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TIME-INCONSISTENCY AND SAVING: EXPERIMENTAL EVIDENCE FROM LOW-INCOME TAX FILERS

Damon Jones
Aprajit Mahajan

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ABSTRACT

We conduct a field experiment designed to test theories of time-inconsistency, namely a "Beta-Delta" model of present bias. The experiment takes place in the context of a saving decision made by low-income tax filers who can deposit their income tax refund into an illiquid account. We find qualitative evidence consistent with present-biased preferences. The tradeoff between an earlier payment or a later one is much more skewed toward taking the early payment when the decision is made on the spot than when the decision is made in advance. We estimate a and of 0.34 and 1.08 over an 8-month horizon, respectively, which translates into an annual discount rate of 164%.

Damon Jones
Harris School of Public Policy
University of Chicago
1155 East 60th Street
Chicago, IL 60637
and NBER
damonjones@uchicago.edu

Aprajit Mahajan
Dept. of Agricultural & Resource Economics
University of California, Berkeley
219 Giannini Hall
Berkeley, CA 94720-3310
and NBER
aprajit@gmail.com

1 Introduction

A growing body of economic literature emphasizes the importance of time-inconsistent preferences.[1] This deviation from standard models can have implications in a number of domains, including health, labor supply and addictive behavior.[2] In this paper, we examine a specific class of time-inconsistent preferences, present-biased preferences,[3] in the context of savings decisions among low-income households, a population of particular interest in this setting. For example, self-control problems may lead to poverty traps.[4] Similarly, it has been shown that in the presence of limited credit supply, self-control problems may prevent households from accumulating savings and making lumpy purchases with non-linear payoffs, such as acquiring a vehicle or moving to a new neighborhood.[5] Furthermore, self-control problems may prevent households from optimally allocating resources inter-temporally in the presence of lumpy transfers such as food-stamps.[6] On the other hand, solutions to self-control problems, such as illiquid savings vehicles, may render low-income households more vulnerable to negative income shocks in the short-run.

We conduct and analyze a field experiment designed to examine the relevance of self-control related problems in explaining observed saving behavior. We develop formal tests for the presence of present-bias by offering respondents monetary rewards for planning to save and actually saving. The main intervention offered respondents one of two types of financial rewards — one closer in time and one further in the future — if they saved or committed to saving their income tax refund for a specific length of time. This was combined with a second intervention — a "soft-commitment" device that offered households a financial reward if their actual saving decision at the time they receive a refund corresponded to their stated decision months earlier.[7] Together these interventions allow us to identify the parameters of a quasi-hyperbolic discounting model, also known as the "β-δ"

[1] See e.g. DellaVigna (2009); Frederick, Loewenstein, and O'donoghue (2002), for reviews.

[2] See e.g. DellaVigna and Malmendier (2004b); Kaur, Kremer, and Mullainathan (2014); Rabin and O'Donoghue (2001).

[3] See Laibson (1997); O'Donoghue and Rabin (1999).

[4] Banerjee and Mullainathan (2010)

[5] Bernheim, Ray, and Yeltekin (2013)

[6] Shapiro (2005)

[7] In our setting, we use the term "soft-commitment" to describe a decision that increases the future incentive to save, but does not completely determine the future saving decision. This is not to be confused with the usage in Bryan, Karlan, and Nelson (2010), where "soft-commitment" devices — those with primarily psychological penalties — are contrasted with "hard commitment devices" — those with primarily real economic penalties.

model, [8] which nests the standard model of time-consistent preferences in a model of present bias.

Our experiment results in several pieces of evidence in favor of present-bias. In our primary analysis, we obtain point estimates for the time discount factor β in the range 0.19–0.45 — almost always significantly lower than the null hypothesis of 1. This parameter is recovered using a set of discrete outcomes — soft-commitment and saving decision — and relies on a quasi-linearity assumption. We therefore turn to an alternative method developed by Andreoni and Sprenger (2010), which relies on continuous saving decisions and allows for risk aversion. In this case, we are able to identify the composite parameter $\beta\delta$, which ranges from 0.06–0.49. This overlaps well with the range of the same composite parameter using our primary analysis — 0.20–0.52. Since our primary point estimates of β are estimated among the sample that conditions on non-attrition, we additionally calculate bounds on these estimates that account for any non-random attrition. Due to high rates of attrition, these upper bounds on β remain no longer rule out a value of 1. In our preferred specification, the estimates of β and δ — 0.34 and 1.08, respectively, over an 8 month horizon — translate into an annual discount rate of 164%. This falls in the range of previous estimates — 49%–238%.[9]

Our project makes a number of contributions to the existing literature. First, we complement the existing set of time preference studies — which tend to be laboratory or artefactual experiments over moderate or hypothetical stakes — by varying relatively large stakes in a "natural" field experiment. The income tax refund can typically be as much as 40% of annual income and the decision of whether or not to save it in an illiquid account is not uncommon in our setting. In recent work, Giné, Goldberg, Silverman, and Yang (2013) implement a field experiment in which quantitative measures of time preference and time-inconsistency are correlated with the likelihood of revising one's savings plans. Meier and Sprenger (2010) elicit time-preference parameters from a sample similar to ours: low-income tax filers in the US, while Eckel, Johnson, and Montmarquette (2014) elicit time preferences from a sample of primarily working poor, Canadian households. In the three aforementioned studies time-preferences are elicited by incentivized choice experiments: either

[8] See Strotz (1955a) and Laibson (1997). While not the only parameterization for time-inconsistent preferences (see e.g. Gul and Pesendorfer (2004) or Benhabib, Bisin, and Schotter (2010) for alternative formulations), the β-δ model is the workhorse for empirical work on the topic.

[9] See Laibson, Repetto, and Tobacman (2007); DellaVigna and Paserman (2005); Fang and Silverman (2009).

the "convex time budget" or "multiple price list" method.[10] They may be regarded as "artefactual" field experiments in the parlance of Harrison and List (2004). Our study contributes to this line of work by measuring time-preferences in the course of decisions that are routine to the agents, i.e. the decision to save one's income tax refund.[11] Thus, we aim for a context that is akin to a "natural" field experiment (Harrison and List (2004)).[12]

Our second contribution is that we complement existing methods of eliciting time preferences by exploring the implications of a soft-commitment device — i.e. a non-binding commitment mechanism — and using these theoretical implications to inform our experimental design. A standard approach to qualitatively detecting is demonstrating a demand for commitment.[13] Our method differs from these previous results in that our soft-commitment is intentionally non-binding, but nonetheless incentivizes revelation of time-inconsistency.[14]

In addition, our test does not hinge on detecting a demand for commitment. Similar to Read, Loewenstein, and Kalyanaraman (1999), access to the commitment technology in our experiment is exogenous, and the comparison of outcomes between these experimental groups is used to shed light on time preferences. Essentially, we rely on the relative effectiveness of two monetary incentives — one received earlier in time and another received later in time — and the difference in this comparison when a decision is made in advance or on-the-spot. In this sense, our test of present-bias is similar tests that rely on aggregate preference reversals over either hypothetical or real payoffs at different points in time.[15] We show how such aggregate preference reversals may be used to identify the presence of present-bias under relatively weak assumptions, when there is not uncertainty regarding preferences over time. However, once we allow for uncertainty, we must place

[10]See Andreoni and Sprenger (2010) for a discussion and comparison of the two methods and see Frederick, Loewenstein, and O'donoghue (2002) for an extensive review of the time-preference literature.

[11]Another recent field experiment carried out in a "natural" setting is conducted by Sadoff, Samek, and Sprenger (2015), who use food purchasing decisions to test for time-inconsistency.

[12]Hausman (1979) and Wanrner and Pleeter (2001) estimate time preference parameters in "natural" decision-making contexts, but do not explicitly test for time-inconsistent preferences. Harrison, Lau, and Rutström (2005) test for dynamic inconsistency using an "artefactual" field experiment.

[13]See Bernatzi and Thaler (2004); Ashraf, Karlan, and Yin (2006); Duflo, Kremer, and Robinson (2009); Tarozzi, Mahajan, Yoong, and Blackburn (2009); Gine, Karlan, and Zinman (2010); Mahajan and Tarozzi (2010).

[14]One exception to this set of studies is Beshears, Choi, Laibson, Madrian, and Sakong (2013), where the commitment technology is also only partially binding.

[15]See Thaler (1981); Ashraf, Karlan, and Yin (2006); Meier and Sprenger (2010); Tanaka, Camerer, and Nguyen (2010); Bauer, Chytilová, and Morduch (2012).

some restrictions on preferences — i.e. quasi-linearity. In return, our model can allow for shocks to preferences over time if we use an appropriately rich experimental design. This method also allows us to extend beyond qualitative evidence of present-bias and estimate the parameters of a "β-δ" model. We describe our approach in more detail in Section 5.

A third contribution is that the outcomes we examine are of direct interest to policy makers. The specific context for our study is that of savings among low-income tax filers. Previous research has demonstrated that among tax filers there exists a demand for savings options[16] , a positive effect of a match on savings[17] and a demand for illiquid savings options, such as a savings bond.[18] Our contribution includes examining the effects of commitment mechanisms in the context of income tax refund-based saving in the US. Our method differs from these previous results in that our soft-commitment is intentionally non-binding, but nonetheless incentivizes revelation of time-inconsistency. Thus, our test does not hinge on detecting a demand for commitment. The paper proceeds as follows: Section 2 describes the design of the field experiment. We present the reduced form results from the experiment in Section 4. We then outline a basic theoretical model in Section 5 and present the structural estimates for the model in Section 6. Section 7 concludes with a discussion of the results.

2 Study Design

2.1 Institutional Details and Sample Selection

Our intervention takes place in the context of the non-profit tax preparation industry, where low- and moderate-income tax filers receive free tax preparation assistance.[19] These non-profits — referred to as Volunteer Income Tax Association (VITA) organizations — typically offer additional services to their customers, including enrollment assistance for public benefits and financial counseling.

[16] Beverly, Schneider, and Tufano (2006)

[17] Duflo, Gale, Liebman, Orzag, and Saez (2006)

[18] Tufano (2008)

[19] Third-party tax preparation is quite common among low-income households, particularly those eligible for various refundable credits — which are the object of many saving promotion policies. For instance, IRS public use data suggests that 70% of EITC recipients in 2006 used a tax preparer.

The setting is useful for the purposes of experimental study as a majority of tax filers receive a relatively lumpy payment during tax season — the income-tax refund.[20] Due to refundable credits such as the EITC and Child Tax Credit (CTC), as well as overwithholdings, the income tax refund is likely to be the single largest payment received by low-income households during the year. This lumpy payment generates a number of issues regarding consumption smoothing, including possible borrowing in anticipation of the loan and the allocation of the payment optimally over future periods. In response, VITA sites and policymakers have encouraged saving among low-income tax filers, often by using a combination of illiquid and matched saving vehicles. These include US Treasury bonds (Tufano, 2008), tax-deferred retirement accounts (Duflo, Gale, Liebman, Orzag, and Saez, 2006), and, in the case of New York City, a short-term, illiquid CD with a relatively high matching rate, the SaveNYC account.[21] While we remain agnostic regarding the optimality of saving one's income tax refund among this population, we nonetheless use this decisionmaking context — natural to our study participants — as the point of departure for experimental design.

Tax preparation typically lasts from early February to mid April, with the bulk of refund recipients filing during the first few weeks of February or near the April deadline. In many cases, tax filers interact with a VITA site solely during the tax season. In addition, there tends to be high turnover in clients from year to year. Our partner, The Financial Clinic, based in New York City, features a suite of services that include tax preparation, financial counseling and legal assistance. The Financial Clinic offers some year-round services, such as debt management counseling and legal assistance with tax matters. Their interaction with clients throughout the year is key to our experimental design, which involves reaching tax filers in advance of the tax season. It is also our hope that the scope of their services — tax preparation, legal advice and financial counseling — generated a relatively high level of baseline trust, which, if absent, can confound studies of time-inconsistent preferences (Andreoni and Sprenger, 2012).

Our initial pool of potential participants consists of all clients at the VITA site from the previous tax filing season. In order to focus on filers with significant refunds, we only consider clients with

[20]Note that over 80% of US tax filers receive income-tax refunds with the share being even higher for low-income households (IRS public use data). In our study, the average refund size was about $2,095 for Tax Year (TY) 2009.

[21]Now called the SaveUSA New York City account. See http://www.nyc.gov/html/ofe/html/policy_and_programs/saveusa.shtml for details on this program.

tax refunds of at least $300 in the previous year.[22] In addition, we restrict our study to those clients whose primary language is either English or Spanish. In our main sample, this results in a total of 833 clients approached in the fall of 2010, who are randomly assigned to one of six experimental groups.[23]

Our sampling frame, while not representative of the US population, is of direct interest to policymakers. There are various policy initiatives aimed at encouraging low-income tax filers to save[24] and our intervention is well suited to examine issues surrounding the take-up such a program. In addition, we are not concerned that our estimates are biased due to the refund restrictions we have made. In particular, prior research (Jones, 2012) demonstrates that the income tax refund is the result of largely passive behavior on behalf of the tax filer, especially in the case of low-income tax filers that receive the EITC.[25]

2.2 Conceptual Overview

To fix ideas, consider an individual who files her taxes in February and chooses *in February* between receiving her income tax refund immediately or placing it in an illiquid, interest bearing savings account until October. Suppose that the individual is offered a fixed payment as an incentive to save and that this additional incentive can either be received immediately (February) or is delayed until money is withdrawn from the savings account (October). As compared to the baseline take-up rate of this savings account, the relative effect of the incentive, immediate or delayed, can give one a sense of the time discount between the two periods, February and October.[26] Alternatively, suppose

[22] Analysis of 2008 and 2009 tax data at this tax preparer indicates that past refunds are highly predictive of future refunds.

[23] In the year after our main study, we attempted to collect more data among a second pool of 994 clients. We discuss this additional data collection in Section G below.

[24] Examples of this include: numerous changes to the tax filing process including the ability to have refunds deposited directly into savings accounts or invested in savings bonds, a national pilot to offer matched savings to low-income filers (Save USA) and potentially the Savers Credit, though this last program targets a broader range of incomes.

[25] In an appendix of that paper, the author shows that the high level of overwithholding cannot be fully explained by a preference for forced savings. Other work (Jones, 2010) suggests that it is safe to assume that the Advance EITC and its low take-up can be ignored due to dramatically low awareness of this program.

[26] The use of monetary incentives to identify discount factors in relies on an assumption of borrowing constraints. This is perhaps reasonable for the low-income EITC recipients in our study and can additionally be confirmed using our survey data among participants.

that the individual were asked at an earlier date (e.g. in December of the previous year) to choose between receiving the refund in February or saving it until a future date (e.g. October) and thereby earning some interest as in the earlier setting. Again, suppose that a fixed savings incentive is either delivered early (in February) or late (in October) as above.

Standard theories of present-biased preferences predict that when asked at this earlier date, respondents may be more inclined to choose the savings option. In addition, from this pre-tax season vantage point, the relative effect of the early incentive versus the late incentive will be less pronounced. Intuitively, when making a decision in December, the tax filer already has to wait two months to receive the refund. Waiting an additional number of months to receive a larger payment may not seem so costly. The individual is more aware of the benefits of saving vis-a-vis the cost of putting the money aside when both occur far enough in the future. Typical models of present-bias also suggest that if a sophisticated tax filer can make a binding, early decision in December to save in February, she may do so as a means of self-control. However, if the commitment device is not completely binding, a sophisticated tax filer may avoid it, realistically believing that commitment will ultimately be undone. As we explain below, the use of a partial commitment mechanism will be key in allowing the researcher to infer preferences in December over outcomes in February and October.

2.3 Experimental Arms

Our actual experimental design builds upon the stylized setting described above. However, institutional constraints require the actual experiment to differ in important ways. In particular, we are not able to implement binding commitments to save in the months prior to the the tax season. In practice, individuals may face uncertainty regarding the level of the refund and/or their ultimate preferences over saving. Thus, we offer a "soft-commitment" option that incentivizes participants to follow through with their initial commitment, but also allows revisions to the final saving plan. Nonetheless, our experiment identifies time inconsistency by making use of the same key variation. That is, we compare the relative responsiveness to an incentive received earlier in time (e.g. February) to one received later in time (e.g. October) for individuals making a decision in advance (e.g.

December) or on the spot (e.g. February).[27]

The intervention has six experimental arms. In particular, each participant belongs to either the non-commitment or commitment group — the nomenclature is explained below. Within each of these groups, there are three arms. In the non-commitment group, there is a control arm, an early incentive treatment arm and a late incentive treatment arm. Within the commitment group, there is likewise a control arm, an immediate incentive treatment arm and a delayed incentive treatment arm. A timeline of the of the field experiment is provided in Figure A.1 and graphical representation of each experimental group's incentives are provided in Figures A.2–A.6.

2.3.1 Non-Commitment Group

Tax filers in the non-commitment group make an on-the-spot savings decision at the tax site (e.g. in February), in response to variation in incentives paid either immediately (e.g. in February) or at a delayed time (e.g. in October). We use the outcomes in this group to inform us about the discount between the present and a future period. This group was contacted in December 2010. First, they received a notice in the mail, from the Financial Clinic, informing them that we would be calling them to explain a savings opportunity. Importantly, the mailing included information briefly summarizing the key, treatment group-specific, incentives.[28] We then followed up the initial mailing with phone outreach to enroll the tax filer in the study. During the call, survey data on demographic characteristics and financial behavior were collected, and the participant was also asked to consider a matched savings account: the SaveUp Account. Participants were informed that if they return to The Financial Clinic tax preparation site to file their taxes, they would have the opportunity to open the SaveUp Account. In order to obtain the match, the tax filer must keep her savings amount in an illiquid account for 8 months (that is until October 2011). In return for allocating at least $300 to this account, the tax filer would receive a 50% match for each dollar

[27]For convenience we identify the pre-tax filing period as December, the tax filing period as February and the post-tax filing period as October. In practice, pre-tax interventions are made between late November and early December, tax filing is carried out between February and April, and savings withdrawals may be made as early as October or as late as December. However, the period of time between decisions and payments at the tax site and payments later in the year is held roughly fixed at 8 months across individuals.

[28]Samples of the mailings sent to teach treatment group are provided in Appendix E.

saved between \$300 and \$1,050.[29]

Participants who only received this information comprise the non-commitment control arm (group 1). The incentives for this control arm are represented graphically in Figure A.2. Within the set of non-commitment arms, there are two treatment arms who are additionally offered either an "immediate" incentive of \$50 (group 2), given at the time of filing taxes, or a "delayed" incentive of \$50 received after the 8 month saving period (group 3), in return for saving. A graphical representation of the incentives for members in these two treatment arms is provided in Figures A.3 and A.4.[30]

When participants in these three arms arrived to file their tax returns during the following tax season — February through April — they were presented with the opportunity to open a SaveUp account, featuring the savings incentives described above. The Financial Clinic is able to open the savings account with money from the income tax refund, via a direct deposit option on the income tax return. If an account is opened, the funds cannot be withdrawn for 8 months, at which point all savings matches are credited to the account. Members of treatment arm 2 who open an account receive their "immediate" incentive when making the initial deposit (see Figure A.3), while members of treatment arm 3 receive the "delayed" incentive 8 months after opening the account (see Figure A.4). Additional survey data, similar to that collected over the phone, was collected at the tax site.

Concretely, a member of the non-commitment control group had the opportunity to receive up to \$375 in saving incentives if they chose to save \$1,050. A member of the non-commitment treatment groups could have received up to \$425 in saving incentives — which includes an extra \$50 incentive over the control arm for saving.

[29]Though generous, our match is less generous than pre-existing programs offered by e.g. New York state. The Save NYC account offers a 50% match on every dollar, until \$1,000 (see http://www.nyc.gov/html/ofe/html/poverty/save.shtml), and has been expanded to other cities across the US. Importantly, the Save NYC account is not simultaneously offered to our study participants.

[30]Even though the "immediate" match is given up front, it is forfeited by deducting the match amount from the savings account in the event of an early withdrawal. Thus, it still serves as a conditional match.

2.3.2 Commitment Group

Participants in the commitment group made two decisions. Their first, soft-commitment decision was made prior to the tax season (e.g. December) in response to incentives paid earlier in the future (e.g. February) or later in the future (e.g. October). We use this first set of decisions to learn about the discount between two future payments, one further in the future than the other. Commitment group members also made a final savings decision at the tax site. This group was similarly contacted during the December preceding the tax filing season with a mail notice followed by phone outreach. In the mailing and over the phone they were provided with the relevant information for their experimental arms.[31] Members of the commitment group were given surveys and informed of a saving opportunity, the SaveUpFront Account, in a similar fashion to members of the non-commitment groups.

In contrast to members of the non-commitment group, commitment group members were asked in December to make a soft-commitment regarding a savings account. Importantly, they either had the option of soft-committing to save or soft-committing to *not* save. The SaveUpFront account is similar to SaveUp Account, with a minimum deposit of $300, and a 50% match on every additional dollar deposited up until $950.[32] The commitment was "soft" in that the ultimate savings decision could deviate from the commitment. However, the soft-commitment still mattered for future incentives. If the tax filer softly committed to saving, the savings account would include an additional $100 in savings incentives conditional on saving at least $300. Alternatively, if the tax filer soft-committed to not saving, she still had the option to save at the tax site, but now would receive a $75 payment in October should she *not* have saved. Thus, the commitment reinforced decisions in either direction, and therefore can be distinguished from "cheap talk." Participants who only received this treatment comprised the commitment control arm (group 4). The incentives for this arm are illustrated in Figure A.5.

The remaining members of the two commitment treatment arms were offered an additional

[31] Samples of the mailings for these experimental arms are also provided in Appendix E.

[32] We chose different upper limits for the two groups since commitment group members received additional incentives for following through with commitments. In particular, this allowed our scripts across treatments to be roughly equivalent when we stated "you will have the opportunity to receive as much as "$425" in savings incentives." Furthermore, we see that the upper limit is typically not binding in our study.

incentive for soft-committing to save. For soft-committing to save, the individual received a $50 incentive — either an "early" incentive, given at the time of filing taxes (group 5), or a "late" one received 8 months after the tax season (group 6). Importantly, *the incentive was kept regardless of the final savings outcome* — a key difference from the incentives for the non-commitment group. As will be explained below in Section 5, linking this incentive to the commitment decision, but keeping it independent of the final saving decision is key for separately identifying the discount parameters in a quasi-hyperbolic discounting model. The incentives for the two commitment treatment arms are illustrated in Figures A.6 and A.7.

At the time of tax preparation, members of the commitment group were reminded of their prior soft-commitment. They were also reminded of the features of the SaveUpFront Account, which depended on the previous, soft-commitment decision as demonstrated in Figures A.5–A.7. They then could make a final savings decision, which, importantly, could differ from their soft-commitment. There was, however, a penalty for deviating, i.e. the forgone commitment reward. The SaveUPFront Accounts were similarly funded via direct deposit from the income tax refund and had a similar maturity horizon of 8 months. Additional survey data was likewise collected from members of the commitment treatment groups when taxes were filed.

A member of the commitment control arm had the opportunity to receive up to $425 in saving and commitment incentives — $100 for following through on a soft-commitment to save and $325 for saving the maximum of $950. A member of the commitment treatment arms could receive up to $475 in incentives — which includes an extra $50 over the control arm just for having soft-committed to saving.

3 Data

As mentioned earlier, our sample is the pool of prior year tax clients at the collaborating non-profit, the Financial Clinic. We restrict the study to tax filers who had a federal income tax refund above $300 and who spoke either English or Spanish. This resulted in an initial pool of 833 potential study participants.

