INTERNATIONAL MONETARY FUND

Divergence in Post-Pandemic Earnings Growth:

Evidence from Micro Data

Sophia Chen and Do Lee

WP/24/222

2024
OCT

WORKING PAPER

 WP/24/222

IMF Working Paper
Research Department

Divergence in Post-Pandemic Earnings Growth: Evidence from Micro Data
Prepared by Sophia Chen and Do Lee *

Authorized for distribution by Maria Soledad Martinez Peria
October 2024

ABSTRACT: We use a comprehensive employer-employee dataset to examine post-pandemic worker earnings in the US. Our findings reveal that earnings grew faster in counties that were less severely impacted at the onset of the pandemic. This divergence in growth was both substantial and persistent, particularly for lower-paid and nonmanagerial workers, as well as for those in smaller firms. Both wage increases and additional hours contributed to this earnings growth. This evidence aligns with a job-ladder framework, where labor market competition leads to a dispersion of earnings across counties but compresses earnings among workers in counties with strong labor markets. Our findings provide a microfoundation for the wage Phillips curve and have direct implications for stabilization policies.

RECOMMENDED CITATION: Sophia Chen and Do Lee. 2024. Divergence in Post-Pandemic Earnings Growth: Evidence from Micro Data”. IMF Working Paper No. 2024/222.

JEL Classification Numbers:	J23, J31, E24
Keywords:	Earnings; Wage; Labor Market Conditions; Pandemic
Author’s E-Mail Address:	ychen2@imf.org, dql204@nyu.edu

* The authors are grateful to Philip Barrett, Jason Greenberg, Deniz Igan, and Sole Martinez Peria for helpful comments and suggestions.

WORKING PAPERS

Divergence in Post-Pandemic Earnings Growth:

Evidence from Micro Data

Prepared by Sophia Chen and Do Lee[1]

[1] The authors are grateful to Philip Barrett, Jason Greenberg, Deniz Igan, and Sole Martinez Peria for helpful comments and suggestions.

1 Introduction

The post-pandemic U.S. labor market saw a rapid divergence in earnings growth across the country. Counties that experienced the lowest earnings losses at the onset of COVID-19 saw the fastest earnings growth in 2020-2021. Figure 1 illustrates this pattern by tracing the evolution of average earnings of workers in counties sorted by earnings growth between December 2019 and April 2020. The data come from a proprietary employer-employee dataset by Homebase, covering 9 million workers in over 1 million establishments in the US. In counties within the top 10 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 10 percentile saw only a 5 percent rise during the same period.

This divergent pattern is the key motivation for this paper. We investigate drivers of earnings growth after the pandemic. We document a new fact: post-pandemic worker earnings grew faster in counties that experienced smaller labor market shocks at the onset of the pandemic, both compared to both the same counties before the pandemic 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 growth response to plausibly exogenous labor market shocks, and after controlling for a rich set of county-, sectoral, and worker-level factors. We estimate that, between April 2020 and December 2021, average earnings were 18 percent higher in counties with stronger labor markets——defined as counties experiencing a shock to labor market conditions one standard deviation above the average at the onset of the pandemic. The baseline shock is defined as a shift-share shock to the vacancy-to-unemployment ratio (in log), and similar results are obtained with alternative shock definitions. 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. Our finding is robust to alternative measures of labor market shocks and specifications.

A divergence in growth is also observed between counties that received more labor market support through the Paycheck Protection Program (PPP). As part of the Coronavirus Aid, Relief, and Economic Security (CARES) Act, the PPP provided liquidity to small and mid-size businesses through guaranteed and forgivable loans, supporting the labor market by covering payroll expenses. The PPP boosts local labor market demand through two main channels. To receive loan forgiveness, businesses that obtained PPP loans had to maintain their employment and compensation levels unchanged, showing a direct effect of PPP funds on labor demand. Additionally, PPP loans could be used for non-payroll expenses, easing financial pressures that might otherwise result in cuts to employment and wages. Exploiting variations in the supply of PPP loans across counties as in Granja et al. (2022), we show that average earnings were 32 percent higher in counties with greater PPP support—defined as counties where PPP loan supply was one standard deviation above the average. This difference persisted until mid-2021.

These results suggest that a large and persistent shift in worker earnings occurred disproportionately in areas with stronger local labor markets immediately after the pandemic. Emerging literature after the pandemic highlights the significant role of labor market competition in driving nominal wage growth within a job-ladder framework. In a labor market that has experienced a large negative shock but is rapidly tightening, and with inelastic labor supply, more productive firms use competitive offers to attract workers. Less productive firms must either match these offers or risk losing their workers. As a result, workers earn higher wages as they climb the job ladder. The search-and-match frictions between firms and workers contribute to an adjustment process that is both persistent and widespread.

Consistent with this framework, aggregate data show a close link between wage growth and labor market strength, 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). This evidence is compelling and highlights the need for more detailed studies. Microdata are particularly valuable for addressing limitations inherent in aggregated series or survey data. Aggregated data often lack the granularity needed to explore cross-sectional variation, and the low frequency of survey data limits the analysis of high-frequency changes typical of the post-pandemic labor market. Moreover, aggregate time series or survey data do not account for heterogeneity in workers' skills. Our data's richness allows us to overcome these limitations, increasing the precision of our estimates and enabling a thorough examination of the underlying channels.

A key channel of earnings growth in the job-ladder framework is workers moving from lower-paid to higher-paid jobs. Aggregating observations from Homebase to the national level, we find supporting evidence that non-management employees experienced higher earnings growth than managers (Figure 2). The Current Employment Statistics survey (CES) provides consistent evidence (Figure A4).[1] To uncover the drivers of these differences, we again exploit variation in labor market strength across counties. If these earnings differentials were driven by competition for workers, we would expect lower-paid and nonmanagerial workers to experience faster earnings growth in stronger labor markets. Our results confirm this expectation. Our worker-level estimations suggest that the earnings difference between lower-paid and higher-paid workers is 75 percent higher in counties with stronger labor markets, while the difference between nonmanagerial and managerial workers is similar at 71 percent.

As workers move up the job ladder, they can secure jobs with higher wages and more hours, with the latter being particularly relevant for underemployed workers. We find evidence that both wage and hours adjustments contribute to earnings growth. Specifically, wage increases account for about one-third of the earnings growth, while additional hours contribute approximately two-thirds on average.

Moving up the job ladder can occur both within and across firms. When workers receive more competitive outside offers, their current employer may attempt to match the offer to retain them, or risk losing them. Consequently, we expect to see earnings growth for both job movers and job stayers, which is confirmed by our data. Post-pandemic earnings growth is, on average, 15 percent higher for

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

workers who switched firms compared to those who stayed. This difference is also somewhat larger in counties with strong labor markets, though the magnitude is relatively small. Job switchers benefit from productivity gains when moving to more productive firms, in addition to the general gains from labor market competition that affect both job switchers and stayers. However, 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.

The job-ladder framework also has implications for firms. As outside offers come from more productive firms, the current employer must match these offers to retain workers in a tightening labor market. Given that marginal costs for workers are often lower in less productive firms, this competition can result in higher within-firm earnings growth for workers in such firms.[2] We therefore expect to observe higher earnings growth among workers in less productive firms. Although our data do not provide direct information on firm productivity, we use firm size as a proxy, as smaller firms tend to be less productive than larger ones. We find that workers in smaller firms—defined as those with fewer than 50 employees—experience earnings growth that is 20 percent higher on average in counties with strong labor markets compared to workers in larger firms.

The granularity of our data is crucial for identifying these channels. In particular, we are able to control for time-invariant worker skills, worker composition, and time-varying demand shocks in narrowly defined state-by-industry groups. This helps mitigate concerns that simultaneity or measurement errors are driving our results.

Taken together, our results suggest that workers in counties with stronger labor markets experienced immediate and persistent post-pandemic gains driven by labor market competition. These gains are disproportionately higher for lower-paid workers, nonmanagerial workers, and workers in smaller firms. These results also have significant distributional implications. We observe that tightening local labor markets have increased earnings disparities across counties after the pandemic, while simultaneously narrowing earnings differences among workers within counties with stronger labor markets.

Our findings contribute to the emerging literature on 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 strength. 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 from a job-ladder framework but they do not examine divergence in earnings growth across different areas and across firms. Our finding on the relative movement on low-wage workers from microdata complement their finding from the aggregate data. The use of microdata not only allows us to address shortcomings

[2] For less productive firms to cover higher labor costs, their marginal revenue must also increase, as evidenced by post-pandemic price hikes, particularly in the service sector.

in the aggregate as we discussed, but also allows us to explore the additional variation across local labor markets. This cross-sectional variation is important because, as predicted by a new Keynesian Philips curve, slack in the local market is a key determinant of wages. In addition, our data allows us to examine wages and working hours separately, offering a comprehensive view of worker earnings, as well as to quantify various channels of adjustment at both the worker and firm levels. More generally, our findings add to the literature on the role of labor market conditions on employment and wage outcomes outside of the pandemic period (Moscarini and Postel-Vinay, 2017; Haltiwanger et al., 2018; Hershbein and Kahn, 2018; Barrett et al., 2024). Finally, our paper is also related to the literature that examine the effect of PPP. Granja et al. (2022) exploit regional heterogeneity in PPP lending and find small employment impact of the PPP. The costs per job under the program is estimated by exploiting variation eligibility rule (Autor et al., 2023) and in delay in loan provision (Domash and Summers, 2022). But these papers do not consider earnings or wages, or link employment outcome to 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 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 offer a microfoundation for aggregate wage adjustment based on labor reallocation within a job ladder. They also elucidate the channels of this adjustment: it occurs through both within-job transitions and 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) demonstrate that incorporating wage adjustment frictions, as described in this paper, into the price Phillips curve estimation reveals labor market slack as a key driver of the large and persistent inflation in service prices following 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 may not fully represent aggregate employment.

Despite these limitations, 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 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

[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 2024 for all employees but not for production and nonmanagerial employees (A4).

0.51 for hourly wages and 0.69 for employment.

In summary, 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 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).

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

2.2 Stylized facts

In this section, we elaborate the three key facts on the divergent trends in worker earnings growth from the Homebase data, as discussed earlier in the paper. Where possible, we supplement these findings with supporting evidence from official statistics to strengthen the 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 10$^{\text{th}}$ percentile, between the 10$^{\text{th}}$ and 90$^{\text{th}}$ percentiles, and above the 90$^{\text{th}}$ 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. The Quarterly Census of Employment and Wages (QCEW) provides quarterly data on employment and earnings reported by employers, covering over 95 percent of the U.S. workforce. 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 2 plots the time trends of earnings for managerial and nonmanagerial workers. An 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 a 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 2 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 3 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 Earnings growth and labor market conditions

3.1 The role of labor market strength

3.1.1 Empirical specification

We aim to understand post-pandemic worker earnings and assess how local labor market strength affects earnings growth. We do this by exploiting variation in the local labor market strength across U.S. counties. This approach has advantages over using time series data over a relatively short period and is potentially confounded with concurrent trends that may affect earnings.

