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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 local 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 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 4.7 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 arising from 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 modestly 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 adds one more difference by considering the employment recovery gap between low-wage and high-wage businesses within the *same* local labor market and then applies a standard fixed effect estimator to these gaps. By comparing low- and high-wage businesses we difference out common demand shifts arising from the UI stimulus and construct a measure of the disincentive effects of UI benefits only. The basic identifying assumption to eliminate the bias from the confounding demand effects is that neighboring low- and high-wage businesses share the positive demand effects of the UI stimulus. Our research strategy 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.

Our identifying assumption is more likely to hold the more local the labor market and the more homogeneous the businesses in that market. To that end, 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, such as restaurants and retail businesses (Kurmann, Lalé, and Ta, 2021).[1] The data provides us with a panel of businesses with daily data on employment, the hourly wage, hours worked by each employee, business zip code, and industry affiliation. In addition, we match each business with indicators of quality and price from Yelp and daily customer visits based on cell phone tracking data from Safegraph.

We first establish a link between the pandemic UI benefits and the employment recovery gap using an event study approach. We document that while the income supplement of \$600 per week under the CARES Act is in effect, the employment recovery in low-wage businesses significantly lags the employment recovery in neighboring high-wage businesses. But upon expiration of the income supplement of \$600, in the summer of 2020, employment in low-wage businesses rapidly gains ground and nearly closes the employment recovery gap with their high-wage neighbors.

Second, we analyze how the employment recovery gap varies with the relative generosity of the pandemic income supplement of \$600. According to our estimates, for the median labor market, the rise in the replacement rates from the income supplement of \$600 held back the employment recovery in low-wage businesses by 1.5 percentage points more than high-wage businesses.[2]

To quantify the overall impact of the CARES Act on employment recovery we build a labor search model. As long as high-wage businesses are also affected by the pandemic UI benefits—either directly or through general equilibrium effects—the employment recovery gap that we estimate may not represent the aggregate effect of UI on employment. The model allows us to construct a proper counterfactual which is the employment recovery gap in the absence of pandemic UI benefits. In addition, the model allows us 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.

[1] 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. For a more detailed discussion see Kurmann, Lalé, and Ta (2021).

[2] We also show that in labor markets with a higher replacement rate gap, hours per employee as well as hourly wages increase relatively faster in low-wage businesses which supports our arguments that our estimates capture differential labor supply constraints instead of differential demand or productivity effects.

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, constant over time, wage offers drawn from an exogenously given distribution. Unemployed workers randomly search for jobs that pay different wages and accept a job if the offered wage is higher than their reservation wage (McCall, 1970; Albrecht and Axell, 1984). UI benefits are a function of the worker's past wage so that 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 overstates the magnitude of the employment recovery gap by a large degree, driven mainly by the large decline in low-wage employment. What is surprising is that the expansion of the UI eligibility criteria alone has a bigger impact on the decline of low-wage employment than either the generous income supplement or the extended maximum duration of pandemic benefits on their own. Using data from the Current Population Survey (CPS) we document that 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.

We present a reduced-form solution that can reconcile the model with the data. We introduce a probability that workers lose their UI eligibility if they refuse a job offer. This assumption effectively reduces the outside option for workers and increases the acceptance rates for low-wage firms. With a 10 percent probability of losing benefits upon job refusal, we can match the estimated 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 only has 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 4.7 percentage points higher between April and December 2020. This employment loss represents around 25 percent of the average employment loss in the Leisure and Hospitality sector during the same period.[3]

Our paper contributes to the literature studying the labor market consequences of UI benefits by separating the disincentive from the stimulative effects of UI policies. Two papers that also make this distinction are Di Maggio and Kermani (2021) and Hellwig (2021). Both rely on a comparison in employment between non-tradable sectors — that are more affected by local demand conditions — versus tradable sectors (in the spirit of Mian and Sufi, 2014). 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]

Our paper also contributes to the literature studying the extent to which 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. (2022) 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

[3] The unemployment duration elasticities implied by the model are also modest and in line with the low-to-middle range of pre-pandemic estimates (e.g., Schmieder and von Wachter, 2016). The overall employment impact turns out to be large, however, due to the sheer size of the UI provisions as well as their joint implementation.

[4] Hagedorn et al. (2013) compare adjacent counties in different states that experience changes in UI for reasons tied to the state's performance and find that increasing the duration of benefits increased unemployment rates substantially. In contrast, Dieterle, Bartalotti, and Brummet (2020) and Boone et al. (2021) find, using a similar BCP research design but accounting for cross-border spillovers and a longer sample, respectively, that the negative effects of UI were small. Subsequent research that uses data on job applications and variation in the real-time measurement error of the unemployment rate also finds a limited effect of UI (e.g., Marinescu, 2017; Chodorow-Reich, Coglianese, and Karabarbounis, 2019). A BCP design is not well-suited to study the effects of pandemic supplements because these provisions occurred simultaneously across counties.

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 in the absence of pandemic benefits, employment would have been modestly higher. We argue that the limited effects estimated in the aforementioned papers likely represent a mix of disincentive effects, stimulative effects, and possible other confounding local demand shocks.

A notable study is Finamor and Scott (2021) who, like us, use Homebase data but use variation at a relatively broad geographical level, namely a state. They find that a higher replacement rate is not associated with a lower labor market re-entry of workers. When we conduct our research design at a similar broad level of geographical aggregation our estimates become small and insignificant which highlights 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. (2022)). 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.

2 Research design

Consider the following data-generating process

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

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. 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.

Our empirical design could equally be applied to workers instead of businesses. In such a case, variable $y_{j,c,t}$ would be some labor market indicator for worker j (e.g., employment status) in labor market c and period t, and $R_{j,c,t}$ would be the worker-specific replacement rate. The reason we focus on businesses instead of workers is that in our data (analyzed in detail in Section 3) workers typically work for short periods before permanently exiting the sample. Thus, we cannot guarantee a long balanced panel unless we analyze the employment recovery from the perspective of employers.

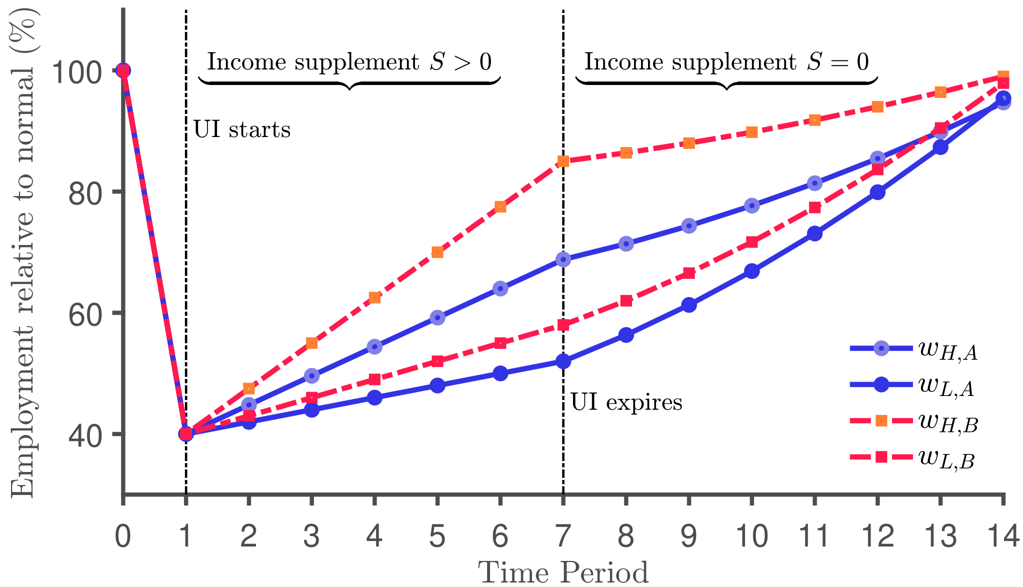
The coefficient β captures the negative impact of a higher replacement rate on business employment, i.e., it is a measure of the disincentive effect. Business employment is also affected by observable factors, $X_{j,c,t}$, and unobserved factors, $u_{j,c,t}$, that are potentially correlated with $R_{j,c,t}$. 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., productivity). 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 are at the same time more productive and therefore 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. In this example, there are two labor markets, A and B, each populated with two businesses (assumed to have the same productivity). Labor market A is populated by relatively high-wage businesses while labor market B is populated by relatively low-wage businesses.

As a consequence of the pandemic, businesses experience a large decline in their employ-

Figure 1: Example of Research Design



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

ment and gradually recover back to normal. With the same pandemic supplement S, on average, labor market B has higher replacement rates relative to labor market A. 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.[5] 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

[5]This is a relevant concern since Ganong et al. (2022) show that pandemic-UI recipients quickly spent a substantial portion of their benefits. Using county-level correlations, we also verify that the generosity of the $600 weekly supplement is positively correlated with changes in consumer spending at the beginning of the pandemic (see Appendix B).

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 in empirical Section 3, and also demonstrate in the simple example below, 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. Within labor markets, the business with a relatively high replacement rate has more difficulty recovering relative to the business with a relatively low replacement rate, and this difference is wider the larger 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 demand shifts that may be correlated with the generosity of the replacement rates (e.g., local demand shifts due to pandemic regulations or shifts in consumer preferences). At the same time, our strategy does not eliminate components of $u_{j,c,t}$ that are not common (e.g., productivity). But as long as the difference in productivity between neighboring low- and high-wage businesses remains constant over time, we can obtain an unbiased estimate by adding labor market fixed effects in regression (3). This is the main regression specification we analyze in the empirical analysis of Section 4. Finally, note that our research design identifies relative effects between neighboring businesses that do not necessarily represent aggregate effects. In Section 5 we rely on a structural model to translate our estimates into aggregate UI effects.

Example of research design We demonstrate more precisely our arguments using a simple example. Assume, as in Figure 1, that there are two labor markets, A and B, each populated with a low-wage and a high-wage business, $j = \{L, H\}$. The wage of business j in labor market c is given by $w_{j,c} = (1 + \varepsilon_j)\overline{w}_c$ where $\overline{w}_c$ is the average wage in the labor market and $\varepsilon_j \in \{-\epsilon, \epsilon\}$. Labor market A is populated by relatively high-wage businesses while labor market B is populated by relatively low-wage businesses, i.e., $\overline{w}_A > \overline{w}_B$. For simplicity, we assume that the dispersion ϵ is the same in both labor markets. Assuming a proportional pre-pandemic UI replacement rate ρ, the business-specific replacement rate is

$$R_{j,c,t} = \frac{\rho\overline{w}_c + S_t}{w_{j,c}} = \frac{\rho\overline{w}_c + S_t}{\overline{w}_c(1 + \varepsilon_j)} = \rho(1 + \varepsilon_j)^{-1}\left[1 + \frac{S_t}{\rho\overline{w}_c}\right]. \tag{4}$$

In addition, we assume for simplicity that $\mathbf{X}_{j,c,t} = 0$ for all j, c, t, and that the demand shifter of business j is given by

$$u_{j,c,t} = \psi_j \frac{S_t}{\rho \overline{w}_c}, \tag{5}$$

where ψ_j gives the sensitivity of employment in business j when the relative purchasing power $\frac{S_t}{\rho \overline{w}_c}$ increases. The demand shift is stronger in labor market B, because the relative purchasing power increases more than in labor market A when the supplement S is in effect.

To simplify the analysis we define T_1 and T_2 as periods where the income supplement is or is not in effect, respectively (indexed by $\tau = \{T_1, T_2\}$ so that $S_{T_1} > 0$ and $S_{T_2} = 0$). Second, $\overline{x}_{c,\tau}$ is the average of variable x across businesses of labor market c in period τ. For example, $\overline{R}_{c,\tau} = \frac{\rho}{1-\epsilon^2}\left[1 + \frac{S_\tau}{\rho \overline{w}_c}\right]$.

