Effects of Monitoring on Mortgage Delinquency: Evidence from a Randomized Field Study

J. Michael Collins[*], Cäzilia Loibl[†], Stephanie Moulton [‡], Anya Samak [§]

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Abstract

The purchase of a home is the starting point of a series of consumer decisions that require managing current consumption while planning for future expenses and preparing for negative financial shocks. Mistakes can be costly; missed mortgage payments can place the consumer at risk of mortgage default. Through a randomized field experiment with first-time homebuyers, we test the impact of monitoring on debt repayment. A treatment offer of free 'telephone financial coaching' at quarterly intervals for one year after purchase significantly influenced financial behavior, including lowering mortgage default rates among borrowers with subprime credit histories (credit scores below 680). These results suggest that relatively low-cost procedures embedded into loan servicing may increase adherence to timely repayments, thereby reducing the probability of default.

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Note: Authors Listed in Alphabetical Order

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[*]University of Wisconsin-Madison
[†]Department of Consumer Sciences, The Ohio State University
[‡]John Glenn School of Public Affairs, The Ohio State University
[§]University of Wisconsin-Madison

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1 Introduction

The rapid rise of mortgage defaults in 2008 proceeding a boom in low-income first-time homeownership calls into question the long-term sustainability of offering mortgages to riskier borrowers. Mistakes made are costly at the household and community level; missed mortgage payments can place the homeowner at risk of mortgage default, with profound negative impacts for the consumer, the housing market and the economy at large. Regulatory changes, such as those included under the Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010 (Pub.L. 111203), specifically seek to limit risky mortgage characteristics that have been associated with higher rates of default. However, there is concern that certain households may be disproportionately disadvantaged by such policy changes and eliminated from portions of the credit market entirely (Quercia et al. 2012). If continuing to offer loans to higher risk borrowers is a policy goal, effective strategies to offset the potentially higher default risk of such mortgages will become a critical, yet challenging, objective.

First time homebuyers typically use a highly leveraged loan to be able to purchase a home. Being younger and lower income, these households may have less experience managing their finances and lower levels of wealth to deal with unexpected expenses such as home repairs and property taxes, or financial shocks such as a loss of income or illness. Home equity is typically too illiquid for such shortfalls, especially within the first few years after the purchase of a home with a mortgage. New homebuyers also have strong demand for housing related goods and services after moving in, and such spending might derail household budgets in the first year after buying a home. Households also are likely suffer from common behavioral biases, including myopic decision frames, procrastination, and/or difficulties with self-regulation which result in less than optimal money management behaviors.

These biases may present a substantial problem. Up to 1 in 10 new mortgage borrowers is seriously delinquent in the first year of their new loan (Anderson and Dokko 2010). Consumer Expenditure Survey data show that the median household shifts 5% of annual income to household durable goods, home-related consumption and home maintenance/improvement services (Siniavskaia 2008). Self control and attention problems may underlie early payment defaults as borrowers overspend on goods and services for the home and cannot make timely mortgage payments. Borrowers may also not be experienced in making regular loan payments or underestimate the potential for unexpected budget shortfalls. For example, Anderson and Dokko (2010) show that liquidity problems related to paying property tax bills contribute to defaulting on mortgages due to borrowers being unprepared for large tax payments.

We test a program aimed at increasing attention to mortgage payments among first time homebuyers. Based on a 2011-2012 randomized field experiment we estimate treatment effects in the order of a 10 percentage point reduction in cumulative ('ever') default rates within the first year of owning a home among subprime borrowers, relative to an average default rate of 15%.[1] Effects are primarily for borrowers with lower credit scores (below 680), who may have less established histories of timely debt repayment. Estimates hold up to a variety of identification tests. Much of the effect seems to be related to the use of automated payments and a tendency toward more savings and less revolving (mainly credit card) debt.

This paper begins with a review of prior studies on inattention and biases related to self-regulation failures, followed by a description of the specific field experiment. We then continue with an overview of the methods of analysis, findings and related robustness checks. We conclude with a brief discussion of the policy and practice implications of this field experiment, limitations and suggestions for future research.

2 Loan Repayment and Inattention, Monitoring and Self-regulation

Attention is an increasing focus of behavior modification programs across a number of domains, from health to personal finance. Inattention has been shown to be related to a number of potential biases in markets where consumers are systematically not attentive to product attributes, including fundamental information such as prices (Gabaix and Laibson 2006; Reis 2006). Several studies suggest that even relatively modest interventions can increase the salience of a behavior for consumer financial decisions (Stango and Zinman 2011; Zwane et al. 2011). In fact, one mechanism that may underlie the effects of financial incentives for savings shown in other studies (Duflo and Saez 2003; Mills et al. 2008) could be related to the focusing effects of these programs (in addition to the direct pecuniary effects). Increased salience can potentially draw attention to a particular future goal or opportunity, causing consumers to segregate potential future gains (or losses) into

[1] Purchase dates are staggered but the minimum mortgage ownership time was 350 days; the longest 660 days.

discrete categories (Tversky and Kahneman 1986). The role of interventions used to boost attention have been evaluated in other settings, including health care. For example, patient adherence to prescribed protocols can be enhanced using text messaging reminders (Pop-Eleches et al. 2011; Miloh et al. 2009).

Several studies in household finance focus on how limited attention may create a present bias in intertemporal choices where people are inattentive to future consequences related to savings (Karlan et al. 2010; Karlan and Zinman 2012). Recently this framework has been applied to credit management and debt repayment(Gal and McShane 2012; Karlan and Zinman 2012). Paying a mortgage or spending on current consumption could be considered an example of such an intertemporal choice. The decision requires a consideration of the future consequences of current expenditures paired with the potential of triggering a payment default, as opposed to forgoing current expenditure opportunities and paying down a mortgage in a timely way.

Along with reminders, people may also show improvements in behaviors when provided an external monitor, especially for tasks that require self control. This is related to several constructs in behavioral decision making, including the planning fallacy (people systematically underestimate the time required for tasks) (Buehler et al. 2010) and self-control failures (Fudenberg et al. 2012; Gul and Pesendorfer 2004). Prior work predicts that more self-aware individuals (so called 'sophisticates') may recognize their own limited self-control and reveal demand for constraints or monitoring to enhance their capacity for self-regulation (Karlan et al. 2010).

One way to encourage people to overcome self-control problems is to link people's long-term goals to shorter-run behavioral intentions. Establishing specific implementation intentions can improve the likelihood of goal attainment by establishing links between specific situations and the desired behavioral responses (Brandstatter et al. 2001; Gollwitzer 1999; Baumgartner and Pieters 2008). Goal directed reminders have been associated with increased savings (Karlan et al. 2010; Kast et al. 2012), perhaps due to increased attention or heightened salience effects that overcome procrastination (Loibl and Schraff 2010; Ariely and Wertenbroch 2002).

External monitoring can prove more effective than self-monitoring in terms of adherence to goals, as it increases perceived accountability on four dimensions: (1) expectations of being observed; (2) identifiability; (3) expectations that performance will be assessed, and (4) expectations of the need to give reasons for actions or inactions (Lerner and Tetlock 1999).

The application of external monitoring to financial behaviors is relatively new. One form of an intervention that serves as a reminder, provides external monitoring and connects goals to implementation intentions is financial coaching (Collins and O'Rourke 2012). Coaching has the potential to increase the self-regulation through accountability to goals defined by the client but monitored by the coach.

