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## *Economic Impact Payments and Household Spending during the Pandemic*

**ABSTRACT** Households spent only a small fraction of their 2020 Economic Impact Payments (EIPs) within a month or two of arrival, consistent with pandemic constraints on spending, other pandemic programs and social insurance, and 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 that 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 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.

*Conflict of Interest Disclosure:* Laura Erhard and Jake Schild are employees of the Bureau of Labor Statistics (BLS). BLS reviewed the research to ensure that the paper does not take a political stance and that it has correctly and accurately described and analyzed the consumer expenditure data that the BLS produces and releases to the public. The authors did not receive financial support from any firm or person for this paper or from any firm or person with a financial or political interest in this paper. The authors are not currently an officer, director, or board member of any organization with a financial or political interest in this paper.

*Brookings Papers on Economic Activity*, Fall 2022: 81–130 © 2023 The Brookings Institution.

In response to the economic consequences of the pandemic, the United States government distributed three waves of Economic Impact Payments (EIPs) to American households. In March 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 roughly \$300 billion in 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 over \$400 billion in EIPs.<sup>1</sup>

While these payment programs were modeled 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 that 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 experiencing the impact of the pandemic 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 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 suggest even lower

1. The CRRSA Act was included as a part of the Consolidated Appropriations Act of 2021, which was signed into law on December 27, 2020.

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, Parker, and Souleles (2006) and Parker and others (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), and more often 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 explicit 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 nonrandom differences in spending responses over time and better suited for the variation across households in the EIP programs. In terms of being 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 in recent literature on treatment effects (Borusyak, Jaravel, and Spiess 2021; Orchard, Ramey, and Wieland 2022).

In terms of being 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; almost half of these EIPs were disbursed by direct deposit during the week of April 10, and almost 90 percent of 2020 EIPs were disbursed within the first five weeks.<sup>2</sup> As a result, our main analysis leans heavily on comparing the spending of similar households that do and do not receive EIPs and that receive EIPs of different amounts relative to their typical spending amounts. Receipt

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

status is primarily driven by whether the Internal Revenue Service (IRS) had the information to disburse the payment and whether the household was ineligible due to too high income or citizenship status.<sup>3</sup> Section III presents 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 on nondurable goods and services in response to the receipt of an EIP. For the first round of payments in 2020, 95 percent confidence intervals imply that people increased their spending on nondurable goods and services as measured (roughly 44 percent 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.<sup>4</sup> We find a similar average propensity to increase consumer spending (marginal propensity to increase consumer expenditures, or 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 appears to be underreported in the CE survey, and therefore these spending responses may be underestimated. Nonetheless, all three estimated spending responses on the broad measure of nondurable 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.<sup>5</sup> Particularly during the first wave of EIPs, many types of consumption were constrained by the prevalence of the disease or by government restrictions which, together with

3. For the first round of EIPs for example, 3.8 percent 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 percent of tax units were not eligible for an EIP because their incomes were too high or they did not meet the citizenship requirements, for example, a couple with one noncitizen spouse that filed jointly; see sections I and II (Murphy 2021).

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

5. Cajner and others (2020) and Cox and others (2020) document the large diversity in outcomes in the pandemic recession.

diminishing marginal utility on unaffected goods and services, could have held back the overall expenditure response to the payments. Indeed, Guerrieri and others (2022) 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 toward durable goods during the pandemic.<sup>6</sup>

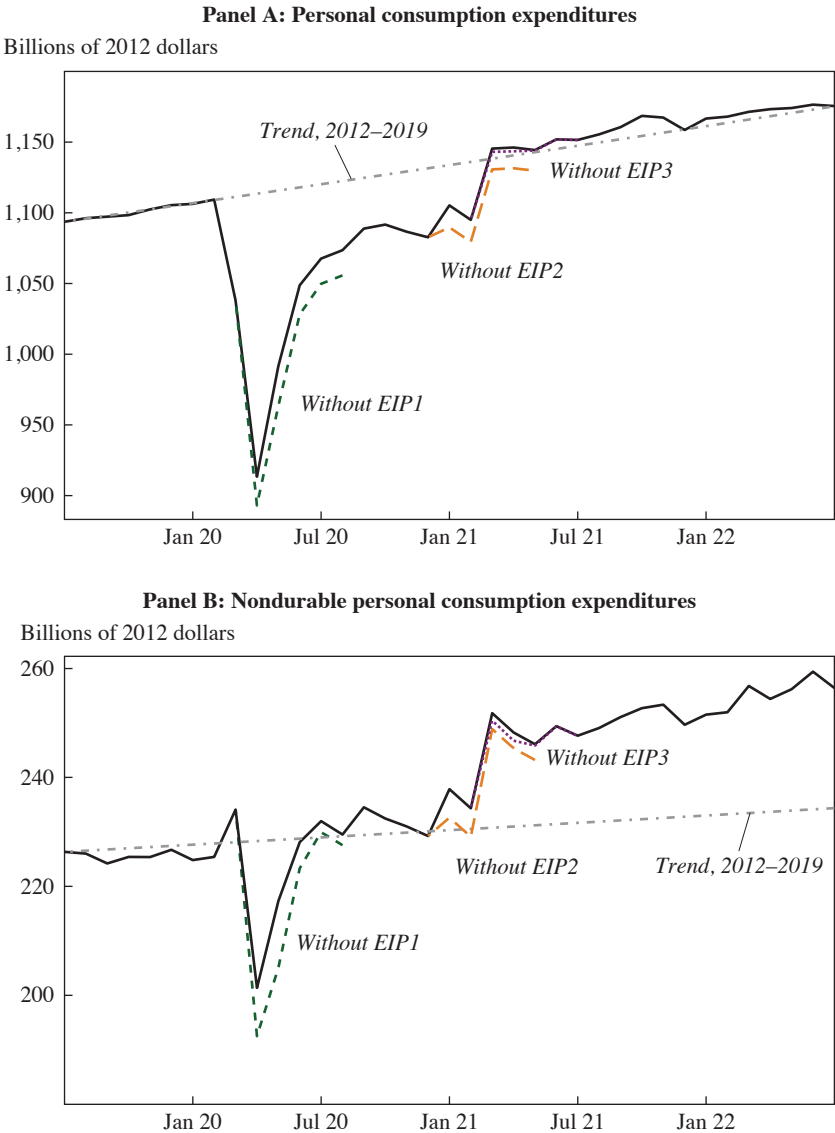
Particularly for the second and third round of EIPs, these low spending responses are also consistent with households on average already having plenty of liquid funds. As the constraints on spending relaxed, the pandemic reduction in spending coupled with other government support (e.g., the paycheck 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 one) and 60 percent (round two) 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 Greig, Deadman, and Sonthalia 2021; Greenwood, Laarits, and Wurgler 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 the increase in spending implied by our estimates assuming that the contemporaneous spending response occurs evenly over the month of

6. In total, we estimate that about 24 percent 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.

**Figure 1. Implied Change in Real Personal Consumption Expenditures Directly Due to Disbursement of EIPs**



Source: Authors' calculations, based on National Income and Product Accounts.

Note: 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 4 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.

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 counterfactuals 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 nondurable goods and services to which the EIPs may have contributed with delay through temporary decreases in debt or increases in saving.<sup>7</sup>

Our second main finding is that while the average spending response to the EIPs is 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 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. Differences in liquidity across households are less important for the second two rounds.<sup>8</sup> Our second measure is based on whether a household 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 of 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 wealth and low debt capacity would be more efficient

7. Note that for nondurable PCE, we use MPC estimates from a CE measure that includes some services and semidurable spending, so it likely overestimates the spending effects of the EIPs.

8. 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.

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.<sup>9</sup> 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). 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, and so on, where the disincentives of these programs are better understood and potentially better minimized (Ganong and others 2022).<sup>10</sup>

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 (Bodkin 1959; Agarwal and Qian 2014; Kueng 2018), over time (Souleles 1999; Parker 1999; Stephens 2003; Farrell, Greig, and Hamoudi 2019; Baugh and others 2021), and using randomization in policy in either dimension (Agarwal, Liu, and Souleles 2007; Broda and Parker 2014; Parker 2017; Lewis, Melcangi, and Pilossoph 2019).<sup>11</sup> 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.<sup>12</sup>

9. Past payments sent out either as pandemic insurance or as a stimulus program have increasingly targeted these populations to some extent by excluding households with high incomes the previous year.

10. 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 it is equivalent to an MPC of 100 percent out of a payment program. That is, rapid government spending raises aggregate demand by more than payment programs with equivalent costs, although obviously the goods and services purchased will differ, as will the distributional effects of the policies.

11. Most closely related, Fagereng, Holm, and Natvik (2021) measure the spending response of (random) lottery winners.

12. Kubota, Onishi, and Toyama (2021), Feldman and Heffetz (2022), and Kim, Koh, and Lyou (2020) measure the spending responses to tax payments disbursed in response to the pandemic in Japan, Israel, and South Korea, respectively.



The first rapid analysis of the spending changes caused by the EIPs, Meyer and Zhou (2020), used Bank of America transactions data and reported 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 percent of EIPs were disbursed) relative to those that did not. Daily spending increased by an average of 50 percent year over year between April 15 and 16 for households with incomes below \$50,000 and by only 3 percent for households with incomes above \$125,000. Also using aggregated data, Chetty and others (2020) find that over this same couple of days, credit card spending in zip codes in the bottom quarter of the distribution of average household income rose by 25 percentage points while those in the top quarter of the distribution rose by only 8 percentage points. Finally, also using zip code-level data and using incidental differences in timing in EIP disbursements across zip codes, Misra, Singh, and Zhang (2021) infer an MPC of 50 percent in the few days after an EIP arrived.

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 and others (2020), and Cooper and Olivei (2021) find that people's out-of-account spending rises cumulatively by 46 percent, 25–40 percent, and 66 percent of their first-round EIPs, respectively, within a few weeks of receipt.<sup>13</sup> 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.<sup>14</sup> There are other possible reasons 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 unlinked credit cards, payments of overdue bills from past consumption, or transfers to investment accounts).<sup>15</sup> Alternatively, respondents in the CE survey could

13. Karger and Rajan (2021) also estimate a 39 percent MPC for the second round of EIPs.

14. The accounts used in Karger and Rajan (2021) are skewed toward lower income households (average annual income of \$20,880); the households in Baker and others (2020) 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) use Factiveus data covering lower-income households many of whom are unbanked.

15. Baker and others (2020) include car loans and mortgage payments as consumption-related spending, whereas this paper includes interest payments on mortgage loans as part of consumption-related spending, but not payments on the principal.

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.<sup>16</sup>

## I. The Economic Impact Payments

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.<sup>17</sup> For every \$100 of AGI over the threshold, the stimulus payment was reduced by \$5.

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 phaseout rate for this second round of EIPs were the same as for the first round.<sup>18</sup>

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 additional payments of \$1,400 for

16. The CE is a small data set, with a similar number of recipients to that in Baker and others (2020), 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.

17. In December 2020, the phaseout 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. 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.

each qualifying dependent. They were also distributed slightly more broadly along several small dimensions, and included a definition of “qualifying dependent” that was expanded to include dependents over the age of 17. The upper income thresholds were the same as in the first and second rounds; however, the phaseout rule was more aggressive so that the larger amounts did not lead to EIPs being received higher up the income distribution. Specifically, rather than a constant phaseout rate, income thresholds were set such that tax filers with a 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.<sup>19</sup> For example, an individual with no dependents, base payment of \$1,400, had a phaseout 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 phaseout rate of \$56 for every \$100 of AGI over \$75,000. Figure 2 displays the EIP amounts as a function of income for various family structures for the first, second, and third round of EIPs.

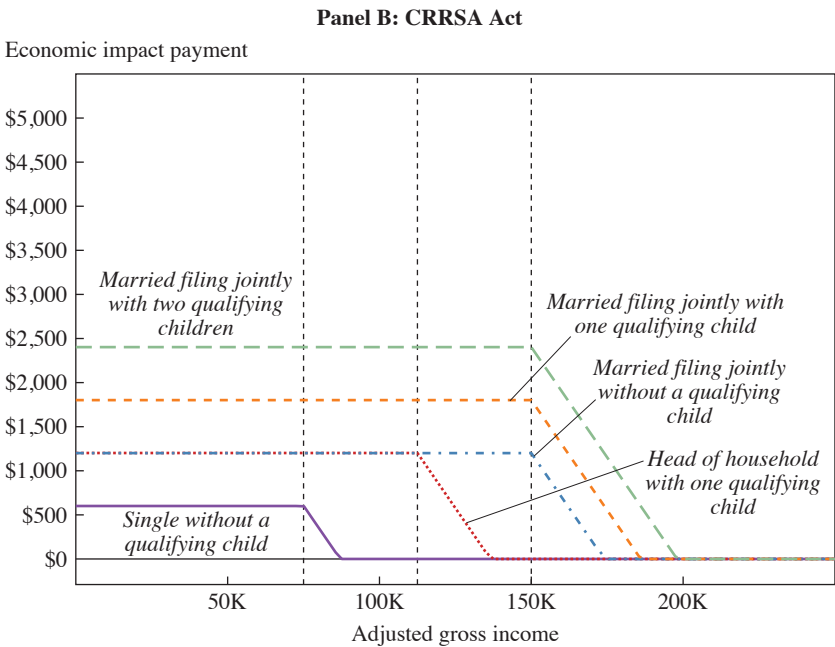
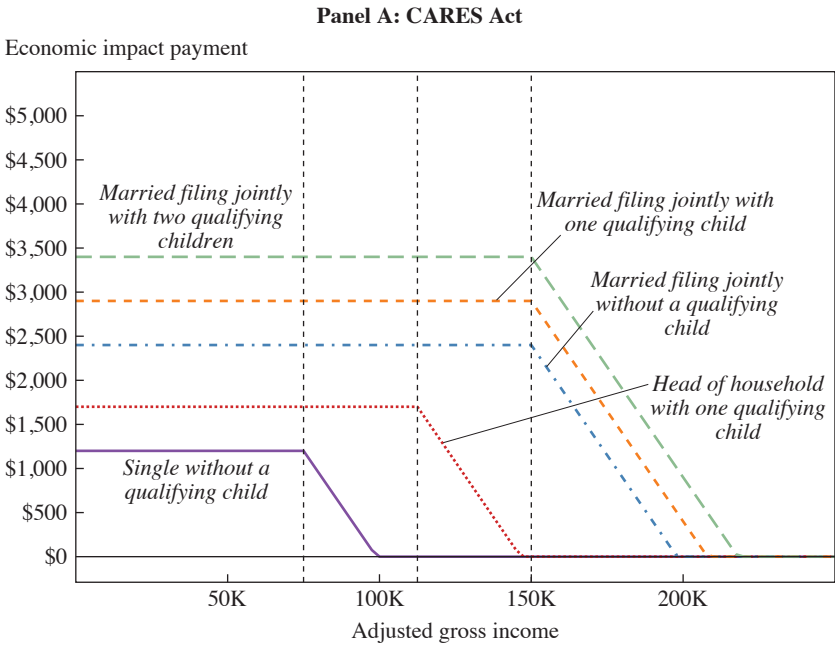
In addition to households receiving different amounts of EIPs, households also received them at different times. In each round, most taxpayers who had included their bank information when filing a recent tax return (e.g., for a refund) received their EIP during the first week of disbursement. For EIP1, 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 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.<sup>20</sup> For EIP1, this constituted roughly 30 million households (Murphy 2021). The IRS also collected information on eligible households from the Social Security Administration and the Department of Veterans Affairs (and the Railroad Retirement Board).

The IRS began depositing EIP1s into bank accounts mid-April 2020, and using the information that the IRS was able to gather and process in time, roughly 105 million or about 62 percent of all EIPs were disbursed in April 2020 (Murphy 2021). For eligible households without the necessary bank information, the EIPs arrived starting in mid-April by mailing a paper check or prepaid EIP card. The disbursement of checks occurred

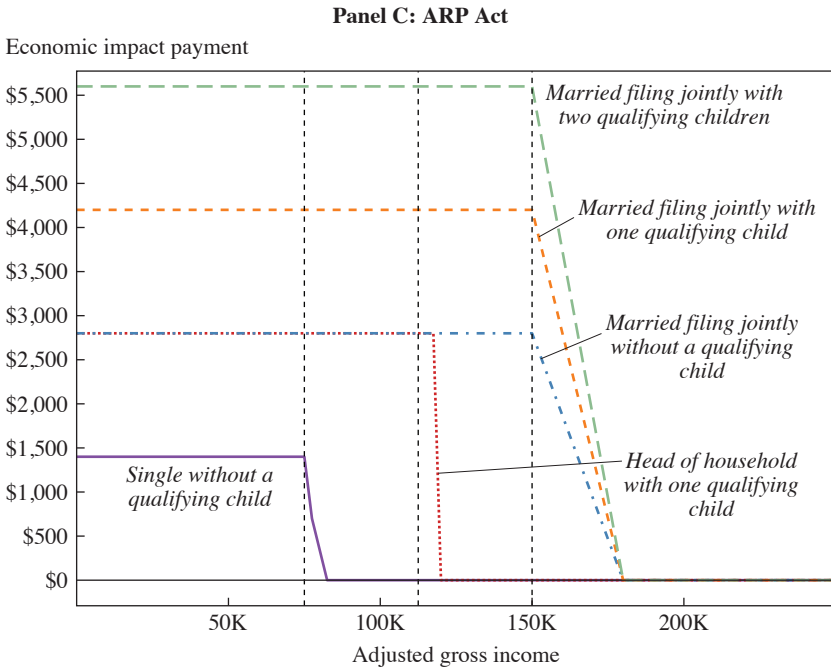
19. If a 2020 tax return had not yet been filed, then 2019 AGI from the 2019 tax return filed in 2020 was used instead.

20. IRS, “Get My Payment,” <https://www.irs.gov/coronavirus/get-my-payment>; no longer available, but as of this writing, there are links to further information.

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



**Figure 2.** Economic Impact Payment Amounts as a Function of AGI and Family Structure (*Continued*)



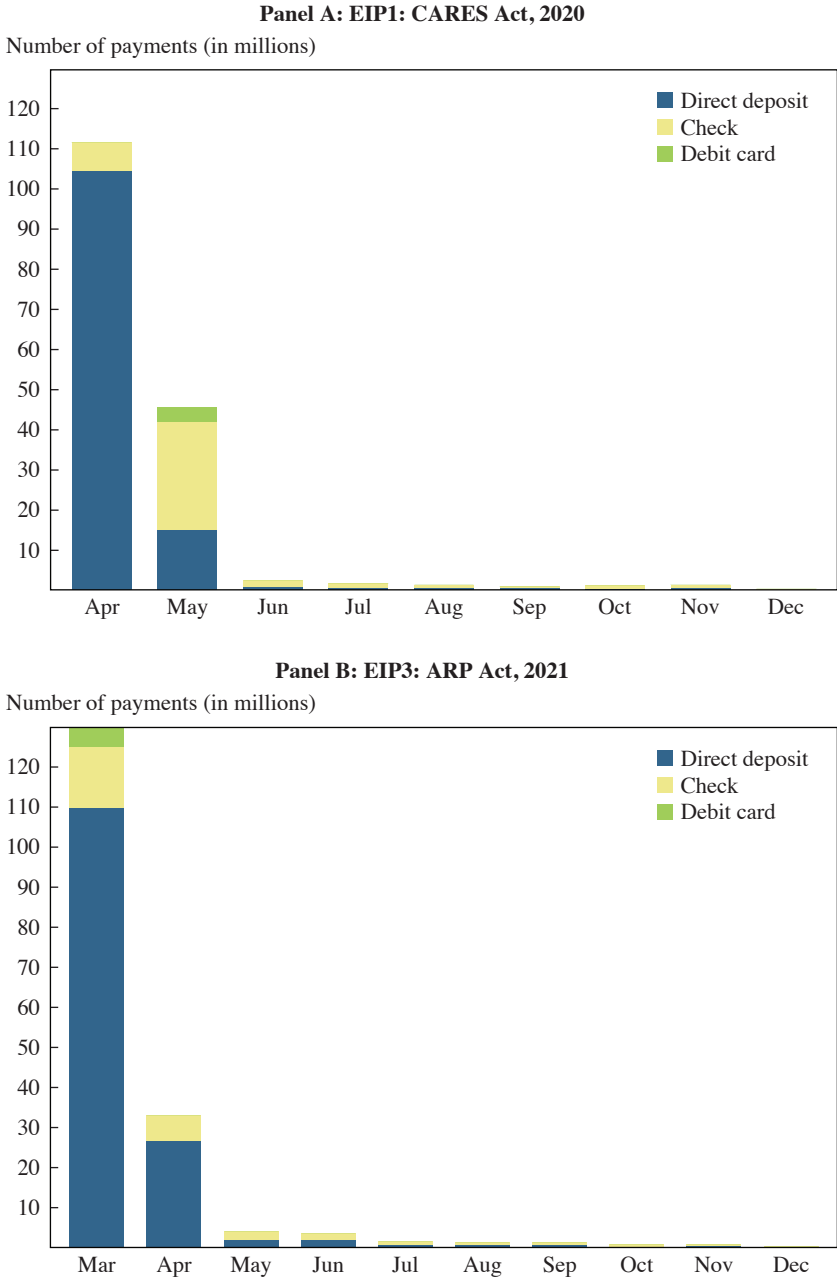
Source: Authors' calculations.

with a greater delay. By the end of April only about 7 million checks (4 percent of EIPs) were sent out. Most of the checks were sent out in May, about 27 million or 16 percent of EIPs, and all of the EIP cards were sent out in May, about 4 million or 2 percent of EIPs. About 95 percent of all first round EIPs were delivered in the first two months of disbursement. The remainder of the EIPs continued to trickle out over the rest of 2020. Figure 3, panel A, 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. Almost all of the EIP2s were distributed in January 2021. Daily Treasury statements show some EIP2s were also being disbursed in February, which was 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

**Figure 3. The Disbursement of EIP Payments over Time and by Mode of Distribution**



Source: Bureau of the Fiscal Service.

