Consumer Spending and the Economic Stimulus Payments of 2008*

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September 2010 (First draft December 2009)

Abstract: We measure the response of household spending to the economic stimulus payments (ESPs) disbursed in mid-2008, using special questions added to the Consumer Expenditure Survey and variation arising from the randomized timing of when the payments were disbursed. We find that, on average, households spent about 12-30% (depending on the specification) of their stimulus payments on non-durable expenditures during the three-month period in which the payments were received. Further, there was also a substantial and significant increase in spending on durable goods, in particular vehicles, bringing the average total spending response to about 50-90% of the payments. Relative to research on the 2001 tax rebates, these spending responses are estimated with greater precision using the randomized timing variation. The estimated responses are substantial and significant for older, lower-income, and home-owning households. We further extend the literature in two ways. First, we find little evidence that the propensity to spend varies with the means of delivery (paper check versus electronic transfer). Second, we evaluate a complementary methodology for quantifying the impact of tax cuts, which asks consumers to self-report whether they spent their tax cuts. The response of spending to the ESPs is indeed largest for self-reported spenders. However, self-reported savers also spent a significant fraction of the payments.

* We thank Jeffrey Campbell, Joel Slemrod and session participants at the 2009 ASSA meeting, and seminar participants at the Board of Governors of the Federal Reserve System, the Federal Reserve Bank of Chicago, Kellogg, Michigan, Princeton, Wisconsin and Wharton for helpful comments. We thank the staff of the Division of Consumer Expenditure Surveys at the Bureau of Labor Statistics for their work in getting the economic stimulus payment questions added to the Consumer Expenditure Survey. Parker thanks the Zell Center at the Kellogg School of Management for funding. The views expressed in this paper are those of the authors and do not necessarily correspond to those of the U.S. Census Bureau or the Bureau of Labor Statistics. Parker: Kellogg School of Management, Northwestern University, http://www.kellogg.northwestern.edu/ faculty/parker/htm/; Souleles: The Wharton School, University of Pennsylvania, http://finance.wharton.upenn.edu/ ~souleles/; Johnson: Division of Housing and Household Economic Statistics, U.S. Census Bureau; McClelland: Division of Price and Index Number Research, Bureau of Labor Statistics.

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 U.S. tax filers. The desirability of this historically-important use of fiscal policy depends critically on the extent to which these tax cuts directly changed household spending, as well on any subsequent multiplier or price effects.

In this paper, we measure the direct spending effect caused by the receipt of the ESPs, the existence of which is a necessary (though not sufficient) condition for the efficacy of this counter-cyclical policy. We begin by measuring the average spending response of households, using variation in the randomized timing of when the ESPs were disbursed. Further, to help improve the understanding of consumption in this recession and our models of consumer behavior in general, we also analyze the heterogeneity in the spending response across households with different characteristics and across different categories of consumption spending. Finally, we evaluate whether another well-known and complementary methodological approach to identifying the impact of tax cuts -- asking consumers to self-report whether they spent (or intend to spend) their tax cuts (e.g., Shapiro and Slemrod, 1995) -- indeed identifies households that do and do not actually spend their tax cuts.

We measure the spending effects of the 2008 ESPs using a natural experiment provided by the structure of the payments. The ESPs 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 funds transfers (EFTs) into their bank accounts. Most importantly, within each delivery method, the timing of receipt was determined by the final two digits of the recipient's Social Security number (SSN), 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

[1] The last four digits of an 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).

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 expenditures for a stratified random sample of U.S. households. These supplemental questions ask CE households to report the amount and month of receipt of each stimulus payment they received, as well as the means of receipt of each payment (mailed paper check versus EFT). These questions allow us to measure the spending impact of the payments and to study the extent to which the means of receipt influences the propensity to spend. We also designed and added a question that 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). Our question asks households who previously reported receiving payments to directly report whether their payments were mostly spent, mostly saved, or mostly used to pay down debt.

Summarizing our main findings, on average households spent about 12-30% of their stimulus payments, depending on the specification, on non-durable consumption goods and services (as defined in the CE survey) 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 of a transitory increase in income like that induced by the one-time payments. We also find a significant effect on the purchase of durable goods and related services, primarily the purchase of vehicles, bringing the average response of total consumption expenditures to about 50-90% of the payments in the quarter of ESP receipt.

These results are statistically and economically broadly consistent 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. The estimated spending responses are statistically and economically similar for ESPs received by EFT compared to those received by mail, although there is little temporal variation in the former group with which to identify the key effect. We also find some evidence of an ongoing though smaller response in the subsequent

three-month period following that of the receipt of an ESP, but this response cannot be estimated with precision.

The point estimates of the fraction of the ESPs spent suggest that, relative to the effects of the 2001 tax rebates estimated in Johnson, Parker, and Souleles (2006) (JPS), in 2008 the spending effect was slightly smaller for nondurable expenditures but more targeted towards durables. While this finding may be due to sampling error, it may also reflect some of the differences in the details of the tax cut 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, consistent with some prior research that finds that larger payments 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.[2]

Our results suggest a significant macroeconomic effect of the 2008 ESPs. The point estimates imply that the ESPs directly caused an increase in consumer demand for CE-defined nondurable expenditures of $32 to $80 billion (at an annual rate) in the second quarter of 2008 and $15 to $36 billion (at an annual rate) in the third quarter. For total CE spending, our estimates imply a direct increase of about 1.3 to 2.3 percent of personal consumption expenditures (PCE) in the second quarter, and 0.6 to 1.0 percent of PCE in the third quarter (again at annual rates).[3] We return to these numbers in the conclusion, but here note again that these direct effects on nominal spending demand may have also led to higher prices (not only increases in real spending) and/or additional spending through multiplier effects.

As for results that further inform theories of credit markets and consumer behavior, across households, the responses are largest for older and low-income households, groups which have substantial and statistically significant spending responses. According to the point estimates, the responses are largest for high-asset households but this spending response is not

[2] 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).

[3] These figures are based on estimates in Tables 4 and 5 and so omit statistically-insignificant lagged spending. The calculations assume that the contemporaneous estimates represent spending done in the month of receipt and the month after. Using estimates from Table 7 that include lagged spending effects, the corresponding estimates are, for CE-defined nondurable expenditures, $66 billion in the second quarter and $75 billion in the third, and for total spending, $198 billion in the second quarter and $227 billion in the third, or 1.9 and 2.2 percent of PCE.

statistically significantly different from zero. Further, motivated by the collapse of the housing market in 2008, we find that homeowners on average spent more of their ESPs than did renters, a difference that is statistically significant at the ten percent level.

Finally, turning to the evaluation of self-reports, we find that households that self-report that they mostly spent their ESPs on average did spend more than self-reported savers, yet self-reported savers (including those reporting they reduced debt) also spent a statistically and economically significant fraction of their payments. This suggests that relying on self-reports can understate the actual causal change in spending in response to changes in income like tax cuts.

In addition to analyzing the amount of spending directly caused by the 2008 ESPs, our paper builds on the related literature in a number of ways. First, relative to JPS, as discussed below, we measure with greater precision the response of spending using only random variation in the timing of ESP receipt. Second, we consider whether the delivery method (check versus EFT) affects the amount of spending. This is an important consideration since the 2008 tax cut was the first large tax cut to use EFTs, and EFTs seem likely to be used increasingly frequently in the future. Third, we evaluate the accuracy of the "subjective" self-reported responses to the payments, by comparing the self-reports to our causal estimates using the data on actual spending and ESP receipt. Such an evaluation is useful given the potential benefits of such subjective surveys: e.g., they can be put into the field and analyzed quickly after policy changes, and they can be used to evaluate hypothetical policies and the relevance of different theoretical reasons for households' reported behavior. As a result, the results from such surveys are widely reported.

This paper is structured as follows. Sections I and II briefly describe the literature and relevant aspects of ESA 2008. Section III describes the CE 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. Sections VII and VIII examine the differences in response across different households, and across different categories of expenditure, respectively. After a concluding section, the Appendices contain additional information about ESA 2008 and the data.

I. Related Literature

Of the many papers that test the consumption-smoothing implications of the rational-expectations LCPIH, the most closely related to our work is the set of papers that use household-level data and quasi-experiments to identify the effects on consumption from predictable changes in income, in particular changes caused by tax policy. Deaton (1992), Browning and Lusardi (1996), JPS, and Jappelli and Pistaferri (2010) review these literatures well.[4]

Our paper is most closely related to JPS, which uses a similar module of questions appended to the CE survey to study the 2001 income tax rebates. JPS finds a relatively large response in nondurable expenditure, amounting to about 20-40% of the rebates on average during the three-month period in which they were received, but no significant response in durable goods. Unlike the current study, however, JPS is unable to identify the response in nondurables with precision using only the random variation in timing of rebate receipt. JPS finds larger than average responses for households with low liquid wealth or low income, and a significant though decaying lagged spending effect, so that on average roughly two-thirds of the rebates was spent cumulatively during the quarter of receipt and subsequent three-month period.

