Household Spending Responses to the Economic Impact Payments of 2020: Evidence from the Consumer Expenditure Survey*

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December 2021

Abstract

Using the Consumer Expenditure Survey and variation in amount, receipt, and timing of receipt of Economic Impact Payments (EIPs) authorized by the CARES Act, this paper estimates that people spent less of their EIPs in the few months following arrival than in similar previous policy episodes and than estimated by existing studies using other types of data. Accounting for volatility during the pandemic and comparing the consumer spending behavior of broadly similar households, people spent roughly 10 percent (standard error 3.4) of their EIPs on non-durable goods and services in the three months of arrival, with little evidence of additional spending in the subsequent three months or on durable goods. People who report mostly spending their EIPs spent 14.3% (3.7) of their EIPs compared to 5.9% (8.3) and -1.6% (5.0) for those who report mostly paying off debt and saving respectively. People with low liquid wealth and people receiving their EIPs on debit cards spent at higher rates: 21.7% (6.4) and 36.8% (24.6) respectively, with economically larger estimates for total spending.

JEL codes: E21, E62, H31, D91

Keywords: Consumption, tax rebates, economics impact payments, Life-Cycle model, Permanent-Income Hypothesis, liquidity constraints, fiscal policy

*Jianmeng Lyu provided truly excellent research assistance. For useful discussions we thank Peter Ganong, Thesia Garner, Jianmeng Lyu, and Claudia Sahm. Parker: JAParker@mit.edu; Schild: Schild.Jake@bls.gov; Erhard: Erhard.Laura@bls.gov; Johnson: johnsods@umich.edu.

Following the rapid spread of COVID-19 in the United States, the President declared a national emergency on March 13, 2020. To address the resulting severe downturn in economic activity, Congress passed the Coronavirus Aid, Relief, and Economic Security (CARES) Act at the end of March. The Act authorized $2.2 trillion of spending on programs that included the disbursement of $300 billion in one-time Economic Impact Payments (EIP) to the vast majority of Americans. These payments were larger than previous tax rebates that the government implemented in 2001 and 2008, previous periods of economic distress. The 2020 economic situation was also quite different in comparison. In 2001 and 2008, tax rebates were disbursed as the economy entered what then appeared to be mild recessions, driven primarily by a decline in the stock market and a slowdown in the housing sector respectively. The government referred to these prior rebates as 'stimulus,' and encouraged people to spend their payments to help the economy. In contrast, the pandemic recession in the spring of 2020 was caused by a large collapse in both demand and supply, as people — partly at the behest of the government — cut back on both consuming and producing goods and services which risked exposure to COVID-19.

The question we address in this paper is: did these factors lead to different consumer spending responses to these EIPs disbursed in 2020 than to the economic rebates of 2001 and to the economic stimulus payments of 2008?[1] Using 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 timing of receipt, and we compare our estimated responses to those in Johnson et al. (2006) and Parker et al. (2013) which employ the same dataset and similar methods. We have three main findings.

First, consistent with the decline in the ability to spend during the pandemic, the CE data show low spending responses relative to tax rebate programs in 2001 and 2008. Ninety-five percent confidence intervals imply that people increased their spending by between 3 and 16 percent of their EIP on (CE-measured) non-durable goods and services during the three month CE reference period during which the EIP arrived. This marginal propensity to increase consumer spending (MPC) is substantially smaller than found both in studies of previous tax rebate programs and in studies of this EIP program using aggregated data or information on select populations although, as we discuss just below, these latter studies primarily measure the response of total (out-of-pocket) spending. However, we find similar point estimates — that 8-11% of EIPs are spent — when we also include either spending on

[1] There were two later rounds of EIPs that we do not yet have the data to analyze, a second round starting at the very end of December 2020 and mainly disbursed in January 2021, and a third round mainly distributed in March 2021.

durable goods or spending during the subsequent three month period, only with larger standard errors in each case.

Second, we find significant differences in spending across households. Consistent with a role for liquidity constraints, households with little ex ante liquidity spent more of their EIPs. Specifically, households in the bottom third of the distribution of liquid wealth – those with less than $3,000 available – spent 20 to 30 percent of the EIPs during the three months of arrival. Also possibly indicative of low income and assets, point estimates imply that the 2% of recipients who received the EIPs on debit cards – people without bank information on file with the IRS or who received their EIPs on federal benefit cards – spent the vast majority of their EIPs during the three months of arrival (although standard errors are large given the small sample size). These results are broadly consistent with heterogeneity in spending responses uncovered in other studies.

Finally, consistent with the Parker (2017) analysis of spending responses to tax rebates in 2008, the self-reported use of the EIP is highly informative about differences in spending rates across household estimated using our revealed-preference method. We find that households that self-reported mostly using their EIPs for expenses spent 14.3% (3.7) of their EIPs on non-durable goods and services. On the one hand, they do not appear to consume most of their EIPs. On the other hand, they spent at more than double the rate of households who reported mostly using their EIPs to pay off debts (5.9% (8.3)), and at a much higher average rate than households who reported mostly saving their EIPs (-1.6% (5.0)).

While we use the same approach and the same CE Survey as the cited previous studies, our identification of the effect of the EIPs is based on slightly different variation because of differences between the EIPs and previous tax rebates. In order to provide timely pandemic insurance to people economically impacted by the pandemic and not covered by other programs, the EIPs were distributed both rapidly and broadly, implying less variation in timing of receipt and in receipt status. In terms of timing, almost half of the EIPs were disbursed by direct deposit during the week of April 10, and about 90% of EIPs were distributed by the end of May. This variation in timing was not random but was driven by administrative and informational issues. In terms of receipt status, there were two primary determinants of whether a household received an EIP in 2020 or not: 3.8% of eligible households did not receive an EIP in 2020 because the IRS did not have the necessary information to disburse their EIP, and 16% of tax units were not eligible for an EIP because their incomes were too high or they did not meet the citizenship requirements (e.g., a couple with one non-citizen spouse that filed jointly; see Sections 1 and 2).

While there are fewer non-recipients and less variation in timing than in past tax rebate programs, the EIPs of 2020 had more variation in dollar amount. Partly this variation reflects differences in family structure. The EIPs consisted of a basic payment of $1,200 per adult and an additional $500 per child, so that a two-parent, two-child, family of four typically received $3,400. Partly this variation reflects differences in income levels. EIPs were reduced by 5% of adjusted gross income above $150,000 for married couples filing jointly, above $112,500 in the case of a head of household, and above $75,000 for individuals using any other filing status.

What accounts for the lower rate of spending relative to previous lump-sum tax rebates? On the one hand, the pandemic provides a natural explanation. Consumption in many sectors was constrained by the disease and by government restrictions which, together with diminishing marginal utility on unaffected goods and services, could have held back the overall consumption response to the payments. Indeed, Guerrieri et al. (2020) make this assumption to study the macroeconomic consequences of the pandemic. Because the pandemic hit household incomes extremely unequally, it is also possible that the pandemic caused larger differences across households in spending responses than in previous recessions.[2]

On the other hand, our estimated spending rates are below those of existing studies that use account-level data on financial transactions to measure the spending response of households to the EIPs. Specifically, Karger and Rajan (2021) and Baker et al. (2020) find that people spent 46% and 25-35% of their EIPs respectively within a few weeks of receipt. So was this time different or were there substantial propensities to spend out of EIPs?

One reason that Karger and Rajan (2021) and Baker et al. (2020) report larger spending responses than found in the CE is that both papers analyze populations that are likely to have larger spending responses than average: the accounts used in the former are skewed heavily towards lower income households, and the households in the latter are those that have opted to use a financial app designed to help them save. A second possibility for differences between findings is the different ways in which the studies measure consumer expenditures: financial transactions vs. recalled consumer expenditures. For example, a potential explanation for some of the difference could be debt payments that are mis-characterized as consumption-related, such as payments to un-linked credit cards or transfers to pay loans or unpaid balances from other merchants. Alternatively, respondents

[2]For households who did not lose income, the spending responses to liquidity would be significantly lower than if there were no constraints on consuming, while for households who did lose income, the severity of the recession may have caused liquidity constraints to bind more tightly and thus raised spending responses. Cajner et al. (2020) and Cox et al. (2020) document the large diversity in outcomes.

in the CE survey could forget to report EIP-induced purchases.

A third distinct possibility is that the differences arise from statistical issues, primarily the statistical uncertainty inherent in any estimator. The CE is a small dataset, with a similar number of recipients to that in Baker et al. (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. Another possibility related to statistical issues is bias in our estimator. Specifically, a number of recent papers have raised concerns with estimation of treatment effects from variation in the timing of treatment (e.g. Borusyak and Jaravel, 2018). Because our estimator uses other sources of variation, these critiques do not directly apply to our analysis, as we discuss at the end of Section 4, but we conduct a bootstrap evaluation of our estimator and find that it is essentially unbiased in the face of substantial heterogeneity in spending responses across households. With substantial additional variation in the distribution of spending responses (MPCs) across months of receipt, our estimator has some bias, but not in any particular direction and far too small to close the gap between estimates across papers.[3]

The final possibility for differences in findings is differences in specification or variation in the data that identifies the spending response. Our present study and the other studies of the 2020 EIPs all use similar specifications and variation, although at different time horizons, making specification an unlikely explanation for difference among studies of the 2020 EIPs. However, the studies of different tax payment programs in different years use different sources of variation to identify spending effects. These differences are an unavoidable consequence of differences in the distribution of payments across episodes, as discussed above, and are a possible cause of differences in estimated rates of spending if some sources of variation are not valid for identifying the spending effect of the arrival of a payment.

This paper builds on a significant literature on the response of consumer expenditures to lump-sum tax payments.[4] Most studies of the spending response to previous tax payments have estimated the response to payments using variation in spending between recipients and non-recipients (e.g. Bodkin, 1959; Agarwal and Qian, 2014; Kueng, 2018), over time

[3]If average spending rates actually decline by a quarter over the first two months, then our estimated average spending rate would be biased down by 5%, suggesting the best estimate of actual spending would be higher by roughly 0.5% of the EIP. If average MPCs instead doubled over the first two months, then the best estimate of the true rate of spending would be 13% lower than our estimate (i.e. an MPC of 9% instead of 10%). Thus, this evidence suggests our estimator is not substantially downward biased.

[4]There is also an enormous literature on the response of consumer spending to various types of variation in income and liquidity which we do not attempt to review here.

(e.g. Souleles, 1999; Parker, 1999; Stephens, 2003; Farrell et al., 2019; Baugh et al., 2020), and using randomization in policy (Agarwal et al., 2007; Broda and Parker, 2014; Parker, 2017; Lewis et al., 2021, in addition to those already cited).[5] But unlike the previous disbursement in the US in 2001 and 2008, the timing of the CARES Act EIPs was not in any way randomized across households, and the variation in timing that does exist is quite limited because the majority of EIPs were disbursed in April. Because of this, studies of the spending response to the 2020 EIPs – including this study and Baker et al. (2020) and Karger and Rajan (2021) – focus on comparing spending before receipt to spending after receipt, comparing spending between recipients to non-recipients, and comparing households receiving different sized EIPs.[6]

The first rapid analysis of the spending changes caused by the EIPs, Meyer and Zhou (2020), used Bank of America transactions data and reports large increases in aggregated card spending on the day of and the day following receipt of an EIP associated with bank accounts that received EIPs on April 15 (when over 40% of EIPs were disbursed) relative to those that did not. Daily spending increased by an average of 50% year over year between April 15 and 16 for households with incomes below $50,000 and by only 3% for households with incomes above $125,000. Also using aggregated data, Chetty et al. (2021) finds that over this same couple of days, card spending in zip codes in the bottom quarter of the distribution of average household income rose by 25% while those in the top quarter of the distribution rose by only 8%. Finally, also using zip code level data and using incidental differences in timing in EIP disbursements across zip codes, Misra et al. (2021) infers an MPC of 50% in the few days after an EIP arrives.

In addition, a number of papers measure how people report that they will use or did use their EIPs.[7] Most reliably, Sahm et al. (2020) reports the results of a survey of households that asks identical questions to those asked of households following the 2008 stimulus payments. In response to the EIPs, 18% of respondents report that their EIPs will cause them to "mostly increase spending," only one percent lower than in 2008. This is slightly higher than Coibion et al. (2021) which finds that only 15 percent of households in the Nielsen Consumer Panel report that they mostly spent or expect to spend their EIPs. Among these households, the average spending rate is 40%. Armantier et al. (2020) finds a slightly larger number in the New York Fed Survey of Consumer Expectations survey

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

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

[7]These papers build on similar studies of past tax rebate programs: Shapiro and Slemrod (1995, 2009); Sahm et al. (2010, 2012).

in which the average household reports consuming 29% of its EIPs. Garner et al. (2020) and Boutros (2020) provide in depth analysis of the U.S. Census Bureau's Household Pulse Survey (HPS) in which 59% of respondents state that they "will mostly pay for expenses" with their EIPs. Finally, in the CE Survey, which we analyze in Section 6, about 58% of households report that they have or will mostly "pay for expenses" with their EIPs. In 2008, the CE survey asked questions more similar to those in Sahm et al. (2020) and found that 32% of households would "mostly spend" their tax payments.

