Disincentive Effects of Pandemic Unemployment Benefits*

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Unemployment insurance (UI) benefits act as a disincentive to labor supply but also act as a demand stimulus. In equilibrium, the two effects combine, which may explain why numerous studies have found only a modest negative labor market response to the generous pandemic UI benefits. Using matched employee-employer data on the restaurant and retail sector from Homebase, we show that the employment recovery of low-wage establishments was significantly slower than the recovery of high-wage establishments in local industry markets with relatively more generous UI benefits. As local stimulus is shared between neighboring establishments, our estimation picks up the disincentive effects of UI. We build a quantitative model of labor search with heterogeneous firms and workers. When we calibrate the model to micro-level estimates of the elasticity of unemployment duration to UI, we can replicate the macro-level disincentive effect of UI we document in the data.

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1 Introduction

Between March 2020 and April 2020 the U.S. economy shed 22 million jobs, a consequence of stay-at-home orders and business restrictions to slow down the spread of COVID-19. In response, the U.S. embarked on an unprecedented expansion of unemployment insurance (UI) programs. Beginning with the March 2020 CARES Act and continuing to various extents through the end of Summer 2021, the federal government enlarged and extended eligibility criteria for the unemployed to receive UI and supplemented state UI benefits so that for many recipients, UI payments considerably exceeded what they had earned in their previous job (e.g. Ganong et al. 2020).

Naturally, these policy interventions have fueled a vigorous debate about the extent to which UI supplements have slowed down the employment recovery. Nonetheless, several studies have found only modest negative responses of employment, job search, and vacancy creation (e.g., Altonji et al. 2020; Coombs et al. 2021; Ganong et al. 2021; Marinescu et al. 2021) concluding that the disincentive effect of UI benefits are small or at least much smaller than what standard search models predict.

In this paper, we argue that the disincentive effects of pandemic UI are sizable and that relatively standard search models can effectively reproduce these estimates. Our main argument is that UI benefits not only act as a disincentive to supply labor. They also act as an automatic stabilizer that, if sufficiently generous, may stimulate demand by raising disposable income of the unemployed, thereby helping the employment recovery (e.g., Kekre, 2021). In equilibrium, the two effects combine, which may explain why the above studies have found only relatively small negative labor market responses of pandemic UI benefits.

We disentagle these two countervailing forces using a novel empirical design. The main idea is to analyze whether low-wage establishments recovered slower than high-wage establishments in local industry markets with relatively more generous UI benefits. According to standard job search theory, establishments paying lower wages should have a greater difficulty attracting workers in the presence of generous UI supplements and hence, experience a slower employment recovery than establishment paying higher wages for otherwise identical jobs. Local demand shifts, arising potentially from higher UI benefits, cancel out in our estimation, as the local stimulus is shared between the neighboring stores. Therefore, our estimation picks up the disincentive effect of UI benefits.

To that end, we use data from Homebase (HB), a scheduling and payroll administration provider used by more than 100,000 small businesses in the U.S. The data provides us with a unique worker-establishment matched panel of daily data on employment, hourly wages, and hours worked as well as measures of newly posted vacancies and number of applications per vacancy. As shown in Kurmann et al. (2021) and documented further below, the HB data is highly representative of the type of low-wage workers in service sector establishments that were most affected by the pandemic and that benefited most from the pandemic UI benefits.

We document several new facts about the labor market recovery from the pandemic. First, establishments that paid relatively high wages (within a given local-industry cell) prior to the pandemic regained employment faster than low-wage establishments. Second, despite the faster employment recovery, hours per worker in high-wage establishments increased by less than in low-wage establishments. Third, hourly wages paid by low-wage establishments grew at a faster pace than the ones paid by high-wage establishments, thus partially closing the pre-pandemic wage difference. These combined findings suggest that low-wage establishments faced stronger labor supply constraints and reacted to these constraints by increasing hours worked of their existing employees and by raising wages at a faster pace.

Next we assess how much of the difference in the employment recovery gap between high- and low-wage establishments within a given local-industry cell can be explained by pandemic UI benefits. We find sizable effects. According to our preferred specification, the \$600 pandemic supplement under the CARES Act, decreased low-wage employment by 5 percent more than high-wage employment. This estimate is ar order of magnitude larger the existing estimates which supports our notion that our estimate represents more closely the disincentive effect of UI benefits. The negative employment impact for low-wage establishments is also confirmed by looking around the introduction and expiration of the different rounds of pandemic UI supplements. For example, in July 2020, when the initial \$600 supplement expired, the employment recovery gap between high- and low-wage establishments was around 8 percent. Three months later, the gap had declined to 4 percent.

The causal interpretation of these estimates hinges on differences between high- versus low-wage establishments not being driven by underlying characteristics or confounding shocks that affect differentially the two groups. We show that high- versus low-wage stores in the same local-industry cell have parallel trends in employment, average hours, and separation/hiring rates prior to the pandemic. In addition, we control for other plausible local return-to-work hurdles such as Covid-19 health risk and school closings. Finally, it seems unlikely that high-wage stores attracted disproportionate customer traffic during the recovery: as we discussed, both hours per worker and hourly wages grew faster post-pandemic for low-wage stores.

Our main identification assumption is that local stimulus is shared between low- and high-wage neighboring stores (e.g., restaurants in downtown Manhattan). While we cannot explicitly test this assumption, we show that in broader geographical areas and sectors (e.g., restaurants in the whole N.Y. state) the estimates become small and insignificant. In broader areas, local stimulus is less likely to be shared across establishments so that the estimated effect also captures the stimulative effects of UI, as well as, potentially other demand confounding factors including region- and sector-specific shocks.

Can a model of labor search replicate the disincentive effect we document in the data? It is important to ask this question as recent research has argued that relatively standard job search models cannot fit to the "small" estimated equilibrium effects of pandemic UI benefits (Boar and Mongey, 2020; Ganong, Greig, Noel, Sullivan, and Vavra, 2021). Nonetheless, these papers evaluate the models based on the overall effect of UI and not the independent estimate of the disincentive effect. Naturally, we explore whether a search model can match the larger disincentive effects that we estimate based on our methodology.

The model is a quantitative equilibrium job-search model with firm wage posting. There are heterogeneous workers and heterogeneous firms. Each firm tries to fill a single job position by posting a wage. Workers randomly search for vacant jobs and accept a job offer if the wage is higher than the reservation wage. Since UI benefits are a ratio of past labor market earnings, the reservation wage distribution is an equilibrium object, i.e., it depends on the workers' individual history (similar to Ljungqvist and Sargent, 1998, 2008). We discipline the reservation wage distribution based on the empirical micro-level estimates of the elasticity of duration of unemployment to unemployment benefits extension.

We introduce in the model a large separation shock, i.e., a Covid-19 shock, combined with an increase in the generosity of the unemployment insurance benefits equal to the pandemic supplements. We solve for the transitional dynamics as the economy returns to normal. The model generates a slower recovery for low-wage firms relative to high-wage firms that is qualitatively and quantitatively close to the patterns from the Homebase data. The model predicts that the UI benefits decreased employment in low-wage stores by 2.8% while 0.2% for high-wage stores. The relative effect of –2.5% is within the range of our estimates.

Our paper contributes to the ongoing debate on the consequences of pandemic UI benefits for the labor market recovery. According to many of the existing studies, there is little evidence these supplements discouraged people from returning to work. Coombs et al. (2021) compare the exit rates in 19 states that withdrew early from the pandemic UI benefits in the summer of 2021, compared to 23 states that retained the benefits, and find that in the absence of pandemic benefits, employment would be 0.3 percentage points higher. Ganong, Greig, Noel, Sullivan, and Vavra (2021) use bank-account data and document that the job finding rate increases by 0.76 percentage point when the $600 supplement expired (in the summer of 2020). The implied employment losses are between 0.5-0.7%. Petrosky-Nadeau and Valleta (2021) also estimate that the $600 supplement had modest negative impact on the monthly job finding rate.

We argue that these studies estimate the overall effect of UI arising from both the disincentive effects and the stimulative effects. Our paper develops a methodology to estimate the disincentive effect independently. Although the policy relevant statistic is the overall effect, there are many reasons why independent estimates of the separate effects are informative. First, it is important to know if small negative effects of UI arise because both the disincentive effect and the stimulative effect are moderate or because large disincentive effects are counteracted by equally large stimulative effects. We find that it is the latter: the disincentive effects of pandemic UI turn out to be quite sizable indicating that labor markets with higher replacement ratios also benefited significantly by the local demand stimulus. Second, independent estimates of the disincentive effects are useful because they help discipline or evaluate macroeconomic models of job search. We find that our larger estimated negative employment effect is broadly in line with a quantitative job search model with heterogeneous workers and firms.

Methodologically, the closest paper to ours is Hagedorn, Karahan, Manovskii, and Mitman (2013) who use a border county pair econometric design to analyze the effects of an increase in the duration of benefits during the Great Recession. In our design the border is virtual and represented by the mean of wages that divides establishments in low- and high-wage groups. Although Hagedorn, Karahan, Manovskii, and Mitman (2013) emphasized the distinction between micro and macro effects of UI benefits, we emphasize the distinction of (macro) disincentive versus (macro) stimulative effects.

Hagedorn, Karahan, Manovskii, and Mitman (2013) find that permanently increasing duration from 26 to 99 weeks increased unemployment rate from 5% to 9%. On the other hand, Dieterle, Bartalotti, and Brummet (2020) and Boone, Dube, Goodman, and Kaplan (2021) show that the same research design but accounting for cross-border spillovers or using a different dataset and a longer time period, results into small negative macro UI effects. Subsequent research that uses data on job applications and variation in real-time measurement error of the unemployment rate also point to a small macro effect of unemployment insurance (e.g., Marinescu, 2017; Chodorow-Reich, Coglianese, and Karabarbounis, 2019). None of the aforementioned studies speak to the distinction between disincentive and stimulative effects of policies. Moreover, it is difficult to make comparisons between the Great Recession and the pandemic economic episode. We find it plausible that unemployed workers respond more to a generous increase in their replacement ratios as with the pandemic UI policies relative to simply an extension of the duration of their benefits as in the UI policies that took place during the Great Recession.

The paper is organized as follows. Section 2, describes the empirical design and the data. Section 3 documents the labor market patterns regarding the recovery of low- and high-wage stores. Section 4 estimates the disincentive effect of pandemic UI benefits on the employment recovery. Section 5 sets up the quantitative model. Section 6 describes the main model experiments and Section 7 concludes.

2 Local-industry Research Design and Data

This section provides a brief overview of the expansion of UI programs during the pandemic. Then, we describe our local-industry research design and introduce the data used for estimation.

2.1 Pandemic unemployment insurance

The 2020 CARES Act that was signed into law on March 27, 2020 set off an unprecedented expansion of UI programs in the U.S. Eligibility for UI was extended to self-employed and gig workers through the Pandemic Unemployment Assistance (PUA) program; benefit duration was increased by an additional 13 weeks beyond state benefit exhaustion through the Pandemic Emergency Unemployment Compensation (PEUC) program[1]; and from the beginning of April 2020 through the end of July 2020, everyone who qualified for UI received an additional $600 in weekly benefits through the Federal Pandemic Unemployment Compensation (FPUC) program.

As Ganong et al. (2020) estimate, this $600 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. Most claimants received these benefits only with several weeks of delay as the massive increase in jobless claims in the beginning of the pandemic led to large backlogs in state UI office approving and processing the payments. However, claimants typically received backpay for delayed payments with their first check.

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 and ran from August 1 to September 5, 2020. This additional supplement was administered through the Federal Emergency Management Agency, which resulted in processing delays and meant that payment in many states occurred only after the September 5 expiration.

In late December 2020, Congress passed another round of UI benefits of $300 per week as part of the Consolidated Appropriations Act that took effect in 2021 and lasted through March 2021. As part of the American Rescue Plan passed in March 2021, this $300 weekly supplement was then further extended through September 6, 2021 together with expanded eligibility provisions. However, starting in June 2021, several states started to opt out of

[1] Since most states themselves increased UI duration, this meant that eligible workers did not exhaust benefits until at least the end of 2020.

these extensions out of concern that they unnecessarily reduced labor supply and held back the employment recovery.

2.2 Local-industry research design

Identifying the disincentive effects of UI in the data is challenging. As documented by Ganong et al. (2021), recipients of pandemic UI benefits quickly spent a large portion of their benefits, thereby stimulating demand for goods and services. If part of this spending occurred at the local level, then the stimulative effect is larger in places with higher UI replacement ratios, implying a positive demand effect that counteracts and potentially even outweighs the negative disincentive effect.

We develop two empirical specifications to disentagle these two counterveiling forces. Let j denote a store and c denote a geographical cell that j belongs to (e.g., zip code, county etc.). For simplicity, we assume that in each cell there are two stores, low-wage store j and high-wage store j'.

In our first specification, the replacement rate of cell c at time t is $R_{c,t}$. Employment of store j in time t, $y_{j,c,t}$, is influenced jointly by the disincentive effects of $R_{c,t}$ on labor supply but also the stimulative effects of $R_{c,t}$ on consumer demand. More formally, assume that the replacement rate, $R_{c,t}$, affects employment of j at time t, $y_{j,c,t}$, through the following specification

$$y_{j,c,t} = \beta_j \times R_{c,t} + \eta_{j,c,t}, \tag{1}$$

where β_j is the overall response of store j's employment to the replacement ratio and $\eta_{j,c,t}$ is the error term. For illustrative purposes we can decompose β into a supply effect (β_S) and a demand effect (β_D) with $\beta_j \equiv \beta_{j,S} + \beta_{j,D}$.

According to standard labor search models, establishments paying lower wages should have a greater difficulty finding workers relative to the high-wage establishments of the same local cell c. Hence, the labor supply effect, $\beta_{j,S}$, is j-specific. The main assumption in our methodology is that low- and high-wage stores share the stimulative effects of UI. This is plausible for neighboring stores that are members of the same narrow local industry market. Formally, we assume that $\beta_{j,D} - \beta_{j',D} \to 0$ as the distance between the stores $d(j, j') \to 0$. We explicitly test this assumption by considering the effects of UI at different levels of geographical and sectoral aggregations.

Given these assumptions we can take the difference between stores j and j' within cell c:

$$\Delta y_{c,t} = \Delta\beta \times R_{c,t} + \Delta\eta_{c,t} \tag{2}$$

where $\Delta y_{c,t} \equiv y_{j,c,t} - y_{j',c,t}$ and $\Delta\beta \equiv (\beta_{j,S} - \beta_{j',S})$. Since the stimulative effects of the UI affect equally stores j and j' they are now cancelled out. Note that this method allows us to identify the differences in the disincentive effect of store j relative to group j'. To estimate the average disincentive effect we rely on a structural model.[2]

Our second specification follows more closely the border county pair methodological design (see for example, Dube, Lester, and Reich, 2021). The starting point of this specification is the regression:

$$y_{j,c,t} = \beta^S \times R_{j,c,t} + \underbrace{\beta^D \times \bar{R}_{c,t}}_{=u_{c,t}} + \eta_{j,c,t},$$

where $R_{j,c,t}$ is the replacement rate of the low- or high-wage labor market and $\bar{R}_{c,t}$ is the mean replacement rate in cell c.

