Economic Impact Payments and Household Spending During the Pandemic*

Jonathan A. Parker
MIT

Jake Schild
BLS

Laura Erhard
BLS

David S. Johnson
NAS

October 2022

Abstract

Households spent only a small fraction of their 2020 Economic Impact Payment (EIPs) within a month or two of arrival, consistent with i) pandemic constraints on spending, ii) other pandemic programs and social insurance, and iii) the broader disbursement of the EIPs compared to the economic losses during the early stages of the pandemic. While these EIPs did not fill an urgent economic need for most households, the first round of EIPs did provide timely pandemic insurance to some households who were more exposed to the economic losses from the pandemic. Households with lower liquid wealth entering the pandemic and those less able to earn while working from home each raised consumption more following receipt of their EIP. While our measurement for later EIPs is not as reliable, our estimates suggest even less spending on average to the second and third rounds of EIPs. Our point estimates imply less short-term spending on average than in response to economic stimulus payments in 2001 or 2008. While our analysis lacks the power to measure longer-term spending effects, the lack of short-term spending contributed to strong household balance sheets as the direct economic effects of the pandemic on households waned.

*Prepared for the Fall 2022 Brookings Papers on Economic Activity (BPEA) conference. Jianmeng Lyu and Ian Sapollnik provided excellent research assistance. For useful discussions, we thank Peter Ganong, Thesia Garner, Jianmeng Lyu, Claudia Sahm, Gianluca Violante, and in particular Karen Dynan, Jan Eberly, and Matt Rognlie as well as participants at the 2022 Society of Government Economists Annual Conference, the Conference on Direct Stimulus Payments to Individuals in the Covid-19 Pandemic at the Athens University of Economics and Business and University of Exeter Business School (online), a Bundesbank "Friendlyfaces" Workshop, and the Fall 2022 BPEA Conference. The initial phases of this work occurred while David was at the University of Michigan. Parker: JAParker@mit.edu; Schild: Schild.Jake@bls.gov; Erhard: Erhard.Laura@bls.gov; Johnson: johnsods@NAs.edu.

In response to the economic consequences of the pandemic, the United States government distributed three waves of Economic Impact Payments to American households. In March of 2020, following the declaration of a national emergency, Congress passed the Coronavirus Aid, Relief, and Economic Security (CARES) Act. The Act authorized more than $2 trillion of spending on programs that included the disbursement of $300 billion in Economic Impact Payments (EIPs) to the vast majority of Americans. In December 2020 with the pandemic continuing, the Coronavirus Response and Relief Supplemental Appropriations (CRRSA) Act authorized a second wave of roughly $150 billion in EIPs, and in March 2021, the American Rescue Plan (ARP) Act authorized a third round of just under $400 billion in EIPs.

While these payment programs were modelled on stimulus payment programs that the government had implemented at the beginning of recessions in both 2001 and 2008, the economic situation in the pandemic was entirely different. The pandemic caused a large collapse in production as well as demand, as people — partly at the behest of the government — cut back on both producing and consuming goods and services which risked exposure to COVID-19. Thus, the EIPs were not intended to stimulate demand for consumption but rather to provide pandemic insurance, ensuring that people who had unexpectedly lost their livelihoods could continue to cover their consumption needs and financial obligations. The EIPs were not targeted to those who had lost their incomes, but were widely distributed, presumably for reasons of feasibility and expediency, as well as to get aid to people who were impacted but not eligible for aid through other programs.

In this paper, we study the responses of consumer spending to the arrival of the EIPs and evaluate the extent to which the EIPs provided widespread, urgently-needed pandemic insurance. Focusing first on the spending response to the first round of EIPs, we estimate that the spending of the average household rose only a small amount over the couple of months following the arrival of their EIP, when compared to households that received later EIPs or did not receive EIPs at all, suggesting that the typical recipient was not in dire need of the EIP. We do however find larger spending responses both for those households with low levels of ex ante liquid wealth and for those more reliant on earnings from jobs with tasks that could not be done from home. While our data do not measure the arrival of the second and third rounds of EIPs as well as they do the first round, our estimates suggests even lower average, short-term spending responses to these final two rounds. Finally, we find some evidence of spending over the three months following our initial short-term spending estimates, but lack the statistical power to measure the spending effects of any round of EIPs over a longer period; we can only conclude that the lack of short-term spending contributed to strong household balance sheets as the economic effects of the pandemic waned following the three rounds of EIPs.

Our results are based on analysis of the Consumer Expenditure (CE) Interview Survey. We measure the average response of consumer spending to the receipt of an EIP using variation across households in receipt, in amount conditional on receipt, and in when they received a payment. As a baseline, we compare our estimates of spending to those reported in Johnson et al. (2006) and Parker et al. (2013) for the 2001 and 2008 tax payments using exactly the methodology employed in these papers. But there are substantial differences not only between program goals, but also between the structure of these payment programs and the structure of the EIP programs. The EIPs were disbursed more widely, more rapidly (and so less drawn out over time), more by direct deposit, and rounds one and three were larger than the payments in 2001 and 2008. Most importantly, the EIPs were disbursed without any randomization. Thus, while we compare our estimates to the spending responses estimated in the earlier literature, our main analysis uses an estimator that is both more robust to non-random differences in spending responses over time and better-suited for the variation across households in the EIP programs. In terms of more robust, our main analysis employs a method that is unbiased in the presence of significant difference in spending responses over time (for the same round of EIPs), a concern of a recently literature on treatment effects (see e.g. Borusyak et al., 2022; Orchard et al., 2022).

In terms of better-suited for the variation across households in the EIP programs, each round of EIPs was distributed mostly during one month and without any random variation across months. For example, the first round of EIPs had the most variation in timing and almost half of these EIPs were disbursed by direct deposit during the week of April 10 and almost 90% of 2020 EIPs disbursed within the first five weeks.[1] As a result, our main analysis leans heavily on comparing the spending of similar households that do and do not receive EIP and that receive EIPs of different amounts relative to their typical spending amounts. Receipt status is primarily driven by whether the IRS had the information to disburse the payment and whether the household was ineligible due to too high income or citizenship status.[2] Section III present our method including how we further modify the canonical method for the extreme volatility in expenditures during the pandemic.

Our first main finding is that the CE data show only small short-term spending increases

[1]We do not study the spending responses to EIPs that were received as part of income tax refunds or implicitly as lower tax payments.

[2]For the first round of EIPs for example, 3.8% of eligible households did not receive an EIP in 2020 because the IRS did not have the necessary information to disburse their EIP, and 16% of tax units were not eligible for an EIP because their incomes were too high or they did not meet the citizenship requirements (e.g., a couple with one non-citizen spouse that filed jointly; see Sections I and II).

on non-durable goods and services in response the receipt of an EIP. For the first round of payments in 2020, ninety-five percent confidence intervals imply that people increased their spending on non-durable goods and services as measured (roughly 44% of total expenditures measured in the CE) by between 4.6 and 15.8 percent of their EIP during the three-month CE reference period during which the EIP arrived.[3] We find a similar average propensity to increase consumer spending (MPC) for the second, smaller round of EIPs, disbursed mainly in January 2021 when the economy was somewhat more open. For the third round of EIPs in the spring of 2021, our estimates imply almost no spending response. An important caveat to these second two results is that receipt of these EIPs appear to be under-reported in the CE survey, and therefore these spending responses may be underestimated. Nonetheless, all three estimated spending responses on the broad measure of non-durable goods and services in the CE Survey are small, and suggest that most EIP dollars were not providing urgently-needed pandemic assistance.

These relatively low spending responses are consistent with the fact that the EIPs were disbursed far more broadly than the income losses caused by the pandemic, with the presence of pandemic constraints on spending, and with the large increase in household account balances during the pandemic. Roughly 145 million EIPs were disbursed by mid-2020 while employment dropped by 22 million during the pandemic recession.[4] Particularly during the first wave of EIPs, many types of consumption were constrained by the prevalence of the disease and/or by government restrictions which, together with diminishing marginal utility on unaffected goods and services, could have held back the overall expenditure response to the payments. Indeed, Guerrieri et al. (2020) make this assumption to study the macroeconomic consequences of the pandemic, and our results show some evidence of additional spending on durable goods for the first two EIP rounds, consistent with the shift in aggregate retail spending from services and towards durable goods during the pandemic.[5]

Particularly for the second and third round of EIPs, these low spending responses are also consistent with households on average already having plenty of liquid funds. As the constraints on spending relaxed, the pandemic reduction in spending coupled with other

[3]This propensity to increase consumer spending within a few weeks of the arrival of the first round of EIPs are somewhat lower than found in previous studies using aggregated data or information on select populations, issues we discuss below.

[4]Cajner et al. (2020) and Cox et al. (2020) document the large diversity in outcomes in the pandemic recession.

[5]In total, we estimate that about 24% of EIPs were on average spent in the three-month period in which they arrived on all CE expenditures. The spending responses to the EIPs were on average more tilted to durable goods than the spending responses to the 2001 tax rebates, but not that dissimilar from those to the 2008 economic stimulus payments.

government support (e.g. the paycheck protection program and expanded unemployment insurance benefits) including earlier EIPs may have raised average liquidity and lowered the need for households to spend during the second and third waves of EIPs. Finally, the third round of EIPs was large relative to all other payments and larger transitory increases in income in theory raise liquidity themselves and lead to smaller shares of the increase being spent in the short-run.

Did the EIPs cause later spending? We find some evidence of continued higher spending in the months following the three-month period of receipt, although these are fairly statistically uncertain. We estimate that the roughly 45 percent (round 1) and 60 percent (round 2) of people's EIPs were spent after the concurrent and subsequent three month period. We measure essentially no spending increase in response to the third-round of EIPs at any horizon. Our analysis has no power to estimate spending responses at longer horizons. However, following the disbursement of the EIPs, credit card balances decreased, liquid account balances increased, and stock prices for "meme"' retail stocks increased (see Grieg et al., 2022; Greenwood et al., 2022). Strong household balance sheets typically raise expenditures, and so may have contributed to higher demand as the pandemic waned.

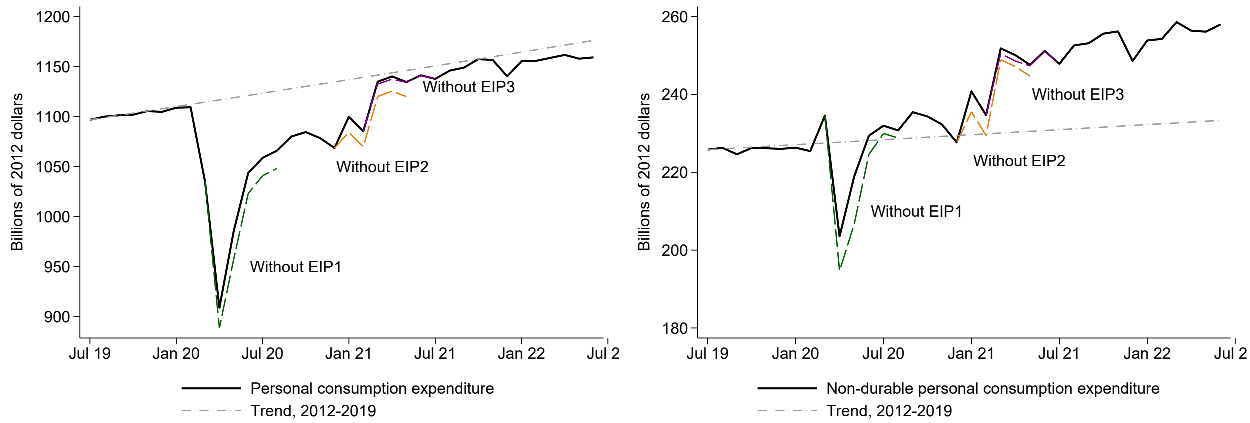
Figure 1 summarizes these findings by showing that the direct, short-run spending responses to the EIPs were relatively small. The Figure plots observed real personal consumption expenditures (PCE) and the same series subtracting off the increase in spending implied by our estimates assuming that the contemporaneous spending response occurs evenly over the month of receipt and the first following month and that the lagged spending response occurs evenly over the following three months. The lines without different EIPs in Figure 1 are thus not true counter-factuals, but are simply PCE without the partial-equilibrium effect of the EIPs on consumer spending based on this simple accounting exercise. Figure 1 not only shows the relatively small increase in direct spending implied by our estimates, but also highlights the extremely strong rebound in consumer demand for non-durable goods and services to which the EIPs may have contributed with delay through temporary decreases in debt or increases in saving.[6]

Our second main finding is that, while the average spending response to the EIPs are modest, we find significantly higher short-term spending responses for households that are more exposed to the economic losses from the pandemic, consistent with these households using the EIPs to fund spending that they could not easily do otherwise. Our first measure of exposure is low ex ante liquid wealth. For the first round of EIPs, households in the

[6] Note that for non-durable PCE, we use MPC estimates from a CE measure that includes some services and semi-durable spending so likely over-estimates the spending effects of the EIPs.

Figure 1: Implied change in Real Personal Consumption Expenditures directly due to disbursement of EIPs

(a) Personal Consumption Expenditures **(b)** Non-durable Personal Consumption Expenditures



Notes: Monthly personal consumption expenditures in billions of 2012 dollars (August 17, 2022). The trend line is the average monthly growth rate of real PCE from January 2012 to December 2019 applied to the real value of PCE from July 2019. Without EIP series are constructed by subtracting from PCE the spending implied by the MPC estimates from Table C.11 and the monthly EIP payments from the EIP Dashboard, Bureau of the Fiscal Service as of December 15, 2021. We assume that the contemporaneous spending occurs evenly in the month of receipt and the subsequent month, and that lagged spending occurs evenly over the following three months. We assume negative estimated spending is actually zero.

bottom third of the distribution of liquid wealth – those with less than $2,000 available ex ante – spent at roughly two and a half times the rate of those in the middle third, while those in the top third of the distribution of liquid wealth (above $12,500) had roughly no spending response. Later differences in liquidity across households are less important for the second two rounds.[7] Our second measure is based on whether a households earns a significant share of its income from work that is unlikely to be able to be done from home or remotely. Households with lower ability to work from home spent more out of their first-round EIPs when they arrived. We find no such evidence for later rounds of EIPs.

