Online Appendix to "Spending and Job-Finding Impacts of Expanded Unemployment Benefits: Evidence from Administrative Micro Data"

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A Additional Institutional Detail and Data Description

A.1 Simultaneous Policy Changes in the Summer of 2021

Nearly every state terminated expanded benefit eligiblity (PEUC) at the same time that they terminated the $300 weekly benefit supplement in the summer of 2021.[45] The presence of two simultaneous policy changes is a conceptual challenge for trying to estimate the separate effect of the $300 supplement. Unemployed workers who are still eligible for regular benefits will see a decline in benefits of $300 per week (call this treatment 1) while those who were only eligible under PEUC will lose their benefits entirely, and hence will see a decline in benefits of $300 + regular benefits (call this treatment 2).

We can partially separate households subject to treatment 1 from those subject to treatment 2 by focusing on either households with short observed unemployment duration or on households where we see benefits continuing after PEUC expires. However, using this approach to analyze of the expiration of the $300 supplement has two limitations relative to the analysis of earlier supplement changes. First, while all UI recipients maintained eligibility around earlier supplement changes, in the summer of 2021 only 30% of recipients maintained eligibility because of the simultaneous expiration of PEUC (treatment 2). This means that the sample size of workers who continue to receive benefits after these changes is smaller than in prior time periods. Second, and related, those recipients that do remain are not randomly selected. They are specifically those with low UI durations who have not yet rolled onto PEUC.

A.2 States Included

We divide states into three groups:

1. Fully included (15 states)
2. Partially included (29 states + DC)
3. Not in sample (6 states)

44 states plus DC are included in the benchmarking analysis in Section 2.2.1 and in the spending analysis in Section 3. Six states are excluded because we are unable to identify a transaction string that is unique to UI payments. In some states we are able to identify a transaction string that appears to include both UI payments but they are mixed with other transfer programs. The one exception to this is that we include California, which appears to use the same transaction string for UI, Disability Insurance, and Paid Family Leave. However, in public data, only 7% of California recipients of payments from the Employment Development Department in 2020 are receiving disability insurance (DI) or paid family leave (PFL). In addition, we drop anyone with benefits greater than the maximum weekly benefit for UI, which drops a large share of the DI and PFL recipients.

Relative to the 44 state sample, additional data cleaning is needed for the job-finding analysis in Section 4 because we need to know the state's withholding rate for income taxes (discussed below)

[45] Congress also set up a second program (PUA) for workers ineligible for regular UI. Most states also terminated PUA at the same time that they terminated PEUC. In most states we are not able to separate regular UI from PUA in the JPMCI data. Based on data from a few states for which we can separate PUA payments, PUA recipients are a very small share of the main analysis sample in our paper. We believe that PUA is a smaller share of our analysis sample because our filters require consistent labor market attachment in 2018 and 2019.

and we drop workers receiving more than the state's weekly maximum benefit and less than the state's weekly minimum benefit. We attempted to clean data for the 16 largest states by number of UI payments, which collectively account for 97% of the UI payments in the bank data. We succeeded for all of the states except Florida, which accounts for 6% of the UI payments, and Colorado, which accounts for 2% of the UI payments. Florida has a high rate of false exits because of PUC overpayments in May 2020 (enough to distort the aggregate time-series for job-finding) and Colorado has an anomalously high exit rate at the start of December 2020. The set of states that are included in the job-finding analysis therefore accounts for 89% of the total UI payments in the bank data.

Finally, we make two further restrictions for some parts of the job-finding analysis. In the difference-in-difference expiration analysis in Section 4.2, we exclude states for which we are unable to separate LWA from regular payments (LWA payments make it impossible to measure the true date of exit): Texas, Connecticut, Louisiana, and Wisconsin. The remaining sample is CA, GA, IL, IN, MI, NJ, NY, OH, OR, and WA. In the difference-in-difference onset analysis, we exclude Oregon and Michigan. Oregon has an anomalously high exit rate for December 2020. Michigan sees a very high share of its low-benefit workers exit UI receipt because of the PUA/PEUC cliff. Unfortunately, the data cleaning procedure we use for other states to handle the cliff (described in the note to Figure 4) is not effective in Michigan because there still is a sharp increase in measured exits even after data cleaning. The remaining sample is CA, CT, GA, IL, IN, LA, NJ, NY, OH, TX, WA, and WI. Finally, for analysis which requires a consistent job-finding rate throughout the entire sample period (e.g., Figure 4 and Section 4.1) we use the nine states (CA, IL, IN, NJ, NY, OH, OR, and WA) that are present in both the expiration analysis and the onset analysis.

A.3 Unemployment and Employment Spells

We measure unemployment insurance spells (henceforth "unemployment spells") using the payment of unemployment benefits. An unemployment spell starts with a worker's first benefit payment in the sample frame, which is January 2019. In most states, a spell ends when a worker has three consecutive weeks with no benefit receipt. In states which pay benefits every other week, we instead define a spell end as four consecutive weeks without benefit receipt.

We measure employment outcomes using receipt of labor income paid by direct deposit. An employment spell begins with a worker's first paycheck from an employer. We identify employers using a version of the transaction description associated with a payroll direct deposit which is purged of personal identifying information (see Ganong et al. 2020 for additional details). An employment spell ends (henceforth a "separation") if a worker has five consecutive weeks with no paycheck from that employer. We define a separation as being associated with an unemployment spell if a worker has a separation between eight weeks before and two weeks after the start of an unemployment spell. This eight week lag allows for time for UI claims to be filed, processed, and paid, while the two week lead accounts for the fact that last paychecks can be paid after the date of last employment. 55 percent of benefit recipients have a detected separation at the time of benefit receipt.

We do not detect separations for every benefit recipient for two reasons. First, the JPMCI data do not include labor income paid via paper check or direct deposit labor income without a transaction description that mentions payroll or labor income. Second, in some cases more than eight weeks elapse between the last paycheck and first benefit payment; this scenario can arise if a state UI agency is slow to process a worker's benefit claim or if a worker does not file for benefits immediately after separation.

We combine information on unemployment and employment spells to separate UI exits to a new job from UI exits to recall, which is when an unemployed worker returns to their prior employer. We are able to observe recalls only for unemployed workers for whom we also observe a job separation. We define a worker as having been recalled when they begin an employment spell with their prior employer between five weeks before and five weeks after the end of a benefit spell. We choose these thresholds based on the timing of job starts relative to the end of unemployment spells. The data on unemployment spells and employment spells jointly offer something comparable to the administrative datasets used to study unemployment in European countries (DellaVigna et al. 2017; Kolsrud et al. 2018; Schmieder, von Wachter, and Bender 2012).

A.4 Sample Restrictions

Households in our analysis sample must meet two account activity screens: 1) at least five transactions per month and 2) annual pre-pandemic labor income of at least $12,000. We impose these screens to focus on workers whose primary bank account is at Chase. For households that get benefits in 2020 (but not in 2019), we impose the transaction screen from Jan 2018-Mar 2020, and the labor income screen in 2018 and again in 2019. For households that get benefits in 2019, we impose the transaction screen for Jan 2018-Mar 2020 and the labor income screen in 2018. Among households that meet the activity screens, 11.6 percent receive unemployment benefits at some point during the pandemic. This is lower than the rate for the U.S. as a whole, primarily because JPMCI only captures benefits paid by direct deposit. The Census Household Pulse Survey shows that 31 percent of households with at least one working-age person received UI benefits between March 13, 2020 and the end of October 2020. Finally, we limit the sample to customers who are present in the sample from January 2020 through March 2021 with positive inflows, positive outflows, and non-null account balance in every month.

The narrowest sample we use is a sample of customers who meet the account activity screens described in the prior paragraph and receive benefits from one of nine states (CA, IL, IN, MI, NJ, NY, OH, OR, and WA). Figure A-21 shows that aggregate unemployment surges at the start of the pandemic and then declines as the economy recovers. We also analyze data on a random sample of 187,000 *employed* workers who meet the transaction and labor income screens for 2018 and 2019, do not ever receive UI benefits in 2019 and 2020, and do not have a job separation in 2020.

A.5 Additional Variable Detail

We measure age as the age of the primary account holder (the first name listed on the bank account) at the start of an unemployment spell.

The 2020 stimulus checks authorized by the CARES Act had maximum amounts of $1200 per single adult (and $2400 per married filing jointly) and $500 per child. For the subset of people who receive stimulus by direct deposit, we can infer the number of children in the household from the stimulus amount. If a household receives multiple stimulus checks, we use the value of the first. If a household does not receive a payment by direct deposit, but deposits a paper check whose sum is a multiple of $1200 (or $2400) and a multiple of $500, we infer the number of children from that.

JPMCI has hand-categorized firms into 20 industry groups based on NAICS codes for approximately 2,000 employers associated with the most bank accounts. Some households have multiple labor income streams in their bank account. We assign households to industries using the firm that paid them the most in the three months prior to UI receipt. Industry is available for about one-quarter of UI spells.

To limit the influence of high income, high spending, and high asset households on means and MPC estimates, income, spending, and balances are winsorized at the 90th percentile.

B Appendix to Section 3 (Spending Responses)

B.1 Summer MPCs

In this section we discuss the estimation of MPCs to the expiration of $300 supplements in the summer of 2021. We focus on households that receive UI benefits for the entire month after supplements end so that we can estimate an MPC. However, we note that since the PEUC extended benefit program expired at the same time as $300 supplements, this restriction means we measure MPCs only for a selected subset of short-duration unemployed. We must focus on this subset of households with continuing benefits because for households without continuing benefits, we cannot distinguish benefit exhaustion from job finding. This in turn means that we cannot estimate an MPC for these households since we do not know whether they face a benefit change of $300 or of $300 + regular benefits.

We analyze the expiration of $300 supplements in September for many states but also analyze the expiration of $300 at the end of June for states which ended supplements early. We follow the same empirical strategy discussed in Section 3.2. For the September states, our IV approach compares unemployed to employed households, with *Post* equal to one in October and zero in August 2021. For the early states, *Post* equals one in August and zero in June 2021. For the MPCs based on early expiration of benefits, the treatment group is always the unemployed in those early states, while we consider two different control groups. One compares to employed households in the same early states while the other compares unemployed households in early states to unemployed households in states which kept supplements until September. Figures A-11 and A-10 show that just like for the end of $600 and start of $300 supplements, there is a sharp change in weekly spending in the exact week when $300 supplement expire. Table 1 shows we also find similar MPCs.

The MPCs that we estimate when using employed workers as a control group are very similar to the MPCs we estimate when using unemployed workers in other states as a control group. This is useful because all of the other policy changes except for the summer 2021 expirations apply to all unemployed workers at the same time, and so we must rely on employed workers as the control. The similarity of these results in the one episode where we can use both control groups suggests that employed workers are indeed a valid control group more generally.

B.2 MPCs by Pre-pandemic Liquidity: Additional Details

This section provides additional detail on the MPC heterogeneity calculations in Section 3.3.

Since we measure checking account balances once per month, we define the liquidity buffer for household i in month t as $(\textit{checking balance}_{i,t} - 0.5\ \textit{spend}_{i,t})/(\textit{spend}_{i,t})$. Dividing by $\textit{spend}_{i,t}$ accounts for differences across households in permanent income (as proxied by the level of spending). Subtracting 0.5 $\textit{spend}_{i,t}$ from the numerator accounts for the fact that funds in the checking account are both a financial reserve and used to cover monthly transaction costs. Simpler definitions of the liquidity buffer generate similar results. See Kaplan, Violante, and Weidner (2014) for more discussion.

For both Figure 3 and Table 2, we define the employed control groups by matching on income quintile and stimulus check timing (as in our other MPC results) and *also* by (above or below-median) liquidity. We then re-weight the above-median and below-median liquidity unemployed to have the same income and stimulus check timing as the average unemployed household in the data. We do this so that our analysis captures the differences across the two liquidity groups in liquidity and is not driven by confounding differences in income or stimulus check timing but results are nearly identical when we re-compute results without this re-weighting.

B.3 MPC Robustness

This section describes a number of robustness checks for the MPC estimates in Section 3.2.

First, we decompose our MPCs into separate categories. This is useful for understanding how benefit expansions affected households' lives, for gauging whether MPCs might be driven by pandemic-specific forces which might not generalize to other recessions, and for assessing whether spending responses to UI might differ from more general spending patterns. Table A-7 reports the MPCs on the subset of spending transactions we can assign to granular categories based on merchant category codes (for card transactions) and text descriptions (for electronic transactions). The category with the largest response was groceries, accounting for around 20% of the increase in categorized spending in each episode.

In general, the MPCs do not appear to be driven by any unusual spending patterns. In particular, this table shows that the share of the MPC explained by each category is similar to these households' spending shares across categories in 2019, prior to the pandemic.[46] That is, *marginal* spending responses by category are similar to *average* spending levels across categories. While there are some shifts in spending relative to pre-pandemic patterns, these mirror aggregate trends and return to more normal patterns later in the pandemic. For example, unemployed households in April 2020 have an elevated share of spending on home improvement, but this is not surprising since overall spending on home improvement went up early in the pandemic. By 2021, spending patterns out of UI are close to pre-pandemic spending patterns.

While our analysis focuses on spending responses to UI, it is also interesting to look at effects on debt. Table A-8 shows that the marginal propensity to repay auto, mortgage, and student loan debt is very small. This may be a consequence of various debt moratoria during the pandemic reducing incentives to pay down debt. The fact that debt paydown is slightly larger for car payments than for mortgage loans and student loan debt is consistent with this explanation. We note that there is a separate literature that often finds sizable marginal propensities to repay debt in response to stimulus checks. It would not be particularly surprising if unemployed households valued liquidity more than employed households and so were less likely to pay down debt than the population as a whole. Furthermore, we note that we cannot distinguish revolving credit card payments (i.e. debt paydown) from non-revolving credit card payments (i.e. spending paid off within the month) so we do not include credit card bill payments in this table. It is possible that households used some of the UI benefit supplements to pay off revolving credit card debt, which are unable to measure.

Second, we explore the robustness of our results to controlling for more observable differences between the unemployed and employed since any deviations between these groups not driven by supplements would contaminate our MPC estimates. All of our MPC results control for the pre-separation income of the unemployed as well as for the timing of stimulus checks because we view these as the most salient identification threats. Table A-9 shows the results of controlling for additional observable characteristics that potentially differ between unemployed and employed. This table re-computes MPCs progressively adding controls for location, for age, and for the presence of children,

[46] Since each MPC design comes from a different month in 2020/2021, we compare to spending levels in the same months in 2019 to eliminate potential seasonality.

which we think of as capturing other potential differences that might affect spending patterns during this time period. Location captures differential pandemic threats as well as government restrictions on spending, age proxies for differential health risk of spending, and the presence of children proxies for school-closure related constraints. Putting in all of these controls makes essentially no difference for point estimates (generally they are within .01 of the original estimates), but slightly decreases precision.

Third, spending patterns are nearly identical for unemployed households who are recalled and those are who are not, suggesting that spending responses are not driven by the unusually high recall rates during the pandemic. For example, Figure A-22 selects all households who are continuously unemployed from April 2020 through August 2020 and exit unemployment in September 2020 (the first month after \$600 supplements expire) and then further splits these households into those who return to their old employers ("Exit to recall") and those who do not ("Exit to new job"). This figure shows that spending patterns over this entire period are nearly identical for both groups, suggesting that different patterns of recall are not driving spending patterns.[47] We obtain similar conclusions if we repeat these comparisons of recalled and non-recalled households exiting in each month from June 2020 through November 2020.

Fourth, our results are also robust to limiting the sample to households for whom we are likely to observe a more complete lens on spending. For example, many Chase customers have non-Chase credit cards. We will understate MPCs if these households increase spending on non-Chase credit cards when receiving supplements. Table A-10 shows that when we limit the sample to households with no ACH payments made towards non-Chase credit cards, MPCs are two to five cents higher. This suggests that the presence of non-Chase credit cards leads us to slightly understate MPCs in our baseline sample. As further evidence of this point, we have computed the response of non-chase credit card debt payments to supplements and find an increase in payments of 0.04. As noted when we discussed debt payments, we cannot separate these marginal payments between revolving and non-revolving credit. However, it is likely that at least some of this increase in debt payments is a result of increased spending rather than increased pay down of revolving credit on non-Chase credit cards.

As a second example of a potential bias arising from having an incomplete lens on spending, Chase customers may make some debt payments using paper checks, which our methodology will misattribute as spending (because we do not observe the content of account outflows where the payment method is paper check). This might lead us to overstate the MPC. Table A-10 therefore reports a robustness check which limits the sample to households which use their account for debt payments via ACH—thereby mitigating the concern that paper checks are mistakenly including some debt payments as spending—and finds that MPCs are indeed slightly (zero to four cents) lower than the baseline estimates but still large.

In addition, although our baseline MPC estimate follows much of the prior literature by looking at means, Table A-10 shows that estimates using the median change in spending and income results in MPCs that are two to seven cents higher.

Fifth, our conclusions are robust to alternative spending measures. For example, using the narrow *card and cash* measure of spending which is less subject to concerns about misclassification still delivers

[47] Since we do not know how recall expectations evolve, we show the entire time-series and not just the MPC in August to demonstrate that spending evolves identically over the whole period.

large spending responses. Table A-11 shows that MPCs out of this more narrow spending measure are mechanically slightly smaller but the elasticity of this narrow spending measure to benefits is actually larger. For example, Table A-1 shows that the narrow measure drops 40% of spending, but the MPC is reduced across the three research designs by only 24 to 33%. This implies that the responses observed in card and cash spending are proportionally larger than the responses in total spending and so when expressed as elasticities, the response of this subset of spending is larger than the response of total spending. This suggests that the large spending responses that we find are not driven by misclassification.

Sixth and finally, the MPC estimates in this paper define the denominator as household income (more specifically: total inflows to Chase deposit accounts, excluding transfers). However, one could also define the denominator as the change in UI benefits alone, excluding all the other components of income. Relative to Table 1, using UI benefits as the denominator leads to a smaller MPC in four cases and a larger MPC in two cases. We prefer the MPC estimates which use supplement changes as instruments for the change in total income because they implicitly adjust for differential shocks to income between the treatment and control group.

C Appendix to Section 4 (Job-Finding Responses)

C.1 Measuring Weekly Benefit Amount

Some individuals have UI inflow amounts that vary from week to week, for example, due to backpay. We require a single weekly benefit amount to calculate a percentage change in benefits from a supplement. We estimate the benefit amount $b_{i,pre}$ as the median benefit paid to an individual in the two-month period before the \$600 supplement expiration or the two-month period before the \$300 supplement onset. We then drop the first payment and the final payment and compute a median for the remaining weeks. Some states (CA, FL, MI, CO, TX, and IL) pay benefits once every two weeks and so we divide the median payment by two to capture the amount paid per week.

We measure state UI minimum and maximum benefits using the January 2020 "Most Recent Significant Provisions of State UI Laws" publication from the Department of Labor. If a state pays a dependent allowance we use the maximum benefit with dependents and the minimum benefit without dependents. We measure each state's rate of income tax *withholding* using Whittaker and Isaacs (2022).

For the difference-in-difference analysis, we estimate workers' regular weekly benefit amounts in the absence of any supplements. For the \$300 supplement reinstatement, we estimate workers' regular weekly benefit amount as $wba_i = b_{i,pre}/(1 - withholding)$. For the \$600 supplement expiration, we estimate workers' regular weekly benefit amount as $wba_i = \frac{b_{i,pre} - (1-withholding)600}{1-withholding}$. California and New Jersey did not withhold from the supplement so we instead use $wba_i = \frac{b_{i,pre}-600}{1-withholding}$.

We limit the sample to workers with plausible regular weekly benefit amounts wba_i. Define each state's minimum weekly benefit as b_{min} and maximum as b_{max}. We keep workers with $wba_i \in [(1 - withholding)b_{min}, b_{max}]$. These restrictions will remove customers who have a median payment that includes substantial backpay.

Calculating wba_i requires knowing whether a worker decided to withhold, but we generally do not observe withholding at the worker level. Because more than 50% of UI recipients withhold in every state in our sample, our default assumption is that workers *are* withholding at the rates reported in Whittaker and Isaacs (2022). However, if a worker has $wba_i > (1 - withholding)b_{max}$ then the withholding assumption implies that they are receiving an invalid weekly benefit amount. Thus, for workers with $wba_i \in ((1 - withholding)b_{max}, b_{max}]$, we recalculate wba_i assuming that $withholding = 0$.

Recall that our object of interest is the *change* in benefits from the expiration or onset of a supplement, which we construct in equation (3). For the \$300 supplement reinstatement, we estimate equation (3) as $PctChange_i = \frac{2\times 300}{2wba_i+300}$. For the \$600 supplement expiration, we estimate equation (3) as $PctChange_i = \frac{-2\times 600}{2wba_i+600}$.

