Serenity Now, Save Later?
Evidence on Retirement Savings Puzzles from a 401(k) Field Experiment*

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This Version: September 2019

PRELIMINARY – PLEASE DO NOT CIRCULATE

ABSTRACT

Economists have advanced several psychological frictions to explain why many US employees, eligible for economically attractive 401(k) plans, save insufficiently for retirement. We causally investigate four candidate frictions through a field experiment randomizing 1,137 under-saving employees at a large US firm to an information- or incentive-based treatments embedded within a broader survey assessing each friction's baseline incidence. We present four main findings: (1) We corroborate existing research on the prevalence of low *retirement literacy* but find that the experimental provision of clear, specific, and personalized, recommendations did not increase savings, even among employees with the most severe literacy deficits. (2) We find no evidence that *enrollment complexity* impedes savings—few employees perceived enrollment as overly complex administratively and simplifying enrollment did not increase savings. (3) In an analysis of *employee confusion*, we estimate that at least one-quarter of 401(k) non-participants falsely believed they were enrolled—these employees enrolled at high rates upon being prompted to observe their actual enrollment status. (4) Finally, we present new direct evidence implicating *present-focus* as a cause of low 401(k) engagement by documenting the willingness of employees to increase savings in response to a small reward (a $10 Amazon gift card) but not to clarification of the much larger, but delayed, benefit implied by the plan match. Calibrations suggest that the prevailing beta-delta framework of present-biased employees cannot rationalize the observed patterns. We propose an alternative, anxiety-based, decision-making model to potentially explain these patterns as well as the broader set of empirical savings puzzles.

***Acknowledgements:** We thank several individuals for their generous feedback including Linda Babcock, Karna Basu, David Card, Keith Chen, James Choi, Leah Clark, Carol Conell, Stefano DellaVigna, Jonathan Guryan, Hilary Hoynes, Damon Jones, Ryan Kessler, Botond Köszegi, David Laibson, Robin Lipp, George Loewenstein, Vikram Pathania, Chris Price, Emmanuel Saez, Frank Schilbach, Jesse Shapiro, Justin Sydnor, Lowell Taylor, as well as seminar audiences at Carnegie Mellon University, the Russell Sage Foundation, MDRC, Hunter College and the Boulder Summer Conference on Consumer Financial Decision-Making. Stephanie Rifai, Ben Schenck, and Cassandra Taylor provided invaluable project support. The project was funded in part by The PNC Center for Financial Services Innovation and was reviewed by Carnegie Mellon's Institutional Review Board.

1 INTRODUCTION

Despite its canonical position within economics, the traditional life-cycle model of savings struggles to explain several empirical features of how working Americans save.[1] For example, several analyses conclude that a large share of US households save too little to ensure financial security in retirement, without a return to the workforce or a substantial change to consumption (GAO 2017, Munnell et al. 2014, Mutchler et al. 2016). Further, employees eligible for 401(k) plans, the most common form of employer-sponsored savings in the US, often fail to take up generous matching incentives typically associated with such plans (Madrian 2013), but do respond to largely non-economic plan features such as the structure (Madrian & Shea 2001) and complexity (e.g., Beshears et al. 2013) of enrollment, the presence of auto-escalation (Thaler and Benartzi 2004), the framing of incentives (Duflo et al. 2006; Choi et al. 2017), and the "psychological" design of online enrollment interfaces (Bhargava, Conell-Price, Mason and Benartzi 2018). Because of these factors, a significant share of *actual 401(k) enrollees* appear headed towards retirement insecurity.

Economists have advanced at least four distinct departures, or frictions, from the standard economic framework to explain (at least some of) the puzzles associated with 401(k) eligible employees.[2] A first friction, which we refer to as *retirement literacy*, implies that employees save sub-optimally because of low financial literacy or numeracy (e.g., Lusardi & Mitchell 2007, 2011), information processing errors such as the exponential-growth bias (e.g., Stango and Zinman 2009, Goda et al. 2015), or otherwise inaccurate beliefs about retirement-relevant considerations such as the length or expense of one's retirement. Studies have documented deficits in retirement literacy and a negative correlation with savings outcomes across a range of settings (see Hastings 2013 for a review). A second friction of plan confusion refers to the possibility that employees may save too little, or neglect to claim matching incentives, due to confusion regarding plan details such as the plan's default contribution rate or the generosity of its match. Confusion regarding eligibility and benefits have been cited as a barrier to take-up in other program contexts (Daponte et al. 1999, Bartlett et al. 2004, Bhargava and Manoli 2015, Chetty et al. 2013). A third departure suggests that the perception of enrollment complexity could potentially lead employees to irrationally delay, or avoid, 401(k) plan engagement due to a range of behavioral mechanisms (e.g., Anderson 2003, Beshears et al. 2013, Bertrand et al. 2004).

A final, widely cited, friction pertains to the possibility that employees privilege immediate, relative to delayed, flows of utility. Borrowing from the taxonomy of Ericson and Laibson (2018),

[1] Lifecycle savings models predict that people will save and dissave to smooth consumption over expected changes in income (Modigliani 1954). There is ongoing debate as to whether the well-documented drop in consumption after retirement in the US constitutes a failure to smooth consumption (Aguiar and Hurst 2005, Hurst 2008).
[2] Frictions such as liquidity constraints may also contribute to these puzzles. We return to these in Section 3.

present-focus has been offered as an explanation for low plan participation, overconfidence regarding future participation, and the large increase in participation routinely observed after the introduction of automatic enrollment (e.g., Madrian and Shea 2001). Economists have commonly modeled delayed enrollment as arising from present-biased employees whose intertemporal decisions are governed by beta-delta time-preferences (e.g., Laibson 1997, O'Donoghue and Rabin 1999a). While studies have shown correlations in measures of present-bias with savings outcomes (Goda et al. 2015, Brown and Previtero 2016), perhaps the most direct evidence implicating present bias in savings comes from studies documenting the demand for commitment in developing countries (see Bryan, Karlan, and Nelson 2010 for review) and a recent field experiment (Blumenstock et al. 2018) showing that various measures of present-bias predicted employee responsiveness to the introduction of a savings default.[3]

Despite the regularity with which these frictions are cited as explanations for empirical anomalies, with the exception of enrollment complexity (Choi et al. 2009, Beshears et al. 2013), there is limited causal evidence linking these frictions to the retirement savings of US employees. We attempt to provide evidence on the causal role of these four candidate frictions through a unique field experiment, embedded within a detailed survey, administered to 1,137 under-saving employees at a large US firm. Three features of our research setting, and empirical strategy, make it particularly promising for understanding the savings decisions of employees. First, and most critically, we embed the field experiment within an extensive survey capturing a range of economic, psychographic, and decision-making measures, and supplement this with mechanistic tests in the lab. While the differential response to the experimental treatments provides insight into how reducing each friction affects savings behavior at the margin, the surveys permits us to document the baseline precedence of these friction (and any correlation with savings) and to test whether employees suffering most severely from a specific friction differentially benefit from the corresponding treatment. For example, an experimental treatment providing clear, specific, and personalized savings recommendations permits us to estimate the average causal effect of guidance on marginal savings, the linked-survey permits us to estimate the baseline prevalence of low retirement literacy, and together, they permit us to estimate the efficacy of the treatment among employees with the most severe deficits in retirement literacy.

Second, our study disproportionately targets employees at risk for financial insecurity in retirement—as inferred from age, salary and savings—at a firm providing them generous (and widely varying) incentives to save. That is, the firm supplemented a more standard dollar-for-dollar match on contributions with a minimum match that, for many of the employees in our sample, resulted in an

[3] Blumenstock et al. (2018) involves a salary-linked savings account where returns come entirely from match incentives and the match level is experimentally varied allowing them to price the default's effect relative to financial incentives. This study also links responsiveness to defaults with survey measures of present bias and a behavioral measure of present bias.

expected return to each marginal dollar of sustained contribution ranging from 100 to 500% (median of 125%). Due to the generosity, and variability, of the match, our setting offers a strong litmus test for assessing predictions about savings from the standard economic model. Finally, while it is typical for field experiments in complicated institutional settings to test isolated hypotheses, we sought to simultaneously test candidate explanations within the same sample. To the best of our knowledge, within the literature investigating 401(k) savings, ours is the first study to simultaneously test the causal importance of multiple frictions, the first to integrate experimental tests of marginal response with direct measures of baseline frictions, and the first to directly test present-focus using time-varying incentives.

We administered the field study by inviting a few thousand employees, situated below pre-specified savings and income thresholds, to participate in an online survey marketed as an employer-sponsored opportunity to provide confidential feedback about the workplace. We speculate that the marketing of the survey, along with a reminder email and lottery prizes for participation, contributed to our relatively high response rate. A series of initial survey modules, beyond capturing background detail, assessed the employee-specific incidence of each of friction by eliciting a range of retirement-relevant beliefs and assessing various decision-making biases. Respondents were then conditionally randomized to one of several experimental variants of a final module that promised to assess the employee's preparedness for retirement based on the provided inputs. After assessing the employee's preparedness, the final module offered savings guidance to each employee, using calculations adapted from commercial retirement calculators, provided step-by-step instructions to any employee seeking to adjust their contribution (or newly enroll in the plan), and, finally, asked respondents to introspect about their savings decision and their future intent to save. To test the candidate frictions, the final module experimentally varied in (1) the specificity of the guidance provided (i.e., a specific contribution target, or a generic prompt to increase one contributions), (2) the presence of clarification regarding the plan's generous match, and (3) the presence of a small, immediate, reward to encourage employees to engage their savings decision by visiting the portal (a $10 Amazon gift card). Outcome measures for the study were generated from administrative records from the pay-period following the end of the survey.

We report four primary findings, each corresponding to a tested friction. First, while we corroborate previous research indicating widespread deficits in retirement literacy (in the form of biased retirement-relevant beliefs and low financial literacy), we find that these deficits do not meaningfully contribute to low 401(k) plan engagement in this setting. Specifically, the experimental provision of clear, personalized, and specific guidance *does not* lead to an increase in average savings despite successfully improving the accuracy of employee beliefs. Moreover, the provision of guidance does not increase savings even among employees who most severely underestimate how much to save. One possible reconciliation of our results with those of the extensive literature on retirement literacy is that some of

biases in retirement-relevant beliefs actually have offsetting implications for savings—that is, employees appear to both overestimate, relative to actuarial baselines, how long they will be able to work (implying the need to save less), as well as the length of their retirement (implying the need to save more). While employees systematically underestimate how much they ought to save, most employees also recognize the insufficiency of their current level of savings.

Second, we present converging evidence rejecting the role of administrative complexity of enrollment (or contribution adjustment) in diminishing plan engagement in this setting. As initial evidence, our survey indicates that 77 percent employees in our sample perceive the time required to adjust one's contribution as modest (i.e., requiring a matter of minutes or less). Consistent with the absence of the widespread perception of enrollment/adjustment as being prohibitively complicated, we find that respondents in the baseline condition, which effectively simplified administrative enrollment through clear instructions and a direct link to the enrollment portal, were not more likely to increase contributions relative to out-of-sample comparison groups. Employees perceiving enrollment/adjustment as time-intensive (i.e., requiring more than minutes) were no more likely to increase their contributions in response to simplification than their counterparts.

Third, we offer perhaps the first evidence that employee confusion about their 401(k) plan may explain a non-trivial share of under-saving. We distinguish between two specific types of employee confusion—confusion about plan features such as the match and default rate and confusion about one's enrollment status. Regarding the former, we find that while 30% of employees had inaccurate beliefs about the match, with two-thirds of these employees underestimating its generosity, clarifying match generosity *did not* increase average savings (we cannot rule out a modest increase to savings among those whose underestimation is most severe). However, in an unplanned analysis, we document that over one-third of 401(k) non-participants in our sample erroneously reported themselves as enrollees. Subsequent analysis suggests that the majority of these misreports likely reflect genuine employee confusion as opposed to inattention or willful exaggeration. Misreporting employees, who had a heightened chance of observing their actual enrollment status by virtue of being randomized into the reward condition of the experiment, were more than three times as likely to increase their contribution than their counterparts. While the scope of confusion we document might seem implausible, one can speculate that such confusion could arise from the dizzying complexity of the benefit program landscape at this, and many other, large US firms. As illustration, newly hired employees at this firm had to determine enrollment in up to 12 benefit programs, each with varying rules governing eligibility and default enrollment.

Finally, we present some of the first direct evidence implicating present-focus as a cause of adverse savings behavior among 401(k) eligible employees. Specifically, we found that a small immediate reward for visiting the enrollment portal (a $10 Amazon gift card) led 7 to 10% of employees

to increase their contribution while information regarding large delayed benefits associated with the plan match did not increase savings. The relative, and absolute, response to the small reward was robust and extended to plan non-participants, participants who were eligible for matching incentives, as well as employees who explicitly underestimated the generosity of the match (and for whom the match clarification could be interpreted as new information). The treatment effect persisted over the next several months for which we have administrative data and at least one-half of contribution adjustments entailed increases of more than one percent, suggesting that the influence of rewards on savings was not born from strategic nominal adjustment employees expected to quickly reverse. To contextualize the magnitude of the effect, we project that the behavioral response of employees, previously situated below the match threshold, resulted in an average (max) gain of $488 ($1,500) via the plan match after 6 months and $1126 ($3,000) after 1 year.

What explains why a significant share of employees responded to the small immediate financial incentive from the field study but not the far larger, but delayed, incentive associated with the match? To clarify mechanisms underlying the experimental response, as well as the baseline decision of many employees to delay savings, we calibrate a simple framework of intertemporal savings decisions, adapted from DellaVigna (2018). The framework features a utility-maximizing employee, subject to present-bias taking the form of beta-delta preferences, who must decide whether to delay savings in a 401(k) plan with a generous match. The calibrations imply that to rationalize employee behavior would not only require implausibly large psychological hassle costs of enrollment but that such hassle costs fall within a similarly implausible margin of $10 relative to the net present financial value of the plan match. Moreover, the calibrations suggest that for the non-participants in our sample, the baseline decision to delay enrollment can be rationalized for at most several days, and only then, if one assumes significant hassle costs of enrollment. Leveraging our access to directly elicited employee beliefs, we show that because most employees expect to delay savings for months, rather than days, the beta-delta framework cannot rationalize the baseline behavior of employees.

We conclude by discussing alternative models of present-focus that might better reconcile employee behavior in this setting as well as more broadly. In particular, we advance one promising model, informed by research in neuroscience and psychology, that presumes that employees face a high degree of proximal financial anxiety and that such anxiety imposes a psychological cost of contemplating the enrollment decision. Critically, the model also presumes employees are systematically, but mistakenly, optimistic about a reduction of such anxiety at some point in the intermediate future—that is at a horizon measured in months rather than days. The model shows how employees, cognizant of the inadequacy of their current savings, significantly delay enrollment despite their intent to delay enrollment only moderately. Finally, in this account, the experimental gift card derives its potency as an emotional

inducement that alters the affective calculus of the savings decision rather than its nominal financial value. We present evidence from our survey and field experimental data consistent with this anxiety-based account—the majority of employees in our sample report high proximal levels of financial anxiety, the severity of this anxiety predicts employee response to the reward, and employees exhibit optimism about future levels of anxiety, but only over medium and long-run horizons.

Our findings regarding the efficacy of a nominal financial reward, the inefficacy of personalized guidance and clarification of plan generosity, and the striking degree of confusion about enrollment status should be of prescriptive use to policymakers and plan designers seeking to improve the savings of at-risk employees. As opposed to costly investments in financial education, plan marketing, and decision-aids, such as popular financial calculators, our findings suggest that one could increase employee engagement by leveraging inexpensive, but immediate, rewards and/or by increasing the transparency of each employee's actual enrollment status. More generally, our alternative account for how present-focus affects savings implies different strategies for structuring (and marketing) 401(k) plans than either the standard economic model or a model in which savings delays arise from beta-delta preferences.

The present research draws from, and contributes to, several strands of the existing literature on retirement savings. Centrally, our study offers novel, direct, evidence, involving behavioral response to time-varying incentives, for the widely-embraced assertion that present-focus plays a central role in 401(k) savings (e.g., Madrian and Shea 2001, Thaler and Bernatzi 2004). However, our calibrations, informed by evidence on the savings intentions of employees, challenge the prevailing practice of modeling such present-focus through beta-delta models of limited self-control, even allowing for significant hassle costs associated with enrollment. Second, while we find evidence consistent with the literature in documenting the pervasiveness of deficits in retirement literacy (including measures of financial literacy) (e.g. Lusardi & Mitchell 2011; Goda et al. 2014), we provide experimental evidence rejecting, at least in our sample of at-risk employees, the presumed causal link between such literacy and savings. Third we highlight another example in which confusion, in the context of a benefit program, hinders engagement, but in this case such confusion involves the novel dimension of mistaken enrollment status. Finally, our findings contribute to the growing list of examples in which consequential economic decisions appear to emerge from non-standard decision-processes, that extend beyond simple information frictions, and have significant implications for optimal policy design.[4]

[4] The possibility that decisions may be more accurately captured through non-standard, and possibly non-deliberative, models of decision making has been asserted across a range of consumer financial decisions including savings (e.g., Benartzi and Thaler 2007), the choice of health insurance (e.g., Ericson and Starc 2012), and tax response (Chetty et al. 2009, Finkelstein 2009).

2 INSTITUTIONAL BACKGROUND AND RESEARCH SETTING

2.1. 401(k) Plan Structure and Employee Engagement

401(k) Plan Structure. In recent decades, 401(k) plans have evolved to become a primary channel through which employees save for retirement. These defined-contribution plans, named after the sub-section of the tax code from which they arose as the result of the Revenue Act of 1978, permitted qualified US employees to contribute a capped share of pre-tax salary into a tax-deferred individual savings account. In the traditional version of the plan, employee contributions are typically deducted automatically from an employee's paycheck and gains are not subject to taxation until disbursement, and even then, only as ordinary income.[5] As of 2014, 401(k) plans comprised 78% of all private employer-sponsored retirement plans and covered 77 million employees (EBSA 2016).

Beyond favorable tax-treatment and portability, a distinguishing feature of most 401(k) plans is an often generous employer match.[6] Intended to encourage employee savings, these matching incentives typically involve an employer matching some share of an employee's contribution (often 50 percent or 100 percent) up to a maximum annual threshold usually ranging from 3 to 6 percent of annual salary. A recent industry survey indicated 75% of employer-sponsored 401(k)s plans offered matching incentives (PLANSPONSOR DC Survey 2017). While the generosity of plan matches varies widely, the modal match structure entails an employer contribution of 50 cents for every 1 dollar contributed by the employee up to 6 percent of annual salary.

Employee Engagement. One can characterize plan engagement by inspecting the rate at which employees participate and the average contribution rate of enrollees. Prior to the advent of automatic enrollment, employee participation in 401(k) plans was low and differed substantially by employee age and income (e.g., Madrian and Shea 2001). The aggressive adoption of automatic enrollment, particularly by larger plans, in recent years, has led to sharp increases in participation, and has reduced the inequality in participation across employee sub-groups. Of plans with automatic enrollment, participation rates are usually in the range from 85 to 90% (Vanguard 2016). While automatic enrollment has increased participation, because of the low default contribution rates associated with most plans, not only do most employees not contribute to the maximum allowable contribution each year, most employees do not contribute up to the limit under which an employee can accrue matching contributions. In recent years, many firms with automatic enrollment installed for new hires, have been known to administer one-time "sweeps" through which they automatically enroll non-participating existing employees into their plan at

[5] This excludes 401(k) contributions designated as Roth deferrals.

[6] Legislation enacted shortly after the introduction of 401(k)s tied contribution limits for highly compensated employees to the level of contributions by lower compensated employees, effectively creating an incentive for plan sponsors to encourage participation at all income levels (Tax Reform Act of 1984).

the default rate unless they opt-out (see BCBM 2018 for discussion). Such sweeps presumably help employers increase average participation across eligible employees of varying tenure.

In theory, one might expect participation and average contribution rates to be sensitive to the generosity of a plan's matching incentives. However, the data suggests a modest relationship between the presence, and generosity, of a plan match, and employee engagement (e.g., Papke & Poterba 1995, Choi et al. 2002; Duflo et. al. 2006; Kusko et al. 1997, see Madrian 2013 for a review). A 2014 industry study illustrates the frequency with which employees fail to take-up matching incentives in reporting that, across 550 401(k) plans covering 4.4 million participants, 25% of plan-eligible employees failed to fully take-up matching incentives, forgoing an average of $1,336 or 2.4% of their annual salary (Financial Engines 2015). The prevalence of unclaimed matching dollars (even in settings absent of liquidity constraints, e.g., Choi et al. 2011), as well as the substantial influence of automatic enrollment on participation, offer two empirical puzzles for most standard economic models of employee savings.

2.2. 401(k) Plan in the Present Research

Our field partner offered it's 40,000 benefit-eligible employees a 401(k) plan with a fairly typical plan structure but for, perhaps, the unusual generosity of the plan match. Specifically, as depicted in Figure 1, the firm instituted automatic enrollment for new hires beginning in 2015.[7] Under this enrollment regime, new hires who did not actively opt out were enrolled at an annual contribution rate of 4% allocated to a target-date fund. In June 2015, the firm informed existing employees who became benefit-eligible prior to 2015 and were contributing less than 4% of an imminent enrollment sweep. The sweep automatically enrolled such employees, but for those actively opting out, at a 4% contribution rate at the end of July 15th.

A distinguishing feature of the 401(k) plan was the generosity of the plan match. Not only did the plan match employee contributions, dollar-for-dollar, up to 4% of eligible compensation, but it installed a minimum match floor of $2000 each year for any enrollee contributing at least 4%. For employees earning less than $50k each year and not taking full-advantage of the match, the match floor effectively generated a return to the marginal dollar of contribution exceeding 100%. Within our sample, employees of this type faced returns to the marginal dollar of contribution ranging from 100 to 500%, with a median return of 125%. Overall, plan participation at our partner firm seems similar to national averages for plans with automatic enrollment. As of July 2016, among new hires eligible for automatic enrollment, or existing employees subject to the enrollment sweep, the opt-out rate was approximately 10 percent.

[7] Prior to 2015, employees had to actively opt in to the 401(k) plan either through the firm's online savings portal or over the phone with an HR representative. Employees could also change their contribution rate online or by phone on any day and this would change the amount of their salary withheld and deposited to their 401(k) from each paycheck starting at the next payday that was 1-5 business days after the change was initiated.

