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

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Abstract

We show that the largest increase in unemployment benefits in U.S. history had large spending impacts and small job-finding impacts. This finding has three implications. First, increased benefits were important for explaining aggregate spending dynamics—but not employment dynamics—during the pandemic. Second, benefit expansions allow us to study the MPC of normally low-liquidity households in a high-liquidity state. These households *still* have high MPCs. This suggests a role for permanent behavioral characteristics, rather than just current liquidity, in driving spending behavior. Third, the mechanisms driving our results imply that temporary benefit supplements are a promising countercyclical tool.

JEL codes: D14, E21, E24, E62, E70, J64, J65

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1 Introduction

This paper analyzes the spending and labor market impacts of the largest increase in unemployment insurance (UI) benefits in U.S. history. The government added supplements of $300-$600 per week on top of regular UI benefits at various points during the pandemic as part of its policy response. These supplements more than doubled typical benefit levels, leading most unemployed workers to receive *more* income from unemployment than they had from their prior jobs (Ganong, Noel, and Vavra 2020). In total, nearly half a trillion dollars in supplements were paid out through this program. We combine administrative bank account data, several causal research designs, and a dynamic structural model to estimate and interpret the effects of this unprecedented increase in benefits. Overall, we find that this increase had large effects on spending but small effects on job-finding.

Analyzing this massive increase in benefits is useful for three reasons. First, this program was large enough to have major impacts on aggregate economic activity and cross-household inequality. Second, the scale and persistence of these transfers provides a unique laboratory for testing implications of heterogeneous agent macro models. The supplements were big enough to push unemployed households from what is usually a low-liquidity state into a high-liquidity state. This setting allows us to directly assess the common and quantitatively important assumption that high marginal propensities to consume (MPCs) arise from temporarily low liquidity. Third, countercyclical benefit levels have never before been attempted at this scale, and there is no prior evidence about their impact. Understanding their effects can inform future policy design.

Measuring the impact of expanded benefits requires a dataset with information on spending, liquidity, employment transitions, and unemployment benefit receipt. We build such a dataset by using de-identified bank account transactions from the universe of Chase customers. We observe the precise week that millions of individual households begin receiving UI supplements and trace out the high-frequency impact of these supplements on spending, job finding, and the evolution of liquid balances.

We first analyze spending impacts. Beginning with time-series patterns, we find a strong relationship between supplement levels and the spending of the unemployed. Spending rises when $600 supplements begin in April 2020, falls when they end in July 2020, rises when $300 supplements begin in January 2021, and falls again when they end in the summer of 2021.

Most strikingly, we see that while the $600 supplement is available, the spending of unemployed households *rises* after job loss, both in absolute terms and relative to the spending of employed households. The increase is a reversal of the usual decline in spending that occurs during unemployment. Moreover, this increase is particularly notable since employed households substantially reduced spending during the pandemic.

Next, we estimate MPCs out of benefits using two types of research designs. First, we use variation induced by processing constraints from overwhelmed state UI agencies at the onset of the pandemic. We compare unemployed workers who receive benefits immediately after job loss to those who lose jobs at the same time but face delays in benefit receipt. Second, to identify the effects of other supplement changes, we compare the spending of unemployed workers to that of employed workers matched on relevant observable characteristics.

Across all designs, spending responds sharply in the exact week in which benefit levels change. These spending responses are large across all specifications—with estimated one-month MPCs between 0.27 and 0.42—and statistically precise with standard errors of three cents or less. These findings are robust to a number of measurement and sample choices, are not driven by the unusually high recall rate during the pandemic, and are not driven by any unusual category-specific spending patterns of the unemployed.

This high MPC is particularly notable because the supplements are themselves so large that recipient households have elevated liquidity. Indeed, the median household who becomes unemployed during the pandemic moves *up* an entire tercile of the pre-pandemic liquidity distribution. Yet these households still have large spending responses to subsequent supplement changes. Thus, temporarily low current liquidity cannot be the main force driving these high MPCs.

Additional analysis of MPC heterogeneity within the unemployed further bolsters this conclusion. As an extreme example, consider a lottery jackpot winner. Whether they were short on cash before winning the lottery should have no impact on their post-lottery-winning MPC. This same logic applies to a policy-driven liquidity increase: it should erase any correlation between pre-lottery wealth and post-lottery-winning MPC. Yet we find that households with lower liquidity measured *years before the pandemic* have higher MPCs to UI supplements throughout the pandemic, suggesting that some permanent household characteristic drives both high MPCs and low liquidity.

We next examine the impact of supplements on job-finding. In contrast to the supplements' large effect on spending, we find that they had only small effects on job finding. Looking first at descriptive patterns, we see a dramatic decline in the job-finding rate at the start of the pandemic beginning a month before the start of the $600 supplements. We then see a small increase in the job-finding rate when the $600 supplements expire and a small decline in the job-finding rate when the $300 supplements begin.

To estimate the precise causal effects of the supplements on job finding, we use two research designs that exploit distinct sources of variation in benefits. First, we use an interrupted time-series design which relies on the overall expiration or onset of benefits. Second, we use a dose-response difference-in-difference design which compares workers with smaller and larger changes in benefits. Both research designs yield similar results. The implied duration elasticity of the $600 supplements is 0.06-0.11 while for the $300 supplements it is 0.10-0.22. These job-finding effects are small, both relative to overall fluctuations in job finding during the pandemic and relative to pre-pandemic estimates of the effects of benefits on unemployment duration.

In the second part of the paper we interpret these empirical results through the lens of an intentionally standard dynamic structural model. We discipline key features of the model using the causal estimates from our empirical analysis.

The model is crucial for three reasons. First, the model helps us translate the high-frequency reduced-form effects around policy changes we measure into the total dynamic effects of the policy that we are interested in. For example, if households search harder as the expiration of the $600 supplement approaches, then the change in job finding at expiration would understate the supplement's effect in earlier months. Second, it helps us interpret the magnitudes of our empirical estimates. For example, using our model, we can compare the MPC that we estimate out of targeted UI supplements to the much larger literature on the MPC out of untargeted stimulus checks, after accounting for the differences between these two policies. Third, the model enables us to quantify the role of specific mechanisms driving the spending and job-finding effects. Understanding mechanisms is necessary both for explaining this key episode in recent macroeconomic history and for identifying which forces generalize beyond the pandemic. Such forces may provide new lessons about household behavior and future policy design.

Although a standard model is unable to match the data, we show that a model with two key departures from the standard model is able to closely replicate our empirical results. The standard model features perfect foresight and moderate discount rates calibrated to prior evidence on average MPCs. In this model, households respond in anticipation of future policy changes and save most of their UI supplements. In contrast, the data shows no anticipatory response and less-than-expected incremental saving, even though households have relatively high liquidity. To match these empirical patterns, it is necessary for households in the model to have myopic expectations, acting as if they are surprised by supplement changes even when such changes should be perfectly predictable. Furthermore, to match the high MPCs we document even for high-liquidity households, unemployed households must have especially high discount rates. In particular, in order for our model to match both prior evidence on the MPC out of universal stimulus payments and our evidence on the MPC out of targeted UI supplements, we need unemployed households to have discount rates that are twice as high as those in the general population.

We first use this adjusted model to quantify the overall effects of UI supplements. Since households in the model have limited anticipatory responses, the policy's dynamic effects are muted. Mirroring the reduced-form empirical estimates, the model implies that expanded UI supplements had large effects on spending and small effects on employment. This conclusion holds both relative to overall fluctuations during the pandemic as well as compared to pre-pandemic evidence. When comparing to aggregate fluctuations during the pandemic, we estimate that UI supplements explain only 5% of aggregate employment shortfalls while they explain more than 20% of the recovery in aggregate spending. If we compare the best fit model to a standard pre-pandemic calibration, we find an elasticity of unemployment duration to supplements that is 80% smaller and an increase in spending while supplements are in effect that is 66% larger.

This leads to the first of three main lessons from our paper: benefit supplements were important for explaining the aggregate dynamics of spending–but *not* the dynamics of employment–during the pandemic. Furthermore, from a distributional perspective, the large spending responses that we document can help explain why spending rose most for low-income households during the pandemic (Cox et al. 2020) even though these same households had the biggest declines in labor income (Cajner et al. 2020).

We next dig deeper to understand *why* the spending response was so large and the job-finding response was so small.

For spending, two forces explain the large response to supplements. First, supplements target households who have lost their jobs. In the absence of supplements, these households are low liquidity since regular benefits do not fully replace lost earnings. This fact can help explain why unemployed households respond strongly to the *first* dollar of supplements they receive. However, in total, the supplements are so large that they actually drive households fully out of this liquidity-constrained state. Thus, low liquidity alone cannot explain why supplements drive the spending of unemployed *above* that of employed workers. Instead, unemployed households must also have some permanent behavioral characteristics such as impatience or present-bias that lead to high MPCs even with high liquidity. This is especially important for explaining our empirical finding that households with lower liquidity years before the pandemic have higher MPCs to UI supplements throughout the pandemic, even after the supplements substantially increase liquidity.

This leads to the second of three main lessons from our paper: permanent household characteristics—and not just current liquidity—are also important for understanding household consumption patterns. Prior work has documented an empirical correlation between low liquidity and high MPCs. Drawing on this evidence, a large literature has developed models in which high MPCs arise solely from temporarily low liquidity (see Kaplan and Violante 2014 and Angeletos et al. 2001). In most economic environments, it is hard to disentangle the direction of causality that drives the liquidity-MPC correlation: households facing a temporary liquidity crunch will have high MPCs, and households with permanently high MPCs will run down their liquidity. However, the pandemic UI supplements provide a quasi-random positive shock to liquidity which allows us to observe the spending responses of previously low-liquidity households in a high-liquidity state. The high spending responses we observe provide direct evidence that temporarily low liquidity cannot be the only explanation for high MPCs.

For job finding, two types of forces explain why the employment distortion is small relative to pre-pandemic estimates. First, supplements were temporary and implemented in a recession, when the new job-finding rate was already depressed. This limits the scope for supplements to affect employment. Second, businesses recalled many laid-off workers when they reopened and there were pandemic-induced reductions in the sensitivity of job finding to benefits. The second group of forces are unlikely to generalize beyond the pandemic, but the first force explains about half of the reduced employment distortion and should generalize: short-lived increases in unemployment benefit levels, when the job-finding rate is depressed, are likely to induce small employment distortions.

The particular forces driving large spending and small employment effects lead to the third main lesson from our paper: temporary benefit supplements are a promising countercyclical tool. The effect of temporary supplements on unemployment duration will be reduced when the job-finding rate is depressed during a recession. The large spending effects of temporary supplements to the unemployed will simultaneously be beneficial in recessions if aggregate demand is too low. Our results suggest that targeted payments to unemployed households during recessions can provide a useful complement to near-universal stimulus checks, which the federal government has frequently relied on to stimulate demand. Even for a policymaker who is indifferent to distributing \$1 to unemployed households versus \$1 to employed households, we find that it would be preferable from a stimulus perspective to give up to a \$2,000 one-time payment targeted to unemployed households before giving \$1 of untargeted stimulus to all households.

Our paper connects to several additional strands of past research. There is a rich empirical literature analyzing the effect of pandemic UI supplements.[1] Our paper makes two main contributions relative to this literature. First, while other contemporaneous work has evaluated the labor market impacts of expanded benefits, our paper is the first to study spending impacts.[2] We show that the effects on spending were much larger than the effects on employment. Second, we develop a structural model to help interpret the empirical patterns in the data. This is crucial for enabling us to identify which lessons are likely to generalize beyond the pandemic environment as well as for interpreting the magnitude of the results.

Our paper is also connected to a growing literature on the relationship between MPCs and liquidity. For example, Kueng (2018) finds that high-income households with substantial liquidity have a high

[1] See, e.g., Bartik et al. (2020), Coombs et al. (2022), Dube (2021), Finamor and Scott (2021), Holzer, Hubbard, and Strain (2021), Hornstein et al. (2022), Marinescu, Skandalis, and Zhao (2021), and Petrosky-Nadeau and Valletta (2021).

[2] This complements other work studying the spending response to pandemic stimulus checks: Baker et al. (2023), Parker et al. (2022), Chetty et al. (2023).

MPC out of payments from the Alaska Permanent Fund, Fagereng, Holm, and Natvik (2021) finds that even high-liquidity households still have a substantial MPC out of lottery winnings, and Gerard and Naritomi (2021) shows that unemployed households have high MPCs out of severance payments that substantially increase liquidity. In addition, Baugh et al. (2021) shows that high-liquidity households exhibit substantial MPCs to tax refunds, and Aydin (2022) shows that high-liquidity households exhibit substantial responses to credit limit increases. Theoretical models by Lian (2021), Boutros (2022), Andreolli and Surico (2021), and Ilut and Valchev (2023) provide psychological foundations for such high MPCs. Furthermore, several papers provide complementary evidence that permanent heterogeneity is important for explaining high MPCs.[3] Aguiar, Bils, and Boar (2020), and Gelman (2021) combine panel data with a structural consumption model to disentangle the separate role of permanent and transitory forces on MPCs. Parker (2017) shows that MPCs are correlated with persistent characteristics like impatience in surveys, Patterson (2023) shows a relationship between unemployment risk and MPCs, and Garber et al. (2021) shows that "consumption binging" is systematically related to individual characteristics in administrative data.

Our setting complements prior empirical work in this area because we directly observe pre-transfer liquidity positions paired with a large and persistent increase in liquidity. We can use this variation to show that households who were low liquidity years before they received a transfer continue to have high MPCs even after they have been moved to a high liquidity state. This new evidence bolsters the case that permanent household characteristics are important drivers of spending behavior.

Finally, our paper also contributes to a literature analyzing the optimal cyclicality of unemployment benefits.[4] Relative to this literature, we provide direct empirical micro evidence of the powerful demand impacts of countercyclical benefits. We also identify a mechanism (permanent heterogeneity) that amplifies the potency of countercyclical UI relative to untargeted stimulus checks. In addition, we document a force in favor of countercyclical benefit levels arising from the interaction between supplement length and labor market conditions. This builds on important prior work by Schmieder, von Wachter, and Bender (2012), which shows that a similar force also applies to countercyclical benefit durations.

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

The Coronavirus Aid, Relief and Economic Security (CARES) Act implemented a variety of policies in response to the emerging pandemic. One provision was a massive expansion of unemployment benefits. The CARES Act established a $600 per week supplement from April-July 2020 paid in addition to 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

[3] We use the term "permanent" to follow the existing literature, but we are unable to distinguish between truly permanent heterogeneity and persistent heterogeneity.

[4] See e.g., Kroft and Notowidigdo (2016), Landais, Michaillat, and Saez (2018a), Landais, Michaillat, and Saez (2018b), Mitman and Rabinovich (2015), Kekre (forthcoming), and McKay and Reis (2021).

would not otherwise qualify for regular benefits, through the creation of the Pandemic Unemployment Assistance (PUA) program. Unemployed workers who qualified for UI through the PUA program were also eligible for the $600 supplements. Because of data constraints, our analysis does not distinguish between regular benefits and PUA. However, most benefit recipients in the analysis sample are receiving regular benefits.[5] Finally, the CARES Act also established Pandemic Emergency Unemployment Compensation (PEUC), which extended benefit eligibility to those who would have otherwise exhausted unemployment benefits.

The original CARES Act legislation authorized $600 supplements through the end of July 2020. 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 some 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.[6]

At the end of December 2020, new legislation authorized a $300 per week supplement through mid-March 2021. The supplement was later extended to last a total of eight months. The PEUC and PUA supplements were also extended through early September 2021. In June 2021, several states unexpectedly ended expanded unemployment benefits sooner than the legislated end date out of concern that these programs were having negative effects on labor markets. We discuss how multiple policies changing at the same time in summer 2021 complicates measurement of the effects of supplements during this time period in Appendix A.1.

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 2018 through October 2021. Our primary sample runs through February 2021 because this is when we can most reliably measure job finding and separate the effects of supplements from effects of expanded benefit eligibility. However, we also provide some analysis of supplements through the summer of 2021. Benefit eligibility extensions mean that UI exits from April 2020 through February 2021 rarely reflect benefit exhaustion and therefore usually reflect a return to work.[7] For this reason, we generally use job finding and UI exit interchangeably throughout the paper. The unit of observation is household-by-week. Our primary analysis sample consists of 1,458,481 unemployment benefit spells from 44 states in 2020. Figure A-1 shows a map of which states are in the sample. The exact set of states included in each part of the analysis is dependent on data availability and is described in Appendix A.2.

We measure unemployment insurance spells and labor income using information from direct deposits. We combine information on unemployment and employment spells to separate UI exits to a new job from UI exits to recall, which is when an unemployed worker returns to their prior employer. The details of how we construct these spells are described in Appendix A.3. We impose activity

[5]In the two states where JPMCI can distinguish between regular benefits and PUA (Ohio and New Jersey), 74% of observed UI spells are for households receiving regular benefits. Among households who meet the account activity screens described below, the share is even higher.

[6]On August 8, an executive order announced a "Lost Wages Assistance" (LWA) program to provide supplements for six more weeks. However, long delays meant these LWA payments nearly always occurred *after* the program expired, so we focus on the $600 and $300 supplements.

[7]For example, the California Policy Lab (Bell et al. 2022) calculates that fewer than 3 in 1000 recipients exhausted benefits during this time period. However, beginning in March 2021, there are a number of UI exits that do not reflect job finding, due to a technical issue with how UI systems pay claims for spells that last longer than a year.

screens to ensure we capture workers whose primary bank accounts are at Chase and who have stable employment prior to the pandemic (see Appendix A.4 for details).

We construct two main measures of spending. Our preferred *total spending* measure sums spending on Chase credit cards, Chase debit cards, cash withdrawals, paper checks, and various electronic payments. This measure excludes debt payments on mortgages, cars, student loans, and credit cards, as well as transfers to other accounts. While this is the most comprehensive measure of spending we can observe in Chase accounts, we note that it nevertheless excludes most durable purchases and so most closely corresponds to broad non-durable spending.[8] Nevertheless, two concerns arise from this measure. First, it includes some payments where the payee cannot be identified.[9] It is thus possible that some of these transactions may not actually be spending. Second, it includes spending with potential for timing-related measurement error if there is a delay between when a paper check is written and when it is deposited.

We therefore also report results for a more narrow *card and cash* spending measure which excludes all paper checks and most electronic payments.[10] Since it omits many recurring payments and eliminates the timing-related measurement error induced by paper checks, card and cash spending is better suited for measuring week-to-week spending changes caused by week-to-week income changes. Nevertheless, this more narrow measure omits all payments to unknown recipients, even though many of these actually are spending. In this sense it understates actual MPCs.

We also measure household income and checking account balances. We define income as total inflows to Chase deposit accounts, excluding transfers. This definition captures take-home income because we only observe income after taxes and other deductions (such as retirement account contributions and health insurance premiums) are withheld. We exclude transfers (e.g., from other bank accounts, money market accounts, and investment accounts) to avoid double-counting income. Checking account balances are measured at a monthly frequency as the account balance on the final business day of the month. We sum balances across all accounts for households with multiple Chase checking accounts. Finally, we observe stimulus check receipt, age, number of children, and industry of work for selected subsamples. Additional detail is provided in Appendix A.5.

Table A-1 provides summary statistics on the main flow measures of interest—income, UI benefits, total spending, and card and cash spending—as well as checking account balances.

2.2.1 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 Department of Labor (DOL) data to the number of households receiving unemployment benefits in the JPMCI data. From early March to June, Figure A-2a shows that these series rose by a factor of 15 in DOL and a factor of 17 in JPMCI.[11]

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. Figure A-2b shows that the states with the largest increase

[8] Most durables like cars and houses are financed, and our spending measure will not include these purchases.

[9] We are unable to identify the recipient of payments made by paper check. Furthermore, apart from debt payments and transfers to other bank accounts, we are unable to categorize the majority of remaining electronic account outflows (e.g., those made via wire transfer, ACH, and other electronic channels).

[10] We include electronic payments for utility bills because this is a form of non-durable spending.

[11] The increase in UI payments to households that meet the account activity screens in JPMCI is slightly smaller, likely reflecting the fact that the pandemic recession was particularly severe for underbanked households, who are likely to be omitted because of these account activity screens.

in UI claims in DOL also have the largest increase in JPMCI, and vice versa. Figure A-2c shows that there is a strong cross-state correlation between benefit levels in DOL and benefit levels in JPMCI, although weekly benefit levels are a bit higher in JPMCI than in DOL. This implies that UI 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. 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.

Finally, the JPMCI data capture shifts in the industry composition of unemployment. Figure A-3 shows that industries with the largest increase in unemployment in DOL, such as retail and accommodation & food services, also have the largest increase in JPMCI. Industries with the smallest increase in unemployment in DOL such as construction also have the smallest increase in JPMCI.

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 and cross-industry heterogeneity which can be captured using statistics reported by the DOL. For additional analysis of the representativeness of unemployed households in JPMCI data, see Ganong and Noel (2019).

3 Spending Responses to Expanded Unemployment Benefits

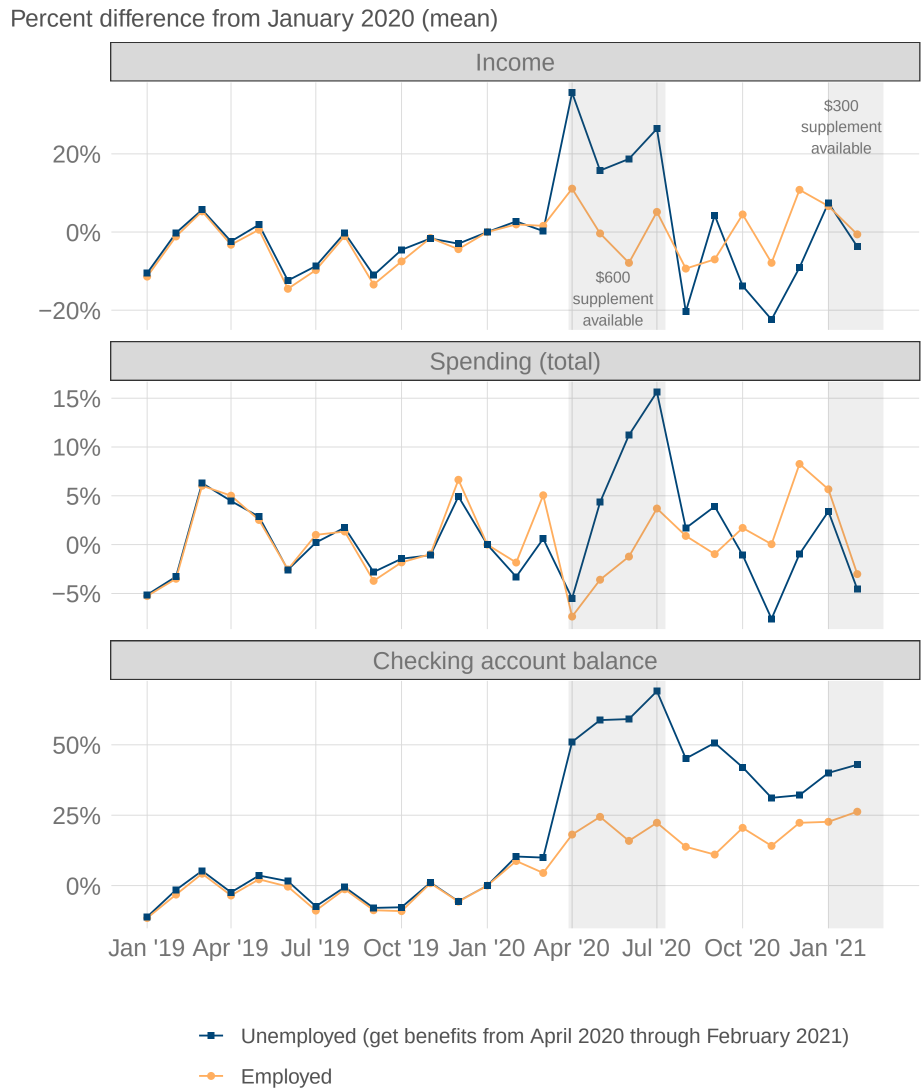
This section explores the empirical effects of unemployment benefits on spending. We begin with descriptive analysis. We next identify MPCs separately out of the onset of benefits, the expiration of the $600 supplement, the onset of the $300 supplement, and the expiration of the $300 supplement. Each of these empirical exercises has distinct advantages and disadvantages, but they all lead to the same conclusion: spending is highly sensitive to changes in unemployment insurance benefits.

3.1 Time-Series Patterns: Spending of the Unemployed *Rises* After Job Loss When Expanded Benefits Available

Figure 1 compares changes in income, spending, and checking account balances for households who become unemployed and receive UI from April 2020 through February 2021 to similar households who remain employed through February 2021. Throughout the paper, whenever we compare employed to unemployed households, we reweight the employed sample so that it exactly matches two observable characteristics of the unemployed: 2019 income quintile and date of stimulus check receipt. Matching by income is potentially important since low-income households were more likely to become unemployed during the pandemic (see Table A-1) and households at different points of the income distribution may have spending that evolves differently over the pandemic. Matching by stimulus check date is potentially important since these stimulus checks arrive around the time that both the $600 and $300 supplements start. We later show results for a richer set of observable controls, and conclusions are also similar without any matching.

Figure 1 shows that income for the unemployed rises and falls with the ebb and flow of benefit supplements. Prior to the start of unemployment, month-to-month changes in income are nearly identical for the two groups. Note that the matching procedure described above generates similarity in only

Figure 1: Income, Spending, and Account Balances of Unemployed Versus Employed



Notes: This figure compares income, spending, and checking account balances of unemployed and employed households using JPMCI data. The blue line shows households that receive unemployment benefits from April 2020 through at least February 2021. The orange line shows employed households who are matched on 2019 income quintile as well as date of receipt of stimulus checks. The $600 supplement is first paid in the middle of April, so May is the first complete month during which households have the opportunity to spend the supplement.

the *level* of income; the similarity of pre-pandemic *changes* in income between the two groups is not mechanical. Beginning with the start of unemployment, income of the two groups diverges substantially. Since the combination of regular UI plus the $600 supplement results in average replacement rates above 100 percent, income actually rises substantially for the unemployed from April through July 2021. At the end of July, the $600 supplements expire, and so the income of the unemployed falls below that of the employed. Income rises briefly in September 2020 with the payment of temporary LWA (Lost Wages Assistance) supplements. Finally, income rises again for the unemployed in January 2021 when the $300 supplement begins.

