American Economic Review 2024, 114(9): 2898–2939
https://doi.org/10.1257/aer.20220973

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

By Peter Ganong, Fiona Greig, Pascal Noel, Daniel M. Sullivan, and Joseph Vavra*

We show that the largest increase in unemployment benefits in US 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* E21, E24, E32, E62, E71, G51, J65)

This paper analyzes the spending and labor market impacts of the largest increase in unemployment insurance (UI) benefits in US 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

* Ganong: University of Chicago and NBER (email: ganong@uchicago.edu); Greig: JPMorgan Chase Institute (email: fgreig@gmail.com); Noel: University of Chicago and NBER (email: pascal.noel@chicagobooth.edu); Sullivan: JPMorgan Chase Institute (email: sullivan.dm3@gmail.com); Vavra: University of Chicago and NBER (email: joseph.vavra@chicagobooth.edu). Stefano DellaVigna was the coeditor for this article. We thank Joe Altonji, Adrien Auclert, Gabriel Chodorow-Reich, Arin Dube, Jason Furman, Jon Gruber, Greg Kaplan, Rohan Kekre, Bruce Meyer, Matt Notowidigdo, Heather Sarsons, Jesse Shapiro, Daphne Skandalis, Amir Sufi, Rob Vishny, and numerous seminar participants for helpful conversations. We thank Maxwell Liebeskind, who was a coauthor on several of the earlier JPMorgan Chase Institute (JPMCI) policy briefs on pandemic UI. We thank Philips Ametsikor, Samantha Anderson, Timotej Cejka, Rupsha Debnath, Jonas Enders, Theodore Grayer, Jay Leeds, Isaac Liu, Michael Meyer, Liam Purkey, Peter Robertson, John Spence, Nicolas Wuthenow, and Katie Zhang for excellent research assistance. We thank the Becker Friedman Institute and the Kathryn and Grant Swick Faculty Research Fund at the University of Chicago Booth School of Business for financial support. This research was made possible by a data-use agreement between three of the authors and the JPMorgan Chase Institute, which has created de-identified data assets that are selectively available to be used for academic research. All statistics from JPMCI data, including medians, reflect cells with multiple observations. The opinions expressed are those of the authors alone and do not represent the views of JPMorgan Chase and Co.

† Go to https://doi.org/10.1257/aer.20220973 to visit the article page for additional materials and author disclosure statement(s).

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 \$0.03 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 from the thirty-eighth to the sixty-third percentile 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 that relies on the overall expiration or onset of benefits. Second, we use a dose-response difference-in-difference design that 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 show 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 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 percent of aggregate employment shortfalls, while they explain more than 20 percent 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 percent smaller and an increase in spending while supplements are in effect that is 66 percent 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

[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).

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

[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,b); Mitman and Rabinovich (2015); Kekre (2023); and McKay and Reis (2021).

I. 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.

A. *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 to 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 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 online Appendix A.1.

B. *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

[5] In the two states where JPMCI can distinguish between regular benefits and PUA (Ohio and New Jersey), 74 percent 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.

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. Online Appendix 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 online 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 online Appendix A.3. We impose activity screens to ensure we capture workers whose primary bank accounts are at Chase and who have stable employment prior to the pandemic (see online 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 nondurable 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.

[7] For example, the California Policy Lab (Bell et al. 2022) calculates that fewer than 3 in 1,000 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.

[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 nondurable spending.

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 online Appendix A.5.

Online Appendix 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.

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 (ETA 2019–2020a,b) to the number of households receiving unemployment benefits in the JPMCI data. From early March to June, online Appendix Figure A-2, panel A 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. Online Appendix Figure A-2, panel B shows that the states with the largest increase in UI claims in DOL also have the largest increase in JPMCI, and vice versa. Online Appendix Figure A-2, panel C 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. Online Appendix Figure A-3 shows that industries with the largest increase in unemployment in DOL, such as retail and accommodation and 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 do 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

[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.

analysis of the representativeness of unemployed households in JPMCI data, see Ganong and Noel (2019).

II. 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.

A. *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 online Appendix 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 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,

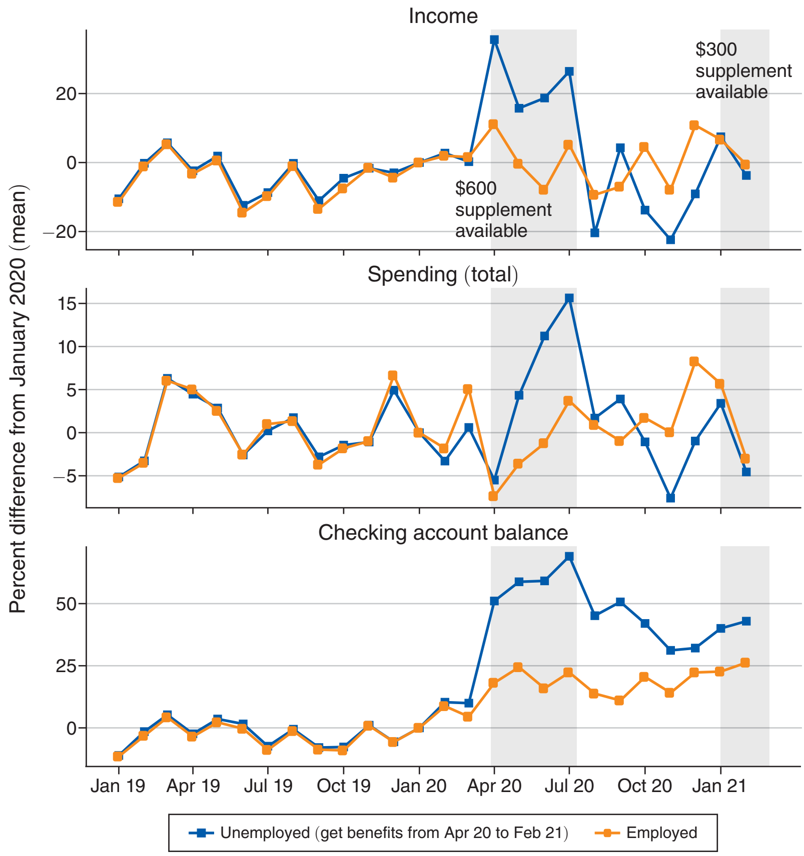

Figure 1. Income, Spending, and Account Balances of Unemployed versus Employed

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.

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 percent, 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 United States 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]

B. *Estimating MPCs*

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. 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

[12] Online Appendix Figure A-4 shows that similar patterns hold for medians instead of means, and online Appendix 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.

[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.

