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Using fresh starts to nudge increased retirement savings

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ABSTRACT

We conducted a field experiment to study the effect of framing future moments in time as new beginnings (or "fresh starts"). University employees (N = 6,082) received mailings with an opportunity to choose between increasing their contributions to a savings plan immediately or at a specified future time point. Framing the future time point in relation to a fresh start date (e.g., the recipient's birthday, the first day of spring) increased the likelihood that the mailing recipient chose to increase contributions at that future time point without decreasing their likelihood of increasing contributions immediately. Overall, fresh start framing increased retirement plan contributions in the eight months following the mailing. Our findings represent the first experimental demonstration of the benefits of fresh start framing in a consequential field setting.

1. Introduction

Decades of research have demonstrated that people frequently make decisions that are poorly aligned with their long-term best interests (Duckworth, Milkman, & Laibson, 2018). We overeat, smoke too much, exercise too little, underinvest in education and skill development, and save insufficiently for retirement. The consequences of these myopic decisions can be severe. In an attempt to combat this problem, employers, health care providers, marketers, and benevolent policy makers are increasingly offering us opportunities to improve ourselves—to take steps that will put us on a better life trajectory. There are now countless weight loss programs, smoking cessation programs, retirement savings programs, and healthy meal plans available for purchase, and sometimes for free. Predictably, however, convincing people to take the steps required to enroll in these programs is challenging.

For many programs, individuals are offered both the opportunity to enroll now and the opportunity to choose now to enroll at a future point in time. For example, local health care centers often offer multi-week smoking cessation programs, and smokers who wish to quit can register for a program beginning immediately or a program beginning many weeks in the future. Many banks invite customers to schedule recurring savings deposits such that the regular transfer of funds from a checking to a savings account will begin either immediately or at a future point in time. In the example studied in this paper, employees of an organization that provides a retirement savings plan can elect to increase their plan contributions immediately or can elect now to have their plan contributions increase in the future. We study whether framing the future point in time when a goal-congruent activity could be scheduled as a "fresh start" (i.e., labeling the date as "New Year's" or "the first day of spring" or "your birthday") increases adoption of self-controlled, goal-directed choices.

A growing literature suggests that there are certain points in time when people feel particularly motivated to pursue their goals. Specifically, moments that feel like new beginnings—so-called "fresh starts"—have been shown in correlational studies to be popular times for tackling positive change (Dai, Milkman, & Riis, 2014). Laboratory research has also demonstrated that reminding people that today or an upcoming day corresponds to the beginning of a new time period (e.g., that it is the first day of the week or month or the start of spring) increases goal motivation and goal pursuit on the date in question (Dai, Milkman, & Riis, 2015; Davydenko & Peetz, 2019; Hennecke & Converse, 2017).

We hypothesize that when presenting both an immediate enrollment option and a delayed enrollment option, framing the delayed option as occurring right after a fresh start (instead of describing it as occurring after an equivalent time interval) will increase take-up of the delayed option for at least five reasons. First, fresh start framing makes the delayed option feel further off in the future (Peetz & Wilson, 2013; Tu &

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Soman, 2014) and thus the delayed option becomes an even more appealing time-frame for adopting future-oriented behaviors (Milkman, Rogers, & Bazerman, 2010; Prelec & Loewenstein, 1997). Second, temporal landmarks that signal new beginnings are more salient than ordinary dates when people recall past activities or plan future activities, and the greater salience of the delayed option may increase individuals' likelihood of selecting it (Robinson, 1986; Rubin & Kozin, 1984; Shum, 1998; Soster, Monga, & Bearden, 2010). Third, fresh start dates promote big-picture thinking and high-level construal, which prompts people to focus on the desirability (rather than feasibility) of pursuing long-term goals (Hennecke & Converse, 2017; Trope & Liberman, 2003). Fourth, people are more optimistic about their future capabilities on or after future fresh start dates (Dai et al., 2015; Koo, Dai, Mai, & Song, 2020) and thus may be more willing to sign their future selves up for a self-improvement program beginning shortly after a fresh start date that is framed as such. Fifth, situational cues—including temporal landmarks—can influence beliefs regarding the appropriate course of action (Gersick & Hackman, 1990; Gersick, 1988, 1989; Kay & Ross, 2003; Liberman, Samuels, & Ross, 2004; Weber, Kopelman, & Messick, 2004), and fresh start framing may serve as a cue that it is appropriate to initiate goal pursuit at the future moment in question.

In addition to considering whether take-up of the delayed enrollment option is increased by framing it as occurring shortly after a fresh start, it is important to consider the effect this framing has on take-up of the *immediate* enrollment option. If take-up of the delayed enrollment option is higher with fresh start framing, it must be that fewer individuals are selecting the immediate enrollment option or that more individuals total are choosing to enroll (or both). A decrease in take-up of immediate enrollment would represent an unintended negative consequence of fresh start framing, and in extreme cases, fresh start framing could undermine goal pursuit if it simply delayed enrollment, causing individuals who would have otherwise signed up immediately to instead delay their enrollment without increasing the total number of individuals who choose to enroll.

However, there are reasons to believe that framing a delayed enrollment option as a fresh start will not decrease and might even increase take-up of the immediate option. We noted earlier that dates associated with new beginnings are more salient than other dates (Robinson, 1986; Rubin & Kozin, 1984; Shum, 1998; Soster et al., 2010). Describing a delayed enrollment option in relation to a fresh start date may attract people's attention to the *overall choice set*, increasing their likelihood of making an active election (rather than setting the decision aside and not enrolling). Also, as mentioned previously, fresh start framing may encourage individuals to consider the decision at a high level of construal (Hennecke & Converse, 2017; Trope & Liberman, 2003). A high level of construal may increase the attractiveness of both delayed enrollment *and* immediate enrollment by causing individuals to focus on the desirability of achieving long-term savings goals.

In this paper, we present a field experiment examining the effects of framing a future date as a fresh start when inviting people to enroll in a program promoting long-term goal pursuit on that future date. Specifically, we present a between-subjects study that was designed to increase the retirement plan contributions of employees at four major U.S. universities. Employees received a mailing encouraging them either to enroll in a retirement savings plan for the first time (if they were not already participating in the plan) or to increase their contributions to a retirement savings plan (if they were already contributing to the plan). If a mailing recipient returned a response form with a box checked indicating that they wanted to save more, that individual was enrolled in the plan (or, in the case of someone already contributing to the plan, that individual's contribution rate to the plan was increased). Past research has shown that such simplified enrollment and contribution escalation mailings are an effective means of increasing retirement savings rates (Beshears, Choi, Laibson, & Madrian, 2013).

In our experiment, mailings offered employees the option to agree now to have their retirement plan administrator immediately increase their plan contribution rate. Employees also had the option to agree now to have their retirement plan administrator increase their plan contribution rate at a *future* point in time. We randomly assigned employees to receive different versions of our mailings to test the effect of psychologically linking that future savings opportunity with a fresh start date. The fresh start dates we examined were temporal landmarks associated with new beginnings: the recipient's next birthday, New Year's, or the first day of spring. We found that compared to mailings that described the future savings opportunity without reference to a temporal landmark (e.g., "in 2 months"), mailings that described the future savings opportunity as occurring shortly after a fresh start date (e.g., "after your next birthday") increased take-up of the future savings opportunity. Furthermore, fresh start framing did not decrease take-up of the immediate savings opportunity, and it increased cumulative savings contributions over the eight months following the mailing by roughly 25% more than other mailings. Placebo mailings that linked the future savings opportunity to "control" temporal landmarks that are not typically associated with new beginnings (Thanksgiving, Martin Luther King Day, or Valentine's Day) did not have positive effects on take-up of the future savings opportunity or cumulative savings contributions over eight months.

To further explore the effect of fresh start framing on immediate savings adoption as well as delayed savings adoption, we conducted a follow-up laboratory experiment in which participants compared a mailing with a delayed savings opportunity framed as a fresh start to a mailing that invited people to begin saving at an equivalent time delay without mentioning a temporal landmark. Participants were asked to predict which mailing would more effectively convince recipients to enroll in a savings program. Participants predicted that the fresh start mailing was likely to encourage both more delayed enrollment *and* more immediate enrollment.

Our paper contributes to the literatures on self-control, fresh starts, and choice architecture in several ways. First, while a growing body of correlational evidence (Dai et al., 2014; Dai, 2018) and laboratory evidence (Dai et al., 2015; Dai, 2018; Davydenko & Peetz, 2019; Hennecke & Converse, 2017) suggests that fresh starts are ideal moments for encouraging self-controlled behaviors, to our knowledge only one previous paper has tested whether fresh start framing can change decisions in a consequential field setting. To increase adherence to prescription medication regimens, Dai et al. (2017) sent one reminder mailer to a sample of individuals, varying whether or not the reminder was sent on a date linked to a fresh start and also whether or not the reminder highlighted a fresh start date as an opportunity to begin taking medications regularly. This experiment generated null results, perhaps because it was too difficult to capture the attention of a sufficient number of reminder recipients or because the mailer did not arrive at recipients' homes in time for the fresh start date to feel relevant, making it challenging to draw strong conclusions regarding the efficacy of fresh start framing in the field. The current paper provides the first *affirmative* field demonstration of fresh start framing as a means of encouraging self-improvement. It therefore answers an important, open question about the external validity and policy value of fresh start framing as a means of promoting behavior change.

Another key contribution of this research is that it establishes how applying a fresh start framing to an opportunity to begin *future* goal pursuit affects *immediate* decisions about goal pursuit. The impact of future fresh starts on present behavior is an understudied topic. Koo et al. (2020) show that when an upcoming temporal landmark is made salient, people decrease their current motivation to pursue an ongoing goal. However, this evidence considers situations in which individuals are deciding whether and how to *continue* pursuit of a goal. In contrast, the current paper focuses on the *initiation* of goal pursuit, a stage of the overall goal pursuit process with distinct psychological mechanisms at play (Gollwitzer & Sheeran, 2006; Rothman, Baldwin, Hertel, & Fuglestad, 2011; Voils et al., 2014). Whereas Koo et al. (2020) document a demotivating effect of a future fresh start on continued goal pursuit,

we find that applying a fresh start framing to a future opportunity to begin pursuing a goal does not decrease and may even increase immediate initiation of goal pursuit. This is despite the fact that making goal pursuit in the future more attractive might be expected to decrease immediate goal initiation by reducing the relative attractiveness of the option to begin right away. This finding not only extends our theoretical understanding of the effects of fresh starts but also provides a critical insight about how fresh start framing could help managers and policy makers seeking to promote the initiation of goal pursuit.

This paper also contributes to a growing and influential literature on methods for increasing 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, Webb, & Golub-Sass, 2012). A wide range of factors may lead to under-saving, including under-appreciation of the frequency of "exceptional" expenses that increase current spending (Sussman & Alter, 2012), the availability of credit (Soman & Cheema, 2002), a dispositional tendency to experience too little pain when spending (Rick, Cryder, & Loewenstein, 2008), and present bias (Angeletos, Laibson, Repetto, Tobacman, & Weinberg, 2001; Benartzi, Peleg, & Thaler, 2013). Past research has demonstrated that nudges can be a potent tool for increasing savings rates (e.g., Beshears et al., 2013; Hershfield et al., 2011; Karlan, McConnell, Mullainathan, & Zinman, 2016; Madrian & Shea, 2001; Soman & Cheema, 2011; Thaler & Benartzi, 2004). Our results highlight the promise of fresh start framing for upcoming savings opportunities as a new and effective nudge for encouraging saving.