We collect a range of administrative and survey data. We have access to administrative tax return data from IRS Form 1040. This includes filing status, age, adjusted gross income (AGI), unemployment insurance, number of dependents, earned income tax credit (EITC), child tax credit, withholdings, tax liability, federal and state income tax refund, and whether direct deposit was used to receive a refund. We use data for both the prior tax year (2009) and the current one (2010), which are collected between February and April (tax-filing season) in 2010 and 2011, respectively. Additional client intake data for each tax-filing season are collected by the collaborating non-profit. These data include gender, race/ethnicity, primary language, marital status, highest level of education, zip code, tax filing date, tax preparation site location and type of bank accounts owned. We have prior year data — tax year 2009 data collected during the tax filing season of 2010 — for all baseline participants and only have current year data — tax year 2010 data collected during tax filing season 2011 — for those participants that return to file their taxes with the non-profit in 2011. Soft-commitment decisions for the commitment group members are recorded during the initial phone call in late 2010, while saving decisions and saving amounts are recorded at the tax site in early 2011.

During our initial phone interviews, which happen in the winter of 2010 prior to the current tax filing season, we collect data on savings and debt levels. Following Lusardi, Schneider, and Tufano (2011), we measure credit constraints by asking participants the likelihood of unexpectedly owing $2,000, the ability to come up with this amount of cash and the potential source of this cash. Finally, after explaining the SaveUp and SaveUpFront accounts to participants, we quiz them on the parameters of the savings vehicle, to measure comprehension. During the initial phone survey, the pre-commitment decision is also recorded. In our follow up survey, which takes place during the current tax filing season, we repeat questions regarding savings, debt, credit constraints and comprehension.

3.1 Descriptive Statistics

Table 1 provides descriptive statistics across the six treatment groups for a range of pre-intervention characteristics. As can be seen, the treatment groups are balanced at the onset. The average age is about 41, and the sample is two-thirds female. The over-representation of females is in part due to income restrictions on free tax preparation clients and the concentration of EITC benefits among single female-headed households. For this same reason, average income is low, at $17,600 and the share married is only 11 percent. The average federal refund is $2,500 and state refund is $666, which together is well above the maximum allowable SaveUp deposit. About one-half of the sample has a high school equivalent degree, but only 4 percent have a college degree or higher. Nearly half of the sample is African-American, another 30 percent is Hispanic and only 5 percent is white. The low share of Asian filers, less than 3 percent, is in part due to the sample being restricted to English and Spanish speaking tax filers — the lower Manhattan location of our tax sites results in a majority of Asian filers who do not have English as a first language. Finally, three-fourths of the sample reports some type of bank account.

3.2 Sample Attrition

One challenge to the longitudinal design of the experiment is a high attrition rate. Table 2 shows the likelihood of remaining in the sample at each of the three key stages of the intervention. The first key stage is the pre-tax season phone call (in December) during which consent is obtained and soft-commitment decisions are recorded for the commitment groups. As can be seen, we are able to contact less than one-third of the potential participants over the phone. An additional 13 percent of all respondents are lost due to a failure to obtain consent for the study after being reached on the phone. Finally, another 6 percent are lost between the pre-tax-filing season phone call and the actual appearance at the tax site where final savings decisions are made. We note in Table 2 that survival rates rise considerably when we are able to contact respondents on the phone. From this perspective, the consent rate rises from 15 percent to 51 percent and the share that appears at the tax site rises from 9 percent to 28 percent. In other words, a majority of the drop off in sample size occurs before our first phone call with individuals.

While the high levels of drop-off are less than ideal, this appears to be a structural feature of the population at hand — low income tax filers. Recall that we draw our initial sample from the roster of tax filers at the nonprofit in the previous year. The likelihood of maintaining the same address and contact information among this population from one year to the next appears to be considerably low. Furthermore, the year-to-year turnover at our tax site is high — which is not surprising given that the nonprofit is only one of numerous options for tax filing. Thus, any intervention similarly designed to follow such tax filers over time is likely to face similar rates of attrition.[33]

A more serious threat to our experiment is any difference in attrition across treatment groups, shown in Table 2. Attrition is significantly higher for potential participants in the non-commitment group — arms 1–3 have a significantly lower probability of being reached by phone, consenting to the study (by phone) or appearing on-site at The Financial Clinic, relative to arms 4–6. Recall that prior to being called, participants receive treatment-specific informational mailings. It is possible that differential attrition arises because commitment groups on average receive larger financial incentives than non-commitment groups.

Despite these significant differences across the two broad experimental groupings, attrition within the two broad experimental groups does not significantly differ across sub-arms — arms 1–3 do not differ from each other, and arms 4–6 do not differ from each other. The reader should keep this in mind once we turn to our parameter estimates in Section 6.2. The identification of the key parameters largely relies on comparisons between treatment and control arms within the two major experimental groups — so, differential attrition across the two broad experimental groups is less of a concern. It should be additionally noted that conditional on being contacted by phone, consent rates and on-site appearances are comparable across all six experimental arms. Again, the differences appear to be driven by pre-phone factors, i.e. differences in experimental arm-specific information contained in the initial mailings.

Given that we do not observe statistically significant differences in attrition within each broad experimental arm, we report results below both unconditionally and conditional on non-attrition. Conditioning on non-attrition allows us to examine the direct response to the incentives we present, although it may introduce selection bias in the case of nonrandom attrition. In Section 4.3 we discuss a bounding method that is robust to this potential bias.

[33] Chetty and Saez (2013) find that 73% of tax filers in the Chicago Metropolitan Area return to H&R Block for tax preparation in the second year of their experiment. In our case, the probability should be expected to be much lower, since the market share of The Financial Clinic is significantly less than that of H&R Block.

4 Experimental Results

We now turn to summarizing the reduced form results of the experiment. We begin by describing the main outcomes, soft-commitment and saving, across the experimental groups. We then report results conditional on non-attrition, to better disentangle actual soft-commitment and saving decisions from issues of attrition. In this case, we also make use of bounding methods that allow set identification in the presence of differential attrition and/or in the presence of selection bias induced by the use of estimates conditional on non-attrition.

4.1 Outcomes by Experimental Arm

Table 3 presents the overall Intent-to-Treat (ITT) soft-commitment and saving outcomes across the six experimental arms. We observe a significantly higher probability of soft-committing to save among the early and late commitment treatment arms (arms 5 and 6) relative to the commitment control arm (group 4), which soft-commits at a rate of 5 percent. However, the timing of the incentives does not appear to matter, as the early and late incentive arms soft-commit at a similar rate of 14 percent. We do not observe any statistically significant differences in saving or the saving amount. The point estimates for saving range between 4 percent and 9 percent, with saving among the commitment group slightly higher than the non-commitment group. Finally, the amount of saving among the the early and late incentive, commitment arms (arms 5 and 6) is on average \$48 and \$61, respectively. The saving amount among the remaining experimental arms ranges between \$31 and \$38. While we fail to detect significant differences in most outcomes, the difference across arms in the direct response to incentives is masked by an overall high rate of attrition.

To disentangle attrition from the direct effect of incentives, we report in Table 4 outcomes conditional on being reached by phone and officially entering into the study. Among this group, we can observe an active soft-commitment to save or soft-commitment to not save, in response to the incentives presented to each treatment arm. Those who are not reached by phone might be doing so due to a lack of interest in the savings account, but may also be unreachable to due to unrelated factors, such as a change in address or busy schedule. The rates of soft-committing to save are now scaled up, once we adjust for phone contact. After conditioning on phone consent, we observe similarly significant differences in the probability of soft-committing to save among the early and late incentive arms — 0.69–071 in arms 5 and 6 — relative to the commitment control arm — 0.37 in arm 4. However, we observe the similar qualitative pattern when comparing outcomes — the relative timing of the incentives does not appear to matter. Among this arm, the saving outcomes are an order of magnitude larger after we adjust for phone contact. The commitment group appear to have a slightly higher saving probability — 0.24–0.36 among arms 4–6 — relative to the non-commitment group — 0.13–0.17 among arms 1–3. A similar pattern is apparent when looking at the saving amount, which ranges from $141 to $258 among arms 4–6 and from $96 to $154 among arms 1–3. However, the differences in saving and saving amount are never statistically significant in this table.

In Table 5 we additionally condition on appearance at the tax preparation site. This is done, again, to disentangle additional attrition in the experimental sample between the time of phone contact and tax filing season. Given the level of attrition in our setting, we loose a significant amount of power at this stage. While the probability of soft-committing to save among the early and late incentive arms — 0.56 and 0.64, respectively — remain higher than the that among the commitment control arm — 0.31 — the difference is no longer statistically significant. With respect to saving outcomes, we now observe a statistically significant difference in the probability of saving and the saving amount among the immediate incentive arm — 100 percent and $834, respectively in arm 2 — relative to the non-commitment control arm — 0.43 and $320, respectively in arm 1. The delayed incentive group also has a higher point estimate than the non-commitment control group for saving and saving amount — 0.64 and $478 in arm 3 — but the difference is not as stark as that between the immediate incentive and non-commitment control arms.

4.2 Treatment Effects Conditional on Non-Attrition

As already discussed above, the overall pattern in outcomes is a combination of attrition and active decisions regarding soft-commitment and saving. In order to highlight the direct response to incentives, we estimate treatment effects among those who are present at the relevant stage of decisionmaking.[34] We begin by examining the soft-commitment decision among the commitment group (arms 4-6). Recall respondents can soft-commit to either save or to not save. The key variation here is the additional incentive given for soft-committing to save for treatment arms 5 and 6 — the "early", i.e. February, and "late", i.e. October, incentives, respectively. We estimate OLS regressions of the form

$$C_n = \alpha_C + \gamma_e T_{5,n} + \gamma_l T_{6,n} + \Gamma_C \mathbf{X}_n + \varepsilon_{C,n} \tag{1}$$

where C_n is a dummy that equals one if individual n soft-commits to saving and zero if the individual soft-commits to not saving, $T_{j,n}$, $j \in \{5, 6\}$, are dummies for arms 5 and 6, arm 4 is the omitted group and $\mathbf{X}_n$ is a vector of predetermined covariates. We run these regressions among the individuals who have non-missing soft-commitment outcomes, i.e. those who were successfully reached over the phone. We conduct conventional robust inference as well as randomization inference using 500 replications.

The effect on soft-commitment is shown in the first row of Table 6. The early and late incentives almost double the likelihood of soft-committing to save relative to the control group; the treatment arms increase soft-commitment by 30-35 percentage points from a baseline of 37 percentage points. The treatment effects are statistically distinguishable from zero and are robust to randomization inference.[35] The effect is nearly identical across the early and late treatment arms and is not sensitive to covariate adjustments — columns (2) and (4), respectively. The results suggest that the incentives play a substantively important role in encouraging the soft-commitment to save, but that their relative timing — whether they are to be received in February or October — does not additionally affect agents' choices in December. We also show in Table 6 that attrition rates for the treatment arms are higher, but not in a statistically significant way.

We now turn to the effect on saving among the non-commitment groups. The saving decision is made at the tax site, e.g. in February, and the key variation is an additional incentive to save for treatment groups 2 and 3 — the "immediate", i.e. February, and "delayed", i.e. October, incentives,

[34] We justify conditioning on non-attrition here by relying on the fact that there are generally not statistically significant differences in attrition across treatment and control arms, *within* our broad experimental groups. In Appendix F we present results treating missing soft-commitment and saving decisions as zeros, i.e. we present intent-to-treat (ITT) effects. We additionally bound these effects using the method of Horowitz and Manski (2000).

[35] In seven of the 8 cases $p < 0.05$ and in one case $p < 0.1$.

respectively. We estimates regressions of the form

$$S_n = \alpha_S + \gamma_i T_{2,n} + \gamma_d T_{3,n} + \Gamma_S \mathbf{X}_n + \varepsilon_{S,n}. \tag{2}$$

where S_n is a dummy variable for saving, $T_{j,n}$, $j \in \{2, 3\}$, are dummies for arms 2 and 3, arm 1. is the omitted group and $\mathbf{X}_n$ is again a vector of predetermined covariates. We restrict analysis to those who with non-missing savings decisions, i.e. those who actually arrive at the tax site. We conduct conventional robust inference as well as randomization inference using 500 replications.

Table 7 presents the treatment effect on saving among the non-commitment groups in the first row. In column (1), we see that the "immediate" incentive, group 2, roughly doubles the likelihood of saving, with an increase of 57 percentage points in the saving share, relative to a baseline of 43 percent. This difference is statistically significant and robust to randomization inference.[36] The "delayed" incentive, group 3, likewise appears to increase saving, although the point estimate of 20-23 percentage points is not statistically distinguishable from zero. In this case, the timing of the incentive appears to matter — the point estimate for the immediate incentive, i.e. a February payment considered in February, is between two and three times as large as that of the delayed incentive, i.e. an October payment considered in February. In columns (2) and (4), we see that the points estimates are somewhat sensitive to controlling for observables, but the qualitative pattern remains — the immediate incentive effect is nearly twice as large as the delayed incentive effect. We observe a statistically significant difference in attrition in one case — treatment arm 2 versus the control arm — although the difference is only marginally significant ($p < 0.1$).

4.3 Treatment Bounds

We have focused on treatment effects estimated conditional on non-attrition. From this perspective, the potential outcome for each member of our sample is the soft-commitment or saving decision when exposed to the the relevant treatments and incentives. However, in the case that we are unable to contact the respondent by phone and/or expose her to the treatment offer at the tax site,

[36] In three of four cases $p < 0.05$ and in the remaining case $p < 0.1$.

it seems reasonable to treat her lack of response as a missing data problem. It is important to note that in Tables 6 and 7 the differences in attrition between the treatment and control arms, within experimental groups, are generally not distinguishable from sampling variance at conventional levels. Nonetheless, one may be concerned that the attrition, and therefore missing data, is nonrandom, in which case we cannot obtain a point estimate for our treatment effects. However, set identification is possible via a method proposed by Behaghel, Crépon, Gurgand, and Le Barbanchon (2009), which allows us to bound the treatment effects.[37]

We outline the bounding method in more detail in Appendix C. The bounds apply to a treatment effect among the set of individuals who do not drop out of the sample when offered the immediate/delayed or the early/late incentives. In the parlance of the Local Average Treatment Effect (LATE) literature, we can bound the treatment effect on the set of compliers and always takers, where "take-up" refers to non-attrition. This effect is akin to the treatment on the treated and is therefore a policy relevant one. Importantly, the bounds are valid even if there is a direct effect of the treatment on attrition. The bounds rely on an assumption of monotonicity. That is, the effect of the soft-commitment or saving treatments on attrition are weakly in the same direction for all individuals.

In Table 6 we show that the attrition rate for the commitment treatment arms 5 and 6 is nominally lower than — though within sampling variance of — that of the commitment control arm 4. Intuitively, the prospect of receiving a higher net payout may increase the likelihood of answering the phone and/or consenting to the study. This difference implies bounds on our conditional treatment effects. The bounds on the two treatments for soft-committing to save are very similar, with a lower bound of 10 percentage points and an upper bound of 46 percentage points. In Table 7, we actually have a counterintuitive pattern — the attrition rate is higher among the non-commitment treatment groups. We nonetheless report bounds on the treatment effect on saving. Since the bounds on the treatments for group 2 and 3 overlap, we cannot rule out that they have the same effect on saving. Again, we note that although potentially differential attrition motivates the analysis of these bounds, the differences in attrition are generally not statistically different between

[37] In Appendix F we use an alternative method proposed by Horowitz and Manski (2000) to bound the unconditional ITT effects.

the corresponding treatment and control arms at conventional levels. It also important to note in columns (1) and (2), the lower bound on the treatment effect in experimental arm 2 is negative. We will return to this issue when using these treatment effect bounds to calculate bounds on the discount factors of interest.

5 Model

We now outline a model of soft-commitment and saving behavior, in order to provide a formal link between our experimental design, empirical methodology and the relevant economic theory on time-inconsistency. We model the savings account as an "investment good" as in DellaVigna and Malmendier (2004a) (see also DellaVigna and Malmendier (2002) and DellaVigna (2009) or "immediate cost" activities in O'Donoghue and Rabin (1999)).[38] We begin by specifying preferences and beliefs and then by outlining, for each period, the state space, action space and per-period payoffs. We then present our main results on identifying time-inconsistency.

As outlined in Section 2.3, our identification of time preferences stems from comparing the relative responsiveness to an incentive received earlier in time (e.g. February) to one received later in time (e.g. October) for individuals making a decision in advance (e.g. December) or on the spot (e.g. February). Under the null hypothesis, these comparisons should yield similar results, while standard models of time-inconsistency imply a discrepancy between the two.

A "soft-commitment" decision plays a key role in the model, as it makes decisions prior to tax season incentive compatible. This decision is by design non-binding, reflecting both institutional limitations and the fact that exact refund amounts and liquidity concerns are not known with certainty ex ante. It is important to note, however, that our test of time-consistency is not based on detecting a demand for commitment, as individuals do not choose between being in the commitment or non-commitment groups. Moreover, a subsequent reversal of the commitment is not equivalent to

[38]In mapping the model to the actual field experiment, the discrete choice modeled here is the decision to save at least the minimum amount listed above. Individuals have an additional continuous choice of the amount to save beyond the minimum savings threshold. But note, the immediate and delayed incentives are conditional on the extensive margin decision, and thus map into a discrete choice model. We discuss below how to potentially use the additional continuous choice to estimate time-preferences as in Andreoni and Sprenger (2010).

time-inconsistency in our model. As such, intra-personal comparisons of the commitment decision and actual savings outcome do not constitute our formal test.[39] Along the same lines, a divergence in the levels of aggregate pre-commitment and aggregate savings probabilities are not necessarily equivalent to time-inconsistency in our model, once we allow for uncertainty. Rather, it is the marginal effect of savings incentives relative to these baseline probabilities that is used as described below. To summarize, our test of time-consistency is not based on the demand for nor the direct effect of the soft-commitment on savings probabilities. However, the soft-commitment is a key ingredient in identifying time-inconsistency, as will be shown.

5.1 Preferences and Beliefs

We assume that individuals maximize additively time-separable and stationary — in the sense that the per-period utility function, $u(\cdot)$, is time-invariant[40] — utility functions with potentially quasi-hyperbolic discounting, i.e. "β-δ" preferences (see Strotz (1955b), Phelps and Pollak (1985), Laibson (1997) and O'Donoghue and Rabin (1999)).[41] That is, in period t, the present discounted value of a consumption stream $\{c_s\}_{s=t}^{T}$ is

$$v_t = u(c_t) + \beta \sum_{\tau=t+1}^{T} \delta^{\tau-t} \cdot u(c_\tau).$$

where $\beta < 1$ generates time-inconsistent, and in particular present-biased preferences. In addition, the agent holds beliefs about future objective functions, parameterized by $\hat{\beta}$. That is, in period t,

[39] Though we do not make explicit use of revision behavior, this does not mean that it is uninformative regarding time consistency. We can observe revision behavior based on participant decisions and/or early withdrawal of savings. In addition, we allow for shocks to information regarding the refund level. Giné, Goldberg, Silverman, and Yang (2013) explicitly use commitment revisions to explore nature and presence of time-inconsistent preferences.

[40] In Section 5.3, we actually relax the assumption of time-invariance. In Section 5.4, where we introduce uncertainty, we also allow for shocks to preferences over time that affect the level of saving, which constitute a particular form of time-variance. However, in this case we must also impose time-invariance of the marginal utility of consumption through a quasi-linear functional form for utility in order to gain traction.

[41] Our use of "stationary" and "time-inconsistent" is not to be confused with that of Halevy (2014b). Halevy (2014b) uses the term non-stationary to refer to preferences that exhibit static preference reversals — e.g. preferring one dollar today to two dollars tomorrow while simultaneously preferring two dollars received in $t + 1$ periods to one dollar in t periods. Furthermore, Halevy (2014b) uses the term "time-inconsistent" to refer to preferences that exhibit dynamic preference reversals — e.g. stating today a preference for two dollars at time $t+1$ over one dollar at time t, but stating the opposite when time t arrives. β-δ preferences are both non-stationary and time-inconsistent in the sense used by Halevy (2014b) when $\beta < 1$.

the agent believes that the following objective function will be maximized in period $t+k$:

$$v_{t+k} = u(c_{t+k}) + \hat{\beta} \sum_{\tau=t+k+1}^{T} \delta^{\tau-t} \cdot u(c_\tau).$$

Our null hypothesis is that individuals are time-consistent — i.e. $\beta = \hat{\beta} = 1$. Alternatively, individuals may be present-biased agents, either characterized by complete sophistication — $\beta = \hat{\beta} < 1$ — or naïveté — $\beta < \hat{\beta} \leq 1$ (see O'Donoghue and Rabin (2001)). We do not impose restrictions on the extent of naïveté, i.e. we allow for $\hat{\beta} < 1$. Note that, in period t, all agents discount utility between period $t+k$ and $t+k+j$ by a factor of δ^j, but believe that when period $t+k$ arrives, utility will be discounted by a factor of $\hat{\beta}\delta^j$. When period $t+k$ actually arrives, utility is discounted by $\beta\delta^j$. Note, our method in general allows for more general deviations form time-consistency, i.e. future-bias ($\beta > 1$) as opposed to present-bias ($\beta < 1$). Although, we assume the present-bias formulation in what follows for ease of exposition.

5.2 State Spaces, Action Spaces and Payoffs

We formulate a discrete choice model that takes place over 3 periods. These correspond to our experimental time periods of (approximately) December (pre tax filing season), February (tax filing season) and October (post tax filing season). Choices are made by agents during the first two periods. The key choices are the "soft-commitment" decision (a_1) in period 1 and the savings decision (a_2) in period 2. We model the savings decision as the consumption of an investment good, as in DellaVigna and Malmendier (2004a). When saving, there is an up front cost c incurred in period 2 and a benefit b realized in period 3. Finally, agents make decisions conditional on an experimentally assigned set of incentives (i, d, e, l, p), which correspond to the immediate, delayed, early and late incentives, and the commitment reward, respectively. Each of these experimentally assigned amounts is a payment received by agents in either period 2 or period 3 and their value is a function of the actions a_1 and a_2 as described more formally below and presented visually in Figures A.2–A.6.

5.2.1 Period 1

State Space: $x_1 \in \mathcal{X}_1$, where x_1 are pre-intervention observables that potentially affect the agent's utility in period 1. Note that our experimental incentives (i, d, e, l, p) — the immediate (i), delayed (d), early (e) and late (i) incentives and the commitment reward (p) — are orthogonal to this state variable, due to random assignment.

Action Space: $a_1 \in \{0, 1\}$[42] , where $a_1 = 1$ indicates that in period 1 the agent makes a soft-commitment to save in period 2, and $a_1 = 0$ if the agent makes a soft-commitment to *not* save in period 2. Note that both in the model and the experiment we do not allow for an alternative third action in which the agent rejects the first two actions.[43]

Utility Flow in Period 1: To focus only on essential details it is assumed that the individual receives no direct utility flow from actions taken in period 1. All incentives will be derived from the effect on utility flows in periods 2 and 3 as a result of actions taken in period 1.

5.2.2 Period 2

State Space: $a_1 \in \{0, 1\}$, i.e. the soft-commitment decision in period 1. It is straightforward to incorporate additional state variables using standard approaches but those are eliminated here in the interest of brevity. Note that members of the non-commitment group (NC) do not have an opportunity to make a period 1 decision and therefore, the state variable is irrelevant for their period 2 decision.

Action Space: $a_2 \in \{0, 1\}$, where $a_2 = 1$ indicates that the agent chooses to save and $a_2 = 0$ if the agent decides not to save. Recall that members of the commitment option groups are allowed to deviate from their prior soft-commitment.

[42]Note that in the model, we only model actions related to the experimental choices. In principle, agents could make other decisions (e.g. change their saving and/or consumption behavior) in response to the intervention. If there is an opportunity for financial arbitrage, then decisions over monetary payments may not reveal time preferences (Coller and Williams, 1999, see). Cubitt and Read (2007) and Augenblick, Niederle, and Sprenger (2014) argue that time preferences ought to be estimated using primary rewards, not monetary ones, although Halevy (2014a) provides a defense of monetary rewards. In addition, Meier and Sprenger (2010), who also study time preferences among lower-income tax filers at a free tax-preparation nonprofit, suggest that arbitrage is not a great concern with experimental returns such as ours, 50% over 8 months. We further assume that individuals in our sample face credit constraints, in which case the saving decision during tax season is an inter-temporal utility trade-off, rather than a mere arbitrage opportunity.