After households become unemployed and receive $600 weekly supplements, their spending rises substantially *above* pre-pandemic levels. The middle panel of Figure 1 shows the evolution of monthly spending for the two groups. Like income, spending of the unemployed evolves nearly identically to the employed prior to the point of unemployment and then rises sharply at the start of unemployment in April 2020. This is especially notable when compared to the declining spending of employed households in this early part of the pandemic. Usually unemployed households reduce spending relative to employed households (Gruber 1997), but during the period of $600 supplements, these normal patterns are reversed. This sustained increase in relative spending occurs for the entire time the $600 supplement is in place. When the supplement terminates at the end of July, there is then an immediate decline in spending. This is followed by a temporary rebound when unemployed households receive temporary LWA supplements in September. Spending then remains depressed until the $300 supplements begin in January 2021. These supplements lead to a median replacement rate of 100% and the spending of unemployed and employed households is similar after they begin. Thus, we find a strong relationship between unemployment benefit levels and the spending of the unemployed throughout the pandemic.[12]

The bottom panel of Figure 1 shows that there is also a large and sustained increase in the checking account balances of unemployed households, both in absolute terms and relative to employed households. Increases in income for unemployed households during this period were so large that they accumulated additional savings even as their spending increased.

Our finding of significantly increased spending among unemployed workers is reminiscent of the striking pattern documented in Gerard and Naritomi (2021). That paper shows that unemployed workers who receive severance pay in Brazil from 2010 to 2012 sharply increase spending upon unemployment. Thus, both severance pay in Brazil and pandemic supplements in the U.S. are settings where workers should expect low income in the future (if they remain unemployed) and nevertheless *increase* their spending in absolute terms in the present because of a temporary influx of cash. We complement the Gerard and Naritomi (2021) evidence in two ways. First, we show that such an increase in spending can last for many months. Second, because we can also track liquid balances, we are able to show that such an increase can persist even when households have been pushed far off their liquidity constraints. These relationships between liquidity and spending will be important when we turn to model interpretations and policy implications.

We now use difference-in-difference research designs to estimate the MPC out of benefits. Some of these research designs will use comparisons between unemployed and employed, as in Figure 1, while others will rely on comparison between various groups of unemployed households.[13]

3.2 Estimating MPCs

3.2.1 Waiting for Benefit Receipt

State unemployment agencies were overwhelmed by the large increase in unemployment claims at the start of the pandemic, meaning that the payment of many claims was delayed. We use these delays in payment to identify the causal impact of benefits on spending. We compare the spending of a treatment group of unemployed households who receive benefits promptly after filing a claim to the spending of a control group of unemployed households who experience delays in receiving benefits.

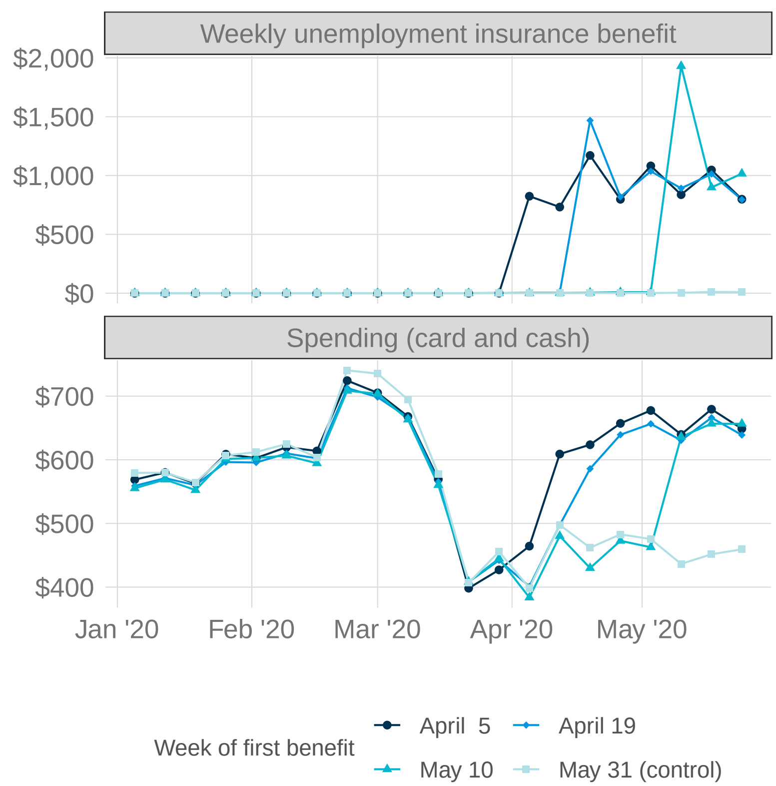
[12] Figure A-4 shows that similar patterns hold for medians instead of means, and Figure A-5 shows that all of the same patterns are present when looking at the subset of spending on card and cash.

[13] The employed are only a valid control group for the unemployed in times where their income would have moved similarly absent supplements. This assumption would clearly be violated at the start of unemployment in April 2020, when the income of unemployed would have fallen relative to the employed absent supplements.

To construct these groups, we compare cohorts of unemployed households, all of whom stop receiving paychecks at the end of March but differ in the date of first benefit payment. Since households who face delays ultimately receive back pay for missed payments, the treatment is therefore the *timing* of the arrival of liquidity, analogous to how Johnson, Parker, and Souleles (2006) study variation in the timing of stimulus checks.[14] Focusing on the difference between two groups of unemployed households removes any direct effects of job loss itself on spending and isolates the effect of benefit receipt.

The top panel of Figure 2 shows weekly patterns of unemployment benefits for four different groups of unemployed households. We look at weekly relationships because this provides the sharpest illustration of the exact temporal relationship between benefits and spending. We note that this requires spending which can be measured precisely at the weekly level. For this reason, we focus on weekly card and cash spending for this high frequency analysis. By construction, benefits are zero for each group prior to the first benefit week and then jump in the first week of benefits. Groups that start benefits later have larger jumps because of back pay. All groups have similar spending in levels and trends prior to the start of UI benefits. The bottom panel shows that spending jumps sharply in exactly the week when benefits start. It then remains at an elevated level in subsequent weeks.

Figure 2: Impact of Delays in Unemployment Benefits on Spending



Notes: This figure shows mean benefits and spending for several cohorts of unemployed households using JPMCI data. All households stop receiving paychecks at the end of March, but differ in the date of first benefit payment.

To estimate a monthly MPC for total spending we use a difference-in-difference design which compares spending for a treatment group that receives benefits at the start of April 2020 to a control

[14] If the waiting group expects to receive benefits in the future, then the waiting MPC will only capture liquidity effects and not any income effects of benefit receipt on spending. Thus, the waiting MPC should be a weak lower bound on the true MPC out of benefits.

group that receives benefits at the start of June.[15] We compute a monthly MPC because it allows us to use this broader spending measure and because it eases comparison to the prior literature. Figure A-6 shows that patterns for total spending are similar patterns to card and cash spending in Figure 2. We estimate an instrumental variables (IV) regression with the second stage given by equation (1) and the first stage given by equation (2):

$$c_{i,t} = \psi + MPC \times \widehat{y}_{i,t} + Treat_i + Post_t + \varepsilon_{i,t}, \tag{1}$$

$$y_{i,t} = \alpha + \beta Post_t \times Treat_i + Treat_i + Post_t + \epsilon_{i,t} \tag{2}$$

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 $t = March$.[16] 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 ($cov(Post_t \times Treat_i, \varepsilon_{i,t}) = 0$).[17]

We estimate a one-month MPC out of UI benefits of 0.42 in Table 1. This estimate implies that nearly half of unemployment benefits are spent in the first month after receipt. Gauging whether this is large or small requires a model, since the MPC out of benefits depends on the expected persistence of benefit changes, whether these changes are anticipated or not, and on household liquidity. In Section 5, we develop such a model and show that this MPC is large relative to prior work.

Although we think this waiting design provides our sharpest identification strategy, it has two important limitations: First, it measures the MPC out of total unemployment benefits (regular benefits plus the $600 supplements) rather than the response to the supplements alone. Second, it measures spending during the early, most uncertain days of the pandemic and so it might have less generalizability. For these reasons, we turn to an analysis of supplement changes at later dates.

3.2.2 Supplement Changes

We next show that changes in benefit supplements alone also have a substantial impact on spending. We study the expiration of the $600 supplement at the end of July 2020, the onset of the $300 supplement in January 2021, and the end of the $300 supplement in the summer of 2021.

Our main identification strategy compares unemployed and employed households, so our identifying assumption is that spending *changes* at a point in time would have been the same for the unemployed and employed absent supplement changes. While this assumption is unlikely to hold for the time-series as a whole, there are no obvious economic events that should violate this assumption at the exact time that the supplements expire or begin. As in Section 3.1, we use a group of employed households

[15] Because this is a two-period two-group research design, it is not subject to the concerns raised in the recent literature on staggered implementation difference-in-differences.

[16] The $600 supplement is first paid in the middle of April 2020, so *May* is the first complete month during which households have the opportunity to spend the supplement.

[17] Two types of evidence suggest that delays in benefits are orthogonal to other determinants of spending behavior. First, owing to the high volume of claims, overall delays in payments to eligible claimants were much longer than usual (Bitler, Hoynes, and Schanzenbach 2020) and it is unlikely that state UI systems were able to prioritize claims in ways that would be correlated with spending behavior. Second, Figure 2 shows that not only are the trends in card and cash spending prior to benefit receipt similar by cohort (the standard parallel pre-trends test), but the *levels* of spending are similar across cohorts as well. The similarity of levels and of pre-trends for spending suggests that the identifying assumption is satisfied.

Table 1: Marginal Propensity to Consume out of Unemployment Benefits

	Research Design	One Month MPC (Total Spending)
1	Waiting for benefits	0.42
		(0.01)
2	Expiration of $600 supplement	0.31
		(0.01)
3	Onset of $300 supplement	0.28
		(0.01)
4	Expiration of $300 supplement (June states)	0.35
		(0.02)
5	Expiration of $300 supplement (September states)	0.27
		(0.01)
6	Expiration of $300 supplement (June vs. September states)	0.39
		(0.03)

Notes: This table shows estimated one-month MPCs using spending (total) for several different unemployment benefit changes using equations (2) and (1). The waiting for benefits design compares unemployed households receiving benefits to those who face benefit delays. Rows (2) through (5) compare unemployed households to a sample of employed households matched on pre-separation income and date of stimulus checks. Row (6) compares unemployed households in states which ended $300 in June to unemployed households in states which ended $300 in Sept. The total number of observations (treatment + control) in these five designs is respectively N=52,094; N = 247,770; N = 145,978; N= 153,277; N=47,623. Standard errors are clustered by household.

matched on pre-pandemic income and stimulus check date, since these are the most obvious potential confounds. To validate this identifying assumption, we study the evolution of spending for unemployed and employed households prior to the policy changes. Figures A-7 and A-8 show similar pre-trends prior to the expiration of the $600 supplement and the onset of the $300 supplement respectively.

The supplements have an immediate visible impact on spending. Spending on card and cash drops sharply at the expiration of the $600 supplement (Figure A-7) and rises sharply at the onset of the $300 supplement (Figure A-8). Effects on total spending are also noticeable in Figures A-9 and A-10, but a bit harder to detect visually because of week-to-week fluctuations in spending for both the unemployed and employed.

We estimate the one-month MPC out of supplement changes using the IV approach in equations (2) and (1). For expiration of the $600 supplements, we define $t = \{July, August\}$, and set $Post$ equal to 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 begin benefits by June 14 at the latest and continue receiving benefits through at least August 30. For onset of the $300 supplements, we define $t = \{December, January\}$, and set $Post$ equal to one in January and zero in December. The treatment group is households that are unemployed from November 2020 through February 2021. Table 1 shows that we find an MPC of 0.31 at the expiration of $600 and an MPC of 0.28 at the onset of $300 supplements.

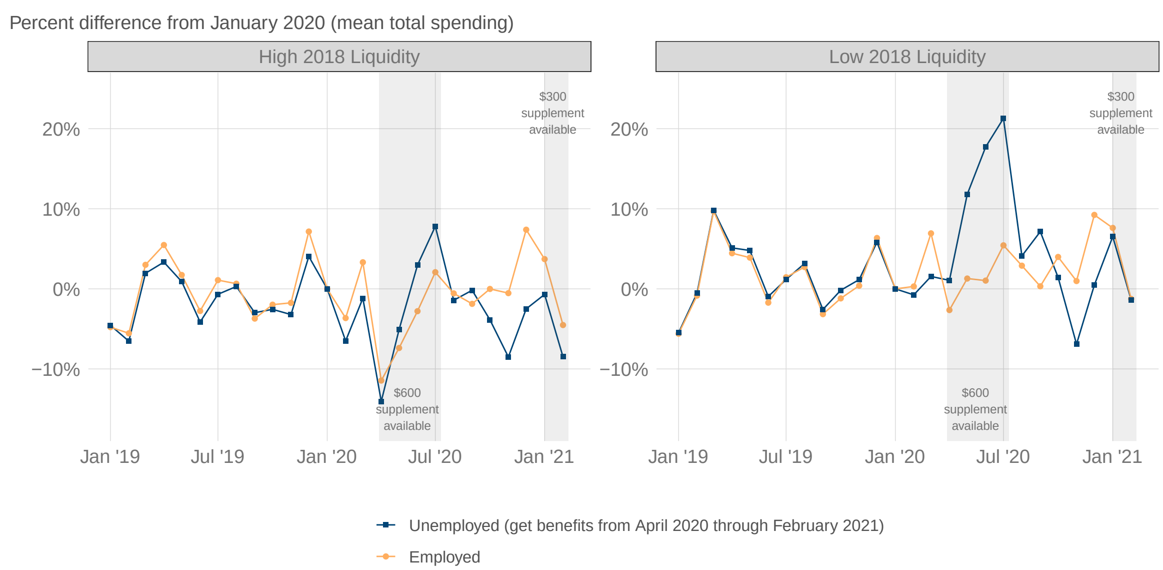
Figures A-11 and A-12 show the time-series of spending for the summer of 2021. Table 1 also shows estimates of MPCs to the end of $300 supplements. As we discuss in more detail in Appendix A.1, the fact that these supplements expire at the same time as expanded eligibility programs (PEUC and PUA) means that we can only estimate these MPCs for a subset of short-duration unemployed households who continue to receive benefits after all pandemic unemployment programs end. Thus, we focus less on these results. However, two observations are of note: first, the estimate of MPCs in row (5) of Table 1 for the expiration of the $300 supplements is similar to the estimate in row (3) for the start of the $300 supplements. Second, since states vary in the timing of their expiration, we can construct controls that use unemployed households in other states and compare this to identification strategies that instead compare to employed households. Row (6) in Table 1 shows that a design using unemployed households as controls produces an estimate similar to the design in row (4), which uses employed households as controls. This suggests that employed households are likely to be an appropriate control group even in the other instances where we have no natural unemployed group as a control. Appendix B.1 provides additional detail on this analysis.

3.3 Persistent Spending Differences *within* the Unemployed

Our results thus far have focused on estimating the average MPC to UI supplements for the unemployed. In this section we show that there is a strong relationship between liquidity buffers measured long before the pandemic and spending responses to UI supplements. We measure median monthly liquidity buffers for each household in 2018, intentionally choosing a measure years before the UI supplements in 2020-2021. See Appendix B.2 for additional details.

Figure 3 shows that households with below-median liquidity buffers in 2018 have larger spending responses to UI supplements than households with above-median liquidity buffers in 2018. Table 2 repeats our causal identification strategies splitting by liquidity and similarly shows that the MPC to each supplement change is about twice as large for households with low liquidity years before the pandemic.

Figure 3: Spending by Pre-Pandemic Liquidity



Notes: This figure compares the spending of unemployed and employed households separately by liquidity. The left panel shows results for households with high (above median) 2018 liquidity and the right shows results for those with low (below median) 2018 liquidity.

In Section 5.3.3, we interpret this evidence through the lens of a structural model of consumption. This evidence is useful for evaluating the extent to which "temporary low liquidity causes high MPCs"

Table 2: Marginal Propensity to Consume out of Unemployment Benefits by Liquidity

Design	High Liquidity MPC	Low Liquidity MPC	Difference
Waiting for Benefits	0.29	0.53	0.24
$600 expiration	0.20	0.42	0.22
$300 onset	0.19	0.35	0.16
$300 expiration Sept states	0.18	0.36	0.18
$300 expiration June states	0.22	0.46	0.24
$300 expiration June vs. Sept states	0.24	0.51	0.26

Notes: This repeats Table 1 splitting by high (above median) 2018 liquidity and low (below median) 2018 liquidity.

versus "permanently high MPCs cause low liquidity". The fact that pandemic-era MPCs are so much higher for households with low liquidity years prior to the pandemic supports the latter view.

3.4 Robustness and Additional MPC Results

Appendix B.3 shows four additional results. First, we decompose the MPCs into separate categories, and show that the category-level MPCs do not suggest unusual spending patterns. Further, the fact that the spending of employed households is depressed during the pandemic makes the large MPCs we estimate even more noteworthy. Second, we show that spending patterns are nearly identical for households who are recalled and those are who are not, suggesting that MPCs are not driven by the unusually high recall rates during the pandemic. Third, to address concerns about MPC estimates being biased by selection into unemployment, we show that our results are robust to using a richer set of controls when comparing employed and unemployed households. Fourth, our conclusions are also robust to limiting the sample to households for whom we observe a more complete lens on spending, and also to alternative measures of spending.

4 Disincentive Effect of Benefit Supplements

This section estimates the effect of benefit supplements on the exit rate from unemployment. This is particularly useful because there was widespread uncertainty over whether more generous benefits were responsible for low re-employment rates during the pandemic.[18] Our estimates imply an elasticity of unemployment duration with respect to both the $300 and $600 supplements which is smaller than typical estimates in the past literature.

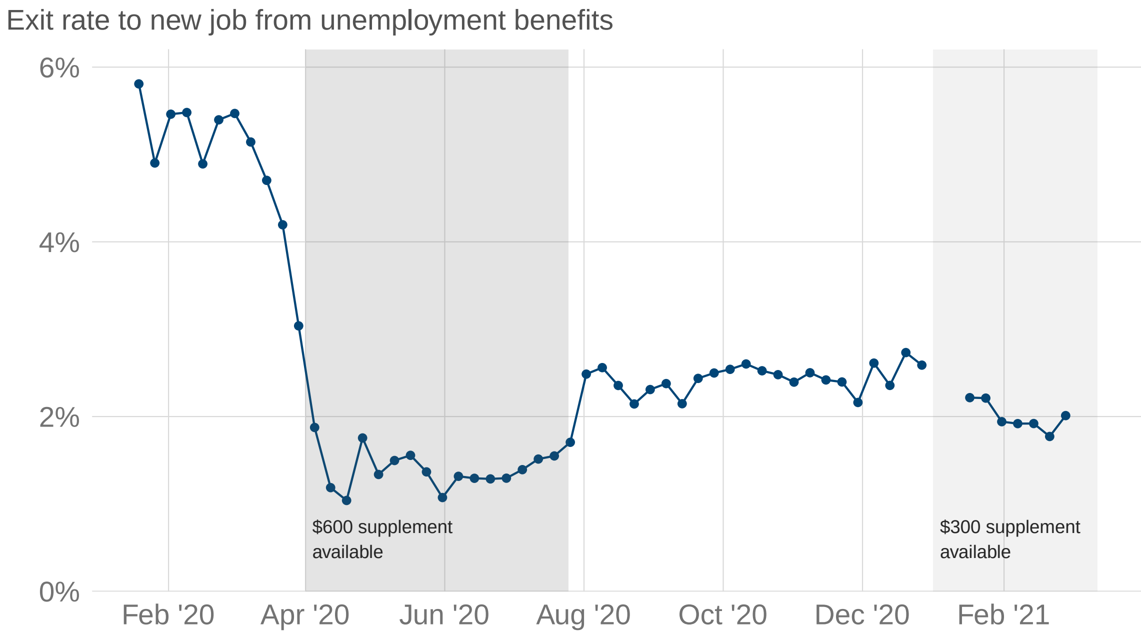
We focus primarily on the exit rate to new jobs rather than to recalls. This is because UI eligibility requires recipients to accept any offer of "suitable work," which should reduce the sensitivity of recalls to benefits. Nevertheless, Figure A-13 shows patterns for the recall rate and the total exit rate, and Section 4.4 discusses additional results for recall.

Figure 4 shows that at the start of the pandemic, the weekly new job-finding rate plunges by four percentage points and remains depressed thereafter. It also shows that the job-finding rate modestly

[18] For example, a contemporaneous IGM survey of economists showed that a majority were uncertain about whether supplements were a "major disincentive to work" (Initiative on Global Markets 2021). The share who were uncertain about the disincentive was higher than for more than 93% of the other IGM survey questions asked since January 2020.

rises and falls with the expiration and onset of the supplements. These descriptive patterns suggest that the supplements may modestly reduce job-finding rates, but that these effects were dwarfed by other factors holding back job finding during the pandemic.[19] We turn next to research designs that identify the causal effect of supplements on job finding.

Figure 4: Exit Rate from Unemployment Benefits to New Job



Notes: This figure shows the exit rate to new jobs in JPMCI data. There was a brief lapse in pandemic UI eligibility expansions (PUA and PEUC) at the end of 2020, so many workers stop receiving UI benefits on January 3 and January 10, as shown in Figure A-15. However, this does not reflect a change in the new job-finding rate, so we drop these dates in the figure and estimation.

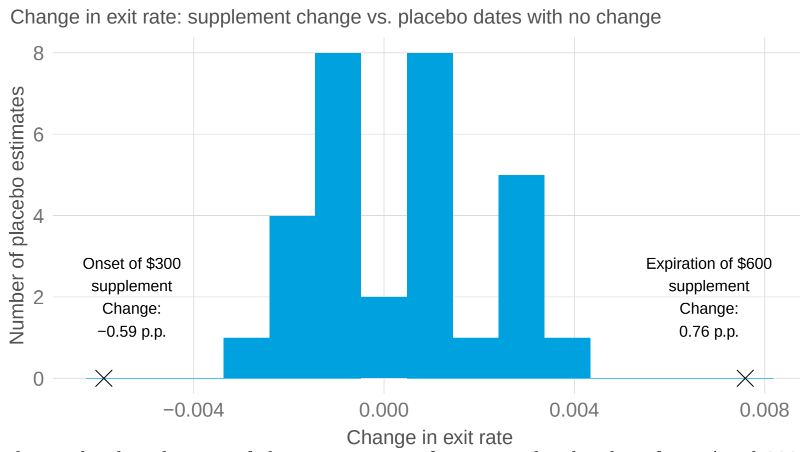
4.1 Research Design 1: Interrupted Time-series

We use an interrupted time-series design to estimate the effect of the supplements on the new job-finding rate. Specifically, we study the change in the average job-finding rate in the two weeks prior to the policy change relative to the first four weeks after the policy change as illustrated in Figure A-16. The identifying assumption is that this rate would have been constant had there been no change in the supplement. This is a strong assumption, but we are using high-frequency weekly data so any confounding changes must occur at exactly the same time as the changes in supplements. In the next section, we use an alternative research design which is not subject to potential confounding high-frequency aggregate shocks. We find that the job-finding rate rises by 0.76 p.p. when the $600 supplement expires, and falls by 0.59 p.p. after the onset of the $300 supplement. These effects are economically small, as we discuss in more detail in Section 4.3.

To assess statistical significance, we rely on the fact that the legislated duration of pandemic unemployment policies in the CARES Act was based on a highly uncertain forecast in March 2020 about the duration of the pandemic. This motivates an approach to inference which treats the exact

[19] The sharp decline in the new job-finding rate during the pandemic is not driven by changes in the composition of who is unemployed; Figure A-14 shows that very similar aggregate dynamics are apparent for the subset of workers who were unemployed before the pandemic began.

Figure 5: Distribution of Placebo Estimates



Notes: This figure shows the distribution of the test statistic for every placebo date from April 2020 through February 2021, where we define placebo windows as those with no policy change. 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 $p = 1/31$.

date of the policy change as random. 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 5 compares the distribution of the changes in the job-finding rate at the placebo dates to the changes at the actual implementation dates. The observed changes at the policy implementation dates are more extreme than any of the changes at the 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 $.032 = 1/31$ if we include the own implementation date and exclude the implementation date of the other policy.

4.2 Research Design 2: Difference-in-difference

As a complement to the interrupted time-series analysis, we use an alternative difference-in-difference design to estimate the impact of supplements on job finding. Because the supplements 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% under a \$600 supplement, while a worker with pre-separation earnings of \$1,200 per week and a regular weekly benefit of \$600 would see their replacement rate rise to 100%. This heterogeneity in the intensity of treatment motivates a dose-response difference-in-difference research design. To measure the intensity of treatment for each worker we compute the symmetric percent change in benefits around supplement changes:

$$PctChange_i = \frac{2(b_{i,post} - b_{i,pre})}{b_{i,pre} + b_{i,post}}. \tag{3}$$

We measure the worker's pre-treatment benefit $b_{i,pre}$ as the median weekly payment in the two-month period before a policy change. Given $b_{i,pre}$, we then impute $b_{i,post}$ based on statutory rules.[20]

Figure 6 shows the evolution of exit rates, dividing workers into groups with higher-than-median $PctChange_i$ and lower-than-median $PctChange_i$. Two lessons emerge from the figure. First, the two groups have similar trends in the job-finding rate before the policy changes. Second, the policy changes induce differential changes in exit rates for those who are differentially treated by the policy changes: when the \$600 supplement ends, the job-finding rate rises more for the higher $PctChange$ group, and when the \$300 supplement begins, the job-finding rate falls more for the higher $PctChange$ group. Figure 7 uses a bin scatter of benefit growth and exit rate changes to show that the same conclusions hold using the full range of $PctChange$ instead of splitting by above/below median changes, and that this relationship is roughly linear in $PctChange$.

To quantify the causal effect of replacement rates on the job-finding rate, we use a difference-in-differences regression that captures the variation in Figure 7. Let t index periods, i index workers, and e_{it} be an indicator for exit to new job. We use data on eight weeks where the supplement is not available and eight weeks where the supplement is available as captured by the indicator $SuppAvail_t$. We estimate the model:

$$e_{it} = \gamma PctChange_i + \alpha SuppAvail_t + \beta SuppAvail_t \times PctChange_i + \varepsilon_{it} \tag{4}$$

We discuss the assumptions needed for this regression to identify the causal effect of benefits on job-finding in Appendix C.2.[21] The key coefficient of interest in equation (4) is $\hat{\beta}$, which captures how the job-finding rate changes for more-treated vs less-treated workers. Table A-2 shows that at \$600 expiration, we estimate $\hat{\beta} = -0.016$ and at \$300 onset, we find a similar coefficient of $\hat{\beta} = -0.020$. These effects are precisely estimated with a standard error of 0.001 with standard errors clustered at the household level.