We start by comparing pre- and post-pandemic average earnings at the worker level using the following regression:

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

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 growth rate in Y from January 2020 to month t measured as log difference. $Post_t$ is a dummy variable equal to one from April 2020 to December 2021. As Figure 4 shows, labor market conditions tightened to unprecedented levels as measured by unemployment or the vacancy-to-unemployment ratio in April 2020 followed by rapid loosening and stabilization during 2021. $Shock_c$ is a county-specific measure of labor market strength to be discussed in Section 3.1.2. This specification relates local labor market condition to the average post-pandemic growth in outcome Y relative to the pre-pandemic period. We standardize $Shock_c$ to 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 control 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 February 2020 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 timing and persistence of the impact of shocks to labor market strength.

A significant 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 4, 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 strength. 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

[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.

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 relationship. In Section 3.4, we also consider traditional measures for labor market tightness.

We construct the shock to local labor market strength, $Shock_c$, 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 4, 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 tradable-demand instrument used by Hazell et al. (2022), this approach captures the idea that national variation demand for specific tradable goods will have varying effects on local demand in non-tradable sectors, depending on the local exposure to the impacted 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:

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

where:

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

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

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.

[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 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 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

[9]We discuss local banking sector conditions in more details in Section 3.2.

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 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 strength, such as low local unemployment driven by shifts in worker earnings that affect labor supply. 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 strength, while columns 4 to 6 report results using the Bartik shock. Across both measures, we observe a significant positive effect of local labor market strength 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 strength leads to a 6 percent increase in hourly wage, a 12 percent increase in hours worked, and an 18 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 strength. For example, local labor force participation may decrease following the pandemic, which could be negatively correlated with labor market strength. The Bartik shock helps mitigate this issue by isolating variations in labor market strength driven by industry-specific demand shocks.

Despite the differences in coefficient magnitude between the two measures, both suggest that wage and hours contribute similarly to total earnings growth. Hourly wage accounts for approximately one-third of the total earnings increase, while hours worked explain about two-thirds.

Figure 5 plots the estimated coefficients α_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, the levels of earnings, wages, and hours in February and March 2020 are nearly identical to those in January 2020, suggesting 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 is persistent, with a sharp initial response in April 2020 that gradually decreases. In April 2020, a one-standard-deviation increase in the Bartik shock to labor market strength is associated with approximately a 9.7 percent increase in hourly wage, a 14.6 percent increase in hours worked, and a 23.4 percent increase in total earnings. These effects taper off to around 15.5 percent, 10.1 percent, and 5.9 percent respectively in the second half of 2020. By the end of 2021, the impacts decrease further, with the coefficients falling to about 9.8 percent for hourly wage, 7.2 percent for hours, and 3.0 percent for total earnings.

The sustained response in total earnings is largely driven by the persistence in working hours. By the end of 2021, the effect on hours remains substantial and statistically significant at the 1 percent level, while the impact on hourly wages approaches close to zero. This pattern suggests that while initial wage growth fades over time, the lasting increase in total earnings comes from workers maintaining higher hours worked.

3.2 The role of PPP

In our second research design, we leverage the variation 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]

[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.

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.

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 funds 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 funds in different areas was likely 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 was mediated through improvements in local labor market conditions, such as the vacancy-to-unemployment ratio. Understanding this mechanism helps evaluate how different aspects of labor

[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.

[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.

adjustment played a role. For instance, businesses may have reduced employee counts (contributing to unemployment), added new positions (increasing vacancies), or adjusted the wages and hours of current employees without changing headcounts or vacancies.

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 funds across geographic regions. The key idea is to use variations in PPP loan supply 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

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

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 PPP loan supply 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

[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.

to 0.04.

To evaluate the role of PPP on post-pandemic earnings, we estimate the following regression:

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

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 growth rate in Y from January 2020 to month t measured as log difference, $Z_{i,j,k,c,t}$ 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. We estimate

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

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 February 2020 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 strength measure, while columns 4 to 6 report our preferred results, using the Bartik shock. We observe similar coefficients of local labor market strength as reported in Table 2. Additionally, PPP exposure shows significant positive effects. In our preferred estimates (columns 4 to 6), a one-standard-deviation increase in PPP exposure leads to a 13 percent, 19 percent, and 32 percent rise in hourly wage, hours, and total earnings, respectively. Furthermore, PPP exposure modestly amplifies the effects of the Bartik shock, with a one-standard-deviation increase in PPP exposure increasing the Bartik shock's marginal effect by less than one-sixth. The relative contributions of hourly wage and hours to total earnings remain similar to previous estimates, with wage contributing about one-third and hours contributing two-thirds.

Figure 5 plots the estimated coefficients by month. The path for the coefficient of local labor market strength ($\boldsymbol{\alpha}_1$) mirrors that of Figure 5, reflecting a persistent effect. Regarding PPP exposure ($\boldsymbol{\alpha}_2$), we observe a large response between April and June 2020, aligning with the peak disbursement of PPP funds in the first two rounds. The effects taper off towards the end of 2020, and by May 2021, they are no longer statistically significant. However, there is a slight uptick in December 2020 and January 2021. This increase could potentially be explained by delayed loan forgiveness decisions, year-end fiscal adjustments, or new hires as businesses sought to meet forgiveness criteria. The indirect effect of PPP exposure ($\boldsymbol{\alpha}_3$) follows a similar trajectory to the direct effect, but with a magnitude less than one-tenth of that of $\boldsymbol{\alpha}_2$.

3.3 Differential impacts on workers and firms

We have discussed evidence on the role of labor market strength in driving earning growth. To better understand the mechanisms underlying these results, we delve into the differential impacts across various workers and firms. According to the job-ladder framework, intensified labor market competition in a tightening market would propel workers up the job ladder. This could manifest as upward movement on the wage ladder (from lower-paying to higher-paying jobs) or the hours ladder (from part-time to full-time employment). Labor reallocation may happen both within firms or across firms. In this section, we formally quantify the relative contributions of these reallocation margins.

We estimate the following regression:

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

for establishment i, worker j, 4-digit NAICS industry k, county c, and (monthly) time t. *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. 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, indicating that tight labor markets have a more pronounced impact on nonmanagerial workers compared to managers. Across all columns, we observe that nonmanagerial workers experience significantly larger increases in hourly wages, hours worked, and total earnings. Based on our preferred estimates in columns 4 to 6 (using the Bartik shock), the impact on nonmanagerial workers' average hourly wage is 107 percent greater than that of managers, 50 percent greater for hours worked, and 71 percent greater for total earnings.

A similar pattern emerges when comparing low-wage and high-wage workers, as shown in Table 5. 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 (based on the Homebase sample) and zero otherwise. The results suggest that the effect on low-wage workers is 62 percent greater for average hourly wage, 53 percent greater for hours worked, and 85 percent greater for total earnings compared to high-wage workers.

Table 6 compares the effects of a tight labor market on job switchers versus 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 tight labor markets have a greater impact on job switchers' hourly wages, hours, and total earnings compared to job stayers, with differences ranging from 4 percent to 7 percent. Despite this, job switchers experience significantly higher post-pandemic earnings growth compared to job stayers, averaging 8 percent more for hourly wages and hours, and 15 percent more for total earnings. However, the triple interaction term suggests that the impact of tight labor markets on this difference is relatively modest. It's important to note that this analysis may not capture all job switchers, particularly 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.

Table 7 examines the effects on workers in firms of different sizes. Smaller firms, which generally offer lower wages and fewer hours, show higher post-pandemic earnings growth compared to larger firms, particularly in tighter labor markets. We classify firms as "small" if they employ fewer than 50 workers and "large" if they employ 50 or more. The impact of a tight labor market on workers in smaller firms is 128 percent greater for average hourly wage, 50 percent greater for hours worked, and 72 percent greater for total earnings compared to workers in larger firms.

In sum, we find that workers in counties with stronger labor markets realized both immediate and sustained earnings gains post-pandemic. Notably, these gains were disproportionately higher for lower-paid and nonmanagerial workers, as well as those employed by smaller firms. These varied outcomes across different workers and firms are consistent with the mechanisms of labor market competition as predicted by the job-ladder framework.

These results carry important distributional implications. Figure 7 illustrates the changes in the distribution of worker 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 strength as measured by the Bartik shock. The figure demonstrates that counties with stronger labor market conditions experienced a more pronounced narrowing of the earnings distribution, primarily driven by a compression in the earnings gap between the 10$^{\text{th}}$ percentile and the 50$^{\text{th}}$ percentile of the earnings distribution.

3.4 Discussion

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

3.4.1 Alternative data sources

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 this, 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.

The results from the QCEW sample of accommodation and food services are qualitatively similar to those from the Homebase data. We observe a persistent response of wages to a tight labor market, although the initial response in Q2 2020 is more subdued in the QCEW sample. The direct and indirect effects of PPP exposure are similarly positive in 2020 but decline to close to zero in 2021. Additionally, the QCEW results for accommodation and food services are quantitatively similar to those for all industries. One exception is that, in the sample of all industries, the response to the labor market shock, $Shock_c$, temporarily dropped to levels not significantly different from zero in Q1 2021, whereas in the accommodation and food services sample, the response remained positive through 2021, consistent with the Homebase results. Finally, the estimated magnitudes of wage responses to labor market shocks are similar between Homebase and QCEW, but the estimations for wage responses to PPP are substantially smaller in QCEW compared to Homebase.

3.4.2 Alternative measures of labor market strength

We then explore whether the choice of labor market strength 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 positively to the quit rate and negatively to 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.

3.4.3 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 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 strength evolved and resembled a transition path from a large shock back to a steady state, as shown in Figure 4. 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 strength. To do this, we define the contemporaneous shock, $Shock_{c,t}$, to labor market strength 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.