Agarwal, Liu, and Souleles (2007) finds consistent results using credit card data and direct indicators of being credit constrained; in particular, the spending responses are largest for consumers that are constrained by their credit limits. 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. Johnson, Parker, and Souleles (2009) finds qualitatively similar responses to the 2003 child tax credit payments, using CE data.[5]

A few other studies also investigate the 2008 ESPs. First, 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 than average for low asset and low income households. Second, using data from a payday lender, Bertrand and Morse (2009) finds that receipt of an ESP initially reduces the probability of taking out a payday loan. The

[4] For a survey of recent fiscal policy, see e.g., Auerbach and Gale (2009).
[5] Coronado, Lupton, and Sheiner (2006) also study the 2003 child payments, using the Michigan Survey.

magnitude of the reduction in debt is modest relative to the ESPs, and, after two cycles, borrowing returns to its pre-ESP level on average. For the most constrained borrowers, by contrast, debt does not decline, consistent with the spending dynamics discussed in Agarwal, Liu, and Souleles (2007).

Third, Shapiro and Slemrod (2009) use the Michigan Survey to analyze the 2008 stimulus payments, and finds similar results as in Shapiro and Slemrod (2003a), 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 Michigan survey results provide no clear evidence of greater spending by low-income or potentially constrained households.[6]

Finally, Bureau of Labor Statistics (2009) reports various summary statistics about the CE data on the ESPs and self-reported usage. Nearly half of CE households reported that they used their ESPs mostly to pay down debt, 18% reported they mostly saved their ESP, and 30% reported that they mostly spent it, more than found in Shapiro and Slemrod (2009).

II. The 2008 Economic Stimulus Payments

ESA 2008 provided ESPs to the majority of U.S. households (roughly 85% of "tax units"). The ESP consisted of a basic payment 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 eligible households, the basic payment was generally

[6] In 2008, of the 80 percent of respondents who report they will mostly save their ESP, the majority (about 60 percent) report that they will mostly pay down debt (as opposed to accumulate assets). See also Sahm, Shapiro and Slemrod (2010). The Michigan Survey includes additional subjective questions about expected future spending. 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.

[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.

the maximum of $300 ($600 for couples filing jointly) and their tax liability up to $600 ($1,200 for couples). Households without tax liability received basic payments of $300 ($600 for couples), so long as they had at least $3,000 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). Given this structure, the stimulus payments were more targeted to lower-income households than were the 2001 income tax rebates.

The key to our measurement strategy is that the timing of ESP disbursement was effectively randomized across households. Table 1 shows the schedule of ESP disbursement. For recipients who had provided the IRS with their personal bank routing number (i.e., for direct deposit of a tax refund), the stimulus payments were disbursed electronically over a three-week period ranging from late April to mid May. [8] The IRS mailed a notice to the recipients in advance of the EFTs. Appendix A provides an example of this notice. For households that did not provide a personal bank routing number, the payments were mailed using paper checks over a nine-week period ranging from mid May to mid July.[9] The recipients of these payments received a similar notice in advance.[10] Importantly, within each method of disbursement, the particular timing of the payment was determined by the last two digits of the recipients' Social Security numbers, which are effectively randomly assigned.

[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" or paying their tax preparation fee from their refund. These situations (in which the tax preparer's number is used) 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 payments. Finally, due to human and computer error, about 350,000 households (less than 1 percent) did not receive the child tax credit component of their ESP with their main ESP. The IRS took steps to identify these households and sent all affected households paper checks for the amount due for just the child credit, starting in early July.

[10] For paper checks, the notices were mailed about a week before the checks were mailed. For EFTs, the notices were sent a couple of business days before the direct deposits were supposed to be credited. The recipients' banks also were notified a couple of days before the date of the electronic transfers, and some banks might have credited some of the electronic payments to the recipients' accounts a day or more before the official payment date. For example, some EFTs deposited on Monday April 28 were reported to the banks on Thursday April 24, and some banks seem to have credited accounts on Friday April 25.

In aggregate the stimulus payments in 2008 were historically large, amounting to about $100 billion, which in real terms is double the size of the 2001 rebate program. According to the Department of the Treasury (2008), $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 PCE. The stimulus payments constituted about two-thirds of the total ESA package, which also included various business incentives and foreclosure relief.[11] This paper focuses on the stimulus payments, as recorded in our CE dataset.

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 five times. After an initial interview that collects demographic and income information, households are interviewed up to four more times, at three month intervals. In interviews 2 through 5 (after the introductory interview 1), households report their expenditures during the preceding three months (the "reference period"). In addition to surveying households about their expenditures, the CE survey also gathers (less-frequent) information about demographic characteristics, income, and wealth. New households are added to the survey every month, so the data can be used to identify spending effects from ESPs disbursed in different months. We use the 2007 and 2008 waves of the CE data (which include interviews in the first quarter of 2009).

Two special modules of questions about the 2008 ESPs were added to the CE survey in interviews conducted between June 2008 and March 2009, which covers the crucial time during which the payments were disbursed.[12] The first module of questions was phrased to be consistent with the style of other CE questions and the 2001 tax rebate questions. These 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 and if so, the amount of each payment and the date it was received. Going beyond the 2001 questions, this first module also

[11] For more details on ESA, see e.g., CCH (2008) and Sahm, Shapiro and Slemrod (2010).
[12] Ideally, since some ESPs arrived in April, the survey would have been in the field in May, e.g. for respondents whose last interview was in May.

asked, for each payment, whether it was received by check or direct deposit. These questions were asked in all five CE interviews (including the first).[13]

The second module, also new in 2008, was asked only once and only of households that had previously reported a payment. These households were asked whether the payment led them "mostly to increase spending, mostly to increase savings, or mostly to pay off debt." The wording of this question closely follows the main question in the Michigan Survey of Consumers analyzed by Shapiro and Slemrod (2009). Appendix B contains the language of the survey instruments.

Turning to our use of the data, for each household-reference period, we follow JPS and sum all stimulus payments received by the household in that three-month period to create our main economic stimulus payment variable, *ESP*. 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 alcoholic beverages. Much previous research has studied such expenditure on food, largely because of its availability in most years of the Panel Study of Income Dynamics, but it is a narrow measure of expenditure. Our second measure of consumption expenditures is a subset of nondurable expenditures, "strictly nondurable" expenditures, which follows Lusardi (1996) and includes CE categories like utilities, household operations, gas, and personal care. Third, our broadest and main measure of spending on nondurable goods and services is nondurable expenditure, which follows previous research using the CE survey and includes semi-durable categories like apparel, health and reading materials. Finally, total expenditures also includes durable expenditures such as home furnishings, entertainment equipment, and auto purchases.[14] Appendix C provides further details about the data.

For our analysis, we use only data on households that have at least one interview during the period in which the ESP questions were in the field. The resulting sample period starts with interviews in September 2007 (when period *t* in equation (1) below covers expenditures in June

[13] In interview 1, the reference period is only one month.

[14] Unlike in JPS, we find that the spending effect on total expenditures in 2008 is estimated with relative statistical precision. This could in part reflect the larger number of payments (about 30 percent more) in the sample in 2008, and the larger size (over double) of these payments. Indicative of an improvement in data quality, there is also a decline in the ratio of the standard deviation of the change in household-level expenditures to the average level of expenditures between 2001 and 2008 for all our major categories, which is likely related to the CE survey's transition in 2003 from using survey booklets to using computer-assisted CAPI software.

to August 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.[15]

Figure 1 shows our calculations of the aggregate amount of ESPs reported in the raw CE data by month, and the corresponding amount of ESP disbursement reported in the Daily Treasury Statements (DTS) (Department of the Treasury (2008)). During 2008, the ESPs reported in the CE survey aggregate to $94.6 billion while those reported in the DTS aggregate to $96.2 billion. The temporal pattern of ESP receipt is broadly similar across the two sources, but the CE data has fewer ESPs reported during the peak month of May and more in the following months. This suggests the possibility that some households took time to notice their receipt of an ESP, or that there is tendency to report a somewhat later date of receipt than actually occurred.

Table 2 presents summary statistics for our final full sample and subsamples that we further analyze. The average value of *ESP*, conditional on a positive value, is about $1000. Households that receive ESPs by EFT have slightly higher expenditures, are slightly younger, have higher incomes and liquid assets, and have larger ESPs, than households that receive the payments by mail.

Table 3 shows more information about the distribution of ESPs in our dataset. Panel A shows that, consistent with the payments specified by ESA, most reported ESPs are in multiples of $300, with about 55% of reports reflecting just the (maximum) basic payments of $600 or $1,200. Panel B shows the pattern of ESPs by interview reference period. During the expenditure reference period that covers the main time of disbursement of the payments (May - July), about two-thirds of households report receiving a payment.

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:

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

[15] Our initial analysis of the ESP data uncovered a peculiar pattern in the raw data. When we notified the BLS, they determined that there had been an internal processing error, and worked rapidly to release a corrected version of the ESP data, and it is this corrected version that we use.

where i indexes households and t indexes time, C is either household 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 the average of all other concurrent aggregate factors; and X are control variables (here 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 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 $I(ESP>0)$, used 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 arrival of a stimulus payment.[16]

Most of the recent literature on the LCPIH focuses on *testing* the null hypothesis that β_2 is zero using variation in predictable changes in income and the *assumption* that the residual ($u_{i,t+1}$) is orthogonal to all information potentially known to a household at the start of period t (such as the predictable change in income). By contrast, we can use the randomized timing of ESP receipt to ensure orthogonality between the residual and the predictable change in income that comes with the arrival of an ESP. This allows us to *estimate* β_2 and thus measure the causal effect of the payments on expenditure, regardless of whether the LCPIH is true or not. That said, our estimate still provides a direct test of the LCPIH.[17] The rational-expectations LCPIH (or Ricardian equivalence) implies that $\beta_2=0$. Even if instead households were actually surprised by

[16] 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 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.

