Consumer Spending and the Economic Stimulus Payments of 2008*

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Abstract: Using special questions added to the Consumer Expenditure Survey, we measure the response of household spending to the economic stimulus payments (ESPs) disbursed during the spring and summer of 2008. We also extend the related literature in a number of ways. First, we are able to measure the spending response using only random variation in the timing of ESP receipt. Second, we test whether the method of delivery (paper check versus electronic transfer) matters for spending. Third, we evaluate a complementary approach to identifying the impact of tax cuts, which asks consumers to self-report whether they spent their tax cuts. We find that, on average, households spent about 12-30% of their stimulus payments on nondurable consumption goods during the three-month period in which the payments were received. Further, there was also a significant effect on durables purchases, in particular auto purchases. Based on point estimates, the estimated responses are largest for lower-income households and homeowners. The responses are also largest for self-reported spenders, though self-reported savers also spent a significant fraction of the payments. The responses do not significantly differ across paper checks and electronic transfers.

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In the winter of 2007-08, facing the fallout from an increasingly severe financial crisis and already contemplating the limitations of traditional monetary policy, Congress and the Administration turned to fiscal policy to help stabilize the U.S. economy. The Economic Stimulus Act (ESA) of 2008, enacted in February 2008, consisted primarily of a 100 billion dollar program that sent economic stimulus payments (ESPs) to 130 million Americans. As implied by their name, the goal of the payments was to stimulate household spending. However, skeptics argued that the payments would have little effect on spending, because they represented a transitory increase in income and moreover the government would ultimately have to finance them by taxing more or spending less in the future.

In this paper we analyze this historically important change in fiscal policy. We begin by estimating the average response of household spending to the stimulus payments. We also analyze the heterogeneity in the spending response across different types of households and consumption goods, with an eye towards understanding the implications for models of consumer behavior and for fiscal policy. Further, we evaluate the accuracy of another, well known and complementary, methodological approach to identifying the impact of tax cuts, which asks consumers to self-report whether they spent (or intend to spend) their tax cuts.

The 2008 stimulus payments varied across households in amount, method of delivery, and timing. Typically, single individuals received $300-$600 and couples received $600-$1200; in addition, households received $300 per child that qualified for the child tax credit. Households received these payments through either paper checks sent by mail or electronic transfers into their bank accounts. Notably, within each delivery method, the timing of receipt was determined by the final two digits of the recipient's Social Security number, digits that are effectively randomly assigned.[1] We exploit this random variation to cleanly estimate the causal effect of the payments on household spending, by comparing the spending of households that received payments in a given period to the spending of households that received payments in other periods.

To conduct our analysis, we worked with the staff at the Bureau of Labor Statistics (BLS) to add supplemental questions about the payments to the ongoing Consumer Expenditure (CE) Survey, which contains comprehensive measures of household-level expenditure for a stratified

[1] The last four digits of a Social Security number (SSN) are assigned sequentially to applicants within geographic areas (which determine the first three digits of the SSN) and a "group" (the middle two digits of the SSN).

random sample of U.S. households. Like a similar module of questions about the 2001 tax rebates analyzed by Johnson, Parker, and Souleles (2006) [JPS], the 2008 questions ask CE households to report the amount and month of receipt of each stimulus payment they received. The 2008 survey also includes some new types of questions. It asks about the method of delivery of each payment (mailed paper check versus electronic transfer). Further, it asks households who received payments to directly self-report whether they mostly spent or mostly saved their payments. This question mimics the questions in the Michigan Survey of Consumers that have been used to study recent changes in tax policy, as in Shapiro and Slemrod (2003a). One of the advantages of such questions is that they can be put into the field and analyzed quickly after the announcement of policy changes.

In addition to analyzing the 2008 ESPs, our paper builds on the related literature in a number of ways. First, relative to JPS, we are able to measure with precision the response of spending even when using only random variation in the timing of ESP receipt. Second, we consider whether the delivery method (check versus electronic) affects the amount of spending. This is an important consideration since the 2008 tax cut was the first large tax cut to use electronic transfers, and electronic transfers will be used increasingly frequently in the future. Third, we evaluate the accuracy of the self-reported responses to the payments, by comparing the results to the results using the data on actual spending and ESP receipt.

Summarizing our main findings, on average households spent about 12-30% of their stimulus payments, depending on the specification, on nondurable consumption goods during the three-month period in which the payments were received. This response is statistically and economically significant. Although our findings do not depend on any particular theoretical model, the response is inconsistent with both Ricardian equivalence, which implies no spending response, and with the canonical life-cycle/permanent income hypothesis (LCPIH), which implies that households should consume at most the annuitized value (on the order of about 5%) of a transitory increase in income like that induced by the one-time payments. We also find a significant effect on durables purchases, bringing the average response of total consumption expenditures to about 50-90% of the payments in the quarter of ESP receipt. In particular, auto purchases were greater than they would have been in the absence of the payments.

These results are broadly consistent and significant across specifications that use different forms of variation, including specifications that rely on just the randomized timing variation

within each of the two delivery methods. We also find some evidence of an ongoing though smaller response in the subsequent three-month period after ESP receipt, but this response cannot be estimated with precision.

Across households, according to the point estimates, the responses are largest for lower-income households, older households, and homeowners, although these differences are not statistically significant. The responses are also largest for self-reported spenders, yet self-reported savers (including those reporting they reduced debt) also spent a statistically and economically significant fraction of their payments. The responses do not significantly differ across paper checks and electronic transfers.

This paper is structured as follows. Sections I and II briefly describe the literature and relevant aspects of ESA 2008. Section III describes the data and Section IV sets forth our empirical methodology. Section V presents the main results regarding the short-run response to the economic stimulus payments, while Section VI examines the longer-run response. Section VII examines the differences in response across different households and consumption goods, and a final section concludes. The Appendix contains additional information about the data.

I. Related Literature

Many papers have tested the consumption-smoothing implications of the rational-expectations LCPIH. One related set of papers uses household-level data and quasi-experiments to identify the effects on consumption from various changes in household income. A smaller set of papers estimates the consumption effects from changes in tax policy in particular. See Deaton (1992), Browning and Lusardi (1996), and Johnson, Parker, and Souleles (2006) for reviews.[2]

Our paper is most closely related to JPS (2006), which uses a similar module of questions appended to the CE survey to study the 2001 income tax rebates. JPS finds that households spent about 20-40 percent of their rebates on nondurable goods during the three-month period in which they received their rebates. There is also a significant though decaying lagged spending effect, so that roughly two-thirds of the rebates was spent cumulatively during the quarter of receipt and subsequent three-month period. The responses are largest for households with low liquid wealth or low income, which is suggestive of binding liquidity constraints. Agarwal, Liu, and Souleles (2007) finds consistent results using credit card data and direct indicators of being credit

[2] For a survey of recent fiscal policy, see e.g., Auerbach and Gale (2009).

constrained; in particular, the spending responses are largest for consumers that are constrained by their credit limits. Johnson, Parker, and Souleles (2009) finds qualitatively similar responses to the 2003 child tax credit payments, using CE data.[3]

Our paper is also closely related to studies that evaluate tax policy by asking households to self-report whether they spent tax cuts. Shapiro and Slemrod (2003a) finds, using the Michigan Survey of Consumers, that about 22% of respondents who received (or expected to receive) a 2001 rebate report that they will mostly spend their rebate. The authors calculate that, under certain assumptions, this result implies an average marginal propensity to consume (MPC) of about one third, which is consistent with the short-run response of expenditure in JPS estimated from data on actual spending and rebate receipt. More recently, Shapiro and Slemrod (2009) uses the Michigan Survey to analyze the 2008 stimulus payments and finds similar results, with about 20% of respondents reporting that they will mostly spend their payment. This again corresponds to an average MPC of about one third. This response is larger than expected under the LCPIH for a transitory tax cut, and it implies a noticeable expansionary effect on aggregate consumption in the second and third quarters of 2008, the period during which and shortly after most of the payments were disbursed. The Michigan survey results provide no clear evidence of greater spending by low-income or potentially constrained households.[4]

Also studying the 2008 ESPs, using scanner data on a subset of nondurable retail goods in the first few weeks after the payments started to be sent out, Broda and Parker (2008) finds that spending on such goods increased by a significant amount, 3.5% in the four weeks after payment receipt. The increase is larger for low asset and low income households. Using the CE survey, we will examine broader measures of expenditure over a longer period of time.[5]

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[3] Coronado, Lupton, and Sheiner (2006) also study the 2003 child payments, using the Michigan Survey.