4.3 Interpreting Magnitudes

What do the results from our two different identification strategies imply for the effect of supplements on job finding? Table 3 shows our headline estimates of how UI supplements affect the job-finding rate. We estimate that the \$600 supplement reduces the weekly job-finding rate by 0.76 p.p. using the interrupted time-series estimates and by 1.45 p.p. using the difference-in-difference estimates. The \$300 supplement reduces the job-finding rate by 0.59 p.p. using time-series estimates and by 1.18 p.p using difference-in-difference estimates. Thus, the difference-in-difference estimates are about twice the size of the time-series estimates.[22] However, as we discuss below, all four of these estimates are economically small. Because the two different research designs rely on orthogonal sources of variation in benefits, the similarity of the estimates (in terms of their economic effects) across the two designs

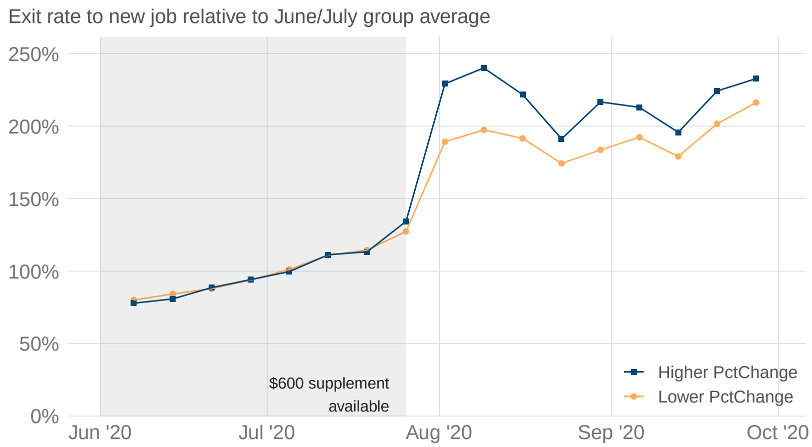
[20]This imputation is necessary because we do not observe $b_{i,post}$ for workers who find a job before the \$600 supplement has expired or before the \$300 supplement has been reinstated. See Appendix C.1 for details.

[21]We note that while Cox models are frequently used in the literature on UI disincentives, they are not suited for studying within-spell policy changes. Our baseline specification uses a linear probability model given the linearity shown in Figure 7 as well as the linearity implied by our theoretical model in Section 5.

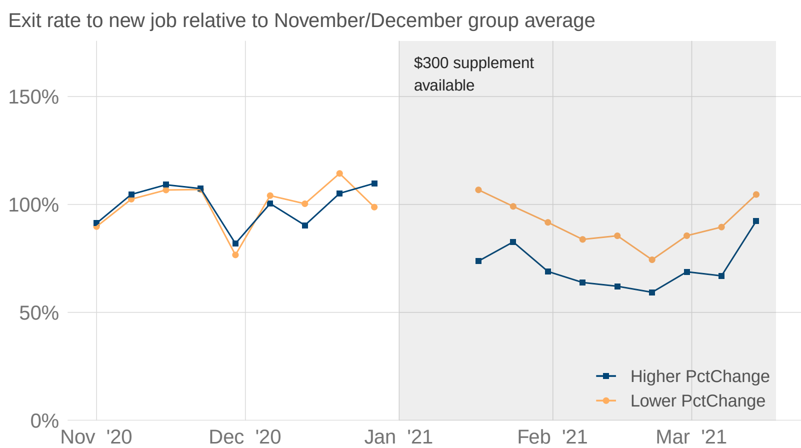
[22]This effect is directly implied by the interrupted time-series estimate, but additional assumptions are required to get to this total effect from the marginal effects identified by the difference-in-difference design. Although our analysis treats the two research designs as estimating the same parameter, one possibility for why time-series estimates are smaller is if "micro" disincentive effects of UI (the effect of giving one worker more benefits) is bigger than the "macro" disincentive effect (the effect of giving all workers more benefits). See Appendix C.3 for further discussion.

Figure 6: Effect of Expanded Benefits: Event Study

(a) Expiration of \$600



(b) Onset of \$300



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 orange line shows workers with a lower-than-median replacement rate with the supplement and the 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 before the policy change (June and July 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 arising from the lapse in expanded UI eligibility. Figures A-17a and A-17b show the same figure but without a normalization in the pre-period. Figures A-17c and A-17d show that the same patterns hold when we look at the total job-finding rate (which includes both new job finding and recalls).

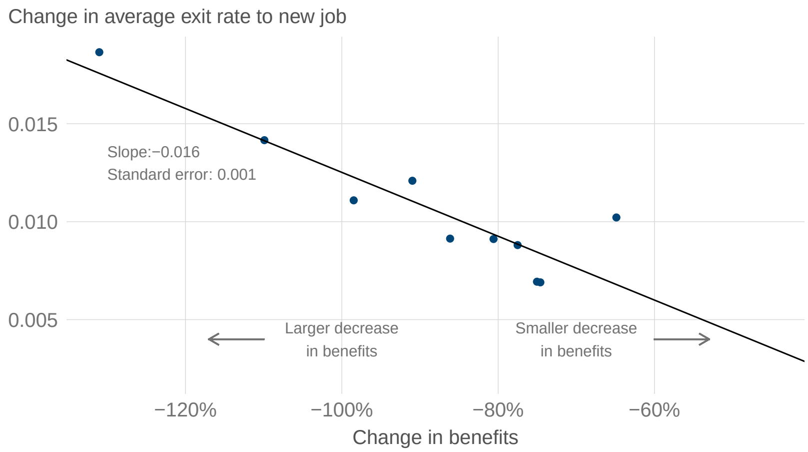
bolsters the conclusion that the supplements had small effects on the job-finding rate.

It is also useful to compare the effects of the \$600 supplement to the effects of the \$300 supplement. We report comparisons in two ways. First, we convert each estimate of the full supplement effect into an implied per-week causal effect of increasing benefits by \$100 relative to a no-supplement baseline.[23] Table 3 shows that effects per \$100 were similar for both the \$600 supplements and the

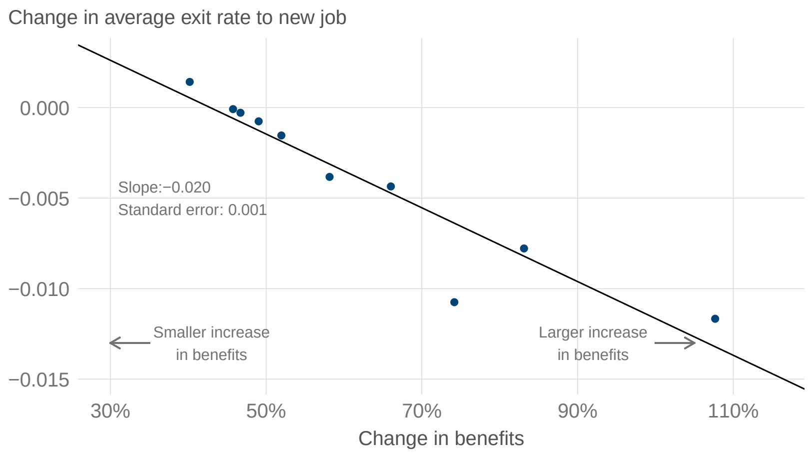
[23] The models in Section 4.2 are estimated using symmetric percent change $PctChange_i$. The average of $PctChange_i$ is 81% for the \$600 supplement and 57% for the \$300 supplement. Note that because we are using symmetric percent change in equation (3), $PctChange_i$ is not linear in the size of 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%.

Figure 7: 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 (3). The top panel shows the difference in the average new job-finding rate between Jun 1-Jul 31 and Aug 1-Sep 31. The bottom panel shows the difference in the average new job-finding rate between Nov 1-Dec 31 and Jan 15-Mar 15. The slope estimates correspond to the $\hat{\beta}$ coefficients reported in Table A-2.

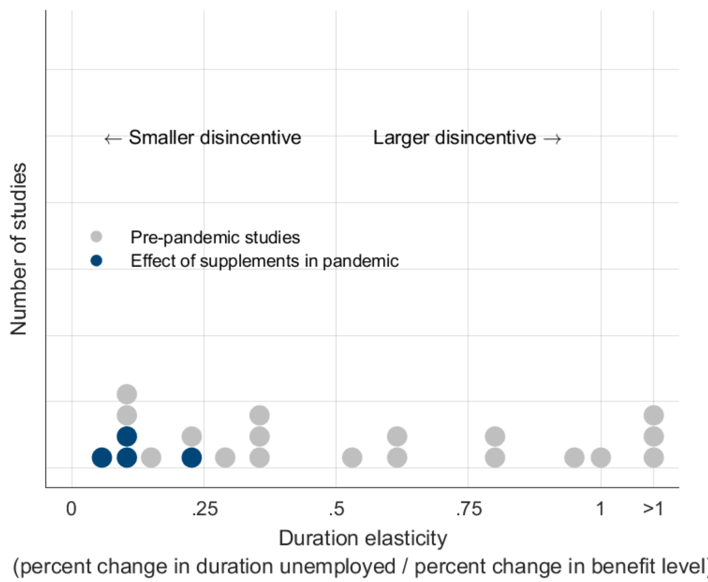
Table 3: Effect of Supplements on Weekly Job-Finding Rate

	Interrupted time-series		Difference-in-difference	
Effect of...	Entire supplement	per \$100	Entire supplement	per \$100
\$600 expire	-0.76	-0.19	-1.40	-0.35
\$300 onset	-0.59	-0.20	-1.18	-0.45

Notes: This table shows the effect in percentage points of benefit supplements on the weekly new job-finding rate. Row one uses estimates from the \$600 expiration and row two uses estimates from the \$300 onset. Note that because we use symmetric percent changes, the per \$100 effect is not 1/6 (1/3) of the total effect of \$600 (\$300). See footnote 23.

$300 supplements. Second, since elasticities are scale invariant, we compute a duration elasticity to benefit levels. We calculate this duration elasticity from the effects in Table 3 by assuming constant per-week effects on the job-finding rate while supplements are in place. Details of this calculation are in Appendix C.4. We find that the duration elasticity is 0.06 to the $600 supplements and 0.10 to the $300 supplements using estimates from the time-series design, and 0.11 and 0.22 using estimates from the difference-in-difference design. While elasticities using difference-in-difference estimates are larger, overall Figure 8 shows that these duration elasticities are significantly smaller than estimates in the prior literature surveyed in Schmieder and von Wachter (2016). These findings are consistent with the hazard elasticities reported in Bell, Hedin, and Schnorr (2022), Petrosky-Nadeau and Valletta (2021), and Coombs et al. (2022).[24]

Figure 8: Pandemic Elasticity Estimates Compared to Prior Literature



Notes: The pre-pandemic estimates are from the literature review by Schmieder and von Wachter (2016).

4.4 Additional Job-Finding Results and Robustness

Appendix C.5 shows various robustness results and extensions, which we briefly summarize here. First, we include additional fixed effects so that identification comes only from comparing the job-finding rate for workers with different $PctChange$ who are in the same state, are the same age, and worked in the same industry. Second, we show that similar results obtain when re-estimating a weekly event study design rather than our baseline specification which pools together all pre- and post-weeks. Third, we show that specifications which look at proportional changes or explicitly model exit as a binary outcome deliver similar conclusions.

Fourth, we analyze how the supplements affect recalls. While it appears that the expiration of the $600 supplement might have had a small effect on recalls, this evidence is hard to interpret, and

[24]Other studies which tend to find small effects but do not report hazard or duration elasticities include Bartik et al. (2020), Finamor and Scott (2021), Holzer, Hubbard, and Strain (2021), and Marinescu, Skandalis, and Zhao (2021).

even the upper bound of plausible causal impacts on recalls still implies small aggregate employment effects. There is no evidence of any effect of the \$300 supplement on recalls.

Finally, we estimate the disincentive effects of benefits on job finding using the expiration of the \$300 supplements in September 2021. The analysis of this policy change is complicated by the simultaneous expiration of expanded benefit eligibility (PEUC and PUA programs). This complication ultimately reduces precision of our estimates substantially. Nevertheless, we find point estimates which are similar to those that we estimate for the start of the \$300 supplements.

5 Model

In this section, we develop a model that helps us interpret the reduced form results along three dimensions. First, it helps us better gauge whether the effects we measure are big or small. Second, it lets us explore whether accounting for dynamics changes our interpretation of the reduced-form empirical results. For example, if households gradually increase search as the expiration of supplements approaches, then the change in job finding at expiration would understate the supplement's overall effect. Third, it helps us to understand what effects are likely to generalize beyond the pandemic and to what extent alternative policies would have had different effects.

5.1 Model Description

Our model combines an incomplete markets consumption-savings problem with a model of costly job search. Each of these elements is intentionally standard because part of the goal of the model is to understand what a standard model calibrated to pre-pandemic evidence predicts about the effect of supplements on spending and job finding. We describe the main elements here and provide additional details in Appendix D.1.

Households choose consumption c and savings a to maximize Constant Relative Risk Aversion utility, subject to a no-borrowing constraint $a \geq 0$. When employed, a household i receives a constant wage w_i. w_i differs across households but is constant over time for each household. When employed, households face a constant exogenous separation rate into unemployment.

When unemployed, households find a job with probability $f_{i,t} = recall_t + search_{i,t}$ where $recall_t$ is a common exogenous recall rate and $search_{i,t} \in [0, 1 - recall_t]$ is household $i's$ endogenous choice of search effort. Search effort induces additive disutility $\psi(search_{i,t})$. When unemployed, households receive regular unemployment benefits, which are proportional to w_i. Regular benefits last for 6 months in normal times but are extended to 12 months at the start of the pandemic.

Beginning from this initial steady state, the economy is hit by a sequence of aggregate policy changes that capture changes in UI policy over the pandemic: weekly supplements of \$600 are added to unemployment benefits from April-July 2020 and then \$300 supplements are added from January-August 2021. We focus on the evolution of unemployed households relative to employed households in both the model and data to remove pandemic effects that affect everyone equally.[25]

[25]For example, the model includes stimulus checks and transitory changes in impatience to match the large increase in savings in April 2020, but these play little role in our analysis and so we leave their discussion to Appendix D.1.

5.1.1 Modeling Choices: Disciplining Job Search Costs, Expectations, and Impatience

Most of our parameter choices follow the prior literature. We summarize them in Table A-3 and discuss them further in Appendix D.1. Here, we discuss three key model components: search costs, expectations, and impatience. For each component, we examine two scenarios. First, we study a "standard" calibration which targets empirical evidence from the pre-pandemic literature and makes assumptions about beliefs which are standard in the literature. This model has low job-search costs, perfect foresight, and moderate discounting. However, relative to the data this model produces too much anticipatory behavior and too much saving. We therefore also study an "alternative" calibration with higher search costs, myopic expectations, and high discounting in order to fit these data patterns.

Cost of Job Search: We assume that $\psi(search) = k_0 \frac{(search)^{(1+\phi)}}{1+\phi} + k_1$ and pick the search cost parameters in one of two ways. In the standard calibration, we calibrate search costs to generate a monthly new job-finding rate of 0.28 to match the JPMCI data before March 2020 and a benefit duration elasticity of 0.5 to match the median estimate from Schmieder and von Wachter (2017). In the alternative pandemic calibration, we instead calibrate search costs to minimize squared deviations between the model and data time-series of new job-finding over the course of the pandemic.[26]

Expectations: We must make an assumption about how long households expect supplements to last. In the standard calibration, we assume that households have perfect foresight (which, by construction, is correct ex post) about the length of supplements. In the alternative "myopic expectations" calibration, households instead expect supplements to continue through the end of their benefit spell, and they are then surprised when supplements actually end.

Impatience: We also calibrate the discount factor in one of two ways. In the standard calibration, we assume a normal level of patience and set $\beta = 0.99$ monthly (an 11 percent annual discount rate) to generate a 3-month MPC of 0.25 in response to a \$500 stimulus check sent to all households. Kaplan and Violante (2022) argues that macro models should target a value for this MPC of 0.15-0.25. Havranek and Sokolova (2020) and Orchard, Ramey, and Wieland (2022) argue that this empirical range may be overstated. We purposely choose an MPC at the high end of empirical estimates to be conservative, since even using this high target generates spending responses to UI which are too small. In the alternative high impatience calibration, we instead pick $\beta = 0.98$ (a 22 percent annual discount rate) to target the 1-month MPC of 0.42 in the waiting for benefit receipt design.

5.1.2 Model Fit Comparisons

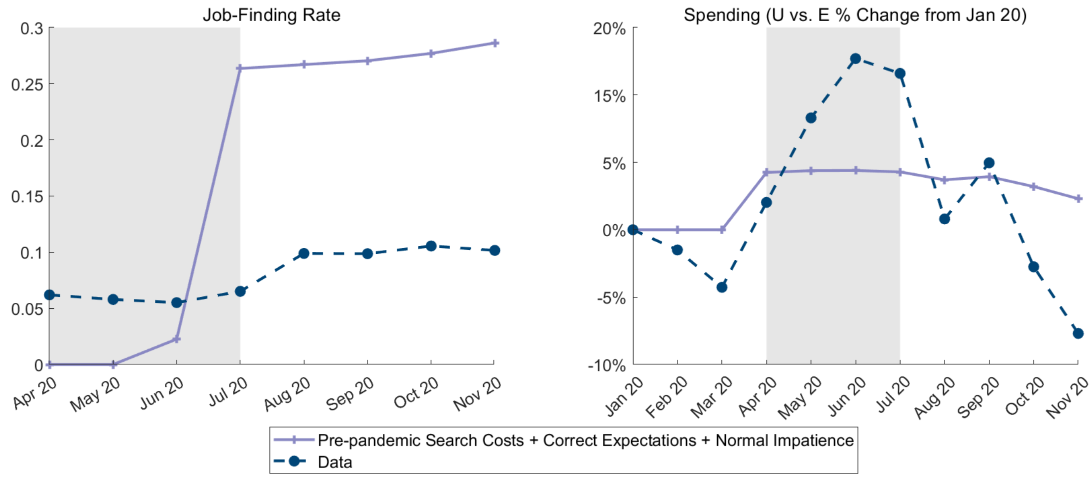
Figure 9a shows that the standard model calibration is a poor fit to both job-finding and spending patterns. This model calibrated to match pre-pandemic job-search behavior predicts that almost no workers take a new job while the generous supplements are in place, and that job-finding rises sharply when they expire. But the data shows much more muted patterns. In contrast, the opposite is true with respect to spending. Even though this model is calibrated to match the upper end of MPC estimates to \$500 stimulus checks, it still implies responses to supplements which are much smaller than the data. This is especially true when the \$600 supplements end: households in this model with perfect foresight anticipate the end of supplements and thus save to smooth consumption.

Panel 9b shows the effect of progressively introducing each of the three alternatives discussed in Section 5.1.1. The model in red shows the effect of re-calibrating *search costs* to target the pandemic

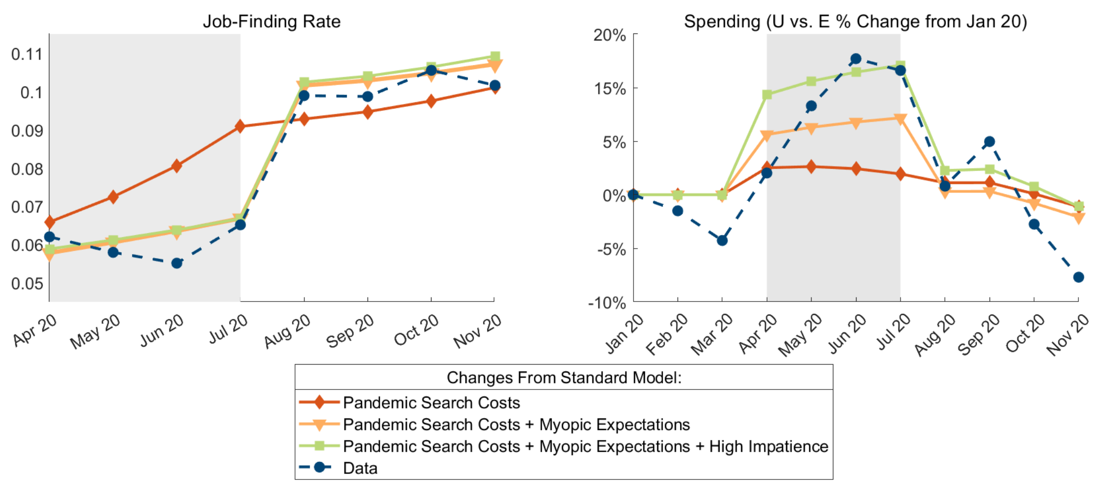
[26] We assume constant search costs over the pandemic, so we have 3 parameters and 11 targets for monthly job-finding (Apr 2020-Feb 2021).

Figure 9: Job-Finding and Spending Responses to $600 Supplement: Models vs. Data

(a) Standard Model w/Pre-Pandemic Search Costs



(b) Alternative Models



Notes: This figure shows monthly time-series of various models vs. the data in response to the $600 supplements. The left panels show the new job-finding rate (based on converting Figure 4 to a monthly frequency) and the right panels show the spending of unemployed (as a ratio to employed, based on Figure 1). The standard pre-pandemic model calibrates to pre-pandemic evidence on duration elasticities and MPCs out of stimulus checks. The alternative models all calibrate search costs to match the level of job-finding in the data and then the three models vary in the expectations about the renewal of the $600 and in what MPC is targeted.

job-finding series instead of pre-pandemic duration elasticity estimates. Because the level of the job-finding rate even with no supplements is so much lower than than during normal times, and the change in the job-finding rate in response to large changes in supplements is so modest, the calibration that best matches these patterns implies that job search was more costly and less responsive to monetary incentives during the pandemic. This model is by design a better fit for the job-finding rate than the pre-pandemic model, but it implies that both the job-finding rate and spending evolve too smoothly: if households anticipate that the $600 will expire in August, they begin searching more in the months before that, and they smooth consumption in response to this predictable decline in income.

We next change from perfect foresight to *myopic expectations*, so that households are surprised by each change in supplements. The orange line shows that this model is capable of generating a sharp jump in the job-finding rate, and roughly symmetric spending responses to the start and end of supplements, as in the data.

Nevertheless, spending responses in this model are still too small. Both the increase in spending while the supplements are in effect and the decrease in spending when they expire in August are about half that in the data. Notably, households in the data remain highly sensitive even after they have accumulated significant liquidity. We therefore need some force above and beyond temporarily low liquidity to explain why the MPC of unemployed households is greater than the MPC of the general population (as measured by stimulus check MPCs).[27]

Hence, in the third and final change to the standard pre-pandemic model we *increase impatience* of unemployed households relative to the general population to target the reduced form MPC of 0.42 when waiting for benefits. This calibration requires unemployed households to have discount rates twice as high as those of the general population (22% annually versus 11%). The green line shows that this "best fit" model which includes all three changes closely matches time-series patterns for both job-finding and spending.[28]

Figure A-19 shows that this best-fit model with all three changes is also a good fit to the $300 supplements in January 2021. Furthermore, an additional untargeted job-finding moment is consistent with the best-fit model. Although we pick search costs in the best fit model to target the time-series for the average job-finding rate, Figure A-20 shows that this best-fit model also matches the results from the difference-in-difference research design for job-finding, which provides additional validation for the linearity assumption in the difference-in-difference design.

Thus, the key elements necessary to jointly fit empirical job-finding and spending patterns are more costly job search during the pandemic, unemployed households that behave more impatiently than the general population, and unemployed households who act as if they are surprised by changes in benefits. Although these three elements are departures from the standard pre-pandemic model, each seems plausible relative to actual household behavior.

First, job search may have indeed been more costly during the pandemic. For example, face-to-face interviews were logistically challenging and working conditions were more difficult. This modeling choice echoes the decline in search efficiency in the model by Mitman and Rabinovich (2021).

Second, the relationship between unemployment risk and impatience that we need to fit the data is consistent with the correlation between ex-ante unemployment risk and MPCs shown in Patterson (2023). The fact that a high degree of impatience among the unemployed can help explain their spending patterns is also consistent with the models of present-bias in Ganong and Noel (2019) and Gerard and Naritomi (2021).[29]

[27] In Appendix D.5, we provide more quantitative decompositions of the specific forces like targeting and persistence which shape the MPC out of stimulus checks relative to the MPC out of UI.

[28] The model overstates spending of unemployed relative to employed in March and April 2020 because of two measurement issues. First, most unemployed households experience a delay of a few weeks between job loss and the start of benefits. Second, the $600 weekly supplement is not paid out until halfway through April. Appendix E.1 and Figure A-18 show that our conclusions are unchanged in a more complicated model that is fit to these high frequency patterns immediately after job loss.

[29] However, as we discuss in Appendix E.2, these models miss the pandemic patterns unless they further introduce myopic expectations. Specifically, a model with heterogeneity in present-bias and search costs but with correct expectations implies spending responses which are asymmetric relative to the data (responding much more to the start than to the end of the $600 supplements) and implies a temporary spike rather than a sustained increase in job finding when the $600 supplements expire. Nevertheless, after assuming myopic expectations, a model with high exponential

Third, the myopic expectations needed to fit the data imply that households respond strongly today to policy changes today but respond little today to policy changes in the future. There are three ways to rationalize this pattern: 1) The policy changes may have truly been surprises, even to those paying attention. 2) Households may pay limited attention to policy news even after it is announced. 3) Households may pay attention to policy news but nevertheless fail to fully adjust their behavior due to some behavioral frictions. Without explicit data on expectations, these three interpretations are observationally equivalent. Although there was substantial policy debate (and therefore uncertainty) surrounding the expiration of the $600, there was no similar debate around the expiration of $300 supplements in September 2021. This suggests that information or behavioral frictions are probably more likely explanations for the consistently large spending responses than are explanations based on legitimate legislative uncertainty.

5.2 Magnitudes

We now use the model to quantify how supplements affected job-finding and spending during the pandemic, including accounting for dynamic effects. For job-finding, we estimate a duration elasticity of 0.07 in response to the $600 supplements and 0.11 in response to the $300 supplements. These estimates are similar to the ones in Section 4.3 which do not allow for dynamics. This implies that job-finding dynamics in the model are of limited quantitative importance, a point to which we return below in Section 5.3.1.

Table 4: Effects of Supplements on Spending

	$600 supplement	$300 supplement
1-month MPC out of 1st month of supplements	0.29	0.31
3-month MPC out of 1st 3 months of supplements	0.38	0.34
Total MPC through month supplement ends	0.40	0.46
Total MPC through 3 months after supplement ends	0.53	0.60

Using the model we can measure the effect of supplements on spending at different horizons. Table 4 reports spending effects over various horizons in the best fit model. Defining Δc_t and Δy_t as the difference in consumption and income for a household with and without supplements in month t, we compute $\frac{\sum_{t=1}^{T} \Delta c_t}{\sum_{t=1}^{T} \Delta y_t}$ for various values of T. In particular, for each supplement we show results for T equal to 1 month, 3 months, the length of the supplement period, and the length of the supplement period plus 3 months. It is these longer horizon MPCs which most directly answer policy questions about the overall effects of supplements on spending of the unemployed, but these cannot be reliably measured in the data.

We find one-month MPCs of 0.29 and 0.31 to the $600 and $300 supplements respectively. The share of all supplements spent in the period while the supplements are still in place is even higher, with an MPC of 0.40 and 0.46 to the $600 and $300 supplements. By three months after supplement expiration, MPCs rise to 0.53 and 0.60. As already noted in Section 5.1.2, these MPCs are large

discounting or a model with present-bias in time preference can be equally successful in fitting the spending data.

relative to *quarterly* MPCs out of stimulus checks of 0.15-0.25. We discuss this comparison in more detail in Appendix D.5.