Despite the generosity of the 401(k) plan, 77% of employees invited to participate in the survey and 73% of respondents neglected to take full advantage of the match.

3 THEORETICAL FRAMEWORK OF EMPLOYEE SAVINGS

To organize our hypotheses, we introduce a simple theoretical framework describing an employee's decision to save and then outline the four conceptual departures we aim to test. Absent these departures, standard lifecycle savings frameworks suggest three channels that might explain an employee's decision not to participate in a 401(k) plan with matching incentives. First, they may have an authentic preference for consuming more in the present than in retirement, (i.e., low desired income replacement ratio in retirement). Second, they may accurately perceive the costs of enrolling to be very high and expect that these costs will be lower in the future. Finally, they may have very high costs of saving due to liquidity constraints that make it difficult for them to maintain current consumption and finance savings.

We generalize a standard model of savings that allows for both time-consistent exponential discounters and present-biased discounters. Specifically, our framework adapts the notation and exposition of DellaVigna (forthcoming) who models the 401(k) enrollment decision of an employee in a setting that closely resembles that of the present research and accommodates present bias of the beta-delta form.[8] We then consider three departures capturing the possibility that the employee is not fully informed about the net benefits from making 401(k) contributions, confused about the employer matching formula and may be averse to any complexity involved in initial enrollment costs.

3.1. The Savings Decision

We begin by describing the employee's decision of whether to contribute more to their 401(k). For simplicity, we focus on the decision of a current non-participant in our 401(k) facing the binary decision of whether or not to begin contributing 4% of her salary today, i.e., fully taking up the match. Her alternative is to do nothing today and face the same decision tomorrow. We assume that the employee has beta-delta type preferences with her total utility given by:

$$U_t = u_t + \beta \sum_{s=1}^{\infty} \delta^s u_{t+s}$$

[8] DellaVigna adapts O'Donoghue & Rabin's (1999b) model of a binary savings enrollment decision for exponential and present-biased discounters to explain the effects of automatic enrollment on 401(k) participation in the institutional setting of Madrian & Shea's seminal 2001 study

Where $U_t = (u_t, u_{t+1}, \ldots)$ represents her total utility from instantaneous utility in period *t* and the present discounted value of instantaneous utility experienced in all future periods, which we treat as days. The employee's discount factor is given by $\beta\delta$ between today (*t*) and tomorrow (*t + 1*) and by δ between any two days in the future with $\beta, \delta \in (0,1)$. We will consider both the case of the exponential discounter ($\beta = 1$), and the present biased discounter ($\beta < 1$) as well as distinguishing between cases where the decision-maker is sophisticated about her future time preferences, (i.e., she accurately perceives β, denoted $\hat{\beta} = \beta$), and the fully naive present-biased discounter who thinks that her future self will not be present-biased ($\beta < 1,\ \hat{\beta} = 1$). We further assume that this is a one-time contribution decision, so once she starts contributing 4% of her income to the 401(k), she continues to do so in every period until she retires at *T*. For simplicity, we assume that in period *T* she receives her accumulated savings as a lump-sum and normalize the value of never saving to 0.

Simply put, the employee will change her savings and begin fully taking up the match today if the one-time cost of taking this action, which we denote by *k*, is exceeded by her net present value of the delayed benefit from deferring her savings. For simplicity, we assume constant marginal utility of consumption renormalized to 1 and that long-term discounting equals the interest rate, that is, $\delta = 1/(1 + r)$. Under these assumptions, i.e., abstracting away from any change in the marginal utility of consumption and expected market growth, the net utility gain an employee receives from contributing an additional *s* dollars in a period can be written as:

$$b = \tau_0 s + \mu - \tau_R(s + \mu)$$

Where τ_0 is the tax rate for consumption today that she avoids by deferring income, μ is the effective return on her savings from the employer match, and $\tau_R \leq \tau_0$ is the tax rate she will pay if she consumes her income after retirement. We think of the cost of acting, *k*, as the employee's opportunity costs of time. To capture the fact that these vary on different days, we model this by drawing *k* in each period from a uniform distribution over potential cost. Since the employee will act (i.e., increase her savings rate) whenever *k* is below some threshold, her probability of acting in any period, which we denote Pr[Increase], is decreasing in *k*.

3.2. Standard Model Baseline: Because the exponential discounter is time-consistent she will either act today or never since delaying would only reduce the matching incentive and tax benefit she collects. Thus, she will act today whenever the net benefit from this action exceeds zero:

$$-k + \sum_{t=1}^{\infty} \delta^t b \geq 0$$

We can simplify her decision rule by noting the Taylor series expansion for $\frac{\delta}{1-\delta}$ and see that she will invest whenever:

$$k \leq \frac{\delta b}{1-\delta} = \frac{\delta(\tau_0 s + \mu - \tau_R(s+\mu))}{1-\delta}$$

3.3. Psychological Frictions

Friction 1: Present Bias ($\beta < 1$). The first psychological departure we are interested in is already embedded in this framework, the existence of present bias of the beta-delta form (i.e. $\beta < 1$). A key insight from O'Donoghue and Rabin (1999a) is that a sophisticated present-biased discounter will not delay action very long because she foresees that she will face similar temptations not to act in the future. In this case, O'Donoghue and Rabin derive a bound on the maximal delay t^* for a sophisticate that makes her indifferent between acting today or in t^* days. This employee will enroll today rather than enrolling in t^* days whenever:

$$-k + \beta\delta\frac{b}{1-\delta} \geq \beta\delta^{t*}\left(-k + \frac{\delta b}{1-\delta}\right)$$

Using a Taylor expansion approximation for $\delta \to 1, (1-\delta^{t*}) \approx (1-\delta)t^*$, the sophisticated present-biased employee's decision rule is to enroll whenever:

$$k \leq \frac{\beta\delta(1-\delta^{t*})b}{(1-\beta\delta^{t*})(1-\delta)} = \frac{\beta b}{1-\beta}t^* = \frac{\beta(\tau_0 s + \mu - \tau_R(s+\mu))}{1-\beta}t^*$$

With a maximal delay of: $t^* = k\frac{1-\beta}{\beta\delta}$.

In contrast to the sophisticate, a fully naive present-biased employee anticipates that she will act like an exponential discounter at her next opportunity to invest, tomorrow. Thus, she will invest today whenever:

$$k \lesssim \frac{\beta(\tau_0 s + \mu - \tau_R(s+\mu))}{1-\beta}$$

And a naïve employee will delay forever if enrollment costs are above this decision threshold and below $\frac{\delta(\tau_0 s+\mu-\tau_R(s+\mu))}{1-\delta}$, her anticipated cost of delaying. We will consider the sophisticated present-biased employee for the remainder of this section and relate this to actual beliefs in our observed data.

Prediction. Note that since the probability of acting each day is proportional to the threshold *k*, the more extreme an employee's present bias (lower β) the less likely she is to increase savings:

$$\frac{\partial \Pr[\text{Increase}]}{\partial \beta} > 0$$

Friction #2: Deficit in Retirement Literacy ($\hat{b} < b$). The next departure we consider captures the possibility that deficits in literacy about financial needs in retirement affect enrollment decisions. We can think of this as a situation where an employee's perceived benefit from deferring income, denoted $\hat{b}$, is lower than the actual deferred benefit, *b*. Incorporating this possibility into the decision rule of a sophisticated present-biased discounter, the employee's decision rule is to enroll whenever:

$$k \leq \frac{\beta \hat{b}}{1-\beta} t^*$$

Prediction: In this case, the more that the employee underestimates the deferred benefit of saving, that is, as $b - \hat{b}$ increases, the less likely she is to increase her contribution:

$$\frac{\partial \Pr[\text{Increase}]}{\partial (b - \hat{b})} < 0$$

Friction #3: Plan Confusion ($\hat{\mu} < \mu$). The third departure we consider reflects the possibility that people underestimate or are completely unaware of the employer contributions they will receive conditional on participation. In the case where an employee is completely unaware of the match this lowers her perceived *b*, the net utility gain from increasing saving by a dollar, from $b = \tau_0 + \mu - \tau_R(1+\mu)$ to $\hat{b} = \tau_0 - \tau_R$.

In general, the perceived benefit associated with a given perceived match $\hat{\mu}$ can be denoted: $\hat{b} = \tau_0 + \hat{\mu} - \tau_R(1+\hat{\mu})$. Correspondingly, the decision rule for a present-biased employee can be found by substituting this value of *b* into her decision rule:

$$k \leq \frac{\beta\delta(1-\delta^{t*})(\tau_0 s + \hat{\mu} - \tau_R(s+\hat{\mu}))}{(1-\beta\delta^{t*})(1-\delta)} \approx \frac{\beta(\tau_0 s + \hat{\mu} - \tau_R(s+\hat{\mu}))}{1-\beta} t^*$$

Prediction: As in the case of financial literacy deficits leading to $\hat{b} < b$, the more an employee underestimates the match, the less likely she is increase savings:

$$\frac{\partial \Pr[\text{Increase}]}{\partial (\mu - \hat{\mu})} < 0$$

Friction #4: Enrollment Complexity ($\tilde{k} > k$). A final departure captures the possibility that the employee perceives higher enrollment costs than those associated with the opportunity cost of time spent

implementing her change. We can think of enrollment costs in this case as some $\tilde{k} = k + k'$ where *k* still captures the opportunity costs of time for implementing a savings change, but the employee also perceives some additional cost *k'* associated with action. While we remain agnostic as to the specific source of these costs, we can think of them generally as psychological costs associated with action.

Again, higher perceived costs will simply mean that an employee is less likely to perceive realized enrollment costs to meet her decision rule since she now requires the following to act:

$$\tilde{k} = k + k' \leq \frac{\beta(\tau_0 s + \hat{\mu} - \tau_R(s + \hat{\mu}))}{1 - \beta} t^*$$

Prediction: Thus, an employee with higher perceived costs may not find it worthwhile to enroll in some cases where her counterpart without this friction would act.

$$\frac{\partial \Pr[\text{Increase}]}{\partial k'} < 0$$

These departures show four different possible frictions which might make an employee less likely to increase her contributions and take up the match. In the next section we describe our empirical strategy for examining the role of these different channels by intervening to reduce frictions associated with retirement literacy, and plan confusion, and by changing the immediate financial benefits of enrollment to offset the tendency of a present-biased employee to delay savings increases.

4 EMPIRICAL RESEARCH DESIGN

4.1. Overview

To test the causal role of our four behavioral frictions in low plan engagement, we administered a field experiment in July 2016 targeting low-saving employees with below-median incomes at a large US firm in the financial services sector.[9] The field experiment was embedded within the final module of an approximately 10 to 15 minute online firm-sponsored survey that was marketed as an opportunity for employees to provide confidential feedback on the workplace on benefit programs. While the survey's initial three modules were designed to elicit information permitting us to estimate the severity of each employee's baseline deficit in measures associated with each of the frictions (and to collect relevant background detail), the final module promised employees an evaluation of their preparedness for retirement based on their recorded responses. This final module, however, experimentally varied the

[9] The firm, which requested anonymity, provides several retail, as well as commercial, financial services. Employees in our sample were engaged across a diversity of functions within the firm of which many were not directly related to finance.

information, and in some cases, incentives, presented to employees to test how alleviating each behavioral friction affected the willingness of employees to increase their savings. While the field experiment was primarily intended to yield a series of between-subject estimates of changes to an employee savings across interventions it also offers compelling estimates of treatment effects relative to out-of-sample comparisons. We describe the sample, experimental treatments, survey, and study implementation in greater detail below.

4.2. Employee Sample

Two factors shaped the composition of the field sample—a desire to target under-saving, low-to-moderately, compensated employees and a preference of our field partner for a sample limited to 5,000 employees and the use of simple screening criteria. Further, because the design of the experimental treatments differed depending on whether an employee's contribution met or exceeded the match threshold, we ultimately invited two distinct employee sub-groups to participate in the field study (i.e., the marketed survey within which the field experiment was embedded). The first, and primary, invitation sample ("Low Savings" arm) comprised the universe of 3,719 plan-eligible employees who satisfied the following screening criteria as of July 2016: (i) 25 to 55 years of age, (ii) earning less than $100k, and (iii) contributing at an annual rate below the 4% plan match threshold. We invited a second sample of 1,000 employees ("Moderate Savings" arm) randomly selected from all plan-eligible employees, as of July 2016, who satisfied the first two screens, involving age and income, and who were contributing to the 401(k) plan at a rate between 4 and 10 percent.

Of the 4,719 employees invited by email to participate in the field study across the samples, 1,332 employees completed the online survey instrument during the approximately 10-day pre-specified study period, resulting in a response rate of 28%. Of respondents, we excluded 105 employees from the analytic sample of the field experiment because they reported contributions exceeding our estimated target contribution rate (making it impossible to assign them to an experimental treatment). The remaining 1,137 employees were randomized to one of the available experimental treatments (780 in the Low Savings arm and 357 in the Moderate Savings arm).[10]

Table 1 describes the demographic background and savings behavior of the invited and respondent sample and provides additional financial detail for the latter group. The table conveys the demographic diversity of the sample and also indicates the similarity of invitees and respondents in gender share, age, tenure, and imputed income, suggesting that response was not strongly predicted by any observed demographic characteristics. In comparison to the broader employee population, the

[10] Assignment to the Low or Moderate savings arm was based on self-reported, and not administrative, enrollment status. This led to the erroneous assignment of 111 employees, who were contributing less than 4 percent, to the Moderate Savings arm, and 9 employees, who were actually contributing at or above 4 percent, to the Low Savings arm.

respondent sample is disproportionately female, reflecting the broader gender disparity in the firm. Finally, as depicted in Appendix Figure A1, relative to the earnings distribution of full-time employees reported in the 2015 CPS, our respondents under-sampled the lowest quartile of earnings, and over-sampled those in the middle two earnings quartiles, reflecting our screening criteria. The figure also reaffirms the approximate similarity between invited employees and respondents across the income distribution.

4.3. Baseline Survey

4.3.1. Structure and Procedure

Overview. In July 2016, employees were invited by email to participate in a 10- to 15-minute survey marketed as an opportunity to provide confidential workplace, and benefit program, feedback. As depicted in the schematic shown in Figure 2, the instrument featured 5 modules of which the first four constituted the *Baseline Survey* while the last module administered the *Field Experiment*.[11] The *Baseline Survey* captured background information and detail required to calculate employee-specific deficits for each friction. The initial four modules were identical across respondents but for some practically necessary customization in language based on an employee's enrollment and contribution status and, because of a desire to expand the breadth of collected variables without lengthening the instrument, select questions were only administered to random sub-samples of respondents (and on day 4 of the survey we added an additional set of questions due to the unanticipated size of the initial response). We now describe the invitation procedure and email, as well as each of the four contents of the baseline survey.

Email Invitation and Reminders. We sent emails to a list of email addresses provided by our partner in mid-July 2016. The emails, which carried the insignia of our partner firm, communicated that we were researchers from CMU that had received permission from the firm to administer a survey to collect confidential workplace and benefit program feedback. The email conveyed the survey deadline and provided each employee a customized link to an online survey instrument hosted on the Qualtrics platform. The email also informed employees of a participation incentive involving entry into a raffle for one of several Apple iPads. Finally, two reminder emails were sent the day prior to, and the day of, the survey deadline, encouraging anyone who had not responded to do so.

4.3.2. Survey Content

Module 1 – Employee Background. The first module of the survey featured questions pertaining the demographic and financial background of employees including their age, income (measured

[11] Note that while we distinguish questions by module, or semantic category, respondents did not strictly proceed through the modules in the order in which we describe them. In some instances, questions from different modules were situated proximally on the survey due to considerations of user experience and the desire to customize certain questions based on initial response.

categorically), tenure at the firm, current level of accumulated savings, and emergency liquidity. One objective of the first module was to collect the inputs that would permit us to calculate recommended contribution rates for each employee using formulas adapted from commercially available "retirement calculators" (some of these inputs were asked in later modules). These calculations of target contribution rates not only informed the subsequent analysis, but, in some cases, were used to customize the guidance that was provided later in the survey as a part of the field experiment.

Module 2 – Benefit Program Knowledge. The second module queried respondents about their knowledge, and understanding, of firm benefit programs, with a central focus on 401(k) plan details and enrollment requirements.[12] Specifically, employees were quizzed about their 401(k) eligibility (note that administrative records indicated that all respondents were plan-eligible), their enrollment and contribution status, and their knowledge about automatic enrollment, the default contribution rate, and the match. We also asked employees about their future intent to enroll or change their contribution over varying horizons (3 months for participants, and 6 months to 3 years for non-participants), and to inform planned tests of enrollment complexity, we elicited employee perceptions of the complexity, and time-costs, associated with enrollment.

Beyond assessing benefit program knowledge, we directly asked respondents to introspect as to why automatic enrollment was so successful in increasing plan participation. Specifically, after describing the documented influence of automatic enrollment on 401(k) participation, we asked respondents to identify the best of four candidate explanations (as well as the option of typing in an alternative explanation): (i) automatic enrollment helped employees overcome low plan awareness, (ii) automatic enrollment reduced time-costs, complexity of opt-in enrollment, (iii) automatic enrollment helped employees overcome procrastination associated with opt-in enrollment, (iv) employees, uninterested in enrollment, procrastinated *opting-out* of automatic enrollment.[13] Finally, as a gauge of respondent attentiveness to survey questions, we included a question to designed to assess whether the respondent read the text of the survey question.[14]

Module 3 – Retirement Literacy. A third category of questions helped to assess whether employees harbored plausible beliefs across a wide range of retirement-relevant outcomes. As an

[12] We asked employees about their awareness, and to a lesser extent, understanding, of multiple benefit programs in order to mask the purpose of the survey, out of a broader interest to understand benefit literacy, and in deference to our partner firm which was interested in understanding employee perceptions of benefit programs beyond the 401(k) plan.

[13] The practice of directly asking target populations the question of broader interest was inspired by Bhargava and Manoli (2015) who directly asked EITC eligible non-claimants about the causes for non-claiming.

[14] The attention check was a generic question, ending with an acknowledgement that sometimes respondents may not always have time to read each question carefully, and an instruction that for respondents to convey that they are reading questions carefully to simply click "Next" to proceed to the next question rather than marking any of the available responses. We did not plan to exclude respondents who failed this attention check from experimental assignment or the main analyses, we but we do report the robustness of the main findings to such exclusion.

example, the module elicited employee expectations about the age at which they would retire, the duration of retirement, the income replacement ratio required to maintain their current standard of living, as well as the minimum annual 401(k) contribution that someone like them would need to save to avoid retirement insecurity (explained as a situation where they'd have to return to the workforce or rely on means-tested benefits). As a secondary outcome of interest, we also asked employees to gauge their confidence with respect to retirement preparedness.

Module 4 – Decision-Making. A fourth module presented employees with a series of decision-making assessments designed to assess the presence of financial literacy, present bias, and financial anxiety. Specifically, the module included a modified multiple price list (MPL) to gauge present bias in the context of an effort task, an adapted version of the conventional test of financial literacy, and questions assessing an employee's current, as well as projected future, financial anxiety. Due to survey constraints, the adapted MPL was a two-question measure that was limited to capturing severe manifestations of present bias.[15] As an alternative, and less restrictive measure of present focus, we interpret elicitations of future savings intent as a potential proxy for present focus. Some questions were only presented to sub-groups of respondents selected through within-survey randomizations due to ex ante considerations of survey length statistical power.[16]

4.4. Field Experiment

4.4.1. Overview and Procedure

Following the baseline survey (i.e., modules 1 to 4), employees proceeded to the final module which, they were told, would offer an assessment of their preparedness for retirement based on their earlier survey responses. In actuality, respondents were assigned to one of several, experimentally varying, variants of the assessment web-flow (described in detail below). Across all of the experimental treatments, the final module first assessed the respondent's preparedness for retirement (for simplicity, denoted *Retirement Assessment*), asked respondents if they were interested in modifying their contribution rate (i.e., enrolling or changing their existing contribution) and provided those interested with simple instructions as to how to do so (*Savings Decision*). Respondents not initially interested in modifying their rate were asked if they would like to reconsider (*Savings Reconsideration*). For all

[15] The MPL measure involved two questions where respondents chose a preferred option between a sooner smaller length of time (25 minutes) spent on a tedious effort task of counting typos and three larger later time requirements on the same task of increasing length (30, 40, or 50 minutes). In the first question, the two times are today or in one month. The second question added a front-end delay of 1 month with effort in one month or two months. This measure characterizes someone as present-biased if they choose an earlier switching point on the first three-item list than the second.

[16] All respondents after July 22nd completed module (b) on financial anxiety [N = 575] and were randomly assigned to report anticipated future anxiety for either 3 months in the future [N = 286] or 6 months in the future [N = 289]. This module included two additional questions on financial liquidity July 28-July 29 [N = 227]. From July 19-July 22 respondents were randomly assigned to complete either module (a) [N = 373] or (c) [N = 371].

respondents, the module concluded with brief follow-up questions about why a decision was made and future savings intentions (*Savings Follow-up*).

We randomized employees to a specific web-flow conditional on whether their self-reported contribution rate implied full take-up of the match. The conditional random assignment was practically necessitated because only employees contributing below the match threshold would benefit from the experimental treatments that engaged the plan match. Consequently, respondents who were self-reportedly not participating in the 401(k) or were contribution at a rate below the match threshold (i.e., contributing at 0,1,2, or 3 percent) were randomized to one 3 treatments in the Low Savings Arm, while remaining respondents (i.e., those contributing between 4 and 9 percent) were randomized to one of 2 treatments in the Moderate Savings Arm (see footnote 15 for a discussion of discrepancies between self-reported and administrative designations). For those in the Low Savings Arm, as a secondary, and independent intervention, we randomized respondents into one of 2 versions of the savings reconsideration interface. Treatments were evenly sampled and balancing tests indicated that the assignment was indeed random at least with respect to observable characteristics (Appendix Table A4).

4.4.2. Baseline Condition (Generic Guidance)

To facilitate exposition, we first describe the procedural detail and content of the Baseline Condition, and then introduce each experimental treatment in reference to departures from the baseline web-flow. The baseline web-flow consists of four distinct segments described below.

