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

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Abstract

Economists have advanced several psychological frictions to explain why many 401(k)-eligible employees save insufficiently for retirement despite often generous matching incentives. We investigate four candidate frictions as potential explanations through a field experiment randomizing 1,137 low-saving employees at a large US firm to information- or incentive-based treatments embedded in 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*—many employees underestimate retirement savings needs and exhibit low financial literacy—and its correlation with under-saving, but find that providing personalized recommendations does not increase contributions, even among employees with the greatest literacy deficits. (2) In an analysis of *plan confusion*, we estimate that at least a quarter of 401(k) non-participants mistakenly believed they were enrolled—and these employees enrolled at high rates upon observing their actual enrollment status. (3) We find no evidence that *enrollment complexity* impedes savings—few employees perceive high administrative enrollment costs and simplifying the process did not increase savings. (4) Last, we present new direct evidence implicating *present-focus* as a cause of low plan contributions by documenting that a significant share of employees increase contributions in response to a small reward (a $10 Amazon gift card) but not to information highlighting the much larger, but delayed, plan match. Calibrations suggest that widely-used beta-delta models of present bias cannot explain the observed behavior and beliefs of employees. We propose a novel model of financial anxiety and delayed optimism that offers a plausible explanation for our findings—and possibly other retirement savings puzzles—and provides a psychological rationale for encouraging long-run savings by linking existing 401(k) accounts to a more liquid "Serenity Account" intended to address proximate financial anxiety.

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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, many employees appear to save too little for retirement despite access to tax-advantaged 401(k) plans (GAO 2017), express the intent to save in the near future but fail to follow through (Laibson 2015), and do not take full advantage of the often generous matching incentives associated with their plan (Madrian 2013). Equally puzzling for the standard economic model, employees do seem to respond to non-economic features of savings plans such as the presence of automatic enrollment (Madrian and Shea 2001) or variation in the psychological design of the digital enrollment interface (Bhargava et al. 2018).[2]

Behavioral economists have advanced several potential departures from the standard economic framework to explain these empirical puzzles.[3] Four of these departures, each involving a distinct psychological friction, have generated particular interest from the literature. The first friction, which we refer to as *retirement literacy*, implies that employees save too little due to low financial literacy or numeracy (e.g., Lusardi & Mitchell 2007, 2011), information processing errors such as exponential-growth bias (e.g., Stango and Zinman 2009; Goda et al. 2015; Levy and Tasoff, 2016), or otherwise inaccurate beliefs about the timing, length, and cost of retirement. An extensive literature has documented deficits in retirement literacy (see Hastings et al. 2013 for review) and correlations between these deficits and savings and/or retirement planning (Lusardi, 2004; Lusardi and Mitchell, 2007, 2011; Stango and Zinman 2009; Hung et al., 2009; Van Rooij et al., 2012; Levy and Tasoff, 2016). A second friction, *plan confusion*, refers to the possibility that under-saving might arise from an employee's confusion about their benefit plan details like the default contribution rate, the generosity of the plan match, or benefit eligibility. While less evidence exists on such confusion in the context of 401(k) plans, studies have cited confusion as a barrier to take-up across a range of benefit programs (Daponte et al. 1999, Bartlett et al. 2004, Bhargava and Manoli 2015, Chetty et al. 2013). A third friction suggests that perceptions of *enrollment complexity* could cause employees to delay or avoid 401(k) plan engagement due to behavioral considerations such as potentially large "hassle costs" associated with enrollment (e.g., Anderson 2003, Beshears et al. 2013, Bertrand et al. 2004).

[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] Research has also documented the sensitivity of savings to other non-standard factors such as plan complexity (e.g., Beshears et al. 2013), auto-escalation (Thaler and Benartzi 2004), or the framing of incentives (Duflo et al. 2006; Choi et al. 2017).

[3] There are potential explanations for non-saving, even in the context of generous matching incentives, consistent with the standard economic model for which there is arguably less empirical support. Specifically, employees might delay savings due to extreme preferences for consuming more in the present than in retirement, may perceive the costs of enrolling to be extremely high and expect such costs to sharply diminish in the future, or may face very high costs of saving due to liquidity constraints.

A final friction engages the possibility that seemingly irrational savings behavior may emerge from the *present focus* of employees who privilege immediate, relative to delayed, flows of consumption utility.[4] 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 commonly explain delayed enrollment through a model of present bias in which employees with beta-delta (quasi-hyperbolic) preferences differentially discount all delayed benefits relative to immediate costs of enrollment (e.g., Laibson 1997, 1998; Repetto and Tobacman, 1998; O'Donoghue and Rabin 1999a; Diamond and Koszegi, 2003; DellaVigna 2018). While studies have shown correlations in measures of present bias with savings (Goda et al. 2015; Brown and Previtero 2016), perhaps the most direct evidence implicating its role in savings comes from studies in developing countries documenting demand for commitment devices (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 experimental introduction of a savings default.[5]

Despite the regularity with which these frictions are cited, and with the arguable exception of enrollment complexity (Choi et al. 2009, Beshears et al. 2013), the evidence on the causal relationship between these frictions and the savings of US employees remains scarce. Indeed, prominent reviews of the literature have explicitly noted the need for reliable causal estimates of these frictions.[6] We attempt to provide evidence on the causal role of these four candidate frictions through a unique field experiment, embedded within a detailed survey that we administered to 1,137 low-saving employees at a large US firm and targeting each friction through incentive- or information-based interventions. Three features of our empirical strategy and setting make it particularly promising for understanding how psychological frictions affect employee savings decisions. First, and most critically, the field experiment takes place in the context of an extensive survey which captures detailed measures of relevant individual beliefs and decision-making prior to the experimental treatments addressing possible reasons for low saving. While the differential response across experimental treatments provides insight into how each friction affects savings behavior at the margin, the survey also permits us to document baseline prevalence of each friction (and any correlation with baseline savings) and to test whether employees exhibiting specific frictions most strongly also benefit more from the corresponding treatment. For example, an experimental

[4] Following Ericson and Laibson (2018), we adopt the language of *present-focus* instead of "present bias" in recognition of the fact that dynamically inconsistent choice may emerge from mechanisms that do not involve a bias.
[5] 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.
[6] On the relationship between financial education and economic outcomes, Hastings et al. 2013 assert that "… this literature needs large-scale randomized interventions designed to effectively identify causal effects." Beshears et al. 2018 note that "the biggest limitation of this literature is a dearth of studies that credibly estimate causal effects."

treatment providing clear, specific, and personalized contribution recommendations permits us to estimate the average causal effect of guidance on marginal savings, the pre-intervention survey permits us to estimate the baseline prevalence of low retirement literacy, and linking individual surveys to experimental responses permits us to estimate the (differential) efficacy of this guidance for employees with greater deficits in retirement literacy. Second, our study targets employees at-risk of financial insecurity during retirement who have access to an employer-sponsored 401(k) plan that offers strong financial incentives to contribute. This firm matches employee contributions, dollar-for-dollar, up to four percent of annual salary, and guarantees a minimum yearly match of $2,000 for employees contributing at or above the four percent threshold. As a consequence, employees not fully taking-up the match, faced a marginal rate of return for each dollar of contribution that ranged from 100 to 500% (median of 125%). Due to the generosity of the match, low savings in our setting cannot easily be rationalized by the standard economic model. Finally, our design enables us to simultaneously test candidate explanations within the same sample. To the best of our knowledge, ours is the first study of 401(k) contributions 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 several 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 workplace benefits. We speculate that the combination of employer sponsorship, use of reminder emails, and lottery prizes for participation contributed to the high response rate of 28 percent. Beyond capturing demographic and financial background, the survey elicited a range of retirement-relevant beliefs and diagnostic questions assessing employee-specific incidence of each of the candidate frictions. After proceeding through these modules, employees were then randomized to one of several experimental variants of a final module that used prior survey responses to generate a personalized assessment of retirement preparedness. Across all experimental treatments, the assessment truthfully reported that the employee was not “on track” for retirement security based on their present level of savings and plan contribution rate (using the retirement calculator from the plan’s enrollment portal), advised the employee to increase their contribution, provided simple instructions to any employee seeking to adjust their contribution, and asked employees to introspect about their decision and future intent to save. To identify the role of our candidate frictions, the experimental conditions varied in (1) the specificity of provided guidance (i.e., we gave some respondents a specific contribution target while others were merely advised to increase their contribution), (2) the presence of information highlighting the plan’s generous match, and (3) the presence of a small, immediate, reward (a $10 Amazon gift card) to encourage employees to engage their savings decision by

visiting the plan portal during our survey. The primary outcome of interest was whether, and by how much, an employee increased their contribution in response to this final module as indicated through administrative records from the pay-period following the end of the survey.

