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# WHEN A NUDGE ISN'T ENOUGH: DEFAULTS AND SAVING AMONG LOW-INCOME TAX FILERS

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## ABSTRACT

Recent evidence suggests that the default options implicit in economic choices (e.g., 401(k) savings by white-collar workers) have extraordinarily large effects on decision-making. This study presents a field experiment that evaluates the effect of defaults on savings among a highly policy-relevant population: low-income tax filers. In the control condition, tax filers could choose (i.e., opt in) to receive some of their federal tax refund in the form of U.S. Savings Bonds. In the treatment condition, a fraction of the tax refund was automatically directed to U.S. Savings Bonds unless tax filers actively chose another allocation. We find that the opt-out default had no impact on savings behavior. Furthermore, our treatment estimate is sufficiently precise to reject effects as small as one-fifth of the participation effects found in the 401(k) literature. Ancillary evidence suggests that this "nudge" was ineffective in part because the low-income tax filers in our study had targeted plans to spend their refunds. These results suggest that choice architecture based on defaults may be less effective in certain policy-relevant settings, particularly where intentions are strong.



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# 1 Introduction

Manipulating the status quo implicit in economic decisions (i.e., the default) is widely viewed as one of the most robust and influential tools in the arsenal of "nudges" (Camerer et al., 2003). Defaults have been shown to have meaningful impacts on a number of individual decisions ranging from organ donation (Abadie and Gay, 2000; Johnson and Goldstein, 2003) to 401(k) contributions (Madrian and Shea, 2001), where the evidence suggests extraordinarily large effects of defaults on savings decisions. The results from these studies are often assumed to generalize to other settings and populations, especially low-income populations (see Bertrand et al., 2006). However, the extant literature provides no evidence on how defaults affect savings behavior, outside of the population of 401(k) holders.[1] This paper presents the results of a field experiment designed to evaluate whether a default manipulation can affect low-income households' decisions to save with their tax refunds.

Policies designed to encourage savings in this population (i.e., low-income households) have often focused specifically on the propitious "savable moment" thought to exist in tax return settings (e.g., Tufano, Schneider and Beverly, 2005). During the 2009 tax-filing season (IRS 2010), over 50 million tax returns reported adjusted gross income of $30,000 or less but generated positive federal refunds (roughly half of all federal returns leading to refunds). Furthermore, these low-income filers received refunds that averaged approximately $2,000. The widespread receipt of such large (but also somewhat uncertain and comparatively irregular) payments implies that low-income tax filers may be particularly responsive to policy interventions that seek to promote savings. For example, an earlier study by Duflo et al. (2006) find that matching grants lead to increased savings among low-income and middle-income tax filers.

[1] While previous studies have investigated how the impact of defaults on 401(k) participation varies with income (Choi et al., 2004; Beshears et al., 2010), the lower bound income for such groups is substantially larger than (approximately double) the mean income of our subject population.

A recent savings innovation advocated by the Obama administration was clearly motivated by the unique policy opportunities thought to exist in the "savable moment." Specifically, during the 2010 tax-filing season, the IRS implemented new procedures that gave tax filers the option to receive some or all of their refunds in the form of low-risk, relatively liquid U.S. Series I Savings Bonds. In announcing this new policy, President Obama emphasized the potential impact of this new savings opportunity in promoting retirement security (Andrews 2010). Advocates of this new policy underscored the role this savings opportunity could play in the general asset development of low-income households. However, a previous 3-year pilot study of the program conducted at Volunteer Income Tax Assistance (VITA) sites found the take-up rate of U.S. Savings Bonds among low-income tax filers to be quite low. Only about 6 percent of eligible tax filers (i.e., those who had positive refund amounts and were using direct deposit to receive their refunds) purchased savings bonds with their refunds (D2D Fund, Inc., 2009).

This study presents a field experiment, conducted at eight Philadelphia-area VITA sites during the 2010 tax-filing season, that asks whether introducing a default contribution of tax refunds to U.S. Savings Bonds can increase participation in this new program. Specifically, we test whether presenting the option to direct some of one's refund to U.S. Savings Bonds as an "opt-out" choice - where the default presumption is that some savings will occur - increases the amount of bonds purchased relative to when the decision is presented as an "opt-in" choice. Defaults like this have a potential advantage over policies like matching grant interventions, in that they do not require financial outlays. Furthermore, the prior literature suggests that default manipulations should be especially powerful precisely for low-income populations. For example, referring to the success of defaults "among the comfortable," Bertrand et al. (2006) state: "it seems safe to assume that defaults would have at least as substantial an impact on the poor, whose options are inherently inferior, and who may be

less informed about available alternatives." This argument draws on the idea that defaults should have a stronger impact when decision costs are high, or information is limited, all else equal (see, e.g., Madrian and Shea, 2001). Consistent with this view, studies of 401(k) holders find that defaults are particularly effective among the lower-income individuals in those populations (Choi et al., 2004; Beshears et al., 2010). However, the lower-bound income of such 401(k) participants is substantially larger than the income of a substantial fraction of the low-income tax filers receiving tax refunds. It is an open question how well defaults may work among lower-income populations for whom the "savable moment" created by tax refunds is most striking but who also face other pressing economic demands on their resources. By randomly assigning tax filers to these two presentations of the decision to purchase savings bonds with their refunds, we provide the first empirical evidence as to the effectiveness of a default manipulation to increase tax-time savings among this policy-relevant population.

Consistent with the prior pilot data, we found that savings bond participation was fairly low (i.e., roughly 9 percent among our control group) but plausibly related to several of the observed traits of the filers (e.g., the tax refund size conditional on income). However, our main finding is that random assignment to the default manipulation had no detectable effect on the decision to allocate some of the tax refunds to U.S. Savings Bonds. Furthermore, our impact estimates are sufficiently precise that we can reject participation effects as small as one-fifth of those found in default studies of 401(k) holders. We use both the available data on the tax filers and an ex-post survey we conducted among the volunteer tax preparers who participated in our study to examine the robustness of this finding as well as its possible explanations. One prominent factor that could explain why the "nudge" was ineffective was the ex ante awareness among tax filers of the likelihood of receiving a large refund and the strong intentions to spend those refunds. Many filers indicated having prior plans to spend

the refund (as well as difficulties paying bills), and tax preparers identified these plans to spend as the leading explanation, when asked why they thought filers often actively resisted the default. Indeed, to the extent that it conflicted with strong intentions to do otherwise, taking the savings default might have required considerable decision costs, eliminating the decision-cost advantages usually associated with taking the default. This might be quite different for the typical 401(k) holders, if they have a baseline intention to save and just need the encouragement of a nudge to get them to make the decision.

We view these results as providing direct evidence on the challenges of designing policies to encourage savings among low-income households. In particular, the ineffectiveness of a policy lever that has demonstrated such extraordinary efficacy in other settings raises questions about the extent to which tax-filing settings really do constitute a savable moment for the many low-income filers who receive large refunds. However, we also view our findings as providing novel – and more general - evidence on the potential limitations of policy nudges for contexts where decision-makers have strong prior intentions. At the very least, our findings suggest that the broad effectiveness of such policies may depend crucially on their design and on the populations and contexts in which they are applied. The paper proceeds as follows. The next section presents a brief review of the relevant literature on defaults and savings decisions. Section 3 describes the policy innovation introduced during the 2010 tax season, which allowed filers to direct some of their tax refunds to U.S. Savings Bonds. Details of the experimental design and implementation are explained in Section 4. In Section 5 we present the results of our experiment. Section 6 discusses our findings and their implications. Section 7 offers conclusions and some directions for future research.

# 2 Defaults and Economic Decision-Making

Neoclassical economics assumes rational economic agents will save when it is in their best interest to do so. Behavioral economists, observing that many people make decisions that are inconsistent with what perfect rationality would predict, recognize that it may take a nudge to get people to act in their own best interest—in this case, to save. Exploiting status quo bias through the use of defaults is one of the most appealing approaches to influencing behavior. Defaults are generally simple and inexpensive to implement and have the appeal of simultaneously influencing behavior without ultimately limiting choice. Considerable evidence supports the success of defaults in increasing participation rates in retirement savings through automatic enrollments (Beshears et al., 2008; Thaler and Benartzi, 2004; Madrian and Shea, 2001; Choi et al., 2002 and 2004).

Although manipulating the default has been shown to influence behavior, defaults remain a black box, with no widely accepted understanding of why and how they work. We briefly review several of the theoretical explanations for how defaults change behavior. Defaults may offer an implicit or inferred expert recommendation (Madrian and Shea, 2001; Beshears, et al., 2008)). As such, they reduce the complexity of the decision at hand (e.g., whether or not to save and, if so, how much to save) and help decision makers overcome bounded rationality from which they may suffer (see, e.g., Thaler and Benartzi, 2004). In addition, the implied recommendation increases an individual's confidence in engaging in the behavior under consideration. If the default is to participate in an employer's retirement savings plan, it can be perceived as a recommendation from the employer, a recommendation to which expert status is attributed. Accepting the default may also be reinforced by the fact that acts of commission are psychologically more costly than acts of omission (Kahneman and Tversky, 1982). Deviating from the default can raise concerns about making a bad decision and, consequently, suffering regret. But by accepting the default, agents incur, at

worst, costs associated with an error of omission (Choi et al., 2003). Next, depending on the setting in which the decision is being made, agents may accept the default given that they have the option to opt out later, although they may never exercise that option. The default allows them to defer incurring the decision-making costs associated with actively making a decision; this tendency to procrastinate is consistent with hyperbolic discounting (O'Donoghue and Rabin, 1999). Finally, defaults may work because decision makers are not paying attention to the decision at hand; inattention leads to sticking with the default (Samuelson and Zeckhauser, 1988). This is most likely to be relevant regarding decisions about which decision makers do not have strong preferences (Slovic, 1995).

