

# Consumer Spending and the Economic Stimulus Payments of 2008\*

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**Abstract:** Using special questions added to the Consumer Expenditure Survey, we measure the response of household spending to the economic stimulus payments (ESPs) disbursed in mid-2008. We find that, on average, households spent about 12-31% of their stimulus payments on non-durable goods 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 autos. Improving on previous research, these spending responses are estimated with precision using only variation in the timing of ESP receipt. We also find some evidence of an ongoing though smaller response in the subsequent three-month period, though this response cannot be estimated with precision. Further, we find little evidence that the propensity to spend varies with the means of delivery (paper check versus electronic transfer). The estimated responses are substantial and significant for older, lower-income, and home-owning households. Finally, 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 actual spending to the ESPs is indeed largest for self-reported spenders, though self-reported savers also spent a significant fraction of the payments.

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In the winter of 2007-08, facing the fallout from an increasingly severe financial crisis and already contemplating the limitations of traditional monetary policy, Congress and the Administration turned to fiscal policy to help stabilize the U.S. economy. The Economic Stimulus Act (ESA) of 2008, enacted in February 2008, consisted primarily of a 100 billion dollar program that sent economic stimulus payments (ESPs) to 130 million U.S. tax filers.

The desirability of this historically-important use of fiscal policy depends critically on the extent to which these tax cuts and consequent offsetting fiscal outcomes directly changed household spending, as well on any subsequent multiplier or price effects. In this paper, we focus on the direct spending effect, the existence of which is a necessary (though not sufficient) condition for the efficacy of this counter-cyclical policy.

We measure the spending responses of households to the receipt of a stimulus payment by comparing the temporal pattern of spending across households that receive ESPs at different points in time. Further, to 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 payments.

We measure the spending effect of the 2008 economic stimulus payments 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 transfers 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, digits that are effectively randomly assigned.<sup>1</sup> We exploit this random variation to cleanly estimate the causal effect of the payments on household spending, by comparing the

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

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 electronic funds transfer). 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 worked to add 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). This question asks households who previously reported receiving payments to directly report whether they mostly spent or mostly saved their payments.

Summarizing our main findings, on average households spent about 12-31% of their stimulus payments, depending on the specification, on (CE-defined) non-durable consumption goods and services 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 new 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 electronic transfer of funds 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 suggests that, relative to the effects of previous tax rebates such as that 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. Some prior research suggests the possibility that larger payments could lead to a different composition of spending. While JPS finds no significant response of durable goods in 2001, Souleles (1999) finds a significant increase in both nondurable and durable goods (in particular auto purchases) in response to spring-time Federal income tax refunds, which are substantially larger than the 2001 tax rebates.<sup>2</sup>

Our estimates suggest a significant macroeconomic effect of this policy. To give a sense of the impact of the policy, we calculate alternative paths for aggregate consumption that subtract the spending implied by our point estimates and the pattern of distribution of the ESPs. The (blue) solid line in Figure 1 shows the actual National Income and Product Accounts estimates of total aggregate personal consumption expenditures (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 later in the paper. 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.

In terms of results that 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 statistically significantly different from zero. Further, motivated by the collapse of the housing market in

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<sup>2</sup> 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).

2008, we find that homeowners on average spent more of their ESPs when they arrived than 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.

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, we measure with precision the response of spending using only random variation in the timing of ESP receipt. Second, we consider whether the delivery method (check versus electronic) affects the amount of spending. This is an important consideration since the 2008 tax cut was the first large tax cut to use electronic transfers, and electronic transfers seem likely to be used increasingly frequently in the future. Third, we evaluate the accuracy of the self-reported responses to the payments, by comparing the subjective survey responses to our causal estimates using the data on actual spending and ESP receipt. Such evaluation is useful given the benefits of the subjective surveys: they can be put into the field and analyzed quickly after policy changes, used to evaluate hypothetical policies, and used to investigate the relevance of different theoretical reasons for household reported behavior.

This paper is structured as follows. Sections I and II briefly describe the literature and relevant aspects of ESA 2008. Section III describes the data and Section IV sets forth our empirical methodology. Section V presents the main results regarding the short-run response to the economic stimulus payments, while Section VI examines the longer-run response. Section VII examines the differences in response across different households, Section VIII focuses on what categories of expenditure responded most to ESP receipt, and a final section concludes. The Appendix contains additional information about 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 of predictable changes in household income, and of predictable changes caused by tax policy in particular. Deaton (1992),

Browning and Lusardi (1996), Johnson, Parker, and Souleles (2006), and Jappelli and Pistaferri (2010) review these literatures well.<sup>3</sup>

Our paper is most closely related to JPS (2006), which uses a similar module of questions appended to the CE survey to study the 2001 income tax rebates. JPS finds a larger spending response for nondurable spending, estimating that households spent about 20-40 percent of their rebates during the three-month period in which they received their rebates, but no significant spending on durable goods. Unlike the current study, JPS is unable to identify this effect with precision using only random variation in timing of rebate receipt. JPS finds larger 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.<sup>4</sup>

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

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<sup>3</sup> For a survey of recent fiscal policy, see e.g., Auerbach and Gale (2009).

<sup>4</sup> Coronado, Lupton, and Sheiner (2006) also study the 2003 child payments, using the Michigan Survey.

Third, Shapiro and Slemrod (2009) uses 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.<sup>5</sup>

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 report that they use their ESPs mostly to pay down debt, 18% report 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”.<sup>6</sup> For qualifying households, the basic payment was generally 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).

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<sup>5</sup> In 2008, of the 80% of respondents who report they will mostly save their ESP, the majority (about 60%) report that they will mostly pay down debt (as opposed to accumulate assets). See also Sahn, Shapiro and Slemrod (2009). The Michigan Survey includes additional 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.

<sup>6</sup> 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.

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 somewhat 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 tax refunds), the stimulus payments were disbursed electronically over a three-week period ranging from late April to mid May.<sup>7</sup> At the time of each disbursement, the IRS mailed a statement to the household informing them about the electronic transfer of funds.<sup>8</sup> 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.<sup>9</sup> A letter similar to that for EFTs also was sent with the check. Appendix A provides an example of this letter for an ESP distributed by EFT. Importantly, in either case, the particular timing of the payment was determined by the last two digits of the recipients' Social Security numbers, which are effectively randomly assigned.

