Employment Effects of Unemployment Insurance Generosity During the Pandemic

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July 14, 2020

Abstract

The CARES Act expanded unemployment insurance (UI) benefits by providing a $600 weekly payment in addition to state unemployment benefits. Most workers thus became eligible to receive unemployment benefits that exceed their weekly wages. It has been hypothesized that such high benefits encourage employers to lay off workers and discourage workers from returning to work. In this note, we test whether changes in UI benefit generosity are associated with decreased employment, both at the onset of the benefits expansion and as businesses look to reopen. We use weekly data from Homebase, a private firm that provides scheduling and time clock software to small businesses, which allows us to exploit high-frequency changes in state and federal policies to understand how firms and workers respond to policy changes in real time. Additionally, we benchmark our results from the Homebase data to employment outcomes in the Current Population Survey (CPS). We find that that the workers who experienced larger increases in UI generosity did not experience larger declines in employment when the benefits expansion went into effect. Additionally, we find that workers facing larger expansions in UI benefits have returned to their previous jobs over time at similar rates as others. We find no evidence that more generous benefits disincentivized work either at the onset of the expansion or as firms looked to return to business over time. In future research, it will be important to assess whether the same results hold when states move to reopen, and to analyze the effects of high UI replacement rates on reallocation of labor both within and across firms.

*Dana Scott is the primary author of this report. We are very grateful to Homebase for making the data available for this research and thank Ray Sandza and Andrew Vogeley at Homebase for assisting us in understanding the data. We are also grateful to the Cowles Foundation and the Tobin Center for Economic Policy at Yale University for funding. Mistakes and opinions are our responsibility.

1 Introduction

The Coronavirus Aid, Relief, and Economic Stimulus (CARES) Act instituted a variety of economic policy responses to the Covid-19 pandemic. One such policy was a large, temporary expansion of unemployment insurance (UI) benefits known as the Federal Pandemic Unemployment Compensation (FPUC). 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.

When the CARES Act was passed, the UI expansion was set to continue until July 31, 2020. However, as the Coronavirus pandemic has continued into the summer, many states are experiencing surges in virus transmission as they move to reopen. Given the current public health context, policymakers are debating how best to provide social insurance against economic dislocations in the pandemic. The current policy debate on expanded unemployment benefits is focused on both whether such expanded benefits should continue past their original expiration date and whether the fixed \$600 payment is the appropriate form for such benefits to take.

Many academic, journalistic, and anecdotal sources have documented that 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. This results from the fact that the earnings distribution in the U.S. is right-skewed, so the median worker earns less than mean earnings. 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 these facts about the distribution of replacement rates, it is natural to ask whether such high replacement rates (a) encourage employers to lay off workers and (b) discourage workers from returning to work while they are still able to receive UI benefits.

In this note we build on previous work on UI replacement rates under the CARES Act to test whether changes in UI benefit generosity are associated with decreased employment, both at the onset of the benefits expansion and as businesses look to return to work over time. 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 changes in state and federal policies to understand how firms and workers respond to policy changes in real time. Many groups are studying the labor market effects of the Covid-19 pandemic, several of which conduct exercises using the data from Homebase. In particular, Bartik *et al.* (2020) analyze relationships between UI expansion in labor market outcomes using Homebase data. We contribute to this developing literature in two ways. First, we propose a new measure to more effectively capture workers' exposure to increased UI generosity. Namely, we measure the ratio of a worker's post-CARES UI replacement rate to their pre-CARES replacement rate. This has the effect of measuring the extent to which the FPUC increased a worker's unemployment benefits, rather than just measuring the generosity of benefits once the CARES Act became law. Second, we leverage this measure of treatment intensity in an event study design to test whether the expansion of UI benefits decreased employment. To the best of our knowledge, this note is the first to analyze the effect of differential changes to UI benefit generosity on employment.

We find that that the workers who differed in exposure to changes in UI generosity did not experience different declines in employment from the week of March 22 – immediately prior to the passage of the CARES Act – to any of the subsequent weeks. In fact, if anything, groups facing larger increases in benefit generosity experience slight *gains* in employment relative to the least-treated group by early May. We show results from an event study that controls flexibly for the severity of the Covid-19 pandemic as well as heterogeneous state-level business restrictions. We also perform a series of simplified comparisons between the week of March 22 to each of the subsequent six weeks. Our results are robust to benchmarks using data from the Current Population Survey (CPS). These results provide suggestive evidence that, in the aggregate, the expansion in UI benefit generosity did not disincentivize work at the outset, and that high replacement rates did not differentially deter workers from returning to work.

2 Institutional background

2.1 UI benefits eligibility

While the CARES Act greatly expanded eligibility for unemployment insurance, several institutional features that restrict eligibility remained intact. In this section we discuss those features that are directly relevant to the question of the effect of UI generosity on labor supply.

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 heightens one's own risk or caring for an elderly relative – are exempted from this, those who quit for no reason other than general concern about contracting Covid-19 are not eligible for UI. However, it is plausible that at small firms like the ones represented in Homebase, employers and workers could coöperate to lay off workers who would receive higher incomes from UI benefits than from earned wages.

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 specifically 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, it is likely that compliance with this rule is determined by the level of formality of hiring and reporting structures at a given firm; compliance may be lower at small firms where employers interact with workers more informally. Additionally, this feature may lead to overestimates of re-employment since workers with an offer to return would face a stronger incentive to begin working again than those who would have to search for a job to become re-employed.

Together, these features of UI benefits eligibility suggest that if UI expansion decreases labor supply through the mechanism described by critics, it is likely to do so to a greater extent at small firms than at large ones. It is particularly instructive, then, to test for such an effect in a sample of small firms such as those represented in Homebase, because any observed effect could plausibly serve as an upper bound for labor supply's sensitivity to UI generosity.[1]

2.2 Timing of the CARES Act

On Thursday, March 19, 2020, 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 as the bill stalled throughout the week, particularly over the issue of whether benefits would be extended for three months or four. The bill ultimately passed the Senate on Wednesday, March 25. It passed the House of Representatives on Thursday, March 26 and was signed into law on Friday, March 27.

The timing of events in the passage of the stimulus bill is important for the establishment of 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 employers or workers anticipated enhanced unemployment benefits in their extensive-margin labor market decisions that week. That is, 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.