C.2 Identification in the Difference-in-Difference Research Design

Identification in the dose-response difference-in-difference design requires three 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 assumption has a testable prediction: parallel trends prior to the policy change. Figure 6 shows that the data are consistent with this assumption for the exit rate to new jobs. 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 Appendix C.5, we show that nearly identical conclusions obtain when exploiting only within state-age-industry group variation. In addition, we note that if there were a persistent difference in job-finding trends (e.g., low-wage workers have faster employment growth because of business reopenings) then the bias in $\hat{\beta}$ will have opposite signs across the two policy changes because one is a decrease in benefits while the other is an increase in benefits.

Second, we assume that the causal effect of replacement rates on job-finding is homogeneous in the treatment group and control group. This assumption implies that raising a low-wage worker's replacement rate will have the same absolute effect on job-finding as raising a high-wage worker's replacement rate by the same absolute amount, thereby implying a linear relationship between replacement rates and exit rates. De Chaisemartin and D'Haultfœuille (2018) show that this assumption is necessary for identification in dose-response designs.

Two pieces of evidence are consistent with the homogeneity of treatment effects. First, the apparent linearity of the effect of benefit changes on the job-finding rate in Figure 7. Second, we also test this assumption using the structural model from Section 5. Specifically, we calculate the change in the job-finding rate for low-wage and high-wage workers at different sizes of benefit decreases ranging from -70% to -120% (following the support of the x-axis variation in Figure 7). We find that when exposed to the same percentage change in benefits, low-wage workers and high-wage workers in the model have very similar changes in job-finding.

Third, we assume that job seekers did not anticipate the changes in supplements. This is consistent with the results from our best-fit model that households act surprised by policy changes.

C.3 Comparison of Estimates Across Research Designs and Episodes

Comparing the estimates of job-finding disincentive effects across episodes and research designs requires re-scaling the four estimates (two research designs and two policy changes) into common units. Comparing within a given policy change, the interrupted time-series estimates tell us the average effect of the entire supplement while the difference-in-difference estimates tell us the effect of a marginal change in $PctChange$ across workers. We convert from the difference-in-difference estimate $\hat{\beta}$ to an estimate of the average effect of the entire supplement by assuming a homogeneous treatment effect over the entire range $[0, E(PctChange_i)]$ and computing $\hat{\tau} = \hat{\beta}E(PctChange_i)$. We note that this is a stronger assumption than that in the previous sub-section (Appendix C.2) since $min(PctChange_i) > 0$, and so this requires linear extrapolation out of sample.[48]

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 (Figures 7a and 7b). Second, in the best-fit model, the effect of the supplement on the job-finding rate is also close to linear, as shown in Figure A-20.

Although our analysis treats the two research designs as estimating the same parameter, one pos-

[48] Ideally, we would like to compare a treated group receiving a supplement (and thus $PctChange > 0$) to an untreated control group with no supplement (and thus $PctChange = 0$) but we have no such untreated control group and must instead extrapolate from comparisons across groups with different positive $PctChange$.

sibility for why time-series estimates are smaller is if the "micro" disincentive effect of unemployment benefits (the effect of giving one worker more benefits) is bigger than the "macro" disincentive effect (the effect of giving all workers more benefits).[49] The macro effect includes additional equilibrium effects.[50] The difference-in-difference estimate, which compares changes in the job-finding rate for more- and less-treated workers, may be closer to a micro elasticity, while the interrupted time-series estimate, which measures the change in the job-finding rate for all workers, may be closer to a macro elasticity.

C.4 Calculating Duration Elasticities

The duration elasticity is defined as the percentage change in expected unemployment duration caused by a given change in benefit size divided by the percentage change in benefits:[51]

$$\epsilon = \frac{\frac{ED_{\text{with supp}}}{ED_{\text{no supp}}} - 1}{\text{Supp Size/Ave Regular Benefit Size}},$$

where ED is the expected duration of unemployment. We now describe the calculation of these objects in more detail. 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 $recall_t$. We assume e_t and $recall_t$ are constant at their sample averages after the end of the observed data.

We then construct a counterfactual total exit hazard with no supplement: $\lambda_{t,\text{no supp}} = \lambda_{t,\text{with supp}} + \tau_t \times I_t(supp = on)$, where τ_t is an estimate of the effect of a given supplement on the job-finding rate at date t, and $I_t(supp = on)$ is an indicator for whether a supplement is on or off in week t. For the statistical exercises assuming constant effects, τ_t is assumed constant at values from Table 3, while in the model τ_t is calculated from the full model dynamics. Thus, the simple statistical counterfactual without supplements just shifts up the observed job-finding rate by the constant amount τ while the supplement is in effect, while in the model τ_t varies with any dynamic forces. Based on the discussion in Appendix C.5 we assume the recall rate in period t is the same with and without the supplement. So, the shift in the total exit rate λ is given by just the change in the new job-finding rate τ_t.

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 by converting the job-finding hazards λ to a survival function. Specifically, let $S_{t,\text{with supp}} = \Pi_{j=1}^{t}(1-\lambda_{j,\text{with supp}})$ and $S_{t,\text{no supp}} = \Pi_{j=1}^{t}(1-\lambda_{j,\text{no supp}})$ be the cumulative survival functions with and without supplements. The expected duration with supplements is then given by

$$ED_{\text{with supp}} = \lambda_{1,\text{with supp}} + \sum_{j=1}^{\infty} (\lambda_{j+1,\text{with supp}}) (S_{j,\text{with supp}}) (j+1),$$

[49] Many theoretical papers on UI (e.g., Hagedorn et al. 2013 and Landais, Michaillat, and Saez 2018) argue that the micro disincentive effect is insufficient to determine optimal benefit levels; one must also know the macro disincentive effect.

[50] First, it captures the response of new vacancies to more generous benefits. More generous benefits could decrease vacancy creation by reducing match surplus (Hagedorn et al. 2013) or increase it by increasing aggregate demand (Kekre forthcoming). Second, it captures the "rat-race" effects in Michaillat (2012), where discouraging one worker from taking a job may simply lead to another worker taking the job instead.

[51] Elasticities are sometimes approximated using log changes, but this approximation is poor in our context since we study large changes.

and the average duration without supplements is given by

$$ED_{\text{no supp}} = \lambda_{1,\text{no supp}} + \sum_{j=1}^{\infty} \left(\lambda_{j+1,\text{no supp}}\right)\left(S_{j,\text{no supp}}\right)(j+1).$$

Expected duration will depend on both the time-series of job-finding and the number of weeks in which $I_t(supp = on) = 1$. The latter will vary for cohorts that enter unemployment at different dates since those entering closer to supplement expiration will have a shorter period with supplements in effect. This means that the statistical-based duration elasticity will differ for different cohorts but will be maximized in most cases for cohorts starting unemployment in the same week that supplements start. For this reason, we conservatively report duration elasticities for an unemployed cohort starting April 1, 2020 for the \$600 supplements and January 1, 2021 for the \$300 supplements.

Together this procedure gives the numerator of ϵ. The denominator is more straightforward since we know the size of supplements and can directly measure the benefit amount in the data.[52]

C.5 Additional Job-Finding Results and Robustness

C.5.1 Robustness

We conduct three tests to validate the difference-in-difference estimates. First, we estimate a version of equation (4) by week:

$$e_{it} = \gamma PctChange_i + \alpha Week_t + \beta_t Week_t \times PctChange_i + \varepsilon_{it} \tag{C1}$$

This enables an event study interpretation of the coefficients. Figure A-23 shows that treatment effects from the expiration of the \$600 are largest in the three weeks after the policy expires and smaller in the subsequent five weeks. This suggests that long-term effects of expiration on the weekly job-finding rate may be even smaller than the baseline estimates from equation (4) which pool the eight weeks after expiration in Table A-2. The figure also shows a stable treatment effect from the onset of the \$300.[53]

In a second group of checks in Tables A-12a and A-12b, we re-estimate equation (4), adding controls X_i and $X_i SuppAvail_t$ to address concerns about differential trends by group. 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 and finally, we add industry (and industry-by-supplement-available) fixed effects. In this richest specification, identification comes from comparing the job-finding rate for higher- and lower-wage workers with different 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.

[52] To limit the influence of outliers, our empirical specification for computing τ uses the symmetric percent change in benefits as the regressor. Using an empirical specification with the "regular" percent change produces a lower value of τ and thus a lower implied ϵ. Thus, our conclusion of a low ϵ from our preferred specification is conservative.

[53] Figure A-23b indicates that even prior to the onset of the \$300 supplement there is already a gradual trend downward in the job-finding rate for households that receive the largest increase in benefits on January 1, 2021. If we were to use a specification that accounted for this pre-trend in estimation we would likely find that the \$300 supplement has even smaller effects on the job-finding rate.

Third, we explore the robustness of our results to alternative functional forms for estimating job finding effects. Our baseline empirical specification estimates a dose-response difference-in-difference design using a linear probability model, which assumes a constant relation between the absolute change in the exit rate (in percentage points) and the relative change in benefits. This empirical specification is supported by our theoretical model, as shown in Figure A-20. However, the most common empirical specification for estimating disincentive effects of UI is a Cox proportional hazard model, which instead assumes constant proportional rather than absolute effects. The Cox model cannot be used in our setting where policy changes affect unemployed workers in the middle of a spell. However, we can explore the robustness of our results to alternative specifications which estimate relative rather than absolute outcomes (i.e. using the percent change in exit rates rather than the absolute change in exits as the outcome). We explore two alternative specifications. The first is a binned version of our baseline regression which uses 10 deciles of the percent change in benefits but switching to the percent change in exit as the outcome. This specification allows us to measure percent changes for our binary exit outcome, but it collapses the micro data to bins. The second specification is a logit specification which lets us use the micro data with percent changes in outcomes. Table A-13 shows that these alternative specifications produce very similar conclusions to our baseline empirical specification. Duration elasticities remain low and very similar to the baseline specification. Furthermore, duration elasticities are all below hazard elasticities, as implied by the discussion in Section 5.3.2.

C.5.2 Recalls

How did the supplements affect the exit rate to recall? There is some evidence that the expiration of the $600 supplement might have had a small effect on recalls but the evidence is hard to interpret, and even the upper bound of plausible causal impacts on recalls still implies small aggregate employment effects. There is no evidence of any effect of the $300 supplement on recalls.

Figure A-13a shows time-series patterns of recall. The recall rate is highest while the $600 supplement is still in place, suggesting it did not substantially deter recall. Indeed, more than half of unemployed workers return to work before the $600 supplement expires. This figure also illustrates that the recall rate is falling over time (making it hard to know what the counterfactual recall rate would have been in the absence of the supplement) and volatile (making it hard to assess statistical significance). There is evidence of a short-lived increase in recalls in the three weeks after the supplement expires. However, even if we make the aggressive assumption that recalls would have trended down through these three weeks in the absence of supplement expiration, the implied effect on the average duration of unemployment is tiny because this increase in the recall rate is so short lived.

The time-series evidence around the start of the $300 also suggests it had no effect on recalls. If anything recalls *rise* after the supplement takes effect. We also note that the aggregate recall rate is already low even before the onset of the $300 supplement, meaning there is little scope for a further reduction from the $300.

Difference-in-difference results cast further doubt on the possibility of substantial effects of supplements on recalls. Figure A-17e shows that recall is higher for the high replacement rate group after the $600 supplement expires, but *not* in the three weeks when the aggregate recall rate is elevated. Instead, the recall rate for the high replacement group only rises differentially in the subsequent six weeks. Table A-14a finds a $\hat{\beta}$ coefficient for recall that is about two-thirds of the size of the exit to new job coefficient. However, the figure illustrates that this effect is again short-lived, implying that even if this delayed differential recall response is causal, it has a small effect on aggregate employment patterns.

The difference-in-difference evidence that the $300 had tiny effects on recall is even more clear cut. Figure A-17f shows that there is little difference in recalls between workers with above- and below-median replacement rates. Table A-14b re-estimates equation (4) and reports a coefficient (0.003) that is one-seventh of the already small effect on new job-finding and economically indistinguishable from zero.

Overall, the supplement may have changed the timing of some recalls, but there is no evidence of *substantial* recall effects which would change the conclusions we describe in Section 5.2 that effects of supplements on employment were small. However, alternative data and/or research designs are needed to precisely quantify the effect of the $600 supplement on recalls.

C.5.3 Effects in Summer 2021

We replicate the job-finding analysis from Section 4.2 for the expiration of the $300 supplement. We are unable to replicate the interrupted time series analysis because of the contemporaneous expiration of PEUC (discussed in Appendix A.1).[54] To illustrate this, Figure A-24a shows that the exit rate from UI rises sharply to extremely high levels in September 2021 as many workers mechanically lose benefits when PEUC expires.

We thus focus our analysis on a subset of workers who are ex ante likely to be in the regular UI benefit program (not PEUC) and thus exposed only to the supplement expiration. To do so, we limit the sample to workers who have received UI benefits for no more than four weeks as of July 11, 2021.[55] A worker who received UI benefits continuously through the end of October 2021 would therefore only have received 20 weeks of benefits and therefore receive regular UI benefits (and not PEUC) throughout the analysis period.

However, restricting to low observed duration does not guarantee that a worker is indeed covered under regular UI and not PEUC. There is potential for measurement error in duration (for example, a worker might receive payment for many weeks of benefits in a single calendar week because of back pay) meaning that some of these workers may actually be covered by PEUC instead of regular benefits. Indeed, Figure A-24b shows that the exit rate rises substantially at expiration, even after we limit the sample to workers with short observed duration. Nevertheless, if the extent of measurement error in PEUC eligibility is the same in groups with larger and smaller changes in benefits then it still is possible to identify the causal effect of the expiration.

Figures A-24b and A-25 shows workers with a larger decline in benefits had a larger increase in the job-finding rate. However, we note the large rise in the job-finding rate for workers with a -110% decline in benefits. These are workers who would have had regular benefit without the supplement of only about $150 per week. If there is measurement error in the weekly benefit amount and people for whom the weekly benefit amount is particularly noisy also have a particularly high exit rate then that will artificially inflate our estimate of the disincentive.

Table A-15 shows regression estimates as well as several related specifications. Depending on whether we define the maximum allowed observed duration in July as four or eight weeks and whether

[54] We are only able to replicate the job-finding analysis for the expiration in September 2021. Estimates for the expiration in June 2021 are under-powered, so we are unable to make any economically meaningful statement.

[55] We start the counter on benefit receipt as of January 1, 2020. If a worker goes 26 consecutive weeks without receiving UI we assume that they have re-established eligibility for benefits and reset the counter to zero.

or not we include the weeks when the exit rate is mechanically the highest we find that the regression coefficient varies from -0.0137 to -0.0305.

D Additional Model Details and Results

D.1 Model Setup

This section describes the model setup in additional detail. Each month, households choose consumption c and savings a with return r and a no-borrowing constraint $a \geq 0$ to maximize expected discounted utility $E \sum_{t=0}^{\infty} \beta^t U(c)$. When employed, household i has constant wage w_i which differs across households but is constant over time. Employed households become unemployed with constant probability π. When unemployed, a household finds a job at wage w_i with probability $f_{i,t} = recall_t + search_{i,t}$, where $recall_t$ is a common exogenous recall rate and $search_{i,t} \in [0, 1 - recall_t]$ is household $i's$ endogenous choice of search effort. Search effort induces disutility $\psi(search_t)$. Recall requires no search effort or disutility. When households are unemployed, they receive unemployment benefits as well as additional secondary income proportional to the lost job: hw_i.

Income for an unemployed household depends on aggregate UI policy and whether they are waiting for benefits. Regular benefits last 6 months. Benefits for a household newly unemployed during the pandemic last 12 months.[56] Benefit levels depend on the current aggregate UI supplement in place: $m \in \{0, 300, 600\}$. To speak to our empirical research design, we allow for the possibility that an unemployed household may face delays in receipt of UI and in turn later receive backpay. This means unemployed households can be in one of four receipt statuses: $d \in \{normal, delayed, backpay, expired\}$.

The regular benefit policy is intentionally simple: unemployed households receive benefits which replace a constant fraction b of w_i. When available, supplements add m to these baseline benefits. This means that an unemployed household getting benefits without delay receives $bw_i + m$.

Unemployed households can also be in a delayed receipt status and not currently receiving benefits if $d = delayed$. In this case, current earnings are given by hw_i. When households exit this status, they receive backpay equal to $\alpha(bw_i + m)$, where α is chosen to match "backpay" observed in the data. When regular benefits expire after 12 months, income again drops to hw_i. This means that total earnings y for a household with wage w_i, employment status s, supplement m, and delay status d are given by:

$$y(w_i, s, m, d) = \begin{cases} w_i & \text{if } s = e \\ bw_i + m + hw_i & \text{if } s = u \text{ and } d = normal. \\ hw_i & \text{if } s = u \text{ and } d = delayed. \\ \alpha(bw_i + m) + hw_i & \text{if } s = u \text{ and } d = backpay. \\ hw_i & \text{if } s = u \text{ and } d = expired. \end{cases} \tag{D1}$$

The economy begins in a steady-state UI policy environment with $m = 0$ and $d = normal$. Households expect UI policy will never change. Beginning from this initial steady state, the economy is hit by policy changes which mimic UI policy changes over the pandemic. In April 2020, the economy switches from $m = 0$ to $m = 600$ and remains in this state for 4 months. In August 2020, it switches to $m = 0$. In January 2021, it switches to $m = 300$, and in September 2022 it switches back to $m = 0$.

This describes the evolution of actual policy through this period, but we must also specify expectations. We assume the initial switch from $m = 0$ to $m = 600$ is unanticipated.[57] Once the 600 is

[56] To simplify the computational setup, we assume that pandemic benefits are only available for the first unemployment spell and that when a household returns to employment they return to the regular benefits policy for future UI spells.

[57] For computational tractability, we assume employed households continue to expect regular benefits after the pandemic starts until they actually become unemployed. Since we focus on a cohort of unemployed households beginning

implemented, households know for sure that it will last at least 4 months. In our main results, we consider two different specifications for expectations about m after these 4 months.[58] In the perfect foresight specification, households correctly expect that m will revert from 600 to 0 in August. In the alternative myopic expectation specification, households instead expect that $m = 600$ for the duration of their remaining benefit spell and are then surprised in August when it expires. Once $m = 600$ expires in August, households expect that $m = 0$ forever. For the $300 weekly supplements, we study a newly unemployed household in November of 2020. They either anticipate or are surprised that the $m = 300$ supplement begins in January, 2021.[59] Once the $300 supplement begins, households anticipate that supplements will expire in September 2021.

Expectations about UI delay are simpler. Households who are in the $d = nodelay$ state anticipate that they will remain in this state. That is, households do not anticipate delays in benefit receipt. When households are in the $d = delay$ they always assume that they will be in $d = backpay$ next period and that they will be in $d = nodelay$ the period after that. That is, households always anticipate that benefit delays will be resolved next month. However, even though households always anticipate that delays will be resolved once they enter this state, the realized length of $d = delay$ can extend for multiple periods. That is, just as households are surprised by initial delays in benefits, they can also be surprised by a longer than expected waiting period. In our main simulations, the actual benefit delay lasts two months to match what we observe in the waiting design. In prior drafts we explore other specifications for expectations over delays and found similar quantitative conclusions.

Households anticipate a constant recall rate throughout the pandemic, although results are similar if we instead assume perfect foresight over the actual recall rate. We deal with pandemic effects in two ways: First, we focus on the evolution of unemployed households relative to employed households in both model and data. This means any effects of the pandemic which affect all households equally are effectively removed. Second, we directly model several pandemic events. We introduce a one-month discount factor shock to all households in April 2020, which we calibrate to match the decline in spending for employed households during the pandemic.[60] Since we focus on the behavior of unemployed households relative to employed households, this shock has little effect on our conclusions, but it means that we do a better job of hitting absolute spending and liquidity changes over the pandemic rather than just matching relative changes. We also introduce additional one-time unanticipated transfers to replicate stimulus checks in April 2020 and January 2021 as well as LWA payments in September 2020, but this again has little effect on our conclusions.

Letting n represent the expected number of periods until $m = 0$, the household optimization problem of a household unemployed during the pandemic can be written as:[61]

in April, this choice has little practical effect beyond simplifying computation.

[58] Intermediate versions of expectations unsurprisingly produces results between these two version.

[59] We simulate a separate cohort of unemployed households becoming unemployed in November so that we do not have to also model extensions of the duration of regular benefits which happened periodically throughout the pandemic.

[60] Using a sequence of discount factor shocks introduces additional complication but does not change the results much since only April 2020 exhibits a very sharp swing in spending.