In aggregate the stimulus payments in 2008 were historically large, amounting to about \$100 billion which is more than double the size of the 2001 rebate program. According to the 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

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<sup>7</sup> 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 are common, representing about a third of the tax refunds delivered via direct deposit in 2007.

<sup>8</sup> For mailed checks, the notices go out about a week before the check is mailed. For electronic transfers, the notices go out a couple of business days before the direct deposit is supposed to be credited. The banks also get notified a couple of days before the date of funds transfer, and some banks will show the amount on the beneficiary's bank account a day or more before the actual credit 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.

<sup>9</sup> 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 15<sup>th</sup> deadline (Slemrod et al., 1997), this source of variation is small. Nonetheless, we present results below that exclude such late rebates. Finally, due to human and computer error, about 350,000 households (less than 1%) 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 based on just the child credit, starting in early July.

in that quarter. During the third quarter, \$15 billion in ESPs were disbursed, corresponding to about 0.4% of GDP or 0.6% of personal consumption expenditures. The stimulus payments constituted about two-thirds of the total ESA package, which also included various business incentives and foreclosure relief.<sup>10</sup> 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 also gathers (less-frequent) information about demographic characteristics, income, and wealth. New households are added to the survey every month so that the data are effectively monthly in frequency. We use the 2007 and 2008 waves of the CE data (which include interviews in the first quarter of 2009).

Two extra sets of questions about the 2008 ESPs were included in the CE Survey in interviews conducted between June 2008 and March 2009, which covers the crucial time during which the payments were disbursed.<sup>11</sup> The first set 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, the date it was received, and whether it was received by check or direct deposit. These questions were asked in all five CE interviews (including the first).<sup>12</sup>

The second additional question was asked only once and only of households that had previously reported a payment. These households were asked whether they think the payment led them “mostly to increase spending, mostly to increase savings, or mostly to pay off debt.” The

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<sup>10</sup> For more details on ESA, see e.g., CCH (2008) and Shapiro and Slemrod (2009).

<sup>11</sup> 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.

<sup>12</sup> In interview 1, the reference period is only one month.

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 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 and includes semi-durable categories like apparel, health and reading. Finally, total expenditures also includes durable expenditures such as home furnishings, entertainment, and auto purchases.<sup>13</sup> 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 to August 2007 to November 2007) and runs through interviews in March 2009 (when period  $t+1$  covers December 2008 to February 2009). Also, we drop from the sample any household observation ( $t$  or  $t+1$ ) with implausibly low expenditures (the bottom 1% of nondurable expenditures in levels), unusually large changes in age or family size, and uncertain stimulus payment status.<sup>14</sup>

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<sup>13</sup> 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 total number of payments (about 30% more) in the sample in 2008, and the larger size (over double) of these payments.

<sup>14</sup> 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 CE ESP computer file rbt08, and it is this second-release version that we use.

Figure 2 shows our calculations of the aggregate amount of ESPs reported in the raw CE by month and the amount of ESP disbursement reported in the Daily Treasury Statements (DTS) (Department of the Treasury (2008)). During 2008, the ESPs reported in the CE 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 has fewer ESPs reported during the peak month of May and more in the following months, suggesting either that some households took time to notice receipt or that there is tendency to report a later date of receipt than actually occurred.

Table 2 presents summary statistics for our final full sample and subsamples that we also employ. The average value of *ESP*, conditional on a positive value, is about \$1000. Households that receive ESPs by electronic funds transfer have slightly higher expenditures, are slightly younger, and have somewhat higher incomes and liquid assets than households that receive payment by mail.

Table 3 shows more information about the distribution of reported ESPs in our dataset. Panel A shows that, as was the case for the actual ESPs, most reported ESPs are in multiples of \$300, with about 55% reporting 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:

$$C_{i,t+1} - C_{i,t} = \sum_s \beta_{0s} * month_{s,i} + \beta_1 X_{i,t} + \beta_2 ESP_{i,t+1} + u_{i,t+1}, \quad (1)$$

where  $i$  indexes households and  $t$  indexes time,  $C$  is either consumption expenditures or their log;  $month$  is a complete set of indicator variables for every period in the sample, used to absorb the seasonal variation in consumption expenditures as well as 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 stimulus payments received by household  $i$  in period

$t+1$  ( $ESP_{i,t+1}$ ); ii) a dummy variable indicating whether any payment was received in  $t+1$  ( $I(ESP_{i,t+1} > 0)$ ); and iii) a distributed lag of  $ESP$  or  $I(ESP > 0)$ , to measure the longer-run effects of the payments. We correct the standard errors to allow for arbitrary heteroskedasticity and within-household serial correlation. As an extension, to analyze heterogeneity in the response to the payments, we interact  $ESP_{i,t+1}$  with indicators for different types of households. The key coefficient  $\beta_2$  measures the average response of household expenditure to the arrival of a stimulus payment.<sup>15</sup>

Traditional tests of the LCPIH use estimates of predictable change in income to *test* the null hypothesis that  $\beta_2$  is zero. Instead, we use the randomized timing of ESP receipt to provide orthogonality between the residual and the timing of ESP receipt. This allows us to avoid any potential omitted variables bias, and so to *estimate*  $\beta_2$  and measure the causal effect of the payments on expenditure. This estimate still provides a direct test of the LCPIH.<sup>16</sup> The rational-expectations LCPIH implies that  $\beta_2 = 0$ . Even if instead households were actually surprised by the payment,  $\beta_2$  should still be small under the LCPIH, because the one-time payment represents a transitory increase in income (or zero under Ricardian equivalence).

## V. The Short-Run Response of Expenditure

This section presents 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 using all available variation to identify the causal effect of interest, and subsequently narrow the variation by dropping non-recipients or late recipients from our sample and using only variation in the timing of ESP receipt within each means of payment. The following section estimates the lagged response to the payments.

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<sup>15</sup> 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.