[17] 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 bias 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). Most importantly, heterogeneity in such wealth effects (or in β_2) should not be correlated with the timing of ESP receipt, so (the average) β_2 should still be estimated consistently.

the payment, β_2 should still be small under the LCPIH, because the one-time payment represents a transitory increase in income.

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). We begin by estimating (the average) β_2 in the full sample using all available variation. While this variation is analogous to that used in most of the previous LCPIH literature, we can go further and assess the validity of this variation. We refine our identification strategy by dropping non-recipients and late recipients from our sample and by using only the variation in the timing of ESP receipt within each means of payment (check versus EFT). The following section estimates the lagged response to the payments.

A. Variation across all households

We begin by estimating equation (1) using all available households and using *ESP* as the key regressor, which utilizes all of the available information about the payments received by each household, including the dollar amount of the ESP. In Table 4, 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% of the payment, its strictly nondurable expenditures by 8% of the payment, and its nondurable expenditures by 12% 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% of the payment, a substantial and statistically significant amount. This result is relatively precisely estimated, especially considering that the difference with the preceding results largely 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. Unlike most previous literature, we can refine the variation that we use, in order to assess its validity.

The remaining columns of Table 4 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 nondurable expenditures by about $120, which is statistically significant at the 7% level. Total expenditures increased by a significant $495. Compared to an average payment of just under $1,000, 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 4 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 expenditures increased by 2.1%, and total expenditures increased by 3.2%. These are again statistically and economically significant effects. At the average ESP and level of nondurable and total expenditures (Table 2), these results would imply propensities to spend of 0.116 and 0.354 respectively, which are consistent with, though slightly smaller than, the previous results in the table.

Finally, to estimate a value interpretable as a marginal propensity to spend upon the payment's arrival without using variation in ESP amount, 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. As shown in the last set of columns in Table 4, 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. The findings in Table 4 are generally robust across a number of additional sensitivity checks.[18]

B. Variation among households that receive ESPs at some time

The results in Table 4 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. We now investigate the role of this variation using a number of different approaches, for brevity focusing on strictly nondurable expenditures, nondurable expenditures, and total expenditures.

First, in Table 5, Panel A adds to equation (1) an indicator for households that received a payment in *any* reference quarter, $I(\Sigma_{household}\ ESP>0)$, 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 higher-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 5, the estimated coefficients on $I(\Sigma_{household}\ ESP>0)$ 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 4. Hence the results in Table 4 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.

[18] For example, using median regressions or winsorizing the dependent variable lead to very similar results for food, strictly nondurables, and nondurables. For total expenditures, the resulting coefficients are generally smaller than in Table 4, though still statistically and economically significant (substantially larger than those for nondurable expenditures). This reduction in point estimates for total expenditures is consistent with iatrogenic bias, since the distribution of expenditure changes (dC) has much more of its mass in the tails for total expenditures than for nondurable expenditures. In particular, below we find that much of durable spending is the purchase of cars. If the ESPs cause car purchases, then by dropping these "outliers," one obviously biases down the estimates of the average spending caused by the ESP. Weighting the sample leads to very similar results as in Table 4, for all four expenditure aggregates.

Our second approach is more stringent. Panel B of Table 5 excludes from the sample all households that did not report a payment in any of their reference quarters. The advantage of this approach is that, when we do not use variation in ESP amount, the response of spending is identified using only the variation in the timing of payment receipt conditional on receipt. That is, identification comes from 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 this 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, we focus on the randomized variation in the timing of ESP receipt by dropping all households that received late stimulus payments, after the main period of their (randomized) disbursement. Although the timing of late payments is not necessarily endogenous, it is not randomized. The vast majority of households that received late ESPs did so due to filing late tax returns for tax-year 2007, although as seen in Figure 2, there also seem to be some lags in reporting (or in noticing) the payments that lead to a later pattern of receipt in the CE survey. We follow JPS and allow one month's 'grace period' in excluding late ESPs so that we consider a mailed payment late if it is reported received after August, and an electronic payment (or one with missing data on the means of payment) late if it is reported received after June.

Table 5 Panel C shows that the results remain statistically and economically significant. 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. Given that this approach has sufficient power to identify the key parameter of interest, we focus on this sample as our main sample for the balance of the paper.

As a robustness check, Figure 2 compares histograms of the distribution of changes in expenditure for observations during which an ESP is received versus observations during which an ESP is not received. The figure focuses on the sample of on-time recipients and the time period during which the ESPs were being distributed (i.e., when the t+1 interview occurs between June 2008 and October 2008). As shown, there is a larger share of recipients than non-

recipients in most ranges of increases in spending, and a larger share of non-recipients than recipients in most ranges of decreases in spending. (Each cell represents a $300 range in Panel A, and a $600 range in Panel B, so these differences are economically significant). While these histograms do not control for any covariates, they support our main findings non-parametrically in the raw data and show that outliers are not driving the main findings.[19]

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. By contrast, in JPS, analogously limiting the sample to non-late rebate recipients leads to a larger reduction in precision and a loss in statistical significance.

C. Means of receipt

One novel feature of the 2008 ESP program was the use of electronic funds transfers in addition to mailed checks. About 40% of the CE households received their payments via EFTs, and the use of EFTs is likely to increase in the future. This subsection first asks whether the method of payment affects the estimated spending impact of the ESPs. Second, since the means of ESP payment is not randomly assigned and affects the time of receipt, one can think of the ESP program as providing two natural experiments within distinct samples. Accordingly, we proceed to investigate whether the spending response differs across the two means of payment, and whether we can identify the causal effect of a payment from only the difference in arrival times within each group receiving each type of payment.

We begin by estimating the separate response of spending to EFTs and to paper checks, using the analogues of *ESP* and *I(ESP>0)* for ESPs received by check and by EFT. We start with the entire sample of households, including non-recipients, because there is limited temporal variation within ESPs received by EFT.[20] As shown in Panel A of Table 6, the estimated coefficients are generally similar (and not statistically significantly different) across the two delivery methods, across all the columns. If anything, the point estimates are slightly larger for the EFTs. Next, we use only the variation within the households that receive on-time ESPs. The results in Panel B are similar to those in Panel A in that the estimated coefficients are generally similar across the two delivery methods, though now the point estimates are slightly larger for the mailed checks. Not surprisingly, since the EFTs were disbursed over just a few weeks, using

[19]The analogous histograms are very similar for the regressions in Table 4 Panel D.
[20] A few observations have missing values for the method-of-delivery question, and so are dropped from the sample.

just timing variation leads to a significant reduction in power for estimating the effect of EFT receipt, especially for the noisier total expenditure category. And the smaller number of ESPs used to identify the effects of a mailed ESP also raise its standard errors. In sum, these results provide little evidence that the method of delivery affected the average response of spending.

We now turn to the question of whether we can identify the spending effect purely from randomized variation in spending within households that receive ESPs by check on time and within households that receive ESPs by EFT on time. This approach allows for the selection into each group to be non-random. 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).

Panels A and B, already discussed, provide some evidence that the spending effect does not differ by means of receipt. The coefficients in panel B in particular are identified from variation within each group. Importantly, for ESPs received by mail, which provide more temporal variation, the results are statistically significant and broadly similar to the average response in the final panel of Table 5. That is, even separately controlling for receipt of EFTs, using the random variation in the timing of the mailed checks still yields a significant response of spending to the mailed checks.

These results still impose common month dummies and demographic effects (age and changes in family size) across EFT and mailed-check recipients. Also, to gauge the impact of the stimulus program, we want to estimate the *average* response to the stimulus payments. Accordingly, Panel C of Table 6 presents estimates from 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.[21] The resulting coefficient measures the average spending effect of the receipt of an ESP independent of its means of delivery, but allowing for households to be distributed across the different possible means of payment in a way that is correlated with their spending dynamics due to other factors. The results are broadly

[21] About 2 percent of households received both EFTs and paper checks. 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 mailed checks. These coefficients are sometimes significantly different only for the few households who received both EFTs and paper checks, relative to the two main groups.

similar to the previous estimates, even though they are driven by the randomized variation in timing (primarily of paper checks, since the EFTs have limited timing variation).[22]

In sum, our findings remain broadly consistent across specifications that use different forms of variation. Of course, using different variation sometimes induces changes in the point estimates 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.

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). We continue to focus on the sample of households that only receive ESPs on time (Panel C of Table 5).