The remainder of this paper proceeds as follows. In the next section, we review the relevant past research on fresh starts and their impact on goal-oriented behaviors. Then, we describe the design of our field experiment and present the results. We then describe the design and results of our follow-up laboratory study. Finally, we discuss the interpretation of our results, limitations, and key directions for future research.

2. Past research on temporal landmarks and fresh starts

Previous research has explored how individuals' motivation to pursue their goals is shaped by temporal landmarks. Temporal landmarks have been defined as distinct events that "stand in marked contrast to the seemingly unending stream of trivial and ordinary occurrences that happen to us everyday" (Shum, 1998, p. 423). They include transition points on socially shared calendars (e.g., the start of a new season) and special events on personal life timelines (e.g., birthdays; Robinson, 1986; Shum, 1998). Importantly, different temporal landmarks have different properties. In particular, temporal landmarks have been shown to differ in the degree to which people feel they inaugurate new beginnings (Dai et al., 2015). For example, people tend to perceive temporal landmarks that mark the first day of a new calendar period (e.g., the first day of a new season, week, month, or year) as new beginnings. However, temporal landmarks that do not coincide with the start of a new calendar cycle, like Thanksgiving and Valentine's Day, are viewed differently. In addition, first experiences (e.g., a first date or first job after college) tend to be viewed as new beginnings while similar, later experiences (e.g., a ninth date or sixth job after college) are not (Dai et al., 2015; Shum, 1998).

Recent research has shown that the new beginnings or fresh starts inaugurated by temporal landmarks can boost individuals' motivation to pursue their long-term goals and make future-oriented choices (Dai et al., 2014, 2015; Davydenko & Peetz, 2019; Hennecke & Converse, 2017). For instance, people are more likely than usual to search for diet-related information, visit the gym, and create goal commitment contracts following a host of fresh starts, including the beginning of a new week, month, year, and semester, as well as after birthdays (Dai et al., 2014). Laboratory experiments have also shown that reminding people of fresh starts (like New Year's, the first day of spring, and the start of a new week) can spur engagement in goal-related activities on or right after fresh start dates (Dai et al., 2015; Davydenko & Peetz, 2019). Multiple mechanisms appear to be responsible for these findings. For one, when fresh starts arise, they make people feel more separated from their past selves and past failures, increasing their optimism about what they can achieve (Dai et al., 2015; Dai, 2018). In addition, experiencing a fresh start may cause people to pause, step back, and think in "big picture" terms about their lives, which can motivate increased goal pursuit (Alter & Hershfield, 2014; Dai et al., 2014; Liu, 2008).

Although past research on the motivating effects of fresh starts has primarily explored how they impact people's goal pursuit on or right after the fresh start date in question (Dai et al., 2014, 2015; Davydenko & Peetz, 2019), there is some evidence that fresh starts can affect people's *prospective* decisions about goal pursuit as well. For example, in one laboratory experiment (Dai et al., 2015), participants reported being more interested in receiving future reminders to begin pursuing a new goal on March 20th when it was described as "the first day of spring" rather than "the third Thursday in March." In another laboratory study, undergraduates at the University of Pennsylvania reported being more interested in initiating goal pursuit on May 14th when it was described as "the first day of Penn's summer break" than when it was described as "Penn's administrative day" (Dai et al., 2015). Further, laboratory experiments that altered the kinds of calendars would-be dieters viewed showed similar results. Prospective dieters reported a higher likelihood of starting improved eating regimens on the first day of a new month when viewing a calendar featuring days of the month rather than days of the week. On the other hand, when viewing calendars featuring days of the week, Mondays became highly attractive dates for starting a diet (Hennecke & Converse, 2017).

There are several reasons why people may be more motivated to begin pursuing their goals following an upcoming fresh start (e.g., a birthday) than following an unremarkable future date (e.g., in two months). The first has to do with the fact that people are generally more willing to pursue activities with distal rewards at a longer time delay (Milkman et al., 2010; Prelec & Loewenstein, 1997). This is because people are present biased and impatient, but decreasingly so (Prelec & Loewenstein, 1997). Past research suggests that moments following temporal landmarks or fresh starts feel subjectively further off in the future than moments that precede them (Peetz & Wilson, 2013; Tu & Soman, 2014). Therefore, people may be more willing to initiate goal pursuit following dates framed as fresh starts because those dates feel further off, and it is more appealing to pursue goals with delayed rewards in the more distant future.

Fresh start framing may also affect behavior through an attentional channel. Temporal landmarks associated with new beginnings are more salient than other moments in time (Robinson, 1986; Rubin & Kozin, 1984; Shum, 1998; Soster et al., 2010). Therefore, framing a future opportunity for initiating goal pursuit as a fresh start likely increases the attention paid to that option, and increased attention may make individuals more likely to select the option.

Research on construal level offers another explanation for the prediction that individuals are more likely to initiate goal pursuit following dates framed as fresh starts. According to construal level theory, the more distant an event, the more likely people are to think about it at a high construal level (Trope & Liberman, 2003). High-level construals direct attention away from the feasibility of goals and towards the desirability of goal achievement (Liberman & Trope, 1998; Rogers & Bazerman, 2008; Trope & Liberman, 2003; Vallacher & Wegner, 1985), making goal pursuit more attractive. Construal level theory therefore predicts that people will be more willing to initiate goal pursuit following dates framed as fresh starts because such dates feel more

distant, which leads goal pursuit to be construed at a higher level and in turn makes goal pursuit more attractive. Consistent with this account, Hennecke and Converse (2017) found that people were less concerned about obstacles they might face (a feasibility issue) when they thought about starting a goal on a fresh start date (e.g., "on August 1st") than when they thought about starting their goal on the same date described without reference to the fresh start (e.g., "on Saturday").

Another reason people may find goal pursuit after future fresh starts particularly attractive relates to the way people forecast their future capabilities. As mentioned previously, fresh starts help people disconnect from their past failures, increasing their optimism about their capacity to achieve goals (Dai et al., 2015; Dai, 2018). Prospectively, then, people may also believe that their future self after a fresh start will feel more separated from past failures and be better able to tackle goals than their future self after an ordinary date.

A final reason why fresh start dates may be attractive targets for prospective goal initiation relates to common beliefs about when it is appropriate to begin tackling goals. Past research has shown that people often rely on formal and informal rules to decide when they should perform certain actions (March, 1994). Further, situational cues as subtle as the labels assigned to different options can shape behavior by altering beliefs about the appropriate course of action (Kay & Ross, 2003; Liberman et al., 2004; Weber et al., 2004). The cues that shape the behaviors that are viewed as appropriate are sometimes temporal. For example, researchers have shown that when teams face a deadline, the midpoint in their time together serves as a coordinating mechanism for making major changes or pivots in their work (Gersick & Hackman, 1990; Gersick, 1988, 1989). Fresh starts may similarly serve as temporal cues. If people believe that it is appropriate to begin pursuing new goals at new beginnings, this could increase the attractiveness of prospective goal pursuit on future fresh start dates.

Taken together, past research offers a number of reasons to predict that framing a future opportunity for beginning goal pursuit as a new beginning (or fresh start) will increase take-up of that future opportunity. However, both from the perspective of better understanding fresh starts at a theoretical level and from the perspective of better applying fresh start framing as a policy tool, it is also important to consider how framing a future goal initiation opportunity as a fresh start will impact take-up of an *immediate* opportunity to begin goal pursuit.

Anticipating a future fresh start increases optimism about the future and thus can reduce immediate motivation to continue pursuing an *ongoing* goal (Koo et al., 2020), but to our knowledge, no previous research has examined how the salience of future fresh starts impacts immediate motivation to *initiate* goal pursuit. The processes governing initiation and continuation of goal pursuit are distinct (Gollwitzer & Sheeran, 2006; Rothman et al., 2011; Voils et al., 2014), so it is not necessarily true that the results of Koo et al. (2020) extend to the settings considered in this paper. For example, initiation of goal pursuit can be triggered by a one-time, temporary boost in motivation, and it is hindered when individuals forget to act, fail to notice that a good time to begin has arrived, or fail to overcome an initial reluctance to act (Gollwitzer & Sheeran, 2006; Rothman et al., 2011). In contrast, continuation of goal pursuit requires persistent motivation (not merely a temporary boost), and it is hindered when individuals find the goal pursuit process to be unpleasant or realize that pursuing the goal requires more energy than they anticipated (Gollwitzer & Sheeran, 2006; Rothman et al., 2011).

One might conjecture that framing a future opportunity for initiating goal pursuit as a fresh start would reduce take-up of an immediate opportunity for initiating goal pursuit, as making the future opportunity more attractive would render the immediate opportunity less attractive in *relative* terms. However, there are reasons to predict that a fresh start framing of the future opportunity would not decrease or might even increase take-up of the immediate opportunity. These reasons extend the logic of some of the previously articulated explanations for higher take-up of the future opportunity. They rely on the fact that take-up of the immediate opportunity and take-up of the future opportunity can *both* increase if there is a decrease in the likelihood of failing to initiate goal pursuit at all.

First, because moments in time that represent fresh starts are highly salient (Robinson, 1986; Rubin & Kozin, 1984; Shum, 1998; Soster et al., 2010), labeling the future opportunity as coinciding with a new beginning may increase not only the salience of the future opportunity but also the salience of the choice set overall. Once individuals' attention has been drawn to the choice set, they are more likely to consider the options carefully, and some of them may select the immediate opportunity, even though the future opportunity was the first option to attract their focus. In this way, fresh start framing may address factors that hinder immediate initiation of goal pursuit, such as forgetting to act or failing to notice that a good time to begin has arrived. This salience mechanism is probably less effective at addressing factors that hinder continuation of goal pursuit, as individuals who are already pursuing a goal do not face similar attentional hurdles.

Second, because fresh start framing is likely to trigger high-level construals of the goal initiation decision (Hennecke & Converse, 2017), it will tend to promote focus on the desirability of goal achievement (Liberman & Trope, 1998; Rogers & Bazerman, 2008; Trope & Liberman, 2003; Vallacher & Wegner, 1985). Such focus favors both immediate and future opportunities for initiating goal pursuit at the expense of the option of never initiating goal pursuit, helping individuals overcome any reluctance to act. This mechanism grounded in construal level theory, similar to the salience mechanism, is probably less effective at increasing continuation of goal pursuit than at increasing initiation of goal pursuit. The reason is that when communicating with individuals who are in the midst of pursuing a goal, it is probably difficult to redirect their focus away from the practical obstacles to goal achievement.

Combining the arguments related to take-up of immediate and future opportunities for initiating goal pursuit, theory and existing evidence lead to the prediction that applying fresh start framing to a future opportunity to begin pursuing a goal will increase people's likelihood of acting on that goal. In our field context, this means that framing a future opportunity to increase retirement plan contributions in relation to a fresh start date (relative to an unspecified date) will boost people's likelihood of increasing contributions and, likely, their overall savings.

