

Why Don't Households Smooth Consumption? Evidence from a 25 million dollar experiment*

Jonathan A. Parker
MIT and NBER

November 2014

Abstract

Households who regularly report spending in the Nielsen Consumer Panel in 2008 were surveyed about financial and behavioral characteristics as well as their receipt of an Economic Stimulus Payment. Using roughly 25 million dollars in reported payments and the random timing of payments, this paper relates household characteristics to the propensity of households to spend pre-announced, lump-sum income on arrival. The average household spends a highly statistically significant amount on arrival and most of the spending is done by households with low liquidity. Models of beliefs do not explain this result: most households expect the payment, and increases in spending are similar between those expecting and not expecting payments. The propensity to increase spending is not transitory, due to current or recent income shocks or to liquidity management with illiquid assets, suggesting that it is instead due to preferences. Increases in spending are as closely associated with low income in 2006 and with being the type of person that spends and lives for today as they are with low levels of liquidity. Levels of sophistication or planning are also associated with consumption smoothing: spending responds for households that make more use of coupons or deals, or that have made financial plans. Finally, there is scant evidence for an important role for the available measures related to procrastination or self-control.

*For helpful comments, I thank Christian Broda, Chris Carroll, Nicholas Souleles, Steven Zeldes, two anonymous referees on the survey grant application, and participants at seminars at the Board of Governors of the Federal Reserve System, the Consumer Financial Protection Bureau, the Federal Reserve Bank of Boston, Harvard, MIT and the NBER Household Finance meetings July 2014. I thank the MIT Sloan School of Management, the Kellogg School of Management at Northwestern University, the Initiative for Global Markets at the University of Chicago, and the Zell Center at the Kellogg School of Management for funding. I thank Ed Grove, Matt Knain and Molly Hagen at Nielsen for their work on the survey and their careful explanation of the Nielsen Consumer Panel. The results of this paper are calculated based on data from The Nielsen Company (U.S.) LLC and provided by the Marketing Data Center and the University of Chicago Booth School of Business. Contact information: MIT Sloan School of Management, 77 Massachusetts Avenue, Building E62-642, Cambridge MA 02139-4307; <http://japarker.scripts.mit.edu/>.

The canonical assumption that the benefits of additional consumption decline with the level of consumption – that marginal utility is diminishing – implies that people should seek stable consumption over time. Absent financial frictions, expected changes in income should have no contemporaneous effect on marginal utility or consumption. However, while many issues complicate testing, this proposition of consumption smoothing has been frequently rejected: on average, predictable changes in people’s incomes cause significant changes in their spending, with the causal effects concentrated among people with low liquid wealth or low income. Both the significant average spending response and the heterogeneity in response with liquid wealth suggest that household behavior is significantly influenced by liquidity or financial constraints. But, but beyond this ingredient, there is significant disagreement about what causes lack of consumption smoothing.

One possibility is that illiquidity and lack of consumption smoothing are the result of poor income shocks or temporary portfolio illiquidity, as in the textbook buffer stock model or life-cycle/permanent income model (LCPIH) with borrowing constraints (e.g. Zeldes (1989a), Deaton (1991), Carroll (1997), Ludvigson (1999)). When some households experience poor income realizations that lead to low liquid wealth relative to future income, an inability or unwillingness to borrow leads to a close relationship between current income and current consumptions. Similar predictions follow from a model in which households have costly access to high-return, relatively illiquid savings vehicles (Kaplan and Violante (2014)). Households with low liquidity will tolerate some deviations in consumption smoothing to avoid the costs associated with accessing relatively illiquid assets. According to these models, lack of consumption smoothing among low wealth households is due to temporary low liquidity.

An alternative interpretation, incorporated into a number of competing rational and behavioral models, is that low liquidity and an inability or unwillingness to smooth consumption are persistent traits of some households that are due to preferences or behavioral characteristics rather than being situational. The most straightforward version of such a theory is that some households are simply highly impatient, hand to mouth households in extreme. Alternatively, other theories motivated by evidence from laboratory experiments and neurological studies characterize lack of consumption smoothing as due to the limits of human reasoning or the complexity of human motivation in economic behaviors. As examples, lack of consumption

smoothing may be due to limited attention, limited sophistication, limited optimization, or problems of self-control. While according to the basic model, some people are unable to smooth consumption due to temporarily low liquid wealth, according to these alternatives, some people choose not to smooth consumption and not to accumulate liquid wealth due to persistent behavioral characteristics. Why don't households smooth consumption?

This paper studies why households violate consumption smoothing using both a natural field experiment provided by disbursement of the Federal economic stimulus payments of 2008 and data from a specially-designed survey of households that are reporting spending in the Nielsen Consumer Panel (NCP, formerly Homescan Consumer Panel). Lack of consumption smoothing is not caused by an inability of some households to smooth consumption due to temporarily low income (budget constraints); instead persistent characteristics (preferences) cause some households to choose not to smooth consumption and not to accumulate liquid wealth. What behavioral characteristics? Spending responses in this experiment are not associated with inattention or with measures of procrastination or lack of self-control. Instead, lack of consumption smoothing is associated with a measure of impatience and with measures of lack of optimization and lack of planning.

In terms of the experiment, among households receiving stimulus payments by check and among those receiving payments by direct deposit, the week in which the payment was disbursed was determined by the last two digits of the recipient's Social Security number, digits which are effectively randomly assigned. Following previous research that shows the arrival of a payment causes an increase in household spending on average, I use this randomization to identify the causal effect of the receipt of a payment on household spending by comparing the spending patterns of households who receive their payments at different times.¹ Because the timing of the payment is randomly set by the government and is unrelated to a household's characteristics or economic situation, this comparison measures the increase in spending caused by receipt. Because the variation in timing is uncorrelated with household characteristics, comparing differences in spending responses across different groups of households measures which households smooth consumption spending across the receipt of their payment.

¹ Following the methodology of Johnson, Parker, and Souleles (2006), Sahm, Shapiro, and Slemrod (2010), Parker, Souleles, Johnson, and McClelland (2013), Broda and Parker (2014) all estimate increases in spending on average.

In terms of the data, the basic NCP data contain daily information on each household's purchases of household items as well as annual demographic information such as family size and income. In conjunction with Nielsen, a multi-wave survey was designed early in 2008 and then fielded while the stimulus payments were being disbursed in 2008. The survey, administered by email and web for households with web access at home and by mail and barcode scanner for households without, collected information on *i*) the arrival and amount of the first stimulus payment received in order to measure the spending response to the payment, and *ii*) the liquid wealth, behaviors, and expectations of households in order to relate these characteristics to the failure to smooth consumption spending. After dropping households that did not receive payments or did not report valid payments, the supplemental survey measures more than 25 thousand NCP households in 2008 as they receive more than 25 million dollars in randomly-timed stimulus payments.

On average, the spending of households that receive their payments early rises relative to the spending of households that receive their payments later. Specifically, in different specifications, the average household raises its spending on NCP-measured household goods in the week of receipt by around 13 dollars, roughly 9 percent of average weekly spending, or about 1.4 percent of the average payment. These estimates are all highly statistically significant. The increase in spending decays slowly over the following weeks, so that over seven weeks, the receipt of a payment causes an increase in spending of roughly 30 dollars on NCP-measured goods, of 2 and a half to 3 percent more spending, or of roughly 2 and a half percent of the payment.

If spending responses were similar across households, then cross-sectional data on household responses would tell us little about behavioral models. In fact, consistent with previous research, households with significant liquid wealth smooth spending well while households with low liquidity have much larger spending responses. Thus, for an observable factor to be the causes of spending responses, it must exhibit variation in the sample and be correlated with liquidity in order to explain a substantial share of spending responses.

First, if shocks to income cause low liquid wealth and failure to smooth spending, then income growth ought to predict behavior. But households with low income growth are no more likely to spend the payment on arrival than those with high income growth. It is the case that

low income in 2008 predicts high rates of spending from payments. But income in 2006 is as good as income in 2008, and as liquidity, at separating households who spend from those who do not. Thus, while low liquidity is a strong predictor of large spending responses, the evidence suggests that this is not due to current or recent poor income shocks but rather to persistent characteristics of low income households.

Could it be that many households, particularly low liquidity households, did not expect the payments? Few households were surprised to get payments and there is little evidence of a larger spending effect of arrival for those who were not expecting them. In one of two specifications, there is a statistically higher spending response among households surprised by the payments – positively but more importantly negatively also – relative to those expecting their payments. Although this finding is not indicative of an important role for inattention in spending, it is consistent with a link between consumption smoothing and economic sophistication or planning, as revealed by understanding of the stimulus payment program.

Is it the case that households differ in sophistication and unsophisticated households have low incomes, accumulate little wealth, and smooth consumption poorly? First, consistent with Reis (2006), the data provide evidence consistent with lack of planning causing violations of consumption smoothing, and some weaker evidence that it is not only financial planning that matters but that planning is a more general trait that matters across domains. Households that have not made financial plans or do not plan for vacations do not smooth spending across arrival of the payment. Households that have made financial plans smooth consumption well. Only twenty two percent of households plan a great deal for vacations, and while these households smooth spending well the week of arrival, they do less well at a horizon of a month. Finally, households that use more coupons or deals when making purchases smooth spending much better than those that do not. Interestingly, this is particularly true among households with low liquidity.

Finally, I turn to a measure of whether a household is saver or a spender, which might be a measure of impatience, and to measures of self-control and procrastination. Consumption smoothing is highly correlated with whether a household reports being *the sort of people* who would rather spend their money and enjoy today than save more for the future. Households that report being savers smooth consumption; households that report being spenders do not. Not

surprisingly, being a saver is also highly correlated with the level of liquid wealth, so that the type of person is an important predictor of both low wealth and lack of consumption smoothing. And the type of people who are spenders are worse at consumption smoothing even among households with low liquidity. In terms of self-control, there is an economically large but statistically-weak higher propensity to spend on arrival among the small share of the population that frequently regrets past purchases. But this does not explain much of the average spending response. The other 95 percent of the population still exhibits substantial violations of consumption smoothing. To measure procrastination, I sort households by their delay in responding to the supplemental survey. This delay is unrelated to the size of spending response to the arrival of the payment.

Three caveats are in order. First, we are measuring spending rather than consumption, and focusing on one month period that is well measured. Second, the spending studied is only a share of total spending, and it is possible that total responses differ due to different propensities to spend on non-measured goods and services. Third, these results may or may not generalize to other domains of consumption smoothing. For example, less publicized payments may be more unexpected upon arrival and so lead to different, presumably larger, spending responses with consequently possibly different patterns across households. Similarly, much larger or much smaller payments, may lead to quite different responses.

As discussed in Section 6, this evidence has several implications for the modelling of consumption and saving behavior. First, inattention, rational or otherwise, is not an important determinant of month-to-month spending responses to predictable payments. Second, the weight of the evidence is consistent with the majority of lack of consumption smoothing being driven by a persistent household characteristic rather than transitory economics circumstances, such as shocks to labor income or the management of liquid and illiquid assets. Third, these findings are generally consistent with a model with financial frictions in which some households have high levels of impatience. Such a model does not naturally match the evidence on sophistication or planning but potentially could if coupled with frictions to optimization that lead impatient households that violate consumption smoothing to choose not pay costs in the present to plan for future. But, finally, this evidence is also not inconsistent with a more behavioral view in which some households are persistently poor optimizers and so do little planning; a subset of these

frequently regret of purchases or exhibit little understanding of payment programs; and these unsophisticated households and spend money when it arrives leading to low wealth, low income, and poor smoothing of consumption.

This paper builds on two significant literatures.

