Spend to Save or Save to Spend?
A First Look at Round-up Saving *

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Abstract

We study the effect of enrollment in a round-up savings program on consumer spending behavior and financial outcomes. We find that upon enrollment in a round-up savings program, households increase their total spending. This effect is mainly driven by an increase in optional spending. We use a difference-in-differences estimator to measure effects of round-up savings on consumer spending compared to non-enrolled households. We find that consumers increase their spending upon enrollment by approximately $90 per month in the post-enrollment window. Compared to a saving amount of approximately $15 per month after enrollment, these results suggest a short-run problematic effect of round-up savings on household finances, especially for financially constrained households. Our results show that especially lower-income households, who have the highest proportional effect in spending scaled by income, have a higher probability of experiencing canceled transactions, account overdrafts, and bounced checks after enrollment.

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1 Introduction

Saving under liquidity constraints is a challenge for households. Additionally, emergency spending, self-control, and present bias hinder consumers' savings choices.[1] Nudges or commitment devices are possible methods to support households in implementing their saving plans.[2] While perfect commitment devices are difficult to implement in practice, imperfect or partial commitment devices are more common.[3].

Round-up savings programs are popular among consumers and financial institutions and are marketed as a spending-driven partial commitment device to address barriers to saving. Savings are the portion of disposable income that is not spent. However, round-up savings devices require the liquidity-constrained consumer to spend so that they can save. Do consumers spend more or less, upon enrollment in round-up savings devices? Consequently, do they save more? How does all this add up to their liquidity constraint?

A round-up savings program generates small savings amounts from each spending transaction, regardless of the amount. The same \$0.50 savings happen with a \$3.50 cup of coffee or a \$603.50 new set of tires. It can tempt households to spend more often to accumulate higher saving amounts. If maximizing savings through this vehicle is the consumer's objective function, then more spending transactions with smaller average amounts would make sense, as long as the total amount spent would decrease, leaving out more saved financial resources. Alternatively, households can irrationally regard this savings mechanism as allowing them to "save by spending". In that case, households will feel free to spend, alleviating the guilt of spending. While qualitatively appealing, a framework of this kind would lead to inevitable financial distress: keeping income constant and increasing spending mechanically decreases savings, or induces stricter liquidity constraints.

In this paper, we put round-up savings to the test, and investigate changes in consumer behavior upon enrollment in such a program. Our goal is to document systematic changes in consumer financial decisions upon enrollment. We use a transaction-level dataset from 2010 to 2015 containing debit and credit card transactions for all accounts registered in an online account aggregator. We observe the checking account associated with round-up transactions and the savings account to which

[1] See, for instance, Thaler & Shefrin (1981), Laibson (1994), Laibson (1997), Laibson et al. (1998), Baumeister (2002), Shefrin & Thaler (2004), Benhabib & Bisin (2005), Lusardi & Mitchell (2007)

[2] See, for instance, Thaler & Sunstein (2009), Karlan et al. (2016)

[3] See Laibson et al. (1998)

the round-up amounts are transferred. We observe income, spending, debt repayment, transfers, and instances of financial distress in the enrolled accounts, before and after enrollment.

Our first analysis compares each consumer with themselves before enrollment in an event study framework, around the enrollment date. We require the exact account of the consumer to be observable before the first instance of transactions related to the program. We observe a higher proportion of income being spent in the checking accounts associated with the round-up savings vehicle. The average increase in spending is 1% of monthly income, or around $36. Most of this effect is driven by increases in discretionary spending. The dynamic effects do not exhibit any pre-trends, and show a sharp increase of more than 2 percentage points in the spending-to-income ratio in the month following enrollment. For robustness, we run a difference-in-differences analysis where we include a control group of similar households, matched on observables. Using the estimator by Sun & Abraham (2021), we find average treatment effects of similar magnitudes.

We rule out spending substitution by analyzing changes in spending in other checking accounts utilized by the enrolled consumers, upon enrollment. We do not find detectable changes in spending. When investigating potential changes in means of payment, we do not find reductions in cash withdrawal activity. Surprisingly credit card spending increases, as a percentage of income. This is consistent with the interpretation that consumers perceive the savings device as a reduction in the guilt of spending, increasing spending even with payment methods that do not allow them to accumulate savings.

The estimated increases in spending exceed the $15 savings generated by the program, on average, every month. With binding liquidity constraints, given that there is no decrease in spending for other accounts, in cash, or with credit cards, this would imply a higher incidence of financial distress. Consistently, our findings show that after enrolling in round-up savings, households are more likely to experience overdrafts and bounced checks. These incidences of financial distress are not costless, as they usually originate fees, charged by banks.

We assess heterogeneity across the income distribution and show that individuals in the bottom quartile of income exhibit the largest spending increases, as a percentage of their income. While this result is partially mechanic, the effects do not evolve monotonically along the income distribution. Low-income individuals are also the ones who experience the largest increase in the incidence of financial distress. Lastly, they are also the most likely to have canceled round-up transfers due to

insufficient funds. Our results do not support round-up programs as useful saving or self-control devices. Low-income individuals, for whom liquidity constraints are more likely to bind, are especially vulnerable to the welfare consequences of round-up savings program design.

Round-up consumption decisions have appeared in academic work. Kelting et al. (2019) look at rounding-up decisions in connection to donating the difference and find that there are greater acceptance rates and donation likelihoods for round-up requests versus flat requests. Round-up programs are widely used both by legacy large financial institutions and by younger FinTech companies. Despite the widespread adoption, our paper is the first to document changes in consumer behavior upon adoption. It is only possible to do so, by overcoming the data constraint of being able to observe other account activity, adjacent to the round-up product for the individuals who enroll in these programs.

Our paper adds to the literature on the effectiveness of commitment devices. DellaVigna & Malmendier (2006) study gym contracts as commitment devices and find that, without direct financial penalties connected to non-compliance, this commitment device is less effective than hard commitment devices. Milkman et al. (2014) take up a similar experiment and study temptation bundling in the setting of gym visits. They bundle gym visits with audio novels and find an increase in gym visits for the treated group compared to the control group. However, they also report a decrease in the effect over time.

The most similar paper to ours is by Gargano & Rossi (2021), who find in a recent paper that soft, self-designed commitment devices, such as setting a savings goal, increases individuals' saving rate. In their setting, the consumer takes action every time to transfer a pre-designated to their savings account. Conversely, in round-up savings, saving happens passively. Finally, another difference is that in the goal-setting model, savings are not achieved via spending.

Our paper speaks to the challenge of saving under low income. Mullainathan & Shafir (2009) focus on low-income households and show that poor self-control can lead to a state of cognitive dissonance where one's actions and underlying intentions are not aligned. Our findings are consistent with theirs, where low-income individuals enroll in a savings program, which leads to the deterioration of their wealth.

