

Framing the Future: The Risks of Pre-Commitment Nudges and Potential of Fresh Start Messaging

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Abstract: There is growing interest in applying principles from the field of behavioral science to shift individual decisions in desirable directions through “nudges.” However, an important but oft-overlooked feature of nudges is that they can leak information about the implicit recommendations of their designers. In this paper, we report on paired field and laboratory experiments highlighting that a widely-used nudge—encouraging pre-commitment—can backfire, and that this appears to be due to unintentional information leakage. We also highlight the benefits of a previously untested nudge—framing a future opportunity for behavior change as a “fresh start”—as a means of encouraging future-oriented behaviors. We conducted a field experiment intended to nudge increased retirement savings with 8,682 employees from four major U.S. universities. Offering people the opportunity to choose to save now or to save at a time delay significantly decreased savings over a nine-month follow-up period compared to offering people the opportunity to choose to save now, but highlighting that delayed savings will begin after a fresh start (e.g., an employee’s birthday) counteracted this negative effect. A laboratory experiment suggests an explanation: when a nudge encourages good behavior “later” (i.e., pre-commitment), it leaks the message that the target behavior is not an urgent priority, undermining the nudge’s efficacy. Our findings highlight (1) the critical importance of pilot testing nudges to assess the implicit messages that may be unintentionally leaked by their design and (2) the potential of nudges that rely on fresh start framing.

Keywords: choice architecture, nudge, pre-commitment, information leakage, fresh start, savings

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1. Introduction

Governments and corporations are increasingly interested in applying principles from the field of behavioral science to shift individual decisions in desirable directions through “nudges” (Johnson et al. 2012, Ly et al. 2013). Nudges are influence tactics that “alter behavior in a predictable way without forbidding any options or significantly changing...economic incentives” (Thaler and Sunstein 2008). In 2010, the United Kingdom created a “nudge unit” charged with using behavioral insights to improve government processes and services, and numerous other countries and municipalities have since followed suit, including the United States, Australia, Germany, the Netherlands, and Singapore. The World Bank now boasts a nudge unit, and a growing contingent of consulting firms (e.g., ideas42, BEWorks, the Behavioural Insights Team) help for-profit and not-for-profit organizations design nudges.

An important but understudied feature of nudges is that they can leak information about the implicit recommendations of their designers. Information leakage is a well-known contributor to the efficacy of default nudges (McKenzie et al. 2006, Tannenbaum and Ditto 2011): people infer that policy makers are recommending whichever option they set as the status quo or default, such as being an organ donor or a savings plan participant, which helps make defaults sticky (Johnson and Goldstein 2003, Madrian and Shea 2001). Recent research has also shown that penalties convey a stronger signal of disapproval than equivalent rewards for avoiding the same behavior (Tannenbaum et al. 2013).

In this paper, we report on paired field and laboratory experiments highlighting that a widely-used nudge—encouraging pre-commitment—can backfire, and that this appears to be due to information leakage. Allowing pre-commitment (i.e., letting people commit to decisions in advance of their implementation) is a nudge that has previously been shown to combat present bias, the tendency to dramatically overweight immediate utility flows relative to future utility flows (e.g., Read and Van Leeuwen 1998, Rogers and Bazerman 2008). Pre-commitment combats present bias by ensuring that at the time of a decision regarding whether to engage in a valuable, future-oriented behavior (e.g., eating healthier foods, donating more to charity), the long-term benefits and short-term costs of engagement are not asymmetrically discounted (see Milkman et al. 2008 for a review). However, we show that when nudges allow people to elect to engage in good behavior “later” (i.e., pre-commitment), this leaks the message that the target behavior is not an urgent priority, potentially undermining the efficacy of such nudges. Our

findings highlight the critical importance of (1) attending to the implicit messages that may be unintentionally leaked by policy makers through the nudges they design and (2) pilot testing nudges to assess information leakage. On a more positive note, we also highlight the benefits of a previously untested nudge—framing the timing of an opportunity for behavior change as a “fresh start”—as a means of encouraging future-oriented behaviors.

We conduct our research in the domain of retirement savings. It is more important than ever to find effective ways to encourage workers to set aside money for retirement. In 401(k) plans and other defined contribution pension plans—the prevalent form of retirement scheme in the United States today—workers are responsible for the key decisions that determine their long-term financial well-being, including how much to save. Unfortunately, many experts are concerned that savings rates are too low and that a large number of Americans must begin saving more in order to avoid experiencing a drop in their standard of living in retirement (Munnell et al. 2012). Past research has proven that nudges can be a potent tool for increasing savings rates (e.g., Beshears et al. 2013, Hershfield et al. 2011, Karlan et al. in press, Madrian and Shea 2001, Soman and Cheema 2011, Thaler and Benartzi 2004). We present the results of a large field experiment testing the efficacy of two nudges intended to increase retirement savings. Our results highlight both a previously unappreciated risk of information leakage associated with widely-used pre-commitment nudges and the promise of fresh start framing as a new and effective nudge.

A wide range of factors may lead to under-saving, including inattention to the importance of future expenses (Karlan et al. in press), under-appreciation of the frequency of “exceptional” expenses that increase current spending (Sussman and Alter 2012), socioecological cues that emphasize speed over patience (DeVoe et al. 2013), and a dispositional tendency to experience too little pain when spending (Rick et al. 2008). The nudges we selected for tackling under-saving in our field experiment both take aim at another major psychological obstacle to saving: present bias. Because of present bias, short-term sacrifices like spending less that produce long-term benefits like a comfortable retirement can be a significant challenge (Angeletos et al. 2001, Benartzi et al. 2012). As described above, a large literature suggests that pre-commitment (choosing for later as opposed to now) is a nudge that reduces present bias, making it more likely that people will do what is in their long-term best interest (see Milkman et al. 2008 for a

review).¹ In the domain of retirement savings, the benefits of a pre-commitment nudge have not been explicitly tested in isolation, but one influential field study of a program called “Save More Tomorrow” (SMarT) revealed that people save dramatically more for retirement when offered the opportunity to commit to diverting a fraction of their future pay raises to a retirement savings account (Thaler and Benartzi 2004). SMarT’s success in encouraging savings plan contributions has been attributed to two sources. First, SMarT allows people to start saving later rather than now, and as described above, choosing for later is a nudge that should promote future-oriented actions like saving. Second, SMarT prevents savers from experiencing a decrease in their nominal (not inflation-adjusted) take-home pay, since the program only takes savings increases out of pay raises; this feature may contribute to SMarT’s appeal because people find losses relative to a reference point (like a reference paycheck) extremely aversive (Kahneman and Tversky 1979). Importantly, however, the benefits of SMarT have not been tested experimentally, nor have scholars confirmed the presumed sources of the program’s effectiveness. In this paper, we isolate the effect of a nudge offering individuals an opportunity to begin saving later, and we additionally evaluate the benefits of offering an array of different time delays for implementing increased retirement savings contribution rates.

The second type of nudge we test with the goal of increasing retirement savings by reducing the influence of present bias is a fresh start framing nudge. Research on “the fresh start effect” suggests that there are certain recurring points in time when we are particularly motivated to engage in future-oriented behaviors, or in other words, when we are less present-biased. Specifically, we make more future-oriented choices following salient “temporal landmarks” that signal new beginnings (Dai et al. 2014, 2015), including dates such as birthdays and holidays. Recent studies have shown that people search more for the term “diet” on Google, visit the gym more frequently, and create more health-related and health-irrelevant goals following temporal landmarks (Dai et al. 2014). Temporal landmarks that stand between us and a future date make that future date feel more distant (Peetz and Wilson 2013, Tu and Soman 2014). Further, when they arise, temporal landmarks segregate us from our past failures, creating the perception that the “old me” failed, but the “new me” has a clean slate and is thus more capable of taking

¹ For example, making choices that will take effect in the future rather than now leads people to select healthier foods (Milkman et al. 2010, Read and Van Leeuwen 1998) and more educational films (Milkman et al. 2009, Read et al. 1999), donate more to charity (Bremann 2011, Rogers and Bazerman 2008), and discount future cash flows less steeply (Ainslie and Handel 1983).

difficult but beneficial future-oriented actions (Dai et al. 2015). Merely reminding someone about an upcoming temporal landmark in the laboratory (e.g., that March 20th is the first day of spring) increases that individual's interest in pursuing future-oriented behaviors following the upcoming date in question. In this paper, we explore whether offering people the opportunity to begin saving after a future date that is highlighted as a temporal landmark (e.g., “following your next birthday” or “following New Year’s”) increases retirement savings contributions over and above simply offering the opportunity to delay savings increases by an equivalent amount of time.²

In order to test the efficacy of two nudges designed to increase retirement savings—a pre-commitment nudge and a fresh start framing nudge—we conducted a between-subjects field experiment with a sample of 8,682 employees from four major U.S. universities.³ We studied employees' retirement savings contribution rates for nine months following their receipt of a mailing encouraging them to either enroll in a retirement savings plan for the first time or increase their contributions to a plan in which they had already enrolled. The mailings allowed employees to enroll in a retirement savings plan at a pre-selected contribution rate with the contributions allocated to a pre-selected investment vehicle (or increase their savings to a pre-selected contribution rate with the contributions allocated to their existing investment vehicle) by signing and returning a pre-stamped, pre-addressed postcard with a box checked indicating that they wanted to begin saving now (or later, in some experimental conditions). Past research has shown that such simplified enrollment and contribution escalation mailings are an effective means of increasing retirement savings rates (Beshears et al. 2013). We randomly assigned university employees to receive mailings that invited them to begin saving more (a) now (*no delay condition*), (b) now *or* later (*standard delay condition*), or (c) now *or* after a temporal landmark (e.g., after their next birthday, after New Year's; *framed delay condition*). We also

² This nudge may be effective for an additional reason. Since highlighting an upcoming temporal landmark can make people feel more distant from the future (Bartels and Rips 2010, Peetz and Wilson 2013, Tu and Soman 2014), offering people the opportunity to save more after a temporal landmark may lead people to perceive the delay separating them from that future date to be longer, which may make the delayed option more attractive and thus increase employees' willingness to agree to the future savings rate increase.

³ We originally included a fifth university in the experiment. However, this university offers generous employer contributions that are not contingent on employee contributions, and it requires employees to elect dollar contribution amounts instead of contribution rates as a percent of pay. As a result, the mailings used for this university required a different design, and response rates to the mailings at this university were extremely low, making it impossible to perform a meaningful analysis of the effect of different experimental treatments. We therefore drop this university from our analysis.

experimentally varied the time delay offered in the pre-commitment conditions (i.e., the standard delay and framed delay conditions), which ranged from two months to six months. Note that participants in all conditions were asked to mail back our postcard by the same deadline. Thus, participants who chose to increase savings later pre-committed to save more in the future.

