Micro and Macro Disincentive Effects of Expanded Unemployment Benefits *

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Abstract

This note updates the job-finding analysis in Ganong et al. (2021), estimating the disincentive effect of supplemental unemployment benefits between April 2020 and April 2021. We estimate the causal effect of the supplements using both a difference-in-difference research design and an interrupted time-series research design paired with administrative data. These empirical strategies can be used respectively to identify micro disincentive effects (the effect of increasing benefits for one worker) and macro disincentive effects (the effect of increasing benefits for all workers). Both designs imply a precisely estimated, non-zero disincentive effect.

However, the disincentive effect of expanded benefits is quantitatively small: implied duration elasticities are substantially lower than pre-pandemic estimates and suggest that eliminating the supplements would have restored only a small fraction of overall employment losses. Extending the difference-in-difference design through April 2021 suggests that the disincentive effect of the supplements remains modest even after vaccines are broadly available. We conclude that unemployment supplements are not the key driver of the job-finding rate through April 2021 and that U.S. policy was therefore successful in insuring income losses from unemployment with minimal impacts on employment.

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1 Introduction

This note updates the analysis in Ganong et al. (2021) of job search disincentives from supplemental unemployment benefits. In March 2020, the U.S. began an ambitious experiment by establishing supplemental benefits that provide extensive protection from the income losses arising from unemployment. This involved a $600 weekly benefit supplement which raised the median replacement rate to 145% through June 2020 and a $300 weekly benefit supplement which raised the median replacement rate to 95% beginning in January 2021 (Ganong, Noel, and Vavra, 2020).

How have expanded unemployment benefits affected the labor market? This question has captured the attention of policymakers. The current $300 supplement is scheduled to end on September 6, 2021, but 26 states have announced that they would stop paying the supplement sooner than that because they believe that the supplement is holding back the labor market recovery. Most economists, however, are quite uncertain about the effects of the supplement (Initiative on Global Markets, 2021). We provide new evidence on this question using administrative data from JPMorgan Chase Institute (JPMCI) on 800,000 benefit recipients from 10 large states.

We provide evidence that the supplements did in fact reduce the exit rate from unemployment to a new job. The weekly job-finding rate jumps up when supplements expire in August 2020 and falls again when new supplements begin in January 2021. Furthermore, these high-frequency changes in the job-finding rate are largest for workers with the largest change in benefits. We formalize these patterns and estimate precise causal effects of the supplements using both an interrupted time-series research design, which implies a reduction of the job finding rate of 0.6-0.8 percentage point per week and a dose-response difference-in-difference design, which implies a reduction of 1.0 percentage point per week. We provide conditions under which the interrupted time-series design estimates the macro disincentive effect (the effect of increasing benefits for all workers) and the difference-in-difference design estimates the micro disincentive effect (the effect of increasing benefits for one worker).

While non-zero, our estimates imply that the disincentive effects of benefits supplements are small. One simple way to benchmark the causal effect of the supplement is to compare it to overall movements in the job finding rate. Although the new job-finding rate increases from 1.6% per week to 2.4% per week when the first supplement expires, it remains much lower than the rate of 5% per week before the pandemic.

We quantify the disincentive effects of benefit supplements using a simple statistical hazard model as well as a structural job search model matched to our causal estimates. Under both approaches, we find small effects on unemployment durations from the benefit supplements. Specifically, the elasticity of duration with respect to benefits is around 0.1, which is smaller than most pre-pandemic estimates. This in turn implies low effects of the supplements on employment: the $600 supplement reduced employment by less than 0.8% and the $300 supplement reduced employment by less than 0.5%.

The effect of supplements on unemployment duration can be broken into two components, and both are important for understanding why the estimated duration elasticity is so small. First, one needs to know how supplements shift the job finding hazard. Second, one needs to know how a shift in the job finding hazard translates into a change in average unemployment duration.[1]

Understood in this way, there are three forces driving the small duration elasticity. First, our

[1]It is common to assume a constant hazard and infinite duration of benefits, in which case dlog duration = - dlog hazard and this second step is trivial. However, this is a poor assumption in our environment.

causal estimates on new job finding are small. In particular, the effects we estimate are substantially below the causal effects implied by our structural model calibrated to pre-pandemic evidence. Second, the presence of a high recall rate during the pandemic means that a proportional change in the *new* job finding rate translates into less than a proportional change in the *overall* job finding rate. Third, many unemployment spells are long relative to the duration of temporary supplements during the pandemic, which means that proportional changes in overall job finding rates when temporary supplements are in place translate to less than proportional changes in the average duration of unemployment.

Finally, this note provides new evidence on the job finding rate for UI recipients in March and April 2021, after job openings soared and vaccines were broadly available (but before any states announced that they were ending UI benefits earlier than the legislated September expiration). The aggregate job-finding rate rises in the spring of 2021, even for workers with replacement rates higher than 100%. Extending the difference-in-difference design through April 2021 indicates that the disincentive effect of the supplements remains modest. However, this estimate is speculative because it relies on extending the parallel-trends assumption over a longer time horizon than our main estimates.

2 Data and Policy Environment

This section briefly describes the data and policy environment. For additional details, see Ganong et al. (2021).

We measure unemployment benefit spells in 46 states using direct deposit UI payments in bank account data from JPMorganChase Institute (JPMCI) for January 2020 to May 2021. Our analysis focuses on ten out of the eleven states with the largest number of UI recipients in the sample: New York, New Jersey, Texas, Michigan, California, Indiana, Georgia, Ohio, Washington, and Illinois.[2] We define an exit to recall as an exit from UI where a worker starts receiving labor income from a prior employer. We define the residual (exit without recall) as exit to new job. The implementation of extended benefit eligibility through Pandemic Unemployment Emergency Compensation together with pre-existing provisions for benefit extensions mean that exits from unemployment insurance during our sample period (through May 2021) rarely reflect benefit exhaustion and therefore usually reflect a return to work.

Our analysis focuses on two policies: the $600 weekly supplement which expired at the end of July 2020 and the $300 weekly supplement which started January 2021.[3]

[2]These states account for 82% of all spells in the JPMCI data. For these ten states, we have validated that the weekly UI benefit amounts are line with external benchmarks. The eleventh large state is Florida; our conclusions about a modest disincentive effect also hold there, but the estimates face two technical challenges. First, they are noisier because of the state's well-known issues with issuing timely UI payments and the state's very low maximum benefit (which makes it more difficult to execute the difference-in-difference research design). Second, unlike the ten states that make up our main sample, Florida offered a very short duration of regular UI benefits in 2020. Hence, many recipients exhausted both regular benefits and PEUC in the fourth quarter of 2020, making it hard to interpret exits from UI as evidence of finding a job. In a future draft we hope to expand the sample to include additional states. Because the missing states are relatively small in size, we do not anticipate that our estimates will change much from adding these states.

[3]Although there was a temporary "Lost Wages Assistance" supplement paid for weeks claimed in August 2020, it was paid with substantial delay and haphazard implementation. In our prior paper we found little effect of this supplement and because of the nature of its implementation, it is difficult to use that supplement to learn about the disincentive effect of UI benefits.

The JPMCI data have five strengths for studying the disincentive effect of expanded UI: a very large sample size covering multiple states (1.2 million unemployment spells during the pandemic), a weekly frequency, the ability to measure actual UI benefit receipt, the ability to distinguish recalls from new job starts, and the ability to precisely measure the extent of differential trends between treatment and control groups.

First, with data on 1.2 million unemployment spells, we can construct statistically precise estimates of the disincentive effect. For example, we estimate that the micro effect of the $600 supplement was to lower new job-finding by 1.1 p.p. with a confidence interval from 1.0 to 1.2 p.p. For comparison, one estimate of the effect of the $600 supplement using the Current Population Survey relies on 4,000 monthly observations and reports a confidence interval from 0 to 3.2 p.p (Petrosky-Nadeau and Valletta, 2021). A pre-pandemic estimate of the disincentive effect in recessions uses 4,000 spells in the Survey of Income and Program Participation and estimates an elasticity of duration with respect to benefit levels with a 95% confidence interval ranging from 0 to 1 (Kroft and Notowidigdo, 2016). Our large sample sizes covering multiple states, together with information on direct deposit labor income prior to unemployment, also allows us to include industry and state fixed effects to address concerns about confounding trends.[4]

Second, a key strength of the JPMCI data is the ability to observe the job-finding rate by week. This weekly frequency enables us to credibly estimate interrupted time-series models to capture the effect of changes to UI policy on the job-finding rate. Furthermore, in our structural job search model, the validity of our empirical identification strategies depend on the nature of household expectations about benefit changes. Models with different expectations imply different dynamics and very different disincentive effects but are difficult to distinguish with traditional monthly data sources. Using our high frequency data we can distinguish these models and show that models in which benefit changes are a surprise are a much better fit to the data.

Third, the ability to observe actual UI benefit receipt enables us to have confidence that we are capturing labor market patterns for UI recipients. In comparison, a study of workers who report being unemployed in a survey will have both false negatives (not everyone who is unemployed gets UI) and false positives (UI recipients were not required to search for work during the pandemic and so many likely reported being not in the labor force).[5] The ability to observe actual UI benefits also enables us to accurately calculate replacement rates. In comparison, research designs which rely on datasets where UI benefits are not observed need to simulate the benefit level, potentially introducing attenuation bias.

Fourth, the JPMCI data separate recalls from new job starts. This is particularly important for studying the pandemic, when the share of workers expecting recall as well as actually exiting to recall greatly exceeded historical norms. The disincentive effects of UI benefits on recall may differ from new job starts for two reasons. First, unemployment insurance recipients must accept any offer of "suitable work". Second, as Boar and Mongey (2020) demonstrate, a jobseeker is likely to accept a recall at their prior wage over the likely wage loss that would arise from a taking a different job. We show that the high recall rate during the pandemic is important for explaining some of the small response of unemployment durations to changes in the new job finding rate that we observe.

[4] We also have potential scope to control firm fixed effects, which we plan to explore in ongoing work.

[5] For an example of how an analysis of the unemployed can be misleading about the behavior of UI recipients, in Ganong et al. (2021) Figure A-13 shows that the job-finding rate for UI recipients *fell* during the summer of 2020, but the Current Population Survey shows that job finding rate for the unemployed *rose*.

Fifth, we can assess the extent of differential trends in the time period without the supplement, which is a key specification test for the difference-in-difference design. Although this is in principle possible in any panel dataset, the informativeness of the exercise depends on the number of weeks of data in the no-supplement period and the number of benefit recipients in the data. The availability of a large number of benefit recipients (advantage #1) and weekly data (advantage #2) together make this test particularly informative in the JPMCI data.

We note that the JPMCI data are limited in that they only capture claimants with bank accounts at Chase who receive their UI benefits by direct deposit. We show in Ganong et al. (2021) that the JPMCI data match both the cross-state distribution of the amount of benefits as well as the time-series dynamics by state for the number of claims.

3 Descriptive Patterns

We plot the timeseries of the exit rate to new jobs and to recall in Figure 1.[6] The evolution of the new job-finding rate shown in Figure 1a can be divided into three time periods. At the start of the pandemic, the job-finding rate plunges by four percentage points and remains depressed thereafter. Second, the job-finding rate modestly rises and falls with the expiration and onset of the supplements.[7]

Third, the job-finding rate soars temporarily by over two percentage points in March 2021. Many factors may be contributing to this rise, including a surge in job openings and the advent of widespread vaccination. However, at least part of the increase appears related to the requirement that UI recipients re-certify their eligibility one-year after they start receiving benefits, and the large group of workers who lose their jobs at the start of the pandemic hit their one-year mark in March and April of 2021. Even though benefits extensions mean that most of these workers are eligible to continue receiving benefits if they re-certify, we find evidence that exit rates are especially high for workers around their one-year mark.[8] Interestingly, we also find that the share of workers exiting UI that receive payroll income from a new employer is actually higher in March 2021 than in previous months, so the workers exiting coincident with their one-year mark appear to be starting new jobs rather than dropping out of the labor force. Moreover, the total number of UI recipients is declining in many states, as shown in Figure A-5, so this rise in exit rates appears to be a general pattern.

Figure 1b shows that recalls exhibit a very different time-series pattern. Recalls soar with the first wave of reopenings in June and July, followed by a gradual secular decline through the end of December 2020 and then a gradual increase in 2021. Figure A-6 reports the sum of the two series and shows that the total job-finding rate has been lower in the pandemic, so recalls offset much but not all of the decline in the new job-finding rate. In the analysis that follows, we focus primarily

[6] Figure A-1 shows patterns in the number of UI recipients nationally and figure A-2 shows patterns for the 25 largest states in the sample.

[7] A lapse in federal benefits for Pandemic Unemployment Assistance and Pandemic Emergency Unemployment Compensation occurred briefly at the end of December. It took some time for states to restore benefits and so many workers appear to exit UI to a new job on January 3 and January 10, as shown in Figure A-3a. However, this change in the series does not capture a change in the new job-finding rate. We therefore use a “donut” around these dates in estimation below and in the figure.

[8] Figure A-4 plots exit rates separately for the workers who receive their first UI check in March and April of 2020 and all other workers. We see a sharp jump in exits in March and April 2021 for workers who start receiving benefits in March and April 2020. This is consistent with evidence from Bell et al. (2021) using administrative data from California. We also see a rise in exits during this time period even for workers who are not nearing the end of their benefit year, and thus for whom this re-certification requirement is likely not relevant.

on the effect of supplements of the new job-finding rate for reasons discussed above, although we report estimates for recalls as part of our robustness analysis.

4 Disincentive Effect of Benefit Supplements

In Section 4.1, we estimate a macro disincentive effect (the effect of giving all workers more benefits) using an interrupted time-series design. In Section 4.2, we estimate a micro disincentive effect (the effect of giving one worker more benefits) using a dose-response difference-in-difference design. In Section 4.3, we compare the two types of estimates.[9] Section 4.4 reports robustness checks and Section 4.5 discusses suggestive evidence about the effect of the $300 supplement in April and May 2021.

Let individuals be indexed by i. Let $r_i(b)$ be the worker's replacement rate (the ratio of weekly benefits to pre-separation earnings). r_i differs across workers because of differences in state UI policy, differences in the worker's pre-separation earnings, and possibly a flat supplement $b \in [0, B]$. Let e be the job-finding rate, which is a function of the worker's own replacement rate $r_i(b)$ and the replacement rate of other workers $r_{-i}(b)$.

This function e simplifies the environment by assuming that only current replacement rates affect the current job-finding rate. In practice, current replacement rates and expectations about future replacement rates affect the current job-finding rate. In Section 5 we relax this assumption by interpreting the empirical patterns described in this section through the lens of a dynamic model of job search. Another way that this assumption might fail is if savings from *lagged* replacement rates in prior time periods or liquidity from other sources (e.g. Economic Impact Payments) affect their current job-finding rate.[10]

We define three estimands of interest:

$$\tau_{macro}^{[0,B]} = E(e(r_i(B), r_{-i}(B)) - E(e(r_i(0), r_{-i}(0)) \tag{1}$$

$$\tau_{micro}^{b} = \frac{\partial E(e(r_i(b), r_{-i}(B)))}{\partial r_i} \tag{2}$$

$$\tau_{micro}^{[0,B]} = E(e(r_i(B), r_{-i}(B)) - E(e(r_i(0), r_{-i}(B)) = \int_0^B \tau_{micro}^{b} db \tag{3}$$

The micro effect captures the effect of increasing benefits for one worker, while holding benefits constant for all other workers. The macro effect contains two additional channels relative to the micro effect. First, it captures the immediate vacancy creation response to more generous UI benefits. More generous UI benefits could decrease vacancy creation because the match surplus is smaller (Hagedorn et al., 2013) or increase vacancy creation because of increased aggregate demand (Kekre, 2017). Second, it captures the "rat-race" effects in Michaillat (2012) where, if there is a fixed supply of jobs in a recession, discouraging one worker from taking a job may simply lead to another worker taking the job instead of a reduction in equilibirum employment.[11]

[9] A number of theoretical papers on unemployment insurance (cf. (Hagedorn et al., 2013) and Landais, Michaillat, and Saez (2018)) argue that the *micro* disincentive effect of unemployment benefits (the effect of giving one worker more benefits) alone is insufficient for determining the optimal level of benefits; one also needs to know the *macro* disincentive (the effect of giving all workers more benefits).

[10] We hope to explore this channel in future work.

[11] Estimates of the "macro" effect of UI benefits usually include the job-finding rate of unemployed workers who are

We note that our estimates do not capture two channels studied in some prior work measuring the macro response to UI. First, our identification strategies rely on high-frequency responses to policy changes. If vacancy creation or rat-race effects occur with a delay, they will not be captured by our designs. Second, our estimates capture only the effects on exit from unemployment; more generous UI may lead to more entrants to unemployment because of employer-side moral hazard Topel (1983) or to fewer entrants because higher aggregate demand reduces layoffs.

4.1 Interrupted time-series analysis

We use an interrupted time-series design to estimate the effect of the supplements on the job-finding rate. Figure 2a takes Figure 1a and zooms in to the time period where the new job-finding rate is depressed, from April 2020 through the first half of March 2021. We focus on this time period because it coincides with the time period when the pandemic was in full force in the US and vaccines were not yet broadly accessible.

To estimate the effects of the supplement, we compare the average job-finding rate in the two weeks prior to the policy change and first four weeks after the policy change. Using $t = 0$ as the first week after the policy change, we estimate $\hat{\tau}^{[0,B]}_{macro} = \sum_{t=0}^{3} e_t/4 - \sum_{t=-2}^{-1} e_t/2$. The average job-finding rate before and after the policy change are depicted using horizontal red bars in the figure. We extend the potential outcomes notation from the prior section to define $e(r_i(B), r_{-i}(B), t)$ where t captures time and the likely possibility that aggregate shocks have a direct effect on the job-finding rate.

We make the strong assumption that the job-finding rate would have been constant in the weeks just before and after a supplement change had there been no change in the supplement. This assumption can be stated algebraically as $\sum_{t=0}^{3} e(r_i(0), r_{-i}(0), t)/4 = \sum_{t=-2}^{-1} e_t/2$. If this assumption holds, then $\hat{\tau}^{[0,B]}_{macro} = \tau^{[0,B]}_{macro}$. While this is a strong assumption, we note that we are using high-frequency weekly data. This means any confounding changes must occur at exactly the same time as the changes in supplements.[12]

The job-finding rate rises by 0.76 p.p. when the $600 supplement expires. The job-finding rate then falls by 0.56 p.p. after the onset of the $300 supplement (omitting the "donut" discussed in footnote 7). These effects are economically small, as we discuss in more detail in Section 6. Further, we note that the job-finding rate is trending upward prior to the expiration of the $600 supplement; if our estimates were to instead assume that the job-finding rate was rising linearly in the absence of the policy we would estimate an even smaller effect from the expiration of the supplement.