<sup>16</sup> 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  $\beta_2$ . 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  $\beta_2$ ) should not be correlated with the timing of ESP receipt, so  $\beta_2$  should still be estimated consistently.

### 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  $\beta_2$  measure the average fraction of the payment spent on the different expenditure aggregates in each column, within the three-month reference-period in which the payment was received. We find that, during the three-month period in which a payment was received, relative to the previous three-month period, a household on average increased its expenditures on food by about 2 percent of the payment, its strictly nondurable expenditures by 8 percent of the payment, and its nondurable expenditures by 12 percent of the payment. The third result is statistically significant, and larger than implied by the LCPIH. In the fourth column, total consumption expenditures increased on average by 52 percent of the payment, a substantial and statistically significant amount. This result is relatively precisely estimated, especially considering that the difference with the preceding results 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.

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  $\beta_2$  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 \$122, which is statistically significant at the 7% level. Total expenditures increased by a significant \$494. 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 use the change in log expenditures as the dependent variable. On average in the three-month period in which a payment was received, relative to the previous three-month period, nondurable expenditure increased by 2.1%, and total expenditures increased by 3.2%. These are again statistically and economically significant effects. 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,  $\beta_2$  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. For instance, to check that outliers do not drive the finding that ESPs caused a significant increase in expenditures, we plot histograms of the distribution of changes in expenditure for observations during which an ESP is received and for observations during which one is not. Figure 3 shows that, 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). While Figure 3 does not control for any covariates, it verifies our main findings non-parametrically in the raw data.<sup>17</sup>

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<sup>17</sup> Further, using median regressions or winsorizing the dependent variable lead to very similar results for food, strictly nondurable goods, and nondurable goods. For total expenditures, the resulting coefficients are generally smaller than in Table 4, though still statistically and economically significant (e.g., 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

### 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 goods, nondurable goods, and total expenditures.

First, Panel A of Table 5 adds to equation (1) an indicator for households that received a payment in *any* reference quarter,  $I(\sum_{\text{household}} ESP > 0)$ , 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 3, the estimated coefficients on  $I(\sum_{\text{household}} ESP > 0)$  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.

Our second approach is more stringent. Panel B of Table 5 excludes from the sample all households that did not report a payment in some reference quarter. 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

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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.

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 seems to be some lags in reporting (or in noticing) the payments that leads to a later pattern of receipt in the CE. 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 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 make this sample our main sample for the balance of the paper.

Given that this sample represents our main sample for the remainder of the paper, Figure 4 shows that the non-parametric findings in Figure 3 are similar in this sample. Figure 4 uses the sample of on-time recipients and further limits the data to the time period during which the stimulus program was sending out ESPs, i.e., it restricts the sample to changes for which the  $t+1$  interview occurred between June 2008 and October 2008.

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. This significance is in contrast to the results in JPS where analogously limiting the sample to non-late rebate recipients dramatically reduced the precision of the results.

### *C. Means of receipt*

The most novel feature of the 2008 ESP program was the use of electronic funds transfers (EFT) 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 affected the spending impact of a tax rebate. Second, since the means of ESP payment is not randomly assigned and affects the time of receipt, technically there

are two natural experiments conducted within two distinct samples. Thus, we proceed to ask 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 use the entire sample of households, including non-recipients, because there is limited temporal variation within ESPs received by EFT.<sup>18</sup> 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 just timing variation leads to a significant reduction in power for estimating the effect of EFT receipt. And the smaller number of ESPs used to identify the effects of a mailed ESP also raise that coefficient's 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 provides a reasonable amount of 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 paper checks still yields a significant response of spending to the mailed checks.

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<sup>18</sup> A few observations have missing values for the method-of-delivery question, and so are dropped from the sample.

However, these results still impose common month dummies and demographic effects (age and changes in family size) across EFT and paper-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.<sup>19</sup> 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 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).<sup>20</sup>

In sum, our findings remain broadly consistent across specifications that use different forms of variation, including purely random 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 focus on the sample of households that 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 uniformly slightly smaller than the corresponding results in Panel C of Table 5. More

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<sup>19</sup> About 2% 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.

<sup>20</sup> An alternative is to treat the effect of ESPs received by EFT as a weakly identified nuisance parameter and include both ESP by check and ESP by EFT in the regressions in Panel C. In this case, the coefficients 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.

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 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). The *cumulative* change in nondurable 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 nondurable goods and total expenditures).<sup>21</sup>

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 analyzes heterogeneity in the response to the stimulus payment, across different types of households and different subcategories of consumption goods. This analysis provides 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

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<sup>21</sup> The findings are similar using the sample of all households.

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 expected increase in income. To investigate this explanation, we test whether households that were relatively likely to be constrained were more likely to increase their spending upon arrival of a payment. Constrained households may be unable or unwilling to increase their spending prior to the payment arrival. On the other hand, unconstrained households (e.g., high wealth or high income households) may find the costs of not smoothing consumption across the arrival of the payment to be small (Caballero, 1995; Parker, 1999; Sims, 2003; and Reis, 2006).

Expanding equation (1), we interact the intercept and  $ESP_{t+1}$  variables with indicator variables (*Low* and *High*) based on various household characteristics (all from households' second CE interview to minimize any endogeneity). We use three different variables to identify households that are potentially liquidity constrained: age, income (family income before taxes), and liquid assets (the sum of balances in checking and saving accounts). While liquid assets is arguably the most directly relevant of these variables for identifying liquidity constraints, it is the least well measured and the most often missing in the CE data, so we start with the other two variables.<sup>22</sup> 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).<sup>23</sup> 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

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<sup>22</sup> 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).

<sup>23</sup> 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.

households spent relatively less of the payment and old households spent relatively more. However, these differences, while economically large, are not statistically 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 than the typical (baseline middle-income) household on total expenditures. The bottom panel reports the implied total spending for the interacted groups, in absolute terms. (The coefficient in the first row of the table represents the spending for the baseline group.) 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.<sup>24</sup> However, while suggestive of possible role for liquidity constraints, the difference between this result and that for the baseline group, although economically large at about 70 percent 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. If constrained households expect their constraints to bind for a longer period of time, this would reduce the magnitude of their response to a 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

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<sup>24</sup> As discussed below, it is not inconsistent for the average spending response to be larger in magnitude than the average payment, even putting aside the confidence intervals for the former, if enough households buy large durables like autos in response to receiving a rebate.