Note: Months are the disbursement months.

than the first round of EIPs. A full 74 percent of all EIP3s were distributed in March 2021 (62 percent by direct deposit; 8.5 percent by check; and 2.7 percent by EIP cards). By the end of April about 92 percent of all third-round EIPs had been distributed, with the remaining 8 percent 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 percent) 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 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, Capps, and Fix 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 an SSN. This change resulted in 2.9 million people becoming eligible.<sup>21</sup> The ARP Act further expanded the eligibility criteria to anyone with an SSN, which resulted in an additional 2.2 million eligible individuals.<sup>22</sup>

Second, eligible households did not receive an EIP disbursement if they had changed accounts 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 the Social Security Administration). For example, four months after the CARES Act, 10 percent of EIPs had not been disbursed, and 5 percent or 9 million eligible households had not received an EIP by the end of September (Murphy 2021). For EIP2 or EIP3, people who relocated even temporarily during the pandemic and

21. 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.

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

formally changed their addresses or banks accounts became ineligible for EIP disbursement.<sup>23</sup>

Finally, the third reason for households not receiving 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 who fell into either of the first two categories and so did not receive 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 \$45 billion in tax year 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.<sup>24</sup>

In aggregate \$271 billion was disbursed during the first EIP round, \$141 billion during the second EIP round, and \$402 billion during the third EIP round.<sup>25</sup> 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 5.2 percent of GDP or 7.9 percent of PCE in that quarter (figure 3; IRS 2020). The first quarter of 2021 saw \$473 billion of EIPs disbursed, from both the second and third rounds. This represents 8.7 percent of GDP and 3.2 percent of PCE during the quarter. The third EIP round additionally disbursed \$67 billion in the second quarter of 2021, corresponding to 1.2 percent of GDP and 1.8 percent of PCE. The next section describes the EIPs as recorded in our CE data set.

23. IRS, “2021 Recovery Rebate Credit Questions and Answers,” <https://www.irs.gov/newsroom/2021-recovery-rebate-credit-questions-and-answers>.

24. These safe harbor amounts were roughly \$21 billion in tax year 2020 and \$44 billion in tax year 2021 (Splinter 2022).

25. IRS, “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>.



## 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 (BLS).<sup>26</sup> The CE data set contains spending, demographics, and other financial information on households living in the United States. The BLS structures the CE so that a consumer unit (CU) at a given address, which we will refer to as a household, is interviewed up to four times at three-month intervals about their spending over the previous three months (the 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.<sup>27</sup> Online 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 that the questions were not fielded in January 2021.<sup>28</sup> 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).<sup>29</sup> 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 exactly follow previous CE surveys, the

26. Information on the data and methods of survey can be found at US Bureau of Labor Statistics, “Consumer Expenditure Surveys,” <https://www.bls.gov/cex/>.

27. “Overlapping” means that 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.

28. 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 coauthors of this paper.

29. 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 five 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.

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. Online 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 questionnaire 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 percent of EIP1s arrive after May 2020, we focus primarily on analysis that leans heavily on other sources of variation, like amount and recipient status.<sup>30</sup>

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

30. 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).

the reference period (October, November, and December) for households interviewed in January 2021 (when the EIP questions were not asked).<sup>31</sup>

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 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. 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 online appendix A.5.3 and table C.5–C.7.) We discuss these choices in detail in the next section.

Table 1 shows 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 are adjusted up by 50 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).<sup>32</sup> 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 percent of households do not receive an EIP1 according to the CE data compared to 20 percent in reality (3.2 percent of households were eligible tax units who were

31. Less than 2 percent 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 4, the official payment date according to the IRS. Checks for EIP3 did not begin being distributed until January (Murphy 2021).

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

**Table 1. Percentage of EIPs by Month and Percentage of Households Not Receiving EIPs**

	Unweighted CE final sample (1)	Weighted CE final sample (2)	Unweighted CE (adjusted) (3)	Weighted CE (adjusted) (4)	Census Bureau's Household Pulse Survey and US Treasury (5)
<i>Panel A: The distribution of EIPs across months, as a percentage</i>					
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
July to November 2020	2.4	2.3	2.7	2.6	6.8
<i>Panel B: Percentage of households or tax units not receiving an EIP1</i>					
Total (households)	17.0	17.0	24.7	24.6	16.2
Ineligible (tax units)					3.2
Eligible (tax units)					
<i>Panel C: The distribution of EIP2s across months, as a percentage</i>					
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
<i>Panel D: Percentage of households or tax units not receiving an EIP2</i>					
Total (households)	50.8	51.9	52.2	53.0	
<i>Panel E: The distribution of EIP3s across months, as a percentage</i>					
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
June to December 2021	4.6	4.7	4.6	4.6	5.2
<i>Panel F: Percentage of households or tax units not receiving an EIP3</i>					
Total (households)	29.4	29.0	40.5	40.3	

Source: Authors' calculations.

Note: Weighted data using the average of FINLWT1 across all interviews. All samples use available CE data, so interviews through and including September 2021. See online 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 panel B ineligible households is as self-reported in the Census Pulse Survey from Garner, Safir, and Schild (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 come from the Bureau of the Fiscal Service, US Department of the Treasury.

non-recipients in 2020, and 16 percent of households were not eligible for EIPs). In our final CE data set, about 17 percent of households do not receive an EIP1 because we drop households with high incomes. 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 are 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.<sup>33</sup> The average EIP2 amount is \$1,301, and the average EIP3 amount is more than double this amount, \$2,814. Online appendix tables C.1, C.2, and C.3 show the distribution of total EIP amounts received across household reference periods in our CE final sample (unweighted, unadjusted) and that 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 percent 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).

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 February 2021), and 168 million third-round EIPs disbursed in 2021 totaling \$402 billion.<sup>34</sup> 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.<sup>35</sup> Households that receive EIP1 and EIP2 by direct deposit on average have slightly higher expenditures, are slightly younger, have higher incomes, have lower liquidity, and have larger EIPs than households that receive

33. IRS, "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>. When using all CE data, and without aggregating to the three-month reference period level, the average (unweighted, unadjusted) EIP is \$1,837.

34. IRS, "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>.

35. 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 similarly for third-round EIPs, since data from interviews after September 2021 are not yet published.

**Table 2.** The Share of EIPs by Method of Disbursement and Reported Main Use

	<i>EIP1</i>	<i>EIP2</i>	<i>EIP3</i>
<i>Panel A: Distribution of payment methods, as a percentage</i>			
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
<i>Panel B: Distribution of reported main use, as a percentage</i>			
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

Source: Authors' calculations.

Note: Statistics based on CE final sample include only CE households with certain interviews (June or July 2020 for EIP1; February, March, or April 2021 for EIP2; and April, May, or June 2021 for EIP3), with income that does not exceed a certain threshold determined by marital status and family structure, and with cleaning as described in online appendix A.5.3. Weights applied are the average of CU weights across all interviews for that CU.

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 percent of EIP1s in the CE were reported as being received by direct deposit, 23 percent by paper check, and 2 percent by debit card. The Treasury reports that 76 percent of EIP1s were disbursed by electronic deposit, 22 percent by paper check, and 2 percent by debit card during 2020.<sup>36</sup> 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 percent of EIP1s had been distributed by debit card and an additional 1 percent by deposit onto benefit cards (Murphy 2021). Consistent with the increase in direct deposit across rounds, the CE shows the share of households receiving their EIP by direct deposit increasing in each subsequent round.

The BLS also asked households to report on the CE survey whether they spent or saved their EIPs.<sup>37</sup> The responses suggest greater spending than our analysis of expenditures does. Panel B of table 2 shows that 56 percent

36. IRS, "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>.

37. This is the reported preference methodology of Shapiro and Slemrod (1995).

of households report using their EIPs mostly for expenses, and this fraction declines slightly across EIP rounds. 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 percent of households would “mostly spend” their tax payments and 51 percent would “mostly pay down debt.”

More comparable over time, Sahm, Shapiro, and Slemrod (2010, 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 percent of respondents report that their EIPs will cause them to “mostly increase spending,” only 1 percent lower than in 2008, which suggests little difference in rate of spending between the EIPs and earlier stimulus payments.<sup>38</sup>

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 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) nondurable expenditures on goods and services, which adds semidurable categories like apparel, reading materials, and health care (only out-of-pocket spending by the household) following previous research using the CE survey; and (4) total expenditures, which adds durable expenditures such as home furnishings, entertainment equipment, and auto purchases.<sup>39</sup>

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

38. Garner and Schild (2021), Garner, Safir, and Schild (2020), and Boutros (2021) provide in-depth analysis of the US Census Bureau’s Household Pulse Survey in which 59 percent of respondents state that they “will mostly pay for expenses” with their EIPs. More similar to Sahm, Shapiro, and Slemrod (2020), Coibion, Gorodnichenko, and Weber (2020) show 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 percent. Armantier and others (2020) report a slightly larger number in the Federal Reserve Bank of New York Survey of Consumer Expectations in which households on average say that they consumed 29 percent of their EIPs.

39. The exact definitions are given in online appendix A.3.

or cash, just the amount.<sup>40</sup> 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 US households constructed by the US Census and so when weighted is representative of the US population (along the dimensions of the Census-based strata and conditional on participation in the survey). The main weaknesses relative to existing studies are the relatively small sample size, sampling (e.g., nonresponse) 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 accounts 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, Parker, and Souleles (2006) and Parker and others (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 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  $t$ :

$$(1) \quad \Delta C_{i,t} = \sum_{s=0}^S \beta_s EIPn_{i,t-s} + X_{i,t} \gamma + \tau_t + \epsilon_{i,t}.$$

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

40. For example, terms like “Amazon” or “Starbucks” or “Sammy White’s.” Payments to unlinked credit cards and transfers to other accounts are also difficult to categorize as spending for consumption, debt payment, or saving.



period  $t - s$ .<sup>41</sup> The variable  $\tau_t$  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  $X_{i,t}$  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,  $\epsilon$  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  $\epsilon$  is uncorrelated with the other right-hand-side regressors (and for now maintaining the assumption that  $\beta$ —or its distribution over  $i$ —does vary with EIP arrival date), the key coefficient  $\beta_s$  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  $\Delta C$ ,  $\beta_s$  measures the share of the EIP spent, or the marginal propensity to increase consumer expenditures (MPC).<sup>42</sup> 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 with the methodology, driven by differences between previous tax rebate programs and this one, differences between previous recessions and the pandemic recession, and 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

41. In table 3 and in additional results in the online 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 online appendix, we present some results that use change in log consumption as the dependent variable.

42. When  $\mathbb{1}[EIPn_{i,t-s} > 0]$  is regressed on  $\Delta C$ ,  $\beta_s$  measures the dollars spent. And when  $\mathbb{1}[EIPn_{i,t-s} > 0]$  is regressed on  $\Delta \ln C$ ,  $100 * \beta_s$  measures the percentage increase in spending.

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 phaseout 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 pretax 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.<sup>43</sup> Thus, we adjust each income cutoff up in increments of \$25,000 until more than 80 percent 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 children to be no lower than the cutoff for households that are otherwise the same but without children (i.e., married without children and married with children), if the former has a lower cutoff after increments.<sup>44</sup> 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 EIP round—as our final samples, and it is these samples that are tabulated in section II.

Another difference between previous tax rebates and these EIPs is that there are three waves of EIPs in reasonably rapid succession, and in

43. 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.

44. Online appendix tables C.5, C.6, and C.7 show the selection of resulting cutoffs and the number of recipients in the \$25,000 income ranges above and below each cutoff.

equation (1), the estimated spending responses to one EIP 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 other two 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 recipient 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 that 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 the pandemic in particular, households with higher incomes have much larger changes in dollar spending on average.<sup>45</sup> 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 EIPs 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.<sup>46</sup>

However, groups of people with different incomes—and so with different average levels of consumption spending—experienced roughly similar percentage changes in consumer spending over time (Cox and others 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  $\Delta C$  (see online appendix figure C.1, panels b and 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

45. Online appendix figure C.1, panels a and c, show this across terciles of the income distribution.

46. 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.

variable that equals one for households that receive at least one EIPn, we infer MPCs from the equation:

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

where  $X$  contains (scaled) age, change in family size, and possible previous EIPs. The main coefficient of interest,  $\beta_s$ , still measures the propensity to spend out of an EIP, but by scaling all variables we have transformed the  $\tau$  from absorbing the average change in dollar spending across households in that period to absorbing the average percentage 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 percentage deviation of consumer expenditure rather than dollar deviation. In the CE survey, the average percentage 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 online appendix figure C.2, panel a to panel b and panel c to panel 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 (Borusyak, Jaravel, and Speiss 2021; de Chaisemartin and D'Haultfuille 2020; Goodman-Bacon 2021; Sun and Abraham 2021; Callaway and Sant'Anna 2021; Wooldridge 2021). In our context, estimation of equation (1) would be biased if there is variation in average MPC, or  $\beta_s$ , 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  $\beta_s$  instead of  $\beta_{s,t}$ .<sup>47</sup> To be clear, any variation in the tendency to spend out of EIPs in different rounds (one, two, and three) would not create any bias.

On the one hand, variation in  $\beta_s$  across households receiving the EIP at different times could be significant because when each household received its EIP is nonrandom (unlike in previous payment programs). Later recipients tended to be households for which the IRS did not have their bank

47. 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.

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  $\beta_s$  over time could arise from variation in the economy or the pandemic situation.<sup>48</sup> 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 and others (2021) show 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, Jaravel, and Spiess (2021), which allows differences in MPC or  $\beta_s$  over time and is unbiased under generalized parallel trends (and no treatment anticipation) assumptions.<sup>49</sup> The estimation method can be clearly described as a three-step procedure. Denoting the set of never-treated and not-yet-treated observations as  $\Omega_0$ , in the first step we estimate the time dummies and coefficients on controls using only  $\Omega_0$ :<sup>50</sup>

$$(3) \quad \Delta\tilde{C}_{i,t} = \tilde{X}_{i,t} \gamma + \tau_t + \alpha_{R(i)} + \eta_{i,t} \quad \forall \{i, t\} \in \Omega_0.$$

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  $\Delta\check{C}_{i,t}$ :

$$(4) \quad \Delta\check{C}_{i,t} = \Delta\tilde{C}_{i,t} - \tilde{X}_{i,t} \hat{\gamma} + \hat{\tau}_t - \hat{\alpha}_{R(i)} \quad \forall \{i, t\} \notin \Omega_0.$$

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

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

48. 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.

49. The estimator is also efficient under homoskedasticity and is asymptotically conservative when standard errors are clustered.

50. 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.

Our method solved the issue created by “forbidden comparison,” but note that the third step deviates from Borusyak, Jaravel, and Spiess (2021)—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 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.<sup>51</sup>

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.<sup>52</sup>

#### **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 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 data sets.

Table 3 displays the main spending responses to all three rounds of EIPs, both the average fraction of the EIP that is spent shortly after arrival

51. 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, for example, Baker, Larcker, and Wang (2022) and Roth and others (2022) for the stacked regression method.

52. We make three other choices that differ slightly from previous analyses. As in previous papers, we drop the bottom 1 percent of the distribution in broad nondurable consumer expenditures after adjusting for family size, but instead of estimating the bottom 1 percent using a quantile regression on a linear trend, we drop the bottom 1 percent 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 and others (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**

	MPC				Dollars spent			
	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
<i>Panel A. EIP1</i>								
<i>EIP1</i>	0.011 (0.016)	0.075 (0.020)	0.102 (0.028)	0.234 (0.059)				
1  <i>EIP1</i> > 0]					6.5 (25.3)	96.4 (36.6)	80.8 (46.4)	336.5 (96.6)
<i>Panel B. EIP2</i>								
<i>EIP2</i>	0.034 (0.021)	0.103 (0.031)	0.083 (0.039)	0.247 (0.090)				
1  <i>EIP2</i> > 0]					18.8 (23.6)	80.8 (44.0)	65.6 (52.2)	156.7 (114.4)
<i>Panel C. EIP3</i>								
<i>EIP3</i>	0.036 (0.017)	0.030 (0.016)	0.009 (0.018)	0.015 (0.043)				
1  <i>EIP3</i> > 0]					99.5 (33.8)	86.8 (40.8)	55.1 (42.2)	-36.0 (102.4)
<i>Average quarterly household spending across three waves</i>	\$2,292	\$4,516	\$5,996	\$14,401	\$2,292	\$4,516	\$5,996	\$14,401

Source: Authors' calculations.

Note: Table reports estimation of equations (3)–(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, which has 2,261 observations. For EIP2, the columns have 3,171, 3,171, 3,171, 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.

(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 nondurable goods and services by 10.2 percent of EIP1, and on all CE-measured goods and services by 23.4 percent of EIP1. Taking the perspective of classical statistics, the 95 percent confidence intervals of cumulative spending rule out spending in excess of 16 percent of the EIP on nondurable goods and services and 35 percent on all CE goods and services.

The first four rows of panel B show similar low spending responses for the second round of EIPs. The third and fourth columns show that 8 percent and 25 percent of the EIP2s were used for expenditures on nondurable 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 toward durable goods during the pandemic, the spending response to the EIPs was similarly tilted toward durable goods. 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 rounds. 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 maintain some skepticism that the actual spending response was quite this low, particularly for the third round of the EIPs. However, the lower spending response is consistent both with the rise in liquid balances throughout the pandemic (Grieg, Deadman, and Sonthalia 2021) 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 underreporting of EIP receipt in the CE? Underreporting 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 online 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. Online appendix tables C.8–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 suggests 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 3 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 percent of the average EIP1 on nondurable goods and services (statistically insignificant, column 7) and \$337 or 16 percent 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 percent and 12 percent 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 responses to the third round of the EIPs, particularly because the average EIP3 is one-third 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 4 shows the longer-run response of spending to the receipt of an EIP. The coefficient  $\beta_1$  on  $EIP_{t,t-s}$  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 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 percentage of the EIP).

For EIP1, the cumulative MPC on strictly nondurable and broad nondurable goods and services are both roughly 13 percent and on all CE goods and services is 45 percent (with a standard error of 15.8 percent). 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. Online appendix tables C.11–C.13 show 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 2008, according to studies using the same survey data.

Are these relatively low spending responses due to our differences (improvements) in methodology? No. To show this, we apply the methodology of Johnson, Parker, and Souleles (2006) and Parker and others (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 specifications and columns but on average are not inconsistent with the results shown in table 3 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 percent on food, 7.1 percent on strictly nondurables, 7.7 percent on the broad measure of nondurable goods and services, and 28.0 percent on all goods and services. While all these estimates are statistically insignificant, these point estimates are consistent with those in table 3. But this methodology leads to wildly different conclusions for other specifications (unlike when the same analysis was used on the 2001 and 2008 tax payments), consistent with

**Table 4. The Longer-Term Response of Consumer Expenditures to EIP Receipt**

*Dependent variable: scaled dollar change in spending on:*

	<i>EIP1</i>		<i>EIP2</i>		<i>EIP3</i>	
	<i>Strictly nondurables</i>	<i>All CE goods and services</i>	<i>Strictly nondurables</i>	<i>All CE goods and services</i>	<i>Strictly nondurables</i>	<i>All CE goods and services</i>
$EIPh_t$	0.075 (0.020)	0.234 (0.059)	0.103 (0.031)	0.083 (0.039)	0.030 (0.016)	0.015 (0.043)
$EIPh_{t-1}$	-0.011 (0.020)	-0.017 (0.070)	0.030 (0.038)	-0.013 (0.045)	0.000 (0.010)	-0.150 (0.049)
<i>Implied cumulative fraction of EIP spent over two three-month periods</i>						
	0.139 (0.051)	0.452 (0.158)	0.235 (0.086)	0.153 (0.104)	0.059 (0.036)	-0.119 (0.112)

Source: Authors' calculations.

Note: Table reports  $\beta_0$  and  $\beta_1$  from estimation of equations (3)–(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 EIP1, 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 EIP2, observations are those with an interview in February, March, or April 2021; the columns have 4,815, 4,817, and 4,818 never-treated or not-yet-treated observations. For EIP3, observations are those with an interview in April, May, or June 2021; the columns have 3,474, 3,477, and 3,474 never-treated or not-yet-treated observations, and 3,566, 3,568, and 3,568 treated observations, respectively.

**Table 5.** Estimated MPCs on CE-Measured Nondurable Goods and Some Services

	<i>Full sample, three months of receipt</i>	<i>Recipients only, three months of receipt</i>	<i>Full sample, three months of receipt and subsequent three months</i>
2001 economic rebates	0.386 (0.135)	0.247 (0.213)	0.691* (0.260)
2008 stimulus payments	0.121 (0.055)	0.308 (0.112)	0.347 (0.155)
2020 EIP1	0.102 (0.028)	-0.062 (0.072)	0.124 (0.068)
2020 EIP2	0.083 (0.039)		0.153 (0.104)
2020 EIP3	0.009 (0.018)		-0.030 (0.047)

Sources: Johnson, Parker, and Souleles (2006); Parker and others (2013); and Parker and others (2021).

