Divergence in Post-Pandemic Earnings Growth: Evidence from Micro Data*

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Abstract

We analyze post-pandemic labor earnings using employer-employee data and find that earnings grew faster in counties with tighter labor markets and with greater access to loans through the Paycheck Protection Program (PPP), with the impact of PPP loans especially pronounced in areas with tighter labor markets. This divergence in earnings growth is particularly large for lower-paid, nonmanagerial workers, and those employed in smaller firms. Both wage increases and additional hours worked contributed to the overall growth in earnings. These findings align with a labor market competition framework, where tight labor markets reduce earnings disparities. Access to credit further strengthens the competition by relaxing firms' financing constraints.

JEL Codes: J23, J31, E24

Keywords: Earnings, Wage, Labor Market Conditions, Pandemic

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1 Introduction

The post-pandemic labor market experienced a rapid divergence in earnings growth across the US. Counties that experienced the smallest earnings losses at the onset of COVID-19 recorded the fastest earnings growth in 2020-2021. Figure 1 illustrates this pattern by tracing the evolution of average earnings in counties sorted by earnings growth between December 2019 and April 2020. In counties within the top 10th percentile of highest earnings growth (or lowest earnings losses), average earnings increased by 35 percent between January 2020 and December 2021, whereas counties in the bottom 10th percentile saw only a 5 percent increase during the same period. The data for this figure comes from a proprietary employer-employee dataset by Homebase, covering 9 million workers across over 1 million establishments in the US. Our findings are also corroborated by official statistics from the Quarterly Census of Employment and Wages (QCEW) data.[1]

This divergence in earnings growth is the central motivation of this paper. We aim to quantify this divergence and identify its drivers. The abrupt changes in growth trends, combined with their geographical variation, suggest that the divergence is likely driven by large, region-specific shocks. We examine two potential drivers: shocks to local labor market tightness and variation in loan supply through a large government initiative—the Paycheck Protection Program (PPP). Shortly after the onset of the COVID-19 pandemic, the unemployment rate and vacancy-to-unemployment ratio surged to unprecedented levels in April 2020 (Figure 2). However, the labor market tightened rapidly in the following months with varying pace across the country. To provide financial assistance to small and medium-sized businesses during the pandemic, the PPP was enacted in April 2020 as part of the Coronavirus Aid, Relief, and Economic Security (CARES) Act. The program offered guaranteed, forgivable loans aimed at improving liquidity and mitigating job losses, deploying over $500 billion through commercial banks. However, exposure to PPP loans varied across the country due to significant differences in banks' willingness and capacity to disburse them (Granja et al., 2022).

We use several complementary approaches to examine the role of local labor market tightness and access to PPP loans in driving the divergence in earnings growth. We begin by documenting systemic and robust evidence of this divergence in relation to local labor market tightness: Post-pandemic earnings grew faster in counties that experienced smaller labor market shocks at the onset of the pandemic, compared to both their pre-pandemic trends and to counties that experienced larger shocks. In other words, not only does the unconditional growth trend diverge, as illustrated in Figure 1, but a similar pattern holds when we examine the growth response to plausibly exogenous labor market shocks and after controlling for a rich set of county-, sector-, and worker-level factors. The baseline shock is defined as a shift-share shock to the log of the vacancy-to-unemployment ratio, building on Bartik (1991) and more recently Hazell et al. (2022), which exploits variations in local demand for nontradables. We

[1]The QCEW, administered by the Bureau of Labor Statistics, provides quarterly data on employment and earnings reported by employers for workers who are covered by state unemployment insurance (UI) laws and federal workers under the Unemployment Compensation for Federal Employees (UCFE) program, representing over 95 percent of the U.S. workforce.

estimate that, between April 2020 and December 2021, average earnings were 10 percent higher in counties with tighter labor markets (i.e., one standard deviation above the average) at the onset of the pandemic. This earnings difference persisted at least until the end of our sample period in 2021, even as key labor market indicators rapidly improved and nearly reached or exceeded pre-pandemic levels by that time.

We then present evidence of a divergence in earnings growth associated with PPP exposure. Following Granja et al. (2022), our research design exploits geographical variation in PPP exposure explained by bank characteristics that are plausibly uncorrelated with local market conditions. We find a significantly positive effect of PPP exposure, albeit with a modest magnitude. However, tighter labor market conditions amplify this effect. Specifically, the impact of PPP exposure on earnings growth more than doubles in counties experiencing a labor market shock one standard deviation above the average. In other words, access to PPP loans has a more pronounced effect on earnings growth under tighter labor market conditions.

We outline a simple model to illustrate how a tight labor market drives earnings growth through competition for workers, consistent with the cross-county evidence presented above. The model features a monopsonistic labor market with firms of varying productivity and some wage-setting power. In this framework, less productive firms are smaller and offer lower wages. However, a tighter labor market reduces the friction that supports these pay differentials. Consequently, quits rise faster in smaller (and less productive) firms, leading to faster wage growth for their workers. Moreover, wage gains for low-wage workers are larger for job-switchers than job-stayers.

The model's differential implications for workers motivate our final empirical approach, which examines earnings growth across different workforce segments using a difference-in-differences (DID) method. The granularity of our data is crucial for identification, in addition to the shock specification discussed above, as it allows us to exploit variation at the firm or worker level while controlling for workforce composition, time-invariant worker skills, and time-varying demand shocks in narrowly defined state-by-industry groups. This approach helps mitigate concerns that simultaneity or measurement errors might be driving our results.

We begin by comparing lower-paid and higher-paid workers. We find supporting evidence from aggregated data that nonmanagement employees experienced higher earnings growth than managers, as shown in both the Homebase data (Figure 3) and the Current Employment Statistics survey (CES) (Figure A4).[2] If these earnings differentials were driven by competition for workers, we would expect lower-paid workers to experience faster earnings growth in tighter labor markets. Our DID estimation using worker-level data from the Homebase confirms this prediction. We find that the impact of labor market tightness on the earnings of lower-paid workers is 171 percent greater than on higher-paid workers. Similarly, when comparing workers across different firm sizes, we find that the impact of

[2] Autor et al. (2023) document evidence of higher wage growth at the bottom of the wage distribution from the Current Population Survey (CPS).

labor market tightness on the earnings of workers in smaller firms—defined as those with fewer than 50 employees—is 72 percent greater than for those in larger firms.

We also compare job movers and stayers as the model predicts wage gains when workers move to more productive firms. We find supportive evidence from the data. Earnings growth is, on average, 15 percent higher for workers who switched firms compared to those who stayed. This difference is also somewhat larger in counties with tighter labor markets, though the magnitude is relatively small. This result comes with caveats: our data do not identify workers who switch to better-paid jobs within the same firm, nor do we capture workers who move to firms not covered in the data. Therefore, job switchers in our sample may not be fully representative of job switchers in the broader economy. If workers switch from more productive jobs not covered in the data, our estimates may underestimate the gains for job switchers.

We show that our findings are robust to alternative measurements of the shocks, different specifications, and various data sources. Importantly, we corroborate our results with official statistics and representative samples of the U.S. workforce where data are available. As such, our findings are not driven by peculiarities of the Homebase data, nor are they unique to small firms or the service sector, which are overrepresented in the Homebase dataset. The microdata from Homebase offers several advantages over official data, which often have lower frequency and lack the granularity needed to explore short-term dynamics and cross-sectional variation in the post-pandemic labor market. Moreover, official data often do not provide the necessary information to account for heterogeneity in workers' skills. The richness of our data allows us to overcome these limitations.

Overall, we document systemic evidence of larger earnings growth in counties with tighter labor markets driven by labor market competition, with impacts particularly larger for lower-paid workers, non-managerial workers, and workers in smaller firms. These results have significant distributional implications, as they suggest that tight local labor markets narrow the earnings distribution within counties, primarily by compressing the earnings gap in the lower segments of the distribution.

Our findings contribute to the literature on the post-pandemic labor market in several ways. We provide some of the most granular evidence to date on the relationship between post-pandemic wages and labor market tightness. A few papers use aggregate data to show a close link between earnings growth and labor market tightness, as measured by job openings (Furman and Powell, 2021; Domash and Summers, 2022) and job-filling rates (Crump et al., 2022). Supporting evidence from survey data links job-to-job separations with relative wage growth at the bottom of the wage distribution (Autor et al., 2023). Using the Current Population Survey (CPS) data, Autor et al. (2023) document a compression in the wage distribution driven by the relatively fast growth among low-wage workers. They conclude that this evidence is consistent with labor market competition within a job-ladder framework, but they do not examine the divergence in earnings growth across different areas and firms. Our microdata-based findings on the relative movement of lower-paid workers complement their conclusions. Importantly, our approach allows us to explore cross-sectional variation in labor market tightness, which is crucial because, as predicted by the New Keynesian Phillips curve, slack in the local labor market is a key determinant of wages. Additionally, our data enable us to examine wages and working hours separately, providing a comprehensive view of worker earnings and quantifying various adjustment channels at both the worker and firm levels.

More generally, our findings add to the pre-pandemic literature on the cyclicality of employment and earnings (Moscarini and Postel-Vinay, 2017; Haltiwanger et al., 2018; Hershbein and Kahn, 2018). Finally, our paper is also related to the literature that examines the effect of PPP. Granja et al. (2022) exploit regional differences in the supply of PPP loans and find a small impact on business shutdown, hours worked, and employee counts. Our findings complement theirs by showing the differential impact of PPP depending on labor market tightness, and by extending the analysis to include wages and total earnings. Other papers examine the impact of the PPP by exploiting variation in eligibility rules (e.g., Autor et al., 2023) or delays in loan provision (e.g., Doniger and Kay, 2023). However, these studies do not consider earnings or wages, nor do they link employment outcomes to local labor market conditions.

Our findings also have broader implications. First, they provide a microfoundation for the wage Phillips curve, which describes the inverse relationship between labor market slack and wage inflation. In the New Keynesian literature, this relationship is traditionally modeled with wage-setting frictions due to adjustment costs or incomplete information about the nature of the shock. Our empirical results provide a microfoundation for aggregate wage adjustment driven by post-pandemic labor market competition. This adjustment occurs within-job and through job-to-job transitions, affecting not just hourly wages but also working hours, leading to larger earnings adjustments than wage changes alone. Furthermore, our findings have implications for modeling wage responses to labor market slack in high-frequency data. We find that responses to large shocks are immediate (within a month), significant, and persistent (lasting up to two years).

Second, our results offer important insights into the price Phillips curve post-pandemic. Chen et al. (2024) incorporate wage adjustment frictions into the estimation of the price Phillips curve and show that labor market slack was a key driver of the large and persistent inflation in service prices after the pandemic.

Third, these insights have direct implications for stabilization policies. Heterogeneous responses in earnings to aggregate output are a crucial mechanism for monetary policy transmission in various heterogeneous-agent New Keynesian models. The finding that earnings for lower-paid workers grew faster than those for higher-paid workers after the pandemic contrasts with traditional business cycle findings. Additionally, our observation of divergent earnings across counties indicates increased spatial inequality and asynchronous labor market conditions. These implications for post-pandemic stabilization policies represent promising areas for future research.

The rest of the paper is organized as follows. Section 2 describes the data and measurements. Section 3 summarizes our methodology and presents the empirical findings. Section 4 concludes.

2 Data

2.1 Data sources

Homebase. The dataset provides detailed information on employees at businesses using Homebase software for managing schedules and time clocks. It includes data from over 80,000 businesses and more than 1 million employees across the U.S. The dataset offers daily records of wages and hours for individual employees and includes job-level details such as job duration and type, distinguishing between managerial and nonmanagerial positions. Additionally, it reports the location and industry of each establishment. For our analysis, we use the sample period from January 2019 to December 2021.

The two key advantages of the Homebase data are its breadth and detail. Its extensive coverage of private businesses represents a significant strength compared to Compustat, which only includes publicly traded companies. Additionally, while census and other micro datasets used in previous studies lack wage information,[3] aggregated or survey data such as the CPS and CES do not provide the detailed coverage necessary to capture spatial variations or high-frequency changes in the time series. In contrast, Homebase offers broad coverage across businesses, employees, and locations, allowing for detailed analysis of worker earnings—including both wage and hours components—at a small geographical level and monthly frequency. Moreover, it supports worker-level analysis, which is crucial for addressing compositional biases and disentangling the channels of earnings growth.

The Homebase data also have limitations. First, the dataset provides information on regular wage payments but lacks details on tips, benefits, and overtime payments. Second, while the data encompass a broad range of service sectors, it is skewed towards small businesses in leisure, hospitality, and retail. As a result, it is not representative of aggregate employment by industry and firm size. Nevertheless, it has good coverage across geographic areas. Dvorkin and Isaacson (2022) show that the distribution of employment in the Homebase data across states aligns well with the CPS. All states and the District of Columbia are included in the Homebase data, although a few states are overrepresented (California, Florida, and Texas) or underrepresented (New York and Illinois).

Moreover, aggregate trends in employment, earnings, and wages from the Homebase sample closely align with those from the CPS and CES. Figure A1 illustrates this alignment by plotting monthly series of average earnings from Homebase and CPS, as well as quarterly series from CES. The Homebase series closely tracks the CPS and CES series, with the monthly path of Homebase being particularly similar to that of CES. However, there is an exception in Q1 2020, when CES data show unusually high growth compared to Homebase and CPS. This anomaly in CES reflects compositional changes in the sample, as job losses were disproportionately large among lower-wage workers (Stewart, 2022), which inflated average wages.[4] Cumulative growth in Homebase from January 2020 to December 2021

[3] Early versions of the Homebase dataset also lacked wage information.

[4] This compositional effect is especially evident in the leisure and hospitality sector, where hours for nonmanagerial workers were significantly reduced during COVID-19 lockdowns. The imputed average hourly wages in this sector spike in April 2020 for all employees but not for production and nonmanagerial employees (Figure A4).

is 13.5 percent, compared to 12.2 percent in CPS and 13.4 percent in CES. This close alignment also holds at the month-state level, as shown in Figure A2. The top panel plots average hourly wages at the month-state level from Homebase and CES for 2020-2021, while the bottom panel plots employment for the same period. The correlation between observations from the two datasets is 0.51 for hourly wages and 0.69 for employment.

The results of these comparisons are reassuring. The close tracking of national and state-level trends by the Homebase data supports reasonable inferences about the broader economy. Moreover, the over-representation of small firms and low-wage workers in the Homebase data is particularly useful for our analysis, as this segment of the labor force experienced the most significant changes after the pandemic and was the focus of the PPP. The extensive coverage of this segment in the Homebase data is particularly valuable for our estimations.

We restrict our sample to Homebase firms that reported positive hours between January 2020 and March 2020. We measure each employee's hours worked and wages earned on a monthly basis. Outliers, where hours worked or average hourly wages exceed the top 1 percent of the overall sample, are excluded.

PPP. The SBA provides data on all PPP loans approved through the program, including loan amounts, lender names, and borrower addresses. We match the lender names in the PPP dataset to the names of commercial and savings banks listed in the Call Reports filed as of Q1 2020. A probabilistic record linkage algorithm is used to match the bank names between the two datasets.[5] When a lender name in the PPP dataset matches multiple banks with the same legal name, we assign the match to the bank with the branch closest to the borrower's address. We obtain branch location data from the Summary of Deposits dataset filed as of Q2 2019. Any mergers occurring between Q2 2019 and Q1 2020 are adjusted for using the bank mergers file from the National Information Center. This procedure allows us to match 95.4 percent of the banks and 96.4 percent of the loan amounts approved under the PPP. For the banks matched with the PPP loans, we can link each loan to the financial characteristics of the lender from the Call Reports, specifically using data on the number and amount of small business loans outstanding from each lender.

Other data. We complement the above data with official statistics in various parts of our analyses. We obtain time series and state-level data on average hourly wages from the Current Employment Statistics (CES) program of the Bureau of Labor Statistics (BLS). We obtain quarterly data on median usual weekly nominal earnings from the Current Population Survey (CPS). We obtain monthly data on the aggregate unemployment rate (series code UNEMP) and the labor market vacancy-to-unemployment ratio from the BLS, which is defined as the ratio of total nonfarm job openings (series code JTSJOL) to the unemployment level (series code UNEMPLOY). Employment data at the industry-county level

[5]The matching is performed using Stata's *reclink2* package.

is sourced from the Quarterly Workforce Indicators (QWI). Additionally, we use time series data on unemployment from the Labor Force Statistics database of the Current Population Survey (CPS).

2.2 Motivating facts

In this section, we elaborate on three key facts about the divergent trends in worker earnings growth that motivate our subsequent analysis.

Fact 1: Earnings growth diverged across counties.

Figure 1 plots the time trends of workers' earnings from January 2019 to December 2021, based on a panel of Homebase workers. We rank all counties in the sample according to their average earnings growth between March 2020 and December 2019. Counties are grouped into three categories: below the 10th percentile, between the 10th and 90th percentiles, and above the 90th percentile. For each group, we plot the average earnings of all workers, with each series indexed relative to its value in December 2019.

The series closely follow each other during 2019-2020 but began to diverge in 2021, with the most significant divergence observed between March and April 2020, when the country entered COVID-19 lockdown. In April 2020, average earnings in the bottom decile counties were 20 percent below their December 2019 level, while in the top decile counties, they were 20 percent above the December 2019 level. This difference narrowed by the end of 2020, primarily due to a recovery in earnings in the bottom decile and a slight decline in earnings in the top decile. By December 2020, earnings in the bottom and top decile counties were at 68.9 and 115.5 percent of their December 2019 levels, respectively. This gap persisted through 2021. By the end of our sample in December 2021, the bottom and top deciles were at 110.8 and 156.9 percent of their December 2019 levels, respectively. Therefore, cumulative growth from 2020 to 2021 was over 46.07 percent higher in the top decile counties compared to the bottom decile counties.

The difference in wage growth across counties is also reflected in official statistics. Using QCEW data, we compute the average weekly earnings (QCEW series "average weekly wages") for the service-providing sector at the county level.[6] We then rank all counties in the sample based on the growth of average weekly wages in Q1 2020 compared to Q4 2019. Figure A3 illustrates that the top decile counties experienced significantly faster wage growth compared to the bottom decile counties.

Fact 2: Earnings grew faster for lower-paid and nonmanagerial workers.