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.<sup>25, 26</sup>

### *C. Spending propensities by self-reported spending propensities*

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 used it to pay down debt.<sup>27</sup> 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 subjective 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. In absolute terms their spending is statistically and economically significant (relative to both other groups together). They spent about 35 percentage points more on nondurable goods than the baseline group, the self-reported savers, and this difference is statistically significant. The corresponding difference for total expenditures is even larger in magnitude, at 75 percentage points, but not statistically significant.

On the other hand, we estimate that 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

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<sup>25</sup> 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) for renters and these propensities are statistically significantly different at the 10 percent level.

<sup>26</sup> The results for homeowners do not simply reflect the preceding results for older households. E.g., if one drops from the sample the households older than 65, the coefficients for nondurable expenditure remain very similar to those reported in the table, for all three groups of households. The coefficients for total expenditure remain very similar for renters and homeowners with mortgages. While the coefficient for total expenditure 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.

<sup>27</sup> 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.

about 63 percentage points, albeit insignificant, and the response of nondurable goods is statistically significant and still rather large at 27% of the payment. In this sense self-reported spending may provide a lower bound on the actual amount of spending (consistent with Agarwal, Liu, and Souleles, 2007)).

### **VIII. Differences in Responses across Types of Expenditure**

Turning to differences across goods, 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, at the bottom of the table, the share of the overall increase in nondurable expenditures and total expenditures that is accounted for by each of the subcategories. For benchmarking, one can compare the share of the increase in nondurable expenditures to the average share of each subcategory in nondurable expenditures (top row). Of course, comparisons of different subsets of nondurable expenditure must be interpreted cautiously because of potential non-separabilities across goods.

Further, note that in general the results are statistically weak, with only the coefficient for utilities and household operations being statistically significant. This response is roughly in proportion to the share of this subcategory in nondurable goods. For 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 goods there is much more variability in the dependent variable that is unrelated to the payment regressor. Our previous results, by summing the subcategories into broader aggregates of nondurable goods, averaged out much of this unrelated variability (such as, for example, whether a trip to the supermarket happened to fall just inside or outside the expenditure reference-period).

Panel A of Table 12 provides the analogous decomposition of the response of the durable goods and services part of total expenditures. While there are sizable responses on average in housing (which includes shelter and furniture/appliances) and entertainment (which includes TVs and other electronic equipment), these responses are statistically insignificant and small relative to their share in durable goods. The bulk of the response in durables comes in transportation, spending on which increases by 53% of the payments on average, a statistically and economically significant amount. Panel B shows that this result is driven by purchases of

vehicles, primarily new vehicles. In particular, 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.<sup>28</sup>

## **IX. Conclusion**

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 on randomized timing variation.

The estimated spending response on nondurables is generally smaller in magnitude (though not significantly different) than the response in nondurable goods from the 2001 tax rebate, which could reflect the more transitory nature of the 2008 tax cut. Nonetheless, the response in 2008 is larger than implied by the LCPIH or Ricardian equivalence. Moreover, the composition of spending is different than in 2001, with 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. 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


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<sup>28</sup> 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.

not statistically significantly different from zero. Homeowners are estimated to have higher spending propensities than renters.

Finally, the responses are largest for self-reported spenders, supporting the informativeness of self-reported usage. But self-reported savers (including those reporting they reduced debt) also spent a statistically and economically significant fraction of their payments.

## 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](http://www.irs.gov)*

[www.irs.gov](http://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
<b>= Your Calculated Stimulus Payment</b>	<b>\$ 600.00</b>

**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](http://www.irs.gov) or call 1-866-234-2942.

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

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

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

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

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

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**Table 1: The timing of the economic stimulus payments**

<u>Panel A: Payments by electronic funds transfer</u>		<u>Panel B: Payments by paper check</u>	
<u>Last two digits of taxpayer SSN</u>	<u>Date ESP funds transferred to account by</u>	<u>Last two digits of taxpayer SSN</u>	<u>Date ESP check in the mail by</u>
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**

Variable	Full sample		On-time recipients		Households with only on-time and only by mail ESPs		Households with only on-time and only EFT ESPs	
	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)
<i>ESP</i>	215	(472)	307	(538)	284	(496)	332	(581)
<i>I(ESP&gt;0)</i>	0.23	(0.42)	0.32	(0.47)	0.32	(0.47)	0.31	(0.46)
<i>ESP   ESP&gt;0 (N=690)</i>	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**

