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I examine whether receipt of a $300 tax rebate by payday borrowers affects their likelihood of borrowing, loan size, or default behavior. Results from fixed-effects models show that the rebate decreases the probability of taking out a payday loan in the short run. These impacts are most apparent among credit-constrained, infrequent borrowers. Those who take out loans around the time of the rebate borrow amounts typical of their normal borrowing behavior but are more likely to default. Overall, however, the rebate's effects are small and short-lived, suggesting a muted response to this cash windfall in payday borrowing and repayment. (*JEL*: D12, D14)

1. Introduction

Payday loans are a controversial form of short-term credit in which borrowers promise to pay lenders half of their take-home pay from their next paycheck, plus interest, in a matter of days. Borrowers typically use them to obtain a couple of hundred dollars of cash until their next payday, at a cost of 400–600% APR. Demand for these loans has been growing rapidly since their inception in the early 1990s, and more recent figures show that as many as 10 million U.S. households use payday loans each year (Survey of Consumer Finances, 2007). Though the loans' durations are short—lasting just until a borrower's subsequent payday—the modal customer relies on these

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loans heavily and repeatedly once she enters the market. Payday borrowers in one recent sample had loans outstanding for 25% of the year (Carter et al., 2012). Other work has shown that in their first two years in the market, individuals borrowed about $2,000 on average from one lender (Skiba and Tobacman, 2011). The high intensity of use characterizing the payday loan industry is due to the fact that borrowers often "roll over" their loans by paying only interest on the maturation date, thereby extending the loan for another pay cycle and accumulating additional interest. Most borrowers in this market have exhausted all traditional credit options, a constraint that heightens their reliance on this single source of credit (Bhutta et al., 2013).

In large part because of the rollover cycles described above, payday loans have attracted considerable regulatory attention. The Office of the Comptroller of the Currency, the Federal Deposit Insurance Corporation, and the Consumer Financial Protection Bureau all recently announced plans to restrict payday lending practices.[1] Thus, the landscape of payday loan regulation will certainly be changing. Whatever constraints policymakers place on payday lenders, the consensus seems to be that appropriate and successful regulation should help reduce the intensity of use.

This paper explores the effect that a lump sum income shock has on patterns of payday loan borrowing, using a $300 tax rebate check that most Americans received in 2001. I focus on this income shock because (1) its magnitude was the same size as a typical payday loan plus fees and (2) it was distributed in a quasi-random way, creating a natural experiment that allows me to analyze its causal effect on consumers' behavior. I use a panel dataset of tens of thousands of payday loan borrowers to study how this exogenous cash infusion changed consumers' payday borrowing behavior, comparing their behavior in a nineteen-week window of interest around the receipt of the rebate to their typical behavior (derived from months of borrowing records outside of this nineteen-week window).

Other researchers have used tax rebates largely to test for the existence of liquidity constraints. My strategy closely follows that of Agarwal et al. (2007), who look at how tax rebates affect credit card use, and Bertrand and Morse (2009), who also study payday borrowers' response to tax rebates.

1. See, for example, Consumer Financial Protection Bureau (2013). Regulation is also active at the state level: 13 states ban payday lending outright (Carter, 2012).

However, while Bertrand and Morse's study is based on a survey with a low response rate, I have the full payday loan borrowing histories of 46,609 people who borrowed from a large payday lender operating in 13 states, yielding a larger, longer, and richer dataset.

Even though the cash infusion could have been used to retire payday loan debt, all of the effects I estimate here are small and short-lived. The overall (non-)response by payday loan borrowers to the $300 cash infusion suggests that a larger, structural change, rather than a one-time income shock, may be needed to break the cycle of payday loan borrowing.

2. Background

2.1. Payday Loans

Payday lenders supply cash on the spot in exchange for a check post-dated to the borrower's next payday. A typical $250 loan lasting two weeks comes with a $45 interest fee.

Whether such high-interest loans enhance or damage consumers' welfare has been hotly debated, with research yielding mixed results.[2] My focus here is not on exploring the overall welfare effects of payday loans, but rather on assessing whether a $300 shock helps interrupt the cycle of borrowing behavior.

2.2. The Income Shock

In an effort to prompt spending, the federal government mailed $300 rebate checks to most single people and $600 checks to most couples in the fall of 2001.[3] The expected time of receipt was based on the last two digits of the tax filer's Social Security number; Table 1, re-created from information distributed by the Treasury Department, indicates when consumers were told to expect their checks. Since these last two digits are

2. See Caskey (2012) for an overview of this research.

3. My work follows a long line of literature studying consumers' reactions to tax rebates. Most of this work tests the "Permanent-Income Hypothesis," the canonical rational-actor model that makes stark predictions about how consumers would react to changes in income. For a nice review of this literature, see Stephens (2003).

Table 1. Expected Date of Rebate Receipt

Last two digits of SSN	Receive check in the week of
00–09	July 23
10–19	July 30
20–29	August 6
30–39	August 13
40–49	August 20
50–59	August 27
60–69	September 3
70–79	September 10
80–89	September 17
90–99	September 24

Notes: This table shows consumers' expected date of receipt of their tax rebate. This date depended on the last two digits of a consumer's Social Security number. Source: Internal Revenue Service, 2001.

random,[4] and since I am able to measure consumers' "normal" borrowing behavior, this setup provides a source of truly exogenous variation that can be used to analyze whether the income shock of the rebate affects a consumer's payday borrowing behavior, relative to her own normal borrowing behavior.

My tax rebate approach is remarkable in that (1) most Americans received checks (with a few exceptions described later in Section 3.2), and (2) the variation is completely exogenous: because a check's arrival is based on the last two digits of an individual's Social Security number, it is truly random. This type of exogeneity is second only to a randomized experiment in terms of its ability to assess causality.

3. Empirical Strategy

3.1. Data

My data consist of the universe of loan applications made from 2001 to 2002 at a national payday lender that operates in 13 states. I restrict the sample by including only those borrowers who had already taken out at least one loan prior to 2001 (this lets me observe "normal" borrowing behavior pre-rebate, a necessity for the empirical design, by ensuring that people had

4. Agarwal et al. (2007) provide additional details on the assignment of Social Security numbers and the disbursement of the 2001 tax credit.

