Employment Effects of Unemployment Insurance Generosity During the Pandemic

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Abstract

In response to the Covid-19 pandemic, the United States enacted the CARES Act, which expanded unemployment insurance (UI) benefits by providing a $600 weekly payment in addition to state unemployment benefits. We test whether changes in UI benefit generosity are associated with decreased employment, both at the onset of the benefits expansion and as businesses began to reopen. We use data from Homebase, a private firm that provides scheduling and time clock software to small businesses, which allows us to exploit high-frequency observations to understand how firms and workers respond to policy changes in real time. While our results show that relative declines in employment and hours occurred in mid-March, we find that the workers with higher post-CARES replacement rates did not experience larger declines in employment or hours of work when the benefits expansion went into effect. They have also returned to their previous jobs over time at similar rates as others.

Keywords: Unemployment Insurance, Employment, COVID-19, CARES Act

JEL Codes: J65, J68

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1 Introduction

The Coronavirus Aid, Relief, and Economic Stimulus (CARES) Act instituted a variety of economic policy responses to the Covid-19 pandemic in the United States. One such policy was a large, temporary expansion of unemployment insurance (UI) benefits known as Federal Pandemic Unemployment Compensation. The expansion provided a $600 weekly payment in addition to any state unemployment benefits for which a worker would have already been eligible.

The payment was designed to replace 100 percent of the *mean* U.S. wage when combined with existing UI benefits. However, the extra $600 weekly payment provided under CARES yields a total UI benefit that is greater than weekly earnings when working for the *median* worker. Ganong *et al.* (2020) estimate *ex post* replacement rates over 100 percent for 68 percent of unemployed workers who are eligible for UI, as well as a median replacement rate of 134 percent. Given the moral hazard effects of unemployment benefits that are well-established in the literature, it is natural to ask whether such high replacement rates affect employment levels *under the distinct conditions of the pandemic*.[1]

In this paper, we test whether higher UI benefits are associated with decreased employment, both at the onset of the benefits expansion and as businesses reopened. We use data from Homebase, a private firm that provides scheduling and time clock software to small businesses, which allows us to exploit high-frequency observations to understand how firms and workers respond to policy changes in real time. The longitudinal data allows us to estimate UI benefits for each worker in our sample and to follow their labor market status through early 2020. Our sample over-represents small businesses with hourly workers. This population is of particular interest for our study, because they were disproportionately affected by the pandemic (Bartik *et al.*, 2020a) and face higher replacement rates after CARES, given their lower earnings.

First, we employ an event study design to test whether exposure to higher replacement rates after the passage of the CARES Act on March 27 is associated with a differential decrease in employment or hours of work. We complement these results with a parsimonious specification testing directly for incremental effects after the benefits expansion. In both analyses, we flexibly control for state-industry-week trends. We find that workers with more generous UI benefits did not experience differential declines in employment after the CARES Act was passed. While there is a negative association between replacement rates and employment, it is fully established *before* March 27. Our study benefits from high-frequency data that allows us to isolate

[1] Schmieder and von Wachter (2016) review the literature on moral hazard effects of unemployment insurance.

the timing of the CARES Act from other drivers of reduced employment beginning in mid-March. Such factors include the pandemic-induced decline in labor demand, fear and concern about public health, and increased childcare costs.

We support our main results with several robustness exercises varying the dates of analysis, the definitions of treatment and outcome variables, and the set of controls. We also test whether exposure to higher replacement rates makes workers less likely to return to work, conditional on working at firms with observably increasing labor demand. Additionally, we test whether workers' wages increase when they are *re*-hired. Acknowledging the limitations of our data and sample restrictions, we conduct additional exercises on a broader sample of workers in the Homebase data by exploiting variation in state-industry median replacement rates. We also replicate our main results using the Current Population Survey (CPS). All of the additional tests support the same conclusion: the negative labor market effects associated with replacement rates are attributable to changes in mid-March; we do not observe negative effects after the passage of the CARES Act. If anything, groups facing larger increases in benefit generosity experience slight *gains* in employment relative to the least-treated group starting in early May. Furthermore, the CPS exercise highlights that our results are not merely an artifact of our sample selection or of the structure of our analyses. Rather, they are representative of trends in the labor market more broadly. These results suggest that, in the aggregate, the expansion in benefit generosity did not decrease employment at the outset, and that high replacement rates did not make workers differentially less likely to return to work.

It is important to emphasize the limitations of our study. First, our empirical strategy is not suitable to estimate the causal effect of replacement rates and should not be interpreted as such. We can, however, test the differential responses before and after the CARES Act. Second, the Homebase data over-represents small businesses and is concentrated in specific sectors, e.g. restaurants. Third, in order to precisely estimate workers' replacement rates we restrict our sample to workers with relatively high attachment to their jobs. Fourth, since our analyses rely on links between firms and workers within the Homebase universe, we cannot observe rehiring activity outside of Homebase firms.

Our analysis contributes to the growing literature on labor market effects of Covid-19. Our findings are consistent with other results in the literature: lower-wage workers, who have higher *ex post* replacement rates, were the most affected in the early weeks of the pandemic. Goolsbee and Syverson (2020) highlight the role of agents' fear and individual choices to stay home in determining economic activity during the pandemic. Altonji *et al.* (2020), Bartik *et al.* (2020a), Cajner *et al.* (2020), Chetty *et al.* (2020), Fairlie *et al.* (2020), Gupta *et al.* (2020), and Montenovo *et al.* (2020) use various data sources – including the Homebase data – to document trends in employment and spending during the pandemic. Ganong *et al.* (2020) estimate *ex post* replacement rates over 100 percent for 68 percent of unemployed workers who are eligible for UI, as well as a median replacement rate of 134 percent. Bartik *et al.* (2020a) use data from Homebase to show that states with higher *ex post* median replacement rates tended to have lower initial decreases in employment and recovered more quickly than others with lower replacement rates. Marinescu *et al.* (2020) use job application and vacancy data to assess whether increasing replacement rates under the CARES Act cause employers to have difficulty rehiring workers. They find that while applications and applications-per-vacancy decreased more for occupations and states with larger increases in the replacement rate, these differences are not explained by the CARES Act alone. These results are consistent with our findings. We contribute to this literature by leveraging high-frequency data linking individuals and firms to directly estimate workers' replacement rates and observe individual employment, hours of work, and re-hiring over time.

Many works analyze the effects of increasing unemployment benefits during recessions, including Mitman and Rabinovich (2015), Hagedorn *et al.* (2016), Landais *et al.* (2018), and Hagedorn *et al.* (2019), who explore the UI duration extension during the Great Recession. The policy change that we analyze provides an advantage: the extra generosity was the same across all states and was not endogenously determined by local economic conditions. Nevertheless, we emphasize that the Covid-19 pandemic presents a unique context – combining a health and an economic crisis – from which it is difficult to generalize labor market effects of UI. First, median replacement rates are over 100 percent for the first time in the United States. Workers may behave much differently when facing current replacement rates than they would under ordinary circumstances. Second, the public health context makes the current moment distinct from other recessions because it creates unique barriers to labor force participation, such as the fear effect documented in Goolsbee and Syverson (2020), safety concerns associated with working, and increased costs of childcare.

The paper proceeds as follows. In the next section we present the institutional setting of unemployment benefits in the United States and changes under the CARES Act. We present the data and sample restrictions in Section 3 and the empirical strategy in Section 4. Section 5 provides the main results. We conduct additional exercises in Section 6.

2 Institutional background

2.1 UI benefits eligibility

To compute an individual's eligibility for unemployment benefits, all states use a worker's earnings in the four most recent completed quarters. Most compute benefits as a percentage of the worker's highest quarterly earnings, second-highest quarterly earnings, or annual earnings, subject to a minimum and maximum benefit level (Ganong *et al.*, 2020). The generosity of benefits varies substantially by state, as shown in Appendix Figure A.2. To compute UI benefits, we use workers' earnings histories in the four quarters of 2019.

While the CARES Act expanded eligibility for UI, several institutional features restricting eligibility remain. First, even under CARES, a worker who quits her job is ineligible for UI. While workers who quit due to exceptional circumstances related to Covid-19 – e.g. having a respiratory condition that makes work risky – are exempted from this, those who quit for no reason other than concern about contracting Covid-19 are not eligible for UI. Second, once a person receives a "suitable offer of employment," they are no longer eligible for UI even if they reject the offer. The Department of Labor states that "a request that a furloughed employee return to his or her job very likely constitutes an offer of suitable employment that the employee must accept" (U.S. Department of Labor (2020)). In practice, compliance with both rules may be lower at small firms where employers interact with workers more informally.

2.2 Timing of the CARES Act

On Thursday, March 19, Senate Republicans introduced a $1 trillion economic relief package. The bill in its original form did not include supplemental unemployment insurance (Sullivan (19 March 2020)). News coverage of the progress of the bill indicates that legislators agreed to include supplemental unemployment benefits on Monday, March 22 (Cochrane *et al.* (22 March 2020)). The structure of unemployment benefits continued to be contested throughout the week, particularly the duration of the benefits extension. The bill passed the Senate on March 25 and the House of Representatives on March 26, and was signed into law on Friday, March 27.

The timing of events in the passage of the stimulus bill is relevant to employers' and workers' plausible responses to the policy intervention. Since supplemental unemployment insurance did not appear in the draft bill until Monday of the week in which it was passed, and was contested in subsequent days, it is unlikely that the decision to open a firm in the week beginning March 21 or to lay off a worker prior to the start of work in that week could have been influenced by anticipation of enhanced unemployment benefits.

3 Data

Our main dataset comes from Homebase, a private firm that provides scheduling and time clock software to small businesses, covering a sample of hundreds of thousands of workers across the U.S. and Canada. Homebase's clients are primarily small firms that require time clocks for their day-to-day operations, nearly half of which are in the food and drink industry. The basic version of Homebase is free to firms. Workers are predominantly hourly, not salaried, employees.[2]

Because of these limitations, insights about the Homebase sample are not representative of the entire labor market. However, as Bartik *et al.* (2020a) note, the population covered by Homebase is of particular policy interest since it represents a segment of the labor market disproportionately affected by the pandemic. In the context of unemployment benefits generosity, the Homebase sample is valuable because it covers workers with relatively low wages: most are in the first and second quintiles of national earnings from the CPS (Altonji *et al.*, 2020). These workers thus experience particularly high UI replacement rates from the addition of the $600 supplemental payment.

We use the data's longitudinal structure to follow workers and firms over time, beginning in 2018. We observe workers' daily shift data, including hours worked, hourly wage, and total earnings.[3] Each worker is linked to a firm. For each firm, we observe state, metro area, and industry.[4] We impose the following data restrictions: i) we keep firms that logged positive hours for at least 5 weeks between 2019-2020, ii) we keep workers who worked at least 20 hours per week in the baseline period (January 19 to February 8, 2020), and iii) we keep only workers who were employed in all quarters of 2019, with positive earnings and at least 300 hours worked in each quarter.

