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Unemployment insurance (UI) acts both as a disincentive for labor supply and as a demand stimulus which may explain why empirical studies often find limited effects of UI on employment. This paper provides independent estimates of the disincentive effects arising from the largest expansion of UI in U.S. history, the pandemic unemployment benefits. Using high-frequency data on small restaurants and retailers from Homebase, we control for demand effects by comparing neighboring businesses that largely share the positive impact of UI stimulus. We find that employment in low-wage businesses recovered more slowly than employment in neighboring high-wage businesses in labor markets with larger differences in the relative generosity of pandemic UI benefits. According to a labor search model that replicates the estimated employment differences between low- and high-wage businesses, the disincentive effects from the pandemic UI programs held back the aggregate employment recovery by 3.4 percentage points between April and December 2020.

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1 Introduction

Unemployment insurance (UI) not only acts as a disincentive for workers to supply labor. It also stimulates consumption, and therefore labor demand, by raising the disposable income of the unemployed. These two opposing effects are hard to distinguish empirically. Yet, having independent estimates of the disincentive effects of UI is of critical importance. It informs policymakers about the full implications of UI provisions and allows economists to properly discipline and evaluate quantitative labor search models often used for policy analysis.

In this paper, we develop an estimation framework that isolates the disincentive effects of the largest expansion of UI programs in U.S. history, the pandemic unemployment benefits. We then use a calibrated search and matching model to quantify the disincentive effects of each of the various provisions of the pandemic UI programs on labor market recovery.

Beginning with the March 2020 CARES Act the U.S. federal government relaxed UI eligibility criteria, extended maximum UI duration, and supplemented state benefits to such a degree that for many recipients UI payments considerably exceeded what they had earned in their previous job (Ganong, Noel, and Vavra, 2020). Yet, the overall message from existing studies (reviewed below) is that pandemic UI benefits had only modest negative effects on job-finding rates and employment. What these studies do not clarify, however, is if these limited estimated effects represent small disincentive effects or a small net effect from opposing labor supply and demand channels.

The small net effect on employment may arise for the following reasons. Average wages paid differ widely across local labor markets. With a uniform UI income supplement, labor markets with lower average wages have higher average replacement rates. Thus, businesses in low-wage markets find it relatively more difficult to attract workers, and employment in these markets recovers more slowly than in high-wage markets. But at the same time, the unemployed in these low-wage labor markets experience a relatively larger increase in their purchasing power, thereby increasing labor demand and helping the employment recovery. Thus, a standard difference-in-difference estimator may show that in labor markets with higher replacement rates, businesses recover faster and not slower.

Our research design controls for confounding demand effects in two ways. First, we difference out common local demand shifts arising from the UI stimulus by comparing the employment recovery of low-wage and high-wage businesses within the same labor market, industry, and price range.[1] Second, we account for idiosyncratic demand shifts between neighboring low- and high-wage businesses by including controls for customer traffic and spending for each

[1] Such local differencing not only reduces the bias arising from the demand stimulus of UI but also from any factor that is correlated with demand shifts and replacement rates as long as the factor is common within labor markets.

individual business.

We apply the proposed research design to data from Homebase, a scheduling and payroll administration provider used by thousands of small businesses in the U.S. The data is representative of small, in-person service sector businesses, mostly restaurants and retail businesses (Kurmann, Lalé, and Ta, 2021).[2] The data provides a panel of businesses with daily data on employment, the hourly wage, hours worked by each employee, business zip code, and industry affiliation. We match this data to Yelp to obtain a price range and to Safegraph to obtain weekly measures of customer traffic and customer expenditures for each individual business. The link between employment, wages, price range, and customer demand at the individual business level and at a high frequency is a unique feature of our analysis.

We study the employment recovery in low- versus high-wage businesses before and after the discontinuation of the CARES Act $600 income supplement.[3] Due to the expiration, some labor markets experience a larger decline in the UI replacement rate gap between low- and high-wage businesses than others. We find that in labor markets with a larger decline in the UI replacement gap, employment in low-wage businesses recovers faster than employment in their high-wage neighbors, thereby closing the employment recovery gap. For this result, it is crucial to define local labor markets narrowly. When we compare businesses at broader levels of geographical aggregation (e.g., within state borders) the estimated effect of replacement rates on the employment recovery gap disappears.

The basic identification assumption is that low- and high-wage businesses in the same labor market, industry, and price range are not differentially exposed to demand and productivity shifts. Based on our comprehensive and linked data we collect several pieces of evidence that support this assumption. First, we confirm that low- and high-wage businesses in the same labor market, industry, and price range are not differentially impacted by local industry demand shifts (such as the number of customers and spending). Second, we show that the inclusion of a variety of controls such as health indicators, stay-at-home regulations, school closures, and differences in measures of business quality and customer demand do not substantially alter our estimated effects. Finally, we show that even though employment of low-wage businesses lagged behind employment of their high-wage neighbors during the period of the $600 supplement, hours per employee and hourly wages of low-wage businesses were growing faster. This correlation is suggestive of differential labor supply shocks not differential demand or

[2]As we document in Appendix A, employment in Homebase businesses tracks administrative data very closely. In addition, we show that average hourly wages and average employee hours are broadly comparable to estimates from the Current Population Survey.

[3]Whereas the introduction of the income supplement in March 2020 coincides with other widespread disruptions in the labor market from pandemic-related closures, the removal of the supplement occurs in August 2020 when the economy is relatively close to its normal capacity and most health-related regulations have been lifted.

productivity shocks.

To quantify the overall impact of the CARES Act on employment recovery we build a labor search model. As long as the employment decisions of high-wage businesses are affected by the pandemic UI benefits, our estimate of the employment recovery gap impact of UI alone does not represent the total impact of UI on employment. With the model, we can construct a proper counterfactual for the overall employment recovery absent pandemic UI benefits. In addition, we use the model to separately quantify the impact of each provision included in the CARES Act: the $600 weekly supplement, the extension of maximum duration of benefits, and the expanded eligibility of beneficiaries.

We should note that our quantitative model is focused directly on the disincentive effects of UI and does not capture the stimulative effects of UI nor other pandemic-related confounding factors such as re-opening restrictions. This is consistent with our empirical design that estimates disincentive effects net of demand effects and other common labor market factors. This also allows us to work within the standard class of search and matching models without imposing additional structure.

In our search model, firms post idiosyncratic and constant wage offers. Unemployed workers randomly search for jobs from the wage offer distribution and accept a job if the offered wage is higher than their reservation wage (McCall, 1970; Albrecht and Axell, 1984). Since UI benefits depend on a worker's last wage, the reservation wage distribution is an equilibrium object (as in Ljungqvist and Sargent, 1998, 2008).

In the model, not all unemployed receive UI, as benefits are restricted based on eligibility and maximum duration. Unemployed who are part of the non-UI state are more keen to accept job offers even from the lower end of the wage distribution. Hence, we calibrate the flow value of the non-UI state such that, conditional on observed UI recipiency and UI replacement rates, the model generates a realistic equilibrium residual wage dispersion. Generating a realistic residual wage dispersion is challenging for standard labor search models (Hornstein, Krusell, and Violante, 2011). In our model, the presence of the non-UI state substantially increases the residual wage dispersion.

We expose the model to a large separation shock, consistent with the evidence of the decline in employment at the onset of the pandemic followed by a large expansion of UI benefits consistent with the provisions of the CARES Act: income supplement, extension of benefit duration, and expanded eligibility. We solve for the equilibrium transitional dynamics of employment in low- and high-wage firms as they return to their steady state.

We find that the baseline model vastly overstates the estimated impact of the expiration of the $600 income supplement on the employment recovery of low- versus high-wage firms. The exaggerated impact arises in the model not only due to the magnitude of the income supplement but also due to the expansion of the UI eligibility criteria. Using data from the Current Population Survey (CPS) we document that for the in-person leisure and hospitality businesses we study, the share of unemployed workers who received benefits during 2020 nearly tripled compared to the pre-pandemic period. As a result, in the model, the CARES Act drains the pool of non-UI unemployed which is the main source of employees for the low-wage firms, so that employment in these firms declines dramatically during the period of $600 income supplement and recovers at an equally fast pace after the expiration.

We reconcile the model with the data by introducing the possibility that workers lose their UI eligibility if they refuse a job offer. This assumption, which is motivated by UI law, effectively reduces the outside option for workers and increases the acceptance rates for low-wage firms. With a probability of losing benefits upon job refusal around 16 percent, we can match the estimated impact of the expiration of the $600 income supplement on the employment recovery gap closely.

Based on this extended model, we compute the employment losses relative to a counterfactual economy without the CARES Act. We find that when implemented in isolation, each provision of the CARES Act has only a modest effect. When implemented jointly, however, the UI programs generate substantial disincentive effects. Specifically, without any of the UI policy changes, the employment recovery would have been on average about 3.4 percentage points higher between April and December 2020. This employment loss represents around 20 percent of the average employment loss in the Leisure and Hospitality sector during the same period. This large employment decline is due to the sheer size and joint implementation of the pandemic UI provisions. The model's implied unemployment duration elasticities are actually quite modest and in line with the low-to-middle range of pre-pandemic estimates (e.g., Schmieder and von Wachter, 2016).

Our paper contributes to the literature that studies to what extent pandemic UI held back the labor market recovery. So far, this literature has found that pandemic UI benefits had only modest negative effects on job-finding rates and employment. Ganong et al. (2024) use individual bank account data and document that the weekly $600 supplements during the first four months of the CARES Act had only a moderate effect on job-finding rates. Petrosky-Nadeau and Valleta (2021) arrive at a similar conclusion using CPS data. Marinescu, Skandalis, and Zhao (2021) find that pandemic UI slightly decreased search effort but did not decrease vacancy creation. Coombs et al. (2021) compare job-finding rates in states that withdrew early from the pandemic UI benefits in the summer of 2021 to states that retained the benefits and find that employment would have been modestly higher in the absence of pandemic benefits. We argue that these limited estimated effects likely represent a mix of disincentive effects, stimulative effects, and possible other confounding local demand shocks.

Di Maggio and Kermani (2021) and Hellwig (2021) also estimate the labor market consequences of UI benefits taking into account both the disincentive and stimulative effects of UI policies. In the spirit of Mian and Sufi (2014), their approach compares the employment effects in non-tradable sectors — that are more affected by local demand conditions — and tradable sectors. In contrast, our methodology controls for local demand effects by comparing relatively homogeneous businesses that are part of the same local market. Thus, our methodology is conceptually closer to the border-county-pair (BCP) research design which has been used extensively to study the effects of expanded UI duration in the aftermath of the 2007-08 financial crisis.[4]

Finamor and Scott (2021), like us, use Homebase data to study labor market search at the state level. They find that a higher replacement rate is not associated with a lower labor market re-entry of workers. As mentioned, when we conduct our research design at a similar broad level of geographical aggregation our estimates become small and insignificant, highlighting the importance of comparing units within narrow local industries.

Finally, our paper contributes to the literature using structural models to quantify the effects of UI benefits. Typically, existing models focus on a single aspect of the UI policy such as the replacement rate (e.g., Landais, Michaillat, and Saez, 2018), the maximum duration of benefits (e.g., Nakajima, 2012; Kekre, 2021; Mitman and Rabinovich, 2022), expanded eligibility (e.g., Michaud, 2023), or one-time additions to usual benefits via income supplements as in the pandemic UI benefits (e.g., Petrosky-Nadeau and Valleta, 2021; Ganong et al., 2024). In contrast, our model quantifies the impact of the CARES Act taking into account all aspects of the pandemic UI policies: income supplements, expanded eligibility, and extended maximum duration. We show that first, expanded eligibility was the most disruptive of the UI policies especially for low-wage businesses, and second, that the large disincentive effects arise from the interaction of these UI policies.

The paper is organized as follows. Section 2 describes the research design. Section 3 outlines the data and Section 4 estimates the disincentive effect of pandemic UI benefits on employment recovery. Section 5 sets up the quantitative model and the calibration. Section 6 describes the model results. Finally, Section 7 concludes.

[4] See, for example, Hagedorn et al. (2013), Dieterle, Bartalotti, and Brummet (2020), Chodorow-Reich, Coglianese, and Karabarbounis (2019), and Boone et al. (2021). A direct application of the BCP design is not well-suited to study the effects of pandemic supplements, because these provisions apply to all counties.

2 Research design

Consider the following data-generating process

$$y_{j,c,t} = \alpha + \beta R_{j,c,t} + \mathbf{X}'_{j,c,t}\gamma + u_{j,c,t} + \varepsilon_{j,c,t}, \tag{1}$$

where $y_{j,c,t}$ is employment for business j (normalized by its value in some initial period t_0) in labor market c and period t, and $R_{j,c,t}$ is the business-specific replacement rate defined as

$$R_{j,c,t} = \frac{b_c(\overline{w}_{c,t_0}) + S_t}{w_{j,c,t_0}}. \tag{2}$$

This replacement rate measures how much the average unemployed worker in labor market c receives in UI payments relative to the wage offered by business j.[5] For the average unemployed, UI receipts depend on state-level benefits $b_c(\overline{w}_{c,t_0})$ where $\overline{w}_{c,t_0}$ is the average hourly wage in labor market c in the initial (pre-pandemic) period t_0 as well as the pandemic UI income supplement S_t (translated into per-hour units). Finally, w_{j,c,t_0} is the average hourly wage of business j in t_0.

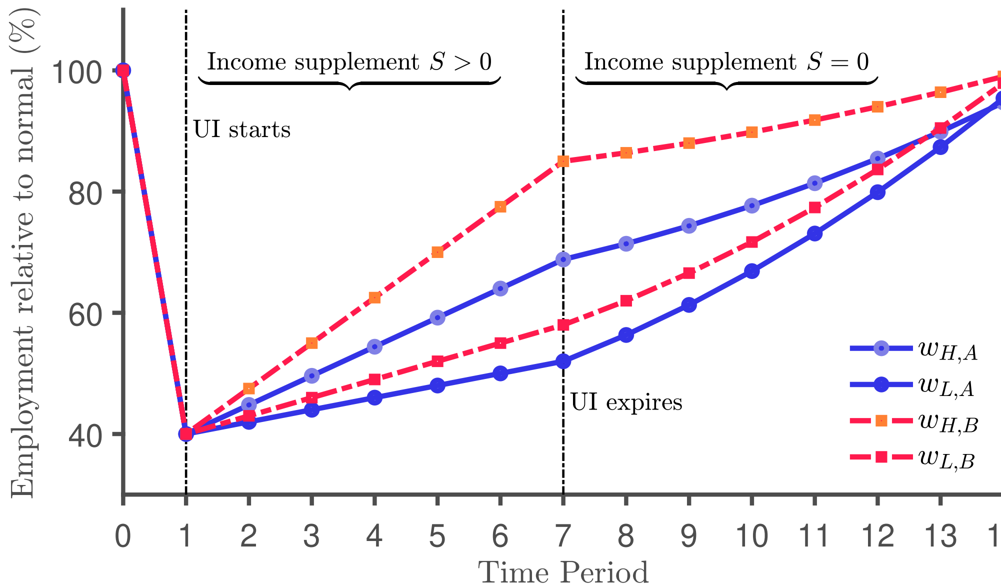
The coefficient β captures the negative impact of a higher replacement rate on business employment, i.e., it is a measure of the disincentive effect. Business employment is also affected by observable factors, $X_{j,c,t}$, and unobserved factors, $u_{j,c,t}$, that are potentially correlated with $R_{j,c,t}$. Unobserved factors can include, on the one hand, common effects such as demand shifts by the UI stimulus in labor market c and, on the other hand, idiosyncratic features of businesses (e.g., idiosyncratic demand shifts from the stimulus or quality differences). Finally, $\varepsilon_{j,c,t}$ denotes a classical error that is, by assumption, uncorrelated with $R_{j,c,t}$.

Unbiased estimation of β requires that $E[R_{j,c,t}u_{j,c,t} \mid \mathbf{X}'_{j,c,t}] = 0$. If UI benefits stimulate local demand or if they are otherwise positively correlated with local demand conditions, then $\hat{\beta}$ is biased upwards (making the estimated coefficient less negative than the actual disincentive effect). On the other hand, if high-wage businesses benefit more from the UI stimulus or are less affected by a negative shock such as the pandemic, then this implies a downward bias of $\hat{\beta}$.

Figure 1 gives a visual example of the identification concern. Consider two labor markets, A and B, each populated with two businesses. Average wages paid by the businesses in labor market A are higher than in labor market B. Because of the pandemic, businesses experience a large decline in their employment and gradually return to normal. With the same pandemic supplement S, labor market B has higher average replacement rates than labor market A.

[5] This definition implies that a business can potentially match with any unemployed worker in the labor market, a characteristic of random search models.

Figure 1: Example of Research Design



Notes: A hypothetical example of labor market recovery highlighting the identification concerns.

But despite the higher average replacement rate in labor market B relative to labor market A, the *average* employment recovery in labor market B is faster than in labor market A. This occurs because unemployed workers in labor market B experience a larger increase in their purchasing power compared to labor market A which translates to a higher demand for local output.[6] As a result, a typical estimation of regression (1) with fixed effects across location and time is not enough to estimate β without bias. This estimator cannot detect the confounding demand effects that shift the average employment recovery higher in labor market B.

We therefore propose to eliminate the confounding demand effects by averaging across low- and high-wage businesses of the same labor market c and then taking the difference. This results in the regression

$$\Delta y_{c,t} = \beta \Delta R_{c,t} + \Delta \mathbf{X}'_{c,t}\gamma + \Delta u_{c,t} + \Delta \varepsilon_{c,t}, \tag{3}$$

where $\Delta y_{c,t} \equiv y_{L,c,t} - y_{H,c,t}$ is the difference between the average employment-to-normal in the low-wage section and the high-wage section of labor market c, $\Delta R_{c,t} \equiv R_{L,c,t} - R_{H,c,t}$ is the difference between the average replacement rate in the low-wage section and the high-wage section of labor market c, and similarly for the other observable factors. As we show

[6]This is a relevant concern since Ganong et al. (2024) show that pandemic-UI recipients quickly spent a substantial portion of their benefits.

in empirical Section 3, the replacement rate gap in low-wage labor markets is larger than in high-wage labor markets.