In sum, while on average the EIPs appear to have gone to many households with incomes that were unharmed by the pandemic, some of the EIPs, mainly in the first round, did support short-term spending for some households, primarily those with low ex ante liquid wealth and those reliant on income that could not be earned by working from home. In terms of future policy, both this paper and the research on consumption responses to tax payments more generally suggest that greater targeting of households with little liquid

[7]For the second round, we find essentially no spending response in the top third of the distribution of liquid wealth, but similar spending responses between the bottom two thirds. Finally, the only evidence for spending in response to receipt of the third round of EIPs is for the middle third of the distribution of liquid wealth.

wealth and low debt capacity would be more efficient in the sense of generating more rapid increases in demand for purposes of stimulus programs or getting more of the payment money to those households most vulnerable to income losses for pandemic insurance.[8] However, there are also potential moral hazard costs of targeting economic need or low liquidity more directly. One approach to minimizing these costs would be to base payments on household characteristics that are less responsive, for example not sending pandemic insurance payments to people who were not previously employed and therefore not at risk of losing their jobs (e.g. people who were retired in 2019 did not lose their jobs in 2020 and on average had increases in wealth). Alternatively, either stimulus or pandemic insurance could be delivered through increasing temporarily the generosity or eligibility of existing government programs that are based on direct targeting, such as unemployment assistance, Temporary Assistance for Needy Families, etc. where the disincentives of these programs are better understood and potentially better minimized (e.g. see Ganong et al., 2022).[9]

Most studies of the spending response to previous tax payments have estimated the response to payments using variation in spending between recipients and non-recipients (e.g. Bodkin, 1959; Agarwal and Qian, 2014; Kueng, 2018), over time (e.g. Souleles, 1999; Parker, 1999; Stephens, 2003; Farrell et al., 2019; Baugh et al., 2020), and using randomization in policy in either dimension (Agarwal et al., 2007; Broda and Parker, 2014; Parker, 2017; Lewis et al., 2021, in addition to those already cited).[10] The disbursement of the EIPs was not randomized in any way across households or time. Because of this, the present study as well as existing studies of the spending response to the EIPs focus on comparing spending before receipt to spending after receipt, comparing spending between recipients to non-recipients, and comparing households receiving different sized EIPs.[11]

The first rapid analysis of the spending changes caused by the EIPs, Meyer and Zhou (2020), used Bank of America transactions data and reports large increases in aggregated card spending on the day of and the day following receipt of an EIP associated with bank accounts that received EIPs on April 15 (when over 40% of EIPs were disbursed) relative to

[8]Past payments sent out either as pandemic insurance or stimulus programs have increasingly targeted these populations to some extent by excluding households with high previous-year incomes.

[9]For pandemic insurance, Romer and Romer (2022) also suggest a role for policy in providing hazard pay. For the purposes of economic stimulus, it is also worth noting that government spending generates immediate spending by definition, and so in this sense is equivalent to an MPC of 100% out of a payment program. That is, rapid government spending raises aggregate demand by more than equivalent-cost payment programs, although obviously the goods and services purchased will differ, as will the distributional effects of the policies.

[10]Most closely related, Fagereng et al. (2021) measures the spending response of (random) lottery winners.

[11]Kubota et al. (2020), Feldman and Heffetz (2020), and Kim et al. (2020) measure the spending responses to tax payments disbursed in response to the pandemic in Japan, Israel, and South Korea respectively.

those that did not. Daily spending increased by an average of 50% year over year between April 15 and 16 for households with incomes below $50,000 and by only 3% for households with incomes above $125,000. Also using aggregated data, Chetty et al. (2021) finds that over this same couple of days, card spending in zip codes in the bottom quarter of the distribution of average household income rose by 25% while those in the top quarter of the distribution rose by only 8%. Finally, also using zip code level data and using incidental differences in timing in EIP disbursements across zip codes, Misra et al. (2021) infers an MPC of 50% in the few days after an EIP arrives.

Our evidence shows lower spending responses than measured in existing studies, all of which use account-level data on financial transactions to measure the spending. Karger and Rajan (2021), Baker et al. (forthcoming), and Cooper and Olivei (2021)) find that people's out-of-account spending rises cumulatively by 46%, 25-35%, and 66% of their first-round EIPs, respectively, within a few weeks of receipt.[12] One likely reason for these larger spending responses than found in the CE Survey data is that these account-level studies cover populations that are likely to have larger spending responses than average.[13] There are other possible reason also, such as the different ways in which the studies measure consumer expenditures. Account-level data on transactions may mischaracterize debt payments or saving as consumption (e.g. paying debt on un-linked credit cards, payments of overdue bills from past consumption, or transfers to investment accounts).[14] Alternatively, respondents in the CE Survey could forget to report EIP-induced purchases. Finally, the differences could arise in part from statistical issues, both the statistical uncertainty inherent in any estimator and the statistical methods that we use.[15]

[12] Karger and Rajan (2021) also estimate a 39% MPC for the second round of EIPs.

[13] The accounts used in Karger and Rajan (2021) are skewed towards lower income households (average annual income of $20,880), the households Baker et al. (forthcoming) are those that have opted to use a financial app designed to help them save (and have average incomes of $36,000), and Cooper and Olivei (2021) uses Facteus data covering lower-income households many of whom are un-banked.

[14] Baker et al. (forthcoming) include car loans and mortgage payments as consumption-related, whereas this paper includes interest payments on mortgage loans as part of consumption-related spending, but not the principal.

[15] The CE is a small dataset, with a similar number of recipients to that in Baker et al. (forthcoming), and standard errors are a substantial share of the differences among the estimates across the papers. The randomness of the estimator may also explain the difference between our estimated spending propensities and those estimated in the CE during previous tax rebate episodes.

I The Economic Impact Payments

In response to the economic fallout from the pandemic, the Federal government passed three pieces of legislation each of which authorized the disbursement of a round of what came to be called Economic Impact Payments (EIPs): the CARES Act of March 2020, the Coronavirus Response and Relief Supplemental Appropriations (CRRSA) Act of 2021, and the American Rescue Plan (ARP) Act of March 2021.[16] We organize our description of the EIP programs around the three ways in which EIPs differed across households: differences in dollar amount conditional on receipt, differences in the time of receipt of the EIP, and whether a household did or did not receive an EIP at all. Unlike when payments were disbursed in 2001 and 2008, none of these three sources of variation are completely unrelated to household characteristics.

In terms of amount, the first round of EIPs (which we call EIP1s) consisted of a base payment of $1,200 for an individual, $2,400 for a couple filing jointly, and additional payments of $500 for each qualifying dependent under age 17. The CARES Act set upper income thresholds for receiving the full payment of $75,000 for an individual, $112,500 for a head of household, and $150,000 for couples filing jointly, where income was based on 2019 adjusted gross income (AGI) if the taxpayer had already filed their 2019 tax return in 2020, otherwise income was based on 2018 AGI as reported in 2019 tax filings.[17] For every $100 of adjusted gross income over the threshold the stimulus payment was reduced by $5.[18]

Second round EIPs, EIP2s, were smaller, consisting of a base payment of $600 for an individual or $1,200 for a couple filing jointly, and additional payments of $600 for each qualifying dependent under age 17. The upper income thresholds and phase-out rate for this second round of EIPs were the same as for the first round.[19]

The third round of EIPs, EIP3s, were substantially larger than EIP1s or EIP2s. They consisted of a base payment of $1,400 for an individual, $2,800 for a couple filing jointly, and

[16]The Coronavirus Response and Relief Supplemental Appropriations Act was include as a part of the Consolidate Appropriations Act of 2021, which was signed into law on December 27, 2020.

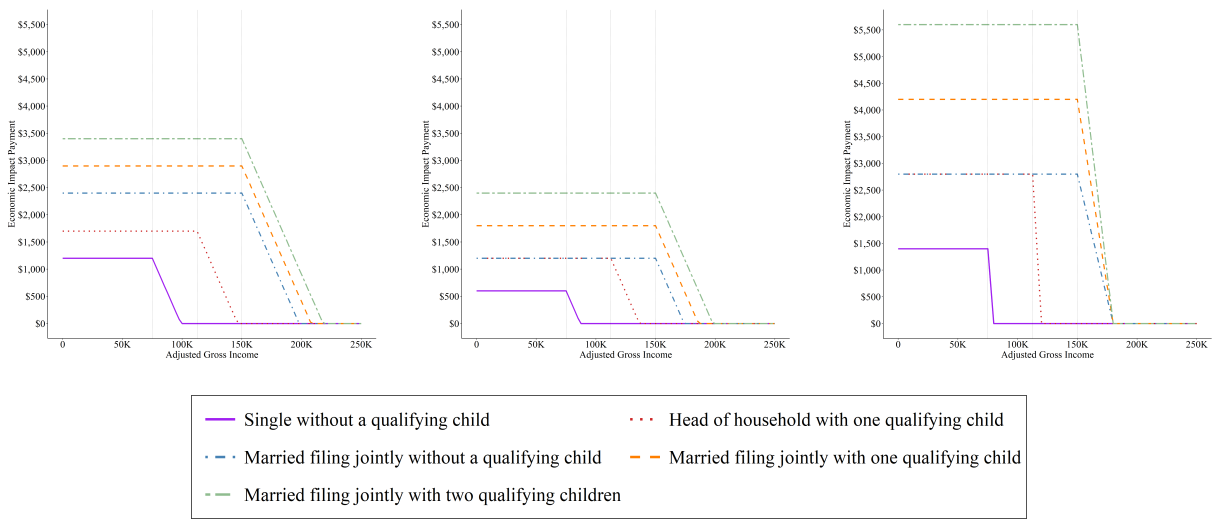
[17]In December 2020, the phase-out threshold for a qualifying widow(er) increased from $75,000 to $150,000, according to the IRS. This change does not affect our analysis.

[18]In an article released by The Hill (Bolton, 2020), Republican senators are referenced saying they want to model the recovery rebate on the stimulus checks former President George W. Bush sent out during the 2008 financial crisis. The 2008 rebate had income thresholds of $75,000 for individuals and $150,000 for couples filing jointly, and were phased out at a rate of $5 for every $100 of income over the threshold.

[19]For the second round of EIP, income is defined as the tax filer's 2019 AGI reported on their 2020 tax filings. If a tax return had not been filed by the time the payments were distributed, the tax filer did not receive an advanced payment and had to claim the Recovery Rebate when filing their 2020 tax return in 2021.

Figure 2: Economic Impact Payment Amounts as a Function of AGI and Family Structure

(a) CARES Act **(b)** CRRSA Act **(c)** ARP Act



additional payments of $1,400 for each qualifying dependent. They were also distributed slightly more broadly along several small dimensions, including that the definition of "qualifying dependent" was expanded to include dependents over the age of 17. The upper income thresholds were the same as the first and second rounds; however, the phase-out rule was more aggressive so that the larger amounts did not lead to EIPs being received higher up the income distribution. Specifically, rather that a constant phase out rate, income thresholds were set such that tax filers with 2020 AGI above $80,000 for an individual, $120,000 for a head of household, and $160,000 for a couple filing jointly, regardless of the number of qualifying dependents, did not receive an EIP3.[20] For example, an individual with no dependents, base payment of $1,400, had a phase out rate of $28 for every $100 of AGI over $75,000, whereas an individual with one qualifying dependent, base payment of $2,800, had a phase out rate of $56 for every $100 of AGI over $75,000. Figures 2a, 2b, and 2c display the EIP amounts as a function of income for various family structures for the first, second, and third round of EIPs, respectively.

In addition to households receiving different amounts of EIPs, households also received them at different times. In each round, most taxpayers who had included their bank information when filing a recent tax return (e.g., for a refund) received their EIP during the first week of disbursement. For EIP1, bank information came from a 2018 or 2019 tax return, and for EIP2 and EIP3, bank information came from a 2019 or 2020 tax return. The IRS also

[20]If a 2020 tax return had not yet been filed, then 2019 AGI from the 2019 tax return filed in 2020 was used instead.

launched a web page where households could enter their information for the IRS if they either had omitted bank information from their returns or were eligible but had not filed 2018 or 2019 returns.[21] For EIP1, this constituted roughly 35 million households. The IRS also collected information on eligible households from the Social Security Administration and the Veterans Administration (and the Railroad Retirement Board).

The IRS began depositing EIP1s into bank accounts on April 10, 2020, and using the information that the IRS was able to gather and process in time, roughly 105 million or about 63% of all EIPs were disbursed in April 2020. For eligible households without the necessary bank information, the EIPs arrived starting two weeks after April 10 by mailing a paper check or pre-paid "EIP" card. The disbursement of checks occurred with a greater delay. By the end of April only about 7 million checks (4% of EIPs) were sent out. Most of the checks were sent out in May, about 27 million or 16% of EIPs, and all of the "EIP" cards were sent out in May, about 4 million or 2% of EIPs. About 95% of all first round EIPs were delivered in the first two months of disbursement. The remainder of the EIPs continued to trickle over the rest of 2020. Figure 3a shows the minimal variation in timing of the distribution of CARES Act EIPs.

