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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.

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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>

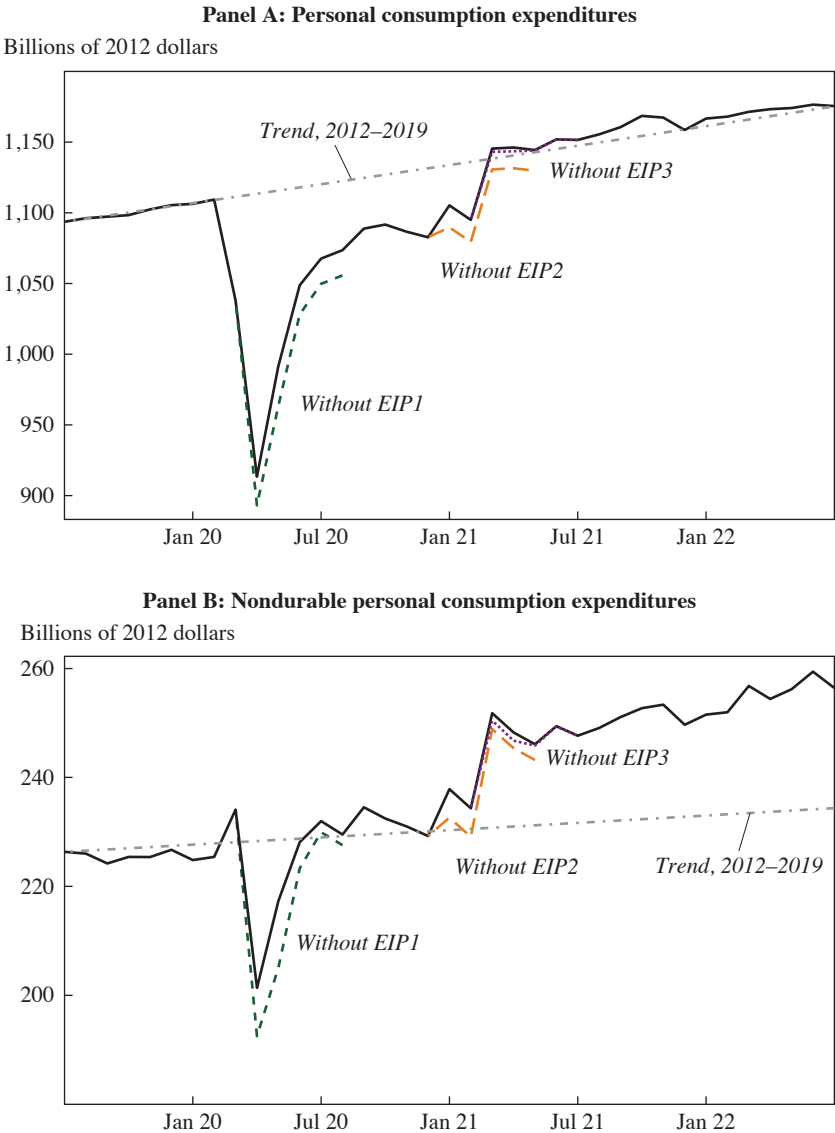
Particularly for the second and third round of EIPs, these low spending responses are also consistent with households on average already having plenty of liquid funds. As the constraints on spending relaxed, the pandemic reduction in spending coupled with other government support (e.g., the paycheck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

