Testing Strategies to Increase Saving and Retention in Individual Development Account Programs

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Abstract: In a series of field experiments we test whether saving and retention rates in a federally funded, matched savings program for low-income families – the Individual Development Account (IDA) program – can be improved through the introduction of program features inspired by behavioral economics. We partnered with eight IDA programs across the U.S. who agreed to randomly assign participants to different experimental conditions. We test the impact of four revenue-neutral changes in key program features: a) holding savers accountable for making savings deposits through phone calls before and after the deposit deadline, b) an increase in the frequency with which deposits are made from monthly to bi-weekly, c) the introduction of a lottery-based incentive structure, whereby match rates are determined in part by a lottery at the time of each deposit, and d) an increase in the savings match from $2 for every $1 saved to $4 for every $1 saved when half of the savings goal was reached. None of our four interventions had the desired effect of increasing savings. To explain the null findings, we speculate that liquidity constraints, rather than cognitive biases, were the primary impediment to saving.

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in Individual Development Account Programs

Saving is often difficult, especially for lower income families. In the United States, tax incentivized savings vehicles like IRAs and 401(k)s were established to encourage families to save. However, all families do not benefit equally from such programs: for example, only 4.5% of households with a net worth in the bottom quartile have an IRA, and those who do hold on average only $8,648. In contrast, households with a net worth in the top quartile hold 53 times that amount in IRA savings, an average of $461,151 (Bricker et al., 2014). Due to differences in holdings, and in benefits that depend on marginal tax rates, the vast majority of tax breaks to encourage saving go to upper-income individuals, who already save much more adequately than those with lower incomes (e.g., Davis et al., 2015, Poterba, 1992).

In response to this gap in asset building, the U.S. Congress introduced the Assets for Independence Act in 1998 (Pub.L. 105-285). The Act's stated goal was to "demonstrate and support an assets-based approach for increasing the economic self-sufficiency of low-income individuals and families." One key feature of the Act was the establishment of Individual Development Accounts (IDAs), a savings program for families with a household income at or below 200% of the poverty level.[1]

IDAs are intended to enable low income individuals to save for one of three permitted purposes: purchasing a home, covering expenses of post-secondary education, or funding a small business (Assets for Independence, 2014). The key incentive of the IDA program is a dollar-for-dollar match provided for saving. Match rates in different programs range from $1 for every $1

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[1] See http://www.acf.hhs.gov/programs/ocs/programs/afi.

saved, up to $8 for every $1 saved (Office of Community Services, 2012). The most common match rate is $2 for every $1 saved, the typical savings target is $25 per month, and the program agencies' average final savings goal is $1,628 (Office of Community Services, 2012).

Despite the generosity of the match, IDAs have been at best a mixed success. Results from an analysis of 2,350 participants in the American Dream Demonstration project – the first large-scale evaluation of IDAs in the U.S. – revealed dropout rates that were high (48%) and net monthly savings (deposits minus withdrawals, excluding the match) for those who didn't drop out that were low – on average $29, or about 2.7% of participants' income (Schreiner and Sherraden, 2005). The most recent Report to Congress by the Assets for Independence Program, which is housed at the U.S. Department of Health and Human Services, reported that even well-established, state-administered, IDA programs achieve limited success (Office of Community Services, 2012). For example, 46% of accounts were inactive in the Indiana IDA program and 41% in the Pennsylvania Family Savings Account (Office of Community Services, 2012). A long-term research project following an IDA program in Tulsa reported that after about 10 years, only 38% of IDA account holder had made an asset purchase (Grinstein-Weiss et al., 2013b).

Standard economic models of lifecycle savings suggest that saving rates can be improved by increasing the return to savings. In the case of the IDA program this means increasing the match rate. However, one program evaluation found that, while higher savings matches increase the likelihood of making a deposit, higher match rates are associated with lower total deposits made by the individual (Schreiner, 2004). If more generous match rates do not motivate greater saving, then it may be necessary to look outside of the standard economic model for obstacles to saving among IDA participants.

Given the limited success of IDAs in increasing targeted saving, we sought to determine whether behaviorally informed modifications of program features could increase success rates. An investigation of the structure of existing IDAs revealed a number of potential shortcomings, and prior applications of ideas from behavioral economics to public policy pointed to specific potential improvements. Several prior studies have found that behavioral interventions that are free, or very low cost, can have effect sizes that are comparable or greater than costly interventions that increase the magnitude of objective incentives. For example, in a study of Danish savers, Chetty et al. (2014) estimated that tax protection of saving yielded only a single Kroner of extra saving for every hundred Kroner of incentives; in contrast changing the automatic put-aside rate (an effectively free intervention) yielded additional savings approximately equal to the amount put aside (that is, there was no crowding out of other forms of saving).

Here we report results from the first randomized controlled trial examining the impact of modifying IDA program features. We implemented field experiments that tested a series of interventions designed to be both simple and equivalent in cost to the program in place at each IDA agency. Our interventions were structured to target systematic biases that can impede savings. First, we addressed the scarcity of attention and self-control that likely hinder savings by increasing the salience of deposit deadlines with reminder calls. Second, we addressed the lack of accountability in the savings program by introducing calls after savings-deposit deadlines were missed (see Lerner and Tetlock, 1999 for review). Third, we implemented twice monthly – as opposed to monthly or quarterly – deposit deadlines, so participants could curb consumption over shorter intervals, and make smaller individual deposits. This intervention also has the advantages of creating more deadlines to save and makes each deposit seem less significant by

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playing on the "peanuts effect" (Markowitz, 1952, Prelec and Loewenstein, 1991, Weber and Chapman, 2005). Fourth, we aimed to increase the subjective value of the match incentive based on people's tendency to overweigh small probabilities by delivering part of the match in the form of a lottery (Gonzalez and Wu, 1999, Kahneman and Tversky, 1979). Finally, we addressed people's preference for improving sequences by increasing the match rate over time (Haisley and Loewenstein, 2011, Loewenstein and Prelec, 1993).

All interventions were tested in randomized field trials, using newly recruited or existing IDA savers. The field experiments were conducted over a period of 20 to 47 months in the years from 2009 to 2013. Data were collected at eight large IDA agencies in Connecticut, Missouri, California, Oregon, Texas, Kentucky, Michigan, and the District of Columbia, including both network and single-site agencies in urban and rural locations. The eight IDA agencies received federal funding, which provides for a standard program structure, but the programs differed with regard to size, match rate, savings goal and monthly deposit requirement. The level of randomization varied from agency to agency and included both individual-level and site-level randomization, based on whether the IDA agency was a single or multi-site operation. In all, our experiments reached 895 participants.

The results from our field experiments were both surprising and disappointing. On average, baseline savings and retention rates in the different programs we examined were quite similar to those reported in prior research. However, the retention and savings rates in the behavioral intervention groups were not statistically different from those for the control groups for any of the interventions. Overall, the results suggest that behavioral problems of the type our interventions targeted may not have been responsible for low savings rates. Of course it is also

possible that other behaviorally informed interventions would have been more successful than those we tested.

Among studies of IDAs, ours is unique in testing whether changes to specific program features can improve outcomes. Our set of randomized controlled trials examining the impact of modifying IDA features adds important experimental evidence to the literature on the effectiveness of community-based efforts in helping lower-income Americans build assets.

The remainder of the paper is structured as follows. Section I provides background on the IDA program, reviews research on IDAs, including tests of interventions intended to increase saving, then explains the rationales for the specific interventions that we test. Section II provides the rational and Section III details of each of the interventions. Section IV presents estimates of the impact of our behavioral interventions on amounts deposited in each savings period, whether a deposit was made in each savings period, total savings, and the ratio of total savings by total savings goal. Section V discusses the results and concludes.

I. Background: Individual Development Accounts (IDAs)

Although individual IDA programs differ on many dimensions, they do share some basic features. All IDA programs offer a savings match, and all can only be used for approved purchases, which typically include closing costs or the down payment for a home purchase, expenses for post-secondary education, and small business expenses. Matching funds are directly paid to the vendor, never to the IDA program participant. To be eligible for the match funds, participants are required to meet a savings goal and to complete financial literacy training, typically about 12 hours (Office of Community Services, 2012).

The program is limited to individuals and families with household incomes below 200% of the federal poverty lines. According to reports to Congress, about half of IDA account holders have household incomes between 150 and 200% of the federal poverty line and a quarter have household incomes below the poverty line. The federal reports do not report household debt of IDA participants. Savers typically deposit into an IDA via phone or online banking, by visiting a bank branch, or by handing a check to the case manager at the IDA program agency. About 44% of IDA savers make unapproved withdrawals; only about 6% of withdrawals are for allowed emergencies, such as certain medical and housing-related expenses (Office of Community Services, 2012). Since program inception in 1998, about 80,000 individuals have opened IDAs. Federal appropriations for the program reached $18.95 million for FY 2016, and IDAs are offered by nearly 2,000 non-profit agencies across the United States.

The largest evaluations of IDA program effectiveness have yielded mixed results. The IDA program was first evaluated by Sherraden and collaborators (Sherraden, 2000) in a program run out of Washington University of St. Louis. The American Dream Demonstration project, which ran from 1997 to 2001 provided initial data that examined the savings behavior of program participants (Grinstein-Weiss et al., 2007, Grinstein-Weiss et al., 2006, Grinstein-Weiss and Zhan, 2006, Schreiner et al., 2006, Sherraden et al., 2004).

The Demonstration project was followed up with a longitudinal study at an IDA program in Tulsa. The goal was to assess the impact of the IDA program with four waves of survey data collection reaching 10 years past enrollment. Randomized assignment of study participants into the IDA program was implemented. Short-term results were encouraging, showing that the treatment group had more financial assets, better savings motivation and behavior (Grinstein-Weiss et al., 2010, Grinstein-Weiss et al., 2008, Huang, 2010, Lombe et al., 2007). The long-

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term results, however, were disappointing. Compared to the control group, IDA program participants did not fare better with regard to the main goals of the program, specifically to increase homeownership and postsecondary education. Randomization to the IDA group produced no significant benefits in homeownership rates, home equity accumulation or foreclosure avoidance, nor on post-secondary degree completion or level of education (Grinstein-Weiss et al., 2013a, Grinstein-Weiss, Sherraden, Gale, Rohe, Schreiner and Key, 2013b).

II. Rationale for Interventions

The lack of long-term impact of IDA programs is surprising from a standard economic perspective. The average match rate offers savers a guaranteed 200 percent rate of return on their investment if they are able to meet their deposit goal. However, though IDAs offer highly attractive rates of return, the same factors that impede saving for low-income families are likely to impede saving in IDAs. Low-income individuals and families are confronted by a number of financial, institutional, social, psychological obstacles that make financial choices more overwhelming (Bertrand et al., 2006). Our IDA program interventions were designed to help participants overcome these barriers to saving success. Our study connects to a broad literature that investigates why many individuals fail to save and what financial innovations might help them save more, such as default options (Carroll et al., 2009, Madrian and Shea, 2001) or commitment devices (Ashraf et al., 2005, Ashraf et al., 2010, Thaler and Benartzi, 2004). The following four sections describe the innovations that we designed for the IDA program.

A. *Reminder and Accountability Calls*

In one of our four interventions we addressed two potential barriers to saving: low attention and lack of accountability. We experimentally manipulated whether participants received calls reminding them to make a deposit before their monthly deadline. Among low-income individuals, energy spent on financial matters of the moment leaves relatively little attention to more important decisions that are less immediate, such as whether and how much to save, and where to locate the money to do so (Mullainathan and Shafir, 2009). Given the demands on the time and attention of low income families (Shah et al., 2012) we hypothesized that IDA savers would benefit from systematic reminders. We additionally placed calls to some respondents after the monthly deadline, asking whether a deposit was made and if not, why it was missed. The accountability calls treatment was inspired by a substantial body of research showing that behavior in different domains is responsive to people's feelings of accountability (see Lerner and Tetlock, 1999 for review).