Table 2. Summary Statistics

	(1) Mean	(2) Std. dev.	(3) *N*
Probability of borrowing	0.02	0.141	4,847,336
Loan size ($)	215.83	157.64	49,211
Probability of default	0.09	0.29	49,211
Age	40.43	12.29	49,211
Maximum loan allowed ($)	405.45	2,398.71	49,163
Number of payday loan applications	19.82	15.75	49,211
Female	0.631	0.48	28,882
Homeowner	0.401	0.49	49,026
Ratio of actual loan size/maximum available	0.76	2.02	38,366
Ratio of loan amount requested/maximum available	0.86	2.63	38,363

Notes: Author's calculations based on administrative data from a major payday lender. The entire sample is constructed of individuals who obtained at least one loan in 2001–2002 from that payday lender. Each observation is an individual-week, so an individual in the sample has 104 observations (52 weeks * 2 years). Sample sizes change in column (3) because covariates are not available for all borrowers and all loans.

actually borrowed before the 2001 rebate). I follow each of these borrowers over the two-year window from the beginning of 2001 to the end of 2002. The resultant dataset tracks a total of 46,609 borrowers over more than four million individual-weeks, where each observation in my regressions is an individual-week. Because borrowers must have obtained a loan from the company before the beginning of 2001, no new borrowers enter the sample over the course of the two-year period, and the sample size remains constant.

The loan records include loan size, start date, maturation date, outcome of the loan (repaid, defaulted on, or rolled over by paying interest only), and the last two digits of the borrower's Social Security number. The administrative records also include detailed demographic information. Borrowers must provide their most recent pay stub, utility bill, and checking account statement at the time of application. Information from these records provides the demographic variables used in my empirical analysis. These variables are: credit score used to approve the payday loan, gender, age, address, net take-home pay, months at current residence, months employed, and checking account balance. Such detailed demographics allow me to explore heterogeneity in the results described below.

Table 2 provides basic summary statistics on borrowers and loans: each borrower has a 2% probability of taking out a loan in a given week. The average borrower is 40 years old; borrows \$216, which would be associated with an interest payment of \$39;[5] and has a default probability of 9%.

3.2. Econometrics

My regressions exploit the timing shown in Table 1 using a fixed-effects model. I estimate the following equation:

$$\begin{aligned} Y_{it} = {} & \alpha + \beta_{r-6}\text{Rebate}_{ir-6} + \beta_{r-5}\text{Rebate}_{ir-5} \\ & + \cdots + \beta_{r-1}\text{Rebate}_{ir-1} + \beta_r\text{Rebate}_{ir} + \beta_{r+1}\text{Rebate}_{ir+1} \\ & + \beta_{r+2}\text{Rebate}_{ir+2} + \cdots + \beta_{r+12}\text{Rebate}_{ir+12} \\ & + \text{Week}_t + FE_i + \epsilon_{it}. \end{aligned} \tag{1}$$

Regressions are estimated for person i at time t, for three separate outcome variables Y_{it}: probability of borrowing (which is an indicator variable equal to one if person i borrowed in week t), loan size (in dollars), and probability of default (also an indicator variable equal to one if default occurred). A linear probability model is used for the binary outcomes (probability of default and probability of borrowing) and OLS is used for loan size. There are nineteen explanatory variables of interest; each is an indicator corresponding to one of nineteen weeks around and including the rebate week r. This nineteen-week window of interest consists of the six weeks leading up to the rebate (Rebate_{r-6}, $\text{Rebate}_{r-5}, \ldots, \text{Rebate}_{r-1}$) the week of the rebate (Rebate_r), and the twelve weeks after the rebate (Rebate_{r+1}, $\text{Rebate}_{r+2}, \ldots, \text{Rebate}_{r+12}$). The coefficient β_r can therefore be interpreted as the change in outcome Y_{it} during the week the rebate is received (week r), relative to typical borrowing behavior in the two-year sample (i.e., in the remaining eighty-five weeks outside of the nineteen-week window of interest). Similarly, β_{r-6} represents the change in outcome Y_{it} in the sixth week before the rebate was received (week $r-6$) relative to typical borrowing behavior in the two-year sample, and so on.

Regressions also include week indicators (Week_t) to absorb variation in payday lending, such as that attributable to monthly and annual cyclical behavior, as well as variation stemming from singular events such as

5. All dollar amounts in the data are deflated to 2002 dollars.

September 11, 2001 (which occurred during the mailings). FE_i are individual fixed effects. ϵ_{it} is a robust error term clustered at the individual level. Some regressions using probability of default also include quadratics of loan size as controls.[6]

A nice feature of Equation (1) is that it controls for individual-specific borrowing behavior and identifies the rebate's impact using within-customer variation in lending. That is, the fixed effects alleviate concerns that differences are driven by variation in lender location in terms of city or region, etc. I only estimate how an individual's borrowing behavior in the six weeks before and twelve weeks after receiving the rebate compares to her own normal borrowing behavior within the two-year sample, not compared to any other borrower's behavior. As described below in Section 4.2, I also implement an alternative specification to confirm that the results are robust to the definition of "normal borrowing behavior."

It should be noted that all empirical research using tax rebates may suffer from measurement error and resultant attenuation bias, as it is not possible to observe with certainty when or if the tax rebate arrived. Below, I describe the three main reasons that borrowers might not have received their rebate per the schedule shown in Table 1 or at all, and I consider the extent to which they might pose problems for my dataset. Certain features of my data suggest that my results likely suffer from less measurement error than the previous literature.

First, borrowers may have been ineligible for a rebate based on their income. Agarwal et al. (2007), who also worry about (credit card) borrowers in their sample being ineligible for the 2001 rebate, report that "89.5 million tax returns received a rebate and 23.5 million did not receive a rebate" (p. 987). Given that the mean income of borrowers in my sample is \$20,313, with the 99th percentile of income below \$50,000, I do not expect that many borrowers were ineligible based on high income. Low income is not a concern for eligibility; even borrowers who earned less than \$6,000 in taxable income could have received a rebate, albeit for an amount smaller than \$300.

Second, borrowers who filed their taxes electronically would have deviated from the schedule in Table 1, instead receiving their rebates earlier

6. Results, available upon request, are robust to the polynomial chosen to control for loan amount. In light of concerns about the endogeneity of loan amount, results without loan amount controls are also presented.

via direct deposit. Bertrand and Morse (2009) provide nice data regarding mailed versus direct deposit rebates for their 2008 sample: using a phone survey of their payday loan participants, they find that 66% of respondents received their rebates in the mail. While I do not have data on whether borrowers received their 2001 rebates in the mail or via direct deposit, trends in electronic tax filing and in the general use of direct deposit suggest that a lower proportion of individuals would have filed their taxes electronically and received their rebates through direct deposit in 2001 than in 2008 (yielding less measurement error in the 2001 data than in the 2008 data). First, use of direct deposit of all kinds has increased considerably since 2001.[7] Second, the number of taxpayers filing electronically was dramatically lower in 2001 relative to 2008: while only 30% of households filed electronically in 2001, this figure increased to 58% in 2008.[8]

Finally, if a household filed their income taxes late, their rebate would arrive late. While I do not have data on the incidence of late filing in 2001 or in 2008, I note that Slemrod et al. (1997) report that 92% of households do not file late.