[43]It can be shown that this third action is weakly dominated.

Utility Flow in Period 2: Payoffs in the second period vary by experimental sub-group. We have two broad experimental groups: (i) the "commitment" group (C) and (ii) the non-commitment group (NC). The former makes both a soft-commitment and saving decision, while the latter only makes a saving decision. Within experimental group C we have three sub-arms: (i) "early" incentive, (ii) "late" incentive and (iii) commitment control arm. Similarly, within experimental group NC we have three sub-arms: (i) "immediate" incentive, (ii) "delayed" incentive and (iii) non-commitment control arm.

In the NC "immediate" incentive arm, agents receive a payment of i if they decide to save, representing the immediate savings incentive. This is the additional \$50 incentive for saving, which is contrasted with an equivalent "delayed" incentive received in period 3 by the NC "delayed" incentive arm.

In the group C "early" incentive arm, if the agent has chosen to soft-commit to saving — $a_1 = 1$ — she receives a payoff e, *irrespective* of her actual savings decision. This is referred to as the early incentive for soft-committing to save, which contrasts with the incentive in period 3 received by agents in the "late" incentive arm. Finally, agents in all arms are assumed to pay a cost c — i.e. the cost of saving c dollars, including a binding borrowing constraint — if they decide to save.

Payoffs for period 2 are summarized in Panel A of Table 8. Flow utility is denoted as a function u of the payoffs. For present purposes, the model will deal mostly with the case where $u(\cdot)$ is the identity function, i.e. utility is quasi-linear.[44] Note in column (2) of Panel A that $u_2^{NC}(\cdot)$, i.e. the utility flow for a member of the non-commitment group, is invariant to the state variable a_1, since there is no soft-commitment decision for the non-commitment group. However, $u_2^{NC}(\cdot)$ does depend on a_2 — the immediate incentive, i, is received in return for saving. In contrast, flow utility for members of the commitment group in column (3) of Panel A, $u_2^{C}(\cdot)$, depends on both a_1 and a_2. In particular, when a soft-commitment is made, i.e. $a_1 = 1$, the early savings incentive e is received. Importantly, the early incentive is received *irrespective* of the final saving decision a_2 (as is the late incentive in period 3). In both columns, a cost c of saving is incurred for all agents that save, i.e. when $a_2 = 1$.

[44] This type of quasi-linearity is found in other models that analyze present-biased preferences, e.g. DellaVigna and Malmendier (2002). We consider relaxing this assumption below in Section 5.5.

5.2.3 Period 3

State Space: $(a_1, a_2) \in \{0,1\} \times \{0,1\}$, i.e. the actions taken in periods 2 and 3.

Action Space: Agents take no action in this period and payoffs are a function of group status and state variables.

Utility Flow in Period 3: Period 3 payoffs are illustrated in Panel B of Table 8. In the non-commitment group (column (2)) if the agent has not saved in period 2, i.e. $a_2 = 0$, then she receives a payoff of 0. If she has saved and is in the "delayed" incentive arm, she receives a savings incentive of d referred to as the "delayed" payoff. In the commitment group, the agent receives a payoff of p if the soft-commitment and actual savings decision coincide, i.e. $a_1 = a_2$. That is, if she has soft-committed to not saving and fulfills that commitment in period 2 or if the agent has soft-committed to saving in period 1 and follows through with that commitment, she receives the payoff p. For members of the "late" incentive arm in group C, a payoff l is received if $a_1 = 1$ *irrespective* of her period 2 saving decision a_2 (as was the early incentive in period 2). This is the "late" incentive for soft-committing to save. Finally, agents in all groups who chose to save in period 2 receive a payoff of b, which is in effect the value of the amount now available to be withdrawn from the savings account.[45]

5.2.4 Discussion

Examining Table 8 one can get a general sense of the source of identification. First, looking at the column (2) for $u_3^{NC}(\cdot)$ one will notice that the incentives (i, d) vary depending on the decision in period 2, a_2 and are by construction invariant to a_1, since members of the NC group do not make soft-commitments. Thus, by observing the response of a_2 to experimental variation in (i, d), one learns about period 2 preferences over utility in period 2 (i) relative to utility in period 3 (d). In contrast, one can see in column (3) for $u_3^{C}(\cdot)$, (e, l) vary depending on the decision in period 1, a_1, but are invariant to period 2 decisions, a_2. Thus, by observing the response of a_1 to experimental variation in (e, l) one learns about period 1 preferences over utility in period 2 (e) relative to utility in period 3 (l). Finally, the comparison of the preferences in period 1 and period 2 provide the

[45] This amount b is inclusive of the variable portion of the savings match, i.e. the 50% on each dollar above \$300.

grounds for testing for time-consistency. Note also that in column (3), Panel B of Table 8 the commitment reward p depends on both the period 1 decision and the period 2 decision. All things equal, soft-committing to save makes saving more likely to occur, while soft-committing to *not* save makes not saving relatively more preferable. In this sense, it is the mechanism by which period 1 decisions can alter period 2 decisions and is necessary for making period 1 decisions nontrivial — without $p \geq l$ all commitment group agents would choose $a_1 = 1$.[46]

5.3 Indentifying Time-Inconsistency Under No Uncertainty

We first solve the model assuming that individuals know in period 1 the costs (c) and benefits (b) of saving in period 2. We will allow for unrestricted heterogeneity in the costs and benefits across agents and the joint distribution of (c, b) is given by the unknown CDF $G(c, b)$.[47] In this case, we can test for time-inconsistency without variation in (i, d, e, l). Therefore, we suppress all of these parameters. Furthermore, we do not need to impose quasilinearity nor time-invariance on the flow utility function $u(\cdot)$. We will reintroduce these instruments and the quasilinear assumption in the next section, where identification in the presence of uncertainty requires it. In Appendix B.1 we derive the following result:

Proposition 1 (Identifying Time-Inconsistency Under No Uncertainty)**.**

If an agent is time-consistent (TC), we have the following prediction for soft-commitment and saving outcomes across the commitment option (C) and non-commitment (NC) option groups:

$$\mathbb{E}\left[a_1^C|TC\right] = \mathbb{E}\left[a_2^C|TC\right] = \mathbb{E}\left[a_2^{NC}|TC\right].$$

[46] Again, note that the discrete decisions a_1 and a_2 are the decisions to soft-commit to save or to actually save above the minimum savings threshold or not. The incentives (e, l) and (i, d) are the incentives received for soft-committing to save or actually saving more than these thresholds. The variable part of the savings match is proportional to the amount saved above the minimum and below the maximum savings deposit and is for convenience collapsed into the (c, b) variables in the model.

[47] When there is no uncertainty, we can actually allow for unrestricted heterogeneity in marginal value of the commitment reward — i.e. $p_n \lesseqgtr p$ — and in the value of the discount parameters $(\beta_n, \beta_n, \delta_n)$. Our results still hold, although we suppress the subscript n for convenience in exposition.

If an agent is present-biased and sophisticated (PBS) and the soft-commitment reward, p, is "sufficiently strong," as defined in Definition B.1, then we have:

$$\mathbb{E}\left[a_1^C|PBS\right] = \mathbb{E}\left[a_2^C|PBS\right] > \mathbb{E}\left[a_2^{NC}|PBS\right].$$

Finally, if an agent is present-biased and naïve (PBN) and the reward p is both "sufficiently strong" and "sufficiently weak," as defined in Definition B.1, then we have:

$$\mathbb{E}\left[a_1^C|PBN\right] > \mathbb{E}\left[a_2^C|PBN\right] > \mathbb{E}\left[a_2^C|PBN\right].$$

Intuitively, when the agent is time-consistent, there is no inaccuracy in beliefs nor is there any conflict in preferences over time. Therefore, when the agent soft-commits to saving, her actions in period 2 are aligned, i.e. $\mathbb{E}\left[a_1^C|TC\right] = \mathbb{E}\left[a_2^C|TC\right]$. Furthermore, the presence of the soft-commitment is not used to alter outcomes, i.e. $\mathbb{E}\left[a_2^C|TC\right] = \mathbb{E}\left[a_2^{NC}|TC\right]$. Similarly, the sophisticated, time-inconsistent agent has accurate beliefs, and thefefore only soft-commits to saving when such an action will be born out, i.e. $\mathbb{E}\left[a_1^C|PBS\right] = \mathbb{E}\left[a_2^C|PBS\right]$. However, when given the opportunity, the sophisticated agent makes use of the soft-commitment to steer outcomes toward more saving: $\mathbb{E}\left[a_2^C|PBS\right] > \mathbb{E}\left[a_2^{NC}|PBS\right]$. Finally, when the agent is naïve and time-inconsistent, she is overconfident in her ability to save in period 2: $\mathbb{E}\left[a_1^C|PBN\right] > \mathbb{E}\left[a_2^C|PBN\right]$. Nonetheless, her naïvete serendipitously increases the likelihood of saving when the soft-commitment is available: $\mathbb{E}\left[a_2^C|PBN\right] > \mathbb{E}\left[a_2^{NC}|PBN\right]$.

Using aggregate outcomes, we can detect present-biased agents in the population whenever $\mathbb{E}\left[a_2^C\right] > \mathbb{E}\left[a_2^{NC}\right]$ and can further detect naïve agents whenever $\mathbb{E}\left[a_1^C\right] > \mathbb{E}\left[a_2^C\right]$. The sufficient strength and weakness of the soft-commitment reward p is explained in Definition (B.1) and is related to the support of the preferences (c, b) given the magnitude of p. In short, as p grows, the ability to distinguish between time-consistent and time-inconsistent agents increases, while the ability to separately identify naïve and sophisticated agents decreases.

5.4 Identifying Time-Inconsistency Under Uncertainty

We now relax the assumption that agents know (c, b) in period 1. Revisions in planning (i.e. $a_1 \neq a_2$) are no longer sufficient to identify time-inconsistency as uncertainty about period 2 costs and benefits (c, b) may well generate this phenomenon, even for time-consistent agents. Thus, it will be necessary to introduce our full set of experimental instruments (i, d, e, l, p) in order to identify time-inconsistency.

We make the following econometric assumptions. Agents are indexed by the variable n. To capture uncertainty over time, we assume that individuals do not know in period 1 the precise values of the cost and benefits of savings (c, b); rather, they know the joint distribution of these parameters with CDF $G_n(c, b)$ at the individual level. One can imagine, for example, that individuals do not know exactly what their income tax refund will be, which generates uncertainty over c_n and b_n, but that each agent still has some private information predictive of the refund.[48] The subscript n allows for ex-ante cross-sectional heterogeneity in preferences. Because c and b will be unobservable to the researcher, we will make use of the average CDF of savings preferences $G(c, b) = \int G_n(c, b)\,\mathrm{dF}(n)$, where $F(n)$ is the CDF of the "type" n.

In period 2, c and b are revealed to the agent, but are still unobservable to the researcher. We will also make a structural assumption that the utility function u is the identity function, i.e. preferences are quasi-linear. Using these assumptions, we solve the model by backward induction in Appendix (B.2). Our results are summarized below:

Proposition 2 (Identifying Time-Inconsistency Under Uncertainty)**.**

Given our experimental payments (i, d, e, l, p) *we can identify* $\beta\delta$ *among the non-commitment option (NC) group members using variation in* (i, d) *as follows:*

$$\frac{\partial \mathbb{E}\left[a_2^{NC}\right] / \partial d}{\partial \mathbb{E}\left[a_2^{NC}\right] / \partial i} = \beta\delta$$

[48] For example, in period 1, an individual who plans to save the entire refund may know that the cost will be $c_n = \bar{c}_n + \varepsilon_n$ and benefit will be $b_n = Rc_n$, where ε_n is a mean-zero shock to the refund level and R is the gross return of the savings account. Indeed, we remind the study participants during our phone interview of their prior year income tax refund, which is predictive of future refunds.

Furthermore, we can identify δ among the commitment option (C) group members using variation in (e, l) as follows:

$$\frac{\partial \mathbb{E}\left[a_1^C\right] / \partial l}{\partial \mathbb{E}\left[a_1^C\right] / \partial e} = \delta.$$

Thus, we use the four reduced form parameters estimated in equations (1) and (2) to recover time preference parameters. In particular, we have the following mapping:

$$\left(\gamma_i, \gamma_d, \gamma_e, \gamma_l\right) = \left(\frac{\partial \mathbb{E}\left[a_2^{NC}\right]}{\partial i}, \frac{\partial \mathbb{E}\left[a_2^{NC}\right]}{\partial d}, \frac{\partial \mathbb{E}\left[a_1^{C}\right]}{\partial e}, \frac{\partial \mathbb{E}\left[a_1^{C}\right]}{\partial l}\right)$$

5.5 Allowing for Risk Aversion

We discuss one method for relaxing the assumption of quasilinearity. In this case, however, we are only able to estimate the composite parameter $\beta\delta$ and must return to the case of certainty. Nonetheless, we can still compare these results to those obtained using the methods developed above in Section 5.4 that separately identify β and δ. Our model in Section 5.4 above focuses exclusively on the discrete decisions to soft-commit to saving and to open a SaveUp account. However, once an account is opened, study participants must also decide how much to deposit in the account, subject to our maximum deposit rules. We make use of these continuous outcomes by applying the convex time budget (CTB) method used in Andreoni and Sprenger (2010) to our context. This method allows one to simultaneous estimate time preferences and risk preferences, using a continuous saving decision. In Appendix D we discuss how this method is applied in our context. While we do not have the same variation as in the original application in Andreoni and Sprenger (2010), we show that our experimental variation allows us estimate a moment that is a function of the composite discount factor $\beta\delta$, a risk preference parameter γ and the growth rate of per-period income flows $\triangle w$ between February and October. We therefore fix $(\gamma, \triangle\omega)$ and back out an estimate of $\beta\delta$ given our data on saving amounts. As we vary the value of $(\gamma, \triangle w)$, we can asses the sensitivity of our results to curvature in the utility function and non-uniform income flows.

6 Estimation Results

6.1 Testing for Time-Inconsistency Under No Uncertainty

While we generally expect uncertainty to play a role in our experimental context, we begin by reporting the results related to the predictions in Proposition 1. If it is indeed the case that agents are fully aware of the costs and benefits of saving in an illiquid account prior to the tax season, then we can conduct a nonparametric test of time-consistency versus present-biased preferences. Table 9 reports the mean soft-commitment and saving outcomes for the commitment (C) and non-commitment (NC) groups. In the first row, the means are calculated conditional on non-attrition at the relevant stage of the experiment. We see that the levels of soft-commitment and actual saving are roughly equal within and across the two groups — we are not able to rule out time-consistency. Note that if saving is correlated with survival, this may create a bias in the conditional outcomes. In the second row, we instead present results among a balanced panel — that is, we restrict attention to commitment group and non-commitment groups members who are both reached by phone and present at the tax site. In this case, we do observe patterns suggestive of present-biased, as outlined in Proposition 1. If there is uncertainty present however, these tests are not conclusive. We therefore turn to estimates of β and δ below that account for uncertainty.

6.2 Estimating β and δ Under Uncertainty

Using the methods outlined above in Section 5.4, we use the four estimated treatment effects in Tables 6 and 7 to recover time preference parameters. The point estimates are combined by jointly estimating equations (1) and (2) and then using the delta method for nonlinear combinations of the coefficients. In Table 11 we present point estimates for β and δ. These results are conditional on non-attrition at either the phone interview or tax site stage. The estimates for δ are very close to 1. The estimates of β are relatively small in magnitude, between 0.34 and 0.45. In Column (3), we can rule out a value of $\beta = 1$ with our 95 percent confidence interval.

By conditioning on participation, we are implicitly assuming that attrition was not differentially affected by experimental arm. It is the case that attrition is not statistically different across experimental arms within our two broad experimental groups, as can be seen in Table 2. However, if differential attrition is a valid concern, our results will be biased. As discussed above, we can potentially use the treatment effect bounds discussed in Section 4.3 to bound our parameter estimates.[49] There is, however, one setback. As mentioned earlier, the lower bound in Table 7 for γ_i, the "immediate incentive" treatment effect, is negative. Our model, however, requires of to impose non-negativity on the treatment effects, or else the discount factors we estimate may be negative. We therefore use the point estimate of γ_i instead of its lower bound, when bounding β in Table 11.[50] The bottom two rows of Table 11 present our upper and lower bounds for β and δ. In this case, we can no longer rule out a value of $\beta = 1$ with our upper bounds.[51]

6.3 Treatment Group Reassignment and Additional Enrollment

As we have discussed previously, the high turnover setting in which we conduct this experiment leads to a fair amount of attrition. We take two additional measures to mitigate this sample attrition. First, some individuals are initially assigned to one of our six treatment groups are sent the initial mailing, fail to be reached by phone prior to the tax season, but do appear at the tax site to file their taxes. In cases where we identify these individuals, they are randomly reassigned to one of the 3 non-commitment groups. In addition, to augment the sample of individuals making on site savings decisions, we enroll a small set of walk-up tax filers, for whom we did not have contact information prior to the tax season. They are similarly randomized into one of the three non-commitment groups.

We cannot enroll any of these individuals into the commitment groups, since that group requires a decision to be made prior to the tax filing season. These additional participants are given little to

[49] We are only able to calculate partial bounds, since, theoretically, our model only allows for strictly positive treatment effects. Thus, we can only bound δ correctly. The lower bounds for group 2 in Table 7 become negative. Thus, to bound β, we use our point estimate for β in combination with the upper or lower bounds for δ.

[50] Specifically, the upper bound on β is calculated as

$$\beta^{UB} = \left(\gamma_d^{UB} \cdot \gamma_e^{UB}\right) / \left(\gamma_i^{LB} \cdot \gamma_l^{LB}\right).$$

When $\gamma_i^{LB} < 0$, we get a negative value for β^{UB}. In this case, we use the point estimate for γ_i, instead of γ_i^{UB}. This creates a downward bias in the upper bound.

[51] In Appendix Table F.6 we report estimates of β and δ using the unconditional ITT effects estimated in Tables F.4 and F.5. In this case, we still estimate δ close to 1, but the estimates of β are highly unstable due to the near-zero treatment effect point estimates in Table F.5.

no information prior to the tax season regarding the savings opportunity and therefore may therefore differ from the members of our main sample. However, we can pool together saving outcomes between this sample and our main sample, in order to gain more power. Table 10 we report the immediate and delayed treatment effects using the pooled sample of non-attrited members of group 2 and 3, reassigned individuals and on-site enrollees. Among this expanded sample, we observe a higher baseline rate of saving in group 1, 52%. Given the bounded nature of the outcome variable, our treatment effects scale down. Nonetheless, we observe a familiar pattern — the immediate incentive treatment effect, 36 percentage points, is much larger than the delayed incentive effect, 7 percentage points. In Table 12 we estimate β and δ among expanded sample. The estimates of β are now somewhat lower — 0.19–0.18. Once again, the upper bound on β does not allow us to rule out $\beta = 1$.

6.4 Estimates Accounting for Risk-Aversion

As discussed in Section 5.5, we can make use of the continuous saving amount decision to provide alternate estimates of time preferences while accounting for risk preferences.[52] In Table 13 we report estimates of $\beta\delta$ for various values of $(\gamma, \triangle\omega)$ — these are the coefficient of relative risk aversion and the growth rate of income flows in February relative to October, respectively. The risk parameter is varied from 1-4, while the growth in income varies between 0%, 10% and 25%. As one may imagine, the fact that individuals are not always saving the maximum amount in the presence of a marginal return of 50% generates a significant level of estimated impatience. Each estimate is tested against the null hypothesis of $\beta\delta = 1$ and $\beta\delta$ is well below 1 in all cases.

In this context low saving may be caused by the fact that higher income flows are expected in October, relative to February. Moving along columns (1)–(3) from left to right, as we assume a larger discrepancy in income flows we estimate slightly less impatience. However, the estimates do not move much — we would need a very high imbalance in income flows to entirely explain away the low saving rates. Saving may also be influenced by risk aversion. In particular, as we increase the value of the risk aversion parameter, we actually need more impatience to fit the data

[52] We explain this method in Appendix D.

— individuals should be saving much more than we observe. Rather than mitigating the finding of high impatience, allowing for risk aversion makes it even harder to explain why individuals do not save the maximum amount. In columns (4)–(6) we directly incorporate those at corner solutions. In our data, there are more individuals at the minimum savings amount than at the maximum suggesting our estimates of $\beta\delta$ were biased up when only using interior savers. The point estimates, however, do not change by a great deal.

With the CTB method, we are not able to test for time-consistency, as we only collect the continuous savings decision at one point in time. Therefore, we cannot directly compare these results to our previous results. However, over reasonable levels of risk aversion — $\gamma \in \{1, 2\}$ — we estimate values for $\beta\delta$ between 0.20 and 0.49. These estimates are generally in line with our estimates of $\beta\delta$ in Tables 11 and 12, which range from 0.20 to 0.52. As mentioned above, our estimate of $\beta\delta$ can be as low as 0.06 at the highest level of risk aversion used, e.g. $\gamma = 4$. Since we cannot separately identify β and δ, we theoretically cannot rule out a value of $\beta = 1$. However, as pointed out by Rabin and O'donoghue (2007), attributing the observed discounting to a simple exponential discounting function, i.e. loading all the discounting on δ, places extreme restrictions on the amount of long-term patience that can simultaneously explain our data.[53] Thus, if we believe that the long-term discount factor must be reasonably close to 1, then our evidence from even the cross section is suggestive of present-bias.

6.5 Alternative Explanation: Information Shocks in February

Our estimates of β and δ are driven by the general patterns of our reduced form treatment effects — the immediate incentive (Table 7, columns (1)–(2)) generates a stronger response in saving relative to the delayed incentive (Table 7, columns (3)–(4)), and this difference is much greater than a similar comparison of the early (Table 6, columns (1)–(2)) and late Table 6, columns (3)–(4)) incentives. In other words, we find $\gamma_i - \gamma_d > \gamma e - \gamma_l$ when estimating equations (1) and (2). This is contrasted with what we would predict under the null of time-consistent preferences — given that both pairs

[53]Specifically, if we calibrate an 8-month discount factor of 0.35 — the midpoint of our estimates — within an exponential discounting framework, this implies that individuals prefer a dollar of consumption today to nearly 7 million times as much in ten years — i.e. $\delta_{10-Years} = 0.35^{120/8} \approx 0.00000001$.

of payments feature variation in a payment in February or a payment in October, we might expect their relative strength to be similar. Though the evidence is consistent with a model of present-bias, there are alternative stories that may explain the results. In particular, a positive shock to the marginal utility of consumption in February or a negative shock to income flows in February, relative to October, would generate a similar pattern. However, it is important to note that in the aggregate, we find individuals responding much more strongly to the immediate incentive, which implies that shocks to marginal utility need to be systematically skewed toward February relative to October. That is, we would need the shock to not average out over the sample. In addition, an unexpectedly low income flow in February is not sufficient. We also need that a comparable drop in income in October is not expected. Using survey data from a similar group of tax filers in the following tax season, we assess the merits of this phenomenon.

In Table 14 we elicit individuals' estimated income growth between two points in time, February and October. We would like to asses the extent to which individuals receive new information about the flows of income in February relative to October. In particular, we would like to know whether the new information generates a poorer outlook for February relative to October, which is an alternative explanation of the patterns we find. In the first row, we ask individuals in December about February and October income flows. We then calculate the implied growth in income between February and October. As can be seen in Column 1, we do find that people on average expect to earn more in October than in February. In the second row, we ask in February the same set of questions, expected income in February and expected income in October. Here, there seems to have been a slightly more negative realization in February relative to October than was expected. This shock in information may very well play a role in causing aggregate savings outcomes to appear less patient than aggregate soft-commitment outcomes. However, we do not believe that this new information is what primarily drives our results as the difference is not statistically significant. Furthermore, we have shown in Section 6.4 that variation in relative income flows would have to be much larger in order to completely explain our results.

