Household Expenditure and the Income Tax Rebates of 2001

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Abstract

Under the Economic Growth and Tax Relief Reconciliation Act of 2001, most U.S. taxpayers received a tax rebate between July and September, 2001. The week in which the rebate was mailed was based on the second-to-last digit of the taxpayer's Social Security number, a digit that is effectively randomly assigned. Using special questions about the rebates added to the Consumer Expenditure Survey, we exploit this historically unique experiment to measure the change in consumption expenditures caused by receipt of the rebate and to test the Permanent Income Hypothesis (PIH) and related models. Households spent about 20-40 percent of their rebates on non-durable goods during the three-month period in which the rebate was received, and additional, smaller but still substantial, amounts in the next two quarters after receipt. The implied effects on aggregate consumption demand are significant. The estimated responses are largest for households with relatively low liquid wealth or low income, consistent with liquidity constraints.

Keywords: consumption, saving, Life-Cycle model, Permanent-Income Hypothesis, liquidity constraints; fiscal policy, tax cuts, tax rebates, windfalls.

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I. Introduction

Policymakers often try to use tax policy to reduce the magnitude of economic fluctuations. They cut income taxes in recessions, assuming that household spending rises in response to the resulting increase in take-home pay, thereby reducing the severity of recessions. Academic economists, however, tend to be more skeptical about the use of tax policy to stabilize business cycles, in large part because the canonical theory of the consumer suggests that consumption should not respond much to temporary changes in taxes. Instead, consumption should increase as soon as consumers learn of an upcoming tax cut, rather than waiting to increase only once take-home pay actually rises. Moreover, the resulting increase in spending should be spread out over consumers' entire lifetimes (or forever), which implies only a small change in spending in response to a temporary tax change, such as one-time tax rebate.

This paper exploits unique data and features of the 2001 income tax rebates to estimate the causal effect of these rebates on household expenditure. The Economic Growth and Tax Relief Reconciliation Act of 2001 sent tax rebates, typically $300 or $600 in value, to about 2/3 of U.S. households over a ten-week period from the end of July to September, 2001. Most importantly for our purposes, the timing of the rebate mailing was based on the second-to-last digit of the Social Security number of the tax filer, a digit that is effectively randomly assigned.[1] Such random variation in tax policy is historically unique.

We conduct our analysis using the Consumer Expenditure (CE) Survey, which, among large household surveys in the U.S., contains the most comprehensive measures of households' expenditures. The regular CE data however does not contain sufficient information to study the 2001 tax rebates. The CE survey does not typically record the timing of taxes and transfers nor the Social Security numbers of households' tax filers. But, shortly after the passage of the Tax Act, the authors worked with the staff of the Bureau of Labor Statistics (BLS) and other government agencies to add a special module of questions about the rebates to the CE Survey. This module was included in the survey from shortly after the rebate mailing began until the end of 2001. It asked every household about the timing and amount of each rebate check they received.

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

We estimate the change in household expenditures due to rebate receipt by comparing the expenditures of households who received rebates at different times. The natural experiment provided by the randomized mailing dates allows us to directly identify the causal effect of the rebate. While our results can be interpreted as a test of the Permanent Income Hypothesis (PIH) and related models, the randomization provides identification under much weaker assumptions than required in previous tests of the PIH that use the time-series properties of the consumption Euler equation.

We find that the tax rebates caused economically significant increases in household expenditure. The average household spent about 20-40% of the rebate on nondurable goods during the three-month period in which the rebate was received, depending on the specification. We also find evidence of additional, smaller but still substantial, spending in the next two quarters after receipt, though these lagged effects are estimated with less precision.

To shed light on the reasons behind this increase in spending, we contrast the spending responses across different types of households and different subcategories of nondurable goods. Households with low levels of liquid assets or low income spent more of the rebate than typical, consistent with their facing liquidity constraints. While not statistically significant, point estimates also suggest larger responses among those with high liquid wealth. We also find some evidence that expenditures on apparel and personal care and miscellaneous nondurable expenditures increased by more than their shares in nondurable goods and our estimated aggregate response would imply.

Given that the Treasury distributed 38 billion dollars in tax rebates, our estimates imply that the rebates directly increased aggregate consumption by 0.8 percent in the third quarter of 2001 and 0.6 percent in the fourth quarter of 2001. The ultimate effects of the rebate on the economy also depend on other factors beyond the scope of this paper, such as the extent to which the increased demand for consumption goods caused the relative price of current goods to increase and/or had a multiplier effect.

The paper is organized as follows. The next section relates our paper to the prior literature. The third section describes the relevant tax law changes and the fourth our use of the CE Survey data. The fifth section discusses identification and inference. The sixth section presents our main results on the short-run response to the rebate, while the seventh section

examines the lagged response. The eighth section examines differences in the response across different types of households and consumption goods. A final section discusses the aggregate impact of the rebates and concludes. Appendixes contain additional information about the data.

II. The Literature

Previous tests of consumption smoothing have often had trouble identifying predictable changes in income, and separating the effect of a predictable income change from other factors concurrently impacting the consumption decision (e.g., changes in monetary policy or the stock market). They also usually required the strong assumption that the characteristics of the household that dictate the size or timing of the predictable income change (or rebate) be uncorrelated with other reasons for differential consumption growth rates.[2] By exploiting the randomized tax rebates we avoid these recurrent problems.

Research using aggregate data to measure whether tax cuts increase consumption expenditures has had difficulty distinguishing the effects of the tax cuts themselves from the economic changes that lead to the cuts, as well as other concurrent macroeconomic factors. Due also in part to the limited number of significant changes in tax policy, there is a lack of consensus about the effects of tax rebates and other tax changes on consumption expenditures (see Franco Modigliani and Charles Steindel (1977), Alan Blinder (1981), Alan Blinder and Angus Deaton (1985), James Poterba (1988), and David Wilcox (1990)).[3]

Our paper builds more directly on the literature using micro data to test whether expected changes in household income are correlated with growth in consumption expenditures (see the surveys by Angus Deaton (1992) and Martin Browning and Annamaria Lusardi (1996)).

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[2] For example, in his paper discussed below, Bodkin was aware that his insurance dividend variable might have been picking up the correlation of the dividend with omitted variables in turn correlated with permanent income. On adding such control variables to Bodkin's original regression, Bird and Bodkin (1965) found much smaller consumption responses.

[3] Modigliani and Steindel, Blinder, and Poterba studied the 1975 tax rebate. They found that consumption responded too much to the rebate, though they came to somewhat different conclusions regarding the relative magnitude of the initial versus lagged response. Blinder and Deaton (1985) found smaller consumption responses when they considered jointly the 1975 rebate along with the 1968-70 tax surcharge. Nonetheless they found consumption to be too sensitive to the pre-announced changes in taxes in the later phases of the Reagan tax cuts. They note that their mixed results are "probably not precise enough to persuade anyone to abandon strongly held a priori views".