3 Study Design

'MyMoneyPath'[2] is a program developed in partnership with the Ohio Housing Finance Agency (OHFA), a state agency that issues tax-favored bonds to fund mortgages for qualified borrowers. From June through December of 2011, *all* first time homebuyers purchasing homes through OHFA's First Time Homebuyer Program were required to complete an online financial assessment. After that session a subset of 425 consenting participants were randomly assigned (using a random number generator) to a treatment group (N=293) required to complete an online goal setting module and assigned to receive an offer for no-cost 'telephone financial coaching' at quarterly intervals after purchase. The telephone sessions were provided by a call-center based nonprofit financial counseling organization trained by the study team. Data were collected through the online system, a follow-up survey and from OHFA administrative records.

3.1 'MyMoneyPath'

MyMoneyPath consists of three parts, as shown in Figure 1: (1) an online financial assessment completed immediately prior to home closing; (2) an online financial planning module that allows participants to set self-identified financial goals and implementation intentions; and (3) telephone-based financial coaching offered at quarterly intervals for the first year after home purchase. While all study participants received the online financial assessment, two-thirds of the participants were also assigned to receive the online financial planning module and telephone based monitoring after purchase.

The online financial assessment collected self-report information from participants about their financial behaviors in five areas (budgeting, borrowing, savings, home and retirement), as well as basic demographic and socio-economic information. Questions targeted behaviors, such as having adequate emergency savings, managing personal debt, and investing in longer term financial goals, thought to be associated with the long term well-being of the new homeowner. After completion of the assessment, participants viewed

[2] see: `www.MyMoneyPath.com` and `www.mymoneycheckup.com`

a concise results sheet reporting the status of their financial health in each of the five areas, coded "red" if the area was in need of immediate attention, "yellow" if the area needed some attention, and "green" if the area was not in need of attention. An illustrative screenshot of the online modules is provided in Figure 9 The content of the financial health assessment and coding for the indicators was developed through interviews with industry experts in conjunction with the National Foundation for Credit Counseling (NFCC). Study participants received a $25 gift card incentive at the completion of the online assessment.

After completing the financial assessment, two-thirds of borrowers were assigned to the treatment group, where they were guided through an online, interactive financial goals module. For each of the five areas above, the online module guided participants through a review of their financial assessment, allowing them to visualize how changes in certain financial variables (e.g., amount saved each month) would affect future time periods, and then were guided to identify specific goals and set implementation intentions for the next year. Finally, all treated borrowers received a letter followed by quarterly emails and telephone calls to offer live financial coaching on these goals. Telephone coaching was designed to: (1) focus on financial goals the borrower entered into the online assessment; (2) systematically work with borrowers to refine these goals into actionable steps; and (3) call back to monitor progress towards goals. Treatment in this study is therefore the combination of the online goals module combined with the offer of the telephone-based coaching. Telephone calls were made to all treated borrowers, although only a subset participated. Regardless, the calls at least served as reminders if not enhancing the perception of external monitoring. Control borrowers only participated in the online assessment. The assessment module was required as part of the mortgage application process and all borrowers took part. All estimated treatment effects are therefore relative to this baseline of borrowers to complete an online assessment only.

3.2 Field Setting and Sample

This program was designed in conjunction with the Ohio Housing Finance Agency, a state housing finance agency. These quasi-public corporations exist in most states and play a significant role in promoting mortgages for lower-income first-time homebuyers (Moulton 2012; Moulton and Quercia 2013). On average, 100,000 homebuyers purchase homes using state mortgage programs annually, providing a potentially scalable opportunity for replication (National Council of State Housing Agencies 2011).

This is an ideal setting for a field study in many ways. Because of the subsidized mortgage loan involved, interest rates and loan terms are held constant across homebuyers at any given point in time. Further, while there are multiple lenders originating loans, all loans are sold to the same loan servicing firm within 60 days of closing, holding constant variation in servicing practices. Importantly, data on borrower loan repayment, credit histories and other information is administratively available. All borrowers are required to take part in activities prior to loan closing, allowing for the implementation of the program evaluated in this study.

3.3 Recruitment, Assignment, and Data Collection

Study enrollment occurred during the seven month period between June 1 and December 31, 2011. During the study period, all prospective homebuyers seeking mortgages through the Ohio Housing Finance Agency's homebuyer program completed the online assessment financial assessment prior to home purchase. Upon completion of the assessment, prospective homebuyers were invited to participate in a study following an IRB approved protocol. Homebuyers who consented to participate received a $25 gift card via e-mail. Figure 1 provides a flow-diagram of the enrollment process. Of the 932 homebuyers completing the assessment, approximately two-thirds (574, or 62%) consented to participate in the study, about two-thirds of whom were randomly assigned to the treatment group. At the conclusion of the initial data collection period (June 30, 2012), 488 (85%) of the consenting participants purchased a home, for whom 425 had complete credit-report and mortgage-origination data.[3] Of the 425 participating homebuyers, 295 had been randomly assigned to the treatment group, completed the online goals module and were offered telephone financial coaching at quarterly intervals after home purchase, commencing within two months of their purchase date and culminating in the anniversary month of their purchase. Of the 295 assigned to the treatment group, 107 (36%) took up at least one offer for financial coaching. An additional $25 gift card was provided as an incentive for the first coaching session completed. All treatment group participants continued to receive offers for coaching by phone, email and letter throughout the study period, potentially serving as an external reminder, regardless of take-up. Over the 12 month program borrowers could have received between 9 and 20 contacts through various modes.

[3] 64 homebuyers completed the online assessment but were not offered telephone financial coaching after purchase and are excluded.

4 Data

4.1 Baseline Characteristics

The data for this study was collected from several different sources. Data on participant demographics and verified income was provided by the Ohio Housing Finance Agency at the time of home closing. Credit report data was provided for closed loans within 60-90 days of home closing, and the one year anniversary of the initial credit report date.[4] Data on mortgage loan attributes and performance was provided at the time of closing and monthly thereafter by the Agency (through the servicer). Finally, online financial health assessment data was completed prior to home closing, and again on the one year anniversary of completion (on or before December 31, 2012). Participants were contacted by email and telephone to complete the one-year follow-up financial health assessment; of the 488 contacted, 225 completed the follow-up assessment, for a response rate of 46 percent. Another incentive of a $25 gift card was provided to all participants completing the follow-up assessment.

Table 1 presents summary statistics of dependent variables and Table 2 presents general descriptive statistics for independent variables. Each table shows columns for all borrowers and then for borrowers with credit scores below 680 (n=272). Borrowers with credit scores below 680 are commonly considered "subprime" and may show differential responses to study interventions than higher credit score borrowers, based on prior research. The average age of the primary borrower was 34 years, with a gross monthly household income of $3,772, or about $46,000 per year. About half (48%) of primary borrowers were female, with an average household slide of 2.6. About one in five primary borrowers were either African-American or Hispanic, and about one in four had completed a college degree. From the credit report data, the average credit report score at the time of purchase was 668, with about 20% of borrowers ever late on any trade line in the past 24 months, and a non-housing debt to income ratio of about 15% (minimum monthly revolving and installment debt payments as a percent of monthly income, excluding the mortgage payment). From the self- reported data, the total amount of money in savings and checking accounts at the time of purchase is about $3,000. Further, 8% of respondents reported that they would rather get $40 now than $60 in a month, suggesting only a small number of borrowers indicating a present-biased discount rate.