As show in Table 7, the presence of the lagged variable does not much alter our previous conclusions about the short-run impact of the payment, although the coefficients on ESP_{t+1} are slightly smaller than the corresponding results in Panel C of Table 5. More interestingly, 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 uniformly 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 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 than the contemporaneous spending effects. For example, in the second-to-last column, for nondurable expenditures using 2SLS, nondurable expenditures rise by 25.4% of the payment in the quarter of receipt. The expenditure change in the next quarter is -9.7%, so that nondurable expenditures in the second three-month period are still higher on net than before payment receipt by 25%-10% ≈ 15.7% of the payment (penultimate row of results). The *cumulative* change in nondurable

[22] An alternative is to treat the effect of ESPs received by EFT as a weakly identified nuisance parameter and include both ESP by mail and ESP by EFT in the regressions in Panel C. In this case, the coefficient on ESP by mail estimates the average effect of an ESP from variation in ESP by mail only among households that receive ESPs by mail. These results are very similar to those in Panel C.

expenditures over both three-month periods is then estimated to be 25.4% + 15.7% =41.1% 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 nondurables and total expenditures).[23]

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

This section and the next section analyze heterogeneity in the response to the stimulus payment, across different types of households and different subcategories of consumption expenditures, respectively. This analysis provides some 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 only non-late payments.

A. Spending propensities by age, income and liquid wealth

The presence of liquidity constraints is a leading explanation for why household spending might increase in response to a previously announced 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 Reis, 2006).

Expanding equation (1), we interact the intercept and ESP_{t+1} variable with indicator variables (*Low* and *High*) based on various household characteristics (all from households' first CE expenditure interview to minimize any endogeneity). We use three different proxy variables

[23] The findings are similar using the sample of all households.

to identify households that may be disproportionately likely to be 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.[24] 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 8 begins by testing whether the propensity to spend the stimulus payments 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).[25] 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 young households spent relatively less of the payment and old households spent relatively more. However these differences, while economically large, are not statistically significant. Nonetheless, in absolute terms the spending by old households (bottom panel for the interacted groups) and by the middle-age households (main panel for the baseline group) are both statistically and economically significant.

The second set of columns in Table 8 tests for differences in spending across income groups. The point estimates suggest that low-income households spent a much larger fraction of their payment on total expenditures relative to the typical (baseline middle-income) household. In absolute terms for total expenditures, of the three groups, only the response for the low-income households is statistically significant. The response is also economically significant, averaging about 125% of the payment.[26] However, while suggestive of possible role for liquidity

[24] 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).
[25] 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 on average.
[26] 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 payment.

constraints, the difference between this result and that for the baseline group, although economically large at about 70% of the ESP, is not statistically significant.

The last set of columns in Table 8 tests for differences by liquid asset holdings. While the point estimates suggest little spending by low-asset households, the associated confidence interval is quite large, and none of the differences (although large in point estimate) 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. For instance, if constrained households expect their constraints to bind for a year or two after receiving a payment, rather than for six months, this would reduce the magnitude of their current response to the payment.

B. Spending propensities by homeownership status

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, Table 9 presents estimates of the spending responses 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.[27, 28]

C. Spending propensities by self-reported spending propensities

[27] Combining homeowners into one group, the estimated spending responses for total expenditures are 1.051 (0.351) percent of the ESP for homeowners and 0.434 (0.454) percent for renters, and these estimates are statistically significantly different at the 10 percent level.

[28] 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 homeowner status. The coefficients for total expenditure remain very similar for renters and homeowners with mortgages. While the coefficient for total expenditure loses 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. In our sample of (non-late) ESP recipients, 32% reported that they mostly spent their payment, 18% reported they mostly saved it, and 50% reported they mostly used it to pay down debt.[29] We interact ESP with indicator variables for self-reports of mostly spend and mostly pay down debt, with mostly save being the baseline category.

Supporting the use of self-reports, Table 10 shows that households reporting that they mostly spent their ESPs did in fact spend more of the payment than the other groups, according to the point estimates. In absolute terms their spending is statistically and economically significant, across all the expenditure categories. In relative terms, they spent about 35% more of the payment on nondurable expenditures 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, at 75% of the ESP on average, but is not statistically significant.

On the other hand, even for the self-reported "non-spenders," the receipt of an ESP caused significant spending. 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%, albeit statistically insignificant, and the response of nondurable expenditures is statistically significant and still rather large at 27% of the payment. In this sense self-reported spending may understate the actual amount of spending (see also Agarwal, Liu, and Souleles, 2007)).

VIII. Differences in Responses across Types of Expenditure

Turning to differences across the different types of expenditures, each column in Table 11 reports the estimated change in spending for each subcategory of expenditures within the broad measure of nondurable expenditures (a complete decomposition). The columns also report, in the bottom panel, the share of the estimated overall increase in nondurable expenditures due to the ESPs that is accounted for by each of the subcategories, and for benchmarking, the average share of each subcategory in nondurable expenditures. Of course, comparisons of different

[29] These results are very close to those in Bureau of Labor Statistics (2009), reported above, which used the entire sample of data on self-reported usage, without considering its relation to actual spending. There is little variation in ESP amount across spenders and savers, but larger ESPs among those who report paying off debt. There is little variation in self-reported spending versus saving across many demographic characteristics, such as income, except that single parents were more likely to report spending.

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 estimated coefficient for utilities and household operations being statistically significant. This response is roughly in proportion to the share of this subcategory in nondurable expenditures. As for the other categories, 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 expenditures 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 expenditures, 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).

Panel A of Table 12 provides the analogous decomposition of the response of the durable goods and services part of total expenditures (*i.e.*, the part of total expenditures not in the nondurables category). 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 not large relative to their category 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. This response is large relative to the share of transportation in durable expenditures. Panel B in turn decomposes the response of the different subcategories of transportation. According to the point estimates, the transportation response is largely driven by purchases of vehicles, primarily new vehicles. The results imply that auto purchases, although weakening during the recession, would have been even weaker in the absence of the payments.

In sum, 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.[30]

IX. Conclusion

[30] This finding is consistent with the previous estimates that suggested a propensity to spend greater than one for certain sub-populations. If the receipt of an ESP leads a household to purchase a vehicle, then their spending response would be many multiples of the ESP.

We find that on average households spent about 12-30% of their stimulus payments, depending on the specification, on (CE-defined) nondurable expenditures during the three-month period in which the payments were received. We also find a significant effect on the purchase of durable goods, primarily the purchase of new vehicles, bringing the average response of total consumption expenditures to about 50-90% of the payments in the quarter of receipt. These results are statistically and economically significant. They remain broadly consistent and significant across specifications that use different forms of variation. Indeed, the point estimates of the spending effect are at the high end of these ranges in specifications that focus most directly on randomized timing variation.

The estimated spending response on nondurable expenditures is generally smaller in magnitude (though not significantly different) than the response in nondurables 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, so that the estimated spending effect on total expenditures is larger than that in 2001 due to a larger role for durables in 2008, perhaps reflecting the larger size of the payments in 2008.

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.

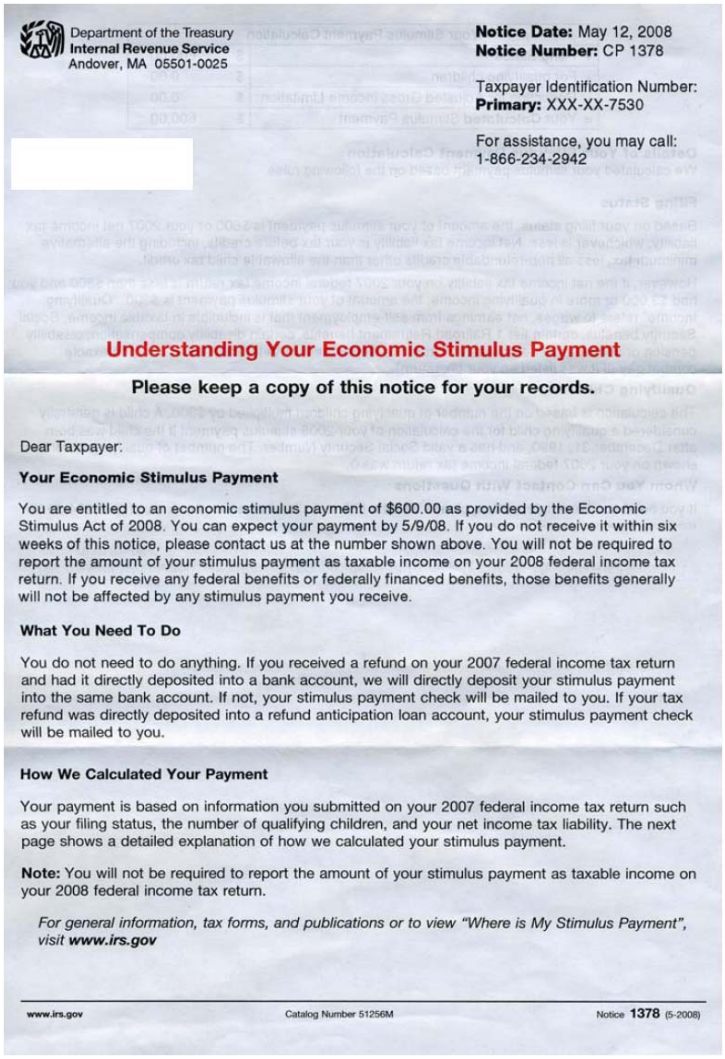
These estimates suggest a significant macroeconomic effect of the 2008 ESPs. To give a sense of the effect, we calculate alternative paths for aggregate consumption that subtract the direct spending caused by the ESPs, as implied by our point estimates and the monthly pattern of distribution of the ESPs. In Figure 3, the (blue) solid line shows the actual National Income and Product Accounts estimates of total aggregate PCE from the third quarter of 2007 to the second quarter of 2009. The dashed lines show this series less estimates of the direct spending effect of the ESP program from different specifications used in the paper. In all cases the implied effects of the ESOS are economically significant. Quantitatively, our preferred point estimates for total expenditures from Tables 4 and 5 imply that the ESPs increased PCE by about 1.3 to 2.3 percent in 2008Q2 and 0.6 to 1.0 percent in 2008Q3 (at annual rates). Of course, this accounting exercise does not include the effects of resource constraints and multiplier effects, but instead simply reveals the magnitude of the direct aggregate spending effect relative to total PCE.

