

Super Savers?

A Randomized Evaluation of Commitment Savings and Financial Counseling in New York City

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ABSTRACT

We partner with a New York City-based credit union to test a commitment savings product and financial counseling among a low-income population. The product, marketed as a Super Saver Certificate of Deposit (SSCD), allows gradual deposits toward a client's savings goal but imposes penalties for missed goals or early withdrawals. We randomly assigned credit union members to a SSCD product offer, an offer of free financial counseling, or a survey-only control group. We find strong demand for both SSCD and counseling that is positively correlated with proxies for behavioral biases. 65.7% of SSCD holders avoided substantial penalties by holding to maturity, and the average closing balance was \$910. However, only 32.3% of SSCD clients met their chosen goal amount, and we do not find significant evidence that either the SSCD product offer or the financial counseling treatment increases savings balances or net assets, or affects borrowing behavior, relative to our control group.

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I. Introduction

Saving money can be difficult. Some U.S. policies subsidize private savings – through channels such as workplace 401(k) programs – and nevertheless many households wish to save more than they do.¹ Growing evidence suggests that self-control issues may contribute to under-saving, so that effective pre-commitment to save more can be welfare-improving for some individuals.² In this study we debut a commitment savings product in the U.S. context and use a randomized control trial to evaluate its effects on savings, net assets, and borrowing behavior.

We also test our commitment product’s effectiveness relative to a more conventional financial counseling service. Whereas commitment savings products – and commitment devices more generally – have unique features to address self-control problems,³ other behavioral factors may be better addressed by one-on-one counseling (or other interventions).⁴ For example, savers may also be challenged by limited numeracy or literacy (Lusardi & Tufano 2009), exponential growth bias for compounding interest (Soll, Keeney, & Larrick 2011; Stango & Zinman 2011a), limited attention (Karlán et al. 2011 & Kast et al. 2012), information overload (Iyengar, Huberman, & Jiang 2004; Bertrand et al. 2010), and planning fallacies (Peetz & Buehler 2009).

Recent research has been mixed on the question of why, and whether, U.S. households are undersaving. The need to save for growing medical expenses and other in-retirement shocks suggests that savings rates are too low for many and perhaps most U.S. households (Skinner 2007; Poterba, Venti, & Wise 2012). On the other hand, some studies that emphasize the heterogeneity of precautionary savings needs (Engen, Gale, & Uccello 1999) or the roles of government transfers and uncertain life expectancies (Scholz, Seshadri, & Khitatrakun 2006) have found evidence of adequate savings for a majority of households – though they also find under-saving concentrated among low-income households. Simulations of savings behavior (Angeletos et al. 2001) have generated a similar pattern when households have heterogeneous self-control problems: some households build adequate savings levels but households with self-control problems do not.

¹ Cite: <http://www.americasaves.org/downloads/www.americasaves.org/PressReleases/02.20.01.pdf>

² Ashraf, Karlán, and Yin (2006), Benartzi & Thaler (2004)

³ See Bryan, Karlán, & Nelson 2010 for a review of commitment devices and their motivation.

⁴ See Collins and O’Rourke (2010) for a review of financial education and counseling evaluations.

Some evidence for the role of self-control and commitment in savings behavior comes from tests of U.S. workplace savings programs, where a combination of defaults and (weakly) sticky choices for contributions to retirement accounts has led to marked savings increases (see e.g., Madrian & Shea 2001, Benartzi & Thaler 2004, Carroll et al 2009, Beshears et al 2011, Choi et al 2011). Other, more direct evidence comes from an evaluation of an explicit commitment savings account in the Philippines (Ashraf, Karlan, & Yin 2006). This “SEED” account had no particular benefits other than its commitment feature to not withdraw funds until a goal was reached, yet it experienced robust demand (28% take-up), particularly among survey respondents with evidence of self-control problems, and within a year it increased total treatment-group savings held at the bank by 81% relative to a control group (ITT estimate).

We worked with the nonprofit organization Neighborhood Trust Financial Partners (formerly Credit Where Credit is Due) in New York City, and its financial institution partner Neighborhood Trust Federal Credit Union, to adapt the original commitment savings concept from the Philippines SEED product and test its impact on savings accumulation among credit union members in New York City. The product, marketed as a Super Saver CD (SSCD), allowed credit union members to develop customized savings goals and commitment terms for the product, as well as create a deposit schedule for regular savings contributions. The commitment device employed by this product was similar to SEED’s – members could not withdraw funds from their Super Saver CD before reaching their savings goal without closing the account, forfeiting earned dividends and forfeiting an initial deposit amount of \$15.

Neighborhood Trust also worked with us to randomize offers of its best-practice financial counseling program. As such this paper is the first randomized evaluation of a counseling program in the U.S.

Our sample consists of 1167 members of Neighborhood Trust Federal Credit Union in New York City. Between March 2010 and May 2011, surveyors approached credit union members in the lobby of their single branch office in Washington Heights (Upper Manhattan) to take a 10-minute survey in exchange for a free subway pass. Those who both completed the survey and also authorized us to conduct soft pulls of their credit report⁵ were then randomized

⁵ Unlike the hard inquiries made by financial institutions as part of a credit application process, soft pulls have no effect on an individual’s credit score and do not appear on the report.

to one of three arms: an offer of the commitment (Super Saver CD) product, an offer of a free financial counseling session, or a survey-only control group that was not offered either the commitment product or the counseling.

We measure all survey respondents' savings balances and debt at the credit union on a quarterly basis throughout 2010 and 2011, and also measure borrowing behavior using credit reports pulled in February 2012. Our estimates of the impacts of the SSCD and the counseling service focus on intention-to-treat effects: effects of product availability (i.e., offers) on savings and debt balances.

We find fairly strong demand for both offers: take-up rates are 21.3% for the CD and 15.9% for counseling.⁶ Another 33.0% made a counseling appointment but did not keep it, so our overall counseling *sign-up* rate was 48.9%, comparable to the 55% sign-up rate in Meier & Sprenger (2008), whereas our SSCD take-up rate is comparable to the 28% take-up rate for the Philippine SEED product (Ashraf, Karlan, & Yin 2006). 32.3% of SSCD clients met their chosen goal, 65.7% held the CD until maturity, and the average SSCD closing balance was \$910.

Demand analysis indicates that CD take-up is strongly positively correlated with female gender, low income, and behavioral factors (exponential growth bias in particular). Counseling take-up is positively correlated with poor baseline financial condition, and various behavioral factors.

We do not find statistically significant evidence of treatment effects on savings or borrowing outcomes. However, our treatment effect results are very imprecise, so these results are not necessarily indicative of inefficacy of either commitment savings products or financial counseling. Instead, this study points toward outcome variables and product designs that may be of interest in future research.

The rest of this paper is organized as follows. Section II discusses our study design and implementation, including the sample frame and setting, details on our randomization, and our outcome data. Section III reviews summary statistics on take-up and product usage, and presents our empirical analysis. Section IV concludes.

⁶ We define counseling take-up as both signing up for *and* showing up for a counseling appointment. An additional 33.0% of the counseling group signed up for counseling but did not come to their appointment.

II. Study Design and Implementation

A. Sample Frame and Setting

Our sample is drawn from Neighborhood Trust Federal Credit Union (“Neighborhood Trust”)’s nearly 4,000 active members. Neighborhood Trust serves the Washington Heights neighborhood of Upper Manhattan, New York, which is home to the largest concentration of immigrants from the Dominican Republic in the United States,⁷ and has a median household income of \$31,000. The poverty rate in the Washington Heights and Inwood neighborhoods is 31%, as compared to New York City’s overall rate of 21%.⁸

Neighborhood Trust gave us access to their sole branch office between March 2010 and May 2011. Our staff worked on-site and was responsible for conducting surveys and treatment offers. Surveys were administered in English or Spanish and were completed on a freestanding computer kiosk located in a private area of the credit union lobby.