The idea behind our design is that the component of $u_{j,c,t}$ that is common between low- and high-wage businesses is eliminated. It is plausible that the stimulative effects arising from local demand shocks are largely shared between neighboring businesses and therefore differenced out. It is equally plausible that within labor markets, a business with a high replacement rate recovers more slowly than a business with a low replacement rate, and this disparity in recovery increases with the difference in the replacement rates. For example, in Figure 1, the employment recovery gap in labor market B is larger than in labor market A because the replacement rate gap is also larger. As a result, our local industry estimator picks up the negative correlation between the replacement rate gap and the employment recovery gap by differencing out common demand effects. Note that our strategy not only deals with common demand effects arising from the UI stimulus but with *any* type of common shifts that may be correlated with the generosity of the replacement rates (e.g., local demand shifts due to pandemic regulations or consumer preferences).

Local differencing does not eliminate components of $u_{j,c,t}$ that differ across neighboring low- and high-wage businesses, most notably idiosyncratic demand and quality factors. For example, low- and high-wage businesses may not benefit proportionately from the UI stimulus or be differentially impacted by the pandemic. We address this issue in two ways.

First, we limit the analysis to a relatively short period around the expiration of the income supplement and include local industry fixed effects in our regression. Most pandemic-related restrictions have been lifted for this period, and the economy is relatively close to its normal capacity. Therefore, persistent differences between low- and high-wage businesses are unlikely to change much and will be picked up by our local industry fixed effects. Second, we directly control for observable differences in time-varying idiosyncratic factors in $\Delta\mathbf{X}'_{c,t}$. These include customer traffic, customer spending, and business quality.

Finally, note that our research design only identifies relative employment effects between neighboring businesses. In Section 5 we use a structural model that matches these relative employment effects to estimate the aggregate employment effects of the pandemic UI expansion.

3 Data

3.1 Employment and customer traffic

We use data from Homebase (HB), a scheduling and payroll administration provider used by thousands of small businesses in the U.S. Most of these businesses are restaurants and retail businesses that are individually owned and operated. The data provides us with a panel of businesses with daily data on employment, hourly wages, and hours worked. Given the nature of HB businesses, workers typically work for short periods before permanently exiting the sample. Thus, to guarantee a long balanced panel we analyze the employment recovery from the perspective of employers instead of individual workers. We combine the HB employment data with Safegraph data tracking the number of mobile devices that have visited establishments around the U.S. on a weekly basis (foot traffic). In addition, Safegraph provides information on credit card spending at the business level. We use these measures as proxies for customer demand.

Our sample covers the period from January 2019 to December 2020 and contains approximately 140,000 unique businesses. We construct a benchmark sub-sample for which (1) businesses are assigned to industries, (2) the sample is "balanced", (3) we have the average wage of businesses, and (4) there are at least two businesses for every local-industry cell.

Industries differ in their employment trajectories during the pandemic. It is therefore important to incorporate information about a business' industry. The HB data contains a business address but does not include consistent information on its industry classification. Based on the name and address of HB records we can match individual businesses to (i) Safegraph's Places of Interest (POI) data, which provides us with consistent industry coding for each business based on the North American Industry Classification System (NAICS) as well as information on the number of customer visits and customer expenditures for each business derived from anonymized cell-phone and credit card spending data; and (ii) Yelp data that includes the price range of the business.[7]

We are interested in how employment in businesses recovers during the pandemic, allowing for temporary business closures and differences in pre-pandemic trends. For this purpose, we construct a "balanced" sample by requiring that businesses are in the sample all of 2019, but may temporarily close in 2020. In our sample, businesses will shut down, especially in March and April 2020, but we do require that they re-open and operate at least until December 2020.

We want to study the relative employment paths of low- and high-wage businesses in local-industry cells. Thus, we restrict the sample to businesses that track not only the number of employees but also payroll, that is, we have information on both the business' employment and wages. Finally, to make the distinction between low- and high-wage businesses in a local-industry cell we drop single business cells. After imposing all of these restrictions we are left with 4,595 businesses for which we have weekly information.

With the baseline sample, we can analyze each business along the pre-pandemic period (i.e., to control for seasonal patterns) and across multiple store characteristics (location, industry,

[7]See Kurmann, Lalé, and Ta (2021) for a detailed description of the matching procedure.

foot traffic, price range, etc.). In Appendix A we show that employment recovery dynamics of the baseline sample is broadly similar to the recovery dynamics of the full HB sample, and Kurmann, Lalé, and Ta (2021) have shown that employment in the full HB sample tracks well employment in the Leisure and Hospitality sector. In addition, we show that the average hourly wages and hours per employee in our baseline HB sample are comparable to estimates from the CPS.

As described in our research design, our goal is to compare labor market outcomes of low- versus high-wage businesses within local industry cells. To that end, we sort businesses as follows. Let $w_{j,c}$ denote the log of the average hourly wage for business j in local-industry cell c, computed over all hourly wages paid to business employees in our base period, January and February 2020. Thus, a business is characterized by its pre-pandemic average hourly wage.

Local industry sorting then is based on a simple regression of business wages $w_{j,c}$ on local-industry dummies d_c:

$$w_{j,c} = a + d_c + e_{j,c}. \tag{4}$$

The residual $\hat{e}_{j,c}$ is the business' deviation (in percentage terms) from the local industry average. We classify a business as a high (low) wage business if the residual wage is higher (lower) than the local-industry average, i.e., $\hat{e}_{j,c} > 0$ ($\hat{e}_{j,c} < 0$).

A local industry is a set of businesses that share the same geographical area, industry, and price range. We define a geographical area by its four-digit zip code. On average, this definition bundles four neighboring zip codes. We define an industry at the two-digit level of NAICS, except for restaurants and bars (NAICS 722410, 722511, 722513, and 722515), which we define as a separate group. Finally, we bundle businesses into relatively inexpensive (one and two Yelp dollar signs) and relatively expensive (three and four Yelp dollar signs). Given our sample, this results in 1,195 local-industry cells with two or more businesses. Most cells have few businesses: the 25^{th} percentile cell has 2 businesses, the median cell has 3 businesses, and the 90^{th} percentile cell has 7 businesses. The largest number of businesses in a cell is 39.

We measure weekly employment in a business by counting the number of hourly-paid employees that worked for at least one day during the week in the business. If the business does not operate during the week, we set the number of employees to zero. Our definition aligns with the way official employment statistics are constructed based on monthly or quarterly payroll data (e.g., the Current Employment Statistics). Measuring business employment based on hourly-paid employees is consistent with our classification of businesses into low- vs. high-wage which is based on hourly wages. We test our results when we include a broader definition of employment that includes salaried workers (e.g., store managers) in the robustness section.

Table 1 reports statistics by business type (low- and high-wage within a local industry cell) averaged over all the days the business operated in 2019. The average number of employees

Table 1: Employment, Wages, and Customer Demand by Business Type

	All Businesses	Low-wage	High-wage
Wages			
Hourly wage ($)	10.9	10.0	11.8
Hours and Employment			
# Employees per business	12.8	12.5	13.2
Hours worked (per day)	4.9	4.8	5.0
Separation rate (%)	9.5	9.6	9.3
Hiring rate (%)	12.4	12.5	12.3
Customer demand & characteristics			
# In-person visits (per week)	1051	999	1098
Spending per transaction ($)	23.8	22.6	24.8
Share of restaurants and bars	0.86	0.85	0.87
# Businesses	4,595	2,194	2,401

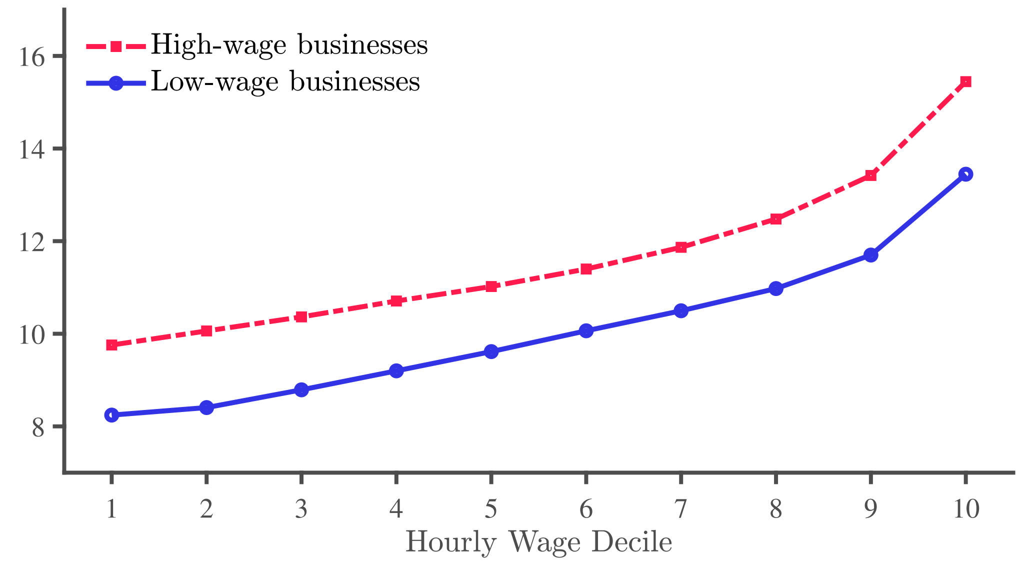
Notes: Averages are calculated for operating businesses in 2019. Low- and high-wage business classification is based on local industry sorting as described in the text.

is 12.8 across all businesses which indicates that the HB data capture mainly very small businesses. Furthermore, low-wage businesses are smaller than high-wage businesses. Employees work 4.9 hours per day and the hourly wage is $10.9. Even within narrowly defined local industries, there is sizable dispersion in hourly wages: high-wage businesses pay on average $1.8 more than low-wage businesses or equivalently, 8 percent more than the average.

Are employees working in low- and high-wage businesses fundamentally different? In Figure 2 we report hourly wages by decile for low- and high-wage businesses. Specifically, for each business, we rank the employees that have worked in the business during 2019 by their average hourly wage. On average, the bottom 10 percent of employees in low-wage businesses receive around $8 per hour while the bottom 10 percent in high-wage businesses receive around $10 per hour. The wage difference remains constant across the wage ladder. Hence, the wage differences do not arise from a few special employees at the top of the ladder but likely from some business fixed effect that affects uniformly the bottom and the top of the wage ladder.

Returning to Table 1, the weekly hiring rate is defined as the number of workers who work in week t but not in $t-1$ divided by the number of employed in t. The weekly separation rate is the number of workers who worked in $t-1$ but not in t divided by the number of

Figure 2: Hourly Wages Within Businesses



Notes: We sort employees in every business based on their hourly wage. We report the average hourly wage for each decile in the wage ladder during 2019.

employed in t. Table 1 reports weekly separation and hiring rates averaged over all the weeks in 2019. The weekly hiring rate and job separation rate in the HB data is 12.4 and 9.5 percent, respectively.[8]

During a typical week in 2019, high-wage businesses had around 100 additional customers relative to low-wage businesses while the average customer spent around $2 more in high-wage businesses. Finally, 86 percent of our sample are restaurants and bars.

3.2 Pandemic unemployment insurance benefits

The CARES Act which was signed on March 27, 2020, and intended to ameliorate the effects of pandemic lockdown measures, set off the largest expansion of UI programs in U.S. history. The Act extended UI eligibility to self-employed and gig workers and those not meeting state requirements on previous work experience through the Pandemic Unemployment Assistance (PUA) program. In addition, the Act increased benefits from the beginning of April 2020 through the end of July 2020 in that everyone who qualified for UI received an ad-

[8]Both of these numbers are larger than the equivalent values implied by monthly or quarterly data as typically reported in the literature (e.g., Hyatt and Spletzer, 2013). Closer inspection reveals that this difference arises because a substantial fraction of employees in the HB data miss work for a week or two and then return to the same business within the same month. The high-frequency weekly HB data capture these transitions, whereas they largely disappear in the time-aggregated monthly and quarterly data (for a related discussion, see Davis et al., 2013).

ditional \$600 in weekly benefits through the Federal Pandemic Unemployment Compensation (FPUC) program. Finally, the Act increased the duration for which benefits could be received by an additional 13 weeks beyond state benefit exhaustion through the Pandemic Emergency Unemployment Compensation (PEUC) program which in combination with increased UI duration at the state level implied that eligible workers did not exhaust benefits until at least the end of 2020.

After FPUC expired, the Trump administration issued an executive order on August 8, 2020, for Lost Wage Assistance (LWA) that was set to \$300 per week. The program was designed to last for six weeks but states did not disperse the benefits immediately after the expiration of FPUC. Only seven states started handing out benefits in August 2020 and most states first started to hand out benefits during the week of September 6^{th} and September 13^{th}. A subsequent \$300 weekly supplement was further extended between January 2021 and September 2021 together with expanded eligibility provisions.

Replacement rate effects Ganong, Noel, and Vavra (2020) estimate that the \$600 FPUC supplement led to a massive increase in replacement rates, nearly tripling typical benefit levels and raising the median replacement rate to 145%, with three-quarters of eligible workers receiving more in UI benefits than their previous labor earnings.

In our regression analysis, equation (3), rather than using the level of the replacement rate, we use the replacement rate gap, that is, the difference between the average replacement rates of low- and high-wage businesses in the same labor market as defined in equation (2).[9] The income supplement increases the gap for all labor markets but increases the gap more for labor markets with a lower overall average hourly wage or a larger difference in the hourly wage between low- and high-wage businesses. For illustration, assume that we have two businesses in labor market c with the low-wage business paying w_{L,c,t_0} and the high-wage business w_{H,c,t_0}. Then the replacement gap is

$$\Delta R_{c,t} = \frac{b_c(\overline{w}_{c,t_0}) + S_t}{w_{L,c,t_0}} - \frac{b_c(\overline{w}_{c,t_0}) + S_t}{w_{H,c,t_0}},$$

which can be written as

$$\Delta R_{c,t} = \underbrace{b(\overline{w}_{c,t_0}) \frac{w_{H,c,t_0} - w_{L,c,t_0}}{w_{H,c,t_0} w_{L,c,t_0}}}_{=a_c} + \underbrace{\frac{w_{H,c,t_0} - w_{L,c,t_0}}{w_{H,c,t_0} w_{L,c,t_0}}}_{=\delta_c} S_t. \tag{5}$$

Thus, labor markets with lower average hourly wages or larger inequality in hourly wages have

[9] The average replacement rate in a labor market, $R_{c,t}$, is strongly correlated with the difference in the replacement rates between low- and high-wage businesses within the labor market, $\Delta R_{c,t}$ (see Appendix B).

higher scaling factors δ_c. In addition, note that by adding a labor market fixed effect in our regression (3) we can eliminate the constant a_c but not the scaling factor δ_c.

Eligibility effects What has received less attention in the discussion of pandemic UI is how the increased UI recipiency through an expansion of UI eligibility has affected labor market outcomes. Larrimore, Mortenson, and Splinter (2023) use IRS data and estimate that the number of UI recipients in 2020 was more than twice that number in 2010 during the Great Recession. The increase in recipiency together with the increased benefits means that total UI benefits paid in 2020 were more than three times those paid in 2010. Using data from the CPS, we show in Section 5.5 that this rise in the recipiency rate is not only related to the expansion of eligibility toward gig and self-employed workers but applies more generally to workers in the Leisure and Hospitality sector. It is difficult to isolate the effects of UI eligibility extensions in the data but they come through clearly in our labor search model of the UI disincentive effects presented in Section 5.

4 Effects of pandemic UI benefits on employment

We show that in our HB sample, the presence of generous UI benefit supplements during the pandemic slows down the employment recovery of low-wage businesses relative to that of high-wage businesses.

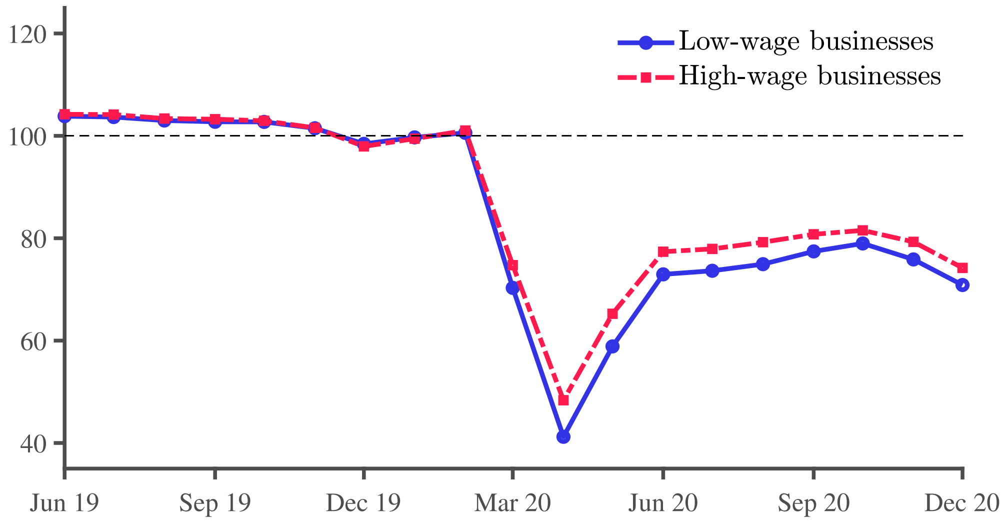
4.1 A first look at the data

We plot the raw time series of employment for low- and high-wage businesses prior and during the pandemic. Figure 3 shows employment aggregated at the monthly frequency. We normalize employment by the average employment in the business in our base period, i.e., January-February 2020. This way we can measure how far businesses are from their "normal" levels.

Before the pandemic, low- and high-wage businesses move tightly together. When the pandemic hits, employment declines to 41 percent of normal in low-wage businesses and to 48 percent of normal in high-wage businesses. Thereafter, employment recovers, but there remains a sizable gap in recovery rates between low- and high-wage businesses which visibly narrows in the summer of 2020. In the next section, we use weekly data to show that the narrowing of the employment recovery gap coincides with the expiration of the $600 income supplement.[10]

[10] We find that recalls do not play an important role in explaining the differential employment dynamics between low- and high-wage businesses (see Appendix B.1).