In sum, while it is surely the case that some fraction of the payday borrowers in my sample did not receive the rebate at all or received it via direct deposit, there still appears to be less measurement error here than in previous papers given that my sample is much lower-income than that of Agarwal et al. (2007) and that my time frame is earlier than that of Bertrand and Morse (2009). In results described in the next section, I do find some significant effects on borrowing behavior that occurred a couple of weeks *after* I expect the rebate to arrive. It is possible that such results are driven by error in my measurement of the timing of the rebate. Readers should keep the above caveats in mind when interpreting any results based on the receipt of tax rebates.

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7. According to NACHA, which is the administering institution for the Automated Clearing House (an electronic network that batch-processes large volumes of credit and debit transactions between financial institutions), the Automated Clearing House processed approximately 3.7 billion direct deposit payments in 2001 (NACHA, 2002) and approximately 4.33 billion direct deposit payments in 2008 (NACHA, 2010). Note that these figures include direct deposit payments of all kinds—payroll, government benefits, tax refunds, etc.

8. This information was obtained in a September 3, 2013 email from Ruth A. Schwartz, Statistics of Income Division of the Internal Revenue Service.

4. Results

4.1. Main Results

Basic comparative statistics on borrower behavior close to the time of the rebate are shown in Table 3. The raw data in column (1), also shown graphically in Figure 1, reveal that the probability of borrowing on a payday loan drops from 2% in the fourth week before the rebate to 1.7% in the week before the rebate, representing an overall 15% decrease. During the week of the rebate, that probability drops further to 1.4%, but quickly returns to almost 2% the next week. By the sixth week after the rebate, the probability of borrowing has increased to 2.4%.

Table 4 shows the main regression results for the three outcomes of interest (probability of borrowing, loan size, and probability of default). These are the fixed-effects regression results outlined in Equation (1), using the sample of 46,609 borrowers and more than four million borrower-weeks. (Of course, not every borrower borrowed every week, so there are many “zero” observations for weeks in which a particular borrower did not have a loan outstanding.)

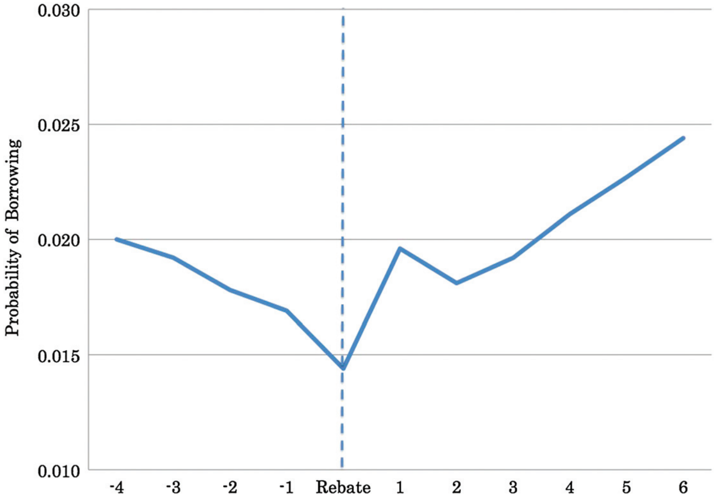
Results in column (1) of Table 4 show that the probability of borrowing dropped by a statistically significant 0.149 percentage points during the rebate week (with a 95% confidence interval of [−0.288, −0.009]). This is a 7.4% decrease relative to the 2% baseline probability of borrowing in the dataset (as reported in Table 2). The decreased probability of borrowing persists two weeks after the rebate: the coefficient is −0.177 percentage points (with a 95% confidence interval of [−0.332, −0.023]), representing an 8.9% decrease relative to the baseline probability of borrowing in the dataset. However, borrowing returns to normal shortly thereafter, with no economically or statistically significant effects after the second week. Joint tests of coefficients show no significant effect in the weeks before the rebate, but a significant (F-statistic: 2.11, p-value: 0.04) impact on borrowing in the week of the rebate and the following six weeks. Coefficients for weeks 7–12 are jointly insignificant, indicating a lack of longer-term persistence in the rebate’s effect.

Columns (2) and (3) in Table 4 show results for the loan amount and the probability of default. Here, I restrict the sample by including only observations from borrowers who had at least one loan outstanding during

Table 3. Summary Statistics for Outcome Variables

	Probability of Getting Loan			Loan Amount			Probability of Default		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Mean	Std. dev.	*N*	Mean ($)	Std. dev.	*N*	Mean	Std. dev.	*N*
Weeks									
−4	0.0200	0.1400	46,609	188.18	157.03	932	0.0864	0.2810	932
−3	0.0192	0.1372	46,609	216.48	154.50	894	0.0990	0.2987	894
−2	0.0178	0.1322	46,609	207.98	164.56	829	0.0573	0.2325	829
−1	0.0169	0.1288	46,609	217.58	164.84	786	0.0700	0.2552	786
Rebate	0.0144	0.1191	46,609	215.57	157.57	671	0.0984	0.2980	671
1	0.0196	0.1386	46,609	217.24	145.31	913	0.1030	0.3041	913
2	0.0181	0.1332	46,609	226.13	152.24	842	0.0986	0.2983	842
3	0.0192	0.1374	46,609	229.77	150.71	897	0.1081	0.3107	897
4	0.0211	0.1437	46,609	222.57	154.35	983	0.0743	0.2623	983
5	0.0227	0.1489	46,609	216.59	148.09	1,058	0.0926	0.2900	1,058
6	0.0244	0.1544	46,609	220.60	146.16	1,139	0.0834	0.2766	1,139
Other observations	0.0235	0.1516	3,122,803	215.45	158.90	73,532	0.0980	0.2973	73,532

Notes: Timing of the rebate is based on the last two digits of SSN, with rebates sent out over ten weeks in July–September 2001, as shown in Table 1. “Other observations” are loans that individuals obtained within calendar years 2001–2002 but were not obtained within the time period of four weeks before to six weeks after their rebates were mailed. Loan amount and probability of default statistics are conditional on obtaining a loan within that time period.



Source: Author's calculations based on administrative data from a major payday lender. The vertical axis measures the mean probability of taking out a payday loan in a given week. The overall sample mean is 0.0203. The horizontal axis measures weeks relative to the mailing of the tax rebate check. The dotted vertical line marks the week the rebate was received.

Figure 1. Probability of Borrowing at Tax Rebate Time.

the observation week. Thus, each week of this restricted panel includes only those borrowers who had a non-zero loan amount that week and who therefore had some possibility of defaulting. I note that these effects are not strictly identified from the quasi-random arrival of the tax rebate, since there is selection into the decision to borrow or the decision not to borrow each week. These results, while only suggestive, do reveal information about the combined effect at the extensive and intensive margins of receiving the rebate, i.e., choosing to borrow and how much. Section 4.2 reports an alternative specification that further explores loan amount and probability of default.

