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ABSTRACT

This paper uses tax rebate payments from the 2008 economic stimulus to estimate the effect of a one-time change in income on three measures of subjective well-being: life satisfaction, health satisfaction, and affect. The income effect is identified by exploiting the plausibly exogenous variation in the payment schedule of the rebates. Using both ordinary least squares and two-stage least squares estimators, I find that the rebates had a large and positive impact on affect, which is explained by a reduction in feelings of stress and worry. For life satisfaction and health satisfaction, there is weaker evidence of a positive impact. Overall, the results show that a temporary increase in liquidity may enhance emotional well-being and that this effect is relatively stronger for low-income respondents.

JEL Classification Codes: I31, H31, E62

Key Words: Subjective well-being, affect, income effect, quasi-experiment, instrumental variable

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I. INTRODUCTION

Over the past 40 years, economists have become more interested in understanding the predictors of subjective well-being (SWB). For example, SWB has been used to study the welfare trade-off between inflation and unemployment (Di Tella, MacCulloch, and Oswald 2001), welfare costs of business cycles (Wolfers 2003), compensating wage differentials for time to commute (Stutzer and Frey 2008) and need to interact with others (Krueger and Schkade 2008b), gender differences in happiness (Stevenson and Wolfers 2009), mental costs of job loss or unemployment (Clark and Oswald 1994; Krueger and Mueller 2011; Kalil and DeLeire 2013), and health and disability (Daly and Gardiner 2013; Finkelstein, Luttmer, and Notowidigdo 2013). However, perhaps the best-known application of SWB in economics has been to answer the question "Does more income make you happier?" (Easterlin 1974).[1]

This paper contributes to this literature by estimating the effect of income on SWB using exogenous variation in the timing of the 2008 economic stimulus tax rebate payments. These rebates were one-time, lump-sum payments worth about $1,000 on average that were sent to about 130 million households and could be expected to have a pure income effect. Because the rebates were disbursed using a close-to-random schedule of payment, the tax rebate program offers an attractive setting for identifying the effect of a medium-sized income change on SWB.

To measure this effect on SWB, I use the Gallup-Healthways Daily Poll (GHDP), a Gallup survey started in 2008 that collects information on several measures of subjective well-being on a day-to-day basis. Specifically, the GHDP collects data on life satisfaction, health satisfaction, and information on affect (sometimes called emotional well-being or experienced

[1] DiTella and MacCulloch (2006), Kahneman and Krueger (2006), Clark, Frijters, and Shields (2008), and Deaton (2008) provide recent literature reviews.

utility) as well as demographic variables. During the time that the U.S. Treasury was issuing tax rebate payments, the GHDP included a daily question asking the respondents whether they had received a tax rebate. To my knowledge, the GHDP is the only survey that collected information on both SWB and 2008 tax rebate receipt.

The exogenous variation in the disbursement of tax rebates is valuable because there is disagreement as to whether income increases SWB. Early literature reported only a weak association between SWB and income across countries—the "Easterlin paradox" (Easterlin 1974). This has puzzled economists because utility, purportedly proxied by SWB, is a key normative measure in economics and because theory predicts income to be a central determinant of well-being; see, for example, Deaton (2008), Kahneman and Deaton (2010), Sacks, Stevenson, and Wolfers (2012), and Stevenson and Wolfers (2008, 2013) for recent research on this topic.

Concurrent with the debate over the SWB-income relationship, some economists have also taken issue with whether SWB is a meaningful measure of economic well-being (for example, Bertrand and Mullainathan 2001; Hamermesh 2004; and McCloskey 2012). This skepticism is in part because laboratory experiments have shown that SWB is sensitive to events that have nothing to do with actual changes in income; for example, Kahneman and Krueger (2006) describe a randomized experiment conducted by Norbert Schwartz (1987) in which a dime was placed on top of a photocopy machine. Subjects who found the dime reported a higher level of life satisfaction than those who did not. However, other research has also shown that objective economic measures—business-cycle volatility, unemployment, and inflation—do correlate negatively with SWB, which would validate it as a meaningful measure.

The tax rebates offer an attractive natural experiment for estimating the effect of income, and several papers have shown that the rebates had a short-term positive causal effect on household spending,[2] health-care utilization, and personal bankruptcies, but a negative effect on payday loans.[3] Given that the rebates had a causal effect on actual choices (that is, on revealed preference), finding that the rebates also had an effect on SWB (that is, on "stated preference") could be viewed as a way to validate (albeit indirectly) the usefulness of SWB as an economic measure. Furthermore, because the rebates were targeted toward low- and middle-income families, they can be used to identify the effect of income for a broad group of people. This is useful because previous research identifies the causal effect of income on SWB using lottery winners, who constitute a specific subset of the population (Lindahl 2005; Gardner and Oswald 2007; and Apouey and Clark 2014).

To answer whether the tax rebates had an effect on SWB, I begin by estimating a linear model using ordinary least squares (OLS), where I regress the SWB measures on an indicator of whether the respondent has received a tax rebate. Because the timing of rebate disbursement was close to random, the coefficient on the tax rebate dummy should be an unbiased, consistent estimate of the causal effect of a one-time rebate payment that a respondent receives now as opposed to later. However, since this key right-hand-side variable is collected through a survey, it could be measured with error, which in turn might attenuate the OLS estimate. I address this

[2] See Shapiro and Slemrod (2009), Sahm, Shapiro, and Slemrod (2010, 2012), Parker et al. (2013), Broda and Parker (2014), Kaplan and Violante (2014a, b), and Misra and Surico (2014) for papers studying the spending response to the rebates. Shapiro and Slemrod (2003), Johnson, Parker, and Souleles (2006), Agarwal, Liu, and Souleles (2007), Evans and Moore (2011, 2012), and Skiba (2014) study a similar, though less generous, tax rebate program from 2001.

[3] See Gross and Tobacman (2014), Gross, Notowidigdo, and Wang (2014), Bertrand and Morse (2009), and Skiba (2014). Personal bankruptcies increased because the rebates allowed liquidity-constrained households to pay administrative fees required to declare bankruptcy.

issue by constructing an instrumental variable (IV) and applying the two-stage least squares (2SLS) estimator.

The instrument is constructed using data from Daily Treasury Statements (DTS) on how many millions of dollars of rebate payments were issued between late April and August of 2008. Specifically, from each DTS report, I calculate the fraction of the rebate-eligible population that had been sent a rebate by that day. Using this variable as an instrument, I can correct measurement error by instrumenting one quantity potentially measured with error—the survey measure of tax rebate receipt—with another measure of that quantity. The DTS data reflect the exogenous timing of rebate payments, which is arguably uncorrelated with unobservables in the regression error term. As I show, this instrument is a strong predictor of the survey measure of rebate receipt.

There are two main findings. First, the OLS estimates show that rebate receipt had a positive effect on all measures of SWB: life satisfaction, health satisfaction, and affect. Second, the 2SLS estimates show that rebate receipt increased affect by 0.60 of a standard deviation—a substantial gain. This increase comes from a large decrease in the probability that respondents experience feelings of stress and worry, which suggests that additional liquidity may temporarily improve emotional well-being. Furthermore, the observed increase in affect is stronger for low-income respondents. However, for the remaining two dependent variables, life satisfaction and health satisfaction, the 2SLS estimates turn out to be large but imprecise and sensitive to alternative specifications.

Why did the rebates lead to an observed decrease in stress and worry but had no statistically significant effect on life satisfaction or health satisfaction? One possible explanation is that rebate payments are too small to change one's life evaluation or improve one's health. As

Krueger and Schkade (2008a) write, “One’s general level of life satisfaction would be expected to change only very slowly over time, because so do most of its known correlates.” Also, Gardener and Oswald (2007) show that among lottery winners there was no statistically significant effect on self-reported health for lottery wins up to £999, an amount similar to the rebates. Affect, on the other hand, measures more immediate feelings of worry or joy, which might be sensitive to current or recent events, such as a one-time payment. As Deaton (2011) writes, “Affect responds to circumstance, while life evaluation does not.”

Why does it matter if affect reacts to circumstance? The answer might lie at the intersection of psychology and economics. Research in psychology has argued that even small income shocks may have surprisingly large effects on cognitive capacity. For example, Mani et al. (2013) demonstrate that inducing low-income people to think of financial distress reduces their cognitive capacity, and the authors argue that this is because of an increase in worries about finances. A link between liquidity constraints and emotional distress may explain why low-income people sometimes make poor financial decisions (Shah, Mullainathan, and Shafir 2012). Ifcher and Zarghamee (2011) provide evidence of a positive causal link between affect and patience. If affect matters for economic decision making, then affect may have implications for policy. I return to these points later in the paper.

The rest of the paper is organized as follows: the next section describes the 2008 tax rebates. Section III describes the data, and Section IV the empirical strategy. The results are presented in Section V and discussed in Section VI. The final section concludes.

II. BACKGROUND ON THE 2008 ECONOMIC STIMULUS TAX REBATES

In 2008, as the United States entered the Great Recession, the Bush administration proposed an economic stimulus package that included tax rebates to low- and middle-income families with the goal of increasing household spending. The proposal, called the Economic Stimulus Act of 2008, was passed in February, and the first payments were issued by late April.

Eligibility for the tax rebate was determined by the 2007 tax returns.[4] However, individuals with no net tax liability who had a qualifying income of at least $3,000 per year were also eligible to receive the rebate. The tax rebates ranged between $300 and $600 for individual tax filers and between $600 and $1,200 for joint filers. The eligibility was phased out at a rate of 5 percent above the annual income limit of $75,000 ($150,000 for couples). Conditional on eligibility, those with a dependent child who qualified for the child tax credit received an additional $300 per child. According to Parker et al. (2013), a majority of U.S. households, about 130 million (or some 85 percent of tax-filing units), were qualified to receive the rebates, and almost $100 billion was issued in stimulus payments.

Parker et al. (2011) report that 30.9 percent of the households in their sample received rebates worth $600, followed by 26.2 percent who received rebates worth $1,200. Hence, over half of the recipient households received rebates ranging between $600 and $1,200. The third most numerous group, 11.2 percent of the households, received $300 in rebate payments; 7 percent received $1,500; and 6.4 percent received $1,800. The average value of the tax rebate payment was about $1,000. The tax rebate payment schedule depended on the last two digits of the filer's Social Security number (SSN). As these two digits of the SSN are assigned

[4] This section is based on Parker et al. (2013) and information from the Internal Revenue Service found at http://web.archive.org/web/20120825050232/http://www.irs.gov/newsroom/article/0,,id=179095,00.html.

sequentially and, in effect, randomly, the timing of when someone received a payment depended on an exogenous factor.

Had the variation in the timing of payment depended only on the SSN, then the process of disbursement of the tax rebates would have been a random-assignment experiment in which the timing of when someone eligible receives his or her payment is random. However, in practice, the payment schedule depended on other nonrandom factors, such as whether the filers included direct deposit information with their 2007 tax returns and whether the taxes were filed on time. The taxpayers who included direct deposit information received payment on three separate occasions in May. Parker et al. (2013) report that about 40 percent of the survey respondents answered that they received the rebate electronically. If the information about direct deposit was not included, checks were mailed from about mid-May to mid-July. Late tax filers received their tax rebate payments a few weeks later than the on-time filers subject to the "randomized" schedule. In sum, because of the not-entirely-random aspects of the disbursement process, the schedule of the tax rebate payments better fits the description of a quasi-experiment.

The 2008 tax rebate disbursement is also different from a typical random-assignment experiment in the following way. In a typical random-assignment experiment, a treatment group receives a treatment, while a control group is never treated. Hence, the treatment effect can be obtained by comparing the outcomes of these two groups. Instead, by the end of the rebate disbursement, all eligible recipients will receive a rebate. Hence, this paper, as well as the existing literature on the effects of rebates, identifies the effect of rebate receipt by comparing the outcomes of rebate recipients to the outcomes of people who have not yet received a rebate but will.

Table 1 shows the payment schedule by SSN and type of deposit. The right-hand column shows the dates by which filers who chose direct electronic deposit were intended to receive tax rebate payment transfers. The left-hand column shows the dates by which the filers who did not use direct deposit were intended to receive their tax rebate payments by mail. May 16 is a common date for both methods of delivery.

Figure 1 shows the daily disbursement of payments reported by the Daily Treasury Statements (DTS).[5] The payment schedule of the tax rebates is clearly visible in the 11 big spikes, which correspond to the dates listed in Table 1. About half of the payments were made on May 2, 9, and 16, which were the dates of disbursement for filers with direct deposit.

Note that some of the disbursement occurred in between the payment schedule dates listed in Table 1. This is because the dates listed in Table 1 pertain to the latest date on which a recipient would receive the tax rebate. The small "bumps" in late July are likely payments made to late tax filers.