Figure 3: Employment Relative to Normal (in percent)



Notes: Monthly averages of the number of employees. Business employment is normalized by the average during January and February 2020. Low- and high-wage business classification is based on local industry sorting as described in the text.

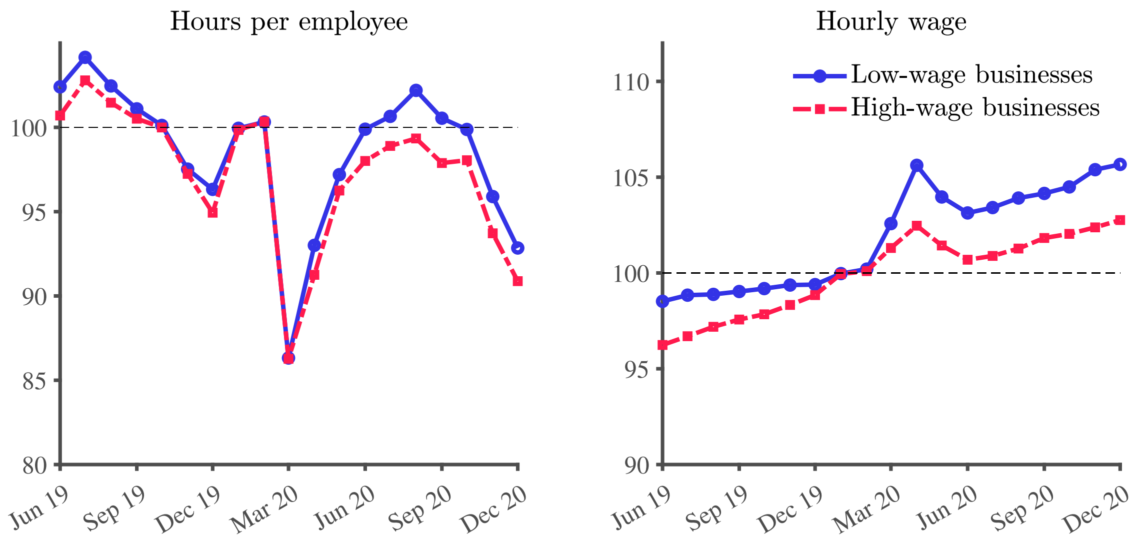
Figure 4 shows hours per employee (left panel) and hourly wages (right panel) aggregated at the monthly frequency. We normalize both measures by their averages in the business during our base period. Along the recovery path, hours per employee and hourly wages grow faster in low-wage businesses, suggesting that supply side channels drive the differential employment recovery of low- versus high-wage businesses reported in Figure 3.[11] If demand differentials were behind the differences in employment recovery we should see a higher hours per employee growth and a faster hourly wage growth for high-wage businesses as well. We confirm this interpretation in the next section where we show that the estimates of the disincentive effects are not impacted by the inclusion of local demand controls.

4.2 Event study analysis

The initiation of the CARES Act coincided with large disruptions in the labor market from pandemic-related restrictions making it hard to isolate the effect of UI on employment. Since the proposed regression (3) from Section 2 estimates the average effect of replacement rate gaps across time and labor markets it may conflate the pandemic downturn with the effects of the replacement rate gap. Furthermore, our quantitative model of Section 5 suggests that

[11]Although hourly wages grow faster in low-wage businesses, their overall level remains below that of high-wage businesses during the pandemic, so the classification of low- versus high-wage businesses remains unchanged.

Figure 4: Hours per Employee and Hourly Wages Relative to Normal (in percent)



Notes: Monthly averages of hours per employee and hourly wages. Both measures are normalized by their averages during January and February 2020. Low- and high-wage business classification is based on local industry sorting as described in the text.

employment adjusts gradually to UI changes. For these reasons, we focus on the weeks before and after the expiration of the \$600 income supplement and estimate the following event-study regression:

$$\Delta y_{c,t} = a_c + (\Delta R_{c,post} - \Delta R_{c,pre}) \sum_{s=T_1}^{T_2} \beta_s \mathbb{1}_{\{s=t\}} + \mathbf{Z}'_{c,t}\theta + \Delta \mathbf{X}'_{c,t}\gamma + \Delta\varepsilon_{c,t}. \tag{6}$$

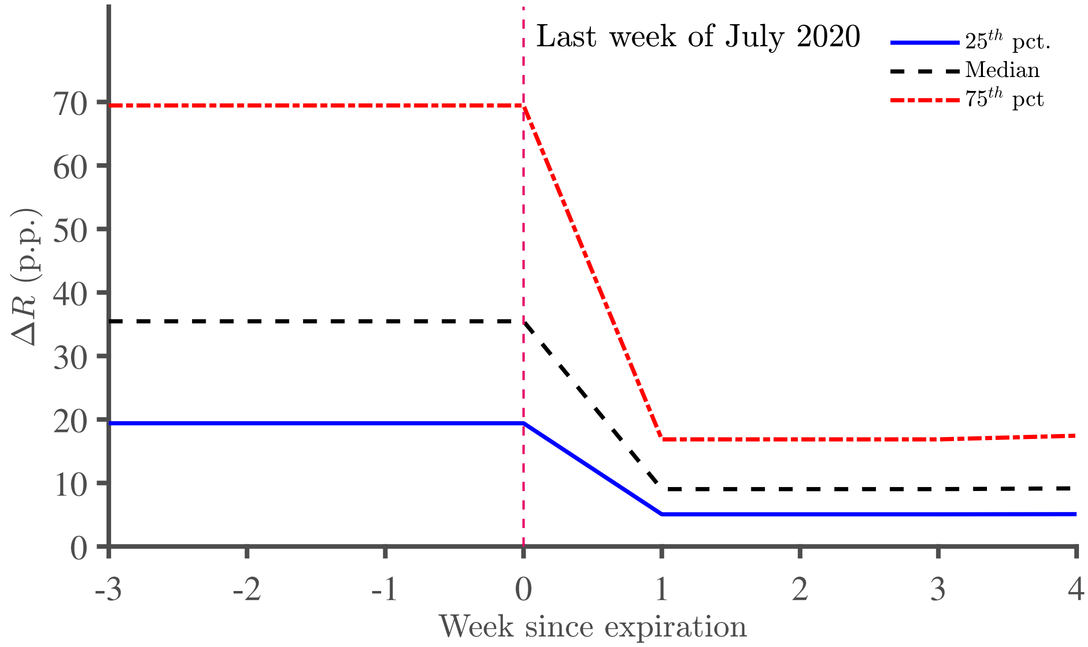
Variable $\Delta R_{c,post} - \Delta R_{c,pre}$ is the time-invariant change of the replacement rate gap in labor market c due to the expiration of the income supplement. We interact this variable with weekly time dummies to estimate separate disincentive effects for each week t in a time window around expiration $[T_1, T_2]$.

Previously we have argued that the replacement rate gap, $\Delta R_{c,t}$, depends on the local average wage and the wage gap between low- and high-wage businesses. Consequently, upon the income supplement expiration, the replacement rate gap declines more for some labor markets than others. From Figure 5 we can see that there is substantial variation in the decline of ΔR_c across local industries.[12]

In regression (6) we add a labor market fixed effect, a_c, which captures fixed differences between low- and high-wage businesses in the same labor market, industry, and price range

[12]The full distribution of the decline in the replacement rate, $\Delta R_{c,post} - \Delta R_{c,pre}$, across labor markets is given in Appendix B.

Figure 5: Replacement Rate Gap around Expiration of $600.

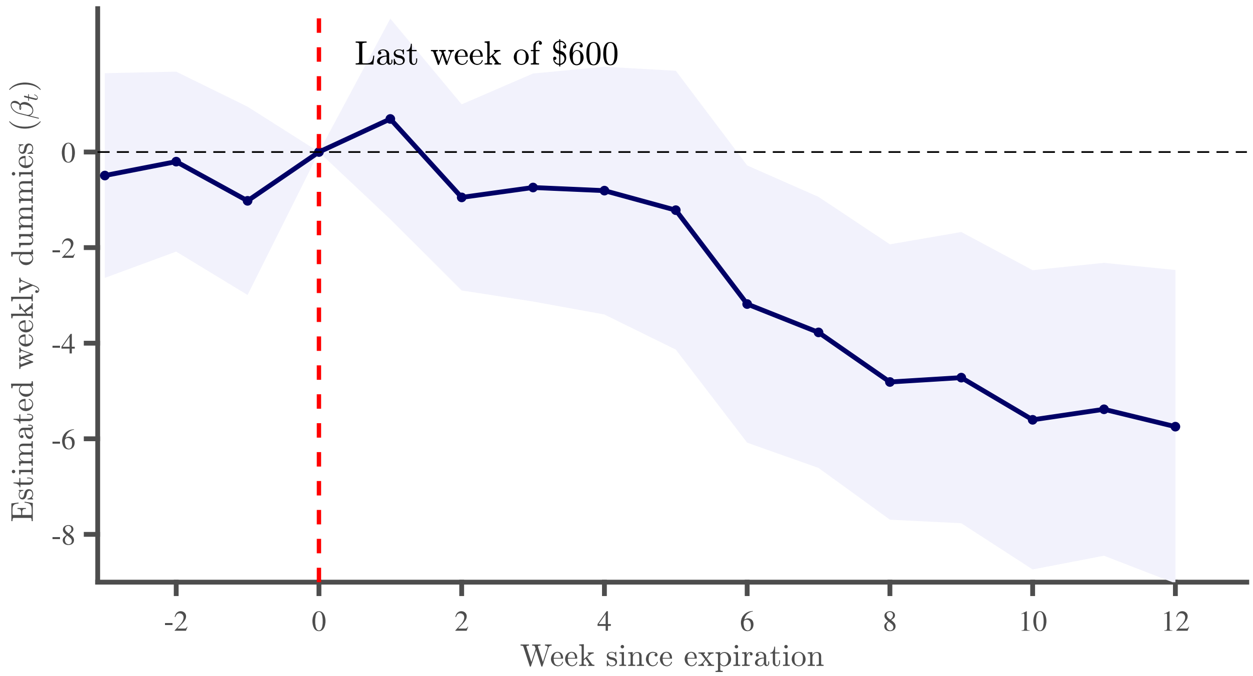


Notes: We plot the difference in replacement rates (ΔR) between the low- and the high-wage labor market, at the 25^{th}, the median, and the 75^{th} percentile of the distribution, around the weeks of the income supplement expiration. The difference is reported in percentage points.

during the period of analysis. We also control for the employment gap in cell c during the same week of 2019 in order to capture seasonal factors as well as two sets of observables. Vector $\mathbf{Z}_{c,t}$ includes *local-industry-wide* controls at week t, namely (1) the number of Covid-19 deaths in a county as a measure of community health risk; (2) stay-at-home regulations taking place in local industry c during week t; (3) the percentage change in visits in schools by week and county relative to January 2020 as a measure of disruptions in schooling (Kurmann and Lalé, 2023); and (4) local-industry-wide demand conditions during week t. This demand control utilizes all available information on the total number of customer visits and total customer spending (relative to the base period of January-February 2020) from all businesses in Safegraph in cell c during week t.

In addition, we include in vector $\Delta\mathbf{X}_{c,t}$ *differences* in the recovery rate in customer traffic and customer expenditures among low-wage and high-wage Homebase businesses, in labor market c and week t, relative to the base period of of January-February 2020. Since we do not have information on customer visits and credit card spending for all Homebase businesses, we cannot always construct the within-cell difference. For this reason we use as a baseline the specification with local-industry-wide controls, $\mathbf{Z}_{c,t}$, and show separately the results using the specification that also includes within-cell differences, $\Delta\mathbf{X}_{c,t}$.

Figure 6: Employment Recovery Around Expiration of \$600 (in p.p.)



Notes: Estimated β's from regression (6) reported in percentage points. The regression includes local-industry-wide controls and local industry fixed effects. Estimates are reported around the expiration of the \$600 income supplement in the summer of 2020. The shaded area represents the 95 percent confidence interval.

In Figure 6 we display the estimated coefficients on the weekly time dummies as well as their 95 percent confidence intervals when we run the baseline regression with local-industry controls and local-industry fixed effects. Standard errors are clustered at the local industry level. The omitted week 0 is the last week of the \$600 income supplement. We find that the pandemic UI benefits disproportionately affected low-wage businesses. In particular, upon expiration, the coefficients turn negative meaning that in labor markets with a larger drop in the replacement rate gap between low- and high-wage businesses (from the expiration of the \$600), there is a faster employment recovery in low- relative to high-wage businesses.

By comparing the employment gap between low- and high-wage businesses in the same labor market, industry, and price range, our baseline regression eliminates the effects of common local demand shifts arising from the UI income supplement. But even within narrow local industries, high-wage businesses may benefit relatively more from the UI demand stimulus than low-wage businesses. Thus, when the UI stimulus expires, the demand may shift back toward low-wage businesses that subsequently increase their employment. It is also possible that the expiration of the UI stimulus might differentially impact the quality of low- and high-wage

Table 2: Effect of \$600 Expiration on Employment Gap (in p.p.)

$\Delta y_{c,t}$ (Employment gap)	(1)	(2)
Replacement rate gap change × weekly dummy		
4 weeks after expiration	−0.80	−1.36
	(1.31)	(1.24)
8 weeks after expiration	−4.80***	−4.35***
	(1.47)	(1.73)
12 weeks after expiration	−5.74***	−4.66**
	(1.67)	(2.03)
Local-industry controls		
Covid-19 deaths	−0.93	1.50
(per 100,000 pop.)	(1.45)	(2.20)
School traffic	0.13	−0.24
(% change)	(0.84)	(1.07)
Customer visits	−0.25	−0.23
	(4.38)	(6.50)
Customer spending	−0.10	−0.02
	(0.15)	(0.80)
Within-cell differences		
Customer visit gap	–	0.31
		(0.77)
Customer spending gap	–	0.28
		(0.21)
# Observations	18,848	6,256

Notes: Estimates of parameters in regression (6) with standard errors in parenthesis. All regressions include cell fixed effects and the employment gap for the same week in 2019. Observations are at the local industry/week level and reported in percentage points. We winsorize variables at the one percent level and cluster standard errors at the local industry level.

businesses, which in turn affects demand for their products.

We can evaluate the validity of these concerns using our comprehensive, high-frequency data. Table 2 shows the estimated coefficients on selected weekly time dummies after the expiration of the income supplement. Specification (1) is our baseline regression that includes local-industry controls, and specification (2) adds within-cell differences as controls.

The baseline regression shows that the level of local-industry demand, such as customer visits or spending, does not have a statistically significant impact on the employment gap of low- and high-wage businesses. In other words, low- and high-wage businesses do not differentially respond to weekly changes in customer demand. We confirm this result also for the employment levels of low- and high-wage businesses in the year prior to the pandemic (Section 4.4).

For specification (2) we find that, as expected, idiosyncratic differences in demand are positively associated with the employment gap between low- and high-wage businesses. Nonetheless, employment in low-wage businesses still recovers faster than employment in high-wage businesses even after controlling for these idiosyncratic differences.[13]

Table 3: Effect of $600 Supplement on Relative Employment Recovery (in p.p.)

Estimate per 100 p.p. $\Delta R - \Delta R_{t_0}$	-5.7
$\times$ Average decline in ΔR	0.46
Effect of $600 supplement	-2.6

Notes: The baseline estimate is translated using the average decrease in the replacement rate gap.

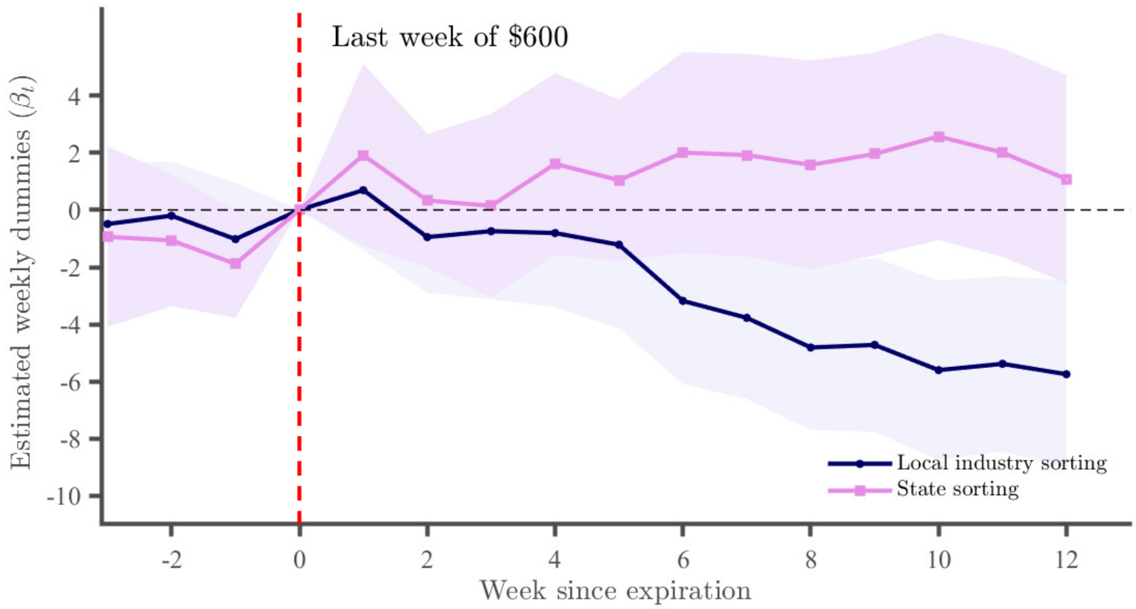
Our baseline specification implies that a 100 percentage point decline in the replacement rate gap, is associated with a 5.7 percentage point rise in low-wage business employment recovery relative to high-wage business employment recovery 12 weeks after the expiration. For the average labor market, the expiration of the CARES Act decreases the replacement rate gap by 46 percentage points, which implies a 2.6 percentage point rise in low-wage employment recovery relative to high-wage employment recovery 12 weeks after expiration (Table 3). This estimate should be interpreted as the effect of the $600 UI supplement on the employment recovery of low- vs. high-wage businesses *conditional* on the presence of the other CARES Act provisions. We emphasize this interpretation since in the structural model of Section 5 we show that the combined effect of the UI policies is much larger than the sum of their individual effects.