<b>Sample:</b>	<u>Full sample</u>		<u>Households with only on-time and only ESPs by mail</u>		<u>Households with only on-time and only ESPs by EFT</u>	
	Number	Percent of ESPs	Number	Percent of ESPs	Number	Percent of ESPs
<b>Panel A: by amount of ESP</b>						
<u>ESP value</u>						
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
<b>Panel B: by expenditure period</b>						
	Mean	Number (pct)	Mean	Number (pct)	Mean	Number (pct)
<u>Expenditure Period</u>	<u>ESP/ESP&gt;0</u>	<u>of ESPs</u>	<u>ESP/ESP&gt;0</u>	<u>of ESPs</u>	<u>ESP/ESP&gt;0</u>	<u>of ESPs</u>
Mar - May, 2008	1,021	467 (15)	858	136 (8)	1,091	308 (30)
Apr - June, 2008	1,009	780 (26)	932	397 (24)	1,091	341 (33)
May - July, 2008	973	924 (30)	909	572 (34)	1,071	298 (29)
June- Aug, 2008	891	539 (18)	867	398 (24)	1,043	71 (7)
July - Sept, 2008	875	223 (7)	917	151 (9)	-	0 (0)
Aug - Oct, 2008	811	62 (2)	735	22 (1)	-	0 (0)
Sept - Nov, 2008	703	22 (1)	-	0 (0)	-	0 (0)
Oct - Dec, 2008	703	20 (1)	-	0 (0)	-	0 (0)
Nov - Jan, 2009	890	11 (0)	-	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 transfer of funds 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				Panel B				Panel C				Panel D			
	Dollar change in				Dollar change in				Percent change in				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
<i>ESP</i>	0.016 (0.027)	0.079 (0.046)	0.121 (0.055)	0.516 (0.179)									0.012 (0.033)	0.079 (0.060)	0.128 (0.071)	0.523 (0.219)
<i>I(ESP)</i>					10.9 (31.7)	74.8 (56.6)	121.5 (67.2)	494.5 (207.2)	0.69 (1.27)	1.74 (0.96)	2.09 (0.94)	3.24 (1.17)				
<i>Age</i>	0.72 (0.34)	-0.23 (0.65)	0.96 (0.81)	6.56 (2.25)	0.70 (0.34)	-0.35 (0.65)	0.77 (0.81)	5.77 (2.24)	0.048 -0.010	0.009 -0.010	0.029 -0.010	0.045 -0.010	0.70 (0.30)	-0.20 (0.60)	1.00 (0.80)	6.60 (2.30)
<i>Change in # adults</i>	198 (55)	448 (106)	561 (118)	452 (375)	198 (55)	448 (106)	561 (118)	452 (375)	8.96 (1.77)	8.43 (1.34)	8.99 (1.32)	4.78 (1.63)	198 (55)	448 (106)	561 (118)	453 (375)
<i>Change in # children</i>	89 (48)	139 (96)	185 (111)	-254 (388)	89 (48)	139 (96)	186 (111)	-252 (388)	4.50 (2.02)	3.35 (1.53)	3.93 (1.50)	1.42 (2.10)	89 (48)	139 (96)	185 (111)	-254 (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 other regressors 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
<u>Panel A: Sample of all households</u>									
<i>ESP</i>	0.073 (0.050)	0.117 (0.060)	0.507 (0.196)				0.071 (0.068)	0.123 (0.081)	0.509 (0.253)
<i>I(ESP)</i>				2.20 (1.09)	2.63 (1.07)	3.97 (1.34)			
<i>I(Σ<sub>household</sub>ESP<sub>t</sub>&gt;0)</i>	12.01 (30.74)	9.58 (36.07)	21.21 (104.00)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	12.66 (33.03)	8.23 (38.79)	20.77 (112.18)
Number of obs	17,478	17,478	17,478	17,475	17,478	17,478	17,478	17,478	17,478
<u>Panel B: Sample of households receiving ESPs</u>									
<i>ESP</i>	0.144 (0.054)	0.185 (0.066)	0.683 (0.219)				0.207 (0.087)	0.252 (0.103)	0.866 (0.329)
<i>I(ESP)</i>				3.97 (1.36)	3.91 (1.34)	5.63 (1.69)			
Number of obs	11,239	11,239	11,239	11,238	11,239	11,239	11,239	11,239	11,239
<u>Panel C: Sample of households receiving only on-time ESPs</u>									
<i>ESP</i>	0.188 (0.058)	0.214 (0.070)	0.590 (0.217)				0.262 (0.092)	0.308 (0.112)	0.911 (0.342)
<i>I(ESP)</i>				4.61 (1.53)	4.52 (1.50)	6.05 (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 columns 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  $I(\sum_{household} ESP_h > 0)$  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
<b>Panel A: Sample of all households</b>									
<i>ESP by Check</i>	0.104 (0.064)	0.141 (0.077)	0.473 (0.215)				0.047 (0.087)	0.097 (0.105)	0.366 (0.307)
<i>ESP by EFT</i>	0.086 (0.066)	0.144 (0.081)	0.583 (0.305)				0.105 (0.082)	0.166 (0.097)	0.669 (0.331)
<i>I(ESP by check)</i>				1.92 (1.31)	2.19 (1.29)	3.59 (1.61)			
<i>I(ESP by EFT)</i>				2.81 (1.44)	3.36 (1.41)	4.00 (1.83)			
N	17,281	17,281	17,281	17,278	17,281	17,281	17,281	17,281	17,281
<b>Panel B: Sample of households only receiving ESPs on time</b>									
<i>ESP by Check</i>	0.220 (0.072)	0.245 (0.086)	0.746 (0.235)				0.241 (0.111)	0.290 (0.134)	0.907 (0.383)
<i>ESP by EFT</i>	0.188 (0.071)	0.218 (0.090)	0.361 (0.317)				0.274 (0.095)	0.305 (0.117)	0.720 (0.401)
<i>I(ESP by check)</i>				4.14 (1.67)	3.99 (1.63)	5.78 (2.03)			
<i>I(ESP by EFT)</i>				5.19 (1.83)	4.84 (1.81)	4.30 (2.38)			
Num of Obs	10,362	10,362	10,362	10,361	10,362	10,362	10,362	10,362	10,362
<b>Panel C: Households only receiving ESPs on time, allowing different effect of all non-ESP regressors by means of payment</b>									
<i>ESP</i>	0.187 (0.066)	0.211 (0.078)	0.529 (0.232)				0.240 (0.128)	0.262 (0.149)	0.784 (0.401)
<i>I(ESP)</i>				3.96 (1.87)	3.64 (1.79)	5.48 (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 columns are multiplied by 100 so as to report a percent change. The final triplet of columns reports results from 2SLS regressions where  $I(ESP > 0)$ , 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			Panel B			Panel C		
	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
$ESP_{t+1}$ or $I(ESP_{t+1})$	0.186 (0.055)	0.201 (0.067)	0.517 (0.211)	3.58 (1.58)	3.92 (1.55)	4.96 (1.96)	0.219 (0.089)	0.254 (0.110)	0.757 (0.360)
$ESP_t$ or $I(ESP_t)$	-0.009 (0.068)	-0.054 (0.080)	-0.288 (0.214)	-2.09 (1.51)	-1.23 (1.50)	-2.22 (1.92)	-0.076 (0.092)	-0.097 (0.113)	-0.278 (0.330)
Implied spending effect in second three-month period	0.177 (0.087)	0.147 (0.104)	0.229 (0.303)	NA	NA	NA	0.143 (0.142)	0.157 (0.178)	0.479 (0.568)
Implied cumulative fraction of rebate spent over both three-month periods	0.363 (0.128)	0.348 (0.155)	0.746 (0.477)	NA	NA	NA	0.362 (0.218)	0.411 (0.273)	1.236 (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			Panel B			Panel C		
	Strictly Non-dur. spending	Dollar change in Non-durable spending	Total spending	Strictly Non-dur. spending	Dollar change in Non-durable spending	Total spending	Strictly Non-dur. spending	Dollar change in Non-durable spending	Total spending
		<u>Interaction: Age</u> Low: $\leq 40$ High: age $>58$			<u>Interaction: Income</u> Low: $\leq 32,000$ High: $> 74,677$			<u>Interaction: Liquid Assets</u> Low: $\leq 500$ High: $> 7,000$	
<i>ESP</i>	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)
<i>ESP*Low</i> (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)
<i>ESP*High</i> (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 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:	Dollar change in		
	Strictly Non-dur. spending	Non-durable spending	Total spending
Baseline group: renters (23% of sample)			
First interaction: owners with mortgages (50%)			
Second interaction: owners without mortgages (27%)			
<i>ESP</i>	0.197 (0.128)	0.213 (0.153)	0.431 (0.455)
<i>ESP*I(Owned with mortgage)</i> (group difference)	0.030 (0.110)	0.043 (0.131)	0.543 (0.394)
<i>ESP*I(Owned without mortgage)</i> (group difference)	0.175 (0.133)	0.260 (0.169)	0.800 (0.514)
<i>N</i>	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 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:	Dollar change in		
	Strictly Non-dur. spending	Non-durable spending	Total spending
	Baseline group: mostly save (18% of sample) First interaction: mostly spend (32%) Second interaction: mostly pay debt (50%)		
<i>ESP</i>	0.230 (0.131)	0.173 (0.162)	0.952 (0.465)
<i>ESP</i> * <i>I</i> (Report mostly spend) (group difference)	0.158 (0.136)	0.349 (0.169)	0.755 (0.496)
<i>ESP</i> * <i>I</i> (Report mostly pay debt) (group difference)	-0.005 (0.126)	0.098 (0.156)	-0.319 (0.453)
<i>N</i>	10,072	10,072	10,072
	Implied total spending		
Households that self-report mostly to increase spending	0.388 (0.115)	0.522 (0.142)	1.707 (0.457)
Households that self-report mostly to pay off debts	0.225 (0.106)	0.271 (0.131)	0.633 (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 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			Panel B: Additional strictly nondurables				Panel C: Additional nondurables		
	Dollar change in			Dollar change in				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
Average share of nondurable Goods	0.250	0.100	0.014	0.238	0.036	0.153	0.017	0.051	0.137	0.006
<i>ESP</i>	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
Implied share of total spending effect	0.05	0.03	0.01	0.06	0.09	0.03	0.01	0.02	0.03	0.00