7 Conclusion

Overall, we find evidence that suggests that our study participants have present-biased preferences. First, we compare the effect of an immediate and delayed incentive offered on-the-spot and find the immediate incentive to generate a greater saving response among study participants. This is compared to an early and later incentive, which were offered in advance of the tax season and had treatment effects on soft-commitment decisions that were effectively invariant to the future timing of the payment. These patterns are consistent with a model where individuals have present-biased preferences. Our results suggest that more immediate incentives for saving can have as much as 2–3 times as much an effect on the likelihood of saving. We then turn to estimating the parameters of a β-δ model using our reduced form treatment effects. Our preferred point estimates for β and δ over an 8-month horizon are $0.34 - 0.45$ and $1.08 - 1.15$ respectively. These translate into a one-year discount factor of $0.38 - 0.56$, or equivalently, a one-year discount rate of 164% at the high end or 79% at the low end. As compared to prior structural estimates, our discount rate is higher than that implied in the life-cycle model estimated by Laibson, Repetto, and Tobacman (2007) (49%), closer to the rate among low-wage workers estimated by DellaVigna and Paserman (2005) (153%) and lower than the rate among single-women with children estimated by Fang and Silverman (2009) (238%).

One challenge to estimation in our context is sample attrition. In particular, differential savings rates across treatment groups may be attributed to sample selection bias. We account for this by estimating bounds on our time-preference parameters. Unfortunately, we lose significant precision after this adjustment and are no longer able to rule out a $\beta = 1$. However, it is important to note that our separate estimates of $\beta\delta$ and δ are accomplished within treatment subgroups that do not face statistically significant differences in attrition. We also incorporate alternative methods of estimating time preferences adapted from Andreoni and Sprenger (2010). In this case, we assess how sensitive our results are to departures from the assumption quasi-linear preferences. We find that uneven flows of income between February and October may account for some, but not all of our estimates of impatience. Furthermore, we find that allowing for a greater level of risk aversion may actually imply that we are under-estimating impatience.

As described above, the contributions of the project are both an empirical test of economic theory and the evaluation of financial policy tools used among low-income tax filers. First, we join a relatively recent literature that uses field experiments to test theories of time-inconsistency. We add to this literature by obtaining additional quantitative estimates of the parameters of a quasi-hyperbolic discounting model and elicit these preferences from choices that take place in a"natural" decisionmaking setting. In addition, our method allows us to identify time-preference parameters with potentially less restrictive assumptions regarding naïveté than commonly found in the literature — we allow for partial naïveté.

Our study also takes place in a policy-relevant setting. Policies and research surrounding the financial decisions made upon receiving one's income tax refund are numerous. In this field, comprised of practitioners and researchers, it is commonly thought a prudent idea to save some or all of the income tax refund, hence the prevalence of savings incentive programs. In our study, we remain agnostic as to whether the savings is the optimal decision, but nonetheless aim to contribute to this policy discussion.

Our perturbation of existing savings incentives allows us to compare alternative methods of encouraging savings. In particular, our study suggests that varying the timing of savings incentives can result in a more cost-effective program design. A larger question is whether this savings results in a welfare improvement, at the very least for the participants. On the one hand, income tax refunds are comprised of over-withholdings and lagged transfers, which suggests that they are prime candidates for spending or debt reduction. On the other hand, these lumpy payments may provide a buffer of savings moving forward that is otherwise hard to build up in the presence of self-control problems. Research shows that even at the monthly frequency, lumpy benefits appear to be drawn down too quickly.[54] Even so, placing the buffer in an illiquid account limits self-insurance possibilities. Thus, it remains an empirical question whether placing a portion of the refund in an illiquid account can aid in a more even, intra-annual allocation of the income tax refund and help provide a buffer against late-year shocks by stemming the draw-down of the income tax refund

[54] Shapiro (2005) shows that during the course of a month, food stamp benefits tend to be exhausted at rate that is consistent with impatience, i.e. time-inconsistent preferences.

earlier in the year.[55]

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Table 1: Baseline Descriptive Statistics

	(1)	(2)	(3)	(4)	(5)	(6)
	Non-Commitment Groups			Commitment Groups		
	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6
Age	40.83	40.84	38.47	41.84	41.44	42.80
	[1.24]	[1.14]	[1.21]	[1.27]	[1.18]	[1.13]
Female	0.68	0.64	0.58*	0.71	0.64	0.66
	[0.04]	[0.04]	[0.04]	[0.04]	[0.04]	[0.04]
AGI	15,813	17,459	17,479	17,986	18,234*	18,681**
	[883]	[948]	[1,048]	[1,110]	[971]	[1,059]
Federal Refund	2,157	1,990	1,858	2,132	2,214	2,222
	[178]	[156]	[145]	[175]	[178]	[193]
NY Refund	730	641	554*	713	721	642
	[73]	[69]	[57]	[70]	[83]	[80]
Depedents	0.65	0.54	0.55	0.65	0.71	0.60
	[0.08]	[0.07]	[0.07]	[0.07]	[0.09]	[0.08]
Married	0.10	0.12	0.14	0.14	0.09	0.11
	[0.03]	[0.03]	[0.03]	[0.03]	[0.02]	[0.03]
High School	0.87	0.79*	0.79*	0.89	0.84	0.84
	[0.03]	[0.04]	[0.04]	[0.03]	[0.03]	[0.03]
College+	0.13	0.21*	0.21*	0.11	0.16	0.16
	[0.03]	[0.04]	[0.04]	[0.03]	[0.03]	[0.03]
African-American	0.48	0.56	0.57	0.52	0.51	0.52
	[0.04]	[0.04]	[0.04]	[0.04]	[0.04]	[0.04]
Asian	0.05	0.02	0.01	0.03	0.04	0.03
	[0.02]	[0.01]	[0.01]	[0.01]	[0.02]	[0.01]
Hispanic	0.33	0.33	0.30	0.34	0.31	0.33
	[0.04]	[0.04]	[0.04]	[0.04]	[0.04]	[0.04]
White	0.07	0.06	0.06	0.04	0.04	0.07
	[0.02]	[0.02]	[0.02]	[0.02]	[0.02]	[0.02]
Bank Account	0.78	0.74	0.83	0.76	0.80	0.75
	[0.04]	[0.04]	[0.03]	[0.04]	[0.03]	[0.04]
N	137	139	140	137	140	140

Note: Descriptive statistics for 6 treatment groups are tax year 2009 and tax-filing season 2010 variables established prior to the intervention. Robust standard errors are reported in brackets. One, two and three stars denote statistically significant difference from treatment group 1 at the 10, 5 and 1 percent levels respectively.

Table 2: Experimental Group Survival Rates

	(1)	(2)	(3)	(4)	(5)	(6)
	Non-Commitment Groups			Commitment Groups		
	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6
Reached by Phone	0.20	0.16	0.23	0.31	0.39	0.39
	[0.03]	[0.03]	[0.04]	[0.04]	[0.04]	[0.04]
p-value between	.	0.32	0.62	0.04**	0.00***	0.00***
p-value within	.	0.32	0.62	.	0.21	0.21
Consented to Study on Phone	0.11	0.09	0.13	0.14	0.21	0.20
	[0.03]	[0.02]	[0.03]	[0.03]	[0.03]	[0.03]
p-value between	.	0.52	0.62	0.46	0.02**	0.04**
p-value within	.	0.52	0.62	.	0.13	0.17
Appeared On Site	0.10	0.04	0.08	0.09	0.08	0.13
	[0.03]	[0.02]	[0.02]	[0.03]	[0.02]	[0.03]
p-value between	.	0.06*	0.49	0.84	0.49	0.49
p-value within	.	0.06*	0.49	.	0.63	0.37
Consented to Study on Phone (Conditional on Phone Contact)	0.54	0.55	0.56	0.44	0.54	0.52
	[0.10]	[0.11]	[0.09]	[0.08]	[0.07]	[0.07]
p-value between	.	0.95	0.84	0.44	0.99	0.88
p-value within	.	0.95	0.84	.	0.35	0.45
Appeared On Site (Conditional on Phone Contact)	0.38	0.23	0.30	0.28	0.20	0.30
	[0.08]	[0.08]	[0.08]	[0.07]	[0.05]	[0.06]
p-value between	.	0.20	0.46	0.32	0.06*	0.43
p-value within	.	0.20	0.46	.	0.37	0.79
N	137	139	140	137	140	140

Note: Sample survival rates are the probability of remaining in the study at each stage of the experiment. Two sets of p-values are reported. The "between" p-value measures compares each experimental group to group 1, while the "within" p-value compares either treatment groups 2 and 3 to group 1 or treatment groups 5 and 6 to group 4. One, two and three stars denote statistically significant differences at the 10, 5 and 1 percent level respectively.

Table 3: Outcomes by Experimental Group

	(1)	(2)	(3)	(4)	(5)	(6)
	Non-Commitment Groups			Commitment Groups		
	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6
Pre-commit	0.00	0.00	0.00	0.05	0.14	0.14
	[.]	[.]	[.]	[0.02]	[0.03]	[0.03]
p-value within	.	.	.	.	0.01***	0.01***
Saving	0.04	0.04	0.05	0.06	0.06	0.09
	[0.02]	[0.02]	[0.02]	[0.02]	[0.02]	[0.02]
p-value between	.	0.98	0.81	0.58	0.61	0.15
p-value within	.	0.98	0.81	.	0.96	0.38
Saving Amount	32.74	36.01	37.56	30.56	47.50	60.67
	[14.07]	[15.11]	[14.64]	[11.14]	[19.30]	[18.18]
p-value between	.	0.87	0.81	0.90	0.54	0.22
p-value within	.	0.87	0.81	.	0.45	0.16
N	137	139	140	137	140	140

Note: Table reports the soft-committment outcomes for treatment groups 1-3, and the saving and saving amount outcomes for all treatment groups. Two sets of p-values are reported. The "between" p-value measures compares each experimental group to group 1, while the "within" p-value compares either treatment groups 2 and 3 to group 1 or treatment groups 5 and 6 to group 4. One, two and three stars denote statistically significant differences at the 10, 5 and 1 percent level respectively.

Table 4: Outcomes by Experimental Group, Conditional on Phone Consent

	(1)	(2)	(3)	(4)	(5)	(6)
	Non-Commitment Groups			Commitment Groups		
	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6
Pre-commit	0.00	0.00	0.00	0.37	0.69	0.71
(Conditional on Phone Contact)	[.]	[.]	[.]	[0.11]	[0.09]	[0.09]
p-value within	.	.	.	.	0.02**	0.01**
Saving	0.13	0.17	0.17	0.26	0.24	0.36
(Conditional on Phone Contact)	[0.09]	[0.11]	[0.09]	[0.10]	[0.08]	[0.09]
p-value between	.	0.81	0.79	0.33	0.36	0.08*
p-value within	.	0.81	0.79	.	0.87	0.49
Saving Amount	95.73	154.17	128.22	141.42	212.07	257.93
(Conditional on Phone Contact)	[72.24]	[105.07]	[71.35]	[61.38]	[86.06]	[74.21]
p-value between	.	0.63	0.74	0.62	0.29	0.11
p-value within	.	0.63	0.74	.	0.50	0.22
N	15	12	18	19	29	28

Note: Table reports the soft-committment outcomes for treatment groups 1-3, and the saving and saving amount outcomes for all treatment groups, conditional on initially consenting to the study by phone. Two sets of p-values are reported. The "between" p-value measures compares each experimental group to group 1, while the "within" p-value compares either treatment groups 2 and 3 to group 1 or treatment groups 5 and 6 to group 4. One, two and three stars denote statistically significant differences at the 10, 5 and 1 percent level respectively.

Table 5: Outcomes by Experimental Group, Conditional on Site Appearance

	(1)	(2)	(3)	(4)	(5)	(6)
	Non-Commitment Groups			Commitment Groups		
	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6
Pre-commit	0.00	0.00	0.00	0.31	0.64	0.56
(Conditional on Site Appearance)	[.]	[.]	[.]	[0.13]	[0.15]	[0.12]
p-value within	.	.	.	.	0.09*	0.16
Saving	0.43	1.00	0.64	0.62	0.73	0.67
(Conditional on Site Appearance)	[0.14]	[.]	[0.15]	[0.14]	[0.14]	[0.11]
p-value between	.	0.00***	0.29	0.33	0.12	0.17
p-value within	.	0.00***	0.29	.	0.56	0.77
Saving Amount	320.43	834.33	478.00	322.08	604.55	471.89
(Conditional on Site Appearance)	[114.79]	[113.19]	[129.79]	[83.58]	[179.58]	[97.99]
p-value between	.	0.00***	0.35	0.99	0.17	0.30
p-value within	.	0.00***	0.35	.	0.14	0.23
N	14	6	11	13	11	18

Note: Table reports the soft-committtment outcomes for treatment groups 1-3, and the saving and saving amount outcomes for all treatment groups, conditional on showing up at the tax preparation stie. Two sets of p-values are reported. The "between" p-value measures compares each experimental group to group 1, while the "within" p-value compares either treatment groups 2 and 3 to group 1 or treatment groups 5 and 6 to group 4. One, two and three stars denote statistically significant differences at the 10, 5 and 1 percent level respectively.

Table 6: ITT Estimates Conditional on Phone Consent – Soft-Commitment Decision

	(1)	(2)	(3)	(4)
	Treatment Group 5 (early incentive)		Treatment Group 6 (late incentive)	
Treatment Effect	0.321	0.306	0.346	0.352
	[0.143]**	[0.143]**	[0.143]**	[0.144]**
	(0.143)**	(0.154)*	(0.148)**	(0.144)**
Control Mean	0.368	0.372	0.368	0.372
	[0.113]***	[0.110]***	[0.113]***	[0.110]***
N	76	76	76	76
Controls	No	Yes	No	Yes
Non-Attrition Rate - Phone Consent				
Treatment Effect	0.068	0.069	0.061	0.058
	[0.045]	[0.046]	[0.045]	[0.045]
	(0.045)	(0.044)	(0.047)	(0.044)
Control Mean	0.139	0.140	0.139	0.140
	[0.030]***	[0.030]***	[0.030]***	[0.030]***
Treatment Bounds				
Upper Bound	0.443	0.428	0.459	0.461
	[0.132]***	[0.130]***	[0.134]***	[0.133]***
Lower Bound	0.113	0.099	0.152	0.169
	[0.161]	[0.157]	[0.166]	[0.170]
N	417	417	417	417
Controls	No	Yes	No	Yes

Note: Treatment effects on soft-committing to save for treatment arms 5 and 6 are relative to control arm 4 and conditional on non-attrition — i.e. being contacted for the initial phone interview. Non-attrition rates in arms 5 and 6, relative to arm 4 are estimated among the entire sample. Upper and lower bounds are calculated using methods outlined by Behaghel, Crépon, Gurgand, and Le Barbanchon (2009). Robust standard errors for the treatment effects are reported in brackets and randomization inference standard errors are reported in parentheses. One, two and three stars denote statistical significance at the 10, 5 and 1 percent level respectively.

Table 7: ITT Estimates Conditional on Site Appearance – Savings Decision

	(1)	(2)	(3)	(4)
	Treatment Group 2 (immediate incentive)		Treatment Group 3 (delayed incentive)	
Treatment Effect	0.571	0.431	0.208	0.225
	[0.139]***	[0.178]**	[0.207]	[0.200]
	(0.255)**	(0.235)*	(0.190)	(0.200)
Control Mean	0.429	0.450	0.429	0.450
	[0.139]***	[0.123]***	[0.139]***	[0.123]***
N	31	31	31	31
Controls	No	Yes	No	Yes
Non-Attrition Rate - Site Appearance				
Treatment Effect	−0.059	−0.059	−0.024	−0.026
	[0.031]*	[0.032]*	[0.035]	[0.035]
	(0.030)*	(0.030)*	(0.030)	(0.031)
Control Mean	0.102	0.103	0.102	0.103
	[0.026]***	[0.027]***	[0.026]***	[0.027]***
Treatment Bounds				
Upper Bound	1.353	1.173	0.380	0.410
	[0.713]*	[0.656]*	[0.364]	[0.357]
Lower Bound	−0.015	−0.176	0.079	0.074
	[0.574]	[0.592]	[0.311]	[0.305]
N	416	416	416	416
Controls	No	Yes	No	Yes

Note: Treatment effects on saving for treatment arms 2 and 3 are relative to control arm 1 and conditional on non-attrition — i.e. appearing at the tax site. Non-attrition rates in arms 2 and 3, relative to arm 1 are estimated among the entire sample. Upper and lower bounds are calculated using methods outlined by Behaghel, Crépon, Gurgand, and Le Barbanchon (2009). Robust standard errors for the treatment effects are reported in brackets and randomization inference standard errors are reported in parentheses. One, two and three stars denote statistical significance at the 10, 5 and 1 percent level respectively.

Table 8: Period 2 and Period 3 Payoffs by Experimental Group

	(1)	(2)	(3)
Panel A: Period 2	(a_1, a_2)	$u_2^{NC}(a_1, a_2)$	$u_2^{C}(a_1, a_2)$
State Variable: (a_1)	(1,1)	$u(i-c)$	$u(e-c)$
	(1,0)	0	$u(e)$
	(0,1)	$u(i-c)$	$u(-c)$
	(0,0)	0	0
Panel B: Period 3	(a_1, a_2)	$u_3^{NC}(a_1, a_2)$	$u_3^{C}(a_1, a_2)$
State Variable: (a_1, a_2)	(1,1)	$u(b+d)$	$u(b+l+p)$
	(1,0)	0	$u(l)$
	(0,1)	$u(b+d)$	$u(b)$
	(0,0)	0	$u(p)$

Note: State variables, action spaces and payoffs for each experimental group are explained in detail in Section 5.2.

Table 9: Testing for Time-Inconsistency Under No Uncertainty

	(1)	(2)	(3)
	$\mathbb{E}[a_1^C]$	$\mathbb{E}[a_2^C]$	$\mathbb{E}[a_2^{NC}]$
Conditional on Non-Attrition	0.618	0.667	0.613
	[0.056]	[0.073]	[0.088]
N	76	42	31
Balanced Sample	0.724	0.759	0.613
	[0.084]	[0.081]	[0.089]
N	29	29	31

Note: Mean outcomes for commitment and non-commitment group members. Means conditional on non-attrition for soft-commitment and saving decision are among those reached by phone and those who appear on site, respectively. The balanced sample conditions all means on appearing on site.

Table 10: ITT Estimates Including Expanded Sample – Savings Decision

	(1)	(2)	(3)	(4)
	Treatment Group 2 (immediate incentive)		Treatment Group 3 (delayed incentive)	
Treatment Effect	0.361	0.374	0.073	0.048
	[0.122]***	[0.113]***	[0.159]	[0.161]
	(0.166)**	(0.154)**	(0.145)	(0.152)
Control Mean	0.517	0.519	0.517	0.519
	[0.117]***	[0.120]***	[0.117]***	[0.120]***
N	59	59	59	59
Controls	No	Yes	No	Yes
Non-Attrition Rate - Site Appearance				
Treatment Effect	−0.064	−0.069	−0.028	−0.027
	[0.033]**	[0.031]**	[0.036]	[0.036]
	(0.032)**	(0.033)**	(0.033)	(0.032)
Control Mean	0.106	0.089	0.106	0.089
	[0.026]***	[0.026]***	[0.026]***	[0.026]***
Treatment Bounds				
Upper Bound	1.110	1.995	0.251	0.256
	[0.696]	[1.993]	[0.321]	[0.378]
Lower Bound	−0.442	−1.376	−0.117	−0.176
	[0.734]	[2.143]	[0.337]	[0.397]
N	473	473	473	473
Controls	No	Yes	No	Yes

Note: Treatment effects on saving for armss 2 and 3 are relative to arm 1 and conditional on non-attrition — i.e. appearing at the tax site. Sample includes reassigned group members not reached by phone and a group of individuals enrolled at the tax site. Non-attrition rates in arms 2 and 3, relative to arm 1 are estimated among the entire sample. Upper and lower bounds are calculated using methods outlined by Behaghel, Crépon, Gurgand, and Le Barbanchon (2009). Robust standard errors for the treatment effects are reported in brackets and randomization inference standard errors are reported in parentheses. One, two and three stars denote statistical significance at the 10, 5 and 1 percent level respectively.

Table 11: Beta Delta Estimates, Conditional on Participation

	(1)	(2)	(3)	(4)
	δ		β	
Point Estimates	1.077	1.152	0.338	0.453
	[0.395]***	[0.428]***	[0.301]	[0.375]
N	76	76	107	107
Controls	No	Yes	No	Yes
Upper Bound	4.078	4.645	1.933	2.413
	[5.914]	[7.462]	[2.740]	[3.242]
Lower Bound	0.344	0.394	0.014	0.014
	[0.384]	[0.411]	[0.060]	[0.061]
N	417	417	833	833
Controls	No	Yes	No	Yes

Note: Estimates for δ and β are calculated using the results from Tables (6) and (7). One, two and three stars denote statistical significance at the 10, 5 and 1 percent level respectively.

Table 12: Beta Delta Estimates, Conditional on Participation & Including Expanded Sample

	(1)	(2)	(3)	(4)
	δ		β	
Point Estimates	1.077	1.152	0.189	0.112
	[0.395]***	[0.428]***	[0.374]	[0.335]
N	76	76	134	134
Controls	No	Yes	No	Yes
Upper Bound	4.078	4.645	2.026	1.734
	[5.914]	[7.462]	[3.316]	[3.036]
Lower Bound	0.344	0.394	0.016	0.005
	[0.384]	[0.411]	[0.040]	[0.018]
N	417	417	858	858
Controls	No	Yes	No	Yes

Note: Estimates for δ and β are calculated using the results from Tables (6) and (10). includes reassigned group members not reached by phone and a group of individuals enrolled at the tax site. One, two and three stars denote statistical significance at the 10, 5 and 1 percent level respectively.

Table 13: Estimates of $\beta\delta$ – Accounting for Risk Aversion

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS			Tobit		
	$\triangle\omega = 0\%$	$\triangle\omega = 10\%$	$\triangle\omega = 25\%$	$\triangle\omega = 0\%$	$\triangle\omega = 10\%$	$\triangle\omega = 25\%$
$\gamma = 1$	0.430***	0.451***	0.489***	0.361***	0.378***	0.405***
	[0.055]	[0.060]	[0.069]	[0.057]	[0.062]	[0.071]
$\gamma = 2$	0.277***	0.306***	0.359***	0.196***	0.214***	0.246***
	[0.071]	[0.081]	[0.102]	[0.062]	[0.070]	[0.086]
$\gamma = 3$	0.178***	0.207***	0.263***	0.106***	0.121***	0.150***
	[0.068]	[0.082]	[0.112]	[0.051]	[0.060]	[0.078]
$\gamma = 4$	0.115***	0.140***	0.193***	0.058***	0.069***	0.091***
	[0.059]	[0.074]	[0.109]	[0.037]	[0.045]	[0.063]
N	20	20	20	46	46	46

Note: Estimates for $\beta\delta$ are calculated using the methods described in Section 5.5. One, two and three stars denote statistically significant difference from 1 at the 10, 5 and 1 percent level respectively.