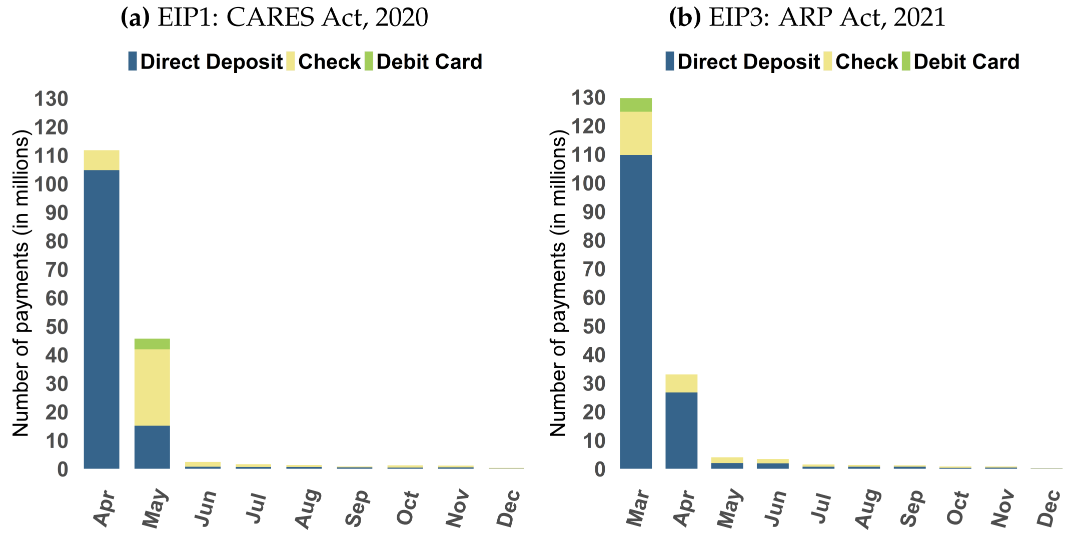
In contrast, the disbursement of the second round of EIPs has almost no variation across months. For EIP2, almost all of the second round EIPs were distributed in January 2021 (see Bureau of the Fiscal Service (2022)). Daily Treasury Statements show some EIP2s also being disbursed in February, which is due to reissuing payments that were initially unable to be delivered.

The disbursement of the third round of EIPs was slightly more drawn out over time than that of the EIP2s, but still more concentrated over time than the first round of EIPs. A full 74% of all EIP3s were distributed in March 2021 (62% by direct deposit; 8.5% by check; and 2.7% by EIP cards). By the end of April about 92% of all third round EIPs had been distributed, with the remaining 8% distributed over the remainder of 2021. Although the IRS distributed a smaller percentage of EIP3 in the first two months of disbursement compared to EIP1, about 5 million more EIPs were distributed during March and April of 2021 than compared to April and May of 2020. Additionally, about 20 million (7%) more EIPs were distributed by direct deposit. Figure 3 displays the variation in the timing of disbursement of EIP1s and EIP3s.

Finally, there is a set of households that either did not receive EIPs at all or who received their EIPs after filing their taxes as part of their income tax refunds or implicitly as reduced

[21] IRS web page "Get My Payment;" https://www.irs.gov/coronavirus/get-my-payment (downloaded October 2021)

Figure 3: The disbursement of EIP payments over time and by mode of distribution



Source: Data acquired from the Bureau of the Fiscal Service (2022). Months are the disbursement months.

income tax payments. There are three main reasons why a household did not receive an EIP during each primary disbursement period. First, an individual was ineligible for an EIP if they did not have a Social Security Number (SSN) valid for employment. The CARES Act was worded such that families were ineligible if they had filed jointly and one of the spouses was not a US citizen, a situation affecting an estimated 14.4 million people (Gelatt et al., January 15 2021). The CRRSA Act changed this requirement. A married couple filing a joint return became eligible for a partial Recovery Rebate credit when only one spouse has a SSN. This change resulted in 2.9 million people becoming eligible.[22] The ARP Act further expanded the eligibility criteria to anyone with a SSN, which resulted in an additional 2.2 million eligible individuals.[23]

Second, eligible households did not receive an EIP disbursement if they had changed accounts and/or addresses during the relevant previous year, if they had not given their information to the IRS, or if the IRS did not otherwise have their information (e.g., from

[22]Of these 2.9 million people, 1.4 million were US citizens or legal immigrants and spouses of an unauthorized immigrant, and 1.5 million were children with one unauthorized immigrant parent. The change in eligibility criteria was applied retroactively, which means not only did these individuals now qualify for the second EIP, but they were also able to claim the first EIP through the Recovery Rebate tax credit on their 2020 tax filing.

[23]These 2.2 million individuals are children whose parents (or parent) are unauthorized immigrants. Since no parent had a SSN, they were ineligible for the first and second EIP, which means their children were also ineligible.

the Social Security Administration). For example, four months after the CARES Act (by the end of July), 10 percent of EIPs had not been disbursed, and 5 percent or nine million eligible households had not received an EIP by the end of September (Murphy, 2021). For EIP2 or EIP3, people who re-located even temporarily during the pandemic and formally changed their addresses or banks accounts became ineligible for EIP disbursement.

Finally, the third reason that households would not receive an EIP is that EIP amounts declined to zero as income increases. As shown in Figure 2 high-income households were not eligible and a significant number of higher-income households that received EIPs in the first two rounds were not eligible for an EIP3.

Taxpayers that fell into either of the first two categories and so were ineligible for a disbursed EIP but were eligible for an EIP, could receive their EIPs as tax credits when they filed their 2020 taxes in 2021 for EIP1 and EIP2, and when they filed their 2021 taxes in 2022 for EIP3. More generally, taxpayers were also eligible to receive a tax credit for any amount by which the EIP they were due based on their final tax information exceeded the amount they had been disbursed. These "true ups" amounted to roughly \$44 billion in tax years 2020 and \$18 billion in tax year 2021 (Splinter, 2022). There was no corresponding payment required however if a disbursed EIP exceeded the amount that should have been disbursed based on the later tax information.[24]

In aggregate \$271 billion was disbursed during the first EIP round, \$141 billion during the second EIP round, and \$390 billion during the third EIP round (Internal Revenue Service, June 15 2022). Alone, any one of these rounds is much larger than the previous 2008 program which disbursed \$120 billion in 2020 dollars, which in turn was close to double the total of the 2001 rebate program. Combined, the three rounds of EIP disbursed more than six times the amount disbursed with the 2008 program. About \$260 billion worth of EIPs were disbursed in the second quarter of 2020, which corresponds to about 5.3 percent of GDP or 8.0 percent of PCE in that quarter (Figure 3 and Internal Revenue Service, May 22 2020). The first quarter of 2021 saw \$473 billion of EIPs disbursed, from both the second and third waves. This represents 2.1 percent of GDP and 3.2 percent of PCE. The third EIP wave additionally disbursed \$67 billion in the second quarter of 2021, corresponding to 0.29% of GDP and 0.42% of PCE. The next section describes the EIPs as recorded in our CE dataset.

[24] These safe harbor amounts were roughly \$21 billion in tax years 2020 and \$43 billion in tax year 2021 (Splinter, 2022).

II The Consumer Expenditure Survey

Data for this study are from the Consumer Expenditure (CE) Interview Survey, a household survey run by the Bureau of Labor Statistics. The CE data set contains spending, demographics, and other financial information on households living in the U.S. The Bureau of Labor Statistics (BLS) structures the CE so that a consumer unit (CU) at a given address, which we will refer to a household, is interviewed up to four times at three month intervals about their spending over the previous three months ("reference period"). New CUs are added to the survey every month, and while a significant dollar share of spending data is reported at the monthly level, a little over half of spending is only reported for the entire three-month reference period. Thus, we use the data at the (overlapping) three-month frequency.[25] Appendix A.2 contains more details about CE files and variables we use in this study.

Following the passage of the CARES Act, the BLS added a module of questions about the EIPs to the CE survey starting with the June 2020 interviews and continuing until the October 2021 interviews, with the exception the questions were not fielded in January 2021.[26] These questions were worded similarly to questions that the BLS added to the CE about stimulus payments in 2008. The questions measure the date of receipt, the number of EIPs received, the amount received, which member or members of the household the payment was for, and the mode of receipt (by check, direct deposit, or debit card).[27] The questions were phrased to be consistent with the style of other CE questions and the questions on previous CE surveys about the 2001 and 2008 tax rebates. Although the wording did not follow exactly previous CE surveys, the module of questions also asked whether the EIP was used mostly to add to savings, mostly to pay for expenses, or mostly to pay off debt. Appendix A.1 contains the language of the CE survey instruments.

The fact that the EIP questions were not included in the May 2020 interview ques-

[25] "Overlapping" means for CUs interviewed within two months of each other the three-month reference period for reporting spending will include some of the same months. For example, a CU who is interviewed in June has a three-month reference period of March, April, and May, and a CU interviewed in July has a three-month reference period of April, May, and June. Both reference periods include April and May; thus, we consider them overlapping.

[26] The module was developed by the BLS partly based on the similar questions from 2008 and in consultation with others in the federal statistical system, particularly those working with the Household Pulse Survey (in which EIP questions had already been asked), and outside researchers, two of whom are co-authors of this paper.

[27] Starting with the July interview the mode of receipt question was expanded to include via tax rebate. Any instances of receipt via tax rebate were dropped, which resulted in 5 relevant rebates being excluded. Prior to the July interview, CUs who received an EIP via their tax rebate were asked to not include it when reporting EIP receipt.

tionnaire means that, even for EIP1 where the distribution of EIPs was somewhat drawn out over time, we have very little power to identify the impact of the arrival of EIP1s on spending using only variation in the timing of receipt across households. The vast majority of EIP1s were disbursed in April and May. And while April and May are in different three-month expenditure recall periods for households on the May interview cycle, they are not for households on the June or July interview cycle. Thus, we cannot compare how spending differs between April and May depending on whether an EIP1 is received in April or May. Since EIP2 and EIP3 have very little variation in the timing of receipt, and since only about 10% of EIP1s arrive after May 2020, we focus primarily on analysis that leans heavily on other sources of variation, like amount and recipient status.[28]

A second implication of the lack of EIP questions on the May 2020 survey is that we have no way to tell whether households interviewed in May received the EIP1 or not during the previous three months. The reference period for the May 2020 interview includes April when over half of all EIPs were disbursed. Thus, we drop all households on this interview cycle because we cannot compare the spending of those receiving different EIPs at different times (or not at all) since we do not have the EIP information. More precisely, we restrict our sample to households that had an interview during June or July of 2020 when the EIP questions were asked and the three-month recall periods include April and May 2020. This restriction drops roughly one third of households – those in the interview cycle that includes May 2020, as well as any other households that are missing interviews in June or July 2020 interviews. To be clear, we use all available interviews for the households that have interviews in June or July 2020 (provided the observation has the other necessary data and a consecutive interview also with valid data). However, the loss of the observations on the May interview cycle reduces statistical power.

We face a similar, but less significant challenge for households interviewed in January 2021. In this case, we assume no EIPs were received in the references period (October, November, and December) for households interviewed in January 2021 (when the EIP questions were not asked).[29]

We construct two main samples of CE households for each EIP round. For each round, we limit the sample to households with interviews during the main period of disbursement: June and July 2020 for EIP1, February, March, or April 2021 for EIP2, and April, May, or

[28]We investigated measuring the spending response to the EIP1 using the data at the monthly frequency and only the CE expenditure categories that are collected by month, but found weak statistical power (consistent with the conclusions of prior work with the CE).

[29]Less than 2% of EIP1s were distributed over October, November, and December. EIP2s began being distributed by direct deposit during the last few days of December, but did not clear until January 4th, the official payment date according to the IRS. Checks for EIP2 did not begin being distributed until January.

June for EIP3. For each, we construct first a broad sample we refer to as *all households* that makes minimal additional drops and follows exactly earlier analyses of tax rebates in the CE. Details are provided in Appendix A.5.3. Second, motivated both by the unprecedented nature of the pandemic and programmatic differences between the EIPs and previous tax rebates, we construct our *final sample* by adjusting the way in which older households and households with very low levels of reported expenditures are dropped and dropping high income households who are mostly ineligible for EIPs (see details in Appendix A.5.3 and Table C.5 to Table C.7). We discuss these choices in detail in the next section.

Tables 1 show that the monthly distribution of EIPs reported in the CE line up reasonably well with other official statistics. The first two columns of Table 1 show statistics for our final sample (which drops high-income households as described subsequently); the second two columns show statistics for the CE data including all (available) interview months. For EIP1, April data for the raw CE sample is adjusted up by fifty percent to account for our dropping one third of recipients, those interviewed in May when the EIP questions were not asked. The CE data have slightly fewer EIP1s reported during the peak month of April and more in the following months than the US Treasury reports. This difference is consistent with some time delay between disbursement and receipt for mailed payments and with some households taking time to notice EIPs deposited into their accounts (and with the possibility that some households report a later date of receipt than actually occurred).[30] For later rounds of EIP, the monthly distribution lines up well with what we know from other sources also.

Columns 3 and 4 of Panel B in Table 1 show that 24% of households do not receive an EIP1 according to the CE data compared to 20% in reality (3.2% of households were eligible tax units who were non-recipients in 2020, and 16% of households were not eligible for EIPs). In our final CE dataset, about 17% of households do not receive an EIP1 because we drop households with high incomes (as noted on page 21). As shown in panels D and F, these numbers are larger for EIP2 and EIP3, and while EIP3 was phased out more rapidly with income, so that fewer households received the payments, these numbers suggest that the CE data is missing some EIPs.

In terms of dollar amounts, the average value of EIP1s received in a reference period, conditional on a positive value, is $2,098, slightly higher than the average individual EIP of $1,676 reported by the IRS (Internal Revenue Service, June 15 2022).[31] The average EIP2

[30] In the final sample, about 10% of households that get EIPs report multiple EIPs. About 50% of these report EIPs in more than one month of which about 60% report receiving EIPs in different reference periods.