Prior research examining the effect of reminders in different savings contexts has found positive effects. One field experiment with customers of two major banks in the U.K. found that text alerts reduced overdraft charges by 6%, and by 24% when they were combined with a mobile banking app. (Hunt et al., 2015). However, some work has found that the effect of reminders may depend on financial circumstances, and on the specific content of the reminder (Bracha and Meier, 2014, Karlan et al., 2013). For example, in an experiment among borrowers in the Philippines, loan repayment behavior only improved among borrowers who received messages that contained their loan officer's name. The authors' interpretations of their findings

emphasize the role of personal relationship between borrowers and bank employees (Karlan, Morten and Zinman, 2013).

Feelings of accountability may also explain the finding. Most IDA programs do little to hold participants accountable for making savings deposits. We designed the accountability call intervention to increase the salience of accountability on all four of the dimensions outlined by Lerner and Tetlock (1999): 1) the expectation of being observed, 2) identifiability, 3) the expectation that performance will be assessed by another, and 4) the expectation that one will have to give reasons for actions.

B. *Frequency of Deposits*

Our second experiment exploited the role of deposit timing for savings. Most IDAs recommend monthly deposits, and some recommend quarterly ones. Given the difficulty that IDA participants have in accumulating funds – whether due to self-control problems or interpersonal demands on liquid funds – we hypothesized that such long inter-deposit intervals discourage saving. Specifically, we hypothesized that a schedule of smaller, more frequent deposits would produce larger cumulative savings than a schedule of larger, less frequent deposits, despite the additional effort required to make the deposit, by visiting or calling a bank. As such, we experimented with a bi-weekly deposit schedule.

A bi-weekly deposit schedule will align closer with the typical bi-weekly payday of low-income individuals and related spikes in expenditures for eating out and fresh foods reported in U.K.'s Family Expenditure Survey (Stephens Jr., 2006). Beyond playing on liquidity constraints, introducing more frequent deposits also exploits a behavioral effect known as the "peanuts

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effect" – the undervaluing of small dollar amounts – both gains and losses (Markowitz, 1952, Prelec and Loewenstein, 1991, Weber and Chapman, 2005). The peanuts effect may help to explain the popularity of rent-to-own agreements, in which small payments are made over an extended period of time, even though the final sum is higher than the one-payment retail price (Bates and Dunham, 2003).

A final benefit of having a more frequent deposit schedule is that it imposes more deadlines to save, and counters procrastination. A field experiment on the effect of deadlines on the quality of university papers, for example, found that more frequent deadlines improved grades (Ariely and Wertenbroch, 2002). Similarly, increasing the number of deposit due dates could help savers put away money with greater regularity.

C. *Savings Lotteries*

Our third intervention aimed to take advantage of individuals' tendency to overvalue events that occur with small probabilities. We implemented a scheme whereby, instead of guaranteeing a medium-sized match on all deposits, we offered a smaller, guaranteed, match on each deposits, a 20% change of winning a larger match, and a 1% chance of winning a very large match on the deposit. If nothing was deposited at a deposit deadline, no match was obtained. We hypothesized that linking successful savings behavior to the chance of winning a larger match would motivate savers to deposit regularly.

This intervention aimed to take advantage of the motives underlying the popularity of lotteries and other forms of gambling. Gambling, and especially lottery purchasing, is especially popular among lower income households (Kearney, 2005), who are also the group that is eligible

for IDA participation. The attractiveness of combining lotteries and saving is even more directly suggested by the success of lottery-linked savings vehicles, although most of the evidence on their success comes from other countries, given legal restrictions on the offering of such accounts in the U.S. Perhaps the best known of such lottery-linked savings products are the "premium bonds" sold by the British government (Kearney et al., 2011). Some of the best evidence on lottery-linked savings accounts comes from a large retail and commercial bank in South Africa from 2005 to 2008. Besides robust effects on overall savings amounts in the raffle accounts, the program was associated with overall greater regular savings (Cole et al., 2014). One of the few savings lotteries run in the U.S. is the save-to-win program at credit unions in Nebraska from 2012 to 2013, which was found to reduce other forms of gambling, such as casino gambling, by half relative to untreated counties (Cookson, 2014). The program was, however, also accompanied with anti-gambling advertising campaign, which may have been responsible for some or all of the effect.

For the IDA program, we expected that highly personalized, lottery-linked, incentives to save would produce a greater "bang for the buck" compared to the guaranteed IDA match. Additionally, lottery-based incentives introduce entertainment and suspense to the routine of savings. For the IDA program we used a structure in which a portion of the participant match was distributed by a lottery that was linked to participant deposits. Lottery odds were structured so that expected net winnings, coupled with the guaranteed match, equaled traditional match rates. We also structured the lottery such that participants chose a draw number that was theirs for the duration of the program. Research has indicated that continued lottery participation becomes more likely when participants are assigned numbers that have personal meaning – such as one's postal code – rather than a random number (Zeelenberg and Pieters, 2004).

D. *Improving Sequences*

Our final intervention exploits people's preference for improving sequences (Haisley and Loewenstein, 2011, Hoelzl et al., 2011, Loewenstein and Prelec, 1993). For instance, in Loewenstein and Sicherman (1991) study participants gave higher rankings to sequences of income streams that increased in payoff over time than to sequences that decreased, especially for wage profiles (as compared with rental investment income). The choices were made even though the net present values of the decreasing streams were greater than the increasing ones (Guyse et al., 2002).

In the IDA program intervention we offered match rates that increased with the amount that the individual saved. Once individuals reached the midpoint of their savings goal, the match rate of subsequent deposits doubled. This set-up sets an intermediate goal and rewards progress while keeping the expected value of the match equal to the control-group match, since withdrawals could not be made until the end of the savings schedule. We hypothesized that incentives that grew over time would make completion of the program more likely.

III. The Experiments

We implemented four experimental approaches, each one testing one or several of our interventions. We began by testing the reminder and accountability intervention alone, then added the increased deposit frequency, and then the lottery intervention. The final design tested the improving sequences intervention, and also included reminder calls.[2] The experiments were conducted in collaboration with eight IDA program agencies, which resulted in 48

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[2] We included the calls in all our experiments because they had the added advantage of allowing us to remind each group of the features of their IDA program, including our interventions.

implementation sites. To recruit research sites, we used personal contacts to program agencies (1 recruit), referrals by the Corporation for Enterprise Development (CFED), a lobbyist organization in Washington (3 recruits), and a CFED scheduled national webinar for IDA agencies in February 2010 (4 recruits). The goal was to collaborate with larger IDA programs (at least 100 enrollments in the following two years), electronic data management, high dropout rates, and low use of direct deposit. Recruitment occurred from March 2009 to October 2010.

Randomization was done at different levels to accommodate the enrollment practices at the sites: either at the saver or agency level, and either among existing IDA clients or new enrollees. We excluded IDA program participants who indicated in program-administrative paperwork that they were intending to use direct deposit for their IDA savings.[3] As shown in Appendix Table A3 to A10, randomization was effective. We detail site-specific differences between control and treatment participants below.

An important potential limitation of our randomization approach is that the consent forms stated in which experimental group the saver was assigned to. An example of a consent form is provided in Appendix Figure A1. In hindsight, the better approach would have been to create a generic consent form that covered all experimental groups and have the research team make the assignment to the experimental groups once consent forms were returned. We adopted the approach we did in response to pressure from IDAs for participants to know, at the outset, what they were getting into. Despite the imperfection of the randomization approach, we are not aware of cases in which IDA program applicants rejected the program described in the consent form and refused to participate in the research project. The consent form was part of the typically thick IDA program enrollment package, which collects demographic and financial details for program

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[3] Only about 10% of program participants use direct deposit services to make their savings deposit (Office of Community Services, 2012).

applicants, includes detailed program information, and provides the savings contract. We think it is very unlikely that this design flaw actually had a substantive impact on our results. In addition, if our randomization strategy have had an effect, it should have worked in bias of finding an effect, but this was not the case.

Each experiment engaged new and/or existing IDA program clients at the participating agency. We engaged in no additional participant recruitment beyond the agency's own practices. For existing participants, consent was obtained by mail or in person at an IDA program's financial education workshop or one-on-one counselling meeting. For new clients, consent was obtained during enrolment in the IDA program. The experiments lasted from 20 to 47 months. Project implementation started in March 2009 at the first collaborating non-profit agency and was ended at all partner agencies in February 2013.

We collected both administrative data and bank deposit data. Administrative data included demographic information collected at program enrollment, which included IDA participants' gender, race, marital status, educational status, employment status, age, number of children and adults in the household, and household income. Program-specific information included dates of IDA account opening and program exit, updates on the status of each program participant (active, graduated, dropout), the initial savings goal, the recommended monthly or quarterly savings goal, the initial asset goal for the savings, and the match rate. For purposes of data analysis, we used the consent date provided on the research consent form as the treatment start date. Bank deposit data were provided in monthly sums for each study participant. The exception was the lottery intervention, for which each bank deposit of each participant was made

available electronically. The following four sections describe each of the four experiments in greater detail. All experiment details are also illustrated in Table 1.[4]

A. *Reminder and Accountability Experiment*

In this experiment, participants were asked to enroll in the automated phone system to: (1) receive reminder calls two to three days before deposit deadline and (2) accountability calls two to three days after deposit deadline. The calls were dispatched through an automated phone service, offered by ifbyphone.com. Reminder calls were short, automated calls lasting only a few seconds. Accountability calls were automated calls and inquired about whether a deposit was made, the amount deposited, and the reason for missed deposits. The text of the calls is provided in Appendix Table A2.

This experiment was implemented at four IDA agencies in D.C., Kentucky, Michigan and Texas among a total of 499 savers over a period of 40 months, from November 2009 to February 2013. Savers were randomized between three groups: treatment group 1, with reminder calls at the end of each month (210 savers), treatment group 2 with reminder and accountability calls at the end of each month (147 savers), and a no-call control group (142 savers). Descriptive statistics of the sample are shown in Table 2a.

[Table 2a about here]

The randomization approach was tailored to IDA program agencies. In D.C., randomization occurred at the individual saver level through computer-based random assignment of existing savers and randomization at the individual-saver level by manually alternating the consent forms as new savers were enrolled. New IDA participants were assigned to treatment

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[4] Appendix Table A1 provided further details.

and control groups one-by-one, in the order in which they walked into the agencies to enroll in the IDA program. The following sequence was used: Client 1: Treatment group; Client 2: Control group; Client 3: Treatment group; Client 4: Control group; etc. In Texas and Michigan, randomization occurred at the individual saver level by manually alternating the consent forms by month of orientation session (Texas) or month of enrollment (Michigan). In Kentucky, randomization occurred at the agency-level. In these locations, we aimed for a balance of urban and rural agencies and about the same number of savers in each of the experimental groups.

Randomization results for the four data collection sites for this intervention are shown in Appendix Table A3 to A6. Among our 21 demographic and program administrative covariates we find statistically significant differences between control and treatment participants in: one covariate (in Michigan, Texas), three covariates (in Kentucky), and seven covariates (in D.C.) for. For instance, in D.C., control and treatment groups differed by percentage of male study participants ($p<0.10$, Control vs T2), African-American ($p<0.10$, Control vs T1, T2), college education ($p<0.10$, Control vs T2), home purchase and postsecondary education ($p<0.10$, Control vs T2). At the Texas site, the groups differed again by percentage of male study participants ($p<0.10$, T1 vs T2). At the Kentucky implementation sites, the number of children ($p<0.10$), the savings goal to purchase/renovate a home ($p<0.10$), and the average match rate ($p<0.05$) were different. At the Michigan sample, household income ($p<0.05$) differed significantly between treatment and control group.

B. *Increased Frequency Experiment*

In this experiment, program participants who would ordinarily have been asked to deposit on a monthly schedule were instead asked to deposit on a biweekly schedule, on the 15th and 30th of each month. Both groups were provided with reminder and accountability phone calls at each deposit deadline.