4.3 The Role of Local Industry Sorting

Our research design controls for demand effects in two ways. First, we compare neighboring businesses that share to some extent the positive impact of UI stimulus. Second, we

[13]Note that based on our findings, the slight decline in the estimate is not related to the additional controls, but to the reduction in the sample size.

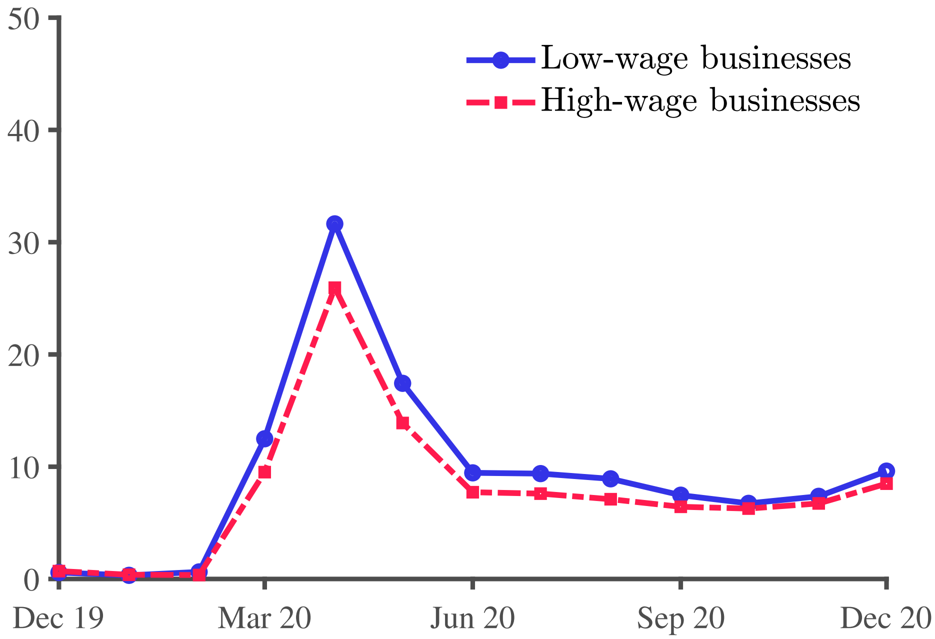
Figure 7: Expiration of \$600: Local Industry vs. State Sorting



Notes: Estimated β's from regression (6) for local industry sorting (baseline) and when cells are grouped at the state level. Estimates are reported in percentage points. We winsorize variables at the one percent level and cluster standard errors at the local industry and state level, respectively. The shaded area represents the 95 percent confidence interval.

include observable measures of idiosyncratic demand in neighboring low-wage and high-wage businesses. In Figure 7 we explore how the estimates change if we define a local industry more broadly. In particular, we compare low- and high-wage businesses within a U.S. State. Using such broad variation, the estimated effect of the replacement rate gap on the employment rate gap turns positive and becomes statistically insignificant. This happens because we compare businesses located in areas with different average wages and businesses in relatively low-wage areas benefit more from the relatively faster increase in the purchasing power of the unemployed in these areas. It is also possible that separate regions experienced different local demand shocks during the pandemic which may further confound the estimates. This exercise explains why we find different results from Finamor and Scott (2021) who find that with state-fixed effects only a higher replacement rate is not associated with a lower labor market re-entry of workers in HB data. Thus, our research design highlights the importance of comparing units within narrow local industries.

Figure 8: Share of Stores Closed During the Week (Averaged over a Month)



Notes: Share of stores with zero employees in a week reported in percent (monthly average).

4.4 Discussion and Robustness

We conduct a series of robustness exercises. A detailed analysis of the estimates is reported in Appendix C, Table C-5.

Low- vs high-wage response to local demand in 2019 A key identifying assumption is that low- and high-wage businesses within a local labor market do not differentially react to demand shocks. We test if this assumption holds prior to the pandemic. We run the following regression using 2019 data:

$$y_{j,c,t} = a_j + \tau_t + \theta_0 \text{Demand}_{c,t} + \theta_1 \text{Demand}_{c,t} \times \mathbb{1}_{\{\text{high} w_j\}} + \varepsilon_{j,c,t}$$

where $y_{j,c,t}$ is the employment of business j in cell c during week t and demand is either total customer visits or total spending in cell c and week t both normalized relative to their base period of January-February 2019. Demand is interacted with a dummy variable taking the value of one if the business is classified as high-wage. A business is classified as low- or high-wage based on the 2020 base period as in the baseline specification. We find that θ_1 is not significantly different from zero for either demand measure. Thus, low- and high-wage businesses in the same location, industry, and price range do not respond differentially to a local demand shift. These results are consistent with the statistically insignificant local demand effects for employment gaps around the expiration of the UI income supplement we documented in Table 2.

Effects of hourly wages and hours per employee Did the expiration of the $600 have an effect on hours per employee and hourly wages? We run the baseline regression (6) using differences in hourly wages and hours per employee between the low- and high-wage businesses as the left-hand side variable. Neither variable shows a statistically significant effect. Thus, the expiration of the income supplement affected the extensive margin of employment but not the intensive margin or the wages paid (see for example, Jager et al., 2020).

Business closures Our baseline sample includes businesses that operated continuously during the pandemic, as well as businesses that closed and re-opened. In fact, most of the HB businesses are open during the period of the generous income supplement. The average share of closed stores is 30 percent in April 2020, 16 percent in May 2020, and 7 percent in June 2020, see Figure 8. When we restrict the sample to the businesses that continuously operated during the pandemic the estimated coefficient at week 12 after expiration increases in absolute terms to −6.0 and is statistically significant at the one percent level.

Quality of a business We test the robustness of our results by including the difference in the quality of low- and high-wage businesses as an additional control. Our time-varying measure of business quality is specific to restaurants that comprise most of the businesses in our sample. Typically, in higher quality restaurants there are more employees per customer relative to lower quality restaurants. Thus, our quality measure is total hours worked at the business divided by the number of customer visits. Based on 2019 data, we find that high-wage businesses devote, on average, 1.13 hours worked for one customer while low-wage businesses devote, on average, 1.02 hours worked for one customer. When we also include differences in quality as part of the controls, the estimated replacement rate gap coefficient at week 12 after expiration changes to −3.9 and is statistically significant at the 5 percent.

Expanded sample In the baseline sample, local industry sorting is based on Yelp prices, which drops businesses without price information. As a check, we allow businesses for which we do not have Yelp-related information to form their own cell. This considerably increases the sample size to 10,558 businesses. For the expanded sample, the estimated replacement rate gap coefficient at week 12 after expiration changes to −4.5 and is statistically significant at the one percent level. Furthermore, in the baseline, we measure business employment based on hourly employees. When we also include salaried workers (managers) as part of the employment, the estimated replacement rate gap coefficient at week 12 after expiration changes only slightly to -5.4.

Base period and weights In the baseline specification we have normalized our time series with respect to the base period of January-February 2020. When we widen the base period to July 2019-February 2020, the estimate of the replacement rate gap coefficient decreases in absolute value to -5.3. Also, the benchmark specification weighs each cell equally. Once we weigh each cell by the number of businesses inside the cell, i.e., more populated cells take a higher weight, the coefficient on the replacement rate gap at week 12 changes to -5.4. In both cases the estimate remains statistically significant at the one percent level.

5 A model for the disincentive effects of pandemic UI

In this section, we build a labor search model and explore if we can quantitatively replicate the estimated disincentive effects of pandemic UI policies. Building on McCall (1970) and Albrecht and Axell (1984), we analyze how the job acceptance decision is affected by expanded UI benefits. The model features probabilistic eligibility of UI among the unemployed, probabilistic expiration of UI benefits, and benefits that depend on past wages as in, e.g., Ljungqvist and Sargent (1998, 2008).

5.1 Environment

Time is discrete and the discount factor is β. The economy is populated by a unit measure of workers and a fixed measure of firms, M. Workers are either employed or unemployed. A worker's utility is increasing and concave in consumption, $\mathcal{U}(c)$. Workers cannot save and consumption is equal to the wage w when employed and to an income supplement b when unemployed. Workers do not derive utility from leisure.[14]

Each firm (business) consists of one job that is characterized by (constant) wage w drawn from an exogenous distribution with probability density function $g(w)$. Note that the equilibrium offer distribution will differ from $g(w)$ since not all offers will be accepted. There is no exit or entry of jobs. Instead, when a filled job gets hit by a separation shock δ it becomes vacant. The measure of vacancies that offer wage w is denoted $v(w)$. We distinguish between active vacancies (total measure of $\tilde{v}$) and inactive vacancies (total measure of $v - \tilde{v}$). When

[14] We essentially assume that the non-pecuniary benefits from not working are small for workers who are at their borrowing constraint. These benefits are likely to be further reduced by the time spent searching for a job and the direct disutility from being unemployed.

a vacancy has zero probability of being accepted it remains inactive in which case it does not influence the matching process.

Employed workers separate from their jobs with probability δ and become unemployed. With probability p_R newly separated workers are eligible for UI benefits and the level of benefits is a function of the previously earned wage, $b_R(w)$. These are the UI-unemployed. Workers ineligible for UI, the non-UI unemployed, receive social benefits that also depend on the past wage, $b_N(w)$.[15] We assume that both benefits are increasing functions of the wage. Finally, in each period, UI unemployed lose eligibility with probability p_N and become non-UI unemployed.

Every period, unemployed workers randomly receive a new job offer with probability $\lambda(\theta)$, where $\theta = \tilde{v}/u$ denotes labor market tightness defined as the ratio of total active vacancies $\tilde{v}$ to all unemployed workers u, whether or not they are UI-eligible. Matching of unemployed workers with vacancies is governed by a constant returns to scale function, implying that vacancies meet unemployed workers with probability $\lambda(\theta)/\theta$. The total probability of receiving a job offer that pays wage w is $\lambda(\theta)\tilde{v}(w)/\tilde{v}$ where $\tilde{v}(w)$ is the measure of active vacant jobs paying wage w and $\tilde{v}$ is the measure of all active vacancies. Upon receiving a job offer with wage w', an unemployed worker with previous wage w will accept the offer if the wage exceeds the reservation wage associated with the value of remaining unemployed, $w^*(w)$.

After characterizing the steady state of this environment, we model the Covid-19 pandemic as a large one-time separation of employed workers into unemployment, followed by an unanticipated expansion of UI. To capture the various aspects of the CARES Act, this expansion consists of a temporary increase in the level of benefits for UI-eligible workers, a temporary increase in the probability of becoming UI-eligible upon job separation, and a temporary increase in the probability of remaining UI-eligible while unemployed.

5.2 Value functions

We describe the value functions of employed and unemployed workers. We use a time subscript when parameters or functions change along the transition from their steady state value. The value of a worker employed at wage w is

$$W_t(w) = \mathcal{U}(w) + \beta \left\{(1-\delta)W_{t+1}(w) + \delta \left[p_{R,t}U_{R,t+1}(w) + (1-p_{R,t})U_{N,t+1}(w)\right]\right\}, \tag{7}$$

[15] Adding heterogeneity to the non-UI unemployed eases the computation of the transition and is not crucial for our results. Hence, we choose a simple way to add heterogeneity without introducing an additional state variable.

where $U_R(w)$ and $U_N(w)$ denote the values of being unemployed while being eligible and ineligible for UI benefits, respectively. These values are given by[16]

$$U_{R,t}(w) = \mathcal{U}\left[b_{R,t}(w)\right] + \beta\left\{p_{N,t}\left[\lambda(\theta_t)\int \max\left\{W_{t+1}(w'), U_{N,t+1}(w)\right\}\frac{\tilde{v}_t(w')}{\tilde{v}_t}dw' + \left[1-\lambda(\theta_t)\right]U_{N,t+1}(w)\right]\right.$$
$$\left. + (1-p_{N,t})\left[\lambda(\theta_t)\int \max\left\{W_{t+1}(w'), U_{R,t+1}(w)\right\}\frac{\tilde{v}_t(w')}{\tilde{v}_t}dw' + \left[1-\lambda(\theta_t)\right]U_{R,t+1}(w)\right]\right\} \tag{8}$$

and

$$U_{N,t}(w) = \mathcal{U}\left[b_N(w)\right] + \beta\left\{\lambda(\theta_t)\int \max\left\{W_{t+1}(w'), U_{N,t+1}(w)\right\}\frac{\tilde{v}_t(w')}{\tilde{v}_t}dw' + \left[1-\lambda(\theta_t)\right]U_{N,t+1}(w)\right\}. \tag{9}$$

The value functions are increasing in the wage since flow utility is increasing in the wage, either directly when employed or indirectly when unemployed because benefits are increasing in the prior wage. Thus, a job offer is accepted if the associated wage exceeds the reservation wage, $w_i^*(w)$, which is implicitly defined by

$$W_{t+1}\left[w_{i,t}^*(w)\right] = U_{i,t+1}(w) \text{ for } i \in \{R, N\}. \tag{10}$$

Note that $w_{i,t}^*(w)$ in (10) captures the decision to work in period $t+1$, not t. This somewhat unusual definition helps streamline notations.

The reservation wage of non-UI unemployed is lower than the reservation wage of a UI unemployed since the benefits of non-UI unemployed are uniformly lower

$$w_{N,t}^*(w) \leq w_{R,t}^*(w). \tag{11}$$

Finally, $w_{\min}^*$ is the lowest acceptable wage for an unemployed worker

$$w_{\min,t}^* = \min_w w_{N,t}^*(w). \tag{12}$$

Vacancies with a wage below the lowest acceptable wage, i.e., $w < w_{\min,t}^*$, are inactive.

[16]We assume that UI-eligible unemployed learn whether their benefits expire before receiving a job offer. This timing assumption is consistent with the fact that, at least under normal circumstances, benefits duration is known in advance. However, alternative timing assumptions would generate very similar quantitative results.

5.3 Transition of measures

The measure of employed workers and hence, of the filled jobs that receive wage w evolves according to

$$e_{t+1}(w) = (1-\delta)e_t(w) + F_t(w)\frac{\tilde{v}_t(w)}{\tilde{v}_t}\lambda(\theta_t)u_t, \tag{13}$$

where $F(w)$ denotes the job acceptance rate, i.e., the probability that a random unemployed worker accepts a job offer paying wage w (to be defined below).

The measure of UI-eligible unemployed evolves according to

$$u_{R,t+1}(w) = p_{R,t}\delta e_t(w) + [1 - f_{R,t}(w)](1 - p_{N,t})u_{R,t}(w), \tag{14}$$

where $f_{R,t}(w) = \lambda(\theta_t)\int_{w' \geq w^*_{R,t}(w)} \frac{\tilde{v}_t(w')}{\tilde{v}_t}dw'$ is the probability that an UI-eligible worker with previous wage w is offered a job and accepts it. The measure of UI-ineligible unemployed evolves according to

$$u_{N,t+1}(w) = (1 - p_{R,t})\delta e_t(w) + [1 - f_{N,t}(w)][p_{N,t}u_{R,t}(w) + u_{N,t}(w)], \tag{15}$$

where $f_{N,t}(w) = \lambda(\theta_t)\int_{w' \geq w^*_{N,t}(w)} \frac{\tilde{v}_t(w')}{\tilde{v}_t}dw'$ is the probability that an UI-ineligible unemployed is offered a job and accepts it.

Furthermore, we have by definition

$$u_t = \int u_{R,t}(w)dw + \int u_{N,t}(w)dw, \tag{16}$$

where $u_{R,t}(w)$ denotes the measure of unemployed workers receiving benefits $b_{R,t}(w)$ and $u_{N,t}(w)$ the measure of unemployed workers who instead receive $b_N(w)$.

The measure of vacancies (active or inactive) paying wage w evolves according to

$$v_{t+1}(w) = \delta e_t(w) + \left[1 - F_t(w)\frac{\lambda(\theta_t)}{\theta_t}\right]v_t(w). \tag{17}$$

The distribution of vacant jobs (active and inactive) is determined by the following equation:

$$e_t(w) + v_t(w) = g(w)M, \tag{18}$$

where $g(w)$ is the exogenous distribution of wage offers and M is the measure of firms (or total jobs, filled and vacant). As mentioned, vacancies with wages that are not acceptable to unemployed workers, i.e., vacancies with wages $w < w^*_{\min}$, or equivalently $F(w) = 0$, will not be active and have $e(w) = 0$. We define all active vacancies as

$$\tilde{v}_t = \int_{w \geq w^*_{\min,t}} v_t(w) dw \tag{19}$$

Aggregating equation (18) over $w \geq w^*_{\min,t}$ we have

$$e_t + \theta_t(1 - e_t) = M \int_{w > w^*_{\min,t}} g(w), \tag{20}$$

where we have used that e is zero at wage levels less than $w^*_{\min,t}$, $\theta = \frac{\tilde{v}}{u}$, and that all workers in the economy are of measure one.

Given these definitions, the job acceptance rate $F(w)$ can be expressed as

$$F_t(w) = \frac{1}{u_t} \left\{ (1 - p_{N,t}) \int_{w' \geq w^*_{R,t}(w)} u_{R,t}(w') dw' + \int_{w' \geq w^*_{N,t}(w)} \left[u_{N,t}(w') + p_{N,t} u_{R,t}(w') \right] dw' \right\}. \tag{21}$$

Furthermore, the aggregate job-finding rate (i.e., the hazard rate of exiting unemployment) is

$$JFR_t = \lambda(\theta_t) \int \frac{\tilde{v}_t(w)}{\tilde{v}_t} F_t(w) dw, \tag{22}$$

and the aggregate vacancy fill rate is

$$VFR_t = \frac{\lambda(\theta_t)}{\theta_t} \int \frac{\tilde{v}_t(w)}{\tilde{v}_t} F_t(w) dw. \tag{23}$$

5.4 Stationary equilibrium

A stationary equilibrium is a set of value functions $\{W, U_R, U_N\}$, reservation wages $\{w^*_R, w^*_N, w^*_{\min}\}$, job acceptance rate F, distributions $\{e, u_R, u_N, u, v, \tilde{v}\}$, and market tightness θ such that:

1. The value functions $\{W, U_R, U_N\}$ satisfy equations (7), (8), and (9).

2. The reservation wages $\{w^*_R, w^*_N, w^*_{\min}\}$, satisfy equations (10) and (12).

3. The (un)employment and vacancy distributions $\{e, u_R, u_N, u, v, \tilde{v}\}$ satisfy equations (13), (14), (15), (16), (19), and (20).

