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**Save More Today or Tomorrow: The Role of Urgency in Pre-Commitment Design**

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| Journal:            | <i>Journal of Marketing Research</i>                                                                                                                                                                                                                                    |
| Manuscript ID       | JMR.18.0459.R4                                                                                                                                                                                                                                                          |
| Manuscript Type:    | Revised Submission                                                                                                                                                                                                                                                      |
| Topics and Methods: | Behavioral Economics < Theoretical Foundation, Consumer behavior < Theoretical Foundation, Behavioral Decision Theory < Theoretical Foundation, Field experiments < Method of discovery, Lab experiments < Method of discovery, Public policy, legal < Substantive area |
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# Save More Today or Tomorrow: The Role of Urgency in Pre-commitment Design

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**Acknowledgment:** We thank our partners at the universities and at the retirement savings plan administrator for implementing the field experiment. We also thank Alessandro Previtiero for his helpful comments and Hae Nim Lee, Predrag Pandiloski, and Christine Nguyen for excellent research assistance.

**Financial Disclosure:** This material is based upon work supported by the National Science Foundation Graduate Research Fellowship Program under Grant No. DGE-1650604. We acknowledge financial support from the National Institutes of Health (grants P01AG005842 and P30AG034532), the Social Security Administration (grant RRC08098400-06-00 to the National Bureau of Economic Research as part of the SSA Retirement Research Consortium), the Pension Research Council/Boettner Center, the TIAA Institute, the Wharton School, the Wharton Behavioral Lab, the UCLA Anderson School of Management, and the UCLA Behavioral Lab. The opinions and conclusions expressed are solely those of the authors and do not represent the opinions or policy of NIH, SSA, any agency of the Federal Government, the Pension Research Council/Boettner Center, the TIAA Institute, or the NBER. See the authors' websites for lists of outside activities.

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**Abstract:** To encourage farsighted behaviors, past research suggests that marketers may be wise to invite consumers to pre-commit to adopt them “later”. However, the authors propose that people will draw different inferences from different types of pre-commitment offers, and that these inferences can help explain when pre-commitment is effective at increasing adoption of farsighted behaviors and when it is not. Specifically, the authors theorize that simultaneously offering consumers the opportunity to adopt a farsighted behavior now or later (i.e., offering “simultaneous pre-commitment”) may signal that the behavior is not urgently recommended; however, offering consumers the opportunity to adopt that behavior immediately and then, only if they decline, inviting them to adopt it later (i.e., offering “sequential pre-commitment”) may signal just the opposite. In a multi-site field experiment (N=5,196), the authors find that simultaneously giving consumers the chance to increase their savings now or later *reduced* retirement savings. Two pre-registered lab studies (N=5,080) show that simultaneous pre-commitment leads people to infer that taking action is not urgently recommended, and such inferences predict less adoption of recommended behaviors. Importantly, offering sequential pre-commitment increases inferred urgency, predicting greater adoption. Together, this research advances knowledge about the limits and potential of pre-commitment.

**Keywords:** pre-commitment, inference making, farsighted decisions, choice architecture, field experiment

Consumers often face decisions about whether and when to engage in behaviors that have immediate costs and long-term benefits (e.g., saving for retirement, updating malware protection software, undergoing preventative health screenings, and receiving a vaccine). When invited to adopt such farsighted activities immediately, people often decline because the immediate costs loom large relative to the distant benefits (Frederick, Loewenstein, and O'Donoghue 2002). Marketers and policy makers seeking to encourage farsighted choices frequently offer “pre-commitment” as a solution: By inviting people to pre-commit to adopt the behavior in the future, it makes the costs feel less aversive, which extant theory and evidence suggest should increase adoption (Milkman, Rogers, and Bazerman 2009; Milkman, Rogers, and Bazerman 2010; Read and Van Leeuwen 1998; VanEpps, Downs, and Loewenstein 2016). For example, companies like Apple and Zoom invite customers who are due for a large software update to install their updates later; Wikipedia offers the option to pre-commit to donate later; and Stickk.com (a popular goal-setting website) allows users to begin goal pursuit on a future date. Across hundreds of employers, pre-commitment to saving is offered through the Save More Tomorrow program, which allows employees to commit now to start saving for retirement in the future (Benartzi and Thaler 2013; Thaler and Benartzi 2004). While previous research suggests that pre-commitment should unambiguously increase overall take-up of farsighted behaviors, we propose that the effects of pre-commitment on the adoption of farsighted behaviors may be more nuanced, and we present evidence consistent with our theorizing.

The current research begins with the premise that there are different ways that a marketer can offer consumers the opportunity to pre-commit to a farsighted behavior. Previous research has focused on single-option choice sets, asking how offering the option to adopt a farsighted behavior at a delay (vs. now) impacts take-up (e.g., Rogers and Bazerman 2008). However, when

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pre-commitment is offered in practice, marketers commonly offer each consumer both the option to adopt the behavior “now” and the option to adopt the behavior “later”. Marketers do this presumably because it allows firms to satisfy consumers’ heterogeneous preferences (i.e., attracting consumers who prefer delay *in addition to* those who prefer to start immediately), while expediting the adoption of offered behaviors. In this paper, we propose the first theory to examine two common strategies that marketers use to offer both the pre-commitment option and the option to adopt the behavior immediately. We term these two strategies “simultaneous pre-commitment” and “sequential pre-commitment” (illustrated in Figure 1). We further identify a novel mechanism that helps explain the differential effects of simultaneous and sequential pre-commitment on farsighted decisions.

When offering simultaneous pre-commitment, marketers present the option to adopt a farsighted behavior now and the pre-commitment option *side-by-side*. When Zoom offers their software update, for instance, they offer the option to update it now or at a future date, and these options are offered simultaneously, side-by-side. Similarly, when Wikipedia solicits donations, they simultaneously offer consumers the options to donate “now” or “later”. Indeed, in surveys we conducted, the majority of industry professionals (73.8% of N=229), as well as marketing professors (62.4% of N=85), predicted that offering simultaneous pre-commitment to encourage a policy-relevant behavior—enrollment in a retirement plan—would lead individuals to save more compared to only offering the option to enroll now (see Web Appendix A for more details). However, in the current research, we find that simultaneous pre-commitment does not necessarily increase adoption of farsighted behaviors and can even backfire, leading people to delay important behaviors they would otherwise have engaged in immediately. Such delays can be costly, as waiting longer to begin saving reduces accumulated wealth, waiting longer to

update software increases the likelihood of malware attacks, waiting longer for a health screening reduces the likelihood of detecting a disease early enough to cure it, and waiting longer for a vaccine reduces the likelihood of having protection at the time of disease exposure. We theorize and show that simultaneous pre-commitment signals that the marketer who designed the adoption opportunity does not view the offered behavior as very urgent. It is as if the marketer is saying either now or later will suffice.

When offering sequential pre-commitment, marketers first give consumers the option to adopt the farsighted behavior immediately, and then, only if the initial offer is declined, do they offer the option to adopt the behavior later. For example, in one of the original implementations of the Save More Tomorrow retirement savings program, employees were only offered the option to pre-commit to save in the future if they had already rejected an offer to start saving immediately (Thaler and Benartzi 2004). In contrast to simultaneous pre-commitment, we theorize that sequential pre-commitment heightens urgency: By offering immediate adoption before the option to delay the action, the marketer is signaling that they prefer the action be taken sooner rather than later. We further show that this inference about urgency can help explain why sequential pre-commitment effectively increases adoption of farsighted behaviors.

In the remainder of this paper, we first develop our theory, position it in the literature, and motivate our hypotheses. We then present evidence supporting our theory from one large-scale field experiment, one vignette-based laboratory experiment, and one incentive compatible laboratory experiment. We end with a discussion of the implications of our research.

***THEORY***

*Previous Theories of Pre-commitment*

In this paper, when we state that people are offered “pre-commitment”, we mean that they are offered an option to commit now to adopt a behavior at a future point in time. We consider pre-commitment to be a specific type of “commitment device” (Rogers, Milkman and Volpp 2014) because, broadly speaking, when people are offered a commitment device, they are offered the option to commit to restrict a future choice set (Ashraf, Karlan, and Yin 2006; Schwartz et al. 2014). Our research specifically examines pre-commitment.

Multiple streams of research have found that offering consumers a choice to pre-commit to a farsighted behavior increases take-up. For instance, when making choices for the future (vs. for now), people are more likely to select healthy foods (Milkman, Rogers, and Bazerman 2010; Read and Van Leeuwen 1998; VanEpps, Downs, and Loewenstein 2016), rent educational films (Milkman, Rogers, and Bazerman 2009; Read, Loewenstein, and Kalyanaraman 1999), and support policies that bolster environmental sustainability (Rogers and Bazerman 2008).

The most common explanation for this pattern is that many people tend to exhibit present bias (O’Donoghue and Rabin 1999). Present-biased consumers struggle to make farsighted decisions because they over-weight the immediate costs associated with such behaviors (e.g., the taste disadvantages of healthy food, the discomfort of a vaccine or colonoscopy) while steeply discounting the future benefits (e.g., longevity). Previous research has argued that pre-commitment offers should be attractive to present-biased consumers; if the farsighted behavior (e.g., eating a healthy diet, receiving a shot) starts or occurs in the future, the disutility of incurring the costs will be heavily discounted—making the behavior seem less aversive (Milkman, Rogers, and Bazerman 2008). Accordingly, as long as a decent share of consumers

are present-biased, offering them a chance to adopt a farsighted behavior in the future (i.e., to pre-commit) should, on average, increase overall take-up of the farsighted behavior.

In addition to present bias, theories about resource slack and pain of payment lead to a similar prediction that pre-commitment should increase adoption of farsighted behaviors. That is, people expect to have more discretionary resources (i.e., “resource slack”) in the future than they do in the present (Zauberman and Lynch 2005), and people find spending resources less painful when they have more resources (Morewedge, Holtzman, and Epley 2007). As a result, people should anticipate that, compared to adopting a farsighted behavior now, pre-committing to adopt it in the future will be less painful and thus more attractive.

When a consumer is offered both the option to adopt a behavior “now” and the option to adopt the behavior “later”, we still assume that some combination of the aforementioned mechanisms identified in previous work should make the pre-commitment offer attractive to some extent. However, our focus is on an additional mechanism that has been neglected by extant theory about pre-commitment and may counterbalance these previously studied benefits.

***Precommitment and Inferred Urgency***

We argue that to understand when pre-commitment fails it is necessary to consider people’s inferences about the marketer offering pre-commitment. Generally speaking, past research has shown that consumers make inferences about marketers’ motives and recommendations (Kardes, Posavac, and Cronley 2004), particularly based on the options they offer and the way those options are arranged (Benartzi 2001; Lieberman, Duke, and Amir 2019; Krijnen, Tannenbaum, and Fox 2017). For example, people assume the option that marketers set as a “default” is what they recommend (Brown and Krishna 2004; McKenzie, Liersch, and Finkelstein 2006).



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Extending this work, we propose that people make inferences about the *urgency* of marketers' implicit recommendations. In previous marketing research, urgency has typically been defined as an objective characteristic of tasks (often referred to as "task urgency"; Zhu, Yang, and Hsee 2018). Tasks with upcoming deadlines, for instance, have greater task urgency than those with more distant deadlines. We argue that urgency can also describe a person's subjective judgment that it is better to take action sooner rather than later. Importantly, consumers may make sense of how urgent a marketer thinks it is for them to take action based on the marketer's implicit and explicit recommendations. For instance, if a financial advisor recommends that her clients start saving *immediately*, the clients may perceive that the financial advisor considers saving to be an urgent priority. In the current research, we examine how consumers make inferences about the urgency of marketers' recommendations from the presentation of choices marketers offer. We define "inferred urgency" as the inference by a consumer that a marketer recommends adoption of a behavior sooner rather than later. Below, we first theorize about how the design of a pre-commitment offer affects consumers' inferred urgency and then hypothesize about the consequences of this for consumer choice.

We specifically propose that consumers make inferences about the urgency of a marketer's implicit recommendation from the order in which a pre-commitment option is presented. Previous research has shown that consumers hold "position-based beliefs" (Valenzuela and Raghurir 2009). Consumers assume that options presented at eye-level, for instance, are placed there by the retailer because they are more popular than the options on the bottom of the shelf (Valenzuela and Raghurir 2009). Building on this work, we argue that consumers do not just hold position-based beliefs about *where* options are placed in a display but also make inferences based on *when* marketers present options in a sequence. We specifically

propose that consumers will view the temporal ordering of options as an intentional decision by the marketer that signals how strongly the marketer recommends certain options relative to salient alternatives.

We argue that the two common ways marketers design pre-commitment offers—simultaneously and sequentially—send contrasting signals about the urgency of a marketer’s recommendation. When a marketer offers consumers simultaneous pre-commitment, the options to adopt the behavior “now” and “later” are presented side-by-side in the same menu. In presenting these options side-by-side, the marketer does not signal a clear preference between the options. Without additional information, consumers may naturally infer that a marketer endorses both options equally (Fox, Ratner, and Lieb 2005; Tannenbaum, Fox, and Goldstein 2013). To consumers, it is as if the marketer is saying, “either doing it now or later will suffice.” When a marketer offers consumers sequential pre-commitment, however, they are offering the option to adopt a given behavior “now” first, and only after their offer is rejected do they offer the option to adopt it “later.” We propose that this presentation implies an ordinal ranking of the marketer’s recommendations. It would be natural for consumers to infer that the marketer is not outright endorsing the “later” option, but rather treating it like a contingency plan to ensure that if consumers do not adopt what the marketer is offering now, they will at least adopt the behavior at some point in the future. To consumers, it is as if the marketer is saying, “you should do this as soon as you can!”

We formally hypothesize that:

**Hypothesis 1:** Compared to not offering a pre-commitment option, offering simultaneous pre-commitment will decrease consumers’ inferences about the urgency with which a

behavior's adoption is recommended, whereas sequential pre-commitment will increase the inferred urgency.

### ***Inferred Urgency and Farsighted Decisions***

When people infer a behavior is urgently recommended, this should subsequently influence their decisions. Previous work has shown that consumers' choices are often influenced by their inferences about marketers' recommendations (Smith, Goldstein, and Johnson 2013). For instance, labeling health care plans "gold," "silver," and "bronze" conveys to consumers what marketers consider to be the best, middle, and worst plans, which alters consumers' insurance choices, even when the labels are assigned arbitrarily (Ubel, Comerford, and Johnson 2015). And, when people infer that a default option is recommended by the marketer, they are typically more likely to choose that option (McKenzie, Liersch, and Finkelstein 2006). Other work has shown that when marketers communicate the urgency of a task by highlighting an upcoming deadline, people are more motivated to do it (d'Adda, Galliera, and Tavoni 2020; Zhu, Yang, and Hsee 2018).

Bridging this work, we argue that when consumers infer a behavior is urgently recommended, they should be more likely to adopt the behavior. In the context of pre-commitment offers, our research focuses on inferred urgency's influence on two choice outcomes: immediate adoption and overall adoption of a farsighted behavior. Both of these outcomes have important consequences for consumer well-being, and the two outcomes together present a comprehensive evaluation of how pre-commitment design affects engagement in farsighted behaviors.

***Immediate adoption.*** Immediate adoption refers to whether consumers commit to adopt a behavior immediately (i.e., choosing the "do it now" option). When studying farsighted

behaviors, immediate adoption is particularly important to examine because the benefits of these behaviors typically accumulate over time. For instance, saving earlier (vs. later) in life results in greater accumulated savings; updating software sooner (vs. later) increases likelihood of stopping a malware attack; getting screening exams sooner (vs. later) increases likelihood of catching a disease early enough to cure it, and receiving a vaccine sooner (vs. later) increases the likelihood of being protected at the time of disease exposure.

When consumers infer a behavior is urgently recommended by a marketer, they believe that the marketer recommends they adopt the behavior sooner rather than later, which should in turn increase *immediate adoption* of the behavior. Given that simultaneous pre-commitment signals lower urgency (compared to no pre-commitment or sequential pre-commitment) per our earlier theorizing, and given that this lack of urgency should reduce immediate adoption, we expect simultaneous pre-commitment to decrease immediate adoption of farsighted behaviors.<sup>1</sup> More formally, we hypothesize that:

**Hypothesis 2:** Offering simultaneous pre-commitment will decrease the immediate adoption of farsighted behaviors compared to offering sequential pre-commitment or making no pre-commitment offer.

**Overall adoption.** Overall adoption refers to whether consumers commit to adopt a behavior at any point in time (i.e., choosing either the “do it now” option or the “do it later” option). When consumers infer that a marketer recommends they promptly adopt a behavior as

<sup>1</sup> Sequential pre-commitment and no pre-commitment (the control condition) should have the same level of immediate adoption by design, as these two conditions are identical until after consumers make decisions about immediate adoption (see Figure 1). Thus, we do not make predictions about the effect of sequential pre-commitment (vs. no pre-commitment) on immediate adoption of farsighted behaviors. This also presents a minor exception to Hypothesis 4 (introduced later in the Theory section): we do not predict that inferred urgency mediates the effects of sequential pre-commitment (vs. no pre-commitment) on immediate adoption of farsighted behaviors.

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soon as possible, this should in turn increase *overall adoption* (i.e., prompting them to commit to do it, either immediately or at a future time).

The predicted impact of simultaneous pre-commitment (relative to no pre-commitment) on overall adoption is unclear due to competing mechanisms. On one hand, simultaneous pre-commitment may capitalize on people's preference for delaying the adoption of farsighted behaviors (as found in previous research), which should increase overall adoption by getting people who otherwise would not sign up to choose the pre-commitment option. On the other hand, it reduces inferred urgency, which should curb adoption of the farsighted behavior. Therefore, we do not make predictions about the effect of simultaneous pre-commitment (vs. no pre-commitment) on overall adoption of farsighted behaviors because the effect will depend on the relative strength of these opposing mechanisms.

However, sequential pre-commitment should unambiguously increase overall adoption of farsighted behaviors relative to not offering a pre-commitment option, because it both signals greater urgency and capitalizes on people's preference to pursue farsighted behaviors at a time delay. Further, compared to offering simultaneous pre-commitment, offering consumers sequential pre-commitment should also clearly boost overall adoption: both types of pre-commitment leverage people's preference for delaying the costs associated with farsighted activities, but we expect sequential pre-commitment to signal a greater sense of urgency than simultaneous pre-commitment. We therefore hypothesize that:

**Hypothesis 3:** Sequential pre-commitment will increase overall adoption of farsighted behaviors, compared to making no pre-commitment offer or offering simultaneous pre-commitment.

