We will present results from both “Spending and Job Search Impacts of Expanded Unemployment Benefits: Evidence from Administrative Micro Data” and a more recent update “Micro and Macro Disincentive Effects of Expanded Unemployment Benefits”. Both are uploaded (p2-73) and (p73-121) in this pdf.

Spending and Job Search Impacts of Expanded Unemployment Benefits: Evidence from Administrative Micro Data *

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February 11, 2021

Abstract

How did the largest expansion of unemployment benefits in U.S. history affect household behavior? Using anonymized bank account data covering millions of households, we provide new empirical evidence on the spending and job search responses to benefit changes during the pandemic and compare those responses to the predictions of benchmark structural models. We find that spending responds *more* than predicted, while job search responds an order of magnitude *less* than predicted.

In sharp contrast to normal times when spending falls after job loss, we show that when expanded benefits are available, spending of the unemployed actually *rises* after job loss. Using quasi-experimental research designs, we estimate a large marginal propensity to consume out of benefits. Notably, spending responses are large even for households who have built up substantial liquidity through prior receipt of expanded benefits. These large responses contrast with a theoretical prediction that spending responses should shrink with liquidity.

Simple job search models predict a sharp decline in search in the wake of a substantial benefit expansion, followed by a sustained rebound when benefits expire. We instead find that the job-finding rate is quite stable. Moreover, we document that recall plays an important role in driving job-finding dynamics throughout the pandemic. A model extended to fit these key features of the data implies small job search distortions from expanded unemployment benefits.

Jointly, these spending and job finding facts suggest that benefit expansions during the pandemic were a more effective policy than predicted by standard structural models. Abstracting from general equilibrium effects, we find that overall spending was 2.0-2.6 percent higher and employment only 0.2-0.4 percent lower as a result of the benefit expansions.

*This paper subsumes two short prior notes: “Consumption Effects of Unemployment Insurance During the Covid-19 Pandemic” from July 2020 and “The Unemployment Benefit Boost: Initial Trends in Spending and Saving When the $600 Supplement Ended” from October 2020. We thank Adrien Auclert, Arin Dube, Jesse Edgerton, Jason Furman, Bruce Meyer, Daniel Silver, and Ivan Werning for helpful conversations, seminar participants at the AEA, Chicago Booth Micro Lunch, Opportunity Insights, Johns Hopkins, and CFPB for suggestions, and Shantanu Banerjee, Isaac Liu, Peter Robertson, Nicolas Wuthenow and Katie Zhang for excellent research assistance. This research was made possible by a data-use agreement between three of the authors and the JPMorgan Chase Institute (JPMCI), which has created de-identified data assets that are selectively available to be used for academic research. All statistics from JPMCI data, including medians, reflect cells with multiple observations. The opinions expressed are those of the authors alone and do not represent the views of JPMorgan Chase Co. Ganong, Noel, and Vavra gratefully acknowledge the Becker Friedman Institute at the University of Chicago for financial support.

1 Introduction

The COVID-19 pandemic caused massive disruption to the labor market. The government responded to this disruption with the largest increase of unemployment insurance (UI) benefits in history. Benefit levels rose by $600 per week between April and July 2020 before expiring in August and then rising again by $300 for six weeks. The $600 supplement nearly tripled typical benefit levels, with resulting benefits replacing 145 percent of lost income for the median worker.

This paper uses anonymized bank account data covering millions of households to estimate consumption and job search responses to these unprecedented changes in benefits and then compares those empirical responses to predictions from benchmark structural models. We find that spending responds *more* than predicted while job search responds about an order of magnitude *less* than predicted. Abstracting from general equilibrium effects, our estimates suggest that total spending was about 2.0-2.6 percent higher and employment only 0.2-0.4 percent lower as a result of these benefit expansions.

Analyzing these impacts is useful for several reasons. First, these policy interventions are large enough to matter for aggregate spending and employment: between April and August, a total of $263 billion was paid out in $600 weekly supplements. Second, this policy has important distributional consequences: unlike typical stabilization tools such as broad-based stimulus checks, this policy is targeted to a subset of the population hard hit by the recession. Overall, one-quarter of all working-age people received benefits during this period. Third, the canonical approach to evaluating changes in UI generosity trades off the consumption-smoothing benefits of increased spending against the moral hazard costs of discouraged job search. Measuring each of these effects therefore helps inform this type of normative policy analysis for the most aggressive expansion of benefits on record. Finally, the scope, persistence, expiration, and then partial reinstatement of the $600 supplement provides a unique laboratory for testing implications of structural models which have previously been untestable.

These impacts are hard to estimate empirically because they require data which jointly measures spending, employment transitions, and unemployment receipt for the same households. We overcome this challenge by using a dataset of de-identified bank account transactions from the universe of Chase customers to build household-by-week measures of spending and job search from January 2019 through October 2020. Crucially, we can measure both labor income and UI benefit receipt in our data using identifiers from direct deposits.

In the first part of the paper we leverage this dataset to document three new basic facts about labor market patterns during the pandemic. First, a distinguishing feature of the pandemic-era labor market is a high fraction of UI recipients returning to their prior employer (henceforth "recalls"). While about 20 percent of UI exits reflect a recall in normal times, this share rises to 75 percent in May 2020 before falling back below 50 percent by the end of October. Second, long-term unemployment has soared relative to 2019. As of the end of October, about half of the unemployed had continuously received benefits since the start of the pandemic. Third, we identify the emergence of repeated unemployment; more than half of the newly unemployed in October previously received benefits during the pandemic. Although this empirical finding is novel with respect to the pandemic, it is consistent with job-ladder models and other data on labor market flows.[1]

In the second part of the paper we examine the spending response to UI benefits. We begin with a descriptive analysis of time-series patterns of income and spending for employed and unemployed

[1] For job-ladder models, see e.g. Stevens (1997); Hall (1995); Jarosch (2015); Krolikowski (2017); Pinheiro and Visschers (2015) and for labor market flows see Chodorow-Reich and Coglianese (2020).

households during the pandemic. The starkest result from this analysis is that while the $600 supplement is available, the spending of unemployed households actually *rises* after job loss, both in absolute terms and relative to the spending of employed households. This spending increase is particularly striking, since overall spending was substantially depressed during the pandemic (Chetty et al., 2020; Cox et al., 2020), and it stands in sharp contrast to normal times when spending falls sharply after job loss (Ganong and Noel, 2019).

How could spending rise for households who had both lost their job and were weathering a pandemic? Suggestive evidence comes from the time-series pattern of income. Given the $600 weekly supplement, income also *rises* substantially during unemployment, again in sharp contrast to normal times when UI benefits only replace around half of lost income. When the $600 supplement expires, we see an immediate decline in spending, followed by a sharp rebound in spending once households receive a new temporary $300 weekly supplement about one month later.

Next, we turn from descriptive analysis to causal estimates of the spending response to various changes in unemployment benefit levels during the pandemic. We estimate marginal propensities to consume (MPCs) for households at different points in their unemployment spells and in different liquidity positions using three research designs which exploit the three different policy changes we observe.[2]

The main finding from our causal analysis is that spending consistently rises and falls with benefit levels, *even* for households that have built up substantial liquidity through prior receipt of expanded benefits. We show parallel pre-trends between treatment and control groups in all three research designs. In each case, spending responds sharply exactly in the week in which benefit levels change: rising when benefits begin and when they increase in level, and falling when benefit levels drop. The spending responses are quantitatively large across all specifications, with estimated one-month MPCs between 29 and 43 cents. We estimate high MPCs even in response to the $300 supplement payments received in September, when unemployed households have substantially more liquidity than they do at the onset of unemployment in April. Our empirical strategies allow us to directly identify one-month MPCs, but spending responses over longer horizons are also of direct policy relevance. There is more uncertainty about these numbers since their estimation requires much stronger assumptions. However, using two different strategies, we estimate an MPC through the end of our sample (six months) of 62 to 69 cents.

In the final step of our spending analysis, we compare our causal empirical estimates to predictions from a benchmark structural model which matches pre-pandemic evidence on MPCs. This serves three purposes. First, it allows us to benchmark the size of our empirical estimates against predictions one might make from a model based on pre-pandemic evidence. Second, it allows us to relate the causal estimates we obtain in our empirical analysis to particular policy counterfactuals of interest. Finally, our unique empirical setting with identified spending responses to a sequence of policy changes allows us to assess *dynamic* implications of the standard consumption model, which are not typically testable.

We work with a standard quantitative consumption-savings model. Households face idiosyncratic unemployment and earnings risk and a borrowing constraint, calibrated to match prior evidence on spending responses to temporary tax rebates. This standard model predicts significant spending responses at the onset of unemployment, when households are in a temporarily low-liquidity state, but much smaller responses later in the unemployment spell after households have built up liquidity from

[2] We follow the convention in the literature of referring to the average change in spending divided by the average change in income induced by these policy changes as an MPC. However, we note that this empirical strategy actually measures the average propensity to consume (APC). Since these unemployment benefit changes are large, the MPC and APC can potentially differ substantially, which will be important in our discussion of model implications.

the initial supplements. It has not been previously possible to test this type of prediction because we know of no other empirical setting where researchers have measured an MPC *after* households received a series of large transfers.

We find that the empirical spending response to expanded benefits is generally *larger* than predicted by the standard model, especially to benefit increases that occur after households have already built up substantial liquidity from prior transfers. Specifically, the model can match the estimated MPC at the onset of unemployment (when households have low liquidity), and at the expiration of the $600 supplement (as long as this expiration is treated as a surprise), but cannot match the response to the $300 supplement in September. When households have liquidity built up from a prior stream of transfers, the standard model predicts that they should smooth consumption around a new transfer, but this is not what we find empirically. Thus, we conclude that non-standard model elements will be necessary to match the sustained spending response to UI benefits that is observed in the data. Furthermore, models which do not match these persistently high responses are likely to understate the aggregate benefits of expanded unemployment insurance.

Having documented the benefits of expanded unemployment insurance from increased spending, in the third part of the paper we investigate the potential costs from discouraged job search.

Our central empirical result about job-finding behavior is that the exit rate from unemployment is relatively constant between May and October, despite massive fluctuations in benefit levels over this time period. Over half of all unemployed workers receiving the $600 supplement return to work *before* the supplement expires. Many of these workers return to their prior employers. Indeed, although the exit rate is relatively constant over this time period, the fluctuations that we do see are driven almost entirely by fluctuations in recalls. Finally, the exit rate remains low even after the $600 supplement expires and the median replacement rate plummets from 145 percent to 50 percent. This finding is qualitatively inconsistent with a large disincentive effect from the benefit supplement.

To quantitatively estimate the impact of the benefit supplement on job search, we compare our empirical results to predictions from a benchmark structural model of job search. We study a model with endogenous, costly job search. We begin by calibrating the model to match exit rates in the pre-pandemic period and to prior estimates of the disincentive effect of higher benefit levels. The model predicts that when the supplement is available, agents either search very little or do not search at all. Search then rises to pre-supplement levels when the supplement expires, as in the classic model of UI benefit exhaustion by Mortensen (1977).

We find that the job search response in the data is much *smaller* than predicted by the baseline model. In contrast to the model, we find in the data that exit rates largely remain low even after the supplement expires. The disagreement between model and data holds whether we model the expiration as expected or as a surprise. Furthermore, this disagreement is not due to any unique feature of our dataset or sample frame. Indeed, we show that similar empirical patterns hold in monthly unemployment exit rates calculated from the Current Population Survey, although these monthly estimates have less precision than the weekly estimates we construct with our account data.

Although the baseline model cannot explain these patterns, we show that a model with two empirically plausible enhancements generates a tight match to the data. First, we incorporate the large increase in recalls during the pandemic. We assume that recalls are exogenous following Katz (1986), so a recalled employee has no choice but to return to work or give up their benefits (as is required by law). Second, we calibrate the cost of job search to match the dynamics of UI exit for non-recalled employees during the pandemic. Because exit rates are so low, and are similar with and without the supplement, this calibration implies that job search was much more costly during the pandemic than during normal times. This is consistent with evidence from Marinescu, Skandalis, and Zhao (2020) that applications per vacancy increased significantly during the pandemic, or more generally with the possibility that it is difficult to search for a job during a health emergency. This substantially higher cost of job search translates into a much lower disincentive effect of increased benefits, since it is costly for workers to search.

We use the enriched model to quantify the disincentive effects of the \$600 supplement. The ideal experiment would compare job-finding of unemployed benefit recipients who were randomly assigned to receive the supplement to those who were not. Because every recipient was eligible for the supplement, such a design is infeasible and we instead examine a counterfactual without the supplement in a model that matches the data from job-finding in this period.

We find that the job search disincentive effect is an order of magnitude smaller than would have been expected based on models calibrated to pre-pandemic behavior. This is because both recalls and high costs of job search diminish the household response to incentives. Therefore, search is only minimally affected even when replacement rates move dramatically. Whereas estimates of the elasticity of unemployment duration with respect to benefit levels are around 0.5 in normal times, we estimate an elasticity of 0.02 or less during the pandemic. This is smaller than every prior elasticity estimate from 18 microeconomic studies reviewed in a recent meta-analysis by Schmieder and von Wachter (2016).

Combined, the results in our paper provide an accounting of the costs and benefits of increased UI levels and allow a partial estimation of their macroeconomic impacts. While we do not attempt to develop a framework to make welfare conclusions about the optimality of this particular benefit increase, we can make positive statements about its impact when abstracting from general equilibrium effects. Specifically, using our estimates of MPCs and job search disincentive effects, we can calculate implied partial equilibrium impacts on aggregate spending and employment. This analysis suggests that the \$600 supplement increased total spending by 2.0-2.6 percent between April and July 2020 and that the reduction in job search from the supplement only decreased employment by 0.2-0.4 percent. While we do not attempt to integrate our estimates into an equilibrium macro model, our most conservative partial equilibrium estimates imply that total employment was actually *increased* as a result of the supplement as long as \$453,000 in additional spending translates into at least one additional job. This is very likely the case since prior estimates of costs per job typically range from \$25,000-\$125,000 (Chodorow-Reich, 2019).[3]

This paper contributes to several additional strands of the literature. First, our estimates may be useful for the active literature on the optimal cyclicality of unemployment benefits (Kroft and Notowidigdo, 2016; Kekre, 2017; Landais, Michaillat, and Saez, 2018; Mitman and Rabinovich, 2015; Schmieder, von Wachter, and Bender, 2012). Although economic theory suggests that unemployment benefit levels should vary over the business cycle, countercyclical benefits have never before been attempted at this scale in the U.S. (or in any other country), so there is no prior evidence about their impacts. The main arguments in favor of countercyclical UI are that aggregate demand is depressed—so there is a desire for stimulus —and that the labor market is depressed—so the moral hazard effect of UI may be smaller than usual. This argument depends on the quantitative strength of these forces. We

[3]Alternatively, our conservative estimate is that as long as the employment multiplier is at least 0.34, the \$600 supplements increased employment on net. A 0.34 employment multiplier is well below estimates of 1.3-1.8 in Nakamura and Steinsson (2014).

do find that spending responds sharply even to significant and persistent increases in UI benefit levels during a recession, and that job search distortions are minimal.

However, we caution that our conclusions about the relative costs and benefits of countercyclical UI during the period we study may not generalize to other times (or even to other phases of the pandemic period). For spending, one might expect that responses could be larger in normal recessions when opportunities to spend are not deliberately curtailed for public health reasons. On the other hand, the job search impacts may also be larger in non-pandemic recessions, for example if job search costs don't rise as sharply. Indeed, these relative costs and benefits may even evolve throughout the pandemic period itself. In particular, as the labor market tightens and the recall share falls, the job search disincentive effects could increase.

Second, our estimates also relate to the debate between targeted versus universal stimulus payments. For the last 20 years, the federal government has regularly used universal or near-universal tax rebate payments at the onset of recessions. An alternative approach for fiscal stimulus is to target payments to certain households, such as the unemployed, who may be in a particularly vulnerable state. The private benefits of such targeted transfers are higher, and since these households are more likely to spend the transfers, the aggregate demand impacts are also likely higher (Elmendorf and Furman, 2008). Such targeted transfers have occasionally been implemented in the past, but never at this scale and never in a way that has allowed an econometric identification of their spending impacts.

We show that the spending impacts from targeted transfers are indeed substantial, even in a pandemic. Comparing these spending impacts to those from untargeted transfers is more challenging due to differences in time horizons and spending definitions. However, some simple comparisons suggest that the responses we estimate to targeted unemployment benefits (which are likely depressed in part from the pandemic) are nevertheless larger than estimated spending responses to universal transfers in the past.

Our results are also consistent with a special role for targeted payments when economic shocks are concentrated in certain sectors. As shown theoretically by Guerrieri et al. (2020), universal payments may be less effective in this situation because sectoral shutdowns break links in the standard Keynesian feedback loop. In contrast, targeted transfers to workers in affected sectors should have large spending impacts and are even more crucial when the standard links are broken. The large spending response we document to exactly these types of transfers suggest that expanded benefits may have mitigated, or even reversed, negative spillover effects that may have otherwise occurred from a Keynesian supply shock.

Finally, the significant spending response to large targeted payments that we document helps resolve a puzzle emerging from distributional dynamics across income groups during the pandemic. Cajner et al. (2020) documents that labor income *fell* the most for low-income households, while Cox et al. (2020) shows that spending *grew* the fastest for these same households. Our finding that unemployed households—concentrated at the lower end of the income distribution—were induced by receipt of expanded UI benefits to *increase* their spending both in absolute terms and relative to employed households, can help explain why spending grew the fastest for the group with the largest labor income declines.

The remainder of the paper proceeds as follows. Section 2 describes the expansion of benefits during the pandemic and the dataset we use to analyze its impacts. Section 3 uses this dataset to document several new basic facts about the labor market during the pandemic. Sections 4 and 5 analyze the impact of expanded benefits on spending and job search, respectively, and compare these effects to predictions from benchmark models. Section 6 concludes.

2 Institutions and Data

We begin with a brief discussion of the changes in unemployment insurance policies over the course of the pandemic and then describe the data that we use to analyze their impacts.

2.1 Expansion of Unemployment Benefits

2.1.1 $600 supplement for seventeen weeks

The Coronavirus Aid, Relief and Economic Security Act (CARES Act) was signed into law on March 27, 2020 and implemented a variety of policy responses to the emerging pandemic. One important provision of the CARES Act was a massive expansion of unemployment benefits. The CARES Act established Federal Pandemic Unemployment Compensation (FPUC) which provided a supplement of $600 per week from April-July for everyone receiving unemployment benefits, on top of any amount already allotted by regular state unemployment insurance. The CARES Act also expanded eligibility for unemployment benefits to many self-employed and "gig" workers who would not otherwise qualify for regular benefits, through the creation of the Pandemic Unemployment Assistance (PUA) program. Importantly, unemployed workers who qualified for UI through the PUA program were also eligible for the $600 FPUC supplements. Because of data constraints, our analysis does not distinguish between regular benefits and PUA. Finally, the CARES Act also established Pandemic Emergency Unemployment Compensation (PEUC), which extended benefit eligibility by an additional thirteen weeks for those who would have otherwise exhausted unemployment benefits.

The $600 supplements resulted in an unprecedented increase in unemployment benefits. Ganong, Noel, and Vavra (2020) show that the $600 supplements raised median replacement rates to 145%. Appendix Figure A-1 compares the change in replacement rates across OECD countries both in the pandemic and in the Great Recession. No countries instituted large benefit increases in the Great Recession, while during the pandemic, several countries experimented with large increases in unemployment benefits. However, no other country implemented benefit increases as large as those in the United States. Between April and August, a total of $263 billion was paid out in $600 weekly supplements. With these expansions of unemployment benefits and the sudden rise in the unemployment rate, unemployment benefits amounted to 7.0 percent of total personal income in June of 2020—a record far exceeding the 1.3 percent peak during the Great Recession.

Importantly for some of our identification strategies, many state unemployment agencies were overwhelmed by the large increases in unemployment claims at the start of the pandemic, meaning that the payment of many claims was delayed. However, we note that claimants are typically eligible for "backpay" once these claims are eventually processed. Thus, processing backlogs in large part represent a delay rather than a reduction in total benefits.

The original CARES Act legislation authorized $600 supplements through the end of July. As the end of July approached, the fate of the expanded unemployment benefits remained unclear. Congressional Democrats advocated a continuation of the $600 supplement, while congressional Republicans advocated a $400 supplement. Perhaps surprisingly, the two sides failed to reach any legislative compromise and the supplement fell to zero at the start of August.

2.1.2 $300 supplement for six weeks

On August 8, the Trump administration issued an executive order for "Lost Wages Assistance" (LWA), which provided funding to states to pay a supplement applying to claims filed for weeks of unemployment ending on August 1 or later. The executive order called for a $400 supplement, but was then was revised down to $300 one week later.[4]

LWA was implemented under authority of the Federal Emergency Management Agency, and political coverage at the time suggests that the announcement was largely a surprise. Since the program announcement was a surprise, it was not coordinated in advance with state unemployment agencies that actually administer the payments. This led to a haphazard roll out of these $300 supplements. The number of weeks for which the $300 supplement was available was initially unclear, but nearly every state ultimately allowed workers to earn the supplements for the six benefit weeks from August 1 through September 5. However, while there was uniformity across states in the calendar weeks in which $300 supplements were accrued, there was substantial heterogeneity across states in when these accrued supplements were actually paid. Arizona paid out first in late August. Many states paid out in mid September, and Wisconsin and New Jersey did not pay the supplements until the end of October. Thus, although recipients' eligibility for $300 LWA supplements expired by September 5, LWA payments nearly always occurred *after* the program expired. This delayed timing of supplements contrasts with the $600 FPUC payments, which were mostly paid contemporaneously with regular benefit payments for the same week.

2.1.3 Extended Duration of Unemployment Benefits: Implications for Exits

The implementation of PEUC extended benefit eligibility on top of pre-existing provisions for benefit extensions mean that exits from unemployment insurance during our sample period rarely reflect benefit exhaustion and therefore usually reflect a return to work. In particular, a worker who separated from their job at the start of the pandemic would not have exhausted benefits by the end of October. The number of weeks of benefits that are available in a state depends on three programs: regular state benefits (12-28 weeks), federally-funded PEUC (13 weeks everywhere), and the federal-state partnership Extended Benefits (6-20 weeks, where available). By December 2020, eight out of the nine states in our job search sample offered up to 52 weeks of benefits (Indiana offered up to 39 weeks). In addition, the requirements for job search were largely waived so someone who stopped working because of, for example, childcare needs would be eligible for benefits. For this reason, we generally use job finding and UI exit interchangeably throughout the paper.

2.2 Data

Our analysis sample is drawn from the 44 million households with a checking account in the JPMorgan-Chase Institute (JPMCI) data from January 2019 through November 2020. The unit of observation is household-by-week. Our primary analysis sample consists of 844,000 households that get unemployment insurance (UI) benefits via direct deposit from 31 states or the District of Columbia in 2019 and/or

[4] https://www.cnn.com/2020/08/14/politics/unemployment-benefits-trump-executive-action/index.html

2020.[5] Appendix Figure A-2 shows a map of which states are in the sample.

In addition to receiving UI benefits, these households also 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 the analysis on workers for whom their Chase account is their primary bank account (Ganong and Noel, 2019). For households that get benefits in 2020 (but not in 2019), we impose the transaction screen from for 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 minimum 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.[6] 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.

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. A spell ends when a worker has three consecutive weeks with no benefit receipt in most states.[7] The 844,000 households have 1,155,000 spells. We include workers with multiple benefit spells in our analysis. Appendix A.1.1 provides additional details on data cleaning.

In some of our analysis, we use a subsample of nine states where we observe payments of the $300 supplement known as Lost Wages Assistance (LWA). It is useful to separate payments of this supplement for two reasons. First, we measure the spending response to supplement receipt in Section 4.2. Second, because the supplement was paid with a significant delay, it has the potential to contaminate measures of spell length. For example, a worker might receive their last regular UI benefit on August 15, but not receive LWA until the end of October. We implement our analysis of job-finding only in the nine states where we are able separate LWA from regular UI benefits and therefore can construct reliable estimates of spell length: California, New Jersey, New York, Georgia, Michigan, Ohio, Illinois, Washington, and Indiana.[8]

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 an encrypted version of the transaction description associated with a payroll direct deposit (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 receipt.

We do not detect separations for every benefit recipient for two reasons. First, the JPMCI data

[5]When a household has multiple workers, we do not know which worker received unemployment benefits.

[6]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 28 percent of households with at least one working-age person received UI benefits between March 13, 2020 and the end of October 2020.

[7]In six states in the sample, benefits are paid every other week (California, Colorado, Florida, Illinois, Michigan, and Texas). In these states, we define the end of a spell as four consecutive weeks without benefit receipt (instead of three weeks).

[8]However, we can construct reliable measures of spells in the other states before LWA is paid out. Our conclusions about UI exits are very similar across all states in this time period.

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 the first benefit payment; this scenario can arise when 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 spells and employment spells to measure *recalls*, 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 same prior employer between five weeks before and three 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; Schmieder, von Wachter, and Bender, 2012; Kolsrud et al., 2018).

2.2.1 Measuring Household Income, Spending, and Assets

We construct two measures of spending. Our main measure (*total spending*) adds spending on credit and debit cards, cash, paper checks and various electronic payments. This is generally our preferred spending measure since it provides a comprehensive view of household decisions and captures a large share of spending in national accounts.[9] However, we are also interested in identification evidence which relies on fairly high frequency weekly variation. The presence of paper checks in total spending introduces high frequency measurement error in the timing of spending, since there are often lags between the time that checks are written and the time that they are eventually cleared and posted to accounts. For this reason, we also report results for a more narrow measure of spending (*card and cash*) which adds just spending on credit and debit cards plus cash. This measure is less comprehensive, but it can be measured with more accuracy at weekly frequencies.

In addition to household spending, we also measure household income and checking account balances. We define income as total inflows to Chase deposit accounts, excluding transfers. Importantly, this measures take-home income, meaning that we only observe income after taxes and other items (e.g., retirement account contributions, health insurance payments, etc.) are withheld. We exclude transfers (such as transfers from other bank accounts, money market accounts, and investment accounts) to avoid double-counting income; for example, money that a household transfers from its investment account to its checking account may have previously been counted as income if it was initially deposited as labor income to the checking account before being invested. Checking account balances are measured at a monthly frequency and defined as a household's checking account balance on the final business day of the month. For households that have multiple Chase checking accounts, we define the balance as the sum of balances across all accounts. Changes in checking account balances provide qualitative evidence of the evolution of household savings. However, the checking account does not capture all of households' savings, and we hope to offer a more comprehensive measure in the next draft of this paper.

Table A-1 provides summary statistics on the main flow measures of interest: income, unemployment

[9] While we refer to this measure as total spending, we note that it does not capture everything that shows up in aggregate spending. For example, our total spending measure does not capture owner occupied housing services. It can potentially capture down payments on car purchases but will not capture the entire value of the car sale for anyone using financing.

benefits (which are a subset of income), total spending, and card and cash spending. Two facts stand out from this table. First, while the median monthly income of households that remain employed throughout 2020 is higher than the income of households that experience unemployment both in the pre-pandemic period $5459 vs. $4570 in Jan-Feb 2020) and in the pandemic period ($5359 vs $4942 in Apr-Oct 2020), the gap in median incomes *shrinks* during the pandemic, when most unemployed households first experience unemployment. This reflects that many unemployed households experienced an increase in income while receiving supplemental unemployment benefits. Second, it is notable that same pattern exists for total spending: just as the income gap between employed and unemployed households shrinks during the pandemic, so does the gap in median total spending.

2.2.2 Comparison to External Benchmarks

The massive increase in unemployment benefits is readily apparent in the JPMCI data. We compare the number of continued claims in the Department of Labor (DOL) data to the number of households receiving unemployment benefits in the JPMCI data. From early March to June, Appendix Figure A-3 shows that these series rose by a factor of 15 in DOL and a factor of 17 in JPMCI. The figure also shows that the increase in UI payments to households that meet the account activity screens in JPMCI is a bit smaller, rising by a factor of 11. It is not surprising that this subsample has a smaller increase in unemployment benefit receipt. The pandemic recession has been particularly difficult for underbanked households, who are likely to be omitted because of these account activity screens. We calculate using the Current Population Survey that the unemployment rate from April to September 2020 has been 4 to 5 percentage points higher for underbanked households.

The JPMCI data also reproduce differences across states in the magnitude of the increase in UI as well as the level of weekly UI benefits. Appendix Figure A-4 shows that the states with the largest increase in UI claims (e.g. Florida, North Carolina) in DOL also have the largest increase in JPMCI. Conversely, the states with the smallest increases in DOL (e.g. Wyoming, Idaho, West Virginia) also have the smallest increase in JPMCI. Appendix Figure A-5 shows that there is a strong cross-state correlation between benefit levels in DOL and benefit levels in JPMCI.

The figure also shows that weekly benefit levels are a bit higher in JPMCI than in DOL. UI benefit levels are set based on a worker's pre-separation earnings, so higher benefit levels in JPMCI imply that unemployment benefit recipients in JPMCI have slightly higher pre-separation earnings than the average UI recipient in each state. This pattern is largely explained by the effect of the account activity screen, which imposes a minimum level of pre-separation earnings that is more stringent than the eligibility requirements for UI. However, we note that repeating our job exit analysis without this screen produces very similar results, and we conjecture that the consumption responses we estimate are a lower bound for consumption responses among the full population of UI recipients since this screen induces mild positive selection in terms of labor market attachment and financial well-being.

Overall, we conclude that the JPMCI data does a good job of capturing both the massive national increase in UI receipt as well as the cross-state heterogeneity which can be captured using statistics reported by DOL. For additional analysis of the representativeness of unemployed households in Chase data, see Ganong and Noel (2019).

3 Overview of Unemployment During the Pandemic

Before turning to the effects of the benefit supplements, we first provide a brief description of unemployment during the pandemic as viewed through the lens of benefit receipt in JPMCI. In particular, we document the presence of simultaneous long-term continuous unemployment together with shorter-term but *repeated* unemployment spells. While it is well-known that there is less long-term unemployment than at comparable points in the Great Recession, this hides the fact that there is a much larger amount of repeated unemployment.

Figure 1a shows the massive increase in unemployment at the start of the pandemic and Figure 1b shows the number of benefit starts. These patterns are already well understood based on the Department of Labor data. However, a key strength of our data is the ability to construct *spells* of unemployment and track workers' labor market experiences over the course of the pandemic to date. Table 1 summarizes our findings.

First, we find that there is much more instability in the labor market than before the pandemic. Figure 1b shows that the number of unemployment starts *and* the number of exits is highest in the summer. Even in the fall, as the overall unemployment rate (as measured by the Current Population Survey) fell, the number of people entering and exiting unemployment was about four times bigger than before the pandemic. Because the number of people exiting slightly exceeded the number of people entering, the total number of benefit recipients declined slightly.

Second, we document a high prevalence of long-term continuous unemployment. Table 1 shows that 53 percent of the people who are unemployed in October have been unemployed continuously since May. This pattern accords with external evidence from the Current Population Survey. Long-term unemployment raises particular concerns about scarring and skill depreciation relative to shorter-term unemployment.

Third, at the same time that many workers are experiencing long-term continuous unemployment, we uncover new evidence that many workers are experiencing *repeated* unemployment. Table 1 shows statistics which summarize the repeated experience of unemployment during the pandemic. From the end of August onward, more than half of UI starts reflect workers who already previously received UI during the pandemic.[10] Our findings echo evidence from Chodorow-Reich and Coglianese (2020) who find a high degree of labor market instability among the recently unemployed in the labor market during the pandemic.

One consequence of this pattern of *repeated* unemployment is that the amount of time that a worker has been unemployed in their most recent spell understates the extent to which they have experienced labor market displacement during the pandemic. For example, Appendix Table A-2 shows that although the median spell length is 8 weeks, the median number of total weeks of unemployment is 12. Similarly, the 75th percentile spell length is 19 weeks, while the 75th percentile of total weeks of unemployment is 23. This implies that estimates of unemployment duration from the Current Population Survey (which asks about the length of the current unemployment spell) understate the full extent of unemployment experiences during the pandemic.

Together, the presence of both long-term continuous unemployment and repeated unemployment indicate that there is a subset of the US population which has been particularly impacted by the economic effects of the pandemic. Realistic models of the labor market should seek to capture this high

[10] Because we observe household-level data, it is also possible that these patterns reflect unemployment spells by two different members of a household.

prevalence of repeated unemployment spells. In particular, among those who have not been continuously unemployed, the typical experience has been one of repeated unemployment. Many of the workers who were recalled in the summer were laid off again in the fall.

4 Spending Responses to Expanded Unemployment Benefits

This section explores the empirical effects of unemployment benefits on spending. We begin with descriptive analysis. We next identify one-month causal responses separately to the start and to the expiration of the $600 supplements as well as to the temporary $300 supplements. Finally, we provide estimates of spending responses to the $600 supplements over longer horizons. Each of these empirical exercises has distinct advantages and disadvantages, but they all lead to the same bottom-line conclusion: spending consistently rises and falls with unemployment insurance benefits.

4.1 Descriptive Patterns: Spending of the Unemployed *Rises* After Job Loss When Expanded Benefits Available

We begin our empirical analysis with a descriptive exploration of income and spending time-series patterns from January 2019 through October 2020. In particular, Figure 2 compares changes in spending and income for households who become unemployed in April 2020 to households who remain employed throughout 2019-2020. As shown in Table A-1 the unemployed have somewhat lower pre-separation income than the employed during the pandemic. This is not surprising since the pandemic resulted in disproportionate job losses for low-income workers. In order to construct a more comparable comparison group, we thus re-weight the employed sample so that it matches the distribution of pre-separation income for the unemployed and also matches the frequency of Economic Impact Payment (EIP) receipt by date.[11] Matching along income is potentially important since households at different points of the income distribution may have spending that evolves differently over the pandemic, and matching the timing of EIP date is potentially important since these payments arrive around the time that $600 supplements start.[12]

Figure 2 shows that until the start of unemployment in April 2020, monthly income evolves nearly identically for the two groups. Beginning with the start of unemployment, income of the two groups diverges substantially. Since the unemployed group in this period receives regular UI plus the $600 supplement result in average replacement rates above 100 percent, income actually *rises* substantially for the unemployed from April through July (both relative to their pre-pandemic income and relative to the income of the employed) At the end of July, the $600 supplements expire, and so the income of the unemployed falls below that of the employed. In September, many of these unemployed households receive $300 supplements and their income again rises, before falling again in October.

Figure 2 also shows the evolution of monthly spending for the two groups. The middle panel shows evolution of total spending, while the bottom panels shows the evolution of more narrow cash and card spending. As discussed in 2.2 we generally prefer the more comprehensive spending measure for monthly analysis and use the more narrow measure for sub-monthly analysis.

[11] More specifically, we construct cells with counts of unemployed and employed by 2019 income quintile and EIP receipt date and construct weights so that the proportion of the employed matches the proportion of unemployed in each cell.

[12] In practice, all of our results are nevertheless quite similar if we instead use the raw unweighted employed as a comparison group.

This figure shows that spending evolves nearly identically for the unemployed and employed prior to the point of unemployment. However, after households become unemployed (and receive $600 supplements), their spending rises substantially *above* pre-pandemic levels.[13] This is even more notable when compared to the declining spending of employed households during the pandemic. In normal periods of time, unemployed households reduce spending relative to employed households, but during the period of $600 supplements, these normal patterns are totally reversed.[14] When the supplements end at the end of July, there is an immediate decline in spending. This is then followed by a rebound in spending when unemployed households receive $300 supplements in September, followed by another decline in October when this second round of supplements ends. Thus, as a descriptive statement, we find a strong relationship between unemployment benefit levels and the spending of the unemployed throughout the pandemic. Finally, it is important to note that the increases in income for unemployed households during this period were so large that these households accumulated additional savings throughout this period even as their spending increased, as shown Appendix Figure A-6. This large increase in liquidity for unemployed households will be important when we turn to model implications of our spending facts.

Overall, the fact that the spending and income of the unemployed move closely with spending and income of the employed prior to the pandemic and then diverge sharply in a way that mirrors the timing of benefit supplements suggests that these supplements indeed boosted the spending of the unemployed during this period. However, it is difficult to interpret these patterns as causal estimates, since a causal interpretation of these time series trends would require the untenable assumption that all differences in spending between these two groups over this entire time period were driven by differences in observed income over the same period. This assumption is unlikely to hold, for a variety of reasons. For example, it will be violated if unemployed households update their expected duration of unemployment during the pandemic and these expectations affect their spending.