The experiment was implemented at two savings program agencies, in Missouri and Connecticut over a period of 40 months, from November 2009 to February 2013. Overall, 110 savers participated in this experiment. Fifty-three savers were assigned to the bi-weekly treatment group, with the remaining 57 savers in the control group. A description of the sample characteristics is presented in Table 2b. Randomization in Missouri was done at the saver level among existing clients using computer randomization. In Connecticut, we enrolled only new participants, randomly assigning them to the treatment and control groups according to the order of their arrival at the IDA office.

[Table 2b about here]

We have complete demographic characteristics for 45 treatment and 49 control group members. As shown in Appendix Table A7, the Missouri site shows significant differences between control and treatment group with regard to number of children ($p<0.10$) and household income ($p<0.05$). Randomization at the Connecticut sample worked well with regard to demographic characteristics (see Appendix Table A8).

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C. *Lottery Incentive Experiment*

The lottery-incentive experiment tested the effectiveness of a bundle of interventions introduced together. Along with the bi-monthly deposit deadline, reminder and accountability calls at each deposit deadline, participants in the treatment group received a lottery-based savings match. Control participants, by contrast, did not participate in the lottery or received calls, and were asked to follow the conventional monthly deposit schedule. We implemented this "kitchen sink" treatment condition with the intent of maximizing the likelihood of improving savings outcomes with our modified program features.

Control participants received a match of $1.50 for every dollar deposited. The lottery-based savings match, in contrast, was structured so that 50 cents of the $1.50 match that control group savers received was to be given out by lottery. For a given deposit period (every two weeks for treated participants), any deposited funds were matched at a lower, guaranteed rate of $1 for every $1 saved; funds deposited in the period, however, were eligible to win a higher rate of $3 or $15 for every dollar deposited.

The lottery match was distributed as follows: every participant in the treatment group was randomly assigned a two-digit number at the time they enrolled in the IDA program. Participants received their personal lottery number printed on a personalized certificate. At the time of each deposit deadline on the 15th and 30th of each month, lottery winners were determined by the two numbers after the decimal point of the closing Dow Jones Industrial Average (DJIA) of that day.[5] If one digit of a participant's lottery number matched one of the DJIA numbers, the person received a $3:$1 match on the funds deposited in that period (a 20% chance of winning). If both

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[5] The DJIA is a statistically random number, widely published and available, a feature which created transparency about the selection process.

digits matched the DJIA numbers, the person received a large, $15:$1 match (a 1% chance of winning). This arrangement had an expected value equivalent to the $1.5:$1 match.

The lottery-incentive experiment was tested at one IDA program agency in California from March 2009 to September 2012, over the course of 43 months. Eighty-seven savers participated in this experiment: 42 in the treatment group and 45 in the control group. Randomization at the saver level was computerized. Any family members and friends of subjects who had already been recruited were automatically assigned to the same group to avoid an overspill of information about the research project. The characteristics of the lottery experiment sample are shown in Table 2c.

[Table 2c about here]

As shown in Appendix Table A9, there are some differences between the two groups. The control group has a significantly higher number of college-educated savers, ($p<0.05$). The control group is also marginally different with regard to a higher household income ($p<0.15$) and younger age ($p<0.15$). The final average treatment group match rate was $1.59; the control group match was $1.50. The control group selected a slightly higher overall savings goal at program enrollment ($p<0.10$) and was more likely to save for a home ($p<0.05$).

D. *Increasing Match Experiment*

Our final intervention tested the effects of increasing incentives over time. In the treatment condition, savers faced a lower $2 match rate until reaching the midpoint of the savings goal, at which point the match rate on subsequent deposits increased to $4 per $1 saved. Over time, the match was equivalent to the original $3:$1 match that the control group received.

The switch was programmed in the savings program database, and savers were informed of their earned match in their monthly savings statements. In this experiment both the treatment group and the control group received reminder phone calls at the end of the month.

This experiment took place across a network of IDA programs in Oregon, at 38 non-profit agencies over a period of 26 months, from January 2011 to February 2013. We randomized at the agency level. Most participants were new IDA program enrollees (16 existing participants participated). As shown in Table 1, 193 savers participated in the experiment: 74 in the treatment group (match rate increase plus reminder calls at the end of each month) and 119 in the control group (constant match rate, and reminder calls at the end of each month). A description of the characteristics of savers is shown in Table 2d.

[Table 2d here]

As shown in Appendix Table A10, comparison of means between the two groups shows that the control group has a higher number of African-American (p<0.000) and fewer white study participants (p<0.10). The control group was less likely to save for home purchase (p<0.000) and more likely to save for post-secondary education (p<0.000), as indicated at program enrollment.

E. *Estimation Strategy*

We consider successful participation in the IDA program to constitute making consistent, regular deposits that lead to accumulation of savings and, eventually, to a matched withdrawal and purchase of an intended asset. We employ two main analytical strategies to test the efficacy of our behavioral interventions. The first approach measures whether those exposed to the

treatment conditions exhibit better savings outcomes than control participants at the end of the study period. The second approach measures whether there is a systematic difference in savings *trajectories* over the course of the study period between treatment and control subjects.

In approach (1), we explore the effect of the interventions on two measures of savings. First, we examine a continuous measure of total saving net of any unmatched emergency withdrawals the saver may have made. We are also interested in whether a saver successfully completed the program. This is a more difficult outcome to quantify, since the savings required to have successfully completed the IDA program varied by agency, and across savers by stated saving goal. We construct an indicator that equals 1 for participants whose total savings net of unmatched withdrawals met or surpassed their initial stated savings goal (*Met Goal*).[6]

For approach (1), we treat the savings data as a cross-section that captures the final savings and asset positions of IDA program participants. We estimate variants of the following model to test the effects of our interventions:

$$S_i = \alpha + \gamma treatment + X'_i \beta + \varepsilon_i \tag{1}$$

The key coefficient from (1) is γ, which captures the average difference in savings outcome between treated and control participants. In some analyses, we additionally control for the variables included in X_i, including measures of gender, age, race, marital status, educational achievement, work status, household size and income, the participant's stated savings deposit goal and asset goal. X_i also includes indicators for agency (in the frequency and accountability experiments) and the effective match rate faced by lottery participants. In all cases we estimate OLS or linear probability models with robust standard errors.

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[6] We allow for a margin of error in constructing this outcome so that those who came within 5% of their savings goal are considered to have met the goal. We also examined a more liberal measure of meeting a savings goal where we use the smallest match-eligible saving goal available at a saver's agency as the benchmark.

The results obtained from approach (1) provide evidence as to whether our behavioral interventions improved the final positions of IDA program participants. However, even if estimates of model (1) reveal no beneficial impact, our treatments could still have affected the progression and savings patterns of IDA participants. We investigated these issues with approach (2), which tests if the treatment improved progress in the IDA program. We investigate three outcomes: an indicator that equals 1 if the participant made a deposit in a given month (*Made a Deposit),* the value of any given monthly deposit (*Deposit Amount),* and a running total of total savings, net of any unmatched withdrawals (*Running Total).*[7] While there are other outcomes that could measure program efficacy, such as the likelihood of dropping out or time to program completion, we believe that the selected outcomes provide cleaner measures of success in the our data.

For approach (2), we estimate the following model, which provides evidence as to whether treated participants exhibited different savings trajectories than control participants.

$$S_{it} = \alpha + \gamma treatment_i + \delta M_{it} + \eta(treatment_i \ x \ M_{it}) + X'_i\beta + \varepsilon_{it}, \tag{2}$$

where S_{it} is the outcome indicator variable at a particular point in time, and M_{it} represents the duration of program participation (in months) at time t. In some specifications, we also interact the treatment indicator with the time-in-program variable. The results obtained from model (2), therefore, show us both whether the average savings outcomes of treated participants are different from those of control participants (γ) and whether the progression of saving behavior is significantly different for treated participants (η). We again estimate OLS or linear probability models with robust standard errors clustered at the participant level.

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[7] Participants were able to withdraw money from their IDA accounts. These withdrawals, however, were not eligible for the match funding. The proportion of savers who made any unmatched withdrawal ranged from between 8% and 25%, depending on agency. Repeating analyses ignoring unmatched withdrawals left results largely unchanged.

Before presenting results of the regression analyses, we present several figures that illustrate the basic, descriptive patterns evident in the data. The regression results that follow formalize the same basic relationships that the descriptive (pictoral) analyses reveal.

First, in Figures 1a to 1d, using simple bar graphs we compare the final savings positions for participants who were treated versus not. We show how the total amount saved (*Total Deposit*) and meeting the total savings goal (*Met Goal*) outcomes vary by treatment status, along with two additional variables: the likelihood that a participant made any deposits, and a more liberal measure of having met the savings goal, which indicates participants who came within 5 percent of their agency's lowest available savings target. We additionally plot the evolution of average cumulative savings as participants progressed in the program, by treatment group. These graphs also include linear fit lines for each group, which represent the simple correlations between cumulative savings and time in program for each group.

Because of the small sizes of IDA programs, generally on average only 103 IDA participants per federally funded program (Office of Community Services, 2012), each experiment was implemented using a relatively small number of participants.

IV. Results

A. *Reminder and Accountability Experiment*

Figure 1a presents simple bar graphs that compare the final savings positions for participants who were treated versus not. These show how the total amount saved (*Total Deposit*) and meeting the total savings goal (*Met Goal*) outcomes vary by treatment status, along with two additional variables: the likelihood that a participant made any deposits, and a more liberal

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measure of having met the savings goal, which indicates participants who came within 5 percent of their agency's lowest available savings target. The total amount saved was $989 in the control group, $1,018 in the reminder-calls group, and $988 in the reminder-and-accountability calls group. In Figure 1a, there are no apparent differences between the average final savings positions of treated and control participants.

Figure 1b plots the evolution of average cumulative savings as participants progressed in the program, by treatment group. These graphs also include linear fit lines for each group, which represent the simple correlations between cumulative savings and time in program for each group. shows some indication of positive effects of both treatments on the savings rate. The slope of the correlation lines for both the reminder calls alone, and for the reminder and accountability calls, is greater than that of the control line. This indicates that participants in both treated groups may have made slightly quicker progress in reaching their savings goal than control participants.

[Figure 1 about here]

We investigate whether the patterns revealed in Figure 1 are statistically significant using regression analysis. The results of these analyses for the reminder and accountability experiment are presented in Table 3a. The main treatment effects are estimated using two indicator variables, one for each call type. In our approach, all participants in both treatment groups are coded 1 for the reminder call indicator, while only those in the reminder and accountability treatment are coded 1 for the accountability call indicator, implying that the estimated coefficient on the *accountability* variable is the marginal effect of the accountability calls, relative to the reminder calls alone.

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Columns 1 through 4 of the table report the results of simple regressions on the cross-sectional outcomes. For these outcomes we show results from models estimated with only the treatment and site indicators (Columns 1 and 3), as well as from models including a set of controls obtained from the agency's administrative data. The results mirror Figure 1a. We find minimal evidence of differences in final savings outcomes between the treated and control participants. Participants who received the reminder calls alone saved about as the same amount as the control group and were about equally likely to have met their savings goal. The estimated marginal effects of the accountability calls are always negative, but largely insignificant: only the estimated accountability effect on meeting or surpassing the savings goal (the *Met Goal* outcome) is significant at the 10% level in the model with no controls.

The remainder of the table shows regression results for the outcomes that measure progress in the program. For these outcomes, along with estimates on the treatment indicators, we also show the estimated coefficients on the *Months in Program* variable and its interaction with each treatment variable. Thus, we estimate the average effect of the treatments on the progression outcomes, as well as the dynamic effect of the treatments on savings progress. In the interest of space, we only report results from models excluding controls.