The top panel of Figure 3 plots the time trends of earnings for managerial and nonmanagerial workers. A worker is classified as a "manager" if they are identified as a "manager" or "general manager" in the Homebase data. For each series, the level in the same month in 2019 is indexed to 100. Earnings for managers and nonmanagers were almost identical in January and February 2020. Managerial

[6] The QCEW does not provide separate data on average hourly wages and the number of hours worked per week.

earnings experienced an 8.9 percent drop in March 2020 but recovered in April. The trend remained relatively flat thereafter, with the end-2021 level similar to January and February 2020. In contrast, earnings for nonmanagers dropped to 52.5 percent of the 2019 level in April but then reversed, with particularly high growth observed in mid-2020 and throughout 2021. By December 2021, earnings of nonmanagers reached 133.3 percent of the 2019 level. The difference between the two series indicates that average earnings for nonmanagers had a cumulative growth of 29.7 percentage points higher than that of managers during 2020-2021.

The middle and bottom panels of Figure 3 plot hours worked and average hourly wages, respectively. On average, nonmanagers experienced 27.4 percentage points higher cumulative growth in hours and 1.5 percentage points higher growth in hourly wage compared to managers. The larger drop in hours for nonmanagers relative to managers observed in Q4 2020 is consistent with the significant job losses for lower-paid workers noted in CES data (Stewart, 2022). Interestingly, the average hourly wage for both managers and nonmanagers showed an uptick in April 2020, suggesting that job losses within each group were relatively larger for lower-paid workers. These changes highlight the challenges of using aggregated data to characterize earning dynamics due to compositional bias. Micro data, as we demonstrate in the next section, are valuable in addressing these challenges.

Higher wage growth for nonmanagers is also reflected in official statistics. Figure A4 shows the average hourly wage data from the CES for all employees compared to production and nonsupervisory employees. After 2021, the wages of production and nonsupervisory employees diverged significantly from the overall trend, with the difference reaching 3 percentage points above the 2019 level by the end of 2022.

Fact 3: Earnings grew faster for workers in small firms.

Figure 4 plots the time trends of earnings by firm size. Workers in Homebase are grouped based on firm size into the following bins: 19 or fewer employees, 20 to 49 employees, 50 to 249 employees, and 250 or more employees. Workers from firms of all sizes saw a similar decline in earnings in April 2020. However, employees in smaller firms experienced a more rapid recovery. By December 2021, the average earnings of workers in firms with 19 or fewer employees had increased to 131.3 percent of their 2019 level, compared to just 115.1 percent for those in firms with over 250 employees. This stronger earnings growth in smaller firms is driven by faster increases in both wages and hours worked.

The earning growth difference between small and large firms is also evident in official statistics. We use QCEW data to compute the average weekly earnings of private sector employees across various firm size bins. The average weekly earnings data is based on employees covered by QCEW from the first quarter of each year. Figure A5 plots the time paths, indexed relative to the same quarter in 2019. The figure shows a divergent trend in earnings between small and large firms from 2020 to 2023. In 2023, the average earnings for firms with fewer than 20 employees was at 130.7 percent of the 2019 level, whereas for firms with more than 250 employees, it was at 120.9 percent, indicating a 9.8 percent cumulative difference.

3 Drivers for the divergence in earnings growth

Motivated by the stylized facts on the divergence in earnings growth, we proceed to quantify this divergence and examine its drivers in this section. We begin by exploring the role of local labor market shocks, followed by an analysis of the PPP loans. Next, we examine worker- and firm-level evidence, motivated by implications from models of labor market competition.

3.1 The role of labor market tightness

3.1.1 Empirical specification

We aim to understand how local labor market competition affects the divergence in earnings growth. We do this by exploiting variation in the local labor market tightness across U.S. counties. Specifically, we compare average earnings at the worker level before and after the pandemic, examining the impact of shocks to local labor market tightness using a DID regression:

$$\Delta Y_{i,j,k,c,t} = \alpha_0 + \alpha_1 Post_t \times Shock_c + \boldsymbol{\beta}' \boldsymbol{Z}_{i,j,k,c,t} + e_{i,j,k,c,t} \tag{1}$$

for establishment i, worker j, 4-digit NAICS industry k, county c, and (monthly) time t. The sample is a panel of Homebase workers from January 2020 to December 2021. Y denotes nominal total earnings and its two components: average hourly wage and hours worked. $\Delta Y_{i,j,k,c,t}$ denotes the log difference in Y from January 2020 to month t. $Post_t$ is a dummy variable equal to one from April 2020 to December 2021. $Shock_c$ is a county-specific labor market shock to be discussed in Section 3.1.2. We standardize $Shock_c$ to have mean zero and unit standard deviation so the coefficients can be interpreted as the effect of a one-standard-deviation increase in the shock. $\boldsymbol{Z}_{i,c,t}$ is a set of controls to be specified below.

Our next specification examines the impact over time. We estimate

$$\Delta Y_{i,j,k,c,t} = \alpha_0 + \boldsymbol{\alpha}_1' [\boldsymbol{I}_t \times Shock_c] + \boldsymbol{\beta}' \boldsymbol{Z}_{i,j,k,c,t} + e_{i,j,k,c,t} \tag{2}$$

for establishment i, worker j, 4-digit NAICS industry k, county c, and (monthly) time t. As in equation (1), $Shock_c$ is a county-level shock. $\boldsymbol{I}_t$ is a vector of (monthly) time dummies from July 2019 to December 2021, where January 2020 is the reference period. Through an exhaustive set of $\boldsymbol{I}_t \times Shock_c$ interactions, the regression estimates the cumulative impact of the shock between January 2020 and subsequent months. All other variables are the same as in equation (1). This specification allows us to investigate the persistence of the impact of the labor market shock.

A challenge in estimating the dynamic responses of labor market outcomes to shocks is that the sample composition may change systematically over time. For instance, if workers who lost their jobs differ intrinsically from those who remained employed, the estimated responses would not have a consistent interpretation throughout the period. We address this challenge through two approaches.

First, we exclude workers who appear in the database for only a brief period. Specifically, we limit our analysis to workers who remained in the database for at least two years between 2019 and 2021, including periods of temporary layoffs. Temporary layoffs are defined as workers who were employed at the start of the sample (with positive earnings), experienced layoffs (with zero earnings during the middle of the sample), and returned to work by the end of the sample period. This restriction balances sample size with minimizing compositional changes. If we further restricted the analysis to workers present throughout the entire sample period, we could ensure consistent sample composition without changes. However, this would result in a significant loss of about two-thirds of the sample. Excluding temporarily laid-off workers would also introduce bias, particularly against low-paid workers in contact-intensive service industries who were disproportionately affected by temporary job losses at the start of the pandemic.

Our second approach involves controlling for worker fixed effects in the regression. By doing so, we base our estimates on variations in the outcomes of the same workers over time, further mitigating concerns about compositional bias.

Our sample period is from January 2020 to December 2021. Our final sample includes 3.1 million observations across 3,110 counties. Given that some industries are overrepresented in the Homebase sample, we weight each observation using its industry's pre-pandemic share of the labor force in 2019. This allows us to draw inference at the aggregate level. Fixing the weights at the pre-pandemic level ensures that our estimates are based on fixed weights regardless of any potentially endogenous change in local labor market conditions. We cluster standard errors by county and month to address possible correlations within county and month.

3.1.2 Identification

COVID-19 triggered an unprecedented shock to the U.S. labor market. As shown in Figure 2, BLS data on unemployment and the vacancy-to-unemployment ratio experienced extremely large monthly increases in April 2020.

Our analyses are based on several measures of labor market tightness. We use the vacancy-to-unemployment ratio in the baseline and use quit rate and unemployment rate in robustness specifications. The vacancy-to-unemployment ratio has its origins in the search model developed in Blanchard and Diamond (1989). This model shows that the ratio of firms looking for workers (i.e., vacancies) to workers looking for jobs (i.e., unemployment) is a sufficient statistic for the state of the labor market.[7] As Ball et al. (2022) argue, the vacancy-to-unemployment ratio is more suitable than the traditional unemployment gap measure for the post-pandemic period due to an upward shift in the Beveridge curve. The Beveridge curve, which illustrates the relationship between job vacancies and the number of unemployed workers, has moved higher, suggesting a similar shift in the unemployment and wage

[7] See Pissarides (2000) for detailed theoretical explanation and Blanchard et al. (2022) and Ball et al. (2022) for recent empirical applications to the post-pandemic U.S. labor market.

relationship. In Section 3.3, we also consider traditional measures for labor market tightness.

We construct the shock to local labor market tightness, $Shock_c$, in equation (1) using a shift-share method, also known as a Bartik shock, following Bartik (1991).[8] Specifically, we derive the shock to the county-level vacancy-to-unemployment ratio based on each county's employment shares in the 2-digit industries of the North American Industry Classification System (NAICS) over 2017-2018 and the national change in the vacancy-to-unemployment ratio at the 2-digit industry level between April 2019 and April 2020. As depicted in Figure 2, April 2020 marked the lowest point of the national vacancy-to-unemployment ratio. We use April 2019 as the baseline for pre-pandemic levels, while accounting for potential seasonality in monthly data.

The shock leverages the variation in the vacancy-to-unemployment ratio across industries, as shown in Figure A6. This variation underpins our research design using the Bartik shock. Similar to the nontradable-demand instrument used by Hazell et al. (2022), this approach captures the idea that national variation in demand for specific tradable goods will have varying effects on local demand in non-tradable sectors, depending on the local exposure to the affected tradable sectors.

Specifically, let $V_{k,t}$ denote the number of job vacancies, measured by job openings for industry k in month t from the Job Openings and Turnover Survey (JOLTS) from the BLS, and U denote the unemployment level from the CPS. The shock in county c at the onset of COVID-19, $Shock_c$, is defined as the change in the projected labor market conditions between April 2019 and April 2020:

$$Shock_c = (\widehat{\Delta ln(V)}_{c,\text{April 2020}} - \widehat{\Delta ln(V)}_{c,\text{April 2019}}) - (\widehat{\Delta ln(U)}_{c,\text{April 2020}} - \widehat{\Delta ln(U)}_{c,\text{April 2019}}), \tag{3}$$

where:

$$\widehat{\Delta ln(V)}_{c,\text{April 2020}} = \sum_{k=1}^{K} \phi_{c,k,2017-2018} * (\ln(V_{k,\text{April 2020}}) - \ln(V_{k,\text{January 2020}})) \tag{4}$$

$$\widehat{\Delta ln(V)}_{c,\text{April 2019}} = \sum_{k=1}^{K} \phi_{c,k,2017-2018} * (\ln(V_{k,\text{April 2019}}) - \ln(V_{k,\text{January 2019}})) \tag{5}$$

for 2-digit NAICS industries $k = 1, \ldots, K$ (excluding public administration). $\phi_{c,k,2017-2018}$ is the average employment share of industry k in county c during 2017-2018. We define $\widehat{ln(U)}_{c,t}$ growth similarly.

Figure A6 illustrates the significant variation in labor market shocks across industries at the onset of COVID-19. Service industries experienced large shocks overall, with particularly severe impacts in contact-intensive sectors, highlighting the widespread disruptions caused by the COVID-19 lockdown. For example, accommodation and food services faced one of the most negative shocks, while industries like information, finance, and insurance saw positive shocks, likely due to greater opportunities

[8] See, for instance, Hershbein and Kahn (2018) and Soh et al. (2022) for applications of this approach in the context of the Great Recession and the COVID-19 pandemic.

for remote work. Outside of the service sector, construction and manufacturing also experienced substantial negative shocks, likely due to both their contact-intensive nature and the effects of supply chain disruptions.

The top panel of Figure A7 reveals significant variation in labor market shocks across different counties. The impact tends to be greater in the Northeast and Midwest regions, though there is also notable variation within regions and within individual states. Urban centers generally experience higher exposure to these shocks, but there are substantial differences among rural areas as well. This suggests that the labor market impact from the pandemic were not uniformly distributed, even within similar geographical areas.

Our identification strategy for estimating equations (1) and (2) using the Bartik shock follows the approach of Borusyak et al. (2022). Identification hinges on the quasi-random assignment of industry demand shocks, implying that these shocks are, in expectation, uncorrelated with relevant unobservables. However, identification could be threatened if preexisting trends in earnings growth were more prevalent in MSAs with industry mixes that would make them more or less susceptible to the demand shock.

Figure A8 directly addresses this concern by analyzing the trends in earnings. This figure plots the earnings path for counties in different decile groups of the Bartik shock. It shows nearly identical trends from early 2019, underscoring the lack of divergent pre-pandemic trends across counties exposed to varying levels of the Bartik shock.

To further alleviate concerns about differential pre-trends, we include a comprehensive set of controls, $\boldsymbol{Z}_{i,c,t}$. These controls include worker fixed effects, state$\times$industry$\times$month fixed effects, and lags of the labor market shock measured in March, February, and January 2020, along with county-level controls. The county-level controls account for heterogeneity in income (log median household income), public health conditions (COVID-19 cases and deaths per capita), and banking sector conditions (average tier 1 capital and core deposit ratios of local banks).[9] Worker fixed effects capture time-invariant factors, such as skills, that can influence wages and hours. As previously mentioned, incorporating this control also helps reduce compositional bias, ensuring that the analysis focuses on within-worker variations over time rather than differences across workers. The state$\times$industry$\times$month fixed effects control for time-varying demand shocks at the state-industry level. This is crucial given that our identification relies on pre-pandemic industry composition within each county. Our identification could be compromised by independent technology shocks occurring simultaneously in industries that faced more severe pandemic-related demand shocks, or by systematic measurement errors in industry shares, which could cause spurious correlations in the shock across counties. The inclusion of these fixed effects helps mitigate concerns about simultaneity and measurement errors by purely relying on variations across counties within narrowly defined state and 4-digit industry groups.

Additionally, for quasi-random assignment, the shock-level law of large numbers must hold—meaning

[9] We discuss local banking sector conditions in more details in Section 3.1.4.

the instrument includes many independent shocks with small average exposure. Calculating shocks at the detailed 2-digit NAICS industry level aligns with this condition. Borusyak et al. (2022) demonstrate that leave-one-out averages (excluding observations from MSA i) when calculating $\widehat{ln(V)}_{i,t}$ and $\widehat{ln(U)}_{i,t}$ are generally unnecessary when multiple regions contribute to each industry demand shock.

In addition to the Bartik shock, we construct a direct measure of the vacancy-to-unemployment ratio at the county level. For unemployment data, we rely on the Bureau of Labor Statistics' Local Area Unemployment Statistics. To measure local job vacancies, we use proprietary data from Indeed, a global job search engine. The Indeed dataset aggregates job postings from various sources, including job listing websites, employer career sites, and applicant tracking systems, while removing duplicate listings. When a job is posted on multiple platforms, it is counted only once. Our final dataset includes 142 million job postings, covering 421 occupations (based on ISCO-08 classifications), 2.9 million companies, and 576 counties. We aggregate these job postings at the county-month level.

One potential concern with the Indeed data is its applicability as a broad proxy for job openings. For instance, job postings on Indeed may not represent the exact number of available jobs, as listings might stay online after being filled, or certain openings might not be advertised online. Additionally, the rise of remote work during the pandemic could reduce the relevance of this measure for local labor markets. Nonetheless, Barrett et al. (2024) demonstrate that total job postings on Indeed exhibit similar dynamics to the official JOLTS job openings at the national level. At the state-month level, the correlation between the two datasets is as high as 0.96.

We prefer the Bartik measure over the direct measure for two key reasons. First, directly measuring vacancies and unemployment at the county level can introduce substantial measurement errors. Second, the direct measure might capture county-specific conditions unrelated to labor market tightness. With these considerations, we assess the robustness of our findings using both measures.

3.1.3 Results

Table 2 presents the results from estimating equation (1). Columns 1 to 3 show estimates using the direct measure of labor market tightness, while columns 4 to 6 report results using the Bartik shock. Across both measures, we observe a significantly positive effect of local labor market tightness on post-pandemic hourly wage, hours worked, and total earnings.

Using the Bartik shock, our preferred measure, a one-standard-deviation increase in labor market tightness is associated with a 3 percent increase in hourly wage, a 7 percent increase in hours worked, and a 10 percent increase in total earnings. The direct measure shows qualitatively similar results but with larger magnitudes, which likely capture additional local conditions beyond labor market tightness. For example, local labor force participation may decrease following the pandemic, which could be negatively correlated with labor market tightness. The Bartik shock helps mitigate this issue by isolating variations in labor market tightness driven by industry-specific demand shocks.

Figure 5 plots the estimated coefficients $\boldsymbol{\alpha}_1$ from equation (2) over time, revealing two key insights:

the absence of pre-pandemic trends and the persistent effects of the pandemic shock.

First, to examine pre-pandemic trends, we extend the data back to July 2019. We see that the levels of earnings, wages, and hours in pre-pandemic months are nearly identical (relative to January 2020), suggesting that there were no preexisting trends before the pandemic. This is a crucial finding because the identification strategy could be undermined if counties with industry mixes more or less susceptible to pandemic demand shocks had notable pre-pandemic trends in earnings growth.

Second, the impact of the labor market shock started increasing shortly after the lockdown and, under the Bartik shock, peaks by July 2020. A one-standard-deviation increase in the Bartik shock to labor market tightness is associated with approximately a 6 percent increase in total earnings in the second half of 2020. These effects persist until the end of 2021, with the exception of a slight decrease in early 2021. The increase in total earnings were driven by both hours worked and hourly wages.

3.1.4 The role of PPP

In our second research design, we leverage variation in the supply of PPP loans across counties. To counter the sharp decline in economic activity and the widespread closure of small businesses at the onset of the COVID-19 pandemic, the U.S. Congress introduced the PPP as part of the CARES Act. This program provided liquidity to businesses with 500 or fewer employees through guaranteed and forgivable loans.[10] The loan terms regarding the maximum amount and interest rate were uniform for all businesses.[11]

According to the Small Business Administration (SBA), a PPP loan could be forgiven if two conditions were met. First, the funds had to be used for payroll costs, mortgage interest, rent, and utility payments within an eight-week period, with at least 75 percent of the loan allocated to payroll. Second, businesses were required to maintain both their employee headcount and compensation levels.[12]

The first round of PPP funding began on April 3, 2020. Due to high demand, the initial \$349 billion fund was fully allocated within just two weeks, by April 16. Following this, a second bill was passed on April 24, adding another \$320 billion to the program. Applications for the second round of PPP funding were accepted starting on April 27. In the first two weeks of this second round, 60 percent of the funds were disbursed, but the pace of disbursement slowed considerably thereafter. By early July, more than \$130 billion remained available, and the application rate in July and August remained low. This suggested that the second round of funding had sufficiently met the demand. Ultimately, the program stopped accepting applications on August 8, with \$525 billion disbursed in total.

[10] An exception was made for businesses in accommodations and food services (NAICS code 72), where the employment threshold applied per physical location.