As before, the main assumption in our methodology is that low- and high-wage stores share the stimulative effects of UI which is plausible for neighboring stores that are members of the same narrow local industry market. In that case, taking the difference between stores j and j' gives

$$\Delta y_{c,t} = \beta^S \times \Delta R_{c,t} + \Delta\eta_{c,t}.$$

A separate but equally important issue is whether we can derive an unbiased estimate. For an unbiased estimate of β, Regression 1 requires that $E[R_{c,t}, \eta_{j,c,t}] = 0$. This is unlikely to be the case. According to Chetty et al. (2020), high-income areas experienced a substantially larger decline in consumer spending at the onset of the pandemic and a slower recovery thereafter than lower-income areas, due to factors that are not directly related to UI benefits. Specifically, let $\eta_{j,c,t} = u_{c,t} + \epsilon_{j,c,t}$, where $\epsilon_{j,c,t}$ is a classical error term with $E[R_{c,t}, \epsilon_{j,c,t}] = 0$ while $u_{c,t}$ captures other unobserved local labor market demand shocks that are correlated with $R_{c,t}$; i.e. $E[R_{c,t}, u_{c,t}] \neq 0$. In our regression 2, by assumption, local labor market demand shocks are cell-specific so $\Delta\eta_{c,t} = \Delta\epsilon_{c,t}$, and the requirement for an unbiased estimate simplifies to $E[R_{c,t}, \Delta\epsilon_{c,t}] = 0$.

2.3 Data

The data we use for our estimation is from Homebase (HB), a scheduling and payroll administration provider used thousands of small businesses in the U.S. Most of these businesses are restaurants and retail stores that are individually owned and operated. The data provides us with a unique worker-establishment matched panel of daily data on employment, hourly wages, and hours worked.

The sample of HB data we use extends from January 2019 to December 2021 and contains

[2] Regression 2 can easily be augmented to include control variables $\mathbf{X}_{c,t}$, cell, and time fixed effects. We include such changes in our benchmark specification.

approximately 215,000 unique establishments (stores). We construct a benchmark core sample of establishments based on four restrictions. First, the Homebase data does not include consistent industry classifications for stores. To the extent that different industries experience different employment trajectories during the pandemic, it is important to incorporate information about a store's industry. For this purpose we match the HB records by name and address to (i) Safegraph's Places of Interest (POI) data, which provides us with consistent NAICS-6 industry coding for each establishment; and (ii) Yelp data that includes among other information, a rating for the store and a price range. Kurmann et al. (2021) provide a detailed description of the matching procedure. After matching stores to industries, we are left with 31,812 stores for which we know their location and industry.

Second, we consider a "balanced" sample by requiring that stores are in the sample for the first two years, from January 2019 to December 2020. Hence, we allow for some store attrition in 2021, but this is not significant. In our sample stores will open and close, especially in March and April 2020, but we do require that they will re-open and operate at least until December 2020. Thus, stores that close permanently during the pandemic are not part of the analysis. This restriction leaves us with 9,316 stores.

Third, we only keep stores that use HB to track not only the number of employees but also payroll, that is, we have information on both the stores' employment and wages. This decreases the number of stores to 6,538. Finally, as we discuss below, we group stores in cells defined by their sector and geography. Stores with no pair (single store cells) are dropped from the sample which leaves us with 4,215 stores and a total of 3,677,242 daily worker observations. While this core sample is relatively small it allows us to analyze each establishment along the pre-pandemic period (i.e., to document the pre-trends) and across multiple store characteristics (location, industry, price range, customer reviews etc.). In the robustness section, we discuss our estimates when we relax some of our sample restrictions.

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

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

$$w_{j,c} = a + d_c + \varepsilon_{j,c}. \tag{3}$$

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

We define a locality by its four-digit zip code, which on average includes four neighboring zip-codes. We define an industry at the two-digit level of the North American Industry Classification System (NAICS), with the exception of restaurants and bars (NAICS 722410, 722511, 722513, and 722515), which we define as a separate group. Given our sample, this results in 1,132 local-industry cells with two or mores stores. Most cells have few stores: the 25^{th} percentile cell has 2 stores, the median cell has 3 stores, and the 75^{th} percentile cell has 4 stores. The largest number of stores in a cell is 32.

We measure weekly employment in a store by counting the unique bodies that worked for at least one day during the week in the store. If the store 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. from the CES, the QCEW, or the QWI).

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

Store type	All	Low-wage	High-wage
# Employees per store	6.0	5.5	6.4
Hours worked (per day)	6.6	6.6	6.7
Hourly wage ($)	11.8	10.8	12.7
Separation rate (%)	8.5	8.7	8.3
Hiring rate (%)	8.5	8.7	8.3
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 stores in rural areas	0.30	0.30	0.29
# Stores	4,218	2,037	2,181

Notes: Averages are calculated for operating stores for the period January 2019-December 2021. Low- and high-wage store classification is based on local industry sorting as described in the text.

Table 1 reports statistics by store type (low- vs high-wage within a local-industry cell) averaged over all the days when a store is operating. The average number of employees of 6.0 across all stores indicates that the HB data captures mainly very small stores.[3] Furthermore, low wage stores are on average smaller than high-wage stores. On average, employees work 6.6 hours per day and the hourly wage is $11.8. Even within narrowly defined local industries,

[3] See Kurmann et al. (2021) for further details on HB establishment characteristics.

there is sizable disersion in hourly wages: low-wage stores pay on average 1.9$ less than high-wage stores.

The weekly hiring rate is defined as the number workers that work at week t but not at $t-1$ divided by the number of employed at time t. The weekly separation rate is the number of workers that worked in $t-1$ but not working at t divided by the number of employed at time t. Table 1 reports weekly separation and hiring rates averaged over all the weeks in the sample. The weekly job separation rate in the Homebase data is 8.7% which is an order of magnitude higher than values commonly found in the labor search literature (typically between 2.5 and 3.5% at the monthly frequency). This substantial difference can be explained by the presence of (recalled) workers who miss work for a week or two and return to the same establishment within the same month. Hiring rates are very similar in magnitudes to the separation rates confirming this hypothesis and also suggesting that the average store is on a steady-state level of employment. We introduce recalls in the quantitative model to be able to capture these large separation rates without counterfactual employment dynamics. Separation and hiring rates are slightly higher in low-wage stores although we show that low-wage stores lost their dynamism after the pandemic.

On average, low- and high-wage stores of the local industry are rated similarly, and high-wage stores are slightly more expensive. Our sample consists of 85% restaurants and bars equally divided between high- and low-wage stores. 70% of the stores are in zip codes of metro areas and 30% in rural zip codes.

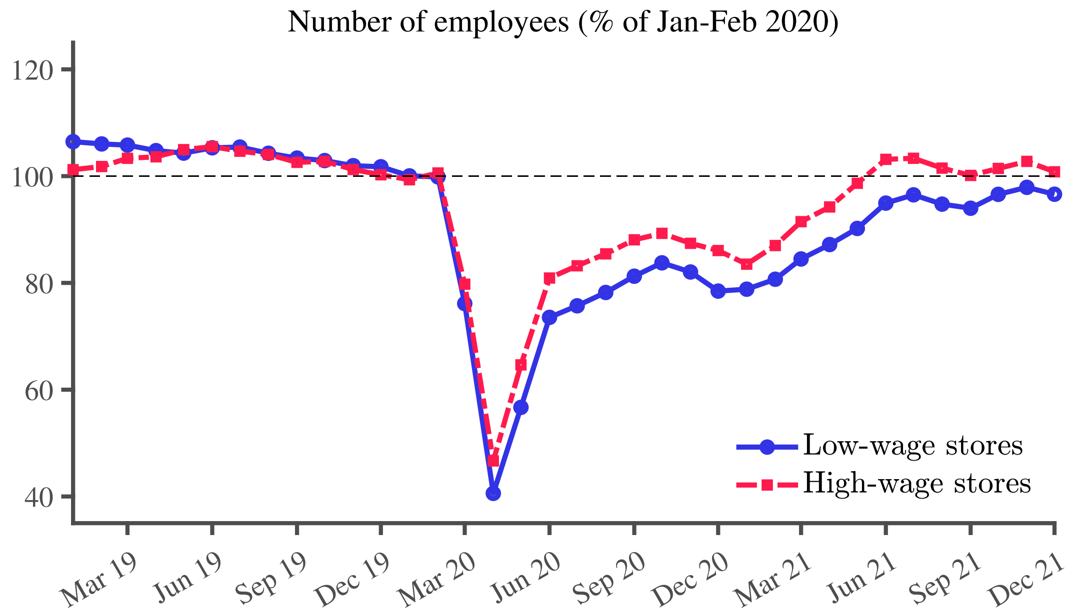
Our data are consistent with several facts from the literature on establishments. First, wages are higher in larger establishments. Second, in high-paying establishments workers work longer hours per day. Third, separations and hires are less frequent in high-paying establishments relative to low-paying establishments.

3 Low- vs. High-Wage Stores During the Pandemic

We start the analysis by considering time series for employment of low- and high-wage stores. Figure 1 shows the average weekly employment, the upper panel of Figure 2 shows average weekly employment during operating weeks, and the lower panel of Figure 2 the share of stores that are closed during a week. All 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 store in our base period, i.e., January-February 2020. This way we can measure how far stores are from their "normal" levels. Second, there are seasonal trends in our sample as stores increase their employment during the months of January to July, and decrease their employment August to

Figure 1: Employment of low- and high-wage stores



Notes: Upper panel hows the monthly averages of number of employees during operating days. Store employment is normalized by the average during January and February 2020 and is seasonally adjusted. Lower panel shows the share of stores that did not operate during a week, averaged at the monthly level.

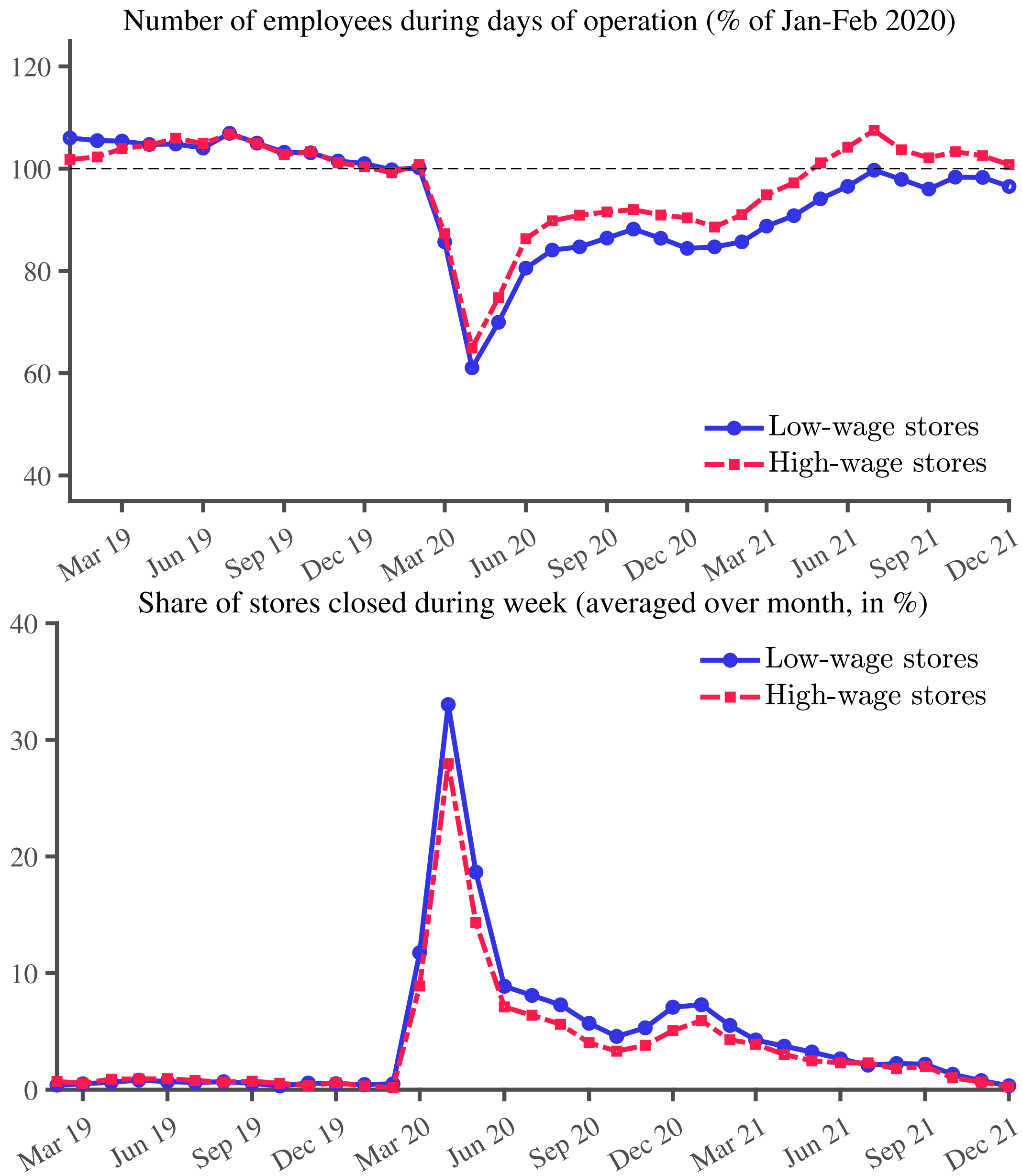
December so we seasonally adjust the employment data.[4]

Before the pandemic, low- and high-wage stores move broadly together. High-wage stores do grow faster during the first months of 2019 which opens the possibility for differential growth paths between the two groups. To account for this, we explicitly control for 2019 pre-trends in our main regression. When the pandemic hits, employment declines to 46% of normal in high-wage stores, and around 41% of normal in low wage stores. Thereafter, employment recovers, but there remains a sizable gap in recovery rates between high- and low-wage stores that generally persists until the end of 2021.

Variations in employment reflect both stores adjusting the number of their employees, conditional on operating, and stores closing down. The lower panel of Figure 2 shows that a larger share of low-wage stores closed during the pandemic relative to high-wage stores. In April 2020, 33% of low-wage stores, were closed during a week in April versus 27% of high-wage stores. Although an important factor, the share of closed stores only partially explains the recovery gap. The upper panel of Figure 2 shows that high-wage stores regained average

[4]We compute the seasonal factors by taking the average monthly number of employees during 2019 and normalizing by the 2019 average. Seasonally adjustment amounts to simply dividing employment in each month by the seasonal factor.