1 The Economic Impact Payments

The CARES Act of March 2020 authorized the disbursement of "recovery rebates" – which quickly became known as Economic Impact Payments (EIPs). We organize our description of this EIP program 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 source of variation are exogenous, and we explore estimates based on different combinations of these in our analysis. As noted, we focus solely on this first round of EIPs (until we have data that cover the second round of EIPs, disbursed starting December 30, 2020 and during January 2021, and the third round, disbursed in March 2021 respectively).

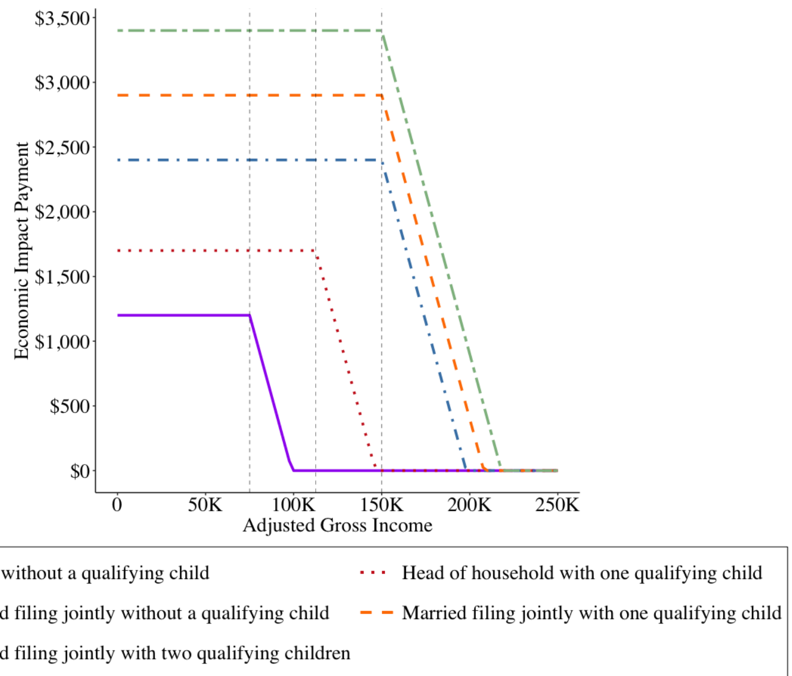
In terms of amount, the EIPs consisted of a base payment of $1,200 for an individual, $2,400 for a couple filing jointly, and additional payments of $500 for each qualifying dependent under age 17. The Act set upper income thresholds of $75,000 for an individual, $112,500 for a head of household, and $150,000 for couples filing jointly for receiving the full payment amount, where income was 2019 adjusted gross income (AGI) if the taxpayer had already filed their 2019 tax return in 2020, otherwise income was 2018 AGI as reported in 2019 tax filings.[8] For every $100 of adjusted gross income over the threshold the stimulus payment was reduced by $5.[9] Figure I displays these amounts as a function of income for various family structures.

In terms of timing, every taxpayer who had filed a 2018 or 2019 tax return and had

[8]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.

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

Figure I: CARES Act Economic Impact Payment Amounts as a Function of AGI and Family Structure

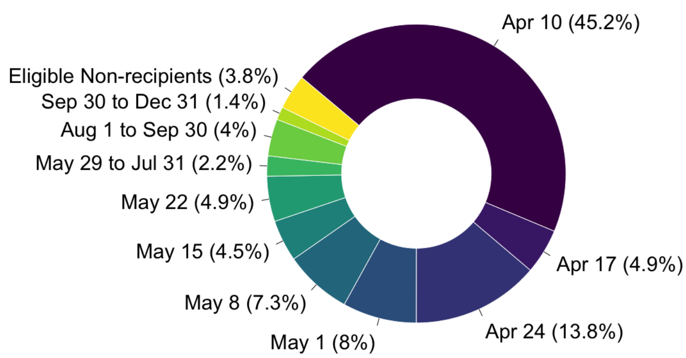


included their bank information (e.g., for a refund) received an EIP by direct deposit into their bank account. The IRS also launched a web page where households could enter their information for the IRS if they either had omitted bank information from their returns or were eligible but had not filed 2018 or 2019 returns (roughly 35 million households).[10] The IRS also collected information on eligible households from the Social Security Administration and the Veterans Administration (and the Railroad Retirement Board). Using the information that the IRS was able to gather and process in time, roughly 80 million or just under half of all EIPs were directly deposited into recipients' accounts on April 15, 2020. For eligible households without the necessary bank information, the EIPs arrived starting two weeks after April 10 by mailing a paper check (22% of EIPs) or pre-paid "EIP" card (2% of EIPs) (Internal Revenue Service, June 28 2021). Among paper checks, the Treasury tried to disburse the EIPs to lower income households first. This process resulted in a steady flow of EIPs distributed over the 6 weeks following the first main disbursement (until late May), and then a continuing trickle over the summer. Figure II.a shows the distribution of EIPs over time and shows that 3.8% of eligible taxpayers had not received their EIPs by the end of 2020. Figure II.b shows a substantial variation in timing even among EIPs disbursed to recipients who filed tax returns.

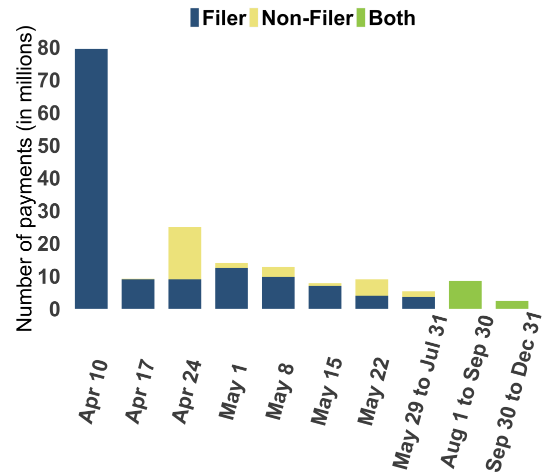
[10] IRS web page "Get My Payment;" https://www.irs.gov/coronavirus/get-my-payment (downloaded

Figure II: The disbursement of the EIP payments over time during 2020

(a) Share of Total Eligible for EIP



(b) Number of Payments by Filing Status



Source: Figures adapted from Murphy (2021) based on US Department of the Treasury (2020). Dates are disbursement dates.

Finally, what determines whether a household did or did not receive an EIP? There are two primary sources of non-receipt. First, families were ineligible if they had filed jointly and one of the spouses was not a US citizen, a situation affecting an estimated 15.4 million people (Murphy, 2021). Second, eligible households would not receive an EIP during 2020 if they had changed accounts and/or addresses, 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). Four months after the CARES Act (by the end of July), 10 percent of EIPs had not been disbursed, and 5 percent or nine million eligible households had not received an EIP by the end of September (Murphy, 2021). There is also a third reason that households would not receive an EIP: as noted, EIP amounts declined to zero as income increases; thus high-income households were not eligible (and are not included in the above numbers).

In aggregate $271 billion was disbursed during 2020 (Internal Revenue Service, June 28 2021). This amount 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. About $260 billion worth of EIPs were disbursed in the second quarter of 2020, which corresponds to about 5.3 percent of GDP or 8.0 percent of PCE in that quarter

October 2021)

(Figure II and Internal Revenue Service, May 22 2020). The next section describes the EIPs as recorded in our CE dataset.

2 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 that collects spending, demographics, and other financial information for households living in the U.S. The CE is structured so that a consumer unit (CU) at a given address, which we will refer to as households, is interviewed up to four times at three month intervals about their spending over the previous three months (”reference period”). New CUs are added to the survey every month, and while a significant dollar share of spending data is reported at the monthly level, a little over half of spending is only reported for the entire three-month reference period. Thus, we use the data at the (overlapping) three-month frequency. Because a small share of EIP payments authorized by the Coronavirus Response and Relief Supplemental Appropriations Act of 2021 were disbursed in December 2020, and because we do not have sufficient data (yet) to study the effect of these second-round EIPs, we drop all interviews taking place after December 2020. 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[11], similar to those used in 2008 which were added to the CE questionnaire. 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). The questions were phrased to be consistent with the style of other CE questions and the questions on previous CE surveys about the 2001 and 2008 tax rebates. Although the wording did not follow exactly previous CE surveys, the module of questions also asked whether the EIP was used mostly to add to savings, mostly to pay for expenses, or mostly to pay off debt. Appendix A.1 contains the language of the CE survey instruments.

The fact that the EIP questions were not included in the May interview questionnaire means that we have very little power to identify the impact of the arrival of the EIP on spending using only variation in the timing of receipt across households. The vast majority

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

of EIPs 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 EIP is received in April or May. Since only about 10% of EIPs arrive after May, we focus primarily on analysis that (also) leans on other sources of variation, like amount and recipient status.[12]

A second implication of the lack of EIP questions on the May survey is that we have no way to tell whether households interviewed in May received the EIP or not during the previous three months. The reference period for the May 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 construct two main samples of CE households. First, we construct a sample we refer to as *all households* that makes minimal additional drops and follows exactly earlier analyses of tax rebates in the CE. Details are provided in Appendix A.5.3. Second, motivated both by the unprecedented nature of the pandemic and programmatic differences between the EIPs and previous tax rebates, we construct our *final sample* by adjusting the way in which older households and households with very low levels of reported expenditures are dropped and dropping high income households who are mostly ineligible for EIPs (see details in Appendix A.5.3 and Table C.3). We discuss these choices in detail in the next section.

Tables I and II show that the distribution of EIPs reported in the CE line up reasonably well with official statistics. The monthly variation in EIP receipt is shown in panel A of Table I. The first two columns show statistics for our final sample (which drops high-income households as described subsequently); the second two columns show statistics for the

[12]We investigated measuring the spending response to the EIP 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).

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

	Unweighted CE final sample	Weighted CE final sample	Unweighted raw CE (adjusted)	Weighted raw CE (adjusted)	Census Bureau's Household Pulse Survey and U.S. Treasury
Panel A: The distribution of EIPs across months, in percent					
April	53.9	54.7	52.9	53.8	66.4
May	36.1	35.3	35.1	34.2	25.7
June	7.5	7.6	8.8	8.9	1.1
Later Months	2.5	2.4	3.2	3.1	6.8
Panel B: Percent of households or tax units not receiving an EIP					
Total (households)	18.8	18.8	24.1	24.2	
Ineligible (tax units)					16.0
Eligible (tax units)					3.2

Notes: Statistics based on 'CE final sample' include 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, and cleaning described in the Appendix A.5.3. 'Raw CE' is based on all 2020 interviews for all households with a May, June, or July interview and is adjusted by *i*) scaling up the number of EIPs received in April by 50% to account for the 1/3 of households (those interviewed in May) not asked about receipt of EIPs in April; and *ii*) assigning these fictional EIPs to May interviews randomly when counting non-recipients. Weights are the average of FINLWT21 across all interviews for that CU. 'Later months' is July to November 2020 (inclusive) for the CE samples and July to December 2020 in the last column. In Panel A, 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 et al. (2020) and eligible households not receiving payments are counted through October 2020 as reported in Murphy (2021).

raw CE data including all interview months. April data for the raw CE sample is adjusted up by fifty percent to account for our dropping one third of recipients, those interviewed in May when the EIP questions were not asked. The CE data have slightly fewer EIPs 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).[13]

Columns 3 and 4 of Panel B in Table I show that 24% of households do not receive an EIP according to the CE data compared to 20% in reality (3.2% of households were eligible tax units who were non-recipients in 2020, and 16% of households were not eligible for

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

Table II: EIP amounts in the CE Survey

Panel A: Distribution of EIP amounts		
EIP value	Number of Observations	Percentage
$EIP = 0$	472	18.3
$0 < EIP < 1200$	93	3.6
$EIP = 1200$	757	29.4
$1200 < EIP < 1700$	42	1.6
$EIP = 1700$	44	1.7
$1700 < EIP < 2400$	107	4.2
$EIP = 2400$	621	24.1
$2400 < EIP < 2900$	31	1.2
$EIP = 2900$	104	4.0
$2900 < EIP < 3400$	21	0.8
$EIP = 3400$	72	2.8
$3400 < EIP < 3900$	89	3.5
$EIP = 3900$	41	1.6
$EIP > 3900$	81	3.1
Total	2575	100
Panel B: Average EIP amount		
	Unweighted	Weighted
Average EIP amount:	$1,952	$1,971

Notes: 2020 Consumer Expenditure Survey (BLS). Statistics based on our final sample which includes only CE household with an interview in June or July 2020, with income that does not exceed a certain threshold determined by marital status and family structure, and cleaning described in the Appendix A.5.3. Weights applied are average CU weights across reference periods. EIP values are the total amount received by a household in the 3-month reference period, as in the main regressions, and counts are un-weighted sums. The average EIP amounts are conditional on receiving an EIP.

EIPs). In our final CE dataset, about 19% of households are non-recipients because we drop households with high incomes (as noted on page 18).