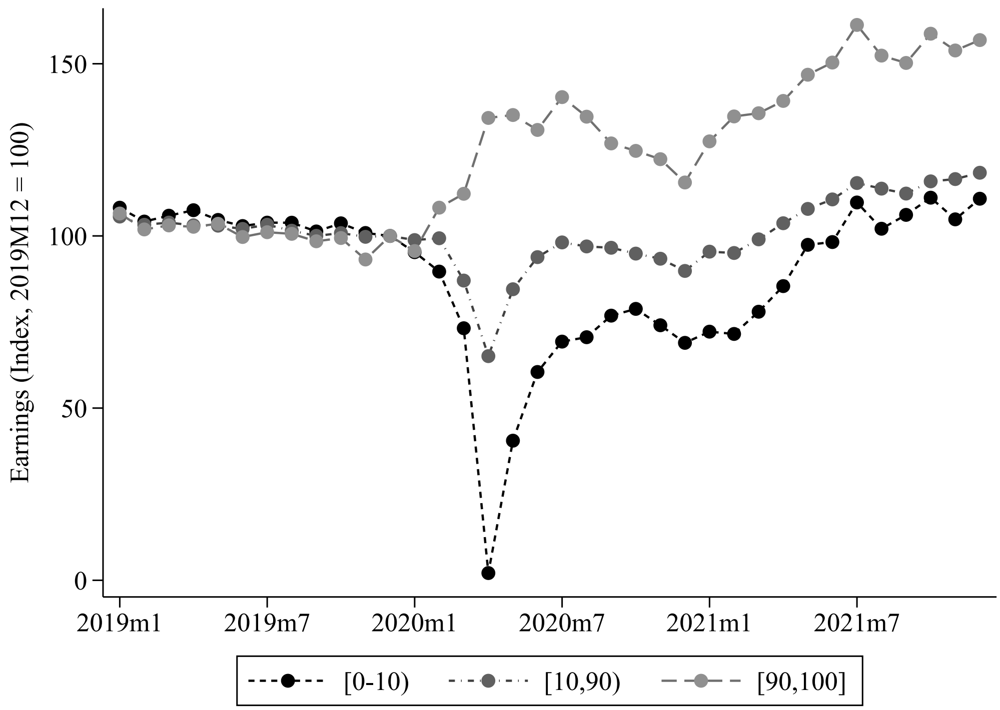
Table A2 shows that the estimated coefficient of the initial shock with this control is very close to the baseline results in Table 2. Using our preferred measures in columns 4-6, a one-standard-deviation increase in labor market strength results in a 5.7 percent increase in hourly wages, compared to 6.4 percent in the baseline. The effect on hours worked is nearly identical, while the resulting impact on total earnings is 16.5 percent, slightly lower than the 18 percent observed 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. Counties with smaller labor market shocks at the onset of COVID-19 saw faster earnings growth afterward. The gains were disproportionately larger for lower-paid, nonmanagerial workers, and those in smaller firms, driven by both higher hourly wages and increased working hours. A divergence in growth is also observed between counties that received more labor market support through the PPP. These trends align with job-ladder models, where labor market competition drives up earnings as workers move upward. Our microdata allows us to test and confirm these predictions by exploring variations across areas and workers.

While our primary focus is on how labor market conditions influence earnings dynamics, our findings also carry significant distributional implications. We observe increasing earnings disparities across geographic regions, while within counties with stronger labor markets, wage inequality among workers decreases. This highlights the evolving patterns of wage and spatial inequality, suggesting asynchronous labor market conditions across regions. These trends offer valuable insights for post-pandemic stabilization policies and present promising directions for future research.

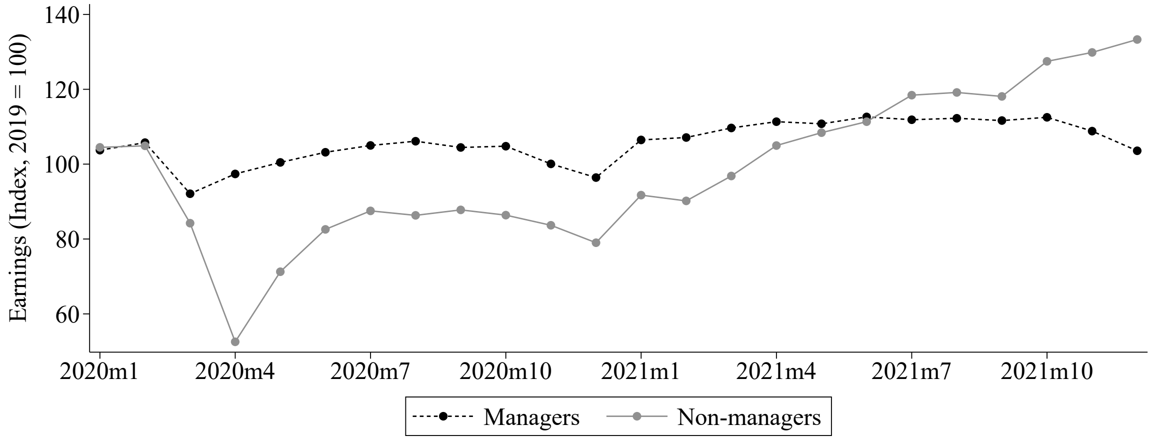
Figure 1: Trends in average earnings by percentile groups



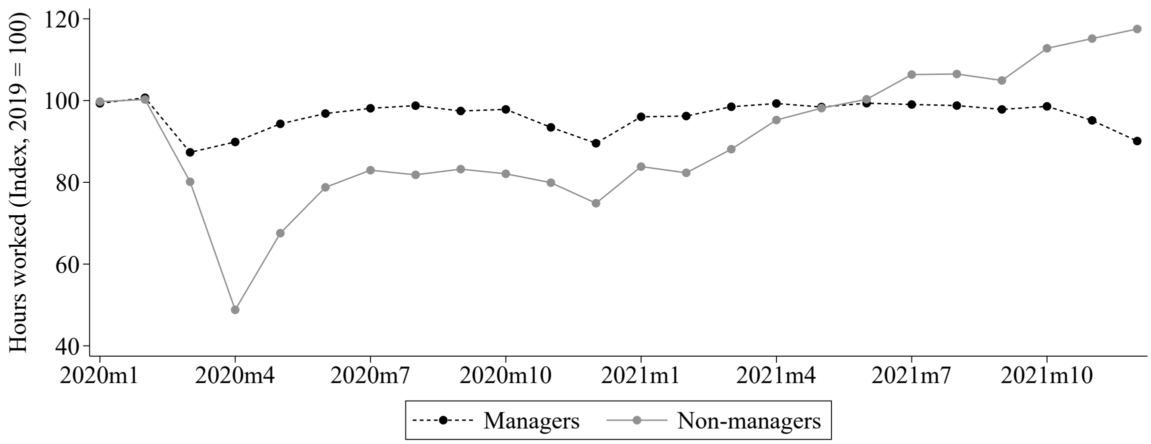
Notes: This figure plots the pre and post COVID-19 trend in average earnings in the Homebase data. The sample is a monthly panel of Homebase workers from January 2019 to December 2021. Workers were sorted into percentile groups based on the county in which they were employed. Counties are ranked by their 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 dynamics by managerial versus nonmanagerial workers: Homebase