Figure 2: Employment and share of low- and high-wage stores closed

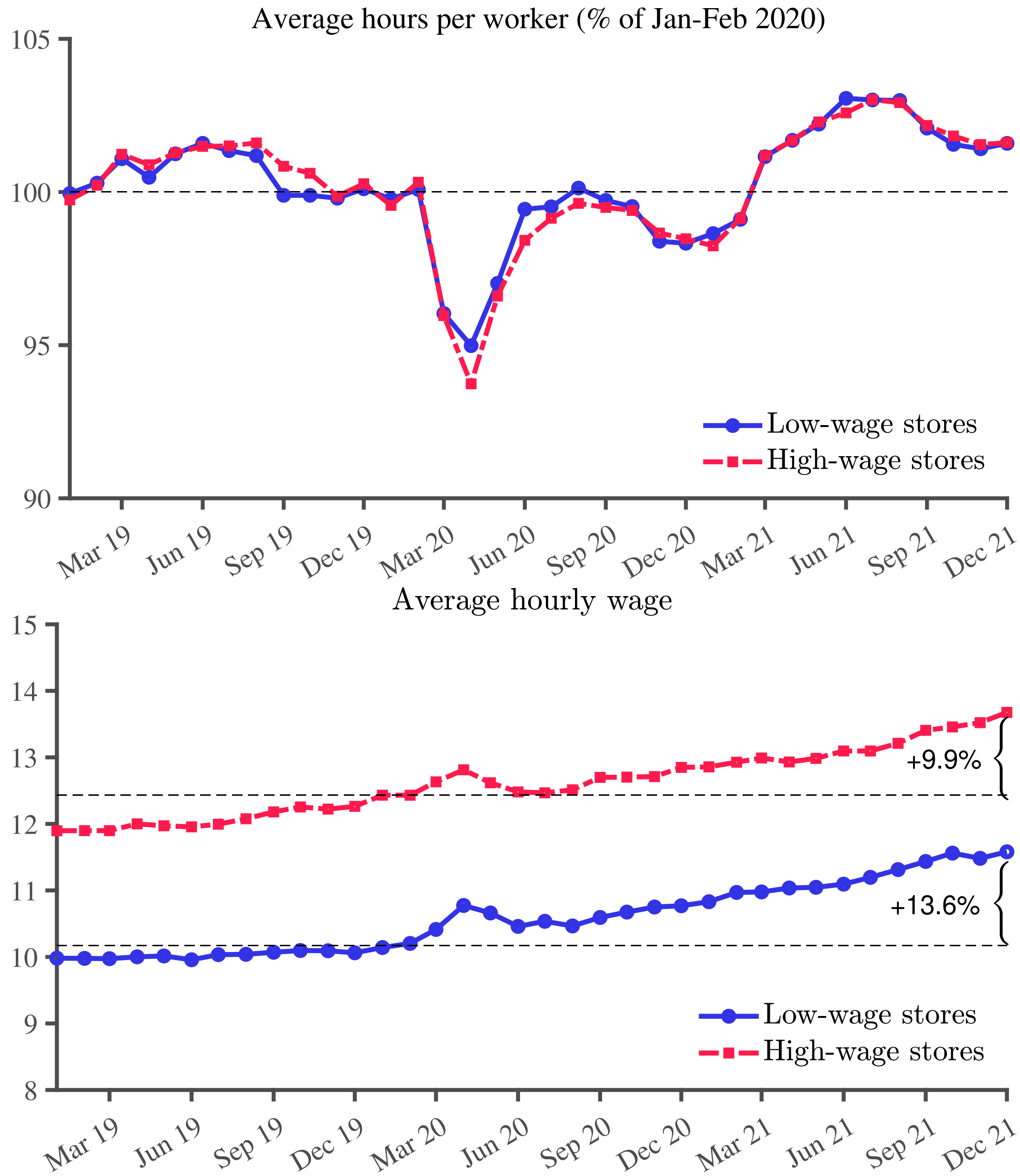


Notes: Upper panel shows monthly averages of weekly store employment for low- and high-wage stores conditional on operation. Store employment is normalized by the average during January and February 2020 and seasonally adjusted. Lower panel shows the weekly fraction of stores open, averaged over the month.

employment during operating weeks substantially faster than low-wage stores.

Figure 3 plots average hours worked (upper panel) and average hourly wage (lower panel) for high- and low-wage stores. Before the pandemic, average hours worked coincide between low- and high-wage stores, while average hourly wages grow faster for high-wage stores than

Figure 3: Average hours per worker and average hourly wage of low- and high-wage stores



Notes: Monthly averages of average hours worked and average hourly wages for low- and high-wage stores. Average hours worked are normalized by the the average in January and February 2020. Average hourly wages are reported in levels (US$). Monthly averages are reported for high- and low-wage stores separately.

for low-wage stores. As the pandemic hits, average hours worked decline for both type of stores for a couple of months and then revert roughly back to their trend. Interestingly, the hours recovery is faster for low-wage stores. For average hourly wages, we observe a temporary hump in the beginning of the pandemic. This hump arises mostly due to selection: when employment declines the highest paid employees are retained. More importantly, the pre-pandemic trend in average hourly wages is reversed along the recovery: low-wage stores increased their hourly wage faster than high-wage stores. From January 2020 to December 2021, the hourly wage increased 13.6% for low-wage stores and 9.9% for high-wage stores.

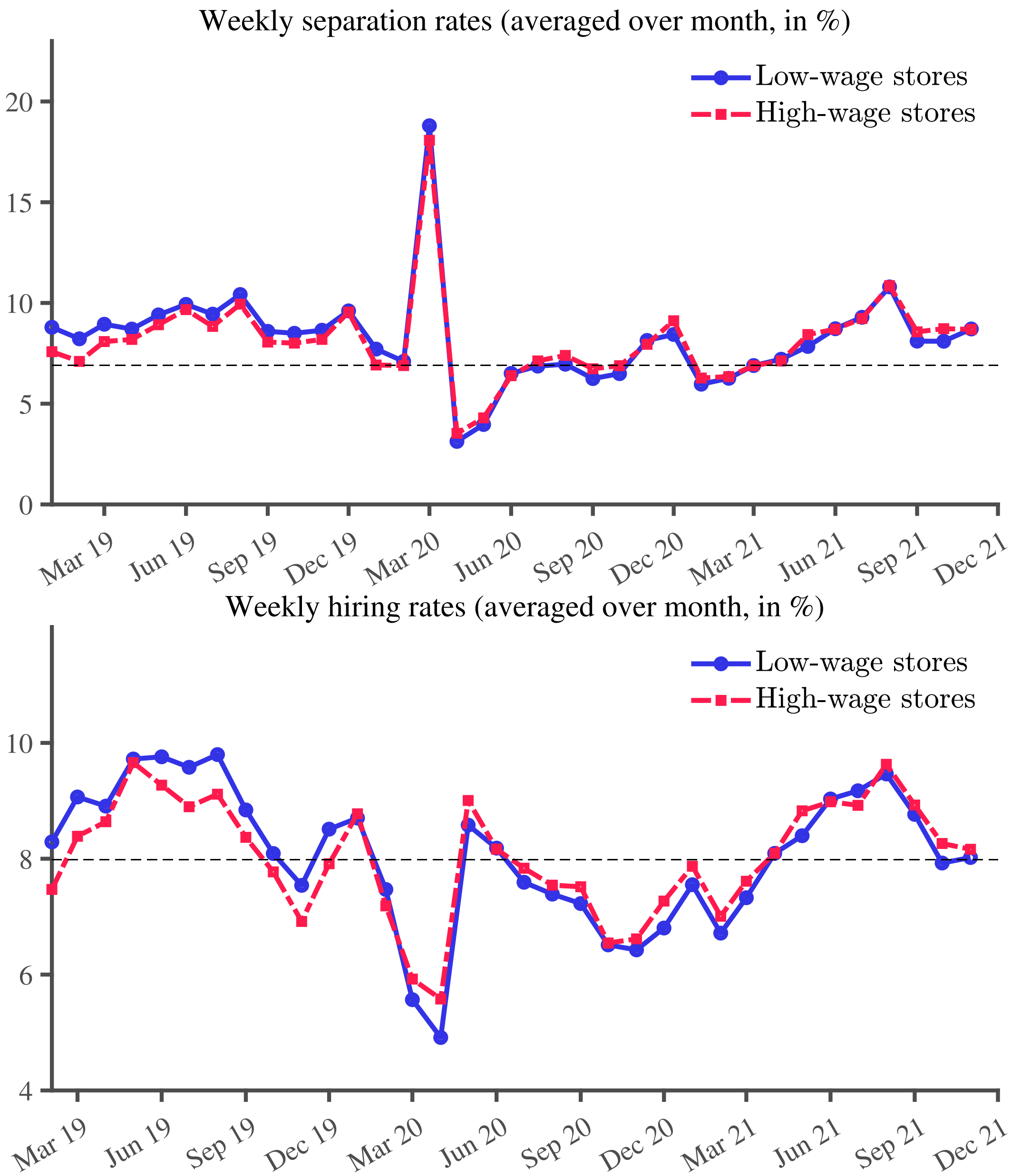
These findings demonstrate the presence of labor supply constraints within our local industries. Along the recovery, low-wage stores attracted relatively fewer employees than high-wage stores. They responded by increasing the average hours of their employees as well as increasing their hourly wage more than high-wage stores. If in contrast, the employment recovery was driven by some group-specific demand shift, e.g., customer traffic returning more toward high-wage stores we would expect to see a faster growth in hours per employee and hourly wages of high-wage stores as well.

Finally, Figure 4 plots the weekly separation and hiring rate averaged over a month. Separation rates are higher for low-wage stores before the pandemic and so are hiring rates. At the beginning of the pandemic, low-wage stores experience a larger separation rates and a lower hiring rates relative to high-wage stores which explains the divergence in employment recovery between groups. Interestingly, high-wage stores become more dynamic relative to pre-pandemic. As separation and hiring rates converge to pre-pandemic levels, the hiring and the separation rate becomes permanently higher for high-wage stores relative to low-wage stores.

We explore now the recovery of low- and high-wage stores for the 30% of stores that closed and re-opened during the pandemic. Figure 5 plots employment, average hours, hourly wage, and the recall rate (the share of employees that have previously worked in the establishment) before and after the stores' closing and the re-opening. We center the plots around re-opening week (denoted as "week 0"). Week -1, depicted as a shaded area in the plots, represents ten weeks before re-opening (during which time most stores were closed).

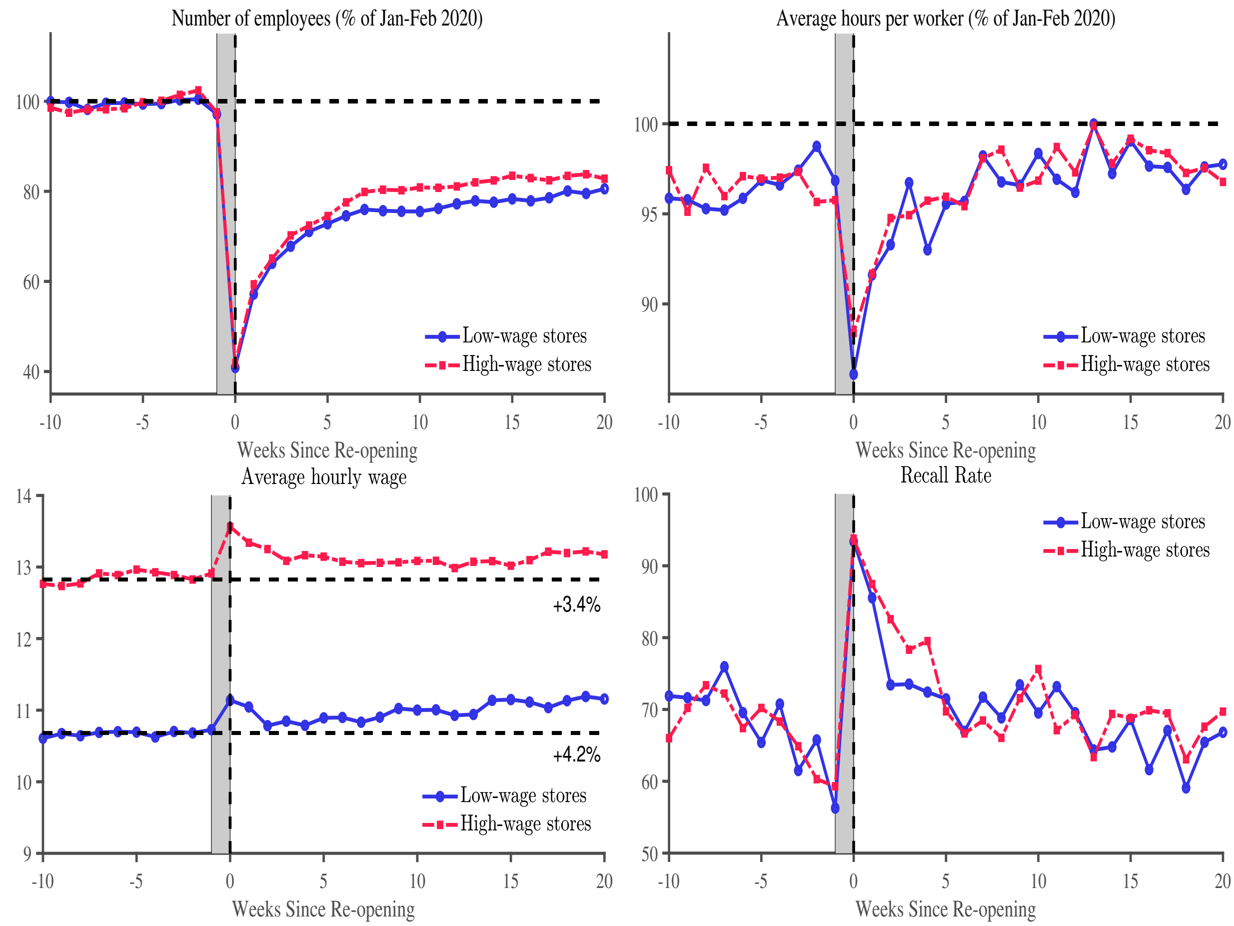
Most of the basic patterns documented for all stores across time broadly hold when we look around the closing and re-opening. First, employment recovered faster for high-wage stores. Second, hourly wages grew faster for low-wage stores. One difference is that hours per employee recover at the same rate for low- and high-wage stores that closed and re-opened. The recall rate is larger for high-wage stores but only during the first month from re-opening (we discuss more the difference between recalled workers and new hires in the next section).

Figure 4: Average separation and hiring rate of low- and high-wage stores



Notes: Monthly averages of hiring and separation rate. Rates are normalized by the the average employment in January and February 2020. Monthly averages are reported for high- and low-wage stores separately.

Figure 5: Dynamics around store re-openings for low- and high-wage stores



Notes: Weekly averages for employment, hours per employee, hourly wage, and recall rates. Week "0" is re-opening week. Closing week differs across stores.

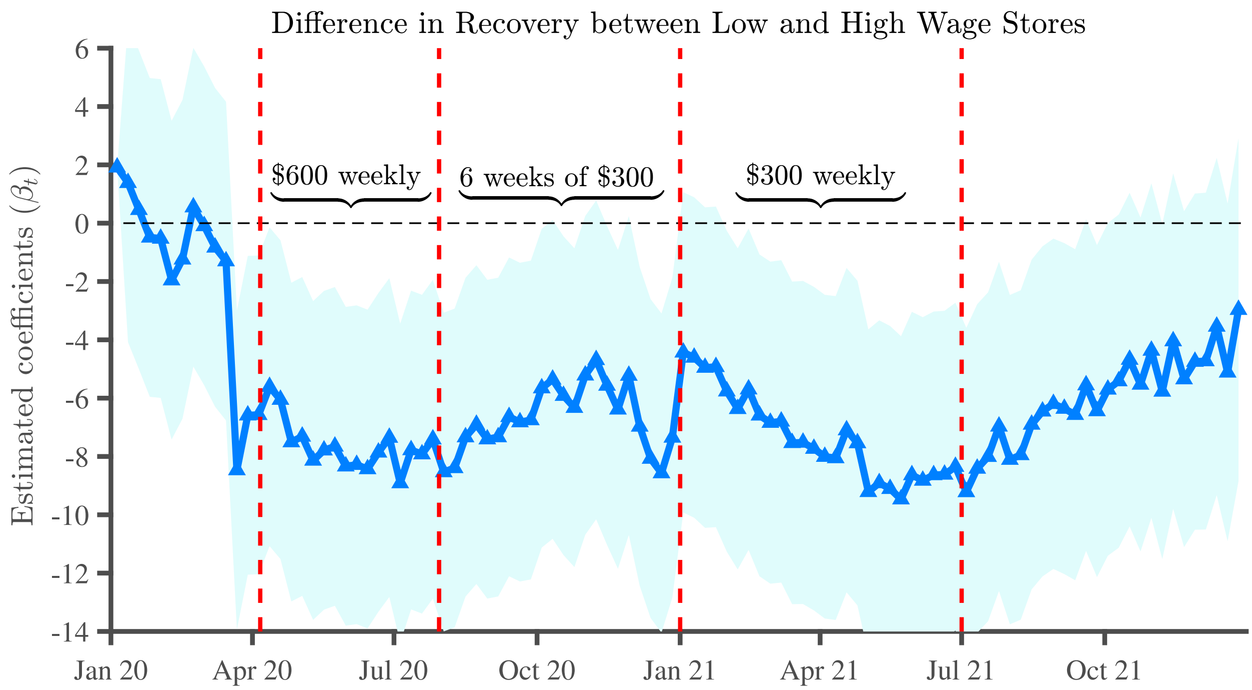
4 Effects of Pandemic UI Benefits on Low vs. High-Wage Stores

We now formally evaluate the extent to which the gap in employment recovery between low- and high-wage stores is related to the relative generosity of pandemic UI benefits across localities.

