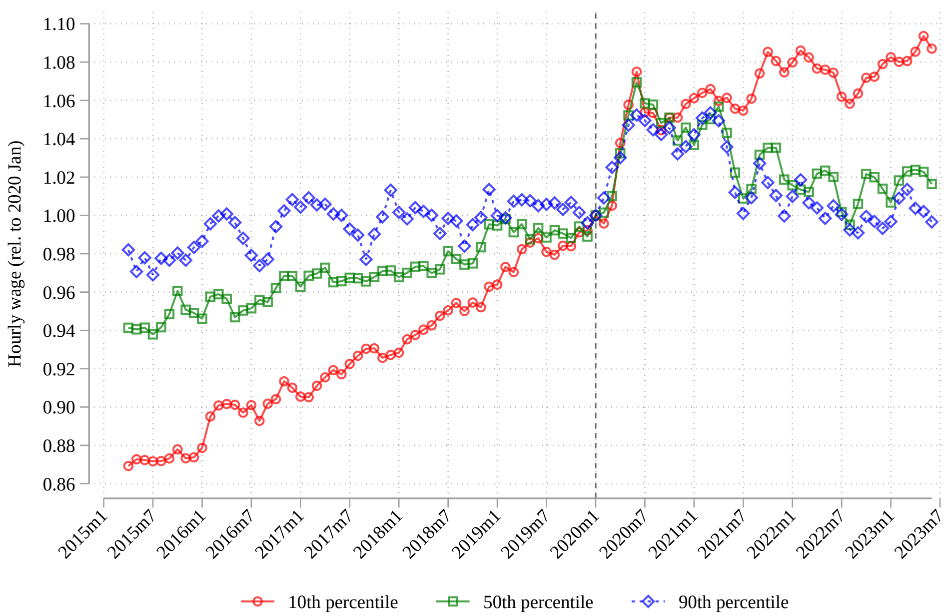
Note: CPS monthly data. Wages are real (June 2023 USD) and smoothed with a 3-month moving average. Occupation task types are identified following Jaimovich and Siu (2020).

Figure A8: Trends in Real Hourly Wages by Quantile and State Minimum Wage Status 2015 – 2023, Relative to January 2020: Holding Demographic Composition Constant