[11] The maximum loan amount was the lesser of 2.5 times the average monthly payroll costs or \$10 million. The average payroll costs were calculated based on the prior year's payroll, excluding compensation exceeding \$100,000 per individual. The loan carried a 1 percent interest rate and a maturity period of 2 years.

[12] An exception allowed businesses to restore their employment and compensation levels if they had laid off workers or reduced wages between February 15 and April 26, 2020.

While the PPP was overseen by the SBA, the loan application process was managed through the banking system. As shown by Granja et al. (2022), access to first-round PPP loans varied significantly across different regions, largely due to variations in banks' ability to process loans. Banks faced pre-existing conditions and capacity constraints that affected their speed in handling PPP loan applications. These constraints included limited staffing to interact with clients, review applications, and submit them to the SBA, as well as lack of pre-existing access to the SBA's application portal.[13] Additionally, banks under active supervisory enforcement actions were unable to submit PPP applications until they received approval from the SBA. These delays contributed to the relative underperformance of some banks in securing funds during the short window before the first-round PPP funds were depleted.

At the local market level, exposure to banks that underperformed during the first round of PPP was a significant factor in determining the aggregate amount of PPP funds received in a given area. This was because it was difficult to substitute local lending relationships quickly, and banks tended to prioritize existing clients when processing loan applications. Granja et al. (2022) demonstrate a strong negative correlation between exposure to banks with supply-side constraints and the share of PPP funds received at the state and ZIP code level by the end of the first round. As a result, access to PPP loans in different areas was plausibly driven by supply-side factors that were unrelated to actual demand for the funds. This supply-side explanation is supported by evidence from the second round of PPP funding, where the relationship between exposure to constrained banks and the share of PPP funds diminished as supply-side bottlenecks eased.

We investigate whether PPP funds had any impact on post-pandemic earnings and whether this impact depended on local labor market tightness. There are two primary channels through which PPP funds can influence wages and hours. First, to qualify for loan forgiveness, businesses that received PPP loans were required to maintain their employment and compensation levels, indicating a direct impact of PPP funds on labor demand. However, up to 25 percent of the PPP loan could be used for nonpayroll costs, suggesting a secondary, indirect channel. By covering nonpayroll expenses, businesses could alleviate financial pressures, which might otherwise lead to reductions in employment and wages.

Our research design is motivated by the observed differences in access to PPP loans across geographic regions. The key idea is to use variations in the supply of PPP loans to divide geographic areas and compare their outcomes. By leveraging the differential exposure to banks that underperformed in processing PPP loans, we aim to isolate the effect of PPP funds on local labor demand. Following the methodology outlined in Granja et al. (2022), we calculate PPP supply at the county level to implement this approach.

In the first step, we compute the relative bank performance in the first round of PPP as

$$PPPE_b = \frac{SharePPP - ShareSBL}{SharePPP + ShareSBL} \times 0.5 \tag{6}$$

[13] Lenders needed valid credentials to submit applications via the SBA portal. Banks without these credentials had to wait until they obtained access, delaying their ability to submit applications.

where $SharePPP$ is bank b's PPP loans as a share of all PPP loans and $ShareSBL$ is the bank's small business loans (SBL) as a share of all SBL in the fourth quarter of 2020, both measured in terms of number of loans. Simply speaking, $PPPE_b$ measures a bank's performance in distributing PPP relative to its SBL market share. If there were no heterogeneity in PPP performance, we would expect PPP share to follow a similar pattern as SBL share.

In the second step, we compute predicted $PPPE_b$ by regressing bank level $PPPE_b$ on a set of covariates that captures supply-side constraints for a bank to quickly process PPP.[14] The predicted $PPPE_b$ captures bank performance that is explained by these predetermined supply-side factors that are likely to be orthogonal to differences in local demand for PPP loans. In the third step, we map bank level predicted $PPPE_b$ to counties based on information on local bank branch presence from the Summary of Deposits data. We proxy the supply of PPP loans at the county level by PPP exposure, $PPPE_c$, calculated as the weighted average of bank PPP exposure. The weights are defined as the share of the number of branches of each bank in the county or within 10 miles of the center of the respective county.

Granja et al. (2022) do not find evidence that PPP funds were disproportionately disbursed to geographic areas that were initially most affected by the pandemic. There is no consistent relationship between PPP allocation with unemployment claims. $PPPE_c$ and $Shock_c$ have a low correlation equal to 0.04, supporting the validity of our approach of interacting these two variables.

To estimate the effect of labor market tightness and PPP exposure on worker earnings, we estimate the following DID regression:

$$\begin{aligned}\Delta Y_{i,j,k,c,t} = \alpha_0 + \alpha_1 Post_t \times Shock_c + \alpha_2 Post_t \times PPPE_c \\ + \alpha_3 Post_t \times Shock_c \times PPPE_c + \boldsymbol{\beta}' \boldsymbol{Z}_{i,j,k,c,t} + e_{i,j,k,c,t}\end{aligned} \tag{7}$$

for establishment i, worker j, 4-digit NAICS industry k, county c, and (monthly) time t. $PPPE_c$ is exposure to PPP lending as defined in Section 2. As in equation (1), Y is nominal total earnings, average hourly wage, or hours worked. $\Delta Y_{i,j,k,c,t}$ is the log difference in Y from January 2020 to month t, $Z_{i,j,k,c,t}$ is a vector of controls specified in Section 3. Importantly, this includes controls for local banking sector conditions—measured as the average tier 1 capital and core deposit ratios of all banks in the county—to account for potential heterogeneity in the conditions of local banks that may be correlated with local banks' performance in distributing PPP.

We again examine the impact over time by estimating:

$$\begin{aligned}\Delta Y_{i,j,k,c,t} = \alpha_0 + \boldsymbol{\alpha}_1' [\boldsymbol{I}_t \times Shock_c] + \boldsymbol{\alpha}_2' [\boldsymbol{I}_t \times PPPE_c] \\ + \boldsymbol{\alpha}_3' [\boldsymbol{I}_t \times Shock_c \times PPPE_c] + \boldsymbol{\beta}' \boldsymbol{Z}_{i,j,k,c,t} + e_{i,j,k,c,t}\end{aligned} \tag{8}$$

[14] Following Granja et al. (2022) Table 2, the covariates include a measure of the bank's labor intensity, a dummy variable for pre-existing SBA lender, the bank's SBA loans as a share of SBA loans, a dummy variable for banks that had an active enforcement action when the PPP was launched and a dummy variable for Wells Fargo Bank.

for establishment i, worker j, 4-digit NAICS industry k, county c, and (monthly) time t. $\boldsymbol{I}_t$ is a vector of (monthly) time dummies from July 2019 to December 2021, where January 2020 is the reference period. Y, $\Delta Y_{i,j,k,c,t}$, $PPPE_c$, and $Z_{i,j,k,c,t}$ are as defined in equation (7).

Table 3 presents the results from equation (7). Columns 1 to 3 use the direct labor market tightness measure, while columns 4 to 6 report our preferred results, using the Bartik shock. We observe similar coefficients of local labor market tightness as reported in Table 2. Additionally, PPP exposure has significantly positive effects. The effect of PPP exposure for counties with an average-sized labor market shock is moderate, a one-standard-deviation increase in PPP exposure leads to a 1 percent, 1 percent, and 2 percent rise in hourly wage, hours, and total earnings, respectively, in our preferred estimates (columns 4 to 6). Nevertheless, tighter labor market conditions amplify the effect of PPP exposure. A one-standard-deviation increase in the Bartik shock increases the marginal effect of PPP exposure on wages, hours, and earnings by 44 percent, 15 percent, and 19 percent, respectively.

Figure 6 plots the estimated coefficients by month. The path for the coefficient of local labor market tightness ($\boldsymbol{\alpha}_1$) in panel (a) mirrors that of Figure 5, reflecting a persistent effect. Regarding PPP exposure ($\boldsymbol{\alpha}_2$), panel (b) shows significantly positive effects on hourly wages, hours, and total earnings between May and August 2020, with the strongest impacts coinciding with the peak disbursement of PPP loans during the first two rounds. These effects begin to taper off and are no longer statistically significant by the end of the year. However, there is a slight uptick in December 2020, which could be explained by year-end fiscal adjustments or new hires as businesses sought to meet forgiveness criteria. More importantly, panel (c) reveals a significantly positive interaction effect between PPP exposure and local labor market tightness ($\boldsymbol{\alpha}_3$), suggesting that PPP exposure had a larger effect in counties with tighter labor markets. This interactive effect remains significant until mid-2021, although by then the direct effect had already diminished.

Figure 6 also confirms the absence of a pre-trend in the interactive effect during the months leading up to the pandemic, strengthening the validity of our identification strategy based on parallel trends. Interestingly, for the direct effect of PPP exposure, panel (b) shows that counties with higher exposure experienced relatively larger declines in total earnings in August 2019 and January 2020, driven by reductions in hours worked, while there was no differential pre-trend for hourly wages. One possible explanation is that the decline in hours reflects seasonal fluctuations in labor demand (particularly for service workers in the sample during the low-demand months of August and January). Nonetheless, if not for seasonality, the declining pre-trend in counties with higher PPP exposure could lead to an underestimation of the post-pandemic increase, thus potentially biasing the results against finding a significant effect of PPP exposure.

3.2 Differential impacts on workers and firms

The results presented so far point to a direct link between labor market tightness and earnings growth by exploiting variations across counties. To illustrate how a tight labor market drives earnings growth through competition for workers, we outline a simple model.[15] This model also motivates our final empirical approach, which examines earnings growth across different segments of the workforce. It features a monopsonistic labor market with firms of heterogeneous productivity and with some wage-setting power. As a result, less productive firms are smaller and offer lower wages; however, a tighter labor market leads to relatively faster wage growth within these firms. In this framework, labor supply elasticity increases with labor market tightness due to job search friction, which leads to faster wage growth in smaller (and less productive) firms compared to larger (and more productive) firms. Moreover, the wages of lower-paid workers grow faster than those of higher-paid workers. In the subsequent analysis, we formally test these implications. We note that the model is intentionally simple, aimed to illustrate the key mechanism rather than contribute the paper's primary insights, which are empirical in nature.

We estimate the following DID regression:

$$\begin{aligned}\Delta Y_{i,j,k,c,t} = \alpha_0 &+ \alpha_1 Post_t \times \textit{\textbf{Low-Wage}}_j + \alpha_2 Post_t \times Shock_c \\ &+ \alpha_3 Post_t \times Shock_c \times \textit{\textbf{Low-Wage}}_j + \boldsymbol{\beta}' \boldsymbol{Z}_{i,j,k,c,t} + e_{i,j,k,c,t}\end{aligned}$$

for establishment i, worker j, 4-digit NAICS industry k, county c, and (monthly) time t. The variable *Low-Wage*$_j$ is a dummy equal to one if a worker's average hourly wage is below the 75$^{\text{th}}$ percentile within their state and industry in the sample and zero otherwise. Y, $\Delta Y_{i,j,k,c,t}$, $PPPE_c$, and $Z_{i,j,k,c,t}$ are as defined in equation (1).

Table 4 presents the results. We find that low-wage workers experienced significantly higher increases in hourly wages, hours worked, and total earnings than high-wage workers in response to a tight labor market. Based on our preferred estimates in columns 4 to 6, the impact of labor market shocks on low-wage workers is 71 percent greater than on high-wage workers in terms of average hourly wage, 9 percent greater in terms of hours worked, and 22 percent greater in terms of total earnings.

We also compare nonmanagerial and managerial workers, as nonmanagerial workers tend to have lower pay. A similar pattern emerges, as shown in Table 5. *Nonmanager*$_j$ is a dummy variable equal to one if the worker is classified as an "employee" and equal to zero if classified as a "manager" or "general manager" in the Homebase data. We find that the impact of labor market shocks on nonmanagerial workers is 60 percent greater than on managerial workers for average hourly wage, 38 percent greater for hours worked, and 43 percent greater for total earnings.

The model predicts that wages in smaller firms grow faster than in larger firms when the labor market tightens. We classify firms as "small" if they employ fewer than 50 workers and "large" if they employ 50 or more. Table 6 shows that the impact of labor market shock on workers in smaller firms is 129 percent greater for average hourly wage, 34 percent greater for hours worked, and 53 percent greater for total earnings compared to workers in larger firms.

[15] See online Appendix.

Table 7 compares job switchers and job stayers. The variable *Job-Switcher*$_j$ is a dummy variable indicating whether a worker changed jobs during the sample period. The results show that labor market shocks have a greater impact on the hourly wages, hours worked, and total earnings of job switchers compared to job stayers. The impact is 170 percent larger in terms of hourly wages, 49 percent larger in terms of hours worked, and 71 percent larger in terms of total earnings. One caveat of this analysis is that our sample does not capture all job switchers, in particular those moving to jobs not covered by the Homebase data. As Homebase data tend to overrepresent lower-paid and smaller jobs, our estimates might underestimate the actual differences between job switchers and stayers if higher-paid job transitions are not included.

In sum, we find that workers in counties with tighter labor markets had larger earnings growth after the pandemic. Notably, the growth was higher for lower-paid and nonmanagerial workers, as well as those employed by smaller firms and those who switched jobs. These varied outcomes across different workers and firms are consistent with the predictions of a labor market competition model.

These results carry important distributional implications. Figure 7 illustrates the change in the distribution of earnings between December 2021 and December 2019 across counties. The distribution is measured by the log ratio of earnings at the 90$^{\text{th}}$ percentile relative to the 10$^{\text{th}}$ percentile (i.e., the log 90/10 ratio). This is further broken down into the log 90/50 ratio and the log 50/10 ratio. Counties are ranked based on labor market tightness as measured by the Bartik shock. The figure demonstrates that counties with tighter labor market conditions experienced a more pronounced narrowing of the earnings distribution, primarily driven by a compression of the earnings gap between the 10$^{\text{th}}$ and 50$^{\text{th}}$ percentiles.

3.3 Robustness

In this section, we discuss the robustness of our results with respect to different specifications, measurements, and data sources.

3.3.1 Alternative data sources

As discussed earlier, the Homebase data have several advantages over official statistics due to its higher frequency and more granular coverage, with worker- and firm-level observations. One concern, however, is its representativeness. Do the results from the Homebase sample reflect peculiar features of the sample, or do they represent more general outcomes? In Section 2, we show that the time path for wage growth in the Homebase data closely mirrors that in the QCEW data. To further validate these findings, we use the QCEW data to estimate equation (8) at the county-quarterly level. The results are presented in Figure A9 for the sample of all industries and Figure A10 for the accommodation and food services sector, which is over-represented in the Homebase data.

Results from the full QCEW sample are qualitatively very similar to those from the Homebase data.

We observe a persistent response of wages to a tight labor market, except for a decline in 2021Q1, as shown in Figure 5. The direct effects of PPP exposure are similarly positive in 2020, as seen in Figure 6, but here the effect becomes positive again for the rest of 2021. The indirect effect of PPP follows nearly the same path as in Figure 6. Additionally, the accommodation and food services sample from QCEW shows similar results, with the exception that there is no decline in the effect of labor market tightness in 2021Q1, while the effect of PPP exposure becomes insignificant in 2021. Overall, both the Homebase and QCEW samples consistently show positive effects of labor market tightness and PPP exposure on wage growth at least through 2020. These results are reassuring in that our baseline findings are not driven by peculiar features of the Homebase data, nor are they unique to small firms or the service sector, which are overrepresented in the Homebase data.

3.3.2 Alternative measures of labor market tightness

We then explore whether the choice of labor market tightness measure affects the results. To address this, we re-estimate equation (1) using two alternative measures: the quit rate and the unemployment rate. Specifically, we define the Bartik shock to the quit rate, $Shock_c^Q$, following the same approach as equation (3), but with the JOLTS quit rate replacing the vacancy-to-unemployment ratio. Similarly, the Bartik shock to the unemployment rate, $Shock_c^U$, is defined using the unemployment rate from the CPS Labor Force Statistics instead of the vacancy-to-unemployment ratio. Both shocks are standardized to have a unit standard deviation.

Table A1 presents the results using these alternative measures. The findings are qualitatively similar to those based on the vacancy-to-unemployment ratio: hourly wages, hours, and total earnings respond negatively to the quit rate and the unemployment rate. The coefficients based on the quit rate are slightly larger than those based on the vacancy-to-unemployment ratio, which in turn are larger than those based on the unemployment rate.

3.3.3 Additional controls

Our baseline result focuses on the dynamic response to the labor market shock at the onset of the pandemic. One natural question is whether the results are driven by evolving labor market condition during the recovery stage. After the large initial shock in April 2020, aggregate indicators of labor market tightness evolved and resembled a transition path from a large shock back to a steady state, as shown in Figure 2. While we do not attempt to empirically distinguish the effects from the initial shock and evolving transition dynamics, we can examine whether our results are robust to controlling for contemporaneous labor market tightness. To do this, we define the contemporaneous shock, $Shock_{c,t}$, to labor market tightness as

$$Shock_{c,YearMt} = (\Delta\widehat{ln(V)}_{c,YearMt} - \Delta\widehat{ln(V)}_{c,2019Mt}) - (\Delta\widehat{ln(U)}_{c,YearMt} - \Delta\widehat{ln(U)}_{c,2019Mt}) \quad (9)$$

for $Year \in (2020, 2021)$. To distinguish $Shock_{c,t}$ from $Shock_c$ as defined in equation (3), we refer to the former as the contemporaneous shock and the latter as the initial shock.