4.1 Event Study around UI programs

We start by considering the evolution of low- and high-wage employment around the introduction and expiration of the unemployment insurance programs. As described above, weekly UI supplements were handed out in several rounds during the pandemic. Initially, the Federal Pandemic Unemployment Compensation (FPUC) handed out an additional \$600 weekly federal unemployment insurance supplement on top of the usual state unemployment benefits. The program expired in August 2020 and was replaced by the Lost Wages Assistance (LWA) program that handed a weekly \$300 benefit supplement. The program was designed to last for six weeks but states dispersed the benefits not immediately after the expiration of FPUC. Only seven states handed out benefits in August 2020 and most states handed out benefits during the week of September 6^{th} and September 13^{th}. Finally, from January 2021, FPUC and a \$300 weekly supplement was extended up to September 2021. Nonetheless, several states decided to opt out from the program as early as July 2021.

Figure 6: Employment Recovery Around UI Programs



Notes: Estimated b's from Regression 4. The coefficients represent the average low-wage store employment minus the average high-wage store employment (both relative to their pre-pandemic level). Dates of initiation and expiration of the several unemployment insurance programs are reported. The shaded area represents the 95% confidence intervals.

To quantify the employment recovery gap between low- and high-wage stores, we estimate the regression:

$$\Delta y_{ct} = a + \sum_t b_t(\mathbb{1}\{\text{week=t}\}) + u_{ct} \tag{4}$$

where $\Delta y_{c,t}$ is the difference in employment y between low-wage and high-wage stores in cell c and week t, and b_t denotes their difference.

Figure 6 shows the estimates (the b_t's) from equation 4 as well as the 95% confidence intervals. The employment gap (low-wage minus high-wage employment) declines at the onset of the pandemic, before the CARES Act came into effect. So, this initial opening of the gap cannot be attributed to pandemic UI benefits. This is a combination of more low-wage stores closing and of lower employment in continuing low-wage stores (relative to their high-wage counterparts). From April through the end of July, the gap remains largely constant, i.e., low-wage stores do not recover more quickly from their larger initial decline than high-wage stores. Starting with the expiration of the $600 weekly supplement (end of July 2020), the employment gap starts to gradually become smaller, going from around -8 percent to -4 percent by the end of 2020. This means that after the expiration of $600, low-wage stores are catching up with the high-wage stores. Starting January 2021, as the $300 weekly supplement started, the employment gap becomes again more negative, declining back to -8 percent. After July 2021, as different states start to opt out of the pandemic UI programs, the gap then becomes again smaller.

The graphical evidence suggests that low-wage stores are significantly lagging in the recovery of their employment when a weekly unemployment supplement is in effect. In contrast, low-wage stores are catching up with high-wage stores in periods following the expiration of these programs.

Next, we decompose the differential employment recovery into differences in hiring rates and differences in separation rates. Specifically, employment in store j that belongs in cell c, at week t is given by

$$E_{j,c,t} = E_{j,c,t-1} + H_{j,c,t} - L_{j,c,t} \tag{5}$$

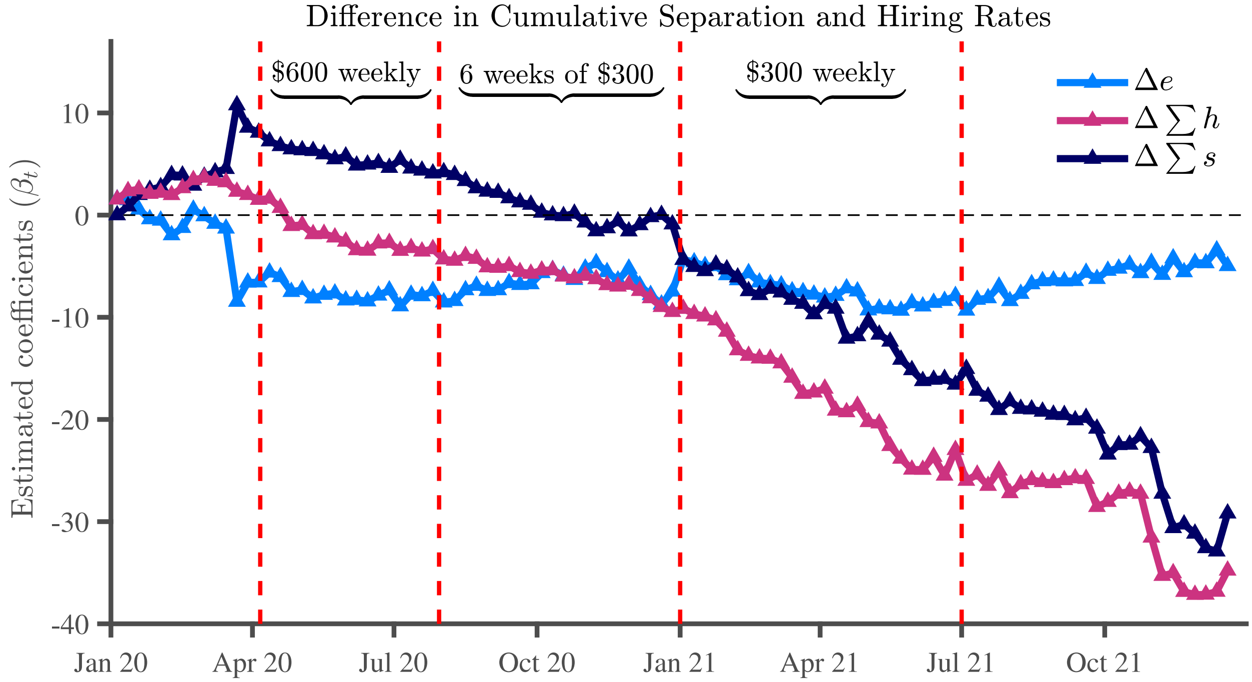
where $E_{j,c,t}$ is the number of employees in week t, $H_{j,c,t}$ is the number of new hires at t, i.e. the workers that work at t but not at $t-1$, and $L_{j,c,t}$ is the number of workers laid off, i.e. that worked in $t-1$ but not working at t. A natural question is if UI has the same impact on hires and separations or if one of these flows is more sensitive to the UI supplements?

We normalize both the number of layoffs and hires of each week for store j by the (average) number of employees per week in our base period, January and February 2020, and derive the hiring rate, $h_{j,c,t}$ and the separation rate $l_{j,c,t}$. Assuming that $t = 0$ is the first week of January 2020, we can thus derive the following equation $e_{j,c,t} \approx 1 + h_{j,c,t} - l_{j,c,t}$. Substituting forward this equation we derive an equation linking the employment-to-normal ratio at some week t to the cumulative hiring and separation rates up to that week:

$$e_{j,c,t} \approx 1 + \sum_{s=1}^{t} h_{j,c,s} - \sum_{s=1}^{t} l_{j,c,s}.$$

Finally, we calculate the within cell difference—between low- and high-wage stores—of the

Figure 7: Difference in Separation and Hiring Rates



Notes: Estimated b's from Regression 4 with different left-hand side variables: The difference between low-wage and high-wage stores (a) the cumulative hiring rate, (b) the cumulative separation rates, and (c) employment. Dates of initiation and expiration of the unemployment insurance programs are included.

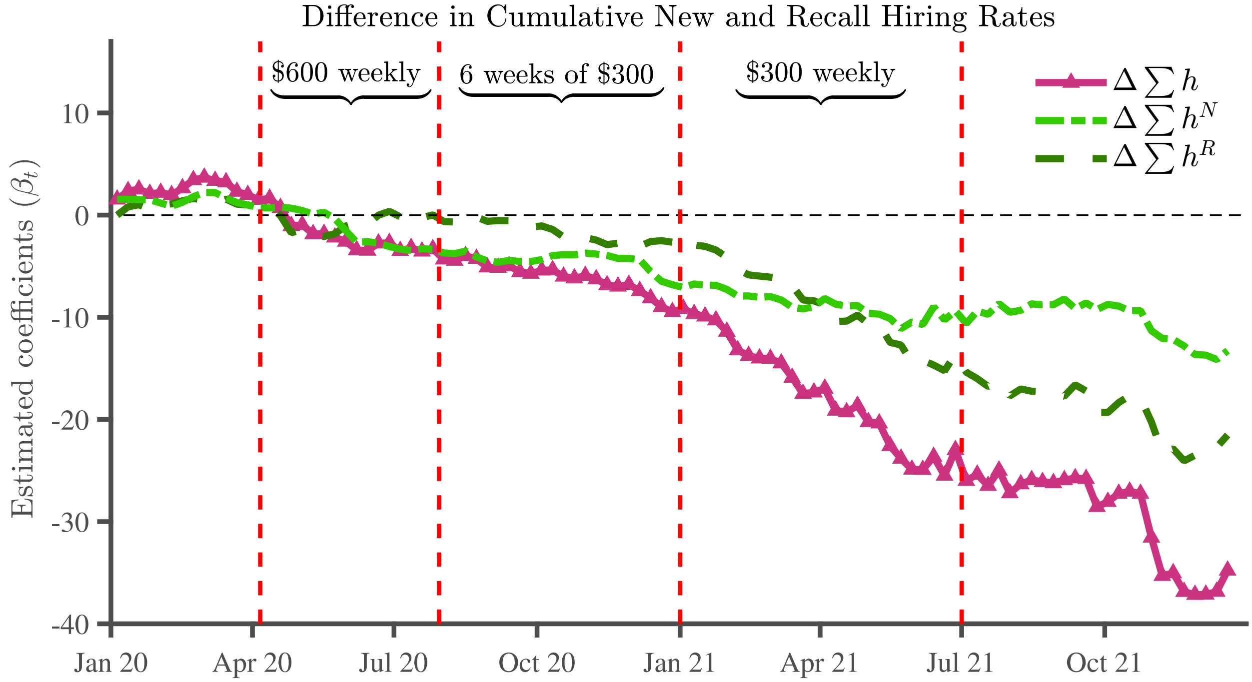
cumulative hiring and separation rates to derive

$$\Delta e_{c,t} \approx \Delta \sum_{s=0}^{t} h_{c,s} - \Delta \sum_{s=0}^{t} l_{c,s}. \tag{6}$$

Figure 7 plots the estimated coefficients from regression 4 when we use as a left-hand side variable $\sum_{s=0}^{t} h_s$ and $\Delta \sum_{s=0}^{t} l_s$, respectively. As we saw in Figure 4, both separation and hires are higher for low-wage stores than high-wage stores before the pandemic but become lower than high-wage stores after the pandemic. Thus, the difference (low-vs-high-wage stores) in both the cumulative separations and the cumulative hiring is increasing up to March 2020 and starts to decline after the beginning of the pandemic.

During the periods that the pandemic UI supplements are in effect, hires in low-wage stores decline faster than the decline in separations—leading to a decline in employment of low-wage stores relative to high-wage stores. When the pandemic UI supplements expire, separations decline faster than hires for low-wage stores leading to an increase in the employment of low-wage stores relative to high-wage stores.

Figure 8: Difference in New and Recall Hires



Notes: Estimated b's from Regression 4 using as left-hand side variables: (a) the cumulative hiring rate, (b) the cumulative hiring rate of new, and (c) the cumulative hiring of recalled workers.

Fujita and Moscarini (2017) document that a large share of workers return to their previous employer after a jobless spell. In this part, we distinguish between recalled hires and new workers. A new worker at time t is working for store j for the first time during week t. A recalled worker at week t is working for store j in week t as well as in some week s such that $s < t - 1$. Thus, a recalled worker must have skipped at least a week of work to be classified as recalled.

We can define the within cell difference in cumulative hires equal to the cumulative hires of new and recalled workers:

$$\Delta \sum_{s=0}^{t} h_{c,s} = \Delta \sum_{s=0}^{t} h^N_{c,s} + \Delta \sum_{s=0}^{t} h^R_{c,s}.$$

As Figure 8 shows, the difference for hires of new and recalled workers remained relatively stable before the Cares Act introduction. The differential employment recovery before and after CARES Act can be largely attributed to new hires and only partially to recalls (as we saw in Figure 5). Nonetheless, after the $300 of January 2021, recall hires are driving the differential employment recovery.

4.2 Benchmark Regression Specification

We evaluate the effect of pandemic UI benefits more formally, by estimating the regression described in Section 2, augmented to include additional control variables and a cell fixed effect.

$$\Delta y_{c,t} = \beta R_{c,t} + \mathbf{X}'_{c,t}\gamma + \delta \Delta y_{c,t,2019} + a_c + \eta_{c,t}, \tag{7}$$

where $\Delta y_{c,t}$ is the difference in the mean outcome y between low-wage and high-wage stores. We consider several outcome variables: employment, average hours per employee, and hourly wages. We run the regression using data between January 2020 and December 2021 and normalize variables with respect to the base period January-February 2020.

$R_{c,t}$ is the weekly replacement ratio in cell c and week t and is equal to the UI paid out in cell c in week t divided by the weekly earnings (i.e., the product of hourly wages and working hours) averaged across stores in cell c, during the week t. The UI payment is a combination of two components. First, the usual state-level formulas used to calculate UI payments computed using the average weekly earnings of cell c at time t. Second, the pandemic UI supplement in effect during week t (i.e., either zero, \$300, or \$600). Note that weekly earnings are measured based on the base period January-February 2020 and hence, they do not vary over time, only across cells. Thus, the replacement rates $R_{c,t}$ vary across time t due to the different amounts of UI payments during the several rounds of pandemic supplements, and vary across industries/regions c as we compute the replacement rate using the weekly earnings of the base period of the cell.

We include in the regression a set of observables, $\mathbf{X}_{ct}$. The controls include the number of Covid-19 deaths in a county as a measure of community health risk.[5] This factor reflects the popular narrative that fear of Covid-19 infection poses a hurdle for the non-employed to re-enter the workforce. Second, disruptions from school closings have been mentioned as another potential hurdle to return to work. We use geolocation data from Safegraph to find the percentage hchange in visits in schools by week and county relative to January 2020 (Kurmann, Lale, and Ta, 2021).

Alongside the set of control variables $\mathbf{X}'_{c,t}$, we include the pre-pandemic gap in variable y between low- and high-wage stores, for local industry c at the same week t of 2019 (denoted as $\Delta y_{c,t,2019}$). This takes care of potential seasonality that may drive the gap in a specific week during the year. Finally, we include in the regression a cell fixed effect a_c to control for potentially differential changes between low- and high-wage stores that are time-invariant and specific to the cell.

[5] In the appendix we consider alternative measures such as the number of positive Covid-19 cases and hospitalizations and find similar results.

Table 2 shows the regression results. We include two columns for each variable, one without any controls or fixed effect and one with the full set of controls and the fixed effect. Each observation represents a cell in a particular week between 2020-2021. Observations are lower for hours per employee and hourly wages since stores with employment information not always have information on hours and wages. The specifications including pre-trends also miss observations on hours and wages from 2019 which explains why the sample size declines further in columns (4) and (6).