4. The acceptance rate $F(w)$ satisfies equation (21).

5. Market tightness θ is given by $\tilde{v}/u$.

5.5 Calibration

We calibrate the model parameters such that the steady state fits pre-pandemic averages in the U.S. service sector. A summary of our parameters and calibration strategy is given in Table 4.

As in our empirical analysis, we specify the model's period to a week. Thus, we set $\beta = 0.9992$, consistent with an annual real interest rate of 4 percent. For the worker's utility function, we use a constant relative risk aversion specification $\mathcal{U}(x) = \frac{x^{1-\gamma}-1}{1-\gamma}$ with a coefficient of relative risk aversion $\gamma = 2$. The total number of jobs is set to $M = 1.013$ so that the equilibrium derived $\theta = 1.2$ which was the vacancy-to-unemployment ratio before the pandemic.

Matches are given by a Cobb-Douglas matching function: $m = \kappa u^{1-\eta}\tilde{v}^{\eta}$. Based on Şahin et al. (2014), we set the elasticity of matches to vacancies $\eta = 0.5$. We set κ to match the pre-pandemic average job-finding rate for low-wage workers. Many workers in the Leisure and Hospitality sector are characterized by relatively short employment and non-employment spells. Thus, instead of targeting monthly job-finding rates from CPS or other surveys, which are potentially subject to time-aggregation issues, we use evidence on vacancy fill rates and job separation rates from JOLTS and LEHD. We estimate vacancy fill rates from JOLTS following Davis et al. (2013) and then use LEHD data to account for the hires undercounted in JOLTS. For the Leisure and Hospitality sector, for the period 2015-2019, we find an adjusted weekly vacancy fill rate of $VFR = 0.28$ when we focus on hires from non-employment. Based on labor market tightness $\theta = 1.2$, we get a target job-finding rate equal to $JFR = 0.23$.

The job separation rate is based on LEHD data. For the same time period and focusing again on separations into non-employment, the weekly job separation rate is $\delta = 0.016$. Together, the job-finding rate and separation rate imply an unemployment rate equal to 6.4 percent.

For pre-pandemic unemployment benefits, we specify the function $b_R(w) = \rho_R w$ and set the replacement rate $\rho_R = 0.51$, which is the median replacement rate prior to the pandemic implied by our HB data (see Table B-3 in Appendix B). We set the pre-pandemic expiration probability of unemployment benefits to $p_N = 0.038$, which corresponds to an expected duration of 26 weeks equal to the maximum number of regular unemployment benefits eligibility in most U.S. States.

The benefit recipiency probability after job separation, p_R, combines UI eligibility with UI takeup. From the March CPS-ASEC, we calculate the fraction of those who reported receiving UI in the previous calendar year, conditional on being unemployed for at least one week following employment in Leisure and Hospitality. For the period 2010-2019, the average of this share is about 12.5%. However, linking CPS data with IRS tax data Larrimore, Mortenson, and Splinter (2023) shows that only about half of CPS respondents who receive UI benefits during an unemployment spell report these payments. We adapt their procedure to adjust UI recipiency rates in the March CPS data (see Appendix E for details) and find a 20 percent share of UI recipients before the pandemic and a 60 percent share for 2020.[17] The last estimate will be relevant for the CARES Act experiment that is described in the next section. To compute the model-equivalent statistic we generate a panel of workers using Monte Carlo simulation and ask the workers the same question asked in the CPS. The estimated probabilities are $p_R = 0.14$ for the steady state and $p'_R = 0.70$ for the pandemic experiment.

To match the equilibrium wage distribution (displayed in Table B-2 in the appendix), we use the dispersion of the firm wage distribution, σ, to match the upper half of the equilibrium wage distribution, and we use the non-UI replacement rate, ρ_N, to match the lower half of the wage distribution.

We assume that the exogenous firm wage distribution $g(w)$ is log-normal with a mean normalized to one and standard deviation σ. We set $\sigma = 0.10$ such that wages of filled jobs at the 75^{th} percentile are 6 percent higher than for the median filled job, similar to what we observe within local-industry cells in our HB data prior to the pandemic.

Standard search models do not generate much wage dispersion at the lower end of the wage distribution. Hornstein, Krusell, and Violante (2011) show that unemployed workers will turn down low-wage offers, given observed transition rates out of unemployment and reasonable calibrations of the flow return to unemployment. Introducing a non-UI state that has a sufficiently low utility flow, together with preferences that have a sufficiently high curvature, allows us to increase wage dispersion for the bottom half of our residual-wage distribution. In the HB data, the 10^{th} percentile of (residual) log wage is 87 percent of the mean (see Table B-2 in Appendix B). We are able to match this target through a non-UI replacement rate of $\rho_N = 0.15$.

Before continuing, it is instructive to compare our calibration of ρ_N to direct calibrations in the literature based on data from U.S. social assistance programs. Pavoni and Violante (2007) compute the median allotment of food stamps and find that it is worth \$397 per month. Given the average monthly wage of \$1,500 in their model, this implies a replacement ratio of social assistance equal to 26 percent. Foster and Rojas (2018) report that the average recipients of TANF and SNAP receive \$3,072 and \$3,928 per year, which yields \$135 of social assistance per week. Compared to average hourly earnings from the CPS for NAICS 71-72, this puts the replacement ratio at almost 30 percent. While these replacement ratios are higher than the

[17] Our measure for the combined eligibility and take-up rate of UI is lower than the recipiency rates estimated in Birinci and See (2022) using 1996-2016 SIPP data (equal to 0.35). But Birinci and See (2022) also note that eligibility and take-up shares are lower for low-income groups, such as the kind of workers that comprise our HB sample.

Table 4: Parameter Values

Parameter	Notation	Value	Target/Reference
Externally set parameters:			
Discount factor	β	0.9992	4% annual interest rate
Risk aversion	γ	2	–
Job separation rate	δ	0.016	LEHD
Matching function elasticity	η	0.5	Şahin et al. (2014)
Replacement rate, UI	ρ_R	0.51	UI system
Expiration probability	p_N	0.038	UI system
Calibrated parameters			
Total number of jobs	M	1.013	Steady state $\theta = 1.2$
Recipiency probability	p_R	0.15	CPS and IRS data
Matching efficiency	κ	0.23	Weekly job finding rate of 0.23
Wage dispersion	σ	0.10	Wage dispersion upper half
Replacement rate, non-UI	ρ_N	0.15	Wage dispersion bottom half

Notes: The table reports the parameter values of the model. The model period is set to be one week.

$\rho_N = 0.15$ that we obtain from our indirect calibration, we note that these empirical estimates also include recipients that are non-attached to the labor force over the long-term so that the social supplements likely replace a larger share of their previous market income. In contrast, in our environment, non-UI unemployed are attached to the labor force and generally return to employment relatively quickly.

5.6 Transitional dynamics

We model the pandemic transition as an initial shock to employment, followed three weeks later by an unanticipated UI expansion. First, in period $t = 0$ we reduce employment in high-wage and low-wage businesses such that, consistent with the HB data, total employment declines by 60 percent. We allocate workers who lose their jobs to UI and non-UI unemployment based on the steady state recipiency probability.

Second, the UI system changes at $t = 3$, i.e., three weeks after the initial separation shock, which corresponds to the first week of April 2020. The changes involve the magnitude of benefits, the duration for which benefits are paid, and the set of workers who are eligible to receive the benefits. We assume that these changes were not anticipated as of $t < 3$, and that their expiration is anticipated.

The CARES Act provided supplemental benefits of $600 per week on top of usual state benefits through the end of July 2020. We model this change as an increase in weekly unemployment benefits from $b_R(w) = 0.51w$ to $b'_R(w) = 0.51w + S$ that lasts for 16 weeks, setting S such that the average replacement rate equals about 2.2 which is the replacement rate during the CARES Act in our HB sample (see Table B-3 in appendix).

In addition, the CARES Act extended benefits for 13 weeks beyond the usual state-level duration of benefits so that, effectively, benefits did not expire until the end of December 2020. We model this change as a decrease in the benefit expiration probability from $p_N = 0.038$ to $p'_N = 0$ that lasts for 39 weeks.

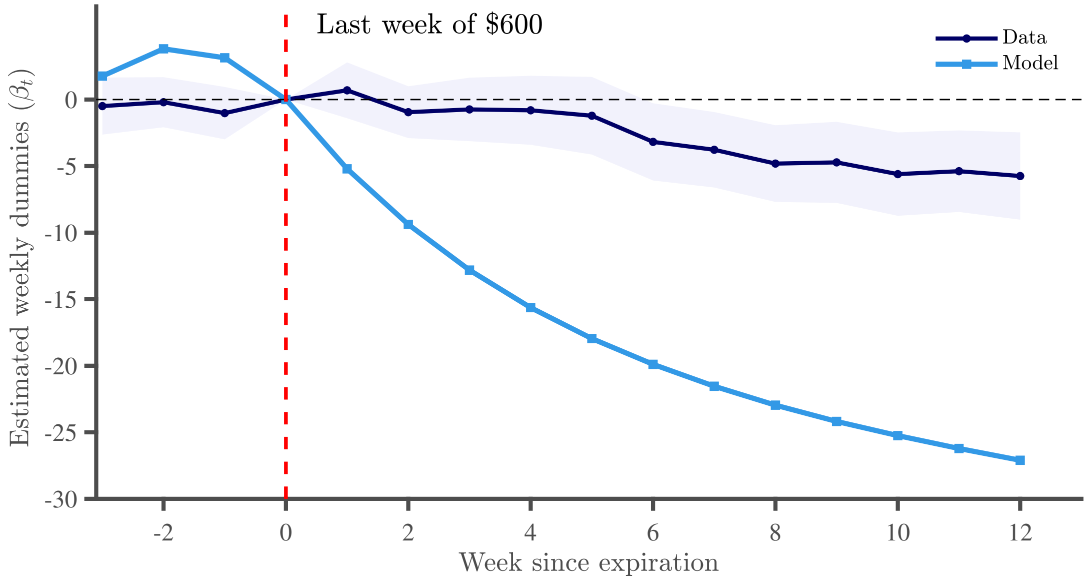
Finally, the CARES Act expanded eligibility, among others, to part-time workers and workers who previously did not qualify for benefits due to insufficient quarterly earnings. In addition, it is likely that the more generous UI increased take-up rates among the eligible. We model this as an increase in the recipiency probability from $p_R = 0.14$ to $p'_R = 0.7$ targeting a 60 percent share of unemployed who have reported receiving UI during 2020 in the CPS.

We assume that a share of workers who lost their jobs at $t = 0$ and were not eligible for UI during the first three weeks of the recession become eligible once the CARES Act comes into effect (at $t = 3$). Without this assumption we cannot match the high pandemic UI recipiency share we document from the CPS. The size of unemployed who shift to eligibility at $t = 3$ is given by the number of separated workers (Δe) who would have taken UI had eligibility been available from $t = 0$ and provided they did not find a job until $t = 3$. Thus, at each wage w, the shift increases $u_{R,3}(w)$ by $\Pi_{t=0}^{2}(1 - f_{N,t}(w))(p'_R - p_R)\frac{u_{N,3}(w)}{u_{N,3}}\Delta e$. This shift affects 40 percent of non-UI unemployed workers.

All three policy changes are assumed to be unexpected when they start, but their expiration is fully anticipated as announced. We also assume that there are no follow-up changes to UI policy. As various additional changes to UI policy occurred, through Fall 2020 and then 2021, we focus our analysis on the implications for the 16 weeks for which supplemental benefits were initially expected to be in place.

We solve for the perfect foresight transition equilibrium as follows. We assume that the economy converges to the pre-pandemic steady state after some time T. We guess a path for market tightness, $\{\theta_t\}_{t=0}^{T}$, and the measure of vacancies $\{v_t(w)\}_{t=0}^{T}$. We solve the value functions and reservation wages starting from period T and moving backward. Next, we simulate forward the transition paths for employment, unemployment, and vacancies using the value functions and transition equations and obtain a new guess for the paths of market tightness and vacancies. We repeat the process until this algorithm converges. The transition path is defined formally in Appendix D.

Figure 9: Effects of \$600 Expiration on Employment Recovery: Model vs. Data



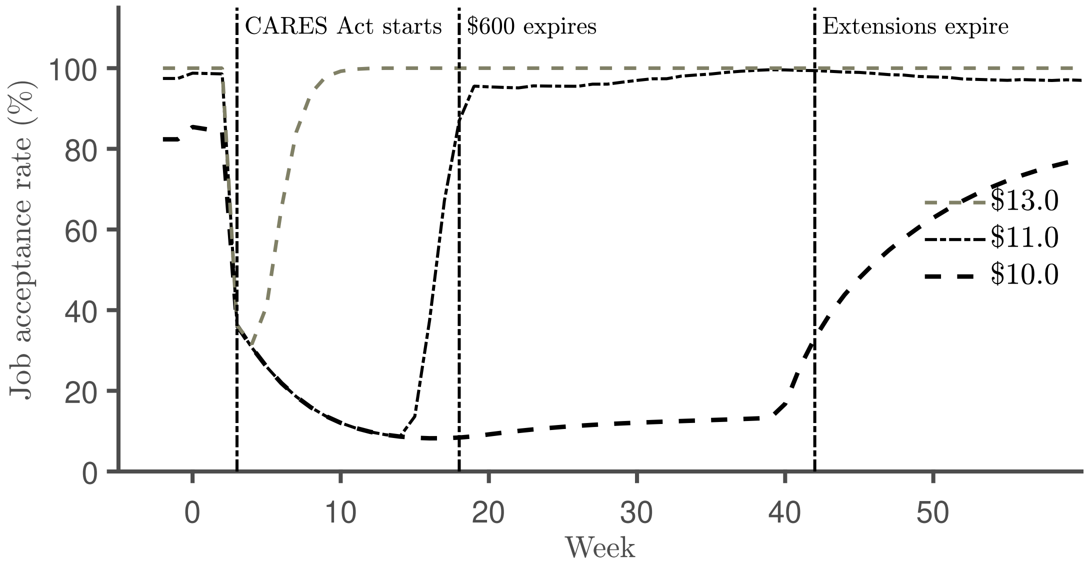
Notes: The figure plots the estimated β's from regression (6) in the model versus the data. The shaded area represents the 95 percent confidence interval of the data estimates.

6 Quantitative results

Our quantitative analysis focuses on the disincentive effects of UI as represented by the employment recovery gap between low-wage and high-wage jobs. The quantitative model does not capture demand effects or other common labor market factors (e.g., pandemic restrictions), which is appropriate given that the disincentive effects are measured within narrow local industry markets. We use the model-generated data on employment and replacement rates to run the exact same regression as in the data (regression (6)) for the same time window. Figure 9 compares the estimates in the model versus the data. We find that the baseline model overstates the estimated impact of the expiration of the \$600 supplement on the employment recovery gap by a large degree.

Figure 10 helps to understand why the baseline model performs poorly. We plot acceptance rates, $F(w)$, for three businesses that offer \$10.0 and \$11.0 per hour (low-wage businesses) and \$13.0 per hour (high-wage business), respectively. Businesses can attract different groups of unemployed. The high-wage business offering \$13 per hour can attract all types of unemployed workers (both those without UI benefits and those with benefits) and that is why $F(w)$ is 100 percent in the steady state. The same is true for the low-wage business offering \$11 per hour. The low-wage business offering \$10 per hour can only attract from the non-UI unemployed (the u_N set) and that is why the acceptance rate is around 80 percent in the steady state.

Figure 10: Acceptance Rate for Different Job Offers: Model



Notes: The figure reports the fraction of unemployed (in regular benefits and social assistance) that would accept the wage offered by a business, each week. Businesses offering $10 and $11.0 dollars per hour are low-wage businesses and the business offering $13 dollars per hour is a high-wage business. Dates of initiation and expiration of the CARES Act provisions are reported.

The CARES Act has two major effects. First, it decreases the acceptance rates of the UI-unemployed (the u_R group) by offering $600 per week and extending the duration of the benefits. Second, it increases the pool of the UI-unemployed and decreases the pool of the non-UI unemployed by expanding eligibility.

Initially, the UI-unemployed reject the $13 offer. The rejection is temporary though and lasts only for a few weeks. Anticipating the expiration of the pandemic supplement, UI-unemployed workers gradually start to accept the offer so that the acceptance rate converges to its steady state relatively fast and prior to the expiration of the supplement.

The business offering $11 per hour also experiences a rejection from the UI-unemployed. But on top of that the share of non-UI unemployed in total unemployment (who accept its offer) immediately declines due to the expanded eligibility. The share of non-UI unemployed then continues to shrink because first, new unemployed are continuously allocated to u_R which continues to increase (thus, shrinking the share of non-UI unemployed to total unemployment), and second, because UI-unemployed now accept job offers at a relatively lower rate and their share increases even more. When the $600 is close to expiring the acceptance rate rapidly bounces back to its normal levels.

Finally, the business offering $10 per hour recovers at the slowest pace. This business also experiences a decline in acceptance rates because the share of the non-UI unemployed shrinks.

For this business, the expiration of the \$600 supplement does not immediately improve the acceptance rates because the business was not attracting workers from the UI-unemployed in the first place. Over time, however, the share of non-UI unemployed is increasing because the relative acceptance rate of UI-unemployed has now increased. But this process is very slow and only when the eligibility extension expires (around week 42) can this business begin its employment recovery.

The behavior of the relative offer acceptance rates at low- and high-wage businesses helps us understand the employment gap dynamics in Figure 9. The low-wage businesses hire mainly from the pool of non-UI unemployed, which prior to UI eligibility extension represented about 80 percent of the unemployed (since only about 20 percent of workers in this segment of the labor market receive UI benefits). But with expanded eligibility the UI recipiency rate sharply increases and the low-wage businesses have a substantially smaller pool to hire from. As a result, employment in low-wage firms declines dramatically during the period of \$600 income supplement and recovers at an equally fast pace after the expiration.