The seminal studies of Ronald Bodkin (1959) and Mordechai E. Kreinin (1961) examined windfalls like insurance dividends to WWII veterans and German restitution payments. More recently, Nicholas S. Souleles (1999, 2002), Jonathan A. Parker (1999), and Chang-Tai Hsieh (2003) use larger, more representative samples and exploit greater variations in fiscal policy.[4]

Two other papers study the impact of the 2001 tax rebates on household spending. Using innovative questions added to the Michigan Survey of Consumers, Matthew Shapiro and Joel Slemrod (2003) found that only 21.8% of respondents who received (or expected to receive) a rebate report that they will mostly spend their rebate. They calculate that this result is consistent with an average marginal propensity to consume of about one third, very close to the present paper's estimate of the contemporaneous response of expenditures. However, they find no evidence that liquidity constraints play a role in this response and no evidence of a lagged effect on expenditures.[5]

A concurrent paper by Sumit Agarwal, Chunlin Liu, and Nicholas S. Souleles (2003) exploits the random timing of the rebates to identify the dynamic response of credit-card payments, spending, and debt to the rebates. They find that households initially use some of their rebates to increase credit card payments and thereby pay down debt, but soon afterwards credit card spending rises such that debt returns back near its pre-rebate levels. These dynamics of spending are consistent with the dynamics we find in the present paper. The present paper complements Agarwal, Liu and Souleles (2003) by estimating the magnitude of the effect of the rebate on all expenditures, not just credit card spending.[6]

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[4] Souleles (1999) found that consumption responds significantly to the federal income tax refunds that most taxpayers receive each spring. Hsieh found smaller responses by Alaskans to their oil rebates than to their federal income tax refunds. Parker found that household spending responded significantly to changes in take-home pay that occurred for high-income households that hit the Social Security tax cap. Other related studies include Wilcox (1989), Souleles (2000), Browning and Collado (2001), Gross and Souleles (2002), and Stephens (2003), among others.

[5] Of the 78% of respondents who report they will mostly save their rebate, the majority (about three-fifths) report that they will mostly pay down debt (as opposed to accumulate assets). Shapiro and Slemrod (2002, forthcoming) used a novel follow-up survey in 2002 to try to determine whether there was a lagged response to the rebate. They found that, of respondents who said they initially mostly used the rebate to pay down debt, most report that they will "try to keep [down their] lower debt for at least a year." They found similar results for those who report they will save by accumulating assets.

[6] That is, their credit card dataset does not record spending via cash or checks, nor spending on other credit cards held by the account-holders in their data. Also, it does not record whether a card-holder actually received a rebate. Agarwal, Liu and Souleles estimate the timing of rebate receipt based on the card-holder's social security number. However they note that the card-holder might not be the tax filer (whose social security number determined the actual timing of receipt), and that some card-holding households did

III. The 2001 Tax Rebates

The Economic Growth and Tax Relief Reconciliation Act of 2001 made substantial reductions in personal and estate tax rates, initially forecast to reduce revenues by around 10 trillion dollars over ten years. The Act reduced the income tax rate applied to income in the lowest income tax bracket from 15 percent to 10 percent, with this change applied retroactively to income earned from the start of 2001. The tax rebates represented an advance payment of this tax cut. The first income tax bracket applied to the first $6,000 of income for a single individual filing a return, and to the first $12,000 of income for a married couple filing jointly, so that most households were sent rebates of $300 or $600. The Internal Revenue Service (IRS) determined the rebate amounts for each tax filer based on its year 2000 tax return.

In aggregate, the rebates disbursed in 2001 totaled 38 billion dollars, and so represent about 1.5 percent of GDP, and 2.2 percent of aggregate personal consumption expenditures, in the third quarter of 2001. The rebates represented the dominant component (about 84%) of the tax cuts implemented in the first year of the Tax Act. The timing of the remaining, smaller components in 2001 is independent of the randomized timing of the rebates analyzed here. For more details about the Act, see Auerbach (2002), Kiefer et al. (2002), and Shapiro and Slemrod (2003; 2002, forthcoming).

We exploit two key features of the rebate mailing. First, the rebate checks were not mailed all at once, but rather in different weeks that were randomly assigned to households, as described in the introduction. Thus, the date at which a household received a rebate is independent of all household characteristics.[7] Second, in applying the results to the PIH, our

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not receive a rebate at all. Agarwal, Liu and Souleles attribute the slight delay before credit card spending rises to a couple of features particular to credit cards; e.g., the need for constrained card-holders (those whose balances start near their credit limits) to first make payments before they can spend, and to the delay before payments and spending register on the credit card statement.

[7] One possible exception is that a household that filed its year 2000 tax returns late may have been mailed its rebate after the ten-week period ending in September. Since 92 percent of taxpayers typically file at or before the April 15th deadline (see for example Slemrod et al. (1997)), this non-random source of variation is small. We also present results below that exclude rebates received late in 2001.

empirical approach allows the rebates to be treated as pre-announced: Congress passed the Tax Act in May, 2001; and of course expectations of a tax cut arose even earlier.[8, 9]

IV. The Consumer Expenditure Survey

The CE interview survey contains detailed measures of the expenditures of a large, stratified random sample of US households. CE households are interviewed four times, three months apart. In each interview the households report their expenditures during the preceding three months. New households are added to the survey every month so that the data are effectively monthly in frequency. In addition to surveying households about their expenditures, the CE also gathers some information about their demographic characteristics, income, and wealth.

The special module of questions about the 2001 rebates covers the crucial period during which and after the rebates were mailed: the module went into the field in the second week of August, and remained there through the end of December. Households were asked whether they received a rebate, how many rebate checks they received, and then the month and amount of each rebate received. These questions were asked at the end of the interview, after households completed their usual reporting of consumption expenditures and other information. They were written so as to be consistent with the style of other CE questions. Appendix A contains the survey instrument. Appendix B describes how we construct from the raw data the measures used below of the rebates received in each expenditure reference-period. The response rate to the new module was quite good. Only about 3% of the rebate amounts were flagged as invalidly missing (e.g., 'don't knows' or refusals), and only about another 4% of the months-of-receipt were flagged as invalidly missing.

[8] Indeed, tax cuts were a central element of George W. Bush's platform in the 2000 election. Moreover, the Treasury sent taxpayers a letter shortly in advance of the rebate informing them of the size of their upcoming rebate and the particular week in which it would be disbursed: "We are pleased to inform you … you will be receiving a check in the amount of $[amount] during the week of [mm/dd/yy]."

