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# Behavioral Interventions to Increase Tax-Time Saving: Evidence from a National Randomized Trial

We provide new large-scale experimental evidence on policies that aim to boost household saving out of income tax refunds. Households that filed income tax returns with an online tax preparer and chose to receive their refund electronically were randomized into eight treatment groups, which received different combinations of motivational saving prompts and suggested shares of the refund to save—25% and 75%—and a control group, which received neither. In treatment conditions where they were presented, motivational prompts focused on various savings goals: general, retirement, or emergency. Analysis reveals that higher suggested that allocations generated increased allocations of the refund to savings but that prompts for different reasons to save did not. These interventions, which draw on lessons from behavioral economics, represent potentially low-cost, scalable tools for policy makers interested in helping low- and moderate-income households build savings.

Many American households have accumulated very little wealth in general and little contingency savings in particular. Lack of access to assets in a time of emergency can cause material hardship and can turn what might have been a minor economic issue, such as a car repair or a spell of unemployment, into a major economic setback. However, there are opportunities to address the lack of contingency savings in US households,

and the annual windfall that comes from the income tax refund is a particularly compelling one. Concerns about the level of saving and the possibilities raised by large refund checks have drawn the interest of policy makers and researchers, motivating efforts to encourage households to save part or all of the refund. Several previous papers, noted below, have examined ways to encourage households to save portions of refunds or to save at the time of tax filing. That literature has reported mixed success.

This article provides new evidence from a large-scale experiment examining how public policies could affect household saving out of income tax refunds. In the experiment, households that filed income tax returns with a preparer, received refunds, and chose to receive the refunds electronically were randomized into nine different groups: a control group and eight treatment groups. The eight treatment groups were exposed to different combinations of four saving prompts—none, general saving, retirement saving, or emergency saving—and two suggested savings allocations of the refund (25% and 75%). Members of the control group received neither a saving prompt nor a suggested allocation.

The goal of the experiment is to examine ways to help low- and moderate-income households build contingency savings. The interventions are based on strategies, especially priming and anchoring, informed by behavioral economics (Epley and Gilovich 2006; Kahneman 2003; Tversky and Kahneman 1974). Because the experiment is built into a preexisting, widely used platform for tax filing, the interventions are potentially scalable and cost effective. We use contributions directly deposited to a savings account as a proxy in measuring efforts to build contingency savings.

Our central results are that higher suggested savings allocations generate higher allocations to savings accounts, but the effects are small. Moreover, the various prompts reminding people of different reasons to save generally have no effect or have a negative impact.

Three sets of empirical results support these findings. First, treatment raised the probability of contributing to a savings account (i.e., the probability of putting the whole refund in a savings account or of splitting the refund into more than one account) and the amount contributed to a savings account. About 7.2% of control-group members contributed at least part of the refund to a savings account. In comparison, 9.8% of the treatment groups contributed at least part. The average amount contributed to a savings account was $73 among control-group members and $93 among treatment recipients. The average savings account contribution was $393 among refund splitters in the control group and $778 among refund splitters in the treatment groups. The savings-allocation results parallel

the refund-splitting results: higher suggested savings allocations increased saving behavior, but the prompts for reasons to save did not increase saving.

Second, treatment slightly raised the probability of splitting the refund between a checking and a savings account. The probability of splitting rose from an extremely low 0.4% in the control group to an average of 1.3% in the eight treatment groups. The saving prompts had a negative effect on the likelihood of splitting, yet the likelihood of splitting grew with the suggested amount of the savings allocation, and the relationship is statistically significant.

Third, among refund splitters, the allocation suggestions had an even more substantial effect. The share of the refund dedicated to a savings account was 16% among refund splitters in the control group, 34% among refund splitters shown the suggestion to save 25% of the refund, and 64% among refund splitters shown the suggestion to save 75% of the refund.

Our results confirm and extend previous findings, but they also raise new puzzles. The positive impact of the suggested savings allocations extends the scope of the idea embedded in previous work: the power of suggestion strongly influences saving behavior (see, e.g., Madrian and Shea 2001; Karlan et al. 2010). In contrast, the absence of a positive effect from the saving prompts is not intuitive; it is unclear why reminding people of specific reasons to save should have a smaller impact on saving than does not reminding them. We do not resolve that issue here, but we believe that the result, if replicated in future research, may provide information important to understanding better the motivations for saving behavior. Hence, the result may ultimately enable the design of more effective saving policies.

## RELATED RESEARCH

A significant share of the population has low levels of contingency savings. In 2011, the typical household with income below the poverty line had about $700 in liquid assets (Bricker et al. 2011), and nearly half of households indicated that they did not think they could find $2,000 if an unexpected need arose (Lusardi, Schneider, and Tufano 2011). In the wake of the Great Recession, a wide range of households continue to face high levels of economic insecurity (Hacker et al. 2010; Sandoval, Rank, and Hirschl 2009) and are increasingly aware of the associated risks (Hurd and Rohwedder 2010; Taylor et al. 2010). Households that lack contingency savings are exposed to material hardship (Chase, Gjertson, and Collins 2011; Couch, Daly, and Gardiner 2011; Heflin and London 2011; Rawlings and Gentsch 2008). They may also face heightened risk for

future economic problems (Shah, Mullainathan, and Shafir 2012; Weller and Logan 2009).

Many factors are related to the prevalence of low contingency savings among low- and moderate-income households. Most obviously, the marginal utility of current consumption may be quite high for households in or near poverty and borrowing constraints may be tight. These considerations may discourage them from shepherding resources for future consumption. Households may have little slack in their monthly budgets, with most resources going to pay for regular, essential expenditures (Barr 2012; Im and Busette 2010; Neuberger, Greenstein, and Orszag 2006). Among households with discretionary income, saving may be inhibited by behavioral orientations, habits, and potential conflicts among family members over preferences concerning use of the funds.