We report four primary findings, corresponding to each candidate friction. First, while we corroborate previous research indicating widespread retirement literacy deficits—captured both by underestimates of adequate saving rates and low assessed financial literacy—and a positive association between financial literacy and baseline saving behavior, we conclude that these deficits *do not* independently cause low 401(k) plan engagement. That is, experimental provision of a clear, personalized, and specific recommendation does not change the average likelihood that employees increase their saving rate, despite improving the accuracy of beliefs regarding savings needs. This guidance does not increase savings even among employees who underestimate saving needs at baseline or who score the lowest in financial literacy. Our analysis does provide some insight toward reconciling these results with the extensive literature on retirement and financial literacy. For example, while employees in our sample systematically underestimate saving needs, most employees—even those already participating in the 401(k) plan— do recognize that they are saving insufficiently. This is illustrated in the cross-section where the variance in the degree to which employees underestimate how much to save does little to explain the variance in degree to which employees actually under-save. We also document how employee biases pertaining to two central retirement-relevant beliefs—standard inputs in most commercial retirement savings calculators—actually have offsetting implications for savings. Relative to actuarial baselines, employees appear to overestimate both 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).

Second, 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 plan confusion—confusion about plan features such as the match limit and default rate and confusion about one's actual plan enrollment status. Regarding the former, we find that 40% of employees had inaccurate beliefs about the match of whom three-quarters underestimated its generosity. While such underestimation of the match was strongly correlated with non-participation and failure to take-up the match at baseline, we find that clarifying match incentives *did not* lead more employees to increase savings on average, nor did it differentially increase saving for employees who underestimated the plan match. However, in an unplanned analysis, we find that over a third of 401(k) non-participants in our sample erroneously reported non-zero contributions at baseline. Subsequent analysis of other survey responses suggests that these misreports largely reflect confusion as opposed to inattention to the survey or willful exaggeration. Consistent with this interpretation, among employees misreporting baseline

enrollment, those who were more likely to observe actual enrollment status by virtue of random assignment to the reward condition of the experiment, were more than three times as likely to increase their plan contribution rate than their counterparts in other experimental conditions. While the scope of this confusion seems almost implausible given the high financial stakes, 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 were asked to make enrollment decisions in up to 12 benefit programs, each with varying rules governing eligibility and enrollment.

Third, we present evidence against the role of perceived enrollment complexity in inhibiting plan enrollment or higher contribution rates. In our sample, 77 percent of employees perceived the time required to adjust one's contribution as modest (i.e., requiring a matter of minutes or less). Consistent with low baseline perceptions of administrative complexity, we find that respondents in a control condition, for whom administrative enrollment adjustment was simplified through clear instructions, a direct link to the enrollment portal, and messaging indicating that the adjustment requires only a few seconds, were no more likely to increase contributions than out-of-sample comparisons to behavior preceding research interventions. Further, employees perceiving enrollment changes as time-consuming (i.e., requiring more than minutes) were not differentially more responsive to simplification than their counterparts. Together, our findings on enrollment complexity and plan confusion suggest that benefit complexity across benefit programs may impede engagement as much as, or more than, the complexity of an individual program.

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) caused 7% of employees to increase their contribution rate while information regarding large delayed benefits associated had no marginal impact on saving behavior. This relative, and absolute, response to the small reward is robust across employees who were not participating in the plan, participants eligible for additional match incentives, and employees who underestimated match generosity at baseline (and thus for whom the match clarification treatment provided new information). Among employees tagged as exhibiting present-focus through a subsample survey assessment, a striking 50% increased their contribution in response to the small reward. This treatment effect persisted over the next several pay periods for which we observe administrative data and at least one-half of contribution adjustments entailed increases of more than one percent of salary, suggesting the response to the reward did not reflect a strategic adjustment employees expected to quickly reverse. For employees contributing below the match threshold, we estimate that their response to the $10 reward would have resulted in an average (max) gain of $488 ($1,500) via the plan match after 6 months and $1126 ($3,000) after 1 year assuming no reversal after our observation ended.

What explains why a significant share of employees responded to this 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 with present-focused 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 an unreasonably narrow margin of $10 relative to the net present financial value of the plan match. Moreover, for employees not contributing to the plan at baseline, the calibrations show that beta-delta discounting can only rationalize this as delaying enrollment for at most several days, even assuming substantial hassle costs of enrollment. Given our baseline survey elicitation of employee beliefs, we reject this form of rationalization on the basis that non-participating employees by in large anticipate delays before saving changes on the order of weeks or months, rather than days.

We conclude by advancing a novel alternative model of present-focused behavior motivated by the prevalence of financial anxiety in surveys of financial well-being and research in neuroscience and psychology on anxiety's impact on cognition decision-making. The model assumes that low-saving employees experience high states of financial anxiety, that such anxiety imposes significant psychological costs of contemplating and carrying out 401(k) enrollment, and that employees are optimistic (perhaps overly optimistic) about reductions in anxiety, though over intermediate, rather than immediate, time horizons. Specifically, we write down a model in which a stochastic process governs an employee's transition from a high to low state of financial anxiety. Employees expect such a transition to occur over a horizon of several months rather than days. This model of anxiety with optimism about diminished anxiety in the future implies that utility-maximizing but financially anxious employees may decide to delay plan enrollment until they face lower financial anxiety as long as the forecast reduction in experienced disutility from enrollment outweighs the expected costs of delay due to foregone matching incentives. In this account, the small gift card reward in our experimental intervention derives its potency from effectively reducing the anxiety-related costs of visiting the benefit enrollment portal (perhaps by shifting enrollee attention to reward attainment rather than the underlying issue of their retirement savings).

To investigate the plausibility of the proposed model, we first calibrate the model with inputs from our field data and show that it can account for the baseline failure of employees in the field to enroll, as well as their beliefs about intended future savings, without assuming implausibly high psychological costs of enrollment, low values of beta, or prohibitively complex decision processes. We then present

empirical evidence from the field sample, and from a supplementary survey sample of employees, to test the validity of the assumptions and main predictions of the model. Among the employees in our field sample, we document widespread reports of high financial anxiety, systematic optimism that such anxiety will decline over intermediate time horizons, a negative correlation between current anxiety and plan engagement, and a positive correlation between the anticipated timing of anxiety reductions and intentions to save. To our knowledge, we are the first to document this particular dynamic with respect to forecasts of future anxiety. We present additional analysis from our supplementary survey of employees, designed to provide more detailed forecasting elicitations over lengthier horizons, that replicates the pattern of high financial anxiety and optimistic forecasts about reductions in anxiety over intermediate, but not immediate, time horizons.

Our findings suggest several lessons for policymakers and for saving plan designers seeking to improve the financial well-being of employees. First, the inefficacy of personalized savings recommendations that we observe even among employees with low financial literacy, should dampen expectations about behavior change in response to costly investments in just-in-time financial education, plan marketing, and decision-aids, strategies which are currently pursued by many large financial services firms and advocated by industry experts. The striking degree of confusion about benefit plan enrollment we document suggests that a more efficient strategy for increasing plan engagement might be to increase the visibility of information about benefit enrollment status, for example by using inexpensive rewards for engaging and revisiting savings decisions. Finally, our finding of employee responsiveness to financially modest rewards, but not clarification of far more generous matching incentives, imply that the current structure and marketing of 401(k) plans are not optimally suited to the psychology shaping actual enrollment decisions for many employees. Informed by this, we describe a design for a retirement savings benefit plan structured so that enrollees initially contribute to a highly liquid "Serenity Account" for immediate needs that automatically transitions accumulated savings above a reserve threshold into a less-liquid retirement savings account such as a 401(k). This notion of a second account, adjacent to a primary retirement savings account, has been advocated in recent years in various forms (e.g., "sidecar account", "emergency savings account"). Our research offers a psychological rationale for these designs by suggesting that addressing concerns about immediate financial security may be a necessary precursor to engagement with longer-term savings decisions.

The present research draws from, and contributes to, several strands of the existing theoretical literature on retirement savings. First, while we find evidence consistent with the large literature documenting deficits in financial literacy and retirement planning (e.g. Lusardi & Mitchell 2011; Goda et al. 2014; Hastings et al. 2013), our experimental evidence from low-saving employees does not support a causal link between such literacy and savings. Second, we add to the growing number of examples in

which program confusion or complexity serves as a barrier to engagement (e.g., Bhargava and Manoli, 2015), but in the present setting we speculate that the confusion arises from the complexity of the large number of varying benefit programs employees are asked to engage, rather than the administrative complexity of a single program.