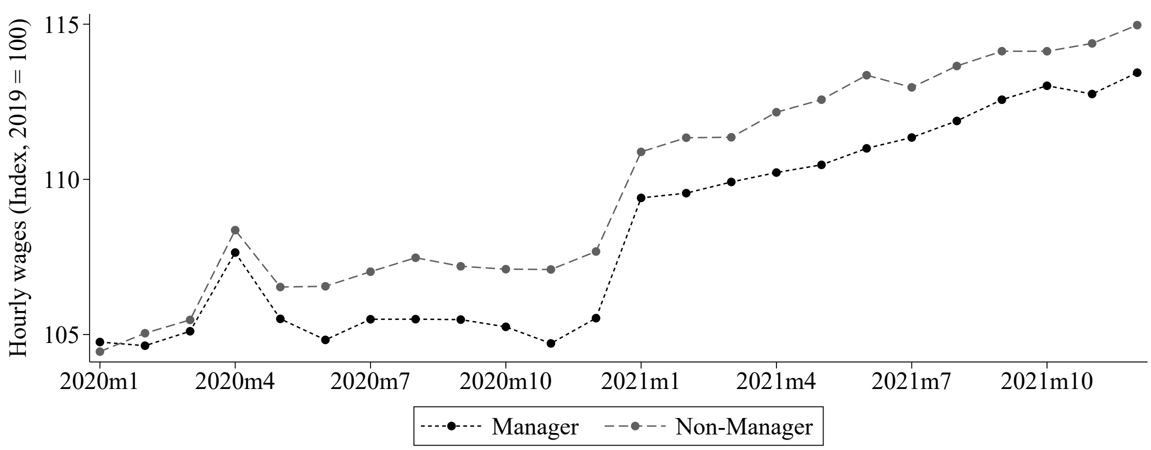
(a) Earnings



(b) Hours worked

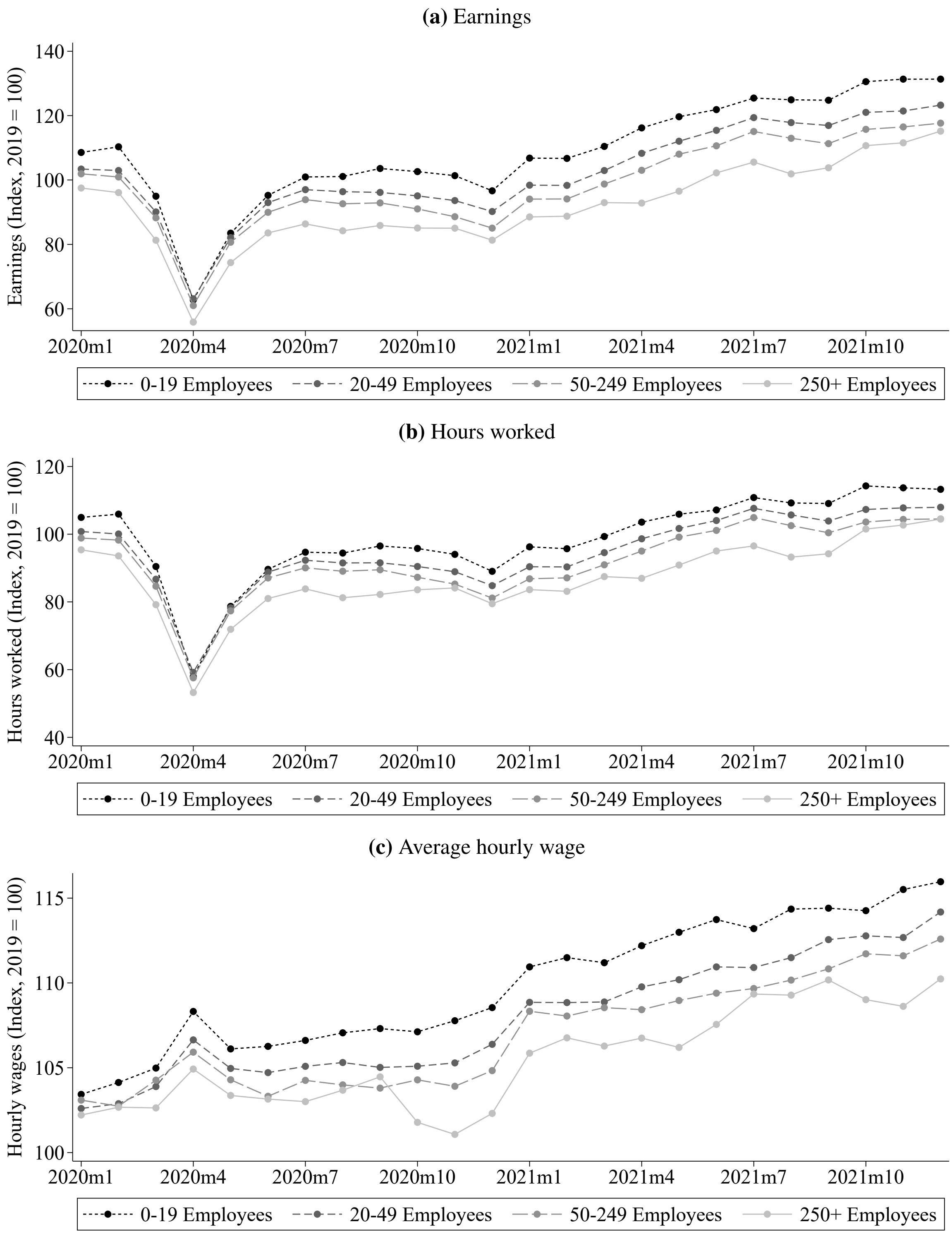


(c) Average hourly wage



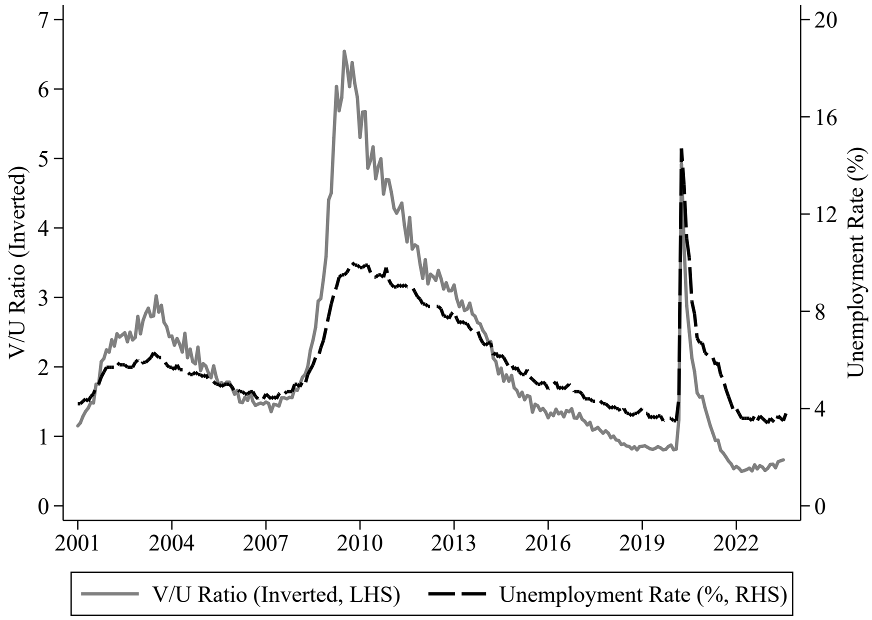
Notes: This figure plots the average hourly wage, hours worked, and earnings per employee in the Homebase data by managerial and nonmanagerial workers. Total earnings is the product of average nominal hourly wage and hours worked. A worker is considered managerial if it is classified as a “manager” or “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 3: Labor market dynamics by firm size: Homebase



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.

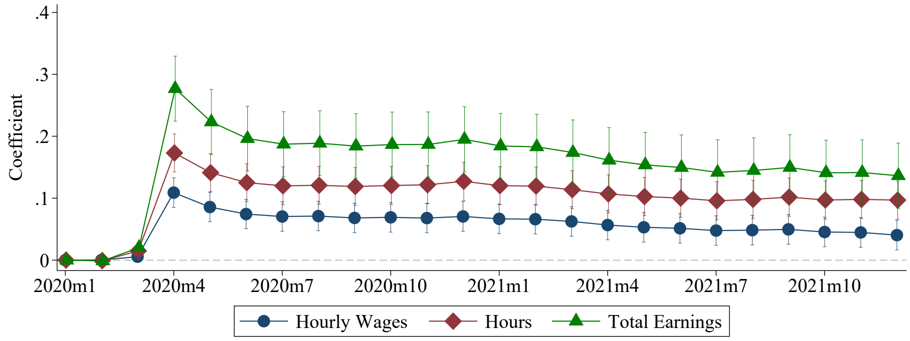
Figure 4: Labor market strength



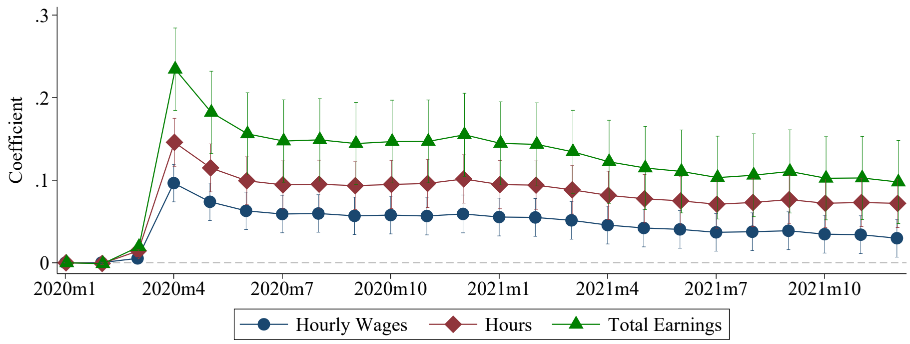
Notes: This figure plots the monthly time series for the U.S. vacancy-to-unemployment ratio and unemployment rate. On April 2020, both measures jumped up sharply to unprecedented levels. Source: Bureau of Labor Statistics.

Figure 5: Time path: baseline results

(a) OLS



(b) Bartik Instrument



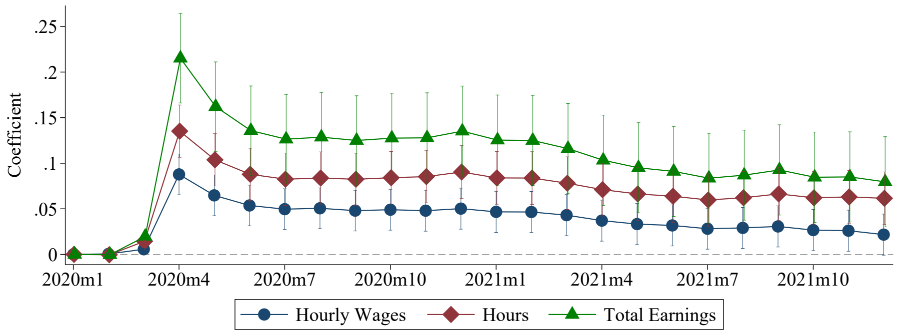
Notes: This figure plots the estimated coefficients $\boldsymbol{\alpha}_1$ from the following regression:

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

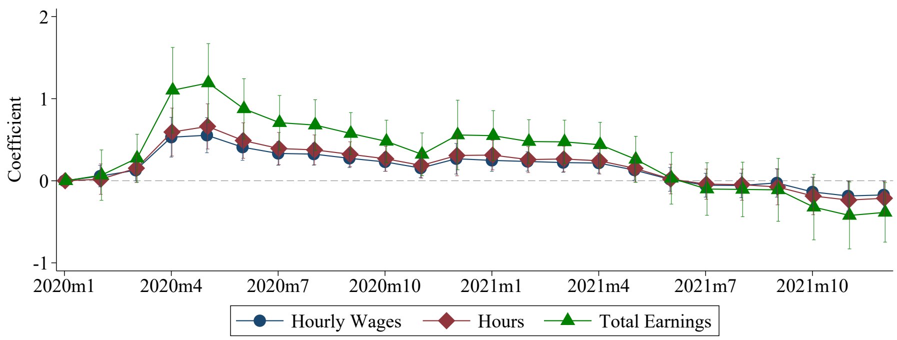
for establishment i, worker j, 4-digit NAICS industry k, county c, and (monthly) time t. The sample is a panel of Homebase workers from January 2020 to December 2021. Y denotes nominal average hourly wage, hours worked, or total earnings. $\Delta Y_{i,j,k,c,t}$ denotes the growth rate in Y from January 2020 to month t measured as log difference. $\boldsymbol{I}_t$ is a vector of (monthly) time dummies from February 2020 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

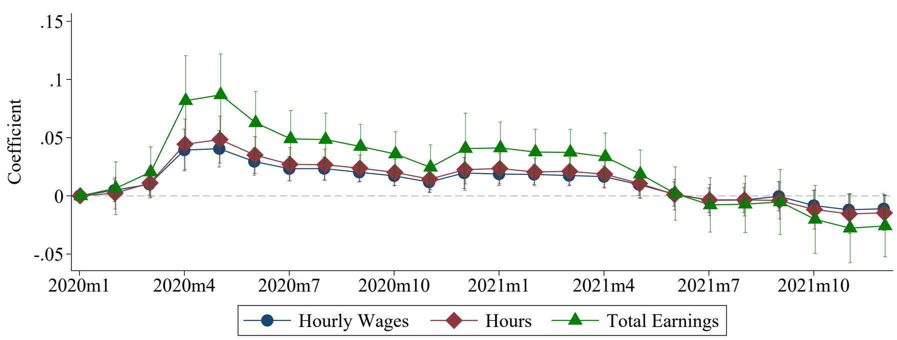
(a) Local labor market shock ($\boldsymbol{\alpha}_1$)



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



(c) Local labor market strength × PPP exposure ($\boldsymbol{\alpha}_3$)

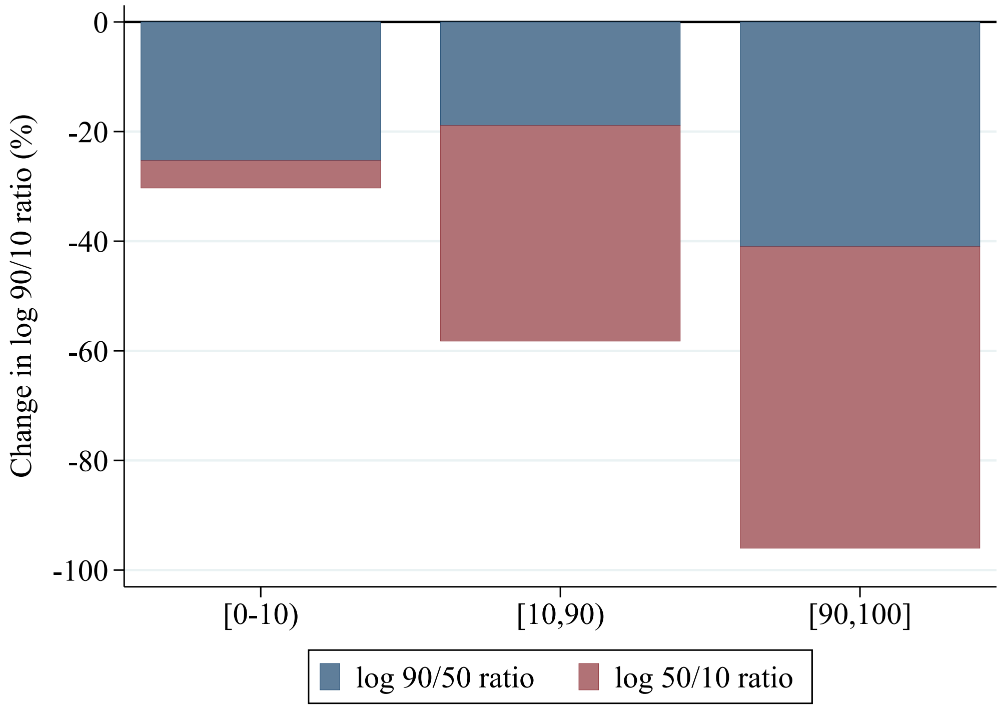


Notes: This figure plots the estimated coefficients from the following regression:

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

$Shock_c$ is the Bartik shock to local labor market strength 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: Pre versus post COVID-19 change in log 90/10 ratio across counties



Notes: This figure plots the pre and post COVID-19 change in the 90/50 (blue), 50/10 (red), and 90/10 log earnings ratios (sum of blue and red). 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 strength 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
Δ*Hourly Wages*$_{i,j,k,c,t}$	3,134,354	0.011	0.710	-2.773	0.000	2.708
Δ*Hours*$_{i,j,k,c,t}$	3,134,354	0.071	0.843	-3.231	0.000	3.321
Δ*Total Earnings*$_{i,j,k,c,t}$	3,134,354	0.074	1.485	-5.766	0.000	5.735
Non-Manager$_j$	3,134,354	0.884	0.320	0.000	1.000	1.000
Job-Switcher$_j$	3,134,354	0.404	0.491	0.000	0.000	1.000
Low-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. $\Delta Y_{i,j,k,c,t}$ denotes the growth rate in Y from January 2020 to month t measured as log difference. 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-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-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 strength defined in equation (3). $Shock_{c,t}$ is the contemporaneous shock to labor market strength 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 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
$Post_t \times Shock_c$	0.1142***	0.1743***	0.2830***	0.0643***	0.1187***	0.1806***
	(0.0173)	(0.0244)	(0.0402)	(0.0161)	(0.0225)	(0.0370)
Observations	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354
R^2	0.49	0.48	0.49	0.49	0.48	0.49
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. $\Delta Y_{i,j,k,c,t}$ denotes the growth rate in Y from January 2020 to month t measured as log difference. $Post_t$ 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 strength. 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
$Post_t \times Shock_c$	0.1036***	0.1584***	0.2556***	0.0638***	0.1167***	0.1771***
	(0.0151)	(0.0208)	(0.0344)	(0.0153)	(0.0212)	(0.0350)
$Post_t \times PPPE_c$	0.1128***	0.1581***	0.2708***	0.1345***	0.1904***	0.3208***
	(0.0406)	(0.0582)	(0.0949)	(0.0431)	(0.0614)	(0.1001)
$Post_t \times PPPE_c \times Shock_c$	0.0083***	0.0116***	0.0198***	0.0102***	0.0142***	0.0240***
	(0.0031)	(0.0044)	(0.0072)	(0.0032)	(0.0045)	(0.0074)
Observations	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354
R^2	0.49	0.48	0.49	0.49	0.48	0.49
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. $\Delta Y_{i,j,k,c,t}$ denotes the growth rate in Y from January 2020 to month t measured as log difference. $Post_t$ 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 strength. $PPPE_c$ denotes the exposure to PPP lending defined in 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: Managerial versus nonmanagerial workers

	(1) Hourly Wages	(2) Hours	(3) Total Earnings	(4) Hourly Wages	(5) Hours	(6) Total Earnings
$Post_t$ × *Non-Manager*$_j$	0.0690***	0.0677***	0.1319***	0.0749***	0.0729***	0.1428***
	(0.0014)	(0.0016)	(0.0028)	(0.0014)	(0.0016)	(0.0028)
$Post_t \times Shock_c$	0.0627***	0.0843***	0.1408***	0.0347***	0.0593***	0.0917***
	(0.0097)	(0.0114)	(0.0193)	(0.0102)	(0.0120)	(0.0202)
$Post_t \times Shock_c$ × *Non-Manager*$_j$	0.0395***	0.0346***	0.0725***	0.0370***	0.0294***	0.0650***
	(0.0005)	(0.0006)	(0.0010)	(0.0006)	(0.0006)	(0.0010)
Observations	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354
R^2	0.67	0.77	0.74	0.67	0.77	0.74
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. $\Delta Y_{i,j,k,c,t}$ denotes the growth rate in Y from January 2020 to month t measured as log difference. $Post_t$ 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 strength. 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: 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*$_j$	0.0742***	0.0888***	0.1560***	0.0813***	0.0892***	0.1619***
	(0.0036)	(0.0044)	(0.0075)	(0.0042)	(0.0050)	(0.0087)
$Post_t \times Shock_c$	0.1273***	0.1872***	0.3046***	0.0715***	0.1269***	0.1952***
	(0.0121)	(0.0171)	(0.0278)	(0.0120)	(0.0172)	(0.0276)
$Post_t \times Shock_c \times$ *Low-Wage*$_j$	0.0565***	0.0693***	0.1195***	0.0610***	0.0673***	0.1208***
	(0.0033)	(0.0041)	(0.0069)	(0.0037)	(0.0045)	(0.0077)
Observations	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354
R^2	0.61	0.62	0.62	0.61	0.62	0.62
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. $\Delta Y_{i,j,k,c,t}$ denotes the growth rate in Y from January 2020 to month t measured as log difference. $Post_t$ 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 strength. 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: Job-switchers versus job-stayers

	(1) Hourly Wages	(2) Hours	(3) Total Earnings	(4) Hourly Wages	(5) Hours	(6) Total Earnings
$Post_t$ × *Job-Switcher*$_j$	0.0453***	0.0470***	0.0897***	0.0754***	0.0838***	0.1533***
	(0.0112)	(0.0146)	(0.0250)	(0.0125)	(0.0162)	(0.0279)
$Post_t \times Shock_c$	0.0699***	0.1171***	0.1833***	0.0554***	0.1097***	0.1634***
	(0.0148)	(0.0203)	(0.0336)	(0.0152)	(0.0211)	(0.0348)
$Post_t \times Shock_c$ × *Job-Switcher*$_j$	0.0024***	0.0028**	0.0050***	0.0037***	0.0044***	0.0078***
	(0.0008)	(0.0011)	(0.0019)	(0.0009)	(0.0012)	(0.0020)
Observations	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354
R^2	0.50	0.49	0.50	0.50	0.49	0.49
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:

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

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. *Job-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. Y denotes nominal average hourly wage, hours worked, or total earnings. $\Delta Y_{i,j,k,c,t}$ denotes the growth rate in Y from January 2020 to month t measured as log difference. $Post_t$ 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 strength. 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: By firm size

	(1) Hourly Wages	(2) Hours	(3) Total Earnings	(4) Hourly Wages	(5) Hours	(6) Total Earnings
$Post_t \times Small_i$	0.1818***	0.1772***	0.3488***	0.2083***	0.2037***	0.4011***
	(0.0085)	(0.0099)	(0.0176)	(0.0085)	(0.0099)	(0.0176)
$Post_t \times Shock_c$	0.0538***	0.0957***	0.1456***	0.0337**	0.0815***	0.1146***
	(0.0134)	(0.0181)	(0.0300)	(0.0139)	(0.0191)	(0.0314)
$Post_t \times Shock_c \times Small_i$	0.0469***	0.0475***	0.0931***	0.0433***	0.0410***	0.0830***
	(0.0007)	(0.0009)	(0.0014)	(0.0007)	(0.0009)	(0.0015)
Observations	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354
R^2	0.71	0.70	0.71	0.71	0.70	0.71
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

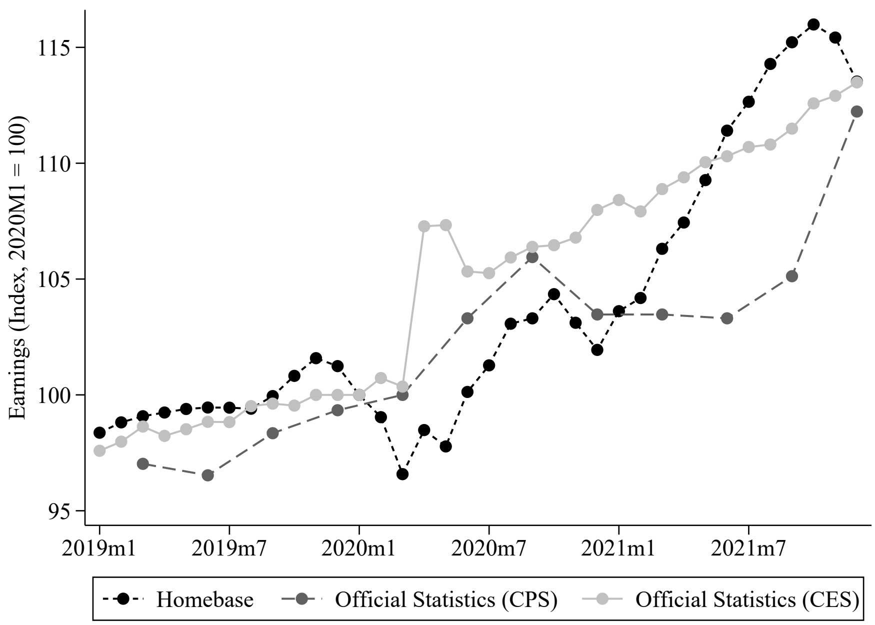
Notes: This table reports the estimated coefficients from the following regression:

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

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. $\Delta Y_{i,j,k,c,t}$ denotes the growth rate in Y from January 2020 to month t measured as log difference. $Post_t$ 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 strength. 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.