**Mediation via Inferred Urgency.** Given our predictions that simultaneous and sequential pre-commitment impact consumers’ inferences about how urgently action is recommended and that heightened inferred urgency spurs immediate and overall adoption of farsighted behaviors, we formally hypothesize:

**Hypothesis 4:** Inferred urgency will mediate the effects of simultaneous and sequential pre-commitment (vs. not offering a pre-commitment option) on both the immediate and overall adoption of farsighted behaviors.

The four hypotheses are summarized in Table 1.

***Theoretical Implications***

Overall, the current paper aims to make three main contributions. First, we contribute to research on pre-commitment and farsighted decision making by drawing a theoretical and practical distinction between simultaneous and sequential pre-commitment. Although commonly used in practice and thus worthy of systematic investigation, these types of pre-commitment have not previously been distinguished from one another and rigorously studied.

Second, we contribute to research on inference making (Kardes, Posavac, and Cronley 2004) and information leakage (McKenzie, Liersch, and Finkelstein 2006) by uncovering a novel, consequential inference people draw from choice sets: the inferred urgency of the marketer’s implicit recommendation. We theorize about why people form inferences about the urgency of recommendations and why inferred urgency can spur people to take prompt action. Further, we argue and show that inferred urgency can help explain when offering pre-commitment increases adoption of farsighted behaviors and when it does not.

Third, we present a large, real-world experimental test of pre-commitment, arguably one of the most commonly used “nudge” interventions. Our research suggests that a seemingly small

difference in the way a popular idea is implemented (e.g., the simultaneous vs. sequential presentation of a pre-commitment option) can change its effects. We discuss generalizable lessons for scaling promising marketing strategies in the field.

The remainder of this paper is organized as follows. We begin our investigation with a field experiment studying simultaneous pre-commitment, and then we present two additional well-powered, pre-registered laboratory studies testing all four of our hypotheses. The results of each study are summarized in Table 2. Our pre-registrations, materials, non-proprietary data, and code are available at: <https://researchbox.org/434>.

### ***STUDY 1: HOW SIMULTANEOUS PRE-COMMITMENT AFFECTS RETIREMENT SAVINGS DECISIONS IN THE FIELD***

To test our theory's main predictions about the impact of offering simultaneous pre-commitment in the field, we report on the results of two conditions from an experiment involving real savings decisions.<sup>2</sup> A companion paper (Beshears et al. 2021) compares a third condition from this field experiment with one of the conditions examined in our paper to explore a separate research question (see additional details in the following sections and Web Appendix B).

#### ***Methods***

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<sup>2</sup> Note that although the field experiment fits as a test of our theory's predictions, it was conducted before the development of our theory. For full transparency, we were originally hoping, based on prior research, that offering a pre-commitment option would increase retirement savings. We were surprised by the negative impact of our pre-commitment design on savings, which prompted us to develop our theory and pre-register Studies 2-3 to deductively test our theory.

Four U.S. universities (labeled Universities A, B, C, and D to preserve their anonymity) collaborated with us on our field experiment.<sup>3</sup> Each university began by identifying a retirement savings plan in which they hoped to increase employees' contributions. All universities then identified employees who were not enrolled in this "targeted plan" and therefore had a contribution rate of zero, encouraging them to sign up to save in the targeted plan. One university (University D) also identified employees who were contributing to the targeted plan, but not at the level necessary to take full advantage of their employer's matching contributions; these employees were encouraged to save more, rather than to start saving. Table 3 presents more information about the targeted plans offered by the four universities, and Table W2 in Web Appendix C details other (non-targeted) savings plans.

One retirement plan record keeper shared by all four universities sent out mailings in early October of 2013 to university employees' homes. The mailings provided employees with an opportunity to increase their savings contributions by filling out and mailing back a simple form on a pre-stamped, pre-addressed postcard. If an employee checked a box indicating they wanted to save and then signed and returned the postcard, that employee would be enrolled in the plan at a preselected contribution rate with their contributions allocated to a preselected fund. At all four universities, the preselected fund on the mailing was a lifecycle fund, which provided a diversified portfolio with a mixture of equity, bond, and money market funds tailored to the employee's age. The preselected contribution rate was 3% of the employee's pay for Universities A-C and 5% for University D. If an employee who was already contributing to the targeted plan

<sup>3</sup> The experiment originally included a fifth university. However, this university requested that employees elect dollar contribution amounts instead of contribution rates. Consequently, this university had different mailing designs from other universities. Further, this university had a very low response rate (only .6% of employees at this university increased their contribution rate by the end of our study period across the Simultaneous Pre-commitment and No Pre-commitment conditions, compared to an average of 13.0% at the other universities). Thus, it was not possible to do a meaningful analysis for this university, and we excluded its data from our analysis.



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elected to save more (only relevant to University D), their contributions would increase to the preselected rate with the contributions allocated according to their existing asset allocation.

Employees were randomly assigned to different experimental conditions, which determined the exact mailing they received (see Figure 2 and Web Appendix B for additional details on our stratified random assignment process). In this paper, we only analyze employees who were randomly assigned to receive either a No Pre-commitment mailing or a Simultaneous Pre-commitment mailing ( $N = 5,196$ ;  $M_{\text{age}} = 43.10$ ,  $SD_{\text{age}} = 12.05$ ; 52.3% female). The No Pre-commitment mailing offered employees the opportunity to immediately increase their contribution rate to the targeted plan. The Simultaneous Pre-commitment mailing offered employees the opportunity to increase their contribution rate to the targeted plan either immediately or after a time delay (e.g., “in two months”) ranging from two to six months. Both mailings are displayed in Figure 3.

The experiment included another group of employees who were randomly assigned to receive a different type of Simultaneous Pre-commitment mailing, which offered them an opportunity to increase their contribution rate to the targeted plan either after a labeled temporal landmark (e.g., their birthday, the first day of spring, Thanksgiving, Valentine’s Day) or immediately. A companion paper (Beshears et al. 2021) compares the enrollment decisions of employees who received these distinctive Simultaneous Pre-commitment mailings linked with temporal landmarks and employees who received the standard Simultaneous Pre-Commitment mailings studied here. The objective of Beshears et al. (2021) was to test whether inviting people to boost their contribution rate after a “fresh start” date (e.g., a birthday, the first day of spring; following Dai, Milkman and Riis, 2015), increases savings over and above inviting people to increase contributions at an equidistant future time point (e.g., in 2 months). Beshears et al.

(2021) does not report results from the No Pre-commitment condition studied here because the paper solely explores the effect of inviting savings following fresh start dates and not the effects of offering pre-commitment. See Web Appendix B for more information about Beshears et al. (2021).

This experiment's randomization was stratified by university because the universities varied on important features such as the targeted plans' characteristics. Randomization was also stratified by birth month (within each university) because employees' birthdays partially determined which mailing they received; only those whose birthday fell into November 2013-March 2014 had the opportunity to be randomized to receive the option to save more after their birthday.

In all conditions, mailing recipients who wanted to increase their contribution rates had to send back their response card by November 1, 2013. If they chose to save at a higher rate immediately via the mailer, their contribution rate would increase to the preselected rate in November 2013. If they chose to save more at a delay (e.g., in five months), their contribution rate would automatically increase to the preselected rate at the predetermined time (e.g., in March 2014).

### ***Data***

Our university partners first pulled a cross-sectional snapshot of information about all plan-eligible employees in August 2013, including their current contributions to the targeted plan, contributions to all other non-targeted savings plans, birth date, hire date, termination date, salary, and position (faculty versus staff). Our conditions are reasonably well balanced across baseline employee characteristics, with the only statistically significant difference being that the mean salary of employees in the No Pre-commitment condition ( $M = \$56,505.19$ ,  $SD =$

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\$35,234.21) is slightly less than that of employees in the Simultaneous Pre-commitment condition ( $M = \$58,505.26$ ,  $SD = \$36,111.88$ ;  $p = .043$ ; See Table W1 in Web Appendix B). To ensure that the slight imbalance detected on this dimension is accounted for, our regressions control for baseline employee characteristics, including salary decile.

After the study concluded, our university partners provided information on each employee's contributions to the targeted plan and all other retirement savings plans as well as their pay for each pay cycle from August 2013 through June 2014.<sup>4</sup> We measured the impact of our mailing by observing changes in employees' retirement plan contributions (made by mail, phone or online).

## **Variables**

To comprehensively measure the effects of offering simultaneous pre-commitment on savings, we created three outcome variables, which are described below.

***Immediate adoption.*** *Immediate adoption* is a binary variable that takes on a value of one for people who increased their contribution rate to the targeted plan immediately after receiving our mailing and zero for others. We constructed this variable by examining whether an employee's contribution rate in November 2013 (the first month our mailings could have triggered increased contributions) was higher than their rate in September 2013 (the month right before our mailings were sent out).<sup>5</sup>

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<sup>4</sup> We cannot publish any data from our field experiment due to the nondisclosure data agreement we signed with our field partners. However, if any researcher is interested in replicating our analyses, they should contact us, and we will try to have them added to our nondisclosure data agreement so that individual scholars may be able to work with our field data.

<sup>5</sup> In the manuscript, we focus on when and how contributions to the targeted plan changed because our mailings encouraged employees to increase savings in the targeted plan. However, as shown in Table W4 in Web Appendix E, our effects are robust if we comprehensively examine the impact of our pre-commitment design using employee contributions to *all* savings plans offered by their employer (including the targeted plan).

**Overall adoption.** *Overall adoption* is a binary variable that takes on a value of one for people who increased their contribution rate to the targeted plan by the end of our study period and zero for others. We constructed this variable by examining whether an employee's contribution rate in June 2014 (the last month in which we received data on employees' contributions and pay) was higher than their contribution rate in September 2013.<sup>6</sup>

**Average savings rate.** To capture employees' cumulative retirement savings (adjusted for their salary) during our study period, we calculated every employee's *average savings rate* by taking the total number of dollars the employee contributed to the targeted plan from November 2013 through June 2014 and dividing it by the employee's total pay during the same period. This outcome variable ranges from 0 to 1, representing the percentage of an employee's total pay that was contributed to the targeted savings plan during our study period.

**Analysis Strategy**

To estimate the causal impact of the Simultaneous Pre-commitment mailing (compared to our No Pre-commitment mailing that only invited people to save now), we relied on the following ordinary least squares (OLS) regression specification:

$$(1) \quad outcome_i = \alpha + \beta \text{ simultaneous pre-commitment}_i + \gamma' X_{ij} + \sum_j (\delta_j I[university_i = j] + \zeta_j' X_{ij} I[university_i = j]) + \varepsilon_i$$

where  $i$  indexes employees and  $j$  indexes universities. We estimated this regression once with each of the outcome variables explained above. The coefficient on the indicator for simultaneous pre-commitment is the estimate of the causal impact of the Simultaneous Pre-commitment

<sup>6</sup> We also calculated overall adoption using an alternative method that was meant to capture the direct responses to the mailings. Specifically, we only counted someone as enrolling if the first time their contribution rate increased (relative to their rate in September 2013) was either in November 2013 (i.e., the immediate enrollment option) or the specific month when pre-commitment was invited in their mailing. Note that our results do not change substantively when we examine this narrower outcome (see Table 5 and Web Appendix E).

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condition relative to the No Pre-commitment condition. In order to increase statistical power, we estimated a single treatment effect across universities instead of separate treatment effects for each university, but this decision does not invalidate the interpretation of the coefficient as a causal effect, since randomization was stratified by university.  $X_{ij}$  is a vector of controls: gender, age decile, tenure decile, salary decile, faculty status, and birth month, where decile breakpoints are calculated separately for each university.  $I[university_i = j]$  is an indicator variable that takes a value of one when employee  $i$  is associated with university  $j$  and a value of zero otherwise.<sup>7</sup> The  $\delta_j$  and  $\zeta_j$  coefficients allow the intercept term and the coefficients on the control variables to vary by university, accounting for differences across universities in their average responsiveness to our mailings and differences across universities in the relationship between the control variables and the outcome variable. Given that retirement savings decisions are largely determined by socioeconomic circumstances, controlling for employees' demographics, income, and employer characteristics in analyses of interventions designed to increase savings rates can dramatically enhance statistical power and is consistent with standard practice in retirement savings research (for a review, see Choi 2015; see Web Appendix D for more information). We report heteroskedasticity-robust standard errors. For the binary outcomes, we report linear probability regressions here rather than logistic regressions for the ease of interpretation, but the results do not substantively differ when we estimate logistic regressions (see this and all other robustness checks summarized in Table 5 and described in detail in Web Appendix E).

## Results

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<sup>7</sup> Some employees at University D were already enrolled in the targeted plan before the experiment started, and the experimental mailings encouraged them to further increase their contribution rates. Because these employees are qualitatively different from those who were not yet contributing, in our analyses we treat the two groups of employees as belonging to separate "universities" by including two distinct "university" indicator variables for those two groups. Also, note that in the summation shown in Equation 1, we omit one university indicator variable to avoid collinearity.

**Immediate Adoption.** Consistent with Hypothesis 2, employees' probability of immediately increasing their contributions to the targeted plan was 1.9 percentage points lower in the Simultaneous Pre-commitment condition than in the No Pre-commitment condition ( $p = .006$ ; Table 4 Model 1). In terms of its relative effect, this represents a 25.3% decrease, relative to the 7.5 percent of employees who immediately increased their contribution rate to the targeted plan in the No Pre-commitment condition.

**Overall Adoption.** Importantly, when we look at the full study period through June 2014, there was not a statistically significant difference between conditions in employees' probability of increasing their contributions to the targeted savings plan ( $p = .301$ ; Table 4 Model 2).

Given that simultaneous pre-commitment decreased the number of people who immediately increased their contribution rate to the targeted saving plan but did not significantly affect the total number of savers, what we can conclude is that simultaneous pre-commitment led some people to delay saving who otherwise would have started saving immediately.<sup>8</sup> When people delay saving, they save less overall than they would have if they started saving earlier. We next formally tested whether offering simultaneous pre-commitment ultimately reduced accumulated savings.

**Average Savings Rate.** Table 4 Model 3 indicates that receiving the Simultaneous Pre-commitment mailing (rather than the No Pre-commitment mailing) decreased average saving rates to the targeted plan by .14 percent of pay ( $p = .046$ ). In relative terms, receiving the Simultaneous Pre-commitment mailing caused a 16.5% decrease in savings in targeted plans during our study period compared with the No Pre-commitment mailing (for which the average savings rate was .85 percent of employee pay).

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<sup>8</sup> We confirmed that, conditional on employees deciding to increase contributions, the Simultaneous Pre-commitment mailing did not affect the magnitude of increases in contribution rates to the targeted plans ( $p = .311$ ).

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In additional models reported in Web Appendix E, we examined how the treatment effect on each of the outcome variables varied by university. The effects of simultaneous pre-commitment on both immediate and overall adoption did not significantly vary by university, though the decrease in average savings rates was more prominent in two universities.

## *Discussion*

Contrary to past research demonstrating the benefits of pre-commitment, we find in a large field experiment conducted with four employers that inviting consumers to save more “now or later” (i.e., offering simultaneous pre-commitment) did not increase the share of people contributing to a retirement savings plan. In fact, offering simultaneous pre-commitment (relative to simply inviting consumers to save now) on average decreased overall retirement wealth because some people opted to delay increasing their contribution rates to the savings plan and thus saved over a shorter time horizon than they would have otherwise.

As mentioned earlier, in a companion paper (Beshears et al. 2021), the Simultaneous Pre-commitment condition from this field experiment is used in conjunction with an additional experimental condition not analyzed here to examine the effects of offering pre-commitment shortly after fresh start dates. That paper finds that offering pre-commitment after fresh starts (e.g., birthdays, the first day of spring) increases overall adoption and average savings rates compared to offering pre-commitment after an equidistant future time point that is not associated with such a temporal landmark. The primary contribution of Beshears et al. (2021)—to demonstrate the impact of linking opportunities to save with dates that feel like fresh starts—is theoretically distinct from the key contribution of Study 1 in our paper, which is to show the impact of simultaneous pre-commitment (vs. not offering pre-commitment). See Table W15 in

Web Appendix F for a summary of how Beshears et al. (2021) and Study 1 in the current paper differ in terms of their conditions, research questions, and contributions.

We next present the results of a post-test to examine whether our theory about inferred urgency could potentially explain why the pre-commitment offer in our field experiment backfired.

***Post-test: Initial Evidence on Inferred Urgency.*** We theorize that when an immediate enrollment option and a pre-commitment option were presented side-by-side in our field experiment (i.e., via simultaneous pre-commitment), employees may have inferred that their university's HR department did not urgently recommend retirement savings. In a pre-registered online experiment ( $N = 1,499$  Mechanical Turk workers), we confirmed that the Simultaneous Pre-commitment mailing from our field experiment conveyed a less urgent recommendation to save than our No Pre-commitment mailing ( $p < .001$ ; see Web Appendix G for more details on this post-test). In the same post-test, we also confirmed that people believed a *Sequential* Pre-commitment offering—that is, sending a second mailing inviting employees to save later only if they neglected an initial mailing with the offer to save immediately—conveyed a more urgent recommendation to save than either our No Pre-commitment mailing ( $p = .060$ ) or the Simultaneous Pre-commitment mailing ( $p < .001$ ).

Together, the findings from this post-test offer tentative support for a mechanism that might explain the results we observed in the field. That is, simultaneous pre-commitment may have reduced immediate adoption of savings and failed to increase overall adoption because the mailing signaled that saving was not urgently recommended. This post-test also reveals that offering *sequential* pre-commitment signals that saving is highly urgent. We next present two large laboratory experiments designed to deductively test our full theory about how simultaneous



and sequential pre-commitment may differentially impact the inferences consumers draw about the urgency of marketers' recommendation and, in turn, consumers' farsighted decisions.

## ***STUDY 2: INFERRED URGENCY AND FARSIGHTED DECISIONS***

We conducted a pre-registered laboratory experiment in which participants decided (hypothetically) whether and when to enroll in three different benefits programs offered by a new employer. This study tests all four of our hypotheses.

### ***Methods***

As pre-registered, we recruited workers on Prolific who were fully employed at a firm other than Prolific and passed one brief attention check. A total of 2,682 participants satisfied these selection criteria and completed the study ( $M_{\text{age}} = 34.7$ ,  $SD_{\text{age}} = 9.1$ ; 40.3% female).