6.1 Reconciling the model with the data

In the baseline model employment recovery overstates the effect of the \$600 expiration on the employment recovery gap relative to what we document in the data. In this section, we propose a reduced-form solution to reconcile the model with the data: a probability of losing UI eligibility upon refusal of a job offer.[18]

In the baseline model, UI recipients can reject a wage offer without losing their UI status. According to UI law, an unemployed who refuses suitable work loses eligibility. In addition, employers can themselves contest the eligibility of a claimant. However, it is difficult to determine how prevalent such denials were for unemployed who refused to return to work. Still, for workers, losing the eligibility status could be perceived as a real possibility.

Assume that unemployed workers who reject an offer, lose their eligibility status with probability χ. As a result, the value function for an unemployed who receives UI benefits is

[18] Boar and Mongey (2020) consider multiple other possibilities that can explain why some workers returned to work in spite of the generous UI benefits such as workers being unable to return to their old job even if they wanted to or lower wages after an unemployment spell. Their proposed solutions are less likely to apply to our sample of restaurant businesses where worker tenure is typically short-lived.

written as follows:

$$
\begin{aligned}
U_{R,t}(w) = \mathcal{U}\left[b_{R,t}(w)\right] + \beta p_{N,t}&\left[\lambda(\theta_t)\int \max\left\{W_{t+1}(w'), U_{N,t+1}(w)\right\}\frac{\tilde{v}_t(w')}{\tilde{v}_t}dw' + \left[1-\lambda(\theta_t)\right]U_{N,t+1}(w)\right] \\
&+ \beta(1-p_{N,t})\left[\lambda(\theta_t)\int \max\left\{W_{t+1}(w'), (1-\chi)U_{R,t+1}(w) + \chi U_{N,t+1}(w)\right\}\frac{\tilde{v}_t(w')}{\tilde{v}_t}dw'\right] \\
&+ \beta(1-p_{N,t})\left[1-\lambda(\theta_t)\right]U_{R,t+1}(w). \qquad (24)
\end{aligned}
$$

Accordingly, the reservation wage of UI recipients is given by:

$$
W_{t+1}\left[w^*_{R,t}(w)\right] = (1-\chi)U_{R,t+1}(w) + \chi U_{N,t+1}(w). \tag{25}
$$

The measure of these workers evolves according to

$$
u_{R,t+1}(w) = p_{R,t}\delta e_t(w) + \left[1 - \left(f_{R,t}(w) + \chi(1 - f_{R,t}(w))\right)\right](1-p_{N,t})u_{R,t}(w), \tag{26}
$$

where, as before, $f_{R,t}(w) = \lambda(\theta_t)\int_{w' \geq w^*_{R,t}(w)} \frac{\tilde{v}_t(w')}{\tilde{v}_t}dw'$ is the probability that an UI-eligible worker with previous wage w is offered a job and accepts it. Finally, the measure of UI-ineligible unemployed evolves according to

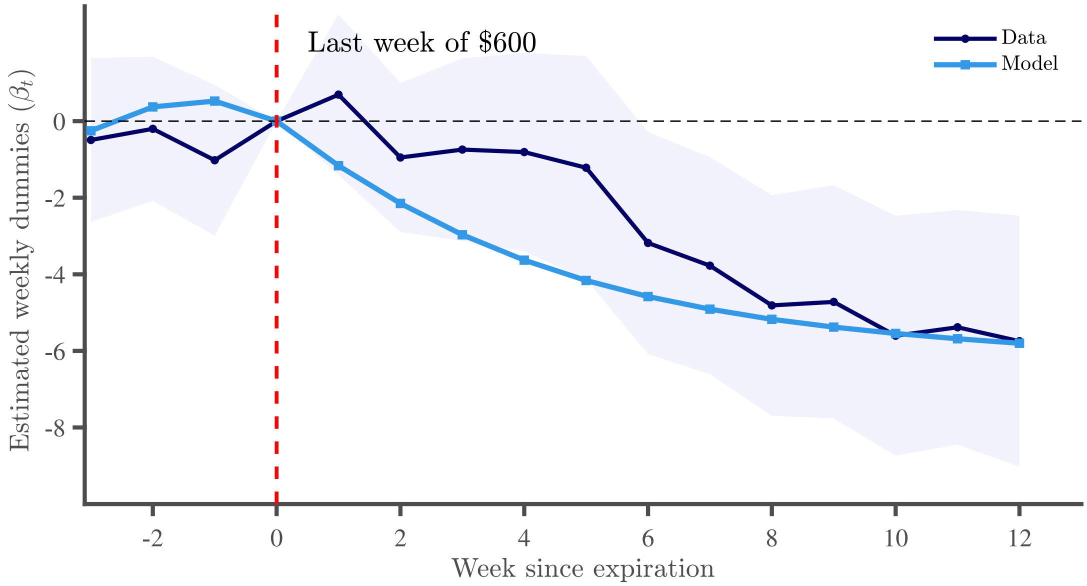
$$
\begin{aligned}
u_{N,t+1}(w) =& (1-p_{R,t})\delta e_t(w) + \left[1 - f_{N,t}(w)\right]\left[p_{N,t}u_{R,t}(w) + u_{N,t}(w)\right] \qquad (27) \\
&+ \chi(1 - f_{R,t}(w))(1-p_{N,t})u_R(w), \qquad (28)
\end{aligned}
$$

where the last term measures the flow of UI recipients whose benefits did not expire but were lost by rejecting a job offer. All other model equations are the same.[19]

We set the probability of losing eligibility upon job refusal, $\chi = 16.5\%$, to match the estimated impact of the \$600 expiration on the employment recovery gap between low- and high-wage businesses 12 weeks after expiration (the estimated coefficient from regression (6)). Figure 11 shows that the model is able to match the data closely and also reproduces the gradual decline in the estimated β's over time. Search frictions prevent low-wage firms from recovering their employment all at once. As a result, the \$600 expiration has a larger effect on the employment recovery gap over a longer horizon compared to the weeks closer to the expiration.

[19]The extended model can still match the benchmark targets and thus, does not need a re-calibration of other parameter values.

Figure 11: Employment Recovery Gap: Model with UI Loss upon Job Refusal



Notes: The figure plots the estimated β's from regression (6) in the model with the probability of losing UI upon job refusal versus the data.

6.2 Aggregate employment losses from pandemic UI benefits

We compute the effect of the CARES Act on employment recovery and quantify separately the impact of each provision. We study the employment losses within the model with a probabilistic UI loss upon job refusal which is able to closely replicate the estimated UI effects from the data.

In Table 5 we compute the employment loss of each UI policy provision when implemented separately when combined with the other two policies, and finally, the overall employment losses from all UI policy provisions. To measure the employment loss associated with a policy we compute the difference between the employment-to-normal level when the policy is in effect to the employment-to-normal level when the policy is not in effect and take the average over the period April to December 2020.

The aggregate employment recovery loss associated with each UI policy when implemented separately is modest. However, when combined, the pandemic UI policies generate substantial aggregate employment losses. Conditional on the other two provisions being in effect, the income supplement holds back the employment recovery by 1.6 percentage points, the extended maximum duration by 1.5 percentage points, and the expanded eligibility by 2.9 percentage points. Without any of the UI policy changes, the employment recovery would have been, on average, 3.4 percentage points closer to normal between April and December

Table 5: Employment Recovery Loss (in percentage points)

Each provision alone	
$600 Additional UI Benefits	0.2
Extended Duration of UI Benefits	0.2
Expanded Eligibility of UI Benefits	1.0
Each provision when other provisions are in effect	
$600 Additional UI Benefits	1.6
Extended Duration of UI Benefits	1.5
Expanded Eligibility of UI Benefits	2.9
CARES Act (all provisions combined)	3.4

Notes: The table reports the employment recovery loss with respect to each CARES Act provision using simulated data from the transition equilibrium. In the top three lines, the losses are computed when the other two provisions are inactive while the middle three lines compute the losses when the other two provisions are in effect. Estimates are reported in percentage points.

2020. This employment loss represents around 20 percent of the average employment loss in the Leisure and Hospitality sector during the same period. The employment loss peaks at almost 8 percentage points about 10 weeks after the initiation of the CARES Act (Figure 12). Thereafter, the disincentive effect gradually diminishes as the $600 supplement expires and the expanded eligibility and extended duration policies draw closer to an end.

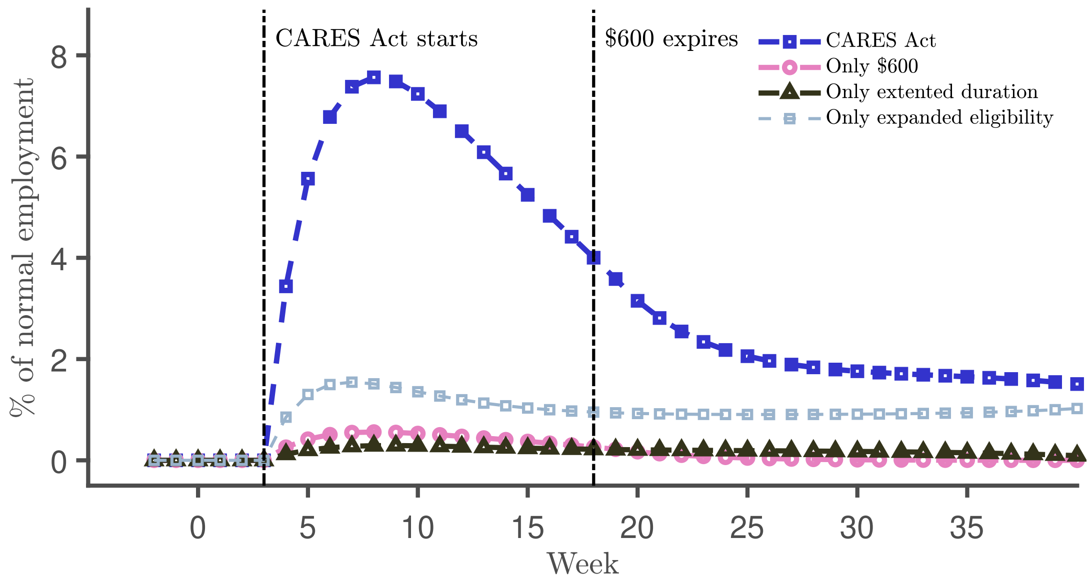
In sum, our quantitative results underline the importance of jointly taking into account the different UI policy changes brought about by the CARES Act.

6.3 Unemployment duration elasticities

Using the model's transition path we compute unemployment duration elasticities (see Appendix F for more details). Similarly to Table 5, we calculate the unemployment duration elasticity of each of the three UI provisions when implemented alone and when implemented in combination.

Table 6 reports the results. The $600 supplement alone increases the average unemployment duration by 8 percent (from 3.4 weeks to 3.7 weeks). Since in our model, the replacement rate rises by 343 percent (from 0.51 to 2.2), this implies a duration elasticity of 0.02. In turn, the extended benefits alone increase unemployment duration by 6 percent. According to the CARES Act, benefits did not expire until the end of the year (39 additional weeks) when the usual duration of state-level benefits would be in effect. Thus, the (potential) duration of benefits rose from 26 weeks to 65 weeks, or an increase of 150 percent, which generates

Figure 12: Employment Losses due to CARES Act UI policies



Notes: The figure reports the aggregate loss of pandemic UI programs as a percent of normal employment. Separate losses for each program are reported by simulating the model with only a single provision at a time.

an elasticity of 0.03. Expanded eligibility increases the expected duration of unemployment by 19 percent. The model inferred probability of receiving benefits rose from 14 percentage points to 70 percentage points, or by 400 percent which generates an elasticity of 0.04.

Table 6 also reports the duration elasticities of each of the provisions conditional on the other two provisions being in effect. For example, when the $600 is activated on top of the other two provisions, unemployment duration increases by 85 percent implying an elasticity of 0.24. When the extended benefits provision is activated on top of the other two provisions, unemployment duration increases by 65 percent implying an elasticity of 0.43. When the expanded eligibility provision is activated on top of the other two provisions, unemployment duration increases by 112 percent implying an elasticity of 0.28.

Our numbers are in the low-to-middle range of existing estimates. With respect to the duration elasticity out of benefit extensions, Katz and Meyer (1990) find estimates between 0.3-0.5. Johnston and Mas (2018) and Landais (2015) find even larger elasticities between 0.4-0.8. On the lower end of the estimates Rothstein (2011) estimates an elasticity of 0.06 and Farber and Valletta (2015) estimate an elasticity of 0.15. Schmieder and von Wachter (2016) report a similarly large divergence in the estimates of the duration elasticity of benefit supplements. For the U.S., the median is 0.38, and the range is from 0.1 to 1.2.

Although the duration elasticities implied by our model are modest, the employment im-

Table 6: Unemployment Duration Elasticities implied by CARES Act Provisions

	Unemployment Duration	Elasticity
Each provision alone		
\$600 Additional UI Benefits	+8%	0.02
Extended Duration of UI Benefits	+5%	0.03
Expanded Eligibility of UI Benefits	+19%	0.04
Each provision when other provisions are in effect		
\$600 Additional UI Benefits	+85%	0.24
Extended Duration of UI Benefits	+65%	0.43
Expanded Eligibility of UI Benefits	+112%	0.28

Notes: The table reports the unemployment duration elasticity with respect to each CARES Act provision. The upper panel assumes the other two provisions are not in effect and the lower panel that the other two provisions are in effect. The duration elasticity is calculated using simulated data from the transition experiment.

pact of the CARES Act turns out to be large due to the sheer size of the UI programs as well as their joint implementation.

7 Conclusion

Our paper asks if the limited effects of the pandemic UI benefits on employment, as estimated by several studies (e.g., Coombs et al. (2021); Marinescu, Skandalis, and Zhao, 2021; Ganong et al. (2024); and others), arise from small disincentive effects or represent a mix of disincentive and stimulative effects acting in opposite ways?

Based on high-frequency data on small restaurants and retailers, we find that employment in low-wage businesses recovers significantly slower than employment in neighboring high-wage businesses in labor markets with larger differences in UI replacement rates. Our research design controls for the local stimulative effects of UI programs by comparing neighboring businesses that largely share the positive effects of the UI stimulus. Our results are robust to a variety of controls including customer traffic and spending measured at the business level.

A search and matching model augmented to include a probability of losing UI eligibility upon job refusal can replicate the disincentive effects we estimate from the data. According to the model, the disincentive effects of pandemic UI programs held back the employment recovery by 3.4 percentage points, on average, between April and December 2020. We find that expanded eligibility of UI benefits is the most disruptive provision and disproportionately decreases employment in low-wage businesses.

We conclude that the disincentive effects of pandemic UI programs are sizable and the limited effects typically estimated in the literature likely reflect a mix of disincentive and stimulative effects.

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Appendix: For Online Publication

A Details about Homebase data and representativeness

In this appendix, we provide additional details about the Homebase (HB) data and assess its representativeness. For an even more detailed analysis, see Kurmann, Lalé, and Ta (2021).

A.1 Details about HB data

The HB data consist of anonymized daily records of individual hours worked and wages of employees, linked longitudinally to the establishment where they work and the firm that owns the establishment (although almost all firms are single-establishment firms). The data is recorded in real time and is used by many of the businesses for payroll processing. The majority of establishments are in sectors associated with in-person services: retail trade (NAICS 44-45), education and health (NAICS 61-62), leisure and hospitality (NAICS 71-72), and other services (NAICS 81).

The raw HB data contain about 140,000 unique establishments (called businesses in the main text and below) between January 2019 and December 2020. To assess the representativeness of the HB data, we distinguish between the "full sample" and the "baseline sample". The full sample consists of all businesses that are active for at least three consecutive weeks with at least 40 weekly tracked hours across its employees, and for which we can assign a consistent NAICS code by matching them by name and address to Safegraph's Places of Interest (POI) database (see Kurmann, Lalé, and Ta (2021) for details). The baseline sample is a subset of this full sample and consists of businesses that are present in the sample all of 2019 and at the end of 2020, report consistent wage information for their employees, and have at least one neighboring business in the same local industry cell (see the main text for details).

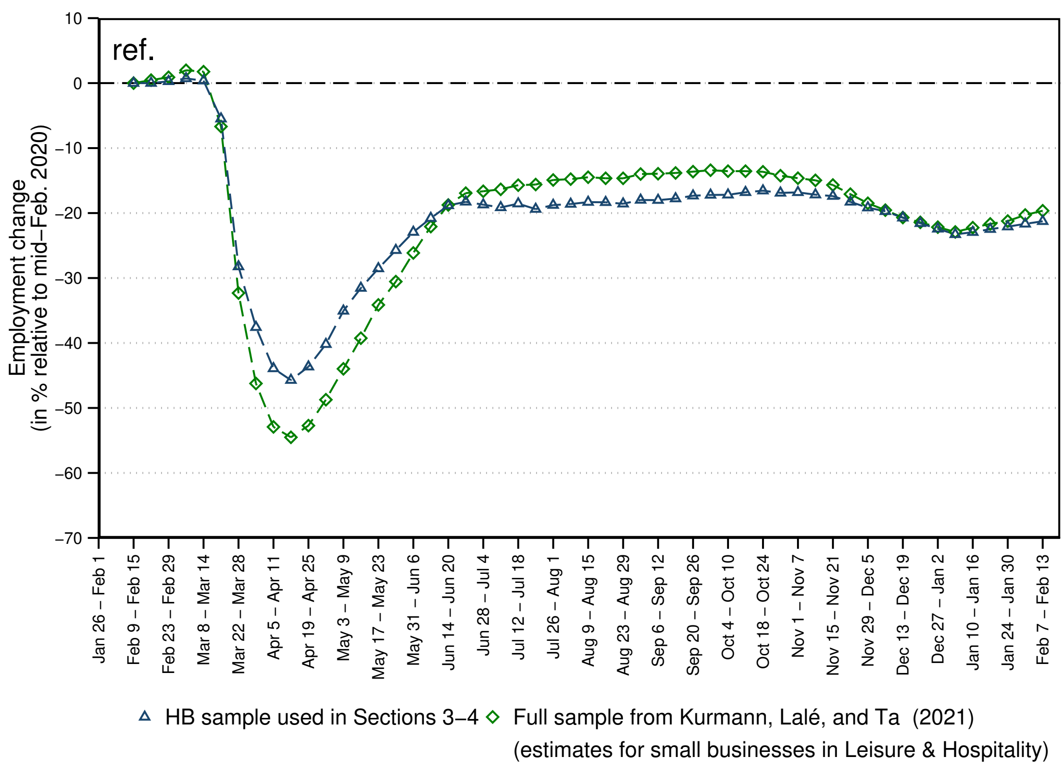
The full sample consists of over 51,000 businesses with over 500,000 unique employees, covering 2377 counties with a total population of over 300 million. The baseline sample consists of 4,595 businesses with over 80,000 unique employees, covering 527 counties with a total population of about 220 million. As these statistics suggest, essentially all businesses are small, employing fewer than 50 workers.