3. Material and methods for the field experiment

3.1. Design of the field experiment

For our field experiment, we collaborated with four U.S. universities, which we will refer to as Universities A, B, C, and D.[1] We originally included a fifth university in the experiment, but this university offers generous employer contributions that are not contingent on employee contributions and also 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 the response rate was extremely low—0.6% of employees at this university increased their contribution rates following our mailings, whereas 12.6% of employees at the other four universities increased their contribution rates following our mailings. The low response rate at the fifth university makes it impossible to perform a meaningful analysis of the effect of different experimental treatments, so we drop this university from our analysis.

At each of the Universities A, B, C, and D, most employees were eligible to contribute to multiple retirement savings plans simultaneously. University officials selected one of the retirement plans to be the *targeted plan* in which they aimed to increase their employees'

[1] The first three universities elected to remain unnamed, but University D decided to identify itself as the University of Pennsylvania.

contributions. Mailings were sent to employees who were not enrolled in the targeted plan and therefore had a contribution rate of zero. At University D, mailings were also sent to employees who had a strictly positive contribution rate in the targeted plan but who were not contributing at the level necessary to earn all employer matching contributions, which were made dollar-for-dollar up to a certain fraction of an employee's salary. A retirement plan record keeper for these universities sent mailings in early October 2013 to university employees' homes. For employees who were not enrolled in the targeted plan, the mailings provided an opportunity to begin contributing to the plan. For employees who were enrolled in the targeted plan at a low contribution rate, the mailings provided an opportunity to increase their contributions. All mailings asked employees to reply by filling out and mailing back a simple form, but the mailings also included information about the websites and telephone numbers that employees could use to change their contribution rates. See Online Appendix A for example mailing templates.

At all four universities, when employees enrolled in the targeted plan by returning the response form included in the mailings, their contributions were invested in a lifecycle fund. Lifecycle funds provide a diversified portfolio with a mixture of equities and fixed income instruments tailored to the employee's age. At University D, when employees who were already enrolled in the targeted plan increased their contributions by returning the response form included in the mailings, their incremental contributions were invested according to the fund allocations of their existing contributions. In all cases, employees could change their investment allocations at any time using the retirement plan website or call center. 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 employee contributions dollar-for-dollar up to 5% of the employee's base salary. Detailed information about the targeted plans can be found in Table 1, and information about other (non-targeted) savings plans offered to employees at the four universities is available in Online Appendix B.

Our objective was to compare contributions employees would make to their retirement savings plans when randomly invited to begin saving after a fresh start date that was labeled as such (we call this "fresh start framing") versus after an equivalent time delay given no label whatsoever. As a placebo test, we also examined the effect of inviting employees to begin saving after a temporal landmark that was not a fresh start date. Our mailings were sent in October 2013, and we explored inviting employees to begin saving in two, three, four, five, or six months. These delays corresponded to contribution rate increases in December 2013, January 2014, February 2014, March 2014, and April 2014, respectively, and holidays we identified as linked to these delays were Thanksgiving, New Year's, Martin Luther King Day, Valentine's Day, and the first day of spring (the Spring Equinox).[2] Employees with birthdays between November and March could also be invited to begin saving after their upcoming birthday. We conducted a pre-registered pilot study (N = 200 Amazon Mechanical Turk workers) to document that New Year's, the first day of spring, and birthdays represent fresh starts and feel like the beginning of a new cycle, while Thanksgiving, Martin Luther King Day, and Valentine's Day do not.[3]

In our field experiment, we stratified employees by university and birth month, and we randomized them into a series of *control* conditions, a series of *holiday framing* conditions, and a *birthday framing* condition.[4] Those assigned to one of the *control* conditions were given the opportunity to sign up to save (or to save more) either immediately or after a time delay that was described in terms of a specific number of months (e.g., "in 2 months"). The inclusion of an immediate savings option was required by the organizational partners that implemented the experiment because employees might find it confusing not to have the opportunity to elect an immediate contribution rate increase (an opportunity that our organizational partners had offered on other occasions). It also serves our research objective of examining an authentic, real-world setting in which individuals can choose to initiate goal pursuit either immediately or at a delay. Employees assigned to one of the *holiday framing* conditions received mailings identical to the mailings received by employees in the *control* conditions, except the time delay was described with reference to an annual holiday (e.g., "after Thanksgiving"). Finally, those assigned to the *birthday framing* condition received mailings identical to the mailings received by employees in the *control* and *holiday framing* conditions, except the time delay was described with reference to the recipient's birthday (i.e., "after your next birthday").

As mentioned above, we offered savings enrollment time delays of two, three, four, five, or six months, and because our mailings were sent in October 2013, these delays corresponded to contribution rate increases in December 2013, January 2014, February 2014, March 2014, and April 2014, respectively. The corresponding holidays linked to these

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	*Eligibility for Employee Contributions*	*Automatic Employer Contribution Rates (Regardless of Whether the Employee Contributes)*
	i) Regular full-time staff (with monthly or weekly pay cycles) OR	i) 1.5% (employee age < 30)
	ii) Full-time faculty and academic support staff in a benefits-eligible title OR	ii) 3% (employee age 30–39)
	iii) Limited-service staff scheduled to work at least 35 h per week for a minimum of 9 months per year (with monthly or weekly pay cycles)	iii) 4% (employee age ≥ 40)
	Eligibility for Employer Contributions	*Matched Employer Contributions*
	All employees who are eligible for employee contributions (described above), are age 21 or older, and have at least one year of prior service	Dollar-for-dollar match on employee contributions up to 5% of employee's salary

[2] Given the logistical constraints faced by our partner organizations, we were not able to test a larger number of holidays. We picked one holiday per month corresponding to the 2-month delay (Thanksgiving), 3-month delay (New Year's), 4-month delay (Martin Luther King Day), 5-month delay (Valentine's Day), and 6-month delay (the first day of spring).

[3] We asked participants whether they thought each of the six temporal landmarks indicated a time when a new cycle in their lives begins. Participants were significantly more likely to view New Year's (91%), the first day of spring (58%), and their birthday (83%) as the beginning of a new cycle than to view Thanksgiving (18%), Martin Luther King Day (11%), and Valentine's Day (10%) as the beginning of a new cycle ($p < 0.001$ for all tests comparing one of the first three temporal landmarks to one of the last three temporal landmarks). See Online Appendix C for details.

[4] In addition, some employees were randomly assigned to a *no delay* condition, which only offered employees the opportunity to sign up to save (or to save more) immediately. As we show in a companion paper, contributions were statistically significantly higher in the *no delay* condition compared to the *control* conditions. We argue in our companion paper that this surprising finding is a result of the *control* conditions' inadvertent messaging, due to the offer of a delayed savings option, that savings is not an urgent priority. This result is not related to fresh start framing, so we do not focus on it in this paper.

delays were Thanksgiving (a placebo temporal landmark), New Year's (a fresh start date), Martin Luther King Day (a placebo temporal landmark), Valentine's Day (a placebo temporal landmark), and the first day of spring (a fresh start date). Employees with birthdays in November through March could be assigned to the *birthday framing* condition (a fresh start date) with a matched time delay, but employees with birthdays in April through October could not. We therefore used different randomization schemes for these two groups of employees. For the rest of the paper, the term "birthday cohort one" refers to the group of employees with birthdays between November and March, and the term "birthday cohort two" refers to the group of employees with birthdays between April and October.

As illustrated in Fig. 1, we randomly assigned employees in birthday cohort one to receive a *control* mailing, a *holiday framing* mailing, or a *birthday framing* mailing with equal probability. The time delay offered to an employee in this group was matched to the month of the employee's birthday. For example, employees whose birthdays were in November were offered the opportunity to increase savings immediately or "in 2 months" if they were assigned to the *control* condition; they were offered the opportunity to increase savings immediately or "after Thanksgiving" if they were assigned to the *holiday framing* condition; and they were offered the opportunity to increase savings immediately or "after your next birthday" if they were assigned to the *birthday framing* condition. Across all three experimental conditions, if employees with November birthdays chose to begin saving on the future date indicated, their retirement plan administrator increased their savings rates at the same time in December.

Employees in birthday cohort two were randomly assigned across all five *control* conditions (two, three, four, five, or six months of delay) and all five corresponding *holiday framing* conditions, regardless of birth month. The probability of assignment was equal across conditions, except the three-month *control* condition and the New Year's *holiday framing* condition were twice as likely to be assigned as the other eight conditions.[5] Employees in birthday cohort two were never assigned to the *birthday framing* condition.

3.2. Data from the field experiment

To analyze the results of our field experiment, we study data provided by our four university partners. These universities pulled a cross-sectional snapshot of information about all employees eligible for the targeted retirement plan in August 2013, including information about each employee's: (1) current contributions to the targeted plan, (2) current contributions to non-targeted plans, (3) gender, (4) birth date, (5) hire date, (6) termination date, (7) salary, and (8) 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 pay, contributions to the targeted plan, and contributions to non-targeted plans for each pay cycle through June 2014. We did not obtain data indicating which employees returned response forms electing to enroll in (or increase contributions to) a retirement savings plan,[6] but we can infer who responded (or was influenced by our mailing to change their contributions via another channel) by observing all changes in retirement plan contributions over the months following our mailing.

3.3. Analysis strategy for the field experiment

To capture the effect of the different experimental treatments on savings, we calculate four outcome variables for each employee in our study. First, we construct an indicator variable (*delayed option take-up*) for whether the employee's contribution rate to the targeted plan increased in the future month that was designated in the experimental mailing as the proffered time of a delayed savings increase (December 2013, January 2014, February 2014, March 2014, or April 2014, as appropriate given the contents of the employee's mailing), relative to the prior month.[7] For example, if the delayed savings opportunity offered to an employee was in 2 months, then this variable would take a value of one if the employee's contribution rate in December was higher than in November, and it would take a value of zero otherwise. This outcome variable allows us to measure the direct effect of fresh start framing on take-up of the delayed savings option (the option that was framed as a fresh start in some experimental conditions).

Second, we construct an indicator variable (*immediate option take-up*) for whether the employee's contribution rate to the targeted plan increased in November 2013, relative to October 2013 (when the experimental mailings were sent).[8] This outcome variable allows us to measure the effect of fresh start framing on take-up of the *immediate* savings option, an effect that is of both theoretical and practical interest.

Third, we construct an indicator variable (*immediate or delayed option take-up*) that takes a value of one if either of the first two indicator variables takes a value of one. This variable captures whether the employee increased savings immediately *or* at the suggested delay,[9] and is relevant for policy. For the purposes of promoting long-run savings accumulation, it would be counterproductive if fresh start framing increased take-up of delayed savings but decreased take-up of immediate savings by a similar or larger magnitude, causing no change overall or even lower take-up of savings opportunities.