All participants were asked to imagine that they were offered three benefits programs as a new, full-time employee at Company X: a retirement savings plan, a life insurance program, and a health savings account. They were told that all programs were optional, and money would be deducted from their take-home pay for each program they enrolled in. Participants were required to correctly answer two comprehension check questions before proceeding, and they were allowed to keep trying until they got these comprehension check questions right. Participants were then randomly assigned to one of three conditions: the No Pre-commitment condition, the Simultaneous Pre-commitment condition, or the Sequential Pre-commitment condition.

In the No Pre-commitment condition, participants only had the option to enroll in each benefits program immediately. Specifically, they read: "... if you check the 'Enroll now' box to enroll in a program, Company X will start providing you with the given benefit now and begin deducting from your paycheck as soon as possible." Then they indicated which program(s) they would enroll in by marking the corresponding checkbox(es).

In the Simultaneous Pre-commitment condition, participants had the option to enroll in each benefits program either immediately or in six months. The instructions explained: "...you can choose 'Enroll now,' which means Company X will start providing you with the relevant benefit now and begin deducting from your paycheck as soon as possible. Or you can choose 'Enroll in 6 months,' which means Company X will start providing you with the relevant benefit in 6 months and begin deducting from your paycheck in 6 months." After reading these instructions, participants indicated which program(s) they would enroll in by marking the corresponding checkbox(es). For any programs they selected, they then decided when to enroll (either now or in six months).

In the Sequential Pre-commitment condition, participants were first given the option to enroll in each program immediately, and then for the programs they did not enroll in immediately, they were offered the option to enroll in six months. The condition looked identical to the No Pre-commitment condition through the first (immediate) enrollment decision. Participants who did not immediately enroll in all three programs were next told, "Imagine that the day after you submitted your enrollment decisions, Company X follows up and sends you another online enrollment form." Participants were then given the option to enroll in the remaining programs in six months by checking the corresponding checkbox(es).

Next, participants in all conditions answered two questions about Company X: "To what extent do you think Company X recommends that employees enroll in the benefits programs as soon as they can?" and "To what extent do you think Company X urgently recommends that employees enroll in the benefits programs" (1 = Not at all; 7 = Very much). We adapted these items from past research on information leakage (McKenzie, Liersch, and Finkelstein 2006) and task urgency (Zhu, Yang, and Hsee 2018) to assess participants' inferences about how urgently

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Company X recommended they sign up for the benefits programs. The two items were collapsed into a single measure of *inferred urgency* because they hung together well ( $r = .70, p < .001$ ).

Finally, we included a set of questions to assess whether decision difficulty could be an alternative mechanism for our predicted results. Specifically, we adapted four items from an existing decision difficulty scale (Goodman et al. 2013): “To what extent [did you find the choice difficult/were you overwhelmed/were you frustrated/were you annoyed] when you were making your enrollment decision?” (1 = Not at all; 7 = Very much). The items hung together well ( $\alpha = .85$ ) and were averaged into one measure of *decision difficulty*. To establish the discriminant validity of these two mechanism measures, we used an exploratory factor analysis and confirmed that the four decision difficulty items loaded on a separate factor than the two urgency items (see Web Appendix H for details). We also checked that the composite score of decision difficulty and that of inferred urgency are only correlated at  $r = .15$ . At the end of our study, participants were asked about their age, gender, education, and income.

We focus on two pre-registered outcome variables in this study. The first is *immediate adoption*, which measures the number of benefits programs participants elected to enroll in immediately (i.e., by selecting the “Enroll now” option). The second outcome of interest is *overall adoption*, which measures the total number of benefits programs participants elected to enroll in (i.e., by selecting either the “Enroll now” or “Enroll in 6 months” option).

## Results

For analyses that include all three conditions, we relied on ordinary least squares (OLS) regressions with heteroskedasticity-robust standard errors where the key predictors are indicators for the Simultaneous Pre-commitment and Sequential Pre-commitment conditions, with the No

Pre-commitment condition serving as the reference group. All mediation analyses use 5,000 bootstrapped samples to estimate 95% confidence intervals (CI) around the indirect effects.

**Inferred urgency.** Providing support for Hypothesis 1, compared to those in the No Pre-commitment condition ( $M = 4.88$ ,  $SD = 1.52$ ), we found that participants in the Simultaneous Pre-commitment condition rated Company X's implicit recommendation to enroll in its benefits programs as less urgent ( $M = 4.55$ ,  $SD = 1.63$ ;  $b = -.33$ ,  $p < .001$ ,  $d = .21$ ), whereas participants in the Sequential Pre-commitment condition rated Company X's recommendation to enroll as more urgent ( $M = 5.22$ ,  $SD = 1.30$ ;  $b = .34$ ,  $p < .001$ ,  $d = .24$ ).

**Immediate Adoption.** Confirming Hypothesis 2 and replicating the results from our field experiment, participants in the Simultaneous Pre-commitment condition signed up for fewer benefits immediately ( $M = 1.49$ ,  $SD = .89$ ) than participants in the No Pre-commitment condition ( $M = 1.86$ ,  $SD = .78$ ;  $b = -.37$ ,  $p < .001$ ,  $d = .44$ ). And, consistent with Hypothesis 4, this reduction in immediate adoption was significantly mediated by a drop in the inferred urgency of the recommendation to enroll ( $b = -.009$ ; 95% CI =  $[-.019, -.001]$ ).

**Overall Adoption.** Consistent with the results of our field experiment, there was not a statistically significant difference in overall adoption between the Simultaneous Pre-commitment condition ( $M = 1.84$ ,  $SD = .81$ ) and the No Pre-commitment condition ( $M = 1.86$ ,  $SD = .78$ ;  $b = -.017$ ,  $p = .659$ ,  $d = .02$ ). Inferred urgency significantly and negatively mediated the relationship between simultaneous pre-commitment and overall adoption ( $b = -.015$ , 95% CI =  $[-.026, -.006]$ ), providing support for Hypothesis 4. These results are in line with our theory that offering

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people a chance to enroll “now or later” (simultaneous pre-commitment) decreases inferred urgency, which curbs overall adoption of a farsighted behavior.<sup>9</sup>

In support of Hypothesis 3, participants in the Sequential Pre-commitment condition ( $M = 2.14$ ,  $SD = .82$ ) signed up for more benefits on average than participants in the No Pre-commitment condition ( $M = 1.86$ ,  $SD = .78$ ;  $b = .28$ ,  $p < .001$ ,  $d = .35$ ). Inferred urgency significantly and *positively* mediated this effect ( $b = .013$ , 95% CI = [.003, .025]), suggesting that sequential pre-commitment increases inferred urgency, which predicts greater overall adoption (offering further support for Hypothesis 4).

Using a Wald test, we confirmed that participants in the Sequential Pre-commitment condition enrolled in more benefits on average than participants in the Simultaneous Pre-commitment condition ( $p < .001$ ,  $d = .37$ ), consistent with Hypothesis 3. And, we confirmed that inferred urgency significantly and positively mediated this difference ( $b = .019$ , 95% CI = [.002, .037]), further supporting Hypothesis 4. In other words, compared to the Simultaneous Pre-commitment condition, people may have enrolled in more benefits programs in the Sequential Pre-commitment condition in part because they inferred that adoption was more urgently recommended.

When we added decision difficulty as another potential mediator in all of the aforementioned mediation models, inferred urgency always remained a significant mediator. See detailed results of these multi-mediator models in Web Appendix I.

## Discussion

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<sup>9</sup> It is possible for simultaneous pre-commitment to have a negative indirect effect on overall adoption via inferred urgency but a null main effect if, as described in our theoretical development, there is a positive indirect effect via a different mechanism (e.g., related to present bias or anticipated resource slack).

Study 2 presents support for our complete theory and tests all four of our hypotheses. Offering simultaneous pre-commitment (i.e., an invitation to enroll in benefits “now or later”) decreases the inferred urgency of adopting farsighted behaviors, whereas offering sequential pre-commitment (i.e., an invitation to enroll in benefits “later” only if people don’t enroll “now”) increases inferred urgency (Hypothesis 1). Furthermore, simultaneous pre-commitment decreases immediate adoption of farsighted behaviors (Hypothesis 2), but fails to increase overall adoption (replicating the findings from our field experiment). Meanwhile, sequential pre-commitment increases overall adoption of farsighted behaviors (Hypothesis 3). Importantly, inferred urgency significantly mediates these effects (Hypothesis 4), helping explain the divergent impact of different forms of pre-commitment on consumer choice.<sup>10</sup>

**STUDY 3: AN INCENTIVE COMPATIBLE TEST OF OUR FULL THEORY**

In Study 3, we sought to conceptually replicate Study 2 with an incentive compatible design in a distinct context. Specifically, we invited people to take a real, 10-minute financial well-being assessment and tested our full theory by examining whether and when they chose to take it.

This study also aims to reconcile our findings with previous literature by measuring an additional mechanism that should make pre-commitment attractive based on extant theory. Specifically, previous work suggests that the immediate costs of adopting a farsighted behavior

<sup>10</sup> In Web Appendices J and K, we report a pre-registered two-condition version of this study—only containing the Simultaneous Pre-commitment and No Pre-commitment conditions (N = 1,161 MTurk participants). There, we replicated all of the results concerning those two conditions in Study 2. In this study, we also showed that the indirect effect of inferred urgency remained significant after controlling for alternative mechanisms including decision difficulty, confusion, and perceived thoughtfulness.

should feel less aversive when people contemplate taking up the behavior later because they steeply discount future costs (Frederick, Loewenstein, and O'Donoghue 2002) and expect to have more resources in the future (Zauberman and Lynch 2005). In this sense, taking the financial well-being assessment in our study should feel less costly when pre-commitment is an option, regardless of how it is offered. We operationalized this mechanism with a measure of perceived convenience.

### Methods

For this pre-registered study, we recruited 2,398 MTurk participants ( $M_{age} = 39.73$ ,  $SD_{age} = 12.06$ ; 49.0% female) who passed an attention check. To provide a cover story for the purpose of the study, we first asked participants to take a financial knowledge test (Knoll and Houts 2012). Participants next reported their employment status, age, gender, education, and income. Then, we offered them an opportunity to take an optional, unpaid financial well-being assessment. We explained:

*In collaboration with Dr. [anonymized], a [university affiliation] professor and world expert on financial decision making, we have prepared an assessment that will provide feedback on your financial well-being and offer scientific tips for improving your financial future. Completing the assessment will take about 10 minutes. It is voluntary and won't affect your pay. But we hope that taking the assessment will be worth your time in the long run.*

At this point, participants were randomly assigned to one of three conditions. In the No Pre-commitment condition, participants were invited to take the financial assessment immediately. In the Simultaneous Pre-commitment condition, participants were invited to take the financial assessment and given the choice to either complete it now or in one week. In the Sequential Pre-commitment condition, participants were invited to take the assessment immediately. If they declined, participants were then invited to take the assessment in one week.

All participants who chose to take our assessment received a real financial well-being assessment at the time they elected.

After participants made their choice(s), they responded to additional questions. First, we asked participants “To what extent do you think we urgently recommend that you take the financial assessment?” (1 = Not at all; 7 = Very much) to measure *inferred urgency*.

We also asked participants to rate how inconvenient it would be to take the assessment (1 = Not at all inconvenient; 7 = Very inconvenient). We reverse-coded this measure and included “perceived convenience” as a competing mediator in each of the reported mediation models (see Figure 4 and Web Appendix L for the full results).

We focus on two pre-registered outcome variables in this study. *Immediate adoption* is a measure of whether a participant elected to take the financial well-being assessment immediately (by selecting the “now” option). *Overall adoption* is a measure of whether a participant ever enrolled (by selecting the “now” option or the “in 1 week” option).

## Results

***Inferred Urgency.*** Consistent with Hypothesis 1, participants inferred that our implicit recommendation that they take the assessment was *less* urgent in the Simultaneous Pre-commitment condition ( $M = 3.57$ ,  $SD = 1.89$ ;  $b = -.30$ ,  $p = .002$ ,  $d = .16$ ) and *more* urgent in the Sequential Pre-commitment condition ( $M = 4.23$ ,  $SD = 1.81$ ;  $b = .36$ ,  $p < .001$ ,  $d = .20$ ) than it was in the No Pre-commitment condition ( $M = 3.87$ ,  $SD = 1.79$ ).

***Immediate Adoption.*** Offering some support for Hypothesis 2, participants in the Simultaneous Pre-commitment condition were marginally less likely to immediately enroll in the assessment (28.2 percent) than participants in the No Pre-commitment condition (32.0 percent;  $b = -.038$ ,  $p = .098$ ). In terms of its relative effect, this represents an 11.9% decrease in immediate



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adoption. In support of Hypothesis 4, this marginal negative effect was mediated by inferred urgency ( $b = -.011$ , 95% CI =  $[-.020, -.004]$ ). This result is consistent with our theory that simultaneous pre-commitment may reduce immediate adoption of farsighted options because it decreases inferred urgency.

**Overall Adoption.** Offering simultaneous pre-commitment (inviting people to take the assessment “now or later”) resulted in greater overall adoption of the assessment (48.0 percent enrolled) than not offering pre-commitment (32.0 percent enrolled;  $b = .160$ ,  $p < .001$ ; representing a 50.0% relative increase). Supporting Hypothesis 4, inferred urgency *negatively* mediated the relationship between simultaneous pre-commitment and overall adoption ( $b = -.014$ , 95% CI =  $[-.025, -.005]$ ), as illustrated in Figure 4 Panel A. This suggests that offering consumers simultaneous pre-commitment decreases inferred urgency, which may curb take-up of farsighted activities like completing a financial well-being assessment.

Confirming Hypothesis 3, we found that participants in the Sequential Pre-commitment condition were more likely to enroll in the assessment (57.4 percent enrolled) than participants in the No Pre-commitment condition (32.0 percent enrolled;  $b = .254$ ,  $p < .001$ ). In terms of its relative effect, this represents a 79.4% increase in overall adoption. Again, supporting Hypothesis 4, inferred urgency *positively* mediated this effect ( $b = .014$ , 95% CI =  $[.006, .023]$ ), as illustrated in Figure 4 Panel B. These results are consistent with our theory that sequential pre-commitment may increase overall adoption of a farsighted behavior because it signals heightened urgency.

In addition, a Wald test confirmed that participants in the Sequential Pre-commitment condition were more likely to enroll in the assessment than participants in the Simultaneous Pre-commitment condition ( $p < .001$ ; representing a 19.6% relative increase), which supports

Hypothesis 3. And, we confirmed that the difference in overall adoption between the Sequential Pre-commitment and Simultaneous Pre-commitment conditions was also mediated by inferred urgency ( $b = .028$ , 95% CI = [.018, .040]), in line with Hypothesis 4.

**Perceived Convenience.** Consistent with predictions from previous research, as shown in Figure 4, offering pre-commitment increased the perceived convenience of taking the assessment, regardless of whether the pre-commitment offer was simultaneous ( $b = .37$ ,  $p < .001$ ) or sequential ( $b = .29$ ,  $p = .001$ ). When participants perceived greater convenience, they were more likely to enroll in the assessment ( $b = .09$ ,  $p < .001$  for simultaneous pre-commitment and  $b = .08$ ,  $p < .001$  for sequential pre-commitment). Perceived convenience helps explain why the pre-commitment offers increased enrollment in the assessment (indirect effects:  $b = .034$ , 95% CI = [.018, .050] for simultaneous pre-commitment and  $b = .024$ , 95% CI = [.010, .040] for sequential pre-commitment). Of particular importance, perceived convenience did not differ between the two pre-commitment designs ( $p = .36$ ), and thus, it cannot explain why sequential pre-commitment resulted in greater overall adoption than simultaneous pre-commitment (see Web Appendix L for more details).

**Follow-Through Behavior.** Finally, we measured whether participants actually completed the optional financial well-being assessment. Compared to participants in the No Pre-commitment condition (in which 9.6% completed the assessment), participants were more likely to complete the assessment if they were assigned to the Sequential Pre-commitment condition (18.8% completed it;  $b = .092$ ,  $p < .001$ ) or the Simultaneous Pre-commitment condition (15.5% completed it;  $b = .059$ ,  $p < .001$ ). A Wald test confirmed that participants in the Sequential Pre-commitment condition were marginally more likely to finish the assessment than participants in the Simultaneous Pre-commitment condition ( $p = .061$ ).

## Discussion

In an incentive compatible experiment, we again found support for our theory that inferred urgency shapes the way different forms of pre-commitment influence consumer choice. Of note, unlike in our field experiment and in Study 2 where offering simultaneous pre-commitment did not have a significant effect on overall adoption, in this study offering simultaneous pre-commitment increased overall adoption of a financial well-being assessment compared to not offering pre-commitment. This may be because the positive mechanisms that extant theory predict to make pre-commitment offers attractive had a strong influence in this study. Specifically, as shown earlier, participants in the Simultaneous Pre-commitment condition found taking a financial assessment more convenient than participants in the No Pre-commitment condition (see Figure 4). Our evidence suggests that the lack of urgency signaled by the simultaneous pre-commitment offer still curbed take-up of the financial well-being assessment, but the added convenience of this offer mattered more to participants. As a result, simultaneous pre-commitment had a *positive* total effect on the take-up of the assessment in this study. This suggests that accounting for the relative magnitudes of various competing mechanisms is necessary in order to predict the main effect of simultaneous pre-commitment on overall adoption of farsighted behaviors. Sequential pre-commitment, however, increases adoption by harnessing both convenience and heightened urgency. Consequently, sequential pre-commitment has an unambiguously positive effect on the overall adoption of farsighted behaviors and produces more adoption of such behaviors than simultaneous pre-commitment.

## GENERAL DISCUSSION

Offering pre-commitment—that is, offering people a choice to commit now to do something later—has previously been theorized to uniformly increase the adoption of farsighted behaviors. In this paper, we reexamine this assumption. We develop a theory about how the design of pre-commitment offers can lead consumers to draw inferences about the urgency of a marketer’s recommendation to act, which helps determine when pre-commitment will promote the adoption of farsighted behavior and when it will not.