Regarding the implementation of new electronic means of delivering tax cuts, the responses do not significantly differ across paper checks and electronic transfers.

Across households, the responses are largest for older and low-income households, groups which have substantial and statistically significant spending responses. According to the point estimates, the responses are largest for high-asset households but this spending response is not statistically significantly different from zero. Also, homeowners are estimated to have higher spending propensities than renters.

Finally, the responses are largest for self-reported spenders, supporting the informativeness of surveys that elicit self-reported usage, whose results receive much attention. However, self-reported savers (including those reporting they reduced debt) also spent a statistically and economically significant fraction of their payments, suggesting that relaying on self-reports can understate the actual amount of spending

Appendix A: A notification letter for an ESP by electric funds transfer



Department of the Treasury
Internal Revenue Service
Andover, MA 05501-0025

Notice Date: May 12, 2008
Notice Number: CP 1378

Taxpayer Identification Number:
Primary: XXX-XX-7530

For assistance, you may call:
1-866-234-2942

Understanding Your Economic Stimulus Payment

Please keep a copy of this notice for your records.

Dear Taxpayer:

Your Economic Stimulus Payment

You are entitled to an economic stimulus payment of $600.00 as provided by the Economic Stimulus Act of 2008. You can expect your payment by 5/9/08. If you do not receive it within six weeks of this notice, please contact us at the number shown above. You will not be required to report the amount of your stimulus payment as taxable income on your 2008 federal income tax return. If you receive any federal benefits or federally financed benefits, those benefits generally will not be affected by any stimulus payment you receive.

What You Need To Do

You do not need to do anything. If you received a refund on your 2007 federal income tax return and had it directly deposited into a bank account, we will directly deposit your stimulus payment into the same bank account. If not, your stimulus payment check will be mailed to you. If your tax refund was directly deposited into a refund anticipation loan account, your stimulus payment check will be mailed to you.

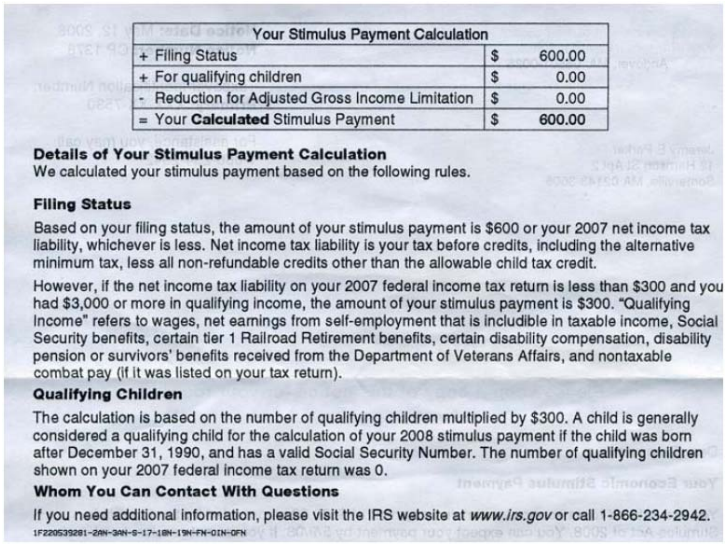
How We Calculated Your Payment

Your payment is based on information you submitted on your 2007 federal income tax return such as your filing status, the number of qualifying children, and your net income tax liability. The next page shows a detailed explanation of how we calculated your stimulus payment.

Note: You will not be required to report the amount of your stimulus payment as taxable income on your 2008 federal income tax return.

For general information, tax forms, and publications or to view "Where is My Stimulus Payment", visit ***www.irs.gov***

www.irs.gov Catalog Number 51256M Notice **1378** (5-2008)



Your Stimulus Payment Calculation		
+ Filing Status	$	600.00
+ For qualifying children	$	0.00
- Reduction for Adjusted Gross Income Limitation	$	0.00
= Your **Calculated** Stimulus Payment	$	**600.00**

Details of Your Stimulus Payment Calculation
We calculated your stimulus payment based on the following rules.

Filing Status

Based on your filing status, the amount of your stimulus payment is $600 or your 2007 net income tax liability, whichever is less. Net income tax liability is your tax before credits, including the alternative minimum tax, less all non-refundable credits other than the allowable child tax credit.

However, if the net income tax liability on your 2007 federal income tax return is less than $300 and you had $3,000 or more in qualifying income, the amount of your stimulus payment is $300. "Qualifying Income" refers to wages, net earnings from self-employment that is includible in taxable income, Social Security benefits, certain tier 1 Railroad Retirement benefits, certain disability compensation, disability pension or survivors' benefits received from the Department of Veterans Affairs, and nontaxable combat pay (if it was listed on your tax return).

Qualifying Children

The calculation is based on the number of qualifying children multiplied by $300. A child is generally considered a qualifying child for the calculation of your 2008 stimulus payment if the child was born after December 31, 1990, and has a valid Social Security Number. The number of qualifying children shown on your 2007 federal income tax return was 0.

Whom You Can Contact With Questions

If you need additional information, please visit the IRS website at *www.irs.gov* or call 1-866-234-2942.

1F220539281-2AN-3AN-S-17-1BN-19N-FN-OIN-QFN

Appendix B: 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 C: 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 tax rebate variable. The 2008 data require fewer consistency checks and adjustments, however. This is because by 2008 the CE survey used 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), strictly nondurable expenditures 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 expenditures (broadly defined) adds spending on apparel goods and services, health care (excluding payments by employers or insurers), and reading materials, following Lusardi (1996) but excluding education. Total expenditure adds spending on education, housing (including furniture and appliances and shelter but excluding utilities and household operations, which are already included in nondurable expenditures), 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 1: The timing of the economic stimulus payments

Panel A: Payments by electronic funds transfer		Panel B: Payments by paper check	
Last two digits of taxpayer SSN	Date ESP funds transferred to account by	Last two digits of taxpayer SSN	Date ESP check in the mail by
00 – 20	May 2	00 – 09	May 16
21 – 75	May 9	10 – 18	May 23
76 – 99	May 16	19 – 25	May 30
		26 – 38	June 6
		39 – 51	June 13
		52 – 63	June 20
		64 – 75	June 27
		76 – 87	July 4
		88 – 99	July 11

Source: Internal Revenue Service (http://www.irs.gov/newsroom/article/0,,id=180247,00.html)

Table 2: Sample Statistics

Sample:	Full sample		On-time recipients		Households with only on-time and only by mail ESPs		Households with only on-time and only EFT ESPs	
Variable	Mean	(std dev)	Mean	(std dev)	Mean	(std dev)	Mean	(std dev)
Expenditures on:								
Food	1,902	(1,356)	1,910	(1,305)	1,792	(1,336)	2,045	(1,176)
Strictly nondurables	4,298	(2,657)	4,361	(2,440)	4,077	(2,418)	4,690	(2,304)
Nondurables	5,342	(3,296)	5,461	(2,973)	5,090	(2,943)	5,901	(2,827)
Total	10,492	(8,124)	10,591	(7,228)	9,694	(6,999)	11,713	(7,162)
Change in Expenditures on:								
Food	7.7	(1,130)	12.7	(1,115)	16.8	(1,164)	6.2	(1,018)
Strictly nondurables	59.0	(1,918)	59.1	(1,820)	48.4	(1,807)	73.3	(1,796)
Nondurables	46.7	(2,279)	49.3	(2,180)	38.2	(2,174)	62.1	(2,148)
Total	-87.5	(7,361)	-80.9	(7,165)	-36.3	(6,675)	-153.6	(7,628)
Level of:								
Number of Adults	1.9	(0.8)	1.9	(0.8)	1.9	(0.8)	1.9	(0.7)
Number of Children	0.7	(1.1)	0.8	(1.1)	0.7	(1.1)	0.9	(1.1)
Change in:								
Number of Adults	0.0	(0.24)	0.0	(0.23)	0.0	(0.24)	0.0	(0.23)
Number of Children	0.0	(0.19)	0.0	(0.19)	0.0	(0.20)	0.0	(0.18)
Age	48.4	(14.9)	48.5	(14.8)	50.6	(15.4)	45.4	(13.2)
ESP	215	(472)	307	(538)	284	(496)	332	(581)
I(ESP>0)	0.23	(0.42)	0.32	(0.47)	0.32	(0.47)	0.31	(0.46)
ESP | *ESP* >0 (N=690)	960	(520)	971	(518)	899	(490)	1082	(527)
Income	74,770	(148,814)	66,387	(108,738)	58,153	(103,035)	77,310	(112,638)
Liquid Assets	9,553	(20,193)	9,959	(20,145)	9,244	(19,454)	11,165	(21,466)

Notes: The first two samples correspond to those used in Table 4 and Table 5 Panel C. The final two samples together with households that receive payments both by electronic transfer of funds and by check, comprise the sample used in Table 6 Panel C. The samples used to calculate income and liquid assets data include only households with valid information on these variables and so are subsamples of the samples used in these tables. For the income and assets variables, the on-time recipients sample corresponds to the samples used in the final two triplets of columns in Table 8.