[9] We focus on the behavior of expenditure after rebate receipt. A not-mutually-exclusive alternative empirical approach would be to try to estimate the expected effect of the tax cuts on permanent income. However, such an approach would be much harder to implement, for many reasons. First, it is unclear how large and permanent consumers actually expected the tax cuts to be (Shapiro and Slemrod, 2003). Second, their expectations of tax-cut induced changes in permanent income would have evolved over time, starting at least as early as the 2000 election campaign. (E.g., there is no single point in time at which the tax cut went from being entirely unexpected to being entirely expected.) Third, the evolution of these expectations does not provide randomized cross-sectional variation that we can exploit, unlike the timing of rebate receipt.

We focus on three measures of consumption expenditures. First, we study expenditures on food, which include food consumed away from home, food consumed at home, and purchases of alcoholic beverages. A large amount of previous research has studied expenditures on food, largely because of its availability in the Panel Study of Income Dynamics, but it is a narrow measure of expenditures. Our second and main measure of consumption expenditures is nondurable expenditures (broadly defined). However, the NIPA definition of nondurable goods includes some semi-durables like apparel, so we also consider a more narrow, third definition of consumption expenditures, nondurable goods strictly defined, following Lusardi (1996). In preliminary analysis we also considered a fourth definition, BLS total expenditures, including durable expenditures like auto and truck purchases. However the response of total expenditures to the rebates was never significant. This is not surprising, for two reasons. The rebates are small relative to total expenditures and, more importantly, including the expenditures on durable goods dramatically increased the variability of household expenditures and decreased precision in estimation.[10] Thus, in keeping with previous research, we focus on nondurable expenditures.[11] We drop from our sample households with implausibly low expenditures (the bottom 1% of nondurable expenditures), unusually large changes in age or family size, and uncertain tax rebate status. Appendix B describes these definitions and our sample in more detail.

Our baseline sample uses the 2000 and 2001 waves of the CE survey, with the sample period starting with interviews in January 2001 (when period $t+1$ in equation (2) below covers expenditures in October 2000 to December 2000) and running through interviews in March 2002 (when period $t+1$ covers December 2001 to February of 2002). Where mentioned, we extend this baseline sample period by adding data from the recently released 2002 wave in order to allow for additional lags of the rebate in the analysis.

Table 1 presents summary statistics for our dataset. For each household-quarter, we sum all rebate checks received by the household in that quarter to create our main rebate variable.

[10] Also, the introduction of zero-percent auto financing during our sample period significantly altered expenditures on autos.

[11] Generalizing across specifications, estimates of the effect of the tax rebate on total expenditures are measured with a standard error about four times the size of that on nondurable consumption, and the point estimates often (and implausibly) imply that less money is spent on total expenditures than on nondurable goods.

The average household rebate value conditional on receiving at least one rebate check is $479. Of households receiving rebates, 27 percent report receiving $300 in rebates and 54 percent report receiving $600 in rebates.[12] The three-month reference period (July-September) for households interviewed in October 2001 covers the entire ten-week period during which rebate checks were mailed. Of these households, 57 percent report receiving a rebate during this period. [13]

V. Economic Theory and Identification

The recent literature testing the PIH and related models has typically relied on the time-series properties of the consumption Euler equation. Formally, according to the Euler equation, households equate the marginal utility of consumption today with the expected marginal utility of consumption in the next period:

$$u'(C_{i,t}) = E_t\,[(1-\delta_{i,t})^{-1}(1+r_t)u'(C_{i,t+1})], \qquad (1)$$

where $C_{i,t}$ is nondurable consumption by household i in period t; E_t is the expectations operator; $u'(.)$ is the marginal utility function, decreasing because of diminishing marginal benefits of consumption; $\delta_{i,t}$ is the potentially household-specific discount rate; and $1+r_t$ is the price of consumption in t relative to $t+1$. Starting with Hall (1978), many papers assume a functional form for the utility function or linearize equation *(1)* and exploit the time-series properties of the expectation error to estimate preference parameters and test the Euler equation. Motivated by the alternative hypothesis (that actually predates the null hypothesis) that households to some extent consume income when it arrives, the tests often focus on whether predictable changes in income are statistically significant when included in the Euler equation.

While this approach can estimate the model and test the null hypothesis, it is not suitable for our purposes because it cannot estimate outside of the null hypothesis the causal impact

[12] The household rebate value need not be equal to $300 or $600. Households with 2000 tax liabilities smaller than $300 (or $600) could receive smaller rebates; households with multiple tax-filers could receive multiple checks; taxpayers filing as heads of households could receive a $500 check.

[13] Despite the potential for measurement error, this result is close to estimates of rebate receipt based on (unpublished) Treasury estimates: about 89.5m tax returns received a rebate while 23.5m did not receive a rebate, and about 22.9m households did not file and so also did not receive rebates (Office of Tax Analysis).

of a predictable change in income on consumption growth.[14] Moreover, in the present context, the traditional approach to Euler equation estimation is inappropriate because there is insufficient time-series variation across our sample period to effectively exploit the usual time-series properties of the expectation error.

By contrast, we can directly identify and estimate the impact of the rebate on consumption growth because the randomized rebate receipt is uncorrelated with each household's expectation error and any unobserved heterogeneity. Our initial, baseline estimating equation is

$$c_{i,t+1} - c_{i,t} = \Sigma_s \beta_{0s} * month_{s,i} + \boldsymbol{\beta_1' X}_{i,t} + \beta_2 R_{i,t+1} + u_{i,t+1}, \qquad (2)$$

where c is either consumption expenditures or their log, *month* is a set of indicator variables for every period in the sample to absorb the seasonal variation in consumption expenditures, as well as other concurrent macro factors; and X are control variables (here age and changes in family composition) included to absorb some of the preference-driven differences in the growth rate of consumption expenditures across households. $R_{i,t+1}$ represents the total dollar amount of rebates received by household i in period $t+1$ *(Rebate)*, or a dummy variable indicating whether any rebate was received in *t+1 (I(Rebate>0))*. If consumption expenditures are smoothed across rebate receipt, the null hypothesis under the PIH treating the rebates as pre-announced, then β_2 should equal zero. We report standard errors calculated to allow for arbitrary heteroskedasticity and within-household serial correlation.