In a field experiment (Study 1), we show that, contrary to experts’ predictions, *simultaneously* offering consumers the option to start saving now and the option to pre-commit to save in the future (i.e., simultaneous pre-commitment) causes people to save less money over our study period than only offering them the option to start saving now. This is because offering simultaneous pre-commitment reduces the number of people who save immediately, without causing more people to save overall. Two pre-registered online experiments (Studies 2 and 3) support our theoretical account of why simultaneous pre-commitment reduces total saving in our field experiment and, conversely, why offering *sequential* pre-commitment has been shown to increase savings (Thaler and Benartzi 2004). Specifically, simultaneous pre-commitment decreases inferred urgency, which helps explain why it reduces immediate adoption and may not increase overall adoption. Moreover, offering sequential pre-commitment—that is, inviting the future adoption of a farsighted behavior only after people decline to adopt it immediately—increases inferred urgency, which helps explain why it increases overall adoption compared to both simultaneous pre-commitment and not offering pre-commitment. Altogether, across one field and two laboratory experiments including over 10,000 participants, involving diverse populations, and examining a variety of farsighted behaviors, we provide robust support for our

theory about the role of inferred urgency in consumers' responses to different types of pre-commitment offerings.

### ***Practical Implications***

The current research has important implications for marketers and policy makers hoping to increase the adoption of farsighted behaviors like saving. First, our research sheds light on how to design effective pre-commitment strategies. Prior research has primarily shown that offering people a single pre-commitment option (i.e., inviting people to commit now to adopt a behavior later) leads to more future-oriented choices than only inviting people to adopt the behavior now. But when applying this knowledge, marketers and policy makers may assume that they can simply add a pre-commitment option on top of an immediate adoption option (perhaps due to their assumption that offering more options can better cater to individuals' heterogeneous preferences). They may particularly favor simultaneous pre-commitment because sequential pre-commitment can be costlier to implement (given that it requires repeated communication). These same considerations motivated us to test the efficacy of simultaneous pre-commitment in our field experiment. However, our work suggests that simultaneous pre-commitment is less effective than sequential pre-commitment and sometimes (as in our field experiment) less effective than only offering the option to adopt a behavior immediately. Importantly, it can lead consumers to delay action in contexts where such delays are costly (e.g., delays to enroll in savings programs can lead to less accumulated savings, delays to update software can increase likelihood of malware attacks, delays to obtain recommended health screenings can prevent early disease detection, and delays to vaccinate can lead to unnecessary illness).

Furthermore, our findings highlight the value of understanding when choice sets presented by marketers and policy makers inadvertently communicate an urgent recommendation

(or a lack thereof) to take action. More generally, our research suggests that seemingly innocuous aspects of the design of interventions (e.g., the simultaneous vs. sequential presentation of a pre-commitment option) can shape people’s inferences and responses. As marketers attempt to leverage psychological principles (e.g., present bias) to motivate behavior change and adopt interventions from prior research, it is natural to modify the designs of those interventions to fit specific field settings, but these modifications can inadvertently leak information that ultimately harms the efficacy of the interventions. This points to the critical need to pilot-test interventions and probe what inferences they produce (Reiff et al. 2022). After such pilots, marketers and policy makers can then revise their designs to guard against unintentional information leakage before rolling out interventions at scale.

***Limitations and Future Directions***

Our research has several limitations, which also open up interesting directions for future research. First, our studies find that inferred urgency *partially* mediates the relationship between pre-commitment offers and farsighted decisions, suggesting that additional processes beyond those we theorized about in this paper may influence people’s responses to pre-commitment offers. For instance, participants may choose randomly over options in a given menu, which could have contributed to the observed effects of the pre-commitment offers in our hypothetical scenario in Study 2. Since simultaneous pre-commitment is the only offer we studied that included “now” and “later” enrollment options on the same menu, random choice could partially explain why people in the simultaneous pre-commitment condition were less likely to enroll immediately than those only given the option to enroll “now” in the control condition.

Another limitation of this research is that sequential pre-commitment is the only offer we studied that asks people *twice* whether they would adopt a behavior, and the mere repetition

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could contribute to the offer's positive effects on overall take-up of farsighted behaviors.<sup>11</sup>

Future research should further explore these alternative accounts and others.

We also study farsighted behaviors involving both monetary (Studies 1 and 2) and time (Study 3) costs, and we find support for our theory about inferred urgency across both resource types. That said, the effect of simultaneous pre-commitment on overall adoption seemed to vary across resource types; simultaneous pre-commitment had a null effect in Studies 1 and 2 but a positive effect in Study 3. As we proposed earlier, the impact of simultaneous pre-commitment on overall adoption appears to depend on the relative magnitude of competing mechanisms: a lack of inferred urgency reduces adoption, while mechanisms related to present bias and resource slack increase adoption. The latter positive mechanisms may have played a stronger role in driving the impact of simultaneous pre-commitment when time costs (as opposed to monetary costs) were involved.<sup>12</sup> Future research should further investigate how the overall impact of pre-commitment offers varies with the resource type.

Further, all of the pre-commitment offers we study involve *non-binding* commitments; that is, after committing to do something later (e.g., to increase their savings contribution rate in six months), people can always change their mind and nullify their decisions. An alternative design for pre-commitment offers could include *binding* commitments, which require people to stick to their initial commitment, and this design feature may be a key determinant of take-up

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<sup>11</sup>To test this account, sequential pre-commitment could be compared to an additional condition that first asks people whether they would like to adopt a behavior immediately, and then if they decline, asks them to consider immediate adoption again (e.g., "Are you sure about not increasing your savings now?"). Existing theory and the results of Study 3 suggest that offering people the immediate enrollment option twice may be less effective than sequential pre-commitment at increasing adoption of farsighted behaviors because the former does not include a pre-commitment offer and cannot leverage the psychology that people feel less averse to adopting farsighted behaviors in the future.

<sup>12</sup> We speculate that this may be because people typically expect to have more discretionary time in the future than in the present but expect less growth when thinking about their discretionary money (Zauberman and Lynch 2005). Thus, pre-commitment options that allow people to delay time costs into the future—when they think they will have more time to spend—will be particularly attractive (more so than pre-commitment options that delay monetary costs).

(Karlan and Linden 2015). Future research should test whether our theory about inferred urgency also applies to binding pre-commitment offers.

In addition, though our research suggests that simultaneous pre-commitment leads consumers to infer that the adoption of a farsighted behavior is not urgently recommended, marketers and policy makers may be able to improve the effectiveness of simultaneous pre-commitment offers by changing the framing of options. For instance, when Google prompts consumers to update their notification settings, they offer simultaneous pre-commitment, presenting the options “continue” or “ask me later.” However, Google prints “continue” in bright blue letters, while “ask me later” appears in a light grey font, which may signal that the company recommends completing the update sooner rather than later. Future research testing different strategies for changing the information leaked by pre-commitment offers would be valuable. Alternatively, Beshears et al. (2021) suggest leveraging a conceptually distinct psychological process to improve simultaneous pre-commitment. They show that offering pre-commitment with the delayed behavior starting shortly after a moment that feels like a fresh start (e.g., after a birthday, the first day of spring) increases retirement savings (relative to offering pre-commitment at an equivalent, unlabeled time delay; e.g., in 2 months). Future work can more broadly examine how to frame pre-commitment options in ways that enhance overall adoption of farsighted behaviors.

Finally, it would be valuable to study moderators of the effects documented in our research. For instance, the extent to which people are influenced by the urgency of a marketer’s recommendation may depend on consumers’ trust in that marketer. Future research is needed to understand how underlying attitudes towards whoever presents choices may moderate responses



to implicit recommendations and thus influence the effects of different types of pre-commitment offerings.

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Figure 1. Summary of the Three Choice Sets Studied in this Paper.

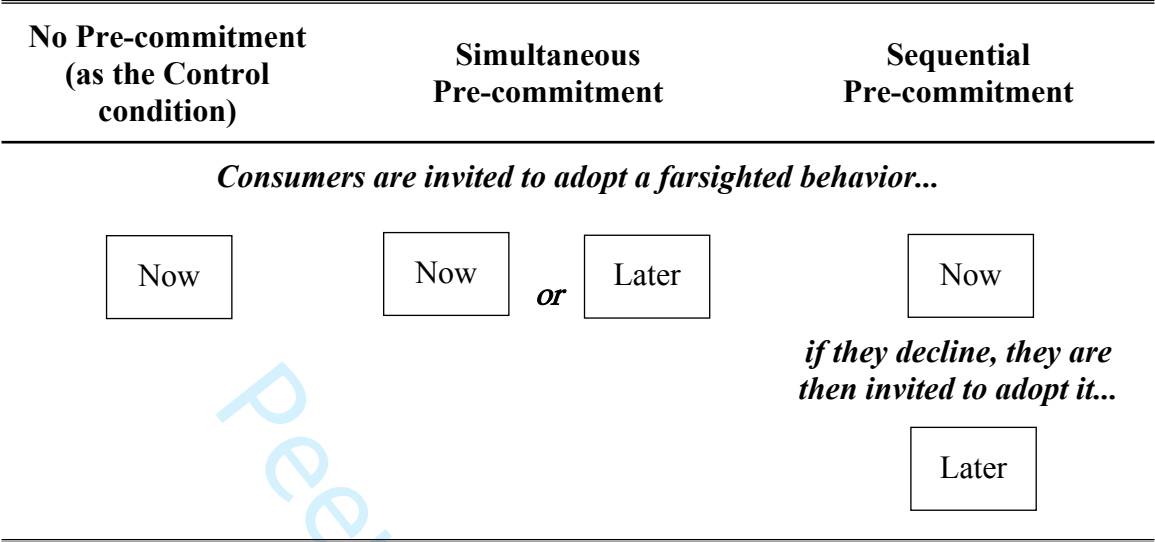


Table 1. Summary of the Theory’s Key Predictions.

| Measure                                       | Effects of Simultaneous<br>Pre-commitment<br>(vs. No Pre-commitment)                                            | Effects of Sequential<br>Pre-commitment                                                                                                                      |
|-----------------------------------------------|-----------------------------------------------------------------------------------------------------------------|--------------------------------------------------------------------------------------------------------------------------------------------------------------|
| Inferred Urgency                              | Decrease (H1)                                                                                                   | Increase (H1)                                                                                                                                                |
| Immediate Adoption of<br>Farsighted Behaviors | Decrease (H2) Because of<br>Decreased Urgency (H4)                                                              | No Effect Because Sequential<br>Pre-commitment and No Pre-<br>commitment Are Identical up to<br>the Point of the Immediate<br>Adoption Decision <sub>a</sub> |
| Overall Adoption of<br>Farsighted Behaviors   | Ambiguous Effect Because<br>Decreased Urgency (H4) May<br>be Offset by Counteracting<br>Mechanisms <sub>b</sub> | Increase (H3) Because of<br>Increased Urgency (H4)                                                                                                           |

<sub>a</sub> For more information see footnote 1. <sub>b</sub> For more information see the theory section on overall adoption.

**Table 2. Summary of Key Results.** We report raw means and standard deviations in parentheses. Statistically significant differences between conditions are indicated by stars (\*  $p \leq .10$ , \*\*  $p \leq .05$ , \*\*\*  $p \leq .01$ ) and were estimated using the primary analytical model specified in each study. Checkmarks in the far-right column indicate that inferred urgency mediates all reported differences in adoption between conditions.

|         | Sample & Measures  | Details                                                                                                    | No Pre-commitment | Simultaneous Pre-commitment | Sequential Pre-commitment | Mediation via Inferred Urgency |
|---------|--------------------|------------------------------------------------------------------------------------------------------------|-------------------|-----------------------------|---------------------------|--------------------------------|
| Study 1 | Sample             | N = 5,196 university employees                                                                             | n = 2,600         | n = 2,596                   |                           |                                |
|         | Immediate adoption | Whether employees enrolled in a savings plan immediately (=1), or not (=0)                                 | .075 (.26)        | ***<br>.059 (.23)           |                           |                                |
|         | Overall adoption   | Whether employees enrolled in a savings plan immediately or at a delay (=1), or not at all (=0)            | .123 (.33)        | n.s.<br>.116 (.32)          |                           |                                |
| Study 2 | Sample             | N = 2,682 participants from Prolific                                                                       | n = 901           | n = 895                     | n = 886                   |                                |
|         | Inferred urgency   | Inference about the urgency of the employer's recommendation (1-7 response scale)                          | 4.88 (1.52)       | ***<br>4.55 (1.63)          | ***<br>5.22 (1.30)        |                                |
|         | Immediate adoption | The number of employee benefits participants enrolled in immediately (ranging from 0-3)                    | 1.86 (.78)        | ***<br>1.49 (.89)           | ***<br>1.93 (.85)         | ✓                              |
| Study 3 | Sample             | N = 2,398 participants from MTurk                                                                          | n = 806           | n = 794                     | n = 798                   |                                |
|         | Inferred urgency   | Inference about the urgency of the researcher's recommendation (1-7 response scale)                        | 3.87 (1.79)       | ***<br>3.57 (1.89)          | ***<br>4.23 (1.81)        |                                |
|         | Immediate adoption | Whether participants enrolled in a financial assessment immediately (=1), or not (=0)                      | .320 (.47)        | *<br>.282 (.45)             | **<br>.331 (.47)          | ✓                              |
| Study 3 | Overall adoption   | Whether participants enrolled in a financial assessment immediately or at a delay (=1), or not at all (=0) | .320 (.47)        | ***<br>.480 (.50)           | ***<br>.574 (.49)         | ✓                              |

**Table 3. Descriptions of Targeted Plans.** The table is identical to the one presented in Beshears et al. (2021) because it describes the same retirement plans in the same universities.

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

**Figure 2. Randomization.** Employees who received the No Pre-commitment mailings were not included in the Beshears et al. (2021) paper and are only analyzed here. Employees who received the Simultaneous Pre-commitment mailings linked with temporal landmarks were not included in analyses in this paper (this condition is shaded grey). The Simultaneous Pre-Commitment condition linked with temporal landmarks was oversampled because the stratification procedure was designed to allow Beshears et al. (2021) to make inferences about the relative impact of different types of temporal landmarks referenced such as birthdays, the start of spring, new year’s, etc. (see details in Web Appendix B).

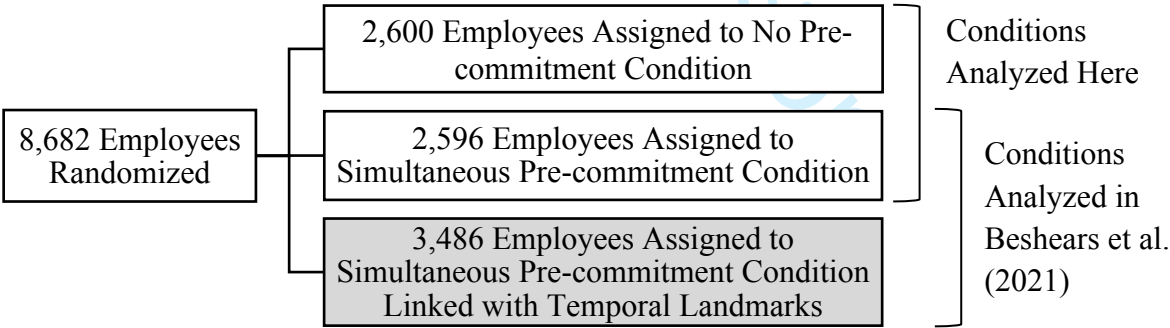




Figure 3. Mailings from Field Experiment (Study 1)

### No Pre-commitment Mailing

**Stop Waiting... Start Saving!**

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

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

By checking ☒ **Yes**, you will:

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

Once you enroll in the TDR, you have the freedom to change your contribution rate and investment options at any time. To find information about other investments should you want to invest in other funds, visit [redacted] or call [redacted]

Tear at perforation

**Your response is needed by November 1, 2013!**

☒ **Yes! Enroll me in the TDR NOW.**

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

Signature \_\_\_\_\_
Date \_\_\_\_\_

\* Subject to IRS limits.  
\*\* Your enrollment time, contribution rate and investment options will not change unless you make another election. Your signature indicates that you know that you can change your elections at any time by visiting [redacted] or calling [redacted]

### Simultaneous Pre-commitment Mailing

**Stop Waiting... Start Saving!**

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

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

By checking ☒ **Yes**, you will:

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

Once you enroll in the TDR, you have the freedom to change your contribution rate and investment options at any time. To find information about other investments should you want to invest in other funds, visit [redacted] or call [redacted]

Tear at perforation

**Your response is needed by November 1, 2013!**

☐ **Yes! Enroll me in the TDR NOW.**

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

☐ **Yes! Enroll me in the TDR IN 2 MONTHS.**

– I will begin contributing 5% of my eligible pay on a pre-tax basis **in 2 months.**\* I direct that my contribution be invested in a TIAA-CREF target-date fund, based on my age and estimated retirement date, if I have not previously selected investment funds. If I have previously selected investment funds, I direct that my contribution be invested in those funds.\*\*

Signature \_\_\_\_\_
Date \_\_\_\_\_

\* Subject to IRS limits.  
\*\* Your enrollment time, contribution rate and investment options will not change unless you make another election. Your signature indicates that you know that you can change your elections at any time by visiting [redacted] or calling [redacted]

**Table 4. The Effect of Offering Employees Simultaneous Pre-commitment in Study 1.**

Model 1 reports an ordinary least squares (OLS) regression where the dependent variable is a binary variable reflecting whether an employee immediately increased their contribution rate to the targeted savings plan. Model 2 reports an OLS regression where the dependent variable is a binary variable reflecting whether an employee increased their contribution rate to the targeted savings plan by the end of our study period. Model 3 reports an OLS regression where the dependent variable is an employee’s average savings rate in the targeted savings plan during our study period.

|                                    | Model 1:<br>Immediate<br>Adoption | Model 2:<br>Overall<br>Adoption | Model 3:<br>Average Savings<br>Rate |
|------------------------------------|-----------------------------------|---------------------------------|-------------------------------------|
| <i>Simultaneous Pre-commitment</i> | -.019***<br>(.007)                | -.009<br>(.009)                 | -.0014**<br>(.001)                  |
| <i>University FEs</i>              | Yes                               | Yes                             | Yes                                 |
| <i>Controls</i>                    | Yes                               | Yes                             | Yes                                 |
| <i>University FE x Controls</i>    | Yes                               | Yes                             | Yes                                 |
| R-squared                          | .07                               | .08                             | .11                                 |
| Observations                       | 5,196                             | 5,196                           | 5,196                               |

\*  $p \leq .10$ , \*\*  $p \leq .05$ , \*\*\*  $p \leq .01$

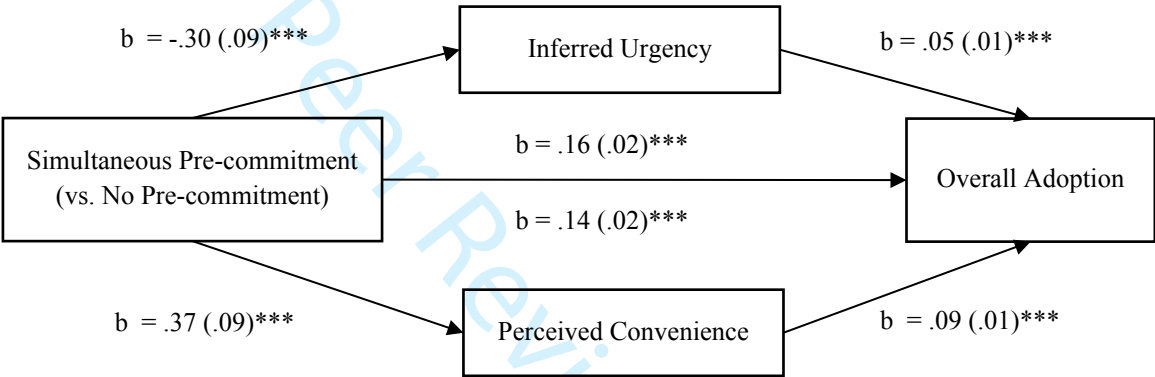
Notes: Standard errors robust to heteroskedasticity are reported in parentheses. Controls include gender, age decile, tenure decile, tenure decile, salary decile, faculty status, and birth month.