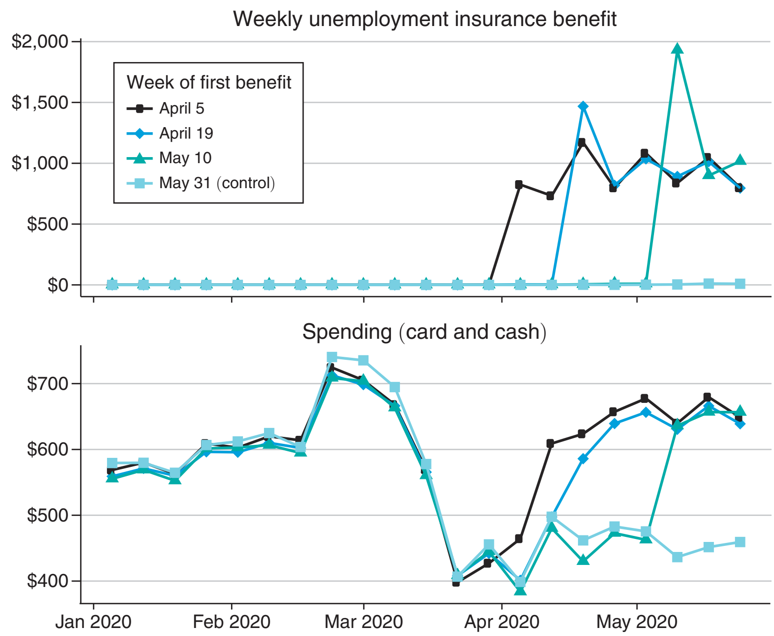

Figure 2. Impact of Delays in Unemployment Benefits on Spending

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.

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 that 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.

To estimate a monthly MPC for total spending, we use a difference-in-difference design that compares spending for a treatment group that receives benefits at the start of April 2020 to a control 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. Online Appendix Figure A-6 shows that patterns for total spending are similar patterns to card and cash spending

[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.

Table 1—Marginal Propensity to Consume out of Unemployment Benefits

	Research design	One-month MPC for 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 versus 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 2–5 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 that ended \$300 in June to unemployed households in states that ended \$300 in September. The total number of observations (treatment + control) in these six designs is, respectively, 1: 52,094; 2: 308,059; 3: 247,770; 4: 145,978; 5: 153,277; and 6: 47,623. Standard errors, in parentheses, are clustered by household.

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):

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

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

where $t = \{March, May\}$, $Treat = 1$ for households who become unemployed at the end of March and start benefits in April, $Treat = 0$ 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 $(\text{cov}(Post_t \times Treat_i, \varepsilon_{i,t}) = 0)$.[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 IV, 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

[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.

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.

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 IIA, we use a group of employed households 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. Online Appendix 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 (online Appendix Figure A-7) and rises sharply at the onset of the $300 supplement (online Appendix Figure A-8). Effects on total spending are also noticeable in online Appendix 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 1 in August and 0 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 1 in January and 0 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.

Online Appendix 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 online 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

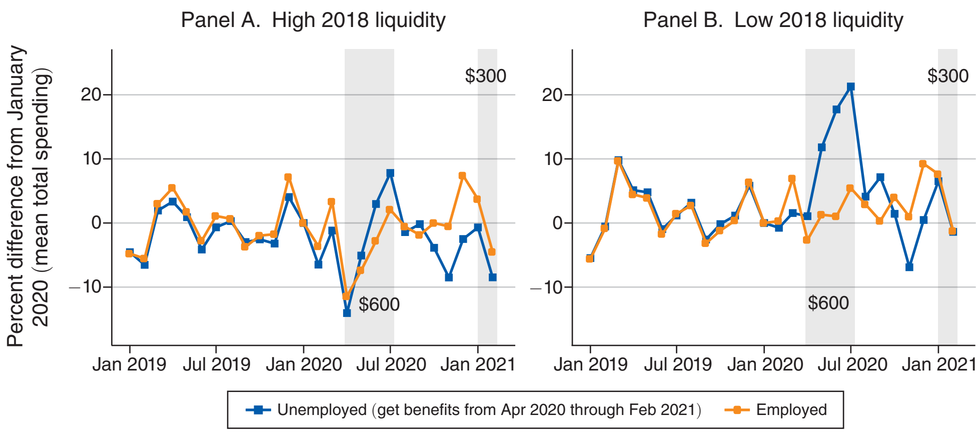

Figure 3. Spending by Pre-pandemic Liquidity

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.

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. Online Appendix B.1 provides additional detail on this analysis.

C. *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 online 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.[18]

In Section IVC, we interpret this evidence through the lens of a structural model of consumption. This evidence is useful for evaluating the extent to which "temporary

[18] Households with low liquidity buffers are more likely than average to have children, be single, be female, be slightly younger, and have slightly lower income. While these characteristics differ on average across liquidity groups, they nevertheless have little power for predicting liquidity: regressing low liquidity jointly on these observables yields an R^2 of only 0.02, implying that these readily observable statistics cannot be used to reliably tag low-liquidity households.

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 (0.02)	0.53 (0.02)	0.24 (0.03)
\$600 expiration	0.20 (0.01)	0.42 (0.01)	0.22 (0.02)
\$300 onset	0.19 (0.02)	0.35 (0.02)	0.16 (0.02)
\$300 expiration Sept. states	0.18 (0.02)	0.36 (0.02)	0.18 (0.02)
\$300 expiration June states	0.22 (0.03)	0.46 (0.03)	0.24 (0.04)
\$300 expiration June versus Sept. states	0.24 (0.05)	0.51 (0.04)	0.27 (0.06)

Notes: This repeats Table 1 splitting by high (above-median) 2018 liquidity and low (below-median) 2018 liquidity. The sample size for each liquidity group is roughly half that in Table 1. Standard errors, in parentheses, are clustered by household.

low liquidity causes high MPCs" 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.

D. *Robustness and Additional MPC Results*

Online 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.

III. 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 reemployment rates during the pandemic.[19] 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, online

[19] 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 of those who were uncertain about the disincentive was higher than for more than 93 percent of the other IGM survey questions asked since January 2020.

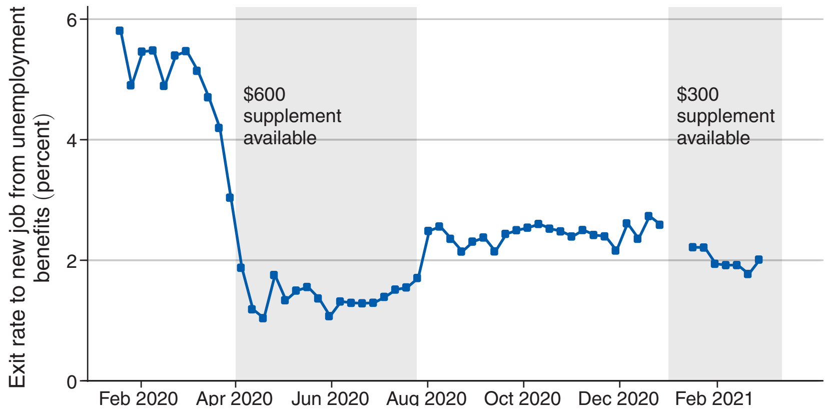

FIGURE 4. EXIT RATE FROM UNEMPLOYMENT BENEFITS TO NEW JOB

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 online Appendix 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.