To assess statistical significance, we conduct inference treating the exact date of the policy implementation as random. We view this assumption as plausible because the duration of the original $600 supplement (17 weeks) was chosen at a time when the duration of the pandemic and thus economic conditions 17 weeks in the future were highly uncertain. Similarly, the legislation which created the $300 supplement coincided with the renewal of other expiring federal pandemic unemployment pro-

not eligible for benefits. This group is not included in our estimates. However, this group is much smaller than at any prior time in U.S. history because traditionally-ineligible workers are covered through Pandemic Unemployment Assistance.

[12] This does not rule out all potential confounds. For example, seasonality in e_t could occur at high frequencies, and there are other policy changes (e.g. Economic Impact Payments) occurring at the same time that the $300 payments start in January, which might directly affect the job-finding rate (although these payments would, if anything, likely reduce job search and lead us to overstate rather than understate the magnitude of the already small effects we measure).

grams. The original duration of these programs (39 weeks for Pandemic Unemployment Assistance) and thus the exact date of their scheduled expiration was subject to the same uncertainty about the duration of the pandemic.

We compare the change in the job-finding rate at the actual dates of policy implementation to the change in the job-finding rate at 30 placebo dates where there was no implementation of a new policy. Figure 2b compares the distribution of the change in the job-finding rate at the placebo dates to the changes at the actual implementation dates. The observed changes at the policy implementation are more extreme than any of the changes at 30 placebo dates. Thus the p-value for the null hypothesis that the policy has no effect and the change we observe occurred at random is $1/(30+1)$ if we include the own implementation date and exclude the implementation date of the other policy.

We view the ability to make statistically precise statements about the macro disincentive effect of unemployment benefits as a strength of this analysis relative to the prior literature. Only a handful of prior papers estimate both the macro and micro disincentive effect of UI. Johnston and Mas (2018) and Karahan, Mitman, and Moore (2019) estimate the micro and macro effects of a benefit cut in Missouri in the Great Recession. These papers estimate the macro effects of the benefit cut using a synthetic control method. It is not possible to compute standard errors using this method. Fredriksson and Söderström (2020) estimate the micro and macro effects of changes in UI benefits in Sweden. The paper finds a macro elasticity of 3 and a micro elasticity of 1.5; however, the design is unable to reject equality of the micro and macro elasticities.

4.2 Dose-response difference-in-difference analysis

As a complement to the interrupted time-series analysis, we use a difference-in-difference design to estimate the causal impact of the supplement on job-finding. Because the legislation added a constant dollar amount to every worker's benefit, there is heterogeneity in the change in the replacement rate (the ratio of benefits to pre-separation earnings). For example, a worker with pre-separation earnings of \$600 per week and a regular weekly benefit of \$300 would see their replacement rate rise to 150%, while a worker with pre-separation earnings of \$1,000 per week and a regular weekly benefit of \$400 would see their replacement rate rise to 100%. Intuitively, we use heterogeneity in replacement rates r across individuals under the supplement to estimate the effect of the replacement rate on the job-finding rate, using the period without the supplement to control for any underlying heterogeneity between the groups absent the supplement.

This heterogeneity in the intensity of treatment motivates a dose-response difference-in-difference research design to estimate τ^b_{micro}. We first provide qualitative, graphical evidence that the effects of the supplements vary with the size of the increase in replacement rates, then describe the assumptions needed for identification of the causal effects of the supplements, and finally provide quantitative estimates.

4.2.1 Graphical Evidence

To measure the intensity of treatment for each worker we compute the percent change in benefits at the expiration or onset of a supplement. Because we will want to compare one event where a supplement expires and another event where a supplement begins, we use the average value of benefits with and without the supplement in the denominator (symmetric percent change):

$$PctChange_i = \frac{2(b_{i,post} - b_{i,pre})}{b_{i,pre} + b_{i,post}}. \tag{4}$$

We measure the benefit amount as the median weekly payment in the two-month period before the policy change. This calculation uses a slightly wider subsample than the interrupted timeseries design because we require an estimate of the weekly benefit amount in the pre period.

Figures A-7a and A-7b show the evolution of exit rates, dividing workers into those higher-than-median $PctChange_i$ ("more treated") and lower-than-median $PctChange_i$ ("less treated"). These figures show evidence of a reversal in the level of job-finding rates between the "more treated" and "less treated" groups. The job-finding rate is higher for the "more treated" group when there is no supplement and is lower when the supplement is available. It is challenging, however, to compare the two series because the level of the job-finding rate is slightly different: the low-wage workers who make up the "more treated" group have higher job-finding rates in the absence of the supplement.

To ease comparison between the two groups, we normalize the job-finding rate by the time period where the supplement is unavailable. Most event study designs compare a pre-period where the policy is not in effect and a post-period where the policy is in effect; it is therefore conventional to normalize the level of the outcome variable between the treatment and control group in the pre-period. We follow this convention for the onset of the $300 supplement in Figure 3b, and normalize average exit rates to be the same between the "more treated" and "less treated" groups in November and December. This pre-period in November and December corresponds to the period without the supplement. However, for the expiration of the $600 supplement shown in Figure 3a, the period where the policy is not in effect corresponds to the period *after* July 31. We therefore normalize average exit rates to be the same in August and September.

Two lessons emerge from comparing job-finding rates by replacement rate in Figures 3a and 3b. First, the two groups have similar trends in the job-finding rate in the absence of the supplement. Second, during the period where the supplement is available, the job-finding rate is lower for the group with higher replacement rates. This is consistent with a disincentive effect of the supplement. Figure A-8 shows standard errors for the difference in the exit rate between the two groups.

To fully exploit the variation in replacement rates in the data, we also construct the change in the job-finding rate separately by deciles of $PctChange_i$. Figures 4a and 4b show the relationship between the change in benefits and the change in the job-finding rate. In Figure 4a, a larger decrease in benefits is associated with a larger increase in the job-finding rate. In Figure 4b, a larger increase in benefits is associated with a larger decline in the job-finding rate. The relationships appear to be close to linear.

4.2.2 Identification and Estimation

In this section we exploit the full scope of our micro data to estimate causal micro effects in the cross-section. Let t index periods, i index workers and e_{it} be an indicator for exit to new job. We use data on two months where the supplement is not available and two months where the supplement is available as captured by the indicator $SuppAvail_t$. We estimate the additive model:

$$e_{it} = \gamma PctChange_i + \alpha SuppAvail_t + \beta SuppAvail_t \times PctChange_i + \varepsilon_{it} \tag{5}$$

Identification in the dose-response difference-in-difference design requires two assumptions. First, we make the standard orthogonality assumption: $\varepsilon_{it} \perp SuppAvail_t, PctChange_i$. The economic content of this assumption is that high and low-wage workers (who differ in $PctChange_i$) would have had the same trend in job-finding absent the policy change.

This first assumption has a testable prediction: parallel trends during the period when the policy is not in effect. Figures 3a and 3b show that the data appear to be consistent with this assumption for the exit rate to new jobs.[13] We also note that, unlike the interrupted time-series design, this identification strategy is robust to the presence of aggregate shocks that affect the job-finding rate equally for high and low-wage workers.

Second, we assume that the causal effect of replacement rates on job-finding is homogeneous in the treatment group and the control group. This assumption implies that raising a low-wage worker's replacement rate will have the same effect as raising a high-wage worker's replacement rate. de Chaisemartin and D'HaultfŒuille (2018) show that this assumption is necessary for identification in dose-response DiD. One reason that low-wage workers might be more sensitive to replacement rates is because they tend to have shorter employment durations. However, as we discuss above, the apparent linearity of the effect of benefit changes on the job-finding rate is consistent with a constant treatment effect.

Table A-1 reports estimates of equation 5. The key coefficient of interest is $\hat{\beta}$ which captures how the job-finding rate changes for more-treated vs less-treated workers. At expiration, we estimate $\hat{\beta} = 0.014$ and at onset, we find a similar coefficient of $\hat{\beta} = 0.017$. These effects are precisely estimated and highly significant.

We also estimate a version of equation 5 by week:

$$e_{it} = \gamma PctChange_i + \alpha Week_t + \beta_t Week_t \times PctChange_i + \varepsilon_{it} \tag{6}$$

This enables an event study interpretation of the coefficients. Figure A-12 shows standard errors for $\hat{\beta}_t$.

4.3 Comparison of micro and macro estimates

Comparing the micro and macro estimates requires rescaling the four estimates described above (two research designs and two policy changes) into common units.

Comparisons within an episode require extrapolating the effect of a marginal change in replacement rates (τ^b_{micro}) into the effect of the entire supplement ($\tau^{[0,B]}_{micro}$), which extrapolates well beyond the range of variation available in the data. Continuing the example from the beginning of the section, we want to use the causal effect estimated from comparing the job-finding rates of recipients with replacement rates of 100% and 150% to estimate the job-finding rate for a worker with a replacement rate of 50% in the absence of any supplement.[14] We extrapolate by multiplying the

[13] While this parallel pre-trend is reassuring, one might still be concerned about differential labor market trends for high and low-wage workers due to the uneven incidence of the pandemic across industries, locations and workers of different ages, all of which are potentially correlated with wage levels. However, in Section 4.4, we show that nearly identical conclusions obtain when exploiting only within state-age-industry group variation.

[14] This is beyond the support of the data because we cannot measure replacement rates for workers who are at the maximum benefit level, since we infer wages from benefit payments. If we could, then we could directly measure the effects on the exit rate for very high wage unemployed workers who have replacement rates near 0.5 even with the $600.

estimated $\hat{\beta}$ by $E(PctChange_i)$ for each supplement change.

Two types of evidence bolster the plausibility of such an extrapolation. First, within the empirical variation available in the data, the relationship between the intensity of treatment (size of the change in benefits) and the outcome (change in the exit rate) appears to be linear in Figures 4a and 4b. Second, in analysis of a structural model of job-finding in Section 5.2.2, we show that the effect of the supplement on the job-finding rate is close to linear in the size of the effect of the supplement.

Table 1 shows our headline estimates of how UI supplements affect the job finding rate. Table 1 shows that the macro effect of the \$600 supplement is to reduce the weekly job-finding rate by 0.76 p.p. and that the micro effect was to reduce it by 1.14 p.p. It also shows that the macro effect of the \$300 supplement is to reduce the job-finding rate by 0.56 p.p. and the micro effect is a reduction of 0.98 p.p. As we discuss in Section 6, these effects on the job finding rate are non-zero but economically small.

It is also useful to compare the effects of the \$600 supplement to the effects of the \$300 supplement. To compare across episodes with different supplement sizes, we convert each estimate of the full supplement effect into an implied causal effect of increasing benefits by \$100 relative to a baseline with no supplement.[15] These results show effects of the policies per \$100 were similar for both the \$600 supplements which came earlier in the pandemic and the \$300 supplements which came later in the pandemic.

4.4 Robustness of main estimates

We conduct a number of tests to probe the robustness of the results. In one group of checks, we report estimates for alternative measures of UI exit: exit to recall in the sample where separation is observed, any exit (new job or recall) in the sample where separation is observed, and any exit (not conditional on whether separation is observed).[16] Figure 1b shows that the aggregate exit rate to recall is low around the onset of the \$300 supplement. Figure A-11b shows that there is little difference in the recall rate by replacement rate group around the onset of the \$300. Table A-2a re-estimates equation 5 for these three additional measures and shows that incorporating recalls into the measure of job-finding has little effect on our estimates.

In contrast, recalls are an important part of the aggregate story around the expiration of the \$600, but the interpretation in terms of the disincentive effect of the supplement is ambiguous. Incorporating recalls into our estimates of equation 5 in Table A-2b substantially increases the estimates of $\hat{\beta}$. To understand why $\hat{\beta}$ increases, note that Figure A-11a shows that there is an increase in recalls in the more treated group in the *no-supplement* period. This suggests that the parallel trends assumption may not be satisfied around the expiration of the \$600 for recall.

It is possible that employers delayed the recall of some of their workers until after the supplement expired, and further that they disproportionately did so for workers with high replacement rates. However, one feature of Figure A-11a which is inconsistent with this story is that there is no difference in the recall rates between more treated and less treated workers in the three weeks immediately

[15] The models in Section 4.2 are estimated using symmetric percent change $PctChange_i$. The average of $PctChange_i$ is 81% for the \$600 supplement and 57% for the \$300 supplement. Note that because we are using symmetric percent change in equation 4, $PctChange_i$ is not linear in the size of the supplement. Relative to a no-supplement baseline, paying a \$100 supplement has an average value of 20% for $PctChange_i$. We therefore rescale the estimates from the \$600 supplement by 20%/81% and the estimates from the \$300 supplement by 20%/57%.

[16] All of the analysis to date has focused on the sample where a separation is observed, because this screen is necessary to separate exits to recall from exits to new job.

after the supplement expires. The effect of the \$600 supplement is thus uncertain and in future work, we hope to more thoroughly investigate the effect on recalls.

In a second group of checks in Tables A-3a and A-3b, we re-estimate equation 5, adding different controls X_i and $X_i SuppAvail_t$ to control for differential trends. First, we add state (and state-by-supplement available) fixed effects, so that identification comes from comparing the job-finding rate for higher- and lower-wage workers with different benefit replacement rates in the same state. Second, we add age (and age-by-supplement-available) fixed effects, so that identification comes from comparing the job-finding rate for higher- and lower-wage workers with different replacement rates who are in the same state and are the same age. Third, we add industry (and industry-by-supplement-available) fixed effects, so that identification comes from comparing the job-finding rate for higher- and lower-wage workers with different benefit replacement rates who are in the same state, are the same age, and worked in the same industry. Our estimates of $\hat{\beta}$ change little from incorporating these control variables.

4.5 Effect of supplements in April 2021

In our final set of empirical results, we extend the time horizon for the estimates of the disincentive of the \$300 supplement in Table A-4.

These estimates are more speculative for two reasons. First, the no-differential trends assumption needs to hold for a longer time period. Second, the no-differential trends assumption becomes more speculative when there are large aggregate shocks.

The first column of Table A-4 shows that in the sparsest specification with no controls, the disincentive effect appears to fall nearly to zero for the period from the end of March through early May 2021. However, this is not our preferred specification because the assumptions required to interpret this as a true causal effect may be violated during this time period. In particular, a disproportionate surge in labor *demand* for low-wage workers, for whom the benefit supplement represented a larger percent change (and hence were "more treated" by the policy), could lead to higher exits for these workers. This would lead to a lower implied response to the supplements even if the true causal effect of the supplement was unchanged.

There are two reasons to believe this type of effect may indeed be biasing the estimates in Column 1, which has no controls for labor demand. First, data from the BLS does suggest a particularly large increase in labor demand for low-wage workers in the leisure and hospitality industry during March and April 2021. Second, while the point estimate in Column 1 uses a continuous measure of treatment which therefore puts more weight on observations at the tails of workers who were most (and least) treated, the binary measure of treatment shown in Figure 3b gives equal weight to these groups, and indicates a *constant* disincentive effect throughout the beginning on May.

To address this concern we add increasingly stringent controls as in Table A-3b , looking within groups of workers who are more likely to be facing similar changes in labor demand from late March to early May 2021. Column 2 in Table A-4 adds state (and state-by-supplement available) fixed effects, Column 3 adds age (and age-by-supplement available) fixed effects, and finally Column 4 adds industry (and industry-by-supplement available) fixed effects. The point estimates in these specifications show a nearly identical disincentive effect from late March to early May as compared with the beginning of the year.

We conclude that after accounting for potential differences in labor demand by looking within local labor markets segmented by industry, the disincentive effect appears similar throughout the first quarter of 2021. Thus, the advent of broadly available vaccines and a surge in job openings does not appear to have magnified the disincentive effect, at least through early May 2021. However, we caution that this conclusion relies on more speculative assumptions and believe more data is needed to make definitive conclusions about the effect of benefit supplements beyond the early months of 2021.

5 Model

5.1 Motivation and Setup

In this section we extend the theoretical model developed in Ganong et al. (2021) to match the causal estimates developed in the previous section. This serves three purposes.

First, the validity of the "model-free" regression approaches depends on assumptions which can be tested in the model but not in the data. In particular, estimating the micro disincentive effects of the \$300 and \$600 supplements using the difference-in-difference approach requires a linear extrapolation assumption discussed in Section 4.3. Furthermore, both the difference-in-difference approach and the interrupted time-series approach must make the assumption that the disincentive effects are constant over time, an assumption discussed at the start of Section 4. This assumption would be violated if households anticipate and adjust their current search behavior substantially in response to future benefit changes.

Second, the structural model allows us to construct counterfactuals which help with comparisons to prior empirical estimates. In particular, the prior empirical literature typically estimates disincentive effects to small increases in benefits, typically lasting for around 26 weeks. Furthermore, these estimates come from economic environments which differ from the pandemic in many ways. In contrast, our empirical estimates measure the response to much larger changes lasting for either 17 weeks (\$600 supplement) or 36 weeks (\$300 supplement) during the pandemic. Using our structural model, we can compute counterfactuals which allows us to disentangle the separate role of differences in policy from differences in the economic environment when comparing our disincentive effects to prior estimates. In particular, we can use the model to calculate the response to a small counterfactual 26 week increase in benefits like that studied in the prior literature but holding fixed all other aspects of the economic environment estimated on pandemic-era data.

Third, the structural model is useful for better disentangling the channels through which benefit changes manifest in household search decisions and ultimate unemployment durations. That is, it helps us to interpret our simple "reduced-form" causal estimates.

Our theoretical analysis largely follows the "enriched" model from Ganong et al. (2021), with enhancements necessary to speak to our new empirical evidence. We refer the reader to Ganong et al. (2021) for most of the details of the setup but briefly recap the key features of the environment here before describing changes relative to Ganong et al. (2021). In the model, an unemployed worker with prior wage w receives an unemployment benefit bw for 52 weeks, faces a cost of searching for a job which again pays wage w and also has an exogenous probability of recall which can result in them gaining employment without any search effort. The pandemic unemployment supplements are flat payments F added to unemployment benefits which do not depend on a worker's past wage.

In actuality, the \$600 payments are in effect from April-July, 2020 and the \$300 supplements are in effect from January-September, 2021, but we allow for expectations of benefit duration which differ from actual duration. For example, we explore models where the expiration of the \$600 supplements in August was expected and others where the expiration was a surprise and discipline these expectations using implications for observed search behavior.

Our model in this note has an important enhancement from that in Ganong et al. (2021), which is necessary to speak to our new evidence. In particular, we introduce cross-household heterogeneity in wages in order to speak to the cross-sectional empirical evidence. In particular, we assume that there are five different types of households indexed by i with five different wage levels w_i, which we discipline using pre-job loss income data by quintiles for the unemployed in JPMCI. This heterogeneity in wages together with a flat benefit supplement means that the replacement rate is higher for low wage than for high wage workers. In addition, we extend the sample of our analysis to run through March 2021, and we allow for search costs to differ in the period of time when \$300 and \$600 supplements were in effect.