**Table 5. Robustness Checks for the Effect of Offering Simultaneous Pre-commitment in Study 1.** Each row corresponds to a robustness check testing the effects of simultaneous pre-commitment (vs. no pre-commitment). Unless otherwise specified, the models are similar to the primary model specified in the Analysis Strategy section. In the right three columns, we report the coefficients from the regressions in the relevant row, with heteroskasticity robust standard errors in parentheses and significance indicated by \*  $p \leq .10$ , \*\*  $p \leq .05$ , \*\*\*  $p \leq .01$ . Full descriptions of each robustness check are reported in Web Appendix E.

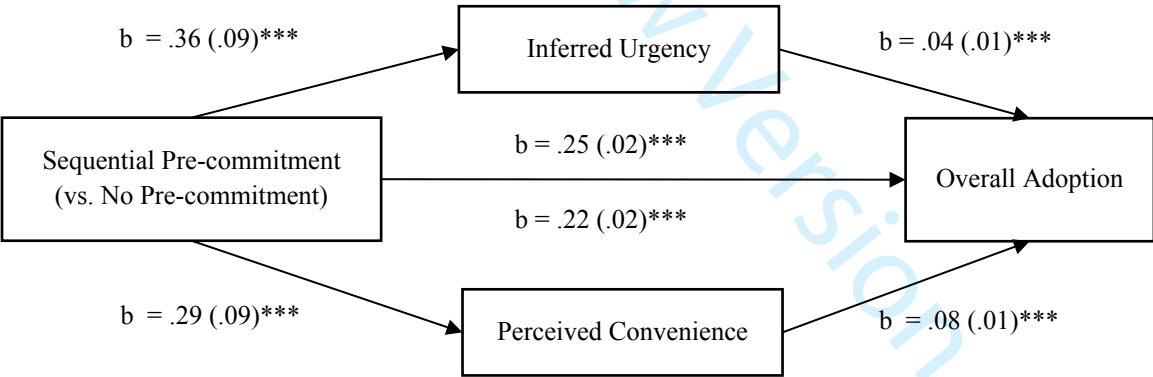
| Robustness Check                                                                                                                                                                                                                                                                                                                                                                                                                       | Results                |                    |                      |
|----------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|------------------------|--------------------|----------------------|
|                                                                                                                                                                                                                                                                                                                                                                                                                                        | Immediate Adoption     | Overall Adoption   | Average Savings Rate |
| <b>Measuring Decisions with Cards Mailed back to the One University that Tracked Responses.</b> Here we measure immediate and overall adoption by analyzing the choices made on returned response postcards at the one university that shared this information (N=2,029). See Table W8.                                                                                                                                                | -.016*<br>(.009)       | -.008<br>(.009)    |                      |
| <b>Measuring Overall Adoption Differently.</b> To measure overall adoption in a way meant to capture employees' direct responses to our mailers, we only counted someone as enrolling if the first time their contribution rate increased (relative to their rate in September 2013) matched one of the month(s) offered to them in our mailing. See Table W9.                                                                         |                        | -.003<br>(.007)    |                      |
| <b>Running Logistic and Fractional Logit Regressions.</b> Here we rely on logistic regression to analyze immediate and overall adoption and fractional logistic regression to analyze average savings rates. The coefficients reported are in terms of log odds ratios (and thus have different interpretations than those from the other robustness checks). See Table W10.                                                           | -.359***<br>(.118)     | -.099<br>(.092)    | -.163**<br>(.081)    |
| <b>Dropping Those with Missing Data.</b> Here we drop data from employees who have missing data for salary or contributions in all months in the study period (rather than assuming 0's). See Table W11.                                                                                                                                                                                                                               | -.020***<br>(.007)     | -.010<br>(.009)    | -.0015**<br>(.001)   |
| <b>Dropping Those with Missing Data in Key Months.</b> Here we drop data from employees who have missing data for salary or contributions in one of the key months used to calculate immediate and overall adoption (rather than assuming 0's). See Table W12.                                                                                                                                                                         | -.021***<br>(.008)     | -.009<br>(.010)    |                      |
| <b>Varying the Cutoff for a Contribution Rate Increase.</b> Here we use different thresholds to measure immediate and overall adoption. Since we constructed the adoption variables by assessing whether increases in contribution rates occurred, here we ensure our results are not spuriously driven by how we rounded contribution rates. See Table W13. We counted a contribution rate as having increased if the increase was... | $\geq .1\%$ of salary  | -.020***<br>(.007) | -.009<br>(.009)      |
|                                                                                                                                                                                                                                                                                                                                                                                                                                        | $\geq .01\%$ of salary | -.021***<br>(.007) | -.008<br>(.009)      |
| <b>Including Limited Controls.</b> Here we control for the interaction between university and birth month (due to the stratified random assignment employed) but drop other controls (i.e., gender, age, tenure, salary, and faculty status). See Table W14.                                                                                                                                                                           | -.017**<br>(.007)      | -.007<br>(.009)    | -.0012*<br>(.001)    |

**Figure 4. Multi-Mediator Models for Overall Adoption (Study 3).** This figure depicts how inferred urgency and perceived convenience explain the effects of offering simultaneous pre-commitment (Panel A) and sequential pre-commitment (Panel B) on overall adoption of the financial well-being assessment. All regression coefficients are unstandardized, and standard errors are presented in parentheses. The coefficients above the paths from Simultaneous Pre-commitment and Sequential Pre-commitment to Overall Adoption represent the total effects, and the coefficients below the paths represent the direct effects. Coefficients significantly different from zero are followed by asterisks (\* $p \leq .10$ , \*\* $p \leq .05$ , \*\*\*  $p \leq .01$ ).

Panel A. Simultaneous Pre-commitment



Panel B. Sequential Pre-commitment



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## Save More Today or Tomorrow: The Role of Urgency in Pre-Commitment Design

### Web Appendix

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Our pre-registrations, materials, non-proprietary data, and code are available at  
<https://researchbox.org/434>.

These materials have been supplied by the authors to aid in the understanding of their paper. The AMA is sharing these materials at the request of the authors.

WEB APPENDIX A. EXPERT PREDICTION SURVEYS

Panel A. Survey with Marketing Professors

**Methods.** We emailed all 408 tenure-track marketing professors at the top 30 U.S. business schools (as ranked by *U.S. News and World Report*) on November 15, 2017 and invited them to take a short survey. The survey asked professors to imagine that a company was testing two campaigns with the goal of increasing employees' contributions to its retirement saving plan. The first campaign offered employees the option to start saving now (i.e., the No Pre-commitment campaign) and the second mailing gave employees the choice to either (i) start saving now or (ii) pre-commit to begin saving in four months (i.e., the Simultaneous Pre-commitment campaign). Note that we did not label these campaigns this way in the survey. The survey asked professors to predict which mailing would lead employees to save more over nine months.<sup>1</sup> Demographic information was not collected on respondents. As pre-registered, we stopped data collection after seven days and excluded five professors who reported that they were familiar with our Study 1 results, leaving us with 101 professors for analysis.

**Results.** Of the 85 respondents who clearly favored one campaign over the other (rather than predicting the two campaigns would yield the same results or selecting "I don't know"), the proportion who believed the Simultaneous Pre-commitment campaign would lead to more savings (62.4%) was significantly higher than 50% ( $p = .023$  in a two-sided, one-sample proportion test). As a more conservative test, we also analyzed the whole sample of 101 professors and treated their responses as a ternary outcome variable, taking the following three values: "Simultaneous Pre-commitment" when the prediction was that the Simultaneous Pre-commitment campaign would lead to more savings, "No Pre-commitment" when the prediction was that the No Pre-commitment campaign would lead to more savings, or "other" when the prediction was that the two campaigns would lead to the same amount of savings or the response was "I don't know". We then calculated the p-value for the following null hypothesis: the proportion of the population responding Simultaneous Pre-commitment = the proportion of the population responding No Pre-commitment = some fixed number  $x$ . We chose  $x$  to generate the

<sup>1</sup>We think "save more over the nine months" is likely interpreted by survey respondents as the accumulated savings during that time window, which corresponds closely with the average savings rate variable we used in our field experiment as an outcome measure.



most conservative p-value by setting  $x$  equal to half of the sample proportion who responded either Simultaneous Pre-commitment or No Pre-commitment (i.e.,  $x = .5 * (53 + 32) / 101 = .421$ ). Under the null hypothesis with  $x = .421$ , we calculated the probability of seeing a proportion of Simultaneous Pre-commitment responses (out of Simultaneous Pre-commitment plus No Pre-commitment responses) that is as extreme or more extreme in the two-sided sense than our observed probability (i.e.,  $53 / (53 + 32) = .624$ ). This probability (i.e., the p-value for this test) is .041.

### **Panel B. Survey with Financial Advisors**

**Methods.** We recruited financial advisors specialized in retirement savings at (1) a symposium organized by Voya Financial on September 27, 2017, and (2) an event organized by LPL Financial on October 2, 2017. In the middle of each event, consultants were presented with a slide describing two savings campaigns—a campaign that only presented people with the option to start saving now (i.e., the No Pre-commitment campaign) and a campaign that allowed people to either start saving now or pre-commit to begin saving in four months (i.e., the Simultaneous Pre-commitment campaign). Note that we did not label these campaigns this way on the slide. We then invited the audience to take an anonymous poll. Financial advisors were asked to predict which campaign would lead to more savings over the following nine months. Demographic information on respondents was not collected in this one-question poll. A total of 239 financial advisors responded to the poll.

**Results.** Of the 229 respondents who favored one campaign over the other (as opposed to predicting the two campaigns would yield the same results or selecting “I don't know”), the proportion who believed the Simultaneous Pre-commitment campaign would lead to more savings (73.8%) was significantly higher than 50% ( $p < .001$  in a two-sided, one-sample proportion test). We conducted the same conservative analysis on the whole sample of 239 financial advisors as we did to marketing professors. Here,  $x$  again equals to half of the sample proportion who responded either Simultaneous Pre-commitment or No Pre-commitment. That is,  $x = .5 * (169 + 60) / 239 = .479$ . Under the null hypothesis with  $x = .479$ , we calculated the probability of seeing a proportion of Simultaneous Pre-commitment responses (out of Simultaneous Pre-commitment plus No Pre-commitment responses) that is as extreme or more extreme in the two-sided sense than our observed probability (i.e.,  $169 / (169 + 60) = .738$ ). This probability (i.e., the p-value for this test) is less than .001.

**WEB APPENDIX B. ADDITIONAL INFORMATION ON SAMPLE AND  
RANDOMIZATION (STUDY 1)**

***Randomization***

Employees were randomized into one of three primary mailing conditions: the No Pre-commitment mailing, the Simultaneous Pre-commitment mailing, and the Framed Simultaneous Pre-commitment mailing.<sup>2</sup> Employees assigned to the No Pre-commitment mailing were encouraged to sign up to save (or to save more) immediately. Those assigned to the Simultaneous Pre-commitment mailing were given the opportunity to sign up to save (or to save more) either immediately or after a time delay ranging from two to six months (e.g., “in two months”). Finally, those assigned to the Framed Simultaneous Pre-commitment mailing received a mailing identical to the Simultaneous Pre-commitment mailing, except that the pre-commitment time window referenced (e.g., “in two months”) was replaced by a reference to a temporal landmark associated with the same length of delay (e.g., “following your next birthday,” “following Thanksgiving”). The temporal landmarks were either holidays (Thanksgiving, New Year’s, Martin Luther King Day, Valentine’s Day, and the Spring Equinox) or employees’ birthdays. Some of these temporal landmarks feel like the beginning of a new cycle and are associated with fresh starts (New Year’s, the first day of spring, employees’ birthdays), and some are ordinary holidays that do not signal a new beginning (Thanksgiving, Martin Luther King Day, Valentine’s Day; Beshears et al. 2021). Note that in the current paper we only used data for employees assigned to receive either the No Pre-commitment mailing or the Simultaneous Pre-commitment mailing.

As explained in the manuscript, randomization was stratified by birth month within each university because birthdates partially determined which mailings employees received. Specifically, as illustrated in Figure W1, employees were divided into two sub-groups before randomization: those with birthdays between November and March (hereafter referred to as the “November-March birthday group”), and others (hereafter referred to as the “April-October birthday group”). Those with birthdays between November and March were divided evenly among four experimental mailings: the No Pre-commitment mailing, the Simultaneous Pre-

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<sup>2</sup> This condition is labelled in the manuscript as “the Simultaneous Pre-commitment Condition Linked with Temporal Landmarks.”



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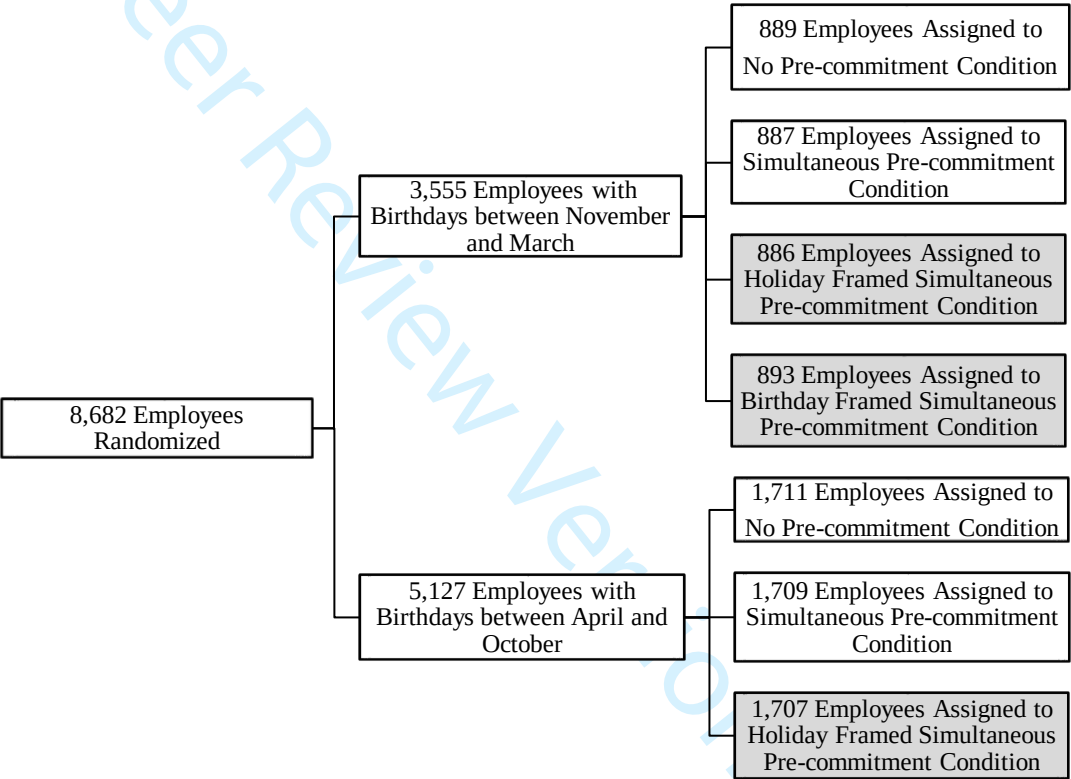
commitment mailing, and two sub-categories of the Framed Simultaneous Pre-commitment mailings—the Birthday Framed Simultaneous Pre-commitment mailing that offered employees an opportunity to begin saving (or to save more) following their next birthday and the Holiday Framed Simultaneous Pre-commitment mailing that offered employees an opportunity to begin saving (or to save more) following a future holiday. Among employees in the November-March birthday group that were offered pre-commitment, the length of time until the offered pre-commitment option would take effect was determined by employees' birth month. For instance, consider an employee whose birthday is in December. Recall that our mailings went out in early October. If this employee were assigned to the Simultaneous Pre-commitment mailing, she would be offered the opportunity to start saving “in three months” (i.e., in January). If she were assigned to the Holiday Framed Simultaneous Pre-commitment mailing, she would be offered the opportunity to start saving “after New Year's” (i.e., in January). If she were assigned to the Birthday Framed Simultaneous Pre-commitment mailing, she would be offered the opportunity to start saving “after her next birthday” (again, in January).

Employees with birthdays between April and October were divided evenly among receiving the No Pre-commitment mailing, the Simultaneous Pre-commitment mailing, and the Holiday Framed Simultaneous Pre-commitment mailing. Among employees in the April-October birthday group who were offered pre-commitment, the length of time until the offered pre-commitment would take effect was randomized to be from two to six months (in the Simultaneous Pre-commitment condition) or from Thanksgiving to the Spring Equinox (in the Holiday Framed Simultaneous Pre-commitment condition). Every employee in the Holiday Framed Simultaneous Pre-commitment condition was yoked with an employee in the (un-framed) Simultaneous Pre-commitment condition who was offered the opportunity to start saving (or to save more) at the same time delay. For example, an employee who was randomly assigned to have the opportunity to begin saving after New Year's was yoked with an employee who had the opportunity to begin saving in three months. Notably, past research suggests that New Year's is a particularly meaningful fresh start opportunity (Dai et al. 2014). Thus, among employees in the Holiday Framed Simultaneous Pre-commitment condition, we oversampled assignment to the sub-group that allowed employees to pre-commit to begin saving (or to save more) “after New Year's”. Correspondingly, we oversampled assignment to the sub-group of the

(un-framed) Simultaneous Pre-commitment condition that allowed employees to pre-commit to begin saving (or to save more) “in three months.”

