

# RESEARCH DIALOGUE

Issue no. 109

AUGUST 2013

## WHAT WILL MY ACCOUNT REALLY BE WORTH? EXPERIMENTAL EVIDENCE ON HOW RETIREMENT INCOME PROJECTIONS AFFECT SAVING

Gopi Shah Goda  
Stanford University and NBER

Colleen Flaherty Manchester  
University of Minnesota

Aaron Sojourner  
University of Minnesota and IZA

### ABSTRACT

Many investment companies have begun providing their defined-contribution pension participants with individualized, retirement income projections. The U.S. Congress is currently considering whether to require them all to do so. Evidence on the potential impact is scant, though a large body of economic research suggests that individuals are not currently making optimal retirement-saving decisions. Through a field experiment, we measure how provision of retirement income projections along with enrollment information affects individuals' contributions to employer-sponsored retirement accounts. Using administrative data prior to and following the intervention, we measure the effect on participation and the level of contributions. Those sent retirement income projections were more likely to change their contribution level and they increased annual contributions more than did those who received no intervention. Results from a follow-up survey provide corroborative evidence and show heterogeneous effects of the intervention by rational and behavioral factors known to affect saving. We find that a combination of factors contributed to this treatment effect, including anchoring, which suggests that care is warranted in the design and communication of projections.

JEL Codes: H3, J2

Keywords: defined contribution plans, financial literacy, lifetime income disclosures

Acknowledgments: We are grateful to Jackie Singer and Shelly Wymer for their assistance with administering this project. The research reported herein was performed pursuant to a grant from the U.S. Social Security Administration (SSA) funded as part of the Financial Literacy Center. The authors also acknowledge additional support provided by the TIAA-CREF Institute and the University of Minnesota Carlson School of Management. The authors thank John Beshears, Jeff Brown, Wandu Bruine de Bruin, Katherine Carman, James Choi, Courtney Coile, Adeline Delavande, Maria Fitzpatrick, Bill Gale, Damon Jones, Amit Kramer, Ron Laschever, Annamaria Lusardi, Erzo F.P. Luttmer, Dayanand Manoli, Olivia Mitchell, Enrico Moretti, Victor Stango, Robert Willis, Joanne Yoong and seminar participants at the NBER Aging meetings, University of Illinois, University of Chicago, and George Washington University for comments. The opinions and conclusions expressed herein are solely those of the authors and do not represent the opinions or policy of SSA, any agency of the Federal Government, or any other institution with which the authors are affiliated. © 2012 Goda, Manchester and Sojourner. All rights reserved.



Financial Services

Any opinions expressed herein are those of the authors, and do not necessarily represent the views of TIAA-CREF, the TIAA-CREF Institute or any other organization with which the authors are affiliated.

## 1. INTRODUCTION

With the shift toward defined contribution (DC) retirement plans, Americans' retirement security increasingly requires individuals to make responsible, informed wealth accumulation decisions over their working years (Hacker 2006; Even and Macpherson 2007; Skinner 2007). Among Americans with pensions, the share with only a traditional defined benefit pension fell from 60 to 10 percent between 1980 and 2003. Over the same period, the share with only a DC plan rose from 17 to 62 percent (Buessing and Soto 2006). Because individuals only have one shot at saving for retirement, the stakes are high and the consequences of suboptimal choices on economic well-being are potentially large.

Economists debate the extent to which Americans save too little, too much, or just the right amount for retirement (Scholz et al. 2006; Ameriks et al. 2007; De Nardi et al. 2010; Lusardi and Mitchell 2011). In standard models of retirement saving, individuals with low levels of saving are interpreted as responding optimally given a strong taste for present rather than future consumption, anticipation of steep earnings growth, or binding liquidity constraints.

However, a growing body of work raises concerns about how well-equipped individuals are to make optimal saving decisions. They may be cognitively constrained, as evidenced by low rates of financial literacy (Lusardi and Mitchell 2007). Many are affected by behavioral factors outside of standard models, such as procrastination or inertia (e.g., Thaler and Benartzi 2004, Choi, Laibson, Madrian and Metrick 2004), default rules (e.g., Madrian and Shea 2001; Beshears, Choi, Laibson and Madrian 2006; Mitchell, Mottola, Utkus and Yamaguchi 2009; Goda and Manchester 2010), peers (e.g., Duflo and Saez 2002; Duflo and Saez 2003; Beshears, Choi, Laibson, Madrian and Milkman 2011), or how information is conveyed or framed (Bernheim, Fradkin and Popov 2011; Choi, Haisley, Kurkoski and Massey 2012).

A key requirement for optimal saving decisions is an accurate understanding of both the accumulation of retirement saving contributions to assets at retirement, and the decumulation of assets to income in retirement. Many individuals systematically underestimate the effects of exponential growth (Eisenstein and Hoch 2007; Stango and Zinman 2009; McKenzie and Liersch 2011), which distorts one's view of intertemporal budget constraints and could lead to suboptimal saving decisions. How individuals adjust saving in response to reductions in this bias is subject to countervailing income and substitution effects and is determined by elasticity of intertemporal substitution (EIS). The income effect occurs because current assets will be worth more than previously expected, which encourages additional consumption now and reduced saving. The substitution effect occurs because the opportunity cost of current consumption rises, encouraging additional saving.

Providing retirement income projections, or information on how contribution elections made today would map into future account balances and income at retirement, could assist in retirement decision-making. Indeed, the U.S. Congress is considering the Lifetime Income Disclosure Act (S. 267; HR. 1534), which would require DC plan administrators to annually provide retirement income disclosures that would project the value of a lifetime annuity that the plan participant could purchase at retirement given her current retirement savings. This kind of intervention has commonsense, bipartisan policy appeal as it does not entail a saving subsidy or mandate and can be provided at negligible marginal cost. However, the effects of such a policy have never before been tested. Existing evidence on correcting exponential growth bias and saving decisions is limited to lab evidence that shows increased saving motivation after learning about exponential growth (McKenzie and Liersch 2011) and evidence in a developing country context (Song 2012).

Importantly, assumptions are inherent in the policy of offering projections and, if these incidental aspects of the projections affect saving behavior, a policy of providing projections might reduce welfare. First, projection assumptions may inadvertently shift individuals' beliefs about likely retirement ages or rates of investment return. Second, the assumptions may have anchoring effects in that individuals may respond differently to larger-valued projections relative to smaller values, or be influenced by the hypothetical contribution amounts used in the projections.

This study evaluates how providing retirement income projections affects retirement contributions using a large-scale field experiment involving nearly 17,000 employees at the University of Minnesota. We contribute to the literature in several ways. First, using administrative data, we measure the causal effect of our informational intervention on contributions to employer-provided, tax-deferred retirement savings accounts. We find that providing income and balance projections along with general plan information and materials assisting people through the steps of changing contribution rates resulted in a 1.2 percentage point increase in the likelihood one changes their contributions relative to a control group, which received no information, over a six-month period, an increase of 29 percent relative to the control group. In addition, individuals sent this treatment increased their annual contributions by \$85 more than the control group during the study period. Because only a small portion of the sample changed contribution levels, the magnitude of the increase among those who made a change was much larger, approximately \$1,150 dollars per year. Additional features of the experiment yield insights into which components of the treatment generate the observed effects. In particular, our findings suggest that both the provision of retirement planning materials and the projections contribute positively to the average treatment effect, although there is not strong evidence that either the planning materials or the projections alone induced a significant increase in contributions.

Second, a unique feature of our study design is that we randomize the assumptions used to generate the projections across employees. We find that a higher assumed retirement age has a significant positive impact on the propensity to change one's contribution amount. We also find that both a higher assumed retirement age and higher assumed hypothetical contribution amount induce a larger increase in contributions relative to those assigned the lower assumed retirement age and contribution amount, though the assumed rate of investment return does not appear to have an effect.

Third, we supplement administrative records on contributions with a follow-up survey administered after the intervention, providing rich data on individual characteristics and behaviors. We utilize responses from the survey to provide corroborative evidence regarding the effectiveness of the intervention by exploring the impact of our interventions on additional aspects of the saving decision-making process. Among survey respondents, those sent full income disclosures were more likely to report having recently engaged in and being more informed about retirement planning, having higher certainty about the amount of income they expect to have in retirement, and greater satisfaction with their overall financial condition relative to the control group. While the fact that the survey respondents are a non-random subsample of the population warrants caution when interpreting these results, these findings corroborate the sense that the intervention did influence saving decisions and that results are not driven by chance alone.

Using the uniquely rich information from the follow-up survey, we evaluate the interaction of our intervention with standard economic and behavioral economic factors known to affect saving. In terms of factors that influence optimal saving decisions, we find that, among survey respondents, individuals who display higher rates of time discounting, as well as those who report tighter liquidity constraints, are significantly less likely to respond to the intervention. In addition, we find evidence that behavioral economic factors affect individuals' response to the intervention. We analyze the interaction between measures of time-inconsistent preferences and treatment and find that, among survey respondents, individuals who report a tendency to procrastinate are significantly less likely to respond to the intervention. Heterogeneity in preferences underscores a virtue of policies like this one that provide information rather than subsidizing or mandating particular behaviors, and our findings suggest that the effectiveness of informational treatments may depend heavily on behavioral factors.

Fourth, our findings are relevant for public policy, employers, and financial-service firms. Our findings suggest that on average, individuals contribute more when provided with information about how current saving translates into income in retirement along with enrollment information. In addition, our results suggest that care should be taken in the design of such interventions, as the assumptions used may influence the response.

Finally, we explore potential mechanisms that may have generated the observed effects. Our results suggest that the treatment effects we estimate are due to a combination of factors, including improved understanding of the relationship between contributions and retirement income, salience, and psychological cues, but were unlikely to have been driven by a shift in beliefs regarding rates of investment return and expected retirement ages.

The remainder of our paper proceeds as follows. Section 2 describes our experimental design, including details regarding our treatment groups and randomization procedure, and Section 3 explains our analytic approach. Section 4 presents and discusses results on the effect of the intervention on saving behavior, the saving process, and heterogeneous effects of the intervention. Section 5 explores possible mechanisms by which these effects operate. Section 6 concludes the paper.

## **2. EXPERIMENTAL DESIGN**

### **2.1 SETTING AND SAMPLE CHARACTERISTICS**

The setting of our study is the University of Minnesota. Nearly all employees at the University participate in Social Security and a retirement plan that mandates relatively high levels of retirement saving.<sup>1</sup> In addition to these mandatory plans, most employees are also eligible to participate in Voluntary Retirement Plans (VRPs), which allow them to make additional tax-deferred contributions of up to \$33,000 per year if they desire. Participants can choose to make a flat dollar amount election each pay period or contribute a percentage of their salary.<sup>2</sup>

For our experiment, we consider employees eligible to participate in the VRPs who were under age 65 at the time of our intervention. Our sample consists of 16,881 employees dispersed among 1,385 departments across 5 different campuses and extension offices who were employed by the University in both October 2010 (Period 1, prior to intervention) and May 2011 (Period 2, following the intervention). We obtain administrative data from the Office of Human Resources with the assistance of an independent third party in order to protect employee anonymity. We observe each employee's VRP contribution decision in each period.<sup>3</sup>

Table 1 describes the administrative data for our study sample. In Period 1, 24.1 percent participate in a VRP while 24.9 percent participate in Period 2. Including contributions of zero for non-participants, the average contribution rate is 3.19 percent of salary prior to the intervention, equivalent to \$2,324 per year. In Period 2, the average contribution rate is 3.33 percent of salary, equivalent to \$2,450 per year.

Table 1 also includes a summary of the demographic characteristics of our sample. The majority of the sample is female (55.7 percent) and the average age is just under 45 years. Average employment tenure at the University is 12.3 years and average salary is nearly \$60,000. Employees eligible for the faculty retirement plan make up approximately 41 percent of the sample. The majority of the sample works at the Twin Cities campus, followed by the coordinate campuses of Duluth, Morris, Crookston, and Rochester. Approximately 6 percent of the sample works in off-campus locations. The age and gender composition of the sample is similar to a nationally-representative sample employed at firms with pension benefits, though more highly educated and more likely to be married and more likely to be white. We discuss the differences between our sample and a nationally-representative sample in greater detail in Appendix A.

- 1 Civil servants and non-faculty bargaining unit employees participate in the Minnesota State Retirement System (MSRS), while faculty, academic professionals, and administrators participate in the Faculty Retirement Plan (FRP). MSRS participants receive a defined benefit pension equal to 1.7 percent of the average of their five-highest salaries for each year of service starting at age 65 and reduced benefits for early retirement. Employees hired before July 1, 1989 are governed by a slightly different set of rules. The employee and employer each contribute 5 percent of the employee's gross salary to the retirement plan. FRP is a defined contribution plan in which eligible participants make a required tax-deferred contribution of 2.5 percent of their covered salary, matched by a 13 percent contribution by the University.
- 2 There are two choices of VRP, the Optional Retirement Plan (ORP) and the Section 457 Plan. Participants must choose between several different vendors and investment options within each plan. Employees face a maximum annual tax-deferred contribution of \$33,000 (\$16,500 in each plan). Contributions automatically cease once a \$16,500 annual plan limit is reached. Individuals age 50 and above are allowed to make additional catch-up contributions of \$5,500 in the ORP plan annually.
- 3 We never observe VRP account balances or values of mandatory retirement accounts. This prevents us from offering total retirement income projections, as laid forth in the Lifetime Income Disclosure Act. We therefore focus our interventions on providing projections of additional retirement balance and income from hypothetical additional contributions while working, a marginal decision relevant for everyone.

## 2.2 TREATMENT GROUPS AND INTERVENTION

We randomly assign employees to four groups, a control group and three treatment groups, to examine the role of different aspects of the intervention. Table 2 provides a summary of the different informational interventions and treatment group sizes. The control group received no intervention. The most basic treatment, the *planning treatment*, provides general information on saving for retirement, steps to sign up for or change contributions to a VRP, and a chart describing VRP options. This planning treatment reduces transaction and cognitive costs of saving but includes no income projection component.