When we focus purely on variation in the timing of rebate receipt among households receiving rebates, the random nature of that timing ensures that our identifying assumption is met and we estimate a causal effect. In particular it is worth emphasizing that our identification scheme allows us to avoid potential omitted variables bias and other confounding factors, at both the household and aggregate levels. By contrast, in most previous studies the income gain at issue (e.g., a windfall) was usually systematically related to various household characteristics. For instance, suppose that high-income households (who are more likely to own stocks) receive larger windfalls (or other predictable income gains); and that for other reasons the stock market happens to rise at the time of the windfall,

[14] That is, this estimation method is based on the time-series properties of the expectation error under the assumption that the model is true and there is no effect of predictable income changes on consumption growth. A statistically significant relationship between predictable changes in income and consumption growth rejects the model so that a significant coefficient on the predictable change in income cannot be interpreted causally.

leading high-income households to increase their consumption expenditures. In this case the estimated effect of the windfall on expenditures would be exaggerated by the stock market appreciation. During our sample period there are probably large changes in spending patterns induced by concurrent macroeconomic events, such as the recession, changes in monetary policy, the terrorist attacks of 9/11, etc. Nonetheless, all these events, even if their impact is correlated with household characteristics, are uncorrelated with the randomly assigned date when households received their tax rebates.

Following the main results, we investigate the dynamic response of expenditures by adding lags of the rebate variable to equation (*2*). We also examine the response of different subcategories of spending by changing the definition of consumption expenditures in equation (2), and finally we study the role of borrowing constraints by interacting the rebate variable with information about the liquidity of the household.

VI. Main Results

We begin by studying the short-run change in consumption expenditures caused by rebate receipt, using all the available information about the rebates received by each household. We subsequently limit the variation that we exploit to just the timing of rebate receipt, which is guaranteed to be exogenous because it is randomized. As we focus in on timing alone, we reduce the amount of variation that identifies the key parameter, β_2, and thereby reduce the power of our estimator. The short-run effects presented in this section are very similar to the short-run effects estimated in specifications in the following section that include lagged rebate variables. For ease of exposition, and in response to data limitations discussed below, we begin by focusing on the short-run effects alone.

The first three columns of Table 2 display the results of estimating equation (2) with the dollar change in consumption expenditures as the dependent variable and the contemporaneous amount of the rebate (*Rebate*) as the key independent variable, using all available rebate data. Here β_2 measures the average fraction of the rebate spent on each expenditure category, within the three-month period of rebate receipt. We find that, during the three-month period in which a rebate was received, relative to the previous three-month period, a household on average increased its expenditures on food by 11 percent of the rebate, its expenditures on non-durable goods strictly defined by 23 percent of the rebate, and

its expenditures on non-durable goods broadly defined by 37 percent of the rebate. The latter two results are both economically and statistically significant, and counter to the PIH.

These results identify the effect of a rebate from variation in both the timing of rebate receipt and in the amount of the rebate. While variation in the rebate amount is possibly uncorrelated with the residual in equation (2), strictly speaking it is not purely random. The amount of the rebate depends upon household characteristics -- primarily whether the household contains a married couple that filed jointly and how many returns were filed by the household.

The remaining columns of Table 2 exploit only variation in whether a rebate was received at all, not the dollar amount of rebates received. The second three columns of Table 2 display the results with the key independent variable being an indicator that equals one if household *i* receives a rebate of any size during period *t+1* (*I(Rebate>0)*). In this case, β_2 measures the average dollar increase in expenditures caused by the receipt of a rebate. During the three-month period in which a rebate was received, relative to the previous three-month period, households on average increased their expenditures on food by $51, their expenditures on non-durable goods strictly defined by $94, and their expenditures on non-durable goods broadly defined by $177. Relative to an average rebate of about $500, these results are consistent with those in the prior columns that included variation in the magnitude of the rebates received. To help calibrate the size of the effect of the rebate, the third panel of Table 2 uses the change in log expenditures as the dependent variable. On average in the three months in which a rebate is received, relative to the previous three-month period, consumption expenditures increased by 2.7 percent, 1.7 percent, and 3.1 across the three categories of expenditure. Again these results are both economically and statistically significant.

Finally, since it is interesting to estimate a value interpretable as a marginal propensity to spend upon the rebate's arrival, we estimate equation (2) by two-stage least squares (2SLS). We use the indicator variable, *I(Rebate>0),* and the other independent variables to instrument for the rebate amount, *Rebate*. In this case, as in the first three columns, β_2 measures the fraction of the rebate that is spent within the three-month period. As shown in the last three columns of Table 2, the estimated marginal propensities to spend (11 percent, 20 percent, and 37 percent) remain statistically significant and are very close to those estimated without

treating *Rebate* as potentially non-exogenous. According to these results, the full amount of variation in the rebate amount *Rebate* used in the first three columns can be taken to be exogenous. Comparing these two sets of results formally (the first three columns to the last three columns), for each definition of consumption expenditures a Hausman test does not reject the hypothesis that the variation in the rebate amount is exogenous.

Overall, the results across the various specifications are quite consistent. The estimated propensities to spend (in the first three and last three columns) imply dollar increases in spending that are in line with the dollar-amount results using *I(rebate)* as the dependent variable (the second set of three columns).[15]

These results identify the response of expenditures by comparing the behavior of households that receive a rebate in a given period to the behavior of households that do not. However, recall that some households did not receive rebates at all. It is possible that such households are not a valid control group. While most other studies would simply have to assume that their control group is valid, we can further explore this in two ways.

First, we control for rebate receipt, by allowing the expenditure growth of households that do not receive a rebate at all in the sample to differ from the expenditure growth of households that do receive a rebate. Table 3 Panel A shows that this does little to affect our conclusions. We include a separate intercept for households that received a rebate in *some* sample reference quarter (*I(Total Rebates >0)*). In this case the main regressor (*I(Rebate>0)*) captures only high-frequency variation in the timing of rebate receipt -- receipt in quarter $t+1$ in particular -- conditional on receipt in some quarter. The separate intercept is never statistically significant. And in each regression, the point estimates of the effect of the rebate are actually larger. That is, even controlling for whether a household received a rebate, expenditures significantly increase in the particular quarter of rebate receipt.

Our second approach is much more severe. We discard all households from our sample that do not receive a rebate (or, more precisely, those that are not known to have received a rebate using the available data) and the few that receive the rebate late. In this case we are identifying the response of consumption expenditures only from purely random variation, at

[15] Using average levels of expenditures, the percent changes in consumption in the third panel of results implies dollar consumption change and propensities to consume of $40 and 0.083 for food, $55 and 0.113 for strictly non-durable goods and $129 and 0.268 for non-durable goods, again broadly consistent with the other columns.

the cost of a substantial loss in power due to the resulting reduction in sample size.[16] This approach effectively identifies the impact of the rebate from only the behavior of households that have both consecutive interviews covering the rebate period and report a rebate only in the earlier interview or only in the later interview. Recall that the tax module was in the field through December 2001. We thus identify our main parameter from only two groups of households: those with interviews in August (covering 2 percent of rebates received) and in November (27 percent of rebates); and those with interviews in September (19 percent of rebates) and December (20 percent of rebates). We lose all information from households interviewed in October (31 percent of rebates).[17] Table 3 Panel B shows that, consistent with the reduction in power, statistical uncertainty rises dramatically so that a 95 percent confidence interval contains both no response and much larger responses than previously estimated. With the caveat that the standard errors are large, we note that point estimates are somewhat lower than in Table 2 but generally similar. This is formally confirmed by Hausman tests: in no column can we reject the hypothesis that the coefficients in this restricted subsample are the same as in the larger sample employed for Table 2. Although these tests have limited power, as before the greater variation in the original, larger sample can be taken to be exogenous.