The last requirement is the most restrictive. We impose it because we need to observe a worker's 2019 earnings history to accurately compute their unemployment benefits. Since we only observe an individual's work history when their firm is in the Homebase data, this restriction aims to exclude individuals who worked in other jobs during 2019.[5] We compute pre- and post-CARES UI benefit replacement rates using

[2]While some firms could also list their salaried employees in the data, their earnings are not usually recorded. We thus exclude them from our analysis.

[3]Wages and earnings are available for approximately 52% of the workers. This variation is determined at the firm level, since some firms use the software just to register shifts but not to track payroll.

[4]We show industry composition in table A.1.

[5]Some workers in Homebase may have worked for other firms, either full-time prior to their employment at the Homebase

the calculator developed by Ganong *et al.* (2020) using state identifiers and earnings in the four quarters of 2019. When computing workers' quarterly earnings, we floor their wages at the state minimum wage. Many workers' posted wages are below the state minimum because they work for tips. However, U.S. labor law requires that employers must "top up" workers' wages to the state minimum wage if their effective wage in a given week is below the state minimum.

The resulting dataset has 29,005 workers. We present descriptive statistics in table 1. In the base period, which covers the three weeks between January 19 and February 8, these individuals worked on average 36.9 hours per week and earned an average hourly wage of \$13.3, resulting in average weekly earnings of \$495.41, similar to average weekly earnings in 2019 of \$479.50. Under state benefit schemes prior to the CARES Act, the unemployment benefit levels in our sample result in replacement rates from 6.4% to 88.6%, with a mean of 55.1%. After the CARES Act, replacement rates range from 27.3% to 410.8%, with a mean of 192.4%. We group the sample into quintiles by post-CARES replacement rate. The range, mean, and median of each quintile are presented in panel B of table 1.

We note that the above restrictions necessarily exclude the shortest-tenured workers at a given firm, as well as workers at newly established firms. Our sampled workers may be less likely to be laid off in an economic downturn than shorter-tenured workers. In addition, the Homebase data is subject to some additional limitations. Notably, workers who have been furloughed (i.e. are still employed by a firm but are not working any hours) are not distinguishable from workers who have been formally laid off. In order to overcome this limitation we code all workers that do not report hours for three days in a row as being non-employed. This choice is likely to overestimate any effect of the policy on non-employment because lower-earning workers have higher replacement rates and also are more likely to have irregular or reduced work schedules during the pandemic. Thus they would be more likely to be erroneously coded as non-employed.[6] In the Appendix we also show that our conclusions are robust to more traditional labor market data from the CPS. However, using this data imposes constraints on our analysis, since it does not allow us to track workers' employment and hours on a daily basis or to follow them for more than four months. Particularly in the early weeks of the pandemic, the policy and public health situation changed rapidly, so we lose granularity with lower-frequency data.

member firm or part-time concurrently with their employment at the Homebase member firm. Workers' UI benefits are computed on the basis of their total earnings in the last four quarters, so we drop workers for whom we are likely to not be observing their full earnings history. Furthermore, if a worker has multiple jobs and is only laid off from the job at the Homebase member firm, they would not be eligible for UI benefits.

[6]As discussed in section 5.2, the conclusions are robust to changes in this definition.

We supplement our dataset with two additional sources. To track start and end dates of state-level restrictions in response to the pandemic, we use the COVID-19 US state policy database maintained by Raifman *et al.* (2020). We use four types of restrictions: stay-at-home orders, closures of non-essential businesses, restrictions on restaurants, and closures of gyms. Additionally, we use data on new Covid-19 cases from *The New York Times* to measure the pandemic's severity across states.

4 Empirical approach

4.1 Measuring replacement rates

The *ex post* replacement rate R_{ijs} for individual i working in industry j in state s is determined by her pre-CARES UI benefits (UI_{ijs}^{Pre}), the additional \$600, and her weekly earnings in a chosen reference period (w_{ijs}). In our baseline specification we choose w_{ijs} to be her average weekly wage in 2019. Formally:

$$R_{ijs} = \frac{UI_{ijs}^{\text{Pre}} + 600}{w_{ijs}}. \tag{1}$$

We also estimate our specifications with an alternative definition of the treatment variable. Since the *ex post* replacement rate is mechanically related to the *ex ante* replacement rate, we test the robustness of our main results to a specification in which we exploit variation from the differential change in replacement rates from the incremental \$600. We substitute the replacement rate with the *replacement rate ratio* R_{ijs}^{ratio}, formally:

$$R_{ijs}^{ratio} = \frac{R_{ijs}}{R_{ijs}^{\text{Pre}}} = \frac{\frac{UI_{ijs}^{\text{Pre}}+600}{w_{ijs}}}{\frac{UI_{ijs}^{\text{Pre}}}{w_{ijs}}} = 1 + \frac{600}{UI_{ijs}^{\text{Pre}}}, \tag{2}$$

which measures the change in UI generosity. By construction, R_{ijs}^{ratio} is 1 for all workers pre-CARES. This definition has the advantage of removing part of the mechanical effect of wages on the denominator of equation 1 and of changing the direction of the effect of *ex ante* state generosity on the replacement rate variable.[7] While this measurement strategy does address some endogeneity concerns with respect to the states' *ex ante* replacement rates, it is not clear that R_{ijs}^{ratio} is the relevant price in workers' labor supply decisions.

[7] *Ex ante* more-generous states have higher R but lower R^{ratio}, holding wage constant.

4.2 Event study

Leveraging the frequency of the data, we explore how workers facing different UI generosity post-CARES behave before and after the benefits expansion. Our main analysis consists of two strategies. Our first strategy looks into workers' weekly employment status and hours worked over time depending on their *ex post* replacement rate. We estimate the event study specification:

$$Y_{ijst} = \alpha_0 + \sum_{\tau=0}^{T} \sum_{g=2}^{5} \beta_t^g R_{ijs}^g \mathbb{1}\{t = \tau\} + \eta_{jst} + \varepsilon_{ijst}, \tag{3}$$

where Y_{ijst} is the outcome for worker i associated with industry j in state s during week t. R_{ijs}^g is an indicator that worker i's replacement rate after the CARES Act places them in replacement rate quintile g. η_{jst} is a state-industry-week fixed effect, which subsumes all state-industry weekly variation, including the severity of the pandemic in each state and states' restrictions on business activities.

This strategy allows us to explore the labor market dynamics for individuals with different replacement rate levels, before and after the CARES Act. We explore the differential treatment intensity, that is, the generosity of UI measured in terms of the replacement rate, to empirically assess whether workers with higher R have differential employment and hours of work trajectories.

While the first strategy allows us to investigate the full dynamics of the labor market outcomes, in the second strategy, we combine the pre- and post-CARES coefficients to test directly if there were differential responses after CARES. With this parsimonious specification we leverage the frequency of the data, estimating the following specification at the daily level:

$$Y_{ijsdt} = \alpha_1 + \gamma R_{ijs} + \delta Post_{dt} \times R_{ijs} + \eta_{jst} + \nu_d + \varepsilon_{ijsdt}^1, \tag{4}$$

where R_{ijs} is the replacement rate, $Post_{dt}$ is an indicator for the period after the CARES Act, and ν_d a day-of-the-week fixed effect. All other variables and parameters are the same as equation 3.[8] We estimate this specification in the window between March 15 and July 27. The initial date is chosen to define the pre-period as the time when the economy had already been affected by the pandemic, but the CARES Act had not yet passed. As in the first approach, we test if higher replacement rates are associated with differential labor market outcomes after CARES. In both specifications, we cluster standard errors at the worker level

[8] Panel (a) of table 2 shows results with the replacement rates in quintiles as in equation 3; panel (b) shows results using the continuous replacement rate.

following Bertrand *et al.* (2004) and Abadie *et al.* (2017).

Our strategies do not estimate a causal effect of the replacement rate on labor outcomes, as we do not estimate a counterfactual path of labor outcomes without the benefits expansion. Instead, we explore differences in the treatment intensity and assess empirically whether a relatively higher replacement rate is associated with lower employment or hours after CARES. If that is true, δ will be negative in equation 4 and the β_t^gs will be declining in equation 3 after March 27.

The most important assumptions in our analysis are i) that individuals did not anticipate the decision and exit the labor force before the Act was approved, and ii) there are no other factors that correlate with R and are simultaneous to the CARES act. We argue that anticipation is unlikely for two reasons. First, the timeline of negotiations indicates that the $600 additional benefit was not agreed upon until at least Tuesday, March 24. Even if workers stopped working the following day, they would be coded as employed in the entire pre-period. Second, workers will face any increased incentive to exit only after they are able to receive extra benefits. Even though CARES became law on March 27, workers would not become eligible for expanded benefits until the following week. Indeed, in many states, implementation took effect on a much longer timeline and workers were not able to receive enhanced benefits for several more weeks. In order to violate ii), a factor would need to correlate with labor market outcomes and R, within state-industry, and have a differential effect before and after March 27th, such that the pre-CARES replacement rate would not capture it.

5 Results

5.1 Event Study

Figure 1 shows weekly trends for employment and hours of work for workers in our sample by replacement rate quintile. Individuals with higher replacement rates had a larger reduction in hours of work and are less likely to be employed relative to the baseline period (January 19 to February 8).[9] However, it is clear that both drops occurred in mid-March, before the CARES Act. Differential changes in labor outcomes are concurrent with the beginning of the labor market shock of the pandemic, but not with the legislation. All groups behave similarly after the Act took effect; we do not see any evidence of more negative results for the higher quintiles.

[9] An individual is coded as employed in a given week if she worked any positive hours in that week.

In Appendix Figure A.1 we reproduce one graph from Altonji *et al.* (2020) to show that the drop occurred at the same time – the two weeks of March prior to the passage of the CARES Act – for all industries and that no similar trend is detected in 2019. From this figure is also clear that there is heterogeneity in the size of the effect by industries which motivates our event study specification, where we control for state-industry effects that vary over time.

Figure 2 plots the β_t^g coefficients on the interaction between the week dummies t and the replacement rate quintiles g in equation 3 with 95% confidence intervals. The coefficients represent the percentage change in hours (panel a) and probability of employment (panel b) relative to the first replacement rate quintile in a given week. We control for state-industry-week effects in both panels.

The results are similar to the averages without controls shown in figure 1: workers with different UI generosity did not experience different declines in hours and employment after CARES. While the workers with the highest replacement rates experience the largest declines in employment relative to the January baseline, the differential decline occurs entirely in the weeks prior to the passage of the CARES Act. Furthermore, the figure suggests that workers with larger increases in benefit generosity are no slower to return to work than others with more modest UI increases. Even if many states experienced implementation delays of several weeks, we could expect at least some drop in the first full week and a significant drop relative to the pre-period once all states had implemented the expanded UI benefits, when controlling for state-industry-week effects. We observe no such pattern. Appendix Figure A.3 reproduces the same analysis using the replacement rate ratio as the treatment effect, yielding similar results.