Appendix Figure A-13 shows patterns for the recall rate and the total exit rate, and Section IIID discusses additional results for recall.

Figure 4 shows that at the start of the pandemic, the weekly new job-finding rate plunges by 4 percentage points and remains depressed thereafter. It also shows that the job-finding rate modestly 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.[20] We turn next to research designs that identify the causal effect of supplements on job finding.

A. *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 online Appendix 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 that is not subject to potential confounding high-frequency aggregate shocks. We find that the job-finding rate rises by 0.76 percentage points when the $600 supplement expires and falls by 0.59 percentage points after the onset of

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

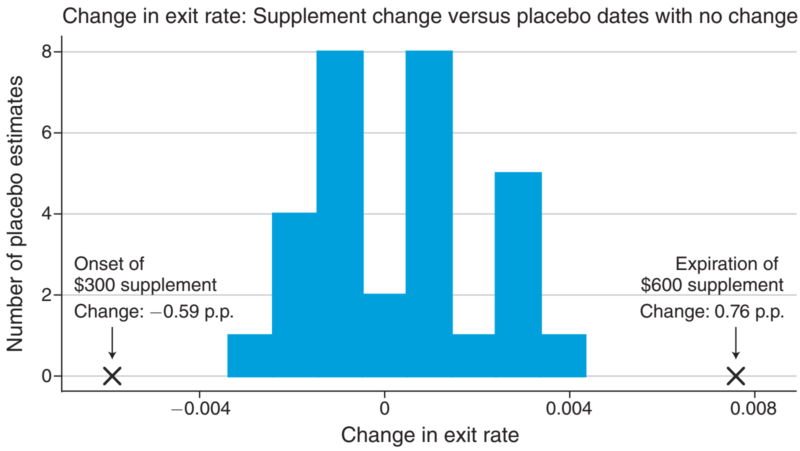

Figure 5. Distribution of Placebo Estimates

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 $p = 1/31$.

the $300 supplement. These effects are economically small, as we discuss in more detail in Section IIIC.

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 that treats the exact 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 $0.032 = 1/31$ if we include the own implementation date and exclude the implementation date of the other policy.

B. *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 percent 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 percent. 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 pretreatment 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.[21]

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 binscatter 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 online Appendix C.2.[22] The key coefficient of interest in equation (4) is $\hat{\beta}$, which captures how the job-finding rate changes for more treated versus less treated workers. Online Appendix 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.

C. *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

[21] 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 online Appendix C.1 for details.

[22] 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 IV.

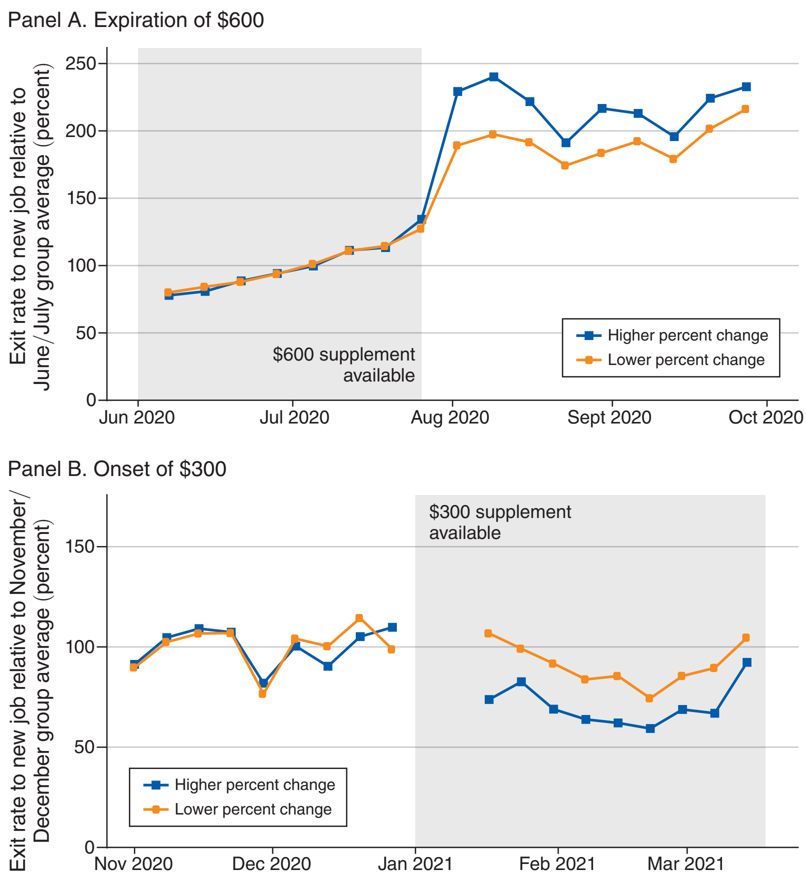

Figure 6. Effect of Expanded Benefits: Event Study

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. Online Appendix Figure A-17, panels A and B show the same figure but without a normalization in the pre-period. Online Appendix Figure A-17, panels C and D show that the same patterns hold when we look at the total job-finding rate (which includes both new job finding and recalls).

UI supplements affect the job-finding rate. We estimate that the $600 supplement reduces the weekly job-finding rate by 0.76 percentage points using the interrupted time series estimates and by 1.45 percentage points using the difference-in-difference estimates. The $300 supplement reduces the job-finding rate by 0.59 percentage points using time series estimates and by 1.18 percentage points using difference-in-difference estimates. Thus, the difference-in-difference estimates are about twice the size of the time series estimates.[23] However, as we discuss below, all