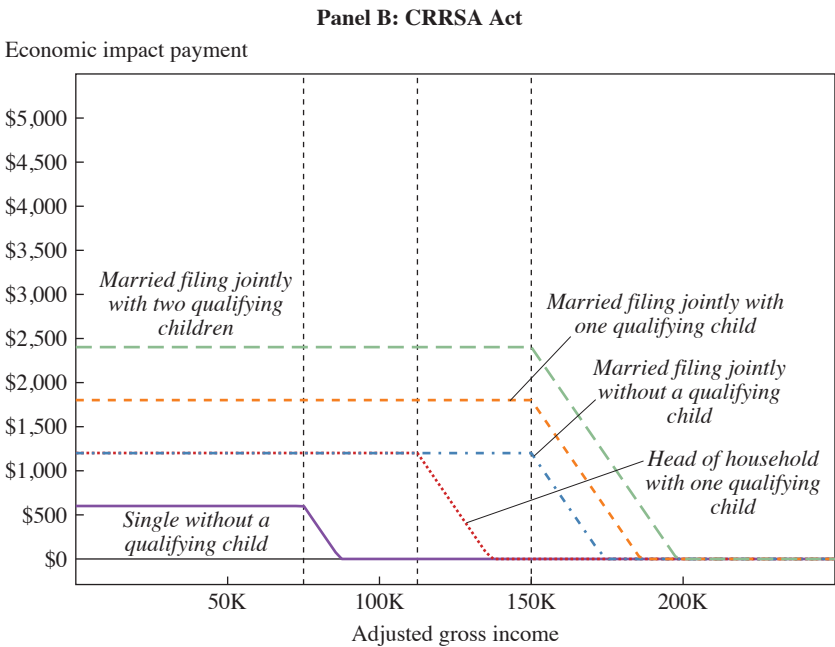
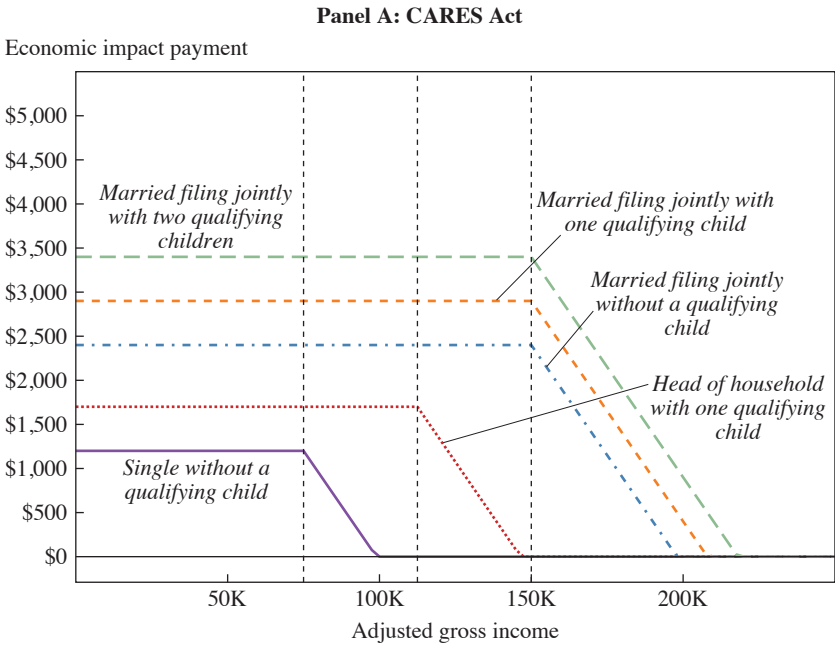
In addition to households receiving different amounts of EIPs, households also received them at different times. In each round, most taxpayers who had included their bank information when filing a recent tax return (e.g., for a refund) received their EIP during the first week of disbursement. For EIP1,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

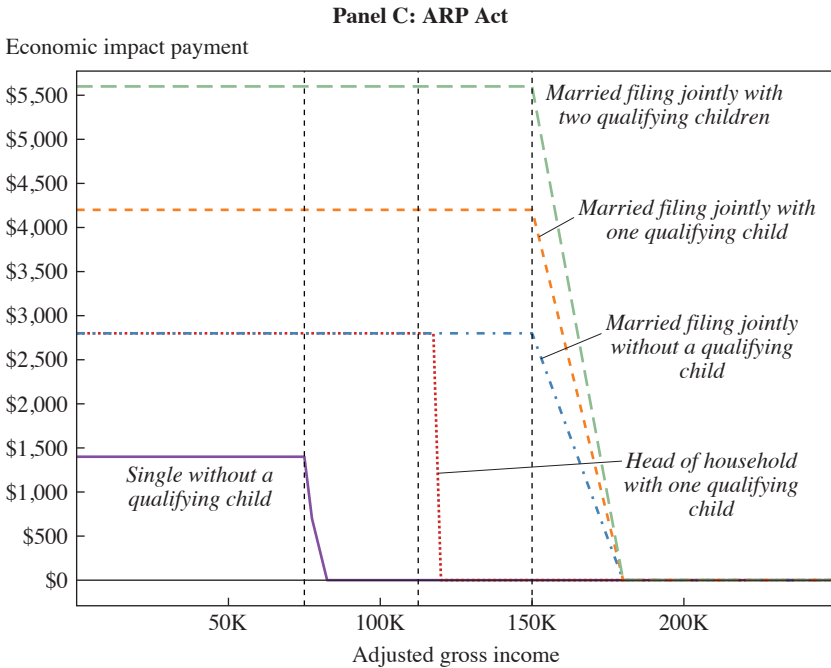
19. If a 2020 tax return had not yet been filed, then 2019 AGI from the 2019 tax return filed in 2020 was used instead.