5.2 Regression Results

We supplement the graphical evidence with a parsimonious comparison of labor market outcomes on days before and after the passage of the CARES Act. Our benchmark specification compares the dates between March 15 and March 27 (pre-CARES, during the pandemic) to the dates between March 28 and July 27 (post-CARES). Results are presented in table 2.

We present two specifications: first, with the replacement rate quintiles (Panel A); second, with the continuous replacement rate (Panel B). The results resemble those in the event study approach: the majority of the negative association between employment or hours of work and replacement rates is explained by variation occurring before – not after – CARES. In the first three columns we show that this result is robust to controls for industry-week, state-industry-week, and firm-week fixed effects. The next three columns show the same for hours worked. Panel A shows that all quintiles had, relative to the first one, lower employment and lower hours worked in the pre-period. However in the post period, all coefficients are small and in some cases even positive. Panel B shows the specification with continuous replacement rate, which is coded such that a 100% replacement rate implies $R = 1$. In each case the coefficient on $Post * R$ is close to zero. Even the most negative results, in columns (1) and (3), are very small – they indicate that a 100 percentage-point in replacement rate would be associated with just a 0.6 percentage-point decline in employment.

We subject our main specification to several robustness tests. First, we recognize that the rapid economic downturn beginning in early March may make our results sensitive to the definition of the pre period in pre/post specifications. To address this concern, we vary the start date of the period we designate as prior to the CARES Act from our benchmark specification. Appendix Table A.2 shows results using start dates of March 10, 12, 15 (baseline), 17, and 20. Second, while in the main specification we define $t = 0$ as March 27, the date on which the CARES Act was passed, several sources have documented that there were delays in implementing the enhanced benefits. To address this, we vary the event date to April 3, April 10, April 17, and April 24. Panel B of Appendix Table A.2 shows results using each of these event dates. Setting a later event date in fact only increases the coefficients on $Post * R$. However, in each case there is little change in the coefficients and the overall finding remains the same: most of the negative association between replacement rate and labor outcomes is explained before CARES.

Third, we test three alternative definitions of the treatment variable (R). In our main specification we floor workers' wages at their states' minimum wages to account for U.S. labor laws, as discussed in section 3. In the second column of Appendix Table A.3 we relax this assumption and use R without the floored wages. In the third column we estimate the specification with the replacement rate ratio (R^{ratio}) as defined in equation 2. In the fourth column we use replacement rates relative to weekly earnings in the baseline period (January 19 to February 8) instead of 2019 earnings.[10] In each variation the results remain qualitatively the same.

Fourth, we vary the measure indicating whether a worker is employed on a given day. Since we use daily data, it is difficult to disentangle whether a worker has in fact become unemployed. In Appendix Table A.4 we test different non-employment definitions: a worker is considered non-employed if she reports zero

[10]Some workers in the data have no earnings in the base period. In this analysis we only considered those with average weekly earnings of at least 200 dollars in these weeks. This excludes 4.7% of individuals.

hours for one day, three days (baseline), five days or the entire week. The results again remain qualitatively the same regardless of the definition of non-employment. In the last column we also show that using a Logit specification for employment does not change the result.

Together, these results find no evidence to support the hypothesis that the CARES Act had strong negative effects on employment and hours either at the onset of the expansion or as firms looked to return to business over time. This is consistent with descriptive evidence in Bartik *et al.* (2020a) and in Marinescu *et al.* (2020).

6 Additional analyses

Our main results do not show evidence of a negative effect of the CARES Act on employment or hours between March and July. The results are subject to two major caveats. First, replacement rates are endogenously determined by wages. Second, our selected sample of workers and firms may behave differently from other agents of interest in the pandemic labor market. While we cannot perfectly resolve these questions with our main strategy and data, we conduct several additional exercises to test the strength of our findings.

6.1 Controlling for wages

In our baseline specification, we do not control for base period hourly wages or earnings. We choose this specification because once we control for earnings, we rely exclusively on variation coming from (1) the state's ex-ante generosity level and (2) the functional forms of the UI benefit formulae and of the replacement rate.[11] It is problematic to use a treatment that varies only at the state level because different states saw wide variation in the impacts of the pandemic in different weeks. To mitigate this concern, when we control for wages we also include controls for state-level restrictions on business activities and the number of Covid-19 cases reported at the state level in the previous 7 days.

This exercise attempts to control for the established fact that workers with lower wages were more likely to be laid off during the pandemic. In column 2 of Table 3 we add a control for the worker's wage in the three-week base period; in column 3 we include the worker's base earnings interacted with week. The inclusion of hourly wage does not change the coefficients while the introduction of base earnings explains almost all of the drop before CARES. Even then, there is no systematic negative effect after the passage of

[11]There is another source of variation once base period hourly wages and earnings are not *perfectly* correlated with earnings 2019 which was used to compute the replacement rate.

the Act – the coefficient on *Post* changes from 0.0004 to -0.003. Additionally, the reduction in the magnitude of the coefficient on replacement rate from -0.12 to 0.012 suggests that baseline earnings partially explain the negative relationship between replacement rate and employment before the CARES Act was passed. We also show in Appendix Figure A.4 the event study specification with the wage and earnings controls.

6.2 Rehiring at firms with increasing labor demand

So far, our analysis includes all firms whose workers meet our sample restrictions. However, since many firms have ceased operating entirely during the pandemic, our null result could be driven by the total lack of labor demand. We aim to address this concern by testing whether firms that have *increasing* labor demand in fact experience difficulty in rehiring workers. This allows us to exclude the effect of depressed labor demand across the economy.

First, we test whether workers at firms that are growing have differentially lower probabilities of employment if they have higher UI replacement rates. We define a firm as "growing" based on a leave-out measure of growth in hours worked. Specifically, for worker i at firm j in week t, we define j to be growing if the number of hours worked by workers other than i is higher in week t than in a chosen reference week t^*. The hours growth rate, HG_{ijt}, given by

$$HG_{ijt} = \frac{\sum_{k \neq i} h_{kj,t}}{\sum_{k \neq i} h_{kj,t^*}} - 1. \tag{5}$$

In column 4 of table 3 we fix the reference week to be the week before the passage of the CARES Act (the week of March 22). In column 5 we compare all weeks with the one immediately before, i.e. we compare hours worked in week t to hours worked in week $t-1$. Our sample in weeks after CARES consists of firm-week observations in which the firm is growing. In weeks prior to CARES, we include all firms that are in the sample in at least one week after CARES. Even using the restricted sample of growing firms, we do not find a negative effect on labor outcomes of the additional benefits. Figure A.5 in the appendix shows figures for an event study specification for this sample of growing firms.

6.3 Wages

Our analysis so far has focused on effects of expanded UI generosity during the pandemic on employment and hours of work. However, it is possible that the expanded benefits could also affect wages of either all working individuals or those that are re-hired by raising the value of a worker's outside option. In columns 6 and 7 of table 3 we estimate equation 4 on the outcome of log-hourly wages for all working individuals (column 6) and for the subset of workers who are re-hired (column 7). We define a worker as rehired if she returns to her job after reporting zero hours for at least one week and consider their wage when they were re-hired. We do not find any effect of the replacement rate after CARES on wages.

6.4 Samples

Our preferred specification takes advantage of the ability to observe a worker's full earnings history in 2019 to calculate the actual benefits for which she would be eligible. However, this requires us to restrict our sample to the set of workers who work continuously at the same firm throughout 2019, and remain employed there in 2020. Among workers in lower-wage service jobs with high rates of turnover, such as those represented in the Homebase data, these workers have relatively long tenures. It is plausible, then, that among workers at a given wage those analyzed in our main specification are more strongly attached to their jobs for unobserved reasons. Additionally, we can only observe job finding among workers who return to their prior jobs. Because of the legal frictions presented in section 2.1, workers receiving offers to return to their prior jobs may be less sensitive to UI replacement rates than those who would have to search for a new job. One might be concerned that workers with higher replacement rates might be less likely to search for new jobs than those with lower replacement rates, and that our analysis would not capture this variation since we cannot observe workers' probabilities of finding new jobs.

To address this concern, we construct an additional specification in which we include each worker who worked at least 20 hours per week on average during our three-week base period, regardless of their history in 2019. We attribute to each worker the median replacement rate for their state-industry from our main analysis[12]. Table A.5 shows results from the specification using state-industry median replacement rate in place of the individual replacement rate. We do not find evidence of negative effects of the median state-industry replacement rates after the CARES Act. This finding is consistent with the evidence from Bartik *et al.* (2020b) using heterogeneity in state generosity levels to compare outcomes for workers in the Homebase sample.

Finally, given that the Homebase data covers a limited subset of the U.S. labor market, we also replicate our main results on probability of employment using a nationally representative sample, the CPS. We present

[12] We only keep state-industries that have at least 35 workers.

our sample restrictions, strategy and results in Appendix A. Our results (in Figure A.3 and in Table A.6) are qualitatively the same: while there is a large negative association between replacement rates and probability of employment, it is attributable to changes before the passage of the CARES Act. There is no statistically or economically significant change in differential employment levels after the CARES Act.[13] These results also address the concern that our results leave out workers who would need to search to find new jobs, since they follow the individual regardless of their firm.

7 Conclusion

We leverage high-frequency data from daily shifts of workers in small businesses in the United States to test whether individuals with ex-post higher UI replacement rates were less likely to be employed, were working fewer hours, or were less likely to be re-hired after the CARES Act than those with lower replacement rates. While we do find this negative association, we show that it is explained by changes that occur before the benefit expansion — indicating that it was driven by the pandemic itself and not by the policy response to it. This paper provides evidence that expansions in UI replacement rates did not increase layoffs at the outset of the pandemic or discourage workers from returning to their jobs over time. We note that our results do not necessarily imply that such responses do not exist – rather, they suggest that expanding UI generosity has not depressed employment in the aggregate.

We emphasize that our results do not speak to the disemployment effects of UI generosity during more normal times. The severity of the decline in labor demand and the health risks to workers make the current pandemic different. While explanations for these findings exceed the scope of this paper, we conjecture that the unique conditions of the pandemic may explain this null result. First, there has been a broad-based decline in labor demand. Second, several other factors may also drive workers to choose to stay home: fear, risk, and generalized concern about the pandemic could play a large role, as argued by Goolsbee and Syverson (2020), and childcare costs have risen substantially. Third, for many workers health insurance and other benefits are tied to their jobs. Fourth, UI – and more importantly the extra benefit – is limited in duration. Taken together, these factors limit the perceived value of the expanded UI, increase the value of having a job, and may decrease current and future job-finding rates.

[13]The CPS sample ends in the first week of June, since July data are not yet available. Our results in Homebase indicate that relative employment continued to rebound after the end of the CPS sample. As CPS data becomes available it will be valuable to continue to validate results from Homebase with results from the CPS.