Furthermore, as Ganong *et al.* (2020) note, the $600 size of the supplemental payment was designed to replace 100 percent of the mean U.S. wage when combined with mean state UI benefits. Journalistic discourse about the supplemental insurance reflects this intention, rather than the practical effect of replacing more than 100 percent of median wages. For example, Cochrane and Fandos (23 March 2020) wrote on March 24, ”The two sides had previously agreed to expand the program considerably, to include self-employed and part-time workers who traditionally have not been eligible, and to cover 100 percent of wages to the average worker.” While there has been

[1] The effects of UI generosity on lost output, as opposed to employment, depend on the skill levels of those affected as well as labor supply responses.

significant academic, journalistic, and anecdotal coverage of replacement rates above 100 percent since the CARES Act passed, the timing of events and language used in the week leading up to its passage indicate that anticipation effects of replacement rates over 100 percent, at least on the extensive margin, are unlikely in the week beginning March 21.

3 Data and Sampling

3.1 Homebase

Homebase is 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 has made these data available to several teams of researchers. Further discussions of the Homebase data can be found in Altonji *et al.* (2020), Bartik *et al.* (2020), Chetty *et al.* (2020), and Kurmann *et al.* (2020).

As other researchers have noted, the firms in the Homebase dataset are not representative of the entire US labor market. Homebase's clients are primarily small firms that require time clocks for their day-to-day operations, over half of which are in the food and drink industry. Additionally, workers in our sample of the data are hourly workers, not salaried employees.[2] Because of these limitations, insights about the Homebase sample should not be viewed as representative of the entire labor market. Indeed, replication of this analysis on different and more representative samples is an important area for future work. However, as Bartik *et al.* (2020) 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 particularly valuable because it covers workers with relatively low wages – most are in the first and second quintiles of national earnings as reported in the CPS. These workers experience relatively large changes in benefits generosity from the addition of the $600 supplemental payment compared to higher-earning workers. Therefore, we would expect our results from this sample to overestimate the effects of more generous benefits on labor market responses. That is, if there is no evidence of moral hazard in this group, there is unlikely to be any in a more representative sample. On the other hand, the drop in labor demand stemming from the decline in consumer demand and operating restrictions were especially pronounced in the food and drink industry. This would have made moral hazard less relevant.

In addition to the sample selection caveats, the Homebase data is subject to some additional limitations. Notably, workers who have been furloughed (i.e. are still employed by a firm but

[2] While some firms list their salaried employees in the data (primarily managers), their wages are coded at zero and they do not clock in and out. Since Homebase's software is not designed to consistently track these workers' earnings, we exclude them from our analysis.

are not working any hours) are not distinguishable from workers who have been formally laid off. In the context of labor market responses to changes in unemployment insurance generosity, this distinction is important. For instance, consider a firm that furloughs its employees at $t = 1$ when a state mandates that the firm shut down, but then moves to lay off some or all of its workers at $t = 2$ when the unemployment insurance available to its employees increases substantially. We would not be able to observe the layoff at $t = 2$ and would code the workers' exit from the firm as a layoff at $t = 1$. While this data limitation will lead us to underestimate the effect of changes in unemployment benefit generosity on employment, this effect is mitigated by the specific policy implications in which we are interested. Namely, we aim to understand the implications of generous UI benefits for workers' choices between paid work and unemployment in which they receive UI. As states begin to reopen and policymakers consider whether increased UI generosity disincentivizes workers from returning to productive work, the choice between UI benefits and furlough is not of particular importance.

3.2 Current Population Survey (CPS)

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 or if she reports that she has a job but was not at work in the reference week. This means that we may count as employed some workers who were on furlough, who would have been counted as unemployed in the Homebase data as described in section 3.1. Our definition of employment in the CPS is consistent with eligibility requirements for UI (i.e. a person cannot be employed, but furloughed, and receive UI). However, this means that employment levels in the CPS data will tend to be higher than those in the Homebase data. We expect that, if furlough rates differ across replacement rate ratio categories, they are if anything likely to be *lower* among those facing more generous UI. To verify this, we show additional results in Appendix Figure 1 and Appendix Table 1 in which we define as unemployed any worker who reports that they had a job but were not at work in the reference week. Our results in the CPS are robust to this alternative definition.

3.3 UI benefits calculator

We compute pre- and post-CARES UI benefit replacement rates using the calculator developed by Ganong *et al.* (2020). To compute an individual's eligibility for unemployment benefits, all states make use of a worker's four-quarter earning history. Most states compute benefits as a percentage of the worker's highest quarter earnings, second-highest quarter earnings, or annual earnings in the four most recent completed quarters prior to filing, subject to a minimum and maximum benefit level. In our case, to compute UI benefits, we use workers' earnings histories in the four completed quarters of 2019.

To improve precision in our simulations of individuals' UI benefits, we restrict our Homebase sample to only those workers who worked at a given firm for at least 10 weeks in each quarter at an average of at least 30 hours in each week worked – that is, only those workers who worked full-time at Homebase firms for all of 2019. 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 the period over which UI benefits are calculated, either in a full-time capacity prior to their employment at the Homebase member firm or in a part-time capacity concurrently with their employment at the Homebase member firm. In addition, we further restrict our sample to workers who: (1) worked at least 16 hours in the base period, defined as the two weeks from January 19 to February 1; (2) worked at a firm in the base period that recorded at least 40 worker-hours during that period; and worked at the same firm both throughout 2019 and during the base period.

We note that by imposing this restriction we necessarily exclude the shortest-tenured workers at a given firm, as well as workers at newly established firms. It is plausible that our sampled workers are less likely to be laid off in an economic downturn than shorter-tenured workers. Furthermore, firms established in the last year may be more likely to shut down than longer-running ones. This selection problem may lead us to underestimate the effect of UI expansion on employment. However, while excluded workers and firms may be more sensitive to the economic shock imposed by the pandemic, conditional on wages there is no *a priori* reason to believe that they will be more likely to lay off workers in response to the change in unemployment benefits in particular.

In the CPS data, we impose sampling restrictions to be able to compute UI benefits. To compute benefits, as described in section 3.3, 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.

3.4 State policies and Covid-19 incidence

To track start and end dates of various state-level restrictions in response to the pandemic, we use the COVID-19 US state policy database (CUSP) maintained by Raifman *et al.* (2020). We use three types of restrictions. First, we use stay-at-home orders, which restrict people's movement outside the home to visits to essential businesses and public services. Second, we use required closure of non-essential businesses as a proxy for restrictions on business activity. Third, we use restrictions of restaurants to takeout-only, which is of particular value in our data set given the substantial overrepresentation of bars and restaurants in our sample. Fourth, we use mandatory closures of gyms, since these saw particularly stringent shutdown requirements and there are many Homebase firms in the health and fitness industry. Additionally, we use data on new Covid-19 cases *per capita* from the Johns Hopkins University Center for Systems Science and Engineering (CSSE) to measure the severity of the pandemic in each state.