The other two treatments add components of the retirement income projection. The *balance treatment* adds a customized projection of how hypothetical additional contributions would translate into additional assets at retirement. This is intended to improve individuals' understanding of the accumulation phase. To the balance treatment, the *income treatment* adds a customized projection of the additional annual retirement income that would be generated. By adding information regarding the decumulation phase, the income treatment aims to help people understand the full mapping from current contributions to retirement income. The balance treatment provides only partial information because it only shows the projected relationship between contributions and savings at retirement.

The treatment materials consist of a four-page color brochure sent through internal mail. The first page was designed to prompt individuals to think about their retirement goals (see Figure B-1 in Appendix B). The brochure was designed not to encourage people to save more or to save less, but to encourage them in a neutral manner to reflect on whether they are on target to achieve their retirement income goals. For individuals in the balance and income treatment groups, the second page contains the customized account balance projection (balance group) or both the balance and income projections (income group). The left-hand side of Figure B-2 shows an example second page for an employee in the income group. The top graphic contains the customized conversion of additional contributions to additional account balance at retirement, while the bottom graphic contains the customized conversion of additional contributions to additional annual income in retirement. This page is omitted for the planning treatment group. Enrollment requires choosing a VRP, deciding on a contribution election (i.e. either an amount or rate), selecting an investment company, and finally allocating the contribution to different investment options. This process is described step-by-step in an attempt to reduce the cognitive costs associated with enrollment in the third page of the brochure (Lusardi, Keller and Keller 2009), and is on the right-hand side of Figure B-2. This is page 2 for the planning treatment group. Figure B-3 reproduces the brochure's final page, a side-by-side comparison of the features of the two VRP options.

All groups that received a mailing also received a postcard to request a VRP enrollment kit from the Office of Human Resources. In addition, all individuals who participated in a VRP as of Period 1 were provided with a contribution change form to reduce the transaction costs involved with making a change in their election. Finally, individuals in the balance and income treatment groups were also provided with access to an online customization tool designed to mimic the information provided in the printed materials. Online tools of this type are readily available via investment companies' websites and would serve as complementary tools to any policy initiative surrounding income disclosure by plan sponsors. The online tool had the added ability to adjust assumptions regarding marital status, expected retirement age, and expected investment returns. Those in the income group could also add in other sources of retirement income and expected Social Security benefits to get a more comprehensive picture of their retirement savings portfolio.<sup>4</sup> Figure B-4 provides an example screenshot of the online tool for a member of the income treatment group.

4 Projections on the printed materials were in nominal dollars. Individuals could input expected rate of inflation using the online tool.

## 2.3 RANDOMIZATION

We perform the randomization of our four treatment groups by department in order to mitigate possible contamination across groups, as the main intervention was delivered via department-based mail. We use matched-quad randomization (matched-pair randomization with four treatment groups) for the assignment to ensure that the groups are balanced on observable characteristics that may be related to changes in plan participation. To form the matched quads, we first block departments on quartiles of VRP participation rate, quartiles of average age, and quartiles of average salary. Within block, the largest 4 departments forms one quad, the fifth to eighth largest forms another quad, and so on, and treatment assignment is randomized within quad. This process ensures each treatment group contains a similar number of individuals and that only very small departments were in “quads” of less than 4.

This process resulted in a total of 1,396 departments assigned to treatment groups from 374 quads. Department size ranges from 1 to 225. Because our analytic sample drops individuals no longer employed in Period 2, it includes slightly fewer departments in total. The number of departments and individuals in each treatment group is listed in that last two rows of Table 2. Unsurprisingly, the randomization procedure produced treatment groups balanced on observable characteristics. Further details are available in Appendix C.

## 2.4 PROJECTIONS AND ASSUMPTIONS

We create customized projections of the additional account balance at retirement and the additional annual income in retirement that additional hypothetical contributions  $c$  would generate. We observe each individual’s current age  $a$  and assume a retirement age  $g$ . We also assume a gross annual rate of return during accumulation,  $R$ . The projection of additional per-period contributions  $c$  into additional account balance at retirement  $b$  is performed as follows:

$$b = \frac{cR^{(g-a-1+1/26)}}{R^{1/26-1}} \quad (1)$$

Additional contributions  $c$  are assumed to begin immediately and continue once per pay period, or every two weeks, for a total of 26 times per year until retirement at age  $g$ .

The translation of additional balance at retirement  $b$  into additional income in retirement  $y$  is simply:

$$y = p_g b \quad (2)$$

where  $p_g$  represents the price of a joint annuity that pays \$1 each year from retirement at age  $g$  until death for a married couple. The values  $p_g$  were retrieved from the Income Solutions Annuity Calculator for married males and females of different ages. Married individuals are assumed to be the same age and receive joint life annuities that pay the survivor 100% of the benefit after the first member of the couple dies.<sup>5</sup>

Each individual in the balance treatment receives intervention materials with age-specific balance projections only, not income projections. Those in the income treatment receive both age-specific balance and income projections. In order to avoid creating a false sense of precision, projected balances were rounded to the nearest \$1,000 and annual retirement incomes to the nearest \$100.

The intent of this kind of disclosure intervention is to help people improve their understanding of the relationships in equations (1) and (2), not for the arbitrary values of  $(c, g, R)$  used in the projections to influence behavior. To assess how using these assumptions in projections affects saving behavior and beliefs, we randomly assign alternative values of the 3 parameters among individuals in the balance and income treatment groups. Each person is randomized into one of 12 groups at the individual level, assigning one of three different rates of return, one of two different retirement ages, and

<sup>5</sup> The calculator is available at <https://www.incomesolutions.com/AnnuityCalculator.aspx>. The values used in this study were obtained September 14, 2010.

one of two different sets of hypothetical additional contribution levels. The assumed net investment return is either 3, 5, or 7 percent and retirement age is either 65 or 67. The set of hypothetical additional contribution values displayed on the horizontal axes of the projection graphs is either {\$0, \$50, \$100, \$250} or {\$0, \$100, \$200, \$500}.<sup>6</sup> By holding the relative magnitude of the contribution amounts in each set constant across the two versions (e.g.,  $50/100 = 100/200$ ), the graph itself remains fixed for everyone within treatment. Only the hypothetical contribution amounts printed under the axes, the projected balance or income amounts printed on top of the bars, and the text of the assumptions printed on the brochure vary between parameter treatments.

As a supplemental way to measure the influence of projection assumptions, we construct a single, comprehensive measure that accounts for the fact that how the combination of assumptions map into projections depends on an employee's age. To do so, we construct a ratio for each individual in the balance and income treatment groups of the realized projection printed on that individual's brochure and the corresponding value that would be shown if the assumptions that generate the lowest projection (3 percent return, retirement age of 65, and the lower-valued contribution axes) had been used. We label this ratio the "Relative projection magnitude" (RPM). This measure normalizes the different effects of the assumptions by age. For instance, for older employees, increasing the retirement age has a larger effect on projections than does increasing the investment return. For younger employees, investment return affects the projected values more. The range of values for RPM is between 1 and 9. We analyze its log so that a one-unit change more closely approximates a doubling of the RPM.

## 2.5 SUPPLEMENTAL FOLLOW-UP SURVEY

We supplement administrative data with data collected from a web-based, follow-up survey. This data allows us to analyze heterogeneity in the effects of the treatment with respect to characteristics not available in the administrative records, such as time preferences, barriers to saving, and financial literacy. In addition, we investigate the effect of the interventions on the saving process to provide corroborative support for the treatment effect. Finally, the survey asks about beliefs regarding expected retirement income, expected rates of return, and expected retirement ages in order to assess the effects of the interventions on these beliefs.<sup>7</sup>

In order to eliminate the possibility that the survey itself affected saving behavior, the survey was administered after the second administrative data pull. An independent, third party matched survey responses to administrative data and provided de-identified data for analysis. The overall response rate of the follow-up survey was approximately 22 percent.<sup>8</sup>

While this response rate is similar to response rates found in many research studies, there is concern that survey responders may differ somewhat from the overall population of employees at the University of Minnesota. We find that while survey responders are more likely to be female, comprise a greater proportion of faculty, and are more likely to be VRP participants, the survey sample appears balanced on observable demographics across treatment groups. In addition, the observed treatment effects are substantially higher among the sample of follow-up survey responders. Additional details regarding the representativeness of the survey population are provided in Appendix C.

6 For instance, the example in Figure B-2 uses {\$0, \$100, \$200, \$500}.

7 To the extent possible, we use validated survey questions from tested sources, such as Lusardi and Mitchell (2007); the National Financial Capability Study led by FINRA and designed by a multi-disciplinary team, including Annamaria Lusardi and Robert Willis; the Health and Retirement Study; and the General Social Survey as described in Oreopoulos and Salvanes (2011).

8 To encourage response, all individuals were sent a letter explaining the survey's purpose prior to being sent the email invitation with link. A \$2 monetary non-conditional incentive was provided to a random subsample at the outset of the experiment; however, no additional monetary incentive was provided for completing the survey. The incentive subsample's letter describing the survey also included a hand-written, "Thank you, [name]!" printed on their letter. This subsample had 9 percentage points greater response rate. All individuals who had not answered the survey after approximately two weeks were sent an email reminder.

### 3. EMPIRICAL METHODS

We examine both the propensity to make any change in one's saving behavior as well as the magnitude and direction of the change using two primary outcomes. Our first outcome is an indicator that the employee made any active change in his or her contribution between periods: 1( $\Delta$  Contribution). This includes changes in participation status as well as changes in contribution election among participants. Second, we measure the change between periods in the level of elected annual contributions,  $\Delta$  Contribution Amount.<sup>9</sup>

We generate formal estimates of treatment effects on saving behavior in a regression framework. Given the experimental methodology, analysis is straightforward. We estimate the following equation:

$$S_i = \alpha + T_i\delta + X_i\beta + \eta_q + \epsilon_{i,d} \quad (3)$$

where  $S_i$  is one of our two outcome measures,  $T_i$  is a vector of treatment group dummy variables with control omitted,  $X_i$  is a vector of demographic controls, and  $\eta_q$  are randomization-quad fixed effects. The error term ( $\epsilon_{i,d}$ ) is clustered at the department-level ( $d$ ), the unit of primary randomization. The individual covariate vector  $X_i$  contains quadratics in baseline individual age and tenure, log salary, percent change in salary between periods, and indicators for gender, faculty, and campus location. We also estimate the effect of the assumptions used in the projections by restricting the sample to individuals in the balance and income groups and estimating effects of the randomly-assigned projection assumptions on the same saving outcomes.

## 4. RESULTS

### 4.1 EFFECTS ON SAVING BEHAVIORS

Figure 1 depicts the unconditional means of the outcomes by group along with 95 percent confidence intervals. The percentage of the sample that changes their contribution is 4.88 percent overall, but ranges from 4.09 in the control group to 5.30 in the income group (panel a). The average change in the contribution amounts is +\$122.08 per year and is highest among those sent the full income projections: +\$157.29 (panel b). The change in amount increases with each additional layer of treatment. These descriptive measures provide suggestive evidence that each component of the income disclosure influences saving behavior.

The following results estimate the treatment effects using the multivariate regression model (i.e. equation 3) and discuss the statistical significance of the estimates.

We first evaluate the effect of the interventions on the propensity to change one's contribution election. Results are reported in the first three columns of Table 3. Column (1) reports treatment effects relative to the control group when using only the three treatment indicators. This simply tests for mean differences across groups. Column (2) adds randomization-quad fixed effects to control for observed and unobserved differences within quad. Column (3) adds individual covariates.

In column (1) the percentage of the planning group that changes their contribution election is not significantly different from that in the control group, though the balance and income group propensities are significantly higher. The estimated effects with additional covariates (columns (2) and (3)) are very similar in magnitude to column (1), though more precise and statistically significant. The results in column (3) indicate that the planning group is 0.8 percentage points more likely than the control group to change contributions, a marginally significant effect. The balance and income groups are 1.4 and

<sup>9</sup> The majority of participants elect a dollar amount per pay period. A minority elect contributions as a share of pay, which induces a mechanical increase in contribution amount following a salary increase absent an active change. These two kinds of participants are randomly assigned across treatment groups. We exclude mechanical changes in 1( $\Delta$  Contribution) and  $\Delta$  Contribution Amount. Including both active and mechanical changes or using saving rates relative to salary rather than amounts produce very similar results.

1.2 percentage points more likely to change respectively, with both effects statistically significant at the one percent level. Given the control group's propensity to change of 4.77 percent, these effects are economically meaningful. For instance, the income treatment raises the propensity to change one's election by 29 percent ( $\frac{1.2}{4.09} = 0.29$ ) and the balance treatment by 34 percent.<sup>10</sup>

In order to understand the magnitude and direction of the changes, we repeat the analysis using  $\Delta$  Contribution Amount as the outcome. Results analogous to columns (1)–(3) are reported in columns (4)–(6) of Table 3. Only the income treatment produces a significantly different mean change in contributions, as reported in column (4). The results are robust to adding quad fixed effects and individual covariates in columns (5) and (6). Compared to the control group, individuals in the income treatment increased their saving by an additional \$85.42 annually. The standard deviation of  $\Delta$  Contribution Amount in the control group is \$1,849; therefore, our intervention produced a 0.046 effect size.

Compared to the control group mean change of +\$83.40, the income treatment more than doubled the average change. Further, the average changes contain zeros for the vast majority of employees who made no changes. Among employees who made an active change, those in the income treatment group increased saving by \$1,152 more per year than did control-group changers. Figure 2 shows the distribution of changes among changers for each treatment group, as well as the difference in densities between the planning, balance, and income groups' densities and the control group. People made a wide range of positive and negative changes. The positive effects of the Income treatment appear to be driven by the absence of large negative changes, many more small positive changes, and a few more large positive changes. While the control group's density appears approximately normal and is smooth across 0, the Income-treatment group's density appears to have a discontinuity at 0 with mass shifted from small negative to small positive values.<sup>11</sup>

To better understand what features of the full intervention contribute to this increase in contributions, we can compare the treatment effects among the income group to those in the planning and balance groups using the estimates from Table 3. Relative to the control group, neither the planning nor balance treatment displayed a statistically significant increase in contributions. Only the income group did. However, when using the planning group as the omitted category, the balance and income effects are positive, but not statistically significant. No one or two layers alone induced a significant increase in contributions.