We qualify our work with several caveats. First, our main sample is not representative of the full U.S. labor market. The firms in Homebase overrepresent the food and drink industry, and workers in our sample tend to be hourly wage workers. Additionally, the sample selection criteria in our main results exclude part-time workers and those working at a given firm for less than a year. Second, while we do control flexibily for state-industry-week trends there could be additional sources of unobserved state-industry level variation in employment outcomes that we do not account for here. Future research might explore alternative identification strategies to attempt to address this issue.

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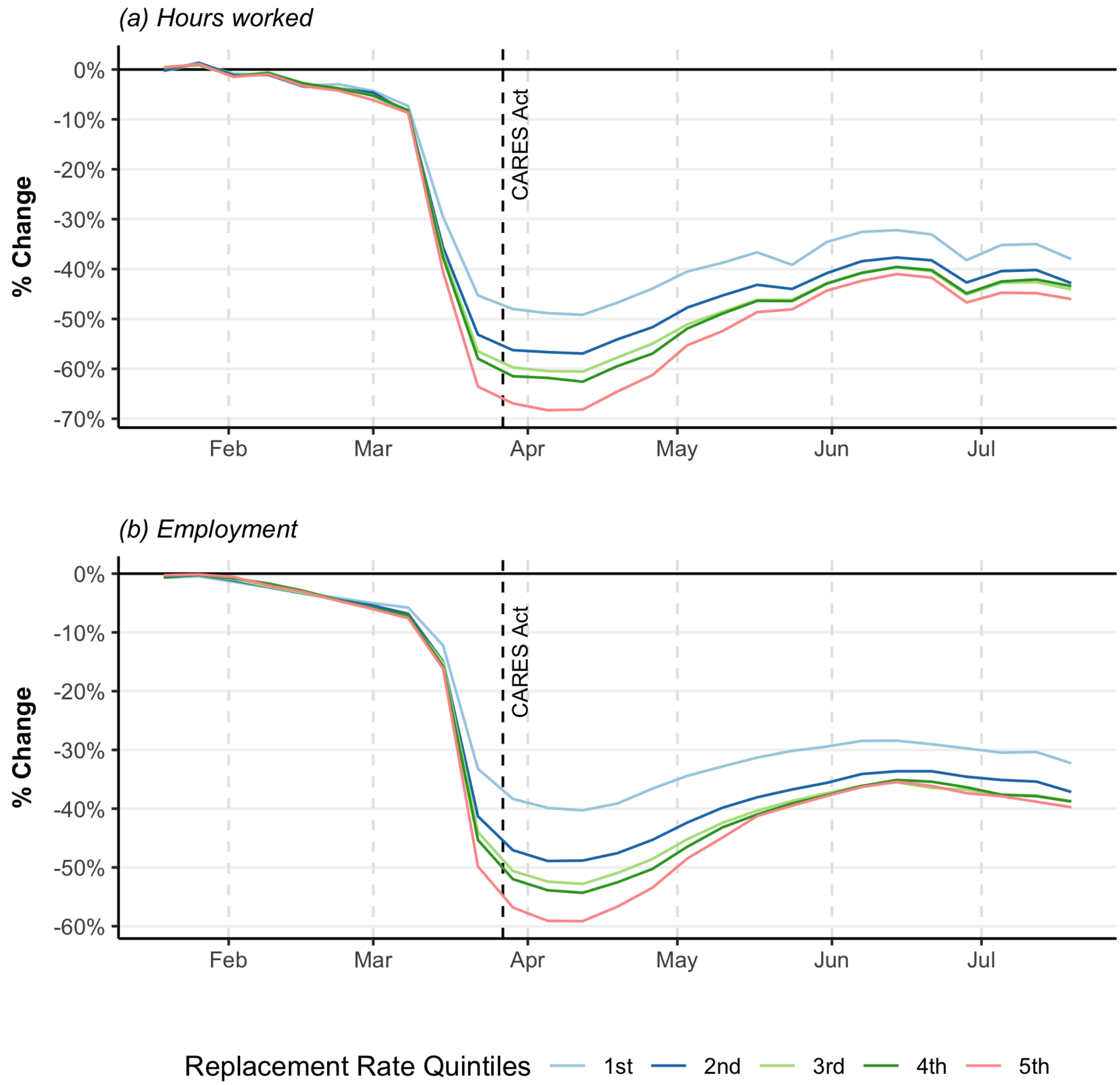
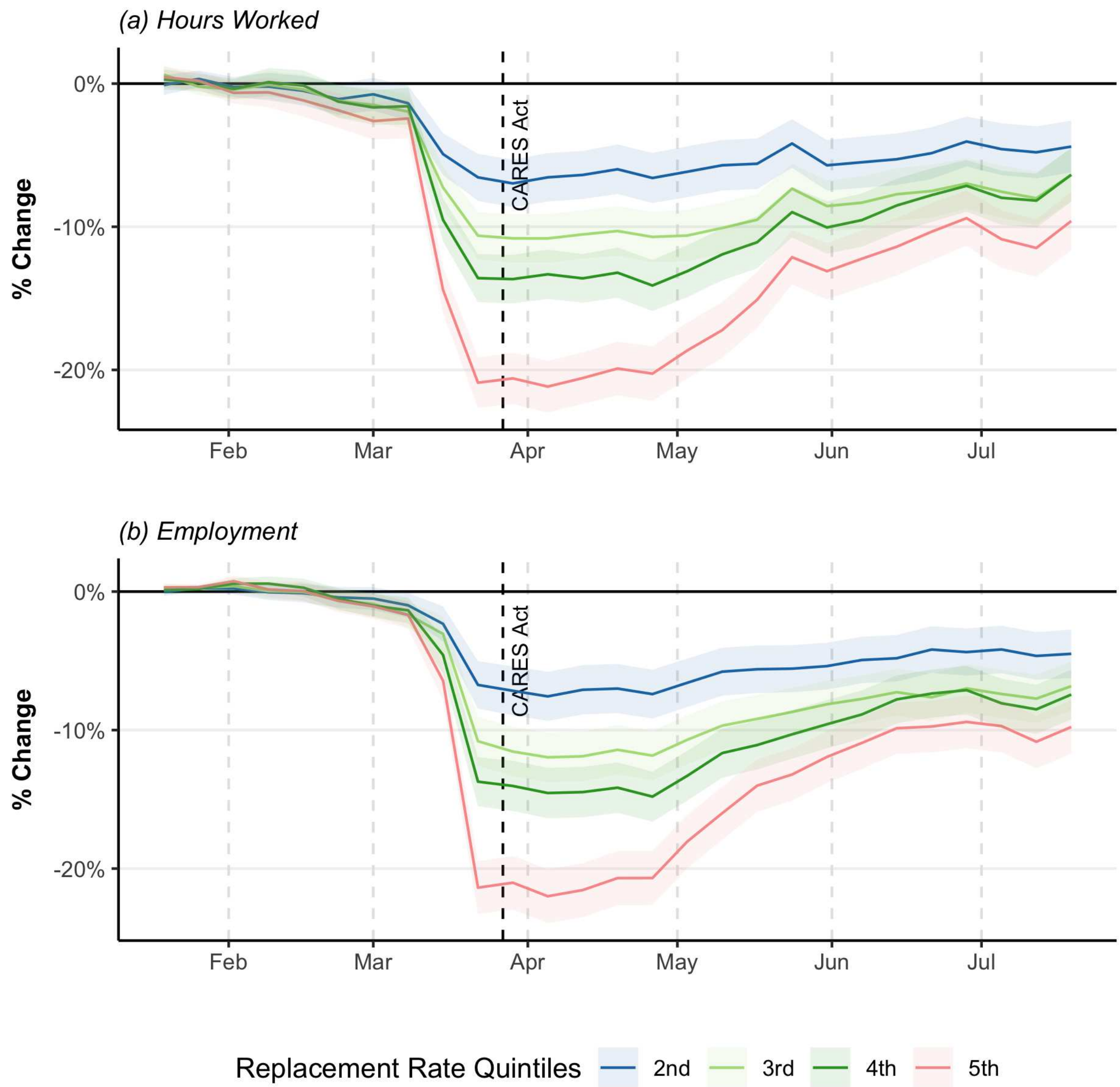
Figure 1: Hours and Employment trends by Replacement Rate Quintiles



Notes: These figures show weekly trends for hours and employment for workers in the Homebase data compared to the baseline period (January 19 to February 8). Hours worked is define as the sum of hours worked in that week. Employment is a dummy variable that equals 1 if the employee had positive hours in that week. Workers are divided into 5 equal size groups (quintiles) where the 1st quintile groups the 20% of workers with the lowest replacement rates and the 5th quintile those with highest replacement rates. The vertical line indicates the day CARES act was passed (March 27).

Figure 2: Event Study of changes in hours and employment, by replacement rate quintile



Notes: These figures show the event study specification in equation 3 showing the estimated β_t^g coefficients for each quintile of post-CARES replacement rate. The omitted category is the first quintile — i.e., those with lowest replacement rates. The regression was estimated in the weekly data and the specification includes state-industry-week fixed effects. The outcomes are weekly hours worked compared to the baseline (January 19 to February 8) and probability of employment, where individuals were coded as being employed (employment = 1) if they worked any positive hours in the week. The vertical line indicates the day CARES act was passed (March 27). Standard errors were estimated using cluster at the worker level. The shaded areas represent 95% confidence intervals.

Table 1: Descriptive Statistics

Variable	N	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
Panel A - Workers								
Weekly Hours in base period	29,005	36.968	8.409	20.000	31.253	36.737	41.360	100.163
Hourly Wage in base period	28,912	13.317	4.734	2.130	10.500	13.000	15.000	95.000
Weekly Earnings in base period	28,922	495.406	221.202	0.303	355.672	467.133	598.717	4,355.363
Weekly Earnings in 2019	29,005	479.495	203.115	50.914	353.512	452.581	572.102	3,989.656
Pre-CARES Replacement Rate	29,005	0.551	0.064	0.093	0.520	0.547	0.584	0.886
Post-CARES Replacement Rate	29,005	1.924	0.495	0.273	1.587	1.860	2.199	4.108
Panel B - Post-Cares Replacement Rate, for each quintile								
Q1	5,801	1.318	0.190	0.273	1.237	1.369	1.460	1.527
Q2	5,801	1.641	0.063	1.527	1.587	1.641	1.695	1.747
Q3	5,801	1.861	0.067	1.747	1.803	1.860	1.918	1.980
Q4	5,801	2.124	0.090	1.980	2.046	2.118	2.199	2.294
Q5	5,801	2.678	0.335	2.294	2.416	2.582	2.856	4.108

Notes: Panel A presents the descriptive statistics for the workers in our Homebase sample, which encompasses individuals who (1) worked for at least 300 hours in each quarter of 2019 (2) worked at least 20 hours in the base period, defined as the three weeks from January 19 to February 1; (3) worked at the same firm throughout 2019 and in the base period, which firm recorded at least 5 weeks of positive hours. "Pre-CARES replacement rate" and "Post-CARES replacement rate" indicate the ratio of UI benefits for which the worker was eligible based on their 2019 earnings to their average weekly earnings in 2019, before and after the passage of the CARES Act, respectively. Workers with pre-CARES replacement rates of zero are excluded from our analysis. Note that the minimum hourly wage in the base period reflects the minimum wage in some states for workers who receive tips. In our analysis we floor these wages at the state non-tipped minimum to reflect the provision in U.S. labor law that if a worker does not earn the state minimum wage in wages + tips, their employer must pay them the difference. Panel B shows the descriptive statistics for the Post-CARES Replacement Rate for each quintile.