A. Federal or no minimum wage

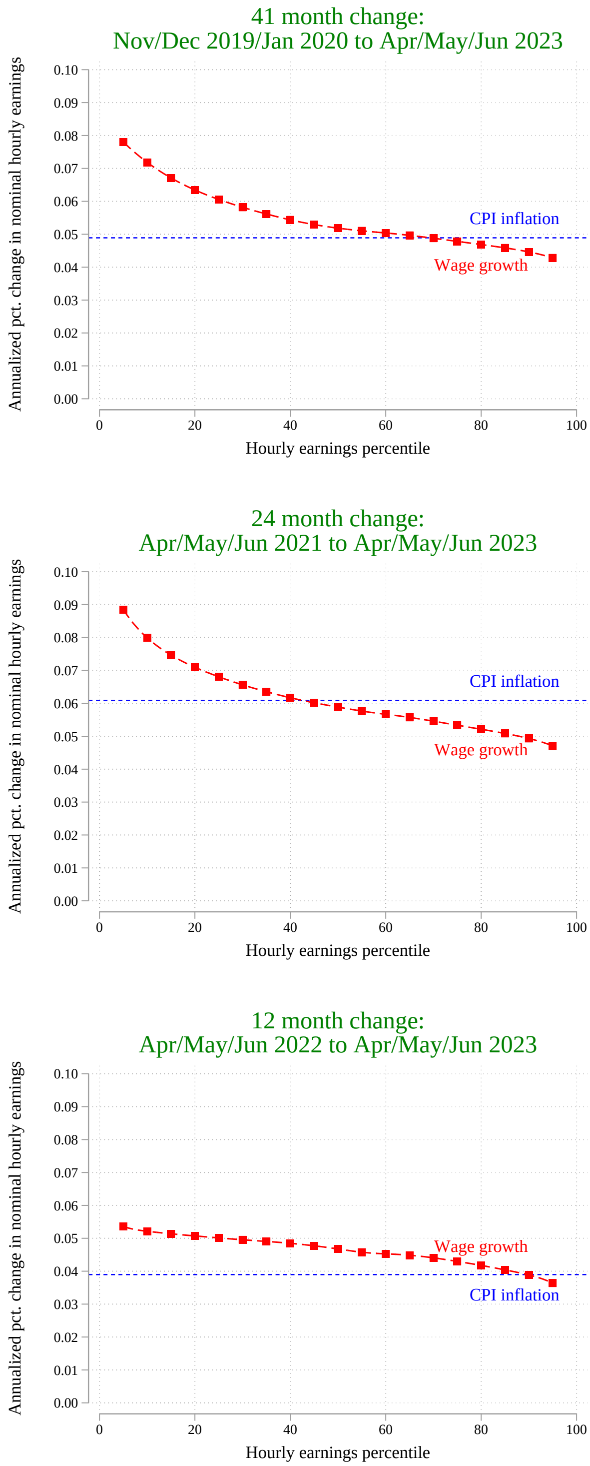


B. State minimum wage above federal level



Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020 using inverse probability weighting based on age, education, race/ethnicity, gender, country of birth, and region. Wages are real (June 2023 USD). Wage quantiles are constructed by month and are smoothed with lowess and a 3-month moving average. In panels B and C, we identify thirty US states (including Washington DC) with a minimum wage above the federal level in 2019. Sixteen states have a minimum wage equal to the federal level, $7.25, and 5 states have no minimum wage.

Figure A9: Annualized Percentage Changes in Nominal Hourly Earnings by Percentile, Adjusted for Demographic Composition: Changes Over 41, 24 and 12 Months



Note: CPS monthly data. Adjusted to maintain demographic composition in January–March 2020. Wage percentiles are smoothed with lowess. Inflation is calculated using annualized, seasonally unadjusted CPI-U.

Table A1: Log 90/10, 90/50, and 50/10 Ratios Over Time

	(1) ln(90/10)	(2) ln(90/50)	(3) ln(50/10)
	A. The Great Compression		
1940	1.609	0.767	0.842
1950	1.373	0.566	0.807
Δ	−0.237	−0.202	−0.035
	B. The Great Divergence		
1979	1.267	0.670	0.596
2019	1.552	0.858	0.694
Δ	0.285	0.188	0.098
	C. The Unexpected Compression		
2019	1.552	0.858	0.694
2023	1.466	0.831	0.635
Δ	−0.086	−0.027	−0.059

Note: Panel A uses 1% samples of 1940 and 1950 decennial Census data from IPUMS. Panels B and C use CPS monthly data from NBER (1979) and IPUMS (2019, 2023). For each analysis, the sample is limited to individuals between 16-64 years old. For the latter two periods, imputed wages are excluded and percentiles are smoothed using lowess. For each panel, the first two rows display the log ratio for the start and end of the time period. For each panel, the third row reports the difference in the log ratio between the start and end of the time period. For panel C, 2023 includes January - June 2023.

Table A2: Coefficient on Tightness from Regressions of Wage Change on State Labor Market Tightness - Historical Estimates

	(1)	(2)	(3)		(4)	
	Pooled *1980-2023*	Pooled *1980-2023*	Pre *1980-2019*	Post *2021-2023*	Pre *1980-2019*	Post *2021-2023*
Overall	0.0073***	0.0078***	0.0070***	0.0161***	0.0075***	0.0165***
	(0.0007)	(0.0008)	(0.0007)	(0.0046)	(0.0008)	(0.0049)
Quartile 1	0.0073***	0.0097***	0.0084***	0.0263***	0.0093***	0.0289***
	(0.0007)	(0.0009)	(0.0007)	(0.0062)	(0.0009)	(0.0062)
High School, under 40	0.0072***	0.0104***	0.0086***	0.0275***	0.0100***	0.0326***
	(0.0007)	(0.0011)	(0.0009)	(0.0066)	(0.0010)	(0.0065)
Controls:		X			X	X