Table II shows the distribution of total EIPs received across household-reference-periods in our CE final sample (unweighted, unadjusted). The average value of EIPs received in a reference period, conditional on a positive value, is $1,952 (median $1,700), slightly higher than the average individual EIP of $1,676 reported by the IRS (Internal Revenue Service, June 28 2021).[14] Consistent with the payments specified by CARES, most reported EIPs are at the base amounts or in multiples of $500 above them: about 55% of households report payments of $1,200 (the basic payment for a single filer) or $2,400 (a couple filing separately or getting the basic payment as joint filers or a single filer with two children).

[14] When using all CE data, and without aggregating to the three-month reference period level, the average (unweighted, unadjusted) EIP is $1,837.

According to the IRS, there were 162 million first-round EIPs disbursed in 2020 totaling $271 billion (Internal Revenue Service, June 28 2021). In the weighted, aggregated CE data, and scaling up for the interviews missing we find 141 million EIPs totaling $263.4 billion (139 million EIPs totaling $259.5 billion after we drop high-income households).[15] Households that receive EIPs by direct deposit on average have slightly higher expenditures, are slightly younger, have higher incomes, higher liquidity, and have larger EIPs, than households that receive the payments by mail.

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 semi-durable categories like apparel, reading materials, and health care (only out-of-pocket spending by the household) following previous research using the CE survey; 4) total expenditures, which adds durable expenditures such as home furnishings, entertainment equipment, and auto purchases.[16]

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. Figure III.a shows that 75% of EIPs in the CE were reported as being received by direct deposit, 23% by paper check, and 2% by debit card.[17] The Treasury reports that 76% of EIPs were disbursed by electronic deposit, 22% by paper check, and 2% by debit card during 2020.[18] Though there were no explicit instructions, CE respondents likely reported EIPs that were deposited onto federal benefit cards (Direct Express Cards) as received by debit card, and while directly comparable numbers from the Treasury are not available, through June 2020, 3% of EIPs had been distributed by debit card and an additional 1% by deposit onto benefit cards (Murphy, 2021).

In terms of reported use of EIPs, the CE survey shows much greater usage for spending than in 2008.[19] Figure III.b shows that people report that 56% of EIPs are mostly used to cover expenses, 26% are mostly added to saving, and only 18% are mostly used to pay off

[15] The lower number in the CE is in small part a result of not including information from CE interviews after December 2020.

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

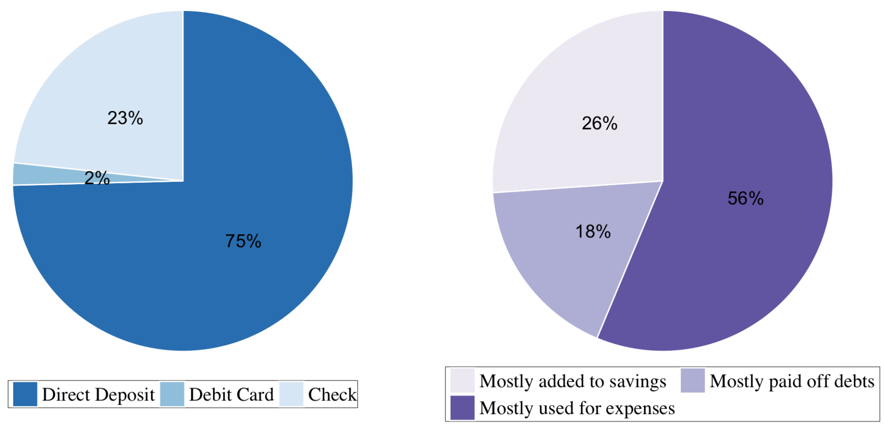
[17] These are fractions of EIPs and are very similar to the fractions of household-interviews reporting receiving an EIP by each method shown in Table VII.

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

[19] Schild and Garner (2020) provide a detailed analysis of the responses to the same questions in the Household Pulse Survey.

Figure III: The share of EIPs by method of disbursement and reported main use in the CE survey

(a) Distribution of payment methods **(b)** Distribution of reported main use



Source: 2020 Consumer Expenditure Survey. The sample is the final sample which includes only CE household with an interview in June or July 2020, with income that does not exceed a certain threshold determined by marital status and family structure, and cleaning described in the Appendix. Weights applied are average CU weights across reference periods.

debt. In 2008, households reported only 32% of EIPs were mostly saved and 51% were used mostly to pay down debt. These numbers are not strictly comparable due to wording changes in the survey, but the change is larger than seem possible from just wording changes.

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 counterparty, or, for some transactions like checks or cash, just the amount.[20] Second, the CE tracks spending and EIP receipt by individual consumer units, rather than by accounts (and linked credit or debit cards). Finally, the CE is a stratified random sample of U.S. households constructed by the U.S. Census and so when weighted is representative of the US population (along the dimensions of the census-based strata and conditional on participation in the survey). The main weaknesseses relative to existing studies are the relatively small sample size,

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

sampling (e.g., non-response) error, and the presence of measurement error in expenditures and EIP receipt.

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

3 Preliminary estimates using the previous methodology

In this section, we first briefly describe the estimation method used by Johnson et al. (2006) and Parker et al. (2013) to estimate the spending responses to the tax rebates disbursed in 2001 and 2008. Second, using this methodology, we replicate Tables 2 and 3 in both papers for the 2020 EIPs and show that estimated spending responses are small, statistically weak, and inconsistent across specifications. Finally, we argue that this instability and lack of power are due to both economic and programmatic differences between previous episodes and this one, and describe how we refine this methodology and adopt identifying assumptions that are better suited to these EIPs and to the pandemic situation. Section 4 shows that the resulting estimates are more consistent across specifications than the estimates in this section and imply small spending responses to the EIPs.

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

$$\begin{Bmatrix} \Delta C_{i,t} & \text{or} \\ \Delta \ln C_{i,t} & \end{Bmatrix} = \sum_{s=0}^{S} \beta_s \begin{Bmatrix} EIP_{i,t-s} & \text{or} \\ \mathbb{1}[EIP_{i,t-s} > 0] & \end{Bmatrix} + \gamma_1 age_{i,t} + \gamma_2 \Delta FamSize_{i,t} + \tau_t + \epsilon_{i,t} \qquad (1)$$

where $\Delta C_{i,t}$ (or $\Delta lnC_{i,t}$) represents change in consumer expenditures (or change in log of consumer expenditures) between t and $t-1$, $EIP_{i,t-s}$ is the total dollar amount of payments received by household i in period $t-s$, $\mathbb{1}[]$ is an indicator function, and τ represents a complete set of time effects for every period in the sample, used to absorb the seasonal variation in consumer expenditures as well as the average effect of all other concurrent aggregate factors. Both $age_{i,t}$ and $\Delta FamSize_{i,t}$ control for the life-cycle pattern of spending and for changes in consumption needs. Finally, ϵ captures movements in consumer expenditures due to individual-level factors such as changes in income, expectations, and consumption needs, as well as measurement and recall error in expenditures.

Provided ϵ is uncorrelated with the other right-hand-side regressors, the key coefficient β_s measures the average partial-equilibrium response of household consumer expenditures to the arrival of the EIP during the three-month period s periods after the EIP arrives. When $EIP_{i,t-s}$ is regressed on ΔC, β_s measures the share of the EIP spent, or the marginal propensity to increase consumer spending (MPC). When $\mathbb{1}[EIP_{i,t-s} > 0]$ is regressed on ΔC, β_s measures the dollars spent. And when $\mathbb{1}[EIP_{i,t-s} > 0]$ is regressed on ΔlnC, $100 * \beta_s$ measures the percent increase in consumer spending.

Using ordinary least squares (OLS), the estimated β's 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. We also estimate equation 1 by two-stage least squares (2SLS), treating $EIP_{i,t-s}$ as endogenous and using $\mathbb{1}[EIP_{i,t-s} > 0]$ as the instrument, so as not to use variation in EIP amount to identify the spending effect. All inference is conducted clustering residuals at the household level to allow arbitrary heteroskedasticity and within-household serial correlation in the residuals.

Table III presents the results of estimating equation 1 on the sample of all CE households and shows that while the point estimates imply substantial spending responses to the EIPs, many are statistically insignificant and they imply quite different spending behavior across different specifications and identifying variation.[21] Panel A of Table III presents estimates from regressing EIP on ΔC in equation 1 with $S = 0$. Point estimates suggest MPCs of 4.2% on food, 7.0% on strictly non-durable goods, 7.6% on the broad measure of non-durable goods, and 28.4% on all goods and services. None of these estimates are statistically significant. Panel B shows estimates using an indicator variable for receipt in place of EIP amount and implies that, in the three months in which the EIP arrives, spending increases by \$149 on food, \$289 on strict non-durables, \$370 on non-durables, and \$1,307 on all goods and services, with all but the first being statistically significant. For the average EIP, these estimates would imply MPCs of 7%, 14%, 18%, and 63% respectively, roughly double those from estimating the MPC directly in panel A.[22]

Panel C displays estimates of the percent change in consumer expenditures during the three months in which an EIP arrives. These estimates suggest that spending on food increases by 2.5 percent (food) during the three months of EIP arrival, and all other categories increase by 1.5 percent, but like the first set of estimates in Panel A, are all

[21] These reported contemporaneous responses do not change much when we include lagged EIP, as we report in Appendix Table C.1.

[22] Conditional on a positive $EIP_{i,t}$, the unweighted average $EIP_{i,t}$ in this sample is \$2,072.

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

	Food and alcohol	Strictly Nondurables	Nondurable goods	All CE goods and services	Food and alcohol	Strictly Nondurables	Nondurable goods	All CE goods and services
Est. method	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
	Panel A. MPC. Dependent variable: ΔC				*Panel B. Dollars spent. Dependent variable: ΔC*			
EIP	0.042	0.070	0.076	0.284				
	(0.032)	(0.044)	(0.059)	(0.217)				
I(EIP)					148.8	288.9	370.4	1306.9
					(89.6)	(129.8)	(167.1)	(644.9)
	Food and alcohol	Strictly Nondurables	Nondurable goods	All CE goods and services	Food and alcohol	Strictly Nondurables	Nondurable goods	All CE goods and services
Est. method	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS
	Panel C. Pct change in spending. Dependent variable: $\Delta \ln C$				*Panel D. MPC. Dependent variable: ΔC*			
EIP					0.073	0.141	0.181	0.639
					(0.044)	(0.063)	(0.082)	(0.315)
I(EIP)	2.47	1.47	1.52	1.49				
	(3.01)	(2.00)	(1.93)	(2.33)				

Notes: Table reports β_0 from estimation of equation 1 with $S = 0$. The coefficients in panel C are multiplied by 100 so as to report a percent change. Regressions also include interview month dummies, age, and change in the size of the CU. The sample is the sample of all CE households with an interview in June or July 2020 and is otherwise constructed as in previous research papers (see Appendix). Standard errors included in parentheses are adjusted for arbitrary within-household correlations and heteroskedasticity. In 2SLS regressions, EIP indicator, together with control variables, are used as instruments for the EIP amounts. All regressions have 5,240 observations except for the first two columns of panel C which have 5,226 and 5,235, respectively.

statistically insignificant. Given average EIPs and average spending on each category these estimates imply MPCs of 2.8%, 3.3%, 4.5%, and 10.2%, roughly half or less of those in Panel A.[23] The estimates in Panel D use the indicator of EIP receipt as an instrument for the amount, and so, like the first set of numbers in panel A, are estimates of MPCs, but do not use differences in the amount of the EIP across households to identify them. These final estimates are generally statistically significant, and imply MPCs of 7% on food, 14% and 18% on the two nondurable measures, and 64% on total, roughly double the estimates in Panel A (but similar to those in Panel B). While these estimates are not dissimilar from the MPC estimates in Parker et al. (2013) (8% on food, 13% on nondurables, and 52% on

[23]Conditional on receiving an EIP, the unweighted average expenditures over a three-month period are \$2,308, \$4,622, \$6,233, and \$14,005 on food, strictly nondurables, nondurables, and all CE goods and services respectively.

total), they have less statistical significance and are not consistent with the other panels of Table III.

In sum, the estimates in Table III are statistically weak and unstable. Having much greater statistical uncertainty than those observed in previous analyses of tax rebate programs is probably a result of both the smaller sample size and the pandemic, which increased the volatility of expenditures over time for most households. The resulting instability is consistent with substantial statistical uncertainty, but may also be due to important differences both between previous tax rebate programs and this one, and between previous recessions and the pandemic recession. Specifically, there are two serious concerns about the validity of the estimation strategy that generate the estimates in Table III in this context.

Our first concern, common to analyses of previous tax rebate programs, is whether or not a household gets an EIP is not randomly assigned. Johnson et al. (2006) and Parker et al. (2013) proceeded to drop all non-recipients and estimate the MPC using only quasi-random variation in timing across recipients. Unfortunately, unlike earlier disbursements, the timing of the disbursement of the EIPs was not randomized in any way. As described in Section 1, there was also much less variation in timing in 2020 and even less measured in the CE (for the reasons described in Section 2). For completeness, we present results of this exercise in Appendix Table C.2 under the maintained assumption that the timing variation is as good as random, but, as expected given the lack of variation, the standard errors are typically 50% to 100% larger than in Table III. Additionally, the estimates are more variable and many are negative; so, we learn little from this exercise.