III. DATA

The data come from two sources: the Gallup-Healthways Daily Poll (GHDP) and the daily statements, or DTS, from the U.S. Treasury. The GHDP is conducted daily by telephone interviews with a random sample of about 500 to 1,000 individuals aged 18 or older living in the United States. Each day a new cross section is drawn, and the survey is conducted seven days a week, excluding major holidays. Gallup collects the data using a dual-frame, random-digit dialing of both landlines and cell phones. The interviews are conducted with a respondent who is 18 years of age or older, living in the household, and who had his or her birthday most recently.

[5] I obtain this data from http://www.fms.treas.gov/dts/index.html.

A. Tax Rebate Receipt

Between April 27 and July 1, 2008, and then again between July 21 and 23 of that year, Gallup asked about 500 randomly selected respondents per day whether they had received a tax rebate.[6] The question read, "As part of an economic stimulus package passed by Congress, most individuals will receive a rebate check of between $300 and $600 from the federal government. These rebates are just starting to be sent to Americans or deposited in their checking accounts. Have you personally received this stimulus package rebate from the federal government, or not?" The answer is measured as a "yes" or a "no."

The survey did not collect information on SSNs or whether the tax rebate was received by mail or electronically by direct deposit. However, Parker et al. (2013) did not find that the consumption response differed depending on the method of payment delivery, and in their main results they pool the tax rebate recipients who submitted direct deposit information with those who received their checks by mail. Figure 2 shows the fraction of GHDP respondents reporting that they have received the tax rebate.[7]

When constructing the GHDP time series in Figure 2, I exclude individuals who report having an imputed gross monthly income equal to or more than $6,000. This is because the tax rebate question does not unambiguously distinguish between eligible and noneligible nonrecipients—that is, the respondents who answer "no" because they had too high an income in the previous year and will not receive a tax rebate. I select the $6,000 per month cutoff because it is close to the income limit at which the eligibility for the tax rebates was phased out ($75,000

[6] Between April 27 and May 5, about 1,000 respondents were surveyed about the tax rebate question, and on June 11 only about 250 respondents were surveyed. Also, the survey was not administered on May 30, June 18, and June 27, 2008.

[7] Table A.1 in the appendix shows, day by day, the fraction of GHDP respondents who say they received the tax rebates.

per year for individual filers and $150,000 per year for couples). I impose the same restriction on the regression analysis sample as well.

In order to impose this restriction, I convert the categorical measure of household gross monthly income into a measure of individual-equivalent income in the following way. The GHDP asks about total current monthly gross income of the household and collects the data in 10 bins. I transform this variable into a continuous measure by assigning the value at the midpoint of each bin. Then, I infer the number of adults in the household by using information about marital status. I create this individual-equivalent measure of monthly income by dividing the gross income measure by the square root of the number of adults in the household (two if married or living with a partner and one if otherwise).

B. Validation of the Question on Tax Rebate Receipt

In order to validate the tax rebate survey question, I compute a comparable measure using data from the DTS. The DTS time series is constructed by, for each day, summing the cumulative amount of payments issued by that date and dividing it by the total of about $92.185 billion issued in tax rebate payments by the end of August 2008. This time series shows, day by day, the fraction of eligible recipients who have been sent their rebate payment. The pattern observed in Figure 2 follows a series of stepwise increments that correspond to the 11 big spikes visible in Figure 1. The series also shows that by late July nearly all of the payments had been sent.

Comparing the GHDP series with the DTS series provides visual validation of the GHDP question. The GHDP time series is very close to the DTS time series; however, the GHDP series generally indicates a lower fraction of tax rebate recipients than the DTS time series. This suggests measurement error, the extent of which can be inferred by computing a reliability ratio

by regressing the dummy for whether a respondent has received a rebate on the DTS time series; see Ashenfelter and Krueger (1994), who use this method to validate a years-of-schooling variable by correlating two mismeasured variables. In my case, the reliability ratio equals 77 percent, which means that less than a quarter of the variation in the rebate series is due to mismeasurement. The reasons for this measurement error are further discussed in Section IV. To correct for measurement error, I use the DTS time series as an instrumental variable for tax rebate receipt in GHDP, which is also discussed in Section IV.

C. Measures of Subjective Well-Being

I use three GHDP questions measuring different aspects of subjective well-being.

The first outcome is a life satisfaction "ladder of life" question: "Please imagine a ladder with steps numbered from zero at the bottom to ten at the top. The top of the ladder represents the best possible life for you, and the bottom of the ladder represents the worst possible life for you. On which step of the ladder would you say you personally feel you stand at this time?" This life satisfaction question is measured on an 11-point scale from zero (worst possible) to 10 (best possible), which is standardized and reported in "effect sizes." The benefit of using this transformation is that it allows one to estimate linear regression models and to interpret the estimated coefficients as standard deviation changes, denoted as σ.

The second outcome is affect ("Did you experience the following feelings a lot yesterday: enjoyment, happiness, physical pain, worry, sadness, stress, anger?"), which is measured as either a "yes" or a "no." "Affect" is sometimes also called emotional well-being or experienced utility; see Kahneman and Krueger (2006) and Kahneman and Deaton (2010). Following Krueger and Schkade (2008a), I compute net affect by subtracting the average of questions on negative affect (physical pain, worry, sadness, stress, anger) from the average of questions on

positive affect (enjoyment, happiness). In order to interpret the estimates in terms of effect sizes, I also standardize this outcome variable.

As a third outcome, I use a question about satisfaction with health ("Are you satisfied or dissatisfied with your health?"), which takes on one of two values: satisfied or dissatisfied. Including this variable allows me to relate the findings to research on the positive gradient between income and health.

As is discussed further in the paper, in order to separate the effect of tax rebate receipt on SWB from a general timing effect, it is important to understand how the SWB measures behave over time. Due to its size, there are enough observations in the GHDP to show how the mean of the three SWB measures fluctuate day by day. Figure 3 shows the time path of the three unscaled SWB measures during 2008 and 2011—the two years of GHDP data available to me—for all of the GHDP respondents. The shaded area denotes the time window when the tax rebate question was asked (that is, from April 27 to July 23).

Panel A in Figure 3 shows that during 2008, mean life satisfaction was trending down. During the time when the tax rebate question was asked, average life satisfaction fluctuated between 6.7 and 6.8. For life satisfaction in 2011, the scale is different, because in 2011 satisfaction with life was overall much higher. Note that in 2011, the data only range from about 6.9 to about 7.1.

Panel B shows that mean net affect behaves differently over time than mean life satisfaction. Except for an increase around the Fourth of July, it remains relatively constant during the period in which the tax rebate question is asked. For net affect, the data range and the behavior over time is not as different between 2008 and 2011 as for life satisfaction.

Panel C shows the time path of mean health satisfaction (this question was discontinued in the latter part of 2008 and so is not reported for 2011). Mean health satisfaction is also relatively constant, with about 0.82–0.83 of the sample expressing satisfaction with their health. I return to the discussion of the time-series properties of SWB measures in Section V.A, where I plot the time series of the three SWB measures separately for rebate recipients and nonrecipients.

IV. METHODS

I begin by discussing the OLS estimator and, given the data, its limitations in estimating the effect of tax rebate receipt on SWB. In light of these limitations, I then discuss how to correct the analysis using a two-stage least squares estimator.

A. Ordinary Least Squares Estimator

The tax rebates were distributed among the rebate-eligible population using a quasi-randomized payment schedule, in which the date on which one received a rebate depended on the last two digits of one's SSN. Hence, the treatment is the receipt of the rebate now as opposed to later because, in the end, all eligible recipients will receive a rebate. An ideal comparison group for estimating the effect of this treatment would be the future recipients, that is, the respondents who, because of the exogenous variation in the timing of rebate disbursement, have not yet received the rebate but will. The comparison between the recipients and this comparison group could be accomplished by estimating the following model using OLS:

$$(1) \qquad SWB_{it} = \alpha + \delta TR_{it} + u_{it},$$

where i indexes the respondents and t the day when the survey is collected. SWB denotes a measure of either life satisfaction, net affect, or health satisfaction; TR equals one if the

respondent had received a tax rebate by day t and zero otherwise; u denotes i's unobservable traits. The coefficient of interest is δ, the difference between the SWB of someone who had received a rebate by day t and someone who had not (but would in the future).

The identifying assumption of this model is that whether a respondent has received a rebate or not by day t is independent of any individual characteristics: $E(u|TR) = 0$. If so, the OLS estimator of δ is a consistent estimate of the causal effect of the tax rebate receipt on SWB. There are, however, reasons why the OLS estimator might not be consistent.

Given the way the tax rebate question is phrased in the survey, a respondent could say he or she had not received a rebate for either of two reasons: a respondent may have been eligible for a rebate but due to randomization had not yet received the rebate, or the respondent may have been ineligible because of high income in the previous year and would never receive a rebate. Including respondents who will never receive a rebate in the comparison group will likely violate the identifying assumption $E(u|TR) = 0$, because these ineligibles differ from recipients in many ways, some unobserved. Including individuals who answer "no" because they are ineligible (and will never receive the rebate) will lead to understating true proportion of respondents who had received a rebate by day t (as evident from Figure 2), and this will attenuate the OLS estimate of δ.

To address this measurement problem, as a first step, I exclude all respondents whose individual-equivalent monthly gross income is equal to or exceeds $6,000. As mentioned in Section III.A, I choose this cutoff because it is close to the tax rebate phase-out region. Second, I account for observable differences between recipients and nonrecipients by including a vector of controls, described in the next subsection. Third, I correct the bias in the OLS estimate of δ by constructing and applying an instrumental variable (IV). Intuitively, when using the IV technique

to identify the effect of rebate receipt on SWB, the identifying variation in TR will consist of the variation that can be changed by the instrument. Specifically in this case, to identify the effect of rebate receipt on SWB, I need to apply IV to isolate the variation in TR in the sample consisting of recipients and future recipients, that is, people who have not yet received the rebate but will. Below, I discuss the instrument and its limitations in more detail.

B. Two-Stage Least Squares Estimator

In order to construct a valid instrumental variable (IV), one needs a variable that is correlated with TR, but not with the regression error, u. Had the data included information on SSNs, then these could have been used as instruments for TR. Instead, a coarser IV is constructed by using data on tax rebate payments reported by the DTS.

The rationale for using the DTS data as an instrument is that 1) it only uses the variation in TR that is induced by the exogenously determined schedule of payments used by the Treasury and hence is plausibly uncorrelated with the regression error term u, and 2) it is a relevant IV, as the daily tax rebate payments issued by the Treasury strongly predict the day-by-day likelihood of tax rebate receipt.

Formally, I estimate the model using a two-stage least squares (2SLS) estimator, given by the two-equation system (2):

$$(2) \qquad SWB_{it} = \delta TR_{it} + x'_{it}\beta + \tau_{it} + \alpha_1 w + \alpha_2 w^2 + u_{it}$$

$$TR_{it} = \pi DTS_{it} + x'_{it}\theta + \tau_{it} + \gamma_1 w + \gamma_2 w^2 + v_{it},$$

where x is a vector of observables, τ is a vector of day-of-week dummies, w is a weekly time trend, DTS denotes the instrumental variable, and u and v are error terms. The instrument is constructed in the following way. For each statement from DTS on day t, I calculate the cumulative amount of dollars disbursed in rebate payments up to day t and divide this sum by the

entire aggregate dollar value of rebate payments made in accordance with the Economic Stimulus Act ($92.185 billion). This instrument, shown in Figure 2 as a time series, is the fraction of the population eligible for the tax rebates that have been sent a rebate by that day.

I use this between-day variation in the fraction of (potential) rebate recipients to instrument the survey indicator for whether a respondent has received a tax rebate on that day. In practice, this approach corrects measurement error by instrumenting the survey measure of rebate receipt with an aggregate measure of potential rebate receipt.[8]

Since rebate receipt is increasing over time, it is important to control for time trends. All of the specifications include a linear and square weekly time trend, w. The regressions also control for a vector of day-of-week dummies, τ. To account for observable differences between recipients and nonrecipients, I include the vector x, which consists of variables that are likely to be fixed with respect to rebate eligibility. The vector x includes a constant, a flexible polynomial in age, race/ethnicity indicators, gender, an indicator for whether the respondent is married, number of children younger than 18 years who live in the household, the respondent's highest level of educational attainment, and which state the respondent lives in.

For the main results, I present separate results where I do and do not condition the regressions on self-reported current monthly household income. This is because self-reported current income might increase mechanically as a result of the tax rebate payment and so could be endogenous. As it turns out, controlling for income matters little for the δ-coefficients.

[8] If the data from DTS contain some measurement error, it is likely that the measurement error in DTS and the measurement error in the survey are uncorrelated.