The typical estimator with fixed effects across labor markets and time cannot eliminate the correlation between the replacement rate R and demand shifts u. Defining $\Delta_{c,\tau}$ as differences in variables between both time periods and labor markets, we have:

$$\begin{aligned}
\Delta_{c,\tau}\overline{y}_{c,\tau} &= [\overline{y}_{A,T_2} - \overline{y}_{A,T_1}] - [\overline{y}_{B,T_2} - \overline{y}_{B,T_1}] \\
\Delta_{c,\tau}\overline{R}_{c,\tau} &= [\overline{R}_{A,T_2} - \overline{R}_{A,T_1}] - [\overline{R}_{B,T_2} - \overline{R}_{B,T_1}] = \frac{S_{T_1}}{1-\epsilon^2}\left[-\frac{1}{\overline{w}_A} + \frac{1}{\overline{w}_B}\right] \\
\Delta_{c,\tau}\overline{u}_{c,\tau} &= [\overline{u}_{A,T_2} - \overline{u}_{A,T_1}] - [\overline{u}_{B,T_2} - \overline{u}_{B,T_1}] = \overline{\psi}\frac{S_{T_1}}{\rho}\left[-\frac{1}{\overline{w}_A} + \frac{1}{\overline{w}_B}\right].
\end{aligned}$$

Thus, a regression of $\Delta_{c,\tau}\overline{y}_{c,\tau}$ on $\Delta_{c,\tau}\overline{R}_{c,\tau}$ generates a biased coefficient since $\Delta_{c,\tau}\overline{R}_{c,\tau}$ remains correlated with the unobserved component $\Delta_{c,\tau}\overline{u}_{c,\tau}$. Instead, our local industry design transforms the data in terms of differences between businesses of the same labor market c. Just for comparison with the previous estimator, we denote the operator differencing variables between businesses of the same labor market as $\Delta_{j\in c}$. For the baseline specification (3) we use the simpler notation Δ to denote the differencing of variables between businesses of the same labor market and rely on fixed effects to demean other dimensions of the data. This gives us:

$$\begin{aligned}
\Delta_{j\in c}y_{j,c,\tau} &= y_{L,c,\tau} - y_{H,c,\tau} \\
\Delta_{j\in c}R_{j,c,\tau} &= R_{L,c,\tau} - R_{H,c,\tau} = \left[1 + \frac{S_\tau}{\rho \overline{w}_c}\right]\left[\frac{2\rho\epsilon}{1-\epsilon^2}\right] \\
\Delta_{j\in c}u_{j,c,\tau} &= u_{L,c,\tau} - u_{H,c,\tau} = (\psi_L - \psi_H)\frac{S_\tau}{\rho \overline{w}_c}.
\end{aligned}$$

The basic identification assumption behind our research design is that $\psi_L = \psi_H$, i.e., demand effects arising from the UI stimulus equally benefit low- and high-wage businesses in the same narrowly defined labor market c. The weaker assumption that neighboring businesses only partially share the benefits from the UI stimulus is still able to reduce the bias from the demand component. For example, if $\psi_H = \chi\psi_L$ with $\chi \leq 1$ then $Cov(\Delta_{j\in c}R_{j,c,\tau}, \Delta_{j\in c}u_{j,c,\tau}) = (1-\chi)\psi_L\left[\frac{2\epsilon}{\rho(1-\epsilon^2)}\right]V(\frac{S_\tau}{\overline{w}_c})$ which tends to 0 as $\chi \to 1$.

Furthermore, as we mentioned, our local industry estimator cannot eliminate unobserved factors that are not common between low- and high-wage businesses, most notably productivity. But to the extent that the difference in productivity between low-wage and high-wage businesses is time-invariant, we can eliminate its effect by further differencing the data along the labor market dimension. For example, if $u_{j,c,\tau} = \psi_j\frac{S_\tau}{\rho\overline{w}_c} + z_j$, where z_j is the productivity of business j, we can eliminate the time-invariant difference $z_L - z_H$ from the unobserved component by adding labor market fixed effects.

3 Data

3.1 Employment data (Homebase)

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.

Our HB data 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 differed in their employment trajectories during the pandemic. It is therefore important to incorporate information about the industry where the business operates. 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 the number of customer visits for each business derived from anonymized cell-phone data; and (ii) Yelp data that includes indicators of quality and price range of the business.[6] After matching businesses to industries, we are left with 30,665 businesses for which

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

we know their location and industry.

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. This restriction leaves us with 9,335 businesses.

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. This decreases the number of businesses to 6,550. To make the distinction between low- and high-wage businesses in a local-industry cell we drop single business cells which leaves us with 4,219 businesses for which we have daily 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, 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 closely 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:

$$w_{j,c} = a + d_c + e_{j,c}. \tag{6}$$

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 and industry. 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

Table 1: Employment, Hours, and Wages by Business Type

	All Businesses	Low-wage	High-wage
# Employees per business	6.4	6.0	6.9
Hours worked (per day)	6.7	6.6	6.7
Hourly wage ($)	11.3	10.3	12.2
Separation rate (%)	8.3	8.6	8.1
Hiring rate (%)	9.9	10.2	9.6
Yelp rating (1-5)	4.03	4.01	4.05
Share with $1 Yelp price	0.41	0.44	0.39
Share of restaurants and bars	0.85	0.84	0.86
Share of businesses in rural areas	0.30	0.30	0.29
# Businesses	4,219	2,037	2,182

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.

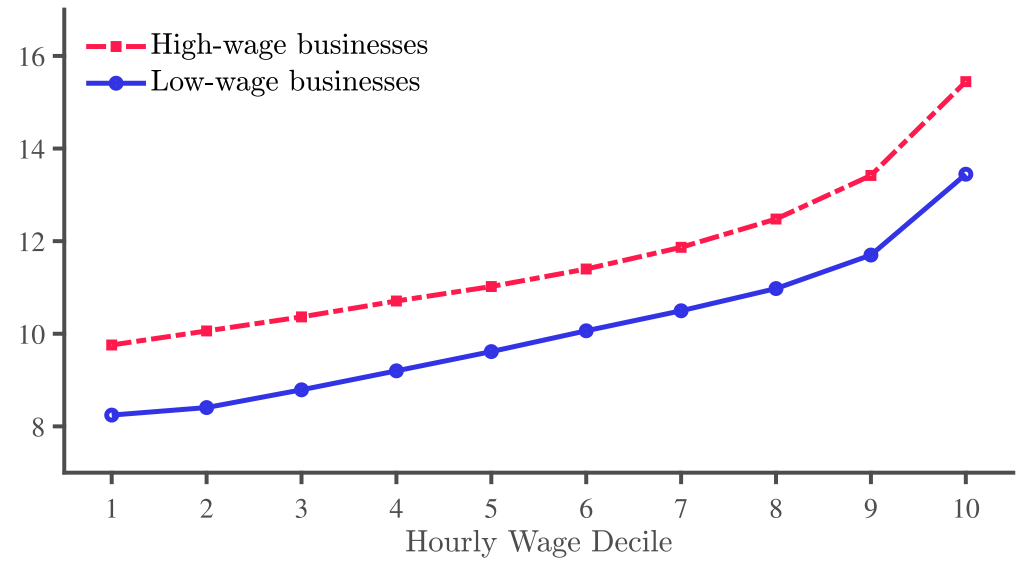
a separate group. Given our sample, this results in 1,134 local-industry cells with two or more businesses. Most cells have few businesses: the 25^{th} percentile cell has 2 businesses, the median cell has 3 businesses, and the 90^{th} percentile cell has 7 businesses. The largest number of businesses in a cell is 32.

We measure weekly employment in a business by counting the unique bodies 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 bodies 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).

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 is 6.4 across all businesses which indicates that the HB data capture mainly very small businesses (Kurmann, Lalé, and Ta, 2021). Furthermore, low-wage businesses are on average smaller than high-wage businesses. On average, employees work 6.7 hours per day and the hourly wage is $11.3. Even within narrowly defined local industries, there is sizable dispersion in hourly wages: high-wage businesses pay on average $1.9 more than low-wage businesses or equivalently, 7 percent more than the average.

In Figure 2 we report hourly wages by decile for low- and high-wage businesses. On average, the bottom 10 percent of employees in low-wage businesses receive around $8 per

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.

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 employed in t. Table 1 reports weekly separation and hiring rates averaged over all the weeks in the sample. The weekly hiring rate and job separation rate in the HB data is 9.9 and 8.3 percent, respectively. 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).[7]

Concerning industry and location, our sample consists of 85 percent of restaurants and bars, 70 percent of the businesses are in metro areas and 30 percent in rural areas. Finally, the Yelp data, on average, rate low- and high-wage businesses of the same local industry similarly, and have high-wage businesses slightly more expensive.

[7]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)).

3.2 Pandemic unemployment insurance benefits

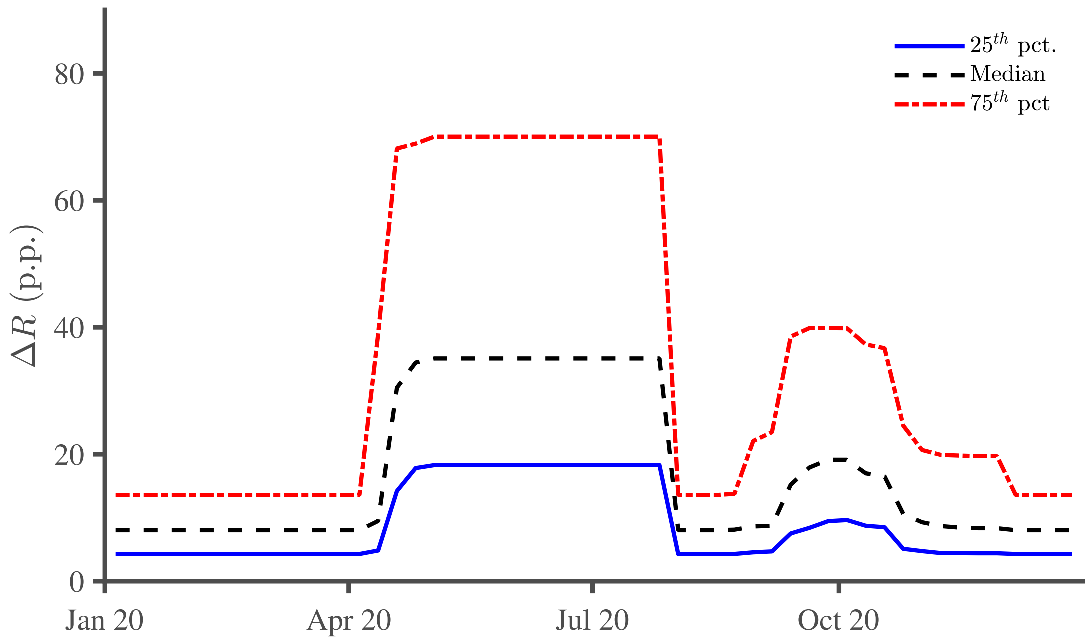
The CARES Act which was signed on March 27, 2020, and intended to ameliorate the effects of pandemic lockdown measures, set off the largest expansion of UI programs in U.S. history. The Act extended UI eligibility to self-employed and gig workers and those not meeting state requirements on previous work 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 additional $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. For our analysis, we focus on the UI policies implemented during 2020 (i.e., the CARES Act) and not during 2021. The CARES Act was implemented relatively fast in response to an unanticipated shock. It is more difficult to argue that the subsequent policies that started in 2021 were similarly unanticipated.

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.

We plot the impact of the FPUC supplement on our measures of local industry replacement rate gaps, $\Delta R_{c,t}$, in Figure 3. The replacement rate gap is the difference between the average replacement rates of low- and high-wage businesses in the same labor market with the business-level replacement rate defined as in equation (2). In normal times the replacement rate gap is not zero as the low-wage businesses have higher implied replacement rates (about 7 percentage points higher for the median labor market). When the pandemic UI programs come into effect the gap increases for all labor markets but increases more so for the labor markets with either a lower overall average hourly wage or a larger difference in the hourly wage between low- and

Figure 3: Variation in the Replacement Rate Gap



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

high-wage businesses.[8] To see this algebraically, 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 we have that

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

which can be written as:

$$\Delta R_{c,t} = \underbrace{b(\overline{w}_{c,t_0}) \frac{w_{H,c,t_0} - w_{L,c,t_0}}{w_{H,c,t_0} w_{L,c,t_0}}}_{=a_c} + \underbrace{\frac{w_{H,c,t_0} - w_{L,c,t_0}}{w_{H,c,t_0} w_{L,c,t_0}}}_{=\delta_c} S_t. \tag{7}$$

Labor markets with generally low hourly wages or larger inequality in hourly wages have higher scaling factors δ_c. In addition, note that by adding a labor market fixed effect in our regressions 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

[8]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 C).