Table 14: Changes in Expected Income Growth

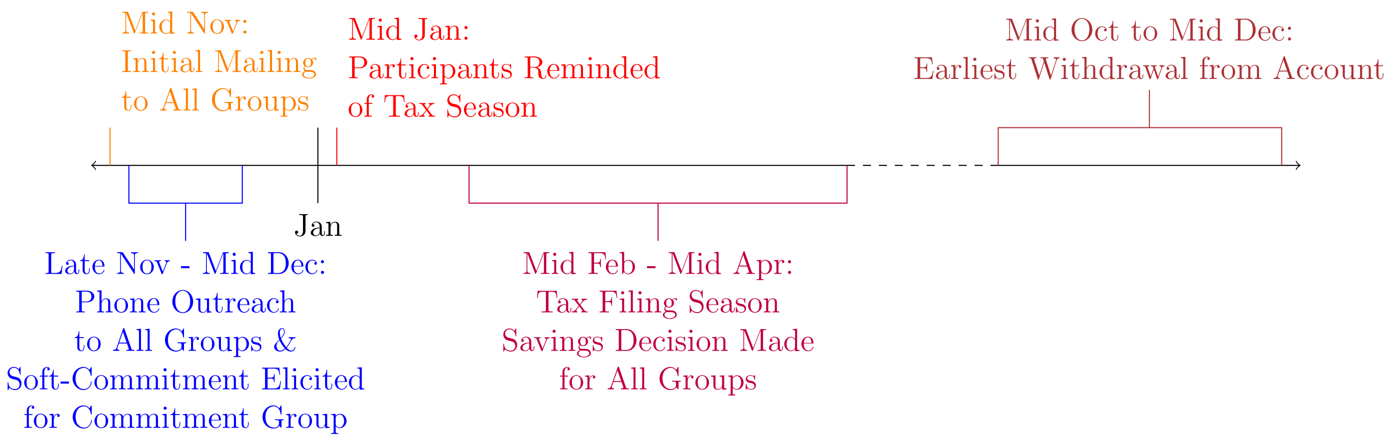
	(1)	(2)	(3)	(4)
	Full Sample	Non-Commitment Group	Commitment Group	Consistent Panel
Expected Growth (DEC)	0.096**	0.058	0.134*	0.066
	[0.046]	[0.047]	[0.079]	[0.043]
Expected Growth (Feb)	0.124***	0.174***	0.104***	0.098***
	[0.025]	[0.059]	[0.027]	[0.036]
Difference	0.028	0.116	−0.029	0.032
	[0.053]	[0.075]	[0.083]	[0.056]
N	225	87	120	88

Note: Expected growth in income is collected via survey during a second year of data collection. All agents are asked about the flow of income in February and October. In the first row, the question is asked prior to the tax season and in the second row the question is asked on site during the tax season.

Appendix A: Additional Figures

A.1 Timeline

Figure A.1: Experimental Timeline



A.2 Extensive Form Representation of SaveUp and SaveUpFront Accounts

The examples below correspond to a tax filer who is deciding whether or not to save $600 of the income tax refund.

Figure A.2: Non-Commitment Control Group (T1)

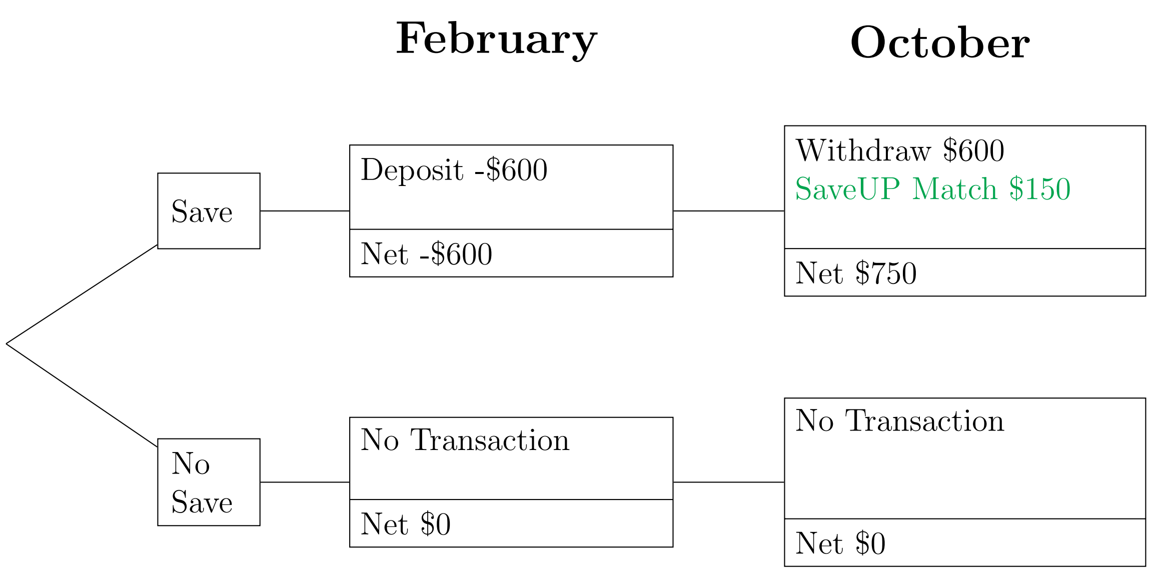


Figure A.3: Immediate Incentive Treatment Group (T2)

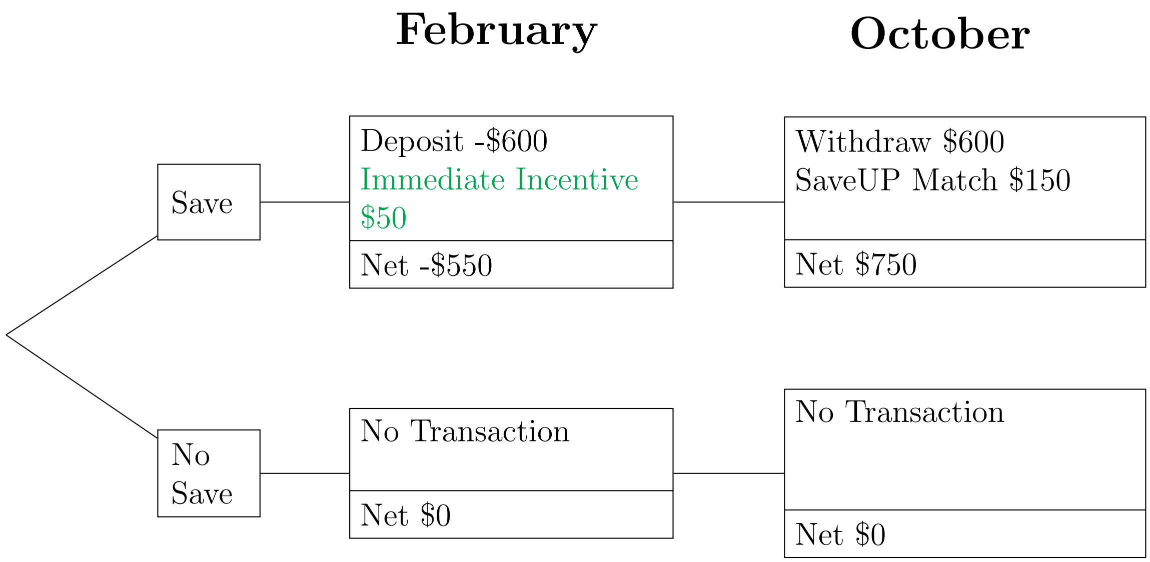


Figure A.4: Delayed Incentive Treatment Group (T3)

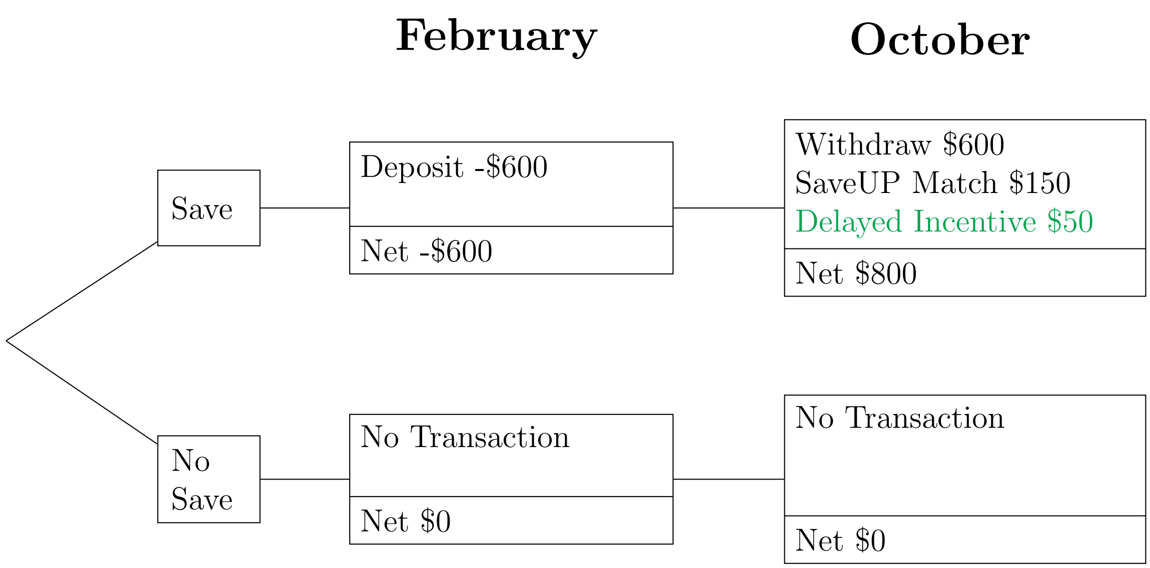


Figure A.5: Commitment Control Group (T4)

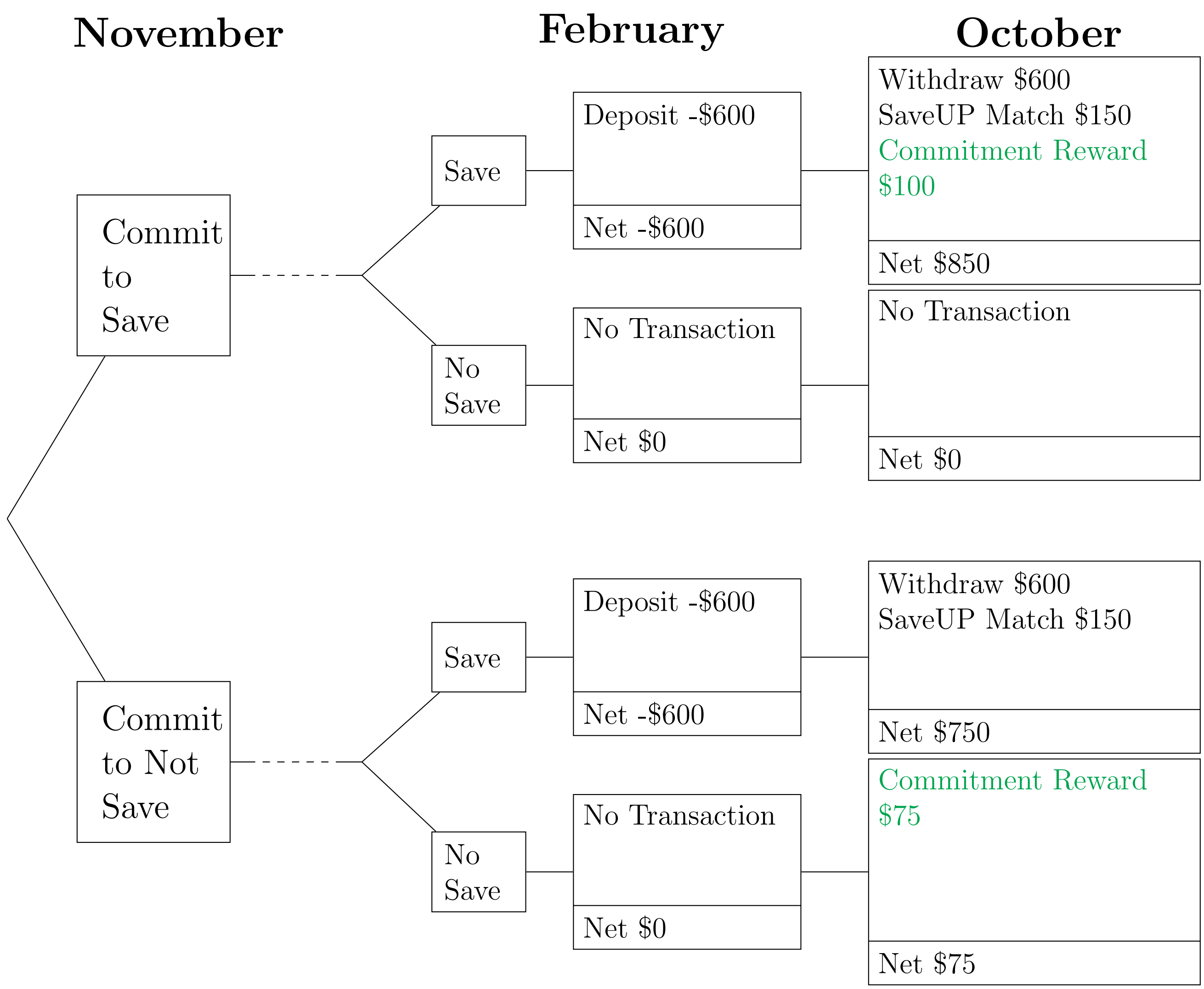


Figure A.6: Early Incentive Treatment Group (T4)

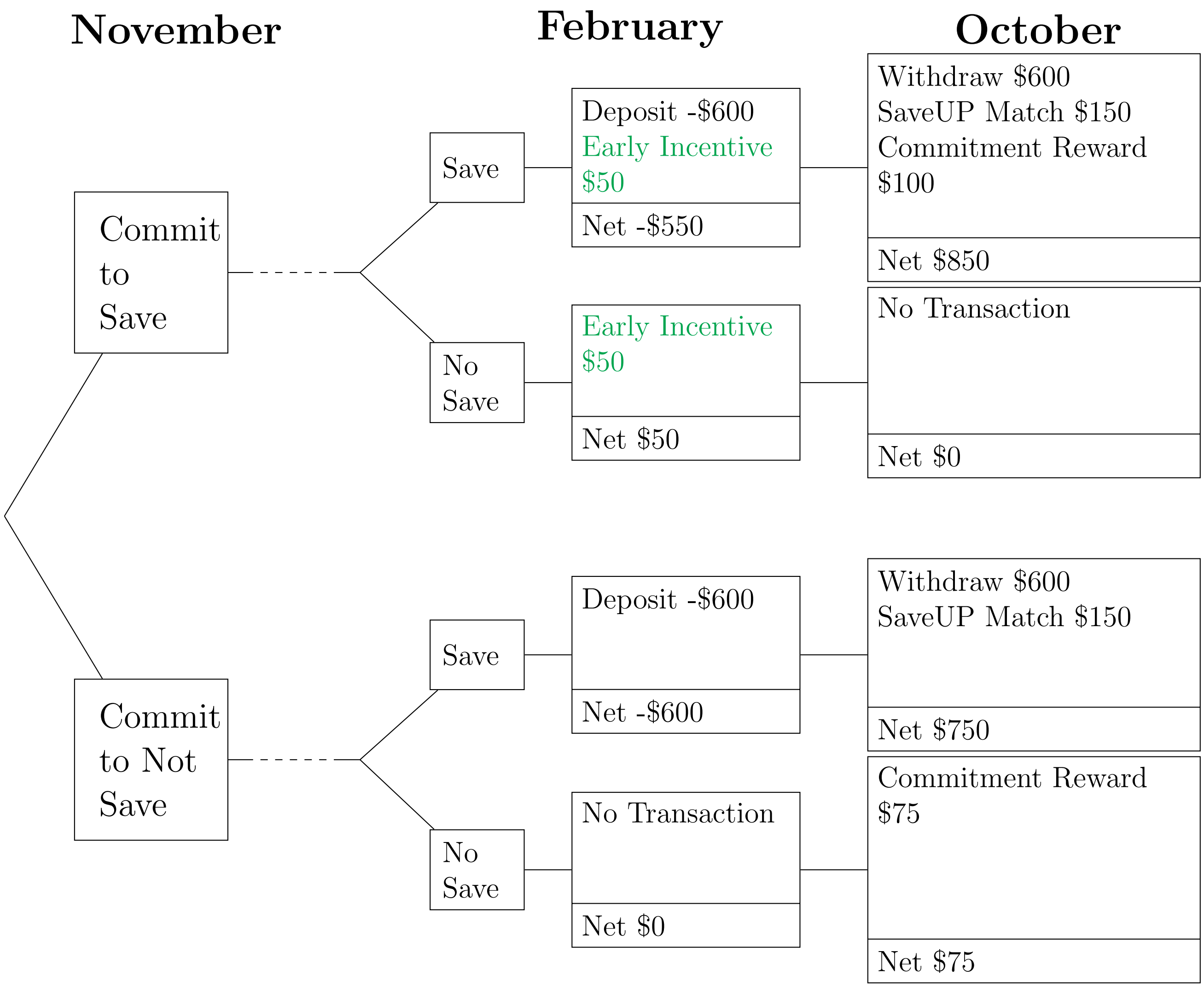
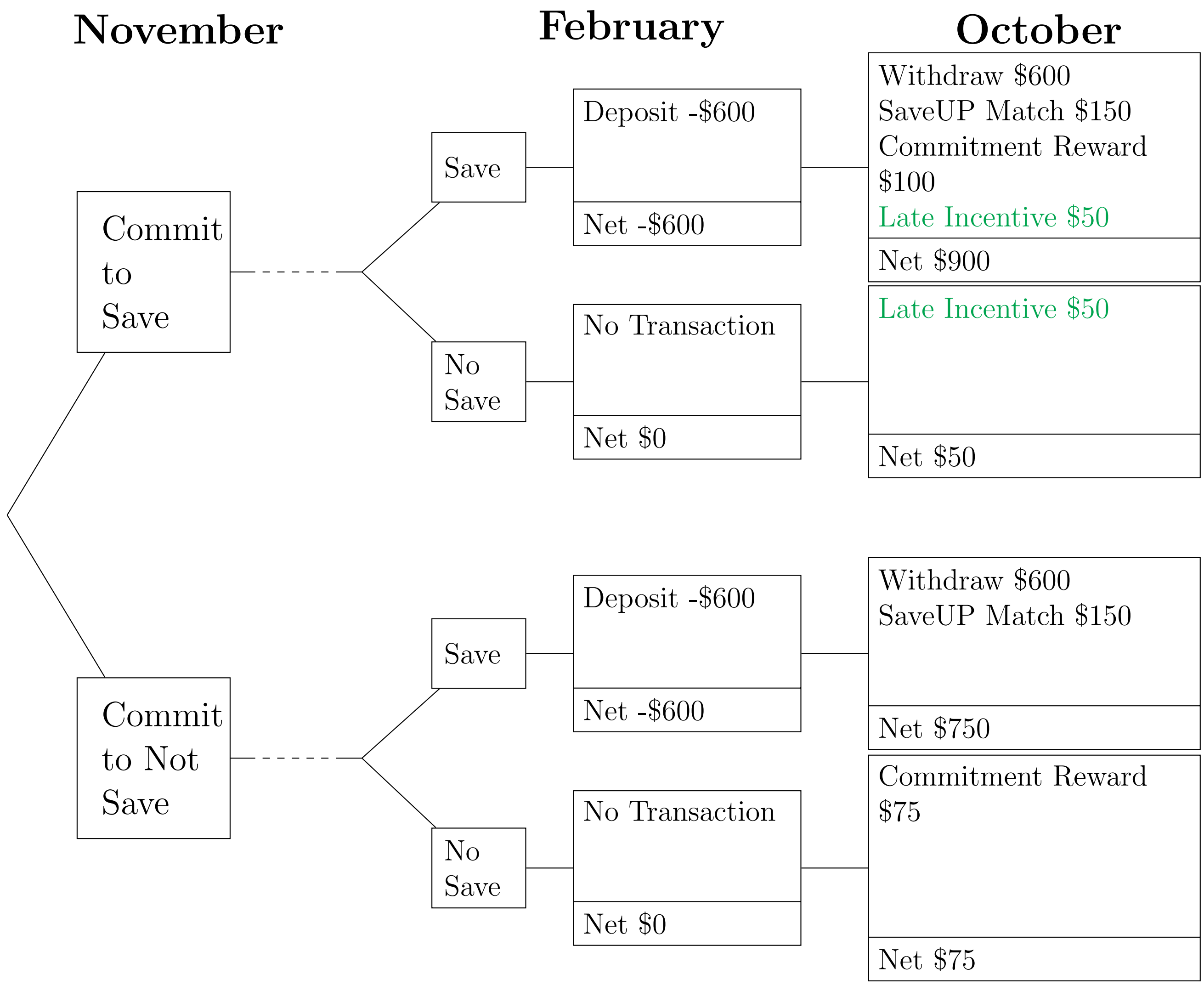


Figure A.7: Late Incentive Treatment Group (T5)



Appendix B: Model of Time-Inconsistent Preferences

B.1 Solving the Model with No Uncertainty

To illustrate our result, we solve the model working backward from period 2. Here the decision on whether to save or not, a_2 is made. Next, we solve the model in period 1, where the soft-commitment decision, a_1 is made. We remind the reader of the stream of payoffs for members of the commitment option (C) group over periods 2 and 3 as a function of actions in period 1 and 2:

$$v_2^C(a_1, a_2; \beta, n) = u_2^C(a_1, a_2; n) + \beta\delta u_3^C(a_1, a_2; n) = \begin{cases} -c_n + \beta\delta b_n & \text{if } a_1 = 0, a_2 = 1 \\ \beta\delta p & \text{if } a_1 = 0, a_2 = 0 \\ -c_n + \beta\delta(b_n + p) & \text{if } a_1 = 1, a_2 = 1 \\ 0 & \text{if } a_1 = 1, a_2 = 0 \end{cases}$$

where n indexes individuals, the superscript C denotes the commitment groups, the subscript 2 indicates that v is evaluated from the perspective of period 2, and the parameter β captures potential time-inconsistency. We have normalized the stream of utility when $(a_1, a_2) = (1, 0)$ to zero in periods 2 and 3. Recall that the experimental incentives (i, d, e, l) have for the time being been suppressed. Payoffs for members of the non-commitment option (NC) group can be thought of as a special case where $p \equiv 0$, i.e. there is no soft-commitment mechanism:

$$v_2^{NC}(a_1, a_2; \beta, n) = u_2^{NC}(a_1, a_2; n) + \beta\delta u_3^{NC}(a_1, a_2; n) = \begin{cases} -c_n + \beta\delta b_n & \text{if } a_2 = 1 \\ 0 & \text{if } a_2 = 0 \end{cases}$$

Note that for completeness, we include the variable a_1 as argument in the utility function even though it is irrelevant for members of the NC group. For convenience, we use as shorthand $u(c) \equiv c_n$, $u(b) \equiv b_n$ and $u(b+p) \equiv b_n + p$. Note, however, we do not need to assume quasilinear preferences in the case with no uncertainty. The marginal utility of the commitment reward p can be defined as $p_n \gtreqless p$ for each individual, and the results will still hold.

B.1.1 Period 2

Consider an agent in the commitment option (C) group. In period 2, there are two possible states of the world. In the first state, a soft-commitment to save has been made — $a_1 = 1$. Thus, an additional "commitment reward" of p is realized in period 3 if she saves. Alternatively, in the case that a soft-commitment to *not* save has been made — $a_1 = 0$ — an additional "commitment reward" of p is realized in period 3 if she does *not* save. She therefore employs the following strategy:

$$a_2 = \begin{cases} 1 & \text{if } a_1 = 1 \text{ and} - c_n + \beta\delta b_n \geq -\beta\delta p \\ 1 & \text{if } a_1 = 0 \text{ and } - c_n + \beta\delta b_n \geq \beta\delta p \\ 0 & \text{otherwise} \end{cases}$$

Next, consider an agent in the non-commitment option (NC) group. This agent has no previous soft-commitment history and therefore employs the following strategy:

$$a_2 = \begin{cases} 1 & \text{if } - c_n + \beta\delta b_n \geq 0 \\ 0 & \text{otherwise} \end{cases}$$

That is, she will save if the cost in period 2 (c_n) is smaller than the discounted benefit of saving in period 3 ($\beta\delta b_n$).

It follows that the soft-commitment is reinforcing in that it makes the decision in period 2 of C group members more likely to follow the soft-commitment relative to an identical member of the NC group. This fully characterizes behavior in the NC group, where $\beta = 1$ is substituted for time-consistent agents.