In the next section of this paper, we talk about the data and the institutional setting. Section 3 discusses methodology and baseline results. In the fourth section, we report results from a

difference-in-differences analysis. In section 5, we conduct several robustness checks when it comes to substitution hypotheses and an enrollment shock. Section 6 then discusses in more detail who is affected by enrollment in a round-up savings program and how they are affected. In the last section we conclude.

2 Data and Institutional Setting

We study a round-up savings program offered by a major U.S. financial institution. To participate in the program, a customer is required to have a checking account and a savings account with the financial institution. Once enrolled in the program, every time a customer uses their debit card for a purchase, the transaction amount is rounded up to the nearest dollar and the difference is transfered to the customer's savings account. If a customer pays for a coffee worth $2.50, the transaction will show up on the checking account together with a pending round-up transaction of $0.50. This process is continued throughout the day and at the end of the day, the cumulative round-up amount is posted and transferred to the customer's savings account at the same financial institution. Round-up amounts range from $0.01 to $1.

We use transaction-level data from an online account aggregator for the period 2010-2015. From transaction descriptions, we observe transactions associated with the round-up savings program. Online account aggregators are one-stop shops consumers utilize to organize information from different financial accounts (e.g. checking, savings, credit card, brokerage, retirement, mortgage, and student loan) in one place. Data are limited to checking account, savings account, and credit card transactions. This makes the baseline dataset a transaction-level panel with information on the amount, date, and description of each transaction that a household makes. The full dataset is de-identified to protect the identity of the households. Households and accounts are given a unique user ID, which makes it possible to follow a household's transactions over time. More information on the dataset can be found in Baugh et al. (2018) and Baugh & Correia (2022).

In the context of account aggregator data, sample selection is a valid concern. This issue has been explored thoroughly by Baker & Yannelis (2017), Baker (2018), and Gelman et al. (2020) who find that in the case of their account aggregator data, there's a higher concentration of younger and male individuals using the service than there is in the U.S. consumer population. While it could

influence the external validity of the results, round-up saving is used heavily by FinTech companies whose average user base is generally younger and male as well (e.g. D'Acunto et al. (2020), Gargano & Rossi (2021)).

There are two possible ways for individuals to enroll in the program. First, they can find out about it and choose to enroll. Second, they can be advised or nudged to enroll after opening a checking and savings account or after opening a savings account when they already have a checking account. Enrollment can be done in person, via phone or online. Anecdotal evidence is heavy on the second scenario, with households reporting being enrolled by default. Because consumers self-select into the round-up savings program, our results are to be interpreted with caution. We see them as documenting changes in behavior upon enrollment, which can be revealing of how consumers regard round-up savings, rather than seeing them as the causal impact of round-up savings in consumer financial outcomes.

To attenuate concerns about self-selection into enrollment, we identify an enrollment shock in the Summer of 2012. This enrollment shock can be seen in Figure 1. We run a the same analysis as in the baseline results and find similar results for this subsample of around 2,000 accounts.[4] Figure 2 reports a frequency count of enrollment dates for the enrolled accounts after applying several filters to the data. The figure shows a higher enrollment activity for the early years from 2010 to the end of 2012 compared to the years 2013 to 2015.

Table 1 shows summary statistics at the household-month level for the sample of round-up users and non-round-up users, respectively. We track 7,774 households through the time frame of April 2010 to May 2015. The round-up household subsample is found after using several filters on the group of round-up households. Most importantly, households had to be in the sample for three months before enrollment, they had to enroll during the sample window and they could not show any signs of a sudden increase in income upon enrollment. Around 50% of households were already enrolled in the program at the beginning of the sample and thus are not included in the final round-up sample.

Round-up users are matched using coarse exact matching method on several variables. The resulting sample consists of 7,774 households in the round-up enrolled sample and the same number

[4]The differences in figures 1 and 2 stem from data filtering steps. If the financial institution enrolled a large number of accounts by default, and a large number individuals chose to unenroll, we will be running this analysis for individuals enrolled in this period, who did not unenroll.

in the non-round-up sample. The characteristics of these households are reported in 1 on a monthly level. Average inflows, which consists of income, transfers into the account and deposits, for both samples are approximately $3,500. The reported outflows, mainly those that can be paid for with a debit card, amount to approximately $2,000. This does not include expenses such as rent payments or debt repayment.

Table 1 further shows the subcategories which make up the main spending categories non-discretionary (necessary) and discretionary (optional) spending. Subcategory choice was based on whether debit card payment is possible for the respective item. The asterisks next to restaurant, retail, entertainment and travel signifies their assignment to a more conservative definition of the category optional spending. The more conservative definition is used throughout the analysis.

Figure 3 reports a frequency distribution of the monthly total round-up amount transferred from the checking to the savings account for all round-up users. Most monthly transfers to savings range between 0 and 5 dollars.

3 Spending to Save

In this section, we track spending from checking accounts directly associated with the round-up program. We follow account activity for 10 months, including the 3 months before enrollment, the enrollment month, and 6 months after enrollment. We start by estimating changes in average spending, conditional on observables. To analyze the dynamics of the effect, we also estimate event-study regressions. Lastly, to address selection in timing of enrollment, we use a differences-in-differences specification.

3.1 Pre- vs. Post- Enrollment

We evaluate changes in consumer choices after enrollment in round-up savings, relative to prior enrollment. With income constant, a round-up savings program achieves its goal if it leads to a decrease in spending, leaving more saved resources in the possession of the consumer. It is credible that round-up savings would lead to such an outcome: by leading to quicker depletion of liquid resources the consumer has access to, it could function as a disciplining device.

However, by design, it is a program that requires the act of spending, for savings to be accumulated, which goes against the life cycle model by Modigliani and Brumberg (1954). That is, in order to increase savings, consumption needs to be decreased to stay within the budget constraint. This motivates a direct look into spending around enrollment in the program. To do so, we start by comparing spending before and after the program for individuals who enroll in the program, and estimate a fixed-effects panel regression of the following form:

$$Y_{i,c,t} = \alpha_{c/i} + \alpha_t + \beta Enrollment_{i,c,t} + \varepsilon_{i,t}, \tag{1}$$

where $Y_{i,c,t}$ is total spending, discretionary spending or non-discretionary spending scaled by income for user i, from treatment cohort c at time t. $Enrollment_{i,c,t}$ is an indicator variable equal to 0 if the user is not enrolled in the round-up savings program at time t and 1 once they enroll. $\alpha_{c/i}$ varies between treatment cohort or household fixed effects across specifications. α_t are calendar time fixed effects, respectively. The standard errors are clustered at the household-level. The coefficient of interest, β represents the conditional difference in monthly spending before and after enrollment in the round-up savings program, controlling for cohort-specific characteristics and time-effects.

Table 2 reports an average increase in monthly total spending of roughly 1% of income (0.85%-1.91%). This is mostly driven by discretionary spending (0.78-1.41% of income) with modest effects on non-discretionary spending. Given the average value of pre-enrollment inflows of $3,232, this translates to an increase of approximately $36 of total monthly spending from the accounts enrolled in the program.