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. 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 Event study analysis

As a first step, we characterize the employment recovery of low-wage businesses relative to high-wage businesses following the pandemic in March 2020, and how the relative recovery relates to broad changes in the magnitude of UI benefits. For this purpose, we estimate the following weekly regression for 2020 data:

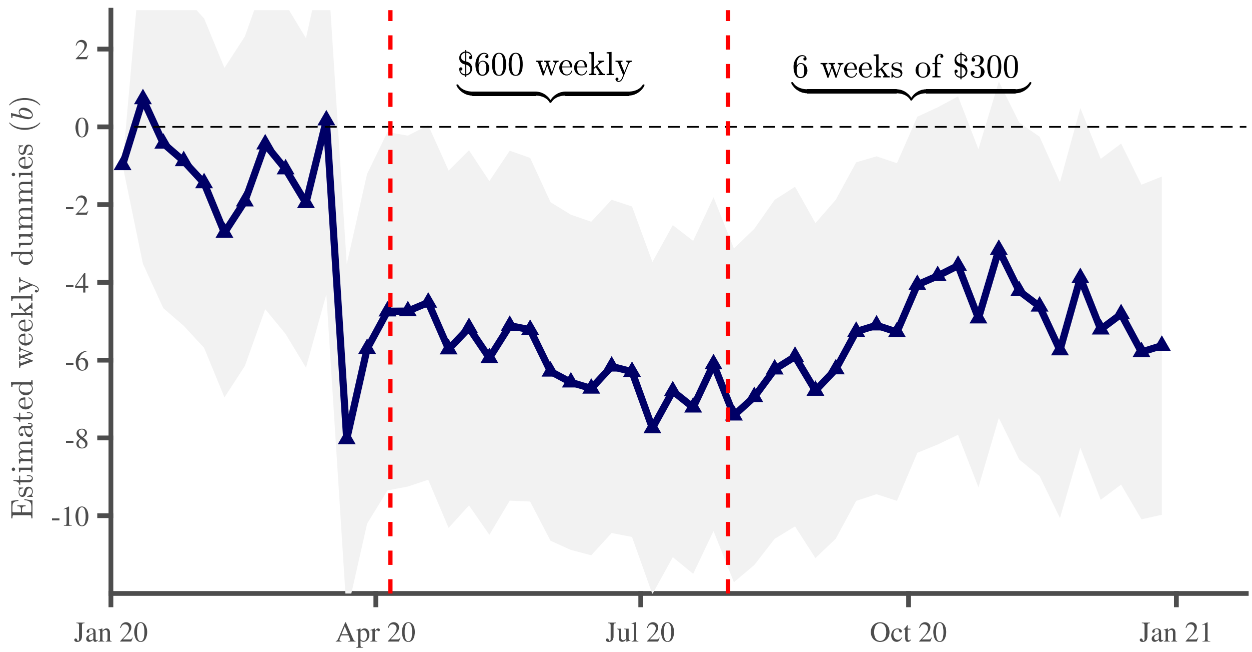
$$\Delta y_{c,t} = \sum_{s} b_s \mathbb{1}_{\{s=t\}} + \mathbf{X}'_{c,t}\gamma + \delta \Delta y_{c,t,2019} + \varepsilon_{c,t}, \tag{8}$$

where $\Delta y_{c,t} = y_{L,c,t}/y_{L,c,t_0} - y_{H,c,t}/y_{H,c,t_0}$ is the employment recovery rate gap which is expressed in percentage points and is calculated using the difference in average employment among low-wage and high-wage businesses, in labor market c and week t, relative to the base period t_0 of January-February 2020.

The weekly dummies capture the average weekly employment recovery gap controlling for a set of observables, $\mathbf{X}_{c,t}$, and one-year lagged employment gaps. The observables include (1) the number of Covid-19 deaths in a county as a measure of community health risk; (2) 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); (3) the difference in the number of weekly customers between the low- and high-wage businesses in cell c (relative to the base period of January-February 2020); and (4) the difference in price range between low- and high-wage businesses as measured in Yelp. Finally, we include the one-year lagged employment gap, to control for potential seasonal factors.

Figure 4 shows the estimated coefficients on the weekly time dummies as well as their 95 percent confidence intervals. We also mark the period from April 2020 to August 2020 when

Figure 4: Employment Recovery Around UI Programs



Notes: Estimated b's from Regression 8. The coefficients represent the average low-wage business employment minus the average high-wage business employment (both relative to their pre-pandemic level). Dates of initiation and expiration of the income supplement are reported. The shaded area represents the 95 percent confidence interval.

the CARES Act provided an additional $600 weekly supplement and the subsequent period when the Lost Wages Assistance (LWA) program provided a $300 weekly supplement for six weeks. We can see that after an initial bigger drop of employment in low-wage businesses at the onset of the pandemic, low-wage businesses are catching up with high-wage businesses and the employment gap narrows from the end of March 2020 to early April 2020. With the arrival of the CARES Act the employment gap not only stops closing, but it starts to widen until the $600 weekly supplement expires at the end of July 2020. Thereafter, employment in low-wage businesses rapidly gains ground and partially closes the employment gap to the high-wage businesses.[9]

[9]For a detailed evaluation of pre- and post-pandemic trends in the time series of employment, hours per employee, hourly wage, separation, and hiring rates, for low- and high-wage businesses during 2019 and 2020, see Appendix D.

Table 2: Effect of Pandemic UI Replacement Rate

$\Delta y_{c,t}$	Employment gap		Hours per employee gap		Hourly wages gap	
	(1)	(2)	(3)	(4)	(5)	(6)
Replacement rate gap	−6.52***	−5.73***	0.41	0.39	1.88***	1.52**
(per 100 p.p.)	(1.32)	(1.34)	(0.60)	(0.62)	(0.65)	(0.63)
Covid-19 deaths	−1.28	−1.45	−0.21	−0.11	0.65*	0.75*
(per 100,000 pop.)	(0.98)	(1.04)	(0.42)	(0.43)	(0.37)	(0.39)
School traffic	1.61***	1.78***	−0.02	−0.13	−0.51***	−0.57***
(% change)	(0.44)	(0.45)	(0.18)	(0.19)	(0.15)	(0.16)
Customer visits gap	–	7.52***	–	0.61***	–	−0.50***
	–	(0.76)	–	(0.22)	–	(0.18)
# Observations	59,065	51,834	52,741	46,700	48,952	43,519

Notes: Estimates from regression 9 including the control variables. Observations are at the local industry/week level. We winsorize variables at the one percent level and cluster standard errors at the local industry level.

4.2 The effect of income supplements on employment recovery

We now evaluate the direct effect of the pandemic income supplements in our local industry sample by replacing the weekly fixed effects in equation (8) with the replacement rate gaps:

$$\Delta y_{c,t} = \beta \Delta R_{c,t} + \mathbf{X}'_{c,t}\gamma + \delta \Delta y_{c,t,2019} + a_c + \eta_{c,t}. \quad (9)$$

Essentially, we are estimating regression (3) described in Section 2. For this regression, we also include a cell fixed effect a_c to control for time-invariant differences between low- and high-wage businesses, most notably constant differences in their productivity. Finally, we also estimate the impact of replacement rate gaps on the recovery rate for several alternative left-hand side variables: employment, average hours per employee, and hourly wages.

Table 2 shows the regression results. Estimates are reported with and without customer traffic as a control variable. Each observation represents a cell in a particular week during 2020. There are fewer observations when we include customer traffic as a control since this variable is not available for all businesses. Similarly, there are fewer observations for the alternative dependent variables hours per employee and hourly wages since businesses with employment information do not have information on hours and wages for every week of 2020.

In our preferred specification that controls for customer traffic, a 100 percentage point increase in the business replacement rate gap, is associated with a 5.7 percentage point decline in low-wage business employment relative to high-wage business employment. For the median labor market the CARES Act increases the replacement rate gap by 27 percentage points, see Figure 3, which implies a 1.5 percentage point decline in low-wage employment relative to high-wage employment. 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.

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

Estimate per 100 p.p. ΔR	-5.7
$\times$ Median increase in ΔR	0.27
Effect of $600 supplement	-1.5

Notes: The baseline estimate in Table 2 is translated using the median increase in the replacement rate gap in Figure 3.

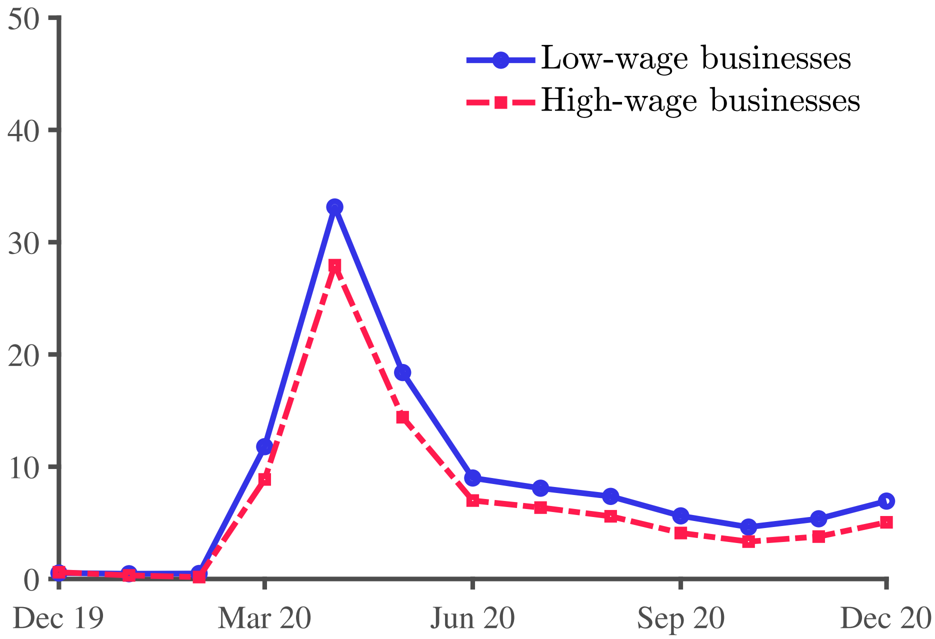
Two pieces of evidence corroborate our claim that our estimates do not arise from productivity differences between low- and high-wage businesses. First, when we control for differential customer visits the estimated employment effect changes little. Second, an increase in the replacement rate gap is associated with a positive but not significant increase in hours per employee gap of 0.3 percentage points, and a positive and statistically significant increase in the hourly wage gap by 1.5 percentage points.[10] These findings suggest that the pandemic UI generated stronger labor supply constraints for low-wage businesses which reacted to these constraints by increasing the hours worked by their existing employees and by raising their wages at a faster pace. Had the faster employment gains in high-wage businesses been driven by differential productivity shifts (as opposed to UI) we should expect their hours per employee and hourly wages to also grow faster than in low-wage businesses.

4.3 Discussion and robustness

The basic identification assumption to control for demand effects is that neighboring businesses share most of the positive impact of UI stimulus. We find this a plausible assumption,

[10]This result differs from Jager et al. (2020) who analyze the sensitivity of wages to UI benefits using four UI reforms in Austria between the period 1976-2001 and find that wages in that case are insensitive to UI benefits changes.

Figure 5: 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).

especially if the neighborhood is defined in a narrow way (e.g., four-digit zip code in our baseline). As an exercise, 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 coefficient for the replacement rate gap is -0.4 and statistically insignificant. The decline (in absolute value) in the employment response is natural since businesses located 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 additionally confound the estimates.

This exercise explains why we find different results from Finamor and Scott (2021) who also use HB data but use state fixed effects and find that a higher replacement rate is not associated with a lower labor market re-entry of workers. Thus, our research design highlights the importance of comparing units within narrow local industries.

Another concern is the extent of business closures in HB while the $600 income supplement is dispersed. Figure 5 shows that the average share of closed stores is 30 percent in April 2020, 16 percent in May 2020, and 7 percent in June 2020. Thus, most of the HB businesses are open during the period of the generous income supplement.

Next, we conduct a series of robustness exercises. We focus on the implications for the estimated coefficient for the replacement rate gap in the employment gap regression. A detailed analysis of the estimates is reported in Table E-5, Appendix E.

Continuously open businesses Our baseline sample includes businesses that operated continuously during the pandemic and businesses that closed and re-opened. If we restrict the sample to the businesses that continuously operated during the pandemic the estimated coefficient on the replacement rate gap declines from −5.7 to −3.9. This attenuation implies that part of the disincentive effect operates through low-wage businesses remaining closed at higher rate than high-wage businesses.

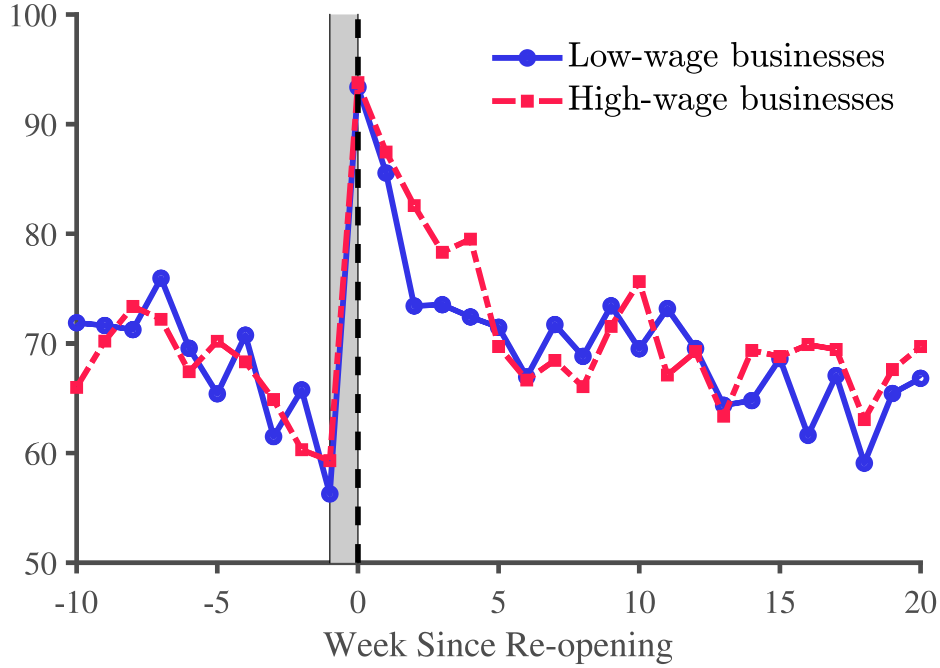
Tips and commission Recorded hourly wages in HB likely represent base rates and do not include overtime, tips, and commissions (OTC). OTC is particularly relevant in the Leisure and Hospitality sector, which accounts for the majority of the HB sample. To address this concern, we use information from the CPS and adjust individual wage rates in the HB data with an OTC amount estimated from the CPS data (see Appendix A for details). With the imputed wages that include tips and commissions, the estimated coefficient on the replacement rate gap is −3.7.