As the point estimates increase with each additional layer of treatment, each part of the income treatment (i.e. planning materials and balance and income projections) seemed to contribute positively to the positive treatment effect on saving changes. The positive point estimate of the planning treatment suggests that the mailing alone, with projections omitted, may have induced a response by some combination of raising the salience of these accounts and reducing the transaction costs associated with changing participation status and contribution levels. The fact that the point estimates of the balance and income treatments are larger than the planning effect's estimate suggest that the projections per se may have some positive effect on contributions.

#### **4.2 EFFECTS ON THE SAVING PROCESS**

Changes in saving behavior must occur through a multi-step process. We test whether the interventions affected engagement in steps and attitudes that are likely part of this process using data from our follow-up survey described in Section 2.5 on our sub-sample of survey respondents.

<sup>10</sup> We combine initial participants and non-participants when presenting results for ease of exposition. As discussed in Section 2.2, however, the components of the intervention varied somewhat across these two groups. When we estimate the treatment effects separately for these groups, we find similar results in that individuals in the balance and income groups were more likely to make a change in contributions relative to the control group among both initial non-participants (marginally significant) and initial participants; the planning group estimate was marginally significant only among initial participants. These results are available upon request.

<sup>11</sup> When examining initial participants and initial non-participants separately, the point estimates suggest that this effect is driven by changes among initial participants rather than differences in the amount contributed between new participants in each treatment group (results available upon request).

The survey provided respondents with the following statements:

- “It is difficult to find information that will help me decide how much to save for retirement.”
- “I am better informed about retirement planning than I was 6 months ago.”
- “In the last 6 months, have you tried to figure out how much you need to save for retirement?”
- “I understand how savings today could affect my retirement income.”
- “How certain are you about the amount of annual retirement income you expect your household to have?”
- “Overall, thinking of your assets, debts and savings, how satisfied are you with your current personal financial condition?”

Respondents were asked to rate their agreement, level of certainty, or satisfaction level on a 7-point scale with the exception of the third question which required a simple Yes/No response.<sup>12,13</sup> To conduct our analysis, we construct Z-scores of the scaled responses for each item by subtracting the sample mean and dividing by the sample standard deviation.

Table 4 displays the results of estimating Equation 3 on the outcome measures described above, including quad fixed effects and the same individual covariates. The dependent variables in Columns 1, 2, 4, 5 and 6 are specified as Z-scores; therefore, the interpretation of a coefficient  $\delta$  on a particular treatment group dummy indicates that the treatment increased the outcome measure by  $\delta$  standard deviations relative to the control group. The dependent variable in Column 3 is a simple binary measure with Yes coded as 1.

The income treatment had a statistically significant impact on almost all measured aspects of the retirement saving process. Specifically, the point estimates indicate that, relative to the control group, the income group’s difficulty in finding information to decide how much to save for retirement is 0.12 standard deviations lower; they are 0.20 standard deviations higher in their informedness about retirement planning relative to 6 months prior; they are 5.1 percentage points (or 12 percent) more likely to have figured out how much to save for retirement; they are 0.10 standard deviations more certain about their retirement income; and 0.078 standard deviations higher in their financial satisfaction. None of the treatment groups differed significantly in their reported understanding of how savings today can affect income in retirement. One potential explanation for this is that responses are bunched in the highest two categories (see Figure D-1) and are too noisy to detect a significant effect.

It is important to remember that these results are estimated on the sub-sample of employees who responded to the survey. Nonetheless, these results are interesting for a number of reasons. First, they provide evidence that the income disclosures have important implications for various steps in the retirement planning process. There are significant effects on steps that would conceivably occur prior to making changes in contributions (finding information, being informed about retirement planning, and figuring out how much to save for retirement) as well as outcomes that may be more apparent later in the process (being more certain about their expected retirement income and more satisfied with their financial condition). Second, these results show that individuals in the planning and balance groups, who were sent either no income projections or incomplete income projections, generally do not have statistically different outcomes relative to the control group, suggesting that full income projections drive the observed effects. Finally, the results suggest that the treatment effects on contributions to the retirement account are not spurious or driven by a small group of outliers and are the result of more informed saving decisions.

12 The distributions of responses to all survey items are provided in Figures D-1 to D-4 in Appendix D.

13 All survey questions offered respondents the ability to answer “Don’t know” and “Prefer not to say” in order to maintain comparability with the validated survey questions and improve the quality of the provided responses. These responses were coded as missing in this and subsequent analysis.

### 4.3 HETEROGENEITY OF EFFECTS ON CONTRIBUTIONS

We investigate the presence of heterogeneity in the effect of our interventions by measuring characteristics believed to influence saving decisions. In particular, we investigate the degree to which treatment effects interact with factors that would matter in a standard rational-actor model (time preference and liquidity constraints), measures of time-inconsistent preferences (self-control and procrastination), and measures of limited cognition (comfort using retirement planning aides and financial literacy). For each measure, we convert individuals' survey responses into Z-scores and then investigate the impact of interactions between the Z-score and treatment indicators on  $\Delta$  Contribution Amount.<sup>14</sup> The interpretation of the coefficients of these interactions is the change in the treatment effect for someone with a one standard deviation higher Z-score on the measure.

#### 4.3.1 STANDARD FACTORS: TIME PREFERENCE AND LIQUIDITY

Importantly, it may be optimal for some individuals not to respond to the intervention. In particular, even if the intervention increased understanding of exponential growth, individuals with high time discounting or high liquidity constraints may not find it optimal to save more, especially in our context which is characterized by high rates of mandatory saving. In fact, a potential benefit of a financial literacy intervention as opposed to forced saving is that it allows for heterogeneous response to the information, which we can investigate using the rich data from our survey.

Our measure of time preference comes from respondents rating how much they agree or disagree with the following statement on a 7-point scale: "Nowadays, a person has to live pretty much for today and let tomorrow take care of itself."<sup>15</sup> The average sample respondent agrees with this statement. To measure liquidity constraints the survey asks, "In a typical month, how difficult is it for you to cover your expenses and pay all your bills?" with options, "Not at all," "Somewhat," and "Very." The average respondent is not liquidity constrained.

The first two columns of Table 5 display the results of estimating Equation 3 on the change in contribution amount among our survey subsample, including the Z-score of the response to the statement indicated in the column heading along with the Z-score interacted with our treatment dummies, and our standard set of control variables. Here, we are interested in whether the effects of our interventions vary across different survey responses, conditional on completing the survey.

There is evidence of heterogeneity in the treatment effects with respect to both factors predicted by the standard model. Specifically, a one standard deviation increase in our measure of time discounting is associated with a \$208 reduction in the change in contribution amount for the income group, suggesting that individuals with higher discount rates are less likely to respond to the income treatment. Liquidity constraints also seem to matter as theory would predict. Those who report being more constrained increase their saving less in response to both the balance and income treatments. Specifically, a standard deviation increase in one's response to the difficulty in covering expenses reduces the income treatment effect by \$181 and the balance treatment effect by \$198. Therefore, these findings confirm that this intervention had at least some scope for individuals to react rationally to the information.

#### 4.3.2 TIME-INCONSISTENCY

As opposed to rational economic factors, the role of behavioral influences, such as time-inconsistency and tendencies to procrastinate, on retirement saving is now well-known. To measure these factors, the survey asks how strongly respondents agree or disagree with the following statements:

- "When I make a plan to do something, I am good at following through."
- "I tend to put off thinking about how much money I need to save for retirement."

<sup>14</sup> We present the results for contribution amounts for ease of exposition; results on contribution changes are similar and available upon request.

<sup>15</sup> The General Social Survey has long used this item and Oreopoulos and Salvanes (2011) discuss its value as a measure of time preference.

As reported in column (3), a one standard deviation increase in one's self-reported ability to follow through with plans is associated with a \$320 increase in the income treatment's effect on changes in saving. As reported in column (4), a one standard deviation increase in putting off thinking about saving for retirement leads to a \$233 decrease in the effect of the income treatment.

These results point to an interaction between informational and behavioral factors, which are often treated as separate, alternative explanations for lack of saving. In particular, the findings suggest that policies that seek to inform individuals about the return to saving will be most effective among those without tendencies to procrastinate.<sup>16</sup>

#### 4.3.3 COGNITION BARRIERS

Cognitive barriers are the other set of factors hypothesized to influence retirement saving decisions. We can evaluate how our treatment interacts with other cognitive limitations. To do this, we provide two statements regarding cognitive barriers and ask for respondents' agreement on a 7-point scale:

- "I find most retirement planning information easy to use."
- "I find it overwhelming to think about how much I need to save for retirement."

We also measure financial literacy. We present results using a composite financial literacy measure that combines both self-assessed and objective measures based on responses to a standard set of financial literacy questions.<sup>17</sup>

As the last three columns of Table 5 show, we find no evidence that cognitive barriers to saving mediate the estimated treatment effects. We also examine whether there are non-linear interactions between cognitive limitations and our interventions, as theory may predict that those with low cognitive ability would be overwhelmed by the treatment and those with high cognitive ability would already know the content of the interventions. However, we find no evidence of these non-linear interactions. These results suggest that the intervention was equally effective on individuals who are more and less cognitively capable. One possibility for this finding is that our sample is more educated and more financially savvy (as well as more homogeneous in these two dimensions) as compared to most Americans (see Tables A-1 and A-2).

We also explored interactions with available demographic and administrative variables, such as age, income, and participation in a the faculty retirement plan, but did not find evidence of statistically significant interactions. These findings imply that while these characteristics may influence one's level of saving, they do not affect the response to the treatment in terms of changes in saving amounts.

#### 4.4 EFFECTS OF PROJECTION ASSUMPTIONS ON OUTCOMES

As discussed earlier, an important part of any policy aimed at requiring the disclosure of retirement income projections is the decision about what assumptions to use in the calculation. Assumptions regarding the rate of investment return and retirement age affect the magnitude of the projected values and could affect one's response to the information or beliefs about those future values. In addition, any hypothetical contribution amounts used to illustrate the projections may affect the behavior of individuals due to anchoring effects.

16 It is possible that these items capture something other than procrastination tendencies, such as a general inability to complete a task or other cognitive barriers.

17 The first measure of self-assessed financial literacy comes from the answer to, "On a scale from 1 to 7, where 1 means very low and 7 means very high, how would you assess your overall financial knowledge?" The second measure is a composite of the following statements:

- "I regularly keep up with economic and financial news."
- "I am pretty good at math."
- "I am good at dealing with day-to-day financial matters, such as checking accounts, credit and debit cards, and tracking expenses."

The questions which test actual financial literacy are provided in Appendix E. The distribution of responses is provided in Figure D-3. Survey respondents tend to score themselves highly on self-assessed financial literacy measures and answer, on average, approximately four out of six financial literacy questions correctly. We construct Z-scores for each of the four self-assessed financial literacy questions and for the number of questions correctly answered on the financial literacy quiz. The composite measure is simply the sum of the Z-scores across these components. Analysis based on any of these dimensions of financial literacy alone yields similar results.

Restricting the sample to individuals in either of these two treatment groups, we study the effect of the different projection assumptions — rate of investment return, retirement age, and hypothetical additional contribution amounts — on our two measures of saving behaviors.

The results in columns (1), (2), and (3) in Table 6 show the effect of each assumption separately on the propensity to make a contribution change,  $\Delta$  Contribution). Column (4) includes the natural log of the relative projection magnitude, described earlier. The coefficient on  $\ln(\text{RPM})$  is the estimated effect of a one-unit increase — corresponding to an approximate doubling of the balance and income projections — has on one's propensity to make a change. Column (5) shows the results of including all of the assumptions and the  $\ln(\text{RPM})$  simultaneously.

The results suggest only the retirement age assumption had a significant effect on the propensity to make any change. In column (4), we see that the relative magnitude projection, as captured by the  $\ln(\text{RPM})$ , has no significant direct effect. In column (5), when all factors are included, the effect of the higher retirement age persists, suggesting the retirement age assumption affects the response through a channel other than the size of the projection. Table 7 repeats the specifications described above with the continuous outcome measure,  $\Delta$  Contribution Amount. The results suggest that a higher assumed retirement age and higher-valued axes both lead to larger positive changes in saving, as does the relative projection magnitude. For instance, presenting individuals with the higher-valued axes instead of the lower-valued axes increased annual contributions by \$104 on average, or by an additional \$1,233 per year among changers. However, we find no evidence that the assumed rate of investment return affects contribution behavior. In addition, the effect of the relative projection magnitude disappears after retirement age is controlled for.

## 5. MECHANISMS

The increase in saving we see among treatment group participants could be due to a variety of different factors, and the welfare implications of providing retirement income disclosures depend on why individuals responded in the observed way. For instance, if individuals did not have adequate understanding of how current contributions map into income in retirement, and the intervention improved that understanding, then the resulting changes could be thought to be welfare improving. However, if, for instance, the treatment effect that we observe is due to psychological cues to save more, then the welfare implications are less clear.

In this section, we describe the evidence related to four different categories of explanations to the extent made possible by the design of our field experiment:

1. Improved understanding of how contributions map into retirement income
2. Shifted beliefs about uncertain future events
3. Increased salience
4. Psychological anchors or cues

### 5.1 IMPROVED UNDERSTANDING

The intended effect of the policy that would require plan sponsors to disclose income projections on quarterly statements is to inform plan participants of their expected retirement income. However, this may not occur if individuals do not notice the income disclosures or do not have the cognitive skills to incorporate the disclosures into their retirement planning. In addition, improved understanding may or may not give rise to a treatment effect on contributions either because of factors preventing individuals from taking action in their saving decisions or because responses across individuals may be offsetting.

Several of our findings support the idea that the intervention improved understanding of the mapping between contributions and retirement income. The strongest evidence comes from our follow-up survey which shows statistically significant treatment effects on several proxies of improved understanding (Table 4). Income group participants

responded that they were better informed about retirement planning and were more certain about their retirement income. Furthermore, several of the coefficients on the income group are statistically different from those in the planning group that received the placebo brochure with no income projections. The role of this mechanism is plausible given that prior research has found that exponential growth bias may be a widespread issue (Stango and Zinman 2009), that similar information intervention can increase individuals' reported intentions to save (McKenzie and Liersch 2011), and that a very intensive intervention can raise saving rates in a very different population (Song 2012).