For each supplement episode we estimate one version of the model which targets the microeconomic difference-in-difference estimates and a separate version of the model which targets the macroeconomic interrupted time-series estimates. More specifically, given a set of model parameters, we simulate the average new job finding rate (averaging across households of different wages) at each point in time. We then compute the interrupted time-series estimates exactly as in the data given this simulated job finding rate. Similarly, we compute the job finding rate by each individual wage group and then run the same difference-in-difference regression in the model that we run in the data. To pin down the model's "macro calibration" we adjust the model parameters to hit the interrupted time-series coefficient in the data while for the model's "micro calibration" we adjust parameters to hit the difference-in-difference regression coefficient.

5.2 Model Results

5.2.1 Expectations and Dynamics

We begin our analysis of model results by looking at the role of expectations and search dynamics. Figure 5a shows how the evolution of search in the model compares to the data before and after the expiration of the \$600 supplement in August. The dashed line in black shows the new job finding rate in the data. The red line shows results from a version of the model where households correctly anticipate that the supplement will expire in August while the blue line shows results from a version of the model where expiration is a surprise. Parameters in both models are picked to try to match the overall time-series as closely as possible. When the supplement is expected to expire near future, search rises substantially in advance of expiration, in contrast to the data.[17] This suggests that a surprise expiration is more consistent with the data than an expected expiration in a model with optimal search.

Figure 5b shows how the evolution of search in the model compares to the data before and after the start of the \$300 supplement in January. Here we assume that the start of benefits in January was a surprise but contrast two different expectations for their duration. Supplements were signed into law at the end of January with a scheduled expiration in mid-March; however, the "American

[17]The surprise expiration model exhibits a much more mild upward trend prior to expiration; this arises because households anticipate the exhaustion of regular UI benefits after 52 weeks.

Rescue Plan" which was signed into law on March 11 extended these supplements to September. Thus we explore two different expectations about the duration of benefits. In one model, households anticipate that benefits will expire in March and are then surprised when they are extended to September. In the other, households anticipate that benefits will last through September when they are started in January. Behavior in the model in which benefits are expected to last for an extended period of time is more consistent with the data than in the model where benefits are expected to expire and then are extended unexpectedly.

One way of interpreting these results for both the $600 and $300 episodes is that observed search behavior is broadly consistent with naive expectations in which households expect whatever benefit level they are currently receiving to continue for a long period of time in the future: households receiving the $600 are surprised when they stop in August and households receiving the $300 are not surprised when they continue in March.

We note again that the ability to distinguish these two very different models of expectations hinges crucially on the weekly data available in JPMCI. If we only had data on the monthly job-finding rate, we would be unable to distinguish between these models. Why does this matter? Models with different expectations imply very different disincentive effects and have very different implications for the validity of our empirical strategy.

Reassuringly, our model results provide some support for the assumption of constant effects underlying our causal empirical estimates, at least for the months immediately around the policy changes. Figure 6 shows this more concretely. To construct this figure, we begin by computing a model counterfactual without the supplements. We can then calculate the difference Δ_t in each week t between the job finding in this no supplement counterfactual and that in the model in which there are supplements. Δ_t summarizes the effect of the supplement on job search in each week. Figure 6 shows the time-series of Δ_t divided by its value in the week of the policy change. When this ratio is equal to one, the effect of the supplement on job finding in a given week is the same as the effect in the week when the policy changes.

Figure 6 shows that under the expectations which better fit the observed job finding data (shown in blue in Figures 5 and 6), the effects of the supplements are relatively constant for most of the time that supplements are in place. Effects die off rather than remaining constant around the time of expiration of the $300 supplement in September, but it is important to note that this prediction occurs many months after the current support of the data so it cannot be tested yet (note that the time-span covered by Figure 6 is extended relative the observed series in 5b in order to capture these dynamics). Within the scope of the data currently available, effects are relatively constant. Overall, these results support the assumption underlying our regression-based procedure: effects measured around the time of policy changes as in the regressions provides a useful summary of policy effects over time. However, the model results under other expectations show that, even though this assumption appears reasonable in our empirical context, it need not work in general. Intuitively, when there are substantial anticipation effects of future policy changes, impacts measured at the date of policy changes can deviate from true policy effects. For example, when the $600 is expected to expire at the beginning of September, search ramps up substantially just before expiration. This in turn means that distortions measured at the time of expiration understate the total effect of the policy.

5.2.2 Non-linearities and Mapping Cross-Section to Aggregate Effects

As discussed in 4.3, measuring the micro effects of the \$600 and \$300 supplements based on the observed cross-sectional variation in replacement rates across households requires a linear extrapolation out of sample. If the effects of supplements on search are non-linear, then a linear regression estimated over the support of the data need not recover the correct micro effect of the \$300 or \$600 policy.[18]

A closely related observation is that $\hat{\alpha}$, the coefficient on *SuppAvail*, provides a measure of deviations between time-series based disincentive estimates and cross-section based disincentive estimates. One interpretation of empirical results that $\hat{\alpha} \neq 0$ is that $\tau^{[0,B]}_{macro} \neq \tau^{[0,B]}_{micro}$. However, non-linearities could also lead to measured $\hat{\alpha} \neq 0$ even in an environment where the true $\tau^{[0,B]}_{macro} = \tau^{[0,B]}_{micro}$.

Our model allows us to simultaneously explore both of these misspecification issues. In particular, we can assess the linearity of relationships between replacement rates and disincentive effects across households with different replacement rates in the model. We can also ask whether difference-in-difference regressions run in data simulated from the model produce $\hat{\alpha} = 0$. Our model does not include any forces like congestion which can lead to deviations between macro and micro disincentives, so in our model $\tau^{[0,B]}_{macro} = \tau^{[0,B]}_{micro}$, and $\hat{\alpha} \neq 0$ should be interpreted as evidence of misspecification rather than as evidence of differences between macro and micro disincentive effects. Further, not only does this provide a check of misspecification, it also simultaneously provides a natural corrective to any such misspecification: micro estimates in the model should be adjusted by the value of $\hat{\alpha}$, and this same model based correction should also be applied to empirical estimates from the cross-section.

Figure A-13 shows relationships between replacement rates and average exit rates in the models calibrated to match the empirical micro evidence. These models are calibrated to hit the same slopes as in Figure 4, but the model does not impose anything about linearity and imposes no restrictions on the intercept α. Relationships are nevertheless close to linear in the model, again bolstering our empirical approach. Furthermore, the model calibrated to the expiration of \$600 generates a nearly zero value $\hat{\alpha} = -.0015$, suggesting that there is little misspecification when extrapolating from cross-sectional estimates. There is more moderate evidence of non-linearities in the model at the onset of \$300, driven by the influence of households with the highest replacement rates. The model regression at the onset of the \$300 generates a value $\hat{\alpha} = .0051$. Interestingly, this is very similar to the value of 0.006 in the data. Correcting the empirical estimates for the degree of misspecification exhibited by the model would reduce the empirical micro disincentive effect of the \$300 and indeed lead to a level that closely aligns with the macro estimate.

Finally, it is useful to note that there is little tension in the model between hitting the micro difference-in-difference regressions on the cross-section and hitting the macro interrupted time-series estimates. The macro calibration of the \$600 supplement implies a cross-sectional regression coefficient of 0.015, which is very close to the empirical coefficient of -0.014 in Table A-1. Similarly, the micro calibration which targets this value of -0.014 implies an interrupted time-series coefficient of 0.0086 while this value is 0.008 in the data. This means that there is little trade-off between hitting the micro and macro targets at expiration. At onset, the macro calibration implies a cross-section coefficient of -.004 while the empirical value is -.0174. This is a more substantive departure, but it

[18] We note that this issue of potential misspecification is not driven by our particular data or analysis and applies equally to other papers using cross-sectional variation to estimate disincentive effects of supplements.

takes only a modest change to the calibration to hit this. In particular, the micro calibration implies an interrupted time-series coefficient of -.0074 vs. a target of -.006, so a mild amount of additional sensitivity of search to benefits rapidly amplifies the cross-section coefficients while having only a modest effect on the time-series jump. This difference between -.0074 and -.006 is not statistically significant, so in that sense the cross-section and time-series coefficients at onset can be matched simultaneously in the model. Figure 7 demonstrates this consistency visually by showing that the models calibrated to micro distortions have time-series implications which are very similar.

6 Interpreting Magnitudes

In this section, we interpret the magnitude of the disincentive effects implied by the causal estimates in 4. We do so using both a simple statistical hazard model and the structural model developed in Section 5.

We begin by reporting the effect of the supplements on average unemployment duration, as measured by a duration elasticity:

$$elasticity = \frac{\left(\frac{\text{Ave U Duration w/ Supplement} - \text{Ave U Duration no Supplement}}{\text{Ave U Duration no Supplement}}\right)}{\left(\frac{\text{Ave Benefit w/ Supplement} - \text{Ave Benefit no Supplement}}{\text{Ave Benefit no Supplement}}\right)}.$$

The counterfactual exercises necessary to compute this duration elasticity in the structural model are straightforward. We complement these model counterfactuals with a simpler statistical calculation which does not rely on our model structure and instead uses only the results from the empirical regressions. In particular, call the total exit hazard observed in the data (which includes the effect of the supplement when it is in place) $\lambda_{t,\text{with supp}} = e_t + recall_t$, with observed new job finding rate e_t and observed recall rate $recall_t$.[19] We then construct a counterfactual total exit hazard with no supplement: $\lambda_{t,\text{no supp}} = \lambda_{t,\text{with supp}} + \tau_{supp} \times I_t(supp = on)$, where τ_{supp} is an estimate of the effect of a given supplement on the job finding rate, summarized in Table 1, and $I_t(supp = on)$ is an indicator for whether a supplement is on or off in week t. That is, the simple statistical counterfactual without supplements just shifts up the observed job finding rate by the constant amount τ_{supp} while the supplement is in effect. Given $\lambda_{t,\text{with supp}}$ and $\lambda_{t,\text{no supp}}$ we can compute expected unemployment durations with and without the supplements and thus the duration elasticity.

The first two rows of Table 2 show implied duration elasticities in response to the $600 and $300 supplements, computed using the structural model as well as the statistical hazard regression based approach.[20] The model implied duration elasticities are generally very similar to those under the statistical approach, with the potential exception of the effects of the $300 supplement based on the micro difference-in-difference estimates. Implied duration elasticities in that regression based specification are somewhat larger than those implied by the structural model because they assume constant distortion effects from January until September, while the structural model implies that disincentive effects should decline as expiration approaches in September (as illustrated in Figure 6).

[19]We assume e_t and $recall_t$ are constant at their sample averages after the end of the observed data.

[20]The macro calibrations target the size of the interrupted time-series estimates and the micro calibrations target the size of the micro difference-in-difference estimates. However, as discussed above, these two calibration approaches yield fairly similar conclusions.

All of these duration elasticities are small. One way to get a sense of this is to compare these estimates directly to duration elasticities estimated in the prior literature. Five of our eight estimates are below *every* prior elasticity estimate from 18 microeconomic studies reviewed in a recent meta-analysis by Schmieder and von Wachter (2016). All are below the 25th percentile of the estimates in the prior literature (0.28), and even our highest estimate of 0.18 is in line with the lowest estimates in the prior literature. Furthermore, it is important to note that the prior literature typically studies small benefit changes usually lasting for around 26 weeks, while we are computing responses to large benefit changes of different lengths. Table 3 shows that if we use the model to calculate duration elasticities in response to small 26 week policy counterfactuals which more closely correspond to the prior literature, the estimated elasticities are even lower.[21] Table 3 also illustrates the small size of the elasticities we estimate by comparing them to those implied by the model calibrated to pre-pandemic evidence discussed in Ganong et al. (2021). This model, calibrated to pre-pandemic estimates, implies an elasticity which is an order of magnitude larger than the models calibrated to job search during the pandemic.

The effect of supplements on unemployment duration can be broken into two components, and both are important for understanding why the estimated duration elasticity is so small: First, one needs to know how supplements shift the job finding hazard. Second, one needs to know how a shift in the job finding hazard translates into a change in average unemployment duration.

Understood in this way, three forces drive the small duration elasticity. First, our causal estimates of the effects of supplements on new job finding are small. In particular, the effects we estimate are substantially below the causal effects implied by the structural model calibrated to pre-pandemic evidence. That model implies that the \$600 supplements should have reduced the job finding rate by 8 percentage points while we find a decline of around 1 percentage point, and it implies that the \$300 supplements should have reduced the job finding rate by 4.8 percentage points while we find a decline of 0.5-1 percentage points.[22]

Second, the presence of a high recall rate during the pandemic means that a proportional change in the new job finding rate translates into less than a proportional change in the overall job finding rate. Third, many unemployment spells are long relative to the duration of supplements during the pandemic, which means that proportional changes in overall job finding rates when supplements are in place translate to less than proportional changes in the average duration of unemployment. Put differently, the presence of these second and third forces means that one cannot apply the common approximation that $d \log \text{duration} = -d \log \text{hazard}$ to back out the effects of changes in the new job finding rate on unemployment duration. Table A-5 demonstrates that accounting for the presence of recalls and the finite duration of supplements dramatically lowers the duration elasticity arising from a given shift in the new job finding rate.[23]

In addition to these duration elasticities, we also estimate the effects of the supplements on

[21]Note that these values are slightly higher than comparable statistics reported in Ganong et al. (2021), which is a result of expansions of the underlying data sample.

[22]Note that the pre-pandemic model does not distinguish between recalls and new job finding and instead targets the total job finding rate: this leaves some additional room for declines in the job finding rate relative to a model targeting only the new job finding rate, as the pre-pandemic model implies that search drops to zero while the benefits are in place. If we instead target the new job finding rate alone then there is a decline of around 5 percentage points instead of 8 percentage points from the \$600 supplements.

[23]In fact, the presence of *either* a high recall share or a long duration of unemployment relative to supplement lengths is sufficient to substantially reduce the elasticity, and the interaction between the two forces then lowers elasticities slightly more than either alone.

overall employment using the procedure described in Ganong et al. (2021). The employment effects of the \$600 policy are measured from April-July 2020 while the employment effects of the \$300 policy are measured from January-March 2021.[24] The second set of results in Table 2 show that the \$600 supplement on average reduced employment by around 0.75% through disincentive effects on job search while the \$300 supplement reduced employment by 0.31-0.53%. These changes are relatively small when compared to either the decline of 15% observed during the start of the pandemic or the employment decline of 6.5% still remaining by March 2021. They are also substantially below the employment effects that would be implied by pre-pandemic estimates of disincentive effects. In particular, the pre-pandemic calibration of the model would have implied an employment decline of 4.5% in response to the \$600 and 2% in response to the \$300. While the \$600 supplement had a greater effect on employment, the last group of results shows that this was almost entirely driven by its larger size. Estimated disincentive effects per \$100 are fairly similar for the \$600 and the \$300 supplements.

Our finding that the disincentive effect of the \$600 supplement is small is consistent with several other papers. Dube (2021) estimates a macro effect using cross-state variation in replacement rates. Finamor and Scott (2021) and Petrosky-Nadeau and Valletta (2021) estimate a micro effect using cross-individual variation in replacement rates. Our empirical estimates are distinguished from these prior estimates in three ways: inclusion of both micro and macro disincentive estimates, a potential reduction in bias (because we observe actual UI receipt and actual UI benefit levels), and tighter statistical precision. We are not aware of any other estimates of the effect of the \$300 supplement. Finally, we use a structural framework to interpret and further bolster the credibility of the empirical conclusions that disincentive effects of the supplements thus far are small.

Why is the causal effect of benefits increases on exit rates during our time period so much smaller than estimates from prior studies? There are four classes of explanations. First, the fact that recalls make up a large share of exits during this time period implies that some workers may be waiting to be recalled to their *old* jobs, and so their search for *new* jobs may be less impacted by financial incentives. This force may be weaker while the \$300 supplement is in place than when the \$600 is in place because the recall rate is lower during this time period. Second, prior research finds that the distortion is likely to be smallest in a recession, perhaps because labor demand is low (Landais, Michaillat, and Saez, 2018; Mercan, Schoefer, and Sedláček, 2020; Kroft and Notowidigdo, 2016). Third, the pandemic may reduce job search above and beyond a normal recession, perhaps because it is difficult to search for a job during a public health emergency, or because employers who are recruiting may be doing so for positions with above-average health risk, or finally because school and daycare closures mean that some workers are unable to accept new jobs due to childcare responsibilities. Fourth, Chetty (2008) documents much smaller causal impacts of UI benefits on exit rates among benefit recipients who are not liquidity constrained. Because the \$600 supplement was large enough to bring nearly every recipient off their liquidity constraint by itself, and because on top of this most workers also received three rounds of tax refund payments, the job-finding response may be more similar to the response previously estimated for recipients who are not liquidity constrained. We do not attempt to distinguish between these four hypotheses in this note.

[24] These calculations require observed data on employment and unemployment over time, so we cannot yet extend calculations through September, 2021 for the \$300 supplement.

7 Conclusion

Expanded unemployment benefits mean many households have replacement rates above 100%, leading to natural concerns about disincentive effects. However, we estimate the causal effects of the \$600 on employment from April-July, 2020 and of the \$300 supplements from January-early March, 2021 and find they are small. While it is more challenging to identify *causal* effects further into the spring, we provide suggestive evidence that the disincentive effects of supplements likely remained small through the end of April, 2021 when our data currently ends.

This update leaves several questions unanswered which we hope to address in future research. First and most importantly, *why* were the disincentive effects in response to the largest expansion of unemployment insurance benefits in US history so much smaller than would have been predicted on the basis of estimates in the prior literature? While some of this effect is driven somewhat mechanically by the presence of a recall rate which is large relative to the new job finding rate and by the presence of unemployment spells which are long relative to supplement durations, the causal effects on the new job finding rate that we estimate are themselves well below what one would predict based on pre-pandemic estimates.

Second, given the quantitative importance of recalls, better understanding the interaction between firm recall decisions and unemployment benefits is also important. Third, it is important to know whether the low disincentive effects we estimate thus far will continue to hold as the labor market further tightens. As our data sample continues in time, there will potentially be scope to estimate additional causal effects on disincentives as various states end supplements early and when supplements likely expand for all remaining states in September. However, identification approaches based on this variation will need to contend carefully with non-random variation in expiration and thus concerns about confounding trends. We hope to make progress on this front in future work.