[4] In 2008, of the 80% of respondents who report they will mostly save their ESP, the majority (about 60%) report that they will mostly pay down debt (as opposed to accumulate assets). See also Sahm, Shapiro and Slemrod (2009). The Michigan Survey includes additional questions to try to determine whether there was a lagged response to the rebate. Of respondents who said they will initially mostly use the rebate to pay down debt, most report that they will "try to keep [down their] lower debt for at least a year." (There are analogous results for respondents who said they will save by accumulating assets.) The Survey included similar questions in 2001 and yielded similar results (Shapiro and Slemrod, 2003b). By contrast, using data on actual spending in 2001, Agarwal, Liu, and Souleles (2007) finds that, while on average households initially used some of their rebates to increase credit card payments and thereby pay down debt, the resulting liquidity was soon followed by a substantial increase in spending.

[5] Using data from a payday lender, Bertrand and Morse (2009) finds that receipt of an ESP reduces the probability of taking out a payday loan for two pay cycles. The effect dissipates by the third cycle, and the magnitude of the reduction in debt is modest relative to the stimulus payments. Such results are qualitatively consistent with the spending dynamics discussed in Agarwal, Liu, and Souleles (2007).

As noted above, our paper builds on the previous literature in a number of additional ways. First, as a result of the new types of questions added to the CE survey in 2008, we can consider whether the delivery method matters, as well as the accuracy of the self-reported responses to the payments. Second, although the results in JPS remained consistent (in the Hausman sense) across specifications that used different forms of variation, they lost statistical significance when limited to only randomized timing variation. Here we further explore this issue and obtain greater precision. Third, there are some potentially important differences in the details of the tax cut (as discussed in the next section) and economic environment in 2008 compared to earlier periods. For instance, on average the stimulus payments in 2008 were about twice the size of the rebates in 2001. Some prior research suggests the possibility that larger payments could lead to a different composition of spending. While JPS finds no significant response of durable goods in 2001, Souleles (1999) finds a significant increase in both nondurable and durable goods (in particular auto purchases) in response to spring-time Federal income tax refunds, which are substantially larger than the 2001 tax rebates.[6] Also, in 2008 housing and mortgage markets were in turmoil and so we will also examine the response of homeowners and mortgage borrowers to the ESP.

II. The 2008 Economic Stimulus Payments

ESA 2008 provided ESPs to the majority of U.S. households (roughly 85% of "tax units"). The ESP consistent of a basic payment, typically $600 or $1200, and -- conditional on eligibility for the basic payment -- a supplemental payment of $300 per child that qualified for the child tax credit. To be eligible for the basic payment, a household needed to have positive net income tax liability, or at least sufficient "qualifying income".[7] For qualifying households, the basic payment was generally the maximum of $300 ($600 for couples filing jointly) and their tax liability up to $600 ($1200 for couples). Households without tax liability received basic

[6] See also Barrow and McGranahan (2000) and Adams, Einav, and Levin (2009) for related results for the EITC and for subprime auto sales. Federal tax refunds currently average around $2500 per recipient, whereas the average rebate in 2001 came to about $480 (JPS).

[7] While the stimulus payments were commonly referred to as "tax rebates," strictly speaking they were advance payments for credit against tax year 2008 taxes. To expedite the disbursement of the payments, they were calculated using data from the tax year 2007 returns (and so only those filing 2007 returns received the payments). If subsequently a household's tax year 2008 data implied a larger payment, the household could claim the difference on its 2008 return filed in 2009. However, if the 2008 data implied a smaller payment, the household did not have to return the difference.

payments of $300 ($600 for couples), so long as they had at least $3000 of qualifying income (which includes earned income and Social Security benefits, as well as certain Railroad Retirement and veterans' benefits). Eligibility started to phase out at a threshold of $75,000 of adjusted gross income (AGI) ($150,000 for couples), with the basic payment being reduced by five percent of the amount by which AGI exceeded the threshold. (Thus the payments completely phased out at $87,000 for individuals and $174,000 for couples). Because of this phase-out for higher income households, and the payments to households without tax liability, the stimulus payments were generally more targeted to lower income households than were the 2001 income tax rebates.

In terms of timing, for recipients who had provided the IRS with their bank routing number (i.e., for direct deposit of tax refunds[8]), the stimulus payments were disbursed electronically over a three-week period ranging from late April to mid May. Otherwise, the payments were mailed (using paper checks) over a nine-week period ranging from mid May to mid July.[9] In both cases, the particular timing of the payments was determined by the last two digits of the recipients' Social Security numbers, which are effectively randomly assigned.

In aggregate, the stimulus payments in 2008 were historically large, amounting to about $100 billion, which is more than double the size of the 2001 rebate program. According to the Treasury, $78.8 billion in ESPs were disbursed in the second quarter of 2008, which corresponds to about 2.2% of GDP or 3.1% of personal consumption expenditures in that quarter. During the third quarter, $15 billion in ESPs were disbursed, corresponding to about 0.4% of GDP or 0.6% of personal consumption expenditures. The stimulus payments constituted about two-thirds of the total ESA package, which also included various business incentives and foreclosure relief.[10] This paper focuses on the stimulus payments, as recorded in our CE dataset.[11]

[8] Payments were directly deposited only to personal bank accounts. Payments were mailed to tax filers who had provided the IRS with their tax preparer's routing number as part of taking out a "refund anticipation loan". The latter are common, representing about a third of the tax refunds delivered via direct deposit in 2007.

[9] Due to the electronic deposits, about half of the aggregate stimulus payments were disbursed by the end of May. While most of the rest of the payments came in June and July, taxpayers that filed their 2007 return late could receive their payment later than the above schedule. Since 92 percent of taxpayers typically file at or before the normal April 15th deadline (Slemrod et al., 1997), this source of variation is small. Nonetheless, we present results below that exclude such late rebates.

[10] For more details on ESA, see e.g., CCH (2008) and Shapiro and Slemrod (2009).

[11] Our empirical approach focuses on consumers' response to the receipt of their stimulus payments, a point in time that our data identifies. Our methodology cannot estimate the magnitude of any earlier response that may have occurred in anticipation of the payments, both because the passage of ESA cannot be separated from other aggregate

III. The Consumer Expenditure Survey

The CE interview survey contains detailed measures of the expenditures of a stratified random sample of U.S. households. CE households are interviewed up to four times, three months apart, to collect expenditure information. In each interview households report their expenditures during the preceding three months (the "reference period"). New households are added to the survey every month so that the data are effectively monthly in frequency. In addition to surveying households about their expenditures, the CE also gathers (less-frequent) information about their demographic characteristics, income, and wealth. We use the 2007 and 2008 waves of the CE data (which include interviews in the first quarter of 2009).