Table 3: The distribution of reported economic stimulus payments

Sample:	Full sample		Households with only on-time and only ESPs by mail		Households with only on-time and only ESPs by EFT	
			Panel A: by amount of ESP			
ESP value	Number	Percent of ESPs	Number	Percent of ESPs	Number	Percent of ESPs
0<ESP<300	47	1.5	26	1.6	10	1.0
ESP=300	343	11.2	220	13.1	69	6.8
300<ESP<600	77	2.5	40	2.4	16	1.6
ESP=600	943	30.9	558	33.3	278	27.3
600<ESP<900	52	1.7	31	1.8	13	1.3
ESP=900	168	5.5	99	5.9	55	5.4
900<ESP<1200	42	1.4	27	1.6	11	1.1
ESP=1200	800	26.2	440	26.3	287	28.2
1200<ESP<1500	27	0.9	15	0.9	9	0.9
ESP=1500	213	7.0	88	5.3	104	10.2
1500<ESP<1800	25	0.8	11	0.7	12	1.2
ESP=1800	195	6.4	74	4.4	99	9.7
1800<ESP<2100	7	0.2	1	0.1	4	0.4
ESP=2100	63	2.1	25	1.5	31	3.0
2100<ESP<2400	4	0.1	0	0.0	4	0.4
ESP=2400	23	0.8	9	0.5	9	0.9
2400<ESP<2700	1	0.0	1	0.1	0	0.0
ESP=2700	7	0.2	4	0.2	2	0.2
2700<ESP<3000	2	0.1	1	0.1	1	0.1
ESP=3000	10	0.3	4	0.2	4	0.4
ESP>3000	3	0.1	2	0.1	0	0.0
			Panel B: by expenditure period			
Expenditure Period	Mean *ESP*| *ESP* >0	Num (%) of obs with ESP>0	Mean *ESP*| *ESP* >0	Num (%) of obs with ESP>0	Mean *ESP*| *ESP* >0	Num (%) of obs with ESP>0
Mar - May, 2008	1,021	467 (33)	858	136 (33)	1,091	308 (88)
Apr - June, 2008	1,009	780 (57)	932	397 (76)	1,091	341 (100)
May - July, 2008	973	924 (68)	909	572 (98)	1,071	298 (100)
June- Aug, 2008	891	539 (39)	867	398 (78)	1,043	71 (20)
July - Sept, 2008	875	223 (16)	917	151 (28)	-	0 (0)
Aug - Oct, 2008	811	62 (5)	735	22 (4)	-	0 (0)
Sept - Nov, 2008	703	22 (2)	-	0 (0)	-	0 (0)
Oct - Dec, 2008	703	20 (1)	-	0 (0)	-	0 (0)
Nov - Jan, 2009	890	11 (1)	-	0 (0)	-	0 (0)
Dec - Feb, 2009	435	4 (0)	-	0 (0)	-	0 (0)

Notes: The first sample corresponds to that used in Table 4. The second and third samples together with households that receive payments both by electronic funds transfer and by check, comprise the sample used in Table 6 Panel C.

Table 4: The contemporaneous response of expenditures to the economic stimulus payments estimated using all households

Dependent Variable:	Panel A Dollar change in				Panel B Dollar change in				Panel C Percent change in				Panel D Dollar change in			
	Food	Strictly Non-durables	Non-durable spending	Total spending	Food	Strictly Non-durables	Non-durable spending	Total spending	Food	Strictly Non-durables	Non-durable spending	Total spending	Food	Strictly Non-durables	Non-durable spending	Total spending
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	0.523
	(0.027)	(0.046)	(0.055)	(0.179)									(0.033)	(0.060)	(0.071)	(0.219)
I(ESP)					10.9	74.8	121.5	494.5	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	0.72	-0.23	0.96	6.56	0.70	-0.35	0.77	5.77	0.048	0.009	0.029	0.045	0.70	-0.20	1.00	6.60
	(0.34)	(0.65)	(0.81)	(2.25)	(0.34)	(0.65)	(0.81)	(2.24)	-0.010	-0.010	-0.010	-0.010	(0.30)	(0.60)	(0.80)	(2.30)
Change in # adults	198	448	561	452	198	448	561	452	8.96	8.43	8.99	4.78	198	448	561	453
	(55)	(106)	(118)	(375)	(55)	(106)	(118)	(375)	(1.77)	(1.34)	(1.32)	(1.63)	(55)	(106)	(118)	(375)
Change in # children	89	139	185	-254	89	139	186	-252	4.50	3.35	3.93	1.42	89	139	185	-254
	(48)	(96)	(111)	(388)	(48)	(96)	(111)	(388)	(2.02)	(1.53)	(1.50)	(2.10)	(48)	(96)	(111)	(388)
Num of obs	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 also 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 columns are multiplied by 100 so as to report a percent change. The last four columns report results from 2SLS regressions where the indicator variable for ESP receipt is used and othe rregressors are used as instruments for the amount of the ESP.

Table 5: The contemporaneous response of expenditures among households receiving payments

Dependent Variable:	Dollar change in			Percent change in			Dollar change in		
	Strictly Non-durables	Non-durable spending	Total spending	Strictly Non-durables	Non-durable spending	Total spending	Strictly Non-durables	Non-durable spending	Total spending
Estimation method:	OLS	OLS	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS
				Panel A: Sample of all households					
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)			
$I(\Sigma_{household} ESP_t > 0)$	12.01	9.58	21.21	-0.01	-0.01	-0.01	12.66	8.23	20.77
	(30.74)	(36.07)	(104.00)	(0.01)	(0.01)	(0.01)	(33.03)	(38.79)	(112.18)
Number of obs	17,478	17,478	17,478	17,475	17,478	17,478	17,478	17,478	17,478
				Panel B: Sample of households receiving ESPs					
ESP	0.144	0.185	0.683				0.207	0.252	0.866
	(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)			
Number of obs	11,239	11,239	11,239	11,238	11,239	11,239	11,239	11,239	11,239
				Panel C: Sample of households receiving only on-time ESPs					
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)			
Number of obs	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 coumns are multiplied by 100 so as to report a percent change. The final triplet of columns report results from 2SLS regressions where the indicator variable for ESP receipt is used and other regressors are used as instruments for the amount of the ESP. The variable $I(\Sigma_{household} ESP_h > 0)$ is an indicator for households that received an economic stimulus payment in some reference quarter, whereas $I(ESP > 0)$ indicates receipt in the contemporaneous quarter ($t+1$) in particular.

Table 6: The response of expenditures to stimulus payments by means of receipt

Dependent Variable:	Dollar change in			Percent change in			Dollar change in		
	Strictly Non-durables	Non-durable spending	Total spending	Strictly Non-durables	Non-durable spending	Total spending	Strictly Non-durables	Non-durable spending	Total spending
Estimation method:	OLS	OLS	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS
				Panel A: Sample of all households					
ESP by Check	0.104	0.141	0.473				0.047	0.097	0.366
	(0.064)	(0.077)	(0.215)				(0.087)	(0.105)	(0.307)
ESP by EFT	0.086	0.144	0.583				0.105	0.166	0.669
	(0.066)	(0.081)	(0.305)				(0.082)	(0.097)	(0.331)
I(ESP by check)				1.92	2.19	3.59			
				(1.31)	(1.29)	(1.61)			
I(ESP by EFT)				2.81	3.36	4.00			
				(1.44)	(1.41)	(1.83)			
N	17,281	17,281	17,281	17,278	17,281	17,281	17,281	17,281	17,281
				Panel B: Sample of households only receiving ESPs on time					
ESP by Check	0.220	0.245	0.746				0.241	0.290	0.907
	(0.072)	(0.086)	(0.235)				(0.111)	(0.134)	(0.383)
ESP by EFT	0.188	0.218	0.361				0.274	0.305	0.720
	(0.071)	(0.090)	(0.317)				(0.095)	(0.117)	(0.401)
I(ESP by check)				4.14	3.99	5.78			
				(1.67)	(1.63)	(2.03)			
I(ESP by EFT)				5.19	4.84	4.30			
				(1.83)	(1.81)	(2.38)			
Num of Obs	10,362	10,362	10,362	10,361	10,362	10,362	10,362	10,362	10,362
				Panel C: Households only receiving ESPs on time, allowing different effect of all non-ESP regressors by means of payment					
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)			
Num of Obs	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, a full set of month dummies, and indicators for a) ever receiving an ESP by check, b) for ever receiving an ESP by electronic transfer of funds, and c) for being in both category (a) and (b). In panels B and C, there are also separate sets of all other control variables for households in categories a, b, and c. The sample of households is those reporting receiving only on-time ESPs. 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),* its interactions, and the other regressors are used as instruments for ESP and its interactions.