Finally, our study offers rare direct evidence of the widely-embraced view that present-focus plays a central role in the insufficient savings of employees (e.g., Madrian and Shea 2001, Thaler and Bernatzi 2004). Our calibrations, informed by the experimental evidence and directly elicited employee beliefs, challenge the prevailing interpretation of present-focused behavior through beta-delta discounting (accompanied by significant enrollment hassle costs). The alternative theoretical framework we propose of anxiety with optimistic forecasts—particularly if extended to assume that employees are *over*-optimistic in forecasting future hedonic costs—offers a potential psychological explanation for unifying several related empirical puzzles pertaining to employee retirement savings behavior. These puzzles extend beyond insufficient accumulation of wealth to include the failure to take-up generous matching incentives, the high efficacy of automatic enrollment on plan participation, the gap between stated and realized savings, and low demand for commitment devices.

2 BACKGROUND AND INSTITUTIONAL SETTING

2.1. Overview of 401(k) Plan Structure, Engagement, and Retirement Preparedness

Plan Structure. In recent decades, 401(k) plans have become a primary channel, along with Social Security and private savings, through which US employees at for-profit institutions save for retirement. These plans, named after the sub-section of the legislation, The Revenue Act of 1978, from which they arose, permit qualified employees to contribute, via automatic deduction, a capped share of pre-tax salary into a portable, tax-deferred, and immediately vested, savings account.[7] As of 2016, 401(k) plans comprised 80% of all employer-sponsored retirement plans and covered 82.7 million employees.[8] Beyond favorable tax-treatment and portability, a distinguishing feature of most 401(k) plans is an often generous employer match.[9] Intended to encourage employee savings, a plan match entails an employer contribution equivalent to some share of an employee's annual contribution up to a threshold usually ranging from 3 to 6 percent of annual salary (less commonly, employees may contribute to employee accounts on a non-matching basis). A recent industry survey indicated 75% of employer-sponsored 401(k) plans offered matching incentives, and while the generosity of such incentives varied widely, the modal plan match

[7] Accumulated assets are not subject to taxation until disbursement, and even then, only as ordinary income, excluding contributions designated as Roth deferrals.
[8] See Table A1(a) of the Private Pension Plan Bulletin Abstract of 2016 Form 5500 Annual Reports published by EBSA (2018).
[9] 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).

involved an employer contribution of 50 cents for every dollar of employee contribution up to 6 percent of annual salary (PLANSPONSOR DC Survey 2017).

Plan Engagement. Three often-cited metrics help to characterize 401(k) plan engagement: plan participation, average participant contribution rate, and the share of full match take-up among eligible employees. 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 decades, has led to sharp increases in participation as well as greater parity in participation across employee sub-groups. Of plans with automatic enrollment, participation rates typically reside around 85 to 90% (Vanguard 2016). While a majority of enrollees fully take-up the available match, one industry review of 550 401(k) plans covering 4.4 million participants found that 25% of eligible employees failed to fully take-up their plan match, forgoing an average of $1,336 or 2.4% of their annual salary (Financial Engines 2015).

While one might expect plan participation, average contribution, and full match take-up to increase with the generosity of a plan, several studies point to a modest relationship between the presence, and generosity, of a plan match, and plan engagement (e.g., Papke & Poterba 1995, Choi et al. 2002; Duflo et. al. 2006; Kusko et al. 1997, see Madrian 2013 for a review). Indeed, in an examination of newly eligible employees at several hundred automatic-enrollment plans at small to mid-size employers, one study found that employees were less likely to fully take-up the plan match in plans with more generous matching incentives—this is likely attributable to the fact that highly generous plans often set their plan's default rate below the plan's matching threshold (Bhargava et al. 2018)

Retirement Preparedness. While automatic enrollment has delivered well-documented gains in participation, recent industry and academic studies have asserted that a significant share of 401(k) *enrollees* may be insufficiently prepared for retirement. An analysis by Fidelity found that 32% of working US households were "not on track" for retirement, while the aforementioned study of automatic-enrollment plans projected that approximately 20% of 401(k) enrollees would fail to save enough to avoid "retirement poverty". Several factors likely contribute to the significant share of employees at-risk for retirement insecurity, despite plan participation, including low contribution rates, lack of full match take-up, insufficient changes to contribution rates over time, and the propensity of many employees to take costly early-withdrawal loans (see Beshears et al. 2018).

2.2. 401(k) Plan Environment at Partner Firm

Our field partner offered its more than 40,000 benefit-eligible employees a 401(k) plan with several features representative of plans more broadly. Specifically, the firm instituted automatic enrollment for new hires beginning in 2015 at a default contribution rate of 4 percent (and a target-date

fund as the default investment allocation). In June 2015, the firm informed existing employees, who became benefit-eligible prior to 2015 and were contributing less than 4 percent, that they would be automatically enrolled in the 401(k) plan at a 4 percent contribution rate the following month unless they decided to actively opt-out of this "enrollment sweep."[10] As of July 2016, approximately 10 percent of new hires eligible for automatic enrollment or existing employees subject to the enrollment sweep opted-out of their enrollment. Overall, participation at the firm appears similar to national averages for plans with automatic enrollment. As with most large firms, employees could adjust their plan enrollment or contribution rate by accessing an online benefits portal via the firm's online intranet and proceeding through a simple web-flow.[11] Plan adjustments were implemented after an approximately 1 to 5 day lag.

While the firm, like many counterparts, offered a plan match, the match was distinctive in its generosity. The plan matched employee contributions, dollar-for-dollar, up to 4 percent of eligible compensation and additionally guaranteed any employee contributing at least 4 percent over the course of the year a minimum annual match of $2,000. For those earning less than $50k annually and not yet contributing at the threshold, the minimum match implied an effective return that could significantly exceed 100% assuming the employee maintained their contribution rate the subsequent calendar year. For employees in our sample contributing less than 4 percent, the match implied an expected return to each marginal dollar ranging from 100% to 500% with a median marginal return of 125%. Despite the generosity of the firm's 401(k) plan, slightly less than one-half of plan enrollees invited to partake in the field study took full-advantage of the plan match.

3 THEORETICAL FRAMEWORK OF EMPLOYEE SAVINGS

To organize tests of candidate frictions, we introduce a simple theoretical framework to describe an employee's decision to save. The framework adapts the notation and exposition of DellaVigna (2018) who models the potential influence of present bias, and hassle costs associated with enrollment, on the savings decision of an employee in the context of a 401(k) plan with matching incentives.[12] We initially consider the case of a fully-informed employee maximizing utility subject to an exponential discount function and then consider specific departures from this baseline corresponding to each psychological friction of interest.

3.1. The Savings Decision

[10] Conversations with industry executives suggest that the periodic administration of "enrollment sweeps" targeting tenured but non-participating, or otherwise low-saving, employees has become increasingly common at large firms.
[11] In principle, employees could also contact a benefits representative by phone but we were told this was an uncommon practice.
[12] 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.

We define the savings decision for a non-participating benefit-eligible employee as a choice between enrolling in a 401(k) plan now or delaying enrollment to a future period, indexed in number of days. For simplicity, we restrict our attention to the specific decision to enroll at a contribution rate of 4 percent in a plan that offers a dollar-for-dollar match up to a 4 percent threshold. We assume that the employee may be subject to present bias captured through beta-delta preferences. We specify the employee's total utility by the following equation:

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

where $U_t = (u_t, u_{t+1}, \ldots)$ represents the present discounted value of instantaneous utility associated with future periods, *t*, indexed in days. $\beta\delta$ denotes the employee's discount factor between today and tomorrow, while δ denotes the discount factor between any two days in the future ($\beta, \delta \in (0,1)$). For additional tractability, and in recognition of the considerable inertia that empirically governs year-to-year savings, we assume that once an employee decides to enroll, she continues to contribute at 4% each year until she retires at time *T* when she receives accumulated savings as a lump-sum. We additionally normalize the value of never saving to 0.

To assess the conditions under which an employee will enroll we must specify the costs and benefits of enrollment. We denote the costs of enrollment by *k* and interpret such costs to include the opportunity time-costs associated with administrative enrollment. To capture the fact that these costs may vary across days, in practice, we can interpret *k* in a specific period as being drawn from a uniform distribution over potential costs. We next define the net utility gained from *s* dollars in a given period by *b*. To simplify, we normalize constant marginal utility of consumption to 1 and assume that the long-term discounting factor equals the interest rate, such that, $\delta = 1/(1 + r)$. This permits us to specify the net utility gain an employee receives from contributing *s* dollars in a period 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 savings from the employer match, and $\tau_R \leq \tau_0$ is the tax rate in retirement. We first consider the decision to save, or enroll, for a baseline employee, whose behavior is subject to the standard model, and then proceed to the more interesting case in which employees exhibit psychological frictions, or departures from the standard baseline model of enrollment.

3.2. Standard Model Baseline ($\hat{\beta} = \beta = 1$)

A utility-maximizing employee with time-consistent preferences will enroll immediately if the present value of future expected benefits from enrollment exceeds the costs of enrollment. Otherwise, the employee will never enroll. We describe the enrollment decision with the following inequality:

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

Noting the Taylor series expansion for $\frac{\delta}{1-\delta}$ allows us to rewrite the decision rule as:

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

Prediction: The probability of enrollment increases in b and decreases in k.