The extra questions about the 2008 ESPs were included in the CE Survey in interviews conducted in June 2008 to March 2009, which cover the crucial time during which the payments were disbursed. The first set of questions asked households whether they received any "economic stimulus payments… also called a tax rebate" since the beginning of the reference period for the interview. If so, the questions asked for the amount of each payment and the date it was received, and whether it was received by check or direct deposit. These questions were phrased to be consistent with the style of other CE questions, and were asked in all interviews that households had during the period in which the ESP questions were in the field.

Households reporting a payment were subsequently asked whether they think the payment led them "mostly to increase spending, mostly to increase savings, or mostly to pay off debt." This question was asked just once of each household. The wording of the question closely follows the main question in the Michigan Survey of Consumers analyzed by Shapiro and Slemrod (2009).

Appendix A contains the language of the survey instrument. We follow JPS in constructing the total payments received by each household in each three-month expenditure reference-period.

We also follow JPS in our definition of expenditures. Specifically, we focus on a series of increasingly aggregated measures of consumption expenditures. First, we study expenditures on food, which include food consumed away from home, food consumed at home, and purchases of

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effects captured by our time dummies, such as seasonality, and because there is no single point in time at which a tax cut went from being entirely unexpected to being entirely expected.

alcoholic beverages. Much previous research has studied such expenditure on food, largely because of its availability in the Panel Study of Income Dynamics, but it is a narrow measure of expenditure. Our second and main measure of consumption expenditures is nondurable expenditures, which is a broad measure of expenditures on nondurable goods and services, following previous research. Third, we also consider a subset of nondurable expenditures, "strictly nondurable" expenditures, which excludes semi-durable goods like apparel, following Lusardi (1996). Finally, total expenditures includes both nondurable expenditures and durable expenditures, such as auto purchases.[12] Appendix B provides further details about the data.

Our sample includes only households that had at least one interview during the period in which the ESP questions were in the field. The resulting sample period starts with interviews in December 2007 (when period *t+1* in equation (1) below covers expenditures in September 2007 to November 2007) and runs through interviews in March 2009 (when period *t+1* covers December 2008 to February 2009). Also, we drop from the sample any household observation (*t* or *t+1*) with implausibly low expenditures (the bottom 1% of nondurable expenditures in levels), unusually large changes in age or family size, and uncertain stimulus payment status.

Table 1 presents summary statistics for our dataset. For each household-reference quarter, we sum all stimulus payments received by the household in that quarter to create our main economic stimulus payment variable, *ESP*. During the consumption reference period that covers the main time of disbursement of the payments (May - July), about two-thirds of households report receiving a payment. The average value of *ESP*, conditional on a positive value, is about $1000.

IV. Empirical Methodology

Consistent with specifications in the previous literature (e.g., Zeldes (1989a), Lusardi (1996), Parker (1999), Souleles (1999), and JPS), our main estimating equation is:

$$C_{i,t+1} - C_{i,t} = \Sigma_s \beta_{0s} * month_{s,i} + \boldsymbol{\beta_1}' \mathbf{X}_{i,t} + \beta_2 ESP_{i,t+1} + u_{i,t+1}, \qquad (1)$$

where C is either consumption expenditures or their log; *month* is a complete set of indicator variables for every period in the sample, used to absorb the seasonal variation in consumption expenditures as well as all other concurrent aggregate factors; and X are control variables (here

[12] Unlike in JPS, the response of total expenditures is estimated below with relative precision. This could in part reflect the larger total number of payments (about 30% more) in the sample in 2008, and the larger size (over double) of these payments.

age and changes in family size) included to absorb some of the preference-driven differences in the growth rate of consumption expenditures across households. $ESP_{i,t+1}$ represents our key stimulus payment variables, which take one of three forms: i) the total dollar amount of stimulus payments received by household i in period $t+1$ ($ESP_{i,t+1}$); ii) a dummy variable indicating whether any payment was received in $t+1$ ($I(ESP_{i,t+1}>0)$); and iii) a distributed lag of ESP or $I(ESP>0)$, to measure the longer-run effects of the payments. We correct the standard errors to allow for arbitrary heteroskedasticity and within-household serial correlation. As an extension, to analyze heterogeneity in the response to the payments, we interact $ESP_{i,t+1}$ with indicators for different types of households.

The key coefficient β_2 measures the average response of household expenditure to the stimulus payments. Using the randomized timing of ESP receipt helps us avoid any potential omitted variables bias, and so provides a clean estimate of the causal effect of the payments. For nondurable expenditures, this estimate provides a direct test of the LCPIH. Since Congress passed ESA in February, 2008, and expectations of some tax cut arose even earlier, the payment can be thought of as being pre-announced.[13] In this case, the rational-expectations LCPIH implies that $\beta_2=0$. Even if instead households were actually surprised by the payment, β_2 should still be small under the LCPIH, because the one-time payment represents a transitory increase in income. β_2 should also be zero under Ricardian equivalence.

V. The Short-Run Response of Expenditure

This section estimates the short-run change in consumption expenditures caused by receipt of the stimulus payments, using the contemporaneous payment variables ESP_{t+1} and $I(ESP_{t+1}>0)$ in equation (1). The following section estimates the lagged response to the payments.

In light of potential measurement error and sample-size limitations, in working with household-level data on expenditure it is in general important to use the largest possible sample

[13] Since February 2008 can fall in period t for some sample households receiving a payment, any announcement effect from the passage of ESA could potentially attenuate our estimate of β_2. However, whenever information about the tax cuts underlying the ESPs became publicly available, whether preceding the actual passage of ESA or not, any resulting wealth effects should be small, and should have arisen at the same time(s) for all consumers, so their average effects on expenditure would be picked up by the corresponding time dummies in equation (1). Even heterogeneity in such wealth effects would not be correlated with the timing of ESP receipt, so β_2 should still equal zero when estimated using randomized timing variation.

and as much variation as possible in the relevant independent variables. Accordingly we begin by estimating equation (1) utilizing all of the available information about the payments received by each household, using *ESP* as the key regressor. While this variable is analogous to that used in most tests of the LCPIH, we can go further and investigate its validity by limiting the amount of variation that we utilize, e.g. by using $I(ESP>0)$, which includes only variation in whether a payment was received at all in a given period, not the dollar amount of payments received.

In Table 2, the first set of four columns displays the results of estimating equation (1) by ordinary least squares (OLS), with the dollar change in consumption expenditures as the dependent variable and the contemporaneous amount of the payment (ESP_{t+1}) as the key independent variable, which uses all available payment information. The resulting estimates of β_2 measure the average fraction of the payment spent on the different expenditure aggregates in each column, within the three-month reference-period in which the payment was received. We find that, during the three-month period in which a payment was received, relative to the previous three-month period, a household on average increased its expenditures on food by about 2 percent of the payment, its expenditures on strictly nondurable goods by 8 percent of the payment, and its expenditures on nondurable goods by 12 percent of the payment. The third result is statistically significant, and larger than implied by the LCPIH. In the fourth column, total consumption expenditures increased on average by 52 percent of the payment, a substantial and statistically significant amount. This result is relatively precisely estimated, especially considering that the difference with the preceding results reflects durables expenditures, which are much more volatile than nondurable expenditures.

These results identify the effect of a payment from variation in both the timing of payment receipt and the dollar amount of the payment. While the variation in the payment amount is possibly uncorrelated with the residual in equation (1), it is not purely random since the amount depends upon household characteristics such as tax status, income, and number of dependents.

The remaining columns of Table 2 use only variation in whether a payment was received at all in a given period, not the dollar amount of payments received. The second set of columns in the table uses the indicator variable $I(ESP_{t+1}>0)$ in equation (1). In this case β_2 measures the average dollar increase in expenditures caused by receipt of a payment. The estimated responses again increase in magnitude across the successive expenditure aggregates. During the three-

month period in which a payment was received, relative to the previous three-month period, households on average increased their expenditures on nondurable goods by $122, which is statistically significant at the 7% level. Total expenditures increased by a significant $494. Compared to an average payment of about $1000, these results are consistent with the previous estimates in the first set of columns, which also used variation in the magnitude of the payments received.