We now turn to estimating the lagged response of consumption expenditures, and how this response differs across households. Because these extensions are even more demanding of the data than the short-run responses estimated in Table 2, we return to using all the available households.

VII. The Longer-Run Response of Expenditures

Relative to the PIH, the estimated fraction of the rebate spent in the first quarter of receipt is quite large. However the fraction is substantially less than one, so it remains an open question as to what happens to spending subsequently.

[16] For example, consider a household whose last CE interview is in 9/01. Even if they report no rebates in the reference period 6/01 to 8/01, we cannot tell whether they received a rebate after 8/01. Thus we drop all of the interviews of this household.

[17] Since the households interviewed in October are surveyed only once about rebate receipt, to remain in the sample they must have received a rebate in the previous three months. In this case we cannot disentangle the effect of rebate receipt from contemporaneous macroeconomic factors (i.e., *I(rebate)* and the October month dummy are the same across these households).

Panel A of Table 4 shows the results of estimating our main specifications when we include as an additional regressor the first lag of the rebate variable. First, note that the presence of the lagged variable does not alter our previous conclusions about the contemporaneous impact of the rebate. The coefficients on *Rebate* are quite similar to those in Table 2. Second, the receipt of a rebate causes a *change* in expenditures one quarter later (i.e., from the first quarter after rebate receipt to the second) that is negative and smaller in absolute magnitude than the contemporaneous impact. While the coefficients on the lag of the rebate variable are typically not statistically significantly different from zero, for nondurable goods broadly defined they are statistically significantly different from the negative of the coefficient on the contemporaneous rebate. Thus, the receipt of the rebate raises expenditures both in the period in which the rebate is received and in the subsequent period. For example, the second column shows that expenditures on nondurable goods rise by 38% of the rebate in the quarter of receipt. The expenditure change in the next quarter is -9%, so that expenditures in the second three-month period are still higher than before the arrival of the rebate by 38%-9%=27%. This 27% result is significant at the 95 percent level. Accordingly, the *cumulative* change in expenditures on nondurable goods over both quarters is estimated to be 38% + 27% = 68% of the rebate, and is highly statistically significant (bottom row of Panel A).

To estimate whether the rebate raises consumption expenditures for a longer period we add a second lag of the rebate variable to our regression. This requires that we extend the sample period of our data to include the 2002 CE data.

Panel B of Table 4 shows that the additional data and regressor do not change our previous conclusions about the impact of the rebate on expenditures in the contemporaneous period or the subsequent period, using the rebate variable and its first lag. More interestingly, the coefficients on the *second* lag of the rebate variable are all negative, suggesting that the growth in expenditures continues to decline following their increase upon rebate receipt, but these coefficients are all imprecisely estimated. The level of expenditures in the second three-month period following rebate receipt is no longer statistically significantly different than the level before receipt. Further, while the share of the rebate spent during all three periods is still large and statistically different from zero (bottom row of Panel B), this share is not significantly different from the share that we estimated was spent during the first two periods

(Panel A), and the statistical uncertainty of the three-period estimate is much larger. For example, the 95 percent confidence interval for the cumulative response of nondurable goods estimated in the second column extends from 7 percent of the rebate to 160 percent of the rebate.

In sum, the pattern of coefficients suggests a large increase in expenditures on rebate receipt, then a decaying but still substantial effect in the subsequent two quarters. Households spent about two thirds of their rebates on nondurable consumption goods in the quarter of receipt plus the subsequent three months. Since the response in the next three months (using the second rebate lag) is very imprecisely estimated, the balance of the paper focuses only on the contemporaneous rebate and its first lag, using the original sample.

VIII. Differences in Responses Across Households and Goods

This section analyzes heterogeneity in the response to the rebate, across different types of households and different subcategories of consumption goods. Part of the motivation is to provide some evidence as to why household expenditures responded as found above to the rebate. We begin by investigating whether liquid or illiquid households were more likely to increase expenditures upon arrival of a rebate. Households with low wealth may be unable or unwilling to increase their spending prior to the arrival of a rebate because of liquidity constraints, the lack of funds or credit available for spending. On the other hand, households with high wealth may find the costs of not smoothing consumption across the arrival of the rebate to be small.[18]

Expanding equation (*2*), we interact the intercept, rebate and lagged rebate variables with indicator variables (*Low* and *High*) based on various household characteristics referring to households' first interview. Table 5 begins with two proxies for liquidity constraints, age and income (family income before taxes), starting with age since it is better measured in the CE surveys. We also use a direct measure of liquid assets (the sum of reported balances in checking and saving accounts), but this variable is often missing in the CE data.

Both young and old households are more likely to be liquidity constrained or face substantial risk to future consumption. Young households have low liquid wealth and high

[18] See the arguments of Caballero (1995), Parker (1999), Sims (2001), and Reiss (2004).

consumption growth; old households may be living pension check to pension check.[19] Nonetheless, the first two columns of Table 5 show no statistically significant differences in the response of expenditures across age groups. However, based on the point estimates there is some evidence that young (younger than 40) and old (older than 55) households spent more of their rebates upon receipt.

The middle and final pairs of columns show that households with low liquid wealth or low income spent a greater share of their rebate checks when the checks arrived relative to the typical household. These differences are statistically significant. Households with low wealth also spent more over the following three months, though this difference is not significant. Based on the point estimates, households with high income or wealth also seem to have spent a greater fraction of the rebate on receipt, although the difference between these groups and the middle groups is not statistically significant.

In sum, we find evidence that households with low income and low wealth consumed more of their rebates, which is potentially consistent with the existence of liquidity constraints. Note that these low income or low wealth households are consuming most of their rebate in the first quarter after receipt. They are not saving much of the rebate to smooth expenditures in subsequent quarters. This could be either because they expect to have higher income in the near future (e.g., due to a recovery) or because they have a high propensity to consume one-time or highly liquid funds.[20]

What did households buy with their rebates? Table 6 displays the results of estimating our main dynamic regression with different dependent variables measuring expenditures on subcategories within nondurable expenditures. Few estimates are statistically significant. For these narrow subcategories of expenditures there is much more variability in the dependent variable that is unrelated to the rebate regressor. Our previous results, by summing the subcategories into broader categories of nondurable goods, averaged out much of this unrelated variability, such as for example whether a trip to the supermarket happened to fall just inside or outside the expenditure reference-period.