Note: N=4,233. Table reports estimates of β from equation (5). For the full 1980–2023 period, we regress annualized log real wage change (between $t1$ and t) on negative standardized unemployment (in $t1$), where t is half-year periods.The dependent variable is the change in log real wage and the main explanatory variable, tightness, is an average of the standardized EE separation rate and negative standardized unemployment rate, both measured at the state level. Regressions are estimated overall, and separately for each demographic group and quartile. Columns 1 and 2 report estimates pooled over the full 1980–2023 period. In columns 3 and 4 we interact our coefficient of interest with an indicator for the post-pandemic period, to estimate the effect separately over the 1980–2019 and 2021–2023 period. All specifications include state and time fixed effects. Wage quartiles are estimated by state and half-year. In all specifications, we exclude observations that overlap with 2020 to avoid capturing pandemic-related wage effects. Demographic controls include include state-by-period population shares of age groups, education groups, race (Black) indicator, Hispanic ethnicity and sex. We also control for the state Covid-19 death rate per 100,000 people as of June 2023. Standard errors in parentheses are clustered on state. *Data Sources:* Wage and demographic data from CPS monthly files, data for 1980–1981 obtained from NBER and data from 1982 onwards from IPUMS; seasonally-adjusted state unemployment rates from BLS LAUS; and seasonally-unadjusted EE separation rates from LEHD J2J Flows.

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table A3: Coefficients from Regressions of Change in the WFH share on Measures of State Labor Market Tightness

	(1)	(2)
Tightness	0.0155***	0.0201***
	(0.0054)	(0.0070)
Std. -Unemp	0.0096***	0.0116**
	(0.0035)	(0.0048)
Std. EE sep	0.0002	0.0026
	(0.0077)	(0.0084)
Controls:		X

Note: N=181. Table reports estimates from a regression of the change in work-from-home shares (between $t-1$ and t) on tightness (at $t-1$) from 2021-2023, where t is half-year periods. Tightness is calculated as the average of the standardized EE separation rate and the negative, standardized unemployment rate, measured at the state level. We include state and time fixed effects. All specifications include state and time fixed effects. The specification in column 2 includes as controls state-by-period population shares of age groups, education groups, race (Black), Hispanic ethnicity and sex. We also control for the state Covid-19 death rate per 100,000 people as of June 2023. Standard errors in parentheses are clustered on state. *Data Sources:* Wage and demographic data from CPS monthly files; seasonally-adjusted state unemployment rates from BLS LAUS; seasonally-unadjusted EE separation rates from LEHD J2J Flows; Covid-19 death rates from CDC (2020); and WFH share data are obtained from Hansen et al. (2023).

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table A4: Relationship Between Employment-to-Employment Separations and Industry Wage Premia

	Overall		HS, under 40		All others	
	(1)	(2)	(3)	(4)	(5)	(6)
Period 1: 2015–2019						
Ind. Wage Premium	-0.0208^{***}	-0.0141^{***}	-0.0236^{***}	-0.0155^{***}	-0.0162^{***}	-0.0131^{***}
	(0.0031)	(0.0027)	(0.0040)	(0.0040)	(0.0031)	(0.0029)
Ind. Wage Premium^2	0.0230^{*}	0.0069	-0.0001	-0.0111	0.0178^{*}	0.0078
	(0.0119)	(0.0099)	(0.0123)	(0.0111)	(0.0099)	(0.0087)
	N=1,921,670		*N=284,569*		*N=1,629,695*	
Period 2: 2021–2023						
Ind. Wage Premium	-0.0212^{***}	-0.0132^{***}	-0.0350^{***}	-0.0258^{***}	-0.0143^{***}	-0.0107^{***}
	(0.0031)	(0.0024)	(0.0040)	(0.0046)	(0.0030)	(0.0025)
Ind. Wage Premium^2	0.0337^{***}	0.0174^{*}	0.0319^{*}	0.0207	0.0237^{**}	0.0143^{*}
	(0.0130)	(0.0095)	(0.0178)	(0.0173)	(0.0102)	(0.0085)
	N=735,378		*N=110,086*		*N=622,652*	
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 (based on equation 7). Specifications in even numbered columns include state fixed effects; controls include indicators for race, Hispanic 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. Estimates for the age and education groups included in "All others" (columns 5 and 6) are reported in Table A5. Estimates from this table are used for calculating the elasticities reported in Figure 23, panel A of Figure 24, and in the first two panels of Table 4. 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 $x = \{-.3, 0, .3\}$. Standard errors in parentheses are clustered by industry.