These MPCs imply that households who received the supplement for all the weeks it was available increased their spending by several thousand dollars. A worker unemployed from April through July 2020 receives \$10,200 in \$600 weekly supplements. \$4,080 is spent by the end of July and \$5,400 is spent by the end of October.

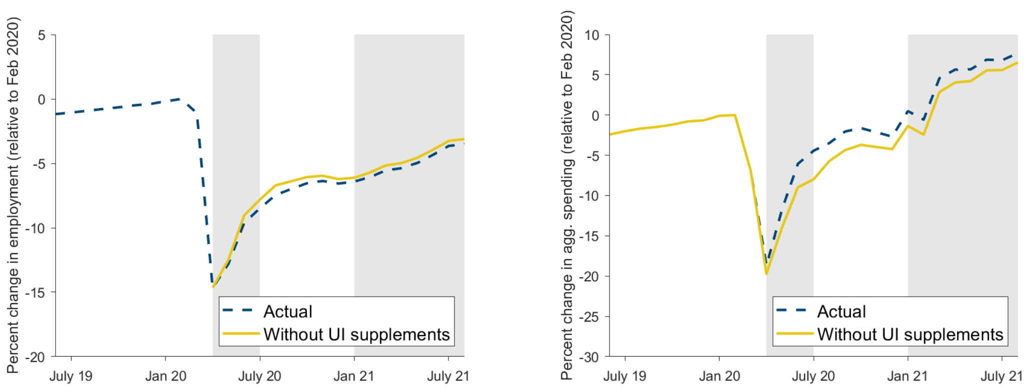
Drawing on these individual effects (small for job-finding and large for spending), it is natural to then ask how the supplements affected aggregate fluctuations in employment and spending during the pandemic. In Figure 10 we use the best fit model scaled by the total number of workers receiving unemployment benefits in the data to generate a simple partial equilibrium counterfactual for aggregate employment and spending over the course of the pandemic had there been no \$300 or \$600 supplements.[30]

Figure 10 shows that although the supplements had some negative effects on employment, these effects were small relative to overall employment changes during the pandemic. The \$600 weekly supplements from April 2020 to July 2020 reduced employment by an average of 0.6% while the \$300 supplements reduced employment by an average of 0.4%. Overall this amounts to only around 5% of the overall employment gap generated by the pandemic, so these supplements played a small role in explaining aggregate employment dynamics during this time period.

The effects of supplements on spending were three to five times larger than their effects on employment. Specifically, the \$600 and \$300 supplements boosted aggregate spending by an average of 2.7% and 1.5%, respectively. This means that supplements helped to close a large fraction of the aggregate spending gap during the pandemic.[31] Before they expired at the end of July 2020, the supplements were responsible for 21% of the spending recovery during the pandemic.

Figure 10: Aggregate Implications

(a) Employment (b) Spending



Notes: This figure shows aggregate employment and spending dynamics implied by the best-fit model. We first estimate individual effects and then aggregate up these individual effects by scaling by the number of workers receiving benefits at each date to arrive at a (partial equilibrium) aggregate effect. The left panel compares to total employment from the BLS Bureau of Labor Statistics (2019 - 2021) establishment survey while the right panel compares to PCE spending from the Bureau of Economic Analysis (2019 - 2021).

[30]Since the payroll survey excludes self-employed workers we reduce the counts of benefit recipients when scaling employment effects by the number of self-employed PUA UI recipients in DOL data.

[31]We emphasize that this is the *micro* effect of the supplements, aggregated up to the level of the entire economy. It is possible that general equilibrium channels could further amplify or dampen the aggregate impact of the supplements.

5.3 Understanding Mechanisms

Now that we have quantified the effects of supplements on job finding and spending, we use the best fit model as well as additional empirical work to shed light on three additional questions: First, why are dynamic job-finding effects limited? Second, why are job-finding effects smaller than most prior estimates (as shown in Figure 8)? Third, what can we learn about the role of permanent heterogeneity versus temporary low liquidity for explaining high MPCs?

5.3.1 Limited Job-Finding Dynamics

In the model, the potential for dynamic search effects arises from two sources: anticipatory search and liquidity accumulation.[32] However, both effects end up being quantitatively small.

The fact that there are limited dynamics from anticipatory search follows immediately from the fact that the model only fits the spending and job-finding time-series if households are surprised by policy changes. The lack of anticipatory dynamics is thus explained simply by the fact that households do not adjust behavior in advance of policy changes they do not anticipate.

Another potential concern about dynamics arises from liquidity effects on job search. Prior research finds that unemployment benefits reduce job search in part by relaxing liquidity constraints (Card, Chetty, and Weber 2007; Chetty 2008). Figure 1 shows that the supplements were associated with a large increase in liquidity for the unemployed. If the job-finding rate remains depressed *after* the \$600 supplement expires because of this elevated liquidity, then the research designs in Section 4 will understate the full effect of the supplements. We use two complementary approaches to address this concern.

First, we show that this bias is small in the model because liquidity constraints only affect search when they bind. Liquidity is already elevated by April 2020 when supplements start and then grows further from there, so the additional liquidity accumulation caused by supplements has little additional effect on search.[33] The model implies that supplements reduce the monthly job finding rate by 3.6% just prior to expiration (capturing both the effect of incentives and the effect of liquidity) and by only 0.16% after expiration (capturing only the effect of liquidity). Thus, liquidity effects account for only 4% of the total causal effect in this context.

Second, we study empirically how the estimates in Section 4 vary with liquidity. Using a triple-difference design, Tables A-4 and A-5 show that a one-standard deviation increase in liquidity is associated with a decline in the disincentive effect of the \$600 from -.0163 to -.0135, and with a decline in the disincentive effect of the \$300 from -.020 to -.0186. A simple extrapolation from this cross-sectional heterogeneity in treatment effects to the time-series of liquidity for the unemployed during the pandemic implies that liquidity accumulation reduces the disincentive coefficient $\hat{\beta}$ by only 5%, from -.017 in April 2020 to -.0164 in July 2020.[34] This 5% estimate based on cross-sectional heterogeneity is in line with the 4% estimate from the structural model.

[32] We note that it is possible that other forces not captured by the model such as changing aggregate conditions might also lead to effects of supplements which vary over time. However, we note that the dynamic event study specification shown in Figure 6 also suggests that effects in the data are relatively constant over time.

[33] The model includes borrowing constraints, and it replicates the finding from the prior literature that liquidity has important effects on job search. Specifically, it replicates untargeted results from Card, Chetty, and Weber (2007) that two months of severance pay reduces the subsequent log job-finding hazard by 0.076-0.109. Performing this same exercise in the model delivers a value of 0.076, so the model yields credible liquidity effects.

[34] Mean checking account balances increase \$1,388 for unemployed relative to employed households from April to July 2020, and the standard deviation of balances in the regression sample is \$4,897. Thus -.0164-(1328/5018)*.0022=-.017.

5.3.2 Small Employment Magnitudes

Four forces explain why the unemployment duration elasticity we estimate is lower than estimates in the prior literature. First, the supplements we study are temporary. Second, the supplements are implemented in a labor market with a depressed job-finding rate. These two forces are likely to also be relevant in "normal" (non-pandemic-induced) recessions. Third, the large share of recalls further dampens the effect of the supplements. Fourth, the per-week behavioral response to the supplements while they are in effect (the hazard elasticity) is also lower than in prior times. The analysis is summarized in Figure 11, which use five dots—with four transitions from the first dot to the fifth dot—to quantify the importance of each force.

The most important conclusion from Figure 11 is that there is a mechanical wedge between duration and hazard elasticities which varies with supplement length. For example, if the hazard elasticity is one, then permanently doubling benefits will double the average duration of unemployment. Thus, the duration elasticity will also be one.

In contrast, the shorter the supplement, the more that effects on the *total* duration of unemployment spells are muted relative to effects on the *per-week* hazard rate of re-employment. As an extreme example, suppose that the hazard elasticity were so large that job-finding dropped to zero when supplements were in place. The effect on unemployment duration would be negligible if the benefit increase lasted only for a single day.[35] The past literature typically studies the effect of relatively long-lived benefit changes where the mechanical wedge is small and there is little distinction between the hazard and duration elasticities. In our context, this distinction matters.

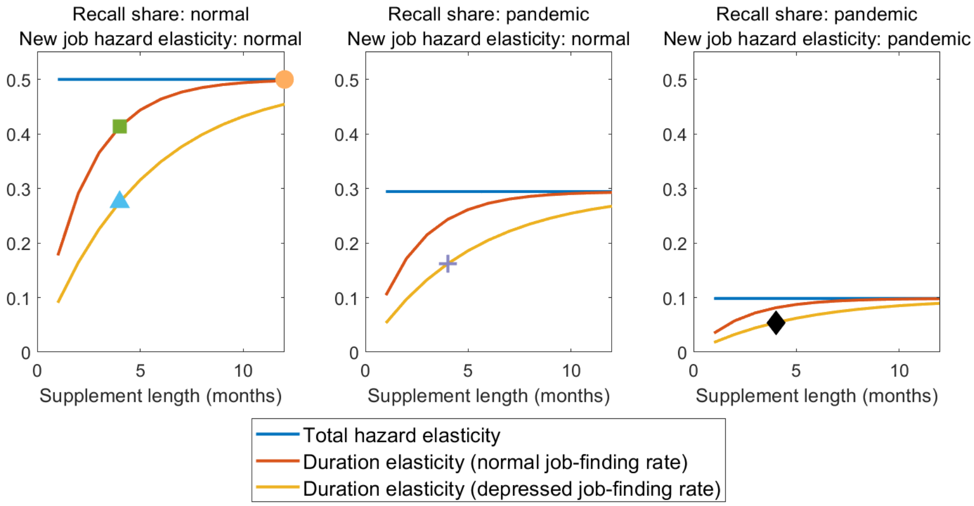
The left panel of Figure 11 quantifies the extent to which distortions shrink as supplement length becomes shorter. We choose a job-finding hazard elasticity of 0.5, to match pre-pandemic estimates from the past literature and the horizontal blue line captures this hazard elasticity.[36] The orange circle captures the response to long-lived benefit changes, and the red line shows the implied duration elasticity for shorter supplements. A mechanical wedge between the duration elasticity in red and hazard elasticity in blue emerges since a change in the hazard translates less than one-for-one into a change in duration if that change is short-lived. The green square at four months corresponds to the length of the $600 supplement and shows that the short length of the supplement alone can explain a reduction in the duration elasticity from 0.5 to about 0.4.

Second, the effect of temporary supplements is further diminished in a setting where the baseline job-finding rate is depressed. Suppose that a large UI supplement cuts the job finding rate in half. Reducing the job-finding rate from 50% to 25% for one month will lead to larger growth in the duration of unemployment than will reducing the job-finding rate from 5% to 2.5% for that month. The yellow line in Figure 11 repeats the same exercise as the red line but with a level of the baseline job-finding rate chosen to match the depressed job-finding rate during the pandemic (in fall 2020 when no supplements were in place). When the baseline job-finding rate is low, the hazard elasticity converges more slowly to the duration elasticity and so the wedge is larger for any supplement length. The blue triangle shows that the duration elasticity in response to a four-month supplement is then around 0.25, even though the hazard elasticity remains at 0.5. Accounting for these first two forces—short supplements and a depressed job-finding rate—explains about half of the difference between the

[35] We further explore the likely effects of a one-day benefit increase (also known as severance pay) in Section 6.

[36] To simplify intuition, we analyze these effects under the assumption of a constant hazard elasticity, both over time and as a function of supplement length. In models of optimal search, the hazard elasticity itself would decline as the length of supplements declines, amplifying the conclusion that short supplements lead to lower duration elasticities.

Figure 11: Forces for the Low Unemployment Duration Elasticity



Notes: The figure computes the duration elasticity to supplements of different lengths. Within each panel we compute the duration elasticity under a normal pre-pandemic level of the total job-finding rate as well as for a depressed job-finding rate. Within a panel we do not change the recall share as we move from the normal to the depressed scenario, meaning both recall and new job-finding rates decline proportionately. Moving from the left panel to the middle panel, we raise the recall share from its pre-pandemic to its pandemic value. Moving from the middle panel to the right panel, we lower the new job hazard elasticity from its pre-pandemic value to its pandemic value. This means that the yellow line in the right panel corresponds the full set of pandemic forces while the red line in the left panel corresponds to the full set of normal conditions.

pre-pandemic duration elasticity of 0.5 and the pandemic duration elasticity of 0.07.

In addition to these first two forces—which should remain relevant in "normal" recessions outside the pandemic—there are additional forces for generating the low duration elasticity that are pandemic-specific. The third force for a low duration elasticity is the large recall share of total exits during the pandemic. We assume that recalls are insensitive to changes in benefit levels based on both institutional constraints as well as empirical evidence in Appendix C.5. If a larger fraction of exits from unemployment are insensitive to benefit increases, then these increases will generate smaller employment distortions. The purple cross in the middle panel of Figure 11 shows that the elevated recall share in the pandemic combined with the depressed pandemic level of job-finding implies a duration elasticity of 0.18 in response to a four month supplement. Fourth and finally, the job-finding effects that we estimate in Table 3 imply a lower per-week hazard elasticity than implied by pre-pandemic estimates.[37] The right panel of Figure 11 recomputes the results with this lower hazard elasticity and shows that this reduced sensitivity of search to benefits during the pandemic lowers the duration elasticity of a four month supplement from 0.18 (the purple cross) to 0.07 (the black diamond). This black diamond corresponds to the $600 supplement duration elasticity in our best fit model and completes the steps moving us from the pre-pandemic yellow dot to our pandemic estimate.

[37] In prior drafts, we explored whether any worker-level observable characteristics like age (as a proxy for health risk), the presence of kids (as a proxy for childcare constraints) or differential recall rates help account for the reduction in the hazard elasticity but ultimately found no systematic evidence of heterogeneity. Since the small behavioral response to supplements does not appear to be driven importantly by any of these obvious channels, we think that the most likely remaining explanation is a shift induced by the pandemic's effect on working conditions, including exposure to health risk (which may be only imperfectly proxied by age), discomfort from masking, and more challenging customer interactions.

5.3.3 High MPCs: Permanent Heterogeneity vs. Transitory Low Liquidity

Our spending results have implications for consumption modeling. Many papers document an empirical correlation between MPCs and low liquidity, and a large literature has developed models in which liquidity constraints play a key role in generating high MPCs. For example, the leading two-asset precautionary savings models are able to simultaneously match the distribution of wealth and the high MPC out of stimulus checks in the data, because many households have high wealth but little liquidity (see Kaplan and Violante 2022 for a review of this literature). However, it is hard to know whether temporary low liquidity is the primary cause of high MPCs, as these models typically assume, or if some households with permanently high propensities to spend will save less and therefore have low liquidity.

Differentiating "temporary low liquidity causes high MPCs" from "permanently high MPCs cause low liquidity" is challenging because both liquidity and MPCs are endogenous. The large pandemic UI transfers generate a unique, quasi-random increase in liquidity. The spending responses that we observe in this environment imply that permanent heterogeneity—and not just temporary low liquidity—is important for explaining high MPCs. Three empirical results lead to this conclusion.

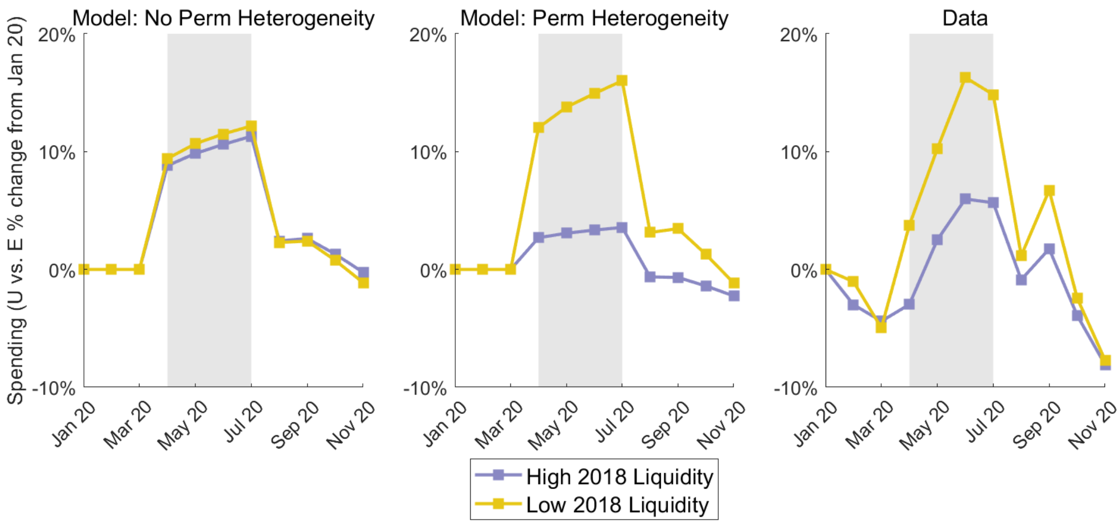
First, we find large MPCs to UI supplements throughout the pandemic, even though unemployed households have much higher than normal liquidity during this time. The median household who becomes unemployed during the pandemic moves *up* an entire tercile of the pre-pandemic liquidity distribution (from the 35th percentile to the 65th percentile). Indeed, the fact that the replacement rate with supplements is above 100% means that the typical unemployed household has temporarily *high* rather than temporarily low liquidity during the pandemic. The fact that MPCs remain large even in this high liquidity environment suggests that the people who became unemployed in the pandemic have high MPCs for some reason above and beyond low liquidity. Indeed, the precise reason that the best-fit model requires unemployed households to be more impatient than the general population is to hit the fact that the MPC remains high even when supplements push households into this temporarily high liquidity state.

Second, as discussed in Section 3.3, liquidity measured years before the pandemic predicts higher MPCs to UI supplements throughout the pandemic. In a model where temporary bad luck is the source of low liquidity and high MPCs, one's liquidity position years in the past should have little impact on future MPCs. This is especially true if some specific "good luck" occurs in between.

As an extreme example, consider a lottery winner. Whether they were short on cash before winning the lottery should have no impact on their post-lottery-winning MPC. This same logic applies when all households experience a policy-driven liquidity increase, as is the case at the start of the pandemic. The fact that low past liquidity predicts high future MPCs, even after large UI transfers erase this low liquidity state, implies that this pattern must reflect some permanent household characteristic and not just temporary low liquidity.

To illustrate this more concretely, we return to the best-fit model. First, to evaluate the view that "temporary low liquidity causes high MPCs", we split households into two groups and exogenously vary liquidity across the two groups in March 2020 to match the variation in pre-pandemic liquidity underlying Figure 3. Figure 12 shows that the model predicts very similar spending behavior for both groups. This is because even though this model (by construction) matches the substantial empirical heterogeneity in pre-pandemic liquidity, once households begin receiving $600 supplements, they are no longer liquidity constrained. At that point, no further source of heterogeneity remains in this model to drive heterogeneous spending effects.

Figure 12: Spending Responses by Liquidity: Data vs. Alternative Models



Notes: This figure compares spending by liquidity in two alternative models and in the data. Each line shows the spending of unemployed relative to the spending of employed households for a particular liquidity group. The model with no permanent heterogeneity splits unemployed households in the best-fit model into two groups and exogenously varies the liquidity state to match the variation across groups in pre-pandemic liquidity in the data. The model with permanent heterogeneity varies the discount factor across groups to match this same variation. See text for details. In the right panel, the yellow data line is the ratio of the two lines in Figure 3 panel (a) while the purple line is the ratio of the two lines in Figure 3 panel (b). The average of the yellow and purple lines is not equal to the spending response in Figure 9b because here we match treatment and control groups on liquidity.

Second, to evaluate the "permanently high MPCs cause low liquidity" view, we again divide unemployed households into two groups and this time vary the discount factor across the two groups to match the same variation across groups in pre-pandemic liquidity. Both versions of the model thus match the heterogeneity in liquidity in the data, but the model with permanent heterogeneity does so by varying discount factors across groups while the model with no permanent heterogeneity does so by varying only households' liquidity state variable. Figure 12 shows that the model with permanent heterogeneity in discount factors implies substantial heterogeneity in spending responses by pre-pandemic liquidity, in line with the data.[38]

Finally, the "permanently high MPCs cause low liquidity" view is supported by a third dimension of the data on pre-pandemic liquid assets. Specifically, we split the sample into two groups: workers who become unemployed during the pandemic and those who do not. We then measure liquidity *prior* to the pandemic, when both groups are still employed. Even when employed pre-pandemic, households who later become unemployed have about 25% less liquidity conditional on income than households who remain employed, suggesting those households have a lower propensity to save. Thus, finding low liquidity for households who will become unemployed directly connects the first piece of evidence in this section which compares across groups (high MPCs of unemployed relative to employed) to the second piece of evidence which compares within-group (among those who become unemployed).

[38] Note that liquidity still also matters for MPCs in the model. With no discount factor heterogeneity and in a normal environment (no supplements and no pandemic-induced liquidity increases), the MPC of unemployed households is 0.3 larger than that of employed households, reflecting the role of current income and liquidity. Adding discount factor heterogeneity raises the gap to 0.44, implying that two-thirds of the MPC variation with unemployment status in the best-fit model is explained by current economic circumstances, while one-third is due to permanent characteristics.

Together, this evidence strongly points to permanent heterogeneity: some households have high propensities to spend. These spenders are going to spend when they have money, and so they will generally also have low liquidity. In this sense, low liquidity is a symptom of persistent propensities to spend and not just a transitory state.

Our result that permanent heterogeneity is important for explaining spending patterns complements recent research arriving at similar conclusions using a different source of variation and methodology. Athreya, Mustre-del Río, and Sánchez (2019), Gelman (2021), Calvet et al. (2022) and Aguiar, Bils, and Boar (2020) document empirical patterns in household panel data, which they show require permanent heterogeneity when viewed through the lens of structural household consumption models. Parker (2017) uses Nielsen data to show that high MPCs are correlated with survey measures of impatience and lack of financial planning as well as income from years in the past. These studies use observational data in normal economic environments, so they do not have exogenous variation in liquidity and must therefore rely on more structure to infer causality. In contrast, we study an environment with a large quasi-random increase in liquidity as an identifying source of variation.

6 Policy Implication

The mechanisms driving the small employment effects and large spending effects of UI supplements have a policy implication that is likely to extend beyond the pandemic. Specifically, short-lived "severance-like" UI supplements that trigger on during recessions could provide boosts to aggregate demand without much distortion to job finding.

6.1 A New Countercyclical Motive for Temporary Supplements

In the classic Baily-Chetty formula, it is optimal to provide greater insurance when resulting employment distortions are smaller. We show that short-lived benefit increases during recessions are likely to induce small employment distortions. If workers are only eligible for increased benefits for a small part of their spell, this will have a small effect on job search decisions. Indeed, pure severance payments that pay out only at the start of an unemployment spell should not distort job search decisions.[39] Furthermore, the job-finding rate typically declines during recessions. Even if the hazard elasticity is constant, the fact that job-finding rates typically fall during recessions provides a rationale for temporary countercyclical benefits. When the no-supplement job-finding rate falls, the wedge between duration and hazard elasticities grows (as shown in Figure 11). This means that the duration elasticity—which is the welfare-relevant cost of expanding benefits—is lower for any given level of the hazard elasticity, and this pushes up optimal benefit levels.

This conclusion is complementary to two prior strands of work on optimal benefit levels over the business cycle. First, the hazard elasticity may be lower in recessions.[40] Second, Schmieder, von Wachter, and Bender (2012) show that even if the behavioral response to more generous benefits (in the form of longer Potential Benefit Duration) is constant over the business cycle, the welfare-relevant distortion is smaller. This is because PBD extensions mechanically affect more workers when the

[39] If households are liquidity constrained, then severance payments may also reduce inefficiency (Chetty 2008).

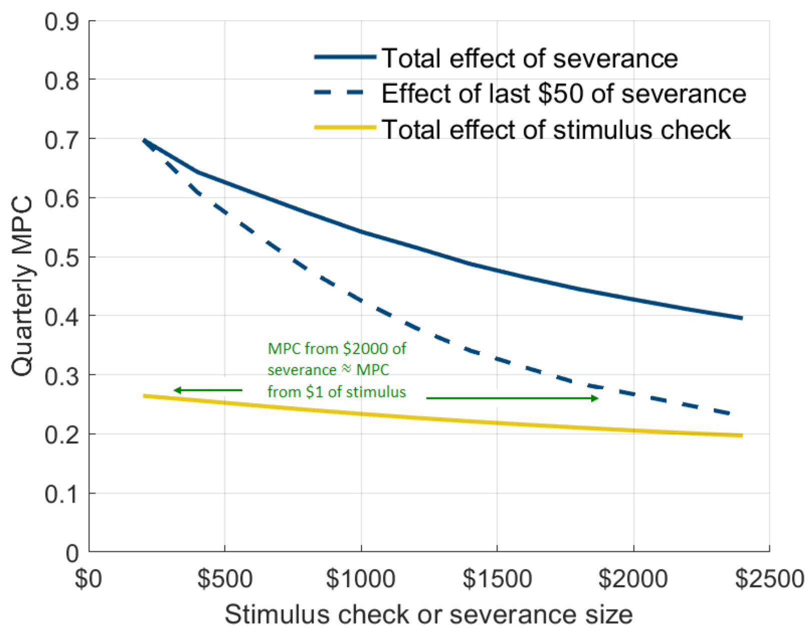
[40] Kroft and Notowidigdo (2016) evaluate this channel by studying how the *hazard* elasticity changes with the unemployment rate (they then make the common assumption that this hazard elasticity is the same as the *duration* elasticity). They find that the hazard elasticity falls modestly as the unemployment rate rises, suggesting that UI benefits should be slightly more generous during recessions. See also Landais, Michaillat, and Saez (2018b).

job-finding rate is depressed. Although we study a different temporary benefit policy (supplements), we similarly find a smaller distortion in recessions for the same underlying economic reason: the ratio of the behavioral cost (which is constant) to the mechanical cost (which rises as more households benefit from the expansion) is lower.

6.2 Aggregate Demand Management via Targeted Payments

For spending, targeted one-time stimulus to unemployed households ("severance") can boost aggregate demand more cost effectively than universal one-time payments to all households ("stimulus checks"). To explore the effects of severance payments, we analyze the effects of one-month supplements. We do this in a model environment that removes any pandemic-specific effects in order to more closely approximate a typical recession. Since employment effects from severance are minimal, we focus on the spending impacts. We focus on how the spending impacts of severance (which is targeted to the unemployed) compares to the spending impacts of alternative universal stimulus payments (which go to the population as a whole), as a way to evaluate their potential as tools for aggregate demand management. This analysis is shown in Figure 13. The solid lines compare the average quarterly MPC out of a severance to the MPC out of an equal-sized stimulus check, for an individual receiving each transfer. The dashed line shows the marginal effect of the last $50 of severance.