First, of the research that has developed the methodology and evidence relating to the consumption-smoothing implications of the rational-expectations LCPIH, the most relevant is the set of papers that use household-level data and quasi-experiments to measure the changes in consumption caused by predictable changes in household income, and by predictable changes in tax policy in particular.² The present paper builds most directly on Johnson, Parker, and Souleles (2006) and Agarwal, Lui, and Souleles (2007) which both find significant spending responses to the receipt of the 2001 Federal tax rebates that are concentrated among low income and low asset households.³ Salm, Shapiro and Slemrod (2010), Parker, Souleles, Johnson and McClelland (2011), and Broda and Parker (2012) differ on the role of low assets or income in the spending response to the 2008 stimulus payments.⁴

Second, this paper contributes to the literature in household finance that evaluates the importance of household abilities, beliefs, or motives by relating indicators of these features to household wealth, consumption, or saving behaviors. Agarwal, Driscoll, Gabaix, and Laibson (2009) shows a link between ability, by way of age, and financial mistakes, and Grinblatt, Keloharju, and Linnainmaa (2010) which shows a positive relationship between IQ and stock market participation. Finally, Bernheim, Skinner, and Weinberg (2001), Hurst (2003), Ameriks, Caplin, and Leahy (2003), and Lusardi and Mitchell (2007) present evidence that differences in wealth across households are not well captured by behavior in the standard model even with financial frictions but are instead consistent with some features of models of behavior incorporating rules of thumb, mental accounts, problems with self-control, or an important role for planning.

² See the general discussions in Deaton (1992), Browning and Lusardi (1996), and Jappelli and Pistaferri (2010). Work on spending responses to other predictable changes in income induced by fiscal policy includes Shapiro and Slemrod (1995), Parker (1999), Souleles (1999, 2002), Barrow and McGranahan (2000), Hsieh (2003), Coronado, Lupton, and Sheiner (2006), Johnson, Parker, and Souleles (2009) and the higher-frequency work of Stephens (2003, 2006).

³ See also Shapiro and Slemrod (2003a, 2003b).

⁴ See also Bureau of Labor Statistics (2009), Shapiro and Slemrod (2009), Bertrand and Morse (2009) and Gross, Notowidigdo, and Wang (2012).

The theoretical models of behavior that motivate the analyses of the paper are discussed as the evidence is presented.

This paper is structured as follows. The next section describes the data, sample and survey of characteristics used in the paper. Section 2 describes the economic stimulus payment program and survey of receipt and expectations. Section 3 presents our estimation methodology and Section 4 contains baseline results on the average effect of a payment on spending and the heterogeneity in response associated with liquidity. Section 5 presents our main results on which households increase spending on arrival of a payment and the implications for consumer theory. Section 6 discusses the interaction of our main findings with liquid wealth and Section 7 discusses the theoretical implications of these results. A final section concludes.

1. The Nielsen Consumer Panel

The subjects for this study are a subset of the households in the 2008 NCP. The NCP is a panel survey of U.S. households in 52 metropolitan areas that contains measures of demographic characteristics and tracks spending mainly on household goods with Universal Product Codes (UPCs, what I will refer to as “barcodes”). Households that choose to participate in the NCP are given barcode scanners to use to input the total amount spent and then to scan the items they purchase at the conclusion of every shopping trip.⁵ Barcodes are concentrated in grocery, drugstore and mass-merchandise sectors, and so the recorded expenditures cover goods such as food and drug products, small appliances and electronic goods, and mass merchandise products excluding apparel. Participants get newsletters and personalized tips and reminders via email and/or mail to upload spending information and to answer occasional surveys. For regularly uploading information, participants are entered in prize drawings and receive Nielsen points that can be accumulated and used to purchase prizes or ‘gifts’ from a catalogue.

Participants are surveyed when they initially join the survey and at the end of each subsequent calendar year about their demographic characteristics, and these answers are used as the demographic information for the following calendar year. Low performing households are

⁵ For individual items, if the purchases were made at a store with Scantrack technology, the price is automatically downloaded from the store’s database. Otherwise the household enters the price paid. The household also enters any deals or coupons used that might affect the price paid into the scanner. For more details on the NCP see See Broda and Weinstein (2008).

dropped, and about 80% of Nielsen households are retained from year to year. When replacing participants, Nielsen seeks to add new households with characteristics that make the panel more representative. Nielsen produces sampling weights that can be used to make the sample representative of the U.S. population along 10 demographic dimensions (including income). These weights are used throughout the analysis.

While the NCP is limited in the scope of spending that it covers, it has numerous benefits for the purpose at hand. First, while I primarily use information on total trip spending rather than the large amount of detail available on products (approximately 700,000 different goods are purchased at some point by household in the sample), the use of scanners in real time and administrative price data increase the accuracy of reported expenditures. The temporal precision allows analysis of weekly spending responses which increases the statistical power of the analysis given that the stimulus payments were randomized across weeks. Second, the NCP is relatively large: there are around 60,000 active households (of the roughly 120,000 households in the panel at any point in 2008) that meet the static reporting requirement used by Nielsen to define participating households for the period January to April 2008. Finally, Nielsen has in place a system to survey the households in the NCP. Nielsen typically uses these supplemental surveys to conduct marketing studies for corporate clients, conducting the surveys, analyzing the results, and delivering complete analyses to clients.

Christian Broda and I worked with Nielsen in March and April of 2008 to write and conduct a survey of the NCP households about both their characteristics and their receipt of economic stimulus payments. The next section describes these payments and the survey. The data employed in this study are a combination of the responses to this survey, data licensed from Nielsen, and data available through the Kilts-Nielsen Data Center at The University of Chicago Booth School of Business.⁶

2. The 2008 Economic Stimulus Payments and the NCP supplemental survey

The random variation in pre-announced lump-sum payments to the NCP households is provided by the Economic Stimulus Act, passed by Congress in January and signed into law on February 13, 2008. In total, the Act called for \$100 billion in economic stimulus payments to be

⁶ Data are available at: <http://research.chicagobooth.edu/nielsen/>.

disbursed to about 130 million eligible taxpayers. Each stimulus payment 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. The basic payment was generally the maximum of \$300 (\$600 for couples filing jointly) and a taxpayer's 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). The stimulus payment amount was reduced by five percent of the amount by which adjusted gross income exceeded a threshold of \$75,000 for individuals and \$150,000 for couples. All income information was based on tax returns for year 2007. Thus the amount was zero for low-income households which had neither positive net income tax liability nor sufficient qualifying income, and also zero for sufficiently high-income households.⁷

More importantly, because it was not administratively possible for the IRS to mail all stimulus checks or letters accompanying direct deposits at once, within each method of disbursement, the week in which the payment was disbursed was determined by the last two digits of the recipient's Social Security, digits which are effectively randomly assigned.⁸ For recipients that 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 three one-week periods ranging from late April to the middle of May.⁹ The IRS mailed a statement to the household informing them about the deposit to arrive a few business days before the electronic transfer of funds.¹⁰ The Appendix contains an example of this letter. For recipients that did not provide a personal bank routing number, the payments were mailed (using paper checks) in one

⁷ 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.

⁸ 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).

⁹ The payment was mailed for any tax return for which the IRS had the tax preparer's routing number, as for example would occur as part of taking out a 'refund anticipation loan' or paying a tax preparation fee from a refund. These situations are common, representing about a third of the tax refunds (not rebates) delivered via direct deposit in 2007.

¹⁰ Banks also get notified a couple of days before the date of funds transfer, and some banks showed the amount on the beneficiary's bank account a day or more before the actual credit date. For example, some electronic transfers deposited on Monday April 28 were known to the banks on Thursday April 24, and some banks seem to have credited accounts on Friday April 25.

of nine one-week periods ranging from the middle of May to the middle of July.¹¹ The IRS sent a notification letter one week before the check was mailed. Table 1 shows the schedule of payment disbursements.

To measure the payments received by NCP households, a supplemental survey was administered to the households in the NCP that consists of two parts, each to be answered by “the adult most knowledgeable about your household’s income tax returns.” Part I contains characteristics questions pertaining to the household’s liquid assets and behaviors related to planning, spending, and self-control. Part II first describes the program of economic stimulus payments and then asks “Has your household received a tax rebate (stimulus payment) this year?” Households that respond yes, are then asked about the amount and date of arrival of their stimulus payment, whether it was received by check or direct deposit, the extent to which the amount was expected, whether the household mostly saved or spent the stimulus payment, and the amount of spending across categories of goods.

The survey was fielded in multiple waves, with each wave following the standard procedures that Nielsen uses to survey the consumer panel households. For households with internet access and who were in communication with Nielsen by email the survey was administered in three waves in a web-based form, and for households without access and in contact with Nielsen by regular mail the survey was administered in only two waves in a paper/barcode scanner form, since the distribution time was slower and the preparation time greater. Repeated surveying was conditional on earlier responses. Households completing part I of the survey (household characteristics) in any wave were not asked Part I again. Households reporting payment information in Part II were not re-surveyed.¹² Households that responded to

¹¹ Taxpayers who filed their tax returns after April 15 received their payments either in their allotted time based on their SSN, or as soon as possible after this date (about two weeks after they would receive a refund). Taxpayers filing their return after the extension deadline, October 15, were not eligible for payments. Since 92 percent of taxpayers typically file at or before the normal April 15th deadline (Slemrod et al., 1997) and the vast majority of late returns are filed close to October 15, there should be very few ESPs that are distributed during the main program and have their distribution date set by the lateness of the return. Finally, due to human and computer error, about 350,000 households (less than 1%) did not receive the child tax credit component of their payment with their main payment. 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.

¹² The survey thus only measures the first ESP received by a household, or, if more than one was received prior to answering Part II of the survey, the household was instructed to report the larger. The decision not to allow reporting multiple payments and not to re-survey households that report payments significantly reduced the cost of the survey at the cost of missing only a few payments.

the first question on Part II that they don't know whether they had received a stimulus payment, that they have not received one and "expect to," or respond that they "are unsure whether I will get any" do not proceed to Part II and are re-surveyed with Part II in a later wave (if there is one). Finally, households that respond "No, and I am definitely not getting one" do not proceed and are not re-surveyed.

In terms of timing, the surveys covered the main period during which payments were distributed with random timing. A supplementary on-line appendix gives the timing of the surveys, the invitations and reminders, survey, response rates, and information about data access.

The repeated nature of the survey implies that the recall window for the payment is relatively short: one month for the email/web survey when it is first fielded and just over one and a half months for the mail/scanner survey when it first arrives. The survey was administered to all households meeting a Nielsen static reporting requirement for January through April 2008, which amounted to 46,620 households by email/web and 13,243 by mail/barcode scanner.¹³ For both types of survey, the response rates were 72% to the first wave, and 80% after all waves, giving 48,409 survey responses (of which some are invalid for various reasons).

To proceed, I drop all households from the analysis that: i) do not report receiving a payment (roughly 20 percent of the respondents); ii) do not report a date of payment receipt; iii) report not having received a payment in one survey and then later report receiving a payment prior to their response to the earlier in a later survey; iv) report receiving a payment after the date they submitted the survey; v) report receiving a payment by direct deposit (by mail) outside the period of the randomized disbursement by direct deposit (mail), and vi) do not report means of receipt but report receiving a payment outside both periods of randomized disbursement.¹⁴ These cuts reduce the sample to 28,937 households reporting receiving a total of over 26 million dollars in payments.

These responses are merged with the information on total spending on each trip taken by each household during 2008 from the KILTS NCP which includes only households that meet the Nielsen static reporting requirement for 2008. These data are collapsed down to total spending

¹³ Thus 79% are surveyed by email/web. According to the October 2009 Current Population Survey, 69% of households have computer access at home (U.S. Census Bureau, Population Division, Education & Social Stratification Branch <http://www.census.gov/population/www/socdemo/computer/2009.html>).

¹⁴ I allow a two day grace period for reporting relative to survey submit dates, and a seven day grace period for misreporting relative to the period of randomization. I do not adjust the reported date of receipt in either case.

per week per household.¹⁵ I consider weeks in which no expenditures are reported to be weeks with zero expenditures, with the exception that if a household stops reporting expenditure during 2008. I consider spending data from that point on missing rather than zero for these ending weeks of the year.

This sample selection is not random. But it is (presumably) uncorrelated with the randomization, and so creates no bias for estimation of the average treatment effect in the remaining sample. But it is important to note that given heterogeneity in treatment effect, non-random sample attrition may create bias for inference if there are differences in treatment effects between households dropped from the sample and households that are included. It is also true that there is selection involved in which households are recruited and participate in the NCP survey.