4 Empirical approach

4.1 Measurement of UI benefits generosity

The UI replacement rate is determined by two inputs: first, the individual's earnings history from the four prior completed quarters; and second, their state's schedule of benefits. When studying the effects and policy implications of changes to the UI replacement rate, it is important to note that the variation in treatment comes not from the replacement rate *per se*, but from the *change* in benefits generosity that results from the CARES Act. For individual i in state s the replacement rate under CARES is given by:

$$repl_{CARES,is} = \frac{UI_{CARES,is}}{w_{2019,is}} = \frac{UI_{2019,is} + 600}{w_{2019,is}} = repl_{2019,is} + \frac{600}{w_{2019,is}} \tag{1}$$

where $UI_{2019,is}$ is the benefit amount for which they would have been eligible in January 2020 and $w_{2019,is}$ is the average weekly wage in 2019, the reference period for calculating UI benefits. The variation we aim to exploit comes from the differential change in replacement rates from the incremental \$600. To measure increases in UI generosity, we compute the ratio of an individual's replacement rate under CARES to their replacement rate prior to CARES. We refer to this measure as the *replacement rate ratio* for worker i in state s:

$$r_{is} = \frac{repl_{CARES,is}}{repl_{2019,is}} = \frac{UI_{CARES,is}}{UI_{2019,is}} = 1 + \frac{600}{UI_{2019,is}} \tag{2}$$

Using this measure instead of the raw replacement rate has the effect of directly measuring the change in UI generosity rather than either the *ex ante* or *ex post* generosity. See Table 4 for an illustrative example of how replacement rates are determined by both state-level benefit generosity and workers' wages. We note that using this measure does not in itself resolve the central endogeneity problem inherent in studying replacement rates. Since the replacement rate ratio is still correlated with workers' wages, we control for wages in our main specification. However, future work should explore alternative identification strategies to address wage endogeneity.

4.2 Event study

To analyze the effects of the passage of the CARES Act on employment for workers facing different changes to UI benefits generosity, we show results from a linear probability event study model. We estimate the probability of employment for individual i in replacement ratio group g, industry j, state k, at time t

$$\begin{aligned} y_{igjkt} = & \sum_{s \neq 3} \alpha_s \mathbb{1}\{s = t\} + \sum_b \beta_b \mathbb{1}\{b = g\} + \\ & \sum_{s \neq 3} \sum_b \gamma_{b,s} \mathbb{1}\{b = g\} \mathbb{1}\{s = t\} + \sum_{s \neq 3} \delta_{sigjk} \mathbb{1}\{s = t\} X_{igjk} + \varepsilon_{igjkt} \end{aligned} \tag{3}$$

where $\mathbb{1}\{s = t\}$ is a full set of week dummies, $\mathbb{1}\{b = g\}$ are dummies indicating membership in replacement ratio group g, and X_{igjk} is a vector of controls containing pre-CARES replacement rate, baseline wage, and industry in the baseline specification. In an additional specification we add time-varying controls for (a) new Covid-19 cases *per capita* reported in a given state-week and (b) indicators for whether states had active policies of each of the following types in a given week: stay-at-home orders, mandatory closures of non-essential businesses, mandatory closures of restaurants except for takeout, and mandatory closures of gyms. In all specifications we allow the effect of the controls to vary over time by interacting them with the full set of week dummies.[3]

5 Results

Figure 2 plots the γ_{gt} coefficients on the interaction between the week indicators t and the replacement rate ratio bins g in equation 3. Conceptually, the coefficients indicate the change in probability of employment relative to the reference group in a given week. The reference group is defined as workers who have a replacement rate ratio less than 2.5, i.e. workers whose replacement rate increases by less than 150 percent. The base period is defined as the two weeks from January 19 to February 1. The outcome variable of interest is an indicator for employment. We code a worker as employed when they record nonzero hours in a given week. We control for industry, baseline replacement rate, baseline wage in both panels. In panel (b) we add controls for the number of new cases of Covid-19, and whether the state had instituted each of the following business restrictions: (1) stay-at-home order, (2) mandatory closure of non-essential businesses, (3) mandatory closure of restaurants except for takeout service, and (4) mandatory closure of gyms. The shaded areas around each line represent 90% confidence intervals.

The figure shows that the workers who differed in exposure to changes in UI generosity did not experience different declines in employment from the week of March 22 – immediately prior to the passage of the CARES Act – to any of the subsequent weeks. While the workers with the largest changes in UI generosity 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. As discussed in section 2.2 it is unlikely that firms and workers could have acted in anticipation of expanded UI replacement rates, so the null result comparing the week of March 22 to subsequent weeks is the relevant one to assess. 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.

If there were an aggregate negative effect of incremental UI generosity on employment at Homebase firms, one would expect to observe two patterns: (1) a significant drop in relative em-

[3] We obtain qualitatively similar results using a logit model of the probability of employment. We also obtain qualitatively similar results when we measure the change in UI generosity using the difference $repl_{CARES,is} - repl_{2019,is}$ rather than the replacement rate ratio r_{is}.

ployment from the week of March 22 (immediately prior to the passage of CARES) to the first full week in which the Act was law (March 29) and (2) decreases in relative employment over time as workers with more-generous UI expansions were slower to return to work over time. Even if, as has been documented, many states experienced implementation delays for several weeks afterward, we could expect at least some drop in the first full week and a significant drop relative to the baseline once all states had implemented the expanded UI benefits, when controlling for variation in states' business operation restrictions over time. However, figure 2 shows no drop at all after the passage of CARES – if anything there appears to be a small, though statistically insignificant, increase in employment in the early weeks. Furthermore, workers facing larger UI expansions 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 serves as suggestive evidence against concerns that UI generosity disincentivizes returns to work.

Figure 3 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-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 (though the group with replacement rate ratios of 3.0-3.5 is an exception). 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.

We supplement the graphical evidence with a series of simplified DiD linear probability models to estimate the effect of increased UI generosity on employment by comparing employment in the week of March 22, immediately prior to the passage of the CARES Act, to each of the six subsequent weeks. We compare to each of the following six weeks to account for the fact that many states experienced implementation delays in expanding their UI systems. We use the same controls as in the main specification. Standard errors are clustered at the worker level.