Figure 13: Spending Impacts of Severance vs. Untargeted Stimulus



Notes: This figure shows the quarterly spending responses to severance as well as to one-time stimulus checks of various sizes in a non-pandemic environment with normal liquidity levels. The x-axis compares severance payments and untargeted stimulus of equal size and the y-axis reports the quarterly MPC. We compute these responses in the best-fit model with discount factor heterogeneity. We calibrate the degree of heterogeneity in this model so that it still produces a quarterly MPC out of $500 stimulus checks of 0.25. Solid lines show MPCs out of the entire transfer while the dashed line shows MPCs out of the last $50 of UI.

We find that the spending impact of severance is larger than the impact of universal stimulus checks, although this difference declines with the size of the transfer. The impact of severance is larger than that of universal stimulus checks because severance targets individuals with a high propensity to spend while universal stimulus checks do not. The difference is largest for small transfers, because when transfers are small the unemployed are temporarily liquidity constrained *and* have a high propensity to spend for any level of liquidity due to the permanent heterogeneity discussed in Section 5.3.3. For large transfers, unemployed households are no longer liquidity constrained and only the effect of targeting those with permanently high propensities to spend remains.

The combination of low pre-transfer liquidity and high persistent propensity to spend at any liquidity means that even large severance payments targeted specifically to the unemployed may be beneficial relative to untargeted stimulus. Figure 13 shows that the spending impact from the last dollar of a $2,000 one-time payment to the unemployed is larger than the spending impact of the first dollar of untargeted stimulus.

The conclusion that severance pay is an attractive way to stimulate aggregate demand builds on two prior strands of the literature on unemployment benefits. First, it expands on Gerard and Naritomi (2021), which shows that unemployed households have a high propensity to spend severance payments. Second, severance payments can be interpreted as a means of front-loading UI benefits, and in this sense our results relate to several other papers which argue for front-loading (Shavell and Weiss 1979; Hopenhayn and Nicolini 1997; Mitman and Rabinovich 2021). Lindner and Reizer (2020) provides empirical evidence showing front-loading benefits led to shorter durations. Our analysis of severance adds a novel motive for front-loading, which is that it is an effective way to stimulate aggregate demand.

The analysis in this section is relatively simple, but it demonstrates the potential power of temporary UI supplements that are triggered by recessions. This is potentially a much cheaper way of providing fiscal stimulus than unconditional stimulus checks. However, there are two caveats worth emphasizing. First, severance pay is not targeted to the long-term unemployed who are most in need of insurance. A full analysis of optimal policy would incorporate both the aggregate demand motive we study and the insurance motive studied in much of the prior literature. Second, the larger the "severance-like" payment, the larger incentive there might be for employees and employers to collude, generating false terminations to claim this benefit. Such a policy would therefore need to be carefully designed to mitigate this risk.

7 Conclusion

We use administrative bank account data to estimate the causal effects of the largest UI expansion in U.S. history. Our reduced-form research designs and dynamic structural model deliver consistent conclusions: expanded benefits had large effects on spending but small effects on job finding. The small job-finding effects were driven in part by the fact that supplements were temporary and implemented in an environment with an already depressed job-finding rate. The large spending effects were driven in part by the fact that they were targeted towards households with high spending propensities. These conclusions have lessons for future policy design: countercyclical severance-like payments should be considered alongside stimulus checks as an additional instrument for fiscal stimulus.

These conclusions also suggest some avenues for future research. First, it would be useful to understand what the behavioral characteristic *is* that gives rise to differences in consumption behavior (and is correlated with unemployment risk). This is important for determining whether other forms of targeted stimulus might achieve the same ends without working through the UI system. Second, it would be interesting to understand *why* households had myopic expectations about changes in UI supplements. If households exhibit myopic expectations with respect to other policy changes, this likely alters trade-offs between current and future actions and has important consequences for the design of dynamic policies.

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

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A Additional Institutional Detail and Data Description

A.1 Simultaneous Policy Changes in the Summer of 2021

Nearly every state terminated expanded benefit eligiblity (PEUC) at the same time that they terminated the \$300 weekly benefit supplement in the summer of 2021.[41] The presence of two simultaneous policy changes is a conceptual challenge for trying to estimate the separate effect of the \$300 supplement. Unemployed workers who are still eligible for regular benefits will see a decline in benefits of \$300 per week (call this treatment 1) while those who were only eligible under PEUC will lose their benefits entirely, and hence will see a decline in benefits of \$300 + regular benefits (call this treatment 2).

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

A.2 States Included

We divide states into three groups:

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

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

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

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

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

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

A.3 Unemployment and Employment Spells

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

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

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

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

A.4 Sample Restrictions

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

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

A.5 Additional Variable Detail

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

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

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

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

B Appendix to Section 3 (Spending Responses)

B.1 Summer MPCs

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

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

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

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

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

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

For both Figure 3 and Table 2, we define the employed control groups by matching on income quintile and stimulus check timing (as in our other MPC results) and *also* by (above or below-median) liquidity. We then re-weight the above-median and below-median liquidity unemployed to have the same income and stimulus check timing as the average unemployed household in the data. We do this so that our analysis captures the differences across the two liquidity groups in liquidity and is not driven by confounding differences in income or stimulus check timing.

B.3 MPC Robustness

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

First, we decompose our MPCs into separate categories. This is useful for understanding how benefit expansions affected households' lives, for gauging whether MPCs might be driven by pandemic-specific forces which might not generalize to other recessions, and for assessing whether spending responses to UI might differ from more general spending patterns. Table A-6 reports the MPCs on the subset of spending transactions we can assign to granular categories based on merchant category codes (for card transactions) and text descriptions (for electronic transactions). The category with the largest response was groceries, accounting for around 20% of the increase in categorized spending in each episode.

In general, the MPCs do not appear to be driven by any unusual spending patterns. In particular, this table shows that the share of the MPC explained by each category is similar to these households' spending shares across categories in 2019, prior to the pandemic.[42] That is, *marginal* spending responses by category are similar to *average* spending levels across categories. While there are some shifts in spending relative to pre-pandemic patterns, these mirror aggregate trends and return to more normal patterns later in the pandemic. For example, unemployed households in April 2020 have an elevated share of spending on home improvement, but this is not surprising since overall spending on home improvement went up early in the pandemic. By 2021, spending patterns out of UI are close to pre-pandemic spending patterns.

While our analysis focuses on spending responses to UI, it is also interesting to look at effects on debt. Table A-7 shows that the marginal propensity to repay auto, mortgage, and student loan debt is very small. This may be a consequence of various debt moratoria during the pandemic reducing incentives to pay down debt. The fact that debt paydown is slightly larger for car payments than for mortgage loans and student loan debt is consistent with this explanation. We note that there is a separate literature that often finds sizable marginal propensities to repay debt in response to stimulus checks. It would not be particularly surprising if unemployed households valued liquidity more than employed households and so were less likely to pay down debt than the population as a whole. Furthermore, we note that we cannot distinguish revolving credit card payments (i.e. debt paydown) from non-revolving credit card payments (i.e. spending paid off within the month) so we do not include credit card bill payments in this table. It is possible that households used some of the UI benefit supplements to pay off revolving credit card debt, which are unable to measure.

Second, we explore the robustness of our results to controlling for more observable differences between the unemployed and employed since any deviations between these groups not driven by supplements would contaminate our MPC estimates. All of our MPC results control for the pre-separation income of the unemployed as well as for the timing of stimulus checks because we view these as the most salient identification threats. Table A-8 shows the results of controlling for additional observable characteristics that potentially differ between unemployed and employed. This table re-computes MPCs progressively adding controls for location, for age, and for the presence of children, which we think of as capturing other potential differences that might affect spending patterns during

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

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

Third, spending patterns are nearly identical for unemployed households who are recalled and those are who are not, suggesting that spending responses are not driven by the unusually high recall rates during the pandemic. For example, Figure A-22 selects all households who are continuously unemployed from April 2020 through August 2020 and exit unemployment in September 2020 (the first month after $600 supplements expire) and then further splits these households into those who return to their old employers ("Exit to recall") and those who do not ("Exit to new job"). This figure shows that spending patterns over this entire period are nearly identical for both groups, suggesting that different patterns of recall are not driving spending patterns.[43] We obtain similar conclusions if we repeat these comparisons of recalled and non-recalled households exiting in each month from June 2020 through November 2020.

Fourth, our results are also robust to limiting the sample to households for whom we are likely to observe a more complete lens on spending. For example, many Chase customers have non-Chase credit cards. We will understate MPCs if these households increase spending on non-Chase credit cards when receiving supplements. Table A-9 shows that when we limit the sample to households with no ACH payments made towards non-Chase credit cards, MPCs are two to five cents higher. This suggests that the presence of non-Chase credit cards leads us to slightly understate MPCs in our baseline sample. As further evidence of this point, we have computed the response of non-chase credit card debt payments to supplements and find an increase in payments of 0.04. As noted when we discussed debt payments, we cannot separate these marginal payments between revolving and non-revolving credit. However, it is likely that at least some of this increase in debt payments is a result of increased spending rather than increased pay down of revolving credit on non-Chase credit cards.

As a second example of a potential bias arising from having an incomplete lens on spending, Chase customers may make some debt payments using paper checks, which our methodology will misattribute as spending (because we do not observe the content of account outflows where the payment method is paper check). This might lead us to overstate the MPC. Table A-9 therefore reports a robustness check which limits the sample to households which use their account for debt payments via ACH—thereby mitigating the concern that paper checks are mistakenly including some debt payments as spending—and finds that MPCs are indeed slightly (zero to four cents) lower than the baseline estimates but still large.

In addition, although our baseline MPC estimate follows much of the prior literature by looking at means, Table A-9 shows that estimates using the median change in spending and income results in MPCs that are two to seven cents higher.

Fifth, our conclusions are robust to alternative spending measures. For example, using the narrow *card and cash* measure of spending which is less subject to concerns about misclassification still delivers large spending responses. Table A-10 shows that MPCs out of this more narrow spending measure

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

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

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

C Appendix to Section 4 (Job-Finding Responses)

C.1 Measuring Weekly Benefit Amount

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

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

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

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

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

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

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

Identification in the dose-response difference-in-difference design requires three assumptions.

First, we make the standard orthogonality assumption: $\varepsilon_{it} \perp SuppAvail_t, PctChange_i$. The economic content of this assumption is that high and low-wage workers (who differ in $PctChange_i$) would have had the same trend in job-finding absent the policy change. This assumption has a testable prediction: parallel trends prior to the policy change. Figure 6 shows that the data are consistent with this assumption for the exit rate to new jobs. While this parallel pre-trend is reassuring, one might still be concerned about differential labor market trends for high and low-wage workers due to the uneven incidence of the pandemic across industries, locations, and workers of different ages, all of which are potentially correlated with wage levels. However, in Appendix C.5, we show that nearly identical conclusions obtain when exploiting only within state-age-industry group variation. In addition, we note that if there were a persistent difference in job-finding trends (e.g., low-wage workers have faster employment growth because of business reopenings) then the bias in $\hat{\beta}$ will have opposite signs across the two policy changes because one is a decrease in benefits while the other is an increase in benefits.

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

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

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

C.3 Comparison of Estimates Across Research Designs and Episodes

Comparing the estimates of job-finding disincentive effects across episodes and research designs requires re-scaling the four estimates (two research designs and two policy changes) into common units. Comparing within a given policy change, the interrupted time-series estimates tell us the average effect of the entire supplement while the difference-in-difference estimates tell us the effect of a marginal change in $PctChange$ across workers. We convert from the difference-in-difference estimate $\hat{\beta}$ to an estimate of the average effect of the entire supplement by assuming a homogeneous treatment effect over the entire range $[0, E(PctChange_i)]$ and computing $\hat{\tau} = \hat{\beta}E(PctChange_i)$. We note that this is a stronger assumption than that in the previous sub-section (Appendix C.2) since $min(PctChange_i) > 0$, and so this requires linear extrapolation out of sample.[44]

Two types of evidence bolster the plausibility of such an extrapolation. First, within the empirical variation available in the data, the relationship between the intensity of treatment (size of the change in benefits) and the outcome (change in the exit rate) appears to be linear (Figures 7a and 7b). Second, in the best-fit model, the effect of the supplement on the job-finding rate is also close to linear, as shown in Figure A-20.

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

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

sibility for why time-series estimates are smaller is if the "micro" disincentive effect of unemployment benefits (the effect of giving one worker more benefits) is bigger than the "macro" disincentive effect (the effect of giving all workers more benefits).[45] The macro effect includes additional equilibrium effects.[46] The difference-in-difference estimate, which compares changes in the job-finding rate for more- and less-treated workers, may be closer to a micro elasticity, while the interrupted time-series estimate, which measures the change in the job-finding rate for all workers, may be closer to a macro elasticity.

C.4 Calculating Duration Elasticities

The duration elasticity is defined as the percentage change in expected unemployment duration caused by a given change in benefit size divided by the percentage change in benefits:[47]

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

where ED is the expected duration of unemployment. We now describe the calculation of these objects in more detail. Call the total exit hazard observed in the data (which includes the effect of the supplement when it is in place) $\lambda_{t,\text{with supp}} = e_t + recall_t$, with observed new job-finding rate e_t and observed recall rate $recall_t$. We assume e_t and $recall_t$ are constant at their sample averages after the end of the observed data.

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

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

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

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

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

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

and the average duration without supplements is given by

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

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

Together this procedure gives the numerator of ϵ. The denominator is more straightforward since we know the size of supplements and can directly measure the benefit amount in the data.[48]

C.5 Additional Job-Finding Results and Robustness

C.5.1 Robustness

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

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

This enables an event study interpretation of the coefficients. Figure A-23 shows that treatment effects from the expiration of the \$600 are largest in the three weeks after the policy expires and smaller in the subsequent five weeks. This suggests that long-term effects of expiration on the weekly job-finding rate may be even smaller than the baseline estimates from equation (4) which pool the eight weeks after expiration in Table A-2. The figure also shows a stable treatment effect from the onset of the \$300.[49]

In a second group of checks in Tables A-11a and A-11b, we re-estimate equation (4), adding controls X_i and $X_i SuppAvail_t$ to address concerns about differential trends by group. First, we add state (and state-by-supplement-available) fixed effects, so that identification comes from comparing the job-finding rate for higher- and lower-wage workers with different benefit replacement rates in the same state. Second, we add age (and age-by-supplement-available) fixed effects, so that identification comes from comparing the job-finding rate for higher- and lower-wage workers with different replacement rates who are in the same state and are the same age. Third and finally, we add industry (and industry-by-supplement-available) fixed effects. In this richest specification, identification comes from comparing the job-finding rate for higher- and lower-wage workers with different replacement rates who are in the same state, are the same age, and worked in the same industry. Our estimates of $\hat{\beta}$ change little from incorporating these control variables.

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

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

Third, we explore the robustness of our results to alternative functional forms for estimating job finding effects. Our baseline empirical specification estimates a dose-response difference-in-difference design using a linear probability model, which assumes a constant relation between the absolute change in the exit rate (in percentage points) and the relative change in benefits. This empirical specification is supported by our theoretical model, as shown in Figure A-20. However, the most common empirical specification for estimating disincentive effects of UI is a Cox proportional hazard model, which instead assumes constant proportional rather than absolute effects. The Cox model cannot be used in our setting where policy changes affect unemployed workers in the middle of a spell. However, we can explore the robustness of our results to alternative specifications which estimate relative rather than absolute outcomes (i.e. using the percent change in exit rates rather than the absolute change in exits as the outcome). We explore two alternative specifications. The first is a binned version of our baseline regression which uses 10 deciles of the percent change in benefits but switching to the percent change in exit as the outcome. This specification allows us to measure percent changes for our binary exit outcome, but it collapses the micro data to bins. The second specification is a logit specification which lets us use the micro data with percent changes in outcomes. Table A-12 shows that these alternative specifications produce very similar conclusions to our baseline empirical specification. Duration elasticities remain low and very similar to the baseline specification. Furthermore, duration elasticities are all below hazard elasticities, as implied by the discussion in Section 5.3.2.

C.5.2 Recalls

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

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

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

Difference-in-difference results cast further doubt on the possibility of substantial effects of supplements on recalls. Figure A-17e shows that recall is higher for the high replacement rate group after the \$600 supplement expires, but *not* in the three weeks when the aggregate recall rate is elevated. Instead, the recall rate for the high replacement group only rises differentially in the subsequent six weeks. Table A-13a finds a $\hat{\beta}$ coefficient for recall that is about two-thirds of the size of the exit to new job coefficient. However, the figure illustrates that this effect is again short-lived, implying that even if this delayed differential recall response is causal, it has a small effect on aggregate employment patterns.

The difference-in-difference evidence that the $300 had tiny effects on recall is even more clear cut. Figure A-17f shows that there is little difference in recalls between workers with above- and below-median replacement rates. Table A-13b re-estimates equation (4) and reports a coefficient (0.003) that is one-seventh of the already small effect on new job-finding and economically indistinguishable from zero.

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

C.5.3 Effects in Summer 2021

We replicate the job-finding analysis from Section 4.2 for the expiration of the $300 supplement. We are unable to replicate the interrupted time series analysis because of the contemporaneous expiration of PEUC (discussed in Appendix A.1).[50] To illustrate this, Figure A-24a shows that the exit rate from UI rises sharply to extremely high levels in September 2021 as many workers mechanically lose benefits when PEUC expires.

We thus focus our analysis on a subset of workers who are ex ante likely to be in the regular UI benefit program (not PEUC) and thus exposed only to the supplement expiration. To do so, we limit the sample to workers who have received UI benefits for no more than four weeks as of July 11, 2021.[51] A worker who received UI benefits continuously through the end of October 2021 would therefore only have received 20 weeks of benefits and therefore receive regular UI benefits (and not PEUC) throughout the analysis period.

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

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

Table A-14 shows regression estimates as well as several related specifications. Depending on whether we define the maximum allowed observed duration in July as four or eight weeks and whether

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

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

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

D Additional Model Details and Results

D.1 Model Setup

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

Income for an unemployed household depends on aggregate UI policy and whether they are waiting for benefits. Regular benefits last 6 months. Benefits for a household newly unemployed during the pandemic last 12 months.[52] Benefit levels depend on the current aggregate UI supplement in place: $m \in \{0, 300, 600\}$. To speak to our empirical research design, we allow for the possibility that an unemployed household may face delays in receipt of UI and in turn later receive backpay. This means unemployed households can be in one of four receipt statuses: $d \in \{normal, delayed, backpay, expired\}$.

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

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

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

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

This describes the evolution of actual policy through this period, but we must also specify expectations. We assume the initial switch from $m = 0$ to $m = 600$ is unanticipated.[53] Once the 600 is

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

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

implemented, households know for sure that it will last at least 4 months. In our main results, we consider two different specifications for expectations about m after these 4 months.[54] In the perfect foresight specification, households correctly expect that m will revert from 600 to 0 in August. In the alternative myopic expectation specification, households instead expect that $m = 600$ for the duration of their remaining benefit spell and are then surprised in August when it expires. Once $m = 600$ expires in August, households expect that $m = 0$ forever. For the \$300 weekly supplements, we study a newly unemployed household in November of 2020. They either anticipate or are surprised that the $m = 300$ supplement begins in January, 2021.[55] Once the \$300 supplement begins, households anticipate that supplements will expire in September 2021.

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

Households anticipate a constant recall rate throughout the pandemic, although results are similar if we instead assume perfect foresight over the actual recall rate. We deal with pandemic effects in two ways: First, we focus on the evolution of unemployed households relative to employed households in both model and data. This means any effects of the pandemic which affect all households equally are effectively removed. Second, we directly model several pandemic events. We introduce a one-month discount factor shock to all households in April 2020, which we calibrate to match the decline in spending for employed households during the pandemic.[56] Since we focus on the behavior of unemployed households relative to employed households, this shock has little effect on our conclusions, but it means that we do a better job of hitting absolute spending and liquidity changes over the pandemic rather than just matching relative changes. We also introduce additional one-time unanticipated transfers to replicate stimulus checks in April 2020 and January 2021 as well as LWA payments in September 2020, but this again has little effect on our conclusions.

Letting n represent the expected number of periods until $m = 0$, the household optimization problem of a household unemployed during the pandemic can be written as:[57]

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

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

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

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

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

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

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

D.2 Model Parameters

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

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

We pick the remaining parameters of the model to target more complicated model objects using an indirect inference procedure to choose the discount factor and simulated method of moments to choose the search cost parameters.[58]

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

We assume that $\psi(search_t) = k_0 \frac{(search_t)^{(1+\phi)}}{1+\phi} + k_1$ and pick the parameters of this search cost function in one of two ways. In our "pre-pandemic" calibration, we calibrate search costs to generate a pre-pandemic job-finding rate of 0.28 and an elasticity of average unemployment duration to a small 6 month change in benefits of 0.5. This is the median estimate from the Schmieder and von Wachter (2017) meta-analysis. Since we only have two moments and three parameters, the pre-pandemic calibration is not identified without additional restrictions and so we impose $k_1 = 0$.[59]

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

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

The search cost parameters are most interpretable in terms of implications for job search elasticities. The calibrated pre-pandemic search cost parameters imply a job search hazard elasticity to a small benefit change for 6 months of 0.66 and the best-fit search cost parameters imply a hazard elasticity of 0.29.[60]

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

D.3 Model Solution

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

to account for statistical uncertainty. In our context there is little cross-sectional uncertainty since we have very large samples, and we have no aggregate shocks in our model so there is no aggregate uncertainty. Thus, statistical uncertainty over parameters in the model would be approximately zero.

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

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

of employment and unemployment. Given optimal policies, we then simulate the model for 1,000 households of each of each wage group (5,000 households total) and compute average statistics.

D.4 Best Fit Model - Additional Results

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

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

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

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

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

Table A-15 shows that each of these forces is important for understanding the spending responses to supplements. Each row varies one element at a time to illustrate the forces shaping spending responses to supplements during the pandemic. The first row shows the one month MPC out of combined regular benefits plus the $600 supplement ($2,400 monthly) for an unemployed household who is currently receiving no benefits during the pandemic in our best fit model. This combination of elements replicates the empirical environment for the waiting design in Section 3.2.1. Since our best-fit model is calibrated to match this MPC, the model MPC equals the empirical MPC of 0.42 by construction. This MPC mixes the spending responses to regular benefits and supplements. Thus, in row 2 we compute the model MPC to supplements alone, which are targeted at a household already receiving regular unemployment benefits. This MPC of 0.29 corresponds to the supplement MPC in Table 4. We note that this value is essentially the same as the untargeted MPC of 0.30 and 0.26 to the end of the $600 and start of $300 supplements estimated in Section 3.

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

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

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

Summarizing the results from this Appendix, persistence and targeting towards households with high propensities to spend are key forces for high MPCs out of supplements. Their large size decreases the MPC since it relaxes liquidity constraints, but on net the first two forces (i.e. persistence and targeting) dominate and so spending responses to UI supplements are much larger than spending responses to stimulus checks. This conclusion should generalize beyond the pandemic, since these forces are not pandemic-specific. Indeed, this conclusion is likely to be even stronger in a more normal recession when aggregate spending is not reduced due to pandemic-specific reasons (i.e., Table A-15 row 3 is greater than row 2).

E Alternative Models

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

E.1 Time-Aggregation Issues

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

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

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

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

E.2 Alternative Behavioral Models without Myopic Expectations

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

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

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

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

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

The reason this model does not generate a sustained increase in job finding is because of composition effects. Panel (c) shows that the jump in job finding is driven by the hyperbolic type with low elasticity of search costs (the orange line). This group barely searches when supplements are in place and then searches hard after supplements expire. However, since this type searches at much higher rates after expiration relative to the exponential type, it exits more rapidly and so its share of the population declines, as shown in Panel (e). This composition effect leads to a decline in the average job finding rate which is inconsistent with the data.[61]

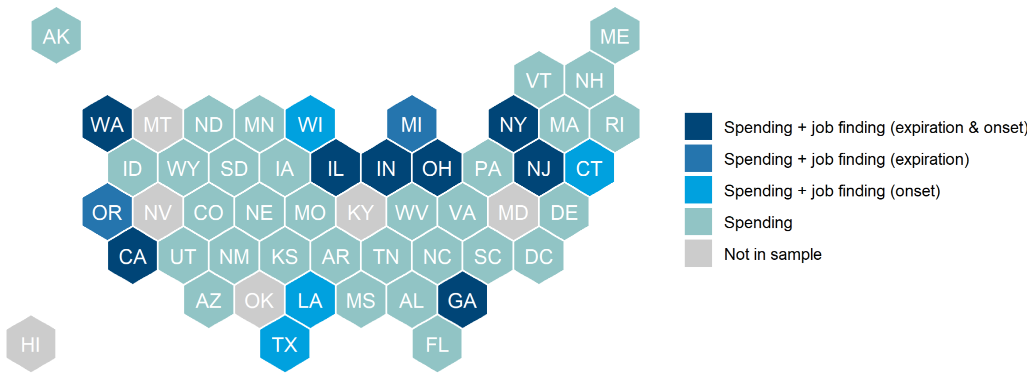
The reason this model implies counterfactual asymmetry in spending is because households respond more strongly to an unexpected change in income (the start of $600) than to an expected change in income (the end of $600 with perfect foresight). This is especially true for the standard consumers without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

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

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

Additional Figures and Tables

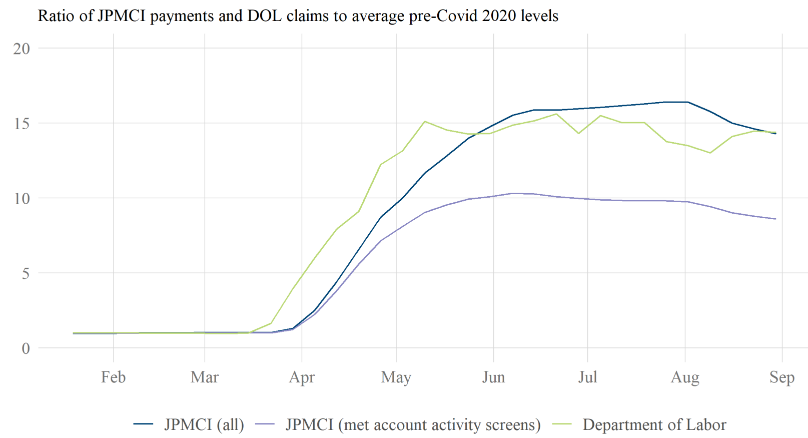
Figure A-1: States included in the JPMCI sample



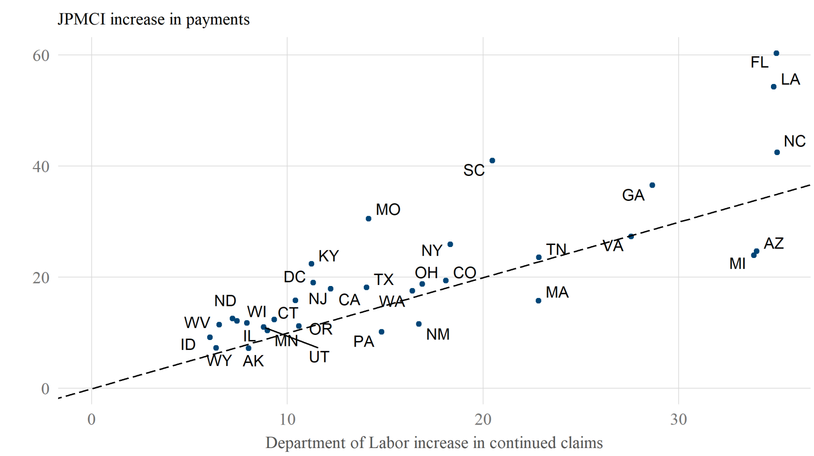
Notes: This figure shows the subset of states which are included in various analyses. See Appendix A.2 for details.