Finally, we calculate each employee's average contribution rate in the targeted plan from November 2013 through June 2014 (*average contribution rate*). Specifically, we calculate the total number of dollars the employee contributed to the targeted plan from November 2013 through June 2014, divided by the employee's total pay from November 2013 through June 2014. We scale this outcome variable so that it ranges from 0 to 100. For example, if an employee contributed 10% of her pay, this variable would take a value of 10. We calculate a contribution rate over this time period because November 2013 is the first calendar month after we sent our mailings, while June 2014 is the latest month for which we received data on employees' savings contributions and pay. This outcome variable is important for policy. Financial

[5] Past research suggests that New Year's is a particularly meaningful fresh start opportunity (Dai et al., 2014). We conjectured that the New Year's *holiday framing* condition would be one of the most effective *holiday framing* conditions for increasing retirement plan contributions, so we placed more of our sample in this cell and its paired control.

[6] Note that this information was not retained by our organizational partners and so is not available. Importantly, the outcome variables that we calculate can more comprehensively capture the effects of our mailings on changes in savings rates made via all possible channels and thus are, arguably, superior outcome measures.

[7] We do not know the exact timing within a month of the implementation of delayed savings rate increases at each university, but the construction of this variable captures the range of possibilities. Also, 469 out of 6,082 individuals in our sample do not have data for a month that is needed for constructing this outcome variable. These individuals either took a leave of absence or separated from employment permanently during our sample period. For these 469 individuals, we set the outcome variable to missing. However, our results are robust to setting the outcome variable equal to zero instead.

[8] Similarly, we do not know the exact timing within November 2013 of the implementation of immediate savings rate increases at each university, but the construction of this variable captures the range of possibilities. For the 364 individuals who do not have data for a month that is needed for constructing this outcome variable, we set the outcome variable to missing. However, our results are robust to setting the outcome variable equal to zero instead.

[9] For the 509 individuals who do not have data for a month that is needed for constructing this outcome variable, we set the outcome variable to missing. However, our results are robust to setting the outcome variable equal to zero instead.

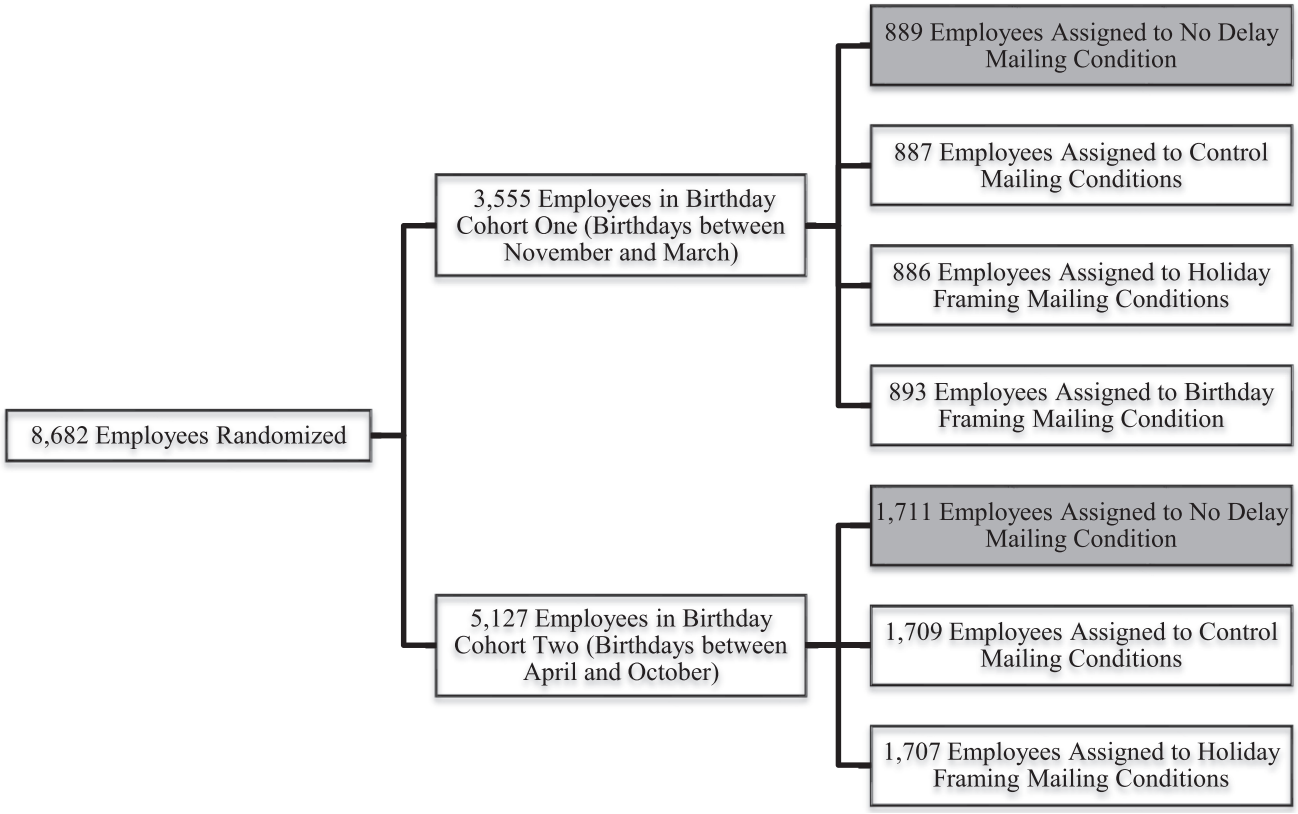

Fig. 1. Study Flow. Note: 42 randomized employees were not included in this figure or in the analysis because they did not have data collected, were terminated before the baseline data collection, or had conflicting dates of birth recorded in the data set. Employees in the *no delay* condition (marked in grey) are not included in this paper. They were only offered the opportunity to sign up to save (or to save more) immediately. As we show in a companion paper, contributions were statistically significantly higher in the *no delay* condition compared to the *control* conditions. We argue in our companion paper that this surprising finding is a result of the control conditions' inadvertent messaging, due to the offer of a delayed savings option, that savings is not an urgent priority. This result is not related to fresh start framing, so we do not focus on it in this paper. Thus, the conditions reported in the current paper include 6,082 employees.

advisors often recommend a contribution rate that is a fraction of income so savings can provide a stream of retirement income that is proportional to working-age income (e.g., 80% of working-age income). The rationale is that individuals with high income during their working years need to save more to maintain the same standard of living during retirement (e.g., to meet mortgage or rent obligations). Also, the average contribution rate captures both the decision of *whether* to increase savings and the decision, conditional on increasing savings, of *how much* to increase savings. The reasons for predicting that framing a future opportunity to begin saving as a fresh start will promote more saving, described in Section 2, apply to both of these decisions.

Our main analysis focuses on savings decisions related to the targeted savings plan, but as a robustness check, we also recalculate the four outcome variables combining data from all available savings plans (including both targeted and non-targeted plans). The outcome measures that focus only on the targeted savings plan are our primary measures because the mailings we sent offered the opportunity to increase savings in the targeted plan. The outcome measures that consider all available savings plans together are useful because they allow us to study whether incremental contributions to the targeted plan induced by our fresh start framing were simply a shift of contributions away from non-targeted plans.

We use ordinary least squares (OLS) regressions to analyze all four outcome variables.[10] We rely on the following regression specification to estimate the effects of framing our mailings around new-beginning temporal landmarks (fresh starts) relative to simply stating the number of months of delay before contributions will be increased (e.g., "in 2 months"):

$$(1)\quad outcome_i = \alpha + \beta\, fresh\ start\ framing_i + \gamma_1\, placebo\ holiday\ framing\ (birthday\ cohort\ one)_i + \gamma_2\, placebo\ holiday\ framing\ (birthday\ cohort\ two)_i + \gamma_3\, 3\text{-}month\ delay\ (birthday\ cohort\ two)_i + \gamma_4\, 4\text{-}month\ delay\ (birthday\ cohort\ two)_i + \gamma_5\, 5\text{-}month\ delay\ (birthday\ cohort\ two)_i + \gamma_6\, 6\text{-}month\ delay\ (birthday\ cohort\ two)_i + \delta' X_i + \varepsilon_i.$$

In this equation, i indexes employees, and *outcome* is one of the four outcome variables.

The first predictor variable, the *fresh start framing* indicator, takes a value of one if the employee was assigned to the New Year's *holiday framing* condition, the first day of spring *holiday framing* condition, or the

[10] As Angrist and Pischke (2009) have argued, OLS regressions are appropriate even when the outcome variable is dichotomous or bounded. In our case, we are ultimately interested in learning about the conditional expectation of the outcome variable given the set of independent variables. The OLS model estimates the population regression function, which is the best linear approximation to this conditional expectation function in terms of minimizing mean squared error. As a robustness check, we perform logit and fractional logit regressions, and the results do not change.

birthday framing condition. The indicator takes a value of zero otherwise.

The second predictor variable, the *placebo holiday framing (birthday cohort one)* indicator, only takes on non-zero values for employees in birthday cohort one. It takes a value of one if the employee was in birthday cohort one and was assigned to the Thanksgiving *holiday framing* condition, the Martin Luther King Day *holiday framing* condition, or the Valentine's Day *holiday framing* condition. It takes a value of zero otherwise.

The next five predictor variables only take on non-zero values for employees in birthday cohort two. The *placebo holiday framing (birthday cohort two)* indicator takes a value of one if the employee was in birthday cohort two and was assigned to the Thanksgiving *holiday framing* condition, the Martin Luther King Day *holiday framing* condition, or the Valentine's Day *holiday framing* condition. It takes a value of zero otherwise. Each *Y-month delay (birthday cohort two)* indicator (where *Y* takes a value of 3, 4, 5, or 6) takes a value of one if the employee was in birthday cohort two and was offered a delay of Y months, regardless of whether the employee was assigned to a *control* condition (and the delay was therefore framed as "in Y months") or the employee was assigned to a *holiday framing* condition. The indicator takes a value of zero otherwise. Note that the *2-month delay (birthday cohort two)* indicator is omitted to avoid collinearity.

Finally, X_i is a vector of control variables. In all specifications, it includes indicator variables for each university interacted with indicator variables for each birth month. When conducting statistical tests, it is important to control for the interaction of university and birth month, both to reflect our stratified randomization procedure (by university and birth month) and to absorb variation across universities in mailing response rates and in the contribution rates suggested by the mailings, which would otherwise reduce statistical power. In some specifications, the set of control variables is expanded to include indicator variables for each university interacted with indicator variables for employee gender, age deciles, tenure deciles, salary deciles, and faculty status.[11] We report heteroskedasticity-robust standard errors.

The coefficient of interest in regression (1) is β, which tells us the effect of fresh start framing relative to framing that does not reference temporal landmarks. This coefficient is a weighted average of several comparisons: Employees in the *birthday framing* condition, the New Year's *holiday framing* condition, or the first day of spring *holiday framing* condition are compared to employees in *control* conditions with equivalent lengths of delay. There are two benefits to setting up the regression equation so that the coefficient β is a weighted average of several comparisons. First, the statistical test of the hypothesis that the coefficient β is zero provides a single statistical test to demonstrate that there is an effect of fresh start framing relative to stating the number of months of delay. Second, combining the comparisons into one estimate increases the statistical power of the hypothesis test.