Table 5 shows that in each two-way comparison, an increase in replacement rate ratio is associated with an increase in employment relative to the week immediately prior to the passage of the CARES Act. In the first three weeks, the replacement rate ratio is associated with a small and insignificant decrease in employment relative to the week of March 22; however, the coefficients are positive and insignificant in the fourth and fifth weeks, and positive and significant in the week of May 3. Column (1) reports effects of the replacement rate ratio on employment is 0.003 lower in the week of March 29 than it was in the week of March 22. In the week of May 3, the effect of the replacement rate ratio on employment is 0.019 higher than it was in the week of March 22. In early weeks the relative differences in employment are economically small and statistically insignificant, but in the week of May 3 higher replacement rate ratios predict a higher rate of employment. Table

6 shows that there is not a significant difference by replacement rate ratio in employment relative to the week immediately prior to the passage of the CARES Act for workers in the CPS sample. All reported coefficients are economically small and statistically insignificant.

Together, these results provide suggestive evidence that, in the aggregate, expansions in UI benefit generosity did not disincentivize work at Homebase firms or for workers in the CPS, 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.* (2020) and diminishes concerns that replacement rates significantly above 100 percent could cause a decline in labor supply or discourage workers from returning to work.

6 Conclusion

As policymakers consider whether to extend the expansion of UI generosity past its initial July 31 expiration date, it is important to holistically consider the economic and public health impacts of such a policy. This note provides preliminary 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. As many states struggle with surges in Covid-19 cases as they move to reopen, there are still good reasons to not incentivize everyone to return to work and to continue to support displaced workers regardless of the labor market effects of such social insurance. However, we find no evidence to support concerns about adverse aggregate labor supply effects of expanded UI generosity in the context of the current pandemic.

We qualify our work with several caveats. First, it is impossible to directly estimate the extent to which firms and workers chose not to work as a result of UI expansion, since the effect is offset by the economic stimulus of income expansion that indirectly boosts employment. However, in the aggregate, there is no evidence that the present UI expansion has decreased employment. Finally, our specification does not account for possible confounding effects. While we do control for Covid-19 cases as a proxy for the severity of the pandemic in each state, as well as for state business restrictions, there could be additional sources of unobserved state-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.

We emphasize that our results do not speak to the disemployment effects of UI generosity during more normal times, which is the subject of a vast literature (Schmieder and von Wachter (2016)). The severity of the decline in labor demand and the health risks to workers make the current pandemic different. Rather, our results offer a first step toward understanding the causal dynamics of UI incentives in the context of the current pandemic. We propose to expand our work here in a few ways. First, future work should test for similar effects using event studies around states' reopenings to assess whether workers with larger expansions in UI generosity are less likely to return to work when business restrictions are loosened. Second, we propose to extend our analysis to study firm-level employment to shed light on firms' ability to hire workers to their desired capacity, whether those workers had previously been employed at the firm or not.

Additionally, future work on expanded UI generosity under the CARES Act will have important implications for the reallocation of labor during the pandemic. It has been hypothesized that disincentivizing people from going back to work may hinder reallocation in the labor market. Barrero *et al.* (2020) note that there are many businesses with both net and gross hiring. While our initial event study shows some suggestive evidence that firms in Homebase are in fact rehiring existing workers, and that UI generosity does not predict slower rates of rehiring in either Homebase or the CPS sample, further work to test the effects of expanded UI generosity on (a) whether firms change the headcount or composition of their workforces and (b) whether laid-off workers move to new jobs will be valuable.

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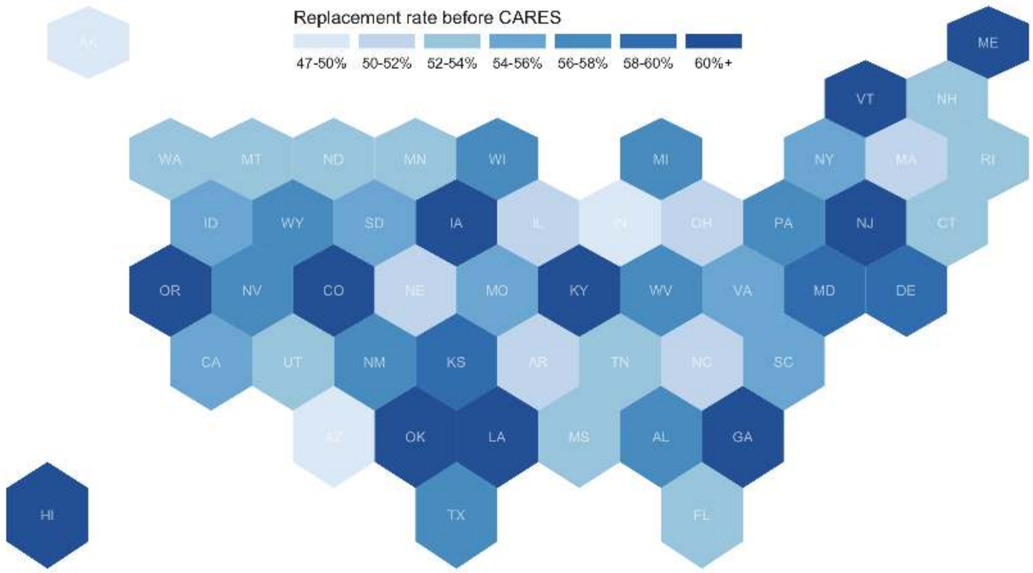
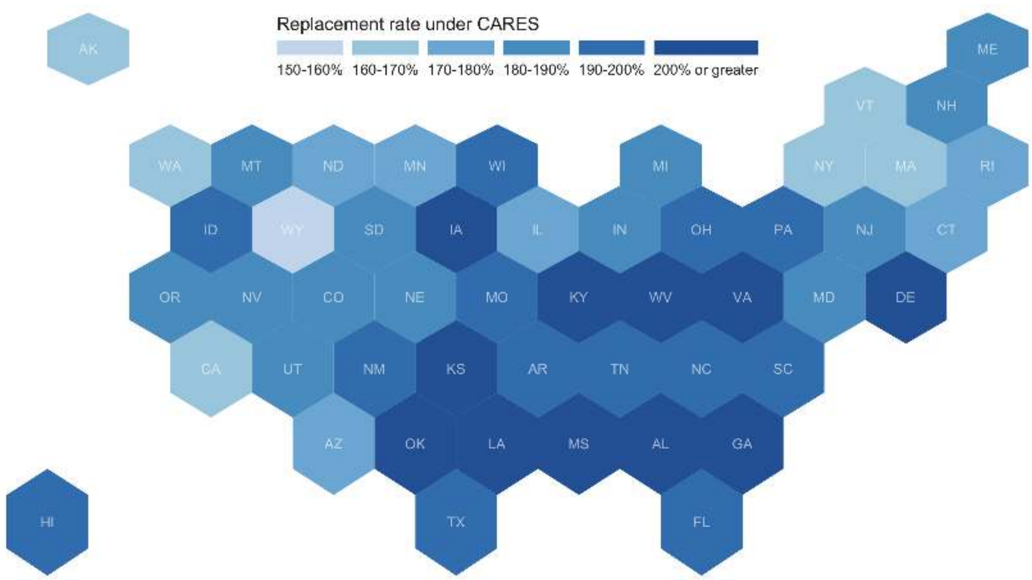
Figure 1: Median UI replacement rates, Homebase sample