3.2 Event Studies

While it is a surprising fact that spending increases after enrollment in a savings program, a graphical description of how that increase takes place, and whether spending trends up before enrolment allows us to better understand consumer choices. To do so, we run an event study regression, stacking observations in event time, using the following specification:

$$Y_{i,\tau} = \alpha_i + \sum_{\tau=-3}^{6} \beta_\tau + \varepsilon_{i,\tau} \tag{2}$$

where $Y_{i,\tau}$ are total spending, discretionary spending or non-discretionary spending over income

for user i at event time τ. The β_τ coefficients are the coefficient of interest and decompose the effect of β in equation (1) in its dynamics around enrollment. α_i denote household fixed effects. In this specification we assume that the counterfactual against which we benchmark post enrollment spending is the pre-enrollment spending levels. The standard errors are clustered at the household-level. By not conditioning on any time fixed effects, we are not absorbing individual-invariant time patterns in spending. However, as pointed out by Borusyak et al. (2024), with each household fixed effect identifying a treatment cohort, and with estimation of dummies for event-time, any estimation of calendar time fixed effects would result in collinearity (event time equals calendar time minus treatment time) .

Figure 4 report the results. We omit the indicator for the period that immediately precedes enrollment ($s = -1$). Consumers increase spending by more than 2% of income in the first 3 months after enrollment, correcting to half of the magnitude in the three months later. This increase persists until at least 6 months into the program. Even though we report our most conservative estimate in column (4) of Table 2 of an total spending increase of 0.851% of income, our dynamic estimation consistently presents higher estimates than that, much more aligned with the estimates on columns (1)-(3). Column (3) is the equivalent estimation where only household fixed effects are included, being therefore our preferred specification, for comparison of approaches. The advantage of column (4) is that the estimate is conditional on macro trends across calendar quarters, which affect spending of all households.

The value of 2.28% for the ratio of total spending to income for the first month after enrollment translates into an approximately $74 increase in spending in the month after enrollment. While these dollar amounts may not appear sizeable when compared to a benchmark of no effect, the goal of the program is to save. Therefore the ex-ante expectation would be to see the program help consumers reduce their spending, rather than increasing it.

Panel C of Figure 4 shows a smaller increase for non-discretionary spending,. It is expected, as rent, gas mileage, and utilities are stickier expenses, than other discretionary spending, such as restaurants, entertainment, and travel. This result validates our interpretation of an alleviation of the guilt of spending, which is more likely to operate in discretionary spending. We attribute the small, but significant increase in non-discretionary spending to the limited level of discretion that still exists within that category. For example, grocery shopping can be influenced by spending guilt

alleviation, even though we consider it an essential expenditure.

The dynamics of the spending increase allow us to speak to alternative mechanisms laid out in the literature. A possible explanation for the observed increase in spending would be the "new toy" effect (Gargano & Rossi, 2021). In that case, we would observe higher levels of spending on the enrolled account right after enrollment but spending would fade away back to the original level, as the novelty of the round-up savings program becomes less salient. We do not observe such a sharp pattern, but we observe a reduction of the initial effect to half of its magnitude, mostly driven by oscillations in optional spending, where the consumer has more discretion as shown in Panel B. Given the dynamic effects we observe, we do not discard a mixed effect of both spending guilt alleviation and a "new toy" effect.

3.3 Difference-in-Differences

We find a sharp and persistent increase in spending immediately after enrollment in round-up savings. We argue that the counterintuitive design of the program, which prompts the consumer to spend to generate savings, might also alleviate the guilt of spending. Our analysis, however, is sensitive to (i) consumers timing their enrollment, and (ii) each consumer's pre-enrollment spending levels not being an ideal counterfactual to benchmark post-enrollment spending against.

We address enrollment timing using a staggered differences-in-differences design, we attenuate concerns regarding determinants of spending that correlate with enrollment across time. Among the many modern estimation procedures for average treatment effects (e.g. De Chaisemartin & d'Haultfoeuille (2020), Sun & Abraham (2021), Callaway & Sant'Anna (2021), Borusyak et al. (2024)) in staggered designs, we choose the Sun & Abraham (2021) estimator as it focuses on the non-contamination of coefficients for leads and lags in overlapping event studies. The estimator relies on a weighted average of average treatment effects for each treated cohort, relative to the control group of units which are never treated.

To address counterfactual suitability, we apply a coarse grid exact matching procedure (Iacus et al., 2012), where for each consumer who enrolls in the round-up savings program we find a similar consumer who does not enroll in the round-up savings program based on their ratios of total spending over income, non-discretionary spending, discretionary spending, debt repayment amounts, inflows, credit card utilization, and overdrafts in the three-month period before enrollment.

Figure 5 reports the results. Consistent with Figure 4, we do not observe detectable pre-enrollment trends in spending. If anything, spending is estimated to slightly decrease before enrollment in round-up savings, however, with a modest magnitude. We observe a sharp and persistent increase in spending, relative to similar non-enrolled households. The dynamics exhibit identical magnitude to our event-study specification (2% to 2.5% of income), and preserve the same shape as in the event-study specification.

4 Liquidity Constraints

So far, we have documented a robust, sharp, and persistent increase in spending using the account associated with the round-up savings program. We argue that a spending-driven savings device and a decrease in the consumer's spending guilt can induce an increase in spending. This argument ignores that, on average, liquidity constraints bind consumer decisions (e.g., Baugh & Correia (2022)). A sample filter we apply in our design is that income remains stable around enrollment in round-up savings. With a binding liquidity constraint, the additional spending we document has to lead to at least one of two scenarios: (i) spending decreases in other checking accounts, credit cards, or cash or (ii) violation of the liquidity constraint, leading to costly financial distress. In this section, we measure changes in these outcomes of interest to shed light on the extended impact of round-up savings on consumer financial decisions.

It is common for account aggregator users to link several checking accounts. After all, that is the goal of an account aggregator: to be able to aggregate one's finances across different institutions. Some individuals have different checking accounts, to execute some sort of mental accounting. Some others never close older accounts due to inertia. Some others appreciate the ability to analyze household-level financial information, where multiple individuals have their own and joint accounts. We start by measuring changes in spending in other checking accounts, not associated with round-up savings program, and report the results in Table 3.

With binding liquidity constraints, an increase in spending in the checking account associated with round-up savings could simply be financed by a decrease in spending in other checking accounts, around enrollment. Our point estimate is negative, suggesting a potential reduction in spending from other accounts. However it displays a large standard error, not allowing us to confirm the

substitution hypothesis. While it is reassuring that we do not reject the null hypothesis of zero change in other accounts, we do not observe other accounts for many of the households who enroll in the round-up savings program. This directly generates larger standard errors, and hinders our ability to estimate dynamic effects beyond 3 months after enrollment, as shown in Figure 6.