Retirement Assessment. After an initial screen explaining that employees would be provided a personalized assessment of their preparedness for retirement, respondents were directed to a subsequent assessment screen. The screen, titled "Your Personal Retirement Evaluation", featured a stylized red-to-green speedometer with the dial pointed towards red. Above the graphic, bolded text indicated that the employee was not on track for retirement preparedness and should take action now: "You should take action now to get on track for a financially secure retirement." Beneath the graphic, we displayed generic guidance suggesting that the respondent increase their contribution rate: "We recommend that you increase [in green type] your [redacted] 401(k) contribution rate."

Savings Decision. After the assessment, a subsequent screen asked respondents if they wanted to modify their contribution rate (i.e., enrolling or otherwise changing their contribution rate): "What would you like to contribute to your [Redacted] 401(k)?". Respondents were presented with a text box to indicate their desired, modified, contribution rate, and were told, if they were not interested in a modification, to leave the box blank. Respondents entering a contribution rate into the text box were led to a new screen that provided simple instructions, as well as an online link to the firm's benefit portal, to

carry out the modification.[17] A forced one-minute pause on the instruction screen was meant to provide respondents time to complete their intended modification after which employees proceeded to a screen where they were asked to confirm that they had modified their rate.

Savings Reconsideration. We asked employees expressing no initial interest in changing their rate, or indicating they had ultimately not changed their rate after proceeding through the instruction web-flow, to reconsider their savings decision: "Are you sure you don't want to change your rate?" Employees indicating a preference to change their contribution were then directed to the instruction screen described above.

Savings Follow-up. Finally, all respondents were asked to introspect as to their savings decision and were also asked about their future intent to save.

4.4.3. Primary Experimental Treatments - Moderate Savings Arm (4 to 9 percent contribution rate)

Respondents channeled into the Moderate Savings Arm were randomized to one of 2 treatment conditions – the Baseline Condition and Specific Guidance (see Figure X). We describe each treatment, and the friction the treatment was intended to test, below:

1. Baseline Condition [Retirement Literacy, Enrollment Complexity] – The baseline condition, described in detail above, served as control condition from which to identify the causal effect of providing specific guidance and to test, via an out-of-sample comparison, the impact of reducing enrollment complexity via generic guidance and simple instructions on plan engagement (i.e., $\frac{\partial \Pr[\text{Increase}]}{\partial(k)} < 0$).

2. Specific Guidance [Retirement Literacy]– This first experimental departure adapted the generic guidance from the retirement assessment of the baseline condition to include a specific, and personalized, recommended rate of contribution: "We recommend that you increase [in green type] your [redacted] 401(k) contribution rate to: <*x*>% [in red type]". For respondents who reached the instruction screen, the recommended rate was displayed again. To calculate the personalized recommendation rate, *x*, we adapted the formula used by the commercial retirement calculator that the employer referred employees to elsewhere in the benefits portal. The inputs into the calculator were largely informed by each respondent's earlier survey responses and the calculations accounted for the plan's match.[18] The provision of a specific recommendation rate

[17] The following steps were displayed: Step 1: Go to Pathfinder from your Intranet or by clicking here <*link*>. Step 2: Expand the Retirement & Investments Panel. Step 3: Click Change or Enroll Today to change your contribution rate.

[18]The calculator recommends a contribution rate projected to ensure a salary-dependent income replacement ratio (125% for salary < $25k, 100% for salary $25-$55k, 80% for salary $55k or higher) for 20 years after age 65, for a single employee in the

was intended to test whether reducing employee under-estimation of the benefits of saving, by clearly specifying how much one should contribute led employees to increase their contribution rate relative to the experimental, or out-of-sample, control. (i.e., $\frac{\partial \Pr[\text{Increase}]}{\partial(b-\hat{b})} < 0$).

4.4.4. Primary Experimental Treatments – Low Savings Arm (0 to 3 percent contribution)

Savings Decision. Respondents channeled into the Low Savings Arm were initially randomized to one of 3 primary treatments—Specific Guidance, Match Clarification, Small Reward (see Figure X). We describe each treatment, and the friction the treatment was intended to test, below:

1. Specific Guidance [Retirement Literacy]: A first treatment providing specific guidance was identical to the intervention in the Moderate Savings Arm. This treatment constituted the experimental control from which we evaluated the two other treatments in this arm. It also provided a test of how specific guidance, and the provision of simple enrollment instructions, affected engagement relative to the out-of-sample control.

2. Match Clarification [Plan Confusion]: A second treatment duplicated the specific guidance web-flow but for an extra screen which clarified the generosity of the plan's lucrative match. The additional screen, encountered prior to the savings prompt, read: "Don't miss out on extra money from [Redacted]. By taking full advantage of the [Redacted] match, you could earn $2,000 or more each year." The screen also displayed a graphic illustrating that the match effectively doubled each dollar of the employee's contribution up to 4 percent of salary beneath which text was included to further describe the $2,000 minimum match floor. The intent of this treatment was to causally test whether reducing employee underestimation about the generosity of the plan match led to increased engagement (i.e., $\frac{\partial \Pr[\text{Increase}]}{\partial(\mu-\hat{\mu})} < 0$). Alternatively, it is possible that heightening the salience of the match could lead to increased contributions even for employees with accurate beliefs.

3. Small Reward [Present Focus]: The third treatment appended to the match clarification web-flow a small reward of a $10 Amazon Gift Card to encourage employees to engage their enrollment status. The availability of the small reward was communicated by text above the

same 5-year age category and income category, accounting for social security. Reflecting national data, we assume employees under 50 have no accumulated savings, while employees 50 or older have $50,000 in accumulated savings. We make standard simplifying assumptions from the personal finance industry of a projected annual inflation rate of 3% and annual market growth rate of 8%. Recommended contribution rates (conditional on expected employer match contributions) range from 3% to 25%.

savings prompt: “To encourage you to think about your financial future, we will email you a $10 Amazon Gift Card if you take action today”. An additional note at the bottom of the same screen explained that employees could receive the gift card either by modifying their 401(k) contribution in any way today or by directly contacting the researchers if, after visiting the enrollment portal, they decided not to modify their contribution. The intervention was motivated by our goal of directly testing whether present-focus hindered employee savings by observing the differential response of employees to the experimental provision of a small reward, relative to the much larger, but delayed match. (i.e., $\frac{\partial \Pr[\text{Increase}]}{\partial(b_{now})} > \frac{\partial \Pr[\text{Increase}]}{\partial(b_{later})}$).

4.4.5. Secondary Experimental Treatments

To increase the statistical power of our test of small rewards, we independently randomized select employees to one of two experimentally variants of the savings reconsideration prompt. The randomization was restricted to respondents across either arm of the study who had progressed to the reconsideration prompt—by indicating that they had not changed their contribution after the original savings prompt—and had not been originally assigned to the small reward treatment (we chose not to offer respondents the small reward two times). Qualified employees were then randomized to a baseline implementation of the savings reconsideration prompt (as described above) or an otherwise identical prompt that offered a small reward——again, a $10 Amazon Gift Card— to encourage employees to “take action today”. As with the original implementation, the availability of the small reward was communicated by text above the savings prompt and additional text clarified that respondents could earn the reward by either modifying their contribution rate or by emailing the researchers of the decision not to do so after visiting the savings portal.

4.4. Data and Empirical Outcomes

The analysis relies on two sources of data. First, to generate our invitation sample of 4,719 employees, estimate cross-sectional relationships, and evaluate the outcomes from the field experiment, we obtained administrative employee records on employee demographics (gender, age, zip code of work location, and income decile within-sample) along with 401(k) enrollment and contribution behavior from January through November 2016. The cross-sectional analyses relied on four measures for each employee generated at the beginning of July 2016 (prior to any research intervention): (i) an enrollment tag, (ii) the contribution rate as a percent of annual salary, (iii) the annual savings rate (i.e., the contribution rate plus the employer match), and (iv) a tag indicating full match take-up (or a contribution rate of at least 4 percent). The main outcome of the field study was an indicator, generated from the administrative data, of whether an employee increased their 401(k) contribution rate between the payday

before our study (July 11, 2016) and the payday after our study (August 8, 2016).[19] We also calculated the change, over this same period, in contribution rate as well as the full match take-up indicator. Table 1 summarizes these outcome variables for the field sample. Second, much of our analysis relies on self-reported data on employee demographics, psychographics, and beliefs that we directly collected from the survey instrument (using the Qualtrics platform where the survey was hosted).

5 EVIDENCE ON CANDIDATE EXPLANATIONS

We now present findings from the field experiment and linked-survey and interpret the evidence with respect to each of the four candidate explanations. To simplify the exposition and facilitate synthesis, after briefly summarizing the overall response to the survey and field study, we organize the evidence by candidate explanation. For each explanation, we first characterize the baseline psychological friction(s) associated with the explanation and document correlations with savings outcomes, then describe how experimental reduction of the friction(s) affect savings, and finally document whether employees differentially respond to the experimental treatments based on the severity of their baseline friction(s).

5.1. Overview of Results

A total of 1,332 out of 4,719 invited employees completed the survey within the ten days during which it was active—a response rate of 28%. After excluding those saving at or above their recommended target, 1,137 under-saving employees were ultimately randomized to an experimental treatment. We presume that the high response rate—when compared to typical email marketing solicitations—was due, at least in part, to the email being sponsored by the firm, the use of reminder emails, and the use of a participation incentive. Table 3 summarizes the survey measures associated with each of the four tested frictions, organized by panel, for the full sample of respondents and for employee sub-groups distinguished by savings status. The sample size for the various measures vary considerably due to the randomization of certain questions (as well as the one-time lengthening of the survey).

The table indicates considerable diversity across employees in several of the captured measures such as financial literacy, plan knowledge, and self-control. This diversity serves to increase the power of correlational estimates between these baseline frictions and savings outcomes. Indeed, as initial evidence for the potential importance of these frictions in shaping savings, the table reveals large, and statistically significant, differences in the average value of several of the measures across participation and match take-up. Notable, the table suggests uniformity across employees in the perception that the enrollment process is not very time-consuming.

[19] Paydays occur twice a month at this firm and our survey window includes one payday (July 25, 2016).

Table 4 summarizes the response to the experimental interventions. Overall, of the 1,137 employees randomized to a treatment, 8.5% employees shifted their contribution rate (by either enrolling, increasing, or decreasing their rate) during the two pay cycles within which the survey was administered. Of shifters, the large majority, 7.4%, (s.e. = 0.008, CI: 5.6% to 8.9%) increased their rate while 1.1% lowered their rate (s.e. = 0.003, CI: 0.3% to 1.8%).

Beyond comparing changes in savings in response to each intervention, we also report and discuss behavioral responses relative to non-experimental samples. The principle out-of-sample comparison is the rate at which invited, but non-responding, employees changed their contribution rate during the period of survey administration. Among this sample, 1.7% modified their contribution rate of which 1.2% increased their rate and 0.5% lowered their rate. Overall, respondents were employees in the study were about five times more likely to change their contribution than non-respondents.

We more formally estimate the behavioral response to each of the experimental conditions with a series of regressions reported in Tables 4 (Low Savings Arm) and 5 (Moderate Savings Arm). The table reports estimates from a series of linear probability models associated with the indicated dependent variable and sample. For example, the first column of Table 4 presents estimates, for employees in the Low Savings Arm, from the following regression:

$$Pr(\text{Increase}_i) = \gamma \text{SpecificGuidance}_i + \theta \text{Match}_i + \beta \text{SmallReward}_i + \varepsilon_i$$

The equation estimates the marginal change in the likelihood of an increase in contribution (either a new enrollment or a rate increase by an enrollee) in response to each of the tested experimental conditions such that: (θ - γ) captures the marginal treatment effect of clarifying the generosity of the plan's match (in addition to specific guidance) relative to specific guidance alone and (β - θ) captures the marginal treatment effect of offering the small reward (in addition to specific guidance and match clarification). Additionally, each of the treatment coefficients yields the marginal treatment effect of the intervention relative to the out-of-sample comparisons. Table 5 reports parallel estimates for treatments in the Moderate Savings arm.[20] The tables report analogous estimates for other enrollment outcomes of interest as well as the secondary set of interventions.[21] To facilitate out-of-sample comparisons, each table also displays average enrollment outcomes for survey non-respondents. Finally, Figures 4 and 5 graphically depict the primary treatment effects for each arm of the field study.

[20] We estimate the following model of primary interventions in the Moderate Savings arm: $Pr(\text{Increase}_i) = \gamma \text{GenericRec}_i + \theta \text{Rec}_i + \varepsilon_i$, where ($\theta$- γ) identifies the marginal effect of the specific recommendation relative to generic guidance.

[21] We estimate the following model of secondary interventions, separately for each experimental arm: $Pr(\text{Increase}_i) = \alpha \text{ Confirm}_i + \pi \text{ Confirm10}_i$, where ($\pi - \alpha$) identifies the marginal effect of offering \$10 after an employee reports not changing their contribution after the initial condition, relative to a prompt to reconsider with no further incentive.

The tables and figures indicate that the provision of savings guidance or clarification of the plan match did not meaningfully contribute to the overall influence of the study on savings. Instead, the efficacy of the field study in raising engagement was nearly entirely driven by the provision of the small reward. The reward led a non-trivial share of employees to increase their contributions, often by multiple basis points, which led to even larger increases in savings due to the plan match. We discuss these results in the context of each candidate explanation in the remainder of this section.

5.2. Retirement Literacy - Candidate Friction #1

5.2.1. Baseline Incidence of Retirement Literacy

Retirement-Relevant Beliefs. We begin by documenting employee beliefs pertaining to a series of retirement-relevant measures (Table 3). The first three of these measures reflect the standard inputs into any calculation or required retirement savings: retirement (starting) age, life expectancy (imputed from age of retirement and expected duration of retirement), and target income replacement ratio (relative to current income). In theory, biased beliefs about these inputs could lead employees to undersave. The table indicates considerable dispersion in expectations across all three inputs and, in particular, with a mean of 88 years, notable optimism with respect to life expectancy. To better understand the reasonableness of these beliefs, Figure 6 plots age-specific averages for the measures relative to actuarial projections from the SSA (life expectancy), and the lower and upper bounds of recommended income replacement ratios from academics and financial advisors.[22,23]

The first panel of figure shows that employees across all ages expect to retire significantly later than recent retirees. At least for the older employees in our sample, for whom the retirement age of recent retirees offers a plausible baseline, this points to significant over-optimism in work-life expectancy. The second panel suggests over-optimism along a second measure in showing that employees expect to significantly outlive their age-adjusted actuarially predicted lifespans. Finally, the third panel suggests that employees, especially those in their 40s and 50s, harbor better-calibrated beliefs of the income replacement ratios required for a financially secure retirement (for the younger employees in our sample, the belief in higher income replacement ratios may be reasonable given expected future income increases). Despite the coarse baselines from generic samples, the figure indicates significant over-optimism in at least two of the three key inputs.

To explicitly assess to what extent these biases in retirement-relevant beliefs could plausibly account for the observed under-saving of an otherwise financially literate employee, we can compare

[22] We predict actuarially-informed life expectancies for 5-year age bins using SSA projections of life expectancy based on 2014 mortality rates. Retrieved in 2017 from https://www.ssa.gov/oact/STATS/table4c6_2014.html.
[23] The appropriate replacement ratio may vary widely due to income as well as preferences. A 2016 GAO report indicates financial advisors recommend replacement ratios ranging from 70 to 85%.

recommended rates of savings, generated from our standard calculator using conventional assumptions for retirement age, retirement duration, and income replacement, with recommended rates calculated using an employee's stated beliefs.[24] The comparison, depicted by the red and blue lines in Figure 7, suggests that for employees who are in their early 40s, or younger, the savings recommendation adjusted for stated beliefs (blue line) does not sharply depart from the actuarially-informed recommendation (red line). For employees starting to save at a later age, adjusting for stated beliefs does lower the recommended rates of savings. However, across employees of all ages, the figure suggests that the presence of biased beliefs does not appear to meaningfully explain the gap between recommended and actual savings. For example, assuming that otherwise rational employees simply underestimated how much they should save due to biased beliefs, would still imply that a majority of employees recognizably under-save. One potential explanation for why the documented biases may not be critical for rationalizing under-saving is that the two sources of employee over-optimism we document have offsetting implications for how much to save – that is, over-optimism about work-life longevity implies a reduced need to save while over-optimism about life expectancy suggests a greater need to save.

As an alternative test of how biases in beliefs might contribute to under-saving, we inspect the accuracy of the fourth retirement-relevant measure from the survey—an employee's perception of the minimal level of annual savings required to ensure retirement security. As reported in Table 3, the mean employee's belief of 13.9% reflects a significant underestimation relative to our employee-specific recommendation (accounting for the plan match) with 66% of employees underestimating the sufficient rate of annual savings. Despite this widespread underestimation of sufficient savings, most employees appear to recognize that their current level of savings nevertheless falls below their perceived level of sufficient savings. Indeed, 71% of our sample report a requisite savings level above their present savings rate and three out of four of the employees report a requisite rate of savings at least twice as high as their current rate.

Figure 7, which compares the average perception of required savings (orange line) with employee's actual savings (black dashed-line), graphically corroborates that systematic underestimation of how much one ought to save does not meaningfully explain the degree of under-saving (or its variation) in our sample. Ultimately, while the analysis points to widespread, and, in some cases, significant, biases in retirement-relevant beliefs, these deficits do little to explain the significant gap between recommended and actual savings rates.

Financial Literacy. We next assess whether deficits in financial literacy might contribute to undersaving. Table 3 reports that the employees in our sample suffer from significant deficits in

[24] Appendix Table A3 summarizes the survey data and assumptions used for each calculation in Figure 7 and for additional calculations adjusting for each belief separately in Appendix Figure A2.

comprehension of interest, inflation, and market growth. Specifically, 39% of our sample misidentify the directional effect of inflation on savings and 48% are unaware that compound interest yields a higher return than simple interest. These patterns indicate a level of financial literacy, among this sample of largely under-saving full-time employees, that is slightly lower than that reflected in a nationally representative sample of US (see Hastings et al. 2013 for a discussion of the 2009 NFCS). In addition to these two common elicitations of literacy, we quizzed employees about their belief of market growth (and compound interest) by asking about the nominal value of a $1,000 investment in a market index fund in 20 years. This elicitation revealed highly varying beliefs in the 20-year average market growth, with an inter-quartile range of 2.7% to 12.2%. Overall, these measures suggest the presence of widespread deficits in retirement literacy that, in theory, could lead many employees to under-save. However, once again, this time as indicated by the simple statistical tests of group means reported in the last column of Table 3, these deficits do not appear to strongly predict plan engagement, as measured by 401(k) participation or match take-up.

5.2.2. Experimental Test of Retirement Literacy

While the survey reveals pervasive deficits across a diverse range of measures, our initial analysis suggests that these baseline frictions seem, at best, likely to explain little of the differences in participation and match take-up. low saving and match take-up in our sample. To more explicitly test for the causal role of low retirement literacy on plan engagement, we document employee response to the experimental provision of specific guidance about the appropriate rate of savings. We interpret specific guidance as one strategy through which to improve retirement literacy by combatting employee underestimation of how much to save. If deficits in retirement literacy involve employees systematically underestimating the financial assets required for a secure retirement, or the rate of current savings required to achieve this asset accumulation, i.e., $(\hat{b} - b) < 0$, as our surveys suggest, and such deficits causally contribute to low plan engagement, specific guidance about how much to save should lead employees to increase contributions, particularly among employees suffering most severely from retirement illiteracy.

The outcome of the field study reject the causal influence of deficits in retirement literacy on plan engagement. Table 4 indicates that, in the Moderate Savings Arm, the experimental provision of personalized savings recommendations does not lead to a differential increase in the share of employee engagement (b = 0.03, s.e. = 0.02) relative to generic guidance (b = 0.03, s.e. = 0.02) (p = 0.98). Similarly, Table 5 indicates no differential effect of the provision of specific guidance (b = 0.02, s.e. = 0.01) relative to the out-of-sample control (b = 0.02, s.e. = 0.002).

A plausible explanation for the absence of response to the savings guidance is that employees simply did not find the recommendation to be credible or did not attend to this information.[25] One distinguishing feature of our experimental design is that we are able to directly measure how exposure to each intervention affected employee beliefs using a within-subject assessment built into the survey [is this true?]. The first two columns in Table 6 report the outcome of simple regressions intended to assess how the interventions affected two measures of retirement literacy—an indicator of whether an employee estimated an annual required rate of savings at least as high as the recommended rate and a secondary measure indicating low confidence in retirement preparedness–captured before and after exposure to the interventions.

The table suggests that the experimental provision of guidance successfully improved retirement literacy for a significant share of respondents. Specific guidance increased the share of employees, in the low saving arm, with beliefs that they should be saving at least as high as the recommended rate ($b = +0.26$, $p < 0.01$). In the moderate savings arm, specific guidance produced an identical shift in beliefs ($b = +0.26$, $p = <0.01$), which reflects a differential increase relative to employees assigned to the generic guidance treatment ($b = +0.10$, $p < 0.01$) (diff test, $p < 0.01$). [maybe statement about confidence in preparedness if we include those estimates]. Overall, the table is consistent with the interpretation that reducing retirement literacy, by providing credible information about how much one ought to save, does not lead to increased 401(k) engagement.

5.2.3. Differential Experimental Response by Baseline Retirement Literacy

While the evidence reviewed to this point offers little support for the causal influence of deficits in retirement literacy on average adverse savings outcomes, the field study was designed to permit an even stronger assessment of whether employees suffering most severely from a particular bias, such as retirement illiteracy, are responsive to the experimental provision of guidance (and, relatedly, whether the severity of baseline illiteracy predicts the magnitude of response to the intervention). Table 7 implements these tests of heterogeneity (restricted to the Low Savings Arm to facilitate comparisons across all interventions) by reporting the treatment effect estimated specifically for employee sub-groups characterized as scoring low or high in bias across measures associated with each of the frictions.