4.2 Causal Evidence

In this section, we provide causal estimates of spending responses to various changes in unemployment benefit levels throughout the pandemic. In particular, we estimate the one-month marginal propensity to consume (MPC) out of income changes induced by three different changes in unemployment benefits. We first estimate the spending response to the onset of unemployment benefits. We next estimate the response to the expiration of the $600 supplements. Finally, we estimate the response to the temporary $300 lost wage assistance payments in September.

These three estimates allow us to quantify the impact of unemployment insurance on spending using different points in time and both positive and negative sources of variation. Importantly, this means that each empirical exercise identifies related but slightly different policy counterfactuals, which we discuss in more detail after developing our structural modeling environment. Our benefit onset design focuses on spending impacts of unemployment insurance when supplements are first available, but it cannot distinguish the separate effects of the $600 supplements from the effects of regular unemployment benefits. The expiration design focuses on spending responses immediately after the $600 supplements ended in July, allowing us to isolate the role of the $600 supplement from that of regular unemployment benefits. Finally, the analysis of the temporary $300 supplement in September provides

[13] Since the $600 supplements begin in the middle of April, May is the first complete month of spending after supplements begin.

[14] For example, Ganong and Noel (2019) find that the spending of the unemployed declines by about 7 percent during normal times.

information about the effect of liquidity on spending responses to benefits, since this payment comes after unemployed households had accumulated substantial savings as a result of months of generous benefits.

4.2.1 Benefit Onset

We begin with an analysis of the spending response to the onset of unemployment benefits. In order to do so, we compare the spending of various cohorts of unemployed households who all lose their jobs at the end of March, but who then begin receiving unemployment benefits at different dates. That is, we compare the spending response of a "treatment" group of unemployed households who receive unemployment benefits early to the spending of a "control" group of unemployed households who face delays in receiving benefits. The identifying assumption is that absent benefit receipt, the spending of the treatment group would have otherwise evolved in the same way as the spending of the control group facing benefit delays. We think this is a reasonable assumption, given that benefit delays were largely driven by back logs in state UI systems, which were plausibly orthogonal to individual circumstances. Furthermore, we note that focusing on the difference between two groups of unemployed households removes any direct effects of job loss itself on spending and isolate the effect of unemployment benefits, since both groups are subject to job loss at the same time.

Figure 3 provides stark evidence of parallel pre-trends to bolster this assumption. The upper panel shows weekly patterns of unemployment benefits for four different groups of unemployed who lose their jobs at the end of March but first receive unemployment benefits at different dates. By construction, benefits are zero for each group prior to the first benefit week and then jump in the first week of benefits.[15] The bottom panel of Figure 3 shows the behavior of spending for these different groups. There are two striking observations: 1) The level of spending and its evolution over time are nearly identical for each unemployed group, prior to the receipt of unemployment benefits. 2) Spending jumps sharply in exactly the week in which benefits start and then remains at an elevated level in subsequent weeks. This strong parallel pre-trend with a sharp jump in spending coinciding exactly with the start of benefits bolsters the case that comparing spending at benefit onset to the spending of a control group facing delays is a valid identification strategy.

In order to quantify causal effects at a month level, we estimate the difference in difference in spending between March and May for unemployment groups who start benefits in April vs. June. More specifically, we run the following instrumental variables regression with first and second stages respectively given by equations 1 and 2.

$$y_{i,t} = \alpha + \beta * Post_t \times Treat_i + Treat_i + Post_t + \epsilon_{i,t} \tag{1}$$

$$c_{i,t} = \psi + MPC \times \widehat{y}_{i,t} + Treat_i + Post_t + \varepsilon_{i,t}, \tag{2}$$

where $t = March, May$, $Treat = 1$ for households who become unemployed at the end of March and start benefits in April and $Treat = 0$ for households who become unemployed at the end of March but start benefits in June. $Post_t = 1$ if $t = May$ and $Post_t = 0$ if $t = March$.

[15]Groups which start benefits later have larger benefit jumps in the first week, since households are typically eligible for "backpay" which results from processing delays. For this reason, although we show all groups to provide additional support for the trends between treatment and control, we focus on estimating the spending responses of initial cohorts which do not receive backpay.

Why do we make these particular timing choices? We showed weekly spending on cash and cards above, since this provides the sharpest test for our "waiting for benefits" research design. However, we are primarily interested in broader measures of spending, which can be somewhat less reliably measured at weekly frequencies due to the presence of paper checks.[16] Furthermore, we are particularly interested in monthly MPCs, since these can be easily compared to our model implications below. Finally, since the $600 supplement begins in mid-April, it is difficult to interpret full-month spending and income in April. For all of those reasons, we focus on the difference-in-difference in spending between March and May, for unemployed groups who start benefits in April vs. June. The identifying assumption is that absent the start of unemployment benefits, the change in spending between March and May for the treatment group would be the same as the change in spending for the control group.[17]

Table 2 row 1 shows results. Focusing on total spending, we find an MPC of 0.43 at onset, implying that nearly half of unemployment benefits were spent in the first month after receipt. We discuss the interpretation of these magnitudes in more detail in Section 4.3.4 after presenting causal responses to additional sources of policy variation.

Importantly, we note now that the nature of this waiting-based research design means that it measures the MPC out of total unemployment benefits (regular unemployment plus the $600 supplements) rather than the response to the supplements alone. This is because the unemployed group that starts receiving benefits in April also receives these additional supplements at the same time. This MPC is of direct interest for understanding the overall role of unemployment benefits on spending during the pandemic. However, it does not tell us the separate effects of regular and supplemental benefits on spending. Furthermore, this waiting design identifies the effects of benefit delay rather than benefits per se, which complicates its structural interpretation in a way that exactly mirrors the interpretation of tax rebate timing in Kaplan and Violante (2014).

To try to understand the direct role of the $600 supplements we pursue two complementary strategies. Using the structural model we develop below, we can map the causal responses at the start of regular UI + $600 that we estimate in our waiting design to the causal effects of starting the $600 supplements alone, which we do not observe in the data. However, we first turn to evidence on the spending response to benefit expiration in order to provide model-free evidence on the effect of the $600 supplement separate from the effects of regular benefits.

4.2.2 Expiration of $600 Benefit Supplements

Our next piece of empirical evidence looks at the spending response to the expiration of the $600 weekly benefit supplement at the end of July. Since long benefit delays were somewhat less common by the end of the summer, we focus instead on a treatment-control approach which compares the spending and income of the unemployed to a group of employed households with similar observables immediately before and after the expiration of the $600 benefits.[18] As in Section 4.1, we choose a group of employed households who has similar pre-pandemic income and also receives similar EIP amounts. The identifying assumption is that income and spending *changes* would have been the same for the unemployed and employed at the time of supplement expiration, if not for the expiration of

[16] Appendix Figure A-7 nevertheless shows that patterns are similar, albeit somewhat noisier when looking at weekly measures of total spend.

[17] More formally, we require that $cov(Post_t \times Treat_i, \varepsilon_{i,t}) = 0$.

[18] In future work, we plan to explore the feasibility of extensions of the waiting for benefits design to these later time periods.

the supplement. This would clearly be a poor assumption when studying the response to the start of unemployment benefits in Section 4.2.1, because at the start of an unemployment spell, any effects of the job loss itself on spending confound the separate causal effects of unemployment benefits.[19]. No such confound between the unemployed and employed is present at supplement expiration for households who became unemployed months before the expiration of the benefit.

Bolstering these claims, Figure 4 shows that weekly spending evolves very similarly for the employed and unemployed from June through August, followed by a sharp drop in the spending of the unemployed immediately after the end of the \$600 supplements. For the same reasons described above, our quantitative estimates of MPCs focus on total monthly spending.[20] We again use the IV approach in Equations 1 and 2, but with $t = July, August$, $Post$ equals one in August, and zero in July. The control group is the set of households with continuous employment, and the treatment group is the set of households who experience job loss at the end of March, begins benefits by June 14 and continue receiving benefits through at least August 30.

Table 2 row 2 shows results. We find an MPC of 0.29. As we will show later, spending responses of this magnitude are larger than what are predicted by the model if households correctly anticipated that the supplements would expire.

4.2.3 Responses to the \$300 Benefit Supplements

Our final piece of empirical spending analysis focuses on the response to the \$300 benefits. These supplements were announced in August and applied to unemployment spells in August but were largely paid out in September. In order to estimate the effects of these payments, we exploit state-level variation in the timing of these payments. In particular, most states paid out the \$300 payments in September, but New Jersey made these payments later, in October. Our empirical specification thus compares the spending of a treatment group which receives payments in September to a control group consisting of unemployed households in New Jersey, who receive payments in October.[21]

Figure 5 shows that spending of the treatment and control group track each other closely until the treatment group receives these \$300 payments, at which time their spending rises.[22] We again compute MPCs using the IV regression in Equations 1 and 2, but now the control group refers to unemployed households in New Jersey, the treatment group is households in other states. These payments were typically made over a six week period beginning in September, so the treatment period is the 6 weeks from 9/6 to 10/11 and the pre-period is the 6 weeks from 7/26 to 8/30.

We also explored an alternative identification strategy following that in Section 4.2.2, which compares the spending of the unemployed and employed around this time. However, at the time unemployed households receive the \$300 payments in September, they are still reducing spending in response to the \$600 expiration in August. This means that there is a moderate pre-trend in which the spending of unemployed declines relative to employed in the weeks preceding the \$300 payment. This differential pre-trend does not exist when comparing across groups of unemployed households in different states,

[19] In addition to the loss of income itself, there may also be changes in home production, work related expenses, etc.

[20] Appendix Figure A-8 shows that similar weekly patterns hold with total spending.

[21] Specifically, our treatment group includes households in eight states that paid out LWA primarily in September: Florida, Georgia, Illinois, Indiana, Michigan, New York, Ohio, and Washington. Note that although we are able to identify LWA payments in California, we exclude California from this analysis because its LWA payments to many households spanned both September and October, and therefore California does not fit into our timing-based treatment/control framework.

[22] See also Appendix Figure A-9 for similar patterns with broader spending measures.

so we prefer the state-based identification strategy. However, we have also computed MPCs using the unemployed vs. employed design and it produces relatively similar estimates.

4.2.4 Estimating Spending Responses Over Longer Time Horizons

The nature of the causal estimates thus far mean that they identify only spending responses in the first month after benefits are changed. As we discuss below, the magnitude of these responses is already sizable when compared to external estimates of spending responses to other policy changes or when compared to implications from structural models. However, spending responses over time horizons longer than a month are clearly also of direct policy interest.

We use two complementary strategies to try to gauge responses at longer horizons. First, we explore an additional empirical strategy which compares the spending and income of unemployed households (relative to employed households) over several months in 2020 after they are eligible for $600 supplements to the spending of unemployed households (relative to employed households) over the same months in 2019 who are not eligible for these supplements. Second, we compute the relationship between one-month MPCs an MPCs over longer horizons in the structural model that we develop below. Neither approach is perfect, but both are likely conservative relative to the true size of MPCs.

The empirical strategy relies on an assumption that differences in the spending of unemployed relative to employed households in 2020 differs from that in 2019 only because of the difference in income across years. Since pandemic effects generally depress spending in 2020, this likely means that the MPCs we estimate with this approach will understate true causal effects. The model estimates over longer horizons are also likely to underestimate the true spending responses, since we show below that the model estimates are smaller than the causal effects we can identify at short-horizons. Nevertheless, both approaches deliver longer horizon spending responses which are large and similar to each other.

Our empirical approach to estimate MPCs at a range of horizons estimates the consumption effects of unemployment using a difference-in-difference specification analogous to equation 2. $Treat_i$ is defined as the group that becomes unemployed (with the employed as a control group) and $Post_i$ is defined as the period in which unemployment occurs (or in the comparable calendar month for the employed group). The pre-period is defined as three to five months prior to receipt of the first unemployment benefit. The specifications differ in two minor ways. First, instead of running a first stage regression, we simply compute the number of weeks within the sample horizon in which the household received the $600 supplement. Second, we estimate equation 2 twice: once for those who become unemployed in 2019 and separately for those who become unemployed at the start of April 2020. In both cases, we require that the household receive unemployment benefits for at least five months. We interpret that difference in the spending response to unemployment between the 2019 cohort and the April 2020 cohort as the causal impact of the $600. We estimate a one month MPC using spending in the month *after* UI payments begin, which corresponds to May 2020 for the pandemic cohort. We estimate a three-month MPC using spending in the three months after UI payments begin, which corresponds to May-July 2020. Finally, we estimate an MPC over the full time horizon available in the data, which is up to six months after UI payments begin (April-October 2020 for the pandemic cohort). We divide by a denominator of one month of PUC (4.3 weeks) for the one month MPC, three months of PUC (13 weeks) for the three month MPC and the full length of PUC (15 weeks) for the full time horizon.

Performing this empirical exercise delivers a one-month MPC out of the $600 benefits of 0.32, a three-month MPC of 0.38 and a six-month MPC of 0.62. It is useful to note that this one-month MPC of 0.32 is similar to the one-month MPC of 0.29 that we identify in our expiration-based strategy, while relying on a distinct source of variation.

4.3 Implications in a Benchmark Consumption-Savings Model

Our results thus far show consistently sizable spending responses to changes in unemployment benefit levels throughout the pandemic. In the next part of the paper, we interpret this "model-free" empirical evidence through the lens of an intentionally standard model of household consumption-savings model with borrowing constraints, which is parameterized to match MPCs in pre-pandemic data.

This modeling exercise has several goals. First, comparing the empirical effects we estimate in the previous sections to predictions from a theoretical model based on pre-pandemic micro data provides a concrete way to benchmark the size of these effects. Second, we can use the model to relate the causal estimates we observe in the data to outcomes of interest which we cannot directly measure in the data. For example, our waiting design compares households starting benefits early to those getting benefits late. This research design estimates the causal effect of receiving regular unemployment plus the $600 benefit, but it does not identify the effect of the $600 benefit alone. In the model, we can relate the total effect that we can measure to the effect of just the $600 benefit that we cannot.

Third, our unique empirical setting with identified spending responses to a sequence of policy changes allows us to test *dynamic* implications of this class of models. In particular, these models predict that as specific households accumulate liquidity, they should respond less strongly to transitory income shocks. The important role of liquidity in the cross-section is widely established in the data, but these dynamic predictions are largely untested. Notably, we find that this model is consistent with the large initial responses to the $600 benefits but it struggles to match the fact that spending continues to respond sharply to later changes in benefits, once households have accumulated substantial liquidity.

4.3.1 Model Description

Our basic modeling framework is intentionally standard. The model time period is monthly and households exogenously supply labor, which is subject to idiosyncratic earnings risk. Households choose consumption c and savings a to maximize the present value of expected utility $E\sum_{t=0}^{\infty}\beta^t U(c)$, subject to a borrowing constraint $a \geq 0$. Households earn interest rate r on savings $a > 0$.

Household earnings risk has two components: when employed, households are subject to idiosyncratic changes in wages w. Idiosyncratic wages follow an AR process: $\log w' = \rho \log w + \epsilon$, with $\epsilon \sim N(0, \sigma)$. Households also face unemployment risk. We denote by $s \in \{e, u\}$ the current unemployment status of a household, with $s = u$ denoting unemployment. This unemployment process follows a first order Markov chain with transition probability $\pi(s'|s)$.[23] When unemployed, households are not paid their wage but are eligible for unemployment benefits.

Income for an unemployed household depends on both aggregate UI policy and whether the household is waiting to receive benefits. The size of unemployment benefits depends on the current aggregate UI supplement in place: $m \in \{0, 300, 600\}$.[24] In addition, in order to speak to some of our empirical

[23] We take the job-finding rate to be exogenous in this model. Because in practice we find only small changes in the job-finding rate in response to the unemployment benefit supplements, we think it is likely that endogenizing job-finding will not change the model's conclusions about consumption. In future work, we plan to study a model with endogenous consumption and endogenous job search.

[24] Note that while log labor earnings as described above are mean zero , we rescale the level of labor earnings in the model so that all series in dollars in the model match the average income unemployed relative to employed in the data

identification strategies, we allow for the possibility that an unemployed household may face delays in receipt of UI and in turn later receive backpay. This means that unemployed households can be in one of three receipt statuses: $d \in \{normal, delayed, backpay\}$.

The normal unemployment benefit policy is kept intentionally simple: when households are unemployed, they are eligible for unemployment benefits which replace a constant fraction b of their lost earnings.[25] When they are available, unemployment supplements add m to these baseline benefits. This means that an unemployed household getting benefits with no delay receives $bw + m$, where we again note that $m = 0$ with normal unemployment benefits and instead equals 300 or 600 under the two supplement policies we study.

Unemployed households can also be in a delayed unemployment insurance status and not currently receiving benefits if $d = delayed$. In this case, their current period earnings are given by $\gamma_0 w$. Allowing for $\gamma_0 > 0$ reflects the fact that income in the data is typically greater than zero even for those waiting for UI, as a result of family labor supply. When households exit this delayed status, the receive backpay equal to $\gamma_b(bw + m)$, where γ_1 is chosen to match the degree of "backpay" observed in the data.

This means that total earnings y are given by

$$y(w, s, m, d) = \begin{cases} w & \text{if } s = e \\ bw + m & \text{if } s = u \text{ and } d = normal. \\ \gamma_0 w & \text{if } s = u \text{ and } d = delayed. \\ \gamma_1(bw + m) & \text{if } s = u \text{ and } d = backpay. \end{cases} \tag{3}$$

The actual and expected evolution of m and d depends on the particular state. The economy begins in a steady-state UI policy environment in which $m = 0$ and $c = normal$ and households expect that UI policy will never change. Beginning from this initial steady state, the economy is hit by a specific monthly sequence of realized aggregate policy changes which are meant to capture changes in UI policy from April-October, 2020. We first describe this sequence of realized policy changes, and then describe several alternatives for how expectations evolve over time. In April, the economy switches from $m = 0$ to $m = 600$ and it remains in this state for 4 months. In August, it switches to $m = 0$. In September, it switches from $m = 0$ to $m = 300$. Finally, in October, it switches back to $m = 0$.[26] The implied income path for an average unemployed worker under this sequence of policies is given by the solid blue line in Figure 6.

While this describes the evolution of actual policy through this period, we must also specify how expectations about the evolution of m and d are formed. We begin with a discussion of m. We assume that the initial switch from 0 to 600 is an unanticipated event. Once FPUC is implemented, households then know for sure that FPUC will last *at least* through July, since this duration was implemented in the initial legislation. However, expectations about renewal are more difficult to discipline. In order to bound a range of plausible expectations, we report results for two different renewal expectations. In the first specification of expectations, households expect that m will revert from 600 to 0 permanently in August. In the alternative specification of expectations, households instead expect that $m = 600$

under different UI policies.

[25] That is, for computational simplicity, we ignore benefit caps and assume no limit on benefit duration. Our model features no permanent heterogeneity in labor earnings, so ignoring benefit caps is not a major restriction. In addition, for the period of time in which we focus our analysis in the data, most households are far from benefit exhaustion, so modeling finite duration would have little quantitative effect on our conclusions about consumption.

[26] Note that our data currently ends before the second round of $300 payments were implemented in December 2020 legislation but it can easily be extended to incorporate these payments in the future.

indefinitely, and are then surprised in August when it expires.[27] Once m=600 expires in August, households expect that $m = 0$ forever. Thus, they are again surprised by the start of m=300 in September. Since the $300 supplement was announced more prominently as a temporary policy, we assume that although households are surprised by the switch from $m = 0$ to $m = 300$, they expect the change from $m = 300$ back to 0 in October.[28]

We consider two polar scenarios regarding expectations about the duration of the $600 weekly supplement. The supplement was legislated to expire at the end of July and we evaluate a scenario where expiration is expected in our model. However, as we discuss in Section 2.1, both Democrats and Republicans proposed extending the duration of the supplement. It is likely that at least some recipients were surprised when Congress did not legislate an extension and the supplement lapsed. We therefore evaluate a second scenario in which recipients expect that the supplement will continue for as long as they receive unemployment benefits and are then surprised when the supplement expires at the end of July.

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 processing. 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. However, while households always anticipate that delays in benefit receipt will be resolved the following period, 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.[29]

Letting n represent the expected number of periods until $m = 0$, the household optimization can be expressed recursively as

$$
\begin{aligned}
V(a, w, s, m, d, n) = \max_{c,a'} U(c) + \beta E_{w',s',m',n',d'} V(a', w', s', m', d', n') \\
& s.t. \\
& a' + c = y(w, s, m, d) + (1 + r)a, \\
& a' \geq 0, \\
& \log w' = \rho \log w + \epsilon, \\
& \text{Equation 3}, \\
& \text{and expectations of } m', d', n'.
\end{aligned}
$$

4.3.2 Calibration

Our model is monthly. We set the implied annual interest rate $r = .04$. We assume that the utility function is given by $U(c) = \frac{c^{1-\gamma}}{1-\sigma_u}$ and set $\gamma = 2$. We calibrate the monthly wage process to match the annual persistence and standard deviation in the PSID calculated by Floden and Lindé (2001).

[27] We have also explored versions of the model in which households expect supplement renewal with some probability $\in (0, 1)$ rather than the extremes of 0 or 1. This model is modestly more complicated to solve, but unsurprisingly it generates results that are between the two exercises we report.

[28] Put differently, the 300 supplement is a standard purely transitory income shock.

[29] While it is straightforward conceptually to introduce stochastic waiting times, this substantially complicates the computation of the model so we abstract from it for now.

Specifically, they find an annual value $\rho_{annual} = .91$ and $\sigma_{annual} = .21$, so we choose $\rho = .9922$ and $\sigma = .032$.[30] We calibrate the employment-unemployment transition matrix to match evidence on quarterly recessionary job separation and finding rates in Krueger, Mitman, and Perri (2016). This implies that the monthly transition matrix is given by:[31]

$$\begin{bmatrix} \pi_{uu} & \pi_{ue} \\ \pi_{eu} & \pi_{ee} \end{bmatrix} = \begin{bmatrix} 0.317 & 0.683 \\ 0.0288 & 0.9712 \end{bmatrix} \tag{4}$$

This implies a steady-state value of unemployment of 8.3 percent.[32]

In our baseline calibration we pick a discount factor of $\beta = 0.9$, but we show robustness to alternative discount factors. Given the other parameter choices, $\beta = 0.9$ implies that the model generates a 3-month marginal propensity to consume of 0.27, which is in line with the target in Kaplan and Violante (2014) of 0.25.[33]

4.3.3 Results

Table 3 shows model counterparts to the causal policy effects we identify in our empirical work. As mentioned above, we compute results for two versions of the model: in one the expiration of the $600 is expected, and in the other the expiration is a surprise. We begin by briefly summarizing these results before turning to a more thorough discussion of the model mechanisms which deliver them.

The first row shows that both models imply sizable MPCs at onset of unemployment benefits, in line with empirical estimates. The second row shows that expectations play an important role in determining the MPC when the $600 expires. The spending response to expiration is much larger when the expiration of the $600 supplement is a surprise than in the version of the model where households anticipate the supplement will expire. Finally, the third row shows that the spending response to the the $300 supplement in September is small in both versions of the model.

What explains these results? To understand the spending response at benefit onset, it is useful to note that the presence of borrowing constraints and income risk implies a concave consumption function, as shown in Figure 7 Panel (a). The marginal propensity to consume out of a one-time, unanticipated change in income is given by the slope of the consumption function. Figure 7 Panel (b) shows that this MPC declines rapidly with assets: the MPC is one for households with no assets but drops rapidly as households accumulate assets before eventually converging to a constant, low value as the consumption function converges to linearity.

The fact that households with low liquidity respond more strongly to *marginal* changes in income is important for understanding the model responses to benefit onset. In these policy experiments, the spending of an unemployed household receiving regular benefits plus the $600 supplement is compared to an unemployed household who currently receives no unemployment benefits at all. Under our baseline

[30] $\rho = \rho_{annual}^{\frac{1}{12}}$ and $\sigma = \left(\left(\frac{\sigma^2_{annual}}{(1-\rho_{annual})^2} \right) \left(1 - \rho^2\right) \right)^{\frac{1}{2}}$.

[31] That is, π^3 is equal to their recession value transition matrix

[32] We have also explored alternative calibrations instead targeting the non-recession transition process from Krueger, Mitman, and Perri (2016). If we pick the discount rate in these two calibrations to generate the same level of savings, the model calibrated to match the greater persistence of unemployment in recessions implies modestly larger spending responses to the 4 month $600 supplement. In future work, we plan to explore versions of the model in which the job separation and finding rate vary across time.

[33] We note that the CEX data underlying this target is not directly comparable to the spending measures in our JPMCI data. In future work, we hope to replicate this evidence from earlier time periods using consistent JPMCI data definitions.

calibration of β, this unemployed household waiting for benefits runs down their savings and so has $a = 0$ and an MPC of 1. This fact that unemployment benefits target low liquidity households with large spending responses helps explain the large model spending responses to the onset of unemployment benefits.

However, we note that two important features of the $600 supplements lead to responses which deviate from this MPC. 1) The benefit supplements are not marginal changes in income, they are large changes in income: in the absence of these supplements, the monthly income of unemployed households falls by 20%. With the supplements, income instead rises by 30%. As mentioned in footnote 2, while we follow the convention in the literature by describing our causal estimates as MPCs, they are more accurately described as APCs and should be compared to APCs to the same size shocks in the model. 2) The $600 supplements are not transitory: these supplements were provided with an initial duration of four months, and in some versions of the model, households expect the supplements to then be further renewed.

Figure 8 panel (a) illustrates the role of the size of income changes in affecting spending responses. The blue line in this figure is the same as in Figure 7 (b), and shows what fraction of a one-time marginal income shock is spent in the first month, for different levels of pre-shock assets. The red line recomputes this spending ratio but simply using a larger change in income, equal to the size of the benefits supplement. The figure shows that for unemployed who start with little assets, this *average* propensity to consume out of a large income increase is much smaller than the *marginal* propensity to consume out of a small income increase. This is because the large income increase essentially pushes households away from being liquidity constrained.

Figure 8 panel (b) illustrates how the dynamics of household liquidity in the model interact with unemployment over time under different benefit policies. The blue line in this figure again shows the MPC for an unemployed household as a function of their current asset positions. The red-dashed line is drawn at the average MPC of 0.22 for the economy as a whole (averaging over all household income states and endogenous asset choices).[34] The colored dots are then illustrate MPCs for households at different asset positions of interest. Households who are waiting for benefits for a month have a large decline in income, exhaust their assets and thus have an MPC of 1 out of temporary increases in income. Households who receive regular unemployment benefits are shown in purple. They also have a decline in income and draw down assets, but since they receive regular benefits, this income loss and asset draw-down is less extreme than those waiting for benefits. These households have higher MPCs (0.30) than the economy as a whole (0.22) but well-below those who are waiting for benefits. In contrast, the benefit supplements are large enough that households receiving regular benefits plus the $600 supplement actually have greater liquidity and lower MPCs than when they were employed. This means that they have MPCs which are lower than the average MPC in the economy. After one month of expanded benefits (in green), the MPC of the unemployed drops to 0.12 and after four months of expanded benefits (in blue) the MPC drops to 0.06. Overall, the large size of unemployment benefit supplements reduces the share of the benefits which are spent in the model by increasing liquidity and reducing the share of marginal dollars which are spent.

However, the second feature which complicates comparisons between causal empirical estimates and MPCs out of transitory income changes works in the opposite direction: the $600 supplements

[34] This average MPC is not the same as the 0.27 discussed in calibration, since that number was a 3-month response to a $500 transfer rather than a 1-month response to a $1 transfer.

are not transitory. The fact that these supplements had some persistence increases the share of the supplements which are spent in the model. Figure 9 shows that households spend more of the first month's supplement in the first month when they expect the supplement to last four months (in yellow) than when they expect it to last one month (in red). This is the standard result that consumption smoothing motives will imply larger responses to more persistent than to transitory income changes. Unsurprisingly, spending responses in the first month are even greater when households expect the supplement to be renewed after four months (in purple) than when they do not (in yellow). However, even in this case, the spending response is well below one. This is interesting, because in this case households expect the supplement policy to last forever. One might then expect that this would imply both an MPC and APC of 1. However, this is not the case because even though the policy is expected to last forever, households do not remain unemployed forever. The expected duration of unemployment thus bounds the expected duration of supplement receipt, even if households expect the supplement to be renewed perpetually.

Overall Figure 8 together with illustrate the simultaneous importance of targeting, transfer size and persistence in determining spending responses in the model. UI supplements are targeted towards households who are initially low liquidity and they are somewhat persistent. These forces generate large spending responses. Conversely, the transfers themselves are quite large, which relaxes these liquidity constraints and reduces the share which is spent.[35]

These same model forces also explain the model implications for spending responses at the $600 expiration as well as those to the temporary $300 supplement. In stark contrast with the data, the version of the model in which expiration is anticipated generates very little spending change in response. This is because expiration of the $600 is anticipated and the supplements are large, so households accumulate enough liquidity over the four months to smooth spending when the $600 ends. The alternative version of the model where expiration is a surprise is a better fit to the data. Households who are unemployed at the time of unexpected expiration face an unanticipated persistent decline in income and cut spending in response.[36] This spending cut is smaller than the spending rise at onset, because households have more liquidity at expiration than onset, but there is still a sizable change in spending since this is a large and at least somewhat persistent decline in income. Thus, the model in which households initially expect the $600 supplement to be renewed and are then later surprised is a better fit to spending patterns.

However, neither version of the model is able to match the response to the one-month $300 supplement paid out in September. This is because this is a smaller, transitory increase in income and unemployed households at that point still have substantial liquidity remaining from the $600 supplements paid in the prior months. Standard models built on consumption smoothing motives predict that spending responses to this temporary supplement should be much lower than at onset, because households have substantial liquidity at this point.[37] Thus, we conclude that while the version of the model with surprise expiration does the best job of fitting the data, it still implies spending which responds in a less consistently strong way to changes in benefits than what we observe in the data.

[35]We refrain from presenting an exact decomposition of the role of each effect since the model is non-linear so that effects are not additive and depend on the ordering of the decomposition. However, we note that under various different alternatives, all three effects are quantitatively important.

[36]The fact that this is a more persistent decline in income also explains why the response is larger than the response to the $300 temporary increase despite substantial liquidity.

[37]Note that we focus for simplicity on a single-asset model, but this same logic would extend to more sophisticated two-asset models as in Kaplan and Violante (2014). These models provide an explanation for why high wealth households with low liquidity often have high MPCs. They do not explain high MPCs for high liquidity households.

When considering the ability of the model to jointly fit the various pieces of causal evidence, it is also important to note that we picked the discount factor to match prior estimates of spending responses to tax rebates, as in Kaplan and Violante (2014). However, Table A-3 shows that changing the discount factor, and thus average liquidity in the economy, affects the average size of MPCs but does not change these relative comparisons across policy experiments. Calibrating the economy to a lower degree of liquidity can amplify spending responses to the $300 supplement, but the model then implies spending responses to the onset of the $600 supplements which are too large relative to data.

Finally, we note that the model can also be used to explore the relationship between causal estimates that we observe in the data and other counterfactuals of interest that we do not directly observe. For example, our waiting research design at benefit onset identifies the causal response to the start of regular UI + $600 but it does not identify the causal response to the start of the $600 benefit alone. In the model, we can separately compute the response of spending to regular UI and to the $600. We concentrate on the model in which expiration is a surprise, since this model better fits our empirical evidence. In that model, the MPC out of regular UI + $600 is 0.44 while the MPC out of the $600 supplement alone is 0.37.[38] This means that the MPC out of the $600 benefit is 85 percent as large as the MPC out of the $600 benefit + regular UI. Since the MPC at onset in the model and data are essentially identical, applying this same ratio to our empirical estimate of 0.43 also implies an MPC out of the $600 at onset to 0.37.

We can also use the model to compute the relationship between 1-month responses to the $600 and responses at longer horizons. At 1-,3-, and 6-month horizons the model respectively implies an MPC out of the $600 supplements of 0.37, 0.46, and 0.69. These patterns are consistent with the empirical evidence in Section 4.2.4 that spending responses at longer horizons are substantially greater than the already sizable responses we find at one-month horizons.

4.3.4 Discussion of Magnitudes and Aggregate Implications

Are the spending responses to benefit changes that we estimate large or small? Our first way of answering this question was explored in the previous section: we compared the causal effects that we measure to those predicted by a relatively standard structural model and found empirical responses which were bigger than empirical predictions. This theoretical benchmarking is the first sense in which the spending responses that we find are large.

However, it is also useful to compare the effects we estimate to those in the large literature estimating spending responses to one-time tax rebates. These estimates are a particularly useful point of comparison for several reasons: they are widely studied and credible, they are common targets in the literature, and perhaps most importantly, they are a type of countercyclical stimulus policy which is frequently used in practice.

As discussed in our modeling results, the various MPCs that we estimate each map to slightly different policy counterfactuals. The onset MPC from the waiting for benefits research design is in many ways most directly comparable to the MPCs out of tax rebates, since the identification of tax rebate effects focuses on "first" payments and similarly compares households who get these payments early to households who get payments late.[39] For this reason, we focus on a comparison of our onset

[38] In the model in which expiration is anticipated, the MPC out of the $600 alone is 0.27.

[39] Since tax rebates are one-time payments, they are by definition "first" payments rather than coming after several previous payments as in all of our other MPC exercises.

MPC estimate of 0.43 to tax rebate estimates in the literature.[40]

Kaplan and Violante (2014) summarize the findings from this literature and argue for a target 3-month non-durable MPC of 25 cents. Using Nielsen spending data, Broda and Parker (2014) find that the 1-month MPC out of rebates is 30-50 percent less than the 3-month response. Applying this same ratio to the 0.25 MPC suggests a 1-month MPC of non-durables to tax rebates of 0.125 to 0.175, which is substantially below the one month MPC of 0.43 that we estimate to the start of unemployment benefits.

However, it is important to note that non-durable spending is a subset of the broader spending we measure.[41] While the prior literature also measures MPCs out of broader spending, the statistical precision of those estimates is low and thus makes it hard to make comparisons with our estimates. Overall our analysis suggests that unemployment benefits were spent at a higher rate than past tax rebates, but in future work we hope to make these comparisons more precise using more directly comparable measures of spending.

What are the aggregate spending implications of the expanded unemployment benefits? A full general equilibrium accounting of the effects on aggregate spending is beyond the scope of this paper. However, it is useful to provide a simple back-of-the-envelope partial equilibrium calculation to get a sense of magnitudes. In order to do so, we multiply our estimated MPC to the \$600 benefits times the aggregate total of \$600 paid out from May to July and divide this by aggregate PCE spending over the same period of time.

If we use the MPC of 0.29 estimated from the expiration experiment, then this exercise implies that the \$600 supplements increased aggregate spending from May-July by 2.0%.[42] This calculation is likely a lower bound on partial equilibrium spending effects, since it uses the MPC from the end of the \$600 period when households have substantially more liquidity and this MPC measures only the spending effects over the first month. Alternatively, we can use the 3-month MPC from the comparison of unemployed in 2019 and 2020 of 0.38, which implies an increase in aggregate spending of 2.6 percent from the \$600 supplements. Thus, our estimates suggest that unemployment benefit expansions during the pandemic substantially boosted aggregate spending. However, we note that this calculation abstracts from a variety of equilibrium considerations.[43]

5 Job Finding Response to Expanded Unemployment Benefits

The canonical approach to evaluating changes in unemployment insurance generosity trades off the consumption-smoothing benefits of increasing spending against the disincentive effects of more generous benefits. Having analyzed spending effects, we now turn to the effect of expanded benefits on job search. To do so, we examine the dynamics of exits and recalls and interpret them through the lens of a job

[40] In addition to comparing responses to benefit changes to transitory rebates, it is also interesting to ask whether the response to benefit changes during the pandemic differs from the response during normal times. While Ganong and Noel (2019) do not report an MPC which directly compares to our spending measures, they do report an onset MPC out of total account outflows of 0.83. Performing this same exercise, we find a value of 0.69, suggesting that the pandemic did have some effect on depressing responsiveness.