Figure A-2: UI Claims in JPMCI versus DOL

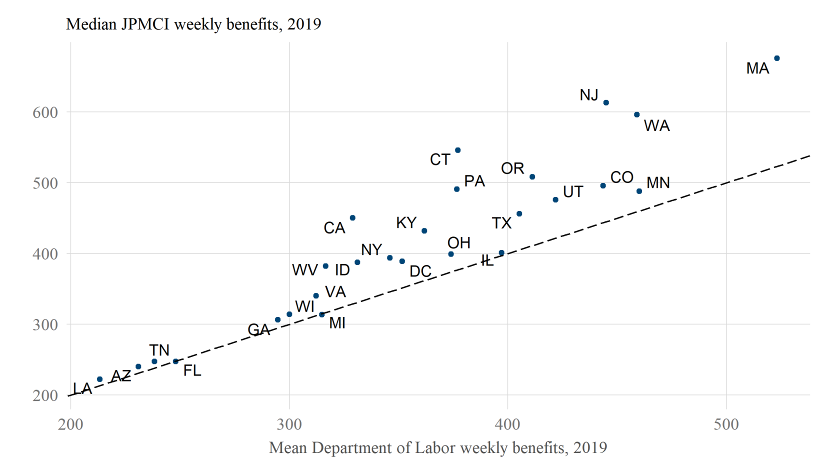
(a) Time-series



(b) State-level Change at Pandemic Onset

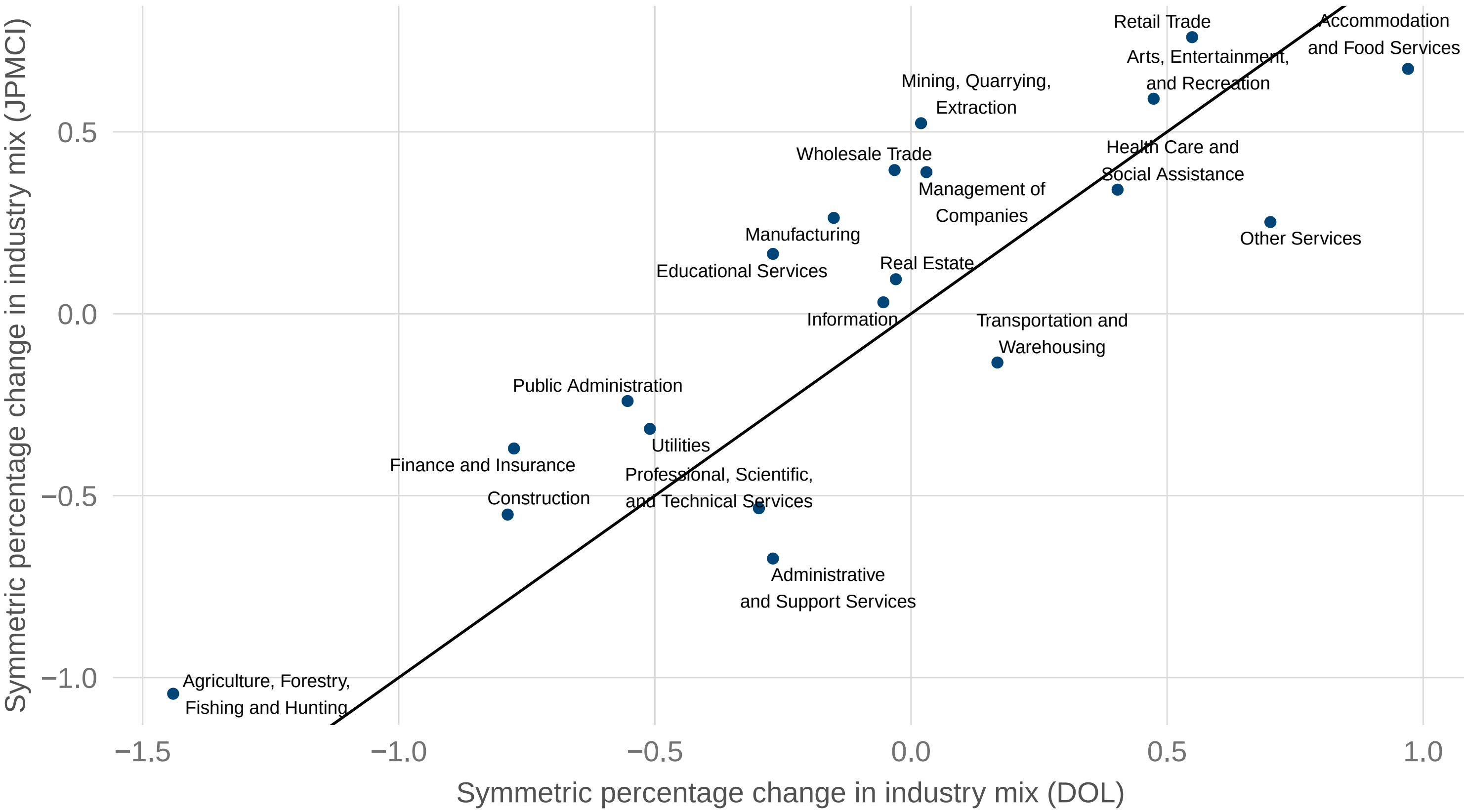


(c) Weekly Benefit Amount Pre-Pandemic



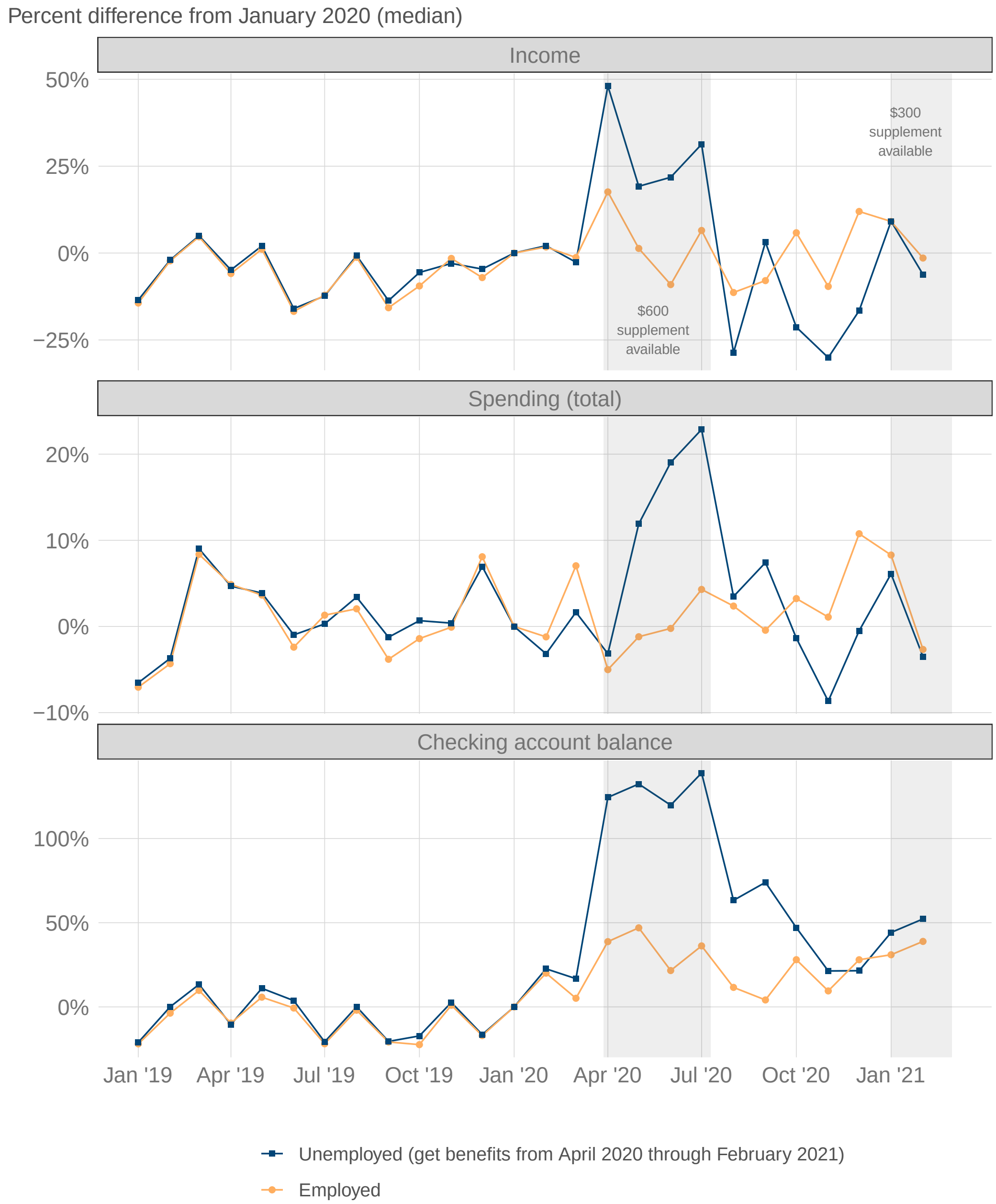
Notes: This figure compares total claims and benefits levels in JPMCI data to Department of Labor ETA Form 539 Employment and Training Administration (2019 - 2020c) and Form 5159 Employment and Training Administration (2019 - 2020b). Panel (a) compares aggregate time-series patterns, panel (b) compares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

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



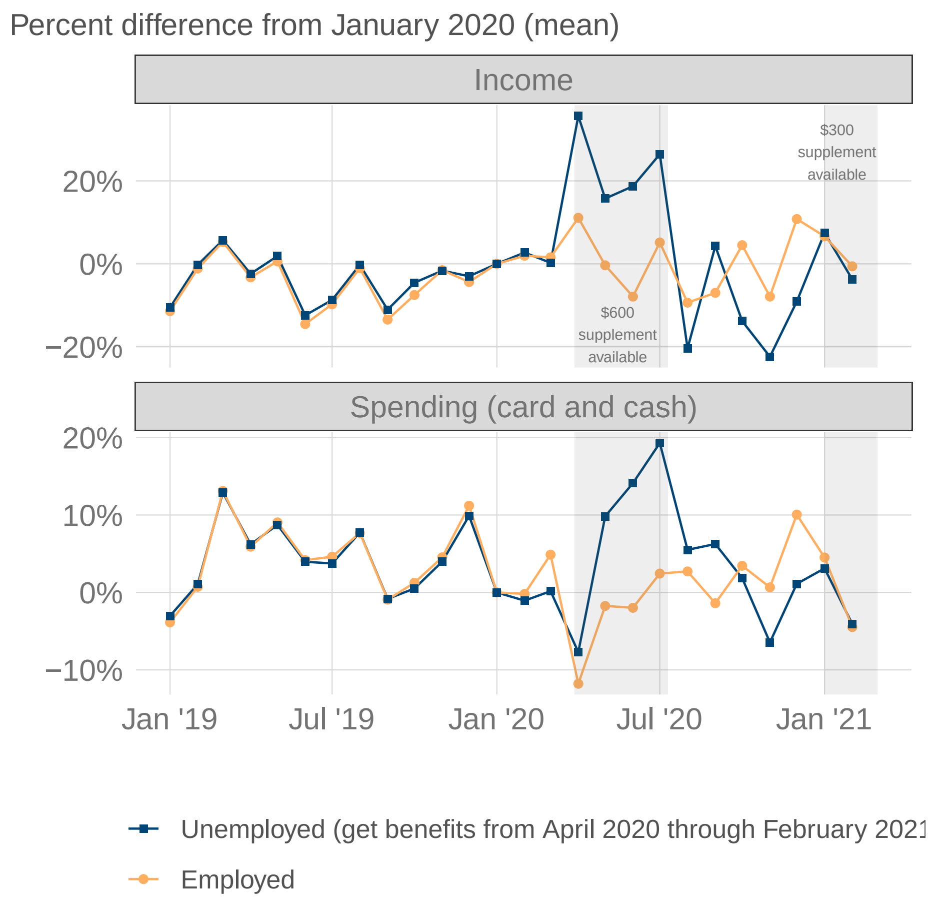
Notes: This figure compares the change in unemployment composition in DOL to JPMCI. The diagonal line is a 45-degree line. Making these data sets comparable requires two adjustments. Letting i denote industry, t denote period and s state, we start by defining total JPMCI claims as c_{it}^{JPMCI} and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

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



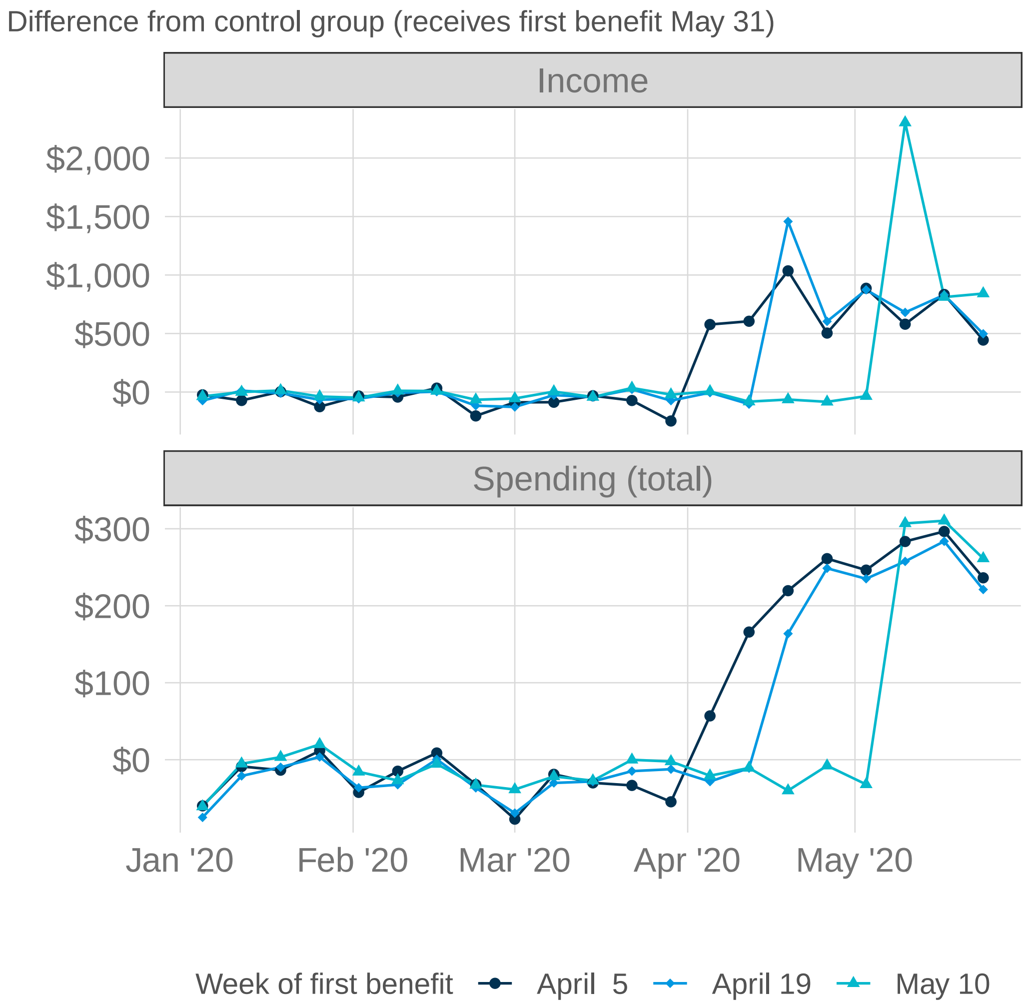
Notes: This figure replicates Figure 1 using sample medians instead of means.

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



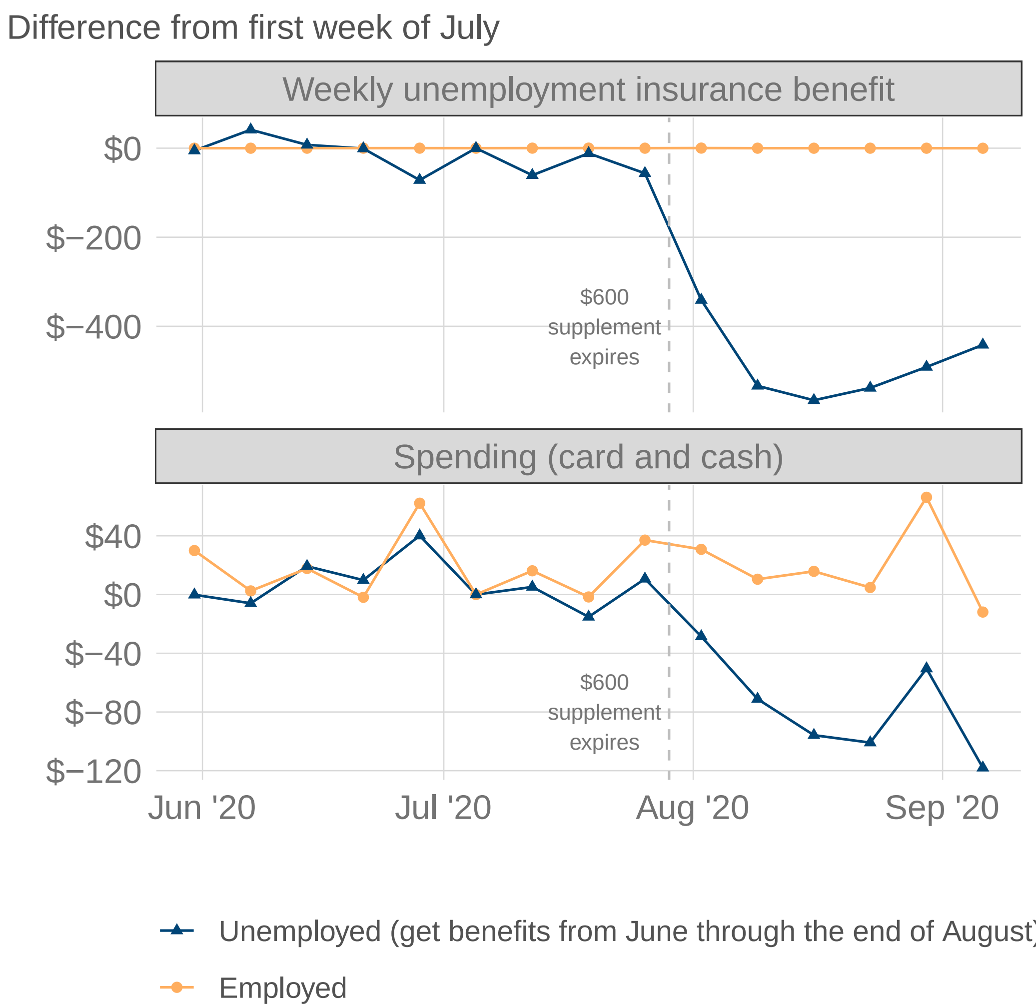
Notes: This figure shows that the spending (total) patterns in Figure 1 also hold for spending (card and cash).

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



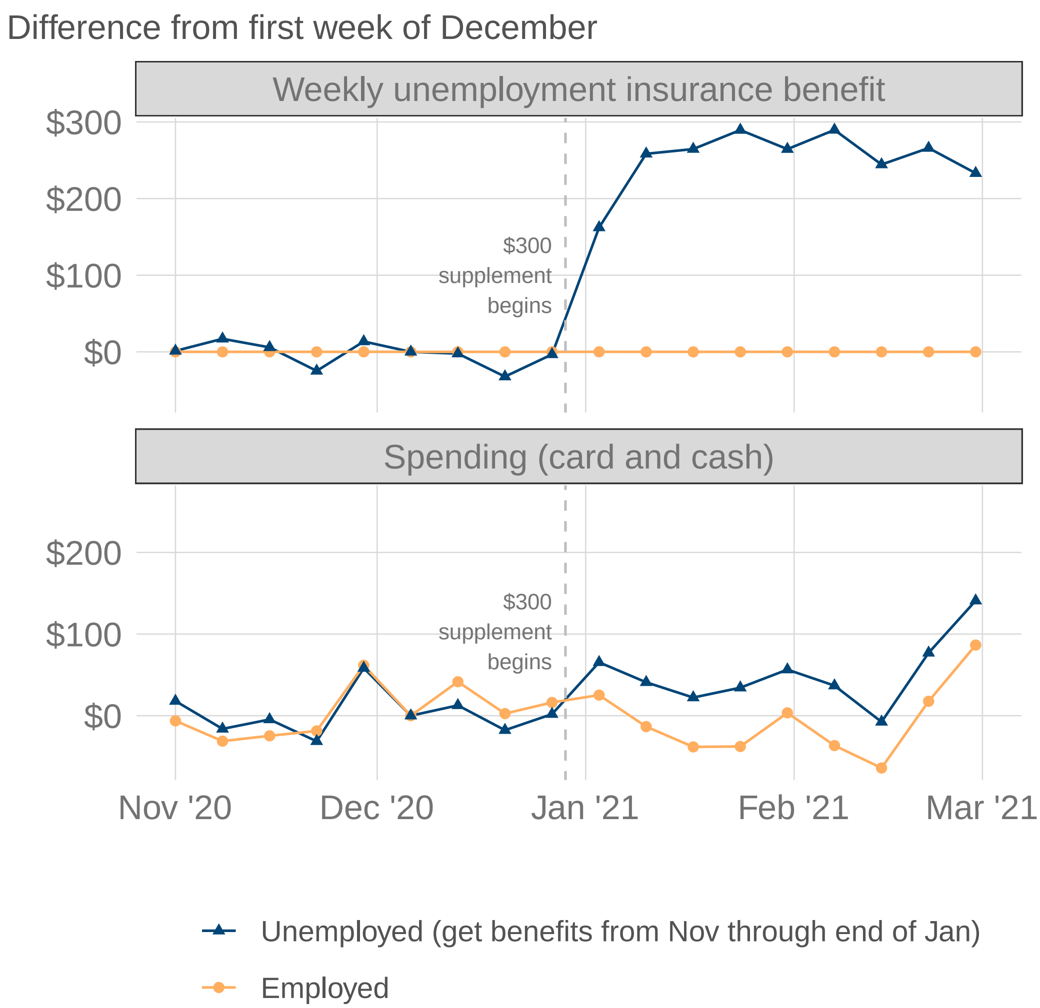
Notes: This figure shows mean income and spending (total) differences from the May 31 control group for various cohorts in the waiting for benefit receipt research design. Our MPC is based on the April 5th treatment group, which has no benefit delay.