(a) Before CARES



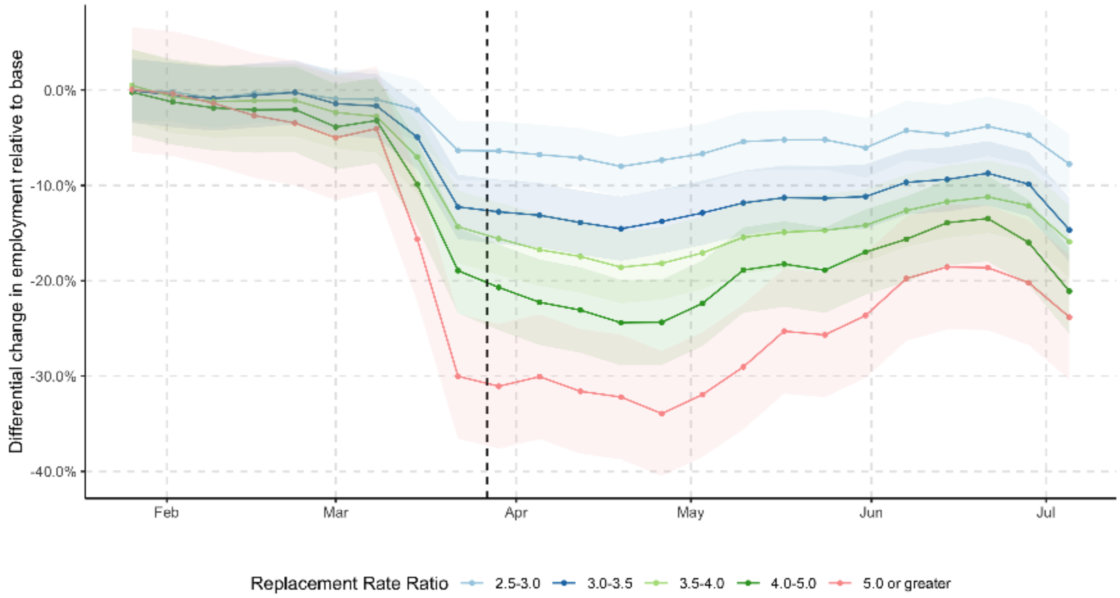
(b) After CARES



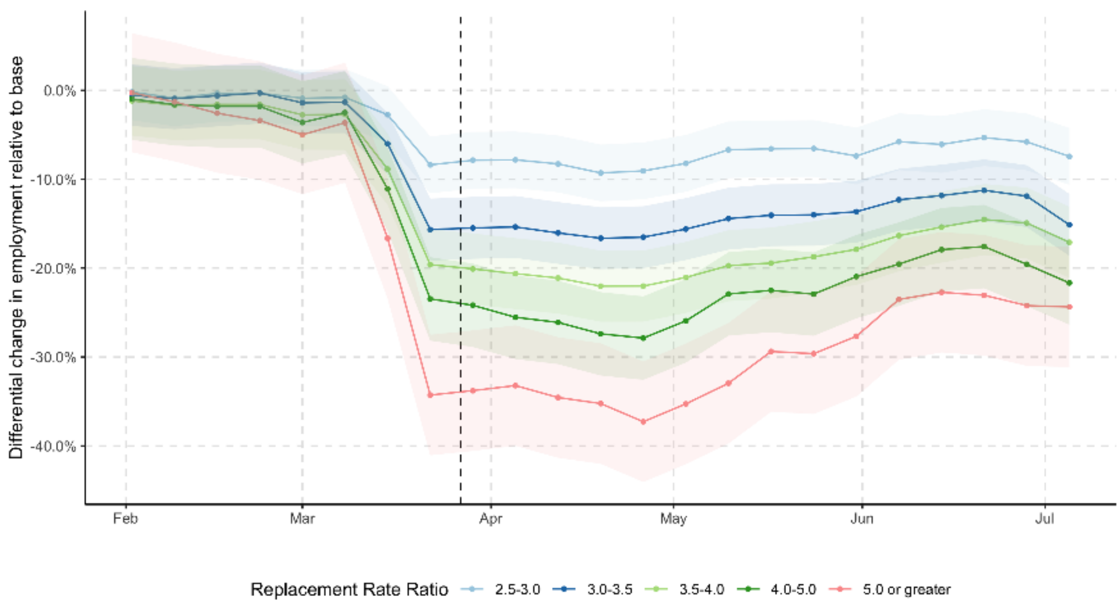
Notes: These figures show the median UI replacement rate by state for workers in the Homebase data, (a) according to state benefits schedules as of January 2020 and (b) as of the passage of the CARES Act. Under the CARES Act, all UI recipients in each state became entitled to an additional $600 federal 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.

Figure 2: Event study: effects of replacement rate ratio on probability of employment

(a) Without controls for state business restrictions



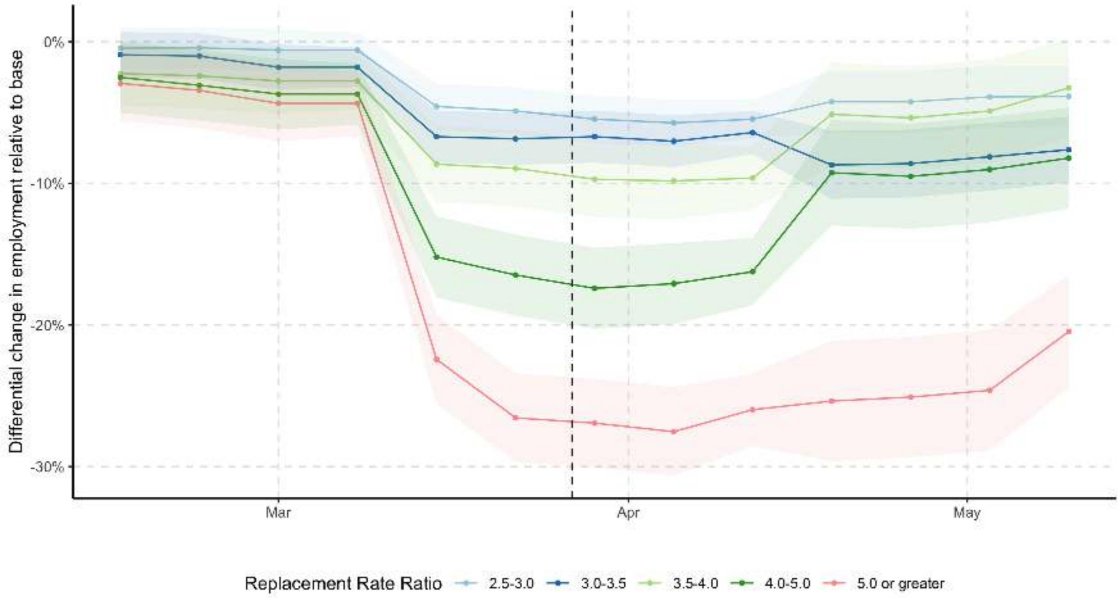
(b) With controls for state business restrictions and new Covid-19 cases