[31] When using all CE data, and without aggregating to the three-month reference period level, the average

Table 1: Percent of EIPs by month and percent of households not receiving EIPs

	Unweighted CE final sample	Weighted CE final sample	Unweighted CE (adjusted)	Weighted CE (adjusted)	Census Bureau's Household Pulse Survey and U.S. Treasury
	Panel A: The distribution of EIP1s across months, in percent				
April 2020	53.8	54.6	53.1	54.1	66.4
May 2020	36.3	35.4	35.3	34.3	25.7
June 2020	7.5	7.7	8.9	9.0	1.1
Jul to Nov 2020	2.4	2.3	2.7	2.6	6.8
	Panel B: Percent of households or tax units not receiving an EIP1				
Total (households)	17.0	17.0	24.7	24.6	
Ineligible (tax units)					16.2
Eligible (tax units)					3.2
	Panel C: The distribution of EIP2s across months, in percent				
December 2020	24.3	24.2	19.6	19.4	0
January 2021	68.6	68.5	64.2	63.7	100
February 2021	7.1	7.3	16.2	16.9	0
	Panel D: Percent of households or tax units not receiving an EIP2				
Total (households)	50.8	51.9	52.2	53.0	
	Panel E: The distribution of EIP3s across months, in percent				
March 2021	68.2	68.8	65.8	66.2	73.8
April 2021	23.7	23.3	25.9	25.8	18.8
May 2021	3.5	3.2	3.6	3.4	2.3
Jun to Dec 2021	4.6	4.7	4.6	4.6	5.2
	Panel F: Percent of households or tax units not receiving an EIP3				
Total (households)	29.4	29.0	40.5	40.3	

Notes: Weighted data using the average of FINLWT21 across all interviews. All samples use available CE data, so interviews through and including September 2021. See Appendix A.5.3 for CE sample construction and adjustments for months in which EIP questions were not asked. 'Unweighted CE' includes all households with interviews in these months. In Panels A, C, and E, months are recipient months in the first four columns but are disbursement months in the last column. In the final column of Panels B ineligible households is as self-reported in the Census Pulse Survey from Garner et al. (2020) and eligible households not receiving payments are counted through October 2020 as reported in Murphy (2021). For Panels C and E, the disbursement data comes from the Bureau of the Fiscal Service, Department of the U.S. Treasury.

amount is $1,301, and the average EIP3 amount is more than double this amount, $2,814. Appendix tables C.1, C.2, and C.2, shows the distribution of total EIP amounts received across household-reference-periods in our CE final sample (unweighted, unadjusted) and shows households (correctly) report most amounts at the standard EIP amounts disbursed in each round. For example, consistent with the payments specified by CARES, most reported EIP1s are at the base amounts or in multiples of $500 above them: about 55% of households report payments of $1,200 (the basic payment for a single filer) or $2,400 (a couple filing separately or getting the basic payment as joint filers or a single filer with two children).

According to the IRS, there were 162 million first-round EIPs disbursed in 2020 totaling $271 billion, 147 million second-round EIPs totaling $141 billion (as of early Februray 2021), and 167 million third-round EIPs disbursed in 2021 totaling $390 billion (Internal Revenue Service, June 15 2022). In the weighted CE data, and scaling up for the interviews missing for first-round EIPs, we find 138 million first-round EIPs totaling $261 billion, 79 million second-round EIPs totaling $106 billion, and 111 million third round EIPs totaling $254 billion.[32] Households that receive EIP1 and EIP2 by direct deposit on average have slightly higher expenditures, are slightly younger, have higher incomes, lower liquidity, and have larger EIPs, than households that receive the payments by mail, but for EIP3, households that receive the payments by direct deposit are slightly older, and have lower incomes.

The fractions of EIPs reported by households as received by direct deposit, by paper check, and by debit card match very closely the fractions reported by the Treasury as disbursed by these methods. Panel A of Table 2 shows that 75% of EIP1s in the CE were reported as being received by direct deposit, 23% by paper check, and 2% by debit card. The Treasury reports that 76% of EIP1s were disbursed by electronic deposit, 22% by paper check, and 2% by debit card during 2020.[33] Though there were no explicit instructions, CE respondents likely reported EIPs that were deposited onto federal benefit cards (Direct Express Cards) as received by debit card, and while directly comparable numbers from the Treasury are not available, through June 2020, 3% of EIP1s had been distributed by debit card and an additional 1% by deposit onto benefit cards (Murphy, 2021). Consistent with the increase in direct deposit across waves, the CE shows the share of households receiving their EIP by direct deposit increasing in each subsequent wave.

(unweighted, unadjusted) EIP is $1,837.

[32]The lower number in the CE for first-round EIPs is in small part a result of not including information from CE interviews after December 2020, and similar for third-round EIPs, since interviews after September 2021 is not yet published.

[33]https://www.irs.gov/statistics/soi-tax-stats-coronavirus-aid-relief-and-economic-security-act-cares-act-statisticsEIP1 (Downloaded Oct 28, 2021).

Table 2: The share of EIPs by method of disbursement and reported main use

	EIP1	EIP2	EIP3
	Panel A: Distribution of payment methods, in percent		
By direct deposit	74.5	77.7	84.6
By check	23.4	15.8	11.7
By debit card	2.1	6.5	3.7
	Panel B: Distribution of reported main use, in percent		
Mostly for expenses	56.4	54.5	51.9
Mostly paid off debts	17.8	19.8	19.1
Mostly added to savings	25.9	25.7	29.0

Notes: Statistics based on 'CE final sample' include only CE households with certain interviews (June or July 2020 for EIPI, February, March, or April 2021 for EIPII, and April, May, or June 2021 for EIPIII), with income that does not exceed a certain threshold determined by marital status and family structure, and cleaning described in Appendix A.5.3. Weights applied are the average of CU weights across all interviews for that CU.

The BLS also asked households to report on the CE Survey whether they spent or saved their EIPs (the reported preference methodology of Shapiro and Slemrod, 1995). The responses suggest greater spending than our analysis of expenditures does. Panel B of Table 2 shows that 56% of households report using their EIP1s mostly for expenses, and this fraction declines slightly across EIP waves. There is also a significant increase in the share of households reporting mostly saving their EIPs in round three relative to earlier EIPs. In 2008, the BLS added different questions to the CE survey that were more similar to those in Shapiro and Slemrod (1995, 2009) and found that 32% of households would "mostly spend" their tax payments and 51% would "mostly pay down debt."

More comparable over time, Sahm et al. (2010) and Sahm et al. (2020) ask the same questions in both 2008 and 2020 (not in the CE Survey) and the changes in answers suggest only very slightly lower spending responses in 2020 than in 2008. In response to the EIPs, 18% of respondents report that their EIPs will cause them to "mostly increase spending," only one percent lower than in 2008, which suggests little difference in rate of spending between the EIPs and earlier stimulus payments.[34]

Following previous research on spending responses using the CE, we construct four measures of consumer expenditures at the three-month frequency: 1) food, which includes

[34] Schild and Garner (2020), Garner et al. (2020), and Boutros (2020) provide in depth analysis of the U.S. Census Bureau's Household Pulse Survey (HPS) in which 59% of respondents state that they "will mostly pay for expenses" with their EIPs. More similar to the Sahm et al. (2020) shares, Coibion et al. (2021) shows that only 15 percent of households in the Nielsen Consumer Panel report that they mostly spent or expect to spend their EIPs. Among these households, the average spending rate is 40%. Armantier et al. (2020) reports a slightly larger number in the New York Fed Survey of Consumer Expectations survey in which households on average say that they consumed 29% of their EIPs.

food consumed away from home, food consumed at home, and purchases of alcoholic beverages; 2) strictly nondurable expenditures, which includes some services and adds expenditures such as household operations, gas, and personal care following Lusardi (1996); 3) non-durable expenditures on goods and services, which adds semi-durable categories like apparel, reading materials, and health care (only out-of-pocket spending by the household) following previous research using the CE survey; 4) total expenditures, which adds durable expenditures such as home furnishings, entertainment equipment, and auto purchases.[35]

Relative to the administrative data used in the studies of the EIPs discussed in the introduction, there are three main advantages of using the CE interview survey as well as three weaknesses. The first advantage is that the CE contains detailed measures of consumer expenditures rather than just the transaction counterpart, or, for some transactions like checks or cash, just the amount.[36] Second, the CE tracks spending and EIP receipt by individual consumer units, rather than by accounts (and linked credit or debit cards). Finally, the CE is a stratified random sample of U.S. households constructed by the U.S. Census and so when weighted is representative of the U.S. population (along the dimensions of the census-based strata and conditional on participation in the survey). The main weakness relative to existing studies are the relatively small sample size, sampling (e.g., non-response) error, and the presence of measurement error in expenditures and EIP receipt.

The next section discusses how and why our estimation methodology differs from previous approaches, as well as presenting the results of applying the previous methodology exactly to estimate the average spending response to the EIPs. The following section presents our baseline estimates of spending rates based on an approach that account for the differences both between previous tax rebates and the 2020 EIPs, and between previous recessions and the pandemic recession.

III Estimation method

In this section, we first briefly present the way Johnson et al. (2006) and Parker et al. (2013) estimate the consumer expenditure responses to the tax rebates disbursed in 2001 and 2008. We then refine this methodology and adopt identifying assumptions that are better suited to estimating the spending effects of these EIPs given programmatic differences, the

[35]The exact definitions are given in Appendix A.3.

[36]E.g., terms like Amazon or Starbucks or Sammy White's. Payments to un-linked credit cards and transfers to other accounts are also difficult to categorize as spending for consumption, debt payment, or saving.

pandemic situation, and the possibility of cross-cohort differences in spending propensities within each EIP round.

Using samples analogous to our sample of all CE households, the previous papers estimate an equation analogous to the following equation for household i with consumer expenditures, $C_{i,t}$, observed for (overlapping) three-month period t:

$$\Delta C_{i,t} = \sum_{s=0}^{S} \beta_s EIPn_{i,t-s} + \boldsymbol{X}_{i,t}\gamma + \tau_t + \epsilon_{i,t} \tag{1}$$

The key regressor is $EIPn_{i,t-s}$, the total dollar amount of economic impact payments from round $n \in \{1,2,3\}$ received by household i in three-month period $t-s$.[37] The variable τ_t is a complete set of time effects for every period in the sample that control for the seasonal variation in consumer expenditures as well as the average effect of all other concurrent aggregate factors. The control variables $\boldsymbol{X}_{i,t}$ contain age ($age_{i,t}$) and change in family size ($\Delta FamSize_{i,t}$) which control for the life-cycle pattern of spending and for changes in consumption needs. Finally, ϵ captures movements in consumer expenditures due to individual-level factors such as changes in income, expectations, and consumption needs, as well as measurement and recall error in expenditures.

Provided ϵ is uncorrelated with the other right-hand-side regressors (and for now maintaining the assumption that β (or its distribution over i) does vary with EIP arrival date), the key coefficient β_s measures the average partial-equilibrium response of household consumer expenditures to the arrival of the EIP during the three-month period s periods after the EIP arrives. In our main analysis, in which $EIPn_{i,t-s}$ is regressed on ΔC, β_s measures the share of the EIP spent, or the marginal propensity to increase consumer expenditures (MPC).[38] These estimated MPCs are based on three sources of variation: whether a household receives an EIP or not, variation in the (overlapping) three-month period in which the EIP is received, and variation in the amount of the EIP.

As we show at the end of Section IV, estimates of the spending responses based on this exact methodology — while having the advantage of being most comparable to earlier work — are small, statistically weak, and unstable compared to these earlier analyses. The first finding may simply reflect reality, but the second two may be indicative of problems

[37]In Table C.8 and in additional results in the appendix, we sometimes replace this regressor with $\mathbb{1}[EIPn_{i,t-s} > 0]$, an indicator variable for whether an EIP from round n is received (in the period $t-s$) at all. In the appendix, we present some results that use change in log consumption as the dependent variable.

[38]When $\mathbb{1}[EIPn_{i,t-s} > 0]$ is regressed on ΔC, β_s measures the dollars spent. And when $\mathbb{1}[EIPn_{i,t-s} > 0]$ is regressed on ΔlnC, $100 * \beta_s$ measures the percent increase in spending.

with the methodology, driven by: i) differences between previous tax rebate programs and this one, ii) differences between previous recessions and the pandemic recession, and iii) concerns raised recently about consistent estimation if MPCs vary across households such that the distribution of $\beta_{s,i}$ changes over time.

Our first main concern, is differences between previous tax rebates and these EIPs. Relative to the earlier studies, the timing of the disbursement of the EIPs was not randomized in any way and was far more limited, both in reality (as described in Section I) and observed in our data (for the reasons described in Section II). Therefore our estimation necessarily relies more on differences in spending patterns across households with different EIP amounts, including those that do not receive EIPs (at least only as part of lower tax payments or higher refunds in the following year).

Our solution is to make the sample of non-recipients more similar to recipients by excluding households with high incomes from our analysis. Motivated by the phase-out of the EIPs described in Section I, for each EIP round, we first posit an income cutoff at the nearest \$25,000 above the income level (rounded to the nearest \$25,000) at which a household would no longer receive an EIP. Different cutoffs apply to households with different family structures – whether the household contains children and whether it has one single adult, a married individual or couple, or multiple adults. In addition, note that recipient status is not a clean function of CE income because EIPs are disbursed based on adjusted gross income rather than the pre-tax income we observe in the CE, because reported income has some error, and because the IRS uses calendar year income for either 2018 or 2019 and neither year nor filing status is collected as part of the CE Survey.[39] Thus, we adjust each income cutoff up in increments of \$25,000 until more than 80% of the observations with incomes in the \$25,000 range just above the cutoff are from non-recipients. Additionally, we set the cutoff for households with kids to be no lower than the cutoff for households that are otherwise the same but without kids (i.e., married without kids and married with kids), if the former has a lower cutoff after increments.[40] This process omits a few recipients. However, more importantly, it leaves some households in our analysis who are non-recipients due to having too much income but who still have incomes similar to our recipients and who therefore are potentially a good comparison group for those households who do receive EIPs. We refer to the three resulting samples — one for each

[39] Information on income is collected as part of the CE Survey, but these questions ask about income earned in the past twelve months, which may not correspond to a calendar year. Additionally, tax filing status is not asked about in the survey, but imputed values are provided in the data. Imputations of filing status and tax liabilities are done using the National Bureau of Economic Research's TAXSIM program.