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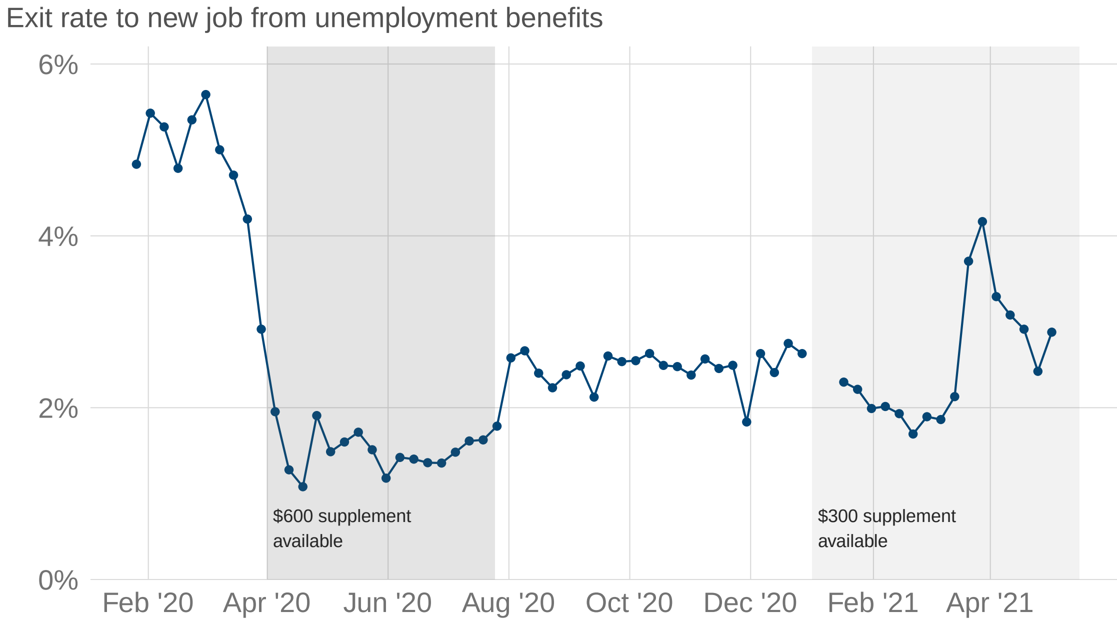
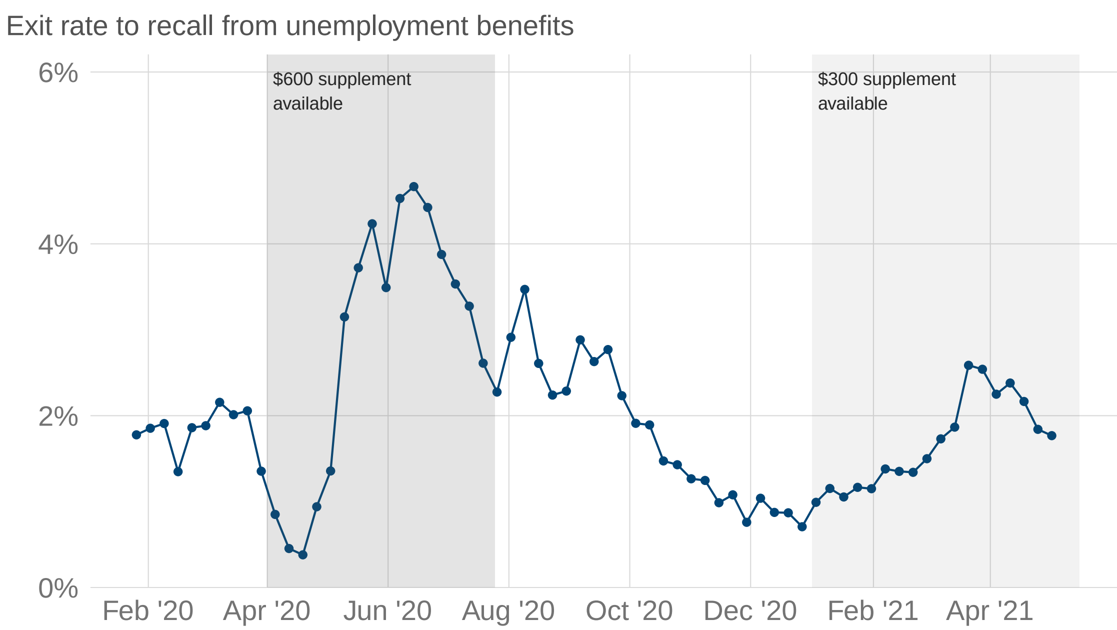
Figure 1: Exit Rate from Unemployment Benefits

(a) New Job



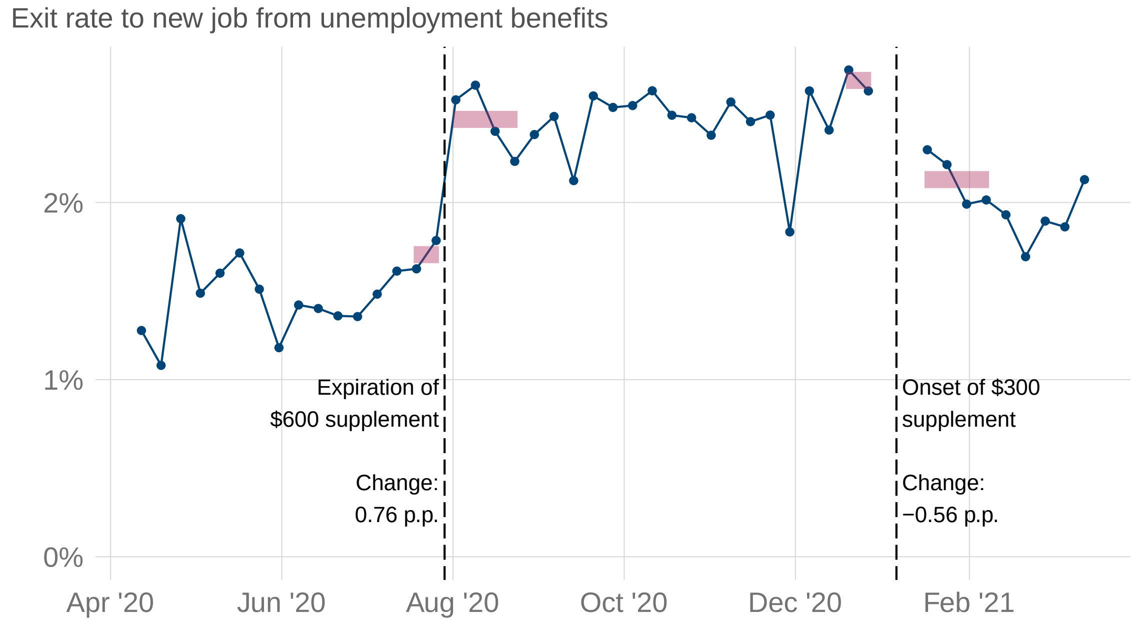
(b) Recall



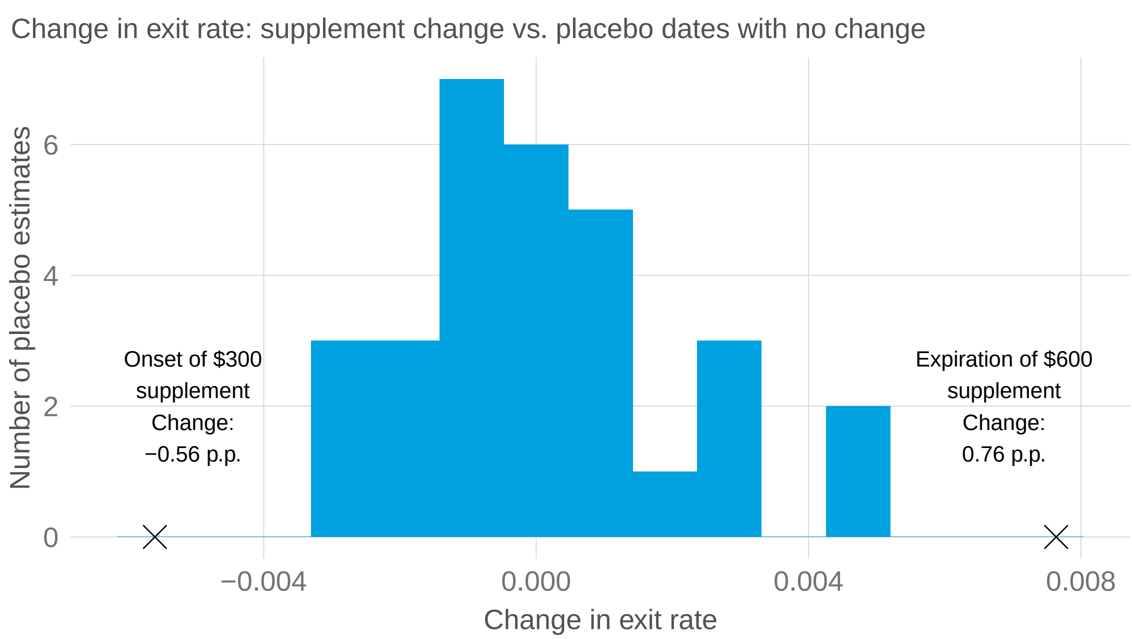
Notes: This figure shows the exit rate to new jobs and to recall in the JPMCI data from February 2020 to May 2021. UI exit is defined as three contiguous weeks without receipt of UI benefits. Recall is measured using receipt of labor income from a prior employer. New job is defined as a UI exit without a recall. There is a surge in exits on January 3 and 10, which reflect a lapse in federal benefits rather than true exit to new job (see Figure A-3a) and we therefore omit these weeks. The last week of November which has an unusually low job-finding rate is the week that contains Thanksgiving.

Figure 2: Effect of Expanded Benefits on Job-Finding: Interrupted Timeseries Design

(a) Interrupted Timeseries Estimate



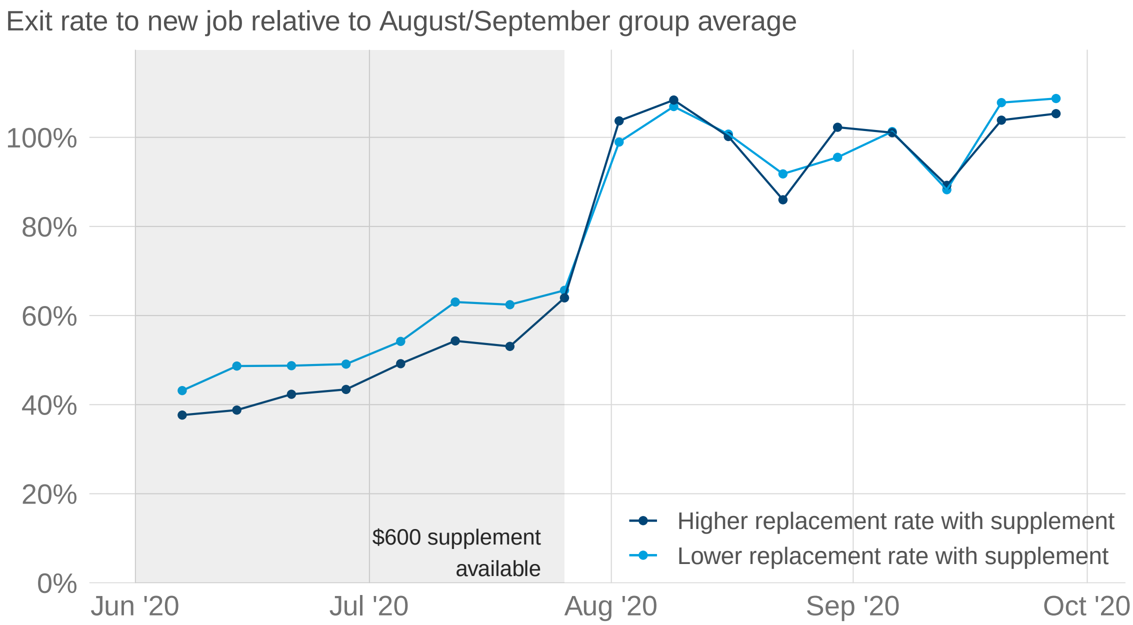
(b) Distribution of Placebo Estimates



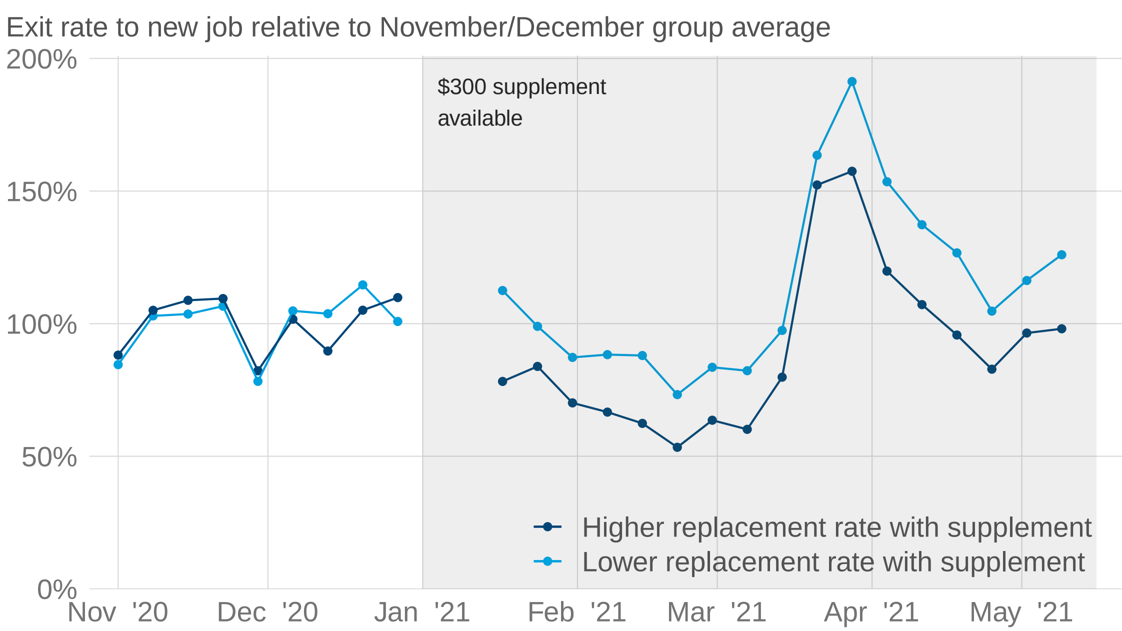
Notes: The top panel of this figure shows the exit rate to new job in the JPMCI data from April 2020 through February 2021. The red horizontal bars indicate the average exit rate in the two weeks prior to and four weeks following a change in the supplement amount. We form a test statistic for the impact of the supplement using the difference between the red horizontal bars. We omit January 3 and 10 because they show a mechanical surge in exits arising from a policy lapse. We recompute the test statistic for every placebo date shown in the top panel, where we define placebo windows as those with no policy change. The bottom panel of this figure shows the distribution of the test statistic using blue bars. The changes at the actual supplement changes are more extreme than the changes at any of the placebo dates. If we assume that the date of the supplement change is random, this implies that we reject the null hypothesis of no effect of the supplement with $p \leq 1/31$.

Figure 3: Effect of Expanded Benefits: Event Study

(a) Expiration of $600



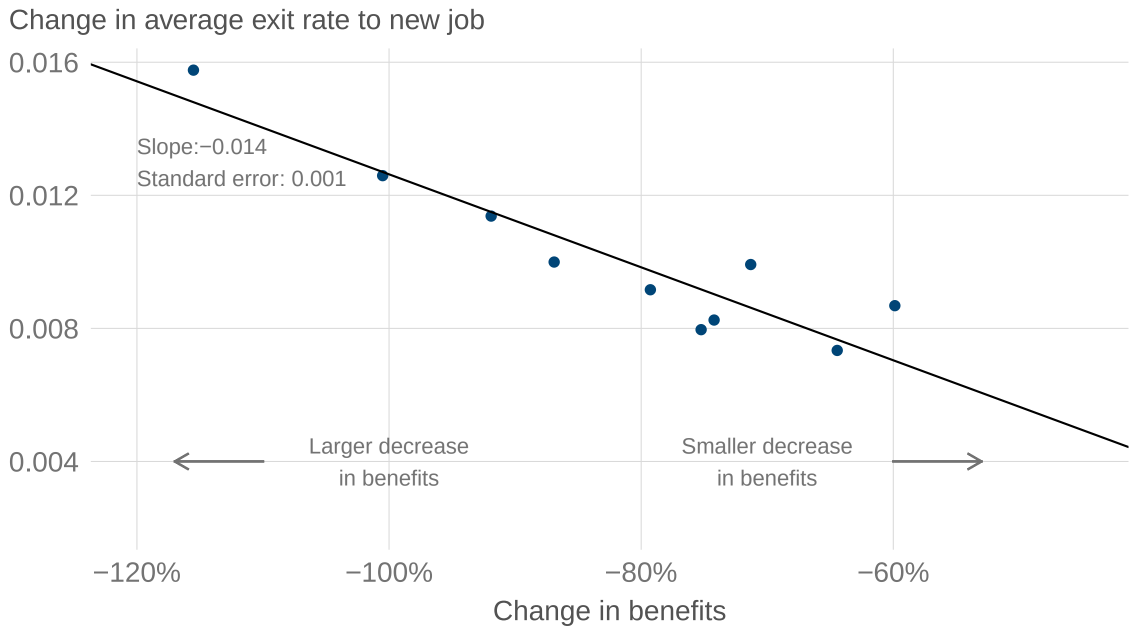
(b) Onset of $300



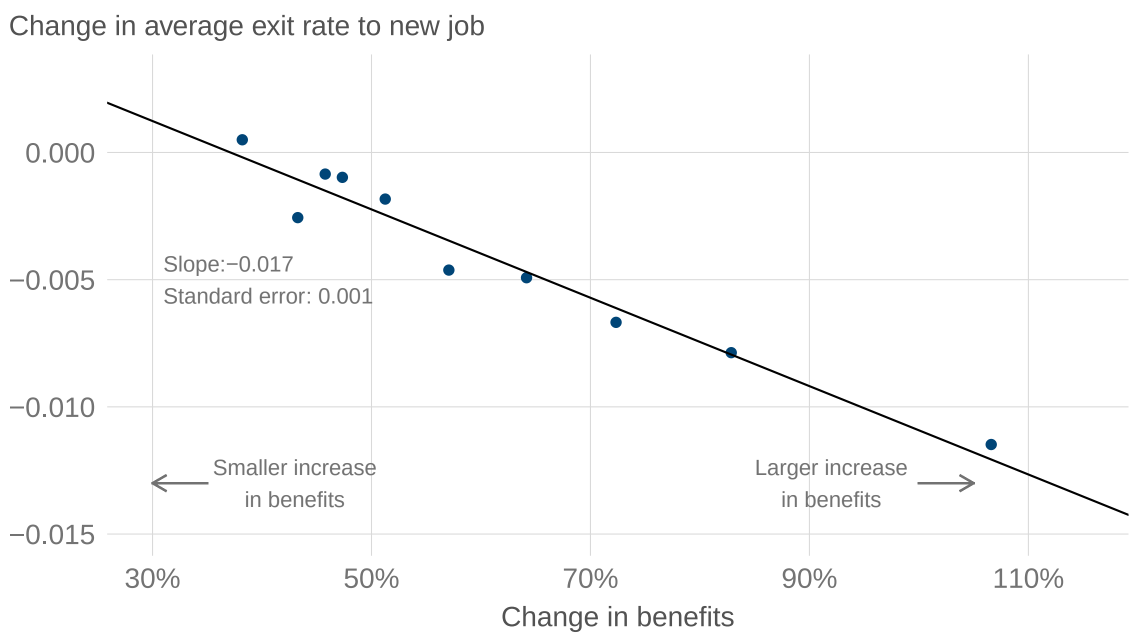
Notes: This figure shows the exit rate to new job around the expiration of the $600 weekly supplement and the onset of the $300 weekly supplement in the JPMCI data. The light blue line shows workers with a lower-than-median replacement rate with the supplement and the dark blue line shows workers with a higher-than-median replacement rate with the supplement. Exit rates are normalized by the average exit rate during the period without the supplement (August and September for the expiration of the $600 and November and December for the onset of the $300). Panel (b) omits a mechanical surge in exits on January 3 and 10. See Figure A-7 for a version without a normalization, Figure A-8 for standard errors on the difference in the exit rate between the two groups, Figure A-9 for the total exit rate and Figure A-11 for exit to recall. See Section 4.2.1 for details.