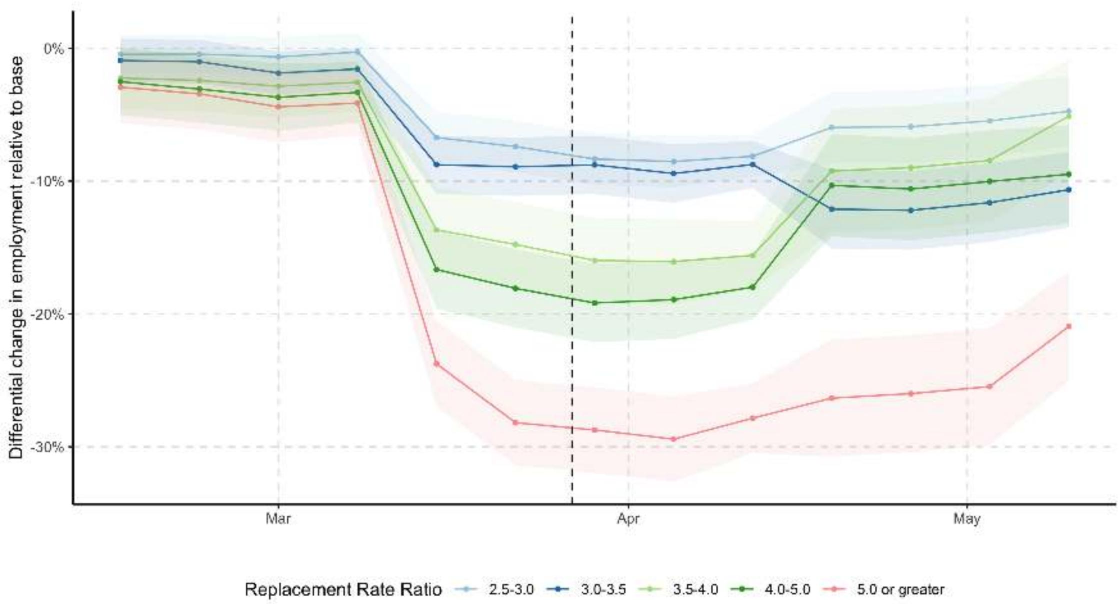
Notes: This figure show the γ_{gt} coefficients from equation 3 for replacement rate ratio group g at time t. In panel (b) we control for state restrictions as described in section 4.2 as well as for the number of new Covid-19 cases in a state in the week. The outcome y_{igjkt} is an indicator for whether individual i was employed at time t. The sample consists of all individuals in Homebase who (a) work at least 10 weeks in each quarter of 2019 for an average of 30 hours per week worked; (b) work at least 16 hours in the third and fourth weeks of January 2020; and (c) work at firms which record at least 40 worker hours in the third and fourth weeks of January 2020. The coefficients indicate the change in probability of employment relative to the reference group in the base period. The reference group is defined as workers who have a replacement rate ratio less than 2.5, i.e. workers whose replacement rate increases by less than 150 percent. The base period is defined as the two weeks from January 19 to February 1. The shaded areas around each line represent 90% confidence intervals. They are based on standard errors that are clustered at the worker level.

Figure 3: Event study: effects of replacement rate ratio on probability of employment – CPS

(a) Without controls for state business restrictions



(b) With controls for state business restrictions and new Covid-19 cases



Notes: This figure show the γ_{gt} coefficients from equation 3 for replacement rate ratio group g at time t. In panel (b) we control for state restrictions as described in section 4.2 as well as for the number of new Covid-19 cases in a state in the week. The outcome y_{igjkt} is an indicator for whether individual i was employed at time t. The sample consists of all individuals in the CPS who (1) had sufficient earnings history in 2018 to be eligible for UI benefits, earned an average hourly wage of at least \$7.25, and are U.S. citizens as of the 2019 ASEC; (2) were employed as of the week of February 8, 2020. The coefficients indicate the change in probability of employment relative to the reference group in the base period. The reference group is defined as workers who have a replacement rate ratio less than 2.5, i.e. workers whose replacement rate increases by less than 150 percent. The base period is defined as the week of February 9. The shaded areas around each line represent 90% confidence intervals. They are based on standard errors that are clustered at the worker level.

Table 1: Summary statistics of preferred Homebase sample

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Hours worked in base period	27,029	37.942	9.044	8.000	33.015	42.360	100.000
Hourly wage in base period	27,029	13.537	4.764	2	11	15.5	98
Pre-CARES replacement rate	27,029	0.554	0.074	0.093	0.520	0.588	1.225
Post-CARES replacement rate	27,029	1.853	0.440	0.273	1.559	2.100	4.358
Replacement rate ratio	27,029	3.350	0.707	1.729	2.857	3.700	7.207
Worked during week of 3/29	27,029	0.513	0.500	0	0	1	1

Notes: the workers in our Homebase sample: (1) worked for at least 10 weeks in each quarter of 2019, for an average of 30 hours per week; (2) worked at least 8 hours in the base period, defined as the two 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 40 worker-hours during the base period. "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.

Table 2: Summary statistics of preferred CPS sample

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Hours worked in base period	11,405	40.733	9.782	2	40	42	99
Hourly wage in base period	11,405	31.539	31.758	7.250	16.026	36.538	487.588
Pre-CARES replacement rate	11,405	0.390	0.150	0.013	0.272	0.500	2.057
Post-CARES replacement rate	11,405	1.175	0.758	0.041	0.685	1.468	15.482
Replacement rate ratio	11,405	2.981	1.435	1.729	2.279	3.182	43.000
Worked during week of 3/29	11,343	0.540	0.498	0.000	0.000	1.000	1.000

Notes: the workers in our preferred CPS sample: (1) had sufficient earnings history in 2018 to be eligible for UI benefits, earned an average hourly wage of at least $7.25, and are U.S. citizens as of the 2019 ASEC; (2) were employed as of the week of February 8, 2020. “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.