[40] Appendix Tables C.5, C.6, and C.7 show the the selection of resulting cutoffs and the number of recipients in the \$25,000 income ranges above and below each cutoff.

EIP round — as our *final samples* and it is these samples that are tabulated in Section II.

Another differences between previous tax rebates and these EIPs is that there are three waves of EIPs in reasonably rapid succession, and in equation (1), the estimated spending responses to one EIPs may be biased by responses to other EIPs. In response, in our main analysis of the spending responses to EIP2 and EIP3, we include in X as control variables the same distributed lags of the two other EIPs when observed as we do for the main EIP of interest. This control is imperfect since we do not observe all earlier EIPs received and since there is cross-household correlation between recipients status and potentially even amount for EIP2 and EIP3. Thus we also check (and find similar results) when we estimate our responses without these controls.

Our second main concern is related to the fact the pandemic was a time of unprecedented consumption volatility during which people with different levels of consumer expenditures had vastly different dollar changes over time. During the early stages of pandemic in particular, households with higher incomes have much larger changes in dollar spending on average.[41] These differences across households suggest that the time dummies in equation (1) do a poor job of capturing the average dollar change in spending for households with different incomes. Since income and average expenditure are also related to recipient status and EIP amount, these differences may well create bias in estimates of MPCs. For example, if there are large changes in dollar spending in April 2020, when most EIP1s were disbursed, that are not caused by EIP receipt or amount conditional on receipt and yet correlated with receipt or amount, then estimates from equation (1) would be inconsistent.[42]

However, groups of people with different incomes — and so with different average levels of consumption spending — experienced roughly similar percent changes in consumer spending over time (e.g., see Cox et al., 2020). We find for example that, for a given time period t, differences in $\Delta \ln C$ across terciles of the income distribution are lower than differences in ΔC (see Appendix Figures C.1.b and C.1.c).

Our solution therefore is to scale all the variables in our regression by $\bar{C}_i$, the average consumer expenditure (of each type) for family i and also to allow a different regression intercept for households that never receive a given EIP. Letting $\tilde{X}_{i,t} = X_{i,t}/\bar{C}_i$ for any variable X and $R(i)$ be an indicator variable that equals one for households that receive at

[41] Appendix Figures C.1.a and C.1.c show this across terciles of the income distribution.

[42] Previous recessions analyzed in earlier work had less variation in average change in dollar spending by income. And previous analyses found similar MPCs across different specifications, most importantly between results using log change in consumer spending and those using dollar change.

least one EIPn, we infer MPCs from the equation:

$$\Delta\widetilde{C}_{i,t} = \sum_{s=0}^{S} \beta_s \widetilde{EIPn}_{i,t-s} + \widetilde{\boldsymbol{X}}_{i,t}\gamma + \tau_t + \alpha_{R(i)} + \epsilon_{i,t} \tag{2}$$

where X contains (scaled) age, change in family size, and possible previous EIPs. The main coefficient of interest, β_s, still measures the propensity to spend out of an EIP, but by scaling all variables we have transformed the τ from absorbing the average change in dollar spending across households in that period to absorbing the average percent change in consumer expenditures across households in that period. Similarly, $\alpha_{R(i)}$ allows a different average growth rate of expenditure between recipients and non-recipients, and the residual is in terms of a percent deviation of consumer expenditure rather than dollar deviation. In the CE survey, the average percent change in spending measured in this way is significantly more similar for households across terciles of standards of living as measured by their average level of income (compare Appendix Figures C.2.a to C.2.b and C.2.c to C.2.d).

Our third and final main concern is related to the developing literature addressing potential bias in difference-in-differences type estimation with both different groups treated at different times and heterogeneity in average treatment effect across groups (e.g. Borusyak and Jaravel, 2018; de Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021; Callaway and Sant'Anna, 2021; Wooldridge, 2021; Borusyak et al., 2022). In our context, estimation of equation (1) would be biased if there is variation in average MPC, or β_s, across households receiving the EIP in question in different months. The bias would arise from (implicitly) comparing the expenditure responses of households receiving EIPs at different times to infer the evolution of expenditure after EIP receipt. Equation (1) assumes that each household's expenditure response is given by β_s instead of $\beta_{s,t}$.[43] To be clear, any variation in the tendency to spend out of EIPs in different waves (1, 2, and 3) would not create any bias.

On the one hand, variation in β_s across households receiving the EIP at different times could be significant because when each household received its EIP is non-random (unlike in previous payment programs). Later recipients tended to be households for which the IRS did not have their bank information or physical address, and so have slightly lower incomes and expenditures on average. In addition, the pandemic period was a period of unprecedented economic volatility, and variation in β_s over time could arise from variation

[43] In a dynamic specification where leads and lags are added, there is also the additional problem of "contamination," see Sun and Abraham (2021) for details.

in the economy or the pandemic situation.[44] On the other hand, most of our variation comes from comparing recipients to non-recipients (always a valid comparison) and comparing people receiving different amounts of EIPs. Further, Parker et al. (2021) shows through simulation that there is minimal bias for quite substantial variation in average treatment effect over time for the first round of EIPs, where the variation in timing of receipt is the greatest of the three.

Our solution is to follow Borusyak et al. (2022), which allows differences in MPC or β_s over time and is unbiased under generalized parallel trends (and no treatment anticipation) assumptions.[45] The estimation method can be clearly described as a three-step procedure. Denoting the set of never-treated and not-yet-treated observations as Ω_0, in the first step we estimate the time dummies and coefficients on controls using only Ω_0:[46]

$$\Delta\widetilde{C}_{i,t} = \widetilde{\boldsymbol{X}}_{i,t}\gamma + \tau_t + \alpha_{R(i)} + \eta_{i,t} \qquad \forall\{i,t\} \in \Omega_0 \tag{3}$$

In the second step, for treated observations only, we compute the difference between observed scaled change in expenditure and the scaled change in expenditure predicted by controls and time, denoted by $\Delta\check{C}_{i,t}$.

$$\Delta\check{C}_{i,t} = \Delta\widetilde{C}_{i,t} - \widetilde{\boldsymbol{X}}_{i,t}\hat{\gamma} - \hat{\tau}_t - \hat{\alpha}_{R(i)} \qquad \forall\{i,t\} \notin \Omega_0 \tag{4}$$

Thus, $\Delta\check{C}_{i,t}$ is an estimate of the household-level spending response to the EIPs. In the third step, we run WLS regression of the new dependent variable on the EIP variable(s) of interest:

$$\Delta\check{C}_{i,t} = \sum_{s=0}^{S} \beta_s \widetilde{EIP1}_{i,t-s} + \check{\epsilon}_{i,t} \tag{5}$$

Our method solved the issue created by "forbidden comparison," but note that the third step deviates from Borusyak et al. (2022) – we rely on regressions to compute average MPC instead of aggregating individual effects using proposed weights. This change allows us to exploit the differences in treatment intensity and to compare different specifications. To the

[44] Also, the CE interview structure could lead to heterogeneity. Even for households that received the payment on the same day and had the same spending response in reality, if they were interviewed in different months and hence had different reference periods, the measured spending response would differ.

[45] The estimator is also efficient under homoskedasticity and is "asymptotically conservative" when standard errors are clustered.

[46] As noted, for EIP2 analysis, $\widetilde{EIP1}_{i,t-s}$ and $\widetilde{EIP3}_{i,t-s}$ are added as controls. Similarly, for EIP3 analysis, $\widetilde{EIP1}_{i,t-s}$ and $\widetilde{EIP2}_{i,t-s}$ are added as controls.

best of our knowledge, those features cannot yet be achieved for our specific setting by any of the new estimators to date. The disadvantage is that the weights used in the regressions are not as explicit, and could be hard to interpret.[47]

To better approximate the average response, we also use the average CE weight across all interviews for each household. In practice whether one weights or not (or whether one uses replication weights) makes very little difference to the estimates.[48]

IV The average MPC in response to the arrival of each EIP

This section presents the results of our analysis of the spending responses to all three rounds of the EIPs using the same survey data source, the CE Survey, as was used in studying the 2001 and in 2008 tax payments. We show that the estimated, short-term spending responses out of EIPs are small whether we use the new and improved estimation method just described or the exact same method as used in the studies of the 2001 and 2008 payments. The estimated spending responses are small both relative to the responses estimated for the past tax payments and relative to other estimates of spending responses to these EIPs that are based on other populations and datasets.

Table C.8 displays the main spending responses to all three rounds of EIPs, both the average fraction of the EIP that is spent shortly after arrival (first four columns) and the average dollar amount that is spent (last four columns). These results come from our main estimation method of equation 2 (the three-step, unbiased procedure) with $S = 1$.

The first four columns of the first row of Panel A show that the first round of EIPs was not spent rapidly after receipt and so on average was not providing urgently-needed pandemic insurance. During the three-month reference period in which a payment was received, a household on average increased its spending on non-durable goods and services by 10.2% of EIP1, and on all CE-measured goods and services by 23.4% of EIP1. Taking the perspective of classical statistics, the 95% confidence intervals of cumulative spending rule out spending in excess of 16% of the EIP on non-durable goods and services and 35% on

[47] However, some early evidence shows that after addressing "forbidden comparison", the weighting issue is unlikely to lead to significant bias since the estimate will be a convex weighted average, see Baker et al. (2022) and Roth et al. (2022) for the stacked regression method, for example.

[48] We make three other choices that differ slightly from previous analyses. As in previous papers, we drop the bottom 1% of the distribution in broad non-durable consumer expenditures after adjusting for family size, but instead of estimating the bottom one percent using a quantile regression on a linear trend, we drop the bottom 1% in each interview to account for the volatility across time during our sample due to the pandemic. Second, we do not drop households older than 85. Finally, we choose to follow Panel A of Table 3 in Parker et al. (2013) rather than Table 2, which means allowing a different average growth rate of expenditure between recipients and non-recipients. Our estimates are largely insensitive to these three choices.

Table 3: The contemporaneous response of consumer expenditures to EIP receipt

	Food and alcohol	Strictly Nondurables	Nondurable goods and services	All CE goods and services	Food and alcohol	Strictly Nondurables	Nondurable goods and services	All CE goods and services
	MPC				*Dollars spent*			
				Panel A. EIP1				
$\widetilde{EIP}1$	0.011	0.075	0.102	0.234				
	(0.016)	(0.020)	(0.028)	(0.059)				
$\mathbb{1}[\widetilde{EIP1} > 0]$					6.5	96.4	80.8	336.5
					(25.3)	(36.6)	(46.4)	(96.6)
				Panel B. EIP2				
$\widetilde{EIP}2$	0.034	0.103	0.083	0.247				
	(0.021)	(0.031)	(0.039)	(0.090)				
$\mathbb{1}[\widetilde{EIP2} > 0]$					18.8	80.8	65.6	156.7
					(23.6)	(44.0)	(52.2)	(114.4)
				Panel C. EIP3				
$\widetilde{EIP}3$	0.036	0.030	0.009	0.015				
	(0.017)	(0.016)	(0.018)	(0.043)				
$\mathbb{1}[\widetilde{EIP3} > 0]$					99.5	86.8	55.1	-36.0
					(33.8)	(40.8)	(42.2)	(102.4)
				Average quarterly household spending across three waves				
	$2,292	$4,516	$5,996	$14,401	$2,292	$4,516	$5,996	$14,401

Notes: Table reports estimation of equations 3 to 5 with $S = 1$, with scaled dollar change in consumption as the dependent variable and using weighted least squares using average weights. Each pair of rows uses the final sample for that EIP round. Standard errors included in parentheses are adjusted for arbitrary within-household correlations and heteroskedasticity. Besides separate intercepts, regressions also include interview month dummies, scaled age and change in the size of the CU, and controls for the other EIPs for EIP2 and EIP3. For EIP1, the four columns have 3,541, 3,543, 3,543, and 3,544 treated observations, and 2,264 never-treated or not-yet-treated observations except for the first column that has 2,261. For EIP2, the columns have 3,171, 3,171, 3,175, and 3,175 treated observations, and 5,002, 5,004, 5,004, and 5,005 never-treated or not-yet-treated observations. For EIP3, the columns have 3,566, 3,566, 3,568, and 3,567 treated observations, and 3,465, 3,474, 3,477, and 3,474 never-treated or not-yet-treated observations.

all CE goods and service.

First four rows of Panels B show similar low spending responses for the second round of EIPs. The third and fourth columns show that 8% and 25% of the EIP2s were used for expenditures on non-durable goods and services and total CE-measured expenditures respectively within the three-month period of receipt. These first two panels are consistent with the hypothesis that, because households tilted spending towards durable goods during the pandemic, the spending response to the EIPs was similarly tilted towards durable. Compared to past stimulus programs, the share of spending going to durable goods does appear higher than in 2001, but it is not higher than in 2008 and the statistical strength of both comparisons is weak.

Finally, the first four rows of Panel C show even lower spending responses for the third round of EIPs than for the first and second round EIPs. Spending in response to EIP3 receipt was economically (and statistically) close to zero. As noted, because it is possible that some households that received EIP2 or EIP3 payments failed to report them, one should be maintain some skepticism that the actual spending response were quite this low, particularly for the third round of the EIPs. However, the lower spending response are consistent both with the rise in liquid balances throughout the pandemic (see Grieg et al., 2022) and with the large dollar size of the third round of the EIPs.