There is some evidence, however, that may weaken this interpretation. In particular, when asked specifically about the link between saving now and income in retirement, individuals in the income group did not appear statistically distinguishable from those in the other treatment groups (Table 4, Column 4). While we attribute this to range restriction, it does give one pause when making strong conclusions regarding this mechanism.

## 5.2 SHIFTED BELIEFS

A possible unintended consequence of an intervention that provides retirement income projections is to shift beliefs about investment returns and retirement ages, as these assumptions are required to generate the projections. If beliefs are shifted as an incidental effect of the treatment, individuals may change contributions away from the optimal level suggested by their priors. Features of our study were designed to mitigate this effect, including our use of phrasing that cautions against reliance on the assumptions used in the projections and providing individuals with access to an online calculator that allows individuals to manipulate the assumptions to their desired values.

In addition, our experiment provides a unique perspective to analyze the effect of the intervention on beliefs, as the assumptions were randomized and our follow-up survey includes questions designed to elicit beliefs about the key assumptions that may have been affected by the treatment. In general, the evidence suggests that the treatment effect we find was not operating through shifted beliefs. First, as shown in Columns 1 and 2 of Table 8, there was no treatment effect on beliefs regarding expected retirement age and expected investment return. Second, Table 8, Columns 3 and 4 show no evidence that the randomized assumptions used for the balance and income groups affected beliefs. Third, the mean expected retirement age and expected investment return in the control group, which was not subjected to income projections, are in line with the assumptions used in the income projections. Finally, Table 7 shows that the effect of a higher retirement age is higher contributions; however, this finding is hard to reconcile with an economic model of shifting beliefs, as a greater accumulation period (i.e. higher retirement age) should reduce optimal retirement contributions, holding all else constant.

However, in results that are available upon request, we find evidence that the income treatment increased certainty regarding expected retirement age and expected investment returns, suggesting that while mean beliefs may have stayed the same, the distributions of beliefs may have collapsed due to the treatment. It is unlikely that this compression of beliefs is responsible for the treatment effects we observe because such compression would likely result in reduced saving for risk-averse individuals. However, it is possible that the compression of beliefs is welfare-reducing.

## 5.3 INCREASED SALIENCE

By including general information about sources of retirement income and planning for retirement, the treatment materials increased the salience of Voluntary Retirement Plans (VRP). Our experiment was designed to separate the effects of the income projections from the effects of this general retirement information by the inclusion of the planning treatment group to partial out the effect of increased saliency.<sup>18</sup>

Our evidence shows that the planning treatment group did have a statistically significant difference in their propensity to make a change relative to the control group who received no intervention. However, the level of contributions among the planning group are not statistically different from that in the control group; therefore, the changes made by the planning

<sup>18</sup> While we focus on saliency as a mechanism in this subsection, we cannot separately identify saliency from transaction costs because the materials also included steps for enrolling or changing VRP contributions.

group were not, on average, higher or lower than the changes made in the control group. When comparing the level of contributions among those in the income group to those in the planning group, while the point estimates of the income group are higher, the two values are not statistically distinguishable. We also investigate whether our treatment effect varies by age or years to retirement, because if the effect were operating through salience, it is plausible that those closer to retirement pay more attention to the materials. We find no evidence that treatment effects vary by age.<sup>19</sup>

These results are suggestive that salience is an important factor in whether an individual takes action to change their contributions. Salience appears to be less important in determining the level of contributions, though it cannot be ruled out that the treatment effect that we estimate for the income group is completely a result of individuals simply receiving mailings about retirement planning.

#### **5.4 PSYCHOLOGICAL ANCHORS OR CUES**

While the intervention contained language that was generally neutral toward retirement saving, encouraging individuals to think about optimal levels of saving rather than cues to save more, certain aspects may have subtly encouraged increased saving. For instance, restrictions on privacy necessitated that we devise the intervention in terms of changes, which gave rise to language such as “additional contributions.” Indeed, companies who have voluntarily started providing projections have also framed their projections in these same terms.

Our findings provide mixed evidence as to the effect of these psychological cues and anchors. On one hand, the findings in Table 7 show that larger projected income values, primarily through a higher retirement age or higher hypothetical contribution amounts, led to increased saving, suggesting that higher numbers caused individuals to save more even though they had no bearing on the income projections per se. This evidence of anchoring is related to concurrent work by Choi, Haisley, Kurkoski and Massey (2012); however, they find evidence that responses are influenced by low anchors, while we find evidence of sensitivity to high anchors.

On the other hand, there was no evidence that the particular contribution amounts illustrated acted as focal amounts. For instance, there was no increased prevalence of elections (or changes in elections) at exactly \$200 or \$500 among those assigned the high-valued axis nor at \$50 or \$250 among those assigned the low-valued axis.

Overall, it is likely that the treatment effects we observe were due to a combination of factors, including improved understanding, salience, and psychological cues, but were unlikely to be primarily driven by a shift in beliefs. While the welfare implications are not definitively clear, the evidence we find can inform economists’ understanding of retirement decisionmaking and policy discussions regarding income disclosures.

## **6. CONCLUSION**

The shift toward DC retirement plans has placed much of the responsibility and risk for retirement security in the hands of individuals rather than institutions. Optimal retirement saving behavior in this current landscape requires an understanding of the relationship between current contributions and income in retirement, which requires a level of financial sophistication that many Americans may lack. We find that sending individuals information about the accumulation and decumulation phases of retirement saving as well as how to enroll leads to greater contributions to a tax-deferred account relative to those who received no information. Because the intervention provided information about the effects of exponential growth, the increased contributions may have operated by de-biasing employees’ understanding of this central feature of retirement savings. However, there is evidence that the treatment also generated its effects by increasing the salience of retirement saving and by psychological cueing effects.

The results of our follow-up survey provide corroborative evidence that the intervention influenced saving decisions. On the one hand, we find that higher discount rates and liquidity constraints mitigate the effects of our interventions, which is consistent with known trade-offs in saving decisions and supports the under-appreciated fact that not responding

<sup>19</sup> These results are not reported but are available upon request.

to an intervention may be optimal. On the other hand, we find that the effect of the intervention was dampened by procrastination tendencies suggesting that policies designed to increase financial literacy will interact with other behavioral considerations. Furthermore, those sent the income projections report less difficulty finding information regarding retirement planning, are better informed about retirement planning, and are more likely to have figured out how much to save. They also rated themselves higher in overall financial satisfaction.

This study provides proof of concept for a policy that requires no additional mandate on individuals or subsidy for saving. Providing retirement income projections – an extremely low-cost intervention – can affect individuals' contributions. The effects manifested were not large on average and were found in only in a small share of the sample; thus, this policy is not likely to lead to a saving revolution. However, among those who made changes, effects were substantial. The findings from the study also pose a policy challenge by demonstrating the sensitivity of saving behavior to projection assumptions. Individuals may be susceptible to overly-optimistic assumptions and induced to oversave, or, analogously, to undersave from overly-pessimistic projections.

The study offers the first direct evidence of lifetime income disclosure's potential impact. Despite the fact that we cannot evaluate the effect on overall saving behavior, finding that the intervention affected contributions to the employer-provided retirement account is important for evaluating the potential impact of the policy because plan sponsors of these accounts are the policy target. The policy intervention is still under debate in Congress and the findings from this study may be informative. However, the intervention tested here differs in some dimensions from the current congressional proposal.

First, while our intervention was a one-time, dedicated mailing, the policy proposed would include income projections in quarterly statements. Therefore, when extrapolating our results to the policy initiative, it is important to net out the treatment effects due to increased salience, as adding income projections to quarterly statements is unlikely to induce changes through this channel. At the same time, however, repeated exposure to the treatment may work to increase response.<sup>20</sup> Second, while the proposed policy would only require projections be sent to those with active DC accounts, this intervention was also sent to individuals not currently contributing. Third, we did not have access to current account balances and therefore could not provide total projected retirement income. Fourth, the sample of employees at the University of Minnesota is more highly educated, more financially literate, and engaged in higher levels of mandatory retirement saving than Americans generally. While there is room for debate, there are reasons to think each of these demographic differences would lead these study results to understate the true effects of the policy in the national population.

20 In addition, our intervention was sent via an employee's work mail, while the proposed initiative would be sent to one's home or email. It is unclear how this difference would translate into a saving response.

**REFERENCES**

- Ameriks, John, Andrew Caplin, John Leahy, and Tom Tyler, “Measuring Self-Control Problems,” *American Economic Review*, 2007, 97 (3), 966–972.
- Bernheim, B. Douglas, Andrey Fradkin, and Igor Popov, “The Welfare Economics of Default Options: A Theoretical and Empirical Analysis of 401(k) Plans,” NBER Working Paper 17587, National Bureau of Economic Research 2011.
- Beshears, John, James J. Choi, David Laibson, and Brigitte C. Madrian, “The Importance of Default Options for Retirement Savings Outcomes: Evidence from the United States,” NBER Working Paper No. 12009, National Bureau of Economic Research 2006.
- Beshears, John, James J. Choi, David Laibson, Brigitte C. Madrian, and Katherine L. Milkman, “The Effect of Providing Peer Information on Retirement Savings Decisions,” NBER Working Paper No. 17345, National Bureau of Economic Research 2011.
- Buessing, Marris and Mauricio Soto, “The State of Private Pensions: Current 5500 Data,” Technical Report, Center for Retirement Research at Boston College February 2006.
- Choi, James J., David Laibson, Brigitte C. Madrian, and Andrew Metrick, “For Better or For Worse: Default Effect and 401(k) Savings Behavior,” in David A. Wise, ed., *Perspectives in the Economics of Aging*, Chicago, IL: University of Chicago Press, 2004.
- Choi, James J., Emily Haisley, Jennifer Kurkoski, and Cade Massey, “Small Cues Change Savings Choices,” NBER Working Paper 17843, National Bureau of Economic Research 2012.
- De Nardi, Mariacristina, Eric French, and John B. Jones, “Why Do the Elderly Save? The Role of Medical Expenses,” *Journal of Political Economy*, 2010, 118 (1), 39–75.
- Duflo, Esther and Emmanuel Saez, “Participation and Investment Decisions in a Retirement Plan: The Influence of Colleagues’ Choices,” *Journal of Public Economics*, 2002, 85 (1), 121–148.
- Duflo, Esther and Emmanuel Saez, “The Role of Information and Social Interactions in Retirement Plan Decisions: Evidence from a Randomized Experiment,” *Quarterly Journal of Economics*, 2003, 118 (3), 815–842.
- Eisenstein, Eric M. and Stephen J. Hoch, “Intuitive Compounding: Framing, Temporal Perspective, and Expertise,” Working Paper, Cornell University 2007.
- Even, William and David Macpherson, “Defined Contribution Plans and the Distribution of Pension Wealth,” *Industrial Relations*, 2007, 46 (3), 551–581.
- Goda, Gopi Shah and Colleen Flaherty Manchester, “Incorporating Employee Heterogeneity into Default Options for Retirement Plan Selection,” NBER Working Paper 16099, National Bureau of Economic Research 2010.
- Hacker, Jacob S., *The Great Risk Shift*, Oxford University Press, 2006.
- Lusardi, Annamaria and Olivia Mitchell, “Financial Literacy and Retirement Preparedness: Evidence and Implications for Financial Education,” *Business Economics*, 2007, 42 (1), 35–44.
- Lusardi, Annamaria and Olivia Mitchell, “Financial Literacy and Planning: Implications for Retirement Wellbeing,” NBER Working Paper 17078, National Bureau of Economic Research 2011.
- Lusardi, Annamaria, Punam Anand Keller, and Adam M. Keller, “New Ways to Make People Save: A Social Marketing Approach,” NBER Working Paper 14715, National Bureau of Economic Research 2009.

Madrian, Brigitte C. and Dennis F. Shea, “The Power of Suggestion: Inertia in 401(k) Participation and Savings Behavior,” *Quarterly Journal of Economics*, 2001, 116 (4), 1149–1525.

McKenzie, Craig R.M. and Michael J. Liersch, “Misunderstanding Savings Growth: Implications for Retirement Savings Behavior,” *Journal of Marketing Research*, 2011, 48.

Mitchell, Olivia S., Gary R. Mottola, Stephen P. Utkus, and Takeshi Yamaguchi, “Default, Framing, and Spillover Effects: The Case of Lifecycle Funds in 401(K) Plans,” NBER Working Paper 15108, National Bureau of Economic Research 2009.

Oreopoulos, Philip and Kjell G. Salvanes, “Priceless: The Nonpecuniary Benefits of Schooling,” *Journal of Economic Perspectives*, 2011, 25 (1), 159–184.

Scholz, John Karl, Ananth Seshadri, and Surachai Khitatrakun, “Are Americans Saving ‘Optimally’ for Retirement?,” *Journal of Political Economy*, 2006, 114 (4), 607–643.

Skinner, Jonathan, “Are You Sure You’re Saving Enough for Retirement?,” *Journal of Economic Perspectives*, 2007, 21 (3), 59–80.

Song, Changcheng, “Financial Illiteracy and Pension Contributions: A Field Experiment on Compound Interest in China,” mimeo., University of California, Berkeley 2012.

Stango, Victor and Jonathan Zinman, “Exponential Growth Bias and Household Finance,” *Journal of Finance*, 2009, 64 (6), 2807–2849.

Thaler, Richard and Shlomo Benartzi, “Save More Tomorrow: Using Behavioral Economics to Increase Employee Saving,” *Journal of Political Economy*, 2004, 112 (1), S164–S187.