Other barriers to saving affect individuals across the income spectrum. Many people are heavily biased toward the present over the future (Angeletos et al. 2001; Frederick, Loewenstein, and O'Donoghue 2002; Meier and Sprenger 2010; Stango and Zinman 2009). People may follow the status quo, maintaining established saving and spending habits even when those are suboptimal or harmful (Benartzi and Thaler 2007; Madrian and Shea 2001). Moreover, many flee from the complicated choices associated with establishing accounts and managing investments, and others make poor choices (Ariely and Norton 2008; Ariely et al. 2009; Mullainathan and Shafir 2009).

Policies to boost saving must address these barriers, and several considerations suggest that tax refunds present opportunities for encouraging saving behavior. First, for many households, the refund is the largest check received all year. The average refund was $2,794 in 2012 (Internal Revenue Service 2013). As a result, tax time is often the moment when their budgets have the most slack. Second, people appear to be more willing to save irregular windfalls than regular income from sources like paychecks (Mammen and Lawrence 2006; Romich and Weisner 2000; Shefrin and Thaler 1988). Third, some institutional features may help households save the tax refund. Tax filers may split their refund into multiple accounts (e.g., checking and savings) and use the refund to purchase United States savings bonds directly via the tax form (Tufano 2011).

Despite these considerations, households may have come to expect that they will spend the full refund and make plans to do so. Such intentions may limit the success of tax-time interventions in boosting saving (Mammen and Lawrence 2006; Romich and Weisner 2000; Saez 2009).

Several programs and experiments have focused on helping households save more of their refund. Administered at Volunteer Income Tax

Assistance (VITA) sites offering free tax preparation to low-income households, the Refund to Assets program presented savings opportunities but had no effect on the average amount of the refund remaining in savings several months later (Beverly, Schneider, and Tufano 2006). Another VITA-based program, the Extra Credit Savings Program, offered savers a cash incentive and account access, but findings indicate that this program also had only modest impacts on participants (Beverly, Tescher, and Romich 2004).[1] Even if they are successful, however, VITA-based programs may not be scalable. In total, over three million taxpayers use VITA, and there are more than 12,000 VITA sites nationwide (US Department of Treasury 2011). To roll out a program servicing the full tax-filing population would require a significant scale-up of resources. Such a program would require training thousands of additional preparers and overseeing implementation at numerous sites.

Additional research provides evidence on successful efforts to encourage saving. A field experiment conducted with a large, for-profit tax-preparation firm found that participants receiving saving incentives were more likely to make retirement contributions and made larger average contributions if offered larger matching incentives (Duflo et al. 2006). In contrast, the more widely available and equally lucrative Saver's Credit had low take-up rates. Saez (2009) extends this line of work, showing, among other things, that take-up of the Saver's Credit is higher among participants notified several months before filing season that they would be eligible for a saving subsidy. This provides further evidence that suggests that people plan in advance to use their refunds. This line of research demonstrates the feasibility of implementing saving programs via tax-preparation programs at the time of tax filing.

## BACKGROUND AND EXPERIMENTAL DESIGN

This study reports results from an experiment conducted with TurboTax Freedom Edition, a free online platform offered by Intuit as part of the Internal Revenue Service's Free File Alliance.[2] The product is intended

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1. The New York City Office of Financial Empowerment developed $aveNYC, a VITA-based program that offered new, free savings accounts and matching incentives to keep the tax refund in savings for a year. A quasi-experimental evaluation of the program observed positive statistically significant effects on savings account balances 1 year after the intervention (Key et al. 2015). However, these findings could not be replicated in an evaluation with a second cohort. The program, renamed $aveUSA, has been expanded to other cities and is currently undergoing experimental evaluation.

2. Because the product is offered through the Free File Alliance, Intuit may not market the product or earn a profit from its use.

for use by low- and moderate-income filers. In 2012, use of the Freedom Edition was limited to filers with adjusted gross income below $31,000, those eligible for the Earned Income Tax Credit, and active-duty service members with adjusted gross income below $57,000. In that year, about 70% of filers in the United States were eligible for free tax-filing services and 3.1 million filers submitted returns through the program (Internal Revenue Service 2012).
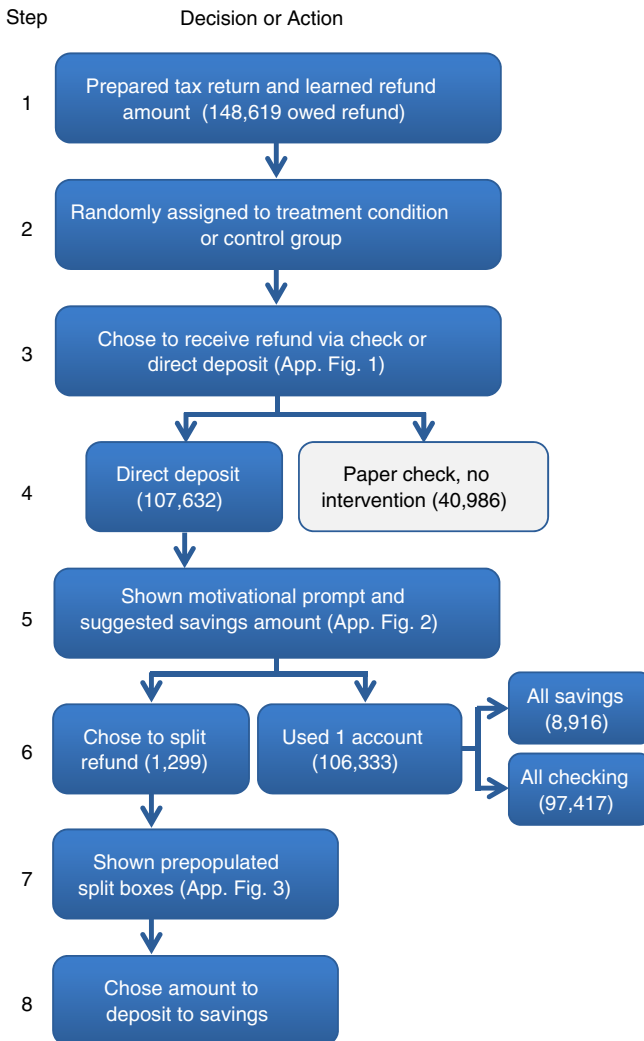
TurboTax products use a guided questionnaire to gather and process tax-filing information. At various points in the questionnaire, the software suggests specific actions that a filer might take to reduce tax liability (e.g., reporting certain types of expenditures). Throughout the process, filers are shown a running estimate of the refund or amount owed. The estimate is based on the information entered upto that point.