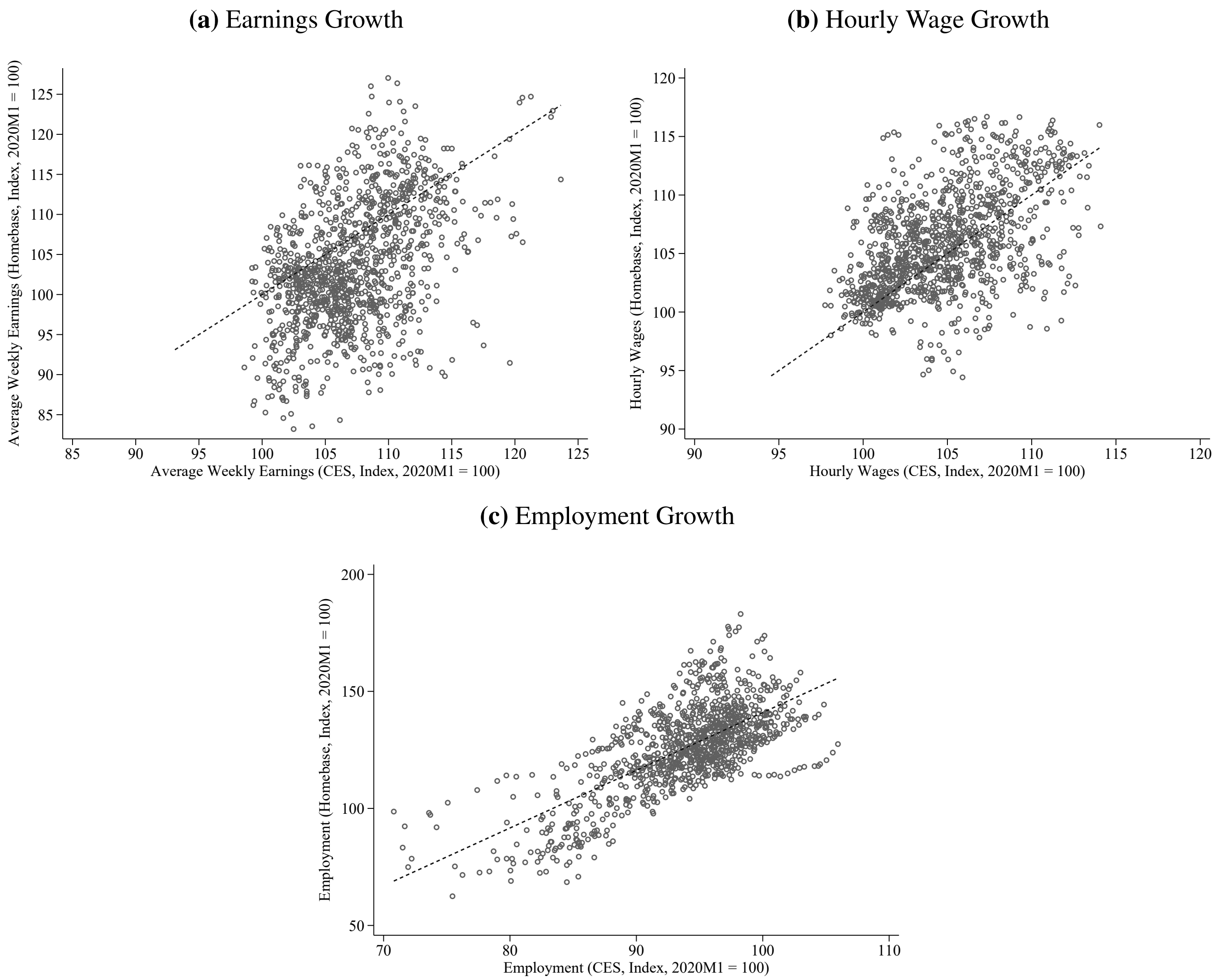
A Appendix

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



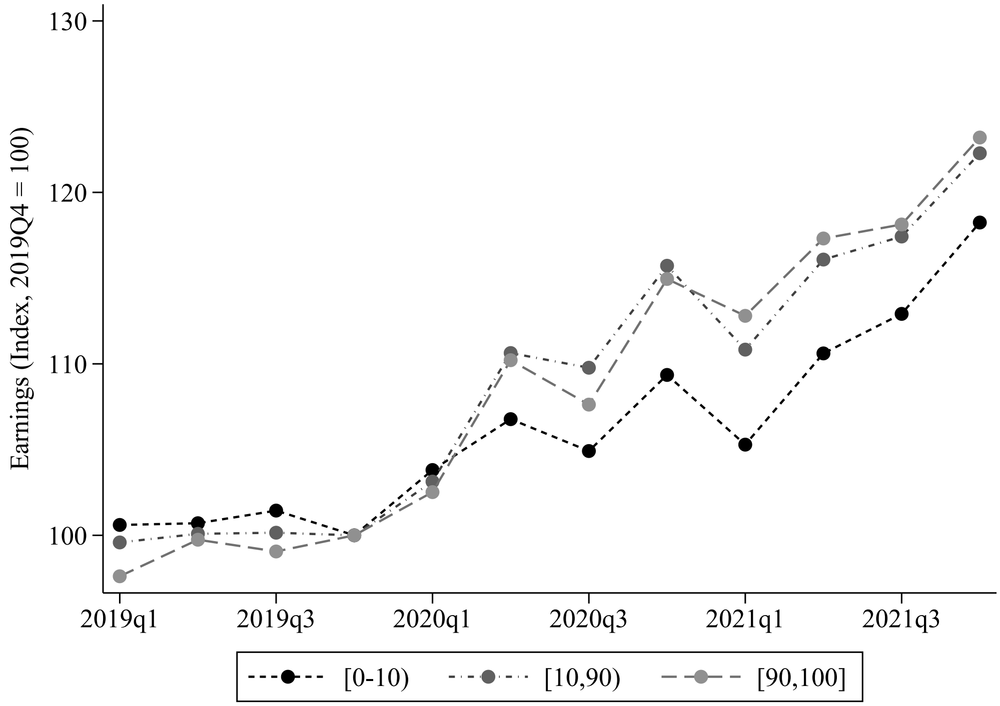
Notes: This figure compares the pre and post COVID-19 trend in average earnings in the Homebase data against the trends in official statistics. We consider two sources of official statistics, both from the BLS. The Current Population Survey (CPS) line plots the growth in median usual weekly nominal earnings 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



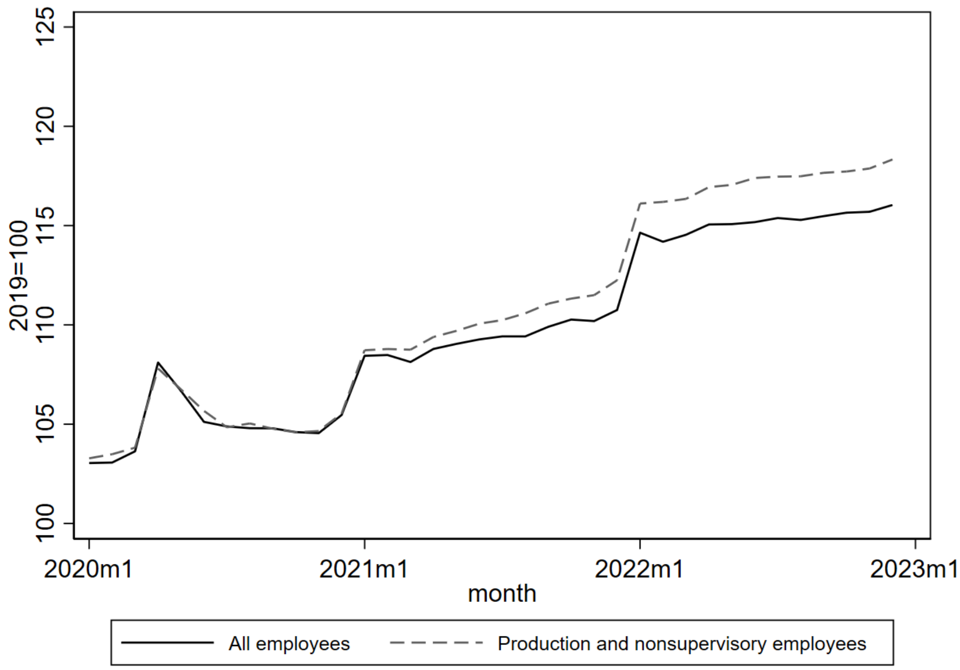
Notes: This figure compares growth in earnings, hourly wages, and employment relative to January 2020 between the Homebase data and official statistics (CES), by month and state. The Current Employment Statistics (CES) data uses state-level average weekly earnings for earnings (SMU[2-digit state FIPS 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)



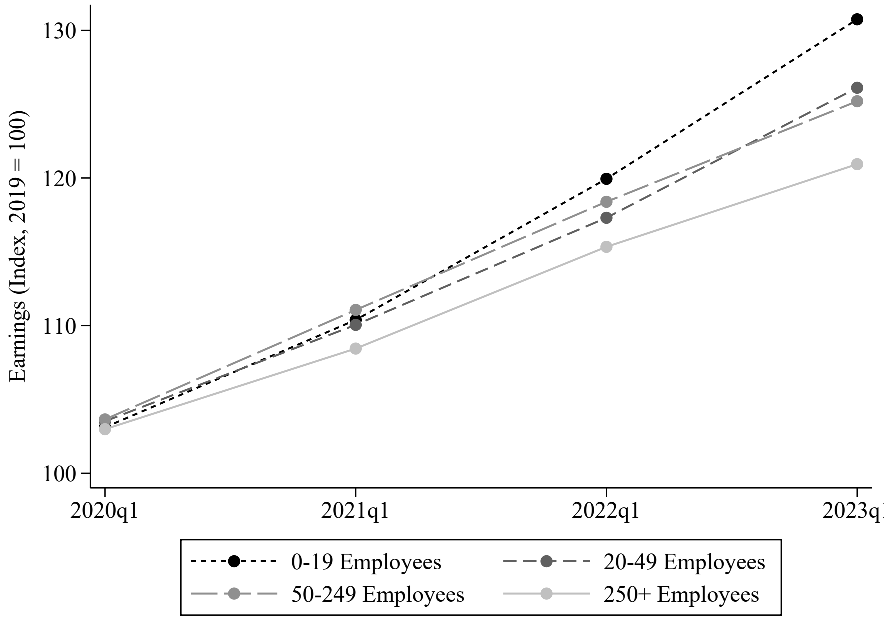
Notes: This figure plots the pre and post COVID-19 trend in average weekly wages in the official statistics from the Quarterly Census of Employment and Wages (QCEW). The sample is a quarterly panel of counties covering the service-providing sector workers from Q1 2019 to Q4 2021. Counties are ranked 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



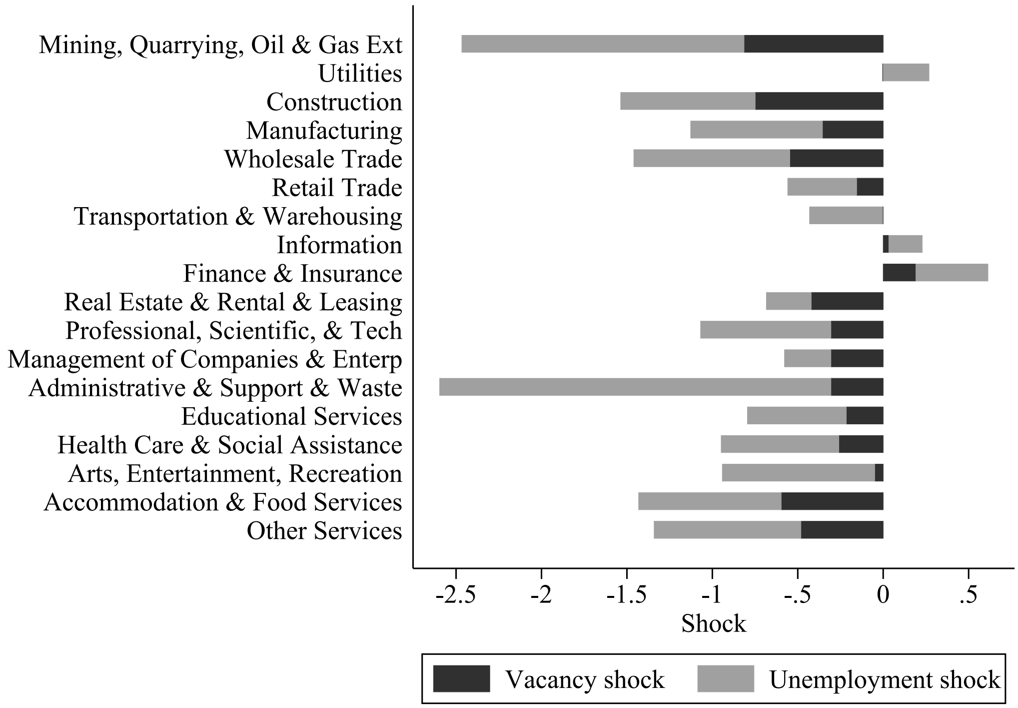
Notes: This figure plots the average hourly wage in the leisure and hospitality sector. The blue line is for all employees and the red line is for production and nonsupervisory employees. Wages are seasonally adjusted. The level in the same month in 2019 is indexed to 100. Source: Bureau of Labor Statistics.