3.3. Psychological Frictions

Friction 1: Present Bias ($\beta < 1$). The initial psychological friction we incorporate in the model is present bias realized through beta-delta preferences. While present bias permits delays in actions such as enrolling in a 401(k) with a generous match, a key insight from O'Donoghue and Rabin (1999a) is that a sophisticated present-biased discounter will not delay action indefinitely due to knowledge that she will encounter similar temptations to not act in the future. 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 $\delta \to 1, (1-\delta^{t*}) \approx (1-\delta)t^*$, a sophisticated present-biased employee will enroll if:

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

Such an employee will delay no more than $t^* = k\frac{1-\beta}{\beta b}$ days.

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. She will invest today if:

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

and will delay indefinitely if enrollment costs are above this decision threshold and below her anticipated cost of delay, $\frac{\delta(\tau_0 s+\mu-\tau_R(s+\mu))}{1-\delta}$. We assume sophistication among present-biased employees for remainder of this section and revisit the possibility of naiveté when we present actual data on employee beliefs.

Prediction. For a sophisticated present-biased employee, the likelihood of enrollment rises with β, such that $\frac{\partial \Pr[\text{Enroll}]}{\partial \beta} > 0$, and differentially rises with an immediate, rather than delayed, increase in benefits: $\frac{\partial \Pr[\text{Increase}]}{\partial (b_{now})} > \frac{\partial \Pr[\text{Increase}]}{\partial (b_{later})}$.

Friction #2: Deficit in Retirement Literacy ($\hat{b} < b$). The next friction we consider captures the possibility that deficits in understanding about one's financial needs in retirement, or how much savings is sufficient to meet such needs, can causally influence enrollment. We model deficits in retirement literacy as distorted beliefs about the benefits of saving and specifically restrict attention to instances in which an employee underestimates the expected benefits of savings, such that $\hat{b} < b$. An employee suffering from deficits in literacy (along with potential present bias) will enroll if: $k \leq \frac{\beta \hat{b}}{1-\beta} t^*$.

Prediction: A prediction of the model is that the likelihood of enrollment for an employee who underestimates the benefits of saving increases as the magnitude of such deficits decrease: $\frac{\partial \Pr[\text{Enroll}]}{\partial (\hat{b}-b)} > 0$.

Friction #3: Plan Confusion ($\hat{\mu} < \mu$). The third friction we consider captures the possibility that employees underestimate, or are unaware of, the generosity of the plan match. In general, we denote the perceived net utility gain from savings as a function of the perceived match $\hat{\mu}$, so that $\hat{b} = \tau_0 + \hat{\mu} - \tau_R(1+\hat{\mu})$. The decision rule for an employee who underestimates plan generosity can be written as:

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

Prediction: A prediction of the model is that the likelihood of enrollment for an employee underestimating plan generosity increases as the magnitude of underestimation decreases: $\frac{\partial \Pr[\text{Enroll}]}{\partial (\hat{\mu}-\mu)} > 0$

Friction #4: Enrollment Complexity ($\tilde{k} > k$). A final friction captures the possibility that the employee overestimates the time-costs associated with administrative enrollment because of a belief that administrative enrollment is highly complicated. We can think of enrollment costs in this case as some

$\tilde{k} = k + k'$ where k captures the true opportunity time-cost of enrollment and k' reflects overestimation due to a perception of enrollment complexity. Alternatively, invoking the possibility of hassle costs associated with small administrative burdens (e.g., Bertrand and Mullainathan 2004), one could interpret k' as reflecting the presence of psychological costs of enrollment that significantly exceed the economic value of the time required to enroll. The decision-rule for an employee who overestimates the costs of enrollment due is identical to that displayed above, but for the replacement of k with k'.

Prediction: A prediction of the model is that the likelihood of enrollment for an employee who overestimates the costs of enrollment, due to the belief in enrollment complexity, decreases as the belief in enrollment complexity increases: $\frac{\partial \Pr\,[\text{Enroll}]}{\partial k'} < 0$

In the next section we describe the research design which outlines how our strategy for both measuring the baseline value of each friction via survey measures and then intervening to reduce each friction through an information- or incentive-based intervention.

4 EMPIRICAL RESEARCH DESIGN

4.1. Overview

To investigate the causal role of our four behavioral frictions in producing low plan engagement, we administered a field experiment targeting low-saving employees at a large US firm in the financial services sector in July 2016.[13] The field experiment was embedded within an online survey marketed as an opportunity, endorsed by the firm, for employees to provide confidential feedback on the workplace and employee benefit programs. The first several modules of the instrument captured background demographic and financial information, along with several questions and assessments, that enabled us to estimate each employee's target savings rate as well as their incidence to each of the tested frictions.

The final module, while ostensibly providing respondents with a personalized evaluation of their retirement preparedness and an opportunity to adjust their plan contribution/enrollment, exposed respondents to experimentally varying information, and in some cases financial incentives, designed to reduce each friction. While the differential savings response of employees across experimentally varying web-flows constitutes our central evidence on the causal effect of each friction, we also present evidence inferred from comparing the savings behavior of employees within-the experiment to that of an out-of-sample comparison group during the study period and to that of in-sample employees during a control period prior to the study period. We describe the sample, procedures, and experimental treatments in greater detail below.

4.2. Employee Sample

Two considerations largely shaped the composition of the employee sample—a desire to target under-saving, low-to-moderately compensated, employees and the firm's request to limit the invited sample to no more than 5,000 employees. Additionally, because we aspired to assign employees to a distinct set of interventions based on whether or not they were fully taking up the available plan match, we invited two non-overlapping samples of employees to participate in the online survey. The first, and primary, invitation sample (i.e., the Low Savings Arm, or "Low Arm") comprised the universe of 3,719 plan-eligible employees who, as of July 2016, were: (i) 25 to 55 years of age, (ii) earning less than $100k annually, and (iii) contributing at an annual rate below the 4 percent match threshold (inclusive of a 0 percent contribution). A second sample of 1,000 employees (i.e., the Moderate Savings Arm, or "Moderate Arm") comprised a random selection of all plan-eligible employees who, as of July 2016,

[13] 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.

were: (i) 25 to 55 years of age, (ii) earning less than $100k annually, and (iii) contributing at an annual rate from 4 to 10 percent.

Of the 4,719 employees ultimately invited by email to participate in the field study across the two arms, 1,332 employees completed the online survey within the approximately 10-day pre-specified study period. Of these respondents, we excluded 205 employees from experimental analysis because they reported a contribution rate that met or exceeded our estimate of their recommended contribution rate (making it impossible to assign them to an experimental treatment). The remaining 1,137 employees were randomized to one of the experimental treatments within the two study arms (780 employees were treated in the Low Arm and 357 were treated in the Moderate Arm).[14]

Table 1 describes basic demographic background and savings behavior of employees in the invited and respondent samples as well as additional detail for actual survey respondents. We draw two insights from the table pertaining to the representativeness of the analytic sample. First, the table conveys the demographic diversity of the field sample across age, gender, income, marital and family status, education, and race though we note that the samples are disproportionately female, reflecting the broader gender disparity at the firm. Given the particular interest in generalizing insights from the study to the broader population of low-to-moderate income households, Appendix Figure A1 compares the cumulative income distribution of the invited and respondent samples to a contemporaneous, nationally representative, sample of full-time US employees drawn from the 2015 CPS. The figure indicates that the income of employees in the study resembles the national cross-section but for modestly over-sampling the middle two earnings quartiles. A second insight from the table is the approximate similarity, across demographic and savings variables, between the invited and respondent samples. This similarity suggests that observable demographics did not strongly predict response to the survey invitation, a pattern that we return to in our construction of a credible out-of-sample comparison group.

4.3. Survey of Psychological Frictions

4.3.1. Structure and Procedure

Overview. In late July 2016, employees were invited by email to participate in a 10- to 15-minute survey described as an opportunity to provide confidential feedback on the workplace and benefit programs. As Figure 1 schematically depicts, the instrument featured five modules of which the first four constituted the survey of employee frictions (as well background information) and the last administered the field experiment.[15] The initial four modules were identical across respondents excluding instances in

[14] Assignment to the Low or Moderate Arms 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.

[15] Note that while we distinguish questions by module or semantic category for ease of explanation, modules prior to the experiment were not organized as separate sections for respondents in the order in which we describe them here. In some

which we customized questions to reflect an employee's specific enrollment/contribution status, or prior answers, or in which questions were included in modules administered to random sub-samples of employees by design.[16]

Email Invitation and Reminders. We invited employees to participate in the survey via an email sent to a list of addresses provided to us by the firm. The emails, which carried the insignia of the firm, communicated that the survey was administered by researchers from Carnegie Mellon University who had received the firm's permission to collect feedback from employees on the quality of the workplace and the administration of benefit programs. The email promised confidentiality and indicated that anonymized insights from the survey would be delivered to the firm to help advance employee well-being. The email provided each employee a customized link to the online survey, hosted on the Qualtrics platform, and emphasized the survey deadline. To encourage response, we informed employees that by completing the survey they would qualify for a raffle where they could win one of several Apple iPads. A reminder email was sent to each employee the day before, and the day of, the deadline.