[41] In ongoing work we hope to use merchant classification codes to disaggregate our spending measures by type, which would allow for more precise comparisons.

[42] PCE from May-July is \$3.45 trillion and \$230 billion of supplemental payments are made. $0.29 \times 230/3450 = .02$.

[43] Aggregate spending effects could be higher than these estimates if spending multipliers are large, or they could be smaller if spending multipliers are depressed due to pandemic related supply constraints. We do not attempt to quantify the size of multipliers during the pandemic, but after estimating employment effects from discouraged job search in the next section, we discuss the size of multipliers which would be required to balance these effects.

search model. We demonstrate that although a simple model calibrated to match the pre-pandemic labor market cannot match the empirical patterns from 2020, a model augmented with recalls and high job search costs can. We then use this model to construct a counterfactual estimate of the job-finding rate without the $600 supplement. We estimate that the disincentive effect of the supplement is an order of magnitude smaller than prior microeconomic estimates from normal (non-pandemic, non-recession) economic conditions.

5.1 The Exit Rate from Unemployment and the Recall Share: Empirical Estimates

In Section 3, we described broad patterns in terms of flows into and exits out of unemployment. In this section, we provide further analysis of the exit rate from unemployment, which is the ratio of the number of recipients exiting to the total number of benefit recipients. This exit rate is a common empirical target for models of job search.

One metric of the strength of the labor market is the speed at which benefit recipients find work. Before the pandemic, the labor market was very strong by this metric as recipients found work quickly (as compared to historical norms). Appendix Figure A-10 shows that the 2019 average weekly exit rate is a bit over 10 percent. The figure also shows that in the first nine weeks of 2020, exit rates are very similar to their 2019 counterparts. This suggests that, in the absence of the pandemic, exit rates would have continued to follow 2019 patterns.

The pandemic instead made it much harder to find work: exit rates plummeted in the week of March 15, which is shortly after a national emergency was declared. Figure 10a shows the weekly exit rate. We note that the decline in exit rates occurred two weeks *before* the $600 supplement was implemented, and that exit rates have remained low throughout 2020. These patterns continue to hold under a number of alternative specifications shown in Appendix Figures A-11a and A-11b.

A distinguishing feature of the pandemic labor market is the high rate of UI recipients returning to their prior employer ("recalls"). Figure 10b shows the evolution of the recall share in 2020.[44] In January and February 2020 we estimate that about 21 percent of unemployment exits reflect recall to a prior job.[45] At the same time that the exit rate peaks in late May, the recall share peaks around 75 percent. By the fall, however, the recall share has fallen below 50 percent. These are, to our knowledge, the first estimates of the employee recall share in 2020.[46]

The dynamics of the exit rate in the summer of 2020 are driven by the dynamics of recalls. Figure 10c shows the exit rate separately for benefit recipients who are recalled and those who are not recalled. The figure reveals starkly different patterns by labor market outcome after exit. The exit rate to recall rises from its pre-pandemic mean by more than a factor of two in May and June 2020. Exits without

[44] As discussed in the data section, we only are able to observe recalls for the subset of workers for whom we observe a job separation. Appendix Figure A-11c shows that exit rates for workers for whom we observe a job separation tend to be lower, both pre-pandemic and during the pandemic. However, the dynamics of UI exit are quite similar for the two groups.

[45] This estimate is in the range of other recent estimates for the recall share in the U.S.; it is above an estimate using the Quarterly Workforce Indicators data and below an estimate from the Survey of Income and Program Participation (Fujita and Moscarini, 2017).

[46] The employee recall share is challenging to estimate because there is no other contemporaneously-available dataset which captures employment by firm and also nonemployment. Forsythe et al. (2020) infer employee recalls using workers in the Current Population Survey who return to employment in the same industry, but this is only feasible for nonemployment spells of one to two months. There are estimates of the fraction of workers that each firm has recalled for firms that use ADP as a payroll processor (Cajner et al., 2020) or use Homebase as scheduling software (Kurmann, Lale, and Ta, 2020).

recall show the opposite pattern. As we discuss in Section 2.1, very few UI recipients are exhausting benefits during this time period in the nine states in the job search sample. We therefore interpret exits without recall as workers beginning a new job at a different employer. The rate of new job starts falls by a factor of three from its pre-pandemic level and remains depressed through the summer. Because the exit rate without recall is constant from April through the end of July, all of the changes in the exit rate during this period reflect changes in exits with recall.

Our finding of a high recall share during the summer has two implications. First, it allays a concern that was prominent early in the pandemic that workers would not return to their prior jobs when businesses reopened. Although benefit recipients are required to accept "suitable work"—including returning to their prior job if recalled—this provision may have been difficult to enforce when the unemployment insurance system was overwhelmed with claims. The fact that so many workers exit benefit receipt to return to work at their prior employer suggests that neither the high benefit level nor any potential non-enforcement of suitable work requirements deterred many recipients from returning to work. Second, our findings thus imply that actual recalls have been qualitatively consistent with aggregate patterns in employees' recall expectations. In particular, they are consistent with evidence from the Current Population Survey (CPS) that many workers who initially became unemployed expected to be recalled, as reflected in their survey responses about their reason for unemployment (Gallant et al., 2020; Chodorow-Reich and Coglianese, 2020; Kudlyak and Wolcott, 2020; Bick and Blandin, 2020).

We next examine whether the removal of benefit supplements might have affected the UI exit rate. We structure our discussion by analyzing exits chronologically during four different time periods.

First, many workers returned to work before the $600 supplement expires. For the cohort separating in April, 59 percent return to work before the supplement expires and 39 percent return to work before the beginning of June. When we define the denominator as every worker whose unemployment spell begins between March 22 and July 26, 53 percent return to work before the supplement expires. Intuitively, an early return to work is inconsistent with a disincentive effect of the supplement; we formalize this idea in the next section.

Second, an additional 3.3 percent of benefit recipients exited the program in the weeks coinciding with the expiration of the $600 supplement. Figure 10a shows that weekly exit rates rise from 5.5 percent the week before expiration to 8.0 percent, 8.5 percent, and 6.9 percent, respectively, in the three weeks immediately following expiration of the $600 supplement.[47] Altogether, if the exit rate had remained constant at a rate of 5.5 percent per week, then there would have been (8.0 + 8.5 + 6.9 - 5.5*3 =) 7.0 percent more benefit recipients as of the week of August 17. Because so many UI recipients had already returned to work, this corresponds to (7.0 * (1 - 0.53) =) 3.3 percent of all of the workers who began unemployment between March 22 and July 26.

Although it appears the spike in exits was caused by the expiration of the supplement, its size and duration implies only a small aggregate disincentive effect of the supplement. Three pieces of evidence suggest that the spike was caused by the benefit expiration. First, Appendix Figure A-12 shows that exits are concentrated among workers who have low weekly benefits. These workers have the highest benefit replacement rates and therefore the greatest disincentive from the supplement because the

[47] This empirical pattern is consistent with much of the recent literature on the dynamics of exit from UI around benefit cliffs. For example, DellaVigna et al. (2017) show using data in Hungary that UI exits spike when benefit amounts predictably fall by a factor of two. Schmieder, von Wachter, and Bender (2012) show using data in Germany that job-finding spikes in the month where regular UI benefits exhaust and only a smaller means-tested benefit remains available. The pattern also echoes the spike in job-finding at benefit exhaustion in the U.S. documented in Katz (1986) and Ganong and Noel (2019).

supplement is a fixed \$600 irrespective of the regular weekly benefit amount. Second, a spike at this time of year is not typical; Appendix Figure A-10 shows that exit rates are smooth in the end of July in the prior year. Third, the spike appears in all states in our primary sample despite these states experiencing a wide range of economic conditions. Thus, we conclude that a small share of workers and firms have flexibility over when to begin (re)employment and waited until the supplement expired to (re)start an employment relationship. However, although the spike in exits from these workers is visually apparent in Figure 10a, a key lesson from our quantitative job search model in Section 5.2 is that because the spike is concentrated in a small subset of the total unemployed population, and because it is short-lasting, it actually implies a *small* aggregate disincentive effect of the \$600 supplement.

Third, Figure 10a also shows no change in exit rate around the expiration of the \$300 supplement in early September. As we discuss in Section 2.1, the administration of the \$300 supplement was haphazard. In the nine states in our primary job search sample, the supplement had not even begun to be paid as of early September and it is quite possible that UI recipients were not aware that the supplement would be paid at all.

Fourth, we find that exit rates remain depressed even after the \$600 supplement expires. A low UI exit rate *during* the summer is consistent with either a disincentive effect of the \$600 and/or a depressed labor market. However, the fact that exit rates *remain* similarly low after the supplement expired means that either the disincentive effect is modest, or that the disincentive effect is large and something else shifted in the opposite direction exactly coincident with the supplement's expiration that made it harder to find jobs in August than in July. Although we are unable empirically to distinguish between these two hypotheses, we are unaware of any macroeconomic change that would have made it more difficult to find jobs in August than it was in July. In fact, models with a vacancy creation margin such as Hagedorn et al. (2013) would predict that employers would post more vacancies after the supplement expires, which would make it *easier* to find a job in August. Thus, our findings are qualitatively inconsistent with a large disincentive effect. To quantitatively estimate the impact of the supplement on job-finding (beyond the spike alone) requires a structural model. We therefore develop such a model in the next subsection.

5.2 Model of the Exit Rate from Unemployment During the Pandemic

To interpret the data on job-finding, we turn to a structural model of job search. We use the model to perform two types of exercises. In the first exercise, we use a standard baseline model to ask "what effect of the \$600 supplement would be predicted using only data and studies from before the pandemic?" In the second exercise, we enrich the model to include key features of the pandemic-era labor market and ask "what can we conclude about disincentive effects of the \$600 after estimating preference parameters to match job search behavior during the pandemic?"

We model unemployment as a series of decisions of how much to search for an indefinitely lasting job at a known wage as in Lentz and Tranæs (2005). In each period t, the worker picks a probability s_t of entering employment in the following period. Search effort is costly, and we take $c(s_t) = k\frac{s_t^{(1+\gamma)}}{1+\gamma}$ as in Paserman (2008). The agent must consume their entire income in each period, and receives utility of consumption $ln(c_t)$. Income is composed of a time varying unemployment insurance benefits component, b_t, and additional income of h. Since the value of h is taken from the data, this is best thought of as income from other sources such as spousal labor income. The agent discounts the future at rate β.

The value of employment is therefore $V = \frac{ln(w)}{1-\beta}$ while for the value of unemployment the agent solves:

$$W_t = \max_{s_t} ln(b_t + h) - c(s_t) + \beta\left(s_t V + (1 - s_t)W_{t+1}\right)$$

5.2.1 Model Calibration

The model is calibrated to the same economic environment from Section 4.3.1 as much as possible. Although the time period of the model is weekly, we use the same income process and annualized discount factor as in Section 4.3.1. For simplicity, in the baseline model we assume an infinite duration for standard unemployment benefits (following Mitman and Rabinovich, 2020). As in Section 4.3.1 we assume two polar scenarios regarding expectations about the duration of the $600 weekly supplement. In the "expect expiration" case the supplement is expected to expire at the end of July, as legislated. In the "surprise expiration" case households expect to be eligible for 52 weeks of the supplement and then are surprised when it is not renewed at the end of July. We note that the second scenario, in which the expiration of the supplement is a surprise, is more consistent with our findings from the model and empirical estimates of spending behavior.

Unlike the spending model, where job search was exogenous, in this model job search is endogenous and we need therefore need to calibrate the cost of job search. We choose the parameters of the search cost function to match the weekly UI exit rate before the pandemic and existing estimates of the disincentive effect of UI benefits. To match the disincentive effects in microeconomic studies, we use the median estimate from a recent meta-analysis by Schmieder and von Wachter (2016). They analyze 18 studies and find that for each dollar increase in the level of benefits, the median increase in total government expenditure is $1.35 when assuming that UI is financed by a 3 percent payroll tax rate. This model parameterization implies an elasticity of duration with respect to benefit levels of 0.33, which is consistent with Landais (2015).

5.2.2 Baseline Model Results

Figure 11a shows that the baseline model fails to predict the dynamics of job search. In particular, it predicts much larger fluctuations in search than we see in the data. Figure 11a shows that when expiration is expected, search rises gradually in advance of expiration. This echoes the classic pattern of rising search effort prior to expiration in the canonical Mortensen (1977) job search model. Immediately before expiration, job finding has risen nearly all the way to the no-supplement level of job-finding, as in the model by Petrosky-Nadeau (2020).

Figure 11a also shows that when expiration is a surprise, the model predicts no search, when the supplement is available, and then shows a sharp jump up when recipients are surprised by the benefit's expiration. This pattern arises because when replacement rates are very high due to the supplement, it is optimal for households to wait until closer to expiration to begin their search. In both scenarios, the model generates predictions that are inconsistent with the data, since in the data the job-finding rate falls modestly from May through the end of July, spikes up briefly when the supplement expires but then remains low for the rest of August and September. In this sense, we conclude that job search responds less in the data to changes in the level of benefits than predicted by the baseline model.

As a robustness check, we reproduce the same analysis using the Current Population Survey to measure job finding in Appendix Figure A-13. These data are less well suited because outcomes are only observed monthly and because we do not know which unemployed workers are receiving benefits.

We nevertheless find a similar pattern of divergence between model and data using this alternative measure of job-finding.

5.2.3 Enriched Model Calibration and Results

Having shown that the baseline model makes predictions which look very different from the data, we now turn to estimating parameters which do match the data during the pandemic. Incorporating data on recalls and allowing the cost of job search to be different during the pandemic results in a job search model whose patterns are consistent with the data.

We begin with a minor change to the baseline model that improves realism but is not quantitatively important. Instead of having an infinite duration of *standard* benefits as in Mitman and Rabinovich (2020), we assume that the maximum duration of standard benefits is 52 weeks, which matches the median duration of benefits in 2020. Standard benefits reflect 26 weeks of regular Unemployment Compensation (a state program), 13 weeks of Pandemic Emergency Unemployment Compensation (a federal program), and 13 weeks of Extended Benefits (a federal-state partnership). States vary in how many weeks they offer of both regular unemployment compensation and Extended Benefits; our choices of 26 weeks and 13 weeks reflect the median duration of benefits available to a claimant as of December 1, 2020. Although limiting standard benefits to 52 weeks in this way is more consistent with the policy environment, it has little quantitative impact on the model.

We then add three simple but important modifications. First, we modify the job search model to allow for the possibility of recall. Our model of recalls follows the conventions in Katz (1986). After adding a time-varying exogenous probability of recall, p_t, the value of unemployment is now:

$$W_t = \max_{s_t} ln(b_t + h) - c(s_t) + \beta\left((p_t + s_t)V + (1 - (p_t + s_t))W_{t+1}\right)$$

As in Katz (1986), we make the strong assumption that the probability of recall is exogenous. The agent knows the probability of recall in each period, but does not know in which period she will be recalled. We further assume that the agent always accepts the recall offer. This assumption can be motivated by both institutional rules and economics. The institutional motivation comes from the requirement that unemployment insurance recipients accept any offer of "suitable work". The economic motivation comes from Boar and Mongey (2020), who demonstrate that a jobseeker is likely to accept a recall at their prior wage over the likely wage loss that would arise from a taking a different job.

Second, we allow search to become more costly during the pandemic. We assume search cost parameters in the pre-pandemic period are the same as in the baseline model, but then allow them to change discretely once the pandemic hits. This modeling choice echoes the decline in search efficiency in the model by Mitman and Rabinovich (2020). We estimate the pandemic search cost parameters by minimizing the mean squared error of the distance between the job-finding rate in the model and job-finding rate for new jobs during the pandemic. Figure 10c shows that the new job-finding rate is flat through the summer and only rises slightly after the supplement expires. To match this empirical pattern, we estimate that job search costs are much higher (both in level k and in terms of convexity γ) than they were before the pandemic. Had search costs been low, then we would have seen a large increase in search either in the period leading up to expiration (in the predictable scenario) or at the time of expiration (in the surprise scenario).

When search costs are high, as we estimate to be the case, the importance of expectations about when the supplement will expire is diminished. Intuitively, when it is costly to adjust search effort, the incentives generated by enhanced benefits are muted, so expectations about their duration also have a muted effect. We defer a discussion of reasons why search costs might be higher to Section **??**nally, we add a type with low search costs to match the spike in job-finding at expiration. The spike in job-finding at expiration is quite salient based on visual inspection of Figure 10a. However, it only takes a very small share of recipients with low search costs to match the spike. This is because many UI recipients have already found jobs before expiration of the supplement. We estimate that 0.8-1.7 percent of the sample has low search costs that lead them to delay searching until after the supplement expires. This estimate is even lower than the already-low estimate that 3.3 percent of recipients waited until expiration to find jobs from Section 5.1; the model-based estimate is lower because it captures only the workers who wait to start a new job and ignores the additional workers who are recalled at the time of expiration. Our estimate of the share of such low search cost types is similar to the 1 percent share estimated by DellaVigna et al. (2020) using German data.

All three pieces are necessary to match the data series for job search, as we demonstrate in Appendix Figure A-14a. A model without recalls would miss the surge in job-finding in the May and June and the subsequent decline in job-finding. A model without estimated search costs would predict that search is either rising prior to expiration or jumps up sharply at expiration, while neither of these patterns is present in the data. A model without a type with low search costs would fail to match the size of the temporary spike in job-finding when the supplement expires.

Figure 11b shows that these steps jointly generate a model series that is very similar to the data, regardless of whether the supplement's expiration is expected or a surprise. We define the job-finding rate in the model as the sum of new job search (predicted by the model) and recalls (observed in the data). Thus, if we make the strong assumption that recalls are exogenous, a model with high search costs for most, and low search costs for a very small share of agents, is able to match the data during the pandemic.

5.3 The Enriched Model Has Testable Predictions Which Are Consistent with the Data

The enriched model implies high search costs for most recipients and very low search costs for a small share of agents. Such a model has three additional testable predictions: (1) little difference in job-finding rates even for those with very different replacement rates, (2) low total exit rates for those with little chance of recall, and (3) no spike in exits at supplement expiration for those who searched for jobs before the supplement was available.

We find support for (1) by showing that job search appears insensitive to cross-sectional variation in benefit levels. We estimate that the replacement rate is about 100 percent for workers with weekly benefit amounts above the median and about 185 percent for workers with weekly benefit amounts below the median. Because the policy paid a flat $600 supplement to all unemployed workers, the replacement rate (the ratio of benefits to earnings) was higher for workers with lower pre-separation earnings. The amount of regular UI benefits is tied to a worker's pre-separation earnings, so workers with low benefits in terms of dollars therefore had high replacement rates. Using this fact, we are able to estimate replacement rates in states which pay benefits weekly (New York, New Jersey, Ohio, Indiana, Georgia, and Washington). Figure 12 shows that we find only mildly lower exit rates among workers with high replacement rates than among workers with low replacement rates despite large differences in replacement rates. The fact that the exit rates are so similar is consistent with our enriched model's estimates of a very small disincentive effect.

The group of benefit recipients who became unemployed before the pandemic provides a means to test predictions (2) and (3). Appendix Figure A-15 disaggregates the exit hazard to report results separately for recipients with different start dates. Because the pandemic caused such a large negative shock to labor demand, introspection suggests that benefit recipients who became unemployed before the pandemic are very unlikely to be recalled. The figure shows that this group has a low total exit rate. This pattern is consistent with our finding in the enriched model that opportunities for finding new jobs were very limited during the time period when the supplement was in effect. In addition, the group unemployed before the pandemic has almost no spike in exits at the expiration of the supplement. The model predicts that any benefit recipient with low search costs before the supplement is in effect will have found a job already. Thus, the absence of the spike for this group is consistent with another prediction of the enriched job search model.

5.4 Counterfactual: What Would the Job-Finding Rate Have Been Without the Supplement?

We have shown that the model is consistent with several aspects of the data: (a) the time-series pattern of job search, (b) differences in replacement rates and (c) differences in search by start date of unemployment. We now use the model to study what job search would have been without the $600 supplement.

The ideal research design to estimate the partial equilibrium effect of the $600 supplement would be one where only some recipients were eligible for the supplement. However, because the law made every benefit recipient eligible for the same $600 per week supplement, there is no group of unemployed workers without the supplement who can serve as a control group. Although we used delays in the payment of benefits as an identification strategy for spending, they are not suitable here because the parameter of interest is job search at the time when recipients are *eligible* to receive the supplement. We thus do not have a treatment-control research design for estimating the total job search distortion associated with the supplement. We instead study this question from the perspective of our enriched model which is consistent with the empirical patterns in the data. We also note that our model exercise speaks only to the job-finding disincentive margin and not to the possibility that a higher supplement encourages employers to layoff workers as in Topel (1983).

In the enriched model, the disincentive effect of the $600 supplement is minimal. We visualize this effect in two ways. First, Figure 13 shows the path of predicted job search with and without the supplement. Regardless of expectations, the enriched model shows only a very slight change in the job-finding rate. Second, Appendix Figure A-16 plots survival curves under each scenario. While the baseline model predicts that the supplement dramatically slows unemployment exit, the supplement has almost no impact in the enriched model. The change is small because we estimate that search costs are high and the marginal cost of additional search is very high.

To compare our estimates to the prior literature, we calculate the same summary statistics in the model which quasi-experimental studies have used to describe their results. Table 4 shows that we estimate a duration elasticity of 0.01-0.02 in the enriched model. This is far below typical estimates of duration elasticities, which range from 0.1 to 2.0 depending on the setting (see Meyer and Mok 2007 for one low estimate and Card et al. 2015 for one high estimate). Likewise, the behavioral cost of 0.02

is smaller than every prior estimate from 18 studies reviewed in a meta-analysis by Schmieder and von Wachter (2016).

We then use the model to calculate impact of reduced job search from the $600 supplement on aggregate employment. This calculation requires three inputs. First, we calculate the number of workers starting a regular UI or PUA spell in each month from DOL ETA data. Second, we estimate UI durations for each cohort in each version of the model with and without the supplement. We make the conservative assumption that every worker who exits UI exits to employment; if any workers instead exited to nonemployment then our estimate would overstate the employment disincentive of the supplement. This generates an estimate of the reduction in person-weeks of employment caused by the supplement at each point in time. Finally, we divide by actual employment from the CPS.

Our estimate of a low disincentive effect suggests that the aggregate employment impact of the $600 supplement was over an order of magnitude smaller than would have been expected based on pre-pandemic behavior. Our estimates are reported in the final column of Table 4. Our baseline model is calibrated such that the disincentive effect of increased benefit levels is consistent with prior microeconomic evidence (both in terms of behavioral cost and in terms of elasticity). We find that a model calibrated to these typical disincentive effects would have expected aggregate employment to be 4.2-6.1 percent lower from April to July 2020 as a result of the benefit supplement. In contrast, our enriched model that matches job search patterns during the pandemic suggests that employment was only 0.2-0.4 percent lower as a result of the supplement.

If we stretch the model to be even more conservative we still find small disincentive effects. One suggestive pattern in the data is that the non-recall exit rate is slightly lower in June and July than it was in April and May, and then is persistently slightly higher after the supplement expires (Figure 10c). If we ignore the April and May data in our calibration of the enriched model, we estimate a slightly higher disincentive effect because the average level of search after expiration is consistently above the average level of search before expiration. However, even in this calibration the elasticity is no higher than 0.06 and the aggregate employment impact is no higher than 0.5 percent. Moreover, our estimate of the employment distortion is likely to be an upper bound because our estimate of the total number of employed in the denominator uses the CPS definition, which likely omits some people who receive PUA.

5.5 Connections to Prior Literature

Our findings connect to a large literature on the disincentive effects of unemployment insurance as well as a literature on the trade-off between unemployment insurance and short-term payroll support.

One of the most robust empirical findings in public economics is that higher UI benefits increase the duration of UI receipt. A recent literature review by Schmieder and von Wachter (2016) summarizes 18 studies using what they call the "behavioral cost" of the policy. This is the additional cost of increasing benefits by $1 due to benefit recipients increasing their UI durations. The median behavioral cost estimate is $0.35. As discussed above, our paper's estimate of a behavioral cost of $0.02 is lower than every one of these prior estimates and over an order of magnitude lower that the median prior estimate.

Why is the disincentive effect so much smaller than estimates from the pre-pandemic microeconomic studies? There are four classes of explanations. First, the microeconomic studies show the effect of a benefit increase for a small number of workers, while in this case all unemployment insurance recipients saw an increase in benefits. In models with labor market congestion, the elasticity of job-finding when benefits increase for *all* workers (the "macro" effect) is likely to be smaller than when benefits increase for *some* workers (the "micro effect") (Lalive, Landais, and Zweimüller, 2015; Crépon et al., 2013). Second, prior research finds that the distortion is likely to be smallest in a recession (Landais, Michaillat, and Saez, 2018; Mercan, Schoefer, and Sedláček, 2020; Kroft and Notowidigdo, 2016). Although the labor market is usually slack in a recession, we note that the evidence on labor market tightness during the pandemic is mixed (Marinescu, Skandalis, and Zhao, 2020; Forsythe et al., 2020). Third, the pandemic may reduce job search above and beyond a normal recession, both because it is difficult to search for a job and because employers who are recruiting now may be doing so for positions with above-average health risk. Fourth, Chetty (2008) documents much smaller duration elasticities among benefit recipients who are not liquidity constrained. Because the $600 supplement was large enough to bring nearly every recipient off their liquidity constraint, the job-finding response may be more similar to the response previously estimated for recipients who are not liquidity constrained. We do not attempt to distinguish between these four hypotheses in this paper.

Our finding of a modest disincentive effect of the $600 supplement is in line with two types of prior studies which have compared job-finding for benefit recipients with different replacement rates. Because the *dollar* amount of the supplement is the same for all recipients, any differences in the *replacement rate* arise from differences in earnings or benefit levels before the pandemic.

The first group of studies examines labor market outcomes by cross-sectional differences between groups in replacement rates. Bartik et al. (2020) and Dube (2020) find no meaningful difference in employment comparing states with higher and lower replacement rates. Marinescu, Skandalis, and Zhao (2020) finds slightly fewer applications per vacancy among occupation-state cells with higher replacement rates. This is consistent with our finding in Figure 12 of a modestly lower job-finding rate for individual workers with higher replacement rates when the supplement is available.

The second group uses difference-in-difference methods to study how job-finding changes at the expiration of the $600 supplement for workers with different replacement rates. For example, Finamor and Scott (2021) compares individual workers with different levels of pre-separation earnings. It finds no differential change in job-finding around the time that the supplement expires, which the papers interprets as a finding that the supplement has no disincentive effect.

Our analysis contributes to this literature on the disincentive effect of the $600 supplement in several ways. Our two most important contributions are weekly estimates of the job-finding rate (including recalls) and analysis of this data through the lens of a quantitative model. The papers discussed above statistically measure the correlation between replacement rates and employment outcomes. Our quantitative model enables an economic interpretation of job-finding during the pandemic in terms comparable to the pre-pandemic literature on the disincentive effect of unemployment benefits.

In addition, while the studies discussed above examine the unemployed, our analysis looks specifically at UI recipients. This distinction may be important during a time period when the set of UI recipients diverged substantially from the set of people who were unemployed using the conventional survey definition. Further, because we see actual benefit payments, we are more reliably able to infer UI replacement rates, instead of inferring the UI benefit level from characteristics such as state, occupation, or earnings at a single employer in 2019.

A final contribution of our model is that it clarifies conditions under which a difference-in-difference research design is informative about the effect of the supplement. The design detects an effect of the supplement on job search if there is a sharp change in behavior between the end of July (when the supplement is available) and the beginning of August (when the supplement has lapsed). In the baseline model, the design is informative if the expiration is a surprise, but it is not informative if the expiration is expected. If the expiration is expected, then Figure 11a demonstrates that recipients will already have increased their search effort in the weeks prior to expiration. In the expected expiration scenario in the baseline model, the difference-in-differences design does not identify the causal impact of expiration. The difference-in-differences design would be informative under either information structure in the enriched model, but only because the enriched model already incorporates the effect that the design is trying to measure (i.e. it estimates a high cost of job search to match the dynamics before and after the benefit expiration).

Our findings also connect to a literature which considers the trade off between providing unemployment insurance to workers or short-term payroll support to liquidity-constrained small businesses (e.g. Birinci et al., 2020; Autor et al., 2020; Granja et al., 2020). Specifically, our results undermine one argument for expansion of short-term payroll supports like the Paycheck Protection Program (PPP). One argument for paying workers through their firms rather than through the unemployment insurance system is that it would make workers more likely to return to their prior employer, thereby preventing the destruction of valuable matches between workers and firms. Because many workers were laid off (in spite of PPP), the recall share is informative about whether the risk of worker-firm matches being destroyed was substantial among firms that did *not* receive the "treatment" of PPP. It thus provides an upper bound on the scope for programs such as PPP to improve retention.

Was PPP necessary to prevent the permanent destruction of matches? If the short-term recall share had been 100 percent, then the answer would be "no". On the other hand, if the recall share had been 20 percent, in line with its pre-pandemic level, the answer would be "yes". The finding that most workers who were laid off, paid through the unemployment insurance system, and then re-employed did in fact return to their previous employer implies that additional payroll support was, in many cases, not needed to prevent the destruction of matches.

Further, we emphasize the high short-term recall share is only informative about the value of small business support that is for payroll and lasts one quarter or less. First, covering non-payroll costs for liquidity-constrained businesses (e.g. rent, utilities, insurance) may improve business survival (Hanson et al., 2020; Guerrieri et al., 2020). Second, PPP was only designed to cover up to 2.5 months of small businesses' payroll costs; had an alternative policy lasted longer it is possible that it could have encouraged retention at a longer duration.

6 Conclusion

In this paper, we provide empirical evidence on some of the positive spending benefits and negative employment costs of expanded unemployment benefit levels during the pandemic.

We find that the spending benefits are large: between May and July, unemployed households had already spent roughly 30-40 percent of the $600 supplements. By the end of our data in October, nearly two-thirds had been spent. So many households were unemployed and receiving these expanded benefits that these spending effects matter for aggregates: we estimate that the partial equilibrium effect of the $600 supplements was an increase in aggregate spending of around 2.0-2.6 percent from May through July.

We find that the negative effects of the benefits on employment from discouraged job search were small. Matching the stable behavior of the job finding rate before and after their expiration leads us to conclude that the $600 supplements led to reductions in employment through discouraged job search of 0.2-0.4 percent.

It is important to note that these are both partial equilibrium effects. We do not attempt a full general equilibrium accounting of the effects of the benefit supplements for now. However, we note that estimates of how spending translates into jobs can provide some useful context for these numbers. Specifically, we ask: what employment increase arising from increased spending is required to offset the employment declines from discouraged job search so that the overall *net* effect of the benefit expansions on employment which is positive?

Conservatively using the low end of our spending estimate of 2.0 percent and the high end of our employment cost of 0.4 percent we estimate that the $600 supplements increased total employment as long as the cost per job is below $453,000.[48] This is very likely the case, since this threshold is substantially above prior estimates of costs per job which typically range from $25,000-$125,000 (Chodorow-Reich, 2019).[49]

Furthermore, it is important to note that the empirical estimates in Chodorow-Reich (2019) likely represent upper bounds on the cost of creating a job in 2009 since they are based on cross-region spending variation which provides a lower bound on the size of the national "no-monetary-policy-response" aggregate spending multiplier. However, the size of spending multipliers during the pandemic is very unclear. On the one hand, they may be unusually large because this is a particularly deep recession. On the other hand, they may be reduced as a result of pandemic-related constraints. Finally, we note that our employment costs focus on those arising from discouraged work effort but not from discouraged job creation by firms. While the size of this force in the Great Recession is debated (Chodorow-Reich, Coglianese, and Karabarbounis, 2019; Hagedorn et al., 2013), the fact that job finding exhibited little sustained increase after the expiration of the benefit suggests that it was not a dominant channel at that point in the pandemic. However, one should be cautious in extrapolating from the response to the $600 supplements to the consequences of potential future benefit increases. The $600 supplements were temporary support implemented at the height of pandemic-related labor market disruptions. Benefit increases in more normal times may have more negative macroeconomic consequences.

Overall, much more work is needed to understand the role of general equilibrium forces and effects of supply and demand constraints during this pandemic. However, the relative size of the partial equilibrium spending and employment effects that we estimate means that general equilibrium forces would need to be quite strong to reverse these patterns. The important potential role of unemployment insurance in stabilizing aggregate demand is consistent with arguments in Kekre (2017). Furthermore, even if demand effects might reduce the effectiveness of standard fiscal policy during a pandemic, Guerrieri et al. (2020) show that unusually large degrees of insurance may be optimal. Finally, it is important to note that the presence of the pandemic introduces additional considerations that are not present in a typical recession: in a pandemic, we may want people to stay at home even if it depresses economic activity in the short-run. Unemployment insurance plays an important role in ensuring that exposed workers are able to do so without suffering economic ruin.

[48] Using the high end of our spending estimates and low end of employment costs implies a net positive effect under the weaker condition that $1.2 million of spending leads to at least one job-year.

[49] Alternatively, our conservative estimate is that as long as the employment multiplier is at least 0.34, the $600 supplements increased employment on net. A 0.34 employment multiplier is well below estimates of 1.3-1.8 in Nakamura and Steinsson (2014).