Next, we measure changes in credit card spending around enrollment. If round-up savings decrease the guilt of spending, two opposing scenarios are possible. The first is that the consumer understands that it is less bad to spend from the account where round-up occurs, and shifts their spending from credit card to the debit card associated with the savings program. Conversely, a spending halo effect can happen, in which the action of spending in any form becomes associated with lower guilt. If this is the case, then we should observe an increase in credit card spending. We report the results in Table 3.

We observe an average increase in credit card spending of 0.49%-1.33% of income, much in line with the halo effect hypothesis, and suggesting that the increase in spending documented before does not result from a shift from credit card to debit card usage. The dynamics reported in Figure 7 show an increase of credit card usage immediately following enrollment, with magnitudes around 1% of income per month, doubling at the 6-month mark after enrollment. This is consistent with the use of liquidity provided by credit cards, to mitigate the aggravated liquidity constraint posed by the direct effect of the deduction of round-up amounts from the account, and the indirect effects of the increase in the desire to spend.

Consumers increase their spending with both the debit card associated with the enrolled checking account, as well as with their credit card. They do not reduce spending from other checking accounts. The last possible source of adjustment is the cash margin. It is possible that this program just nudges the consumer to digitize their payments. The consumer starts spending using their cards, while alleviating the use of physical currency. We observe ATM withdrawals, and while not all of it is necessarily spent (it could be saved, given to family or friends, and even deposited in other accounts), it proxies for cash activity. In Table 5 we estimate the change in cash activity upon enrollment. As shown by the four columns there is a significant reduction in cash activity of around 0.8% of income. While this is a sizeable effect, it does not fully offset the increase of 1.23% in debit card spending and the additional 1.3% in credit card spending. Furthermore, not necessarily the full reduction of 0.80% of income in cash activity is a reduction in spending.

We interpret this result as an additional mechanism through which the program operates: replacing the cash change jar, and leading the consumer to a more digital spending activity. Figure 8 shows a steady decrease of cash activity upon enrollment, without any reversal indicative of a new toy effect in the cash to card transition. An alternative and consistent interpretation is that the measure relies on ATM withdrawals and consumers start withdrawing less cash upon enrollment simply because they have less cash to withdraw. In other words, lower withdrawal levels can be an outcome of tighter liquidity constraints.

As shown in Table 6, consumers save an average $15 every month after enrollment. However, they are increasing spending by approximately $36, not decreasing spending on other means of payment, and their income is conditioned not to change around enrollment. With binding liquidity constraints, this sharp change can lead to financial distress. We do not observe account balances, but we do observe transactions associated with bounced checks, overdrafts, and non-sufficient funds fees. The incidence of these transactions is our chosen metric to assess financial distress. First, they do indeed signal when a consumer runs out of liquid holdings. Second, they represent a costly instance of financial distress.

In Table 7 we estimate the change in the propensity to experience an overdraft or a bounced check as an indication of binding liquidity constraints. We see an increase of 2-3 percentage points in the monthly incidence of financial distress. Figure 9 shows that this effect is very sharp and consistently increases during the 6 month window after enrollment.

Lastly, we document the heterogenous effects of liquidity constraints along the income distribution in Table 8. We interact the enrollment indicator with indicators for each quartile of income in our sample, and use as dependent variable the occurrence of a canceled round-up transaction due to insufficient funds. As expected, pre-enrollment none of the income groups experiences any of these cancellations and there are no detectable differences among them. After enrollment, the bottom quartile of income is 20 p.p. likely to experience one such event. This number reduces for half its magnitude for the top quartile of income, and the reduction is monotonic along quartiles of income. Altogether, we take these results as a confirmation of the aggravation of liquidity constraints, which are more likely among the most vulnerable region of the income distribution.

5 The Economics of Round-up Savings

In this section, we take the results and try to reconcile them to explain consumer behavior, and how round-up savings programs are regarded. We start by contrasting the goal of the program, savings, with the way to achieve it, spending. This is especially contradictory when liquidity constraints bind. We take the counter-intuitive design of the program as given when evaluating consumers' perceptions of it, through the choices we observe.

In a round-up program, if the consumer's objective function is to increase savings using the program, the first step to maximize potential savings is maximizing the number of transactions. Thus, we will define a customer as *trier* if they increase the number of transactions.

Additionally, increasing the number of transactions is not enough to achieve the goal. If a consumer increases the number of transactions but does not decrease the amount per transaction, so that their total spending decreases, they are forcibly not saving more. We will define as *achiever* a consumer that upon enrollment in round-up saving, decreases total spending, or, achieves the goal of increasing savings. Consider a 2x2 breakdown between *triers* and *achievers* consumers. Within the achievers, we can split them across *triers* and non-*triers*.

Round-up programs appear especially suited to those who, conditional on wanting to maximize savings through the program, value spending frequency more than spending quantity. These individuals recognize the concavity of the utility function associated with each transaction and prefer many small transactions to a few large spending episodes. The *trier-achiever* reduces transaction amounts and increases transaction frequency so that total spending decreases and they end up increasing savings. The round-up program might provide the extra incentive they need to implement this plan.

The *no-try-achievers*, who reduce their total spending by reducing the number of spending episodes would not need the round-up program to achieve their goal in the first place. They are an uninteresting case because they do not fall prey to the round-up program's embedded feature of rewarding a higher number of transactions.

Among the *non-achievers*, the *trier-non-achiever* is an interesting case study. Triers value spending frequency. If they succumb to an alleviation of spending guilt and associate spending with saving, they can easily end up spending more, than they used to, even when trying to use the

program in their favor. This type of consumer would even act consistently with the halo effect of spending and increase their spending with other means of payment like credit cards or cash. Lastly, the consumer who does not try and does not achieve is very unlikely to be motivated by the objective function of maximizing their savings through the round-up program. If they do, they completely lack the conditions to implement such a plan. A round-up savings program would therefore be of no service to them.

6 Conclusion

We study the changes in consumer behavior upon enrollment in round-up savings programs using data from an account aggregator. We find a sharp and steady increase in spending, which is primarily driven by an increase in discretionary spending.

Across our analyses, we find an average increase in monthly spending in the post-enrollment period of $74 in the account associated with the program while the average amount saved with the program is $15 per month.

We do not find evidence for substitution effects away from other accounts. Credit card spending increases in by the same proportion as debit card spending. While cash activity decreases, the decrease is not enough to finance the increase in spending. Rather, we interpret that as an outcome of a tighter liquidity constraint.

Lastly, we document welfare consequences. We find a sharp and persistent increase in the incidence of costly events of insufficient funds, such as overdrafts or bounced checks. We also see that lower income enrollees are twice more likely to run out of funds, when triggering the round-up savings transfer than their higher income counterparts.

Possible channels to explain the results lie in temptation bundling and gamification. By combining the "want", which is spending in our setting, with the "should", which is saving, households can multitask and passively increase saving by increasing spending. Despite temptation bundling being a good tool to deal with self-control issues, in the case of spending and saving, it can also lead to a deterioration in self-control issues and lead to financial struggles for households. The gamification channel works through a self-imposed challenge to reach a significant savings amount. This effect would be expected to taper off with a decrease in salience.