The bulk of the evidence on the success of defaults in financial settings is based on decisions about 401(k) plans by the white collar, educated workers most likely to be offered such plans. The default manipulations in this literature share a number of structural characteristics. First, decision makers have the option to opt out of the default later. Even if they never exercise that option, participants in a retirement savings program know that they can actively choose to stop participating or lower the amount of their contributions. This knowledge may make it easier to accept the default because acquiescence is not perceived as being permanent or irreversible.

Second, the effects of the decision will (at least mostly) not be felt immediately. Defaulting into a retirement savings account does mean foregoing some near term income, but the implications of the decision are mostly experienced in the longer term. Defaulting into a plan to save some portion of future earnings increases has effects that will only be felt in the future. The experimental evidence showing that defaults work to increase savings is primarily from settings in which the effect of the initial decision is largely deferred.

A third consideration is the size of the default. The typical default contribution rate in the studies of successful 401(k) defaults is approximately 2 to 3 percent of income. However,

a recent study by Beshears, Choi, Laibson, and Madrian (2010) examines retirement savings at a single U.K. firm with an unusually high default contribution rate (i.e., 12 percent of before-tax income). They find that comparatively few employees (only 25 percent) remain with this default and conclude that defaults are less powerful when linked to more extreme decisions.[2] This evidence suggests that the success of savings defaults may depend on the default contribution being a relatively small percentage of income.

The evidence that nudges work in some settings is strong and provocative. However, important questions remain unanswered regarding the exact mechanisms behind their effectiveness and what this might imply about the generalizability of default effects to other, policy-relevant settings. How low-income tax filers might respond to defaults is of particular interest in light of the enthusiasm for leveraging the possibilities embedded in the "savable moment." One reasonable conjecture is that defaults would be more effective in a low-income population where decision-makers may have less complete information about their alternatives (Bertrand et al. 2006). For example, low-income households may be particularly responsive to the expert advice implicitly inferred from the presence of a default choice. However, low-income individuals also experience uniquely constrained economic circumstances. Their economic needs could promote an awareness related to financial resources that makes them relatively unresponsive to the design effects of choice architecture. Our research provides direct evidence on whether a default influences the savings behavior of low-income individuals and, indirectly, suggests the generalizability of nudges to important new settings.

[2] Even though most employees at this firm choose to opt out of the extreme default, they also find that most choose to stay with the default investment allocation, suggesting that the limited effect of the default was not due to the study population aggressively managing their finances.

# 3 A Policy Innovation for the "Savable Moment"

On September 5th, 2009, President Obama announced several new initiatives designed to increase individual retirement savings (Andrews, 2009). One of these initiatives – a new IRS rule that would make it easier for small businesses to automatically enroll employees in retirement plans – explicitly acknowledged the evidence that defaults can exert a major influence on savings decisions. A second, prominent feature of this retirement savings initiative was the introduction of an option, beginning with the 2010 tax-filing season, by which tax filers receiving federal refunds could use this money to purchase Series I U.S. Savings Bonds through their tax returns. These bonds are available in denominations of $50, $100, $200, $500, and $1,000, and they accrue interest, which is exempt from state and local but not federal tax, for 30 years. The rate of return on Series I bonds is based on the inflation rate combined with a fixed return. The purchase of a Series I bond implies some illiquidity. In the first 12 months of ownership, the bonds can only be redeemed under certain extreme conditions (e.g., natural disasters). However, a bond that is cashed in within two to five years of purchase forfeits only 3 months of interest. Bond owners can redeem their bonds without any penalty after five years.

Though President Obama's announcement framed the new opportunity to direct tax refunds to savings bonds around the promotion of retirement savings, advocates of this reform emphasize the particular importance of this refund-based savings opportunity for low to moderate income (LMI) families (e.g., Tufano and Schneider, 2005; Cramer et al., 2010). One key motivation for the appeal of this policy is the claim that tax time constitutes a unique "savable moment" in which LMI households can be more easily encouraged to allocate some of their funds to savings. Because of tax policies like the Earned Income Tax Credit (EITC), LMI households often receive quite large, lump-sum federal tax refunds. For

example, our study participants who are described below had an adjusted gross income under $18,000 but received tax refunds of nearly $2,000 (Table 1). Behavioral economics suggests that tax filers may adopt a mental accounting in which they view this large, infrequent, and comparatively unpredictable income differently than conventional income and, consequently, are more willing to save some of it (Beverly et al., 2006).

Advocates for the new IRS policy also stressed that savings bonds are an attractive savings vehicle for LMI families because they combine broad access to a guaranteed real return with limited transaction costs and the name recognition and reliability of the federal government (e.g., Tufano and Schneider, 2005; Cramer et al., 2010). Some early empirical evidence seemed to confirm that there was extensive, latent demand for savings bonds among LMI households. Specifically, 24 percent of participants in a pilot study of low-income tax filers reported they would be likely to purchase savings bonds with their tax refund. And, when savings bonds were described in the survey instrument, the share of respondents who indicated that they would use their refund to purchase savings bonds increased to 76 percent (Beverly et al., 2006, page 153).

However, in a subsequent pilot study in which tax filers could direct some of their tax refunds to savings bonds, the take-up rate was quite low. Specifically, during the 2007, 2008, and 2009 tax-filing seasons, the Doorways to Dreams (D2D) Fund used a special waiver from the U.S. Department of the Treasury to offer low-income tax filers the opportunity to purchase Series I U.S. Savings Bonds with their tax refunds (D2D, Inc., 2009). The 2009 pilot study occurred at 67 Volunteer Income Tax Assistance (VITA) sites that served over 50,000 clients. VITA sites are managed by non-profit organizations in cooperation with the IRS and provide free tax-filing services to low-income clients using tax preparers who have been trained and certified by the IRS. The tax preparers in these pilot sites also completed computer-based training about U.S. Savings Bonds, and the participating sites marketed

U.S. Savings Bonds using bilingual brochures, posters, and leaflets. Despite these efforts, only 6 percent of those eligible to purchase U.S. Savings Bonds during the 2009 tax-filing season (i.e., a refund of at least $50 and direct deposit) actually did so (D2D Fund, Inc., 2009, page 13). Those who did purchase U.S. Savings Bonds saved, on average, about $200 (i.e., roughly 10 percent of their tax refund).

Overall, this evidence suggests that there might be a unique role for a "default" nudge in promoting tax-time savings among low-income households, particularly with respect to the newly available opportunity to purchase U.S. Savings Bonds. Specifically, there is suggestive pilot evidence that low-income tax filers have a fairly strong intentionality to direct tax refunds to savings bonds (Beverly et al., 2006) but fail to follow through on this intent in field settings (D2D, Inc. 2009). Moreover, the literature on 401(k) savings suggests that default allocations are particularly effective among lower-income workers (e.g., Choi et al., 2004). In the next section, we describe a field experiment in which the role of a default in promoting the purchase of U.S. Savings Bonds among low-income tax filers was tested directly.

# 4 The Savings Bond Experiment

We conducted a randomized field experiment at eight IRS-sponsored Volunteer Income Tax Assistance (VITA) sites during the 2010 tax-filing season, the first year in which the Obama administration's new policy allowing the purchase of savings bonds with tax refunds was implemented nationwide. Eligible tax filers at these sites were presented with either a conventional opportunity to purchase U.S. Savings Bonds with some of their tax refund (i.e., an "opt-in") or with a scenario in which a fixed percentage of their tax refund would be directed by default to U.S. Savings Bonds unless they actively decided otherwise (i.e., an

"opt-out").

### 4.1 Field Setting and Training

The participating VITA sites were located in Delaware and Montgomery counties just outside of Philadelphia, Pennsylvania (5 sites in Chester and 1 each in Holmes, Media, and Norristown). Tax filers who had annual household income lower than $50,000 were eligible to receive free tax-preparation services at these sites. Each client was served by a volunteer tax preparer who was required to first complete extensive training and earn IRS certification. The tax preparers who participated in our study included volunteers from local colleges (i.e., Swarthmore College, Bryn Mawr College, and Villanova University) as well as community volunteers and some employees of our community-partner organizations. Once preparers had completed the IRS-sponsored training, our research team also directed training sessions in which tax preparers were educated about both the key features of U.S. Savings Bonds and the protocol for this study, which is described below. We also complemented this training with additional site-based oversight and feedback to tax preparers by members of the research team.

In addition to the training and oversight of the participating tax preparers, we also implemented an informational and marketing campaign similar to those utilized in the previous pilots (D2D, Inc. 2009). Specifically, each VITA site (e.g., waiting areas) was decorated with posters and with flyers, some of which were designed to motivate interest in and comfort with U.S. Savings Bonds. Others were designed to be informative about the key features of Series I U.S. Savings Bonds (e.g., the guaranteed rate of return). We also included Spanish-language versions of some posters.[3] Furthermore, we placed one of our most informative savings bond flyers on every tax preparation station so tax filers could read the flyer during

[3] See Appendix B for pictures of these posters and flyers.

the session and when being asked to make a decision about savings bonds. Each tax preparer was also provided with a "Frequently Asked Questions (FAQ)" sheet that would allow him to quickly address any queries (Appendix C).