The first panel of the panel summarizing the marginal treatment effect of the specific guidance intervention, on the likelihood of increasing contributions, separately estimated for subgroups of employees based on their score on two central measures of literacy: an indicator for whether the employee perceived their required rate of savings to be below their recommended rate (prior to seeing the

[25] While the overall pattern of survey responses suggests that respondents were attending to questions, we do see evidence consistent with some inattention: 40% of the sample failed our attention check embedding an instruction to skip the next question in a paragraph of instructions. We return to this check later in the section.

intervention) and an indicator denoting a below-median (within-sample) aggregate score on the financial literacy questions (high bias = 0 or 1 correct; low bias = 2 or 3 correct). The estimates from the table, while measured more imprecisely than the main treatment effects, offers no evidence that employees suffering most severely from deficits in retirement literacy, as tagged by either of the two measures, substantively increased savings in response to the provision of specific guidance. The table also indicates no statistically difference in treatment effect estimates across the employees measuring low and high in savings rate underestimation ($p = 0.60$) or in financial literacy ($p = 0.32$).

5.3. Plan Confusion - Candidate Friction #2

5.3.1 Baseline Incidence of Plan Confusion

A second friction we investigate is employee confusion about the generosity of the plan match or plan eligibility. In theory, an otherwise rational employee who underestimated their plan match, or mistakenly believed that they were not plan eligible might limit their plan engagement. The second panel of Table 3 indicates that while nearly all surveyed employees were aware of their plan eligibility, 30 percent had inaccurate beliefs about the match threshold of 4 percent. Two-thirds of confused employees, or 20% of all employees, under-estimated plan generosity. The table may underestimate the degree of match confusion if one presumes greater confusion regarding the guaranteed minimum match of $2,000 than the match threshold (a figure centrally featured in plan marketing materials). The possibility that match confusion, and specifically underestimation of match generosity, contributes to low plan engagement is consistent with the cross-sectional difference in the degree of confusion and underestimation across plan participation ($diff_{conf} = 0.13$, $p < 0.01$; $diff_{underest} = 0.12$, $p < 0.01$) and, among participants, across match take-up ($diff_{conf} = 0.17$, $p < 0.01$; $diff_{underest} = 0.07$, $p < 0.01$).

5.3.2. Experimental Test of Plan Confusion

To understand the causal effect of reducing match confusion/underestimation (or alternatively, the effect of increasing the salience of the match to those with accurate beliefs), we document the behavioral response to the experimental provision of match clarification from the field study (i.e., reducing $|\hat{\mu} - \mu|$). The estimates reported in Table 4 (Column 1) offer no indication that clarifying the match led to an increase in engagement relative to the baseline condition (specific guidance). The table also suggests (Columns 2,3) no meaningful increase in engagement, due to match clarification, for employee sub-groups distinguished by how much of the match was unclaimed (as proxied by their baseline contribution).

5.3.3. Differential Experimental Response by Baseline Plan Confusion

As a final test of the causal role of plan confusion, we inspect the differential response to the experimental clarification of the match across employees based on their degree of plan confusion. Table 7 reports the heterogeneity in experimental response across employees who either did (high bias) or did not (low bias) underestimate plan generosity in the baseline survey (and, for comparison, reports marginal effects for the specific guidance condition). The table reveals a small positive, but not significant, marginal effect of match clarification on increased contributions for high bias employees who underestimated match generosity in the baseline survey (b = 0.03, *ns*), which must be compared to the zero marginal effect associated with specific guidance for the same employees (b = 0.00, *ns*) ($p_{diff} = 0.50$). Employees who did not suffer from match under-estimation did not meaningfully respond to the match clarification, particularly in relation to the baseline guidance treatment.

Ultimately, the baseline survey and field experiment, suggest a significant share of employee underestimation about match generosity, and no strong evidence for a causal link between such underestimation and low plan engagement. However, we cannot rule out the possibility that for a modest share of employees, confusion about the match contributed to low savings. These results are consistent with Choi et al. (2011) who find that clarifying the plan match to a sample of under-saving, elderly, 401(k) eligible employees, did not result in additional savings.

5.3.4 Confusion Pertaining to 401(k) Enrollment Status

Beyond confusion regarding plan eligibility and match generosity, our analysis revealed an unanticipated dimension of confusion—remarkably, 27% of employees reported a 401(k) contribution rate inconsistent with administrative records. As reported in Table 8, as a consequence of these discrepancies, of which most involved employees over-reporting their contribution, 36% of actual non-participants in our sample spuriously claimed enrollment, and 19% believed they were contributing at a rate at or exceeding 4 percent.

There are at least two plausible explanations for the documented discrepancies that do not involve genuine confusion about one's enrollment and/or contribution status. A first possibility is that employees were simply inattentive to the survey instrument and recorded responses subject to random error. However, the systematic direction of the bias—89% (or 0.24 of 0.27) of the discrepancies involve over-reporting—and high frequency of discrepant responses claiming the specific rate of 4 percent (from a menu ranging from 0 percent to 10+ percent) seem inconsistent with random error. To more formally adjust for the potential role of inattention, Table 8 bounds our estimate of confusion by reporting the share of discrepancies restricted to respondents passing the fairly demanding attention screen—which only 60% of respondents overall passed—embedded within the survey. This restriction lowered the estimate of

potential confusion about enrollment status among non-participants from 36% to 30% and of full match take-up among those not taking up the match from 19% to 15%.

A second possibility, not involving confusion, is that employees deliberately exaggerated their contributions towards the outcome respondents perceived as socially desirable (or similarly, as desirable to the researcher). This motive for biased self-reports has long been recognized as a potential feature of experimental response (e.g., Zerbe and Paulhus 1987, Peltier and Walsh 1990). However, the distribution of responses in these data does not immediately appear to be consistent with substantial exaggeration since nearly all discrepant responses involve contribution rates in the low-to-middle, rather than upper, range of the possible response options. To formally assess the possible role of exaggeration, we attempted to adjust the discrepancy rate for respondents who exhibited a pattern consistent with exaggeration on several other survey questions, pertaining to savings, income, education, and financial anxiety, for which one could identify an unambiguous, socially desirable, response.[26] The lower panel of the table reports the adjusted discrepancy rates that reflect various strategies for reclassifying respondents with socially favored responses as non-discrepant. The adjusted estimates, once again, suggest that even the most conservative accommodation of potential exaggeration still leaves a significant residual rate of discrepancy. After adjusting for both inattention and exaggeration, using the technique outlined above, reveals a residual discrepancy rate of 22% of non-participants for enrollment status, and 7% of those not fully taking up the match for match take-up (last line of the table).

Interpreting this residual discrepancy rate as a (conservative) estimate of employee confusion about enrollment/contribution status seems justifiable in light of the institutional and procedural details of enrollment at the partner firm. Consider that newly hired employees at this firm were asked to make enrollment decisions across a wide range of benefit programs—e.g., retirement savings, life insurance, commuting benefits, short and long-term disability insurance, personal accident insurance, medical and prescription health plans, dental insurance, visual care coverage, health savings account, a wellness program—during their initial days of employment. The emergence of confusion about one's program status seems reasonable in light of the complicated, and highly varying, set of rules governing eligibility and default enrollment across benefit programs.

Two additional patterns in our data are suggestively consistent with this account. First, the modal reported contribution rate among discrepant reports was 4 percent—the widely known default rate associated with the firm's 401(k) plan. Second, as reported in Table 7, discrepant respondents, randomized into the small reward condition, were more than three times as likely to increase their

[26] We characterized employees as potential exaggerators if they reported the highest possible categories of savings rate, income category, education, accumulated savings, or lowest financial anxiety as well as the small number of respondents who misreported the contribution change they made after our intervention.

contribution (b = 0.20, p < 0.01) than their counterparts with accurate beliefs (b = 0.06, *ns*), (p_{diff} = 0.04). This pattern is consistent with the possibility that when confused employees learned of their actual administrative enrollment/contribution status, due to a reward-induced visit to the enrollment portal, many increased their contribution to adhere with their original beliefs.[27]

5.4. Enrollment Complexity - Candidate Friction #3

5.4.1. Baseline Incidence of Enrollment Complexity

A third friction we consider is the possibility that even a small degree of enrollment (or, equivalently, rate adjustment) complexity could impede savings if employees associate such complexity with larger psychological hassle costs. As initial evidence on baseline perceptions of the time and effort required to enroll, Table 3 reveals that 77% of respondents perceived the act of enrollment to take only a few minutes, and this belief did not significantly differ across plan participation. Two additional questions similarly suggest that only a small share of employees in our sample viewed offer insight into employee perceptions of enrollment complexity. When directly queried about the reasons for non-participation, only two percent of non-participants indicated that "time constraints" prevented them from changing their contribution rate (Appendix Table X). In a second question asking respondents to speculate as to why automatic enrollment has been so effective at increasing plan participation, only 10% of respondents cited the explanation that automatic enrollment helped to overcome the complexity/hassle of opt-in enrollment. In spite of these generally low estimated time costs, employees not participating in the plan did indicate that the complexity of the enrollment process affects this decision, with nearly half of the sample (49%) indicating that they would be either somewhat or very likely to save more if enrollment was simplified.

5.4.2. Experimental Test of Enrollment Complexity

While the survey offers little evidence for the widespread perception among employees of enrollment as a significant ordeal, the experimental treatment with generic guidance (moderate savings arm) offers an out-of-sample test of whether easing the administrative burden of enrollment causally affects plan engagement. Specifically, comparing the increase to contributions among employees randomly assigned to the generic guidance arm with the out-of-sample comparisons offers a suggestive estimate of the joint influence of generic guidance, and survey participation, on engagement. The treatment, as described above, reduces the administrative burden of enrollment by indicating that the employee ought to increase their contribution and also by communicating that such an increase in enrollment can be implemented in a few simple steps—steps that are then simply communicated to

[27] The closest analogue to this finding relates to work by Dushi and Honig (2015) who document significant discrepancies between self-reported savings from among HRS respondents and actual savings from linked SSA administrative records.

anyone expressing an interest in a modification. We did not expect the first four modules of the survey, particularly given the survey was marketed in a way that did not reference savings, to have a pronounced, independent, effect on 401(k) engagement, if it did have an effect, we assume that such an effect would be both modest and in the direction of increasing engagement.

Table 6 presents initial evidence, using a within-subject measure, documenting that the experimental provision of generic guidance in the moderate savings arm did increase the share of employees who saw enrollment as requiring a minimal time investment. Specifically, the table reports a 0.05 increase in the (already high) share of employees who perceived enrollment to require a few minutes or less after, relative to before, exposure to generic guidance ($p < 0.05$). Correspondingly, Table 5 shows that the experimental provision of generic guidance led to a modest, but imprecisely measured, 0.03 increase in the share of employees increasing their contributions (*ns*) relative to out-of-sample controls where the share of contribution increase was 0.4 ($p_{diff} < 0.10$). We interpret the modest behavioral response to generic guidance, along with the low baseline incidence of perceived enrollment complexity, as suggesting that perceptions of the outsized costs involved with enrollment, whether psychological or economic in origin, represent a causal deterrent to savings for, at most, a small share of employees.

5.4.3. Differential Experimental Response by Baseline Enrollment Complexity

As an additional test of the causal role of enrollment complexity, we report the differential response to the experimental simplification of administrative complexity across employees based on their degree of plan confusion. Table 8 indicates that employees perceiving enrollment to be complex, as defined by perceiving it to require more than minutes to complete, were not more responsive—and in fact were directionally less responsive— to the simplification of enrollment than their counterparts. Given the imprecision of these differential estimates, the low baseline incidence of perceived complexity, and the absence of primary treatment effects associated with simplification, we interpret the evidence as failing to implicate perceptions of enrollment complexity as a barrier to plan engagement in this setting.

5.5. Present Focus - Candidate Friction #4

5.5.1. Baseline Incidence of Present Focus

The fourth, and final, friction we consider is the possibility that the present-focus of employees acts as a barrier to their saving. Table 3 reports two measures for present-focus captured by the survey which indicate that a modest incidence of present-focus among the sample. Our first measure of present-focus, a two-item hypothetical MPL, suggests that 10% of respondents may suffer from fairly severe present-bias. Considering that the measure was limited, due to length and time constraints, to capturing only severe manifestations of present-bias, the incidence we find may not be inconsistent with the 57% rate of present-bias among US adults documented using a more extensive measure (Goda et al. 2015). A

second set of measures that could plausibly be interpreted as reflecting employee present-focus are indicator variables that record whether an employee perceived themselves as having at least a 50 percent (or 75 percent) chance of saving within the next 3 months. This of near-term savings intent implies that the share of present-focus among employees is 21% (50 percent threshold) or 6% (75 percent threshold). reveals that 21% of respondents express short-run intents that are consistent with present bias affecting their savings plans.

As with other measures, Table 3 also summarizes the correlation of these measures with plan participation and match take-up. The table shows no correlation across the MPL measure and plan participation or match take-up, but does reveal a correlation between these outcomes and intentions to save in the near-future. However, we note that the correlation between the savings intent measure could simply be the result of plan engagement causally influencing near-term savings intentions. Overall, the table offers a mixed characterization of present-focus among the sample. A modest share of employees appear to be present-focused using the presented measures and the cross-sectional association between these measures and plan engagement is mixed.

5.5.2. Experimental Test of Present-Focus

To gain clearer insight into the potential role of present-focus on plan engagement, we turn to the field study to compare the marginal response of employees to the experimental provision of the small, immediate, reward for engagement to the clarification of the much larger, but delayed, plan match $\frac{\partial \Pr[\text{Increase}]}{\partial(\hat{b})}$. While the reward reflects an immediate compensation of $10, for employees unaware of the match, the clarification informs employees of what amounts to a $1,844 increase in the perceived present value of retirement benefits assuming a single year of match receipt (that is, ignoring the high degree of inertia in contributions that characterizes most employee samples). Alternatively, under the same assumption that employees do not undo any adjustments to their contribution rate through the end of the year, the clarification of the match implies a net present value of benefit amounting to about $14 per pay period.

We can infer the marginal response of employees to the provision of the $10 reward, as a primary intervention in the low savings arm, or a secondary intervention in either the low or moderate savings arms, through the appropriate pair-wise comparison reported in Table 4 (or depicted in Figure 4). Turning initially to the primary interventions, the table indicates that 7% of employees increased their contribution following exposure to the small reward (in addition to the match clarification and specific guidance) ($p < 0.01$) while only a nominal share of employees increased savings in response to the baseline provision of match clarification and guidance ($b = 0.01$, ns) ($p_{diff} < 0.01$). When implemented as a secondary intervention targeting those who had just declined to raise their contribution, the small reward led 10%

(low savings arm, $p < 0.01$) and 13% (moderate savings arm, $p < 0.01$) of employees to increase their contribution rates, significant improvements in engagement when compared to the baseline response of 2% (low savings arm, ns, $p_{diff} < 0.01$) and 1% (moderate savings arm, ns, $p_{diff} < 0.01$). Overall, of those exposed to the small reward across any of the interventions, 9.8% of respondents increased their contribution, compared to 4.5% of those without exposure. As a reference for comparison, as described in greater detail above, respondents did not meaningfully increase engagement in response to the marginal provision of the plan match.

Notably, among employees assigned to the low savings arm who increased their contribution rate in response to an intervention with the small reward—of whom none were taking full advantage of the match by design (as judged by self-reported contribution)—nearly one-half did so by a sufficiently large margin so as to cause them to fully take-up the plan match (specifically, 47% of respondents to the primary intervention, and 45% of respondents to the secondary intervention, transitioned to full match take-up). The propensity of respondents to significantly increase their contribution rates, along with the recognition that our measures of experimental response were derived from administrative records recorded at least one pay-period following the survey, suggests that employee response to the small reward did not simply reflect a strategic intent to (temporarily) increase contributions by some nominal amount, but instead reflects, at least for many employees, a genuine intent to increase their savings.

5.5.3. Differential Experimental Response by Baseline Present-Focus

To further interrogate the role of present-focus on employee savings, we can examine the differential response of employees to the small reward across baseline measures of present-focus (as well as the differential relative response to the small reward as compared to match clarification). Table 7 summarizes the differential likelihood that an employee increases one's contribution in response to the intervention with the small reward (along with the match clarification and specific guidance) separately for employees characterized as more (high bias) or less (low bias) present-focused as measured by the two survey proxies. As a reference for comparison, the table also reports the analogous treatment effects corresponding to the intervention providing match clarification (and specific guidance).

While this analysis of differential response is particularly limited by the restricted size of the survey sample, those registering high in present bias respond strongly to the intervention containing the reward as evaluated by either the MPL ($b = 0.33$, $p < 0.05$) or intent-to-save ($b = 0.23$, $p = 0.11$) measures. The responsive of these employees is substantially larger than that of employees registering less highly on either the MPL ($b = 0.02$, *ns*, $p_{diff} = 0.11$) or intent-to-save ($b = 0.03$, *ns*, $p_{diff} = 0.01$?) measures. Comparing the estimates associated with the match clarification intervention with the reward

intervention suggests that the responsiveness was driven almost entirely by the provision of the small reward.

Ultimately, the evidence from the survey and the field speaks to a modest to moderate incidence of present-focus among employees as indicated by baseline measures, and a pronounced willingness among such employees to increase their savings in response to a small financial incentive, but not clarification of a far larger, but delayed, plan match. Indeed, our calculations imply that a single pay-period of additional savings for the typical employee in our sample would have yielded more in net present financial value than the $10 value of the gift card. A significant share of employees who responded to the small financial incentive did so by increasing their contributions by a non-trivial margin, in many cases, increasing their likelihood of financial preparedness for retirement.

5.6. Synthesis of Evidence and Interpretation of Magnitudes

Table 8 synthesizes evidence from the survey and the field. The table implies that three of the tested candidate frictions, despite their prevalence and, in two of the three cases, unconditioned correlation with savings outcomes, do not causally contribute to low employee plan engagement in this setting. First, while our findings corroborate other research in documenting widespread deficits in *retirement literacy*—defined here to include both errant beliefs about retirement-relevant inputs and low financial literacy—and their correlation with savings outcomes, intervening to clarify such deficits does not increase savings on average or or among those suffering most severely from such deficits. This is not surprising in light of evidence that while our employees may underestimate how much they should be saving each year (potentially due to many of the factors noted in the literature), most seem to recognize that they are presently saving far less than even the underestimated benchmarks.

Second, while we document substantial *employee confusion* about plan details, notably involving employees underestimating the generosity of the plan match, experimental clarification of the match does not result in increased enrollment despite successfully shifting employee beliefs. While we cannot rule out a small positive effect of the clarification on enrollment for employees underestimating the match, the evidence doesn't implicate match confusion as a major causal determinant of low plan engagement despite the contrary implications of the unconditional correlation. Finally, in considering the role of enrollment complexity, while we document that about one-quarter of employees in our sample perceive enrollment as a lengthy procedure (i.e., requiring more than a few minutes), we show that experimentally simplifying the steps required to adjust one's contribution does not significantly increase contributions relative to plausible out-of-sample comparisons, on average, or differentially across employees varying in their perceptions of the time-intensity of enrollment.

The study does provide causal evidence for at least two barriers. First, as indicated by the table, we document substantial confusion among employees about their enrollment status. Of all employees in the sample, 15 percent have inaccurate beliefs about their current enrollment, including over one-third of non-participants. If one chooses to interpret the reward as an instrument for knowledge of one's actual enrollment status, then confused non-participants were more than three times as likely to enroll after exogenous exposure to their correct enrollment status than unconfused counterparts. Second, and perhaps most centrally, the table provides evidence that present focus constitutes an important barrier to increasing savings and match take-up in the 401(k) setting for up to 10 percent of employees (an open question is whether a more generous reward would have led to even greater engagement). Two arguably conservative lab measures of present focus suggest moderate prevalence in the employee sample, and employees registering high on these measures were substantially more likely to respond to the small reward than their counterparts—neither group was responsive to the clarification of the much larger matching incentive and employees were not sensitive to the magnitude of their foregone match. For employees previously contributing below the match, we estimate that shifting one's contribution to the match threshold would have resulted in an additional $744 (max: $1,500) over 6 months, and $1487 (max: $3,000) over one year, in employer contributions, assuming no subsequent change to contribution.

6 INVESTIGATING MECHANISMS UNDERLYING PRESENT FOCUS

A striking feature of the field study is that a significant share of employees increased their 401(k) contributions, often by a significant margin, after exposure to a small, but immediate, financial incentive, but were largely insensitive to information clarifying large, delayed, matching incentives. Relatedly, the majority of our employee sample neglected to fully take-up the generous plan match as of the pre-study baseline. What mechanisms underlie the apparent present focus of employees? To begin this inquiry, we first evaluate whether the theoretical framework described earlier, in which a fully-informed, but present-biased, employee delays enrollment because of beta-delta preferences (and potentially, psychological hassle costs associated with enrollment). The exercise involves estimating the range of model parameters required to rationalize the findings of the present research and assessing the plausibility of such parameter values vis-à-vis the literature. We then outline several alternative theoretical accounts of present focus and critically consider whether any offer a more successful reconciliation of the present findings.

6.1. Calibrating the Beta-Delta Model of Enrollment

6.1.1. Baseline Enrollment and Match Take-up

We begin by assessing whether the theoretical framework described in Section 3 can plausibly account for an employee's baseline decision to delay enrollment and forego the generous plan match. As before, we restrict our consideration to the simplified decision of an employee deciding whether to either

decline enrollment or to enroll at the match threshold of 4 percent. We initially contemplate the savings decision of an exponential discounter and then turn to a present-biased employee with beta-delta preferences. In each case, we further consider the implications of allowing for potentially significant psychological hassle costs associated with enrollment.

For tractability, we follow DellaVigna (2018) in adopting several simplifying assumptions and the case of a representative employee. Specifically, we consider an employee earning $50,000 annually ($25 per hour) subject to an equivalent marginal tax rate of $\tau_0 = \tau_R = 0.25$ now and at retirement. For such an employee, a 4 percent contribution amounts to roughly $8 pre-tax every working day (4% x 50,000 / 250 days).[28] Finally, we conservatively assume an annual discount factor $\delta^{365} = 0.93$, corresponding to a daily discount factor of $\delta = 0.9998$.

Exponential Employees. Under the specified assumptions, the framework concludes that an employee with a constant per-period discount factor will either enroll immediately or never enroll. The decision to enroll emerges from a comparison of the discounted value of future utility gains and the cost of enrollment, *k*, yielding the following decision criterion for enrollment:

$$k \leq \frac{\delta(\tau_0 s + \mu - \tau_R(s+\mu))}{(1-\delta)}$$

The inequality implies that, because of the generosity of the plan match, an employee should enroll so long as enrollment costs do not exceed $37,493, roughly 75% of annual income. It is self-evident that the criterion would be satisfied if an employee's *k* was constrained to that employee's time-costs of administrative enrollment, through the plan portal, and potentially also inclusive of the time-costs pertaining to deciding an appropriate contribution rate and investment allocation. Concretely, assuming approximately 25 minutes for administrative enrollment yields an approximate k ≈ 10 (i.e., 25/60 * $25/hour), while permitting an additional 2 hours for deliberation would result in an approximate k ≈ 60 (i.e., [120+25]/60 * $25/hour).