B.1.2 Period 1

We now turn to period 1 decisions, which are only made by members of the C group. The agents' strategies now depend on their preferences in period 1 and beliefs in period 1 regarding period 2 preferences $\left(\beta, \hat{\beta}, b_n, c_n\right)$. We will restrict analysis to time-consistent agents ($\beta = \hat{\beta} = 1$), sophisticated, present-biased agents ($\beta = \hat{\beta} < 1$) and fully naïve, present-biased agents ($\beta < \hat{\beta} = 1$). Following the literature on "β-δ" preferences, we solve the model as a sequential game between the period 1 and period 2 "selves". That is, the agent in period 1 solves the following problem:

$$\max_{a_1} v_1^C\left(x_1, a_1; \beta, n\right) = \max_{a_1} \beta\delta \cdot v_2^C\left(a_1, a_2^*; 1, n\right)$$

subject to the constraint:

$$a_2^* = \underset{a_2}{\operatorname{argmax}}\, v_2^C\left(a_1, a_2; \hat{\beta}, n\right)$$

That is, the agent in period 1 chooses a_1 taking into account what she believes the best response will be in period 2 of her future "self," given her beliefs regarding the future self's preferences, $\hat{\beta}$. Note first that the $\beta\delta$ in the maximization problem for period 1 is irrelevant for the choice of a_1.[56] Also, note that in the case of time-consistent preferences or full naïvete we have $\hat{\beta} = 1$, and therefore the period 1 strategy, i.e. a_1, solves the collapsed problem:

$$\max_{a_1, a_2} v_2^C\left(a_1, a_2; 1, n\right)$$

That is, the time-consistent agent and naïve agent behave identically in period 1 when choosing a_1. The a_2 that solves this collapsed problem will also describe period 2 behavior for the time-consistent agent. However, the actual a_2 chosen in period 2 for the naïve agent may differ.

B.1.3 Identifying Time-Inconsistency

Consider the time-consistent agent. Her strategy in period 1 will be:

$$a_1 = \begin{cases} 1 & \text{if } -c_n + \delta b_n \geq 0 \\ 0 & \text{otherwise} \end{cases}$$

where we have assumed that the agent commits and saves when indifferent. Suppose instead that the agent chooses $a_1 = 1$ when $-c_n + \delta b_n < 0$. Then at best, the soft-commitment will induce the period 2 self to save, and the payoff will be $-c_n + \delta b_n + \delta p < \delta p$. In the worse case, the period 2 agent still does not save and the payoff is zero. Had the agent chosen $a_1 = 0$, then the period 2 agent would similarly have chosen not to save, and the payoff would be δp, which is strictly higher. Thus, the introduction of the soft-commitment has no effect on savings outcomes in period 2 for a time-consistent agent. Intuitively, there is no time-inconsistency, and so the period 1 agent takes no steps to alter outcomes. Table B.1 summarizes outcomes for the time-consistent agent over a partitioned preference space.

Now consider a sophisticated, present-biased agent. The outcomes for the sophisticated agent are summarized using a slightly finer partition of the preference space in Table B.2. As in the case the time-consistent agent, the sophisticated, period 1 self would like saving to take place whenever $c_n \leq \delta b_n$. In

[56] Again, we have included the state variable x_1 as an argument in the period 1 value function, even though it is irrelevant for our purposes. Our experimental variation is orthogonal to initial conditions. Nonetheless, we include x_1 to remain as consistent as possible in notation.

Table B.1: Time-Consistent Agent Outcomes

	a_1^C	a_2^C	a_2^{NC}
$c_n \leq \delta b_n$	1	1	1
$c_n > \delta b_n$	0	0	0

Table B.2: Present-Biased, Sophisticated Agent Outcomes

	a_1^C	a_2^C	a_2^{NC}
$c_n \leq \beta\delta b_n$	1	1	1
$\beta\delta b_n < c_n \leq \beta\delta\left(b_n + p\right)$	1	1	0
$\beta\delta\left(b_n + p\right) < c_n \leq \delta b_n$	0	0	0
$\delta b_n < c_n$	0	0	0

the first and last rows, all selves are in agreement. In the second row, the agent uses the soft-commitment to alter the period 2 outcome (i.e. $a_2^C \neq a_2^{NC}$). However, when $\beta\delta b_n + p < c_n$ in row 3, she understands that soft-committing to saving is futile. Even though the period 1 self prefers to save — $c_n \leq \delta b_n$, she understands that the period 2 self will disregard the commitment reward and not save — $-c_n + \beta\delta(b_n + p)$. Therefore, the period 1 self only soft-commits when the soft-commitment reward p is large enough, i.e. $c_n \leq \beta\delta b_n + p$ as in rows 1 and 2. Note, we are only able to distinguish the present-biased, sophisticated agent from the time-consistent agent when $\Pr\left(\beta\delta b_n < c_n \leq \beta\delta b_n + p\right) > 0$, i.e. when there are agents in row 2 we have $a_2^C \neq a_2^{NC}$.

Finally, consider the present-biased, naïve agent. We know her period 1 choice for a_1 will follow the same decision rule as the time-inconsistent agent. However, her period 2 outcomes may differ, due to the presence of $\beta < 1$ in the decision rules described above in Section B.1.1. The outcomes for the naïve agent are summarized in Table B.3. Again, the a_1^C column follows from the previous table for time-consistent agents. Intuitively, the naïve agent would like to always reinforce her counterfactual decisions in period 2. Comparing a_1^C to a_2^{NC}, we see that this is not always the case. In the first and last rows, reinforcement is achieved. In the second row, the naïve agent soft-commits to saving. The actual outcome a_2^C is born out, but this is due to naïve luck. The soft-commitment reward p is large enough to be self-fulfilling. However, an identical member of the (NC) does not save, and thus the presence of the soft-commitment has altered period 2 outcomes. In the third row, the agent again mis-predicts period 2 preferences. Furthermore, the soft-commitment reward is no longer large enough to alter period 2 outcomes. In this case, the reward is forfeited due to overconfidence. Thus, compared to the naïve agent, the sophisticated agent does not commit the error of overconfidence in the third row. We are only able to distinguish the naïve agent from the sophisticated agent if $\Pr\left(\beta\delta\left(b_n + p\right) < c_n \leq \delta b_n\right) > 0$. In other words, if there are no agents in the third row of table, whenever the agent soft-commits to saving the reward of p is large enough to be self-fulfilling.

Table B.3: Present-Biased, Naïve Agent Outcomes

	a_1^C	a_2^C	a_2^{NC}
$c_n \leq \beta\delta b_n$	1	1	1
$\beta\delta b_n < c_n \leq \beta\delta\left(b_n + p\right)$	1	1	0
$\beta\delta\left(b_n + p\right) < c_n \leq \delta b_n$	1	0	0
$\delta b_n < c_n$	0	0	0

The support restrictions on (b_n, c_n) illustrate a key tension in the experimental design. As the commitment reward p grows in size, it becomes easier to distinguish between time-consistent and time-inconsistent agents, as the likelihood of falling in the second row of the Tables B.2 and B.3 increases. However, the ability to distinguish between na´ive and sophisticated agents becomes more difficult, as the likelihood of being in the third row of those tables decreases. We therefore make use of the following definition:

Definition B.1. *Given a distribution of preferences with CDF $G(c, b)$ and current period discount factor $\beta\delta$, a soft-commitment reward is:*

(1) ***Sufficiently Strong*** *if*

$$\Pr\left(\beta\delta b_n < c_n \leq \beta\delta b_n + p\right) > 0$$

and is

(2) ***Sufficiently Weak*** *if*

$$\Pr\left(\beta\delta\left(b_n + p\right) < c_n \leq \delta b_n\right) > 0$$

To gain further intuition regarding these support restrictions, note that sufficient strength requires that there exist agents for whom $c_n > \beta\delta b_n$. Otherwise, all agents and all selves would be in agreement that saving is the preferred action. In this case, there would be no variation in savings or soft-commitments available to aid in identifying time-inconsistency. In addition, sufficient strength requires that $p > 0$. Otherwise, soft-commitment actions in period 1 have no bearing on period 2, and we therefore lose our ability to learn about beliefs. Turning to sufficient weakness, we see that a necessary condition is that $\beta\delta\left(b_n + p\right) < \delta b_n$. In other words, the soft-commitment cannot be so strong that the naïve agent never makes an observably mistaken prediction ($a_1 \neq a_2$). To see this note that soft-committing to saving imposes a negative externality (or perhaps internality) on the period 2 self. The externality is equal to the difference between the net value of saving to the period 1 self and the net value of saving from the standpoint of the period 2 self:

$$\underbrace{\left[-c_n + \delta b_n\right]}_{\text{Period 1 Value}} - \underbrace{\left[-c_n + \beta\delta b_n\right]}_{\text{Period 2 Value}} = (1 - \beta)\delta b_n. \tag{B.1}$$

If the discounted value of the commitment reward, $\beta\delta p$, is larger than the right-hand side of (B.1), then the cost to the period 2 self of complying with an overconfident commitment decision by a naïve period

1 self will be outweighed by the benefit of honoring the commitment. Thus, in order to detect naïvete, we we need $\beta\delta p < (1-\beta)\delta b_n$. Rearranging this inequality, we have the necessary condition for sufficient weakness: $\beta\delta\left(b_n + p\right) < \delta b_n$.

We are now in a position to prove Proposition 1. Note that due to random assignment, the distribution of preferences is identical for the C and NC groups. Thus, integrating over the preference space, within in each column of Table B.1, we have:[57]

$$\begin{aligned} \mathbb{E}\left[a_1^C \middle| TC\right] &= \mathbb{E}\left[a_2^C \middle| TC\right] \\ &= \mathbb{E}\left[a_2^{NC} \middle| TC\right] \\ &= \Pr\left(c_n \leq \delta b_n \left| TC\right.\right), \end{aligned} \tag{B.2}$$

where TC denotes time-consistent agents. This establishes the first result of Proposition 1. Likewise, integrating within the columns of Table B.2, we can show:

$$\begin{aligned} \mathbb{E}\left[a_1^C \middle| PB, S\right] &= \mathbb{E}\left[a_2^C \middle| PB, S\right] \\ &= \Pr\left(c_n \leq \beta\delta b_n \left| PB, S\right.\right) + \Pr\left(\beta\delta b_n < c_n \leq \beta\delta\left(b_n + p\right) \left| PB, S\right.\right) \\ &\geq \Pr\left(c_n \leq \beta\delta b_n \left| PB, S\right.\right) \\ &= \mathbb{E}\left[a_2^{NC} \middle| PB, S\right], \end{aligned} \tag{B.3}$$

where the PB, S denotes a present-biased, sophisticated agent and the inequality in the third line is strict if $\Pr\left(\beta\delta b_n < c_n \leq \beta\delta\left(b_n + p\right) \left| PB, S\right.\right) > 0$. This establishes the second result of Proposition 1. Finally, using Table B.3, we have:

$$\begin{aligned} \mathbb{E}\left[a_1^C \middle| PB, N\right] &= \Pr\left(c_n \leq \beta\delta b_n \left| PB, N\right.\right) + \Pr\left(\beta\delta b_n < c_n \leq \beta\delta\left(b_n + p\right) \left| PB, N\right.\right) \\ &\quad + \Pr\left(\beta\delta\left(b_n + p\right) < c_n \leq \delta b_n \left| PB, N\right.\right) \\ &\geq \Pr\left(c_n \leq \beta\delta b_n \left| PB, N\right.\right) + \Pr\left(\beta\delta b_n < c_n \leq \beta\delta\left(b_n + p\right) \left| PB, N\right.\right) \\ &= \mathbb{E}\left[a_2^C \middle| PB, N\right] \\ &\geq \Pr\left(c_n \leq \beta\delta b_n \left| PB, N\right.\right) \\ &= \mathbb{E}\left[a_2^{NC} \middle| PB, N\right], \end{aligned} \tag{B.4}$$

where the PB, N denotes a present-biased, naïve agent. If $\Pr\left(\beta\delta b_n < c_n \leq \beta\delta\left(b_n + p\right) \left| PB, N\right.\right) > 0$, the first inequality is strict, and if $\Pr\left(\beta\delta\left(b_n + p\right) < c_n \leq \delta b_n \left| PB, N\right.\right) > 0$, the second inequality is strict. This demonstrates the last result in Proposition 1.

B.2 Solving the Model with Uncertainty

We now relax the assumption of no uncertainty. In particular, in period 1, agent n does not exactly know the cost and benefits of saving $\left(c_n, b_n\right)$, but instead faces a distribution of costs and benefits with CDF $G_n\left(c_n, b_n\right)$. In contrast to standard models of dynamic choice: (i) we do not place any restrictions on this joint distribution other than assuming it is smooth enough to allow the application of Leibniz's rule, (ii) we do not assume that $dG_n(b_n, c_n)$ is known to the econometrician and (iii) We also make the simplifying assumption that all uncertainty about both costs in period 2 and benefits in period 3 is resolved in period 2, before action a_2 is taken. In return for relaxing these assumptions, we must now assume homogeneity in time preferences, i.e. everyone has the same value for β. We also make the structural assumption of quasilinear utility. We again solve the model by backward induction, beginning in period 2, where the

[57]Note that this integration over the unobserved heterogeneity is carried out by the econometrician, since she only observes actions, but not by the agent who is assumed to know is values of (b_n, c_n).

decision on whether to save or not, a_2 is made. Next, we turn to period 1, where the soft-commitment decision, a_1 is made. In this case, we require the additional experimental payoffs (i, d, e, l) to achieve identification of time-inconsistency.

B.2.1 Period 2

Now that the full set of experimental incentives are featured, we have the following payoffs for members of the commitment option (C) group:

$$v_2^C(a_1, a_2; \beta, n) = u_2^C(a_1, a_2; n) + \beta\delta u_3^C(a_1, a_2; n) = \begin{cases} -c_n + \beta\delta b_n & \text{if } x_2 \equiv a_1 = 0, a_2 = 1 \\ \beta\delta p & \text{if } x_2 \equiv a_1 = 0, a_2 = 0 \\ -c_n + e + \beta\delta(b_n + l + p) & \text{if } x_2 \equiv a_1 = 1, a_2 = 1 \\ e + \beta\delta l & \text{if } x_2 \equiv a_1 = 1, a_2 = 0 \end{cases}$$

Note that the early and late payoffs (e, l) in periods 2 and 3 respectively do not depend upon the action take in period 2 and this is key for the identification argument below. Next, combined with the quasilinear assumption, the decision in period 2 is the same as in Section B.1.1. This is because all of the uncertainty in the model has been resolved at the start of period 2. The agent in the C group will employ the following strategy:

$$a_2 = \begin{cases} 1 & \text{if } a_1 = 1 \text{ and } -c_n + \beta\delta b_n \geq -\beta\delta p \\ 1 & \text{if } a_1 = 0 \text{ and } -c_n + \beta\delta b_n \geq \beta\delta p \\ 0 & \text{otherwise} \end{cases} \tag{B.5}$$

Now consider a member of the non-commitment option (NC) group. Payoffs for this group are as follows:

$$v_2^{NC}(a_1, a_2; \beta, n) = u_2^{NC}(a_1, a_2; n) + \beta\delta u_3^{NC}(a_1, a_2; n) = \begin{cases} -c_n + i + \beta\delta(b_n + d) & \text{if } a_2 = 1 \\ 0 & \text{if } a_2 = 0 \end{cases}$$

where the state variable a_1 is irrelevant since there is no soft-commitment decision for this group. Similar to the case of no uncertainty, this agent will use the follow strategy:

$$a_2 = \begin{cases} 1 & \text{if } -c_n + \beta\delta b_n \geq -i - \beta\delta d \\ 0 & \text{otherwise} \end{cases} \tag{B.6}$$

B.2.2 Period 1

Only members of the commitment (C) group make decisions in period 1. As before, we solve the model as a sequential game between the period 1 and period 2 selves. Let $v_1^C(x_1, a_1; \beta)$ denote the value function in period 1. The expected value of soft-committing to save, i.e. $a_1 = 1$, is:

$$\begin{aligned} v_1^C(x_1, 1; \beta, n) &= \beta\delta\mathbb{E}\left[v_2^C(1, a_2^*; 1, n)\,|n\right] \\ &= \beta\delta \iint v_2^C(1, a_2^*; 1, n)\, dG_n(c_n, b_n) \\ &= \beta\delta \iint \Big(u_2^C(1, a_2^*; n) + \delta u_3^C(1, a_2^*; n)\Big) dG_n(c_n, b_n) \end{aligned}$$

where

$$a_2^* = \underset{a_2}{\operatorname{argmax}}\, v_2^C\left(a_1, a_2; \hat{\beta}, n\right)$$

Using the strategy in (B.5) for a_2^* in Section B.2.1 above, we can rewrite the value function as

$$v_1^C(x_1, 1; \beta, n) = \beta\delta \cdot \left[\iint\limits_{-c_n+\hat{\beta}\delta b_n \geq -\hat{\beta}\delta p} [-c_n + e + \delta(b_n + l + p)] \, \mathrm{dG}_n(c_n, b_n) + \iint\limits_{-c_n+\hat{\beta}\delta b_n < -\hat{\beta}\delta p} [e + \delta l] \, \mathrm{dG}_n(c_n, b_n) \right]$$

The first component of this expression is the payoff in the event that the agent actually follows through with the soft-commitment to save and the second component is the payoff should the agent fail to follow through. Note that the argument inside the integral features a discount factor of δ, as it reflects the agent's preferences in period 1 over payoffs in period 2 and 3. However, the limits of integration feature a discount factor of $\hat{\beta}\delta$, as the likelihood of following through with the commitment is based on the period 1 agent's beliefs about period 2 preferences over payoffs in period 2 and 3. Put another way, the limits of integration reflect the period 2 self's best response. Using a similar line of reasoning, the expected value of soft-committing to *not* save is

$$\begin{aligned}
v_1^C(x_1, 0; \beta, n) &= \beta\delta\mathbb{E}\left[v_2^C(0, a_2^*; 1, n)\,|n\right] \\
&= \beta\delta \iint v_2^C(0, a_2^*; 1, n)\, dG_n(c_n, b_n) \\
&= \beta\delta \iint \left(u_2^C(0, a_2^*; n) + \delta u_3^C(0, a_2^*; n) \right) dG_n(c_n, b_n) \\
&= \beta\delta \cdot \left[\iint\limits_{-c_n+\hat{\beta}\delta b_n \geq \hat{\beta}\delta p} [-c_n + \delta b_n] \, \mathrm{dG}_n(c_n, b_n) + \iint\limits_{-c_n+\hat{\beta}\delta b_n < \hat{\beta}\delta p} [\delta p] \, \mathrm{dG}_n(c_n, b_n) \right]
\end{aligned}$$

The first part of this expression corresponds to the case where saving takes place despite the soft-commitment to not save, while the second component covers the case where the agent follows through with the soft-commitment to not save. The agent's strategy in period 1 will then be:

$$a_1 = \begin{cases} 1 & \text{if } v_1^C(x_1, 1; \beta, n) \geq v_1^C(x_1, 0; \beta, n) \\ 0 & \text{otherwise} \end{cases}$$

Rearranging $v_1^C(x_1, 1; \beta, n) \geq v_1^C(x_1, 0; \beta, n)$ we have that $a_1 = 1$ if:

$$\begin{aligned}
e + \delta l + \iint\limits_{-c_n+\hat{\beta}\delta b_n \geq \hat{\beta}\delta p} [\delta p] \, \mathrm{dG}(c_n, b_n) + \iint\limits_{\hat{\beta}\delta p > -c_n+\hat{\beta}\delta b_n \geq -\hat{\beta}\delta p} [-c_n + \delta b_n] \, \mathrm{dG}(c_n, b_n) \\
+ \iint\limits_{-\hat{\beta}\delta p > -c_n+\hat{\beta}\delta b_n} [-\delta p] \, \mathrm{dG}(c_n, b_n) \geq 0 \quad \text{(B.7)}
\end{aligned}$$

The first two terms, $e + \delta l$, are the incentives received simply for soft-committing to save. These are received with certainty in period 2. The next component reflects draws of preferences (c, b) such that saving would have happened with or without the soft-commitment. In this case, the additional difference in payoffs is the added commitment reward of p. The second integral reflects draws of preferences where saving would not have happened but for the soft-commitment. We can refer to this as the "region of influence." As p gets larger, this region, the states of the world where the soft-commitment actually affects outcomes in period 2, gets larger. In this case, the additional difference in utility is the net benefit of saving $-c + \delta b$. Finally, the third integral reflects draws of preferences for which saving never happens, even with the soft-commitment to save. Thus, soft-committing to save only results in a forfeiture of the commitment reward p.

B.2.3 Identifying Time-Inconsistency

We now demonstrate the result in Proposition 2, i.e. our estimation strategy for β and δ. First, using variation in (i, d), we estimate $\beta\delta$ from the NC group members in period 2. Note from (B.6) that

$$\begin{aligned} \mathbb{E}\left[a_2^{NC}\right] &= \Pr\left(a_2 = 1 | \, NC\right) \\ &= \iint\limits_{-c_n+\beta\delta b_n \geq -i-\beta\delta d} \mathrm{dG}\left(c_n, b_n\right) \end{aligned}$$

It follows from Leibniz's Rule that

$$\frac{\partial \mathbb{E}\left[a_2^{NC}\right] / \, \partial d}{\partial \mathbb{E}\left[a_2^{NC}\right] / \, \partial i} = \beta\delta$$

That is, the response to the delayed incentive is smaller than the response to the immediate incentive by a factor of $\beta\delta$.

Next, using variation in (e, l), we estimate δ among members of the G group. Using (B.7) we see that:

$$\begin{aligned} \mathbb{E}\left[a_1^{C}\right] &= \Pr\left(a_1 = 1 | \, C\right) \\ &= \Pr\left(v_1^C\left(x_1, 1; \beta, n\right) \geq v_1^C\left(x_1, 0; \beta, n\right)\right) \\ &= \Pr\left(e + \delta l \geq \iint\limits_{-c_n+\hat{\beta}\delta b_n \geq \hat{\beta}\delta p} \left[-\delta p\right] \mathrm{dG}_n\left(c_n, b_n\right) - \iint\limits_{\hat{\beta}\delta p > -c_n+\hat{\beta}\delta b_n \geq -\hat{\beta}\delta p} \left[-c_n + \delta b_n\right] \mathrm{dG}_n\left(c_n, b_n\right) \right. \\ &\qquad \left. + \iint\limits_{-\hat{\beta}\delta p > -c_n+\hat{\beta}\delta b_n} \left[\delta p\right] \mathrm{dG}_n\left(c_n, b_n\right) \right) \end{aligned}$$

Again, using Leibniz's Rule we can show that

$$\frac{\partial \mathbb{E}\left[a_1^{C}\right] / \, \partial l}{\partial \mathbb{E}\left[a_1^{C}\right] / \, \partial e} = \delta$$

This result relies on the assumption of quasi-linearity and the fact that the early and late incentives (e, l) are independent of period 2 actions. Therefore, they only impact period 1 decisions through their impact on the period 1 self's valuation of saving, which is in turn a function of the long-run discount factor δ.

Appendix C: Treatment Bounding Method

C.1 Setup

- Define Z to be treatment assignment, which is binary valued and equal to 1 if the unit is assigned to the treatment group and 0 if the unit is assigned to the control group. In our case, the control group is the commitment control group or the non-commitment control group, and the treatment is one of the early/immediate or late/delayed incentive groups.

- Define C_z, a binary variable, which indicates participation in the study. This is either phone contact status or site appearance for a unit if her treatment is assigned to z. Define the observed random variable

$$C = C_1 Z + C_0 \left(1 - Z\right)$$

- Define Y_z to be the pre-commitment decision or savings decision if the unit's treatment assignment is set to z and define

$$Y = Y_1 Z + Y_0 (1 - Z)$$

Note, we only observe Y when $C = 1$.