Broader sample In the baseline sample, we remove businesses for which we could not find information on their price and quality from Yelp. If we include businesses for which we do not have Yelp-related information, the sample size almost doubles to 7,958 businesses. For the expanded sample the estimated replacement rate gap coefficient increases in absolute value from −5.7 to −6.6.

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 −5.2. 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 increases only slightly in absolute value to −6.0.

Recalls Fujita and Moscarini (2017) document that a large share of workers returns to their previous employer after a jobless spell. We examine if, upon re-opening, the HB businesses disproportionately hired employees who previously worked in the business. In Figure 6 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

Figure 6: 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 grey area).

(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.[11] Our benchmark model, presented in the next section, abstracts from a recall margin. We expand the baseline model to include recalls in Appendix I and discuss the results.

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.,

[11] Our findings are in accord with Ganong et al. (2022) who document that UI supplements did not have substantial effects on recalls.

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.[12]

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 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, $b_N(w)$.[13] 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

[12] 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.

[13] 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.

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

$$W_t(w) = \mathcal{U}(w) + \beta \left\{ (1-\delta) W_{t+1}(w) + \delta \left[p_{R,t} U_{R,t+1}(w) + (1-p_{R,t}) U_{N,t+1}(w) \right] \right\}, \tag{10}$$

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[14]

$$\begin{aligned} U_{R,t}(w) = \mathcal{U}\left[b_{R,t}(w)\right] + \beta \Bigg\{ & p_{N,t} \left[\lambda(\theta_t) \int \max\left\{W_{t+1}(w'), U_{N,t+1}(w)\right\} \frac{\tilde{v}_t(w')}{\tilde{v}_t} dw' + \left[1-\lambda(\theta_t)\right] U_{N,t+1}(w) \right] \\ & + (1-p_{N,t}) \left[\lambda(\theta_t) \int \max\left\{W_{t+1}(w'), U_{R,t+1}(w)\right\} \frac{\tilde{v}_t(w')}{\tilde{v}_t} dw' + \left[1-\lambda(\theta_t)\right] U_{R,t+1}(w) \right] \Bigg\} \end{aligned} \tag{11}$$

and

$$U_{N,t}(w) = \mathcal{U}\left[b_N(w)\right] + \beta \left\{ \lambda(\theta_t) \int \max\left\{W_{t+1}(w'), U_{N,t+1}(w)\right\} \frac{\tilde{v}_t(w')}{\tilde{v}_t} dw' + \left[1-\lambda(\theta_t)\right] U_{N,t+1}(w) \right\}. \tag{12}$$

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

$$W_{t+1}\left[w^*_{i,t}(w)\right] = U_{i,t+1}(w) \text{ for } i \in \{R, N\}. \tag{13}$$

[14]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.

Note that $w^*_{i,t}(w)$ in (13) 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

$$w^*_{N,t}(w) \leq w^*_{R,t}(w). \tag{14}$$

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

$$w^*_{\min,t} = \min_w w^*_{N,t}(w). \tag{15}$$

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

5.3 Transition of measures

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

$$e_{t+1}(w) = (1-\delta)e_t(w) + F_t(w)\frac{\tilde{v}_t(w)}{\tilde{v}_t}\lambda(\theta_t)u_t, \tag{16}$$

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

$$u_{R,t+1}(w) = p_{R,t}\delta e_t(w) + [1 - f_{R,t}(w)](1 - p_{N,t})u_{R,t}(w), \tag{17}$$

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

$$u_{N,t+1}(w) = (1 - p_{R,t})\delta e_t(w) + [1 - f_{N,t}(w)][p_{N,t}u_{R,t}(w) + u_{N,t}(w)], \tag{18}$$

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

$$u_t = \int u_{R,t}(w)dw + \int u_{N,t}(w)dw, \tag{19}$$

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

$$v_{t+1}(w) = \delta e_t(w) + \left[1 - F_t(w)\frac{\lambda(\theta_t)}{\theta_t}\right] v_t(w). \tag{20}$$

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

$$e_t(w) + v_t(w) = g(w)M, \tag{21}$$

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

$$\tilde{v}_t = \int_{w \geq w^*_{\min,t}} v_t(w)dw \tag{22}$$

Aggregating equation (21) over $w \geq w^*_{\min,t}$ we have

$$e_t + \theta_t(1 - e_t) = M \int_{w > w^*_{\min,t}} g(w), \tag{23}$$

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

$$F_t(w) = \frac{1}{u_t}\left\{(1 - p_{N,t})\int_{w' \geq w^*_{R,t}(w)} u_{R,t}(w')dw' + \int_{w' \geq w^*_{N,t}(w)} \left[u_{N,t}(w') + p_{N,t}u_{R,t}(w')\right] dw'\right\}. \tag{24}$$

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

$$JFR_t = \lambda(\theta_t) \int \frac{\tilde{v}_t(w)}{\tilde{v}_t} F_t(w)dw, \tag{25}$$

and the aggregate vacancy fill rate is

$$VFR_t = \frac{\lambda(\theta_t)}{\theta_t} \int \frac{\tilde{v}_t(w)}{\tilde{v}_t} F_t(w)dw. \tag{26}$$

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 (10), (11), and (12).
2. The reservation wages $\{w_R^*, w_N^*, w_{\min}^*\}$, satisfy equations (13) and (15).
3. The (un)employment and vacancy distributions $\{e, u_R, u_N, u, v, \tilde{v}\}$ satisfy equations (16), (17), (18), (19), (22), and (23).
4. The acceptance rate $F(w)$ satisfies equation (24).
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 C-3 in Appendix C). 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 G for details) and find a 20 percent share of UI recipients before the pandemic and a 60 percent share for 2020.[15] 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 C-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 $\sigma = 0.11$ such that wages of filled jobs at the 75^{th} percentile are 7% 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

[15] 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	0.11	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.

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 wage distribution. In the HB data, the 10^{th} percentile of (residual) log wage is 88 percent of the mean (see Table C-2 in Appendix C). 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 $\rho_N = 0.15$ 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, and employment of low-wage businesses declines about 8 percentage points more than employment of high-wage businesses. 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 2.0 which is the median replacement rate during the CARES Act in our HB sample (see Table C-3 in appendix).

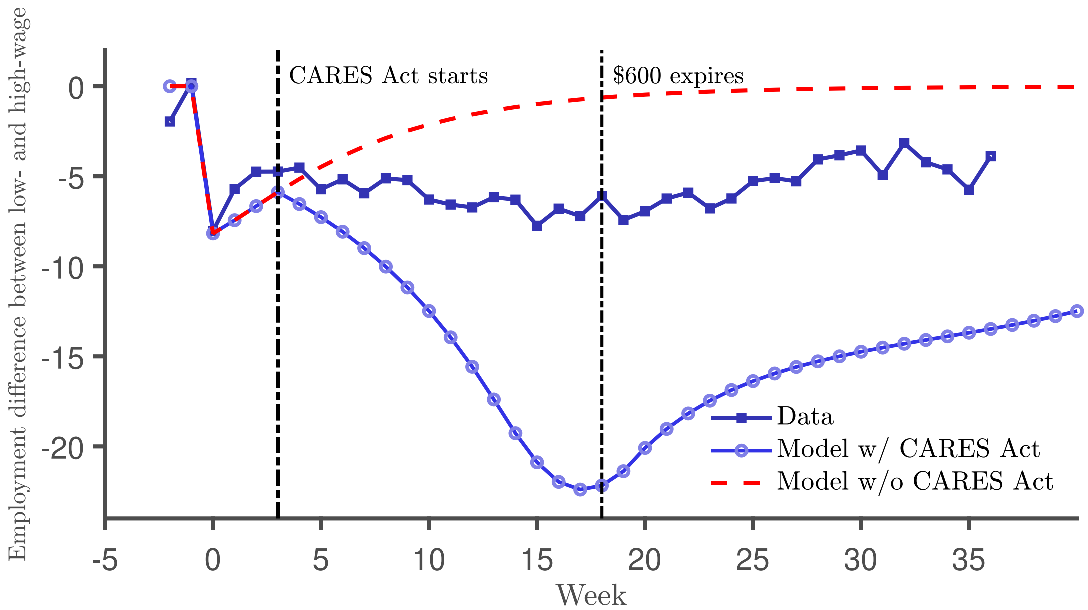
In addition, the CARES Act extended benefits for 13 weeks beyond the usual state-level duration of benefits so that, effectively, benefits did not expire until the end of December 2020. We model this change as a decrease in the benefit expiration probability from $p_N = 0.038$ to $p'_N = 0$ that lasts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

Figure 7: Difference in Employment Recovery in Low- and High-wage Jobs: Model vs. Data



Notes: The figure plots the average low-wage business employment minus the average high-wage business employment (both relative to their pre-pandemic level). "Model with CARES Act" is the benchmark model and "Model without CARES Act" is the benchmark model with the separation shock but not the CARES Act provisions. Data represent the estimated b's from Regression 8.

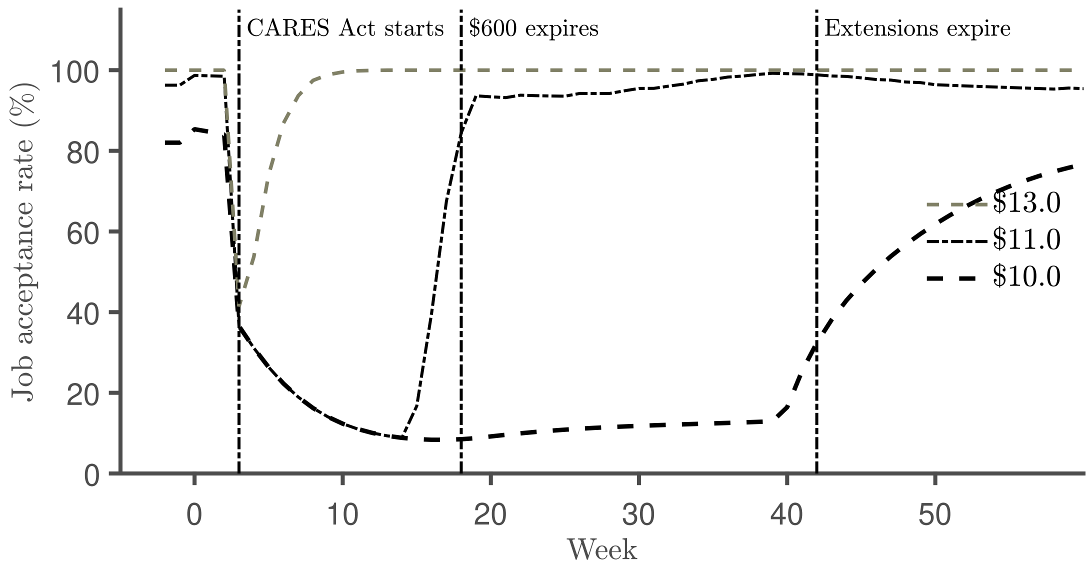
policy. As various additional changes to UI policy occurred, through Fall 2020 and then 2021, we focus most of our analysis on the implications for the 16 weeks for which supplemental benefits were initially expected to be in place.

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 F.

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),

Figure 8: 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.

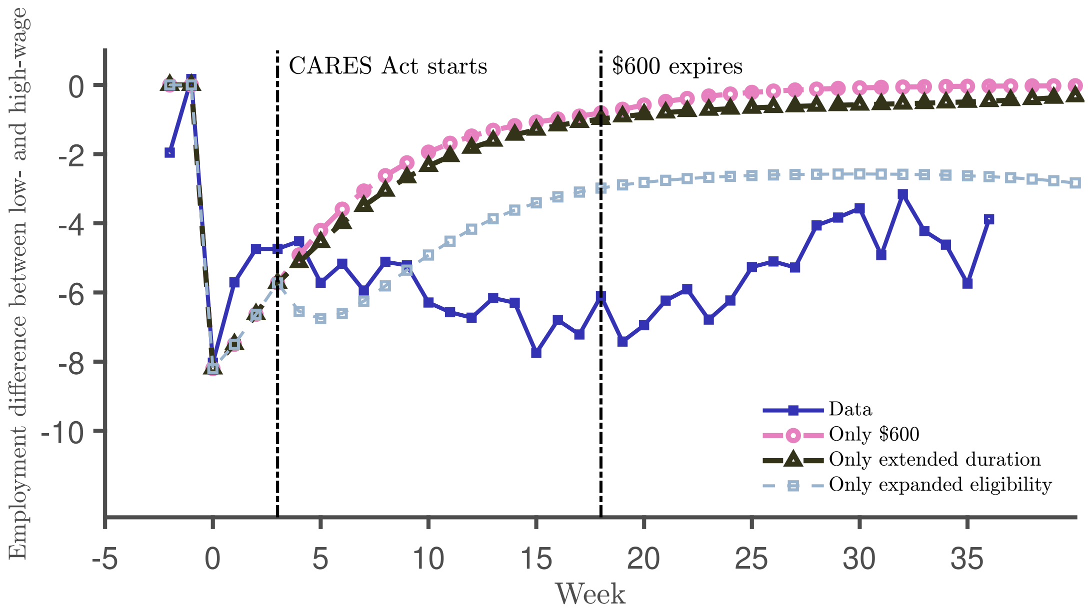
which is appropriate given that the disincentive effects are measured within narrow local industry markets. Figure 7 compares the employment recovery gap with our estimates from Figure 4. We find that the baseline model overstates the decline in the employment recovery gap by a large degree.