As a robustness check, the third set of columns in Table 2 uses the change in log expenditures as the dependent variable. On average in the three-month period in which a payment was received, relative to the previous three-month period, nondurable expenditure increased by 2.1%, and total expenditures increased by 3.2%. These are again statistically and economically significant effects. Considering the magnitudes of nondurable and total expenditures (Table 1), these results are broadly consistent with the previous results in the table.

Finally, since it is interesting to estimate a value interpretable as a marginal propensity to spend upon the payment's arrival, we estimate equation (1) by two-stage least squares (2SLS). We instrument for the payment amount, *ESP*, using the indicator variable, $I(ESP>0)$, along with the other independent variables. As in the first four columns, β_2 then measures the fraction of the payment that is spent within the three-month period of receipt – but in this case without using variation in the magnitude of the payment. As shown in the last set of columns in Table 2, the estimated marginal propensities to spend remain close in magnitude to those estimated in the first four columns, which did not treat *ESP* as potentially non-exogenous.[14]

The results in Table 2 identify the effect on spending by comparing the behavior of households that received payments at different times to the behavior of households that did not receive payments at those times. Since some households did not receive any payment, in any period, the results still use some information that comes from comparing households that received payments to those that never received payments. Table 3 investigates the role of this variation using a number of different approaches, for brevity focusing on strictly nondurable goods, nondurable goods, and total expenditures.

[14] The findings in Table 2 are generally robust across a number of additional sensitivity checks. For instance, using median regressions or winsorizing the dependent variable lead to very similar results for food, strictly nondurable goods, and nondurable goods. For total expenditures, the resulting coefficients are generally smaller than in Table 2, though still statistically and economically significant (e.g., substantially larger than those for nondurable expenditures). This reduction in point estimates for total expenditures is to be expected, since the distribution of expenditure changes (dC) has much more of its mass in the tails for total expenditures than for nondurable expenditures. Weighting the sample leads to very similar results as in Table 2, for all four expenditure aggregates.

First, Panel A adds to equation (1) an indicator for households that received a payment in *any* reference quarter, *I(Total ESP >0)*, which allows the expenditure growth of payment recipients to differ on average from that of non-recipients. In this case, the main regressor $I(ESP_{t+1}>0)$ captures only high-frequency variation in the timing of payment receipt -- receipt in quarter $t+1$ in particular -- conditional on receipt in some quarter. As reported in Table 3, the estimated coefficients on *I(Total ESP >0)* are always small and statistically insignificant. Hence, apart from the effect of the payment, the expenditure growth of payment recipients is on average similar to that of non-recipients over the quarters in the sample period around the payments. Moreover, the estimated coefficients for the effect of the payment (ESP_{t+1} and $I(ESP_{t+1}>0)$) are rather similar to those in Table 2. Hence the results in Table 2 are not driven by differences in expenditure growth between payment recipients and non-recipients over the sample period. That is, controlling for whether a household ever received a payment, spending significantly increases in the particular quarter of payment receipt.

Our second approach is more stringent. Panel B excludes from the sample all households that did not receive a payment in some reference quarter. (Conservatively, it excludes all the households not known to have received a payment based on the available data.) The advantage of this approach is that, when using *I(ESP>0)*, it identifies the response of spending using only the variation in the timing of payment receipt conditional on receipt, by comparing the spending of households that received payments in a given period to the spending of households that also received payments but in other periods. The disadvantage of the approach is that it leads to a reduction in power due to the resulting decline in sample size and effective variation. Nonetheless, the results are broadly consistent with the previous results (especially when considering the confidence intervals). While as expected the standard errors increase, the point estimates are also somewhat larger than before, and so the results are all statistically significant.

Finally, Panel C also excludes the households that received late stimulus payments, after the main period of their (randomized) disbursement, due to filing late tax returns in the preceding year. Although the timing of these payments is not necessarily endogenous, it was not randomized.[15] In JPS, analogously limiting the sample to non-late rebate recipients significantly reduced the precision of the results. Here by contrast the results remain statistically significant.

[15] Extending JPS to accommodate the two delivery methods, we exclude paper checks received after August, and electronic ESPs received after June.

They also remain economically significant, broadly consistent in magnitude with the preceding results. In the final set of columns using 2SLS, on average nondurable expenditures increased by 31% of the payment in the quarter of receipt, relative to the previous quarter, and total expenditures increased by 91% of the payment. In sum, even when limiting the variation to the timing of ESP receipt conditional on (non-late) receipt, the results imply that the ESPs had a significant effect on household spending.

Table 4 examines one of the key new features of the ESP program, the use of electronic funds transfers (EFT). About 40% of the CE households received their payments via EFTs, and the use of EFTs is likely to increase in the future, so it is important to consider whether the method of payment delivery matters. Panel A separately estimates the average response of spending to EFTs and to paper checks, using their analogues of *ESP* and *I(ESP>0)*, starting with the entire sample of households from Table 2.[16] The estimated coefficients are generally similar (and not statistically significantly different) across the two delivery methods, across all the columns. Thus these results provide no evidence that the method of delivery affected the average response of spending.

The rest of Table 4 returns to the investigation of different forms of variation, following Table 3, now taking into account that the method of delivery (paper check versus EFT) was not randomized. For example, households receiving EFTs have somewhat higher income on average than households receiving paper checks, and might also be different in other, hard to observe ways (e.g., perhaps they are more technologically savvy). Panel B again restricts the sample to households receiving non-late stimulus payments, as in the final panel of Table 3. Not surprisingly, since the EFTs were disbursed over just a few weeks, using just timing variation leads to a significant reduction in power for estimating the effect of EFT receipt. More importantly, the results for paper checks remain statistically significant and broadly similar to the average response in the final panel of Table 3. That is, even separately controlling for receipt of EFTs, using the random variation in the timing of the paper checks still yields a significant response of spending to the paper checks.

Strictly speaking, these results still impose common month dummies and demographic effects (age and changes in family size) across EFT and paper-check recipients. One could relax this imposition by estimating equation (1) for separate samples of EFT and paper-check

[16] A few observations have missing values for the method-of-delivery question, and so are dropped from the sample.

recipients, however some households, albeit a small fraction (about 2%), received both EFTs and paper checks. Also, our goal here is to estimate the *average* response to the stimulus payments, using clearly exogenous variation. Accordingly, Panel C of Table 4 estimates a pooled regression that allows for separate time dummies and demographic effects across three groups of households: a) households who received only paper checks; b) households who received only EFTs; c) households who received both paper checks and EFTs. The results are broadly similar to those in the final panel of Table 3, even though they are driven by the random variation in the timing of just paper checks (since the EFTs have limited timing variation).[17]

Overall, our findings remain broadly consistent across specifications that use different forms of variation, including very limited variation. Not surprisingly, at times the point estimates vary somewhat across specifications, especially for total expenditures, but not significantly so relative to the corresponding confidence intervals, and the conclusions regarding statistical and economic significance remain robust. In particular, even when limiting our variation to only randomized variation in the timing of ESP receipt, we find that the payments caused a significant increase in household spending in the short-run.

VI. The Longer-Run Response of Expenditure

To investigate the longer-run effect of the stimulus payments, we add the first lag of the payment variable, ESP_t, as an additional regressor in equation (1), focusing on the households that received non-late payments as in the final panel of Table 3. The resulting estimates are reported in Table 5.