Table 2 shows summary statistics for the data and sample used. Average (weighted) weekly spending in the baseline, static sample is \$149. In comparison, in the 2008 CEX Survey, average spending on a broad measure of nondurable goods is about \$400 per week and total expenditures on goods is about \$800, or 2.6 times larger for CEX broad nondurable goods and 5.3 larger for CEX total expenditures.¹⁶ The spending of households receiving payments by mail is \$16 less than that of households receiving a payment by direct deposit. The average ESP conditional on receiving one is \$898. Households receiving ESP by direct deposit on average have higher payments by about \$190, which is reasonably consistent with their having on average 0.4 members more in these households.¹⁷ This difference is also similar to that found in the CEX: the average ESP in the CEX Survey is \$940 and the average ESP received by direct deposit is \$180 more than the average received by check. As was true for the actual disbursements, most reported payments are clustered at multiple of \$300.¹⁸ More details are provided in the on-line appendix. These features of the distributions of EFTs line up well with those in similar surveys conducted by the SIPP and the CEX (see Parker et al. (2013)).

¹⁵ The main conclusions appear to be substantively similar using data on all households collapsed down from individual items purchase and covering all households surveyed, although this analysis is not included in this draft.

¹⁶ The average household sizes, both among recipients and on-time recipients, are very similar to those in the CE Survey.

¹⁷ Each additional child eligible for the CTC leads to \$300 larger payment, while a married couple receives \$600 more than the equivalent family with an unmarried head.

¹⁸ Households in the mail survey were prompted by the example of \$600 as part of reminding them how to enter a dollar amount on their barcode scanner. There was no amount prompt in the on-line survey.

3. Estimation methodology

I use the following specification to examine the average impact of the receipt of a payment on spending for household i of type j in week t receiving a payment by method m :

$$C_{i,t} = \mu_i + \sum_{s=-L,S} \beta_{s,j} ESP_{i,t+s} + \tau_{m,j,t} + \eta_{i,t} \quad (1)$$

where $C_{i,t}$ is a measure of spending, μ_i is a household-specific intercept that captures differences in the average level of spending across households, $\beta_{s,j}$ are coefficients measuring the spending response on leads (up to L) and lags (up to the largest possible lags, S) of $ESP_{i,t}$, which is a measure of the receipt of a payment by i in t , $\tau_{m,j,t}$ is a set of indicator variables for every week in the sample for each type of household for each method of disbursement (mail or by direct deposit), and finally $\eta_{i,t}$ captures all expenditures unexplained by the previous factors. For measures of household spending, $C_{i,t}$, I use either the dollar amount of NCP spending in week t for household i or the ratio of that level of spending to the average weekly spending of that household during the first 12 weeks of 2008 (prior to the ESP disbursements). The corresponding interpretations of $\beta_{s,j}$ are respectively the dollar change in spending or the percent change in spending caused by receipt. For measures of payment receipt, $ESP_{i,t}$: I use one of three measures: a dummy variable indicating whether any payment was received by household i in week t ; that dummy variable times the amount of payment received by household i ; or that dummy times the average amount of the ESP received by households of type j getting payment by method of receipt m .

It is important to note three features of equation (1). First, the fact that there are time effects interacted with type j and means of receipt implies that differences in the impact of aggregate changes or difference in seasonal spending between recipients with different characteristics of means of disbursement do not bias the estimated $\beta_{s,j}$. That said, this specification is demanding of the data and I find no evidence for all households that this bias is economically significant, so I also report results with a complete set of time dummies interacted only with household type and not with means of receipt.

Second, to evaluate how spending responses differ across households, the $\beta_{s,j}$ are allowed to differ by households type -- by characteristics or responses to survey questions. The $\beta_{s,j}$ are

the key parameters of interest and measure the spending effects of the receipt of a payment for households of type j .

Finally, identification of the key parameters of interest for a type j does not require that households are similar, or unselected, across types. Consistency requires that the variation in $ESP_{i,t}$ be uncorrelated with all other factors that might influence household expenditure besides the receipt-driven variation of interest. Selection into type j -- or more generally correlation of type and average treatment effect -- does not bias estimates of average effects within type. In fact, differences in average treatment effect are the main issues of interest. But it is important to note that selection into the NCP and/or selective attrition out of our sample ex ante or over time could bias population inference of differences in average treatment effects across household types if correlated with treatment effect. For example, if, in the population, the extent of consumption smoothing were uncorrelated with wealth across households, and if low wealth households that smooth consumption well did not respond to our survey and everyone else did, then we would observe in our sample that low wealth households smooth consumption more poorly than high wealth households but this would be true only for our sample and not the population.

In estimation, standard errors are adjusted to allow for arbitrary heteroskedasticity and within-household serial correlations.

4. The average response of spending to the receipt of a payment

This section first shows that there is a significant increase in spending caused by the receipt of a payment on average across all households (only one type j so that $\beta_{s,j} = \beta_s$). Second, this section documents significant heterogeneity in spending response across households. Consistent with previous research, households with low levels of liquid wealth raise spending when the payment arrives while households with significant liquidity smooth spending across the arrival of the payment relatively well. The following section uses heterogeneity in spending response to test a number of theories for lack of consumption smoothing by testing whether each can account for the differences in spending behavior observed across households.

Beginning with the average response, Table 3 shows, for a variety of different regression specifications, that there is a highly statistically significant increase in spending among NCP

household upon arrival of a payment. Each column reports, for a different regression, the coefficients on the included leads, contemporaneous value, and the first 6 lags (of the complete set of lags included) of ESP . The first three columns of Table 3 displays results from regressions that use all available variation in timing, including that due to different method of disbursement. That is, this first set of estimates use equation (1) with $\tau_{m,j,t} = \tau_t$, and so treats all variation over time in the ESP receipt – including that due to receipt by mail vs. direct deposit – as valid for identifying the spending effect of receipt.

In the first column of Table 3, the dependent variable is NCP spending (in dollars per week) and the $ESP_{i,t}$ is an indicator variable whether a payment is received, so that the coefficients on contemporaneous $ESP_{i,t}$ implies that households on average increase their spending by a reasonably precisely estimated \$13.42 in the week that the ESP arrives.

The second three columns display results from regressions that treat the two different methods of disbursement as two separate experiments. That is, this second set of estimates use equation (1) with $\tau_{m,j,t} = \tau_{m,t}$, a full set of week controls interacted with method of receipt, and so treats the two different methods of disbursement as two separate experiments. Column four estimates that households on average raise spending on NCP goods by a slightly lower but still highly statistically significant \$12.50 the week the payment arrives.

The second and fifth columns confirm this finding for a specification in which the dependent variable is dollar spending as a percent of average spending in the first 12 weeks of the year, and imply that spending rises by just under 10 percent of average weekly spending the week of arrival. Given average weekly spending of \$149, these percentages imply spending effects consistent with those in the first and fourth columns that give more weight to higher average spending household relative to the results in the second columns.

Finally, the third and sixth columns in Table 3 report the most important specification for later analysis. In these regressions, dollar spending is regressed on the lead/lag polynomial of the indicator variable for receipt times the average amount of ESP (within in each means of receipt and divided by 100 so as to report a percent). According to these columns, households spend one and a half percent of the ESP the week of arrival. Again, this is highly statistically significant and is consistent with other columns given the average reported ESP amount.

The increase in spending on arrival is quite sharp. There is no evidence of any greater spending two weeks before the arrival of the ESP in any specification – all point estimates are economically small and almost all are negative. None are statistically significant. This suggests that there is very little reporting error in date, as for example due to recall error, at least after removing the clearly erroneous reports.¹⁹

While there is no increase in spending immediately before receipt, there is a continued higher level of spending after the week of receipt. This higher level of spending declines slightly the week after arrival and then declines more rapidly so that the coefficients on weekly spending in all specifications are all individually statistically insignificant by the third week after the week of receipt. The last two rows of the table report the cumulative spending caused by receipt over the four weeks starting with the week of receipt and over the seven weeks starting with the week of receipt respectively. Over four weeks, the cumulative dollar spending (\$33 or \$27), the percent increase in spending over the period (roughly 5%), and the total share of the ESP spent (roughly 3.5 %) are all highly statistically significant.

Over seven weeks, the estimates are similar in terms of total spending, but are less precisely estimated.²⁰ As a result, for the balance of the paper, I focus on consumption smoothing on arrival and over the following four weeks. In general, the pattern of smoothing over seven weeks is roughly similar to that over four weeks but with larger standard errors, although consistent with statistical uncertainty, occasionally the point estimates at seven weeks are significantly lower or higher.²¹

¹⁹ The arrival effect does not measure the extent to which spending may have changed as the stimulus program was developed, announced, and the details fleshed out and made public. Such changes in response to the dissemination of information are orthogonal to the variation used to identify the spending effects and so are not estimated by, and do not pose a problem of bias for, the current method.

²⁰ Note that the percent increase is lower, but since it is an average over seven weeks, and is roughly $4/7^{\text{th}}$ the size of the percent increase in spending over four weeks, the implied total spending amounts are similar.

²¹ These results for average spending are reasonably robust. Similar patterns emerge when restricting to households reporting spending in at least half the weeks or in every week, and when trimming the top and bottom 1% of spending. Similar percentage changes and spending effects relative to average dollar spending are found using as a measure of weekly spending the more volatile and smaller measure of spending constructed as the sum of all individual items purchased instead of the sum of all total trip spending and using households that do not meet the Nielsen static reporting requirement for the year. Finally, while these results are not directly comparable to those from the CEX – the CEX excludes some items like drugstore items that are in the NCP and the NCP does not cover most of the spending categories in the CEX – they are also not inconsistent with them. Parker, Souleles, Johnson, and McClelland (2013) estimate that households raise spending on a broad measure of CEX nondurable goods and services by slightly more, about 2.1 – 4.5 percent of spending.

If the spending response were the same across households, then cross-sectional information would be useless for evaluating models of lack of consumption smoothing. Instead, as I now show, there is significant heterogeneity in spending response across households correlated with liquidity. Households with low levels of liquid wealth in a binary split of the sample spend at a rate roughly three to four times that of liquid households.

Why investigate liquidity? First, an important element in the canonical, textbook explanation of increases in spending when income arrives is the presence of a liquidity constraint or another model of incomplete financial markets. A household experiencing temporarily low income needs to run down liquid wealth or borrow to maintain its level of spending. If a household either is unable to borrow due to a binding liquidity constraint or does not want to borrow due to a high elasticity of precautionary saving with respect to cash on hand, then low current or recent income can indicate that a household has a higher propensity to spend income on arrival. The main prediction is that households with low levels of liquid wealth fail to smooth consumption. Second, this prediction has been widely confirmed in empirical work ((Zeldes (1989b), Johnson, Parker, and Souleles (2006), Agarwal, Liu, and Souleles (2007)).

To measure liquidity, Part I of the supplemental survey contains the question “In case of an unexpected decline in income or increase in expenses, do you have at least two months of income available in cash, bank accounts, or easily accessible funds?” and the respondent can answer yes or no.

Table 4 shows that households with low liquidity, 36 percent of the sample, spend at just under double the rate of the average households, 2.5 to 2.8 percent of the payment the week of arrival and 4.9 to 6.6 percent the four weeks of and following arrival. And while households with sufficient liquid wealth still exhibit a statistically significant increase in spending in response to arrival, they spend only at one fourth the rate of households with insufficient wealth the week the payment arrives, and one half to one third the rate over the four weeks of and following arrival.²² This finding is consistent with previous research and consistent with the presence of liquidity constraints or incomplete financial markets (e.g. Zeldes (1989a), Deaton (1991), and Carroll (1997)). Lack of consumption smoothing is concentrated among households

²²Despite the additional set of time dummies interacted with method of receipt in the regressions of Table 4, the sample weighted average of the spending increases are almost exactly equal to the average spending increase reported in Table 3.

with low liquidity. We now proceed to use this average response and this heterogeneity to evaluate theories for the lack of consumption smoothing.

5. Testing models of lack of consumption smoothing

The significant heterogeneity in spending increases in (at least) the dimension of liquidity implies that one can test models of consumer behavior by evaluating their ability to explain the cross-sectional differences in spending responses. If a model predicts a spending response but no variation in consumption smoothing, then it is inconsistent with the finding that some households smooth consumption well. Further, given the large amount of heterogeneity associated with liquid wealth, any characteristic uncorrelated with liquid wealth is going to miss a large amount of variation in behavior and is unlikely to be the main reason that households fail to smooth consumption. That said, there may be significant further heterogeneity in spending responses within low liquidity and within high liquidity households, and a theory may explain a share of low liquidity households that smooth consumption well, and/or some high liquidity households that smooth consumption poorly.