This first analysis of round-up programs suggests that an exact setup as in the program we study might on average not be the most beneficial one if spending is not controlled and inflows are constrained. FinTech companies might have different tools at hand to attenuate the initial negative impact on financial health. That is, they could work with different setups and notifications to the users once an abnormal increase in spending is detected, for example. Future research could investigate these open questions and provide insights into round-up savings programs offered by financial technology companies to learn more about more recent approaches to round-up savings.

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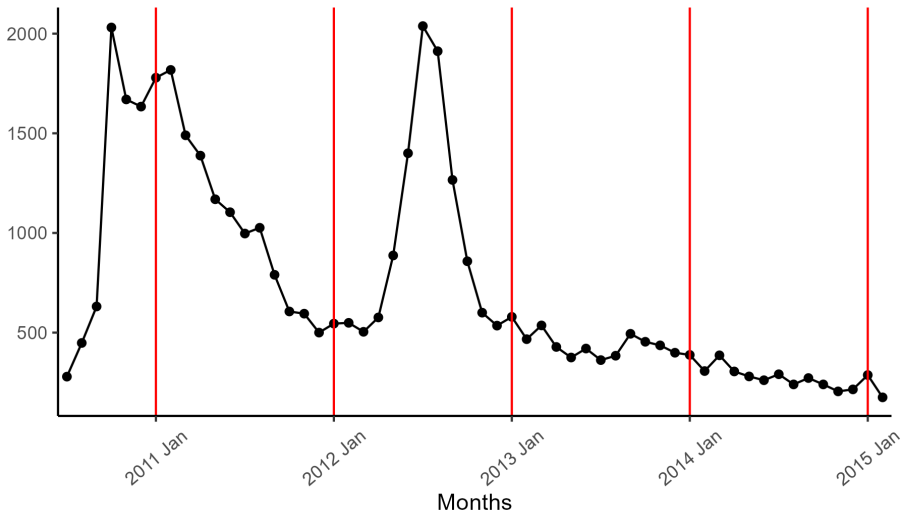
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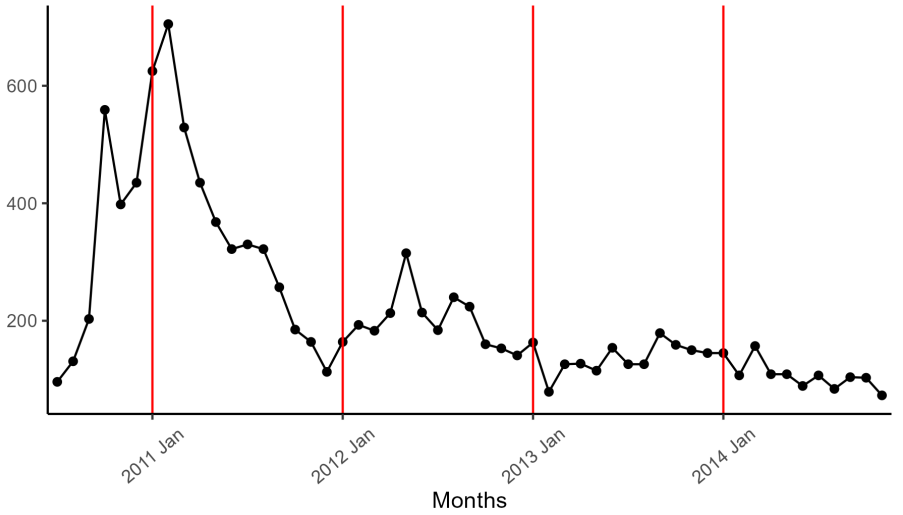
7 Figures and Tables

Figure 1: Enrollment Frequency by Month before Filtering



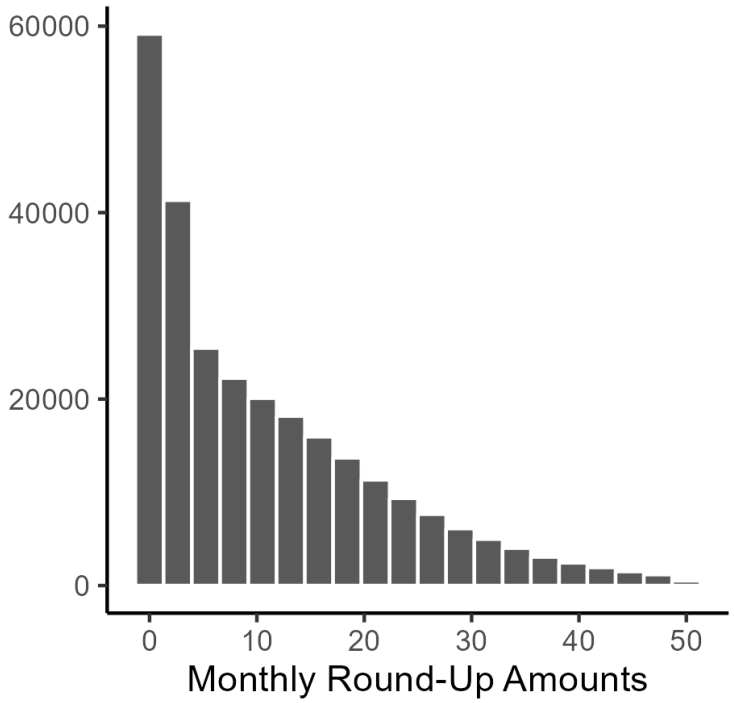
Note: This figure shows the monthly frequency counts for the window of July 2010 to February 2015. Red vertical lines indicate January of each year from 2011 to 2015. This figure represents the unfiltered subsample of enrolled accounts.

Figure 2: Enrollment Frequency by Month after Filtering



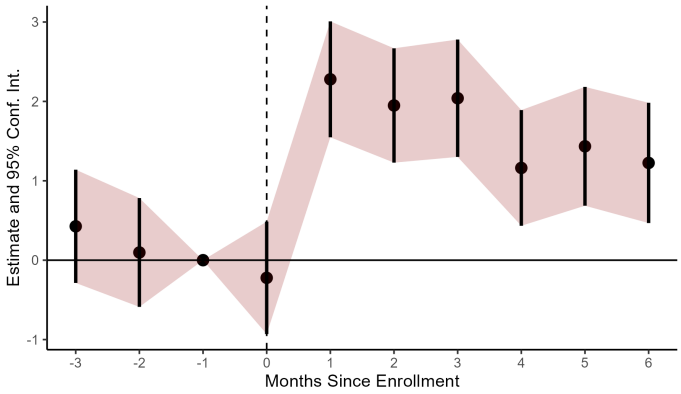
Note: This figure shows the monthly frequency counts for the window of July 2010 to February 2015. Red vertical lines indicate January of each year from 2011 to 2015. This figure represents the filtered subsample of enrolled accounts used for the analysis.