First, note that the presence of the lagged variable does not alter our previous conclusions about the short-run impact of the payment. The coefficients on ESP_{t+1} are broadly similar to the corresponding results in Table 3. Second, the receipt of a payment causes a *change* in spending one quarter later (i.e., from the three-month period of receipt to the next three-month period) that is negative but smaller in absolute magnitude than the contemporaneous change. Since the net effect of the payment on the *level* of spending in the later quarter (relative to the level in the quarter before receipt) is given by the sum of the coefficients on ESP_t and ESP_{t+1}, this implies

[17] Across all the columns in Panel C, the coefficients on the time dummies (jointly) and the demographic variables (jointly) never significantly vary across the two main groups of households, those who received only EFTs and those who received only paper checks. These coefficients are sometimes significant only for the few households who received both EFTs and paper checks, relative to the two main groups.

that, after increasing in the three-month period of payment receipt, spending remains high, but less high, in the subsequent three-month period.

These lagged effects are, however, estimated with less precision. For example, in the second-to-last column, for nondurable expenditures using 2SLS, nondurable expenditures rise by 25% of the payment in the quarter of receipt. The expenditure change in the next quarter is -10%, so that nondurable expenditures in the second three-month period are still higher on net than before payment receipt by 25%-10% = 15% of the payment. The *cumulative* change in nondurable expenditures over both three-month periods is then estimated to be 25% + 15% ≈ 41% of the payment (bottom row). However, neither the 15% change in the second period nor the 41% cumulative change is statistically significant. The second-period and cumulative changes are also insignificant for the other expenditure groups (strictly nondurable goods and total expenditures) using 2SLS in the final set of columns.

In sum, while the point estimates suggest some ongoing though decaying spending response to the ESPs in the subsequent quarter after receipt, this lagged response cannot be estimated with precision, even on average over the sample period. Hence, in the subsequent extensions where we will push the data harder to consider various forms of heterogeneity, we will focus on the short-run response.

VII. Differences in Responses across Households and Goods

This section analyzes heterogeneity in the response to the stimulus payment, across different types of households and different subcategories of consumption goods. This analysis can potentially provide evidence about why household expenditure responded to the payment. For brevity, we report results from the 2SLS specification, instrumenting the payment *ESP* (and any interaction terms) with the corresponding indicator variables for payment receipt *I(ESP>0)* (and their interactions, along with the other independent variables), for the sample of households receiving non-late payments.

The presence of liquidity constraints is a leading explanation for why household spending might increase in response to a previously expected increase in income. To investigate this explanation, we test whether households that were relatively likely to be constrained were more likely to increase their spending upon arrival of a payment. Constrained households may be unable or unwilling to increase their spending prior to the payment arrival. On the other hand,

unconstrained households (e.g., high wealth or high income households) may find the costs of not smoothing consumption across the arrival of the payment to be small (Caballero, 1995; Parker, 1999; Sims, 2003; and Reiss, 2004).

Expanding equation (1), we interact the intercept and ESP_{t+1} variables with indicator variables (*Low* and *High*) based on various household characteristics (all from households' first CE interview to minimize any endogeneity). We use three different variables to identify households that are potentially liquidity constrained: age, income (family income before taxes), and liquid assets (the sum of balances in checking and saving accounts). While liquid assets is arguably the most directly relevant of these variables for identifying liquidity constraints, it is the least well measured and the most often missing in the CE data, so we start with the other two variables.[18] For each variable, we split households into three groups (*Low*, *High*, and the intermediate baseline group), with the cutoffs between groups chosen to include about a third of the payment recipients in each group.

Table 6 begins by testing whether the propensity to spend the stimulus payment differs by age. Because young households typically have low liquid wealth and high income growth, they are disproportionately likely to be liquidity constrained (e.g., Jappelli, 1990; Jappelli et. al., 1998).[19] In the first set of columns in the table, *Low* refers to young households (40 years old or younger) and *High* refers to older households (older than 58), and the coefficients on the interaction terms with these variables represent differences relative to the households in the baseline, middle-age group. The point estimates for the interaction terms suggest that, relative to the baseline middle-aged household, young households spent less of the payment and old households spent more of the payment. However, these differences are not statistically significant.

The second set of columns in Table 6 tests for differences in spending across income groups. The point estimates suggest that low-income households spent a much larger fraction of their payment than the typical (baseline middle-income) household, especially in total expenditures. The bottom panel reports the implied total spending for the interacted groups, in absolute terms. (The uninteracted ESP coefficient in the first row represents the spending for the

[18] The CE survey does not include the direct measures of borrowing and credit constraints used by Jappelli (1990) and Jappelli et. al. (1998), or Agarwal, Liu, and Souleles (2007).

[19] There is also evidence that some older households increase their spending on receiving their (predictable) pension checks (Wilcox, 1989; and Stephens, 2003). Outside the null LCPIH hypothesis of $\beta_2=0$, older households might also spend relatively more because they have shorter time horizons.

baseline group.) For total spending on total expenditures, of the three groups, only the result for the low-income households is statistically significant. It is also economically significant, averaging about 120% of the payment.[20] However, while suggestive of possible role for liquidity constraints, the difference between this result and that for the baseline group, although large at about 70 percentage points, is not statistically significant.

The last set of columns in Table 6 tests for differences by liquid asset holdings. While the point estimates suggest less spending by low-asset households, none of the differences are statistically significant. Indeed, even the total amounts of spending in absolute terms are insignificant for all three groups, for both nondurable expenditures and total expenditures. The loss of precision when using the asset variable might reflect the smaller sample sizes due to missing asset values and measurement error in the available asset values.

One possible complication in assessing liquidity constraints during the sample period is that households might have expected the recent recession to last longer than usual. If constrained households expect their constraints to bind for a longer period of time, that would reduce the magnitude of their response to a payment per period.

Another key characteristic of the recent recession was the large decline in housing wealth and the reduced ability to borrow against home equity. To examine the potential implications for the response to the ESPs, the first set of columns in Table 7 splits the sample according to housing status. The baseline group is renters (23% of the sample), and the two interacted groups are homeowners with a mortgage (50%) and homeowners without a mortgage (27%). The point estimates suggest much larger spending responses by both groups of homeowners relative to renters, though the differences are not statistically significant. In absolute terms, homeowners have large and significant responses in both nondurable expenditures and total expenditures, whereas the response of the renters is much smaller and insignificant.[21]

[20] As discussed below, it is not inconsistent for the average spending response to be larger in magnitude than the average payment, even putting aside the confidence intervals for the former, if enough households buy large durables like autos in response to receiving a rebate.

[21] The results for homeowners do not simply reflect the preceding results for older households. E.g., if one drops from the sample the households older than 65, the coefficients for nondurable expenditure remain very similar to those reported in the table, for all three groups of households. The coefficients for total expenditure remain very similar for renters and homeowners with mortgages. While the coefficient for total expenditure looses significance for homeowners without mortgages, presumably in part due to the reduced sample of such homeowners, it remains large in magnitude; and as in the table, the coefficient for nondurable expenditure remains significant and is largest for homeowners without mortgages, compared to the other two groups.

Finally, we evaluate the alternative methodological approach that identifies the impact of tax cuts by asking consumers to self-report whether they spent their tax cut. Of the CE households that received a stimulus payment, 32% reported that they mostly spent their payment, 18% reported they mostly saved it, and 50% reported they used it to pay down debt. In final set of columns in Table 7, the self-reported spenders did in fact spend more of the payment than the other groups. In absolute terms their spending is statistically and economically significant. They spent about 35 percentage points more on nondurable goods than the baseline group, the self-reported savers, and this difference is statistically significant. The corresponding difference for total expenditures is even larger in magnitude, but not statistically significant. On the other hand, even the self-reported "non-spenders" spent significant, albeit smaller, fractions of the payment in absolute terms. For self-reported savers, the response of total expenditures is statistically significant and large at 95% of the payment on average. For households who reported they paid down debt, the response of total expenditures is still large at about 63 percentage points, albeit insignificant, and the response of nondurable goods is statistically significant and still rather large at 27% of the payment. In this sense self-reported spending may provide a lower bound on the actual amount of spending (consistent with Agarwal, Liu, and Souleles, 2007)).