While the failure to enroll cannot be plausibly rationalized assuming a strictly time-cost interpretation of *k*, it is possible that the employees effectively associate the psychologically aversive task of enrollment with "hassle costs" of a magnitude significantly exceeding the concrete time-costs (e.g., Bertrand and Mullainathan 2004). As a first-order approximation of the magnitude of hassle costs, we appeal to a recent paper that attempted to estimate the hassle costs associated with itemization of federal taxes, another complicated, and widely-dreaded financial task (Benzarti 2017). The analysis calculated that for the typical taxpayer, aversion to itemization had a revealed price equivalent to about 4 times the

[28] We additionally assume: (i) a constant real income over working life, (iii) no change to preferences for future consumption (iv) no change to jobs and no early withdrawals, and (v) no relevant liquidity constraints.

economic time-cost associated with itemization. In our setting, hassle costs of enrollment derived from anything close to this multiplicative factor could not rationalize the failure to enroll.

Present Biased Employee. We now consider an otherwise similar present-biased employee. We initially restrict our attention to the case of a sophisticate aware of her present bias ($\hat{\beta} = \beta < 1$), and for whom the maximum enrollment delay in days, t^*, is given by:

$$t^* = k\frac{1-\beta}{\beta(\tau_0 s + \mu - \tau_R(s+\mu))}$$

Assuming costs of enrollment are restricted to the time-use costs of administrative enrollment (*k = 10*), rationalizing a delay of two weeks, the length of a single pay cycle, would require β = 0.09, a degree of present bias substantially more severe than the 0.5 to 0.9 range asserted by estimates from the field (see Dellavigna 2018) or from the typical estimate of about 0.9 from real effort tasks in the lab (Augenblick et al. 2015, Augenblick and Rabin forthcoming). Abstracting from the two-week pay cycle, for the model to predict a delay of a single week would necessitate $\beta = 0.16$, while a delay of even a single day would require a still severe, but slightly more plausible, β= 0.57.

Figure 8 plots the beta required to rationalize a delay in savings for durations ranging from 1 to 360 days for a sophisticated present biased employee with enrollment costs, *k*, of $10 (solid line), $60 (first dashed line), and $600 (second dashed line). The latter curve reflects the very conservative case where the psychological burden of enrollment is equivalent to 10x the approximate time-costs of enrollment. The curves indicate that, someone with a beta of 0.5 or higher, even assuming an expanded conception of time-costs of enrollment, should delay enrollment no more than 8 days. Assuming instead that the perceived costs of enrollment include psychological hassle costs 10 times as large as time-costs predicts an intended delay of no more than 80 days.

That most of the non-participants in our sample failed to enroll for at least six months prior to the administration of the survey, and in many cases much longer, suggests that sophisticated present bias, as modeled here, cannot accurately describe the baseline behavior of employees in the sample. An alternative possibility is employees are naïve to their present bias may delay savings despite the intent to save in the relatively near future (in the case of a naiveté, we would expect that an employee would, if anything, underestimate the length of enrollment delay).[29] To assess this possibility, we leverage our data on explicit intentions of future enrollment captured from the survey. The grey bars in Figure 8 depict the cumulative empirical distribution of the earliest expected time-horizon by which 401(k) non-participants

[29] The recognition that to understand delayed 401(k) enrollment in the beta-delta framework likely requires naivete and/or large hassle costs is a point previously made by O'Donoghue and Rabin (1999) and DellaVigna (2018).

indicate they are at least "moderately likely" to enroll. The bars indicate that only 9% of surveyed employees intend to save within a month, while only 40% intent to save within 6 months. A second elicitation points to a similar characterization in that only 21% of the surveyed employees indicated that they were more than 50 percent likely to increase their contributions in the next 3 months. Overall, neither measure supports the possibility that the non-enrollment we observe might reflect the informed, and deliberative, behavior employees, subject to plausible hassle costs of enrollment and naive present-bias. Instead, the data suggest that while the large majority of unenrolled employees plan to eventually enroll, most recognize that they will not be able to do for many months, or longer.

We can also estimate how significant perceived hassle costs would need to be to rationalize the decision of employees, subject to present bias, to (intend to) delay enrollment for several months. For a fixed level of β, recall that a sophisticated beta-delta employee would enroll so long as the following condition is satisfied:

$$k \leq \frac{\beta(\tau_0 s + \hat{\mu} - \tau_R(s + \hat{\mu}))}{(1-\beta)} t^* - k'$$

Figure 9 displays the enrollment costs, *k*, that would rationalize an intended delay of varying durations. For example, given a presently unenrolled employee who intends to enroll in 6 months, and assuming a β of 0.5, would imply perceived enrollment costs of at least $1,350. Assuming a β of 0.7 or 0.9 would require hassle costs of at least $3,150, or $12,150, respectively. Once again, the exercise implies that the baseline failure to enroll in this setting cannot be plausibly explained by the beta-delta framework, particularly in light of the limited enrollment complexity perceived by employees (see Table 3).

6.1.2. Experimental Response to the Small Reward

While the calibrations demonstrate the challenge of reconciling the significant share of baseline non-enrollment, we turn now to the field experiment, where some employees opted to enroll in response to a small financial reward, but not clarification of the much larger, but delayed, plan match. The shift in enrollment due to the presence of $10 suggests that, for a deliberative, well-informed, employee, suffering from beta-delta present bias and perceived hassle costs of enrollment, the present-value of future discounted utility flows associated with enrollment must fall precisely within $10 of enrollment costs. To illustrate, consider an employee earning $50,000 annually with a beta of 0.9 who originally anticipates an enrollment delay of six months but decides to enroll after exposure to the small reward. For the model to rationalize this behavioral response would require the employee—and all similar employees—to associate enrollment with hassle costs that fall within the implausibly precise range of $12,140 and $12,150.

Overall, calibrations not only reject the plausibility of rationalizing baseline non-participation through the widely used beta-delta model of employee savings, but point to the even greater implausibility of rationalizing the observed experimental response.

6.3 Alternative Frameworks for Understanding Present Focus

We consider a series of alternative models that might more accurately account for the low plan engagement of the employees in our sample. We initially discuss alternate approaches to modeling present bias from the economics literature, then discuss approaches informed by psychologists, and finally suggest a promising mechanism informed by the literature as well as the present findings.

6.3.1. Alternative Economic Frameworks for Present Bias

Time-Varying Preferences. The beta-delta or quasi-hyperbolic discounting adopted in our theoretical framework reflects the most popular current formalization of time-varying preferences in economics (Laibson 1997, O'Donoghue and Rabin 1999, 2001). However, several alternative discounting functions have also been proposed. One such model assumes that any decision made by an individual in the initial period is subject to a fixed cost (Benhabib et al. 2010). In theory, one could imagine that such a model might account for the delayed enrollment in our setting. Yet our calibrations suggest that to predict the behavior we observe, the fixed costs in such a model would also have to be extremely large (i.e., of the magnitude of the discussed hassle costs), and closely linked to the present discounted value of the benefits of enrollment. Another strategy for theoretically representing time-varying discount functions is to engage the possibility of context-specific discount rates tied to factors such as affect (Vallacher 1993, Loewenstein 1996) or poverty (Banerjee and Mullainathan 2010)

Dual-Self Models**.** Another longstanding approach to modeling self-control problems is to directly model the interaction of distinct systems that differ in their time-horizons (i.e., one more myopic and the other more farsighted) or in the manner in which they make decisions (i.e., one more patient and one more impatient) (e.g. Shefrin and Thaler 1988 and Fudenberg and Levine 2006; see Gilovich and Griffin 2010 for a review of the psychology literature). Generally speaking, for a model in which behavior is determined by strategic interaction between competing selves to explain the observed delays in enrollment, would require that the preferences of the myopic, or impatient self, dominates those of the more deliberate, or patient self. It is not clear how the stated preference for delays, typically on the order of several months, could be readily explained by such models. [30]

[30] Researchers have also proposed specific forms of preferences over sets, procedural rationality, and subadditivity that could produce such reversals (e.g., Gul and Pesendorfer 2001, Rubenstein 2003, Read 2001)

6.3.2. Psychological Mechanisms

Psychologists have proposed several distinct mechanisms that could produce present bias, with varying implications for optimal policy design (see Urminsky and Zauberman 2014 for an excellent review). For example, the economic models described above might each be viewed as ways of summarizing the interaction of affective influences shaping immediate utility (Loewenstein 1996) and more deliberate considerations of all deferred consequences. Both behavioral and neuroscientific evidence has corroborated the influence of affective factors on intertemporal choice (e.g., Shiv and Fedorikhin 1999, McClure et al. 2004)

Yet several other proposed mechanisms could also produce present bias. For example, the tendency to visualize immediate outcomes more concretely than delayed ones may contribute to heightened focus on the present (e.g., Liberman and Trope 1998, Malkoc et al. 2005; Malkoc and Zauberman, 2006) and a reduced sense of connectedness to far-future selves may lead us to place less weight on outcomes with long delays (Parfit 1984, Hershfeld et al. 2011). Another mechanism that might drive present bias is a tendency to underappreciate the impact of immediate consumption choices for future outcomes due to either underestimation of or inattention to the opportunity costs of immediate actions (Zauberman and Lynch 2005, Frederick et al. 2009) or failures to plan (Lynch et al. 2010).

Each of these mechanisms may contribute to present bias in 401(k) savings decisions but we think that another underappreciated factor that may shape behavior in our setting is an affective motivation to avoid engaging financial decisions due to anxiety about this domain.

6.4. An Anxiety-Based Account of Present Focus

Motivation and Background. We suggest a new approach to explain employee behavior in our setting, motivated by the prevalence of financial anxiety in our sample and the broader US population and previous research on the behavioral impacts of anxiety and stress. Specifically, we propose that anxiety about the financial domain plays a central role in how individuals engage consequential decisions regarding their future financial security and may exacerbate self-control problems in these settings. We first describe previous research on anxiety and its implications for decision-making. We then outline a conceptual framework of how anxiety might drive observed behavior in our setting.

The general phenomenon of stress has been widely studied as a neurobiological and psychological construct associated with adverse effects on cognitive and decision-making outcomes. Broadly defined as a state emerging from situations in which one's regulatory system is unable to meet the requirements of the environment (McEwan and Stellar 1993), stress is associated with a number of physiological pathways that contribute to adverse effects on decision-making. Specifically, researchers have documented the role of anxiety in creating deficits in attentional control (Eysenck et al. 2007),

emotional regulation (Park et al. 2016), memory (Wolf 2009), and executive function (Arnsten 1998). While research on the effects of anxiety or stress in economic domains is scarcer, some evidence exists that stress is associated with increased risk aversion and impulsivity (Haushofer and Fehr 2014).

Financial anxiety has specifically been defined as having a strong negative affective response to engaging with one's personal finances (Shapiro and Burchell 2012). The prevalence of anxiety about finances has been consistently documented in national surveys (APA 2017). Researchers have also documented that financial anxiety is characterized by avoidance of the financial domain (Shapiro and Burchell 2012). In terms of economic decision-making, the idea of avoidance as a response to anxiety-inducing thoughts is of particular interest because it suggests a motivation for the low engagement and behavioral inertia documented in retirement savings and other financial contexts.

An Anxiety-Based Model of Enrollment. In our own setting, we suggest an account of how financial anxiety might drive employee behavior. This account has two main elements: First, the desire to avoid high psychological costs associated with this domain exacerbates any self-control problems. Second, being induced to take the first concrete step in the savings process lowers perceptions of remaining costs.

We conceive of the psychological costs employees face as spanning a broad spectrum of potential inputs. These range from cognitive costs of determining budget implications of different savings rates to immediate hedonic disutility of thinking about an unpleasant domain, perhaps regretting past decisions and anticipating negative future outcomes. Without specifying all possible inputs, we propose that financial anxiety is a positive predictor of these costs. That is, an employee who feels more financial anxiety will find engaging with savings more unpleasant because it involves confronting her anxieties.

Clinical research on interventions aimed at overcoming anxiety are potentially instructive for understanding why inducing people to take the first step in the process of changing their 401(k) contribution might lead them to follow through on this action, while identifying costly consequences of inaction would not lead them to undertake the initial step themselves. For example, Cognitive Behavioral Therapy (CBT) incorporates repeated confrontation with anxiety-inducing situations to learn that such situations do not produce the anticipated catastrophic results (Mineka & Thomas 1999), suggesting that exposure may correct overestimates of psychic costs.

Suggestive Empirical Evidence from Field Study. A striking pattern in our survey responses is the high prevalence of financial anxiety across income levels. Overall, 57% of respondents indicate that they feel at least "a fair amount" of anxiety about their finances, with almost a quarter (24%) reporting that they feel "a lot" of anxiety. Figure 10, Panel A depicts the distribution of self-reported financial anxiety across the income distribution in our sample. As we would expect given objective financial pressures,

anxiety is most common for lower earning employees, yet we note that it is persistent even at higher incomes, with nearly 20% of employees earning over $75,000 reporting high anxiety about their finances.

Beyond offering a potential explanation for large costs associated with savings engagement, our data also offer insight into the relationship between current and anticipated anxiety. Notably, if employees are still considering hedonic costs of anxiety in a cost-benefit framework, high proximal anxiety should only deter behavior today if they expect these costs to be lower in the future. Yet, only 16% of respondents in our field sample told us that they expect to be less anxious about their finances in a few months.[31] To better understand projections of future anxiety we posed more extensive questions to a sample of 1000 adults recruited from Amazon Mechanical Turk. We find similar patterns as in the field of high current anxiety and Figure 10 Panel B shows the average expected change in financial anxiety over varying future horizons (3 months, 6 months, or 1 year). Both between-subject and within-subject comparisons in this sample indicate that people expect larger drops in anxiety over a year than over a few months. This might encourage longer delays in engaging with financial decisions. Alternatively, contemporaneous anxiety may change judgments of immediate costs but not predictions of future behavior if people underappreciate how much future anxiety will impact their decisions. This would be analogous to the literature on projection bias showing that people underappreciate future influences of visceral factors such as hunger or emotions even while these factors affect their immediate decisions (see Loewenstein 1996).

Finally, we look at how high financial anxiety is associated with responsiveness to our $10 intervention. Table 9 summarizes how predicted increases in 401(k) contributions in response to our interventions differ across the subset of employees for whom we elicit financial anxiety. The top half of the table describes differences in estimated responses to the $10 offer between employees who self-report low or high financial anxiety. Judging by point estimates, we see directional evidence that more anxious employees responded more sharply to the $10 intervention than less anxious counterparts (Primary intervention: 0.10 vs. 0.03 effect size, Secondary intervention: 0.07 vs. 0.06). The bottom half of the table compares the small number of employees who report both high financial anxiety and an anticipated drop in future anxiety to all other cases. Directionally, the differences in predicted savings increases across these groups are also consistent with employees being most responsive to the $10 intervention if they have high current and lower future anxiety. While we have limited power to examine this relationship in our sample, these patterns point to the idea that anxiety not only raises current psychic costs of savings engagement but also makes people more susceptible to behavior change in response to a small nudge. We think this is an important avenue for research that merits further exploration.

[31] Some employees answer this question for a horizon of 3 and others for 6 months.

7 CONCLUSION

We describe findings from a field experiment, embedded within a survey of beliefs and decision-making, to understand the role of candidate psychological frictions in the savings of predominantly at risk employees at a large US firm. The research design aspired to test not only the average marginal effect of reducing each friction through information- and incentive-based interventions, but to additionally examine whether the employee-specific incidence of each friction moderated the behavioral response to the experimental treatments.

The study yields four results we see as contributing to the rich existing literature investigating the retirement savings behavior of US employees. First, we corroborate existing research on the prevalence of low *retirement literacy*, as well as positive correlations between literacy deficits and poor savings, but find that the experimental provision of clear, specific, and personalized, recommendations did not increase savings, even among employees with the most severe literacy deficits. Second, we find no evidence that *enrollment complexity* impedes savings—few employees perceived enrollment as overly complex administratively and simplifying enrollment did not increase savings. Third, in an analysis of *employee confusion*, we find substantial evidence for confusion about plan details, such as the underestimation of match generosity, but do not find that such confusion causally leads to low plan engagement (despite positive correlations between match underestimation and match take-up). However, in an unplanned analysis, we estimate that at least one-quarter, and potentially more than one-third, of 401(k) non-participants falsely believed they were enrolled. These employees enrolled at high rates upon being prompted to observe their actual enrollment status. Finally, we present new direct evidence implicating *present-focus* as a cause of low 401(k) engagement by documenting the willingness of employees to increase savings in response to a small reward (a $10 Amazon gift card) but not to clarification of the much larger, but delayed, benefit implied by the plan match. Calibrations, informed by the baseline behavior of employees, their response to the experimental treatments, and their intentions regarding future savings from the survey, suggest that the prevailing beta-delta framework of present-biased employees cannot rationalize the observed patterns. We propose an alternative, anxiety-based, model to potentially explain these patterns as well as the broader set of empirical savings puzzles.

We highlight two important limits to the present research. First, by design, our sample does not represent the broader sample of 401(k) eligible employees within, or outside of, the firm. We do see our results as generalizing to populations of under-saving, or below-median earning, full-time employees in the US with the potential caveat that at our firm enrollment may be more administratively straightforward than at other firms. Second, given our administrative data is restricted to 401(k) plan activity over a limited period, we cannot observe if employees offset increases to 401(k) contributions with diminished

savings through external channels, subsequent decreases in 401(k) contributions, or increases in debt (perhaps resulting from early-withdrawal loans, see Beshears et al. 2017).

In spite of these limitations, we interpret the findings of this paper as offering valuable insight into our understanding of theoretical models of savings, as well as the optimal design, and effective marketing, of retirement savings plans. With respect to the latter, an obvious initial lesson is that (non-monetary) rewards may be more effective at engaging previously unengaged employees than larger, but less proximal, financial incentives (such as those associated with the plan match). The use of tangible, non-monetary, rewards, as well as gamification, has been explored more extensively in contexts involving health (e.g., medical adherence, exercise, healthy eating), education, and labor productivity. A second lesson is that the considerable resources that US firms invest in financial education, and just-in-time decision-aids, may be less effective than far cheaper interventions that focus less on remedying deficits in understanding, and more on addressing barriers such as confusion about one's enrollment status and present focus. A final lesson is that if the specific mechanisms underlying the present focus we document may offer a roadmap to more effective plan design. For example, if affective misforecasting over intermediate horizons leads to savings delays, this suggests the promise of strategies such as coupling 401(k) plans with transitory "serenity accounts" intended to reduce financial anxiety by permanently increasing access to short-term liquidity.

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Figure 1.