Past research on present bias and the fresh start effect suggested that we would observe the largest increase in employees' retirement contributions over the nine-month period following our mailing in the *framed delay condition*, followed by the *standard delay condition*, with employees in the *no delay condition* contributing the least to their retirement savings plans. However, past research on information leakage (McKenzie and Nelson 2003, McKenzie et al. 2006, Sher and McKenzie 2006, Tannenbaum and Ditto 2011, Tannenbaum et al. 2013) suggests that savings in the pre-commitment conditions could be hindered by an unintended implicit message embedded in the mailing. Specifically, offering employees an opportunity to delay savings increases may unintentionally leak the message that saving is not considered an urgent priority. Indeed, we find that relative to the no delay condition, savings rates are lower in the standard delay condition, consistent with this theory. However, the negative effect of offering a delayed option is undone when the delayed option is associated with a fresh start. In a follow-up laboratory experiment, we find evidence that people infer that their employer is less urgently recommending retirement savings if they receive a standard delay mailing than if they receive a no delay mailing. These findings highlight that when building interventions to promote future-oriented behaviors by encouraging take-up at a time delay, policy makers risk perversely conveying that the recommended action is not urgent and can wait. Our research thus underscores the importance of attending to the implicit messages that may be unintentionally leaked by the choice architecture selected when designing nudges and the value of pilot testing. On a more positive note, our findings also highlight the potential benefits of fresh start framing as a new type of nudge capable of encouraging future-oriented behaviors.

The remainder of this paper proceeds as follows. We begin by presenting the design and results of our field experiment. We then present a follow-up study conducted in the laboratory to test the hypothesis that information leakage could explain our finding that offering people the opportunity to increase savings at a delay actually reduced savings rates. Finally, we conclude with a discussion of the implications of our findings.

2. Field Experiment: How Pre-Commitment and Fresh Start Framing Affect Retirement Savings Decisions

2.1. Experimental Methods

Four U.S. universities collaborated with us on our field experiment. To protect the anonymity of our four university partners, we will simply refer to them as Universities A, B, C, and D. Each university began by identifying a retirement savings plan in which they would like to increase their employees' contributions (hereafter referred to as the *targeted plan*). At all universities, mailings were sent out to employees who were not enrolled in the targeted plan and therefore had a contribution rate of zero. At one university (hereafter referred to as University D), mailings were also sent out to employees with a positive contribution rate in the targeted plan who were not contributing at the level necessary to take full advantage of their employer's matching contributions, which were made dollar-for-dollar up to a certain fraction of an employee's salary. One retirement plan record keeper for these universities sent out mailings in early October of 2013 to university employees' homes. The mailings provided employees with an opportunity to either begin saving (for those not enrolled) or increase their savings contributions (for those enrolled but saving at a low rate) by filling out and mailing back a simple form. See Supplementary Materials for the mailing templates.

Employee randomization to experimental conditions was stratified at the birth month level, and employees were randomized into three primary conditions: the *no delay condition*, the *standard delay condition*, and the *framed delay condition*. Employees assigned to the no delay condition were encouraged to sign up to save (or to save more) immediately. Those assigned to the standard delay condition were given the opportunity to sign up to save (or to save more) either immediately or after a time delay (e.g., "in two months") ranging from two to six months. Finally, those in the framed delay condition received a mailing identical to the mailing received by employees in the standard delay condition, except the time delay reference (e.g., "in two months") was replaced by a reference to a temporal landmark with the same time delay (e.g., "following your next birthday," "following Thanksgiving"). The temporal landmarks were either holidays (Thanksgiving, New Year's, Martin Luther King Day, Valentine's Day, and the Spring Equinox) or employees' birthdays.

Since delayed savings opportunities were offered in the five months following our experimental mailing (November 2013-March 2014), only employees whose birthday fell into

this period could be randomized to receive a message offering them the opportunity to begin saving after their next birthday. Thus, prior to randomization, employees were divided into two sub-groups: those with birthdays between November and March (hereafter the *birthday group*), and others (hereafter the *no birthday group*). They were then randomized to conditions as illustrated in Figure 1. Specifically, those with birthdays between November and March were divided evenly among four experimental conditions: the no delay condition, the standard delay condition, and two sub-categories of the framed delay condition—the *birthday-framed delay condition* that offered employees an opportunity to begin saving (or to save more) following their next birthday and the *holiday-framed delay condition* that offered employees an opportunity to begin saving (or to save more) following a future holiday. The length of the delay offered to employees in the birthday group was determined by their birth month. For instance, consider an employee whose birthday is in December. Recall that our mailings went out in early October. If this employee were assigned to the standard delay condition, she would be offered the opportunity to start saving “in three months” (or in January). If she were assigned to the holiday-framed delay condition, she would be offered the opportunity to start saving after New Year’s (or in January). If she were assigned to the birthday-framed delay condition, she would be offered the opportunity to start saving after her next birthday (again, in January).

Those with birthdays between April and October were divided evenly among the no delay condition, the standard delay condition, and the holiday-framed sub-category of the framed delay condition. The delay offered to employees in the no birthday group was randomized to be from two to six months (in the standard delay condition) or from Thanksgiving to the Spring Equinox (in the framed delay condition). Every employee in the framed delay condition was yoked with an employee in the standard delay condition who was offered the opportunity to start saving (or to save more) at the same time delay. For example, an employee who was randomly assigned to have the opportunity to begin saving after New Year’s was yoked with an employee who had the opportunity to begin saving in three months. Notably, past research suggests that New Year’s is a particularly meaningful fresh start opportunity (Dai et al. 2014). Thus, among employees in the framed delay condition, we oversampled assignment to the “after New Year’s” time delay. Correspondingly, we oversampled assignment to the three-month delay in the standard delay condition.

At all four universities, the fund to which employees were encouraged to make contributions was a lifecycle fund. Lifecycle funds provide a diversified portfolio with a mixture of equity, bond, and money market funds tailored to the employee's age. The contribution rate suggested by the mailings was 3% of the employee's pay for all universities except University D, which had a suggested contribution rate of 5%. The suggested rate was 5% for University D because University D matches employees' contributions dollar-for-dollar up to 5% of the employee's base salary.⁴ Detailed information about the targeted plans can be found in Table 1, and information about non-targeted savings plans is available in Appendix A.

2.2. Data

We analyze data provided by our four university partners. These universities pulled a cross-sectional snapshot of information about all eligible employees in August 2013 including information about each employee's: (1) current contributions to the savings plan targeted in our mailings, (2) current contributions to all other non-targeted savings plans, (3) birth date, (4) hire date, (5) termination date, (6) salary, and (7) position (in the form of an indicator for faculty versus staff). We relied on information from this first data pull to conduct our stratified random assignment of employees to experimental conditions. We then received data from our university partners including information on each employee's contributions to the targeted retirement savings plan and all other retirement savings plans as well as their pay for each pay cycle through June 2014.

2.3. Variables and Analysis Strategy

To capture the effect of the different experimental treatments on savings, we created two different outcome variables. It is worth noting that we calculate these variables for each employee *both* for the targeted savings plan *and* for all available savings plans (including the targeted and non-targeted plans).

Average contribution rate. For every employee, we calculate the employee's average contribution rate from November 2013 through June 2014. Specifically, we calculate the total number of dollars the employee contributed to the targeted plan⁵ (or all plans) from November

⁴ The Internal Revenue Service sets maximum annual contribution amounts to retirement plans. For both 2013 and 2014, the limit was \$17,500, and employees 50 years of age or older could contribute an additional \$5,500. At some universities, some employees with more than 15 years of service may have been eligible for additional contributions.

⁵ Though our simplified enrollment mailings encouraged employees to contribute to a pre-selected investment vehicle (i.e., a lifecycle fund), we examine employees' contributions to all funds available in the targeted plan.

2013 through June 2014, divided by the employee's total pay from November 2013 through June 2014. We calculate a savings rate over this time period because November 2013 is the first month during which contribution rate increases triggered by responses to our mailing were implemented, while June 2014 is the latest month for which we received data on employees' contributions and pay.

Increased contribution rate indicator. For every employee, we construct a variable indicating whether or not the employee's May 2014 contribution rate to the targeted plan (or all plans) was higher than the employee's September 2013 contribution rate to the targeted plan (or all plans). We rely on September 2013 and May 2014 for our contribution rate comparison because our mailings were sent in early October 2013 and because April 2014 was the last month during which contribution rate increases triggered by responses to our mailing were implemented. Thus, comparing September 2013 to May 2014 captures all contribution rate increases that could have been the direct result of our mailing.

We use ordinary least squares (OLS) regressions to predict our outcome variables, *average contribution rate* and *increased contribution rate indicator*.⁶ We first rely on the following regression specification to test the differences in average contribution rate across the no delay, standard delay, and framed delay conditions:

$$(1) \quad \text{average contribution rate}_i = \beta_0 + \beta_1 \text{delay indicator}_i + \beta_2 \text{framed delay indicator}_i + \delta' X_i + \varepsilon_i$$

where i indexes an employee. The first predictor variable, the *delay indicator*, indicates whether or not a given employee received a mailing offering any form of delayed savings option. This variable takes a value of one for both employees in the standard delay condition and those in the framed delay condition. Our second predictor variable, the *framed delay indicator*, indicates whether or not a given employee received a mailing offering a delayed option that was associated with a temporal landmark (either a holiday or the employee's next birthday). This variable takes a value of one for employees in the framed delay condition and zero for employees in all other conditions. The coefficient on the *delay indicator* reflects the difference in average contribution rates between the standard delay and no delay conditions, whereas the coefficient on the *framed delay indicator* reflects the difference in average contribution rates between the

⁶ As a robustness check, we also perform logistic regressions when the outcome variable is *increased contribution rate indicator*. See Appendix B.

standard delay and framed delay conditions. X_i is a vector of controls, including gender, age decile, tenure decile, salary decile, faculty status, and birth month. We allow the coefficients on the control variables to vary by university and calculate decile breakpoints separately for each university.⁷ We report heteroskedasticity-robust standard errors (ε_i).