4.3.2. Survey Content

The survey featured four initial modules intended to capture the information required for us to estimate each employee's recommended savings rate, assess employee-specific incidence to each friction, and to otherwise complete the planned analyses. For ease of exposition, we describe each of these modules separately, and in chronological order. The actual sequence of questions in the survey was determined by considerations of user-experience and programming logistics.

Module 1 – Employee Background. A first category of questions queried employees about their demographic and financial background as well as their participation in firm benefit programs. Specifically, we collected data on age, income (measured categorically), tenure at the firm, current level of accumulated savings, and emergency liquidity. We also asked employees to report whether or not they participated in a number of employee benefit programs including the firm's 401(k) plan. For those participating in the 401(k) plan we also collected their self-reported contribution rate. Beyond informing our subsequent analyses, we used data from this module to generate a real-time savings recommendation for every employee and to assign each employee to one of the two study arms.

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 prior responses.

[16] On day 4 of the survey window we extended a set of questions including measures of anxiety, not included in the initial launch, due to unanticipated strong patterns in responses which we discuss below.

Module 2 – Program Beliefs. A second set of questions assessed respondent knowledge, and understanding, of firm benefit programs, with a particular focus on their 401(k) plan.[17] As to the 401(k) plan, employees were quizzed about plan eligibility (administrative records indicated that all respondents were eligible) and knowledge of plan features such as automatic enrollment, the default contribution rate, and details of the plan match. The survey also asked employees about their intent to enroll or change their contribution over varying future horizons (3 months for participants, and 6 months to 3 years for non-participants), as well as their perception of the administrative complexity of enrollment. We additionally asked respondents to introspect as to why automatic enrollment was so successful in increasing plan participation, and as a gauge of attentiveness, we included a question designed to assess whether the respondent read the text of the survey question.[18,19] We used the data from this model, in part, to generate various measures pertaining to the frictions of plan confusion and enrollment complexity.

Module 3 – Retirement Literacy. A third set of questions investigated a range of retirement-relevant beliefs. As an example, to generate an indirect measure of how much employees believed they ought to save each year for a financially secure retirement, we asked them to provide the inputs typically used by retirement savings calculators to generate savings recommendations— most notably, the expected age of retirement, the expected duration of retirement, and the income replacement ratio required to maintain one's current standard of living. To generate a more direct measure of such beliefs, we directly asked employees to estimate the minimum annual 401(k) contribution that someone like them would need to make each year, assuming they continued to work with their current employer, to avoid retirement insecurity (explained as a financial situation that obviated the need to return to the workforce or rely on means-tested benefits). We used data from this module to estimate direct and indirect measures of employee-specific retirement literacy.

[17] We asked employees about their awareness, and 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.
[18] Specifically, after describing the documented influence of automatic enrollment on 401(k) participation, we asked respondents to identify the best of four candidate explanations (or a written alternative): (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. 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.
[19] The attention check was a generic question about work life, where the instructions included an acknowledgement that sometimes respondents may not have time to read each question carefully, and an instruction to indicate that they are reading these instructions by simply proceeding to the next question without selecting any of the available responses.

Module 4 – Decision-Making. A fourth module presented employees with a series of diagnostics intended to assess employee-specific incidence of present bias, financial literacy, and financial anxiety. To avoid a lengthy survey instrument, employees were randomly assigned to only a sub-set of these diagnostics.[20] Specifically, (1) we assessed present bias by asking employees to make a series of pair-wise choices involving inter-temporal effort tradeoffs (adapted from standard multiple price list procedures), (2) we assessed financial literacy and market knowledge by asking employees two widely-used questions about inflation and compound interest and a third question about expected growth of equity investments over long time horizons, and (3) we asked employees to self-report present feelings of financial anxiety and to forecast how they expect such feelings to change over varying future horizons.[21]

4.4. Field Experiment

4.4.1. Overview and Procedure

Following the initial survey modules, employees proceeded to a final module which, they were told, would offer an assessment of their preparedness for retirement based on their earlier survey responses. In actuality, respondents progressed through one of several experimental variations of the assessment web-flow based on up to two random assignments. As depicted in Figure 1, across all of the experimental treatments, the final module first provided an assessment of the employee's preparedness for retirement (*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 expressing initial disinterest in modifying their rate were asked if they would like to reconsider the decision (*Savings Reconsideration*). Last, they were asked follow-up questions about their savings decision and future intentions to save. (*Savings Follow-up*).

[20] 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].

[21] In addition to the common questions about inflation and interest, popularized by Lusardi and Mitchell (2008), 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. Due to survey constraints, we adapted the conventional MPL with a two-question measure intended to capture severe manifestations of present bias. The 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.

Random Assignment. In an initial assignment, respondents were randomly assigned to a treatment based on whether their self-reported contribution implied full take-up of the plan match. Respondents self-reportedly not participating in the 401(k) or contributing at a rate of 1,2, or 3 percent were randomized to one of the three primary experimental treatments in the Low Arm while remaining respondents (i.e., those contributing between 4 and 10 percent, inclusive) were randomized to one of two primary treatments in the Moderate Arm. Balancing tests indicate that the, equally sampled, treatment groups were observationally comparable (Appendix Table A1). In a second assignment, respondents who decided not to increase their contribution at the initial savings decision and, who were not offered a small reward via their primary experimental treatment, were independently randomized to one of two experimental variations of the savings reconsideration prompt.

4.4.2. Baseline Condition - Generic Recommendation

To simplify the description of the interventions, we first describe the web-flow associated with a baseline condition that delivered a generic recommendation to employees (one of the interventions in the Moderate Arm) and then we proceed to describe each experimental treatment by referencing departures from this baseline. The baseline web-flow consists of four distinct segments described below (Appendix Figure A2 displays key screenshots).

1. 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 screen, titled "Your Personal Retirement Evaluation." The screen featured a stylized graphic of a red-to-green speedometer, with the dial pointed towards red, above which bolded text indicated that the employee was not on track for retirement and should take action now: "You should take action now to get on track for a financially secure retirement." Text beneath the graphic encouraged employees to increase their contribution rate: "We recommend that you increase [in green type] your [redacted] 401(k) contribution rate."

2. Savings Decision. Following the display of the retirement assessment and generic guidance, a subsequent screen asked respondents if they wanted to modify their contribution rate after conveying that the modification would require only seconds: "What would you like to contribute to your [Redacted] 401(k)?". Respondents were presented with a text box to indicate the rate at which they desired to contribute (they were instructed to leave the box blank if they were not interested in changing their rate). Respondents entering a contribution rate into the text box were led to a screen that provided simple instructions, and an online link to the firm's benefit portal, to

carry out the modification.[22] A forced one-minute pause on the instruction screen was intended to provide respondents time to carry-out the instructions after which employees proceeded to a screen where they were asked to confirm that they had modified their rate.

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

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

4.4.3. Primary Experimental Treatments

Low Savings Arm. Respondents in the Low Arm were randomized to one of the three primary experimental treatments—Specific Recommendation, Match Clarification, Small Reward. We describe each treatment, and the friction the treatment was intended to test, below (Appendix Figure A3 displays screenshots for experimental departures to the baseline web-flow):

1. Specific Recommendation [Retirement Literacy]: This experimental treatment adapted the baseline design 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 employees could access elsewhere on the benefits portal. The calculator inputs were informed from earlier survey responses and the recommendations took into account the plan match.[23] We designed the treatment to test whether

[22] 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.

[23] 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 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%.

reducing employee underestimation of how much to save, or equivalently, reduce underestimation of the benefits of saving, would lead employees to increase their contribution relative to the out-of-sample comparisons (i.e., $\frac{\partial \Pr[\text{Increase}]}{\partial(b-\hat{b})} < 0$). The treatment also served as the in-sample control for tests of match clarification and the small reward.

2. Match Clarification [Plan Confusion]: A second treatment duplicated the specific recommendation web-flow but for an extra screen which clarified the generosity of the plan match. The additional screen, encountered prior to the initial 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." A graphic illustrated that the match effectively doubled each dollar of the employee's contribution up to 4 percent of salary and additional text explained the $2,000 minimum match. 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, the clarification of the match could have affected behavior by heightening the salience of matching incentives even for employees with accurate beliefs.