B. On-Site Process Flow and Sample Creation

Credit union members visiting the branch during the study period were approached by our bilingual surveyor while they waited in line for a teller or loan officer, and were invited to take a 10-minute survey about their financial wellbeing. All respondents were offered a \$10.50 subway pass as an incentive to participate in the study. The baseline survey collected demographic information and qualitative data on respondents’ financial situation, financial attitudes and financial literacy levels. In addition, the survey included questions that sought to measure behavioral biases that may influence financial decision-making, such as time-inconsistent preferences, exponential growth bias, and limited attention. As part of the survey, participants also authorized us to conduct soft pulls of their credit report for research purposes.

Upon survey completion, our electronic survey tool created a random treatment assignment for the respondent and prompted the surveyor to offer the respondent the chance to open a Super Saver CD product or enroll in a free financial counseling session, or, if the

⁷ Migration Policy Institute (2004).

⁸ New York Office of the State Comptroller (2008)

respondent was assigned to the control group, to end the survey with no product offer. The randomization process was not visible to the surveyor or the respondent.

Members who were placed in the Super Saver CD group were then given the opportunity to open a Super Saver CD on the spot with a surveyor, who shared written marketing materials, explained product features, and provided an application form. Members were encouraged to open the product at the time of taking the survey but were also able to return to the credit union and open the product at a later date. Research staff conducted follow-up marketing calls to members of this treatment group, in addition to mailing additional marketing materials to members' homes, in an effort to promote the product.

Members who were placed in the counseling group were given the opportunity to sign up for a free counseling session at the conclusion of the survey. Appointments with a counselor were then scheduled by the surveyor in person, and later the member was called by phone to confirm the appointment. Counseling appointments were pre-scheduled for up to three weeks after the initial survey date.

C. Analysis of Sample Characteristics and Randomization Integrity

Our full sample consists of 1167 survey respondents. Baseline sample characteristics, presented in Table 1, column (1), reveal a low-income population with low educational attainment and a marked degree of financial distress. With regard to income, a majority of the sample (55%) reported an annual income of less than \$20,000 a year, and 85.7% reported earning less than \$40,000. When asked to describe their own financial situation, 35.9% responded that it was "Bad" or "Very Bad." Respondents' education levels were also low. Nearly a quarter of those surveyed had not completed high school. Twenty-seven percent of respondents had only a high school diploma, while about 17% had a bachelor's degree or higher. Sixty-three percent of the sample is female, and the mean age at baseline was 49. Although our survey did not include questions about ethnicity or country of origin, 73.8% of survey respondents chose to take the survey in Spanish.

Survey responses indicate that many households experienced financial distress during the past year. Nineteen percent of respondents were unable to make rent, mortgage or utility payments at some point during the twelve months prior to taking the survey, while 6.4% reported

moving in with a friend or family member because they could not afford mortgage, rent or utility bills. Fourteen percent of respondents reported that they or their family had needed to “skip meals or reduce the size of meals because there was not enough money for food.” Nearly a quarter of the population had been turned down for credit or did not receive as much credit as they had applied for in the previous year, while 40% were dissuaded from applying for credit due to their financial situation. Eighteen percent of the sample reported usage of expensive small-dollar loan products including refund anticipation loans, payday loans, auto title loans, pawn loans, loan shark loans, or rent-to-own arrangements, despite restricted access to some of these products in tri-state area due to state laws, and the tendency for survey respondents to underreport borrowing on these types of instruments (Karlan & Zinman 2008; Zinman 2010).

The baseline survey also included a set of questions to measure possible behavioral biases relevant to financial behavior: time-inconsistent preferences, exponential growth bias, and limited attention. First, we follow Ashraf, Karlan, & Yin (2006), among others, in asking respondents to make hypothetical tradeoffs between rewards at immediate and 1-month horizons, and at 6-month and 7-month horizons. In particular we gave respondents tradeoffs between a \$65 reward sooner and an \$80 reward later; respondents who chose the immediate reward were asked how much the later reward would need to be to induce them to wait. The monthly discount factor associated with preferring the \$65 reward over the \$80 reward, 0.81, is in line with the average monthly discount factor of 0.83 found by Meier & Sprenger (2010) among a similarly low-income urban U.S. population. Nevertheless, only 15% of respondents in our sample chose the \$65 reward at both horizons, indicating the sample had a (relatively) high average monthly discount rate, which in turn suggests our question was a relatively coarse measure of time-inconsistency. This question identified present-biased (future-biased) preferences in just 8% (3%) of the sample, considerably lower percentages than the 28% (20%) in Ashraf, Karlan, & Yin (2006), the 36% (9%) in Meier & Sprenger (2010), and the 20% (12%) measured using identical questions in Karlan & Zinman (2012). To complement this first question, we also elicit a general, self-reported measure of self-control problems, asking respondents to indicate their level of agreement or disagreement with the statement, “I often find that I regret spending money, I wish that when I had cash, I was better disciplined and saved my money rather than spent it.” Notably, 80% of respondents agreed with the question to some degree. While other

batteries of questions can collect richer data on self-control and time-inconsistency (Gine et al., 2011), the two questions we used have the advantage of brevity.

Two other questions addressed possible limited attention and exponential growth biases. For the former, we asked respondents how much time they spend thinking about their “financial situation”; 61% of respondents answered “a lot,” and only 12.8% of respondents answered “hardly at all.” For the latter, we elicited open-ended answers to a question about compounding 7% interest on a principal of \$1000 over ten years. We considered any response within \$200 of the true value to be “correct”; 5% of respondents fell within this range. Twenty-seven percent of respondents answered with a linear approximation (\$1700-\$1767, with nearly all in this bin answering \$1700). A plurality (48%) underestimated, while 13% overestimated. 7% of respondents did not answer the question.

The remaining columns in Table 1 present some checks of randomization integrity. Columns (2), (3) and (5) respectively show control group, savings treatment group, and counseling treatment group baseline means. Column (4) presents p-values for t-tests of differences in means between the savings treatment group and the rest of the sample, while column (6) does the same for the counseling treatment group. Whereas a small number of these p-values are below conventional thresholds of $p=0.10$, 0.05 , or 0.001 , the number of statistically significant differences is about what one would expect to find by chance. Table 1 also reports p-values for F-tests from OLS regressions ($N=1167$) of whether the subject is assigned to a SSCD offer or counseling offer based on a combination of all baseline variables. The baseline balance variables jointly predict assignment to the counseling group, so we including baseline balances as a control variable in our treatment effect regressions. The survey variables do not jointly predict assignment to any group, and later we see that adding controls for the survey variables to our treatment effect regressions does not change the results.

D. Product and Treatment Design

The Super Saver CD (SSCD) is a balance-building Certificate of Deposit whereby an individual makes an initial commitment to save a goal amount (up to \$10,000) within a customizable maturity term (up to 18 months), by making weekly or monthly deposits towards that goal. Individuals had the option of opening 3, 6, 12 or 18 month SSCDs, respectively with

APYs of 0.60%, 0.75%, 1.20%, and 1.35%.⁹ SSCDs could be opened with a low initial deposit of \$15, eliminating the large minimum balance that can make traditional CD products inaccessible to low-income savers. As with a traditional CD, once funds are deposited into the SSCD they cannot be withdrawn by the owner without closing the account and forfeiting accumulated interest earned; the SSCD also imposed a \$15 penalty on early account closures. Super Saver CDs that reach maturity but fail to meet their stated savings goal forfeited earned interest only. Upon opening a SSCD, members are shown a payment schedule with dates and required deposit amounts, and they can choose to set up automated transfers to the SSCD from their regular savings account or make manual deposits at the credit union.