C. Limitations of the IV Estimator

A potential downside of using the DTS variable as an IV is that since it is collected at a more aggregate level than the survey question, using it as an instrument will result in a loss of efficiency in the 2SLS estimate of δ. Because of this problem, when the point estimates are large and imprecisely estimated, but robust to specification checks, I interpret the estimates as economically large. However, when the point estimates are large and imprecisely estimated, but sensitive to specification checks, I interpret the estimates as only weak evidence.

Another concern is that when using an IV to instrument a dichotomous variable classified with error (as is often the case with survey questions), the IV estimator, $\hat{\delta}_{IV}$, is biased upwards and is not a consistent estimator of δ; see Bound, Brown, and Mathiowetz (2001). In such case, the IV estimator is still valuable because it allows one to bound the true effect, which will lie between the attenuated OLS estimator and the IV estimator. However, because the variability in the GHDP tax rebate measure in Figure 2 is not in excess of what one might expect given the sample size, it is not likely that the IV estimator greatly overstates δ.

D. Effect over Time

Because of the way the tax rebate question is phrased, I observe whether a respondent has received a rebate, but not when he or she received it. Note that although each day a new random sample of respondents are interviewed, as time progresses, the likelihood of receiving the rebate at an earlier date than the date of the interview increases. Hence, although the likelihood of being interviewed regarding rebate receipt does not change over the time the tax rebate question is asked, I mechanically observe more tax rebate recipients the further out in time the survey is conducted. For example, in May, an average tax rebate recipient would have had the tax rebate for about two weeks, and in June, an average recipient would have had the tax rebate for about a

month. Therefore, the estimate of δ is an average of current and recent responses, which will understate the immediate effect of the tax rebate on SWB if there is a fade-out.

Early on in the rebate disbursement process, the time gap between when someone is interviewed and when that person received the payment is smaller (but so is the number of respondents who report receiving the rebate). To see if the results differ, I conduct a separate analysis for the first two months of the disbursement, April and May (note that May is the first full month of the disbursement).

V. RESULTS

Table 2 shows the descriptive statistics of the key variables for the main estimation sample using the 2008 GHDP data. On a scale from 0 (worst possible) to 10 (best possible), the average life satisfaction equals about 6.4. On average, positive affect dominates over negative affect. Finally, about 8 out of 10 respondents are satisfied with their health. These means are very similar to the averages reported in Deaton (2008) and Kahneman and Deaton (2010). Looking at the descriptive statistics separately by receipt status, Table 2 shows that nonrecipients are more likely to be older and nonwhite.[9] Recipients are more likely to be married, to have more children, to have had at least some college, and to have higher monthly household income. There is no statistical difference with respect to gender.

Some of the differences are consistent with determinants of eligibility for the rebate; for example, married respondents might be more likely to respond that they received a rebate

[9] Table 2 also shows that the GHDP tends to oversample older respondents. The survey includes sample weights that reweight the sample to match the national distribution in terms of age, gender, region, race, and education. Since the aim of the paper is not primarily descriptive, I do not use the sample weights in the analysis, but rather control for all of the above observables directly. Repeating the analysis using sample weights does not change the main results; these results are available from the author. For a discussion of the use of sample weights in regressions, see Deaton (1997), Angrist and Pischke (2009), and Solon, Haider, and Wooldridge (2015).

because a spouse has received it. As with many surveys, it is likely that some variables are measured with error. Combined, these differences in observable characteristics might bias estimates that compare recipients and nonrecipients; this provides a rationale for regression adjustment and for using the IV method. As a robustness check, in Section V.D, I also conduct a subgroup analysis where I stratify the sample along the observables for which recipients and nonrecipients differ.

A. Differences in SWB among Recipients and Nonrecipients over Time

To anticipate the main results, Figure 4 presents a time series of unadjusted daily averages of each of the three SWB measures in April, May, and June, stratified by whether the respondent has received the rebate (denoted by the thick solid line) or not (denoted by the thin solid line).[10] Each of the graphs also includes two vertical dashed lines. The first line denotes May 2, the date by which the first large batch of rebate deposits were made; the second line denotes May 16, the date by which the first large batch of rebate checks were sent.

The aim of showing these time series is to see whether, in the weeks after the rebate deposits or checks are made or sent out, there is a change in the time series of SWB of early rebate recipients (that is, the recipients in the first weeks of May) versus the SWB of nonrecipients. The period to the left and to the right of the first dashed line is particularly interesting, as it denotes the beginning of the 2008 tax rebate "natural experiment." However, because the SWB time series presented in Figure 4 are unadjusted with respect to preexisting differences between recipients and nonrecipients, it is important to keep in mind that the visual

[10] I omit the last three dates in July as they occur late during the disbursement. By late July, the majority of eligibles would have received a rebate.

difference in SWB in Figure 4 should not be interpreted as evidence of a causal effect of the rebate.

Starting with Panel A of Figure 4, it appears that the mean life satisfaction of rebate nonrecipients is relatively constant, but the mean life satisfaction of recipients first increases, then plummets, and then increases again. This behavior of life satisfaction, especially early on in the disbursement, is puzzling and (again, anticipating the conclusion), among the three SWB measures, the analysis pertaining to life satisfaction turns out to be the least robust.

Panel B of Figure 4 is different. Before May 2, there appears to be no difference in net affect between recipients and nonrecipients. After May 2, the net-affect time series for rebate recipients goes up, whereas the net-affect time series for nonrecipients stays relatively flat until the end of May and then decreases. The similarity of mean net affect of recipients and nonrecipients before May 2 and the later increase in the net affect of recipients early on in the disbursement could be consistent with a rebate effect. As I will show later, the estimates pertaining to net affect are the most robust estimates in the paper.

Finally, for health satisfaction, presented in Panel C, we see that recipients report better health satisfaction than nonrecipients even before May 2, but, again, this could be due to not accounting for group differences in Table 2.

B. Baseline Results

In this section, I proceed to estimate the causal effect of tax rebate receipt on SWB. Table 3 presents estimates of δ. As a way to gauge the extent of bias discussed in the previous section, I report estimates using both OLS and 2SLS, with and without controlling for demographics, and with and without controlling for household income dummies. For each of the three outcome variables—life satisfaction, net affect, and health satisfaction—the coefficients in the odd-

numbered columns show the OLS estimates of the effect of the tax rebate. Column (1) only controls for time trends and day-of-week dummies; column (3) additionally controls for demographics, and column (5) additionally controls for a vector of gross monthly household income dummies. Each even-numbered 2SLS column uses the same set of controls as the preceding odd-numbered OLS column but instruments the tax rebate dummy with the DTS instrument.

The OLS estimates show a small increase of about 0.05σ in life satisfaction, which decreases to about 0.02σ and loses precision once I control for income. In all odd-numbered columns, the OLS effect on net affect remains robust and precisely estimated at about 0.09–0.11σ. When using health satisfaction as an outcome, the probability of reporting satisfaction with health goes up by 0.01–0.03 and is significant at least at the 10 percent level.

The loss of precision of some of the OLS estimates is relevant because the OLS estimates can be thought of as a lower bound of the effect of the rebate, especially if the corresponding 2SLS point estimate is sizable but imprecise. From Table 3, it appears that only the OLS estimate using net affect as the outcome variable remains precise at a 5 percent significance level.

Turning to the 2SLS estimates, Table 3 shows that the estimates are greater in absolute value than the OLS estimates, which suggests that the OLS estimates are attenuated. The 2SLS point estimates show a 0.3σ increase in life satisfaction, a 0.6σ increase in net affect, and a 17–19 percentage point increase in health satisfaction. These point estimates are large; however, the 2SLS estimates are less efficient, and only the increase in net affect is statistically significant at a 10 percent significance level in all of the specifications. Below the 2SLS estimates, Table 3 shows the estimated π-coefficients from the 2SLS models in Equation (2) and *F*-test statistics of

the excluded DTS instrument.[11] The magnitude of the *F*-statistic is about 40, which is well above the Stock, Wright, and Yogo (2002) "rule-of-thumb" value of 10, making it a relevant instrument.

Table 4 complements Table 3 by showing robustness checks of the main results from Table 3. Row A in Panel A once again shows the OLS estimates from Table 3, column (5). These OLS estimates are included here to serve as a benchmark for the falsification tests conducted in Section V. Row B once again presents the 2SLS estimates from Table 3, column (6) and these 2SLS estimates serve as a benchmark for the robustness checks described in the paragraph below. Row C presents the reduced-form regressions of the 2SLS model from Table 3, column (6). The reduced-form regression is obtained by regressing each of the three SWB measures directly on the instrumental variable DTS, demographic variables, household income dummies, and time effects. The reduced-form estimates echo the 2SLS estimates in the sense that the reduced-form coefficient is only statistically significant when using net affect as an outcome variable.

In Table 4, Panel B, I examine how robust the main 2SLS results from Panel A, row B are to different specifications of the dependent variables. First, I use the whole year of the 2008 GHDP survey to net out the time trends from each of the three measures of SWB. The rationale for this specification check is to strengthen the claim that the results identify the effect of tax rebate receipt as opposed to a general time effect on SWB. First, using data for the year 2008, I regress each of the three measures of SWB on a full set of 52 week-of-year dummies and seven day-of-week dummies and save the residuals. Then, I use these residuals as outcome variables in the 2SLS model from Equation (2). This specification nets out the time trends in SWB across the

[11] Table A.2, in the appendix, reports other coefficients from the first-stage regressions.

entire year (as well as within-week variation in SWB), as opposed to holding constant the time effects observed only during the observation window when the tax rebate question was asked. Table 4, Panel B, row A presents the δ-coefficients from estimating Equation (2) on this residualized outcome (see "robustness check 1").

Second, I append the 2011 GHDP data to the 2008 data and regress life satisfaction and net affect (the health satisfaction question was not asked in 2011) on a full set of 52 week-of-year dummies, seven day-of-week dummies, and year dummies and also save the residuals. Then, I use these residuals as outcome variables in the 2SLS model from Equation (2). Because I use two years of data, this specification nets out the predictable seasonal variation in life satisfaction and net affect. Panel B, row B in Table 4 presents the δ-coefficients from estimating Equation (2) on this residualized outcome (see "robustness check 2").

In comparing the results in rows A and B of Panel B in Table 4 to the main 2SLS results in the upper panel, two points are worth making. First, when using net affect as the outcome variable, the estimated δ-coefficients remain robust at about 0.6σ. Second, when using life satisfaction, the estimated δ-coefficients in rows A and B are more sensitive to alternative specifications regarding time trends and seasonality. The lack of 2011 data on health satisfaction limits what can be said about seasonality effects for this outcome.

Finally, in Table 4, Panel B, row C, I conduct a placebo test of the reduced-form estimates. Specifically, I assign the 2008 DTS payments to the same dates in the 2011 GHDP data set and estimate a reduced-form regression for the 2011 life satisfaction and 2011 net affect.[12] Since there was no rebate disbursement in 2011, the aim of this regression is to show

[12] In the 2008 GHDP data, the category "Hispanic" was one of the five mutually exclusive race/ethnicity categories. The 2011 GHDP uses different categories, so that "Hispanic" is not a mutually exclusive category with respect to race. Hence, the race/ethnicity indicators are not directly comparable across 2008 and 2011 data; however,

that the 2011 reduced-form estimates are small and imprecise, so as to strengthen the argument that the 2008 reduced-form estimates show the effect of rebates. Row C shows that, for the regression using net affect as an outcome variable, the point estimate is small and imprecise.[13] For life satisfaction, the point estimate is comparable in absolute size to the 2008 reduced-form estimate, but is negative and insignificant.

Since the only δ-coefficients always precisely and robustly estimated are the ones in the regressions that use net affect as the dependent variable, in order to see whether anything can be learned from how the seven components of net affect react to the rebates, Table 5 shows the estimates of δ using each emotion (enjoyment, happiness, pain, worry, sadness, stress, anger) as a separate binary outcome variable. Table 5 presents point estimates obtained using OLS linear probability models and 2SLS, where each 2SLS estimate is shown below the OLS estimate. All of the regressions in Table 5 control for demographic variables and, as with Table 3, the table reports estimates from regressions that control for income dummies and for those that do not.

The OLS estimates suggest that receiving the tax rebate increased feelings of enjoyment and happiness and decreased daily feelings of pain, worry, sadness, stress, and anger. Turning to the 2SLS estimates, Table 5 shows that, relative to the baseline, all of the estimates are large; however, the increase in net affect in Table 3 is driven by the statistically significant decrease in the probability of reporting feelings of worry and stress by about 40 percentage points from a base of about 30 percent. This is a very large decrease, and this result is robust whether I control for household income dummies or not.