We conduct further analyses to evaluate whether it is more effective to associate a time delay with a future holiday or with an employee's next birthday. As described earlier in Section 2.1 (Experimental Methods), employees in the birthday group (who had birthdays falling between November and March) were randomly assigned to one of four conditions: the no delay condition, the standard delay condition, the birthday-framed delay condition, or the holiday-framed delay condition. Employees in the no birthday group (who had birthdays falling between April and October) were randomly assigned to eleven different conditions: the no delay condition, standard delays of 2-6 months (i.e., five sub-groups of the standard delay condition), and holiday-framed delays of 2-6 months (i.e., five sub-groups of the framed delay condition). As a result, we can only compare the effect of birthday framing with the effect of holiday framing among employees in the birthday group. Consequently, we use the following regression specification to separately estimate the effect of different framings for employees in the birthday group and the effect of different framings and delays for employees in the no birthday group:

$$\begin{aligned}
 (2) \quad \text{average contribution rate}_i = & \gamma_0 + \gamma_1 \text{delay indicator}_i \\
 & + \gamma_2 \text{birthday-framed delay indicator}_i \\
 & + \gamma_3 \text{holiday-framed delay indicator}_i \\
 & + \sum_{k=2}^6 \gamma_{k+2} k\text{-month delay indicator}_i \\
 & + \sum_{k=2}^6 \gamma_{k+7} k\text{-month framed delay indicator}_i \\
 & + \theta' X_i + \zeta_i
 \end{aligned}$$

where i indexes an employee and X_i is the same vector of controls included in (1). The first three predictor variables are used to classify employees in the birthday group based on the experimental condition to which they were randomly assigned. Specifically, the *delay indicator* equals one if a given employee in the birthday group received any of the three delay options (i.e., standard delay, birthday-framed delay, or holiday-framed delay), and it equals zero otherwise.

⁷ As explained earlier, University D sent mailings to employees who were not enrolled in the targeted plan as well as employees who were not contributing sufficiently to obtain the full employer match. We allow the coefficients on the control variables and decile breakpoints to differ between these two groups of employees at University D.

The *birthday-framed delay indicator* equals one if a given employee in the birthday group received a mailing offering her the opportunity to begin saving (or to save more) after her next birthday, and it equals zero otherwise. The *holiday-framed delay indicator* equals one if a given employee in the birthday group received a mailing offering her the opportunity to begin saving (or to save more) after a holiday, and it equals zero otherwise. Note that these first three predictor variables all equal zero for employees in the no birthday group. They allow us to compare the effects of offering a standard time delay and mentioning the delay in relation to an employee's birthday or a holiday for employees in the birthday group. For employees in the no birthday group, the *k-month delay indicator* equals one if a given employee in either the standard delay condition or the framed delay condition was offered the opportunity to begin saving (or to save more) at a (standard or framed) delay of k months, and the *k-month framed delay indicator* equals one if a given employee in the framed delay condition was offered the opportunity to begin saving (or to save more) after a holiday corresponding to a k -month delay, where k includes all integers ranging from 2 to 6; these predictor variables equal zero otherwise. The *k-month delay indicator* variables and *k-month framed delay indicator* variables equal zero for all employees in the birthday group. The *k-month delay indicator* variables allow us to evaluate the impact of offering different standard delays on savings decisions among employees in the no birthday group. The *k-month framed delay indicator* variables allow us to estimate the effect of associating a time delay with a holiday (versus offering an objectively identical standard delay) and to compare the framing effects of different holidays among employees in the no birthday group.⁸

To examine how our interventions affect the likelihood of employees increasing their contribution rates from September 2013 to May 2014, we use the same regression specifications detailed above (in equations (1) and (2)) to predict our second outcome variable of interest: the *increased contribution rate indicator* instead of *average contribution rate*.

⁸ Recall that employees in the no birthday group were *randomly* assigned to have the opportunity to save beginning in k months where k ranged from 2 to 6, while employees in the birthday group received a time delay that was determined by their birth month and thus was not exogenous. Therefore, we can only examine the effects of different lengths of delays and different holiday framings among employees in the no birthday group.

2.4. Results

2.4.1. Employee Characteristics and Balance Checks

Table 2 summarizes the characteristics of the employees in our field experiment. Slightly more than half of the employees are female. The mean age is 43 years, and the mean time since beginning work with the employer (i.e., job tenure) is 9.5 years. The mean salary is nearly \$60,000 annually, and slightly more than 10% of employees in our sample are faculty members. Statistical tests comparing these employee characteristics across our three primary experimental conditions (no delay, standard delay, and framed delay) indicate that the conditions are balanced. The only statistically significant differences were as follows: the mean salary of employees in the no delay condition was less than that of employees in the standard delay condition or the framed delay condition. The no delay condition also had a higher fraction of females compared to the framed delay condition, although this difference is only marginally statistically significant. We control for all of these individual characteristics in our analysis of treatment effects.

2.4.2. The Effect of Control Mailings Relative to No Mailing

Before studying the effect of different versions of our mailing on savings, we analyze the effect of our control condition (the no delay mailing) relative to no mailing at all. That is, we examine whether sending a no delay mailing increased retirement savings. Mailings were sent to employees' homes in October 2013, and employees who responded to the no delay mailing had their contribution rate increases implemented over the course of November 2013. To capture the overall impact of this mailing, we examine employees who received the no delay mailing and compare their contribution rates in the targeted plans in December 2013 to their contribution rates in the targeted plans in October 2013 (two months earlier). We observe that slightly more than 6% of employees in the no delay condition had a higher contribution rate in December 2013 than in October 2013. As a benchmark, we study the same group of employees but compare their contribution rates in the targeted plans in October 2013 to their contribution rates in the targeted plans in August 2013 (two months prior to the date when our mailings were sent). Since targeted employees were identified by each university in August 2013, increases in contribution rates over the two-month period between August 2013 and October 2013 (the time of our mailing) reflect how contribution rates might have changed over the two-month period between October 2013 and December 2013 if our mail campaign were not implemented. Less than 4% of employees had a higher contribution rate in October 2013 than in August 2013. The difference

between the frequency of August-to-October contribution rate increases and the frequency of October-to-December contribution rate increases is highly statistically significant ($p < 0.001$). While we cannot rule out seasonal effects as an alternative explanation for this finding, the evidence suggesting that the mailing had a positive overall impact on savings is consistent with previous work on simplified contribution increase mechanisms (Beshears et al. 2013).

2.4.3. The Effect of Offering a Delayed Option

We begin by focusing on how offering a delayed option without fresh start framing affects savings decisions; then we turn to the effects of layering fresh start framing on top of a standard delay offering. Table 3 reports the results from OLS regressions where the dependent variable is either the average contribution rate over our nine-month follow-up period or the increased contribution rate indicator variable, and we examine both dependent variables for targeted savings plans as well as for all savings plans. Contrary to past research on the benefits of pre-commitment for increasing engagement in future-oriented behaviors, but consistent with an information leakage account whereby offering a time-delayed enrollment option signals that saving is not an urgent priority, Model 1 indicates that offering an unframed, standard delay option *decreases* average contribution rates for targeted plans by 14 basis points of pay ($p < 0.05$). Model 2 indicates an even stronger negative effect of offering a standard delay on savings for all plans: average contribution rates for all plans are lower in the standard delay condition compared to the no delay condition by 27 basis points of pay ($p < 0.01$).

Based on past research on pre-commitment, the impact of offering a delayed option on average contribution rates can be broken down into two different effects. First, the availability of a delayed option may cause an employee to agree to increase her contribution rate when that employee would not have otherwise agreed to such an increase. This first effect combats present bias and should lead to an increase in savings. Second, the availability of a delayed option may decrease savings because an employee who would have otherwise agreed to an immediate contribution rate increase may instead opt for a delayed increase, shortening the length of time during which the employee has a higher contribution rate. If the first effect outweighs the second effect, offering a delayed option should generate a positive impact on average contribution rates. Our finding that the standard delay condition has lower average contribution rates than the no delay condition (Models 1 and 2 in Table 3) indicates that the second effect actually dominates in our context during our observation period. However, it is theoretically possible that a longer time

horizon for measuring average contribution rates would reverse this conclusion, as the greater number of people signing up to increase savings eventually outweighs the temporarily low contribution rates induced by the delayed option. Models 3 and 4 in Table 3, where the dependent measure is the increased contribution rate indicator, suggest that this possibility is unlikely. If anything, employees in the standard delay condition are *less* likely than employees in the no delay condition to have a higher contribution rate in May 2014 than in September 2013, although this effect is not statistically significant for targeted plans or all plans (both p 's > 0.17). In other words, contrary to past research on pre-commitment but consistent with an unintentional information leakage account, we observe no evidence that offering a delayed option attracts more employees to sign up to increase savings.

Table 4 replicates the finding that among employees with birthdays falling between November and March, offering a standard delay option lowers both average contribution rates and the likelihood of employees increasing their contribution rates between September 2013 and May 2014, although the differences are not statistically significant (all p 's > 0.11), in large part due to the smaller sample size. Among employees with birthdays falling between April and October, average contribution rates and the likelihood of exhibiting a higher contribution rate in May 2014 versus September 2013 are generally lower for the five standard delay conditions compared to the no delay condition, regardless of the length of delay.

2.4.4. The Effect of Fresh Start Framing

Next, we turn to the effect of fresh start framing on retirement savings decisions, holding constant the offering of a time delay. Model 1 in Table 3 indicates that relative to the standard delay condition, the framed delay treatment marginally significantly increases average contribution rates for targeted plans by 10 basis points of pay ($p < 0.10$). Model 2 in Table 3 examines differences in average contribution rates for all plans and shows that the framed delay condition has (directionally) higher average contribution rates than the standard delay condition ($p = 0.19$). When we focus on the increased contribution rate indicator as the dependent variable in Models 3 and 4, we see that employees in the framed delay condition are directionally more likely than employees in the standard delay condition to have a higher contribution rate in May 2014 relative to September 2013, although the differences are not statistically significant for either targeted plans ($p = 0.60$) or for all plans ($p = 0.43$).

To evaluate what drives the positive framing effect on average contribution rates, we separately test the effects of birthday framing and holiday framing using regression specification (2) described above. First, we focus on employees whose birthdays fell between November and March. The coefficient estimates on the birthday-framed delay indicator and holiday-framed delay indicator variables in Models 1-4 in Table 4 suggest that the positive effect of a framed delay on savings is concentrated in the birthday-framed delay condition. Specifically, Model 1 shows that compared with the standard delay condition, associating a time delay with an employee's next birthday increases average contribution rates for targeted plans by 22 basis points of pay ($p < 0.05$). Model 2 suggests that the effect of birthday framing on savings becomes stronger for all plans, with birthday framing increasing average contribution rates for all plans by 33 basis points of pay ($p < 0.05$). Further, Models 3 and 4 indicate that the birthday-framed delay condition also has a higher likelihood of exhibiting a higher contribution rate in May 2014 versus September 2013 than the standard delay condition ($p < 0.10$ for targeted plans and $p < 0.05$ for all plans). As for the effects of holiday framing, Models 1-4 altogether show that average contribution rates and the likelihood of exhibiting a higher contribution rate in May 2014 versus September 2013 are higher in the holiday-framed delay condition than in the standard delay condition, but the differences are not statistically significant (all p 's > 0.20). Figure 2 displays the predicted values of our outcome variables (the average contribution rate and the increased contribution rate indicator) in each condition for both targeted plans and all plans among employees born between November and March. For the purposes of constructing this figure, all control variables are fixed at their means.