Figure 4: Effect of Expanded Benefits: Difference-in-Difference Binscatter

(a) Expiration of $600



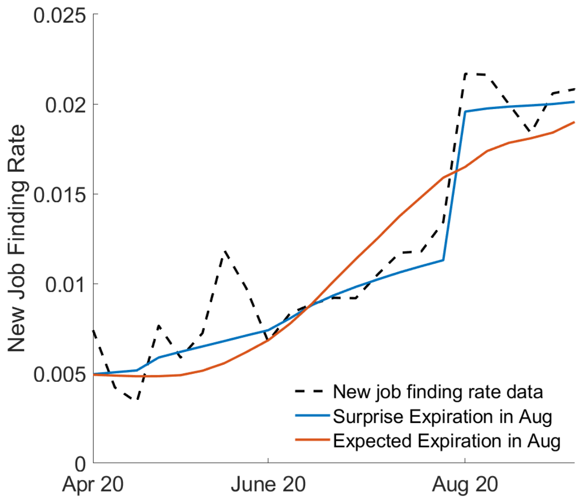
(b) Onset of $300



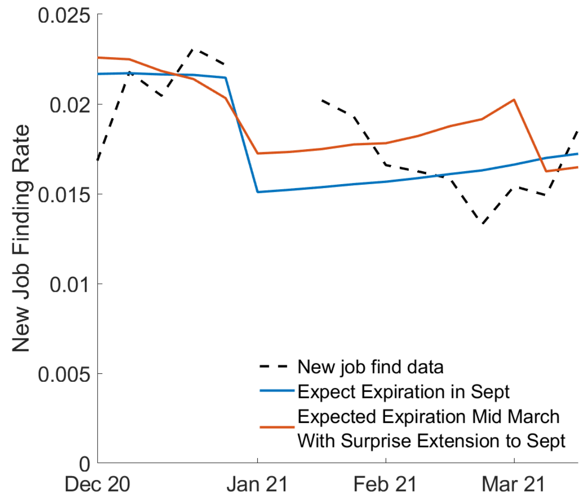
Notes: This figure shows the change in the new job-finding rate at the expiration and onset of benefit supplements separately for deciles of the change in benefits as measured using equation 4. The top panel shows the difference in the average new job-finding rate between Jun 1-Jul 31 and Aug 1-Sep 31. It shows that a larger decrease in benefits at expiration of the $600 is associated with a larger increase in the job-finding rate. The bottom panel shows the difference in the average new job-finding rate between Nov 1-Dec 31 and Jan 15-Mar 15. that a larger increase in benefit at the onset of the $300 is associated with a smaller increase in the job-finding rate. The slope estimates correspond to the $\hat{\beta}$ coefficients reported in Table A-1.

Figure 5: Model: Role of Expectations

(a) Expiration of $600



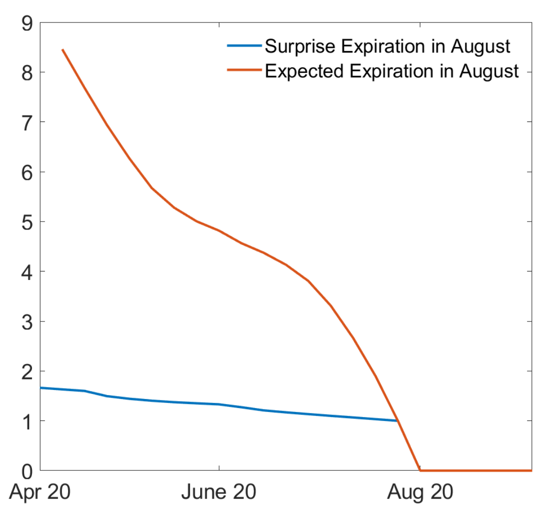
(b) Onset of $300



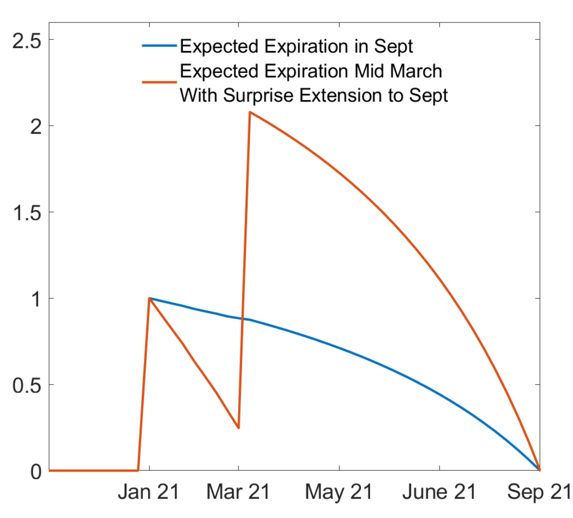
Notes: This figure compares model new job finding rates under different expectations about supplement duration to the times-series of the new job finding rate in the data. The panel simulating the expiration of the $600 compares a model where the realized expiration of supplements in August was a surprise to one where it was expected. The panel simulating the $300 supplement compares a model where households anticipate that supplements will last until September to one where they initially expect supplements to last until March and are then surprised when supplements are extended until September.

Figure 6: Dynamic Effects of Supplements Under Different Expectations

(a) Job Finding Effect of $600 Supplement Each Week Relative to Effect in Final Week



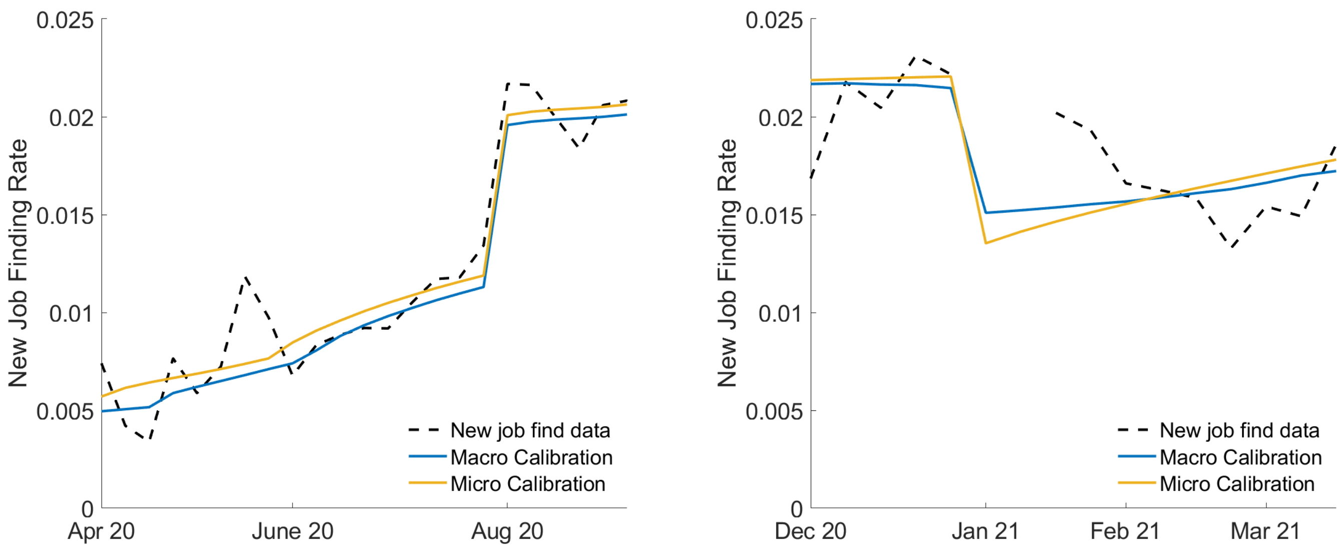
(b) Job Finding Effect of $300 Supplement Each Week Relative to Effect in Week of Onset



Notes: This figure shows the time-series of the effect of the supplement on job finding in a given week divided by the effect of the supplement in the week of the policy change. When this ratio is equal to one, the effect of the supplement on job finding in a given week is the same as the effect in the week when the policy changes.

Figure 7: Targeting Micro vs. Macro Estimates Delivers Similar Results

(a) Expiration of $600 (b) Onset of $300



Notes: This figure shows time-series implications for the models targeting the interrupted time-series evidence (macro calibration) and the difference-in-difference evidence (micro calibration) have very similar aggregate time-series implications. In that sense, there is little tension in the model between matching micro and macro facts.

Table 1: Macro and Micro Disincentive Effects of Expanded Benefits on Job-Finding

	Macro effects		Micro effects	
Effect of...	Entire supplement	per $100	Entire supplement	per $100
$600	-0.76	-0.18	-1.1	-0.26
$300	-0.56	-0.21	-0.98	-0.38

Notes: This table compares the macro and micro effects of unemployment benefit supplements on the new job-finding rate. The first row uses estimates from the expiration of the $600 and the second row uses estimates from the onset of the $300. The macro estimates use an interrupted timeseries design and the micro estimates use a differences-in-differences design. Because we are comparing supplement increases and decreases, both of which are very large in size, we use a symmetric percent change calculation (see equation 4). We also compute the effect of increasing benefits by $100 relative to a baseline with no supplement. See Section 4.3 for details on how we convert estimates of the effect of the entire supplement to an effect of a $100 supplement.

Table 2: Disincentive Magnitudes

	Macro Calibration		Micro Calibration	
	$600	$300	$600	$300
	(1)	(2)	(3)	(4)
Duration Elasticity (structural model)	0.10	0.11	0.09	0.09
Duration Elasticity (statistical model)	0.07	0.10	0.10	0.18
Employment Loss (structural model, %)	0.77	0.34	0.74	0.48
Employment Loss (statistical model, %)	0.54	0.31	0.79	0.53
Employment Loss (structural model, % per $100)	0.13	0.11	0.12	0.16
Employment Loss (statistical model, % per $100)	0.09	0.10	0.13	0.18

Notes: This table reports the magnitude of disincentive effects of supplements on unemployment durations and employment levels. The macro effects calibration targets the empirical interrupted time-series results while the micro effects calibrations target the difference-in-difference results. The structural model effects convert the dynamic model effects of benefit supplements in the model into effects on average unemployment durations. The statistical model estimates perform the same calculation but using the constant effect on job finding estimated in Table 1, as described in more detail in the text. The statistical estimates for the macro calibration use the coefficients from the interrupted time-series regressions in Table 1, which that model is calibrated to match. The statistical estimates reported for the micro calibration use the coefficients from the difference in difference regression in Table 1, which that model is calibrated to match. Total employment effects convert changes in job search into % declines in employment as in Ganong et al. (2021) and effects per $100 divide the $600 effects by 6 and the $300 effects by 3. Employment effects for the $600 supplement are calculated from April through July 2020 and employment effects for the $300 supplement are calculated January through mid-March 2021.

Table 3: Pre-Pandemic Comparison

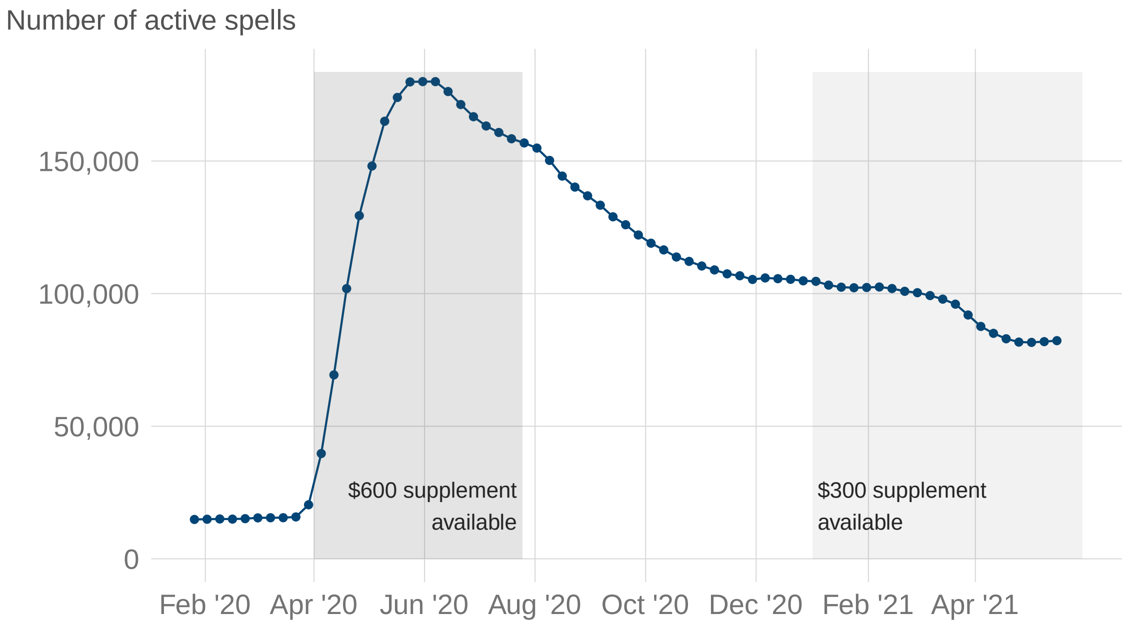
	Pre-Pandemic Comparison (1)	Macro Calibration (2)	Micro Calibration (3)
Duration elasticity to small 26 week supplement	0.47	0.05	0.04

Notes: This table reports the duration response to a small 26 week supplement. It does so both for a pre-pandemic calibration as in Ganong et al. (2021), the macro calibration which targets interrupted time-series evidence and the micro calibration which targets cross-section difference-in-difference evidence. The micro and macro responses to these small 26 week supplements are similar for the $600 and $300 micro and macro calibrations, so we report numbers just for the calibrations based on targeting $600 empirical moments.

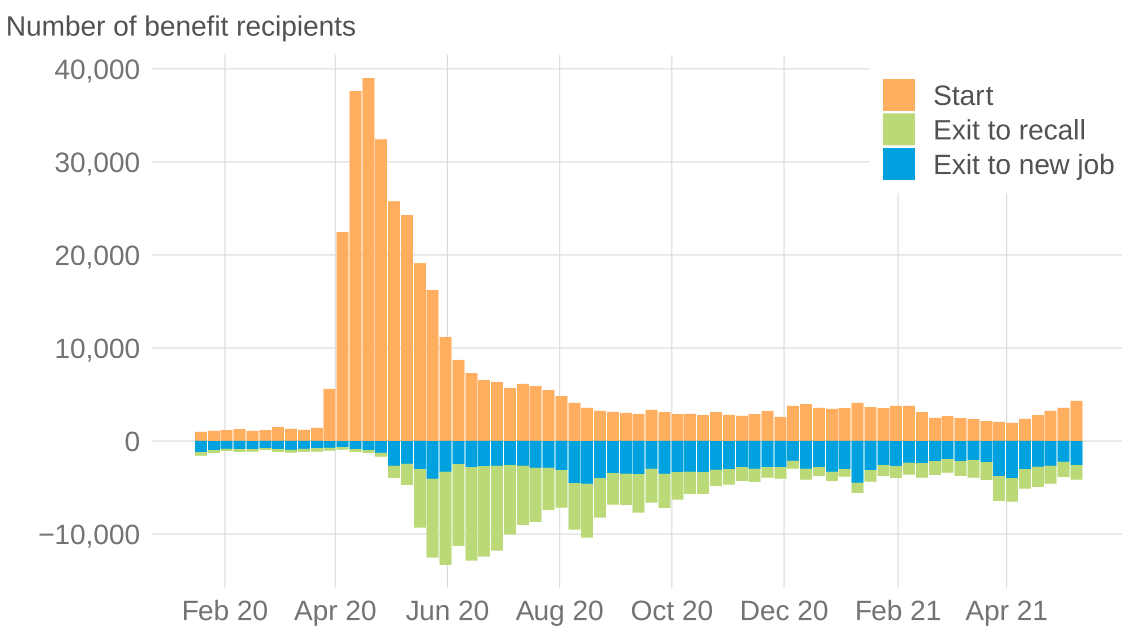
A Appendix

Figure A-1: Patterns of Unemployment Insurance Receipt

(a) Number of Recipients

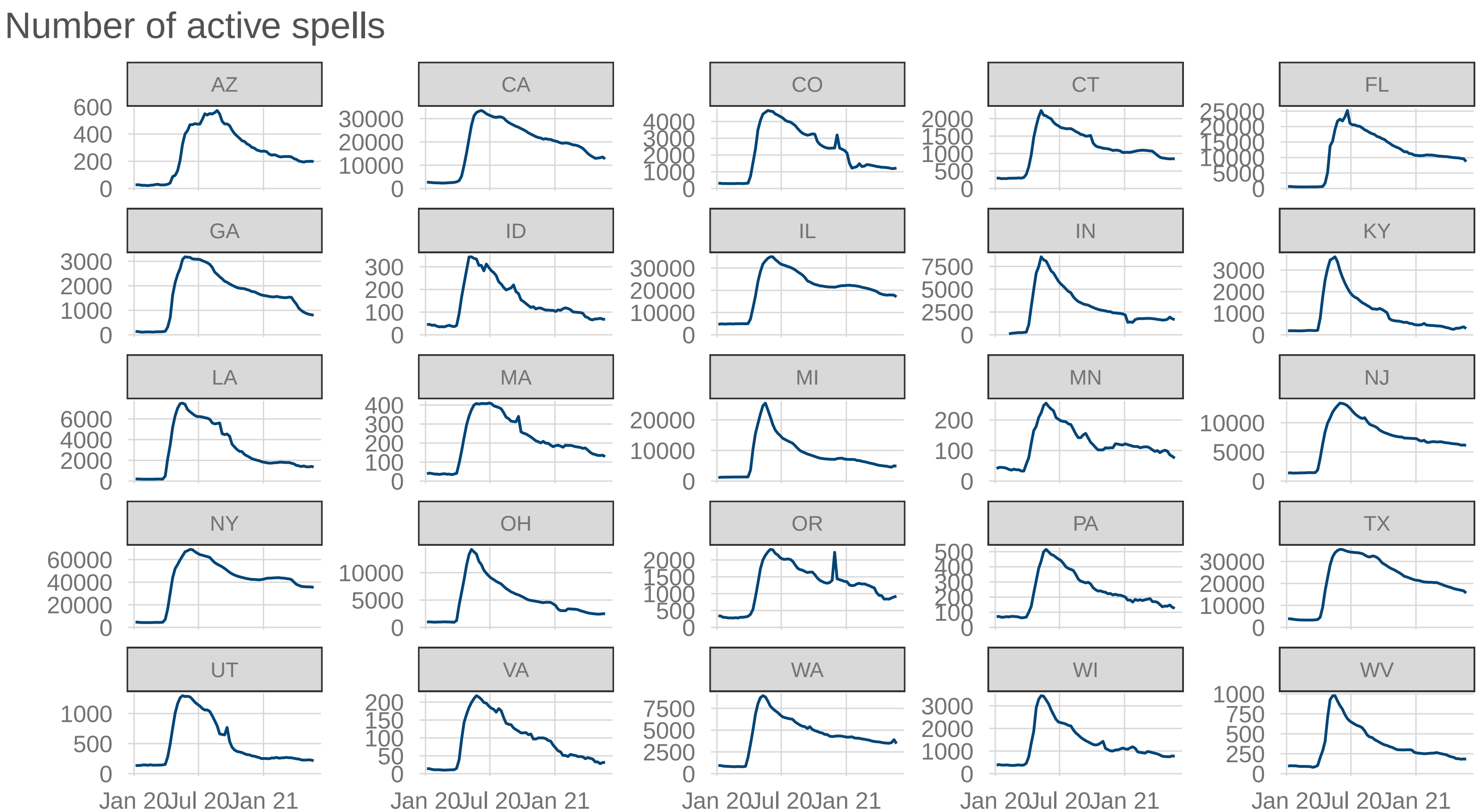


(b) Number of Starts and Exits



Notes: This figure shows the number of unemployment insurance recipients, the number of starts, the number of exits to recall, and the number of exits to a new job in the JPMCI data.