The interventions analyzed in this experiment ran from March 15, 2012, through April 17, 2012, the latter date being the end of the tax-filing season for that year. TurboTax Freedom Edition users who filed their taxes during this window and were owed a refund were randomly assigned to one of nine groups: eight treatment groups and one control group. The interventions given to the eight treatment groups varied by the saving prompt and by a suggested savings allocation (which we also call an *anchor*). Specifically, the treatment conditions exposed filers to a unique combination of prompt and anchor, including several conditions in which an anchor was presented without a prompt. The three prompts that were tested encouraged filers to save for general goals, an emergency, and retirement. Treatment participants exposed to the suggested savings anchor were shown one of two such suggestions: one anchor suggested that filers allocate 25% of their refund to a savings account, and another anchor suggested that they allocate 75%.

Figure 1 provides an overview of the interventions and randomization process. The interventions were embedded at the end of the tax-preparation process, when a filer finished entering all information and tax liability or refund amount was determined. In Step 1, filers prepared their taxes through the Freedom Edition's normal guided process. After completing the process, 148,619 filers were owed refunds; filers who owed taxes were excluded because the purpose of the interventions was to encourage filers to save their refund. In Step 2, the Intuit software randomized these filers into one of the nine groups noted above.

In Step 3, prior to being shown any of the intervention screens, the filer chose how he or she would receive the refund: via direct deposit or a paper check (see Figure A1 in the Appendix, for a screenshot of the refund choice). About one third of those eligible for a refund (40,986 filers) chose

FIGURE 1
*Flowchart of Participant Experience*

| Step | Decision or Action |
|------|--------------------|

1 — Prepared tax return and learned refund amount (148,619 owed refund)

2 — Randomly assigned to treatment condition or control group

3 — Chose to receive refund via check or direct deposit (App. Fig. 1)

4 — Direct deposit (107,632) | Paper check, no intervention (40,986)

5 — Shown motivational prompt and suggested savings amount (App. Fig. 2)

6 — Chose to split refund (1,299) | Used 1 account (106,333) → All savings (8,916) / All checking (97,417)

7 — Shown prepopulated split boxes (App. Fig. 3)

8 — Chose amount to deposit to savings

*Note:* App. = appendix; fig. = figure. Sample sizes are shown in parentheses.

to receive the refund by check. Because those who chose a paper check were unable to split the refund deposit into multiple accounts, they are excluded from the analytic sample (Step 4), which includes 107,632 filers.[3]

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3. It would have been more appropriate to randomize filers into treatment and control groups after they chose whether to accept the refund by check or electronic deposit (i.e., after Step 4), since

In Step 5, these filers were shown a screen that differed across treatment assignments in two ways: the saving prompt and the suggested savings amount. Figure A2 in the appendix shows several examples of these screens. The message in the first panel prompts the participant to consider savings goals (the general prompt) and suggests that they save 25% of the refund. A specific dollar amount recommendation is also displayed. That recommendation is simply the product of 25% and the already calculated refund amount for the particular tax filer whose panel is shown. The second panel of Figure A2 also suggests saving 25% of the refund, but the motivational prompt references retirement saving. The final panel prompts the participant to save for a rainy day (the emergency prompt) and suggests saving 75% of the refund. Participants in other treatment groups received different combinations of prompts and savings anchors; two treatment groups were shown a savings anchor of 25% or 75% but no motivational prompt.

The screen represented by the three panels in Figure A2 compels a filer to choose whether to split the refund. In Step 6 of the experiment, 98.8% of filers (106,333) chose to have their refund deposited into only one account. The remaining 1.2% of filers (1,299) chose to split their refund and were shown additional intervention screens, as illustrated in Figure A3 in the appendix, which included two boxes for entering account information. In Step 7, the savings and checking boxes were prepopulated with the suggested savings amount associated with their treatment status. A filer could change the allocation by increasing or reducing the amount in the savings box and clicking the "recalculate" button. In Step 8, the software required the filer to enter routing and account information for each account and to choose the amount for deposit to the savings account.

## DATA AND METHODS

The data were generated through participants' interactions with the TurboTax Freedom Edition software and collected administratively by Intuit. The analytic data set includes information on the 107,632 filers who were randomly assigned to a treatment status (eight treatment conditions or the control group) and chose to receive their refund electronically. Because of the mode of data collection, there are no missing data.

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the remaining sample—those who chose electronic deposit—constitutes this study's analytic sample. Instead, randomization occurred in Step 2, before people chose whether to take the refund electronically. The timing of randomization reflected constraints in the software by which the intervention was delivered. As we show below, however, the various treatment and control groups are virtually identical in observable characteristics. Further tests (not shown) indicate that filers who chose a check were not observationally different from those who chose electronic deposit.