Table 2: Replacement Rate and Employment Recovery

$\Delta y_{c,t}$	Employment		Hours		Hourly wages	
	(1)	(2)	(3)	(4)	(5)	(6)
Replacement rate	-2.05***	-1.70***	0.28	0.33**	0.77***	0.46***
	(0.62)	(0.40)	(0.22)	(0.15)	(0.27)	(0.15)
Covid-19 deaths	–	-0.30	–	-0.24	–	0.41*
(per 100,000 pop.)		(0.60)	–	(0.21)	–	(0.28)
School traffic	–	0.93*	–	0.04	–	0.23
(% change)		(0.52)	–	(0.18)	–	(0.16)
$\Delta y_{c,t,2019}$	–	0.09***	–	0.04***	–	0.01
		(0.01)	–	(0.01)	–	(0.01)
Cell fixed effect a_c	No	Yes	No	Yes	No	Yes
# Observations	110,586	110,586	103,863	103,156	97,333	94,112

Notes: Estimates from regression 7 including the control variables. Observations are at the local industry/week level. We winsorize the left-hand side variables at the 1%. We cluster standard errors at the local industry level.

We find that a 100 p.p. rise in the unemployment insurance replacement rate (which corresponds roughly to the increase in the ratio due to \$600 pandemic supplement), decreases low-wage store employment by 1.7 p.p. relative to high-wage store employment, increases low-wage store hours per employee by 0.3 p.p., and increases low-wage store hourly wages by 0.4 p.p.

One more death by Covid-19 (as a weekly average per 100,000 county residents) is associated with a decrease in the low-wage employment by 0.3 p.p., a decline in hours by 0.2 p.p. and with an increase in wages by 0.4 p.p. A 1% increase of traffic toward school establishments in a local industry is associated with an increase in the employment recovery rate of low-wage stores by 0.9 p.p, and negligible changes in the hours and hourly wage of low-wage stores.

Table 3 shows the regression estimates for employment, separations, new hires, and recall. The response of employment can be roughly decomposed to an increase in separations of low-wage stores by 0.2 and a decline in hiring by 1.6 p.p. The decline in hiring is mostly driven by new hires (-1.4 p.p. decline) and less by the recall margin (a decline on 0.2 p.p.).

Table 3: Replacement Rate, Separations, New Hires, and Recalls

$\Delta y_{c,t}$	Separations	Hires	Recalls	New Hires
	(2)	(3)	(4)	(5)
Replacement rate	0.29	-1.65	-0.21	-1.44
	(2.14)	2.04	(0.88)	(1.54)
Covid-19 deaths	-1.69	-1.58	-2.41	0.83
(per 100,000 pop.)	(4.63)	4.68	(2.00)	(3.46)
School traffic	-5.72	-5.07	-1.91	-3.16
(% change)	(4.19)	4.35	(1.71)	(3.16)
Cell fixed effect a_c	Yes	Yes	Yes	Yes
# Observations	110,586	110,586	110,586	110,586

Notes: Estimates from regression 7 including the control variables. Observations are at the local industry/week level. We cluster standard errors at the local industry level.

We next evaluate the effect of pandemic UI benefits using our second specification:

$$\Delta y_{c,t} = \beta \Delta R_{c,t} + \mathbf{X}'_{c,t}\gamma + \delta \Delta y_{c,t,2019} + a_c + \eta_{c,t}, \tag{8}$$

where $\Delta R_{c,t}$ is the difference in the replacement ratios between the low- and high-wage labor market of cell c. We consider the same outcome variables as before and include the a set of observables, $\mathbf{X}_{ct}$.

We find that a 100 p.p. rise in the unemployment insurance replacement rate, decreases low-wage store employment by 5.8 p.p. relative to high-wage store employment, has a modest and insignificant effect on low-wage store hours, while it increases low-wage store hourly wages by 2.4 p.p. These estimates for the decline in employment are an order of magnitude larger than existing estimates which supports our interpretation that our estimation captures closer the disincentive effects of UI pandemic supplements. The effect of the control variables remain relatively similar with before.

Table 5 shows the regression estimates for employment, separations, new hires, and recall. With this specification the decline in hiring is driven by recalls and less by new workers.

Table 4: Replacement Rate and Employment Recovery

$\Delta y_{c,t}$	Employment		Hours		Hourly wages	
	(1)	(2)	(3)	(4)	(5)	(6)
Replacement rate	−10.91***	−5.84***	-4.78***	0.50	5.74***	2.48***
	(3.15)	(1.24)	(1.08)	(0.48)	(1.38)	(0.75)
Covid-19 deaths	–	−0.30	–	−0.26	–	0.40**
(per 100,000 pop.)		(0.60)	–	(0.21)	–	(0.19)
School traffic	–	1.02*	–	0.17	–	0.26
(% change)		(0.54)	–	(0.18)	–	(0.16)
$\Delta y_{c,t,2019}$	–	0.09***	–	0.04***	–	0.01
		(0.01)	–	(0.01)	–	(0.01)
Cell fixed effect a_c	No	Yes	No	Yes	No	Yes
# Observations	110,586	110,586	103,863	103,156	97,333	94,112

Notes: Estimates from regression 8 including the control variables. Observations are at the local industry/week level. We winsorize the left-hand side variables at the 1%. We cluster standard errors at the local industry level.

Table 5: Replacement Rate, Separations, New Hires, and Recalls

$\Delta y_{c,t}$	Separations	Hires	Recalls	New Hires
	(2)	(3)	(4)	(5)
Replacement rate	−1.77	−7.99	−7.18*	0.89
	(5.13)	5.35	(4.02)	(2.59)
Covid-19 deaths	2.18	2.09	2.97	−1.04
(per 100,000 pop.)	(2.79)	2.89	(2.27)	(1.35)
School traffic	0.39	1.42	0.90	−0.39
(% change)	(2.64)	2.71	(2.14)	(1.19)
Cell fixed effect a_c	Yes	Yes	Yes	Yes
# Observations	110,586	110,586	110,586	110,586

Notes: Estimates from regression 7 including the control variables. Observations are at the local industry/week level. We cluster standard errors at the local industry level.

4.3 The Role of Local Industry Sorting

We find sizable negative effects of pandemic UI on employment recovery. We interpret our estimate as a disincentive effect by assuming that the local demand stimulus by the UI benefits is equally shared by neighboring stores within a local-industry. The assumption is more plausible in local level of aggregations (e.g., restaurants in downtown Manhattan) relative to broader levels of aggregations (e.g., restaurants in the whole N.Y. state). Thus, a key test for our method is to explore if estimates become less negative as we consider broader local industry aggregations.

Figure 9 plots the estimates from regression 7 for various levels of geographical and industry aggregation. The x-axis shows different levels of local sorting. We sort stores based on the national, state, and county distribution of wages, as well as the benchmark local sorting that consists of grouping stores based on 4-digit zip codes. The left panel shows the estimates when we do not sort based on industry and the right panel when we do industry sorting as in the benchmark. Hence, the estimate "National" in the left panel is the case of no local industry sorting while the estimate "Benchmark" in the right panel is the Benchmark case of local industry sorting.

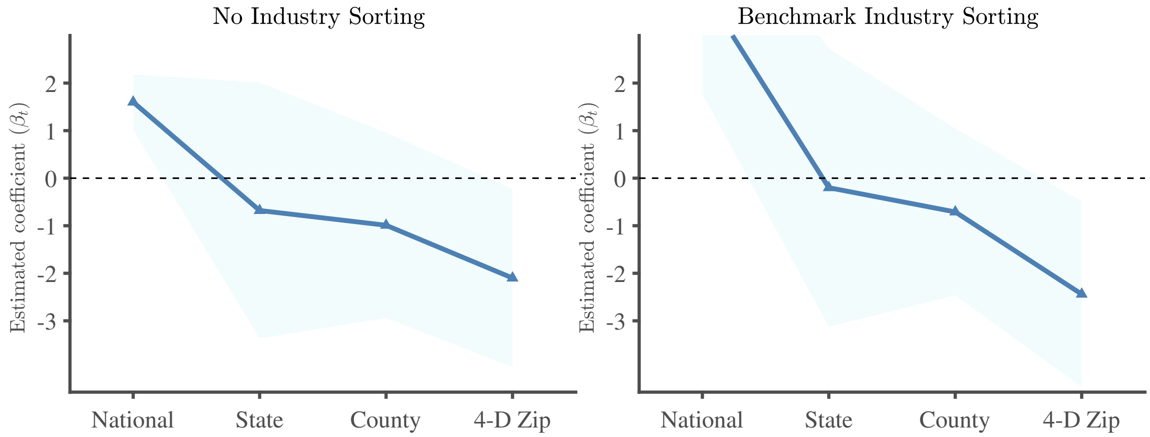
As we sort based on narrower geographical aggregations (i.e. we move along the x-axis in both the left and right panel), the coefficients turns from positive or zero to negative. The coefficient further decreases when we sort based on the industry (i.e. when we move from the left to the right panel). Hence, both dimensions of sorting are important to explain our results.

Why do the estimates become small and insignificant when we sort stores without reference to their local industry (i.e., mixing all geographical locations and sectors), or even when we sort based on broader geographical areas such as states? Our interpretation is that when the local industry is not properly controlled for, the estimated effect captures a variety of factors including region- and sector-specific shocks, and thus, should not be interpreted as the disincentive effect of UI on labor supply. Indeed, the recent research on the effects of the pandemic UI benefits has employed state level controls or is based on state variation (see for example, Altonji et al., 2020; Coombs et al., 2021; Ganong, Greig, Noel, Sullivan, and Vavra, 2021, Marinescu et. al, 2021). Our results show that state controls is not sufficient to control for the local stimulative effect of policies and one needs to narrow the local industry at finer geographical levels.

4.4 Robustness

Sample Restrictions We relax two selection criteria that applied to the benchmark sample. First, we allow stores to enter up to the end of 2019. This increases our final sample to 5,900 stores. We run regression 7 without $\Delta y_{c,t,2019}$ as there is no guarantee that the each cell will have an observation for the same week of 2019. The UI estimate decreases in absolute values to $\beta = -1.3$. We next allow for stores that have no information from Yelp. These

Figure 9: Effect of UI on Employment Recovery: Local Industry Sorting



Notes: Estimates β's from Regression 7. The x-axis represents different levels of local aggregation. The left panel shows estimates without industry sorting and the right panel with the benchmark definition of industry sorting. Shaded area represents 95% confidence intervals.

stores have no online presence so we are less confident about their NAICS code based on our matching procedure. This increases our sample stores to 7,958 stores. In this case the estimate is unaffected and equal to $\beta = -1.7$. Once we relax both assumptions the number of stores is $\beta = -1.2$.

Measurement of Employment For our benchmark, we define weekly employment in the store as the number of unique bodies that work in the store during the week. Another definition is to use the total labor employed in the store during the week, i.e., the daily average number of employees, independent of whether they are the same or different workers. When we use the "total labor" definition the estimate increases in absolute value to $\beta = -2.0$. Hence, the results are robust to alternative definitions of employment. We also measure employment during the days of operation (i.e., without the "zeros"). The estimate goes to -4.62.

Base Period and Continuously Open Stores We have normalized our time series with respect to the base period of January-February 2020. When we choose the wider base period of July 2019-February 2020 the estimate decreases in absolute value to $\beta = -1.5$. In addition, when we keep only the stores than did not close during the recovery the estimate remain unchanged to $\beta = -1.7$.

K-Means Clustering The benchmark grouping of stores is based on a single dimension: the wage of the local industry. Here we sort stores along multiple dimensions: i.e., the hourly wage, the price of the store and the Yelp rating. To take into account all the store characteristics we use the K-Means clustering algorithm to classify stores as "high" or "low". With this broader classification, the estimate increases slightly in absolute value to $\beta = -1.5$.

Weights The benchmark specification treats each cell equally. Here we weight each cell by the number of stores inside the cell, i.e., more populated cells take a higher wage. The estimate remains in absolute value to $\beta = -1.7$ and is statistically significant at the 1%.

5 Quantitative Model

In this section, we employ a quantitative labor search model to analyze the interaction between unemployment benefits and the slower employment recovery of low-wage establishments relative to high-wage establishments. In our model the introduction of pandemic UI benefits affects workers' reservation wages and ultimately their decision to re-enter the labor market. Our model takes into account that the pandemic UI benefits were introduced at a time of severe labor market disruption, when many jobs become vacant and more workers become unemployed. We attempt to capture all these effects jointly using a quantitative model that is tightly calibrated to the empirical patterns presented in the previous sections.

5.1 Economic Environment

Time is discrete, runs forever and is indexed by the subscript t. The economy is populated by a continuum of workers who are either employed or are searching for work, and a continuum of firms that either have a vacant job or have a filled job position. A filled position pays a wage w, which is idiosyncratic to the firm and drawn from an exogenous distribution $G(w)$ upon creation of the firm. We do not allow for wage bargaining since, as reported by Hall and Krueger (2012), this feature is hardly present in the unskilled segment of the labor market, where restaurant and retail job positions are concentrated. Labor productivity, denoted as y, is uniform across firms, which implies that high-wage firms make lower per-period profits per worker compared to low-wage firms. These differences are partially offset by the fact that high-wage firms are more likely to attract unemployed workers when they have a vacant position to fill or to retain their incumbent workers, compared to low-wage firms.

To attract unemployed workers, a firm posts a vacancy at a per-period cost $c_v > 0$. The probability that a vacancy meets a worker is determined by the ratio between the total number of posted vacancies and the number of unemployed workers. This probability, which is denoted as $q(\theta_t)$, is a decreasing, convex function of market tightness θ_t. An unemployed worker meeting a firm that pays w, turns down the job offer if w is lower than her reservation wage. Thus, conditional on meeting an unemployed worker, the offer to work at wage w is accepted with probability $F_{U,t}(w)$, reflecting the distribution of reservation wages among the unemployed. As we explain below, a key feature of our model is that $F_{U,t}(w)$ is an equilibrium object in our model.[6]

Firms get hit by two types of separation shocks. First, an exit probability δ^e, in which case firms shut down permanently and leave the market. This shock is only relevant in the steady state where an equal measure of new firms enter to keep the number of firms constant. Second, conditional on survival, the firm may be separated from its worker with probability δ^s in which case the job remains vacant. We allow only for job separation shocks in the transition to match our sample of fully balanced HB stores.

An important dimension of our model is that we allow for a recall option. In particular, when the job separation shock hits, the worker is only temporarily separated from the firm and draws a probability r of resuming production next period from a distribution $H(r)$. If agents reject the recall option upon drawing r, the job is destroyed and the firm and worker are returned to the pool of vacant jobs and unemployed workers, respectively.[7]

Workers derive utility from consumption x_t according to a function $u(x_t)$. There is no saving as we think of workers employed at small firms in our data as being mostly hand-to-mouth. Hence, consumption x_t is equal to the wage w when employed and to welfare benefits b when unemployed. Each unemployed worker randomly searches for a job. Typically, directed search occurs with regard to broader labor markets (occupations, cities, or large employers). Since the model is mapped to data from narrowly defined local industries we view random search as more appropriate. During unemployment, workers meet firms with vacant positions according to a per-period probability $f(\theta_t)$, which is an increasing and concave function of tightness. Upon meeting a firm with a vacant job w, a worker chooses whether to accept the job or to continue searching for a better job. Upon job separation (either temporary or permanent), a workers' unemployment benefits become $b = b(w)$, meaning that they are calculated out of the wage w from the last job. This is exactly how the model generates an endogenous distribution of reservation wages $F_{U,t}(w)$ that arises from the (observed) distribution of wages among employed workers.[8]

During normal times (i.e., the steady state), unemployed workers receive regular unemployment benefits $b_R(w)$ with probability p_1^R, where the probability reflects both the likelihood of applying to UI benefits (which depends partly on UI eligibility rules) and the success rate of UI applications. Regular UI benefits expire with a per-period probability p_0^R. When regular benefits expire or when the worker did not receive regular benefits in the first place, the worker receives a social assistance income $b_S(w)$ that has infinite duration but pays substantially less

[6]This differentiates our model from the wage-posting model of **?**.