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.

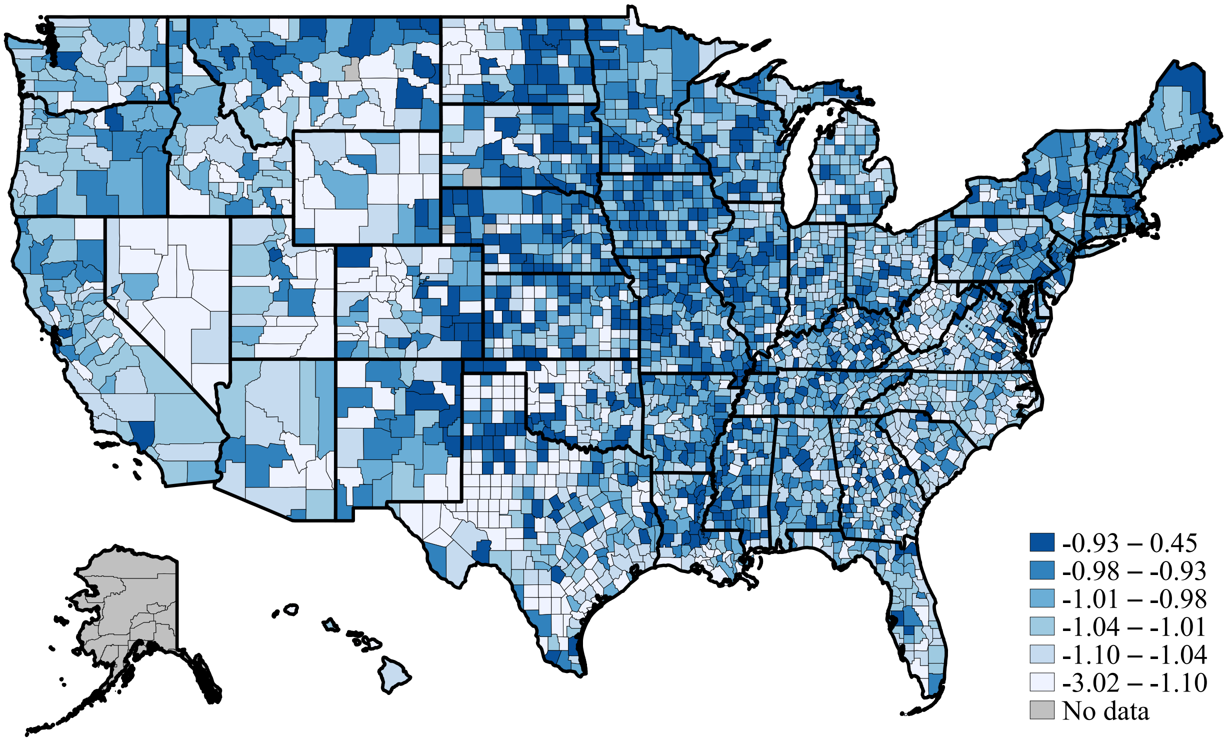
Figure A6: Industry-level shocks



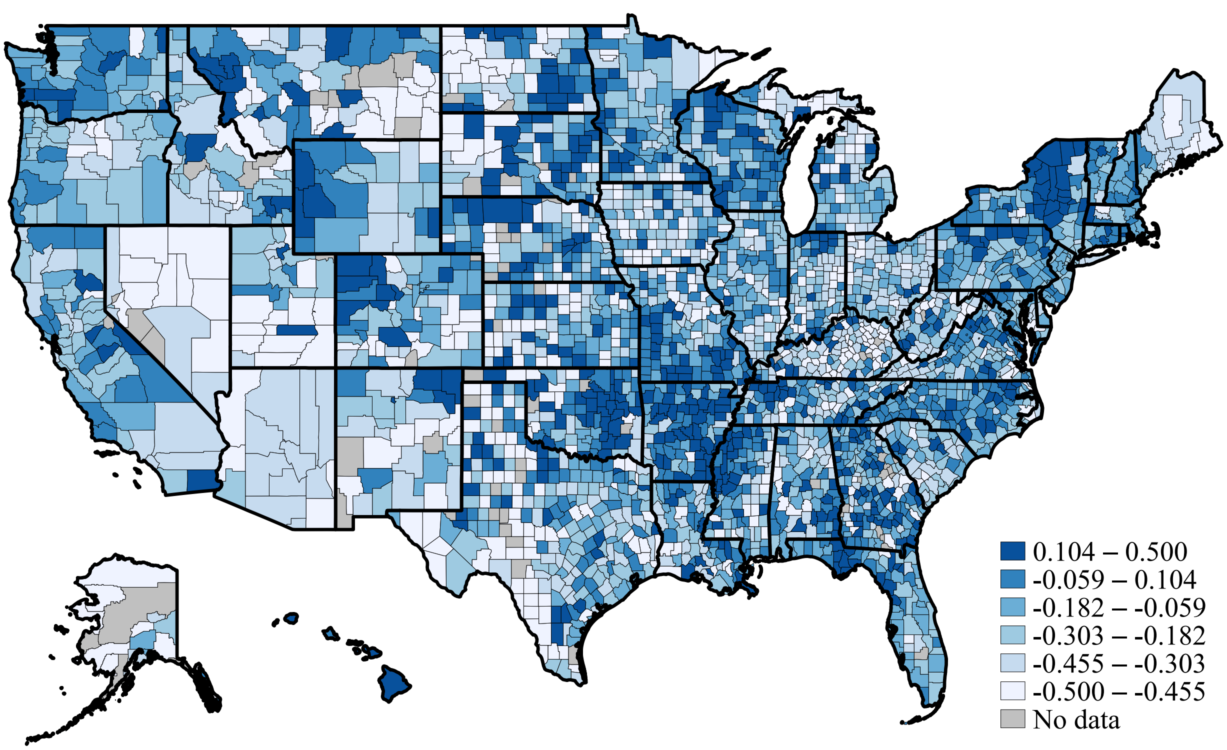
Notes: This figure plots the distribution of the initial labor market shock, $Shock_{c,k} = (\Delta \ln(V)_{k,\text{April 2020}} - \Delta \ln(V)_{k,\text{April 2019}}) - (\Delta \ln(U)_{k,\text{April 2020}} - \Delta \ln(U)_{k,\text{April 2019}})$, across 2-digit NAICS industries k. The initial labor 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 strength ($Shock_c$)

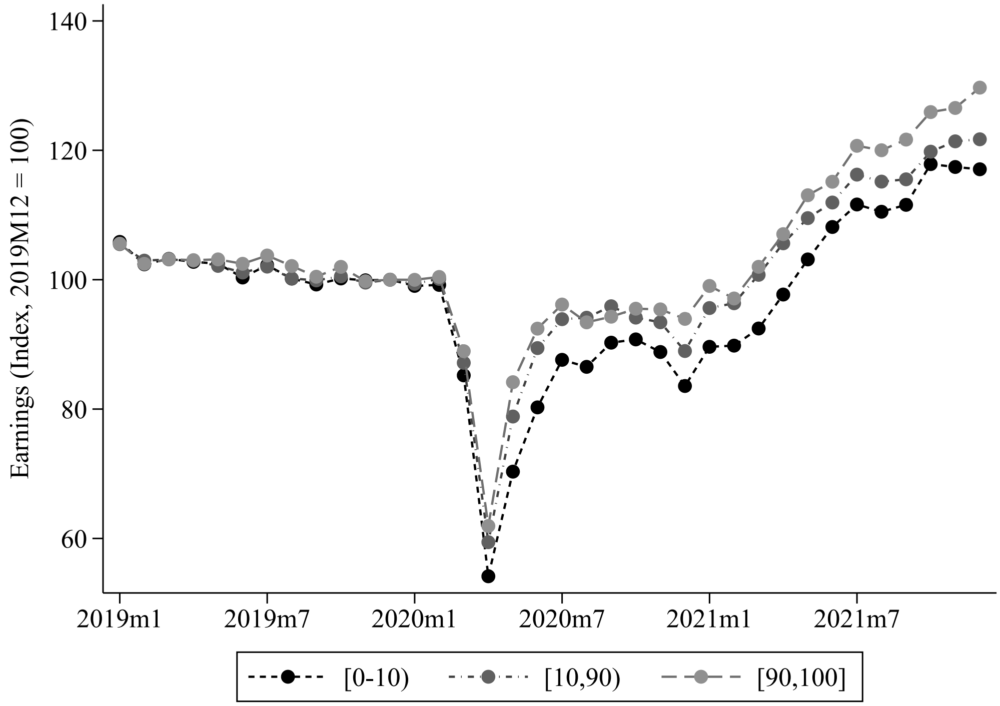


(b) PPP exposure ($PPPE_c$)



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.