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



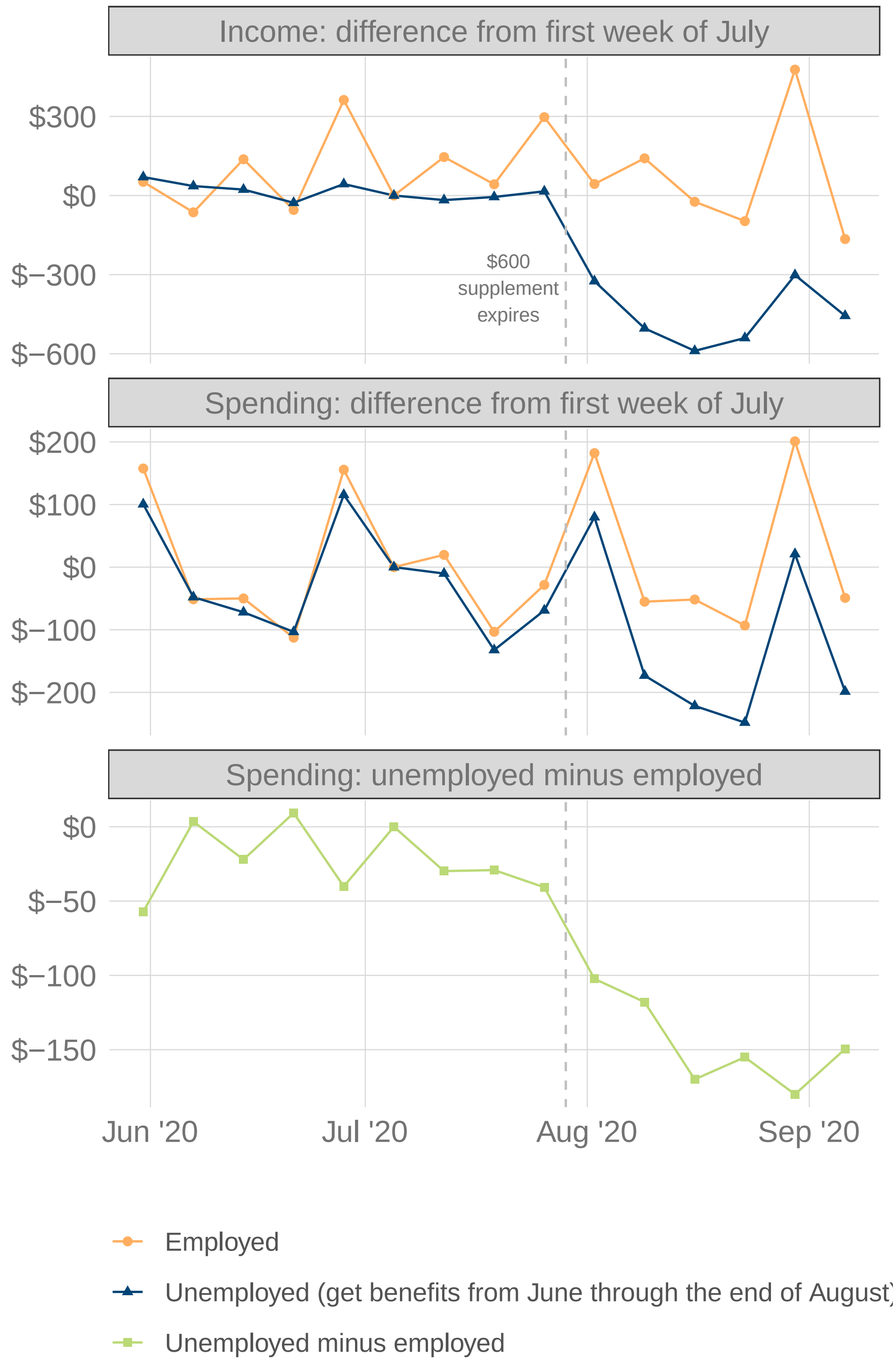
Notes: This figure measures the causal impact of the expiration of the $600 supplement on spending. The benefit amount declines over two weeks in August (rather than one week) because some states pay benefits once every two weeks and therefore paid out the supplement for the last week of July during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

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



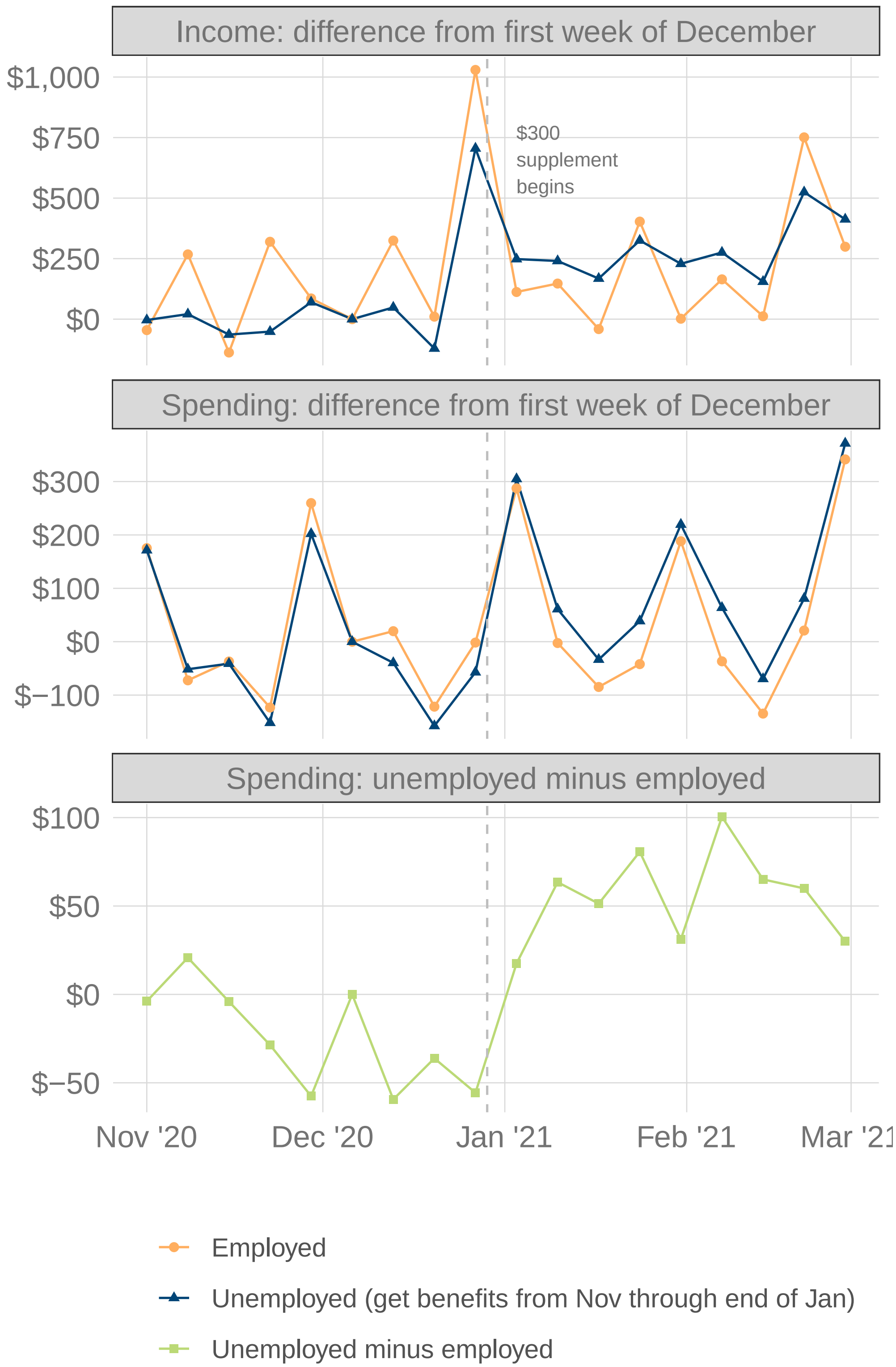
Notes: This figure measures the causal impact of the onset of the $300 supplement on spending. The benefit amount rises over two weeks in January (rather than one week) because some states pay benefits once every two weeks. The control group is employed workers who are matched on 2019 income levels as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

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



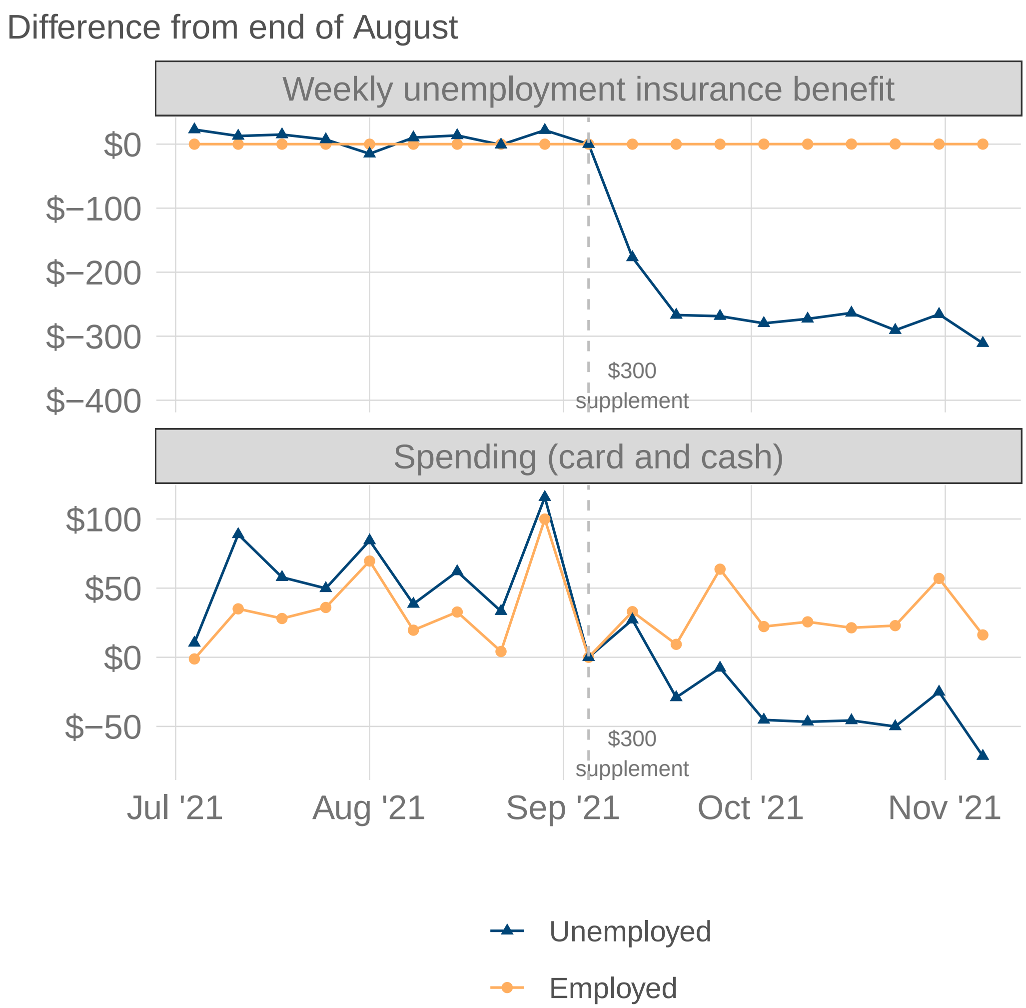
Notes: This figure measures the causal impact of the expiration of the $600 supplement on spending. The benefit amount declines over two weeks in August (rather than one week) because some states pay benefits once every two weeks and therefore paid out the supplement for the last week of July during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

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



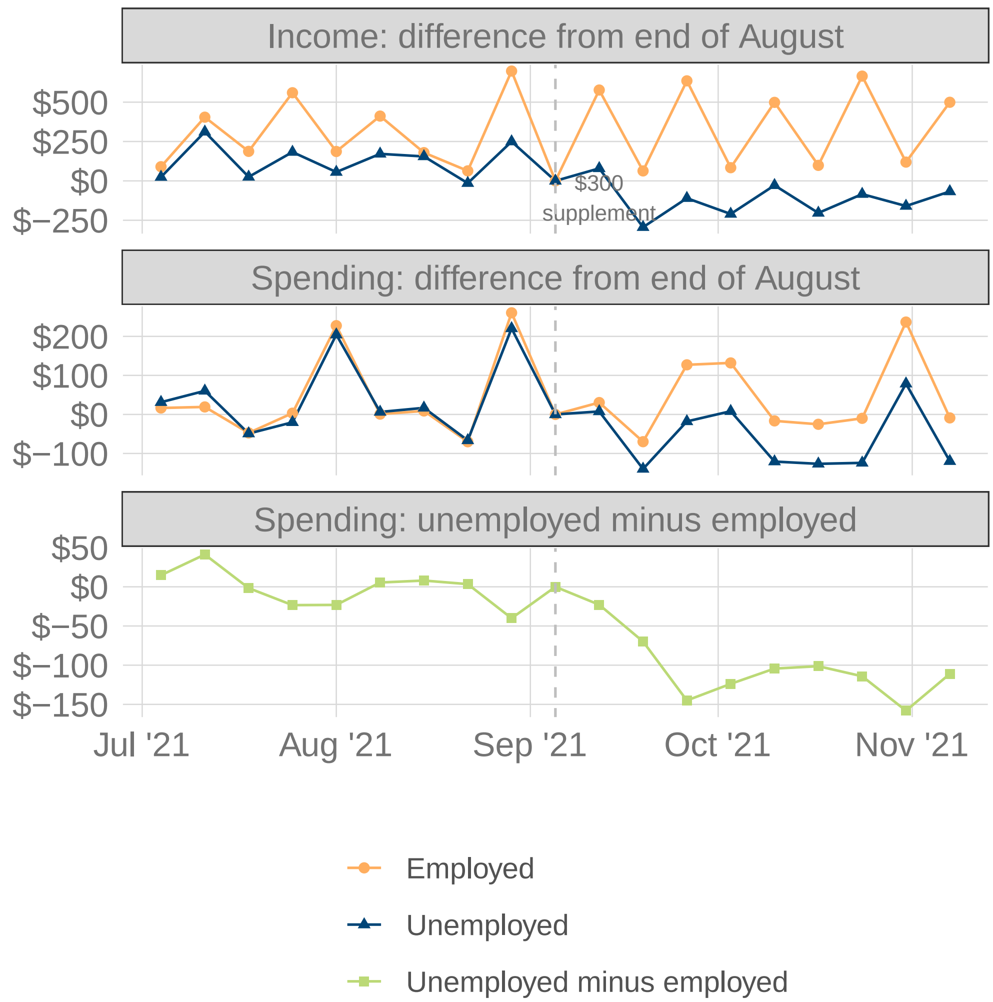
Notes: This figure repeats Figure A-8 but for total income and spending. The control group is employed workers who are matched on 2019 income levels as well as date of receipt of stimulus checks. The dependent variables are mean benefits and mean spending, measured as a change relative to the last week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

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



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

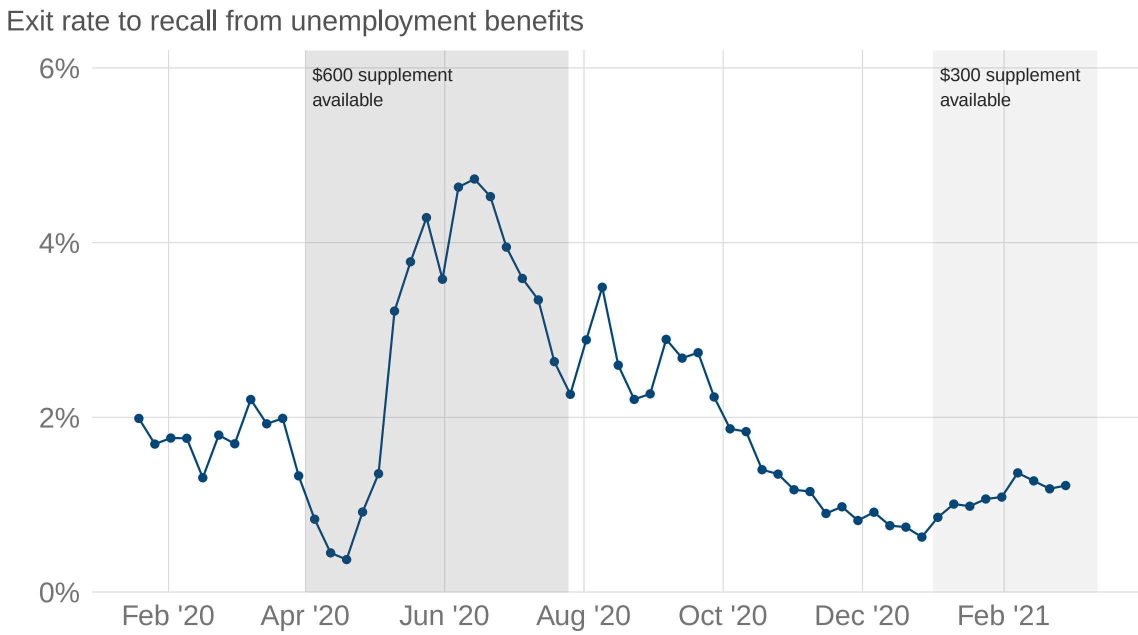
Figure A-12: Impact of Expiration of the $300 Supplement on Spending (Total)



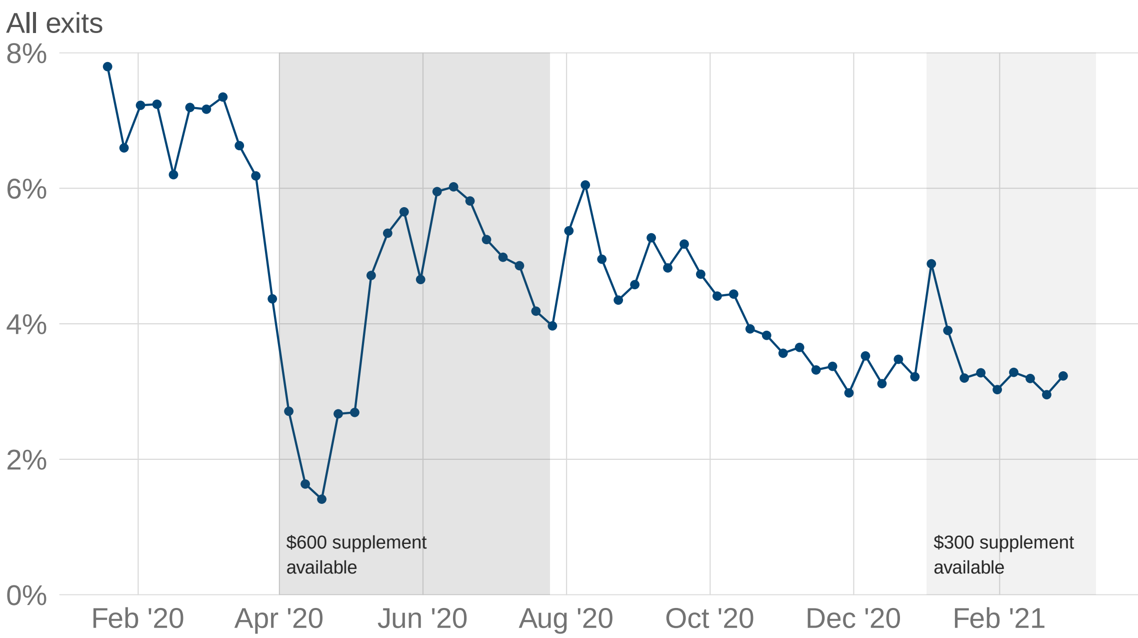
Notes: This figure repeats Figure A-11 but for total income and spending. The control group is employed workers who are matched on 2019 income levels as well as date of receipt of stimulus checks. The dependent variables are mean benefits and mean spending, measured as a change relative to the first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

Figure A-13: Exit Rate from Unemployment Benefits

(a) Recall

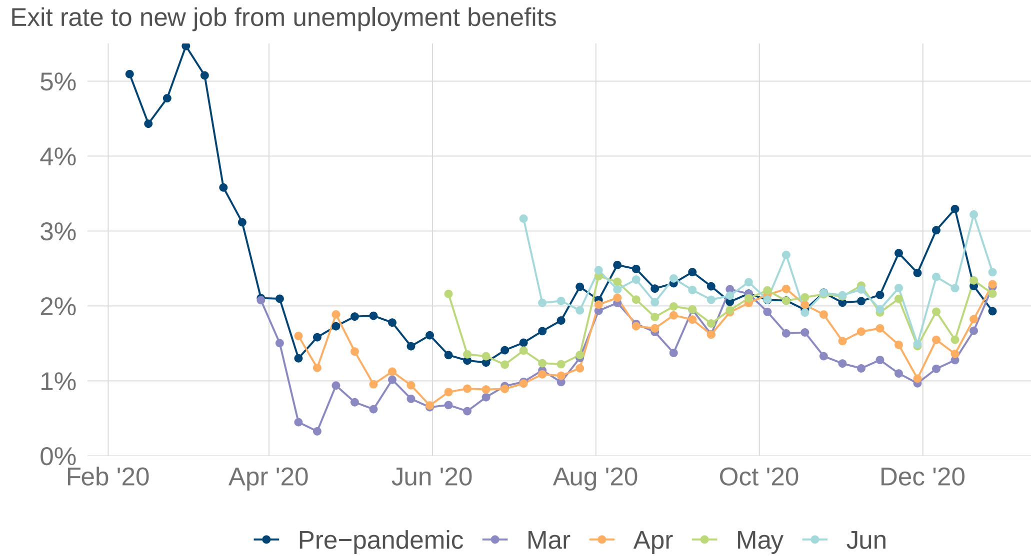


(b) Total (New Job + Recall)



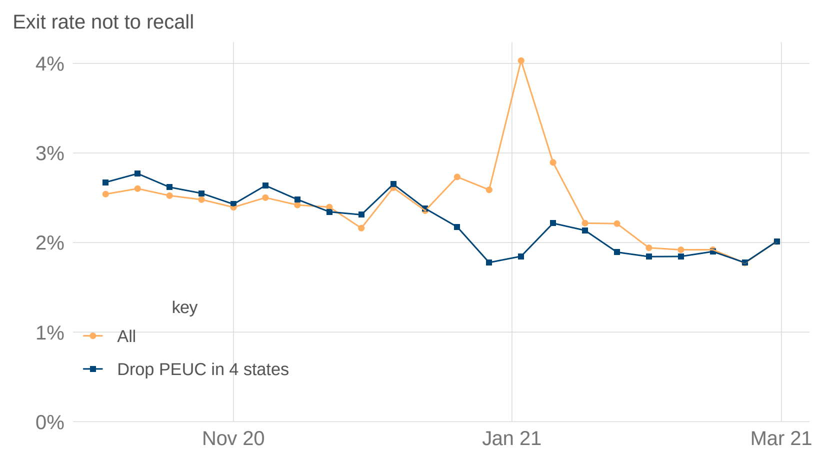
Notes: This figure shows the exit rate to recall and the total exit rate in the JPMCI data. UI exit is defined as three contiguous weeks without receipt of UI benefits. Recall is measured using receipt of labor income from a prior employer. Exit rate to new job is from Figure 4.

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



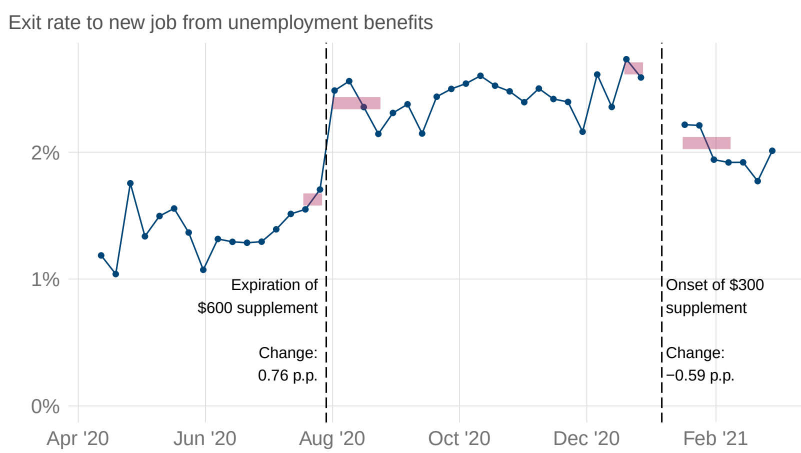
Notes: We define the pre-pandemic group as those who started receiving unemployment insurance benefits during or before the week of March 15, 2020.

Figure A-15: Exit Rate at Expiration of PEUC



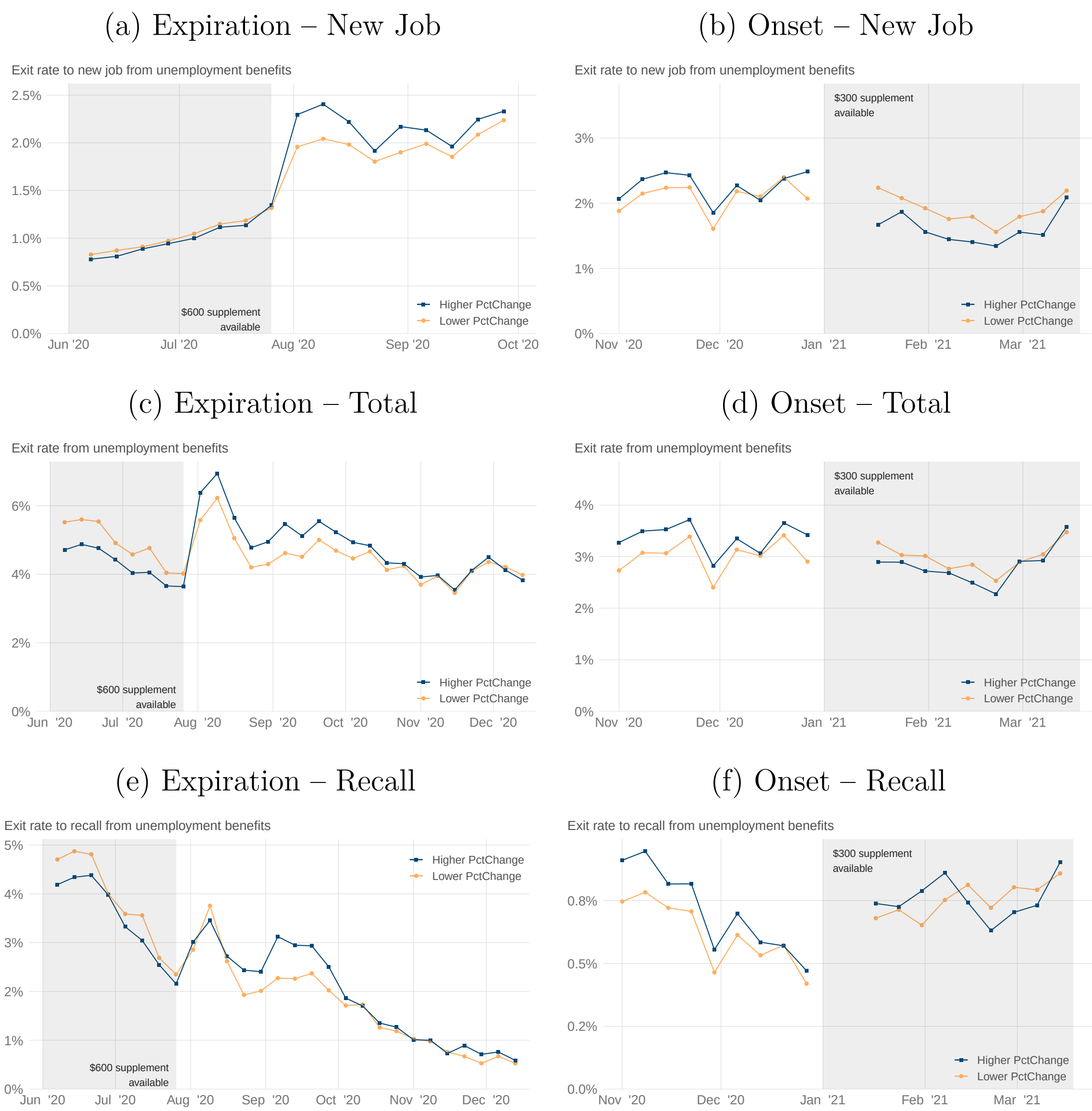
Notes: This figure shows the evolution of the exit rate not to recall from October 2020 through February 2021. The orange series is the same as the one shown in Figure 4, except that here the series includes January 3 and January 10. The y-axis title is the "exit rate not to recall" instead of the "exit rate to new jobs" because some of the exits arise from a policy seam. The blue series drops the 71,000 households that have received at least 20 weeks of benefits in 2019 and 2020 in Indiana, California, New Jersey, and Ohio. These households are likely to be recipients of Pandemic Emergency Unemployment Compensation, which temporarily lapsed at the end of December and these four states were slow to restore benefits after the lapse. The difference between the blue series and the green series reveals that the lapse triggered a surge in *measured* exits in four states. In additional unreported results, we find that the measured exits in the blue series do not show evidence of starting a new job via direct deposit of payroll from a new employer. We therefore omit January 3 and January 10 from the plot in Figure 4.

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



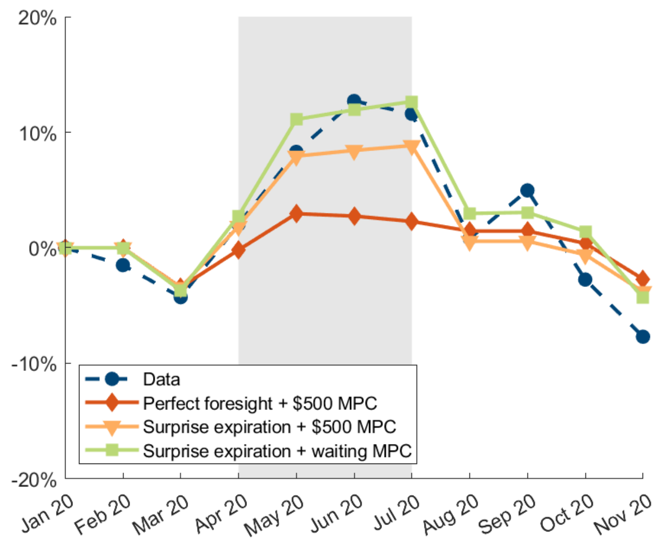
Notes: This figure shows the exit rate to a new job in the JPMCI data from April 2020 through February 2021. The red horizontal bars indicate the average exit rate in the two weeks prior to and four weeks following a change in the supplement amount.