Figure 3: Histogram for Round-Up Amounts $\leq$ \$50

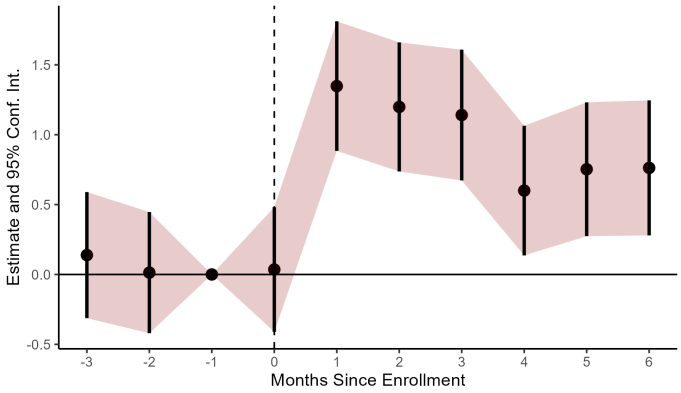


Note: This figure shows the frequency distribution of round-up saving amounts less than or equal to \$50 for the full, non-filtered sample of households who are enrolled in the round-up program.

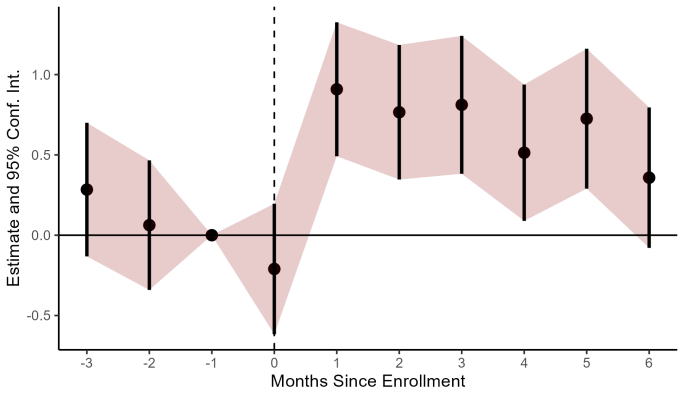
Figure 4: Spending Dynamics Around Enrollment



(a) Total Spending / Income



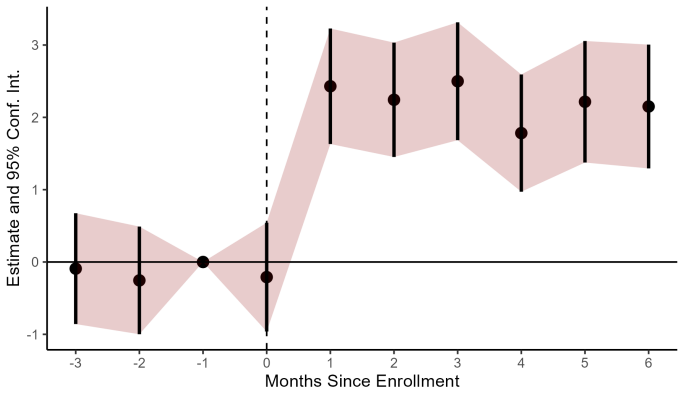
(b) Optional Spending / Income



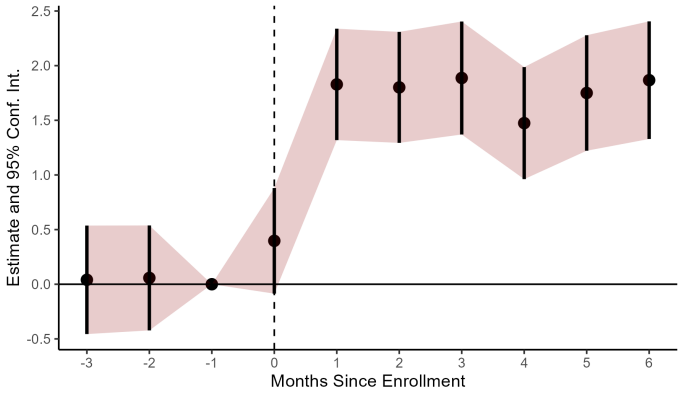
(c) Necessary Spending / Income

Note: This figure displays coefficient estimates and 95 % confidence intervals of the coefficients for an event-study regression with household fixed effects. Outcomes are total spending in panel (a), optional spending in panel (b), and necessary spending in panel (c) over income. The standard errors are clustered at the household level. The omitted time period is month -1, the month prior to enrollment.

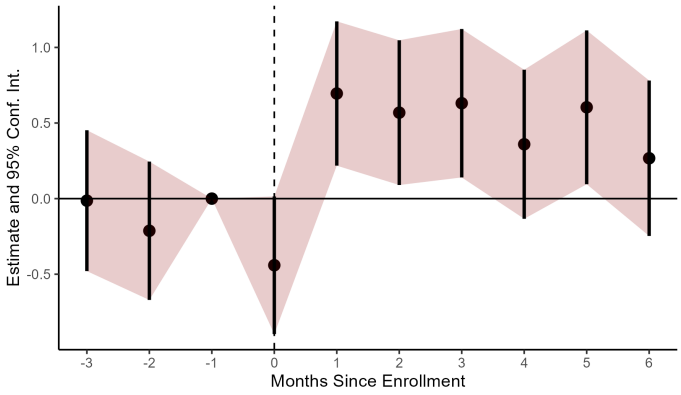
Figure 5: Differences-in-differences: Spending in Enrolled Accounts