Thus, rather than dropping non-recipients from our analysis, we take steps to make the sample of non-recipients more similar to recipients by excluding households with high incomes from our analysis. Motivated by the phase-out of the EIPs described in Section 1, we posit an income cutoff at the nearest $25,000 above the income level (rounded to the nearest 50,000) at which a household would no longer receive an EIP, based on whether the household contains children and whether it has one adult, a married couple, or multiple adults. However, recipient status is not a clean function of CE income because EIPs are disbursed based on adjusted gross income rather than the pre-tax income we observe in the CE, because reported income has some error, and because the IRS uses calendar year income for either 2018 or 2019 and neither year nor filing status is observed in the CE. Thus, we adjust each income cutoff up in increments of $25,000 until more than one third of the CE households with incomes in the $25,000 range just above the cutoff are non-recipients (or there are no recipients). Table C.3 shows the resulting cutoffs and the number of recipients in the $25,000 income ranges above and below each cutoff. This process omits a few recipients. 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 this as our *final sample* and it is this sample that is tabulated in Section 2.

Our second concern is related to the fact that the early stages of the pandemic were a time of unprecedented consumption volatility during which people with different levels of consumer expenditures had vastly different dollar changes over time. As we show in Appendix Figures C.1.a and C.1.c, households with higher incomes have much larger changes in dollar spending on average during the pandemic period. 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 likely create bias in the estimates in Table III in Panels (A, B, and D) in which the dependent variable is in dollars (ΔC). For example, if there are different large changes in dollar spending in April, 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 the estimates of Table III would be inconsistent.[24]

However, groups of people with different incomes – and so with different average levels of consumption – experienced roughly similar percent changes in consumer spending over time (e.g., see Cox et al., 2020). We find for example that differences across terciles of the income distribution are lower in the log specification of Table III (see Appendix Figures C.1.b and C.1.c). Thus instead of estimating equation 1, we scale the time dummies by average spending, so that the time dummies capture the average percent change in spending in each three month period rather than the dollar change. To do this, we divide the dependent variable and all regressors other than τ_t by the average spending amount for each household. In this specification, the time effects absorb the average percent change in consumer expenditures in each period. We confirm that in the CE survey, the average percent change in spending measured in this way is significantly more similar for households across terciles of standards of living as measured by their average level of expenditure (compare Appendix Figures C.2.a to C.2.b and C.2.c to C.2.d).[25]

[24]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 in the equivalent of Table III.

[25]We make three other choices that differ slightly from previous analyses. As in previous papers, we drop the bottom 1% of the distribution in broad non-durable consumer expenditures, but instead of estimating

Based on these arguments, we scale the variables in our regression by $\bar{C}_i$, the average consumer expenditure (of each type) for family i. Letting $\tilde{X}_{i,t} = X_{i,t}/\bar{C}_i$ and $R(i)$ be an indicator variable that equals one for households that receive at least one EIP, we estimate the following equation for each of the four different measures of consumer spending that we use:

$$\Delta\widetilde{C}_{i,t} = \sum_{s=0}^{S} \beta_s \left\{ \begin{matrix} \widetilde{EIP}_{i,t-s} \quad \text{or} \\ \widetilde{\mathbb{1}[EIP_{i,t-s} > 0]} \end{matrix} \right\} + \gamma_1 \widetilde{age}_{i,t} + \gamma_2 \Delta \widetilde{FamSize}_{i,t} + \tau_t + \alpha_{R(i)} + \epsilon_{i,t} \qquad (2)$$

The main coefficient of interest, β_s, still measures the propensity to spend out of an EIP, but by scaling all variables we have transformed the τ from absorbing the average change in dollar spending across households in that period to absorbing the average percent change in consumer expenditures across households in that period. The term $\alpha_{R(i)}$ allows a different average growth rate of expenditure between recipients and non-recipients.

Finally, to better approximate the average response, we estimate the equation using 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.

4 The average MPC in response to the arrival of an EIP

This section shows that people increased their spending by 5-10 percent of their EIP during the three months of arrival. Taking the perspective of classical statistics, the 95% confidence intervals of cumulative spending rule out spending in excess of 26% of the EIP on nondurable goods and 42% on all CE goods and services over the three to six months following EIP receipt. These estimated average spending responses are small, relative both to past rebates and to other estimates on other populations in the literature.

We present estimates of equation 2 on our main sample in Panel A of Table IV, which shows that during the three-month reference period in which a payment was received, a household on average increased its spending on food by a statistically significant 6 percent of the EIP received and on nondurable goods by 10 percent of the EIP.[26] In contrast, the

the bottom one percent using a quantile regression on a linear trend, we drop the bottom 1% of change in non-durable expenditure 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, who are about 5% of the sample. Finally, we choose to follow Panel A of Table 3 in Parker et al. (2013) rather than Table 2, which means allowing a different average growth rate of expenditure between recipients and non-recipients. Our estimates are largely insensitive to these three choices.

[26]We again follow the literature by estimating equation 2 initially only including the contemporaneous EIP

Table IV: The contemporaneous response of expenditures to EIP receipt

	Food and alcohol	Strictly Nondurables	Nondurable goods	All CE goods and services	Food and alcohol	Strictly Nondurables	Nondurable goods	All CE goods and services
Est. method	WLS	WLS	WLS	WLS	WLS	WLS	WLS	WLS
	Panel A. Dependent variable $\Delta\widetilde{C}$; MPC				*Panel B. Dependent variable $\Delta\widetilde{C}$; Dollars spent*			
$\widetilde{EIP}$	0.062 (0.013)	0.033 (0.025)	0.099 (0.034)	0.081 (0.075)				
$\widetilde{I(EIP)}$					99.4 (46.5)	8.2 (46.8)	88.8 (57.4)	141.8 (152.6)
	Food and alcohol	Strictly Nondurables	Nondurable goods	All CE goods and services	Food and alcohol	Strictly Nondurables	Nondurable goods	All CE goods and services
Est. method	WLS	WLS	WLS	WLS	2SLS	2SLS	2SLS	2SLS
	Panel C. Dependent variable $\Delta \ln C$; Pct change in spending				*Panel D. Dependent variable $\Delta\widetilde{C}$; MPC*			
$\widetilde{EIP}$					0.052 (0.018)	0.006 (0.034)	0.066 (0.042)	0.099 (0.107)
$I(EIP)$	0.26 (3.99)	2.15 (2.67)	2.91 (2.57)	-0.65 (3.20)				

Notes: Panels A, B, and D report β_0 from estimation of equation 2 with $S = 0$; Panel C reports β_0 from equation 1 with $S = 0$ and a separate intercept for non-recipients. The coefficients in panel C are multiplied by 100 so as to report a percent change. Regressions also include interview month dummies, scaled (except Panel C) age and change in the size of the CU, and a separate intercept for non-recipients. 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. Weights applied are average weights. Standard errors included in parentheses are adjusted for arbitrary within-household correlations and heteroskedasticity. In 2SLS regressions, scaled EIP indicator, together with control variables, are used as instruments for the scaled EIP amounts. For Panel A, Panel B, and Panel D, all regressions have 5,314 observations except for the first columns that have 5,309 observations. For Panel C, the four columns have 5,286, 5,306, 5,312 and 5,314 observations, respectively.

estimates of the MPCs on strictly nondurable goods and total spending are both relatively small and statistically insignificant. However, even these estimates are still informative as the upper bound of the 95% confidence interval for the MPC on total expenditures is 22.8% ($\approx 8.1\% + 1.96 \times 7.5\%$).

The remaining panels of Table IV show similar MPCs or implied MPCs for other specifications that are largely statistically insignificant from zero, which reject large spending responses. Panel B shows that, in the three-month period of EIP receipt, spending rises by \$99 for food and statistically insignificant amounts (\$8, \$89, and \$142) for the three increas-

($S = 0$) because, as in the previous literature, including more lags does not change the inferences that one makes (as we show in Table V).

Table V: The longer-run response of expenditures to EIP receipt

	Dependent variable: scaled dollar change in spending on							
	Food and alcohol	Strictly Nondurables	Nondurable goods	All CE goods and services	Food and alcohol	Strictly Nondurables	Nondurable goods	All CE goods and services
Estimation method	WLS	WLS	WLS	WLS	2SLS	2SLS	2SLS	2SLS
$\widetilde{EIP}_t$	0.058	0.045	0.095	0.100	0.049	0.025	0.063	0.114
	(0.014)	(0.025)	(0.033)	(0.075)	(0.019)	(0.034)	(0.043)	(0.116)
$\widetilde{EIP}_{t-1}$	-0.042	0.038	-0.081	-0.142	-0.027	0.039	-0.057	-0.152
	(0.023)	(0.009)	(0.038)	(0.088)	(0.030)	(0.009)	(0.050)	(0.136)
	Implied cumulative fraction of EIP spent over two three-month periods							
	0.074	0.128	0.109	0.058	0.072	0.090	0.069	0.077
	(0.037)	(0.052)	(0.073)	(0.182)	(0.051)	(0.071)	(0.105)	(0.314)

Notes: Table reports β_0 and β_1 from estimation of equation 2 with $S = 1$. Regressions also include interview month dummies, scaled age and change in the size of the CU, and a separate intercept for non-recipients. 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. Weights applied are average weights. Standard errors included in parentheses are adjusted for arbitrary within-household correlations and heteroskedasticity. In 2SLS regressions, scaled EIP indicators, together with control variables, are used as instruments for the scaled EIP amounts. All regressions have 5,309 observations except for the first and fifth columns that have 5,314 observations.

ingly broader categories. Given the (weighted) average $EIP_{i,t}$ conditional on receiving an EIP is \$2,088, these point estimates imply MPCs of 4.8%, 0.4%, 4.3%, and 6.8% respectively. Panel C reports estimates of equation 1 on this sample. None are statistically significant.[27] Finally, Panel D of Table IV uses only variation in receipt and not the dollar amount of payments received, and finds MPCs and standard errors similar to those in Panel A.

While we find that people spend only a small fraction of their EIPs during the three months in which they arrive, do they spend measurably more in the subsequent three-month period? We only find evidence of continued higher spending for strictly nondurable goods. Table V presents the results of estimating equation 2 with $S = 1$. The coefficient β_1 on $E\tilde{I}P_{i,t-1}$ measures the decline in spending during the three-months following receipt, so that $\beta_0 + \beta_1$ measures the increase in spending in the second three months relative to prior to receipt. 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 period of receipt and the subsequent period (as a percent of the EIP). The cumulative MPC on food is 7.4 % and on strictly non-durable goods is 12.8% while the rest

[27] At the (weighted) average EIP and spending on each set of goods and services conditional on a positive $EIP_{i,t}$ (\$2,249, \$4,477, \$5,999, and \$13,544), these point estimates imply MPCs of 0.3%, 4.6%, 8.4%, and -4.2% respectively.

of the MPCs are statistically insignificant but rule out large cumulative spending responses over three to six months; the upper bound on the 95% confidence interval is 25.3% of the EIP ($\approx 10.9\% + 1.96 \times 7.3\%$) for cumulative spending on nondurable goods and is 41.5% ($\approx 5.8\% + 1.96 \times 18.2\%$) for cumulative spending on all CE goods and services.

A possible concern with the results so far is that they identify the MPC in part from variation in timing of receipt. A recent literature has shown that in certain circumstances estimated average treatment effects can be biased when identified from variation in the timing of treatment (Borusyak and Jaravel, 2018; Goodman-Bacon, 2021; Sun and Abraham, 2021; Callaway and Sant'Anna, 2021). Our approach is unlikely to suffer from these problems not only because we estimate a dynamic model in a circumstance in which treatment effects likely go to zero, but also because we identify the MPC from variation in EIP amount (treatment intensity) and from the different behavior of recipients and non-recipients as well as timing of treatment. Nonetheless, we perform a bootstrap evaluation of our estimator assuming a data generating process based on our main estimates (Table V column 3) as described in Appendix B. Our estimator of the MPC has trivial bias and excellent coverage when MPCs are assumed to be either homogeneous or uniformly distributed between zero and twice our estimated MPC. When we suppose that the true distribution of treatment effects also rises significantly over time — doubling between April and May (and continuing to increase), we find that our estimator is biased slightly upwards by about 12.7% (i.e. $E[\hat{\beta_0}] = 1.13$), so the true spending response would be smaller than we estimate on average. On the other hand, if MPCs falls significantly over time (starting large and declining by a quarter between April and May), our estimator would be biased slightly downward by about 4.5% ($E[\beta_0]$ or $= 0.95E[\beta_0]$).[28] These simulations suggest our estimator is not significantly downward biased.[29]

What do people spend their EIPs on? Estimates of spending on broad categories of goods and services are typically more precise than estimates of spending on narrow categories because broad categories average the differences in spending across types of goods that are due to idiosyncratic reasons that people might buy more of some type of good at one time rather than another (e.g., replacing a broken semi-durable or celebrating a special occasion). However four subcategories show significant spending responses

[28] As described in Appendix B, we draw β_s for CUs who first received their EIPs in April from $U(0, \hat{\beta}_s)$, in May from $U(0, 2\hat{\beta}_s)$, in June from $U(0, 3\hat{\beta}_s)$, and later from $U(0, 4\hat{\beta}_s)$. We reverse this pattern when we assume that MPCs decline over time. When MPCs vary over time, we compare the bootstrap distribution of $\hat{\beta}_0)$ to the simple average of β_0 across all simulations.