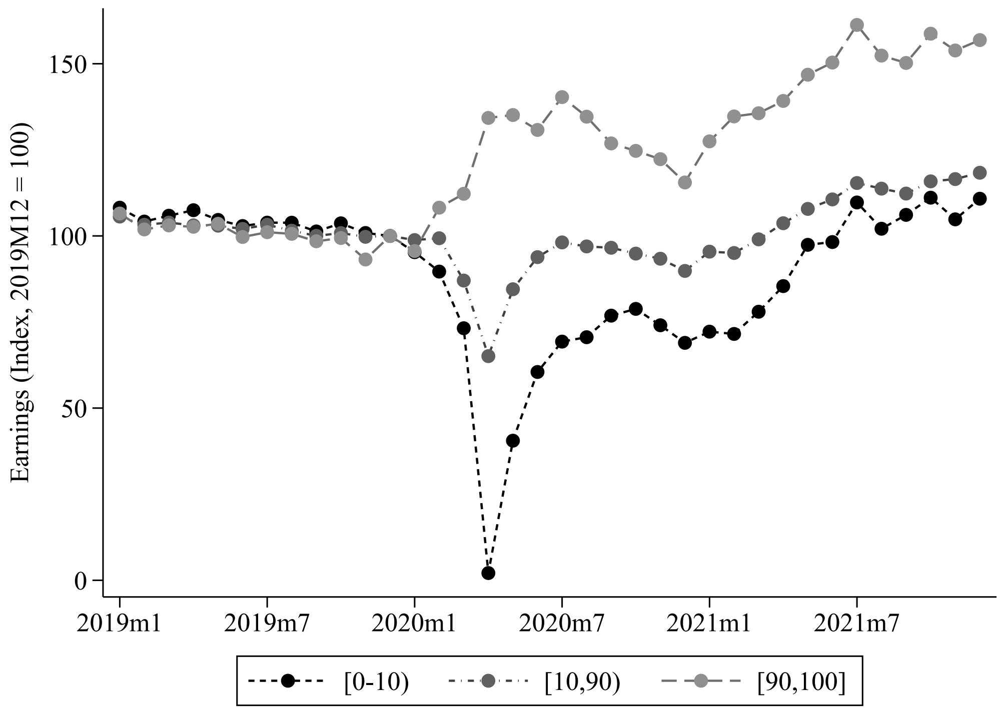
Table A2 shows that the estimated coefficient of the initial shock with this control is very close to the baseline results in Table 2. Using our preferred measures in columns 4-6, a one-standard-deviation increase in labor market tightness results in a 2.8 percent increase in hourly wages, compared to 6.6 percent in the baseline. The effect on hours worked is nearly identical, while the resulting impact on total earnings is 9.6 percent, very similar to those in the baseline.

4 Conclusion

The post-pandemic U.S. labor market underwent rapid changes. Using proprietary microdata, we document new insights into the divergence of earnings growth across geographic areas, workers, and firms. By exploiting cross-sectional variations with a DID framework, we find that counties with tighter labor markets and those with greater access to PPP loans saw faster earnings growth. The effect of PPP loans is more pronounced in counties with tighter labor market conditions. Earnings growth was particularly larger for lower-paid, nonmanagerial workers, and those in smaller firms, driven by both higher hourly wages and increased working hours. These findings align with the predictions of a labor market competition framework, as tighter labor markets enhance workers' outside options in such settings. Our microdata allows us to test and confirm these predictions by exploring variations across areas and workers.

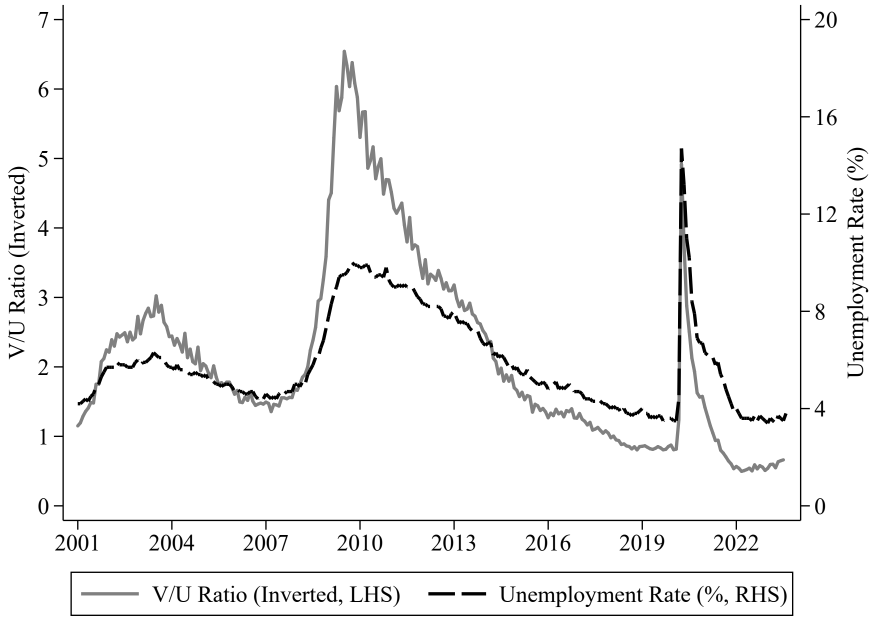
Our findings also carry significant distributional implications. We observe increasing disparities in earnings growth across geographic regions, while within counties with tighter labor markets, the disparities among workers decrease. This highlights the evolving patterns of wage and spatial inequality, suggesting asynchronous labor market recoveries across regions. These findings offer valuable insights for post-pandemic stabilization policies and present promising directions for future research.

Figure 1: Trends in average earnings by percentile groups



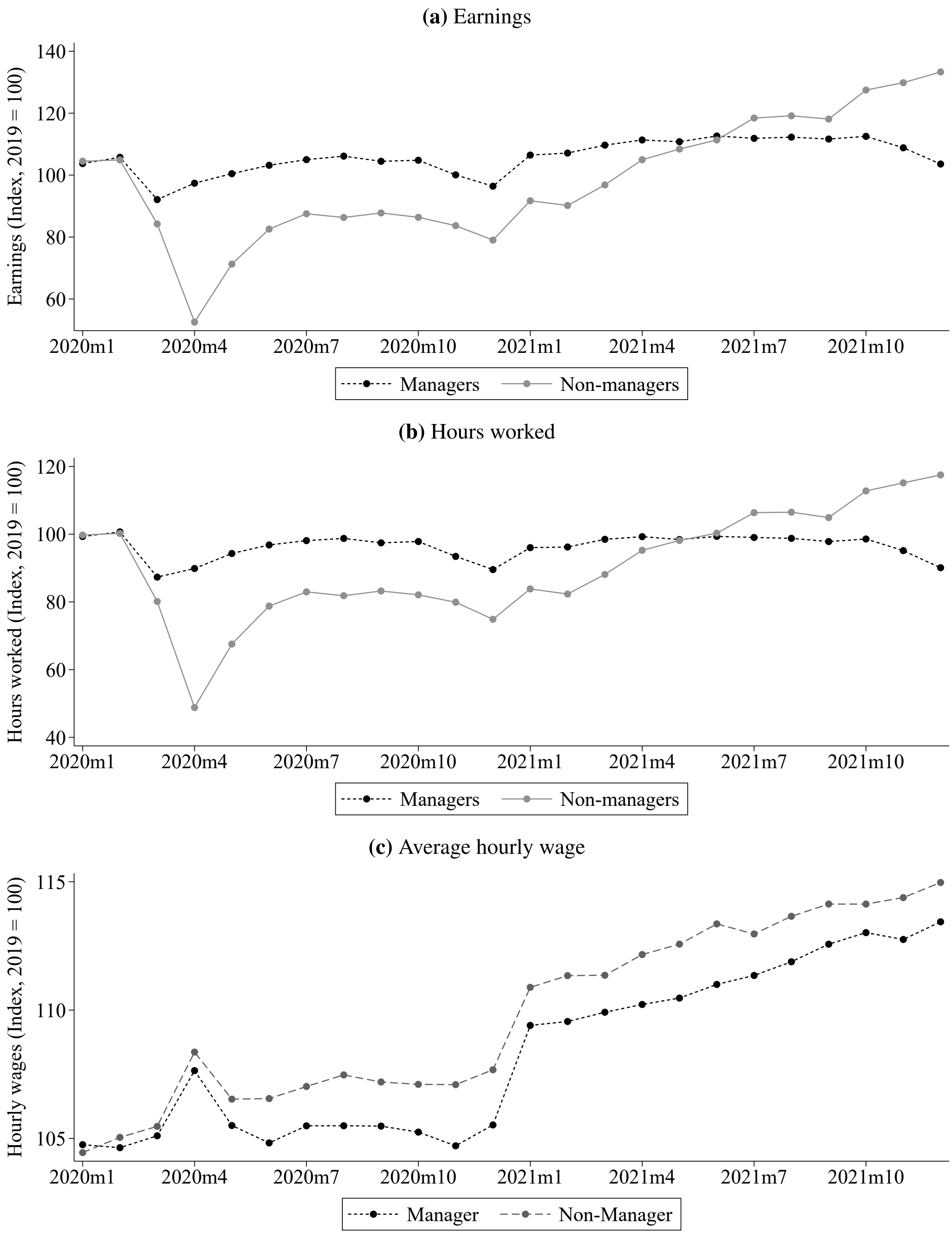
Notes: This figure plots the pre and post COVID-19 trend in average earnings in the Homebase data. The sample is a monthly panel of Homebase workers from January 2019 to December 2021. Workers were sorted into percentile groups based on the county in which they were employed. Counties are ranked by their average earnings growth in April 2020 relative to December 2019. Each series has been expressed as indices relative to its value in December 2019. The Homebase sample includes workers that were on temporary layoffs, defined as workers that were initially employed (reported positive earnings), were laid off during the middle of the sample (reported zero earnings), and later returned to work again before the end of the sample. The sample is restricted to workers that were in the database for at least 2 years between 2019 and 2021, including periods of temporary layoffs. Source: Homebase and authors' calculations.

Figure 2: Labor market tightness



Notes: This figure plots the monthly time series for the U.S. vacancy-to-unemployment ratio and unemployment rate. On April 2020, both measures jumped up sharply to unprecedented levels. Source: Bureau of Labor Statistics.

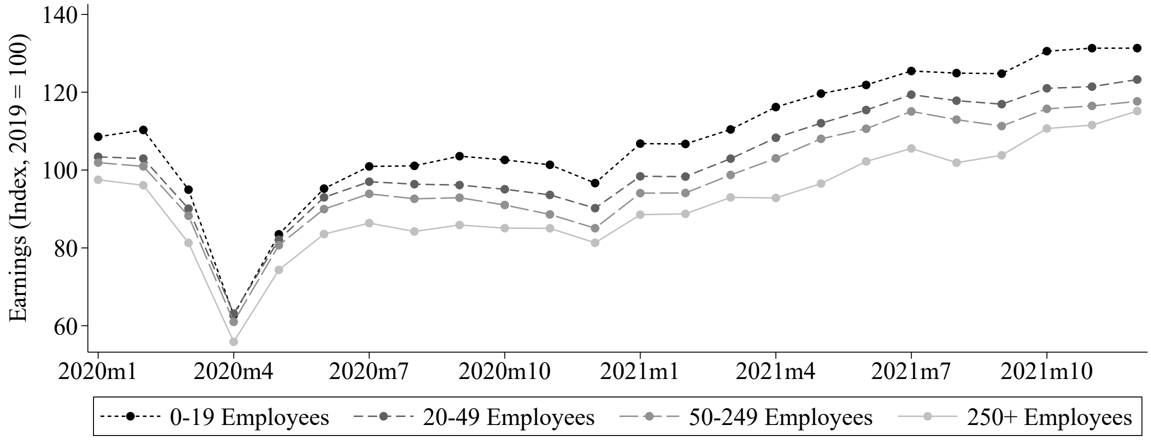
Figure 3: Labor market dynamics by managerial versus nonmanagerial workers: Homebase



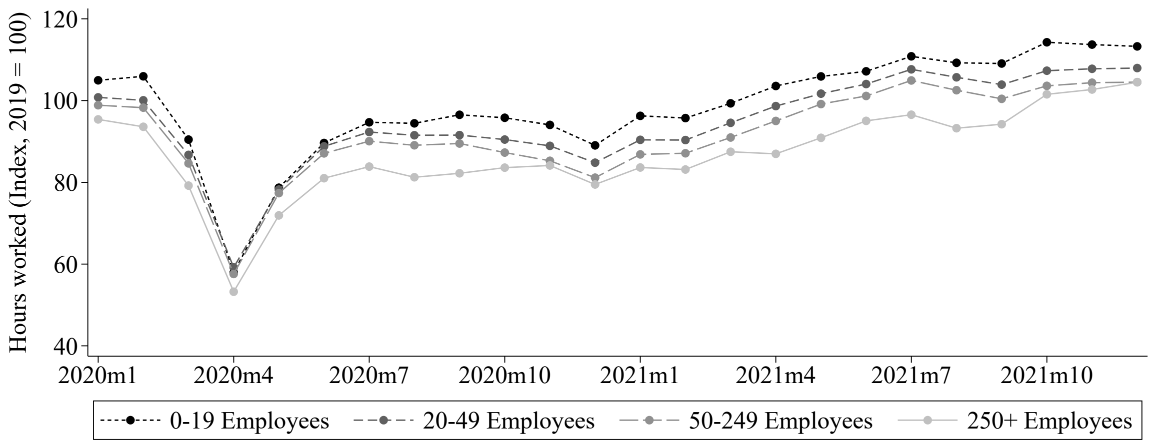
Notes: This figure plots the average hourly wage, hours worked, and earnings per employee in the Homebase data by managerial and nonmanagerial workers. Total earnings is the product of average nominal hourly wage and hours worked. A worker is considered managerial if it is classified as a "manager" or "general manager" in the Homebase data. We weight each observation using its industry's pre-pandemic share of the labor force in 2019. For each series, the level in the same month in 2019 is indexed to 100. Source: Homebase and authors' calculations.

Figure 4: Labor market dynamics by firm size: Homebase

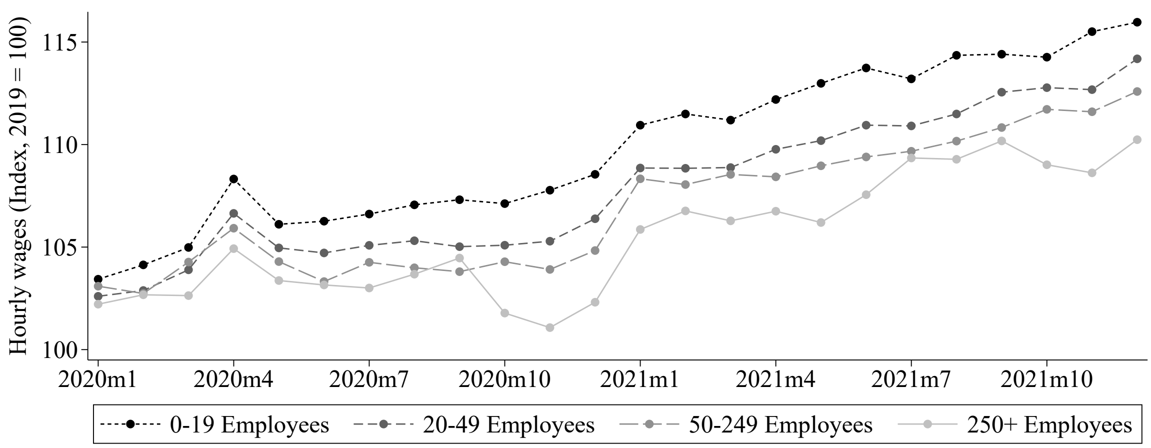
(a) Earnings



(b) Hours worked



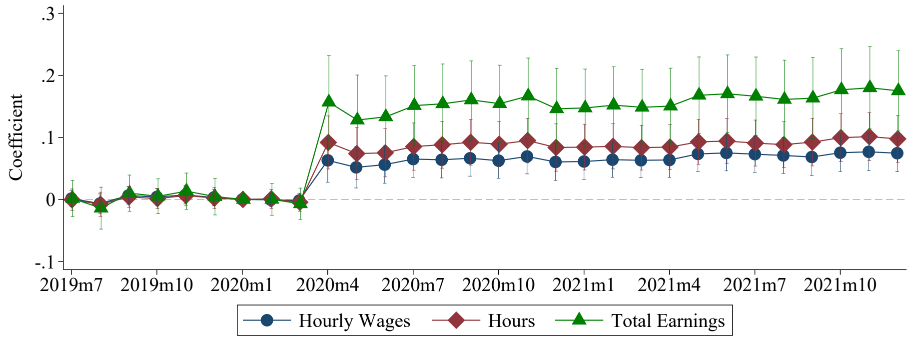
(c) Average hourly wage



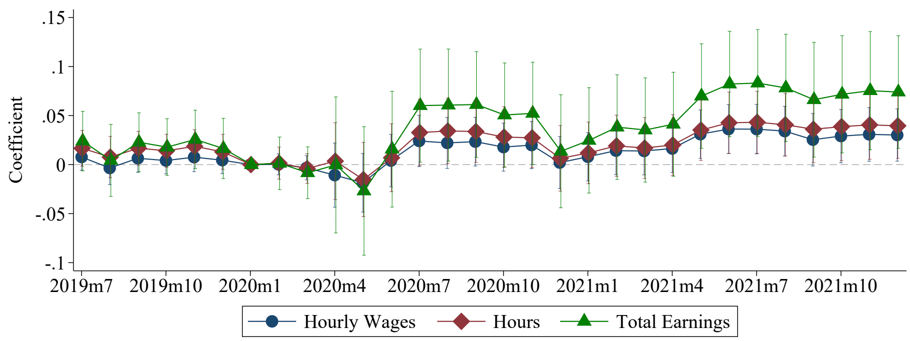
Notes: This figure plots the average hourly wage, hours worked, and earnings per employee in the Homebase data by various size bins of their employer firm. Total earnings is the product of average nominal hourly wage and hours worked. Each firm is classified into size bins based on the number of employees under its payroll in the Homebase data. We weight each observation using its industry's pre-pandemic share of the labor force in 2019. For each series, the level in the same quarter in 2019 is indexed to 100. Source: Homebase and authors' calculations.