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[19] See Jappelli (1990), Wilcox (1989), and Stephens (2003).

[20] Buffer stock models can generate large propensities to consume in response to transitory income gains. Adding hyperbolic discounting of the sort studied by David Laibson, George-Marios Angeletos, Andrea Repetto, Jeremy Tobacman, and Stephen Weinberg (2001) can generate even larger short-run responses.

With this caveat about statistical significance in mind, based on the point estimates there is some evidence that expenditures on food, both at home and away from home, responds strongly to rebate receipt. Expenditures on food at home rise by less than their share in nondurable expenditures and our previous estimates would suggest, but stay persistently high. Expenditures on food away from home increase by more than their share would suggest, but this rise is transitory. There is no noticeable response of purchases of alcohol. Within the remainder of nondurable goods, we find a strong response (relative to their shares in nondurable expenditure) in personal care (and miscellaneous nondurable goods), apparel, and health expenditures.

IX. Conclusion

This paper finds significant evidence that households spent much of their 2001 income tax rebate checks. Specifically, households spent 20-40 percent of their rebates on non-durable consumption expenditures during the three-month period in which the check arrived. We also find evidence of additional, smaller but still substantial, spending in the next two quarters, though these lagged effects are estimated with less precision. The expenditure responses are largest for households with relatively low liquid wealth or low income, which is consistent with liquidity constraints.

What does this imply in terms of the economic stimulus provided by the rebate checks? In aggregate, the rebates totaled 38 billion dollars, or about 2.2 percent of aggregate personal consumption expenditures (PCE) and 7.5 percent of nondurable PCE in the third quarter of 2001. Applying our estimated propensities to spend, this implies that the receipt of the tax rebates directly raised total PCE by about 0.8 percent in the third quarter of 2001 and 0.6 percent in the fourth quarter, and raised nondurable PCE by 2.9 percent and 2.0 percent in the third and fourth quarters. Since these calculations do not include any potential effect on durable goods nor any multiplier effects, the full impact of the rebates on total PCE is possibly even larger. On the other hand, these calculations assume that the increases in expenditure did not increase prices, but only real consumption expenditure.

While we measure the causal impact of the rebates from cross-section information, without using movements in aggregate consumption, the behavior of both aggregate consumption and saving data is broadly consistent with our findings. Figure 1 shows the

growth rate of real total and nondurable PCE in the quarters surrounding the rebate disbursement. The first half of 2001 was in recession, and the latter half of 2000 and the first half of 2001 had low consumption growth. When the rebates were mailed out, PCE growth rose substantially, and the recession ended in November 2001. The personal saving rate rose from 1.9 percent and 1.1 percent in the first two quarters of 2001 to 2.8 percent in the third quarter when the rebates were mailed out, a pattern and magnitude consistent with households initially saving about two-thirds of the rebates (see Shapiro and Slemrod (2003; 2002, forthcoming)). The household saving rate then fell again to 1.0 percent in the fourth quarter of 2001 and rose to 2.5 and 2.8 percent in the first two quarters of 2002. Our findings are consistent with this pattern under various scenarios, e.g. if consumption was initially depressed by terrorist attacks of September 11th but this effect was offset by the lagged spending effects of the rebate through the end of 2001.[21]

While we focus on consumers' response to the receipt of the rebates, we cannot directly estimate whether there was an earlier response in anticipation of the rebate. This is because there is no single point in time at which a tax cut went from being unexpected to being expected; rather expectations of a tax cut grew over a long period, starting at least as early as the 2000 election. Moreover, since the announcement of the tax cut, and the anticipations of it that arose even earlier, represent aggregate shocks, they cannot be separated from other aggregate effects captured by our time dummies. Nonetheless, our results imply that the anticipatory response is likely to be small, since we already find large responses at the time the rebate checks arrived.

We conclude with a caveat. While the 2001 rebates did stimulate consumer spending, without knowing the correct structural model underlying these results, we cannot conclude that future tax rebates will have quantitatively the same effect. In 2001 the rebates were part of countercyclical stabilization policy. The spending response to other tax rebates may differ across time and circumstances. For instance, the response might be smaller outside of a recession or given a different situation for household balance sheets.

[21] Shapiro and Slemrod note that some of the decline in saving in 2001:Q4 is due to increased spending on durable goods, particularly cars. But since durable goods are a small share of total consumption, the rise in the saving rate was still caused mostly by changes in other, nondurable components of consumption.

Appendix A: CE Survey Instrument

INTRO: Earlier this year a Federal law was passed cutting income tax rates and expanding certain credits and deductions. This year many households will receive a tax rebate check in the mail.

1. Since the 1st of (month, 3 months ago) have you (or any members of your CU [consumer unit]) received a tax rebate?
   1.YES—go to 2
   2. NO—end of interview

2. For each check received:

	check1	check2	check3	check4	check5
a. In what month did you receive the rebate?	__ __	__ __	__ __	__ __	__ __
b. What was the amount of the rebate?	$_____ _	$_____ _	$_____ _	$_____ _	$_____ _

3. **For Interview number 2 and 5 and New Consumer Units**: Did you already report the amount of this rebate in Section 22 , question 13, which asks about tax refunds?

Appendix B: CE Sample and Construction of Rebate Variables

We first construct a rebate variable from the raw data from question 2 (Appendix A). We then use the flags and other information to set the sample so that observations for which we are unsure about the validity of the rebate variable are not used. Our rebate variable is the sum of all rebates reported during the three-month expenditure reference period. If any of these magnitudes is missing, rebate is set to missing.

Second, to maximize sample size, we use some rebate information from later interviews to fill in missing data in earlier interviews. Specifically, for interviews with no raw tax data, and for which the subsequent interview reports a rebate as having been received during the first interview's reference period, we treat the later interview's information as valid. (In particular, this completes the data for some of the households that were interviewed in early August before the tax rebate module was in the field.) Third, we use some rebate information from earlier interviews to create rebate measures for the reference period of the subsequent interview. For example, occasionally the first interview with tax data records a rebate received within the interview month itself (i.e., after the corresponding reference period), and the following interview reports no rebate for that same month. We treat this as a valid rebate response for the second reference period, since it is more likely to have been received then. Finally, the first interview sometimes reports no rebate, but the second interview records a rebate received during the first interview's reference quarter. In this case we assume that the household made a recall error in the second (more distant) interview and that the timing of the rebate reported in that interview is off. We therefore treat the rebate as if it occurred in the second interview reference-period if there is no other rebate already recorded for that period.

Rebate is set to zero for all observations covering reference periods ending June 2001 or earlier and starting October 2001 or later (unless a late rebate was reported) – periods during which the rebate questions were not on the CE survey.