[7]Since agent cannot bargain over recalls, some job separations are inefficient, in the sense that some surplus from the agent who would (unilaterally) prefer to accept the recall option could be transferred to the other, and make both agents better off, compared to dissolving the job match.

[8]Notice that in most search models, unemployment benefits b do not depend on a worker's previous labor market history. The model closest to ours in this respect is Ljungqvist and Sargent (1998, 2008).

than UI benefit compensations. Incorporating a social assistance state is important to match the elasticity of unemployment spells to the duration of regular benefits. The pandemic UI benefits are provided on top of this system: with probability p_1^P workers receive benefits $b_P(w)$ and these benefits expire with a per-period probability p_0^P.

During recalls, workers may or may not receive UI benefits, depending on the stochastic recipiency of UI benefits described in the above paragraph. As already mentioned, when deciding on whether to accept the recall option, agents make a binding decision – after the decision is made, they cannot refuse to work when called back by the probability r. This assumption seems reasonable given that eligibility to UI benefits is terminated if a worker who is recalled by her employer refuses to return to work.[9] To streamline the model, we assume that workers make decisions on the recall option before stochastic recipiency of UI benefits is realized. Thus, they compare the expected value of being on recall against the expected value of being unemployed, where both expectations are computed with respect to the stochastic UI rules encapsulated in p_1^R and p_1^P.

5.2 Asset Values

Both workers and firms discount the future at rate $\beta^{-1}-1$. We let $J_t(w)$ denote a firm's asset value of a filled job that pays a wage w, $J_t^s(w,r)$ the asset value of this job being on hold with a recall probability r, and $V_t(w)$ the asset value of posting a vacancy to advertise job w. The asset value of a filled job is given by.

$$\begin{aligned} J_t(w) = y - w + \beta(1-\delta^e)\Bigg[&(1-\delta^s)J_{t+1}(w) \\ &+\delta^s\left(\int \max\left\{J_{t+1}^s(w,r) - V_{t+1}(w), 0\right\} s_{t+1}^W(w,r)\,dH(r) + V_{t+1}(w)\right)\Bigg], \end{aligned} \tag{9}$$

where

$$s_t^W(w,r) = \mathbb{1}\left\{\bar{W}_t^s(w,r) > \bar{U}_t(w)\right\} \tag{10}$$

[9]Refusal to accept a recall is hard to identify empirically, but there is an argument that this law would be implemented in practice. One would expect firms to inform the government (UI office) that their worker refused to come back to work because the UI system has an experience rating system that penalizes firms for additional layoffs by increasing tax rates. Thus, to avoid paying additional taxes, firms would have the incentive to challenge their worker's UI claim if that worker refuses to go back to work. During the pandemic, however, there may have been many exceptions to this rule, and state UI offices may have been more lenient toward those cases: if the worker has COVID or is recovering from it, if she is taking care of a family member with COVID, if she does not have childcare due to COVID related reasons, etc. We thank Serdar Birinci and Shigeru Fujita for bringing our attention to these issues.

is an indicator that takes the value of 1 if, when given the recall option, the worker would accept it, and is 0 otherwise. In this indicator function, $\bar{W}_t^s(w,r)$ denotes the worker's expected value of being on recall, and $\bar{U}_t(w)$ is her expected asset value of being unemployed, which we define momentarily. Thus, in Equation (9), with probability $1-\delta^e$ the firm survives until next period; with probability $1-\delta^s$ it is operative during the next period; with probability δ^s there is a separation, which may be temporary if the worker accepts the recall option and the firm is better off retaining the worker than destroying the job. The latter decision depends on the asset value $J_t^s(w,r)$, which solves

$$J_t^s(w,r) = \beta(1-\delta^e)\left[rJ_{t+1}(w) + (1-r)J_{t+1}^s(w,r)\right]. \tag{11}$$

Notice that while on recall, the firm is not making profits but is also not spending resources on vacancy posting. On the other hand, when the firm attempts to fill a vacant job, it incurs a cost c_v in each period and its value is given by

$$V_t(w) = -c_v + \beta(1-\delta^e)\left((1-q(\theta_t)F_{U,t}(w))V_{t+1}(w) + q(\theta_t)F_{U,t}(w)J_{t+1}(w)\right). \tag{12}$$

The beginning-of-period tightness θ_t pins down the probability of randomly meeting an unemployed workers, $q(\theta_t)$. In the continuation value the probability that matters for job acceptance is $F_{U,t}(w)$ (which accounts for the fact that workers' entitlement to UI benefits evolve during period t). With probability $1-q(\theta_t)F_{U,t}(w)$, the job remains unfilled and the firm keeps advertising the job next period. In Equations (9), (11), (12), the "death" shock δ^e turns the value of a firm into 0, which is the value of being inactive in the stationary equilibrium of the model, where there is free entry of firms (Section (5.4) below). Based on Equation (12), it is clear that there are instances in which a firm drawing a wage w would prefer to remain inactive: a firm would not post vacancies if its w is too low to attract sufficiently many workers ($F_{U,t}(w) \to 0$), or if w is so high that it would not profitably hire workers to repay vacancy posting costs ($J_{t+1}(w) \to 0$).

On the worker's side, we let $W_t(w)$ denote the asset value of employment in a firm that pays w, and denote by $U_{i,t}(w)$ the value of receiving UI benefits $i \in \{P,R,S\}$ after being separated from job w. $W_t(w)$ is given by

$$\begin{aligned} W_t(w) = u(w) + \beta\Big[&\delta^e\bar{U}_{t+1}(w) + (1-\delta^e)\big((1-\delta^s)W_{t+1}(w) \\ &+\delta^s\Big(\int \max\left\{\bar{W}_{t+1}^s(w,r) - \bar{U}_{t+1}(w), 0\right\} s_{t+1}^J(w,r)\,dH(r) + \bar{U}_{t+1}(w)\Big)\Big)\Big], \end{aligned} \tag{13}$$

where

$$s_t^J(w,r) = \mathbb{1}\{J_t^s(w,r) > V_t(w)\} \tag{14}$$

is an indicator that takes the value of 1 if the firm would opt for the recall option upon being hit by the separation shock δ^s, and is 0 otherwise. Together with $s_t^W(w,r)$, $s_t^J(w,r)$ defines a reservation probability threshold above which the worker-firm pair accepts the recall option. Let $\underline{r}_t^J(w)$ and $\underline{r}_t^W(w)$ define the recall probabilities such that the firm, respectively the worker are indifferent between accepting or rejecting the option or not; these are defined by

$$J_t^s\left(w, \underline{r}_t^J(w)\right) = V_t(w) \quad \text{and} \quad \bar{W}_t^s\left(w, \underline{r}_t^W(w)\right) = \bar{U}_t(w). \tag{15}$$

Given that both parties must accept the recall option, the reservation probability threshold is:

$$\bar{r}_t(w) = \max\left\{\underline{r}_t^J(w), \underline{r}_t^W(w)\right\}. \tag{16}$$

In Equation (13), with probability δ^e the firm dies and the worker is sent to unemployment; with probability $1-\delta^e$ the firm survives and production takes place next period with probability $1-\delta^s$; with probability $(1-\delta^e)\,\delta^s$, the separation shock triggers to decision to accept the recall option or to dissolve the match. As described above, decisions over the recall option are made before the stochastic recipiency of UI benefits is realized, by comparing $\bar{W}_t^s(w,r)$ against $\bar{U}_t(w)$. These are defined as:

$$\bar{W}_t^s(w,r) = p_1^P W_{P,t}^s(w,r) + \left(1-p_1^P\right)\left(p_1^R W_{R,t}^s(w,r) + \left(1-p_1^R\right) W_{S,t}^s(w,r)\right), \tag{17}$$

$$\bar{U}_t(w) = p_1^P U_{P,t}(w) + \left(1-p_1^P\right)\left(p_1^R U_{R,t}(w) + \left(1-p_1^R\right) U_{S,t}(w)\right). \tag{18}$$

The linearity of these equations with respect to asset values implies that the worker's decision over the recall option depends on the weighted comparisons of $W_{i,t}^s(w,r)$ against $U_{i,t}(w)$, where $i \in \{P,R,S\}$ indexes the different benefits that she may receive. The asset values $W_{i,t}^s(w,r)$ are given by

$$\begin{aligned} W_{P,t}^s(w,r) = u\left(b_P(w)\right) + \beta\Big[&\delta^e \bar{U}_{P,t+1}(w) + (1-\delta^e)\left(rW_{t+1}(w) + (1-r)\right. \\ &\times \left(\left(1-p_0^P\right) W_{P,t+1}^s(w,r) + p_0^P\left(p_1^R W_{R,t+1}^s(w,r) + \left(1-p_1^R\right) W_{S,t+1}^s(w,r)\right)\right)\Big], \end{aligned} \tag{19}$$

$$\begin{aligned} W_{R,t}^s(w,r) = u\left(b_R(w)\right) + \beta\Big[&\delta^e \bar{U}_{R,t+1}(w) + (1-\delta^e)\left(rW_{t+1}(w)\right. \\ &+ (1-r)\left(\left(1-p_0^R\right) W_{R,t+1}^s(w,r) + p_0^R W_{S,t+1}^s(w,r)\right)\Big], \end{aligned} \tag{20}$$

$$W_{S,t}^{s}(w,r)=u(b_S(w))+\beta\left[\delta^{e}U_{S,t+1}(w)+(1-\delta^{e})\left(rW_{t+1}(w)+(1-r)W_{S,t+1}^{s}(w,r)\right)\right]. \tag{21}$$

where we define $\bar{U}_{P,t}(w)=\left(1-p_0^P\right)U_{P,t}(w)+p_0^P\left(p_1^R U_{R,t}(w)+\left(1-p_1^R\right)U_{S,t}(w)\right)$ and $\bar{U}_{R,t}(w)=\left(1-p_0^R\right)U_{R,t}(w)+p_0^R U_{S,t}(w)$. In Equations (19)–(21), as in Equation (11), conditional on survival of the firm, the agents wait for the shock r to hit and resume production. While on recall, a worker's UI benefits evolve over time according to the same rules as if she were unemployed: pandemic UI are exhausted with probability p_0^P, making the worker receive regular benefits based on the recipiency probability p_1^R, and regular benefits expire with a per period probability p_0^R.

Last, the asset values of unemployment depend on market tightness θ_t as well as the equilibrium distribution of vacant jobs that are advertised during period t, $G_{V,t}(w)$. These values solve:

$$\begin{aligned}U_{P,t}(w)=u(b_P(w))+\beta\Bigg[p_0^P\Bigg(p_1^R\Bigg(&(1-f(\theta_t))U_{R,t+1}(w)\\&+f(\theta_t)\int\max\{W_{t+1}(w'),U_{R,t+1}(w)\}\,dG_{V,t}(w')\Bigg)+\left(1-p_1^R\right)\Bigg((1-f(\theta_t))U_{S,t+1}(w)\\&+f(\theta_t)\int\max\{W_{t+1}(w'),U_{S,t+1}(w)\}\,dG_{V,t}(w')\Bigg)\Bigg)+\left(1-p_0^P\right)\Bigg((1-f(\theta_t))U_{P,t+1}(w)\\&+f(\theta_t)\int\max\{W_{t+1}(w'),U_{P,t+1}(w)\}\,dG_{V,t}(w')\Bigg)\Bigg],\end{aligned} \tag{22}$$

$$\begin{aligned}U_{R,t}(w)=u(b_R(w))+\beta\Bigg[p_0^R\Bigg(&(1-f(\theta_t))U_{S,t+1}(w)\\&+f(\theta_t)\int\max\{W_{t+1}(w'),U_{S,t+1}(w)\}\,dG_{V,t}(w')\Bigg)+\left(1-p_0^R\right)\Bigg((1-f(\theta_t))U_{R,t+1}(w)\\&+f(\theta_t)\int\max\{W_{t+1}(w'),U_{R,t+1}(w)\}\,dG_{V,t}(w')\Bigg)\Bigg],\end{aligned} \tag{23}$$

$$\begin{aligned}U_{S,t}(w)=u(b_S(w))+\beta\Bigg(&(1-f(\theta_t))U_{S,t+1}(w)\\&+f(\theta_t)\int\max\{W_{t+1}(w'),U_{S,t+1}(w)\}\,dG_{V,t}(w')\Bigg).\end{aligned} \tag{24}$$

Notice that in Equations (22)–(24), an unemployed worker's state variable w refers to her earnings in the previous job. This state variable persists until the worker accepts a new job.

The "max" operator in these equations defines a worker's reservation wage. The reservation wage is the value of the wage that makes the worker indifferent between accepting and rejecting a job offer at time t, given the benefits that she receives while being unemployed:

$$W_t\left(\underline{w}_{i,t}(w)\right) = U_{i,t}(w), \tag{25}$$

$i \in \{P, R, S\}$. We will show that workers who receive more generous UI benefits have higher reservation wages, and that a longer expected duration of benefits increases reservation wages.

5.3 Law of Motion

We analyze the distribution of workers across labor market states as well as their evolution. Let $e_t(w)$ denote the number of workers employed at wage w at time t; $\tilde{e}_{i,t}(w, r)$ the number of wage-w workers who are on hold with a recall probability r and receiving benefits $i \in \{P, R, S\}$ at time t; and $u_{i,t}(w)$ the number of unemployed workers receiving benefits $i \in \{P, R, S\}$ at time t and were previously employed at wage w. Employment at wage w at time $t+1$ is given by

$$e_{t+1}(w) = (1-\delta^e)(1-\delta^s)\, e_t(w) + (1-\delta^e)\int \tilde{e}_t(w, r)\, r dr + f(\theta_t)\, g_{V,t}(w)\, F_{U,t}(w)\, \bar{u}_t, \tag{26}$$

where $\tilde{e}_t(w, r)$ denotes the total number of workers on recall with wage w and probability r, i.e.

$$\tilde{e}_t(w, r) = \tilde{e}_{P,t}(w, r) + \tilde{e}_{R,t}(w, r) + \tilde{e}_{S,t}(w, r), \tag{27}$$

$\bar{u}_t$ denotes the total number of unemployed workers, i.e.

$$\bar{u}_t = \int u_{P,t}(w) + u_{R,t}(w) + u_{S,t}(w)\, dw, \tag{28}$$

and $g_{V,t}(w)$ is the density of vacancies advertised at wage w. In Equation (26), there are two types of employment inflows: recalled workers and new hires. This distinctive feature of the model is key to bring it to data, as discussed further below in Section 5.5. The law of motion for workers on recall is:

$$\tilde{e}_{P,t+1}(w, r) = (1-\delta^e)(1-r)\left(1-p_0^P\right)\tilde{e}_{P,t}(w, r) + p_1^P(1-\delta^e)\,\delta^s e_t(w)\, h(r)\, \mathbb{1}\{r > \bar{r}_t(w)\}, \tag{29}$$

$$\tilde{e}_{R,t+1}(w,r) = (1-\delta^e)(1-r)\left(\left(1-p_0^R\right)\tilde{e}_{R,t}(w,r) + p_1^R p_0^P \tilde{e}_{P,t}(w,r)\right) + p_1^R\left(1-p_1^P\right)(1-\delta^e)\delta^s e_t(w) h(r) \mathbb{1}\{r > \bar{r}_t(w)\}, \tag{30}$$

$$\tilde{e}_{S,t+1}(w,r) = (1-\delta^e)(1-r)\left(\tilde{e}_{S,t}(w,r) + p_0^R\tilde{e}_{R,t}(w,r) + \left(1-p_1^R\right)p_0^P\tilde{e}_{P,t}(w,r)\right) + \left(1-p_1^R\right)\left(1-p_1^P\right)(1-\delta^e)\delta^s e_t(w) h(r)\mathbb{1}\{r > \bar{r}_t(w)\}. \tag{31}$$