### 4.2 Study Eligibility and Participants

The VITA tax-filing season began in early February of 2010 and concluded on April 15, 2010. Upon arrival to VITA sites, clients completed an intake procedure that confirmed their eligibility and that they had the appropriate documentation for completing their taxes. They would then go to a tax-preparation station (i.e., a desk, cubicle or table, depending on the site) where a trained volunteer would prepare their taxes using "TaxWise" software. Near the end of the tax-preparation process, the preparer would make a determination as to whether the tax filer was eligible for study participation.

Only tax filers who received federal tax refunds of at least $50 were eligible because that is the smallest denomination of the bonds. Furthermore, by IRS design, only those respondents receiving their tax refunds through direct deposit were eligible to purchase U.S. Savings Bonds (and, by implication, eligible for the study). Specifically, tax filers interested in using their refund to purchase U.S. Savings Bonds had to use the form that allowed "refund-splitting" across direct-deposit accounts.[4] Therefore, as in the earlier pilot studies (D2D, Inc. 2009), the eligible study participants were those using a direct deposit account to receive a federal tax refund of at least $50.

Table 1 presents data on the observed characteristics of the study participants (n = 259)

[4] The IRS implemented the option to purchase savings bonds through Form 8888, which allows a tax filer to split his or her refund into two or three direct deposit accounts. To purchase savings bonds with a refund, the tax preparer used a specific IRS routing number and the account number "BONDS" on this form, treating savings bonds as another direct deposit account. Therefore, only filers who were receiving the remainder of their refund by direct deposit could purchase U.S. savings bonds through their IRS tax returns.

using both the key study data collected as part of the experimental procedures described in the next section (e.g., federal refund amount and adjusted gross income) and the participant responses to a "site survey" completed at the conclusion of each tax-preparation session. This brief survey elicited information about demographic traits (e.g., age, sex, race), educational attainment, and whether the client had financial hardship (i.e., trouble with any bills).[5] The results indicate that participants received, on average, federal tax refunds of roughly $1,900, an amount equal to more than 10 percent of the average AGI of $17,990. The average age of study participants was 37 years; over 68 percent of the study participants were female and over 44 percent of study participants identified their race/ethnicity as black.[6] Nearly 38 percent of participants identified themselves as having dependents. And, interestingly, the results in Table 1 indicate that nearly 70 percent stated that they had trouble paying bills while only 17 percent stated they had plans to save some of their refund.

As with any empirical study, experimental or otherwise, it is useful to consider whether inferences based on this study population are likely to have broader "external validity." For example, the participants in this study are a select sample both because they chose to use the VITA services and because they had to arrive at the participating sites when participating tax preparers were there. Furthermore, as noted above, the use of direct deposit for receiving tax refunds was a requirement for having the option to choose U.S. Savings Bonds.

The empirical relevance of concerns like these is perhaps best evaluated through conducting replication studies with heterogeneous design features and populations. However, several factors indirectly suggest that our findings are likely to have broader policy generalizability. For example, though clients at VITA sites could conceivably respond differently to policy

[5] The response rate to the survey varied by question but was generally in the range of 85 to 90 percent; data on AGI, refund amounts and saving decisions are available for all study participants.

[6] Nearly all of the remaining study participants who answered this question self-identified as Caucasian rather than Hispanic or Asian so the reference category constructed here is for non-Black respondents who answered the race/ethnicity question.

interventions than other low-income tax filers, they are also a uniquely important population both because of their prevalence and because such settings are likely to be particularly salient for interventions that seek to exploit the "savable moment."[7] Similarly, relying only on tax filers using direct deposit is a policy-relevant screen because it is a binding requirement for saving in this important setting. Regardless, it should be noted that the use of direct deposit in this population is actually quite high (roughly 60% of those receiving refunds), perhaps because VITA sites strongly encourage its use.

The available data from the participating VITA sites provide another way to examine this question. For example, using site-level data from the end-of-year reports, we found that both the average AGI and refunds amounts of the study participants were quite similar to those of the other VITA clients at these sites.

The detailed site-survey data available from VITA clients who did not participate in the study provide an additional way to make such comparisons (Table 1). These data indicate that the study participants were similar to the non-participants with regard to several traits such as race, the presence of dependents and whether they stated they had trouble paying bills. However, these comparisons also suggest some notable and statistically significant differences (Table 1). For example, those included in the study tended to be younger, female, and were more likely to have attended (though not completed) college. These distinctions could conceivably reflect how observables that predict lower-income employment (i.e., comparative youth and moderate education) also drive receipt of the Earned Income Tax Credit (EITC), the existence of non-trivial refunds and, by implication, eligibility for purchasing U.S. Savings Bonds and appearing in this study. Interestingly, the study participants were also somewhat less likely to file their tax returns during the last few weeks of the tax season (Table 1). A possible explanation for this difference is that tax filers anticipating larger

[7] During the 2010 tax year, over 3 million tax returns were filed at VITA sites (IRS 2010).

refunds (and, therefore, eligible to purchase U.S. Savings Bonds) tended to file early because they had some awareness of the likelihood of a meaningful refund and specific intentions for spending that money. Another way that this study engages issues related to heterogeneity is by evaluating our treatment estimates among subgroups defined by key observed traits such as having filed late in the season. However, it should be noted that there might be interesting external-validity issues that are beyond the scope of this study. For example, if responsiveness to choice defaults is mediated by region-specific cultural norms, results based on tax filers who are not in or near large, northeastern cities such as Philadelphia may differ.

### 4.3 Permuted Block Randomization and Treatment Balance

Once clients' tax refunds and study eligibility were determined, tax preparers presented them with either a control or treatment version of a "Your Refund / Savings Bond Worksheet." These worksheets are presented in Appendix A, and their presentation and design is discussed in detail below. We randomized clients to either the control (i.e. opt-in) or treatment version of the worksheets using a straightforward procedure that could be understood as a version of the "permuted block" randomization strategies commonly used in clinical trials in medicine (Schulz and Grimes, 2002). Specifically, each tax preparer used a glued pad of worksheets (Appendix A) that alternated between treatment and control versions. Preparers were instructed to use the top worksheet for consecutive clients.

This approach ensured that - within each site and day - every consecutive pair of tax clients would include one treatment and one control (i.e., a permutation between treatment states within each "block" of 2 consecutive subjects at the same site and at the same time). We viewed this design feature as an important part of the study because we anticipated a relatively small sample size and wanted to ensure that these study participants were well balanced across the two conditions. That is, by varying treatment status within each site on

a rolling basis throughout the tax-filing season, we sought to balance across treatment and control conditions the unobserved subject traits that are potential internal-validity threats.

The effectiveness of a random-assignment procedure with respect to balancing unobserved traits cannot be definitively established. However, an important, ad hoc way of assessing the performance of a random-assignment procedure is to compare how observed subject traits, particularly those that influence the outcomes of interest, are balanced across treatment states. We provide evidence on this issue in Table 2, which reports the results of auxiliary regressions in which treatment status is the dependent variable and observed subject traits are the regressors.

Overall, this evidence suggests that the randomization procedure effectively balanced subjects across the treatment and control conditions. For example, the F-tests from these regressions consistently indicate that the observed subject traits are jointly insignificant determinants of treatment status (i.e., the null hypothesis that $R^2 = 0$ cannot be rejected in any of these regressions). Furthermore, almost all of the observed subject traits have statistically insignificant "effects" on treatment status. One exception is that the weakly significant relationship suggesting that mid-season filers (March 1 - March 21) were more likely to receive the treatment worksheet than early filers. However, this relationship would not be statistically significant after correcting for multiple comparisons. Furthermore, our evidence indicates that mid-season filers were not more likely to purchase savings bonds than early filers so this pattern would not suggest a threat to the internal validity associated with random assignment.

### 4.4 The Treatment Contrast

After a client's federal tax refund had been calculated and her eligibility to purchase U.S. Savings Bonds confirmed, preparers were instructed to introduce the client to the "Your

Refund / Savings Bond Worksheet" with a simple statement like, "Now that we've calculated your refund, please review & complete this worksheet on how you will receive your refund. Let me know if you have any questions." Preparers were specifically trained not to mention savings bonds in their introduction and to hand the worksheet to the filer, allowing the filer time to complete it.[8] If filers had trouble reading or understanding the worksheet, preparers were trained to simply read through the worksheet with the filer, following the script on the worksheet. When filers asked specific questions about savings bonds, preparers referred to a U.S. Series I Savings Bond "Frequently Asked Questions" (FAQ) sheet that was kept at the tax preparation station (see Appendix C). Preparers were told that they should not give advice to tax filers, offer a "sales pitch," overemphasize the possibility of opting out of the default, or influence a filer's decision about savings in any way. Throughout the tax season, members of our research team continuously monitored the tax preparation procedures at our study sites to ensure that tax preparers were implementing the experiment in accordance with this design.