[4]The follow-up credit report data was collected 12 months after the initial credit report date for 96.5 percent of participants; however, because of constraints from the funder, data on the remaining 3.5 percent was collected 10 to 11 months after the initial report date, on March 15, 2013.

Differences between treatment and control group borrowers at baseline are compared to test for the consistency of the random assignment. Differences in pairwise contrasts of baseline characteristics in Table 2 are not significant, suggesting that the randomization process was valid.

4.2 Mortgage Default and Financial Health

The primary outcome of interest in this analysis is mortgage default. Here we define mortgage default to be equivalent to serious mortgage delinquency, given the short duration of time since purchase. Mortgage default is coded '1' if the borrower was ever 60 or more days late on their mortgage payment as of February 28, 2013, and '0' otherwise.[5] While the primary outcome of interest is mortgage default, a few other measures are explored including reductions in revolving or installment debt balances, increases in savings and use of manual (vs. direct deposit) mortgage payments.

5 Methods

Because of the randomized study design, comparisons of means between treatment and control group participants is the primary specification. However, additional covariates commonly associated with the outcomes are also included as a robustness check.

First, we employ the following equation to estimate average treatment effects for outcome Y for borrower, i:

$$Y_i = \alpha_0 + \beta_1 Treatment_i + \epsilon_i \tag{1}$$

where Y_i is alternately the borrower defaulting on the loan (missing 2 or more payments), credit score, installment debt levels, savings levels and use of automatic payments. Because the treatment was randomly assigned, β_1 provides a causal estimate of the effects of the program on client, i. ϵ_i is a HuberWhite corrected standard error to produce heteroscedasticity-consistent estimates.

A second equation includes a vector of controls in the off-chance that assignment was unbalanced based on observable characteristics of study participants (particularly since consent and attrition may not be random). This specification includes $\boldsymbol{X_i}$ which includes

[5] As of February 28, 2013, the amount of time elapsed since closing was an average of 510 days, or 17 months.

credit score at loan application (the median score of up to 3 collected) which is presented by 5 categorical variables to deal with the non-linear form of credit score measures. The borrower's prior 24 months count of any delinquencies on any payments as measured in the credit report is also included, as is income (measured at loan application) debt-to-income ratio, reported savings and number of days since the borrower took out the mortgage (a crude measure of relative exposure to default risk). Other characteristics include gender, age, college education, minority race and household size. Also included is a measure of time preferences commonly used in surveys, which asks for a choice between \$40 today versus \$60 in a month. This reduced form model produces average treatment effects, β_1, conditional on measured characteristics:

$$Y_i = \alpha_0 + \beta_1 Treatment_i + \boldsymbol{\lambda X_i} + \epsilon_i \tag{2}$$

Dichotomous outcomes are estimated using a probit model with marginal effects coefficients presented. Continuous outcomes are estimated using an OLS model including a control for baseline levels, in effect providing an average change in the outcome associated with treatment assignment. Because we expect the effects of treatment to be stronger for lower-credit quality (greater default risk) we also restrict the sample to approximately 272 (out of 425 total) borrowers with credits scores below 680, a common cutoff for subprime credit quality.

Average treatment effects for all borrowers assigned to treatment, regardless of whether the study participant cooperated with the treatment, is useful as an estimate of overall effects for a pool of loans without the bias introduced from borrowers self-selecting into a program. This is also known as intent to treat (ITT). But since the coaching program is not mandatory and some borrowers will not cooperate, the effect of the coaching treatment on the treated (TOT) may also be of interest. But because borrowers who cooperated may signal other characteristics correlated with outcomes, a simple indicator for 'participant' would not provide unbiased estimates. Instead we use assignment to treatment (ITT) as a predictor for take-up of the treatment (TOT) using a two-stage least squares instrumental variable approach. All IV estimates us a limited information maximum likelihood (LIML) estimator, since this is general more efficient and consistent than 2SLS for smaller sample sizes.

6 Results

Figure 2 shows comparisons of mean default rates overall. For the total sample, 10 percent of borrowers had experienced default, with slightly lower rates for treatment group participants. However, when the sample is limited to borrowers with credit scores below 680, the differences between treatment and control group participants are statistically significant, with 12.5 percent of treatment group participants experiencing mortgage default, compared with 23.6 percent of control group participants (Figure 3).

Figure 4 plots the distribution of credit scores by category (<= 620; 620-650; 650-680; 680-720; and =>720). While there appears to be an increase in borrowers with low credit scores (<620) on the follow-up credit-report, there do not appear to be systematic differences in credit scores for treatment and control group borrowers.

Figure 5 shows that borrowers assigned to the treatment group have slightly lower installment debt balances on their follow-up credit reports. Figure 6 shows the proportion of borrowers in each group for whom revolving debt (typically credit cards) increase by $2,000 or more. The proportion of borrowers who have an increase in revolving debt of $2,000 or more is 25% for treatment group participants, compared with 36% for controls. Treatment group participants are significantly less likely to report making manual mortgage payments on the follow-up assessment (Figure 7), suggesting that they may be utilizing automated payments as a mechanism to reduce their likelihood of mortgage delinquency. Figure 8 shows that a higher proportion of treatment group borrowers report that they are "saving any money" at the time of follow-up.

Overall these comparisons show that treatment is associated with lower default, but primarily among low credit score borrowers. The mechanism that may have produced lowered default is the use of automatic payment of mortgage payments. Borrowers assigned to treatment also report saving more and appear to accumulate slightly less installment debt. Neither is reflected in credit scores in the study period however.

Table 3 begins the average treatment effect estimates for default, displaying marginal effects from the mean. Column 1 shows overall estimates of lower default among those borrowers assigned to treatment, although not at standard levels of significance. Restricting the sample to borrowers with credit scores under 680 in Column 2 produces larger estimates of the effect of treatment on default, and now at the 5% statistical significance level. Adding controls in Columns 3-4 provides consistent estimates. Overall default among lower credit score borrowers is reduced by about 41%—a very large reduction about the same as the coefficient on the lowest credit score category (<620) relative to the 650-680 category.

Table 4 provides OLS estimates of changes in credit scores, installment debt, and revolving debt for all borrowers (columns 1 to 3) and only low credit score borrowers (columns 4 to 6). None of these estimates are statistically significant at standard levels. This may be due to the noisy measurement of account balances, particularly for revolving accounts as measured at specific moment in time.

In Table 5, we provide marginal effects of probit regressions results for an indicator of revolving (column 1) and installment (column 2) account balances that have increased by $2,000 or more. Here treatment group borrowers are 32% less likely to have an increase in revolving debt of $2,000 or more.

Self-reported measures are also shown in Table 5 in columns 3, 4 and 5. Borrowers in the treatment group are 46% more likely to report saving money at follow-up, and are 53% less likely to report making manual mortgage payments (using in automated direct deposit mortgage payments instead). There are no significant differences between treatment and control group participants on reported use of a household budget, however, which might be predicted as a mechanism for money management or paying bills in a timely way.