(a) Total Spending / Income



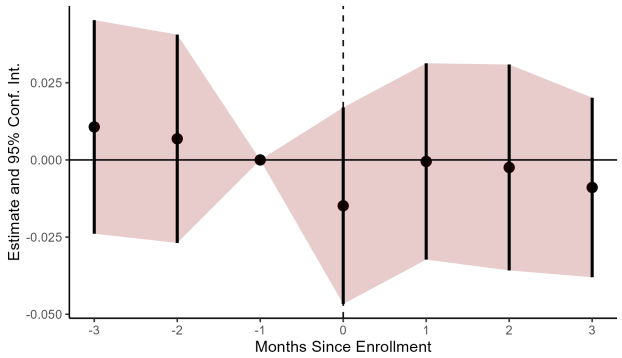
(b) Optional Spending / Income



(c) Necessary Spending / Income

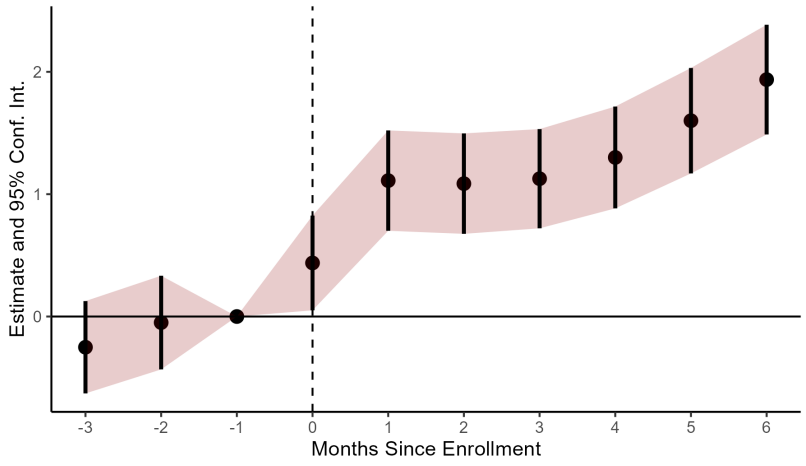
Note: This figure displays coefficient estimates and 95 % confidence intervals of the coefficients for an event-study differences in differences regression (Sun & Abraham, 2021) with cohort fixed effects and calendar year fixed effects. Outcomes are total spending in panel (a), optional spending in panel (b), and necessary spending in panel (c) over income. The standard errors are clustered at the household level. The omitted time period is month -1, the month prior to enrollment.

Figure 6: Other Checking Accounts Spending Dynamics Around Enrollment



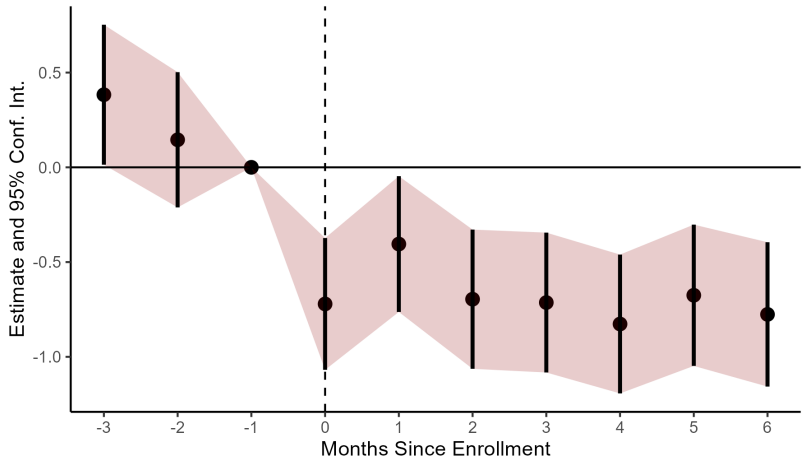
Note: This figure displays coefficient estimates and 95 % confidence intervals of the coefficients for an event-study regression with household fixed effects. Outcome is total spending on other checking accounts over income. The standard errors are clustered at the household level. The omitted time period is month -1, the month prior to enrollment.

Figure 7: Credit Card Spending Dynamics Around Enrollment



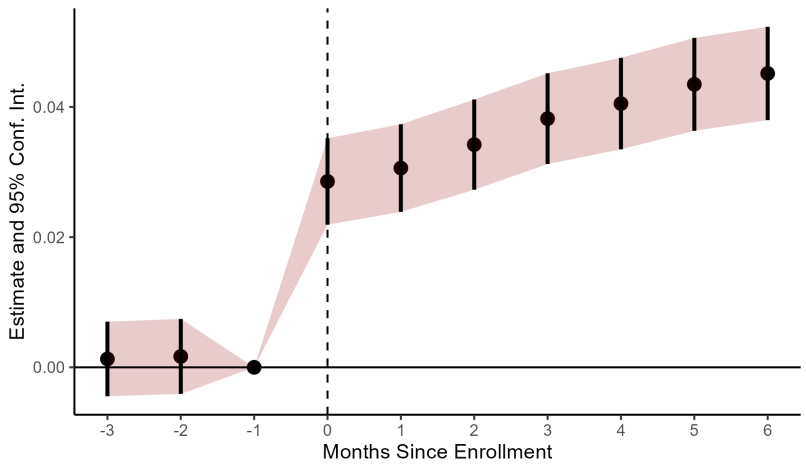
Note: This figure displays coefficient estimates and 95 % confidence intervals of the coefficients for an event-study regression with household fixed effects. Outcome is credit card spending over income. The standard errors are clustered at the household level. The omitted time period is month -1, the month prior to enrollment.

Figure 8: Cash Activity Dynamics Around Enrollment



Note: This figure displays coefficient estimates and 95 % confidence intervals of the coefficients for an event-study regression with household fixed effects. Outcome is cash activity over income. The standard errors are clustered at the household level. The omitted time period is month -1, the month prior to enrollment.

Figure 9: Financial Distress Dynamics Around Enrollment



Note: This figure displays coefficient estimates and 95 % confidence intervals of the coefficients for an event-study regression with household fixed effects. Outcome is propensity to overdraft or bounce a check. The standard errors are clustered at the household level. The omitted time period is month -1, the month prior to enrollment.

Table 1: **Summary Statistics**

	ROUND-UP ONLY			NON-ROUND-UP ONLY		
Variable	**Mean**	**Std. Dev.**	**Median**	**Mean**	**Std. Dev.**	**Median**
Inflows	$3,522	$2,352	$3,013	$3,457	$2,430	$2,924
Necessary Spending	$524	$364	$442	$501	$370	$421
Utilities	$163	$169	$113	$169	$178	$118
Gas	$90	$88	$66	$76	$87	$48
Groceries	$243	$225	$176	$230	$234	$158
Healthcare	$27	$36	$15	$25	$38	$11
Optional Spending	$1,056	$753	$861	$999	$788	$803
Restaurant*	$93	$78	$73	$72	$78	$49
Retail*	$311	$262	$241	$300	$279	$226
Entertainment*	$43	$61	$21	$36	$63	$13
Travel*	$46	$76	$18	$39	$79	$10
Misc. Cons.	$20	$27	$10	$16	$27	$6
Misc. Bills	$230	$429	$69	$266	$514	$73
Uncategorized	$313	$304	$229	$270	$316	$177
Credit Card	$473	$755	$204	$462	$776	$153
Round-Up Saving	$10	$7	$9	-	-	-
Overdraft Fee Amt.	$3	$8	$0	$2	$7	$0
Households		7,774			7,774	

Note: This table provides summary statistics from my sample from 2010 to 2015. The unit of observation is account-month. The first three columns present summary statistics for the subset of accounts which are enrolled in the round-up program, the last three columns present summary statistics for all matched accounts that are not enrolled in a round-up program. Variables are winsorized at the 1% level. The asterisks signify assignment to a more conservative definition of the category discretionary spending.