The financial counseling treatment group was offered a free one-on-one counseling session from Neighborhood Trust Financial Partners. During these sessions, trained financial counselors work individually with members to alleviate financial crisis and establish a foundation for financial security. Hour-long sessions focus on goal-setting, budgeting, financial discipline, debt management and saving. As part of this intervention, clients receive a comprehensive financial diagnostic, a copy of their credit report, and an individualized budget that includes regular savings behaviors. Clients can return for free follow-up sessions if necessary.

E. Non-Compliance with Treatment Assignment

We faced two types of non-compliance with treatment assignment. First, some clients enrolled in a ten-session financial empowerment course administered by Neighborhood Trust during the study period; this course offered financial education classes (i.e. classroom instruction, not personal consultation), and *also* offered a Super Saver CD to all graduates.¹⁰

Participation rates in the course were low but non-negligible: 13 out of 381 clients assigned to the savings treatment (3.4%) participated in the course; 17 out of 389 clients assigned to the control group (4.6%) participated; and 24 out of 397 clients assigned to the counseling treatment (6.0%) participated. When we discuss our results below (Section III-C), we describe

⁹ These APYs were higher than average APYs for secondary-market CDs at the time, e.g. 0.32% for a 6-month CD (see <http://research.stlouisfed.org/fred2/series/CD6M?cid=121>).

¹⁰ This SSCD was identical to our treatment product, except that it offered additional options for maturity terms and had no maximum balance.

some econometric tools for managing this non-compliance, and also discuss how much it reduces our statistical power.

Second, some survey respondents returned to the credit union after their initial survey and chose to take the survey a second time (possibly to get another subway pass). Because survey respondents were matched with account numbers *after* treatment assignment, the research team's on-site staff could not reliably match these re-surveyed individuals with their original treatment assignment, and some re-surveyed clients were assigned to a second treatment group. For example, out of the 17 clients who were re-surveyed after being initially assigned to the control group, 8 individuals were randomly assigned to the savings treatment in a second survey, and 7 individuals were assigned to the counseling treatment in a second survey.¹¹ In total, there were 50 clients out of 1167 total study participants (4.2%) who were assigned to more than one group through re-surveys. A large fraction of these 50 may have resulted from two joint-account holders each taking the survey once.¹²

We conduct all analysis using our original treatment assignment. This decreases our statistical power to test for differences between a treatment group and the control group (since some control group respondents were treated through re-surveys), and similarly we have decreased power to test for differences between the two treatment groups. However the risk of upward bias to our estimates of treatment effects – which would be due to possible positive complementarities between the savings and counseling treatments – is minimal, since only 5 respondents both took up a Super Saver CD and also showed up for a counseling session.

F. Outcome Data

To measure savings outcomes, we collected quarter-end account data from the credit union for all study participants, regardless of treatment group or take-up status. These data were collected for all four quarters of 2010 and 2011, so for all respondents we have at least three

¹¹ Likewise: among clients originally assigned to the savings treatment, 6 were assigned to the control group in a second survey, while 9 were assigned to the counseling group in a second survey; among clients originally assigned to the counseling treatment, 7 were assigned to the savings treatment in a second survey, while 12 were assigned to the control group in a second survey. Additionally there was one client who took the survey three times and was randomly assigned to all three treatment/control groups.

¹² Of these 50 re-surveyed accounts, there were 22 jointly-held accounts, such that some multiple surveys may have been due to each of the two account holders taking the survey once. In all such instances, we measure treatment assignment at the account level, i.e. *all* joint account holders are assigned to a treatment group when one joint account holder is assigned to that treatment group.

quarters of outcome data, and for any respondents who were surveyed in March 2011 or earlier we have a full 4 quarters of outcome data. These outcome data include quarter-end stocks, but no flows such as deposits, and hence our observed account balances are a somewhat noisy measure of maximum or average balances *over* a whole quarter.

Other outcome data are drawn from clients' TransUnion credit reports. A first round of member credit reports was pulled between January and February 2011 but excluded individuals who joined the study sample after January 2011. A second round of reports for the entire study sample was pulled in February 2012. These credit reports contain an individual's FICO score and an overview of total revolving and installment trade accounts including current balances, credit limits, amounts past due, and payment amounts for installment loans. Credit reports also include details on individual lines of trade, including payment and delinquency patterns, debts placed for collection, inactive and closed accounts, consumer-initiated disputes, and write-offs.

III. Takeup, Usage, and Impacts

A. Takeup

Table 2 presents results from an OLS regression of SSCD and Counseling take-up decisions on individual characteristics from the baseline survey. Overall, 21.3% of the savings treatment group opened a SSCD; the dependent variable is 1 for these individuals and 0 otherwise. We limit the sample here to the 381 individuals who were originally assigned to the savings treatment group.¹³

Baseline balances in column (1) do not predict takeup. But two significant coefficients from column (2) of Table 2 deserve note. First, we find that women were significantly more likely to take up the SSCD than men. The magnitude, at 0.11, is economically significant as well.¹⁴ Our data do not allow us to isolate the mechanisms underlying these differences; e.g., they might be attributable to surveyor effects, or to a desire on the part of female respondents to maintain control over household resources.¹⁵

¹³ As discussed earlier, the means of the baseline variables for this group are presented in Table 1.

¹⁴ The marginal effect for "female" in a logistic regression is 0.11 (p=0.042).

¹⁵ See Ashraf, Karlan, and Yin (2006).

Second, the OLS regressions suggest that lower-income respondents are more likely to take up the SSCD. Relative to respondents with incomes of \$60,000 or more, individuals with incomes less than \$20,000 (who constitute 55% of our sample) were 18% more likely to create a SSCD account. The equivalent figure for individuals reporting incomes of \$20,000-\$40,000 is 16%, while individuals with incomes of \$40,000-\$60,000 were not significantly more likely to open a SSCD than those with incomes higher than \$60,000. This could perhaps be explained by the relatively small penalty (\$15 and accumulated interest) for early withdrawal, which may not be economically meaningful for high income individuals looking for commitment savings products. The low opening deposit (\$15 minimum) and the maximum balance for the SSCD (\$10,000) may also be a factor, as high-income individuals may choose other, higher-balance CD products with higher APYs.

On the other hand, neither education nor age is significantly correlated with take up of the SSCD. We also find that take-up of the SSCD was not significantly correlated with the language in which the respondent chose to take the survey – either English or Spanish – or with survey timing.¹⁶ Nevertheless, one important feature of the results in this table is that the null results are imprecisely estimated; i.e., we lack the statistical power needed to identify precise zeros. Indeed, the survey variables are jointly significant in column (2), with an F-test p-value of 0.008.

Surprisingly, next we see in column (3) that demand for the SSCD is not significantly higher among individuals with time inconsistent preferences, or among respondents who “strongly agree” with the statement that they regret spending money – a possible indication of self-control problems. Although this runs counter to the design of SSCD as a commitment savings device (Bryan, Karlan, & Nelson 2010), this result may be related to the coarseness/miscalibration of our baseline data on time-inconsistent preferences (see Section II-C). Similarly, respondents who report thinking a lot about their financial situation (a variable that attempts to capture limited attention (Karlan et al. 2011)) were no less likely to open a SSCD account than those who did not. On the other hand, we do find striking patterns with respect to our proxy for exponential growth bias (Stango and Zinman 2009): respondents who answered correctly were far less likely to take-up. It may be that the low yields on CDs during recent time periods are less attractive to those who understand compounding. And/or it may be that our

¹⁶ Fixed effects for the month of the survey are not jointly significant.

proxy for exponential growth bias is correlated with other (behavioral) factors not fully captured by our survey questions. An F-test for joint significance of all the behavioral proxies in column (3) suggests ($p=0.06$) that they jointly predict take-up.