To explore whether our findings regarding fresh start framing hold for framing associated with *any* temporal landmark (including temporal landmarks that do not represent fresh starts), we also conduct an analysis that estimates the effects of framing our mailings around placebo temporal landmarks (e.g., Thanksgiving) relative to stating the number of months of delay before contributions will be increased (e.g., "in 2 months"):

$$(2)\quad outcome_i = \eta + \theta\, placebo\ holiday\ framing_i + \kappa_1\, fresh\ start\ holiday\ framing\ (birthday\ cohort\ one)_i + \kappa_2\, Nov.,\ Jan.,\ or\ Feb.\ birthday\ framing\ (birthday\ cohort\ one)_i + \kappa_3\, Dec.\ or\ Mar.\ birthday\ framing\ (birthday\ cohort\ one)_i + \kappa_4\, fresh\ start\ holiday\ framing\ (birthday\ cohort\ two)_i + \kappa_5\, 3\text{-}month\ delay\ (birthday\ cohort\ two)_i + \kappa_6\, 4\text{-}month\ delay\ (birthday\ cohort\ two)_i + \kappa_7\, 5\text{-}month\ delay\ (birthday\ cohort\ two)_i + \kappa_8\, 6\text{-}month\ delay\ (birthday\ cohort\ two)_i + \lambda' X_i + \zeta_i.$$

Again, i indexes employees, and *outcome* is one of the four outcome variables. The *Y-month delay (birthday cohort two)* indicators and X_i are the same as in regression equation (1). We report heteroskedasticity-robust standard errors.

The first predictor variable, the *placebo holiday framing* indicator, takes a value of one if the employee was assigned to the Thanksgiving *holiday framing* condition, the Martin Luther King Day *holiday framing* condition, or the Valentine's Day *holiday framing* condition. The indicator takes a value of zero otherwise.

The next three predictor variables only take on non-zero values for employees in birthday cohort one. The *fresh start holiday framing (birthday cohort one)* indicator takes a value of one if the employee was in birthday cohort one and was assigned to the New Year's *holiday framing* condition or the first day of spring *holiday framing* condition. It takes a value of zero otherwise. The *Nov., Jan., or Feb. birthday framing (birthday cohort one)* indicator takes a value of one if the employee was assigned to the *birthday framing* condition and had a birthday in November, January, or February, and the indicator takes a value of zero otherwise. The *Dec. or Mar. birthday framing (birthday cohort one)* indicator takes a value of one if the employee was assigned to the *birthday framing* condition and had a birthday in December or March, and the indicator takes a value of zero otherwise.

The *fresh start holiday framing (birthday cohort two)* indicator takes a value of one if the employee was in birthday cohort two and was assigned to the New Year's *holiday framing* condition or the first day of spring *holiday framing* condition. It takes a value of zero otherwise.

The coefficient of interest in the second regression is θ, which tells us the effect of placebo holiday framing relative to framing that does not reference temporal landmarks. Like the coefficient of interest from regression equation (1), θ is a weighted average of several comparisons: Employees in the Thanksgiving *holiday framing* condition, the Martin Luther King Day *holiday framing* condition, or the Valentine's Day *holiday framing* condition are compared to employees in *control* conditions with equivalent lengths of delay. As with the first regression equation, there are two benefits to setting up the second regression equation so that the coefficient θ is the weighted average of several comparisons. The statistical test of the hypothesis that the coefficient θ is zero provides a single statistical test for whether there is an effect of placebo holiday framing relative to stating the number of months of delay. In addition, combining several comparisons into one estimate increases the statistical power of the hypothesis test.

We also set up a third regression equation to estimate the effects of fresh start framing relative to placebo holiday framing:

$$(3)\quad outcome_i = \mu + \nu\, fresh\ start\ framing\ (modified)_i + \pi_1\, landmark\ framing\ (birthday\ cohort\ one)_i + \pi_2\, fresh\ start\ holiday\ framing\ (birthday\ cohort\ one)_i + \pi_3\, Dec.\ or\ Mar.\ birthday\ framing\ (birthday\ cohort\ one)_i + \pi_4\, holiday\ framing\ (birthday\ cohort\ two)_i + \pi_5\, 3\text{-}month\ delay\ (birthday\ cohort\ two)_i + \pi_6\, 4\text{-}month\ delay\ (birthday\ cohort\ two)_i + \pi_7\, 5\text{-}month\ delay\ (birthday\ cohort\ two)_i + \pi_8\, 6\text{-}month\ delay\ (birthday\ cohort\ two)_i + \rho' X_i + \xi_i.$$

As in equations (1) and (2), i indexes employees, and *outcome* is one of the four outcome variables. The *Y-month delay (birthday cohort two)* indicators and X_i are the same as in equations (1) and (2). We again report heteroskedasticity-robust standard errors.

The first predictor variable, the *fresh start framing (modified)* indicator, is a modified version of the *fresh start framing* indicator from equation (1). Specifically, for employees with birthdays in December or March who were in the New Year's *holiday framing* condition, the first

[11] We calculate decile breakpoints separately for each university. As explained earlier, University D sent mailings to employees who were not enrolled in the targeted plan as well as employees who were enrolled but not contributing sufficiently in the targeted plan to obtain the full employer match. We interact indicator variables for birth month, gender, age deciles, tenure deciles, salary deciles, and faculty status with two University D indicator variables, one for employees who were not enrolled in the targeted plan and a second for employees who were enrolled but at a low contribution rate. We also calculate separate decile breakpoints for these two groups of employees.

day of spring *holiday framing* condition, or the *birthday framing* condition, the *fresh start framing (modified)* indicator takes a value of zero (instead of one as for the *fresh start framing* indicator from equation (1)). We make this modification because employees with birthdays in December or March were never assigned to an experimental condition featuring placebo holiday framing and cannot contribute to estimating the effect of fresh start framing relative to placebo holiday framing.

The next three predictor variables only take non-zero values for employees in birthday cohort one. The *landmark framing (birthday cohort one)* indicator takes a value of one if the employee was in birthday cohort one and was assigned to either a *holiday framing* condition (regardless of whether the holiday is a placebo temporal landmark or a new-beginning temporal landmark) or the *birthday framing* condition. It takes a value of zero otherwise. The *fresh start holiday framing (birthday cohort one)* and *Dec. or Mar. birthday framing (birthday cohort one)* indicators are defined in the same way as in regression equation (2). We use these two indicators to account for employees who had birthdays in December or March and were assigned to the New Year's *holiday framing* condition, the first day of spring *holiday framing* condition, or the *birthday framing* condition, so these employees do not influence the coefficient estimate for the *fresh start framing (modified)* indicator.

The *holiday framing (birthday cohort two)* indicator only takes on non-zero values for employees in birthday cohort two. It takes a value of one if the employee was in birthday cohort two and was assigned to a *holiday framing* condition, and it takes a value of zero otherwise.

The coefficient of interest in regression (3) is ν, which tells us the extent to which the effect of fresh start framing exceeds the effect of placebo holiday framing. It is a weighted average of several comparisons. In birthday cohort one, employees with birthdays in November, January, or February who were in the *birthday framing* condition are compared to employees with birthdays in November, January, or February who were in the Thanksgiving *holiday framing* condition, the Martin Luther King Day *holiday framing* condition, or the Valentine's Day *holiday framing* condition. In birthday cohort two, the comparison is the difference between: (a) the effect of being in the New Year's or the first day of spring *holiday framing* conditions versus being in the three-month delay or the six-month delay *control* conditions, and (b) the effect of being in the Thanksgiving, the Martin Luther King Day, or the Valentine's Day *holiday framing* conditions versus being in the two-month delay, the four-month delay, or the five-month delay *control* conditions. The statistical test of the hypothesis that the coefficient ν is zero provides a single statistical test of the effect of fresh start framing relative to the effect of placebo holiday framing, and combining several comparisons increases the statistical power of the hypothesis test.

4. Results of the field experiment

4.1. Employee characteristics and balance checks

Table 2 summarizes the characteristics of the 6,082 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.

We perform balance checks separately for birthday cohort one and birthday cohort two, comparing groups of employees who should be similar according to our randomization scheme. Table 2 reports *p*-values from pairwise statistical tests, as described in the table caption. Randomization appears to have been successful, as only a small number of *p*-values (precisely, 4 out of 54 comparisons), and no more than would be expected by chance, indicate even a marginally statistically significant difference.

4.2. The effect of fresh start framing relative to stating the number of months of delay

In columns 1 and 2 of Table 3, we report the results from estimating regression equation (1) with the indicator for take-up of the delayed savings opportunity in the targeted plan as the outcome variable. In column 1, which limits the set of control variables to the interactions between university indicators and birth month indicators, we find that fresh start framing increased take-up of the delayed savings opportunity by 1.4 percentage points ($p < 0.05$) relative to framing that simply stated the number of months of delay before contributions will be increased. In column 2, which includes the full set of control variables, the estimated effect is 1.3 percentage points ($p < 0.05$). These effects amount to a 54% or 50% increase, respectively, relative to the 2.6 percent of employees in the *control* condition who took up the delayed savings opportunity. This finding represents the first affirmative evidence from a consequential field setting that fresh start framing can increase take-up of programs that facilitate pursuit of long-term goals.

In our field experiment, fresh start framing made the delayed savings opportunity more attractive and might therefore be expected to decrease take-up of the immediate savings opportunity as the immediate savings opportunity became relatively less attractive. On the other hand, as described in Section 2, the theory of fresh starts suggests that there are channels by which framing the delayed savings opportunity as a fresh start might *increase* take-up of the immediate savings opportunity. Columns 3 and 4 of Table 3 report the results from regression equation (1) with the indicator for take-up of the immediate savings opportunity in the targeted plan as the outcome variable. Column 3 uses the limited set of control variables, and column 4 uses the full set of control variables. In both specifications, the point estimate for the effect of fresh start framing (relative to framing that states the number of months of delay) is positive but not statistically significant, suggesting that fresh start framing for the delayed savings opportunity does not decrease take-up of the immediate savings opportunity. In column 4, which includes all control variables, the 95% confidence interval for the fresh start framing effect excludes negative effects larger in magnitude than −0.6 percentage points. If we take a Bayesian perspective, Bayes factor calculations indicate that the likelihood of the data being generated by the null model, under which fresh start framing has no effect, is 6 times (in the specification with the limited set of control variables) or 5 times (in the specification with the full set of control variables) as high as the likelihood of the data being generated by the alternative model.[12] That is, we have modest evidence that the null model is more reasonable than the alternative model (Jeffreys, 1961), suggesting that stronger evidence from additional data would be desirable for us to understand how fresh start framing for the delayed savings opportunity affects take-up of the immediate savings opportunity. Thus, we test the possibility that fresh start framing for the delayed savings opportunity might have the potential to *increase* take-up of immediate opportunities to pursue a savings goal in more detail in Section 5 via a high-power laboratory experiment.