Timing of Field Experiment and Evolution of 401(k) Plan Administration

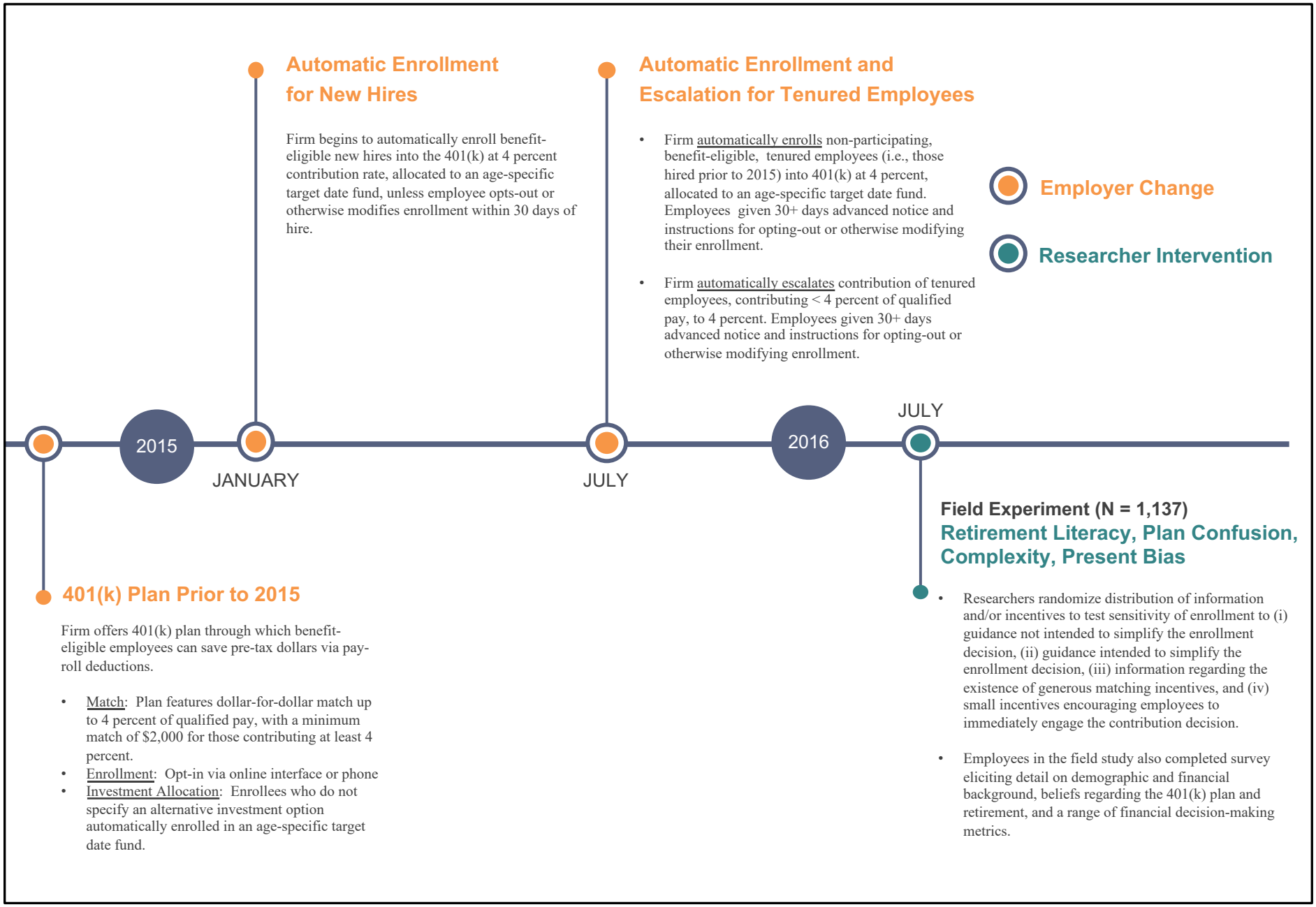


Figure 2.
Research Design Schematic for Survey and Field Experiment

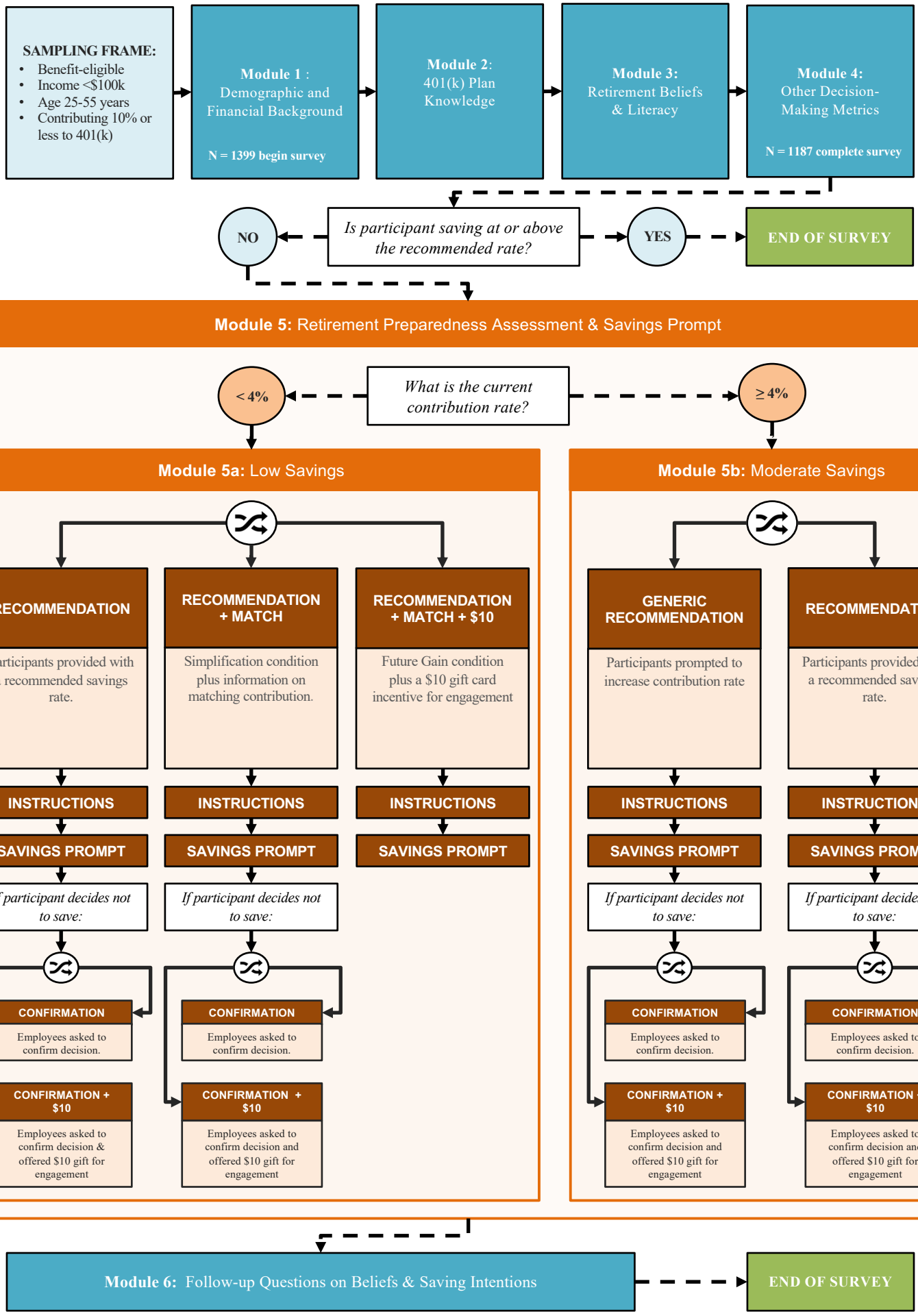
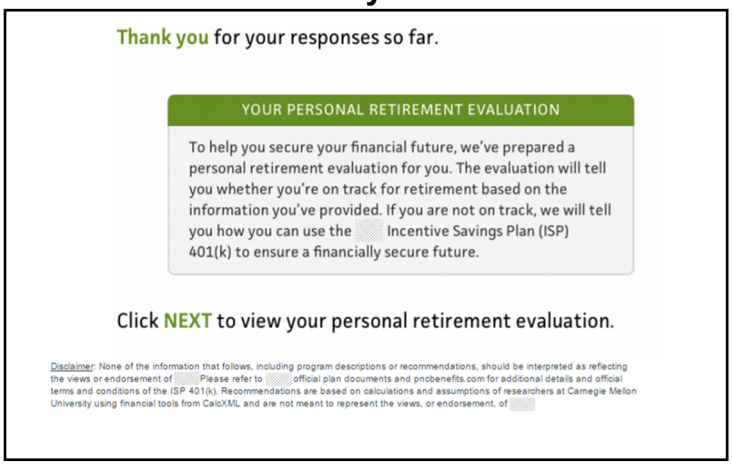
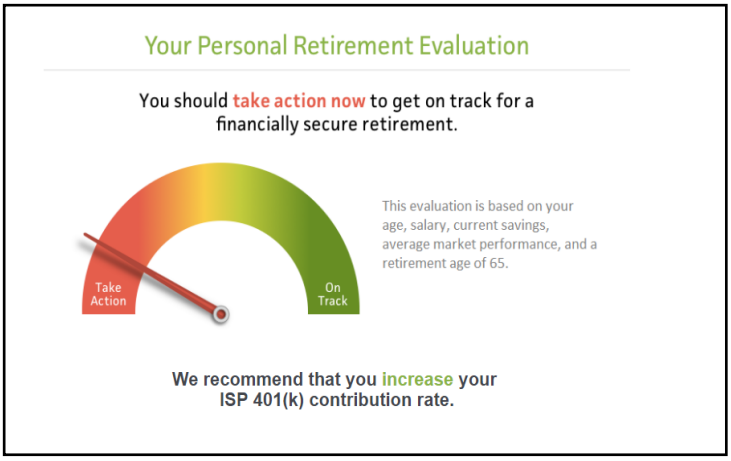


Figure 3.
Field Experiment Interventions (Screenshots)

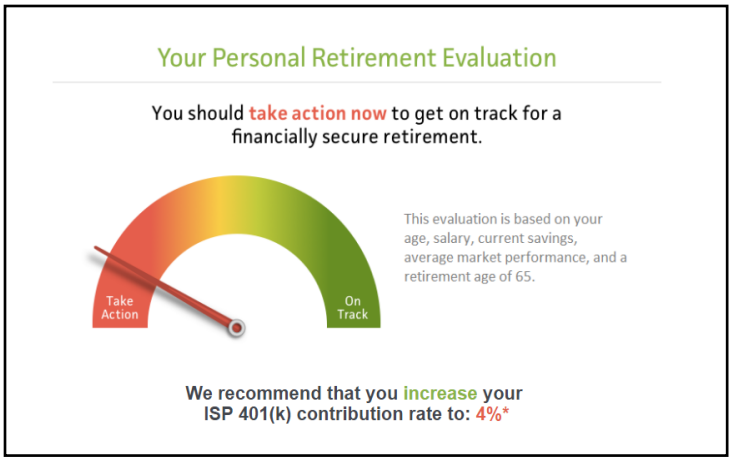
i. Introductory Screen



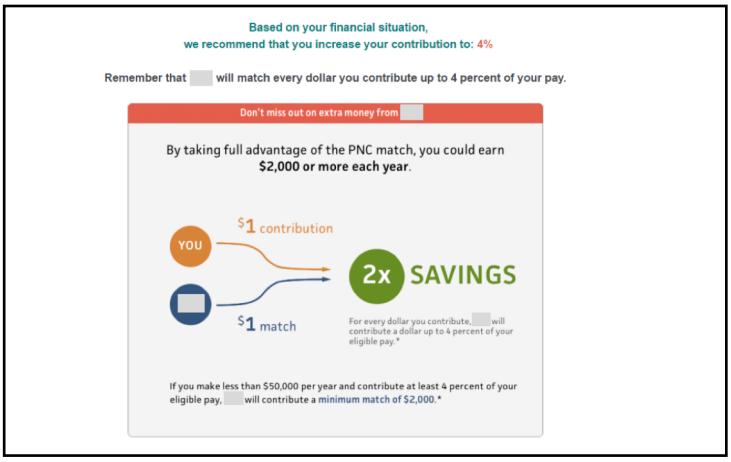
ii. Generic Recommendation



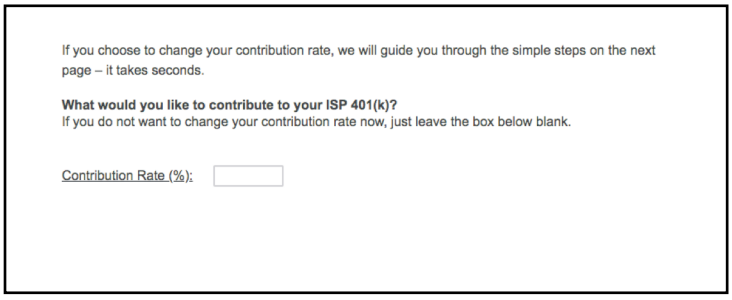
iii. Recommendation



iv. Match



v. Basic Savings Prompt



vi. Savings Prompt with $10

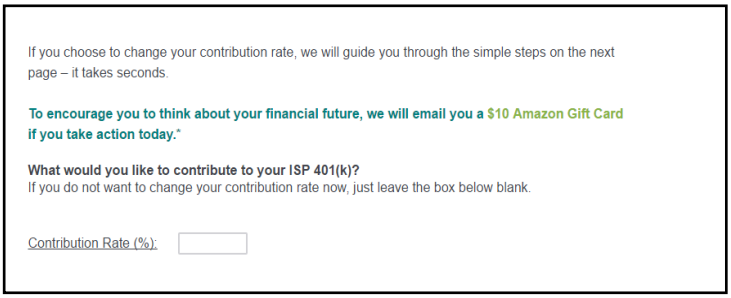
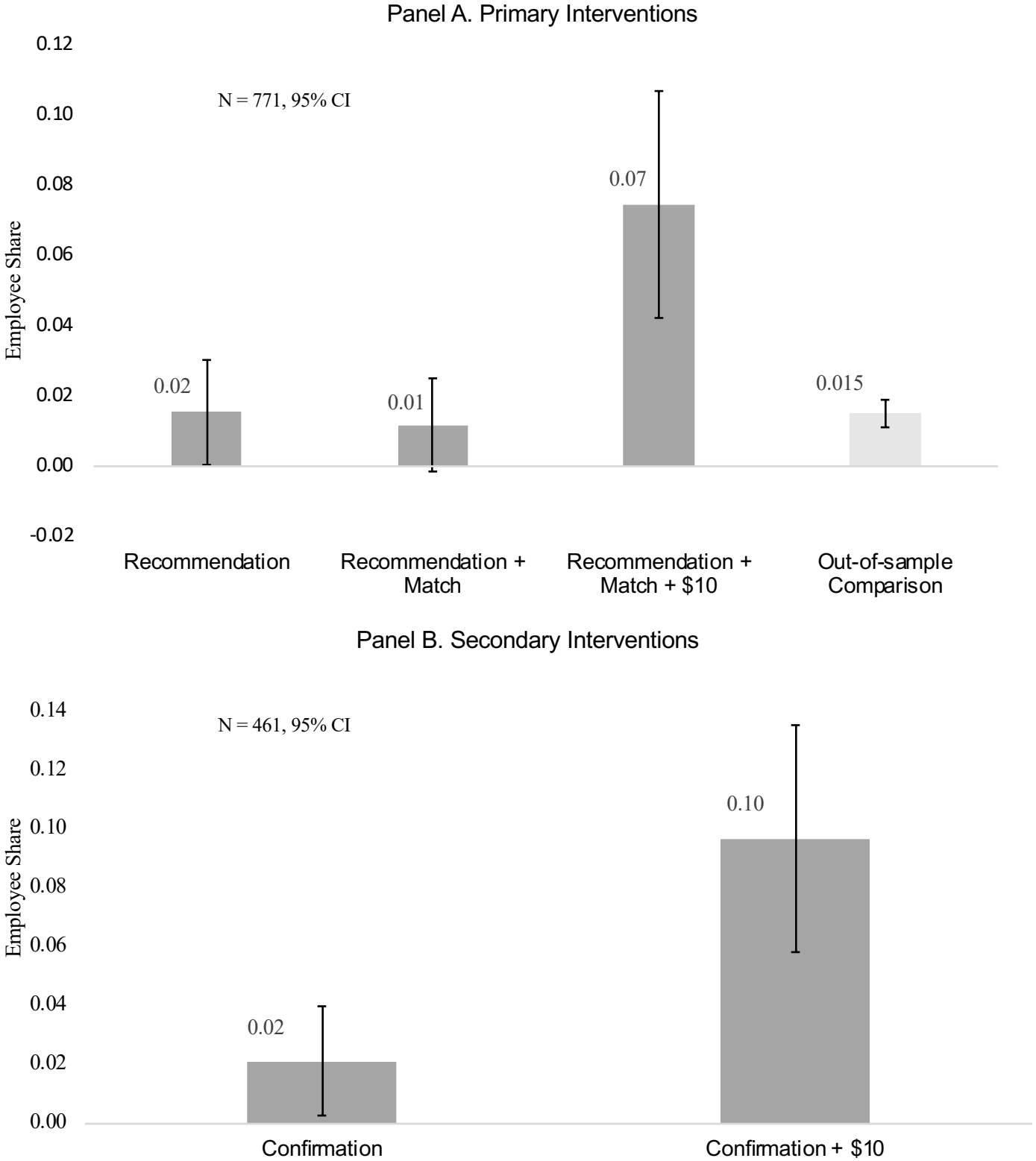
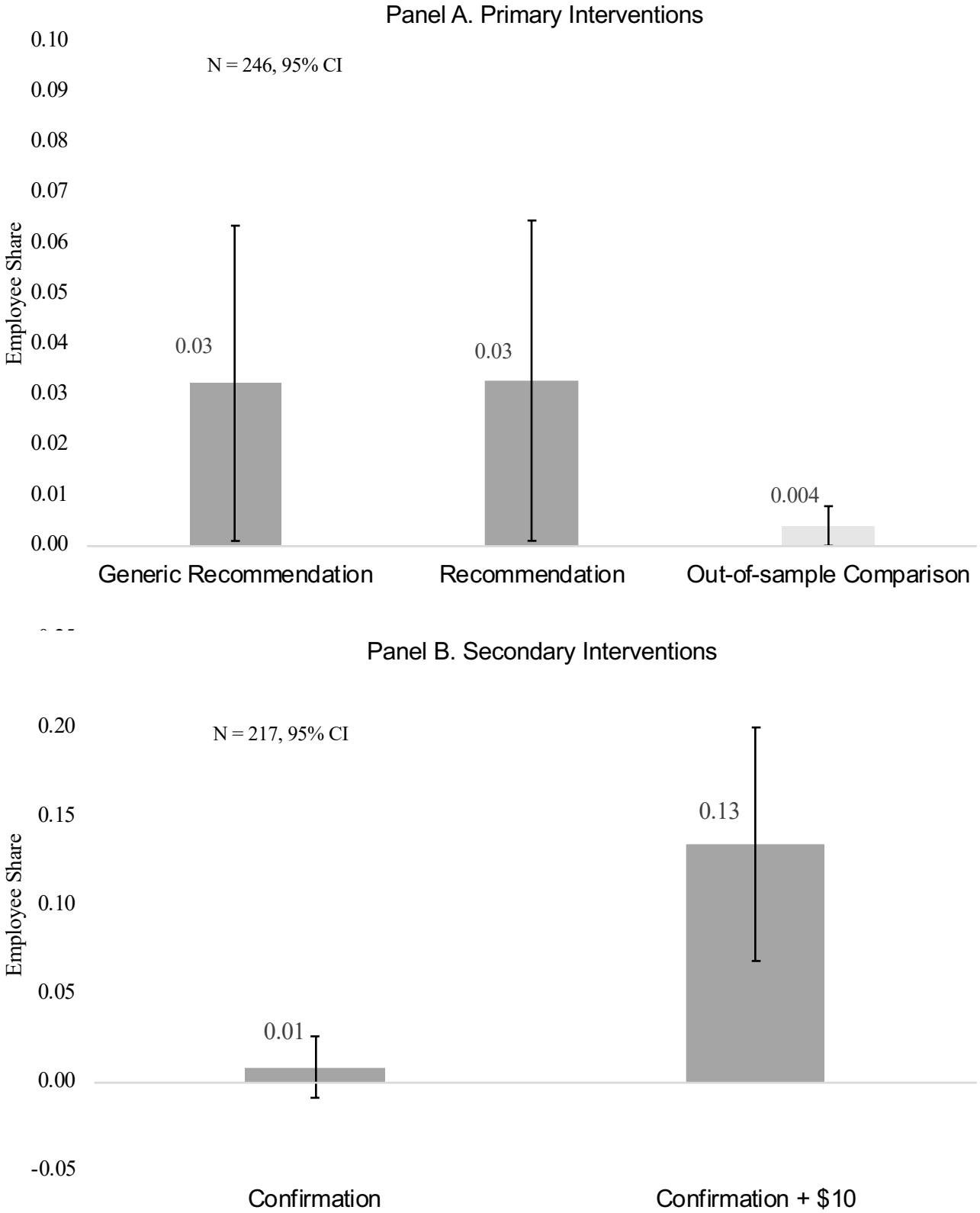


Figure 4.
Employee Share with Increased 401(k) Contribution at Next Pay Period, By Experimental Condition (Low Savings Arm)



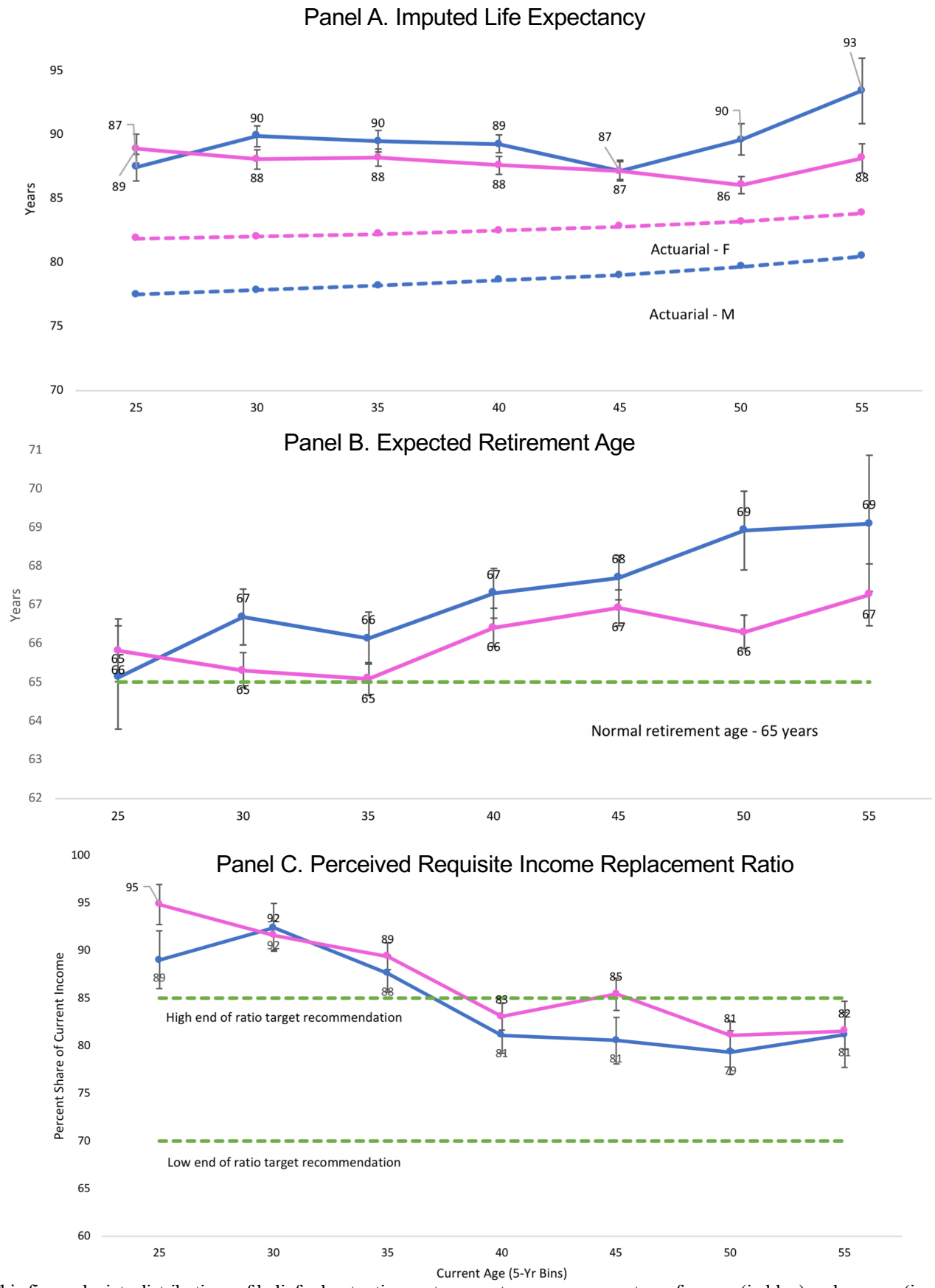
Notes. This figure depicts the share of employees that increased their contribution rate across experimental conditions for the primary intervention (Panel A) and for the secondary intervention (Panel B) for the samples of employees randomly assigned to one of the Low Savings Arm conditions at each intervention. Panel A also presents the share of employees increasing their contribution for the out-of-sample comparison group of invited non-respondents with comparable initial enrollment status to the Low Savings Arm, i.e., 401(k)-eligible employees who were not fully taking up the match at the time of the study. Employees are tagged as increasing their rate relative to pre-intervention enrollment status (July 11, 2016) based on enrollment at the next payday post-intervention (August 8, 2016). Error bars represent 95% confidence intervals based on robust standard errors.