Next, in column (4) we look at responses to survey questions that sought to capture qualitative differences in household situation and financial experience (past product use). None of these variables significantly predicts SSCD take-up.

Column (5) includes all of the survey variables and controls for baseline balances in an OLS regression for SSCD take-up. The strong correlations with gender, income, and exponential growth bias found in the more parsimonious specifications are also present here. Other results in column (5) are consistent with (insignificant but noisily estimated) results in previous columns. An F-test for joint-significance of all baseline variables suggests that they jointly predict take-up ($p\text{-value} = 0.046$).

Columns 6-10 of Table 2 present results from OLS regressions of counseling take-up on baseline characteristics. Overall, 15.9% of counseling-group respondents took up a counseling offer and subsequently went to their counseling appointment, out of a total treatment-group sample of 397 individuals.¹⁷ Another 33.0% made an appointment but did not keep it: hence our overall *sign-up* rate of 48.9% is comparable to the 55% sign-up rate for a similar free counseling program in Meier & Sprenger (2008).

Four interesting patterns emerge in the counseling take-up regressions. First, behavioral factors may predict take-up: 19 of the 20 coefficients are positive. But the p-values on the F-tests for joint significance are 0.466 in the parsimonious specification (Column 8) and 0.411 in the more complete specification (Column 10). Second, respondents who viewed their financial situation as “good” or better were far less likely to take up (Columns 9 and 10). Third, all the survey variables together do not jointly predict take-up. Fourth, timing seems to matter: month-of-entry into the sample is strongly correlated with take-up.

B. Usage

¹⁷ Means of this sample’s baseline variables are presented in Table 1.

Summary statistics for product usage in the savings treatment group are presented graphically in Figures 1A through 1C. There is considerable heterogeneity in how individuals used the Super Saver CD product. As we see in Figure 1A, nearly half of the treatment-group takers chose SSCDs with 18-month maturity terms, while about 30% chose 6-month CDs, and about 20% chose 12-month CDs. Similarly (see Figure 1B) there was a wide range in individuals' chosen goal amounts: the 10th percentile goal amount was \$300, the median was \$1000, and the 90th percentile was \$4000.

Among the 86% of treatment-group SSCDs that had matured (or had been closed) as of March 1, 2012, 32.5% had met their savings goal (see Figure 1C). This rate compares favorably to the comparable rate for the Philippines SEED product, where only 9.7% of clients with *amount*-based goals reached their goal within a year (Ashraf, Karlan, & Yin 2006),¹⁸ but the rate here is less than the 54.9% of clients who reached their savings goals in Karlan et al. (2011). Additionally, 34.3% of SSCD accounts were closed prematurely – before reaching their goal or their maturity date. Nonetheless most individuals made substantial progress toward their goal, with the median SSCD accumulating 56.5% of its goal amount by closure or maturity; among CDs that were not closed prematurely, the median SSCD accumulated 88.9% of its goal amount. Even among the subset of SSCDs that did not reach their savings goal, the median SSCD accumulated 27.9% of its goal amount. Total balances at maturity or closure were also considerable relative to income: the average was \$910, and the median was \$417.

Although these results indicate at least a moderate degree of Super Saver CD success, an important question is whether these savings levels are achieved by trading off with other savings, and whether the average savings levels achieved in the savings treatment group are greater than those achieved by the control group during the study period. We turn to those questions next.

C. Impacts

The focus of our empirical strategy is to compare balances data for control group and treatment group individuals in the four quarters after treatment assignment. Any significant average difference between groups would be attributable to the randomly assigned treatment offers: we estimate intention-to-treat (ITT) effects, or the effects of *offering* counseling or a

¹⁸ On the other hand, the majority of SEED accounts had date-based rather than amount-based savings goals.

SSCD product, rather than the treatment effects on individuals who *took up* the product (treatment-on-the-treated, TOT).

Our balances data are measured on the last day of each quarter in 2010 and 2011. We set each individual’s “baseline quarter” as the quarter immediately preceding the quarter in which she was surveyed, and each individual’s four “treatment quarters” are then the four subsequent quarters, inclusive of the quarter in which she was surveyed. In the savings group, individuals variously chose Super Saver CD term lengths of 3 months, 6 months, 12 months, or 18 months, so some in-sample SSCDs matured shortly after the end of the first or second treatment quarter, and some matured after the end of the fourth treatment quarter. Because of this staggered maturity timeline, we restrict most of our analysis to summary statistics of balances *over* the four treatment quarters: either the maximum observed balance (in our main tables), or the average (in an appendix table).

Our regression analysis starts with an ordinary least squares (OLS) or median regression of the following form:

$$(1) \quad f\left(\text{Max}_{\tau=1,2,3,4}(Savings_{\tau,i})\right) = \alpha + \beta_1 1(Offer_{Savings,i}) + \beta_2 1(Offer_{Counseling,i}) + \gamma Savings_{0,i} + X_{month,i}\xi + \varepsilon_i$$

Here, our dependent variable is the maximum savings balance for individual i over the four treatment quarters indexed by τ . *Savings* always include savings account balances and CD balances; in different specifications we sometimes also include checking account balances.

The explanatory variables are indicator functions $1(Offer_{Savings,i})$ and $1(Offer_{Counseling,i})$ that equal unity when individual i was, respectively, assigned to the savings or the counseling treatment group, and 0 otherwise; second, the level of baseline savings for individual i , $Savings_{0,i}$; and third, a vector of dummy variables for the month in which individual i was surveyed, $X_{month,i}$. Then ε_i is an individual-level error term. The results do not change if we add a complete set of controls for baseline survey variables.

We use several different specifications to address the possible influence of high-balance outliers, mainly: OLS with level balances; OLS with $\log(1+\text{balances})$, and median regression. Results do not change if we drop individuals in the top 5% of baseline balances.

We also use an alternative specification where the five quarters of data for each individual (that is, one baseline quarter and four treatment quarters) are used as a panel, and we add individual-level fixed effects. Hence the vector of month fixed effects, $X_{month,i}$ and the baseline savings levels $Savings_{0,i}$ both drop out of the regression, and we add a vector of dummy variables for the four treatment quarters in the panel, $X_{quarter}$:

$$(2) \quad f(Savings_{\tau,i}) = \alpha + \beta_1 1(Offer_{Savings,\tau,i}) + \beta_2 1(Offer_{Counseling,\tau,i}) + X_{quarter}\xi + \varepsilon_{\tau,i}$$

Here we add a time subscript τ to the treatment indicators, and these indicators are set to be zero whenever τ is 0 (that is, in all baseline quarters for all individuals). Standard errors are clustered by individual when estimating equation (2). Similar to (1), we estimate (2) using both level and logged dependent variables.

Hence we have three main specifications that use cross-sectional data: equation (1) using OLS for savings levels, equation (1) using a median regression for savings levels, and equation (1) using OLS for savings *logs*. Likewise we have two main specifications that use panel data: equation (2) using OLS for savings levels, and equation (2) using OLS for savings *logs*.