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table A5: Relationship Between Employment-to-Employment Separations 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	-0.0081^{**}	-0.0074^{**}	-0.0142^{***}	-0.0130^{***}	-0.0053	-0.0048
	(0.0039)	(0.0032)	(0.0047)	(0.0044)	(0.0034)	(0.0032)
Ind. Wage Premium^2	0.0092	0.0093	0.0265^{***}	0.0240^{***}	0.0089	0.0087
	(0.0078)	(0.0075)	(0.0093)	(0.0085)	(0.0081)	(0.0075)
	N=334,735		*N=319,962*		*N=409,955*	
			Period 2: 2021–2023			
Ind. Wage Premium	-0.0107^{***}	-0.0088^{***}	-0.0106^{***}	-0.0097^{***}	-0.0053^{*}	-0.0054^{**}
	(0.0040)	(0.0033)	(0.0036)	(0.0032)	(0.0029)	(0.0027)
Ind. Wage Premium^2	0.0118	0.0104	0.0267^{***}	0.0232^{***}	0.0060	0.0052
	(0.0101)	(0.0096)	(0.0075)	(0.0069)	(0.0090)	(0.0083)
	N=117,190		*N=135,359*		*N=173,475*	
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 (based on equation 7). Specifications in even numbered columns include state fixed effects; controls include indicators for race, Hispanic 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. Demographic controls used to calculate IWP are the same as the ones above but rather than controlling for age groups, the regression controls for age, age^2 and age^3. Estimates from this table are used for calculating the elasticities reported in panels B through D of Figure 24 and in the last three panels of Table 4. 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 4 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 A6: Poisson Estimates of the Relationship Between Employment-to-Employment Separations and Industry Wage Premia

	Overall	HS, under 40	HS, 40 +	BA+, under 40	BA+, 40+
	(1)	(2)	(3)	(4)	(5)
			Period 1: 2015–2019		
Ind. Wage Premium	-0.6755^{***}	-0.5361^{***}	-0.4421^{**}	-0.5884^{***}	-0.3048
	(0.1463)	(0.1463)	(0.1937)	(0.2125)	(0.2054)
Ind. Wage Premium^2	-0.1395	-0.6806	0.4548	0.5774	0.4054
	(0.5222)	(0.4571)	(0.4543)	(0.4102)	(0.4398)
	N=1921670	*N=284569*	*N=334,735*	*N=319,962*	*N=409,955*
			Period 2: 2021–2023		
Ind. Wage Premium	-0.5806^{***}	-0.8066^{***}	-0.4807^{**}	-0.4332^{**}	-0.3536^{**}
	(0.1179)	(0.1489)	(0.1916)	(0.1696)	(0.1692)
Ind. Wage Premium^2	0.3454	0.2579	0.4293	0.6861^{**}	0.2193
	(0.4702)	(0.6241)	(0.5512)	(0.3388)	(0.4673)
	N=735378	*N=110086*	*N=117,190*	*N=135,359*	*N=173,475*