Note that for the current study we only used data from employees that were assigned to the No Pre-commitment or Simultaneous Pre-commitment conditions (889 + 887 + 1,711 + 1,709 = 5,196).

**Figure W1. Randomization.** This figure shows how employees were assigned to conditions. Note the gray boxes reflect the Framed Simultaneous Pre-commitment condition, which are reported in Beshears et al. (2021) and not included in the current paper.



There were 42 employees that were randomized but not included in the analysis because they did not have data collected, were terminated before the baseline data collection, or had conflicting dates of birth.

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## Balance Checks

**Table W1. Summary Statistics by Condition in Study 1.** This table summarizes the mean and standard deviation (in parentheses) of key control variables used in our analyses by experimental condition. The last column shows p-values from statistical tests comparing the conditions.

|                                    | No Pre-commitment<br>Mailing | Simultaneous<br>Pre-commitment<br>Mailing | Simultaneous<br>Pre-commitment Vs.<br>No Pre-commitment |
|------------------------------------|------------------------------|-------------------------------------------|---------------------------------------------------------|
|                                    |                              |                                           | <i>p-value</i>                                          |
| <b>Female</b>                      | 52.65%                       | 51.85%                                    | .562                                                    |
| <b>Age (years)</b>                 | 43.20<br>(12.32)             | 43.00<br>(11.77)                          | .548                                                    |
| <b>Tenure (years)</b>              | 9.51<br>(9.14)               | 9.54<br>(8.93)                            | .906                                                    |
| <b>Baseline Salary<br/>(\$USD)</b> | 56,505.19<br>(35,234.21)     | 58,505.26<br>(36,111.88)                  | .043                                                    |
| <b>Faculty</b>                     | 11.62%                       | 12.75%                                    | .211                                                    |

WEB APPENDIX C. DESCRIPTIONS OF NON-TARGETED PLANS (STUDY 1)

**Table W2. Non-Targeted Plans.** The experimental mailings encouraged employees to increase contributions to a targeted plan (see Table 3 in the manuscript). This table describes other retirement plans (i.e., “Non-targeted plans”) that were also available to employees.

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

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

Notes: Table W1 is identical to the one presented in Beshears et al. (2021) because it relies on the same retirements plans in the same universities.

**WEB APPENDIX D. SUMMARY OF STANDARD PRACTICES IN RETIREMENT  
SAVINGS RESEARCH (STUDY 1)**

We examined all of the papers covered in the most comprehensive review of the literature on contributions to pension plans in recent years (Choi 2015). We identified 33 empirical papers that analyzed contributions to pension plans using savings data in the field.

We coded the outcome variables that these papers analyzed as well as the regression models that these papers used to predict their outcome variables (see Table W3 for a summary). The most common outcome variable studied was whether individuals participated in a given savings plan ( $n = 21$  papers). Among the papers that examined this outcome variable, 16 papers (76%) used OLS regressions to predict such a binary outcome variable.

The second most common outcome variable studied in these papers was people's average pension plan contribution rate ( $n = 13$  papers). Among the papers that examined this outcome variable, 12 papers (92%) used OLS regressions to predict this outcome variable.

We also coded the control variables that these papers included in their regressions. It is a standard practice to control for employees' demographics (e.g., age, gender), income, and employer characteristics. For example, over 80% of the empirical papers in this survey controlled for age and salary. Among the six papers that reported on *field experiments* testing the efficacy of an intervention in increasing savings behavior, four papers (67%) controlled for age and five papers (83%) controlled for salary.

Table W3. Summary of 33 Empirical Papers on Pension Savings Reviewed in Choi (2015)

| Citation                    | Field      | Dependent Variable: |                            | Controls: |        |
|-----------------------------|------------|---------------------|----------------------------|-----------|--------|
|                             | Experiment | Contribution Rate   | Savings Plan Participation | Age       | Income |
| Ameriks J et al. (2003)     |            |                     |                            | Yes       | Yes    |
| Argento et al. (2015)       |            |                     |                            | Yes       | Yes    |
| Bassett et al. (1998)       |            |                     | Yes (model: OLS)           | Yes       | Yes    |
| Bayer et al. (2009)         |            | Yes (model: OLS)    | Yes (model: OLS)           |           |        |
| Benartzi et al. (2012)      |            |                     | Yes (model: OLS)           | Yes       | Yes    |
| Bernheim and Garrett (2003) |            | Yes (model: OLS)    | Yes                        | Yes       | Yes    |
| Beshears et al. (2013)      |            | Yes (model: OLS)    |                            | Yes       | Yes    |
| Beshears et al. (2015)      | Yes        | Yes (model: OLS)    | Yes (model: OLS)           | Yes       | Yes    |
| Brown JR et al. (2011)      |            |                     |                            | Yes       | Yes    |
| Card and Ransom (2011)      |            | Yes                 |                            | Yes       | Yes    |
| Carroll et al. (2009)       |            | Yes (model: OLS)    |                            | Yes       | Yes    |
| Chetty et al. (2014)        |            | Yes (model: OLS)    |                            | Yes       | Yes    |
| Choi et al. (2002)          |            |                     | Yes (model: OLS)           | Yes       |        |
| Choi et al. (2004)          |            | Yes (model: OLS)    | Yes (model: OLS)           | Yes       | Yes    |
| Choi et al. (2009)          |            | Yes (model: OLS)    |                            | Yes       | Yes    |
| Choi et al. (2010)          | Yes        |                     |                            | Yes       | Yes    |
| Choi et al. (2011)          |            |                     | Yes                        | Yes       | Yes    |
| Choi et al. (2012)          | Yes        | Yes (model: OLS)    |                            |           |        |
| Duflo and Saez (2002)       |            |                     | Yes (model: OLS)           | Yes       | Yes    |
| Duflo and Saez (2003)       | Yes        |                     | Yes (model: OLS)           | Yes       | Yes    |
| Duflo et al. (2006)         | Yes        |                     | Yes (model: OLS)           |           | Yes    |
| Dworak-Fisher (2011)        |            |                     | Yes                        | Yes       | Yes    |
| Engelhardt and Kumar (2007) |            |                     | Yes                        | Yes       | Yes    |
| Even and Macpherson (2005)  |            |                     | Yes                        | Yes       | Yes    |
| Goda et al. (2014)          | Yes        |                     |                            | Yes       | Yes    |
| Huberman et al. (2007)      |            | Yes (model: OLS)    | Yes (model: OLS)           | Yes       | Yes    |
| Lusardi and Mitchell (2007) |            |                     |                            | Yes       | Yes    |
| Madrian and Shea (2001)     |            | Yes (model: OLS)    | Yes (model: OLS)           | Yes       | Yes    |
| Mitchell et al. (2007)      |            | Yes (model: OLS)    | Yes (model: OLS)           | Yes       | Yes    |
| Papke (1995)                |            |                     | Yes (model: OLS)           |           |        |
| Papke and Poterba (1995)    |            |                     | Yes (model: OLS)           |           |        |
| Sethi-Iyengar et al. (2004) |            |                     | Yes (model: OLS)           | Yes       | Yes    |
| Skinnyhorn (2013)           |            |                     | Yes (model: OLS)           | Yes       | Yes    |

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**WEB APPENDIX E. ADDITIONAL ANALYSES AND ROBUSTNESS CHECKS (STUDY 1)****Panel A. Additional Analyses**

**Analysis of enrollment in all plans (Table W4).** In the manuscript we focus on changes to contribution rates in the targeted plan because the mailings containing the experimental manipulation encouraged enrollment in the targeted plans. However, employees could have also changed their contribution rates to non-targeted plans that were also offered by their employer (see Web Appendix C for details). In particular, if employees increased contributions to the targeted plan in response to our mailing by simply shifting contributions away from non-targeted plans, this would not reflect an actual increase in savings. Thus, here we examine when and how contributions to *all* plans—including targeted and non-targeted plans—changed, which allows us to more comprehensively capture the effects of offering simultaneous pre-commitment on retirement savings decisions. The table below uses the primary regression specification, examining effects of the Simultaneous Pre-commitment mailing on immediate adoption, overall adoption, and average savings rate in all plans. Importantly, we obtained qualitatively similar results for all plans and targeted plans.

**Table W4. Analysis of Enrollment in All Plans**

|                                    | Model 1:<br>Immediate<br>Adoption | Model 2:<br>Overall<br>Adoption | Model 3:<br>Average<br>Savings Rate |
|------------------------------------|-----------------------------------|---------------------------------|-------------------------------------|
| <i>Simultaneous Pre-commitment</i> | -.018**<br>(.007)                 | -.012<br>(.009)                 | -.0027***<br>(.001)                 |
| R-squared                          | .09                               | .12                             | .52                                 |
| Observations                       | 5,196                             | 5,196                           | 5,196                               |

Standard errors robust to heteroskedasticity in parentheses. The regressions control for gender, age decile, tenure decile, salary decile, faculty status, and birth month. We allowed the coefficients on the control variables to vary by university. \* $p \leq .10$ , \*\* $p \leq .05$ , \*\*\* $p \leq .01$ .

**Contribution rate increase (Table W5).** For employees who increased their contribution rates from September 2013 (right before our experiment) to June 2014 (the last month of the observation period), we measured the amount of increase in their contribution rate by taking the difference in their contribution rates (as revealed in the payroll data) between September 2013 and June 2014. We refer to this difference as “contribution rate increase.” As explained earlier,

all the mailings offered a default contribution rate of either 3% or 5% which employees could not change via mail, and only University D sent mailings to employees who were already enrolling at a non-zero contribution rate. As a result, among employees who increased their contribution rates to the targeted plans, approximately 50% increased by 3% or 5%. Notwithstanding, some employees set their own contribution rates in the targeted plans via phone or internet. Thus, we could assess whether the Simultaneous Pre-commitment mailing had a separate impact on the size of the increase in contribution rates, independent of its effect on immediate or overall adoption. When we used the OLS regression specification described in the paper to predict contribution rate increase, we did not find evidence that the size of contribution rate changes differed statistically significantly between conditions.

**Table W5. Contribution Rate Increase**

|                                    | Model 1:<br>Contribution Rate<br>Increase |
|------------------------------------|-------------------------------------------|
| <i>Simultaneous Pre-commitment</i> | -.005<br>(.005)                           |
| R-squared                          | .40                                       |
| Observations                       | 621                                       |

Standard errors robust to heteroskedasticity in parentheses.  
The regressions control for gender, age decile, tenure decile, salary decile, faculty status, and birth month. We allowed the coefficients on the control variables to vary by university.  
 $*p \leq .10$ ,  $**p \leq .05$ ,  $***p \leq .01$ .

**Effects by university (Tables W6 and W7).** We added interactions between each university and the Simultaneous Pre-commitment indicator to our primary regression specification. In Table W6, we report the coefficients on the interaction terms, and in Table W7 we report the simple effects of simultaneous pre-commitment in each university (estimated with linear combinations of the coefficients from the regressions in Table W6). Note that some employees at University D were already enrolled in the targeted plan, and they were asked to increase their contribution rates in the experimental mailings. We analyzed their responses (*University D (Already Enrolled)*) separately from the rest of University D’s employees in this experiment who were not yet enrolled (*University D (Not Yet Enrolled)*).

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**Table W6. Interactions by University**

|                                                                                                                                                                                                                                                                                                               | Model 1:<br>Immediate<br>Adoption | Model 2:<br>Overall<br>Adoption | Model 3:<br>Average<br>Savings Rate |
|---------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|-----------------------------------|---------------------------------|-------------------------------------|
| <i>Simultaneous Pre-commitment</i>                                                                                                                                                                                                                                                                            | -.009<br>(.014)                   | -.002<br>(.019)                 | .0007<br>(.001)                     |
| <i>Simultaneous Pre-commitment *<br/>University B</i>                                                                                                                                                                                                                                                         | -.009<br>(.024)                   | -.009<br>(.029)                 | -.0003<br>(.002)                    |
| <i>Simultaneous Pre-commitment *<br/>University C</i>                                                                                                                                                                                                                                                         | -.005<br>(.017)                   | .008<br>(.023)                  | -.0031*<br>(.002)                   |
| <i>Simultaneous Pre-commitment *<br/>University D (Not Yet Enrolled)</i>                                                                                                                                                                                                                                      | -.012<br>(.022)                   | -.025<br>(.029)                 | -.0029**<br>(.001)                  |
| <i>Simultaneous Pre-commitment *<br/>University D (Already Enrolled)</i>                                                                                                                                                                                                                                      | -.041<br>(.034)                   | -.044<br>(.042)                 | -.0018<br>(.002)                    |
| R-squared                                                                                                                                                                                                                                                                                                     | .07                               | .09                             | .11                                 |
| Observations                                                                                                                                                                                                                                                                                                  | 5,196                             | 5,196                           | 5,196                               |
| Standard errors robust to heteroskedasticity in parentheses. The regressions control for gender, age decile, tenure decile, salary decile, faculty status, and birth month. We allowed the coefficients on the control variables to vary by university. * $p \leq .10$ , ** $p \leq .05$ , *** $p \leq .01$ . |                                   |                                 |                                     |

Table W7. Simple Effects of Simultaneous Pre-commitment in Each University

|                                                                                                                    | Model 1:<br>Immediate<br>Adoption | Model 2:<br>Overall<br>Adoption | Model 3:<br>Average<br>Savings Rate |
|--------------------------------------------------------------------------------------------------------------------|-----------------------------------|---------------------------------|-------------------------------------|
| <i>Simultaneous Pre-commitment<br/>in University A</i>                                                             | -.009<br>(.014)                   | -.002<br>(.019)                 | .0007<br>(.001)                     |
| <i>Simultaneous Pre-commitment<br/>in University B</i>                                                             | -.019<br>(.019)                   | -.011<br>(.022)                 | .0005<br>(.002)                     |
| <i>Simultaneous Pre-commitment<br/>in University C</i>                                                             | -.014<br>(.010)                   | .006<br>(.013)                  | -.0024*<br>(.001)                   |
| <i>Simultaneous Pre-commitment<br/>in University D (Not Yet Enrolled)</i>                                          | -.021<br>(.017)                   | -.026<br>(.021)                 | -.0022**<br>(.001)                  |
| <i>Simultaneous Pre-commitment<br/>in University D (Already Enrolled)</i>                                          | -.050<br>(.030)                   | -.045<br>(.038)                 | -.0010<br>(.001)                    |
| Standard errors robust to heteroskedasticity in parentheses. * $p \leq .10$ , ** $p \leq .05$ , *** $p \leq .01$ . |                                   |                                 |                                     |

Panel B. Robustness Checks

**Measuring decisions with cards mailed back to the one university that tracked responses (Table W8).** Only one of the four universities provided us with data indicating which employees returned response postcards and what enrollment decisions they made on those cards. Using the mailing response data from this university, we created two outcome variables. “Immediate adoption” equaled one if an employee elected to *immediately* increase their contribution rate to the targeted plan by marking the immediate option on the response card, and zero otherwise. “Overall adoption” equaled one if an employee elected to increase their contribution rate to the targeted plan by marking either the immediate option or the pre-commitment option on the response card, and zero otherwise. Here, we obtained qualitatively similar results as the results of our primary analysis reported in the paper based on the administrative data. Specifically, using the mailing response data, we estimated that employees’ probability of immediately increasing their contributions to the targeted plan was 1.6 percentage points lower in the Simultaneous Pre-commitment condition than in the No Pre-commitment

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condition ( $p = .075$ ). This represents a 32.7% decrease in immediate adoption relative to the 4.9 percent of employees who immediately enrolled in the targeted plan in the No Pre-commitment condition. And, there was virtually no difference across conditions in employees' probability of increasing their contributions to the targeted savings plan ( $p = .390$ ). The weaker statistical significance for the regression predicting immediate adoption is likely because this analysis relies on a smaller sample of 2,029 employees and thus has less power than our primary analysis using the full sample across four universities.

**Table W8. Measuring Decisions with Cards Mailed back to the One University that Tracked Responses**

|                                    | Model 1:<br>Immediate Adoption | Model 2:<br>Overall Adoption |
|------------------------------------|--------------------------------|------------------------------|
| <i>Simultaneous Pre-commitment</i> | -.016*                         | -.008                        |
|                                    | (.009)                         | (.009)                       |
| R-squared                          | .027                           | .024                         |
| Observations                       | 2,029                          | 2,029                        |

Standard errors robust to heteroskedasticity in parentheses. The regressions control for gender, age decile, tenure decile, salary decile, faculty status, and birth month. We allowed the coefficients on the control variables to vary by university.

\* $p \leq .10$ , \*\* $p \leq .05$ , \*\*\* $p \leq .01$ .

**Measuring overall adoption differently (Table W9).** We calculated overall adoption using an alternative method that was meant to capture employees' direct responses to the mailers. For this measure, we only counted someone as enrolling if the first time their contribution rate increased (relative to their rate in September 2013) corresponded with one of the month(s) offered to them in the mailing. Specifically, for employees in the No Pre-commitment condition, this alternative measure of overall adoption equaled one if their first contribution rate increase was in November 2013 (corresponding to the immediate option), and zero otherwise. For employees in the Simultaneous Pre-commitment condition, overall adoption equaled one if their first increase was in November 2013 or in the pre-commitment month offered in their mailing (e.g., February 2014 for employees whose pre-commitment option was "in four months"), and zero otherwise. Of note, this method only modifies how we measured overall adoption; immediate adoption was always measured based on whether an employee had a contribution rate increase in November 2013 (compared to September 2013). When we estimated

our primary regression specification with this alternate measure of overall adoption as the dependent variable, we obtained substantively similar results.

**Table W9. Measuring Overall Adoption Differently**

|                                    | Model 1:<br>Overall Adoption |
|------------------------------------|------------------------------|
| <i>Simultaneous Pre-commitment</i> | -.003<br>(.007)              |
| R-squared                          | .075                         |
| Observations                       | 5,196                        |

Standard errors robust to heteroskedasticity in parentheses. The regressions control for gender, age decile, tenure decile, salary decile, faculty status, and birth month. We allowed the coefficients on the control variables to vary by university.  
*\*p* ≤ .10, *\*\*p* ≤ .05, *\*\*\*p* ≤ .01.

**Running logistic and fractional logit regressions (Table W10).** We used logistic regressions rather than OLS to predict our binary dependent measures (immediate and overall adoption). We also used fractional logistic regressions rather than OLS to predict average savings rate during the study period—a continuous dependent measure ranging from 0% to 100%. Our results are not meaningfully changed.