The coefficient estimates on the *k-month framed delay indicator* variables (where $k = 2, 3, 4, 5$, and 6) in Table 4 compare different holidays to their corresponding standard delays among employees in the no birthday group. The only holiday that is remarkable is the Spring Equinox; among all of the framed delay groups, the Spring Equinox leads to the highest average contribution rate and the highest likelihood of increasing contribution rates between September 2013 and May 2014 relative to its corresponding, six-month standard delay.⁹ However, this finding was not predicted ex ante and may well be the result of sampling noise.

⁹ Compared to a six-month standard delay, framing a six-month delay in relation to the Spring Equinox increases average contribution rates ($p < 0.05$ for targeted plans and $p < 0.10$ for all plans).

Finally, we assess the combined effects of offering a delayed option and framing the delay in relation to a temporal landmark on retirement savings decisions. For each model in Table 3, the sum of the coefficient on the delay indicator and the coefficient on the framed delay option captures the difference between the framed delay and no delay conditions. Models 1-4 together show that (1) average contribution rates and (2) the likelihood of exhibiting a higher contribution rate in May 2014 than in September 2013 are insignificantly different in the framed delay condition from in the no delay condition (all p 's > 0.11). These results suggest that associating a time delay with a future temporal landmark mitigates the negative effects of offering a delay option on savings.

2.4.5. Robustness Checks

Our results are robust if we (i) drop employees whose total annual contributions to all of the non-targeted plans were on track, as of September 2013, to exceed the IRS limit of \$17,500 for calendar year 2013; (ii) drop employees who have missing data on salary or contributions; (iii) round our impute contribution rates in different ways (e.g., rounding at 1% or 10 basis points of pay); or (iv) use a logistic regression rather than an OLS model to predict the increased contribution rate indicator, though the statistical significance of a small number of predictor variables changes slightly in some cases. Detailed descriptions of our robustness checks are reported in Appendix B.

2.5. Discussion

Our field experiment yielded two primary findings. First, it demonstrated that relative to offering people the option to save more at a standard time delay (e.g., “in two months”), associating a delayed savings option with an upcoming temporal landmark—particularly with an employee’s next birthday—can increase average retirement savings contribution rates. Second, contrary to past research on the benefits of pre-commitment, offering a standard delay option does not lead more people to sign up to increase their savings; if anything, (insignificantly) fewer people sign up to increase their savings. In fact, the presence of a standard delay option decreases overall retirement wealth because some people select the delayed option and thus save over a shorter time horizon than they would have otherwise. The negative effect of offering a standard delay option on savings seemingly contradicts the well-established success of the SMarT program (Thaler and Benartzi 2004) as well as past research showing that choosing for later (as opposed to now) increases people’s willingness to pursue their long-term interests (e.g.,

Milkman et al. 2010). However, it is important to note that our experimental design differs in a few important ways from SMarT's design and the paradigm that past research has used to examine the benefits of pre-commitment. Specifically, employees in the standard delay condition of our field experiment were *simultaneously* offered both the option to save more now and the option to save more later. In contrast, the classic implementation of the SMarT program offered the options *sequentially*—with the option to save more now offered first and the option to save more later offered only to employees who declined the initial offer—and the paradigm often used in previous work has involved a comparison between take-up when *only* the opportunity to engage *now* in the future-oriented activity is offered and take-up when *only* the opportunity to engage *later* in the future-oriented activity is offered. In our field experiment, when the immediate enrollment option and the delayed enrollment option were presented at the same time, employees may have inferred from the juxtaposition of the two options that the human resources (HR) department at their university did not urgently recommend retirement savings. Such an inference about the HR department's recommendation (or “information leakage”) might make employees less likely to sign up for the savings program than if they were only provided with the immediate enrollment option. We conducted a laboratory experiment to test this possibility.

3. Laboratory Experiment: Pre-Commitment Nudges and Information Leakage

In this laboratory experiment, we test the hypothesis that people would infer their employer is less urgently recommending retirement savings if they received a standard delay mailing than if they received a no delay mailing. We also compare both the standard delay mailing and the no delay mailing to a third mailing design created to resemble the first implementation of the SMarT program's pre-commitment component (Thaler and Benartzi 2004), which initially encouraged employees to sign up to save immediately and only offered the opportunity to save at a time delay if they declined the initial offer to save right away. We hypothesize that the SMarT design avoids the information leakage pitfall of the standard delay mailing from our field experiment and conveys that an employer is urgently recommending retirement savings (because of the repeated efforts to encourage sign-ups) while simultaneously capitalizing on the psychological benefits of pre-commitment.

3.1. Method

We recruited participants through Amazon's Mechanical Turk (an online labor market) to take a short survey. The 582 participants ($M_{\text{age}} = 30.9$, $SD_{\text{age}} = 9.6$; 58% male, 42% female) who

met the following criteria were included in our study: they reported a unique Amazon MTurk ID and successfully proceeded past comprehension check screening questions asked at our study's outset (following MTurk research best practices outlined by Mason and Suri 2012).

All 582 participants were asked to imagine that the human resources (HR) department at a company called "Company X" planned to send its employees mailings about the company's retirement savings program. They were asked to imagine that an outside consultant had suggested two different messaging strategies to Company X and were then shown visual depictions of those two messaging strategies. The two strategies participants reviewed were randomly selected from three possible strategies, depending on the experimental condition. The three messaging strategies examined in our experiment differed only subtly. The first messaging strategy was a *no delay mailing* message. It depicted a simplified version of the *no delay mailing* from our field experiment, adapted such that all references to universities and their specific retirement savings plans were replaced with references to Company X and its hypothetical retirement savings program. This *no delay mailing* message encouraged employees to sign up for Company X's retirement savings plan immediately. The second messaging strategy in our set was a *standard delay mailing* message. It depicted a simplified version of the *standard delay mailing* from our field experiment, again adapted so that all references to universities and their specific retirement savings plans were replaced with references to Company X and its hypothetical retirement savings program. The *standard delay mailing* message offered employees two options: the option to start contributing to Company X's retirement savings program immediately and the option to start contributing in six months. The third and final messaging strategy in our set was a *two stage delay mailing* message consisting of two different mailings. The first (stage 1) mailing was identical to the *no delay mailing* message, inviting employees to enroll in Company X's retirement savings program immediately. However, the second (stage 2) mailing was sent if and only if an employee did not reply to the initial *no delay mailing* quickly. In this case, the HR department would send the employee a follow-up mailing offering her the opportunity to enroll in the savings program in six months. This second stage mailing was adapted from the *standard delay mailing* message with two changes: (a) it explained to employees that they received this mailing because they did not respond to the first stage mailing, and (b) it only retained the option to enroll at a six-month delay. This messaging strategy was designed to resemble the first implementation of the SMarT program described by

Thaler and Benartzi (2004), which initially encouraged employees to sign up to save immediately and only offered employees the opportunity to save at a time delay if they declined the initial offer to save right away.

Participants were randomly assigned to one of three experimental conditions, which differed only in which of the three pairs of messaging strategies they were asked to review: (a) the *standard delay mailing* message versus the *no delay mailing* message, (b) the *no delay mailing* message versus the *two stage delay mailing* message, or (c) the *standard delay mailing* message versus the *two stage delay mailing* message. For each pair of messaging strategies—labeled “Mailing A” and “Mailing B” consistently throughout the survey—we randomized and counterbalanced the mailing presentation order. After participants read the two messaging strategies corresponding to their experimental condition, we asked them a series of comprehension check questions to assess their understanding of (a) what options employees would have if Company X chose a given messaging strategy and (b) how an employee’s total amount contributed over the next eight months to Company X’s retirement savings plan would change if she chose to enroll in the plan at a time delay (as opposed to now). Only those participants who passed our comprehension check questions went on to complete our survey and comprised our actual study sample. Participants who failed our comprehension check questions exited our survey.

Next, participants were asked two key questions about the two mailings that they had been asked to review. First, participants were asked to rate how the two mailings differed (if at all) in the implicit recommendation they conveyed from the HR department regarding employees’ retirement savings plan enrollment decision. Specifically, we asked participants: “Which of the two mailings more strongly conveys that the human resources staff urgently recommends that employees enroll in the retirement savings program?” Participants were required to answer this question on a 1-7 response scale with the following anchors: (1) = “Mailing A far more strongly conveys this recommendation,” (4) = “Mailings A and B convey this recommendation with equal strength,” and (7) = “Mailing B far more strongly conveys this recommendation.” We adapted this question from past research on information leakage (McKenzie et al. 2006). Next, participants were asked to predict the relative effects of the mailings on employees’ retirement savings contributions over the next eight months. Specifically, we asked participants: “Which of the two mailings do you predict would lead

employees to contribute a greater total amount of money to the retirement savings program (say, over the coming 8 months)?” This was a multiple choice question with three possible responses: (1) “Sending Mailing A would lead to higher total contributions to the retirement savings program”; (2) “Sending either Mailing A or B would lead to identical total contributions to the retirement savings program”; or (3) “Sending Mailing B would lead to higher total contributions to the retirement savings program.”

Finally, participants were asked their age and gender. See Supplementary Materials for our complete study materials.

3.2. Results

We will present three sets of results, one set for each of our three experimental conditions. For ease of analysis, in all figures and calculations, we will adopt a consistent rule regarding which mailing we treat as Mailing A versus Mailing B for the purposes of interpreting the response scales associated with our key dependent variables.¹⁰ Specifically, we treat the *standard delay mailing* as Mailing A and the *no delay mailing* as Mailing B when these two mailings are in a paired comparison; we treat the *no delay mailing* as Mailing A and the *two stage delay mailing* as Mailing B when they are compared; and we treat the *standard delay mailing* as Mailing A and the *two stage delay mailing* as Mailing B when they are compared. For each paired comparison of mailings, Figure 3 presents the mean response to our measure of the relative implicit recommendation about the urgency of enrolling in the retirement savings program conveyed by Mailing A versus Mailing B, and Figure 4 presents the mean response to our measure of the relative effects of Mailings A and B on employees’ savings contributions in the coming eight months. To test whether participants view Mailing B as conveying a significantly stronger or weaker recommendation to save than Mailing A, we conduct two-sided one-sample student t-tests to compare the mean response for the first dependent variable in each experimental condition with four, the mid-point of our seven-point scale indicating that the two mailings conveyed the same level of urgency. Similarly, to test whether participants expect Mailing B to lead to significantly different savings contributions than Mailing A, we conduct

¹⁰ We do this by simply reverse coding the response scales for our two key dependent variables whenever the order of the mailings in the survey differed from the order assumed by our adopted convention. For example, our convention is to refer to the *standard delay mailing* as Mailing A when it is paired with the *no delay mailing* (which is then referred to as Mailing B). Whenever these two mailings were paired but the *standard delay mailing* was in fact labeled Mailing B in the survey, we reverse code our response scales.

two-sided one-sample student t-tests to compare the mean response for the second dependent variable in each experimental condition with two, the middle option indicating that the two mailings should lead to no differences in contributions to the retirement savings program.¹¹

As depicted in Figure 3-1 and consistent with our hypothesis, we see that participants view the *no delay mailing* as conveying a significantly stronger recommendation to save than the *standard delay mailing* ($M = 5.41$; $t(207) = 10.69$; $p < 0.001$). Further, as depicted in Figure 4-1, we see that participants expect the *no delay mailing* to lead to significantly more contributions to employees' retirement savings accounts than the *standard delay mailing* in the eight months following the mailing ($M = 2.34$; $t(207) = 5.55$; $p < 0.001$).