Figure 8 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.

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-

Figure 9: Employment Recovery Gap: CARES Act UI Provisions



Notes: The figure plots the average low-wage business employment minus the average high-wage business employment (both relative to their pre-pandemic level) when only one of the CARES Act provisions is in effect: (a) the income supplement of $600, (ii) the extended maximum duration, and (iii) the expanded eligibility. Data represent the estimated b's from Regression 8. Dates of initiation and expiration of the $600 income supplement are reported.

unemployed workers gradually start to accept the offer so that the acceptance rate converges to its steady state relatively fast.

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. As a result, the acceptance rate continues to decline and only bounces back when the $600 supplement is close to expiring and the UI-unemployed again start to accept the business' offer.

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 7. 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, the employment recovery gap between low- and high-wage businesses is quite large in the model relative to the data. The gap is widening until the $600 benefits are about to expire and the acceptance rates of UI unemployed increase again.

Figure 9 analyzes the employment gap between low- and high-wage businesses when only one of the pandemic UI policy provisions is in effect. We find that the $600 income supplement and the extended duration of benefits alone generate too little of a response. The expanded eligibility provision has the strongest effect on the employment recovery gap but the shape of the recovery does not match data well. In the next section, we present a model where all provisions are in effect and the model can generate an employment recovery gap close to the data.

6.1 Reconciling the model with the data

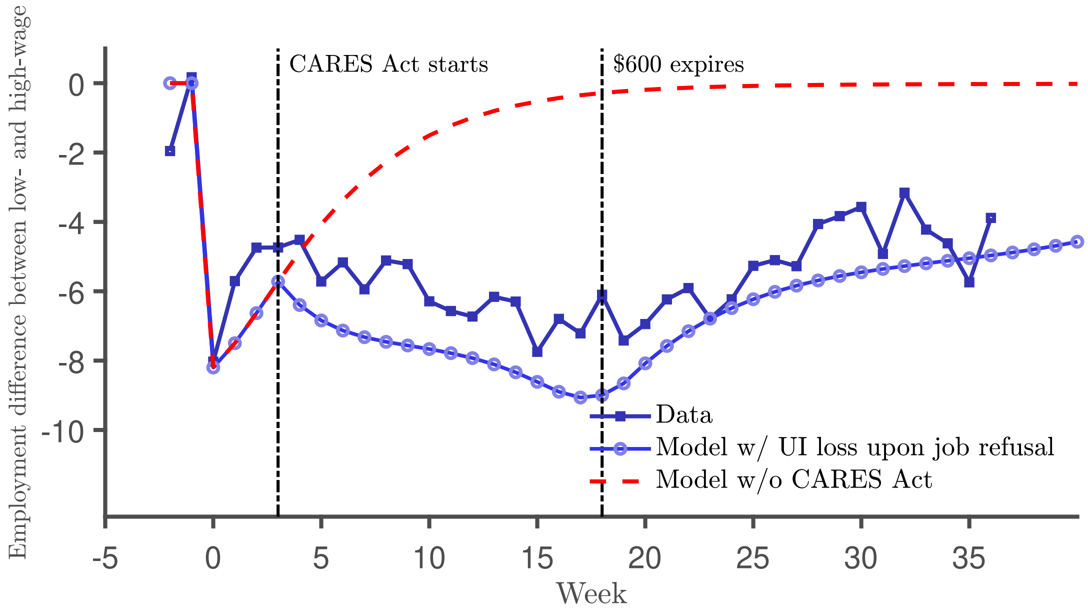
The baseline model generates a low-wage employment gap that is much larger than in the data. In this section, we propose a reduced-form solution to reconcile the model with the data: a probability of losing UI eligibility upon refusal of a job offer.[16]

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

[16] 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.

Figure 10: Employment Recovery Gap: Model with UI Loss upon Job Refusal



Notes: The figure plots the average low-wage business employment minus the average high-wage business employment (both relative to their pre-pandemic level) in the model with the probability of losing UI upon job refusal with and without the CARES Act. Data represent the estimated b's from Regression 8.

written as follows:

$$\begin{aligned} U_{R,t}(w) = &\, \mathcal{U}\left[b_{R,t}(w)\right] + \beta p_{N,t}\left[\lambda(\theta_t)\int \max\left\{W_{t+1}(w'), U_{N,t+1}(w)\right\}\frac{\tilde{v}_t(w')}{\tilde{v}_t}dw' + \left[1-\lambda(\theta_t)\right]U_{N,t+1}(w)\right] \\ & + \beta(1-p_{N,t})\left[\lambda(\theta_t)\int \max\left\{W_{t+1}(w'), (1-\chi)U_{R,t+1}(w) + \chi U_{N,t+1}(w)\right\}\frac{\tilde{v}_t(w')}{\tilde{v}_t}dw'\right] \\ & + \beta(1-p_{N,t})\left[1-\lambda(\theta_t)\right]U_{R,t+1}(w). \end{aligned} \tag{27}$$

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

$$W_{t+1}\left[w^*_{R,t}(w)\right] = (1-\chi)U_{R,t+1}(w) + \chi U_{N,t+1}(w). \tag{28}$$

The measure of these workers evolves according to

$$u_{R,t+1}(w) = p_{R,t}\delta e_t(w) + \left[1-\left(f_{R,t}(w) + \chi(1-f_{R,t}(w))\right)\right](1-p_{N,t})u_{R,t}(w), \tag{29}$$

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

$$u_{N,t+1}(w) = (1 - p_{R,t})\delta e_t(w) + [1 - f_{N,t}(w)]\,[p_{N,t}u_{R,t}(w) + u_{N,t}(w)] \tag{30}$$
$$+ \chi(1 - f_{R,t}(w))(1 - p_{N,t})u_R(w), \tag{31}$$

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.[17]

We set the probability of losing eligibility upon job refusal, χ, equal to 10 percent in order to match the employment recovery gap we estimated in the data. Figure 10 compares the employment recovery gap between low- and high-wage businesses in the transition. The employment recovery gap in this model matches the data closely. First, the model generates the abrupt decline in the employment gap once the CARES Act comes into effect (at t=3) without generating too steep of a decline. Second, the model captures well the faster recovery of low-wage businesses once the $600 supplement expires. The employment recovery gap when the $600 supplement is in effect (i.e., during the first sixteen weeks of the CARES Act) is about 1.7 percentage points lower than when the $600 supplement has expired, which is fairly close to our empirical estimate of 1.5 percentage points.

Thus, the introduction of a small probabilistic loss of UI eligibility increases the incentives of UI-unemployed to accept low-wage offers, and the model can match the differential employment recovery we estimated in the data. In Appendix I, we study a version of the baseline model where unemployed workers can be recalled by their former employer and UI is denied for those who refuse to return to work upon getting recalled. In such a model, the probability of UI loss upon job refusal is effectively the probability of being recalled to a previous employer.

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 differential employment recovery we document in 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

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

Table 5: Employment Recovery Loss (in percentage points)

Each provision alone	
$600 Additional UI Benefits	0.3
Extended Duration of UI Benefits	0.3
Expanded Eligibility of UI Benefits	1.2
Each provision when other provisions are in effect	
$600 Additional UI Benefits	2.1
Extended Duration of UI Benefits	2.3
Expanded Eligibility of UI Benefits	3.9
CARES Act (all provisions combined)	4.7

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.

to the employment-to-normal level when the policy is not in effect and take the average over the period April to December 2020.

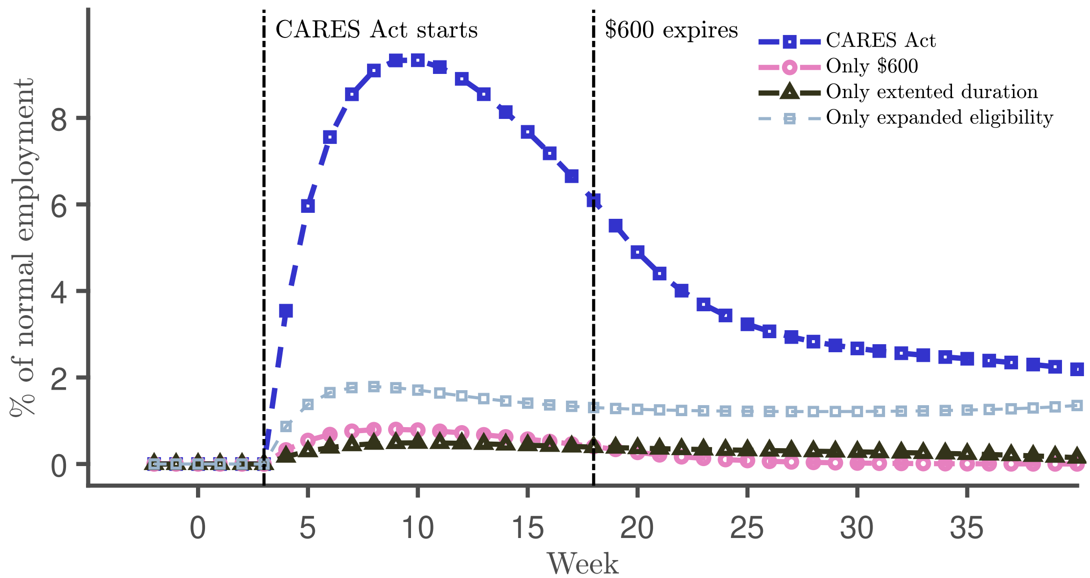
The aggregate employment recovery loss associated with each UI policy when implemented separately is modest. However, when combined, the pandemic UI policies generate substantial aggregate employment losses. Conditional on the other two provisions being in effect, the income supplement holds back the employment recovery by 2.1 percentage points, the extended maximum duration by 2.3 percentage points, and the expanded eligibility by 3.9 percentage points. Without any of the UI policy changes, the employment recovery would have been, on average, 4.7 percentage points closer to normal between April and December 2020. This employment loss represents around 25 percent of the average employment loss in the Leisure and Hospitality sector during the same period. The employment loss is non-linear (Figure 11), peaking at almost 8 percentage points about 10 weeks after the initiation of the CARES Act. 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 H for more details). Similarly to Table 5, we calculate the unemployment duration

Figure 11: 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.

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 11 percent (from 3.5 weeks to 3.9 weeks). Since in our model, the replacement rate rises by 292 percent (from 0.51 to 2.0), this implies a duration elasticity of 0.03. In turn, the extended benefits alone increase unemployment duration by 8 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 an elasticity of 0.05. Expanded eligibility increases the expected duration of unemployment by 20 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 0.05.

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 83 percent implying an elasticity of 0.28. When the extended benefits provision is activated on top of the other two provisions, unemployment duration increases by 68 percent implying an elasticity of 0.45. When the expanded eligibility provision is activated on top of the other two provisions, unemployment

Table 6: Unemployment Duration Elasticities implied by CARES Act Provisions

	Unemployment Duration	Elasticity
Each provision alone		
$600 Additional UI Benefits	+11%	0.03
Extended Duration of UI Benefits	+8%	0.05
Expanded Eligibility of UI Benefits	+20%	0.05
Each provision when other provisions are in effect		
$600 Additional UI Benefits	+83%	0.28
Extended Duration of UI Benefits	+68%	0.45
Expanded Eligibility of UI Benefits	+104%	0.26

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.

duration increases by 104 percent implying an elasticity of 0.26.

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 impact 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. (2022); 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.

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 4.7 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 consists 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 contains 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 ”estimation 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 that we can match by name and address to Safegraph's Places of Interest (POI) database to attribute a consistent NAICS code (see Kurmann, Lalé, and Ta (2021) for details). The estimation 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).