Note: The asterisk (\*) denotes a large MPC driven in part by one outlier in spending on food.

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 nondurables, 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 percent, 14 percent, 18 percent, and 61 percent, respectively, roughly double those from estimating the MPC directly (the average of  $EIP_{it}$  conditional on receipt is \$2,098). Online 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.<sup>53</sup>

53. Johnson, Parker, and Souleles (2006) and Parker and others (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 and others (2021) shows that the standard errors are typically 50 percent to 100 percent larger than in table 6 here and online appendix 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.

**Table 6. The Response of Consumer Expenditure to EIP Arrival Estimated on Recipients and Non-recipients Using the Methodology Previously Applied to Tax Rebates**

	MPC					Dollars spent		
	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
<i>Panel A. EIP1</i>								
<i>EIP1</i>	0.043 (0.032)	0.071 (0.044)	0.077 (0.059)	0.280 (0.217)				
1[EIP1 > 0]					157.3 (89.9)	296.4 (130.2)	375.0 (167.8)	1278.8 (647.5)
<i>Panel B. EIP2</i>								
<i>EIP2</i>	0.011 (0.029)	0.037 (0.044)	0.030 (0.055)	0.008 (0.325)				
1[EIP2 > 0]					-57.1 (51.7)	-11.1 (79.3)	-10.1 (99.5)	-498.7 (749.8)
<i>Panel C. EIP3</i>								
<i>EIP3</i>	0.001 (0.013)	0.001 (0.017)	0.005 (0.023)	0.222 (0.149)				
1[EIP3 > 0]					14.2 (45.1)	-6.3 (70.3)	22.7 (91.4)	702.1 (648.7)

Source: Authors' calculations.

Note: Table reports  $\beta_1$  from estimation of equation (1) with  $S = 0$  with dollar change in consumption as the dependent variable and 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 online 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.

## 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 experienced the greatest impact economically as a result of 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 is 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 that households entering 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. 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:

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

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  $X$ ) 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 (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 accounts, and certificates of deposits at the start of the households' first interview (reported in the last interview).<sup>54</sup> Table 7 shows that, for EIP1, 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 percent, 22 percent, and 48 percent on food, nondurable 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 nondurable 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 for households with lower liquidity (Parker 2017; Olafsson and Pagel 2018; Ganong and others 2020; Baugh and others 2021; Fagereng, Holm, and Natvik 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 percent of their EIPs on nondurable goods and services, the middle third in terms of liquidity spent 11 percent, while the most liquid households are estimated to spend a negative amount. The MPCs on total expenditures are more related to liquidity: 41 percent, 22 percent, and -5 percent 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 Garner and Schild (2021), which shows that in the Household

54. Even the low liquidity group has substantial reported wealth, and in particular the distribution of reported liquid wealth is much higher in these 2020 data than it was in 2008. In Parker and others (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 online appendix A.4. More likely, this difference reflects changes in the CE survey and the financial accounts that it covers. In 2008 the CE asked about balances in checking and saving accounts separately, but 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**

		<i>Dependent variable: scaled dollar change in spending on:</i>							
		EIP1		EIP2		EIP3			
		Bottom third ≤ 2,000 Top third ≥ 12,667		Bottom third ≤ 2,000 Top third ≥ 12,000		Bottom third ≤ 2,000 Top third ≥ 10,000			
		All CE goods and services		All CE goods and services		All CE goods and services			
		Food and alcohol	Nondurables	Food and alcohol	Nondurables	Food and alcohol	Nondurables		
		Food and alcohol	Nondurables	Food and alcohol	Nondurables	Food and alcohol	Nondurables		
$EIP_{n_t}$		0.039 (0.033)	0.087 (0.064)	-0.032 (0.071)	0.112 (0.111)	0.078 (0.035)	0.132 (0.062)	0.081 (0.197)	
$EIP_{n_t} \times \text{Bottom third}$		0.016 (0.051)	0.130 (0.095)	0.050 (0.084)	0.009 (0.157)	-0.018 (0.048)	-0.099 (0.087)	-0.065 (0.219)	
$EIP_{n_t} \times \text{Top third}$		0.013 (0.046)	-0.188 (0.102)	-0.090 (0.085)	-0.255 (0.139)	0.048 (0.101)	-0.129 (0.099)	-0.057 (0.267)	
<i>p</i> -value for test of equality of responses		0.942	0.011	0.107	0.077	0.784	0.355	0.957	
<i>Implied propensity to spend by group</i>									
Least liquid third		0.055 (0.039)	0.217 (0.070)	0.018 (0.046)	0.121 (0.112)	0.060 (0.034)	0.033 (0.062)	0.016 (0.095)	
Most liquid third		0.052 (0.032)	-0.101 (0.079)	-0.122 (0.048)	-0.143 (0.083)	0.126 (0.094)	0.003 (0.077)	0.024 (0.180)	

Source: Authors' calculations.

Note: 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, and 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  $H_0: \beta_{0, \text{Bottom third}} = 0$  and  $\beta_{0, \text{Top third}} = 0$ . For EIP1, 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, and 1,609 treated observations, respectively. For EIP2, 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 EIP3, 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.



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, the latest in the pandemic, and the most likely to be 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 percent (6 percent) on nondurable goods and services, compared to 3 percent (6 percent) and 0.3 percent (8 percent) 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 appears 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 classifications by occupation and education in Mongey, Pilossoph, and Weinberg (2021) and Dingel and Neiman (2020). For individuals 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. Online 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

**Table 8.** The Response of Consumer Expenditures to EIP1 Receipt by the Exposure of Income to Inability to Work from Home in 2020

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

Source: Authors' calculations.

Note: 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, and 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  $\beta_{0,Middle\ third} = 0$ .

to the first round of EIPs. The 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 37 percent (19 percent) on nondurable goods and services, while the most exposed third had a similar average MPC of 29 percent (16 percent), and an MPC on total expenditures of 84 percent (45 percent), 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 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 on 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 online appendix B.3 for details).

Using this second measure, 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. There are 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 nondurable 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 Discussion**

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, which 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 was being disbursed. These factors appear to have led to less spending on nondurable goods and services (CE-measured) in response to the arrival of the first round of EIPs than was the case with the tax rebates in 2001 and 2008 and to have tilted what spending response there was toward durable goods. We observe low and similar spending responses to the first and second rounds of EIPs but very little short-run spending in response to the third round, consistent with preexisting high levels of financial resources, although the response is not as cleanly measured as the first two rounds of EIPs.

**Table 9. The Contemporaneous Response of Consumer Expenditures to EIP Receipt by Ability to Work from Home**

		Dependent variable: scaled dollar change in spending on:											
		EIP1				EIP2				EIP3			
		Bottom third ≤ 89.4% Top third ≥ 99.1%				Bottom third ≤ 87.0% Top third ≥ 98.8%				Bottom third ≤ 84.1% Top third ≥ 97.7%			
		Food and alcohol		All CE goods and services		Food and alcohol		All CE goods and services		Food and alcohol		All CE goods and services	
$EIP_{it}$		-0.014 (0.027)	0.048 (0.041)	0.189 (0.086)	0.061 (0.035)	0.131 (0.071)	0.265 (0.144)	0.026 (0.036)	-0.021 (0.038)	0.015 (0.080)			
$EIP_{it} \times \text{Middle third}$		0.031 (0.038)	0.090 (0.062)	0.158 (0.149)	-0.042 (0.049)	-0.131 (0.095)	-0.211 (0.230)	0.024 (0.039)	0.033 (0.050)	0.219 (0.122)			
$EIP_{it} \times \text{Least able third}$		0.090 (0.037)	0.109 (0.069)	0.239 (0.144)	-0.004 (0.053)	-0.024 (0.100)	-0.072 (0.216)	0.016 (0.039)	0.066 (0.048)	0.143 (0.106)			
<i>p</i> -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			
<i>Implied propensity to spend by group</i>													
Middle third		0.016 (0.026)	0.138 (0.047)	0.346 (0.121)	0.019 (0.034)	0.000 (0.063)	0.054 (0.179)	0.049 (0.015)	0.012 (0.033)	0.233 (0.093)			
Least able third		0.076 (0.026)	0.157 (0.055)	0.428 (0.116)	0.057 (0.040)	0.110 (0.070)	0.193 (0.160)	0.042 (0.016)	0.045 (0.029)	0.158 (0.070)			

Source: Authors' calculations.

Note: 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  $H_0: \beta_{it, \text{bottom third}} = 0$  and  $\beta_{it, \text{middle third}} = 0$ . For EIP1, all regressions have 3,470 observations. For EIP2, all regressions have 3,099 observations. For EIP3, all regressions have 3,463 observations.

Were the EIPs effective? The goal of previous tax rebate 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.<sup>55</sup> 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 household balance sheets over the past year, a period of strong demand and rising inflation.

**ACKNOWLEDGMENTS** 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 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 S. Johnson was at the University of Michigan.

55. Sahm (2021) debates these issues.

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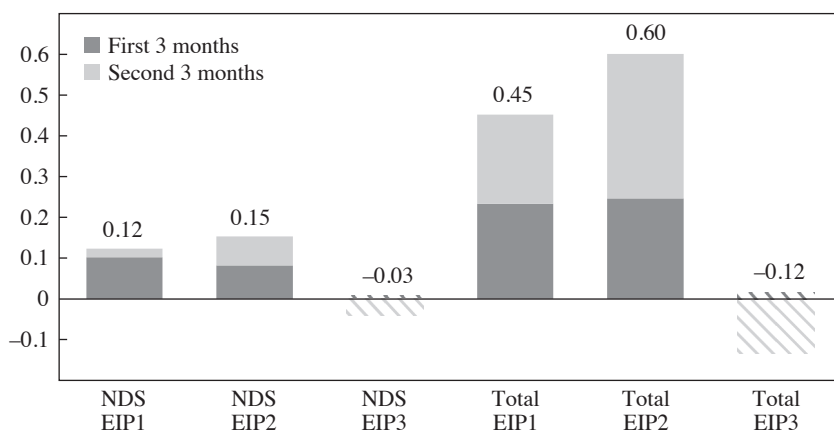
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## *Comments and Discussion*

### COMMENT BY

**KAREN DYNAN** The United States mounted a massive fiscal response to the onset of the COVID-19 pandemic in March 2020, with a key goal being to limit the economic fallout from an unprecedented shutdown of the economy. More fiscal support was passed over the subsequent year as the pandemic persisted and continued to disrupt economic activity. An important piece of this response was the three waves of direct payments to households, or Economic Impact Payments (EIPs), legislated in March 2020, December 2020, and March 2021. Economic research on how these payments affected households is crucial to designing policies that will be effective at fighting future recessions. Parker, Schild, Erhard, and Johnson provide a thoughtful analysis that contributes to an important emerging literature on this question.

This paper, like nearly all studies of the EIPs to date, explores the response of household spending to the EIPs. It stands out for its use of data from a survey of households, the Consumer Expenditure (CE) Survey, as opposed to the administrative financial records that have been the basis of most of the other studies. The paper thus represents an important complement to the rest of the literature. One advantage of the CE survey is that respondents are asked to report all types of expenditures, allowing for the construction of a very comprehensive measure of consumption, whereas studies based on credit and debit card records, for example, cannot tell you about types of spending for which cards are not typically used, such as motor vehicle purchases. The CE survey also has a more representative sample than many sources of financial records, and being able to look at households from all

**Figure 1.** Point Estimates of Cumulative Marginal Propensity to Consume

Source: Author's calculations, using data from table 4 on p. 115.

Note: Bars labeled "NDS" show estimates of the response of nondurables and services consumption to the first, second, and third rounds of EIPs; bars labeled "Total" show estimates of the response of total consumption to the first, second, and third rounds.

points in the income distribution is useful for understanding how to best target future payments. Finally, because the survey asks for a wide range of other types of information—such as income, employment, demographics, and wealth—it provides a richer set of potential covariates. This information also allows the authors to identify households in different wealth groups and with different work-from-home potential, which, in turn, facilitates their analysis of the degree to which the payments protected particularly vulnerable households from having to reduce their consumption as a result of economic disruptions from the pandemic.

My discussion will highlight three issues. The first is what to make of the key results in the paper and in what sense they represent “relatively low spending responses,” as the authors say in the introduction. Figure 1 summarizes the baseline results, showing the point estimates of the marginal propensity to consume (MPC) out of the different waves of EIPs after six months. For now, focus on the solid bars, which show the results for the first two waves; the results for the third wave are discussed below. The left set of bars show the estimated MPCs for expenditures on nondurable goods and services and the right set of bars show the estimated MPCs for total consumption. For the first two rounds of EIPs, the response of nondurables and services spending is indeed low, at 0.12 to 0.15. But the graph also shows that the response of total consumption to the EIPs is much higher.

For total consumption, the MPC ranges from 0.45 to 0.60. I do not find these estimated MPCs very far out of line with work on direct payments to households in earlier recessions or with the findings from the rest of the literature on the pandemic EIPs—particularly considering much of the latter literature focuses on lower-income households, who would generally be expected to have higher propensities to consume.

The comparison merits flagging because the results for the broader measure of consumption suggest that there was indeed a meaningful spending response to the EIPs over the first six months after they went out, at least for the first two waves of EIPs. More generally, it raises questions about what measure of consumption should be the basis for analysis in this context. Over the decades, a large share of studies of consumer spending at the household level have focused on nondurables and services because many theoretical predictions align best with this subset of consumption. But when evaluating EIPs (as well as other countercyclical support directed at households), there is a good case for focusing on broader consumption measures for gauging the degree of stimulus to the macroeconomy since what matters in that context is the full amount of the payment that goes back into the economy. It is also not clear that ignoring purchases of durable goods is the best way to assess the broader effects on household welfare.

As the authors note, the comparison does imply a shift to durable goods relative to the spending response to direct payments in earlier recessions. This finding is unsurprising given what we saw in aggregate consumption data. Aggregate consumer spending on services ran well below its earlier trend in 2020 and 2021, and aggregate consumer spending on durable goods ran well above its earlier trend. The finding is also what one would expect in a period when many people were limiting spending on high-contact services in order to avoid exposure to the virus and reconfiguring their homes to adapt to remote work.

This shift in consumption underscores the unsurprising point that pandemic-specific factors influenced the nature of the response of households to direct payments from the government in this particular episode. One might then ask whether such results are useful at all for informing the use of such payments as a countercyclical tool in future recessions. Given the energy that has already been put into this area of study (with more likely to come), I hope that these authors and others will give some thought as to how to shed light on what their results might look like in the absence of special pandemic factors. For example, something might be learned from comparing results for groups that might have been more or less likely to alter their consumption response because of the pandemic—such as old

people and young people or people living in blue states and red states. It also might be useful to compare results for categories of consumption that are likely to be more or less responsive to payments in pandemic times, following Cooper and Olivei (2021), who define categories of “socially distant sensitive spending.”

The second issue that I want to highlight is what I will call the “EIP3 mystery.” Returning to figure 1, consider the hatched bars, which show the point estimates for the MPC out of the third wave of EIPs after six months. In contrast to the results for the first two waves (shown by the solid bars), the point estimates for the MPC out of the third wave of EIPs suggest a diminished response for both spending on nondurable goods and services and total consumption spending. This pattern holds even for low-wealth families, as shown in table 7 of the paper. I find the result to be very surprising.

The first thing that we should ask is whether the EIP3 results are plausible given economic conditions at the time. One supporting narrative would be that Americans—across the income distribution—were just very comfortable financially by early 2021 and therefore not constrained in any way that would lead additional income to spur additional spending. It is correct that, despite enormous job loss in 2020, generous government support meant that many Americans’ incomes were as high or higher than they were before the pandemic. Between this support and the fact that the pandemic limited consumption opportunities, many did more saving than usual in 2020 (Aladangady and others 2022). However, in order to explain the much lower estimated MPC for EIP3 than for EIP1 and EIP2, one needs to make the case that Americans were in better financial shape in March 2021 than they were when the two earlier waves were disbursed. I do not see the available data being strongly supportive of that view. Incomes were not particularly robust at that point, with expanded unemployment benefits having ended six months earlier and job postings just beginning to pick up. Various measures of financial stress from the US Census Bureau’s Household Pulse Survey, such as the share of respondents reporting difficulty paying their expenses and the share reporting food insecurity, were basically in line with readings over much of the pandemic period. In early March 2021, the JPMorgan Chase Institute’s estimates of median checking account balances for the first and second quartiles of the income distribution were about at the average level seen over the pandemic to date (Greig and Deadman 2022).

I also do not see a strong case for Americans being less interested in spending when EIP3 was disbursed than at the time of the two earlier waves. Although households may have stocked up on many types of durable goods over the preceding year, pent-up demand for services was presumably high

and the ramping up of vaccination rates was making it much safer to act on that demand.

What about the direct evidence regarding EIP3? Although there is as yet little formal analysis (beyond this study), the informal evidence suggests a material spending response. Table 2 in the paper shows that when CE survey respondents were directly asked how they would use EIP3, more than half said they would use it mostly for expenses—a share that is only a bit lower than that for the earlier two waves. Similarly, Gelman and Stephens (2022) find that the share of Household Pulse Survey respondents reporting they spent out of EIP3 was as large or larger than for the earlier waves. Further, data on credit and debit card spending from the Opportunity Insights Economic Tracker point to a jump in spending after EIP3 was disbursed.<sup>1</sup>

The second question one might ask is what problems there might be with the data or methodology in the paper that could explain why the estimated MPC for EIP3 is so low. The authors flag that the share of CE survey respondents who report not receiving a third payment is much too large (although the degree of underreporting appears to be worse for EIP2) and, in the online appendix, they show some increase in the estimated MPC when they impute what appear to be missing payments. Other possible sources of bias include problems separating the impact of EIP2 from that of EIP3 when the two waves occurred within a couple of months of each other.

All in all, I think the authors are right to downplay their EIP3 results given the concerns about their accuracy. But I also find it dissatisfying that the results and arguments about their validity are so inconclusive and believe it is imperative that researchers dig more deeply to understand the effects of EIP3. The question is important because of the implications for future policy design as well as its relevance to the current debate over inflation. With regard to the latter, there is much speculation that EIP3 helped to spark the sharp increase in inflation that began in spring 2021 by fueling excessive consumer demand (Ngo 2022). But at face value this story does not seem very consistent with extremely low estimated MPC over the six months following the disbursement of EIP3. (Of course, this is not to say that an even greater lagged response could also be contributing to later inflationary pressures.)

Although outside the scope of this exercise, it would be interesting to see if the EIPs might have contributed to the rise in inflation through an effect on labor supply. In particular, did they help fund spells out of the labor

1. Opportunity Insights Economic Tracker, [tracktherecovery.org](https://tracktherecovery.org).

force, exacerbating the worker shortage? It does not seem like it would be too hard to explore this question with this data set or a different source.

The third issue I want to highlight is the stimulus role of direct payments to households versus their social insurance role. Macroeconomic textbooks tend to fixate on stimulus as the goal of fiscal measures put in place during recessions. When considering stimulus, the effectiveness of a government spending program is gauged by its multiplier (the amount by which it ultimately raises aggregate demand), which is higher when the MPC is larger. Thus, a finding of a small MPC is sometimes viewed as suggesting a policy was not all that effective.

But stimulus would be an odd primary goal during a pandemic. Encouraging people to spend when a material part of the economy is shut down could be inflationary, and moreover, any type of spending that leads people to get close to each other could foster further spread of the virus. Contrary to what some believe, the arguments against stimulus were largely recognized in the policy community when the spring 2020 COVID-19 relief fiscal packages were put together. (Some experts did make a stimulus-related case that preserving or augmenting spending power would lead to fewer layoffs because businesses would be more confident that demand would be strong once economic activity could safely resume [Blanchard 2020].)

A strength of this paper is that the authors are very clear that stimulus should not be seen as the primary goal of the EIPs. Rather, the authors emphasize the role of the EIPs as social insurance—specifically, in their capacity to prevent hardship associated with current or potential future job loss. And they argue that the higher EIP spending responses for households most likely to experience such hardship—those with low liquidity and low work-from-home potential—demonstrate that the EIPs (at least the first two waves) were successful as social insurance.

I view the social insurance goal as even more expansive than argued by these authors. Specifically, the payments not only had the capacity to prevent immediate hardship in the face of job loss but also were aimed at reducing the likelihood that economic fallout from the COVID-19 pandemic would leave lasting scars on household finances. Such scars were a major consequence of the Great Recession, which (together with the global financial crisis) resulted in deeply weakened household balance sheets for many years. For example, Dettling, Hsu, and Llanes (2018) show that in 2016 wealth for working-age families in the lower 60 percent of the income distribution was still more than 30 percent below the average in 2007. Weak household finances can impair individual household welfare and economic



mobility through a variety of channels (Dynan and Wozniak 2021). Also, as discussed by Portes (2020), recession-induced scarring of economic structures can result in slow recoveries and, possibly, permanently lower potential output at the macroeconomic level.

One implication is that research aimed at shedding light on the near-term spending response to EIPs and other countercyclical measures (particularly the response over just a few months) represents only a piece of what is needed to fully assess the benefits of this type of fiscal support for households. There are other important questions to which researchers have not devoted nearly as much attention. For example, were households who experienced unemployment able to maintain higher spending over the long run than those who lost jobs in earlier recessions? Did the EIPs allow households to repay debt or raise their savings on a lasting basis? Did they enable people to quit their jobs and find better ones? Did EIPs facilitate investments like homeownership, starting a business, or postsecondary education?