**TABLE 1: SUMMARY STATISTICS: ADMINISTRATIVE DATA**

	<b>MEAN</b>	<b>SD</b>	<b>MIN</b>	<b>MAX</b>
1(VRP Participant, pre)	0.241	0.428	0	1
1(VRP Participant, post)	0.249	0.432	0	1
VRP Contr. Rate, pre	3.189	8.933	0	100
VRP Contr. Rate, post	3.334	9.157	0	100
VRP Contr. Amount, pre	2323.9	6407.7	0	38500
VRP Contr. Amount, post	2450.1	6589.2	0	38500
1(Female)	0.557	0.497	0	1
Age	44.89	11.16	19	64
Tenure	12.34	9.387	0.301	46.64
Salary, pre	58386.9	32527.5	480.7	686587.5
Salary, post	59227.1	33348.7	480.7	686587.4
1(Faculty Ret. Plan)	0.412	0.492	0	1
1(Twin Cities campus)	0.810	0.393	0	1
1(Crookston campus)	0.0129	0.113	0	1
1(Duluth campus)	0.0890	0.285	0	1
1(Morris campus)	0.0206	0.142	0	1
1(Rochester campus)	0.00427	0.0652	0	1
1(Off-campus)	0.0636	0.244	0	1
Observations	16881			

**TABLE 2: TREATMENT GROUP SUMMARY**

	<b>CONTROL</b>	<b>PLANNING</b>	<b>BALANCE</b>	<b>INCOME</b>
General information on saving for retirement and signing up for VRP		✓	✓	✓
Customized information regarding conversion of hypothetical additional contributions to <b>additional account balance</b> at retirement			✓	✓
Customized information regarding conversion of hypothetical additional contributions to <b>additional annual income</b> in retirement				✓
Number of departments	344	335	327	327
Number of individuals	4,063	4,334	4,353	4,131

Notes: VRP stands for Voluntary Retirement Plan and is a tax-deferred savings plan to which employees in the sample can contribute.

**TABLE 3: EFFECT OF INTERVENTIONS**

<b>OUTCOME:</b>	<b>1(Δ CONTRIB.)</b>			<b>Δ CONTRIB.AMT.</b>		
	<b>(1)</b>	<b>(2)</b>	<b>(3)</b>	<b>(4)</b>	<b>(5)</b>	<b>(6)</b>
1(Planning)	0.007 (0.005)	0.009** (0.004)	0.008* (0.004)	23.926 (43.326)	35.979 (37.695)	36.322 (38.473)
1(Balance)	0.012** (0.005)	0.014*** (0.004)	0.014*** (0.004)	56.074 (44.032)	57.482 (40.768)	59.703 (40.850)
1(Income)	0.012** (0.005)	0.013*** (0.004)	0.012*** (0.004)	73.890* (43.465)	89.363** (36.988)	85.424** (37.017)
Quad FEs	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
Adj. R <sup>2</sup>	0.000	0.013	0.022	0.000	-0.002	0.000
Control Mean	0.0409	0.0409	0.0409	83.4000	83.4000	83.4000
Departments	1,385	1,385	1,385	1,385	1,385	1,385
Individuals	16,881	16,881	16,881	16,881	16,881	16,881

Notes: 1(Δ Contrib.) is an indicator for whether there was any change in the election and Δ Contrib. Amt. is Period 2 annual contribution dollar amount minus Period 1 annual contribution dollar amount. Control group is the excluded category. Sample is restricted to employees present in both Period 1 and Period 2. Standard errors clustered at unit of randomization (Department) with unit of stratification fixed effects. Control variables in columns (3) and (6) include a gender indicator variable, quadratic in age, quadratic in tenure, ln(salary), percentage change in salary, faculty indicator, and indicators for different campuses. \*Significantly different at the 10% level; \*\*at the 5% level; \*\*\*at the 1% level.

**TABLE 4: EFFECTS OF INTERVENTIONS ON ADDITIONAL ASPECTS OF SAVING PROCESS**

	(1)	(2)	(3)	(4)	(5)	(6)
	<b>Diff. to find info</b>	<b>Better informed</b>	<b>Figured ret. savings</b>	<b>Understand sav-inc</b>	<b>Ret. income certainty</b>	<b>Financial satisfaction</b>
1(Planning)	-0.061 (0.044)	0.067 (0.042)	0.022 (0.021)	-0.002 (0.046)	-0.000 (0.040)	-0.001 (0.040)
1(Balance)	-0.052 (0.048)	0.084** (0.041)	0.018 (0.023)	-0.019 (0.046)	0.009 (0.042)	-0.021 (0.039)
1(Income)	-0.123** (0.048)	0.201*** (0.045)	0.051** (0.021)	0.060 (0.052)	0.102** (0.040)	0.078* (0.040)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Adj, R <sup>2</sup>	0.026	0.029	0.101	0.016	0.211	0.116
Control Mean	0.0460	-0.0588	0.4356	0.0037	-0.0261	-0.0093
Departments	984	993	994	991	965	992
Individuals	3,573	3,641	3,624	3,651	3,406	3,649

Notes: Dependent variable in Column (3) represents binary response to, “In the last 6 months, have you tried to figure out how much you need to save for retirement?” Dependent variables in remaining columns represent Z-scores of scaled survey responses for difficulty in finding retirement planning information, improvement in being informed about retirement planning, understanding the link between saving now and income in retirement, certainty in expected retirement income, and satisfaction with personal financial condition. Control group is the excluded category. Sample is restricted to employees present in both Period 1 and Period 2 who responded to follow-up survey. Respondents who answer “Don’t know” or “Prefer not to say” were omitted. Standard errors clustered at unit of randomization (Department) with unit of stratification fixed effects. Control variables include a gender indicator variable, quadratic in age, quadratic in tenure, ln(salary), percentage change in salary, faculty indicator, and indicators for different campuses. \*Significantly different at the 10% level; \*\*at the 5% level; \*\*\*at the 1% level.

**TABLE 5: HETEROGENEITY IN EFFECT OF INTERVENTIONS ON  $\Delta$  CONTRIBUTION AMOUNT**

	RATIONAL		TIME INCONSISTENCY			COGNITIVE BARRIERS		
	High disc. rate (1)	Liq. constr. (2)	Follows through (3)	Puts off planning (4)	Easy to use (5)	Overwhelming (6)	Fin. Lit. (7)	
I(Planning)	172.013 (108.767)	146.577 (107.192)	158.047 (108.731)	158.972 (108.019)	135.272 (111.412)	142.515 (108.752)	115.836 (103.845)	
I(Balance)	183.451 (129.409)	173.288 (128.781)	157.524 (128.283)	151.808 (128.161)	132.553 (136.818)	135.518 (128.686)	160.858 (128.438)	
I(Income)	401.579*** (116.170)	401.085*** (117.583)	383.300*** (113.821)	386.044*** (114.392)	397.911*** (120.961)	374.424*** (116.508)	386.580*** (114.769)	
Z-Score	2.394 (51.336)	-26.171 (53.890)	-137.920* (76.033)	4.925 (72.924)	-18.209 (72.609)	-32.877 (89.855)	2.904 (22.197)	
Z-Score X I(Planning)	-35.523 (79.224)	-43.309 (75.277)	159.423* (94.861)	33.134 (100.055)	32.419 (104.544)	86.585 (124.129)	15.819 (30.587)	
Z-Score X I(Balance)	-2.977 (84.743)	-180.700** (83.514)	108.396 (118.079)	111.631 (116.930)	125.195 (128.355)	27.426 (136.601)	0.988 (32.733)	
Z-Score X I(Income)	-207.885*** (78.986)	-197.623** (93.474)	320.155*** (114.142)	-233.232** (117.454)	151.770 (120.100)	17.821 (128.115)	45.078 (38.427)	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Adj. R <sup>2</sup>	-0.018	-0.014	-0.016	-0.016	-0.018	-0.020	-0.016	
Control Mean	108.7115	118.6636	120.2792	120.5272	125.8465	135.0172	121.5295	
Departments	992	986	995	992	979	989	991	
Individuals	3,663	3,604	3,675	3,668	3,512	3,648	3,619	

Notes: The dependent variable is  $\Delta$  Contribution Amount, Period 2 annual contribution dollar amount minus Period 1 annual contribution dollar amount. Z-score represents standardized response to survey question in column header. Heterogeneous effects captured by coefficient on interaction between treatment group indicator and Z-score. Control group is the excluded category. Sample is restricted to employees present in both Period 1 and Period 2 and who responded to the follow-up survey. Respondents who answer "Don't know" or "Prefer not to say" were omitted. Standard errors clustered at unit of randomization (Department) with unit of stratification fixed effects. Control variables include a gender indicator variable, quadratic in age, quadratic in tenure, ln(salary), percentage change in salary, faculty indicator, and indicators for different campuses. \*Significantly different at the 10% level; \*\* at the 5% level; \*\*\* at the 1% level.

**TABLE 6: EFFECT OF ASSUMPTIONS ON 1( $\Delta$  CONTRIBUTION)**

	(1)	(2)	(3)	(4)	(5)
1(Income)	-0.002 (0.004)	-0.002 (0.004)	-0.002 (0.004)	-0.002 (0.004)	-0.002 (0.004)
1(Inv Ret = 5%)	-0.002 (0.006)				0.002 (0.007)
1(Inv Ret = 7%)	-0.004 (0.006)				0.004 (0.011)
1(Ret Age = 67)		0.012** (0.005)			0.016** (0.006)
1(High Axes)			0.006 (0.005)		0.016 (0.012)
ln(RPM)				0.005 (0.006)	-0.015 (0.016)
Controls	Yes	Yes	Yes	Yes	Yes
Adj. R <sup>2</sup>	0.028	0.029	0.028	0.028	0.029
Balance Mean	0.0526	0.0526	0.0526	0.0526	0.0526
Departments	681	681	681	681	681
Individuals	8,484	8,484	8,484	8,484	8,484

Notes: Dependent variable is 1( $\Delta$  Contribution), which is an indicator for whether there was any change in the election between Periods 1 and 2. Sample is restricted to employees in the Income and Balance treatment groups who present in both Period 1 and Period 2. Balance group is the excluded category. Standard errors clustered at unit of randomization (Department) with unit of stratification fixed effects. Control variables include a gender indicator variable, quadratic in age, quadratic in tenure, ln(salary), percentage change in salary, faculty indicator, and indicators for different campuses. \*Significantly different at the 10% level; \*\*at the 5% level; \*\*\*at the 1% level.

**TABLE 7: EFFECT OF ASSUMPTIONS ON  $\Delta$  CONTRIBUTION AMOUNT**

	(1)	(2)	(3)	(4)	(5)
1(Income)	23.976 (32.765)	24.504 (32.729)	25.188 (32.711)	25.906 (32.843)	24.459 (32.675)
1(Inv Ret = 5%)	19.135 (53.581)				29.815 (76.277)
1(Inv Ret = 7%)	-27.962 (53.756)				-6.262 (108.591)
1(Ret Age = 67)		73.612* (43.478)			82.884* (49.030)
1(High Axes)			103.881** (44.217)		135.058 (122.515)
ln(RPM)				95.017** (48.376)	-45.441 (157.242)
Controls	Yes	Yes	Yes	Yes	Yes
Adj, R <sup>2</sup>	-0.003	-0.003	-0.003	-0.003	-0.003
Balance Mean	139.4744	139.4744	139.4744	139.4744	139.4744
Departments	681	681	681	681	681
Individuals	8,484	8,484	8,484	8,484	8,484

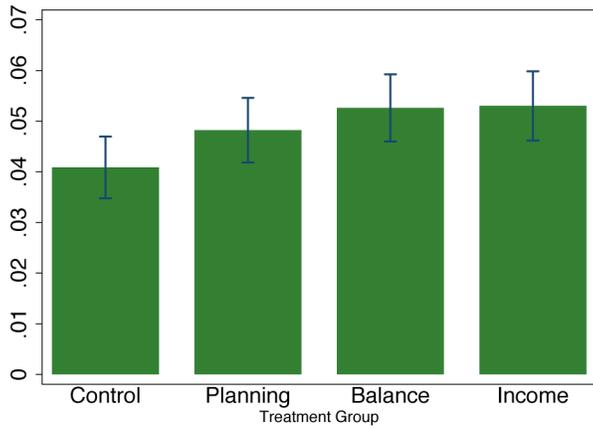
Notes: Dependent variable is  $\Delta$  Contribution Amount, which is Period 2 contribution dollar amount minus Period 1 contribution dollar amount. Sample is restricted to employees in the Income and Balance treatment groups who present in both Period 1 and Period 2. Balance group is the excluded category. Standard errors clustered at unit of randomization (Department) with unit of stratification fixed effects. Control variables include a gender indicator variable, quadratic in age, quadratic in tenure, ln(salary), percentage change in salary, faculty indicator, and indicators for different campuses. \*Significantly different at the 10% level; \*\*at the 5% level; \*\*\*at the 1% level.

**TABLE 8: EFFECTS OF INTERVENTIONS AND ASSUMPTIONS ON RETIREMENT AGE AND INVESTMENT RETURN BELIEFS**

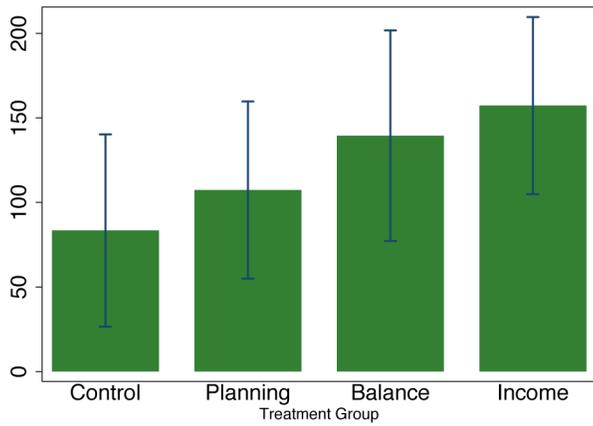
	(1)	(2)	(3)	(4)
	Exp. Ret Age	Exp. Return	Exp. Ret Age	Exp. Return
1(Planning)	0.383** (0.162)	0.131 (0.119)		
1(Balance)	0.281 (0.184)	0.095 (0.122)		
1(Income)	-0.086 (0.174)	0.104 (0.119)	-0.336 (0.208)	0.131 (0.134)
Inv Return (%)			-0.033 (0.061)	0.038 (0.040)
1(Ret Age = 67)			0.216 (0.209)	0.091 (0.141)
1(High Axes)			0.141 (0.213)	0.024 (0.135)
Controls	Yes	Yes	Yes	Yes
Adj, R <sup>2</sup>	0.090	0.023	0.077	0.010
Control Mean	65.6266	5.2896		
Balance Mean			66.0049	5.3682
Departments	940	847	455	394
Individuals	3,188	2,440	1,537	1,151

Notes: Dependent variable is as indicated in column heading. Control group is the excluded category in Columns 1 and 2; balance group is the excluded category in Columns 3 and 4. Sample is restricted to employees present in both Period 1 and Period 2 who responded to follow-up survey. Columns 3 and 4 restrict attention to the balance and income groups. Respondents who answer “Don’t know” or “Prefer not to say” were omitted. Standard errors clustered at unit of randomization (Department) with unit of stratification fixed effects. Control variables include a gender indicator variable, quadratic in age, quadratic in tenure, ln(salary), percentage change in salary, faculty indicator, and indicators for different campuses. \*Significantly different at the 10% level; \*\*at the 5% level; \*\*\*at the 1% level.