Turning to differences across goods, each column in Table 8 reports the estimated change in spending for each subcategory of goods within the broad measure of nondurable expenditures (a complete decomposition). The columns also report, at the bottom of the table, the share of the overall increase in nondurable expenditures that is accounted for by each of the subcategories. For benchmarking, one can compare these results to the average share of each subcategory in nondurable expenditures (last row). Of course, comparisons of different subsets of nondurable expenditure must be interpreted cautiously because of potential non-separabilities across goods.

Further, note that in general the results are statistically weak, with only the coefficient for utilities and household operations being statistically significant. This response is roughly in proportion to the share of this subcategory in nondurable goods. The point estimates also suggest a disproportionately large response in personal care (and miscellaneous items), tobacco, and apparel, though these responses are nonetheless statistically insignificant. For such narrow subcategories of goods there is much more variability in the dependent variable that is unrelated to the payment regressor. Our previous results, by summing the subcategories into broader aggregates of nondurable goods, averaged out much of this unrelated variability (such as, for

example, whether a trip to the supermarket happened to fall just inside or outside the expenditure reference-period).

Table 9 provides the analogous decomposition of the response of the durable goods part of total expenditures. While there are sizable responses on average in housing (which includes shelter and furniture/appliances) and entertainment (which includes TVs and other electronic equipment), these responses are statistically insignificant and small relative to their share in durable goods. The bulk of the response in durables comes in transportation, spending on which increases by 53% of the payments on average, a statistically and economically significant amount. Further analysis shows that this result is driven by vehicle purchases. Receipt of a stimulus payment increased the probability of purchasing a vehicle, relative to the counterfactual of no payment, and such purchases are large enough in magnitude that they imply large average responses to the payments.

VIII. Conclusion

We find that on average households spent about 12-30% of their stimulus payments, depending on the specification, on nondurable consumption goods during the three-month period in which the payments were received. This response is statistically and economically significant. It is generally smaller in magnitude (though not significantly different) than the response in nondurable goods from the 2001 tax rebate, which could reflect the more transitory nature of the 2008 tax cut. Nonetheless, the response in 2008 is larger than implied by the LCPIH or Ricardian equivalence. Moreover, the composition of spending is different than in 2001, perhaps reflecting the larger size of the payments in 2008. We find a significant effect on durables purchases, bringing the average response of total consumption expenditures to about 50-90% of the payments in the quarter of ESP receipt. In particular, the results imply that auto purchases, although weakening during the recession, would have been even weaker in the absence of the payments.

These results are broadly consistent and significant across specifications that use different forms of variation, including specifications that rely on just the randomized timing variation within each of the two delivery methods. We also find some evidence of an ongoing though smaller response in the subsequent three-month period after ESP receipt, but this response cannot be estimated with precision.

Across households, according to the point estimates, the responses are largest for lower-income households, older households, and homeowners, although these differences are not statistically significant. The responses are also largest for self-reported spenders, yet self-reported savers (including those reporting they reduced debt) also spent a statistically and economically significant fraction of their payments. The responses do not significantly differ across paper checks and electronic transfers.

Appendix A: The 2008 ESP Survey Instrument

a) The following questions were asked in all CE interviews in June 2008 – March 2009:

[Earlier this year/Last year] the Federal government approved an economic stimulus package. [Many households will receive a one-time economic stimulus payment, either by check or direct deposit/Previously you or your CU [[consumer unit]] reported receiving one or more economic stimulus payments.] This is also called a tax rebate and is different from a refund on your annual income taxes.

Since the first of the reference month, have you or any members of your CU received a/an additional
10. Tax rebate? [Economic Stimulus Payment]
99. None/No more entries

Who was the rebate for? [enter text] _____
* Collect each rebate separately and include the name(s) of the recipient(s).

In what month did you receive the rebate? [enter text] _____

What was the total amount of the rebate? [enter value] _____
* Probe if the amount is not an expected increment such as $300, $600, $900, $1,200, etc

Was the rebate received by - ?
1. check?
2. direct deposit?

Did you or any members of your CU receive any other tax rebate [economic stimulus payment]?
1. Yes
2. No
If yes, return to "Who was the tax rebate for?"

b) The following question was asked (during June 2008 – March 2009) of households that previously reported receiving an economic stimulus payment. Once the question was answered, it was not asked again.

[Earlier in this interview/Last interview/Previously] [you/your consumer unit] reported receiving a one-time tax rebate that was part of the Federal government's economic stimulus package. Did the tax rebate lead [you/your consumer unit] mostly to increase spending, mostly to increase savings, or mostly to pay off debt?

1. mostly to increase spending
2. mostly to increase saving
3. mostly to pay off debt

Appendix B: The CE Data

We construct the economic stimulus payment variable from the CE ESP data (Appendix A) in a similar manner to JPS's construction of the 2001 rebate variable. The 2008 data require fewer consistency checks and adjustments, however. This is because by 2008 the CE survey used computer-assisted (CAPI) software to input and cross-check respondents' replies. Moreover, with a few exceptions, each interview records only ESPs received during the months of the interview's reference period. We adjusted the exceptions to bring their payments into the appropriate consumption reference period.

We use the following definitions of the other main variables. Age is the average age of the head and spouse when the household is a married couple, otherwise it is just the age of the head. The number of children is calculated as the number of members of the household younger than 18.

Following Lusardi (1996), expenditures on strictly nondurable goods include expenditures on food (away from home, at home and alcoholic beverages), utilities (and fuels and public services), household operations, public transportation and gas and motor oil, personal care, tobacco, and miscellaneous goods. Nondurable goods (broadly defined) adds expenditures on apparel goods and services, health care expenditures (excluding payments by employers or insurers), and reading materials, following Lusardi (1996) but excluding education. Total expenditure adds expenditures on education, housing (including furniture and appliances and shelter but excluding utilities and household operations, which are already included in nondurable goods), transportation (including vehicle purchases, maintenance, and insurance, but excluding public transportation and gas and motor oil), and entertainment (e.g., including TVs and other electronics, as well as fees).

Turning to the sample, we omit observations missing any of the key data that we use in our regressions. Our sample omits the bottom one percent of nondurable consumption expenditures in levels (after adjusting for family size and allowing for a time trend), since this data implies implausibly small (often negative) consumption expenditures. Finally, we drop household observations that report living in student housing, that report age less than 21 or greater than 85, that report age changing by more than one or a negative amount between quarters, or that report changes in the number of children or adults greater than three in absolute magnitude. When we split the sample based on income, we drop households flagged as

incompletely reporting income. When we split based on liquid assets, we drop households if the asset information used in computing initial assets (as the difference between final assets and the change in assets) is topcoded.