[29] In addition, we find little heterogeneity in MPC by month. Estimating equation 3 with S=1 and with g as month of arrival, we find very similar MPCs for April and May, 12.6% (4.4)) and 12.3% (5.1) respectively (and large standard errors for all later months, all exceeding 14).

(standard errors in parentheses): the MPC on food at home is 5.2% (1.9), the MPC on gas, fuel and public transportation is 2.1% (0.8), the MPC on apparel is 2.2% (0.5), and the MPC on entertainment is 3.5% (1.2).[30]

5 Differences in spending responses across households

In this section we show that, consistent with a role for liquidity constraints, households with little ex ante liquidity spent more of their EIPs.[31] Also indicative of low income and assets, the 2% of recipients who received the EIPs on debit cards – people without bank information on file with the IRS – spent the vast majority of their EIPs during the three months of arrival, although with large standard errors due to the small sample size.

We estimate different MPCs for different groups of recipients by interacting the EIP variables in equation 2 with a group-membership indicator variable, denoted $g(i)$. We use the equation:

$$\Delta\widetilde{C}_{i,t} = \sum_{s=0}^{S} \beta_{g(i),s} \left\{ \begin{matrix} \widetilde{EIP}_{i,t-s} \quad \text{or} \\ \widetilde{\mathbb{1}[EIP_{i,t-s} > 0]} \end{matrix} \right\} + \gamma_1 \widetilde{age}_{i,t} + \gamma_2 \Delta \widetilde{FamSize}_{i,t} + \alpha_{g(i)} + \tau_t + \epsilon_{i,t} \qquad (3)$$

where $g(i)$ is the group to which household i belongs so that the spending response of interest varies by group as well as horizon ($\beta_{g(i),s}$) and so that the intercept or average growth rate of spending also differs by group ($\alpha_{g(i)}$).

We begin by investigating differences in spending propensity by age, by income, and by liquidity. We are primarily interested in whether MPCs are larger for households with low liquidity. But while liquidity in the CE is better measured than it used to be, it is still measured imperfectly and is still missing for a non-trivial share of our sample (partly due to the fact that we do not yet have complete data for 2021). Thus, we also study differences across age groups because young households are more likely to be liquidity constrained due to higher expected income growth and little savings, while older households are less likely to be constrained because they are likely to be retired and so at lower risk of losing labor income. We also study differences by income (annual income during the 12 months prior to the first interview). Low income households may be more likely either to be the

[30]See Appendix Table C.5. In previous rebate programs, the increases in personal care and miscellaneous expenses was disproportionately large, but the MPC in 2020 is -0.1% in 2020. In 2008, the MPC on transportation (mainly purchases of new vehicles) was disproportionately large, and here is it -0.4% (1.6).

[31]Although they look at the second round of EIP's, Schild and Garner (2020) use HPS data to show households reporting higher levels of financial difficulty are more likely to use their EIP mostly for spending.

Table VI: The propensity to spend by age, income, and liquidity

	Dependent variable: scaled dollar change in spending on								
	Panel A: By age			*Panel B: By income*			*Panel C: By liquidity*		
	Food and alcohol	Nondurables	All CE goods and services	Food and alcohol	Nondurables	All CE goods and services	Food and alcohol	Nondurables	All CE goods and services
	Age Bottom third: ≤ 49 Top third: ≥ 65			Income Bottom third: $\leq 38,122$ Top third: $\geq 87,852$			Liquidity Bottom third: $\leq 3,000$ Top third: $\geq 16,000$		
$\widetilde{EIP}_t$	0.040 (0.027)	0.129 (0.060)	0.202 (0.118)	0.041 (0.025)	0.102 (0.051)	0.073 (0.101)	0.046 (0.040)	0.117 (0.084)	0.089 (0.204)
$\widetilde{EIP}_t \times$ *Bottom third*	-0.004 (0.035)	-0.028 (0.066)	-0.048 (0.132)	0.025 (0.027)	-0.030 (0.058)	0.028 (0.108)	0.061 (0.047)	0.101 (0.087)	0.235 (0.206)
$\widetilde{EIP}_t \times$ *Top third*	0.026 (0.028)	-0.102 (0.065)	-0.189 (0.121)	-0.008 (0.032)	-0.061 (0.064)	-0.092 (0.160)	0.025 (0.040)	-0.170 (0.093)	-0.349 (0.226)
p-value for test of equality of responses	0.461	0.172	0.201	0.421	0.635	0.720	0.384	0.002	0.009
	Implied spending by group								
Bottom third	0.036 (0.028)	0.102 (0.040)	0.154 (0.099)	0.067 (0.013)	0.073 (0.039)	0.101 (0.078)	0.106 (0.033)	0.217 (0.064)	0.324 (0.138)
Top third	0.066 (0.012)	0.027 (0.040)	0.013 (0.082)	0.033 (0.027)	0.041 (0.052)	-0.019 (0.149)	0.071 (0.007)	-0.054 (0.069)	-0.259 (0.189)

Notes: All regressions use equation 3 and so also include 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, and, for Panel C, with valid liquidity. 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 heteroskedasticity-robust F-test for H_0: $\beta_{0,Bottom\ third} = 0$ and $\beta_{0,Top\ third} = 0$. For Panel A and B, all regressions have 5,314 observations except the first columns that have 5,309 observations. For Panel C, all columns have 2,485 observations.

type of household that spends out of income or to have temporarily low income and so be more likely to be liquidity constrained. On the other hand, they could simply be retired with no labor income.

Table VI shows little evidence that MPCs differed across either the age or income distribution (within our sample of mostly recipients). While point estimates suggest that a slightly lower MPC for households 65 or older, one cannot reject the hypothesis that all three thirds of the age distribution have the same MPC. Measuring income as pre-tax income in the 12 months before the first CE interview, we find statistically significant spending response for the bottom and middle thirds of the income distribution in our sample, of 10 percent and 13 percent of the EIP on nondurables respectively. And while point estimate for the top third of the income distribution is an MPC of only 3%, one again cannot reject that all three groups have the same MPC.

However we do find evidence of higher spending among the households with the least liquid wealth. We measure liquid wealth as the sum of balances in checking accounts, saving accounts, money market account, and certificates of deposits at the start of the households first interview (reported in the last interview). Households in the bottom third of the distribution of liquidity – those with less than $3,000 available, which is still a substantial amount – have statistically significant MPCs of 11%, 22%, and 32% on food, nondurable goods, and all CE goods and services respectively (Table VI Panel C). 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, we can reject the equality of MPCs across these three groups for spending on both non-durable goods and all CE goods and services.[32] While previous research on tax rebates in the CE Survey has only sometimes found a statistically significant differences related to liquidity, analyses with better measures of liquidity have generally also found a larger MPC for households with lower liquidity (e.g. Parker, 2017; Olafsson and Pagel, 2018; Ganong et al., 2020; Baugh et al., 2020; Fagereng et al., 2021).

Another way to measure low liquidity is to look at households who received EIPs on debit cards. There are two reasons that a household would receive an EIP on a card rather than as a direct deposit or check. First, some EIPs were deposited onto federal benefit cards (Direct Express Cards), which are debit cards issued by the government that allow people to receive and spend benefits like Social Security without needing a bank account. Given the CE questions, we expect that a household receiving an EIP by this means would likely report it as received by card, but of course they might instead report it as received by direct deposit. The second reason that a household would receive an EIP by card is that the IRS did not have their bank account information and instead sent them an EIP debit card as "...part of Treasury's U.S. Debit Card program, which provides prepaid debit card services to federal agencies for the electronic delivery of non-benefit payments" (Internal Revenue

[32] Even the low liquidity group has substantial reported wealth, and in particular the distribution of reported liquid wealth is much higher in this 2020 data than it was in 2008. In Parker et al. (2013) the 33rd percentile in the distribution of liquid wealth was only $500. One possibility is changes in the distribution of respondents, although this appears unlikely as we discuss in Appendix A.4. More likely, this difference reflects changes in the CE Survey and the financial accounts that it covers. While in 2008 the CE asked about balances in checking and saving accounts separately, in 2013 the CE survey switched to asking a single question about total liquidity across a larger set of types of accounts, and starting in 2017 the survey introduced an initial question asking whether there was a zero balance in these accounts. The latter change was associated with a reduction in the number of households reporting zero balances. In Table VI, 54% of the sample does not have valid liquidity information, compared to 41% in the 2013 paper, this difference also reflects the fact that we do not yet have access to the liquidity question for households whose final interview is in 2021.

Table VII: The response of expenditures to EIP receipt by disbursement method or reported main use

Panel A. By disbursement method				*Panel B. By reported main use*			
	Food and alcohol	Nondurable goods	All CE goods and services		Food and alcohol	Nondurable goods	All CE goods and services
$\widetilde{EIP}_t$ *by direct deposit* *(75% of recipients)*	0.012 (0.020)	0.100 (0.039)	0.109 (0.090)	$\widetilde{EIP}_t$*, used for expenses* *(58% of recipients)*	0.067 (0.010)	0.143 (0.037)	0.189 (0.086)
$\widetilde{EIP}_t$ *by check* *(25% of recipients)*	0.071 (0.008)	0.077 (0.044)	0.025 (0.094)	$\widetilde{EIP}_t$*, paid off debt* *(17% of recipients)*	-0.011 (0.034)	0.059 (0.083)	0.057 (0.149)
$\widetilde{EIP}_t$ *by debit card* *(2% of recipients)*	-0.059 (0.156)	0.368 (0.246)	0.816 (0.489)	$\widetilde{EIP}_t$*, added to savings* *(27% of recipients)*	0.025 (0.025)	-0.016 (0.050)	-0.066 (0.105)
p-value for test of equality of responses	0.014	0.463	0.229		0.032	0.012	0.061
	Implied cumulative fraction of EIP spent over two three three-month periods						
$\widetilde{EIP}$ *by direct deposit*	-0.036 (0.050)	0.110 (0.087)	0.100 (0.208)	$\widetilde{EIP}$*, used for expenses*	0.096 (0.036)	0.193 (0.083)	0.221 (0.200)
$\widetilde{EIP}$ *by check*	0.134 (0.034)	0.097 (0.104)	-0.118 (0.241)	$\widetilde{EIP}$*, paid off debt*	-0.101 (0.076)	-0.042 (0.173)	-0.016 (0.363)
$\widetilde{EIP}$ *by debit card*	-0.310 (0.440)	0.461 (0.603)	0.880 (0.996)	$\widetilde{EIP}_t$*, added to savings*	0.008 (0.064)	-0.072 (0.124)	-0.172 (0.294)
p-value for test equality of responses	0.006	0.837	0.484		0.046	0.362	0.686

Notes: All regressions use equation 3 and so also include interview month dummies, scaled age and change in the size of the CU. In addition, Panel A regressions include separate intercepts by reported method of disbursement (and combination), and Panel B regressions include separate intercepts by reported main use (and combination). The sample is the final sample which includes only CE household with an interview in June or July 2020, with income that does not exceed a certain threshold determined by marital status and family structure, and with valid reported usage of EIP(s). 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 heteroskedasticity-robust F-test for H_0: $\beta_{0,g} = const$. In Panel A, the first column has 5,261 observations while the last two columns have 5,266. In Panel B, the first column has 5,230 observations while the last two columns have 5,233.

Service, May 18 2020).[33] We observe in the CE data more than a third – but not much more than a third – of the households who report receiving an EIP by card are in the bottom third of the distribution of income, and similarly for the bottom third of the distribution of liquidity.

Table VII Panel A shows economically significant larger spending responses by households who got their EIPs by debit card. While the spending responses of households

[33]These cards were mailed starting in late May to recipients whose tax returns were processed by either the Andover MA or Austin TX IRS Service Center.

receiving EIPs by check and by direct deposit are similar, the point estimates show that the 2% of recipients who received the EIPs on debit cards spent 37% of their EIPs on our broad definition of non-durable goods and services during the three months of arrival and more than 80% of their EIPs on all CE goods and services. The small number of households who get their EIPs by debit card means that the spending response is not statistically significantly different from the spending responses to EIPs disbursed by other means, but the point estimate provides corroborating evidence that households with low liquidity spent more of their EIPs.

One might be concerned that what the debit card is picking up is a later receipt of EIP in time. The EIPs disbursed by debit card were disbursed later. If MPCs were rising with time, then EIPs disbursed by card would have higher MPCs than average. However, EIPs disbursed by check were also disbursed later than those sent out by direct deposit and show no greater spending propensity. Thus, the large MPC found for households who received their EIP by debit card seems unlikely to be due to a different timing of disbursement rather than low liquidity.

6 Reported spending and revealed spending

Our previous estimates lie solidly within the revealed-preference approach in which inferences about causal effects are drawn from people's actions in different situations. As noted in the Introduction, there is a large literature devoted to measuring the spending responses to tax rebates using surveys that ask people to report their spending behavior in response to their receipt of a tax rebate. This alternative methodology requires that people correctly infer and report the casual effect, the difference between what they actually did and what they would have done in a different hypothetical situation. We find that, consistent with Parker and Souleles (2019), the self-reported use of the EIP is highly informative about the household's actual spending response to the EIP.