The results in Columns 5 through 7 present a more nuanced story than the simple regressions. For the group which received the reminder calls alone, the likelihood of having made a deposit in a given month appears higher, on average, than the control group. However, the estimated effect in this specification is insignificant. The marginal effect of the accountability calls, however, is negative and significant: relative to the reminder calls alone, the accountability calls appear to decrease the average likelihood of deposit by about 15 percent. We are not able to

detect any significant effects of treatment on the change in likelihood of making a deposit over time, and the estimated coefficients on both interactions terms are close to zero.

Turning to the deposit amount outcome, we see a similar pattern. The reminder calls appear to have had limited effect on average deposit amount: the estimates of both the direct effect, and the effect over time, are small and statistically insignificant. The marginal effect of the accountability calls is negative and significant, indicating that at the outset, participants who received the accountability calls were depositing about half as much on average as control and reminder participants. The positive, marginally significant coefficient on the interaction term, however, suggests that those in the accountability group were increasing their average deposit over time, while participants in the other two groups reduced their average monthly deposit by about 2 dollars per month.

Cumulatively, the results suggest that both calls had a minor effect on savings behavior. Participants in both groups appear to have started savings more slowly than the control participants, but progressed in the program at a faster rate than the control participants. This finding may indicate that the calls were first deemed annoying but that treatment group participants, after getting used to them, caught up on their deposits. The negative and significant coefficient estimate on the accountability call indicator, combined with the estimate on the interaction terms suggests that cumulative savings among those in the accountability group would have surpassed control group savings after about nine months in the program. However, these minor behavior changes did not impact the average final savings positions of treated participants, most likely because many participants stopped saving – either reaching their deposit

goal or dropping out – before the nine-month mark[8]. Overall, there is no evidence that the

reminder calls increased saving, and the accountability calls may have marginally decreased it.

[Table 3a about here]

B. *Increased Frequency Experiment*

Figure 2 shows the descriptive results for the increased frequency experiment. In this

experiment, the treatment appears ineffective. Average final savings positions in each group are

similar. For example, the total amount saved was $714 in the control group and $675 in the

treatment group. Similar to the reminder and accountability experiment, however, there is some

minor evidence of a slower savings rate among treated participants. The regression results

investigating treatment efficacy confirm this. Table 3b shows how outcomes differ between

treated and control participants, and reveals no significant effects of increased frequency on the

total amount saved or the likelihood of having met the deposit goal. Only the running savings

total is significantly different across groups. It is lower for treatment group participants,

however.

[Figure 2 about here]

The cumulative savings total appears to increase at a higher rate among treated

participants, a marginally significant result. The treated group begins saving more slowly, but

their savings surpass control participants after about six months in the program. It appears that

treatment group participants (who are asked for additional effort by making two deposits a

month) do worse at first, but their trajectories improve. Does it imply that as the invention

becomes familiar, IDA participants may start off stronger under it? Given that half the savers in

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[8] The median time in program was 10 months.

this experiment stopped savings after five months or less, it is not surprising that the treatment effect on the saving rate does not lead to a detectable significant difference in the final savings position of the treated. The estimated results do not allow us to discern whether the improved savings rate among treated participants is due to improved consistency in the likelihood of making a deposit or in the amount of the deposits.

[Table 3b about here]

C. *Lottery Incentive Experiment*

While the accountability and increased frequency treatments produced mainly null effects, the lottery treatment may have actually decreased saving. The total amount saved was $1,955 in the control group and $1,821 in the treatment group. The descriptive results in Figure 3 show some minor indication that control participants were more likely to reach their savings goal than treated participants. Further, control participants appear to have saved at a slightly faster rate than treatment participants.

[Figure 3 about here]

Indeed, the regression results in Table 3c confirm these patterns. The estimated effects on cumulative savings' running total (Column 9) do not indicate significant differences between savings trends in the two groups, although the negative signs of the estimated coefficients imply differences in favor of the control group.

[Table 3c about here]

D. *Increasing Match Experiment*

The final experimental intervention was similarly ineffective. The descriptive results in Figure 4 reveal minimal differences between the final savings positions of those who received the increasing match treatment as compared to control participants. The total amount saved was $1,147 in the control group and $1,076 in the treatment group. Comparing progress in program across groups also reveals no significant differences; the savings trajectory and associated trend lines are nearly identical.

[Figure 4 about here]

The regression results in Table 3d confirm the impression conveyed by the figure. We see only a significant difference in the likelihood of making a deposit between treated and control participants, and all estimated coefficients are negative, indicating potential harmful effects of treatment. Treatment participants however increased their deposit likelihood over the course of the program. After 8 months in the program, treated participants became slightly more likely than control participants to make a deposit. Overall, however, our treatment produced only minor effects on behavior.

V. Discussion

This research examined the effectiveness of four behaviorally-informed interventions to improve saving in community-based savings programs for low-income families. We investigated the impact of reminder and accountability calls at deposit deadlines, increased deposit frequency from once to twice monthly, lottery-based distribution of the savings match, and increasing

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savings matches over time. To the best of our knowledge, this is the first study to test whether behaviorally-motivated interventions can help low-income families save in the popular Individual Development Account (IDA) saving program for the poor.

Overall, our results are disappointing. None of the four interventions increased savings. Across experiments, the savings outcomes did not significantly differ between treatment and control groups. This was true whether we examined final savings positions or progress over time. The few significant results we did uncover, moreover, indicate that some of our interventions may have backfired. The accountability calls, for example – which were placed after each deposit deadline – appear to have negatively affected meeting the savings goal, making a deposit, and the deposit amount. The running total of treatment-group deposits in the increased frequency experiment was significantly lower compared to the control group; however, the treatment-group showed slightly faster savings progress. And, in the increasing match condition, the likelihood of making a deposit was lower for the treatment group, although this group also showed faster savings progress over time.

Our sample sizes limit our ability to detect statistically significant differences, but it seems doubtful that positive effects would have emerged with larger samples. As just noted, most of the significant differences we detect are in favor of control group participants. Even with larger sample sizes, it is unlikely that we would have detected differences in the opposite direction to those we find.

The socioeconomic characteristics of our study participants may help to explain the null results. Household incomes ranged from $20,000 to $25,000 and household sizes from three to four persons. In 2011, when a large part of our enrollment occurred, the federal poverty threshold was $18,530 for a household of three and $22,350 for four persons. Our study families are very

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likely living on the margin of their budget constraints. If liquidity constraints, rather than cognitive biases, are their primary impediment to saving, then it may not be surprising that our interventions were unsuccessful.

Findings from the recent literature on scarcity (e.g., Shah, Mullainathan and Shafir, 2012) may also help to explain the results. When resources are scarce, people invest large amounts of time and energy in making ends meet (Edin et al., 2013). Less energy is left for other demands, which, as a result, receive less attention (Mani et al., 2013). The more complicated matching structures introduced by several of our interventions (e.g., the lottery treatment and the increasing match rate treatment) may have been distracting and overwhelming for participants. The relatively low educational attainment among program participants may also indicate low levels of numeracy or comprehension, both factors which could have made the interventions difficult to understand (Peters and Bjalkebring, 2015, Peters et al., 2007).

We explored these potential explanations for our failure in follow-up interviews. From April to June 2011 and January to February 2012 we conducted phone interviews with study participants at our four longest-running research sites to gain insight into their perceptions of the experimental interventions. We conducted interviews with 133 savers (response rate: 42%, N=314). We asked respondents in relevant conditions how they felt about the reminder and accountability calls, which we provided to treatment group savers in all experimental conditions. Answers to this question provide evidence in support of the cognitive limits argument. Savers indicated that the calls : "… could be overwhelming when you are busy, almost like a bill collector;" "… were like telemarketing calls. I knew I had to save, so I just hung up on the calls;" and "… were annoying. Any call that calls you when you don't expect it and you don't know what it is. It would have been better if it was a text message to remind you or let you know that

your savings increased. I got calls when I was talking to an employer and I thought it was another potential employer or something." These responses suggest that the calls were viewed as a nuisance, were distracting, and were hard to manage in the context of everyday life demands. The fact that we used them in all of our experiments may have been a factor in the failure of our interventions.

A second explanation for the failure of the behavioral interventions may have been the distant goal in this savings program. At program enrollment, the savings goal is several years away. The temporal distance of the ultimate asset purchase in the IDA program may have overridden any potential response to the interventions. Future experimentation should explore whether shorter program duration with more attainable overall goals would work better for IDA clientele. Findings for debt repayment in debt management plans have documented that meeting smaller sub-goals motivates consumers to persist in pursuit of an overall goal (Gal and McShane, 2012).

Overall, the results of the present studies document the limitations of behaviorally-informed interventions that aim to facilitate savings among low-income individuals and families. Our research shows that interventions that address systematic biases may be ineffective, especially when poor program performance is due to classical budget and liquidity constraints, rather than to cognitive constraints. Further, we show that behavioral interventions can even worsen outcomes if they are overly complicated or burdensome. These findings speak to the value of field experimental research and of extended data collection time frames. Our results act as an important reminder that behavioral interventions are not failsafe.

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TABLE 1—SUMMARY OF EXPERIMENTS

	"Lottery"	"Increasing match"	"Frequency of deposit"		"Reminders & Accountability"			
Agency	California	Oregon	Missouri	Connecticut	D.C.	Texas	Kentucky	Michigan
Groups	<u>Treatment</u>: Reminder and accountability calls; Twice monthly deposits; Lottery match <u>Control</u>: No interventions	<u>Treatment</u>: Match increase mid-way to savings goal; Reminder calls <u>Control</u>: Reminder calls	<u>Treatment</u>: Twice monthly deposits; Reminder and accountability calls <u>Control</u>: Reminder and accountability calls		<u>Treatment 1</u>: Reminder calls <u>Treatment 2</u>: Reminder and accountability calls <u>Control</u>: No interventions			
Treatment n	42	74	29	74	Treat 1: 30 Treat 2: 32	Treat 1: 124 Treat 2: 103	Treat 1: 43 Treat 2: 0	Treat 1: 13 Treat 2:12
Control n	45	119	21	36	43	85	14	0
Total N	87	193	160		499			
Savings goal options	$2,000 (PSE, ME)* $2,666 (HO)	$1,000/ $2,000/ $3,000	$1,500	$500 $1,000 $1,500 $3,000	$1,000	$2,000	$2,000	$1,000
Monthly suggested deposit	$35/$25 $55/$40 $70/$55	$25	$10	$50	$25	$35	$20	$20
Match rate	$1.50:$1	$3:$1	$2:$1 $1:$1	$2:$1 $1:$1	$3:$1	$2:$1	$2:$1	$2:$1 $3:$1
Start date	03/2009	01/2011	11/2009	10/2010	11/2009	09/2010	10/2010	09/2010
End date	09/2012	02/2013	03/2012	02/2013	05/2012	02/2013	02/2013	04/2012
# data collection sites	1	38	1	6	1	1	19	3

*HO=home purchase, PSE=post-secondary education, ME=micro-enterprise development

TABLE 2A—SUMMARY STATISTICS OF REMINDER AND ACCOUNTABILITY EXPERIMENT

Variable	Control Group Mean	Control Group Standard Deviation	Treatment Group Reminder Mean	Treatment Group Reminder Standard Deviation	Treatment Group Accountability Mean	Treatment Group Accountability Standard Deviation
			A. By Number of Deposits			
Deposit amount (US$)	85.10	263.74	89.78	320.41	89.37	279.71
Made a deposit	0.52	0.50	0.58	0.49	0.58	0.49
Deposit amount, running total (US$)	507.61	822.41	586.10	1,022.37	566.08	1,008.36
N deposits	1,666		2,432		1,640	
			B. By Number of Study Participants			
Met goal	0.23	0.42	0.21	0.41	0.16	0.37
Total saved (US$)	989.21	1,243.55	1,018.49	1,365.59	988.27	1,467.42
Made no deposits	0.13	0.33	0.13	0.34	0.08	0.28
Male	0.29	0.46	0.22	0.42	0.32	0.47
White	0.39	0.49	0.45	0.50	0.31	0.47
Married	0.22	0.41	0.19	0.40	0.27	0.44
College degree	0.34	0.48	0.38	0.49	0.38	0.49
Full-time employment	0.73	0.44	0.77	0.42	0.83	0.38
Age	33.48	10.02	35.56	10.38	34.64	9.98
Number adults in household	Not available from all sites					
Number children in household	1.66	1.32	1.46	1.27	1.69	1.38
Household income (US$)	21,497.90	11,123.62	21,074.85	12,646.39	20,957.29	10,608.63
Savings goal (US$)	1,816.18	466.70	1,735.44	537.98	1,784.25	480.33
Effective match rate	34.19	167.01	21.27	30.11	37.10	62.42
Periodic savings goal (US$)	2.30	0.49	2.58	1.49	2.29	0.45
N study participants	142		210		146	