Table 7: The longer-run response of expenditures to stimulus payments

Dependent Variable:	Panel A Dollar change in			Panel B Percent change in			Panel C Dollar change in		
	Strictly Non-durables	Non-durable spending	Total spending	Strictly Non-durables	Non-durable spending	Total spending	Strictly Non-durables	Non-durable spending	Total spending
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)	(0.080)	(0.214)	(1.51)	(1.50)	(1.92)	(0.092)	(0.113)	(0.330)
Implied spending effect in second three-month period	0.177	0.147	0.229	NA	NA	NA	0.143	0.157	0.479
	(0.087)	(0.104)	(0.303)				(0.142)	(0.178)	(0.568)
Implied cumulative fraction of rebate spent over both three-month periods	0.363	0.348	0.746	NA	NA	NA	0.362	0.411	1.236
	(0.128)	(0.155)	(0.477)				(0.218)	(0.273)	(0.892)
Number of Observations	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. The sample includes only households that receive only on-time rebates. 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 $I(ESP)$, along with the other regressors, are used as instruments for ESP.

Table 8: The propensity to spend across different households

Dependent variable:	Panel A: Strictly Non-dur. spending	Panel A: Dollar change in Non-durable spending	Panel A: Total spending	Panel B: Strictly Non-dur. spending	Panel B: Dollar change in Non-durable spending	Panel B: Total spending	Panel C: Strictly Non-dur. spending	Panel C: Dollar change in Non-durable spending	Panel C: Total spending
	Interaction: Age Low: ≤ 40 High: age >58			Interaction: Income Low: ≤ 32,000 High: > 74,677			Interaction: Liquid Assets Low: ≤ 500 High: > 7,000		
ESP	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* (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* (group difference)	0.100 (0.121)	0.044 (0.151)	0.414 (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)
Number of obs	10,488	10,488	10,488	8,592	8,592	8,592	5,071	5,071	5,071
	Implied total spending								
Low group	0.166 (0.092)	0.195 (0.114)	0.491 (0.394)	0.253 (0.137)	0.239 (0.180)	1.283 (0.564)	0.116 (0.173)	0.022 (0.205)	0.007 (0.566)
High group	0.369 (0.136)	0.389 (0.168)	1.366 (0.498)	0.183 (0.105)	0.206 (0.133)	0.773 (0.464)	0.246 (0.162)	0.200 (0.202)	0.934 (0.678)

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 only non-late stimulus payments. All results are from 2SLS regressions where *I (ESP>0)* and 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 rebate recipients in each grouping.

Table 9: The propensity to spend by homeownership status

Dependent variable:	Strictly Non-dur. spending	Dollar change in Non-durable spending	Total spending
	Baseline group: renters (23% of sample) First interaction: owners with mortgages (50%) Second interaction: owners without mortgages (27%)		
ESP	0.197 (0.128)	0.213 (0.153)	0.431 (0.455)
*ESP*I(Owned with mortgage)* (group difference)	0.030 (0.110)	0.043 (0.131)	0.543 (0.394)
*ESP*I(Owned without mortgage)* (group difference)	0.175 (0.133)	0.260 (0.169)	0.800 (0.514)
N	10,380	10,380	10,380
	Implied total spending		
Homeowners with mortgages	0.227 (0.093)	0.256 (0.112)	0.974 (0.364)
Homeowners without mortgages	0.372 (0.135)	0.473 (0.175)	1.231 (0.508)

Notes: All regressions also include separate intercepts for owners with mortgages and owners without, 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 only non-late stimulus payments and excludes households that occupy without payment of cash rent or that live in student housing. All results are from 2SLS regressions where $I(ESP>0)$ and 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 10: The propensity to spend by self-reported usage

Dependent variable:	Strictly Non-dur. spending	Dollar change in Non-durable spending	Total spending
	Baseline group: mostly save (18% of sample) First interaction: mostly spend (32%) Second interaction: mostly pay debt (50%)		
ESP	0.230	0.173	0.952
	(0.131)	(0.162)	(0.465)
*ESP*I(Report mostly spend)*	0.158	0.349	0.755
(group difference)	(0.136)	(0.169)	(0.496)
*ESP*I(Report mostly pay debt)*	-0.005	0.098	-0.319
(group difference)	(0.126)	(0.156)	(0.453)
N	10,072	10,072	10,072
	Implied total spending		
Households that self-report mostly to increase spending	0.388	0.522	1.707
	(0.115)	(0.142)	(0.457)
Households that self-report mostly to pay off debts	0.225	0.271	0.633
	(0.106)	(0.131)	(0.393)

Notes: All regressions also include separate intercepts for each self-reported usage category, 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 self-reporting usage and only receiving only non-late stimulus payments. All results are from 2SLS regressions where *I (ESP>0)* and 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 11: The propensity to spend on different categories of non-durable goods and services

Dependent variable:	Panel A: Food Dollar change in			Panel B: Additional strictly nondurables Dollar change in				Panel C: Additional nondurables Dollar change in		
	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 (0.032)	0.025 (0.033)	0.011 (0.007)	0.059 (0.027)	0.083 (0.049)	0.027 (0.039)	0.007 (0.009)	0.022 (0.021)	0.025 (0.048)	-0.001 (0.003)
Implied share of nondurable spending	0.16	0.08	0.04	0.19	0.27	0.09	0.02	0.07	0.08	0.00
Average share of nondurable Goods	0.25	0.10	0.01	0.24	0.04	0.15	0.02	0.05	0.14	0.01

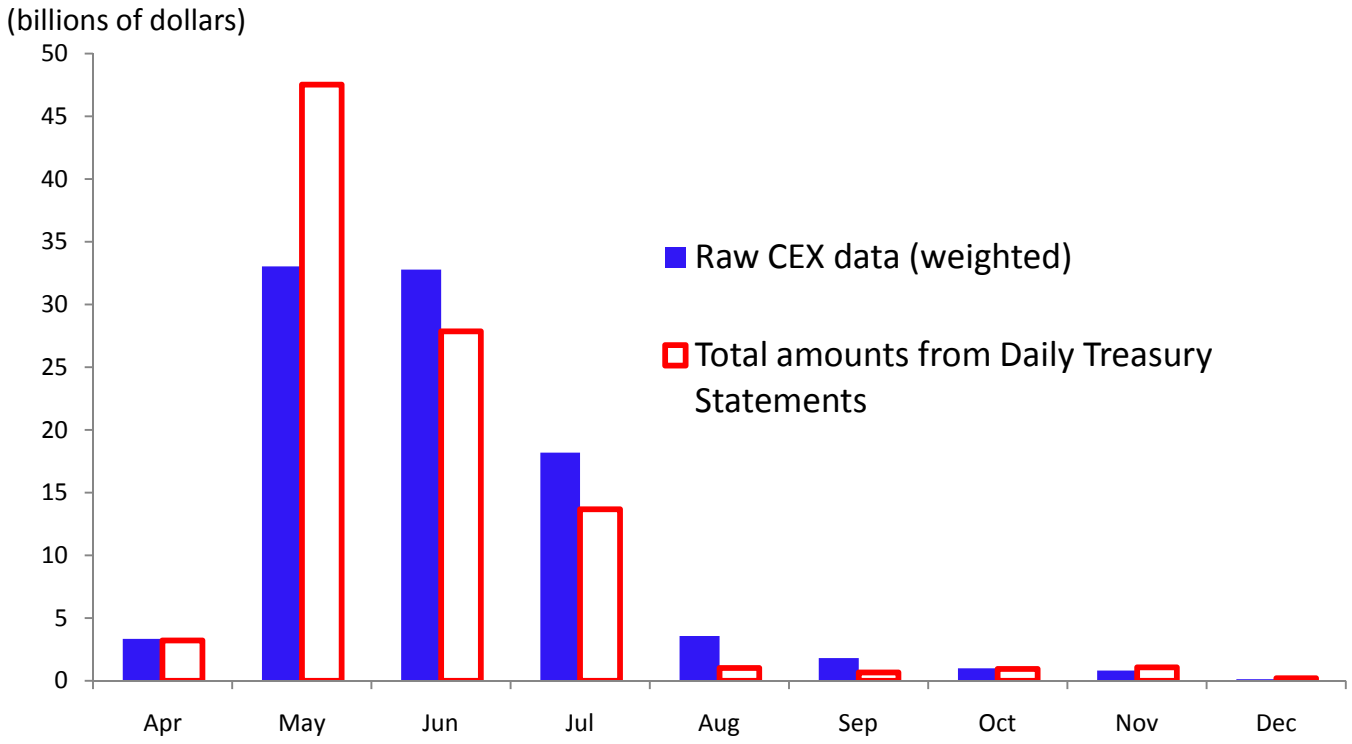
Notes: N=10,488 for all regressions. All regressions also include the change in the number of adults, the change in the number of children, the age of the household, and a full set of month dummies. Sample includes only households receiving only non-late payments. Reported standard errors are adjusted 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 12: The propensity to spend on different categories of durable spending