Table 6 estimates Treatment On Treated (TOT) for default using random assignment as an instrumental variable (IV). Much like the ITT estimates in Table 3 effects are only significant for low credit score borrowers. Effect sizes are slightly smaller with a still impressive 29-31% decrease in defaults. Likewise, the estimates in Table 7 are re-assuring as robustness checks of the TOT estimates.

7 Discussion

These results provide promising evidence that simple attention-focusing interventions can have significant impact on borrower repayment patterns. This intervention, targeted to first-time home buyers, is associated with reduced mortgage default for borrowers with subprime credit scores. To the extent that low-cost interventions can be integrated into credit markets, default risks may be reduced to levels comparable to higher credit score borrowers.

The data may help explain how default rates were affected. We find some evidence that treated borrowers have lower revolving debt; they are less likely to incur a significant amount ($2,000 or more) of additional credit card debt within the first year after purchase. This is potentially important, as rising credit card balances could crowd out mortgage payments after home purchase. To the extent that planning, reminders and monitoring might help restrain this increase, borrowers may be less constrained by non-mortgage debt.

Further, we find some evidence that treated borrowers report larger savings amounts one year after purchase than control group borrowers. Overall, the amount of self-reported savings declines within the first year after purchase, in line with predicted spending pressures. However, treated borrowers are more likely to report saving money. We do not find evidence of using a budget is related to treatment. However, treated borrowers are more likely to automate their mortgage payments, perhaps consistent with sophisticated borrowers imposing constraints which will overcome predictable biases.

Recall that all borrowers completed an online assessment; treated borrowers were randomly assigned to an online financial management module and a telephone offer of financial coaching. Simply being contacted with an offer of coaching may have served as a reminder to pay the mortgage. Other mechanisms such as text messages or automated phone calls could potentially be equally effective. However, if it is the sense of being monitored, contact from a 'real' person may be necessary to influence behavior (even if calls are not answered).

It is important to caution that our sample is drawn from a select group of income qualified homebuyers participating in a publicly subsidized homeownership program. It is difficult to predict whether or not the results would hold up in a less structured program. Nonetheless, this program relies on private lenders to originate mortgages that conform with federal guidelines (all are federally guaranteed), privately serviced and sold to private investors in the secondary market. Thus, many of the characteristics of private market originations are still in place, increasing the potential for replication.

More broadly the act of setting a plan, even privately, then committing to implementation intentions with at least the potential for (or threat of) external monitoring appears to have economically significant effects on borrower behavior. The lack of use of such interventions in credit markets suggests a potential arbitrage opportunity, although incentives to capture such gains may be diffused so much in the lender market its not clear what institutions could capitalize on this potential innovation. Mortgage markets price and sell loans based on observable characteristics standardized in automated data systems. Lenders tend to sell loans rapidly to investors. A program like MyMoneyPath may ultimately require a role for the public sector to pilot or mandate interventions. Perhaps, taxpayers at the least could benefit from lowered default risks on federally-guaranteed mortgages.

These results also suggest that rigid credit underwriting regulations, as have been introduced since the start of the 2008 housing crisis, may undervalue alternative avenues for expanding credit access paired with well-designed behavioral mechanisms.

Attention-focusing mechanisms appear to have the potential to enhance credit markets through the use of technology and the application of recent insights from the behavioral economics and consumer decision making literature. Further studies might narrow this analysis to simpler assessment and goals modules, combined with automated email, text message or voice mail reminders customized to individual borrower goals.

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8 Tables and Figures

Table 1: Summary Statistics: Dependent Variables

	(1) All Borrowers			(2) 680 Credit Score		
	Control mean/sd	Treatment mean/sd	Total mean/sd	Control mean/sd	Treatment mean/sd	Total mean/sd
Treatment	0	1	0.694	0	1	0.735
	(0)	(0)	(0.461)	(0)	(0)	(0.442)
IV: TOT	0	0.359	0.249	0	0.355	0.261
	(0)	(0.481)	(0.433)	(0)	(0.480)	(0.440)
Default	0.131	0.0949	0.106	0.236	0.125	0.154
	(0.338)	(0.294)	(0.308)	(0.428)	(0.332)	(0.362)
Default, lt680	0.236	0.125	0.154	0.236	0.125	0.154
	(0.428)	(0.332)	(0.362)	(0.428)	(0.332)	(0.362)
Credit post	641.9	644.1	643.5	600.8	614.6	610.9
	(98.77)	(79.33)	(85.64)	(72.54)	(69.52)	(70.47)
Installment debt-post	29779.7	28000.8	28545.0	24908.5	28149.8	27291.8
	(30542.6)	(26426.3)	(27725.9)	(29328.9)	(26759.7)	(27445.3)
Total Revolving Debt	5711.7	4238.9	4689.4	5836.5	3843.7	4371.2
	(5914.2)	(4660.9)	(5115.3)	(6704.4)	(4180.7)	(5038.5)
Amt chk/sav	1175	1625.8	1487.9	861.5	1076.5	1019.6
	(2615.8)	(3902.1)	(3561.3)	(2999.5)	(2576.3)	(2690.8)
SR saving-post	0.536	0.708	0.655	0.485	0.681	0.629
	(0.502)	(0.456)	(0.477)	(0.508)	(0.469)	(0.485)
Manual pay-post	0.870	0.721	0.767	0.909	0.725	0.774
	(0.339)	(0.450)	(0.424)	(0.292)	(0.449)	(0.420)
Rev bal up 2k+	0.362	0.251	0.285	0.306	0.220	0.243
	(0.482)	(0.434)	(0.452)	(0.464)	(0.415)	(0.429)
Observations	425			272		

Table 2: Descriptive Statistics

	(1) All Borrowers			(2) 680 Credit Score		
	Control mean/sd	Treatment mean/sd	Total mean/sd	Control mean/sd	Treatment mean/sd	Total mean/sd
Treatment	0	1	0.694	0	1	0.735
	(0)	(0)	(0.461)	(0)	(0)	(0.442)
IV: TOT	0	0.359	0.249	0	0.355	0.261
	(0)	(0.481)	(0.433)	(0)	(0.480)	(0.440)
CR lt 620	0.162	0.132	0.141	0.292	0.195	0.221
	(0.369)	(0.339)	(0.349)	(0.458)	(0.397)	(0.415)
CR 620-650	0.215	0.275	0.256	0.389	0.405	0.401
	(0.413)	(0.447)	(0.437)	(0.491)	(0.492)	(0.491)
CR 650-680	0.185	0.275	0.247	0.319	0.400	0.379
	(0.389)	(0.447)	(0.432)	(0.470)	(0.491)	(0.486)
CR 680-720	0.223	0.159	0.179	0	0	0
	(0.418)	(0.367)	(0.384)	(0)	(0)	(0)
Ever late any tradeline 24 mo	0.208	0.217	0.214	0.333	0.290	0.301
	(0.407)	(0.413)	(0.411)	(0.475)	(0.455)	(0.460)
OHFA mntly inc at purchase (000)	38.56	37.32	37.70	38.27	37.52	37.72
	(12.08)	(12.31)	(12.24)	(12.58)	(12.52)	(12.51)
DTI ratio non-housing debt at purchase	0.145	0.144	0.144	0.133	0.149	0.145
	(0.142)	(0.245)	(0.219)	(0.0858)	(0.289)	(0.252)
Female	0.446	0.471	0.464	0.500	0.470	0.478
	(0.499)	(0.500)	(0.499)	(0.504)	(0.500)	(0.500)
Age	33.31	32.25	32.58	34.97	33.73	34.06
	(10.64)	(10.06)	(10.24)	(10.41)	(10.34)	(10.35)
College	0.362	0.353	0.355	0.167	0.270	0.243
	(0.482)	(0.479)	(0.479)	(0.375)	(0.445)	(0.429)
Minority	0.115	0.153	0.141	0.181	0.195	0.191
	(0.321)	(0.360)	(0.349)	(0.387)	(0.397)	(0.394)
HH Size	2.400	2.431	2.421	2.708	2.605	2.632
	(1.309)	(1.286)	(1.292)	(1.409)	(1.378)	(1.384)
Days since home purchase	506.7	515.4	512.8	500.9	513.9	510.4
	(65.49)	(62.15)	(63.24)	(69.33)	(60.54)	(63.12)
Total savings	2987.1	3239.1	3162.0	2543.9	2822.3	2748.6
	(3295.3)	(3340.0)	(3324.5)	(2631.1)	(2840.4)	(2784.5)
Wants 40 now (vs 60 in 1 month)	0.0615	0.0949	0.0847	0.0556	0.0950	0.0846
	(0.241)	(0.294)	(0.279)	(0.231)	(0.294)	(0.279)
Observations	425			272		