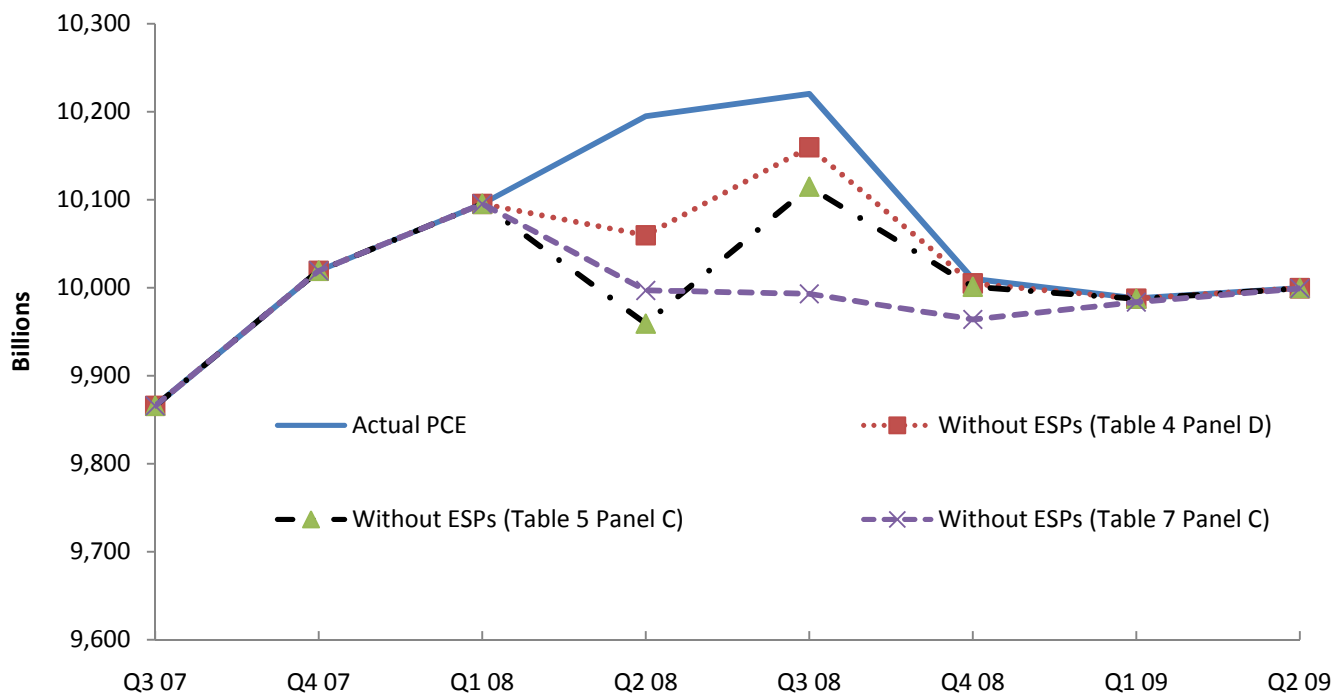
Notes: N=10,488 for all regressions. All regressions also include the change in the number of adults, the change in the number of children, the age of the household, and a full set of month dummies. Sample includes only households receiving only non-late payments. Reported standard errors are adjusted 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				Panel B: Subcategories of transportation				
	Dollar change in				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
Ratio of average spending on category to average spending on all durable goods	0.556	0.134	0.041	0.270	0.070	0.065	0.006	0.038	0.091
<i>ESP</i>	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
Implied share of total spending effect	0.11	0.08	-0.11	0.58	0.39	0.14	0.01	0.01	0.03