I control for them in the 2011 regressions. The results are virtually identical if I reestimate the 2008 and 2011 regressions without controlling for race/ethnicity.

[13] I randomly split the 2011 data in half, so that the sample equals on average 500 respondents per day. I keep data for the same period for which the 2008 rebate data was collected: April 27–July 1 and July 21–July 23, 2011.

C. Effects in April and May

Table 6 shows the point estimates of δ where the sample has been restricted to April and May, which incurs a loss of about 6,700 observations.

By and large, the OLS point estimates in Table 6 are similar to the OLS estimates reported in Table 3, which is estimated using the entire sample. With the exception of regressions using life satisfaction as an outcome variable, for which in column (4) the 2SLS point estimate is smaller than the preceding OLS point estimate, the 2SLS estimates of δ are greater than the OLS estimates. The 2SLS estimates in Table 6, are, however, smaller than the 2SLS estimates in Table 3 and never precisely estimated.

One reason why all of the point estimates are less precise could be that only 17 percent of the survey respondents in this restricted sample report that they received a rebate. The *F*-statistic test for weak instruments in Table 6 indicates that the instrument is about 30 and hence still relevant, but lower than the *F*-statistic used for the entire sample.

In addition to remarking on the lower precision, it is worth commenting on why the estimates are generally smaller than those reported in Table 3. If we believe that the effects should be stronger earlier on in the disbursement process, then we would expect the coefficients in April and May to be larger in absolute size than in Table 3 and this is not the case. However, the fact that the April and May regressions using life satisfaction as an outcome variable yield smaller and less precise estimates could result from the puzzling behavior of life satisfaction reported in Figure 4, which indicates that life satisfaction of recipients *decreased* after the rebates were sent. If so, then the results for life satisfaction estimated using the entire sample (reported in Table 3) are driven by the weaker identification that occurs in June and July and should be regarded as less convincing. On the other hand, the 2SLS coefficients on regressions

using net affect and health satisfaction as outcomes are also smaller, which is puzzling. Fortunately, for net affect, we can obtain a better understanding by looking at the effect of rebate receipt on the separate components of affect in April and May.

To do so, Table 7 repeats the analysis from Table 5 using the sample restricted to April and May. For stress and worry, both the OLS and 2SLS δ-coefficients are greater in absolute value than the corresponding OLS and 2SLS coefficients estimated using the overall sample in Table 5. For example, the 2SLS probability of reporting worry goes down by about 45 percentage points in Table 7, but in Table 5 it goes down by 40 percentage points. In Table 7, the 2SLS likelihood of reporting stress goes down by about 64 percentage points, but in Table 5 it goes down by 40 percentage points. This suggests that the reduction in negative feelings is stronger early on, but also that the fade-out of the effect is slow.

Compared to Table 5, the 2SLS δ-coefficients in Table 7 that use outcomes other than stress, worry, or sadness are either less negative (see anger and pain) or turn from positive to negative (see enjoyment and happiness). However, only the 2SLS δ-coefficients using stress or worry as outcomes are statistically different from zero. One speculation as to why we observe a decrease in positive affect is that this reaction is specific to the economic environment of 2008. Since the rebates were a part of the economic stimulus, they might have been a reminder of uncertain economic prospects, and although they alleviated stress and worry, they might have decreased positive affect as well. Because the δ-coefficients from models using negative affect as an outcome are more negative in April and May (in particular stress, worry, and sadness) but the δ-coefficients from models using positive affect as an outcome are also negative, together this contributes to the impact on net affect becoming marginally smaller. Hence, Table 7

demonstrates that, compared to positive affect, liquidity might have an asymmetrically stronger impact on negative affect.

D. Subsample Analysis

In this subsection, I begin by examining how robust the main 2SLS results in Table 3, column (6), are to alternative assumptions regarding the imputed income cutoff. Previous research (see Misra and Surico 2014) finds that the effect of the tax rebates on spending is relatively stronger for low-income groups; hence, one might expect the SWB of lower-income individuals to be more sensitive to income.

Rows A and B in Table 8 show the 2SLS δ-coefficients estimated using analysis samples defined by two different individual-equivalent income cutoffs: below or above $2,000 (but less than $6,000) in gross monthly income. When comparing the results in rows A in Table 8 to the main 2SLS results in column (6) of Table 3, it is worth noting that, when restricting the sample to relatively lower-income respondents, the estimated δ-coefficients are more positive than the corresponding estimates in Table 3. The point estimate is statistically significant for the model using net affect as an outcome, whereas the δ-coefficients on the other two outcome variables are large but not precisely estimated.[14] In row B, the estimation sample includes higher-income respondents, and the *F*-statistic is above 30 but, even for net affect, not statistically different from zero. Overall, the finding that lower-income respondents react more strongly to the rebates suggests the presence of binding liquidity constraints, although this test is only indirect.

As discussed previously, the descriptive statistics in Table 2 show that recipients and nonrecipients differ not only in terms of income but also in terms of age, race, marital status,

[14] The analysis sample used in row A is smaller and so the *F*-statistic from the *F*-test of weak instruments is close to the "rule-of-thumb" value of 10, which may put into question how informative these estimates are.

number of children, and educational attainment. Rows C through N in Table 8 check the sensitivity of the main 2SLS estimates in Table 3 to sample cuts along these characteristics. Hence, in row C of Table 8, I begin by restricting the sample to respondents that do not have children living with them in the household and reestimate the main 2SLS specifications from Table 3 for all three measures of SWB. Below, in row D, I do the same for respondents that have children, etc.

The following insights emerge. The δ-point estimates in rows C–N in Table 8 are only stable when using net affect as an outcome variable, whereas for life and health satisfaction, the coefficients are sensitive to sample cuts. For example, when using net affect as an outcome variable, for both men and women, the δ-coefficients are estimated to be about 0.6σ, although the estimates are not precise. If one compares the same rows to the estimates that use life satisfaction or health satisfaction as outcomes, one can see that the latter are often large but sensitive. In conclusion, only when I use net affect as the dependent variable are the estimated δ-coefficients robust to alternative specifications.

In Table 9, I present 2SLS estimates of δ from additional two-way sample cuts. First, I use the cutoff of whether individual-equivalent gross monthly income is below or above $2,000 (but less than $6,000). Second, for each income cutoff, I additionally stratify the sample by the same demographic indicators as used in Table 8.

Holding a respondent's characteristics fixed and comparing the δ-coefficients across the two panels, it is clear that the δ-coefficients are greater in absolute value for lower-income households: in panel A, δ is estimated to be around 2σ, while in panel B, δ is estimated often to be below 1σ and is never precise. Within each panel, the δ-coefficients are qualitatively very

similar; there are, however, two exceptions: in panel A, the estimates for the subgroups carrying the labels "is not white" and "is not married" are outliers with respect to the low IV *F*-statistics.[15]

E. Falsification Tests: False Outcomes and Randomized Placebo Tests

In this section, I conduct falsification tests of the proposed IV identification strategy. To do this, I use the demographic controls from Table 2 as "false outcomes" and reestimate the 2SLS model separately for each false outcome. The idea is that after I instrument tax rebate receipt, the rebates should not show a statistically significant "effect" on these outcomes.

The false-outcome analysis consists of 72 regressions using the following outcomes: each of the 50 states, each of the seven household income dummies, each of the five education-level dummies, each of the four race/ethnicity indicators, gender, an indicator for whether a person is married, an indicator for whether there are any children in the household, the number of children, an indicator for whether the respondent is 35 years or older, and the respondent's age in years.[16] All of the regressions control for day-of-week dummies and a linear and squared weekly trend. Figure 5 graphs the distribution of the *p*-values of the estimated "effect" of tax rebates on each of these false outcomes. The figure also shows the *p*-values associated with the 2SLS δ-coefficients from Table 3, column (2), which control for the same time effects as the false outcome regressions.

Figure 5 shows that the *p*-values associated with the estimated δ-coefficients on the false outcomes are smaller than 0.10 three times out of 72. These statistically significant "effects" occurred when estimating regressions that use the following outcome variables: an indicator for

[15] Note that in panel A, the IV *F*-statistic is greater than 10 in only one case ("is married").

[16] For categorical outcome variables, I always omit one category. For example, in the regressions using each state as an outcome variable, I omit the District of Columbia; for the income category regressions, I omit the highest income category; etc.

living in Connecticut (p-value = 0.03), an indicator for living in Kentucky (p-value = 0.05), and an indicator for the respondent being white (p-value = 0.05).

As a final falsification test, I conduct placebo randomization tests. To do this, I use a random-number generator to reassign tax rebate receipt at random, and then, conditional on the observables x and time effects, I reestimate the effect of the placebo receipt on SWB. First, for each respondent in my estimation sample, I randomly generate a number between zero and one. Second, I create a placebo rebate receipt dummy variable that equals one if, on day t, this random number is less than the mean value of the DTS tax rebate receipt on day t and zero otherwise. Hence, on any given day, the mean value of the placebo rebate receipt dummy has the same value as the mean value of the "true" DTS measure on that day. I repeat this procedure 1,000 times and record the values of the placebo δ-coefficients. I do this for the 2008 analysis sample as well as for data from 2011. For 2011, I randomly split the data in half, so that the sample equals on average 500 respondents per day. The estimates from this model correspond the closest to the OLS δ-coefficients from Table 4, Panel A, row A, which, as argued in Section V.B, can be thought of as a lower bound of the effect.

Figure 6 graphs the distribution of these placebo δ-coefficients using life satisfaction (in Panel A), net affect (in Panel B), and health satisfaction (in Panel C) as outcome variables for the years 2008 and 2011. For each outcome, the dashed vertical line corresponds to the OLS δ-coefficient from Table 4, Panel A, row A. Note that the health satisfaction question was discontinued in the latter part of 2008 and was not asked in 2011; therefore, the placebo analysis is only done for 2008.

Figure 6 shows that the distribution of placebo δ-coefficients is centered at zero. The results from Figure 6 echo the results from Table 4, Panel A, row A in that the simulated

distribution excludes these OLS δ-coefficients for the model using net affect as an outcome, but not for the model using life satisfaction. That the simulated distribution of δ-coefficients for life satisfaction in 2011 does not exclude the life satisfaction OLS δ-coefficient could reflect the findings from Table 4, where the 2011 reduced-form estimate in the model of life satisfaction was sizable. In conclusion, the falsification tests suggest that the IV identification strategy is sound and the 2SLS estimator is useful in identifying the effect of tax rebate receipt on net affect.

VI. DISCUSSION

The most robust result of the paper is that tax rebate receipt increased net affect by between 0.1σ (estimated by OLS) and 0.6σ (estimated by 2SLS) and that this increase is due to a decrease in stress and worry. For the remaining outcome variables, life satisfaction and health satisfaction, although the δ-coefficients estimated using 2SLS are often large, they are not robust and are mostly imprecise. Therefore, in subsection A, I discuss the economic meaning of the increase in affect. In subsection B, I discuss the mechanism behind the increase in affect and policy implications.

A. Interpretation of Effect Sizes

To gauge the size of the impact on net affect, consider first Lindahl (2005), who finds that the index of self-assessed health increases by 0.037–0.045σ for lottery winners who, on average, won about $550 per win and year.[17] In comparison, a point estimate in the range of 0.1–0.6σ represents a large increase. Second, in order to place my estimate in a different context, consider that in my sample, the net affect of married respondents is about 0.20σ higher than the

[17] As I am not aware of studies using exogenous variation in income to study its impact on affect, I relate these findings to papers looking at the effect of income on self-reported health, which is a strong correlate of SWB.

net affect of respondents who are not married (not shown).[18] Taken at face value, this suggests that a one-time payment of about $600–$1,200 buys more emotional well-being than marriage.

At least one reason why my point estimate is large is that the responses are measured shortly after the respondent received the rebate. In contrast, Lindahl's data on self-reported health is collected years after the respondent had won the lottery. By the same token, the positive association between affect and marriage is typically greater earlier in a relationship, whereas I observe a lower, "steady-state" marriage-affect premium.[19]

B. Discussion of the Main Results and Policy Implications

The results show that the rebates improved affect and that this estimated effect is stronger for low-income than for high-income respondents. Through what mechanisms might this improvement have occurred? One possibility is that the rebates led to increased consumption spending (see the citations in footnote 2), which in turn improved affect. But according to (Parker et al. 2013), households consumed only 12–30 percent of their rebates on average, and it seems unlikely that the estimated improvement in affect could be explained entirely by such a small increase in consumption.[20]

Another possibility is that rebate receipt had a direct impact on affect. Research has shown that merely inducing people to think about money can have immediate behavioral consequences, possibly because thinking about money causes a change in their emotional well-being (Mani et al. 2013). Hence, perhaps simply receiving the rebate alleviated stress and worry

[18] Krueger et al. (2009) list marriage as a robust correlate of SWB. The meta-analysis conducted by Lyubomirsky, King, and Diener (2005) shows that 0.20 is a typical effect-size estimate of the marriage-affect premium.