We present results for the three cross-sectional specifications in columns (1) through (3) of Table 3, where the dependent variable (either levels or logs) is defined using the *maximum* savings balance over the four treatment quarters. In columns (4) through (6) we present results for three analogous regressions where the dependent variable is defined using *mean* savings balance over the four treatment quarters. Next, in columns (7) through (8), we present results for our two main specifications using panel data – i.e. equation (2) using levels and logs.

Appendix Table 1 then presents results for eight analogous regressions where *Savings* is expanded to include checking account balances, in addition to SSCD balances and savings account balances. Results are qualitatively similar between Table 3 and Appendix Table 1.

Reviewing the results in Table 3, two patterns are noteworthy. First, standard errors on the estimated treatment effects for both the savings and the counseling treatments are large relative to the coefficients in all ten columns, meaning our 95% confidence intervals for the size of treatment effects include large positive and also large negative effects. For example, in column (1), we cannot reject with more than 95% confidence that the true effect of offering our counseling treatment (ITT) is to increase maximum four-quarter savings by about \$1,150 on average, or to decrease maximum savings by about \$450 on average, relative to the control group. The imprecision of these estimates is due to several factors: sample size, take-up rates that are (understandably) much lower than 100%, non-compliance with treatment assignment, and the high variance of individual savings balances over time. Second, the prevalence of positive coefficients on the counseling treatment dummies (6 out of 8), is cause for cautious optimism about the efficacy of basic financial counseling services in increasing savings levels. The signs on savings treatment indicators are more mixed however (3 negative out of 8, and 4 negative out of 8 in Appendix Table 1).

Another important outcome to test is (change in) net assets inclusive of debt held at the credit union. Particularly for the counseling treatment, it may be the case that treated individuals increase (or decrease) their net assets by paying down their debt, even while they do not significantly increase their savings. We thus estimate versions of equations (1) and (2) where the dependent variable is net assets. (We do not estimate a logged version of equation (1) or (2) in this case, because net assets are negative for nearly 25% of the sample.) Results, which we do not display here, are qualitatively similar to our results for savings: there is a prevalence of positive coefficients on the indicators for the counseling treatment, but none of the treatment effects is statistically significant and standard errors are quite large.

We also use an instrumental variable (IV) strategy, instrumenting for take-up with the random assignment, to estimate treatment-on-the-treated effects and mitigate the statistical power loss of treatment non-compliance (Section II-E). Results for these IV regressions are qualitatively similar to the results in Table 3.

We next turn from analyzing balance (and net asset) outcomes to analyzing effects on individuals' credit reports. As described in Section II-B above, we pull credit reports on in-sample individuals in or around February 2011, and again in February 2012. Since some

individuals had not yet entered the sample as of February 2011, we focus our analysis here on the February 2012 credit report outcomes. Our sample size is smaller here because identifying information provided by 217 respondents did not match any credit report on file with the credit bureau, whereas 29 individuals could not be matched due to typographical errors. We found reports for 921 individuals in our sample, 780 of whom had credit scores.

Similar to our analysis of net assets held in the credit union, our analysis of credit report outcomes considers the possibility that treated individuals might decrease (or increase) their debt even if they do not significantly change their savings levels. Credit reports allow us to observe debt levels *not* held at the credit union – allowing for treatment spillover to non-credit union debts – and provide us with broader measures of indebtedness and financial condition. Here, we estimate a variant of equation (1) where we exclude baseline data, since these data were unavailable for credit report outcomes:

$$(3) \quad f(\text{CreditReportOutcome}_i) = \alpha + \beta_1 1(\text{Offer}_{\text{Savings},i}) + \beta_2 1(\text{Offer}_{\text{Counseling},i}) + X_{\text{month},i} \xi + \varepsilon_i$$

The independent variables are the same as in equation (1). The dependent variable $\text{CreditReportOutcome}_i$ for individual i is variously (i) an indicator for whether an individual has a credit score, (ii) an individual’s credit score (“FICO score”), (iii) the number of active trade lines on an individual’s credit report in the past year, (iv) an individual’s total outstanding credit card debt (mean), (v) total credit card debt (median), (vi) the log of an individual’s total outstanding credit card debt, (vii) an individual’s credit utilization rate in the past year, (viii) the number of negative trade lines on an individual’s credit report in the past year, which is a measure of mild delinquency, (ix) a dummy, measuring severe delinquency, for whether an individual has any delinquent or 90-day-late trade lines in the past year, and (x) the number of delinquent or 90-day-late trade lines in the past year. Estimated coefficients for regressions using these dependent variables are presented in columns (1) through (10) of Table 4. All columns are estimated using OLS with robust standard errors, except for column (5), which estimates equation (3) for total credit card balances using a median regression.

In Table 4 results, we again see large standard errors for most coefficient estimates. There are two marginally significant treatment effects, but this is about what one would expect to find by chance: if you run enough regressions, you will get some spurious significance.

IV. Conclusion

This study implemented a randomized-controlled trial of a commitment savings product and financial counseling among a low-income population at Neighborhood Trust, a New York City credit union. We tracked balance data and credit report data over roughly two years to estimate effects of savings product and counseling offers on saving and borrowing behavior.

The savings product enjoyed considerable demand, with a take-up rate over 20%, comparable to the take-up rate seen for a similar product in a developing-country context (e.g. 28% in Ashraf, Karlan, and Yin (2006)). Furthermore we find evidence that credit union clients used the product successfully to accumulate savings. The median SSCD accumulated 56.5% of its goal amount by closure or maturity, for a treatment-group average SSCD savings balance of \$910.

Although these results are encouraging, we nevertheless do not find significant evidence that the savings product offer increased the treatment group's savings relative to the control group, or affected the treatment group's credit behavior relative to controls. It is possible that this null result is due to small treatment effects – perhaps because of substitution between SSCD savings and other savings at the credit union – but our empirical analysis also faced several challenges that limit our ability to estimate treatment effects precisely. Adding hundreds of additional observations from a similar study, underway in Washington, DC, should improve our statistical power.

Similarly, we find fairly strong demand for financial counseling services but no significant evidence it changes savings or borrowing behavior. Nevertheless, the pattern of (positive) signs on the regression coefficients in our analysis of counseling treatment effects may be cause for cautious optimism that financial counseling can raise both savings levels and net assets.

We hope that this study points the way for further research in designing and testing both commitment savings products and financial counseling. Future research with larger sample sizes will permit the estimation of heterogeneous treatment effects – for example, by behavioral factors – and will improve precision in estimating main effects. Other research may also test stronger commitment devices, financial counseling content that is tailored more closely to mitigate behavioral factors, or positive complementarities between counseling and commitment.