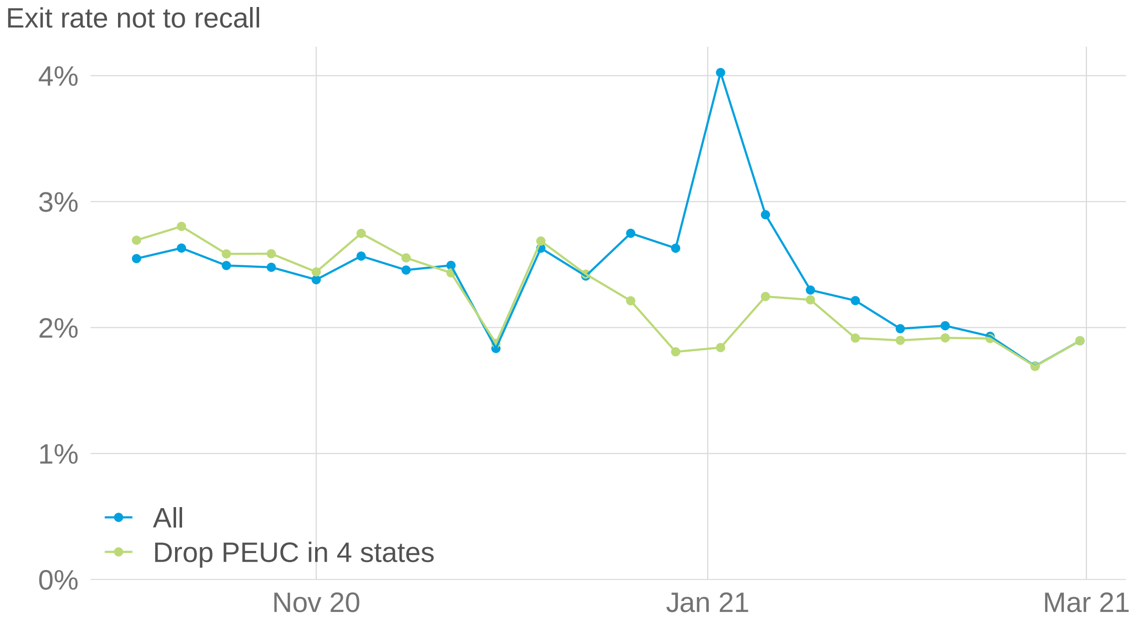
Figure A-2: Number of Active Spells by State



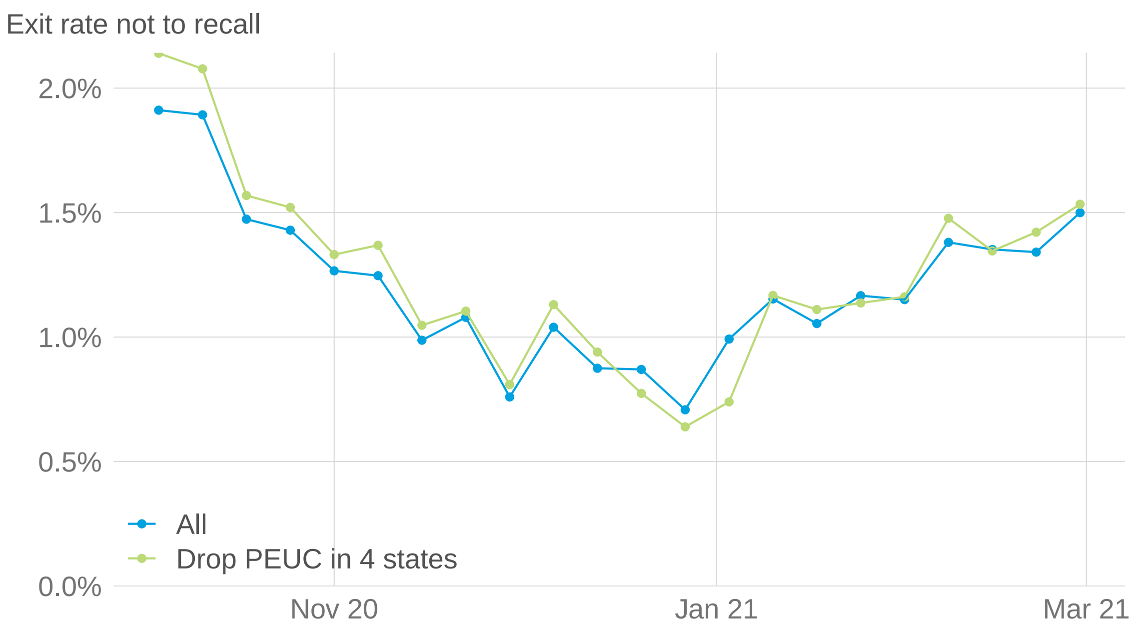
Notes: This figure shows the number of active spells in the JPMCI data.

Figure A-3: Exit Rate at Expiration of PEUC

(a) Not to Recall

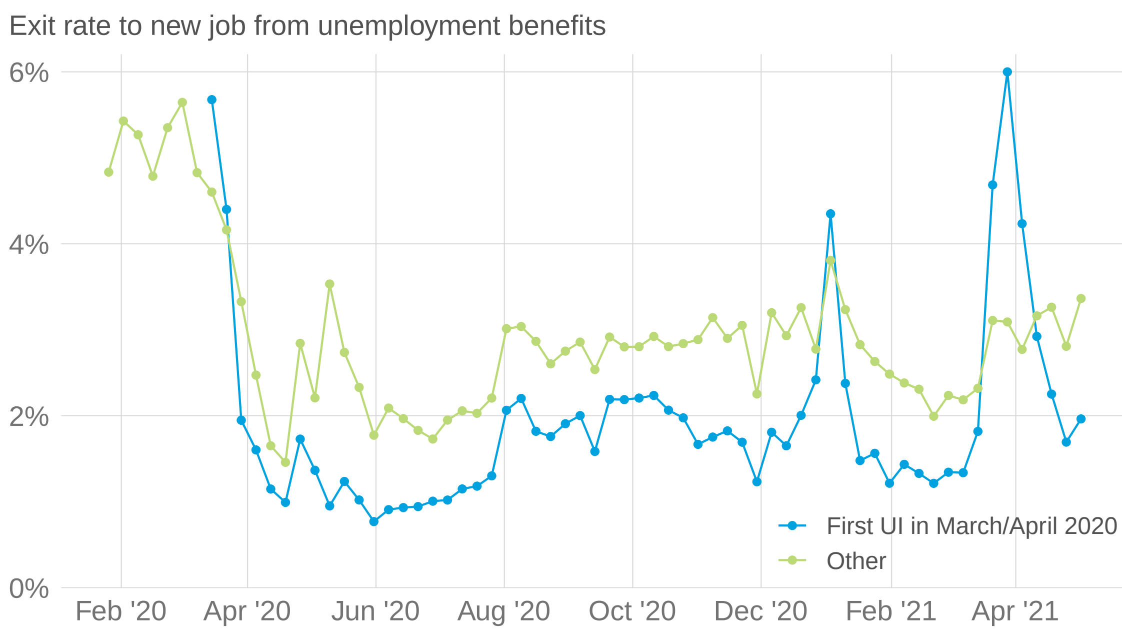


(b) To Recall



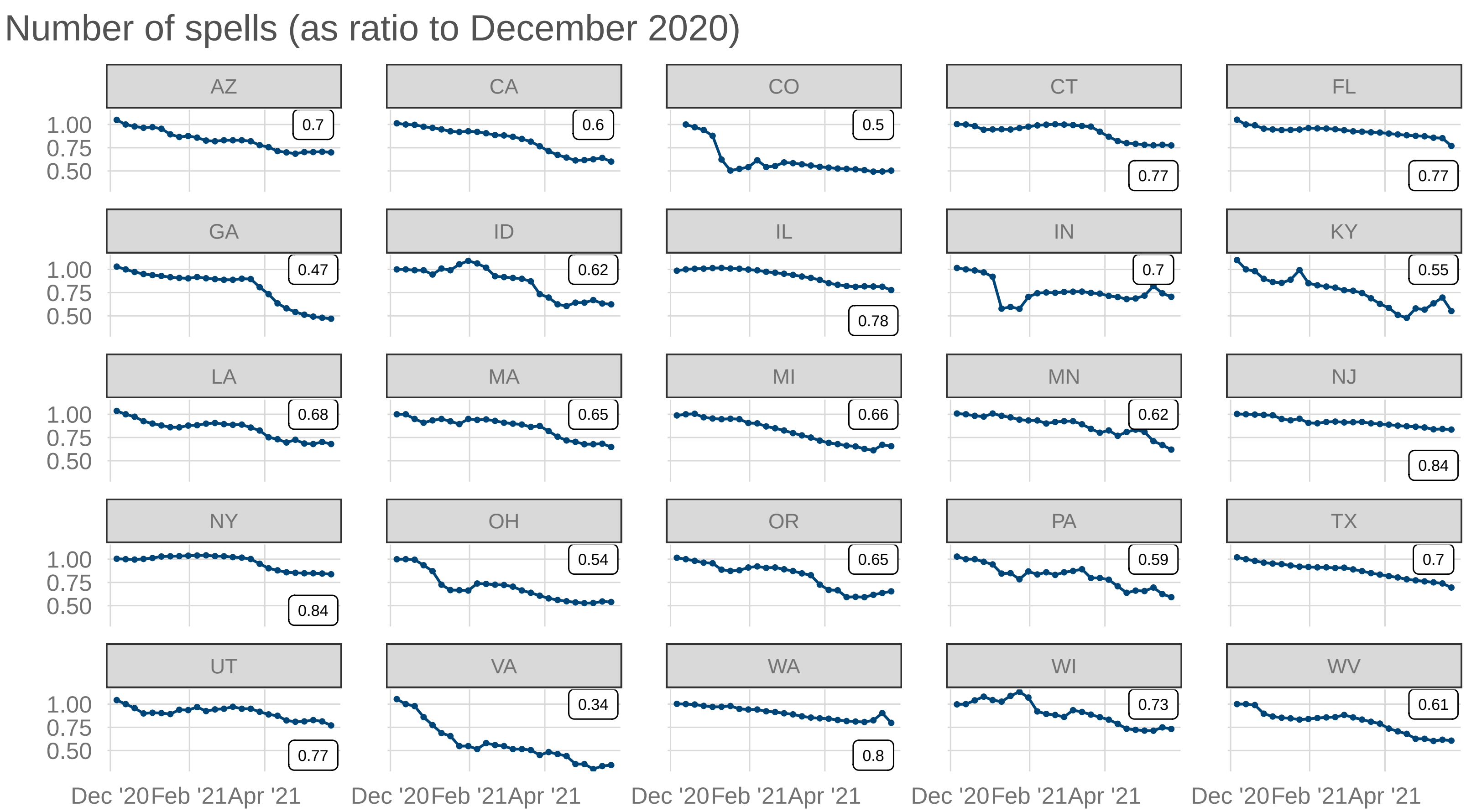
Notes: This figure shows the evolution of the exit rate from October 2020 through February 2021. The top panel shows exit not to recall and the bottom panel shows exit to recall. The blue series is the same as the one shown in Figures 1a and 1b, except that here the series includes January 3 and January 10. In the top panel, we refer to this as the "exit rate not to recall" instead of the "exit rate to new jobs" because some of the exits arise from a policy seam. The green series drops the 71,000 households that have received at least 20 weeks of benefits in 2019 and 2020 in Indiana, California, New Jersey, and Ohio.
These households are likely to be recipients of Pandemic Emergency Unemployment Compensation, which temporarily lapsed at the end of December and these four states were slow to restore benefits after the lapse. The lapse triggered a surge in *measured* exits from benefit receipt that were not accompanied by evidence of starting a new job via direct deposit of payroll from a new employer. We therefore omit them from the plot in Figure 1a.

Figure A-4: Exit Rate by Start Date of Receipt



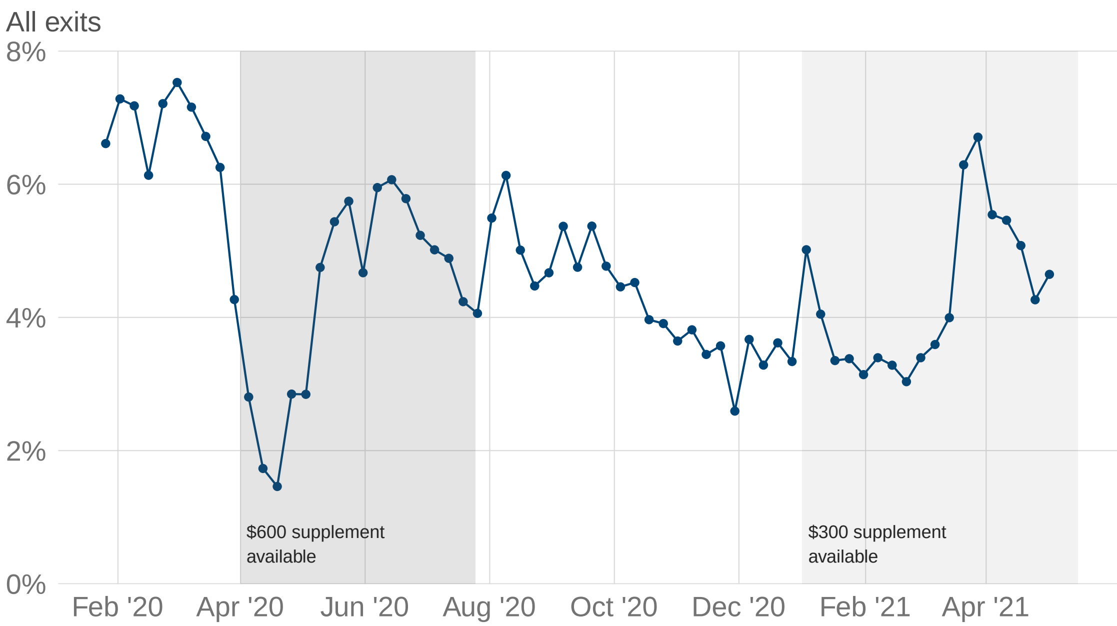
Notes: This figure plots non-recall exit rates separately by the month of initial UI receipt. We see a sharp jump in March and April 2021 for workers who started receiving benefits in March and April 2020 and who were therefore likely near the end of their benefit year. Because the JPMCI data also shows that these workers were just as likely to receive payroll income from a new employer after exiting, we continue to refer to these exits as ”exits to new jobs”.

Figure A-5: Number of Active Spells by State Since December 2020



Notes: This figure shows the change in the number of active spells since December 2020 in the JPMCI data.

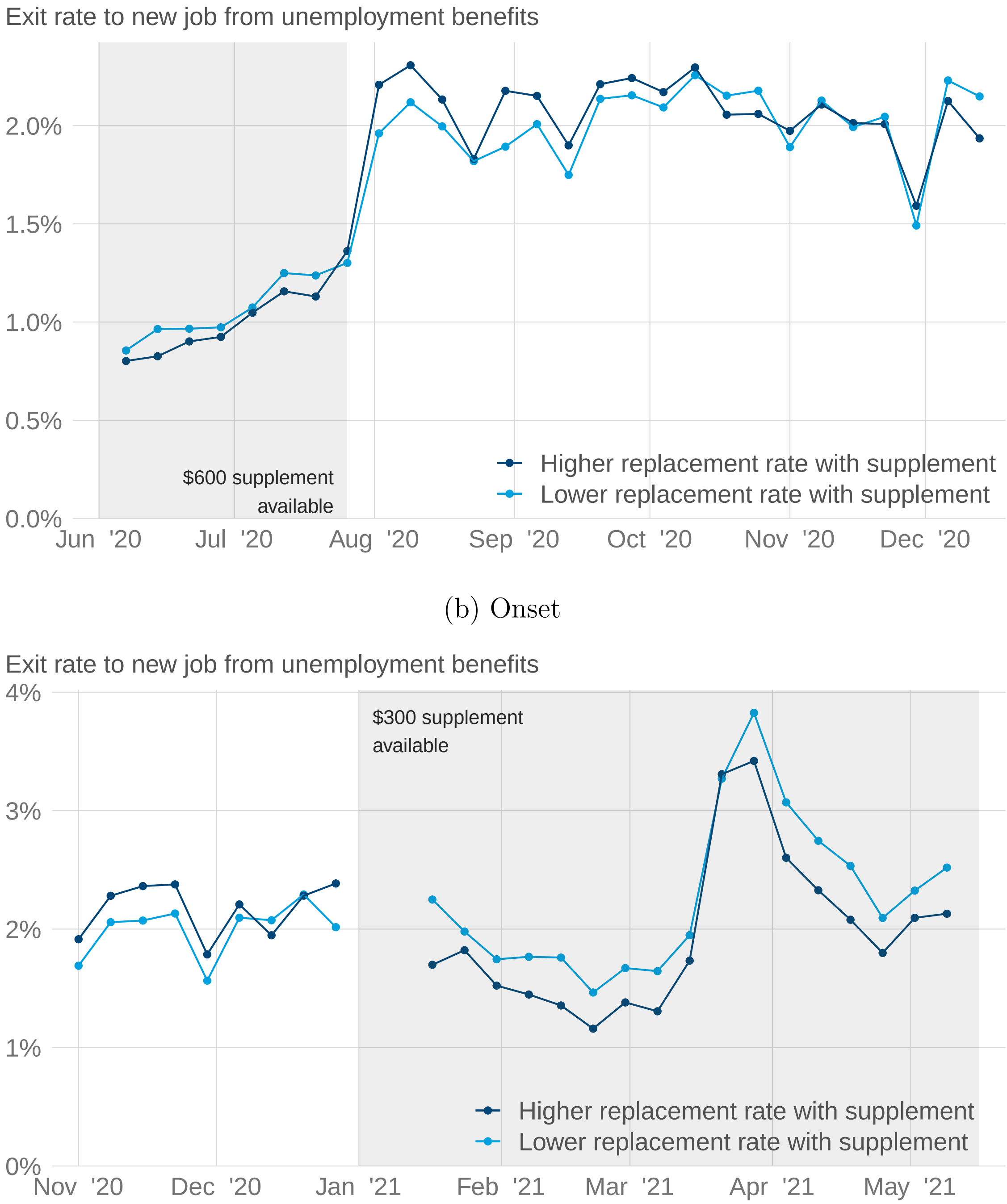
Figure A-6: Total Exit Rate from Unemployment Benefits



Notes: This figure shows the total exit rate from unemployment benefits, summing over exits not to recall shown in Figure 1a and exits to recall shown in Figure 1b.