Exactly how did the treatment and control worksheets differ? In the control version of the worksheet, the tax filer was presented with her adjusted gross income (AGI), the amount of her federal tax refund, and a brief introductory statement about U.S. Series I Savings Bonds. The filer was then instructed to fill out the amount of her refund she wished to direct to savings bonds, using the following text: "Indicate the amount of U.S. Savings Bonds you want here. (Enter $0 if no bonds purchased; your amount must be a multiple of $50.)" If a filer in the control group took no action, the default was that she would purchase zero savings bonds.

In the treatment version of the savings bond worksheet, the filer was again presented with

[8] Tax preparers were also given reminder cards that included this language and some basic reminders about the experiment protocol. These cards remained at the tax preparation stations for tax preparers to refer to as needed.

her AGI, estimated tax refund, and a brief statement about U.S. Savings Bonds. However, the treatment worksheet also included a small chart, in which an amount of savings bonds, reflecting approximately 10 percent of the tax filer's refund amount, was circled by the tax preparer. The worksheet instructed the filer that "the circled amount below - approximately 10% of your refund - will be automatically directed to U.S. Series I Savings Bonds in your name unless you decide to change that amount." Further down the page, the worksheet gave the filer the opportunity to "opt out" with the following text: (Optional) "If you would want a different amount of U.S. Savings Bonds, indicate the amount here. (Enter $0 if no bonds purchased; your amount must be a multiple of $50.)"

The fundamental distinction between the treatment and control worksheets involves what would happen if the filer took no action. Filers who were assigned to the control condition and took no action purchased no U.S. Savings Bonds. If a filer in the treatment condition took no action, she would purchase the amount of savings bonds circled in the chart, the default reflecting approximately 10 percent of her refund. However, the default manipulation did not limit the choice set available to individuals: Tax filers were always free to choose to receive zero savings bonds (or any other desired amount).

We set the default amount of savings bonds equal to approximately 10 percent of a filer's refund because savers in the earlier pilot study conducted at VITA sites spent about 10 percent of their refunds on savings bonds (D2D Fund, Inc. 2009). We attempted to prevent filers from mistakenly perceiving the ˜10% default to be 10 percent of income, which might have discouraged saving. Specifically, a filer's AGI and federal refund amount were clearly listed at the top of his worksheet, and, in the case of treatment-group filers, were followed by the smaller default amount. Explicitly reminding filers of their incomes and refund amounts before introducing the ˜10% default amount should have reduced the likelihood of mental accounting biases, whereby filers imagined the default bond contribution as larger than it

was.

It should also be noted that we took steps to minimize the possibility of treatment contamination; that is, that filers might overhear or see another filer receiving the alternative presentation of the savings bond decision. First, at many of the VITA sites in our study, including the two largest sites, tax preparation sessions took place in office cubicles separated by high dividers. At all sites, tax preparation sessions began at staggered times and took different amounts of time to reach the point at which the savings bond decision was introduced. In addition, we generally observed that tax filers and tax preparers attempted to enhance the feeling of privacy at the sites by, for example, speaking quietly and referring to sensitive information on paper rather than aloud. We also instructed tax preparers to keep the treatment and control materials in a closed folder when not in use. There was no indication from either tax preparers or clients that clients had perceived the treatment contrast that was implemented.

# 5 Results

## 5.1 Effect of the Default Manipulation on Tax Filers' Savings Bond Purchases

The default manipulation in our study may affect the savings decisions of low-income tax filers in two key ways. At the extensive margin, the introduction of the default equal to approximately 10 percent of the filer's refund may increase participation in the savings bond program. At the intensive margin, the default may increase the amount of bonds purchased among those who participate.

Table 3 presents evidence on the participation effect. The results shown are from regressions of an indicator for having purchased any savings bonds on treatment status and a number of controls for observable characteristics that may impact savings decisions. The

parsimonious model (column 1) includes only controls for the amount of the filer's federal refund and the filer's AGI. Subsequent models introduce controls for quadratics in the filer's federal refund amount and AGI (2); basic demographic characteristics like age, gender, and race, educational attainment, dependents, and filing status (3); site dummies (4); an interaction term for female filers with dependents (5); indicators for having a "high fidelity tax preparer" or a preparer who thought savings bonds were a good idea for filers (6); and dummies for filing mid-season or late in the season (7).[9]

Regardless of the model, we find treatment status to have no statistically significant effect on the probability that filers in our sample purchase a positive amount of savings bonds with their refunds.[10] Moreover, 95 percent confidence intervals around our point estimates suggest a maximum treatment effect of approximately 8 percentage points, which is far smaller than the range of estimates from the literature on default effects on savings participation in the context of 401(k) contributions (see, e.g., Madrian and Shea, 2001, who report a 50 percentage point increase in participation rates for new hires).[11] Our finding that introducing a default of positive savings has no effect on low-income tax filers' participation may be surprising in light of the existing literature showing large default effects on savings decisions in the context of 401(k) contributions among middle-to-upper class workers offered

[9] Specifically, a "high fidelity tax preparer" is a preparer who responded to our tax preparer survey and answered "agree" or "strongly agree" (6 or 7 on a 7-point scale) to the statement, "I prepared and presented the savings-bond worksheets according to the procedures." Tax preparers who felt savings bonds were a good idea were those who responded to our tax preparer survey and responded "agree"or "strongly agree" to the statement, "It is a good idea for filers to put some of their tax refunds into savings bonds."

[10] The results from estimation of analogous probit and logit models are qualitatively similar and find no significant effect of treatment status on savings bond participation. These results are available upon request.

[11] Savings participation rates in the control condition (9 percent) are also much smaller than participation rates induced by "active choice" interventions described in the 401(k) literature. For example, Carroll et al. (2008) report a 28 percentage point increase in savings participation relative to the standard opt-in procedure. Thus, our population appears to be less susceptible to such active-choice interventions.

401(k) accounts by their employers. We discuss potential explanations for the differing results below.

We also examine savings decisions at the intensive margin, but again our findings suggest no treatment effect. Results from both Tobit and OLS specifications show that treatment status does not significantly impact the amount of savings bonds purchased, conditional on participation.[12] Moreover, the treatment did not appear to cause filers who saved to be more likely to choose the "default" amount of savings bonds associated with their refunds (approximately 10 percent). Indeed, the percentage of treatment-group savers who chose the default amount of bonds did not differ significantly from the fraction of control-group savers who did so.

### 5.2 Treatment Effect Heterogeneity

While we find no evidence of a treatment effect on savings bond take-up, the results in Table 3 indicate that some observable characteristics do predict savings bond participation among low-income tax filers in our sample. For example, our estimates suggest that having a larger federal refund increases the likelihood of purchasing savings bonds; the coefficient on the square of the federal refund amount is negative, suggesting concavity in this relationship. Black filers in our sample were about 10 percentage points more likely to buy savings bonds with their refund, while being a female with dependents increased the likelihood of savings bond take-up by about 18-20 percentage points. Filers who had a tax preparer that reported feeling that savings bonds were a good idea for filers were about 9-10 percentage points more likely to buy savings bonds.

Finally, those who filed during the last 3 weeks of tax season were significantly more likely to buy savings bonds than those who filed during the first four weeks of the season.

[12] These results are available from the authors upon request.

This may suggest that late filers did not have the same pressing needs as early filers to spend their refund paying bills or reducing debt and thus were more able to save. Therefore, while the default manipulation had no effect on savings, it was not the case that savings decisions at tax filing time were completely insensitive to characteristics or situation (see also Duflo et al. (2006), for evidence that financial incentives can induce saving a portion of tax refunds).

That savings bond participation is predicted by filer characteristics like refund amount, race, motherhood, time of filing, and tax preparer traits suggests possible heterogeneity in treatment effects. For instance, the default manipulation may increase savings among certain subgroups of taxpayers, such as those who file late in the season or those with large refunds.

Accordingly, in Table 4 we present estimated treatment effects for various subgroups of low-income tax filers, splitting our sample first by filer traits, including race, gender, dependents, education, filing status, AGI (greater than or less than median), refund amount (greater than or less than median), whether filers reported currently having trouble with any bills, and whether filers had their taxes prepared by the same VITA site in the previous year. These results are displayed in the top panel of Table 4 and are consistent with our previous findings. Again, we find no evidence of a treatment effect among any of these subgroups of filers, nor do we find significant differences in the estimated effect of the treatment across relevant subgroups.[13]

Next, we consider whether treatment effects differed among clients in our sample depending on when in the tax season they filed. We find no statistically significant treatment effect among filers who filed early in the tax season (during the month of February), midway through the season (between March 1st and March 21st), or late in the season (March 22nd through April 15th). However, recall from Table 3 that clients who file late (in the last 3 weeks of tax season) are more likely to purchase savings bonds than those who file early in

[13]The results are qualitatively unchanged when we estimate regressions that include interactions between the treatment and the relevant characteristics using the full sample of 259 observations.

the tax season. In short, while intentions to save (spend) appear to be stronger among late (early) filers, our default manipulation does not "nudge" late filers to save more often.

Finally, perhaps the default increased savings bond participation among filers who had a certain type of tax preparer. Similar results have been found in other experiments implemented by tax preparers during tax preparation sessions (see, e.g., Chetty and Saez, 2009, or Duflo et al., 2006). Moreover, our initial results suggested that having a tax preparer who reported feeling that savings bonds were a good idea for filers raised the probability a filer bought savings bonds by about 9-10 percentage points. If these tax preparers implemented the experiment with the desired treatment-control contrast while other tax preparers did not, we may find a treatment effect among filers who were served by these "enthusiastic" preparers.