Three outcomes are of interest in this study. We examine whether the filer devoted any part of the refund to a savings account and whether he or she split the refund. We also examine the amount of the refund allocated to savings accounts. That amount may be expressed in dollars or as a share of the refund amount. The key independent variable is the randomly assigned treatment status. To improve the precision of the regression estimates, we control for adjusted gross income, filing status (e.g., married filing jointly, single, head of household), number of dependents, and refund amount.

Table 1 displays descriptive statistics for sample members by treatment status as well as by anchor and saving prompt status. Very few differences are statistically significant at a 5% level. The average of the analytic sample's adjusted gross income is about $13,000, and the average income tax refund was about $1,030. Approximately 10% of sample members chose a filing status that indicated they were married: married filing jointly or married filing separately. The average number of dependents claimed per filer was just less than 0.2.[4] As the final row shows, even the differences in average values across groups for any variable are tiny in economic terms.

Randomized assignment to treatment conditions makes the empirical design straightforward. Our basic specifications are logistic and two-limit Tobit models. The choice between these specifications depends on whether the dependent variable is binary or continuous. Whether the filer contributes any amount to a savings account is a binary outcome, as is whether the filer splits the refund. For these outcomes, we employ standard logistic regressions:

$$\Pr\left(Y_i\right) = \left(\alpha + \beta \mathbf{T}_i + \gamma \mathbf{X}_i + \varepsilon_i\right) / \left[1 + \left(\alpha + \beta \mathbf{T}_i + \gamma \mathbf{X}_i + \varepsilon_i\right)\right], \quad (1)$$

where $Y_i = 1$ if filer $i$ devotes any of the refund to a savings account or splits the refund, and $Y_i = 0$ otherwise; $\Pr(Y_i)$ is the probability that the outcome (saving or splitting) is observed for filer $i$; $\mathbf{T}$ is a vector of treatment status; $\mathbf{X}$ is a vector of control variables (including adjusted gross income, federal refund amount, filing status, and number of dependents); $\varepsilon$ is the standard error term; and $\alpha$ is the intercept. The coefficient vector $\boldsymbol{\beta}$ reflects the impact of treatment assignment on the outcome of interest.[5]

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4. Using adjusted gross income and the number of members in the household, we estimate that roughly half of the sample would be classified as having income below the poverty line.

5. Logistic regression may underestimate the probability of binary events that are rarely observed (King and Zeng 2001). Because refund splitting occurred in only 1.3% of cases, we employed an alternative specification of equation 1. This specification used penalized likelihood estimation to adjust for the rare nature of the outcomes (Firth 1993; Zorn 2005). The results (not reported) are consistent with those from the unadjusted logistic regression and thus address fears of bias. The large sample size

TABLE 1
*Sample Characteristics by Treatment Condition (Means)*

| Treatment Condition | N | Federal Refund ($) | Proportion Married (%)[a] | Number of Dependents | Adjusted Gross Income ($) |
|---|---|---|---|---|---|
| Control | 11,963 | 1,040 | 10.0 | 0.20 | 13,147 |
| All treatment | 95,669 | 1,017 | 9.8 | 0.19 | 13,025 |
| No prompt, 25% split | 11,930 | 1,014 | 10.1 | 0.19 | 13,040 |
| No prompt, 75% split | 11,963 | 1,021 | 9.8 | 0.19 | 12,930 |
| Emergency, 25% split | 12,049 | 1,031 | 10.0 | 0.19 | 13,114 |
| Emergency, 75% split | 12,069 | 1,025 | 9.9 | 0.19 | 13,040 |
| Goals, 25% split | 11,719 | 1,012 | 9.5 | 0.18 | 12,964 |
| Goals, 75% split | 11,983 | 1,031 | 9.5 | 0.19 | 13,025 |
| Retirement, 25% split | 11,925 | 1,016 | 9.9 | 0.19 | 13,039 |
| Retirement, 75% split | 12,032 | 986* | 9.6 | 0.17* | 12,990 |
| Anchor | | | | | |
|   25% split | 47,622 | 1,019 | 9.9 | 0.19 | 13,054 |
|   75% split | 48,047 | 1,016 | 9.7 | 0.19 | 12,996 |
| Prompt | | | | | |
|   No prompt | 35,856 | 1,025 | 10.0 | 0.19 | 13,039 |
|   Emergency | 24,118 | 1,028 | 10.0 | 0.19 | 13,077 |
|   Goals | 23,702 | 1,022 | 9.5 | 0.19 | 12,995 |
|   Retirement | 23,956 | 1,001 | 9.7 | 0.18 | 13,042 |
| Group max. − group min. | … | 54 | 0.6 | 0.03 | 157 |

*Note:* Max. = maximum; min. = minimum.
[a]Filing status is listed as married filing jointly or married filing separately.
*Significant difference from control (*t*-test, 95% confidence level).

Because an extremely high proportion of the sample contributed none or all of their refund to a savings account, we use a two-limit Tobit model to estimate the amount (as a proportion of the total refund) contributed to a savings account. The proportion saved must be no lower than zero and no higher than 1. Thus, the two-limit Tobit specification for proportion saved follows:

$$Y_i^* = \alpha + \beta T_i + \gamma \mathbf{X}_i + \varepsilon_i, \qquad (2a)$$

$$Y_i = 0 \text{ if } Y_i^* \leq 0, \qquad (2b)$$

$$Y_i = Y_i^* \text{ if } 0 < Y_i^* < 1, \qquad (2c)$$

$$Y_i = 1 \text{ if } Y_i^* \geq 1, \qquad (2d)$$

where $Y_i^*$ is the desired proportion of the refund saved and $Y_i$ is the actual proportion saved.

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likely limits concerns about corner solutions that underestimate treatment effects because, although the outcomes are rare, there are still over 1,000 cases from which to draw inferences about parameters.