As depicted in Figure 3-2 and consistent with our prediction and with the success of the SMarT program, we see that participants view the *two stage delay mailing* as conveying a significantly stronger recommendation to save than the *no delay mailing* ($M = 4.56$; $t(180) = 4.57$; $p < 0.001$). Further, as depicted in Figure 4-2 and consistent with SMarT's success, we see that participants expect the *two stage delay mailing* to lead to significantly more contributions to employees' retirement savings accounts than the *no delay mailing* in the eight months following the mailing ($M = 2.29$; $t(180) = 4.88$; $p < 0.001$).

Finally, as depicted in Figure 3-3, we see that participants view the *two stage delay mailing* as conveying a significantly stronger recommendation to save than the *standard delay mailing* ($M = 5.17$; $t(192) = 8.42$; $p < 0.001$). This helps to explain the success of the SMarT program but failure of the *standard delay mailing* in our field experiment. Further, as depicted in Figure 4-3 and again helping to explain the different results of SMarT and our *standard delay mailing* in the field, we see that participants expect the *two stage delay mailing* to lead to significantly more contributions to employees' retirement savings accounts than the *standard delay mailing* in the eight months following the mailing ($M = 2.17$; $t(192) = 2.57$; $p = 0.01$).¹²

¹¹ For all of the analyses reported below, our results are robust when we analyze our data non-parametrically using a Wilcoxon signed-rank test (all p 's < 0.02).

¹² Another way to provide employees with a pre-commitment option is to only offer them the option to save more at a time delay. We expect that such a *delay only mailing* should “leak” a weaker recommendation to save than our *standard delay mailing* because our *standard delay mailing* at least presents employees with an option to save immediately. Following this logic, a *delay only mailing* should also convey a weaker recommendation to save than the *no delay mailing* and the *two stage delay mailing*. To test these predictions and to provide further insight into information leakage in the context of pre-commitment nudges, we recruited a non-overlapping sample of 611 participants from Amazon's Mechanical Turk to participate in a separate study. We randomly assigned them to compare a *delay only mailing*—which encouraged participants to pre-commit to increasing their savings in six

4. Conclusion and Discussion

Despite the suggestion from previous research that having the option to delay the implementation of a contribution rate increase leads to higher savings, our field experiment indicates that offering an option to delay retirement savings plan enrollment leads to less retirement wealth accumulation. A follow-up laboratory experiment indicates that when a nudge allows people to pre-commit to engaging in a good behavior “later,” it leaks the message that the target behavior is not an urgent priority, which may explain why this nudge backfired in our field experiment. Given the growing interest in using nudges to shift individual decisions in desirable directions, it is critically important to recognize that nudges can leak unintended information about the implicit recommendations of their designers. Our paired field and laboratory experiments highlight when and why a widely-used nudge—encouraging pre-commitment—can backfire due to information leakage. Happily, we also find a solution to this problem: a SMarT-style implementation of pre-commitment—whereby people are first invited to save more now and are only encouraged to pre-commit to saving later if they decline to save now—conveys that saving is an urgent priority.

Our field experiment also highlights the benefits of a previously untested nudge—framing the timing of a future opportunity for behavior change as a fresh start—as a means of encouraging future-oriented behaviors. We find that encouraging employees to increase their savings either now or following their next birthday is a successful way to boost savings relative to offering identical savings options with no mention of a fresh start temporal landmark. Our findings extend past laboratory research demonstrating the efficacy of fresh start framing nudges (Dai et al. 2015) and suggest that similar nudges may be promising in other domains. However, it is important to note that because the benefits of fresh start framing were driven by highlighting birthdays in this study (rather than the full range of temporal landmarks tested), it may not be the fresh start, per se, that propelled this nudge’s efficacy. One alternative explanation is the possibility that mentioning an upcoming birthday increased the salience of aging, which may have made saving for retirement more appealing (Hershfield et al. 2011).

months—with one of the three mailings included in our main laboratory experiment. As hypothesized, the *no delay*, *standard delay*, and *two stage delay mailings* were all rated as conveying stronger recommendations to save and were all expected to lead to greater total savings contributions than the *delay only mailing* (all p ’s < 0.001).

Our findings in this paper highlight the critical importance of (1) attending to the implicit messages that may be unintentionally leaked by policy makers through the nudges they design and (2) pilot testing interventions to address this concern. They also point to the untapped potential of fresh start framing nudges. Future field studies examining the efficacy of fresh start framing nudges (and of simply framing a given day as a fresh start without presenting delayed options) in other settings would be extremely valuable. A greater focus in future research on the implicit messages conveyed by different nudge implementations would also be tremendously useful to scholars and policymakers alike.

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Table 1. Descriptions of Targeted Plans

University	Eligibility	Employer Contributions
A	All employees on the University's payroll with FICA deductions	None
B	All employees whose annual contribution limit to the targeted plan is at least \$200	None
C	All paid employees OR students with a stipend	None
D	<p><u>Eligibility for Employee Contributions</u></p> <p>i) Regular full-time staff (with monthly or weekly pay cycles) OR ii) Full-time faculty and academic support staff in a benefits-eligible title OR iii) Limited-service staff scheduled to work at least 35 hours per week for a minimum of 9 months per year (with monthly or weekly pay cycles)</p> <p><u>Eligibility for Employer Contributions</u></p> <p>All employees who are eligible for employee contributions (described above), are age 21 or older, and have at least one year of prior service</p>	<p><u>Automatic Employer Contribution Rates (Regardless of Whether the Employee Contributes)</u></p> <p>i) 1.5% (employee age < 30) ii) 3% (employee age 30 - 39) iii) 4% (employee age ≥ 40)</p> <p><u>Matched Employer Contributions</u></p> <p>Dollar-for-dollar match on employee contributions up to 5% of employee's salary</p>

Table 2. Summary Statistics by Condition. This table summarizes key control variables used in our analyses by experimental condition. The last three columns show p-values from statistical tests comparing pairs of conditions.

	No Delay	Standard Delay	Framed Delay	No Delay vs. Standard Delay	No Delay vs. Framed Delay	Standard Delay vs. Framed Delay
				<i>p-value</i>		
Female	52.65%	51.85%	50.29%	0.56	0.07	0.23
Age (years)	43.20 (12.32)	43.00 (11.77)	43.44 (11.93)	0.55	0.45	0.15
Tenure (years)	9.51 (9.14)	9.54 (8.93)	9.52 (9.05)	0.91	0.97	0.93
Baseline Salary (\$USD)	56,505.19 (35,234.21)	58,505.26 (36,111.88)	59,509.52 (39,849.63)	0.04	0.002	0.31
Faculty	11.62%	12.75%	12.77%	0.21	0.18	0.99

Table 3. The Effect of Offering a Delayed Option and the Effect of Associating the Delay with a Temporal Landmark. This table reports the results of OLS regressions where the dependent variable is either an employee's average contribution rate during the experimental period (November 2013-June 2014; Models 1 and 2) or an indicator variable for having a higher contribution rate in May 2014 than in September 2013 (Models 3 and 4). The same regression specifications are used for targeted plans and for all plans available to employees. All regressions include a constant, and all controls are interacted with an indicator for an employee's university.

Dependent variable	Average contribution rate		Increased contribution rate indicator	
	Model 1: Targeted plans	Model 2: All plans	Model 3: Targeted plans	Model 4: All plans
Delay indicator	-0.139 ** (0.070)	-0.270 *** (0.099)	-0.011 (0.009)	-0.014 (0.010)
Framed delay indicator	0.101 * (0.061)	0.113 (0.086)	0.005 (0.009)	0.007 (0.010)
<i>Control variables</i>				
university x female	Yes	Yes	Yes	Yes
university x age decile	Yes	Yes	Yes	Yes
university x tenure decile	Yes	Yes	Yes	Yes
university x salary decile	Yes	Yes	Yes	Yes
university x faculty status	Yes	Yes	Yes	Yes
university x birth month	Yes	Yes	Yes	Yes
R-squared	0.09	0.52	0.06	0.09
Observations	8,682	8,682	8,682	8,682

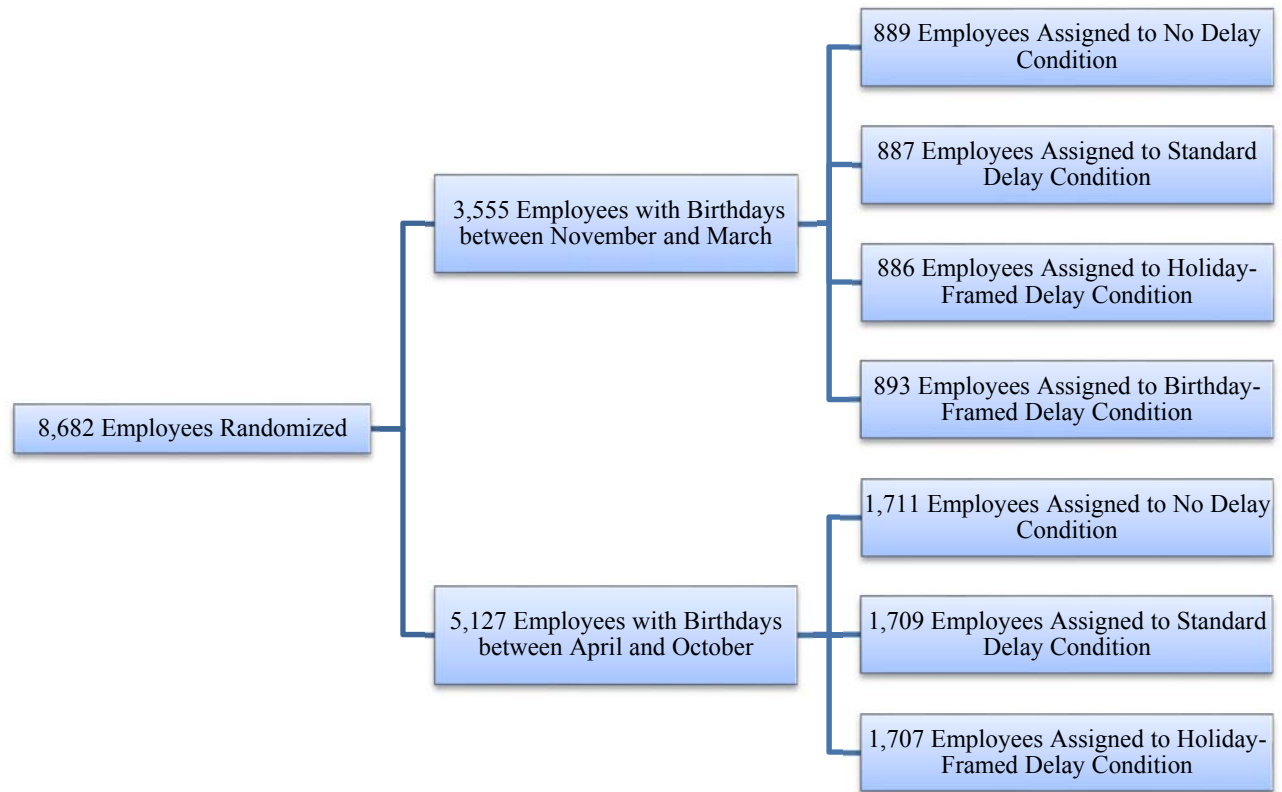
Standard errors robust to heteroskedasticity in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4. The Effect of Different Lengths of Standard Delays and the Effect of Different Framed Delays. This table reports the results of OLS regressions where the dependent variable is either an employee's average contribution rate during the experimental period (November 2013-June 2014; Models 1 and 2) or an indicator variable for having a higher contribution rate in May 2014 than in September 2013 (Models 3 and 4). The same regression specifications are used for targeted plans and for all plans available to employees. All regressions include a constant, and all controls are interacted with an indicator for an employee's university.