There is only weak evidence in column (2) that loan amounts are affected by receipt of the rebate: loans taken out six weeks before the rebate are \$16.70 larger than normal and loans taken out eight weeks after the rebate are \$9.48 smaller than normal. None of the joint tests for coefficients in this regression are statistically significant.[9]

9. Results are robust to inclusion of flexible controls (up to fourth-order polynomial) in loan amount.

Table 4. Fixed-Effects Regressions

	(1)	(2)	(3)
Weeks Relative to Rebate	Probability of Borrowing	Loan Amount ($) Conditional on Borrowing	Probability of Default Conditional on Borrowing
Weeks before rebate			
−6	0.0286	16.6992*	0.4520
	(0.0543)	(8.9239)	(1.5508)
−5	−0.0030	5.6801	1.6352
	(0.0553)	(8.8377)	(1.7768)
−4	0.0779	−0.1343	3.4733**
	(0.0602)	(8.6436)	(1.5225)
−3	0.1250*	8.7529	−0.2709
	(0.0663)	(7.4318)	(1.3769)
−2	0.0076	−2.8921	0.4711
	(0.0683)	(7.3198)	(1.2641)
−1	−0.0196	11.9636	2.1440*
	(0.0709)	(7.6493)	(1.2605)
Rebate week	−0.1488**	4.8214	2.7757**
	(0.0711)	(7.4445)	(1.3028)
Weeks after rebate			
1	0.0601	1.5391	1.2720
	(0.0792)	(6.4393)	(1.0544)
2	−0.1773**	−0.3543	0.8819
	(0.0787)	(7.0960)	(1.1206)
3	−0.1282	5.0052	1.2479
	(0.0814)	(6.3244)	(1.1129)
4	0.0346	0.7948	1.0424
	(0.0828)	(5.9831)	(0.9616)
5	−0.0796	−3.6579	0.7224
	(0.0834)	(5.6428)	(0.9695)
6	−0.0686	−2.1679	1.2672
	(0.0856)	(5.2910)	(0.8949)
7	−0.1066	2.6133	3.1674***
	(0.0852)	(5.2004)	(0.9080)
8	−0.0445	−9.4768*	0.1688
	(0.0833)	(5.1341)	(0.8701)
9	−0.0500	6.4932	1.1644
	(0.0810)	(4.8858)	(0.8216)
10	0.0424	1.3382	1.6609**
	(0.0850)	(4.7148)	(0.8127)
11	−0.0745	1.8142	2.6433***
	(0.0835)	(4.7025)	(0.8128)
12	0.0697	0.9014	0.0648
	(0.0849)	(4.5277)	(0.7409)
Constant	0.1952***	108.9456***	−19.5559***
	(0.0221)	(22.5189)	(3.6037)

(continued)

Table 4. Continued

	(1)	(2)	(3)
Weeks Relative to Rebate	Probability of Borrowing	Loan Amount ($) Conditional on Borrowing	Probability of Default Conditional on Borrowing
Observations	4,008,374	91,562	91,652
Individuals	46,609	44,099	44,099
R^2	0.01	0.07	0.05

Notes: All results are from fixed-effects regressions using panel data, with individual fixed effects and indicators for each week to absorb time effects. I report the effects for each of the six weeks before the rebate arrived, the rebate week, and each of the twelve weeks after the rebate arrived. Each observation is an individual-week. Robust standard errors clustered at the individual level are in parentheses.
*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

Column (3) indicates that those who borrow around the time they receive their rebate are more likely to default on these loans. Notably, the probability of default increases by 2.78 percentage points for loans taken out during the rebate week (with a 95% confidence interval of [0.489, 6.457]), representing a 31% increase relative to the baseline 9% probability of default in the dataset (as reported in Table 2). Only the coefficients for weeks 7–12 after the rebate are jointly significant (F-stat: 3.64, p-value: <0.01).

While some effects are significant and precisely estimated, overall the results do not indicate much of a change in borrowing behavior due to a cash infusion. Even where effects are economically large, as with loan size and the probability of borrowing, behavior rebounds within a few weeks.

4.2. Robustness Checks

In the main specification in Equation (1), the excluded eighty-five weeks—i.e., the weeks that represent "normal borrowing behavior"—include weeks after the rebate (specifically, weeks 13 through the end of the sample). Even though, as shown in Table 4, the effects of the rebate wear off quickly, I begin with an alternative specification that includes only pre-rebate weeks in the definition of "normal borrowing behavior."

This first alternative specification, also employed by Bertrand and Morse (2009), simply includes a single indicator for all weeks after the first post-rebate week. This approach provides an estimate of the rebate's total long-run effect in a single coefficient. Note that while Bertrand and Morse had data on just three post-rebate pay cycles, here I have data on as many as seventy-five post-rebate weeks, meaning that this indicator can capture up to seventy-four weeks.[10] Results are shown in Table 5. The overall effect of the rebate after the second week is again small and insignificant in all cases.[11] For instance, there is an insignificant 0.053 percentage-point drop in the probability of borrowing after the first week, which is only a 2.7% decrease relative to the baseline 2% probability of borrowing in the dataset overall. Similarly, conditional on borrowing, there is an insignificant $1.14 increase in loan amount after the first week, representing only a 0.5% increase relative to the baseline $215.83 loan amount in the sample. Again conditional on borrowing, there is also an insignificant 1.64 percentage-point increase in the probability of default after the first week, though this does represent an 18% increase relative to the baseline 9% probability of default in the dataset.

A second alternative specification limits the sample to the 1,081 individuals who had a loan outstanding when the rebate was announced on June 7, 2001.[12] This allows me to analyze how individuals with already-outstanding payday loans responded to the announcement of a future cash influx. Results are shown in Table 6. Column 1 shows that the probability of borrowing for this group is negative in almost all cases, although the effects are not statistically significant except for the six weeks before the rebate arrived. The effect for the sixth week before the rebate is a 1.17 percentage-point decrease in their probability of borrowing (with a 95% confidence interval

10. The earliest rebates arrived on July 23, 2001. For these, the first post-rebate week began on July 30, while the second post-rebate week began on August 6. Thus, for these earliest rebates, the indicator would capture the remaining twenty-two weeks in 2001 between August 6 and the end of the year, plus fifty-two weeks in 2002, for a total of seventy-four weeks.

11. A more flexible strategy estimates separately the effects for each of weeks 1–12 after the rebate, in addition to an indicator for all subsequent post-rebate weeks (i.e., for week 13 through the end of the sample). This is similar in spirit to the approach in Table 5, in that no post-rebate weeks are included in "normal borrowing behavior." Results, shown in Appendix Table 1, are very similar to those in Tables 4 and 5.

12. The tax rebate was part of the Economic Growth and Tax Relief Reconciliation Act of 2001, which was introduced on May 15, 2001. Although President George Bush ran for months on a platform of major tax reform, the rebate checks were not part of his original tax plan, so I do not expect borrowers to have anticipated the rebate before the bill was passed on June 7, 2001.