- We assumed that $(Y_1, Y_0, C_1, C_0) \perp Z$

- The following distributions are identified

$$\begin{array}{l} \Pr(Y, Z|C = 1) \\ \Pr(Z, C) \end{array}$$

C.2 Bounding $\theta_2 \equiv \mathbb{E}[Y_1 - Y_0|C_1 = 1]$

We can place bounds on the treatment effect for a subgroup, namely those who are successfully contacted by phone or appearing at the tax site. Following Behaghel, Crépon, Gurgand, and Le Barbanchon (2009), we can see that:

$$\begin{aligned} \theta_2 &\equiv \mathbb{E}[Y_1 - Y_0|C_1 = 1] \\ &= \frac{\mathbb{E}[(Y_1 - Y_0) C_1]}{\Pr(C_1 = 1)} \\ &= \frac{\mathbb{E}[Y_1 C_1 - Y_0 C_0 - Y_0 (C_1 - C_0)]}{\Pr(C_1 = 1)} \\ &= \frac{\mathbb{E}[Y_1 C_1] - \mathbb{E}[Y_0 C_0] - \mathbb{E}[Y_0 (C_1 - C_0)]}{\Pr(C_1 = 1)} \\ &= \frac{\mathbb{E}[YC|Z = 1] - \mathbb{E}[YC|Z = 0] - \mathbb{E}[Y_0 (C_1 - C_0)]}{\Pr(C = 1|Z = 1)} \end{aligned}$$

To further simplify, use an assumption of monotonicity:

Assumption C.1. $C_1 \geq C_0$

Now, assuming $C_1 \geq C_0$ and using the fact that $Y_0 \in \{0, 1\}$

$$\begin{aligned} 0 &\leq \mathbb{E}[Y_0 (C_1 - C_0)] \\ &\leq \mathbb{E}[C_1 - C_0] \\ &= \Pr(C_1 = 1) - \Pr(C_0 = 1) \\ &= \Pr(C = 1|Z = 1) - \Pr(C = 1|Z = 0) \end{aligned}$$

Thus, we have bounds on θ_2:

$$\begin{aligned} \theta_{2,ub} &= \frac{1}{\Pr(C = 1|Z = 1)} \Big(\mathbb{E}[YC|Z = 1] - \mathbb{E}[YC|Z = 0] \Big) \\ \theta_{2,lb} &= \frac{1}{\Pr(C = 1|Z = 1)} \Big(\mathbb{E}[YC|Z = 1] - \mathbb{E}[YC|Z = 0] - \big[\Pr(C = 1|Z = 1) - \Pr(C = 1|Z = 0) \big] \Big) \end{aligned}$$

Note, if monotonicity is in the other direction, i.e. $C_1 \leq C_0$, then we simply switch the order of the bounds.

Appendix D: Convex Time Budget Method

In this appendix, we demonstrate the application of the Convex Time Budget (CTB) method developed by Andreoni and Sprenger (2010). In general, this method allows for identification of both time preference parameters, (β, δ), and risk preferences, e.g. the coefficient of relative risk aversion, (γ). We show below that using the variation in our experimental incentives, we are able to recover the composite discount factor $\beta\delta$, conditional on the value of γ and the level of income flows in period 2 and 3. The present value, in period 2, of utility over periods 2 and 3 will be:

$$v_2\left(c_2, c_3\right) = u\left(c_2 + \omega_2\right) + \beta\delta u\left(c_2 + \omega_3\right)$$

where ω_2 and ω_3 are the flows of income in periods 2 and 3. The choice variables, c_2 and c_3, are additional consumption funded out of the income tax refund received in period 2, R. The agent can either consume all of the refund, or use it to open a SaveUp/SaveUpFront account, which yields a return of r for every dollar deposited above a minimum deposit of $\underline{D}$ and below the maximum deposit of $\bar{D}$. The intertemporal budget set is therefore:

$$c_2 + \frac{c_3}{(1+r)} = R + i + e + \frac{d+l+p}{(1+r)}$$

where (i, d, e, l, p) are the experimental payoffs described in Section 5. We assume a Constant Relative Risk Aversion (CRRA) functional form for flow utility, $u(\cdot)$:

$$u(c) = \frac{c^{1-\gamma} - 1}{1-\gamma}$$

where γ is the coefficient of relative risk aversion. The maximization problem is therefore

$$\max_{c_2, c_3} = \frac{\left(c_2 + \omega_2\right)^{1-\gamma} - 1}{1-\gamma} + \beta\delta\frac{\left(c_3 + \omega_3\right)^{1-\gamma} - 1}{1-\gamma}$$

subject to the constraints:

$$\begin{aligned}
[\lambda] &: \quad c_2 + \frac{c_3}{(1+r)} = R + i + e + \frac{d+l+p}{(1+r)} \\
[\mu] &: \quad c_2 \geq \left[R + i + e - \min\left(R + i + e, \bar{D}\right)\right] \cdot \mathbf{1}\left\{c_3 > 0\right\} \\
[\nu] &: \quad c_2 \leq R - \left[\underline{D} - i - e\right] \cdot \mathbf{1}\left\{c_3 > 0\right\} \\
[\kappa] &: \quad c_3 \geq 0
\end{aligned}$$

The first constraint is again the intertemporal budget constraint. The second constraint captures the fact that if a savings account is opened, i.e. $c_3 > 0$, any money left over after depositing the maximum amount, $\bar{D}$, is consumed in period 2. If there is no savings account opened, then we only require c_2 to be positive. The third constraint requires c_2 to be small enough to allow the minimum deposit $\underline{D}$, when an account is opened, i.e. $c_3 > 0$. The final constraint reflects an assumed borrowing constraint.

The Kuhn-Tucker conditions include the following:

$$\begin{aligned}
\left(c_2^* + \omega_2\right)^{-\gamma} &= \lambda^* - \mu^* + \nu^* \\
\beta\delta\left(c_3^* + \omega_3\right)^{-\gamma} &= \frac{\lambda^*}{(1+r)} - \kappa^*
\end{aligned}$$

where c_t^* is the potentially unobserved optimal net consumption choice in period $t \in \{2, 3\}$. The two equations can be combined to yield:

$$\ln\left(\frac{c_2^*+\omega_2}{c_3^*+\omega_3}\right)^{-\gamma}=\ln\left(1+r\right)+\ln\beta\delta+\ln\left(\frac{\lambda^*-\mu^*+\nu^*}{\lambda^*-\kappa^*\left(1+r\right)}\right) \tag{D.8}$$

For those at an interior solution — i.e. saving a positive amount strictly within the minimum and maximum deposit — $\mu^*=\nu^*=\kappa^*=0$ and equation (D.8) reduces to:

$$\ln\left(\frac{c_2^*+\omega_2}{c_3^*+\omega_3}\right)^{-\gamma}=\ln\left(1+r\right)+\ln\beta\delta \tag{D.9}$$

For known values of r, γ, ω_2 and ω_3, we can identify $\beta\delta$ in a regression using the observed amounts of c_2 and c_3 among interior savers. We use average monthly income, based on the prior year's tax return, as the value of ω_2 and set $\omega_3=(1+\triangle\omega)\,\omega_2$, $\triangle\omega\in\{0,0.1,0.25\}$. We report the results of these OLS regressions in columns (1)–(3) of Table 13. This approach, however, ignores the savers at corner solutions of the maximum deposit, the minimum deposit or no saving. Following Andreoni and Sprenger (2010), we incorporate those at a corner solution using a doubly censored Tobit regression with variable limits. The contributions to the likelihood function are as follows:

$$\ln\left(\frac{c_2^*+\omega_2}{c_3^*+\omega_3}\right)^{-\gamma}=\ln\left(1+r\right)+\ln\beta\delta \qquad \text{if} \quad \mu^*=\nu^*=\kappa^*=0$$

$$\ln\left(\frac{c_2^*+\omega_2}{c_3^*+\omega_3}\right)^{-\gamma}>\ln\left(\frac{\max\left(R+i+e-\bar{D},0\right)+\omega_2}{d+l+p+(1+r)\min\left(R,\bar{D}\right)-r\underline{D}+\omega_3}\right)^{-\gamma} \qquad \text{if} \quad \mu^*>0,\nu^*=\kappa^*=0$$

$$\ln\left(\frac{c_2^*+\omega_2}{c_3^*+\omega_3}\right)^{-\gamma}<\ln\left(\frac{R+i+e-\underline{D}+\omega_2}{d+l+p+\underline{D}+\omega_3}\right)^{-\gamma} \qquad \begin{array}{ll}\text{if} & \nu^*>0,\mu^*=\kappa^*=0\\ \text{or} & \nu^*>0,\kappa^*>0,\mu^*=0\end{array}$$

The first row, again, corresponds to interior savers. The second row holds for those saving the maximum deposit amount and the third row holds for either those saving the minimum deposit amount or those who have chosen to not save at all. The results of the doubly censored Tobit regression are reported in columns (4)–(6) of Table 13.

Appendix E: Experimental Treatment Materials

Figure E.8: Initial Mailing: Non-Commitment Control (T1) – Year 1

Save UP — Rewarding Saving

The Financial Clinic would like you to participate in our pilot test of a new savings account: **Save UP**.

In about a week, we will contact you by phone to discuss **Save UP** in more detail.

If you decide to participate in our pilot, file your taxes with us and use the **Save UP** account, you can earn up to $375.

Here is how the Save UP account works:

- √ You have to file your taxes at one of our designated Money UP sites and have an income tax refund of more than $300
- √ You have to deposit at least $300 in the Save UP account
- √ You must keep your money in the account for at least 8 months
- √ For every dollar above $300, we will deposit an additional 50% match in the account

For Example, if you deposit $600 in a Save UP account in February and leave your money in that account until September, you will be able to withdraw $750 in September.

Take a look at the visual aids in included to see more examples of how the Save UP account works.

Group 6

February | **September**

Deposit $800 → Save → Withdraw $800 + $250 (Save UP Match) = Total: $1,050

If you deposit $800 in February ... You can withdraw $1,050 in September

- √ If you deposit $800, then you have put in $500 above the required $300.
- √ An additional 50% of this $500 will be put in the Save UP account
- √ That means an additional $250 will be put in the account
- √ When you withdraw from the account after 8 months, you will have a total of $1,050
- √ If you withdraw your money early, you will forfeit the Save UP match

Group 6

February | **September**

Deposit $400 → Save → Withdraw $400 + $50 (Save UP Match) = Total: $450

If you deposit $400 in February ... You can withdraw $450 in September

- √ If you deposit $400, then you have put in $100 above the required $300.
- √ An additional 50% of this $100 will be put in the Save UP account
- √ That means an additional $50 will be put in the account
- √ When you withdraw from the account after 8 months, you will have a total of $450
- √ If you withdraw your money early, you will forfeit the Save UP match

Group 6

Figure E.9: Initial Mailing: Immediate Incentive (T2) – Year 1

Save UP

Rewarding Saving

The Financial Clinic would like you to participate in our pilot test of a new savings account: **Save UP**.

In about a week, we will contact you by phone to discuss **Save UP** in more detail.

If you decide to participate in our pilot, file your taxes with us and use the **Save UP** account, you can earn up to $425.

Here is how the Save UP account works:

- √ You have to file your taxes at one of our designated Money UP sites and have an income tax refund of more than $300
- √ You have to deposit at least $300 in the Save UP account
- √ You will get $50 cash back for depositing the required $300
- √ You must keep your money in the account for at least 8 months, and you will receive $50
- √ For every dollar above $300, we will deposit an additional 50% match in the account

For Example, if you deposit $600 in a Save UP account in February, you receive $50 back immediately, and if you leave your money in that account until September, you will be able to withdraw $750 in September.

Take a look at the visual aids in included to see more examples of how the Save UP account works.

Group 4

February

Deposit $800
Receive $50 Back

Save

September

Withdraw $800
+
$250
(Additional Save UP Match)
=
Total: $1,050

If you deposit $800 in February, you get $50 back ....

You can withdraw $1,050 in September

- √ For depositing at least $300 you get $50 back in February
- √ If you deposit $800, then you have put in $500 above the required $300
- √ An additional 50% of this $500 will be put in the Save UP account
- √ That means an additional $250 will be put in the account
- √ When you withdraw from the account after 8 months, you will have a total of $1,050
- √ If you withdraw your money early, you will forfeit the Save UP match, including the $50

Group 4

February

Deposit $400
Receive $50 Back

Save

September

Withdraw $400
+
$50
(Additional Save UP Match)
=
Total: $450

If you deposit $400 in February, you get $50 back ....

You can withdraw $450 in September

- √ For depositing at least $300 you get $50 back in February
- √ If you deposit $400, then you have put in $100 above the required $300
- √ An additional 50% of this $100 will be put in the Save UP account
- √ That means an additional $50 will be put in the account
- √ When you withdraw from the account after 8 months, you will have a total of $450
- √ If you withdraw your money early, you will forfeit the Save UP match, including the $50

Group 4

Figure E.10: Initial Mailing: Delayed Incentive (T3) – Year 1

Save UP Rewarding Saving

The Financial Clinic would like you to participate in our pilot test of a new savings account: **Save UP**.

In about a week, we will contact you by phone to discuss **Save UP** in more detail.

If you decide to participate in our pilot, file your taxes with us and use the **Save UP** account, you can earn up to $425.

Here is how the Save UP account works:

- √ You have to file your taxes at one of our designated Money UP sites and have an income tax refund of more than $300
- √ You have to deposit at least $300 in the Save UP account
- √ You must keep your money in the account for at least 8 months
- √ You will get $50 for depositing the required $300
- √ For every dollar above $300, we will deposit an additional 50% match in the account

For Example, if you deposit $600 in a Save UP account in February and leave your money in that account until September, you will be able to withdraw $800 in September.

Take a look at the visual aids in included to see more examples of how the Save UP account works.

Group 5

February | **September**

Deposit $800 → Save → Withdraw $800 + $50 (Save UP Match) + $250 (Additional Save UP Match) = Total: $1,100

If you deposit $800 in February ... | You can withdraw $1,100 in September

- √ For depositing at least $300 you get a match of $50
- √ If you deposit $800, then you have put in $500 above the required $300
- √ An additional 50% of this $500 will be put in the Save UP account
- √ That means an additional $250 will be put in the account
- √ When you withdraw from the account after 8 months, you will have a total of $1,100
- √ If you withdraw your money early, you will forfeit the Save UP match, including the $50

Group 5

February | **September**

Deposit $400 → Save → Withdraw $400 + $50 (Save UP Match) + $50 (Additional Save UP Match) = Total: $500

If you deposit $400 in February ... | You can withdraw $500 in September

- √ For depositing at least $300 you get a match of $50
- √ If you deposit $400, then you have put in $100 above the required $300
- √ An additional 50% of this $100 will be put in the Save UP account
- √ That means an additional $50 will be put in the account
- √ When you withdraw from the account after 8 months, you will have a total of $500
- √ If you withdraw your money early, you will forfeit the Save UP match, including the $50

Group 5

Figure E.11: Initial Mailing: Commitment Control (T4) – Year 1

Save UPfront

Rewarding Saving
Rewarding Planning

The Financial Clinic would like you to participate in our pilot test of a new savings account: **Save UPfront**.

In about a week, we will contact you by phone to discuss **Save UPfront** in more detail.

If you decide to participate in our pilot, file your taxes with us and use the **Save UPfront** account, you can earn up to $425.

Here is how the Save UPfront account rewards **saving**:

- √ You have to file your taxes at one of our designated Money UP sites and have an income tax refund of more than $300
- √ You have to deposit at least $300 in the Save UPfront account
- √ You must keep your money in the account for at least 8 months
- √ For every dollar above $300, we will deposit an additional 50% match in the account

Here is how the Save UPfront account rewards **planning**:

- √ If you pre-commit now to saving in February, your savings match will increase by $100
- √ If you don't pre-commit now to saving, you still have the option to save, but will get $75 even if you don't save

For Example, if you pre-commit to saving in December, deposit $600 in a Save UPfront account in February and leave your money in that account until September, you will be able to withdraw $850 in September.

Take a look at the visual aids in included to see more examples of how the Save UPfront account work

Group 3

If you pre-commit to saving:

February

Deposit $800

If you deposit $800 in February ...

Save

September

Withdraw $800
+
$100
(Commitment Bonus)
+
$250
(Save UPfront Match)
=
Total: $1,150

You can withdraw $1,150 in September

- √ If you pre-commit to saving, you have the option of saving in February
- √ Since you have pre-committed, you get a $100 bonus for saving at least $300
- √ If you deposit $800, then you have put in $500 above the required $300.
- √ An additional 50% of this $500 will be put in the Save UPfront account
- √ That means an additional $250 will be put in the account
- √ When you withdraw from the account after 8 months, you will have a total of $1,150
- √ If you withdraw your money early, you will forfeit the Save UPfront match, including the $100

Group 3

If you don't pre-commit to saving:

February

Deposit $800

If you deposit $800 in February ...

Save

September

Withdraw $800
+
$250
(Save UPfront Match)
=
Total: $1,050

You can withdraw $1,050 in September

Or

$75
(Non-Commitment Payment)

If you do not save, you receive $75 in September

- √ If you do not pre-commit to saving, you will be given a different savings option in February
- √ If you deposit $800, then you have put in $500 above the required $300.
- √ An additional 50% of this $500 will be put in the Save UPfront account
- √ That means an additional $250 will be put in the account
- √ When you withdraw from the account after 8 months, you will have a total of $1,050
- √ If you withdraw your money early, you will forfeit the Save UPfront match
- √ However, if you do not save at all, since you did not pre-commit, you will receive $75 in September

Group 3

Figure E.12: Initial Mailing: Early Incentive (T5) – Year 1

Save UPfront

Rewarding Saving
Rewarding Planning

The Financial Clinic would like you to participate in our pilot test of a new savings account: **Save UPfront**.

In about a week, we will contact you by phone to discuss **Save UPfront** in more detail.

If you decide to participate in our pilot, file your taxes with us and use the **Save UPfront** account, you can earn up to $475.

Here is how the Save UPfront account rewards **saving**:

- √ You have to file your taxes at one of our designated Money UP sites and have an income tax refund of more than $300
- √ You have to deposit at least $300 in the Save UPfront account
- √ You must keep your money in the account for at least 8 months
- √ For every dollar above $300, we will deposit an additional 50% match in the account

Here is how the Save UPfront account rewards **planning**:

- √ If you pre-commit now to saving in February, you will receive $50 in February, no matter what
- √ If you pre-commit now to saving, your savings match will increase by $100
- √ If you don't pre-commit to now to saving, you still have the option to save, but will get $75 even if you don't save

For Example, if you pre-commit to saving in December, you get $50 in February. AND, if you deposit $600 in a Save UPfront account in February and leave your money in that account until September, you will be able to withdraw $850 in September.

Take a look at the visual aids in included to see more examples of how the Save UPfront account work

Group 1

If you pre-commit to saving:

February

Deposit $800
Receive $50 Back
(Commitment Bonus)

If you deposit $800 in February, you'll get $50 ...

Save

September

Withdraw $800
+
$350
(Save UPfront Match)
=
Total: $1,150

You can withdraw $1,150 in September

Or

Receive $50
(Commitment Bonus)

If you do not save, you still receive the $50 Commitment Bonus

- √ If you pre-commit to saving, you have the option of saving in February
- √ Since you have pre-committed, you get a $50 bonus in February no matter what
- √ If you deposit $800, then you have put in $500 above the required $300.
- √ An additional 50% of this $500 will be put in the Save UPfront account
- √ That means an additional $250 will be put in the account, plus another $100 for pre-committing
- √ When you withdraw from the account after 8 months, you will have a total of $1,150
- √ If you withdraw your money early, you will forfeit the Save UPfront match, including the $100

Group 1

If you don't pre-commit to saving:

February

Deposit $800

If you deposit $800 in February ...

Save

September

Withdraw $800
+
$250
(Save UPfront Match)
=
Total: $1,050

You can withdraw $1,050 in September

Or

$75
(Non-Commitment Payment)

If you do not save, you still receive $75 in September

- √ If you do not pre-commit to saving, you will be given a different savings option in February
- √ If you deposit $800, then you have put in $500 above the required $300.
- √ An additional 50% of this $500 will be put in the Save UPfront account
- √ That means an additional $250 will be put in the account
- √ When you withdraw from the account after 8 months, you will have a total of $1,050
- √ If you withdraw your money early, you will forfeit the Save UPfront match
- √ However, if you do not save at all, since you did not pre-commit, you will receive $75 in September

Group 1

Figure E.13: Initial Mailing: Late Incentive (T6) – Year 1

Save UPfront

Rewarding Saving
Rewarding Planning

The Financial Clinic would like you to participate in our pilot test of a new savings account: **Save UPfront**.

In about a week, we will contact you by phone to discuss **Save UPfront** in more detail.

If you decide to participate in our pilot, file your taxes with us and use the **Save UPfront** account, you can earn up to $475.

Here is how the Save UPfront account rewards **saving**:

- √ You have to file your taxes at one of our designated Money UP sites and have an income tax refund of more than $300
- √ You have to deposit at least $300 in the Save UPfront account
- √ You must keep your money in the account for at least 8 months
- √ For every dollar above $300, we will deposit an additional 50% match in the account

Here is how the Save UPfront account rewards **planning**:

- √ If you pre-commit now to saving in February, you will receive $50 in September, no matter what
- √ If you pre-commit now to saving, your savings match will increase by $100
- √ If you don't pre-commit to now to saving, you still have the option to save, but will get $75 even if you don't save

For Example, if you pre-commit to saving in December, deposit $600 in a Save UPfront account in February and leave your money in that account until September, you will be able to withdraw $900 in September.

Take a look at the visual aids in included to see more examples of how the Save UPfront account work

Group 2

If you pre-commit to saving:

February

Deposit $800

If you deposit $800 in February ...

Save

September

Withdraw $800
+
$50
(Commitment Bonus)
+
$350
(Save UPfront Match)
=
Total: $1,200

You can withdraw $1,200 in September

Or

$50
(Commitment Bonus)

If you do not save, you still receive the $50 Commitment Bonus

- √ If you pre-commit to saving, you have the option of saving in February
- √ Since you have pre-committed, you get a $50 bonus in September no matter what
- √ If you deposit $800, then you have put in $500 above the required $300.
- √ An additional 50% of this $500 will be put in the Save UPfront account
- √ That means an additional $250 will be put in the account, plus another $100 for pre-committing
- √ When you withdraw from the account after 8 months, you will have a total of $1,200
- √ If you withdraw your money early, you will forfeit the Save UPfront match, including the $100

Group 2

If you don't pre-commit to saving:

February

Deposit $800

If you deposit $800 in February ...

Save

September

Withdraw $800
+
$250
(Save UPfront Match)
=
Total: $1,050

You can withdraw $1,050 in September

Or

$75
(Non-Commitment Payment)

If you do not save, you receive $75 in September

- √ If you do not pre-commit to saving, you will be given a different savings option in February
- √ If you deposit $800, then you have put in $500 above the required $300.
- √ An additional 50% of this $500 will be put in the Save UPfront account
- √ That means an additional $250 will be put in the account
- √ When you withdraw from the account after 8 months, you will have a total of $1,050
- √ If you withdraw your money early, you will forfeit the Save UPfront match
- √ However, if you do not save at all, since you did not pre-commit, you will receive $75 in September

Group 2

Figure E.14: Initial Mailing: All Groups – Year 2

Save UP

Rewarding Saving

The Financial Clinic would like you to participate in our pilot test of a new savings account: **SaveUP**.

In about a week, we will contact you by phone to discuss **SaveUP** in more detail.

If you decide to participate in our pilot, file your taxes with us and use the **SaveUP** account, you can earn up to $475.

Here is how the SaveUP account works:

- √ You have to file your taxes at one of our designated MoneyUP sites and have an income tax refund of more than $300
- √ You have to deposit at least $300 in the SaveUP account
- √ You must keep your money in the account for at least 8 months
- √ For every dollar above $300, we will deposit an additional 50% match in the account

For Example, if you deposit $600 in a SaveUP account in February and leave your money in that account until October, you will be able to withdraw $750 in October.

Take a look at the visual aid in this mailing to see more examples of how the SaveUP account works.