Focusing on the contemporaneous three-month response of spending both on food (including alcoholic beverages) and on nondurable goods, Table VII Panel B shows that households that self-reported mostly using their EIPs for expenses are the only group that spent a statistically significant amount of their EIPs during the three months of receipt. These "spenders" spent at more than double the rate of households who report mostly saving or paying off debts during the three months of arrival on nondurables, and more than three times the rate on all CE goods and services. We can reject the equality of these spending responses across the three groups at the 3% and 1% levels for food and non-durable goods respectively, but only at the 6% level for all CE goods and services.

7 Concluding remarks

What are the main lessons from the findings in this paper? First, the 2020 time period does appear different from the 2008 and 2001 periods of economic distress. The pandemic limited the types of goods and services that one could spend on and many households reduced spending. There were also other policy responses, including extended and expanded unemployment insurance, and the Paycheck Protection Program that transferred money to small and medium sized businesses with some incentives to maintain payroll, both of which were intended to help offset any lost income. Finally, the depth and duration of the pandemic was uncertain when this first round of EIPs were being disbursed. These factors appear to have led to less spending out of the EIPs than out of the tax rebates in 2001 and 2008.

Were the EIPs effective? The goal of previous tax rebates programs was to increase demand and so their efficacy is largely related to the speed and size of the spending responses. In contrast, the policy goal of the EIPs was insurance, this 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. As such, the EIP program should not be considered ineffective simply because consumers spent their EIPs slowly or saved them as insurance against even worse personal economic conditions. The EIPs could have filled spending needs beyond the time horizon accurately measured by this (and other) studies. However, the small spending response that we find is consistent with one of the main criticisms of the program: that it was poorly targeted because a majority of its funds went to people who were not made financially worse off by the pandemic, such as retirees (e.g. Sahm, 2021, discusses these issues).

In addition to these broad lessons about tax rebate policy, we also confirm two important general points about consumer behavior and data respectively. First, spending responses to the liquidity provided by the EIPs are higher for households with fewer ex ante liquid assets, consistent with the modelling of consumer behavior in leading macroeconomic models. Second, the reports people provide of how they used their EIPs is highly informative, consistent with the findings of Parker and Souleles (2019) and with the increasing use of similar self-reported causal response data in research and in the evaluation and formulation of policy.

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On-line Appendix for

Household Spending Responses to the Economic Impact Payments of 2020: Evidence from the Consumer Expenditure Survey

by

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January 4, 2022

A Further information about the CE and our use of it

A.1 The CE survey instruments

The following questions were asked in all CE interviews during 2020 starting in June:[1]

In response to the coronavirus, the Federal government began sending stimulus payments, that is the coronavirus (COVID-19) related economic impact payment, directly to many households, either by check, direct deposit, or debit card. Since the first of (reference month), have (you/you or any members of your household) received a stimulus payment from the Federal government? Do not include refunds on annual income taxes, unemployment compensation, or payments from an employer.
10. Stimulus Payment
99. None/No more entries

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

In what month was the stimulus payment received? [enter text] ________
* Probe if month entered is not in the reference period.

What was the total amount of the stimulus payment? [enter text] ________
* Probe if amount is less than 100 or greater than 5000.

Was the stimulus payment received by ...
1. check?
2. direct deposit?
3. debit card?

How did or will (you/you or any members of your household) use the stimulus payment?
1. Mostly to pay for expenses
2. Mostly to pay off debt
3. Mostly to add to savings

Did (you/you or any members of your household) receive any other stimulus payments?
1. Yes
2. No
If yes, return to“Who received the stimulus payment?”

[1] The wording reflected here represents how the questions were asked in July 2020 after minor, non-substantive changes from the June wording

A.2 CE files and variables

This study uses the CE interview survey only and construct the panel primarily using data from 2019Q3 to 2020Q4 CE FMLI files and the CNT20 file.[2] All results use only public-use data (available at: https://www.bls.gov/cex/pumd.htm). In detail, the FMLI files contain CU interview month ($QINTRVMO$), calibration final weight ($FINLWT21$), demographics that broadly includes age of reference person and spouse (AGE_REF, $AGE2$), family size (FAM_SIZE), number of kids ($PERSLT18$), sex of the reference person (SEX_REF), marital status ($MARITAL1$), housing tenure ($CUTENURE$), pre-tax family annual income in the last 12 month ($FINCBTXM$), total value of liquid assets a year before ($LIQUDYR$ and $LIQUDYRX$), and category-specific expenditures in the current calendar quarter ($XXXXXXCQ$) and the previous calendar quarter ($XXXXXXPQ$).[3] The CNT20 file contains data on EIP receipt ($CONTCODE = 800$) including the amount ($CONTEXPX$), the month of receipt ($CONTMO$), the disbursement method ($CHCKEFT$, where 1 is "Check," 2 is "Direct Deposit," and 3 is "Debit Card"), and usage ($REBTUSED$, where 1 is "Mostly to pay for expenses," 2 is " Mostly to pay off debts," 3 is "Mostly to add to savings").

We process the CE data to obtain the dependent and independent variables used in the analyses. A CU's expenditure in a reference period is the sum of $XXXXXXCQ$ and $XXXXXXPQ$. The first difference in consumer expenditures is the consumer expenditures in the current reference period minus consumer expenditures in the previous reference period. For $\Delta FamSize_{i,t}$, we use FAM_SIZE to take the first difference. If $AGE2$ is not missing, we use the average of AGE_REF and $AGE2$ as the control variable $age_{i,t}$. If $AGE2$ is missing, then $age_{i,t} = AGE\_REF$. $EIP_{i,t-s}$ is the total dollar amount of payments received by household i in period $t-s$. We provide details about EIP variables and related cleaning in Appendix A.5.2.

Estimating equations 2 and 3 also require CU average weights, average expenditure, income, liquidity, and group indicators. For weights, we compute and use a CU's average $FINLWT21$ over all $FINLWT21$ assigned to the CU in its 2019 and 2020 interviews. A CU's average expenditure $(\bar{C}_i)$ is calculated similarly. For income, we consider a CU's first reported $FINCBTXM$, which reflects the CU's annual income during the 12 months prior to the first interview they appear in the sample. For liquidity, we use $LIQUDYRX$ (reported by CUs in their fourth interviews only), which measures the total value of

[2]In general, we avoid using 2021Q1 data since the IRS started to send out the second round of EIPs in late December 2020. The only case where 2021Q1 is used is Panel C of Table VI where we need CUs' liquid assets information.

[3]In comparison to $FINCBTAX$ that only uses reported income, $FINCBTXM$ uses both reported income and imputed income, and thus, has fewer missing values.

checking, savings, money market accounts, and certificates of deposits (CDs) one year before the date of the interview.[4] Due to missing data, our analyses using these variables is based on fewer observations. The non-recipient categorical variable for a CU has the value 1 if the CU never receives any EIP in the final sample, and 0 otherwise. Categorical variables for the other categories (e.g, for expenses, or by check) are constructed analogously.

A.3 Definitions of consumer spending

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

A.4 Effect of the pandemic on data quality

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

[4] Everyone who does not have such accounts ($LIQUDYR = 0$) is assigned with $LIQUDYRX = 0$. We keep valid, topcoded $LIQUDYRX$.

A.5 Further details on data processing

A.5.1 CUs in the panel

As a first step, a CU can potentially have observations in the all CE sample or final sample if it satisfies both of the following: a) the CU was interviewed in June or July and b) the CU must have at least two consecutive interviews. The first condition implies that we do not include CUs interviewed in May, since one can never know whether such a CU receives an EIP in April. The second condition is for computing the first difference. These two conditions are necessary but not sufficient for a CU to be in our samples, given that the all CE sample and final sample drops outliers (as noted in Appendix A.5.3) and the final sample also drops CUs with high income (Table C.3). For analysis of differences across households, we also drop households that do not have the information necessary to assign them to a group.

A.5.2 Cleaning EIP variables

Below are some assumptions we adopt for cleaning EIP variables. Both ii) and iii) affect very few observations, and regression results are robust if one changes the assumptions.

i) The CNT20 file contains all EIP information collected by the CE. If a CU does not have a documented EIP in the CNT20 file, there are two possibilities: the CU did not receive an EIP or the CU did not report receipt. The CE does not flag non-response regarding EIP, so one cannot distinguish the former from the latter. We assume that everybody who does not have a documented EIP in the CNT20 file did not receive an EIP. Also, we keep EIPs flagged as "Valid value; imputed or adjusted in some other way," which affects only a small number of observations in the sample. EIP is set to 0 for all months before April 2020.

ii) Where the method of disbursement of an EIP is missing, we treat it as missing.[5]

iii) Where the mode of usage is missing for an EIP, we do one of the following: a) where there is at least one other EIP reported in the same interview, and the other EIP or EIPs all have the same reported usage, we apply that usage to the missing response. b) where there are multiple other EIPs reported in the same interview with different

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

uses, we keep usage for that EIP missing. c) where there is no other EIP in the same interview, we keep usage as missing.[6]

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

A.5.3 Cleaning the sample

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

i) Drop every observation living in student housing ($CUTENURE = 6$).

ii) Drop every observation with $AGE\_REF > 85$ or $AGE\_REF < 21$; and with $AGE2 > 85$ or $AGE2 < 21$ if $AGE2$ is not missing. Keep observations that have missing $AGE2$.

iii) Drop every observation with change in $AGE\_REF > 1$ or change in $AGE\_REF < 0$, if the reference person has the same sex (SEX_REF) in the two consecutive interviews. Similarly, we drop every observation with change in $AGE2 > 1$ or change in $AGE2 < 0$, if the reference person has the same sex (SEX_REF) and marital status ($MARITAL1$). Keep observations that has missing change in $AGE2$.

iv) Drop every observation that has change in FAM_SIZE greater than 3 or less than -3.

[6] For completeness, we list all three cases. One can check that case b) does not affect the final sample.

v) Drop the bottom 1% observations with the lowest nondurable expenditures after adjusting for family size and time trend: $a)$ Compute nondurables expenditures per capita, counting kids as 0.6 adults. $b)$ Create a time trend variable by setting interview month December 2019 (the earliest interview month in our panel) as 0, January 2020 as 1, March 2020 as 3, April 2020 as 4, and so on. $c)$ Run a quantile regression of expenditure per capita on time trend for the 1st percentile. $d)$ Drop all observations with fitted values greater than the observed values (that is, all observations below the regression line).

We apply the above data cleaning procedure to the sample used in Section 3 (i.e., *all households*) to exactly follow the methodology developed by Johnson et al. (2006) and Parker et al. (2013). The three modifications we make for our *final sample* (used in Section 2 and Section 4 onward) are:

i) Modification to ii) above: We keep observations with $AGE\_REF > 85$ or $AGE2 > 85$, who are about 5% of the sample and consist of a lot of recipients.

ii) Modification to v) above: We drop the bottom 1% of the distribution of the change in nondurable consumer expenditures, but instead of estimating the bottom one percent using a quantile regression on a linear trend, we run a quantile regression of change in nondurable consumer expenditures per capita (defined as change in expenditure divided by the number of family members in the reference period) on interview month dummies, which is equivalent to dropping the bottom 1% in each interview, to account for the volatility across time during our sample due to the pandemic.

iii) In addition, we drop CUs with income above a certain threshold determined by marital status and family structure, as discussed in Section 3. Table C.3 shows the thresholds.

For details about how the data cleaning process affects the sample, see Table C.6.[7]

B Boostwtrap evaluation of our estimator

As a basis for our simulations, we estimate equation 2 with $S = 1$ using expenditures on non-durable goods and services, as in Table V column 3, but without using the CE

[7]The main results in the paper are robust under certain perturbations in the sample. For instance, when we ignore all data cleanings and do not drop CUs based on income, the results stay quantitatively close to the results from the final sample, and do not lead to different conclusions. In general, statistically significant coefficients remain significant and shift by less than 10%, and the change in standard errors are usually smaller.

weights (i.e. using OLS). We save the estimated coefficients including time fixed effects. We denote the coefficients on EIP_t and EIP_{t-1} as $\hat{\beta}_0$ and $\hat{\beta}_1$, respectively ($\hat{\beta}_0 = 0.091$ and $\hat{\beta}_1 = -0.065$). We construct a data set in which every observation in our main sample has its EIP variable(s) (EIP_t and EIP_{t-1}), estimated month fixed effect, age, change in family size, and the dummy variable indicating whether the household is a non-recipient.

B.1 The bootstrap procedure

We generate artificial datasets by drawing residuals and household-level $\beta_{s,i}$ and computing the values of the dependent variable as follows. First, we create pools of residuals and draw residuals with replacement at the household level (rather than the observation level). We draw residuals at the household level since residuals for the same CU are correlated across different interviews. In the data, some households have one observation (thus one residual), some have two, and the rest have three. We hence create a residual pool for each type of household and draw residual for a household from the corresponding pool. For instance, a household with two residuals has its residual drawn from the pool of residuals for households that have two residuals.