3. Small Reward [Present Focus]: The third treatment duplicated the match clarification web-flow and introduced a small reward, an $10 Amazon Gift Card, to encourage employees to engage their enrollment decision. The availability of the small reward was communicated by text above the 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 screen clarified that employees could receive the gift card either by modifying their 401(k) contribution in any way today or by directly contacting the researchers to indicate that, after visiting the enrollment portal, they decided not to modify their contribution. The intervention was designed to test whether present-focus hindered employee savings by permitting us to compare the differential response of employees to the experimental provision of a small reward, relative to clarification of the much larger, but delayed, plan match. (i.e., $\frac{\partial \Pr[\text{Increase}]}{\partial(b_{now})} > \frac{\partial \Pr[\text{Increase}]}{\partial(b_{later})}$).

Moderate Savings Arm. Respondents routed through the Moderate Arm were randomly assigned to one of two experimental treatments—the Generic or Specific Recommendations. Again, we describe each treatment, and indicate the friction the treatment was intended to test, below.

1. Generic Recommendation [Enrollment Complexity] – This experimental treatment was identical to the baseline condition described above. It permits us to identify, relative to out-of-sample comparisons, the impact of reducing enrollment complexity through the provision of generic guidance that employees should increase their contribution and clarification that such an increase requires minimal time (i.e., $\frac{\partial \Pr[\text{Increase}]}{\partial(k)} < 0$). The treatment also serves as control condition from we identify the causal effect of providing a specific recommendation.

2. Specific Recommendation [Retirement Literacy]– This experimental treatment was identical to specific recommendation treatment in the Low Arm. The treatment was intended to test whether reducing increasing the accuracy of employee beliefs about how much to save led employees to increase their contribution rate relative to the generic recommendation control (i.e., $\frac{\partial \Pr[\text{Increase}]}{\partial(b-\hat{b})} < 0$).

4.4.4. Secondary Experimental Treatments

To increase the statistical power of our test of small rewards, we independently randomized select employees across the two study arms to one of two experimental variations of the savings reconsideration prompt. The randomization was restricted to respondents who had progressed to the savings reconsideration prompt (by indicating that they had not changed their contribution after the original prompt) and who had not been initially randomized to the small reward treatment (we chose not to offer respondents the small reward twice). We randomized employees within this restricted sample to the savings reconsideration prompt in the baseline condition or an otherwise identical prompt that offered the same small reward——a $10 Amazon Gift Card— to encourage employees to "take action" now. As with the earlier implementation of the reward, fine print clarified that respondents could earn the reward by either modifying their contribution rate or by emailing the researchers of their decision not to do so.

4.5. Data and Empirical Outcomes

Our empirical analysis relies on data collected from employees via the online survey and linked administrative data provided by the firm. Specifically, 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. We used these administrative data to create several baseline measures to characterize employee savings prior to the launch of the study such as an enrollment indicator, the contribution rate as a percent of annual salary, the annual savings rate (i.e., the contribution rate plus the employer match), and an indicator for full match

take-up. To assess field experimental outcomes, we used administrative records to calculate changes in these same measures during the study period (i.e., between the payday prior to study launch on July 11th, 2016 and following the close of the study on August 8th, 2016).[24]

5 EVIDENCE ON CANDIDATE FRICTIONS

We now present findings from the field experiment and linked-survey. 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 friction. For each friction, we report the baseline incidence among employees, the naïve correlations between friction-specific measures and savings outcomes, the average response to the experimental treatment designed to reduce that friction, and, finally, the differential experimental response between employees varying in their baseline incidence.

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, resulting in 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. The summary statistics reported in Table 1 suggest that no major observable differences across the invited and respondent samples.

To facilitate interpretability, we generated several dichotomous measures to characterize the incidence of each friction. Table 3 reports the average of these measures for the full sample of respondents and for employee sub-groups distinguished by their degree of plan engagement. The final columns of the table report p-values from tests of mean differences to clarify whether the frictional measures significantly predict variation across two savings outcomes—plan participation and full match take-up. For example, the table reveals that 20 percent of respondents scored a zero on the two-item test of financial literacy and that literacy predicts plan participation (24 percent for non-participants, 16 percent for participants, $p = 0.02$) and full match take-up (23 percent for no take-up, 11 percent for full take-up, $p = 0.04$). The sample size for the measures vary due to our strategy of randomizing sets of questions across respondents and a one-time change adding an additional set of questions). Given that in some cases we calculated the dichotomous measures from multiple underlying survey measures (e.g., the

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

measure of indirect underestimation), Appendix Table A2 summarizes an expanded set of survey measures organized by friction.

Overall, table 3 documents evidence of moderate to high incidence across the four decision frictions. The most pervasive friction pertains to the retirement-relevant beliefs of employees; nearly one-half of employees either directly, or indirectly, underestimate how much they should contribute each year to ensure retirement security. The survey indicates more moderate, but still substantial, incidence of low financial literacy, plan confusion about the plan match and plan enrollment status (but not plan eligibility), and perceptions of high enrollment complexity. The two coarse measures of present focus presented in the table do not offer a precise diagnosis of baseline incidence but instead indicate that a small share of employees exhibit clear present focus, inferred from the preference reversal in our abbreviated MPL elicitation, and a large share of employees indicate an intent to save in the near-term future. Critically, several of the frictional measures—including financial literacy, underestimation of the plan match, overestimation of one's own plan contribution and the near-term intent to save—predict, in the theoretically prescribed direction, employee savings, as indicated by plan participation and full match take-up. Notably, there is no naïve correlation between perceptions of enrollment complexity and savings and mixed patterns in the measures of belief underestimation. We revisit these naïve correlations in the subsequent discussion of each friction.

Turning to the field study, administrative records indicate that 8.5% of employees randomized to a treatment modified their contribution rate during the two pay cycles of survey administration. Most modifications involved an increase (7.4%, CI: 5.6% to 8.9%) rather than a decrease (1.1%, CI: 0.3% to 1.8%) in contribution rate. Of the increases in contributions, more than one-half involved an increase of more than one percentage point. As a point of comparison, 1.7% (1.2%) of employees invited to the survey, but who did not respond, modified (increased) their contribution rate. Overall, in-sample employees were more than six times more likely to increase their contribution than non-respondents.

We can more formally estimate employee response to the experimental treatments across both study arms through a series of regressions. Table 4 reports estimates of response for two outcomes—an indicator variable denoting an increase to contribution rate and an indicator variable denoting whether the increase in contribution newly results in full take-up of the plan match—from linear probability models with indicator variables also representing each experimental treatment (with the constant suppressed). The first three columns of Panel A report the marginal effect of the primary experimental treatments in the Lower Arm on increased contribution rates as estimated by the following equation:

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

The equation estimates the marginal share of employees increasing their contribution (either resulting in a new enrollment or a rate increase by an existing participant) in response to each of the experimental treatments. More specifically, (θ - γ) captures the marginal effect of clarifying the generosity of the plan match on increases in contribution (i.e., the marginal effect of clarifying match generosity, in addition to providing a specific recommendation, relative to only providing a specific recommendation), and (β - θ) similarly captures the marginal effect of providing a small reward. To facilitate comparison with the non-experimental reference groups, the table also reports the average rate of increased contribution among invited, but non-respondent, employees as well as the same rate for in-sample employees during the period prior to the study. The fourth column of the table reports the analogous estimate of employee response for the two treatments in the Higher Arm.[25] The last two columns of the table summarize the change in the share of employees who fully take-up the plan match in response to the experimental treatments while Panel B reports analogous estimates across study arms for the secondary interventions.[26] Figures 2 and 3 graphically depict these treatment effects.

The exhibits collectively indicate that while most employees did not respond to the provision of savings recommendations, or to information clarifying the generosity of the plan match, a non-trivial share of employees, in both of the study arms, did increase contributions in response to the offer of the small reward, delivered either as a primary or secondary treatment. Among employees not taking full advantage of the plan match at the onset of the study, more than one-half of the increased contributions led to full take-up of the plan match.

5.2. Retirement Literacy - Candidate Friction #1

5.2.1. Baseline Incidence of Retirement Literacy

Retirement-Relevant Beliefs. We begin by assessing the accuracy of several retirement-relevant beliefs which, in theory, could lead employees to undersave. The first three measures in Appendix Table A2 summarize employee beliefs regarding the standard inputs required by most commercial savings calculators that estimate annual savings targets: retirement (starting) age, life expectancy (imputed from age of retirement and expected duration of retirement), and the target income replacement ratio (relative to current income). To better understand the plausibility of these beliefs, Appendix Figure A4 plots age-specific averages for the measures relative to actuarial projections of life expectancy, the average

[25] To estimate response to the primary interventions in the Moderate Arm, we estimate: $Pr(\text{Increase}_i) = \gamma \text{GenericRec}_i + \theta \text{Rec}_i + \varepsilon_i$.