The bottom panel of Table 4 displays estimated effects of the ˜10% default on savings bond participation among tax filers with preparers who responded to our preparer survey, who felt confident that they followed the experimental protocol, or who were enthusiastic about savings bonds as being a good idea for low-income tax filers.[14] Because filers may have been more likely to trust an implicit recommendation from a non-student tax preparer, the final two rows compare estimated treatment effects for filers served by student and non-student tax preparers. Once again our evidence suggests that the default manipulation had no significant impact on savings participation decisions among low-income tax filers, even those served by high-quality, enthusiastic, or non-student tax preparers.

In short, our results indicate that the default manipulation in our experiment had no discernable impact on low-income tax filers' savings decisions. Moreover, 95 percent confidence intervals around our key treatment effect estimates (in Table 3) suggest that the

[14] Recall that this last subgroup of preparers responded "agree" or "strongly agree" (6 or 7 on a 7-point scale) to the statement, "It is a good idea for filers to put some of their refunds into savings bonds."

treatment raised savings bond participation by no more than approximately 8 percentage points. That is, the upper bound on our estimate of the treatment effect is far smaller than the estimated default effects on savings participation found in the 401(k) literature. We compare our default manipulation to those in the literature on 401(k) defaults below.

### 5.3 Comparing the Savings-Bond and 401(k) Defaults

The default manipulation in this study incorporated two features thought to be important for defaults to influence behavior. First, there was an implicit recommendation to save 10 percent of the refund in the treatment condition, while there was no specific recommendation in the control condition. The recommendation aspect of default interventions is thought to influence behavior, by lowering decision costs: To the extent that people avoid saving due to the decision costs associated with choosing how much to save, the recommendation lowers costs and works in the direction of increasing saving. Second, the passive decision in the treatment condition was buying bonds, whereas the passive decision in the control condition was to buy no bonds. To the extent that the decision costs—cognitive and psychological (e.g., regret)—are lower for passive decisions than for active decisions, the treatment condition should favor buying bonds relative to the control condition.

However, there are some noteworthy differences between our default intervention, and the main type of default intervention considered in prior 401(k) studies of defaults. One difference is the size of the savings default, which is relatively low in our intervention. Specifically, because U.S. Series I Savings Bonds must be bought in multiples of $50, the default amount actually reflected 10 percent of a filer's refund rounded down to the nearest multiple of $50. The average default bond amount in our sample actually reflected only 6.5 percent of the tax refund and only 1.1 percent of adjusted gross income. In contrast, the default contribution rate in 401(k) studies is typically at least 2 to 3 percent of income and, in two prominent

studies, 6 percent of income (Choi et al., 2002 and 2004). The relatively low default amount in our study would seem to have made the default more palatable for subjects and enhanced the potential treatment effect, all else equal.

Another difference between our treatment contrast and those in 401(k) defaults is the source of the implicit recommendation. In our study the recommendation was presented on an official-looking form, and could have been interpreted as coming from either the tax assistance organization, or possibly the IRS. Having the recommendation delivered by form, rather than verbally by the tax preparer, made clear to subjects that the recommendation was not from the individual tax preparer. In contrast, the defaults in 401(k) studies were set by employers, and thus the recommendation was from employers. It is not clear whether the advice of tax advice organizations, or an employer, should be more or less authoritative. What is clear is that our subjects came to the non-profit organizations seeking help, and thus they should have been particularly open to receiving advice, if anything, potentially enhancing the power of the default.

One difference that could have weakened the power of our default was the perhaps more limited scope for procrastination compared to the 401(k) setting. In the case of automatic enrollment in 401(k) plans, individuals knew that they could always change their mind about their savings contribution rate by making a phone call (Madrian and Shea, 2001). To the extent that individuals are naïve hyperbolic discounters, they might anticipate making this change, but always put off making the call to the future. This phone call option tends to make the default seem less binding ex ante, and might enhance participation, while in fact procrastination ensures that the default is very binding. In our setting, by contrast, individuals made a one-time decision to save some of their refund, with no explicit opportunity to change their mind later. This might have made the default seem more binding, and discouraged participation, all else equal. On the other hand, the decision was binding only

regarding the current refund, and not next year's refund, and as discussed above, the savings rate was relatively low, both of which would have worked in the other direction, making the default seem like less of a commitment.[15]

One other possible concern about the treatment contrast in this study is noteworthy. Our impact estimates could, in theory, understate the true effect of the default if the control condition also constituted a type of treatment that encouraged savings participation. In particular, the control worksheet compelled the attention of participating subjects to the savings decision. The "fine print" in the instructions asked the subjects to write zero if they wanted no bonds. This was a stipulation of the VITA administrators, who wanted it to be clear that zero was an option. However, if these control conditions caused subjects to feel they had to make an active decision to buy zero bonds, as opposed to passively going with zero, it might be expected to have enhanced savings participation. As shown by Carroll et al. (2009), forcing 401(k) holders to actively choose between zero or positive savings contributions rates increased savings participation rates (although substantially less than the standard intervention of making positive saving the default). Since going with the default in our treatment condition did not require writing anything on the worksheet at all, the decision costs of choosing a positive savings amount were still lower in the treatment condition than control. However, an active-decision aspect to the control condition would have worked in the direction of reducing the treatment contrast. In light of our results, the empirical relevance of this concern is quite limited. More specifically, consider the extreme case where all of the savings participation in the control condition was actually due to the hypothesized active-decision nature of the control worksheet. Under this scenario, the true impact of the default treatment (i.e., relative to a genuinely inert control condition in which

[15] To the extent that individuals are sophisticated hyperbolic discounters (see Laibson, 1997; O'donogue and Rabin, 1999), one might expect that a more binding commitment would make the default even more attractive.

no one saves) would simply be the mean savings-participation rate among those assigned to the default treatment (i.e., roughly 9 percent). This upper bound on our impact estimate is still a fraction (i.e., no more than 20 percent) of the default-participation effects found in 401(k) studies and, therefore, consistent with our main findings in suggesting some possible limitations on the effects of defaults.

# 6 Discussion and Conclusions

Our study is the first to try a default manipulation as a way to influence savings decisions of low-income tax filers. More generally, ours is one of the few to investigate how defaults affect savings behavior, outside of the particular setting of decisions about 401(k) contributions. Thus, our experiment provides an opportunity to see whether the large impact of default interventions in the previous literature generalize to other, policy relevant settings. The fact that we find no discernible effect of the default manipulation raises questions about the power of defaults for different populations and the mechanisms underlying default effects. In this section, we discuss potential explanations for the contrasting results, drawing on ancillary evidence from our surveys of tax filers and tax preparers.

In our survey of tax preparers, we asked about several potential reasons for the weak effect of the treatment, and one explanation emerged as the clear favorite: 79 percent of preparers observed that filers seemed to resist the savings default due to strong prior plans to use the refund for consumption (see Table 5). Our survey of tax filers provides corroborating evidence, indicating that 75% of filers had the expectation that they would spend the refund. As argued in Section 5, the savings default was sufficiently low that going with the default should have been financially feasible. However, the decision-cost advantages of going with the default may have been offset by the psychological costs associated with deviating

from pre-existing plans. A large body of evidence documents the tendency for people to dislike inconsistency with the status quo, or previously held expectations.[16] In fact, default interventions are argued to work partly because they establish a particular status quo, and thus harness status quo bias (Camerer et al., 2003). However, pre-existing plans would tend to create a competing status quo, which could undermine the power of the default manipulation. The fact that going with the default entailed the psychological cost of deviation from previous plans would also have "raised the stakes" of the decision, making it more likely that individuals felt the need to make an active, deliberative choice when considering the default. Having already incurred the fixed costs of making an active decision, individuals would have found the default even less attractive.

In short, a nudge is not a shove. Indeed, 401(k) defaults may be powerful precisely because they coincide with the pre-existing intentions to save of relatively affluent individuals. Previous studies argue that 401(k) holders want to save for retirement, but have trouble actually initiating savings contributions, due to uncertainty about the optimal contribution, the decision costs associated with figuring this out, and the tendency to procrastinate (Madrian and Shea, 2001). The default can thus improve welfare, by providing an implicit recommendation, and lowering the decision costs of choosing a specific savings contribution rate, while moving behavior in a direction that is consistent with previous intentions. To the extent that the default does coincide with intentions of 401(k) holders, this avoids the problem that arises when defaults clash with pre-existing plans.

Our findings offer an important caveat to the common assumption that defaults should have an especially powerful impact on low-income populations. Due to less sophistication, and greater uncertainty, in financial matters, as well as potentially higher decision costs, it

[16] People tend to be "loss averse" with respect to the status quo, or expectations, which means they dislike deviating from these reference points. See, e.g., Kahneman and Tversky (1979), Samuelson and Zeckhauser (1988), Koszegi and Rabin (2006), and Abeler et al. (forthcoming).

has been argued that low-income individuals should be highly susceptible to the implicit recommendation and decision-cost reducing aspects of default interventions, all else equal (Bertrand et al.,2006). However, all else may not be equal, in terms of intentions, or expectations, to save. To the extent that low-income filers do not have strong intentions to save at tax time, defaults may have little effect. One implication is that default interventions aimed at low-income populations might need to be augmented with ex ante efforts to shift expectations.