Second, we sample household-level $\beta_{s,i}$ from the distribution of $\beta_{s,i}$ we propose. We vary the $\beta_{s,i}$ distribution to test the property of our estimator under different situations. In particular, we have four versions of $\beta_{s,i}$ (hence four versions of simulations), as we describe subsequently.

Third, we compute the dependent variable ($\Delta\tilde{C}_{i,t}$) for each observation using the artificial data set. That is, we compute $\Delta\widetilde{C}_{i,t}$ using the saved data set according to the data generating process indicated by equation 2 but with household-specific $\beta_{s,i}$.

We construct 1,000 such datasets. On each dataset $j = 1, ..1, 000$, we estimate equation 2 by OLS and save the estimated $\hat{\beta}^j_s$ and standard errors. We then compute the average of the 1,000 estimated $\hat{\beta}^j_s$'s and the average standard errors, and plot the distribution of estimated $\hat{\beta}^j_s$, lines for the mean, the truth, and 95% confidence intervals using the average standard error. The truth is defined as the average treatment effect of the treated over all simulations.

B.2 The distributions of MPCs

We use four versions of the distribution of $\beta_{s,i}$, which leads to four versions of simulations.

1. Constant $\beta_{s,i} = \hat{\beta}_s$.

2. MPCs vary across households. For each CU, draw $\beta_{s,i}$ from $U(0, 2\hat{\beta}_s)$, a uniform distribution with mean = $\hat{\beta}_s$ that runs from 0 to $2\hat{\beta}_s$.

3. MPCs vary across households and increase over time. We draw $\beta_{s,i}$ for CUs who first received their EIPs in April from $U(0, \hat{\beta}_s)$, for CUs who first received EIPs in May from $U(0, 2\hat{\beta}_s)$, for CUs who first received EIPs in June from $U(0, 3\hat{\beta}_s)$, and for other CUs from $U(0, 4\hat{\beta}_s)$. To be clear, we use the same household-specific draw from the uniform distribution to construct both $\beta_{0,i}$ and $\beta_{1,i}$ so that some households are high-MPC households and some households low-MPC households.

4. MPCs vary across households and decrease over time. Analogously to version 3, we draw $\beta_{s,i}$ from $U(0, 4\hat{\beta}_s)$, $U(0, 3\hat{\beta}_s)$, $U(0, 2\hat{\beta}_s)$, and $U(0, \hat{\beta}_s)$, for CUs who first received EIPs in April, May, June, or others, respectively.

Note that for distributions 1 and 2, the true average MPC is simply $\hat{\beta}_s$. For distributions 3 and 4 however, the average MPC is given by the average treatment effect on the treated:

$$\beta_s^j = \frac{1}{I} \sum_{i=1}^{I} \beta_{s,i}^j$$

B.3 Results of bootstrap evaluation of estimator

Figures C.3.a and b show simulation results from Versions 1 and 2 respectively, again, based on our final sample and equation 2. When the treatment effect is distributed uniformly between zero and twice our point estimate, our estimator appears effectively unbiased and the standard errors on average produce a reasonably accurate measure of the actual variation in the estimator. Note that the variation in the estimator reflects not only the uncertainty coming from drawing residuals, but also the uncertainty associated with drawing from the distribution of MPC. The standard errors we report in our main tables account for only the former.

Figures C.3.c and d show simulation results from Versions 3 and 4 respectively, in which the distribution of MPCs rises or falls over time. In each case, there is bias. When MPC are increasing (dramatically) over time (panel c), there is a slight downward bias in our estimated MPCs. Thus, our estimates are likely to slightly understate the true MPC if MPCs rose over time for example as people who had not yet received EIPs exhausted their liquidity. On the other hand, panel d of Figure C.3 shows that if MPCs fell over time, due for example to the roll-out of other parts of the CARES Act like the paycheck protection program and expanded unemployment benefits, then our estimator may be a slight overestimate of households' true average propensity to spend their EIPs following arrival and the true spending rate might be slightly lower.

C Additional Tables and Figures

Table C.1: The response of consumer expenditures to EIPs estimated on recipients and non-recipient including lagged EIP and using the methodology previously applied to tax rebates

	Food and alcohol	Strictly Nondurables	Nondurable goods and services	All CE goods and services	Food and alcohol	Strictly Nondurables	Nondurable goods and services	All CE goods and services
Estimation method	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Panel A. Dependent variable ΔC, MPC					Panel B. Dependent variable ΔC, Dollars spent			
EIP	0.041	0.070	0.075	0.279				
	(0.032)	(0.044)	(0.059)	(0.216)				
I(EIP)					146.3	283.9	356.7	1233.8
					(87.7)	(127.2)	(165.2)	(638.9)

	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
Estimation method	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS
Panel C. Dependent variable $\Delta \ln C$, Pct change in spending					Panel D. Dependent variable ΔC, MPC			
EIP					0.071	0.139	0.174	0.599
					(0.043)	(0.062)	(0.081)	(0.312)
I(EIP)	2.57	1.49	1.39	1.38				
	(2.97)	(1.97)	(1.92)	(2.30)				

Notes: Table reports β_0 from estimation of equation 1 with $S = 1$. The coefficients in panel C are multiplied by 100 so as to report a percent change. Regressions also include interview month dummies, age, and change in the size of the CU. The sample is the sample of all CE household with an interview in June or July 2020 and is otherwise constructed as in previous research papers (see Appendix). Standard errors included in parentheses are adjusted for arbitrary within-household correlations and heteroskedasticity. In 2SLS regressions, EIP indicators, together with control variables, are used as instruments for the EIP amounts. All regressions have 5,240 observations except for the first two columns of panel C which have 5,226 and 5,235, respectively.

Table C.2: The estimated response of expenditures to the EIPs in a sample consisting of only recipients households and using the method previously applied to tax rebates

	Food and alcohol	Strictly Nondurables	Nondurable goods and services	All CE goods and services	Food and alcohol	Strictly Nondurables	Nondurable goods and services	All CE goods and services
Estimation method	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS
Panel A. Dependent variable ΔC, MPC					Panel B. Dependent variable ΔC, Dollars spent			
EIP	0.006	-0.026	-0.062	-0.174				
	(0.038)	(0.053)	(0.072)	(0.233)				
I(EIP)					18.6	-182.4	-293.6	-897.8
					(135.8)	(239.5)	(305.1)	(830.4)

	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
Estimation method	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS
Panel C. Dependent variable $\Delta \ln C$, Pct change in spending					Panel D. Dependent variable ΔC, MPC			
EIP					0.010	-0.093	-0.150	-0.460
					(0.070)	(0.123)	(0.157)	(0.428)
I(EIP)	3.43	-1.27	-2.62	-3.45				
	(5.84)	(3.47)	(3.60)	(4.08)				

Notes: Table reports β_0 from estimation of equation 1 with $S = 0$. The coefficients in panel C are multiplied by 100 so as to report a percent change. Regressions also include interview month dummies, age, and change in the size of the CU. The sample is the sample of all CE household with an interview in June or July 2020 who report receiving an EIP at some point, and is otherwise constructed as in previous research papers (see Appendix). Weights applied are average weights. Standard errors included in parentheses are adjusted for arbitrary within-household correlations and heteroskedasticity. In 2SLS regressions, EIP indicators, together with control variables, are used as instruments for the EIP amounts. All regressions have 3,565 observations except for the first columns of panel C has 3,563.

Table C.3: Income cutoff values for the final sample and number of observations nearby

CU type	Income cutoff	Below cutoff by 0 to 25K		Above cutoff by 0 to 25K	
		recipients	non-recipients	recipients	non-recipients
Single, no kids	\$150K	6	12	3	11
Single, with kid(s)	\$175K	168	23	0	0
Married, no kids	\$275K	9	14	5	13
Married, with kid(s)	\$325K	9	6	1	12
Adults, no kids	\$300K	7	0	0	0
Adults, with kid(s)	\$350K	3	0	0	0

Notes: Data Source: 2019-2020 Consumer Expenditure Survey (BLS), final sample. CU types "Single, no kids" and "Single, with kid(s)" include every CU that has one (and only one) unmarried adult. CU types "Married, no kids" and "Married, with kid(s)" can include CUs that have more than 2 adults, as long as the reference person is married. Similarly, CU types "Adults, no kids" and "Adults, with kid(s)" can include CUs that have more than 2 adults, as long as the reference person is single.

Table C.4: The longer-run response of expenditures to EIP receipt including interactions between interview month dummies and log income

	Dependent variable: scaled dollar change in spending on			
	Food and alcohol	Strictly Nondurables	Nondurable goods and services	All CE goods and services
$\widetilde{EIP}_t$	0.059	0.032	0.084	0.072
	(0.015)	(0.026)	(0.034)	(0.080)
$\widetilde{EIP}_{t-1}$	-0.045	0.040	-0.080	-0.160
	(0.024)	(0.009)	(0.040)	(0.099)
	Implied cumulative fraction of EIP spent over two three-month periods			
	0.072	0.103	0.087	-0.016
	(0.039)	(0.054)	(0.075)	(0.197)

Notes: Table reports β_0 and β_1 from estimation of equation 2 with $S = 1$. Regressions also include interview month dummies, scaled age and change in the size of the CU, and separate intercepts for recipients and non-recipients, and interactions between interview month dummies and log income. The sample is the final sample which includes only CE household 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. Weights applied are average weights. Standard errors included in parentheses are adjusted for arbitrary within-household correlations and heteroskedasticity. All results are from WLS regressions. All regressions have 5,299 observations except for the first column that has 5,294 observations.

Table C.5: The propensity to spend on subcategories of expenditures

	Dependent variable: scaled dollar change in spending on						
	Panel A: Food			*Panel B: Additional categories in strictly nondurables*			
	Food at home	Food away from home	Alcoholic beverages	Utilities, household operations	Personal care and misc.	Gas, motor fuel, public transport	Tobacco products
Coefficient on $\widetilde{EIP}_t$	0.052	0.008	0.003	-0.009	-0.004	0.021	0.003
	(0.019)	(0.006)	(0.002)	(0.011)	(0.006)	(0.008)	(0.003)
Implied share of MPC on							
Food	0.83	0.12	0.04				
Strict nondurables				-0.28	-0.12	0.62	0.09
Share of avg. spending on							
Food	0.69	0.26	0.05				
Strict nondurables				0.32	0.05	0.11	0.02
	Panel C: Additional categories in nondurables			*Panel D: Additional categories in total CE spending*			
	Apparel	Health	Reading	Housing	Entertainment	Education	Transportation
Coefficient on $\widetilde{EIP}_t$	0.022	0.031	-0.001	-0.025	0.035	-0.005	-0.015
	(0.005)	(0.017)	(0.001)	(0.034)	(0.012)	(0.010)	(0.044)
Implied share of MPC on							
Nondurables	0.22	0.31	-0.01				
Total CE spending				-0.31	0.44	-0.06	-0.19
Share of avg. spending on							
Nondurables	0.03	0.22	0.00				
Total CE spending				0.33	0.01	0.05	0.16