How might our estimated spending response to EIP2 and EIP3 be lowered by under-reporting of EIP receipt in the CE? Under-reporting implies that some households in our control group were actually treated, and so reduces the difference we measure between groups. To shed light on this possibility, we calculate EIP receipt and amount from the rules of each round of EIP and the TAXSIM imputations contained in the CE as described in Appendix A.6. We create alternative measures of our EIP variable for each round of EIP by assuming that any EIP arrived in the first two months of that round. We then conduct our main analysis using these imputed EIPs and dropping any CE household with a recall period that does not contain both of the critical two months. Appendix Tables **??** to C.10 show the results of our analysis. Using imputed EIPs, the estimated MPCs for EIP2 are smaller than those in our main analysis suggesting we are not overestimating the spending response in the second round. For the third round however, analysis of these alternative measures suggest that our estimated MPCs out of EIP3 are indeed underestimated but this alternative analysis still finds them to be relatively small.

The last four columns of Table C.8 show the dollar spending response to receipt of an EIP (rather than the MPC) and imply smaller spending responses. These columns are based on our main estimation but replacing our measure of EIP amount with an indicator variable of EIP1 receipt, $\mathbb{1}[EIPn_{i,t-s} > 0]$, so that these estimates do not identify the spending effect using any information about EIP amounts across recipients. The estimated dollar spending responses to the arrival of EIP1 are \$81 or 3% of the average EIP1 on non-durable goods and services (statistically insignificant, column 7) and \$337 or 16% of the average EIP1 on all measure CE expenditures (statistically significant, column 8). For EIP2 the spending responses of \$66 and \$157 respectively (statistically insignificant), are 5% and 12% of the average EIP2 and so imply even less spending than the specification in the first four columns. Finally, the last four columns also continue to show very small spending

Table 4: The longer-term response of consumer expenditures to EIP receipt

	Dependent variable: scaled dollar change in spending on								
	Panel A: EIP1			*Panel B: EIP2*			*Panel C: EIP3*		
	Strictly non-durables	Nondurables	All CE goods and services	Strictly non-durables	Nondurables	All CE goods and services	Strictly non-durables	Nondurables	All CE goods and services
$\widetilde{EIPn}_t$	0.075	0.102	0.234	0.103	0.083	0.247	0.030	0.009	0.015
	(0.020)	(0.028)	(0.059)	(0.031)	(0.039)	(0.090)	(0.016)	(0.018)	(0.043)
$\widetilde{EIPn}_{t-1}$	-0.011	-0.080	-0.017	0.030	-0.013	0.107	0.000	-0.049	-0.150
	(0.020)	(0.028)	(0.070)	(0.038)	(0.045)	(0.124)	(0.010)	(0.019)	(0.049)
	Implied cumulative fraction of EIP spent over two three-month periods								
	0.139	0.124	0.452	0.235	0.153	0.601	0.059	-0.030	-0.119
	(0.051)	(0.068)	(0.158)	(0.086)	(0.104)	(0.257)	(0.036)	(0.047)	(0.112)

Notes: Table reports β_0 and β_1 from estimation of equations 3 to 5 with $S = 1$. Regressions also include interview month dummies, a separate intercept for non-recipients, scaled age, and change in the size of the CU. Panels B and C additionally control for the other EIP waves. The sample is the final sample which includes only CE households with income that does not exceed a certain threshold determined by marital status and family structure. Regressions are conducted using weighted least squares, where the weights applied are average weights. Standard errors included in parentheses are adjusted for arbitrary within-household correlations and heteroskedasticity. For Panel A, observations are those with an interview in June or July 2020; the columns have 2,264 never-treated or not-yet-treated observations and 3,543 treated observations. For Panel B, observations are those with an interview in February, March or April 2021; the columns have 4,815, 4,817, 4,818 never-treated or not-yet-treated observations and 3,171, 3,175, and 3,175 treated observations, respectively. For Panel C, observations are those with an interview in April, May or June 2021; the columns have 3,474, 3,477, 3,474 never-treated or not-yet-treated observations, and 3,566, 3,568, and 3,568 treated observations, respectively.

responses to the third round of the EIPs, particularly because the average EIP3 is 1/3 bigger than the average EIP1.

We have measured EIP-driven spending in the short term to evaluate whether the EIPs provided urgently-needed pandemic insurance, and we now turn to evaluating subsequent spending, which is informative both about pandemic insurance but over a three-month longer period and, for longer horizons, about the contribution of EIPs to the rapid pandemic recovery and potentially inflation. In terms of pandemic insurance, we find some evidence of continued higher spending for EIP1 and EIP2, but no evidence of any continued spending for EIP3. In terms of increases in demand over any longer periods, we lack the statistical power to add any evidence on the potential contribution of EIPs to strong demand or inflation during the second half of 2021 or beyond.

Table C.11 shows the longer-run response of spending to the receipt of an EIP. The coefficient β_1 on $\widetilde{EIP}_{i,t-1}$ measures the decline in spending during the three-months following receipt, so that $\beta_0 + \beta_1$ measures the increase in spending in the second three months

Table 5: Estimated MPCs on CE-measured non-durable goods and some services

	Full Sample, Three-months of receipt	Recipients Only, Three-months of receipt	Full Sample Three months of receipt and subsequent three months
2001 Economic Rebates	0.386	0.247	0.691*
	(0.135)	(0.213)	(0.260)
2008 Stimulus Payments	0.121	0.308	0.347
	(0.055)	(0.112)	(0.155)
2020 EIP 1	0.102	-0.062	0.124
	(0.028)	(0.072)	(0.068)
2020 EIP 2	0.083		0.153
	(0.039)		(0.104)
2021 EIP 3	0.009		-0.030
	(0.018)		(0.047)

Source: Johnson et al. (2006)), Parker et al. (2013), and Parker et al. (2021) and current paper. The * denotes a large MPC driven in part by one outlier in spending on food.

relative to the previous three months. The bottom row of the table reports $\beta_0 + (\beta_0 + \beta_1)$, the sum of the contemporaneous spending and this additional spending, which is then the total spending during both the three-month period of receipt and the subsequent three-month period (as a percent of the EIP).

For EIP1, the cumulative MPC on strictly nondurable and broad non-durable goods and services are both roughly 13% and on all CE goods and services is 45% (with a standard error of 15.8%). For EIP2, the MPCs are slightly higher, consistent with the more open economy and the smaller size of the payments. Finally, for EIP3, we find no evidence that EIP3s were spent during the three months of receipt or during the subsequent three month period. Appendix Tables **??** to C.13 shows that using our imputed EIP measures described above does not change the conclusion of small spending effects.

Table 5 summarizes our finding of low spending response to these EIPs, and compares the spending responses to those of earlier stimulus payment programs. The MPCs out of the EIPs are substantively lower than MPCs out of tax payments disbursed in 2001 and in 2008 according to studies using the same survey data.

Are these relatively low spending response due to our differences (improvements) in methodology? No. To show this, we apply the methodology of Johnson et al. (2006) and Parker et al. (2013) exactly and estimate spending responses to each round of EIPs on the sample of all CE households. The estimated spending responses are unstable across

Table 6: The response of consumer expenditure to EIP arrival estimated on recipients and non-recipients using the methodology previously applied to tax rebates

	Food and alcohol	Strictly Nondurables	Nondurable goods and services	All CE goods and services	Food and alcohol	Strictly Nondurables	Nondurable goods and services	All CE goods and services
	MPC				*Dollars spent*			
				Panel A. EIP1				
EIP1	0.043	0.071	0.077	0.280				
	(0.032)	(0.044)	(0.059)	(0.217)				
$\mathbb{1}[EIP1 > 0]$					157.3	296.4	375.0	1278.8
					(89.9)	(130.2)	(167.8)	(647.5)
				Panel B. EIP2				
EIP2	0.011	0.037	0.030	0.008				
	(0.029)	(0.044)	(0.055)	(0.325)				
$\mathbb{1}[EIP2 > 0]$					-57.1	-11.1	-10.1	-498.7
					(51.7)	(79.3)	(99.5)	(749.8)
				Panel C. EIP3				
EIP3	0.001	0.001	0.005	0.222				
	(0.013)	(0.017)	(0.023)	(0.149)				
$\mathbb{1}[EIP3 > 0]$					14.2	-6.3	22.7	702.1
					(45.1)	(70.3)	(91.4)	(648.7)

Notes: Table reports β_0 from estimation of equation 1 with $S = 0$ with dollar change in consumption as the dependent variable and using weighted least squares using average weights. Standard errors included in parentheses are adjusted for arbitrary within-household correlations and heteroskedasticity. Regressions also include interview month dummies, age, and change in the size of the CU. The samples are constructed as in previous research papers (see Appendix). Panel A has 5,634 observations and includes the sample of all CE households with an interview in June or July 2020. Panel B has 8,302 observations, includes the sample of all CE households with an interview in February, March, or April 2021, and additionally includes controls for EIP1 and EIP3. Panel C has 7,335 observations, includes the sample of all CE households with an interview in April, May or June 2021, and additionally includes controls for EIP1 and EIP2.

specifications and columns, but on average are not inconsistent with Table C.8 above for EIP1 and EIP3 (results for EIP2 suggest even smaller spending responses).

More precisely, we estimate equation (1) on samples that are constructed exactly as in these earlier papers, and replicate Table 2 in both of these papers, for all three rounds of EIPs. As shown in the first four columns of Table 6, for EIP1, point estimates suggest MPCs of 4.3% on food, 7.1% on strictly nondurables, 7.7% on the broad measure of non-durable goods and services, and 28.0% on all goods and services. While all these estimates are statistically insignificant, these point estimates are consistent with those in Table C.8. But this methodology leads to wildly different conclusions for other specifications, unlike found in the analysis of the 2001 and 2008 tax payments, and consistent with the arguments for our preferred specification in Section III. The last four columns of Panel A show estimates using an indicator variable for receipt in place of EIP1 amount and implies that, in the three months in which the EIP1 arrives, spending increases by $157 on food, $296 on strictly nondurables, $375 on non-durables, and $1,279 on all goods and services, with all but the first being statistically significant. For the average EIP1, these estimates would imply MPCs of 7%, 14%, 18%, and 61% respectively, roughly double those from estimating the MPC directly (the average of $EIP_{i,t}$ conditional on receipt is $2,098). Appendix Table C.4 shows the results of estimation for the two other specifications used in previous research and these estimated spending responses are all statistically insignificant and, again, imply quite different MPCs than Table 6. [49]

V EIPs as Pandemic Insurance

While we find low average spending responses to the EIPs, the EIPs may nonetheless have filled urgent economic needs for some subset of households, presumably those who were the most impacted economically by the pandemic. In this section, we construct observable measures of economic vulnerability to the economic consequences of the pandemic and evaluate whether households that were more exposed spent more of their EIPs to maintain or increase their consumer spending in the short run. We focus both on households with little ex ante liquid wealth and on households with labor income exposed to the pandemic as measured from their ability to work from home. While the average spending response to the EIPs are low, consistent with payments not being required to fill short-term spending needs for most households, we find two pieces of evidence that the EIPs did raise spending and so provided potentially important assistance to some households. First, we show households that entered the pandemic period with little ex ante liquid wealth spent a larger share of their EIP1s. For EIP2 and EIP3, there is little to no evidence that households with low liquid wealth had higher MPCs. Second, we show that households whose incomes were more exposed to the pandemic — those with lower ability to work from home – spent more out of their first-round EIPs when they arrived. For the second round of EIPs we find no such pattern of MPC related to the ability to work from home.

[49] Johnson et al. (2006) and Parker et al. (2013) both report estimates of MPCs (in Table 3) that rely only on variation in time of receipt by dropping all households that never receive stimulus payments. In these earlier episodes this variation was closer to purely random. Given the lack of variation in timing in the EIP programs, estimates of the MPC in analogous samples that drop households that never receive EIPs have very large standard errors. For EIP1, the program with the largest variation in timing of disbursement, Appendix Table C.1 in Parker et al. (2021) shows that the standard errors are typically 50% to 100% larger than in Table 6 and C.4, as expected given the lack of variation. Additionally, the estimates are more variable and many are negative; so, we learn little from this exercise.

For the third round, there is some evidence of a small effect.

We estimate different MPCs for different groups of recipients by interacting the EIP variables in equation 2 with a group-membership indicator variable, denoted $g(i)$ so that the spending response of interest varies by group as well as horizon. We use the equation:

$$\Delta\widetilde{C}_{i,t} = \sum_{s=0}^{S} \beta_{g(i),s}\widetilde{EIPn}_{i,t-s} + \widetilde{\boldsymbol{X}}_{i,t}\gamma + \alpha_{g(i)} + \tau_t + \epsilon_{i,t} \tag{6}$$

which also allows the intercept or average growth rate of spending to differ by group ($\alpha_{g(i)}$). For studying the MPC of EIP2 and EIP3, we also interact the controls for other EIPs (in $\widetilde{X}$) with the indicator for group membership. To be clear, consider the MPC for EIP2. We estimate equation 6 using our imputation estimator and the procedure described in equations equations 3-5.

First, we split the sample of households by their ex ante liquid wealth and find that households that entered the pandemic with low liquidity had strong spending responses to the first round of EIPs in the CARES Act. We measure liquid wealth as the sum of balances in checking accounts, saving accounts, money market account, and certificates of deposits at the start of the households first interview (reported in the last interview).[50] Panel A of Table 7 shows that households in the bottom third of the distribution of liquidity – those with less than \$2,000 available, which is still a substantial amount – have statistically significant MPCs of 6%, 22%, and 48% on food, non-durable goods and services, and all CE goods and services respectively. While the difference between each of these MPCs and the corresponding MPC of either of the other third of the distribution is not statistically significant, they are economically large, and we can reject the equality of MPCs across these three groups for spending on both non-durable goods and services and all CE goods and services.