We drop a rebate observation when: 1) the lead-in question 1 states that a rebate was not received but there is a rebate reported in question 2; 2) the lead-in question states that a rebate was received but there is no rebate reported for any month; 3) there is a valid positive rebate amount but the associated month-of-receipt is either missing or flagged as invalid; 3) a rebate is reported as received in a certain month but the rebate amount is missing, invalid or zero.

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

Following Lusardi (1996), our expenditures on nondurable goods (strictly defined) include expenditures on food (away from home, at home and alcoholic beverages), on utilities (fuels and public services), on household operations, on public transportation, on gas and motor oil, on personal care, on tobacco, and on miscellaneous goods. Nondurable goods adds expenditures on apparel and services, on the rest of transportation, on health care expenditures (excluding payments by employers or insurers), and on reading materials.

Turning to the sample, we omit observations missing any of the key data that we use in our regressions. Our sample omits the bottom one percent of nondurable consumption expenditures in levels (after adjusting for family size and allowing for a time trend), since this data implies implausibly small consumption expenditures. Finally, we drop household

observations that report living in student housing, that report age less than 21 or greater than 85, that report age changing by more than one or a negative amount between quarters, or that report changes in the number of children or adults greater than three in absolute magnitude. When we split on income, we drop households flagged as incompletely reporting income.

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Table 1: Summary statistics

Panel A: Sample statistics (N=21,239 observations)

Variable	Mean	Standard Deviation	Minimum	Maximum
Expenditures on:				
Food	1,460	1,060	0	24,799
Strictly nondurables	3,140	3,358	90	284,216
Nondurables	4,112	3,890	357	300,155
Change in Expenditures on:				
Food	-8	892	-16,392	22,360
Strictly nondurables	49	1,594	-20,363	49,860
Nondurables	76	1,973	-31,884	52,978
Change in:				
Number of Adults	0.0	0.3	-3	3
Number of Children	0.0	0.2	-3	3
Age	49.6	16.5	21	85
Rebate value	53.4	161.7	0	1,500
Rebate value| Rebate>0	479.8	173.9	2	1,500
I(Rebate)	0.111	0.315	0	1
Income (N=14,605)	45,451	35,582	-2,500	320,800
Liquid Assets (N=7,829)	7,210	15,699	-31,000	136,200

Panel B: Distribution of Positive Rebate Values

Rebate value	Number of Observations	Percent of Postive Rebates
0<Rebate<300	172	7.3
Rebate=300	639	27.0
300<Rebate<600	233	9.8
Rebate=600	1,275	53.9
Rebate>600	47	2.0

Panel C: Means of Rebate Variables by Period

Three month period	Rebate	I(Rebate)	Rebate| Rebate>0
May - July, 2001	30.8	0.07	442.2
June - Aug, 2001	152.3	0.33	467.7
July - Sept, 2001	279.4	0.57	489.5
Aug - Oct, 2001	254.5	0.52	487.8
Sept - Nov, 2001	167.1	0.36	469.7

Note: based on sample for baseline regression using nondurable goods (Table 2).

Table 2: The contemporaneous response of expenditures to the tax rebate

Dependent Variable:	ΔC Dollar change in			ΔC Dollar change in			ΔlnC Precent change in			ΔC Dollar change in		
	Food	Non-durable goods (strict)	Non-durable goods	Food	Non-durable goods (strict)	Non-durable goods	Food	Non-durable goods (strict)	Non-durable goods	Food	Non-durable goods (strict)	Non-durable goods
Estimation method:	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS
Rebate	0.105 (0.056)	0.233 (0.115)	0.368 (0.135)							0.106 (0.058)	0.198 (0.112)	0.370 (0.136)
I(Rebate>0)				50.5 (27.5)	94.4 (53.5)	176.6 (64.8)	2.74 (1.37)	1.74 (1.05)	3.13 (1.01)			
Age	0.106 (0.253)	-0.477 (0.448)	0.255 (0.556)	0.096 (0.252)	-0.511 (0.447)	0.221 (0.555)	0.002 (0.017)	-0.015 (0.014)	0.005 (0.013)	0.106 (0.253)	-0.492 (0.448)	0.257 (0.555)
Change in adults	139.5 (38.0)	268.0 (62.5)	369.7 (74.4)	140.0 (38.0)	269.1 (62.6)	371.4 (74.5)	7.11 (1.51)	6.30 (1.19)	7.00 (1.16)	139.5 (38.0)	268.2 (62.5)	369.7 (74.4)
Change in children	73.0 (34.2)	147.5 (62.3)	200.6 (78.1)	73.2 (34.2)	147.8 (62.3)	201.2 (78.1)	4.40 (1.91)	4.40 (1.34)	5.16 (1.34)	73.0 (34.2)	147.5 (62.3)	200.6 (78.1)

Notes: N=21,239 for all regressions. All regressions include time dummies for every three-month period. Reported standard errors are adjusted for arbitrary within-household correlations and heteroskedasticity. The third set of three columns are multiplied by 100 so as to report a percent change. The last three columns report results from two-stage least squares regressions where *Rebate* is treated as possibly not exogenous and *I(Rebate)* with the other regressors are used as instruments.

Table 3: The contemporaneous response of expenditures: extensions

Dependent Variable:	ΔC Dollar change in Non-durable goods (strict)	ΔC Dollar change in Non-durable goods	ΔlnC Percent change in Non-durable goods (strict)	ΔlnC Percent change in Non-durable goods	ΔC Dollar change in Non-durable goods (strict)	ΔC Dollar change in Non-durable goods
			Panel A: All households			
Estimation method:	OLS	OLS	OLS	OLS	2SLS	2SLS
Rebate	0.285	0.443			0.254	0.470
	(0.133)	(0.157)			(0.137)	(0.166)
I(Rebate>0)			2.34	4.06		
			(1.32)	(1.27)		
I(Total Rebates>0)	-36.8	-54.3	-0.73	-1.13	-32.7	-57.9
	(25.7)	(31.7)	(0.61)	(0.61)	(27.0)	(33.3)
N	21,239	21,239	21,239	21,239	21,239	21,239
			Panel B: Only households observed receiving rebates on time			
Estimation method:	OLS	OLS	OLS	OLS	2SLS	2SLS
Rebate	0.169	0.215			0.087	0.199
	(0.166)	(0.193)			(0.224)	(0.263)
I(Rebate>0)			1.59	2.17		
			(2.17)	(2.11)		
N	6,801	6,801	6,801	6,801	6,801	6,801

Notes: All regressions also include the change in the number of adults, the change in the number of children, the age of the household head, and time dummies for every three-month period. Reported standard errors are adjusted for arbitrary within-household correlations and heteroskedasticity. The final pair of columns report results from two-stage least squares regressions where Rebate is treated as possibly not exogenous and I(Rebate) with the other regressors are used as instruments