Plausible theories then must predict variation in consumption smoothing along an observable characteristic that is correlated with liquidity. Such a relationship raises the question of whether this characteristic causes low liquidity or whether this characteristic is caused by or merely correlated with low liquidity. This paper does not observe plausible exogenous variation in the characteristic or liquidity, and so cannot distinguish the direction of causation. For some theories, it may even be impossible to exogenously vary the characteristic (as for a preference parameter for example).

5.1 Heterogeneity in consumption smoothing: transitory state or persistent characteristic?

This subsection shows that lack of consumption smoothing is a persistent characteristic and not due to temporarily low liquidity. One hypothesis is that lack of consumption smoothing is caused by a series of poor income shocks, as in the models of such as Zeldes (1989a), Deaton (1991), and Carroll (1997), or by a transitory low level of liquid assets due to fixed costs of portfolio adjustment, as in the model of Kaplan and Violante (2014). Alternatively, persistent behavioral traits cause low liquid wealth and, either directly or indirectly through low liquidity,

cause spending responses. Most obviously, this behavioral trait could be due to preferences, but it could also be due to nonlinearities in budget constraints such as caused by means tested benefit programs.

This section shows that while measured only crudely, recent income growth and consumption smoothing are unrelated across households -- transitory income shocks play no measureable role. Instead, the level of income in 2008 however has a strong correlation with both liquidity and consumption smoothing. Households with low current income smooth consumption poorly while households with high current income smooth consumption well. But a similar relationship exists for income in 2007 and, even more strikingly for income in 2006, two years prior to the payments. Thus, lack of consumption smoothing is a persistent characteristic, not primarily driven by transitory bad income shocks or costs of accessing illiquid wealth and temporary low liquid wealth.

Annual income is reported in the NCP at the end of each calendar year for the previous calendar year. I use the NCP reports of annual income for each household in 2006, 2007, and 2008, taken from survey years 2008, 2009, and 2010 respectively. Income is reported in 19 income ranges. The ranges are each less than or equal to \$5,000 for incomes less than \$50,000, then rise through \$10,000, \$25,000 ranges until the highest two ranges covering an income range or \$150,000 to \$200,000 and \$200,000 and above. A household is defined as having an income increase if it reports moving to a higher range and a decrease if it reports moving to a lower range.

Panels A in Table 5 show spending responses for households whose income moves from to a lower range, stays in the same range, and moves to a higher range from 2007 to 2008, the year of the payment program. There is no evidence of any differential spending response across categories of income growth. Panel B repeats this exercise for income changes from 2006 to 2007. In Panel B, there is no evidence that households that have had declines in income spend more of their payments on receipt than households whose incomes have stayed in the same range who in turn spend more than those whose incomes have increased. In fact, there is some evidence of the reverse, that household spending responses are increasing in income growth from 2006 to 2007. These results on income growth are inconsistent with the view that the high spending response of low income households is due to temporarily low income.

Panel C of Table 5 splits households into three roughly equal groups according to 2008 income.²³ The bottom 36 percent of households by 2008 income – those with annual labor incomes of less than \$35,000 – consume, both on impact and cumulatively, at more than double the rate of the middle income group. The group with highest 2008 income does not consume a statistically significant fraction of the payment in either specification or at either horizon. This is consistent with the textbook model of liquidity constraints, in which households are unable to borrow or to insure future labor income risk so that temporarily low income leads them to violate consumption smoothing.

However, this same pattern is evident in Panel D when households are split according to their incomes in 2007. And more strikingly still, the same pattern is evidence in Panel E using income in 2006: low income in 2006 indicates poor consumption smoothing in 2008, and high income in 2006 indicates good consumption smoothing in 2008. This evidence is completely at odds with a model in which either poor income shocks or portfolio management of cash flows and illiquid high return assets have caused both low liquidity and large spending responses for some households. Rather this evidence shows that some households that have persistent low income are poor at smoothing consumption. In fact, low income in 2006 is as good as if not better than contemporaneous liquidity (Table 4) at separating the households who spent from those who did not.

How does income interact with liquidity in explaining spending responses? Table 6 displays results splitting households by income and liquidity. Panel A and B show that 2008 income level is correlated with income: 45 percent of households with low liquidity have low income in 2008 while 31 percent of households with sufficient liquidity have low income. Panel C and D show that this correlation is just as strong for income in 2006. Moreover, conditional on sufficient liquidity in 2008, households with low income in 2006 have significant spending responses (Panel C) while households with high incomes do not. And conditional on low liquidity (Panel D), there are statistically significant differences in the size of the spending response at four weeks between households with high incomes in 2006 and those with low incomes. This is not to say that liquidity does not have additional explanatory power conditional

²³ These ranges/choices follow the industry standard, see Zeldes (1989b), Jappelli, Pischke and Souleles (1998), Jappelli (1990), and Souleles (1999).

on income also, but simply that a large share of the variation in spending response in 2008 across households both unconditionally (Table 5 panel E) and conditional on current liquidity (Table 6 Panels C and D) is explained by household income in 2006.

5.2 Inattention and beliefs: did some households spend due to being surprised by payments?

This subsection shows that the payments did not cause spending because they were unexpected. Most households expected the payments, there are significant spending responses for those who were expecting their payments, and there is little evidence of a larger spending response among households surprised to get payments. However, there is some weak evidence linking understanding of the stimulus program and consumption smoothing: in one of two specifications spending response are greater for the households that incorrectly understood the payments by not expecting the payment, underestimating the amount, or (primarily) overestimating the amount.

One reason that the arrival of a pre-announced payment might cause an increase spending is that some households might be surprised by the arrival of the payment despite available, and in this case even provided, information about the payment. Motivated in part by the findings of excess sensitivity, recent modeling of human behavior has considered the costs of gathering and processing information and how economizing on these costs might alter the predictions of the canonical model of consumption and saving (e.g. Caballero (1995), Reis (2006), and Sims (2003)). While these models need not imply strong spending response to receipt, Reis (2006) shows that households with large costs of optimization choose to be ‘inattentive savers’ who follow a saving plan and optimally let consumption track income.

While these models also have implications for the relationship between measures of planning and spending responses, this subsection assesses the role of expectations in the spending response. The supplemental survey asked households who reported that they received a payment: “Was this about the amount your household was expecting?” Households were given the following to choose from to answer: ‘No, and we were surprised to get any rebate at all,’ ‘No, and it was less than we were expecting,’ ‘No, and it was more than we were expecting,’ ‘Yes, and we’ve known the approximate amount since February,’ ‘Yes, and we’ve known the

approximate amount since March,’ ‘Yes, and we’ve known the approximate amount since April,’ ‘Yes, but we only learned about it recently,’ and ‘Not sure/don’t know.’

Panel A of Table 7 contains the results for three groups of households and shows four main results. First, the last row of Panel A shows that most households expected the payment when it arrived. Only 5 percent of households were surprised by the arrival and 7 percent found that it was more than they were expecting (columns 2 and 5). Thirteen percent of households found that their payment was less than they expected (columns 3 and 6). Second, even those households who had been expecting the payment in the correct amount had significant spending response to the arrival on impact and cumulatively over 4 weeks (columns 1 and 4). Third, while few households were surprised by the ESP, there is some statistically weak evidence that this led to a higher propensity to spend on impact but no evidence of more spending over four weeks.²⁴

Finally, and most interestingly, there is not only no decline in spending for households who received payments that were less than they were expecting, but in fact, point estimates suggest that these households spent at higher rates than the average household. One can reject the equality of the contemporaneous spending response between households who are expecting the payment (columns 1) and those who are negatively surprised (column 3) (t-statistic 2.08). This is not the case for the four week response (t-statistic = 1.47) nor at either horizon in the second set of columns, in which the responses across groups are more similar.

Panels B and C split households by liquidity. The share of households in each group shows that a larger share of households with low liquid wealth are negatively surprised by the amount of their payments (18 percent of households with low liquid wealth and only 10 percent of households with sufficient liquid wealth). That is, the mistake of expecting a larger than actual ESP was more prevalent among households with insufficient funds. This is not true of simply being surprised by the payment. The share of households who learned only recently of were surprised is similar among recipients with and without sufficient liquid funds.

In terms of the estimates coefficients, smaller samples lead to less statistical power in within each level of liquidity. For households with sufficient liquid wealth, where one might expect news to carry the largest effect, there is mixed evidence of any role for expectations in

²⁴ The propensity to spend is larger for households surprised by the payment in the first triplet of columns, but not in the second triplet in which the distribution by mail and by direct deposit are treated as separate experiments.

spending responses. For households with low liquid wealth, there is some evidence that those who expected the payment has smaller spending responses, but even this is statistically weak (the strongest finding is the contemporaneous spending response between columns 1 and 2 which has a t-statistics of 1.60).

In sum, with few households surprised by the amount of the ESP upon arrival, and with scant evidence for increases and decreases in spending in the response for those households who were surprised, there is no evidence that inattention caused the violations of consumption smoothing. But there is some evidence that more households with low liquidity were negatively surprised by their payments, and, while statistically weak, that these households have the largest propensity to spend on arrival despite this negative surprise. Thus, while there is no evidence that households spent money they were not expecting, these findings do suggest that households with low levels of economic sophistication or planning as measured by understanding of the stimulus payment program have little ability to smooth consumption.

5.3 Sophistication: financial planning, planners, and coupons use

This section provides evidence on the role of that lack of planning as a common trait across behaviors causes violations of consumption smoothing. Households that have made financial plans in the last two years smooth spending across arrival well, and a small share of households that plan extensively for vacation smooth seem to smooth consumption well. Financial planning is more important than planning for vacation, in that spending responds to payment for households that plan for vacations but not for households that have made financial plans. Finally, households that make use of coupons or deals when shopping smooth consumption significantly better than those that do not.

Lusardi (1999) theorizes that careful planning – through making better investment choices and considering the need to save – is a major determinant of wealth accumulation for retirement. Further, the paper shows evidence that differences in the propensity to plan explain a significant amount of the differences in wealth accumulation in observed in the US. Ameriks, Caplin, and Leahy (2003) show that households who say that they have made a financial plan have much greater financial wealth after controlling for large set of other possible determinants of wealth accumulation (including income and many measured of traditional preferences).

Finally, as noted, Reis (2006) shows that households with large costs of optimization choose to be ‘inattentive savers’ who follow a saving plan and optimally let consumption track income.²⁵

Two questions were asked in the supplemental NCP survey to measure the importance of planning behavior for consumption smoothing. The first question mimics the Ameriks, Caplin, and Leahy (2003) question: “In the last few years, have you gathered together your household’s financial information, reviewed it in detail, and formulated a financial plan for your household’s long term future?” The second question relates not to financial planning, but to planning as a trait in another sphere, in this case for vacation: “Before going on a vacation, how much time do you spend examining where you would most like to go and what you would like to do?” with possible answers: ‘A great deal of time,’ ‘Quite a bit of time,’ ‘A little time,’ ‘Almost no time,’ and ‘Do not go on vacation.’ Households responding that they do not go on vacation are dropped from the analysis of this question.

Panel A of Table 8 shows that households that have made financial plans are much better at smoothing consumption than those that have not. First, note that there is a lot of variation in this question in this sample, with roughly half the households responding each way. Second, planning is highly correlated with liquidity. Panels B and C shows that 79% of households that plan have sufficient liquidity and 69% of households with sufficient liquidity plan. Panel A shows economically large differences in spending responses. For households that have formulated financial plans, the spending response is only borderline statistically significant and roughly a third as large as for those how have not planned. About three quarters of the total spending response in the sample is accounted for by households that do not make financial plans. Finally, financial planning has some explanatory power beyond liquidity. Among households with liquidity, those that do not plan do not smooth spending. While pairwise comparisons are rarely statistically significant level, a joint test of equality of responses between planners and non-planners in both Panels B and C rejects equality with 95% (90%) confidence for both contemporaneous and four week responses using all variation in timing of receipt (using only variation of timing within each means of disbursement).