Figure 5.
Employee Share with Increased 401(k) Contribution at Next Pay Period by Experimental Condition (Moderate Savings Arm)



Notes. This figure depicts the share of employees that increased contribution rates across experimental conditions for the primary intervention (Panel A) and for the secondary intervention (Panel B) for employees randomly assigned to one of the Moderate Savings Arm conditions at each intervention. Panel A also presents the share of employees increasing their contribution for the out-of-sample comparison group of invited non-respondents with comparable initial enrollment status to the Moderate Savings Arm, i.e., 401(k)-eligible employees who were fully taking up the match at the time of the study. Employees are tagged as increasing their rate relative to pre-intervention enrollment status (July 11, 2016) based on enrollment at the next payday post-intervention (August 8, 2016). Error bars represent 95% confidence intervals based on robust standard errors.

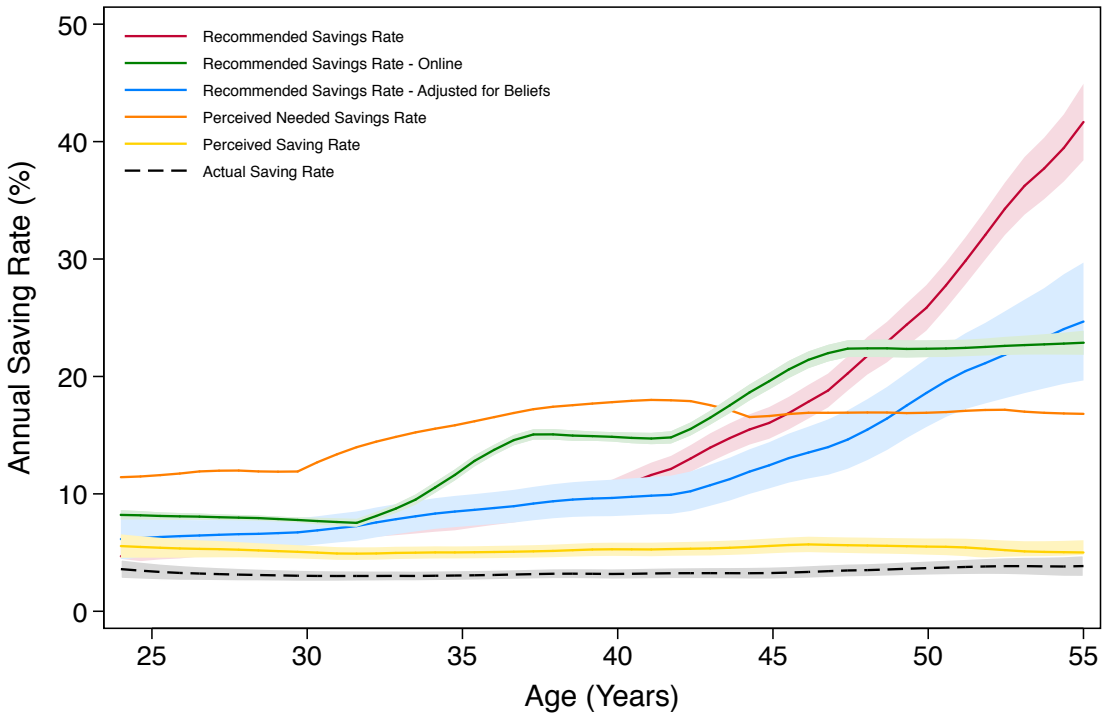
Figure 6.
Employee Beliefs Regarding Retirement



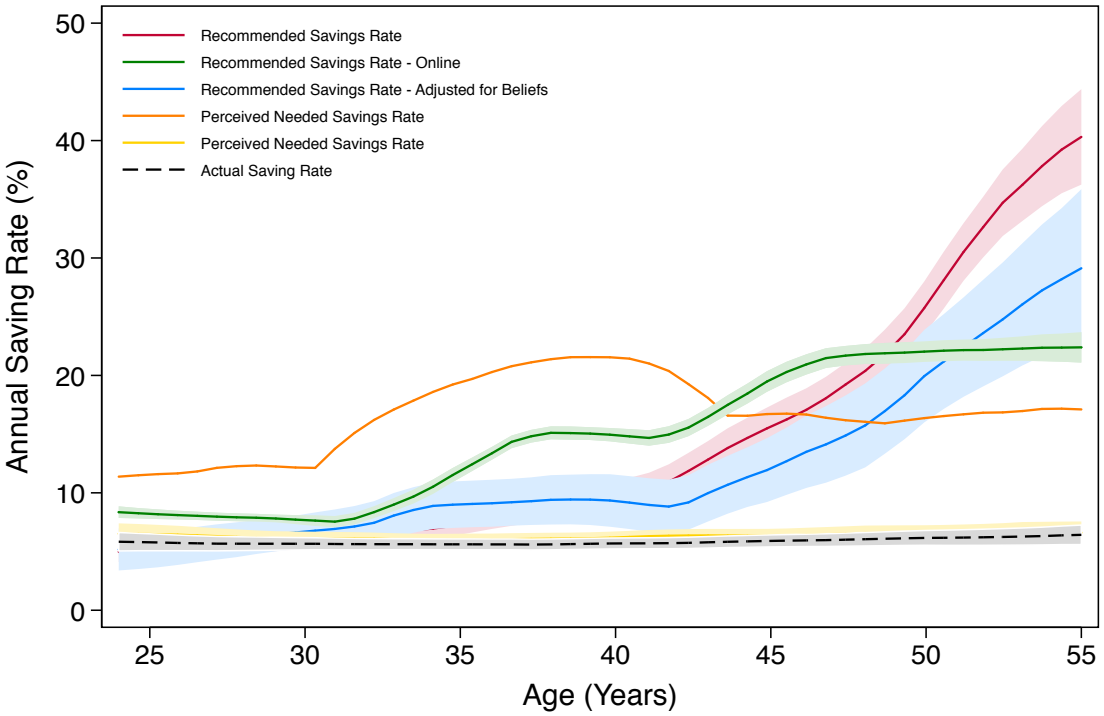
Notes. This figure depicts distributions of beliefs about retirement parameters across current age for men (in blue) and women (in pink). Panel A shows employees' imputed life expectancy with the benchmark of actuarial predictions by age and gender. Life expectancy is imputed by adding expected retirement age and anticipated retirement length. Panel B shows expected retirement age with the benchmark of the current median retirement age of 63 the dashed line. Panel C shows employees' requisite income replacement ratios with the benchmarks demarcating a range of minimum income replacement ratios typically suggested in the personal finance industry from 70% of 85%.

Figure 7.
Recommended Savings Rates Adjusted by Retirement Beliefs and Actual Saving

Panel A. Savings Rates Inclusive of 401k Non-Participants



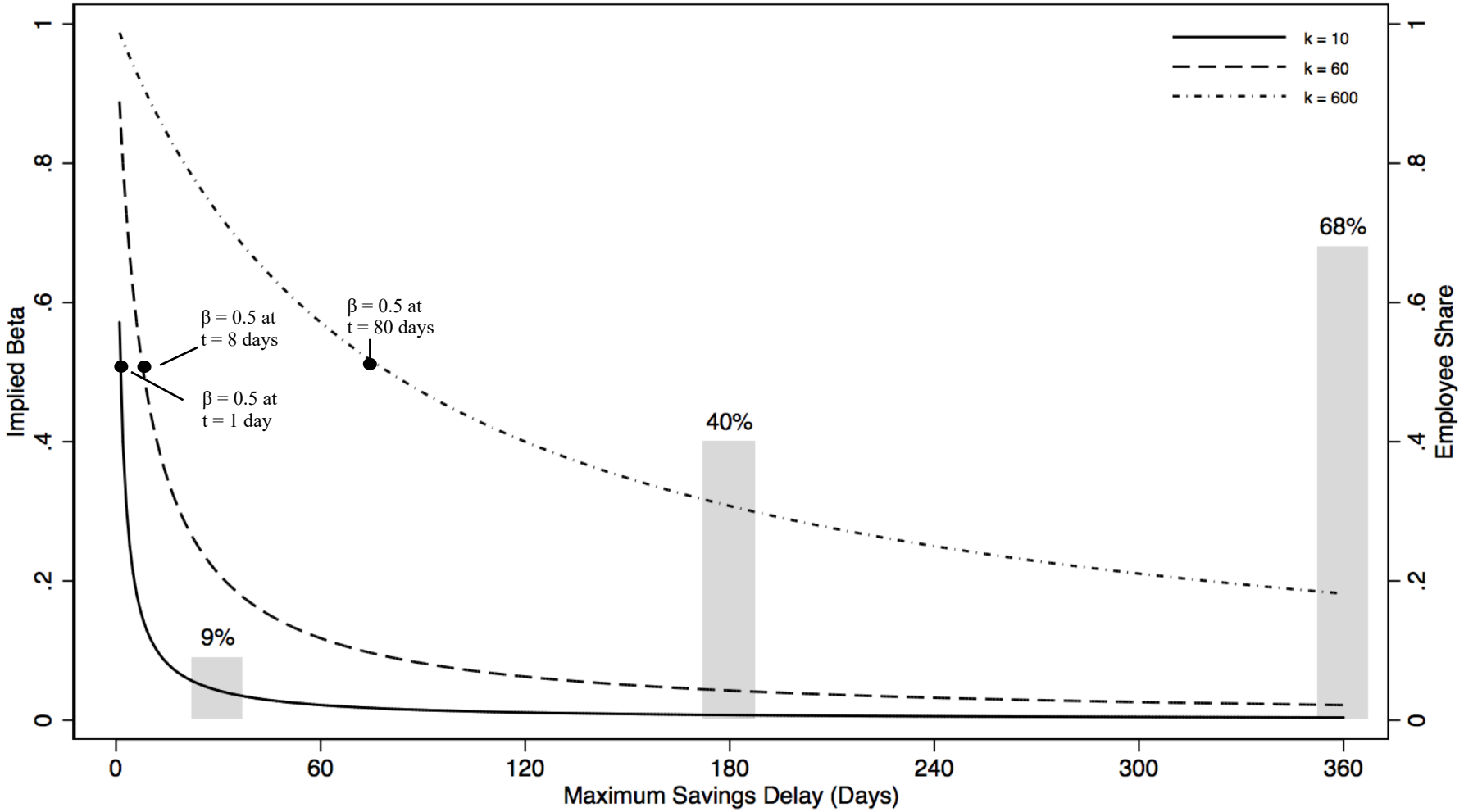
Panel B. Savings Rates for 401k Participants Only



Notes. This figure depicts local-mean smoothers of employees' recommended savings rates, self-reported savings rates, and actual savings rates observed in administrative data across current age. Employees' perceptions of the required rate they should be saving for a financially comfortable retirement have much larger variance than the other measures and a local mean smoother is plotted in orange without a confidence interval to avoid obscuring other measures. The local-mean smoother is calculated at every 50 points and is displayed with a 95% confidence interval. Panel A presents this information for the entire survey sample, inclusive of 401(k) non-participants. Panel B presents this information for the subsample of 401(k) participants. For the assumptions used to calculate the three different recommendations, see Appendix Table A3, Inventory of Saving Recommendation Rates and Parameter Changes.

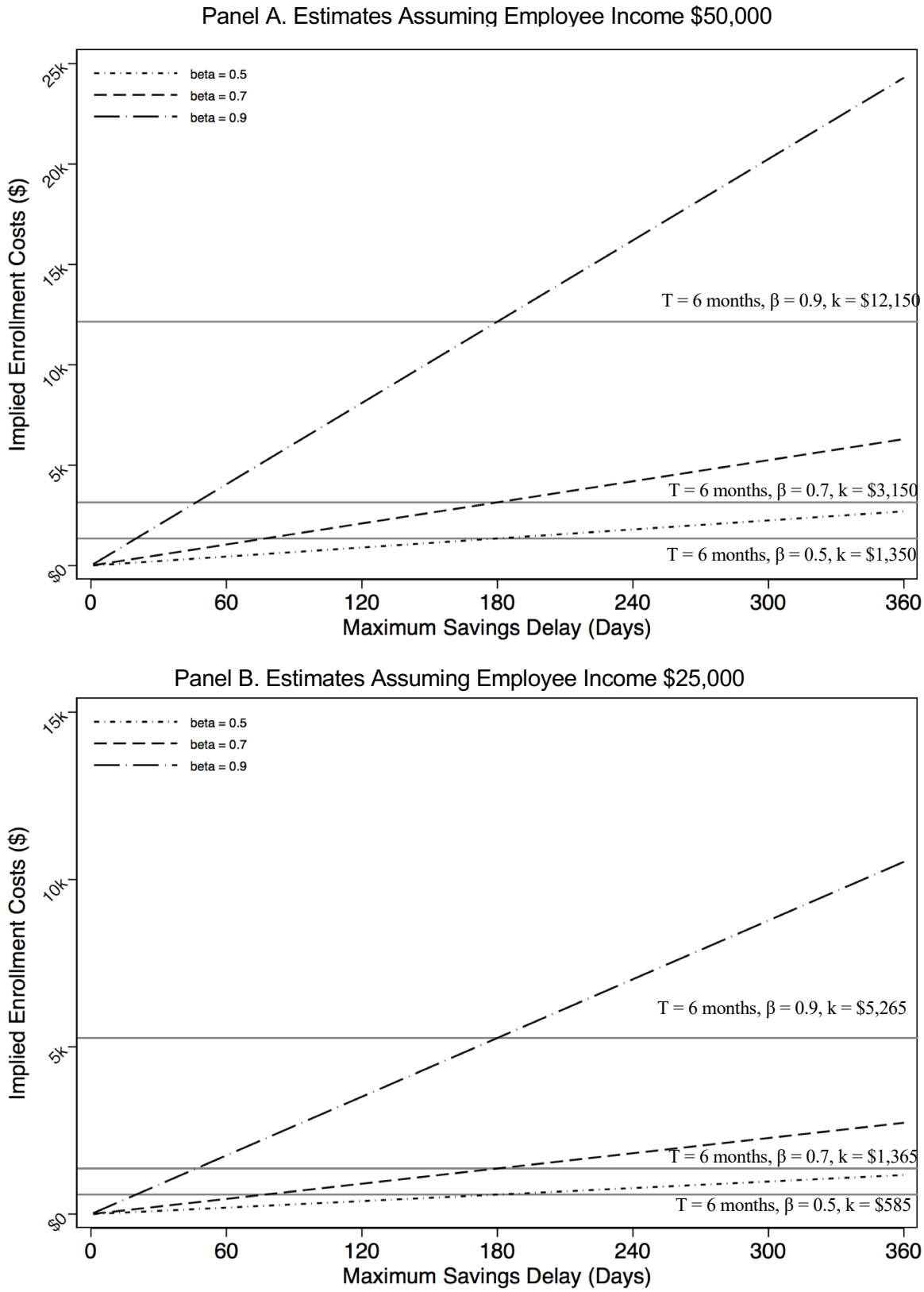
Figure 8.

Implied Beta as a Function of Maximal Delay for Present-Biased Employees



Notes: This figure depicts the implied beta parameter as a function of the maximum delay in match take-up for a sophisticated present-biased employee as specified by the model in the text. Estimates are separately displayed for enrollment costs of k = 10, 60, and 600 to show the effect of a range of possible costs associated with enrollment, including effort costs and potentially psychological costs. All estimates assume an annual salary of $50,000 with a 25% marginal tax rate at the point of contribution and withdrawal. The cumulative empirical distribution (light gray bars) reflects the expected timing of participation, rather than match take-up, among employees self-reporting as 401(k) non-participants prior to the experimental intervention. Employees are tagged as intending to participate at the earliest time-horizon—among the 1 month, 6 month, or 12 month options—for which they report being "moderately" or "very" likely to participate.

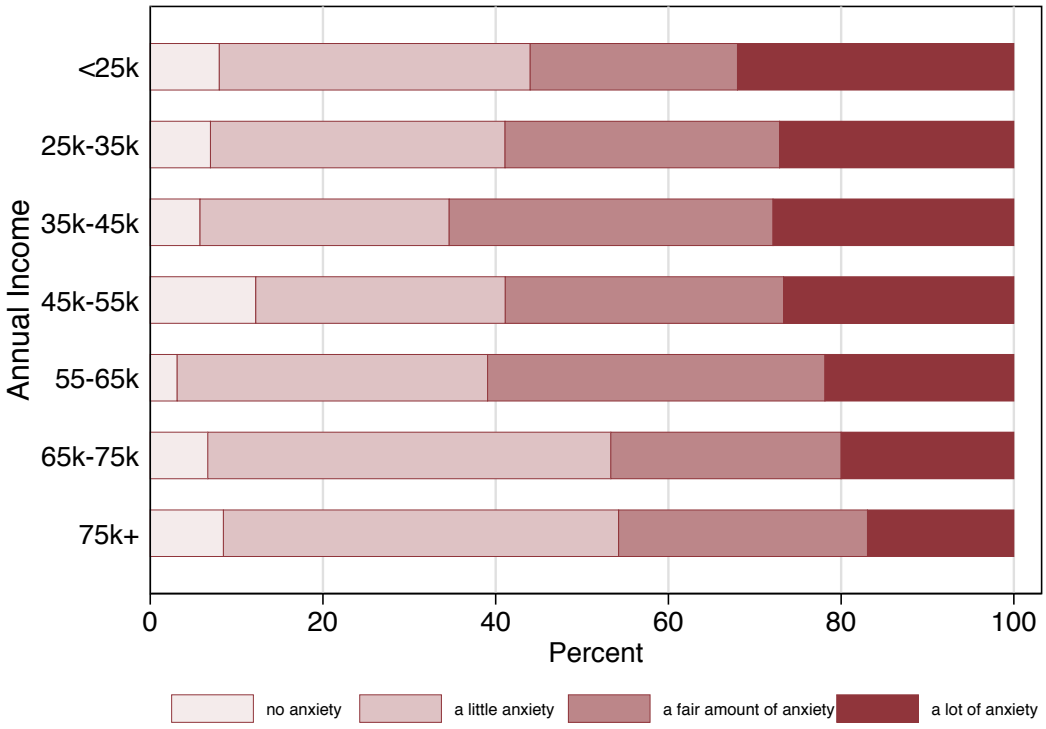
Figure 9.
Enrollment Costs Implied by Savings Delay for Sophisticated Present-Biased Employees



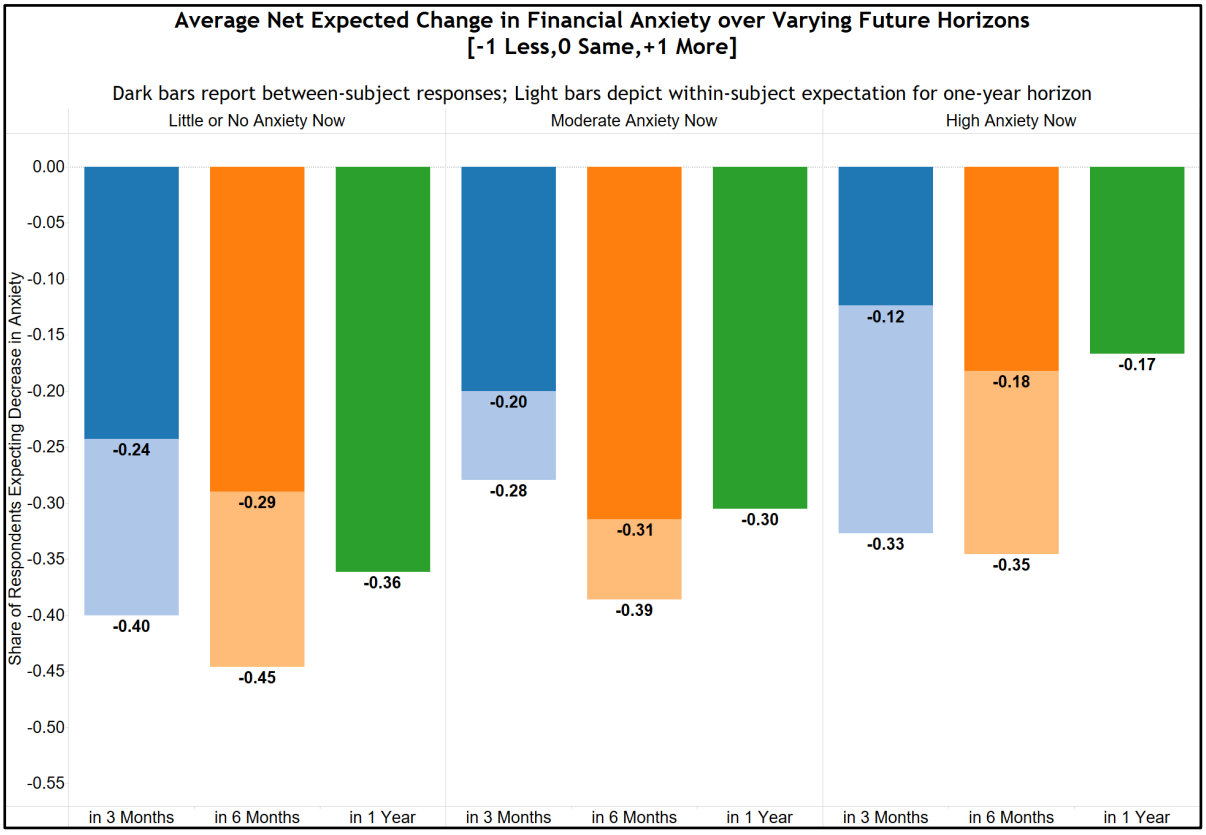
Notes. This figure depicts enrollment costs, *k*, as a function of the maximum delay in match take-up for a sophisticated present-biased employee as specified by the model in the text. Estimates are separately displayed for β = 0.9, β = 0.7, and β = 0.5 and the gray lines denote the enrollment costs implied by a six-month savings horizon for each beta. Panel A assumes a $50,000 employee salary with a 25% marginal tax rate at the point of contribution and withdrawal, Panel B assumes a $25,000 employee salary with a 15% (contribution) and 25% (withdrawal) marginal tax.