[23] 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

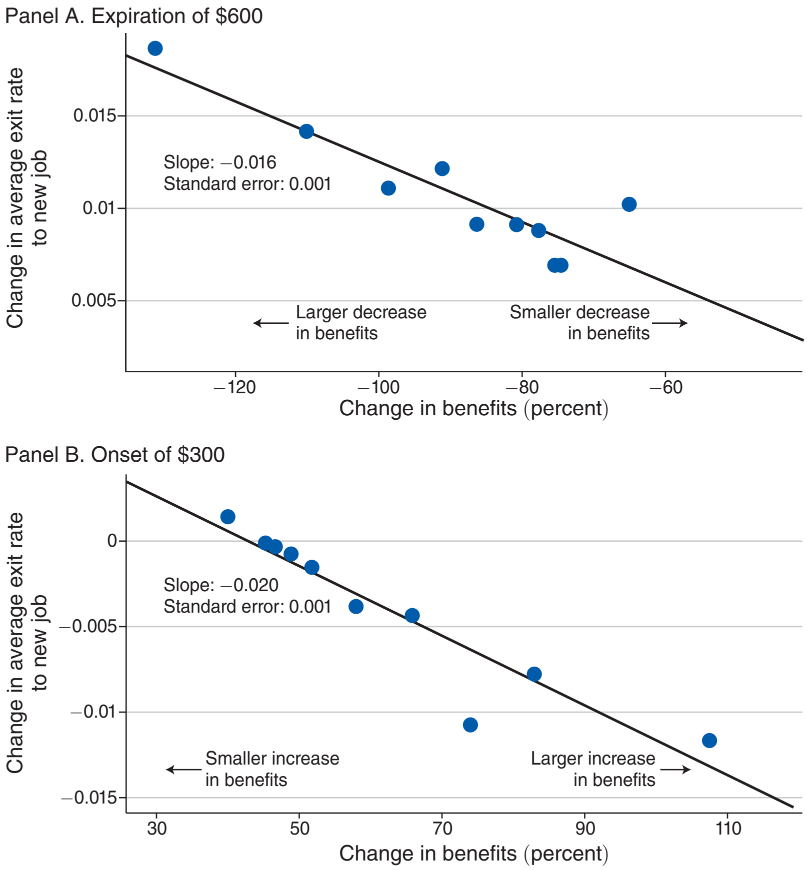

Figure 7. Effect of Expanded Benefits: Difference-in-Difference Binscatter

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). Panel A shows the difference in the average new job-finding rate between June 1–July 31 and August 1–September 31. Panel B shows the difference in the average new job-finding rate between November 1–December 31 and January 15–March 15. The slope estimates correspond to the $\hat{\beta}$ coefficients reported in online Appendix Table A-2.

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 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.[24] Table 3 shows that

estimates are smaller is if "micro" disincentive effects of UI (the effect of giving one worker more benefits) are bigger than the "macro" disincentive effect (the effect of giving all workers more benefits). See online Appendix C.3 for further discussion.

[24] The models in Section IIIB are estimated using symmetric percent change $PctChange_i$. The average of $PctChange_i$ is 81 percent for the $600 supplement and 57 percent for the $300 supplement. Note that because we

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 1 uses estimates from the $600 expiration, and row 2 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 24.

effects per $100 were similar for both the $600 supplements and the $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 online 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 et al. (2022); Petrosky-Nadeau and Valletta (2021); and Coombs et al. (2022).[25]

D. *Additional Job-Finding Results and Robustness*

Online 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 reestimating a weekly event study design rather than our baseline specification, which pools together all pre- and post-weeks. Third, we show that specifications that 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 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.

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 percent for $PctChange_i$. We therefore rescale the estimates from the $600 supplement by 20 percent/81 percent and the estimates from the $300 supplement by 20 percent/57 percent.

[25] Other studies that 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).

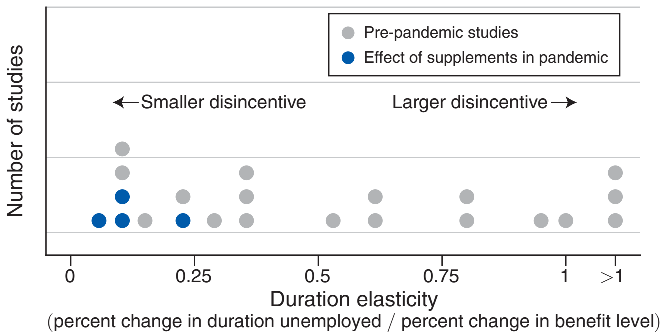

FIGURE 8. PANDEMIC ELASTICITY ESTIMATES COMPARED TO PRIOR LITERATURE

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

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 that are similar to those that we estimate for the start of the \$300 supplements.

IV. 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.

A. *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 online Appendix D1.

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 to July 2020, and then \$300 supplements are added from January to 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.[26]

Modeling Choices: Disciplining Job Search Costs, Expectations, and Impatience.—Most of our parameter choices follow the prior literature. We summarize them in online Appendix Table A-3 and discuss them further in online 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 that targets empirical evidence from the pre-pandemic literature and makes assumptions about beliefs that 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.[27]

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

[26] 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 online Appendix D.1.

[27] We assume constant search costs over the pandemic, so we have 3 parameters and 11 targets for monthly job finding (April 2020–February 2021).

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 that 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.

Model Fit Comparisons.—Figure 9, panel A 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 show 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 that 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.

Figure 9, panel B shows the effect of progressively introducing each of the three alternatives discussed previously. The model in red shows the effect of recalibrating *search costs* to target the pandemic 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 that 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