In this regard it is noteworthy that the survey-based pilot survey discussed above (Beverly et al., 2006, page 153) found that a large fraction of low-income tax filers, roughly 75 percent, expressed an interest in buying savings bonds. This might seem to indicate a strong intention to save, but in fact a much lower fraction, only about 6 percent, actually purchased bonds in a subsequent study conducted at tax time with real savings decisions (D2D Fund, Inc., 2009, page 13). Thus, intentions to save seemed to be weak at the moment of tax filing, similar to our findings. One interpretation is that the expressed interest in savings bonds found by the survey study was partly driven by experimenter demand, and many subjects overstated their willingness to buy savings bonds in the future. However, an alternative interpretation could be that there is a difference between plans to buy bonds in the more distant future, and intentions to buy in the present. Although speculative, this would imply that the source of strong intentions to spend the tax refund at tax time could partly be high short-run discount rates, which differ from lower, long-term discount rates in the sense of hyperbolic discounting (see, e.g., O'Donoghue and Rabin, 1999). If hyperbolic discounting were more prevalent in low-income populations, this could be a reason why low-income populations are more difficult to nudge.

There are also other potential explanations for why default effects were weak for our specific population. Although viewed as less important than filers having plans to spend,

preparers indicated that unfamiliarity with savings bonds may have limited the impact of the treatment (see Table 5). It is hard to say with certainty whether our subject population had a much worse understanding of bonds than 401(k) holders had of the properties of those accounts. The informational campaign, including the stand-up flyer on the tax-preparer's desk, should have helped improve understanding. However, to the extent that discomfort with relatively simple and attractive savings assets like bonds makes low-income individuals resistant to defaults, this is an important caveat regarding the policy relevance of defaults for low-income populations.

Another possibility is that increasing savings using defaults was simply not feasible for members of the treatment group, due to financial constraints. To the extent that individuals were so constrained, or at least perceived themselves to be so constrained, this would have clashed strongly with the default intervention. Similar to having plans to spend, a feeling of being constrained could have meant that taking the default entailed substantial decision costs, and thus that it did not provide a path of least resistance. This would have undermined the ability of the default to influence decisions. Previous evidence, however, suggests that even low-income tax filers probably have some scope for increasing savings, for example if given sufficient inducement in the form of a matching grant (Duflo et al., 2006).

As discussed above in Section 5, there are also some differences between our default intervention and the interventions used in the 401(k) setting, most notably the scope for procrastination. In our setting, people had to make a one-time decision about how much of their refund to save, rather than having the option to change their mind later. On the other hand, there was no commitment for next year's refund, and the commitment is smaller in percentage terms than the typical 401(k) default. In other words, the difference seems to be whether there is a month-long commitment of 6% of income, or a yearlong commitment of 1.1% of income. If this difference explains the contrasting results, then it indicates that

incorporating a procrastination element is crucial, something that is important to take into account when designing policy.

In summary, our findings raise important questions about the applicability, and optimal design, of default interventions for policy measures. Further research is needed to assess the power of defaults for different populations, and to open the black box of the mechanisms behind default effects.

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Figure 1: Savings Bond Participation among Control and Treatment Groups

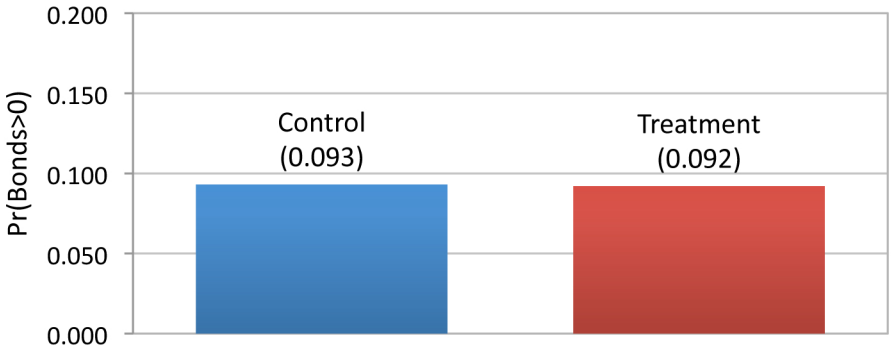

Table 1: Mean Characteristics of Low-Income Tax Filers at Study VITA Sites

| Variable | Filers Included in Study | Filers Not Included in Study | P-values on t test that means are equal |
|---|---|---|---|
| Federal Refund Amount ($) | 1905.60 | ----- | |
| | (2013.90) | | |
| Adjusted Gross Income (AGI) ($) | 17990.3 | ------ | |
| | (14382.40) | | |
| Amount of Savings Bonds Purchased ($) | 9.27 | ------ | |
| | (37.497) | | |
| Any Savings Bonds Purchased? | 0.093 | ------ | |
| | (0.291) | | |
| Female | 0.683 | 0.562 | **0.0008** |
| | (0.466) | (0.497) | |
| Sex Missing | 0.085 | 0.110 | 0.2604 |
| | (0.279) | (0.313) | |
| Age | 36.9 | 41.3 | **0.0067** |
| | (21.6) | (22.4) | |
| Age Missing | 0.131 | 0.117 | 0.5433 |
| | (0.338) | (0.321) | |
| Black | 0.444 | 0.490 | 0.209 |
| | (0.498) | (0.500) | |
| Race Missing | 0.108 | 0.126 | 0.4532 |
| | (0.311) | (0.332) | |
| Any Dependents | 0.378 | 0.356 | 0.5335 |
| | (0.486) | (0.479) | |
| Any Dependents Missing | 0.139 | 0.149 | 0.7140 |
| | (0.347) | (0.356) | |
| HS Dropout/Education Missing | 0.154 | 0.188 | 0.2292 |
| | (0.362) | (0.391) | |
| HS/GED Completer | 0.375 | 0.462 | **0.0174** |
| | (0.485) | (0.499) | |
| Some College/Associate's Degree | 0.363 | 0.252 | **0.0008** |
| | (0.482) | (0.435) | |
| Bachelors Degree | 0.108 | 0.097 | 0.6317 |
| | (0.311) | (0.297) | |
| Filing Status Single | 0.571 | 0.605 | 0.3489 |
| | (0.496) | (0.489) | |
| Filing Status Missing | 0.120 | 0.126 | 0.7898 |
| | (0.325) | (0.332) | |
| Early Filer (2/1 - 2/28) | 0.375 | 0.319 | 0.1150 |
| | (0.485) | (0.467) | |
| Mid-season Filer (3/1 - 3/21) | 0.386 | 0.297 | **0.0010** |
| | (0.488) | (0.457) | |
| Late Filer (3/22 - 4/15) | 0.239 | 0.332 | **0.0063** |
| | (0.428) | (0.471) | |
| Having Trouble with Any Bills? (N=207, 586) | 0.691 | 0.688 | 0.9301 |
| | (0.463) | (0.464) | |
| Plans to Save Some of Refund (N=107, 138) | 0.168 | 0.119 | 0.2710 |
| | (0.376) | (0.324) | |
| Number of Observations | 259 | 675 | |

*Notes:* Filers were eligible for the study if they had positive federal refunds greater than $50 and were using direct deposit to receive their refunds. Filers were not included in the study if they were ineligible (i.e., did not have refunds greater than $50 or were not using direct deposit) or if they were served by a preparer who was not trained to participate.

Table 2: Treatment-Control Balance
(Results from regressions of treatment status on observables; standard errors in parentheses)