Table 9 reports a similar set of tests for vacation planning. Again, there is lots of variation in the sample, but time spent planning vacations is almost uncorrelated with liquidity.

²⁵ See also Lusardi and Mitchell (2007).

Across degrees of planning, moving across the columns, 65%, 68%, and 67% have sufficient liquidity. The remainder of the table points a statistically weak case that vacation planning matters for consumption smoothing. In Panel A households that spend a great deal of time planning for vacations do not raise spending the week of arrival as much as the other households (statistically significant at the 90% confidence level only). The point estimates have the same pattern over the entire month, but the economic significance is smaller and larger standard errors preclude making any conclusion with confidence.

Looking at households with different levels of liquidity, in Panel C there is a tendency for those who spend a great deal of time to smooth spending better than others among households with low liquidity. In sum, not only are the differences across vacation planners statistically weaker than the differences across financial planners, only twenty two percent of households plan a lot and smooth consumption well, and forty four percent of the sample spends “quite a bit of time” planning for vacations and these households giving this “middle” response still show a substantial spending responses similar to households that plan less.

The final variable dealing with sophistication is the extent to which households use coupons or deals when making NCP purchases. When households scan in individual purchased items, they are asked to input whether they used a coupon or deal in that purchase. For participating stores, this information is also provided by the store. I calculate the share of individual reported purchases that use coupons or deals, and split households evenly into those whose coupon use is low and those whose coupon use is high.

On the one hand, one might expect households that have fewer resources to spend more time on conserving them, suggesting that households that use coupons should have low liquidity and smooth spending poorly. On the other hand, if households that use coupons plan – that is, spend more time and effort optimizing – then they may not only make better use of the resources that they have but also do a better job of inter-temporal optimization and thus of consumption smoothing.

In the NCP, coupon use has only a correlation of 0.07 with liquidity. While the households are evenly split in Panel A of Table 10, among households with sufficient liquidity slightly more use coupons (Panel B), and among households with insufficient liquidity, slightly fewer use coupons. Panel A shows however that coupon use is economically significantly and

statistically significantly related to spending responses at the one week horizon, with high coupon use households smoothing spending better. At the one month horizon, the difference is statistically weaker, but the same pattern is present. Conditioning on liquidity, because it is largely uncorrelated with coupon use, does not change this ordering, but it also reduces the sample size used to estimate each coefficient, and standard errors rise as a result.

In sum, these results are consistent with an important role for financial planning as a determinant of consumption smoothing, and with a role for optimization as revealed by coupon use, but the case for an important role for planning as a general trait in other spheres of activity rather than only financial planning is weak.

5.4 Spenders, savers, self-control, and procrastination

This section shows first that households that self-identify as the type of household that lives for today and spends has a large spending response to the payment that households that identify as more patient do not. Second, there is an economically large but statistically-weak higher propensity to spend on arrival among the small share of the population that frequently regrets past purchases. But this does not explain much of the aggregate spending response. The other 95 percent of the population still exhibits substantial violations of consumption smoothing. And while this weak evidence is suggestive of a role for sophisticated people with self-control problems in spending responses, such a role does not appear when sorting households by the timing of their response to the surveys. Across households, the size of spending response to the payment decreases with the delay in responding to the supplemental survey.

A substantial literature has suggested that households can be modelled either as having heterogeneous impatience or as having heterogeneous behaviors with some standard lifecycle consumers and other households simply consuming their incomes (e.g. Campbell and Mankiw (1989), Krusell and Smith (1998)). The NCP supplemental survey asked households to characterize themselves as spenders or savers: “In general, are you or other household members the sort of people who would rather spend your money and enjoy it today or save more for the future?” with a binary choice of ‘spend now’ and ‘save for the future.’ There is lots of variation in the responses, with two thirds of households reporting that they are the type to save for the future. Low liquidity is significantly correlated with being a spender, a correlation of 0.31.

Causation of course could run in either direction, but the phrasing as the “type of people” was designed to avoid households simply responding based on current behavior.

Consistent with persistent differences in households, households that report being the type of people who spend and enjoy today have much larger spending response than those that report being the type who save. Table 11 Panel A shows that the self-reported spending types exhibit large violations of consumption smoothing; the self-reported saving types smooth consumption the week of arrival but still show a statistically significant spending response over the month following arrival that is about half the size of the self-reported spending types. In the week of arrival, saving types have an economically small and statistically insignificant increase in spending, spending-types have a statistically significant and three times larger increase in spending. Over a month, the cumulative spending of spending types is estimated to be double that of saving types, but this difference is not statistically significant.

Panel B and C of Table 11 show that this pattern continues to hold among households with low liquidity, but the only statistically significant (at the 90% level) differences are found for households with low liquidity the week the payment arrives when spending of spending types is just under double that of saving types. Among households with sufficient liquid wealth, there is little evidence of greater spending by households who characterize themselves as spenders.

Another possible reason for spending payments when they arrive is that some households have difficulty not spending liquidity. For example, one theory suggests that some households spend more of the payment on arrival than they would have chosen to had they been able to commit not to spend as much at an earlier time (see Laibson, Angeletos, Repetto, Tobacman, and Iinberg (2001); Gul and Pesendorfer (2004a and 2004b)). I investigate this proposition in two ways.

First, to investigate whether differences in the degree to which households perceive their past spending to be suboptimal, households were asked “Many people sometimes buy things that they later wish they had not bought. About how often do you or other household members make purchases that you later regret?” and households could answer: Often, Occasionally, Rarely, or Never. Panel A of Table 12 shows that there is significant variation in the population in response to this question. About forty percent of households regret purchases often or occasionally, while

60 percent do so rarely or never. But the variation is almost unrelated to liquidity. And frequency of regret explains little of the differences in spending responses across households.

The contemporaneous response to the arrival of the payment is almost identical between households who often or occasionally regret purchases (columns 2 and 5) and those who rarely/never do (columns 3 and 6). Columns 1 and 4 break out only those households who say that they often regret past purchases. Among this small set of households, the contemporaneous and cumulative spending responses are much larger, but the contemporaneous responses are only borderline statistically significantly different from zero and the cumulative responses are statistically even weaker.

Panels B and C show variation conditional on sufficient liquidity and low liquidity. There is no evidence for a role of regret in spending responses for households with sufficient liquid wealth in Panel B. In Panel C, there is some evidence that households that rarely or never regret past purchases actually increase spending more in response to payment arrival than households that regret purchases more often. While this pattern is evidence against the sophisticated models of self-control, or at least against the existence of a significant number of households who have not successfully managed their issues of self-control, this evidence is consistent with the existence of some households that are naive about their self control problems. That is, if the primary source of variation were not problems of self-control, but instead the perceptions of them and therefore the wherewithal to manage them, then a response of never or rarely regretting would signal lack of understanding and lack of management of self-control problems which could to larger spending responses.

As a second approach, I estimate whether the spending response is higher for households that delay responding to the supplemental survey, relative to households that respond rapidly.²⁶ Households that delay response may have more problems of short-term self-control. Dividing the sample into three groups whose responses to the survey were rapid, medium and slow, reveals almost no correlation between delay in survey response and liquidity. Table 13 further shows no evidence that procrastination is associated with worse consumption smoothing. In fact, if anything, the more that the survey is procrastinated, the lower the spending response to receipt

²⁶ Rapid/medium/slow is responding the day of the survey/in days 1-7/after 8 or more days for email surveys, and in the first 5 days/in days 6 to 12/after 13 or more days for mail surveys.

of a payment. This result is not close to statistically significantly for the contemporaneous response but for the cumulative responses, the difference between the third column and the first and second columns has a t-statistic of 1.75, and between the sixth column and the fourth and fifth columns has a t-statistic of 1.55. Among households with sufficient liquidity, only those who respond to the survey in a middling amount of time spend significant amount on arrival (Panel B). Among households with low liquidity, there is no noticeable pattern.

In sum, while there is statistically weak evidence that there is a small portion of the population with ex-post perceived self-control problems that are associated with violations of consumption smoothing, there is no evidence that theories of hyperbolic discounting or self-control explain a significant portion of the observed differences in spending responses in this natural experiment.

7. Conclusion and discussion of results

This evidence has several implications for the modelling of consumption and saving behavior.

Inattention, rational or otherwise, is not an important determinant of month-to-month spending responses to predictable payments. This paper shows that the economic stimulus payments of 2008 caused significant spending increases and at the same time were widely anticipated. The significant average response to the largely-expected payments -- a nine to ten percent increase in spending the week the payment arrived, a five percent increase over the first month, and a three and a half percent increase the 7 weeks including and following arrival -- is almost entirely due to the behavior of the subpopulation of households that have less than two months of income in liquid assets. Thus, on one level, the view that households smooth consumption across predictable changes in income subject only to the financial friction posed by a borrowing constraint receives significant support.

However, the weight of the evidence is consistent with spending responses being driven by a persistent household characteristic rather than transitory economics circumstances, such as shocks to labor income. Most convincingly, low income two years prior to the payments is highly correlated with large spending responses to the payments. Income growth, although not measured precisely, explains almost none of the variation in spending response. Also a number

of behaviors and measures of household type are also significant explanators of who smooths spending and who does not, including being the type of households that lives for today, having made a financial plan in the last two years, and using more coupons or deals when making purchases. Statically weak support also comes from the larger responses of households that are disappointed in their payment amount and of households that do not plan for vacations, as well as from the small share of people who often regret past purchase.

Whatever this persistent characteristic is must be associated with low liquid assets. Could this be impatience? A model in which some households are highly impatient could produce large spending responses among households with low liquidity as identified by the saver-spender distinction. And costly investment in skills can explain a relation between impatience and low labor income. But other evidence suggests a role for sophistication or planning, so that, if high levels of impatience are the driving force, costly planning or optimization may be an additional causal channel through which impatience affects spending.

A more nuanced interpretation is that the behavior of some households is not well characterized by the standard model and this different behavior, perhaps due to low levels of economic sophistication or ability, may cause both low levels of liquid wealth and poor consumption smoothing. Low ability or economic sophistication could be a persistent characteristic, and it could cause both poor economic outcomes like wealth, income, and consumption smoothing, and outcomes like lack of planning, lack of coupon use, frequent regret of purchases, and little understanding of payment programs like the stimulus program. In this view, high spending responses to predictable income and lack of planning, being a 'spender,' etc. are all symptoms of low levels of sophistication, for want of a better term.

Two final caveats to the findings of this paper are worth mentioning. First, specific characteristics of the payments of 2008 may be pertinent for the findings of this paper. More research is required to know how broadly the patterns of behavior uncovered apply. Second, as should be clear, in this paper preferences were not manipulated. Thus, while this paper relates heterogeneity in treatment effects to household characteristics, causation could run from characteristics to spending responses, or the reverse, or both could be caused by a third factor. As examples, high levels of impatience could cause low liquidity and high spending responses (and financial planning). Financial planning could be caused by high wealth which also causes

small spending responses (and makes households think about and live for the future). And coupon use, low liquidity, and large spending responses could all be caused by low income relative to future income.

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

Details of Your Stimulus Payment Calculation

We calculated your stimulus payment based on the following rules.