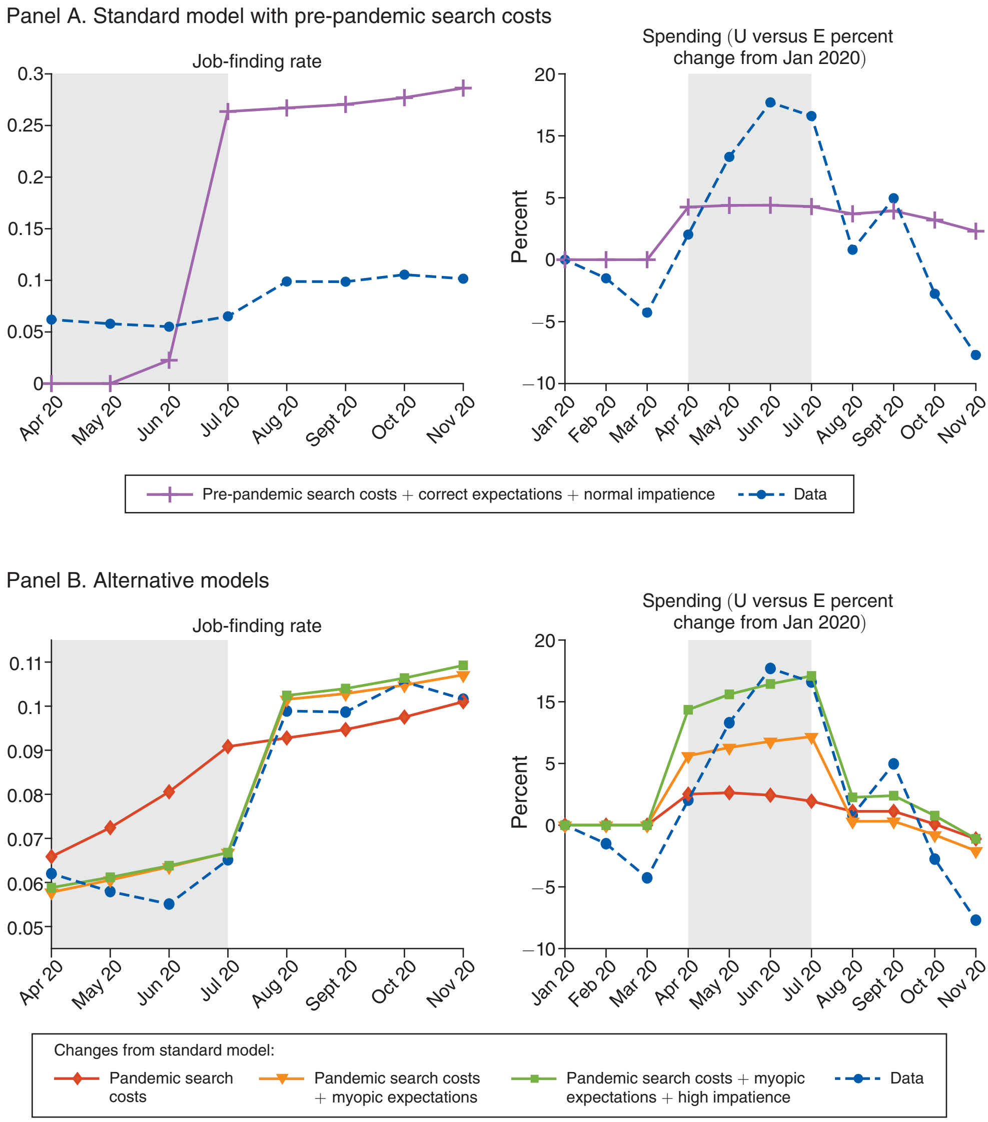

FIGURE 9. JOB-FINDING AND SPENDING RESPONSES TO $600 SUPPLEMENT: MODELS VERSUS DATA

Notes: This figure shows monthly time series of various models versus 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.

explain why the MPC of unemployed households is greater than the MPC of the general population (as measured by stimulus check MPCs).[28]

Hence, in the third and final change to the standard pre-pandemic model, we *increase impatience* of unemployed households relative to the general population

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

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 percent annually versus 11 percent). 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.[29]

Online Appendix 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, online Appendix 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 who 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).[30]

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: i) The policy changes may have truly been surprises, even to those paying attention. ii) Households may pay limited attention to policy news even after it is announced. iii) 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

[29] 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. Online Appendix E.1 and online Appendix 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.

[30] However, as we discuss in online 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 that 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 discounting or a model with present bias in time preference can be equally successful in fitting the spending data.

Table 4—Effects of Supplements on Spending

	\$600 supplement	\$300 supplement
1-month MPC out of first month of supplements	0.29	0.31
3-month MPC out of first 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

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.

B. *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 IIIC, 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 in Section IVC.

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 $\sum_{t=1}^{T} \Delta c_t \ / \ \sum_{t=1}^{T} \Delta y_t$ for various values of T. In particular, for each supplement, we show results for T equal to one month, three months, the length of the supplement period, and the length of the supplement period plus three months. It is these longer horizon MPCs that 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.[31] As already noted in Section IVA, these MPCs are large relative to *quarterly* MPCs out of stimulus checks of 0.15–0.25. We discuss this comparison in more detail in online Appendix D.5.

[31] Note that while large, the fact that this MPC is also well below one means that liquidity in the model is elevated in fall 2020 just like in the data (Figure 1). Specifically, in both the model and the data at the end of 2020, households still maintain about half of the additional liquidity they had accumulated through July from the \$600 supplements.

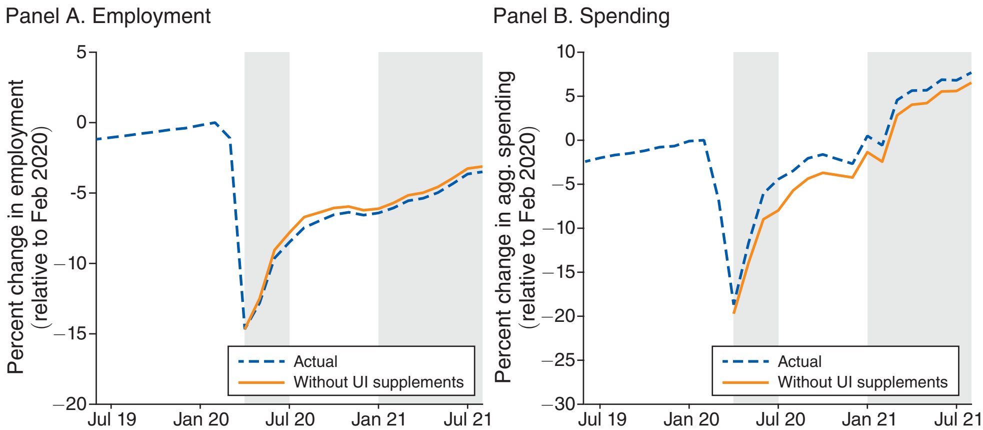

FIGURE 10. AGGREGATE IMPLICATIONS

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 Bureau of Labor Statistics (2019–2021) establishment survey, while the right panel compares to PCE spending from the Bureau of Economic Analysis (2019–2021).

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. By the end of July, \$4,080 is spent, 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.[32]

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 percent, while the \$300 supplements reduced employment by an average of 0.4 percent. Overall, this amounts to only around 5 percent 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 percent and 1.5 percent, respectively. This means

[32] 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.

that supplements helped to close a large fraction of the aggregate spending gap during the pandemic.[33] Before they expired at the end of July 2020, the supplements were responsible for 21 percent of the spending recovery during the pandemic.

C. *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?

Limited Job-Finding Dynamics.—In the model, the potential for dynamic search effects arises from two sources: anticipatory search and liquidity accumulation.[34] 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 III 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.[35] The model implies that supplements reduce the monthly job finding rate by 3.6 percent just prior to expiration (capturing both the effect of incentives and the effect of liquidity) and by only 0.16 percent after expiration (capturing only the effect of liquidity). Thus, liquidity effects account for only 4 percent of the total causal effect in this context.

[33] 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.

[34] 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 that 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.

[35] 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 reduce 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.

Second, we study empirically how the estimates in Section III vary with liquidity. Using a triple-difference design, online Appendix Tables A-4 and A-5 show that a 1 standard deviation increase in liquidity is associated with a decline in the disincentive effect of the \$600 from −0.0163 to −0.0135, and with a decline in the disincentive effect of the \$300 from −0.020 to −0.0186. 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 percent, from −0.017 in April 2020 to −0.0164 in July 2020.[36] This 5 percent estimate based on cross-sectional heterogeneity is in line with the 4 percent estimate from the structural model.

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 uses 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 that 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 reemployment. 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.[37] 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.[38] The orange circle captures the response to

[36] 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, $-0.0164 - (1328/5018) \times 0.0022 = -0.017$.

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

[38] 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.