In Equations (29)–(31), $h(r)$ denotes the density function of the probability distribution of the r's, i.e. $h(r) = H'(r)$. The inflows of workers into recalls depend on firms' survival and separation shocks, as well as on the probability of exercising the recall option, r, and whether it is acceptable to both parties. Last, the measures of unemployed workers $u_{P,t}(w)$, $u_{R,t}(w)$, $u_{S,t}(w)$ evolve over time according to

$$u_{P,t+1}(w) = \left(1 - f(\theta_t)\bar{G}_{V,t}\left(\underline{w}_{P,t}(w)\right)\right)\left(1-p_0^P\right)u_{P,t}(w) + p_1^P\delta^e e_t(w) + \delta^e \int \left(1-p_0^P\right)\tilde{e}_{P,t}(w,r)\,dr + p_1^P(1-\delta^e)\delta^s H(\bar{r}_t(w))e_t(w), \tag{32}$$

$$u_{R,t+1}(w) = \left(1 - f(\theta_t)\bar{G}_{V,t}\left(\underline{w}_{R,t}(w)\right)\right)\left(\left(1-p_0^R\right)u_{R,t}(w) + p_1^R p_0^P u_{P,t}(w)\right) + p_1^R\left(1-p_1^P\right)\delta^e e_t(w) + \delta^e\left(1-p_0^P\right)\int \left(1-p_0^R\right)\tilde{e}_{R,t}(w,r) + p_1^R p_0^P\tilde{e}_{P,t}(w,r)\,dr + p_1^R\left(1-p_1^P\right)(1-\delta^e)\delta^s H(\bar{r}_t(w))e_t(w) \tag{33}$$

$$u_{S,t+1}(w) = \left(1 - f(\theta_t)\bar{G}_{V,t}\left(\underline{w}_{S,t}(w)\right)\right)\left(u_{S,t}(w) + p_0^R u_{R,t}(w) + \left(1-p_1^R\right)p_0^P u_{P,t}(w)\right) + \left(1-p_1^R\right)\left(1-p_1^P\right)\delta^e e_t(w) + \left(1-p_1^R\right)\left(1-p_1^P\right)(1-\delta^e)\delta^s H(\bar{r}_t(w))e_t(w) + \delta^e\left(1-p_0^P\right)\int \tilde{e}_{S,t}(w,r)p_0^R\tilde{e}_{R,t}(w,r) + \left(1-p_1^R\right)p_0^P\tilde{e}_{P,t}(w,r)\,dr. \tag{34}$$

There are two types of unemployment inflows in Equations (32)–(34): exogenous and endogenous. Exogenous inflows are either directly from employment or from workers who were on recall at a firm that is hit by the "death" shock, δ^e. Endogenous unemployment inflows come from worker-firm pairs hit by the separation shock, δ^s, that draw a recall option such that r is below the reservation threshold $\bar{r}_t(w)$. Unemployment outflows depend on the probability of meeting a vacancy, $f(\theta_t)$, on workers' reservation wages, and on the distribution of vacant jobs among posted vacancies. $\bar{G}_{V,t}(w)$ denotes the tail distribution of $G_{V,t}(w)$, i.e. the probability that a vacant job offers a wage at least as high as w, so that $\bar{G}_{V,t}\left(\underline{w}_{i,t}(w)\right)$ is the probability that a job is acceptable to an unemployed workers with benefits $b_i(w)$.

We can use the set of above equations to express $F_{U,t}(w)$, the fraction of unemployed workers whose reservation wage is lower than w (and would therefore accept a job offer that pays w). We have

$$
\begin{aligned}
F_{U,t}(w') = \frac{1}{\bar{u}_t}\Bigg(\int_{\left\{w:w'>\underline{w}_{P,t+1}(w)\right\}} & \left(1-p_0^P\right) u_{P,t}(w)\,dw \\
&+ \int_{\left\{w:w'>\underline{w}_{R,t+1}(w)\right\}} \left(1-p_0^R\right) u_{R,t}(w) + p_1^R p_0^P u_{P,t}(w)\,dw \\
&+ \int_{\left\{w:w'>\underline{w}_{S,t+1}(w)\right\}} u_{S,t}(w) + p_0^R u_{R,t}(w) + \left(1-p_1^R\right) p_0^P u_{P,t}(w)\,dw\Bigg).
\end{aligned}
\tag{35}
$$

On the other hand, $G_{V,t}(w)$ can be recovered from the law of motion of vacant jobs. It is given by:

$$
v_{t+1}(w) = \left(1-q(\theta_t)F_{U,t}(w)\right)\left(1-\delta^e\right)v_t(w) + \left(1-\delta^e\right)\delta^s H\left(\bar{r}_t(w)\right)e_t(w) + n_t\frac{g(w)}{\int_{\{w:V(w)>0\}} dG(w)}. \tag{36}
$$

In this equation, n_t denotes the flow of new firms that enter the economy in period t. They draw a wage from the exogenous density function $g(w) = G'(w)$, and only vacancies with a positive asset value get advertised (details follow). In the above equations, the density $g_{V,t}(w)$ is computed as $g_{V,t}(w) = v_t(w)/\bar{v}_t$, where

$$
\bar{v}_t = \int v_t(w)\,dw \tag{37}
$$

and $G_{V,t}(w)$ is the cumulative distribution function of $g_{V,t}(w)$, i.e. $G_{V,t}(w') = \int_0^{w'} g_{V,t}(w)\,dw$. Notice that Equation (36) features an endogenous inflow of vacancies from surviving firms that are hit by the separation shock and cannot exercise the recall option. To complete the description of the equilibrium law of motion, we compute market tightness θ_t as the ratio between total vacancies, $\bar{v}_t$, and unemployed workers, $\bar{u}_t$, defined respectively in Equations

(37) and (28). Since the population of workers is of measure one, we have

$$\bar{u}_t + \bar{e}_t + \tilde{e}_t = 1 \tag{38}$$

where $\bar{e}_t = \int e_t(w)\,dw$ and $\tilde{e}_t = \int\int \tilde{e}_t(w,r)\,dwdr$.

5.4 Stationary Equilibrium

Eventually, we are interested in the dynamics of this economy during the pandemic, and we think of the pre-pandemic period as a stationary equilibrium. The stationary equilibrium serves two purposes. First, it creates a mapping between the exogenous sampling distribution of wages, $G(w)$, and the endogenous distribution of vacancies. Second, it pins down the measure of active firms (that is to say firms that are either posting vacancies or employing a worker) through a free entry condition. Hence it pins down the level of market tightness, which will be key for analyzing the effects of the pandemic shock.

In the stationary equilibrium, the measure of firms is constant and therefore the number of newly entering firms (n_t in Equation (36)) is equal to the number of firms that leave the market in each period, that is to say $\delta^e(\bar{v}_t + \bar{e}_t + \tilde{e}_t)$. Plugging this into Equation (36), and omitting the time subscripts to denote the stationary equilibrium, we the following equation links $v(w)$, the measure of vacant jobs w, to the wage sampling distribution $G(w)$:

$$v(w) = \frac{1}{\delta^e - q(\theta)F_U(w)(1-\delta^e)}\left((1-\delta^e)\delta^s H(\bar{r}(w))e(w) + \frac{\delta^e(\bar{v}+\bar{e}+\tilde{e})g(w)}{\int_{\{w:V(w)>0\}} dG(w)}\right). \tag{39}$$

$e(w)$ denotes the stationary measure of filled jobs, $F_U(w)$ is the job acceptance probability of those jobs in the stationary equilibrium, $\bar{e}+\tilde{e}$ are the total measures of employed and workers on recall. We have: $\bar{e} = 1 - \tilde{e} - \bar{u} = 1 - \tilde{e} - \bar{v}/\theta$ where θ without the time subscript denotes market tightness in the stationary equilibrium. Its value is pinned down by:

$$\int \max\{V(w), 0\}\,dG(w) = c_e. \tag{40}$$

In order to enter the market, a firm pays a one-off cost $c_e > 0$, draws a wage w from the exogenous sampling distribution $G(w)$ and decides whether to remain inactive or post a vacancy. In a stationary economy, the asset value of the latter is $V(w)$. The "max" operator in Equation (40) captures the decision to post a vacancy under free entry of firms in a stationary environment. Under mild conditions, $V(w)$ is hump-shaped with respect to w, since a higher wage w increases the probability that the job is accepted (which lowers the expected duration of vacancy posting) but reduces the profits conditional on filling the job. Since the value of posting a vacancy for jobs that pay either 0 or y are both lower than zero, then if there exist a wage $w \in (0, y)$ such that the value of posting is positive, by the intermediate value theorem $V(w)$ crosses the 0 line at least twice.

5.5 Model Specification and Calibration

In this section, we present the model's specification and calibration. Consistent with our HB analysis, the model's period is a week. For worker's intra-period utility function, we use a CRRA function: $u(x_t) = \frac{x_t^{1-\gamma}-1}{1-\gamma}$, where γ denotes the coefficient of relative risk aversion. To ensure that the job-finding and job-filling probabilities remain below 1, we use the matching function proposed by Den Haan, Ramey, and Watson (2000): $m(u_t, v_t) = \frac{u_t v_t}{\left(u_t^\eta + v_t^\eta\right)^{1/\eta}}$, where η captures the curvature of the matching function. We assume that the exogenous wage sampling function $G(w)$ is a Normal distribution with mean y and standard deviation σ_w, truncated and normalized to integrate to 1 over the $[0, y]$ interval. For the sampling distribution of recall probabilities, $H(r)$, we also rely on a Normal distribution with mean 1 and standard deviation σ_r, truncated and normalized to integrate to 1 over the $[0, 1]$ interval (since r is a probability). Unemployment benefits are a fraction of the previous wage, i.e., $b_i(w) = \rho_i w$, where ρ_i is the replacement ratio and index i denotes the unemployment insurance status: pandemic UI (P), regular UI (R) or social assistance (S).

Given these specifications, the model has 14 parameters for the stationary equilibrium: β, γ, y, η, δ^e, δ^s, σ_w, σ_r, c_v, c_e, ρ_R, ρ_S, p_0^R, p_1^R, and three parameters, the pandemic UI system, ρ_P, p_0^P, p_1^P, that matter only in the transition. The first three parameters, β, γ, y, are set outside the model. Since the model's period is a week, we set $\beta = 0.9992$, consistent with an annual real interest rate equal to 4%. We choose $\gamma = 2$, which is a standard value for risk aversion and normalize labor productivity y to 1.

All remaining parameters are calibrated internally to match a set of data moments and reproduce some features of the UI system in place in the U.S. labor market. Table 3 of Davis, Faberman, and Haltiwanger (2013) indicates that the daily job-filling rate of vacancies in Leisure and Hospitality is 0.069, and that the job-filling rate at small establishments is about 25% higher than job-filling rate on average across all establishment size classes.[10] Thus we take the daily job-filling rate of small firms in this sector to be at 0.086. Assuming 5 business days per week, this yields a target of $1 - (1 - 0.086)^5 = 0.36$ for the weekly job-filling rate (i.e., $q(\theta)$ times the probability that job offers are accepted). The weekly job-filling rate is

[10] Davis, Faberman, and Haltiwanger (2013) do not report job-filling rates by industry × establishment size, and so we assume that the gap between small and larger firms is the same across different industries. Table 3 of their paper shows that the job-filling rate across all establishment sizes is 0.050 vs. respectively 0.061 and 0.066 for establishment sizes 0 to 9 and 10 to 49 employees.

tightly related to the curvature of the matching function, η, in a stationary equilibrium with free entry.

Our HB data show that the weekly separation rate is 8.70%, i.e. that $\delta^e + (1 - \delta^e)\,\delta^s = 0.087$. We use data from the Business Dynamics Statistics (BDS) to compute the death rate of small establishments (those with fewer than 100 employees, since the BDS does not separate establishments below vs. above the cutoff of 50 employees) in the Leisure and Hospitality sector in the pre-pandemic period. We find that the quarterly death rate on average over the years 2015 to 2019 is 8.89%, which yields $\delta^e = 0.0071$ at the weekly frequency. Together with the HB weekly separation rate, it implies $\delta^s = 0.0805$. In addition, the Homebase data shows that recalls account for 70% of all new hires. This data moment is informative to calibrate σ_r, the dispersion of $H(r)$. Intuitively, a higher σ_r increases the chances of drawing r closer to 0, which lowers the probability of a recall upon being hit by the δ^s shock. We calibrate the dispersion of wages σ_w to match empirical wage dispersion in the Homebase data. Specifically, we target the interquartile range of the residual log wage distribution, which is 0.14 across all firms in our dataset.

To calibrate c_v, we follow Elsby and Michaels (2013) who estimate that the expected flow costs of posting a vacancy is 14 percent of quarterly earnings. Note that expected flow costs of posting a vacancy depends on c_v, $q(\theta)$, $g_V(w)$ and $F_U(w)$, i.e. it is again an equilibrium outcome. We set c_v such that the expected costs is equal to $0.14 \times 13 \times \widetilde{w}$ where $\widetilde{w}$ is the equilibrium weekly wage. For the entry cost c_e, we target a start-up cost of 25,000\$, based on simulations run using tools from the Small Business Administration to calculate business start-up costs.[11] We express this number in terms of yearly earnings of employees in the Leisure and Hospitality sector, tabulated from CES data, which is 21,725\$ on average over the years 2015 to 2019. Since firms in our model can hire only one worker, we divide this number by average firm size, i.e. 6 employees according to Table 1. Our final estimate yields a start-up cost worth 19.2% of yearly earnings on a per capita basis. For a given value of c_e, the free entry condition in Equation (40) pins down market tightness, θ.

The remaining parameters relate to UI benefits. In the (pre-pandemic) stationary equilibrium, unemployed workers are either eligible to collect regular UI benefits or receive social assistance. Consistent with U.S. policies, the replacement rate of regular benefits is set at $\rho_R = 0.45$ and the expiration probability p_0^R, matches an expected duration of 26 weeks. The probability of receiving regular benefits, p_1^R, targets the recipiency rates of UI benefits. We use

[11] See https://www.sba.gov/business-guide/plan-your-business/calculate-your-startup-costs. The start-up costs cover one-time expenses (security deposit and first month's rent and utilities; improvement costs such as kitchen improvements, tables ad furnitures, ustensils, etc; inventory such as food and beverage, and miscellaneous expenses such as licenses and permits, legal fees, etc.) and expenses for the first month of operation (rent, property insurance and utilities; payroll and taxes, professional services such as accounting, legal fees, etc.; supplies, marketing and miscellaneous costs such as repairs and maintenance).

Table 6: Parameter Values

Parameter	Notation	Value	Target/Reference
Discount factor	β	0.9992	4% annual interest rate
Risk aversion	γ	2.0	Standard
Labor Productivity	y	1.0	Normalization
Curvature of matching function	η	0.6338	Davis et al. (2013)
Firm exit shock	δ^e	0.0071	BDS
Job separation shock	δ^s	0.0805	HB separation rates
Wage dispersion	σ_w	0.215	HB dispersion in store wages
St. dev. of recall prob. distr.	σ_r	1.0501	HB recall rates
Vacancy posting cost	c_v	0.5188	Elsby and Michaels (2013)
Start up cost	c_e	8.873	BDS
Regular benefits repl. rate	ρ_R	0.45	UI system
Social assistance repl. rate	ρ_S	0.15	Unemp.duration-UI elasticity
Regular UI expiration prob.	p_0^R	0.0385	UI system
Regular UI eligibility prob.	p_1^R	0.075	UI recipiency rates

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

data from the March CPS to compute UI recipiency among workers employed in the Leisure and Hospitality sector. Before the COVID crisis, we find that only about 9.5% of workers who experience unemployment in this sector receive UI benefits.