Table 3: Replacement ratio frequency in CPS vs. Homebase

Replacement Ratio	CPS	Homebase
2.5 or less	0.419	0.121
2.5-3.0	0.233	0.236
3.0-3.5	0.172	0.305
3.5-4.0	0.068	0.186
4.0-5.0	0.058	0.123
5.0 or greater	0.049	0.028
Observations	12,582	27,845

Notes: "Replacement ratio" indicates the ratio of a worker's expected benefits under CARES to their expected benefits prior to CARES, based on their 2019 earnings history for Homebase and 2018 earnings history for the CPS. The workers in our preferred Homebase sample: (1) worked for at least 10 weeks in each quarter of 2019, for an average of 30 hours per week; (2) worked at least 8 hours in the base period, defined as the two 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 40 worker-hours during the base period. The workers in our preferred CPS sample: (1) had sufficient earnings history in 2018 to be eligible for UI benefits, earned an average hourly wage of at least $7.25, and are U.S. citizens as of the 2019 ASEC; (2) were employed as of the week of February 8, 2020.

Table 4: Example benefit amounts and replacement rate ratios within and across states

	$300 Weekly Earnings			**$600 Weekly Earnings**		
State	Pre-CARES	Post-CARES	%Chg.	Pre-CARES	Post-CARES	%Chg.
California	$150	$750	+400%	$300	$900	+200%
Oregon	$195	$795	+307%	$390	$990	+154%

Notes: "Pre-CARES" refers to the benefit amount for which a worker earning an average weekly wage of $300 or $600 in 2019 would have been eligible in their respective state before the passage of the CARES Act. "Post-CARES" refers to the benefit amount for which the same worker would have been eligible after the CARES Act was passed. "% Chg." indicates the percentage change in benefit amount, which, as shown in equation 2, is equivalent to the percentage change in unemployment replacement rate.

Table 5: Week-by-week estimates of the effects of the replacement rate ratio on employment

	Dependent variable: employed					
	(1)	(2)	(3)	(4)	(5)	(6)
	3-29-20	4-5-20	4-12-20	4-19-20	4-26-20	5-3-20
Repl. ratio	-0.126^{***} (0.008)	-0.127^{***} (0.008)	-0.129^{***} (0.008)	-0.132^{***} (0.008)	-0.135^{***} (0.008)	-0.138^{***} (0.008)
Post 3-22* repl. ratio	-0.003 (0.003)	-0.003 (0.004)	-0.002 (0.004)	0.003 (0.004)	0.003 (0.004)	0.019^{***} (0.004)
Observations	54,058	54,058	54,058	54,058	54,058	54,058

Notes: This table shows the estimated coefficients for a series of simplified difference-in-differences linear probability models comparing employment in the week in which the CARES Act was passed to employment in each of the subsequent six weeks. For each regression, the observations on employment are for the week of March 22nd and observations on employment in the week indicated in the column. the sample consists of individuals who were employed full time in a Homebase firm for all four quarters of 2019. "Repl. ratio" indicates the ratio of a worker's expected benefits under CARES to their expected benefits prior to CARES, based on their 2019 earnings history and average wage in 2019. In each specification we control for industry, baseline replacement rate, baseline wage, the number of new cases of Covid-19, and whether the state had instituted each of the following business restrictions: (1) stay-at-home order, (2) mandatory closure of non-essential businesses, (3) mandatory closure of restaurants except for takeout service, and (4) mandatory closure of gyms. Standard errors are clustered at the worker level.

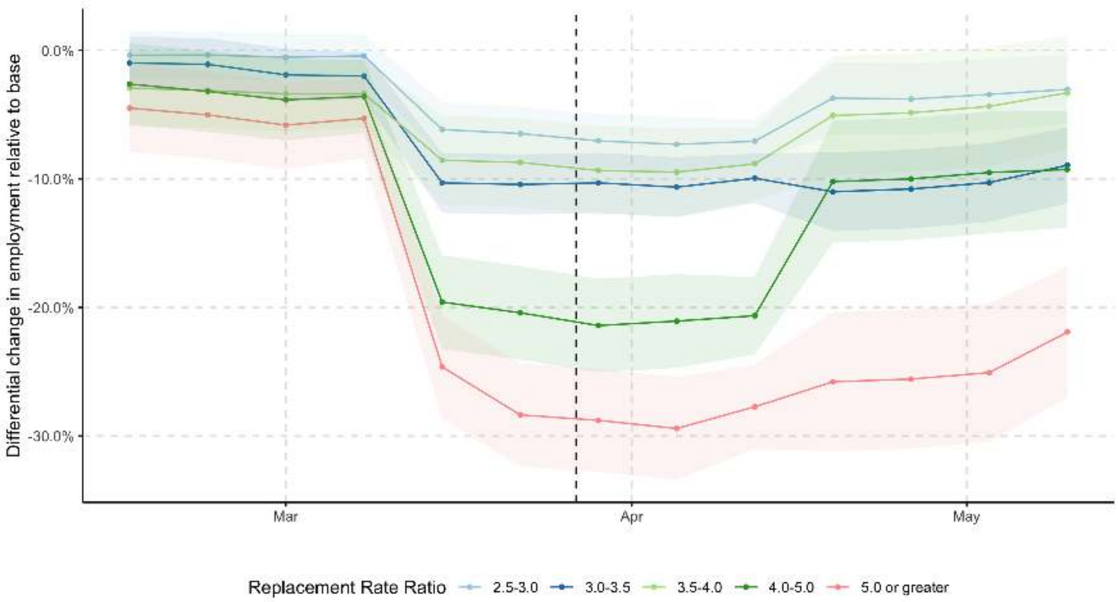
Table 6: Week-by-week estimates of the effects of the replacement rate ratio on employment – CPS

	Dependent variable: employed					
	(1)	(2)	(3)	(4)	(5)	(6)
	3-29-20	4-5-20	4-12-20	4-19-20	4-26-20	5-3-20
Repl.	-0.016^{***}	-0.017^{***}	-0.018^{***}	-0.018^{***}	-0.018^{***}	-0.018^{***}
ratio	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)
Post 3-22*	0.0001	0.0001	−0.001	−0.003	−0.003	−0.002
repl. ratio	(0.0003)	(0.0004)	(0.002)	(0.006)	(0.006)	(0.006)
Observations	23,206	23,206	31,261	15,192	15,192	15,192