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Table 2: The short-run response of expenditures to the stimulus payment

Dependent Variable:	ΔC Dollar change in				ΔC Dollar change in				ΔlnC Percent change in				ΔC Dollar change in			
	Food	Strictly Non-durables	Non-durable goods	Total Cons	Food	Strictly Non-durables	Non-durable goods	Total Cons	Food	Strictly Non-durables	Non-durable goods	Total Cons	Food	Strictly Non-durables	Non-durable goods	Total Cons
Estimation method:	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS
ESP	0.016	0.079	0.121	0.516									0.012	0.079	0.128	0.522
	(0.027)	(0.046)	(0.056)	(0.179)									(0.034)	(0.060)	(0.071)	(0.219)
I(ESP)					10.9	74.7	121.5	494.4	0.69	1.74	2.09	3.24				
					(31.7)	(56.6)	(67.2)	(207.2)	(1.27)	(0.96)	(0.94)	(1.17)				
Age	0.721	(0.226)	0.958	6.6	0.697	-0.345	0.773	5.773	0.049	0.009	0.029	0.045	0.714	(0.225)	0.968	6.6
	(0.344)	(0.645)	(0.806)	(2.247)	(0.343)	(0.645)	(0.807)	(2.240)	(0.010)	(0.010)	(0.010)	(0.010)	(0.344)	(0.646)	(0.808)	(2.257)
Change in # adults	197.7	448.1	560.6	452.4	197.7	448.0	560.6	452.0	8.96	8.43	8.99	4.78	197.7	448.1	560.7	452.5
	(54.5)	(105.6)	(117.8)	(375.0)	(54.5)	(105.6)	(117.8)	(375.0)	(1.77)	(1.34)	(1.32)	(1.63)	(54.5)	(105.6)	(117.8)	(375.0)
Change in # children	89.1	138.6	185.4	-254.5	89.2	139.1	186.1	-251.6	4.50	3.35	3.93	1.42	89.1	138.6	185.3	-254.5
	(47.5)	(96.0)	(111.0)	(387.9)	(47.5)	(96.0)	(111.1)	(387.9)	(2.02)	(1.53)	(1.50)	(2.10)	(47.5)	(96.0)	(111.0)	(387.9)
N	17,478	17,478	17,478	17,478	17,478	17,478	17,478	17,478	17,427	17,475	17,478	17,478	17,478	17,478	17,478	17,478

Notes: All regressions include a full set of month dummies, following equation (1). Reported standard errors are adjusted for arbitrary within-household correlations and heteroskedasticity. The coefficients in the third set of four columns are multiplied by 100 so as to report a percent change. The last set of four columns reports results from 2SLS regressions where *I(ESP)* with the other regressors are used as instruments for *ESP*.

Table 3: The short-run response: extensions

Dependent Variable:	ΔC Dollar change in			ΔlnC Percent change in			ΔC Dollar change in		
	Strictly Non-durables	Non-durable goods	Total Cons	Strictly Non-durables	Non-durable goods	Total Cons	Strictly Non-durables	Non-durable goods	Total Cons
Estimation method:	OLS	OLS	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS
	Panel A: Control for ever receiving a stimulus payment								
ESP	0.073	0.117	0.507				0.071	0.123	0.509
	(0.050)	(0.060)	(0.196)				(0.068)	(0.081)	(0.253)
I(ESP)				2.20	2.63	3.97			
				(1.09)	(1.07)	(1.34)			
I(Total ESP>0)	11.98	9.53	21.15	-0.01	-0.01	-0.01	12.64	8.19	20.71
	(30.73)	(36.06)	(104.00)	(0.01)	(0.01)	(0.01)	(33.03)	(38.79)	(112.20)
N	17,478	17,478	17,478	17,475	17,478	17,478	17,478	17,478	17,478
	Panel B: Only households receiving payments								
ESP	0.144	0.185	0.683				0.207	0.252	0.865
	(0.054)	(0.066)	(0.219)				(0.087)	(0.103)	(0.329)
I(ESP)				3.97	3.91	5.63			
				(1.36)	(1.34)	(1.69)			
N	11,239	11,239	11,239	11,238	11,239	11,239	11,239	11,239	11,239
	Panel C: Only households receiving non-late payments								
ESP	0.188	0.214	0.590				0.262	0.308	0.911
	(0.058)	(0.070)	(0.217)				(0.092)	(0.112)	(0.342)
I(ESP)				4.61	4.52	6.05			
				(1.53)	(1.50)	(1.89)			
N	10,488	10,488	10,488	10,487	10,488	10,488	10,488	10,488	10,488

Notes: All regressions also include the change in the number of adults, the change in the number of children, the age of the household, and a full set of month dummies. Reported standard errors are adjusted for arbitrary within-household correlations and heteroskedasticity. The coefficients in the second triplet of columns are multiplied by 100 so as to report a percent change. The final triplet of columns reports results from 2SLS regressions where *I(ESP>0)* with the other regressors are used as instruments for *ESP*. *I(Total ESP >0)* is an indicator for households that received a payment in some reference quarter, whereas *I(ESP >0)* indicates receipt in the contemporaneous quarter (*t+1*) in particular.

Table 4: Paper checks versus direct deposit (EFT)

Dependent Variable:	ΔC Dollar change in			ΔlnC Percent change in			ΔC Dollar change in		
	Strictly Non-durables	Non-durable goods	Total Cons	Strictly Non-durables	Non-durable goods	Total Cons	Strictly Non-durables	Non-durable goods	Total Cons
Estimation method:	OLS	OLS	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS
	Panel A: All households								
Check	0.110	0.153	0.543				0.082	0.138	0.474
	(0.057)	(0.068)	(0.192)				(0.072)	(0.087)	(0.256)
EFT	0.088	0.134	0.518				0.107	0.153	0.568
	(0.058)	(0.072)	(0.264)				(0.071)	(0.083)	(0.274)
I(Check)				1.65	1.90	3.14			
				(1.11)	(1.09)	(1.35)			
I(EFT)				2.27	2.67	3.06			
				(1.21)	(1.18)	(1.52)			
N	17,415	17,415	17,415	17,412	17,415	17,415	17,415	17,415	17,415
	Panel B: Households receiving non-late payments								
Check	0.227	0.257	0.775				0.279	0.335	0.964
	(0.068)	(0.081)	(0.226)				(0.103)	(0.125)	(0.367)
EFT	0.186	0.210	0.372				0.275	0.304	0.708
	(0.066)	(0.084)	(0.290)				(0.091)	(0.111)	(0.372)
I(Check)				4.44	4.32	5.92			
				(1.59)	(1.55)	(1.94)			
I(EFT)				5.06	4.76	4.50			
				(1.69)	(1.67)	(2.18)			
N	10,451	10,451	10,451	10,450	10,451	10,451	10,451	10,451	10,451

	Panel C: Average response, with separate controls for check vs EFT recipients								
ESP	0.187	0.211	0.529				0.240	0.262	0.784
	(0.066)	(0.078)	(0.232)				(0.128)	(0.149)	(0.401)
I(ESP)				3.96	3.64	5.48			
				(1.87)	(1.79)	(2.23)			
N	10,362	10,362	10,362	10,361	10,362	10,362	10,362	10,362	10,362

Notes: All regressions also include the change in the number of adults, the change in the number of children, the age of the household, and a full set of month dummies. In panel C, there are separate sets of these covariates for a) households that ever received only paper payment checks, b) households that ever received only direct deposit EFTs, and c) households that ever received both paper checks and EFTs. In panels B and C, sample includes only households receiving non-late payments. Reported standard errors are adjusted for arbitrary within-household correlations and heteroskedasticity. The coefficients in the second triplet of coumns are multiplied by 100 so as to report a percent change. The final triplet of columns reports results from 2SLS regressions where *I(ESP>0)* with the other regressors are used as instruments for *ESP*.