In columns 5–8 of Table 3, we use regression Eq. (1) to examine the effect of fresh start framing on our final two outcome variables. The outcome variable in columns 5 and 6 is the indicator for take-up of either the immediate savings opportunity or the delayed savings opportunity in the targeted plan, and the outcome variable in columns 7 and 8 is the

[12] For these calculations, we use the "BayesFactor" R package. We first apply the "lmBF" program to calculate the Bayes factor for the model in regression equation (1) excluding the *fresh start framing* indicator relative to a model with only a constant, using the prior distributions recommended by Liang, Paulo, Molina, Clyde, and Berger (2008). Then we apply the "lmBF" program to calculate the Bayes factor for the model in regression equation (1) including the *fresh start framing* indicator relative to a model with only a constant, again using the recommended prior distributions (Liang et al., 2008). Finally, we divide the first Bayes factor by the second (Rouder & Morey, 2012).

Table 2

Summary Statistics. This table summarizes key sample characteristics across experimental conditions. The top panel focuses on employees in birthday cohort one divided into three groups: those in the *control* conditions, those in the *holiday framing* conditions, and those in the *birthday framing* condition. The bottom panel focuses on employees in birthday cohort two divided into four groups: those in the two-month delay, four-month delay, and five-month delay *control* conditions, which are the time delays matched to placebo holidays; those in the Thanksgiving, Martin Luther King Day, and Valentine's Day *holiday framing* conditions, which are the placebo holidays; those in the three-month delay and six-month delay *control* conditions, which are matched to fresh start holidays; and those in the New Year's and first day of spring *holiday framing* conditions, which are the holidays associated with fresh starts. The left-hand-side columns show sample proportions for dichotomous variables and sample means for continuous variables. The right-hand-side columns show *p*-values from pairwise tests of two proportions for dichotomous variables and two-sample *t*-tests for continuous variables.

Employees in birthday cohort one (birthdays between November and March)						
	Control(1)	Holiday Framing(2)	Birthday Framing(3)	1 vs. 2	1 vs. 3	2 vs. 3
				p-value		
Female	51.85%	50.11%	52.97%	0.37	0.56	0.23
Age (years)	43.00	43.54	43.66	0.24	0.15	0.83
Tenure (years)	9.54	10.00	9.18	0.19	0.29	0.05
Baseline Salary (USD)	58505.26	58666.92	57862.67	0.91	0.65	0.65
Log Baseline Salary (USD)	10.69	10.67	10.67	0.66	0.71	0.95
Faculty	12.75%	11.96%	12.21%	0.54	0.67	0.88

Employees in birthday cohort two (birthdays between April and October)										
	Control Matched to Placebo Holidays(1)	Placebo Holiday Framing(2)	Control Matched to Fresh Start Holidays(3)	Fresh Start Holiday Framing(4)	1 vs. 2	1 vs. 3	1 vs. 4	2 vs. 3	2 vs. 4	3 vs. 4
					p-value					
Female	50.82%	50.00%	52.63%	47.95%	0.73	0.46	0.24	0.28	0.40	0.05
Age (years)	42.56	43.58	42.68	42.98	0.08	0.84	0.45	0.12	0.30	0.59
Tenure (years)	9.20	9.73	9.54	9.17	0.23	0.42	0.95	0.68	0.21	0.39
Baseline Salary (USD)	58407.85	60817.70	58908.02	60799.07	0.19	0.77	0.21	0.31	0.99	0.34
Log Baseline Salary (USD)	10.78	10.72	10.66	10.70	0.35	0.07	0.22	0.38	0.78	0.55
Faculty	11.50%	13.58%	14.12%	13.36%	0.19	0.11	0.24	0.75	0.89	0.65

Table 3

The Effect of Fresh Start Framing on Take-up of Proffered Savings Opportunities and Average Contribution Rates in the Targeted Plan. This table reports the results of ordinary least squares (OLS) regressions. In columns 1 and 2, the dependent variable is an indicator that takes a value of one if the individual's contribution rate in the targeted plan increased in the month that was offered in the individual's mailing as a future opportunity to increase savings. In columns 3 and 4, the dependent variable is an indicator that takes a value of one if the individual's contribution rate in the targeted plan increased in the month immediately after the mailing was received. In columns 5 and 6, the dependent variable is an indicator that takes a value of one if the individual's contribution rate in the targeted plan increased in the month immediately after the mailing was received or increased in the month that was offered in the individual's mailing as a future opportunity to increase savings. Finally, in columns 7 and 8, the dependent variable is the average contribution rate to the targeted plan during November 2013 through June 2014, in units of percentage points of pay. The sample includes employees in the *control*, *holiday framing*, and *birthday framing* conditions. The explanatory variables are listed in the table. The regression specification is also displayed as regression equation (1).

Dependent variable	Delayed Option Take-up		Immediate Option Take-up		Immediate or Delayed Option Take-up		Average Contribution Rates, November-June	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fresh start framing	0.014**	0.013**	0.008	0.010	0.022**	0.023**	0.171**	0.188**
	(0.006)	(0.006)	(0.008)	(0.008)	(0.010)	(0.010)	(0.080)	(0.084)
Birthday cohort one								
Placebo holiday framing	−0.004	−0.005	−0.009	−0.012	−0.013	−0.016	0.006	0.021
	(0.007)	(0.007)	(0.013)	(0.013)	(0.014)	(0.015)	(0.113)	(0.113)
Birthday cohort two								
Placebo holiday framing	−0.010	−0.009	−0.007	−0.005	−0.014	−0.012	−0.063	−0.038
	(0.007)	(0.007)	(0.011)	(0.011)	(0.012)	(0.012)	(0.100)	(0.101)
3-month delay	0.013	0.013	−0.016	−0.016	0.013	0.012	−0.171	−0.167
	(0.010)	(0.010)	(0.014)	(0.014)	(0.016)	(0.016)	(0.111)	(0.115)
4-month delay	−0.005	−0.006	−0.013	−0.013	−0.002	−0.004	−0.056	−0.052
	(0.009)	(0.009)	(0.013)	(0.014)	(0.015)	(0.015)	(0.128)	(0.133)
5-month delay	−0.010	−0.009	−0.011	−0.010	−0.008	−0.006	−0.043	−0.033
	(0.008)	(0.008)	(0.013)	(0.013)	(0.015)	(0.015)	(0.124)	(0.127)
6-month delay	−0.011	−0.010	0.001	0.002	0.009	0.012	0.057	0.113
	(0.010)	(0.010)	(0.016)	(0.016)	(0.018)	(0.018)	(0.166)	(0.172)
Control variables								
university × demographics (female, age decile, tenure decile, salary decile, faculty status)	No	Yes	No	Yes	No	Yes	No	Yes
university × birth month	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,613	5,613	5,718	5,718	5,573	5,573	6,082	6,082

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

Columns 1–2 exclude 469 employees who do not have data for a month that is needed for constructing delayed option take-up. Columns 3–4 exclude 364 employees who do not have data for a month that is needed for constructing immediate option take-up. Columns 5–6 exclude 509 employees who do not have data for a month that is needed for constructing immediate or delayed option take-up.

employee's average contribution rate to the targeted plan from November 2013 through June 2014. Columns 5 and 7 use the limited set of controls, and columns 6 and 8 use the full set of controls. We find that fresh start framing, relative to stating the number of months of delay, elevated the likelihood of immediate or delayed option take-up by 2.2 percentage points in the regression with the limited set of controls ($p < 0.05$) and by 2.3 percentage points in the regression with the full set of controls ($p < 0.05$). Those effects correspond to a 29–30% increase relative to the proportion of employees in the control condition who took up either the immediate or delayed savings opportunity (7.6 percent). Fresh start framing produced an increase of 17 basis points on employees' average savings contribution rates over the eight months following the launch of our experiment in the regression with the limited set of controls ($p < 0.05$) and an increase of 19 basis points in the regression with the full set of controls ($p < 0.05$). This corresponds to a 23–26% increase compared to the average savings contribution rate in the control condition (which was 73 basis points). Thus, from a policy perspective, the evidence indicates that fresh start framing holds promise as a technique for increasing retirement savings accumulation.

To investigate the robustness of the results in Table 3, we repeat the analyses with a variety of adjustments. In Online Appendix Table D1, we replace the outcome variables measuring savings decisions in the targeted plan with analogous outcome variables that combine data across all available plans (including both targeted plans and non-targeted plans). In Online Appendix Table D2, we use logit and fractional logit regressions, as appropriate, instead of ordinary least squares regressions. The results are robust to these adjustments, although in two out of 12 cases, coefficient estimates for the *fresh start framing* indicator that are statistically significant at the 5% level in Table 3 become statistically significant only at the 10% level. In additional analyses (detailed results available from the authors upon request), we also find that the results are robust to dropping employees who have missing data on salary or contributions for a subset of pay periods during our observation window. In another set of extra analyses that are available upon request, we find that the results for the average contribution rate outcome variable are robust if we instead use the number of dollars contributed (i.e., without dividing by pay) during the follow-up period as the outcome variable.

Finally, regression equation (1) pools individuals who received various forms of fresh start framings into a single group (for whom the *fresh start framing* indicator takes a value of one). However, we can also explore the effects of fresh start framing tied to specific fresh start dates. We repeat the analyses presented in Table 3 using a modified version of regression equation (1). We describe the equation in detail in the caption to Online Appendix Fig. D1, which shows the point estimates and 95% confidence intervals for the treatment effects of birthday framing and framings linked to each individual holiday. The large confidence intervals indicate that this analysis has limited statistical power and should be interpreted with caution, but the pattern of point estimates is suggestive. Across the four outcome variables, the point estimates suggest that framing related to birthdays and the first day of spring—two temporal landmarks associated with fresh starts—tended to produce the largest positive effects on savings. Interestingly, contrary to our prior expectations, the effects of framing related to New Year's had point estimates close to zero.[13] Framing related to Valentine's Day tended to produce negative effects on savings, perhaps because Valentine's Day is associated with spending (e.g., purchasing gifts) and not with saving.

4.3. The effect of placebo holiday framing relative to stating the number of months of delay and the effect of fresh start framing relative to placebo holiday framing

The results presented in Table 3 indicate that framing a delayed savings opportunity as a fresh start in our field experiment had a positive effect on retirement savings plan contributions relative to simply stating the number of months of delay before contributions would be increased. We use regression equation (2) to explore whether similar results would hold for framing that referred to *any* temporal landmark. Regression equation (2) is constructed to estimate the effects of framing associated with three placebo holidays that are not linked to fresh starts: Thanksgiving, Martin Luther King Day, and Valentine's Day. In Table 4, we report the results of regressions that use equation (2) with the same outcome variables studied in Table 3. None of the estimated coefficients for the *placebo holiday framing* indicator are statistically significant.[14] Of course, these results are contingent on the specific placebo holidays tested in our field experiment. As suggested in our discussion of Online Appendix Fig. D1, framing related to Valentine's Day may actively *discourage* savings because Valentine's Day is more closely associated with spending. Nonetheless, the findings in Table 4 indicate that framing related to temporal landmarks does not necessarily produce the positive effects on savings that we observed with fresh start framing.

Table 5 presents the results of regressions that use equation (3) to estimate the effect of fresh start framing relative to placebo holiday framing. Fresh start framing increased the likelihood of taking up the delayed savings opportunity by 1.8 percentage points in the regression with the limited set of control variables ($p < 0.05$) and by 1.7 percentage points in the regression with the full set of control variables ($p < 0.05$). These effects amount to a 131%-138% increase in take-up compared to employees who received the placebo holiday framing (1.3 percent of whom took up the delayed savings opportunity).