All in all, I land in the same place as the authors about the social insurance value of the EIPs—they likely provided important protection to economically vulnerable households. In addition, as the authors argue, it would appear that many households that received EIPs were not at particular risk of hardship, suggesting that the same degree of social insurance could have been achieved with less money if the payments were better targeted. In an economy suffering primarily from weak aggregate demand, as would be the case in many recessions, distributing EIPs on a broad basis might still make sense from a stimulus point of view. Finally, while more work needs to be done to assess the contribution that EIPs and other pandemic fiscal support might have made to the sharp rise in inflation since spring 2021, the experience cautions that stimulus measures should be used carefully. Fiscal policymakers need to consider the risk that production will not be able to ramp up as fast as aggregate demand. And monetary policymakers need to be ready to respond if inflation starts to surge.

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#### COMMENT BY

**MATTHEW ROGNLIE** The last two decades have witnessed a revolution in how macroeconomists model household savings and consumption. Gone is the representative agent, with its infinite horizon and low marginal propensity to consume (MPC). In its place, we now have households subject to incomplete markets and credit constraints, with shorter effective horizons and much higher MPCs. The macro consequences of this shift are profound: monetary policy works through different channels, and deficit-financed fiscal policy is vastly more powerful.

This revolution has been driven in part by an influential series of empirical papers documenting high MPCs out of unexpected income shocks. Chief among these are two papers studying the 2001 and 2008 stimulus payments in the United States: Johnson, Parker, and Souleles (2006) and Parker and others (2013).

The recent pandemic brought similar payments but at a vastly larger scale: as noted by the authors, the three Economic Impact Payments (EIPs) in 2020–2021 totaled about \$800 billion, whereas the 2008 program paid about \$120 billion in 2020 dollars (Parker and others 2013), and the 2001 program was smaller still (Johnson, Parker, and Soules 2006). In light of the first two papers' influence, it is only natural to pursue a similar study of the new, far larger payments, and I am delighted these authors—two of whom worked on the first two papers—have taken up the challenge.

And it *is* a challenge, because the key source of identification for previous studies—random variation in the timing of disbursement—is now virtually absent. Instead, the authors must rely on variation in the receipt and amount of EIPs, both of which are nonrandom and determined by variables like income and number of children. If these variables are correlated with fluctuations in consumption that happened for some other reason—quite conceivable in the volatile pandemic environment—then clean identification is in doubt.

The authors, of course, are aware of this challenge and rise to the occasion. Their major conclusion, which I think is quite credible, is that the short-term spending response to the 2020–2021 EIPs was smaller than for the stimulus payments in 2001 and 2008.

One notable aberration is that the authors find seemingly no effect for the third EIP: for broader consumption measures, none of the estimates are statistically significant, and the point estimate on the cumulative two-quarter effect on all Consumer Expenditure (CE) Survey goods and services (table 4) is actually negative. I suspect that this strange result stems from the fact that the effects of the second and third EIPs are not separately identified; the two EIPs happened in short succession and had broadly similar eligibility criteria and phaseout rules. Some of the effect of the third EIP, therefore, is likely being assigned to the second EIP instead, which has a rather high point estimate for the two-quarter overall MPC (0.601).

If we adjust for this issue, however, the paper's core message remains intact: MPCs out of the 2020–2021 payments, though still far too high to be consistent with a permanent income model, were lower than the corresponding MPCs in 2001 and 2008. In the remainder of this discussion, I will explore the macroeconomic implications of this finding. In particular, I ask: If MPCs out of these payments were lower in the first few quarters, does that mean the payments had a smaller effect on aggregate demand? Or was this effect merely delayed? If the latter, perhaps the payments contributed to the surge in excess demand and inflation experienced over the last year and a half.

To help answer these questions, I outline a simple theoretical framework for the dynamics of household consumption following a government transfer. This framework provides several general insights into fiscal transmission—for instance, that excess savings following a transfer dissipate more slowly than a partial equilibrium view would imply, leading to a more persistent output effect. I then perform an experiment where I temporarily decrease MPCs following the transfer, consistent with their apparent decline in the data, and show how this results in a delayed output effect from the transfer. Finally, I discuss two possible deficiencies in my framework: the lack of long-term savings, and the lack of inelastic asset markets. Accounting for the former might decrease the output effect of a transfer, but the latter works in the opposite direction, introducing a new and potentially powerful channel of transmission to aggregate demand.

**THEORETICAL FRAMEWORK** I now sketch a simple framework for the propagation of fiscal transfers in a population featuring limited heterogeneity, with different household types  $i = 1, \dots, N$ . This is a discrete-time version of the continuous-time framework in Auclert, Rognlie, and Straub (2023), which has many of the same results, along with some extensions. All variables are in level deviations from steady state.

Assume that if household  $i$ 's cash on hand in period  $t$ —including both assets from the previous period and income this period—increases by  $x_t$ , then the household will consume an additional  $mpc_i x_t$ , where  $mpc_i \in [0, 1]$  is some type-specific constant.<sup>1</sup> Households are myopic and do not anticipate that future income or taxes will deviate from steady state. The steady-state real interest rate is  $r = 0$ , and the central bank sets its policy rate to maintain  $r_t = r = 0$  in all periods, neither stimulating nor contracting demand.<sup>2</sup> Nominal wages are rigid, production is linear in labor, and at the margin households are forced to supply extra labor hours to fulfill any increase in demand. As a result, if total goods demand increases from steady state by  $y_t$ , the income of each household  $i$  increases by  $\theta_i y_t$ , for some  $\theta_i > 0$  satisfying  $\sum_{i=1}^N \theta_i = 1$ .

1. This can be microfounded as the first-order solution to a model with concave utility in assets; see Auclert, Rognlie, and Straub (2023).

2. These assumptions facilitate a pen-and-paper solution of the model. As Auclert, Rognlie, and Straub (2023) show, relaxing them—by introducing rational expectations of income or monetary policy that raises real interest rates in a boom—tends to shrink and shorten the demand effects of a transfer. On the other hand, monetary policy that cuts real interest rates in a boom—for instance, because it is at the zero lower bound and inflation rises—amplifies the demand effects.

Assume further that household type  $N$  is Ricardian with  $mpc_N = 0$ , which is the MPC consistent with a permanent-income household on its Euler equation in the limit  $r \rightarrow 0$  and  $\beta \rightarrow 1$ . When this household receives additional income, it saves that income forever. All other households, in contrast, are assumed to be non-Ricardian, with  $mpc_i > 0$ . The Ricardian household can be interpreted either as a wealthy infinite-horizon household or as a proxy for other recipients of marginal spending that are unlikely to spend domestically out of their receipts, such as the government or foreigners.

Finally, coming into period 0, assume that the government makes type-specific transfers (EIPs), which effectively increase the initial asset positions  $a_{i,-1}$  of each household type. It rolls over the increased debt from these transfers forever at the real interest rate  $r = 0$ .

The evolution of this economy away from steady state is summarized by the equations

$$(1) \quad y_t = \sum_i mpc_i (a_{i,t-1} + \theta_i y_t) \text{ and}$$

$$(2) \quad a_{i,t} = (1 - mpc_i)(a_{i,t-1} + \theta_i y_t),$$

where, again, both  $y_t$  and  $a_{i,t}$  denote deviations from steady state in levels. The increase in cash on hand—assets and income—for household type  $i$  is  $a_{i,t-1} + \theta_i y_t$ , of which the household consumes  $mpc_i$ . Summing these increments to goods demand across all  $i$  gives output  $y_t$  in equation (1). Equation (2) then gives the evolution of assets: at the end of period  $t$ , household type  $i$  saves the unconsumed portion of cash on hand as assets  $a_{i,t}$ .

There are several ways to solve for equilibrium in this model. First, we can solve equation (1) for each  $t$  sequentially, obtaining

$$(3) \quad y_t = (1 - mpc)^{-1} \sum_i mpc_i a_{i,t-1},$$

where we define  $mpc \equiv \sum_i \theta_i mpc_i$  to be the average MPC out of marginal income and then plug  $y_t$  into equation (2) to obtain assets for the next period. This is a period-by-period Keynesian multiplier, where the impulse  $\sum_i mpc_i a_{i,t-1}$  to spending is amplified by  $(1 - mpc)^{-1}$ .

Alternatively, we can take  $a_{i,-1}$  and the sequence  $\{y_t\}$  to be exogenous, iterate on equation (2) to obtain the implied sequence of assets, and then calculate the implied sequence of consumption  $c_{i,t} = mpc_i (a_{i,t-1} + \theta_i y_t)$ . If there is a shock to income  $\theta_i y_s$  at date  $s$ , then coming into date  $t$ , a fraction

$(1 - mpc_i)^{t-s}$  of that income will remain, of which  $mpc_i$  will be spent at date  $t$ . The matrix  $\mathbf{M}_i$  that maps sequences of income  $\{\theta_i y_s\}$  to consumption  $\{c_{it}\}$  therefore has entries  $M_{its} = mpc_i(1 - mpc_i)^{t-s}$  for  $t \geq s$  and  $M_{its} = 0$  for  $t < s$ . Aggregating across all households  $i$ , the matrix mapping  $\{y_s\}$  to  $\{c_t\}$  is then  $\mathbf{M} \equiv \sum_i \theta_i \mathbf{M}_i$ . This is the matrix of intertemporal MPCs introduced by Auclert, Rognlie, and Straub (2018).

Defining  $c_{it}^{PE} = M_{i,t0} a_{i,-1}$  to be household  $i$ 's partial equilibrium consumption response to the fiscal shock—the path of consumption ignoring any changes in aggregate  $\{y_t\}$ —and aggregating to  $c_{it}^{PE} = \sum_i c_{it}^{PE}$ , equilibrium output is characterized by an intertemporal Keynesian cross

$$(4) \quad \mathbf{y} = \mathbf{M}\mathbf{y} + \mathbf{c}^{PE},$$

where  $\mathbf{y}$  and  $\mathbf{c}^{PE}$  are vectors stacking the sequences  $\{y_t\}$  and  $\{c_t^{PE}\}$ . In this case, it turns out that the solution to equation (4) is given by

$$(5) \quad \mathbf{y} = (\mathbf{I} + \mathbf{M} + \mathbf{M}^2 + \dots)\mathbf{c}^{PE},$$

where  $\mathbf{I}$  is the identity matrix. This is a direct intertemporal generalization of the traditional Keynesian multiplier process, where  $1/(1 - mpc)$  is written  $1 + mpc + mpc^2 + \dots$ .

Partial sums in equation (5) can be interpreted as rounds of general equilibrium adjustment. The sequence  $\mathbf{c}^{PE}$  alone is partial equilibrium spending;  $(\mathbf{I} + \mathbf{M})\mathbf{c}^{PE}$  takes into account that this spending creates additional income, which is spent;  $(\mathbf{I} + \mathbf{M} + \mathbf{M}^2)\mathbf{c}^{PE}$  takes into account the income created by that spending; and so on. After infinitely many rounds, this process converges to the general equilibrium  $\mathbf{y}$ .<sup>3</sup>

*Results about equilibrium.* We can quickly derive several features of equilibrium, summarized as:

- Result 1: in the long run, the Ricardian household owns all the additional assets.
- Result 2: general equilibrium output  $\mathbf{y}$  is greater than partial equilibrium spending  $\mathbf{c}^{PE}$ , and in the long run  $y_t$  decays at a slower rate than  $c_t^{PE}$ .

3. For the general case covered in Auclert, Rognlie, and Straub (2018), this iterative process does not necessarily converge to a finite time path. Here, however, convergence is easy to prove, because the existence of Ricardian households  $\theta_N > 0$  implies that the  $l^1$  norm of  $\mathbf{M}$  is strictly less than one.

- Result 3: the cumulative output effect of the transfer is given by the simple formula:

$$(6) \quad \sum_{t=0}^{\infty} y_t = \theta_N^{-1} \sum_{i=1}^{N-1} a_{t,i-1}.$$

How do we derive these results? Result 1 follows from equation (3), which implies that  $y_t$  is bounded from below by  $(1 - mpc)^{-1} (\min_{i < N} mpc_i) \sum_{i < N} a_{t,i-1}$ . Hence, given total non-Ricardian assets  $\sum_{i < N} a_{t,i-1}$  coming into period  $t$ ,  $y_t$  will be a strictly positive multiple of that, and a share  $\theta_R y_t$  will be received by the Ricardian household and immobilized. Over time, this implies an exponential decline in total non-Ricardian assets, which trickle up (Auclert, Rognlie, and Straub 2023) to the zero-MPC Ricardian household. This is in line with empirical evidence showing that poorer households deplete their transfers more quickly than wealthy ones.

The first part of result 2, that  $\mathbf{y}$  is larger than  $\mathbf{c}^{PE}$ , follows directly from equation (5). To understand the second part, note that if all households receive transfers coming into date 0, then  $c_t^{PE}$  asymptotically decays at a rate of  $1 - \min_{i < N} mpc_i$ , corresponding to the non-Ricardian household with the lowest MPC. But in general equilibrium, this household will receive back the income from some of its own spending, and its assets will not decay as quickly.<sup>4</sup> This leads to a more persistent output effect.

Finally, result 3 comes from the fact that all assets transferred to non-Ricardian households must eventually end up in the hands of the Ricardian household. In general equilibrium, this happens via increases in output, but only a fraction  $\theta_N$  of increased output is earned by the Ricardian household, and hence cumulatively, output needs to increase by  $\theta_N^{-1}$  times the extra assets held by non-Ricardian households.<sup>5</sup> Remarkably, equation (6) makes

4. Formally, we can condense equations (2)–(3) to get a law of motion  $\mathbf{a}_t = (\mathbf{I} - \text{diag}(\mathbf{mpc})) (\mathbf{I} + (1 - mpc)^{-1} \theta \mathbf{mpc}') \mathbf{a}_{t-1}$ , where we stack non-Ricardian households  $i = 1, \dots, N - 1$  in bolded vectors. Perron-Frobenius implies that the matrix mapping  $\mathbf{a}_{t-1}$  to  $\mathbf{a}_t$  has a unique leading positive eigenvalue  $\lambda$  with positive eigenvector  $\mathbf{v}$ , which governs asymptotic decay.

We can write the equation for this eigenvector as  $(\lambda - (1 - mpc_i)) v_i = \frac{1 - mpc_i}{1 - mpc} \theta_i \sum_j mpc_j v_j$ , and from positivity of  $\mathbf{v}$  it follows that  $\lambda \geq 1 - mpc_i$  for all  $i$ , and indeed that strictly  $\lambda > 1 - mpc_i$  if there is any non-Ricardian agent with  $mpc_i < 1$ .

5. Another interpretation is provided by the formula in equation (5). Multiplying a sequence by the row vector of all ones,  $\mathbf{1}'$ , takes its sum. One can show that  $\mathbf{1}'\mathbf{M}$  equals  $(1 - \theta_N)\mathbf{1}'$ , since the entire income share  $1 - \theta_N$  received by non-Ricardian households is eventually spent. Multiplying equation (5) on the left by  $\mathbf{1}'$ , it becomes  $\mathbf{1}'\mathbf{y} = (1 + (1 - \theta_N) + (1 - \theta_N)^2 + \dots)\mathbf{1}'\mathbf{c}^{PE} = \theta_N^{-1}\mathbf{1}'\mathbf{c}^{PE}$ . It is easy to show that  $\mathbf{1}'\mathbf{c}^{PE} = \sum_{i=1}^{N-1} a_{t,i-1}$ , since the cumulative partial equilibrium increase in consumption equals the initial excess assets.

no reference to the MPCs of the non-Ricardian agents: all that matters for the cumulative output effect is that these MPCs are positive, so that any cash received is eventually spent.<sup>6</sup>

#### APPLYING THE FRAMEWORK

*Calibration.* Now that the theoretical framework has been established, I will discuss quantification. I consider a case where there are only three household types. First, type 1 is hand-to-mouth, with  $mpc_1 = 1$ . Second, type 2 has an intermediate  $mpc_2 = 0.2$ , and I call it a target household since it reverts to its steady state asset target at a rate of 0.2 per quarter. Finally, type 3 is Ricardian, with  $mpc_3 = 0$ .

My main calibration will feature all three of these types, with  $\theta_1 = 0.1$ ,  $\theta_2 = 0.4$ , and  $\theta_3 = 0.5$ . In line with the broader interpretation discussed above, the high Ricardian share is intended to capture marginal recipients of aggregate spending that likely have a low or zero MPC: the government (through taxes), foreigners, some business profits, and a small fraction of labor earnings. If aggregate income increases at date  $t$ , these assumptions on  $\theta_i$  imply an aggregate MPC in the first year, quarters  $t$  through  $t + 3$ , of 0.34, and an aggregate MPC in the second year, quarters  $t + 4$  through  $t + 7$ , of 0.10.

Assuming that only 0.1 out of the  $\theta_3 = 0.5$  is earned by labor, we can normalize these intertemporal MPCs by total labor earnings 0.6, obtaining a first-year MPC of 0.56 and a second-year MPC of 0.16. Importantly, these are very close to the first two annual intertemporal MPCs, weighted by labor earnings, reported by Auclert, Rognlie, and Straub (2018) using data from Fagereng, Holm, and Natvik (2021).

Finally, I assume that the transfer is relatively progressive: from the unit transfer, the non-Ricardian households receive a higher share  $a_{i-1}$  than their ordinary share of marginal income  $\theta_i$ . In particular,  $a_{1-1} = 0.2$  and  $a_{2-1} = 0.6$ .

Beyond the main calibration described so far, to better understand mechanisms I will also consider two related calibrations, both of which have only one non-Ricardian household: an “only hand-to-mouth” calibration where  $\theta_1 = 0.5$ ,  $a_{1-1} = 0.8$ , and  $\theta_2 = a_{2-1} = 0$ ; and an “only target” calibration where  $\theta_2 = 0.5$ ,  $a_{2-1} = 0.8$ , and  $\theta_1 = a_{1-1} = 0$ . Note that in all these cases, since the allocation of both the transfer and marginal income between non-Ricardian and Ricardian households is the same, the cumulative output effect implied by equation (6) is identical.

6. Importantly, however, this result is sensitive to the assumption that the central bank holds the real rate  $r_t$  fixed. A rise in  $r_t$  provides another mechanism for moving assets from the non-Ricardian households to the Ricardian household, since the latter will generally increase net savings by more in response.



*Results.* The three panels of figure 1 show the general equilibrium path of output  $y$  in the hand-to-mouth, target, and main calibrations. They also show the rounds of adjustment in equation (5) that converge to  $y$ : the partial equilibrium round  $0 \mathbf{e}^{PE}$ , round 1  $(\mathbf{I} + \mathbf{M})\mathbf{e}^{PE}$ , and round 2  $(\mathbf{I} + \mathbf{M} + \mathbf{M}^2)\mathbf{e}^{PE}$ . Output  $y$  itself can be viewed as round  $\infty$ , since it is the sum  $(\mathbf{I} + \mathbf{M} + \mathbf{M}^2 + \dots)\mathbf{e}^{PE}$ .

Despite identical cumulative output effects (result 3), the three calibrations are strikingly different, with impact multipliers varying by a factor of nine. In the hand-to-mouth calibration, the entire output response happens at  $t = 0$ , as hand-to-mouth households immediately spend both the transfer and the income from the resulting boom, and the excess assets immediately pass to the Ricardian household. In the target calibration, we see the opposite: households slowly draw down their assets, as their increased spending is partly offset by the general equilibrium increase in income, so that assets and spending are more persistent in general than partial equilibrium (result 2). Only a small fraction (about one-ninth) of the cumulative output effect happens on impact.