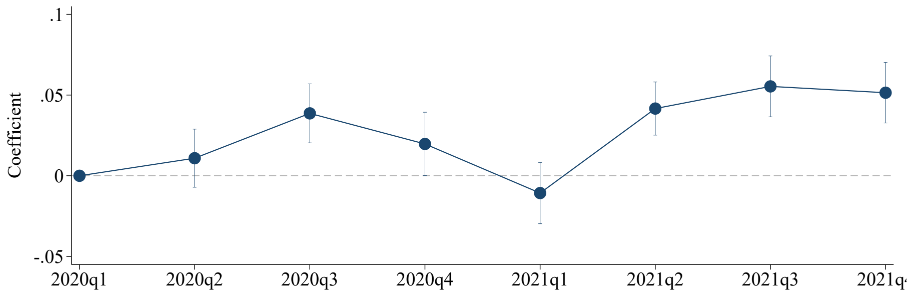
Figure A8: Trends in average earnings by shocks to labor market strength



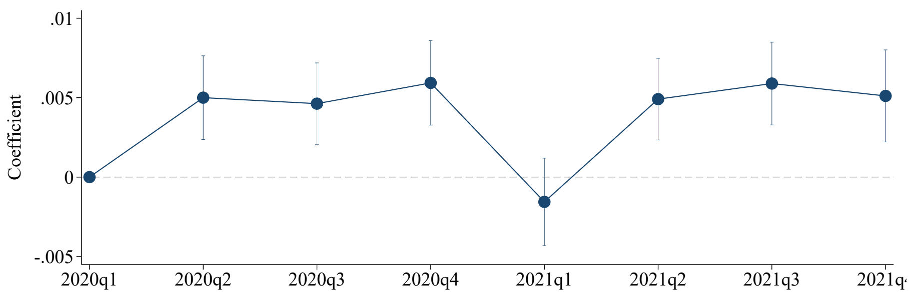
Notes: This figure plots the pre and post COVID-19 trend in average earnings in the Homebase data. The sample is a monthly panel of Homebase workers from January 2019 to December 2021. Workers were sorted into percentile groups based on the county in which they were employed. Counties are ranked by the Bartik shock to local labor market strength 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

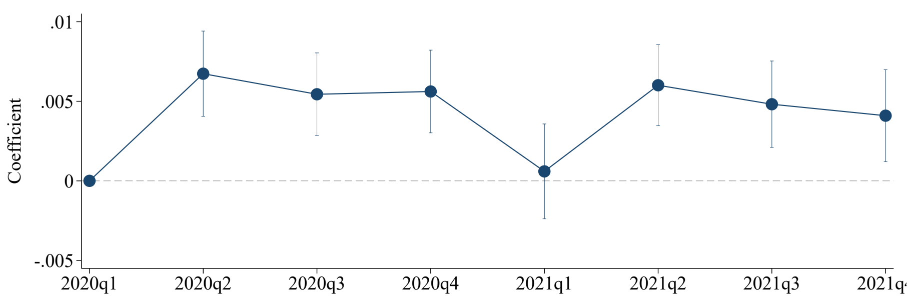
(a) Local labor market shock ($\boldsymbol{\alpha}_1$)



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



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



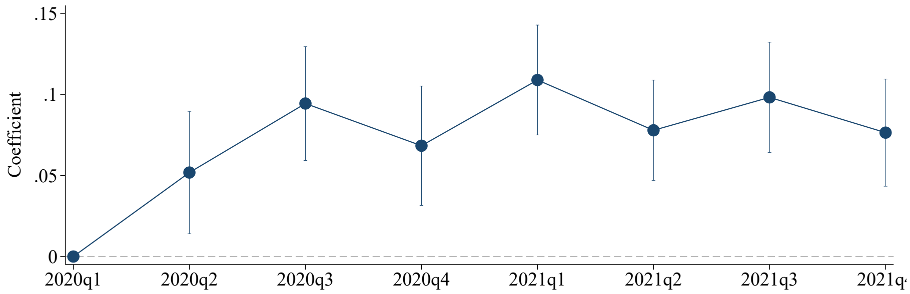
Notes: This figure plots the estimated coefficients from the following regression:

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

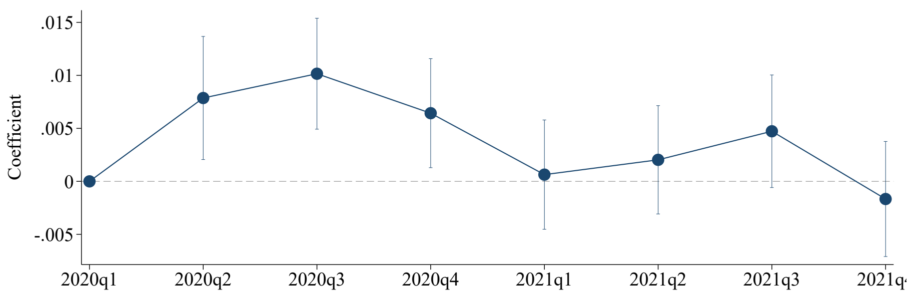
for 2-digit NAICS industry k, county c, and time t. The sample is a quarterly panel of counties by 2-digit NAICS industries over Q1 2020 to Q4 2021 from the QCEW. Y denotes earnings, measured using nominal average weekly wages. $\Delta Y_{k,c,t}$ is the growth rate in Y from Q1 2020 to t measured as log difference. $\boldsymbol{I}_t$ 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

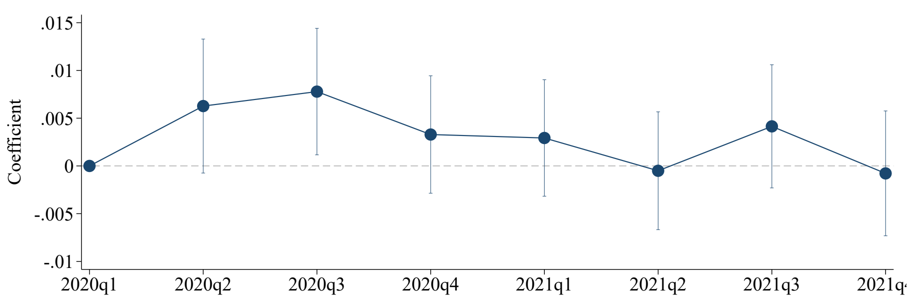
(a) Local labor market strength ($\boldsymbol{\alpha}_1$)



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



(c) Local labor market strength $\times$ 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 the growth rate in Y from Q1 2020 to t measured as log difference. $\boldsymbol{I}_t$ 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 strength

	(1) Hourly Wages	(2) Hours	(3) Total Earnings	(4) Hourly Wages	(5) Hours	(6) Total Earnings
$Post_t \times Shock_c$	0.1213***	0.1460***	0.2631***	-0.0422***	-0.0468**	-0.0834***
	(0.0233)	(0.0309)	(0.0523)	(0.0128)	(0.0185)	(0.0300)
Observations	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354
R^2	0.49	0.48	0.49	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. $\Delta Y_{i,j,k,c,t}$ denotes the growth rate in Y from January 2020 to month t measured as log difference. $Post_t$ 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
$Post_t \times Shock_c$	0.1067***	0.1671***	0.2687***	0.0565***	0.1107***	0.1652***
	(0.0163)	(0.0217)	(0.0364)	(0.0153)	(0.0204)	(0.0340)
Observations	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354	3,134,354
R^2	0.51	0.50	0.50	0.51	0.49	0.50
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. $\Delta Y_{i,j,k,c,t}$ denotes the growth rate in Y from January 2020 to month t measured as log difference. $Post_t$ 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 strength 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.

References

Autor, David, Arindrajit Dube, and Annie McGrew, "The Unexpected Compression: Competition at Work in the Low Wage Labor Market," NBER Working Papers 31010, NBER 2023.

Ball, Laurence M., Daniel Leigh, and Prachi Mishra, "Understanding U.S. Inflation During the COVID Era," NBER Working Papers 30613, NBER 2022.

Barrett, Philip, Sophia Chen, Li Lin, and Anke Weber, "The Dynamic Effects of Local Labor Market Shocks on Small Firms in The United States," IMF Working Papers 2024/063, International Monetary Fund 2024.

Bartik, Timothy J., *Who Benefits from State and Local Economic Development Policies?* number wbsle. In 'Books from Upjohn Press.', W.E. Upjohn Institute for Employment Research, November 1991.

Blanchard, Olivier, Alex Domash, and Lawrence Summers, "Bad News for the Fed from the Beveridge Space," *Peterson Institute for International Economics Policy Brief*, 2022.

Blanchard, Olivier Jean and Peter Diamond, "The Beveridge Curve," *Brookings Papers on Economic Activity*, 1989, *1989* (1), 1–76.

Borusyak, Kirill, Peter Hull, and Xavier Jaravel, "Quasi-Experimental Shift-Share Research Designs: Sampling-based vs. Design-based Uncertainty in Regression Analysis," *The Review of Economic Studies*, 2022, *89* (1), 181–213.

Chen, Sophia, Deniz Igan, Do Lee, and Prachi Mishra, "Inflation and Labor Markets: A Bottom-Up View," IMF Working Papers, International Monetary Fund 2024.

Crump, Richard K, Stefano Eusepi, Marc Giannoni, and Ayşegül Şahin, "The Unemployment-Inflation Trade-off Revisited: The Phillips Curve in COVID Times," NBER Working Papers 29785, NBER 2022.

Domash, Alex and Lawrence H Summers, "How Tight are U.S. Labor Markets," NBER Working Papers 29739, NBER 2022.

Furman, Jason and Wilson III Powell, "What is the Best Measure of Labor Market Tightness?," Peterson Institute for International Economics blog post November 22, 2021, Peterson Institute for International Economics 2021.

Granja, João, Christos Makridis, Constantine Yannelis, and Eric Zwick, "Did the Paycheck Protection Program Hit the Target?," *Journal of Financial Economics*, 2022, *145* (3), 725–761.

Haltiwanger, John C., Henry R. Hyatt, Lisa B. Kahn, and Erika McEntarfer, "Cyclical Job Ladders by Firm Size and Firm Wage," *American Economic Journal: Macroeconomics*, April 2018, *10* (2), 52–85.

Hazell, Jonathon, Juan Herreño, Emi Nakamura, and Jón Steinsson, "The Slope of the Phillips Curve: Evidence from U.S. States," *The Quarterly Journal of Economics*, 2022, *123* (3), 1299–1344.

Hershbein, Brad and Lisa B. Kahn, "Do Recessions Accelerate Routine-Biased Technological Change? Evidence from Vacancy Postings," *American Economic Review*, July 2018, *108* (7), 1737–72.

Moscarini, G. and F. Postel-Vinay, "The Relative Power of Employment-to-Employment Reallocation and Unemployment Exits in Predicting Wage Growth," *American Economic Review Papers and Proceedings*, 2017, *107* (5), 364–68.

Pissarides, Christopher A, *Equilibrium Unemployment Theory*, MIT press, 2000.

Soh, Jiaming, Myrto Oikonomou, Carlo Pizzinelli, Ippei Shibata, and Marina Mendes Tavares, "Did the COVID-19 Recession Increase the Demand for Digital Occupations in the United States? Evidence from Employment and Vacancies Data," IMF Working Papers 2022/195, International Monetary Fund 2022.

Stewart, Jay, "Why was Labor Productivity Growth So High during the COVID-19 Pandemic? The Role of Labor Composition," BLS Working Papers 545, U.S. Bureau of Labor Statistics 2022.

PUBLICATIONS

Divergence in Post-Pandemic Earnings Growth: Evidence from Micro Data
Working Paper No. WP/2024/222