Table 2: Regression results on employment and hours of work, pre- and post-CARES

	Dependent variable:					
	Employment			Hours Worked		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A - Replacement Rate Quintiles						
Q2	-0.058^{***}	-0.059^{***}	-0.062^{***}	-0.689^{***}	-0.689^{***}	-0.736^{***}
	(0.007)	(0.007)	(0.006)	(0.046)	(0.044)	(0.037)
Q3	-0.077^{***}	-0.096^{***}	-0.103^{***}	-1.003^{***}	-1.128^{***}	-1.212^{***}
	(0.007)	(0.007)	(0.006)	(0.045)	(0.043)	(0.038)
Q4	-0.086^{***}	-0.127^{***}	-0.141^{***}	-1.192^{***}	-1.465^{***}	-1.589^{***}
	(0.007)	(0.007)	(0.007)	(0.044)	(0.043)	(0.040)
Q5	-0.107^{***}	-0.191^{***}	-0.190^{***}	-1.494^{***}	-2.069^{***}	-2.100^{***}
	(0.007)	(0.007)	(0.007)	(0.043)	(0.044)	(0.042)
Post*Q2	-0.0004	-0.001	0.003	0.068^{**}	0.060^{*}	0.099^{***}
	(0.006)	(0.006)	(0.005)	(0.034)	(0.034)	(0.032)
Post*Q3	-0.008	-0.005	-0.002	0.059^{*}	0.072^{**}	0.139^{***}
	(0.006)	(0.006)	(0.005)	(0.033)	(0.033)	(0.033)
Post*Q4	-0.006	0.006	0.004	0.086^{***}	0.144^{***}	0.193^{***}
	(0.006)	(0.006)	(0.006)	(0.033)	(0.033)	(0.035)
Post*Q5	-0.001	0.024^{***}	0.010	0.128^{***}	0.260^{***}	0.258^{***}
	(0.006)	(0.006)	(0.006)	(0.032)	(0.034)	(0.037)
Panel B - Continous Replacement Rate						
Replacement Rate	-0.066^{***}	-0.120^{***}	-0.124^{***}	-0.954^{***}	-1.346^{***}	-1.363^{***}
	(0.004)	(0.005)	(0.005)	(0.024)	(0.026)	(0.029)
Post*Replacement Rate	-0.006^{***}	0.0004	-0.006^{***}	0.023^{**}	0.055^{***}	0.017^{**}
	(0.002)	(0.002)	(0.001)	(0.010)	(0.010)	(0.008)
Baseline Mean	0.960	0.960	0.960	5.281	5.281	5.281
Industry-Week FE	Yes	-	-	Yes	-	-
State-Industry-Week FE	-	Yes	-	-	Yes	-
Firm-Week FE	-	-	Yes	-	-	Yes
Observations	3,822,800	3,822,800	3,822,800	3,857,665	3,857,665	3,857,665

Notes: Panel A shows the results from equation 4 for each quintile of replacement rate and Panel B shows the coefficients for the same equation using the continuous replacement rates (rather than the quintile groups). Both are estimated using the daily data from our main sample. The first three columns show results on employment and the last three columns on hours of work. Individuals are coded as employed (employment = 1) if they worked positive hours in any of the last three days. Hours of work is the amount of hours worked in a single day. Continuous replacement rates are coded such that a 100% replacement rate corresponds to $R = 1$. The value for the outcome variable in the base period (Jan19-Feb08) is displayed as the baseline mean. All columns include day of the week fixed effect. Standard errors are clustered at the worker level, starts indicate p-values (p): *p<0.1; **p<0.05; ***p<0.01

Table 3: Regression results: Additional Exercises

	Dependent variable:						
	Employment					log Wages	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Replacement Rate	-0.120^{***}	-0.124^{***}	-0.012	-0.089^{***}	-0.125^{***}	-0.496^{***}	-0.526^{***}
	(0.005)	(0.006)	(0.008)	(0.007)	(0.004)	(0.005)	(0.006)
Post*Replacement Rate	0.0004	-0.003^{*}	-0.003^{**}	0.004	0.003	0.001	-0.005
	(0.002)	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.003)
Sample	Baseline	Baseline	Baseline	Growing Firms	Growing Firms	All working	Re-hired
Industry*Week FE	-	Yes	Yes	-	-	-	-
State*Industry*Week FE	Yes	-	-	Yes	Yes	Yes	Yes
Base Wage*Week	-	Yes	-	-	-	-	-
Base Earnings*Week	-	-	Yes	-	-	-	-
State Case/Restrictions	-	Yes	Yes	-	-	-	-
Observations	3,822,800	3,810,545	3,822,800	567,195	2,028,330	1,445,036	97,008
R^2	0.088	0.050	0.053	0.167	0.085	0.611	0.651

Note: *p<0.1; **p<0.05; ***p<0.01

Notes: The table shows the coefficients associated with R and $Post * R$ from equation 4 estimated using the daily data from our main sample. The first column is the baseline specification. The second and third columns add respectively as controls an interaction of base wages and week and base earnings and week. Given the fact that we cannot control for state FE with this approach, we instead include industry-week FE and controls for states legal restrictions on business activities and number of Covid-19 cases at the state level. Columns 4 and 5 reproduce the baseline specification in the sub-samples of firms that are growing leaving out person is hours. Column 4 keeps firms-week observations with more hours than in the baseline and Column 5 in a week-by-week comparison. Columns 6 and 7 estimate the baseline specification using as the outcome the log of hourly wage, for all individuals working (column 6) and for only re-hired individuals (column 7) which we defined as those individuals that get back to work after at least 7 days of zero hours. All columns include day of the week fixed effect. Individuals are coded as employed (employment = 1) if they worked positive hours in any of the last three days. Hours of work is the amount of hours worked in a single day. Replacement rates are coded such that a 100% replacement rate corresponds to $R = 1$. Standard errors are clustered at the worker level, starts indicate p-values (p): *p<0.1; **p<0.05; ***p<0.01

Appendix A Current Population Survey (CPS)

A.1 Sample Selection

We supplement our results from the Homebase data with benchmarks from the Current Population Survey (CPS), a more representative sample of the US labor market. The CPS is administered monthly and asks about labor market activities in the second week of a given month. Participants respond to the CPS for a period of 4 consecutive months, then rotate out for 8 months, then rotate back in for another period of 4 consecutive months before rotating out permanently. For example, a respondent in our sample may be in the data in February, March, April, And May 2019; they would then rotate in for February, March, April, and May 2020 before rotating out permanently.

While the CPS is administered monthly, the reference-week structure allows us to exploit specific questions about employment to impute weekly employment data in weeks between surveys. We observe respondents in the CPS in the weeks of February 9, March 8, April 12, and May 10. We impute employment in the intervening weeks as follows. If a respondent is employed in both the first and second month, we code them as employed in all intervening weeks. If a respondent is unemployed in both the first and second month, we code them as unemployed in all intervening weeks. If a respondent is employed in the first week and unemployed in the second, we use the number of weeks of continuous unemployment reported in the second month's survey to impute the week in which she became unemployed. If a respondent is unemployed in the first week and employed in the second week, we exclude her from the sample in the intervening weeks. That is, she will appear in the weeks in which she was surveyed, but we drop her from the intervening weeks because we cannot observe in which week she became employed.

In our preferred specification we classify a respondent as employed if she was at work in the reference week *but not* if she reports that she has a job but was not at work in the reference week. This means that we do not count as employed those workers workers who were on furlough, who would have been counted as unemployed in the Homebase data as described in section 3.

In the CPS data, we impose sampling restrictions to be able to compute UI benefits. To compute benefits, as described in section 3, we need to observe quarterly earnings history, which is not available in the monthly CPS. We thus restrict our sample to respondents in the 2020 CPS who answered the 2019 CPS Annual Social and Economic Supplement (ASEC). The 2019 ASEC is administered in February, March, and April of 2019 and asks about labor market activities in calendar year 2018. Following Ganong *et al.* (2020), we restrict our analysis to 2019 ASEC respondents who (1) are US citizens, (2) report hourly earnings in the ASEC of at least the federal minimum wage of $7.25, and (3) would have been eligible for UI benefits prior to the passage of the CARES Act in their state of residence on the basis of their 2018 earnings. Additionally, to ensure that we are comparing similar outcomes in the CPS and in Homebase, we further restrict our sample to workers who were employed as of the February 2020 survey.

A.2 Analysis

We replicate the two main results from our analysis of the Homebase data on the CPS sample. First, we estimate the event study specification in equation 3 on workers' employment status over time. Second, we collapse the pre- and post-CARES periods and explore the association between the replacement rates and the outcomes of interest before and after CARES using the specification in equation 4.

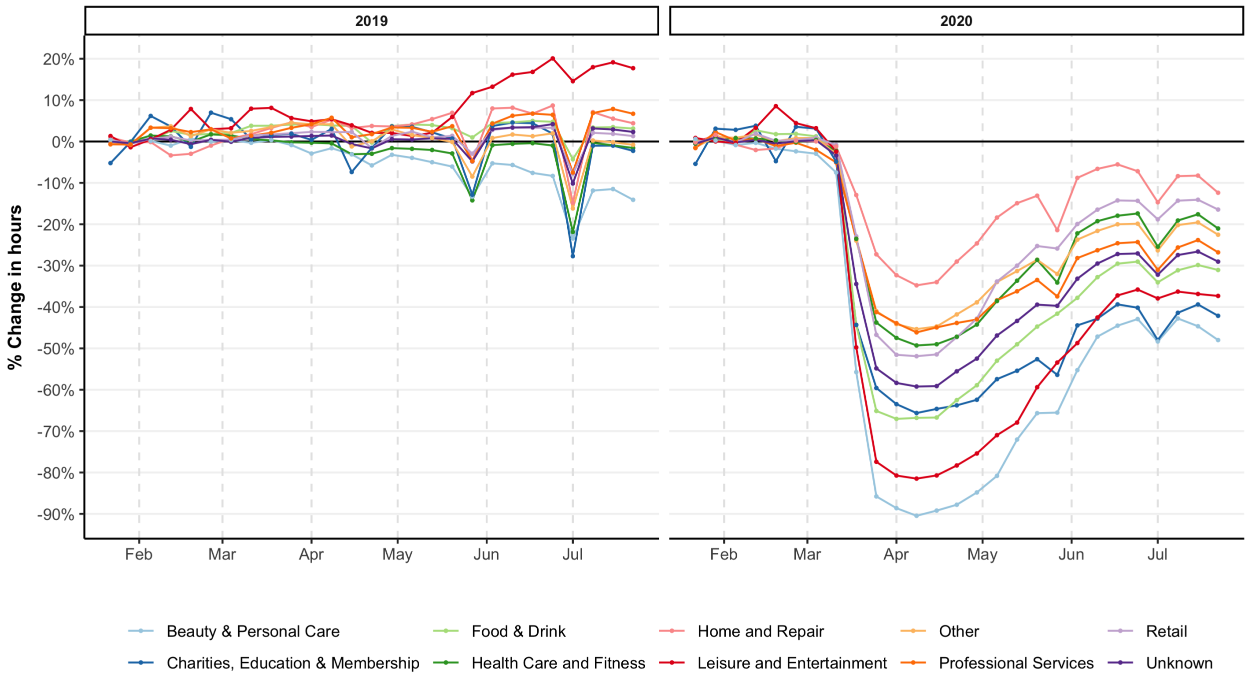
A.3 Results

Appendix Figure 1 shows that these results are robust to the CPS data. While there appears to be a small, but insignificant, drop in employment following the passage of the CARES Act, this is mitigated by controlling for state-industry-week effects, which subsume state-level business restrictions and new Covid-19 cases.