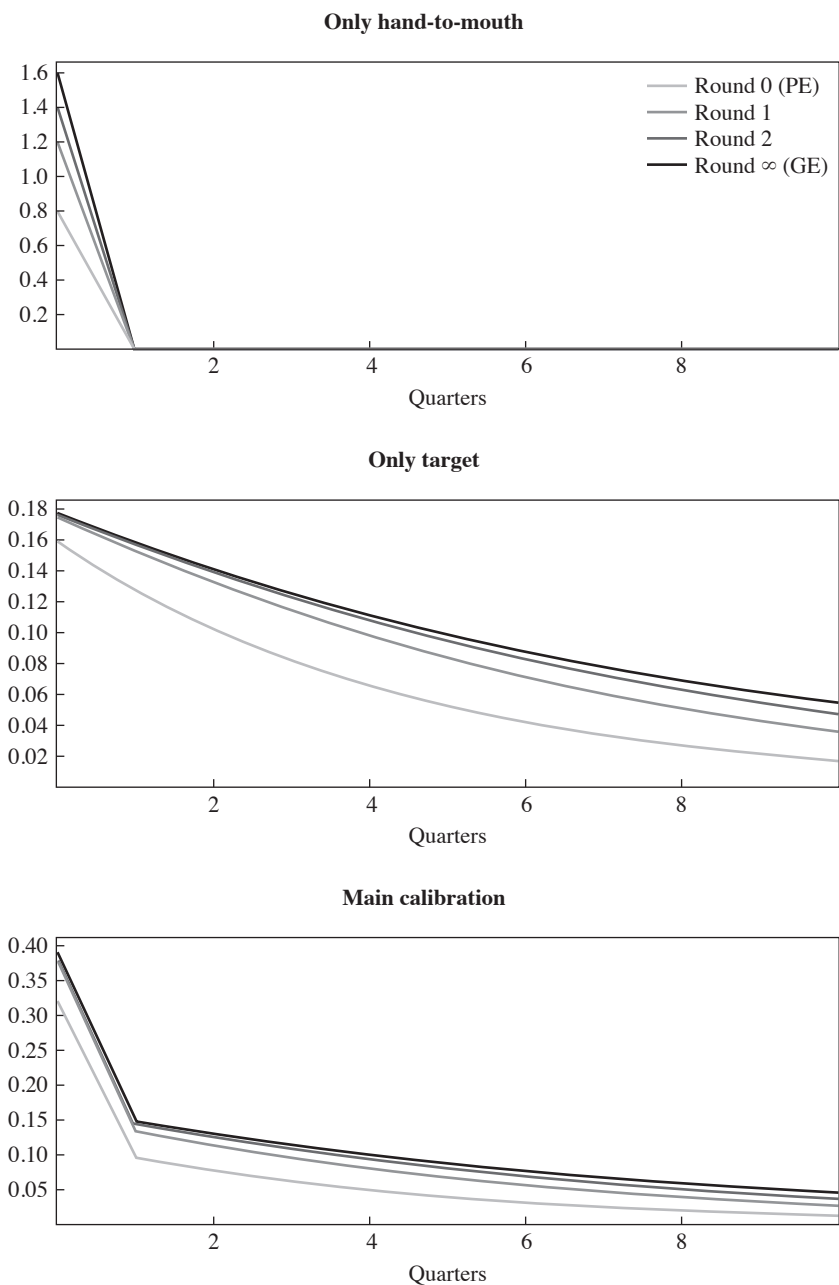
The main calibration, blending hand-to-mouth and target households, is intermediate between these two cases. Thanks to the hand-to-mouth households, there is a spike in output in the quarter of the transfer. But this is still less than one-fourth of the cumulative output effect, which has much higher persistence in general than in partial equilibrium.

The first two panels of figure 2 show the evolution of assets for the main calibration, in both partial and general equilibrium.<sup>7</sup> In the partial equilibrium case, the hand-to-mouth households immediately deplete their assets, and the target households do so at a steady pace, with the vast majority gone after ten quarters. The Ricardian households simply hold on to their initial receipts. In general equilibrium, the hand-to-mouth households still immediately deplete their assets, but the target households do so more slowly, with almost two-thirds of their initial assets remaining after four quarters, and one-third remaining after ten quarters. Total assets remain constant, as assets drawn down by others trickle up to the Ricardian households (result 1).

*Experiment: temporarily lower MPCs.* As discussed earlier, the evidence from Parker, Schild, Erhard, and Johnson suggests that MPCs out of fiscal transfers may have fallen during the pandemic. This could be due to pandemic-specific circumstances (limited opportunities to spend), nonlinearities in the consumption function (with high liquidity from transfers

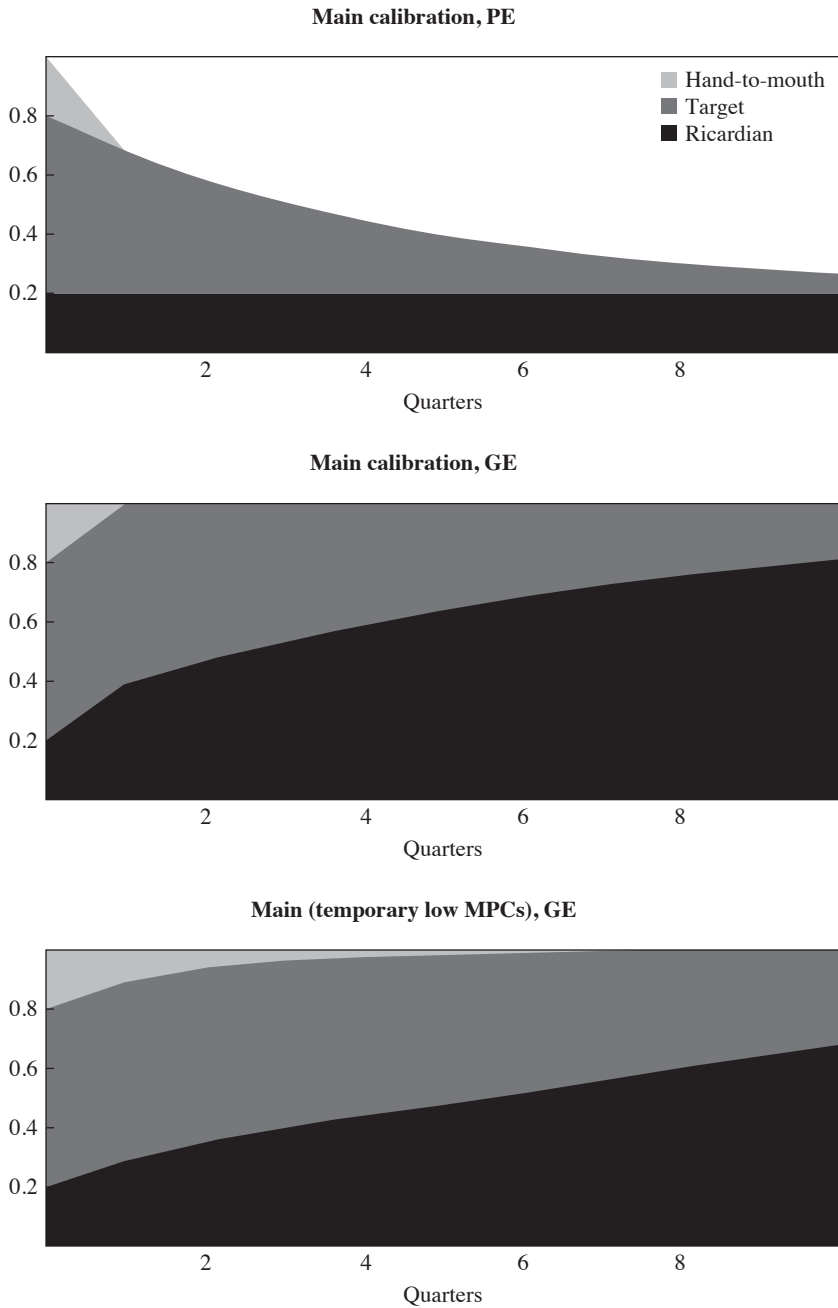
7. At each  $t$ , we plot beginning-of-period assets  $a_{i,t-1}$  rather than end-of-period assets  $a_{i,t}$ , so that the initial transfer is visible at  $t = 0$ .

**Figure 1.** Output Response to Transfer by Household Calibration and General Equilibrium Rounds



Source: Author's calculations.

**Figure 2.** Distribution of Assets across Household Types



Source: Author's calculations.

temporarily depressing MPCs), or both. In either case, it seems unlikely that the decline in MPCs is permanent.

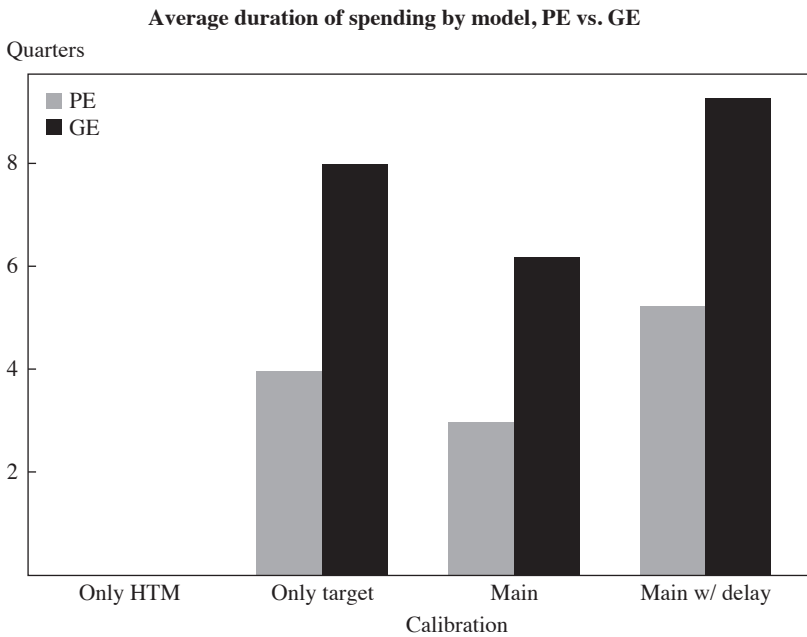
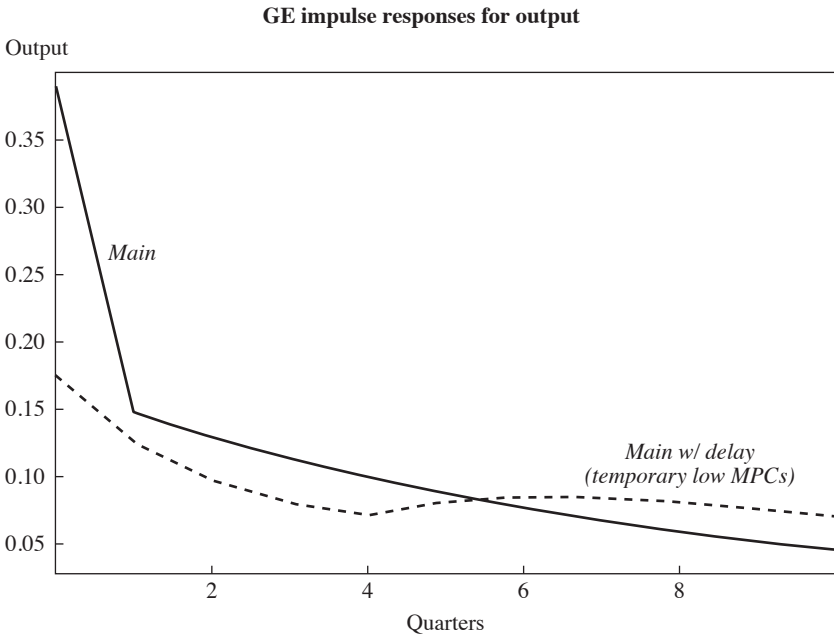
In this experiment, I take a reduced-form approach to think about the effects of declining MPCs. I alter the framework from above by assuming that the MPCs out of excess cash on hand temporarily fall for both hand-to-mouth and target households to half their usual levels,  $mpc_{1t} = 0.5$  and  $mpc_{2t} = 0.1$  for  $t = 0, \dots, 4$ . I assume that these MPCs then converge back to their original levels at a rate of 25 percent per quarter; for example, that  $mpc_{1t} = 1 - 0.5 \cdot (0.75)^{t-4}$  for  $t \geq 4$ . The main calibration is otherwise left unchanged.

The top panel of figure 3 shows the resulting path of output for this delayed spending variant of the model (dashed line), contrasting with the original results (solid line). The impact effect on output, although substantial, is less than half as large, and the path of output is non-monotonic, increasing slightly with the recovery in MPCs after four quarters. Crucially, the cumulative output effect remains the same in both cases (result 3), so that the model with temporarily low MPCs actually has a higher output effect after six quarters, with the gap becoming substantial after eight—making up for the smaller impact effect. The bottom panel of figure 2 shows the corresponding evolution of assets: due to the temporary decline in MPCs, less trickling up of assets takes place than in the original calibration, so that more assets remain with hand-to-mouth and target households, ready to be spent.

Finally, the bottom panel of figure 3 shows the duration of the output increase (or, in partial equilibrium, the increase in household spending) by calibration: the average date at which the increase in output or spending takes place. Across the board, duration is higher in general than partial equilibrium, in line with result 2. Among the original calibrations, it is highest with only target households, and lowest (zero) with only hand-to-mouth households, with the main calibration being in the middle. But the temporary fall in MPCs pushes up duration substantially, to the point where it exceeds every original calibration. Importantly, in all these cases, cumulative output is the same: higher duration simply means that the same overall increase in output is pushed toward later dates.

I suspect that the events of the last few years resemble the delayed-spending case. Although a vast fiscal intervention pushed household liquidity to unprecedented levels, the demand-side effects—though substantial—were not as large as we would normally expect, because MPCs were lower than usual during the pandemic. But since households still had these excess savings on their balance sheets, this merely set us up for a more prolonged

**Figure 3. Impulse Responses and Duration across Different Scenarios**



Source: Author's calculations.

boom in demand—an inflationary boom that, as of the end of 2022, has not yet receded.

**A LINGERING QUESTION FOR FUTURE WORK: THE ROLE OF ASSET MARKETS** The framework I have outlined, although useful, relies on one precarious assumption: that whatever portion of a transfer is not consumed by household  $i$  today is still subject to the same marginal propensity to consume,  $mpc_i$ , in the next period. One can imagine the opposite assumption: that whenever a household receives income, it either consumes that income immediately, or it places the income into long-term savings, out of which the MPC is very low.

In its extreme form, this alternative assumption seems inconsistent with the evidence on intertemporal MPCs highlighted by Auclert, Rognlie, and Straub (2018), which shows that elevated consumption persists for several years following an income shock. (Indeed, I tried to match this evidence in my calibration here.) But that same evidence does allow for some diversion to long-term savings. Indeed, Fagereng, Holm, and Natvik (2021) find that five years after an unexpected income shock, about 10 percent of the income remains unconsumed, and much of this is held in investments like stocks, bonds, and mutual funds.

What if the counterpart of lower MPCs during the pandemic was a much higher allocation to long-term savings? If so, my analysis above would be wrong: it assumes that non-Ricardian households eventually return to their typical high MPCs out of excess assets. If these assets were instead moved to some form of sticky long-term savings, that might never have happened—and the pandemic’s low MPCs might have truly dampened the demand effect from transfers, rather than merely delaying it.

But this raises another question: What vehicles were households saving in, and might those have demand effects in their own right? In a simple model where different assets are highly (perhaps perfectly) substitutable, the answer is no: the high substitutability across assets means that the exact choice of where to save is fungible, and in equilibrium it matters little for aggregate outcomes whether a given household invests in stocks, bonds, or deposits. If, however, we assume inelastic markets, in the spirit of Gabaix and Koijen (2021), this changes. Investing in stocks will push up stock prices, potentially leading other households to increase their consumption due to wealth effects, and also to higher corporate investment spending. Investing in real estate will push up real estate prices, allowing existing owners to lever up and increasing both consumption and construction spending. Even a transfer that is saved, if it is saved in the right places, can push up aggregate demand.

At least superficially, this story seems to fit the pandemic experience: as households flush with cash moved into the stock market and real estate—a process already documented in some papers—prices in both markets surged from late 2020 through 2021. This surge in prices likely contributed to aggregate demand and inflation.

Together with Adrien Auclert, Ludwig Straub, and Lingxuan Wu, in ongoing research I am building a theoretical framework to understand this interaction between inelastic markets and aggregate demand. But a great deal of empirical work is also needed. Perhaps the successors to this paper can document not only the marginal propensity to consume, but also the marginal propensity to save in each kind of asset.

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**GENERAL DISCUSSION** Jason Furman remarked that in nominal terms, personal consumption expenditures were more than 10 percent higher in 2021 than in 2019, a sum of \$1.5 trillion.<sup>1</sup> This, he believed, was a shocking amount of personal consumption expenditure given the pandemic-induced constraints on services and unemployment levels. He contemplated whether the spike in personal consumption would have happened absent the nearly

1. FRED, “Personal Consumption Expenditures,” <https://fred.stlouisfed.org/series/PCE#0>.

\$5 trillion of interventions—maybe, for example, the marginal propensity to consume (MPC) presented in this paper was somehow delayed.<sup>2</sup> He thought it was difficult to explain the elevated level of personal consumption expenditures without some meaningful multiplier on some part of that \$5 trillion.

Steven Braun commented that it is still to be seen whether excess savers from 2020 and 2021 will spend their money in 2022. Robert Gordon agreed with Braun, noting that there was a large amount of excess savings. He provided three pieces of evidence on this point. First, he noted that there was a striking upward jump in personal disposable income and savings at the time of the transfers. Second, he stated that excess savings have risen considerably. Compared to the 2019 rate of 7.6 percent of personal disposable income, the value of personal savings increased to \$2.4 trillion in mid-2021.<sup>3</sup> Based on Matthew Rognlie’s analysis, he interpreted these data as indicating that savings have been gradually shifting from the short-run adjustment agents to the long-run adjustment agents. Third, bank balances have increased by \$4.7 trillion (although, he noted, they are not growing as quickly as they did during the period of transfers).<sup>4</sup> He concluded his point by stating that once this liquidity is created, it doesn’t go away. It’s just shifting from all the people who got the stimulus to the people who saved. William Gale asked if there were data on whether recipients gave money from their Economic Impact Payments (EIPs) to family members, since the options recipients had were to save the money, spend it, or give it away.

Wendy Edelberg also discussed reconciling macroeconomic savings data with the MPCs found in the paper. She agreed with the general principle that if a stimulus payment doesn’t show up in consumer spending, it must flow into some type of saving: either paying off debt, deposited in a checking account, or used to buy assets. However, she claimed, the data do not match this prediction. She noted that while there was a big inflow into deposits in the first quarter of 2021, this inflow subsequently stopped. At the same time, there were increases in consumer debt in 2021. Furthermore, a lot of excess savings observed in 2021 came from higher-income people reducing their spending, rather than from lower-income people reducing their spending from income, which at this point also included the EIPs. The MPCs presented in the paper would suggest that a lot of money went

2. Christina D. Romer, “The Fiscal Policy Response to the Pandemic,” *Brookings Papers on Economic Activity* (Spring 2021): 89–109.

3. Bureau of Economic Analysis, “National Income and Product Accounts,” table 2.6, <https://apps.bea.gov/iTable/?reqid=19&step=2&isuri=1&categories=survey>.

4. FRED, “Deposits, All Commercial Banks,” <https://fred.stlouisfed.org/graph/?g=U00X>.



into savings, but Edelberg did not see evidence of this phenomenon in the saving data.

Louise Sheiner was surprised that those reliant on income from jobs with tasks that could not be performed at home had higher MPCs than those with jobs that could be worked remotely. Since a significant fraction of these individuals were unemployed, many were receiving substantial pandemic unemployment insurance payments.

Austan Goolsbee noted that if MPCs are this low, then the immediate impact of the American Rescue Plan should not have been that inflationary because people were actually saving the money. He asked why, then, is there currently so much inflation?

Pierre-Olivier Gourinchas answered by looking at the transfer multiplier, that is, how much aggregate output \$1 of fiscal transfer to households causes. He noted that there is a body of work that shows that in a situation characterized by supply constraints and with the assumption of regular MPCs, the transfer multiplier is expected to be very low. Gourinchas reported that the pandemic economy had upward of 70 percent of sectors with supply constraints at one point. His recent research with coauthors found low transfer multipliers—on the order of six cents on the dollar—even with reasonable estimates of the MPC.<sup>5</sup> He concluded that if nominal spending went into supply-constrained sectors, then it was not contributing to real economic activity; instead, it was contributing to inflation.

On this topic, Deborah Lucas expressed her belief that the MPC on the EIPs would have turned out even lower if one also took into account forbearance on student loans and interest payments. The magnitude of the loans especially, she noted, were of similar size to the EIPs.

Claudia Sahm noted that the paper under discussion has a different statistical methodology from the studies done in 2001 and 2008 by Jonathan Parker, David Johnson, and colleagues.<sup>6</sup> That methodology was considered a gold standard because of its quasi-random timing of check disbursement based on Social Security numbers. The current study did not have that same

5. Pierre-Olivier Gourinchas, Şebnem Kalemli-Özcan, Veronika Penciacikova, and Nick Sander, “Fiscal Policy in the Age of COVID: Does It ‘Get in All of the Cracks’?,” in *Economic Policy Symposium Proceedings: Macroeconomic Policy in an Uneven Economy* (Jackson Hole, Wyo.: Federal Reserve Bank of Kansas City, 2021).

6. David S. Johnson, Jonathan A. Parker, and Nicholas S. Souleles, “Household Expenditure and the Income Tax Rebates of 2001,” *American Economic Review* 96, no. 5 (2006): 1589–610; Jonathan A. Parker, Nicholas S. Souleles, David S. Johnson, and Robert McClelland, “Consumer Spending and the Economic Stimulus Payments of 2008,” *American Economic Review* 103, no. 6 (2013): 2530–53.

element of exogeneity. Sahm pointed to her study with Matthew Shapiro and Joel Slemrod on the Michigan Survey, which does have consistent identification across groups, where they asked questions about the CARES checks.<sup>7</sup> They found monthly spending percentages roughly similar to previous work. Despite certain pandemic-related factors that may have an impact on spending percentages, such as the tendency for people to spend less on vacations and more on food, she said that the spending percentages tend to have time consistency, and that this paper is not consistent with other papers.

Gourinchas discussed why the MPCs presented in the paper may have been lower than expected. If the EIPs functioned as perfect insurance, then the consumption change would be equivalent between those receiving insurance and those not receiving insurance, therefore creating a zero coefficient on the stimulus. If the evidence points to coefficients that are lower than in normal times, it might actually indicate that payments are going in the right direction.

On the point of insurance, Sahm did not believe that MPCs and the speed of spending alone are good measures of insurance. These measurements should be considered alongside many observations to conclude whether the EIP program was effective.