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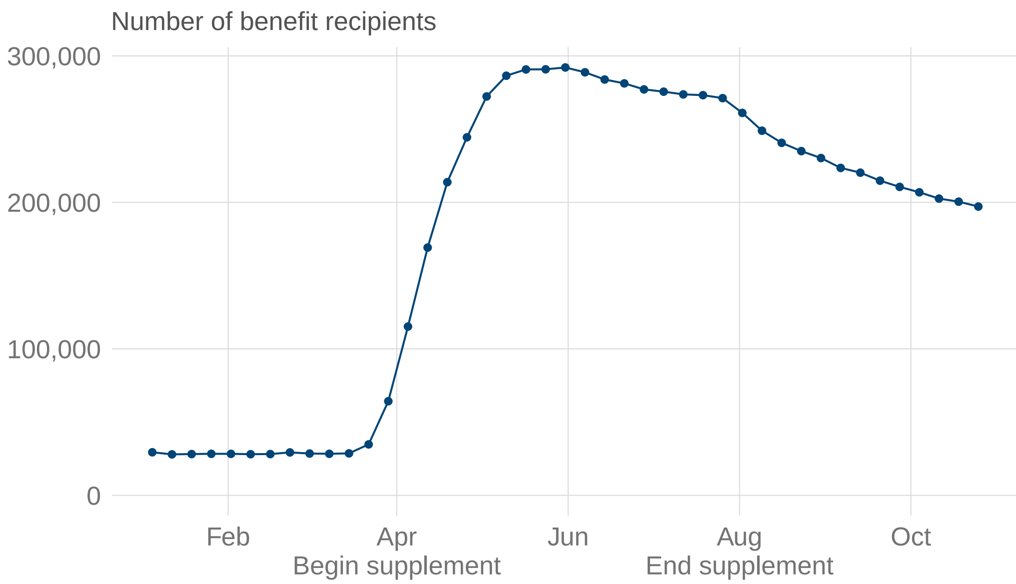
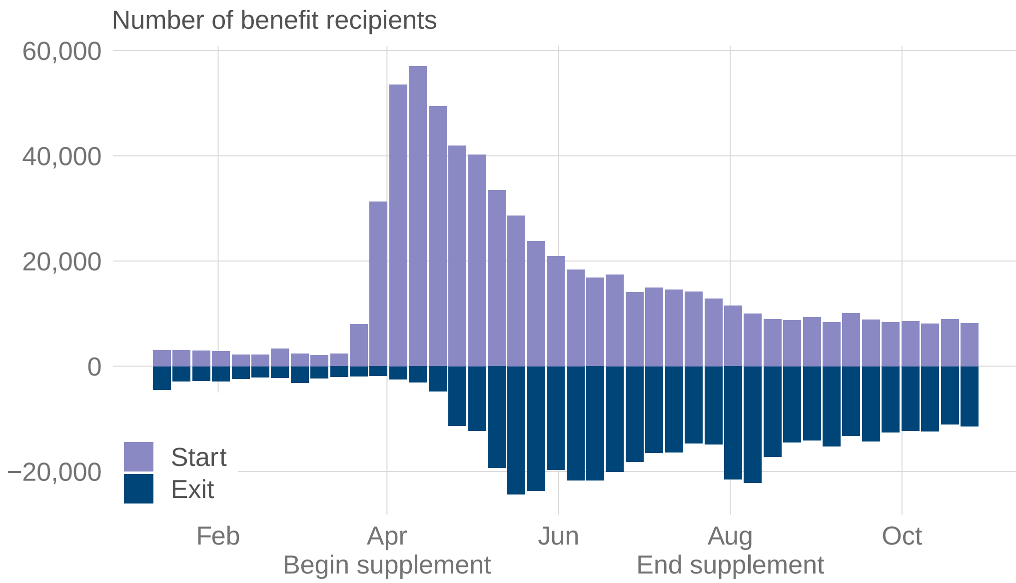
Figure 1: Patterns of Unemployment Insurance Receipt

(a) Total Recipients



(b) Starts and Exits

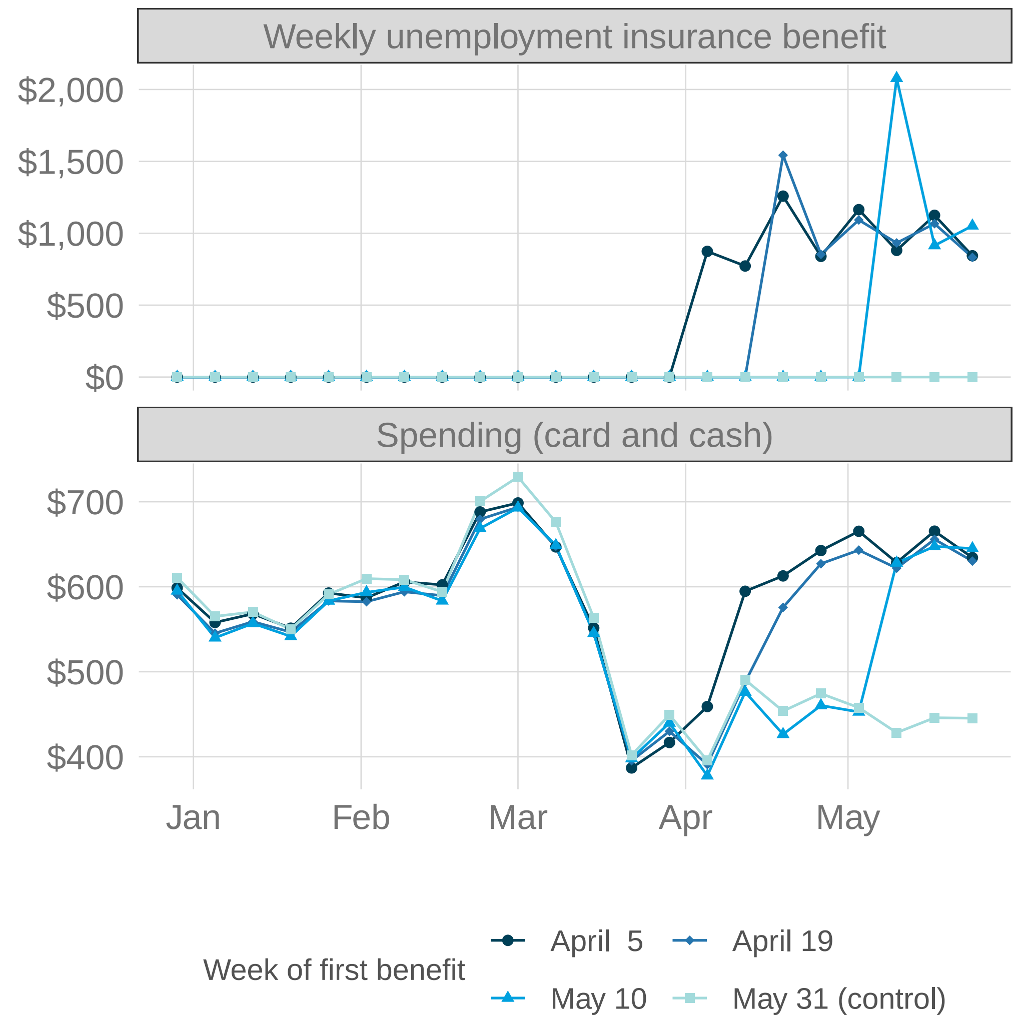


Notes: This figure shows the number of unemployment benefit recipients in the JPMCI analysis sample. Panel (a) reports the number of workers with an active unemployment spell. We define a benefit recipient to be in an active spell so long as they go no longer than three weeks without a benefit payment. Panel (b) shows the number of workers starting a spell and the number of workers ending a spell each week. The dates when the $600 weekly supplement begins and ends are labeled on the x-axis.

Figure 2: Spending of Unemployed Versus Employed

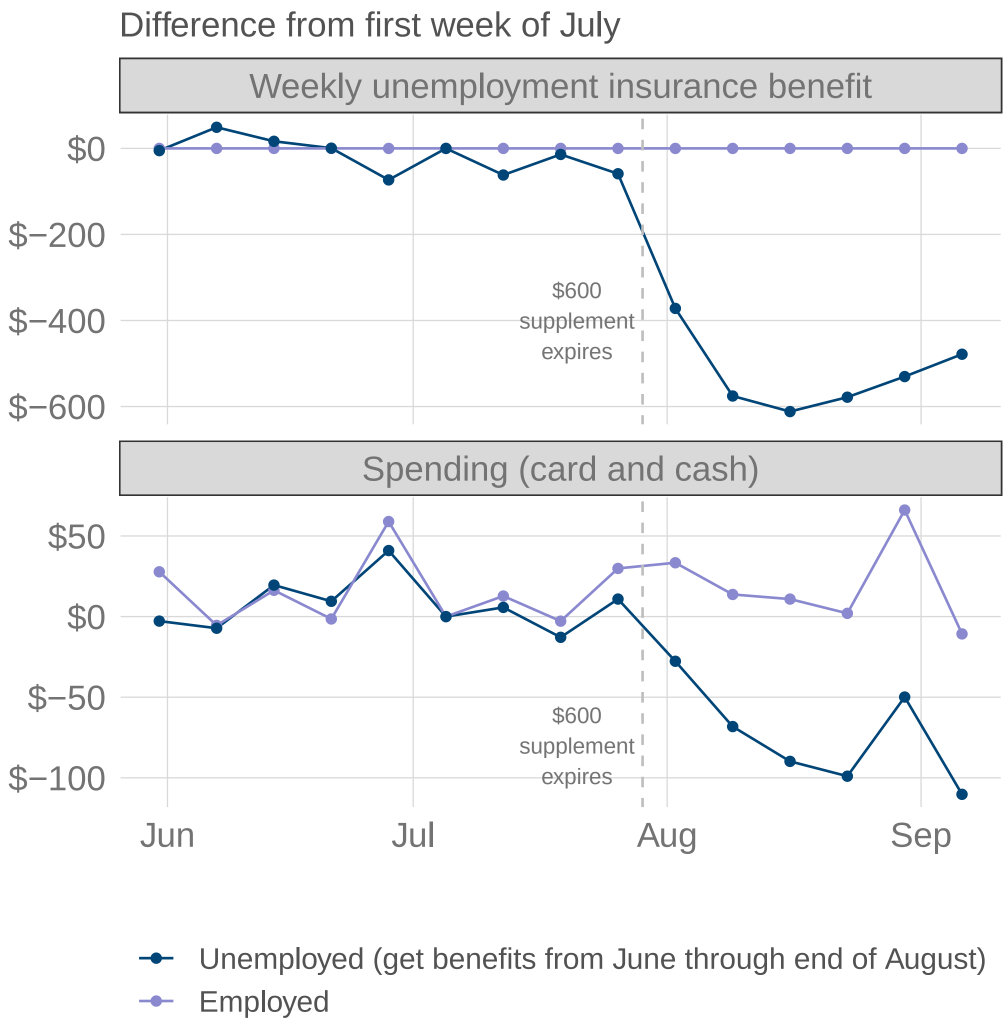
Notes: This figure compares the median income and spending of unemployed and employed households during the pandemic using JPMCI data. The blue line shows the income and spending of households where a worker begins unemployment benefit receipt at the start of April and continues receipt through at least the end of August. The purple line shows the same series for a set of employed households who are matched on 2019 income levels as well as date of receipt of Economic Impact Payment. See Section 4.1 for details.

Figure 3: Impact of Delays in Unemployment Benefits on Spending



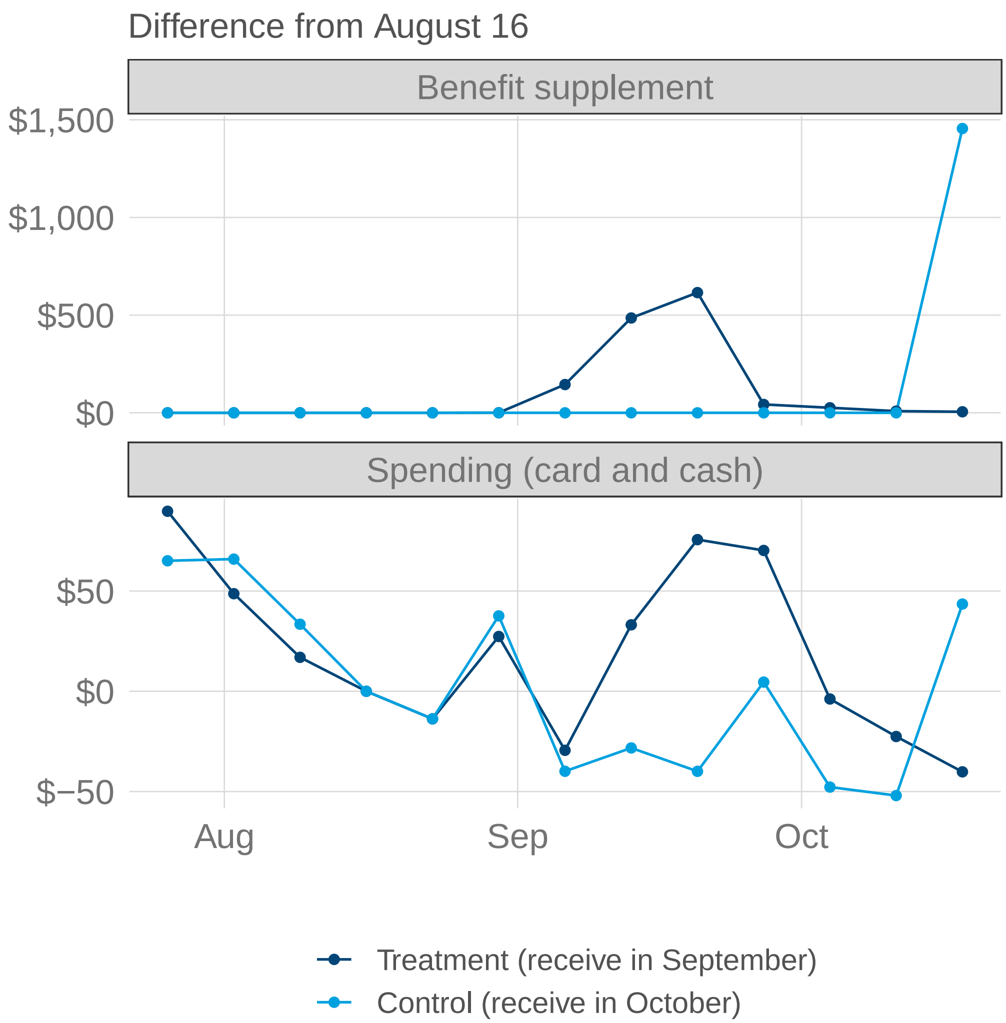
Notes: This figure measures the causal impact of a delay in unemployment benefit receipt on spending using JPMCI data. We select workers who separate from their jobs at the end of March and begin to receive unemployment benefits in different weeks. The dependent variables are mean benefits and mean spending. See Section 4.2.1 for details.

Figure 4: Impact of Expiration of the $600 Supplement on Spending



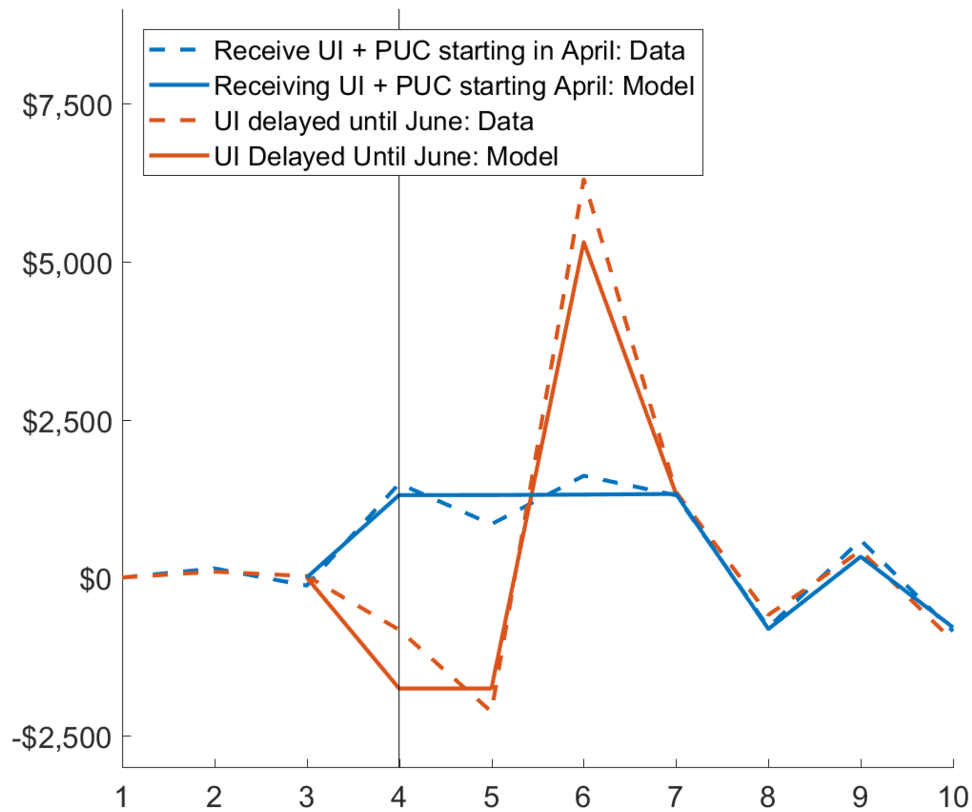
Notes: This figure measures the causal impact of the expiration on the $600 supplement on spending. The benefit amount declines over two weeks in August (rather than one week) because some states pay benefits once ever two weeks and therefore paid out one week's supplement during the first week of August. 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 date of receipt of Economic Impact Payment. The dependent variables are mean benefits and mean spending, measured as a change relative to the first week of July. See Section 4.2.2 for details.

Figure 5: Impact of Receipt of Temporary $300 Supplement on Spending



Notes: This figure measures the causal impact of the temporary $300 supplement on spending. The supplement was paid at different times in different states. The treatment group is eight states which paid the supplement in September. The control group is New Jersey, which paid the supplement at the end of October. The dependent variables are mean benefits and mean spending, measured as a change relative to the third week of August. See Section 4.2.3 for details.

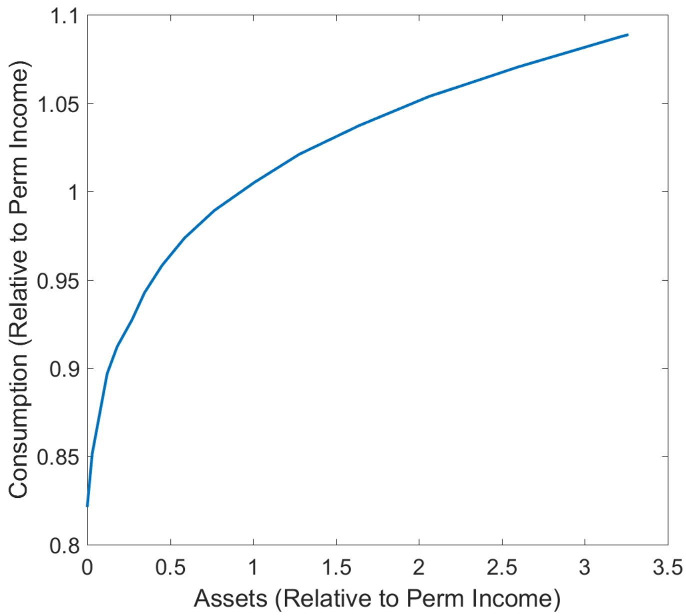
Figure 6: Monthly Income for Unemployed Households Receiving Benefits and Waiting for Benefits (Relative to Employed Households)



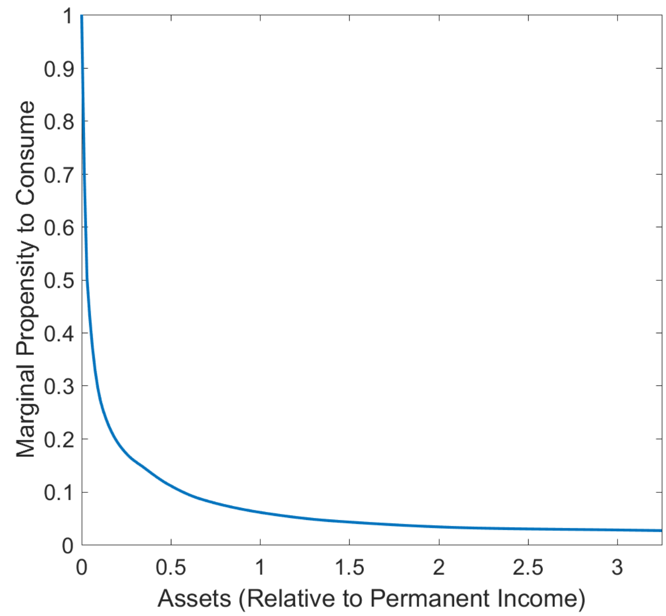
Notes: This figure shows the path of income in the data and model for households who are unemployed from April (Month 4) to October (Month 10) who first begin receiving benefits in either April or in June. To remove seasonality effects from the data, series are computed relative to the employed. Income rises for those receiving benefits in April due to $600 supplements. The spike in income in June for unemployed households who wait to receive UI is driven by the presence of benefit backpay. The jump in income in September is driven by the payment of $300 LWA supplements.

Figure 7: Consumption of the Unemployed

(a) Consumption Function



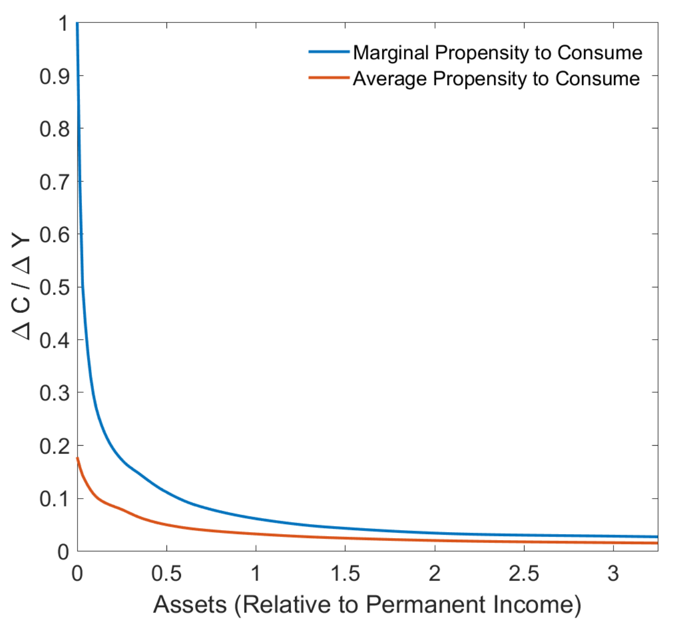
(b) Marginal Propensity to Consume



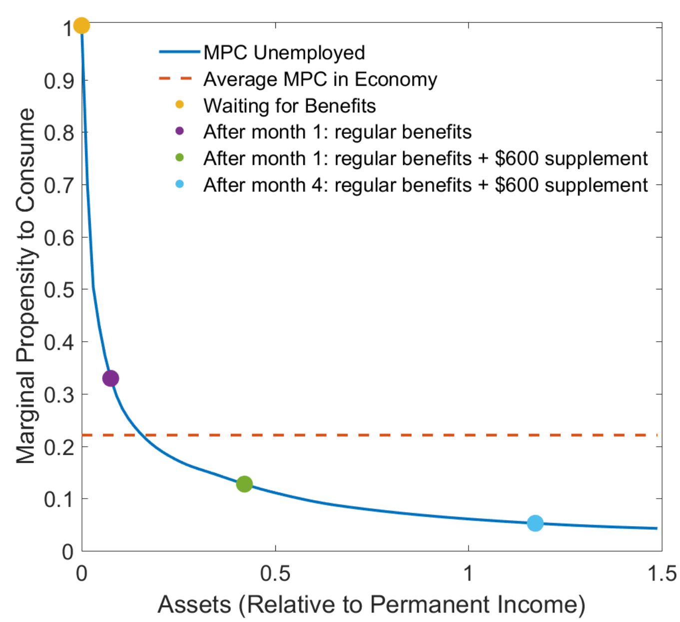
Notes: This figure shows the consumption function and resulting MPC for an unemployed household with the average wage w when employed.

Figure 8: Role of Benefit Size and Liquidity Dynamics

(a) MPC vs. APC

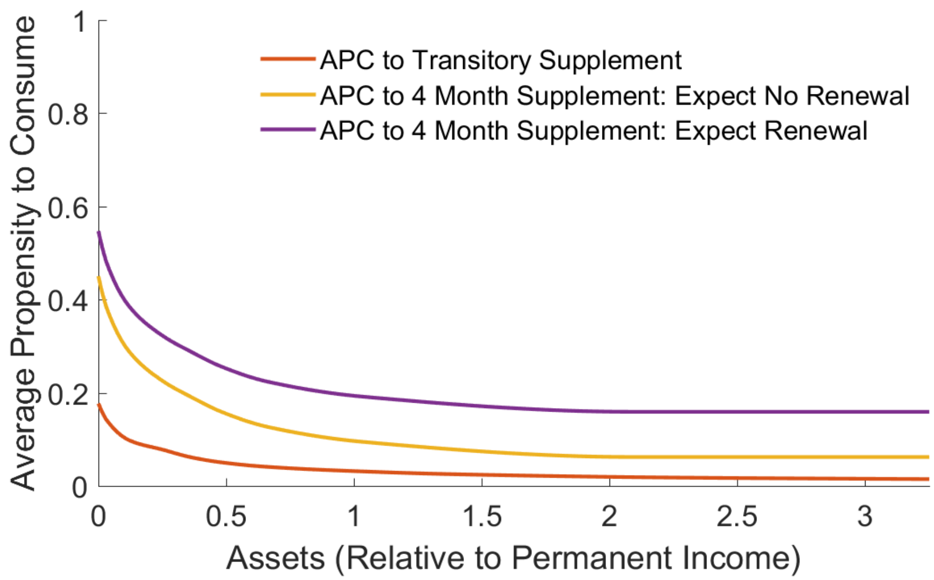


(b) MPC varies with current liquidity



Notes: Panel (a) compares the MPC out of a small temporary income change to the APC out of a larger temporary income change matched to the size of the $600 supplements, for an unemployed worker with the average wage w when employed. Panel (b) shows the MPC for an unemployed worker with average wage w when employed but for average asset levels corresponding to different durations of unemployment and supplement policies.

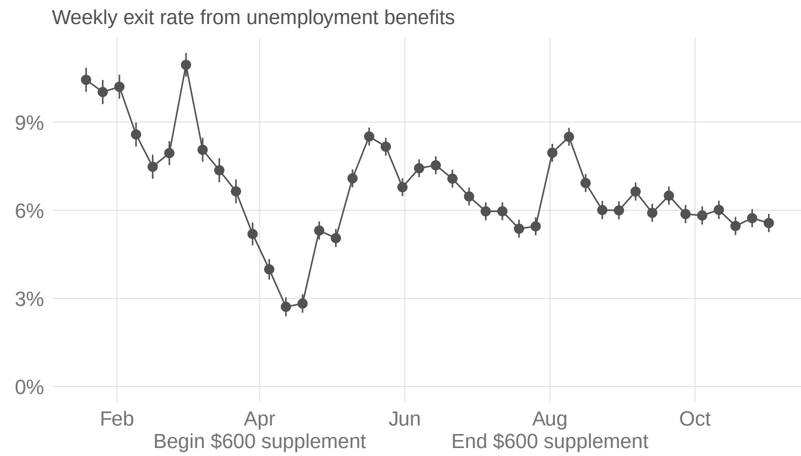
Figure 9: Role of Persistence



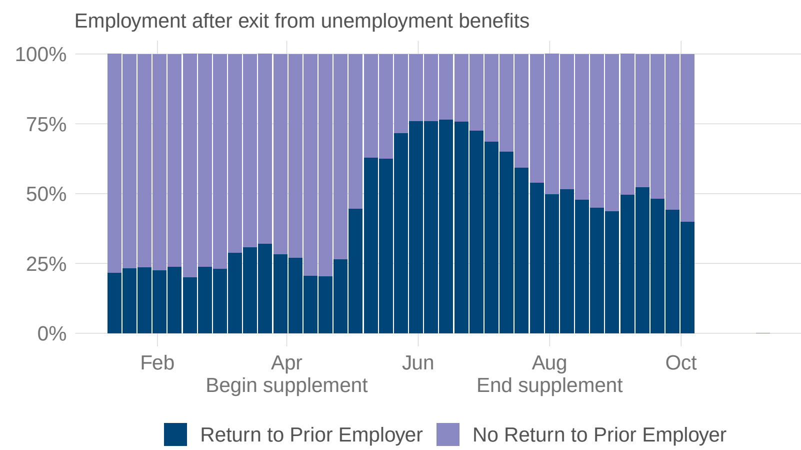
Notes: This compares to APC out of transfers matched to the size of the $600 supplements, but with different degrees of persistence. The transitory supplement is the response to a one-month unexpected change. The four-month supplement with no renewal is expected to last for exactly four months. The four-month supplement with renewal expected is expected to last forever, but the duration is bounded by the expected duration of unemployment.

Figure 10: Exit Rate and Recall Share

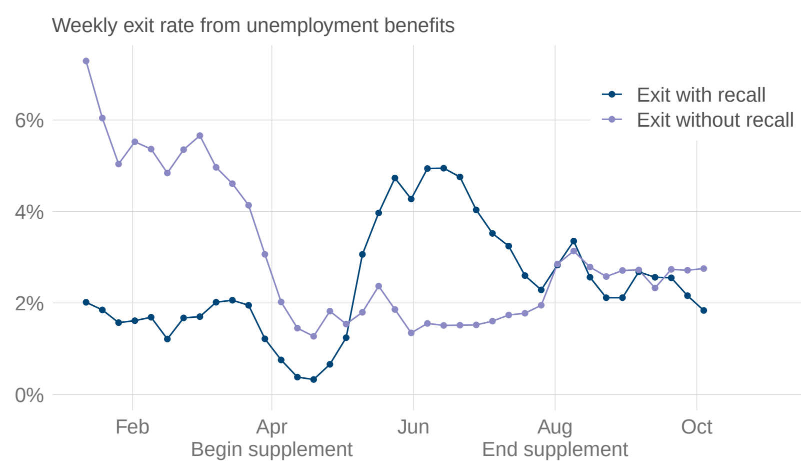
(a) Exit Rate



(b) Recall Share



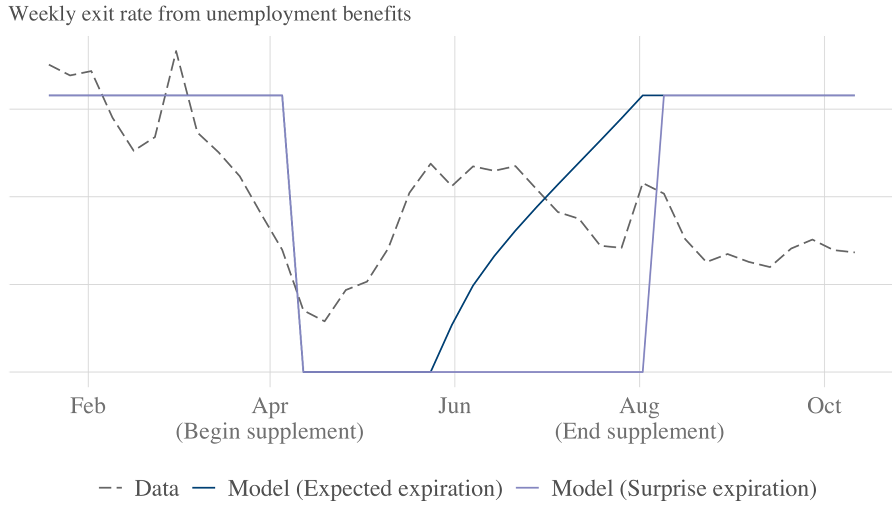
(c) Exit Rate by Recall



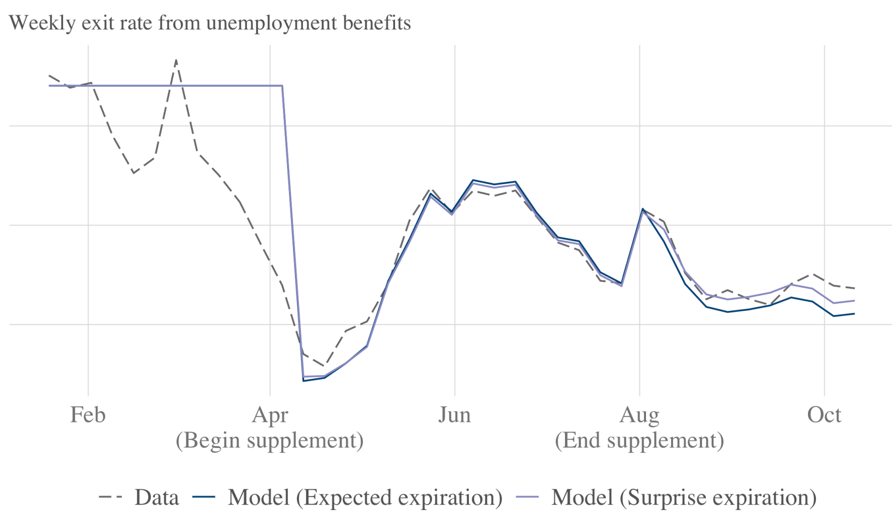
Notes: Panel (a) shows the exit rate with a 95 percent confidence interval, panel (b) shows the fraction of exits that return to a prior employer and panel (c) shows the exit rate by recall. Panels (b) and (c) are estimated using a subsample of UI recipients with an observed job separation. See Section 5 for details.

Figure 11: Exit Rate in Model versus Data

(a) Baseline Model

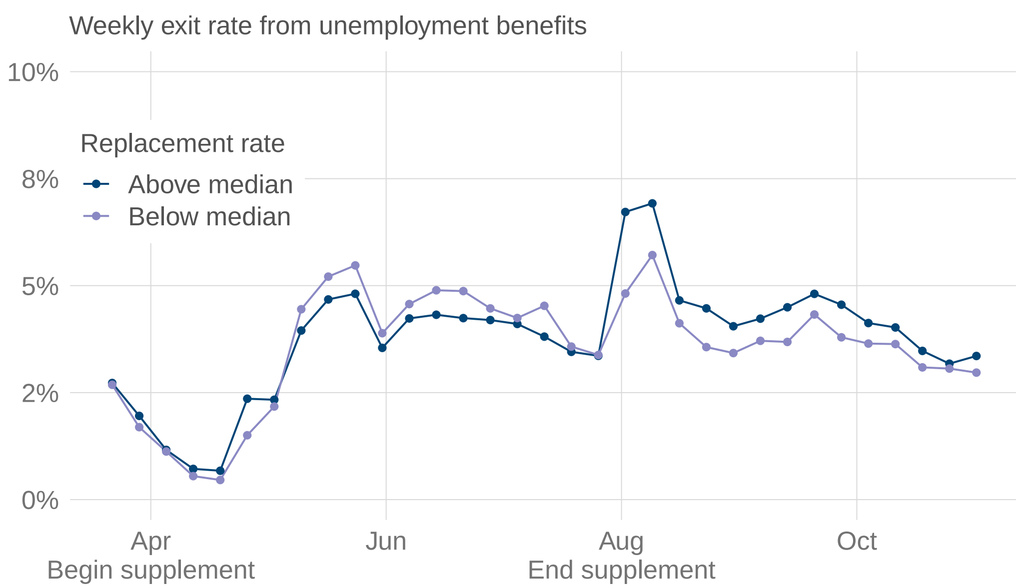


(b) Enriched Model



Notes: This figure shows the predicted evolution of job search during the pandemic in two models. The baseline model in panel (a) is calibrated to match average exit rates before the pandemic and prior estimates of the disincentive effect of UI benefit increases. The enriched model in panel (b) adds recalls to the baseline model and estimates the cost of job search during the pandemic to best match the path of exits from unemployment benefits. See Section 5.2 for details.

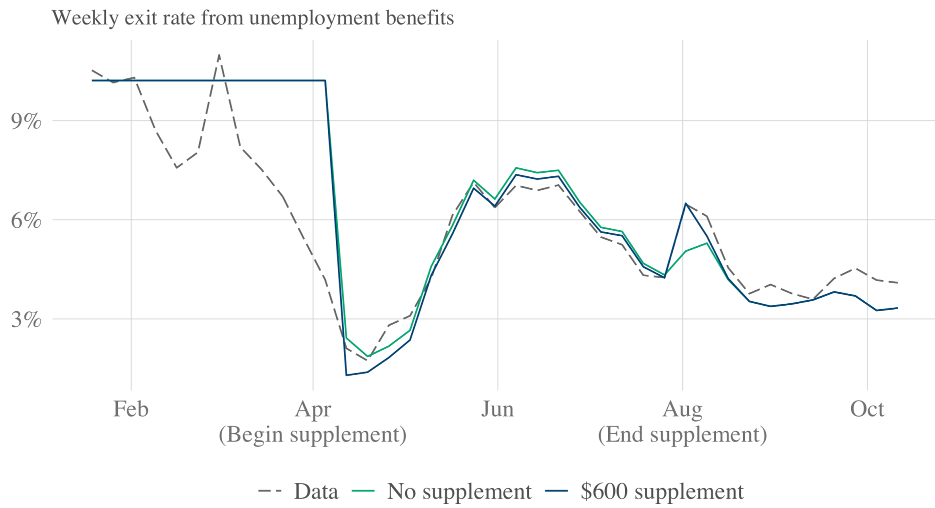
Figure 12: Exit Rate by Replacement Rate



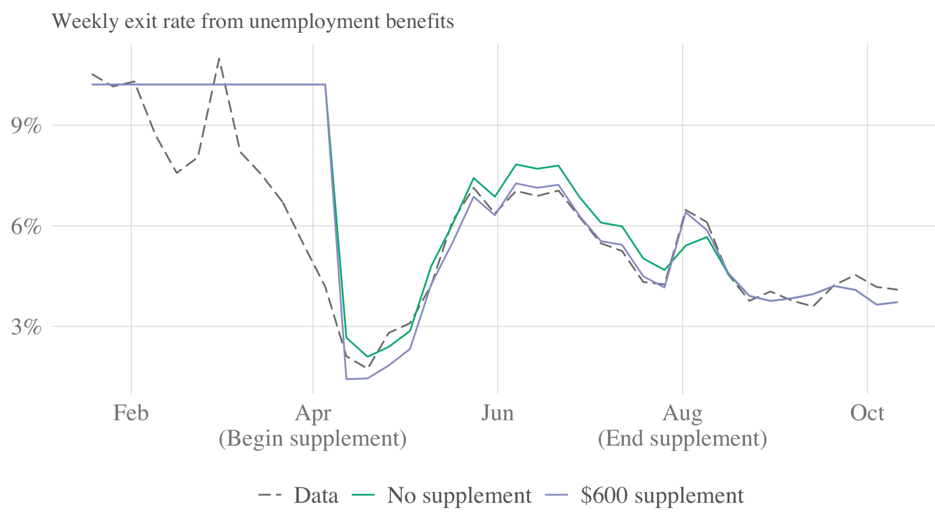
Notes: This figure shows the exit rate from unemployment benefits separately for recipients with above- and below-median replacement rates. See Section 5.3 for details.