[26] We estimate the following model of secondary interventions, separately for each experimental arm: $Pr(\text{Increase}_i) = \alpha\ \text{Reconsider}_i + \pi\ \text{Reconsider10}_i$, where $(\pi - \alpha)$ identifies the marginal effect of offering $10 after an employee reports not changing their contribution after the primary intervention, relative to a prompt to reconsider with no further reward.

retirement age of employees at the time of the survey, and the lower and upper bounds of recommended replacement ratios from academics and financial advisors.[27,28]

The figure hints at systematic biases in employee beliefs for at least two of the three inputs. The first panel suggests substantial over-optimism in life expectancy relative to actuarial projections while the second panel shows that employees, particularly of more advanced age, expect to retire much later than the average living male (59.6 years) or female (60.1) retiree (or the 62.7 years average of living retirees who recently retired in the last one to two years). The third panel indicates more plausible beliefs about the income replacement ratio required for a financially secure retirement (particularly if one believes that, for younger employees with lower earnings, the belief in a higher replacement ratio may be reasonable).

As an initial step in clarifying whether biased beliefs regarding key retirement inputs might lead an otherwise rational employee to undersave, we can compare the actuarially-informed recommended rate of annual savings—that is, the savings rate generated from a standard retirement calculator using conventional assumptions for key inputs—with the analogous recommendation using the stated beliefs of an employee.[29] As reported in Table 3, relative to the actuarial benchmark, adjusting recommendations with stated beliefs would lead 43 percent of employees to indirectly underestimate they should save. As additional evidence on the accuracy of employee beliefs, we can also examine each employee's direct belief regarding the minimal rate of annual contribution required to ensure retirement security. Relative to the actuarial benchmark, and tentatively assuming that employees accurately perceive the generosity of the plan match, this belief elicitation implies that 47 percent of employees directly underestimate how much they ought to save. Overall, the survey measures point to significant underestimation of required savings rates among employees in the sample.

Financial Literacy. We turn next to the survey evidence on financial literacy. Table 3 reports that 20 percent of employees scored zero on the two-question test of literacy (measuring knowledge of interest and inflation) while 18 percent scored zero on a three-question test (also measuring knowledge of typical asset accumulation in equity markets). A significantly higher share of employees scored one or less on the two-question (66 percent) and three-question (61 percent) measures.[30] Performance on the two-question test among our sample of under-saving employees, reflected a slightly lower level of literacy than

[27] 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. Data on average age of contemporaneous retirement was recorded from the 2017 Survey of Household Economics and Decision-making.

[28] 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%.

[29] Appendix Table A3 summarizes the survey data and assumptions used for each calculation in Figure 4 and for additional calculations adjusting for each belief separately in Appendix Figure A2.

[30] The elicitation of 20-year growth in the equity market implied highly varying beliefs of average annual returns with an inter-quartile range of 2.7% to 12.2%.

nationally representative samples of US adults (see Hastings et al. 2013). The table also shows that low scores on the measures of financial literacy, significantly predict adverse savings behavior—e.g., 24 percent of non-participants scored 0 on the 2-item measure compared to only 16 percent of participants (p = 0.06).

5.2.2. Experimental Test of Retirement Literacy

While the survey documents pervasive underestimation in direct, and indirect, beliefs about how much to save, a moderate share of financial illiteracy, and, a strong positive correlation between the latter and adverse savings behavior, we can explicitly test for the causal role of retirement literacy on savings through the field experiment. If deficits in retirement literacy lead employees to systematically underestimate how much they should save each year—or equivalently, cause employees to underestimate the benefits of saving, such that $(\hat{b} - b) < 0$—and such underestimation causes employees to save insufficiently, then providing credible, and specific, recommendations about how much to save should lead employees to increase their contribution. Further, one would expect that employees who underestimate how much to save would be differentially more responsive to the experimental provision of a specific recommendation than their counterparts who do not.

Overall, as indicated in Table 4 and Figures 2 and 3, employees *did not* increase contributions in response to the experimental provision of a specific recommendation about how much to save. Employees in the Low Arm were no more likely to increase contributions in response to the recommendation (b = 0.02, se = 0.01) relative to the non-experimental comparison of within-employee behavior prior to the study. The table shows that the recommendation did not lead many employees to newly participate in the plan (b = 0.02, se = 0.01) or to newly take-up the match (b = 0.01, se = 0.01). Similarly, employees in the Moderate Arm did not differentially respond to the provision of a specific (b = 0.03, se = 0.02) as compared to generic (b = 0.03, se = 0.02) ($p_{diff} = 0.70$) recommendation.

A plausible explanation for the absence of response to the personalized savings guidance is that employees simply did not find the recommendation to be credible or did not otherwise attend to this information.[31] To address this possibility, we designed the field study to include a direct, within-subject, tests of how employee beliefs shift in response to select experimental treatments such as the specific recommendation. Table 5 reports the outcome of simple regressions, with a suppressed constant, capturing how exposure to an experimental intervention changed an employee's retirement literacy—measured via an indicator of whether an employee perceived the minimum annual rate of savings required

[31] While the overall pattern of survey responses suggests that respondents generated sensible responses to questions, there is evidence of inattention as 40 percent of the sample failed a stringent attention check embedded in the survey.

for retirement preparedness to be at least as high as the standard recommendation—and perceived complexity of administrative enrollment (measured via an indicator of whether the employee perceived enrollment/adjustment to require no more than "minutes").

The table indicates that the provision of a specific recommendation improved retirement literacy for a significant share of respondents in both the Low ($b = 0.24$, $p < 0.01$) and Moderate arms ($b = 0.23$, $p < 0.01$) arms. In the moderate savings arm, the gain in literacy induced by the specific recommendation significantly exceeded that induced by the generic recommendation: ($b = 0.15$, $p_{diff} < 0.01$). The estimates affirm o interpretation of Table 4 that an increase in retirement literacy, delivered through belief-changing recommendations about how much to save, did not lead employees to save.

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 retirement literacy on the average savings of in-sample employees, the field study was designed to permit an even stronger assessment of experimental responsive among employees registering high on the frictional measures (and, relatedly, of differential experimental response across employees varying in their scores on such measures). Table 6, which synthesizes the cross-sectional and experimental treatment effects, presents these tests of heterogeneous response (for simplicity, the table is restricted to reporting experimental response among employees in the low savings arm, unless otherwise specified). Specifically, for each dichotomous measure for which the survey indicated non-trivial incidence, the table reports the cross-sectional difference across employees by savings status, the overall treatment effect associated with the pertinent experimental treatment, and the treatment effect estimated separately for employees scoring low (0) or high (1) on the dichotomous measure.

The estimates from the table, while measured more imprecisely than the main treatment effects, offers no evidence that employees with the most distorted beliefs, as indicated by either of the two measures of savings underestimation, meaningfully increased their contribution in response to the specific recommendation. It also indicates no differential response across employees varying in the incidence of the friction. And despite the strong cross-sectional association between financial literacy and savings outcomes, the table similarly indicates that employees scoring lowest in financial literacy do not meaningfully increase contributions in response to the experimental provision of specific guidance nor do they differentially increase contributions relative to their more financially literate counterparts.

5.3. Plan Confusion on Benefit Detail - Candidate Friction #2

5.3.1 Baseline Incidence of Plan Confusion

A second friction of interest reflects employee confusion about 401(k) plan detail such as the generosity of the plan match and plan eligibility. In theory, an otherwise rational employee who

underestimated their plan match, or mistakenly believed that they were not 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, 40 percent had inaccurate beliefs about the contribution threshold up to which the employer matched contributions. The majority of employees with inaccurate beliefs about the match—or 31 percent of all respondents—underestimated its generosity. We suspect that this figure is likely an overly conservative measure of the actual prevalence of match underestimation since the survey did not explicitly ask about the additional $2,000 minimum match provision which increases the match for more than half of our employee sample. Finally, the table indicates significant differences in match underestimation across baseline plan participation and take-up of the full plan match. These differences offer suggestive evidence for the importance of plan confusion in understanding savings outcomes.

5.3.2. Experimental Test of Plan Confusion

To understand the causal effect of reducing match 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 offer no indication that clarifying the match (in addition to providing a specific recommendation) led to an increase in plan engagement relative to the baseline condition (specific recommendation only) with respect to average contribution or match take-up. Additionally, clarifying the match did not lead to an increase in contributions for employees differentiated by the magnitude of the unclaimed match as proxied for by the baseline contribution rate.