Table 3: Effects of Treatment on Default Probit Intent to Treat (ITT) by Credit Score Level at Study Start

	(1) Default b/se	(2) Default, lt680 b/se	(3) Default b/se	(4) Default, lt680 b/se
Treatment	-0.1883	-0.4315**	-0.2541	-0.4055*
	(0.172)	(0.199)	(0.195)	(0.210)
CR lt 620			4.6712***	0.4322*
			(0.284)	(0.252)
CR 620-650			4.4499***	0.2443
			(0.239)	(0.235)
CR 650-680			4.1866***	
			(0.264)	
CR 680-720			3.8126***	
			(0.293)	
Mntly Income(000)			-0.0193**	-0.0214**
			(0.008)	(0.009)
Debt:Income ratio			-0.2054	-0.2305
			(0.221)	(0.230)
Female			0.1301	0.0818
			(0.197)	(0.209)
College			-0.0956	0.0037
			(0.248)	(0.266)
Minority			0.3565	0.3323
			(0.253)	(0.257)
HH Size			0.0964	0.1057
			(0.063)	(0.067)
Days since purchase			0.0017	0.0021
			(0.001)	(0.001)
Total savings			-0.0001	-0.0001
			(0.000)	(0.000)
Pref 40 vs 60 in 1 mo			0.4434	0.0413
			(0.323)	(0.366)
Constant	-1.1228***	-0.7189***	-5.5368***	-1.2247
	(0.139)	(0.163)	(0.892)	(0.922)
Controls	No	No	Yes	Yes
N	425	272	425	272
r2_p	0.004	0.020	0.189	0.114
chi2	1.196	4.721	1455.563	27.584
p	0.274	0.030	0.000	0.016

Marginal effects

Probit. Controls include baseline credit score, delinquent on trades, income, gender, debt to income, age, education, race, time in home, savings, time preferences.

(d) for discrete change of dummy variable from 0 to 1

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 4: No Effects of Treatment on Credit Score and Installment Debt: OLS Intent to Treat (ITT)

	(1) Credit post b/se	(2) Ln Inst post b/se	(3) Ln Revlv post b/se	(4) Credit post b/se	(5) Ln Inst post b/se	(6) Ln Revlv post b/se
Treatment	10.1380	0.1831	-0.1844	10.2085	0.4015	-0.1104
	(7.910)	(0.254)	(0.175)	(8.900)	(0.308)	(0.211)
N	425	424	424	274	274	274
r2	0.365	0.305	0.441	0.175	0.298	0.467
p	0.000	0.000	0.000	0.000	0.000	0.000

OLS.

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 5: Effects of Treatment on Self Reported Savings and Non-use of Automatic Payments Probit Intent to Treat (ITT)

	(1) Rev bal up 2k+ b/se	(2) Inst bal up 2k+ b/se	(3) SR saving-post b/se	(4) Manual pay-post b/se	(5) Use Budget b/se
Treatment	-0.3175**	0.0269	0.4560**	-0.5392**	0.0661
	(0.138)	(0.134)	(0.185)	(0.220)	(0.191)
N	425	424	223	223	225
r2_p	0.010	0.000	0.021	0.026	0.000
chi2	5.311	0.041	6.050	6.014	0.120
p	0.021	0.841	0.014	0.014	0.729

Marginal effects

(d) for discrete change of dummy variable from 0 to 1

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 6: Treatment on Treated (TOT) Effects on Default using Assignment as Instrument (LIML IV Regression), by Starting Credit Score)

	(1) Default b/se	(2) Default, lt680 b/se	(3) Default b/se	(4) Default, lt680 b/se
IV: TOT	-0.0998	-0.3130**	-0.1548	-0.2904**
	(0.095)	(0.158)	(0.097)	(0.139)
Controls	No	No	Yes	Yes
Observations	425	272	425	272
F statistic for weak identification	164.7	109.3	140.6	39.73

LIML IV. Controls include baseline credit score, delinquent on trades, income, gender, debt to income, age, education, race, time in home, savings, time preferences.

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 7: Treatment on Treated (TOT) Efects on Change in Credit Score and Installment Debt (log), Follow-up Self Reports of Saving and Manual Payments using Assignment as Instrument (LIML IV Regression)

	(1) Credit post b/se	(2) Ln Inst post b/se	(3) Ln Revlv post b/se	(4) Rev bal up 2k+ b/se	(5) Inst bal up 2k+ b/se	(6) SR saving-post b/se	(7) Manual pay-post b/se	(8) Use Budget b/se
IV: TOT	28.0299	0.5103	-0.5232	-0.3081**	0.0291	0.3476**	-0.3015***	0.0461
	(21.911)	(0.707)	(0.498)	(0.139)	(0.144)	(0.147)	(0.114)	(0.134)
Observations	425	424	424	425	424	223	223	225

LIML IV. Controls include baseline credit score, delinquent on trades, income, gender, debt to income, age, education, race, time in home, savings, time preferences.