Table 5. Fixed-Effects Regressions

	(1)	(2)	(3)	(4)	(5)
	Probability of Borrowing	Loan Amount ($) Full Sample	Loan Amount ($) Conditional on Borrowing	Probability of Default Full Sample	Probability of Default Conditional on Borrowing
Weeks Relative to Rebate					
−6	0.0420	0.1345	16.8679*	−0.0141	0.3044
	(0.0542)	(0.1433)	(9.3960)	(0.0191)	(1.6729)
−5	0.0129	−0.0205	5.8028	0.0160	1.6962
	(0.0568)	(0.1474)	(9.6570)	(0.0214)	(1.9629)
−4	0.0963	0.0327	−0.9058	−0.0187	3.6704**
	(0.0622)	(0.1573)	(9.7598)	(0.0216)	(1.7767)
−3	0.1456**	0.2672	9.1474	−0.0036	−0.1004
	(0.0692)	(0.1793)	(9.0234)	(0.0246)	(1.6704)
−2	0.0306	−0.2204	−2.9526	−0.0506**	0.4979
	(0.0720)	(0.1854)	(9.4711)	(0.0230)	(1.6671)
−1	0.0054	−0.0420	12.4732	0.0025	2.4638
	(0.0766)	(0.2007)	(9.9950)	(0.0261)	(1.7128)
Rebate week	−0.1226	−0.3599*	5.8949	−0.0333	3.2909*
	(0.0804)	(0.2105)	(10.1321)	(0.0257)	(1.8062)
1	0.0861	0.0311	2.3392	−0.0108	1.6040
	(0.0879)	(0.2281)	(9.6463)	(0.0294)	(1.6530)
Weeks 2	−0.0532	−0.1743	1.1416	−0.0283	1.6429
through *N*	(0.0846)	(0.2173)	(9.7528)	(0.0252)	(1.6656)
Constant	0.0172*	0.0108	17.9506	0.0021	3.3400**
	(0.0098)	(0.0243)	(67.3802)	(0.0028)	(1.4036)
Observations	4,521,073	4,521,073	92,684	4,521,073	92,684
Individuals	46,609	46,609	44,527	46,609	44,527
R^2	0.01	0.01	0.07	0.00	0.05

Notes: All results are from fixed-effects regressions using panel data, with individual fixed effects and indicators for each week to absorb time effects. I report the effects for each of the six weeks before the rebate arrived, the week of the rebate, and the week after the rebate, and collapse all other post-rebate weeks into a single indicator variable. The total number of post-rebate weeks included in this dummy varies since borrowers received the rebate in different weeks, but can include up to seventy-four weeks. Each observation is an individual-week. Robust standard errors clustered at the individual level are in parentheses.
*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

of [0.099, 2.248]). Loan sizes are also significantly smaller by just $2 to $3 a few weeks before and a few weeks after the rebate arrived. The probability of default is significantly lower in the third week before the rebate arrived but insignificant in other weeks.

To more closely examine the rebate's effects on the probability of default, I also examine the raw default rates of borrowers who received their rebate

Table 6. Fixed-Effects Regressions for Subsample

	(1)	(2)	(3)
Weeks Relative to Rebate	Probability of Borrowing	Loan Amount ($) Conditional on Borrowing	Probability of Default Conditional on Borrowing
Weeks before rebate			
−6	−1.1733**	−3.3971**	−0.2719
	(0.5483)	(1.4460)	(0.1687)
−5	−0.7713	−2.9909**	−0.0798
	(0.5694)	(1.4042)	(0.2188)
−4	0.1631	0.0962	0.1494
	(0.6944)	(1.9660)	(0.2893)
−3	−0.1864	−0.4456	−0.3189*
	(0.7402)	(2.0119)	(0.1926)
−2	−0.0433	−1.0407	−0.1933
	(0.7054)	(1.9664)	(0.1592)
−1	−0.2275	−2.7642	−0.1324
	(0.7072)	(1.7449)	(0.1495)
Rebate week	−0.9584	−2.7025	0.1678
	(0.6495)	(1.8032)	(0.2216)
Weeks after rebate			
1	0.0077	−1.2674	−0.0912
	(0.7643)	(1.9841)	(0.1784)
2	−0.4974	−1.6978	0.1206
	(0.7463)	(2.1249)	(0.2186)
3	−0.6620	−3.4534*	−0.1401
	(0.7526)	(1.9578)	(0.1467)
4	0.7381	0.8547	−0.1381
	(0.7928)	(2.1591)	(0.1411)
5	−0.2483	0.7984	−0.0075
	(0.7316)	(2.0916)	(0.1899)
6	−0.6357	0.8793	0.0337
	(0.7620)	(2.3000)	(0.1792)
7	−0.3856	−0.7701	0.2605
	(0.7453)	(2.0826)	(0.2214)
8	−0.2244	−3.4246**	−0.1059
	(0.7102)	(1.6909)	(0.1154)
9	−0.1807	0.1588	−0.0193
	(0.6932)	(1.9182)	(0.1440)
10	−0.5808	1.3778	−0.0268
	(0.6681)	(2.0725)	(0.1416)
11	−0.2809	−0.2279	−0.0052
	(0.5971)	(1.6969)	(0.0976)
12	0.5788	1.7626	−0.0007
	(0.6545)	(1.7941)	(0.1061)
Constant	1.2951***	1.6382***	0.0000
	(0.3527)	(0.5958)	(0.0156)
Observations	92,966	92,966	92,966

(continued)

Table 6. Continued

	(1)	(2)	(3)
Weeks Relative to Rebate	Probability of Borrowing	Loan Amount ($) Conditional on Borrowing	Probability of Default Conditional on Borrowing
Individuals	1,081	1,081	1,081
R^2	0.14	0.09	0.04

Notes: The sample consists of borrowers who had a loan outstanding at the time the 2001 rebate was announced. See text for additional details. All results are from fixed-effects regressions using panel data, with individual fixed effects and indicators for each week to absorb time effects. I report the effects for each of the six weeks before the rebate arrived, the week of the rebate, and each of the 12 weeks after the rebate. Each observation is an individual-week. Robust standard errors clustered at the individual level are in parentheses.
*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

check at the exact time their loans matured. I define a "control group" of borrowers who had a loan outstanding in the fourth week before their rebate arrived (such that their loans mature two weeks before their rebates arrive). Next, I define a "treatment group" of borrowers who had a loan outstanding in the second week before their rebate arrived (such that their loans mature exactly when their rebates arrive). I then compare the default rates for these two groups, finding that the control group's default rate is 6.48% ($N = 4798$) and that the treatment group's default rate is substantially lower at 4.79% ($N = 2526$). However, default rates for loans taken out the week after and two weeks after the rebate are much closer to the overall default rate of 9% (9.84% and 10.21%, respectively). So while there is some evidence that the rebate decreased the probability of default for the week it was received, this effect is short-lived.