TABLE 2B—SUMMARY STATISTICS OF INCREASED FREQUENCY EXPERIMENT

	Control Group		Treatment Group	
	Mean	Standard Deviation	Mean	Standard Deviation
Variable	A. By Number of Deposits			
Deposit amount (US$)	59.17	143.13	66.31	150.86
Made a deposit	0.54	0.50	0.59	0.49
Deposit amount, running total (US$)	403.46	524.67	306.69	465.89
N deposits	737		653	
	B. By Number of Study Participants			
Met goal	0.05	0.23	0.02	0.14
Total saved (US$)	714.96	634.04	675.81	544.88
Made no deposits	0.06	0.24	0.04	0.21
Male	0.14	0.35	0.07	0.25
White	0.20	0.41	0.07	0.25
Married	0.04	0.20	0.04	0.21
College degree	0.22	0.42	0.36	0.48
Full-time employment	0.69	0.47	0.80	0.40
Age	37.59	10.30	37.76	13.01
Number adults in household	1.33	0.59	1.24	0.68
Number children in household	0.94	0.97	1.24	1.23
Household income (US$)	1,864.52	900.18	2,045.13	951.39
Savings goal (US$)	1,857.14	661.44	1,820.98	642.67
Effective match rate	50.00	0.00	50.00	0.00
Periodic savings goal (US$)	1.69	0.47	1.62	0.49
N study participants	49		45	

TABLE 2C—SUMMARY STATISTICS OF LOTTERY INCENTIVE EXPERIMENT

| | Control Group | | Treatment Group | |
	Mean	Standard Deviation	Mean	Standard Deviation
Variable	A. By Number of Deposits			
Deposit amount (US$)	100.87	230.86	97.12	189.59
Made a deposit	0.54	0.50	0.68	0.47
Deposit amount, running total (US$)	950.69	987.08	747.16	697.97
N deposits	794		814	
	B. By Number of Study Participants			
Met goal	0.38	0.49	0.24	0.43
Total saved (US$)	1,954.73	1,078.06	1,820.95	830.04
Made no deposits	0.16	0.37	0.07	0.26
Male	0.31	0.47	0.37	0.49
White	0.64	0.48	0.61	0.49
Married	0.32	0.47	0.33	0.47
College degree	0.36	0.48	0.13	0.34
Full-time employment	0.52	0.51	0.45	0.50
Age	36.75	11.88	40.79	12.68
Number adults in household	2.24	1.23	2.00	1.25
Number children in household	1.18	1.18	1.16	1.24
Household income (US$)	20,680.45	11,602.15	16,911.07	8,656.31
Savings goal (US$)	2,287.56	330.40	2,158.81	287.53
Effective match rate	1.50	0.00	1.59	0.71
Periodic savings goal (US$)	93.62	42.48	48.57	28.12
N study participants	45		42	

TABLE 2D—SUMMARY STATISTICS OF INCREASING MATCH EXPERIMENT

Variable	Control Group		Treatment Group	
	Mean	Standard Deviation	Mean	Standard Deviation
	A. By Number of Deposits			
Deposit amount (US$)	73.05	117.64	72.29	91.30
Made a deposit	0.76	0.43	0.75	0.43
Deposit amount, running total (US$)	608.77	629.87	564.72	511.22
N deposits	1838		1103	
	B. By Number of Study Participants			
Met goal	0.17	0.38	0.14	0.34
Total saved (US$)	1,147.24	806.84	1,076.32	581.15
Made no deposits	0.01	0.09	0.00	0.00
Male	0.22	0.42	0.19	0.39
White	0.68	0.47	0.81	0.39
Married	0.44	0.50	0.35	0.48
College degree	0.36	0.48	0.41	0.49
Full-time employment	0.52	0.50	0.57	0.50
Age	37.53	10.79	37.91	12.37
Number adults in household	1.81	0.95	1.59	0.62
Number children in household	1.91	1.44	1.69	1.45
Household income (US$)	23,035.04	13,503.20	21,148.46	14,451.31
Savings goal (US$)	2,344.21	864.06	2,474.45	826.42
Effective match rate				
Periodic savings goal (US$)	54.85	46.32	61.04	47.47
N study participants	117		74	

TABLE 3A—REGRESSION RESULTS, REMINDER AND ACCOUNTABILITY EXPERIMENT

	Final Savings Position Outcomes				Progress in Program Outcomes		
					Made a deposit	Deposit amount	Running total
Variable	Total Deposit		Met Goal				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reminder Calls	67.248	1.225	-0.004	-0.034	0.059	-6.391	-137.569
	(143.959)	(181.837)	(0.048)	(0.066)	(0.048)	(21.141)	(106.067)
Accountability Calls	-126.505	-150.321	-0.078*	-0.052	-0.089**	-31.839*	-187.380+
	(161.441)	(206.409)	(0.044)	(0.062)	(0.044)	(19.308)	(117.624)
Months in program					-0.010***	-2.262*	17.665**
					(0.003)	(1.218)	(7.286)
Reminder*Months					0.003	0.344	23.598*
					(0.004)	(1.572)	(14.108)
Accountability*Months					0.004	3.155+	13.229
					(0.004)	(2.064)	(21.294)
Control group mean	765	806	0.24	0.29	0.52	59	449
N	488	300	488	300	5738	5738	5738
R-squared	0.029	0.163	0.018	0.080	0.040	0.011	0.145
Demographic Controls	No	Yes	No	Yes	No	No	No

SOURCE - Authors' calculations using data collected throughout experiment; NOTE - All regressions estimated using OLS or linear probability models. Robust standard errors (clustered at the participant level in the progress in program regressions) are reported in parentheses below coefficient estimates. The Reminder variable is an indicator that equals 1 for participants who received the reminder calls, and the Accountability variable equals 1 for participants who received the second call. The models without controls were estimated using only the Treatment indicators, and site identifiers. The Control models included controls for gender, race, age, marital status, education, work status, family size, household income and saving goal; Significance levels: + $p<0.15$; * $p<0.10$; ** $p<0.05$; *** $p<0.01$

TABLE 3B—REGRESSION RESULTS, INCREASED FREQUENCY EXPERIMENT

Variable	Final Savings Position Outcomes				Progress in Program Outcomes		
	Total Deposit		Met Goal		Made a deposit	Deposit amount	Running total
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-29.080	-46.080	-0.032	-0.049	-0.025	-6.991	-160.751*
	(114.656)	(135.590)	(0.036)	(0.034)	(0.071)	(12.820)	(81.339)
Months in program					-0.013***	-0.315	15.969+
					(0.003)	(0.898)	(9.827)
Treatment*Months					0.010	2.090	20.790+
					(0.007)	(1.544)	(13.511)
Control group mean	765	806	0.05	0.06	0.54	59	449
N	110	94	110	94	1390	1390	1390
R-squared	0.007	0.075	0.034	0.178	0.064	0.006	0.202
Demographic Controls	No	Yes	No	Yes	No	No	No

SOURCE - Authors' calculations using data collected throughout experiment; NOTE - All regressions estimated using OLS or linear probability models. Robust standard errors (clustered at the participant level in the progress in program regressions) are reported in parentheses below coefficient estimates. The Treatment variable is an indicator that equals 1 for participants who received the experimental treatment. The models without controls were estimated using only the Treatment indicators, and site identifiers. The Control models included controls for gender, race, age, marital status, education, work status, family size, household income and saving goal; Significance levels: + $p<0.15$; * $p<0.10$; ** $p<0.05$; *** $p<0.01$

TABLE 3C—REGRESSION RESULTS, LOTTERY INCENTIVE EXPERIMENT

| Variable | Final Savings Position Outcomes | | | | Progress in Program Outcomes | | |
| | Total Deposit | | Met Goal | | Made a deposit | Deposit amount | Running total |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-133.775	-117.295	-0.140	0.013	0.081	-6.671	-110.790
	(216.841)	(313.207)	(0.099)	(0.148)	(0.075)	(20.805)	(121.633)
Months in program					-0.007**	1.222	47.673***
					(0.003)	(1.100)	(7.805)
Treatment*Months					0.005	0.191	-8.608
					(0.005)	(1.366)	(10.309)
Control group mean	2131	2287	0.49	0.47	0.54	101	1089
N	79	56	87	60	1608	1608	1608
R-squared	0.005	0.176	0.043	0.154	0.033	0.004	0.343
Demographic Controls	No	Yes	No	Yes	No	No	No

SOURCE - Authors' calculations using data collected throughout experiment; NOTE - All regressions estimated using OLS or linear probability models. Robust standard errors (clustered at the participant level in the progress in program regressions) are reported in parentheses below coefficient estimates. The Treatment variable is an indicator that equals 1 for participants who received the experimental treatment. The models without controls were estimated using only the Treatment indicators, and site identifiers. The Control models included controls for gender, race, age, marital status, education, work status, family size, household income and saving goal; Significance levels: + $p<0.15$; * $p<0.10$; ** $p<0.05$; *** $p<0.01$

TABLE 3D—REGRESSION RESULTS, INCREASING MATCH EXPERIMENT

Variable	Final Savings Position Outcomes				Progress in Program Outcomes		
	Total Deposit		Met Goal		Made a deposit	Deposit amount	Running total
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment	-70.921	-114.638	-0.036	-0.010	-0.087**	-1.157	-17.560
	(100.622)	(93.306)	(0.053)	(0.047)	(0.043)	(8.024)	(42.343)
Months in program					0.013***	1.559**	66.468***
					(0.003)	(0.622)	(6.828)
Treatment*Months					0.012***	0.221	3.830
					(0.004)	(0.857)	(8.464)
Control group mean	1150	1150	0.18	0.18	0.76	73	608
N	191	191	191	191	2941	2941	2941
R-squared	0.002	0.206	0.003	0.331	0.008	0.060	0.468
Demographic Controls	No	Yes	No	Yes	No	No	No

SOURCE - Authors' calculations using data collected throughout experiment; NOTE - All regressions estimated using OLS or linear probability models. Robust standard errors (clustered at the participant level in the progress in program regressions) are reported in parentheses below coefficient estimates. The Treatment variable is an indicator that equals 1 for participants who received the experimental treatment. The models without controls were estimated using only the Treatment indicators, and site identifiers. The Control models included controls for gender, race, age, marital status, education, work status, family size, household income and saving goal; Significance levels: + $p<0.15$; * $p<0.10$; ** $p<0.05$; *** $p<0.01$

FIGURE 1. DESCRIPTIVE RESULTS COMPARING SAVINGS OUTCOMES OF TREATED AND CONTROL PARTICIPANTS, REMINDER AND ACCOUNTABILITY EXPERIMENT

Fig. 1a. Final savings positions

Fig 1b. Progress in program

FIGURE 2. DESCRIPTIVE RESULTS COMPARING SAVINGS OUTCOMES OF TREATED AND CONTROL PARTICIPANTS, INCREASED FREQUENCY EXPERIMENT