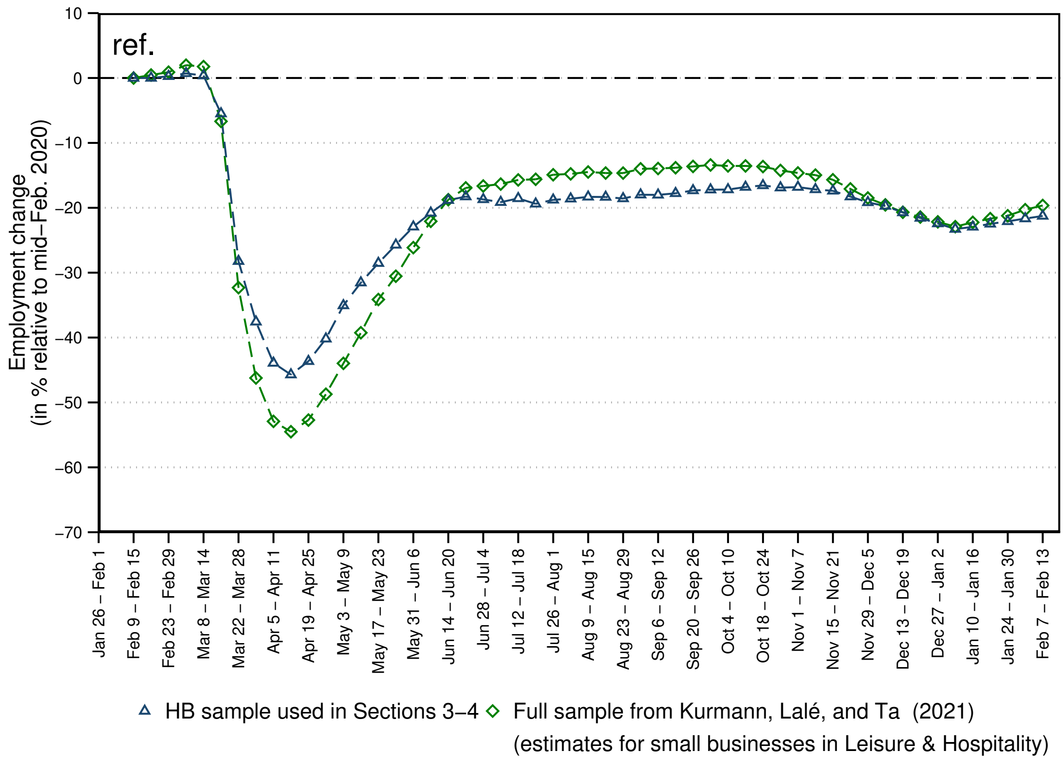
The full sample consists of over 51,000 businesses with over 500,000 unique employees, covering 2377 counties with a total population of over 300 million. The estimation sample consists of 4,219 businesses with over 80,000 unique employees, covering 527 counties with a total population of about 220 million. 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 employment of small businesses in 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 estimation 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).[18]

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 estimation 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 estimation 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 Estimation 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 estimation sample. See text for details.

[18]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 estimation 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 estimation 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 pertains 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.[19] 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 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 estimation sample is therefore also broadly representative of small businesses in 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,

[19] 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 ($)	12.5	11.3	14.8	13.6
Weekly hours	30.6	30.1	30.6	30.1
Weekly earnings ($)	400.1	347.0	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 estimation 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:

$$otc_{ij} = a_{0j} + a_{1j} w_{ij} + \varepsilon_{ij}, \tag{32}$$

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

i in region j.[20] 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.

Given this OTC adjustment, we can now address the above questions. First, how to adjust HB wage data when computing UI replacement rates? Since pre-pandemic state replacement rates are around 0.5 (within eligibility bounds), this matters primarily for computing replacement rates including the weekly pandemic supplement of \$600 during Summer 2020, respectively \$300 during part of Fall 2020 and Spring 2021. When we use weekly OTC-adjusted earnings from our HB sample, we obtain a median replacement rate of around 2. Since we obtain a very similar replacement rate distribution with weekly earnings computed as HB hourly wage rate (without OTC) times 35 weekly hours, we use this approximation as the baseline in the main text (also see Appendix C).

Second, we find that workers in businesses classified as low-wage receive on average a larger OTC adjustment than workers in businesses classified as high-wage. While taking into account OTC reverses the identification of some businesses, our regression results are all robust to this change and, in some cases, even stronger (see Appendix E for details). Moreover, it is important to remember that the OTC adjustment proposed here is based on estimates from CPS data that, for many regions, are surrounded by substantial uncertainty due to the small sample size. Hence, the OTC adjustment is at best a rough approximation. For these reasons, we base the analysis in the main text on unadjusted hourly wages.

B Pandemic UI generosity and economic outcomes across counties

In this appendix, we document the relative generosity of pandemic UI supplements across counties and their relationship with economic outcomes.

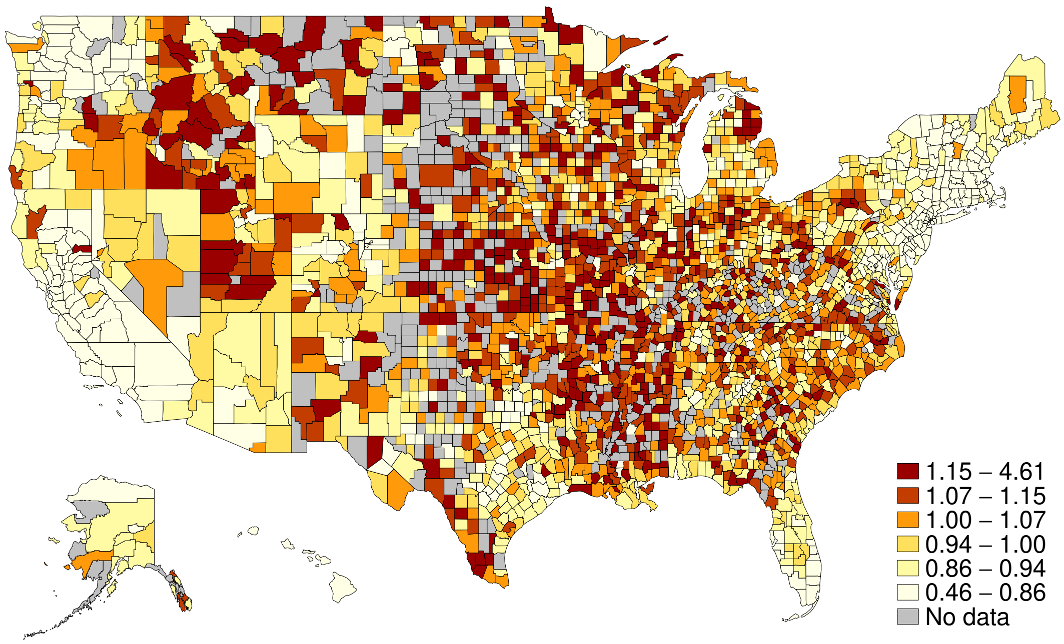
B.1 Relative generosity of pandemic UI supplements

As described in the main text, from the beginning of April 2020 to the end of July 2020, the federal government supplemented state-level UI benefits with an additional \$600 per week through the Federal Unemployment Compensation (FPUC) program. During parts of Fall 2020 and then again from January 2021 through September 2021, additional supplements of \$300 per week were paid out. Since weekly earnings of likely UI recipients vary substantially across different parts of the U.S., the relative generosity of these supplements also varies substantially.

To quantify the spatial variation in relative generosity implied by the \$600 weekly FPUC supplement, we compute the ratio of the \$600 to average weekly earnings of in-person service workers

[20] 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.

Figure B-2: Change in Relative Generosity of UI from \$600 Supplement



Notes: The figure reports the change in relative generosity of pandemic UI implied by the \$600 weekly FPUC supplement across U.S. counties, measured as the ratio of the \$600 weekly supplement to average county weekly earnings for in-person service workers. See text for details.

across U.S. counties. Average weekly earnings are measured for 2019 from the Quarterly Workforce Indicators (QWI). In-person service sectors are defined as retail trade (NAICS 44-45), education and health (NAICS 61-62), leisure and hospitality (NAICS 71-72), and other services (NAICS 81). We only retain counties for which the QWI provides actual data and for which we have employment data from HB.

As Figure B-2 shows, the \$600 weekly supplement implies large spatial variations in the change of UI generosity across counties, ranging from about 50 percent on the West Coast, the Northeast, and Florida to over 200 percent in the Midwest and the Central South region of the U.S.[21]

B.2 Relation to changes in spending and employment at the onset of the pandemic

To illustrate the potential importance of stimulative effects of UI and other demand confounds that occurred around the start of pandemic UI, we report cross-country scatter plots of the relative generosity of the \$600 weekly FPUC supplement (measured as above) against the change in consumer

[21]Note that comparing the relative generosity of the FPUC across counties here is analogous to comparing the difference between replacement rates of low-wage and high-wage businesses across local industries in the text since the effect state UI programs is differenced out.

spending and the change in in-person service sector employment between the end of March 2020 and end of April 2020 (i.e., the first month of FPUC payments) as a percent of the January 2000 - February 2000 average. The county-level consumer spending data is from Chetty et al. (2020) based on credit- and debit-card spending data from Affinity, while county-level employment for in-person service sectors comes from Kurmann, Lalé, and Ta (2021) based on HB data.

As Figure B-3 shows, there is a pronounced *positive* relation between the change in the generosity of UI implied by the \$600 weekly supplement and the change in consumption and employment. Further inspection of the data reveals that this effect is robust to various controls, including industry and state fixed effects. We also find similar results with regard to small business revenues (using data from Womply by Chetty et al. (2020)); and we find that around the expiration of the \$600 weekly supplement at the end of July, there is a corresponding *negative* (although substantially smaller) relation of the two variables with the relative generosity measure.

We take this evidence as highly suggestive of stimulus effects of the \$600 supplement and, potentially, other local labor market confounds that obscure the disincentive effects of UI during the first few months of the pandemic. In other words, research designs that exploit simple variations in UI generosity around the beginning and end of pandemic UI supplements do not identify a disincentive effect but an equilibrium effect that combines disincentive and stimulus effects and, potentially, other confounds. As explained in Section 2, our local-industry design differences out these demand confounds, at least to the extent that they affect low- and high-wage businesses equally.

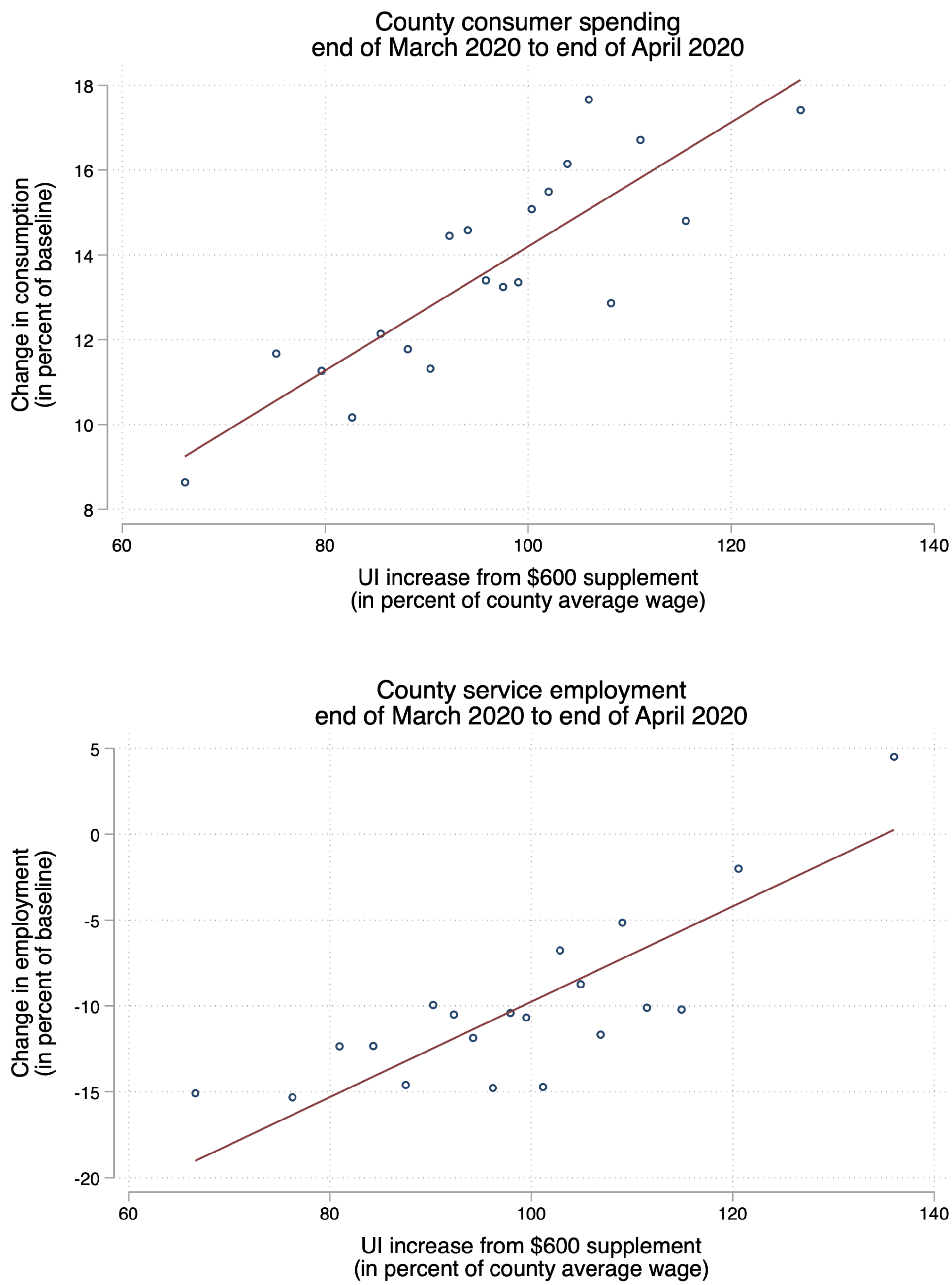
C Cross-sectional statistics

Employment, Hours, and Wages Table C-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 one or two employees. Even businesses at the 90^{th} percentile that employ on average 12 employees would be considered small by normal standards. We winsorize the hourly wage at \$7 per hour. Workers reporting an hourly wage lower than this threshold are likely compensated by tips which we cannot verify in our data. This adjustment does not affect the division of businesses in low-wage vs. high-wage (and hence, the empirical estimates) but affects the cross-sectional dispersion we target in the model. Without this adjustment, the residual of the log-hourly wage at the 10^{th} percentile would be -0.15 instead of -0.12.