Arvind Krishnamurthy introduced some additional information about the insurance value of the EIPs, noting that household balance sheets and the financial sector did not show evidence of scarring, unlike in 2008. In spring 2020, the prices of securities that were linked to consumer defaults—such as credit card asset-backed securities and loan asset-backed securities—plummeted very quickly. However, these asset prices and their spreads soon after returned to normal levels, which Krishnamurthy read as a sign that households were continuing to service their debts, unlike in 2008.<sup>8</sup>

Caroline Hoxby put forth the idea that the EIP targeting was inexcusably bad, especially for the second and third rounds, since the administrative emergency that was active during the first round of EIP had waned. She thought that while the payments had not done a good job at mitigating losses in the consumption of nondurables, they may have encouraged purchases of nondurables such as the technology, furniture, and other items that allow a person to set up a home office. In this sense, although the

7. Claudia Sahm, Matthew Shapiro, and Joel Slemrod, “Consumer Response to the Coronavirus Stimulus Programs,” slides, November 11, 2020, <https://drive.google.com/file/d/1zkMXfn4SQMW1mIWTfuEXM-ZXA6Nse0jR/view>.

8. Markus Brunnermeier and Arvind Krishnamurthy, “Corporate Debt Overhang and Credit Policy,” *Brookings Papers on Economic Activity* (Summer 2020): 447–88.

EIPs may not have worked as intended, the payments may have ensured higher productivity during the pandemic by encouraging consumption of durable goods that improved productivity (for example, home office setup). She believed that the different types of purchases should be identifiable in the data. For instance, durables such as washing machines and cars may not have increased productivity, but electronics, computers, and furniture likely had some effect. This difference could also be isolated by stratifying data between remote and in-person workers.

Gordon agreed with this idea: his research shows that all of the productivity gains since the start of the pandemic were concentrated in the 35 percent of the economy where people primarily work from home. The remaining 65 percent of the economy has negative and zero productivity gains.<sup>9</sup>

Justin Wolfers contended that what appeared to be durables consumption may have followed a pattern more similar to regular consumption of nondurables—as the pandemic began to recede, people’s durable purchases (such as desks and treadmills) may have no longer been used. Durables are traditionally thought as goods that yield an ongoing flow of services for many years, but many of the products bought during the pandemic have fallen out of use.

Hoxby also agreed with Rognlie’s point that it is possible that the third round of EIP was conflated with the second round. Given their closeness in time, she does not believe there is a good way to separately identify them, as a matter of econometrics.

Sheiner noted that one of the benefits of the EIPs was the fact that they went out quickly, providing temporary liquidity to recipients who were unable to immediately benefit from unemployment insurance.

Adjacent to the insurance topic, Wolfers brought up the proposition that the EIPs may have not only served as insurance but also had income effects on people’s labor–leisure decisions, allowing people to stay at home. He thought that evaluating the effect of EIPs on labor supply was a feasible related line of research.

Ayşegül Şahin built off Wolfers’s comment on the labor–leisure choice, adding in some reasons for optimism about the EIPs. Şahin noted that Americans’ desires to work are declining.<sup>10</sup> At the same time, fewer Americans

9. Robert J. Gordon and Hassan Sayed, “A New Interpretation of Productivity Growth Dynamics in the Pre-pandemic and Pandemic Era U.S. Economy, 1950–2022,” working paper 30267 (Cambridge, Mass.: National Bureau of Economic Research, 2022).

10. R. Jason Faberman, Andreas I. Mueller, and Ayşegül Şahin, “Has the Willingness to Work Fallen during the Covid Pandemic?” working paper 29784 (Cambridge, Mass.: National Bureau of Economic Research, 2022).

feel overworked and fewer Americans feel underworked. This evidence suggests that the United States has moved toward a point where work-life balance has improved compared to before the pandemic.

Gale reflected upon some possible areas for further study. He wondered how much impact the EIPs may have had on mental health in relieving pandemic-related anxiety. Braun wondered whether Ricardian equivalence may be influencing EIP recipients' behavior—those who needed the money spent it, while those who didn't need it know they'll be taxed in the future to cover the pandemic spending.

Jonathan Parker addressed some of the topics raised by the discussants in his final remarks. He noted that the goal of the paper was to measure a rapid response to the EIPs. There is substantial weakening of statistical power when any lag responses are measured, so he and his coauthors did not try to make the stronger claim that they measured these lagged responses. The low MPCs that the authors found are not an argument that the EIPs were not spent at all. Rather, they were a piece of evidence that points to the EIPs not being spent immediately. He emphasized Rognlie's model that showed how various agents may spend their payments over short and long periods of time. The authors' intentions were to see whether the payments were being spent rapidly and timely as pandemic insurance.

On-line Appendix for  
Economic Impact Payments and Household  
Spending During the Pandemic

by

Jonathan A. Parker    Jake Schild    Laura Erhard    David S. Johnson

October 18, 2022

## A Further information about the CE and our use of it

### A.1 The CE survey instruments, already updated for BPEA

The BLS asked the following questions in all CE interviews from June 2020 until December 2020, and then again from February 2021 until October 2021 with two minor changes. First, in June 2020, the fifth question did not include the option for the EIP to be received by debit card, which was only added in July 2020 following the addition of this means of disbursement being added in reality. Second, from July 2021 on, the questions were modified to allow respondents to report receiving EIPs as credits when filing their 2020 taxes.

The following wording reflected here represents how the questions were asked in July 2021:<sup>1</sup>

In response to the coronavirus, the Federal government began sending stimulus payments, that is the coronavirus (COVID-19) related economic impact payment, directly to many households, either by check, direct deposit, or debit card. Since the first of (reference month), have (you/you or any members of your household) received a stimulus payment from the Federal government? Do not include refunds on annual income taxes, unemployment compensation, or payments from an employer.

10. Stimulus Payment

99. None/No more entries

Who received the stimulus payment?

\* Select all line numbers who are recipients of this stimulus payment, separate with commas. Enter each stimulus payment separately.

In what month was the stimulus payment received? [enter text] \_\_\_\_\_

\* Probe if month entered is not in the reference period.

What was the total amount of the stimulus payment? [enter text] \_\_\_\_\_

\* Probe if amount is less than 100 or greater than 5000.

Was the stimulus payment received by ...

1. check?

2. direct deposit?

3. debit card?

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<sup>1</sup>From July 2021 on, the first question had the following text added "Others have received it through the Recovery Rebate credit when filing their 2020 federal income taxes" and the fifth question had the following option added "4 TaxCredit as a credit on 2020 federal income taxes?"

How did or will (you/you or any members of your household) use the stimulus payment?

1. Mostly to pay for expenses
2. Mostly to pay off debt
3. Mostly to add to savings

Did (you/you or any members of your household) receive any other stimulus payments?

1. Yes
2. No

If yes, return to “Who received the stimulus payment?”

## A.2 CE files and variables

Data for this study come from the public-use CE interview survey.<sup>2</sup> We construct the panels primarily using the FMLI, CNT, and MEMI files.

The FMLI files contain interview information such as the interview identifier (*NEWID*), interview month (*QINTRVMO*), and calibration final weight (*FINLWT21*). They also document demographics, including the age of reference person and spouse (*AGE\_REF*, *AGE2*), family size (*FAM\_SIZE*), number of kids (*PERSLT18*), sex of the reference person (*SEX\_REF*), marital status (*MARITAL1*), and housing tenure (*CUTENURE*). There are also variables regarding the economics of the household, among which are pre-tax family annual income in the last 12 months (*FINCBTXM*), total value of liquid assets a year before (*LIQUDYR* and *LIQUDYRX*), and category-specific expenditures in the current calendar quarter (*XXXXXXXXCQ*) and the previous calendar quarter (*XXXXXXXXPQ*).<sup>3</sup>

The CNT20 file contains data on EIP receipt (indicated by *CONTCODE* = 800), including the amount (*CONTEXPX*), the month of receipt (*CONTMO*), the disbursement method (*CHCKEFT*, where 1 is “Check,” 2 is “Direct Deposit,” and 3 is “Debit Card”), and usage (*REBTUSED*, where 1 is “Mostly to pay for expenses,” 2 is “Mostly to pay off debts,” 3 is “Mostly to add to savings”).

Finally, the MEMI files contain the same interview information as the FMLI files. They additionally contain demographic and socioeconomic characteristics at the member level, as opposed to the FLMI files which are at the household level.

We use CE variables listed above to construct variables for the analysis of household spending response. A CU’s expenditure in a reference period is the sum of *XXXXXXXXCQ* and *XXXXXXXXPQ*. The first difference in consumer expenditures is consumer expenditures

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<sup>2</sup>Available at: <https://www.bls.gov/cex/pumd.htm>.

<sup>3</sup>In comparison to *FINCBTAX* that only uses reported income, *FINCBTXM* uses both reported income and imputed income, and thus, has fewer missing values.

in the current reference period minus consumer expenditures in the previous reference period. Similarly,  $\Delta FamSize_{i,t}$  is the difference between *FAM\_SIZE* in the current reference period and the previous reference period. If *AGE2* is not missing, we use the average of *AGE\_REF* and *AGE2* as the control variable  $age_{i,t}$ . If *AGE2* is missing, then  $age_{i,t} = AGE\_REF$ .  $EIP_{i,t-s}$  is the total dollar amount of payments received by household  $i$  in period  $t - s$ . We provide details about EIP variables and related cleaning in Appendix A.5.2. When dropping high-income households, we use *FINCBTAX* that makes use of imputation, some of which impute exact amounts within brackets of income reported by respondents.

Our main regressions also require CU average expenditure, average weights, income, liquidity, and group indicators. A CU's average expenditure ( $\bar{C}_i$ ) is the average of all reported expenditures across all its interviews. We compute a CU's average weight (*FINLWT21*) analogously. For income, we consider a CU's first *FINCBTXM*, which reflects the CU's annual income during the 12 months prior to the first interview. For liquidity, we use *LIQUDYRX* (reported by CUs in their fourth/last interviews only), which measures the total value of checking, savings, money market accounts, and certificates of deposits (CDs) one year before the date of the interview.<sup>4</sup> Due to missing data, our analysis using the liquidity variable has fewer observations. The non-recipient categorical variable for a CU equals 1 if the CU never reports any EIP in its interviews and 0 otherwise. Categorical variables for the disbursement method and reported main use follow the same idea. The bottom third and top third cutoffs for age, income, and liquidity groups are the terciles over the weighted sample.

From the MEMI files we obtain industry (*OCCUCODE*), education (*EDUCA*), salary (*SALARYBX* and *SALARYX*), employment status (*WKSTATUS*) and reason for unemployment (*INCNONWK*), all at the member level.

For EIP1, we use all relevant interviews conducted before February 2021 (including January interviews, but not February interviews) to analyze spending responses. We do not include later interviews since very few EIP1 were disbursed by then, and the IRS already started to send out EIP2 and EIP3. The corresponding CE files are *FMLI193* to *FMLI211* and *CNT20*. In addition, we use *FMLI212* but only for one purpose: to obtain liquidity information for CUs that have their last interview in the second quarter of 2021. For EIP2 and EIP3, we use all relevant interviews without restricting the latest interview included. The CE files used for EIP2 are *FMLI202* to *FMLI213*, *CNT20*, and *CNT21*. The CE files used for EIP3 are *FMLI203* to *FMLI213* and *CNT21*. Ideally, interviews conducted in the fourth

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<sup>4</sup>We assign CUs who do not have such accounts ( $LIQUDYR = 2$ ) to have  $LIQUDYRX = 0$ . We keep valid, topcoded *LIQUDYRX*.



quarter of 2021 and the first quarter of 2022 could also be used to analyze the longer-term impact, but these data are not yet available.

### **A.3 Definitions of consumer spending**

Following Lusardi (1996) and Johnson et al. (2006), expenditures on food include food away from home, food at home, and alcoholic beverages. Expenditures on strictly nondurable goods and services include expenditures on food, utilities (and fuels and public services), household operations, public transportation and gas and motor oil, personal care, tobacco, and miscellaneous goods. Non-durable goods and services (broadly defined) add expenditures on apparel goods and services, health care goods and services (only out-of-pocket expenditures by the CU), and reading materials. Total expenditures include those for all CE goods and services.

### **A.4 Effect of the pandemic on data quality**

With the onset of the COVID-19 pandemic, like other household surveys, the BLS modified its survey protocols starting in mid-March for contacting households and conducting interviews to be solely over the telephone. The survey continued to be conducted via telephone only through June, at which point in-person interviews began to resume in select locations. Both the changes to protocol and the pandemic resulted in lower than usual response rates. For the two months that we anchor the sample, response rates are 44.7% in June and 40.2% in July. BLS has studied and continues to study the impact of the pandemic and the protocol changes on the quality of estimates, finding little evidence for nonresponse bias in the Interview survey and no adverse impact to quality due to changes in the mode of the Interview survey. The BLS did report an increase in year-over-year change in the variation in expenditures, measured by the standard error divided by the mean, of between 1 and 2 percent for several expenditure categories. More information on the BLS evaluation of quality during the pandemic can be found on their website: <https://www.bls.gov/covid19/effects-of-covid-19-pandemic-and-response-on-the-consumer-expenditure-surveys.htm>.

## A.5 Further details on data processing

### A.5.1 CUs in the panel

As a first step, a CU can potentially be in the all CE sample or final sample if it satisfies both of the following: *a)* the CU was interviewed in June or July 2020 for the EIP1 panel, in February, March, and April 2021 for the EIP2 panel, and April, May, and June 2021 for the EIP3 panel. *b)* the CU must have at least two consecutive interviews. The first condition implies that we do not include CUs interviewed in May for EIP1 analysis, since one can never know whether such a CU receives an EIP in April. The second condition is for computing the first difference. These two conditions are necessary but not sufficient for a CU to be in our samples, given that the all CE sample and final sample drops outliers (as noted in Appendix A.5.3) and the final sample also drops CUs with high income (Tables C.5 to C.7). For analysis of differences across households, we also drop households that do not have the information necessary to assign them to a group.

### A.5.2 Cleaning EIP variables

Below are some assumptions we adopt for cleaning EIP variables.

- i) The CNT20 file contains all EIP information collected by the CE. If a CU does not have a documented EIP in the CNT20 file, there are two possibilities: the CU did not receive an EIP or the CU did not report receipt. The BLS does not flag non-response regarding EIP in the CE, so one cannot distinguish the former from the latter. We assume that everybody who does not have a documented EIP in the CNT20 file did not receive an EIP. Also, we keep EIPs flagged as “Valid value; imputed or adjusted in some other way,” which affects only a small number of observations in the sample.
- ii) We assume EIP1 were only received between April and November 2020. EIP2 were only received between December 2020 and February 2021. EIP3 were only received in and after March 2021. In other words, we assume that there are no EIPs received particularly late, which is largely consistent with the reality. We essentially also restrict the disbursement period of different rounds of EIPs to be non-overlapping.
- iii) We move November 2020 EIPs that are likely EIP2 to December and label them as EIP2. These are EIPs with payment size smaller than \$600 times family size.<sup>5</sup>

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<sup>5</sup>The number of November rebates (107) reported in the CE is high in comparison to September (34) and October (14). This increase is inconsistent with the IRS data. Moreover, many November payments are small

- iv) We move February 2021 EIPs that are likely EIP3 to March, labeling them as EIP3. Many reported February EIPs are likely EIP3 received in March.<sup>6</sup> First, we move any EIPs with payment size larger than \$600 times family size to March. For the remaining EIPs, we move any EIPs that are multiples of \$1,400 (including common multiples of \$600 and \$1,400) to March.
- v) We drop seven rebates (received by 5 CUs) reported to be received as a tax refund. These rebates are typically small (five of which are less than \$300) in amount and it is unclear whether they are EIP1 or EIP2. In addition, the option of reporting an EIP as “received as tax refund” was added late July 2021. Some rebates reported earlier can potentially be received as tax refund too, but the CU had no ways to report that.
- vi) We assume that January interviewees did not receive any EIPs during the reference period. That is, we assume that they did not receive EIP1 in October and November, and did not receive EIP2 in December. To be consistent, if a CU did not have a January interview, we also assume that no EIPs were received in the reference period of January. Similarly for CUs that did not have a February or March interview, we assume that there are no EIP1 received during the reference periods.
- vii) Where the method of disbursement of an EIP is missing, we treat it as missing.<sup>7</sup>
- viii) Where the mode of usage is missing for an EIP, we do one of the following: *a*) where there is at least one other EIP reported in the same interview, and the other EIP or EIPs all have the same reported usage, we apply that usage to the missing response. *b*) where there are multiple other EIPs reported in the same interview with different uses, we keep usage for that EIP missing. *c*) where there is no other EIP in the same interview, we keep usage as missing.

If a CU receives more than one EIPs in a reference period, variable  $EIP_{i,t}$  is the sum of EIP amounts received by the CU during the reference period. Similarly,  $EIP_{i,t}$  by a certain

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in size and resemble EIP2. About 40% is \$600, and another 40% is \$1,200. The average payment size (\$1,149) is about \$400 smaller than Sep or Oct, and is only half of the average April amount. On the contrary, \$1,149 is pretty close to the average payment size in December (\$1,084) and Jan 2021 (\$1,188).

<sup>6</sup>The IRS reports that EIP2 are all disbursed by the end of January, and they only started to send out EIP3 in March. Hence, there should be very few EIP2, and no EIP3 received in February. However, for the payment size of the February EIPs reported in the raw CE, the mean and quartiles are higher than those received in December 2020 and January 2021 (EIP2) but are closer to those in April 2021 (EIP3). Plus, many Feb EIPs are multiples of \$1,400 instead of \$600.

<sup>7</sup>One may raise the question that if a CU receives more than one EIP in a reference period and does not report disbursement method for at least one EIP, how should we assign EIP by disbursement method variables? This issue does not affect any CU in the final sample.

disbursement method (or for a certain usage) is the sum of EIP payments with the same disbursement method (or usage). If a CU receives multiple EIPs in a reference period and reports more than one disbursement method (or usage), then the CU will have positive values for more than one  $EIP_{i,t}$  by a certain disbursement method (or for a certain usage). For instance, assume CU  $i$  reports 4 EIPs in reference period  $t$ : \$1,200 by check, used for expenses, \$1,200 by direct deposit, used for expenses, and \$1,200 by debit card, used for paying down debt, and another \$500 by debit card, used for paying down debt. Then  $EIP_{i,t} = \$4,100$ ,  $EIP_{i,t}$  by check = \$1,200,  $EIP_{i,t}$  by direct deposit = \$1,200,  $EIP_{i,t}$  by debit card = \$1,700,  $EIP_{i,t}$ , used for expenses = \$2,400,  $EIP_{i,t}$ , paid off debt = \$1,700, and  $EIP_{i,t}$ , added to savings = \$0.

### A.5.3 Cleaning the sample

Following [Johnson et al. \(2006\)](#) and [Parker et al. \(2013\)](#), we clean the panel by dropping noisy observations (e.g., observations that we suspect contain misreporting). We first present a data cleaning process that exactly follows the two previous studies, and then address three modifications we make for the main analysis of this study.

- i) Drop every observation living in student housing ( $CUTENURE = 6$ ).
- ii) Drop every observation with  $AGE\_REF > 85$  or  $AGE\_REF < 21$ ; and with  $AGE2 > 85$  or  $AGE2 < 21$  if  $AGE2$  is not missing. Keep observations that have missing  $AGE2$ .
- iii) Drop every observation with change in  $AGE\_REF > 1$  or change in  $AGE\_REF < 0$ , if the reference person has the same sex ( $SEX\_REF$ ) in the two consecutive interviews. Similarly, we drop every observation with change in  $AGE2 > 1$  or change in  $AGE2 < 0$ , if the reference person has the same sex ( $SEX\_REF$ ) and marital status ( $MARITAL1$ ). Keep observations that has missing change in  $AGE2$ .
- iv) Drop every observation that has change in  $FAM\_SIZE$  greater than 3 or less than  $-3$ .
- v) Drop the bottom 1% observations with the lowest non-durable expenditures after adjusting for family size and time trend: *a*) Compute adult equivalized non-durable expenditures, counting kids as 0.6 adults. *b*) Create a time trend variable by setting interview month December 2019 (the earliest interview month in our panel) as 0, January 2020 as 1, March 2020 as 3, April 2020 as 4, and so on. *c*) Run a quantile regression of equivalized expenditure on time trend for the 1st percentile. *d*) Drop all observations with fitted values greater than the observed values (that is, all observations below the regression line).

We refer to the sample obtained from the above procedure *all households*. The three modifications we make for our *final sample* (used in Section II and Section IV onward) are:

- i) Modification to ii) above: We keep observations with  $AGE\_REF > 85$  or  $AGE2 > 85$ , who are about 5% of the sample and consist of a lot of recipients.
- ii) Modification to v) above: We drop the bottom 1% of the distribution in non-durable consumer expenditures per capita (defined as change in expenditure divided by the number of family members in the reference period), but instead of estimating the bottom one percent using a quantile regression on a linear trend, we drop the 1% observations with the lowest non-durable consumer expenditures per capita in each interview month. This modification is to account for the volatility of spending over time during the pandemic.
- iii) In addition, we drop CUs with income above a certain threshold determined by marital status and family structure, as discussed in Section III. Table C.5 to Table C.7 shows the thresholds.