[61] For simplicity, this notation ignores the discount factor shock. In April 2020, the model is solved using a different β for a single period before transitioning back to this specification

$$
\begin{aligned}
V_u(a, s=u, m, d, n) = & \max_{c, a', search} U(c) - \psi(search) \\
& + \beta E_{m', n', d'}[(search + recall) V_e(a', s=e, m', d', n') \\
& + (1 - search - recall) V_u(a', s=u, m', d', n')] \\
& s.t. \\
& a' + c = y(w_i, s, m, d) + (1+r)a, \\
& a' \geq 0, \\
& \text{Equation } D1, \\
& \text{and expectations of } s', m', d', n'.
\end{aligned}
$$

The value function of an unemployed household pre-pandemic is analogous except that m is always 0, d is never delayed and n lasts for 6 instead of 12 months. The value function of an employed household is also analogous except that $s = e$ so $y(w_i, s, m, d) = w_i$, they have no search decision and they transition to the regular pre-pandemic unemployment value function with exogenous separation rate π.

D.2 Model Parameters

Many of our parameters are standard or map directly to observable objects in the data. We describe these parameters first. We then describe more complicated parameter choices that target simulated moments in more complicated ways. Table A-3 summarizes the resulting model parameters.

We set the annual interest rate $r = .04$. We assume that the utility function is given by $U(c) = \frac{c^{1-\gamma}}{1-\gamma}$ and set $\gamma = 2$. We set the exogenous separation probability $\pi = 0.028$ to match pre-pandemic transitions from Krueger, Mitman, and Perri (2016). We set the expected recall rate $recall_t$ to be constant at its average value of 0.08 but use the actual evolution over the pandemic where relevant. Figure A-26 illustrates the environment by showing income for the unemployed relative to employed in the model and data for a newly unemployed worker with and without a benefit delay in our calibrated model. We set $b = 0.21, h = 0.7, m = 0.35, \alpha = 2.35$ to match household income series for the waiting and receiving UI groups over the pandemic. Specifically, h targets mean household income for unemployed households waiting for benefits, relative to employed households. Then given h we pick b so that $h + b$ targets mean household income for unemployed households receiving benefits in fall 2021 when no supplements were in place, relative to employed households in these same months. Given h and b we then pick m so that $h + b + m$ hits the ratio of mean income for unemployed to employed when the \$600 supplements were in place. Finally we pick $\alpha = 2.35$ to match the mean income of the waiting group relative to the not-waiting group in the first period where the waiting group receives benefits (and thus backpay). We solve the model for five different w_i groups and choose the variation to match mean household income by five quintiles of the replacement rate in JPMCI data.

We pick the remaining parameters of the model to target more complicated model objects using an indirect inference procedure to choose the discount factor and simulated method of moments to choose the search cost parameters.[62]

[62]While we use these estimation procedures for point estimations, we do not perform inference on these parameters

We assume that $\psi(search_t) = k_0 \frac{(search_t)^{(1+\phi)}}{1+\phi} + k_1$ and pick the parameters of this search cost function in one of two ways. In our "pre-pandemic" calibration, we calibrate search costs to generate a pre-pandemic job-finding rate of 0.28 and an elasticity of average unemployment duration to a small 6 month change in benefits of 0.5. This is the median estimate from the Schmieder and von Wachter (2017) meta-analysis. Since we only have two moments and three parameters, the pre-pandemic calibration is not identified without additional restrictions and so we impose $k_1 = 0$.[63]

In our "best-fit" calibration, we instead calibrate search costs to target the time-series of job-finding over the course of the pandemic. Specifically, we pick search cost parameters in the model to minimize the squared percentage deviation between the monthly job-finding rate in the model and data from April 2020 to February 2021. This simulated method of moments-like procedure is over-identified because we have 3 parameters and 11 moments to target.

Table A-3 shows that this yields $k_1 < 0$, implying a *disutility* of unemployment. This contrasts with a common assumption in macro models that unemployment delivers positive utility because of the time available for home production but is consistent with recent experimental findings in Hussam et al. (2022).

The search cost parameters are most interpretable in terms of implications for job search elasticities. The calibrated pre-pandemic search cost parameters imply a job search hazard elasticity to a small benefit change for 6 months of 0.66 and the best-fit search cost parameters imply a hazard elasticity of 0.29.[64]

We calibrate the discount factor in one of two ways. In the pre-pandemic calibration, we pick β so the model matches pre-pandemic evidence on the response of spending to stimulus checks summarized in Kaplan and Violante (2022). Specifically, we pick set $\beta = .99$ monthly to generate a 3-month MPC of 0.25 in response to a 500 stimulus check sent to all households. In our alternative best-fit calibration, we instead pick $\beta = 0.978$ to target the MPC out of UI payments in our waiting design. There is not a mapping directly from the discount factor to the MPC in the model, so this is a simple version of indirect inference. Since we target a single MPC that declines in β, β is exactly identified through this procedure.

D.3 Model Solution

We solve the model using the endogenous grid method with linear interpolation for policy functions off grid points. We use 100 grid points for assets distributed exponentially from 0 to 2000 times median household income. The model must be solved for several different benefit profiles with length up to 13 months (pandemic era benefits last for 12 months and then expire as an absorbing state; regular benefits last for 6 months) as well as the different delay statuses. We solve the model separately for each of the five wage groups. We solve for the value function for employment and regular unemployment benefits iterating to stationary policy functions and then solve for the pandemic-era temporary policies using backward induction from these stationary value functions. We similarly backward induct one period from the stationary value functions to solve for the solutions with the discount factor shock.

to account for statistical uncertainty. In our context there is little cross-sectional uncertainty since we have very large samples, and we have no aggregate shocks in our model so there is no aggregate uncertainty. Thus, statistical uncertainty over parameters in the model is approximately zero. We further note that statistical tests would thus formally reject this model, but also any other model that does not perfectly fit the data.

[63] If we instead impose the value of k_1 we estimate for the pandemic, qualitative conclusions are unchanged.

[64] Note that the hazard elasticity of 0.66 in the former case implies the targeted duration elasticity of 0.5.

To solve for optimal search, we use the first order condition given next period's value functions of employment and unemployment. Given optimal policies, we then simulate the model for 1,000 households of each of each wage group (5,000 households total) and compute average statistics.

D.4 Best Fit Model - Additional Results

The main text focuses on the model-fit for the $600 supplements, but Figure A-19 shows that our conclusions also apply for the model fit to the $300 supplements.

We pick search costs in our best fit model to target time-series variation in the job-finding rate. Since our model features heterogeneity in wages it also has implications for job-finding over the wage distribution which can be compared to our difference-in-difference research design. Figure A-20 shows that the model calibrated to time-series evidence produces difference-in-difference results which align closely with those from the data. This means that the choice of targeting time-series vs. cross-sectional variation makes little difference for our model conclusions. This figure also provides additional support for the linearity assumption imposed by our empirical difference-in-difference research design.

Figure A-27 shows that liquidity effects on job-finding after supplements expire are small demonstrating that dynamic liquidity effects imply little bias for our reduced-form specifications.

D.5 Understanding High MPCs out of UI vs. Stimulus

What explains the large spending responses to unemployment supplements? To understand this, it is useful to compare them to more commonly studied one-time stimulus payments. In particular, recall that our pre-pandemic model is calibrated to match a 0.25 quarterly MPC out of a one-time $500 payment. Relative to these one-time payments, UI supplements differ in their targeting, size, and persistence. Furthermore, these supplements were implemented in a pandemic environment with depressed overall spending and elevated liquidity.

Table A-16 shows that each of these forces is important for understanding the spending responses to supplements. Each row varies one element at a time to illustrate the forces shaping spending responses to supplements during the pandemic. The first row shows the one month MPC out of combined regular benefits plus the $600 supplement ($2,400 monthly) for an unemployed household who is currently receiving no benefits during the pandemic in our best fit model. This combination of elements replicates the empirical environment for the waiting design in Section 3.2.1. Since our best-fit model is calibrated to match this MPC, the model MPC equals the empirical MPC of 0.42 by construction. This MPC mixes the spending responses to regular benefits and supplements. Thus, in row 2 we compute the model MPC to supplements alone, which are targeted at a household already receiving regular unemployment benefits. This MPC of 0.29 corresponds to the supplement MPC in Table 4. We note that this value is essentially the same as the untargeted MPC of 0.30 and 0.26 to the end of the $600 and start of $300 supplements estimated in Section 3.

In row 3, we compute this same MPC but in a "normal" economic environment which eliminates pandemic stimulus checks and discount factor shocks. Since turning off these pandemic forces decreases liquidity, the MPC to UI supplements rises to 0.45. Row 4 then shows that the MPC out of a one-time $2,400 payment falls to 0.29, implying that some of the spending response to supplements comes from the fact that they are persistent rather than transitory transfers. In row 5, we decrease the size of the transfer from $2,400 to $500, which corresponds to the stimulus check sizes that we target in pre-pandemic model calibrations. This increases the MPC substantially due to the concavity of the consumption function. Unemployed households' liquidity constraints and MPCs are relaxed more in response to a large transfer than in response to a small transfer.

Next, we modify who receives the the transfer. Row 6 shows that providing a \$500 transfer to everyone instead of just to unemployed households reduces the MPC from 0.45 to 0.21 since unemployed households have higher MPCs. Finally, row 7 and row 8 show the effect of changing the calibration of the discount factor so that the model hits a quarterly MPC of 0.25 to this \$500 transfer. Row 8 shows that the model hits this quarterly MPC of 0.25 by construction, and row 7 shows that the monthly MPC to this same shock is 0.09 so it can be compared to MPCs from other rows which are calculated at monthly horizons.

Putting all these forces together leads to a one month MPC out of supplements that is more than three times larger than the one month MPC out of \$500 stimulus checks (0.29 vs 0.09).

Summarizing the results from this Appendix, persistence and targeting towards households with high propensities to spend are key forces for high MPCs out of supplements. Their large size decreases the MPC since it relaxes liquidity constraints, but on net the first two forces (i.e. persistence and targeting) dominate and so spending responses to UI supplements are much larger than spending responses to stimulus checks. This conclusion should generalize beyond the pandemic, since these forces are not pandemic-specific. Indeed, this conclusion is likely to be even stronger in a more normal recession when aggregate spending is not reduced due to pandemic-specific reasons (i.e., Table A-16 row 3 is greater than row 2).

E Alternative Models

This section discusses model extensions and alternative models which are mentioned in the main text.

E.1 Time-Aggregation Issues

Our baseline model is monthly and assumes that benefits start immediately upon job loss: households are employed until March 2020 and then become unemployed and start receiving regular benefits plus $600 supplements immediately in April. In the data, there are some high frequency changes around job loss not captured by this simple model. Our data sample selects households who receive their last paychecks at the end of March and receive benefits starting in April. Furthermore, most states did not begin paying supplements until the second half of April even though the amount of supplements covered benefit weeks for the entire month of April. What this means in practice is that in the data, households who become unemployed at the end of March see a small decline in income at the end of March and start of April before benefits start, and then a jump up in income in the second half of April that makes up for the decline in the first half of the month. When aggregated to calendar months, this manifests as a small decline in income in March and a jump up in income in April as households start receiving supplements, but at the weekly frequency, this jump up in April is concentrated in the second half of the month. This means that although April income is above that when employed, this increase occurs primarily in the second half of the month and so does not allow for a full month of spending opportunities.

To address both high frequency timing issues around the start of unemployment and the start of supplements, we make two changes while still maintaining the monthly model period for tractability.

First, we assume that both in regular times as well as during the pandemic, households have a decline in income in the month that they lose their job but do not start unemployment insurance until the following month. We choose this drop in income in the month of job loss to match the observed decline in income in the data. In practice, this drop in income is small since we define the month of job loss as the month when the last paycheck is received. However, this small decline in income in the month of job loss is sufficient to generate declines in spending in the month of job loss. Given this adjusted income process, we assume that households in this extended model lose their jobs in March rather than in April. This model change is sufficient to generate the spending declines in March 2020 observed in the data, as shown in Figure A-18.

Second, we assume the income increase arising from supplements occurs in the second half of April. Specifically, we assume that in the first half of the month, households spend as if they are receiving the regular benefits profile but not supplements. Concretely, we compute spending in April in the time-aggregation model as an equal weighted average of spending under the regular benefits profile and spending under the benefits profile with supplements. Figure A-18 shows that the model with this extension is a good fit to spending in April. Since it is more parsimonious, we use the simpler model in the main text, but this figure shows that that all of our conclusions about our "best fit" model relative to alternative models also hold in this extended model so our results are unchanged if we complicate the model to match these high frequency patterns around the first two weeks of job loss.

E.2 Alternative Behavioral Models without Myopic Expectations

Our best-fit model needs myopic expectations in which households act surprised by benefit changes in order to fit spending and job-finding patterns over the pandemic. It is natural to wonder whether models with more heterogeneity in impatience and job search costs might be able to explain these patterns without "incorrect" expectations. For example, Ganong and Noel (2019) show that a model that includes both hyperbolic and exponential discounting households together with a mix of high and low job search costs can fit empirical patterns around the predictable expiration of benefits. Specifically, a behavioral model with this type of heterogeneity can generate a sharp spending decline and a sharp jump in job finding in response to the predictable end of regular UI benefits. Can a similar model with hyperbolic discounting and heterogeneous job search costs explain patterns in the pandemic without having to introduce myopic expectations? We show now that it cannot.

We consider an extension of our model that allows for the potential for substantial heterogeneity in search costs parameters and discounting across households but that assumes perfect foresight expectations. To include the potential for hyperbolic discounting, we allow for households to discount between now and the next period at a higher rate (using discount factor β) than they discount between all future periods (using discount factor δ). We then re-solve the model for a range of search costs (ten for each parameter), and discount factors (12 for δ and up to 12 for β, with the restriction that $\beta <= \delta$ where $\beta = \delta$ corresponds to exponential discounting). In total this allows for approximately 81,000 different types of households.

The average value of spending and the job-finding rate then depend on the number of households of each type remaining unemployed at each date. This depends on both the initial weight of each household and how quickly each type of household exits unemployment. To pick the initial weights, we use an iterative process to minimize the squared deviation between average job-finding and spending in the model and the data. We begin by picking the weights on each household using the entire vector of 81,000 types. We then further prune this model by iteratively dropping any type assigned a weight $< 0.25\%$. We do this pruning to yield a more parsimonious degree of heterogeneity, which makes it easier to interpret results, but the conclusions are similar when allowing for unrestricted heterogeneity.

This pruning process ultimately results in a parsimonious model with three types: one "hyperbolic" with persistently high MPCs and low elasticity of search costs (31% of households), one hyperbolic type with persistently high MPCs and high elasticity of search costs (12% of households), and a more standard household with exponential discounting and high elasticity of search costs, who has a high MPC when liquidity constrained and a low MPC when not liquidity constrained (57% of households).

Figure A-28 Panels (a) and (b) show that this model misses on two dimensions. First, while the model is able to generate a sharp increase in job finding when supplements expire, it is unable to generate a *sustained* increase in job finding. Second, this model implies counterfactual asymmetry in spending: in the model, the response to the start of the \$600 supplements is substantially larger than the response to the end of the \$600 supplements.

The reason this model does not generate a sustained increase in job finding is because of composition effects. Panel (c) shows that the jump in job finding is driven by the hyperbolic type with low elasticity of search costs (the orange line). This group barely searches when supplements are in place and then searches hard after supplements expire. However, since this type searches at much higher rates after expiration relative to the exponential type, it exits more rapidly and so its share of the population declines, as shown in Panel (e). This composition effect leads to a decline in the average job finding rate which is inconsistent with the data.[65]

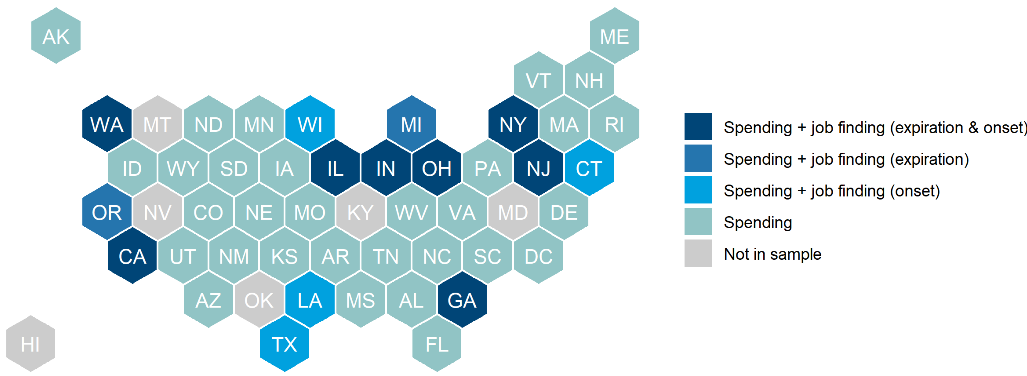
The reason this model implies counterfactual asymmetry in spending is because households respond more strongly to an unexpected change in income (the start of $600) than to an expected change in income (the end of $600 with perfect foresight). This is especially true for the standard consumers without hyperbolic discounting, but holds even for the hyperbolic households because even these households save some small amount in anticipation of expiration.

The myopic expectations in our best-fit model fix both of these issues: if households are surprised by the benefit expiration then search patterns shift in a sustained way after expiration, and spending responses to the start and end of benefits are more similar when both are a surprise.

[65]The type shown in red searches at a high rate throughout the sample period (which is helpful for matching the initial average level of job finding) but few of these households remain by the time of supplement expiration.

Additional Figures and Tables

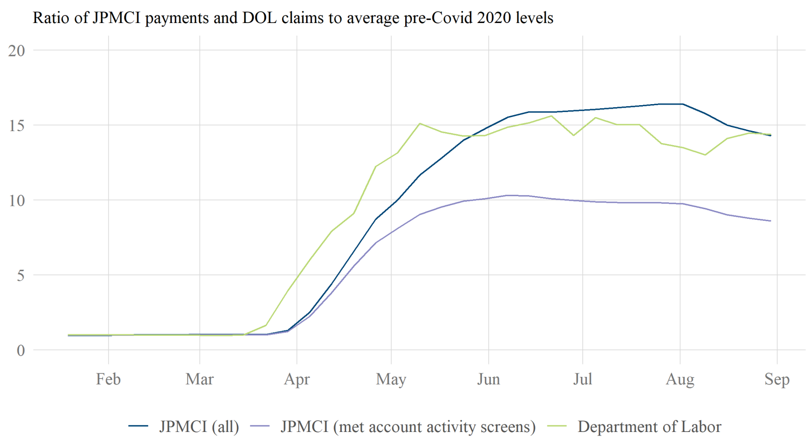
Figure A-1: States included in the JPMCI sample



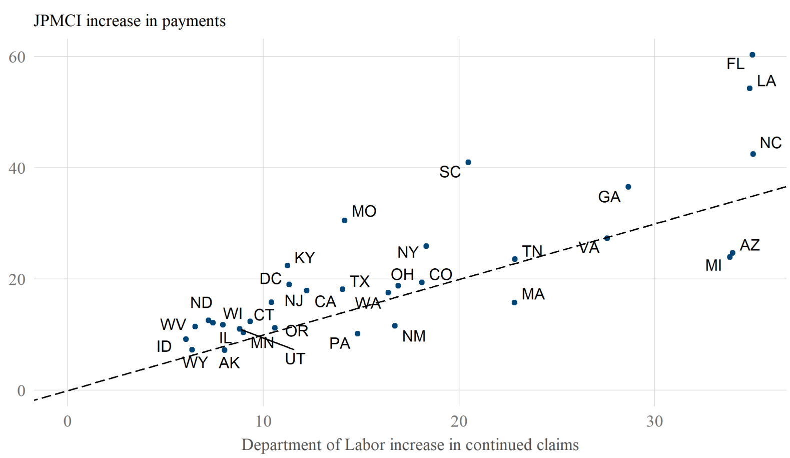
Notes: This figure shows the subset of states which are included in various analyses. See Appendix A.2 for details.