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 (based on equation 7). Demographic controls include indicators for race, Hispanic 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. Demographic controls used to calculate IWP are the same as the ones above but rather than controlling for age groups, the regression controls for age, age^2 and age^3. Standard errors in parentheses are clustered by industry. Estimates from this table are used for calculating the elasticities reported in Table A7. Standard errors in parentheses are clustered by industry.
* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table A7: Poisson Estimates of Employment-to-Employment Separation Elasticities at Different Values of Industry Wage Premia - Poisson Estimates

	(1) IWP=-0.3	(2) IWP=0	(3) IWP=0.3
Overall			
2015-19	-0.5918^{**}	-0.6755^{***}	-0.7592^{*}
	(0.2453)	(0.1463)	(0.4230)
2021–23	-0.7879^{***}	-0.5806^{***}	-0.3734
	(0.2267)	(0.1179)	(0.3682)
Difference	-0.1961	0.0948	0.3858
	(0.3337)	(0.1876)	(0.5602)
High School Educated, Under 40 Years Old			
2015–19	-0.1278	-0.5361^{***}	-0.9444^{***}
	(0.2616)	(0.1463)	(0.3533)
2021–23	-0.9614^{**}	-0.8066^{***}	-0.6519
	(0.4009)	(0.1489)	(0.4050)
Difference	-0.8336^{*}	-0.2705	0.2925
	(0.4782)	(0.2085)	(0.5368)
High School Educated, 40 Years and Older			
2015–19	-0.7150^{*}	-0.4421^{**}	-0.1692
	(0.3786)	(0.1937)	(0.2834)
2021–23	-0.7382^{*}	-0.4807^{**}	-0.2231
	(0.3810)	(0.1916)	(0.3835)
Difference	-0.0233	-0.0386	-0.0539
	(0.5365)	(0.2722)	(0.4763)
Bachelor's Degree or Higher, Under 40 Years Old			
2015–19	-0.9349^{***}	-0.5884^{***}	-0.2419
	(0.0903)	(0.2125)	(0.4509)
2021–23	-0.8448^{***}	-0.4332^{**}	-0.0215
	(0.0836)	(0.1696)	(0.3650)
Difference	0.0900	0.1552	0.2205
	(0.1229)	(0.2716)	(0.5795)
Bachelor's Degree or Higher, 40 Years and Older			
2015–19	-0.5480^{***}	-0.3048	-0.0615
	(0.1513)	(0.2054)	(0.4480)
2021–23	-0.4852^{**}	-0.3536^{**}	-0.2220
	(0.2211)	(0.1692)	(0.4069)
Difference	0.0628	-0.0488	-0.1605
	(0.2676)	(0.2658)	(0.6045)

Note: Table reports elasticities at $x = \{-.3, 0, .3\}$ are estimated using a Poisson regression. Elasticities are estimated by evaluating the derivative of EE separation w.r.t IWP at $x = \{-.3, 0, .3\}$ 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 (based on equation 7). Demographic controls include indicators for race, Hispanic 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. Demographic controls used to calculate IWP are the same as the ones above but rather than controlling for age groups, the regression controls for age, age^2 and age^3. Standard errors in parentheses clustered by industry.

* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table A8: Employment-to-Employment Separation Elasticity Estimates at the Mean of Industry Wage Premia over Varying Periods

	Overall	HS, under 40	HS, 40 +	BA+, under 40	BA+, 40+
	(1)	(2)	(3)	(4)	(5)
2015–2019	-0.6837^{***}	-0.5151^{***}	-0.4596^{**}	-0.6572^{***}	-0.3196
	(0.1322)	(0.1321)	(0.1968)	(0.2211)	(0.2135)
	N=1,921,670	*N=284,569*	*N=334,735*	*N=319,962*	*N=409,955*
$2021_{q1} - 2022_{q2}$	-0.7033^{***}	-0.9096^{***}	-0.6545^{***}	-0.5255^{***}	-0.3618^{*}
	(0.1234)	(0.1817)	(0.2045)	(0.1913)	(0.2091)
	N=461,472	*N=68,460*	*N=74,006*	*N=84,763*	*N=107,934*
$2022_{q3} - 2023_{q2}$	-0.5172^{***}	-0.7075^{***}	-0.2939	-0.4503^{**}	-0.3610^{*}
	(0.1154)	(0.1928)	(0.3318)	(0.1930)	(0.1937)
	N=273,906	*N=41,626*	*N=43,184*	*N=50,596*	*N=65,541*