We complement these basic specifications with several alternatives. We report ordinary least squares (OLS) regressions for both estimates of the amount saved: in dollars and as a proportion of the refund. We also report results for the subsample of filers who split their refund. In addition, equations 1 and 2 combine anchors and prompts into the treatment dummies, we also estimate separate models for the effect of the different anchors and prompts and replace equations 1 and 2a with equations 3 and 4, respectively:

$$\Pr\left(Y_i\right) = \left(\alpha + \beta_1 A_i + \beta_2 M_i + \gamma \mathbf{X}_i + \varepsilon_i\right)$$
$$/\left(1 + \left(\alpha + \beta_1 A_i + \beta_2 M_i + \gamma \mathbf{X}_i + \varepsilon_i\right)\right) \tag{3}$$

$$Y_i^* = \alpha + \beta_1 A_i + \beta_2 M_i + \gamma \mathbf{X}_i + \varepsilon_i, \tag{4}$$

where $A_i$ is the suggested savings anchor (zero, 25%, 75%), $M_i$ is the saving prompt (none, emergency, goals, retirement) assigned to participant $i$, and all other elements are defined as above. The anchor of zero and the prompt to save none of the refund are the omitted values of $A_i$ and $M_i$, respectively. Thus, the control group serves as the reference group.

## RESULTS

### Descriptive Data

The top panel of Table 2 presents estimated means for the measured outcomes, and the bottom two panels (Anchor and Prompt) show the means across groups aggregated by anchor amount and saving prompt. As the table shows, 9.5% of the analytic sample saved at least some portion of the refund, and that is substantially more than the share of sample members who split refunds. Almost 10% of filers in the treatment groups saved part of the refund, as did 7.2% of the control group. This difference between filers in the treatment and control groups is statistically significant ($p < .01$). There were statistically significant differences in saving rates between treatment groups and control, with the no-prompt groups again showing the highest outcomes.

As the table also shows, the estimated rates of splitting are low: about 1.3% of the total sample split the refund, and just 0.4% of the control group did so. Roughly 87% of those who saved any part of the refund saved the entire refund. All eight treatment groups had rates of splitting that were statistically significantly higher than that of the control group (all at $p < .01$). The increase in splitting behavior was small in absolute terms (between .005 and .014) and large in relative terms. Moreover, the proportion that

TABLE 2
*Outcomes by Treatment Status*

| Treatment Condition | Saved Any of Refund (Proportion) | Split Refund (Proportion) | Proportion Saved (Mean) | Amount Saved (Mean, in dollars) |
|---|---|---|---|---|
| Sample average | 0.095 | 0.013 | 0.089 | 91 |
| Control | 0.072 | 0.004 | 0.072 | 73 |
| All treatment groups | 0.098** | 0.013** | 0.091** | 93** |
| No prompt, 25% split | 0.104** | 0.018** | 0.092** | 91** |
| No prompt, 75% split | 0.108** | 0.016** | 0.103** | 107* |
| Emergency, 25% split | 0.096** | 0.012** | 0.087** | 86* |
| Emergency, 75% split | 0.100** | 0.013** | 0.096** | 99** |
| Goals, 25% split | 0.095** | 0.014** | 0.086** | 88* |
| Goals, 75% split | 0.097** | 0.013** | 0.093** | 95** |
| Retirement, 25% split | 0.089** | 0.010** | 0.082** | 87* |
| Retirement, 75% split | 0.092** | 0.009** | 0.088** | 90** |
| Anchor | | | | |
|   25% split | 0.096** | 0.014** | 0.087** | 88** |
|   75% split | 0.099** | 0.013** | 0.095** | 98** |
| Prompt | | | | |
|   No prompt | 0.096 | 0.014 | 0.089 | 90 |
|   Emergency prompt | 0.098 | 0.012 | 0.091 | 93 |
|   Goals prompt | 0.096 | 0.014 | 0.089 | 91 |
|   Retirement prompt | 0.090* | 0.009** | 0.085 | 88 |

*Note:* $N = 107{,}632$. Probability notes identify statistically significant differences from control condition in the upper panel and "Anchor" section or from the no prompt condition in the "Prompt" section (*t*-test).
*$p < .05$; **$p < .01$.

split differed across treatment groups. Although the increase was larger in the treatment groups that were not shown a motivational prompt, there were no statistically significant differences in splitting between the groups that received the same prompt but different suggested savings anchors.

The third column of Table 2 shows the estimated proportions of the refund saved by filers. Those with refunds saved an average of 8.9% of them, control-group members saved 7.2%, and treatment-group members saved 9.1%. The difference between the treatment and control groups is statistically significant ($p < .01$). The highest share—10.3%—was saved by the group that received no prompt and a 75% suggested savings anchor.

The patterns for the allocation of the refund are repeated, not surprisingly, in the estimates of dollar amounts saved (rightmost column of Table 2). Sample members allocated about $91 on average to a savings account. On average, the control group saved $73 and the treatment group saved $93. Average savings by the eight treatment groups ranged from $86 to $107, and all of those groups saved significantly more than the control group did.

The two bottom panels of Table 2 (Anchor and Prompt) report on the same outcomes but present estimates in which treatment conditions are aggregated by anchor amount and motivational prompt. The table shows that participants assigned the 25% and 75% anchor amounts do not differ significantly in the likelihood of splitting the refund or saving any of it. However, the amount and proportion of the refund put into savings were significantly higher among the 75% anchor groups than among the 25% groups.

One possible reason why those who received prompts split their refunds less often may be that they were more likely to save the entire refund. The data do not bear out this possibility, however. The groups shown a retirement-saving prompt, for example, had the lowest share of refund splitters and the lowest likelihood of saving any of the refund. With the exception of the retirement prompt, the content of the prompts does not seem to have a significant effect.