Figure A-2: UI Claims in JPMCI versus DOL

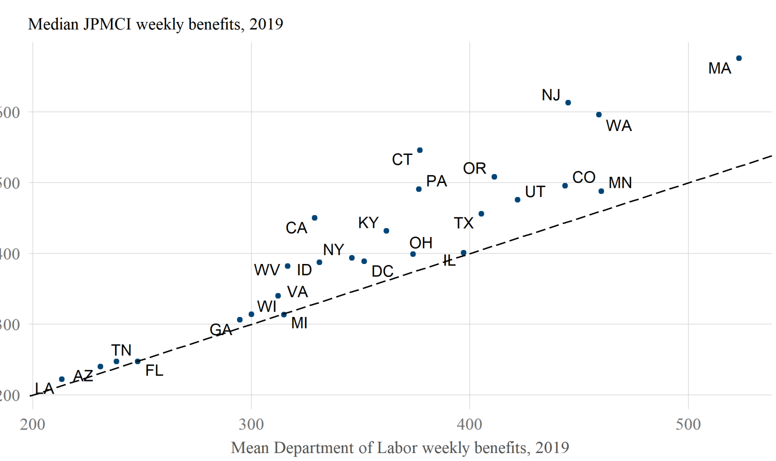
(a) Time-series



(b) State-level Change at Pandemic Onset

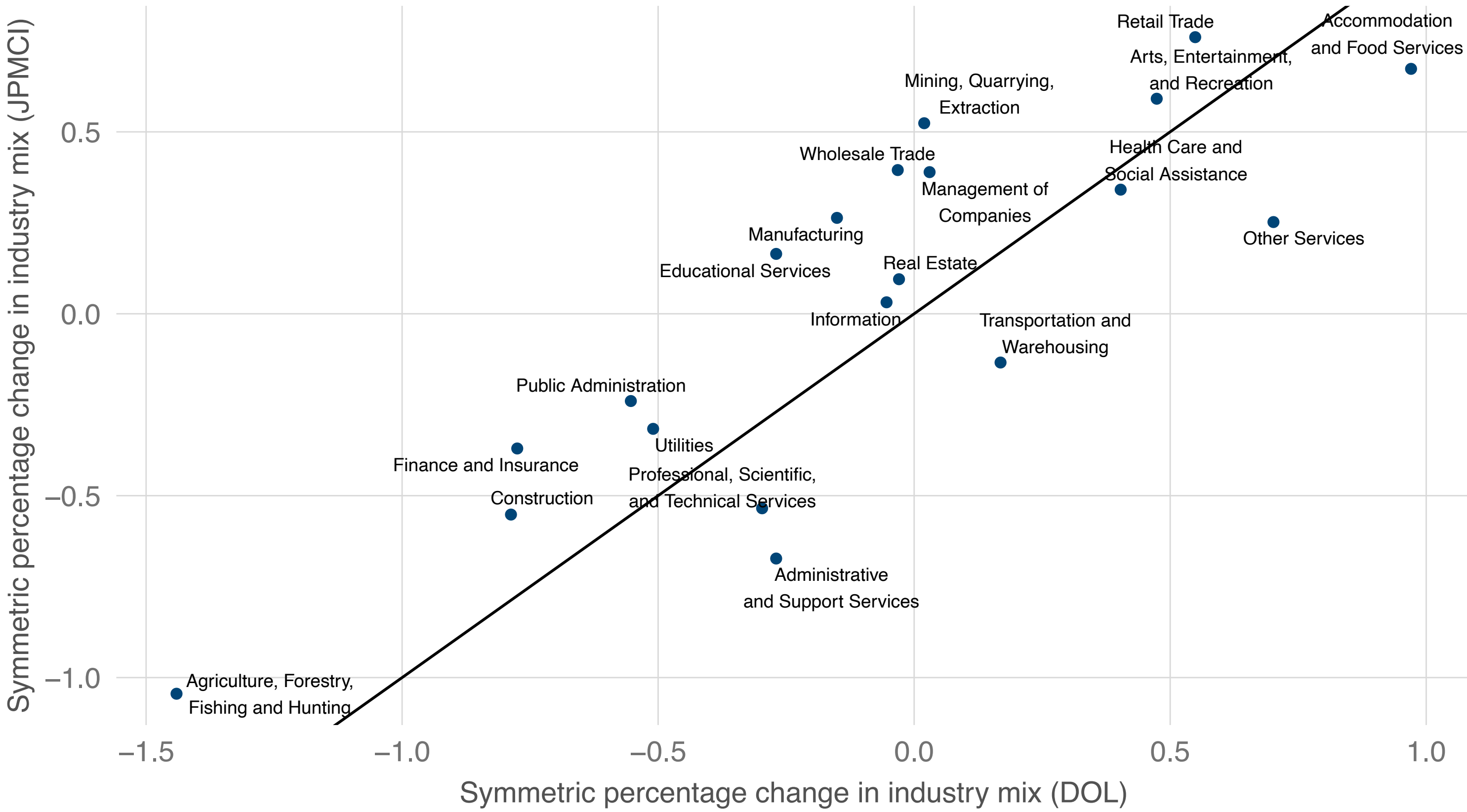


(c) Weekly Benefit Amount Pre-Pandemic



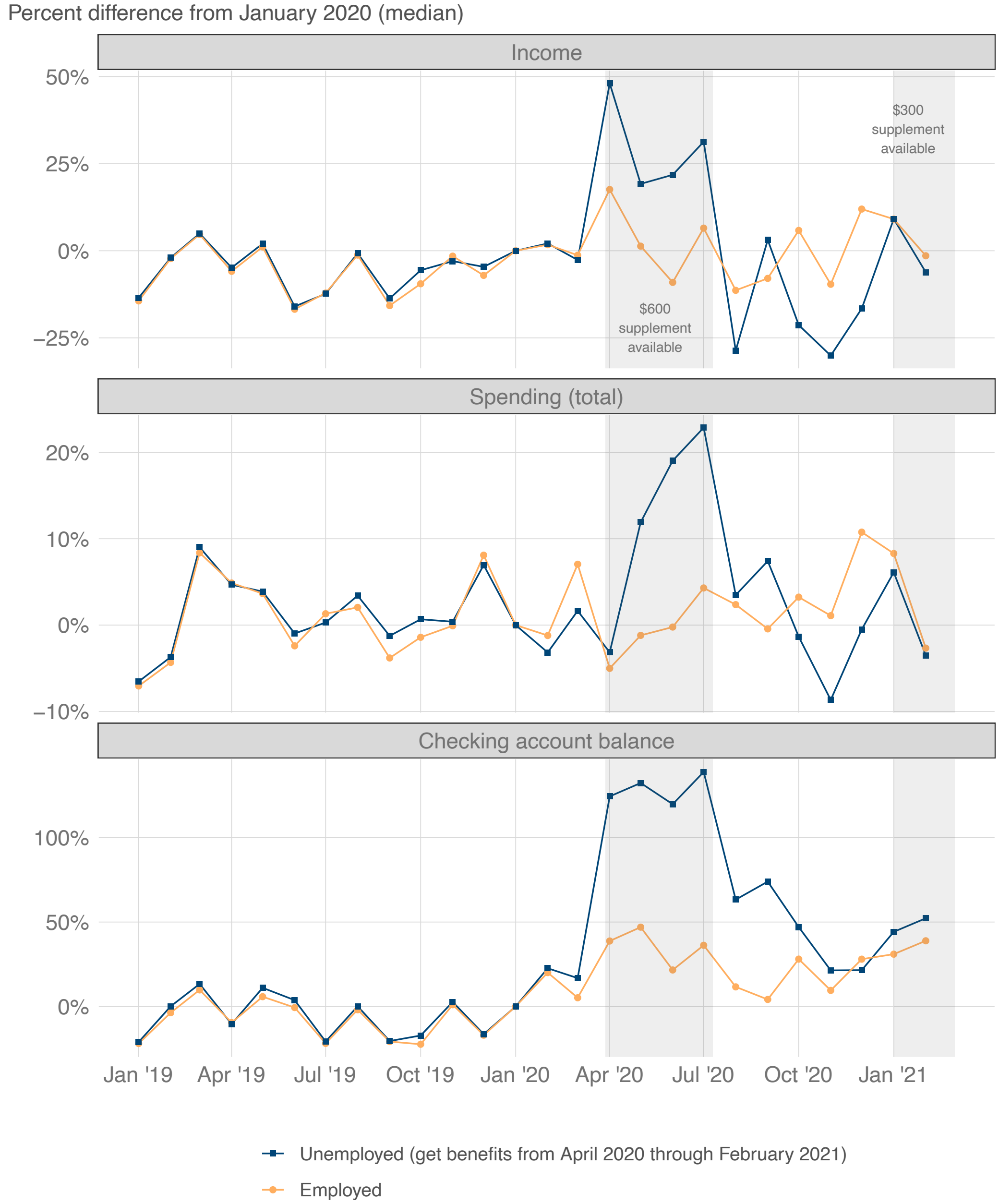
Notes: This figure compares total claims and benefits levels in JPMCI data to Department of Labor ETA Form 539 Employment and Training Administration (2019 - 2020c) and Form 5159 Employment and Training Administration (2019 - 2020b). Panel (a) compares aggregate time-series patterns, panel (b) compares state-by-state changes, and panel (c) compares benefit levels by state. Panel (b) depicts the ratio of the number of payments in May 2020 to the number of payments in 2020 prior to the declaration of national emergency. Panels (b) and (c) include a 45-degree line and drop states with less than 300 observations.

Figure A-3: Changes in Industry Composition of Unemployment, JPMCI vs DOL



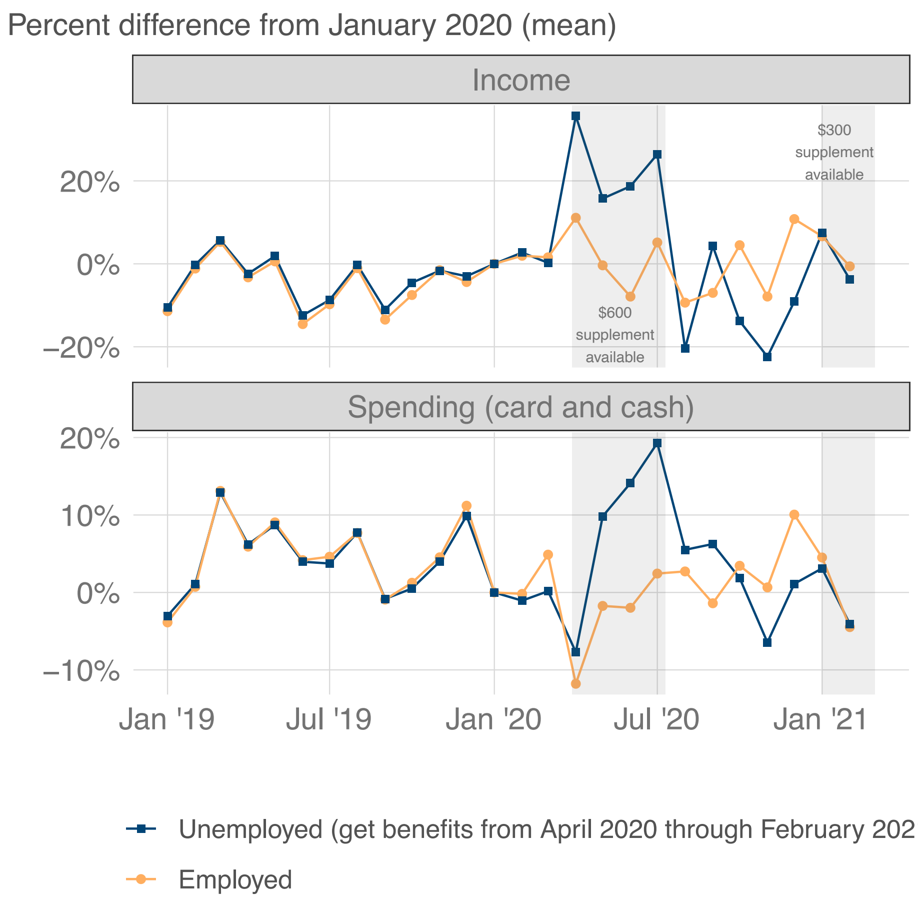
Notes: This figure compares the change in unemployment composition in DOL to JPMCI. The diagonal line is a 45-degree line. Making these data sets comparable requires two adjustments. Letting i denote industry, t denote period and s state, we start by defining total JPMCI claims as c_{it}^{JPMCI} and state-specific claims from DOL ETA Form 203 as c_{ist}^{DOL} (Employment and Training Administration 2019 - 2020a). Form 203 excludes recipients of federal programs for the long-term unemployed, so we drop recipients in JPMCI data with spells > 26 weeks. We also re-weight the DOL data to account for the fact that Chase has more customers in some states than others using weight $w_{st} = \frac{c_{st}^{JPMCI}}{\sum_{s'} c_{s't}^{JPMCI}}$. We then measure industry share in JPMCI as $p_{it}^{JPMCI} = \frac{c_{it}^{JPMCI}}{\sum_{i'} c_{i't}^{JPMCI}}$ and industry share in DOL as $p_{it}^{DOL} = \frac{\sum_s (w_{st} c_{ist}^{DOL})}{\sum_{i'} \sum_s (w_{st} c_{i'st}^{DOL})}$. Unsurprisingly, UI claim shares by industry differ between the two datasets ("Construction" and "Agriculture" are most under-represented in JPMCI and "Administrative and Support Services" and "Finance and Insurance" are most over-represented) but we are primarily interested in the extent to which the *changes* in the composition of unemployment during the pandemic appear in the JPMCI data. To quantify this, we analyze the shift in the composition of UI claims from pre-covid (January 2019 to March 2020) to the height of the pandemic (April 2020 to December 2020). Because the increases in UI claims are so large (and therefore the changes in proportions are highly skewed), we measure composition changes with the symmetric percent change: $2 \times \frac{p_{it} - p_{i,t-1}}{p_{it} + p_{i,t-1}}$.

Figure A-4: Income, Spending, and Balances of Unemployed Versus Employed (Median)



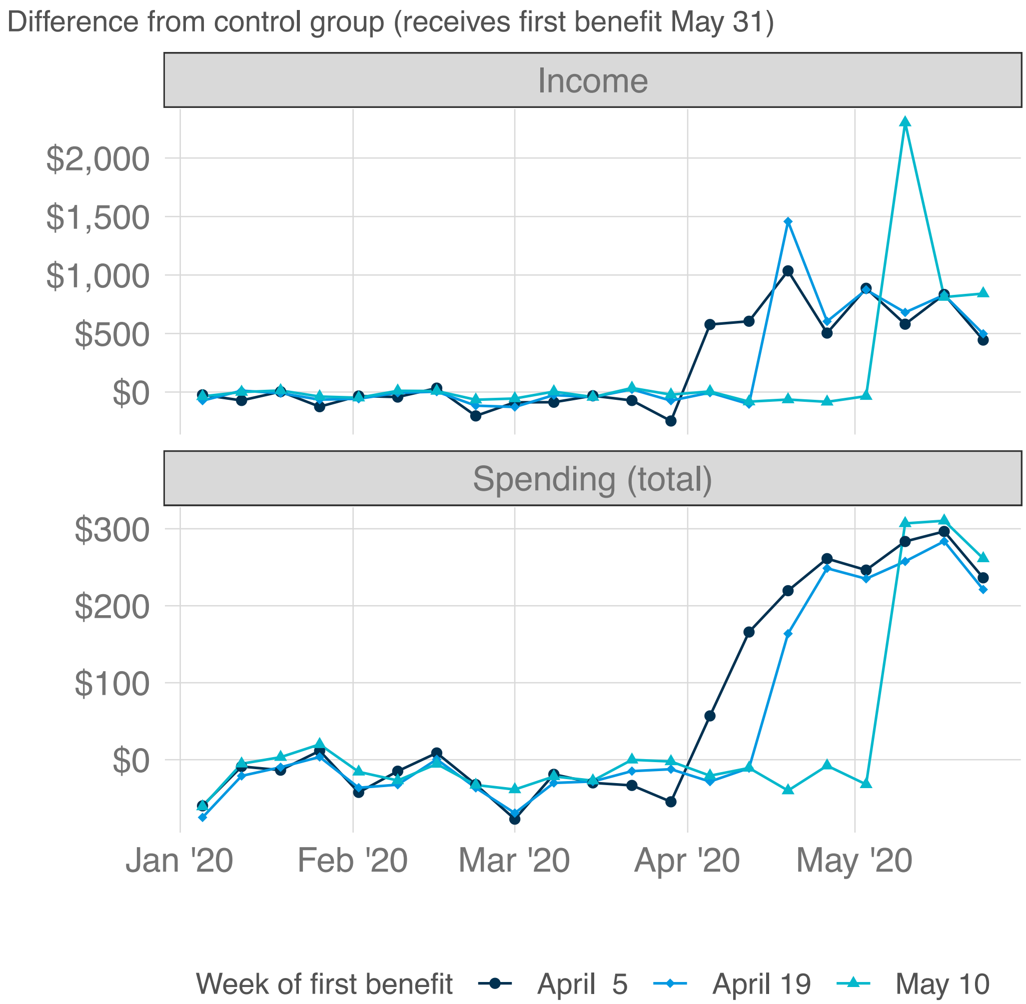
Notes: This figure replicates Figure 1 using sample medians instead of means.

Figure A-5: Spending of Unemployed Versus Employed (card and cash)



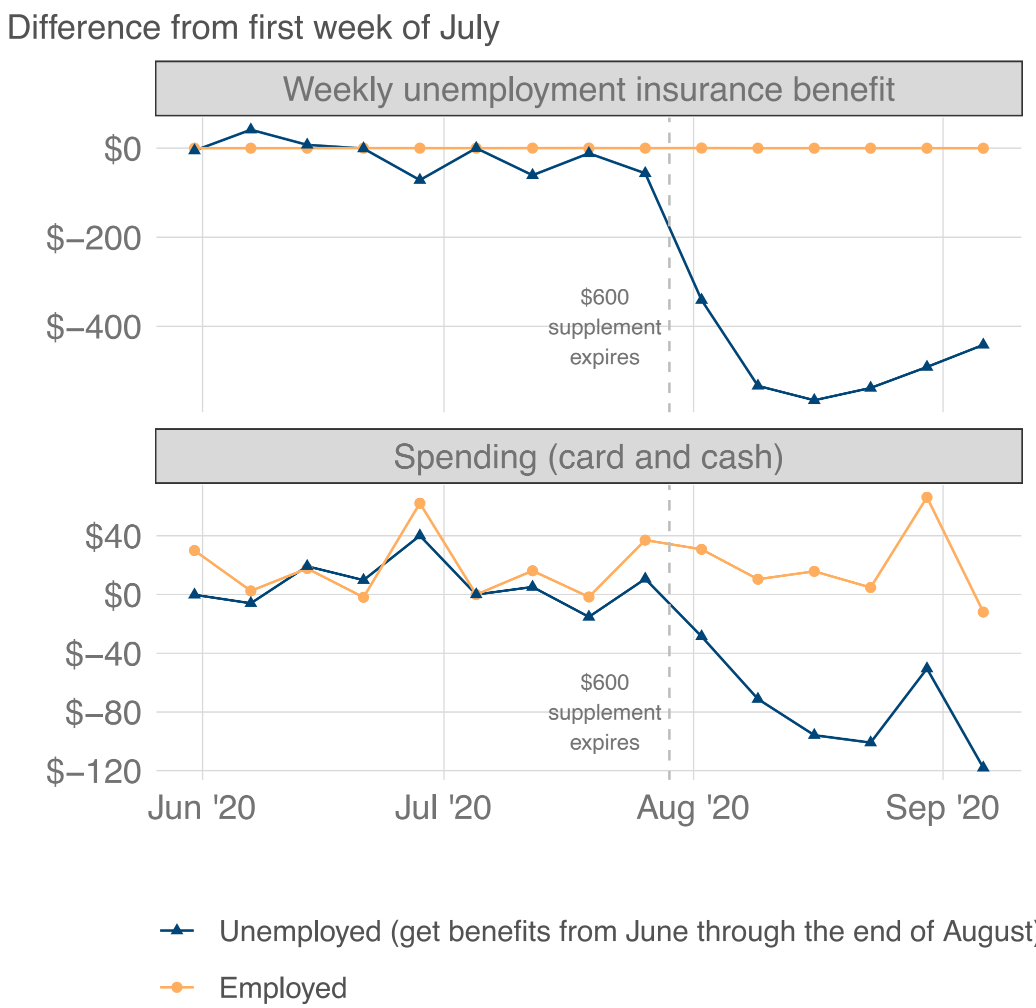
Notes: This figure shows that the spending (total) patterns in Figure 1 also hold for spending (card and cash).