* $p < .1$, ** $p < .05$, *** $p < .01$

Figure 1: Study Design

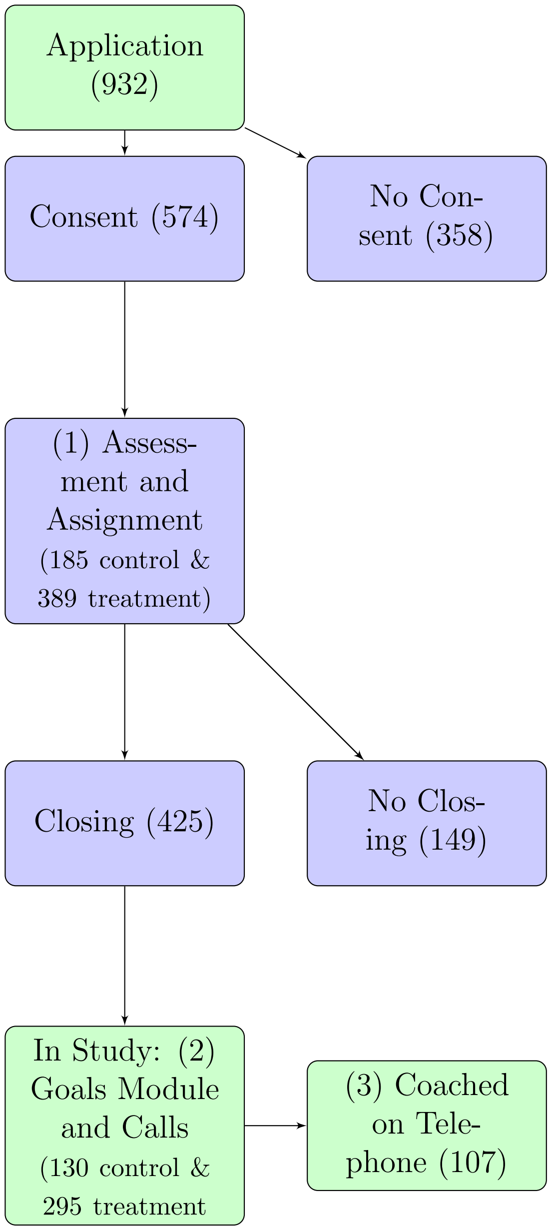

Figure 2: Mean Default: Overall

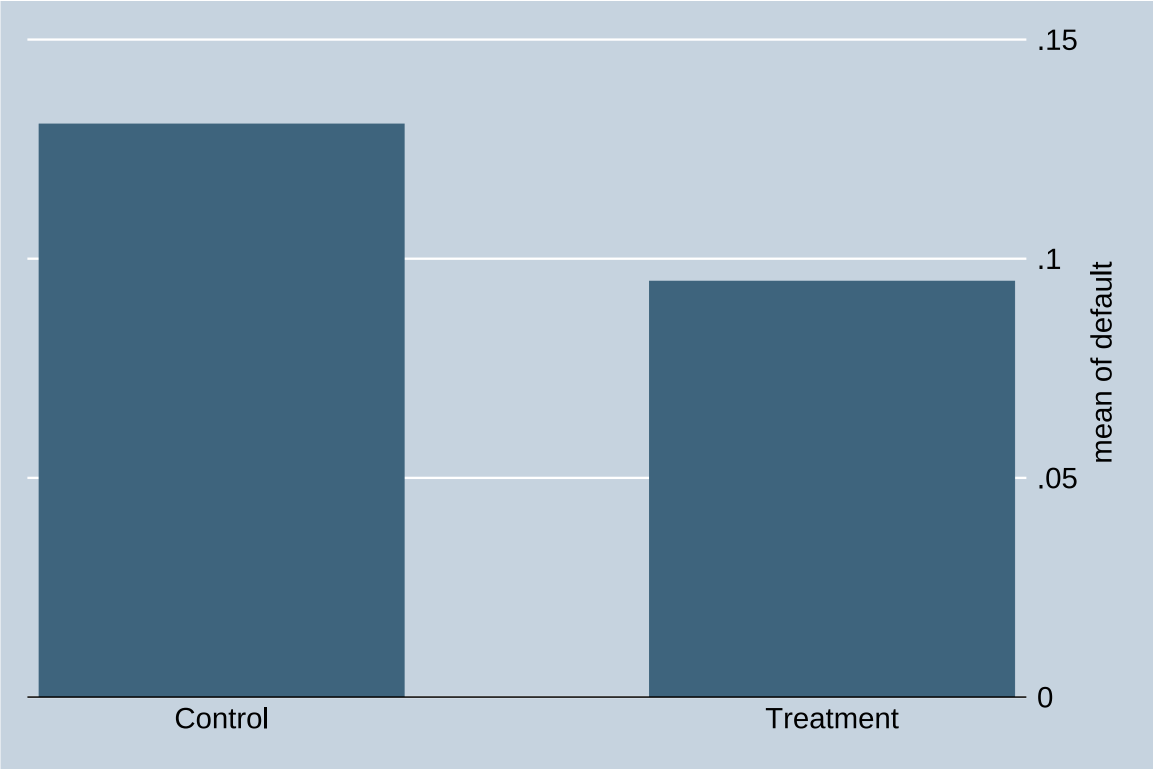

Figure 3: Mean Default: Low Credit Score (lt 680) Only

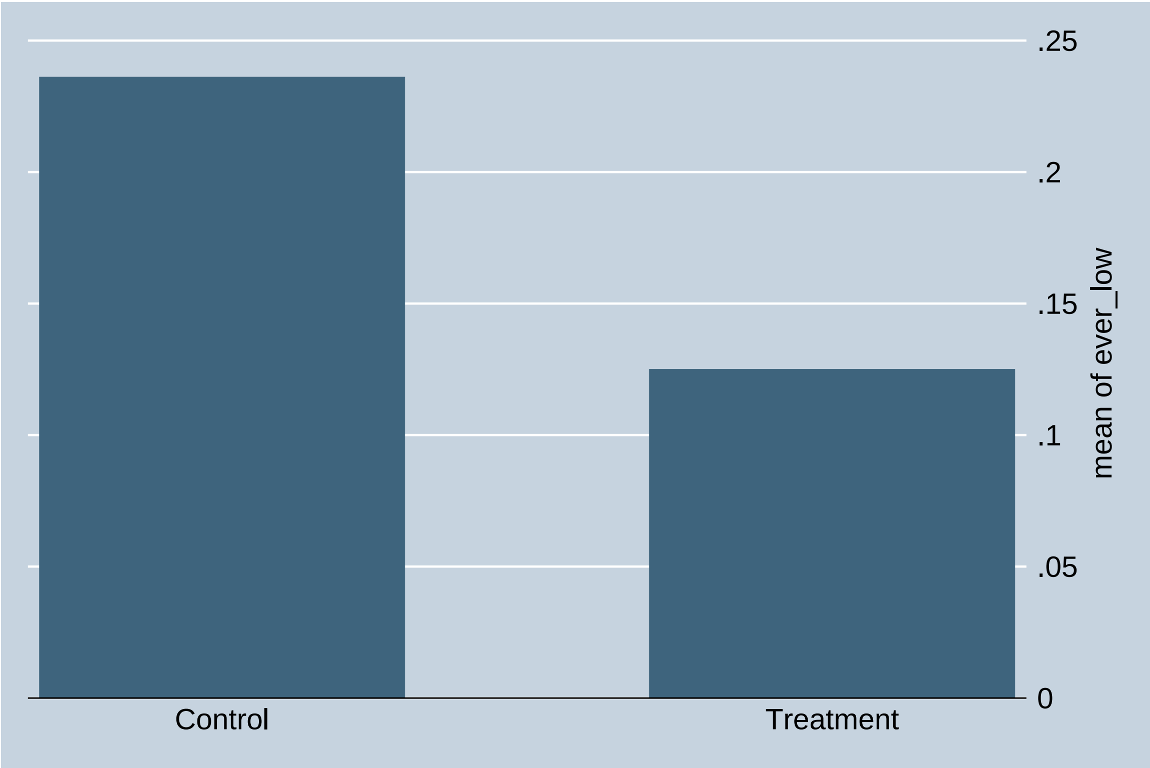