Figure 5: Time path: baseline results

(a) OLS



(b) Bartik Instrument



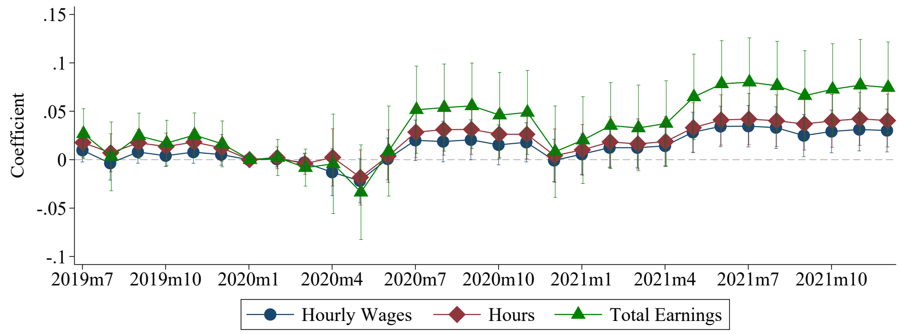
Notes: This figure plots the estimated coefficients $\boldsymbol{\alpha}_1$ from the following regression:

$$\Delta Y_{i,j,k,c,t} = \alpha_0 + \boldsymbol{\alpha}_1'[\boldsymbol{I}_t \times Shock_c] + \boldsymbol{\beta}'\boldsymbol{Z}_{i,j,k,c,t} + e_{i,j,k,c,t}$$

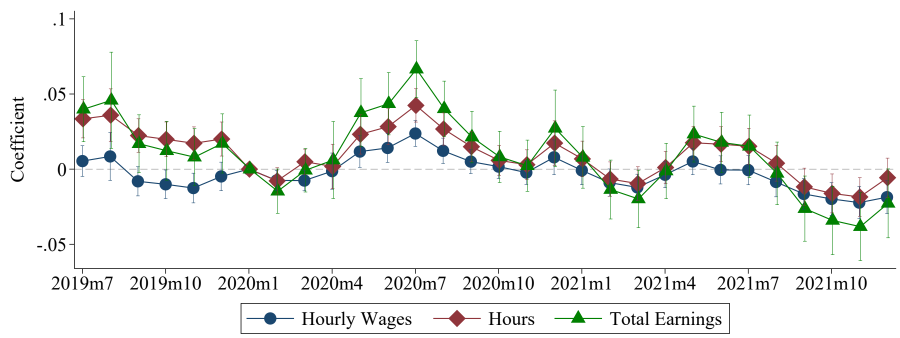
for establishment i, worker j, 4-digit NAICS industry k, county c, and (monthly) time t. The sample is a panel of Homebase workers from July 2019 to December 2021. Y denotes nominal average hourly wage, hours worked, or total earnings. $\Delta Y_{i,j,k,c,t}$ is the log difference in Y from January 2020 to month t. $\boldsymbol{I}_t$ is a vector of (monthly) time dummies from July 2019 to December 2021, where January 2020 is the reference period. The initial labor market shock, $Shock_c$, is the direct measure or Bartik shock to vacancy-to-unemployment ratio. Both measures have been standardized to unit standard deviation. $\boldsymbol{Z}_{i,j,k,c,t}$ contains control variables including worker fixed effects, state-industry-month fixed effects, lags of the labor market shock measured as of March 2020, February 2020, and January 2020, and county-level controls. The county-level controls include log median household income, COVID-19 cases and deaths per capita, and average tier 1 capital and core deposit ratios of all banks within the county. We weight each observation using its industry's pre-pandemic share of the labor force in 2019. Standard errors are clustered by county and time. Vertical whiskers plot 90 percent confidence intervals. Source: BLS, Homebase, and authors' calculations.

Figure 6: Time path: PPP exposure

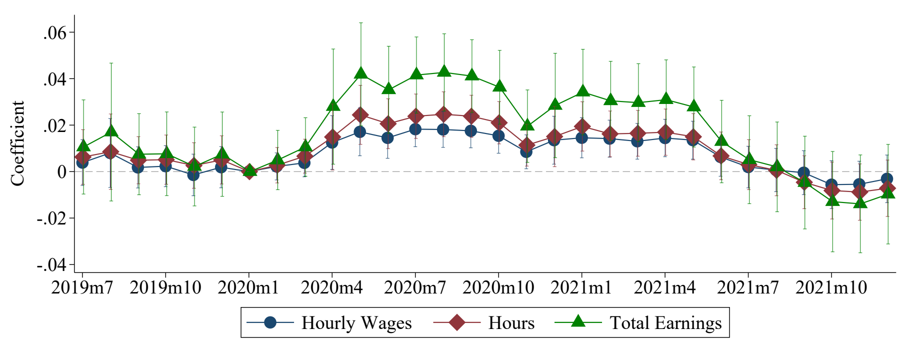
(a) Local labor market shock ($\boldsymbol{\alpha}_1$)



(b) PPP exposure ($\boldsymbol{\alpha}_2$)



(c) Local labor market tightness $\times$ PPP exposure ($\boldsymbol{\alpha}_3$)

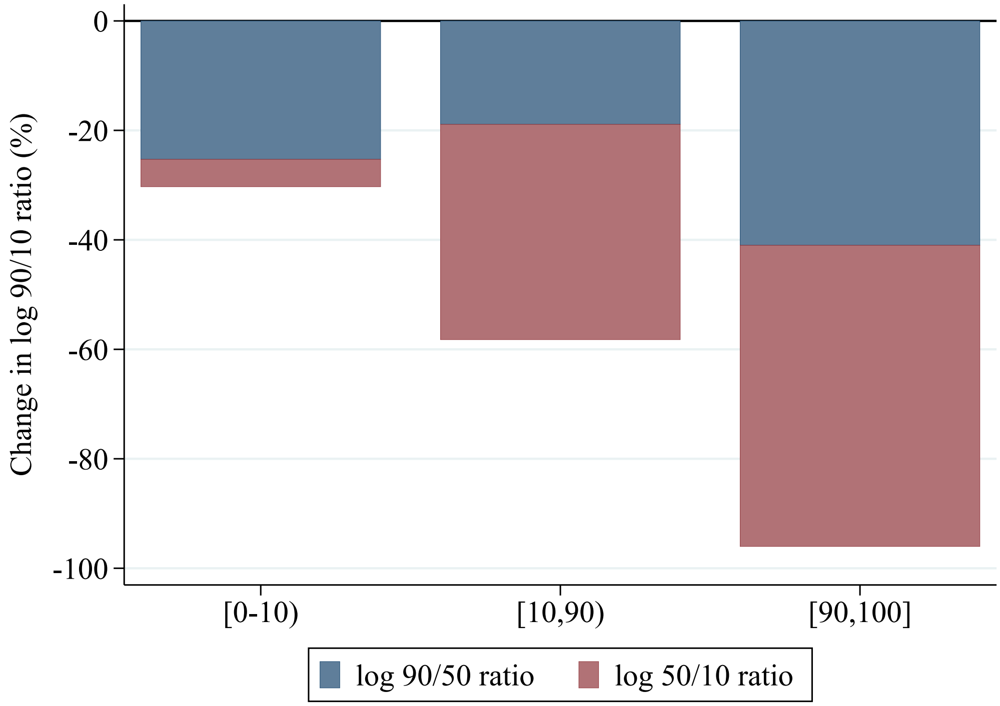


Notes: This figure plots the estimated coefficients from the following regression:

$$\Delta Y_{i,j,k,c,t} = \alpha_0 + \boldsymbol{\alpha}_1'[\boldsymbol{I}_t \times Shock_c] + \boldsymbol{\alpha}_2'[\boldsymbol{I}_t \times PPPE_c] + \boldsymbol{\alpha}_3'[\boldsymbol{I}_t \times Shock_c \times PPPE_c] + \boldsymbol{\beta}'\boldsymbol{Z}_{i,j,k,c,t} + e_{i,j,k,c,t}$$

$Shock_c$ is the Bartik shock to local labor market tightness defined in equation (3), standardized to unit standard deviation. $\boldsymbol{Z}_{i,j,k,c,t}$ contains control variables including worker fixed effects, state-industry-month fixed effects, lags of the labor market shock measured as of March 2020, February 2020, and January 2020, and county-level controls. The county-level controls include log median household income, COVID-19 cases and deaths per capita, and average tier 1 capital and core deposit ratios of all banks within the county. We weight each observation using its industry's pre-pandemic share of the labor force in 2019. Standard errors are clustered by county and time. Vertical whiskers plot 90 percent confidence intervals. Source: BLS, Homebase, SBA, and authors' calculations.

Figure 7: Change in earnings distribution across counties before and after the pandemic



Notes: This figure plots the change in the log 90/50 ratio (blue), log 50/10 ratio (red), and log 90/10 ratios (sum of blue and red) across counties. The change is taken as the difference between December 2021 and December 2019. The sample is a monthly panel of Homebase workers. Workers were sorted into percentile groups based on the county in which they were employed. Counties are ranked based on the Bartik shock to local labor market tightness defined in equation (3). The Homebase sample includes workers that were on temporary layoffs, defined as workers that were initially employed (reported positive earnings), were laid off during the middle of the sample (reported zero earnings), and later returned to work again before the end of the sample. The sample is restricted to workers that were in the database for at least 2 years between 2019 and 2021, including periods of temporary layoffs. Source: BLS, Homebase, and authors' calculations.

Table 1: Summary statistics

Panel A: Worker-Level	Obs	Mean	St. Dev.	Min	Median	Max
$Hourly\ Wages_{i,j,k,c,t}$	3,134,354	9.576	6.978	0.000	10.500	50.000
$Hours_{i,j,k,c,t}$	3,134,354	18.815	15.561	0.000	17.413	87.442
$Total\ Earnings_{i,j,k,c,t}$	3,134,354	242.381	247.696	0.000	179.526	4,372.125
$\Delta Hourly\ Wages_{i,j,k,c,t}$	3,134,354	0.011	0.710	-2.773	0.000	2.708
$\Delta Hours_{i,j,k,c,t}$	3,134,354	0.071	0.843	-3.231	0.000	3.321
$\Delta Total\ Earnings_{i,j,k,c,t}$	3,134,354	0.074	1.485	-5.766	0.000	5.735
$Non\text{-}Manager_j$	3,134,354	0.884	0.320	0.000	1.000	1.000
$Job\text{-}Switcher_j$	3,134,354	0.404	0.491	0.000	0.000	1.000
$Low\text{-}Wage_j$	3,134,354	0.765	0.424	0.000	1.000	1.000
$Small_i$	3,134,354	0.578	0.494	0.000	1.000	1.000
Panel B: County-Level	Obs	Mean	St. Dev.	Min	Median	Max
$Shock_c$ (OLS)	3,110	-0.045	0.007	-0.117	-0.045	0.014
$Shock_{c,t}$ (OLS)	40,430	0.025	0.013	-0.025	0.025	0.132
$Shock_c$ (Bartik)	3,110	-0.551	0.072	-1.241	-0.546	0.195
$Shock_{c,t}$ (Bartik)	40,430	0.662	0.267	-0.184	0.645	2.058
$Shock_c^Q$ (Bartik)	3,110	-0.155	0.028	-0.498	-0.151	0.005
$Shock_{c,t}^Q$ (Bartik)	40,430	0.461	0.059	0.021	0.464	0.697
$Shock_c^U$ (Bartik)	3,110	0.285	0.029	0.035	0.286	0.597
$Shock_{c,t}^U$ (Bartik)	40,430	0.078	0.116	-0.515	0.060	1.078
$PPPE_c$	3,110	-0.161	0.252	-0.500	-0.181	0.500
Log median household income	3,110	10.700	0.225	9.867	10.677	12.538
COVID cases per capita	3,110	0.795	0.147	0.318	0.804	1.102
COVID deaths per capita	3,110	0.012	0.003	0.004	0.013	0.017
Average tier 1 capital ratio	3,110	7.965	0.508	5.874	7.991	9.109
Average core deposit ratio	3,110	1.185	0.161	0.673	1.159	3.371

Notes: This table reports the summary statistics. The worker-level variables are based on a monthly panel of workers from the Homebase data. $Y_{i,j,k,c,t}$ denotes either nominal average hourly wage, hours worked, or total earnings for establishment i, worker j, 4-digit NAICS industry k, county c, and (monthly) time t. Total earnings is the product of average nominal hourly wage and hours worked. $\Delta Y_{i,j,k,c,t}$ is the log difference in Y from January 2020 to month t. For each variable, outlying observations beyond the top and bottom 1 percent have been winsorized. We weight each observation using its industry's pre-pandemic average share of the labor force in 2019. $Nonmanager_j$ is a dummy variable equal to one if the worker was classified as an "employee" instead of "manager" or "general manager." $Job\text{-}Switcher_j$ is a dummy variable equal to one if a worker switched its job during the sample period by moving to a different establishment. $Low\text{-}Wage_j$ is a dummy variable equal to one if a worker's average hourly wage is below the 75th percentile among workers employed in the same state and industry from the Homebase sample. $Small_i$ is a dummy variable equal to one if a worker's employer has less than 50 employees under its payroll. $Shock_c$ is the initial shock to labor market tightness defined in equation (3). $Shock_{c,t}$ is the contemporaneous shock to labor market tightness defined in equation (9). $Shock_c^Q$ and $Shock_{c,t}^Q$ ($Shock_c^U$ and $Shock_{c,t}^U$) are the initial and contemporaneous shocks to quits rate (unemployment rate). $PPPE_c$ is the exposure to PPP lending defined in Section 2. Source: BLS, Homebase, SBA, and authors' calculations.

Table 2: Baseline results

	(1) Hourly Wages	(2) Hours	(3) Total Earnings	(4) Hourly Wages	(5) Hours	(6) Total Earnings
$Post_t \times Shock_c$	0.0890***	0.1238***	0.2106***	0.0288**	0.0698***	0.1014***
	(0.0176)	(0.0224)	(0.0382)	(0.0144)	(0.0180)	(0.0309)
Observations	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354
R^2	0.52	0.51	0.52	0.52	0.51	0.52
Specification	OLS	OLS	OLS	Bartik	Bartik	Bartik
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Worker FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Industry-Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficients from the following regression:

$$\Delta Y_{i,j,k,c,t} = \alpha_0 + \alpha_1 Post_t \times Shock_c + \boldsymbol{\beta}' \boldsymbol{Z}_{i,j,k,c,t} + e_{i,j,k,c,t}$$

for establishment i, worker j, 4-digit NAICS industry k, county c, and (monthly) time t. The sample is a panel of Homebase workers from January 2020 to December 2021. Y denotes nominal average hourly wage, hours worked, or total earnings. $\Delta Y_{i,j,k,c,t}$ is the log difference in Y from January 2020 to month t. $Post_t$ is a dummy variable equal to one for $t \geq$ April 2020. The initial labor market shock, $Shock_c$, is either the direct measure or Bartik shock to local labor market tightness. Both measures have been standardized to unit standard deviation. $\boldsymbol{Z}_{i,j,k,c,t}$ contains control variables including worker fixed effects, state-industry-month fixed effects, lags of the labor market shock measured as of March 2020, February 2020, and January 2020, and county-level controls. The county-level controls include log median household income, COVID-19 cases and deaths per capita, and average tier 1 capital and core deposit ratios of all banks within the county. We weight each observation using its industry's pre-pandemic share of the labor force in 2019. Standard errors are clustered by county and time. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent, respectively. Source: BLS, Homebase, and authors' calculations.

Table 3: PPP exposure

	(1) Hourly Wages	(2) Hours	(3) Total Earnings	(4) Hourly Wages	(5) Hours	(6) Total Earnings
$Post_t \times Shock_c$	0.0903***	0.1252***	0.2133***	0.0244*	0.0627***	0.0890***
	(0.0175)	(0.0224)	(0.0380)	(0.0146)	(0.0183)	(0.0314)
$Post_t \times PPPE_c$	0.0078***	0.0115***	0.0204***	0.0072***	0.0108***	0.0191***
	(0.0027)	(0.0034)	(0.0059)	(0.0027)	(0.0034)	(0.0058)
$Post_t \times PPPE_c \times Shock_c$	0.0072***	0.0085***	0.0158***	0.0104***	0.0124***	0.0227***
	(0.0023)	(0.0029)	(0.0050)	(0.0025)	(0.0031)	(0.0054)
Observations	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354
R^2	0.52	0.51	0.52	0.52	0.51	0.52
Specification	OLS	OLS	OLS	Bartik	Bartik	Bartik
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Worker FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Industry-Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficients from the following regression:

$$\Delta Y_{i,j,k,c,t} = \alpha_0 + \alpha_1 Post_t \times Shock_c + \alpha_2 Post_t \times PPPE_c + \alpha_3 Post_t \times Shock_c \times PPPE_c + \boldsymbol{\beta}' \boldsymbol{Z}_{i,j,k,c,t} + e_{i,j,k,c,t}$$

for establishment i, worker j, 4-digit NAICS industry k, county c, and (monthly) time t. The sample is a panel of Homebase workers from January 2020 to December 2021. Y denotes nominal average hourly wage, hours worked, or total earnings. $\Delta Y_{i,j,k,c,t}$ is the log difference in Y from January 2020 to month t. $Post_t$ is a dummy variable equal to one for $t \geq$ April 2020. The initial labor market shock, $Shock_c$, is either the direct measure or Bartik shock to local labor market tightness. $PPPE_c$ denotes the exposure to PPP lending defined in Section 2. Both measures have been standardized to unit standard deviation. $\boldsymbol{Z}_{i,j,k,c,t}$ contains control variables including worker fixed effects, state-industry-month fixed effects, lags of the labor market shock measured as of March 2020, February 2020, and January 2020, and county-level controls. The county-level controls include log median household income, COVID-19 cases and deaths per capita, and average tier 1 capital and core deposit ratios of all banks within the county. We weight each observation using its industry's pre-pandemic share of the labor force in 2019. Standard errors are clustered by county and time. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent, respectively. Source: BLS, Homebase, SBA, and authors' calculations.

Table 4: High- versus low-wage workers

	(1) Hourly Wages	(2) Hours	(3) Total Earnings	(4) Hourly Wages	(5) Hours	(6) Total Earnings
$Post_t \times$ *Low-Wage*$_j$	0.0667***	0.1039***	0.1669***	0.0668***	0.1040***	0.1670***
	(0.0004)	(0.0005)	(0.0008)	(0.0004)	(0.0005)	(0.0008)
$Post_t \times Shock_c$	0.0975***	0.1379***	0.2327***	0.0359**	0.0826***	0.1211***
	(0.0179)	(0.0239)	(0.0399)	(0.0153)	(0.0204)	(0.0342)
$Post_t \times Shock_c \times$ *Low-Wage*$_j$	0.0500***	0.0750***	0.1223***	0.0614***	0.0903***	0.1479***
	(0.0151)	(0.0243)	(0.0385)	(0.0166)	(0.0266)	(0.0422)
Observations	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354
R^2	0.56	0.59	0.58	0.56	0.59	0.58
Specification	OLS	OLS	OLS	Bartik	Bartik	Bartik
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Worker FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Industry-Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficients from the following regression:

$$\Delta Y_{i,j,k,c,t} = \alpha_0 + \alpha_1 Post_t \times \textit{Low-Wage}_j + \alpha_2 Post_t \times Shock_c \\ + \alpha_3 Post_t \times Shock_c \times \textit{Low-Wage}_j + \boldsymbol{\beta}' \boldsymbol{Z}_{i,j,k,c,t} + e_{i,j,k,c,t}$$

for establishment i, worker j, 4-digit NAICS industry k, county c, and (monthly) time t. The sample is a panel of Homebase workers from January 2020 to December 2021. *Low-Wage*$_j$ is a dummy variable equal to one if a worker's average hourly wage is below the 75th percentile among workers employed in the same state and industry from the Homebase sample. Y denotes nominal average hourly wage, hours worked, or total earnings. $\Delta Y_{i,j,k,c,t}$ is the log difference in Y from January 2020 to month t. $Post_t$ is a dummy variable equal to one for $t \geq$ April 2020. The initial labor market shock, $Shock_c$, is either the direct measure or Bartik shock to local labor market tightness. Both measures have been standardized to unit standard deviation. $\boldsymbol{Z}_{i,j,k,c,t}$ contains control variables including worker fixed effects, state-industry-month fixed effects, lags of the labor market shock measured as of March 2020, February 2020, and January 2020, and county-level controls. The county-level controls include log median household income, COVID-19 cases and deaths per capita, and average tier 1 capital and core deposit ratios of all banks within the county. We weight each observation using its industry's pre-pandemic share of the labor force in 2019. Standard errors are clustered by county and time. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent, respectively. Source: BLS, Homebase, and authors' calculations.