**Table W10. Running Logistic and Fractional Logit Regressions**

|                                    | Model 1:<br>Immediate<br>Adoption | Model 2:<br>Overall<br>Adoption | Model 3:<br>Average<br>Savings Rate |
|------------------------------------|-----------------------------------|---------------------------------|-------------------------------------|
| <i>Simultaneous Pre-commitment</i> | -.359***<br>(.118)                | -.099<br>(.092)                 | -.163**<br>(.081)                   |
| Observations                       | 5,050 <sup>a</sup>                | 5,196                           | 5,196                               |

Standard errors robust to heteroskedasticity in parentheses. The regressions control for gender, age decile, tenure decile, salary decile, faculty status, and birth month. We allowed the coefficients on the control variables to vary by university.  
*\*p* ≤ .10, *\*\*p* ≤ .05, *\*\*\*p* ≤ .01.  
<sup>a</sup>Some observations are automatically dropped out of the logistic regression that predicts immediate adoption (Model 1) because employees in one university x age decile group and one university x tenure decile group all had the value of zero for immediate adoption, leading controls for these groups to perfectly predict immediate adoption (and thus to be inestimable).

**Dropping those with missing data (Table W11).** For the results reported in the paper, we assigned a value of zero to an employee’s contribution rate in a pay cycle if there was missing data for salary or contributions for that pay cycle. We do so because missing values for salary or

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contributions are likely to reflect short-term leaves of absence. Our results are robust if we instead drop employees who have missing data for salary or contributions in all months from November 2013 through June 2014.

**Table W11. Dropping Those with Missing Data**

|                                    | Model 1:<br>Immediate<br>Adoption | Model 2:<br>Overall<br>Adoption | Model 3:<br>Average<br>Savings Rates |
|------------------------------------|-----------------------------------|---------------------------------|--------------------------------------|
| <i>Simultaneous Pre-commitment</i> | -.020***<br>(.007)                | -.010<br>(.009)                 | -.0015**<br>(.001)                   |
| R-squared                          | .07                               | .08                             | .12                                  |
| Observations                       | 4,938                             | 4,938                           | 4,938                                |

Standard errors robust to heteroskedasticity in parentheses. The regressions control for gender, age decile, tenure decile, salary decile, faculty status, and birth month. We allowed the coefficients on the control variables to vary by university.

\* $p \leq .10$ , \*\* $p \leq .05$ , \*\*\*  $p \leq .01$ .

**Dropping those with missing data in key months (Table W12).** Our results are robust if we instead drop employees who have missing data for salary or contributions in one of the key months used to calculate the immediate and overall adoption variables. Specifically, in the regressions reported below, we drop observations with missing salary or contributions data in September 2013 or November 2013 in Model 1, and we drop observations with missing salary or contributions data in September 2013 or June 2014 in Model 2.

**Table W12. Dropping Those with Missing Data in Key Months**

|                                    | Model 1:<br>Immediate<br>Adoption | Model 2:<br>Overall<br>Adoption |
|------------------------------------|-----------------------------------|---------------------------------|
| <i>Simultaneous Pre-commitment</i> | -.021***<br>(.008)                | -.009<br>(.010)                 |
| R-squared                          | .07                               | .09                             |
| Observations                       | 4,618                             | 4,543                           |

Standard errors robust to heteroskedasticity in parentheses. The regressions control for gender, age decile, tenure decile, salary decile, faculty status, and birth month. We allowed the coefficients on the control variables to vary by university. \* $p \leq .10$ , \*\* $p \leq .05$ , \*\*\*  $p \leq .01$ .

**Varying the cutoff for a contribution rate increase (Table W13).** To calculate an employee's contribution rate in a given month, we divided her dollar contributions by her salary

in that month. Since we constructed the overall adoption variable by comparing these imputed contribution rates in June 2014 versus September 2013, we want to ensure that our results are not spuriously driven by how we rounded our imputed contribution rates. For example, if an employee had an imputed contribution rate of 5.030% in September 2013 and 5.033% in June 2014, it is unlikely that this employee increased her contribution rate by .003%; rather, this difference in imputed contribution rates likely reflects a rounding issue. The same issue applies to our construction of the immediate adoption variable. For our main analyses in the manuscript, we only counted a contribution rate increase to be real if the increase was  $\geq 1\%$ . As a robustness check, we counted a contribution rate increase to be real if the increase was  $\geq .10\%$  or  $\geq .01\%$ , and we obtained similar results.

**Table W13. Varying the Cutoff for a Contribution Rate Increase**

| Threshold for defining a real increase | Increase in contribution rate $\geq .1\%$ |                           | Increase in contribution rate $\geq .01\%$ |                           |
|----------------------------------------|-------------------------------------------|---------------------------|--------------------------------------------|---------------------------|
|                                        | Model 1: Immediate Adoption               | Model 2: Overall Adoption | Model 3: Immediate Adoption                | Model 4: Overall Adoption |
| <i>Simultaneous Pre-commitment</i>     | -.020***<br>(.007)                        | -.009<br>(.009)           | -.021***<br>(.007)                         | -.008<br>(.009)           |
| R-squared                              | .08                                       | .09                       | .08                                        | .09                       |
| Observations                           | 5,196                                     | 5,196                     | 5,196                                      | 5,196                     |

Standard errors robust to heteroskedasticity in parentheses. The regressions control for gender, age decile, tenure decile, salary decile, faculty status, and birth month. We allowed the coefficients on the control variables to vary by university.  
\* $p \leq .10$ , \*\* $p \leq .05$ , \*\*\*  $p \leq .01$ .



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**Including limited controls (Table W14).** When controlling for the interaction between university and birth month (because of our stratified random assignment) but no other individual characteristics (i.e., gender, age, tenure, salary, and faculty status), our results remain qualitatively unchanged in magnitude, though the statistical significance is weaker for regressions predicting average savings rate.

**Table W14. Including Limited Controls**

|                                    | Model 1:<br>Immediate<br>Adoption | Model 2:<br>Overall<br>Adoption | Model 3:<br>Average<br>Savings Rate |
|------------------------------------|-----------------------------------|---------------------------------|-------------------------------------|
| <i>Simultaneous Pre-commitment</i> | -.017**<br>(.007)                 | -.007<br>(.009)                 | -.0012*<br>(.001)                   |
| R-squared                          | .03                               | .04                             | .07                                 |
| Observations                       | 5,196                             | 5,196                           | 5,196                               |

Standard errors robust to heteroskedasticity in parentheses. The regressions control for birth month and we allowed the coefficients on the birth month controls to vary by university. \* $p \leq .10$ , \*\* $p \leq .05$ , \*\*\* $p \leq .01$ .

**WEB APPENDIX F. SUMMARY OF DIFFERENCES BETWEEN STUDY 1 AND  
BESHEARS ET AL. (2021)**

Table W15 summarizes how the research questions, conditions, and findings differ between Study 1 and a companion paper, Beshears et al. (2021).

**Table W15. Comparison of Study 1 with Beshears et al. (2021)**

|                                 | Study 1 in the Current Paper                                                                                                      | Beshears et al. (2021)                                                                                                        |
|---------------------------------|-----------------------------------------------------------------------------------------------------------------------------------|-------------------------------------------------------------------------------------------------------------------------------|
| Research Question               | What is the effect of simultaneous pre-commitment on retirement savings decisions?                                                | What is the effect of fresh-start framing on retirement savings decisions?                                                    |
| Conditions Included in Analysis | Simultaneous Pre-commitment VS. No Pre-commitment                                                                                 | Simultaneous Pre-commitment Condition Linked with Temporal Landmarks VS. Simultaneous Pre-commitment <sup>a</sup>             |
| Findings                        | Simultaneous pre-commitment reduced immediate adoption, had a null effect on overall adoption, and reduced average savings rates. | Fresh-start framing had a null effect on immediate adoption, increased overall adoption, and increased average savings rates. |
| Theorized Mechanism             | Simultaneous pre-commitment decreases inferred urgency                                                                            | Fresh start framing heightens motivation to initiate new goals                                                                |

<sup>a</sup>Beshears et al. (2021) draw a distinction between temporal landmarks that feel like the beginning of a new time period and are associated with fresh starts (Birthdays, New Year’s, the first day of spring) and temporal landmarks that are not associated with fresh starts (Thanksgiving, Martin Luther King Day, Valentine’s Day); the latter are used for placebo regressions in their paper.

**WEB APPENDIX G. POST-TEST FOLLOWING STUDY 1****Methods**

We recruited participants through Amazon's Mechanical Turk (MTurk) to take a short survey for a pre-registered study. The 1,499 participants ( $M_{\text{age}} = 36.41$ ,  $SD_{\text{age}} = 11.59$ ; 47.3% female) who successfully answered comprehension check questions were included in our study, as stipulated in our pre-registration. All participants were asked to imagine that the HR department at "Company X" planned to send its employees mailings about the company's retirement savings program. Participants were randomly assigned to read about and evaluate one of three mailings: a No Pre-commitment mailing, a Simultaneous Pre-commitment mailing, or a Sequential Pre-commitment mailing.

The No Pre-commitment mailing depicted a simplified version of the No Pre-commitment mailing from our field experiment, adapted such that all references to universities and specific retirement savings plans were replaced with references to Company X and its hypothetical savings program. The No Pre-commitment mailing encouraged employees to sign up for Company X's retirement savings plan immediately.

The Simultaneous Pre-commitment mailing depicted a simplified version of the Simultaneous Pre-commitment mailing from our field experiment, again adapted to reference only Company X and its hypothetical savings program. The Simultaneous Pre-commitment mailing offered employees two options: the option to start contributing to Company X's savings program immediately and the option to pre-commit to start contributing in four months.

The Sequential Pre-commitment mailing was comprised of two mailings sent in stages. In the first stage, the mailing depicted was identical to the No Pre-commitment mailing, which invited employees to enroll in Company X's savings program immediately. Participants learned that in the second stage, if an employee did not reply to the initial No Pre-commitment mailing, the HR department would send a follow-up mailing with an invitation to pre-commit to enroll in the savings program in four months. This second-stage mailing explained to employees that they received the second offer because they did not respond to the first-stage mailing, and it only presented the option for employees to pre-commit to enroll in four months.

After participants read the mailing(s) associated with their experimental condition, we assessed their understanding with a series of comprehension check questions, and we ended the survey for participants who failed these questions ( $N = 83$ ). 95% of participants passed our

comprehension check. The rate did not differ significantly between the No Pre-commitment condition (97%) and the Simultaneous Pre-commitment condition (96%;  $p = .60$ ), but it was slightly lower in the Sequential Pre-commitment condition than the other conditions (93%; both  $p$ 's  $< .05$ ), likely because this comprehension check involved more questions than the others.

Next, we measured *inferred urgency* by asking: "To what extent will this mailing convey to employees that the human resources staff urgently recommends that employees enroll in the retirement savings program?" (1 = Not at all urgently, 7 = Very urgently). We also included a measure about inferred future opportunities to save based on an earlier version of our theory. We asked: "To what extent will this mailing suggest to employees that they will have other, future opportunities to enroll in the retirement savings program?" (1 = No future opportunities suggested, 7 = Future opportunities strongly suggested).

Finally, we asked participants for their age and gender.

**Results**

Supporting Hypothesis 1, participants reported that the No Pre-commitment mailing conveyed a more urgent recommendation to save ( $M = 5.10$ ,  $SD = 1.42$ ) than the Simultaneous Pre-commitment mailing ( $M = 4.77$ ,  $SD = 1.58$ ,  $t(1,011) = 3.54$ ,  $p < .001$ ). This difference between the two mailings may help to explain why our Simultaneous Pre-commitment mailing failed to increase savings and also reduced immediate adoption rates compared to the No Pre-commitment mailing in our field experiment.

Also consistent with Hypothesis 1, participants reported that the Sequential Pre-commitment mailing conveyed a marginally more urgent recommendation to save ( $M = 5.27$ ,  $SD = 1.30$ ) than the No Pre-commitment mailing ( $M = 5.10$ ,  $SD = 1.42$ ;  $t(993) = 1.89$ ;  $p = .06$ ). Also, as expected, participants reported that the Sequential Pre-commitment mailing conveyed a more urgent recommendation than the Simultaneous Pre-commitment mailing ( $M = 4.77$ ,  $SD = 1.58$ ;  $t(988) = 5.40$ ,  $p < .001$ ).

Based on a previous version of our theory, we hypothesized in our pre-registration and confirmed that, compared to the No Pre-commitment condition ( $M = 3.09$ ,  $SD = 1.73$ ), people inferred that there would be more future opportunities to save in both the Simultaneous Pre-commitment ( $M = 3.96$ ,  $SD = 2.00$ ),  $t(1,011) = 7.48$ ,  $p < .001$ ) and Sequential Pre-commitment conditions ( $M = 3.92$ ,  $SD = 1.92$ ,  $t(993) = 7.19$ ,  $p < .001$ ).

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## WEB APPENDIX H. EXPLORATORY FACTOR ANALYSIS (STUDY 2)

We conducted an exploratory factor analysis on six items: two urgency items and four decision difficulty items. Below are the six items (with shorthand labels in bold). All items use a 7-point Likert scale ranging from 1 = Not at all to 7 = Very much.

**Inferred urgency item 1:** To what extent do you think Company X recommends that employees enroll in the benefits programs as soon as they can?

**Inferred urgency item 2:** To what extent do you think Company X urgently recommends that employees enroll in the benefits programs?

**Decision difficulty item 1:** To what extent were you overwhelmed when you were making your enrollment decision?

**Decision difficulty item 2:** To what extent were you frustrated when you were making your enrollment decision?

**Decision difficulty item 3:** To what extent were you annoyed when you were making your enrollment decision?

**Decision difficulty item 4:** To what extent did you find the choice difficult when you were making your enrollment decision?

To conduct the exploratory factor analysis, we used Ordinary Least Squares (OLS) with an oblimin rotation to find the minimum residual solution. See the factor loadings presented in Table W16 below. Note, factor loadings below .5 are not displayed for ease of visualization.

**Table W16. Factor Loadings**

|                                   | Factor1 | Factor2 |
|-----------------------------------|---------|---------|
| <b>Inferred urgency item 1</b>    |         | .77     |
| <b>Inferred urgency item 2</b>    |         | .91     |
| <b>Decision difficulty item 1</b> | .63     |         |
| <b>Decision difficulty item 2</b> | .91     |         |
| <b>Decision difficulty item 3</b> | .83     |         |
| <b>Decision difficulty item 4</b> | .72     |         |

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As shown in Table W16, the items used to measure “inferred urgency” and the items to measure “decision difficulty” loaded on separate factors. Also, the “inferred urgency” composite (average of the two items) and the “decision difficulty” composite (average of the four items) have a relatively low correlation of Pearson  $r = .15$ . Together these observation suggests that the two constructs are distinct.

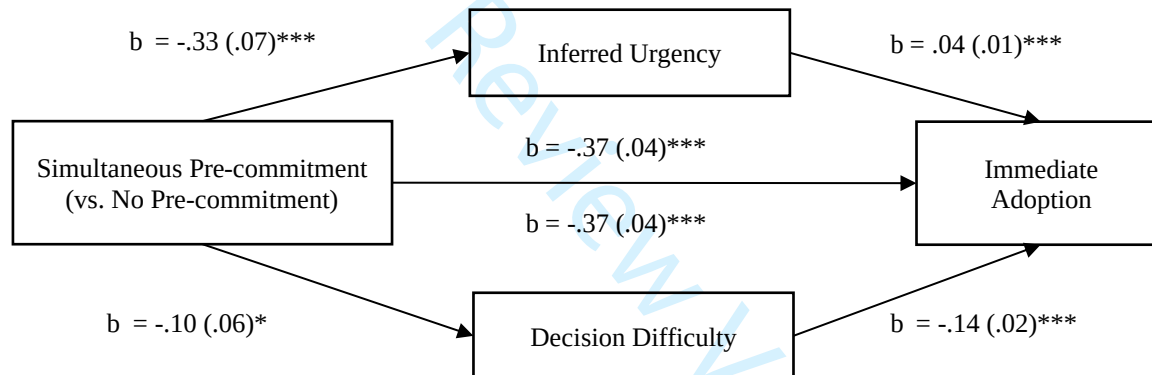
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## WEB APPENDIX I. MULTI-MEDIATOR MODELS (STUDY 2)

Figures W2-W5 depict how the hypothesized mediators explain the effects of offering simultaneous pre-commitment and sequential pre-commitment on immediate and overall adoption of the employee benefits programs. All regression coefficients are unstandardized, and standard errors are presented in parentheses. The coefficients above the paths from Simultaneous Pre-commitment and Sequential Pre-commitment to “immediate adoption” and “overall adoption” represent the total effects and the coefficients below the paths represents the direct effects. Coefficients significantly different from zero are indicated by asterisks (\* $p \leq .10$ , \*\* $p \leq .05$ , \*\*\* $p \leq .01$ ).