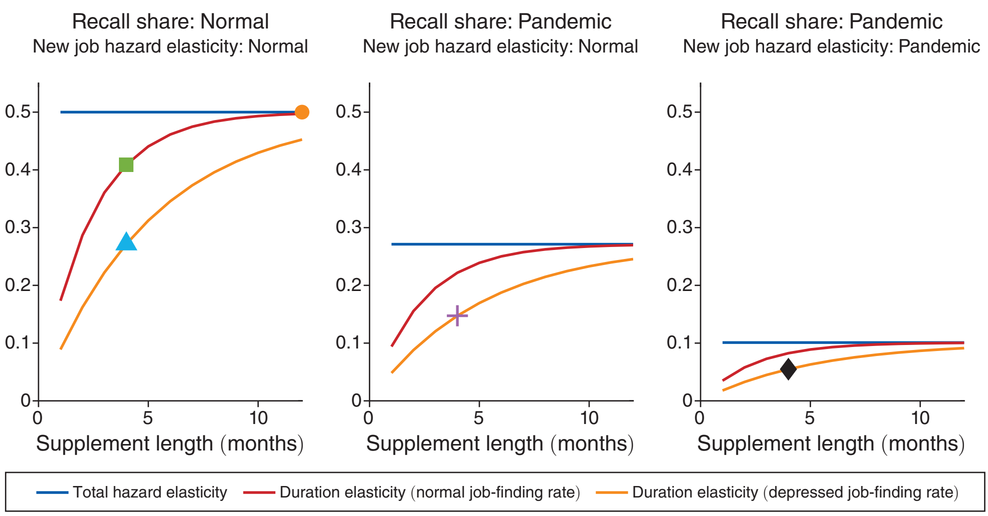

Figure 11. Forces for the Low Unemployment Duration Elasticity

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 orange line in the right panel corresponds to the full set of pandemic forces, while the red line in the left panel corresponds to the full set of normal conditions.

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 4 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 percent to 25 percent for 1 month will lead to larger growth in the duration of unemployment than will reducing the job-finding rate from 5 percent to 2.5 percent for that month. The orange 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 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 online 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.[39] 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 orange dot to our pandemic estimate.

High MPCs: Permanent Heterogeneity versus 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

[39] 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), differential recall rates, or industry-wide changes in labor demand discussed by Marinescu, Skandalis, and Zhao (2021), help account for the reduction in the hazard elasticity but found no systematic patterns. 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 not captured by our covariates.

moves up from the thirty-eighth to the sixty-third percentile of the pre-pandemic liquidity distribution (see online Appendix Table A-6, panel A). Indeed, the fact that the replacement rate with supplements is above 100 percent 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 IIC, 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.

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.[40]

[40]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

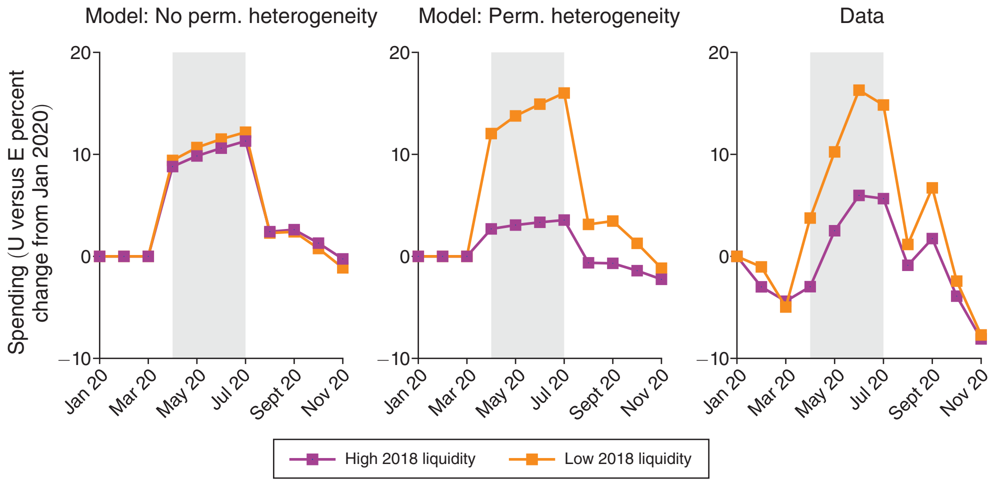

Figure 12. Spending Responses by Liquidity: Data versus Alternative Models

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 orange 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 orange and purple lines is not equal to the spending response in Figure 9, panel B because here we match treatment and control groups on liquidity.

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 percent 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 that compares across groups (high MPCs of unemployed relative to employed) to the second piece of evidence that compares within group (among those who become unemployed).

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

unemployment status in the best-fit model is explained by current economic circumstances, while one-third is due to permanent characteristics.

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.

V. 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. In this section we discuss the potential power of this type of policy. Our analysis is intentionally stylized and abstracts from a number of important mechanisms such as tax financing that would be important for a full quantification.[41]

A. *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.[42] 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.[43] Second, Schmieder, von Wachter, and Bender (2012) show that even if the behavioral response to more generous benefits (in the form of longer Potential

[41] Our goal is not to assess optimal policy. Other policies like severance paid in installments (see, e.g., Gerard and Naritomi 2021) might achieve similar stabilization goals with further benefits for consumption smoothing.

[42] If households are liquidity constrained, then severance payments may also reduce inefficiency (Chetty 2008). While severance limits job search distortions, self control may limit the consumption-smoothing benefits of severance. This concern could be mitigated by spreading severance payments over time, as proposed in Gerard and Naritomi (2021). However, delaying a portion of severance payments would likely reduce their immediate stimulative effects.

[43] 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).

Benefit Duration) is constant over the business cycle, the welfare-relevant distortion is smaller. This is because Potential Benefit Duration extensions mechanically affect more workers when the 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.

B. *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) compare 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.

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 IVC. 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 that argue for front-loading (Shavell and Weiss 1979; Hopenhayn and Nicolini 1997; Mitman and Rabinovich 2021).

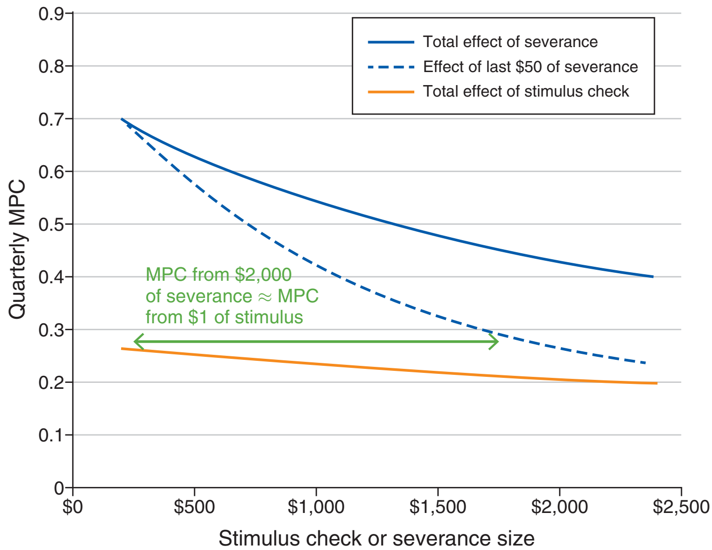

Figure 13. Spending Impacts of Severance versus Untargeted Stimulus

Notes: This figure shows the quarterly spending responses to severance as well as to one-time stimulus checks of various sizes in a nonpandemic 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.