Figure A-6: Impact of Delays in Unemployment Benefits on Spending (Total, Differences)



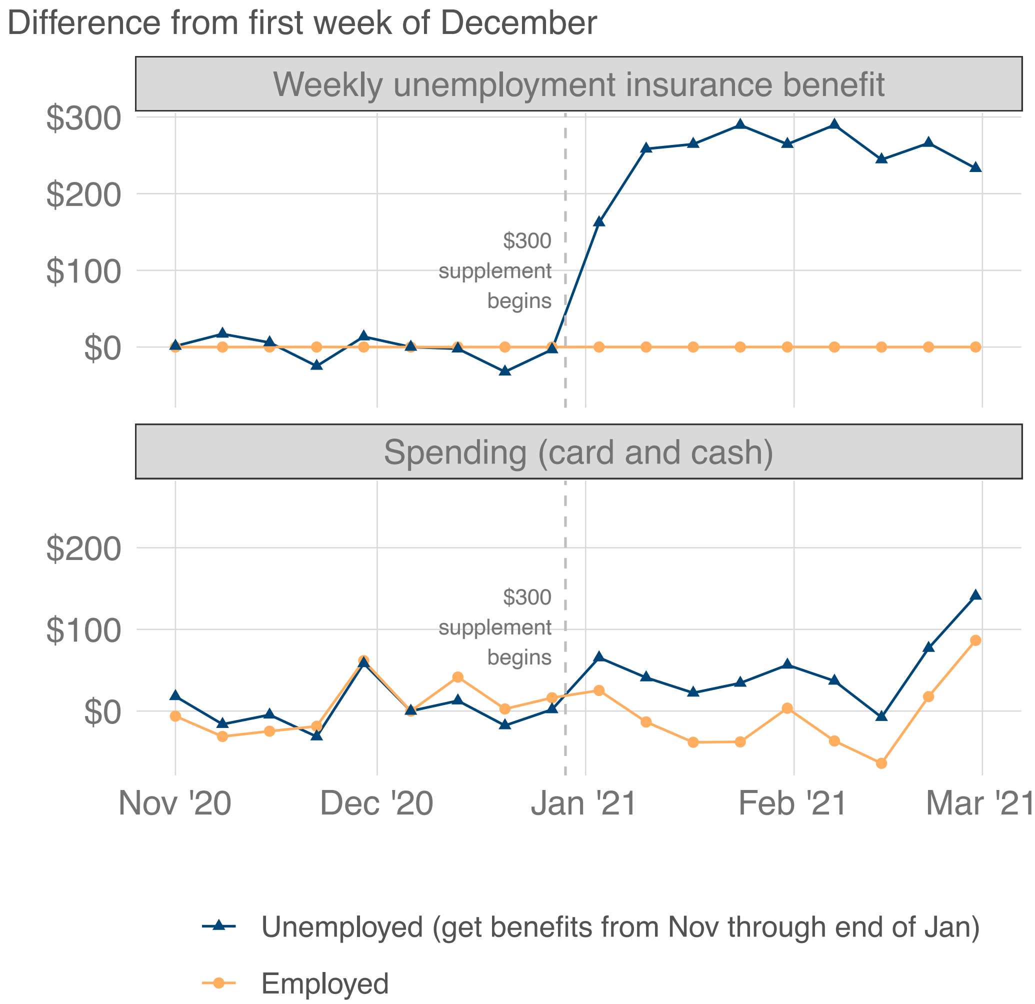
Notes: This figure shows mean income and spending (total) differences from the May 31 control group for various cohorts in the waiting for benefit receipt research design. Our MPC is based on the April 5th treatment group, which has no benefit delay.

Figure A-7: Impact of Expiration of the $600 Supplement on Spending (Card and Cash)



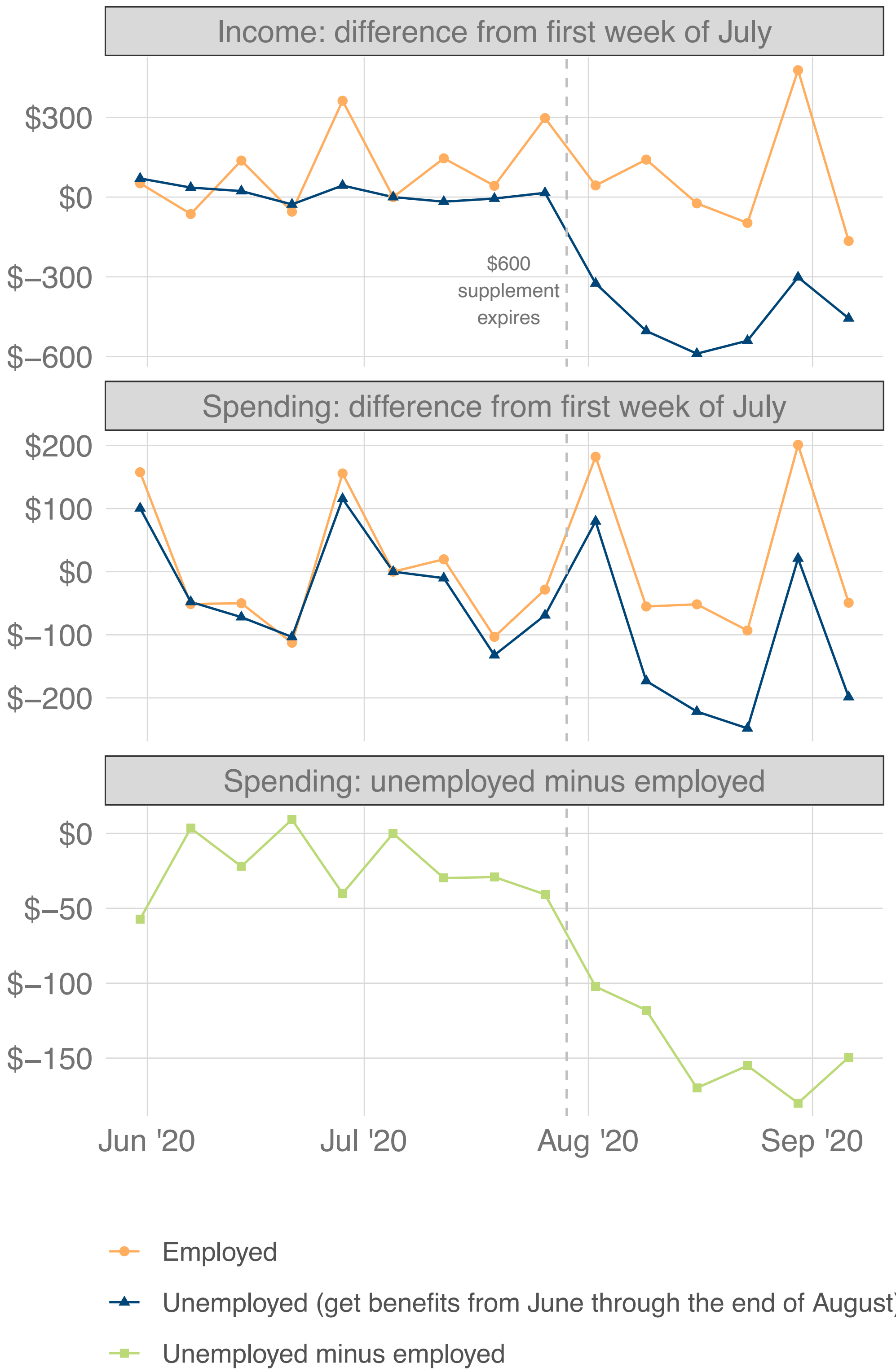
Notes: This figure measures the causal impact of the expiration of the $600 supplement on spending. The benefit amount declines over two weeks in August (rather than one week) because some states pay benefits once every two weeks and therefore paid out the supplement for the last week of July during the first week of August. The benefit amount rises in September because states begin to pay the temporary $300 supplement. The control group is employed workers who are matched on 2019 income levels as well as on the date of stimulus check receipt. The dependent variables are mean benefits and mean spending, measured as a change relative to the first week of July. See Section 3.2.2 for details.

Figure A-8: Impact of Onset of the $300 Supplement on Spending (Card and Cash)



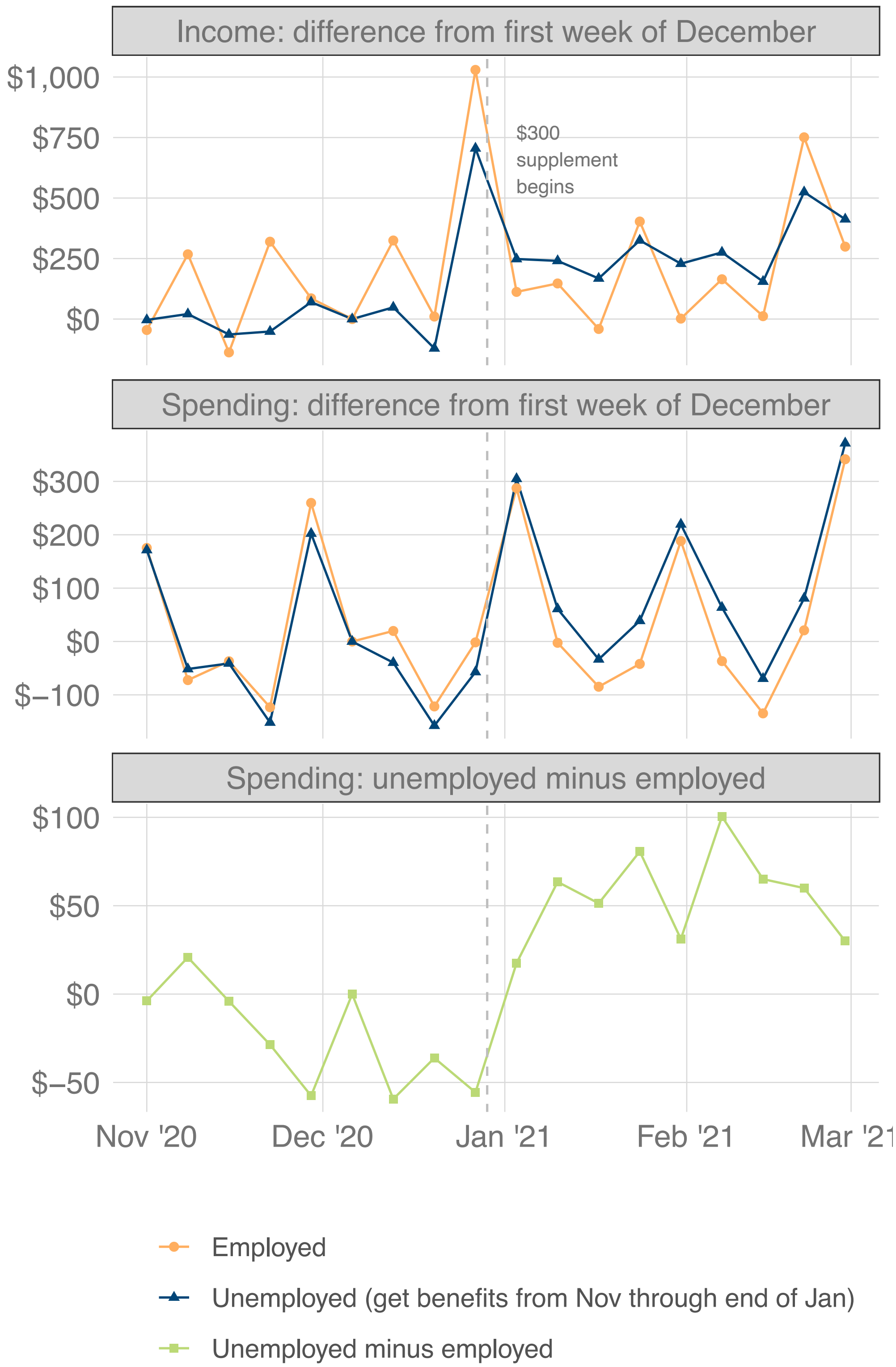
Notes: This figure measures the causal impact of the onset of the $300 supplement on spending. The benefit amount rises over two weeks in January (rather than one week) because some states pay benefits once every two weeks. The control group is employed workers who are matched on 2019 income levels as well as on the date of stimulus check receipt. The dependent variables are mean benefits and mean spending, measured as a change relative to the last week of December. The figure depicts November 2020 through March 2021. See Section 3.2.2 for details.

Figure A-9: Impact of Expiration of the $600 Supplement on Spending (Total)



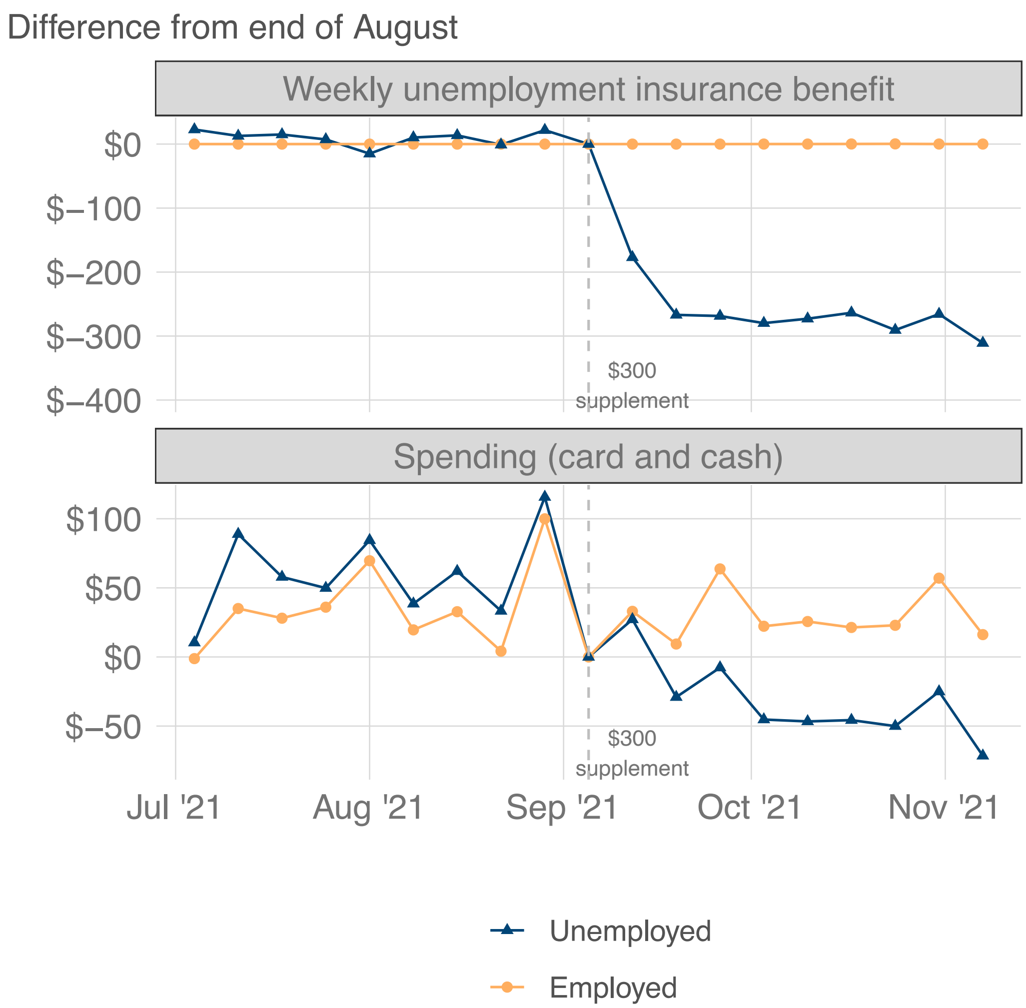
Notes: This figure measures the causal impact of the expiration of the $600 supplement on spending. The benefit amount declines over two weeks in August (rather than one week) because some states pay benefits once every two weeks and therefore paid out the supplement for the last week of July during the first week of August. The benefit amount rises in September because states begin to pay the temporary $300 supplement. The control group is employed workers who are matched on 2019 income levels as well as on the date of stimulus check receipt. The dependent variables are mean benefits and mean spending, measured as a change relative to the first week of July. Spending is noticeably higher in weeks which contain the first of the month, likely because many households pay bills at this time. See Section 3.2.2 for details.

Figure A-10: Impact of Onset of the $300 Supplement on Spending (Total)



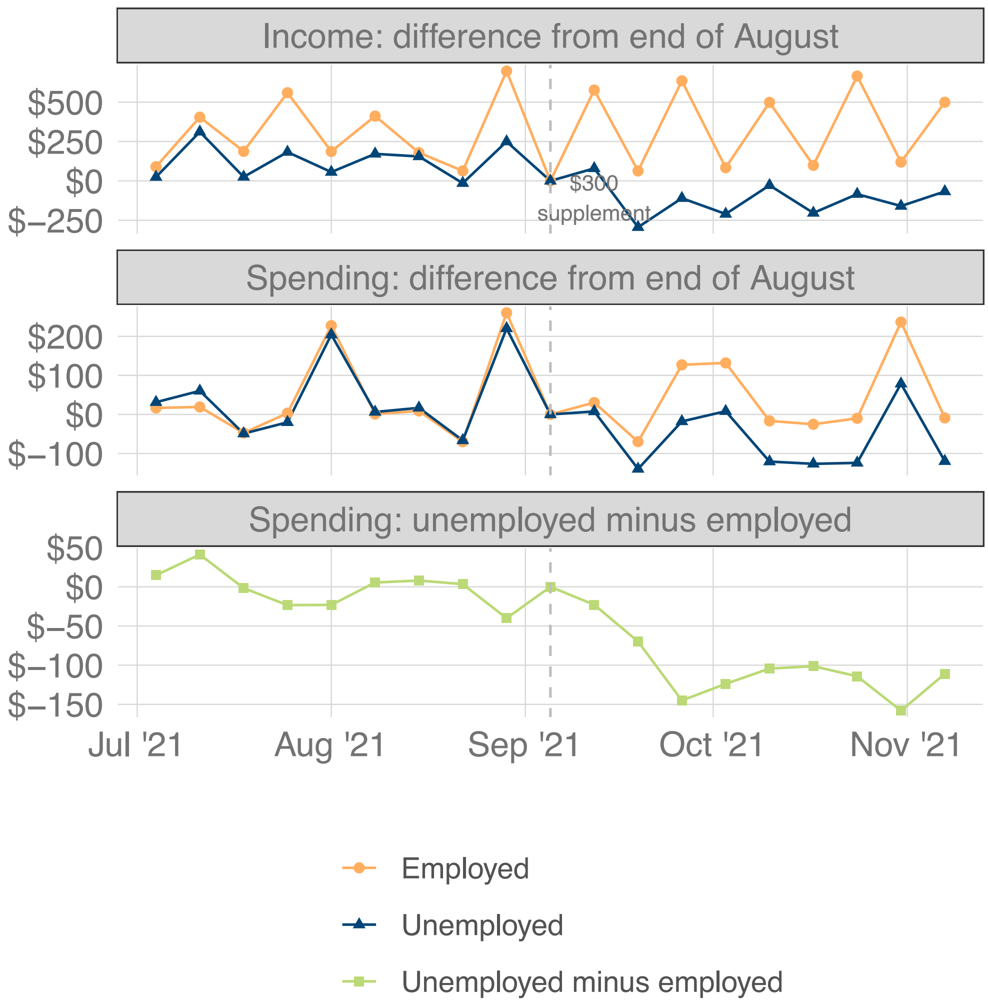
Notes: This figure repeats Figure A-8 but for total income and spending. The control group is employed workers who are matched on 2019 income levels as well as date of receipt of stimulus checks. The dependent variables are mean benefits and mean spending, measured as a change relative to the last week of December. The figure depicts November 2020 through March 2021. Spending is noticeably higher in weeks which contain the first of the month, likely because many households pay bills at this time, so the third panel differences out these high-frequency fluctuations. Income for employed households similarly exhibits spikes at the end of the month for the same reason.

Figure A-11: Impact of Expiration of the $300 Supplement on Spending (Card and Cash)



Notes: This figure measures the causal impact of the expiration of the $300 supplement on spending. The benefit amount falls over two weeks in September (rather than one week) because some states pay benefits once every two weeks. The control group is employed workers who are matched on 2019 income levels as well as on the date of stimulus check receipt. The dependent variables are mean benefits and mean spending, measured as a change relative to the first week of September. The figure depicts July 2021 through November 2021.