Note: Table reports EE separation elasticity coefficients over the pre-pandemic period, and two separate post-pandemic periods. These elasticities are calculated in two steps: first, we regress an indicator for EE separation at time t on 3-digit IWP at time $t-1$ and its square as well as on demographic controls and state fixed effects (based on equation 7). Second, we evaluate the derivative of EE separations w.r.t IWP at the mean, $x = \{0\}$, and divide by the conditional mean of EE separation at $x = \{0\}$ to estimate the elasticity at $x = \{0\}$. The dependent variable, EE separation, is obtained from monthly CPS data. 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. Demographic controls include age, age^2 and age^3 as well as indicators for race, Hispanic ethnicity, age group, education, citizenship, and metro area status. Standard errors in parentheses are clustered by industry.
* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table A9a: Mobility Rates from the Bottom Half of the 3-digit Industry Wage Premia Distribution, 2021–2023 v. 2015–2019

	(1) 2015-2019	(2) 2021-2023	(3) Difference
A. Exit rate from bottom half of IWP			
Overall	0.519***	0.539***	0.020
N=1,490,780	(0.008)	(0.013)	(0.016)
HS, under 40	0.843***	0.996***	0.153***
N=223,749	(0.026)	(0.047)	(0.054)
B. Exit rate from top half of IWP			
Overall	0.403***	0.438***	0.035***
N=1,512,842	(0.007)	(0.011)	(0.013)
HS, under 40	0.596***	0.573***	−0.024
N=225,467	(0.022)	(0.033)	(0.040)
C. Net exit rate from bottom half of IWP			
Overall	0.116***	0.101***	−0.015
N=3,003,622	(0.010)	(0.017)	(0.020)
HS, under 40	0.247***	0.424***	0.177***
N=449,216	(0.034)	(0.057)	(0.066)

Note: Table reports the likelihood of moving between the bottom and top half of the industry wage premium (IWP) distribution. 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, education, citizenship, metro area status, 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 2021-2023, 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 A9b: Mobility Rates from the Bottom Quartile of the 3-digit Industry Wage Premia Distribution, 2021–2023 v. 2015–2019

	(1) 2015-2019	(2) 2021-2023	(3) Difference
A. Exit rate from the bottom quartile of IWP			
Overall	0.987***	1.039***	0.052*
N=734,422	(0.016)	(0.027)	(0.031)
HS, under 40	1.438***	1.766***	0.328***
N=115,785	(0.047)	(0.086)	(0.098)
B. Exit rate from the top three quartiles of IWP			
Overall	0.747***	0.730***	−0.018
N=2,269,200	(0.014)	(0.021)	(0.025)
HS, under 40	1.091***	1.061***	−0.030
N=333,431	(0.043)	(0.065)	(0.078)
C. Net exit rate from bottom quartile of IWP			
Overall	0.240***	0.309***	0.069*
N=3,003,622	(0.020)	(0.033)	(0.039)
HS, under 40	0.347***	0.705***	0.358***
N=449,216	(0.062)	(0.106)	(0.123)

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, education, citizenship, metro area status, 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 2021-2023, 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 A9c: Mobility Rates from the Hospitality Sector, 2021–2023 v. 2015–2019