Table 5: Managerial versus nonmanagerial workers

	(1) Hourly Wages	(2) Hours	(3) Total Earnings	(4) Hourly Wages	(5) Hours	(6) Total Earnings
$Post_t \times$ *Non-Manager*$_j$	0.0095***	0.0146***	0.0238***	0.0093***	0.0145***	0.0235***
	(0.0012)	(0.0015)	(0.0026)	(0.0013)	(0.0015)	(0.0027)
$Post_t \times Shock_c$	0.0900***	0.1252***	0.2130***	0.0288**	0.0699***	0.1015***
	(0.0176)	(0.0224)	(0.0382)	(0.0144)	(0.0180)	(0.0309)
$Post_t \times Shock_c \times$ *Non-Manager*$_j$	0.0238***	0.0334***	0.0561***	0.0174***	0.0264***	0.0433***
	(0.0015)	(0.0018)	(0.0032)	(0.0016)	(0.0018)	(0.0033)
Observations	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354
R^2	0.52	0.51	0.52	0.52	0.51	0.52
Specification	OLS	OLS	OLS	Bartik	Bartik	Bartik
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Worker FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Industry-Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficients from the following regression:

$$\Delta Y_{i,j,k,c,t} = \alpha_0 + \alpha_1 Post_t \times \mathit{Nonmanager}_j + \alpha_2 Post_t \times Shock_c + \alpha_3 Post_t \times Shock_c \times \mathit{Nonmanager}_j + \boldsymbol{\beta}' \boldsymbol{Z}_{i,j,k,c,t} + e_{i,j,k,c,t}$$

for establishment i, worker j, 4-digit NAICS industry k, county c, and (monthly) time t. The sample is a panel of Homebase workers from January 2020 to December 2021. $\mathit{Nonmanager}_j$ is a dummy variable equal to one if the worker was classified as an "employee" instead of "manager" or "general manager" in the Homebase data. Y denotes nominal average hourly wage, hours worked, or total earnings. $\Delta Y_{i,j,k,c,t}$ is the log difference in Y from January 2020 to month t. $Post_t$ is a dummy variable equal to one for $t \geq$ April 2020. The initial labor market shock, $Shock_c$, is either the direct measure or Bartik shock to local labor market tightness. Both measures have been standardized to unit standard deviation. $\boldsymbol{Z}_{i,j,k,c,t}$ contains control variables including worker fixed effects, state-industry-month fixed effects, lags of the labor market shock measured as of March 2020, February 2020, and January 2020, and county-level controls. The county-level controls include log median household income, COVID-19 cases and deaths per capita, and average tier 1 capital and core deposit ratios of all banks within the county. We weight each observation using its industry's pre-pandemic share of the labor force in 2019. Standard errors are clustered by county and time. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent, respectively. Source: BLS, Homebase, and authors' calculations.

Table 6: By firm size

	(1) Hourly Wages	(2) Hours	(3) Total Earnings	(4) Hourly Wages	(5) Hours	(6) Total Earnings
$Post_t \times Small_i$	0.2146***	0.2205***	0.4207***	0.2149***	0.2207***	0.4212***
	(0.0052)	(0.0066)	(0.0112)	(0.0052)	(0.0067)	(0.0113)
$Post_t \times Shock_c$	0.0725***	0.0977***	0.1689***	0.0172	0.0481***	0.0686**
	(0.0162)	(0.0188)	(0.0332)	(0.0138)	(0.0161)	(0.0285)
$Post_t \times Shock_c \times Small_i$	0.0176***	0.0138**	0.0298***	0.0223***	0.0164***	0.0365***
	(0.0045)	(0.0056)	(0.0096)	(0.0049)	(0.0061)	(0.0105)
Observations	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354
R^2	0.58	0.63	0.61	0.58	0.63	0.61
Specification	OLS	OLS	OLS	Bartik	Bartik	Bartik
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Worker FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Industry-Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficients from the following regression:

$$\begin{aligned}\Delta Y_{i,j,k,c,t} &= \alpha_0 + \boldsymbol{\alpha}_1' Post_t \times Small_i + \alpha_2 Post_t \times Shock_c \\ &\quad + \boldsymbol{\alpha}_3' Post_t \times Shock_c \times Small_i + \boldsymbol{\beta}' \boldsymbol{Z}_{i,j,k,c,t} + e_{i,j,k,c,t}\end{aligned}$$

for establishment i, worker j, 4-digit NAICS industry k, county c, and (monthly) time t. The sample is a panel of Homebase workers from January 2020 to December 2021. $Small_i$ is a dummy variable equal to one if a worker's employer has less than 50 employees under its payroll. Y denotes nominal average hourly wage, hours worked, or total earnings. $\Delta Y_{i,j,k,c,t}$ is the log difference in Y from January 2020 to month t. $Post_t$ is a dummy variable equal to one for $t \geq$ April 2020. The initial labor market shock, $Shock_c$, is either the direct measure or Bartik shock to local labor market tightness. Both measures have been standardized to unit standard deviation. $\boldsymbol{Z}_{i,j,k,c,t}$ contains control variables including worker fixed effects, state-industry-month fixed effects, lags of the labor market shock measured as of March 2020, February 2020, and January 2020, and county-level controls. The county-level controls include log median household income, COVID-19 cases and deaths per capita, and average tier 1 capital and core deposit ratios of all banks within the county. We weight each observation using its industry's pre-pandemic share of the labor force in 2019. Standard errors are clustered by county and time. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent, respectively. Source: BLS, Homebase, and authors' calculations.

Table 7: Job-switchers versus job-stayers

	(1) Hourly Wages	(2) Hours	(3) Total Earnings	(4) Hourly Wages	(5) Hours	(6) Total Earnings
$Post_t \times$ *Job-Switcher*$_j$	0.0361***	0.0608***	0.0967***	0.0363***	0.0609***	0.0970***
	(0.0045)	(0.0054)	(0.0095)	(0.0045)	(0.0054)	(0.0095)
$Post_t \times Shock_c$	0.0824***	0.1179***	0.1982***	0.0185	0.0612***	0.0832***
	(0.0177)	(0.0227)	(0.0386)	(0.0146)	(0.0183)	(0.0315)
$Post_t \times Shock_c \times$ *Job-Switcher*$_j$	0.0194***	0.0191***	0.0386***	0.0315***	0.0297***	0.0593***
	(0.0049)	(0.0058)	(0.0103)	(0.0051)	(0.0061)	(0.0108)
Observations	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354
R^2	0.52	0.51	0.52	0.52	0.51	0.52
Specification	OLS	OLS	OLS	Bartik	Bartik	Bartik
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Worker FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Industry-Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficients from the following regression:

$$\Delta Y_{i,j,k,c,t} = \alpha_0 + \alpha_1 Post_t \times \textit{Job-Switcher}_j + \alpha_2 Post_t \times Shock_c + \alpha_3 Post_t \times Shock_c \times \textit{Job-Switcher}_j + \boldsymbol{\beta}' \boldsymbol{Z}_{i,j,k,c,t} + e_{i,j,k,c,t}$$

for establishment i, worker j, 4-digit NAICS industry k, county c, and (monthly) time t. The sample is a panel of Homebase workers from January 2020 to December 2021. *Job-Switcher*$_j$ is a dummy variable equal to one if a worker switched jobs during the sample period by moving to a different establishment. Y denotes nominal average hourly wage, hours worked, or total earnings. $\Delta Y_{i,j,k,c,t}$ is the log difference in Y from January 2020 to month t. $Post_t$ is a dummy variable equal to one for $t \geq$ April 2020. The initial labor market shock, $Shock_c$, is either the direct measure or Bartik shock to local labor market tightness. Both measures have been standardized to unit standard deviation. $\boldsymbol{Z}_{i,j,k,c,t}$ contains control variables including worker fixed effects, state-industry-month fixed effects, lags of the labor market shock measured as of March 2020, February 2020, and January 2020, and county-level controls. The county-level controls include log median household income, COVID-19 cases and deaths per capita, and average tier 1 capital and core deposit ratios of all banks within the county. We weight each observation using its industry's pre-pandemic share of the labor force in 2019. Standard errors are clustered by county and time. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent, respectively. Source: BLS, Homebase, and authors' calculations.

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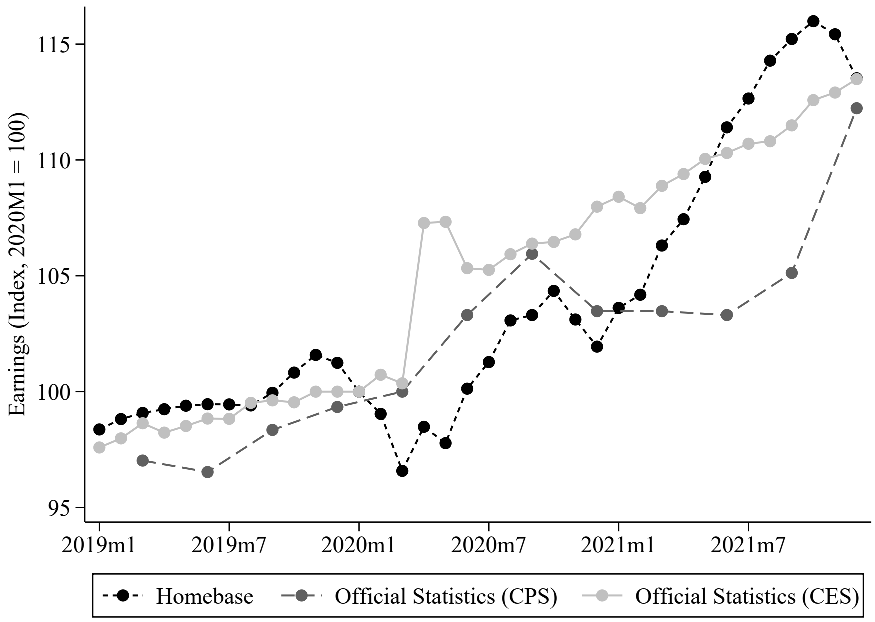
A Online Appendix

A.1 Additional results

Figure A1: Aggregate trends in earnings from Homebase and official statistics

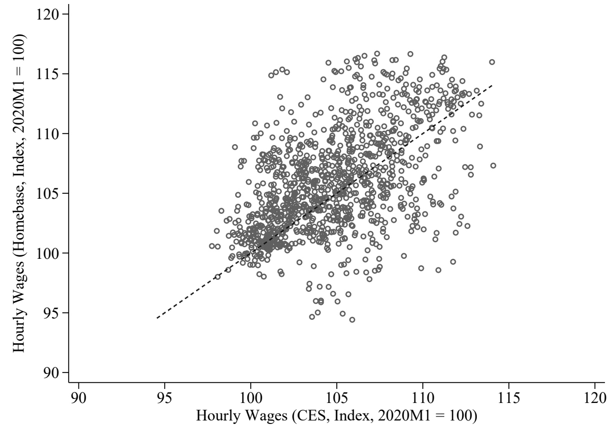


Notes: This figure compares the pre and post COVID-19 trend in average earnings in the Homebase data against the trends in official statistics. We consider two sources of official statistics, both from the BLS. The Current Population Survey (CPS) line plots the growth in median usual weekly nominal earnings of wage and salary workers in service occupations of 16 years and over (LEU0254543400Q). The Current Employment Statistics (CES) line plots the growth in average weekly earnings of all employees from the private service-providing sector (CES0800000011). The correlation between Homebase and CPS is 0.73; the correlation between Homebase and CES is 0.87. Each series has been expressed as indices relative to its value in January 2020 (and Q1 2020 for CPS). The Homebase sample excludes workers that were on temporary layoffs, defined as workers that were initially employed (reported positive earnings), were laid off during the middle of the sample (reported zero earnings), and later returned to work again before the end of the sample. The sample includes all workers that were in the database for at least one month between 2019 and 2021. Source: BLS, Homebase, and authors' calculations.

Figure A2: State-level earnings growth in Homebase and official statistics

(a) Earnings Growth

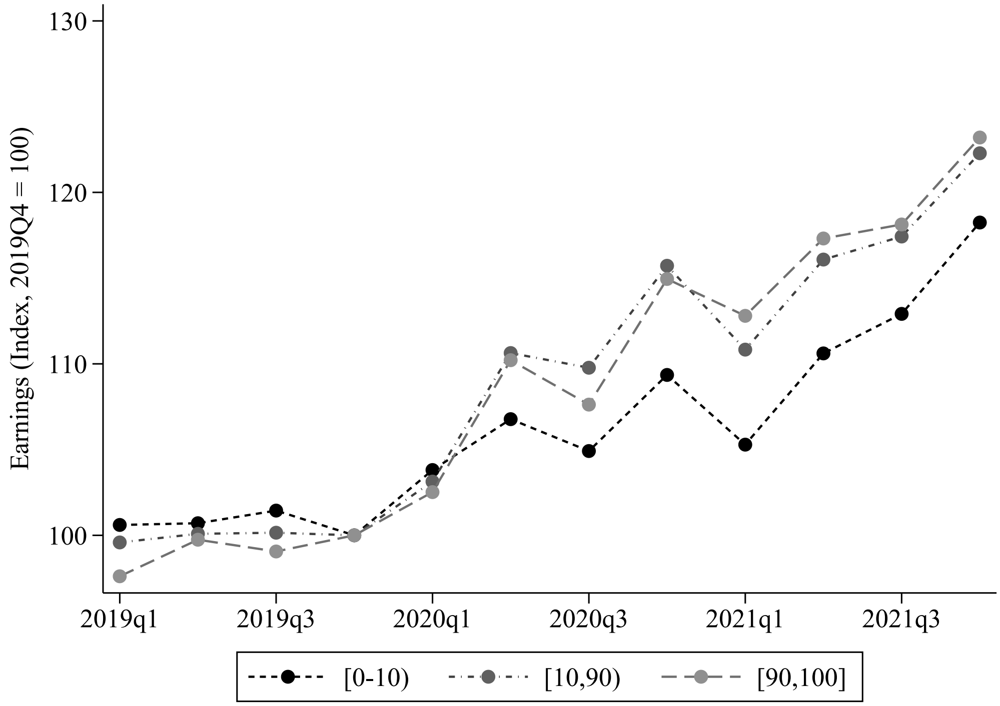
(b) Hourly Wage Growth



(c) Employment Growth

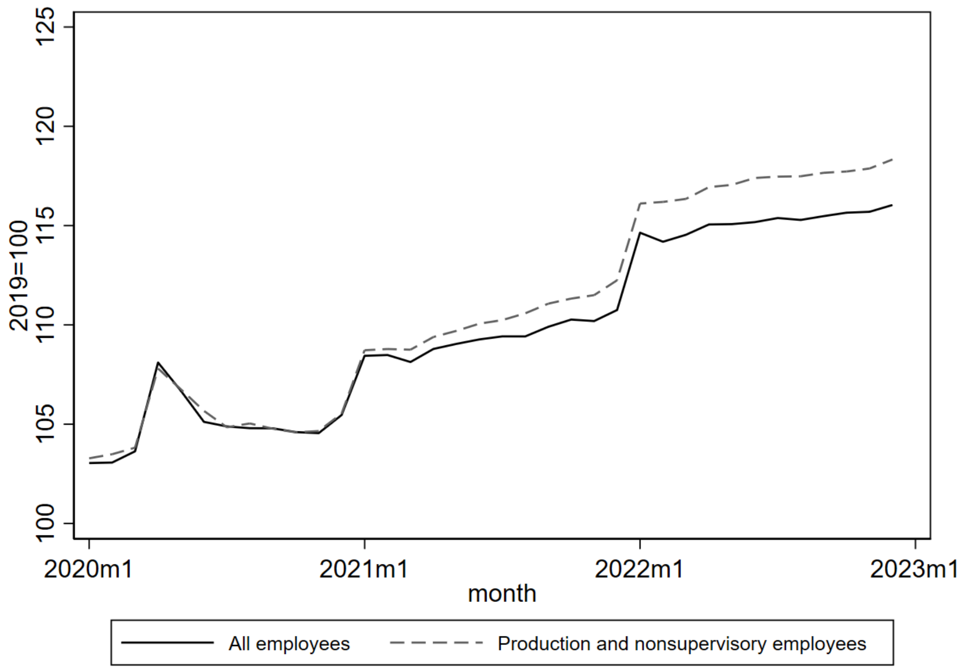
Notes: This figure compares growth in earnings, hourly wages, and employment relative to January 2020 between the Homebase data and official statistics (CES), by month and state. The Current Employment Statistics (CES) data uses state-level average weekly earnings for earnings (SMU[2-digit state FIPS code]000000800000011), average hourly earnings for hourly wages (SMU[2-digit state FIPS code]000000800000003), and number of employees for employment (SMS[2-digit state FIPS code]000000800000001), for all employees from the private service-providing sector. The correlation between Homebase and CES is 0.45 for earnings, 0.51 for hourly wages, and 0.69 for employment. Employment is defined as the number of employees that received any pay during the month in each state. Each series has been expressed as indices relative to its value in January 2020. Dashed line plots the 45 degree line from the origin. The Homebase sample excludes workers that were on temporary layoffs, defined as workers that were initially employed (reported positive earnings), were laid off during the middle of the sample (reported zero earnings), and later returned to work again before the end of the sample. The sample includes all workers that were in the database for at least one month between 2019 and 2021. Source: BLS, Homebase, and authors' calculations.