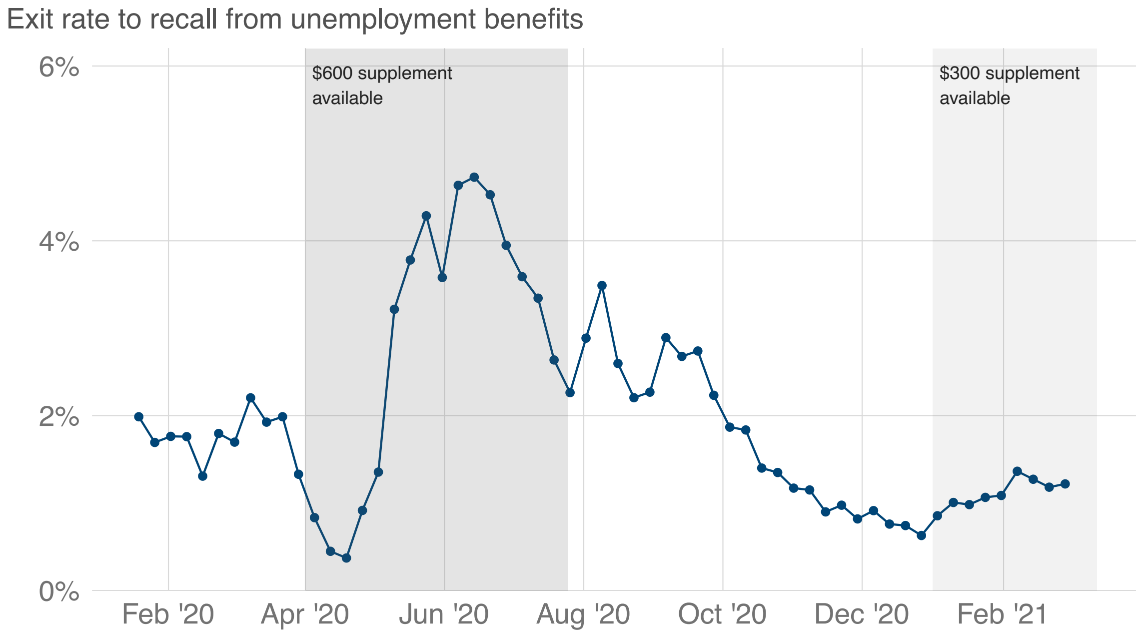
Figure A-12: Impact of Expiration of the $300 Supplement on Spending (Total)



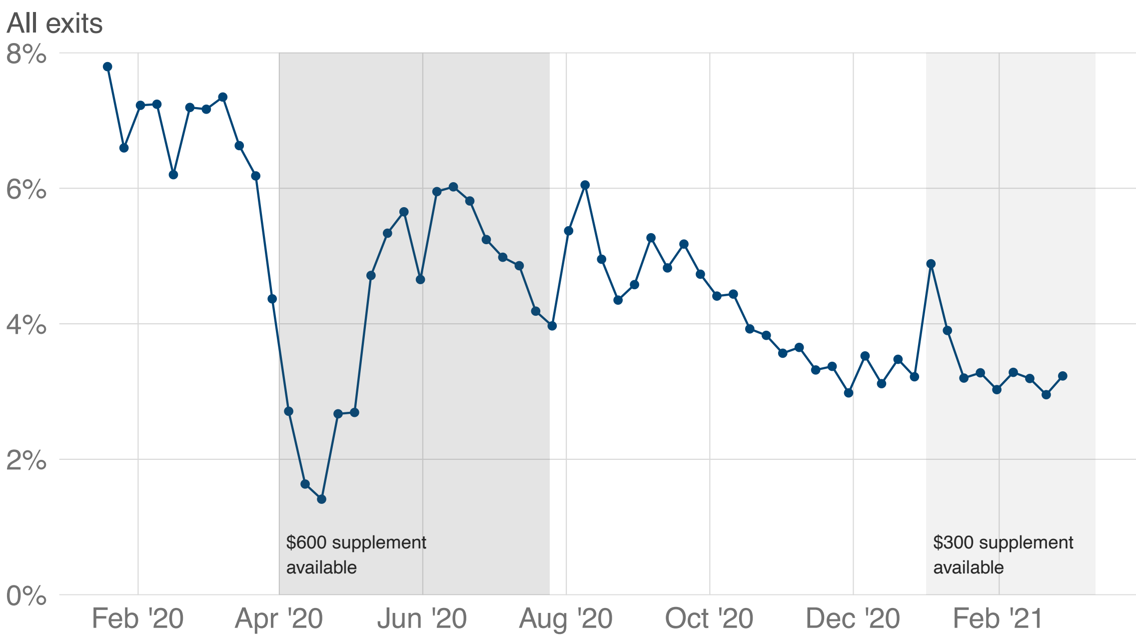
Notes: This figure repeats Figure A-11 but for total income and spending. The control group is employed workers who are matched on 2019 income levels as well as date of receipt of stimulus checks. The dependent variables are mean benefits and mean spending, measured as a change relative to the first week of September. The figure depicts July 2021 through November 2021. Spending is noticeably higher in weeks which contain the first of the month, likely because many households pay bills at this time, so the third panel differences out these high-frequency fluctuations. Income for employed households similarly exhibits spikes at the end of the month for the same reason.

Figure A-13: Exit Rate from Unemployment Benefits

(a) Recall

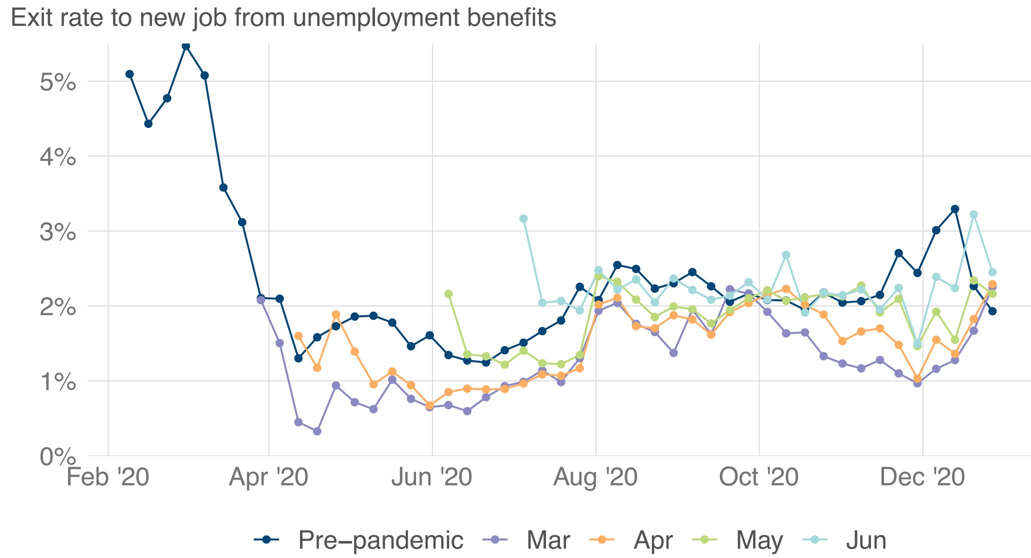


(b) Total (New Job + Recall)



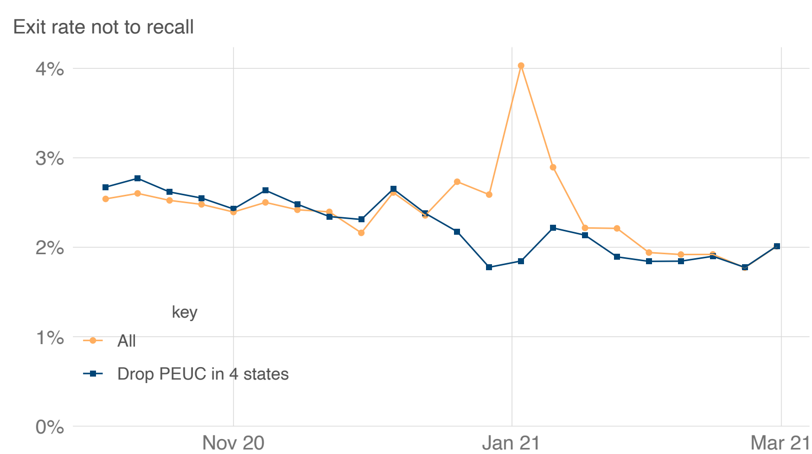
Notes: This figure shows the exit rate to recall and the total exit rate in the JPMCI data. UI exit is defined as three contiguous weeks without receipt of UI benefits. Recall is measured using receipt of labor income from a prior employer. Exit rate to new job is from Figure 4.

Figure A-14: Exit Rate by Start Date of Unemployment Benefit Spell



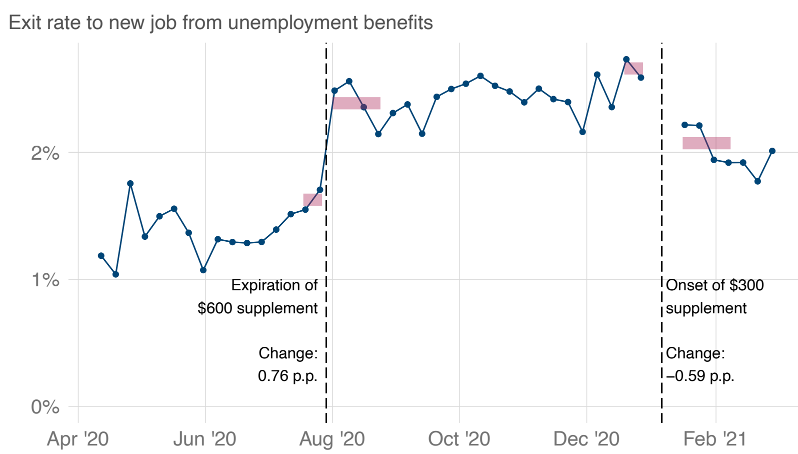
Notes: We define the pre-pandemic group as those who started receiving unemployment insurance benefits during or before the week of March 15, 2020.

Figure A-15: Exit Rate at Expiration of PEUC



Notes: This figure shows the evolution of the exit rate not to recall from October 2020 through February 2021. The orange series is the same as the one shown in Figure 4, except that here the series includes January 3 and January 10. The y-axis title is 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 blue 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 difference between the blue series and the green series reveals that the lapse triggered a surge in *measured* exits in four states. In additional unreported results, we find that the measured exits in the blue series do not show evidence of starting a new job via direct deposit of payroll from a new employer. We therefore omit January 3 and January 10 from the plot in Figure 4.

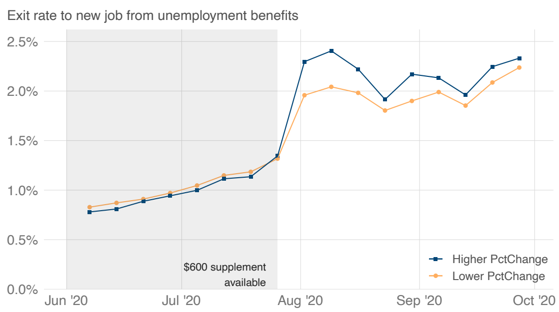
Figure A-16: Effect of Expanded Benefits on Job-Finding: Interrupted Time-series Design



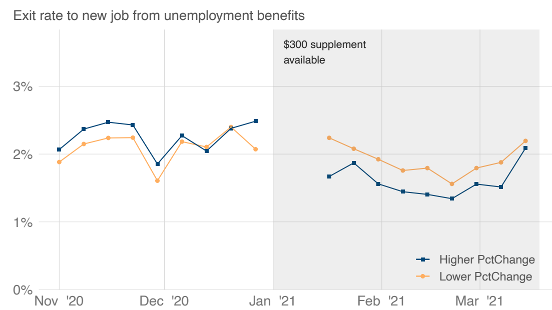
Notes: This figure shows the exit rate to a new job in the JPMCI data from April 2020 through February 2021. The red horizontal bars indicate the average exit rate in the two weeks prior to and four weeks following a change in the supplement amount.

Figure A-17: Exit Rate by Replacement Rate

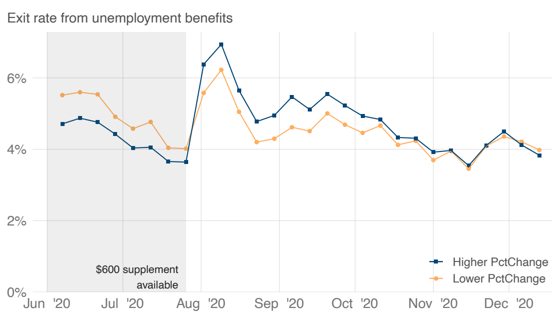
(a) Expiration – New Job



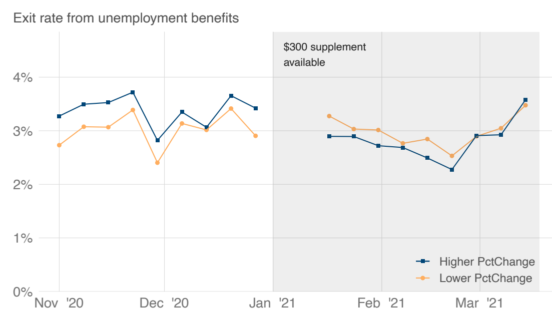
(b) Onset – New Job



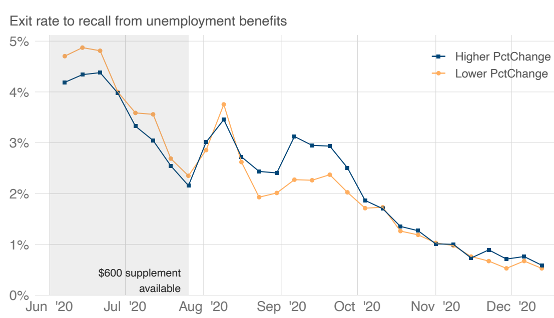
(c) Expiration – Total



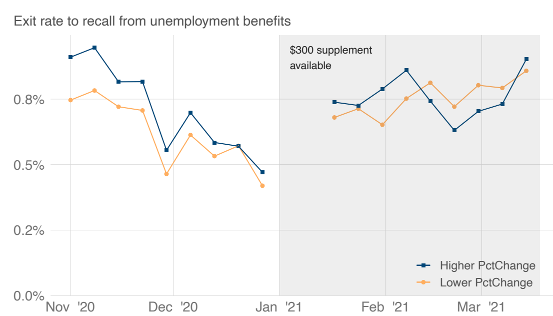
(d) Onset – Total



(e) Expiration – Recall

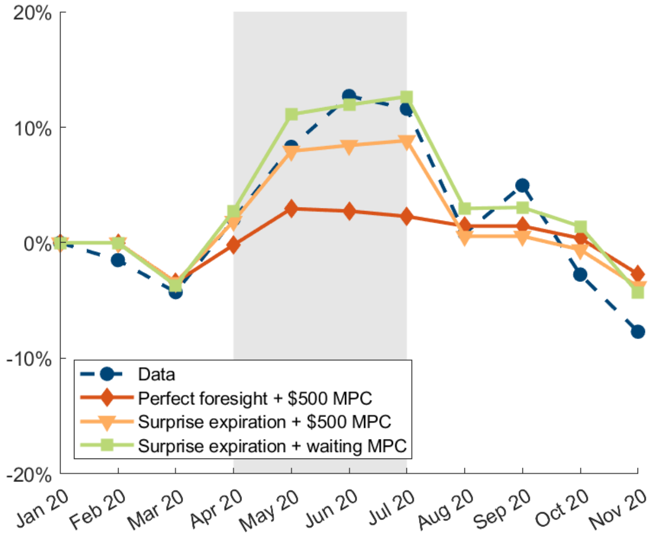


(f) Onset – Recall



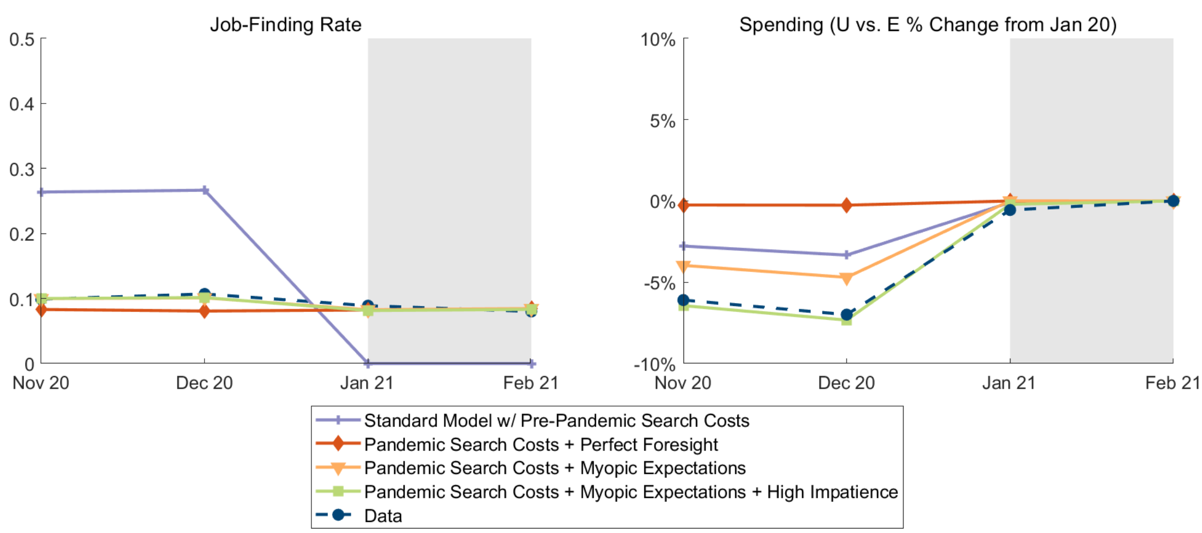
Notes: This figure shows several alternative specifications for Figure 6. Panels (a) and (b) report raw (unnormalized) exit rates to new job. Panels (c) and (d) report "total" exit rates including exit to new job and exit to recall. Panels (e) and (f) report exit rates to recall.

Figure A-18: Spending in Model that Accounts for Time-Aggregation



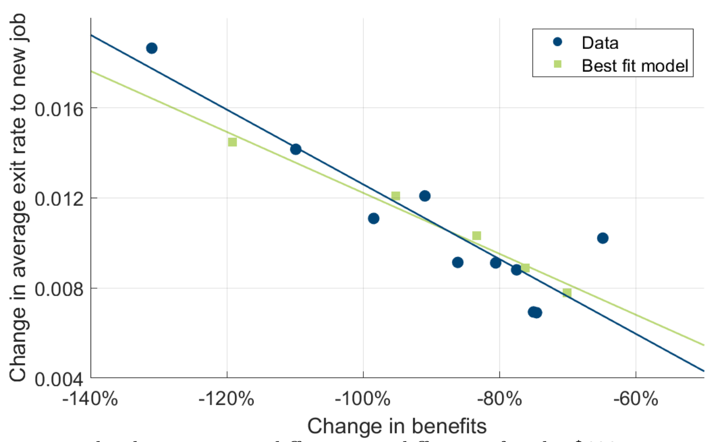
Notes: This figure shows the behavior of spending in the model more closely matches high-frequency data patterns after accounting for time-aggregation issues.

Figure A-19: Job-Finding and Spending Responses to $300 Supplement: Models vs. Data



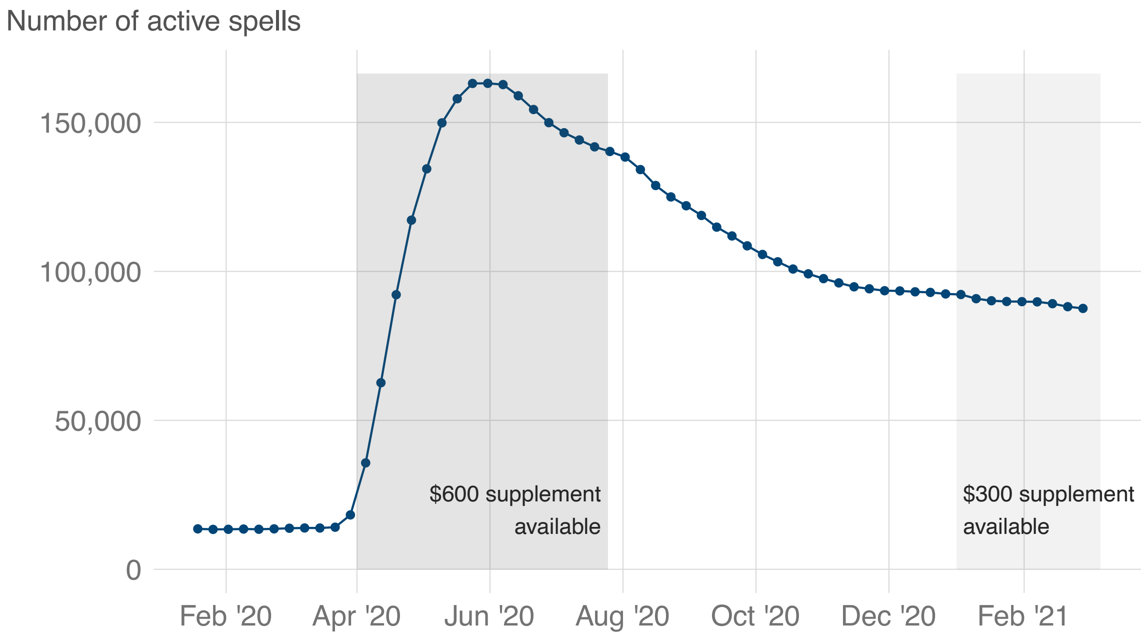
Notes: This figure repeats Figure 9 but showing models vs. data in response to the $300 supplement. In all models except the perfect foresight specification (including the standard model with pre-pandemic search costs), we assume the start of the $300 supplements in January is a surprise. We assume this even in the pre-pandemic model since this is the more natural baseline expectations assumption, as it is unlikely households anticipated the announcement of this policy two months in advance.