Figure 13: Impact of the $600 Supplement on Job Search in the Enriched Model

(a) Expect expiration



(b) Surprise expiration



Notes: This figure shows the counterfactual predictions of search without the $600 supplement as simulated in the enriched model. See Section 5.4 for details.

Table 1: **Unemployment Spells During the Pandemic**

Month	Average active spells	Number of spell starts	Number of spell exits	Exit rate: All	Exit rate: Start UI in April	Exit rate: Job separation observed	Share: Continuously unemployed (since Apr)	Share: Continuously unemployed (since May)	Share: Unemployed repeatedly (since Apr)	Share: Exit to recall
Jan	28,500	13,125	12,742	54%	–	35%	–	–	–	23%
Feb	28,500	11,053	10,398	40%	–	30%	–	–	–	23%
Mar	36,948	28,392	9,203	30%	–	26%	–	–	–	29%
Apr	185,668	207,827	29,532	17%	19%	9%	100%	–	0%	32%
May	286,507	141,156	93,761	34%	31%	26%	70%	100%	3%	70%
Jun	282,776	76,695	85,708	30%	27%	26%	52%	81%	21%	74%
Jul	273,431	62,801	73,962	27%	20%	21%	44%	68%	34%	57%
Aug	243,203	43,809	75,423	31%	20%	23%	40%	62%	44%	48%
Sep	217,316	38,823	58,055	26%	17%	21%	37%	57%	57%	46%
Oct	201,794	37,432	51,225	25%	14%	–	34%	53%	61%	–

Notes: This table shows the number of unemployment spells in our data. The number of active spells is the monthly average, while the spell exits and starts are the sums for each month. Continuously unemployed are uninterrupted spells since April or May. The share of repeated unemployed workers is calculated since the beginning of the pandemic in April. Exit to recalls are workers returning to their previous employer.

Table 2: **Marginal Propensity to Consume out of Unemployment Benefits**

Research Design	Total Spending MPC
Waiting for benefits	0.43
	(0.03)
Expiration of $600	0.29
	(0.01)
$300 supplement	0.42
	(0.03)

Notes: This table shows estimated one-month total spending MPCs for three different unemployment benefit changes using the diff-in-diff research designs discussed in more detail in Section 4.2. The waiting for benefits design compares unemployed households receiving benefits to those who face benefit delays in March and May, the expiration design compares unemployed households to a sample of employed households who have income similar to the pre-separation income of the unemployed in July and August and the $300 supplement design compares unemployed in states that get $300 LWA payments early vs. late. Standard errors are clustered by state.

Table 3: **Model MPCs**

		Model	
Research Design	Data	Expiration Expected	Expiration Surprise
Waiting for benefits	0.43 (0.03)	0.37	0.44
Expiration of $600	0.29 (0.01)	0.03	0.21
$300 supplement	0.42 (0.02)	0.06	0.07

Notes: This table compares empirical MPCs to various policy changes to model equivalent exercises in a model simulation from April-October. The first column repeats the data estimates from Table 2. We compute model estimates under two different versions of expectations. In the expiration expected model, unemployed households anticipate that $600 supplements will expire in August. In the expiration surprise model, unemployed households anticipate that the $600 supplement will be renewed and are then surprised when it expires in August.

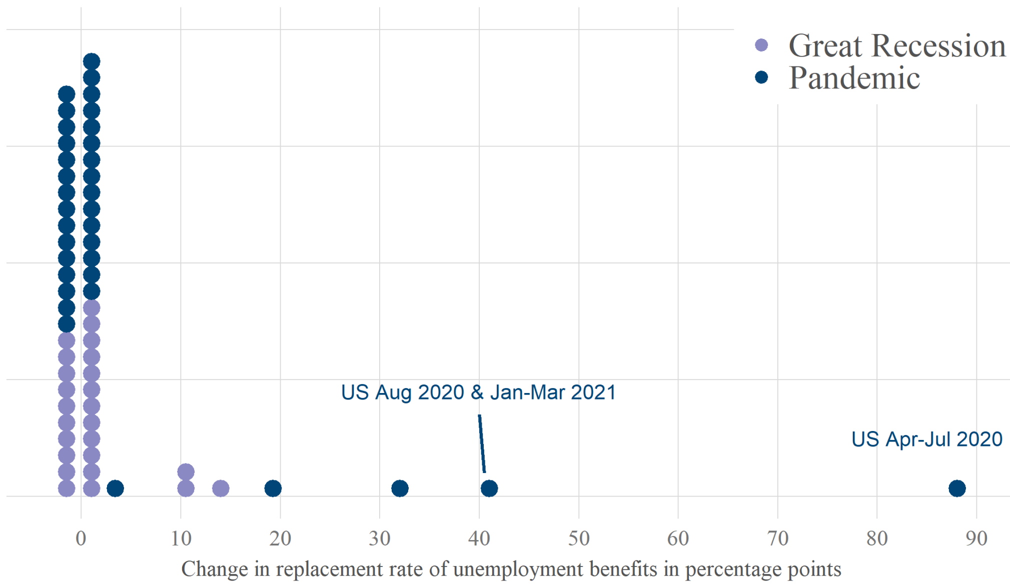
Table 4: **Job Search Disincentive Effects**

	Behavioral cost	Elasticity	Decrease in total employment from $600 supplement
Baseline model (Expect expiration)	0.35	0.33	4.18%
Baseline model (Surprise expiration)	0.35	0.33	6.09%
Enriched model (Expect expiration)	0.02	0.01	0.19%
Enriched model (Surprise expiration)	0.02	0.02	0.39%

Notes: This table shows various distortion statistics for the baseline model and the enriched model. The behavioral cost is the additional cost to the government of increasing benefits by $1.00 due to benefits recipients increasing their UI durations. The elasticity is the elasticity of unemployment duration with respect to benefit levels. Both the behavioral cost and the elasticity are calculated with respect to a marginal change that lasts for 26 weeks in an environment when standard benefits last the full extended 52 weeks. The baseline model is calibrated to target the median behavioral cost of 0.35 from prior microeconomic studies reviewed by Schmieder and von Wachter (2016), while the enriched model is estimated to match actual job search behavior during the pandemic. The decrease in employment is calculated from April through July 2020 by counting how many more person weeks of employment are predicted without the supplement under the model. We count the number of people entering unemployment in each month as the number of people receiving a first payment of PUA or regular UI in that month according to DOL ETA reports data.

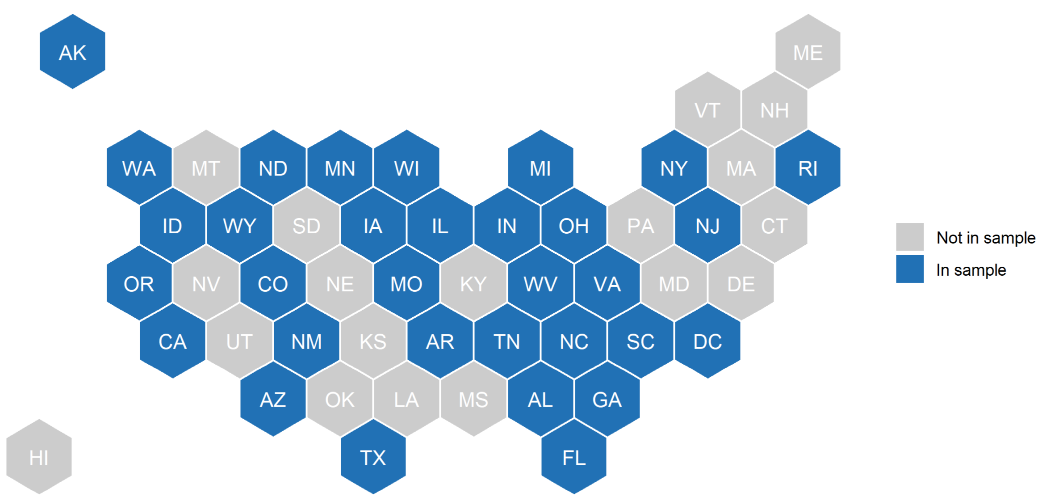
A Appendix

Figure A-1: The Rise in Unemployment Benefit Replacement Rates in OECD Countries



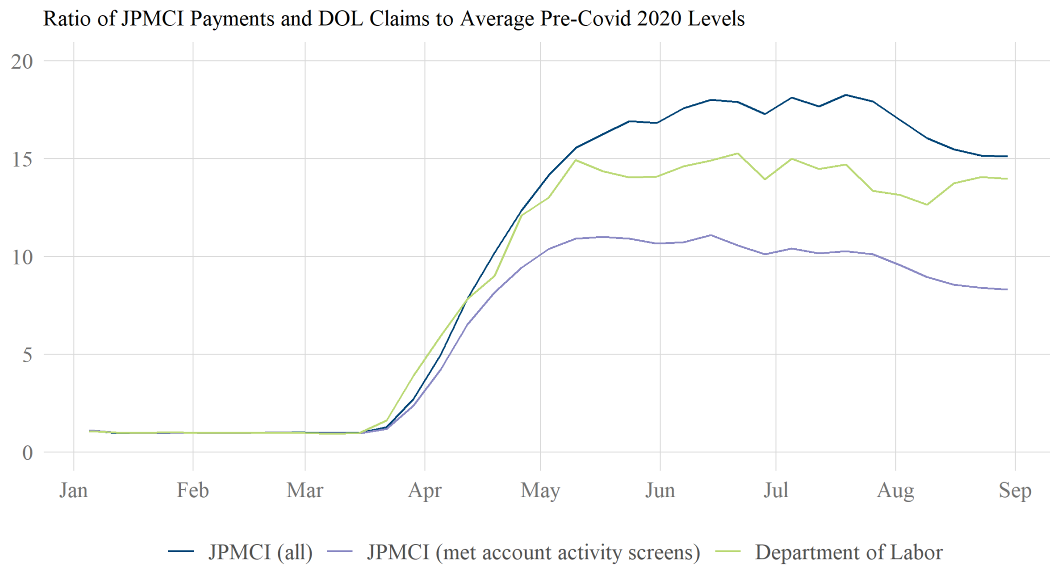
Notes: Each dot represents the increase of an OECD country's replacement rate. The replacement rates are for a single person who earns 67 percent of the average domestic wage. The data until 2019 is from OECD Stats. The replacement rates in response to the pandemic are based on the OECD Employment Outlook 2020 and Ganong, Noel, and Vavra (2020).

Figure A-2: States included in the JPMCI sample



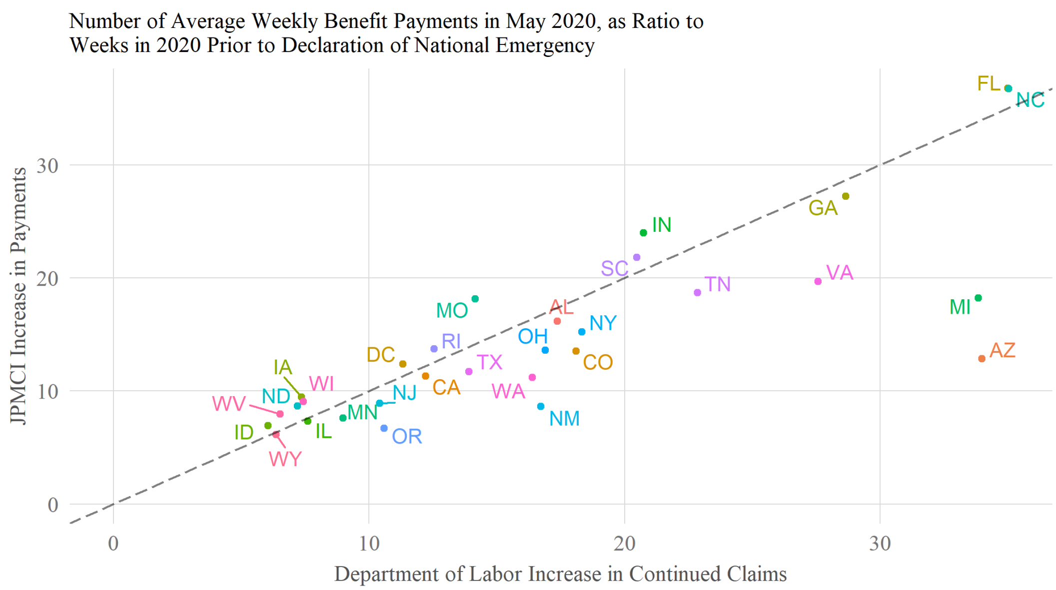
Notes: States are in the JPMCI sample if we are able to identify at least 1000 UI benefit payments by direct deposit.

Figure A-3: Rise in UI payments and claims, JPMCI vs DOL



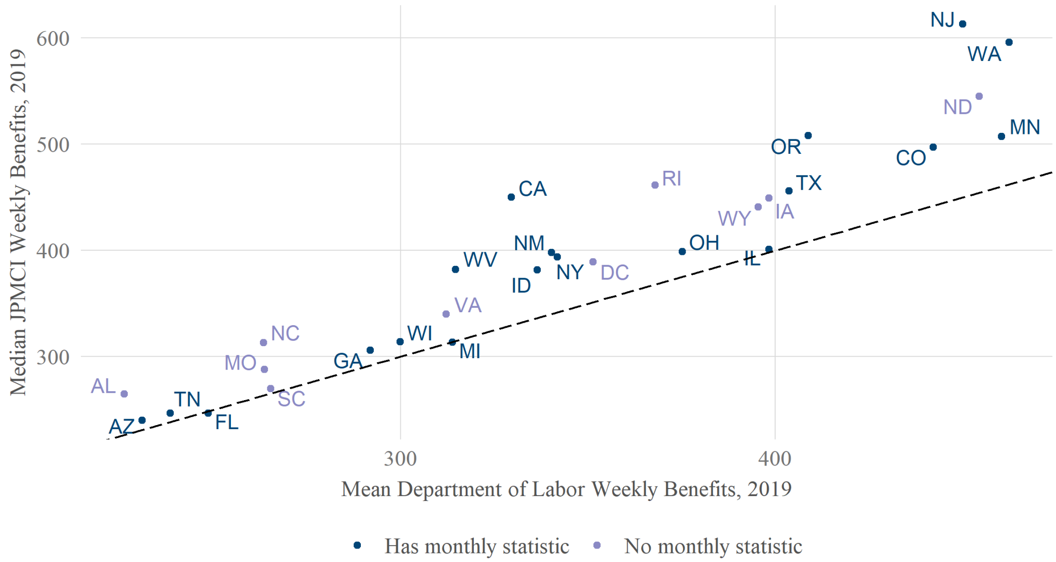
Notes: This figure shows time trends in the number of unemployment benefit recipients in JPMCI and data reported by states to the U.S. Department of Labor (DOL). The series shows payments or claims as a ratio of the 2020 pre-pandemic average, that is, the average weekly number of payments or claims in the period prior to the Declaration of National Emergency. Two series are shown for JPMCI: all JPMCI unemployment insurance benefit recipients and the subset who met account activity screens. The DOL series is lagged by one week to reflect a time lapse between a claim being approved and receiving a payment. The series shows the weekly number of continued claims for regular Unemployment Compensation (including payments made under Unemployment Compensation for Federal Employees, Unemployment Compensation for Ex-Service members, Extended Benefits, Short-Time Compensation and the Self-Employment Assistance Program), Pandemic Emergency Unemployment Compensation, and Pandemic Unemployment Assistance.

Figure A-4: Rise in UI claims during the pandemic by state JPMCI vs DOL



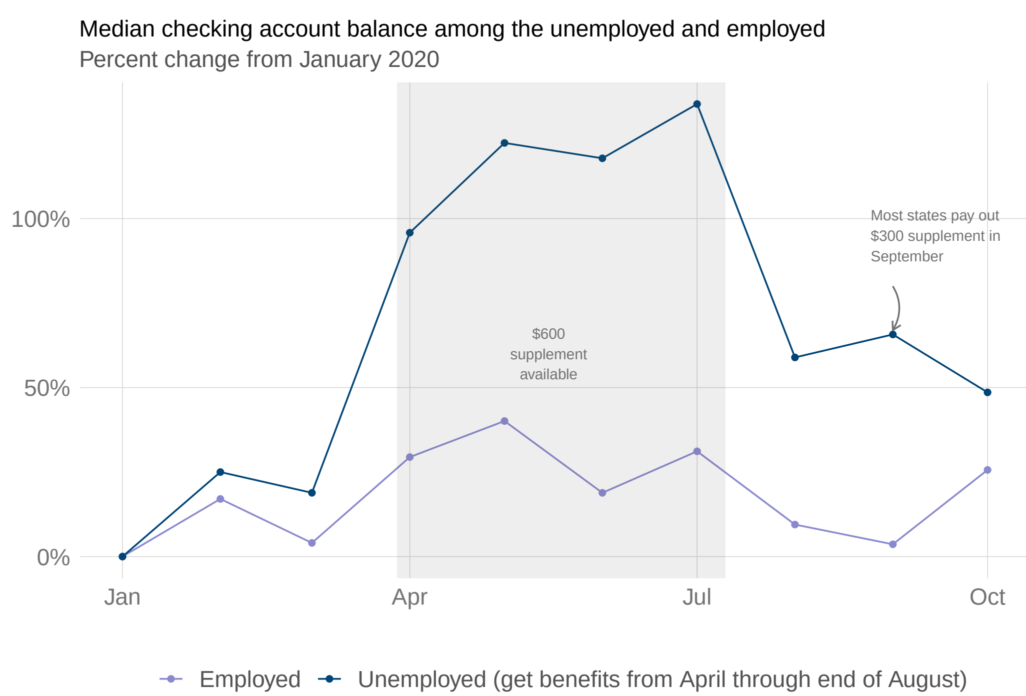
Notes: This figure compares the rise in UI payments in JPMCI with the rise in U.S. Department of Labor (DOL) continued claims during the pandemic. The rise is computed as the average number of weekly payments or claims in May divided through by the average in the pre-pandemic period, that is, the average number of weekly payments or claims in the period prior to the Declaration of National Emergency in mid-March. The 45° line represents an equal rise in JPMCI payments and DOL claims.

Figure A-5: Median benefits by state JPMCI vs DOL



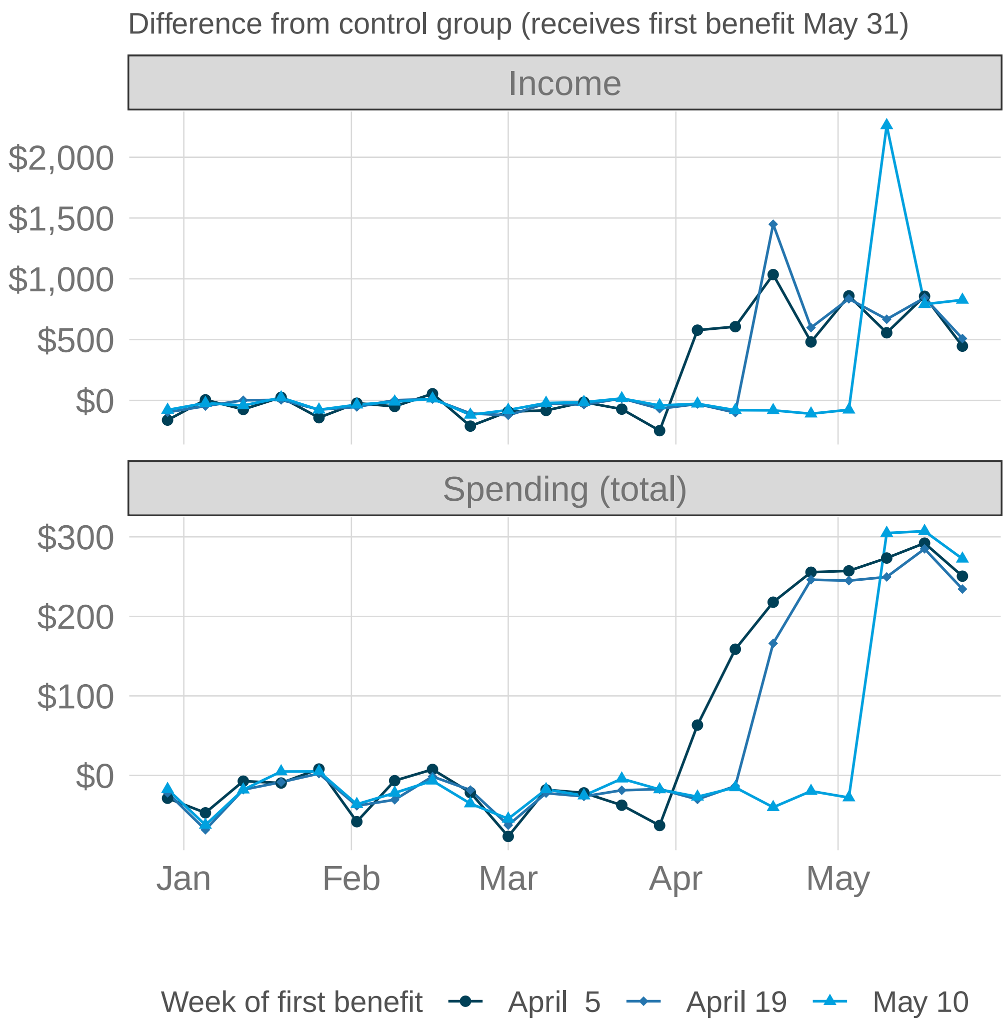
Notes: This figure compares the median JPMCI weekly benefits paid out in 2019 to the mean U.S. Department of Labor (DOL) weekly benefits paid out in 2019. The latter is computed by weighting the mean of DOL weekly benefits for each state and month (dividing total amount compensated through by total number of weeks paid out) by the number of weeks paid out in that period.

Figure A-6: Checking Account Balances



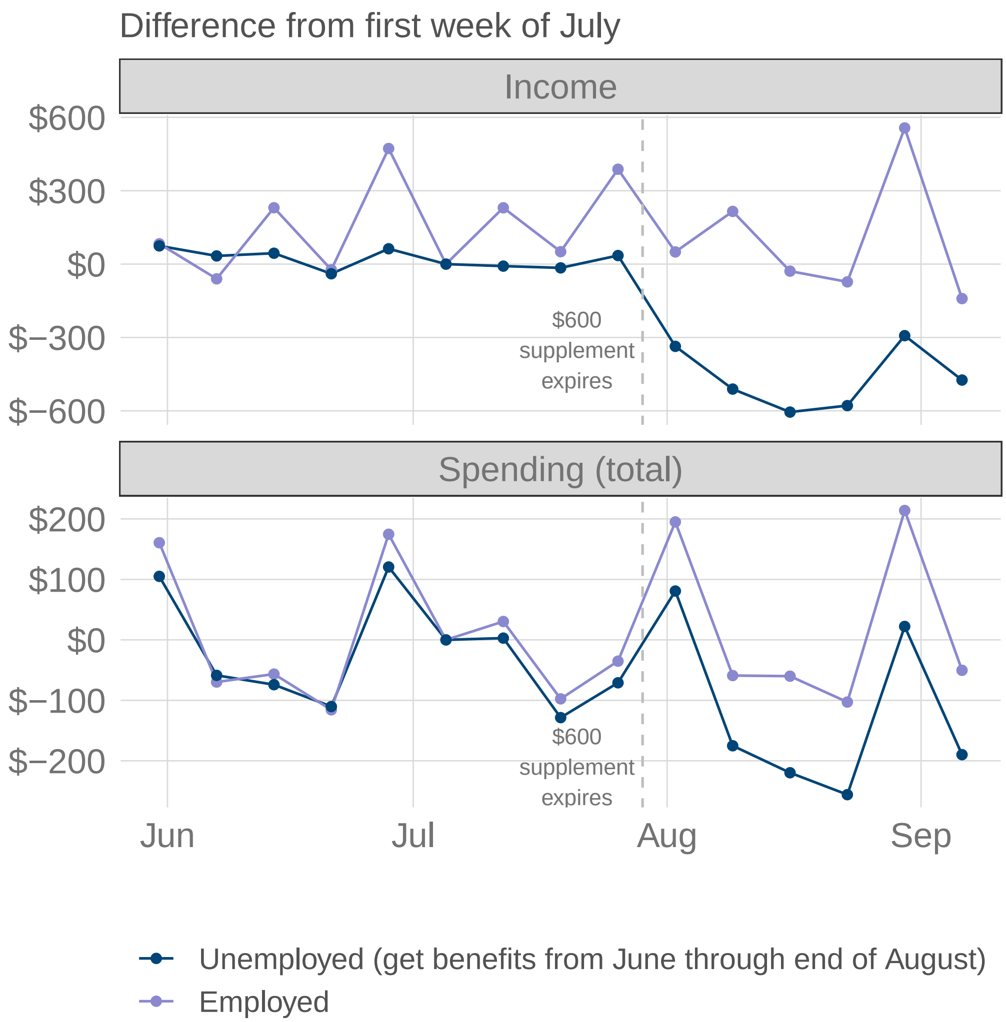
Notes:

Figure A-7: Weekly Total Spending at the Start of Benefit Receipt



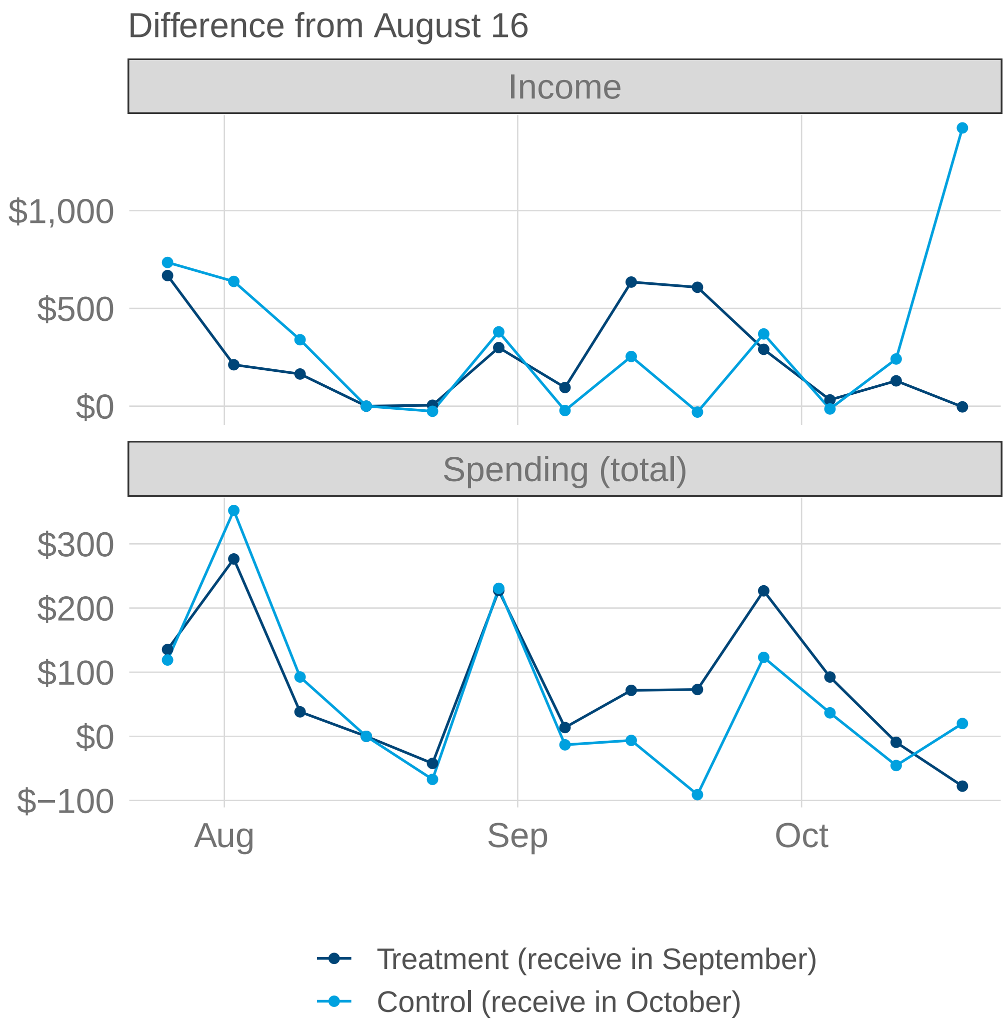
Notes: This figure replicates Figure 3 but for weekly total spending instead of just cash and card spending.

Figure A-8: Weekly Total Spending at the Expiration of the $600 Supplement



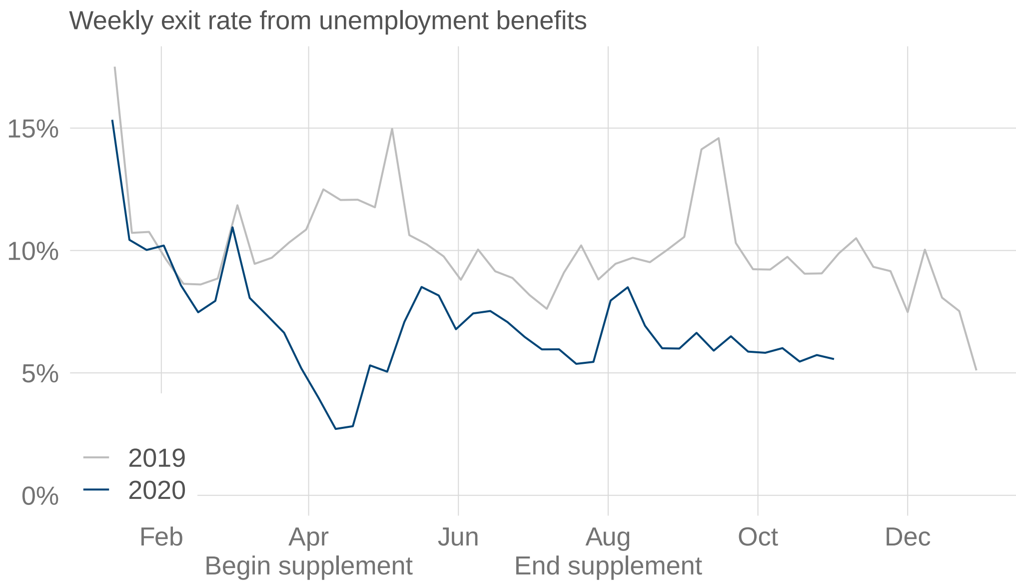
Notes: This figure replicates Figure 4 but for weekly total spending instead of just cash and card spending.

Figure A-9: Weekly Total Spending at Receipt of Temporary $300 Supplement



Notes: This figure replicates Figure 5 but for weekly total spending instead of just cash and card spending.

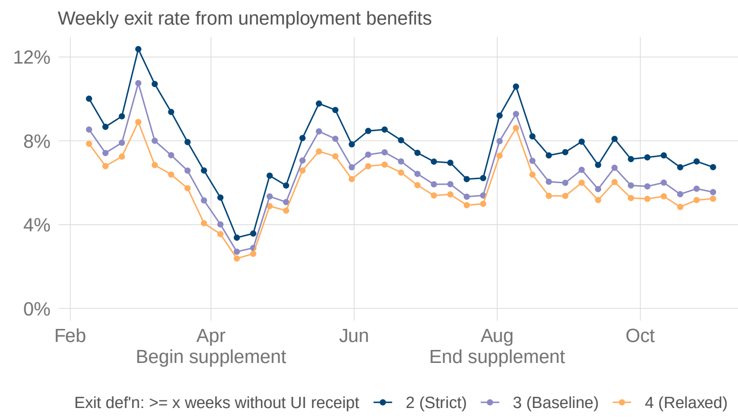
Figure A-10: Exit Rate by Year



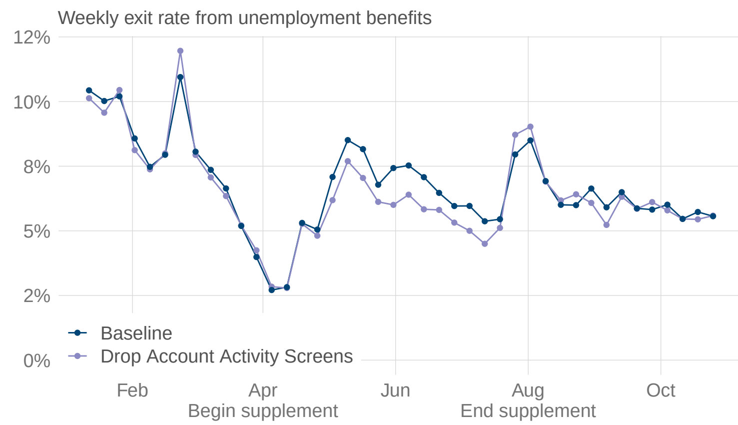
Notes: This figure extends Figure 10a to include data from 2019.

Figure A-11: Exit Rate Robustness

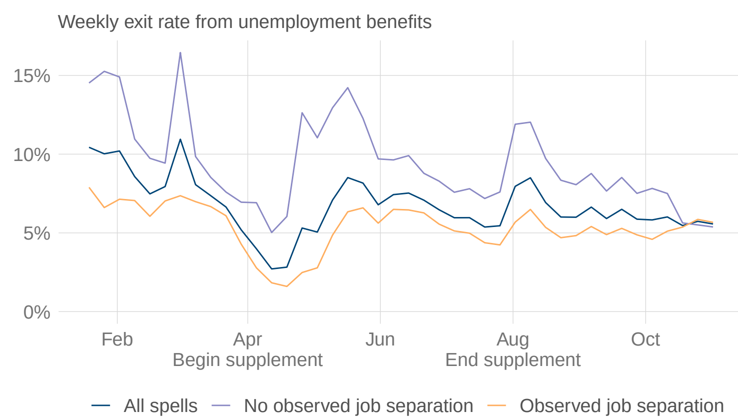
(a) Vary Minimum Weeks Without Receipt to Define Exit



(b) Drop Account Activity Screen

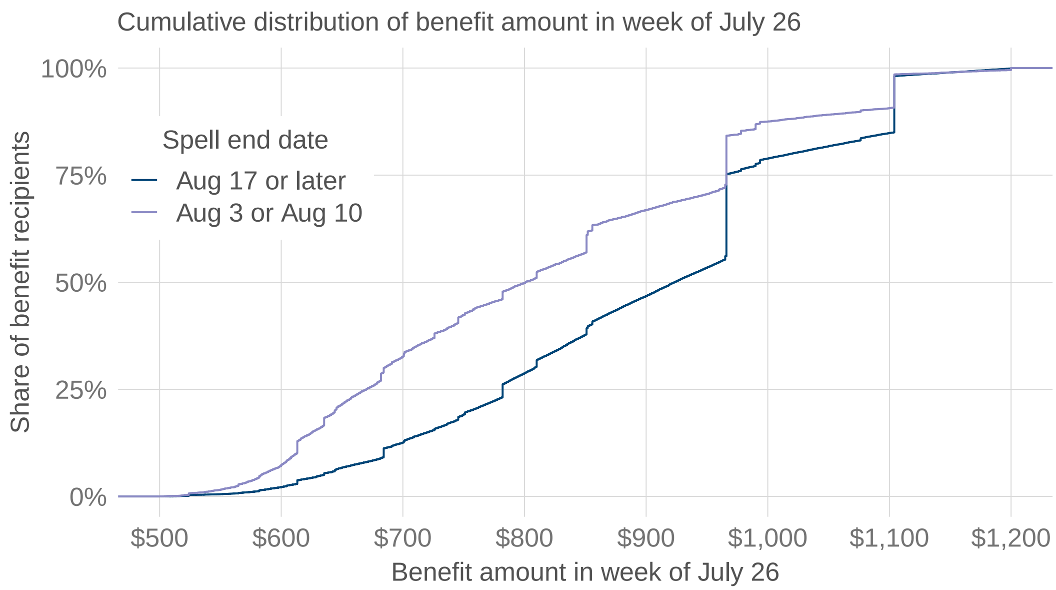


(c) Job Separation Observed at Start of Spell



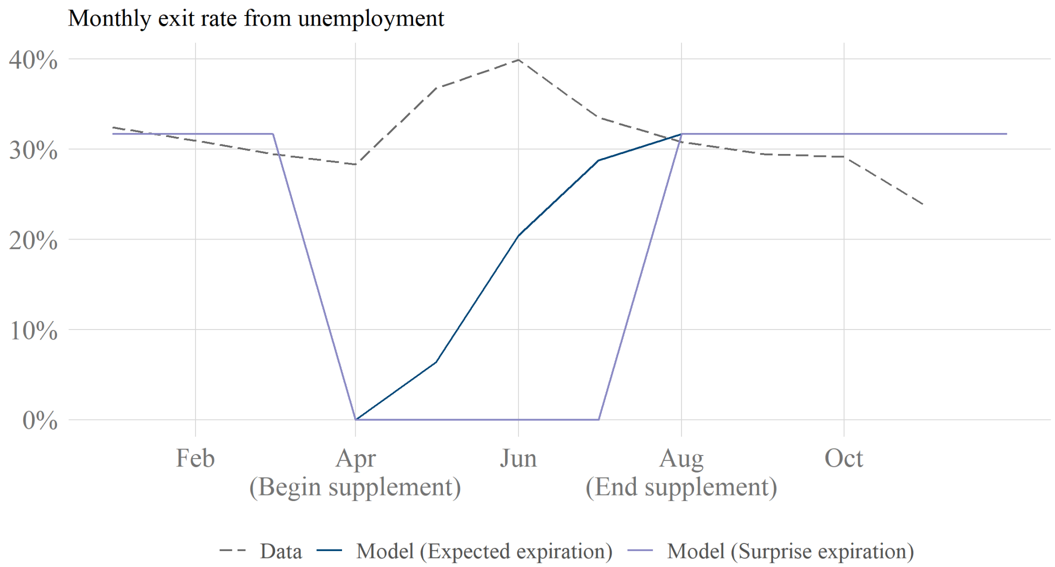
Notes: Panel (a) shows exit rates for alternative definitions of how many weeks without a payment are used to define the end of UI spell. Panel (b) shows exit rates for the universe of UI recipients, thereby dropping the account activity screen discussed in Section 2.2. Panel (c) shows exit rates separately for recipients with and without a separation observed at the start of the spell.