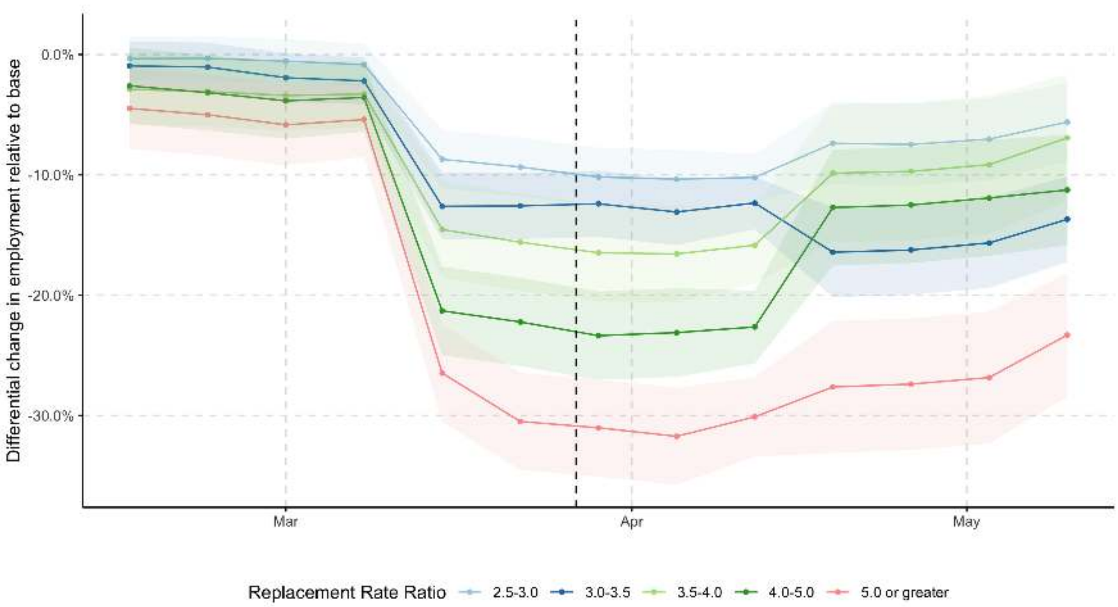
Notes: This table shows the estimated coefficients for a series of simplified difference-in-differences linear probability models comparing employment in the week in which the CARES Act was passed to employment in each of the subsequent six weeks. For each regression, the observations on employment are for the week of March 22nd and observations on employment in the week indicated in the column. In all weeks the sample consists of all U.S. citizens who (1) were in the labor force and earned at least the federal minimum wage in the 2019 ASEC and (2) were employed in the February 2020 CPS. In the weeks of March 8 and April 12 the sample consists of all individuals who were surveyed in the CPS on those dates. In all other weeks, employment is imputed using employment status in the monthly surveys before and after the relevant week; as a result, the sample consists of all individuals who were surveyed in both the month prior to the week and the month following the week. "Repl. ratio" indicates the ratio of a worker's expected benefits under CARES to their expected benefits prior to CARES, based on their 2019 earnings history and average wage in 2019. In each specification we control for industry, baseline replacement rate, baseline wage, the number of new cases of Covid-19, and whether the state had instituted each of the following business restrictions: (1) stay-at-home order, (2) mandatory closure of non-essential businesses, (3) mandatory closure of restaurants except for takeout service, and (4) mandatory closure of gyms. Standard errors are clustered at the worker level.

Appendix Figure 1: Event study: effects of replacement rate ratio on probability of employment – CPS, counting only those at work as employed

(a) Without controls for state business restrictions



(b) With controls for state business restrictions and new Covid-19 cases



Notes: This figure show the γ_{gt} coefficients from equation 3 for replacement rate ratio group g at time t. We define workers listed as having a job but who are not at work as unemployed. In panel (b) we control for state restrictions as described in section 4.2 as well as for the number of new Covid-19 cases in a state in the week. The outcome y_{igjkt} is an indicator for whether individual i was employed at time t. The sample consists of all individuals in the CPS who (1) had sufficient earnings history in 2018 to be eligible for UI benefits, earned an average hourly wage of at least \$7.25, and are U.S. citizens as of the 2019 ASEC; (2) were employed as of the week of February 8, 2020. The coefficients indicate the change in probability of employment relative to the reference group in the base period. The reference group is defined as workers who have a replacement rate ratio less than 2.5, i.e. workers whose replacement rate increases by less than 150 percent. The base period is defined as the week of February 9. The shaded areas around each line represent 90% confidence intervals. They are based on standard errors that are clustered at the worker level.

Appendix Table 1: Week-by-week estimates of the effects of the replacement rate ratio on employment – CPS, only counting those who were at work as employed

	Dependent variable: employed					
	(1)	(2)	(3)	(4)	(5)	(6)
	3-29-20	4-5-20	4-12-20	4-19-20	4-26-20	5-3-20
Repl.	-0.024^{***}	-0.024^{***}	-0.025^{***}	-0.028^{***}	-0.028^{***}	-0.028^{***}
ratio	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)
Post 3-22*	−0.001	−0.001	−0.0005	−0.004	−0.004	−0.003
repl. ratio	(0.001)	(0.001)	(0.001)	(0.006)	(0.006)	(0.006)
Observations	13,554	13,554	20,673	9,908	9,908	9,908

Notes: This table shows the estimated coefficients for a series of simplified difference-in-differences linear probability models comparing employment in the week in which the CARES Act was passed to employment in each of the subsequent six weeks. For each regression, the observations on employment are for the week of March 22nd and observations on employment in the week indicated in the column. In all weeks the sample consists of all U.S. citizens who (1) were in the labor force and earned at least the federal minimum wage in the 2019 ASEC and (2) were employed in the February 2020 CPS. In the weeks of March 8 and April 12 the sample consists of all individuals who were surveyed in the CPS on those dates. In all other weeks, employment is imputed using employment status in the monthly surveys before and after the relevant week; as a result, the sample consists of all individuals who were surveyed in both the month prior to the week and the month following the week. "Repl. ratio" indicates the ratio of a worker's expected benefits under CARES to their expected benefits prior to CARES, based on their 2019 earnings history and average wage in 2019. In each specification we control for industry, baseline replacement rate, baseline wage, the number of new cases of Covid-19, and whether the state had instituted each of the following business restrictions: (1) stay-at-home order, (2) mandatory closure of non-essential businesses, (3) mandatory closure of restaurants except for takeout service, and (4) mandatory closure of gyms. Standard errors are clustered at the worker level.