Table 4: The dynamic response of expenditures to the tax rebate

Rebate Variable:	*Rebate*		*I(Rebate)*		*Rebate*	
Dependent Variable:	ΔC Dollar change in		ΔlnC Percent change in		ΔC Dollar change in	
	Non-durable goods (strict)	Non-durable goods	Non-durable goods (strict)	Non-durable goods	Non-durable goods (strict)	Non-durable goods
	Panel A: lagged rebate and baseline sample (N=20,400)					
Estimation method:	OLS	OLS	OLS	OLS	2SLS	2SLS
Rebate or I(Rebate)	0.244 (0.114)	0.383 (0.134)	1.86 (1.04)	3.27 (1.01)	0.206 (0.111)	0.382 (0.135)
Rebate or I(Rebate) Lagged	-0.157 (0.099)	-0.090 (0.114)	-1.87 (1.06)	-1.49 (1.01)	-0.189 (0.101)	-0.120 (0.117)
Implied total fraction of rebate spent over both three-month periods	0.332 (0.218)	0.675 (0.259)	NA	NA	0.223 (0.211)	0.645 (0.261)
	Panel B: two lags of rebate and extended sample (N=25,835)					
Estimation method:	OLS	OLS	OLS	OLS	2SLS	2SLS
Rebate or I(Rebate)	0.247 (0.114)	0.386 (0.134)	1.78 (1.04)	3.19 (1.01)	0.208 (0.111)	0.384 (0.135)
Rebate or I(Rebate) Lagged	-0.172 (0.097)	-0.108 (0.112)	-2.18 (1.04)	-1.84 (1.00)	-0.210 (0.099)	-0.148 (0.115)
Rebate or I(Rebate) Lagged Twice	-0.010 (0.117)	-0.098 (0.137)	-0.43 (1.21)	-1.93 (1.18)	-0.032 (0.119)	-0.174 (0.139)
Implied total fraction of rebate spent over all three three-month periods	0.388 (0.321)	0.845 (0.390)	NA	NA	0.169 (0.315)	0.684 (0.395)

Notes: All regressions also include the change in the number of adults, the change in the number of children, age of the household, and time dummies for every three-month period. Standard errors are adjusted for arbitrary within-household correlations and heteroskedasticity. The final pair of columns report results from two-stage least squares regressions where Rebate and its lags are treated as possibly not exogenous and I(Rebate) and its lags with the other regressors are used as instruments

Table 5: The propensity to spend across different households

Dependent variable: ΔC

Dollar change in:	Non-durable goods (strict)	Non-durable goods	Non-durable goods (strict)	Non-durable goods	Non-durable goods (strict)	Non-durable goods
	Interaction: Age Low: age < 40 High: age > 55		Interaction: Income Low: < 34,276 High: > 69,000		Interaction: Liquid Assets Low: < 921 High: > 8,000	
Rebate	0.045	0.227	0.097	0.168	-0.272	-0.140
	(0.156)	(0.177)	(0.152)	(0.173)	(0.189)	(0.232)
*Rebate*Low*	0.058	0.082	0.289	0.605	0.609	0.884
	(0.176)	(0.199)	(0.211)	(0.252)	(0.270)	(0.318)
*Rebate*High*	0.284	0.246	0.134	0.137	0.458	0.503
	(0.247)	(0.279)	(0.232)	(0.268)	(0.312)	(0.375)
RebateLagged	-0.189	-0.131	-0.022	-0.021	-0.242	-0.353
	(0.125)	(0.151)	(0.141)	(0.164)	(0.193)	(0.246)
*RebateLag*Low*	0.182	0.119	-0.096	-0.105	0.044	0.246
	(0.151)	(0.189)	(0.190)	(0.239)	(0.260)	(0.348)
*RebateLag*High*	-0.115	-0.027	-0.456	-0.369	-0.232	-0.034
	(0.216)	(0.252)	(0.224)	(0.262)	(0.327)	(0.397)
Low	-7.0	-2.59	7.1	-1.87	-85.0	-103.5
	(20.9)	(25.88)	(25.6)	(30.52)	(29.5)	(36.0)
High	10.9	20.38	37.3	38.64	-41.3	-22.7
	(34.7)	(42.61)	(38.7)	(49.13)	(46.1)	(57.1)
N	20,212	20,212	14,115	14,115	7,705	7,705
N Low	8,926	8,926	6,880	6,880	3,395	3,395
N High	5,732	5,732	2,940	2,940	1,670	1,670

Notes: All regressions also include the change in the number of adults, the change in the number of children, the age of the household head, and time dummies for every three-month period. All results are from two-stage least squares regressions where Rebate and its lag and interactions are treated as possibly not exogenous and I(Rebate) and its lag and interactions with the other regressors are used as instruments. Reported standard errors are adjusted for arbitrary within-household correlations and heteroskedasticity.

Table 6: The propensity to spend on different categories of goods

Dependent variable: ΔC	Panel A: Food			Panel B: Nondurable Goods (Strict)				Panel C: Nondurable Goods			
Dollar change in:	Food at home	Food away from home	Alcoholic beverages	Utilities, Household operations	Personal care and misc.	Gas, motor fuel, public transportation	Tobacco products	Apparel	Transport (incl. gas, fuel, etc.)	Health	Reading
Average share of Nondurable Goods	0.28	0.08	0.02	0.23	0.04	0.11	0.02	0.08	0.49	0.13	0.01
Rebate	0.053	0.044	0.004	0.037	0.067	0.001	-0.001	0.075	-0.190	0.096	0.005
	(0.037)	(0.038)	(0.011)	(0.027)	(0.058)	(0.044)	(0.007)	(0.044)	(0.357)	(0.040)	(0.004)
RebateLagged	0.003	-0.040	0.004	-0.006	-0.071	-0.074	-0.004	0.083	-0.370	-0.015	0.001
	(0.038)	(0.046)	0.011	(0.025)	(0.052)	(0.039)	(0.008)	(0.033)	(0.372)	(0.040)	(0.005)
Implied total fraction of rebate spent over both 3-month periods	0.109	0.048	0.011	0.069	0.062	-0.073	-0.005	0.234	-0.749	0.177	0.010
	(0.083)	(0.077)	(0.022)	(0.055)	0.107	0.083	(0.015)	(0.090)	(0.705)	(0.081)	(0.008)

Notes: N=20,400 for all regressions. All regressions include time dummies for every three-month period. Reported standard errors are adjusted for arbitrary within-household correlations and heteroskedasticity. All results are from two-stage least squares regressions where *Rebate* is treated as possibly not exogenous and *I(Rebate)* with the other regressors are used as instruments.

Figure 1:
Growth rates for personal consumption expenditures