Similar to previous studies, not all observations in the panel are used for the regressions. Scaling the variables essentially drops observations of a CU that has an zero or missing average expenditure. We also drop extreme outlier CUs whose scaled EIP is at least 1.5 times as large as the next largest scaled EIP (this step affect very few CUs). For the analysis that uses log change in expenditure as the dependent variable, observations with negative expenditure are dropped. For the analysis of EIPs by group, observations with relevant missing values (e.g, liquidity, disbursement method, use) are dropped.

## A.6 Imputation of EIPs

Imputed values of the EIPs were created using the MEMI and NTAXI files. The MEMI file contains an interview identifier (*NEWID*), an identifier for the tax unit to which each CU member belongs (*TAX\_UNIT*), the code of the tax payer (*TU\_CODE*, where 1 is "Taxpayer", 2 is "Spouse", and 3 is "Dependent"), the tax unit to which a dependent is a member of (*TU\_DPNDT*), and the age of the CU member (*AGE*). The NTAXI file contains an interview identifier (*NEWID*), an identifier for the tax unit (*TAX\_UNIT*), the filing status (*FILESTAT*, where 1 is "Single", 2 is "Married filing jointly", 3 is "head of household", and 8 is "Dependent tax payer"), and a measure of the Federal adjusted gross

income (*FDAGI*).<sup>8</sup>

We begin by creating a count of the number of dependents by *NEWID* in each tax unit using the *MEMI* file. A dependent can also be a tax filer, which means the tax unit identifier for the dependent will not be the same as the tax unit that claims the dependent (i.e., *TAX\_UNIT*  $\neq$  *TU\_DPNDT*). To create a count of the number of dependents within a tax unit, *TAX\_UNIT* is replaced with *TU\_DPNDT* if the member is a dependent and a tax filer. Two counts of dependents are created, the number of dependents under the age of 17 (*DPNDLT17\_TU*) and the number of dependents regardless of age (*DPNDANY\_TU*). This data is merged with the tax unit level data from *NTAXI*, yielding a dataset with observations at the tax unit level and includes measures of filing status, the number of dependents, and adjusted gross income (*AGI*).

We use this dataset to impute values of *EIP1*, *EIP2*, and *EIP3* following the qualification and phase out rules laid out in the respective legislation. For example, tax units whose filing status is single (*FILESTAT* = 1) and have an *AGI* less than \$75,000 have an imputed value of *EIP1* of \$1,200. The imputed value is reduced by \$50 for every \$1,000 over the \$75,000 threshold, and is \$0 for any single tax filer with *AGI* greater than \$99,000. For the imputation of *EIP1* and *EIP2* *DPNDLT17\_TU* is used to measure the number of dependents. For *EIP3*, the number of dependents is measured by *DPNDANY\_TU* due to the change in definition of “qualifying dependent.” The result is a dataset that contains imputed values for all three waves of *EIPs* at the tax unit level. The imputed values for each *EIP* are summed across tax units to get a *NEWID* level measure of imputed *EIPs*.

Each *CU* have up to four imputations for each *EIP*, one for each interview in which the *CU* participated. Imputations are calculated independently across interviews because the determinants the value of the imputation (*AGI*, number of dependents, etc.) can vary between interviews. To account for this variation and the uncertainty surrounding which set of information corresponds closest to the information used by the *IRS* when calculating the actual payment, the imputations across interviews are combined such that a *CU* has up to four imputed values for each of the three waves of *EIPs*.

The imputed values need to be assigned to a specific interview for each *CU*; however, each wave of *EIPs* were distributed over multiple months, and therefore, it is not immediately obvious which interview to assign the imputation. For simplicity, we assume each *EIP* was received within the first two months of when the wave began being distributed. This means *EIP1* was received in April or May 2020, *EIP2* was received in January or February

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<sup>8</sup>Using data collected during the Interview, the *BLS* creates tax units and then employs the *NBER TAXSIM* model to provide tax unit level measures of wages, the tax burden, etc.

2021, and EIP3 was received in March or April 2021. In order for us to remain agnostic about which month the EIP was received, the *final sample* is restricted to CUs with a reference period that contain both critical months. For example, the sample for EIP3 is restricted to CUs on the May or June 2021 interview cycle. CUs interviewed in May or June have reference periods that contain March and April. In contrast, CUs interviewed in April only have March in their reference period. If these CUs are included in the sample we have to determine whether the imputed value of EIP3 is assigned to the April interview, meaning the EIP was received in March, or the July interview, meaning the EIP was received in April. Since we have no way of making this decision, besides arbitrarily, we drop these CUs from the sample.

The restricted *final sample* is merged with the imputed EIP data.  $IMP_{EIPnt}$  represents the imputed value of  $EIP_n$  at time  $t$ . We compare the four imputed values for the corresponding wave to the reported value of  $EIP_n$  at time  $t$  ( $EIP_{nt}$ ). If any of the four imputed values match the reported value it is assigned to  $IMP_{EIPnt}$ . If none of the imputations match the reported value then the imputation corresponding to the interview during which the EIP could have been received is assigned to  $IMP_{EIPnt}$ . For example, if a CU's second interview occurred in March 2021 then the imputation for EIP3 from the second interview of the CU is assigned to  $IMP_{EIP3t}$ . Note,  $IMP_{EIPnt}$  is the imputed value that will be used for the analysis. All other EIP imputations are dropped after this step.

The analysis of the imputed EIPs follows the procedure laid out by equations 3 to 5. When analyzing EIP2 and EIP3, the first stage includes controls for the other waves of EIPs. In order to maintain our position on not determining which month an EIP was received, the controls for the other EIPs in the first stage are based on the reported values.

The results for the imputed equivalent to Table C.8 can be found in Appendix Tables ?? to C.10. Panel A of the tables shows the estimates of the MPC using the reported *EIP* values and restricted *final sample*. Panel B of the tables shows the estimates of the MPC using the imputed *EIPs* values and restricted *final sample*. Panel C shows the estimates of the MPC using the imputed EIPs but further restricting the *final sample* by removing outliers of imputed EIPs. For EIP1 this means any observation with an imputed value greater than \$4,520 were dropped. For EIP2 and EIP3 any observations with imputed values greater than \$3,527 and \$10,212, respectively, were dropped.<sup>9</sup>

Results for the imputed equivalent to Table C.11 can be found in Appendix Tables ?? to C.13. The same structure described for the previous three tables also applies to these tables.

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<sup>9</sup>These thresholds were determined by adding three times the standard deviation to the average of non-zero imputed EIPs.

## B Ability to work from home

### B.1 Creating a measure of work-from-home ability by industry and education level

Our objective is to measure for each member in the CE, the extent to which the pandemic potentially impacted their income due to an inability to work from home. We do this based on pre-pandemic wage and salary incomes and a *low-work-from-home* measure at the occupation-education level constructed by [Mongey et al. \(2021\)](#), based on data from O\*NET and building on [Dingel and Neiman \(2020\)](#). We take the continuous version of their low-work-from-home measure, which is a tally  $\in [0, 17]$  of the number of in-person activities required of a job. Dividing by 17, we interpret this measure as the share of the job that can be done from home or as the probability that the job can be done at home.

### B.2 Merging into the CE

Ideally, we would observe the occupation of each member in the CE, but the BLS does not ask for this information in the CE Survey. However, the BLS does collect each member's industry and education level. We therefore merge the [Mongey et al. \(2021\)](#) measure into the March 2019 CPS Annual social and economic supplement file, merging at the 4-digit occupation level. Using a cross-walk between industries and occupations in the CPS and those in the CE, we group individuals in the CPS into industry-education cells, and take the average of the work-from-home measure in each cell. We end up with 105 industry-education cells each with a separate value of  $wfh \in [0, 1]$ . We then merge the industry-education averages into the CE.

We set  $wfh = 1$  for any household in which no reference person or spouse/partner has earned income (valid missing earnings), like retirees or people not in the labor force due to illness or disability. We drop CUs with (valid or missing) labor earnings for which either education level or industry is missing, unless they are not in the labor force.

### B.3 Constructing CU-level work-from-home measures

We measure each CU's ability to work from home in two separate ways. Throughout, we only consider the earnings the CU reference member and their spouse or partner.



**Earnings-based measure** The first work-from-home measure we construct is based on pre-pandemic wage and salary earnings, which requires that we limit the sample to households whose first CE survey takes place in 2019Q1, 2019Q2 or 2020Q1.

Whenever individual-level earnings is observed, it is reported as either an exact amount or as a range. When a range is reported, we take the midpoint of this range as an individual's earnings. To minimize dropping of data, if one member within a CU has positive earnings and the other member has a missing value for earnings, we construct the measure as if the member with a missing value has zero earnings. If both members in a CU have missing earnings, we drop the observation. If both members of the CU have no wage and salary earnings (not missing), we keep this observation.

Having done this, for each CU, we construct a measure of the amount of labor income that was not exposed to the pandemic. We do this by taking reported before tax family income in the first interview wave, before the onset of the COVID-19 pandemic, and subtracting an imputed estimate of an amount potentially lost during the pandemic due to a lack of ability to work from home:

$$\text{Retained income share}_i = \frac{\text{total income}_i - \sum_{k \in H} (1 - \text{wfh}_{i,k}) \times \text{earnings}_{i,k}}{\text{total income}_i}.$$

where  $H$  indexes the (zero, one, or two) earners in the household (no one, reference person, and/or spouse/partner). A household for which all work can be done from home will have a retained income share of 1. A household with lower levels of  $\text{wfh}_{i,j}$ , will have a lower share of retained income. Households with no wage and salary earners such as retirees have retained income share of 1. In total, we are able to construct the work from home measure for approximately 91% of CUs with first interviews before the pandemic.

Because we require pre-pandemic income, we only use this measure to analyze EIP1.

**Worker-based measure** 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. Earnings after the onset of the pandemic may already reflect losses incurred by an inability to work from home.

This earnings-less measure is equivalent to the earnings-based measure above but imposing the assumptions that CUs have no source of income apart from labour income

and that all members within a CU are equal earners.

$$\text{Retained worker share}_i = \text{Average}_{k \in H} [\text{wfh}_{i,k}]$$

As noted, we are assuming that retirees and those not in the labour force retain 100% of their income.

## C Additional Tables and Figures

**Table C.1:** EIP1 amounts in the CE Survey

<i>Panel A: Distribution of EIP1 amounts</i>		
EIP value	Number of Observations	Percentage
$EIP = 0$	498 (12808701)	19.0 (19.0)
$0 < EIP < 1200$	99 (2262445)	3.8 (3.4)
$EIP = 1200$	763 (19424546)	29.1 (28.8)
$1200 < EIP < 1700$	43 (1205195)	1.6 (1.8)
$EIP = 1700$	43 (1227011)	1.6 (1.8)
$1700 < EIP < 2400$	108 (2843463)	4.1 (4.2)
$EIP = 2400$	626 (15870540)	23.9 (23.5)
$2400 < EIP < 2900$	30 (801208)	1.1 (1.2)
$EIP = 2900$	104 (2707011)	4.0 (4.0)
$2900 < EIP < 3400$	21 (634388)	0.8 (0.9)
$EIP = 3400$	71 (1974085)	2.7 (2.9)
$3400 < EIP < 3900$	91 (2367474)	3.5 (3.5)
$EIP = 3900$	40 (1067875)	1.5 (1.6)
$EIP > 3900$	83 (2311985)	3.2 (3.4)
Total	2620 (67505928)	100 (100)
<i>Panel B: Average EIP1 amount</i>		
	Unweighted	Weighted
Average EIP amount:	\$2,077	\$2,098

*Notes:* 2020 Consumer Expenditure Survey (BLS). Statistics based on our final sample which includes only CE household with an interview in June or July 2020, 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 average CU weights across reference periods. EIP values are the total amount received by a household in the 3-month reference period, as in the main regressions, and counts are un-weighted sums. Weighted counts and percentages are in parentheses. The number of  $EIP = 0$  essentially is the number of CUs that never received an EIP1 in the panel. The average EIP amounts are conditional on receiving an EIP.

**Table C.2: EIP2 amounts in the CE Survey**

<i>Panel A: Distribution of EIP2 amounts</i>		
EIP value	Number of Observations	Percentage
<i>EIP</i> = 0	2035 (53140461)	51.6 (52.6)
0 < <i>EIP</i> < 600	62 (1369577)	1.6 (1.4)
<i>EIP</i> = 600	671 (16304796)	17.0 (16.1)
600 < <i>EIP</i> < 1200	55 (1406318)	1.4 (1.4)
<i>EIP</i> = 1200	604 (15459617)	15.3 (15.3)
1200 < <i>EIP</i> < 1800	44 (1097892)	1.1 (1.1)
<i>EIP</i> = 1800	190 (5086242)	4.8 (5.0)
1800 < <i>EIP</i> < 2400	29 (807944)	0.7 (0.8)
<i>EIP</i> = 2400	116 (2857817)	2.9 (2.9)
2400 < <i>EIP</i> < 3000	22 (531490)	0.6 (0.5)
<i>EIP</i> = 3000	48 (1280559)	1.2 (1.3)
<i>EIP</i> > 3000	64 (1671696)	1.6 (1.7)
Total	3940 (101014409)	100 (100)
<i>Panel B: Average EIP2 amount</i>		
	Unweighted	Weighted
Average EIP amount:	\$1,281	\$1,301

*Notes:* 2020 and 2021 Consumer Expenditure Survey (BLS). Statistics based on our final sample which includes only CE household with an interview in February, March, or April 2020, 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 average CU weights across reference periods. EIP values are the total amount received by a household in the 3-month reference period, as in the main regressions, and counts are un-weighted sums. Weighted counts and percentages are in parentheses. The number of *EIP* = 0 essentially is the number of CUs that never received an EIP2 in the panel. The average EIP amounts are conditional on receiving an EIP.

**Table C.3: EIP3 amounts in the CE Survey**

<i>Panel A: Distribution of EIP amounts</i>		
EIPIII value	Number of Observations	Percentage
$EIP = 0$	1148 (29146336)	28.8 (28.5)
$0 < EIP < 1400$	291 (7372425)	7.3 (7.2)
$EIP = 1400$	839 (21095236)	21.1 (20.6)
$1400 < EIP < 2800$	253 (6495903)	6.4 (6.3)
$EIP = 2800$	751 (19589152)	18.9 (19.1)
$2800 < EIP < 4200$	130 (3483544)	3.3 (3.4)
$EIP = 4200$	191 (4876644)	4.8 (4.8)
$4200 < EIP < 5600$	63 (1614958)	1.6 (1.6)
$EIP = 5600$	153 (4218936)	3.8 (4.1)
$EIP > 5600$	163 (4498133)	4.1 (4.4)
Total	3982 (102391266)	100 (100)
<i>Panel B: Average EIPIII amount</i>		
	Unweighted	Weighted
Average EIP amount:	\$2,767	\$2,814

*Notes:* 2021 Consumer Expenditure Survey (BLS). Statistics based on our final sample which includes only CE household with an interview in April, May, or June 2021, 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 average CU weights across reference periods. EIP values are the total amount received by a household in the 3-month reference period, as in the main regressions, and counts are un-weighted sums. Weighted counts and percentages are in parentheses. The number of  $EIP = 0$  essentially is the number of CUs that never received an EIPIII in the panel. The average EIP amounts are conditional on receiving an EIP.

**Table C.4:** The response of consumer expenditure to EIP arrival estimated on recipients and non-recipients using the methodology previously applied to tax rebates

Est. method	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS
	<i>Pct change in spending. Dependent variable: <math>\Delta \ln C</math></i>				<i>MPC. Dependent variable: <math>\Delta C</math></i>			
	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
Panel A: EIP 1								
<i>EIP1</i>					0.077 (0.044)	0.145 (0.064)	0.183 (0.082)	0.625 (0.316)
<i>I(EIP1)</i>	2.71 (3.02)	1.58 (2.01)	1.53 (1.94)	1.35 (2.33)				
Panel B: EIP 2								
<i>EIP2</i>					-0.044 (0.040)	-0.008 (0.061)	-0.008 (0.077)	-0.385 (0.576)
<i>I(EIP2)</i>	-1.23 (1.96)	1.16 (1.39)	0.64 (1.33)	-0.06 (1.71)				
Panel C: EIP 3								
<i>EIP3</i>					0.005 (0.017)	-0.003 (0.026)	0.008 (0.034)	0.247 (0.235)
<i>I(EIP3)</i>	3.35 (1.58)	1.26 (1.18)	1.38 (1.12)	2.03 (1.52)				

*Notes:* Table reports  $\beta_0$  from estimation of equation 1 with  $S = 0$ . The coefficients for 2SLS regressions are multiplied by 100 so as to report a percent change. In 2SLS regressions, EIP indicator, together with control variables, are used as instruments for the EIP amounts. 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. Standard errors included in parentheses are adjusted for arbitrary within-household correlations and heteroskedasticity.

**Table C.5:** Income cutoff values for the final sample of EIP1 and number of observations nearby

CU type	Income cutoff	Below cutoff by 0 to 25K		Above cutoff by 0 to 25K	
		recipients	non-recipients	recipients	non-recipients
Single, no kids	\$175K	4	12	0	1
Single, with kid(s)	\$225K	0	0	0	1
Married, no kids	\$400K	5	10	0	4
Married, with kid(s)	\$400K	1	8	0	2
Adults, no kids	\$425K	0	0	0	2
Adults, with kid(s)	\$425K	0	0	0	0

*Notes:* Data Source: 2019-2020 Consumer Expenditure Survey (BLS), final sample. CU types “Single, no kids” and “Single, with kid(s)” include every CU that has one and only one unmarried adult. CU types “Married, no kids” and “Married, with kid(s)” can include CUs that have one or more than one adults, as long as the reference person is married. Similarly, CU types “Adults, no kids” and “Adults, with kid(s)” can include CUs that have two or more than 2 adults, as long as the reference person is single. We posit an income cutoff at the nearest \$25,000 above the income level (also rounded to the nearest \$25,000) at which a household would no longer receive an EIP. For this baseline income level, we assume two kids per household if a household has kid(s) (type 2, 4, and 6), and two adults if the reference person is married or there are more than one adult in the household (type 2 to 6). The income levels at which the six types of households can no longer receive rebate after rounding are \$100K, \$150K, \$200K, \$225K, \$225K, and \$250K, respectively. We adjust each income cutoff up in increments of \$25,000 until about more than 80% of the CE households with incomes in the \$25,000 range just above the cutoff are non-recipients. To be clear, if the \$25,000 interval above an income level contains no recipients nor non-recipients, we continue to adjust up the income level. In addition, we set the cutoff for CUs with kids to be the same as the cutoff for CUs that are otherwise the same but without kids (i.e., married, no kids and married, with kids), if the former has a lower cutoff after increments.

**Table C.6:** Income cutoff values for the final sample of EIP2 and number of observations nearby

CU type	Income cutoff	Below cutoff by 0 to 25K		Above cutoff by 0 to 25K	
		recipients	non-recipients	recipients	non-recipients
Single, no kids	\$125K	38	72	3	59
Single, with kid(s)	\$175K	3	4	0	4
Married, no kids	\$275K	16	38	6	27
Married, with kid(s)	\$275K	9	43	13	28
Adults, no kids	\$250K	4	10	1	6
Adults, with kid(s)	\$275K	2	0	0	1

*Notes:* Data Source: 2020-2021 Consumer Expenditure Survey (BLS), final sample. CU types “Single, no kids” and “Single, with kid(s)” include every CU that has one and only one unmarried adult. CU types “Married, no kids” and “Married, with kid(s)” can include CUs that have one or more than one adults, as long as the reference person is married. Similarly, CU types “Adults, no kids” and “Adults, with kid(s)” can include CUs that have two or more than 2 adults, as long as the reference person is single. We posit an income cutoff at the nearest \$25,000 above the income level (also rounded to the nearest \$25,000) at which a household would no longer receive an EIP. For this baseline income level, we assume two kids per household if a household has kid(s) (type 2, 4, and 6), and two adults if the reference person is married or there are more than one adult in the household (type 2 to 6). The income levels at which the six types of households can no longer receive rebate after rounding are \$75K, \$150K, \$175K, \$200K, \$200K, and \$225K, respectively. We adjust each income cutoff up in increments of \$25,000 until more than 80% of the CE households with incomes in the \$25,000 range just above the cutoff are non-recipients. To be clear, if the \$25,000 interval above an income level contains no recipients nor non-recipients, we continue to adjust up the income level. In addition, we set the cutoff for CUs with kids to be the same as the cutoff for CUs that are otherwise the same but without kids (i.e., married, no kids and married, with kids), if the former has a lower cutoff after increments.