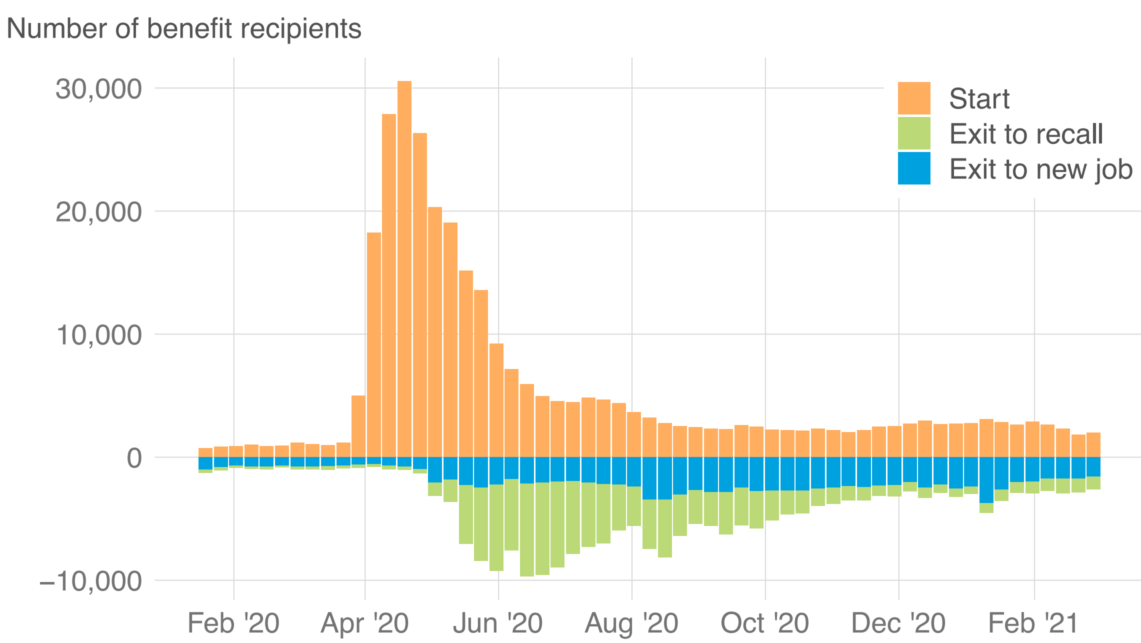
Figure A-20: Model vs. Data Dose-Response Difference-in-Difference



Notes: This figure compares the dose-response difference-in-difference for the $600 expiration in our best fit model to the empirical difference-in-difference.

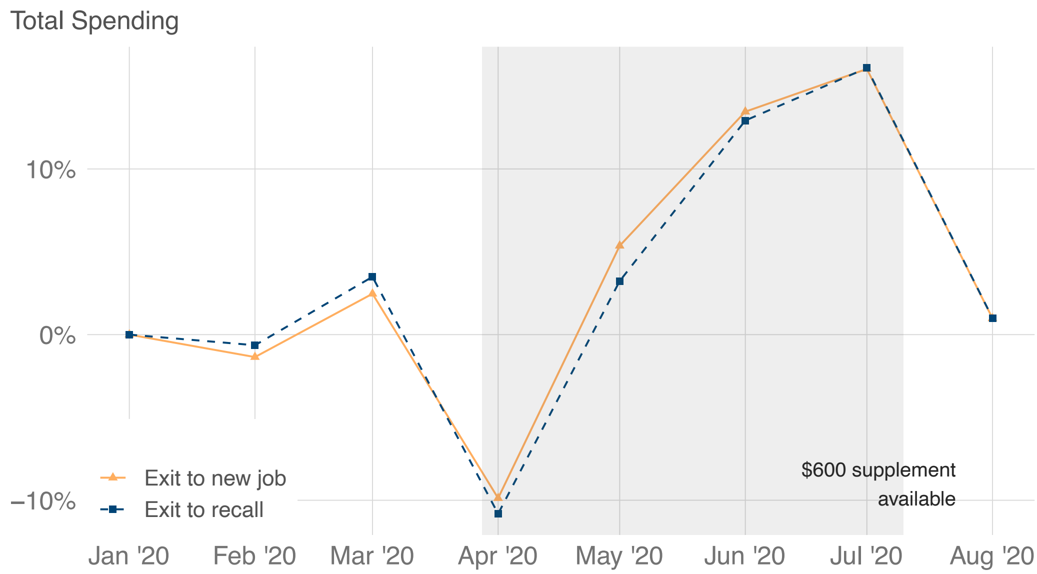
Figure A-21: Patterns of Unemployment Insurance Receipt





Notes: The first panel of this figure reports the number of active unemployment spells by week in JPMCI data. The second panel shows the number of households starting unemployment and leaving unemployment for new jobs and for recall (i.e., returning to their former employer).

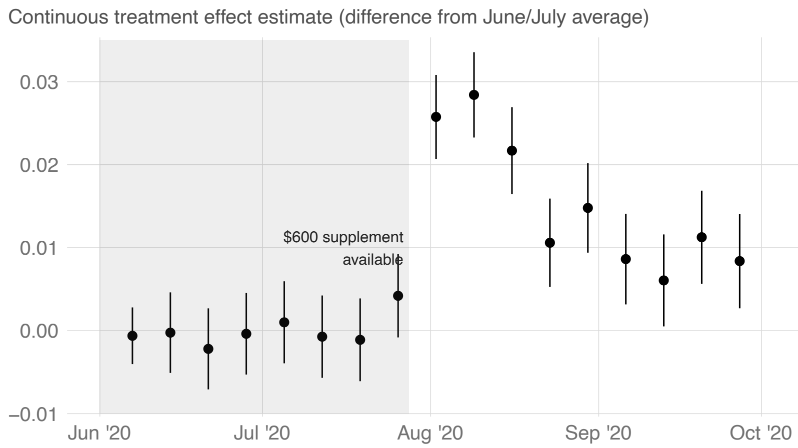
Figure A-22: Spending of Recalled vs. Non-Recalled Workers



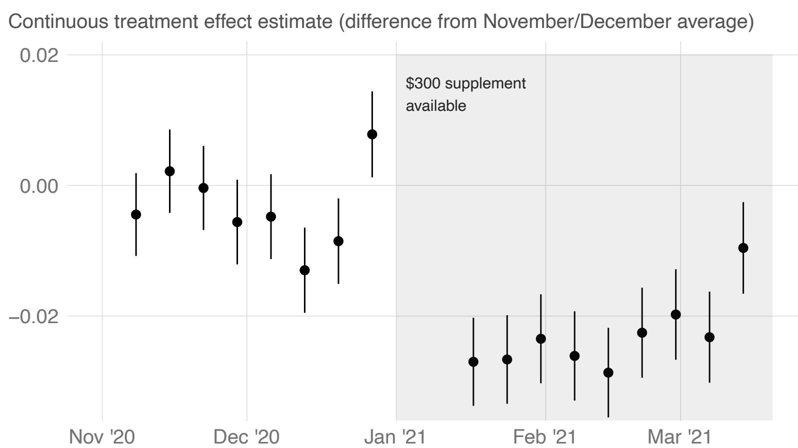
Notes: This figure shows total spending for two different groups of workers who are unemployed continuously from April 2020 through August 2020 and exit unemployment in September 2020. The "Exit to recall" group returns to their previous employer when they exit unemployment while the "Exit to new job" group exits to a new employer.

Figure A-23: Weekly Event Study Coefficients (Continuous Specification)

(a) Expiration of $600



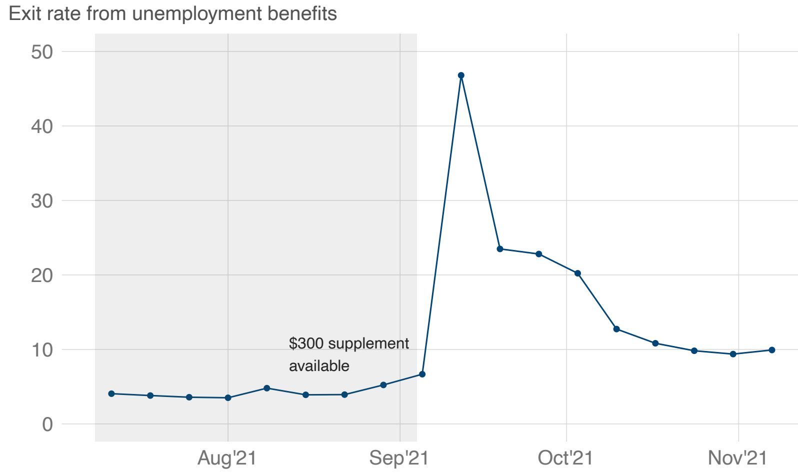
(b) Onset of $300



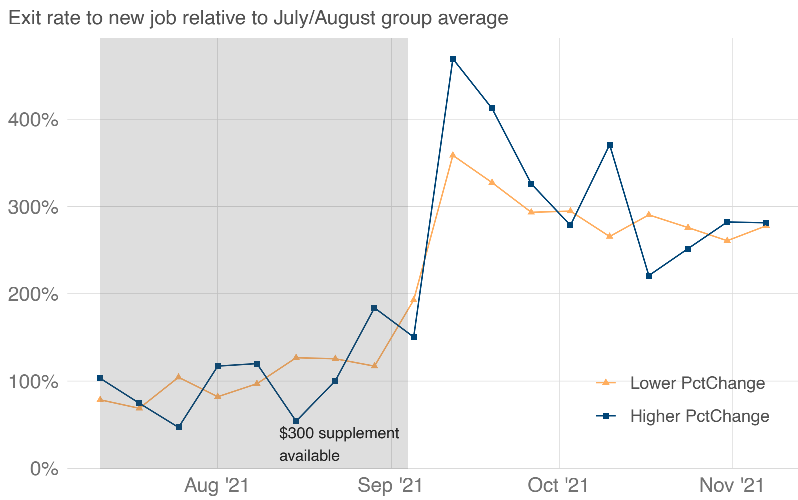
Notes: This figure shows the results from the weekly difference-in-difference specification defined in Section C.5.1 equation (C1). This specification captures the effect of supplements on job-finding in each week around the supplement change. Panel (b) indicates that even prior to the onset of the $300 supplement there is already a gradual trend downward in the job-finding rate for households that receive the largest increase in benefits on January 1, 2021. If we were to use a specification that accounted for this pre-trend in estimation we would likely find that the $300 supplement has even smaller effects on the job-finding rate.

Figure A-24: Exit Rates Around Expiration of Multiple Programs in September 2021

(a) Raw Exit Rate

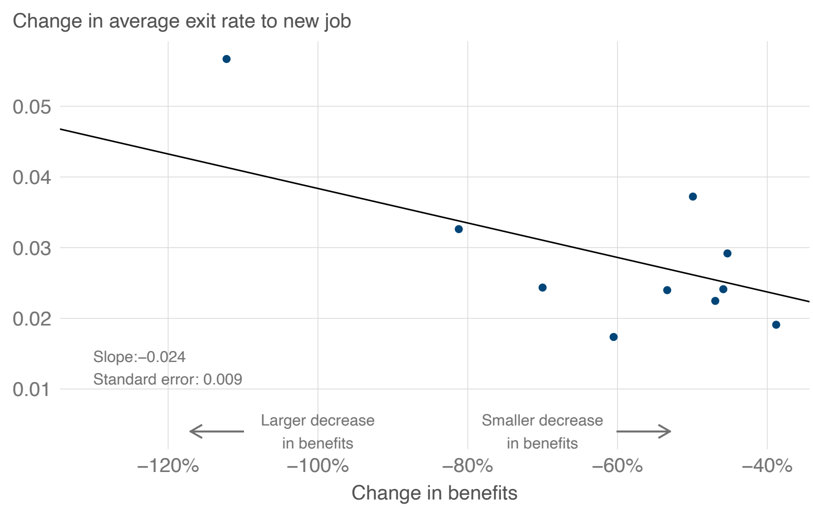


(b) Cleaned Exit Rate by Benefit Level



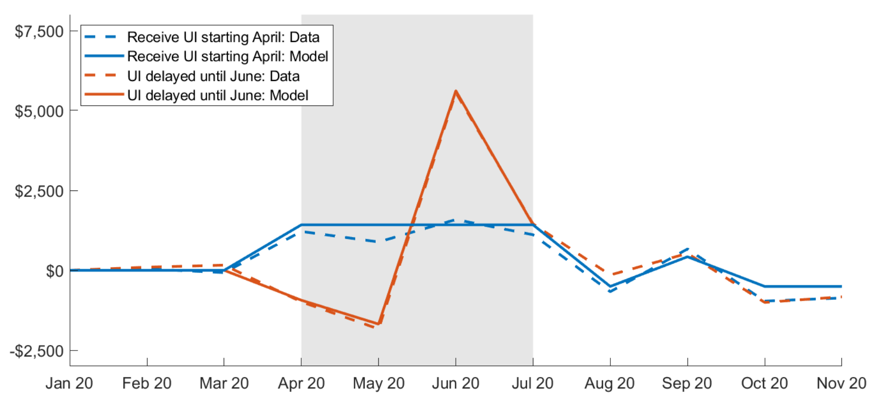
Notes: The top panel of this figure shows the raw exit rate from UI benefits. The surge in exits is caused by the expiration of multiple programs (see Appendix A.1 for details). The bottom panel of this figure replicates Figure 6 for the expiration of the $300 supplement in September 2021, for the subset of workers with short observed duration.

Figure A-25: Effect of Expanded Benefits: Binscatter (Summer 2021)



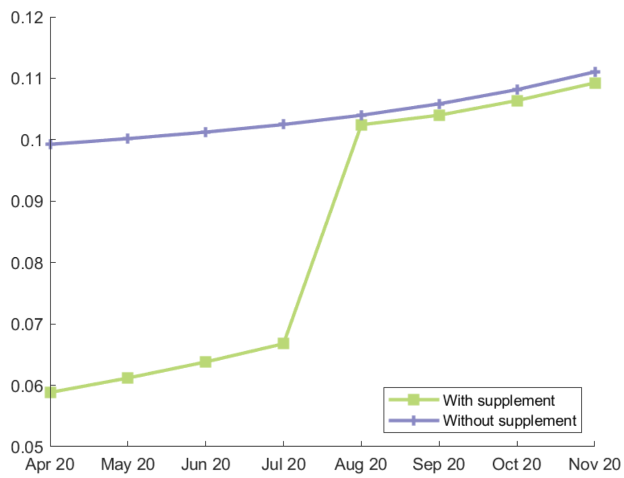
Notes: This figure replicates Figure 7 for the expiration of the $300 supplement in September 2021.

Figure A-26: Income: Model vs. Data



Notes: This figure shows income of unemployed (relative to employed) households in the model and data for unemployed workers receiving benefits immediately in April 2020 as well as those who face a delay in benefit receipt until June 2020.

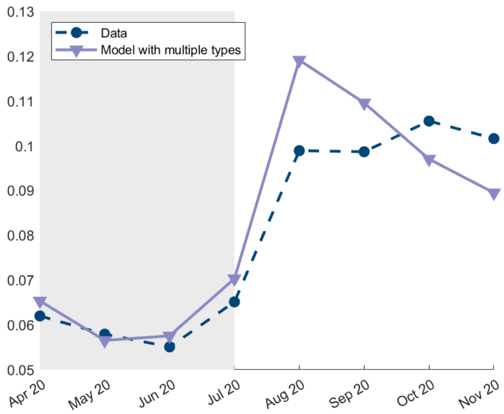
Figure A-27: Job-Finding Rates with and without \$600 supplements



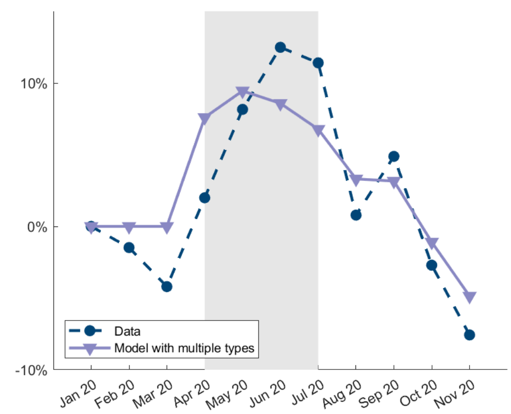
Notes: This figure shows the job-finding rate in the best-fit model with a \$600 supplement compared to a counterfactual job-finding rate had there been no supplement. This shows that liquidity accumulation caused by the supplement slightly lowers the job finding rate even after the supplement expires.

Figure A-28: Model With Multiple Types and Perfect Foresight

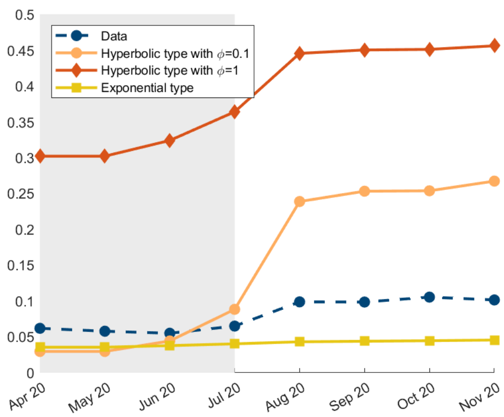
(a) Job Finding - Average



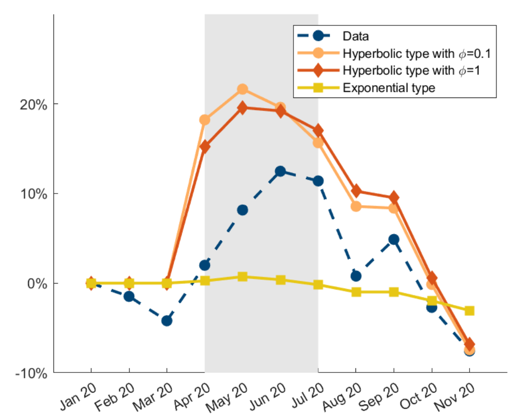
(b) Spending - Average



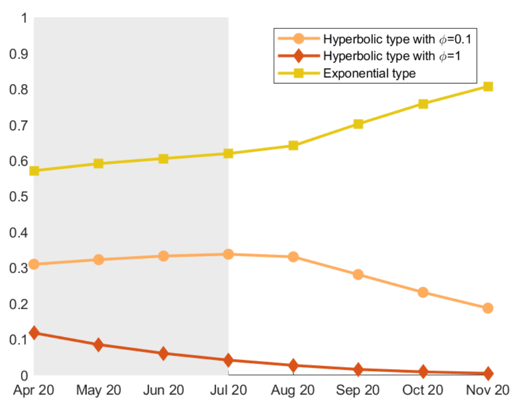
(c) Job Finding - By Type



(d) Spending - By Type



(e) Shares of Each Type By Month



Notes: This figure shows job-finding and spending behavior in a model with perfect foresight and heterogeneity in both discounting and job-search costs. Panels (a) and (b) show behavior averaging across all types, panels (c) and (d) show the behavior of each of the three types, and panel (e) shows the share of each type in each month. See Section E.2 for details.

Table A-1: Monthly Income and Spending in Employed and Unemployed Households

Group (months)	Income	Benefits	Spending: Card and cash	Spending: Total	Account balance
Mean					
Employed (Jan-Feb 2020)	$6850	$0	$2470	$4669	$5262
Employed (Apr-Oct 2020)	$6839	$0	$2322	$4538	$5884
Pandemic unemployed (Jan-Feb 2020)	$5854	$16	$2506	$4248	$3488
Pandemic unemployed (Apr-Oct 2020)	$7036	$3947	$2780	$4638	$5249
Median					
Employed (Jan-Feb 2020)	$5353	$0	$2064	$3834	$2815
Employed (Apr-Oct 2020)	$5466	$0	$1925	$3739	$3389
Pandemic unemployed (Jan-Feb 2020)	$4549	$0	$2109	$3495	$1624
Pandemic unemployed (Apr-Oct 2020)	$5784	$3834	$2477	$4044	$3242

Notes: This table shows monthly values of income, unemployment benefits, spending (card and cash), spending (total), and checking account balances for employed and unemployed households, before and during the start of pandemic. "Employed" households do not receive benefits or have a job separation from January 2020 through February 2021. "Pandemic unemployed" households begin an unemployment spell in April 2020. A very small number of these households also received benefits in a separate spell which ended prior to April 2020 in January and February 2020, which is why the pre-pandemic mean benefits for this group is $16.

Table A-2: 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	0.021***	0.022***
	(0.001)	(0.001)
SuppAvail	0.003***	0.008***
	(0.001)	(0.001)
PctChange:SuppAvail	−0.016***	−0.020***
	(0.001)	(0.001)
Constant	0.002***	0.009***
	(0.001)	(0.001)
Observations	2,120,887	1,790,138
Number of Households	183,144	131,464

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 (4) using a window of eight weeks prior to and eight weeks 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. Standard errors are clustered at the household level. *p<0.1; **p<0.05; ***p<0.01.