Dependent variable	Average contribution rates		Increased contribution rate indicator	
	Model 1: Targeted plans	Model 2: All plans	Model 3: Targeted plans	Model 4: All plans
<i>Born between November and March</i>				
Delay indicator	-0.098 (0.124)	-0.229 (0.172)	-0.012 (0.016)	-0.027 (0.017)
Birthday-framed delay indicator	0.220** (0.111)	0.326** (0.160)	0.028* (0.016)	0.040** (0.017)
Holiday-framed delay indicator	0.036 (0.098)	0.190 (0.148)	0.005 (0.016)	0.017 (0.017)
<i>Born between April and October</i>				
2-month delay indicator	-0.214** (0.108)	-0.463** (0.185)	-0.021 (0.021)	-0.009 (0.023)
3-month delay indicator	-0.172 (0.105)	-0.395*** (0.152)	-0.008 (0.016)	0.004 (0.017)
4-month delay indicator	-0.153 (0.157)	-0.160 (0.241)	-0.022 (0.020)	-0.023 (0.022)
5-month delay indicator	-0.030 (0.149)	0.106 (0.223)	-0.001 (0.021)	-0.007 (0.023)
6-month delay indicator	-0.228* (0.129)	-0.445** (0.173)	-0.004 (0.020)	-0.009 (0.022)
2-month framed delay indicator (Delay framed as post Thanksgiving)	0.140 (0.201)	0.037 (0.275)	-0.004 (0.027)	-0.005 (0.030)
3-month framed delay indicator (Delay framed as post New Year's)	-0.001 (0.117)	0.009 (0.171)	-0.014 (0.019)	-0.032 (0.021)
4-month framed delay indicator (Delay framed as post Martin Luther King Day)	-0.055 (0.168)	-0.248 (0.268)	-0.019 (0.026)	-0.012 (0.028)
5-month framed delay indicator (Delay framed as post Valentine's Day)	-0.253 (0.159)	-0.487* (0.259)	-0.004 (0.029)	0.013 (0.031)
6-month framed delay indicator (Delay framed as post Spring Equinox)	0.622** (0.313)	0.652* (0.354)	0.028 (0.029)	0.024 (0.030)
<i>Control variables</i>				
university x female	Yes	Yes	Yes	Yes
university x age decile	Yes	Yes	Yes	Yes
university x tenure decile	Yes	Yes	Yes	Yes
university x salary decile	Yes	Yes	Yes	Yes
university x faculty status	Yes	Yes	Yes	Yes
university x birth month	Yes	Yes	Yes	Yes
R-squared	0.09	0.52	0.06	0.09
Observations	8,682	8,682	8,682	8,682

Standard errors robust to heteroskedasticity in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure 1. Study Flow



Note: 42 randomized employees were not included in this figure or in the analysis because they did not have data collected, were terminated before the baseline data collection, or had conflicting dates of birth.

Figure 2. The Effects of Different Framings of the Delayed Option. Figure 2 shows the predicted average contribution rate (Panels A and B) and the regression-predicted likelihood of increasing contribution rates between September 2013 and May 2014 (Panels C and D) for an average employee whose birth month is between November and March based on Table 4.

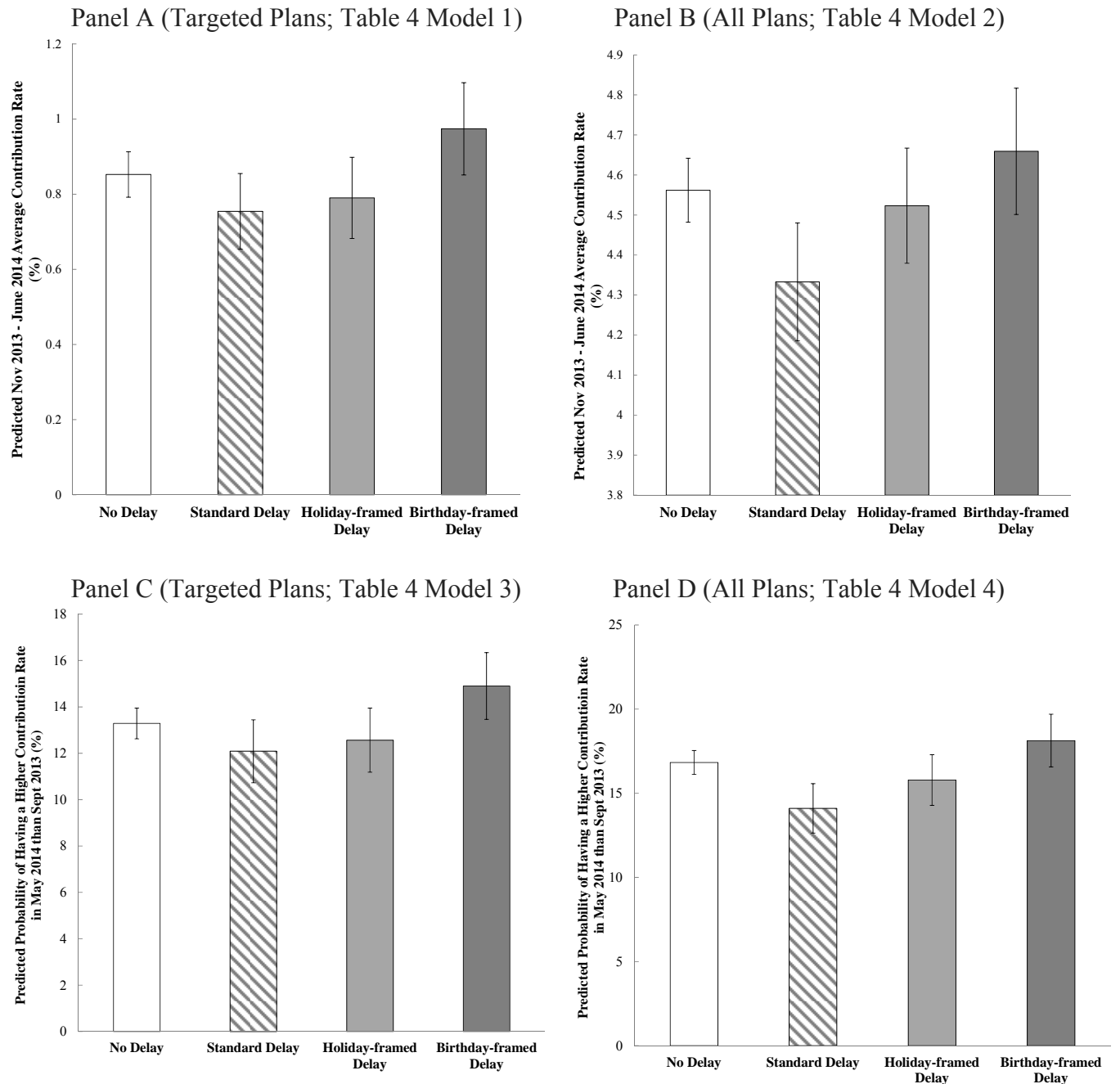
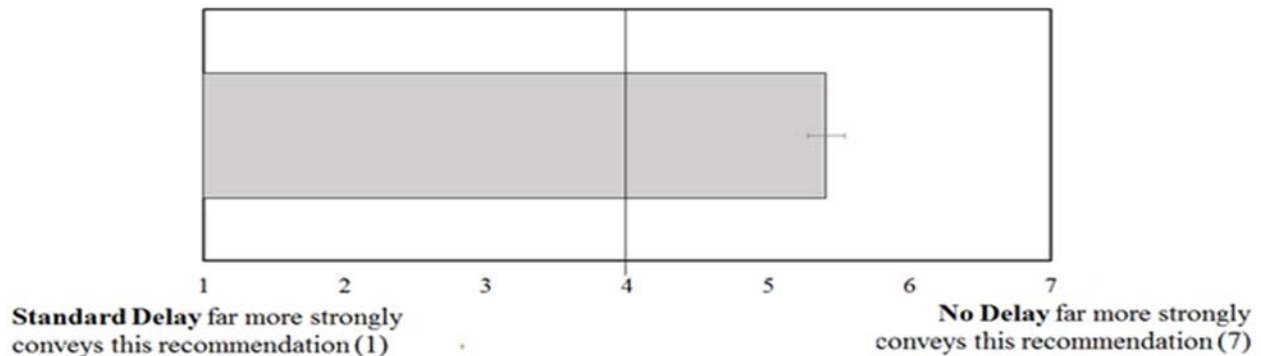
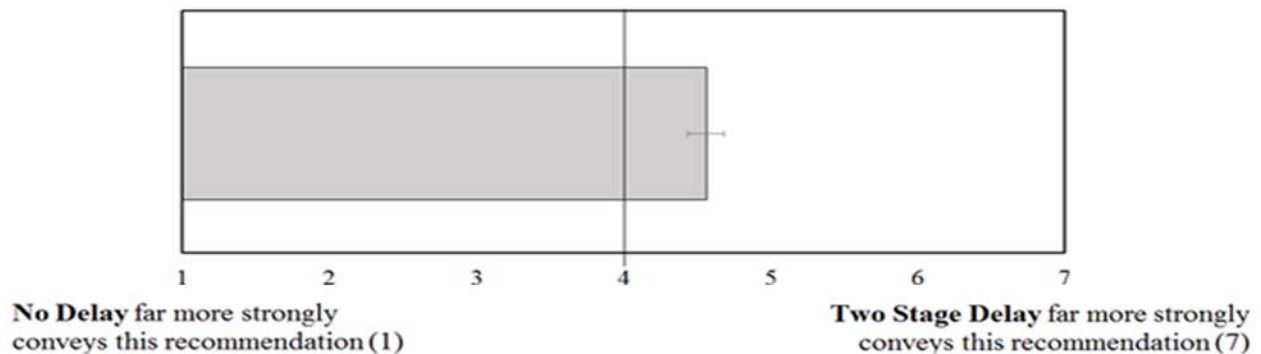


Figure 3. The Effects of Different Types of Delays on Inferences about the Human Resources (HR) Department's Recommendation to Save (Laboratory Experiment). Figure 3 depicts participants' inferences about which of two mailings in a paired comparison more strongly conveyed that Company X's HR staff urgently recommends that employees enroll in a retirement savings program.