Figure A-7: Exit to New Job by Change in Benefits

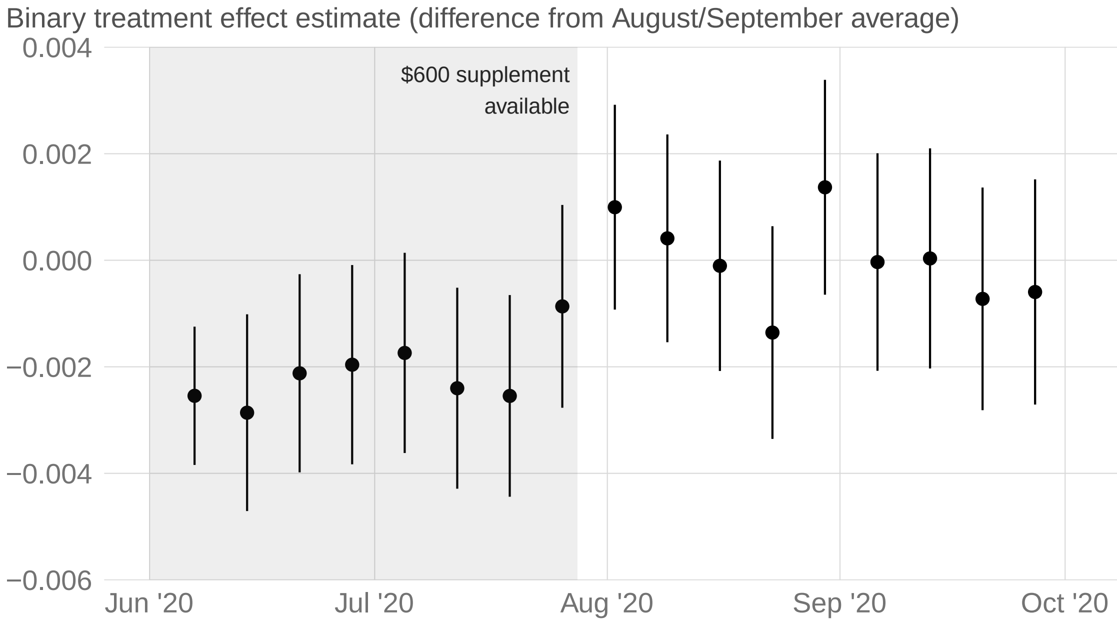
(a) Expiration



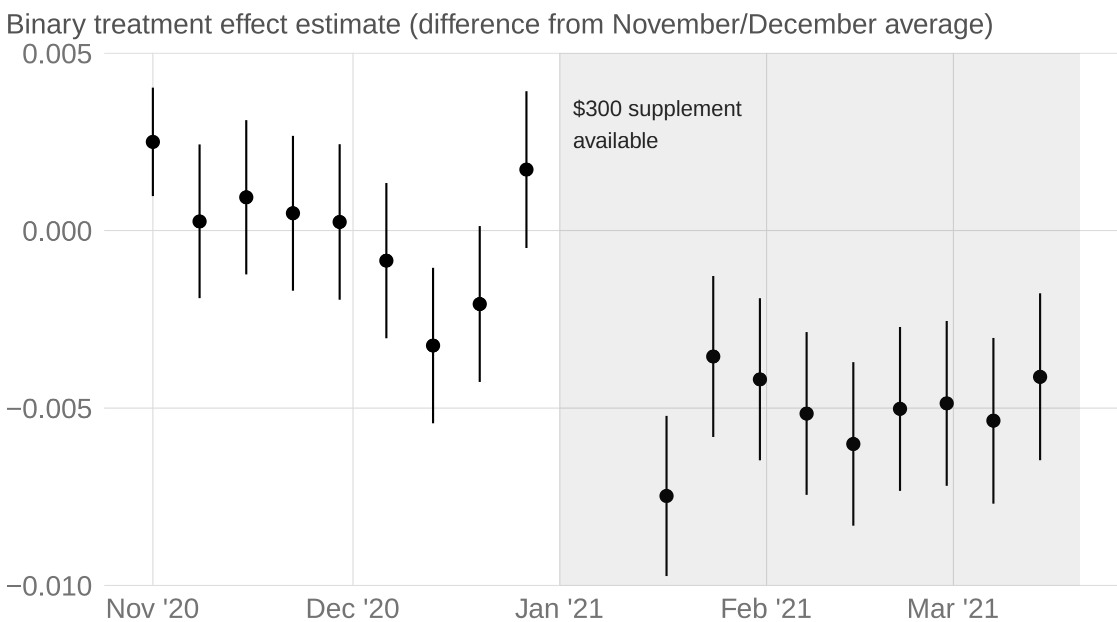
Notes: This figure shows the exit rate to new jobs around the expiration of the $600 weekly supplement and the onset of the $300 weekly supplement in the JPMCI data. The light blue line shows workers with a lower-than-median replacement rate with the supplement and the dark blue line shows workers with a higher-than-median replacement rate with the supplement.

Figure A-8: Weekly Event Study Coefficients (Binary Specification)

(a) Expiration of \$600



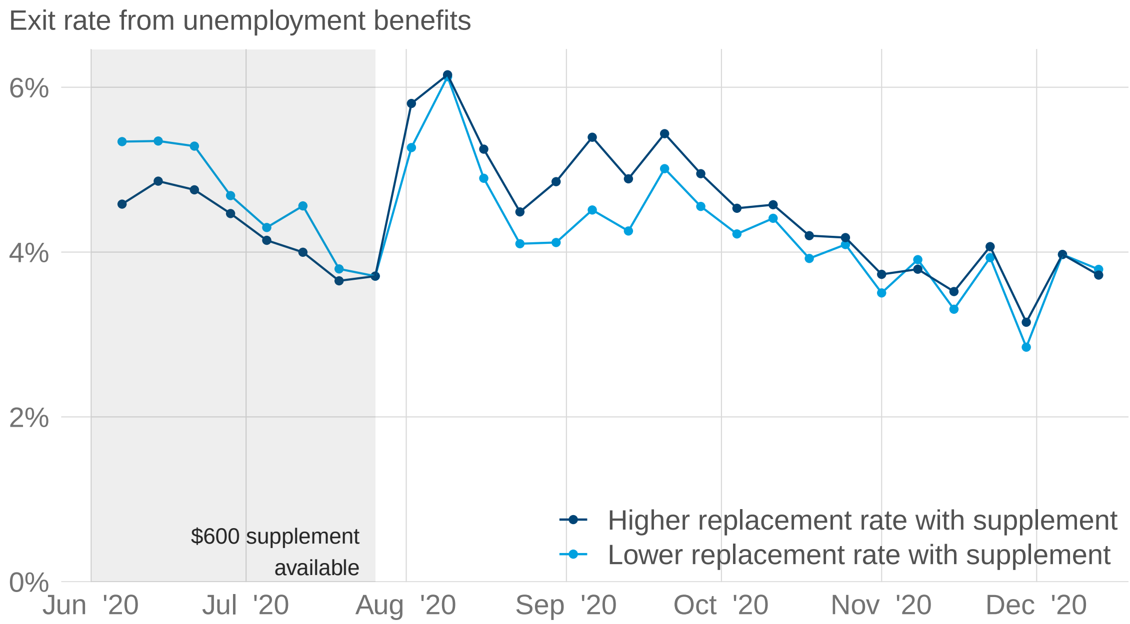
(b) Onset of \$300



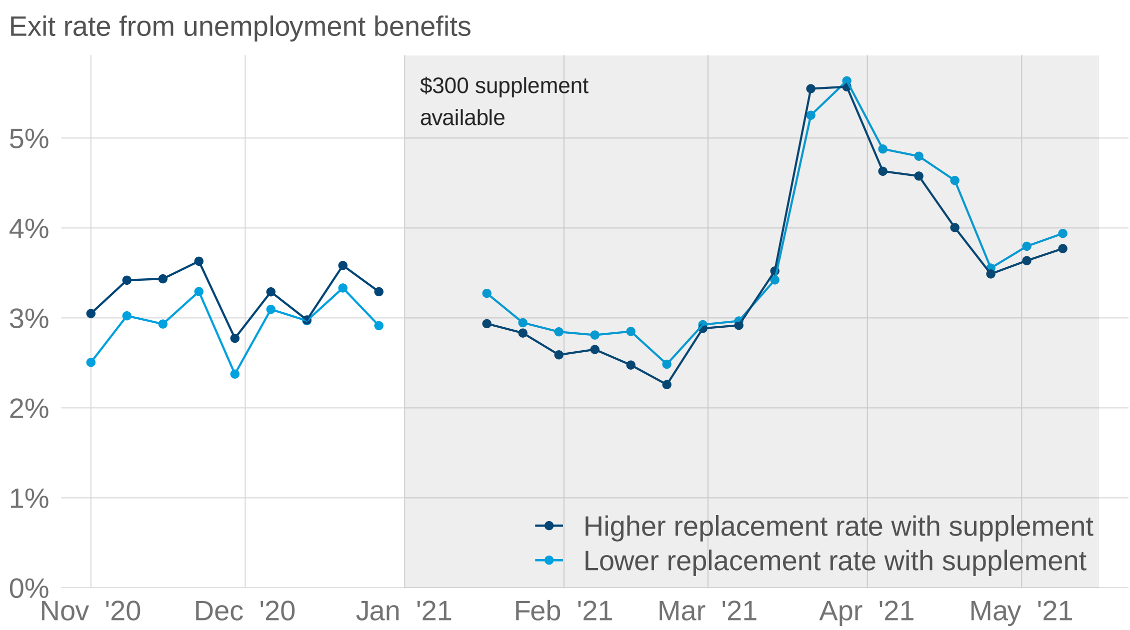
Notes: This figure reports estimates of $\hat{\beta}_t$ from $e_{it} = \gamma 1(PctChange_i > Median) + \alpha Week_t + \beta_t Week_t \times 1(PctChange_i > Median) + \varepsilon_{it}$. All coefficients are reported as differences to the average value of $\hat{\beta}_t$ in the no-supplement period. Vertical lines are 95% confidence intervals.

Figure A-9: Total Exit Rate by Change in Benefits

(a) Expiration of $600



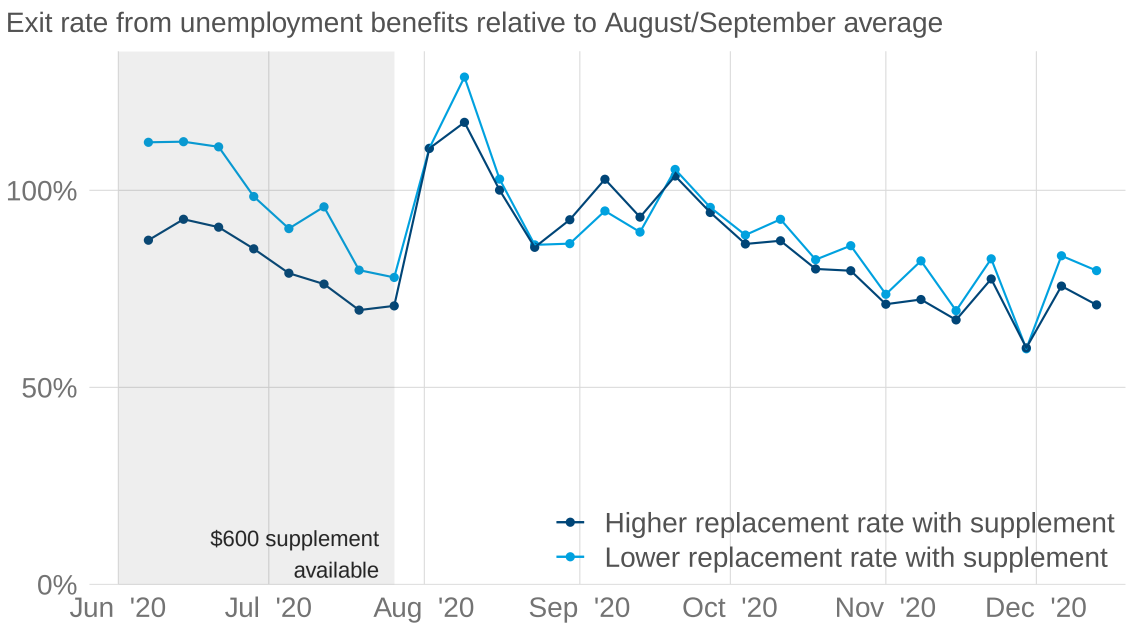
(b) Onset of $300



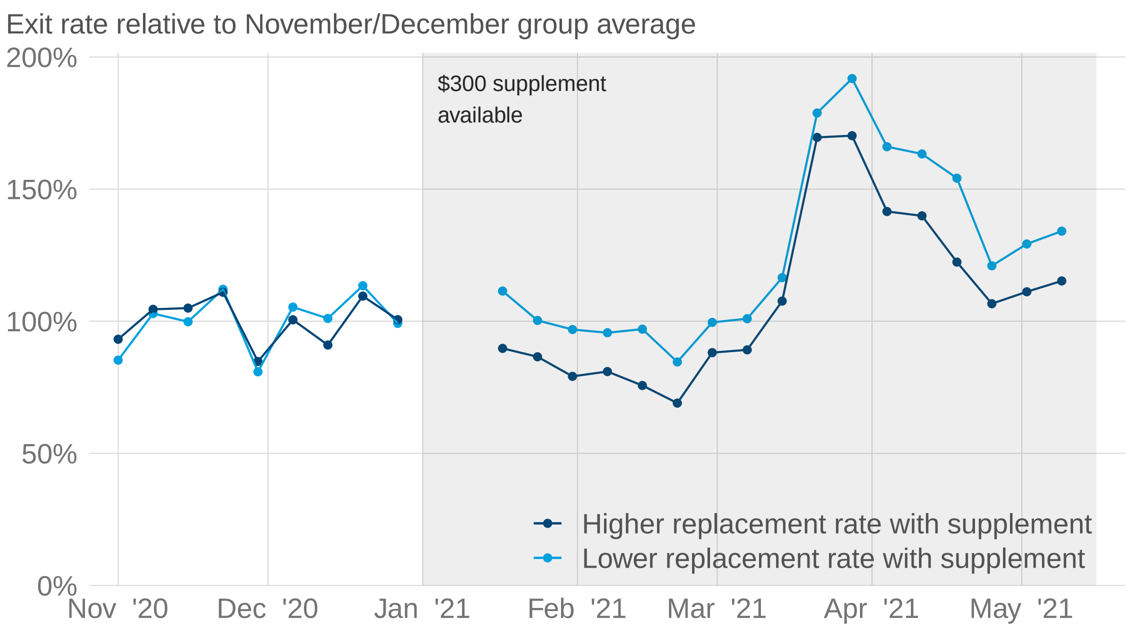
Notes: This figure shows the total exit rate (to recalls and new jobs) around the expiration of the $600 weekly supplement and the onset of the $300 weekly supplement in the JPMCI data. The light blue line shows workers with a lower-than-median replacement rate with the supplement and the dark blue line shows workers with a higher-than-median replacement rate with the supplement. See Figure A-10 for a version of the figure which is normalized by the job-finding rate in the no-supplement period.