	(1) 2015-2019	(2) 2021-2023	(3) Difference
A. Exit rate from Hospitality sector			
Overall	1.397***	1.543***	0.146**
N=212,536	(0.034)	(0.060)	(0.069)
HS, under 40	1.488***	1.729***	0.241**
N=81,837	(0.057)	(0.099)	(0.114)
B. Exit rate from non-Hospitality sector			
Overall	1.029***	1.017***	−0.012
N=3,019,354	(0.029)	(0.045)	(0.053)
HS, under 40	1.064***	1.140***	0.077
N=461,737	(0.048)	(0.077)	(0.090)
C. Net exit rate from Hospitality sector			
Overall	0.368***	0.526***	0.158*
N=3,019,354	(0.043)	(0.072)	(0.084)
HS, under 40	0.424***	0.588***	0.164
N=461,737	(0.073)	(0.123)	(0.143)

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 $p = 0.079$, and for HS under 40, $p = 0.185$. Panel C is the difference between the first two panels. Standard errors in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

Table A10: Price-Phillips Curve Estimates:
Regressions of Δ Log CPI Excluding Shelter on State Labor Market Tightness

	(1)	(2)	(3)		(4)	
	Pooled *2015-2023*	Pooled *2015-2023*	Pre *2015-2019*	Post *2021-2023*	Pre *2015-2019*	Post *2021-2023*
			A. Headline Inflation			
Tightness	0.0161***	0.0159***	0.0037	0.0147***	0.0054*	0.0155***
	(0.0042)	(0.0039)	(0.0025)	(0.0028)	(0.0032)	(0.0030)
Std. Negative Unemp	0.0109***	0.0109***	0.0017	0.0136***	0.0029	0.0132***
	(0.0022)	(0.0022)	(0.0021)	(0.0019)	(0.0023)	(0.0021)
Std. EE Sep	0.0105**	0.0098***	0.0017	0.0093***	0.0024	0.0099***
	(0.0046)	(0.0037)	(0.0026)	(0.0035)	(0.0032)	(0.0032)
			B. Non-Shelter Inflation			
Tightness	0.0058**	0.0062***	−0.0009	0.0050***	0.0015	0.0060***
	(0.0029)	(0.0024)	(0.0020)	(0.0019)	(0.0027)	(0.0020)
Std. Negative Unemp	0.0045***	0.0048***	−0.0029	0.0065***	−0.0010	0.0064***
	(0.0012)	(0.0012)	(0.0018)	(0.0011)	(0.0020)	(0.0011)
Std. EE Sep	0.0024	0.0025	−0.0010	0.0019	0.0002	0.0025
	(0.0042)	(0.0035)	(0.0029)	(0.0037)	(0.0033)	(0.0034)
Controls:		X			X	X

Note: N=374. Table reports estimates of β from equation (10). We regress the change in log CPI between $t-1$ and t on tightness (in $t-1$), where t is half-year periods from 2015 through the second half of 2023. In Panel A we use change in the log of headline CPI as our dependent variable, while Panel B uses CPI excluding shelter. Tightness is calculated as the average of the standardized EE separation rate and the negative standardized unemployment rate, both measured at the state level. We also use these standalone components of tightness as dependent variables. Columns 1 and 2 report estimates pooled over the full 2015–2023 period. In columns 3 and 4, the wage-Phillips curve coefficients are allowed to vary by pre- and post-pandemic periods (2015–2019, 2021–2023). All specifications include state and time fixed effects. In all specifications, we exclude observations that overlap with 2020 to avoid capturing pandemic-related wage effects. Additionally, we exclude price changes from the second half of 2017 to the first half of 2018, as we cannot construct a consistent price series between these periods. Demographic controls include state-by-period population shares of age groups, education groups, race (Black), Hispanic ethnicity and sex. We also control for the state Covid-19 death rate per 100,000 people as of June 2023. *Data sources:* Wage and demographic data obtained from CPS monthly files; seasonally-adjusted state unemployment rates from BLS LAUS; seasonally-unadjusted EE separation rates from LEHD J2J Flows; Covid-19 death rates from CDC (2020); state-level inflation measures for 2015–2017 from Hazell et al. (2022); regional CPI measures from the BLS. Standard errors in parentheses are clustered on state.
* $p < 0.10$, ** $p < 0.05$, *** $p < .01$