Next, I explore whether these coefficients may be masking larger effects by subpopulation.

4.3. Heterogeneity and Anticipation

Table 7 shows results for various subpopulations.[13] For age, intensity of use (number of loan applications made during the two-year period), and

13. I also estimate the heterogeneity results using regressions that do not include any post-rebate weeks in the definition of "normal borrowing behavior," as in Table 5. Results, available upon request, are robust to this alternative definition.

Table 7. Probability of Borrowing by Subpopulation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Age			Intensity of Use (loan applications)			Credit Limit (max loan allowed in $)		
Weeks Relative to Rebate	Young (18–28)	Middle (29–45)	Older (46+)	Low (<9)	Middle (9–26)	High (27+)	Low (<$237)	Middle ($237–$473)	High ($474+)
Weeks before rebate									
−6	0.1161	0.0072	−0.0278	0.0731	−0.0968	0.1796	−0.0306	0.0103	0.1254
	(0.1007)	(0.0807)	(0.1070)	(0.0998)	(0.0792)	(0.1104)	(0.1113)	(0.0755)	(0.1094)
−5	−0.0338	−0.0183	0.0530	−0.0256	−0.0455	0.0881	0.1601	−0.0788	−0.0134
	(0.1001)	(0.0816)	(0.1129)	(0.1002)	(0.0832)	(0.1089)	(0.1205)	(0.0759)	(0.1069)
−4	−0.0541	0.0889	0.1940	−0.0414	0.0997	0.1752	0.0316	0.1237	0.0324
	(0.1041)	(0.0902)	(0.1240)	(0.1053)	(0.0925)	(0.1187)	(0.1234)	(0.0860)	(0.1145)
−3	−0.0357	0.1619	0.2289	−0.0233	0.1799*	0.1999	0.1942	0.1159	0.0741
	(0.1130)	(0.0987)	(0.1397)	(0.1136)	(0.1024)	(0.1328)	(0.1423)	(0.0929)	(0.1251)
−2	−0.1724	0.0343	0.1474	−0.0875	−0.0793	0.2595*	0.1114	0.0398	−0.1550
	(0.1149)	(0.1023)	(0.1441)	(0.1155)	(0.1022)	(0.1450)	(0.1442)	(0.0963)	(0.1294)
−1	−0.2122*	0.0111	0.1228	−0.1185	−0.1116	0.2445	0.0136	−0.0133	−0.0680
	(0.1208)	(0.1056)	(0.1492)	(0.1188)	(0.1049)	(0.1538)	(0.1478)	(0.1004)	(0.1352)
Rebate week	−0.2529**	−0.1680	−0.0100	−0.2230*	−0.1384	−0.0875	−0.1238	−0.1847*	−0.0970
	(0.1239)	(0.1056)	(0.1475)	(0.1187)	(0.1074)	(0.1506)	(0.1498)	(0.0987)	(0.1394)
Weeks after rebate									
1	−0.0516	0.1813	−0.0299	−0.0161	−0.0369	0.3079*	0.1562	0.0715	−0.0645
	(0.1364)	(0.1192)	(0.1626)	(0.1325)	(0.1160)	(0.1742)	(0.1681)	(0.1117)	(0.1488)

(continued)

2	−0.2594*	−0.1274	−0.1789	−0.3476***	−0.1095	−0.0885	−0.3194*	−0.1858*	−0.0155
	(0.1366)	(0.1166)	(0.1650)	(0.1245)	(0.1199)	(0.1723)	(0.1631)	(0.1108)	(0.1533)
3	−0.2771**	−0.0660	−0.0815	−0.0581	−0.2219*	−0.0555	−0.2066	−0.1758	0.0424
	(0.1410)	(0.1199)	(0.1718)	(0.1322)	(0.1216)	(0.1789)	(0.1700)	(0.1138)	(0.1589)
4	−0.1030	0.0554	0.1429	−0.2644**	0.0630	0.3372*	0.1059	−0.0428	0.1236
	(0.1460)	(0.1223)	(0.1720)	(0.1263)	(0.1263)	(0.1855)	(0.1757)	(0.1151)	(0.1605)
5	−0.1401	−0.1175	0.0462	−0.1219	−0.1608	0.1077	−0.1959	0.0235	−0.1744
	(0.1481)	(0.1219)	(0.1751)	(0.1333)	(0.1257)	(0.1841)	(0.1750)	(0.1191)	(0.1548)
6	−0.1224	−0.0741	−0.0064	−0.2899**	−0.0599	0.1837	−0.0200	−0.1140	−0.0272
	(0.1527)	(0.1237)	(0.1820)	(0.1321)	(0.1311)	(0.1904)	(0.1865)	(0.1185)	(0.1625)
7	−0.1311	−0.1752	0.0356	−0.3224**	−0.2086	0.3225	−0.2735	−0.0933	0.0315
	(0.1526)	(0.1235)	(0.1795)	(0.1278)	(0.1280)	(0.1974)	(0.1808)	(0.1187)	(0.1644)
8	−0.0846	0.1147	−0.2734	−0.1212	−0.0665	0.0776	0.0183	−0.0848	−0.0219
	(0.1491)	(0.1251)	(0.1667)	(0.1277)	(0.1272)	(0.1869)	(0.1786)	(0.1159)	(0.1595)
9	−0.1781	−0.1673	0.2775	−0.1933	−0.1348	0.2659	0.0824	−0.1977*	0.1086
	(0.1428)	(0.1183)	(0.1709)	(0.1216)	(0.1231)	(0.1851)	(0.1710)	(0.1112)	(0.1618)
10	0.1230	0.0784	−0.0998	−0.1218	−0.0070	0.3213*	−0.2422	0.2143*	−0.0237
	(0.1548)	(0.1233)	(0.1766)	(0.1287)	(0.1294)	(0.1933)	(0.1754)	(0.1216)	(0.1605)

(*continued*)

Table 7. Continued

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Age			Intensity of Use (loan applications)			Credit Limit (max loan allowed in $)		
Weeks Relative to Rebate	Young (18–28)	Middle (29–45)	Older (46+)	Low (<9)	Middle (9–26)	High (27+)	Low (<$237)	Middle ($237–$473)	High ($474+)
11	−0.1151	−0.0001	−0.1584	−0.2869**	0.0257	0.0104	−0.3040*	0.02740	−0.0443
	(0.1476)	(0.1237)	(0.1722)	(0.1208)	(0.1289)	(0.1915)	(0.1708)	(0.1181)	(0.1627)
12	0.0169	−0.0068	0.2500	−0.0017	0.0626	0.1687	−0.0775	0.1609	0.0315
	(0.1513)	(0.1234)	(0.1793)	(0.1261)	(0.1298)	(0.1945)	(0.1734)	(0.1203)	(0.1655)
Constant	0.1155***	0.2405***	0.2014***	0.2371***	0.2073***	0.1249***	0.2236***	0.1978***	0.1621***
	(0.0325)	(0.0356)	(0.0450)	(0.0409)	(0.0342)	(0.0391)	(0.0475)	(0.0314)	(0.0408)
Observations	1,116,796	1,823,974	1,067,604	1,233,154	1,742,102	1,033,118	1,000,180	2,000,102	1,008,092
Individuals	12,986	21,209	12,414	14,339	20,257	12,013	11,630	23,257	11,722
R^2	0.01	0.01	0.02	0.01	0.01	0.02	0.02	0.01	0.01