Figure A-12: Distribution of Weekly Benefit Among Exits at Expiration of $600 Supplement



Notes: Because the $600 supplement is available to everyone, regardless of their weekly benefit amount, Recipients who have lower weekly benefits have *higher* replacement rates. Sample is only states that pay benefits every week, because it is hard to discern the weekly benefit amount from states that pay fortnightly. Although the pre-tax weekly benefit amount should always exceed $600, a small number of recipients receive payments with weekly benefit amounts lower than $600 because of income tax withholding.

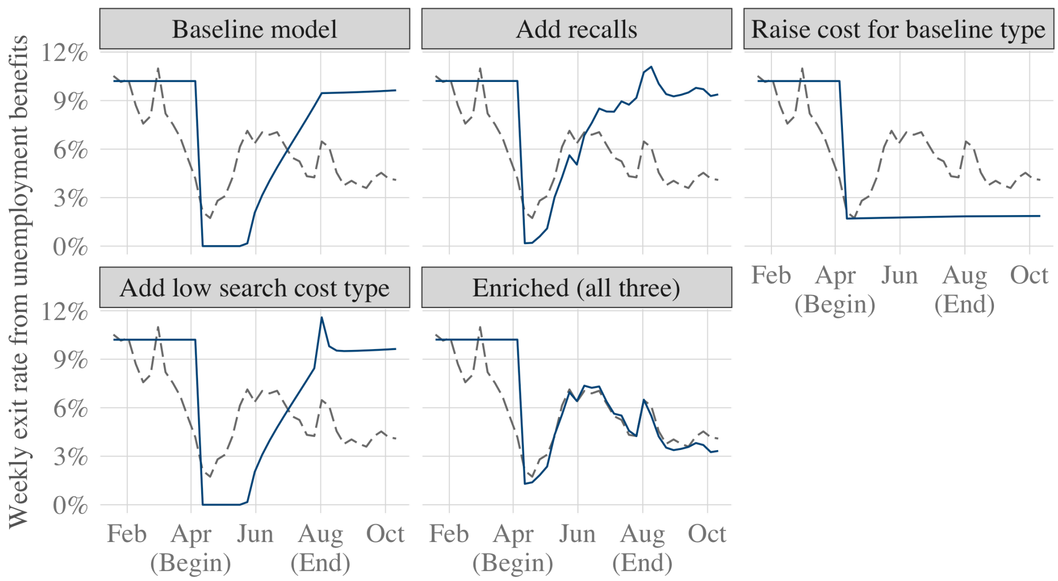
Figure A-13: Exit Rate: Baseline Model versus Data from the Current Population Survey



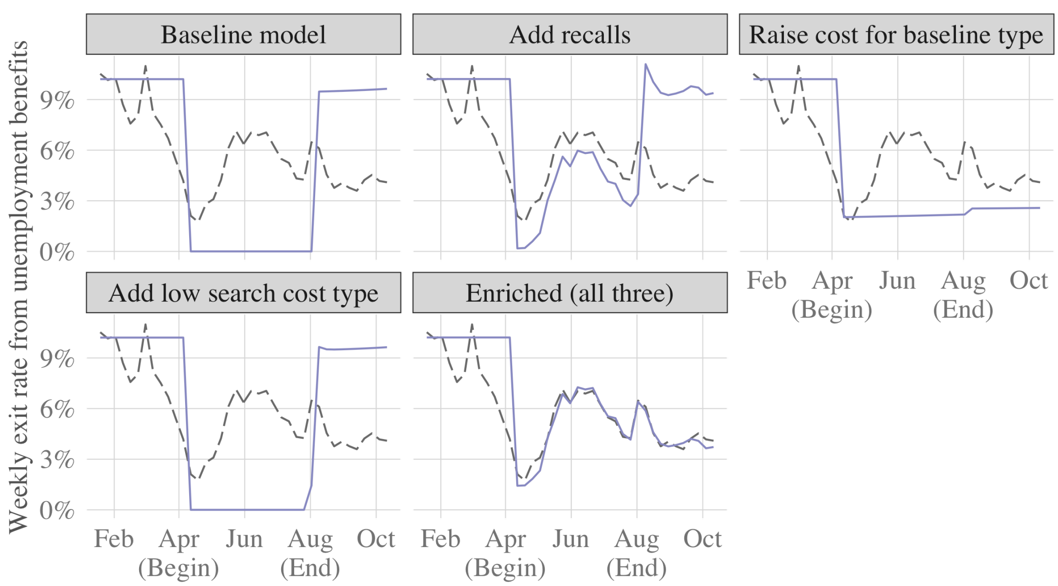
Notes: This figure replicates Figure 11a using monthly unemployment exit rates from the CPS instead of weekly UI exit rates from the Chase sample. See Section 5.2.2 for details.

Figure A-14: Components of Job Search Model

(a) Predictable Expiration

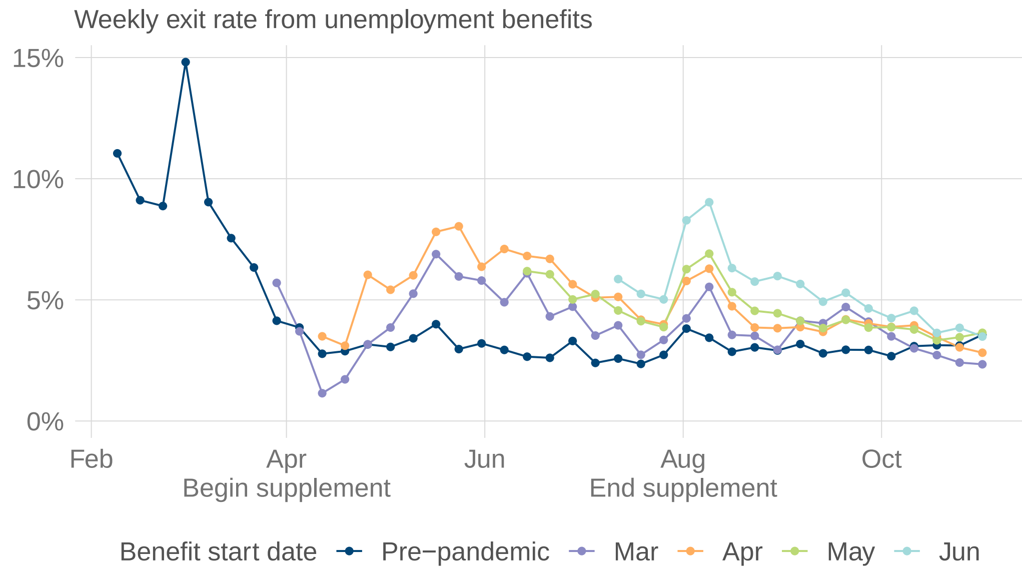


(b) Surprise Expiration



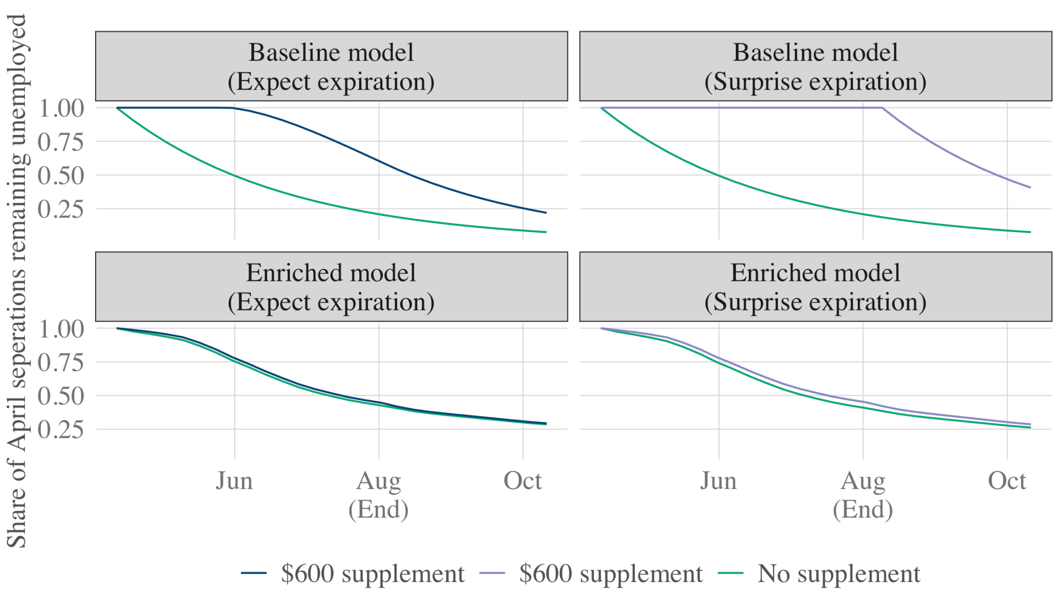
Notes: These plots show predicted search in a range of models. The baseline model in the top left panel is calibrated to match the 0.35 behavioral cost and the level of search in January and February. "Add recalls" re-calibrates the model assuming that recalls are fixed at their average rate in the pre-period and adds the exogenous series of observed recalls during the pandemic. "Add low search cost" includes a small share of types who exit quickly once the benefits expire as explained in the text. "Enriched (all three)" is the full model as calibrated to the search during the pandemic. "Raise cost for the baseline type" maintains the cost function from this model, but does not include the low search cost type and turns off recalls. See Section 5.2.3 for details.

Figure A-15: Exit Rate by Start Date of Unemployment Benefit Spell



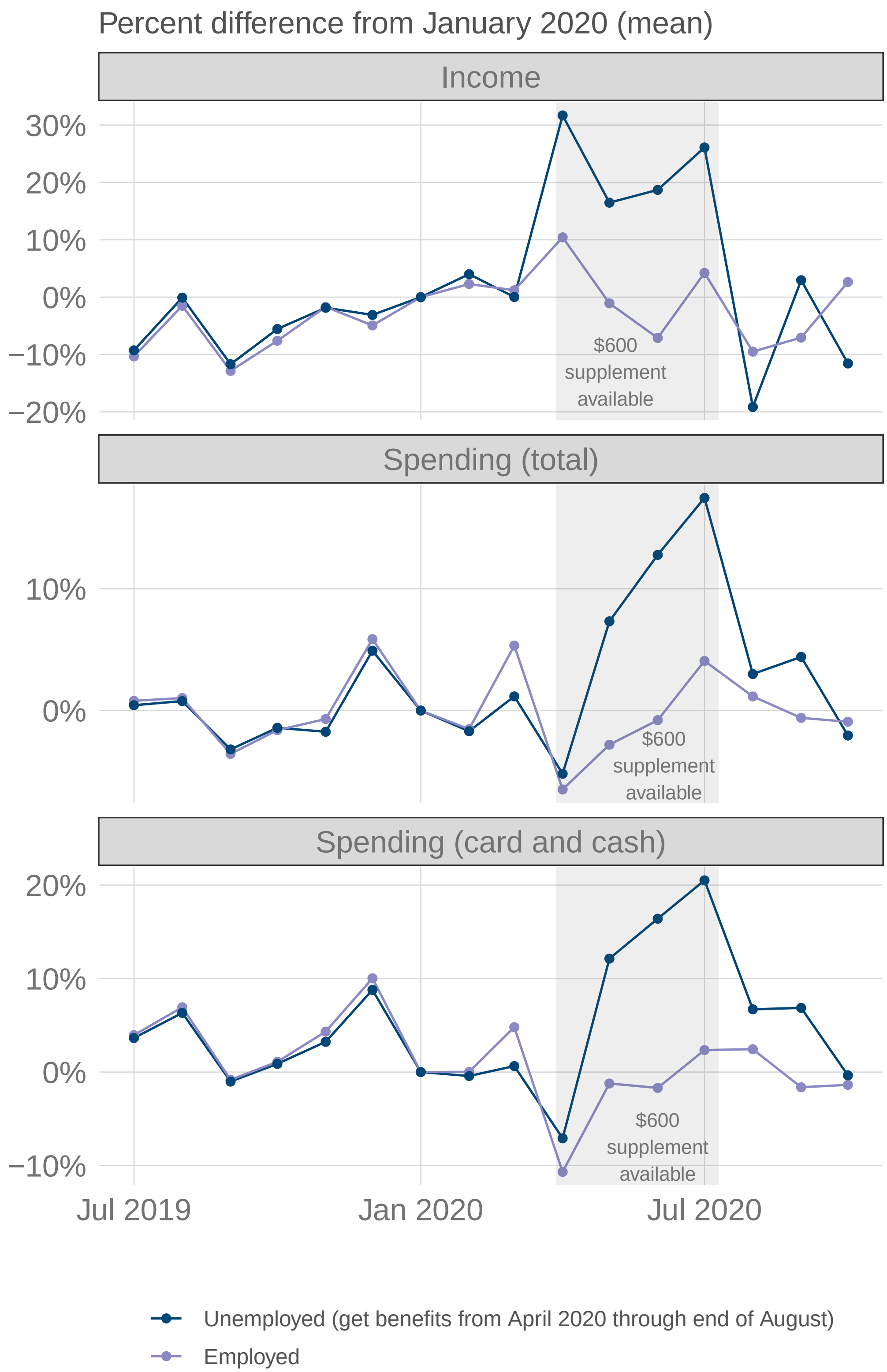
Notes: This exhibit replicates Figure 10a separately by start date of unemployment benefit spell.

Figure A-16: Predicted Survival Curves With and Without the $600 Supplement



Notes: This figure uses the baseline and the enriched models to plot the predicted share of April separations remaining unemployed over the April - October period. The models are used to provide a simulation both with and without the $600 benefit supplement. See Section 5.4 for details.

Figure A-17: Mean Spending and Income by Cohort



Notes: This replicates Figure 2 but for mean income and spending instead of median income and spending.

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

Group (months)	Income	Unemployment benefits	Spending (card and cash)	Spending (total)
Employed (Jan-Feb 2020)	$5459	$0	$2171	$3892
Employed (Apr-Oct 2020)	$5359	$0	$2089	$3858
Unemployed (Jan-Feb 2020)	$4570	$0	$2084	$3437
Unemployed (Apr-Oct 2020)	$4942	$1764	$2226	$3689

Notes: This table shows the median monthly values of income, unemployment benefits, spending (card and cash), and spending (total) for employed and unemployed households, before and during the pandemic. Employed households receive no direct-deposited unemployment benefits to their Chase accounts in 2020, while unemployed households do receive direct-deposited unemployment benefits to their Chase accounts in 2020. In this table, the pre-pandemic period is Jan-Feb 2020, while the pandemic period is Apr-Oct 2020.

Table A-2: **Length of Unemployment Spells During the Pandemic**

		Quantiles		
		25%	50%	75%
Mar-Oct 2020	Number of weeks in spell	3	8	19
Mar-Oct 2020	Number of weeks receiving benefits (across all spells)	5	12	24
Mar-Oct 2019	Number of weeks in spell	2	5	11
Mar-Oct 2019	Number of weeks receiving benefits (across all spells)	3	7	14

Notes: This table shows the length of spells at the 25^{th}, 50^{th}, and 75^{th} percentile. The upper two rows show the spell lengths during the pandemic in weeks. The lower two rows serve as a benchmark and show the lengths for 2019.

Table A-3: **MPCs Robustness to Spending Measure**

Research Design	Card and Cash MPC
Waiting for benefits	0.31
	(0.02)
Expiration of $600	0.23
	(0.01)
$300 supplement	0.36
	(0.02)

Notes: This table shows the robustness of estimated one-month MPCs in Table 2 to using card and cash spending instead of total spending. Standard errors are clustered by state.

Table A-4: **Model APC robustness**

		Model ($\beta = .95$)		Model ($\beta = .85$)	
Research Design	Data	Expiration Expected	Expiration Surprise	Expiration Expected	Expiration Surprise
Waiting for benefits	0.43	0.03	0.10	0.58	0.65
Expiration of $600	0.29	0.01	0.13	0.03	0.26
$300 supplement	0.42	0.02	0.01	0.08	0.12

Notes: This table computes robustness of our main model MPC exercises under alternative discount factors. While increasing the level of impatience can increase MPCs in response to the $300 supplement, this results in MPCs at benefit in the waiting for benefits design which are too high relative to the data.

Table A-5: **Weekly Unemployment Spells During the Pandemic**

				Exit rate			Share			
Week start date	Number of active spells	Number of spell starts	Number of spell exits	All	Start UI in April	Job separation observed	Contin-uously unemployed (since Apr)	Contin-uously unemployed (since May)	Unemployed repeatedly (since Apr)	Exit to recall
2020-01-05	29,424	4,341	4,513	15.3%	–	9.3%	–	–	–	22%
2020-01-12	28,002	3,091	2,922	10.4%	–	7.9%	–	–	–	23%
2020-01-19	28,197	3,116	2,826	10.0%	–	6.6%	–	–	–	24%
2020-01-26	28,377	3,007	2,895	10.2%	–	7.1%	–	–	–	23%
2020-02-02	28,350	2,867	2,431	8.6%	–	7.1%	–	–	–	24%
2020-02-09	28,115	2,198	2,102	7.5%	–	6.1%	–	–	–	20%
2020-02-16	28,203	2,190	2,240	7.9%	–	7.0%	–	–	–	24%
2020-02-23	29,331	3,368	3,211	10.9%	–	7.4%	–	–	–	23%
2020-03-01	28,567	2,448	2,303	8.1%	–	7.0%	–	–	–	29%
2020-03-08	28,400	2,137	2,090	7.4%	–	6.7%	–	–	–	31%
2020-03-15	28,686	2,376	1,905	6.6%	–	6.1%	–	–	–	32%
2020-03-22	34,815	8,033	1,807	5.2%	–	4.3%	–	–	–	28%
2020-03-29	64,274	31,263	2,563	4.0%	–	2.8%	–	–	–	27%
2020-04-05	115,258	53,542	3,125	2.7%	3.5%	1.8%	100%	–	0%	21%
2020-04-12	169,163	57,024	4,775	2.8%	3.1%	1.6%	100%	–	0%	20%
2020-04-19	213,814	49,417	11,348	5.3%	6.0%	2.5%	100%	–	0%	27%
2020-04-26	244,438	41,970	12,346	5.1%	5.4%	2.8%	100%	–	0%	45%
2020-05-03	272,325	40,231	19,292	7.1%	6.0%	4.9%	85%	100%	1%	63%
2020-05-10	286,482	33,445	24,381	8.5%	7.8%	6.3%	76%	100%	1%	63%
2020-05-17	290,761	28,660	23,730	8.2%	8.0%	6.6%	69%	100%	3%	72%
2020-05-24	290,861	23,832	19,731	6.8%	6.4%	5.6%	63%	100%	5%	76%
2020-05-31	292,107	20,977	21,703	7.4%	7.1%	6.5%	59%	93%	8%	76%
2020-06-07	288,816	18,414	21,744	7.5%	6.8%	6.5%	56%	88%	13%	77%
2020-06-14	283,895	16,820	20,089	7.1%	6.7%	6.3%	53%	83%	20%	76%
2020-06-21	281,249	17,445	18,192	6.5%	5.6%	5.6%	50%	78%	26%	73%
2020-06-28	277,142	14,085	16,520	6.0%	5.1%	5.1%	48%	76%	27%	69%
2020-07-05	275,598	14,976	16,437	6.0%	5.1%	5.0%	46%	72%	30%	65%
2020-07-12	273,733	14,572	14,701	5.4%	4.2%	4.4%	45%	70%	31%	59%
2020-07-19	273,212	14,182	14,892	5.5%	4.0%	4.2%	43%	67%	33%	54%
2020-07-26	271,180	12,859	21,574	8.0%	5.8%	5.7%	42%	65%	41%	50%
2020-08-02	261,098	11,492	22,190	8.5%	6.3%	6.5%	41%	64%	39%	52%
2020-08-09	248,952	10,045	17,242	6.9%	4.7%	5.3%	40%	63%	43%	48%
2020-08-16	240,660	8,952	14,454	6.0%	3.9%	4.7%	40%	62%	42%	45%
2020-08-23	235,011	8,805	14,088	6.0%	3.8%	4.8%	39%	60%	46%	44%
2020-08-30	230,292	9,374	15,283	6.6%	3.9%	5.4%	39%	59%	53%	50%
2020-09-06	223,544	8,374	13,211	5.9%	3.7%	4.9%	38%	59%	54%	52%
2020-09-13	220,295	10,122	14,312	6.5%	4.2%	5.3%	37%	57%	60%	48%
2020-09-20	214,832	8,848	12,610	5.9%	4.0%	4.9%	37%	56%	55%	44%
2020-09-27	210,591	8,371	12,261	5.8%	3.9%	4.6%	36%	55%	57%	40%
2020-10-04	206,916	8,589	12,440	6.0%	3.9%	–	35%	54%	59%	–
2020-10-11	202,599	8,123	11,068	5.5%	3.5%	–	35%	53%	60%	–
2020-10-18	200,489	8,959	11,489	5.7%	3.0%	–	34%	52%	61%	–
2020-10-25	197,172	8,173	10,973	5.6%	2.8%	–	33%	51%	63%	–

Notes: This table shows the number of weekly unemployment spells in our data. Continuously unemployed are uninterrupted spells since April or May. The share of repeated unemployed workers is calculated since the beginning of the pandemic in April. Exit to recalls are workers returning to their previous employer

A.1.1 Data Appendix

Although the bank account data are available through the end of November, our estimates are truncated before the end of November because of data constraints. There are two types of constraints. First, not all transactions are reported in the final weeks of the most recent reporting month (November). Therefore, throughout our analysis, we report results only through the end of October. Second, when we examine recalls, we are only able to produce estimate through the beginning of October, because we need five weeks of data *after* a UI exit to ascertain whether a recall has occurred.

To be included in our analysis, households must meet three sample criteria. First, households must meet the account activity screens described in section 2.2. Second, households must have a non-missing checking account balance, positive account inflows, and positive account outflows in every month of January through October 2020. Together, these restrictions increase our confidence that the households in our sample use their Chase accounts as their primary bank accounts during the period of interest.

We drop a small number of unemployment spells. First, we drop 13,000 households that get benefits from multiple states. Second, we drop a small number of households for whom we are concerned that we are missing regular benefit payments, which would lead to a mechanical exhaustion at the expiration of the \$600 supplement. Specifically, we drop 9,700 workers whom we observe (a) receiving just the \$600 supplement (and no regular benefits) in more than half of their UI benefit payments in New Jersey, (b) receiving just a \$600 supplement during the week of July 26 in New Jersey, or (c) receiving just the \$600 supplement less tax withholding in Wisconsin. Altogether, this leaves a sample of 825,000 workers.

In addition to dropping potentially misleading unemployment spells, we also apply a fix to the data in Washington State. While Washington ordinarily pays unemployment benefits on a weekly basis, we observe that it largely did not make payments the week of September 6, 2020 (Labor Day week), and instead deferred these payments to the week of September 13. This leads to an underestimate of the number of exits from unemployment the week of September 6. To correct this underestimate, we reallocate roughly 500 unemployment exits observed the week of September 13 to the week of September 6, such that the exit rate is equal in each of these two weeks.

Micro and Macro Disincentive Effects of Expanded Unemployment Benefits *

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July 29, 2021

Abstract

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

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

*This paper updates and extends the job search results in "Spending and Job Search Impacts of Expanded Unemployment Benefits: Evidence from Administrative Micro Data". We thank Gabriel Chodorow-Reich, Jon Gruber, Rohan Kekre, Bruce Meyer, Matt Notowidigdo, and Heather Sarsons for helpful conversations, seminar participants at the AEA, BFI China, CFPB, Chicago Booth Micro Lunch, Clemson, Federal Reserve Board, Johns Hopkins, NBER Labor Studies, Montana, OECD, Opportunity Insights, RAND, SED, SOLE, the Upjohn Institute, VMACS, and Yale for suggestions, and Peter Robertson and Katie Zhang for excellent research assistance. This research was made possible by a data-use agreement between three of the authors and the JPMorgan Chase Institute (JPMCI), which has created de-identified data assets that are selectively available to be used for academic research. All statistics from JPMCI data, including medians, reflect cells with multiple observations. The opinions expressed are those of the authors alone and do not represent the views of JPMorgan Chase & Co.

1 Introduction

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

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

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

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

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

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

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

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

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

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

2 Data and Policy Environment

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

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

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

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

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

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

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

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

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

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

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

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

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

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

3 Descriptive Patterns

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

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

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

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

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

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

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

4 Disincentive Effect of Benefit Supplements

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

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

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

We define three estimands of interest:

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

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

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

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

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

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

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

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

4.1 Interrupted time-series analysis

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

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

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

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

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

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

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

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

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

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

4.2 Dose-response difference-in-difference analysis

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

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

4.2.1 Graphical Evidence

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

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

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

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

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

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

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

4.2.2 Identification and Estimation

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

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

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

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

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

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

We also estimate a version of equation 5 by week:

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

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

4.3 Comparison of micro and macro estimates

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

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

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

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

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

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

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

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

4.4 Robustness of main estimates

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

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

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

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

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

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

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

4.5 Effect of supplements in April 2021

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

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

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

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

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

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

5 Model

5.1 Motivation and Setup

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

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

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

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

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

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

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

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

5.2 Model Results

5.2.1 Expectations and Dynamics

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

6 Interpreting Magnitudes

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

7 Conclusion

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

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

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

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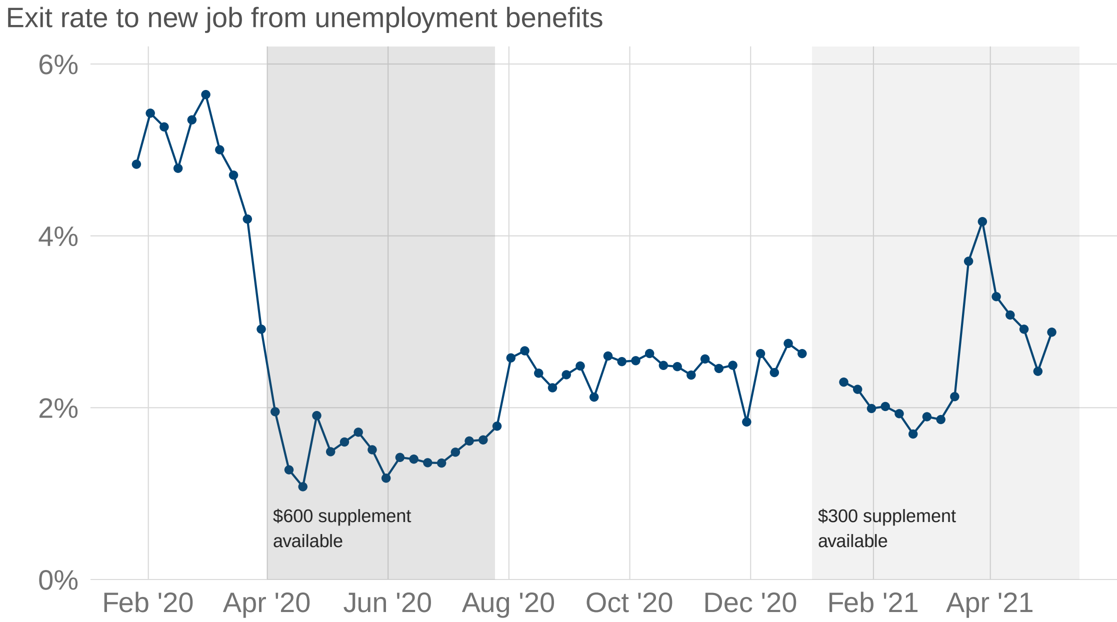
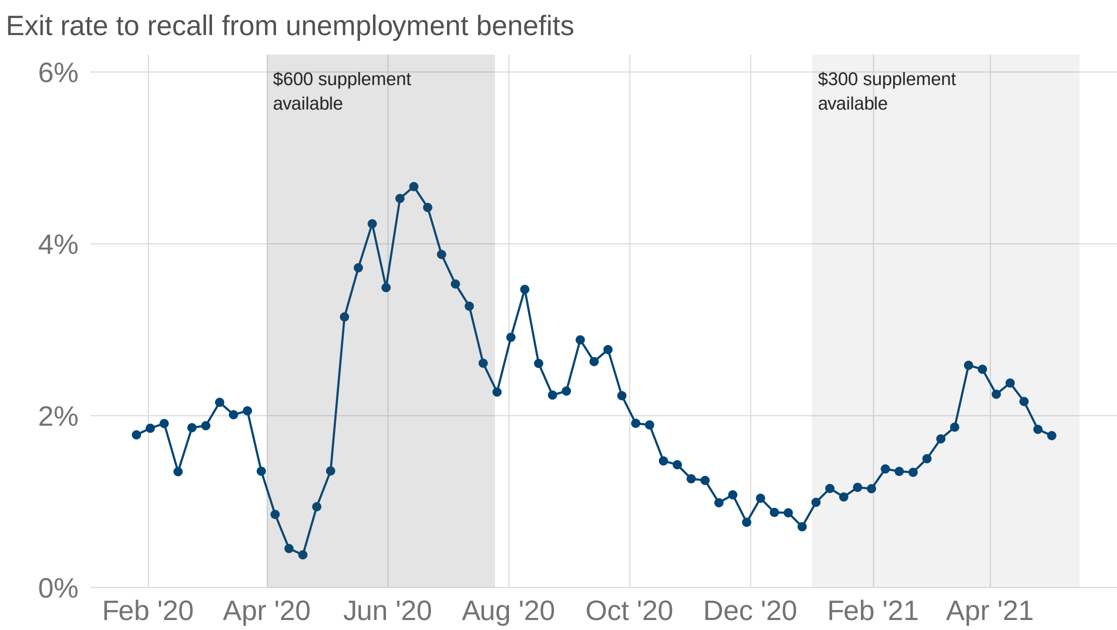
Figure 1: Exit Rate from Unemployment Benefits

(a) New Job



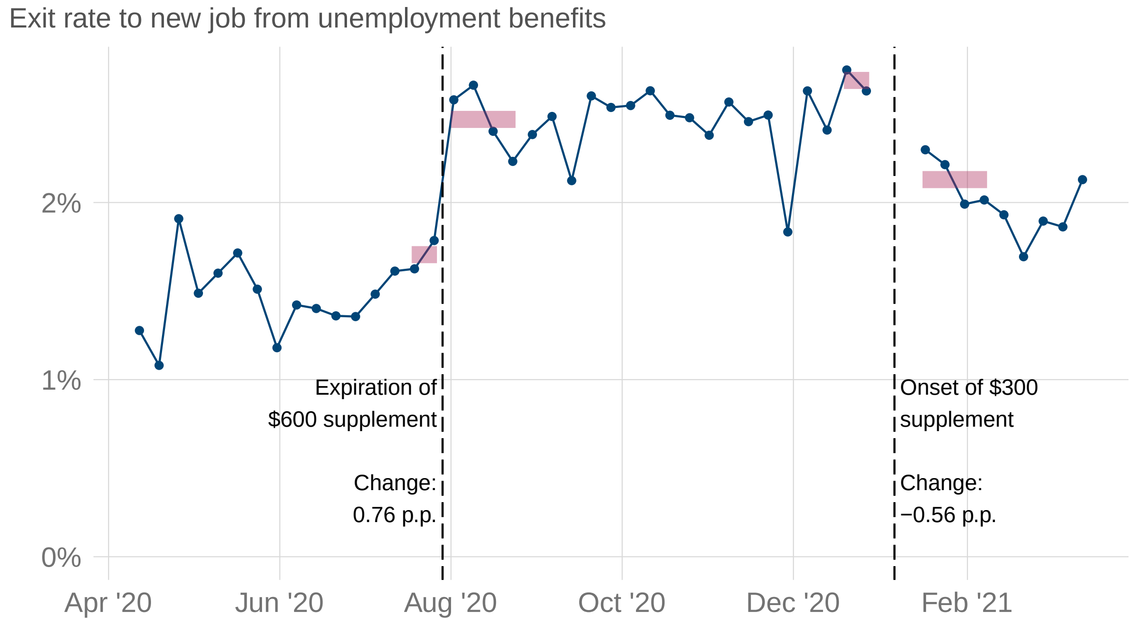
(b) Recall



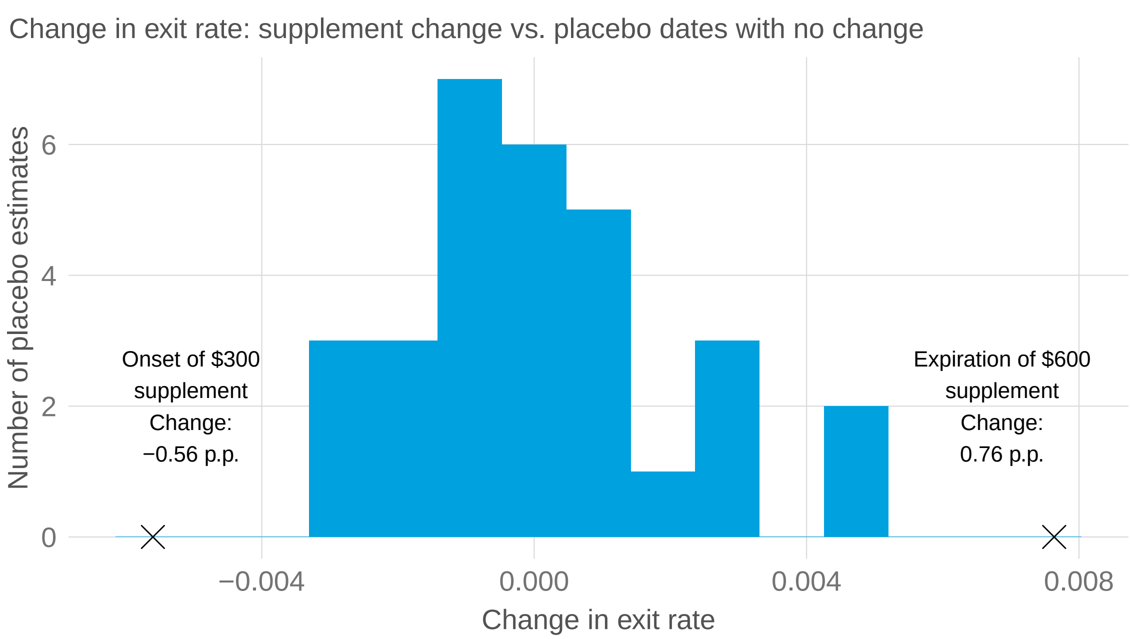
Notes: This figure shows the exit rate to new jobs and to recall in the JPMCI data from February 2020 to May 2021. UI exit is defined as three contiguous weeks without receipt of UI benefits. Recall is measured using receipt of labor income from a prior employer. New job is defined as a UI exit without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