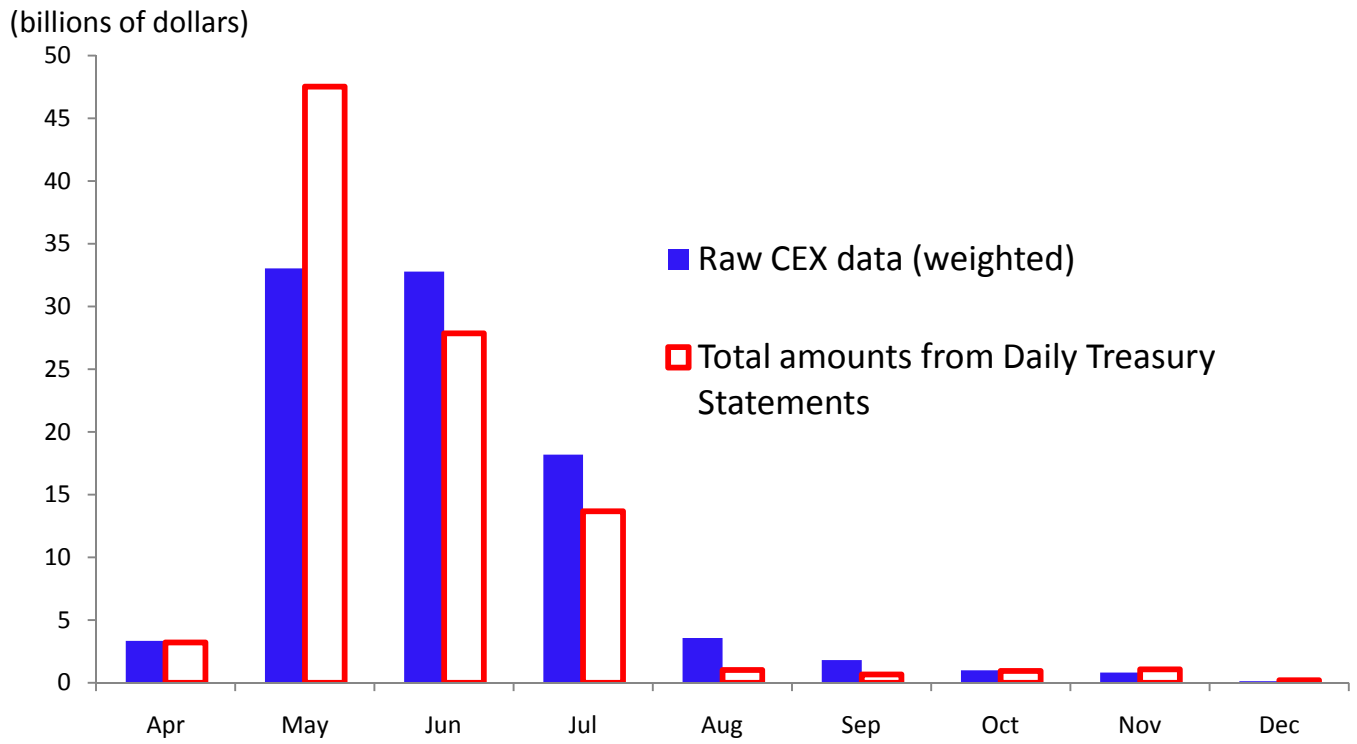
Notes: N=10,488 for all regressions. All regressions also include the change in the number of adults, the change in the number of children, the age of the household, and a full set of month dummies. Sample includes only households receiving only non-late payments. Reported standard errors are adjusted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



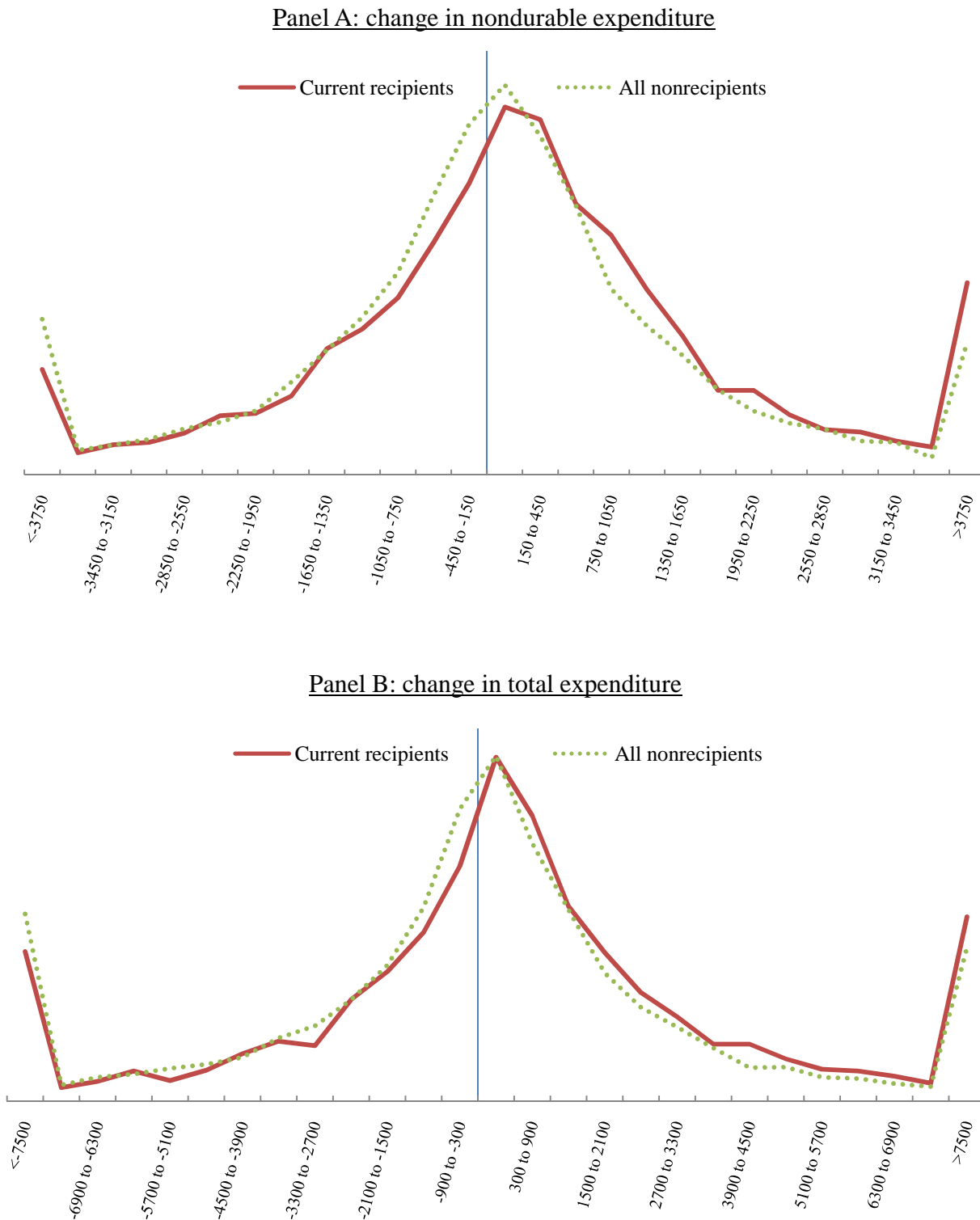
Notes: Alternative scenarios subtract only the direct effect of the stimulus payments on spending. The aggregate effect is calculated by applying the estimated average share of stimulus payments spent to the actual monthly time series of payments. We assume that the measured contemporaneous share spent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