The unemployed worker receives the social assistance income either because she is not eligible or because the regular benefits expired. The replacement rate of the social assistance income ρ_S is a key parameter as it will determine the sensitivity of employment recovery to the unemployment insurance supplements. We set $\rho_S = 0.15$ to target empirical evidence on the elasticity of unemployment duration to unemployment benefits. We show in the next section that the model delivers a precise identification of this parameter. The parameters governing the steady state are described in Table 6. The pandemic UI parameters ρ_P, p_0^P, p_1^P are discussed in the next section where we describe the main quantitative experiment.

5.6 Model Fit

Table 7 shows that the model performs well with respect to the targeted moments. We do not discuss the firm's death rate (from BDS data) and the weekly job separation rate (from HB data) which the calibrated model matches exactly by definition of δ^e and δ^s. The recall option allows the model to generate realistic employment-to-unemployment dynamics.

Table 7: Model Fit

Moment	Target	Model
Recall share among total weekly hires	70%	70%
Interquartile range of residual log wages	0.14	0.14
Expected vacancy posting cost / quarterly earnings	14%	13.7%
Entry cost / annual earnings	19.2%	19.2%
Share of unemployed receiving UI benefits	9.5%	9.4%
Elasticity of unemployment duration to UI	0.24	0.24

The model matches the share of weekly hires that are recalled workers and also implies that the expected duration of a recall conditional on $r > 0$ is 3.1 weeks which seems a plausible estimate.

In the stationary equilibrium of the model, the unemployment rate is at 8.6%, the weekly job-finding rate (the product of $f(\theta)$ and the probability of accepting a job) is 25%, and labor market tightness θ is 0.715. The vacancy rate (defined as $v/(v+e)$) is somewhat high: 7.0%, while Table 1 of **?** indicates the vacancy rate across all establishment sizes is 3.5% in the Leisure and Hospitality sector.[12] This said, the authors' estimate of a 3.5% vacancy rate yields a value of market tightness of 0.70 under the assumption that the unemployment rate is 5%.[13] This value falls close to that of market tightness in the stationary equilibrium of our model.

The model matches the duration elasticity to unemployment benefits extension. As discussed, this moment is matched using the social security replacement rate. Figure 10 shows an intuitive graph of the identification. In the horizontal axis we plot social assistance replacement rate ρ_s and in the vertical the duration elasticity. To compute the duration elasticity in the model we compute, for the worker and the unemployed, the value functions with or without UI.[14] We then increase the expected duration of UI by 10% and compute the value of being unemployed with UI.

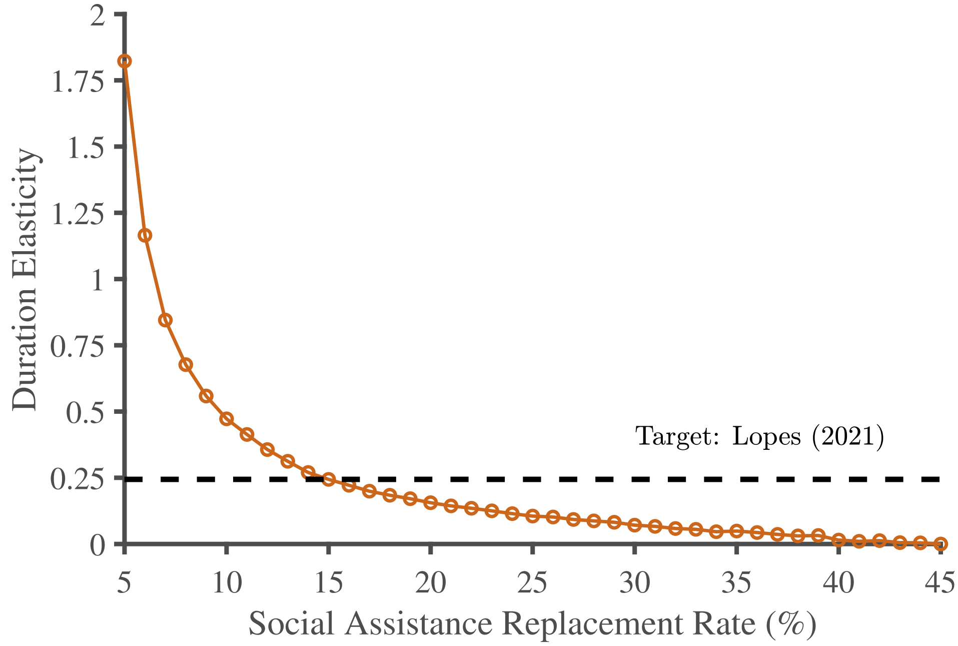
The model predicts that that the elasticity monotonically decreases to 0 as $\rho_s \to \rho_R$.

[12]Davis et al. (2013) do not report vacancy rates by industry × establishment size. Table 1 in their paper indicates that, across industries, the vacancy rate is higher at larger establishments, except for very large establishments (more than 5,000 employees) where the vacancy rate is substantially lower than establishments with 1,000 to 4,999 employees).

[13]Let ζ denote the vacancy rate, defined as $\zeta = v/(v+e)$. Since $e + u = 1$, and market tightness $\theta = v/u$, we can express tightness as a function of ζ and u: $\theta = \frac{\zeta}{1-\zeta}\frac{1-u}{u}$.

[14]To compute these asset values, we keep the job-finding rate and distribution of vacancies (and recall decisions of firms) fixed to the baseline equilibrium.

Figure 10: Identification of the Social Assistance Replacement Rate



Notes: Model-implied elasticity of unemployment duration to unemployment benefits for different levels of social assistance replacement rate.

When the social assistance replacement ratio ρ_S is equal to the regular benefits replacement rate $\rho_R = 0.45$ then the duration elasticity is zero. In this case, the unemployed receives the same amount of income whether the regular unemployment benefits have expired or not. So the regular unemployment benefits effectively last forever. As a result, an increase or decrease in the duration of regular benefits has no impact on the duration of unemployment. As ρ_s decreases then workers' become increasingly sensitive to the duration of their regular benefits.

The duration elasticity has been the focus of an extensive body of research with significant divergence on the estimates. Katz and Meyer (1990) find that if benefit duration decreases from 39 to 35 weeks the average weeks of unemployment go from 18.4 to 17.6, which corresponds to an elasticity of 0.42. Landais (2015) finds that one more week in benefits increases the unemployment spell by 0.2-0.4 weeks, which corresponds to an elasticity around 0.6 on average. Rothstein (2011) finds that a potential benefit duration from 26 to 65 weeks decreased the job finding probability by around 2 p.p. from 22 p.p. base. So the elasticity is 0.06. Lopes (2021) conducts a comprehensive literature review and documents the large divergence in the estimates of the duration elasticity with the estimates ranging between 0.02 to 1.13. The mean of the estimates is 0.244 which we use as our target.

6 Quantitative Experiments

In this section we use the model to analyze the effect of the extended pandemic unemployment insurance benefits on the employment recovery of low- and high-wage small firms. In the model, we define high- and low-paying firms by splitting firms at the median of the steady-state equilibrium wage distribution, as we did in the empirical section.

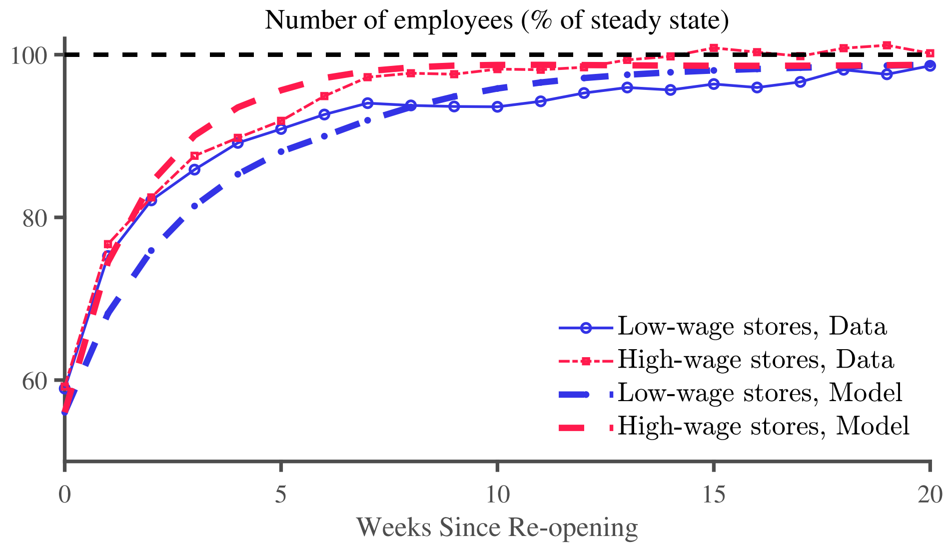
There are two shocks that take the economy out of its steady state. First, a separation shock δ_s (Covid-19 shock) that sheds workers into unemployment and filled jobs into the pool of vacancies. The separation shock takes place only at $t = 0$ and returns to steady state levels from period $t = 1$ onward. We assume that the additional job separations that occur at time $t = 0$ do not provide agents with a recall option. Indeed, Figure 5 shows that recalls play a marginal and short-lived role in the differential recovery of low- and high-wage firms. Furthermore, we are interested in the dynamics of the recovery when vacant jobs and unemployed workers must come together through a search-matching function.

The second shock is the change in the unemployment insurance benefits that mimics the change in the UI system during the pandemic. We set the replacement ratio of pandemic UI benefits ρ_R to 1.45, thus effectively implementing a replacement ratio that is higher by 100 percentage points compared to that of regular UI benefits. As regards the duration of pandemic UI benefits, we set $p_0^P = 1/6$ to capture a duration of pandemic UI of 6 weeks. The actual FPUC came into operation after the initial shock, which limits the duration of pandemic supplements that workers separated from the job in March 2020 may have received and brings it closer to 6 weeks. In line with the data, we assume that only a fraction of the unemployed (either already unemployed at time t or who become unemployed at time $t = 0$) receive the pandemic UI supplements. We set $p_1^P = 6p_0^R$, since as documented in Appendix **??** the recipiency rate of pandemic UI is six times higher than that of regular UI benefits among workers employed in the Leisure and Hospitality sector. We apply the same probability p_1^P to reallocate a fraction of the workers already unemployed at $t = 0$ to pandemic UI (such that the higher UI recipiency rates triggered by the pandemic begin at $t = 0$). We refer to the higher p_1^P and reallocation of unemployed workers to P benefits as the "extended eligibility" of pandemic UI. As long as the FPUC program is in place (the first six weeks after the shock), employed workers may receive pandemic UI upon job separation, and then their probability of receiving pandemic UI returns to 0. Last, during the first 39 weeks of the experiments, we extend the duration of regular UI benefits (R) to 39 weeks, by reducing the probability p_0^R of exhausting these benefits. Given that p_1^P becomes 0 after the end of FPUC and thatp_0^R returns to its baseline value, the economy eventually returns to its steady state equilibrium.[15]

We evaluate if the model can replicate employment recovery of low- and high-wage stores.

[15] Appendix **??** presents our algorithm to compute the transition path of the model.

Figure 11: Employment Recovery in low- and high-wage stores: Model vs. Data



As a reference, we use the employment recovery of re-opening stores (documented in Figure 5). In the data, even six months after reopening, the stores converge to 80% of normal employment suggesting that the Covid-19 shock was more persistent than what we consider in the model. Since we care about the differences in the recovery between wage groups and not the overall recovery, we normalize the empirical series at 100% in week 25.

We show the results in Figure 11. The job separation shock is calibrated to match the decline in employment by about 40% in the re-opening week. The model generates a comparatively slower recovery for low-wage firms, that is qualitatively and to a large extent quantitatively similar to the patterns from the HB data. One difference is that in the model high-wage employment diverges immediately from low-wage employment while in the data divergence (as well as convergence to the steady state) is more gradual.

What generates the different employment dynamics between low- vs. high-wage firms in the model? From the law of motion of employment (Equation 26) we have that that the overall job filling probability for store offering w is $f(\theta_t)\, g_{V,t}(w)\, F_{U,t}(w)$. The separation shock increases the number of unemployed as much as the number of vacancies (i.e., the numerator and denominator of θ increase by the same number). Since we have one worker in each firm and θ is calibrated to a value lower than one, θ increases initially and so does the the probability of meeting a vacant job, $f(\theta_t)$. On the other hand, the probability of acceptance, $F_{U,t}(w)$ decreases, and especially so for low wage firms. This happens because workers become more selective about jobs, by increasing their reservation wage.

It is not clear whether the differential employment recovery shown in Figure 11 arises due

Table 8: Relative and Average Effects of Pandemic UI Effects on Employment

	Combined shocks (1)	Separation shock (2)	Pandemic UI (3)
Low-wage firms	-8.1%	-5.3%	-2.8%
High-wage firms	-4.3%	-4.1%	-0.2%
Low- vs. High-wage firms	-3.7%	-1.2%	-2.5%
All firms	-6.2%	-4.7%	-1.5%

to the job separation shock or the changes in the UI system. Table 8 separates these shocks. In column (1) we report the employment decline when we have both the separation shock and the pandemic UI (i.e., corresponding to Figure 11). In column (2) we report the employment decline when we have only the separation shock and in column (3) their difference which corresponds to the marginal effect of the pandemic UI supplements. The employment decline refers to the decline relative to the steady state over the first 25 weeks.

Table 8 also helps to distinguish between the relative disincentive effects—estimated in the data—from the average disincentive effect. In particular, we report separately the employment decline for low- and high-wage firms, their difference, which is the comparable moment in the data, and finally the average effect.

The average employment decline over the first 25 weeks is 8.1% for low-wage stores and -4.3% for high-wage stores. The pandemic UI suppements decreased employment by 2.8% for low-wage stores and by 0.2% for high-wage stores. Hence, the decline in employment of high-wage stores is mostly attributable to the separation shock and the pandemic UI effects are concentrated on low-wage stores. These results model confirm our initial hypothesis that low- and high-wage stores are differentially affected by the UI. The relative disincentive effect of pandemic UI is 2.5% in the model which is reasonably close to the disincentive effect of 1.7% we estimated in the data.

7 Conclusion

We distinguish between the disincentive and the stimulative effects of pandemic UI benefits by comparing the employment recovery of low- versus high-wage establishments within narrow local industry markets. Employment in high-wage establishments recovered faster while hours per employee and hourly wages grew slower relative to low-wage stores. Our identification assumption is that the local stimulus is shared by neighboring stores of the same local industry, a plausible assumption for narrow levels of aggregations (e.g., zip codes). Indeed, when we aggregate local industries based on broader level of aggregations our estimates become small and insignificant.

We build a quantitative labor search model to explain the slower employment recovery of low-wage establishments relative to high-wage establishments. The model allows us to recover not only the differential employment recovery of low- vs. high-wage stores, but also the absolute impact of unemployment insurance benefits on the recovery of each type of stores. The model calibrated to several labor market moments before and during the pandemic replicates in a reasonable way the differential recovery.

Based on our empirical and theoretical analysis we draw two conclusions. First, when one properly controls for local demand shifts, the disincentive effects of UI turn out to be sizable. Second, a relatively standard quantitative model of labor search is able to replicate the disincentive effect of pandemic UI benefits.

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