[19] Rather than affect, Stutzer and Frey (2006) study life satisfaction as a function of length of marriage and show that it peaks at the time of marriage.

[20] DeLeire and Kalil (2010) estimate that an additional $10,000 in (leisure) spending increases SWB by only 0.14σ.

even if the recipients did not utilize most of the payment. Regardless of the mechanism behind the rebates' influence on affect, it is presumably temporary, and one might ask why these results matter.

Fluctuations in affect can have implications for economic decision making. For example, using a random-assignment laboratory experiment, Ifcher and Zarghamee (2011) show that positive affect has a causal effect on patience. Finding that positive affect impacts time preferences has implications for policy pertaining to low-income populations. For example, welfare recipients have been shown to be relatively present-biased; see, for example, Shapiro (2005) for a study of the consumption-smoothing behavior of food stamp recipients, and Mastrobuoni and Weinberg (2009) for similar evidence focusing on low-income Social Security recipients. Both of these papers conclude that the behavior of the populations they study is either consistent with extreme impatience or with a model of hyperbolic discounting. Given these findings, a welfare-improving policy would either increase the frequency of benefit payments or would consider policies that increase the patience of benefit recipients.

If policymakers are interested in increasing the patience of benefit recipients by improving affect, then more research is needed to understand what determines affect and, in turn, how affect impacts behavior. Ifcher and Zarghamee's (2011) research on the link between affect and patience is related to the literature on how affect can impact economic decision making in general. Recent work in behavioral economics has argued that, for low-income people, binding liquidity constraints may perpetuate distress and limit the capacity of individuals to make informed economic decisions. For example, Mani et al. (2013) show that experimentally inducing low-income people to think about a hypothetical financial problem leads to a decrease in their cognitive abilities. Mullainathan and Shafir (2013) discuss the results of this study and

draw broader implications for the effects of liquidity constraints. The authors hypothesize that reminding low-income subjects about money matters may reduce their cognitive capacity because it increases distress (p. 63), which in turn limits the subject's capacity for processing problems. Support for this is also echoed in other psychological research. Isen (2001) states in her literature review that "positive affect enhances problem solving and decision making."[21]

If relaxing liquidity constraints increases affect, and if this increase in affect can in turn improve economic decision making, then the estimates suggest that the rebates may have had an as yet unaccounted-for benefit. An extension of this research would be to examine the effects of policies such as the Temporary Assistance for Needy Families or the Earned Income Tax Credit on affect. Future research should also focus on gaining a better understanding of the mechanisms that generate the interdependency between income, affect, and economic behavior.

VII. CONCLUSION

This paper uses the 2008 tax rebate payments to estimate the causal effect of a one-time, medium-sized change in income on subjective well-being, measured by questions on satisfaction with life and health as well as questions on affect/emotional well-being. In order to estimate this effect, I use the exogenous timing of tax rebate disbursement, which depends on the last two digits in the tax filer's Social Security number. The randomized schedule of the rebate payments allows me to estimate the causal effect of receiving a payment now as opposed to later.

[21] Another conceivable mechanism is that additional liquidity might make people engage in risky behaviors, such as more drinking and smoking; see Mullainathan and Shafir (2013, Chapter 3) and Apouey and Clark (2014). At the extreme, income shocks have been shown to increase mortality, especially because of substance abuse; see Evans and Moore (2011, 2012). Since GHDP includes data on self-reported smoking, I reestimated the main model for self-reported smokers; however, this analysis did not suggest that smokers were driving the main results.

I estimate this effect on subjective well-being using both ordinary least squares and two-stage least squares methods, where the latter only uses exogenous variation in the schedule of tax rebate disbursement. Using the preferred two-stage least squares estimator, I find that the rebates increased affect by 0.60 of a standard deviation, which is a large gain. This increase is due to a statistically significant and large decrease in stress and worry. The increase in affect is stronger for low-income people. On the other hand, the estimated effects on satisfaction with life and satisfaction with health are positive and sizable, but not statistically different from zero when estimated using the two-stage least squares estimator, and not robust to alternative specifications.

That the tax rebates had an impact on reducing stress and worry is interesting because research in behavioral economics has argued that better emotional well-being, at least in the short run, can increase patience, improve the short-run ability to make informed economic decisions, and strengthen cognitive capacity. Interestingly, low-income people in particular seem to benefit from better emotional well-being. An interesting question for future research is how long these positive effects of improved emotional well-being last. Another extension, which given the data limitations is beyond the scope of this research, should study more fully the mechanisms through which liquidity and emotional well-being can improve economic decision making.

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RESULTS

Table 1 Payment Schedule of the 2008 Tax Rebates

Last two digits of SSN	Tax rebate check received by	Last two digits of SSN	Tax rebate deposit made by
00–09	May 16	00–20	May 2
10–18	May 23		
19–25	May 30	21–75	May 9
26–38	June 6		
39–51	June 13		
52–63	June 20		
64–75	June 27		
76–87	July 4	76–99	May 16
88–99	July 11		

Table 2 Mean (and Standard Deviation) of Characteristics of the Sample

Variable	Received tax rebate? All	Yes	No
Subjective well-being			
Life satisfaction	6.44	6.49	6.42
	(2.08)	(1.96)	(2.13)
Net affect[a]	0.52	0.54	0.50
	(0.40)	(0.39)	(0.41)
Enjoyment	0.85	0.86	0.84
Happiness	0.88	0.90	0.88
Pain	0.26	0.23	0.27
Worry	0.30	0.28	0.31
Sadness	0.18	0.15	0.19
Stress	0.36	0.35	0.37
Anger	0.12	0.12	0.13
Health satisfaction	0.80	0.82	0.79
Received tax rebate	0.32	1.00	0.00
Age	53.20	51.54	53.98
	(16.96)	(16.23)	(17.23)
No. of children < 18 years	0.60	0.66	0.57
	(1.08)	(1.08)	(1.08)
Married	0.54	0.60	0.52
Woman	0.52	0.52	0.53
Race/ethnicity			
Other	0.04	0.03	0.05
White	0.84	0.87	0.82
African-American/Black	0.08	0.06	0.08
Hispanic	0.03	0.02	0.04
Asian	0.01	0.01	0.01
Observations	17,933	5,702	12,231

Variable	Received tax rebate? All	Yes	No
Highest completed level of education			
Less than high school diploma	0.06	0.04	0.07
High school degree or diploma	0.24	0.23	0.24
Technical/vocational school	0.09	0.08	0.09
Some college	0.27	0.28	0.27
College graduate	0.20	0.23	0.19
Postgraduate work or degree	0.14	0.14	0.14
Region[b]			
East	0.22	0.22	0.22
Midwest	0.24	0.26	0.22
South	0.31	0.30	0.31
West	0.24	0.22	0.25
Gross monthly household income (in percent)			
Under $60	0.24	0.14	0.29
$60 to $499	1.52	1.02	1.76
$500 to $999	7.28	5.00	8.34
$1,000 to $1,999	18.94	15.57	20.51
$2,000 to $2,999	19.67	18.52	20.20
$3,000 to $3,999	17.28	18.64	16.65
$4,000 to $4,999	15.14	16.94	14.30
$5,000 to $7,499	19.94	24.17	17.96

NOTE: Author's calculations from the Gallup-Healthways Daily Poll. Universe: Imputed individual-equivalent gross monthly income of less than $6,000; April 27–July 1; July 21–23, 2008.

a. Defined as the average positive affect (enjoyment; happiness) minus average negative affect (physical pain; worry; sadness; stress; anger).

b. In the regression analysis, I control for the state where the respondent lives. Here, because of space considerations, I present regions.

Table 3 Estimated Effects of Tax Rebate Receipt on Subjective Well-Being

Outcome variable	Life satisfaction					
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS
Covariates						
Received tax rebate[a]	0.055***	0.316	0.051***	0.302	0.023	0.317
	(0.017)	(0.379)	(0.017)	(0.359)	(0.017)	(0.356)
Time controls?	Yes	Yes	Yes	Yes	Yes	Yes
Demographics?	No	No	Yes	Yes	Yes	Yes
Household income dummies?	No	No	No	No	Yes	Yes
Outcome variable	Net affect					
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS
Covariates						
Received tax rebate[a]	0.119***	0.664*	0.118***	0.627*	0.092***	0.638*
	(0.018)	(0.381)	(0.018)	(0.363)	(0.017)	(0.361)
Time controls?	Yes	Yes	Yes	Yes	Yes	Yes
Demographics?	No	No	Yes	Yes	Yes	Yes
Household income dummies?	No	No	No	No	Yes	Yes
Outcome variable	Health satisfaction					
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS
Covariates						
Received tax rebate[a]	0.034***	0.194	0.021***	0.168	0.012*	0.173
	(0.007)	(0.154)	(0.007)	(0.148)	(0.007)	(0.148)
Time controls?	Yes	Yes	Yes	Yes	Yes	Yes
Demographics?	No	No	Yes	Yes	Yes	Yes
Household income dummies?	No	No	No	No	Yes	Yes
First stage regression	Received tax rebate					
Instrumental variable (IV)[b]		0.518***		0.528***		0.527***
		(0.080)		(0.079)		(0.078)
IV F-statistic		42.24		45.24		45.23
Observations	17,933	17,933	17,933	17,933	17,933	17,933

NOTE: Universe: Imputed individual-equivalent gross monthly income less than $6,000; April 27–July 1; July 21–23, 2008. Robust standard errors are in parentheses (*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$).

a. Each column labeled OLS shows the estimate of δ from Equation (1), and each column labeled 2SLS shows the estimate of δ from Equation (2). All of the regressions control for day-of-week dummies and a linear and squared weekly trend. Selected regressions control for demographics (age, age squared, age cubed, race/ethnicity, gender, number of children less than 18 years, an indicator for whether the respondent is married, highest completed level of education, and the state where the respondent lives) and monthly household gross income dummies.

b. The instrumental variable, DTS, is the amount of cumulative tax rebate payments issued by the U.S. Treasury up to a given day divided by all tax rebate payments made in 2008. The F-test statistic is from an F-test of weak identification.

Table 4 Robustness Checks of the Main Results

PANEL A: OLS and 2SLS effects of tax rebate receipt and reduced-form estimates

Outcome variable	Life satisfaction	Net affect	Health satisfaction
A. OLS estimates, year 2008	0.023	0.092***	0.012*
	(0.017)	(0.017)	(0.007)
B. 2SLS estimates, year 2008 (main estimates)	0.317	0.638*	0.173
	(0.356)	(0.361)	(0.148)
C. Reduced-form estimates, year 2008	0.167	0.336*	0.091
	(0.186)	(0.186)	(0.077)

NOTE to PANEL A: Universe: see notes to Table 3. Robust standard errors are in parentheses (*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$). Each cell in rows A–C corresponds to a different regression. Row A replicates the OLS estimate of δ from Table 3, column (5), and Row B replicates the 2SLS estimate of δ from Table 3, column (6). Row C shows the reduced-form estimates from the 2SLS model in Equation (2). All of the regressions control for day-of-week dummies, a linear and squared weekly trend, demographics listed in the notes to Table 3, and monthly gross household income dummies. The instrumental variable, DTS, is the amount of cumulative tax rebate payments issued by the U.S. Treasury up to a given day, divided by all tax rebate payments made in 2008. The F-test statistic, from an F-test of weak identification, equals 45.23.

PANEL B: Robustness checks of 2SLS effects of tax rebate receipt and placebo reduced-form estimates

Outcome variable	Life satisfaction	Net affect	Health satisfaction
A. 2SLS, week-of-year dummies netted out, year 2008 (robustness check 1)	−0.049	0.641*	0.138
	(0.353)	(0.363)	(0.147)
B. 2SLS, week-of-year dummies netted out, years 2008 and 2011 (robustness check 2)	0.112	0.622*	0.138
	(0.353)	(0.362)	(0.147)
C. Reduced-form estimates, year 2011 (placebo test)	−0.182	0.024	—
	(0.251)	(0.252)	—

NOTE to PANEL B: Robust standard errors are in parentheses (*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$). Each cell in rows A–C corresponds to a different regression. All of the regressions control for day-of-week dummies, a linear and squared weekly trend, demographics listed in the notes to Table 3, and monthly household income dummies. Row A uses outcome variables that have been residualized with respect to a full year of 52 week-of-year dummies for year 2008 and day-of-week dummies. Row B uses outcome variables that have been residualized with respect to a full year of 52 week-of-year dummies for years 2008 and 2011, day-of-week dummies, and year dummies. Note that data for health satisfaction was only collected in 2008; hence for health satisfaction, rows A and B are the same. The instrumental variable, DTS, is the amount of cumulative tax rebate payments issued by the U.S. Treasury up to a given day, divided by all tax rebate payments made in 2008. The F-test statistic from an F-test of weak identification equals 45.23. Row C shows the reduced-form estimates from the 2SLS model in Equation (2), where the 2008 DTS payment dates have been assigned to the 2011 GHDP data.