Figure A-10: Total Exit Rate by Change in Benefits, Normalized by No-Supplement Period

(a) Expiration of $600



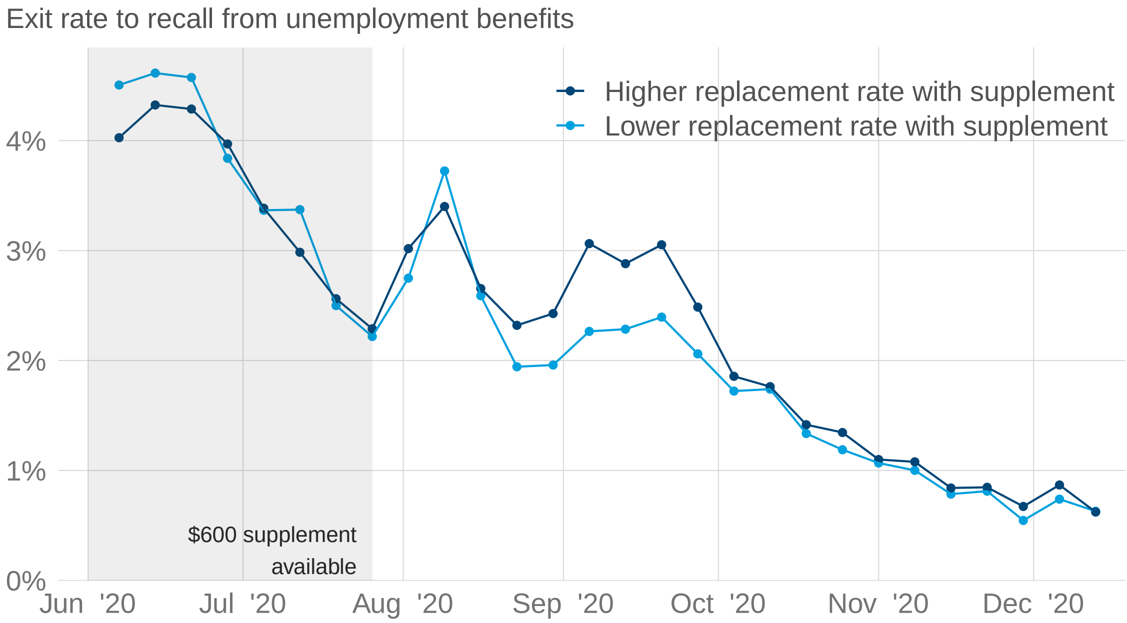
(b) Onset of $300



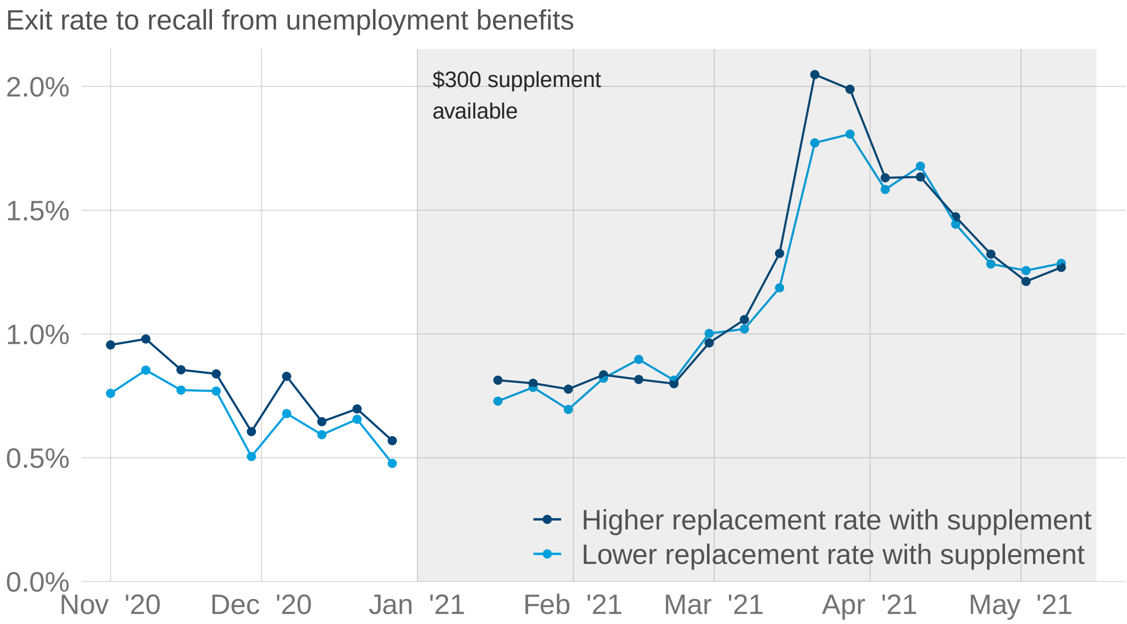
Notes: This figure shows the total exit rate (to recalls and new jobs) around the expiration of the $600 weekly supplement and the onset of the $300 weekly supplement in the JPMCI data. The light blue line shows workers with a lower-than-median replacement rate with the supplement and the dark blue line shows workers with a higher-than-median replacement rate with the supplement.

Figure A-11: Exit to Recall by Change in Benefits

(a) Expiration



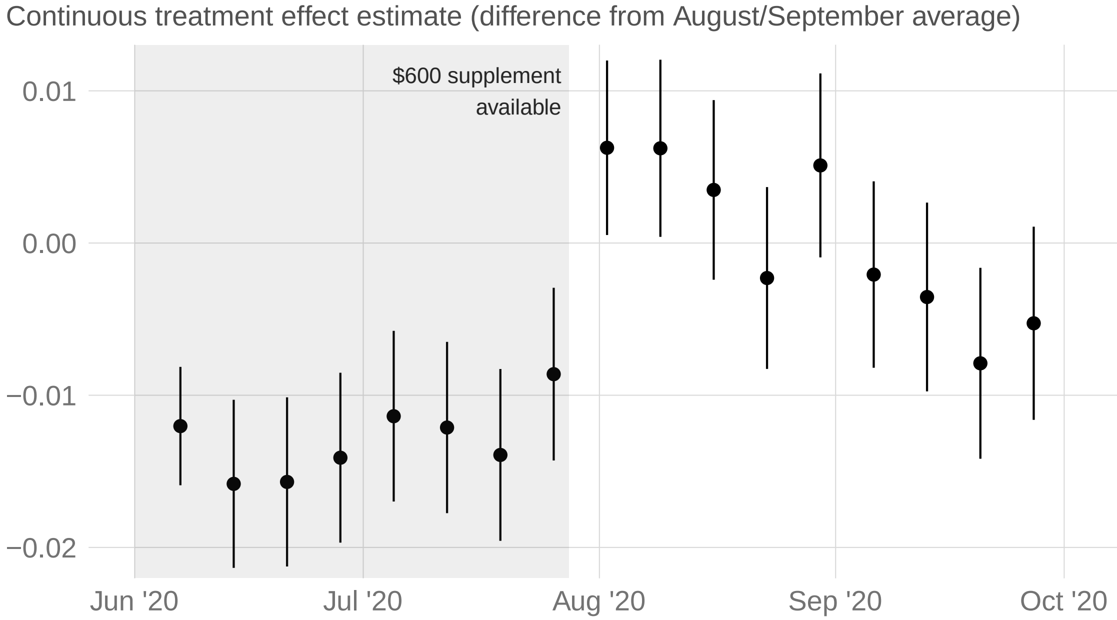
(b) Onset



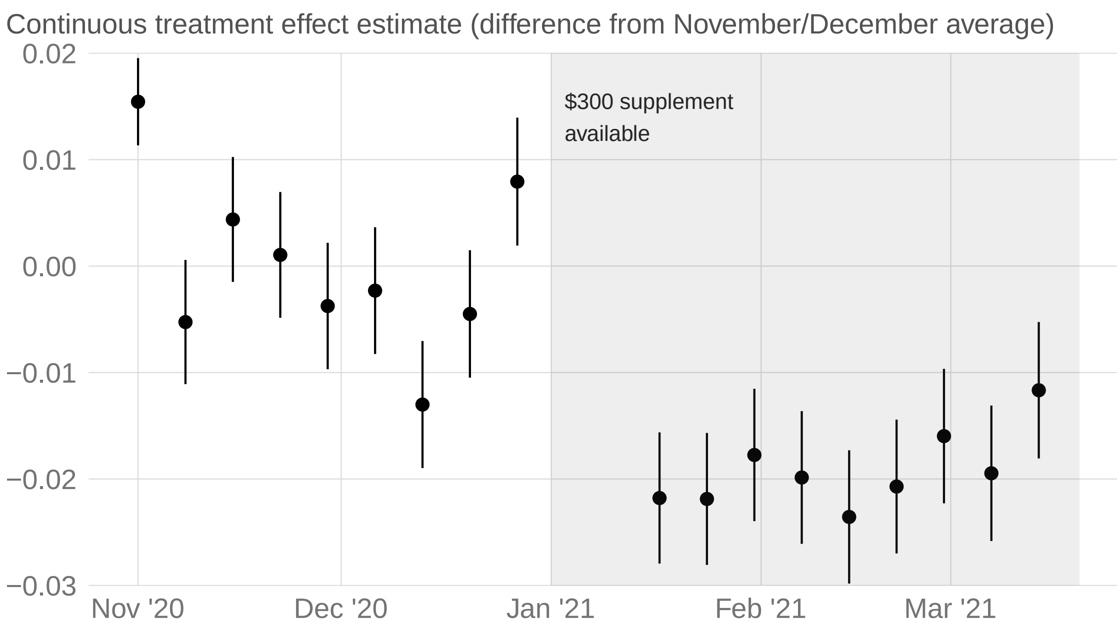
Notes: This figure shows the exit rate to recalls around the expiration of the \$600 weekly supplement and the onset of the \$300 weekly supplement in the JPMCI data. The light blue line shows workers with a lower-than-median replacement rate with the supplement and the dark blue line shows workers with a higher-than-median replacement rate with the supplement.

Figure A-12: Weekly Event Study Coefficients (Continuous Specification)

(a) Expiration of $600



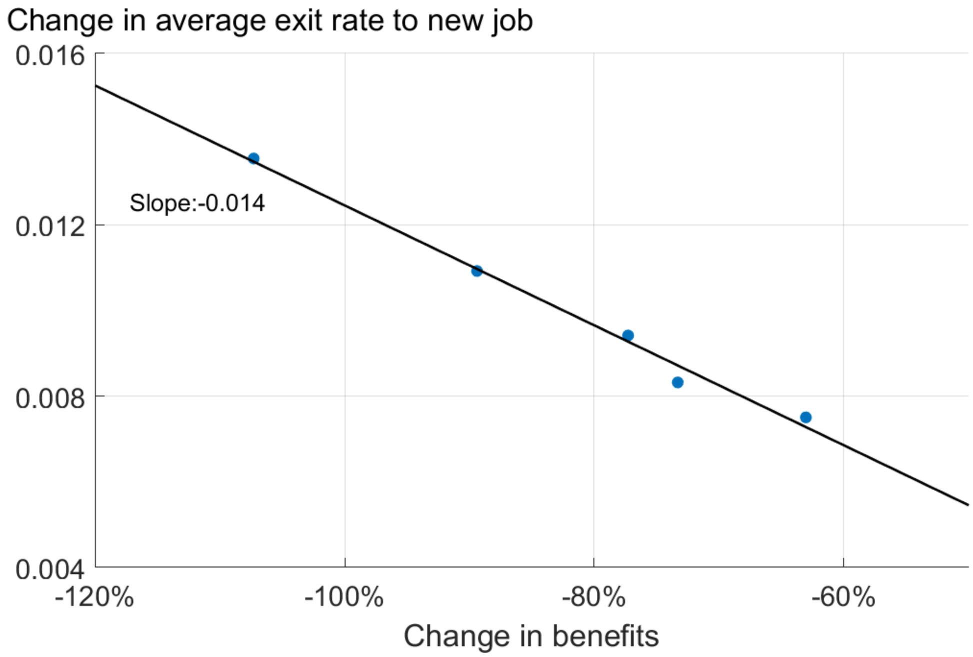
(b) Onset of $300



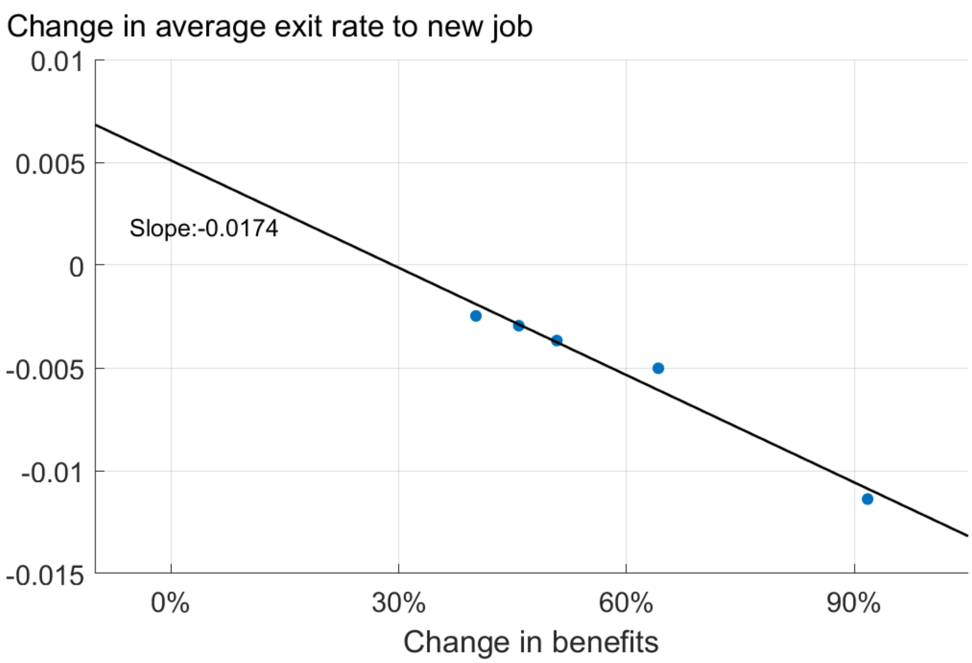
Notes: This figure reports estimates of $\hat{\beta}_t$ from equation (6). Coefficients on the plot are reported as differences to the average value of $\hat{\beta}_t$ in the no-supplement period. Vertical lines are 95% confidence intervals.

Figure A-13: Model Cross-Sectional Relationships

(a) Expiration of $600



(b) Onset of $300



Notes: This figure uses the model with heterogeneity to test the linearity assumption underlying the empirical micro regressions. It shows the change in the job-finding rate at the expiration and onset of benefit supplements in the model by quintiles of the individual change in benefits as measured using equation 4. The models are calibrated to exactly match the empirical $\hat{\beta}$ coefficients reported in Table A-1. The figure shows that the model calibrated to match this slope indeed produces effects which are approximately linear.

Table A-1: Regression Estimates for Effect of Expanded Benefits on Job-Finding

	Dependent variable:	
	Exit to new job	
	Expiration of $600	Onset of $300
	(1)	(2)
PctChange	0.017***	0.017***
	(0.001)	(0.001)
SuppAvail	0.001	0.006***
	(0.001)	(0.001)
PctChange:SuppAvail	−0.014***	−0.017***
	(0.001)	(0.001)
Constant	0.007***	0.011***
	(0.001)	(0.0004)
Observations	2,068,302	1,930,754

*p<0.1; **p<0.05; ***p<0.01

Notes: This table estimates the difference-in-difference model $e_{it} = \gamma PctChange_i + \alpha SuppAvail_t + \beta SuppAvail_t \times PctChange_i + \varepsilon_{it}$ from equation 5 using a window of two months prior to and after the two policy changes (expiration of the $600 supplement and onset of the $300 supplement). For expiration, the supplement available period is June and July 2020 and the no-supplement period is August and September 2020. For onset, the supplement available period is January 15-March 15 2021 and the no-supplement period is November and December 2020.

Table A-2: Micro Effect of Expanded Benefits: Alternative Measures of Exit

(a) Onset of $300

	Dependent variable:			
	New job	Recall	Observe separation	All
	(1)	(2)	(3)	(4)
SuppAvail*PctChange	-0.0174^{***}	-0.0014^{*}	-0.0183^{***}	-0.0128^{***}
	(0.0011)	(0.0007)	(0.0013)	(0.0009)
Observations	1,930,754	1,909,486	1,945,997	3,150,298

*p<0.1; **p<0.05; ***p<0.01

(b) Expiration of $600

	Dependent variable:			
	New job	Recall	Observe separation	All
	(1)	(2)	(3)	(4)
SuppAvail*PctChange	-0.0138^{***}	-0.0105^{***}	-0.0230^{***}	-0.0329^{***}
	(0.0011)	(0.0015)	(0.0018)	(0.0014)
Observations	2,068,302	2,103,500	2,134,730	3,074,113

*p<0.1; **p<0.05; ***p<0.01

Notes: This table reports estimates of $\hat{\beta}$ from equation 5 specified for four different outcome variables. The first column is the same as in Table A-1. Column (2) is exit to recall in the sample where separation is observed, column (3) is any exit (new job or recall) in the sample where separation is observed, and column (4) is any exit (not conditional on whether separation is observed). It is only possible to separate exits to recall from exits to new job in the sample where a separation is observed.

Table A-3: Micro Effect of Expanded Benefits: Robustness to Controls

(a) Expiration of $600

	Dependent variable:			
	Exit to New Job			
	(1)	(2)	(3)	(4)
PctChange*SuppAvail	-0.0138^{***}	-0.0121^{***}	-0.0112^{***}	-0.0106^{***}
	(0.0011)	(0.0011)	(0.0011)	(0.0020)
PctChange	X	X	X	X
SuppAvail	X	X	X	X
State*SuppAvail FE		X	X	X
Age*SuppAvail FE			X	X
Industry*SuppAvail FE				X
Observations	2,070,769	2,070,769	2,052,358	549,784

*p<0.1; **p<0.05; ***p<0.01

(b) Onset of $300

	Dependent variable:			
	Exit to New Job			
	(1)	(2)	(3)	(4)
PctChange*SuppAvail	-0.0174^{***}	-0.0172^{***}	-0.0169^{***}	-0.0175^{***}
	(0.0011)	(0.0011)	(0.0011)	(0.0020)
PctChange	X	X	X	X
SuppAvail	X	X	X	X
State*SuppAvail FE		X	X	X
Age*SuppAvail FE			X	X
Industry*SuppAvail FE				X
Observations	1,946,095	1,946,095	1,926,460	530,781

*p<0.1; **p<0.05; ***p<0.01

Notes: This table reports estimates of $\hat{\beta}$ from equation 5, adding increasingly stringent control variables. The first column is the same as in Table A-1. Column (2) adds state by time fixed effects. Column (3) adds age bin by time fixed effects. Column (4) adds prior industry by time fixed effects. Prior industry is available only for workers who worked at the 1000 largest firms in the data and therefore uses a smaller sample than the other columns.

Table A-4: Micro Effect of Expanded Benefits: Estimates Through May 2021

	Dependent variable: Exit to New Job			
	(1)	(2)	(3)	(4)
PctChange*(Jan 15 - Mar 15)	-0.0174^{***}	-0.0172^{***}	-0.0169^{***}	-0.0175^{***}
	(0.0011)	(0.0012)	(0.0012)	(0.0021)
PctChange*(Mar 22 - May 9)	-0.0019	-0.0125^{***}	-0.0127^{***}	-0.0159^{***}
	(0.0013)	(0.0013)	(0.0013)	(0.0024)
PctChange	X	X	X	X
SuppAvail	X	X	X	X
State*SuppAvail FE		X	X	X
Age*SuppAvail FE			X	X
Industry*SuppAvail FE				X
Observations	2,564,307	2,564,307	2,536,753	701,379

*p<0.1; **p<0.05; ***p<0.01

Notes: This table extends Table A-3b to use a longer time horizon after the onset of the \$300. It reports estimates of $\hat{\beta}$ from equation 5, including increasingly stringent controls and using data through May 9.

Table A-5: Duration Elasticities: Importance of Recall and Finite Duration Benefits

Supplement Duration	Include Recalls	Macro Calibration \$600 (1)	Macro Calibration \$300 (2)	Micro Calibration \$600 (3)	Micro Calibration \$300 (4)
Actual	Yes	0.07	0.10	0.10	0.18
Actual	No	0.08	0.14	0.12	0.24
Infinite	Yes	0.15	0.13	0.25	0.23
Infinite	No	0.38	0.24	0.87	0.72

Notes: This table demonstrates how the regression-based duration elasticities reported in Table 2 duration elasticities are affected by the presence of recalls and finite supplement durations. Each row converts the causal effects on new job finding reported in Table 1 into duration elasticities using the method described in Section 6 but under different assumptions about recalls and supplement durations. The first row, which repeats the regression based duration elasticity results from Table 2, correctly accounts for the presence of recalls and finite durations. Other rows exclude recalls, assume infinite supplement, durations or both.