Previous research on tax rebates that uses the CE Survey has not consistently found a statistically-significant decreasing relationship between spending responses and liquidity. However, analyses with better measures of liquidity have generally found a larger MPC

[50]Even the low liquidity group has substantial reported wealth, and in particular the distribution of reported liquid wealth is much higher in this 2020 data than it was in 2008. In Parker et al. (2013) the 33rd percentile in the distribution of liquid wealth was only \$500. One possibility is changes in the distribution of respondents, although this appears unlikely as we discuss in Appendix A.4. More likely, this difference reflects changes in the CE Survey and the financial accounts that it covers. While in 2008 the CE asked about balances in checking and saving accounts separately, in 2013 the CE survey switched to asking a single question about total liquidity across a larger set of types of accounts, and starting in 2017 the survey introduced an initial question asking whether there was a zero balance in these accounts. The latter change was associated with a reduction in the number of households reporting zero balances.

Table 7: The contemporaneous response of consumer expenditures to EIP by liquidity

	Dependent variable: scaled dollar change in spending on								
	Panel A: EIP1			*Panel B: EIP2*			*Panel C: EIP3*		
	Food and alcohol	Nondurables	All CE goods and services	Food and alcohol	Nondurables	All CE goods and services	Food and alcohol	Nondurables	All CE goods and services
	Bottom third: $\leq 2,000$ Top third: $\geq 12,667$			Bottom third: $\leq 2,000$ Top third: $\geq 12,000$			Bottom third: $\leq 2,000$ Top third: $\geq 10,000$		
$\widetilde{EIPn}_t$	0.039 (0.033)	0.087 (0.064)	0.178 (0.155)	-0.032 (0.071)	0.112 (0.111)	0.220 (0.326)	0.078 (0.035)	0.132 (0.062)	0.081 (0.197)
$\widetilde{EIPn}_t \times$ *Bottom third*	0.016 (0.051)	0.130 (0.095)	0.301 (0.210)	0.050 (0.084)	0.009 (0.157)	0.191 (0.403)	-0.018 (0.048)	-0.099 (0.087)	-0.065 (0.219)
$\widetilde{EIPn}_t \times$ *Top third*	0.013 (0.046)	-0.188 (0.102)	-0.275 (0.243)	-0.090 (0.085)	-0.255 (0.139)	-0.272 (0.449)	0.048 (0.101)	-0.129 (0.099)	-0.057 (0.267)
p-value for test of equality of responses	0.942	0.011	0.044	0.107	0.077	0.492	0.784	0.355	0.957
	Implied propensity to spend by group								
Least liquid third	0.055 (0.039)	0.217 (0.070)	0.479 (0.142)	0.018 (0.046)	0.121 (0.112)	0.411 (0.237)	0.060 (0.034)	0.033 (0.062)	0.016 (0.095)
Most liquid third	0.052 (0.032)	-0.101 (0.079)	-0.097 (0.188)	-0.122 (0.048)	-0.143 (0.083)	-0.052 (0.309)	0.126 (0.094)	0.003 (0.077)	0.024 (0.180)

Notes: All regressions use our imputation estimator to estimate equation (6). Also included are interview month dummies, scaled age and change in the size of the CU, separate intercepts by thirds of the liquidity distribution interacted with other EIPs. The sample is the final sample and all results are from WLS regressions using average weights. Standard errors included in parentheses are adjusted for arbitrary within-household correlations and heteroskedasticity. The tests of equal responses are joint test for H_0: $\beta_{0,Bottom\ third} = 0$ and $\beta_{0,Top\ third} = 0$. For Panel A, observations are those with an interview in June or July 2020; the columns have 1,065, 1,066, and 1,066 never-treated or not-yet-treated observations, and 1,608, 1,609, 1,609 treated observations, respectively. For Panel B, observations are those with an interview in February, March or April 2021; the columns have 1,795 never-treated or not-yet-treated observations and 1,211 treated observations. For Panel C, observations are those with an interview in April, May or June 2021; the columns have 1,387, 1,390, and 1,387 never-treated or not-yet-treated observations, and 892 treated observations.

for households with lower liquidity (e.g. Parker, 2017; Olafsson and Pagel, 2018; Ganong et al., 2020; Baugh et al., 2020; Fagereng et al., 2021).

For the second round of EIPs, the spending responses are higher for households in the bottom two thirds of the liquidity distribution and we can no longer reject equality of the MPCs across the thirds of the distribution of liquid wealth. No spending responses are statistically significant, but point estimates suggest the least liquid households spent 12% of their EIPs on non-durable goods and services, the middle third in terms of liquidity spent 11%, while the most liquid households are estimated to spend a negative amount. The MPCs on total expenditures are more related to liquidity: 41%, 22% and -5% as we move from the lowest to highest third of the distribution of liquid wealth but again with no estimate being statistically significant. These findings are not inconsistent with Schild and Garner (2020) which shows that in the Household Pulse data, households reporting higher levels of financial difficulty are more likely to use their EIP2s mostly for spending.

Finally, for the third round of EIP — the largest in dollar terms and the latest in the pandemic and the most likely understated due to data issues — the middle of the liquidity distribution is the only group estimated to have a statistically significant spending response to the arrival of their EIPs: 13% (6%) on non-durable goods and services, compared to 3% (6%) and 0.3% (8%) for the bottom and top thirds of the distribution of liquid wealth respectively. Again, we cannot reject the null hypothesis of no differential response.

These patterns suggest that the first round of EIPs did meet important liquidity needs for households with little liquid wealth in the early stages of the pandemic, when the economy was most shut down. But later EIP rounds appear less beneficial on this front (or their benefits were less related to liquid wealth). The second round payments were broadly spent at the same average rate as EIP1, consistent with the tendency for households to spend out of small, transitory increases in liquidity, and also with similar constraints on consumer spending from the pandemic as EIP1. And the low spending of the final round of payments, particularly among households with little liquidity is consistent with the large size of the payment, although again our caveat about the low rate of EIP receipt reported in the CE survey applies.

Analysis of our second measure of whether the EIPs provided effective pandemic insurance — based on households ability to work from home – paints a similar picture: the first round of EIPs appear to fill a pandemic insurance need for households but later rounds do not.

We measure the exposure of income to the inability to work from home for EIP1 by the share of pre-pandemic household income that cannot be earned from home. Specifically, for the reference person and any secondary earner, we calculate the share of tasks associated with their job based on their industry and education level following a mapping into the Mongey et al. (2021) and Dingel and Neiman (2020) classifications by occupation and education. For individual's with no earned income (valid missing earnings), like retirees or people not in the labor force, the measure is zero. We then multiply this share by each person's wage and salary income, sum to the household level, and divide by family income. Because we require pre-pandemic income, we only use this measure to analyze EIP1. Appendix B.3 contains complete details.

Table 8 shows that households most reliant on labor income from jobs that cannot be done at home account for most of the spending response to the first round of EIPs. The

Table 8: The response of consumer expenditures to EIP1 receipt by the exposure of income to inability to work from home in 2020

	Dependent variable: scaled dollar change in spending on		
	Food and alcohol	Nondurables	All CE goods and services
	Fraction of EIP1 spent over contemporaneous three-month period		
$\widetilde{EIP1}_t$	0.021	0.052	-0.049
	(0.022)	(0.055)	(0.119)
$\widetilde{EIP1}_t \times$ *Middle third*	0.030	0.176	0.258
	(0.043)	(0.089)	(0.232)
$\widetilde{EIP1}_t \times$ *Least able third*	0.036	0.064	0.367
	(0.038)	(0.083)	(0.188)
p-value for test of equality of responses	0.731	0.225	0.210
	Cumulative fraction of EIP1 spent over contemporaneous and next three-month period		
Most able third	-0.007	-0.135	-0.435
	(0.057)	(0.159)	(0.349)
Middle third	0.126	0.365	0.181
	(0.100)	(0.190)	(0.622)
Least able third	0.117	0.285	0.842
	(0.080)	(0.156)	(0.448)

Notes: All regressions use equation 6. Also included are interview month dummies, scaled age and change in the size of the CU, and separate intercepts by thirds of the distribution. The sample is the final sample which includes only CE households with an interview in June or July 2020, with income that does not exceed a certain threshold determined by marital status and family structure. The work-from-home measure used is the income-based measure. All results are from WLS regressions. Weights applied are average weights. Standard errors included in parentheses are adjusted for arbitrary within-household correlations and heteroskedasticity. The tests of equal responses are joint test for H_0: $\beta_{0,Least\ able\ third} = 0$ and $\beta_{0,Middle\ third} = 0$.

third of households with little to no income exposure have point estimates that imply EIP1 lowered their spending. The third of households with income that was moderately exposed, had an average MPC of 36% (19%) on non-durable goods and services while the most exposed third had a similar average MPC of 29% (16%) (and an MPC on total expenditures of 84% (45%)) during the three-month period of receipt and the subsequent period.

For later EIPs, given the rotating panel structure of the CE, we cannot measure pre-pandemic incomes, and earnings after the onset of the pandemic may already reflect losses

Table 9: The contemporaneous response of consumer expenditures to EIP receipt by the ability to work from home

	Dependent variable: scaled dollar change in spending on								
	Food and alcohol	Nondurables	All CE goods and services	Food and alcohol	Nondurables	All CE goods and services	Food and alcohol	Nondurables	All CE goods and services
	Panel A: EIP1 Bottom third: $\leq$ 89.4% Top third: $\geq$ 99.1%			*Panel B: EIP2* Bottom third: $\leq$ 87.0% Top third: $\geq$ 98.8%			*Panel C: EIP3* Bottom third: $\leq$ 84.1% Top third: $\geq$ 97.7%		
$\widetilde{EIP}_t$	-0.014	0.048	0.189	0.061	0.131	0.265	0.026	-0.021	0.015
	(0.027)	(0.041)	(0.086)	(0.035)	(0.071)	(0.144)	(0.036)	(0.038)	(0.080)
$\widetilde{EIP}_t \times$ *Middle third*	0.031	0.090	0.158	-0.042	-0.131	-0.211	0.024	0.033	0.219
	(0.038)	(0.062)	(0.149)	(0.049)	(0.095)	(0.230)	(0.039)	(0.050)	(0.122)
$\widetilde{EIP}_t \times$ *Least able third*	0.090	0.109	0.239	-0.004	-0.024	-0.072	0.016	0.066	0.143
	(0.037)	(0.069)	(0.144)	(0.053)	(0.100)	(0.216)	(0.039)	(0.048)	(0.106)
p-value for test of equality of responses	0.046	0.188	0.222	0.648	0.326	0.656	0.817	0.376	0.174
	Implied spending by group								
Middle third	0.016	0.138	0.346	0.019	0.000	0.054	0.049	0.012	0.233
	(0.026)	(0.047)	(0.121)	(0.034)	(0.063)	(0.179)	(0.015)	(0.033)	(0.093)
Least able third	0.076	0.157	0.428	0.057	0.110	0.193	0.042	0.045	0.158
	(0.026)	(0.055)	(0.116)	(0.040)	(0.070)	(0.160)	(0.016)	(0.029)	(0.070)

Notes: All regressions use equation 6. Also included are interview month dummies, scaled age and change in the size of the CU, and separate intercepts by thirds of the distribution. The sample is the final sample which includes only CE households with an interview in June or July 2020, with income that does not exceed a certain threshold determined by marital status and family structure. The work-from-home measure used is the non-income measure. All results are from WLS regressions, and the weights applied are average weights. Standard errors included in parentheses are adjusted for arbitrary within-household correlations and heteroskedasticity. The tests of equal responses are joint test for H_0: $\beta_{0,Bottom\ third} = 0$ and $\beta_{0,Middle\ third} = 0$. For Panel A, all regressions have 3,470 observations For Panel B, all regressions have 3,099 observations. For Panel C, all regressions have 3,463 observations.

incurred by an inability to work from home. Therefore, in order to investigate differences in consumption responses across ability to work from home for EIP2 and EIP3, we construct a work-from-home measure that does not rely on observing pre-pandemic wage and salary earnings. We construct a second measure based of the share of wage and salary (potential) earnings that cannot be done from home and the assumption that earners within a family have equal earnings. This measure requires only information on the industry and education of (potential) earners, whether currently working or not (see Appendix B.3 for details).

Using this second measure, Panel A of Table 9 shows findings for EIP1 that align well with our first measure of the ability to work from home based on pre-pandemic income. That is, we find all spending is done by the two thirds of households with the highest level of income exposure during the pandemic, as we did in Table 8. Panels B and C show no significant differences in spending propensities related to the ability to work from home for either of the second two rounds of EIPs, consistent with the waning of the economic impact of the pandemic. If anything, EIP2 spending responses are concentrated among households with no income exposure to the pandemic. For EIP3, only those with incomes that are the most exposed to the pandemic have statistically significant spending response on non-durable goods and services.

In sum, while on average the EIPs appear to have gone to many households with incomes that were unharmed by the pandemic (e.g. retirees, those employed and able to work from home, etc.), some of the EIPs, mainly in the first round, did support short-term spending for some households, those with low ex ante liquid wealth and those reliant on income that could not be earned by working from home.