Filing Status

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

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

Qualifying Children

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

Whom You Can Contact With Questions

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

1F220535281-2AN-3AN-S-17-18N-19N-FN-01N-0FN

References

- Agarwal, Sumit; Liu, Chunlin and Souleles, Nicholas S., 2007, "The Response of Consumer Spending and Debt to Tax Rebates – Evidence from Consumer Credit Data," Journal of Political Economy, 115(6), December, pp. 986-1019.
- Agarwal, Sumit, John Driscoll, Xavier Gabaix, and David Laibson, 2009, "The Age of Reason: Financial Decisions over the Life Cycle and Implications for Regulation," Brookings Papers on Economic Activity, Vol. Fall, pp. 51-117.
- Ameriks, John, Andrew Caplin, John Leahy, 2003, "Wealth Accumulation and the Propensity to Plan," Quarterly Journal of Economics, 118(3), pp. 1007-1047.
- Barrow, Lisa, and Leslie McGranahan, 2000, "The Effects of Earned Income Credit on the Seasonality of Household Expenditures," National Tax Journal, December, 53, pp. 1211-44.
- Bernheim, B. Douglas, Jonathan Skinner, and Steven Weinberg. 2001. "What Accounts for the Variation in Retirement Wealth among U.S. Households?" American Economic Review, 91(4), pp. 832-857.
- Bertrand, Marianne, and Morse, Adair, 2009, "What do High-Interest Borrowers Do with their Tax Rebate?" American Economic Review, 99(2) May, pp. 418-23.
- Broda, Christian, and Parker, Jonathan, 2014, "The Economic Stimulus Payments of 2008 and the Aggregate Demand for Consumption," NBER working paper 20122, May.
- Broda, C. and D. Weinstein, (2008) "Product Creation and Destruction: Evidence and Price Implications," Forthcoming *American Economic Review*.
- Browning, Martin and Lusardi, Annamaria, 1996, "Household Saving: Micro Theories and Macro Facts," Journal of Economic Literature, 34(4), pp 1797-1855.
- Bureau of Labor Statistics, U.S. Department of Labor, 2009, "Consumer Expenditure Survey Results on the 2008 Economic Stimulus Payments (Tax Rebates)," October, (<http://www.bls.gov/cex/taxrebate.htm>).
- Caballero, Ricardo J., 1995, "Near Rationality, Heterogeneity, and Aggregate Consumption," Journal of Money, Credit, and Banking, February, 27(1), pp. 29-48.

- Campbell, John Y., and N. Gregory Mankiw, 1989, "Consumption, Income, and Interest Rates: Reinterpreting the Time Series Evidence," in Olivier Jean Blanchard and Stanley Fischer eds. NBER Macroeconomics Annual, 4, MIT Press, pp 185-246.
- Carroll, Christopher D., 1997, "Buffer-Stock Saving and the Life Cycle/Permanent Income Hypothesis," The Quarterly Journal of Economics, February, 112(1), pp. 1-55.
- Coronado, Julia Lynn, Lupton, Joseph P., and Sheiner, Louise M., 2006, "The Household Spending Response to the 2003 Tax Cut: Evidence from Survey Data," working paper.
- Deaton, Angus, 1991, "Saving and Liquidity Constraints," Econometrica, 59(5), pp. 1221-1248
- Deaton, Angus, 1992, Understanding Consumption, Oxford: Clarendon Press.
- Grinblatt, Mark, Matti Keloharju, and Juhani Linnainmaa, 2010, "IQ and Stock Market Participation," Journal of Finance, forthcoming
- Gross, Tal, Matthew J. Notowidigdo, and Jialan Wang, 2012, "Liquidity Constraints and Consumer Bankruptcy: Evidence from Tax Rebates," NBER Working paper 17087, February.
- Gul, Faruk and Wolfgang Pesendorfer, 2004a, "Self-Control and the Theory of Consumption", Econometrica, 72(1), January, pp.119-158.
- Gul, Faruk and Wolfgang Pesendorfer, 2004b, "Self Control, Revealed Preference and Consumption Choice," Review of Economic Dynamics, 7, pp. 243-264.
- Hsieh, Chang-Tai, 2003, "Do Consumers React to Anticipated Income Changes? Evidence from the Alaska Permanent Fund," American Economic Review, 99, pp. 397-405.
- Hurst, Eric, 2003, "Grasshoppers, Ants and Pre-Retirement Wealth: A Test of Permanent Income," NBER Working Paper 10098, November.
- Jappelli, Tullio, 1990, "Who is Credit Constrained in the U.S. Economy?" Quarterly Journal of Economics, February, 105, pp. 219-234.
- Jappelli, Tullio and Luigi Pistaferri, 2010, "The Consumption Response to Income Changes," NBER Working Paper 15739, February.
- Jappelli, Tullio, Pischke, Jörn-Steffen, and Souleles, Nicholas S., 1998, "Testing for Liquidity Constraints in Euler Equations with Complementary Data Sources," The Review of Economics and Statistics, 80, pp. 251-262.

- Johnson, David S., Parker, Jonathan A., and Souleles, Nicholas S., 2006, "Household Expenditure and the Income Tax Rebates of 2001," American Economic Review, 96, pp. 1589-1610.
- Johnson, David S., Parker, Jonathan A., and Souleles, Nicholas S., 2009, "The Response of Consumer Spending to Rebates During an Expansion: Evidence from the 2003 Child Tax Credit," working paper, April.
- Greg Kaplan, Violante, Giovanni L., 2014, "A Model of the Consumption Response to Fiscal Stimulus Payments," Econometrica, July 82(4), pp. 1199-1239.
- Krusell, Per and Anthony A. Smith, Jr., 1998, "Income and Wealth Heterogeneity in the Macroeconomy," Journal of Political Economy, 106(5), pp. 867-96.
- Laibson, David, Angeletos, George-Marios, Repetto, Andrea, Tobacman, Jeremy, and Iinberg, Stephen, 2001, "The Hyperbolic Consumption Model: Calibration, Simulation, and Empirical Evaluation," Journal of Economic Perspectives, 15(3), pp. 47-68.
- Ludvigson, Sydney, 1999, "Consumption and Credit: A Model of Time-Varying Liquidity Constraints," The Review of Economics and Statistics, August, 81(3): 434-447.
- Lusardi, Annamaria, "Information, Expectations, and Savings," Ch. 3 in Behavioral Dimensions of Retirement Economics, Henry Aaron, ed. (New York: Brookings Institution/Russell Sage Foundation, 1999), pp. 81-115
- Lusardi, Annamaria, and Olivia Mitchell, 2007, "Baby Boomer Retirement Security: The Roles of Planning, Financial Literacy, and Housing Wealth," Journal of Monetary Economics, 54(1), January, 205-224.
- Parker, Jonathan A., 1999, "The Reaction of Household Consumption to Predictable Changes in Social Security Taxes," American Economic Review, September, 89(4), pp. 959-973.
- Parker, Jonathan A., Nicholas S. Souleles, David S. Johnson, and Robert McClelland, 2013, "Consumer Spending and the Economic Stimulus Payments of 2008," American Economic Review, 103(6), October, 2530-53.
- Reis, Ricardo, 2006, "Inattentive Consumers," Journal of Monetary Economics, 53(8) 1761-80.
- Sahm, Claudia R., Shapiro, Matthew D. and Slemrod, Joel B., 2010, "Household Response to the 2008 Tax Rebates: Survey Evidence and Aggregate Implications," Tax Policy and the Economy, 24, 69-110.

- Shapiro, Matthew D., and Slemrod, Joel B., 1995, "Consumer Response to the Timing of Income: Evidence from a Change in Tax Withholding," American Economic Review, March, 85, pp. 274-283.
- Shapiro, Matthew D. and Slemrod, Joel B., 2003a, "Consumer Response to Tax Rebates," American Economic Review, 85, pp. 274-283.
- Shapiro, Matthew D., and Slemrod, Joel B., 2003b, "Did the 2001 Tax Rebate Stimulate Spending? Evidence from Taxpayer Surveys," Tax Policy and the Economy, 17, Cambridge: MIT Press.
- Sims, Christopher A., 2003, "Implications of Rational Inattention," Journal of Monetary Economics, April, 50(3), pp. 665-690.
- Slemrod, Joel B., Christian, Charles, London, Rebecca, and Parker, Jonathan A., 1997, "April 15 Syndrome," Economic Inquiry, October, 35(4), pp. 695-709.
- Souleles, Nicholas S., 1999, "The Response of Household Consumption to Income Tax Refunds," American Economic Review, September, 89(4), pp. 947-958.
- Stephens, Melvin, Jr., 2003, "3rd of tha Month: Do Social Security Recipients Smooth Consumption Between Checks?" American Economic Review, 93(1), 406-422.
- Stephens, Melvin, Jr., 2006, "Paycheck Receipt and the Timing of Consumption," The Economic Journal, 116(513), 680-701.
- Department of the Treasury, "Daily Treasury Statement," Washington: GPO, 2008, various issues.
- Zeldes, Stephen P., 1989a, "Optimal Consumption with Stochastic Income: Deviations from Certainty Equivalence," Quarterly Journal of Economics, 104(2), pp. 275-298.
- Zeldes, Stephen P., 1989b, "Consumption and Liquidity Constraints: An Empirical Investigation," Journal of Political Economy, 97(2), 305-346.

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

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

Table 2: Sample summary statistics

Sample:	Static reporting sample		Static reporting sample with only ESPs by mail		Static sample with only ESPs by direct deposit	
	Mean	std dev	Mean	std dev	Mean	std dev
<u>Observations</u>						
Number of observations	1,131,520		593,684		534,196	
<i>Spending</i>	149.3	185.0	141.3	177.8	157.2	191.4
<i>Spending / Spending > 0</i>	178.7	188.9	166.6	181.8	190.8	195.1
<i>ESP amount</i>	17.3	142.9	15.4	128.6	19.1	155.8
<i>I(ESP amount > 0)</i>	0.019	0.137	0.019	0.137	0.019	0.137
<i>ESP amount / amount > 0</i>	897.9	521.4	800.7	480.6	993.8	541.5
<u>Households</u>						
Number of households	21,760		11,417		10,273	
<i>I(2007 Income < 20,000)</i>	0.15	0.36	0.20	0.40	0.10	0.30
<i>I(20,000 ≤ Income < 50,000)</i>	0.37	0.48	0.40	0.49	0.35	0.48
<i>I(2007 Income ≥ 100,000)</i>	0.13	0.33	0.11	0.31	0.15	0.35
<i>Household size</i>	2.6	1.5	2.4	1.4	2.8	1.5
<i>I(Number children > 0)</i>	0.38	0.49	0.30	0.46	0.46	0.50
<i>I(Children under 6 > 0)</i>	0.15	0.36	0.11	0.31	0.20	0.40

Notes: Each sample includes only households that meet the standard NCP static reporting requirement for the year and report both receipt during the period of the experimental variation and sufficient ESP information for that variable and sample. All samples statistics are weighted by the NCP projection factor for 2008. Calculated based on data from The Nielsen Company (US) LLC and provided by the Marketing Data Center at the University of Chicago Booth School of Business.

Table 3: Estimated spending responses to receipt of payment

Regression Specification: (Interpretation)	Using all variation in time of receipt			Using only variation in timing within each method of receipt		
	Dollars	Spending as pct of	Dollars spent	Dollars	Spending as pct of	Dollars spent
	spent on indicator of ESP (\$ spent)	2008Q1 spending on indicator of ESP (% chg in spending)	on average ESP/100 (MPC, in %)	spent on indicator of ESP (\$ spent)	2008Q1 spending on indicator of ESP (% chg in spending)	on average ESP/100 (MPC, in %)
Two weeks before	-1.48 (1.83)	-2.74 (1.58)	-0.16 (0.20)	-0.64 (2.00)	-1.75 (1.73)	-0.02 (0.23)
Week before	-0.65 (1.88)	-2.16 (1.66)	-0.07 (0.21)	-0.34 (2.17)	-0.86 (1.92)	0.01 (0.25)
Contemporaneous week	13.42 (2.21)	9.03 (1.84)	1.49 (0.25)	12.50 (2.44)	9.86 (2.11)	1.45 (0.29)
First week after	12.17 (2.15)	7.89 (1.91)	1.36 (0.24)	9.93 (2.61)	8.02 (2.40)	1.20 (0.31)
Second week after	4.32 (2.16)	0.92 (2.02)	0.46 (0.24)	2.53 (2.74)	1.14 (2.55)	0.35 (0.32)
Third week after	3.33 (2.28)	1.02 (2.14)	0.39 (0.26)	1.70 (3.04)	1.47 (2.73)	0.32 (0.36)
Fourth week after	1.09 (2.31)	-0.51 (2.17)	0.14 (0.26)	0.09 (3.28)	0.16 (2.85)	0.12 (0.39)
Fifth week after	-1.07 (2.41)	-1.51 (2.46)	-0.08 (0.27)	-1.44 (3.57)	-0.76 (3.22)	-0.06 (0.42)
Sixth week after	-1.98 (2.56)	-0.28 (2.59)	-0.22 (0.29)	-1.74 (3.84)	0.69 (3.72)	-0.13 (0.46)
Four week cumulative or avg. percent increase	33.23 (6.27)	4.71 (1.54)	3.70 (0.70)	26.66 (8.69)	5.12 (2.07)	3.31 (1.04)
Seven week cumulative or avg. percent increase	31.27 (11.51)	2.36 (1.70)	3.53 (1.29)	23.56 (17.64)	2.94 (2.39)	3.24 (2.11)
Number of households	21,760	21,540	21,386	21,690	21,470	21,320

Notes: The regressions in the first panel include fixed effects for each month in the sample and in the second include fixed effects for each month for each means of receipt. All regressions also include household fixed effects and are weighted by the NCP projection factor for 2008. Each sample includes only households that report sufficient ESP information for that specification and receipt during the period of the experimental variation, and meet the standard NCP static reporting requirement for the year. Calculated based on data from The Nielsen Company (US) LLC and provided by the Marketing Data Center at the University of Chicago Booth School of Business.