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 three 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. Third, as payments become large, the unemployed are no longer low liquidity, and so the spending benefit relative to universal payments comes only from targeting payments to households that have persistently higher MPCs. The relevance of this force depends on whether the unemployed in future recessions share these same characteristics. The historical correlation between unemployment risk and MPCs documented by Patterson (2023) suggests this is the case, but these relationships may change over time.[44]

[44] One possible mechanism that could consistently generate this relationship is if impatient workers sort into more unstable jobs.

VI. Conclusion

We use administrative bank account data to estimate the causal effects of the largest UI expansion in US 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 toward 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.

REFERENCES

Aguiar, Mark, Mark Bils, and Corina Boar. 2020. "Who Are the Hand-to-Mouth?" NBER Working Paper 26643.

Andreolli, Michele, and Paolo Surico. 2021. "Less is More: Consumer Spending and the Size of Economic Stimulus Payments." Unpublished.

Angeletos, George-Marios, David Laibson, Andrea Repetto, Jeremy Tobacman, and Stephen Weinberg. 2001. "The Hyperbolic Consumption Model: Calibration, Simulation, and Empirical Evaluation." *Journal of Economic Perspectives* 15 (3): 47–68.

Athreya, Kartik, José Mustre-del Río, and Juan M Sánchez. 2019. "The Persistence of Financial Distress." *Review of Financial Studies* 32 (10): 3851–83.

Aydin, Deniz. 2022. "Consumption Response to Credit Expansions: Evidence from Experimental Assignment of 45,307 Credit Lines." *American Economic Review* 112 (1): 1–40.

Baker, Scott R., Robert A. Farrokhnia, Steffen Meyer, Michaela Pagel, and Constantine Yannelis. 2023. "Income, Liquidity, and the Consumption Response to the 2020 Economic Stimulus Payments*." *Review of Finance* 27 (6): 2271–2304.

Bartik, Alexander W., Marianne Bertrand, Feng Lin, Jesse Rothstein, and Matt Unrath. 2020. "Measuring the Labor Market at the Onset of the COVID-19 Crisis." *Brookings Papers on Economic Activity* 50 (2): 239–68.

Baugh, Brian, Itzhak Ben-David, Hoonsuk Park, and Jonathan A. Parker. 2021. "Asymmetric Consumption Smoothing." *American Economic Review* 111 (1): 192–230.

Bell, Alex, Thomas J. Hedin, Peter Mannino, Roozbeh Moghadam, Carl Romer, Geoffrey Schnorr, and Till von Wachter. 2022. "Increasing Equity and Improving Measurement in the U.S. Unemployment System: 10 Key Insights from the COVID-19 Pandemic." Unpublished.

Bell, Alex, Thomas J. Hedin, Geoff Schnorr, and Till von Wachter. 2022. "UI Benefit Generosity and Labor Supply from 2002-2020: Evidence from California UI records." Unpublished.

Bitler, Marianne P., Hilary W. Hoynes, and Diane Whitmore Schanzenbach. 2020. "The Social Safety Net in the Wake of COVID-19." *Brookings Papers on Economic Activity* 50 (1): 119–45.

Boutros, Michael. 2022. "Windfall Income Shocks with Finite Planning Horizons." Unpublished.

Bureau of Economic Analysis. 2019–2021. "Personal Income and Outlays." United States Department of Commerce. https://www.bea.gov/news/2021/personal-income-and-outlays-august-2021 (accessed July 19, 2022).

Bureau of Economic Analysis. 2019–2021. Personal Consumption Expenditures [PCE]. Retrieved from FRED, Federal Reserve Bank of St. Louis. https://fred.stlouisfed.org/series/PCE (accessed July 18th 2024)

Bureau of Labor Statistics. 2019–2021. "Current Employment Statistics." United States Department of Labor. https://www.bls.gov/news.release/archives/empsit_09032021.pdf (accessed July 19, 2022).

Cajner, Tomaz, Leland Crane, Ryan Decker, John Grigsby, Adrian Hamins-Puertolas, Erik Hurst, Christopher Kurz, and Ahu Yildirmaz. 2020. "The U.S. Labor Market during the Beginning of the Pandemic Recession." Unpublished.

Calvet, Laurent E., John Y. Campbell, Francisco Gomes, and Paolo Sodini. 2022. "The Cross-Section of Household Preferences." NBER Working Paper 28788.

Card, David, Raj Chetty, and Andrea Weber. 2007. "Cash-on-Hand and Competing Models of Intertemporal Behavior: New Evidence from the Labor Market." *Quarterly Journal of Economics* 122 (4): 1511–60.

Chetty, Raj. 2008. "Moral Hazard versus Liquidity and Optimal Unemployment Insurance." *Journal of Political Economy* 116 (2): 173–234.

Chetty, Raj, John Friedman, Michael Stepner, and The Opportunity Insights Team. 2023. "The Economic Impacts of COVID-19: Evidence from a New Public Database Built Using Private Sector Data." NBER Working Paper 27431.

Coombs, Kyle, Arindrajit Dube, Calvin Jahnke, Raymond Kluender, Suresh Naidu, and Michael Stepner. 2022. "Early Withdrawal of Pandemic Unemployment Insurance: Effects on Employment and Earnings." *AEA Papers and Proceedings* 112: 85–90.

Cox, Natalie, Peter Ganong, Pascal Noel, Joseph Vavra, Arlene Wong, Diana Farrell, and Fiona Greig. 2020. "Initial Impacts of the Pandemic on Consumer Behavior: Evidence from Linked Income, Spending, and Savings Data." *Brookings Papers on Economic Activity* 50 (1): 35–69.

Dube, Arindrajit. 2021. "Aggregate Employment Effects of Unemployment Benefits During Deep Downturns: Evidence from the Expiration of the Federal Pandemic Unemployment Compensation." NBER Working Paper 28470.

Employment and Training Administration (ETA). 2019–2020a. Claims and Payment Activities. United States Department of Labor, Washington, DC. https://oui.doleta.gov/unemploy/DataDownloads.asp (accessed July 19, 2022).

Employment and Training Administration (ETA). 2019–2020b. Weekly Claims and Extended Benefits Trigger Data. United States Department of Labor, Washington, DC. https://oui.doleta.gov/unemploy/DataDownloads.asp (accessed July 19, 2022).

Fagereng, Andreas, Martin B. Holm, and Gisle J. Natvik. 2021. "MPC Heterogeneity and Household Balance Sheets." *American Economic Journal: Macroeconomics* 13 (4): 1–54.

Finamor, Lucas, and Dana Scott. 2021. "Labor Market Trends and Unemployment Insurance Generosity during the Pandemic." *Economics Letters* 199: 109722.

Ganong, Peter, Fiona Greig, Pascal Noel, Daniel M. Sullivan, and Joseph Vavra. 2024. *Data and Code for: "Spending and Job-Finding Impacts of Expanded Unemployment Benefits: Evidence from Administrative Micro Data."* Nashville, TN: American Economic Association; distributed by Inter-university Consortium for Political and Social Research, Ann Arbor, MI. https://doi.org/10.3886/E198945V1.