VI Concluding remarks

The pandemic limited the types of goods and services that people could purchase and many households reduced spending. There were also policy responses besides the EIPs, including extended and expanded unemployment insurance, and the Paycheck Protection Program that transferred money to small and medium sized businesses with some incentives to maintain payroll, both of which were intended to help offset any lost income. Finally, the depth and duration of the pandemic were uncertain, particularly when the first round of EIPs were being disbursed. These factors appear to have led to less spending on non-durable goods and services (CE-measured) in response to the arrival of the first round or EIPs than out of the tax rebates in 2001 and 2008, and to have tilted what spending response there was towards durable goods. We observe similar spending responses to the second-round of EIPs, but very little short-run spending in response to the third, consistent with high pre-existing high levels of financial resources; although, the response is not as cleanly measured as the previous EIPs.

Were the EIPs effective? The goal of previous tax rebates programs was to increase demand and so their efficacy is largely related to the speed and size of the spending responses. In contrast, the policy goal of the EIPs was insurance, that is, to provide money to those who lost or would lose employment and who would not be covered by government aid programs. For these individuals, the EIPs could be initially saved and then used to cover a later loss. We find significant spending responses for households with low levels of ex ante liquidity in response to the first round of EIPs during the national emergency at the onset of the pandemic. The smaller amount of spending following the arrival of the December 2020 payments was due to a spending response by those outside the top third of the liquidity distribution. Finally, we find substantially higher spending responses by those reliant on earnings from jobs with tasks that could not be done from home in response to the first-round EIPs (and little evidence on this issue for later EIP rounds).

The small, short-term spending response and its pattern suggest that the EIPs went to many people who did not need the additional funds as urgent pandemic insurance (e.g. Sahm, 2021, debates these issues). However, despite the lack of much immediate spending, the EIPs could have filled the role of pandemic insurance for some households beyond the time horizon accurately measured by this (and other) studies. On the other hand, from a demand management perspective, the unspent EIPs have contributed to strong households balance sheets over the past year, a period of strong demand and rising inflation.

References

Agarwal, Sumit and Wenlan Qian, "Consumption and Debt Response to Unanticipated Income Shocks: Evidence from a Natural Experiment in Singapore," *American Economic Review*, 2014, *104* (12), 4205–4230.

_ , Chunlin Liu, and Nicholas S. Souleles, "The Reaction of Consumer Spending and Debt to Tax Rebates: Evidence from Consumer Credit Data," *Journal of Political Economy*, 2007, *115* (6), 986–1019.

Armantier, Olivier, Leo Goldman, Gizem Koşar, Jessica Lu, Rachel Pomerantz, and Wilbert van der Klaauw, "How Have Households Used Their Stimulus Payments and How Would They Spend the Next?," Technical Report, Liberty Street Economics. October 13 2020.

Baker, Andrew C., David F. Larcker, and Charles C.Y. Wang, "How much should we trust staggered difference-in-differences estimates?," *Journal of Financial Economics*, 2022, *144*, 370–395.

Baker, Scott R., R. A. Farrokhnia, Steffen Meyer, Michaela Pagel, and Constantine Yannelis, "Income, Liquidity, and the Consumption Response to the 2020 Economic Stimulus Payments," *Review of Finance*, forthcoming.

Baugh, Brian, Itzhak Ben-David, Hoonsuk Park, and Jonathan A. Parker, "Asymmetric Consumption Smoothing," *American Economic Review*, 2020, *111*, 192–230.

Bodkin, Ronald, "Windfall Income and Consumption," *American Economic Review*, 1959, *49* (4), 602–614.

Borusyak, Kirill and Xavier Jaravel, "Revisiting Event Study Designs," *SSRN*, 2018.

_ , _ , and Jann Spiess, "Revisiting Event Study Designs: Robust and Efficient Estimation," *SSRN*, 2022.

Boutros, Michael, "Evaluating the Impact of Economic Impact Payments," *Manuscript, Duke University, December*, 2020.

Broda, Christian and Jonathan A. Parker, "The Economic Stimulus Payments of 2008 and the Aggregate Demand for Consumption," *Journal of Monetary Economics*, Fall 2014, *68*, S20–36.

Bureau of the Fiscal Service, "EIP Dashboard," 2022. data acquired from the Bureau of the Fiscal Service, U.S. Department of the Treasury.

Cajner, Tomaz, Leland Crane, Ryan Decker, John Grigsby, Adrian Hamins-Puertolas, Erik Hurst, Christopher Kurz, and Ahu Yildermaz, "The U.S. Labor Market During the Beginning of the Pandemic Recession," *Brooking Papers on Economic Activity*, Summer 2020.

Callaway, Brantly and Pedro H. C. Sant'Anna, "Difference-in-Differences with multiple time periods," *Journal of Econometrics*, December, forthcoming 2021, *225* (2), 200–230.

Chetty, Raj, John Friedman, Nathaniel Hendren, Michael Stepner, and The Opportunity Insights Team, "The Economic Impacts of Covid-19: Evidence from a New Public Database Built Using Private Sector Data," *NBER Working Paper 27431*, June 2021.

Coibion, Olivier, Yuriy Gorodnichenko, and Michael Weber, "How Did U.S. Consumers Use Their Stimulus Payments?," *NBER Working Paper 27693*, 2021.

Cooper, Daniel and Giovanni Olivei, "High-Frequency Spending Responses to Government Transfer Payments," 2021.

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

de Chaisemartin, Clément and Xavier D'Haultfœuille, "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects," *American Economic Review*, September 2020, *110* (9), 2964–2996.

Dingel, Jonathan I. and Brent Neiman, "How many jobs can be done at home?," September 2020, *189*, 104235.

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

Farrell, Diana, Fiona Greig, and Amar Hamoudi, "Tax Time: How Families Manage Tax Refunds and Payments," Technical Report, JPMorgan Chase Institute March 2019.

Feldman, Naomi E. and Ori Heffetz, "A Grant to Every Citizen: Survey Evidence of the Impact of a Direct Government Payment in Israel," *NBER Working paper 28312*, December 2020.

Ganong, Peter, Damon Jones, Pascal J. Noel, Fiona E. Greig, Diana Farrell, and Chris Wheat, "Wealth, Race, and Consumption Smoothing of Typical Income Shocks," *NBER Working Paper 27552*, 2020.

_, Fiona Greig, Pascal Noel, Daniel M. Sullivan, and Joseph Vavra, "Spending and Job-Finding Impacts of Expanded Unemployment Benefits: Evidence from Administrative Micro Data," *Draft Manuscript*, August 2022.

Garner, Thesia I., Adam Safir, and Jake Schild, "Receipt and use of stimulus payments in the time of the Covid-19 pandemic," *Beyond the Numbers: Prices and Spending*, August 2020, *9* (10).

Gelatt, Julia, Randy Capps, and Michael Fi, "Nearly 3 Million U.S. Citizens and Legal Immigrants Initially Excluded under the CARES Act Are Covered under the December 2020 COVID-19 Stimulus," https://www.migrationpolicy.org/news/cares-act-excluded-citizens-immigrants-now-covered, Migration Policy Institute January 15 January 15 2021.

Goodman-Bacon, Andrew, "Difference-in-differences with variation in treatment timing," *Journal of Econometrics*, December, forthcoming 2021, *225* (2), 254–277.

Greenwood, Robin, Toomas Laarits, and Jeffrey Wurgler, "Stock Market Stimulus," NBER Working Paper Series, No. 29827, March March 2022.

Grieg, Fiona, Erica Deadman, and Tanya Sonthalia, "Household Finances Pulse: Cash Balances during COVID-19," JPMorgan Chase Institute, https://www.jpmorganchase.com/institute/research/household-income-spending/household-finances-pulse-cash-balances-during-COVID-19, downloaded August 2022 September 2022.

Guerrieri, Veronica, Guido Lorenzoni, Ludwig Straub, and Ivan Werning, "Macroeconomic Implications of COVID-19: Can Negative Supply Shocks Cause Demand Shortages?," *NBER Working Paper 26918*, 2020.

Internal Revenue Service, "SOI Tax Stats - Coronavirus Aid, Relief, and Economic Security Act (CARES Act) Statistics," https://www.irs.gov/statistics/soi-tax-stats-coronavirus-aid-relief-and-economic-security-act-cares-act-statistics June 15 June 15 2022.

—, "Treasury, IRS release latest state-by-state Economic Impact Payment figures for May 22, 2020," https://www.irs.gov/newsroom/treasury-irs-release-latest-state-by-state-economic-impact-payment-figures-for-may-22-2020 May 22 May 22 2020.

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

Karger, Ezra and Aastha Rajan, "Heterogeneity in the Marginal Propensity to Consume: Evidence from Covid-19 Stimulus Payments," *Federal Reserve Bank of Chicago Working Paper, Working Paper, No. 2020-15*, February 2021.

Kim, Seonghoon, Kanghyock Koh, and Wonjun Lyou, "Spending impact of COVID-19 stimulus payments: Evidence from card transaction data in South Korea," *SSRN*, 2020.

Kubota, So, Koichiro Onishi, and Yuta Toyama, "Consumption responses to COVID-19 payments: Evidence from a natural experiment and bank account data," *Covid Economics*, December 2020, *62* (18), 11–23.

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

Lewis, Daniel, Davide Melcangi, and Laura Pilossoph, "Latent Heterogeneity in the Marginal Propensity to Consume," *Federal Reserve Bank of NY Staff Report*, June 2021.

Lusardi, Annamaria, "Permanent Income, Current Income, and Consumption: Evidence from Two Panel Data Sets," *Journal of Business and Economic Statistics*, January 1996, *14* (1), 81–90.

Meyer, Michelle and Anna Zhou, "COVID-19 and the consumer: data through April 16," *Bank of America Global Research Data Analytics*, 22 April 2020.

Misra, Kanishka, Vishal Singh, and Qianyun Poppy Zhang, "Impact of Stay-at-home-orders and Cost-of-living on Stimulus Response: Evidence from the CARES Act," *NYU Stern School of Business Working Paper*, 2021.

Mongey, Simon, Laura Pilossoph, and Alexander Weinberg, "Which workers bear the burden of social distancing?," *The Journal of Economic Inequality*, 2021, *19* (3), 509–526.

Murphy, Dan, "Economic Impact Payments: Uses, payment methods, and costs to recipients," *Economic Studies at Brookings*, February 2021.

Olafsson, Arna and Michaela Pagel, "The Liquid Hand-to-Mouth: Evidence from Personal Finance Management Software," *Review of Financial Studies*, 2018, *31* (11), 4398–4446.

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

Parker, Jonathan A, "The Reaction of Household Consumption to Predictable Changes in Social Security Taxes," *American Economic Review*, 1999, *89* (4), 959–973.

__ , "Why Don't Households Smooth Consumption? Evidence from a 25 Million Dollar Experiment," *American Economic Journal: Macroeconomics*, October 2017, *9* (4), 153–183.

Parker, Jonathan A., Jake Schild, Laura Erhard, and David S. Johnson, "Household Spending Responses to the Economic Impact Payments of 2020: Evidence from the Consumer Expenditure Survey," *BLS Working paper WP-544*, 2021.

__ **, Nicholas S. Souleles, David S. Johnson, and Robert McClelland**, "Consumer Spending and the Economic Stimulus Payments of 2008," *American Economic Review*, October 2013, *103* (6), 2530–53.

Romer, Christina D. and David H. Romer, "A Social Insurance Perspective on Pandemic Fiscal Policy: Implications for Unemployment Insurance and Hazard Pay," 2022, *36* (2), 3–28.

Roth, Jonathan, Pedro H. C. Sant'Anna, Alyssa Bilinski, and John Poe, "What's Trending in Difference-in-Differences? A Synthesis of the Recent Econometrics Literature," *arXiv:2201.01194 [econ.EM]*, 2022.

Sahm, Claudia R., "Robust evidence for $1400 checks: Relief for families and recovery for all," *Jain Family Institute Policy Brief, February 17*, February 17 2021.

__ **, Matthew D. Shapiro, and Joel Slemrod**, "Household Response to the 2008 Tax Rebate: Survey Evidence and Aggregate Implications," in "Tax Policy and the Economy, Volume 24" NBER Chapters, National Bureau of Economic Research, Inc, November 2010, pp. 69–110.

__ **,** __ **, and** __ , "Consumer Response to the Coronavirus Stimulus Programs," *Presentation November 11, 2020. http://macromomblog.com/wp-content/uploads/2021/02/Rebates-2020-111020.pdf*, 2020.

Schild, Jake and Thesia Garner, "Consumer Response to Economic Impact Payments during the COVID-19 Pandemic and the Role of Subjective Assessments of Well-Being: A View from the U.S. Using a Rapid Response Survey," *Bureau of Labor Statistics Working Paper. https://www.bls.gov/osmr/research-papers/2021/pdf/ec210060.pdf*, 2020.

Shapiro, Matthew D. and Joel Slemrod, "Consumer Response to the Timing of Income: Evidence from a Change in Tax Withholding," *American Economic Review*, 1995, *85*, 274–83.

_ and _ , "Did the 2008 Tax Rebates Stimulate Spending?," *American Economic Review*, May 2009, *99* (2), 374–79.

Souleles, Nicholas S, "The Response of Household Consumption to Income Tax Refunds," *American Economic Review*, 1999, *89* (4), 947–958.

Splinter, David, "Stimulus Checks: True-Up and Safe Harbor Costs," *Joint Committee on Taxation Working Paper.*, June 2022.

Stephens, Melvin, ""3rd of the Month": Do Social Security Recipients Smooth Consumption between Checks?," *American Economic Review*, 2003, *93* (1), 406–422.

Sun, Liyang and Sarah Abraham, "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects," *Journal of Econometrics*, December, forthcoming 2021, *225* (2), 175–199.

US Department of Labor, Bureau of Labor Statistics, *Consumer Expenditure Survey: Interview Survey [computer file]*, Washington, DC: US Department of Labor, Bureau of Labor Statistics, 2020.

Wooldridge, Jeffrey, "Two-Way Fixed Effects, the Two-Way Mundlak Regression, and Difference-in-Differences Estimators," *SSRN*, 2021.