If you have any questions, you can contact us by phone at **(347) 832-8749**, or by email at saveup@thefinancialclinic.org.

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February		**October**
Deposit $800	Save →	Withdraw $800 + $250 (SaveUP Match) = Total: $1,050
If you deposit $800 in February ...		You can withdraw $1,050 in October

- √ If you deposit $800, then you have put in $500 above the required $300.
- √ An additional 50% of this $500 will be put in the Save UP account
- √ That means an additional $250 will be put in the account
- √ When you withdraw from the account after 8 months, you will have a total of $1,050
- √ If you withdraw your money early, you will forfeit the SaveUP match
- √ When we contact you, we will discuss additional options that may allow you to earn up to $475

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Appendix F: Additional Results

Table F.4: ITT Estimates – Precommitment Decision

	(1)	(2)	(3)	(4)
	Treatment Group 5 (early incentive)		Treatment Group 6 (late incentive)	
Treatment Effect	0.092	0.091	0.092	0.089
	[0.035]***	[0.035]**	[0.035]***	[0.035]**
	(0.038)***	(0.038)**	(0.039)**	(0.039)**
Control Mean	0.051	0.052	0.051	0.052
	[0.019]***	[0.019]***	[0.019]***	[0.019]***
Treatment Bounds				
Upper Bound	0.885	0.881	0.892	0.890
	[0.028]***	[0.029]***	[0.027]***	[0.028]***
Lower Bound	−0.770	−0.767	−0.770	−0.769
	[0.038]***	[0.039]***	[0.038]***	[0.038]***
N	417	417	417	417
Controls	No	Yes	No	Yes

Note: Intent-to-Treat effects on soft-committing to save for treatment arms 5 and 6 are relative to control arm 4. Upper and lower bounds are calculated using methods outlined by Horowitz and Manski (2000). Robust standard errors for the treatment effects are reported in brackets and randomization inference standard errors are reported in parentheses. One, two and three stars denote statistical significance at the 10, 5 and 1 percent level respectively.

Table F.5: ITT Estimates – Savings Decision

	(1)	(2)	(3)	(4)
	Treatment Group 2 (immediate incentive)		Treatment Group 3 (delayed incentive)	
Treatment Effect	−0.001	0.000	0.006	0.008
	[0.025]	[0.025]	[0.025]	[0.027]
	(0.023)	(0.026)	(0.025)	(0.026)
Control Mean	0.044	0.043	0.044	0.043
	[0.018]**	[0.018]**	[0.018]**	[0.018]**
Treatment Bounds				
Upper Bound	0.956	0.955	0.928	0.928
	[0.018]***	[0.018]***	[0.023]***	[0.022]***
Lower Bound	−0.898	−0.896	−0.892	−0.887
	[0.027]***	[0.027]***	[0.027]***	[0.028]***
N	416	416	416	416
Controls	No	Yes	No	Yes

Note: Intent-to-Treat effects on saving for treatment arms 2 and 3 are relative to treatment arm 1. Upper and lower bounds are calculated using methods outlined by Horowitz and Manski (2000). Robust standard errors for the treatment effects are reported in brackets and randomization inference standard errors are reported in parentheses. One, two and three stars denote statistical significance at the 10, 5 and 1 percent level respectively.

Table F.6: Beta Delta Estimates – ITT

	(1)	(2)	(3)	(4)
	δ		β	
Point Estimates	1.000	0.981	-9.846	58.960
	[0.456]**	[0.449]**	[405.169]	[11103.555]
N	417	417	833	833
Controls	No	Yes	No	Yes
Upper Bound	9.718	9.792	$-1.42e{+}04$	69566.732
	[3.617]***	[3.674]***	[5.53e+05]	[1.32e+07]
Lower Bound	0.104	0.101	0.001	0.001
	[0.039]***	[0.038]***	[0.003]	[0.003]
N	417	417	833	833
Controls	No	Yes	No	Yes

Note: Estimates for δ and β are calculated using the results from Tables (F.4) and (F.5). One, two and three stars denote statistical significance at the 10, 5 and 1 percent level respectively.

Appendix G: Second Year Data Analysis

In the year following our first study, we returned to the field in order to collect additional data. In the fall of 2011, we recruited participants from a pool of 994 tax filers from the 2010 tax filing season. Table G.7 demonstrates balance in predetermined observables. We conducted essentially the same field experiment as in our first year.

G.1 Addressing Attrition

In an effort to overcome issues with high attrition and differential attrition, we increased the number of RAs used to make phone calls and also made a small change to the initial information mailing sent out. Instead of including experimental arm-specific information in the initial mailing, we sent out a uniform description of the savings product prior to our first phone call. The uniform mailing sent out in the second year is illustrated in Figure E.14. The uniform mailing appears to have eliminated the problem of differential attrition — in Table G.8 there is no statistically significant difference in attrition across the experimental arms. Though the additional outreach effort may have decreased overall attrition some, the low levels of attrition experienced in the first year appear to be a fixture of the sample at hand.

G.2 Outcomes by Experimental Arm

We present the analogue of Tables 3-5 for year 2 outcomes below in Tables G.9-G.11. A key difference to note in Table G.9 is that share who soft-commit to save in the commitment groups is overall higher, but the difference between the control and treatment arms is no longer statistically significant. In fact, treatment arm 5 has a lower level of soft-committing to save than the control arm. We return to this pattern in more detail below. The share saving across the different experimental arms is comparable to that in year 1, while the average savings amounts are somewhat higher in year 2. Conditioning on phone contact or site appearance in Tables G.10 and G.11 has the expected effect on the scale of the outcomes.

G.3 Treatment Effects Conditional on Non-Attrition and Ceiling Effects for Soft-Commitment

Tables G.12-G.13 for year 2 correspond to Tables 6-7 for year 1. As noted above, the treatment effects for soft-committing to save among treatment arms 5 and 6 are no longer statistically significant in Table G.12. In fact, the soft-commitment rate is slightly lower in most cases relative to the control arm 1. We attribute this phenomenon to a "ceiling effect" — the baseline rate of soft-committing to save among members of control arm 1 is much higher in year 2 at about 68%. This rate of soft-committing to save is almost as high as the rate among the treatment arms 5 and 6 in year 1. It appears that relative to such a high baseline, we are unable to incentivize much more soft-commitment to save among the treatment arms in year 2. The negative treatment effects in some sense violate the law of demand, but not in a statistically significant way. Ultimately, the presence of this ceiling effect prevents us from using variation in soft-commitment outcomes to estimate our δ parameter using the method outlined in Section 5.4.

In Table G.13, we find that the baseline savings rate among the control arm 1 in year 2 is very similar to that of year 1 — 40% and 43%, respectively. We estimate a much smaller treatment effect for saving in year 2 — 5 and 3 percentage points for treatment arms 2 and 3 in year 2, compared to 57 and 20 percentage points in year 1. However, we still detect a similar qualitative pattern: the immediate effect (treatment arm 1) is relatively large compared to the delayed effect (treatment arm 2) in both years. The patterns also yield similar quantitative estimates of the composite discount factor, $\beta\delta$, as we will see below.

G.4 Estimation Results

We conduct our test for time-inconsistency under no uncertainty using data from the year 2 in Table G.14, which corresponds to Table 9 in year 1. We again find evidence consistent with time-inconsistency — $\mathbf{E}\left[a_2^C\right] > \mathbf{E}\left[a_2^{NC}\right]$ — and additionally find evidence consistent with the presence of naïve agents — $\mathbf{E}\left[a_1^C\right] > \mathbf{E}\left[a_2^C\right]$. The differences, however, are not statistically different.

As noted above, ceiling effects preclude us from using the early and late treatment effects to estimate δ in year 2. Furthermore, the negative signed effects immediate reject our model. For this reason, we do not use the soft-commitment outcomes in year 2 to estimate our parameters of interest. We can, however, pool saving outcomes across years 1 and 2 when estimating $\beta\delta$ using the method in Section 5.4. We show pooled results, using only soft-commitment outcomes in year 1, in Table G.15. Our estimates of β are only slightly different when pooling data over the two years. We estimate values of β ranging from 0.48-0.50 in when pooling year 1 and 2 savings outcomes, as compared to 0.34-0.45 when using only year 1 outcomes.

Finally, we estimate $\beta\delta$ in Table G.16 using the Convex Time Budget method described in Section D with year 2 savings outcomes. Our estimates of the discount factor using data from year 2 are comparable to those in year 1, especially when looking at the OLS results. However, when incorporating corner outcomes, the estimates are now higher, as more individuals are now found at the upper limit than at the lower limit on savings. For reasonable levels of risk aversion — $\gamma \in \{1, 2\}$ — the estimates for $\beta\delta$ range from 0.23-0.54 in year 2, as compared to 0.20-0.49 in year 1. The sightly higher values of $\beta\delta$ in year 2 relative to year 1 is consistent with the pattern observed when comparing Tables 11 and G.15 — the alternative estimates of β using only year 1 or both year 1 and 2 data, respectively.

Table G.7: Baseline Descriptive Statistics, Year 2

	(1)	(2)	(3)	(4)	(5)	(6)
	Non-Commitment Groups			Commitment Groups		
	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6
Age	48.07	47.01	43.36***	45.62	45.40	43.90**
	[1.22]	[1.21]	[1.19]	[1.13]	[1.13]	[1.07]
AGI	19,536	16,654**	15,515***	18,477	16,522**	17,790
	[1,016]	[912]	[916]	[908]	[932]	[998]
Federal Refund	1,845	1,857	1,963	2,013	1,872	1,691
	[138]	[154]	[160]	[149]	[150]	[134]
NY Refund	532	560	659	633	590	514
	[56]	[60]	[65]	[59]	[56]	[57]
Depedents	0.52	0.58	0.73*	0.52	0.62	0.54
	[0.07]	[0.08]	[0.08]	[0.07]	[0.08]	[0.07]
Married	0.19	0.17	0.15	0.16	0.19	0.13
	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]
High School	0.76	0.75	0.73	0.76	0.76	0.76
	[0.03]	[0.04]	[0.04]	[0.03]	[0.03]	[0.03]
College+	0.24	0.25	0.27	0.24	0.24	0.24
	[0.03]	[0.04]	[0.04]	[0.03]	[0.03]	[0.03]
African-American	0.46	0.39	0.41	0.44	0.39	0.37
	[0.04]	[0.04]	[0.04]	[0.04]	[0.04]	[0.04]
Asian	0.15	0.20	0.17	0.14	0.17	0.13
	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]
Hispanic	0.27	0.25	0.27	0.27	0.28	0.34
	[0.04]	[0.03]	[0.04]	[0.04]	[0.04]	[0.04]
White	0.05	0.10	0.07	0.06	0.08	0.06
	[0.02]	[0.02]	[0.02]	[0.02]	[0.02]	[0.02]
Bank Account	0.83	0.87	0.87	0.89	0.84	0.88
	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]
N	165	166	166	165	166	166

Note: Descriptive statistics for 6 treatment groups are tax year 2009 and tax-filing season 2010 variables established prior to the intervention. Robust standard errors are reported in brackets. One, two and three stars denote statistically significant difference from treatment group 1 at the 10, 5 and 1 percent levels respectively.

Table G.8: Experimental Group Survival Rates, Year 2

	(1)	(2)	(3)	(4)	(5)	(6)
	Non-Commitment Groups			Commitment Groups		
	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6
Reached by Phone	0.22	0.25	0.16	0.22	0.17	0.24
	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]
p-value between	.	0.54	0.20	1.00	0.25	0.62
p-value within	.	0.54	0.20	.	0.25	0.62
Consented to Study on Phone	0.17	0.20	0.14	0.17	0.15	0.22
	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]
p-value between	.	0.49	0.43	1.00	0.64	0.28
p-value within	.	0.49	0.43	.	0.64	0.28
Appeared On Site	0.12	0.12	0.13	0.19	0.16	0.20
	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]	[0.03]
p-value between	.	0.98	0.88	0.09*	0.35	0.04**
p-value within	.	0.98	0.88	.	0.45	0.70
Consented to Study on Phone	0.78	0.80	0.85	0.78	0.89	0.90
(Conditional on Phone Contact)	[0.07]	[0.06]	[0.07]	[0.07]	[0.06]	[0.05]
p-value between	.	0.77	0.45	1.00	0.21	0.15
p-value within	.	0.77	0.45	.	0.21	0.15
Appeared On Site	0.43	0.41	0.50	0.61	0.62	0.63
(Conditional on Phone Contact)	[0.07]	[0.07]	[0.08]	[0.07]	[0.08]	[0.07]
p-value between	.	0.79	0.54	0.08*	0.08*	0.05**
p-value within	.	0.79	0.54	.	0.91	0.82
N	165	166	166	165	166	166

Note: Sample survival rates are the probability of remaining in the study at each stage of the experiment. Two sets of p-values are reported. The "between" p-value measures compares each experimental group to group 1, while the "within" p-value compares either treatment groups 2 and 3 to group 1 or treatment groups 5 and 6 to group 4. One, two and three stars denote statistically significant differences at the 10, 5 and 1 percent level respectively.

Table G.9: Outcomes by Experimental Group, Year 2

	(1)	(2)	(3)	(4)	(5)	(6)
	Non-Commitment Groups			Commitment Groups		
	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6
Pre-commit	0.00	0.00	0.00	0.12	0.08	0.14
	[.]	[.]	[.]	[0.02]	[0.02]	[0.03]
p-value within	.	.	.	.	0.26	0.43
Saving	0.05	0.05	0.05	0.07	0.08	0.13
	[0.02]	[0.02]	[0.02]	[0.02]	[0.02]	[0.03]
p-value between	.	0.81	0.81	0.36	0.26	0.01**
p-value within	.	0.81	0.81	.	0.85	0.10
Saving Amount	34.62	33.48	42.20	56.86	51.54	101.51
	[12.50]	[11.69]	[14.57]	[16.61]	[14.75]	[22.37]
p-value between	.	0.95	0.69	0.28	0.38	0.01***
p-value within	.	0.95	0.69	.	0.81	0.11
N	165	166	166	165	166	166

Note: Table reports the soft-committtment outcomes for treatment groups 1-3, and the saving and saving amount outcomes for all treatment groups. Two sets of p-values are reported. The "between" p-value measures compares each experimental group to group 1, while the "within" p-value compares either treatment groups 2 and 3 to group 1 or treatment groups 5 and 6 to group 4. One, two and three stars denote statistically significant differences at the 10, 5 and 1 percent level respectively.

Table G.10: Outcomes by Experimental Group, Conditional on Phone Consent, Year 2

	(1)	(2)	(3)	(4)	(5)	(6)
	Non-Commitment Groups			Commitment Groups		
	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6
Pre-commit	0.00	0.00	0.00	0.68	0.52	0.67
(Conditional on Phone Contact)	[.]	[.]	[.]	[0.09]	[0.10]	[0.08]
p-value within	.	.	.	.	0.24	0.92
Saving	0.21	0.18	0.17	0.18	0.28	0.36
(Conditional on Phone Contact)	[0.08]	[0.07]	[0.08]	[0.07]	[0.09]	[0.08]
p-value between	.	0.75	0.72	0.74	0.58	0.19
p-value within	.	0.75	0.72	.	0.38	0.09*
Saving Amount	162.96	130.55	136.96	147.57	180.00	273.61
(Conditional on Phone Contact)	[62.57]	[51.80]	[67.95]	[62.33]	[63.44]	[69.54]
p-value between	.	0.69	0.77	0.86	0.85	0.23
p-value within	.	0.69	0.77	.	0.71	0.17
N	28	33	23	28	25	36

Note: Table reports the soft-committment outcomes for treatment groups 1-3, and the saving and saving amount outcomes for all treatment groups, conditional on initially consenting to the study by phone. Two sets of p-values are reported. The "between" p-value measures compares each experimental group to group 1, while the "within" p-value compares either treatment groups 2 and 3 to group 1 or treatment groups 5 and 6 to group 4. One, two and three stars denote statistically significant differences at the 10, 5 and 1 percent level respectively.

Table G.11: Outcomes by Experimental Group, Conditional on Site Appearance, Year 2

	(1)	(2)	(3)	(4)	(5)	(6)
	Non-Commitment Groups			Commitment Groups		
	Group 1	Group 2	Group 3	Group 4	Group 5	Group 6
Pre-commit	0.00	0.00	0.00	0.29	0.27	0.50
(Conditional on Site Appearance)	[.]	[.]	[.]	[0.08]	[0.09]	[0.09]
p-value within	.	.	.	.	0.86	0.08*
Saving	0.40	0.45	0.43	0.39	0.50	0.62
(Conditional on Site Appearance)	[0.11]	[0.11]	[0.11]	[0.09]	[0.10]	[0.08]
p-value between	.	0.75	0.85	0.93	0.50	0.12
p-value within	.	0.75	0.85	.	0.39	0.06*
Saving Amount	285.65	277.90	333.62	302.65	329.08	495.59
(Conditional on Site Appearance)	[85.74]	[79.21]	[94.83]	[74.45]	[74.24]	[79.36]
p-value between	.	0.95	0.70	0.88	0.70	0.07*
p-value within	.	0.95	0.70	.	0.80	0.07*
N	20	20	21	31	26	34

Note: Table reports the soft-committment outcomes for treatment groups 1-3, and the saving and saving amount outcomes for all treatment groups, conditional on showing up at the tax preparation stie. Two sets of p-values are reported. The "between" p-value measures compares each experimental group to group 1, while the "within" p-value compares either treatment groups 2 and 3 to group 1 or treatment groups 5 and 6 to group 4. One, two and three stars denote statistically significant differences at the 10, 5 and 1 percent level respectively.

Table G.12: ITT Estimates Conditional on Phone Consent, Year 2 – Soft-Commitment Decision

	(1)	(2)	(3)	(4)
	Treatment Group 5 (early incentive)		Treatment Group 6 (late incentive)	
Treatment Effect	−0.159	−0.088	−0.012	0.093
	[0.136]	[0.136]	[0.120]	[0.122]
	(0.139)	(0.130)	(0.116)	(0.126)
Control Mean	0.679	0.616	0.679	0.616
	[0.090]***	[0.093]***	[0.090]***	[0.093]***
N	89	89	89	89
Controls	No	Yes	No	Yes
Non-Attrition Rate - Phone Consent				
Treatment Effect	−0.019	−0.014	0.047	0.053
	[0.040]	[0.040]	[0.043]	[0.044]
	(0.041)	(0.042)	(0.041)	(0.042)
Control Mean	0.170	0.167	0.170	0.167
	[0.029]***	[0.029]***	[0.029]***	[0.029]***
Treatment Bounds				
Upper Bound	−0.118	−0.052	0.136	0.242
	[0.168]	[0.175]	[0.160]	[0.145]*
Lower Bound	−0.245	−0.146	−0.082	0.001
	[0.239]	[0.217]	[0.119]	[0.123]
N	497	494	497	494
Controls	No	Yes	No	Yes

Note: Treatment effects on soft-committing to save for treatment arms 5 and 6 are relative to control arm 4 and conditional on non-attrition — i.e. being contacted for the initial phone interview. Non-attrition rates in arms 5 and 6, relative to arm 4 are estimated among the entire sample. Upper and lower bounds are calculated using methods outlined by Behaghel, Crépon, Gurgand, and Le Barbanchon (2009). Robust standard errors for the treatment effects are reported in brackets and randomization inference standard errors are reported in parentheses. One, two and three stars denote statistical significance at the 10, 5 and 1 percent level respectively.

Table G.13: ITT Estimates Conditional on Site Appearance, Year 2 – Savings Decision

	(1)	(2)	(3)	(4)
	Treatment Group 2 (immediate incentive)		Treatment Group 3 (delayed incentive)	
Treatment Effect	0.050	0.046	0.029	0.012
	[0.160]	[0.177]	[0.158]	[0.179]
	(0.162)	(0.165)	(0.165)	(0.168)
Control Mean	0.400	0.407	0.400	0.407
	[0.112]***	[0.125]***	[0.112]***	[0.125]***
N	61	61	61	61
Controls	No	Yes	No	Yes
Non-Attrition Rate - Site Appearance				
Treatment Effect	−0.001	0.004	0.005	0.002
	[0.036]	[0.035]	[0.036]	[0.036]
	(0.034)	(0.036)	(0.033)	(0.037)
Control Mean	0.121	0.121	0.121	0.121
	[0.025]***	[0.024]***	[0.025]***	[0.024]***
Treatment Bounds				
Upper Bound	0.054	0.059	0.045	0.020
	[0.238]	[0.202]	[0.188]	[0.209]
Lower Bound	0.048	0.027	0.003	0.001
	[0.197]	[0.225]	[0.226]	[0.230]
N	497	494	497	494
Controls	No	Yes	No	Yes

Note: Treatment effects on saving for treatment arms 2 and 3 are relative to control arm 1 and conditional on non-attrition — i.e. appearing at the tax site. Non-attrition rates in arms 2 and 3, relative to arm 1 are estimated among the entire sample. Upper and lower bounds are calculated using methods outlined by Behaghel, Crépon, Gurgand, and Le Barbanchon (2009). Robust standard errors for the treatment effects are reported in brackets and randomization inference standard errors are reported in parentheses. One, two and three stars denote statistical significance at the 10, 5 and 1 percent level respectively.

Table G.14: Testing for Time-Inconsistency Under No Uncertainty, Year 2

	(1)	(2)	(3)
	$\mathbb{E}[a_1^C]$	$\mathbb{E}[a_2^C]$	$\mathbb{E}[a_2^{NC}]$
Conditional on Non-Attrition	0.629	0.505	0.426
	[0.052]	[0.053]	[0.064]
N	89	91	61
Balanced Sample	0.750	0.568	0.426
	[0.066]	[0.075]	[0.064]
N	44	44	61

Note: Mean outcomes for commitment and non-commitment group members. Means conditional on non-attrition for soft-commitment and saving decision are among those reached by phone and those who appear on site, respectively. The balanced sample conditions all means on appearing on site.

Table G.15: Beta Delta Estimates, Conditional on Participation, Years 1 and 2

	(1)	(2)	(3)	(4)
	δ		β	
Point Estimates	1.077	1.152	0.496	0.479
	[0.395]***	[0.428]***	[0.645]	[0.645]
N	76	76	168	168
Controls	No	Yes	No	Yes
Upper Bound	4.078	4.645	11.712	12.554
	[5.914]	[7.462]	[73.428]	[88.536]
Lower Bound	0.344	0.394	0.039	0.030
	[0.384]	[0.411]	[0.129]	[0.118]
N	417	417	1330	1327
Controls	No	Yes	No	Yes

Note: Estimates for δ are calculated using Table 6 and estimates for β are calculated using the results from Tables 6, 7 and G.13. One, two and three stars denote statistical significance at the 10, 5 and 1 percent level respectively.

Table G.16: Estimates of $\beta\delta$ – Accounting for Risk Aversion, Year 2

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS			Tobit		
	$\triangle\omega = 0\%$	$\triangle\omega = 10\%$	$\triangle\omega = 25\%$	$\triangle\omega = 0\%$	$\triangle\omega = 10\%$	$\triangle\omega = 25\%$
$\gamma = 1$	0.395***	0.414***	0.447***	0.464***	0.490***	0.536***
	[0.056]	[0.060]	[0.069]	[0.066]	[0.072]	[0.083]
$\gamma = 2$	0.234***	0.257***	0.300***	0.323***	0.360***	0.430***
	[0.066]	[0.075]	[0.092]	[0.091]	[0.105]	[0.133]
$\gamma = 3$	0.139***	0.160***	0.201***	0.225***	0.265***	0.346***
	[0.059]	[0.070]	[0.093]	[0.096]	[0.116]	[0.161]
$\gamma = 4$	0.082***	0.099***	0.135***	0.157***	0.195***	0.278***
	[0.047]	[0.058]	[0.083]	[0.089]	[0.114]	[0.172]
N	27	27	27	56	56	56

Note: Estimates for $\beta\delta$ are calculated using the methods described in Section 5.5. One, two and three stars denote statistically significant difference from 1 at the 10, 5 and 1 percent level respectively.