Table 5 Estimated Effects of Tax Rebate Receipt on Components of Daily Affect

		Regression controls for income?			
Outcome variable	Method	No[a]		Yes[b]	
Enjoyment	OLS	0.023***	(0.006)	0.018***	(0.006)
Enjoyment	2SLS	0.078	(0.133)	0.080	(0.133)
Happiness	OLS	0.024***	(0.006)	0.019***	(0.006)
Happiness	2SLS	0.075	(0.119)	0.077	(0.119)
Pain	OLS	−0.028***	(0.008)	−0.017**	(0.008)
Pain	2SLS	−0.163	(0.162)	−0.169	(0.161)
Worry	OLS	−0.052***	(0.008)	−0.042***	(0.008)
Worry	2SLS	−0.407**	(0.173)	−0.411**	(0.173)
Sadness	OLS	−0.038***	(0.007)	−0.031***	(0.007)
Sadness	2SLS	−0.026	(0.140)	−0.029	(0.140)
Stress	OLS	−0.042***	(0.009)	−0.035***	(0.008)
Stress	2SLS	−0.398**	(0.179)	−0.401**	(0.179)
Anger	OLS	−0.013**	(0.006)	−0.010*	(0.006)
Anger	2SLS	−0.168	(0.123)	−0.170	(0.123)
IV F-statistic[c]		45.24		45.23	
Observations		17,933		17,933	

NOTE: Universe: Imputed individual-equivalent gross monthly income of less than $6,000, April 27–July 1; July 21–23, 2008. Robust standard errors are in parentheses (*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$).

a. Each row labeled OLS shows the estimate of δ from Equation (1), and each row labeled 2SLS shows the estimate of δ from Equation (2). All of the regressions control for day-of-week dummies and a linear and squared weekly trend. All of the regressions control for demographics listed in the notes to Table 3.

b. Same as a., but regressions also control for monthly household gross income dummies.

c. The instrumental variable, DTS, is the amount of cumulative tax rebate payments issued by the U.S. Treasury up to a given day, divided by all tax rebate payments made in 2008. The F-test statistic is from an F-test of weak identification.

Table 6 Estimated Effects of Tax Rebate Receipt on Subjective Well-Being in April and May

Outcome variable	Life satisfaction					
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS
Covariates						
Received tax rebate[a]	0.054**	0.197	0.057**	0.020	0.032	0.078
	(0.027)	(0.552)	(0.027)	(0.557)	(0.026)	(0.544)
Time controls?	Yes	Yes	Yes	Yes	Yes	Yes
Demographics?	No	No	Yes	Yes	Yes	Yes
Household income dummies?	No	No	No	No	Yes	Yes
Outcome variable	Net affect					
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS
Covariates						
Received tax rebate[a]	0.100***	0.654	0.110***	0.512	0.091***	0.572
	(0.026)	(0.549)	(0.026)	(0.557)	(0.026)	(0.550)
Time controls?	Yes	Yes	Yes	Yes	Yes	Yes
Demographics?	No	No	Yes	Yes	Yes	Yes
Household income dummies?	No	No	No	No	Yes	Yes
Outcome variable	Health satisfaction					
	(1) OLS	(2) 2SLS	(3) OLS	(4) 2SLS	(5) OLS	(6) 2SLS
Covariates						
Received tax rebate[a]	0.041***	0.139	0.025**	0.079	0.017	0.091
	(0.011)	(0.218)	(0.011)	(0.224)	(0.011)	(0.221)
Time controls?	Yes	Yes	Yes	Yes	Yes	Yes
Demographics?	No	No	Yes	Yes	Yes	Yes
Household income dummies?	No	No	No	No	Yes	Yes
IV F-statistic[b]		30.51		28.02		28.24
Observations	10,393	10,393	10,393	10,393	10,393	10,393

NOTE: Universe: Imputed individual-equivalent gross monthly income of less than $6,000, April 27–May 31, 2008. Robust standard errors are in parentheses (*** p < 0.01; ** p < 0.05; * p < 0.1).

a. Each column labeled OLS shows the estimate of δ from Equation (1), and each column labeled 2SLS shows the estimate of δ from Equation (2). All of the regressions control for day-of-week dummies and a linear and squared weekly trend. Selected regressions control for demographics listed in the notes to Table 3 and monthly household gross income dummies.

b. The instrumental variable, DTS, is the amount of cumulative tax rebate payments issued by the U.S. Treasury up to a given day divided by all tax rebate payments made in 2008. The F-test statistic is from an F-test of weak identification.

Table 7 Estimated Effects of Tax Rebate Receipt on Components of Daily Affect in April and May

		Regression controls for income?			
Outcome variable	Method	No[a]		Yes[b]	
Enjoyment	OLS	0.020**	(0.010)	0.016	(0.010)
Enjoyment	2SLS	−0.040	(0.199)	−0.033	(0.198)
Happiness	OLS	0.017*	(0.009)	0.013	(0.009)
Happiness	2SLS	−0.121	(0.183)	−0.107	(0.181)
Pain	OLS	−0.023*	(0.012)	−0.014	(0.012)
Pain	2SLS	−0.080	(0.250)	−0.103	(0.247)
Worry	OLS	−0.058***	(0.013)	−0.050***	(0.012)
Worry	2SLS	−0.450*	(0.269)	−0.480*	(0.269)
Sadness	OLS	−0.039***	(0.010)	−0.033***	(0.010)
Sadness	2SLS	−0.301	(0.223)	−0.319	(0.222)
Stress	OLS	−0.047***	(0.013)	−0.043***	(0.013)
Stress	2SLS	−0.642**	(0.289)	−0.663**	(0.289)
Anger	OLS	−0.013	(0.009)	−0.010	(0.009)
Anger	2SLS	−0.150	(0.189)	−0.158	(0.189)
IV F-statistic[c]		28.02		28.24	
Observations		10,393		10,393	

NOTE: Universe: Imputed individual-equivalent gross monthly income of less than $6,000; April 27–May 31, 2008. Robust standard errors are in parentheses (*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$).

a. Each row labeled OLS shows the estimate of δ from Equation (1), and each row labeled 2SLS shows the estimate of δ from Equation (2). All of the regressions control for day-of-week dummies and a linear and squared weekly trend. All of the regressions control for demographics listed in the notes to Table 3.

b. Same as a., but regressions also control for monthly household gross income dummies.

c. The instrumental variable, DTS, is the amount of cumulative tax rebate payments issued by the U.S. Treasury up to a given day, divided by all tax rebate payments made in 2008. The F-test statistic is from an F-test of weak identification.

Table 8 Robustness Checks: 2SLS Effects of Tax Rebate Receipt on Subjective Well-Being Using Different Sample Cuts

Outcome variable	Life satisfaction	Net affect	Health satisfaction
Sample restrictions: income — keep if respondent's…			
A. Gross monthly income[a] < $2,000	0.667	2.607**	0.604
	(0.928)	(1.169)	(0.401)
	[10.51]	[10.51]	[10.51]
B. Gross monthly income[a] ≥ $2,000	0.147	−0.147	−0.009
	(0.342)	(0.355)	(0.146)
	[35.61]	[35.61]	[35.61]
Sample restrictions: other characteristics — keep if the respondent…			
C. Has children	0.595	0.618	-0.058
	(0.464)	(0.473)	(0.179)
	[24.97]	[24.97]	[24.97]
D. Does not have children	0.221	0.714	0.339
	(0.502)	(0.508)	(0.220)
	[23.56]	[23.56]	[23.56]
E. Is white	0.073	0.631*	0.081
	(0.342)	(0.356)	(0.145)
	[43.88]	[43.88]	[43.88]
F. Is not white	1.875	0.325	0.804
	(1.829)	(1.487)	(0.728)
	[3.383]	[3.383]	[3.383]
G. Is married	0.307	0.332	0.186
	(0.320)	(0.313)	(0.135)
	[45.57]	[45.57]	[45.57]
H. Is not married	0.301	1.456	0.104
	(0.951)	(1.071)	(0.391)
	[7.781]	[7.781]	[7.781]
I. Has at least some college	0.121	0.712*	0.099
	(0.359)	(0.392)	(0.157)
	[34.96]	[34.96]	[34.96]
J. Does not have any college	0.838	0.367	0.325
	(0.853)	(0.777)	(0.330)
	[11.42]	[11.42]	[11.42]
K. Is younger than 35 years	−0.173	0.488	0.267
	(0.525)	(0.557)	(0.192)
	[16.71]	[16.71]	[16.71]
L. Is 35 years or older	0.532	0.732*	0.153
	(0.430)	(0.432)	(0.179)
	[32.99]	[32.99]	[32.99]
M. Is a woman	0.611	0.637	-0.023
	(0.466)	(0.468)	(0.190)
	[28.54]	[28.54]	[28.54]
N. Is a man	-0.114	0.647	0.457*
	(0.565)	(0.568)	(0.250)
	[16.71]	[16.71]	[16.71]

NOTE: Robust standard errors are in parentheses (*** p < 0.01; ** p < 0.05; * p < 0.1) and instrumental variable F-statistics are in brackets.

Each cell corresponds to a different regression and shows a 2SLS estimate of δ from Equation (2). All of the regressions control for day-of-week dummies, a linear and squared weekly trend, demographics listed in the notes to Table 3, and monthly household gross income dummies. Each row includes a different analysis sample. Rows A and B define the sample using different imputed individual-equivalent monthly gross income cutoffs. Rows C–N define the sample based on selected respondent characteristics. The instrument, DTS, is the amount of cumulative tax rebate payments issued by the U.S. Treasury up to a given day, divided by all tax rebate payments made in 2008.

a. "Gross monthly income" refers to the imputed individual-equivalent gross monthly income; see the text for details.

Table 9 Robustness Checks: 2SLS Effects of Tax Rebate Receipt on Net Affect Using Sample Cuts by Income and Respondent Characteristics

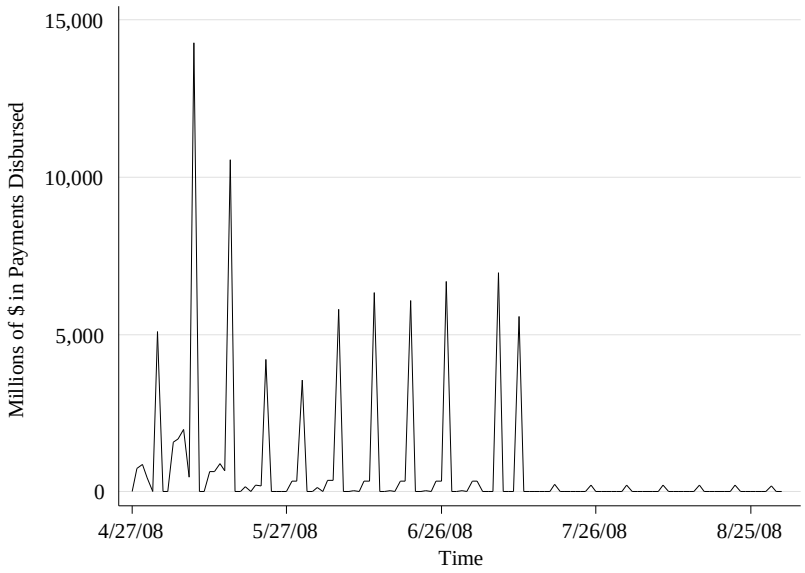
Keep if the respondent:	Has children	Does not have children	Is white	Is not white	Is married	Is not married	Has at least some college	Does not have any college	Is younger than 35 years	Is 35 years or older	Is a woman	Is a man
PANEL A: Keep if respondent's monthly gross income[a] < $2,000												
Received tax rebate	2.837*	2.215	2.329**	2.179	0.785	9.673	1.844*	2.652	1.826*	2.941	3.045*	1.437
	(1.486)	(1.453)	(1.074)	(3.879)	(0.554)	(13.478)	(1.035)	(2.008)	(1.004)	(1.851)	(1.791)	(1.220)
IV F-statistic[b]	[5.99]	[5.12]	[9.37]	[0.832]	[17.22]	[0.538]	[8.26]	[3.32]	[8.59]	[4.22]	[4.68]	[5.34]
PANEL B: Keep if respondent's monthly gross income[a] ≥ $2,000												
Received tax rebate	−0.451	−0.004	−0.118	−0.454	0.129	−0.583	0.219	−1.324	−0.817	0.052	−0.440	0.102
	(0.591)	(0.552)	(0.409)	(1.511)	(0.417)	(0.908)	(0.452)	(0.988)	(0.887)	(0.432)	(0.523)	(0.670)
IV F-statistic[b]	[16.83]	[18.88]	[33.69]	[2.841]	[27.72]	[9.351]	[26.45]	[8.644]	[7.327]	[31.37]	[23.30]	[11.52]

NOTE: Robust standard errors are in parentheses (*** p < 0.01; ** p < 0.05; * p < 0.1). Each cell corresponds to a different regression and shows a 2SLS estimate of δ from Equation (2). The dependent variable is net affect. All of the regressions control for day-of-week dummies, a linear and squared weekly trend, and demographics listed in the notes to Table 3, including monthly household gross income dummies. Each column restricts the sample based on respondent characteristics. Panels A and B define the sample using a different imputed individual-equivalent monthly gross income cutoff.

a. "Gross monthly income" refers to the imputed individual-equivalent gross monthly income; see the text for details.