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}.

Figure B-3: Generosity of Pandemic UI Supplement vs. Changes in Consumption and Employment



Notes: The figure reports binned scatter plots of the relative generosity of the $600 weekly supplement against the change in average county-level consumption (left) and the change in in-person service sector employment (right) between the end of March 2020 and end of April 2020 as a percent of baseline, which is the January 2000 to February 2000 average. See text for details.

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

	Mean	SD	p(5)	p(10)	p(25)	p(50)	p(75)	p(90)
# Employees	6.4	5.1	1	2	3	5	8	12
Hours per worker	6.7	1.6	4.3	4.9	5.7	6.5	7.5	8.5
Hourly wage ($)	11.3	2.7	7.2	8.0	9.5	11.3	13.0	14.7
Residual log-hourly wage ($)	0.0	0.09	-0.16	-0.12	-0.05	0.0	0.07	0.13
Separation rate (%)	8.3	13.9	0.0	0.0	0.0	3.1	12.5	22.5
Hiring rate (%)	9.9	19.3	0.0	0.0	0.0	3.1	13.1	24.6

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 $S_t = \{\$0, \$300, \$600\}$.

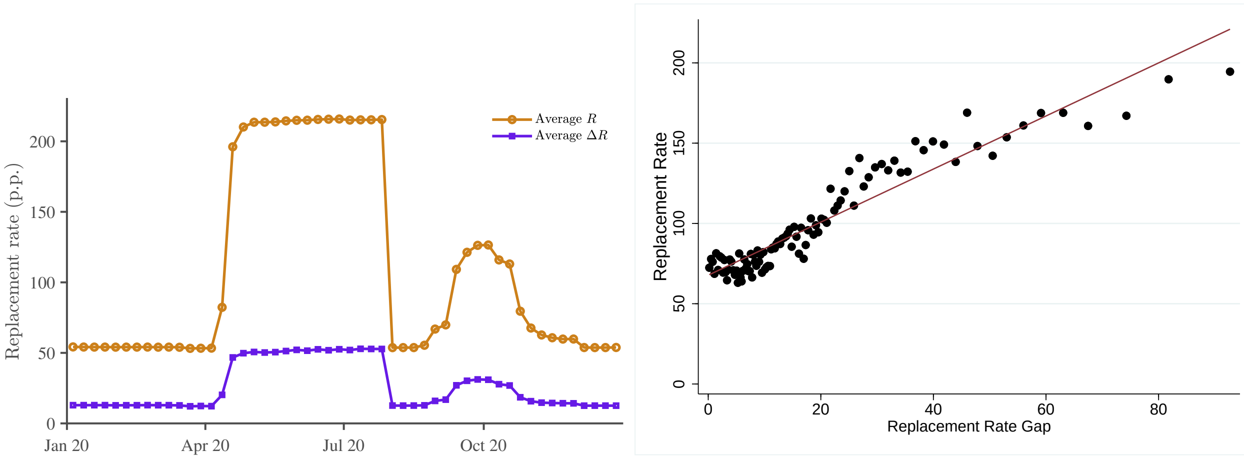
5. We divide the total weekly supplement $b(\overline{w}_{c,t_0}) + S_t$ by the weekly earnings in business j, equal to $w_{j,c,t_0} \times 7 \times 5$. This gives us the business-replacement rate $R_{j,c,t}$ which measures the amount of UI an unemployed would receive in labor market c and period t relative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

Table C-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	0.13	0.41	0.43	0.47	0.51	0.57	0.66
CARES Act	2.11	0.55	1.54	1.62	1.78	2.00	2.32	2.68
Replacement rate gap (ΔR)								
Normal Times	0.12	0.16	0.01	0.02	0.04	0.07	0.57	0.22
CARES Act	0.51	0.65	0.03	0.08	0.18	0.34	0.61	1.00

Table C-3 reports summary statistics for the cross-sectional distribution of the business-level replacement rate $R_{j,c,t}$ as well as 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.

Figure C-4: 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.

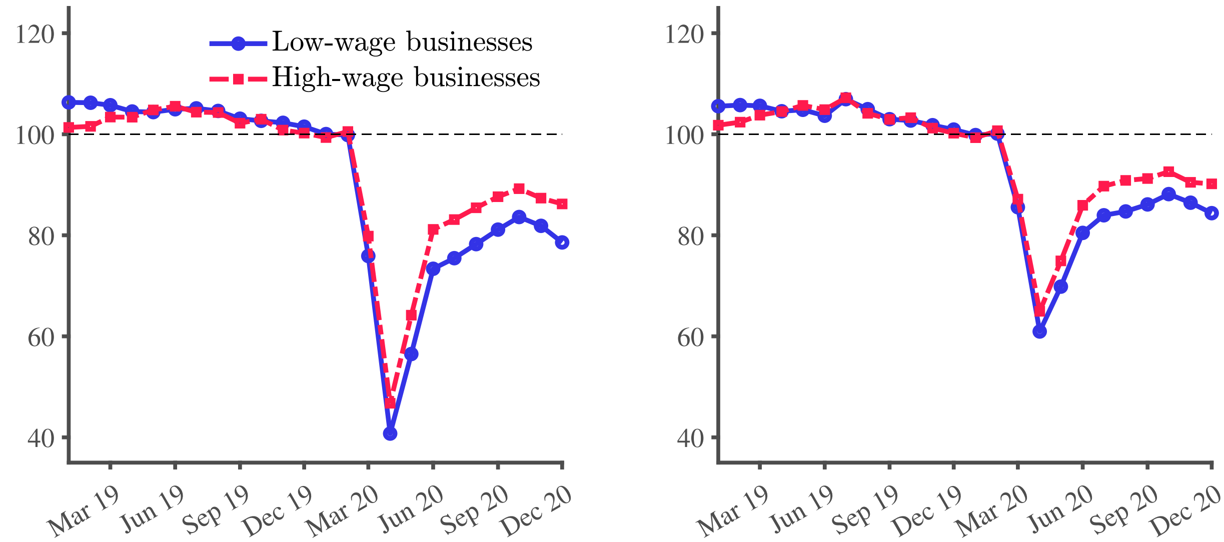
In normal times (i.e., during 2019), the mean replacement rate is 54 percent and the median is 51 percent. During the CARES Act (specifically, between April 2020 and June 2020 where the additional \$600 is in effect), the average replacement rate increased to 2.11 and the median to 2.00. In normal times the replacement rate gap is on average 0.12, with a median of 0.07. With the CARES Act, the replacement rate gap increases on average to 0.51 and for the median labor market to 0.34.

How do the replacement rate R and the replacement rate gap ΔR vary across labor markets and over time? The left panel of Figure C-4 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.

D Trends before and during the pandemic

We plot the raw time series of labor market variables for low- and high-wage businesses during 2019 and 2020. The left panel of Figure D-5 shows employment and the right panel of Figure D-5 shows employment during operating weeks, i.e., without counting the zero observations. Both employment measures are aggregated at the monthly frequency. In these figures, we make two adjustments to the weekly employment data. First, we normalize weekly employment by the average weekly 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. Second, we seasonally adjust the employment data where seasonal factors are computed based on the average monthly number of

Figure D-5: Employment (Left Panel) and Employment Cond. on Operation (Right Panel)



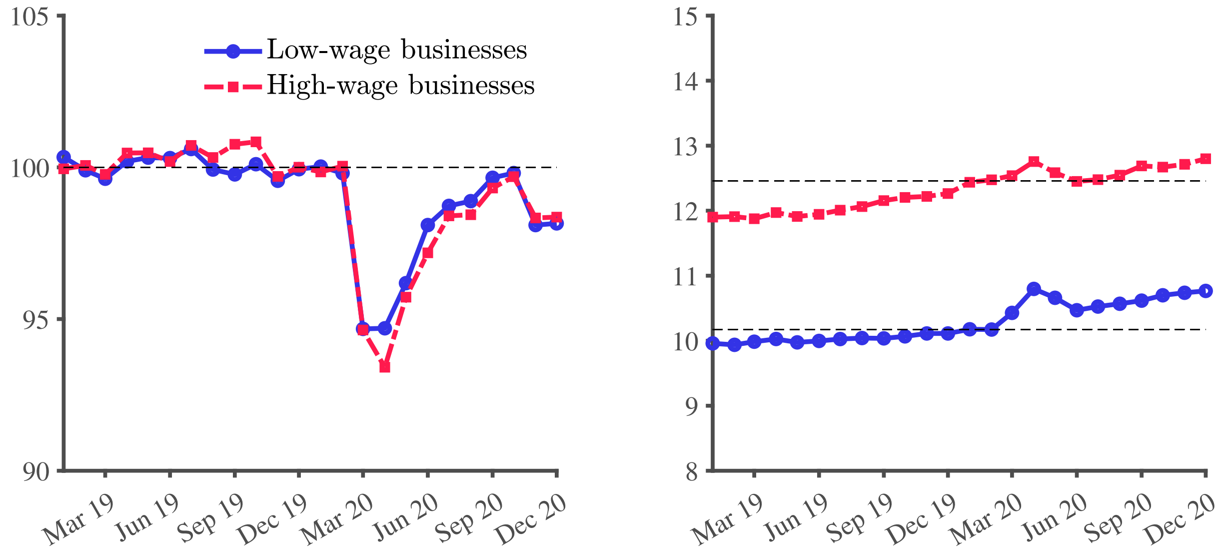
Notes: Monthly averages of the number of employees. The left panel includes days of inactivity and the right panel is average conditional on operation. Business employment is normalized by the average during January and February 2020 and is seasonally adjusted. Low- and high-wage business classification is based on local industry sorting.

employees during 2019.

Before the pandemic, low- and high-wage businesses move broadly together. When the pandemic hits, employment declines to 46 percent of normal in high-wage businesses and around 41 percent of normal in low-wage businesses. Naturally, the decline in the restaurant and retail sector is much larger than the economy-wide decline in employment (around 15 percent). Thereafter, employment recovers, but there remains a sizable gap in recovery rates between high- and low-wage businesses. Specifically, the employment recovery gap is about 8 percentage points between April 2020 and July 2020 (when the \$600 income supplement is in effect) and decreases to about 6 percentage points after July 2020, i.e., when the income supplement expires. This raw comparison of averages coincides with our baseline estimate that the \$600 supplement decreased low-wage employment recovery by 1.5 percentage points. The right panel of Figure D-5 shows average employment during operating weeks. In this case, the employment recovery gap is about 6 percentage points between April 2020 and July 2020 and decreases to about 4 percentage points after July 2020.

The left panel of Figure D-6 plots average hours worked (once more normalized by the January-February 2020 level) while the right panel plots the average hourly wage. Before the pandemic, average hours worked for low- and high-wage businesses coincide, while average hourly wages grew faster for high-wage businesses than for low-wage businesses. As the pandemic hits, average hours worked decline for both types of businesses for a couple of months and then revert roughly back to their trend. Notably, hours recover faster for low-wage businesses up to August 2020. Regarding

Figure D-6: Hour per Employee (Left Panel) and Hourly Wage (Right Panel)



Notes: The left panel shows monthly averages of weekly hours per employee for low- and high-wage businesses. The time series are normalized by the average during January and February 2020. The right panel shows monthly averages of weekly hourly wages for low- and high-wage businesses and is reported in levels.

hourly wages, we observe a temporary hump at the beginning of the pandemic. This hump arises mostly due to selection: when employment declines the highest-paid employees are retained. The pre-pandemic trend in average hourly wages is reversed along the recovery: low-wage businesses increased their hourly wage faster than high-wage businesses. From January 2020 to December 2020, the hourly wage increased 5.9 percent for low-wage businesses and 4.3 percent for high-wage businesses.