| Variable | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Federal Refund (1000s) | 0.0175 | -0.0273 | -0.0301 | -0.0333 | -0.0328 | -0.0300 | -0.0425 | -0.0383 |
| | (0.0157) | (0.0353) | (0.0420) | (0.0432) | (0.0434) | (0.0438) | (0.0434) | (0.0441) |
| Federal Refund (1000s) Squared | | 0.0048 | 0.0049 | 0.0050 | 0.0051 | 0.0046 | 0.0057 | 0.0051 |
| | | (0.0037) | (0.0040) | (0.0040) | (0.0040) | (0.0041) | (0.0040) | (0.0041) |
| AGI (1000s) | -0.0015 | 0.0049 | -0.0003 | -0.0005 | -0.0005 | -0.0004 | 0.0002 | 0.0003 |
| | (0.0022) | (0.0051) | (0.0024) | (0.0025) | (0.0025) | (0.0025) | (0.0025) | (0.0025) |
| AGI (1000s) Squared | | -0.0001 | | | | | | |
| | | (0.0001) | | | | | | |
| Female | | | -0.0001 | -0.0322 | -0.0224 | -0.0404 | -0.0351 | -0.0464 |
| | | | (0.0787) | (0.0850) | (0.1034) | (0.0863) | (0.0847) | (0.1056) |
| Age | | | -0.0001 | -0.0003 | -0.0003 | -0.0001 | -0.0007 | -0.0005 |
| | | | (0.0022) | (0.0023) | (0.0023) | (0.0023) | (0.0023) | (0.0023) |
| Black | | | 0.0415 | 0.0466 | 0.0466 | 0.0419 | 0.0566 | 0.0524 |
| | | | (0.0678) | (0.0714) | (0.0716) | (0.0720) | (0.0714) | (0.0721) |
| Any Dependents | | | 0.1125 | 0.1288 | 0.1513 | 0.1333 | 0.1372 | 0.1444 |
| | | | (0.0922) | (0.0963) | (0.1660) | (0.0970) | (0.0960) | (0.1674) |
| HS/GED Completer | | | -0.1104 | -0.1358 | -0.1368 | -0.1357 | -0.1352 | -0.1335 |
| | | | (0.1230) | (0.1262) | (0.1266) | (0.1271) | (0.1257) | (0.1270) |
| Some College/Associates Degree | | | -0.1068 | -0.1435 | -0.1433 | -0.1471 | -0.1545 | -0.1608 |
| | | | (0.1266) | (0.1314) | (0.1317) | (0.1324) | (0.1311) | (0.1322) |
| Bachelors Degree | | | -0.1052 | -0.1127 | -0.1153 | -0.1189 | -0.1573 | -0.1666 |
| | | | (0.1496) | (0.1532) | (0.1543) | (0.1541) | (0.1548) | (0.1568) |
| Filing Status Single | | | 0.0775 | 0.0817 | 0.0821 | 0.0904 | 0.0607 | 0.0699 |
| | | | (0.0850) | (0.0870) | (0.0872) | (0.0882) | (0.0875) | (0.0887) |
| Female with Dependents | | | | | -0.0301 | | | -0.0006 |
| | | | | | (0.1803) | | | (0.1830) |
| High Fidelity Tax Preparer | | | | | | 0.0873 | | 0.1118 |
| | | | | | | (0.1061) | | (0.1067) |
| Preparer: Bonds a Good Idea | | | | | | -0.0087 | | -0.0004 |
| | | | | | | (0.0824) | | (0.0824) |
| Mid-season Filer (3/1 - 3/21) | | | | | | | 0.1540* | 0.1637** |
| | | | | | | | (0.0796) | (0.0815) |
| Late Filer (3/22 - 4/15) | | | | | | | 0.0691 | 0.0750 |
| | | | | | | | (0.0909) | (0.0929) |
| Site Dummies? | No | No | No | Yes | Yes | Yes | Yes | Yes |
| Observations | 259 | 259 | 259 | 259 | 259 | 259 | 259 | 259 |
| R-squared | 0.0057 | 0.0174 | 0.0368 | 0.0458 | 0.0459 | 0.0489 | 0.0609 | 0.0656 |
| F-test | 0.7332 | 1.1260 | 0.5779 | 0.4903 | 0.4691 | 0.4589 | 0.6047 | 0.5542 |
| Prob > F | 0.4814 | 0.3447 | 0.8990 | 0.9775 | 0.9849 | 0.9898 | 0.9325 | 0.9703 |

*Notes:* Results from LPM/OLS regressions; standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). All regressions include controls for missing age, gender, race, filing status, dependent information and missing preparer data, where applicable.

Table 3: Effect of Default on Savings Bond Participation Among Low-Income Tax Filers

| Variable | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| Treatment | -0.0054 | 0.0005 | -0.0019 | -0.0051 | -0.0043 | -0.0076 | -0.0066 | -0.0093 |
| | (0.0362) | (0.0362) | (0.0365) | (0.0366) | (0.0364) | (0.0362) | (0.0368) | (0.0361) |
| Federal Refund (1000s) | 0.0208** | 0.0582*** | 0.0460* | 0.0457* | 0.0421* | 0.0429* | 0.0471* | 0.0411* |
| | (0.0091) | (0.0204) | (0.0239) | (0.0243) | (0.0242) | (0.0242) | (0.0244) | (0.0241) |
| Federal Refund (1000s) Squared | | -0.0044** | -0.0039* | -0.0040* | -0.0041* | -0.0037 | -0.0039* | -0.0037 |
| | | (0.0021) | (0.0022) | (0.0023) | (0.0023) | (0.0023) | (0.0023) | (0.0023) |
| AGI (1000s) | -0.0014 | -0.0022 | -0.0013 | -0.0011 | -0.0009 | -0.0011 | -0.0013 | -0.0010 |
| | (0.0013) | (0.0030) | (0.0014) | (0.0014) | (0.0014) | (0.0014) | (0.0014) | (0.0014) |
| AGI (1000s) Squared | | 5.320e-06 | | | | | | |
| | | (4.550e-05) | | | | | | |
| Female | | | 0.0134 | 0.0113 | -0.0551 | 0.0159 | 0.0085 | -0.0599 |
| | | | (0.0446) | (0.0478) | (0.0576) | (0.0476) | (0.0476) | (0.0577) |
| Age | | | -0.0006 | -0.0008 | -0.0007 | -0.0007 | -0.0011 | -0.0009 |
| | | | (0.0012) | (0.0013) | (0.0013) | (0.0013) | (0.0013) | (0.0013) |
| Black | | | 0.1020*** | 0.1170*** | 0.1169*** | 0.1137*** | 0.1221*** | 0.1202*** |
| | | | (0.0385) | (0.0402) | (0.0399) | (0.0397) | (0.0402) | (0.0395) |
| Any Dependents | | | 0.0126 | 0.0209 | -0.1320 | 0.0264 | 0.0199 | -0.1361 |
| | | | (0.0525) | (0.0543) | (0.0926) | (0.0537) | (0.0542) | (0.0916) |
| HS/GED Completer | | | 0.0133 | -0.0058 | 0.0012 | -0.0170 | -0.0047 | -0.0070 |
| | | | (0.0699) | (0.0711) | (0.0707) | (0.0703) | (0.0708) | (0.0696) |
| Some College/Assoc. Degree | | | 0.0058 | -0.0178 | -0.0191 | -0.0283 | -0.0173 | -0.0310 |
| | | | (0.0719) | (0.0740) | (0.0735) | (0.0732) | (0.0739) | (0.0725) |
| Bachelors Degree | | | -0.0175 | -0.0296 | -0.0115 | -0.0408 | -0.0215 | -0.0145 |
| | | | (0.0850) | (0.0861) | (0.0860) | (0.0852) | (0.0872) | (0.0859) |
| Filing Status Single | | | -0.0004 | 0.0002 | -0.0020 | -0.0022 | 0.0028 | -0.0020 |
| | | | (0.0483) | (0.0490) | (0.0487) | (0.0488) | (0.0492) | (0.0485) |
| Female with Dependents | | | | | 0.2040** | | | 0.2172** |
| | | | | | (0.1004) | | | (0.1000) |
| High Fidelity Tax Preparer | | | | | | 0.0283 | | 0.0392 |
| | | | | | | (0.0586) | | (0.0584) |
| Preparer: Bonds a Good Idea | | | | | | 0.0970** | | 0.1024** |
| | | | | | | (0.0455) | | (0.0450) |
| Mid-season Filer (3/1 - 3/21) | | | | | | | 0.0192 | 0.0304 |
| | | | | | | | (0.0451) | (0.0449) |
| Late Filer (3/22 - 4/15) | | | | | | | 0.0939* | 0.1102** |
| | | | | | | | (0.0511) | (0.0508) |
| Site Dummies? | No | No | No | Yes | Yes | Yes | Yes | Yes |
| Observations | 259 | 259 | 259 | 259 | 259 | 259 | 259 | 259 |
| R-squared | 0.0216 | 0.0379 | 0.0895 | 0.1155 | 0.1309 | 0.1497 | 0.1289 | 0.1804 |

*Notes:* Results from LPM/OLS regressions; standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). All regressions include controls for missing age, gender, race, filing status, dependent information, and preparer survey data, where applicable.

Table 4: Effect of Default on Savings Bond Participation Among Low-Income Tax Filers, by Filer and Preparer Characteristics

| | | Estimated Treatment Effect | Standard Error | Number of Observations |
|---|---|---|---|---|
| *By Filer Traits:* | | | | |
| (1) | Full Sample | -0.0051 | (0.0366) | 259 |
| (2) | Black | 0.0131 | (0.0748) | 115 |
| (3) | Not Black | -0.0270 | (0.0381) | 144 |
| (4) | Female | 0.0016 | (0.0486) | 177 |
| (5) | Male | 0.0455 | (0.0600) | 82 |
| (6) | Dependents | -0.0285 | (0.0754) | 98 |
| (7) | No Dependents | 0.0217 | (0.0408) | 161 |
| (8) | High School or Less | 0.0198 | (0.0630) | 114 |
| (9) | Some College or More | 0.0227 | (0.0732) | 94 |
| (10) | Single Filer | 0.0067 | (0.0493) | 148 |
| (11) | Not a Single Filer | 0.0123 | (0.0638) | 111 |
| (12) | AGI < Median | -0.0484 | (0.0531) | 129 |
| (13) | AGI > Median | 0.0118 | (0.0580) | 129 |
| (14) | Refund < Median | 0.0265 | (0.0514) | 129 |
| (15) | Refund > Median | 0.0304 | (0.0604) | 129 |
| (16) | Bill Trouble | 0.0527 | (0.0506) | 143 |
| (17) | No Bill Trouble | -0.0117 | (0.0722) | 64 |
| (18) | Taxes Prepared Here Last Year | 0.0188 | (0.0602) | 93 |
| (19) | Taxes Prepared Somewhere Else Last Year | -0.0015 | (0.0489) | 166 |
| *By Time of Filing:* | | | | |
| (20) | Filed Early in the Season (February 1-28) | 0.0809 | (0.0550) | 97 |
| (21) | Filed Midway through the Season (March 1-21) | -0.0191 | (0.0653) | 100 |
| (22) | Filed Late in the Season (March 22-April 15) | -0.1092 | (0.1116) | 62 |
| *By Preparer Traits:* | | | | |
| (23) | Preparer Responded to Survey | -0.0245 | (0.0458) | 197 |
| (24) | High Fidelity Tax Preparer | -0.0096 | (0.0541) | 154 |
| (25) | Thought Bonds Were a Good Idea | -0.0633 | (0.0674) | 124 |
| (26) | Preparer Was a Student Volunteer | -0.0390 | (0.0703) | 109 |
| (27) | Preparer Was a Non-student Volunteer | 0.0069 | (0.0693) | 88 |

*Notes:* Results from LPM/OLS regressions; standard errors in parentheses (*** p<0.01, ** p<0.05, * p<0.1). All regressions take the form of model (4) in Table 3. That is, they include controls for federal refund amount and its square, AGI, gender, age, race, education, filing status, dependents, and site dummies. Each regression also includes controls for missing age, gender, race, filing status, or dependent information.