Furthermore, workers facing larger UI expansions generally appear to be *quicker* to return to work than others, not slower. While they do not fully catch up to pre-Covid levels of relative employment conditional on controls, the gap has diminished over time. This provides corroborating evidence of our main result in the Homebase data.

Appendix Table A.6 replicates the results from columns (1) and (2) of Table 2. The results are again qualitatively similar to those in the Homebase data: While there is a large negative effect of replacement rate on employment, it is explained entirely by variation that occurs before the CARES Act is passed. There is a negative coefficient on the effect of replacement rate after CARES, but it is economically small and not statistically significant.

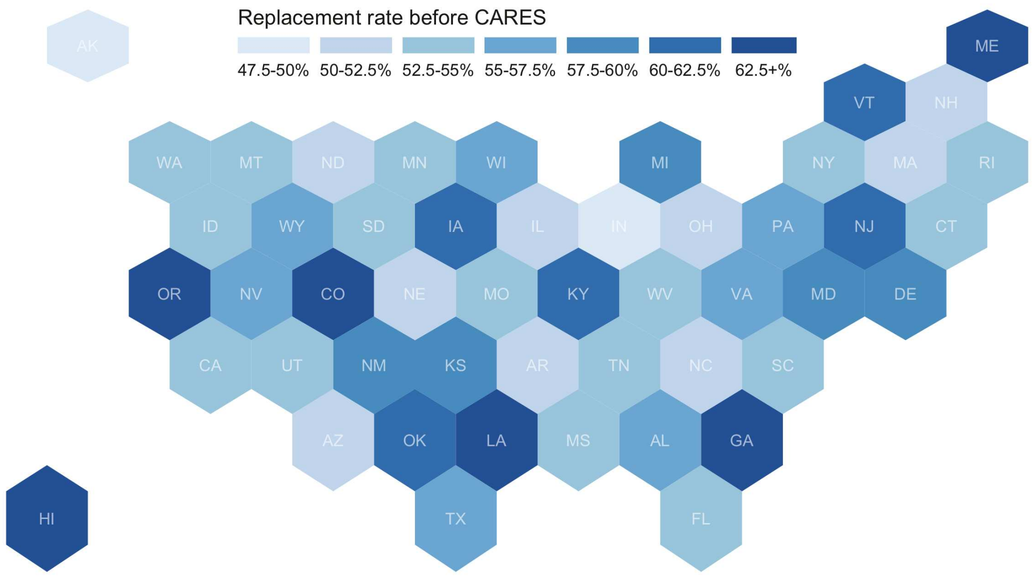
Appendix Figure A.1: Trends in hours of work by industry (2019 and 2020)



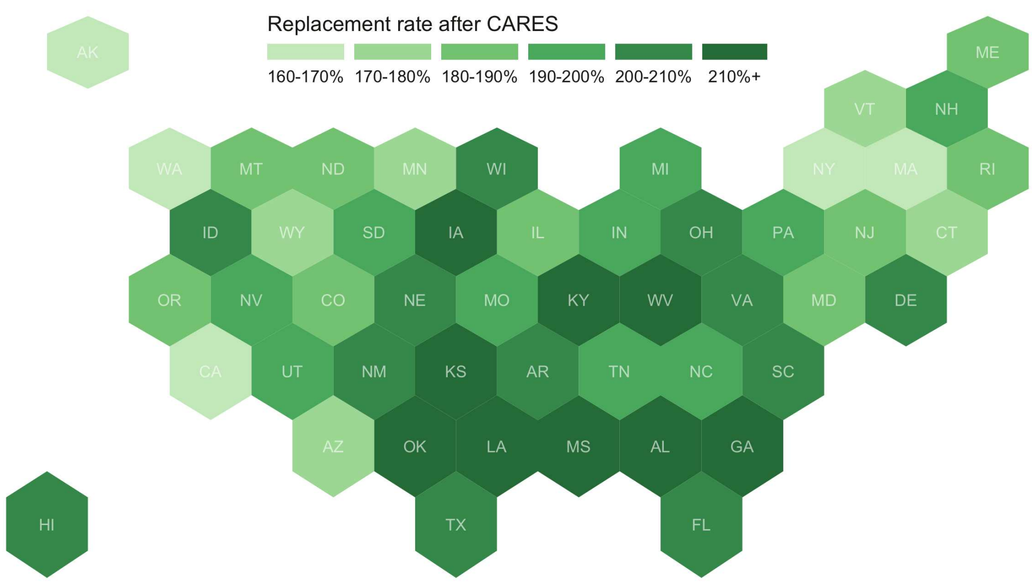
Notes: The figure reproduces figure 2 from Altonji *et al.* (2020). The figure shows the average change in hours of operation for firms in different sectors in 2019 and 2020 in comparison to the baseline period (last two weeks of January and first week of February). All firms that logged positive hours for at least 5 weeks and that worked on average at least 40 hours in the base period are included in the analysis.