Table 4: Spending responses by household liquidity

<i>At least two months available income in liquid wealth?</i>	Using all variation in time of receipt		Using only variation in timing within each method of receipt	
	Yes	No	Yes	No
Contemporaneous week	0.63 (0.29)	2.78 (0.39)	0.66 (0.30)	2.53 (0.42)
<i>t-stat of difference</i>	4.47		3.64	
Four week cumulative increase	2.04 (0.74)	6.57 (1.01)	2.08 (1.03)	4.87 (1.36)
<i>t-stat of difference</i>	3.63		1.64	
Number of households	13,685	7,656	13,654	7,621

Notes: Table reports the propensity to consume NCP goods out of an ESP in percent. The regressions in the first triplet of columns include fixed effects for each month in the sample and in the second triplet of columns include fixed effects for each month for each means of receipt. All regressions include household fixed effects and are weighted by the NCP projection factor for 2008. Each sample includes only households that report receipt during the period of the experimental variation, report sufficient ESP information for that specification, and meet the standard NCP static reporting requirement for the year. Calculated based on data from The Nielsen Company (US) LLC and provided by the Marketing Data Center at the University of Chicago Booth School of Business.

Table 5: Spending responses by income growth and income group

	Using all variation in time of receipt			Using only variation in timing within each method of receipt		
	To lower category	Same category	To higher category	To lower category	Same category	To higher category
<i>Panels A and B: Income growth</i>						
<u>Panel A: Income growth 2007 to 2008</u>						
Contemporaneous week	1.35 (0.57)	0.99 (0.39)	1.70 (0.61)	0.85 (0.64)	0.90 (0.42)	1.29 (0.67)
Four week cumulative increase	4.18 (1.55)	2.45 (1.05)	4.57 (1.61)	1.35 (2.06)	1.84 (1.46)	1.80 (2.11)
Number of households	3,416	7,719	3,051	3,405	7,696	3,042
<u>Panel B: Income growth 2006 to 2007</u>						
Contemporaneous week	1.19 (0.65)	1.28 (0.32)	1.94 (0.61)	0.88 (0.64)	1.10 (0.35)	1.89 (0.65)
Four week cumulative increase	2.20 (1.61)	4.01 (0.86)	5.19 (1.57)	0.12 (2.08)	3.28 (1.19)	4.23 (2.26)
Number of households	3,142	10,051	4,055	3,133	10,023	4,042
<i>Panels C, D and E: Income levels</i>						
	income < \$35,000	\$35,000 ≤ income < \$70,000	\$70,000 ≤ income	income < \$35,000	\$35,000 ≤ income < \$70,000	\$70,000 ≤ income
<u>Panel C: 2008 Income</u>						
Contemporaneous week	2.46 (0.58)	1.40 (0.45)	0.21 (0.49)	2.06 (0.64)	0.87 (0.49)	0.39 (0.50)
Four week cumulative increase	3.78 (1.64)	4.01 (1.16)	2.20 (1.33)	2.68 (2.02)	1.07 (1.46)	1.98 (1.95)
Number of households	5,057	5,303	3,826	5,035	5,289	3,819
<u>Panel D: 2007 Income</u>						
Contemporaneous week	2.56 (0.55)	1.44 (0.40)	0.71 (0.44)	2.39 (0.57)	1.11 (0.44)	0.65 (0.46)
Four week cumulative increase	5.35 (1.44)	3.97 (1.05)	3.17 (1.19)	4.85 (1.81)	3.02 (1.44)	1.73 (1.71)
Number of households	6,067	6,398	4,783	6,049	6,377	4,772
<u>Panel E: 2006 Income</u>						
Contemporaneous week	3.13 (0.57)	1.41 (0.34)	0.56 (0.37)	3.09 (0.59)	1.15 (0.37)	0.59 (0.39)
Four week cumulative increase	6.99 (1.33)	3.44 (0.90)	1.99 (1.02)	8.13 (1.73)	2.16 (1.20)	1.10 (1.42)
Number of households	7,495	7,783	6,063	7,466	7,761	6,048

Notes: The table reports the propensity to consume NCP goods out of an ESP in percent. The regressions in the first triplet of columns include fixed effects for each month in the sample and in the second triplet of columns include fixed effects for each month for each means of receipt. All regressions include household fixed effects and are weighted by the NCP projection factor for 2008. Each sample includes only households that report sufficient ESP information for that specification, report receipt during the period of the experimental variation, and meet the standard NCP static reporting requirement for the year. Calculated based on data from The Nielsen Company (US) LLC and provided by the Marketing Data Center at the University of Chicago Booth School of Business.

Table 6: Spending responses by liquidity and income level

	Using all variation in time of receipt			Using only variation in timing within each method of receipt		
	income < \$35,000	\$35,000 ≤ income <\$70,000	\$70,000 ≤ income	income < \$35,000	\$35,000 ≤ income <\$70,000	\$70,000 ≤ income
	<u>Panel A: Households with sufficient liquid wealth, by 2008 income</u>					
Contemporaneous week	0.91 (0.76)	0.62 (0.50)	-0.06 (0.55)	1.08 (0.82)	-0.03 (0.53)	0.17 (0.56)
Four week cumulative increase	-0.17 1.92	3.55 (1.44)	0.41 (1.51)	0.18 (2.51)	0.89 (1.76)	0.66 (2.26)
Number of households	3,068	3,762	2,964	3,055	3,754	2,962
<u>Panel B: Households with low liquid wealth, by 2008 income</u>						
Contemporaneous week	4.23 (0.90)	2.61 (0.84)	1.21 (1.03)	3.26 (1.01)	2.23 (0.95)	1.20 (1.08)
Four week cumulative increase	8.41 (2.76)	4.74 (1.99)	8.31 (2.86)	5.77 (3.29)	1.10 (2.56)	6.44 (3.66)
Number of households	1,989	1,541	862	1,980	1,535	857
<u>Panel C: Households with sufficient liquid wealth, by 2006 income</u>						
Contemporaneous week	2.33 (0.85)	0.44 (0.41)	0.06 (0.42)	2.46 (0.85)	0.24 (0.43)	0.21 (0.43)
Four week cumulative increase	3.79 (1.70)	2.27 (1.16)	1.04 (1.19)	5.45 (2.33)	1.97 (1.61)	0.79 (1.60)
Number of households	4,147	5,047	4,491	4,134	5,036	4,484
<u>Panel D: Households with low liquid wealth, by 2006 income</u>						
Contemporaneous week	3.79 (0.76)	2.60 (0.56)	1.81 (0.75)	3.69 (0.81)	2.23 (0.61)	1.52 (0.81)
Four week cumulative increase	9.91 (2.01)	5.02 (1.40)	4.45 (2.01)	10.40 (2.53)	2.22 (1.79)	1.53 (3.06)
Number of households	3,348	2,736	1,572	3,332	2,725	1,564

Notes: The table reports the propensity to consume NCP goods out of an ESP in percent. The regressions in the first triplet of columns includes fixed effects for each month in the sample and in the second triplet includes fixed effects for each month for each means of receipt. All regressions include household fixed effects and are weighted by the NCP projection factor for 2008. Each sample includes only households that report sufficient ESP information for that specification, report receipt during the period of the experimental variation, and meet the standard NCP static reporting requirement for the year. Calculated based on data from The Nielsen Company (US) LLC and provided by the Marketing Data Center at the University of Chicago Booth School of Business.

Table 7: Spending responses by household expectations and liquidity

<i>Was this about the amount your household was expecting?</i>	Using all variation in time of receipt			Using only variation in timing within each method of receipt			
	Yes, known since Feb, March, or Apr.	Known more recently, surprised to get any, or more than expecting		Yes, known since Feb, March, or Apr.	Known more recently, surprised to get any, or more than expecting		No, less than expecting
		No, less than expecting	No, less than expecting				
<i>Panel A: All households</i>							
Contemporaneous week	1.19 (0.25)	2.37 (0.83)	2.80 (0.73)	1.30 (0.27)	1.68 (0.81)	2.00 (0.81)	
Four week cumulative	3.31 (0.67)	5.13 (1.74)	6.69 (2.20)	3.42 (0.93)	1.91 (2.03)	4.52 (3.10)	
Number of households	15,991	2,525	2,693	15,956	2,505	2,685	
<i>Panel B: Households with sufficient liquid wealth</i>							
Contemporaneous week	0.39 (0.29)	1.07 (1.18)	2.08 (1.13)	0.59 (0.31)	0.43 (1.15)	1.35 (1.24)	
Four week cumulative	1.69 (0.82)	2.61 (2.08)	4.68 (3.00)	2.56 (1.13)	-0.58 (2.58)	1.99 (4.94)	
Number of households	10,603	1,658	1,317	10,586	1,649	1,314	
<i>Panel C: Households with low liquid wealth</i>							
Contemporaneous week	2.41 (0.46)	4.26 (1.06)	3.41 (0.96)	2.39 (0.50)	3.47 (1.09)	2.55 (1.07)	
Four week cumulative	5.89 (1.12)	8.89 (3.03)	8.74 (3.15)	4.51 (1.58)	5.58 (3.32)	6.89 (3.99)	
Number of households	5,388	867	1,376	5,370	856	1,371	

Notes: The table reports the propensity to consume NCP goods out of an ESP in percent. The regressions in the first triplet of columns include fixed effects for each month in the sample and in the second triplet include fixed effects for each month for each means of receipt. All regressions include household fixed effects and are weighted by the NCP projection factor for 2008. Each sample includes only households that report receipt during the period of the experimental variation, report sufficient ESP information for that specification, and meet the standard NCP static reporting requirement for the year. Calculated based on data from The Nielsen Company (US) LLC and provided by the Marketing Data Center at the University of Chicago Booth School of Business.

Table 8: Spending responses by liquidity and the propensity to plan financially

<i>Formulated a financial plan for long term future?</i>	Using all variation in time of receipt		Using only variation in timing within each method of receipt	
	Yes	No	Yes	No
<i>Panel A: All households</i>				
Contemporaneous week	0.73 (0.32)	2.25 (0.34)	0.83 (0.33)	2.01 (0.36)
<i>t-statistic of difference</i>	3.29		2.42	
Four week cumulative	1.81 (0.78)	5.76 (0.90)	1.72 (1.14)	4.71 (1.18)
<i>t-statistic of difference</i>	3.33		1.82	
Number of households	10,936	10,405	10,902	10,373
<i>t-statistic of difference</i>	<i>Panel B: Households with sufficient liquid wealth</i>			
Contemporaneous week	0.27 (0.37)	1.19 (0.46)	0.38 (0.37)	1.11 (0.49)
<i>t-statistic of difference</i>	1.58		1.19	
Four week cumulative	1.35 (0.90)	3.15 (1.25)	1.41 (1.32)	3.13 (1.64)
<i>t-statistic of difference</i>	1.17		0.82	
Number of households	8,598	5,087	8,578	5,076
<i>t-statistic of difference</i>	<i>Panel C: Households with low liquid wealth</i>			
Contemporaneous week	2.03 (0.65)	3.10 (0.48)	2.06 (0.70)	2.72 (0.52)
<i>t-statistic of difference</i>	1.33		0.76	
Four week cumulative	3.38 (1.60)	7.89 (1.26)	2.44 (2.24)	5.82 (1.68)
<i>t-statistic of difference</i>	2.21		1.21	
Number of households	2,338	5,318	2,324	5,297

Notes: The table reports the propensity to consume NCP goods out of an ESP in percent. The regressions in the first triplet of columns include fixed effects for each month in the sample and in the second triplet include fixed effects for each month for each means of receipt. All regressions include household fixed effects and are weighted by the NCP projection factor for 2008. Each sample includes only households that report receipt during the period of the experimental variation, sufficient ESP information for that specification, and meet the standard NCP static reporting requirement for the year. Calculated based on data from The Nielsen Company (US) LLC and provided by the Marketing Data Center at the University of Chicago Booth School of Business.