**Table C.7:** Income cutoff values for the final sample of EIP3 and number of observations nearby

CU type	Income cutoff	Below cutoff by 0 to 25K		Above cutoff by 0 to 25K	
		recipients	non-recipients	recipients	non-recipients
Single, no kids	\$125K	27	65	3	44
Single, with kid(s)	\$275K	0	0	0	1
Married, no kids	\$225K	22	36	10	46
Married, with kid(s)	\$225K	13	30	8	76
Adults, no kids	\$425K	0	0	0	4
Adults, with kid(s)	\$425K	0	0	0	0

*Notes:* Data Source: 2020-2021 Consumer Expenditure Survey (BLS), final sample. CU types “Single, no kids” and “Single, with kid(s)” include every CU that has one and only one unmarried adult. CU types “Married, no kids” and “Married, with kid(s)” can include CUs that have one or more than one adults, as long as the reference person is married. Similarly, CU types “Adults, no kids” and “Adults, with kid(s)” can include CUs that have two or more than 2 adults, as long as the reference person is single. We 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. For this baseline income level, we assume two kids per household if a household has kid(s) (type 2, 4, and 6), and two adults if the reference person is married or there are more than one adult in the household (type 2 to 6). The income levels at which the six types of households can no longer receive rebate after rounding are \$75K, \$125K, \$150K, \$150K, \$200K, and \$200K, respectively. We adjust each income cutoff up in increments of \$25,000 until more than 80% of the CE households with incomes in the \$25,000 range just above the cutoff are non-recipients. To be clear, if the \$25,000 interval above an income level contains no recipients nor non-recipients, we continue to adjust up the income level. In addition, we set the cutoff for CUs with kids to be the same as the cutoff for CUs that are otherwise the same but without kids (i.e., married, no kids and married, with kids), if the former has a lower cutoff after increments. Note that the \$275K for single with kid(s) look high, but are in fact due to consecutive 25K intervals with no observations – setting this cutoff as 200K gives exactly the same sample as \$275K, but we strictly follow the rules to decide cutoffs. Similarly, for adults with or without kids, setting the cutoffs to be \$350K gives exactly the same sample as \$425K.

**Table C.8:** The contemporaneous response of consumer expenditures to EIP1 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
	<i>MPC</i>				<i>Dollars spent</i>			
	<i>Panel A. Observed</i>							
$\widetilde{EIP1}$	0.011 (0.016)	0.075 (0.020)	0.102 (0.028)	0.234 (0.059)				
$\mathbb{1}[\widetilde{EIP1} > 0]$					6.5 (25.3)	96.5 (36.6)	80.8 (46.4)	336.5 (96.6)
	<i>Panel B. Imputed</i>							
$\widetilde{EIP1}$	0.027 (0.018)	0.051 (0.029)	0.247 (0.027)	0.356 (0.057)				
$\mathbb{1}[\widetilde{EIP1} > 0]$					132.2 (25.3)	32.5 (42.1)	318.1 (48.1)	482.8 (91.0)
	<i>Panel C. Imputed and Restricted</i>							
$\widetilde{EIP1}$	0.097 (0.017)	0.053 (0.030)	0.250 (0.027)	0.354 (0.058)				
$\mathbb{1}[\widetilde{EIP1} > 0]$					132.3 (25.3)	32.6 (42.2)	318.5 (48.1)	482.2 (91.0)
	Average quarterly household spending							
Panels A and B	\$2,258	\$4,429	\$5,962	\$14,381	\$2,258	\$4,429	\$5,962	\$14,381
Panel C	\$2,256	\$4,427	\$5,958	\$14,377	\$2,256	\$4,427	\$5,958	\$14,377

*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. The rows in Panels A and B use the final sample restricted to CUs on the March and April 2021 interview cycle. The rows in Panel C further restrict the sample to all observations with imputed EIP1 value less than \$4,520. 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. For Panel A, the 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 which has 2,261. For Panel B, the columns have 3,981, 3,985, 3,985, and 3,986 treated observations, and 1,822 never-treated or not-yet-treated observations except for the first column which has 1,821. For Panel C, the columns have 3,970, 3,974, 3,974, and 3,975 treated observations, and 1,822 never-treated or not-yet-treated observations except for the first column which has 1,821.

**Table C.9:** The contemporaneous response of consumer expenditures to imputed EIP2 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
	<i>MPC</i>				<i>Dollars spent</i>			
<i>Panel A. Observed</i>								
$\widetilde{EIP2}$	0.026 (0.025)	0.148 (0.037)	0.130 (0.047)	0.324 (0.104)				
$\mathbb{1}[\widetilde{EIP2} > 0]$					18.0 (32.7)	149.9 (58.2)	126.8 (68.7)	343.0 (140.1)
<i>Panel B. Imputed</i>								
$\widetilde{EIP2}$	0.078 (0.029)	0.370 (0.039)	0.235 (0.041)	0.111 (0.096)				
$\mathbb{1}[\widetilde{EIP2} > 0]$					103.0 (28.5)	355.5 (42.5)	235.7 (46.2)	361.2 (120.4)
<i>Panel C. Imputed and Restricted</i>								
$\widetilde{EIP2}$	0.090 (0.032)	0.412 (0.045)	0.262 (0.047)	0.092 (0.109)				
$\mathbb{1}[\widetilde{EIP2} > 0]$					102.8 (28.5)	355.8 (42.5)	236.0 (46.2)	352.1 (46.2)
<i>Average quarterly household spending</i>								
Panels A and B	\$2,345	\$4,631	\$6,111	\$14,734	\$2,345	\$4,631	\$6,111	\$14,734
Panel C	\$2,336	\$4,618	\$6,100	\$14,717	\$2,336	\$4,618	\$6,100	\$14,717

*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. The rows in Panels A and B use the final sample restricted to CUs on the March and April 2021 interview cycle. The rows in Panel C further restrict the sample to all observations with imputed EIP2 less than \$3,527. 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, change in the size of the CU, and controls for observed EIP1 and EIP3. For Panel A, the columns have 1,755 treated observations except for the first column that has 1,753, and 3,568, 3,576, 3,578, and 3,579 never-treated or not-yet-treated observations. For Panel B, the columns have 3,672, 3,677, 3,679, and 3,679 treated observations, and 1,649, 1,654, 1,654, and 1,655 never-treated or not-yet-treated observations. For Panel C, the columns have 3,632, 3,637, 3,639, and 3,639 treated observations, and 1,649, 1,654, 1,654, and 1,655 never-treated or not-yet-treated observations.

**Table C.10:** The contemporaneous response of consumer expenditures to imputed EIP3 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
	<i>MPC</i>				<i>Dollars spent</i>			
<i>Panel A. Observed</i>								
$\widetilde{EIP3}$	0.041 (0.014)	0.042 (0.020)	0.002 (0.023)	-0.001 (0.053)				
$\mathbb{1}[\widetilde{EIP3} > 0]$					133.2 (37.0)	133.4 (57.8)	47.6 (51.8)	-75.7 (122.5)
<i>Panel B. Imputed</i>								
$\widetilde{EIP3}$	0.028 (0.012)	0.140 (0.017)	0.126 (0.021)	0.179 (0.042)				
$\mathbb{1}[\widetilde{EIP3} > 0]$					42.8 (32.1)	282.7 (40.0)	165.8 (47.7)	-191.0 (99.9)
<i>Panel C. Imputed and Restricted</i>								
$\widetilde{EIP3}$	0.011 (0.011)	0.141 (0.017)	0.126 (0.022)	0.173 (0.044)				
$\mathbb{1}[\widetilde{EIP3} > 0]$					44.1 (32.1)	283.5 (40.0)	166.9 (47.8)	-182.4 (100.0)
Average quarterly household spending across three waves								
Panels A and B	\$2,311	\$4,561	\$5,970	\$13,971	\$2,311	\$4,561	\$5,970	\$13,971
Panel C	\$2,301	\$4,548	\$5,961	\$13,940	\$2,301	\$4,548	\$5,961	\$13,940

*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. The rows in Panels A and B use the final sample restricted to CUs on the May and June 2021 interview cycle. The rows in Panel C further restrict the sample to all observations with imputed EIP3 less than \$10,212. 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, change in the size of the CU, and controls for the observed values of EIP1 and EIP2. For Panel A, the columns have 2,429, 2,429, 2,431, and 2,431 treated observations and 2,331, 2,337, 2,338, and 2,335 never-treated or not-yet-treated observations. For Panel B, the columns have 3,205, 3,206, 3,209, and 3,209 treated observations and 1,555, 1,560, 1,560, and 1,557 never-treated or not-yet-treated observations. For Panel C the columns have 3,183, 3,184, 3,187, and 3,187 treated observations and 1,555, 1,560, 1,560, and 1,557 never-treated or not-yet-treated observations.

**Table C.11: The longer-term response of consumer expenditures to EIP1 receipt**

<i>Dependent variable: scaled dollar change in spending on</i>									
	<i>Panel A: Observed</i>			<i>Panel B: Imputed</i>			<i>Panel C: Imputed and Restricted</i>		
	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
$\widetilde{EIP1}_t$	0.075 (0.020)	0.102 (0.028)	0.234 (0.059)	0.051 (0.029)	0.247 (0.027)	0.356 (0.057)	0.053 (0.030)	0.250 (0.027)	0.354 (0.058)
$\widetilde{EIP1}_{t-1}$	-0.011 (0.020)	-0.080 (0.028)	-0.017 (0.070)	-0.092 (0.050)	-0.057 (0.028)	-0.153 (0.070)	-0.092 (0.050)	-0.056 (0.029)	-0.154 (0.070)
<i>Implied cumulative fraction of EIP spent over two three-month periods</i>									
	0.139 (0.051)	0.124 (0.068)	0.452 (0.158)	0.0100 (0.071)	0.438 (0.069)	0.559 (0.153)	0.0136 (0.072)	0.444 (0.070)	0.555 (0.154)

*Notes:* Table reports  $\beta_0$  and  $\beta_1$  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 A and B use the final sample restricted to CUs on the June and July 2020 interview cycle. Panel C further restrict the sample to all observations with imputed EIP1 value less than \$4,520. 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, the columns have 3,543 treated observations except for the last column which has 3,544, and 2,264 never-treated or not-yet-treated observations. For Panel B, the columns have 3,985 treated observations except for the last column which has 3,986, and 1,822 never-treated or not-yet-treated observations. For Panel C, the columns have 3,974 treated observations except for the last column which has 3,975, and 1,822 never-treated or not-yet-treated observations.

**Table C.12:** The longer-term response of consumer expenditures to imputed EIP2 receipt

<i>Dependent variable: scaled dollar change in spending on</i>									
	<i>Panel A: Observed</i>			<i>Panel B: Imputed</i>			<i>Panel C: Imputed and Restricted</i>		
	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
$\widetilde{EIP2}_t$	0.148 (0.037)	0.130 (0.047)	0.324 (0.104)	0.370 (0.039)	0.235 (0.041)	0.111 (0.096)	0.412 (0.045)	0.262 (0.047)	0.092 (0.109)
$\widetilde{EIP2}_{t-1}$	0.080 (0.051)	-0.022 (0.060)	-0.052 (0.155)	0.158 (0.052)	-0.060 (0.050)	-0.015 (0.120)	0.172 (0.052)	-0.051 (0.050)	-0.028 (0.121)
<i>Implied cumulative fraction of EIP spent over two three-month periods</i>									
	0.376 (0.105)	0.238 (0.130)	0.595 (0.305)	0.898 (0.114)	0.410 (0.113)	0.206 (0.269)	0.996 (0.124)	0.472 (0.125)	0.155 (0.297)

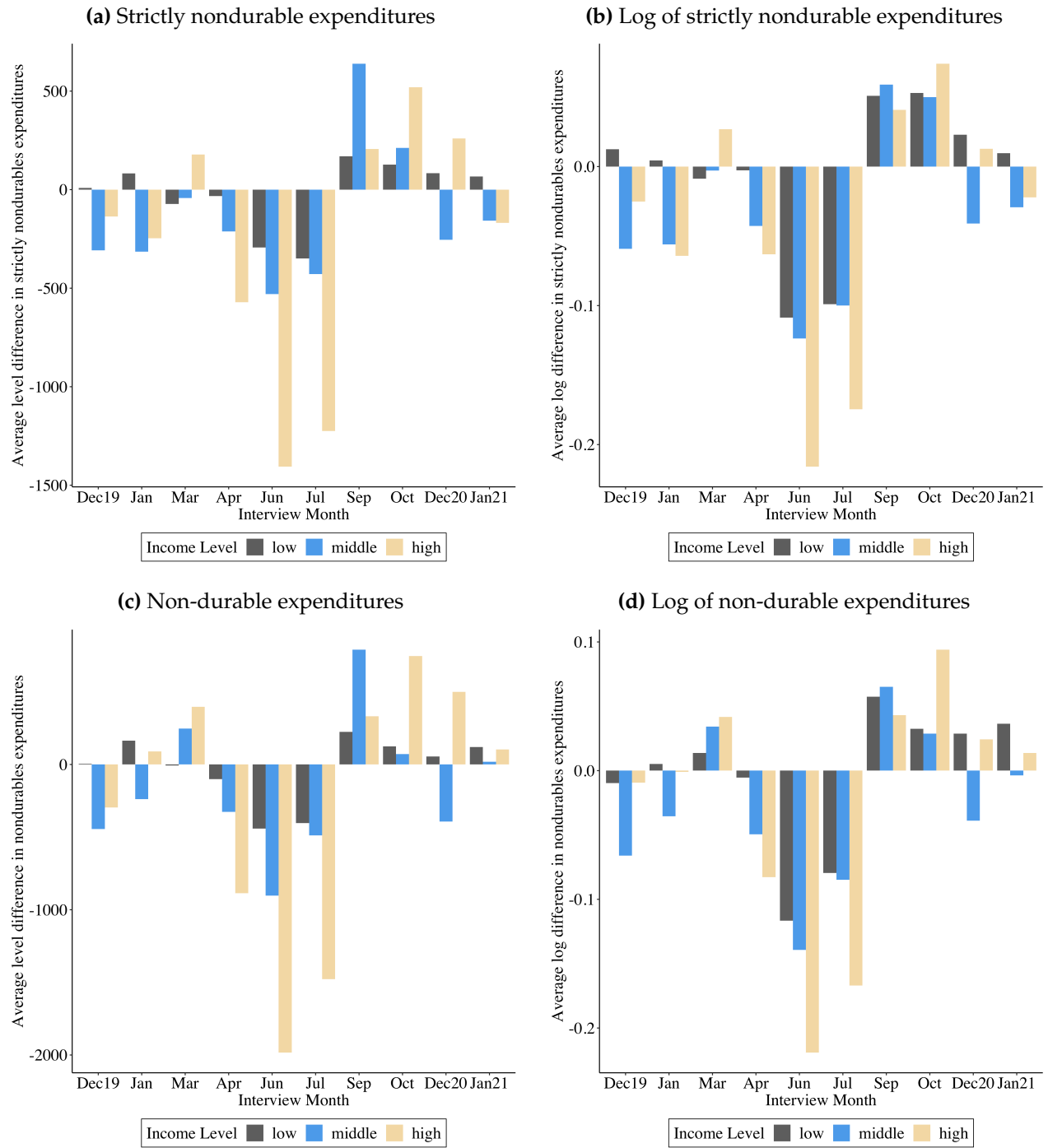
*Notes:* Table reports  $\beta_0$  and  $\beta_1$  from estimation of equations 3 to 5 with  $S = 1$ . Regressions also include interview month dummies, a separate intercept for non-recipients, scaled age, change in the size of the CU, and controls for observed EIP1 and EIP3. The rows in Panels A and B use the final sample restricted to CUs on the March and April 2021 interview cycle. The rows in Panel C further restrict the sample to all observations with imputed EIP2 less than \$3,527. 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, the columns have 1,755 treated observations, and 3,576, 3,578, and 3,579 never-treated or not-yet-treated observations. For Panel B, the columns have 3,679 treated observations except for the first column that has 3,677, and 1,654 never-treated or not-yet-treated observations except for the last column that has 1,655. For Panel C, the columns have 3,639 treated observations except for the first column that has 3,637, and 1,654 never-treated or not-yet-treated observations except for the last column that has 1,655.

**Table C.13:** The longer-term response of consumer expenditures to imputed EIP3 receipt

		<i>Dependent variable: scaled dollar change in spending on</i>								
		<i>Panel A: Observed</i>			<i>Panel B: Imputed</i>			<i>Panel C: Imputed and Restricted</i>		
		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
$\widetilde{EIP3}_t$		0.042 (0.020)	0.002 (0.023)	-0.001 (0.053)	0.140 (0.017)	0.126 (0.021)	0.179 (0.042)	0.141 (0.017)	0.126 (0.022)	0.173 (0.044)
$\widetilde{EIP3}_{t-1}$		0.005 (0.010)	-0.049 (0.022)	-0.164 (0.058)	-0.091 (0.011)	-0.050 (0.018)	-0.275 (0.042)	-0.091 (0.011)	-0.050 (0.018)	-0.274 (0.042)
		Implied cumulative fraction of EIP spent over two three-month periods								
		0.089 (0.044)	-0.045 (0.058)	-0.166 (0.136)	0.188 (0.040)	0.201 (0.053)	0.082 (0.109)	0.191 (0.040)	0.201 (0.054)	0.072 (0.113)

*Notes:* Table reports  $\beta_0$  and  $\beta_1$  from estimation of equations 3 to 5 with  $S = 1$ . Regressions also include interview month dummies, a separate intercept for non-recipients, scaled age, change in the size of the CU, and controls for the observed values of EIP1 and EIP2. The rows in Panels A and B use the final sample restricted to CUs on the May and June 2021 interview cycle. The rows in Panel C further restrict the sample to all observations with imputed EIP3 less than \$10,212. 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 the columns have 2,431 treated observations except for the first column that has 2,431, and 2,337, 2,338, and 2,335 never-treated or not-yet-treated observations. For Panel B, the columns have 3,209 treated observations except for the first column that has 3,206 and 1,560 never-treated or not-yet-treated observations except for the last column that has 1,557. For Panel C the columns have 3,187 treated observations except for the first column that has 3,184 and 1,560 never-treated or not-yet-treated observations except for the last column that has 1,557.

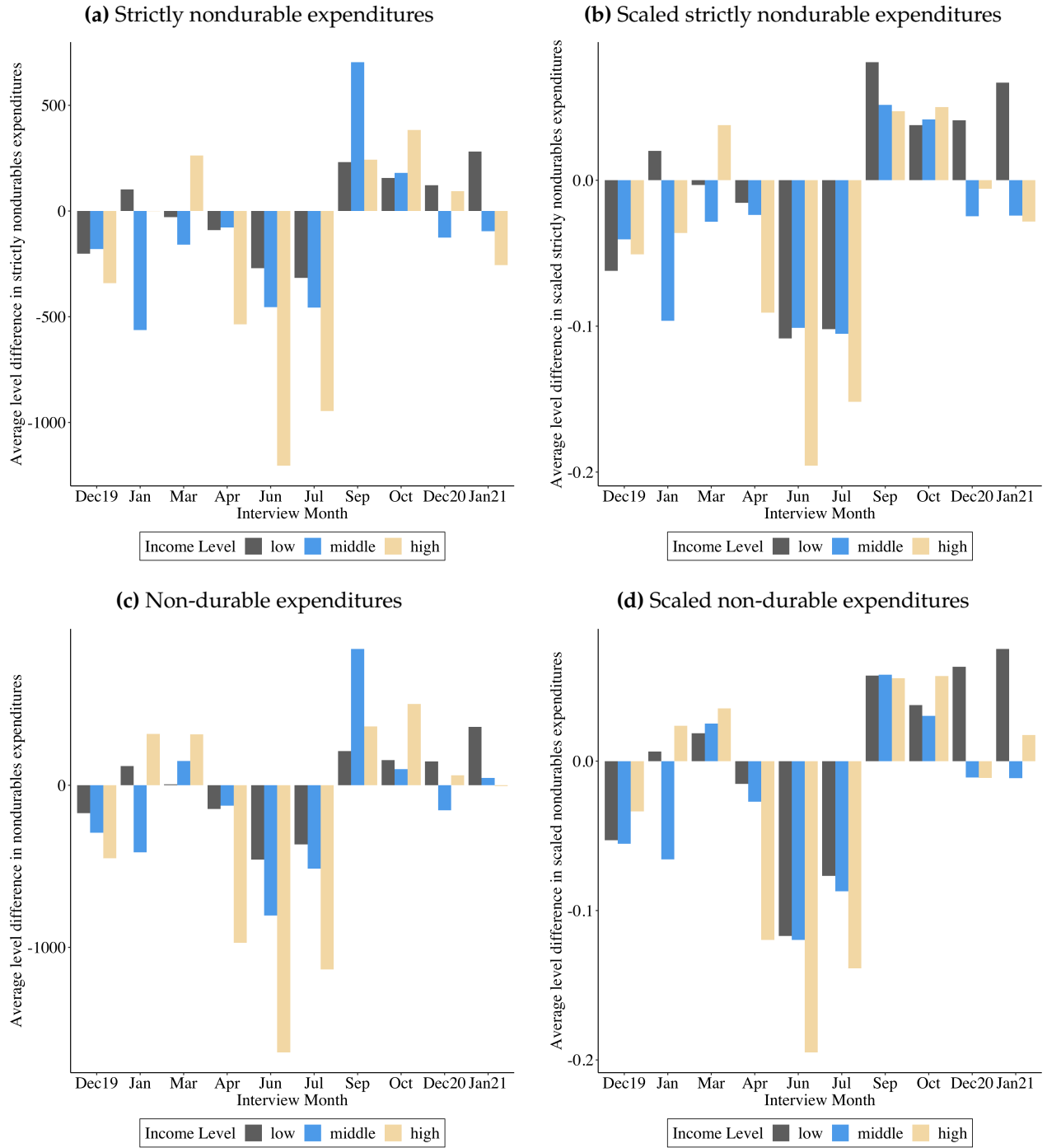
**Figure C.1:** Average change in non-durable expenditures among all CE households



*Note:* CE data, the sample of all households used in Table 6. Each income group contains one-third of the sample. Averages are unweighted.



**Figure C.2:** Average change in scaled non-durable expenditures in the final sample



*Note:* CE data, the final sample of all households used in Table C.8. Each income group contains one-third of the sample. Averages are weighted.