Table 5: Tax Preparers' Impressions of Why Filers Were Sometimes Reluctant to Buy Bonds

| Reason | Percentage who thought reason was "important" or "very important." |
|---|---|
| Filers did not trust the government. | 7.1% |
| Filers did not know enough about bonds, or understand bonds well enough, to feel comfortable buying them. | 49.7% |
| **Filers had specific plans for how to spend their refund, and thus did not save.** | **79.2%** |
| Filers were not opposed to saving, but did not like bonds because in the short run (the first year) they would not be able to access the funds if they wanted to. | 31.0% |
| Filers did not have enough experience with saving to evaluate the attractiveness of the interest rate offered by bonds. | 47.7% |
| Observations | 197 |

*Notes:* 58 preparer responses are weighted by the number of filers for whom they prepared taxes. Preparers were asked, "Please indicate how important each of the following factors was, in your opinion, in explaining why filers were sometimes reluctant to buy bonds." Possible responses were "Not at all important," "A little important," "Somewhat important," "Important," or "Very Important."

# Appendix A (Control Worksheet)

**YOUR REFUND/SAVINGS-BOND WORKSHEET**

*(Note: Only use this form if receiving a refund by direct deposit)*

**Your Adjusted Gross Income (AGI)** $ __ __ __ , __ __ __ . __ __

**Your tax refund** $ __ __ , __ __ __ . __ __

**U.S. Savings Bonds**

U.S. Savings Bonds are a safe and easy way to build savings for the future. You can choose to receive $0 to $5,000 of your refund (in multiples of $50) as U.S. Savings Bonds.

**Indicate the amount of U.S. Savings Bonds you want here**

*(Enter 0 if no bonds purchased; your amount must be a multiple of $50)*

$ __ , __ __ __ . 0 0

Please return this form to your tax preparer

---

FOR YOUR TAX PREPARER

**Complete if filer wants to purchase U.S. Savings Bonds**

On Form 8888, one of the "account spots" will be for the bonds:

1. Line a – Enter **amount of bonds** purchased (must be a multiple of $50)
2. Line b – Enter account number **043736881** in the boxes
3. Line c – Mark **"Savings"** in the box
4. Line d – Enter **"B O N D S"** in the first 5 boxes

******* Staple this worksheet to the filer's site survey when you are finished! *******

Site: _____ Preparer Name: _____ Date: _____

# Appendix A (Treatment Worksheet)

**YOUR REFUND/SAVINGS-BOND WORKSHEET**

*(Note: Only use this form if receiving a refund by direct deposit)*

**Your Adjusted Gross Income (AGI)** $ __ __ __ , __ __ __ . __ __

**Your tax refund** $ __ __ , __ __ __ . __ __

**U.S. Savings Bonds**

U.S. Savings Bonds are a safe and easy way to build savings for the future. You can choose to receive $0 to $5,000 of your refund (in multiples of $50) as U.S. Savings Bonds.

The **circled** amount below - approximately 10% of your refund - will be automatically directed to U.S. Series I Savings Bonds in your name unless you decide to change that amount.

| Tax Refund: | U.S. Savings Bonds: | Tax Refund: | U.S. Savings Bonds: |
|---|---|---|---|
| $0 – $500 | $0 | $2500 – $2999 | $250 |
| $500 – $999 | $50 | $3000 – $3499 | $300 |
| $1000 – $1499 | $100 | $3500 – $3999 | $350 |
| $1500 –$1999 | $150 | $4000 – $4499 | $400 |
| $2000 – $2499 | $200 | $4500 – $4999 | $450 |
| $2500 – $2999 | $250 | $5000 or greater | $500 |

*(Optional)*

**If you would want a different amount of U.S. Savings Bonds, indicate the amount here**

*(Enter $0 if no bonds purchased; your amount must be a multiple of $50)*

$ __ , __ __ __ . 0 0

Please return this form to your tax preparer

FOR YOUR TAX PREPARER

**Complete if filer wants to purchase U.S. Savings Bonds**

On Form 8888, one of the "account spots" will be for the bonds:

5. Line a – Enter **amount of bonds** purchased (must be a multiple of $50)
6. Line b – Enter account number **043736881** in the boxes
7. Line c – Mark **"Savings"** in the box
8. Line d – Enter **"B O N D S"** in the first 5 boxes

***** **Staple this worksheet to the filer's site survey when you are finished!** *****

Site: _____ Preparer Name: _____ Date: _____

# Appendix B



## Buying a U.S. Savings Bond with some of your refund

**Easy:**
Buy today using part of your tax refund, for as little as $50.
Saves you a trip to the bank and adds to your savings today.

**Safe:**
Savings protected by U.S. Government.
If lost or stolen, you can get a replacement bond.

**Fast:**
You receive your paper bond in the mail within a few weeks.

**Smart:**
Pays more interest than many savings accounts and involves no fees.

**Long-Term:**
A $50 bond is worth $50 today and is worth far more later!
Helps achieve long-term goals like education or retirement.
Must be held for a minimum of 1 year before it can be cashed.

**Portable:**
May be cashed at most banks and credit unions, even if you do not have an account there.


## Buying a U.S. Savings Bond with some of your refund

**Easy:**
Buy today using part of your tax refund, for as little as $50.
Saves you a trip to the bank and adds to your savings today.

**Safe:**
Savings protected by U.S. Government.
If lost or stolen, you can get a replacement bond.

**Fast:**
You receive your paper bond in the mail within a few weeks.

**Smart:**
Pays more interest than many savings accounts and involves no fees.

**Long-Term:**
A $50 bond is worth $50 today and is worth far more later!
Helps achieve long-term goals like education or retirement.
Must be held for a minimum of 1 year before it can be cashed.

**Portable:**
May be cashed at most banks and credit unions, even if you do not have an account there.

# Appendix C


## U.S. Series I Savings Bond FAQ Sheet

**Is this new? Could a refund previously be used to buy U.S. Savings Bonds?**
This option is available for the first time starting this tax season.

**What amounts of Savings Bonds can I buy using this method?**
You can buy Savings Bonds in denominations of $50, $100, $200, $500, and $1,000 – at face value.

**Will I get actual paper bond certificates?**
Yes, Savings Bonds will be issued as paper bond certificates (as pictured above) in your name.

**Can I buy Savings Bonds for my children or grandchildren this way?**
Currently, when you file your taxes you can only use your refund to buy Savings Bonds issued in your own name (or in both spouses' names if "married filing jointly.")

**How is the interest rate on Series I Bonds calculated?**
The interest rate for Series I Savings Bonds is currently **3.36%**. This rate combines 2 separate rates:

- A fixed rate of return, which remains the same for the life of the Savings Bond (0.30%).
- A variable rate of return, which is adjusted twice a year to reflect inflation (currently 3.06%).

Thus, if inflation increases, the interest rate on your Savings Bonds will increase to reflect that.

**Where can Savings Bonds be redeemed?**
Savings Bonds can be redeemed at most financial institutions, even if you don't hold an account there.

**Are there redemption restrictions or penalties for early redemption?**
Savings Bonds are designed as longer-term investments and generally cannot be redeemed during the first 12 months after you buy them, unless you live in an area affected by a natural disaster.

**How long will it take to get my Savings Bonds?**
Your Savings Bonds are ordered after the IRS completes processing your tax return. Once they're ordered, it may take **up to three weeks** for your Savings Bonds to arrive in the mail.

**What if a paper Savings Bond is lost, stolen, or destroyed?**
The Bureau of the Public Debt is authorized to replace lost, stolen, or destroyed Savings Bonds. You should keep records of your Savings Bond serial numbers and issue dates in a safe place.

**How are savings bonds taxed?**
Savings Bond interest is exempt from state and local income tax, but is subject to federal income tax upon redemption or maturity. Also, tax benefits may be available when you use the redeemed amount of your Savings Bonds to pay education expenses.

**Whom do I contact if I don't get my Savings Bonds?**
The first step is to check the status of your refund by going to "Where's My Refund" on www.irs.gov or calling 1-800-829-1954. You can get information about your refund 72 hours after the IRS receives your e-filed return. If the IRS has ordered your Savings Bonds, you will need to contact the Treasury at 1-800-245-2804 to inquire about the status of your Savings Bonds.