Figure 2: : Economic stimulus payments during 2008



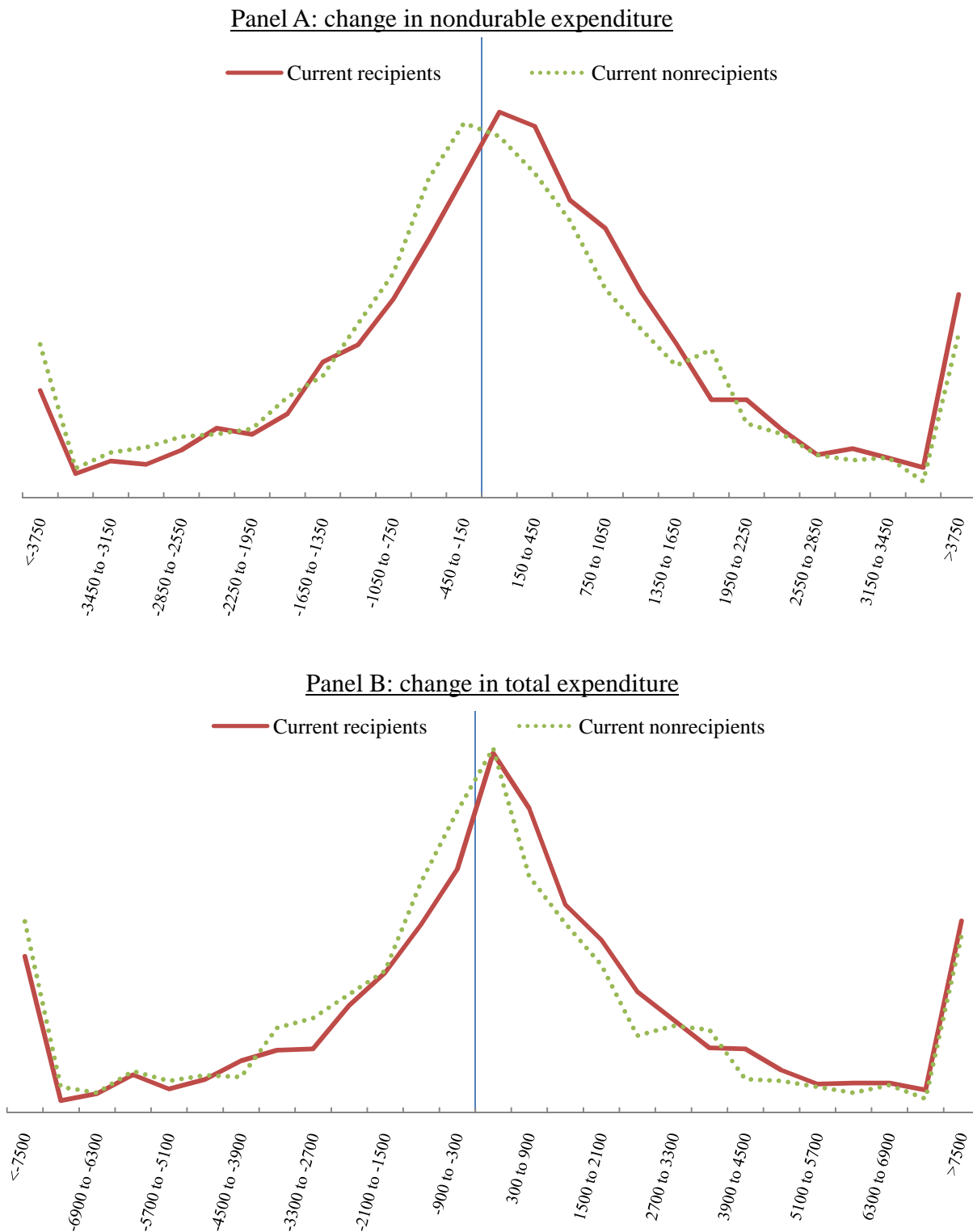
Notes: Source: Daily Treasury Statements and authors' calculations from the CEX.

Figure 3: Histograms of change in expenditure for all households



Notes: Plots are histograms of change in expenditures for the sample in Table 4 split into two subsamples: observations during which an ESP is received, and observations during which one is not. 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 4: 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.