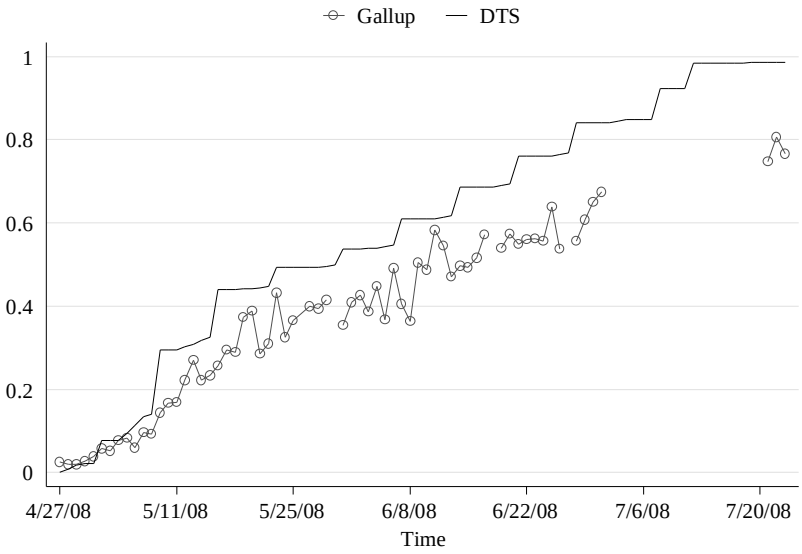
b. The instrumental variable, DTS, is the amount of cumulative tax rebate payments issued by the U.S. Treasury up to a given day, divided by all tax rebate payments made in 2008. The F-test statistic is from an F-test of weak identification.

Figure 1 Disbursement of the 2008 Tax Rebates



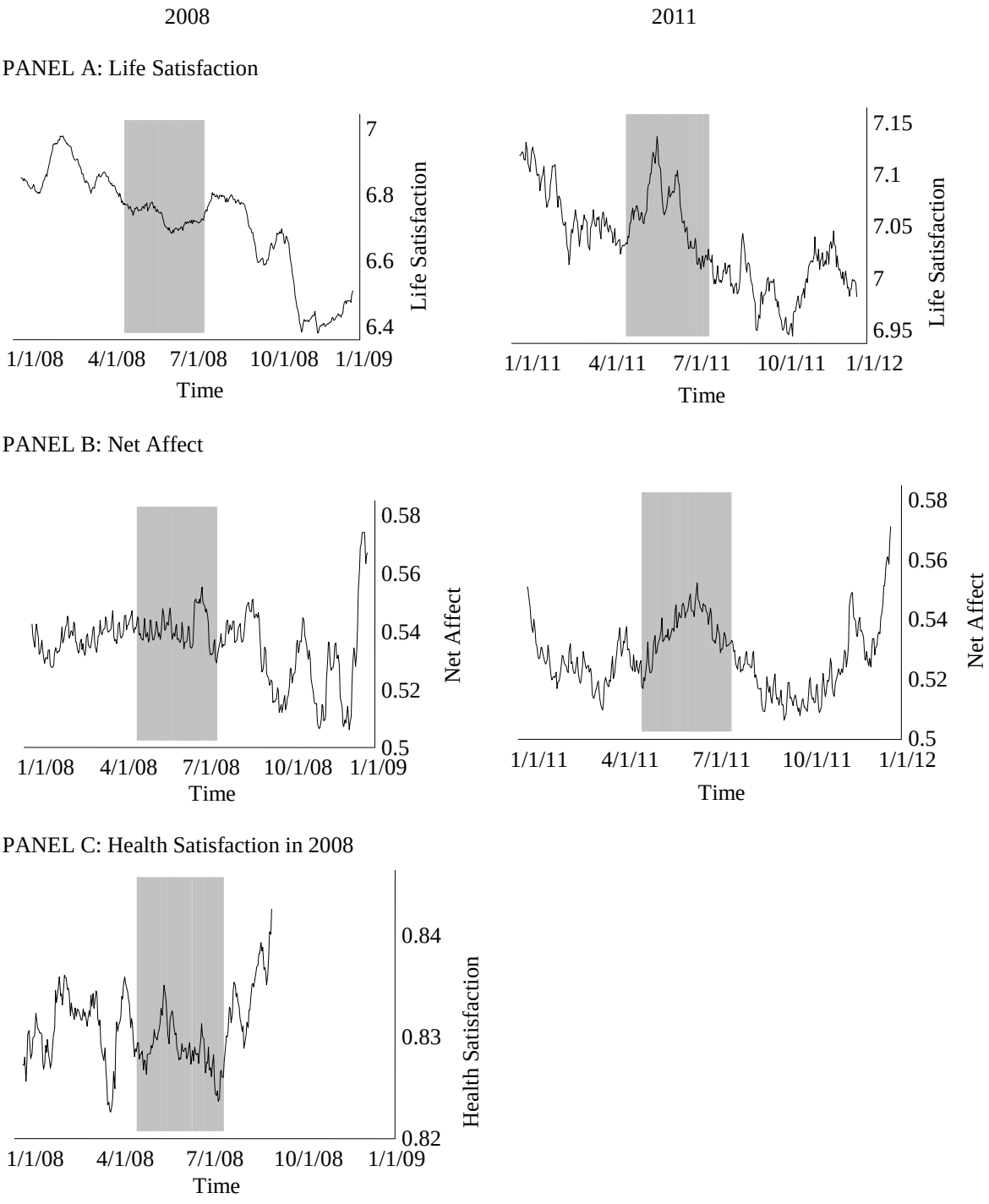
SOURCE: Daily Treasury Statements: http://www.fms.treas.gov/dts/index.html.

Figure 2 Fraction of Tax Rebate Recipients in the Gallup-Healthways Daily Poll and Daily Treasury Statements (DTS)



NOTE: Universe for the Gallup series consist of respondents with imputed individual-equivalent gross monthly income of less than $6,000; April 27–July 1 and July 21–23, 2008. The DTS time series is computed by dividing the cumulative number of $ millions of payments made by a given date by the $92.185 billion in total payments made by August 31, 2008. Note that the survey was not conducted on May 30, June 18, and June 27.

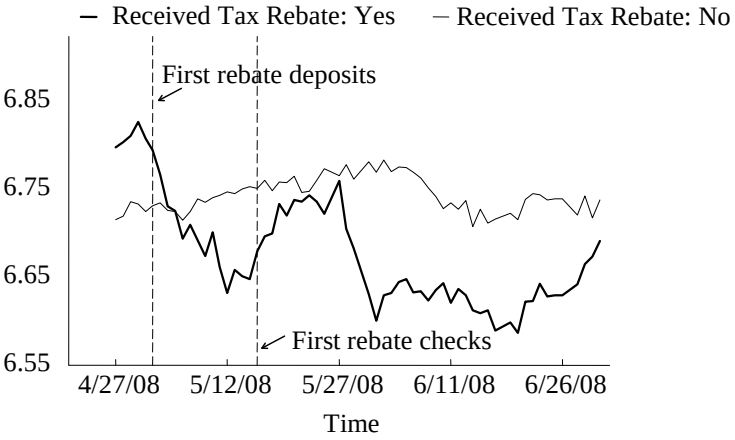
Figure 3 Measures of Subjective Well-Being in 2008 and 2011



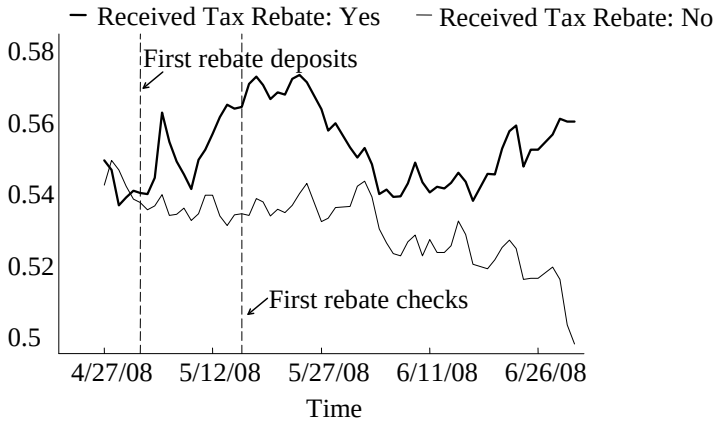
NOTE: Universe consists of all GHDP respondents. Each panel shows 14-day moving averages. For 2008, the shaded area denotes the period when the GHDP question on tax rebates was asked (April 27–July 23). For 2011, the shaded area denotes the same period. Life satisfaction is measured on an 11-point scale from zero to 10. Net affect is calculated as the average positive affect minus average negative affect. Health satisfaction is a 0/1 question (it was not asked throughout 2008 and not in 2011).

Figure 4 Measures of Subjective Well-Being in April, May, and June, by Tax Rebate Recipiency

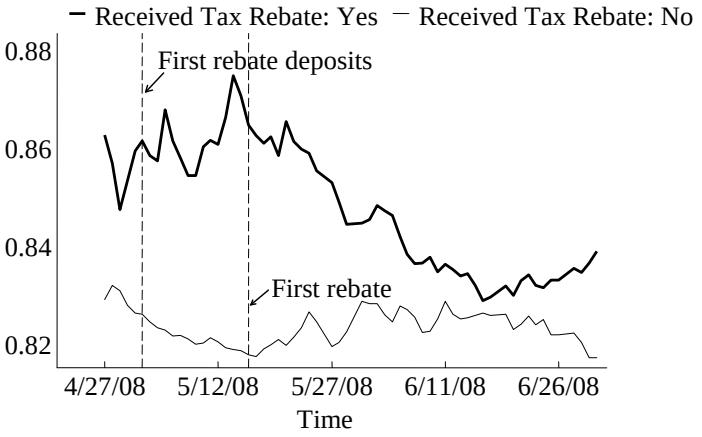
PANEL A: Life Satisfaction



PANEL B: Net Affect

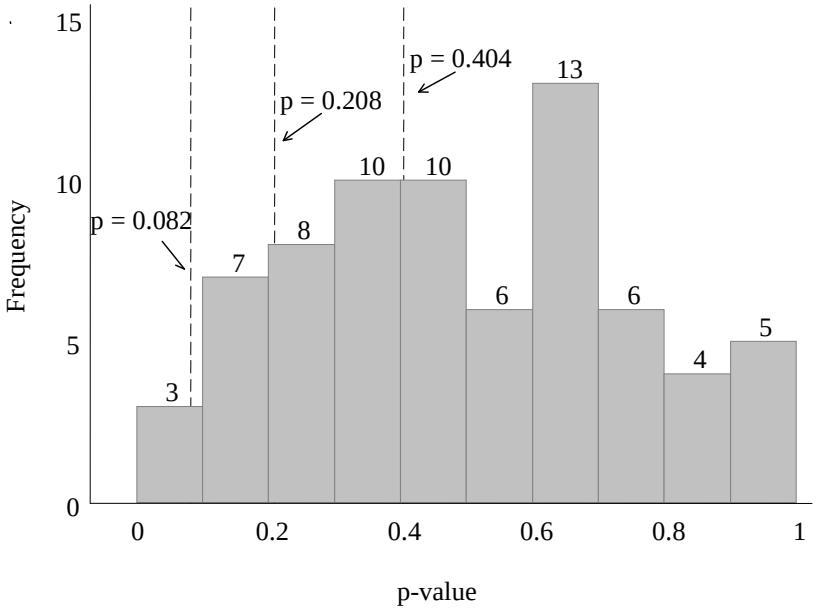


PANEL C: Health Satisfaction



NOTE: Universe consists of GHDP tax rebate recipients and nonrecipients with imputed individual-equivalent gross monthly income of less than $6,000. Each panel shows 14-day moving averages. The first dashed vertical line marks May 2, 2008, which is the first date for receipt of the tax rebate as a deposit. The second dashed vertical line marks May 16, 2008, which is the first date for tax rebate receipt as a check.