Appendix Figure A.2: Median UI replacement rates, Homebase sample

(a) Before CARES

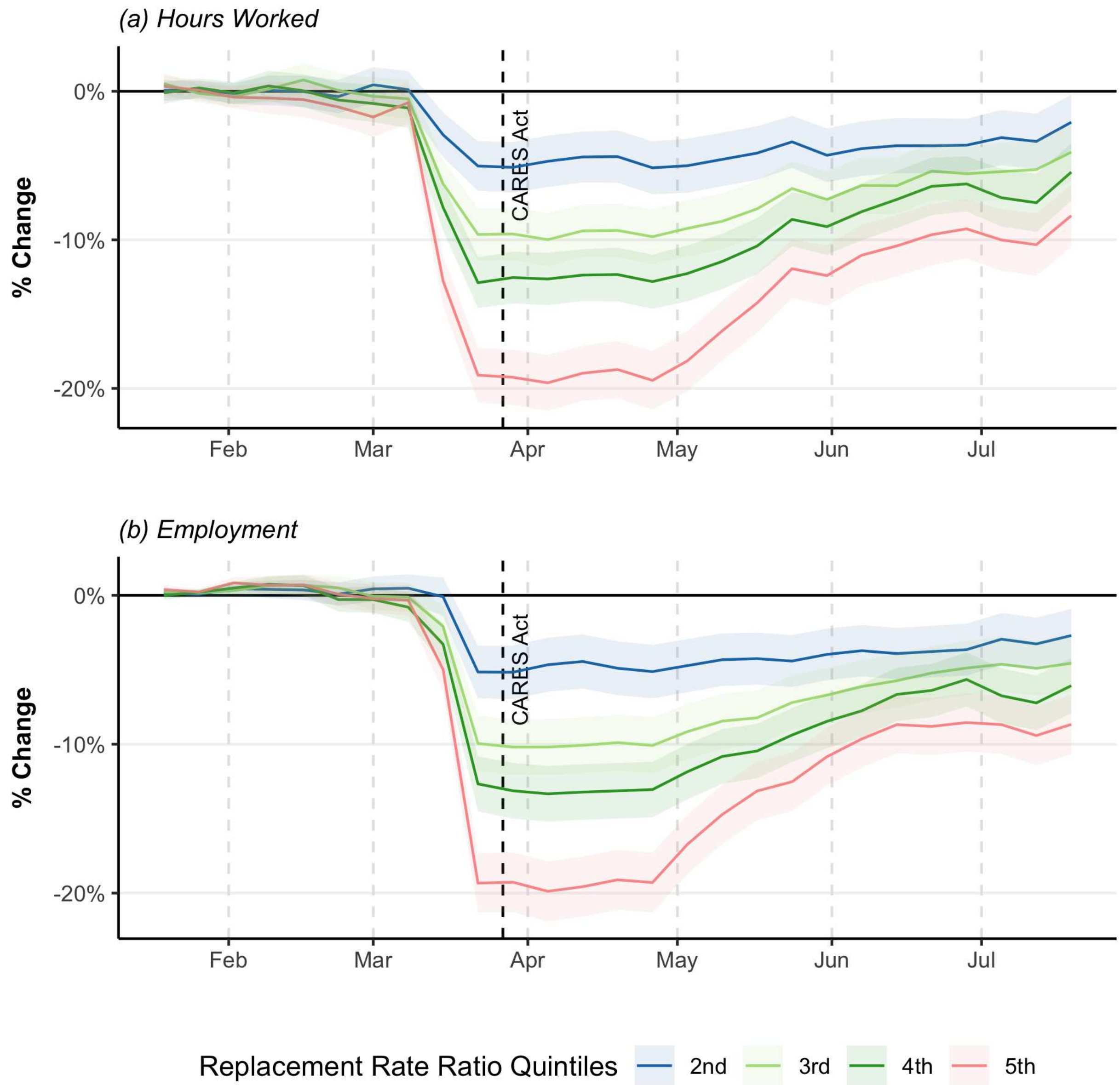


(b) After CARES



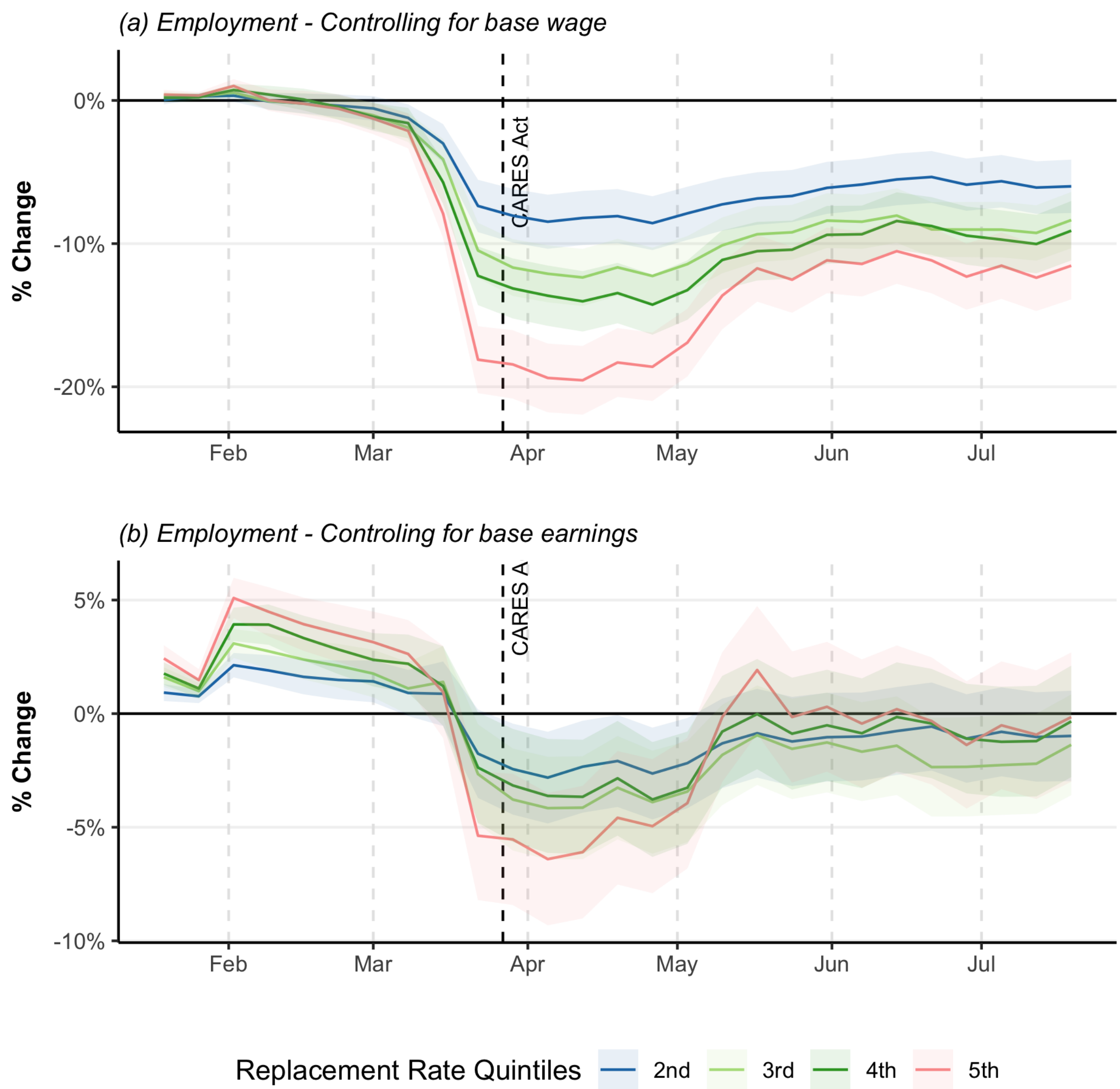
Notes: These figures show the median UI replacement rate by state for workers in the Homebase data, (a) according to state benefits schedules as of January 2020 and (b) as of the passage of the CARES Act. Under the CARES Act, all UI recipients in each state became entitled to an additional $600 federal payment in each week in which they receive UI benefits. To estimate UI benefits for each worker, we floor wages at each state's minimum wage to correct for observations in which employers list an employee's tipped minimum wage (below the state minimum) in Homebase.

Appendix Figure A.3: Event study, by replacement rate ratio quintiles



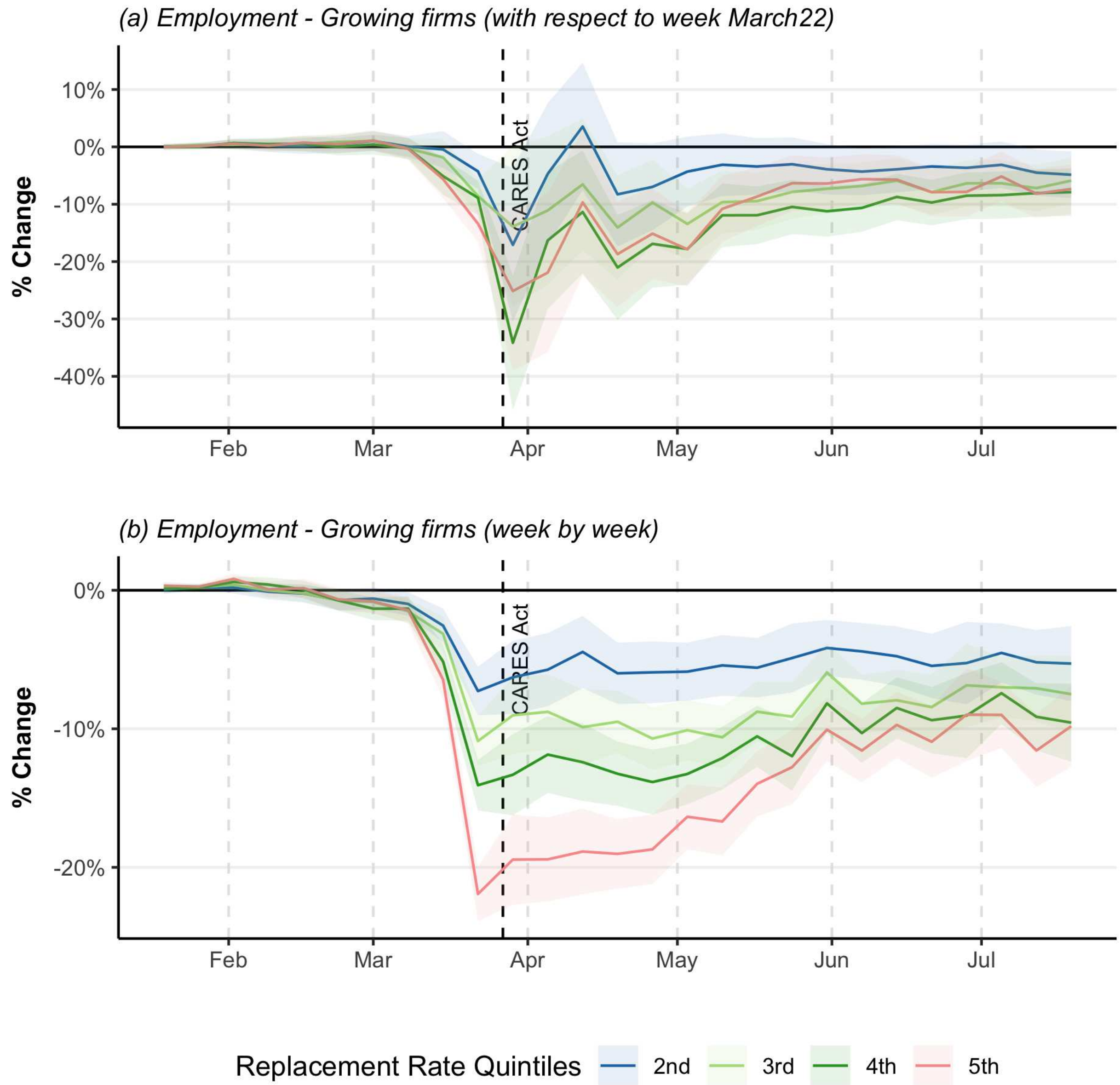
Notes: The figure shows an event study specification from equation 3 showing the estimated β_t^g coefficients for each quintile of the replacement rate ratio (where the omitted category is the first quintile — i.e., those with lowest replacement rates). The regression was estimated in the weekly data and the specification includes state-industry-week fixed effects. The outcomes are weekly hours worked compared to the baseline (Jan19-Feb08) and employment probability, where individuals were coded as being employed (employment = 1) if they worked any positive hours in the week. The vertical line indicates the day CARES act was signed (March 27th). Standard errors were estimated using cluster at the worker level and the shaded areas represent 95% confidence intervals.

Appendix Figure A.4: Event study, controlling for base wages and earnings



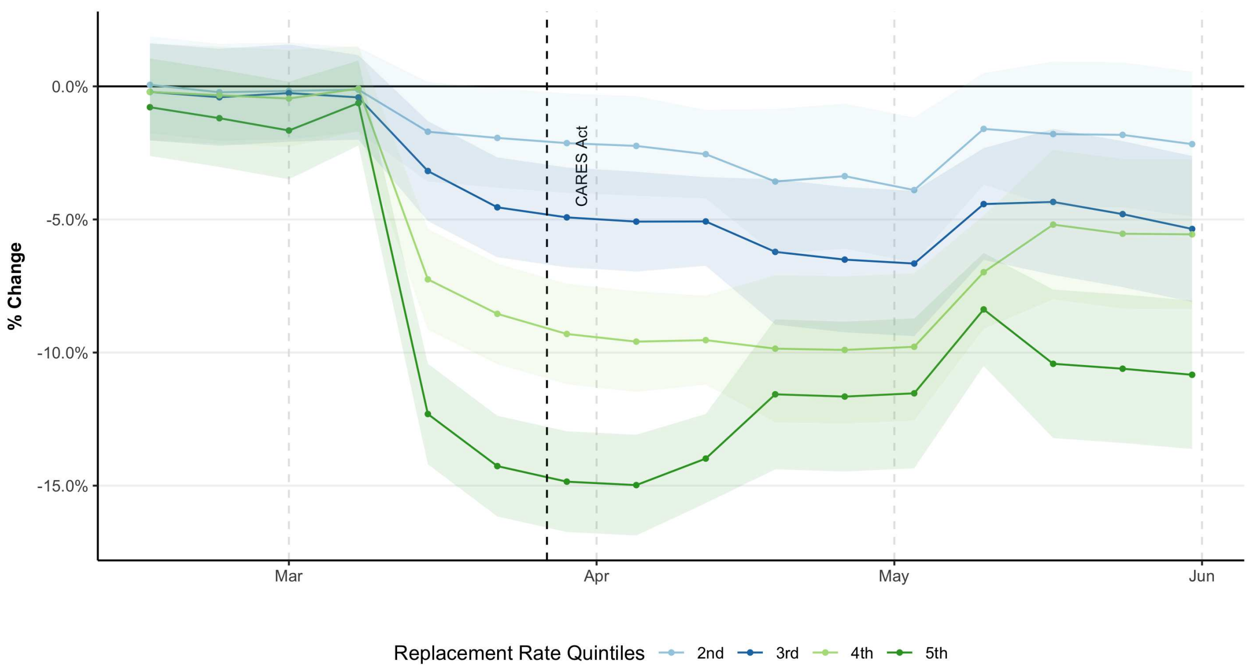
Notes: The figure shows an event study specification from equation 3 showing the estimated β_t^g coefficients for each quintile of the post-CARES replacement rate (where the omitted category is the first quintile — i.e., those with lowest replacement rates). The regression was estimated in the weekly data and the specification includes industry-week fixed effects and controls for the number of Covid-19 cases at the state level and state legal restrictions on business activities. The top figure adds as a control the base (Jan19-Feb08) average hourly wage interacted with weeks and the bottom figure adds the corresponding interactions using base earnings. The outcome is employment probability, where individuals were coded as being employed (employment = 1) if they worked any positive hours in the week. The vertical line indicates the day CARES act was signed (March 27th). Standard errors were estimated using cluster at the worker level and the shaded areas represent 95% confidence intervals.