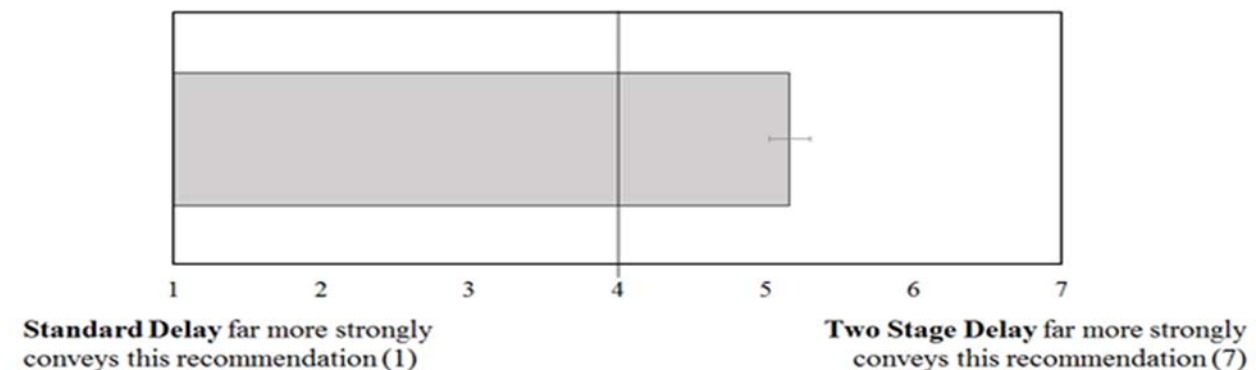
3.1 Participants inferred that the *no delay mailing* conveys a stronger recommendation to save than the *standard delay mailing*



3.2 Participants inferred that the *two stage delay mailing* conveys a stronger recommendation to save than the *no delay mailing*.



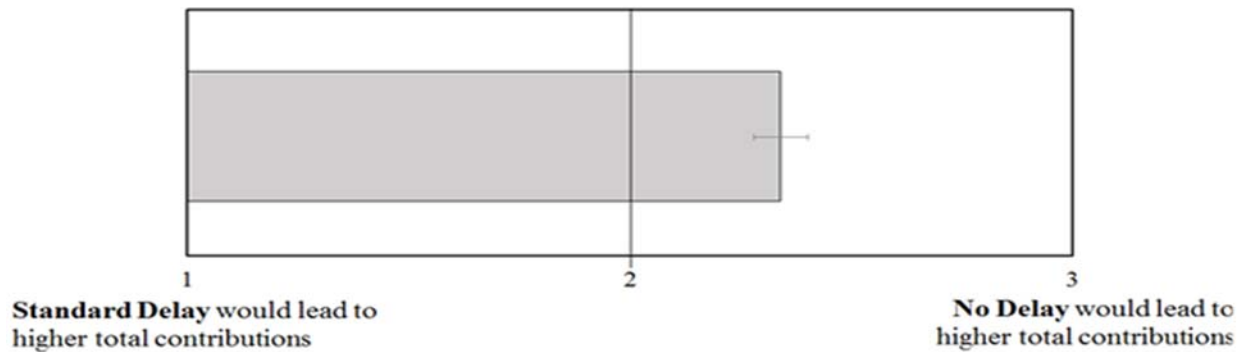
3.3 Participants inferred that the *two stage delay mailing* conveys a stronger recommendation to save than the *standard delay mailing*.



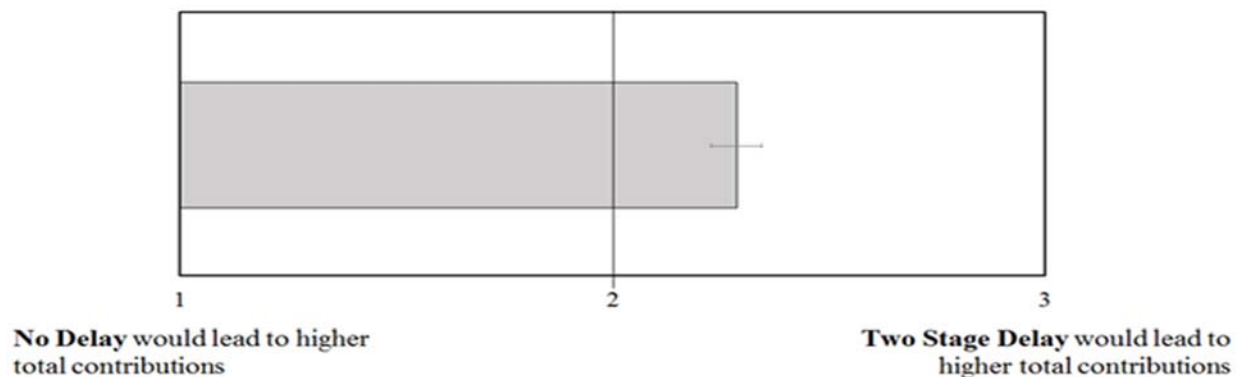
Error bars represent +/- 1 standard error

Figure 4. The Effects of Different Types of Delays on Predicted Contributions to a Savings Program (Laboratory Experiment). Figure 4 depicts participants' predictions about which of two mailings in a paired comparison would lead employees to contribute a greater total amount of money to Company X's retirement savings program (e.g., over the coming eight months).

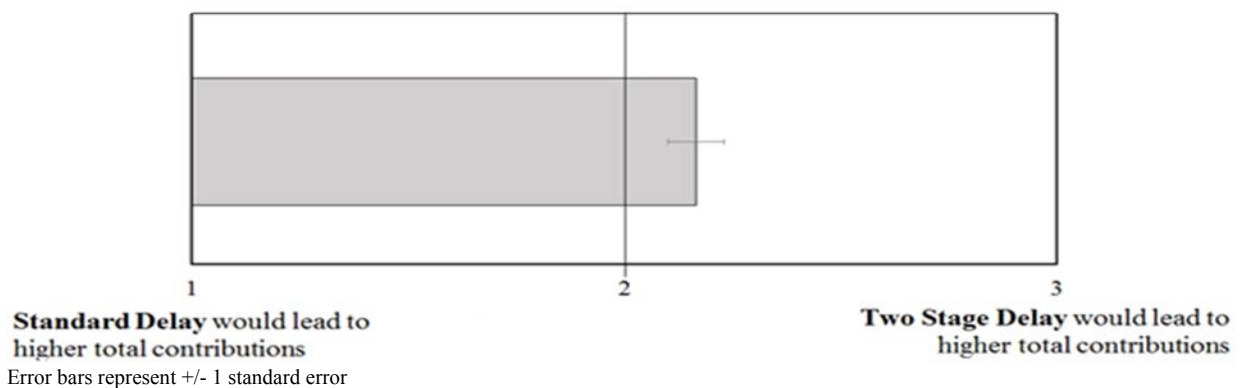
4.1 Participants predicted that the *no delay mailing* would lead to higher total contributions than the *standard delay mailing*



4.2 Participants predicted that the *two stage delay mailing* would lead to higher total contributions than the *no delay mailing*.



4.3 Participants predicted that the *two stage delay mailing* would lead to higher total contributions than the *standard delay mailing*.



Appendix A. Descriptions of Non-Targeted Plans

University	Plan	Eligibility	Employee Contributions	Employer Contributions	Automatic Enrollment
A	<i>Plan 1</i>	Determined based on employee's position and scheduled hours of service	None	The University pays the full cost by contributing 10% of the employee's base pay. The base pay limit was \$255,000 for 2013 and \$260,000 for 2014.	No
B	<i>Plan 1</i>	Regular or fixed-term employees scheduled to work at least 1,000 hours per fiscal year and not currently actively participating in Plan 2	1% of the employee's eligible gross earnings on a pre-tax basis	Matched by an 8% contribution rate from the University	No
	<i>Plan 2</i>	Regular or fixed-term employees who were hired prior to June 30, 1993 and scheduled to work at least 20 hours per week for a minimum of 720 hours per fiscal year	1% of the employee's eligible gross earnings on an after-tax basis	There are several benefit calculation formulas. This plan uses the formula that maximizes employee benefits.	No
C	<i>Plan 1</i>	All faculty and staff member in a benefits-eligible title who are age 21 or older	None	<u>Employer contribution rates when the employee's annual salary is below the Social Security Wage Base:</u> i) 7% (employee age < 50) ii) 10% (employee age ≥ 50) <u>Employer contribution rates when the employee's annual salary is above the Social Security Wage Base:</u> i) 12% (employee age < 50) ii) 15% (employee age ≥ 50)	Yes
	<i>Plan 2</i>	All employees who earn at least 140% of the Social Security Wage Base	An elected percentage of the employee's eligible earnings on a pre-tax basis	None	No
D	<i>Plan 1</i>	All employees except for student workers, hospital employees, leased employees, and those in post-doctoral positions	An elected percentage of the employee's eligible earnings on a pre-tax basis	None	No
	<i>Plan 2</i>	Certain employees who, as of July 2000, worked at least 1,000 hours per year and opted not to be covered by the targeted plan	None	<i>Monthly defined benefit payment:</i> (Final average pay ¹ * Years of participation in Plan 2 * 1.25%)/12	Yes


¹ Final average pay equals average pay for the five years of highest pay that fall within the last ten years of plan participation.