Notes: All results are from fixed-effects regressions using panel data, with individual fixed effects and indicators for each week to absorb time effects. Each observation is an individual-week. Subpopulation categories reflect the lower quartile, interquartile range, and upper quartile. Significance is calculated using robust standard errors clustered at the individual level for the week of the rebate and F-tests of the joint significance of the coefficients for the groups of weeks before and after the rebate.

*Significant at 10%.

**Significant at 5%.

***Significant at 1%.

credit limit, I run regressions separately for the lowest quartile, interquartile range, and highest quartile of each variable. Notably, column (1) shows that the youngest quartile (ages 18–28) is 0.259 percentage points less likely to take out a loan during the week of the rebate (with a 95% confidence interval of [−0.496, −0.010]), representing a 14% decrease from the baseline 1.76% probability of borrowing among this quartile. From the intensity-of-use columns, it is also interesting to note just how much people borrow: the interquartile range (25th–75th percentile) corresponds to taking out 9 to 26 loans during the two-year sample period. Column (5) shows that these medium-intensity borrowers are generally more likely to borrow before the rebate and less likely to borrow after the rebate, though the evidence is weak. Column (6) shows that the highest-intensity borrowers (27+ loans) are more likely to borrow both before and after the rebate relative to their normal borrowing behavior.

Columns (7)–(9) of Table 7 show results for low-, medium-, and high-credit-limit borrowers, as defined by the maximum loan size for which each borrower is eligible based on income. Notably, borrowers with the lowest credit limits are generally less likely to borrow following the rebate.

Many of the effects in the full sample and the subpopulations suggest that individuals are modifying their behavior in anticipation of the rebate. As shown in Figure 1 and in Tables 3 and 4, people are less likely than normal to borrow leading up to the rebate, indicating some form of anticipation. Anticipation could also explain the evidence of larger-than-normal loan amounts before receipt of the rebate shown in Tables 4 and 5. Anticipation also seems stronger among chronic borrowers: middle- and high-intensity borrowers seem more likely than low-intensity borrowers to take out loans in the weeks leading up to the rebate. Table 6 shows additional evidence of anticipation; borrowers who had a loan outstanding at the time the legislation containing the rebate provision was passed took out smaller loans and were less likely to borrow six weeks before the rebate arrived.

5. Discussion

A long line of research has explored the effect of tax rebates on consumers' borrowing, spending, and saving behavior, as I have here. My empirical strategy is the same as that of Agarwal et al. (2007), who study the effect of the 2001 tax rebate on credit card borrowing rather than payday borrowing. They find that borrowers receiving rebates did pay off credit card debt in the short run, but then increased their credit card spending soon afterward. Like the results here, their effects are strongest among high-intensity borrowers, who increased credit card spending by $200 in the nine months after the rebate.

My paper is most closely related to Bertrand and Morse (2009), who were the first to study the effects of tax rebates in the payday loan market. They consider the 2008 tax rebate, which was very similar to the 2001 rebate studied here, and find a 14–17% drop in the probability of borrowing during the week of the rebate that persists during the week after the rebate. Here, I find a seven percent drop in the probability of borrowing during the week of the rebate that persists two weeks later. Bertrand and Morse also find large and statistically significant drops in loan size around the rebate (a drop of $40, representing a 12% decrease). Here, I find only weak evidence of an impact on loan size, with loans before the rebate about $17 larger and loans after the rebate about $9 smaller. I also note that Bertrand and Morse do not consider anticipation effects, nor do they estimate the effect of the tax rebate on default.

As discussed earlier, my research improves on that of Bertrand and Morse and Agarwal et al. in terms of the size and reliability of the dataset, the time period covered, and the partial alleviation of measurement error concerns. Taken together, my results reveal a very modest and short-lived impact of the tax rebate on the three outcomes of interest. Whatever low-income consumers are doing with this sudden influx of cash, the vast majority are not using it to retire payday loan debt. Perhaps this is good news for policymakers who had hoped to stimulate the economy in the short run with tax rebates: borrowers spent (or saved) the money rather than paying off debt.[14] Research studying a more recent rebate supports my findings: the marginal propensity to consume from the 2008 stimulus checks was 52%, meaning that people saved about half and spent about half of those checks (Parker et al., 2013).

14. Indeed, this would be consistent with the well-documented “flypaper effect” (Hines and Thaler, 1995).

Because the size of the income shock I am studying is about the size of a typical payday loan plus the corresponding interest payment, one might have expected borrowers to avoid defaulting on outstanding loans and/or to avoid taking out new loans shortly after receiving the rebate. Furthermore, reasonably assuming that, at 400–600% APR, payday loans are the most expensive form of credit available to a borrower, a rational-actor model would predict that if a consumer has *any* disposable cash (such as that provided by the rebate), she would repay existing payday loan debt rather than taking out an additional loan or rolling over an existing loan.

If we observe no such effects (or only muted effects, as I observe here), and if we adhere to the rational-actor model to explain individuals' behavior, then we must conclude either that (1) there is some urgent need—like emergency medical treatment—that is trumping the otherwise rational decision to eliminate or reduce payday loan debt or (2) borrowers' payday debt burden is so large that a $300 rebate check is not enough to make a dent in their total amount owed. Given the borrowers' intensity of use—recall that the interquartile range of borrowing intensity was 9–26 loans in two years—the latter is a distinct possibility.

An infusion of cash does appear to decrease the frequency of borrowing most significantly for young, credit-constrained, and infrequent borrowers. Thus, regulatory attempts to break the cycle of payday lending may do well to use short-term strategies for these groups and to target older and more frequent borrowers with long-term structural approaches to help decrease their reliance on high-interest credit lines. Financial literacy training, for example, could be an important aspect of these long-term interventions.

The rebate appears to have a larger impact on borrowers with lower credit limits; this may simply indicate that these borrowers are bound by liquidity constraints. A $300 cash infusion had less impact on the budgets of the high-credit-limit borrowers; many of them continued to take out payday loans of about that size despite the rebate. This suggests that simply easing liquidity constraints will not necessarily prompt all payday loan borrowers to break out of the cycle of rolling over high-interest loans.