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

(a) Interrupted Timeseries Estimate



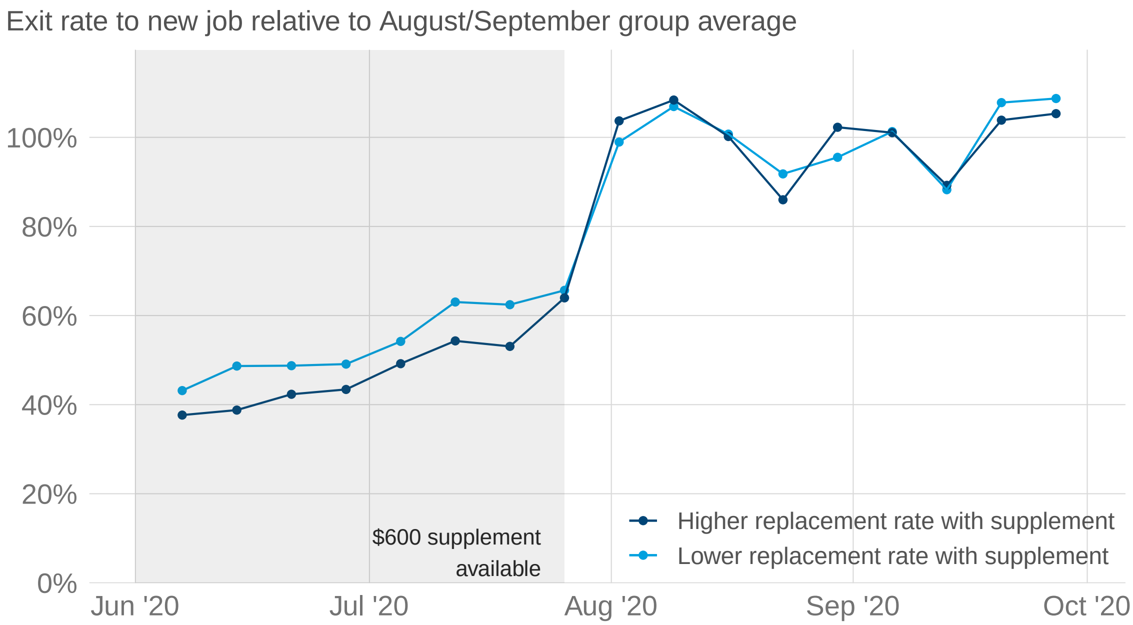
(b) Distribution of Placebo Estimates



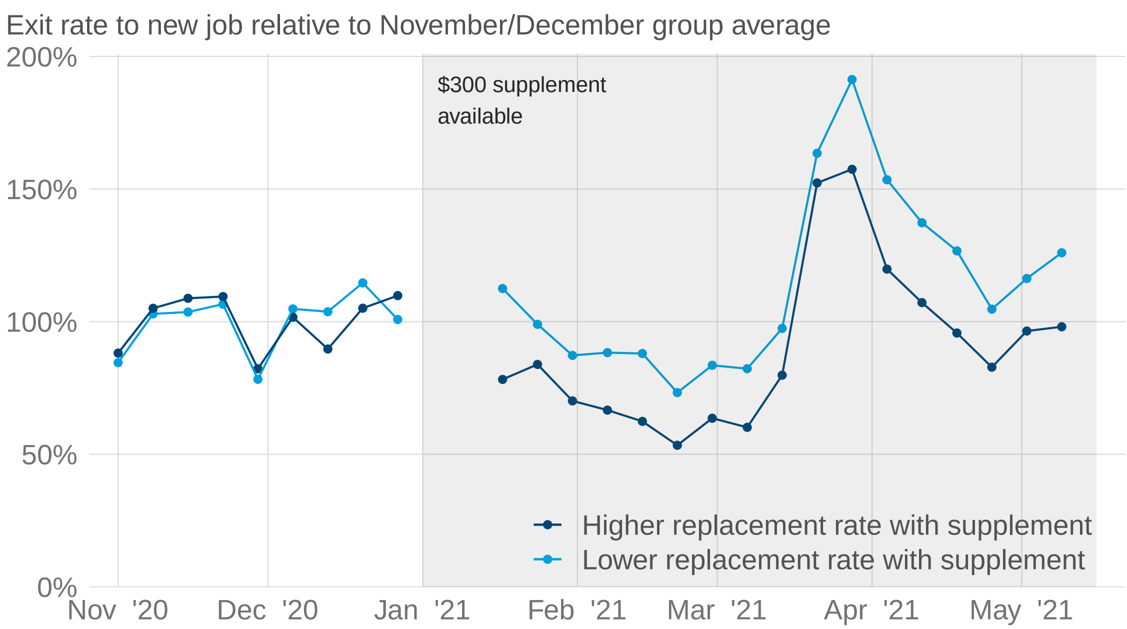
Notes: The top panel of this figure shows the exit rate to new job in the JPMCI data from April 2020 through February 2021. The red horizontal bars indicate the average exit rate in the two weeks prior to and four weeks following a change in the supplement amount. We form a test statistic for the impact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

Figure 3: Effect of Expanded Benefits: Event Study

(a) Expiration of $600



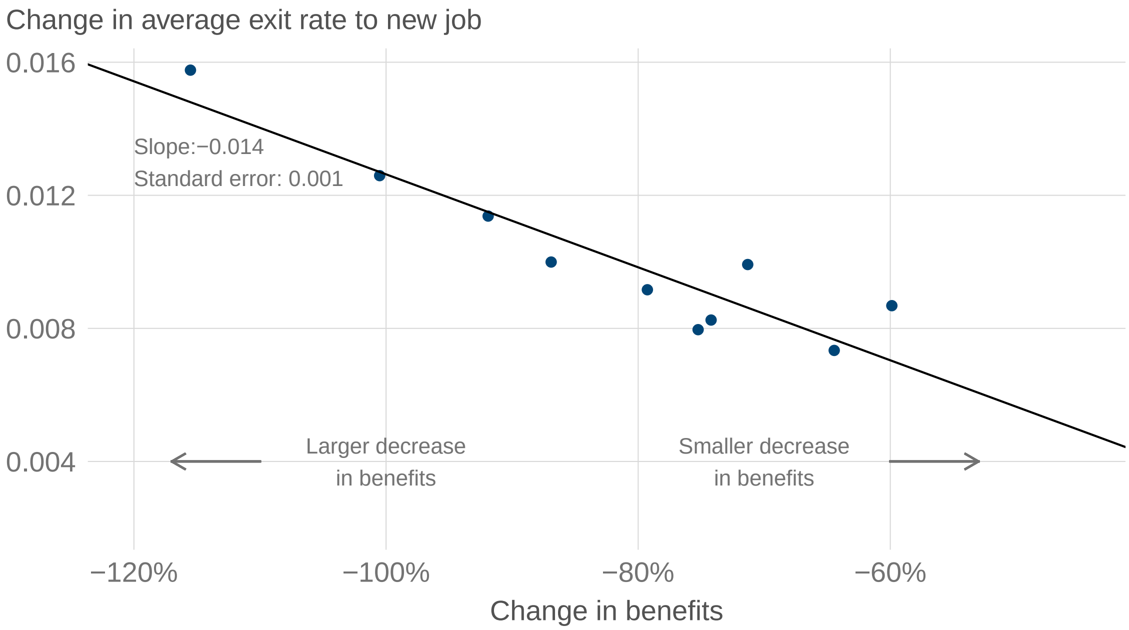
(b) Onset of $300



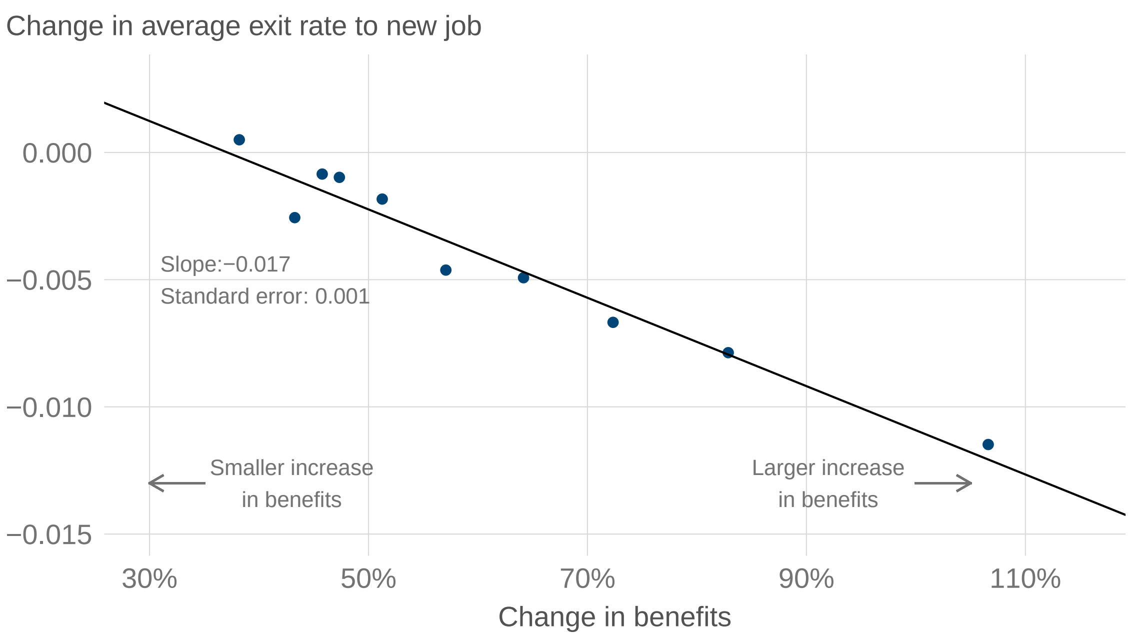
Notes: This figure shows the exit rate to new job around the expiration of the $600 weekly supplement and the onset of the $300 weekly supplement in the JPMCI data. The light blue line shows workers with a lower-than-median replacement rate with the supplement and the dark blue line shows workers with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

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

(a) Expiration of \$600



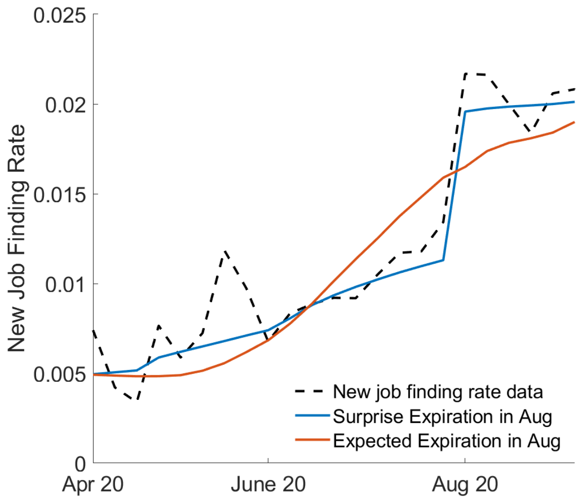
(b) Onset of \$300



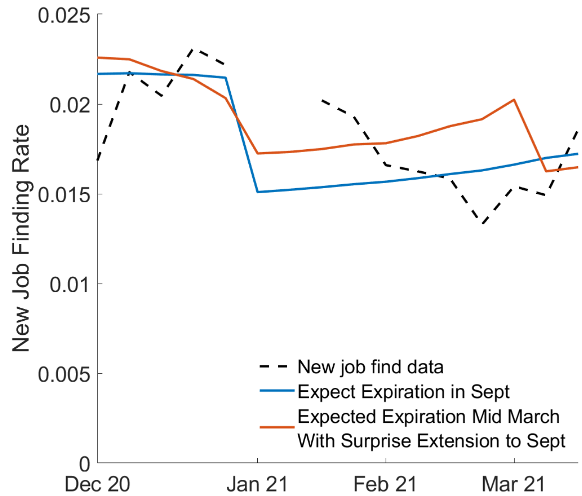
Notes: This figure shows the change in the new job-finding rate at the expiration and onset of benefit supplements separately for deciles of the change in benefits as measured using equation 4. The top panel shows the difference in the average new job-finding rate between Jun 1-Jul 31 and Aug 1-Sep 31.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

Figure 5: Model: Role of Expectations

(a) Expiration of $600



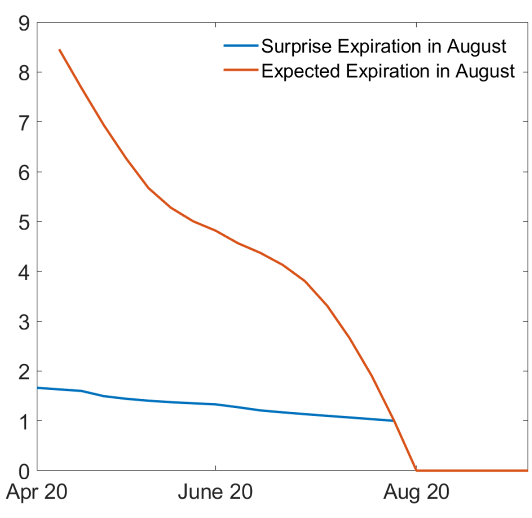
(b) Onset of $300



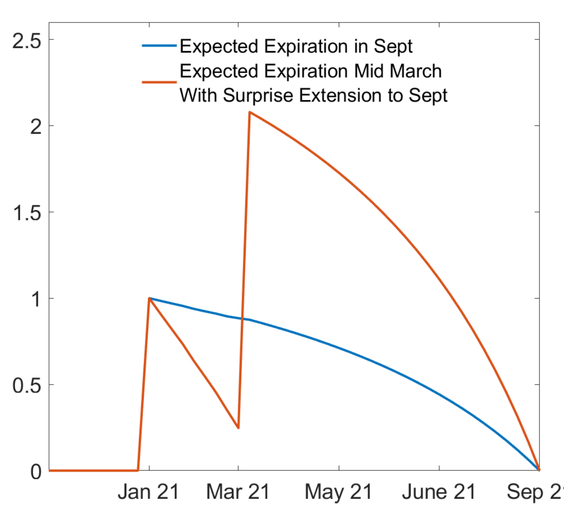
Notes: This figure compares model new job finding rates under different expectations about supplement duration to the times-series of the new job finding rate in the data. The panel simulating the expiration of the $600 compares a model where the realized expiration of supplements in August was a surprise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

Figure 6: Dynamic Effects of Supplements Under Different Expectations

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



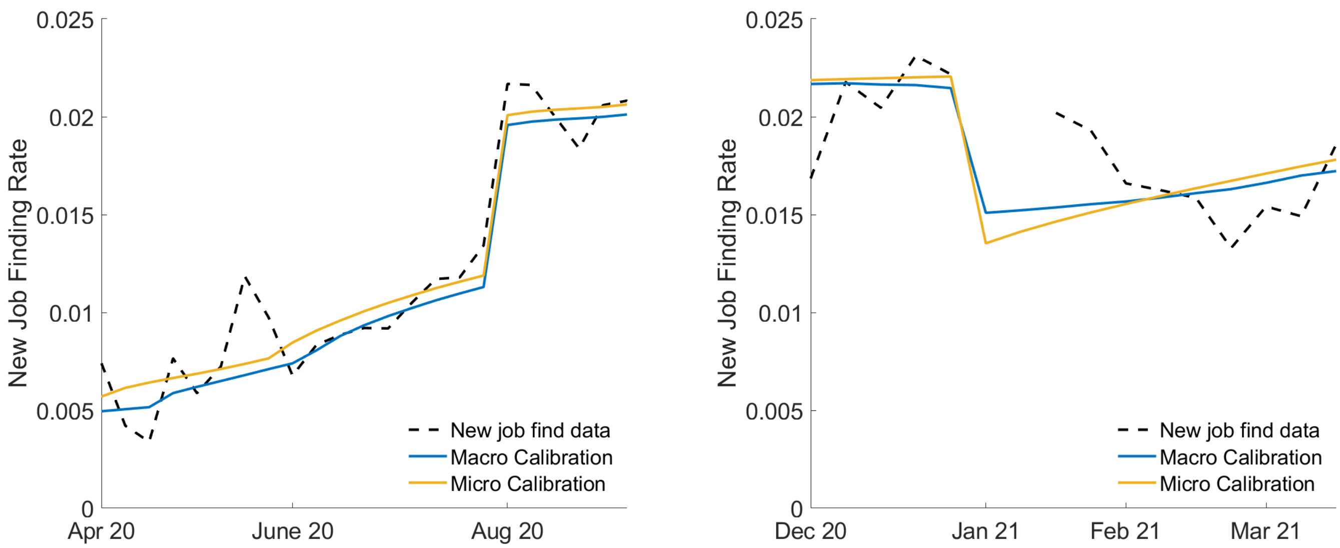
(b) Job Finding Effect of $300 Supplement Each Week Relative to Effect in Week of Onset



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

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

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



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

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

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

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

Table 2: Disincentive Magnitudes

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

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

Table 3: Pre-Pandemic Comparison

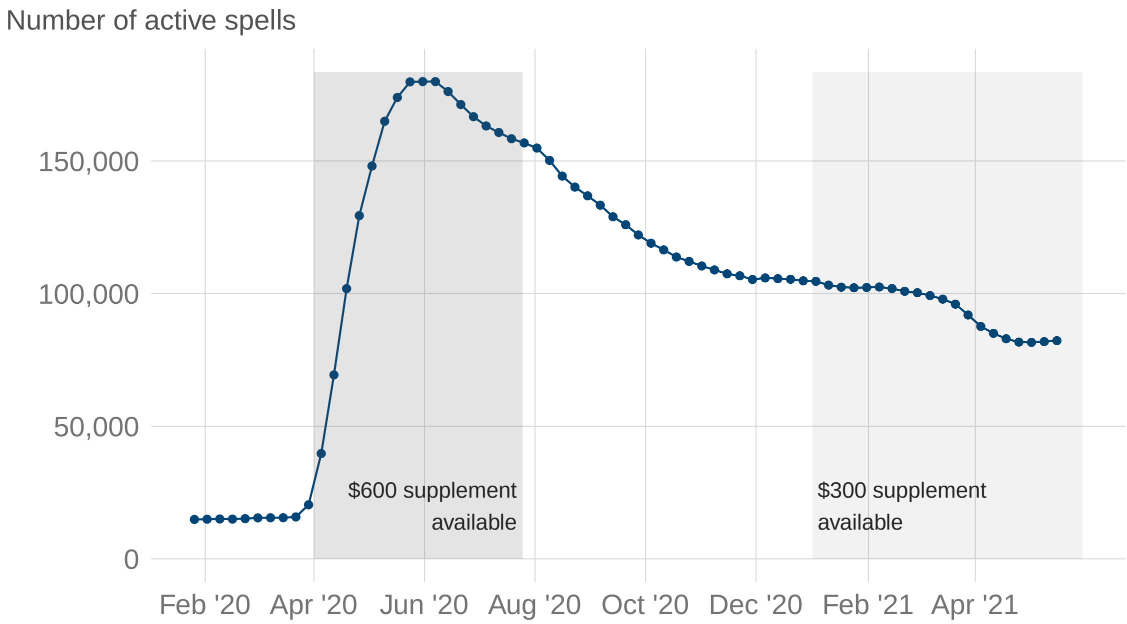
	Pre-Pandemic Comparison (1)	Macro Calibration (2)	Micro Calibration (3)
Duration elasticity to small 26 week supplement	0.47	0.05	0.04

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

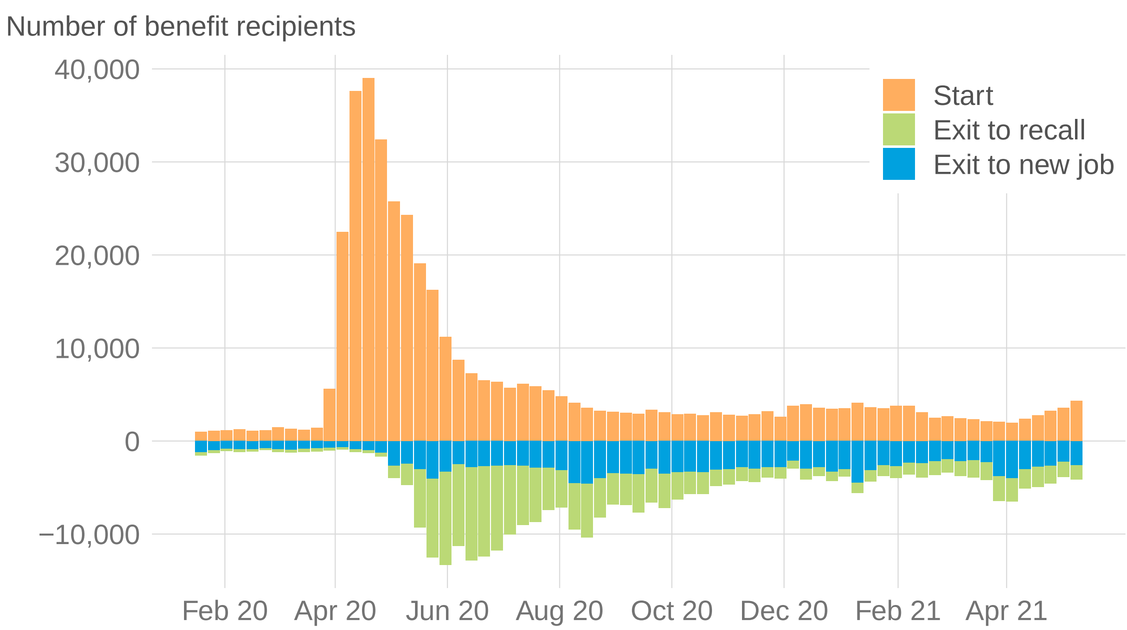
A Appendix

Figure A-1: Patterns of Unemployment Insurance Receipt

(a) Number of Recipients

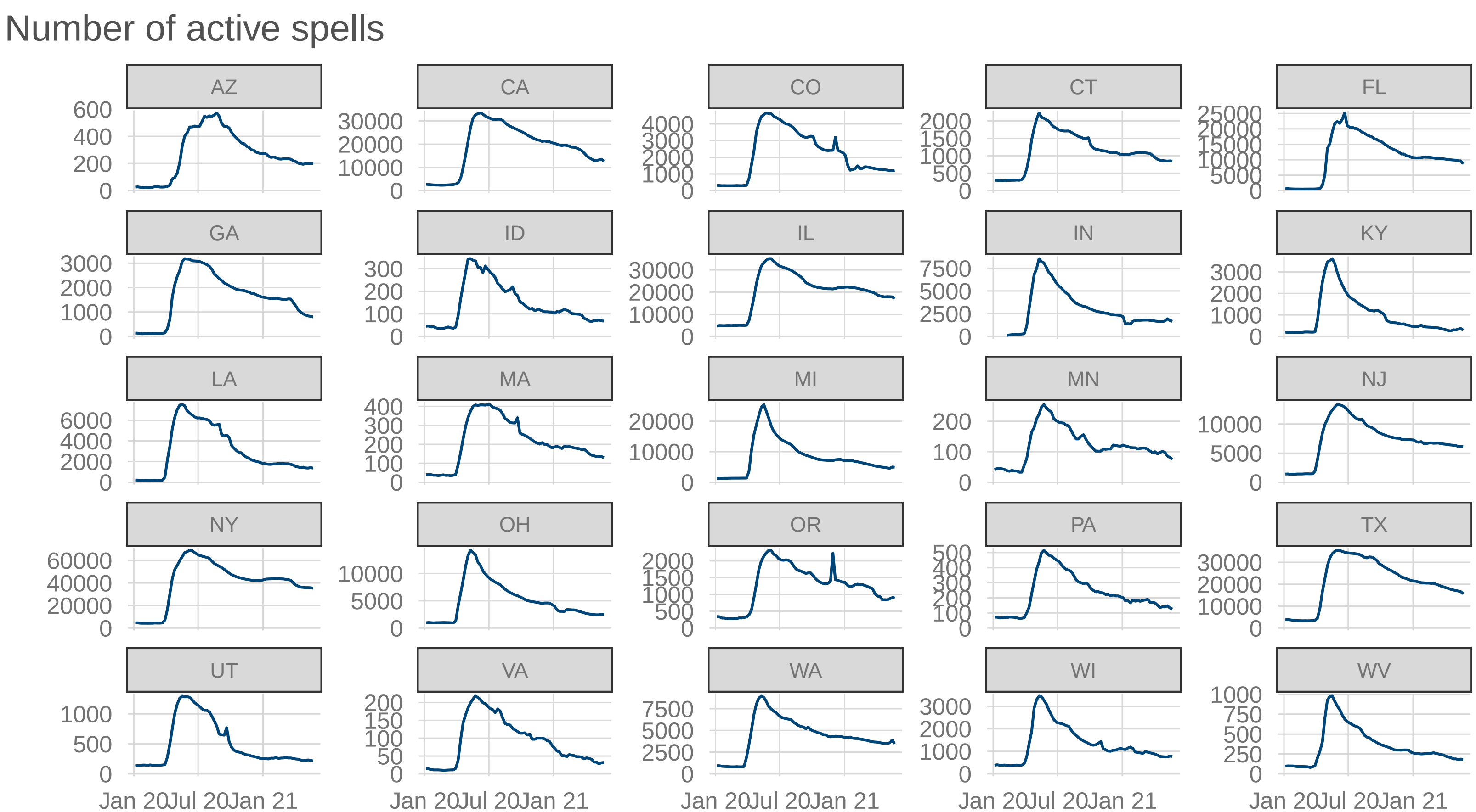


(b) Number of Starts and Exits



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

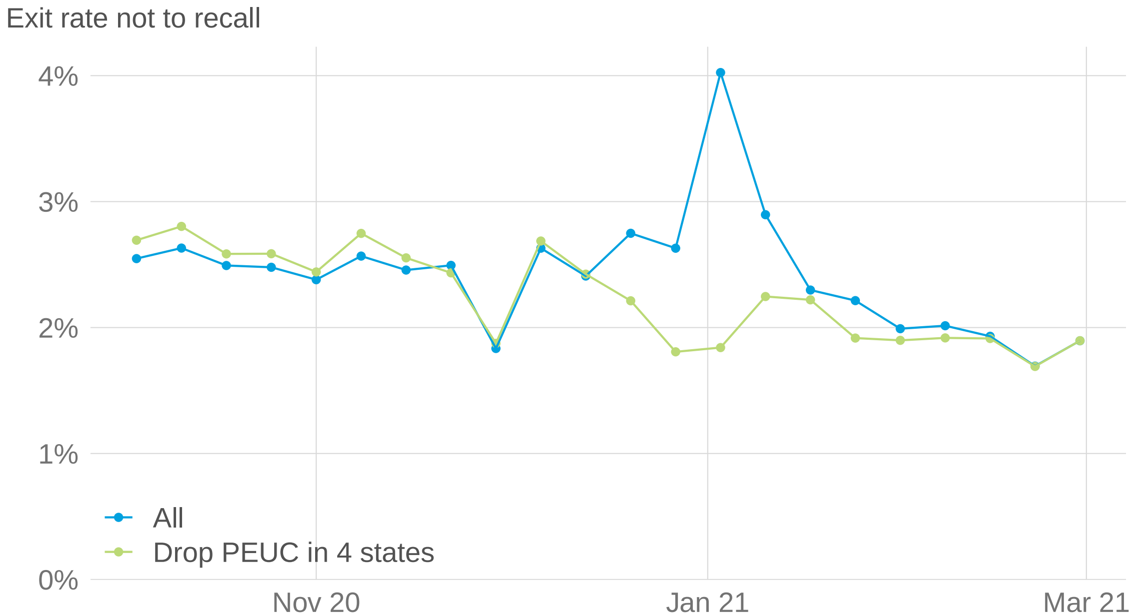
Figure A-2: Number of Active Spells by State



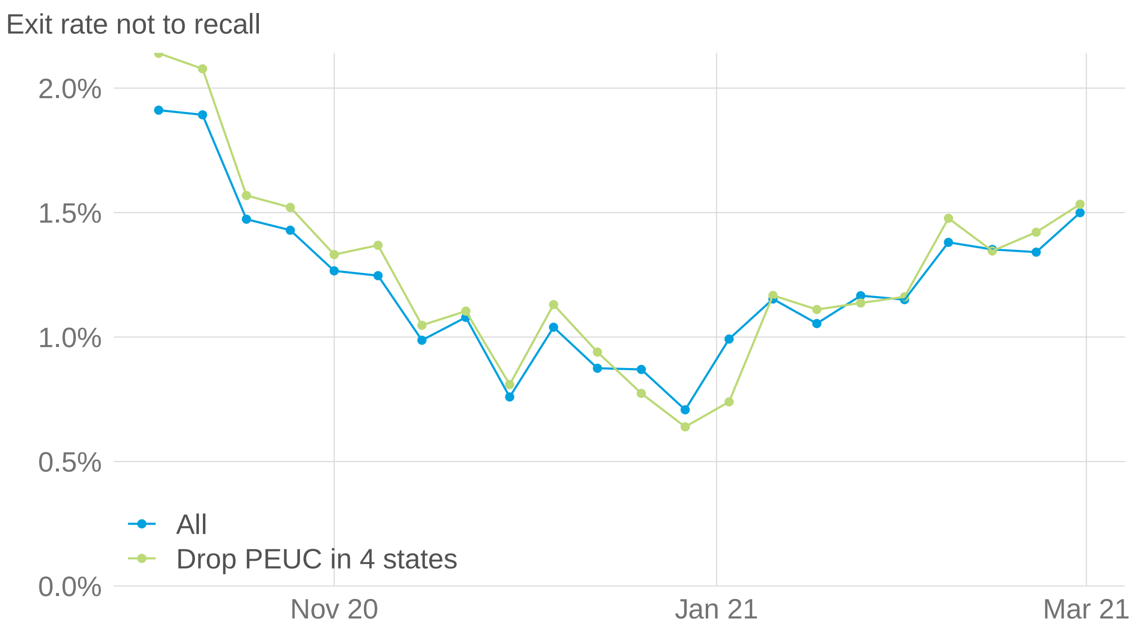
Notes: This figure shows the number of active spells in the JPMCI data.

Figure A-3: Exit Rate at Expiration of PEUC

(a) Not to Recall

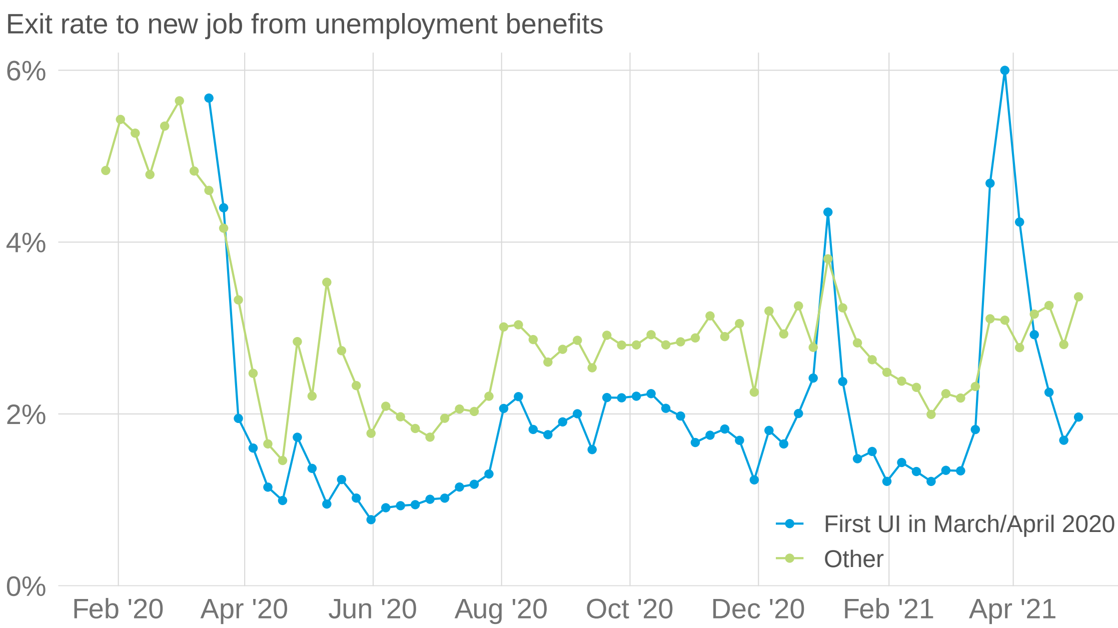


(b) To Recall



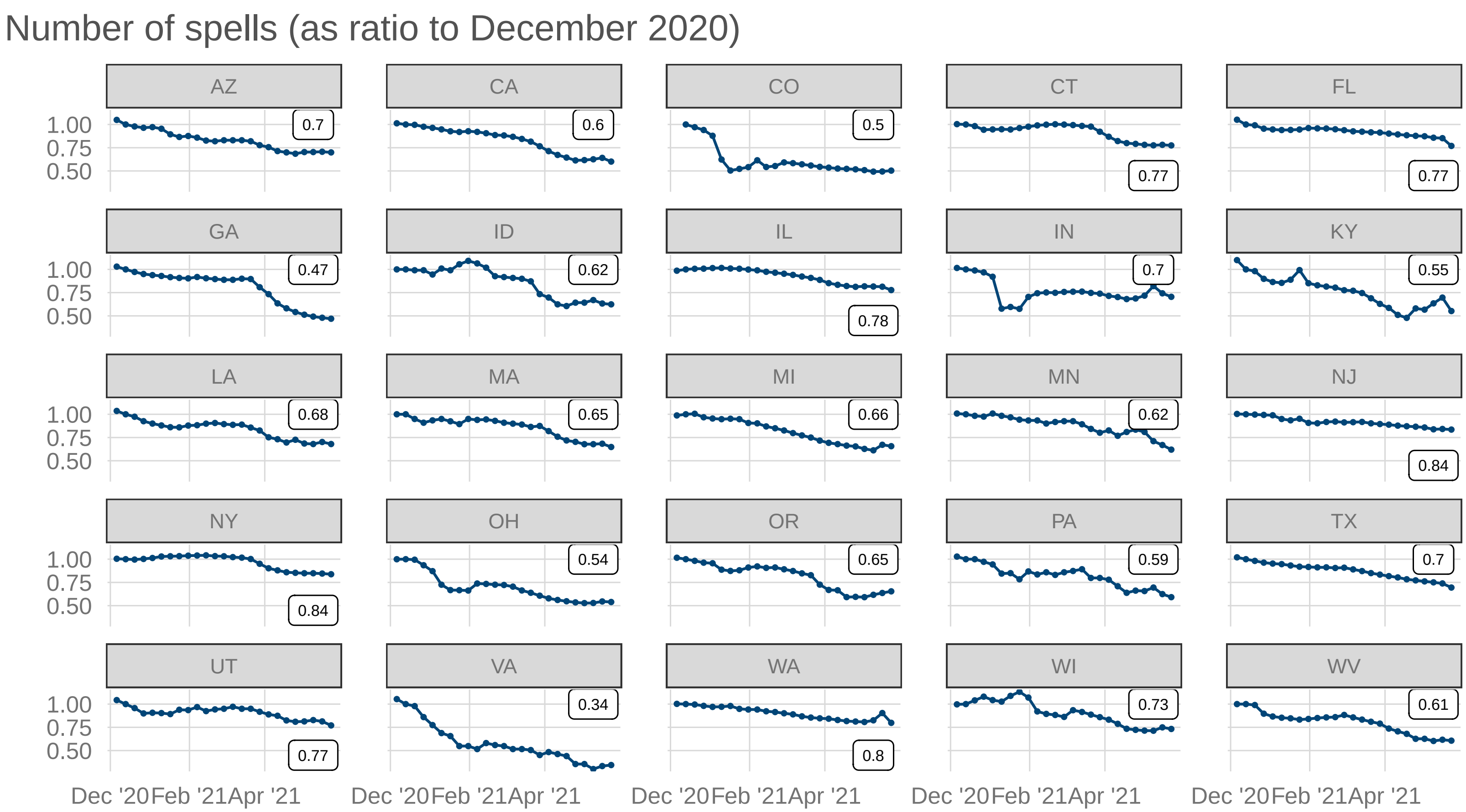
Notes: This figure shows the evolution of the exit rate from October 2020 through February 2021. The top panel shows exit not to recall and the bottom panel shows exit to recall. The blue series is the same as the one shown in Figures 1a and 1b, except that here the series includes January 3 and January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