Ganong, Peter, and Pascal Noel. 2019. "Consumer Spending during Unemployment: Positive and Normative Implications." *American Economic Review* 109 (7): 2383–2424.

Ganong, Peter, Pascal Noel, and Joseph Vavra. 2020. "US Unemployment Insurance Replacement Rates during the Pandemic." *Journal of Public Economics* 191.

Garber, Gabriel, Atif R. Mian, Jacopo Ponticelli, and Amir Sufi. 2021. "Consumption Smoothing or Consumption Binging? The Effects of Government-Led Consumer Credit Expansion in Brazil." NBER Working Paper 29386.

Gelman, Michael. 2021. "What Drives Heterogeneity in the Marginal Propensity to Consume? Temporary Shocks vs Persistent Characteristics." *Journal of Monetary Economics* 117: 521–42.

Gerard, François, and Joana Naritomi. 2021. "Job Displacement Insurance and (the Lack of) Consumption-Smoothing." *American Economic Review* 111 (3): 899–942.

Gruber, Jonathan. 1997. "The Consumption Smoothing Benefits of Unemployment Insurance." *American Economic Review* 87 (1): 192–205.

Havranek, Tomas, and Anna Sokolova. 2020. "Do Consumers Really Follow a Rule of Thumb? Three Thousand Estimates from 144 Studies say 'Probably Not'." *Review of Economic Dynamics* 35: 97–122.

Holzer, Harry J., R. Glenn Hubbard, and Michael R. Strain. 2021. "Did Pandemic Unemployment Benefits Reduce Employment? Evidence from Early State-Level Expirations in June 2021." NBER Working Paper 29575.

Hopenhayn, Hugo A., and Juan Pablo Nicolini. 1997. "Optimal Unemployment Insurance." *Journal of Political Economy* 105 (2): 412–38.

Hornstein, Andreas, Marios Karabarbounis, Andre Kurmann, Etienne Lale, and Lien Ta. 2022. "Disincentive Effects of Pandemic Unemployment Benefits."

Ilut, Cosmin, and Rosen Valchev. 2023. "Economic Agents as Imperfect Problem Solvers." *Quarterly Journal of Economics* 138 (1): 313–62.

Initiative on Global Markets. 2021. "Unemployment Benefits." Initiative on Global Markets. https://www.igmchicago.org/surveys/unemployment-benefits/ (accessed on July 13, 2022).

Johnson, David S., Jonathan A. Parker, and Nicholas S. Souleles. 2006. "Household Expenditure and the Income Tax Rebates of 2001." *American Economic Review* 96 (5): 1589–1610.

JPMorgan Chase Institute. 2018 - 2021. JPMorgan Chase Institute De-Identified Data Assets. JPMorgan Chase Institute, New York, NY. https://www.jpmorganchase.com/institute (accessed July 16, 2024)

Kaplan, Greg, and Giovanni L. Violante. 2014. "A Model of the Consumption Response to Fiscal Stimulus Payments." *Econometrica* 82 (4): 1199–1239.

Kaplan, Greg, and Giovanni L. Violante. 2022. "The Marginal Propensity to Consume in Heterogeneous Agent Models." *Annual Reviews of Economics* 14: 747–75.

Kekre, Rohan. 2023. "Unemployment Insurance in Macroeconomic Stabilization." *Review of Economic Studies* 90 (5): 2439–80.

Kroft, Kory, and Matthew J. Notowidigdo. 2016. "Should Unemployment Insurance Vary with the Unemployment Rate? Theory and Evidence." *Review of Economic Studies* 83 (3): 1092–1124.

Kueng, Lorenz. 2018. "Excess Sensitivity of High-Income Consumers." *Quarterly Journal of Economics* 133 (4): 1693–1751.

Landais, Camille, Pascal Michaillat, and Emmanuel Saez. 2018a. "A Macroeconomic Approach to Optimal Unemployment Insurance: Applications." *American Economic Journal: Economic Policy* 10 (2): 182–216.

Landais, Camille, Pascal Michaillat, and Emmanuel Saez. 2018b. "A Macroeconomic Approach to Optimal Unemployment Insurance: Theory." *American Economic Journal: Economic Policy* 10 (2): 152–81.

Lian, Chen. 2021. "Mistakes in Future Consumption, High MPCs Now." NBER Working Paper 29517.

Lindner, Attila, and Balázs Reizer. 2020. "Front-Loading the Unemployment Benefit: An Empirical Assessment." *American Economic Journal: Applied Economics* 12 (3): 140–74.

Marinescu, Ioana, Daphné Skandalis, and Daniel Zhao. 2021. "The Impact of the Federal Pandemic Unemployment Compensation on Job Search and Vacancy Creation." *Journal of Public Economics* 200: 104471.

McKay, Alisdair, and Ricardo Reis. 2021. "Optimal Automatic Stabilizers." *Review of Economic Studies* 88 (5): 2375–2406.

Mitman, Kurt, and Stanislav Rabinovich. 2015. "Optimal Unemployment Insurance in an Equilibrium Business-Cycle Model." *Journal of Monetary Economics* 71: 99–118.

Mitman, Kurt, and Stanislav Rabinovich. 2021. "Whether, When and How to Extend Unemployment Benefits: Theory and Application to COVID-19." *Journal of Public Economics* 200: 104447.

Orchard, Jacob, Valerie A. Ramey, and Johannes Wieland. 2022. "Micro MPCs and Macro Counterfactuals: The Case of the 2008 Rebates." Unpublished.

Parker, Jonathan A. 2017. "Why Don't Households Smooth Consumption? Evidence from a $25 Million Experiment." *American Economic Journal: Macroeconomics* 9 (4): 153–83.

Parker, Jonathan, Jake Schild, Laura Erhard, and David Johnson. 2022. "Economic Impact Payments and Household Spending During the Pandemic." NBER Working Paper 30596.

Patterson, Christina. 2023. "The Matching Multiplier and the Amplification of Recessions." *American Economic Review* 113 (4): 982–1012.

Petrosky-Nadeau, Nicolas, and Robert G. Valletta. 2021. "UI Generosity and Job Acceptance: Effects of the 2020 CARES Act." Unpublished.

Schmieder, Johannes F., and Till von Wachter. 2016. Table 2 from: "The Effects of Unemployment Insurance Benefits: New Evidence and Interpretation." *Annual Review of Economics* 8 (1): 547–81.

Schmieder, Johannes F., and Till von Wachter. 2017. "A Context-Robust Measure of the Disincentive Cost of Unemployment Insurance." *American Economic Review* 107 (5): 343–48.

Schmieder, Johannes F., Till von Wachter, and Stefan Bender. 2012. "The Effects of Extended Unemployment Insurance Over the Business Cycle: Evidence from Regression Discontinuity Estimates Over 20 Years." *Quarterly Journal of Economics* 127 (2): 701–52.

Shavell, Steven, and Laurence Weiss. 1979. "The Optimal Payment of Unemployment Insurance Benefits over Time." *Journal of Political Economy* 87 (6): 1347–62.