Table 3 presents estimates of the proportions and amounts saved by filers who split their refund. On average, splitters contributed 47% of their refund, or $763, to savings accounts; in contrast, the full sample saved 9%, or $91 (Table 2). One reason for this difference is that the average refund among splitters ($1,623) was larger than that among the full analytic sample ($1,022). The average proportions and amounts saved were significantly higher among refund splitters in all treatment groups than among splitters in the control group, which put an average of 15.5%, or $393, of the refund into savings ($t$-test, $p < .01$).

As shown in the Anchor and Prompt panels of Table 3, 25% and 75% savings anchors had substantial impacts among refund splitters: those exposed to the 75% anchor contributed about 64% of their refund to a savings account, and those shown the 25% anchor contributed about 34% of their refund. The difference between the two anchor groups is statistically significant ($t$-test, $p < .01$). As before, the effects of the saving prompts are weak.

Figure 2 provides more detail on the refund splitters in this sample. The graph shows a clear clustering of participants at the suggested anchoring points, and this signals that refund splitters often accepted the default savings anchors. Over half of splitters in anchored groups saved the exact amount suggested. In the control group, most participants saved either less than 25% or over 75% of the refund.

Figure 2 also shows that refund splitters who were exposed to anchors seldom divided their refund evenly between two accounts. This seems surprising, as an equal split would seem to be a natural default choice for someone who was unsure how to allocate funds across the accounts.

TABLE 3
*Outcomes by Treatment Status (Restricted to Refund Splitters)*

| Treatment Condition | Proportion Saved (Mean) | Amount Saved (Mean, in dollars) |
|---|---|---|
| Sample average | 0.470 | 763 |
| Control | 0.155 | 393 |
| Any treatment | 0.483** | 778** |
| No prompt, 25% split | 0.336** | 530 |
| No prompt, 75% split | 0.668** | 1,061** |
| Emergency, 25% split | 0.346** | 672 |
| Emergency, 75% split | 0.614** | 1,013** |
| Goals, 25% split | 0.351** | 610 |
| Goals, 75% split | 0.632** | 985** |
| Retirement, 25% split | 0.322** | 461 |
| Retirement, 75% split | 0.648** | 890** |
| Anchor | | |
|   25% split | 0.339** | 570 |
|   75% split | 0.636** | 1,000** |
| Prompt | | |
|   No prompt | 0.456 | 739 |
|   Emergency prompt | 0.480 | 843 |
|   Goals prompt | 0.489 | 793 |
|   Retirement prompt | 0.458 | 659 |

*Note:* $N = 1,299$. Probability notes identify statistically significant differences from control condition in the upper panel and "Anchor" section or from the no prompt condition in the "Prompt" section (*t*-test).
*$p < .05$; **$p < .01$.

## Regression Analysis

Given this experiment's large sample and the effectiveness of randomization, it is not surprising that the regression results follow the descriptive findings quite closely. Table 4 reports results of the logistic regression specified in equations 1 and 3. The causal variable of interest is the assigned treatment status. As we note above, the other control variables include adjusted gross income, federal refund amount, filing status, and number of dependents. The table's first two columns show estimates of the impact of these variables on whether any proportion of the refund was saved. The second two columns show estimates of the impact on whether a filer splits the refund. The top panel (Full list of interventions) reports results by treatment status. The bottom panel reports them by anchoring status and saving prompt. All of the treatment conditions are positively associated with the likelihood of saving at least part of the refund and with the likelihood of splitting the refund. The largest coefficients are found for the treatment

FIGURE 2
*Portion of Refund Saved Among Refund Splitters*
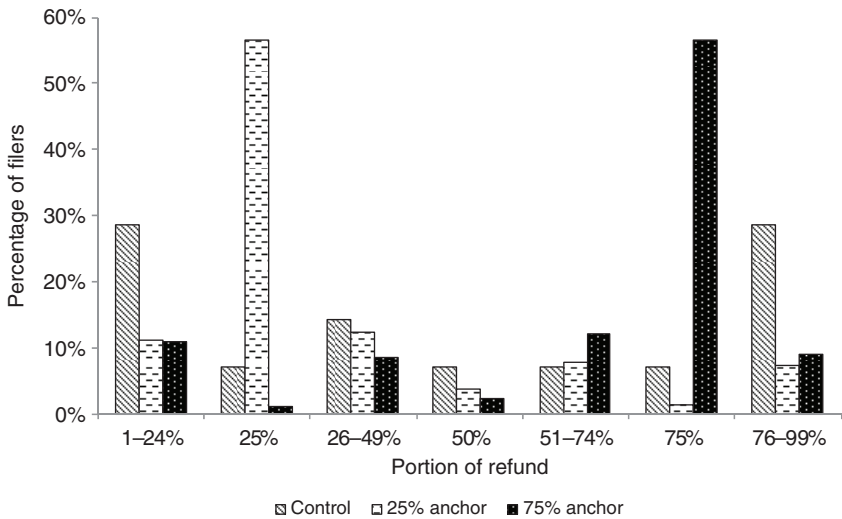


groups with no prompt. Adjusted gross income, the amount of the federal refund, and filing status are also significantly associated with both outcomes (all at $p < .01$). The number of dependents is associated only with the likelihood of saving ($p < .05$). As the bottom panel (Aggregated anchors and prompts) shows, the suggestion of a savings anchor significantly raises the likelihood of both allocating any of the refund to a savings account and of splitting: both outcomes are significantly more likely among filers shown either anchor than among filers in the control group (all at $p < .01$). The specific saving prompts are significantly associated with *decreased* likelihood of allocating funds to a savings account and of splitting (all at $p < .01$).