The point estimates for the effect on the likelihood of taking up the immediate savings opportunity are positive but not statistically significant.[15] The estimated effect on the likelihood of taking up the immediate savings opportunity or the delayed savings opportunity is an increase of 3.1 percentage points both with the limited set of control variables ($p < 0.05$) and with the full set of control variables ($p < 0.05$). These effects correspond to a 55% increase in take-up of the immediate or delayed savings opportunity (pooled) compared to employees who received the placebo holiday framing (5.6 percent of whom took up either the immediate or delayed savings opportunity). During the November-June observation window, fresh start framing increased employees' average contribution rates by 22 basis points both in the regression with the limited set of controls ($p < 0.05$) and in the regression with the full set of controls ($p < 0.10$). This amounts to a 31% increase in average contribution rates when compared with the average contribution rates among employees who received the placebo holiday framing (a population that contributed an average of 72 basis points of their income to savings during our study period). These results suggest that fresh start framing led to greater retirement plan contributions than framing linked to the placebo holidays tested in our field experiment.

Taken together, the results of our field experiment demonstrate the

[13] According to Bayes factor calculations, the likelihood of the data being generated by the model that excludes the effect of New Year's framing is 7–9 times (depending on the outcome variable) as high as the likelihood of the data being generated by the model that includes the effect of New Year's framing. See footnote 12 for an explanation of our methodology for calculating Bayes factors.

[14] Bayes factors indicate that the likelihood of the data being generated by the model that excludes the effect of placebo holiday framing is 5–9 times (depending on the outcome variable) as high as the likelihood of the data being generated by the model that includes the effect of placebo holiday framing. See footnote 12 for an explanation of our methodology for calculating Bayes factors.

[15] Bayes factors indicate that the likelihood of the data being generated by the model that excludes the effect of fresh start framing relative to placebo holiday framing is 3–4 times (depending on the specification) as high as the likelihood of the data being generated by the model that includes the effect of fresh start framing relative to placebo holiday framing. See footnote 12 for an explanation of our methodology for calculating Bayes factors.

Table 4

The Effect of Placebo Holiday Framing on Take-up of Proffered Savings Opportunities and Average Contribution Rates in the Targeted Plan. This table reports the results of ordinary least squares (OLS) regressions. In columns 1 and 2, the dependent variable is an indicator that takes a value of one if the individual's contribution rate in the targeted plan increased in the month that was offered in the individual's mailing as a future opportunity to increase savings. In columns 3 and 4, the dependent variable is an indicator that takes a value of one if the individual's contribution rate in the targeted plan increased in the month immediately after the mailing was received. In columns 5 and 6, the dependent variable is an indicator that takes a value of one if the individual's contribution rate in the targeted plan increased in the month immediately after the mailing was received or increased in the month that was offered in the individual's mailing as a future opportunity to increase savings. Finally, in columns 7 and 8, the dependent variable is the average contribution rate to the targeted plan during November 2013 through June 2014, in units of percentage points of pay. The sample includes employees in the *control*, *holiday framing*, and *birthday framing* conditions. The explanatory variables are listed in the table. The regression specification is also displayed as regression equation (2).

Dependent variable	Delayed Option Take-up		Immediate Option Take-up		Immediate or Delayed Option Take-up		Average Contribution Rates, November-June	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Placebo holiday framing	−0.008	−0.008	−0.005	−0.005	−0.012	−0.011	−0.019	−0.005
	(0.005)	(0.005)	(0.009)	(0.009)	(0.009)	(0.010)	(0.076)	(0.078)
Birthday cohort one								
Fresh start holiday framing	0.016	0.020	−0.001	0.003	0.018	0.024	−0.015	0.021
	(0.013)	(0.013)	(0.017)	(0.017)	(0.020)	(0.021)	(0.167)	(0.168)
November, January, or February	0.011	0.011	0.017	0.023	0.028*	0.035**	0.225	0.216
birthday framing	(0.009)	(0.009)	(0.015)	(0.015)	(0.016)	(0.016)	(0.138)	(0.138)
December or March birthday framing	0.031**	0.034**	0.021	0.027	0.057**	0.065***	0.144	0.193
	(0.014)	(0.014)	(0.018)	(0.018)	(0.023)	(0.023)	(0.175)	(0.172)
Birthday cohort two								
Fresh start holiday framing	0.010	0.007	0.001	−0.001	0.007	0.002	0.177	0.201
	(0.010)	(0.010)	(0.011)	(0.011)	(0.015)	(0.015)	(0.127)	(0.133)
3-month delay	0.016	0.017	−0.012	−0.010	0.022	0.023	−0.152	−0.157
	(0.011)	(0.011)	(0.014)	(0.014)	(0.017)	(0.017)	(0.121)	(0.124)
4-month delay	−0.005	−0.006	−0.013	−0.013	−0.002	−0.004	−0.056	−0.052
	(0.009)	(0.009)	(0.013)	(0.014)	(0.015)	(0.015)	(0.128)	(0.133)
5-month delay	−0.010	−0.009	−0.011	−0.010	−0.008	−0.006	−0.043	−0.033
	(0.008)	(0.008)	(0.013)	(0.013)	(0.015)	(0.015)	(0.124)	(0.127)
6-month delay	−0.008	−0.005	0.005	0.008	0.018	0.022	0.075	0.122
	(0.011)	(0.011)	(0.016)	(0.016)	(0.018)	(0.018)	(0.154)	(0.159)
Control variables								
university × demographics (female, age decile, tenure decile, salary decile, faculty status)	No	Yes	No	Yes	No	Yes	No	Yes
university × birth month	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,613	5,613	5,718	5,718	5,573	5,573	6,082	6,082

Standard errors robust to heteroskedasticity in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Columns 1–2 exclude 469 employees who do not have data for a month that is needed for constructing delayed option take-up. Columns 3–4 exclude 364 employees who do not have data for a month that is needed for constructing immediate option take-up. Columns 5–6 exclude 509 employees who do not have data for a month that is needed for constructing immediate or delayed option take-up.

benefits of framing that associates future savings opportunities with fresh start dates.

5. Follow-up laboratory experiment

The results of our field experiment indicated that framing a future savings opportunity around a fresh start increased take-up of that future opportunity. Interestingly, even though the relatively greater attractiveness of the future opportunity caused by fresh start framing might be expected to decrease take-up of the immediate savings opportunity, our results suggest that such a decrease did not occur. To further investigate this latter finding and to establish that applying fresh start framing to a future savings opportunity might in some situations *increase* take-up of an immediate savings opportunity, we conducted a pre-registered laboratory experiment.[16]

5.1. Material and methods for the laboratory experiment

We recruited participants through Amazon's Mechanical Turk to take a short survey in exchange for $0.85. Following our pre-registered inclusion criteria, the 2,056 participants ($M_{age} = 40.6$, $SD_{age} = 12.9$; 48.7% male, 50.6% female) who accessed our study on a non-mobile device and met the following conditions were included in our study: they successfully completed a CAPTCHA, passed an attention check, and answered comprehension check questions correctly. All screening questions were asked before we collected our dependent variables. If a participant completed the survey more than once, we only retained their first complete response.

All 2,056 participants in our study were asked to imagine that a person named Emily with a birthday in January, February, or March (randomly assigned) works for a large employer with an attractive retirement savings program. She has not enrolled in this program. Her employer plans to send employees like Emily a mailing encouraging them to enroll, but the employer has not decided on the design of the mailing. Two different designs are under consideration. Participants were then shown two candidate mailings: a *fresh start* mailing and a *control* mailing. The *fresh start* mailing was a simplified version of the *birthday framing* mailing from our field experiment, adapted such that all references to universities and their specific retirement savings plans were replaced with references to a hypothetical company's retirement savings program. This mailing would offer Emily two options: the option to start contributing to her company's retirement savings program immediately and the option to start contributing after her next birthday. The *control* mailing was a simplified version of the *control* mailing from our field experiment, again adapted such that all references to

[16] See https://aspredicted.org/pu2gh.pdf.

Table 5

The Effect of Fresh Start Framing, Relative to Placebo Holiday Framing, on Take-up of Proffered Savings Opportunities and Average Contribution Rates in the Targeted Plan. This table reports the results of ordinary least squares (OLS) regressions. In columns 1 and 2, the dependent variable is an indicator that takes a value of one if the individual's contribution rate in the targeted plan increased in the month that was offered in the individual's mailing as a future opportunity to increase savings. In columns 3 and 4, the dependent variable is an indicator that takes a value of one if the individual's contribution rate in the targeted plan increased in the month immediately after the mailing was received. In columns 5 and 6, the dependent variable is an indicator that takes a value of one if the individual's contribution rate in the targeted plan increased in the month immediately after the mailing was received or increased in the month that was offered in the individual's mailing as a future opportunity to increase savings. Finally, in columns 7 and 8, the dependent variable is the average contribution rate to the targeted plan during November 2013 through June 2014, in units of percentage points of pay. The sample includes employees in the *control*, *holiday framing*, and *birthday framing* conditions. The explanatory variables are listed in the table. The regression specification is also displayed as regression equation (3). Note that the *fresh start framing* indicator is modified such that it equals zero (instead of one) for individuals with birthdays in December or March who received fresh start framing (because there is not a *placebo holiday framing* condition against which they can be compared).