Figure A3: Trends in average weekly wages across counties: Official statistics (QCEW)



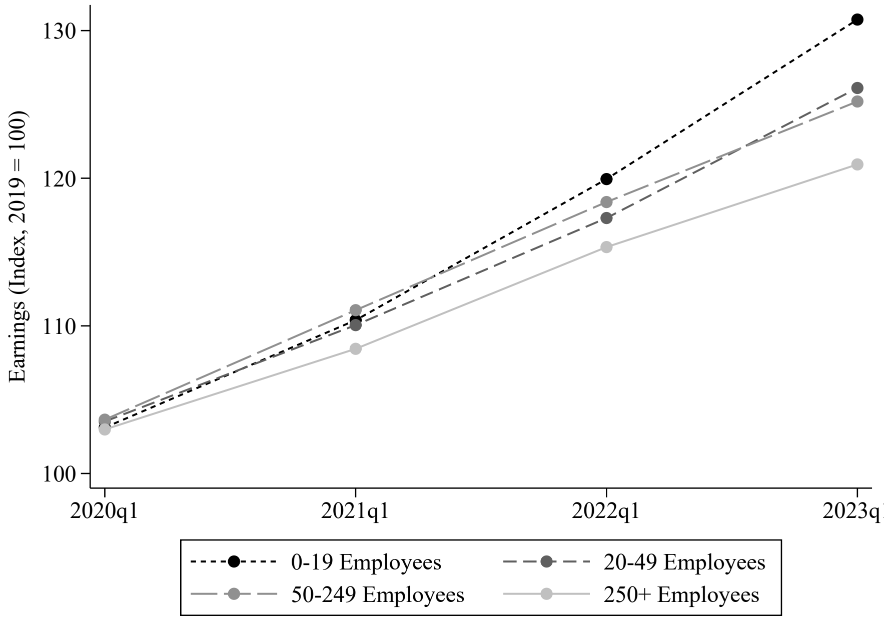
Notes: This figure plots the pre and post COVID-19 trend in average weekly wages in the official statistics from the Quarterly Census of Employment and Wages (QCEW). The sample is a quarterly panel of counties covering the service-providing sector workers from Q1 2019 to Q4 2021. Counties are ranked based on their average earnings growth in Q1 2020 relative to Q4 2019. Each series has been expressed as indices relative to its value in Q4 2019. Source: Bureau of Labor Statistics.

Figure A4: Average hourly wage in leisure and hospitality: CES data



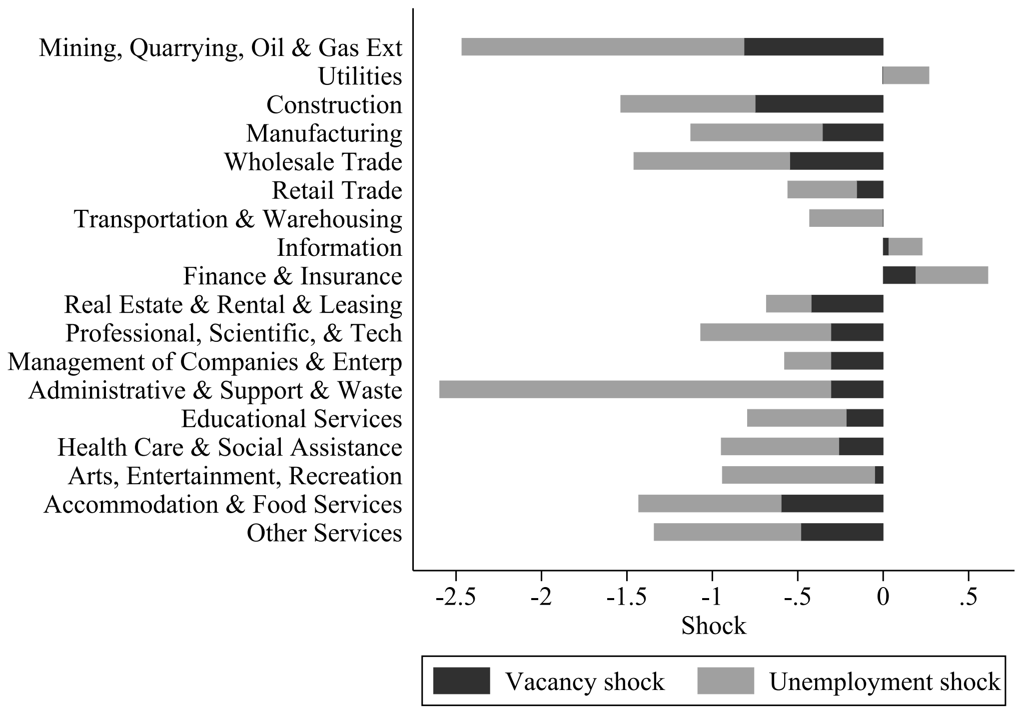
Notes: This figure plots the average hourly wage in the leisure and hospitality sector. The blue line is for all employees and the red line is for production and nonsupervisory employees. Wages are seasonally adjusted. The level in the same month in 2019 is indexed to 100. Source: Bureau of Labor Statistics.

Figure A5: Labor market dynamics by firm size: QCEW



Notes: This figure plots the average earnings of private sector employees by various size bins of their employer. Earnings is measured using average weekly wages based on employees covered in the Quarterly Census of Employment and Wages (QCEW) from the first quarter of each year. For each series, the level in the same quarter in 2019 is indexed to 100. Source: Bureau of Labor Statistics.

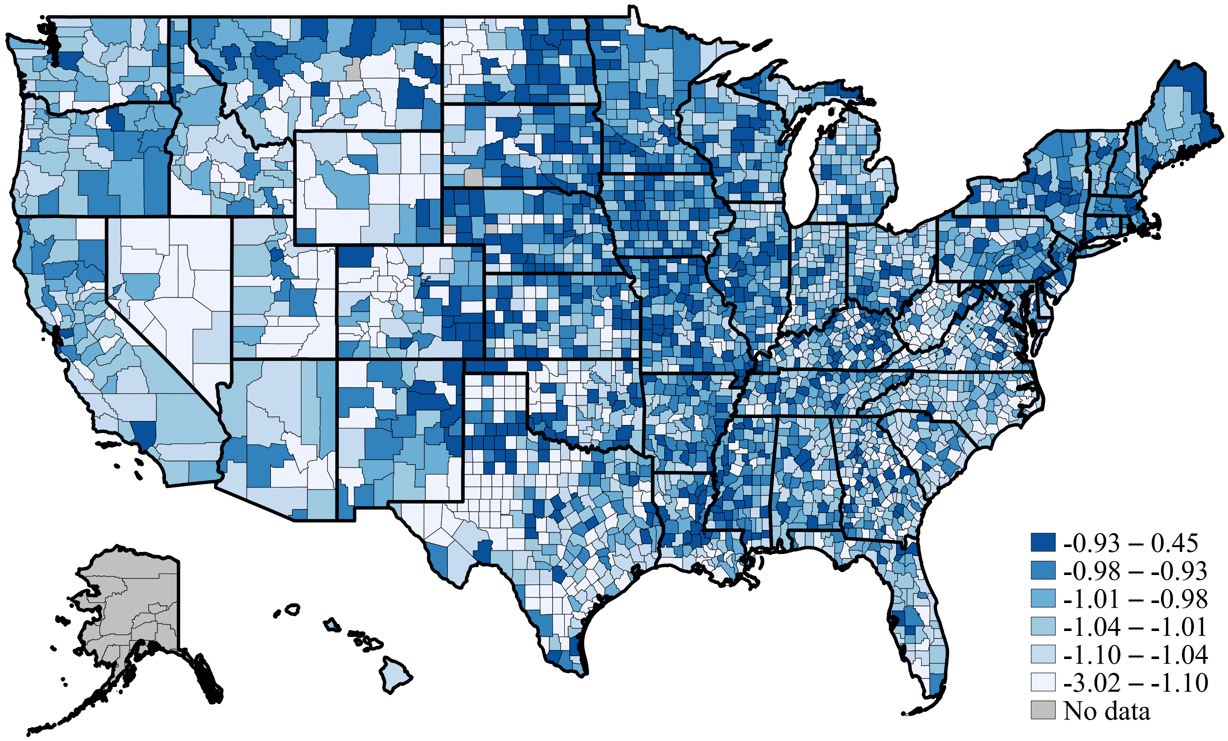
Figure A6: Industry-level shocks



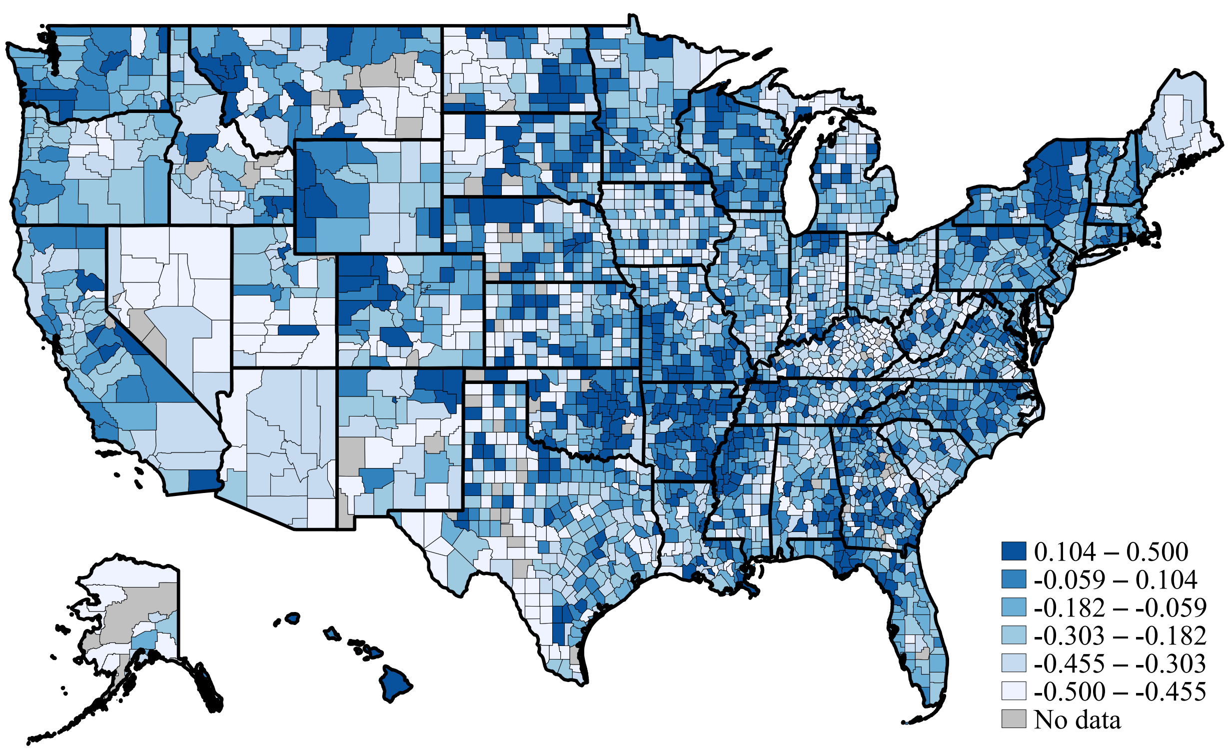
Notes: This figure plots the distribution of the initial labor market shock, $Shock_{c,k} = (\Delta \ln(V)_{k,\text{April 2020}} - \Delta \ln(V)_{k,\text{April 2019}}) - (\Delta \ln(U)_{k,\text{April 2020}} - \Delta \ln(U)_{k,\text{April 2019}})$, across 2-digit NAICS industries k. The initial labor market shock is defined in 2. The *Vacancy shock* bar plots the shock from the number of firms' vacancy postings $(\Delta \ln(V)_{k,\text{April 2020}} - \Delta \ln(V)_{k,\text{April 2019}})$, while the *Unemployment shock* bar plots the shock from the unemployment level $-(\Delta \ln(U)_{k,\text{April 2020}} - \Delta \ln(U)_{k,\text{April 2019}})$. The initial labor market shock $Shock_{c,k}$ is the sum of these two components. Source: Bureau of Labor Statistics and authors' calculations.

Figure A7: Distribution of local labor market shocks and PPP exposure across counties

(a) Local labor market tightness ($Shock_c$)

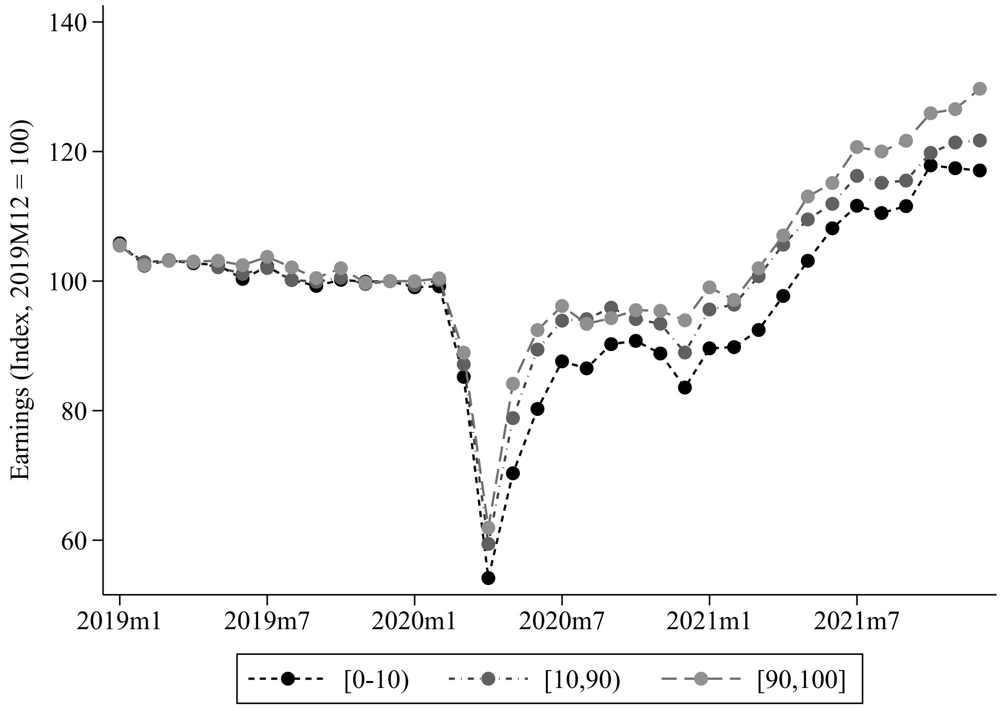


(b) PPP exposure ($PPPE_c$)



Notes: This figure plots the distribution of the initial labor market shock, $Shock_c$, and exposure to PPP lending, $PPPE_c$ across U.S. counties. Both variables are defined in 2. Source: BLS, SBA, and authors' calculations.

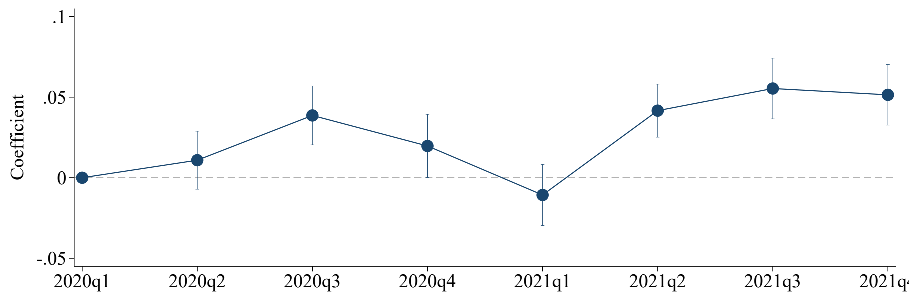
Figure A8: Trends in average earnings by shocks to labor market tightness



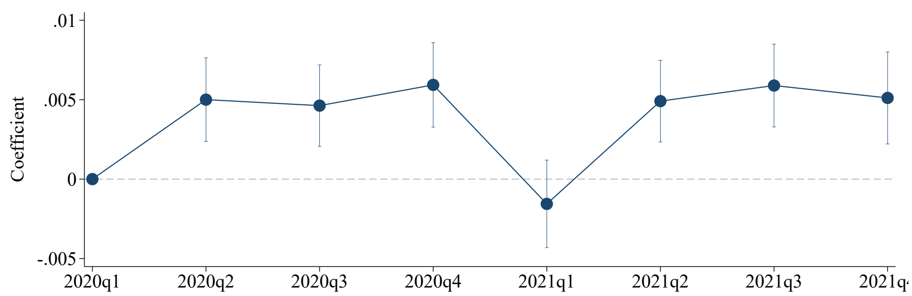
Notes: This figure plots the pre and post COVID-19 trend in average earnings in the Homebase data. The sample is a monthly panel of Homebase workers from January 2019 to December 2021. Workers were sorted into percentile groups based on the county in which they were employed. Counties are ranked by the Bartik shock to local labor market tightness defined in equation (3). Each series has been expressed as indices relative to its value in December 2019. The Homebase sample includes workers that were on temporary layoffs, defined as workers that were initially employed (reported positive earnings), were laid off during the middle of the sample (reported zero earnings), and later returned to work again before the end of the sample. The sample is restricted to workers that were in the database for at least 2 years between 2019 and 2021, including periods of temporary layoffs. Source: Homebase and authors' calculations.