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



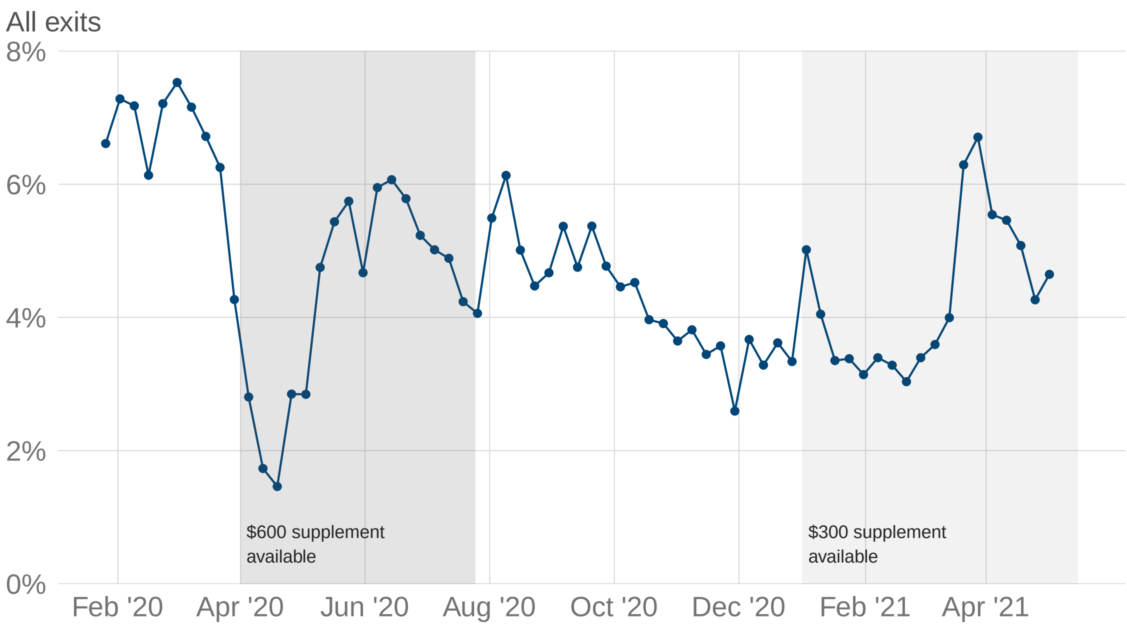
Notes: This figure plots non-recall exit rates separately by the month of initial UI receipt. We see a sharp jump in March and April 2021 for workers who started receiving benefits in March and April 2020 and who were therefore likely near the end of their benefit year. Because the JPMCI data also shows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

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



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

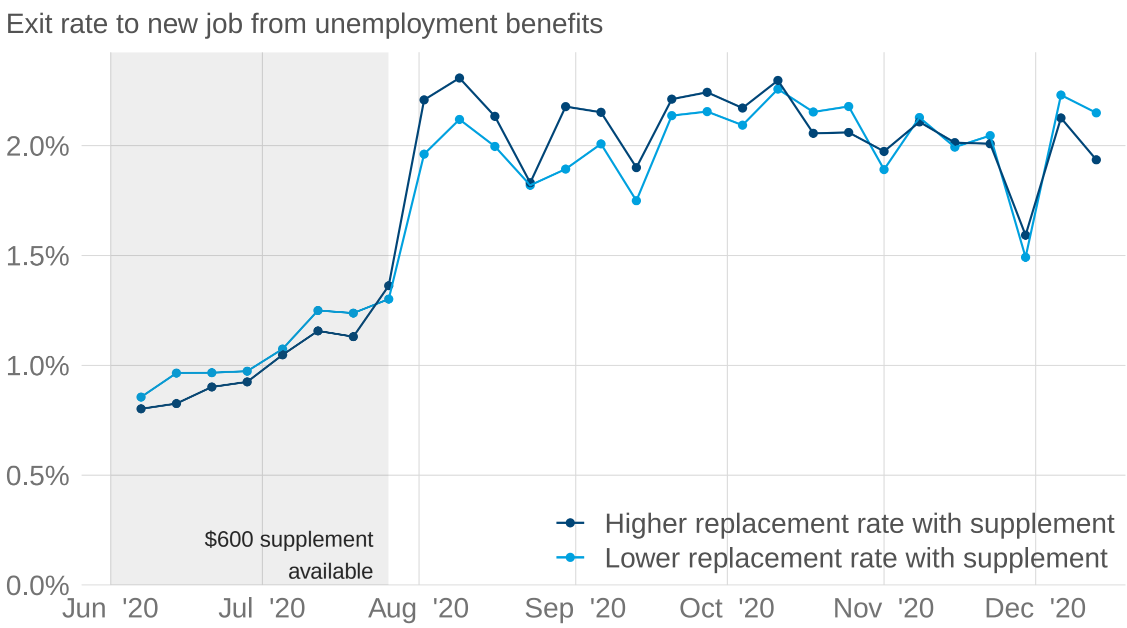
Figure A-6: Total Exit Rate from Unemployment Benefits



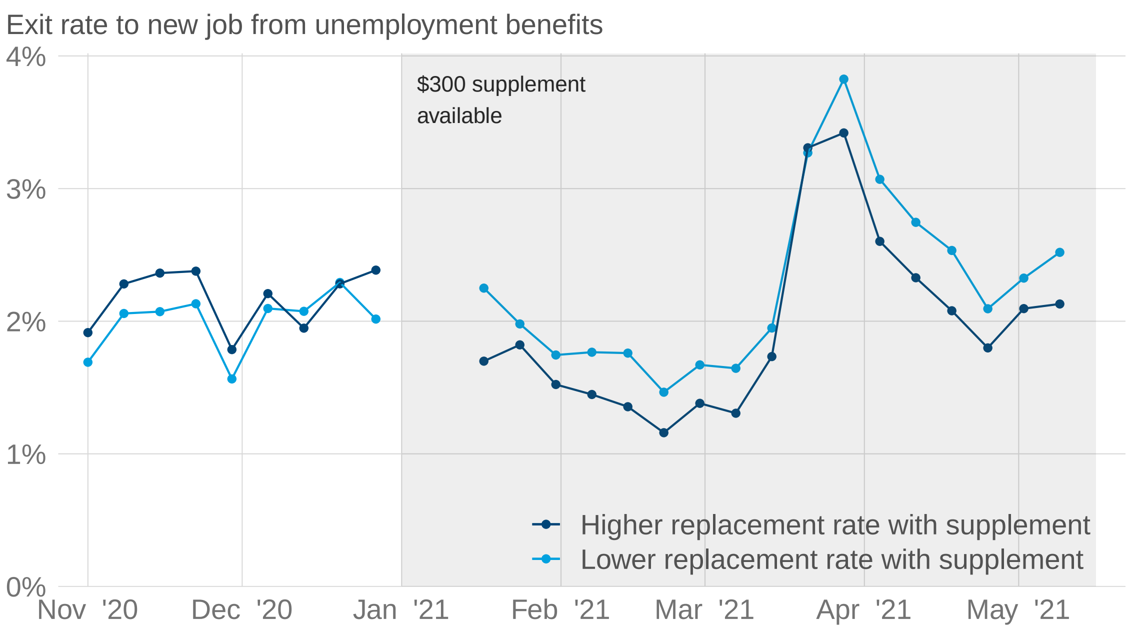
Notes: This figure shows the total exit rate from unemployment benefits, summing over exits not to recall shown in Figure 1a and exits to recall shown in Figure 1b.

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

(a) Expiration



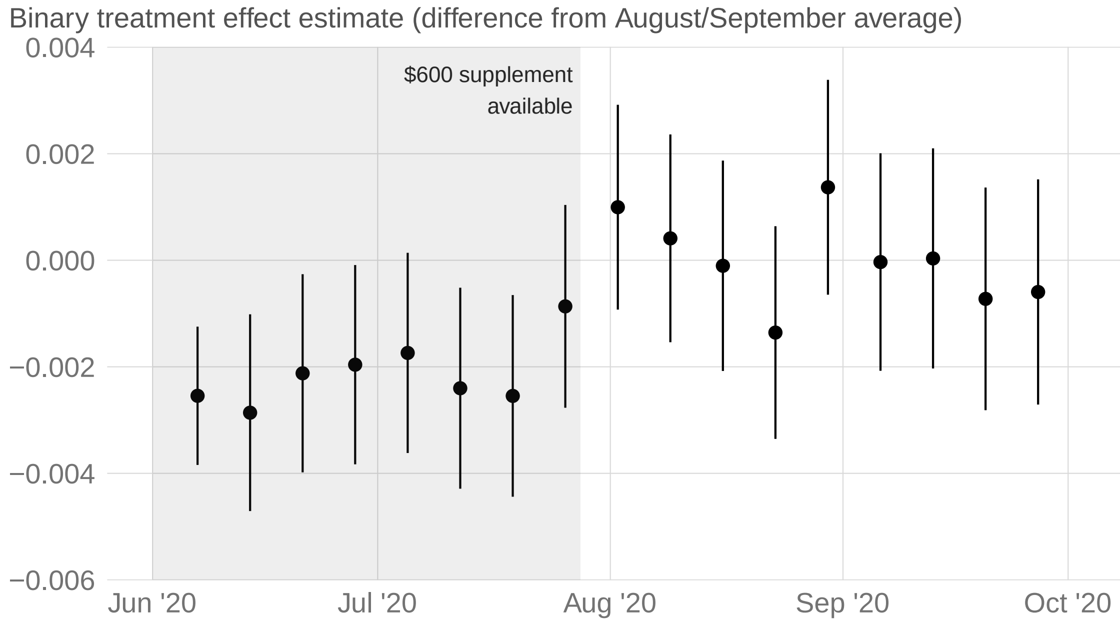
(b) Onset



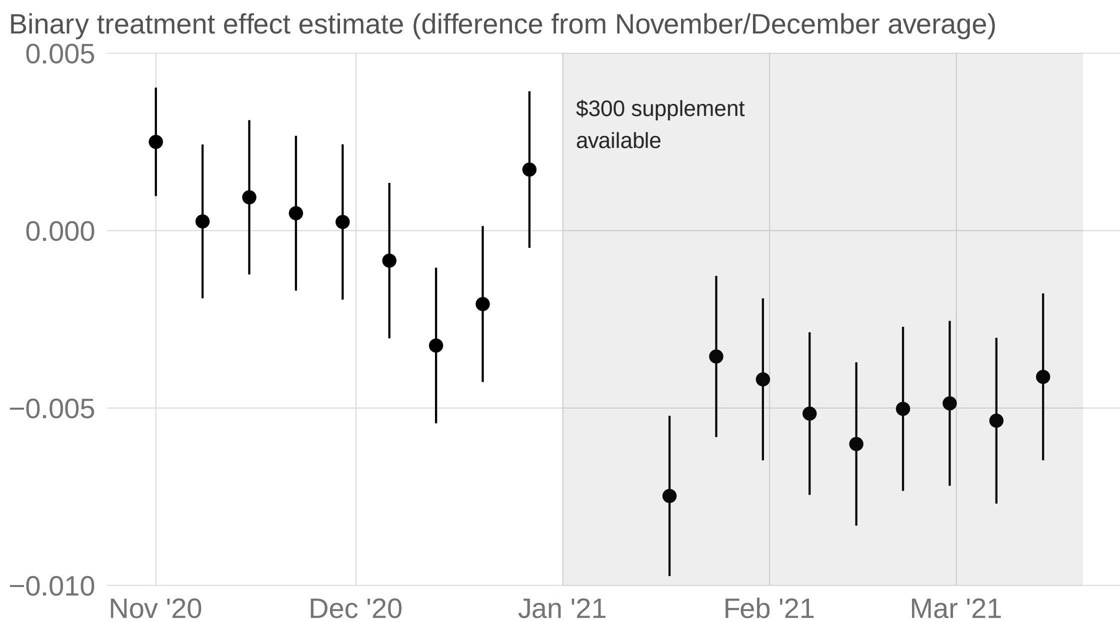
Notes: This figure shows the exit rate to new jobs around the expiration of the $600 weekly supplement and the onset of the $300 weekly supplement in the JPMCI data. The light blue line shows workers with a lower-than-median replacement rate with the supplement and the dark blue line shows workers with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

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

(a) Expiration of $600



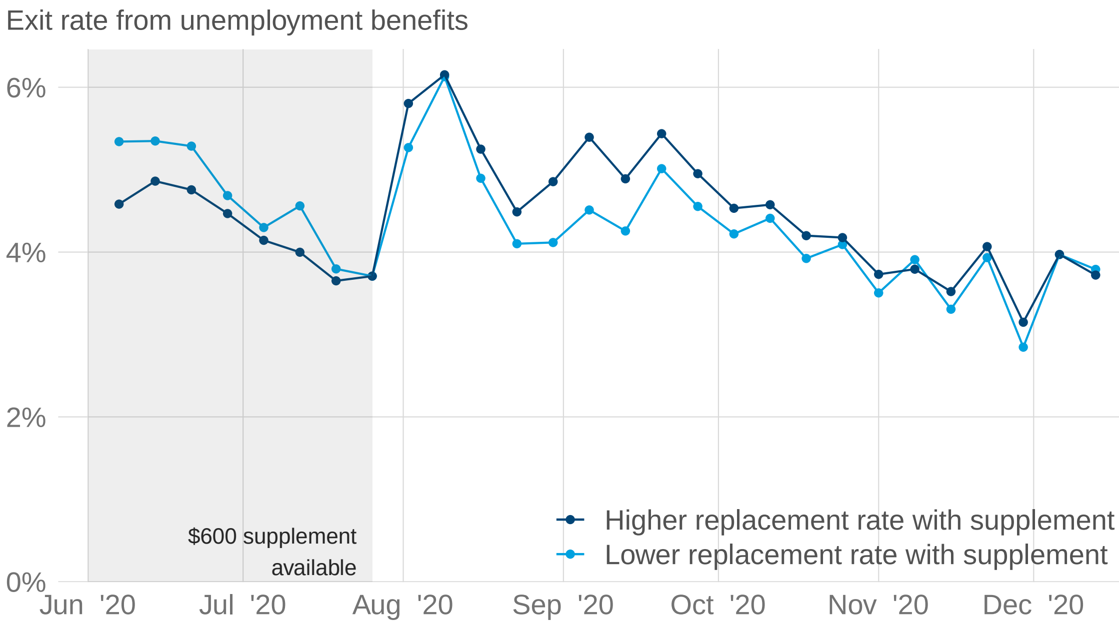
(b) Onset of $300



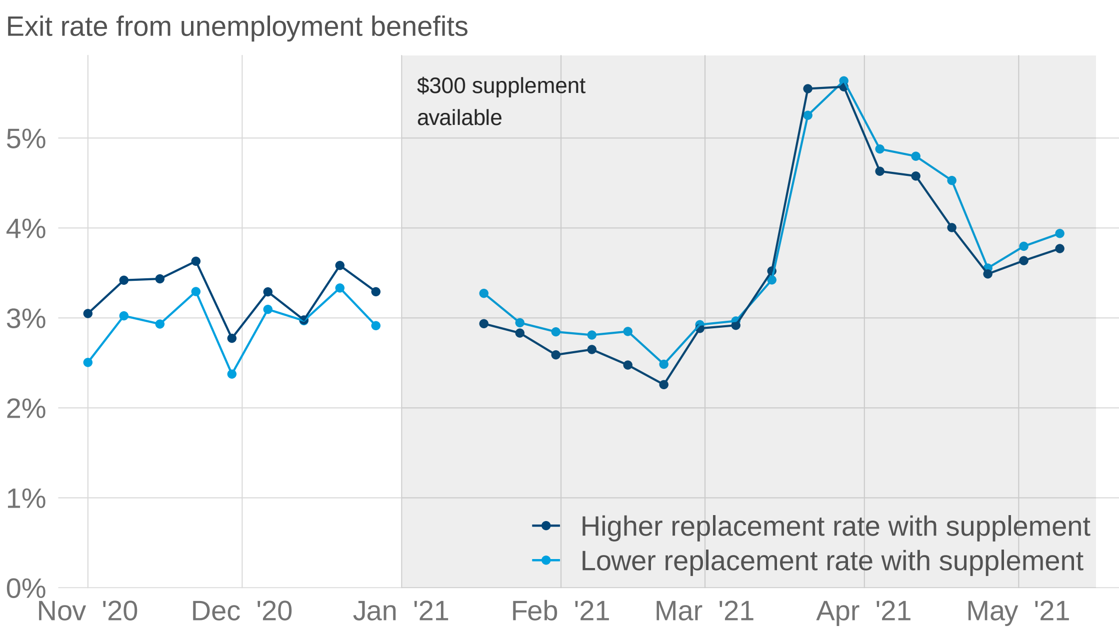
Notes: This figure reports estimates of $\hat{\beta}_t$ from $e_{it} = \gamma 1(PctChange_i > Median) + \alpha Week_t + \beta_t Week_t \times 1(PctChange_i > Median) + \varepsilon_{it}$. All coefficients are reported as differences to the average value of $\hat{\beta}_t$ in the no-supplement period.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

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

(a) Expiration of $600



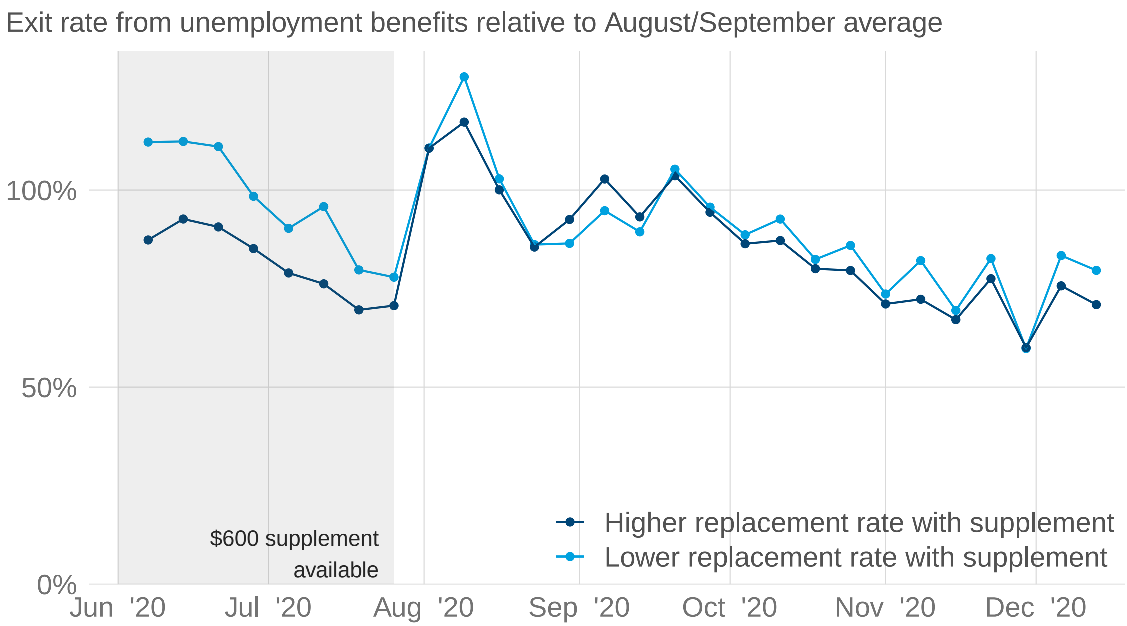
(b) Onset of $300



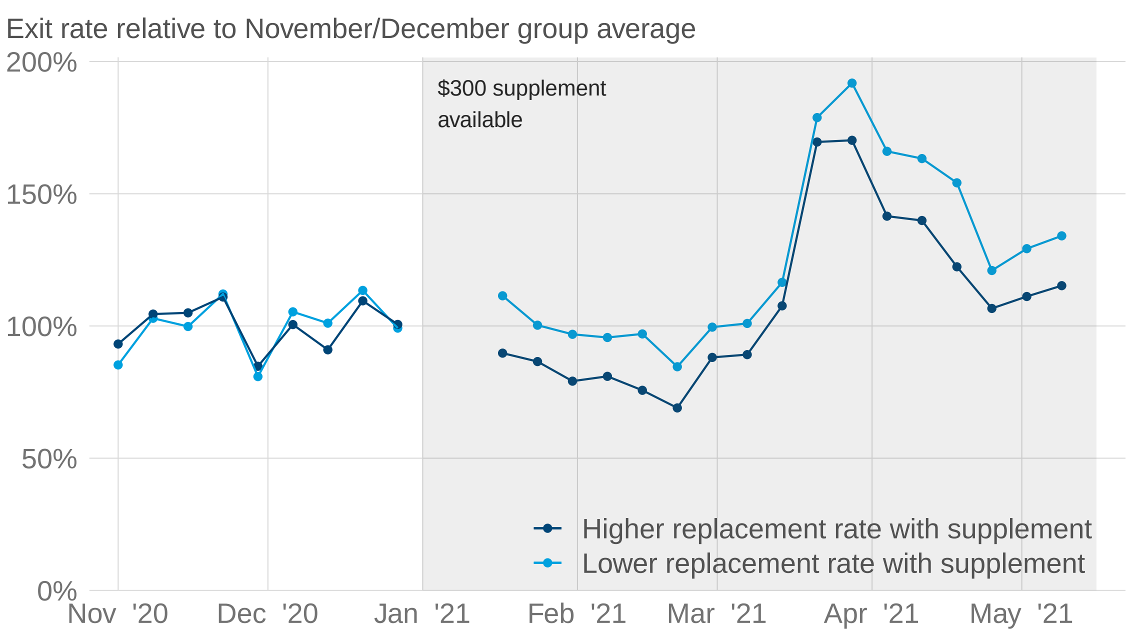
Notes: This figure shows the total exit rate (to recalls and new jobs) around the expiration of the $600 weekly supplement and the onset of the $300 weekly supplement in the JPMCI data. The light blue line shows workers with a lower-than-median replacement rate with the supplement and the dark blue line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

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

(a) Expiration of $600



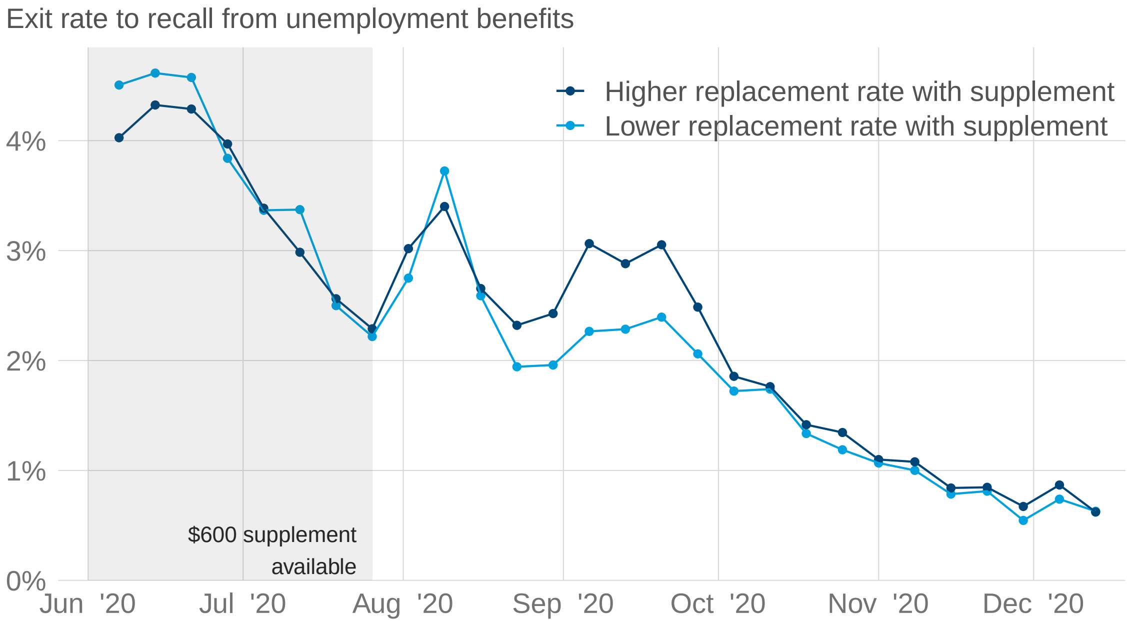
(b) Onset of $300



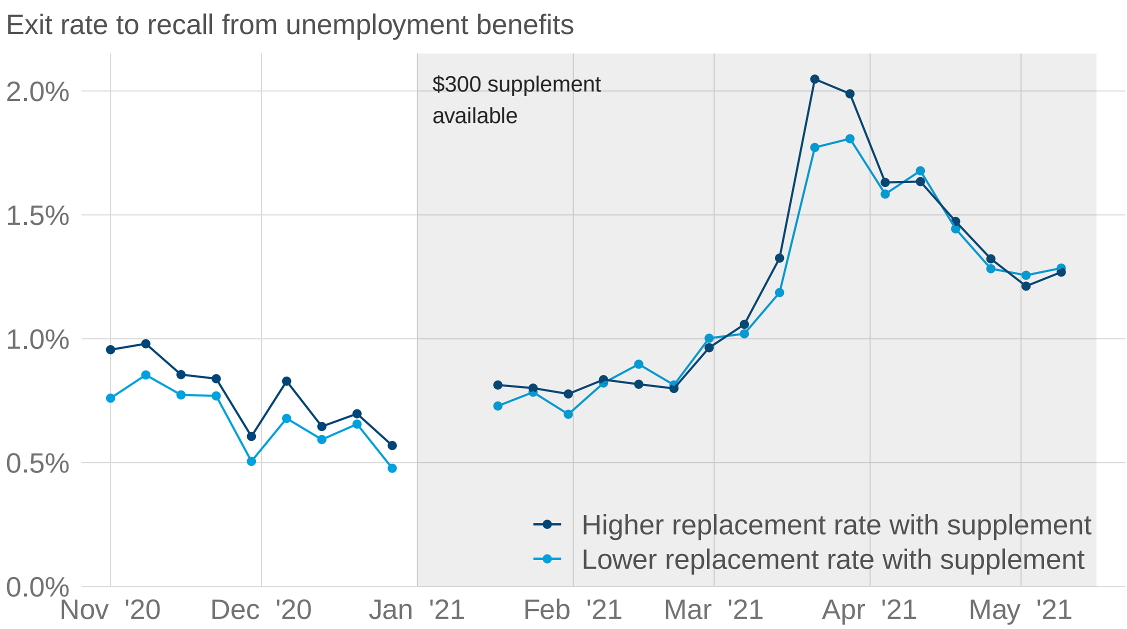
Notes: This figure shows the total exit rate (to recalls and new jobs) around the expiration of the $600 weekly supplement and the onset of the $300 weekly supplement in the JPMCI data. The light blue line shows workers with a lower-than-median replacement rate with the supplement and the dark blue line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

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

(a) Expiration



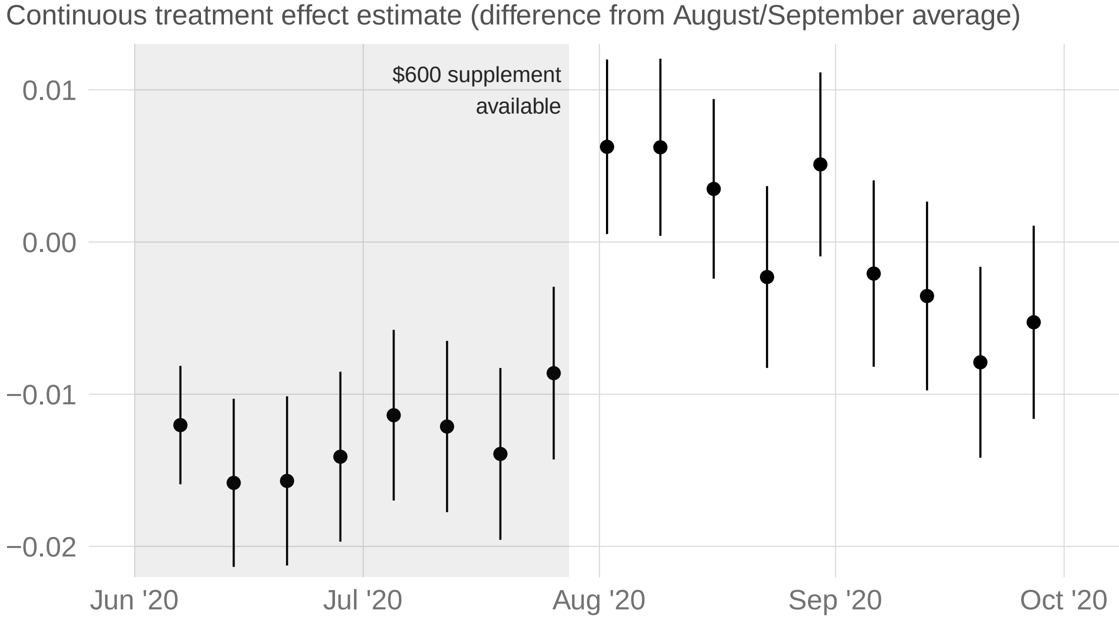
(b) Onset



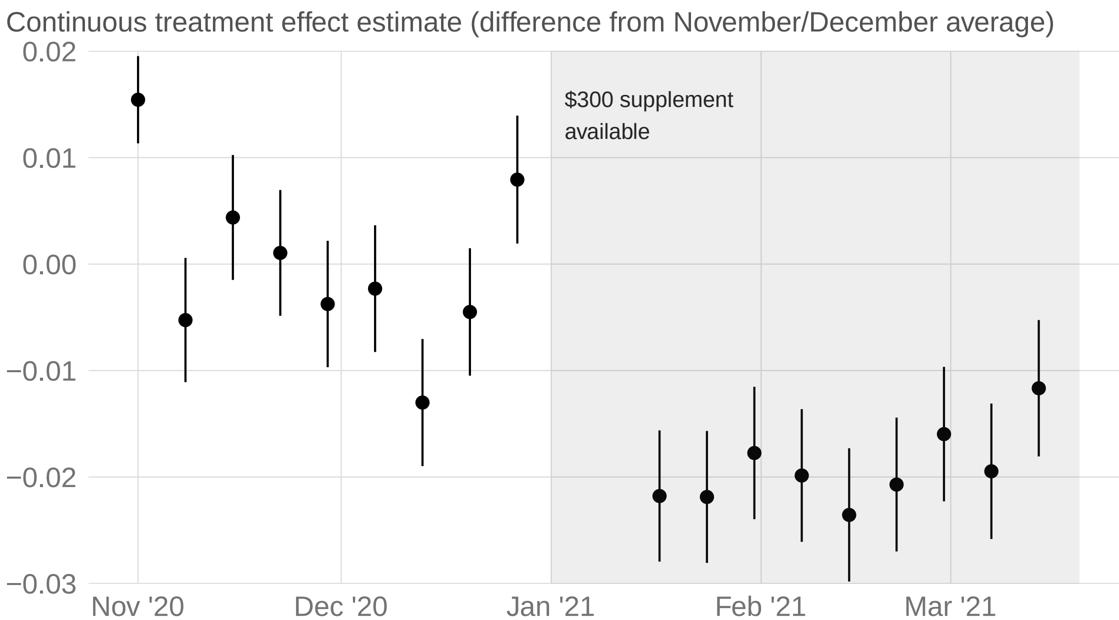
Notes: This figure shows the exit rate to recalls around the expiration of the $600 weekly supplement and the onset of the $300 weekly supplement in the JPMCI data. The light blue line shows workers with a lower-than-median replacement rate with the supplement and the dark blue line shows workers with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

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

(a) Expiration of $600



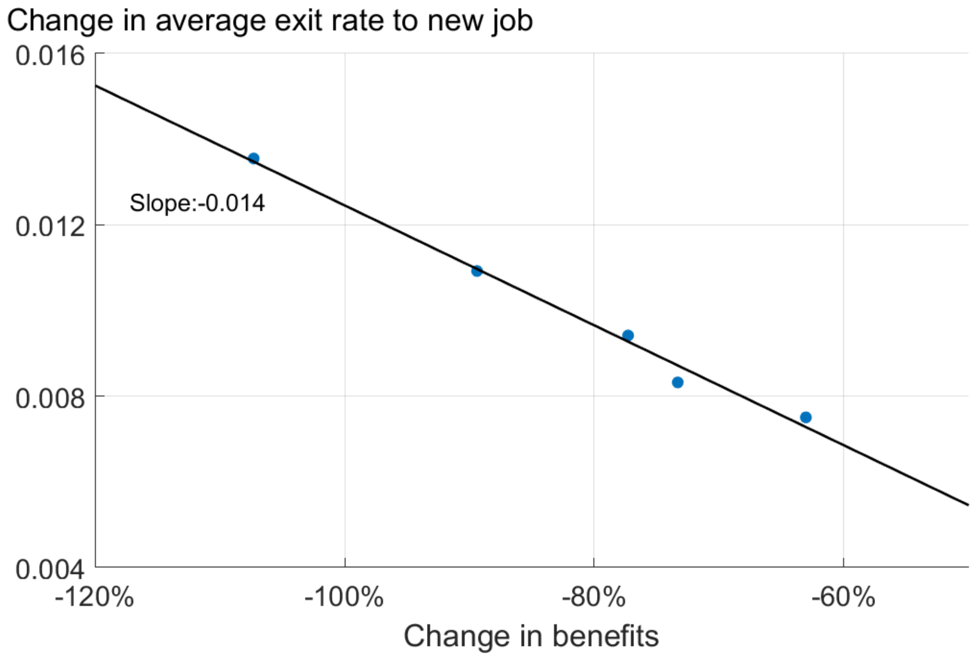
(b) Onset of $300



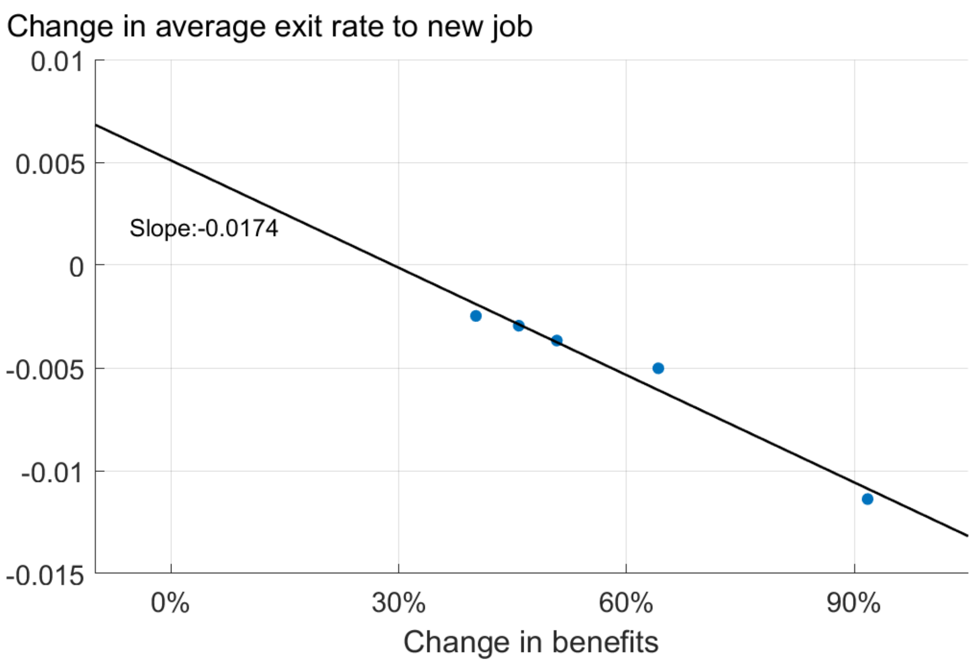
Notes: This figure reports estimates of $\hat{\beta}_t$ from equation (6). Coefficients on the plot are reported as differences to the average value of $\hat{\beta}_t$ in the no-supplement period. Vertical lines are 95% confidence intervals.

Figure A-13: Model Cross-Sectional Relationships

(a) Expiration of $600



(b) Onset of $300



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

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

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

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

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

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

(a) Onset of $300

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

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

(b) Expiration of $600

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

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

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

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

(a) Expiration of $600

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

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

(b) Onset of $300

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

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

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

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

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

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

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

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

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

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