A.2 Representativeness concerning employment dynamics

As documented in Kurmann, Lalé, and Ta (2021), the full sample provides a weekly estimate of small business employment for in-person industries in the U.S. that fits administrative data very closely. Here, we explore the extent to which the employment dynamics in the baseline sample fit the employment dynamics in the full sample. To do so, we compute weekly employment relative to mid-February 2020 with each sample using the recursive weighted estimator proposed by Kurmann, Lalé, and Ta (2021).[20]

Figure A-1 shows the resulting employment estimates for 2020 in leisure and hospitality, the sector accounting for over 80 percent of all businesses in the baseline sample. Results are similar for the other sectors. Overall the fit is very good. While the decline in employment during the first weeks of the pandemic is somewhat larger in the full sample than in the baseline sample, the difference is well within reasonable range compared to other estimates (e.g., Cajner et al. (2020)). From June 2020 onward, the two estimates then move closely together.

Figure A-1: Comparison of HB Employment Dynamics: HB Full Sample versus Baseline Sample



Notes: The figure shows the estimated employment change for small businesses in the leisure and hospitality sector as implied by the full HB sample versus the baseline sample. See text for details.

[20] The full sample contains businesses that disappear entirely from HB, some of which close permanently and some of which simply stopped using the HB service. For an estimate of total employment, it is important to distinguish between the two, Kurmann, Lalé, and Ta (2021). No such adjustment is needed for the baseline sample since it contains only continuing businesses, that is, they can be temporarily closed in 2020 but must reopen before the end of 2020.

A.3 Representativeness concerning wages, hours, and earnings

The other important dimension in which to assess the representativeness of the HB baseline sample is concerning wages, hours worked, and earnings of workers. We use the monthly Outgoing Rotation Group (ORG) files from the Current Population Statistics (CPS) survey to perform this assessment. Since the HB wage and hours data pertain to employees reporting actual hours worked, we retain CPS data for hourly-paid workers only, and we drop all observations with either imputed hours or imputed wages. For sample size reasons, we do not restrict the CPS to workers who respond that they are employed in small businesses.

In the CPS-ORG files, hourly-paid workers provide information on actual hours worked, the hourly wage rate, and weekly overtime, tips, and commissions (OTC) *for the main job*. We compute the average hourly wage and average weekly hours over all observations in 2019, using the regular sampling weights. Average weekly earnings are then computed as average hourly wage times average weekly hours. To compute the average hourly wage with OTC, we divide the sum of average weekly earnings plus weekly OTC with average weekly hours.

In the HB data, we have daily data on the hourly wage and hours worked for each employee. Closer inspection of the data reveals that a substantial fraction of employees average fewer than 20 hours of work per week, either because this is not their main job or because they are part-time workers/work irregularly (i.e., full-time for a few days and then not at all for a few days). While interesting, this means that average weekly hours from the HB data are not directly comparable to the CPS data, which only contains data for the main job. For the below comparison, we restrict the HB sample to employees who work at least 3 days in a given week and business.[21] With this restriction in place, we sum the hours of all remaining workers to the weekly level and then, as in the CPS, compute average hourly wages and average weekly hours across all observations in 2019. As above, we provide results for the leisure and hospitality sector, both because this is the sector where most of the HB businesses are active and because this is where the CPS sample is largest. Results would be similar for the other sectors for which we have data. Table A-1 reports the results. As the first two columns show, the average hourly wage in the CPS is about $1.5 higher than in the HB sample, while average weekly hours are essentially the same. Hence average weekly earnings in the CPS are somewhat higher than in the HB sample. Given that the CPS contains hourly-paid workers not only from small businesses but also larger businesses that pay higher wages on average, this difference should not come as a surprise. We conclude that the HB baseline sample is therefore also broadly representative of small businesses for in-person industries with regard to wages, hours, and earnings.

A potential caveat to this conclusion is that hourly wages recorded by HB may not include OTC, either because they are paid out intermittently and not reported, or because businesses generally only record the base wage rate. This is particularly relevant for the leisure and hospitality sector,

[21] This restriction only affects average weekly hours worked. Average hourly wage rates are very similar with or without this restriction.

Table A-1: Comparison of Wages, Hours, and Earnings: CPS vs HB Estimation Sample

	CPS	HB	CPS w/OTC	HB w/OTC
Hourly wage ($)	12.5	10.9	14.8	13.6
Weekly hours	30.6	30.1	30.6	30.1
Weekly earnings ($)	400.1	328.1	467.2	415.96
# Observations	10,508	1,067,934	10,508	1,067,934

Notes: This table reports average hourly wages, average weekly hours, and average weekly earnings for hourly-paid workers in the leisure and hospitality sector in the CPS and in the HB baseline sample. For the CPS, only data for the main job is considered. For HB, only workers with at least 3 days of work in a given week and business are considered. Averages are computed over all observations in 2019 (monthly for the CPS, weekly for HB). The first two columns report hourly wage and weekly earnings statistics without overtime, tips, and commissions (OTC). The last two columns report hourly wage and weekly earnings statistics with OTC. See text for details.

which accounts for the majority of the HB sample. As the third column of Table A-1 shows, average hourly wages in the CPS are indeed substantially higher than HB hourly wages once OTC is included. This raises two related questions. First, how to adjust the HB wage data for OTC when computing UI replacement rates? Second, do businesses identified as low- vs high-wage differ systematically in the extent to which they report OTC, and therefore, is our identification of low- and high-wage businesses robust to including OTC?

To address these questions, we use information from the CPS. In the ORGs, the reported hourly wage rate for hourly-paid workers excludes OTC. As a first step, we therefore consider the relationship between the hourly wage rate and hourly OTC (i.e., reported weekly OTC divided by actual hours worked). There is a clear kink in this relationship around the federal minimum wage of $7.25/hour: for workers with an hourly wage rate below $7.25, there is an approximately 1:1 negative relationship with hourly OTC, whereas, for workers with an hourly wage rate above $7.25, hourly OTC is constant around $2 on average. This difference is consistent with federal minimum wage law, which mandates that for tipped workers, the minimum base wage is $2.13/hour as long as tips and base wage add up to $7.25/hour or more.

Given this finding, we adjust individual wage rates in the HB data with an OTC amount estimated from the CPS data. Specifically, we run the following regression on CPS hourly-paid workers in Leisure & Hospitality, separately for those with hourly wages below $7.25 and for those with hourly wages at or above $7.25:

$$otc_{ij} = a_{0j} + a_{1j} w_{ij} + \varepsilon_{ij}, \tag{29}$$

where otc_{ij} and w_{ij} denote, respectively, the hourly OTC amount and the hourly wage rate of worker

i in region j.[22] We then add the estimated OTC amount, $\hat{otc}_{ij} = \hat{a}_{0j} + \hat{a}_{1j} w_{ij}$, to the hourly wage of each worker in our HB sample.

As the fourth column of Table A-1 shows, the OTC-adjusted HB hourly wage is on average \$2.30 higher than the baseline HB hourly wage. This difference is the same as for the CPS, which is not surprising since the OTC adjustment is inferred from CPS data.

B Cross-sectional statistics

Employment, Hours, and Wages Table B-2 shows the cross-section of key labor market variables for 2019. The statistics are computed when the businesses are in operation. The table shows that HB data covers small businesses. On some days, a significant fraction of businesses operate with less than five employees.

Table B-2: Employment, Hours, and Wage in the Cross-section

	Mean	SD	p(5)	p(10)	p(25)	p(50)	p(75)	p(90)
# Employees	12.8	9.1	3	4	7	11	16	24
Hours per worker	4.9	1.5	2.6	3.1	3.9	4.8	5.8	6.8
Hourly wage (\$)	10.9	2.7	6.6	7.6	9.1	11	12.7	14.2
Residual log-hourly wage (\$)	0.0	0.11	-0.19	-0.13	-0.06	0.0	0.06	0.13
Separation rate (%)	9.5	14.0	2.7	3.3	4.7	7.1	11.1	16.6
Hiring rate (%)	12.4	10.0	3.5	4.3	6.2	10.0	15.3	23.0

Replacement rates To conduct our empirical analysis we compute a replacement rate at the business level. The procedure to calculate business-level replacement rates is as follows:

1. The average daily hourly wage of business j in cell c in the base period January-February 2020 is w_{j,c,t_0}. We multiply this wage by 7 hours times 5 days times 13 (52) weeks to obtain a quarterly (annual) measure of business-level earnings, w^q_{j,c,t_0}.
2. We take the average of quarterly or annual earnings in cell c denoted as $\overline{w}^q_{c,t_0}$.
3. We combine $\overline{w}^q_{c,t_0}$ with state-level formulas for unemployment insurance income to derive the weekly amount of UI that an unemployed in cell c receives, denoted $b(\overline{w}_{c,t_0})$.
4. $b(\overline{w}_{c,t_0}) + S_t$ is the total weekly amount of UI that an unemployed in cell c receives during week t of the pandemic, where $S_t = \{\$0, \$300, \$600\}$.

[22] We define regions by micropolitan and metropolitan statistical areas. To increase the sample size for this regression, we use CPS-ORG data for 2018 and 2019. For regions with less than 10 observations, we use state-level averages.

5. We divide the total weekly supplement $b(\overline{w}_{c,t_0}) + S_t$ by the weekly earnings in business j, equal to $w_{j,c,t_0} \times 7 \times 5$. This gives us the business-replacement rate $R_{j,c,t}$ which measures the amount of UI an unemployed would receive in labor market c and period t relative to what he/she can receive by being employed in business j.

Table B-3 reports summary statistics for the cross-sectional distribution of the business-level replacement rate $R_{j,c,t}$ and the replacement rate gap $\Delta R_{c,t}$ which is calculated as the average replacement rate in low-wage businesses of cell c minus the average replacement rate in high-wage businesses of cell c. In addition, we report the distribution of the difference in the replacement rate gap across labor markets, after and before the expiration of the income supplement ($\Delta R_{post} - \Delta R_{pre}$).

In normal times (i.e., during 2019), the mean replacement rate is 54 percent and the median is 51 percent. During the CARES Act (specifically, between April 2020 and June 2020 where the additional \$600 is in effect), the average replacement rate increased to 2.23 and the median to 2.14. In normal times the replacement rate gap is on average 0.13, with a median of 0.08. With the CARES Act, the replacement rate gap increases on average to 0.61 and for the median labor market to 0.34. Finally, on average, the replacement rate drops by 46 percentage points after the expiration of the income supplement.

How do the replacement rate R and the replacement rate gap ΔR vary across labor markets and over time? The left panel of Figure B-2 shows the average time series path of R and ΔR which move together as they are both influenced by the pandemic income supplement S_t. In the right panel, we plot a binscatter of $R_{c,t}$ and $\Delta R_{c,t}$ across labor markets and weeks. Labor markets that have a higher difference in the replacement gap also experience higher average UI replacement rates.

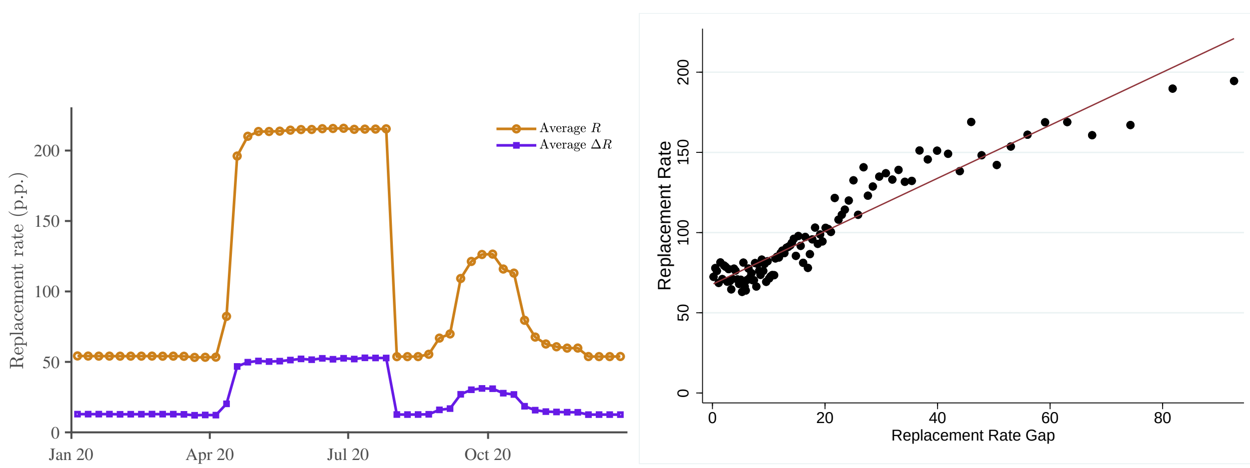
B.1 Recalls in HB businesses

Fujita and Moscarini (2017) document that a large share of workers returns to their previous employer after a jobless spell. We focus on businesses that closed and re-opened and examine if, upon re-opening, the HB businesses disproportionately hired employees who previously worked in the business. In Figure B-3 we plot the recall rate (the share of employees that have previously worked in the business) before and after the business' closing and re-opening. We center the plots around re-opening week (denoted as "week 0"). Week -1, depicted as a shaded area in the plots, represents the time businesses were closed. Recall rates are high only during the first month of re-opening and then converge to pre-pandemic levels. In addition, high-wage businesses have higher recall rates than low-wage businesses but the difference also disappears after the first month. In sum, the recall margin does not seem to play an important role for our HB sample. Our findings are in accord with Ganong et al. (2024) who document that UI supplements did not have substantial effects on recalls. Our benchmark model abstracts from a recall margin but we expand the baseline model to include recalls in Appendix G and discuss the results.

Table B-3: Replacement Rate Distribution

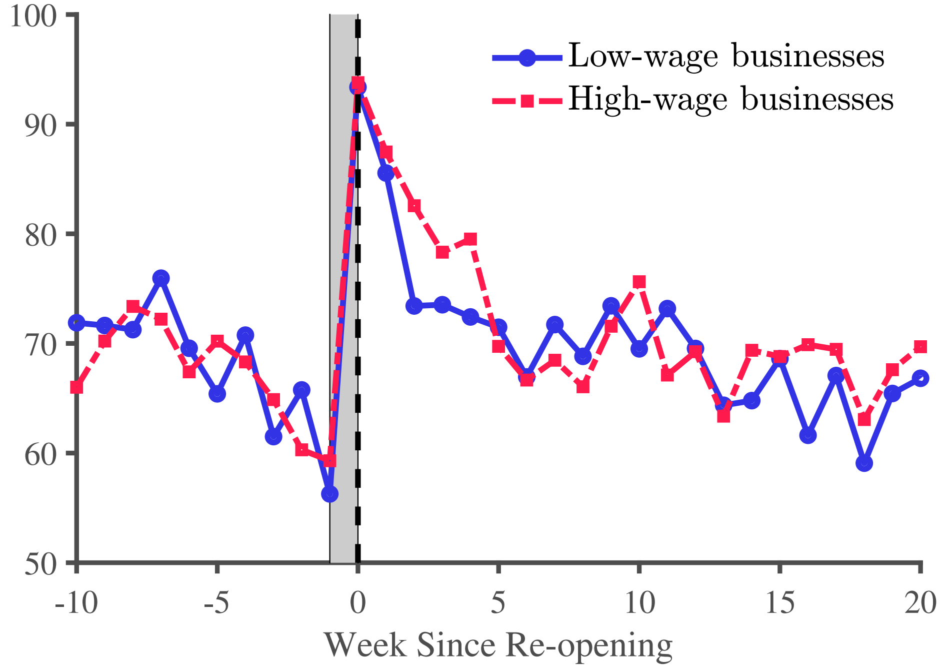
	Mean	SD	p(5)	p(10)	p(25)	p(50)	p(75)	p(90)
Replacement rate (R)								
Normal Times	0.54	0.09	0.48	0.50	0.50	0.51	0.54	0.62
CARES Act	2.23	0.54	1.61	1.73	1.86	2.14	2.47	2.83
Replacement rate gap (ΔR)								
Normal Times	0.13	0.16	0.01	0.02	0.04	0.08	0.16	0.28
CARES Act	0.61	0.86	0.03	0.08	0.19	0.34	0.68	1.21
Decline in replacement								
rate gap ($\Delta R_{post} - \Delta R_{pre}$)	-0.46	0.62	-1.51	-0.94	-0.51	-0.26	-0.14	-0.06

Figure B-2: Replacement Rate R and Replacement Rate Gap ΔR



Notes: The left panel shows the time series of the average replacement rate R in the labor market and the average difference (gap) in replacement rate between the low- and high-wage business of the labor market, ΔR. The right panel shows a binscatter of R and ΔR across labor markets and weeks. The plot is restricted to gaps lower than 100 percent.

Figure B-3: Recall Rates for Low- and High-Wage Businesses Upon Re-opening



Notes: Weekly averages for recall rates. Week "0" is re-opening week. Closing week and closing spell differs across businesses (represented by the gray area).

C Robustness results

We report the estimated coefficients from regression:

$$y_{j,c,t} = a_j + \tau_t + \theta_0 \text{Demand}_{c,t} + \theta_1 \text{Demand}_{c,t} \times \mathbb{1}_{\{\text{high} w_j\}} + \varepsilon_{j,c,t}$$

where $y_{j,c,t}$ is the employment of business j in cell c during week t and demand is either total customer visits or total spending in cell c and week t both normalized relative to their base period of January-February 2019.

A business is classified as low- or high-wage based on the 2020 base period as in the baseline specification. We find that θ_1 is not significantly different from zero for either demand measure. Thus, low- and high-wage businesses in the same location, industry, and price range do not respond differentially to a local demand shift.