Figure A9: County-level earnings: all industries

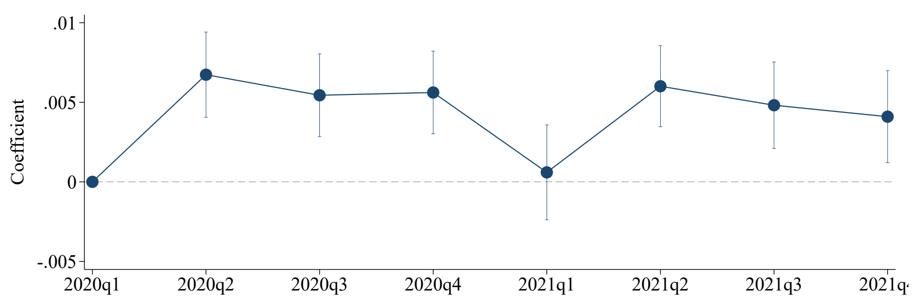
(a) Local labor market shock ($\boldsymbol{\alpha}_1$)



(b) PPP exposure ($\boldsymbol{\alpha}_2$)



(c) Local labor market tightness $\times$ PPP exposure ($\boldsymbol{\alpha}_3$)



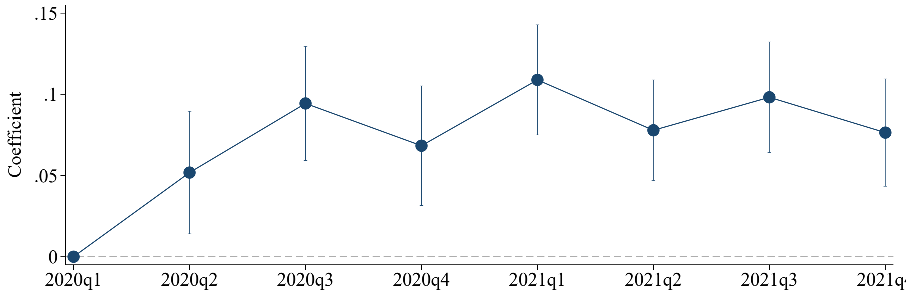
Notes: This figure plots the estimated coefficients from the following regression:

$$\Delta Y_{k,c,t} = \alpha_0 + \boldsymbol{\alpha}_1'[\boldsymbol{I}_t \times Shock_c] + \boldsymbol{\alpha}_2'[\boldsymbol{I}_t \times PPPE_c] + \boldsymbol{\alpha}_3'[\boldsymbol{I}_t \times Shock_c \times PPPE_c] + \boldsymbol{\beta}'\boldsymbol{Z}_{k,c,t} + e_{k,c,t}$$

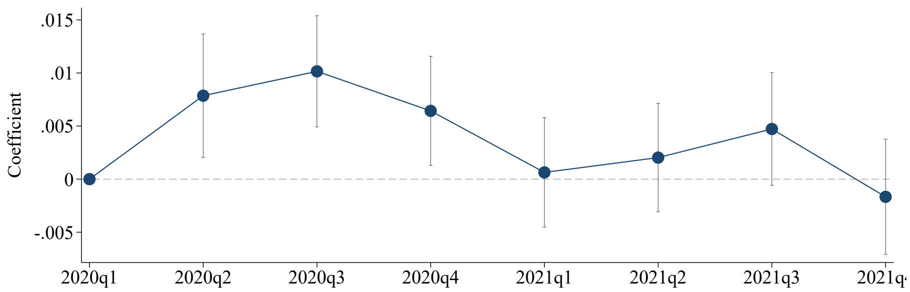
for 2-digit NAICS industry k, county c, and time t. The sample is a quarterly panel of counties by 2-digit NAICS industries over Q1 2020 to Q4 2021 from the QCEW. Y denotes earnings, measured using nominal average weekly wages. $\Delta Y_{k,c,t}$ is log difference in Y from Q1 2020 to t. $\boldsymbol{I}_t$ is a vector of quarter dummies from Q2 2020 to Q4 2021. Initial labor market shocks (Bartik), $Shock_c$, and exposure to PPP lending, $PPPE_c$, are standardized to unit standard deviation. $\boldsymbol{Z}_{k,c,t}$ contains control variables including county fixed effects, state-industry-quarter fixed effects, lags of the labor market shock measured as of March 2020, February 2020, and January 2020, and county-level controls including log median household income, COVID-19 cases and deaths per capita, and average tier 1 capital and core deposit ratios of all banks within the county. We weight each observation using its industry's share of the labor force in 2019. Standard errors are clustered by county and time. Vertical whiskers plot 90 percent confidence intervals.

Figure A10: County-level earnings: accommodation and food services

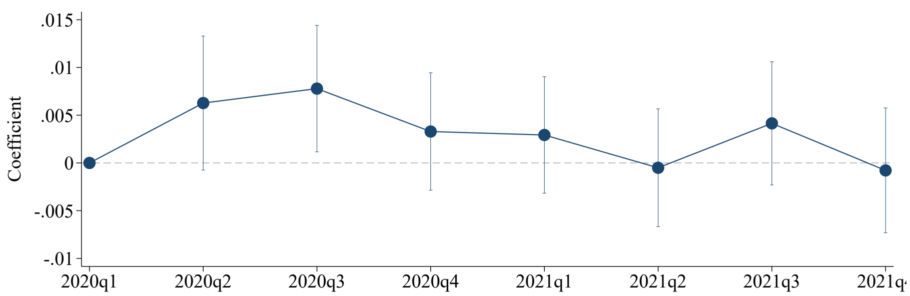
(a) Local labor market tightness ($\boldsymbol{\alpha}_1$)



(b) PPP exposure ($\boldsymbol{\alpha}_2$)



(c) Local labor market tightness $\times$ PPP exposure ($\boldsymbol{\alpha}_3$)



Notes: This figure plots the estimated coefficients from the following regression:

$$\Delta Y_{k,c,t} = \alpha_0 + \boldsymbol{\alpha}_1'[\boldsymbol{I}_t \times Shock_c] + \boldsymbol{\alpha}_2'[\boldsymbol{I}_t \times PPPE_c] + \boldsymbol{\alpha}_3'[\boldsymbol{I}_t \times Shock_c \times PPPE_c] + \boldsymbol{\beta}'\boldsymbol{Z}_{k,c,t} + e_{k,c,t}$$

for 2-digit NAICS industry k, county c, and time t. The sample is a quarterly panel of counties covering the accommodation and food services industry over Q1 2020 to Q4 2021 from the QCEW. Y denotes earnings, measured using nominal average weekly wages. $\Delta Y_{k,c,t}$ is log difference in Y from Q1 2020 to t. $\boldsymbol{I}_t$ is a vector of quarter dummies from Q2 2020 to Q4 2021. Initial labor market shocks (Bartik), $Shock_c$, and exposure to PPP lending, $PPPE_c$, are standardized to unit standard deviation. $\boldsymbol{Z}_{k,c,t}$ contains control variables including county fixed effects, state-industry-quarter fixed effects, lags of the labor market shock measured as of March 2020, February 2020, and January 2020, and county-level controls including log median household income, COVID-19 cases and deaths per capita, and average tier 1 capital and core deposit ratios of all banks within the county. We weight each observation using its industry's share of the labor force in 2019. Standard errors are clustered by county and time. Vertical whiskers plot 90 percent confidence intervals.

Table A1: Alternative measures of local labor market tightness

	(1) Hourly Wages	(2) Hours	(3) Total Earnings	(4) Hourly Wages	(5) Hours	(6) Total Earnings
$Post_t \times Shock_c$	-0.0387***	-0.0534***	-0.0902***	-0.0558***	-0.0746***	-0.1235***
	(0.0032)	(0.0040)	(0.0069)	(0.0132)	(0.0192)	(0.0310)
Observations	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354
R^2	0.52	0.51	0.52	0.49	0.48	0.49
Measure	Quits	Quits	Quits	Unemployment	Unemployment	Unemployment
Specification	Bartik	Bartik	Bartik	Bartik	Bartik	Bartik
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Worker FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Industry-Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficients from the following regression:

$$\Delta Y_{i,j,k,c,t} = \alpha_0 + \alpha_1 Post_t \times Shock_c + \boldsymbol{\beta}' \boldsymbol{Z}_{i,j,k,c,t} + e_{i,j,k,c,t}$$

for establishment i, worker j, 4-digit NAICS industry k, county c, and (monthly) time t. The sample is a panel of Homebase workers from January 2020 to December 2021. Y denotes nominal average hourly wage, hours worked, or total earnings. $\Delta Y_{i,j,k,c,t}$ is the log difference in Y from January 2020 to month t. $Post_t$ is a dummy variable equal to one for $t \geq$ April 2020. Initial labor market shocks, $Shock_c^Q$ (Quits), are defined as in 2, except using the JOLTS quits rate in place of the vacancy to unemployment ratio. $Shock_c^U$ (Unemployment) is defined as in 2, except using the Labor Force Statistics unemployment rate in place of the vacancy to unemployment ratio. The labor market shocks are standardized to unit standard deviation. $\boldsymbol{Z}_{i,j,k,c,t}$ contains control variables including worker fixed effects, state-industry-month fixed effects, lags of the labor market shock measured as of March 2020, February 2020, and January 2020, and county-level controls. The county-level controls include log median household income, COVID-19 cases and deaths per capita, and average tier 1 capital and core deposit ratios of all banks within the county. We weight each observation using its industry's pre-pandemic share of the labor force in 2019. Standard errors are clustered by county and time. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent, respectively. Source: BLS, Homebase, and authors' calculations.

Table A2: Robustness to contemporaneous shocks

	(1) Hourly Wages	(2) Hours	(3) Total Earnings	(4) Hourly Wages	(5) Hours	(6) Total Earnings
$Post_t \times Shock_c$	0.0905***	0.1233***	0.2108***	0.0276*	0.0660***	0.0959***
	(0.0176)	(0.0225)	(0.0383)	(0.0144)	(0.0180)	(0.0309)
Observations	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354
R^2	0.52	0.51	0.52	0.52	0.51	0.52
Specification	OLS	OLS	OLS	Bartik	Bartik	Bartik
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Worker FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Industry-Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table reports the estimated coefficients from the following regression:

$$\Delta Y_{i,j,k,c,t} = \alpha_0 + \alpha_1 Post_t \times Shock_c + \alpha_2 Shock_{c,t} + \alpha_3 Shock_{c,t-1} + \boldsymbol{\beta}' \boldsymbol{Z}_{i,j,k,c,t} + e_{i,j,k,c,t}$$

for establishment i, worker j, 4-digit NAICS industry k, county c, and (monthly) time t. The sample is a panel of Homebase workers from January 2020 to December 2021. Y denotes nominal average hourly wage, hours worked, or total earnings. $\Delta Y_{i,j,k,c,t}$ is the log difference in Y from January 2020 to month t. $Post_t$ is a dummy variable equal to one for $t \geq$ April 2020. The initial and contemporaneous labor market shock, $Shock_c$ and $Shock_{c,t}$, are the initial and contemporaneous shocks to labor market tightness defined in equation (3) and (9), respectively. Both measures are standardized to unit standard deviation. $\boldsymbol{Z}_{i,j,k,c,t}$ contains control variables including worker fixed effects, state-industry-month fixed effects, lags of the labor market shock measured as of March 2020, February 2020, and January 2020, and county-level controls. The county-level controls include log median household income, COVID-19 cases and deaths per capita, and average tier 1 capital and core deposit ratios of all banks within the county. We weight each observation using its industry's pre-pandemic share of the labor force in 2019. Standard errors are clustered by county and time. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent, respectively. Source: BLS, Homebase, and authors' calculations.

A.2 A monopsonistic model of labor market

We present a simple monopsonistic model of the labor market with firms with heterogeneous productivity, building on Card et al. (2018) and Dustmann et al. (2022). In the model, firms have some wage-setting powers because workers care about not only wage but also non-peculiary aspects of the job. As a result, less productive firms are smaller and offer lower wages; however, a tighter labor market leads to relatively faster wage growth in these firms.

A.2.1 Model set up

Market structure Consider a market with J firms and a mass L of workers. For simplicity, assume that all workers have the same skill but heterogeneous preference for working at a firm due to non-pecuniary preference for the firm, such as distance to work. The indirect utility of worker i from working at firm j is

$$U_{ij} = \beta \ln(w_j) + \nu_{ij},$$

where w_j is the wage that firm j pays to all its workers and ν_{ij} captures idiosyncratic preferences for working at firm j. We assume that ν_{ij} are drawn from a Type I Extreme Value distribution. Workers are free to work at any firm they wish given wages posted by the firms. By standard argument (McFadden, 1973), the probability that a worker choose to work for firm

j equals

$$P\big(\underset{k\in 1,\ldots,J}{argmax}(u_{ik}) = j\big) = \frac{w_j^\beta}{\sum_{k=1}^J w_j^\beta} = \frac{w_j^\beta}{\Phi}.$$

For simplicity, we assume the number of firms J is very large, in which case, $\Phi \equiv \sum_{k=1}^J w_j^\beta$ is a constant common to all firms. As a result, firms face a upward-sloping labor supply function, expressed in logs as:

$$\ln(l_j(w_j)) = \beta \ln(w_j) + \ln(L) - \ln(\Phi).$$

As such, the labor supply elasticity is equal to $\beta > 0$.

Optimal labor and wage Firms have a production function of the form $y_j = p_j \ln(l_j)$, where p_j is a firm-specific productivity shifter. They face variable labor costs and a fixed cost F, and make hiring decision to maximize profits:

$$\max_{w_j} p_j \ln(l_j(w_j)) - w_j l_j(w_j) - F. \tag{10}$$

Substituting in the labor supply function and taking the first order condition give the following results on optimal labor and wage:

$$\ln(l_j) = \frac{\beta}{1+\beta}\ln\big(p_j\frac{\beta}{1+\beta}\big) + \frac{1}{1+\beta}(\ln L - \ln\Phi), \tag{11}$$

and

$$\ln(w_j) = \frac{1}{1+\beta}\ln\big(p_j\frac{\beta}{1+\beta}\big) - \frac{1}{1+\beta}(\ln L - \ln\Phi). \tag{12}$$

Differentiating (11) and (12) with respect to p_j gives $\frac{d\ln(l_j)}{d\ln(p_j)} > 0$ and $\frac{d\ln(w_j)}{d\ln(p_j)} > 0$. Therefore, more productive firms employ more workers and pay higher wages. In other words, optimal labor and wage are increasing with productivity. Less productive firms are smaller and pay less.

Comparative statics One implication of the model is that higher labor supply elasticity leads to stronger wage growth in less productive (and smaller) firms. To see this, consider the relative wages between two firms $j \in \{H, L\}$ with different productivity $p_H > p_L$. The optimal wage (12) implies

$$\ln(w_L) - \ln(w_H) = \frac{1}{1+\beta}(\ln(p_L) - \ln(p_H)),$$

Differentiating with respect β gives $\frac{\partial(\ln(w_L)-\ln(w_H))}{\partial\beta} = \frac{\ln(p_H)-\ln(p_L)}{(1+\beta)^2} > 0$. It follows that the wages in less productive (and smaller) firms grow faster than in more productive (and larger) firms following an increase in labor supply elasticity β. Because less productive firms pay lower wages, it follows directly that the wage of lower-paid workers grows faster than that of higher-paid workers.

A.2.2 Labor market tightness and labor supply elasticity

We have shown how wages respond when labor supply becomes more elastic. In this section, we model labor supply elasticity using the ”quit elasticity”—which captures the sensitivity of worker separation to wages—in a simple job search framework following Autor et al. (2023). We show that quit elasticity increases with labor market tightness.

Consider a wage posting framework in which workers face shocks that lead to their separation from current firms. There are exogenous job destruction shocks that cause employed workers to be laid off into unemployment at a rate of δ. In addition, there are exogenous "godfather" shocks, which result in workers accepting outside offers at a rate of χ. In addition, workers also engage in on-the-job search with outside offers arriving at rate λ, and quit if they get a higher wage offer. The separate rate from on-the-job search is thus $\lambda(1-F(w))$, where $F(w)$ is the cumulative wage distribution. Taken together, the job separation rate at wage w is given by

$$S(w) = \delta + \chi + \lambda(1 - F(w)).$$

The quit elasticity with respect to wage is given by

$$\begin{aligned}\epsilon &\equiv \frac{\partial \ln S(w)}{\partial \ln w} \\ &= -\frac{\lambda f(w) w}{\delta + \chi + \lambda(1 - F(w))} < 0.\end{aligned}$$

One immediate implication is that the quit elasticity ϵ decreases with the offer arrival rate λ (i.e., $\frac{\partial \epsilon}{\partial \lambda} < 0$).

Another implication is that a higher offer arrival rate results in the reallocation of workers from low-wage firms to high-wage firms. To see this, recall that the employment-to-employment separation rate is given by $\chi + \lambda(1 - F(w))$. Because $1 - F(w)$ is monotonically decreasing in w, higher offer arrival rate λ increases the separation rate at low-wage firms more than at high-wage firms.

Next we show that offer arrival rate λ is strictly increasing in labor market tightness measured by $\theta \equiv \frac{V}{JS}$, where V is the total number of vacancies posted by employers and JS is the total job-seeking effort exerted by workers. To see this, assume that JS depends on the share of unemployed workers U:

$$\begin{aligned}JS &= U + \phi(1-\delta)(1-U) \\ &= (1 - \phi(1-\delta))U + \phi(1-\delta)\end{aligned}$$

where $\phi > 0$ is the relative efficiency of on-the-job search. Assume $\phi < \frac{1}{1-\delta}$ so that JS is strictly increasing in U:

$$\frac{\partial JS}{\partial U} = 1 - \phi(1-\delta) > 0$$

The number of contacts between firm and workers depends on JS and V from a constant-returns matching function $m(JS, V)$. With this, the offer arrival rate for employed workers can be written as $\lambda \equiv \frac{m(JS,V)}{JS} = m(1,\theta)$. It follows that λ increases in θ:

$$\frac{\partial \lambda}{\partial \theta} = \frac{\partial m(1,\theta)}{\partial \theta} > 0$$

The offer arrival rate λ is also strictly increasing in labor market tightness measured as the vacancy-to-unemployment ratio $\widetilde{\theta} \equiv V/U$:

$$\frac{\partial \lambda}{\partial \widetilde{\theta}} = \frac{\partial \lambda}{\partial \theta}\frac{\partial \theta}{\partial \widetilde{\theta}} = \underbrace{\frac{\partial \lambda}{\partial \theta}}_{>0}\underbrace{\frac{\partial (V/JS)}{\partial (V/U)}}_{>0} > 0.$$

A.2.3 The role of PPP

Consider an exogenous shock to productivity from the pandemic. Firms may borrow from the PPP to finance their wage bills, with an exogenous borrowing limit ξ_j, which increases with PPP exposure.[16] Firms make hiring decision to maximize profits:

$$\max_{w_j} p_j \ln(l_j(w_j)) - w_j l_j(w_j) - F,$$
$$s.t. \quad w_j l_j(w_j) \le \xi_j.$$

In Lagrangian form:

$$\mathcal{L}(w_j) = p_j \ln\left(L\frac{w_j^{\beta}}{\Phi}\right) - L\frac{w_j^{\beta+1}}{\Phi} + \mu_j\left(\xi_j - L\frac{w_j^{\beta+1}}{\Phi}\right)$$

where $\mu_j \ge 0$ is the Lagrange multiplier on the borrowing constraint.

If the borrowing limit is not binding, the problem simplifies to (10). Optimal labor and wage are given by (11) and (12) If the borrowing limit is binding, then the optimal labor and wage depend on the borrowing limit. To see this, consider two firms $j \in \{1, 2\}$ with identical productivity. Firm 1 has a higher borrowing limit than firm 2, $\xi_1 > \xi_2$, such that the borrowing limit is binding for firm 2 but not for firm 1, $\mu_1 = 0$ and $\mu_2 > 0$. Substituting in the labor supply function and taking the first-order condition with respect to wage gives the relative wage for the two firms:

$$\frac{w_1}{w_2} = \left(\frac{1}{1+\mu_2}\right)^{\frac{1}{\beta+1}}$$

Taking logs and differentiating with respect to β gives

$$\frac{\partial(\ln(w_1) - \ln(w_2))}{\partial\beta} = \frac{1}{(\beta+1)^2}(\ln(1+\mu_2)) > 0$$

Therefore, wage at the unconstrained firm (with a higher PPP exposure) grows faster than the wage at the constrained firm (with a lower PPP exposure) following an increase in labor supply elasticity β.

[16] Results are similar if PPP can be used to finance wage bill $w_j l_j$ as well as non-wage costs F.