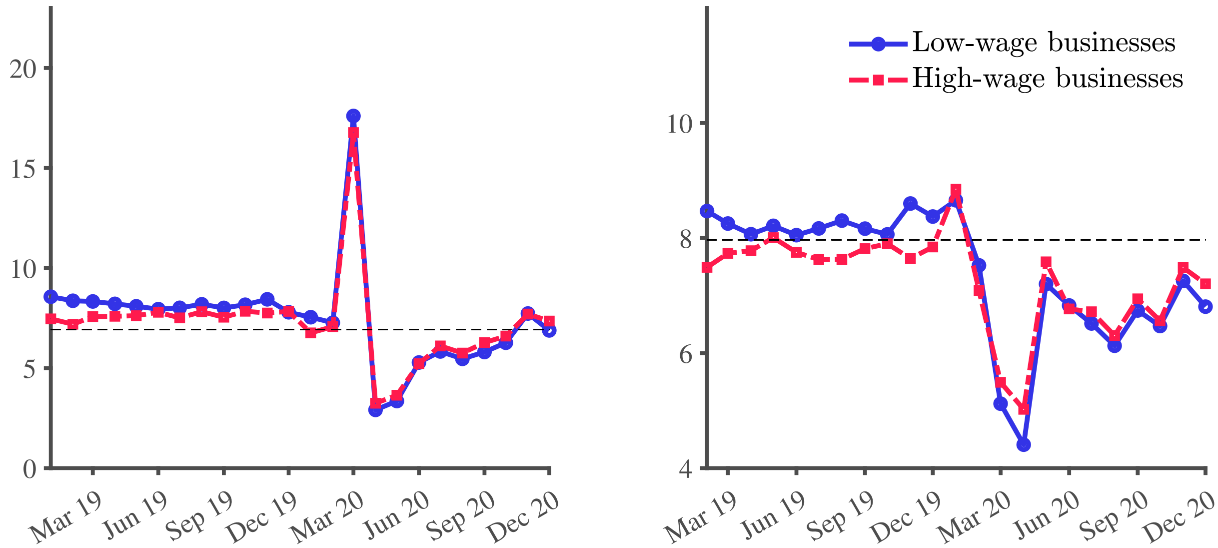
Finally, Figure D-7 shows the weekly separation and hiring rate (averaged over a month). Separation rates are higher for low-wage businesses before the pandemic and so are hiring rates. At the beginning of the pandemic, low-wage businesses experience larger separation rates and lower hiring rates relative to high-wage businesses which explains the divergence in employment recovery between groups.

E Regression results and robustness

We quantify the employment recovery gap between low- and high-wage businesses, by estimating the following regression in the main text:

$$\Delta y_{c,t} = \sum_s b_s \mathbb{1}_{\{s=t\}} + \mathbf{X}'_{c,t}\gamma + \delta \Delta y_{c,t,2019} + \varepsilon_{c,t}.$$

Figure D-7: Separation Rate (Left Panel) and Hiring Rate (Right Panel)



Notes: Monthly averages of weekly separation (left panel) and hiring rates (right panel) for low- and high-wage businesses, reported in percent.

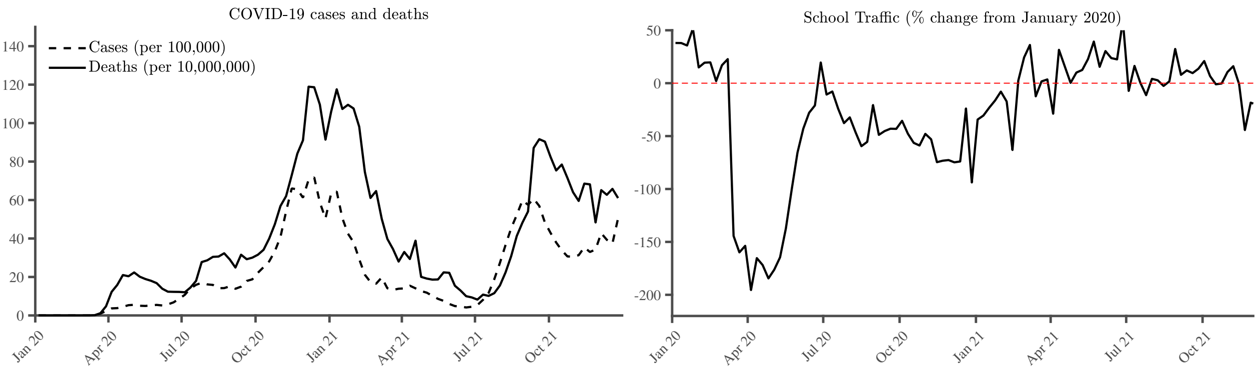
In the text, we discuss how the estimated weekly fixed effects correlate with the pandemic UI supplements. Here we discuss the controls and the seasonal coefficient. The control variables $\mathbf{X}_{c,t}$ include the following variables. First, the number of Covid-19 deaths which are obtained from Covid Act Now (see Kurmann, Lalé, and Ta, 2021). The upper panel in Figure E-8 shows the national time trends for Covid-19 cases (per 10,000,000 population) and deaths (per 100,000 population). Second, the percentage change in visits to schools (right panel in Figure E-8).

Third, our controls include the difference in the number of weekly customers between the low- and high-wage businesses in cell c (relative to the base period of January-February 2020). Both schooling and customer traffic are based on cell phone traffic data from Safegraph.

Fourth, we include the difference in the price range between low- and high-wage businesses as measured in Yelp. Finally, we control for the employment gap in variable y between low- and high-wage businesses for local industry c at the same week t of 2019 (denoted as $\Delta y_{c,t,2019}$). Table E-4 shows the estimates from Regression 8.

Table E-5 reports estimates for the various robustness exercises described in Section 4. First, we restrict the sample to the businesses that continuously operated during the pandemic. Second, we sort businesses into low- and high-wage based on OTC-adjusted wages, as described in Appendix A. Third, we examine the estimates when we look at a broader sample of businesses. Specifically, we keep businesses for which we could not find information on their price and quality from Yelp. Fourth, we choose a wider base period of July 2019-February 2020. Fifth, we weigh each cell by the number of businesses inside the cell; i.e., more populated cells take a higher weight.

Figure E-8: COVID-19 Cases and Deaths and School Traffic



Notes: The left panel shows Covid-19 reported cases and deaths and the right panel shows percent change in school traffic.

Table E-4: Estimates from Regression 8

$\Delta y_{c,t}$	Estimate	S.E.
Weekly dummies	Figure 3	–
Covid-19 deaths (per 100,000 pop)	1.29^{***}	(0.37)
School traffic (% change)	1.16^{***}	(0.38)
Yelp Price Range	-3.14^{***}	(0.24)
$\Delta y_{c,t,2019}$	2.27^{***}	(0.39)
Customer visits	1.12^{***}	(0.19)
# Observations	97,546	

Notes: Estimates from regression 8 including the control variables. Observations are at the local industry/week level. We winsorize variables at the one percent. We cluster standard errors at the local industry level.

F 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:

Table E-5: Alternative Empirical Specifications

$\Delta y_{c,t}$	Employment gap	Hours per employee gap	Hourly wages gap
	(1)	(2)	(3)
Benchmark	-5.73***	0.39	1.52**
	(1.34)	(0.62)	(0.63)
Continuously open businesses	-3.94**	0.99	2.59***
	(1.58)	(0.70)	(0.78)
Sorting with OTC-adjusted wages	-3.70***	0.69*	1.02**
	(0.89)	(0.42)	(0.50)
Broader sample	-6.62***	0.97	1.54***
	(1.24)	(0.61)	(0.57)
Larger base period	-5.22***	0.65	1.54***
	(1.35)	(0.67)	(0.67)
Weights	-6.04***	0.27	0.73
	(1.21)	(0.57)	(0.67)

Notes: Each line reports the estimates from regression 9 for an alternative specification. We winsorize variables at the one percent and cluster standard errors at the local industry level.

1. The value functions $\{W_t, U_{R,t}, U_{N,t}\}_{t=0}^{\infty}$ satisfy equation (10), equation (11), and equation (12), for all t.
2. The reservation wages $\{w^*_{R,t}, w^*_{N,t}, w^*_{\min,t}\}_{t=0}^{\infty}$, satisfy equation (13), and equation (15), 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 (16), equation (17), equation (18), equation (19), equation (22), and equation (23) for all t.
4. The acceptance rate $\{F_t(w)\}_{t=0}^{\infty}$ satisfy equation (24), for all t.
5. Market tightness $\{\theta_t\}_{t=0}^{\infty}$ is given by $\tilde{v}_t/u_t$, for all t

G 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 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.

H 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:

$$ED_{R,t}(w) = \sum_{\tau=0}^{\infty}\Big[\xi_{R,t+\tau}(w) f_{R,t+\tau}(w) + \xi_{N,t+\tau}(w) f_{N,t+\tau}(w)\Big] \times (\tau+1). \tag{33}$$

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

$$ED_{N,t}(w) = \sum_{\tau=0}^{\infty} \zeta_{t+\tau}(w)\, f_{N,t+\tau}(w) \times (\tau + 1)\,. \tag{34}$$

Let $\varepsilon^{U}_{i,t}$ 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:

$$\varepsilon^{U}_{i,t}(w) = \frac{\frac{\tilde{ED}_{i,t}(w)}{ED_{i,t}(w)} - 1}{\frac{\tilde{x}}{x} - 1}. \tag{35}$$

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

$$\varepsilon^{U}_{t} = \frac{1}{u_t} \int \left(u_{R,t}(w)\, \varepsilon^{U}_{R,t}(w) + u_{N,t}(w)\, \varepsilon^{U}_{N,t}(w)\right) dw \tag{36}$$

I 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:

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

where $U^1_{R,t+1}$ and $U^1_{N,t+1}$ 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:

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

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:

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

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:

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

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

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:

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

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

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:

$$\begin{aligned} e_{t+1}(w) = (1-\delta^s)\,e_t(w) + (1-\delta^p)\,r\mathcal{A}_{N,t}^{0}(w,w)\left(u_{N,t}^{1}(w) + u_{R,t}^{1}(w)\right) \\ + \lambda(\theta_t)\frac{\tilde{v}_t(w)}{\tilde{v}_t}F_t(w)\,\bar{u}_t \end{aligned} \tag{37}$$

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

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

The transition equations for unemployment are given by:

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

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

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

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

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:

$$e_t(w) + u^1_{N,t}(w) + u^1_{R,t}(w) + v_t(w) = g(w)\, M. \tag{38}$$

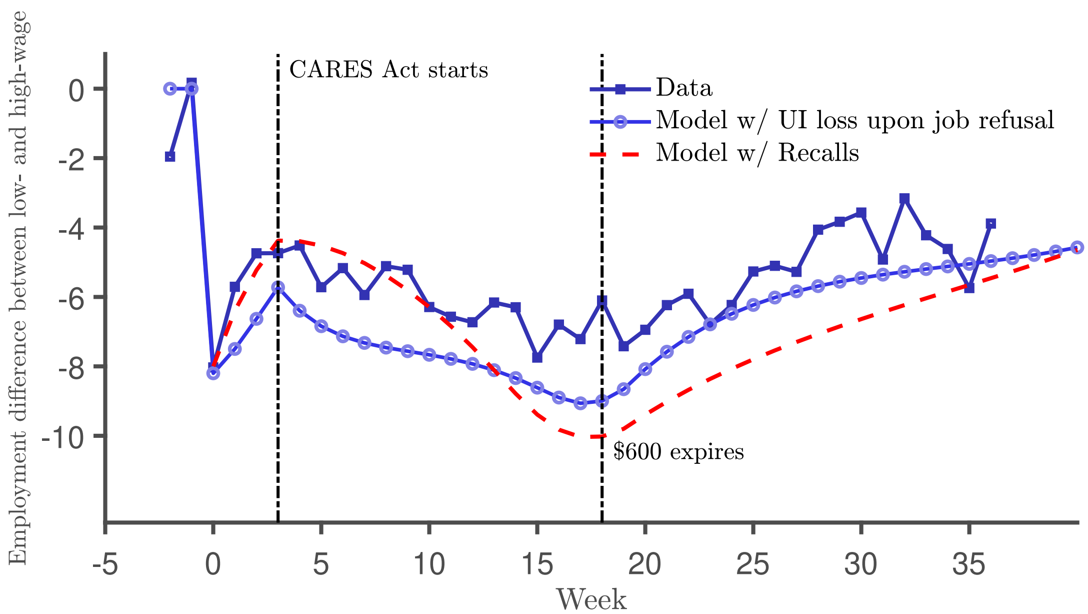
Labor market tightness is given by:

$$\theta_t = \frac{\tilde{v}_t}{\tilde{u}_t} \tag{39}$$

We set the weekly job separation rate as $\delta^s = 0.016$ as in the baseline. The matching technology parameter $\kappa = 0.15$ 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. Given that the average duration of recalls in HB data is about 4 weeks, we set $r = 0.23$.

The effective probability of losing UI upon job refusal in the model with recall is $(1-\delta^p)r = 0.19$, i.e., the probability a worker stays in touch with the employer and that the employer recalls the worker back to work. In the reduced-form model in the main text, the probability of losing UI upon job refusal is equal to $\chi = 0.10$ 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. To highlight this argument, Figure I-9 plots the employment recovery gap in the data, in the model with UI loss upon job refusal and in the model with recalls. The model with recalls can also capture closely the dynamics of the employment recovery gap.

Figure I-9: Employment Recovery Gap: Model with Recalls



Notes: The figure plots the average low-wage business employment minus the average high-wage business employment (both relative to their pre-pandemic level) in the model with the probability of losing UI upon job refusal and the model with recalls. Data represent the estimated b's from Regression 8.