Table A-3: Model Parameters

(a) Parameters Used in All Models

		Parameter	Parameter Value	Target
Preferences	r	Interest Rate	0.04 (annual)	
	γ	Relative Risk Aversion	2	
Income	π	Separation Rate	0.028	Converted from Quarterly CPS transition rates in Krueger, Mitman and Perri (2016)
		Wage Quintiles	0.8154 0.9077 1 1.1080 1.2161	HH Income by Replacement Rate Bins
	h	Secondary Income	0.56	h=HH income U / HH income E when not receiving UI
		Expected Recall Rate	0.08	Monthly Recall Rate 2019
Benefit Rules		Regular Benefit Max Length	6 months	
	b	Regular Benefit Replacement Rate	0.21	h+b = HH income U / HH income E when receiving UI
	m	UI Supplement	0.345	h+b+m = HH income U/ HH income E when receiving UI supplements

(b) Model Specific Choices

Model	**Expectations**	**Monthly** β	**Discount Factor Target**	**Search Parameters**			**Search Target**
				k_0	ϕ	k_1	
Pre-Pandemic	Anticipated Supplement Expiration	0.9905	Quarterly MPC from \$500 check = 0.25	19.2	3.2	0	Duration Elasticity = 0.5 & No supp JF Rate = 0.28 monthly
Pandemic	Anticipated Supplement Expiration	0.9905	Quarterly MPC from \$500 check = 0.25	20.8	0.50	-0.63	Pandemic Job Find Time-Series
Pandemic	Surprise Supplement Expiration	0.9905	Quarterly MPC from \$500 check = 0.25	120.5	1.6	-0.33	Pandemic Job Find Time-Series
Pandemic	Surprise Supplement Expiration	0.9811	UI Waiting Design MPC = 0.42	56.1	1.22	-0.40	Pandemic Job Find Time-Series

Notes: This table summarizes model parameters. See Section D.2 for details

Table A-4: Disincentive by Liquidity – Expiration of $600

	Dependent variable:		
	Base	Control for liquidity	Triple difference
	(1)	(2)	(3)
PctChange	0.0212***	0.0216***	0.0214***
	(0.0010)	(0.0010)	(0.0010)
SuppAvail	0.0034***	0.0034***	0.0032***
	(0.0010)	(0.0010)	(0.0010)
Std Balance		0.0006***	0.0024***
		(0.0001)	(0.0008)
PctChange*SuppAvail	−0.0163***	−0.0163***	−0.0160***
	(0.0011)	(0.0011)	(0.0011)
SuppAvail*Std Balance			−0.0018*
			(0.0010)
PctChange*Std Balance			−0.0023**
			(0.0010)
PctChange*SuppAvail*Std Balance			0.0025**
			(0.0012)
Constant	0.0025***	0.0021***	0.0023***
	(0.0008)	(0.0008)	(0.0008)
PctChange*SuppAvail if balance 1 sd above mean			-0.0135
PctChange*SuppAvail if balance 1 sd below mean			-0.0185
Observations	2,120,802	2,120,802	2,120,802
Number of Households	183,138	183,138	183,138

Note: "Std Balance" measures liquidity using checking account balance at the end of March 2020. Balances are winsorized at the 90th percentile and then standardized to have mean 0 and standard deviation 1. By measuring liquidity in March, we capture liquidity before the household has received any supplement payments. See Section 5.3.1 for additional discussion. Standard errors are clustered at the household level. *p<0.1; **p<0.05; ***p<0.01.

Table A-5: Disincentive by Liquidity – Onset of $300

	Dependent variable:		
	Base	Control for liquidity	Triple difference
	(1)	(2)	(3)
PctChange	0.0220***	0.0220***	0.0215***
	(0.0010)	(0.0010)	(0.0010)
SuppAvail	0.0077***	0.0077***	0.0075***
	(0.0008)	(0.0008)	(0.0008)
Std Balance		−0.0001	0.0027***
		(0.0001)	(0.0005)
PctChange*SuppAvail	−0.0204***	−0.0204***	−0.0200***
	(0.0013)	(0.0013)	(0.0013)
SuppAvail*Std Balance			−0.0003
			(0.0008)
PctChange*Std Balance			−0.0054***
			(0.0010)
PctChange*SuppAvail*Std Balance			0.0014
			(0.0013)
Constant	0.0091***	0.0091***	0.0093***
	(0.0006)	(0.0006)	(0.0006)
PctChange*SuppAvail if balance 1 sd above mean			-0.0186
PctChange*SuppAvail if balance 1 sd below mean			-0.0214
Observations	1,789,831	1,789,831	1,789,831
Number of Households	131,444	131,444	131,444

Note: "Std Balance" measures liquidity using checking account balance at the end of October 2020. Balances are winsorized at the 90th percentile and then standardized to have mean 0 and standard deviation 1. By measuring liquidity in October, we capture liquidity before the household has received any supplement payments. See Section 5.3.1 for additional discussion. Standard errors are clustered at the household level. *p<0.1; **p<0.05; ***p<0.01.

Table A-6: Liquidity Summary Statistics for Treatment Groups in MPC Designs

(a) Full Sample

Design	Median checking account balance		Percentile ranking of median checking account balance		Percentile ranking of median checking account buffer	
	Pre-Pandemic	Month of MPC	Pre-Pandemic	Month of MPC	Pre-Pandemic	Month of MPC
	(1)	(2)	(3)	(4)	(5)	(6)
Continuously unemployed during the pandemic	$1,518	$3,602	0.36	0.60	0.38	0.63
UI onset (waiting)	$1,440	$3,204	0.34	0.57	0.37	0.61
$600 expiration	$1,506	$3,489	0.36	0.59	0.39	0.63
$300 onset	$1,544	$2,201	0.36	0.46	0.39	0.50
$300 expiration June states	$1,414	$2,166	0.34	0.46	0.36	0.50
$300 expiration Sept states	$1,649	$2,600	0.38	0.51	0.40	0.54
$300 expiration June vs. Sept states	$1,414	$2,166	0.34	0.46	0.36	0.50

(b) Low Liquidity

Design	Median checking account balance		Percentile ranking of median checking account balance		Percentile ranking of median checking account buffer	
	Pre-Pandemic	Month of MPC	Pre-Pandemic	Month of MPC	Pre-Pandemic	Month of MPC
	(1)	(2)	(3)	(4)	(5)	(6)
Continuously unemployed during the pandemic	$732	$1,754	0.20	0.40	0.22	0.41
UI onset (waiting)	$772	$1,689	0.21	0.39	0.23	0.41
$600 expiration	$768	$1,777	0.21	0.40	0.23	0.42
$300 onset	$755	$1,056	0.20	0.27	0.23	0.29
$300 expiration June states	$762	$1,135	0.21	0.29	0.23	0.30
$300 expiration Sept states	$778	$1,133	0.21	0.28	0.23	0.30
$300 expiration June vs. Sept states	$762	$1,135	0.21	0.29	0.23	0.30

(c) High Liquidity

Design	Median checking account balance		Percentile ranking of median checking account balance		Percentile ranking of median checking account buffer	
	Pre-Pandemic	Month of MPC	Pre-Pandemic	Month of MPC	Pre-Pandemic	Month of MPC
	(1)	(2)	(3)	(4)	(5)	(6)
Continuously unemployed during the pandemic	$3,364	$7,289	0.58	0.76	0.62	0.80
UI onset (waiting)	$3,208	$6,175	0.57	0.73	0.61	0.78
$600 expiration	$3,291	$7,061	0.57	0.76	0.61	0.79
$300 onset	$3,389	$5,175	0.58	0.69	0.62	0.72
$300 expiration June states	$3,163	$5,644	0.56	0.71	0.59	0.73
$300 expiration Sept states	$3,615	$6,207	0.60	0.73	0.63	0.78
$300 expiration June vs. Sept states	$3,163	$5,644	0.56	0.71	0.59	0.73

Notes: This table reports summary statistics of the liquidity levels of unemployed workers who contribute to the estimation of MPCs. Panel (a) corresponds to the full samples in Table 1, panel (b) corresponds to the low-liquidity samples in Table 2, and panel (c) corresponds to the high-liquidity samples in Table 2. Column (1) reports the median checking account balance as of January 2020 for unemployed workers in each treatment group. Column (2) reports the median checking account balance for these same workers in the MPC month, which we define as the supplement-inclusive month used in the given MPC calculation (so for the "UI onset (waiting)" group this month is May 2020, and for the "$600 expiration" group this month is July 2020). Columns (3) and (4) report the percentiles of median checking account balances relative to the distribution of checking account balances for all employed workers in January 2020. Columns (5) and (6) report the percentiles of median checking account buffers relative to the distribution of checking account buffers for all employed workers in January 2020. See Appendix B.2 for details on the definition of liquidity buffer. Buffers in the month of MPC are normalized by spending in January 2020 rather than in the month of the MPC in order to normalize by pre-pandemic variables. The "continuously unemployed during the pandemic" group is the group that receives unemployment benefits from April 2020 through at least February 2021. Since there is no formal MPC estimation for this group, we use July 2020 as the "Month of MPC" for this group, as this is the month that checking account balances reached their peak.

Table A-7: MPC by Spending Category

	UI onset (waiting)			FPUC expiration		
	MPC	MPC Share	Pre-Pandemic Spend Share	MPC	MPC Share	Pre-Pandemic Spend Share
Auto Repair	0.006	0.036	0.025	0.003	0.029	0.027
Clothing	0.007	0.041	0.020	0.008	0.075	0.026
Department Stores	0.002	0.009	0.005	0.002	0.018	0.007
Discount Stores	0.012	0.071	0.056	0.008	0.072	0.050
Drug Stores	0.002	0.011	0.017	0.001	0.013	0.013
Entertainment	0.003	0.020	0.025	0.001	0.013	0.025
Flights	0.002	0.009	0.003	0.000	0.002	0.003
Groceries	0.040	0.228	0.217	0.020	0.184	0.187
Healthcare	0.003	0.018	0.020	0.004	0.033	0.024
Home Improvement	0.020	0.116	0.063	0.008	0.079	0.050
Hotels & Rental Cars	0.003	0.015	0.019	0.001	0.010	0.027
Insurance	0.004	0.020	0.032	0.003	0.025	0.027
Orgs & Institutions	0.004	0.020	0.025	0.002	0.017	0.025
Other Retail	0.030	0.170	0.104	0.018	0.171	0.103
Prof & Personal Services	0.009	0.048	0.047	0.005	0.048	0.047
Restaurants	0.017	0.094	0.097	0.010	0.093	0.124
Schools	0.000	0.001	0.002	0.001	0.006	0.008
Telecom	0.005	0.029	0.074	0.003	0.032	0.062
Transport	0.005	0.027	0.052	0.002	0.020	0.065
Utilities	0.003	0.016	0.096	0.006	0.060	0.099

	\$300 onset			\$300 expiration		
	MPC	MPC Share	Pre-Pandemic Spend Share	MPC	MPC Share	Pre-Pandemic Spend Share
Auto Repair	0.001	0.005	0.027	0.002	0.016	0.025
Clothing	0.006	0.053	0.025	0.006	0.050	0.030
Department Stores	0.002	0.017	0.009	0.001	0.009	0.010
Discount Stores	0.009	0.083	0.051	0.008	0.065	0.049
Drug Stores	0.002	0.019	0.013	0.001	0.006	0.012
Entertainment	0.002	0.020	0.026	0.004	0.037	0.030
Flights	0.000	0.000	0.004	0.002	0.020	0.009
Groceries	0.023	0.211	0.185	0.017	0.147	0.161
Healthcare	0.002	0.018	0.028	0.003	0.026	0.026
Home Improvement	0.003	0.031	0.044	0.005	0.043	0.040
Hotels & Rental Cars	0.000	0.004	0.026	0.007	0.065	0.037
Insurance	0.001	0.012	0.027	0.001	0.009	0.025
Orgs & Institutions	0.003	0.029	0.025	0.003	0.023	0.023
Other Retail	0.032	0.292	0.108	0.016	0.139	0.100
Prof & Personal Services	0.006	0.056	0.047	0.007	0.061	0.049
Restaurants	0.013	0.115	0.120	0.020	0.170	0.139
Schools	-0.001	-0.008	0.008	0.001	0.009	0.005
Telecom	0.003	0.025	0.066	0.002	0.018	0.058
Transport	-0.000	-0.002	0.062	0.005	0.046	0.082
Utilities	0.002	0.022	0.099	0.005	0.040	0.090

Notes: This table computes MPCs by spending category for the subset of spending transactions we can assign to granular categories based on merchant category codes (for card transactions) and text descriptions (for electronic transactions). MPC share corresponds to the ratio of the category-specific MPC to the MPC for all categorized spending. Pre-pandemic spending share calculates the spending share on a given category using spending of unemployed households in the same calendar months in 2019.

Table A-8: Marginal Propensity to Repay Debt

	UI onset (waiting)	FPUC expiration	\$300 onset	\$300 expiration
Auto Loans	0.003	0.001	0.002	0.003
	(0.001)	(0.001)	(0.001)	(0.001)
Mortgages	0.000	0.004	-0.004	0.008
	(0.002)	(0.002)	(0.003)	(0.002)
Student Loans	0.000	0.000	-0.001	0.000
	(0.001)	(0.000)	(0.000)	(0.000)

Notes: This table computes marginal propensities to repay debt by debt category. Debt payments are categorized using text descriptions from electronic payments. Standard errors are clustered by household.

Table A-9: MPC Robustness to Additional Controls

Research Design	Total Spending MPC			
Waiting for benefit	0.42	0.40	0.40	0.41
	(0.01)	(0.02)	(0.02)	(0.02)
Expiration of \$600 supplement	0.31	0.30	0.30	0.30
	(0.01)	(0.01)	(0.01)	(0.01)
Onset of \$300 supplement	0.28	0.28	0.28	0.27
	(0.01)	(0.01)	(0.01)	(0.02)
Expiration of \$300 supplement (June states)	0.35	0.35	0.35	0.35
	(0.02)	(0.03)	(0.03)	(0.04)
Expiration of \$300 supplement (September states)	0.27	0.26	0.27	0.30
	(0.01)	(0.02)	(0.02)	(0.02)
State∗SuppAvail FE		X	X	X
Age∗SuppAvail FE			X	X
HasKids∗SuppAvail FE				X

Notes: This table re-computes MPC results with additional controls: state, age of the primary account holder, and the presence of children. We do not report results for the June vs. Sept states research design, since these regressions are not identified when including state fixed effects. See Appendix B.3 for additional details. Standard errors are clustered by household.

Table A-10: MPC Robustness to Summary Statistic and Sample

Episode	(1)	(2)	(3)	(4)
Waiting for benefits	0.42	0.48	0.45	0.41
Expiration of \$600 supplement	0.31	0.38	0.34	0.28
Onset of 8-month \$300 supplement	0.28	0.31	0.30	0.26
Summary Statistic	Mean	Median	Mean	Mean
Sample	All	All	No non-Chase credit card	Make ACH debt payments

Notes: This table re-computes our MPC results for a number of alternative samples. The rows compute MPCs to the three identification strategies shown in the main text. Column 1 repeats the main specification from Table 1. Column 2 computes MPCs using median instead of mean spending. Column 3 excludes households who make debt payments to non-Chase credit cards (for whom we are potentially missing some spending). Column 4 restricts to households who make debt payments via ACH (whom we are more confident are not making mis-classified debt payments via paper check). See Appendix B.3 for additional details.

Table A-11: MPC Robustness to Spending Measure

	Spend Total		Spend card and cash	
Episode	MPC	Elasticity	MPC	Elasticity
Waiting for benefits	0.42	0.57	0.31	0.72
Expiration of $600 supplement	0.31	0.42	0.21	0.48
Onset of 8-month $300 supplement	0.28	0.38	0.20	0.45
Lost Wages Assistance	0.31	0.42	0.21	0.48

Notes: This table shows the robustness of our MPC results to the measure of spending. The first columns show MPCs and elasticities for spending (total), which is our preferred specification. The third and fourth columns recompute MPCs and elasticities for spending (card and cash) instead of spending (total).

Table A-12: Effect of Expanded Benefits: Robustness to Controls

(a) Expiration of $600

	Dependent variable:			
	Exit to New Job			
	(1)	(2)	(3)	(4)
PctChange*SuppAvail	−0.0163***	−0.0133***	−0.0139***	−0.0141***
	(0.0011)	(0.0012)	(0.0013)	(0.0023)
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	2,120,887	2,120,887	1,886,942	519,245
Number of Households	183,144	183,144	163,930	44,165

(b) Onset of $300

	Dependent variable:			
	Exit to New Job			
	(1)	(2)	(3)	(4)
PctChange*SuppAvail	−0.0204***	−0.0178***	−0.0199***	−0.0207***
	(0.0013)	(0.0013)	(0.0014)	(0.0026)
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	1,790,138	1,790,138	1,604,130	454,135
Number of Households	131,464	131,464	116,276	32,689

Notes: This table reports estimates of $\hat{\beta}$ from equation (4), adding increasingly stringent control variables. The first column is the same as in Table A-2. 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 1,000 largest firms in the data and therefore uses a smaller sample than the other columns. Standard errors are clustered at the household level. *p<0.1; **p<0.05; ***p<0.01.

Table A-13: Effect of Expanded Benefits: Alternative Functional Forms

Functional Form	Aggregation	Hazard Elasticity		Duration Elasticity	
		$600 Expiration	$300 Onset	$600 Expiration	$300 Onset
Baseline: Absolute pp	Worker-level	0.19	0.27	0.11	0.22
Relative percent change	Benefit change deciles	0.16	0.33	0.09	0.27
Logit	Worker-level	0.16	0.23	0.09	0.19

Notes: This table explores alternative functional forms for estimating job-finding effects. See Section C.5.1 for details.

Table A-14: Effect of Expanded Benefits: Alternative Measures of Exit

(a) Expiration of $600

	Dependent variable:		
	New job	Recall	Total
	(1)	(2)	(3)
SuppAvail*PctChange	−0.0163***	−0.0125***	−0.0288***
	(0.0011)	(0.0013)	(0.0017)
Observations	2,120,887	2,120,887	2,120,887
Number of Households	183,144	183,144	183,144

(b) Onset of $300

	Dependent variable:		
	New job	Recall	Total
	(1)	(2)	(3)
SuppAvail*PctChange	−0.0204***	−0.0031***	−0.0234***
	(0.0013)	(0.0008)	(0.0015)
Observations	1,790,138	1,790,138	1,790,138
Number of Households	131,464	131,464	131,464

Notes: This table reports estimates of $\hat{\beta}$ from equation (4) specified for four different outcome variables. The first column is the same as in Table A-2. Column (2) is exit to recall and column (3) is total exit (new job or recall). Standard errors are clustered at the household level. *p<0.1; **p<0.05; ***p<0.01.

Table A-15: Effect of Expanded Benefits: Summer 2021

	Dependent variable:			
	Exit to New Job			
	Max Observed Weeks Starting from 11 July 2021			
	4 weeks	4 weeks	8 weeks	8 weeks
PctChange*SuppAvail	−0.0171*	−0.0244***	−0.0137	−0.0305***
	(0.0098)	(0.0091)	(0.0090)	(0.0087)
Donut Excluded	Yes	No	Yes	No
Observations	48,376	55,132	69,346	78,591
Number of Households	5,513	5,532	7,638	7,665

Notes: This table estimates the difference-in-difference model from equation (4) using a window of eight weeks prior to and eight weeks after the September expiration of the $300 supplement. The supplement available period is July 11, 2021 to September 4, 2021 and the no-supplement period is from September 5, 2021 to 7 November 7, 2021. Columns (1) and (3) exclude the “donut” period September 5 through 24. Standard errors are clustered at the household level. *p<0.1; **p<0.05; ***p<0.01.

Table A-16: Interpreting MPC Differences Between Waiting for Benefits and $500 Stimulus

	Nature of transfer	Who receives	Calibration	Environment	MPC		
					Horizon	Model	Data
(1)	$2400 Persistent+Reg UI	Unemp no UI	Waiting	Pandemic	Month	0.42	0.42
(2)	$2400 Persistent	Unemp w/ reg UI	Waiting	Pandemic	Month	0.29	
(3)	$2400 Persistent	Unemp w/ reg UI	Waiting	Normal	Month	0.45	
(4)	$2400 One Time	Unemp w/ reg UI	Waiting	Normal	Month	0.29	
(5)	$500 One Time	Unemp w/ reg UI	Waiting	Normal	Month	0.45	
(6)	$500 One Time	Everyone	Waiting	Normal	Month	0.21	
(7)	$500 One Time	Everyone	$500	Normal	Month	0.09	
(8)	$500 One Time	Everyone	$500	Normal	Quarter	0.25	0.25

Notes: This table compares MPCs across various model specifications and shows empirical counterparts, where available. The "Nature of transfer" column shows the particular transfer for which we compute an MPC. The $2,400 monthly transfer corresponds to $600 weekly supplements while $500 transfers correspond to past stimulus checks. "Who receives" shows which households are receiving that transfer. "Calibration" shows the target calibration used in that model. Models either target the MPC from the waiting design or target a 0.25 quarterly MPC out of $500 stimulus checks sent to everyone. "Environment" is either a pre-pandemic environment which includes discount factor shocks and stimulus checks or a normal environment which does not. MPCs are calculated primarily at the monthly horizon but we show also a quarterly result for stimulus checks to everyone to ease comparison with empirical estimates. Note that even though we show effects one element at a time, these interactions are non-linear and so this is not an additive decomposition.

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