In sum, while appropriate regulation will concern itself with the repeated cycle of debt that is typical among borrowers, my results suggest that this cycle does not appear to be interrupted by a positive income shock (i.e., a tax rebate) that is the same size as the loan plus interest. A larger, more dramatic change to borrowers' budgets is needed. At a minimum, it could not be a bad thing to make more credit options available to borrowers who are excluded from the mainstream credit market and hence are likely liquidity constrained.

6. Conclusion

In light of the new evidence I have provided here on the difficulty of breaking payday loan cycles, crafting successful regulation to this end will not be an easy task. Federal regulators are considering several asymmetrically paternalistic policies,[15] such as requiring a month-long cooling-off period (during which borrowers would not be allowed to take out any additional loans) and extending the length of the payday loan term from one pay cycle to two. The idea is that these policies would nudge borrowers to accumulate enough cash to avoid entering or continuing a cycle of debt. My results here suggest that these policies will be ineffective; \$300 is simply not enough to break the payday debt cycle, and most borrowers are unlikely to be both able and inclined to save much more than that during the proposed cooling-off period. Other work suggests similar issues of budgetary constraint: a new study by Pew reports that "[only] 14% of borrowers can afford enough out of their monthly budgets to repay an average payday loan" (Pew Charitable Trusts, 2013). A cooling-off period may just prompt cash-strapped borrowers to turn to other forms of credit that are similarly expensive.

Payday loans are helpful to consumers when used as a stopgap for a short-term budget shortfall and then paid off. However, the typical borrower does not use them this way, and the industry thrives on consumers' long-term, repeated use of the product. To prevent costly months-long cycles of payday borrowing among a severely liquidity-constrained population, policymakers should consider broader changes to the way payday lending operates to ensure that the loans are truly used as a short-term solution. Slight changes to loan terms and small nudges are unlikely to meaningfully change borrowers' debt situations or borrowing behavior.

15. An asymmetrically paternalistic policy is one that helps error-prone individuals make better decisions while imposing small or no costs to others. See Camerer et al. (2003) for an overview.

Appendix

Table A1. Fixed-Effects Regressions

Weeks Relative to Rebate	(1) Probability of Borrowing	(2) Loan Amount ($) Full Sample	(3) Amount ($) Conditional on Borrowing	(4) Loan Probability of Default Full Sample	(5) Probability of Default Conditional on Borrowing
Weeks before rebate					
−6	0.0455	0.1424	17.3118*	−0.0158	0.5841
	(0.0548)	(0.1446)	(9.5405)	(0.0192)	(1.6731)
−5	0.0181	−0.0080	6.4018	0.0138	1.7909
	(0.0577)	(0.1499)	(9.7200)	(0.0216)	(1.9523)
−4	0.1034	0.0484	0.7203	−0.0213	3.6575**
	(0.0634)	(0.1607)	(9.8827)	(0.0218)	(1.7809)
−3	0.1552**	0.2856	9.7442	−0.0070	−0.0572
	(0.0707)	(0.1835)	(9.1972)	(0.0248)	(1.6834)
−2	0.0427	−0.1987	−1.7120	−0.0545**	0.7256
	(0.0741)	(0.1908)	(9.6568)	(0.0234)	(1.6817)
−1	0.0206	−0.0178	13.2625	−0.0021	2.4241
	(0.0794)	(0.2083)	(10.2568)	(0.0265)	(1.7457)
Rebate week	−0.1034	−0.3300	6.2139	−0.0385	3.0760*
	(0.0847)	(0.2223)	(10.5186)	(0.0263)	(1.8372)
Weeks after rebate					
1	0.1107	0.0703	3.0559	−0.0165	1.5991
	(0.0932)	(0.2425)	(10.1725)	(0.0301)	(1.7120)
2	−0.1214	−0.2903	1.2835	−0.0426	1.2351
	(0.0961)	(0.2531)	(11.0010)	(0.0298)	(1.8276)
3	−0.0672	−0.0866	6.7489	−0.0066	1.6239
	(0.1037)	(0.2729)	(11.0570)	(0.0316)	(1.9053)
4	0.1008	0.1573	2.6260	−0.0538*	1.4373
	(0.1127)	(0.2960)	(11.2505)	(0.0314)	(1.8593)
5	−0.0081	−0.2843	−1.7438	−0.0354	1.1352
	(0.1198)	(0.3105)	(11.3736)	(0.0348)	(1.9121)
6	0.0082	−0.1901	−0.1669	−0.0650*	1.6987
	(0.1272)	(0.3309)	(11.6169)	(0.0357)	(1.9247)
7	−0.0246	−0.0857	4.7114	−0.0089	3.6198*
	(0.1310)	(0.3455)	(11.9477)	(0.0376)	(1.9982)
8	0.0425	−0.2131	−7.2938	−0.0491	0.6395
	(0.1373)	(0.3562)	(12.2899)	(0.0369)	(2.0184)
9	0.0419	0.2131	8.7560	−0.0388	1.6523
	(0.1451)	(0.3830)	(12.5184)	(0.0393)	(2.0661)
10	0.1390	0.2299	3.6567	−0.0148	2.1609
	(0.1545)	(0.4059)	(12.5802)	(0.0421)	(2.1001)
11	0.0266	0.1529	4.2084	−0.0359	3.1595
	(0.1560)	(0.4126)	(12.9597)	(0.0419)	(2.1610)
12	0.1749	0.2569	3.3539	−0.0543	0.5936
	(0.1621)	(0.4271)	(13.1588)	(0.0421)	(2.1550)
Weeks 13 through N	0.1221	0.1911	2.6601	−0.0700*	0.5736
	(0.1652)	(0.4327)	(13.4112)	(0.0417)	(2.1857)

(continued)

Table A1. Continued

	(1)	(2)	(3)	(4)	(5)
Weeks Relative to Rebate	Probability of Borrowing	Loan Amount ($) Full Sample	Amount ($) Conditional on Borrowing	Loan Probability of Default Full Sample	Probability of Default Conditional on Borrowing
Constant	0.1952***	0.2189***	108.9519***	0.0086*	−19.5546***
	(0.0221)	(0.0434)	(22.526)	(0.0048)	(3.6037)
Observations	4,008,374	4,008,374	91,652	4,008,374	91,652
Individuals	46,609	46,609	44,099	46,609	44,099
R^2	0.01	0.01	0.07	0.00	0.05

Notes: All results are from fixed-effects regressions using panel data, with individual fixed effects and indicators for each week to absorb time effects. I report the effects for each of the six weeks before the rebate arrived, the week of the rebate, and each of the twelve weeks after the rebate, and collapse all other post-rebate weeks into a single indicator variable. Each observation is an individual-week. Robust standard errors clustered at the individual level are in parentheses.
*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

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