Dependent variable	Delayed Option Take-up		Immediate Option Take-up		Immediate or Delayed Option Take-up		Average Contribution Rates, November-June	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fresh start framing (modified)	0.018**	0.017**	0.016	0.017	0.031**	0.031**	0.222**	0.215*
	(0.007)	(0.007)	(0.011)	(0.011)	(0.013)	(0.013)	(0.108)	(0.111)
Birthday cohort one								
Landmark framing	−0.005	−0.006	−0.001	0.000	−0.005	−0.003	0.045	0.038
	(0.007)	(0.007)	(0.014)	(0.014)	(0.015)	(0.015)	(0.114)	(0.117)
Fresh start holiday framing	0.022	0.025*	0.000	0.002	0.023	0.027	−0.059	−0.018
	(0.015)	(0.015)	(0.022)	(0.022)	(0.025)	(0.025)	(0.202)	(0.207)
December or March birthday framing	0.036**	0.040**	0.021	0.026	0.062**	0.068**	0.099	0.154
	(0.016)	(0.016)	(0.023)	(0.023)	(0.027)	(0.027)	(0.209)	(0.211)
Birthday cohort two								
Holiday framing	−0.009	−0.010	−0.010	−0.011	−0.019*	−0.021*	−0.054	−0.026
	(0.006)	(0.006)	(0.009)	(0.010)	(0.011)	(0.011)	(0.090)	(0.090)
3-month delay	0.016	0.016	−0.017	−0.017	0.016	0.014	−0.166	−0.161
	(0.010)	(0.010)	(0.013)	(0.013)	(0.016)	(0.016)	(0.115)	(0.119)
4-month delay	−0.005	−0.006	−0.013	−0.013	−0.002	−0.004	−0.056	−0.052
	(0.009)	(0.009)	(0.013)	(0.014)	(0.015)	(0.015)	(0.128)	(0.133)
5-month delay	−0.010	−0.009	−0.011	−0.010	−0.008	−0.006	−0.043	−0.033
	(0.008)	(0.008)	(0.013)	(0.013)	(0.015)	(0.015)	(0.124)	(0.127)
6-month delay	−0.008	−0.007	0.001	0.001	0.012	0.013	0.062	0.118
	(0.010)	(0.010)	(0.015)	(0.015)	(0.018)	(0.018)	(0.169)	(0.175)
Control variables								
university × demographics (female, age decile, tenure decile, salary decile, faculty status)	No	Yes	No	Yes	No	Yes	No	Yes
university × birth month	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,613	5,613	5,718	5,718	5,573	5,573	6,082	6,082

Standard errors robust to heteroskedasticity in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Columns 1–2 exclude 469 employees who do not have data for a month that is needed for constructing delayed option take-up. Columns 3–4 exclude 364 employees who do not have data for a month that is needed for constructing immediate option take-up. Columns 5–6 exclude 509 employees who do not have data for a month that is needed for constructing immediate or delayed option take-up.

universities and their specific retirement savings plans were replaced with references to a hypothetical company's retirement savings program. This mailing would offer Emily two options: the option to start contributing to the company's retirement savings program immediately and the option to start contributing in Y months, where Y aligned with Emily's birth month. More precisely, because the experiment took place in October, Y was 4 when Emily's birthday was in January, 5 when Emily's birthday was in February, and 6 when Emily's birthday was in March. Participants were reminded that the *fresh start* mailing and the *control* mailing were actually identical in terms of the enrollment options they provided.

We randomized and counterbalanced the order in which the two mailings were presented to participants, and we used the labels "Mailing 1" and "Mailing 2" when referring to the mailings. After participants read the two mailings, we asked them a series of comprehension check questions to assess their understanding of (a) what options Emily would have if she received either mailing, (b) Emily's birth month, and (c) what organization was deciding which mailing to send to Emily. Only participants who correctly answered our comprehension check questions went on to complete our study and comprised our final sample.

After participants successfully completed the comprehension check questions, they were asked to predict the relative impact of Mailing 1 and Mailing 2 on Emily's willingness to enroll in her employer's retirement savings program. Participants were randomly assigned to make a prediction about either take-up of the immediate enrollment opportunity or take-up of the future enrollment opportunity. Specifically, half of participants were asked: "Which of the above mailings do you think would be more likely to convince Emily to respond to the mailing and elect to enroll in the retirement savings program now?" The other half of participants were asked: "Which of the above mailings do you think would be more likely to convince Emily to respond to the mailing and elect to enroll in the retirement savings program in Y months, which is after her next birthday?" (where Y aligned with Emily's birth month, as described above). Participants chose one of the following five possible responses: (1) "Mailing 1 is definitely more convincing"; (2) "Mailing 1 is somewhat more convincing"; (3) "Mailing 1 and Mailing 2 are equally convincing"; (4) "Mailing 2 is somewhat more convincing"; or (5) "Mailing 2 is definitely more convincing."

Next, participants were asked to explain why they believed the mailing of their choice (Mailing 1 or Mailing 2) would be more convincing or why they believed the two mailings would be equally convincing (if this is what they had indicated). Finally, participants were asked whether the mailings loaded properly on their screens and whether they could easily read the contents of the mailings, along with their age, gender, and education. Only a small fraction of our participants reported any difficulties with images loading (1.14%) or with reading mailings (4.15%). Following our pre-registration, no one was excluded from our analyses for these reasons. See Online Appendix E for

our complete study materials.

5.2. Results of the laboratory experiment

Following our pre-registration, we coded the response scales for our dependent variables such that a high rating (above 3) consistently indicates that the *fresh start* mailing was viewed as more convincing than the *control* mailing, while a low rating (below 3) consistently indicates that the *fresh start* mailing was viewed as less convincing than the *control* mailing. We run one-sample *t*-tests to compare participants' response with 3, the mid-point of the scale. Participants indicated that the *fresh start* mailing was more likely than the *control* mailing to convince Emily to take up the future retirement savings program enrollment option (M = 3.26, SD = 1.15; $t(1025) = 7.16$; $p < 0.0001$). Importantly, participants also indicated that the *fresh start* mailing was more likely than the *control* mailing to convince Emily to take up the immediate retirement savings program enrollment option (M = 3.13, SD = 1.14; $t(1029) = 3.65$; $p = 0.0003$). These findings suggest that framing a future savings opportunity around a fresh start date could not only boost take-up of the delayed opportunity but also enhance the attractiveness of the immediate opportunity.

6. Discussion and conclusions

In a large field experiment, we show that fresh start framing is a tool that can be used to nudge meaningful increases in savings. Specifically, we find that describing an upcoming opportunity to save in terms of its proximity to a fresh start date (e.g., the first day of spring, an upcoming birthday) increases take-up of that future savings opportunity and increases retirement plan contributions overall relative to describing the same upcoming savings opportunity as arriving after an equivalent time delay (e.g., "in 2 months"). This result is not purely driven by the mention of a temporal landmark or a concrete date, as we do not find the same effects when we describe an upcoming savings opportunity in terms of its proximity to a placebo holiday (namely, Thanksgiving, Martin Luther King Day, or Valentine's Day). The primary contribution of this paper is that it represents the first affirmative experimental demonstration of the positive effects of fresh start framing in a consequential field setting, illustrating the potential of this nudge as a means of promoting goal-directed behavior.

Also, even though we might expect framing a future savings opportunity around a fresh start date to decrease the relative attractiveness of an immediate savings opportunity and therefore decrease take-up of the immediate savings opportunity, we do not find evidence of such a decrease in the data from our field experiment. Further, participants in a follow-up laboratory experiment judged framing a future savings opportunity around a fresh start as likely to *increase* take-up of an immediate savings opportunity. Thus, an additional contribution of this paper is that it extends the theory of fresh starts to consider the impact of future fresh starts on immediate motivation to initiate goal pursuit.

Although it is difficult to use the data from our field experiment to differentiate among the many mechanisms theorized to contribute to the effects of fresh start framing, it is interesting to note that our findings are particularly consistent with two theories. These theories can explain both why framing a future savings opportunity around a fresh start increases take-up of that future savings opportunity and why this framing of future savings opportunities might not decrease take-up of an immediate savings opportunity. First, framing a future savings opportunity around a fresh start may increase not only the salience of that future savings opportunity but also the salience of other savings opportunities, including an immediate savings opportunity (Robinson, 1986; Rubin & Kozin, 1984; Shum, 1998; Soster et al., 2010). Second, construal level theory (Trope & Liberman, 2003) would suggest that our fresh start framing conditions led participants to focus on the big picture and think about their savings opportunity at a higher construal level, making goal pursuit more attractive and therefore making both immediate and future contribution rate increases more attractive. Future research should continue to examine the mechanisms through which fresh start framing impacts decision making in order to better predict fresh start framing effects.

It would also be valuable for future research to consider how the effects of fresh start framing vary across distinct populations of individuals. As a first step in this direction, we compare the effects of fresh start framing, relative to stating the number of months of delay, for the faculty versus the staff in our field experiment sample. An employee's status as a faculty member or a staff member is a proxy for education, and prior work has identified heterogeneous treatment effects by education level for retirement plan automatic enrollment, a leading example of a savings nudge (Beshears, Choi, Laibson, Madrian, & Skimmyhorn, in press). Online Appendix Table D3 reports the differences between the estimated fresh start framing effects for the faculty versus the staff. The only difference between these two sets of estimates that reaches at least marginal significance is that the effect of fresh start framing on average contribution rates is marginally statistically significantly higher for faculty than for staff. Our statistical power for detecting treatment effect heterogeneity in the field experiment data is somewhat limited, so future studies of treatment effect heterogeneity will likely require larger sample sizes.

Although our field experiment has many strengths, it also has a number of limitations. First, we had to make a number of compromises on our experimental design because our mailings were sent on a single date and invited employees to increase their savings at some point in the next six months. For instance, our experimental design does not allow us to examine how employees born between April and October respond to an opportunity to begin saving following an upcoming birthday because those employees' birthdays were so far off in the future at the time of our mailing. Also, our analyses are necessarily somewhat complex to account for the need to stratify our randomization scheme by birthday cohort. It would be valuable to address these limitations of our study in future research by offering opportunities to individuals to change future behavior on staggered dates over the course of an entire year.

Second, we predicted that it would be impactful to highlight New Year's as an opportunity to begin saving, but highlighting this particular fresh start date did not produce a noticeable increase in savings (highlighting the first day of spring and birthdays drove our effects). This was a surprise to us and one that we think is worthy of future exploration. It could have been due to noise in the estimation of treatment effects. It also could be that highlighting fresh start dates is less effective when people are more likely to *naturally* think to use those dates for goal pursuit without a prompt. This possibility aligns with our earlier proposition that the effects of fresh start framing operate at least in part through an attentional channel. Many individuals may naturally expect New Year's to serve as an opportunity for a new beginning; thus, highlighting this opportunity may not draw increased attention to the initiation of goal pursuit. Individuals may be less likely to view birthdays and the first day of spring as opportunities for new beginnings, so highlighting these opportunities may draw increased attention to goals.

Another possible reason why New Year's fresh start framing did not increase savings is that New Year's may not be a temporal landmark that is well-aligned with savings goals. This possibility, which suggests that the power of fresh start framing is moderated by the degree of alignment between the temporal landmark and the goal domain, points to intriguing avenues for future research. It would be interesting to use a single experimental framework to estimate fresh start framing effects across a series of goal domains, pairing each domain with the same succession of temporal landmarks. For example, fresh start framing related to New Year's and the first day of spring could be tested in the domains of savings and education. Our field experiment suggests that fresh start framing related to the first day of spring increases savings while framing related to New Year's does not. However, it may be the case that fresh start framing related to New Year's increases investment in education while framing related to the first day of spring does not,

given that New Year's often aligns with the beginning of an academic semester in the U.S. while the first day of spring does not. Such a finding would be of great interest for the purposes of further developing the theory of fresh starts and designing policies to promote goal pursuit.

CRediT authorship contribution statement

John Beshears: Conceptualization, Methodology, Software, Validation, Formal analysis, Investigation, Resources, Data curation, Writing - original draft, Writing - review & editing, Visualization, Supervision, Project administration, Funding acquisition. **Hengchen Dai:** Conceptualization, Methodology, Software, Validation, Formal analysis, Investigation, Resources, Data curation, Writing - original draft, Writing - review & editing, Visualization, Supervision, Project administration, Funding acquisition. **Katherine L. Milkman:** Conceptualization, Methodology, Software, Validation, Formal analysis, Investigation, Resources, Data curation, Writing - original draft, Writing - review & editing, Visualization, Supervision, Project administration, Funding acquisition. **Shlomo Benartzi:** Conceptualization, Methodology, Resources, Writing - review & editing, Supervision, Funding acquisition.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.obhdp.2021.06.005.

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