Figure 1A: SSCD Maturity Terms

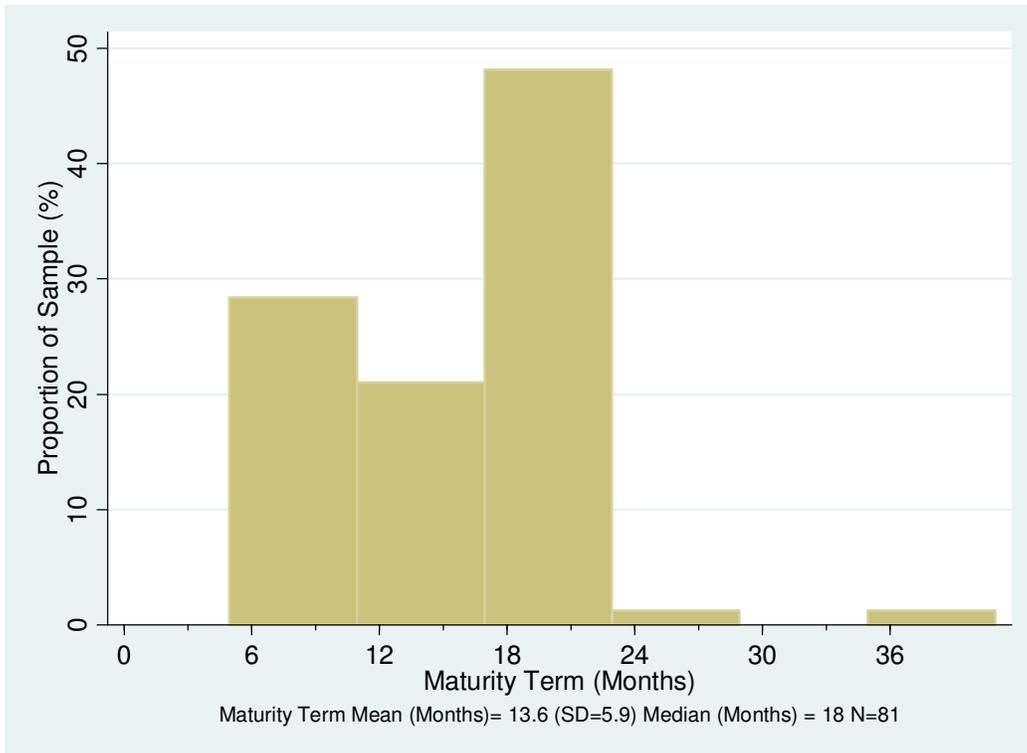


Figure 1B: SSCD Goal Amounts

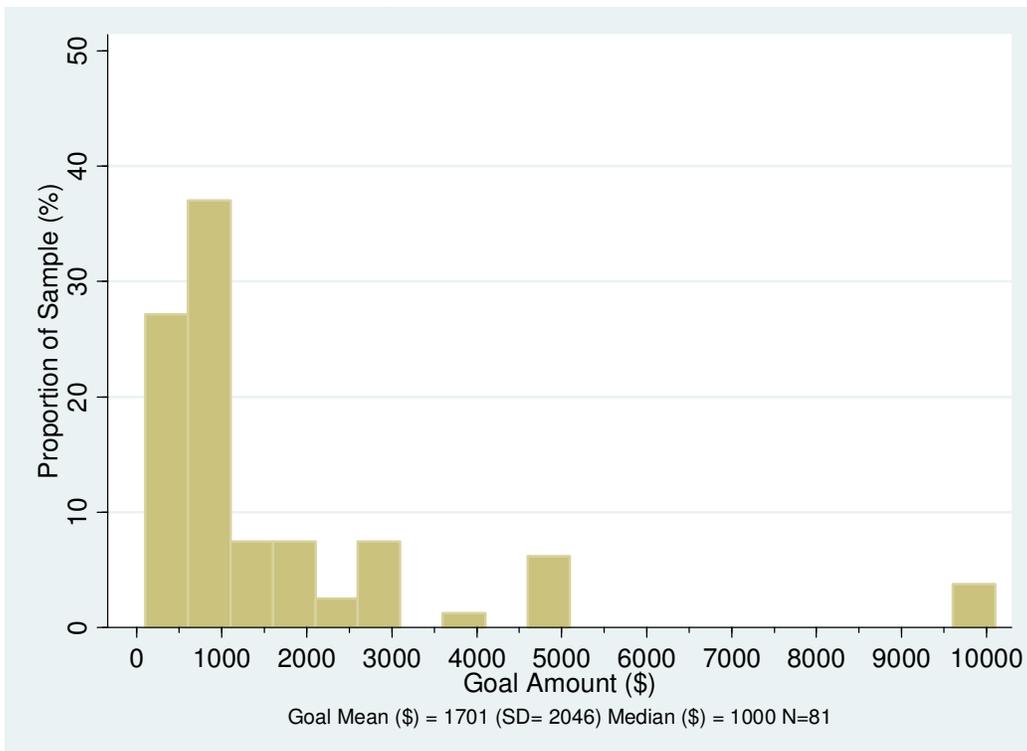


Figure 1C: SSCD Percent of Goals Reached

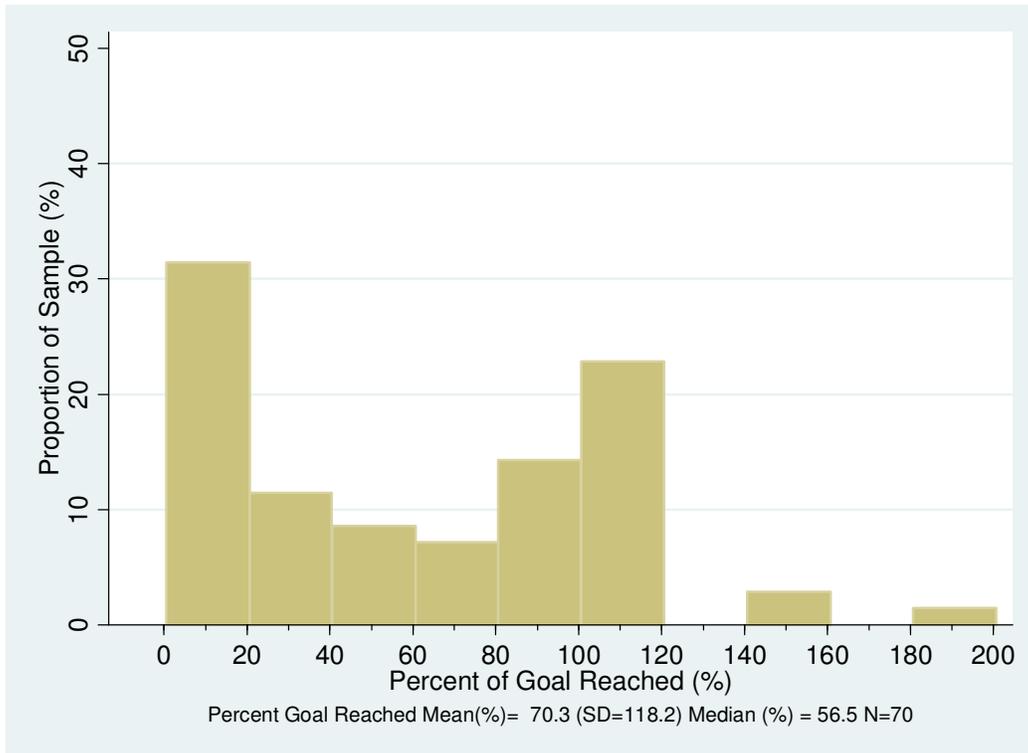


Table 1: Sample Characteristics

	Full Sample N=1167	Control N=389	Treatment = Offered Savings Account N=381	Difference from Control/Coun seling (p-val)	Treatment = Offered Counseling N=397	Difference from Control/CD (p-value)
Baseline Balance Variables						
Savings and CD Accounts	819.02 [2773.25]	1063.03 [3497.39]	698.70 [1991.03]	0.32	695.38 [2601.76]	0.16
Savings, Checking, and CD Accounts	1006.17 [3261.83]	1166.90 [3653.13]	837.79 [2552.78]	0.22	1010.27 [3456.25]	0.85
Net Assets at Credit Union	693.79 [3440.93]	839.17 [3761.46]	572.05 [2784.45]	0.41	668.18 [3677.48]	0.68
Baseline Survey Variables						
Took Survey in Spanish	74%	78%	73%	0.63	71%	0.08*
Female Respondent	63%	62%	62%	0.95	63%	0.92
Age	44.33 [18.62]	46.27 [19.33]	41.91 [18.50]	0.01***	44.74 [17.79]	0.98
Yearly Household Income >\$20k	55%	56%	54%	0.61	55%	0.84
Yearly Household Income \$20k-\$40k	30%	29%	32%	0.57	30%	0.92
Yearly Household Income \$40k-\$60k	10%	10%	9%	0.75	10%	0.78
Yearly Household Income >\$60k	4%	5%	4%	0.86	4%	0.88
Education: High School or Less	51%	54%	48%	0.19	50%	0.52
Education: Some College	43%	39%	46%	0.17	43%	0.54
Education: Bachelors Degree or more	7%	7%	7%	0.93	7%	0.93
Time Inconsistent: Standard	9%	10%	7%	0.15	9%	0.90
Time Inconsistent: Nonstandard	3%	2%	5%	0.07*	3%	0.53
Time Inconsistent: Always impatient	15%	16%	13%	0.16	15%	0.78
Time Inconsistent: Always patient	69%	68%	71%	0.20	69%	0.42
Regrets Spending / Lacks Discipline: Strongly Agree	48%	47%	51%	0.17	46%	0.10
Regrets Spending / Lacks Discipline: Somewhat Agree	32%	34%	31%	0.71	30%	0.83
Compound Interest: Underestimates	48%	47%	48%	0.97	49%	0.75
Compound Interest: Overestimates	13%	13%	13%	0.39	13%	0.38
Compound Interest: Linear Approximation	27%	27%	27%	0.50	26%	0.87
Compound Interest: Missing/unavailable	7%	7%	7%	0.70	8%	0.93
Compound Interest: Correct	5%	5%	5%	0.65	4%	0.41
Thinks About Financial Situation A Lot?	61%	66%	62%	0.57	56%	0.01***
Financial Situation "Good or Better"	24%	23%	23%	0.54	26%	0.30
Household Financial Situation "OK"	40%	40%	42%	0.35	38%	0.36
Household Financial Situation "Not Very Good" or "Bad"	36%	37%	35%	0.84	35%	0.77
Turned down recently for credit?	24%	24%	25%	0.78	23%	0.66
Discouraged from applying?	40%	38%	42%	0.32	41%	0.75
Financial Distress Index (If had late bill payment, forced move, or food cutback responses)	25%	23%	26%	0.80	25%	0.32
<i>Financial Products Held In Past 2 Years (dummies):</i>						
Checking Account	75%	69%	78%	0.03**	80%	0.02**
Savings Account	98%	98%	97%	0.62	98%	0.90
CD/Investment Account	21%	20%	20%	0.83	23%	0.51
Credit Card	49%	48%	51%	0.29	49%	0.67
Auto Loan	5%	6%	4%	0.16	5%	0.83
Negative Checking Balance / Overdraft	5%	6%	4%	0.34	5%	0.88
Retirement Account/401k	12%	11%	12%	0.66	13%	0.36
Prepaid Loan	6%	6%	5%	0.41	7%	0.31
Unconventional Credit (refund anticipation loan, payday loan, auto title loan, pawn loan, loan-shark loan, or rent-to-own arrangement)	18%	19%	16%	0.30	19%	0.84
F-test: baseline balance variables correlated with treatment assignment				0.54		0.03**
F-test: survey variables correlated with treatment assignment				0.24		0.61