Table C-5 reports estimates for the various robustness exercises described in Section 4. In all exercises we report the interaction of the replacement rate gap decline with the weekly dummy estimate 12 weeks after the expiration. First, we analyze the response of hours per employee and hourly wages around the window of expiration. Second, we restrict the sample to the businesses that continuously operated during the pandemic. Third, we add business quality in the set of time-varying controls. Fourth, we examine the estimates for an expanded sample of businesses. Specifically, we

Table C-4: Demand Effects on the Employment Gap in 2019

	Customer visits	Customer spending
θ_0	0.198*	0.015
	(0.114)	(0.010)
θ_1	−0.125	−0.003
	(0.111)	(0.011)

Notes: Estimates effects of demand conditions for the employment gap in 2019.

keep businesses for which we could not find information on their price from Yelp. Fifth, we measure business' employment including salaried workers. Sixth, we choose a wider base period of July 2019-February 2020 and finally, we weigh each cell by the number of businesses inside the cell; i.e., more populated cells take a higher weight.

D Definition of equilibrium transition path

We describe the equilibrium along the transition path. Given the initial job separation at $t = 0$ and a path for the UI policy parameters $\{b_{R,t}, p_{R,t}, p_{N,t}\}_{t=0}^{\infty}$ a transitional dynamics equilibrium is a sequence of value functions $\{W_t, U_{R,t}, U_{N,t}\}_{t=0}^{\infty}$, reservation wages $\{w^*_{R,t}, w^*_{N,t}, w^*_{\min,t}\}_{t=0}^{\infty}$, job acceptance rate $\{F_t\}_{t=0}^{\infty}$, distributions $\{e_t, u_{R,t}, u_{N,t}, u_t, v_t, \tilde{v}_t\}_{t=0}^{\infty}$, and market tightness $\{\theta_t\}_{t=0}^{\infty}$ such that:

1. The value functions $\{W_t, U_{R,t}, U_{N,t}\}_{t=0}^{\infty}$ satisfy equation (7), equation (8), and equation (9), for all t.

2. The reservation wages $\{w^*_{R,t}, w^*_{N,t}, w^*_{\min,t}\}_{t=0}^{\infty}$, satisfy equation (10), and equation (12), for all t.

3. The (un)employment and vacancy distributions $\{e_t, u_{R,t}, u_{N,t}, u_t, v_t, \tilde{v}_t\}_{t=0}^{\infty}$ satisfy equation (13), equation (14), equation (15), equation (16), equation (19), and equation (20) for all t.

4. The acceptance rate $\{F_t(w)\}_{t=0}^{\infty}$ satisfy equation (21), for all t.

5. Market tightness $\{\theta_t\}_{t=0}^{\infty}$ is given by $\tilde{v}_t/u_t$, for all t

E Calibration of recipiency rate

In our calibration, we adjusted UI recipiency rates in the March CPS data by adapting the procedure of Larrimore, Mortenson, and Splinter (2023) (LMS). The authors use CPS and IRS data

Table C-5: Alternative Empirical Specifications

$\Delta y_{c,t}$	Time dummy at week 12
Benchmark	-5.7***
	(1.6)
Hours per employee gap	-0.4
	(1.5)
Hourly wages gap	-0.7
	(0.9)
Continuously open businesses	-6.0***
	(1.7)
Adding business quality	-3.9**
	(1.8)
Expanding sample	-4.5***
	(1.4)
Including salaried workers	-5.4***
	(1.3)
Larger base period	-5.3***
	(1.7)
Weights	-5.4***
	(1.5)

Notes: Each line reports the estimates from regression (6) for an alternative specification (reported in percentage points). We winsorize variables at the one percent and cluster standard errors at the local industry level.

and document that there is substantial underreporting of UI recipiency (and benefit amounts) in the March CPS. They provide IRS estimates of UI recipiency by income centiles for each tax year from 1999 until 2021. We adapted their code to adjust UI recipiency rates in the March CPS data. Specifically, for each centile of the income distribution, LMS's procedure compares the raw number of UI recipiency in the CPS with that from the IRS data; if the latter is larger (which happens to be the case in almost every centile of the distribution), they randomly allocate the number of missing UI recipients so that the sum of the raw and imputed UI recipients in this centile matches the IRS's estimate. We perform a similar allocation, but prioritize the allocation of missing UI receipts to workers who have been working for at least part of the previous year and have been unemployed for at least one quarter. This adjustment is motivated by the fact that, within any centile, these individuals are more likely to have received UI benefits compared to any randomly selected individual.

F Calculation of duration elasticities

We measure the unemployment duration elasticity over a policy change x along the transition. We consider three policy changes: (i) the income supplement where x denotes dollar amounts, (ii) the maximum duration of benefits where x denotes weeks, and (iii) the set of eligible workers for UI where x is a probability.

To calculate the unemployment duration elasticity, let us first define survival functions for workers who receive UI benefits at the beginning of t. $\xi_{i,t+\tau}(w)$ is the probability that these workers are still unemployed by the end of time $t+\tau$ and at that point their status is $i \in \{R, N\}$. In the baseline model, the survival function $\xi_{R,t+\tau}(w)$ satisfies the relation:

$$\xi_{R,t+\tau}(w) = \xi_{R,t+\tau-1}(w)\left(1 - f_{R,t+\tau-1}(w)\right)\left(1 - p_{N,t+\tau}\right), \quad \tau = 1, 2, \ldots,$$

starting from

$$\xi_{R,t}(w) = 1 - p_{N,t}.$$

For the survival function $\xi_{N,t+\tau}(w)$, we can exploit the following mutually exclusive events. For a worker who started unemployment as R in t, and is N at the end of $t+\tau$, it must be that she was either (i) R at the end of $t+\tau-1$ and became N during period $t+\tau$, or (ii) already N at the end of $t+\tau-1$. As a result, we have

$$\xi_{N,t+\tau}(w) = \xi_{R,t+\tau-1}(w)\left(1 - f_{R,t+\tau-1}(w)\right)p_{N,t+\tau} + \xi_{N,t+\tau-1}(w)\left(1 - f_{N,t+\tau}(w)\right), \quad \tau = 1, 2, \ldots$$

starting from

$$\xi_{N,t}(w) = p_{N,t}.$$

Putting it all together, the expected unemployment duration of an unemployed who is in state R (that is to say receiving UI benefits) at t and with previous wage w is:

$$ED_{R,t}(w) = \sum_{\tau=0}^{\infty}\Big[\xi_{R,t+\tau}(w)\, f_{R,t+\tau}(w) + \xi_{N,t+\tau}(w)\, f_{N,t+\tau}(w)\Big] \times (\tau + 1). \tag{30}$$

Next, we compute the expected unemployment duration of an unemployed worker who is in state N in period t. We define the following survival function

$$\zeta_{t+\tau}(w) = \zeta_{t+\tau-1}(w)\left(1 - f_{N,t+\tau-1}(w)\right), \quad \tau = 1, 2, \ldots$$

which allows us to compute

$$ED_{N,t}(w) = \sum_{\tau=0}^{\infty}\zeta_{t+\tau}(w)\, f_{N,t+\tau}(w) \times (\tau + 1). \tag{31}$$

Let $\varepsilon_{i,t}^{U}$ denote the unemployment duration elasticity arising from a policy change from x to $\tilde{x}$, for workers in unemployment state $i \in \{R, N\}$. We have:

$$\varepsilon_{i,t}^{U}(w) = \frac{\frac{\tilde{ED}_{i,t}(w)}{ED_{i,t}(w)} - 1}{\frac{\tilde{x}}{x} - 1}. \tag{32}$$

In the final step, we aggregate across all unemployed workers:

$$\varepsilon_t^U = \frac{1}{u_t} \int \left(u_{R,t}(w)\, \varepsilon_{R,t}^U(w) + u_{N,t}(w)\, \varepsilon_{N,t}^U(w) \right) dw \tag{33}$$

G A Model with Recalls

We expand the baseline model by adding recall unemployment. As in the baseline model, workers are separated from their employer with some probability (denoted here δ^s). But we also allow the separated workers to be in touch with their previous employers with a probability $1-\delta^p$. Unemployed workers who are in touch with their old employers are indexed as $i = 1$ and unemployed workers who are not are indexed as $i = 0$. Unemployed who are in touch with their previous employers can be recalled to their old job with some probability r.

We assume that upon separation, the unemployed keep in touch with their previous employers. As a result, the value function for an employed worker is:

$$W_t(w) = \mathcal{U}(w) + \beta \left[(1-\delta^s) W_{t+1}(w) + \delta^s \left((1-p_{R,t}) U_{N,t+1}^1(w) + p_{R,t} U_{R,t+1}^1(w) \right) \right]$$

where $U_{R,t+1}^1$ and $U_{N,t+1}^1$ are the value functions for an unemployed with and without UI benefits, respectively, that is still in touch with the employer. The value function for an unemployed worker receiving UI, and who is in touch with the employer is given by:

$$\begin{aligned} U_{R,t}^1(w) = \delta^p U_{R,t}^0(w) + (1-\delta^p) \Bigg[& r \left(\mathcal{U}(b_{R,t}(w)) + \beta \max\left\{ W_{t+1}(w), U_{N,t+1}^0(w) \right\} \right) \\ & + (1-r) \Bigg[\mathcal{U}(b_{R,t}(w)) + \beta \Bigg[p_{N,t} \Bigg((1-\lambda(\theta_t)) U_{N,t+1}^1(w) \\ & + \lambda(\theta_t) \int \max\left\{ W_{t+1}(x), U_{N,t+1}^1(w) \right\} \frac{\tilde{v}_t(x)}{\tilde{v}_t} dx \Bigg) + (1-p_{N,t}) \Bigg((1-\lambda(\theta_t)) U_{R,t+1}^1(w) \\ & + \lambda(\theta_t) \int \max\left\{ W_{t+1}(x), U_{R,t+1}^1(w) \right\} \frac{\tilde{v}_t(x)}{\tilde{v}_t} dx \Bigg) \Bigg] \Bigg] \Bigg], \end{aligned}$$

Notice that at time period t, with probability δ^p, an unemployed loses touch with the employer and receives $U_{R,t}^0$, which is defined below. With probability $1-\delta^p$ an unemployed stays in touch with the employer in which case two possibilities arise. First, with probability r an unemployed can be recalled to the previous job. In this case, the unemployed receives at t the unemployment benefit and the next period makes a choice between working at the old employer $W_{t+1}(w)$ or continuing to be unemployed. The basic assumption in this formulation is that UI is denied for an unemployed who has been contacted by the former employer and refused to return to work. Second, with probability $1-r$, an unemployed is not recalled to the old job. In this case, as in the baseline model, an unemployed loses the UI benefits only if they expire which occurs with probability p_N.

The value function for an unemployed worker receiving UI and who is not in touch with the previous employer ($i=0$) is similar to the baseline model:

$$U^0_{R,t}(w) = \mathcal{U}(b_{R,t}(w)) + \beta\Bigg[p_{N,t}\Bigg((1-\lambda(\theta_t))U^0_{N,t+1}(w) + \lambda(\theta_t)\int \max\left\{W_{t+1}(x), U^0_{N,t+1}(w)\right\}\frac{\tilde{v}_t(x)}{\tilde{v}_t}dx\Bigg) + (1-p_{N,t})\Bigg((1-\lambda(\theta_t))U^0_{R,t+1}(w) + \lambda(\theta_t)\int \max\left\{W_{t+1}(x), U^0_{R,t+1}(w)\right\}\frac{\tilde{v}_t(x)}{\tilde{v}_t}dx\Bigg)\Bigg],$$

We report below the value functions of unemployed workers in social assistance, when they are in touch and when they are not in touch with their previous employer:

$$U^1_{N,t}(w) = \delta^p U^0_{N,t}(w) + (1-\delta^p)\Bigg[r\left(\mathcal{U}(b_N) + \beta\max\left\{W_{t+1}(w), U^0_{N,t+1}(w)\right\}\right) + (1-r)\Bigg[\mathcal{U}(b_N) + \beta\Big((1-\lambda(\theta_t))U^1_{N,t+1}(w) + \lambda(\theta_t)\int \max\left\{W_{t+1}(x), U^1_{N,t+1}(w)\right\}\frac{\tilde{v}_t(x)}{\tilde{v}_t}dx\Bigg]\Bigg],$$

$$U^0_{N,t}(w) = \mathcal{U}(b_N) + \beta\left((1-\lambda(\theta_t))U^0_{N,t+1}(w) + \lambda(\theta_t)\int \max\left\{W_{t+1}(x), U^0_{N,t+1}(w)\right\}\frac{\tilde{v}_t(x)}{\tilde{v}_t}dx\right).$$

Let $A(w,w')$ be an indicator that takes the value of 1 if an unemployed worker whose previous job paid w accepts an offer for a job that pays w'. Notations become very cumbersome if we define reservation wages as in the baseline model, so let us instead work with the following indicators for the decision to accept a job:

$$\mathcal{A}^i_{R,t}(w,w') = \mathbb{1}\left\{W_{t+1}(w') > U^i_{R,t+1}(w)\right\}$$

$$\mathcal{A}^i_{N,t}(w,w') = \mathbb{1}\left\{W_{t+1}(w') > U^i_{N,t+1}(w)\right\}$$

Note that as in the baseline model, these definitions for time t refer to decisions made in $t+1$.

The law of motion for employment is similar to the baseline with an additional inflow of recalled workers who accept their old job at wage w:

$$e_{t+1}(w) = (1-\delta^s)\, e_t(w) + (1-\delta^p)\, r\mathcal{A}^0_{N,t}(w,w)\left(u^1_{N,t}(w) + u^1_{R,t}(w)\right) + \lambda(\theta_t)\frac{\tilde{v}_t(w)}{\tilde{v}_t} F_t(w)\, \bar{u}_t \tag{34}$$

$F_t(w)$, the probability that a job offer w gets accepted, is

$$\begin{aligned} F_t(w) = \frac{1}{\tilde{u}_t}\Bigg(&\int (1-p_{N,t})\left(\delta^p u^1_{R,t}(x) + u^0_{R,t}(x)\right)\mathcal{A}^0_{R,t}(x,w)\,dx \\ &+ \int (1-p_{N,t})(1-\delta^p)(1-r)\, u^1_{R,t}(x)\,\mathcal{A}^1_{R,t}(x,w)\,dx \\ &+ \int \left(\left(\delta^p u^1_{N,t}(x) + u^0_{N,t}(x)\right) + p_{N,t}\left(\delta^p u^1_{R,t}(x) + u^0_{R,t}(x)\right)\right)\mathcal{A}^0_{N,t}(x,w)\,dx \\ &+ \int (1-\delta^p)(1-r)\, u^1_{N,t}(x)\,\mathcal{A}^1_{N,t}(x,w)\,dx\Bigg), \end{aligned}$$

The transition equations for unemployment are given by:

$$\begin{aligned} u^1_{R,t+1}(w) &= (1-\delta^p)\,\delta^s p_{R,t} e_t(w) \\ &\quad + \left(1 - \lambda(\theta_t)\int \mathcal{A}^1_{R,t}(w,x)\frac{\tilde{v}_t(x)}{\tilde{v}_t}dx\right)(1-p_{N,t})(1-r)(1-\delta^p)\, u^1_{R,t}(w), \\ u^0_{R,t+1}(w) &= \delta^p\delta^s p_{R,t} e_t(w) \\ &\quad + \left(1 - \lambda(\theta_t)\int \mathcal{A}^0_{R,t}(w,x)\frac{\tilde{v}_t(x)}{\tilde{v}_t}dx\right)(1-p_{N,t})\left(\delta^p u^1_{R,t}(w) + u^0_{R,t}(w)\right), \\ u^1_{N,t+1}(w) &= (1-\delta^p)\,\delta^s(1-p_{R,t})\, e_t(w) \\ &\quad + \left(1 - \lambda(\theta_t)\int \mathcal{A}^1_{N,t}(w,x)\frac{\tilde{v}_t(x)}{\tilde{v}_t}dx\right)(1-r)(1-\delta^p)\left(u^1_{N,t}(w) + p_{N,t}u^1_{R,t}(w)\right), \\ u^0_{N,t+1}(w) &= \delta^p\delta^s(1-p_{R,t})\, e_t(w) + (1-\delta^p)\, r\left(1-\mathcal{A}^0_{N,t}(w,w)\right)\left(u^1_{N,t}(w) + u^1_{R,t}(w)\right) \\ &\quad + \left(1 - \lambda(\theta_t)\int \mathcal{A}^0_{N,t}(w,x)\frac{\tilde{v}_t(x)}{\tilde{v}_t}dx\right)\left(\left(\delta^p u^1_{N,t}(x) + u^0_{N,t}(x)\right) + p_{N,t}\left(\delta^p u^1_{R,t}(x) + u^0_{R,t}(x)\right)\right), \end{aligned}$$

with $\tilde{u}_t = \int \left((1-(1-\delta^p)\, r)\left(u^1_{N,t}(x) + u^1_{R,t}(x)\right) + u_{N,t}(x) + u_{R,t}(x)\right)dx$ and $\tilde{v}_t = \int \tilde{v}_t(x)\,dx$. We have the assumption of a fixed number of jobs as in the baseline:

$$e_t(w) + u^1_{N,t}(w) + u^1_{R,t}(w) + v_t(w) = g(w)\, M. \tag{35}$$

Labor market tightness is given by:

$$\theta_t = \frac{\tilde{v}_t}{\tilde{u}_t} \tag{36}$$

We set the weekly job separation rate as $\delta^s = 0.016$ as in the baseline. The matching technology parameter $\kappa = 0.15$ is set to match a job finding rate equal to 0.23. Parameter $\delta^p = 0.17$ matches a recall share of hires equal to 33 percent, consistent with our data. Given that the average duration of recalls in HB data is about 4 weeks, we set $r = 0.23$, i.e. one-fourth of a month.

The effective probability of losing UI upon job refusal in the model with recall, i.e., the probability a worker stays in touch with the employer and that the employer recalls the worker back to work, is $(1 - \delta^p)r = 0.19$. In the reduced-form model in the main text, the probability of losing UI upon job refusal is equal to $\chi = 0.165$ in order to match the employment recovery gap we documented in the data. Thus, our reduced-form parameterization is reasonably close to the parameter values in a model with recall unemployment. Our parameterized recall model generates similar dynamics around the expiration of the income supplement the baseline.