Fig. 2a. Final savings positions

Fig. 2b. Progress in program

FIGURE 3. DESCRIPTIVE RESULTS COMPARING SAVINGS OUTCOMES OF TREATED AND CONTROL PARTICIPANTS, LOTTERY INCENTIVE EXPERIMENT
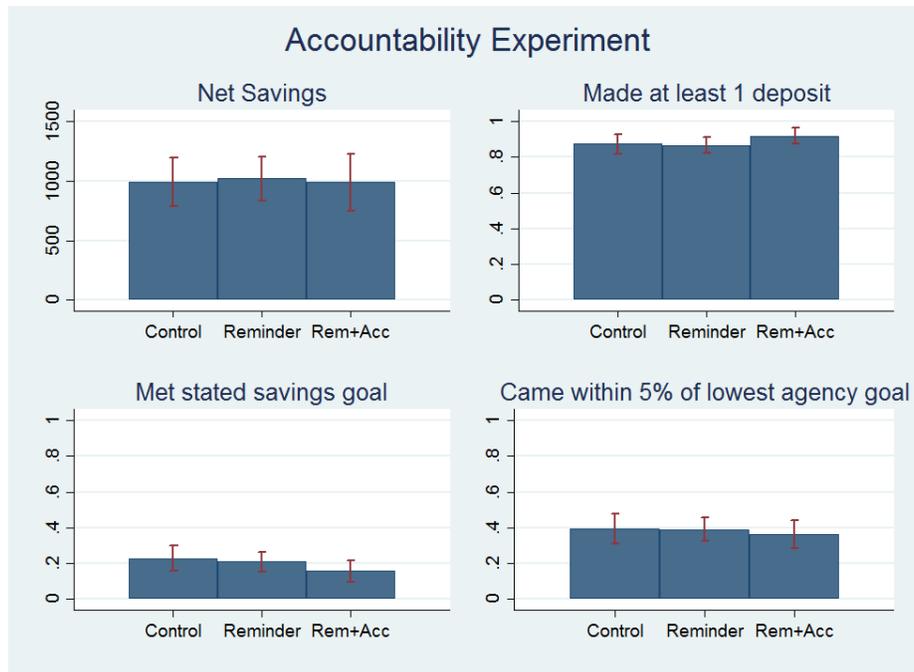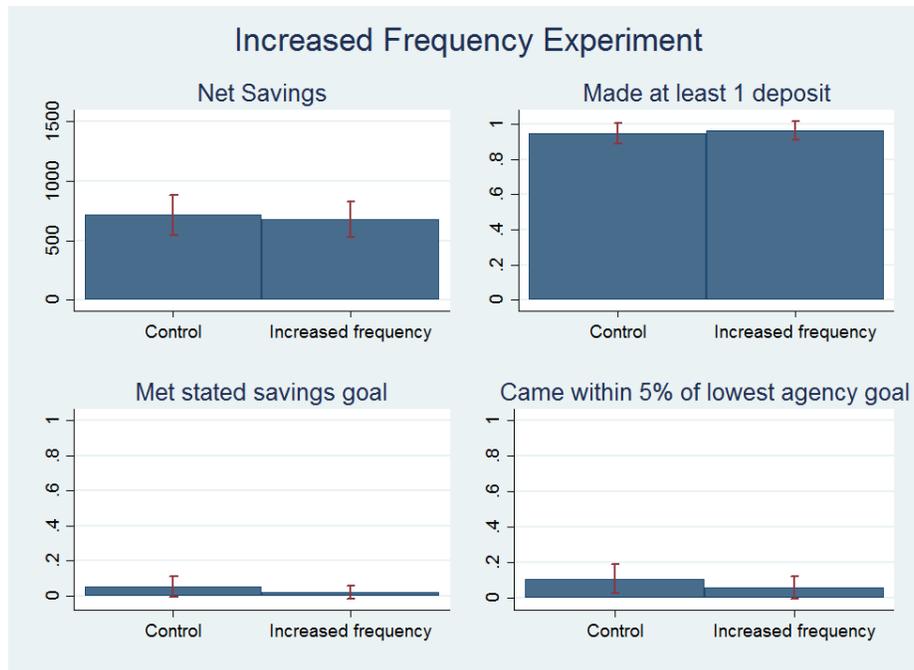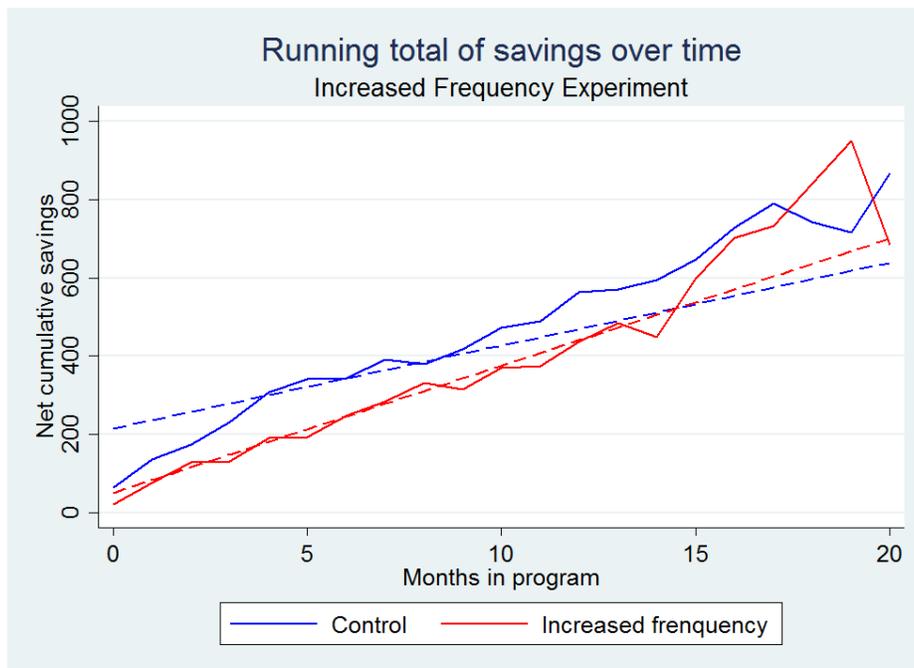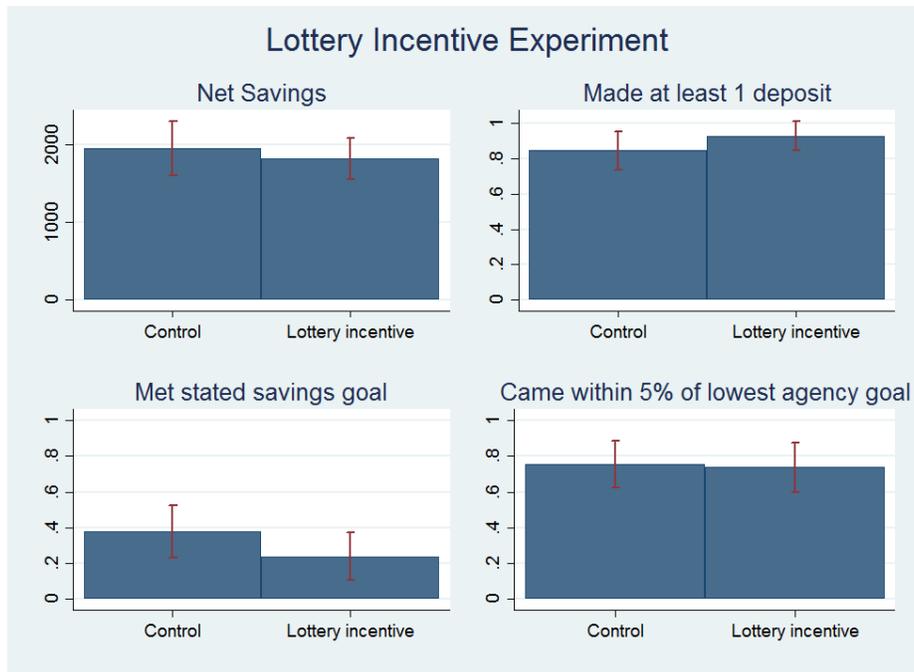

Fig. 3a. Final savings positions
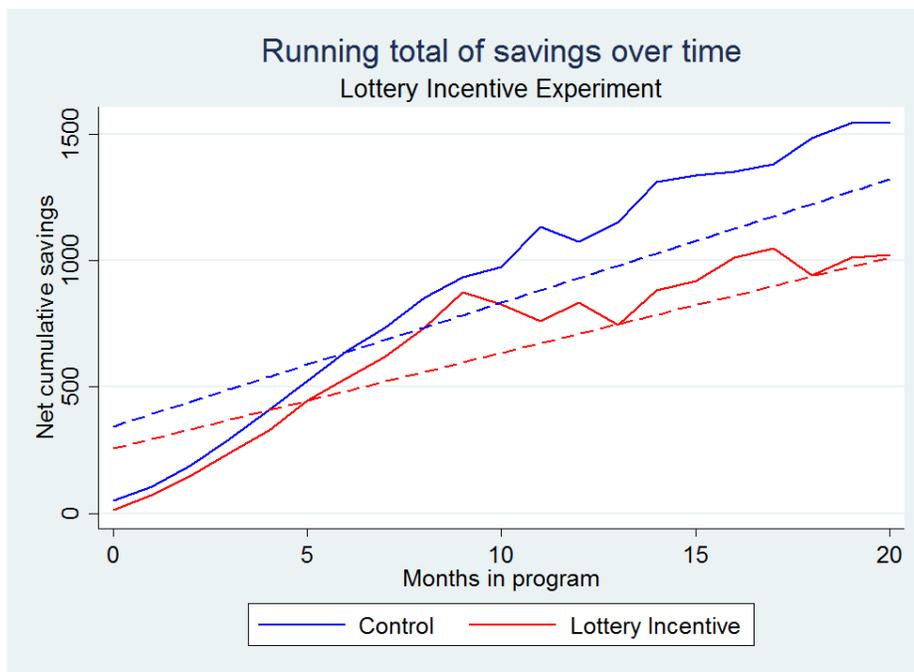
Fig. 3b. Progress in program

FIGURE 4. DESCRIPTIVE RESULTS COMPARING SAVINGS OUTCOMES OF TREATED
AND CONTROL PARTICIPANTS, INCREASING MATCH EXPERIMENT
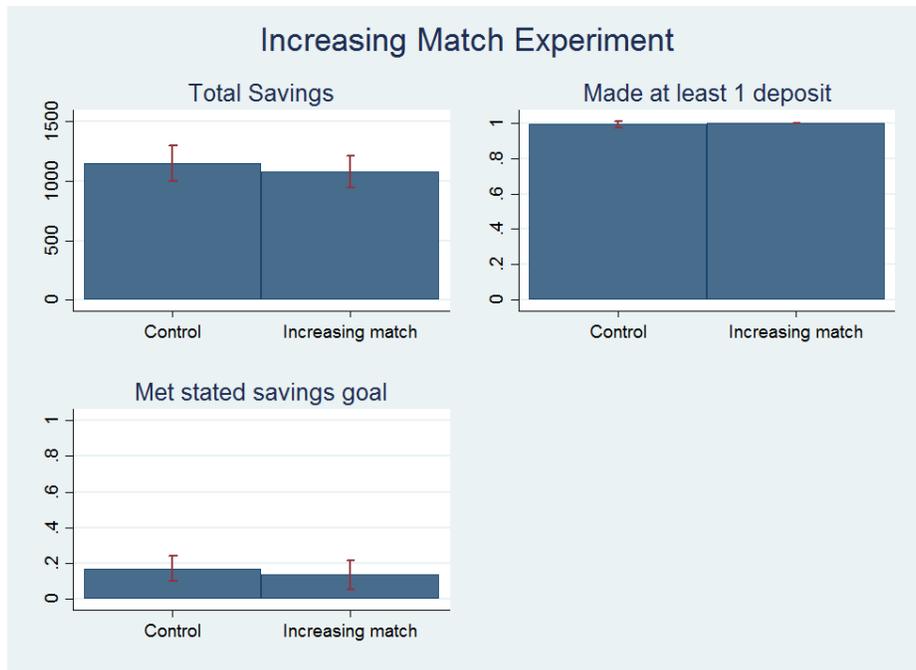
Fig. 4a. Final savings positions