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*)



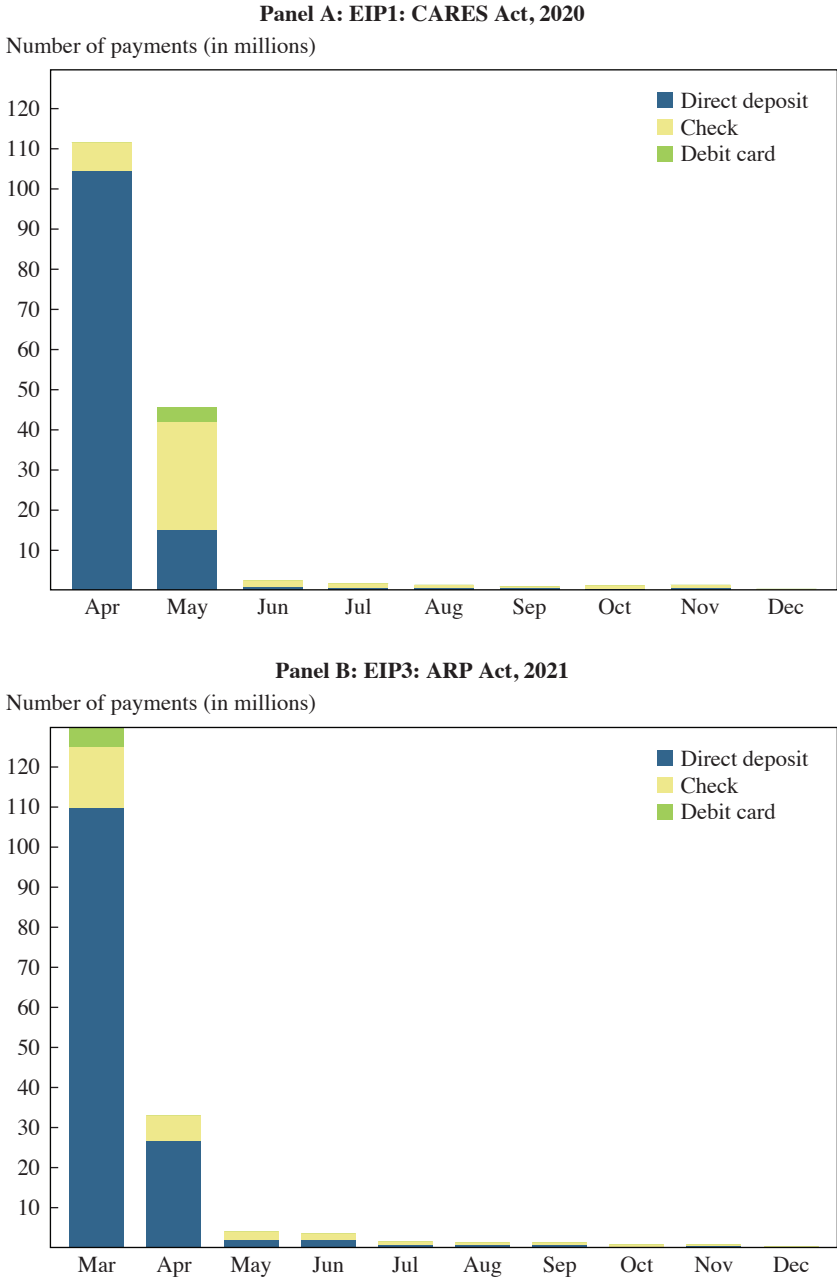
Source: Authors' calculations.

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. Armanier 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[EIP1 > 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[EIP2 > 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[EIP3 > 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)	0.247 (0.090)	0.009 (0.018)	0.015 (0.043)
$EIPh_{t-1}$	-0.011 (0.020)	-0.017 (0.070)	0.030 (0.038)	0.107 (0.124)	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)	0.601 (0.257)	-0.030 (0.047)	-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, 3,474, 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.

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55. Sahm (2021) debates these issues.

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