F-test: all variables correlated with treatment assignment?				0.25		0.36

*p<0.10 **p<0.05 ***p<0.01.

Table 2. Demand Analysis Savings Takeup

Mean Takeup	Savings					Counseling				
	0.213 (1)	0.213 (2)	0.213 (3)	0.213 (4)	0.213 (5)	0.159 (6)	0.159 (7)	0.159 (8)	0.159 (9)	0.159 (10)
Baseline Savings and CD Account Balances(\$/100)	0.001 [0.001]				0.001 [0.001]	0.000 [0.001]				0.000 [0.001]
Took Survey in Spanish		0.025 [0.052]			0.022 [0.061]		0.059 [0.041]			0.061 [0.042]
Female Respondent		0.114*** [0.043]			0.111** [0.048]		-0.015 [0.038]			-0.027 [0.041]
Age		-0.001 [0.002]			-0.001 [0.002]		0.001 [0.001]			0.001 [0.001]
Yearly Household Income <\$20k (omitted->\$60,000)		0.183** [0.077]			0.245*** [0.091]		-0.034 [0.104]			-0.020 [0.119]
Yearly Household Income \$20k-\$40k		0.158** [0.079]			0.196** [0.093]		-0.047 [0.106]			-0.054 [0.116]
Yearly Household Income \$40k-\$60k (omitted - Income>\$60k)		0.159 [0.102]			0.159 [0.111]		0.061 [0.126]			0.050 [0.131]
Some College (omitted - High School Graduate or Less)		-0.063 [0.050]			-0.081 [0.052]		-0.042 [0.042]			-0.037 [0.045]
Education: Bachelors Degree or more		0.111 [0.108]			0.109 [0.109]		0.090 [0.087]			0.077 [0.094]
Time Inconsistent: Standard (omitted - < always patient)			-0.041 [0.092]		-0.074 [0.094]		0.092 [0.083]			0.104 [0.077]
Time Inconsistent: Nonstandard			0.075 [0.107]		0.058 [0.102]		0.134 [0.153]			0.185 [0.148]
Time Inconsistent: Always impatient			0.015 [0.068]		0.025 [0.070]		0.084 [0.061]			0.089 [0.062]
Regrets Spending / Lacks Discipline: Strongly Agree (omitted - "Disagree")			0.025 [0.061]		0.027 [0.063]		0.026 [0.047]			0.006 [0.052]
Regrets Spending / Lacks Discipline: Somewhat Agree			0.014 [0.065]		0.010 [0.069]		0.050 [0.055]			0.063 [0.056]
Compound Interest: Underestimates <\$1700 (omitted- Correct +-\$200)			0.126* [0.069]		0.111 [0.068]		0.074 [0.048]			0.065 [0.063]
Compound Interest: Overestimates >\$2167			0.164** [0.082]		0.172** [0.076]		0.106 [0.073]			0.102 [0.080]
Compound Interest: Linear Approximation \$1700-\$1767			0.193** [0.087]		0.178** [0.086]		0.093* [0.056]			0.104 [0.068]
Compound Interest: Missing/unavailable			-0.059 [0.086]		-0.122 [0.098]		0.178* [0.093]			0.177 [0.109]
Thinks About Financial Situation A Lot?			0.054 [0.047]		0.070 [0.049]		0.007 [0.037]			-0.019 [0.041]
Financial Situation "Good or Better" (omitted - "Bad")				0.032 [0.067]	0.069 [0.069]				-0.405* [0.220]	-0.472* [0.276]
Household Financial Situation "OK" (omitted - "Bad")				0.003 [0.056]	0.028 [0.056]				-0.402* [0.225]	-0.444 [0.283]
Household Financial Situation "Not Very Good"				0.000 [0.000]	0.000 [0.000]				-0.334 [0.230]	-0.385 [0.284]
Turned down recently for credit?				0.024 [0.061]	0.033 [0.064]				0.039 [0.051]	0.050 [0.050]
Discouraged from applying?				0.045 [0.054]	0.025 [0.055]				-0.015 [0.049]	-0.017 [0.051]
Financial Distress Index (If had late bill payment, forced move, or food cutback responses) Financial Products Held In Past 2 Years (dummies):				-0.006 [0.059]	-0.025 [0.059]				-0.026 [0.060]	-0.008 [0.062]
Checking Account				0.018 [0.078]	0.029 [0.080]				0.022 [0.058]	0.049 [0.058]
Savings Account				0.071 [0.261]	-0.076 [0.249]				0.134 [0.188]	0.154 [0.205]
CD/Investment Account				0.063 [0.069]	0.046 [0.069]				0.024 [0.051]	0.027 [0.050]