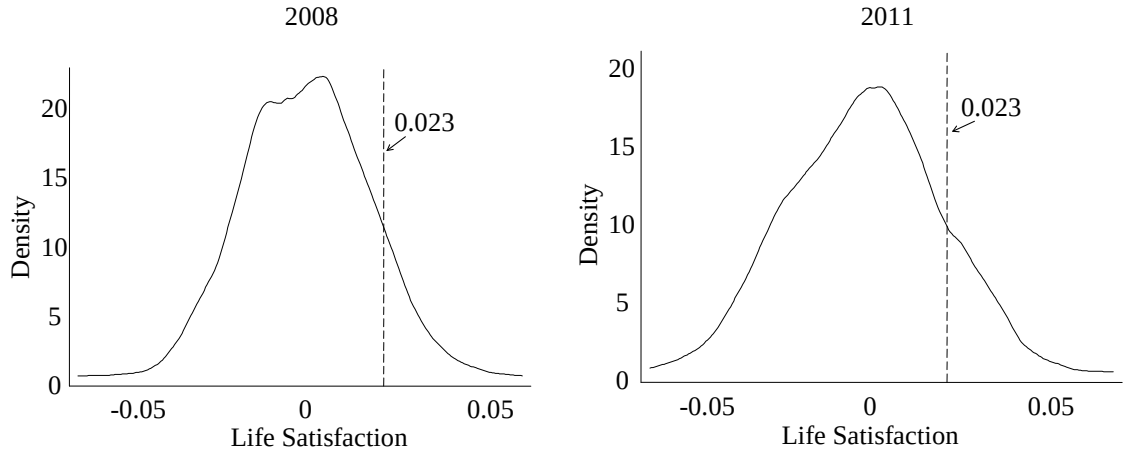
Figure 5 P-values from Falsification Tests



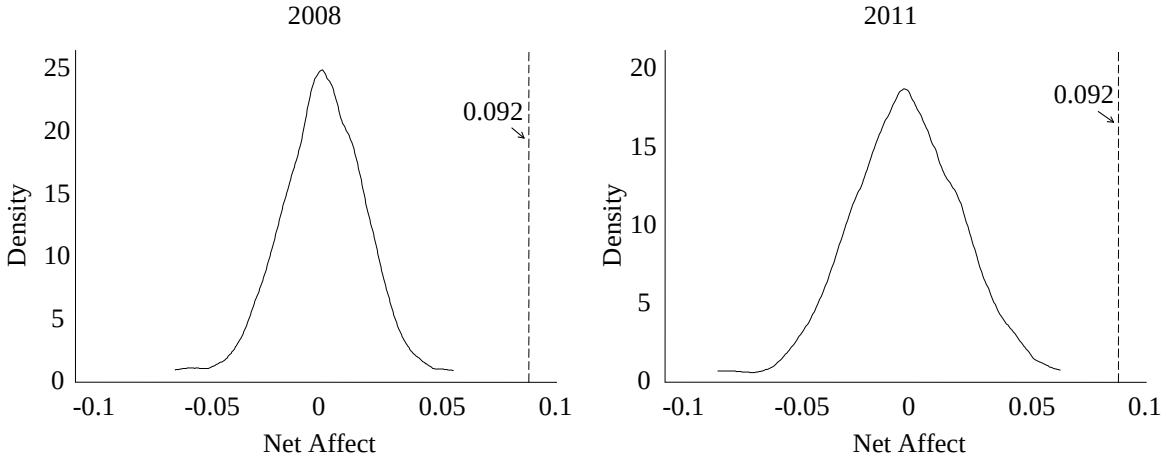
NOTE: Universe: Imputed individual-equivalent gross monthly income of less than $6,000; April 27–July 1; July 21–23, 2008. The figure shows the distribution of *p*-values of estimated δ-coefficients from 72 2SLS regressions that use the predetermined variables from Table 2 and the state that the respondent lives in as false outcomes; see the text for details. The mean *p*-value equals 0.49, and the median *p*-value equals 0.48. All of the regressions control for day-of-week dummies and a linear and squared weekly trend. The vertical dashed lines correspond to the *p*-values associated with the 2SLS δ-coefficients in Table 3, column (2), which is equal to 0.082 for the model using net affect, 0.208 for health satisfaction, and 0.404 for life satisfaction.

Figure 6 Distribution of Coefficients from Randomized Placebo Tests

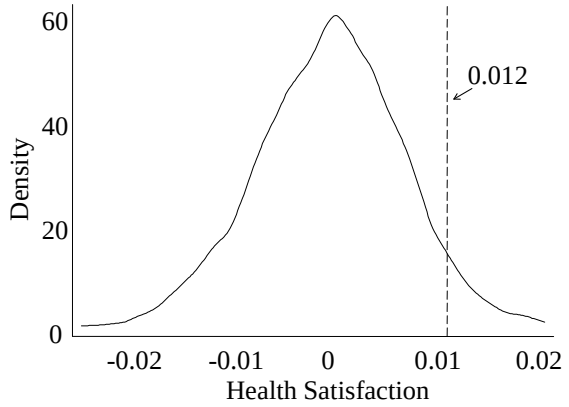
PANEL A: Life Satisfaction



PANEL B: Net Affect



PANEL C: Health Satisfaction in 2008



NOTE: Universe: Imputed individual-equivalent gross monthly income of less than $6,000, April 27–July 1, 2008; July 21–23, 2008; April 27–July 1, 2011; and July 21–23, 2011. Each figure shows the distribution of estimated coefficients from 1,000 randomly assigned tax rebate receipt indicators for years 2008 and 2011. For each outcome, the estimated model corresponds to the OLS regression in Table 4, Panel A, row A. The vertical dashed line corresponds to the relevant point estimate from the regression in Table 4, Panel A, row A (which equals 0.023 for the model using life satisfaction, 0.092 for net affect, and 0.012 for health satisfaction).

APPENDIX

Table A1 Fraction of Tax Rebate Recipients in the 2008 GHDP

Date	Received tax rebate?	Date	Received tax rebate?
April 27	0.026		continued
April 28	0.021	June 11	0.583
April 29	0.020	June 12	0.545
April 30	0.029	June 13	0.471
May 1	0.038	June 14	0.498
May 2	0.057	June 15	0.493
May 3	0.052	June 16	0.517
May 4	0.077	June 17	0.571
May 5	0.083	June 18	
May 6	0.061	June 19	0.539
May 7	0.097	June 20	0.574
May 8	0.092	June 21	0.549
May 9	0.144	June 22	0.561
May 10	0.167	June 23	0.562
May 11	0.169	June 24	0.557
May 12	0.223	June 25	0.639
May 13	0.270	June 26	0.537
May 14	0.222	June 27	
May 15	0.234	June 28	0.556
May 16	0.257	June 29	0.608
May 17	0.294	June 30	0.651
May 18	0.289	July 1	0.674
May 19	0.373	July 2	
May 20	0.388	July 3	
May 21	0.286	July 4	
May 22	0.311	July 5	
May 23	0.432	July 6	
May 24	0.326	July 7	
May 25	0.365	July 8	
May 27	0.400	July 9	
May 28	0.394	July 10	
May 29	0.415	July 11	
May 30		July 12	
May 31	0.355	July 13	
June 1	0.409	July 14	
June 2	0.426	July 15	
June 3	0.386	July 16	
June 4	0.447	July 17	
June 5	0.368	July 18	
June 6	0.492	July 19	
June 7	0.405	July 20	
June 8	0.364	July 21	0.747
June 9	0.505	July 22	0.807
June 10	0.488	July 23	0.767

Table A2 results from the First-Stage Regression. Dependent Variable: Received Tax Rebate

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Entire sample	Entire sample	Entire sample	April and May	April and May	April and May
Covariates						
Instrumental variable, DTS[a]	0.518***	0.528***	0.527***	0.565***	0.539***	0.540***
	(0.080)	(0.079)	(0.078)	(0.102)	(0.102)	(0.102)
Age		0.031***	0.029***		0.022***	0.020***
		(0.004)	(0.004)		(0.005)	(0.005)
Age squared		−0.001***	−0.001***		−0.000***	−0.000***
		(0.000)	(0.000)		(0.000)	(0.000)
Age cubed		0.000***	0.000***		0.000***	0.000***
		(0.000)	(0.000)		(0.000)	(0.000)
No. of children < 18 years		−0.004	−0.004		−0.004	−0.004
		(0.003)	(0.003)		(0.004)	(0.004)
Married		0.053***	0.027***		0.042***	0.025***
		(0.007)	(0.007)		(0.007)	(0.008)
Woman		0.010	0.015**		0.009	0.012*
		(0.006)	(0.006)		(0.007)	(0.007)
Race/ethnicity						
Other		−0.086***	−0.079***		−0.070***	−0.065***
		(0.015)	(0.015)		(0.016)	(0.016)
African-American/Black		−0.078***	−0.072***		−0.052***	−0.048***
		(0.012)	(0.012)		(0.013)	(0.013)
Hispanic		−0.082***	−0.073***		−0.055***	−0.048***
		(0.017)	(0.017)		(0.019)	(0.019)
Asian		−0.010	−0.007		0.032	0.032
		(0.032)	(0.032)		(0.038)	(0.038)
Highest completed level of education						
High school degree or diploma		0.066***	0.048***		0.045***	0.035**
		(0.013)	(0.013)		(0.014)	(0.014)
Technical/vocational school		0.063***	0.038**		0.038**	0.026
		(0.015)	(0.016)		(0.016)	(0.016)
Some college		0.076***	0.048***		0.045***	0.029**
		(0.013)	(0.013)		(0.014)	(0.014)
College graduate		0.093***	0.059***		0.061***	0.041***
		(0.014)	(0.014)		(0.014)	(0.015)
Postgraduate work or degree		0.073***	0.032**		0.039***	0.015
		(0.014)	(0.015)		(0.015)	(0.016)
Days of week						
Monday	0.029***	0.027**	0.025**	0.012	0.010	0.009
	(0.011)	(0.010)	(0.010)	(0.011)	(0.011)	(0.011)
Tuesday	0.020	0.020	0.019	0.022	0.016	0.015
	(0.013)	(0.013)	(0.013)	(0.018)	(0.018)	(0.018)
Wednesday	0.018	0.017	0.016	0.002	−0.005	−0.005
	(0.013)	(0.013)	(0.013)	(0.017)	(0.017)	(0.017)
Thursday	0.004	0.002	0.002	0.004	−0.003	−0.003
	(0.013)	(0.013)	(0.013)	(0.017)	(0.017)	(0.017)
Friday	−0.001	−0.004	−0.004	−0.005	−0.008	−0.009
	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)	(0.013)

Table A2 (Continued)

Sample	(1) Entire sample	(2) Entire sample	(3) Entire sample	(4) April and May	(5) April and May	(6) April and May
Covariates						
Saturday	−0.014	−0.011	−0.011	−0.024*	−0.022*	−0.022*
	(0.011)	(0.011)	(0.011)	(0.013)	(0.013)	(0.013)
Time trends						
Week	0.018	0.015	0.015	−0.032	−0.025	−0.026
	(0.013)	(0.012)	(0.012)	(0.020)	(0.020)	(0.020)
Week squared	0.000	0.000	0.001	0.007***	0.006***	0.007***
	(0.000)	(0.000)	(0.000)	(0.002)	(0.002)	(0.002)
Gross monthly household income						
$60 to $499			0.021			0.032
			(0.063)			(0.062)
$500 to $999			0.036			0.039
			(0.060)			(0.059)
$1,000 to $1,999			0.069			0.047
			(0.059)			(0.058)
$2,000 to $2,999			0.091			0.056
			(0.059)			(0.058)
$3,000 to $3,999			0.126**			0.085
			(0.059)			(0.059)
$4,000 to $4,999			0.135**			0.087
			(0.059)			(0.059)
$5,000 to $7,499			0.139**			0.098*
			(0.059)			(0.059)
State dummies	No	Yes	Yes	No	Yes	Yes
Constant	−0.030*	−0.550***	−0.581***	0.046*	−0.247***	−0.265***
	(0.017)	(0.077)	(0.097)	(0.024)	(0.084)	(0.103)
IV F-statistic	42.24	45.24	45.23	30.51	28.02	28.24
Observations	17,933	17,933	17,933	10,393	10,393	10,393

NOTE: Robust standard errors are in parentheses (*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$).

a. The instrumental variable, DTS, is the amount of cumulative tax rebate payments issued by the U.S. Treasury up to a given day, divided by all tax rebate payments made in 2008.