Appendix B. Descriptions of Robustness Checks

- i. Our results remain qualitatively unchanged if we drop employees whose total annual contributions to all of the non-targeted plans were on track, as of September 2013, to exceed the IRS limit of \$17,500 for calendar year 2013. Those employees would not have been able to save more in the targeted plan. Specifically, we multiply employees' total contribution rates to all non-targeted plans as of September 2013 by their annual salary. For employees whose total contribution rate to all non-targeted plans as of September 2013 was zero or missing, we use their cumulative dollar contributions from January 2013 to September 2013 to measure their total contributions in the year 2013. Then we drop individuals whose 2013 dollar contributions to all non-targeted plans were on track, based on these calculations, to exceed \$17,500.
- ii. Some employees have missing data on salary or contributions. Our results reported in the paper assigned a value of zero to missing data. Our results are robust if we instead drop employees who have missing data on salary or contributions.
- iii. To calculate an employee's contribution rate in a given month, we divide her dollar contributions by her salary in that month. Since we construct the increased contribution rate indicator by comparing these *imputed* contribution rates in May 2014 versus September 2013, we want to ensure that our results are not spuriously driven by how we round our imputed contribution rates. For example, if an employee had an imputed contribution rate of 5.030% in September 2013 and 5.033% in May 2014, it is unlikely that this employee increased her contribution rate by 0.003%; rather, this difference in imputed contribution rates likely reflects a rounding issue. Thus, we have rounded contribution rates in multiple ways (e.g., rounding at 1% or 10 basis points of pay) and obtained similar results.
- iv. Our results do not meaningfully change when we use logistic regressions (rather than OLS regressions) to predict the increased contribution rate indicator.

Mailing Templates for the Field Experiment

Panel A. No Delay



Stop Waiting... Start Saving!

Participating in the University of Pennsylvania Tax-Deferred Retirement Plan (TDR) is a great way to plan for your future, and we've made it easy for you to get started. Simply check ☒ **Yes** on the attached response card, and return it in the enclosed postage-paid envelope by November 1, 2013 to enroll in the TDR.

Start contributing ☒ NOW
We'll take care of the rest!

.....

By checking ☒ **Yes**, you will:

- **Start contributing 5% of your eligible pay to the TDR now.**
- **Invest in an age-appropriate TIAA-CREF lifecycle fund if you have not previously selected investment funds.** Lifecycle funds provide a mix of equity, bond, and money market funds that is tailored to your age and that becomes more conservative as you approach retirement. If you have previously selected investment funds, your contribution will be invested in those funds.
- **Receive the full Penn match.** The University matches your contributions dollar-for-dollar up to 5% of your base salary.

Once you enroll in the TDR, you have the freedom to change your contribution rate and investment options at any time. To find information about other investments should you want to invest in other funds, visit www.hr.upenn.edu/myhr/benefits/retirement#enroll or call 1-877-PENN-RET (1-877-736-6738).

Tear at perforation

Your response is needed by November 1, 2013!


☐ **Yes! Enroll me in the TDR NOW.**
 — I will begin contributing 5% of my eligible pay on a pre-tax basis as soon as administratively possible.* I direct that my contribution be invested in a TIAA-CREF lifecycle fund, based on my age and estimated retirement date, if I have not previously selected investment funds. If I have previously selected investment funds, I direct that my contribution be invested in those funds.**

 Signature

 Date

* Subject to IRS limits.
 ** Your enrollment time, contribution rate and investment options will not change unless you make another election. Your signature indicates that you know that you can change your elections at any time by visiting www.hr.upenn.edu/myhr/benefits/retirement#enroll or calling 1-877-PENN-RET (1-877-736-6738).

Panel B. Standard Delay



Stop Waiting... Start Saving!

Participating in the University of Pennsylvania Tax-Deferred Retirement Plan (TDR) is a great way to plan for your future, and we've made it easy for you to get started. Simply check ☒ **Yes** on the attached response card, and return it in the enclosed postage-paid envelope by November 1, 2013 to enroll in the TDR at your preferred pace.

Start contributing ☒ NOW
or ☒ IN 2 MONTHS
We'll take care of the rest!

.....

By checking ☒ **Yes**, you will:

- **Start contributing 5% of your eligible pay to the TDR either now or in 2 months.**
- **Invest in an age-appropriate TIAA-CREF target-date fund if you have not previously selected investment funds.** Target-date funds provide a mix of equity, bond, and money market funds that is tailored to your age and that becomes more conservative as you approach retirement. If you have previously selected investment funds, your contribution will be invested in those funds.
- **Receive the full Penn match.** The University matches your contributions dollar-for-dollar up to 5% of your base salary.

Once you enroll in the TDR, you have the freedom to change your contribution rate and investment options at any time. To find information about other investments should you want to invest in other funds, visit www.hr.upenn.edu/myhr/benefits/retirement#enroll or call 1-877-PENN-RET (1-877-736-6738).

Tear at perforation

Your response is needed by November 1, 2013!

☐ **Yes! Enroll me in the TDR NOW.**
 — I will begin contributing 5% of my eligible pay on a pre-tax basis as soon as administratively possible.* I direct that my contribution be invested in a TIAA-CREF target-date fund, based on my age and estimated retirement date, if I have not previously selected investment funds. If I have previously selected investment funds, I direct that my contribution be invested in those funds.**

☐ **Yes! Enroll me in the TDR IN 2 MONTHS.**
 — I will begin contributing 5% of my eligible pay on a pre-tax basis in 2 months.* I direct that my contribution be invested in a TIAA-CREF target-date fund, based on my age and estimated retirement date, if I have not previously selected investment funds. If I have previously selected investment funds, I direct that my contribution be invested in those funds.**


 Signature

 Date

* Subject to IRS limits.
 ** Your enrollment time, contribution rate and investment options will not change unless you make another election. Your signature indicates that you know that you can change your elections at any time by visiting www.hr.upenn.edu/myhr/benefits/retirement#enroll or calling 1-877-PENN-RET (1-877-736-6738).

Supplementary Materials

Panel C. Framed Delay (An Example Based on Birthday-framed Delay Mailings)



Stop Waiting... Start Saving!

Participating in the University of Pennsylvania Tax-Deferred Retirement Plan (TDR) is a great way to plan for your future, and we've made it easy for you to get started. Simply check ☒ Yes on the attached response card, and return it in the enclosed postage-paid envelope by November 1, 2013 to enroll in the TDR at your preferred pace.

Start contributing ☒ NOW

or ☒ AFTER YOUR NEXT BIRTHDAY

We'll take care of the rest!

.....

By checking ☒ Yes, you will:

- Start contributing 5% of your eligible pay to the TDR either now or after your next birthday.
- Invest in an age-appropriate TIAA-CREF target-date fund if you have not previously selected investment funds. Target-date funds provide a mix of equity, bond, and money market funds that is tailored to your age and that becomes more conservative as you approach retirement. If you have previously selected investment funds, your contribution will be invested in those funds.
- Receive the full Penn match. The University matches your contributions dollar-for-dollar up to 5% of your base salary.

Once you enroll in the TDR, you have the freedom to change your contribution rate and investment options at any time. To find information about other investments should you want to invest in other funds, visit www.hr.upenn.edu/myhr/benefits/retirement#enroll or call 1-877-PENN-RET (1-877-736-6738).

Tear at perforation

Your response is needed by November 1, 2013!

☐ Yes! Enroll me in the TDR **NOW**.

— I will begin contributing 5% of my eligible pay on a pre-tax basis as soon as administratively possible.* I direct that my contribution be invested in a TIAA-CREF target-date fund, based on my age and estimated retirement date, if I have not previously selected investment funds. If I have previously selected investment funds, I direct that my contribution be invested in those funds.**

☐ Yes! Enroll me in the TDR **AFTER MY NEXT BIRTHDAY**.

— I will begin contributing 5% of my eligible pay on a pre-tax basis **after my next birthday**.* I direct that my contribution be invested in a TIAA-CREF target-date fund, based on my age and estimated retirement date, if I have not previously selected investment funds. If I have previously selected investment funds, I direct that my contribution be invested in those funds.**

Signature _____

Date _____

* Subject to IRS limits.
** Your enrollment time, contribution rate and investment options will not change unless you make another election. Your signature indicates that you know that you can change your elections at any time by visiting www.hr.upenn.edu/myhr/benefits/retirement#enroll or calling 1-877-PENN-RET (1-877-736-6738).

Mailing Templates for the Lab Experiment

Panel D. No Delay

Stop waiting... Start Saving!

Participating in the Company X Retirement Savings Program is a great way to plan for your future, and we've made it easy for you to get started.

Start contributing ☒ NOW
We'll take care of the rest!

By checking ☒ Yes, you will:

- **Start contributing 6% of your eligible pay to the Retirement Savings Program now.**

Once you enroll in the Retirement Savings Program, you have the freedom to change your contribution rate and investment options at any time.

☐ Yes! Enroll me **NOW**.
— I will begin contributing 6% of my eligible pay on a pre-tax basis as soon as administratively possible.

Panel E. Standard Delay

Stop waiting... Start Saving!

Participating in the Company X Retirement Savings Program is a great way to plan for your future, and we've made it easy for you to get started.

Start contributing ☒ NOW
or ☒ **IN 6 MONTHS**
We'll take care of the rest!

By checking ☒ Yes, you will:

- **Start contributing 6% of your eligible pay to the Retirement Savings Program either now or in 6 months.**

Once you enroll in the Retirement Savings Program, you have the freedom to change your contribution rate and investment options at any time.

☐ Yes! Enroll me **NOW**.
— I will begin contributing 6% of my eligible pay on a pre-tax basis as soon as administratively possible.

☐ Yes! Enroll me **IN 6 MONTHS**.
— I will begin contributing 6% of my eligible pay on a pre-tax basis in 6 months.

Panel F. Two-stage Delay

Stage I (Same as the No-delay Mailing)

Stop waiting... Start Saving!

Participating in the Company X Retirement Savings Program is a great way to plan for your future, and we've made it easy for you to get started.

Start contributing ☒ NOW
We'll take care of the rest!

By checking ☒ Yes, you will:

- **Start contributing 6% of your eligible pay to the Retirement Savings Program now.**

Once you enroll in the Retirement Savings Program, you have the freedom to change your contribution rate and investment options at any time.

☐ Yes! Enroll me **NOW**.
— I will begin contributing 6% of my eligible pay on a pre-tax basis as soon as administratively possible.

Stage II

Stop waiting... Start Saving!

Participating in the Company X Retirement Savings Program is a great way to plan for your future, and we've made it easy for you to get started.

You didn't respond when we encouraged you to start contributing **now** so we took that as a "no". How about **starting to contribute ☒ IN 6 MONTHS**
We'll take care of the rest!

By checking ☒ Yes, you will:

- **Start contributing 6% of your eligible pay to the Retirement Savings Program in 6 months.**

Once you enroll in the Retirement Savings Program, you have the freedom to change your contribution rate and investment options at any time.

☐ Yes! Enroll me **IN 6 MONTHS**.
— I will begin contributing 6% of my eligible pay on a pre-tax basis in 6 months.