**FIGURE 1: OUTCOME VARIABLES BY TREATMENT GROUP**



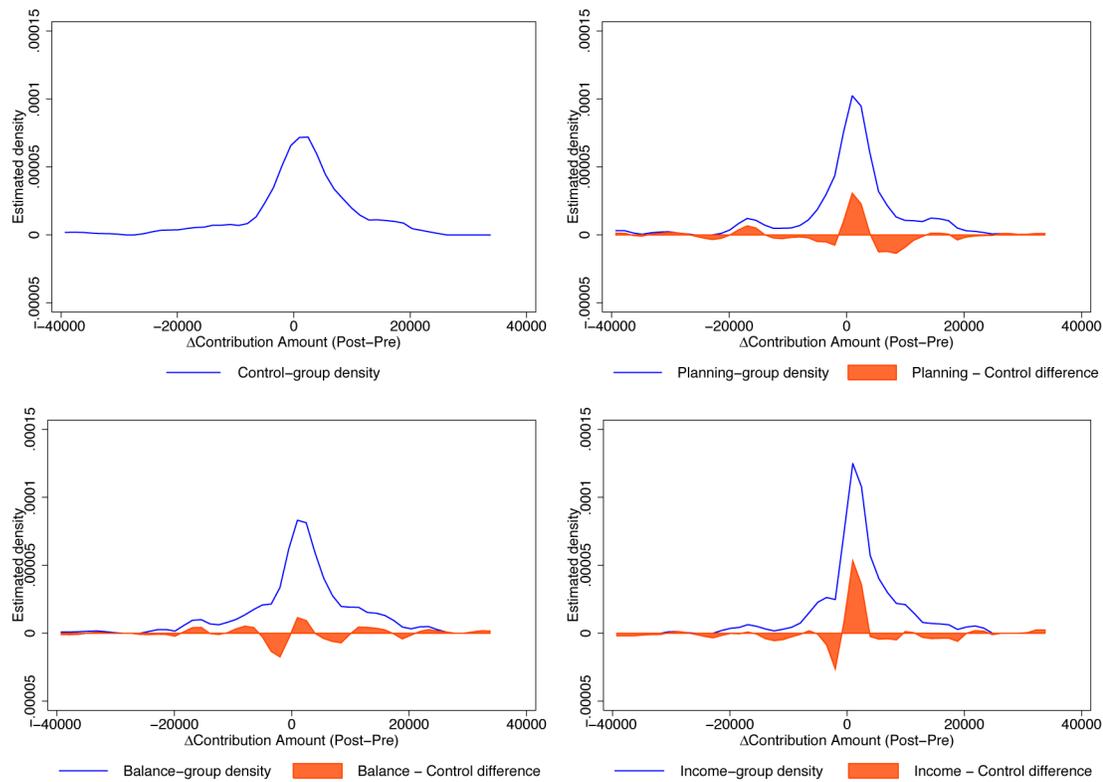
(a)  $1(\Delta \text{ Contribution})$



(b)  $\Delta \text{ Contribution (Amount): Post - Pre}$

Notes:  $1(\Delta \text{ Contribution})$  is an indicator for whether there was any active change in the election.  $\Delta \text{ Contribution Amount}$  is Period 2 contribution dollar amount minus Period 1 contribution dollar amount. Height of bar equals mean of each variable and brackets indicate 95% confidence interval.

**FIGURE 2: DISTRIBUTION OF CONTRIBUTION CHANGES AMONG CHANGERS  
 ( $\Delta$  CONTRIBUTION  $|\Delta$  CONTRIBUTION $\neq$ 0) BY TREATMENT GROUP**



Notes: each panel displays a kernel estimate of the density of  $\Delta$  Contribution Amount among those with non-zero changes for one of the four treatment groups. The estimated difference between each treatment group's density and the control group's density is also graphed.  $\Delta$  Contribution Amount is Period 2 contribution dollar amount minus Period 1 contribution dollar amount.

**APPENDIX A  
COMPARISON TO NATIONAL POPULATION**

We explore how the study sample compares to the national population. Table A-1 compares available demographic characteristics of the full administrative sample and the follow-up survey subsample to the Financial Capability Study's state-by-state, nationally-representative sample sponsored by FINRA.<sup>21</sup> We impose sample restrictions on the FINRA national sample, including working age (18 to 64), employed, and covered by pension plan, to clarify the extent to which these restriction explain observed differences between the FINRA and study samples.

The full sample of University of Minnesota employees is 56 percent female, while the follow-up survey subsample is 63 percent female (Table A-1). In comparison, the FINRA national sample is only 48.7 percent female. When we restrict the FINRA sample to the same ages (18-64) as the study sample, which constitutes 84.8 percent of the national population, the percent female in FINRA increases to 49.8 percent. When we restrict the national sample to employed workers, this leaves a subsample that represents 43.8 percent of the national population and is 54.6 percent female. This reveals that the fraction of the study sample that is female is similar to the fraction of employed Americans who are female. Finally, when we add the employer-provided pension plan restriction the FINRA national, which leaves only

28.7 percent of the original FINRA sample, the fraction female remains stable (54.5 percent) and is comparable to the study sample. Comparing the full study sample to the restricted FINRA sample also reveals that the age profiles are very similar.

The follow-up survey sample, however, is substantially more likely to be white than the most comparable national subsample (90 versus 68 percent). The follow-up survey sample is also somewhat more likely to be married (73 versus 64 percent), much more likely to have a post-graduate degree (52 versus 15 percent), and less liquidity constrained. While the fractions reporting having tried to figure how much savings is needed for retirement are similar, the questions ask about different reference periods. In the study survey, 45.2 percent reporting having done this in the last six months, since the Period 1 data pull. In contrast, the national sample is asked to report having done this ever.

We included select FINRA questions on our follow-up survey to facilitate comparability between the follow-up survey sample and the FINRA respondents in terms of financial literacy (Table A-2). The results suggest that, in comparison to the national subsamples, the study's follow-up survey subsample is more financially satisfied, more likely to keep up with economic and financial news, higher self-assessed financial literacy, higher score on the financial literacy quiz, and is slightly more willing to take risks. However, they report being about equally good at day-to-day financial matters and equally in agreement with the statement, "I'm pretty good at math."

21 FINRA is the Financial Industry Regulatory Authority.

**TABLE A-1: COMPARISON OF STUDY SAMPLE WITH FINRA SAMPLE: DEMOGRAPHICS**

<b>Source:</b>	<b>Study</b>		<b>FINRA</b>			
<b>Sample:</b>	<b>All</b>	<b>Survey Resp.</b>	<b>US</b>	<b>Age 18-65</b>	<b>+Employed</b>	<b>+Empl. Pension</b>
<b>Size/Subsample %</b>	<b>16,881</b>	<b>21.9%</b>	<b>28,146</b>	<b>84.8%</b>	<b>43.8%</b>	<b>28.7%</b>
1(female)	0.56	0.63	0.487	0.498	0.546	0.545
1(white)		0.90	0.685	0.649	0.644	0.679
1(married)		0.73	0.534	0.523	0.547	0.641
Age Indicators						
18-24 years	0.021	0.014	0.135	0.159	0.125	0.056
25-34 years	0.203	0.172	0.171	0.201	0.234	0.223
35-44 years	0.246	0.205	0.183	0.216	0.252	0.278
45-54 years	0.279	0.289	0.196	0.231	0.244	0.275
55-64 years	0.249	0.320	0.163	0.192	0.146	0.167
Education Indicators						
Less than HS		0.000	0.035	0.035	0.019	0.009
HS Grad		0.017	0.293	0.288	0.243	0.193
Some College		0.118	0.419	0.421	0.406	0.398
College Grad.		0.346	0.159	0.168	0.217	0.251
Post-Grad degree		0.520	0.094	0.088	0.115	0.149
Liquidity-ease paying monthly bills						
Very difficult		0.060	0.185	0.200	0.153	0.112
Somewhat difficult		0.282	0.444	0.457	0.459	0.435
Not at all difficult		0.657	0.372	0.344	0.387	0.453
1(figured ret. savings)		0.452*	0.304	0.336	0.415	0.508

\* asks only about last 6 months.

Notes: In the left panel, "All" refers to the full study sample, while "Survey Resp." refers to the follow-up survey subsample. In the right panel, "US" is the state-by-state FINRA nationally representative sample; each subsequent column restricts this sample by the condition listed in the column header.

**TABLE A-2: COMPARISON OF STUDY SAMPLE WITH FINRA SAMPLE: SURVEY RESPONSES**

Source: Sample:	Study Survey Resp.		US		Age 18-65		FINRA + Employed		+ Empl.	Pension
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Fin. satisfaction	5.5	2.7	4.5	2.8	4.3	2.7	4.6	2.6	4.9	2.6
Good at day-to-day	5.8	1.4	5.6	1.6	5.5	1.6	5.5	1.6	5.7	1.6
Pretty good at math	5.5	1.5	5.6	1.7	5.6	1.7	5.6	1.6	5.8	1.6
Keep up with econ. news	4.4	1.7	4.8	1.8	4.7	1.8	4.8	1.7	5.0	1.7
Fin. knowledge (self-assess)	4.3	1.5	4.9	1.3	4.9	1.3	4.9	1.2	5.1	1.2
Quiz items correct, of 5	4.0	1.0	2.9	1.3	2.9	1.4	3.0	1.3	3.2	1.2
Willing to take risks	5.4	2.2	4.3	2.6	4.5	2.6	4.8	2.5	5.1	2.5

Notes: In the left panel, "Survey Resp." refers to the follow-up survey subsample of the study. In the right panel, "US" is the state-by-state FINRA nationally representative sample; each subsequent column restricts this sample by the condition listed in the column header.

**APPENDIX B  
INTERVENTION MATERIALS**

**FIGURE B-1: EXAMPLE BROCHURE: PAGE 1**



*What sources of income will be available to me in retirement?*

- Most University employees participate in **mandatory retirement plans**.
  - The Minnesota State Retirement System provides a traditional pension to most civil service and non-faculty bargaining unit employees.
  - The Faculty Retirement Plan is a defined contribution plan to which most faculty and academic professionals and administrators (P&A employees) contribute.
- Most employees are eligible to participate in **Voluntary Retirement Plans**, which allow up to \$16,500 of pre-tax contributions per year. The two types of Voluntary Retirement Plans are the
  - Optional Retirement Plan (ORP) and
  - Section 457 Deferred Compensation Plan.
- Substantially all University employees also participate in the federal **Social Security** system.

*How much retirement income will I want?*

People typically want retirement income to be between 75% and 90% of their expected income prior to retirement, depending on their desired lifestyle.

**My goal is: \$ \_\_\_\_\_ per year.**

*Am I on target to meet my goals?*

Social Security and mandatory retirement plans provide some, but often not all, of the retirement income desired. Consult plan statements, Social Security benefit statements, or advisors for estimates of retirement income from these sources.

**From Social Security and my mandatory retirement plan, I expect \$ \_\_\_\_\_ per year.**

*What else can I do?*

Voluntary Retirement Plans can help fill any gap. Nearly 5,000 University of Minnesota employees take advantage of these additional tax-advantaged savings opportunities.



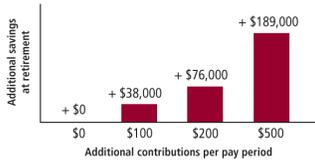
Brought to you by:  
Center for  
Human Resources  
and Labor Studies  
**CARLSON**  
SCHOOL OF MANAGEMENT  
UNIVERSITY OF MINNESOTA

**FIGURE B-2: EXAMPLE BROCHURE FOR INCOME TREATMENT: PAGES 2-3**

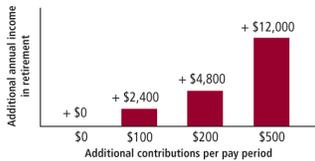
Estimated Retirement Income Statement for [REDACTED]

*If I begin making additional contributions now...*

*...how much additional savings at retirement can I expect to have?*



*...how much additional annual income in retirement can I expect to receive from these additional savings?*



**Assumptions.** The true values of these future outcomes are uncertain and all projections depend on assumptions. The above projections assume:

- You begin additional contributions this year and continue them until retirement at age 67.
- Your assets earn a 5% rate of return annually.
- You are married and use your account balance at age 67 to purchase a joint survivor annuity which pays a fixed amount as long as either you or your spouse is alive.

**Online calculator.** You will soon receive an email giving you access to our online calculator. Here, you can personalize the projections further by changing assumptions regarding your:

- Marital status
- Expected retirement age
- Average annual rate of investment return
- Other sources of retirement income

The online calculator will also allow you to input an expected rate of inflation to reflect the above figures in today's dollars.

*I am not currently enrolled in a Voluntary Retirement Plan. How do I participate?*

- Step 1:** Drop the enclosed Information and Enrollment Kit Request Card in Campus mail (for Twin Cities) or U.S. Mail (for Crookston, Duluth, Morris, or Rochester). In a few weeks you will receive the relevant information and enrollment forms.
- Step 2:** Choose the Optional Retirement Plan or the Section 457 Plan. More information about the differences between the two plans is provided in this brochure.
- Step 3:** Choose your per-pay-period savings rate and complete the Salary Reduction Agreement (ORP) or Retirement Savings Agreement (457 Plan).
- Step 4:** Select an investment company and complete the account enrollment form for the company or companies you select.
- Step 5:** Return the completed forms to:  
University of Minnesota  
Employee Benefits  
100 Donhowe  
319 15th Avenue SE  
Minneapolis, MN 55455-0103

*I already participate in a Voluntary Retirement Plan. How do I change my contribution rate?*

- Step 1:** Fill out the enclosed 1-page Salary Reduction Agreement (ORP) or Retirement Savings Agreement (457 Plan) with your new rate or amount.
- Step 2:** Return the form to:  
University of Minnesota  
Employee Benefits  
100 Donhowe  
319 15th Avenue SE  
Minneapolis, MN 55455-0103

If you want additional information on the investment companies or investment options, please select 'Yes' on the enclosed Information and Enrollment Kit Request Card and drop in Inter-campus mail (for Twin Cities Campus) or U.S. Mail (Crookston, Duluth, Morris, or Rochester).