Fig. 4b. Progress in program

APPENDIX

TABLE A1—SUMMARY OF EXPERIMENTS

	"Accountability"				"Frequency of deposit"		"Lottery"	"Increasing match"
	District of Columbia	**Texas**	**Kentucky**	**Michigan**	**Missouri**	**Connecticut**	**California**	**Oregon**
AFIA federally funded match	yes	yes	yes	yes	yes	no	yes	yes
Min IDA lengths	6 months	6 months	6 months	6 months	6 months	6 months	6 months	6 months
Max IDA lengths	36 months	48 months	36 months	open	open	36 months	78/52/39 months	12/24/26 months
Client management software	Outcome Tracker IDA	Outcome Tracker IDA	AFI2, Excel	Outcome Tracker IDA	Outcome Tracker IDA	Custom-built	AFI2, Excel	Outcome Tracker IDA
Allowable asset purchases**	HO, PSE, ME	HO, PSE	HO, PSE, ME	HO, PSE, ME	HO, PSE, ME, HR	HO, PSE, ME, LD, VE, DC	HO, PSE, ME	HO, PSE, ME, HR, WFD
Fringe economy survey	pre	pre	pre (short form)	pre	pre	no	pre & post	no
Feedback phone survey	yes	yes	no	no	yes	no	yes	no

**HO=home purchase, PSE=post-secondary education, ME=micro-enterprise development, HR=home rehabilitation/repair, WFD=workforce development, LD= Lease Deposit on an Apartment, VE=Vehicle, if needed for Employment, DC=Dependent Child's Post-Secondary Education

TABLE A2—TEXT OF REMINDER AND ACCOUNTABILITY CALLS

Text of reminder calls:	
	"Hello, this is [name of non-profit agency] calling. We're calling to remind you that it's time to make a deposit into your Individual Development Account. Make your deposit before the end of the month to be one step closer to your asset goal! Thank you, and keep saving!"

Text of accountability calls:	
	"Hello. This is [name of non-profit agency] calling. We're calling to find out if you made a deposit into your individual development account. Please take a minute to report your most recent deposit. Did you manage make a deposit this week?"

Yes, I did make my deposit.
"Congratulations! Please tell us how much you deposited. Say the currency amount including the decimal portion, or enter it in with your touch-tone keypad, using the star key for a decimal point." User keys in dollar amount.

No, I didn't make my deposit.
"In a brief message, please tell us why you didn't make your deposit. Begin your recording after the tone. To finish, press any number." User leaves a message explaining missed deposit.

"Thank you for taking the time to report your deposits. Keep up the good work!" |

TABLE A3—BASELINE CHARACTERISTICS OF TREATMENT AND CONTROL GROUP PARTICIPANTS IN THE ACCOUNTABILITY CONDITION, <u>DISTRICT OF COLUMBIA</u> SAMPLE

	"Account-ability"	"Account-ability"	"Account-ability"	"Account-ability"	"Account-ability"	"Account-ability"	"Account-ability"
	District of Columbia	District of Columbia	District of Columbia	District of Columbia	District of Columbia	District of Columbia	District of Columbia
	Control group	Treatment group 1: Reminder call only	Treatment group 2: Reminder and accountability call	p-value: Control vs T1	p-value: Control vs T2	p-value: T1 vs T2	N Control / T1 / T2
N total							43/30/32
Percent male	0.395	0.300	0.187	0.463	0.076*	0.379	43/30/32
Percent white	0.209	0.103	0.062	0.338	0.103	0.662	43/29/32
Percent African-American	0.698	0.897	0.938	0.081*	0.017**	0.662	43/29/32
Percent Hispanic	0.163	0.069	0.031	0.297	0.127	0.600	43/29/32
Percent married, excluding living with partner	0.190	0.166	0.156	0.799	0.766	0.913	42/30/32
Percent college degree, including two-year associate degrees, excluding vocational degrees	0.487	0.500	0.700	0.917	0.090*	0.187	39/30/30
Percent full-time employed	0.500	0.678	0.677	0.204	0.214	0.993	36/28/31
Average age at enrollment in IDA	30.558	30.700	29.468	0.956	0.669	0.608	43/30/32
Average number of adults in household	No data	No data	No data	---	---	---	---
Average number children	1.433	1.500	1.956	0.868	0.274	0.407	30/20/23
Average monthly/annual household income	$24,980	$26,398	$26,885	0.649	0.517	0.878	43/30/32

Table A3 continued	Control group	Treatment group 1: Reminder call only	Treatment group 2: Reminder and accountability call	p-value: Control vs T1	p-value: Control vs T2	p-value: T1 vs T2	N Control / T1 / T2
Percent IDA closed (vs open) at end of research	0.511	0.600	0.406	0.484	0.483	0.204	43/30/32
Percent graduated from IDA at end of research	0.038	0.208	0.222	0.093*	0.100	0.904	26/24/27
Percent dropped out from IDA at end of research	0.154	0.292	0.074	0.314	0.420	0.066*	26/24/27
Average months since IDA opened to end of program/research	26.907	26.700	31.062	0.943	0.138	0.152	43/30/32
Average sum of IDA deposits at end of research, since IDA opened	$1,407	$1,382	$1,194	0.942	0.521	0.388	43/30/32
Average total savings goal	$1,418	$1,383	$1,390	0.836	0.865	0.969	43/30/32
Average monthly savings goal	24.302	27.166	26.562	0.321	0.424	0.838	43/30/32
Average goal of purchasing/renovating a home (vs Microenterprise or post-sec. education)	0.279	0.333	0.500	0.796	0.058*	0.208	43/30/32
Average goal of post-sec. education (vs home ownership or post-sec. education)	0.604	0.433	0.375	0.162	0.063*	0.796	43/30/32
Average match rate	3.000	3.000	3.000	---	---	---	43/30/32

Note: Significance levels: * p<0.10; ** p<0.05; *** p<0.01

TABLE A4—BASELINE CHARACTERISTICS OF TREATMENT AND CONTROL GROUP PARTICIPANTS IN THE ACCOUNTABILITY CONDITION, <u>TEXAS</u> SAMPLE

	"Account-ability" Texas Control group	"Account-ability" Texas Treatment group 1: Reminder call only	"Account-ability" Texas Treatment group 2: Reminder and accountability call	"Account-ability" Texas p-value: Control vs T1	"Account-ability" Texas p-value: Control vs T2	"Account-ability" Texas p-value: T1 vs T2	"Account-ability" Texas N Control / T1 / T2
N total							85/124/103
Percent male	0.262	0.224	0.347	0.605	0.253	0.061*	80/107/95
Percent white	0.438	0.364	0.416	0.472	0.859	0.611	57/74/72
Percent African-American	0.509	0.500	0.521	0.923	0.889	0.865	57/66/71
Percent Hispanic	0.316	0.288	0.380	0.844	0.463	0.281	57/66/71
Percent married, excluding living with partner	0.245	0.208	0.314	0.674	0.433	0.182	57/72/70
Percent college degree, including two-year associate degrees, excluding vocational degrees	0.270	0.338	0.291	0.362	0.871	0.476	85/124/103
Percent full-time employed	0.858	0.878	0.881	0.681	0.667	0.943	85/123/101
Average age at enrollment in IDA	35.203	37.684	36.728	0.109	0.363	0.537	54/73/70
Average number of adults in household	1.611	1.685	1.553	0.538	0.575	0.194	85/124/103
Average number children	1.694	1.524	1.718	0.331	0.897	0.234	85/124/103
Average monthly/annual household income	19,975	21,056	19,887	0.538	0.956	0.491	72/113/84

Table A4 continued	Control group	Treatment group 1: Reminder call only	Treatment group 2: Reminder and accountability call	p-value: Control vs T1	p-value: Control vs T2	p-value: T1 vs T2	N Control / T1 / T2
Percent IDA closed (vs open) at end of research	0.117	0.153	0.087	0.544	0.628	0.158	85/124/103
Percent graduated from IDA at end of research	0.083	0.082	0.059	0.972	0.572	0.607	84/122/102
Percent dropped out from IDA at end of research	0.024	0.057	0.020	0.315	0.844	0.187	84/122/102
Average months since IDA opened to end of program/research	17.023	17.838	18.097	0.522	0.271	0.829	85/124/103
Average sum of IDA deposits at end of research	1,133	1,228	1,160	0.663	0.910	0.760	85/124/103
Average total savings goal	2,000	1,987	2,000	0.267	---	0.221	85/124/103
Average monthly savings goal	$35	$35	$35	---	---	---	85/124/103
Average goal of purchasing/renovating a home (vs Microenterprise or post-sec. education)	0.905	0.887	0.854	0.819	0.372	0.550	85/124/103
Average goal of post-sec. education (vs home ownership or post-sec. education)	0.082	0.112	0.145	0.640	0.254	0.550	85/124/103
Average match rate	2.000	2.000	2.000	---	---		85/124/103

Note: Significance levels: * p<0.10; ** p<0.05; *** p<0.01

TABLE A5—BASELINE CHARACTERISTICS OF TREATMENT AND CONTROL GROUP PARTICIPANTS IN THE ACCOUNTABILITY CONDITION, <u>KENTUCKY</u> SAMPLE

	"Account-ability" Kentucky Control group	"Account-ability" Kentucky Treatment group: Reminder call only	"Account-ability" Kentucky p-value	"Account-ability" Kentucky N control / treatment
N				14/43
Percent male	0.142	0.209	0.714	14/43
Percent white	0.889	0.878	0.930	9/41
Percent African-American	0.111	0.098	0.902	9/41
Percent Hispanic	0.000	0.049	0.499	9/41
Percent married, excluding living with partner	0.142	0.205	0.710	7/34
Percent college degree, including two-year associate degrees, excluding vocational degrees	0.571	0.457	0.691	7/35
Percent full-time employed	0.428	0.527	0.698	7/36
Average age at enrollment in IDA	36.700	34.023	0.505	10/42
Average number of adults in household	1.142	1.714	0.242	7/35
Average number children	2.000	1.000*	0.083	7/35
Average monthly/annual household income	$14,807	$14,633	0.963	6/34
Percent IDA closed (vs open) at end of research	0.928	0.883	0.642	14/43
Percent graduated from IDA at end of research	0.545	0.500	0.789	11/40
Percent dropped out from IDA at end of research	0.364	0.325	0.315	11/40
Average months since IDA opened to end of program/research	10.454	9.404	0.664	11/42
Average sum of IDA deposits at end of research	$713	$547	0.520	14/43
Average total savings goal	$2,000	$1,448**	0.036	8/39
Average monthly savings goal	$20	$20	---	14/43
Average goal of purchasing/renovating a home (vs Microenterprise or post-sec. education)	0.750	0.350*	0.053	8/40
Average goal of post-sec. education (vs home ownership or post-sec. education)	0.000	0.275	0.170	8/40
Average match rate	1.750	3.950	0.041	8/40

Note: Significance levels: * p<0.10; ** p<0.05; *** p<0.01

TABLE A6—BASELINE CHARACTERISTICS OF TREATMENT AND CONTROL GROUP
PARTICIPANTS IN THE ACCOUNTABILITY CONDITION, <u>MICHIGAN</u> SAMPLE