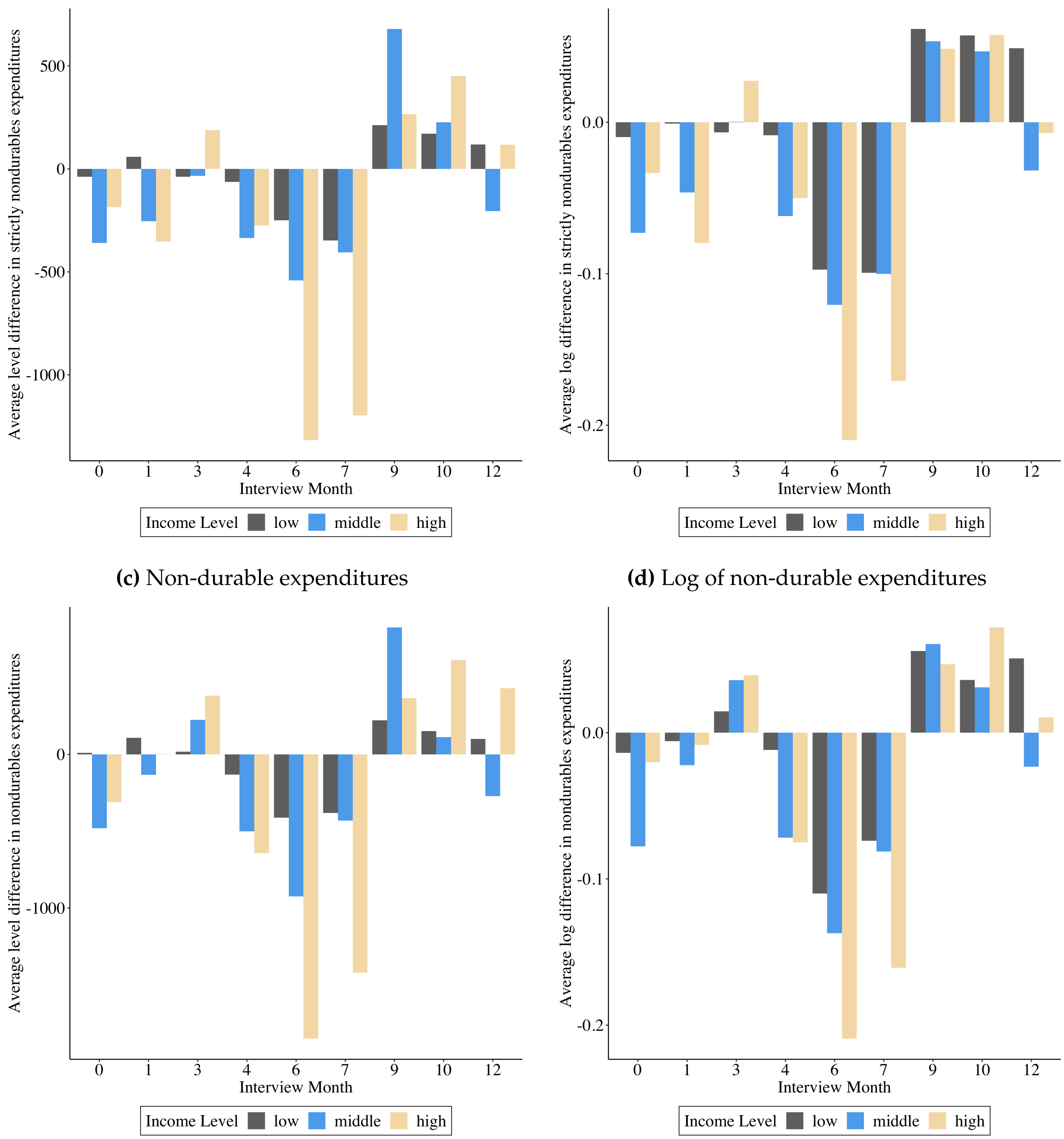
Notes: Table reports β_0 from estimation of equation 2 with $S = 0$ but with scaled expenditure on each subcategory. Regressions also include interview month dummies, scaled age and change in the size of the CU, and a separate intercept for non-recipients. Scaled variables are scaled by the average expenditure of the (smallest) large category the subcategory belongs to (food for Panel A, strictly non-durables for Panel B, non-durables for Panel C, and total expenditures for Panel D). Average spending amounts are the averages over all available interviews. Housing expenditures include total expenditures for housing, including shelter, utilities, fuels, and public services, household operations, and house furnishing and equipment. Entertainment expenditures include fees and admissions, televisions, radios, sound equipment, and other equipment and services. Education expenditures include school tuition, books, supplies, equipment, other school expenses, test preparation, and tutoring services. The sample is the final sample which includes only CE household 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. 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. For Panel A, all columns have 5,309 observations. All other columns have 5,314 observations.

Table C.6: Number of observations in the final sample after each step of cleaning

Sample	Number of observations
Original sample	5800
After dropping observations in student housing	5797
After dropping observations with $AGE\_REF < 21$	5781
After dropping observations with $AGE2 < 21$	5777
After dropping based on change in AGE_REF	5596
After dropping based on change in $AGE2$	5548
After dropping based on change in FAM_SIZE	5546
After dropping based on change in expenditures	5494
After dropping high income CUs	5314

Notes: Data Source: 2020 Consumer Expenditure Survey (BLS).

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



(a) Strictly non-durable expenditures

(b) Log of strictly non-durable expenditures

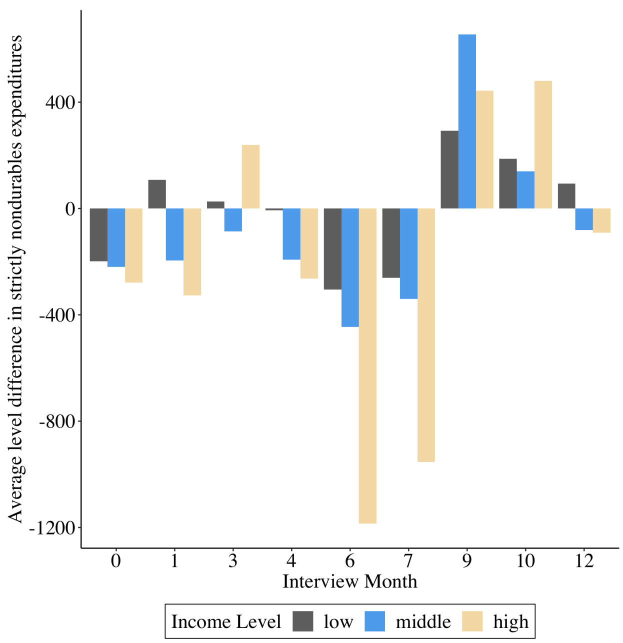
(c) Non-durable expenditures

(d) Log of non-durable expenditures

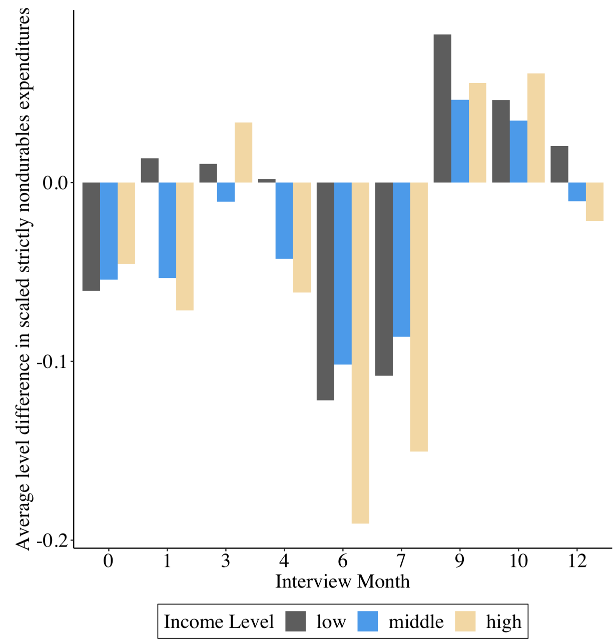
Note: CE data, the sample of all households used in Table III. Each income group contains one-third of the sample.

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

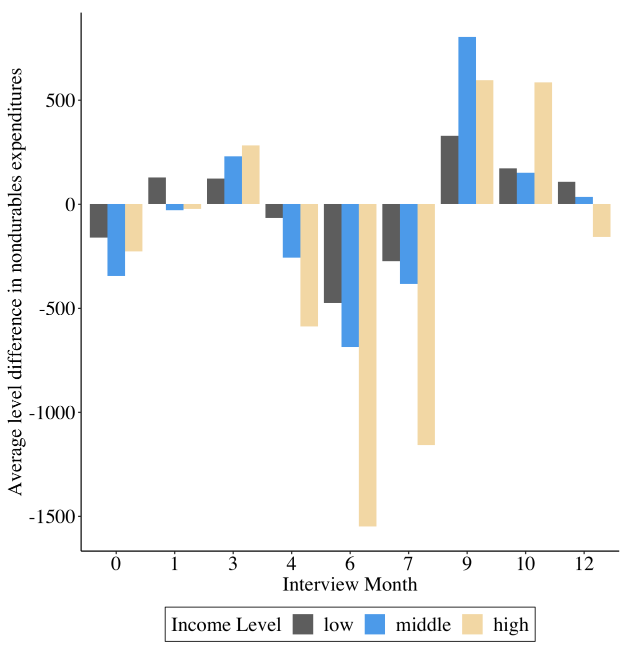
(a) Strictly non-durable expenditures



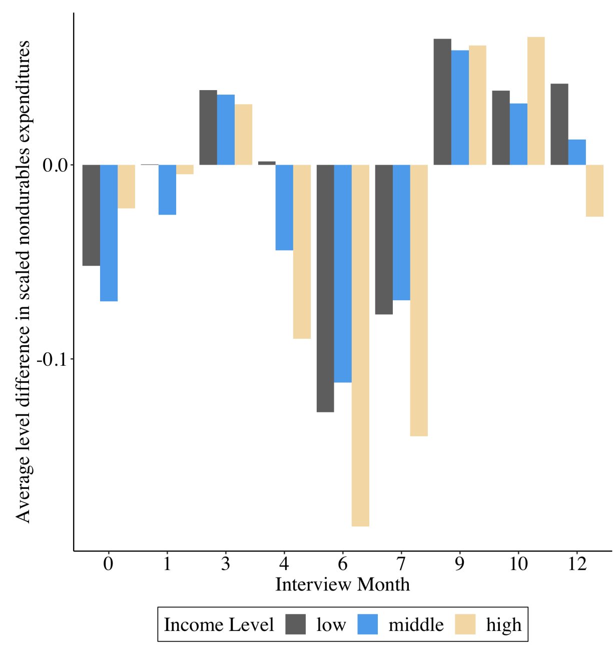
(b) Scaled strictly non-durable expenditures



(c) Non-durable expenditures

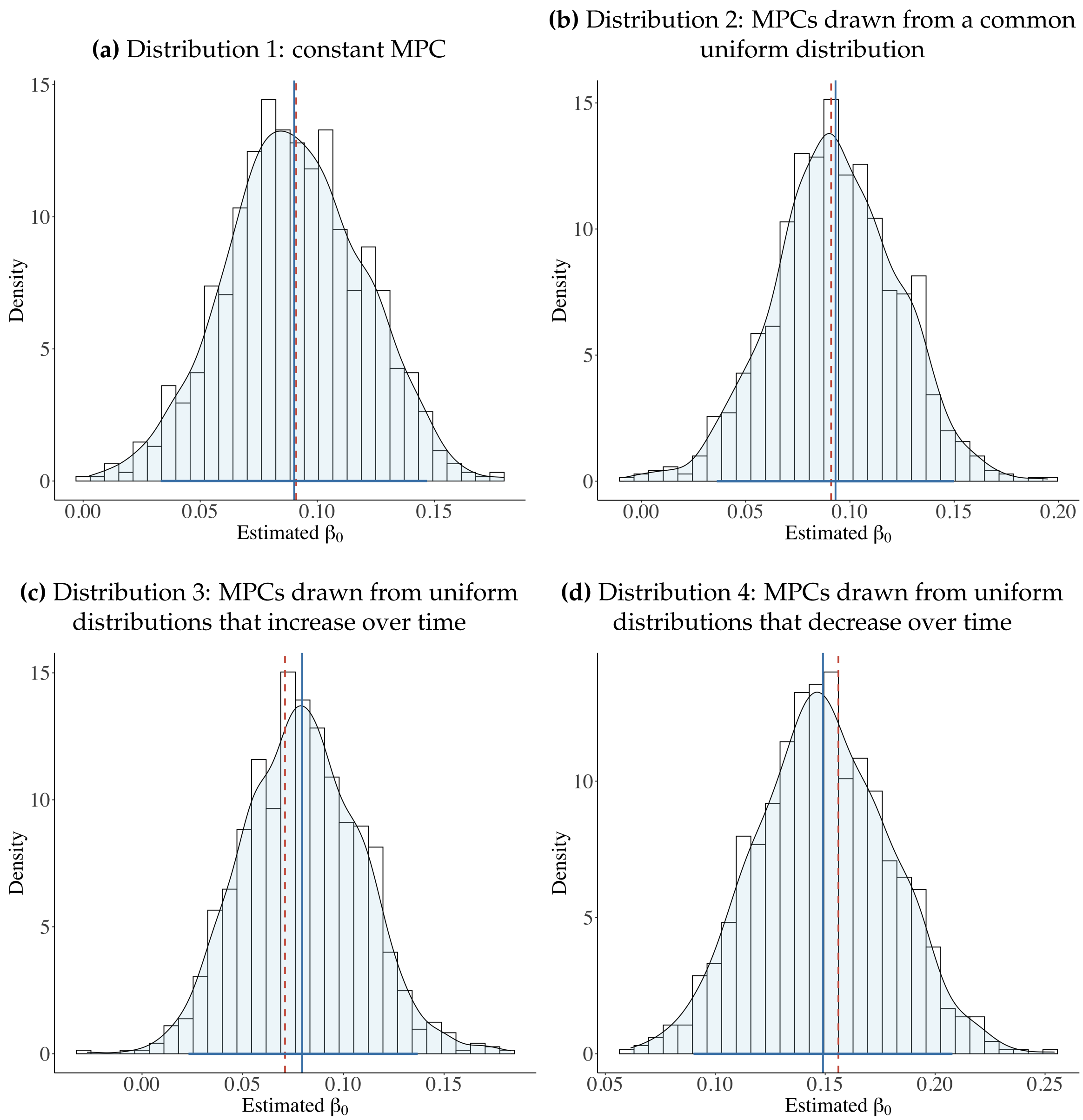


(d) Scaled non-durable expenditures



Note: CE data, the final sample of all households used in Table IV. Each income group contains one-third of the sample.

Figure C.3: Bootstrap simulation results for estimation of β_0



Note: The red dashed line is the true average MPC, while the blue vertical line is the average of the estimated MPCs, and the blue horizontal line is the 95% confidence interval based on the average estimated standard error of the estimate. For (1), the true β_0 is 0.091 and the average estimated β_0 is 0.090. For (2), the true β_0 is 0.091 and the average estimated β_0 is 0.093. For (3), the true β_0 is 0.071 and the average estimated β_0 is 0.080. For (4), the true β_0 is 0.156 and the average estimated β_0 is 0.149. The biases as a percent of true β_0 are -1.1%, 3.3%, 12.7%, and -4.5%.