Table 2: **Round-up Savings Enrollment and Spending**

	(1)	(2)	(3)	(4)
	Panel A: Total Spending / Income			
Enrollment	1.235***	1.919***	1.235***	0.851***
	(0.196)	(0.228)	(0.196)	(0.283)
Observations	77,750	77,750	77,750	77,750
R^2	0.005	0.005	0.405	0.407
Within R^2	0.000	0.001	0.001	0.000
	Panel B: Optional Spending / Income			
Enrollment	0.784***	1.424***	0.784***	0.901***
	(0.127)	(0.146)	(0.127)	(0.180)
Observations	77,750	77,750	77,750	77,750
R^2	0.005	0.006	0.410	0.411
Within R^2	0.000	0.001	0.001	0.000
	Panel C: Necessary Spending / Income			
Enrollment	0.438***	0.491***	0.438***	-0.007
	(0.118)	(0.135)	(0.118)	(0.166)
Observations	77,750	77,750	77,750	77,750
R^2	0.006	0.006	0.439	0.440
Within R^2	0.000	0.000	0.000	0.000
Cohort FE	✓	✓		
Year FE		✓		
Household FE			✓	✓
Year-Quarter FE				✓

Note: This table reports coefficients for a post-enrollment dummy in a regression with varying sets of fixed effects across columns (1) to (4). In this regressions, the unit of observation is household-months, and we measure changes in total spending in Panel A, optional spending in Panel B, and necessary spending in Panel C, scaled by monthly income. In this sample we only individuals who sign up round-up savings. Standard errors are clustered at the household level. Significance levels are *: 0.10; **: 0.05; and ***:0.01.

Table 3: **Round-up Savings Enrollment and Spending on Other Accounts**

	(1)	(2)	(3)	(4)
	Other Checking Spending / Income			
Enrollment	-0.012	-0.010	-0.013	-0.015
	(0.009)	(0.010)	(0.009)	(0.015)
Observations	5,316	5,316	5,316	5,316
R^2	0.035	0.035	0.393	0.396
Within R^2	0.000	0.000	0.000	0.000
Cohort FE	✓	✓		
Year FE		✓		
Household FE			✓	✓
Year-Quarter FE				✓

Note: This table reports coefficients for a post-enrollment dummy in a regression with varying sets of fixed effects across columns (1) to (4). In this regressions, the unit of observation is household-months, and we measure changes in total spending in other accounts, scaled by monthly income. In this sample we only individuals who sign up round-up savings. Standard errors are clustered at the household level. Significance levels are *: 0.10; **: 0.05; and ***:0.01.

Table 4: **Round-up Savings Enrollment and Credit Card Spending**

	(1)	(2)	(3)	(4)
	Credit Card Spending / Income			
Enrollment	1.329***	1.026***	1.329***	0.491***
	(0.120)	(0.133)	(0.119)	(0.162)
Observations	77,750	77,750	77,750	77,750
R^2	0.007	0.007	0.531	0.532
Within R^2	0.001	0.000	0.002	0.000
Cohort FE	✓	✓		
Year FE		✓		
Household FE			✓	✓
Year-Quarter FE				✓

Note: This table reports coefficients for a post-enrollment dummy in a regression with varying sets of fixed effects across columns (1) to (4). In this regressions, the unit of observation is household-months, and we measure changes in credit card spending, scaled by monthly income. In this sample we only individuals who sign up round-up savings. Standard errors are clustered at the household level. Significance levels are *: 0.10; **: 0.05; and ***:0.01.

Table 5: **Round-up Savings Enrollment and Cash Activity**

	(1)	(2)	(3)	(4)
	Cash Activity / Income			
Enrollment	-0.864***	-0.791***	-0.864***	-0.801***
	(0.106)	(0.120)	(0.106)	(0.145)
Observations	77,750	77,750	77,750	77,750
R^2	0.008	0.008	0.500	0.501
Within R^2	0.001	0.000	0.001	0.000
Cohort FE	✓	✓		
Year FE		✓		
Household FE			✓	✓
Year-Quarter FE				✓

Note: This table reports coefficients for a post-enrollment dummy in a regression with varying sets of fixed effects across columns (1) to (4). In this regressions, the unit of observation is household-months, and we measure changes in cash activity, scaled by monthly income. In this sample we only individuals who sign up round-up savings. Standard errors are clustered at the household level. Significance levels are *: 0.10; **: 0.05; and ***:0.01.

Table 6: **Round-up Savings Enrollment and Spending on Other Accounts**

Month	**Mean**	**St. Dev.**	**Min**	**Median**	**Max**
0	9.46	8.75	0.00	6.93	79.17
1	16.23	11.65	0.00	14.03	126.07
2	16.12	12.29	0.00	13.78	229.80
3	15.67	11.81	0.00	13.56	216.06
4	15.33	11.59	0.00	13.26	106.58
5	15.08	11.79	0.00	12.97	107.87
6	14.64	11.73	0.00	12.64	94.38

Note: This table provides summary statistics for the amount saved per event month from enrollment in the round-up program on. Enrollment happens in month 0.

Table 7: **Round-up Savings Enrollment and Financial Distress**

	(1)	(2)	(3)	(4)
	Propensity to Financial Distress			
Enrollment	0.036***	0.038***	0.036***	0.025***
	(0.002)	(0.002)	(0.002)	(0.003)
Observations	77,750	77,750	77,750	77,750
R^2	0.009	0.009	0.313	0.314
Within R^2	0.004	0.003	0.006	0.001
Cohort FE	✓	✓		
Year FE		✓		
Household FE			✓	✓
Year-Quarter FE				✓

Note: This table reports coefficients for a post-enrollment dummy in a regression with varying sets of fixed effects across columns (1) to (4). In this regressions, the unit of observation is household-months, and we measure changes in the propensity to overdraft of bounce a check. In this sample we only individuals who sign up round-up savings. Standard errors are clustered at the household level. Significance levels are *: 0.10; **: 0.05; and ***:0.01.

Table 8: **Liquidity Constraint - Heterogeneity**

	Canceled Transactions	
	(1)	(2)
Enrollment	0.205***	0.202***
	(0.006)	(0.006)
Q2	0.000	0.000
	(0.001)	(0.001)
Q3	0.001*	0.001
	(0.001)	(0.001)
Q4	0.001	0.000
	(0.001)	(0.001)
Enrollment × Q2	-0.033***	-0.032***
	(0.008)	(0.008)
Enrollment × Q3	-0.066***	-0.066***
	(0.008)	(0.008)
Enrollment × Q4	-0.105***	-0.104***
	(0.008)	(0.008)
Observations	77,750	77,750
R^2	0.066	0.066
Within R^2	0.063	0.047
Cohort FE	✓	✓
Year FE		✓

Note: This table reports coefficients for a post-enrollment dummy interacted with dummies for quartiles of the income distribution in a regression with enrollment cohort fixed effects in column (1) and additional calendar year fixed effects in columb (2). In this regressions, the unit of observation is household-months, and we measure changes in the propensity to have a round-up transaction canceled due to insufficient funds. In this sample we only individuals who sign up round-up savings. Standard errors are clustered at the household level. Significance levels are *: 0.10; **: 0.05; and ***:0.01.