Figure 4: Pre-Post Credit Score by Treatment

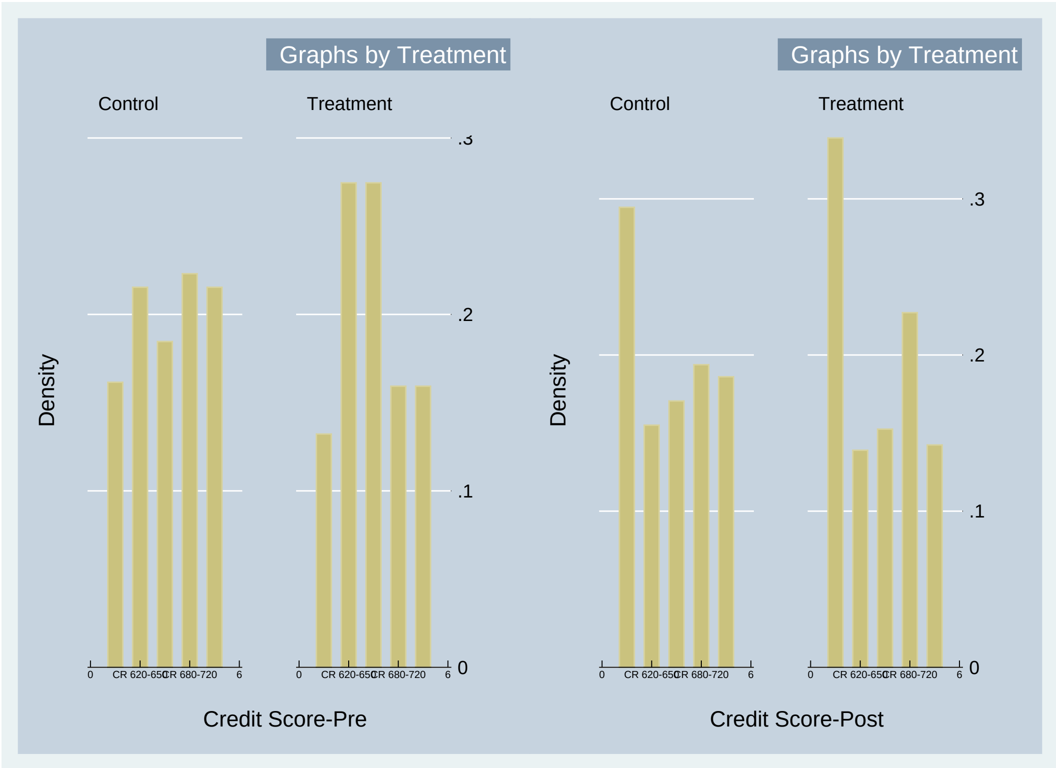

Figure 5: Mean Total Installment Debt

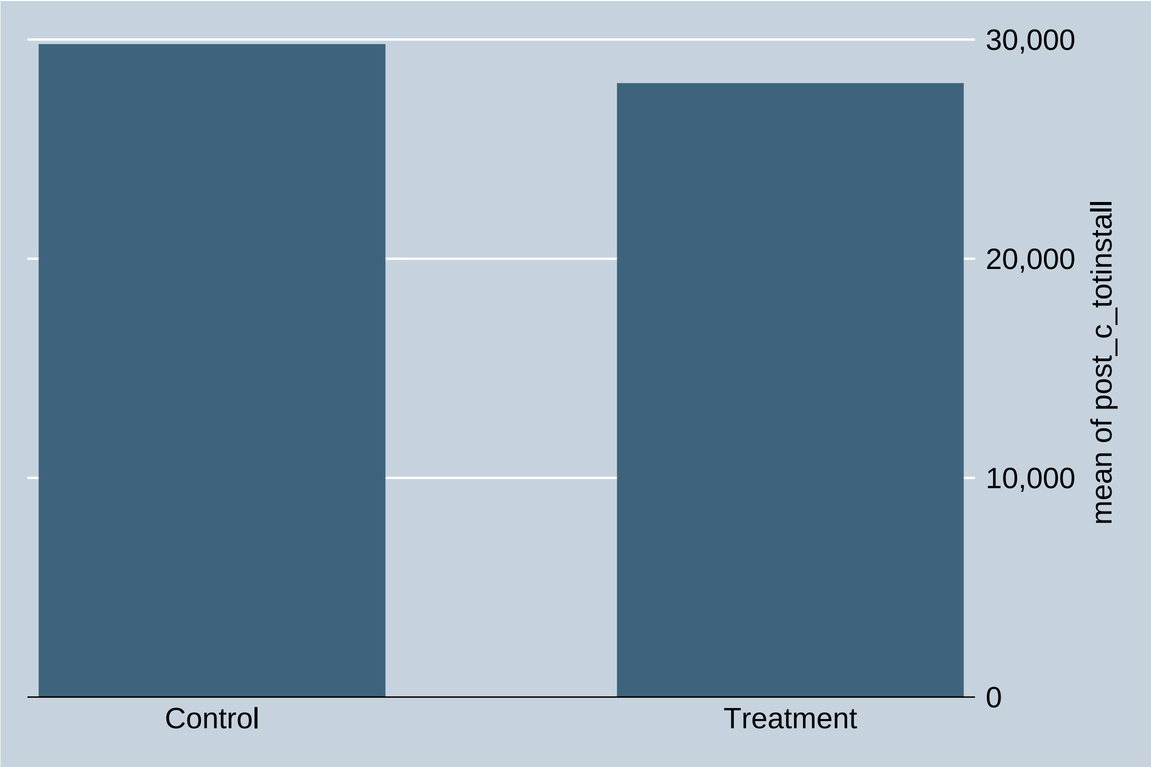

Figure 6: Revolving Debt by Treatment

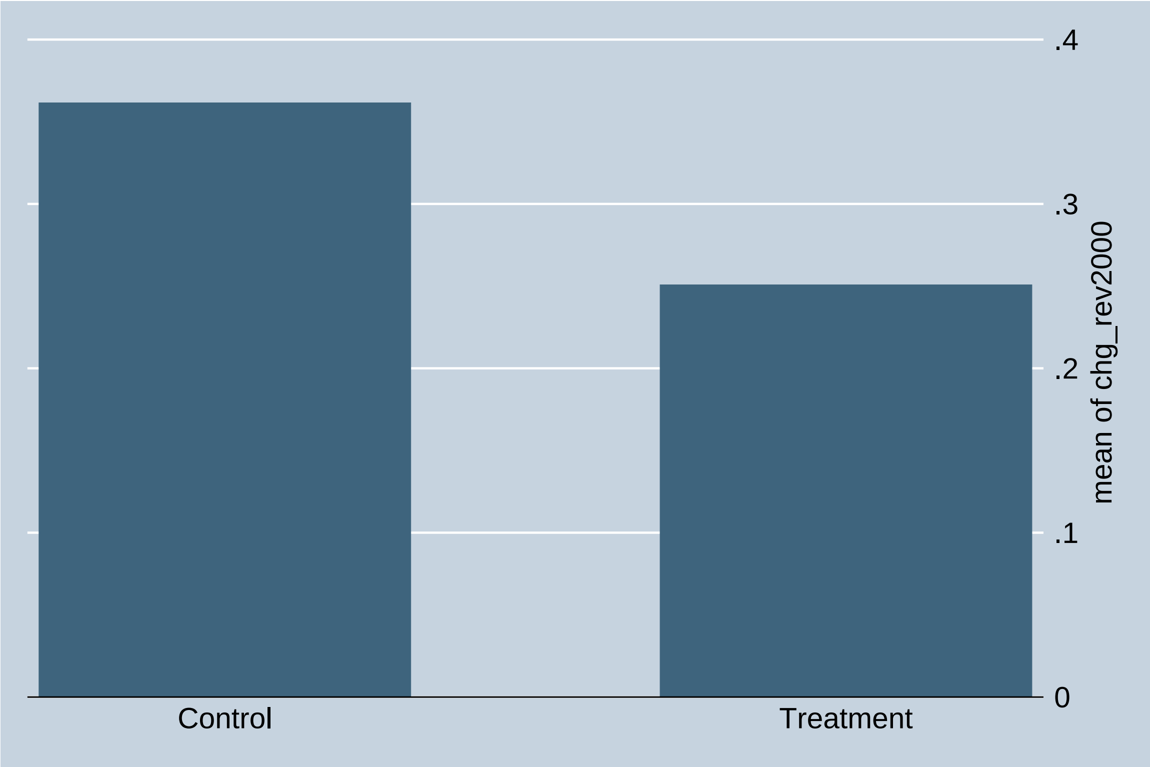