**Got questions? Get answers.**  
Talk to the Employee Benefits Service Center at (612) 624-9090, option 2.

**For more information...**

To learn more about Retirement Savings Plans at the University of Minnesota, you can:

Call the Employee Benefits Service Center at (612) 624-9090 and select Option 2.

Visit [www1.umn.edu/ohr/benefits/retiresave/index.html](http://www1.umn.edu/ohr/benefits/retiresave/index.html).

Attend a Voluntary Retirement Program workshop where you will learn about the two plans and your investment options. Call the Employee Benefits Service Center at (612) 624-9090 and select Option 1 for dates and registration.

**FIGURE B-3: EXAMPLE BROCHURE FINAL PAGE**

*Comparing the Optional Retirement and 457 Plans*

Pension law simplified 457 Plans to bring them more in line with qualified retirement plans like the Optional Retirement Plan. However, differences still exist. To help you understand these differences – so you can make the most of your retirement saving opportunities – review the chart below.

	Optional Retirement Plan	457 Plan
In-service distributions while employed	<ul style="list-style-type: none"> <li>• Distributions may be made for any financial hardship, including college education (at the University’s discretion)</li> <li>• Available after age 59½ for any reason</li> </ul>	<p>Distributions are only available for unforeseeable emergencies (at the University’s discretion) or if:</p> <ul style="list-style-type: none"> <li>• Your balance does not exceed \$5,000</li> <li>• There have been no previous in-service distributions</li> <li>• You’ve made no contributions in the previous two years</li> <li>• You elect such a distribution</li> </ul>
Annual contribution limits	\$200 Minimum \$16,500 Maximum	No minimum \$16,500 Maximum
Catch-up contributions	May be made beginning at age 50	In the three years prior to the year in which you turn age 65, you may contribute up to a maximum of twice that year’s maximum contribution amount
Loans	Up to 50 percent of your balance may be available to you as a loan, to a maximum of \$50,000	Not allowed
Investment options	Range from aggressive growth mutual funds to conservative interest-bearing accounts with Securian Retirement, Fidelity, Vanguard, and DWS Scudder Investments	Range from aggressive growth mutual funds to conservative interest-bearing accounts with Securian Retirement, Fidelity, and Vanguard
Money becomes available	The earlier of termination of employment or reaching age 59½	The later of termination or the calendar year in which you attain age 70½

**FIGURE B-4: ONLINE CUSTOMIZATION TOOL SCREENSHOT: INCOME TREATMENT**

LIDA Calculator Proto V4.0

https://solutions.oms.umn.edu/cgi-bin/qwebcorporate.dll

### Retirement Income Online Customization Tool

**Fill in your characteristics and assumptions below. Then hit the Calculate button at the bottom of the page.**

Current Age:

Expected Retirement Age:

Gender:

Marital Status:

Average Investment Returns: %  
[Historical Rates of Return](#)

Adjust Future Values for Inflation:

**Current Contributions per Pay Period**

U of M Voluntary Retirement Plans:

Other retirement savings accounts:

**Current Account Balances**

U of M Voluntary Retirement Plans:

Other retirement savings accounts:

**Estimated Annual Income at Retirement Age from Other Sources**

Annual Defined Benefit Income:

Annual Social Security Income:   
[Social Security estimation calculator](#)

If I begin making additional contributions now...  
 ...how much savings at retirement can I expect to have?

Additional Contributions per Pay Period	From Current Account Balances	From Current Contributions	From Additional Contributions	Total Savings at Retirement
\$0	\$847,000	\$509,000	\$0	\$1,356,000
\$50	\$847,000	\$509,000	\$64,000	\$1,420,000
\$100	\$847,000	\$509,000	\$127,000	\$1,483,000
\$250	\$847,000	\$509,000	\$471,000	\$1,827,000

...how much annual income in retirement can I expect to receive from these savings?

Additional Contributions per Pay Period	From Current Account Balances	From Current Contributions	From Social Security and Defined Benefit Plans	Total Annual Income in Retirement
\$0	\$51,600	\$31,000	\$10,000	\$92,600
\$50	\$51,600	\$31,000	\$27,400	\$110,000
\$100	\$51,600	\$31,000	\$34,200	\$116,800
\$250	\$51,600	\$31,000	\$36,800	\$119,400

**Assumptions.** The true values of these future outcomes are uncertain and all projections depend on assumptions. The above projections assume:

- You begin additional contributions this year and continue them until you retire at age 65
- Your assets earn a 5% rate of return annually.
- Your projections have not been adjusted for future inflation. This is equivalent to assuming future inflation is 0% per year.
- You are married and use your account balance at age 65 to purchase a joint survivor annuity which pays a fixed amount as long as either you or your spouse is alive.

**APPENDIX C****BALANCE ACROSS TREATMENT GROUPS: FULL AND FOLLOW-UP SURVEY SAMPLES**

Observable characteristics by treatment group are shown for the full administrative sample in Table C-1. Each characteristic was regressed on treatment group indicators with the mean of the characteristic for the control group shown in a row below. We report the F- statistic for the joint test of the hypothesis that all coefficients on the planning, balance and income group indicator variables are zero and report the p-value of the test at the bottom of the table. The shaded columns represent characteristics which were explicitly balanced across treatment groups in the randomization procedure. The table shows that there are very few statistically significant differences in observable characteristics across treatment groups. The only characteristic that differs significantly across the different groups is gender, with a statistically higher percentage of women in the income group. For the remaining characteristics, we fail to reject the null hypothesis that there are differences across the four experimental groups.

In terms of the follow-up survey sample, Table C-2 presents evidence on what factors influence survey response by regressing a dummy variable for survey response on treatment group and incentive group indicators. Column 1 shows that being assigned into one of the three groups sent printed materials significantly reduced the likelihood of response: the response rate was 24 percent in the control group, and 2-3 percentage points lower in the planning, balance, and income groups. These estimates suggest that the reduction in survey response was due to a general hassle factor from receiving repeated communication from the researchers rather than a specific piece of information contained in the balance or income group mailings. Column 2 shows that the small \$2 non-conditional incentive sent at the outset of the experiment led to a statistically significant increase in response rates, and Column 3 shows that the effect of the incentive on response rates did not significantly differ across treatment groups.<sup>22</sup>

We next examine the demographic characteristics of the survey respondents, how they differ from our full administrative sample, and whether the differences in response rates across treatment groups led to observable differences across treatment groups in our survey subsample. Table C-3 shows the results of regressing several observable characteristics on treatment group dummies for the survey subsample. As in Table C-1, we report the mean of the characteristic for the control group and the p-value for the joint test of the hypothesis that all coefficients on the planning, balance and income group indicator variables are zero at the bottom of the table.

Compared to Table C-1, our survey subsample is more likely to be female, has a greater number of faculty, and are more likely to be VRP participants. However, there are very few instances where observable characteristics differ significantly across treatment groups within the survey subsample. The reported p-values are generally higher than conventional levels of significance, with the exception of that for age, where the respondents in the income group are approximately one year older than respondents in the control group. Table C-4 shows the treatment effects of our administrative outcomes in our survey subsample. The estimated treatment effects are larger in magnitude relative to our full administrative sample.

Together, this evidence indicates that survey responders are not an entirely representative sample of our population, as there are some differences in observable characteristics between survey responders and the entire sample, and treatment effects are larger. However, the results in Table C-3 suggest that the differential response rate across treatment groups did not create large imbalances in observable characteristics across treatment groups within the survey subsample. Assuming that the data are missing at random conditional on observables, there are still insights to be gained from the richer set of information available from survey responders.

**TABLE C-1: DEMOGRAPHICS BY TREATMENT GROUP: FULL SAMPLE**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	<b>1(Female)</b>	<b>Age</b>	<b>Tenure</b>	<b>ln(Salary)</b>	<b>% Δ Salary</b>	<b>1(Faculty)</b>	<b>Participant</b>	<b>Cont. Amt.</b>
1(Planning)	0.027 (0.021)	-0.021 (0.165)	-0.070 (0.255)	0.009 (0.013)	-0.004 (0.003)	-0.022 (0.023)	0.001 (0.005)	-70.676 (141.216)
1(Balance)	-0.015 (0.021)	0.041 (0.191)	0.069 (0.259)	-0.008 (0.014)	-0.005 (0.003)	-0.005 (0.022)	0.002 (0.005)	94.308 (139.693)
1(Income)	0.061*** (0.023)	0.134 (0.175)	-0.050 (0.275)	-0.001 (0.014)	-0.004 (0.003)	-0.019 (0.025)	0.002 (0.005)	-86.059 (145.598)
Adj. R <sup>2</sup>	0.057	0.139	0.065	0.241	0.117	0.151	0.087	0.048
Control Mean	0.5388	44.9451	12.4280	10.8471	0.0190	0.4263	0.2419	2348.3678
Departments	1,385	1,385	1,385	1,385	1,385	1,385	1,385	1,385
Individuals	16,881	16,881	16,881	16,881	16,881	16,881	16,881	16,881
F-Statistic	4.3253	0.3811	0.1322	0.4994	0.772	0.4096	0.1123	0.7797
p-value	0.0048	0.7666	0.9409	0.6828	0.5067	0.7461	0.9530	0.5053

Notes: Dependent variable as indicated in column header. Shaded columns indicate variables used to balance randomization. Sample is restricted to employees present in both Period 1 and Period 2. F-statistics for the joint test of the hypothesis that all coefficients on the planning, balance and income group indicator variables equal zero and the p-value of the F-test are reported at the bottom of the table. Standard errors clustered at unit of randomization (Department) with unit of stratification fixed effects. \*Significantly different at the 10% level; \*\*at the 5% level; \*\*\*at the 1% level.

**TABLE C-2: SURVEY RESPONSE BY TREATMENT GROUP AND INCENTIVES**

	(1)	(2)	(3)
1(Planning)	-0.024** (0.010)		-0.021* (0.011)
1(Balance)	-0.039*** (0.010)		-0.042*** (0.011)
1(Income)	-0.028*** (0.010)		-0.030*** (0.011)
1(Incentive)		0.090*** (0.013)	0.083*** (0.029)
1(Incentive) X 1(Planning)			-0.032 (0.039)
1(Incentive) X 1(Balance)			0.006 (0.038)
1(Incentive) X 1(Income)			0.054 (0.039)
Controls	Yes	Yes	Yes
Adj. R <sup>2</sup>	0.043	0.048	0.049
Control Mean	0.2402	0.2489	0.2489
Departments	1,385	1,046	1,046
Individuals	16,881	13,667	13,667

Notes: Dependent variable is indicator variable for survey responder. Control group is the excluded category. 1(Incentive) is indicator variable for receipt of non-conditional \$2 incentive in beginning of study. Sample is restricted to employees present in both Period 1 and Period 2. Columns 2 and 3 restrict attention to the Twin Cities campus because only that campus was eligible to receive the non-conditional incentive. Standard errors clustered at unit of randomization (Department) with unit of stratification fixed effects. Control variables include a gender indicator variable, quadratic in age, quadratic in tenure, ln(salary), percentage change in salary, faculty indicator, and indicators for different campuses. \*Significantly different at the 10% level; \*\*at the 5% level; \*\*\*at the 1% level.

**TABLE C-3: DEMOGRAPHICS BY TREATMENT GROUP: FOLLOW-UP SURVEY SAMPLE**

	<b>1(Female) (1)</b>	<b>Age (2)</b>	<b>Tenure (3)</b>	<b>ln(Salary) (4)</b>	<b>% Δ Salary (5)</b>	<b>1(Faculty) (6)</b>	<b>Participant (7)</b>	<b>Cont. Amt. (8)</b>
1(Planning)	-0.001 (0.027)	-0.060 (0.480)	-0.285 (0.461)	0.000 (0.017)	0.001 (0.003)	-0.058** (0.027)	0.007 (0.018)	-220.877 (309.109)
1(Balance)	0.007 (0.029)	0.096 (0.561)	-0.261 (0.498)	-0.011 (0.020)	0.004 (0.003)	-0.001 (0.028)	0.004 (0.018)	-48.095 (307.736)
1(Income)	0.037 (0.027)	1.096** (0.510)	0.174 (0.516)	0.029 (0.018)	0.004 (0.003)	-0.009 (0.030)	0.025 (0.018)	-139.883 (315.246)
Adj. R <sup>2</sup>	0.038	0.137	0.059	0.189	-0.010	0.088	0.093	0.027
Control Mean	0.6219	46.5902	13.9888	10.9388	0.0129	0.4652	0.3402	3208.2060
Departments	996	996	996	996	996	996	996	996
Individuals	3,688	3,688	3,688	3,688	3,688	3,688	3,688	3,688
F-Statistic	0.8783	2.6859	0.3950	1.6723	0.7318	2.0371	0.6906	0.1998
p-value	0.4518	0.0454	0.7566	0.1713	0.5331	0.1070	0.5579	0.8966

Notes: Dependent variable as indicated in column header. Control group is the excluded category. Sample is restricted to employees present in both Period 1 and Period 2 who responded to follow-up survey. Standard errors clustered at unit of randomization (Department) with unit of stratification fixed effects. Control variables include a gender indicator variable, quadratic in age, quadratic in tenure, ln(salary), percentage change in salary, faculty indicator, and indicators for different campuses. \*Significantly different at the 10% level; \*\*at the 5% level; \*\*\*at the 1% level.