Figure A-17: Exit Rate by Replacement Rate



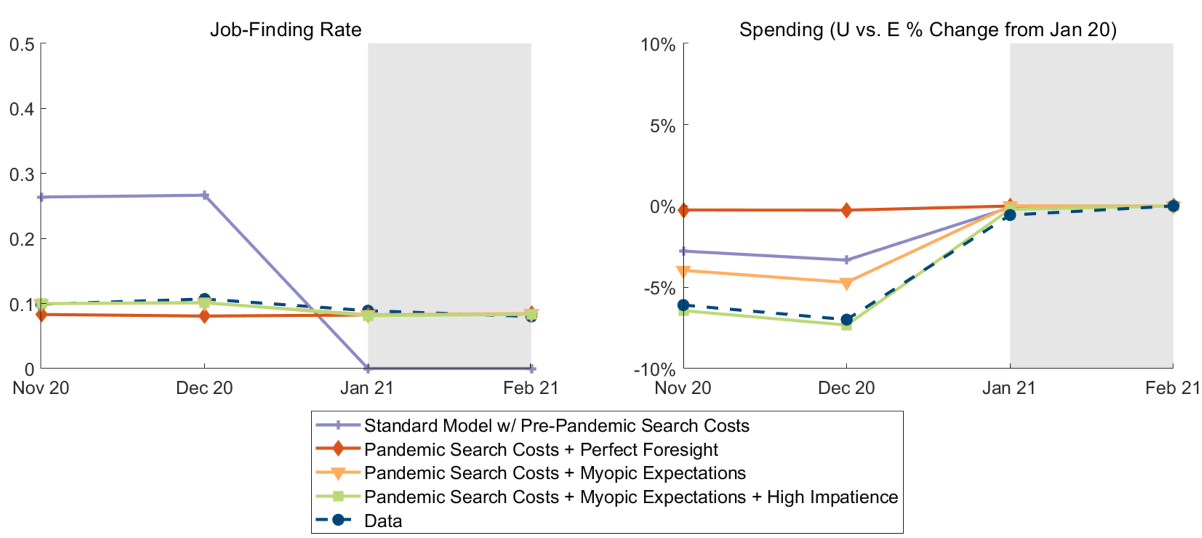
Notes: This figure shows several alternative specifications for Figure 6. Panels (a) and (b) report raw (unnormalized) exit rates to new job. Panels (c) and (d) report "total" exit rates including exit to new job and exit to recall. Panels (e) and (f) report exit rates to recall.

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



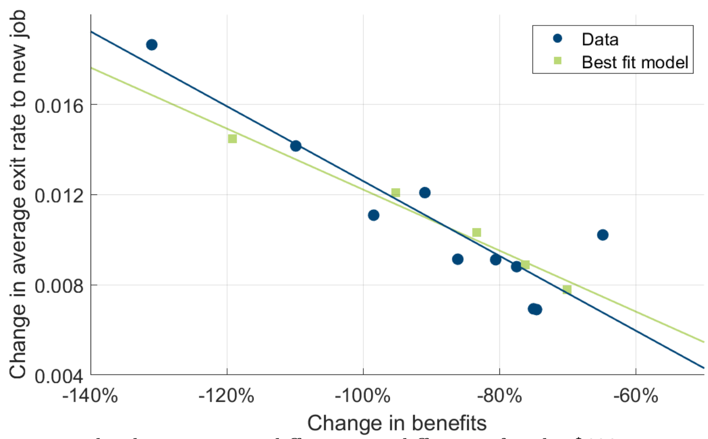
Notes: This figure shows the behavior of spending in the model more closely matches high-frequency data patterns after accounting for time-aggregation issues.

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



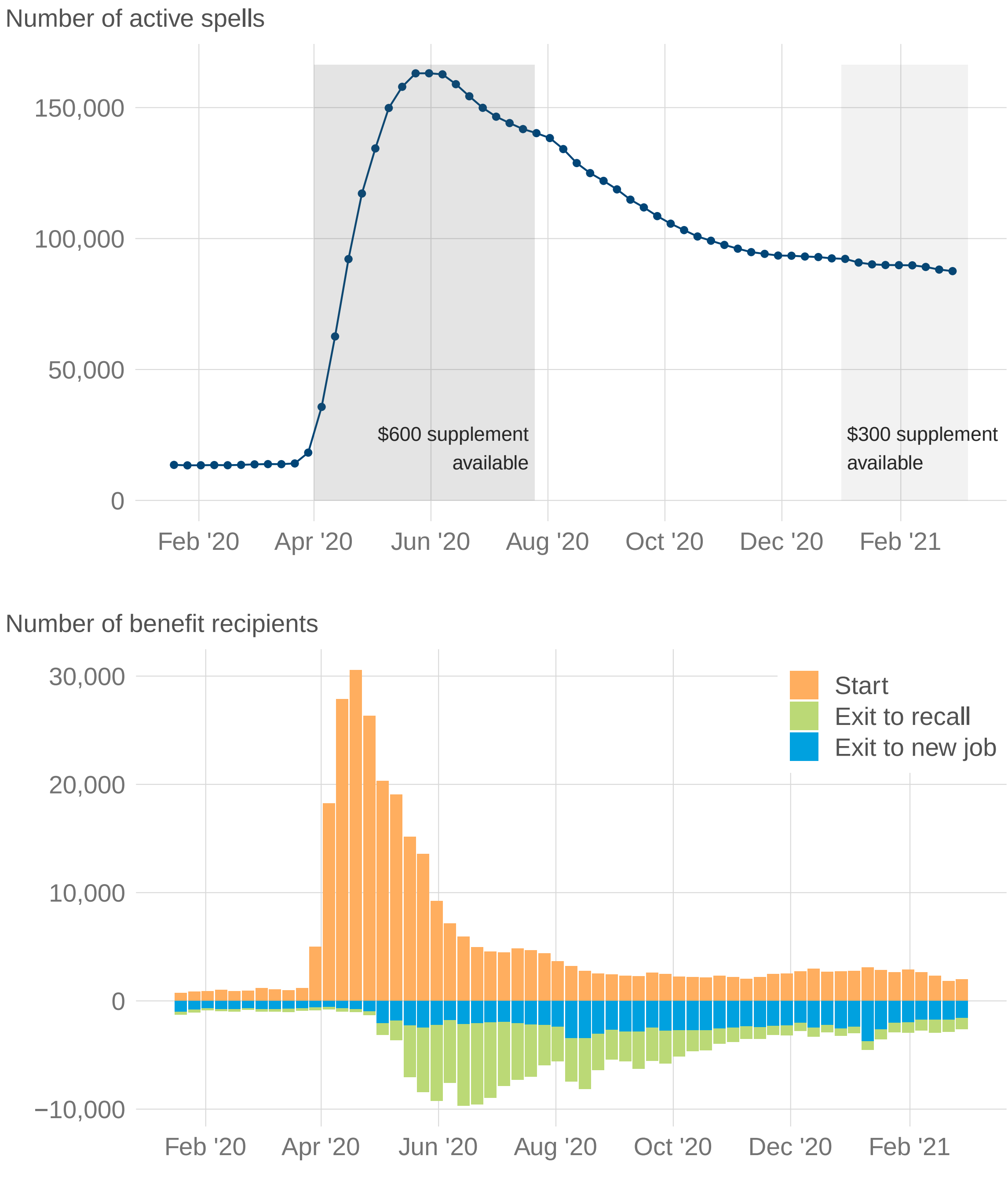
Notes: This figure repeats Figure 9 but showing models vs. data in response to the $300 supplement. In all models except the perfect foresight specification (including the standard model with pre-pandemic search costs), we assume the start of the $300 supplements in January is a surprise. We assume this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

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



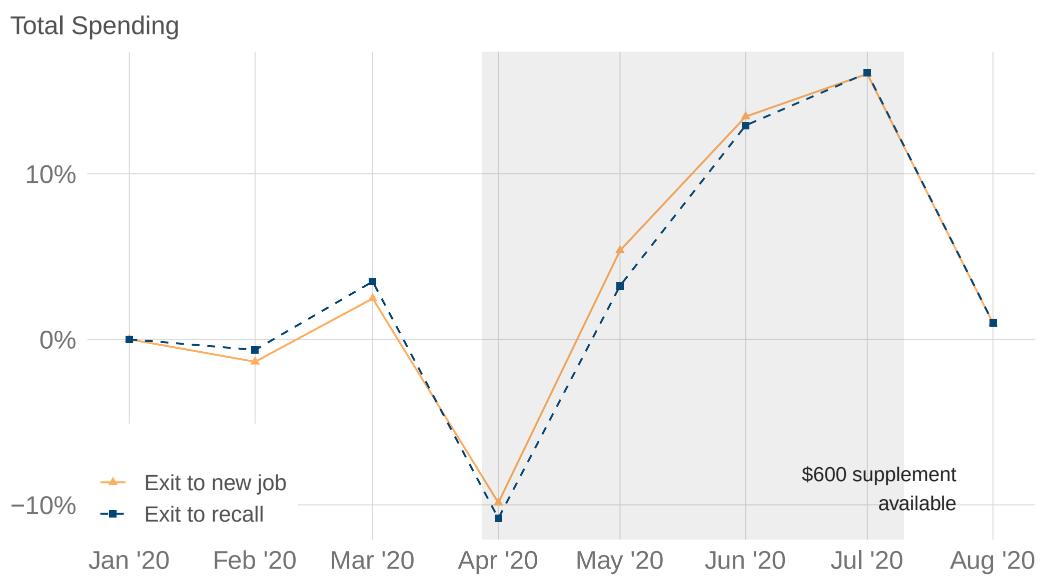
Notes: This figure compares the dose-response difference-in-difference for the $600 expiration in our best fit model to the empirical difference-in-difference.

Figure A-21: Patterns of Unemployment Insurance Receipt



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

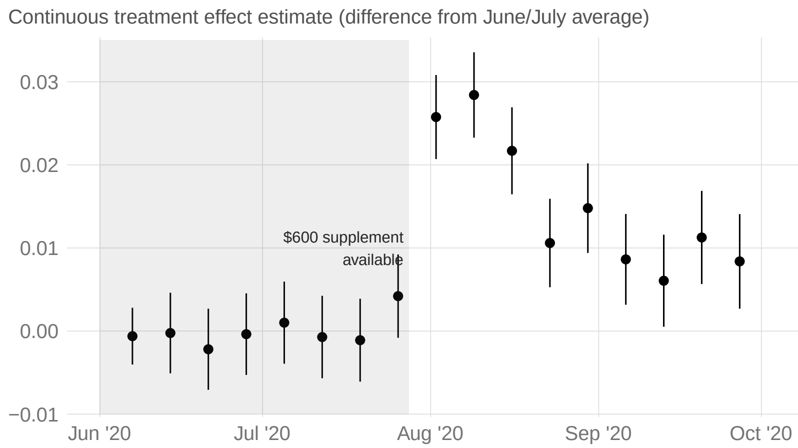
Figure A-22: Spending of Recalled vs. Non-Recalled Workers



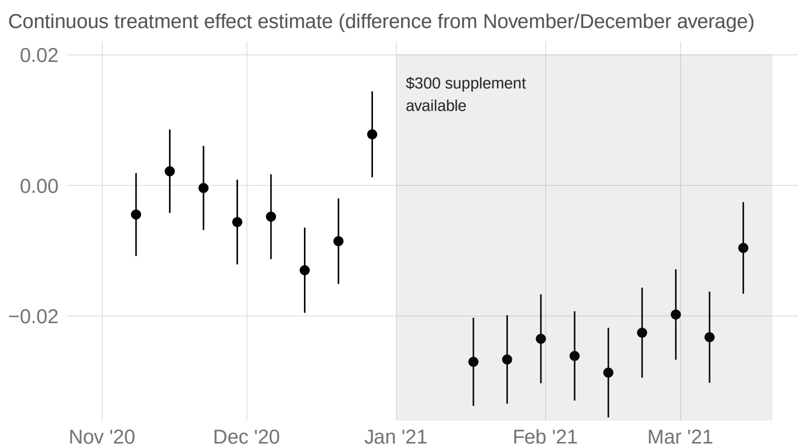
Notes: This figure shows total spending for two different groups of workers who are unemployed continuously from April 2020 through August 2020 and exit unemployment in September 2020. The "Exit to recall" group returns to their previous employer when they exit unemployment while the "Exit to new job"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

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

(a) Expiration of $600



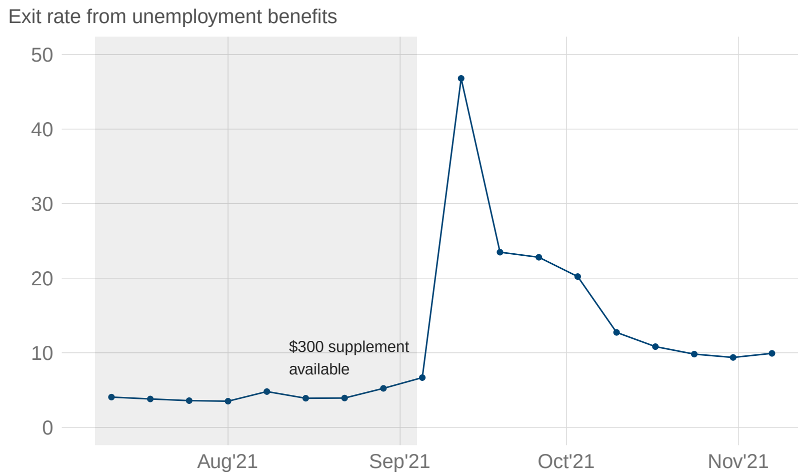
(b) Onset of $300



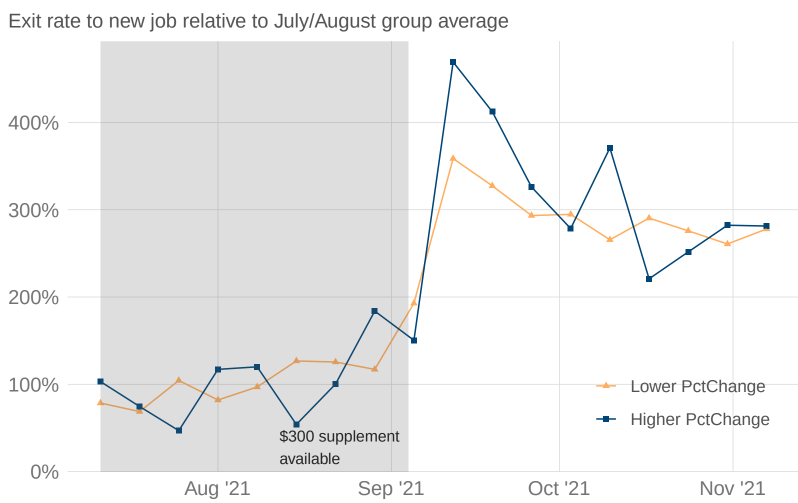
Notes: This figure shows the results from the weekly difference-in-difference specification defined in Section C.5.1 equation (C1). This specification captures the effect of supplements on job-finding in each week around the supplement change. Panel (b) indicates that even prior to the onset of the $300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

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

(a) Raw Exit Rate

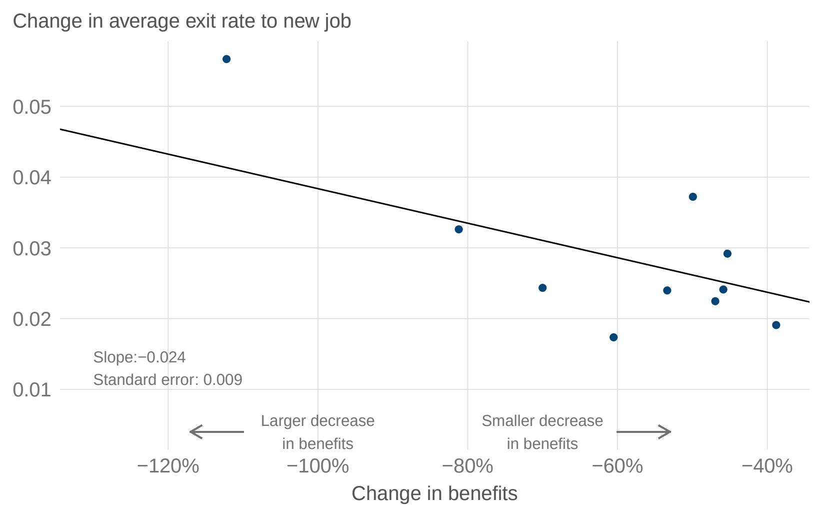


(b) Cleaned Exit Rate by Benefit Level



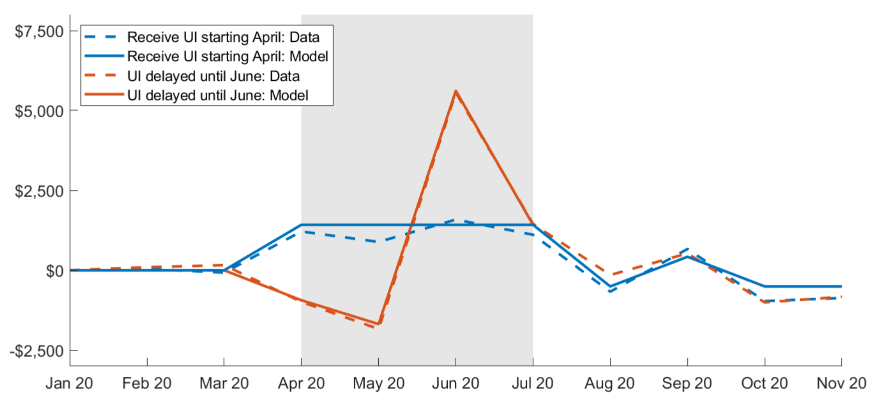
Notes: The top panel of this figure shows the raw exit rate from UI benefits. The surge in exits is caused by the expiration of multiple programs (see Appendix A.1 for details). The bottom panel of this figure replicates Figure 6 for the expiration of the $300 supplement in September 2021, for the subset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

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



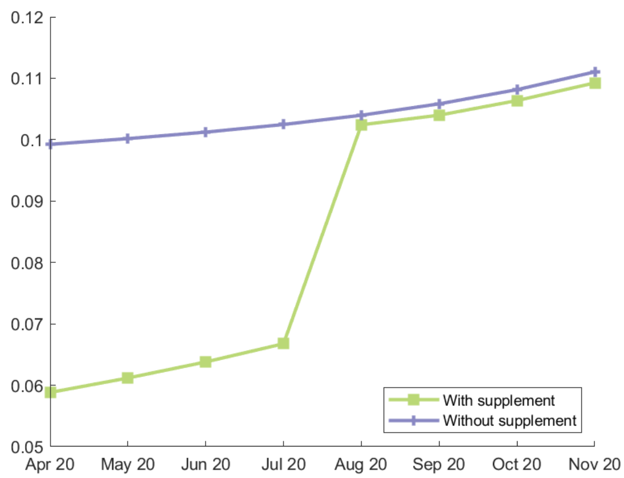
Notes: This figure replicates Figure 7 for the expiration of the $300 supplement in September 2021.

Figure A-26: Income: Model vs. Data



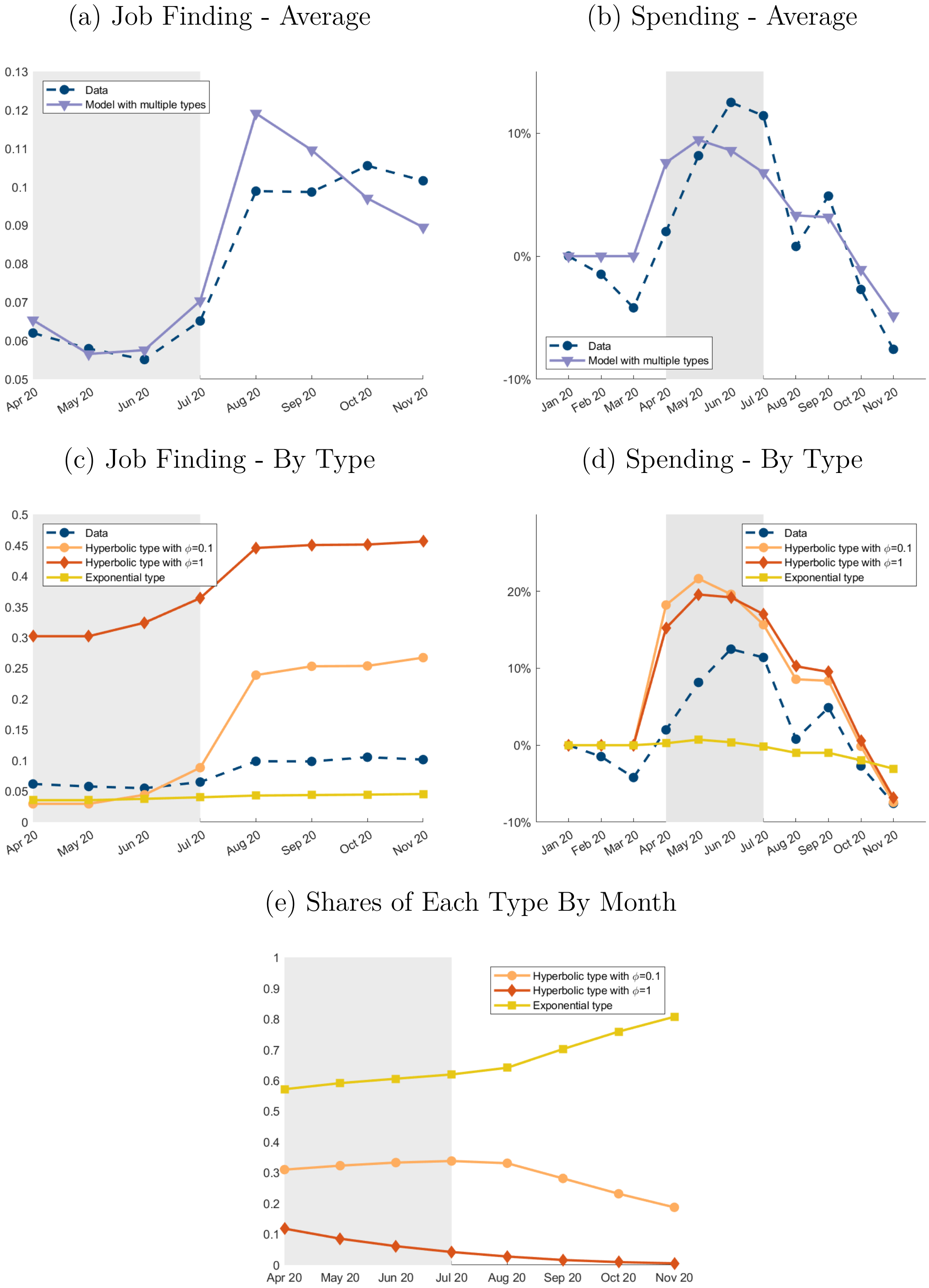
Notes: This figure shows income of unemployed (relative to employed) households in the model and data for unemployed workers receiving benefits immediately in April 2020 as well as those who face a delay in benefit receipt until June 2020.

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



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

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



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

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

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

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

Table A-2: Regression Estimates for Effect of Expanded Benefits on Job-Finding

	Dependent variable:	
	Exit to new job	
	Expiration of \$600	Onset of \$300
	(1)	(2)
PctChange	0.021***	0.022***
	(0.001)	(0.001)
SuppAvail	0.003***	0.008***
	(0.001)	(0.001)
PctChange:SuppAvail	−0.016***	−0.020***
	(0.001)	(0.001)
Constant	0.002***	0.009***
	(0.001)	(0.001)
Observations	2,120,887	1,790,138
Number of Households	183,144	131,464

Notes: This table estimates the difference-in-difference model $e_{it} = \gamma PctChange_i + \alpha SuppAvail_t + \beta SuppAvail_t \times PctChange_i + \varepsilon_{it}$ from equation (4) using a window of eight weeks prior to and eight weeks after the two policy changes (expiration of the \$600 supplement and onset of the \$300 supplement). For expiration, the supplement available period is June and July 2020 and the no-supplement period is August and September 2020. For onset, the supplement available period is January 15-March 15 2021 and the no-supplement period is November and December 2020. Standard errors are clustered at the household level. *p<0.1; **p<0.05; ***p<0.01.

Table A-3: Model Parameters

(a) Parameters Used in All Models

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

(b) Model Specific Choices

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

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

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

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

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

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

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

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

Table A-6: MPC by Spending Category

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

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

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

Table A-7: Marginal Propensity to Repay Debt

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

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

Table A-8: MPC Robustness to Additional Controls

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

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

Table A-9: MPC Robustness to Summary Statistic and Sample

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

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

Table A-10: MPC Robustness to Spending Measure

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

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

Table A-11: Effect of Expanded Benefits: Robustness to Controls

(a) Expiration of $600

	Dependent variable:			
	Exit to New Job			
	(1)	(2)	(3)	(4)
PctChange*SuppAvail	-0.0163^{***}	-0.0133^{***}	-0.0139^{***}	-0.0141^{***}
	(0.0011)	(0.0012)	(0.0013)	(0.0023)
PctChange	X	X	X	X
SuppAvail	X	X	X	X
State*SuppAvail FE		X	X	X
Age*SuppAvail FE			X	X
Industry*SuppAvail FE				X
Observations	2,120,887	2,120,887	1,886,942	519,245
Number of Households	183,144	183,144	163,930	44,165

(b) Onset of $300

	Dependent variable:			
	Exit to New Job			
	(1)	(2)	(3)	(4)
PctChange*SuppAvail	-0.0204^{***}	-0.0178^{***}	-0.0199^{***}	-0.0207^{***}
	(0.0013)	(0.0013)	(0.0014)	(0.0026)
PctChange	X	X	X	X
SuppAvail	X	X	X	X
State*SuppAvail FE		X	X	X
Age*SuppAvail FE			X	X
Industry*SuppAvail FE				X
Observations	1,790,138	1,790,138	1,604,130	454,135
Number of Households	131,464	131,464	116,276	32,689

Notes: This table reports estimates of $\hat{\beta}$ from equation (4), adding increasingly stringent control variables. The first column is the same as in Table A-2. Column (2) adds state by time fixed effects. Column (3) adds age bin by time fixed effects. Column (4) adds prior industry by time fixed effects. Prior industry is available only for workers who worked at the 1,000 largest firms in the data and therefore uses a smaller sample than the other columns. Standard errors are clustered at the household level. *p<0.1; **p<0.05; ***p<0.01.

Table A-12: Effect of Expanded Benefits: Alternative Functional Forms

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

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

Table A-13: Effect of Expanded Benefits: Alternative Measures of Exit

(a) Expiration of $600

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

(b) Onset of $300

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

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

Table A-14: Effect of Expanded Benefits: Summer 2021

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

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

Table A-15: Interpreting MPC Differences Between Waiting for Benefits and $500 Stimulus

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

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

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