Appendix Figure A.5: Event study, subset of growing firms



Notes: The figure shows an event study specification from equation 3 showing the estimated β_t^g coefficients for each quintile of the replacement rate ratio (where the omitted category is the first quintile — i.e., those with lowest replacement rates). The regression was estimated in the weekly data and the specification includes state-industry-week fixed effects. The top figure estimates in the sub-sample of firms that were growing (leaving individual i's hours) with respect to the week of March 22th. The bottom figure estimates in the subsample of firms that were growing after the CARES act, in a week by week comparison. The outcome is employment probability, where individuals were coded as being employed (employment = 1) if they worked any positive hours in the week. The vertical line indicates the day CARES act was signed (March 27th). Standard errors were estimated using cluster at the worker level and the shaded areas represent 95% confidence intervals.

Appendix Figure A.6: Event Study using CPS data



Notes: The figure shows an event study specification from equation 3 showing the estimated β_t^g coefficients for each quintile of post-CARES replacement rate (where the omitted category is the first quintile — i.e., those with lowest replacement rates). The regression was estimated in the imputed weekly CPS data and the specification includes state-industry-week fixed effects. The outcome is employment probability, where individuals were coded as being employed if they reported being employed (employment = 1) and at work during the week. The vertical line indicates the day CARES act was signed (March 27). Standard errors were estimated using clusters at the worker level. The shaded areas represent 95% confidence intervals.

Appendix Table A.1: Industry composition

Industry	Proportion of workers	
	Homebase data	Our sample
Food & Drink	0.483	0.554
Other	0.133	0.076
Retail	0.112	0.122
Unknown	0.112	0.143
Health Care and Fitness	0.053	0.039
Charities, Education & Membership	0.035	0.015
Professional Services	0.030	0.016
Leisure and Entertainment	0.022	0.017
Home and Repair	0.016	0.014
Beauty & Personal Care	0.004	0.005

Notes: The first column shows the proportion of workers in the Homebase data for each industry classification, after removing firms that logged positive hours for less than 4 weeks between January2019 and July 2020. The second column presents the same proportion in our sample, which encompasses individuals who (1) worked for at least 300 hours in each quarter of 2019 (2) worked at least 20 hours in the base period, defined as the three weeks from January 19 to February 1. Transportation category was moved to other given the small number of firms.

Appendix Table A.2: Robustness: Alternative dates

	Dependent variable: Employment				
	(1)	(2)	(3)	(4)	(5)
Panel A - Changing the baseline starting date					
Replacement Rate	-0.120^{***}	-0.107^{***}	-0.111^{***}	-0.123^{***}	-0.121^{***}
	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)
Post*Replacement Rate	0.0004	-0.012^{***}	-0.007^{***}	0.002	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Starting Date	Baseline (Mar15)	Mar10	Mar12	Mar17	Mar20
Baseline Mean	0.960	0.960	0.960	0.960	0.960
State-Industry-Week FE	Yes	Yes	Yes	Yes	Yes
Observations	3,822,800	3,967,825	3,909,815	3,764,790	3,677,775
R^2	0.088	0.101	0.095	0.087	0.086
Panel B - Changing the post date (CARES Act)					
Replacement Rate	-0.120^{***}	-0.128^{***}	-0.131^{***}	-0.138^{***}	-0.139^{***}
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Post*Replacement Rate	0.0004	0.009^{***}	0.014^{***}	0.024^{***}	0.027^{***}
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Post Date	Baseline (Mar28)	Apr03	Apr10	Apr17	Apr24
Baseline Mean	0.960	0.960	0.960	0.960	0.960
State-Industry-Week FE	Yes	Yes	Yes	Yes	Yes
Observations	3,822,800	3,822,800	3,822,800	3,822,800	3,822,800
R^2	0.088	0.088	0.088	0.089	0.089

Notes: The table shows the coefficients associated with R and $Post * R$ from equation 4 estimated at the daily data from our main sample. Panel A varies the starting day of the analysis from the baseline March 15th to March 10, 12, 17 and 20th. Panel B varies the starting day for the *Post* variable from the baseline March 28th to April 03, 10, 17 and 24th. The outcome variable is probability of being employed, where individuals are coded as employed (employment = 1) if they worked positive hours in any of the last three days. The value for the outcome variable in the base period (Jan19-Feb08) is displayed as the baseline mean. Replacement rates are coded such as a 100% replacement rate corresponds to $R = 1$. All columns include day of the week fixed effect. Standard errors are clustered at the worker level, starts indicate p-values (p): *p<0.1; **p<0.05; ***p<0.01

Appendix Table A.3: Robustness: Changing the treatment variable

	Dependent variable: Employment			
	(1)	(2)	(3)	(4)
Treatment	-0.120^{***}	-0.056^{***}	-0.071^{***}	-0.112^{***}
	(0.005)	(0.002)	(0.003)	(0.004)
Post*Treatment	0.0004	0.007^{***}	-0.0004	-0.003
	(0.002)	(0.001)	(0.001)	(0.002)
Treatment Variable	Benchmark (R)	Not Floored R	R^{ratio}	R with 2020 Earnings
Baseline Mean	0.960	0.960	0.960	0.960
State-Industry-Week FE	Yes	Yes	Yes	Yes
Observations	3,822,800	3,822,800	3,822,800	3,643,100
R^2	0.088	0.085	0.088	0.093

Notes: The table shows the coefficients associated with R and $Post * R$ from equation 4 estimated at the daily data from our main sample. Each column uses a different definition of the *treatment* variable. The first column is our benchmarking specification using the post-Cares replacement rate, which is defined over the average weekly earnings in 2019. The second column uses the "not-floored" version of the UI benefits, not imposing that individuals are earning the state minimum wage. The third columns uses as treatment variable the replacement rate ratio, which is the ratio of the replacement rate post and pre-CARES. Finally the last column defines the replacement rate over the earnings of the base period (Jan19-Feb08), excluding 4.7% of individuals that did not make at least 200 dollars of weekly earnings over that period. The outcome variable is probability of being employed, where individuals are coded as employed (employment = 1) if they worked positive hours in any of the last three days. The value for the outcome variable in the base period (Jan19-Feb08) is displayed as the baseline mean. Replacement rates are coded such as a 100% replacement rate corresponds to $R = 1$. All columns include day of the week fixed effect and state-industry-week fixed effects. Standard errors are clustered at the worker level, starts indicate p-values (p): *p<0.1; **p<0.05; ***p<0.01

Appendix Table A.4: Alternative Employment Definitions

	Dependent variable: Employment				
	(1)	(2)	(3)	(4)	(5)
Replacement Rate	-0.120^{***}	-0.115^{***}	-0.110^{***}	-0.098^{***}	-0.542^{***}
	(0.005)	(0.003)	(0.005)	(0.004)	(0.040)
Post*Replacement Rate	0.0004	0.005^{***}	0.0001	-0.004^{**}	0.015
	(0.002)	(0.001)	(0.002)	(0.002)	(0.042)
Employment Window	Baseline (3 days)	1 day	5 days	1 week	Baseline
Method	OLS	OLS	OLS	OLS	Logit
Baseline Mean	0.960	0.682	0.990	0.994	0.960
State-Industry-Week FE	Yes	Yes	Yes	Yes	Yes
Observations	3,822,800	3,857,665	3,791,055	3,857,665	3,805,341
R^2	0.088	0.079	0.096	0.102	0.061

Notes: The table shows the coefficients associated with R and $Post * R$ from equation 4 estimated at the daily data from our main sample. Each column uses a different definition of the *outcome* variable. The first column is our benchmarking specification coding individuals as non-employed if they worked zero hours for the last three days. The following columns change this period to be, respectively 1 day, 5 days and the entire week. The value for the outcome variable in the base period (Jan19-Feb08) is displayed as the baseline mean. The first 4 columns were estimated using OLS and the fifth column using a Logit specification. Replacement rates are coded such as a 100% replacement rate corresponds to $R = 1$. All columns include day of the week fixed effect and state-industry-week fixed effects. Standard errors are clustered at the worker level, starts indicate p-values (p): *p<0.1; **p<0.05; ***p<0.01

Appendix Table A.5: Median Replacement Rate by state-industry

	Dependent variable:	
	Employment	Hours Worked
	(1)	(2)
Median Replacement Rate	-0.103^{***}	-0.996^{***}
	(0.004)	(0.023)
Post*Median Replacement Rate	-0.005^{***}	0.031^{***}
	(0.001)	(0.003)
Baseline Mean	0.940	4.834
Week FE	Yes	Yes
Observations	26,981,413	27,267,793
R^2	0.019	0.028

Notes: In this table we use all workers that: 1) worked at least 20 hours weekly on average in the base period (Jan19-Feb08) and 2) worked in firms that reported positive hours for at least 5 weeks between 2019 and 2020. We attribute to each individual the median replacement rate calculate for their state-industry, where those replacement rates came from our main sample. Week only kept state-industries that had valid replacement rates for at least 35 workers. The table shows the coefficients associated with median R and $Post * R$ by state-industry. Median replacement rates are measured such as a 100% median replacement rate corresponds to $R = 1$. The first column shows results on employment and the second on hours of work. Individuals are coded as employed (employment = 1) if they worked positive hours in any of the last three days. Hours of work is the amount of hours worked in a single day. The value for the outcome variable in the base period (Jan19-Feb08) is displayed as the baseline mean. All columns include day of the week fixed effect and week fixed effects. Standard errors are clustered at the worker level, starts indicate p-values (p): *p<0.1; **p<0.05; ***p<0.01

Appendix Table A.6: Robustness of main results to CPS data

	Dependent variable:	
	Employment	
	(1)	(2)
Replacement Rate	-0.097^{***}	-0.087^{***}
	(0.020)	(0.021)
Post*Replacement Rate	−0.015	−0.017
	(0.011)	(0.011)
Baseline Mean	0.896	0.896
Industry-Week FE	Yes	–
State-Industry-Week FE	–	Yes
Observations	97,826	97,826
R^2	0.409	0.588

Notes: The table shows the coefficients associated with R and $Post * R$ from equation 4 estimated on imputed weekly data from the CPS. Column (1) shows coefficients from a specification controlling for industry-week fixed effects. Column (2) shows coefficients from a specification controlling for state-industry-week fixed effects. The outcome variable is probability of being employed, where individuals are coded as employed (employment = 1) if they report being employed and at work during the week in question. The value for the outcome variable in the base period (January 19-February 8) is displayed as the baseline mean. Replacement rates are coded such as a 100% replacement rate corresponds to $R = 1$. All columns include day of the week fixed effects. Standard errors are clustered at the worker level, starts indicate p-values (p): *p<0.1; **p<0.05; ***p<0.01