Table 5: The longer-run response of expenditures to the stimulus payment

Dependent Variable:	ΔC Dollar change in			ΔlnC Percent change in			ΔC Dollar change in		
	Strictly Non-durables	Non-durable goods	Total Cons	Strictly Non-durables	Non-durable goods	Total Cons	Strictly Non-durables	Non-durable goods	Total Cons
Estimation method:	OLS	OLS	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS
ESP_{t+1} or $I(ESP_{t+1})$	0.186	0.201	0.517	3.58	3.92	4.96	0.219	0.254	0.757
	(0.055)	(0.067)	(0.211)	(1.58)	(1.55)	(1.96)	(0.089)	(0.110)	(0.360)
ESP_t or $I(ESP_t)$	-0.009	-0.054	-0.288	-2.09	-1.23	-2.22	-0.076	-0.097	-0.278
	(0.068)	(0.081)	(0.214)	(1.51)	(1.50)	(1.92)	(0.093)	(0.113)	(0.330)
Implied cumulative fraction of *ESP* spent over both three-month periods	0.362	0.347	0.747	NA	NA	NA	0.363	0.410	1.235
	(0.128)	(0.155)	(0.477)				(0.218)	(0.273)	(0.892)
N	10,488	10,488	10,488	10,487	10,488	10,488	10,488	10,488	10,488

Notes: All regressions also include the change in the number of adults, the change in the number of children, the age of the household, and a full set of month dummies. Sample includes only households receiving non-late payments. Standard errors are adjusted for arbitrary within-household correlations and heteroskedasticity. The coefficients in the second triplet of coumns are multiplied by 100 so as to report a percent change. The final triplet of columns reports results from 2SLS regressions where *I(ESP)* and its lag, along with the other regressors, are used as instruments for *ESP* and its lag.

Table 6: The propensity to spend across different households

Dependent variable: ΔC_{t+1}

Dollar change in:	Strictly Non-dur. goods	Non-durable goods	Total Cons	Strictly Non-dur. goods	Non-durable goods	Total Cons	Strictly Non-dur. goods	Non-durable goods	Total Cons
Interaction:		Age Low: ≤ 40 High: age >58			Income Low: ≤ 32,000 High: > 75,000			Liquid Assets Low: ≤ 500 High: > 7,000	
ESP *(Base group)*	0.269 (0.110)	0.345 (0.133)	0.952 (0.398)	0.157 (0.096)	0.215 (0.124)	0.568 (0.442)	0.297 (0.134)	0.275 (0.164)	0.851 (0.558)
*ESP*Low* *(Low group difference)*	-0.103 (0.101)	-0.150 (0.124)	-0.461 (0.399)	0.096 (0.121)	0.024 (0.155)	0.715 (0.500)	-0.181 (0.156)	-0.253 (0.184)	-0.844 (0.527)
*ESP*High* *(High group difference)*	0.100 (0.121)	0.044 (0.151)	0.415 (0.472)	0.026 (0.113)	-0.009 (0.139)	0.205 (0.466)	-0.051 (0.154)	-0.075 (0.186)	0.083 (0.631)
N	10,488	10,488	10,488	8,592	8,592	8,592	5,071	5,071	5,071
	Implied total spending for interacted groups								
Low group	0.166 (0.092)	0.195 (0.114)	0.491 (0.394)	0.254 (0.137)	0.239 (0.180)	1.282 (0.564)	0.116 (0.175)	0.022 (0.205)	0.007 (0.566)
High group	0.370 (0.136)	0.389 (0.168)	1.366 (0.498)	0.183 (0.105)	0.206 (0.133)	0.773 (0.463)	0.246 (0.162)	0.200 (0.202)	0.934 (0.677)

Notes: All regressions also include separate intercepts for the High and Low groups, the change in the number of adults, the change in the number of children, the age of the household, and a full set of month dummies. Sample includes only households receiving non-late payments. All results are from 2SLS regressions where *I (ESP>0)* and its interactions, along with the other regressors, are used as instruments for *ESP* and its interactions. Reported standard errors are adjusted for arbitrary within-household correlations and heteroskedasticity. All sample splits are chosen to include about 1/3 of payment recipients in each grouping.

Table 7: The propensity to spend: by housing status and self-reported usage

Dependent variable: ΔC_{t+1}

Dollar change in:	Strictly Non-dur. goods	Non-durable goods	Total Cons	Strictly Non-dur. goods	Non-durable goods	Total Cons
Interaction:	Housing Status baseline group: renters (23%) split1: owners with mortgage (50%) split2: owners w/o mortgage (27%)			Self-reported Usage baseline group: mostly save (18%) split1: mostly spend (32%) split2: pay down debt (50%)		
ESP *(Base group)*	0.197 (0.128)	0.213 (0.153)	0.431 (0.455)	0.230 (0.131)	0.173 (0.162)	0.951 (0.465)
*ESP*split1* *(split1 group difference)*	0.030 (0.110)	0.043 (0.130)	0.543 (0.394)	0.158 (0.136)	0.349 (0.169)	0.755 (0.496)
*ESP*split2* *(split2 group difference)*	0.175 (0.133)	0.260 (0.169)	0.800 (0.514)	-0.005 (0.126)	0.098 (0.156)	-0.320 (0.453)
N	10,380	10,380	10,380	10,072	10,072	10,072
	Implied total spending for interacted groups					
split1 group	0.226 (0.093)	0.256 (0.112)	0.974 (0.364)	0.388 (0.115)	0.522 (0.143)	1.706 (0.457)
split2 group	0.372 (0.135)	0.473 (0.175)	1.231 (0.508)	0.225 (0.106)	0.271 (0.131)	0.632 (0.393)

Notes: All regressions also include separate intercepts for the non-baseline groups, the change in the number of adults, the change in the number of children, the age of the household, and a full set of month dummies. Sample includes only households receiving non-late payments. All results are from 2SLS regressions where *I (ESP>0)* and its interactions, along with the other regressors, are used as instruments for ESP and its interactions. Reported standard errors are adjusted for arbitrary within-household correlations and heteroskedasticity.

Table 8: The propensity to spend on different categories of goods: Nondurable goods

Dependent variable: ΔC_{t+1}

Dollar change in:	Panel A: Food			Panel B: Additional strictly nondurable goods				Panel C: Additional nondurable goods		
	Food at home	Food away from home	Alcoholic beverages	Utilities, Household operations	Personal care and misc.	Gas, motor fuel, public transportation	Tobacco products	Apparel	Health	Reading
ESP	0.050	0.025	0.011	0.059	0.083	0.027	0.007	0.022	0.025	-0.001
	(0.032)	(0.034)	(0.007)	(0.027)	(0.049)	(0.039)	(0.009)	(0.021)	(0.048)	(0.003)
Share of estimated nondurable spending	0.161	0.080	0.035	0.193	0.270	0.088	0.023	0.072	0.081	-0.003
	(0.103)	(0.109)	(0.023)	(0.089)	(0.158)	(0.127)	(0.030)	(0.068)	(0.156)	(0.009)
Avg share of nondurable goods	0.250	0.100	0.014	0.238	0.035	0.153	0.017	0.051	0.137	0.006

Notes: N=10,488 for all regressions. All regressions also include the change in the number of adults, the change in the number of children, the age of the household, and a full set of month dummies. Sample includes only households receiving non-late payments. Reported standard errors are adjusted for arbitrary within-household correlations and heteroskedasticity. All results are from 2SLS regressions where *I(ESP)* , along with the other regressors, are used as instruments for *ESP*.

Table 9: The propensity to spend across durable goods

Dependent variable: ΔC_{t+1}

Dollar change in:	Housing	Transportation	Entertainment	Education
ESP	0.099	0.527	0.077	-0.100
	(0.092)	(0.269)	(0.099)	(0.042)
Share of estimated durables spending	0.165	0.874	0.127	-0.166
	(0.153)	(0.447)	(0.164)	(0.070)
Avg share of durable goods	0.618	0.200	0.159	0.023

Notes: N=10,488 for all regressions. All regressions also include the change in the number of adults, the change in the number of children, the age of the household, and a full set of month dummies. Sample includes only households receiving non-late payments. Reported standard errors are adjusted for arbitrary within-household correlations and heteroskedasticity. All results are from 2SLS regressions where *I(ESP)* , along with the other regressors, are used as instruments for *ESP*.