Figure 7: Mean Self Reported Rate of ‘Manual’ Mortgage Payment (vs. auto pay)

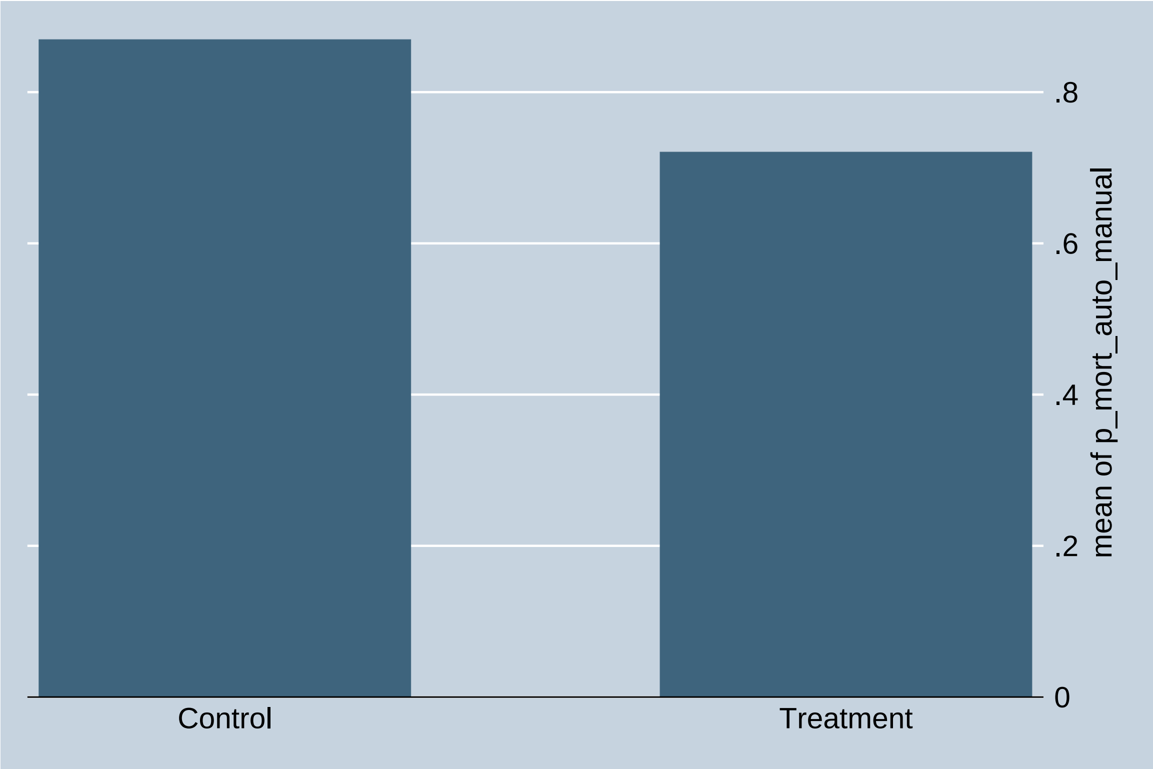

Figure 8: Mean Self Report of 'Saving Money'

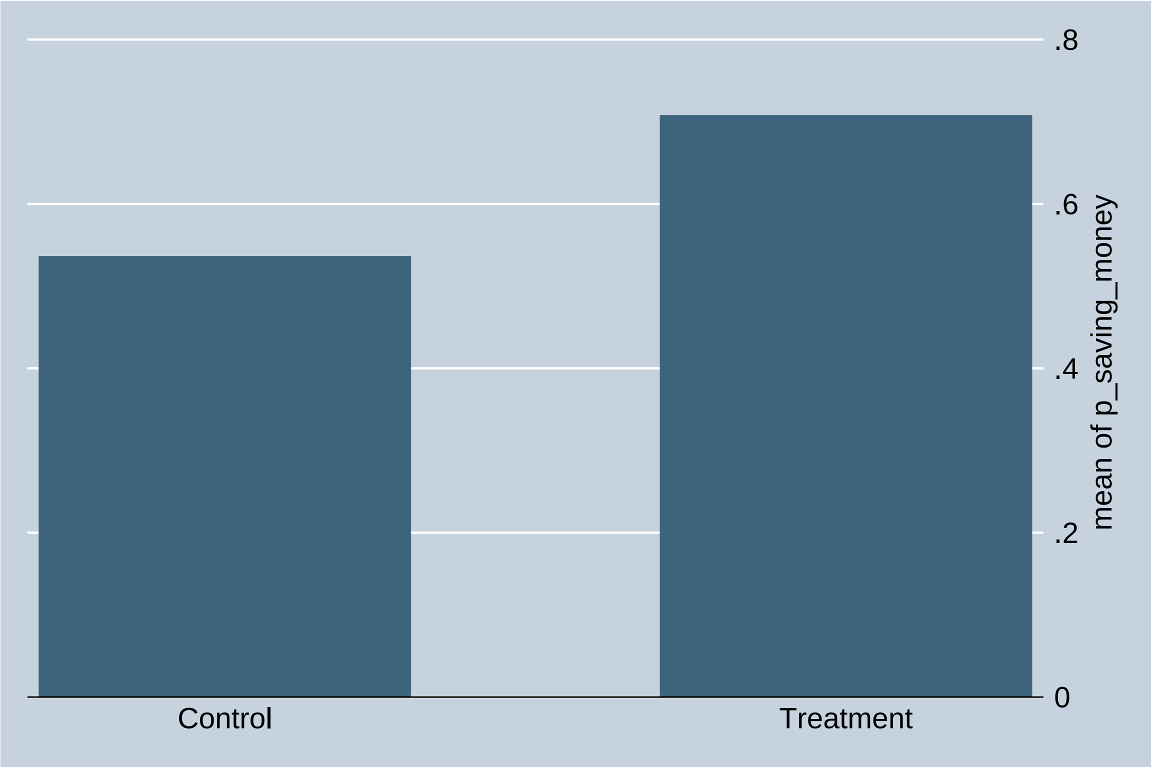

Figure 9: Screenshot of Assessment Tool