Table 9: Spending responses by liquidity and the propensity to plan vacations

<i>Before going on vacation, how much time planning?</i>	Using only variation in timing within each method of receipt					
	Using all variation in time of receipt			Using only variation in timing within each method of receipt		
	A great deal of time	Quite a bit of time	A little time or Almost no time	A great deal of time	Quite a bit of time	A little time or Almost no time
	<i>Panel A: All households</i>					
Contemporaneous week	0.66 (0.49)	1.57 (0.38)	1.87 (0.48)	0.50 (0.50)	1.49 (0.41)	1.79 (0.48)
<i>t-statistic of 'A gret deal...'='A little...'</i>		1.76			1.86	
Four week cumulative	2.90 (1.21)	3.24 (1.02)	3.98 (1.19)	2.00 (1.63)	3.05 (1.51)	3.01 (1.50)
<i>t-statistic of 'A gret deal...'='A little...'</i>		0.64			0.09	
Number of households	4,065	7,818	5,602	4,053	7,796	5,583
	<i>Panel B: Households with sufficient liquid wealth</i>					
Contemporaneous week	0.00 (0.57)	0.66 (0.44)	1.03 (0.64)	0.01 (0.59)	0.79 (0.47)	0.99 (0.60)
<i>t-statistic of 'A gret deal...'='A little...'</i>		1.20			1.17	
Four week cumulative	2.08 (1.52)	1.40 (1.26)	2.12 (1.38)	1.63 (2.03)	2.24 (1.92)	1.65 (1.69)
<i>t-statistic of 'A gret deal...'='A little...'</i>		0.02			0.15	
Number of households	2,630	5,285	3,771	2,625	5,274	3,761
	<i>Panel C: Households with low liquid wealth</i>					
Contemporaneous week	1.63 (0.84)	2.99 (0.68)	3.23 (0.73)	1.21 (0.87)	2.55 (0.74)	3.08 (0.79)
<i>t-statistic of 'A gret deal...'='A little...'</i>		1.44			1.59	
Four week cumulative	4.44 (1.98)	6.49 (1.70)	6.96 (2.20)	2.56 (2.70)	4.19 (2.37)	5.09 (2.88)
<i>t-statistic of 'A gret deal...'='A little...'</i>		0.85			0.02	
Number of households	1,435	2,533	1,831	1,428	2,522	1,822

Notes: The table reports the propensity to consume NCP goods out of an ESP in percent. The regressions in the first triplet of column include fixed effects for each month in the sample and in the second triplet include fixed effects for each month for each means of receipt. All regressions include household fixed effects and are weighted by the NCP projection factor for 2008. Each sample includes only households that report receipt during the period of the experimental variation, sufficient ESP information for that specification, and meet the standard NCP static reporting requirement for the year. Calculated based on data from The Nielsen Company (US) LLC and provided by the Marketing Data Center at the University of Chicago Booth School of Business.

Table 10: Spending response by liquidity and optimization: coupon use

<i>Share of purchases made using coupons or deals</i>	Using all variation in time of receipt		Using only variation in timing within each method of receipt	
	Low	High	Low	High
<i>Panel A: All households</i>				
Contemporaneous week	2.08 (0.34)	0.84 (0.31)	2.14 (0.36)	0.55 (0.32)
<i>t-stat of difference</i>		2.71		3.30
Four week cumulative increase	4.61 (0.83)	2.96 (0.85)	4.39 (1.18)	1.81 (1.12)
<i>t-stat of difference</i>		1.38		1.59
Number of households	10,666	10,663	10,631	10,632
<i>Panel B: Households with sufficient liquid wealth</i>				
Contemporaneous week	0.85 (0.44)	0.43 (0.37)	1.03 (0.45)	0.27 (0.38)
<i>t-stat of difference</i>		0.72		1.29
Four week cumulative increase	2.39 (1.08)	1.71 (0.99)	3.02 (1.59)	1.05 (1.29)
<i>t-stat of difference</i>		0.46		0.96
Number of households	6,172	7,506	6,156	7,491
<i>Panel C: Households with low liquid wealth</i>				
Contemporaneous week	3.49 (0.53)	1.57 (0.54)	3.39 (0.57)	1.07 (0.59)
<i>t-stat of difference</i>		2.55		2.83
Four week cumulative increase	7.24 (1.30)	5.34 (1.60)	5.81 (1.75)	3.31 (2.16)
<i>t-stat of difference</i>		0.92		0.90
Number of households	4,494	3,157	4,475	3,141

Notes: The table reports the propensity to consume NCP goods out of an ESP in percent. The regressions in the first triplet of columns include fixed effects for each month in the sample and in the second triplet include fixed effects for each month for each means of receipt. All regressions include household fixed effects and are weighted by the NCP projection factor for 2008. Each sample includes only households that report receipt during the period of the experimental variation, sufficient ESP information for that specification, and meet the standard NCP static reporting requirement for the year. Calculated based on data from The Nielsen Company (US) LLC and provided by the Marketing Data Center at the University of Chicago Booth School of Business.

Table 11: Spending responses by liquidity and grasshoppers and ants

<i>The sort of people who spend or save?</i>	Using all variation in time of receipt		Using only variation in timing within each method of receipt		N
	Spend now	Save for future	Spend now	Save for future	
<i>Panel A: All households</i>					
Contemporaneous week	2.37 (0.39)	0.92 (0.28)	2.19 (0.43)	0.92 (0.29)	
<i>t-stat of difference</i>		3.00		2.46	
Four week cumulative increase	5.14 (1.05)	2.96 (0.71)	4.09 (1.40)	2.71 (1.00)	
<i>t-stat of difference</i>		1.72		0.80	
Number of households	7,881	13,460	7,852	13,423	
<i>Panel B: Households with sufficient liquid wealth</i>					
Contemporaneous week	0.90 (0.55)	0.52 (0.33)	0.85 (0.59)	0.58 (0.34)	
<i>t-stat of difference</i>		0.59		0.39	
Four week cumulative increase	2.53 (1.60)	1.84 (0.81)	1.65 (2.20)	2.25 (1.14)	
<i>t-stat of difference</i>		0.38		0.24	
Number of households	3,528	10,157	3,522	10,132	
<i>Panel C: Households with low liquid wealth</i>					
Contemporaneous week	3.36 (0.54)	1.92 (0.54)	3.05 (0.59)	1.74 (0.57)	
<i>t-stat of difference</i>		1.90		1.60	
Four week cumulative increase	7.01 (1.39)	5.92 (1.43)	5.60 (1.82)	3.64 (2.03)	
<i>t-stat of difference</i>		0.55		0.72	
Number of households	4,353	3,303	4,330	3,291	

Notes: The table reports the propensity to consume NCP goods out of an ESP in percent. The regressions in the first triplet of columns include fixed effects for each month in the sample and in the second triplet include fixed effects for each month for each means of receipt. All regressions include household fixed effects and are weighted by the NCP projection factor for 2008. Each sample includes only households that report receipt during the period of the experimental variation, sufficient ESP information for that specification, and meet the standard NCP static reporting requirement for the year. Calculated based on data from The Nielsen Company (US) LLC and provided by the Marketing Data Center at the University of Chicago Booth School of Business.

Table 12: Spending responses by liquidity and self control: regret of purchases

<i>About how often do you or other household members make purchases that you later regret?</i>	Using all variation in time of receipt			Using only variation in timing within each method of receipt		
	Often	Often or Occasionally	Rarely or Never	Often	Often or Occasionally	Rarely or Never
	<i>Panel A: All households</i>					
Contemporaneous week	2.80 (1.64)	1.10 (0.34)	1.81 (0.31)	3.64 (1.99)	0.92 (0.38)	1.80 (0.33)
<i>t-statistic of difference</i>			1.53			1.77
Four week cumulative	7.54 (4.83)	3.61 (0.96)	4.04 (0.76)	7.44 (7.06)	2.83 (1.29)	3.61 (1.07)
<i>t-statistic of difference</i>			0.35			0.47
Number of households	481	8,426	12,915	479	8,399	12,876
<i>Panel B: Households with sufficient liquid wealth</i>						
Contemporaneous week	1.27 (2.77)	0.27 (0.45)	0.83 (0.37)	2.77 (3.25)	0.10 (0.49)	0.97 (0.37)
<i>t-statistic of difference</i>			0.96			1.42
Four week cumulative	10.80 (8.54)	3.29 (1.27)	1.37 (0.90)	9.81 (12.40)	2.64 (1.72)	1.78 (1.28)
<i>t-statistic of difference</i>			1.23			0.40
Number of households	221	4,927	8,758	221	4,917	8,737
<i>Panel C: Households with low liquid wealth</i>						
Contemporaneous week	4.17 (2.03)	2.02 (0.52)	3.49 (0.57)	4.40 (2.61)	1.77 (0.57)	3.24 (0.62)
<i>t-statistic of difference</i>			1.91			1.75
Four week cumulative	7.20 (5.82)	4.32 (1.48)	8.68 (1.38)	6.98 (8.15)	2.93 (1.97)	6.65 (1.89)
<i>t-statistic of difference</i>			2.15			1.36
Number of households	260	3,499	4,157	258	3,482	4,139

Notes: The table reports the propensity to consume NCP goods out of an ESP in percent. The regressions in the first triplet of columns include fixed effects for each month in the sample and in the second triplet include fixed effects for each month for each means of receipt. All regressions include household fixed effects and are weighted by the NCP projection factor for 2008. Each sample includes only households that report receipt during the period of the experimental variation, sufficient ESP information for that specification, and meet the standard NCP static reporting requirement for the year. Calculated based on data from The Nielsen Company (US) LLC and provided by the Marketing Data Center at the University of Chicago Booth School of Business.

Table 13: Spending responses by liquidity and self control: survey response time

<i>Speed of response to survey in days</i>	Using all variation in time of receipt			Using only variation in timing within each method of receipt		
	Slow	Medium	Fast	Slow	Medium	Fast
	<i>Panel A: All households</i>					
Contemporaneous week	1.19 (0.46)	1.74 (0.31)	1.15 (0.58)	1.16 (0.49)	1.72 (0.32)	0.75 (0.63)
Four week cumulative	2.52 (1.12)	4.55 (0.81)	2.89 (1.58)	2.63 (1.58)	4.03 (1.05)	0.99 (2.37)
Number of households	6,268	11,826	3,239	6,245	11,796	3,226
	<i>Panel B: Households with sufficient liquid wealth</i>					
Contemporaneous week	0.40 (0.55)	0.90 (0.39)	-0.24 (0.66)	0.48 (0.54)	1.02 (0.40)	-0.62 (0.76)
Four week cumulative	-0.55 (1.34)	3.19 (0.98)	0.91 (2.01)	-0.06 (1.87)	3.72 (1.32)	-0.89 (3.13)
Number of households	4,117	7,438	2,123	4,105	7,425	2,117
	<i>Panel C: Households with low liquid wealth</i>					
Contemporaneous week	2.34 (0.79)	2.88 (0.49)	3.29 (1.03)	2.16 (0.91)	2.61 (0.52)	2.92 (1.03)
Four week cumulative	6.93 (1.92)	6.62 (1.37)	6.01 (2.52)	6.66 (2.76)	4.15 (1.73)	3.48 (3.46)
Number of households	2,151	4,388	1,116	2,140	4,371	1,109

Notes: The table reports the propensity to consume NCP goods out of an ESP in percent. The regressions in the first triplet of columns include fixed effects for each month in the sample and in the second triplet include fixed effects for each month for each means of receipt. All regressions include household fixed effects and are weighted by the NCP projection factor for 2008. Each sample includes only households that report receipt during the period of the experimental variation, sufficient ESP information for that specification, and meet the standard NCP static reporting requirement for the year. Calculated based on data from The Nielsen Company (US) LLC and provided by the Marketing Data Center at the University of Chicago Booth School of Business.