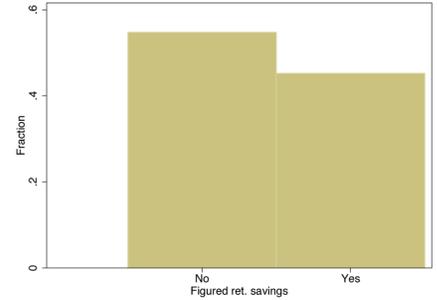
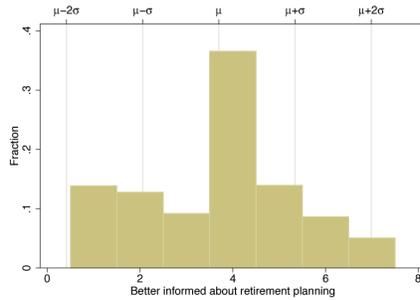
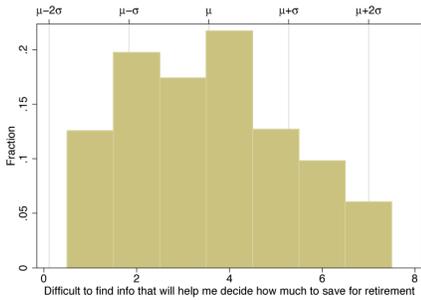
**TABLE C-4: EFFECT OF INTERVENTIONS: FOLLOW-UP SURVEY SUBSAMPLE**

Outcome:	1(Δ Contrib.)			Δ Contrib. Amt.		
	(1)	(2)	(3)	(4)	(5)	(6)
1(Planning)	0.028** (0.013)	0.034*** (0.012)	0.032*** (0.012)	92.410 (94.572)	146.974 (107.036)	156.591 (108.575)
1(Balance)	0.042*** (0.014)	0.051*** (0.013)	0.051*** (0.013)	101.140 (109.785)	145.999 (127.414)	154.868 (128.151)
1(Income)	0.050*** (0.012)	0.058*** (0.012)	0.054*** (0.012)	26.426*** (97.782)	391.536*** (114.756)	85.563*** (114.828)
Quad FEs	No	Yes	Yes	No	Yes	Yes
Controls	No	No	Yes	No	No	Yes
Adj. R <sup>2</sup>	0.004	0.013	0.018	0.002	-0.019	-0.017
Control Mean	0.0625	0.0625	0.0625	119.7863	119.7863	119.7863
Departments	996	996	996	996	996	996
Individuals	3,688	3,688	3,688	3,688	3,688	3,688

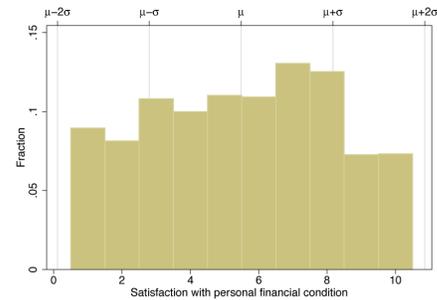
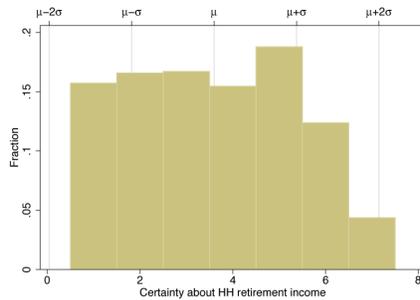
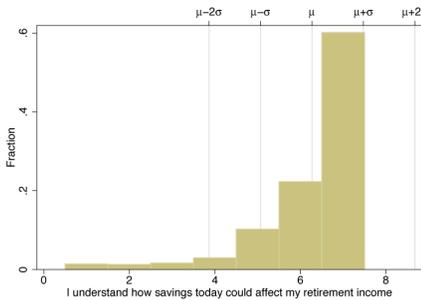
Notes:Notes:1(Δ Contrib.) is an indicator for whether there was any change in the election and Δ Contrib. Amt. is Period 2 contribution dollar amount minus Period 1 contribution dollar amount. Control group is the excluded category. Sample is restricted to employees present in both Period 1 and Period 2 and who responded to the follow-up survey. Standard errors clustered at unit of randomization (Department) with unit of stratification fixed effects. Control variables in columns (3) and (6) include a gender indicator variable, quadratic in age, quadratic in tenure, ln(salary), percentage change in salary, faculty indicator, and indicators for different campuses. \*Significantly different at the 10% level; \*\*at the 5% level; \*\*\*at the 1% level.

**APPENDIX D  
DISTRIBUTIONS OF SURVEY ITEM RESPONSES**

**FIGURE D-1: SURVEY RESPONSES: ALTERNATIVE OUTCOMES**

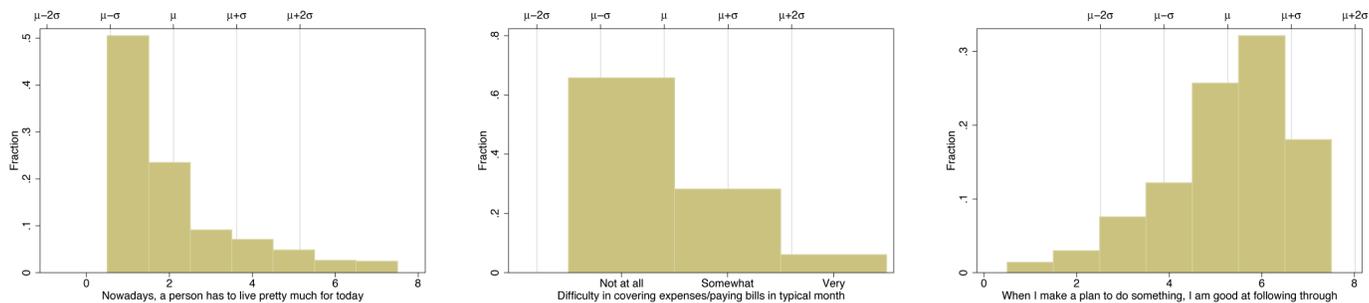


(a) “It is difficult to find information that will help me decide how much to save for retirement.” (b) “I am better informed about retirement planning than I was 6 months ago.” (c) “In the last 6 months, have you tried to figure out how much you need to save for retirement?”

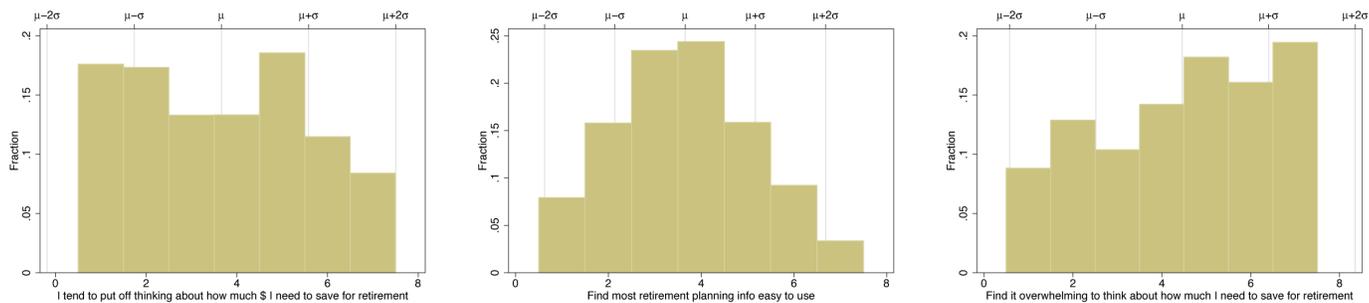


(d) “I understand how savings today could affect my retirement income.” (e) “How certain are you about the amount of annual retirement income you expect your household to have?” (f) “Overall, thinking of your assets, debts and savings, how satisfied are you with your current personal financial condition?”

**FIGURE D-2: SURVEY RESPONSES: MEASURES OF HETEROGENEITY IN RATIONAL-ACTOR FACTORS, TIME-INCONSISTENCY, AND LIMITED COGNITION**

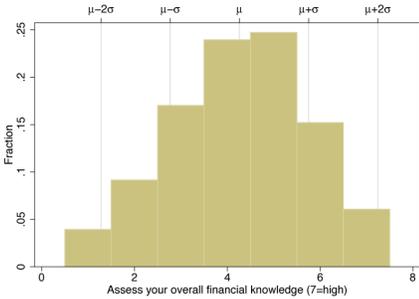


(a) “Nowadays, a person has to live pretty much for today and let tomorrow take care of itself.”  
 (b) “In a typical month, how difficult is it for you to cover your expenses and pay all your bills?”  
 (c) “When I make a plan to do something, I am good at following through.”

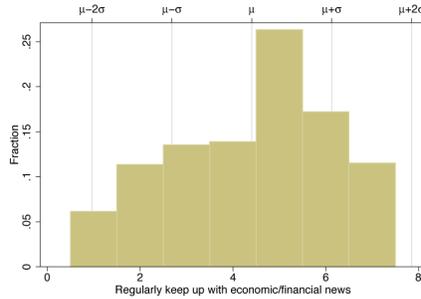


(d) “I tend to put off thinking about how much money I need to save for retirement.”  
 (e) “I find most retirement planning information easy to use.”  
 (f) “I find it overwhelming to think about how much I need to save for retirement.”

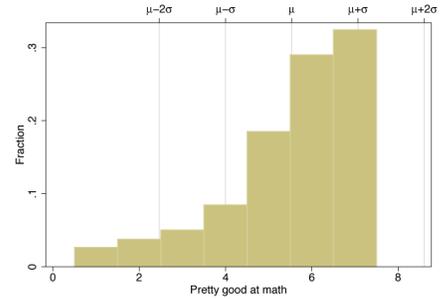
**FIGURE D-3: SURVEY RESPONSES: FINANCIAL LITERACY**



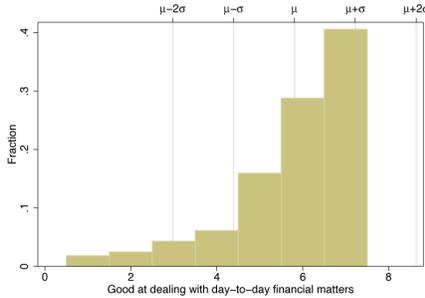
(a) “On a scale from 1 to 7, where 1 means very low and 7 means very high, how would you assess your overall financial knowledge?”



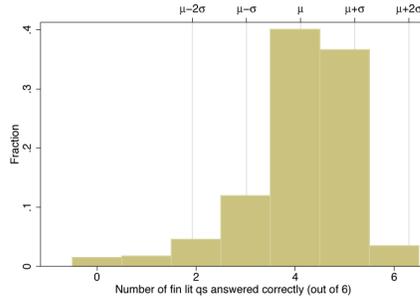
(b) “I regularly keep up with economic and financial news.”



(c) “I am pretty good at math.”



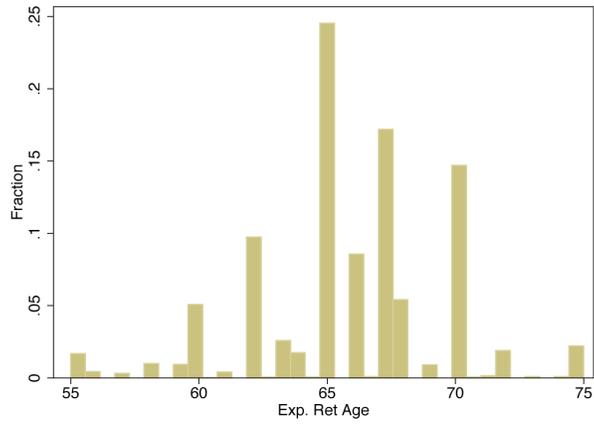
(d) “I am good at dealing with day-to-day financial matters...”



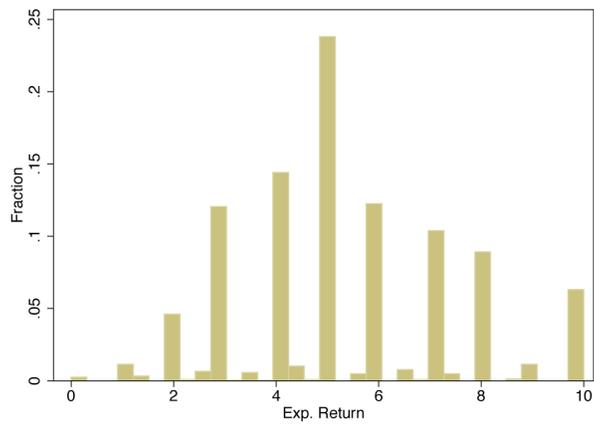
(e) Number of questions answered correctly on financial literacy quiz

Notes: Sample is restricted to employees present in both Period 1 and Period 2 who responded to follow-up survey. Responses exclude individuals who answered “don’t know” or “prefer not to say.”

**FIGURE D-4: SURVEY RESPONSES: RETIREMENT AGE AND INVESTMENT RETURN BELIEFS**



(a) Expected Retirement Age



(b) Expected Rate of Return

Notes: Sample is restricted to employees present in both Period 1 and Period 2 who responded to follow-up survey. Responses exclude individuals who answered “don’t know” or “prefer not to say.”

**APPENDIX E**  
**FINANCIAL LITERACY QUESTIONS**

The following questions comprise the financial literacy quiz provided to survey respondents.<sup>23</sup>  
Correct answers are marked in **bold**.

1. Imagine that the interest rate on your savings account was 1% per year and inflation was 2% per year. After 1 year, how much would you be able to buy with the money in this account?
  - (a) More than today
  - (b) Exactly the same
  - (c) **Less than today**
  - (d) Don't know
  - (e) Prefer not to say
  
2. Do you think the following statement is true or false? "Buying a single company stock usually provides a safer return than a stock mutual fund."
  - (a) True
  - (b) **False**
  - (c) Don't know
  - (d) Prefer not to say
  
3. Suppose you had \$100 in a savings account and the interest rate was 2% per year. After 5 years, how much do you think you would have in the account if you left the money to grow:
  - (a) **More than \$102**
  - (b) Exactly \$102
  - (c) Less than \$102
  - (d) Don't know
  - (e) Prefer not to say
  
4. If interest rates rise, what will typically happen to bond prices?
  - (a) They will rise
  - (b) **They will fall**
  - (c) They will stay the same
  - (d) There is no relationship between bond prices and the interest rate
  - (e) Don't know
  - (f) Prefer not to say
  
5. Do you think the following statement is true or false? "A 15-year mortgage typically requires higher monthly payments than a 30-year mortgage, but the total interest paid over the life of the loan will be less."
  - (a) **True**
  - (b) False
  - (c) Don't know
  - (d) Prefer not to say
  
6. Let's say you have \$200 in a savings account. The account earns 10 percent interest per year. How much would you have in the account at the end of two years? [open-ended; correct answer = **\$242**]

23 Questions taken from Lusardi and Mitchell (2007).