Dependent variable:	Panel A: Subcategories of durable spending Dollar change in				Panel B: Subcategories of transportation Dollar change in				
	Housing (shelter & furnishings)	Entertainment (TVs, Stereos, recreational equip., fees)	Education	Transportation (car purchases, maintenance, insurance)	New vehicle purchases	Used vehicle purchases	Other vehicle purchases	Maintenance and repairs	Insurance, finance chrgs, rental, leases, licenses
ESP	0.099 (0.092)	0.077 (0.099)	-0.100 (0.042)	0.527 (0.269)	0.357 (0.204)	0.123 (0.149)	0.011 (0.054)	0.009 (0.028)	0.027 (0.024)
Implied share of durable spending	0.16	0.13	-0.17	0.87	0.59	0.20	0.02	0.01	0.04
Average spending as a share of average on all durable goods	0.56	0.13	0.04	0.27	0.07	0.06	0.01	0.04	0.09
Implied share of total spending effect	0.11	0.08	-0.11	0.58	0.39	0.14	0.01	0.01	0.03

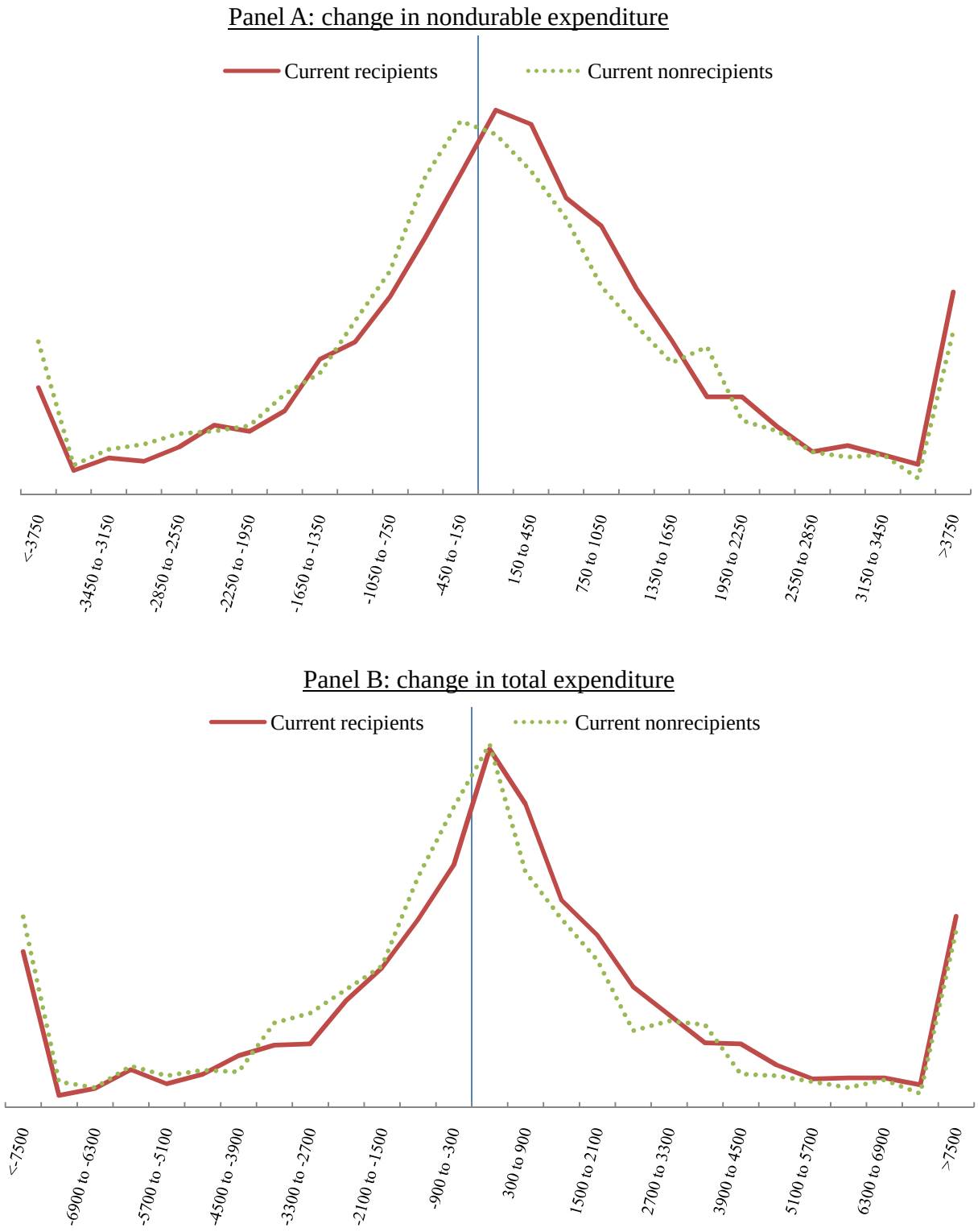
Notes: N=10,488 for all regressions. All regressions also include the change in the number of adults, the change in the number of children, the age of the household, and a full set of month dummies. Sample includes only households receiving only non-late payments. Reported standard errors are adjusted 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* .

Figure 1: Economic stimulus payments during 2008



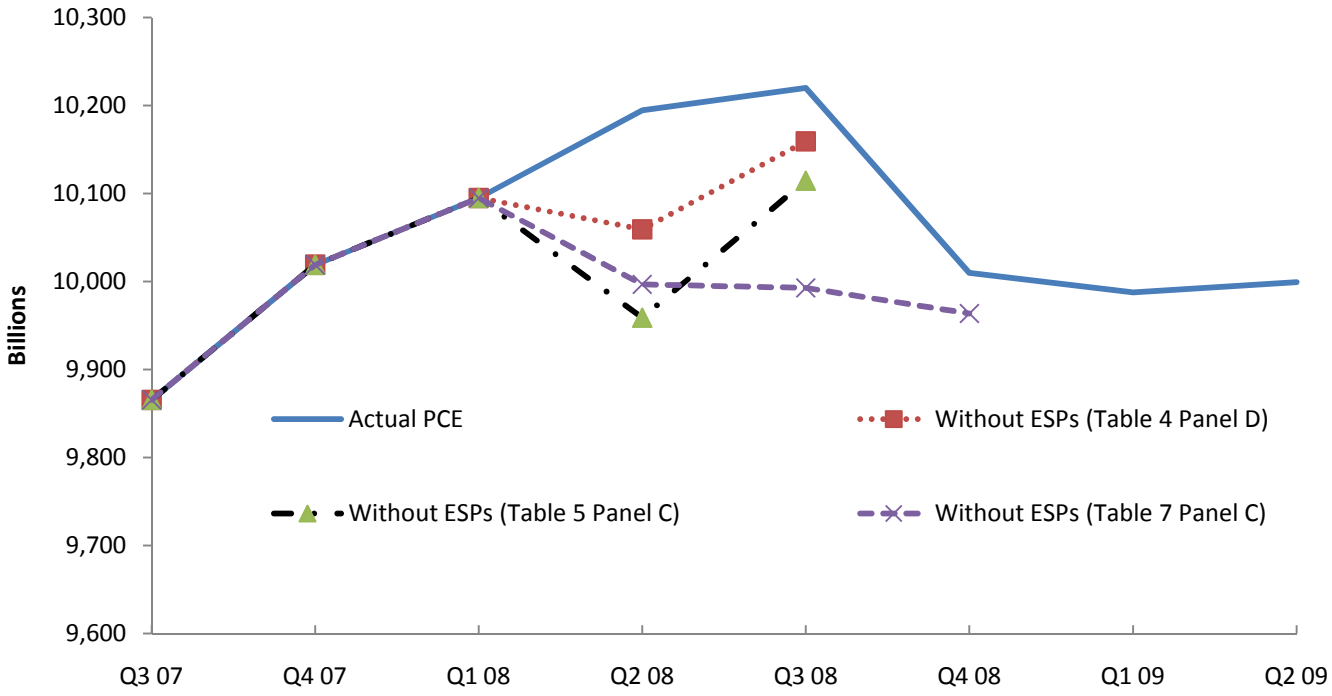
Notes: Source: Daily Treasury Statements and authors' calculations from the CEX.

Figure 2: Histograms of change in expenditure for recipients during the program



Notes: Plots are histograms of change in expenditures in household-periods only for periods in which some-on-time ESPs are received and only for households that only receive on-time ESPs. The total number of households is normalized to be equal across samples so that the histogram is a discrete estimate of the density function.

Figure 3: Actual aggregate personal consumption expenditures and alternatives



Notes: Alternative scenarios subtract only the direct effect of the stimulus payments on spending. The aggregate effect is calculated by applying the estimated average share of stimulus payments spent to the actual monthly time series of payments. We assume that the measured contemporaneous share spent is spent evenly over the month of receipt and the subsequent month, and that any lagged spending occurs evenly over the following three months.