	"Accountability" Michigan Treatment group 1: Reminder call only	"Accountability" Michigan Treatment group 2: Reminder and accountability call	"Accountability" Michigan p-value: T1 vs T2	"Accountability" Michigan N T1 / T2
N total				13/12
Percent male	0.153	0.333	0.378	13/12
Percent white	0.384	0.333	0.800	13/12
Percent African-American	0.583	0.727	0.667	12/11
Percent Hispanic	0.250	0.091	0.590	12/11
Percent married, excluding living with partner	0.153	0.250	0.645	13/12
Percent college degree, including two-year associate degrees, excluding vocational degrees	0.230	0.333	0.673	13/12
Percent full-time employed	0.615	0.750	0.673	13/12
Average age at enrollment in IDA	39.461	36.666	0.476	13/12
Average number of adults in household	1.538	1.416	0.562	13/12
Average number children	1.769	1.333	0.307	13/12
Average monthly/annual household income	$23,694	$15,312	0.028**	13/12
Percent IDA closed (vs open) at end of research	0.307	0.166	0.645	13/12
Percent graduated from IDA at end of research	0.167	0.091	0.590	12/11
Percent dropped out from IDA at end of research	0.167	0.091	0.590	12/11
Average months since IDA opened to end of program/research	14.307	18.333	0.185	13/12
Average sum of IDA deposits at end of research	$909.558	$859.820	0.872	13/12
Average total savings goal	$1,000	$1,000	---	13/12
Average monthly savings goal	$20	$20	---	13/12

Table A6 continued	Treatment group 1: Reminder call only	Treatment group 2: Reminder and accountability call	p-value: T1 vs T2	N T1 / T2
Average goal of purchasing/renovating a home (vs Microenterprise or post-sec. education)	0.923	0.833	0.593	13/12
Average goal of post-sec. education (vs home ownership or post-sec. education)	0.000	0.000	---	13/12
Average match rate	2.923	2.833	0.511	13/12

Note: We do not have a no-call condition at the Michigan site; Significance levels: * p<0.10; ** p<0.05; *** p<0.01

TABLE A7—BASELINE CHARACTERISTICS OF TREATMENT AND CONTROL GROUP PARTICIPANTS IN THE FREQUENCY-OF-DEPOSIT CONDITION, <u>MISSOURI</u> SAMPLE

	"Frequency" Missouri Control	"Frequency" Missouri Treatment	"Frequency" Missouri p-value	"Frequency" Missouri N control/treatment
N total				21/24
Percent male	0.142	0.083	0.652	21/24
Percent white	0.238	0.125	0.443	21/24
Percent African-American	0.750	0.833	0.710	20/24
Percent Hispanic	Not available	Not available	---	---
Percent married, excluding living with partner	0.000	0.041	0.367	20/24
Percent college degree, including two-year associate degrees, excluding vocational degrees	0.250	0.416	0.342	20/24
Percent full-time employed	0.666	0.708	0.770	21/24
Average age at enrollment in IDA	36.500	37.958	0.725	20/24
Average number of adults in household	1.428	1.333	0.586	21/24
Average number children	0.950	1.666*	0.066	20/24
Average monthly/annual household income	$1,349	$1,923**	0.049	20/22
Percent IDA closed (vs open) at end of research	0.714	0.666	0.738	21/24
Percent graduated from IDA at end of research	0.450	0.416	0.824	20/24
Percent dropped out from IDA at end of research	0.250	0.250	---	20/24
Average months since IDA opened to end of program/research	32.857	38.083*	0.093	21/24
Average sum of IDA deposits at end of research	$1,511	$1,863	0.284	21/24
Average total savings goal	$1,500	$1,500	--	21/24
Average monthly savings goal	$50	$50	--	21/24
Average goal of purchasing/renovating a home (vs Microenterprise or post-sec. education)	0.476	0.583	0.556	21/24
Average goal of post-sec. education (vs home ownership or post-sec. education)	0.380	0.208	0.323	21/24
Average match rate	1.809	1.708	0.442	21/24

Note: Significance levels: * p<0.10; ** p<0.05; *** p<0.01

TABLE A8—BASELINE CHARACTERISTICS OF TREATMENT AND CONTROL GROUP PARTICIPANTS IN THE FREQUENCY-OF-DEPOSIT CONDITION, <u>CONNECTICUT</u> SAMPLE

	"Frequency" Connecticut	"Frequency" Connecticut	"Frequency" Connecticut	"Frequency" Connecticut
	Control	Treatment	p-value	N control/treatment
N total				36/29
Percent male	0.194	0.103	0.491	36/29
Percent white	0.194	0.068	0.172	36/29
Percent African-American	0.583	0.586	0.981	36/29
Percent Hispanic	0.194	0.310	0.387	36/29
Percent married, excluding living with partner	0.055	0.068	0.826	36/29
Percent college degree, including two-year associate degrees, excluding vocational degrees	0.166	0.275	0.367	36/29
Percent full-time employed	0.657	0.807	0.254	35/26
Average age at enrollment in IDA	37.515	36.769	0.789	33/26
Average number of adults in household	1.250	1.275	0.878	36/29
Average number children	0.861	0.862	0.996	36/29
Average monthly/annual household income	$1,968	$2,016	0.847	36/29
Percent IDA closed (vs open) at end of research	0.500	0.620	0.338	36/29
Percent graduated from IDA at end of research	0.242	0.153	0.521	33/26
Percent dropped out from IDA at end of research	0.212	0.423*	0.096	33/26
Average months since IDA opened to end of program/research	15.454	14.115	0.446	33/26
Average sum of IDA deposits at end of research, since enrollment	$863	$744	0.428	36/29
Average total savings goal	$1,986	$2,032	0.806	36/29
Average monthly savings goal	$50	$50	--	36/29
Average goal of purchasing/renovating a home (vs Microenterprise or post-sec. education)	0.500	0.586	0.618	36/29
Average goal of post-sec. education (vs home ownership or post-sec. education)	0.138	0.172	0.742	36/29
Average match rate	1.666	1.620	0.705	36/29

Note: Significance levels: * p<0.10; ** p<0.05; *** p<0.01

TABLE A9—BASELINE CHARACTERISTICS OF TREATMENT AND CONTROL GROUP PARTICIPANTS IN THE LOTTERY CONDITION, <u>CALIFORNIA</u> SAMPLE

	"Lottery" CFRC Control group	"Lottery" CFRC Treatment group	"Lottery" CFRC p-value	"Lottery" CFRC N control/treatment
N total				45/42
Percent male	0.311	0.366	0.592	45/41
Percent white	0.644	0.610	0.740	45/41
Percent African American	0.494	0.506	0.808	39/40
Percent Hispanic	0.622	0.512	0.303	45/41
Percent married	0.317	0.325	0.939	41/40
Percent college degree incl. 2yr. degrees	0.357	0.128**	0.017	42/39
Percent full-time employed	0.524	0.450	0.504	42/40
Average age at enrollment in IDA	36.749	40.786	0.131	45/41
Average number of adults in household	2.243	2.000	0.400	37/38
Average number children	1.184	1.162	0.937	38/37
Average annual household income	$20,680	$16,911	0.142	39/37
Percent IDA closed (vs open) at end of research	1.000	1.000	--	45/42
Percent graduated from IDA at end of research	0.622	0.738	0.262	45/42
Percent dropped out from IDA at end of research	0.377	0.261	0.262	45/42
Average months since IDA opened	21.824	21.840	0.995	45/42
Average IDA deposits (across rows)	$1,932	$1,937	0.984	45/42
Average total savings goal	$2,287	$2,158*	0.057	45/42
Average monthly savings goal	$93.62	$97.14	0.742	45/42
Average goal of purchasing a home (vs Microenterprise or post-sec. education)	0.432	0.200**	0.023	44/40
Average goal of post-sec. education (vs home ownership or post-sec. education)	0.159	0.225	0.442	44/40
Average match rate	1.500	1.592	0.386	45/42

Note: Significance levels: * p<0.10; ** p<0.05; *** p<0.01

TABLE A10—BASELINE CHARACTERISTICS OF TREATMENT AND CONTROL GROUP PARTICIPANTS IN THE MATCH-INCREASE CONDITION, <u>OREGON</u> SAMPLE

	"Increasing match"	"Increasing match"	"Increasing match"	"Increasing match"
	Oregon	**Oregon**	**Oregon**	**Oregon**
	Control group	Treatment group	p-value	N control / treatment
N total				119/74
Percent male	0.218	0.189	0.625	119/74
Percent white	0.689	0.811*	0.062	119/74
Percent African-American	0.142	0.000***	0.000	119/74
Percent Hispanic	0.151	0.229	0.183	119/74
Percent married	0.429	0.351	0.287	119/74
Percent college degree incl. 2yr. degrees	0.353	0.405	0.464	119/74
Percent full-time employed	0.513	0.568	0.457	119/74
Average age at enrollment in IDA	37.203	37.911	0.679	119/74
Average number of adults in household	1.815	1.595*	0.074	119/74
Average number children	1.899	1.689	0.325	119/74
Average annual household income	$23,153	$21,148	0.328	119/74
				119/74
Percent IDA closed (vs open) at end of research	0.311	0.284	0.689	119/74
Percent graduated from IDA at end of research	0.193	0.121	0.235	119/74
Percent dropped out from IDA at end of research	0.117	0.162	0.393	119/74
Average months since IDA opened	16.395	14.119**	0.016	119/74
Average IDA deposits (across rows)	$1,183	$1,094	0.406	118/74
Average total savings goal	$2,355	$2,474	0.343	119/74
Average period/monthly savings goal	$55.339	$61.036	0.410	119/74
Average goal of purchasing a home (vs Microenterprise or post-sec. education)	0.353	0.649***	0.000	119/74
Average goal of post-sec. education (vs home ownership or post-sec. education)	0.361	0.108***	0.000	119/74
Average match rate	3.000	Not available	---	---

Note: Significance levels: * p<0.10; ** p<0.05; *** p<0.01

FIGURE A1. EXAMPLE OF A CONSENT FORM

[Logo of university] **[Name of university]**
[Name of college]
[Name of project director]
Consent document

We are researchers from [name of university]. We are working with [name of IDA program agency] to help improve their IDA program, and we need your help. We're inviting you to participate in a pilot IDA program. If you agree, you'll be given:

1. Two phone calls at every deposit deadline. One call will act as a reminder so that you don't miss your deposit due date. The second call, which will be placed after the deposit deadline, will ask you whether you were able to deposit. Each call will last about 30 seconds.
2. A survey to assess your "financial personality". You will be surveyed once now and again when you finish the IDA program. Each survey will take about 20 minutes to complete.

We would also like for your permission to access the IDA program database for your information, like your name, gender and household income, as well as the frequency and amount of your savings deposits at each time of a deposit. All reasonable efforts will be made to keep your study-related information confidential. However, there may be circumstances where this information must be released, for example, if required by state law. Also, your records may be reviewed by various other university or governmental groups. The results of this study will only be reported as averages. Your individual information and survey responses will not be shared with your IDA case manager.

Your participation is voluntary and refusal to participate will not result in any penalty or loss of benefits. If you choose to participate in the study, you may discontinue participation at any time without penalty or loss of benefits. If you choose to discontinue your participation, it will not affect your future relationships with [name of university] or your IDA agency. By signing this form, you do not give up any personal legal rights you may have as a participant in this study. Participating in this research involves minimal risks, such as accidental release of your information. An Institutional Review Board responsible for human subjects research at [name of university] reviewed this research project and found it to be acceptable, according to applicable state and federal regulations and University policies designed to protect the rights and welfare of participants in research.

A copy of this consent document is available to you for your records if you so choose.

BY SIGNING BELOW, YOU AGREE TO PARTICIPATE IN THIS STUDY.

Name _____   **Signature** _____

                                      Date _____

PLEASE PROVIDE A PHONE NUMBER FOR THE REMINDER CALLS:
(_ _ _) _ _ _ - _ _ _ _

For questions, concerns, or complaints about the study, or if you feel you have been harmed by participation you may contact [contact information of project director].

For questions about your rights as a participant in this study or to discuss other study-related concerns or complaints with someone who is not part of the research team, you may contact [contact information of IRB at university].