Table 5 displays results from the Tobit (equations 2a–2d) and OLS models (equation 4) of the interventions' effects on the proportion and amount of the refund deposited into a savings account. Estimates from the OLS and Tobit models should be compared with caution, as the Tobit coefficients correspond with effects of the predictors on an unobserved latent variable. However, the coefficients produced by the two modeling approaches are in the same direction and have similar relative magnitudes.

Results in Table 5 confirm what estimates in Table 2 led us to expect: each of the treatment conditions increases the proportion and amount of the refund saved relative to the proportion and amount saved by the control group. The largest effects are observed among filers shown no prompt

TABLE 4
*Logistic Regressions*

|  | Any Saving | | Refund Splitting | |
| --- | --- | --- | --- | --- |
| Variable | Beta | Odds Ratio | Beta | Odds Ratio |
| A. Full list of interventions |  |  |  |  |
| Control |  |  |  |  |
| No prompt, 25% anchor | 0.403** | 1.496 | 1.461** | 4.312 |
| No prompt, 75% anchor | 0.447** | 1.563 | 1.340** | 3.894 |
| Emergency, 25% anchor | 0.309** | 1.362 | 1.096** | 2.992 |
| Emergency, 75% anchor | 0.362** | 1.436 | 1.099** | 3.002 |
| Goals, 25% anchor | 0.305** | 1.356 | 1.218** | 3.380 |
| Goals, 75% anchor | 0.327** | 1.387 | 1.146** | 3.146 |
| Retirement, 25% anchor | 0.228** | 1.256 | 0.886** | 2.425 |
| Retirement, 75% anchor | 0.262** | 1.300 | 0.731** | 2.077 |
| Adjusted gross income | −0.000** | 1.000 | 0.000** | 1.000 |
| Federal refund | 0.000** | 1.000 | 0.000** | 1.000 |
| Filing status (married) | −0.265** | 0.767 | −0.647** | 0.523 |
| Number of dependents | 0.053* | 1.055 | 0.035 | 1.035 |
| Constant | −2.510** | 0.081 | −5.856** | 0.003 |
| B. Aggregated anchors and prompts |  |  |  |  |
| Control |  |  |  |  |
| 25% split anchor | 0.405** | 1.500 | 1.450** | 4.262 |
| 75% split anchor | 0.444** | 1.559 | 1.372** | 3.942 |
| Emergency prompt | −0.090** | 0.914 | −0.314** | 0.731 |
| Goals prompt | −0.109** | 0.897 | −0.229** | 0.795 |
| Retirement prompt | −0.180** | 0.835 | −0.600** | 0.549 |
| Adjusted gross income | −0.000** | 1.000 | 0.000** | 1.000 |
| Federal refund | 0.000** | 1.000 | 0.000** | 1.000 |
| Filing status (married) | −0.265** | 0.767 | −0.648** | 0.523 |
| Number of dependents | 0.053* | 1.055 | 0.035 | 1.035 |
| Constant | −2.510** | 0.081 | −5.856** | 0.003 |

*Note: N = 107,632.*
*$p < .05$; **$p < .01$.

and the 75% anchor. In the table's lower panel (Aggregated anchors and prompts), a clear pattern is evident in the estimates: exposure to anchors increases the proportion and amount saved, but exposure to prompts decreases the proportion saved.

Table 6 reports OLS estimates for refund splitters. As we expected, the amount and proportion saved are much larger among splitters than among the full sample, and the differential effects of the two (25% and 75%) anchors are clearer: the 75% anchor is positively associated with the level of saving ($p < .01$). The table's lower panel (Aggregated anchors and prompts) shows that assignment to a group with a 25% anchor led to a 19 percentage point increase in the proportion of the refund saved ($p < .01$),

TABLE 5
*Continuous Regressions (Robust Standard Errors)*

| | | OLS | |
|---|---|---|---|
| Variable | Tobit of Percent Saved (Beta) | Amount saved (Beta, in dollars) | Percent saved (Beta) |
| A. Full list of interventions | | | |
| Control | | | |
| No prompt, 25% anchor | 2.464** | 20.558** | 0.021** |
| No prompt, 75% anchor | 2.832** | 35.618** | 0.031** |
| Emergency, 25% anchor | 1.897** | 14.162* | 0.016** |
| Emergency, 75% anchor | 2.281** | 27.474** | 0.024** |
| Goals, 25% anchor | 1.833** | 17.360** | 0.014** |
| Goals, 75% anchor | 2.025** | 22.307** | 0.021** |
| Retirement, 25% anchor | 1.367** | 16.006** | 0.010** |
| Retirement, 75% anchor | 1.653** | 21.748** | 0.018** |
| Adjusted gross income | −0.000** | 0.004 | 0.000** |
| Federal refund | 0.000** | 0.097** | 0.000 |
| Filing status (married) | −1.698** | −18.441* | −0.017** |
| Number of dependents | 0.365* | 9.094 | 0.004 |
| Constant | −19.405** | −32.126** | 0.078** |
| Number of censored observations | 106,373 | – | – |
| B. Aggregated anchors and prompts | | | |
| Control | | | |
| 25% split anchor | 2.494** | 23.202** | 0.022** |
| 75% split anchor | 2.803** | 32.982** | 0.030** |
| Emergency prompt | −0.559** | −7.272 | −0.006* |
| Goals prompt | −0.721** | −8.285 | −0.008** |
| Retirement prompt | −1.139** | −9.224* | −0.012** |
| Adjusted gross income | −0.000** | 0.004 | 0.000** |
| Federal refund | 0.000** | 0.097** | 0.000 |
| Filing status (married) | −1.698** | −18.441* | −0.017** |
| Number of dependents | 0.365* | 9.094 | 0.004 |
| Constant | −19.405** | −32.126** | 0.078** |
| Number of censored observations | 106,373 | – | – |

*Note:* OLS = ordinary least squares regressions. $N = 107,632$.
*$p < .05$; **$p < .01$.

and assignment to a 75% anchor led to a 49 percentage point increase over the control condition. Among refund splitters, only the retirement prompt is significantly associated with the amount contributed to a savings account.