A2 Additional Theoretical Derivations of the Dynamic Wage Posting Model

We have so far ignored the changes in the wage offer distribution $F(w)$. Endogenizing the wage offer distribution, $F(w)$, 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, $w = \kappa(p)$ with $\kappa'(p) > 0$, which will hold in a wide class of models, including wage posting models with heterogeneous employers (Bontemps et al., 1999).[31] The wage offer distribution then simply inherits the productivity distribution of active firms, with $F(w) = H(\kappa^{-1}(w))$.

A key result of the model, which we verify empirically, is that a rise in the offer arrival rate affects EE separations more at low- than at high-wage employers—in other words, the quit elasticity rises differentially at low wage levels. The separation 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 offer arrival rate, λ_e, on the EE elasticity with respect to the firm wage rank depends only on the rank, $r = F(w) = H(k^{-1}(w))$, which, in turn, is a primitive of the model. Specifically, the EE elasticity as a function of r can be written as:

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

The key observation is that a higher offer arrival rate, due to either rising vacancies or falling labor supply, makes EE separations more sensitive to firm wage rank (increases the magnitude of the separation elasticity), as seen in the 'twisting' of the EE separations-firm wage locus shown in Figure 5. 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.[32] 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.[33] 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

[31] 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).

[32] 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 offer arrival rate on the rise in offered wages comes out of standard wage posting models, as in Bontemps et al. (1999).

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

denoting $1 - F_t(\kappa(p))$ as $\overline{F}_t(\kappa(p))$, the law of motion for this share can be written as:

$$L_{t+1}(p) = \underbrace{(1-\delta)(1-\chi)L_t(p) + \left[(1-\delta)\chi(1-u_t) + \lambda_u u_t\right] F_t(\kappa(p))}_{\text{Inflow}} - \underbrace{\left[\lambda_e \overline{F}_t(\kappa(p))\right](1-\delta)(1-\chi)L_t(p)}_{\text{Outflow}}.$$

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

In steady state, we have:

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

Since $\chi * \delta \approx 0$ and $\lambda_e = \phi\lambda_u$:

$$L(p) \approx \frac{F(\kappa(p))}{\left[\left(\frac{1}{\chi+\delta} - 1\right)\phi\lambda_u \overline{F}(\kappa(p)) + 1\right]} \times \frac{\lambda_u}{\lambda_u + \delta},$$

$$\tilde{L}(r) \approx \frac{r}{\left[\left(\frac{1-\gamma}{\gamma}\right)\phi\lambda_u(1-r) + 1\right]} \times \frac{\lambda_u}{\lambda_u + \delta}.$$ [34]

Taking logs,

$$\ln \tilde{L}(r) = \ln(r) - \ln\left[\left(\frac{1-\gamma}{\gamma}\right)\phi\lambda_u(1-r) + 1\right] + \ln(\lambda_u) - \ln(\lambda_u + \delta), \tag{11}$$

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

$$\frac{d \ln \tilde{L}(r)}{d \ln \lambda_u} = \frac{\left(\frac{1-\gamma}{\gamma}\right)\phi\lambda_u(1-r)}{\left(\frac{1-\gamma}{\gamma}\right)\phi\lambda_u(1-r) + 1} + \frac{\delta}{\lambda_u + \delta}. \tag{12}$$

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 derivative in equation (12) 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:

$$\frac{\left(\frac{1-\gamma}{\gamma}\right)\phi\lambda_u}{\left(\frac{1-\gamma}{\gamma}\right)\phi\lambda_u + 1} > \frac{\delta}{\lambda_u + \delta}.$$

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 χ).

[34] $\gamma = \chi + \delta$

Figure 6 in the body of the paper illustrates this point for an increase in λ_e from 0.02 to 0.04, with $\delta = \chi = 0.01, \phi = 0.5$. 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.