**Figure W2. Simultaneous vs. No Pre-commitment: Immediate Adoption Mediation**



| Indirect effects           | <i>b</i> | 95% CI        |
|----------------------------|----------|---------------|
| <i>Inferred Urgency</i>    | -.01     | [-.02, -.005] |
| <i>Decision Difficulty</i> | .01      | [-.003, .03]  |

Figure W3. Simultaneous vs. No Pre-commitment: Overall Adoption Mediation

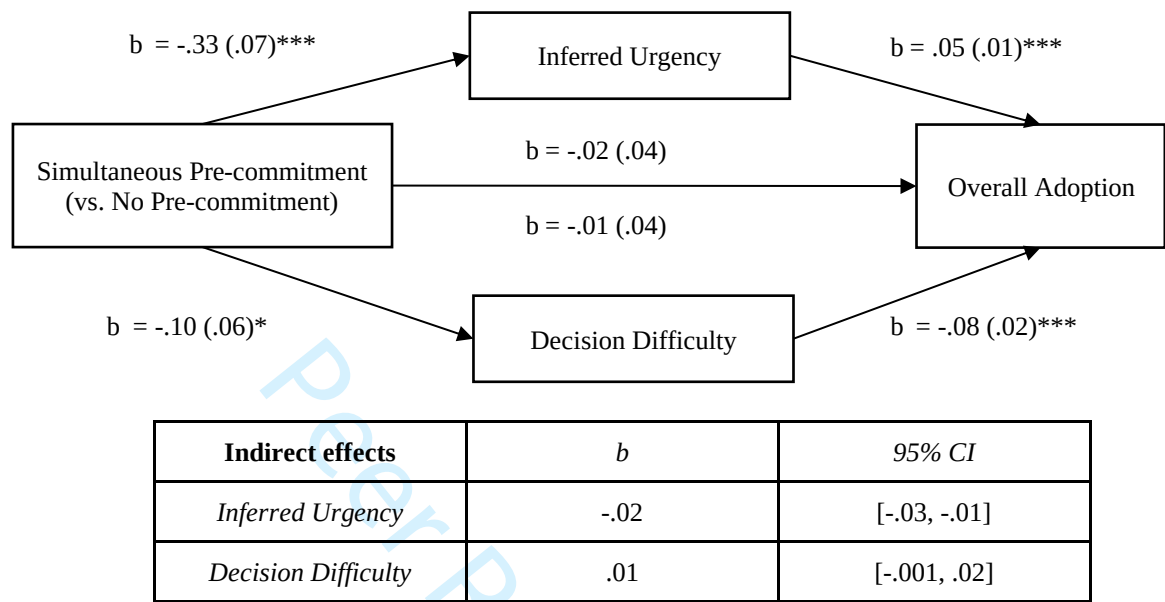
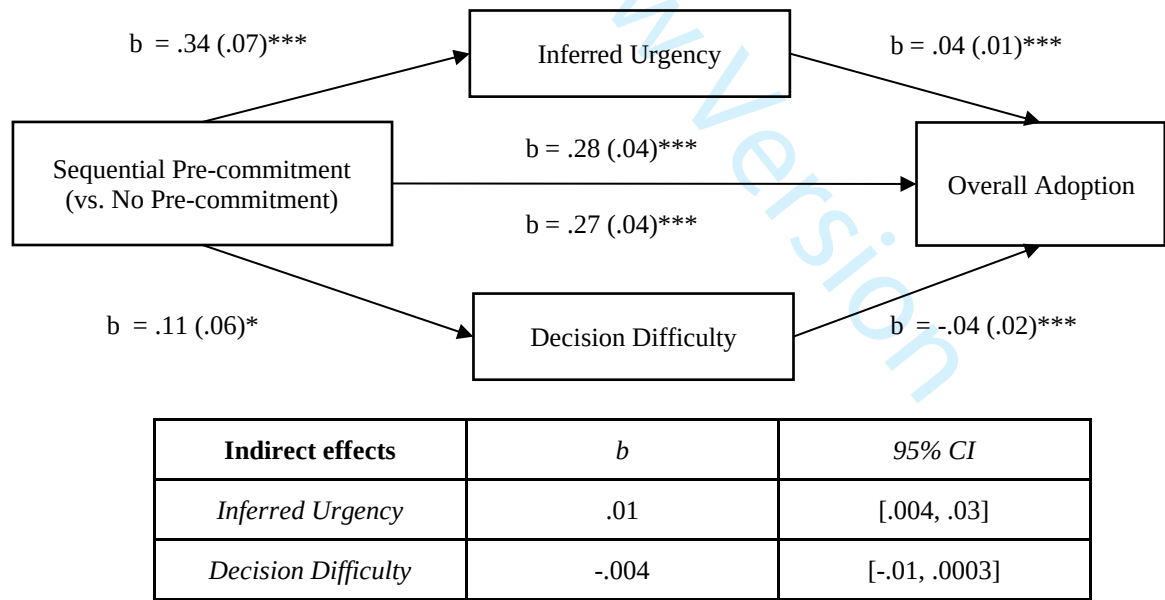


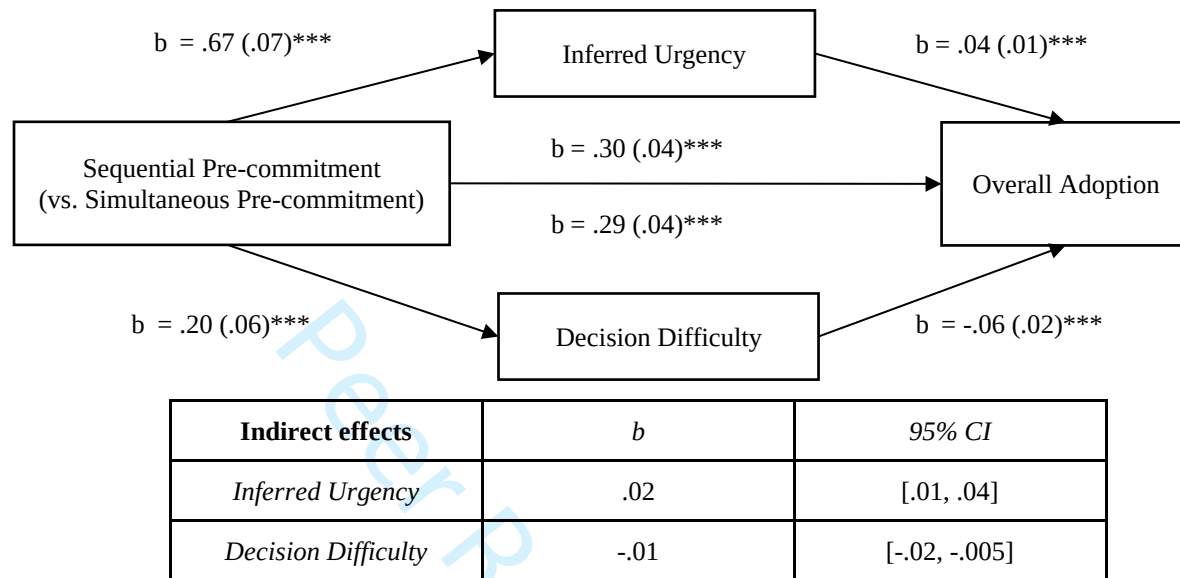
Figure W4. Sequential Pre-commitment vs. No Pre-commitment: Overall Adoption Mediation





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**Figure W5. Sequential Pre-commitment vs. Simultaneous Pre-commitment: Overall Adoption Mediation**



**WEB APPENDIX J. TWO-CONDITION REPLICATION OF STUDY 2**

We conducted a laboratory experiment in which participants decided (hypothetically) whether and when to enroll in three different benefits programs offered by a new employer. This is an exact replication of Study 2 reported in the manuscript except it only included the No Pre-commitment and Simultaneous Pre-commitment conditions, and we used a different set of questions to measure mechanisms.

**Methods**

As stipulated in our pre-registration, we recruited MTurk participants who passed a brief attention check and were fully employed at a firm other than MTurk. A total of 1,161 participants satisfied these selection criteria and completed our pre-registered study ( $M_{age} = 38.24$ ;  $SD_{age} = 10.61$ ; 52.3% female).

The vignette about enrollment in Company X's HR benefits programs is identical to Study 2, however, this study only included the No Pre-commitment and Simultaneous Pre-commitment conditions.

After participants made their enrollment decisions, participants in both conditions answered two questions about Company X: "To what extent do you think Company X urgently recommends that employees enroll in the benefits programs" and "To what extent do you think Company X recommends that employees enroll in the benefits programs as soon as possible?" (1 = Not at all, 7 = Very much). These questions were collapsed into a single measure of *inferred urgency* ( $r = .77$ ,  $p < .001$ ). We next asked two questions measuring participants' *anticipated negative emotion* associated with enrolling in the benefits programs: "If you were a full-time employee at Company X, how painful would it be for you to enroll in any of Company X's benefits programs?" (1 = Not at all painful, 7 = Very painful) and "If you were a full-time employee at Company X, how unpleasant would it be for you to enroll in any of Company X's benefits programs?" (1 = Not at all unpleasant, 7 = Very unpleasant). These two items were collapsed into a single measure ( $r = .74$ ,  $p < .001$ ).

We also asked three questions designed to assess potential alternative mechanisms for our predicted results. Specifically, we measured *decision difficulty* with one item: "How difficult was it for you to make this enrollment decision?" (1 = Not at all difficult, 7 = Very difficult). We measured *confusion* with one item: "How confusing was Company X's enrollment process?" (1 = Not at all confusing, 7 = Very confusing). And we measured the *inferred thoughtfulness* of

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Company X's enrollment process with one item: "How much thought do you think Company X put into the design of the enrollment process?" (1 = Very little thought, 7 = A lot of thought). At the end of our study, participants were asked about their age, gender, education, and income.

## Results

**Inferred Urgency.** Replicating the results of Study 2, compared to those in the No Pre-commitment condition ( $M = 4.15$ ,  $SD = 1.70$ ), participants in the Simultaneous Pre-commitment condition rated Company X's implied recommendation to enroll as less urgent ( $M = 3.56$ ,  $SD = 1.73$ ;  $b = -.59$ ,  $p < .001$ ).

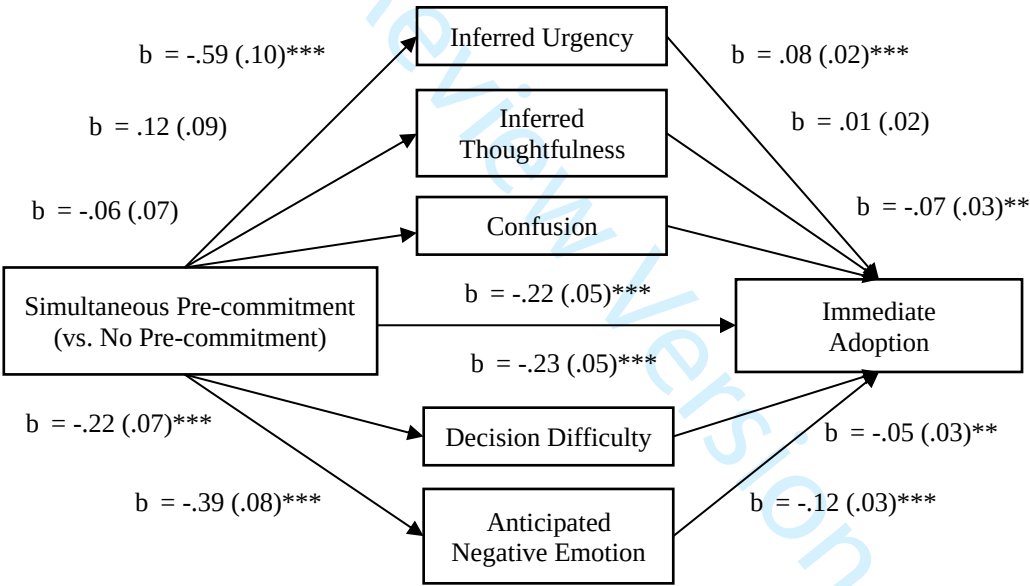
**Immediate Adoption.** Replicating the results from our field experiment and Study 2, participants in the Simultaneous Pre-commitment condition signed up for fewer benefits immediately ( $M = 1.77$ ,  $SD = .96$ ) than participants in the No Pre-commitment condition ( $M = 1.99$ ,  $SD = .86$ ;  $b = -.22$ ,  $p < .001$ ). We estimated a multi-mediator model including all the other potential mediators listed in the methods section, and inferred urgency mediated the effect of simultaneous pre-commitment (vs. no pre-commitment) on immediate adoption ( $b = -.05$ ; 95%  $CI = [-.07, -.03]$ ; See Figure W6).

**Overall Adoption.** Consistent with the results of our field experiment and Study 2, compared to the No Pre-commitment condition ( $M = 1.99$ ,  $SD = .86$ ), offering simultaneous pre-commitment ( $M = 2.03$ ,  $SD = .89$ ) did not significantly change the total number of benefits programs people enrolled in ( $b = .04$ ,  $p = .401$ ). And in a multi-mediator model involving other potential mediators, inferred urgency mediated the effect of simultaneous pre-commitment (vs. no pre-commitment) on overall adoption ( $b = -.06$ , 95%  $CI = [-.08, -.03]$ ; See Figure W7). Also, consistent with previous research that has theorized about the positive impact of pre-commitment, we found evidence that anticipated negative emotions (i.e., painfulness, unpleasantness) *positively* mediated the effect of simultaneous pre-commitment on overall adoption ( $b = .04$ , 95%  $CI = [.02, .07]$ ). That is, when people had the option to enroll in the benefits at a delay (vs. when they could only enroll immediately), they perceived that enrolling would feel less aversive, which predicted greater overall enrollment. Thus, consistent with our theory, the null effect of simultaneous pre-commitment on overall adoption observed in this study may be explained by competing mechanisms: the negative indirect effect via inferred urgency may have negated the positive indirect effect via anticipated negative emotions.

WEB APPENDIX K. MULTI-MEDIATOR MODELS (TWO-CONDITION REPLICATION OF STUDY 2)

Figures W6 and W7 depict how the hypothesized mediators explain the effects of offering simultaneous pre-commitment on immediate and overall adoption of the employee benefits programs. All regression coefficients are unstandardized, and standard errors are presented in parentheses. The coefficients above the paths from Simultaneous Pre-commitment to “immediate adoption” and “overall adoption” represent the total effects and the coefficients below the paths represents the direct effects. Coefficients significantly different from zero are indicated by asterisks ( $*p \leq .10$ ,  $**p \leq .05$ ,  $***p \leq .01$ ).

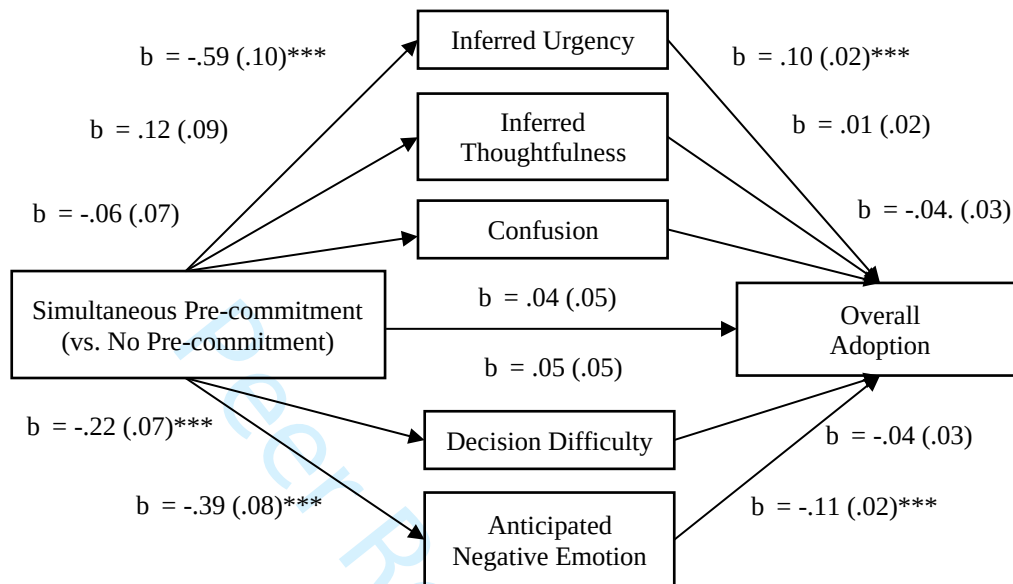
Figure W6. Simultaneous vs. No Pre-commitment: Immediate Adoption Mediation



| Indirect effects             | b     | 95% CI        |
|------------------------------|-------|---------------|
| Inferred urgency             | -.05  | [-.07, -.03]  |
| Anticipated negative emotion | .05   | [.02, .07]    |
| Inferred thoughtfulness      | .0001 | [-.004, .007] |
| Confusion                    | .003  | [-.005, .02]  |
| Decision difficulty          | .01   | [.0001, .03]  |

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**Figure W7. Simultaneous vs. No Pre-commitment: Overall Adoption Mediation**

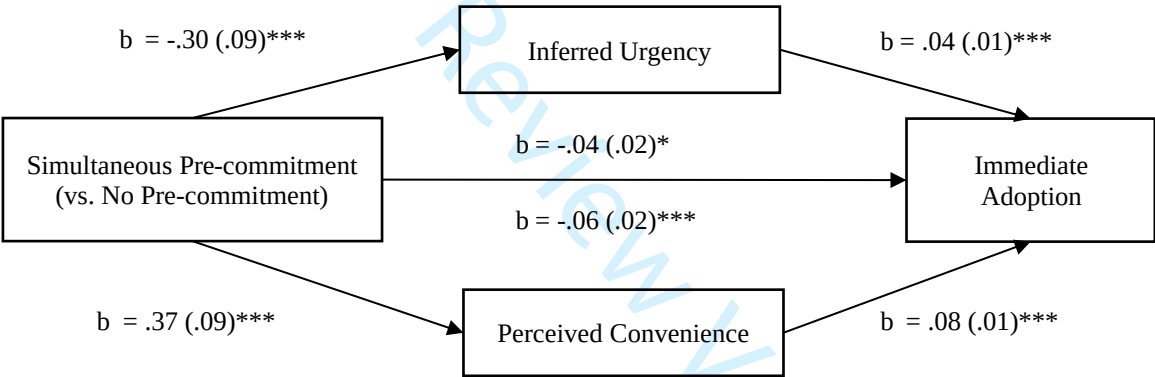


| Indirect effects                    | <i>b</i> | 95% CI        |
|-------------------------------------|----------|---------------|
| <i>Inferred urgency</i>             | -.06     | [-.08, -.03]  |
| <i>Anticipated negative emotion</i> | .04      | [.02, .07]    |
| <i>Inferred thoughtfulness</i>      | .0004    | [-.005, .007] |
| <i>Confusion</i>                    | .002     | [-.003, .01]  |
| <i>Decision difficulty</i>          | .01      | [-.002, .02]  |

WEB APPENDIX L. MULTI-MEDIATOR MODELS (STUDY 3)

Figures W8 and W9 depict how the hypothesized mediators explain the effects of offering simultaneous pre-commitment and sequential pre-commitment on immediate and overall adoption of the financial well-being assessment. All regression coefficients are unstandardized, and standard errors are presented in parentheses. The coefficients above the paths from Simultaneous Pre-commitment and Sequential Pre-commitment to “immediate adoption” and “overall adoption” represent the total effects and the coefficients below the paths represents the direct effects. Coefficients significantly different from zero are indicated by asterisks ( $*p \leq .10$ ,  $**p \leq .05$ ,  $***p \leq .01$ ).

Figure W8. Simultaneous vs. No Pre-commitment: Immediate Adoption Mediation



| Indirect effects             | <i>b</i> | 95% <i>CI</i> |
|------------------------------|----------|---------------|
| <i>Inferred Urgency</i>      | -.01     | [-.02, -.004] |
| <i>Perceived Convenience</i> | .03      | [.02, .05]    |

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**Figure W9. Sequential Pre-commitment vs. Simultaneous Pre-commitment: Overall Adoption Mediation**