## CONCLUSION

This study adds to the growing body of research on interventions that center on tax filing as a potential way to increase saving among low- and moderate-income households. Such interventions have relatively low costs

TABLE 6

*Ordinary Least Squares Regressions for Refund Splitter Subsample*

| Variable | Amount Saved (Beta, in dollars) | Percent Saved (Beta) |
|---|---|---|
| A. Full list of interventions | | |
| Control | | |
| No prompt, 25% anchor | 795.131** | 0.172** |
| No prompt, 75% anchor | 1,255.242** | 0.504** |
| Emergency, 25% anchor | 843.702** | 0.185** |
| Emergency, 75% anchor | 1,203.514** | 0.453** |
| Goals, 25% anchor | 831.924** | 0.187** |
| Goals, 75% anchor | 1,139.778** | 0.472** |
| Retirement, 25% anchor | 688.683** | 0.158** |
| Retirement, 75% anchor | 1,104.982** | 0.452** |
| Adjusted gross income | 0.006* | 0.000 |
| Federal refund | 0.426** | −0.000 |
| Filing status (married) | 126.578 | 0.013 |
| Number of dependents | −17.392 | −0.004 |
| Constant | −1,002.931** | 0.165** |
| B. Aggregated anchors and prompts | | |
| Control | | |
| 25% split anchor | 829.543** | 0.188** |
| 75% split anchor | 1,218.437** | 0.486** |
| Emergency prompt | 0.303 | −0.018 |
| Goals prompt | −36.623 | −0.007 |
| Retirement prompt | −127.773* | −0.032 |
| Adjusted gross income | 0.006* | 0.000 |
| Federal refund | 0.426** | −0.000 |
| Filing status (married) | 128.891 | 0.014 |
| Number of dependents | −18.370 | −0.005 |
| Constant | −1,005.141** | 0.164** |

*Note:* $N = 107{,}632$.

*$p < .05$; **$p < .01$ (Robust Standard Errors).

and are scalable to a broad population. The administrative data provide reliable information on actual behavior rather than intentions.

Compared with counterparts in the control group, treatment-group members were more likely to contribute at least some of the refund to a savings account and more likely to split their tax refund. Treatment participants also made higher average contributions to savings accounts. The absolute effects are quite small. However, given that that less than 8% of control group members contributed some portion of their refund to a savings account, and less than 1% of control group members split their refund, the relative effects are substantial. A higher suggested savings anchor led to more saving behavior. For example, the 50 percentage point difference in the two suggested savings anchors (25% and 75%) is associated with

a 30 percentage point increase in the actual amount of the refund contributed to a savings account. In contrast, the specific saving prompts (for general, emergency, and retirement saving) did not raise contributions to savings accounts and in some cases reduced contributions. This presents a puzzle: it is hard to understand why mentioning these specific reasons to save has less of an impact on saving than does mentioning no reason to save.

This experiment was implemented within a free version of TurboTax, and the average income of sample members was lower than that of the population of US tax filers. This does not compromise the internal validity of the study or the ability to apply the results to low-income households, but it serves as a caveat against applying the results to the population at large.

Another qualification is that we have used contributions to a savings account as a proxy for saving. Our data only capture where refunds were deposited; they do not show whether the refund remained in savings over time. The use of this proxy is consistent with the idea that the choice between a savings and a checking account influences outcomes: deposits into savings accounts are more likely to remain in the accounts for an extended period. This influence may be exerted through attenuated transaction costs (the cost of leaving money in an account is lower than the cost associated with moving it) or through mental accounting processes. Nevertheless, use of this proxy for saving could lead us to overestimate or underestimate refund saving. It would lead us to underestimate saving if participants deposited the refund into a checking account but later moved some portion of it into a savings account or if they saved the refund outside of a savings account. We would overestimate saving if funds that go into a savings account were quickly dissipated. Future work will need to examine the relationship between initial deposits into a savings account and subsequent outcomes.

Another important direction for future research is uncovering the effects of a wide range of aspects of saving interventions. Some of those aspects are implicit. In this article, for example, we show that the impacts of suggested savings anchors differ from the impacts of prompts to save for specified reasons. Numerous other features of the intervention could prove important. For example, it is not clear how well participants understood the information they were given. The estimated treatment effects are due to some combination of behavioral response and individual understanding: that is, a combination of the behavioral response that would occur if tax filers fully understood the prompts and the actual level of understanding they possessed or attention they provided. Likewise, the positioning of

the intervention at the very end of the tax-filing process may mean that participants were cognitively drained or otherwise eager to finish the process. Thus, positioning the interventions at the end of the process may affect the likelihood that the filers will read and consider the motivational prompts and savings anchors as well as the likelihood that they will spend extra time to enter direct deposit information for a second account. Further, participants may have already made plans for use of the refund (e.g., to spend it or to repay debt), so the interventions may have occurred too late to significantly affect many participants' behavior. It may be possible to increase the efficacy of some interventions by targeting tax filers earlier in the tax-preparation process or before the tax season begins. Providing information about the saving behavior of similar tax filers could be used to encourage greater contributions to savings accounts. All of these possibilities represent interesting directions for future research.

## APPENDIX

FIGURE A1

*Screenshot of Experimental Prompt (Step 3 in Figure 1)*

FIGURE A2
*Motivational Prompts (Step 5 in Figure 1)*



FIGURE A3
*Allocation Screen with Emergency Saving Prompt (Step 7 in Figure 1)*

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