Figure 10.
Financial Anxiety and Savings Behavior

Panel A. Self-Reported Financial Anxiety Among Employees



Panel B. Current and Future Assessments of Financial Anxiety (Pilot Study)



Notes. Panel A depicts distributions of reported financial anxiety by income category for employee respondents in the main field study sample. Panel B depicts reported current and anticipated future levels of anxiety from a pilot sample of adults recruited on Amazon Mechanical Turk (N = 1000)

Table 1.
Summary of Demographic and Saving Detail for Employees

	All Sample		401(k) Non-Participants		401(k) Participants		Mean Diff
	Mean	Std. Err.	Mean	Std. Err.	Mean	Std. Err.	(t-test, p-value)
Panel A. Survey Invited Sample (July 2016)							
Employee Characteristics							
N =	4,719	-	2,293	-	2,426	-	-
Male [1,0]	0.35	0.01	0.36	0.01	0.34	0.01	0.35
Age [Yrs]	38.7	0.12	38.5	0.17	39.0	0.17	0.03
Tenure [Yrs]	8.2	0.15	7.7	0.14	8.6	0.15	0.00
Imputed income [$ thousands]	50.9	0.32	48.9	0.45	52.8	0.46	0.00
401(k) Savings Behavior							
Participation [1,0]	0.51	0.01	0.0	-	1.0	-	-
Contribution Rate [% annual pay]	1.6	0.03	0.0	-	3.2	0.04	-
Savings Rate (inclusive of match) [est. % annual pay]	3.1	0.06	0.0	-	6.1	0.07	-
Full Match Take-Up [1,0]	0.23	0.01	0.0	-	0.45	0.01	-
Panel B. Survey Participants (July 2016)							
Employee Characteristics							
N =	1,332	-	568		764	-	-
Male [1,0]	0.33	0.01	0.35	0.02	0.32	0.02	0.29
Age [Yrs]	39.5	0.23	39.0	0.34	39.8	0.30	0.09
Tenure [Yrs]	8.9	0.20	8.3	0.29	9.3	0.28	0.01
Imputed income [$ thousands]	52.4	0.61	50.1	0.89	54.1	0.82	0.00
Married [1,0]	0.58	0.01	0.55	0.02	0.60	0.02	0.04
Any Children [1,0]	0.69	0.01	0.70	0.02	0.67	0.02	0.23
Non-white [1,0]	0.29	0.01	0.33	0.02	0.27	0.02	0.01
College Degree [1, 0]	0.53	0.01	0.52	0.02	0.54	0.02	0.35
Accumulated Savings							
Less than $10k [1,0]	0.43	0.01	0.54	0.02	0.36	0.02	0.00
$10k - $75k [1,0]	0.39	0.01	0.35	0.02	0.41	0.02	0.04
$75k or more [1,0]	0.18	0.01	0.11	0.01	0.23	0.02	0.00
Emergency Liquidity (N = 227)							
Financial Hardship Event in Last Year [1,0]	0.42	0.03	0.41	0.05	0.44	0.04	0.67
Liquid Savings to Cover 3 Months of Expense [1,0]	0.68	0.03	0.75	0.04	0.63	0.04	0.06
401(k) Savings Behavior							
Participation [1,0]	0.57	0.01	0.0	-	1.0	-	-
Contribution Rate [% annual pay]	1.9	0.06	0.0	-	3.3	0.1	-
Self-Reported Contribution Rate [% annual pay]	2.5	0.06	1.3	0.1	3.4	0.1	-
Savings Rate (inclusive of match) [est. % annual pay]	3.7	0.12	0.0	-	6.5	0.13	-
Full Match Take-Up [1,0]	0.27	0.01	0.0	-	0.48	0.02	-

Note: This table summarizes available demographic and savings detail for two analytic samples. Panel A summarizes detail for employees invited to participate in the survey/field study as of July 2016, and Panel B summarizes detail for employees who participated in the survey and reported their age, accumulated savings, and current 401(k) enrollment status. All employees from this group where we calculated a recommendation exceeding their self-reported contribution rate were assigned to an experimental treatment. Imputed incomes are calculated using midpoints for categories of income deciles within invited sample and these imputations were used to calculate eligibility for the minimum match and corresponding estimates of the savings

Table 2.
Candidate Frictions and Empirical Savings Puzzles

Candidate Friction	Illustrative Research	Can Friction Account for Savings Puzzle?			
		Undersaving	Match Neglect	Default Efficacy	Default Inertia
Panel A. Low Retirement Literacy		**Yes**	**No**	**Yes**	**Yes**
Underestimation of Needed Savings through EGB	(Stango & Zinman 2009, Goda et al. 2015)	Yes	No	No	No
Overconfidence about Retirement Earnings	(EBRI 2014)	Yes	No	No	No
General Deficit in Financial Literacy	(Lusardi & Mitchell 2006, 2007; Choi et al. 2004)	Yes	No	Yes	Yes
Panel B. Enrollment Complexity		**Yes**	**Yes**	**Yes**	**Yes**
Psychological Costs of Complexity	(Beshears et al. 2013, Benzarti 2017)	Yes	Yes	Yes	Yes
Defaults as Guidance	(Choi et al. 2007)	No	No	Yes	Yes
Panel C. Present Bias					
Present Bias - Discounting	(Madrian & Shea 2001; Brown et al. 2016; Benartzi & Thaler 2004; Goda et al. 2015)	**Yes**	**Yes**	**Yes**	**Yes**
Panel D. Plan Confusion		**No**	**Yes**	**Yes**	**Yes**
Low awareness of plan and/or default	(Choi et al. 2011, Agnew et al. 2012)	No	No	Yes	Yes
Inattention to matching incentives	(Choi et al. 2011)	No	Yes	Yes	Yes

Note: This table summarizes mechanisms for undersaving suggested by the literature into four general categories, reports key citations from the literature corresponding to each mechanism, and speculates as to whether the mechanism is able to account for each of four empirical savings puzzles discussed in the paper: undersaving, match neglect, default efficacy, and default inertia.

Table 3.
Candidate Mechanisms as Inferred from Survey Measures and Employee Savings

	All Sample		401(k) Non-Participants Contribution = 0%		401(k) Participants All Participants		401(k) Participants Contribution < 4%		401(k) Participants Contribution = 4%+		Mean Difference	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Mean (5)	SD (6)	Mean (7)	SD (8)	Mean (9)	SD (10)	Part (3) - (5)	Match (3 7) - (9)
Panel A. Low Retirement Literacy												
1. Retirement Beliefs (N = 1332)												
Age of Retirement [Years]	66.4	6.15	67.0	6.99	65.9	5.43	66.6	6.33	65.7	5.61	1.02**	0.94*
Imputed Life Expectancy [Years]	88.2	8.44	88.3	8.88	88.1	8.11	87.4	8.06	88.9	8.10	0.15	-1.0
Income Replacement Ratio [0 to 100]	86.2	19.02	88.0	19.72	84.9	18.39	85.9	18.36	83.8	18.38	3.13**	3.37**
Perception of Required Savings [Annual %]	13.9	15.55	15.0	16.76	13.1	14.56	12.7	15.28	13.5	13.76	1.87*	0.52
Perception of Required Savings Below Recommendation [1,0]	0.66	0.47	0.63	0.48	0.68	0.47	0.71	0.46	0.65	0.48	-0.05	0.01
Perception of Required Savings Above Current Saving [1,0]	0.71	0.45	0.84	0.37	0.61	0.49	0.75	0.44	0.48	0.50	0.23***	0.33***
2. Financial Literacy [N = 316]												
Interest Question Correct [1,0]	0.52	0.50	0.56	0.50	0.47	0.50	0.38	0.49	0.57	0.50	0.09	-0.07
Inflation Question Correct [1,0]	0.61	0.49	0.52	0.50	0.70	0.46	0.66	0.48	0.74	0.44	-0.18***	-0.17**
2-Item Financial Literacy Score [0-2]	1.14	0.73	1.21	0.77	1.11	0.69	1.02	0.68	1.18	0.66	0.10	-0.15
Rate of Return Implied by Investment Prediction (%)	5.8	15.7	5.8	15.7	5.8	13.9	6.7	13.9	4.8	13.8	0.0	1.9
Panel B. Plan Confusion												
3. Confusion about Plan Details (N = 1321 to 1346)												
Eligibility Awareness [1,0]	0.98	0.13	0.97	0.17	0.99	0.10	0.99	0.10	0.99	0.10	-0.02*	-0.01
Match Rate Correct [1,0]	0.70	0.46	0.63	0.48	0.76	0.43	0.69	0.46	0.82	0.38	-0.13***	-0.17***
Match Rate Underestimated [1,0]	0.20	0.40	0.27	0.44	0.15	0.35	0.18	0.38	0.11	0.31	0.12***	0.12***
4. Confusion about Plan Status (N = 1302 to 1319)												
Mistaken Belief in Enrollment [1,0]	0.15	0.36	0.36	0.48	0.00	0.00	0.00	0.00	0.00	0.00	0.36***	0.21***
Mistaken Belief in Match Take-Up [1,0]	0.13	0.34	0.25	0.43	0.05	0.21	0.09	0.29	0.00	0.00	0.20***	0.19***
Panel C. Enrollment Complexity												
5. Estimated Enrollment/Adjustment Time (N = 577)												
Enrollment/Adjustment Requires Minutes [1,0]	0.77	0.32	0.74	0.44	0.79	0.41	0.82	0.38	0.77	0.42	-0.05	0.01
Enrollment/Adjustment Requires Hours [1,0]	0.11	0.32	0.11	0.31	0.12	0.32	0.10	0.31	0.13	0.33	-0.01	-0.02
Enrollment/Adjustment Requires More than Hours [1,0]	0.11	0.42	0.15	0.35	0.09	0.29	0.07	0.26	0.11	0.31	0.05*	0.01
Panel D. Present Bias												
6. Present Bias Indicator from Multiple Price List (N = 373)												
Preference Reversal in MPL Discounting Measure [1,0]	0.10	0.30	0.10	0.29	0.10	0.30	0.11	0.31	0.09	0.28	0.00	0.01
7. Intention to Increase Savings Soon (N = 577)												
Likelihood of Savings Increase in 3 Months ≥ 50% [1,0]	0.21	0.41	0.25	0.44	0.18	0.39	0.24	0.43	0.13	0.34	0.07*	0.12**
Likelihood of Savings Increase in 3 Months ≥ 75% [1,0]	0.06	0.24	0.07	0.25	0.05	0.22	0.08	0.27	0.03	0.18	0.02	0.05*

Note: This table summarizes correlational evidence from the employee survey on the four categories of candidate explanations of undersaving and match neglect laid out in Table 2. Each panel displays the measures related to a candidate explanation by row. From left to right, each row reports summary statistics for the specified measure in the full employee survey sample and the subsets of 401(k) non-participants, 401(k) participants, 401(k) participants partially taking up the employer match, and 401(k) participants fully taking up the employer match. The final two rows display the differences in the measure by 401(k) participation and full match take-up status with stars indicating the significance level of 2-sided t-tests of equality. *** p<0.01, ** p<0.05, * p<0.1

Table 4.
Experimental Interventions and Predicted Outcomes - Low Savings Arm

	Increase in Contribution Rate [1,0]			Change to Full Match Take-Up [1,0]		
Experimental Treatment	All < 4%	Non-Savers = 0%	Low Savers 1, 2, or 3%	All < 4%	Non-Savers = 0%	Low Savers 1, 2, or 3%
Panel A. Primary Interventions						
Recommendation	0.02	0.01	0.02	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Recommendation + Match	0.01	0.01	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Recommendation + Match + $10	0.07	0.08	0.07	0.04	0.03	0.04
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)
N	771	419	352	771	419	352
Out-of-Sample Comparison (Non-Respondents to Survey)	0.02	0.01	0.02	0.01	0.01	0.01
	(0.002)	(0.003)	(0.004)	(0.002)	(0.003)	(0.004)
F-Tests of Coefficient Equality (p-value)						
Recommendation v. Out-of-Sample Comparison	0.90	0.95	0.92	0.12	0.25	0.28
Match (with Recommendation)	0.70	0.98	0.60	0.45	0.58	0.60
$10 (with Recommendation + Match)	0.00	0.01	0.02	0.00	0.10	0.04
Panel B. Secondary Interventions						
Confirmation	0.02	0.02	0.02	0.01	0.02	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Confirmation + $10	0.10	0.10	0.09	0.04	0.05	0.03
	(0.02)	(0.03)	(0.03)	(0.01)	(0.02)	(0.02)
N	461	239	222	461	239	222
F-Test of Coefficient Equality (p-value)						
$10 (with Confirmation)	0.00	0.02	0.01	0.04	0.10	0.22

Note: This table presents the main effects of our experimental interventions on savings outcomes for the Low Savings Arm. The estimates in each column come from a series of OLS regressions of our two main savings outcomes—(1) whether an employee increased their 401(k) contribution through the next pay period, (2) whether an employee went from not fully taking up the match to fully taking up the match before the next pay period—for the sample of employees described in each column header. Panel A summarizes regressions of each outcome on indicators for the three primary interventions (Recommendation, Recommendation + Match, and Recommendation + Match + $10) with a suppressed constant. For comparison, below these estimates we report the average of the outcome variable for the out-of-sample comparison group of employees who received no intervention encouraging savings. The bottom three rows of this panel present the results of F-tests of coefficient equality isolating the effects of the recommendation (relative to the out-of-sample comparison group of non-respondents to the survey), match information (comparing responses in the Recommendation vs. Recommendation + Match conditions), and $10 offer (comparing responses in the Recommendation + Match vs. Recommendation + Match + $10 conditions). Panel B summarizes analogous regressions of each outcome on indicators for the two randomly assigned conditions in the secondary intervention (Confirmation, Confirmation + $10). The bottom row of this panel presents the results of an F-test isolating the effect of the $10 offer (comparing the response by employees in the Confirmation + $10 vs. those in the Confirmation condition).

Table 5.

Experimental Interventions and Predicted Outcomes - Moderate Savings Arm

	Increase in Contribution Rate [1,0]		
Experimental Treatment	All 4% or Over	At Match = 4%	Above Match Over 4%
Panel A. Primary Interventions			
Generic Recommendation	0.03	0.03	0.04
	(0.02)	(0.02)	(0.03)
Recommendation	0.03	0.03	0.04
	(0.02)	(0.02)	(0.03)
N	246	153	93
Out-of-Sample Comparison (Non-Respondents to Survey)	0.004	0.002	0.007
	(0.002)	(0.002)	(0.005)
F-Tests of Coefficient Equality (p-value)			
Generic Recommendation v. Out-of-Sample Comparison	0.08	0.19	0.23
Generic vs. Specific Recommendation	0.98	0.95	0.95
Panel B. Secondary Interventions			
Confirmation	0.01	0.00	0.02
	(0.01)	(0.03)	(0.02)
Confirmation + $10	0.13	0.12	0.16
	(0.03)	(0.04)	(0.06)
N	217	137	112
F-Tests of Coefficient Equality (p-value)			
$10 (with Confirmation)	0.00	0.00	0.03

Note: This table presents the main effects of our experimental interventions on savings outcomes for the Moderate Savings Arm. The estimates in each column come from a series of OLS regressions of our main savings outcome—whether an employee increased their 401(k) contribution through the next pay period—for the sample of employees described in each column header. Panel A summarizes regressions of each outcome on indicators for the two primary interventions (Guidance, Simplifying Guidance) with a suppressed constant. For comparison, below these estimates we report the average of the outcome variable for the same period of time for an out-of-sample comparison group of employees who received no intervention encouraging savings. The bottom row of this panel presents the results of F-tests isolating the effect of the generic recommendation (relative to the out-of-sample comparison group of non-respondents) and the specific recommendation (relative to the Generic Recommendation). Panel B summarizes analogous regressions for the same outcome on indicators for the two randomly assigned conditions in the second intervention (Confirmation, Confirmation + Immediate Gain). The bottom row of this panel presents the results of an F-test isolating the effect of the $10 offer (comparing the response by employees in the Confirmation + $10 vs. those in the Confirmation condition). Robust standard errors in parentheses.

Table 6.
Manipulation Check: Changes in Beliefs After Primary Savings Intervention

	Retirement Literacy		Enrollment Complexity
	Δ Share for whom perceived sufficient savings rate ≥ recommended rate	Δ Share "not at all" confident of retirement preparedness	Δ Share for whom perceived time-cost of enrollment = minutes
Panel A. Low Savers Arm			
Recommendation	0.26***	0.05***	0.03
	(0.04)	(0.02)	(0.01)
Recommendation + Match	0.21***	0.02	0.07***
	(0.04)	(0.01)	(0.02)
Recommendation + Match + $10	0.19***	0.03*	0.07***
	(0.03)	(0.02)	(0.03)
Base Rate Prior to Interventions	0.50	0.32	0.78
N	471	370	338
F-Tests of Equality (p-values)			
Recommendation + Match vs. Recommendation	0.36	0.32	0.11
Recommendation + Match + $10 vs. Recommendation + Match	0.73	0.68	0.96
Panel B. Moderate Savers Arm			
Generic Recommendation	0.10***	0.01	0.05**
	(0.03)	(0.01)	(0.02)
Recommendation	0.26***	0.03	0.07**
	(0.05)	(0.02)	(0.03)
Base Rate Prior to Interventions	0.40	0.19	0.77
N	165	165	165
F-Tests of Equality (p-values)			
Specific vs. Generic Recommendation	0.01	0.53	0.56

Note: This table summarizes regressions describing changes in beliefs after the primary savings intervention as a function of indicators for the three primary interventions with a suppressed constant. From Left to Right, the three columns display coefficients for regressions where the dependent variables are indicators for the change in the share of employees who (1) reported being "not at all confident..[that] you are preparing yourself for a financially secure retirement"; (2) reported a perceived necessary savings rate for retirement greater than or equal to the recommendation we calculated for them, and (3) reported that it would take "minutes" to change their 401k contribution rate, at the beginning of the survey and after the primary savings intervention, but before the secondary intervention. Below the regression coefficients, the row labeled "Base Rate Prior to Interventions" displays the share of employees endorsing this belief before interventions. The last two rows in each panel display the results of F-tests of coefficient equality for each experimental condition above. Panel A shows the results of regressions for the Low Savings arm and Panel B for the Moderate Savers arm. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.10.

Table 7.
Confusion about 401(k) Participation and Contribution inferred from Differences in Self-Reported and Administrative Data

	Employee Sample			
	All Sample	401(k) Non-Participants	401(k) Participants	Mean Diff
Panel A. Summary of Discrepencies in Self-Reports				
Inaccurate Self-Report [1,0]	0.27	0.36	0.21	0.15***
Self-Report Contribution Higher than Actual [1,0]	0.24	0.36	0.15	0.21***
Self-Report Contribution Lower than Actual [1,0]	0.04	0.00	0.06	-0.06***
Inaccurate Self-Report of Participation [1,0]	0.15	0.36	0.00	0.36***
Inaccurate Self-Report of Non-Participation [1,0]	<0.01	<0.01	0.00	<0.01
Inaccurate Self-Report of Full Match Take-Up [1,0]	0.13	0.25	0.05	0.20***
Inaccurate Self-Report of Lack of Full Match Take-Up [1,0]	<0.01	0.00	0.01	-0.01**
Average Self-Reported - Actual Rate (Conditioned on Inaccurate Report) [%]	2.51	3.76	0.95	2.81***
	(0.13)	(0.14)	(0.17)	
Total N	1319	559	760	

	401(k) Non-Participants	
Panel B. Employee Confusion after Adjusting for Inattention, Exaggeration	Inaccurately reporting participation	Inaccurately reporting full match take-up
Confused Share of Non-Participants	0.36	0.19
Confused Share of Non-Participants | Attention Screen	0.30	0.15
Confused Share of Non-Participants after Reclassifying Exaggeration (defined below)		
Reported highest savings rate on menu (≥ 10%)	0.30	0.15
Reported highest income category on menu ($100k+)	0.35	0.18
Reported no financial anxiety	0.35	0.18
Reported highest level of education (Graduate School)	0.32	0.16
Reported highest level of accumulated savings ($100k+)	0.32	0.16
Over-reported increase in contribution after intervention	0.35	0.18
Any of the above	0.25	0.10
Confused Share of Non-Participants after Reclassifying Exaggeration | Attention Screen	0.22	0.07

Note: This table summarizes employee confusion about 401(k) enrollment status. Panel A shows the average outcome for a series of measures capturing confusion among all employees, 401(k) non-participants, and 401(k) participants. The final column of this panel presents the results of F-tests of equal prevalence of employee confusion among non-participants and participants. Panel B shows adjustments to conservatively rule out careless errors resulting from inattention to the task. Panel C shows analogous adjustments to conservatively rule out likely exaggerators from the share of employees misreporting by adjusting the empirical shares of inaccurate responses to exclude employees who gave other responses consistent with exaggeration. The bottom row shows the share of employees misreporting after adjusting for all measures of exaggeration after also excluding those characterized by task inattention. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.10.

Table 8.
Synthesis of Survey and Field Evidence on Psychological Frictions and Contribution Adjustment

		Cross-Sectional Evidence	Average Experimental Treatment Effect			
Psychological Friction	Baseline Incidence	E(ΔEnrollment | Friction)	Intervention	Low Bias	High Bias	Low-High Diff (p-value)
1. Retirement Literacy						
Biased Belief in Sufficiency of Contribution [1,0]	0.29	**-0.23**	Specific Guidance	0.01	0.00	0.60
Low Financial Literacy [1,0]	0.39		Specific Guidance	0.00	0.03	0.32
2. Plan Confusion						
Underestimation of Plan Match [1,0]	0.20	**-0.12**	Match Clarification	0.01	**0.03**	0.50
Spurious Belief in Participation [1,0]	0.15	**-0.36**	--	--	--	--
3. Enrollment Complexity						
Perceived Enrollment/Adjustment Time > Minutes [1,0]	0.23	-0.05	Generic Guidance	**0.15**	**0.00**	0.32
4. Present-Focus						
Present-Bias Inferred from MPL [1,0]	0.10	0.00	Reward	0.02	**0.33**	0.11
Near-Term Savings Intent [1,0]	0.21	-0.07	Reward	0.03	**0.23**	0.01