Table 3: Treatment Effects on Savings Balances (Super Saver CD Account Balances + Savings Account Balances)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	Maximum End of Quarter Balance	Maximum End of Quarter Balance	Log (Max End of Quarter Balance + 1)	Mean End of Quarter Balance	Mean End of Quarter Balance	Log (Mean End of Quarter Balance + 1)	Level Balance at End of Quarter	Log (Level Balance at End of Quarter + 1)
Estimator:	OLS	Median	OLS	OLS	Median	OLS	OLS	OLS
Savings Treatment	✓ -146.4 (242.0)	✓ 48.71 (34.03)	✓ 0.0821 (0.165)	✓ -131.7 (161.5)	✓ 29.73* (16.67)	✓ 0.0790 (0.157)	✓ -21.73 (190.4)	✓ 0.197 (0.222)
Counseling Treatment	✓ 339.4 (408.9)	✓ 38.35 (34.32)	✓ -0.134 (0.176)	✓ 159.8 (283.6)	✓ 16.68 (16.34)	✓ -0.166 (0.169)	✓ 247.2 (343.4)	✓ 0.0515 (0.220)
Number of Observations	✓ 1167	✓ 1167	✓ 1165	✓ 1167	✓ 1167	✓ 1161	✓ 5767	✓ 5747
R-Squared	0.178		0.132	0.255		0.141	0.003	0.069
Dependent Variable Baseline Mean [Median]	819.02 [46.34]	819.02 [46.34]	819.02 [46.34]	819.02 [46.34]	819.02 [46.34]	819.02 [46.34]	819.02 [46.34]	819.02 [46.34]
Control variables included:								
Timing of Entry into Study	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Timing of Observation	No	No	No	No	No	No	Yes	Yes
Baseline Dependent Variable	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Individual FES	No	No	No	No	No	No	Yes	Yes

Notes

Standard errors in parentheses, Huber-White for intent-to-treat specifications, clustered on individual for fixed effect specifications. Sample sizes differ for regressions with logged dependent variables because of some negative values for the level dependent variables. Maximum or mean savings balance is the dependent variable in cross-sectional regressions, and is defined over 4 observations taken at treatment-quarter end dates; level balance is the dependent variable in panel regressions, and is simply the observed balance levels at treatment-quarter end dates. All regressions also control for baseline balances, and for month-year of entry (and hence treatment assignment) into our sample, except in the individual fixed effects specifications, where instead we control for the month-year of each observation. *p<0.10 ** p<0.05 *** p<0.01.

Table 4: Treatment Effects on Credit Scores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent Variable:	Has Report and Credit Score (dummy)	Has Report and Credit Score (level)	Active Trade Lines (count)	Credit Card Balance (level)	Credit Card Balance (level)	Credit Card Balance (log level)	Credit Utilization (level)	Negative Trades (count)	Has Delinquent or 90 Days Past Due Account (dummy)	Has Delinquent or 90 Days Past Due Account (count)
Estimator:	OLS	OLS	OLS	OLS	Median	OLS	OLS	OLS	OLS	OLS
Savings Treatment	✓ 0.054 (0.034)	✓ -3.183 (7.675)	✓ -0.100 (0.430)	✓ -656.047* (368.444)	✓ -94.000 (133.029)	✓ -0.315 (0.309)	✓ 0.015 (0.034)	✓ 0.055 (0.212)	✓ 0.004 (0.040)	✓ 0.657 (0.586)
Counseling Treatment	✓ 0.026 (0.034)	✓ -7.996 (7.513)	✓ 0.670 (0.460)	✓ 92.626 (474.450)	✓ -94.000 (139.580)	✓ -0.289 (0.313)	✓ -0.014 (0.033)	✓ 0.117 (0.216)	✓ 0.040 (0.040)	✓ 1.044* (0.596)
Number of Observations	✓ 1,167	✓ 780	✓ 921	✓ 921	✓ 921	✓ 921	✓ 669	✓ 921	✓ 921	✓ 921
R-Squared	0.015	0.009	0.018	0.029	0.009	0.018	0.004	0.014	0.023	0.018
Dependent Variable Baseline Mean [Median]	0.64 [1.00]	652.5 [656.50]	4.62 [3.00]	2696.2 [523.00]	2696.2 [523.00]	4.68 [6.26]	0.44 [0.41]	1.45 [0.00]	0.48 [0.00]	3.55 [0.00]
Control variables included:										
Timing	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes:

Huber-White standard errors in parentheses. Median regression reports bootstrapped standard errors with 1000 repetitions. All regressions control for month of entry into sample. Sample size differs because credit reports, credit scores, and utilization rates are unavailable for some individuals. *p<0.10 ** p<0.05 *** p<0.01.

Appendix Table 1: Replication of Table 3, except including Checking Account balances

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	Maximum End of Quarter Balance	Maximum End of Quarter Balance	Log (Max End of Quarter Balance + 1)	Mean End of Quarter Balance	Mean End of Quarter Balance	Log (Mean End of Quarter Balance + 1)	Level Balance at End of Quarter	Log (Level Balance at End of Quarter + 1)
Estimator:	OLS	Median	OLS	OLS	Median	OLS	OLS	OLS
Savings Treatment	▼ -134.2 ▼ (254.1)	▼ 43.63 ▼ (41.01)	▼ 0.0107 ▼ (0.158)	▼ -149.4 ▼ (166.2)	▼ 15.12 ▼ (17.78)	▼ -0.00257 ▼ (0.151)	▼ -60.35 ▼ (194.1)	▼ 0.166 ▼ (0.229)
Counseling Treatment	▼ 581.1 ▼ (428.1)	▼ 47.81 ▼ (45.04)	▼ -0.0410 ▼ (0.160)	▼ 260.0 ▼ (288.7)	▼ 27.77 ▼ (19.16)	▼ -0.0845 ▼ (0.155)	▼ 281.9 ▼ (353.1)	▼ 0.0690 ▼ (0.227)
Number of Observations	▼ 1167	▼ 1167	▼ 1165	▼ 1167	▼ 1167	▼ 1156	▼ 5767	▼ 5730
R-Squared	▼ 0.219	▼	▼ 0.159	▼ 0.325	▼	▼ 0.165	▼ 0.004	▼ 0.071
Dependent Variable Baseline Mean [Median]	1006.17 [53.26]	1006.17 [53.26]	1006.17 [53.26]	1006.17 [53.26]	1006.17 [53.26]	1006.17 [53.26]	1006.17 [46.34]	1006.17 [53.26]
Control variables included:								
Timing of Entry into Study	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Timing of Observation	No	No	No	No	No	No	Yes	Yes
Baseline Dependent Variable	Yes	Yes	Yes	Yes	Yes	Yes	No	No
Individual FEs	No	No	No	No	No	No	Yes	Yes

Notes

Standard errors in parentheses, Huber-White for intent-to-treat specifications, clustered on individual for fixed effect specifications. Sample sizes differ for regressions with logged dependent variables because of some negative values for the level dependent variables. Maximum or mean savings balance is the dependent variable in cross-sectional regressions, and is defined over 4 observations taken at treatment-quarter end dates; level balance is the dependent variable in panel regressions, and is simply the observed balance levels at treatment-quarter end dates. All regressions also control for baseline balances, and for month-year of entry (and hence treatment assignment) into our sample, except in the individual fixed effects specifications, where instead we control for the month-year of each observation. *p<0.10 ** p<0.05 *** p<0.01.

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