5.3.3. Differential Experimental Response by Baseline Plan Confusion

As a final test of the causal role of employee plan confusion, Table 6 documents the response to match clarification exclusively for employees initially underestimating match generosity (high friction) as well as the differential response of such employees relative to their better-calibrated counterparts (low friction). The table reveals that underestimating employees did not increase contributions in response to match clarification, nor were they differentially more responsive to such clarification than employees who did not underestimate the plan match. Ultimately, the baseline survey and field experiment, suggest that while a significant share of employees appear to underestimate the generosity of the plan match, the experimental provision of information clarifying match generosity does not lead to higher savings.[32]

[32] 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 Plan Confusion on Enrollment Status

While our study was designed to test for plan confusion regarding plan eligibility and match generosity, our analysis revealed an unanticipated dimension of confusion of —remarkably, 28 percent of respondents reported a 401(k) contribution rate inconsistent with administrative records of baseline enrollment. The vast majority of such respondents, 24 percent overall, appeared to *overestimate* their actual plan contribution rate. As Table 7 details, a consequence of these discrepancies in reporting is that 37 percent of actual non-participants in our sample spuriously claimed enrollment and 22 percent of non-participants reported they were contributing at a rate that implied full take-up of the plan match.

There are at least two plausible explanations for the documented discrepancies that do not involve a massive degree of actual confusion about one's own plan enrollment status and/or contribution rate. A first possibility is that employees were simply inattentive to the survey instrument and discrepancies in reported and actual contributions reflect random error. However, the systematic direction of the bias—86% (or 0.24 of 0.28) of discrepancies involve over-reporting—and high frequency of discrepant responses at the specific contribution rate of 4 percent (selected from a menu ranging from 0 percent to 10+ percent) seem inconsistent with random error in response. To more formally consider the potential role of inattention, we can bound our estimate of confusion by counting only those discrepancies associated with the 60% of respondents who passed a fairly stringent attention screen embedded within the survey. As Table 8 details, while this restriction reduces the share of discrepant reports, among actual non-participants, of participation (from 0.38 to 0.35) and full match take-up (from 0.26 to 0.24), it fails to offer a comprehensive explanation for overall discrepancies.

A second possible explanation for the discrepancies that does not involve genuine confusion about plan enrollment, is that employees may have deliberately exaggerated their reported contribution to appear more socially desirable (possibly to the researcher). This motive for biased self-reports has long been recognized as a potential feature of experimental or survey response (e.g., Zerbe and Paulhus 1987, Peltier and Walsh 1990). Once again, the empirical distribution of discrepant reports does not, at least at first glance, appear consistent with substantial exaggeration since nearly all discrepant responses involve contribution rates situated in the low-to-middle range of an ordered response menu. To more formally assess the possible role of exaggeration, particularly among actual non-participants, we can adjust the discrepancy rate by reclassifying responses as exaggeration if they arise from respondents whose answers to other survey questions, for which one can unambiguously establish a socially optimal response, imply exaggeration. The second panel of Table 8 reports the residual discrepancy rate after adjusting for potential exaggeration implied by socially desirable responses to several questions pertaining to savings,

income, education, and financial anxiety.[33] The table suggest that even a highly expansive definition of exaggeration leaves a substantial share of residual discrepancy. Indeed, if one were to reclassify discrepant reports from any respondent who registered the most socially desirable response to *any* of the six questions in the table as exaggeration, the residual rate of discrepancy among non-participants, at 0.24, would still be substantial. As reported in the third panel of the table, restricting the sample to attentive respondents and reclassifying exaggeration using the strategy outlined above would still yield a residual rate of non-participant discrepancy of 0.23 with respect to enrollment status and 0.15 with respect to full match take-up. Ultimately, the table implies that a substantive share, ranging from roughly one-quarter to one-third, of actual plan non-participants may erroneously believe that they are enrolled.

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 administrative complexity during enrollment (or, equivalently, any adjustment to plan contribution) could impede savings if employees associate such complexity with larger psychological hassle costs. As initial evidence on the baseline perceptions of the time and effort required to enroll, Table 3 reveals that 77 percent of respondents perceived the act of adjusting enrollment to take only a few minutes. The likelihood of perceiving enrollment to only require minutes did not significantly differ across enrollment or match status. Two additional questions in the survey offer conflicting perspectives as to the potential importance of enrollment complexity on savings. When directly queried about the reasons for non-participation, less than one percent of non-participants indicated that time constraints prevented them from changing their contribution rate. However, 26 percent of non-respondents indicated that they would be very likely to save more if enrollment was less complicated. Overall, the survey points to a moderate share of employees significantly overestimating the administrative time-costs of enrollment and directly implicating enrollment complexity as a barrier to savings.

5.4.2. Experimental Test of Enrollment Complexity

While the survey offers evidence for the potential role of perceived complexity for a moderate share of employees, the field experiment an out-of-sample tests that can provide illuminate whether easing the administrative burden of enrollment causally affects plan engagement. The test involves comparing the increase in contribution among employees (moderate savings arm) randomly assigned to

[33] 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.

the generic recommendation with the out-of-sample comparisons. The comparison reveals 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 explicitly communicating that such an increase in enrollment can be implemented in a few simple steps, subsequently detailed to any interested respondent. While most respondents were already aware they were saving below the normative ideal, to the extent that the generic recommendation offered new information, it should have nudged respondents into the direction of increased contribution. Analogously, while we do not expect the first four modules of the survey to have meaningfully affect savings behavior in any systematic way, particularly given that the survey was marketed without particular reference to retirement savings and featured content stretching across many benefit programs, to the extent that it did influence employee behavior, we assume such influence would be modest and in the direction of increased engagement.

Did exposure to the generic recommendation reduce employee perceptions of administrative complexity? Table 5 documents 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 ($p < 0.05$). Table 4 shows that the experimental provision of generic guidance does not lead to a significantly higher share of employees increasing their contributions ($b = 0.03$, $se = 0.02$) relative to the non-experimental reference of pre-study contribution behavior by employees in this experimental sample ($b = 0.03$, $se = 0.01$; $p_{diff} = 0.38$). We interpret this lack of 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 represent a causal deterrent to savings for, at most, a small share of employees in the Moderate-Savings Arm.

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 6 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 present focus acts as a barrier to employee saving. Table 3 reports two indicator measures of present-focus captured by the survey which indicate a modest incidence of present focus among the sample. Our first measure of present focus is based on a modified multiple price list (MPL) measure with six hypothetical intertemporal effort allocations—half with and half without a 1-month front-end delay—identifies 9% of respondents as present-focused by their more impatient choices without the front-end delay. Considering that our abbreviated elicitation was limited to capturing present-focus within only a limited range of discount rates this incidence may not be inconsistent with the 57% incidence of present bias estimated among US adults documented by Goda et al. (2015) using a more complete but time-intensive elicitation. A second measure one could plausibly interpret as reflecting present-focus—an employee's intent to save in the near future—indicates that 21 percent of employees express a likelihood of saving within 3 months of at least 50 percent.[34]

As with the prior measures, Table 3 summarizes the correlation of these measures with plan participation and match take-up. The table shows no correlation across the MPL measure and savings, but does suggest a correlation between near-term intent to save and present savings. However, we note that this correlation likely reflects at least some reverse causality since employee intentions to save are presumably driven by their current plan engagement. Overall, the table suggests at least a modest degree of present-focus, using aggressive measures, and offers an indeterminate pattern of association between such measures and plan engagement.

5.5.2. Experimental Test of Present Focus

To gain clearer insight into the potential causal 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 (ignoring the high degree of inertia in contributions that characterizes most employee samples). Alternatively, under the same assumption that

[34] Alternative thresholds above 50 percent produce qualitatively similar results but with different employee incidence.

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 $10 reward, as a primary intervention in the low savings arm, or a secondary intervention in either the low or moderate savings arms, through the associated pairwise comparisons reported in Table 4 (and depicted in Figures 2-3). Turning initially to the primary interventions, the table indicates that 8% of employees increased their contribution following exposure to the small reward in addition to the match clarification and specific recommendation) ($p < 0.01$) while only a nominal share of employees increased savings in response to the baseline provision of match clarification and guidance ($p_{diff} < 0.005$). When implemented as a secondary intervention targeting those who had just declined to raise their contribution, the small reward led 12 percent (low savings arm, $p < 0.01$) and 16 percent (moderate savings arm, $p < 0.01$) of employees to increase their contribution rates, significant improvements in engagement when compared to the baseline response to reconsideration of 3 percent (low savings arm, $p < 0.01$, $p_{diff} < 0.005$) and 1 percent (moderate savings arm, ns, $p_{diff} < 0.01$). Overall, of those offered the small reward across any intervention, 11.8 percent increased their contribution, compared to 5.5 percent of those not assigned to a reward condition. Again, as a reference for comparison, note that respondents did not meaningfully increase engagement in response to the marginal provision of the plan match clarification.

Notably, among employees in 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 percent of respondents to the primary intervention, and 45 percent 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 6 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 recommendation)

separately for employees characterized as more (high bias) or less (low bias) present-focused as measured by the two survey proxies.

While this analysis of differential response is particularly limited by the restricted size of the survey sample, those tagged as more present-focused respond strongly to the intervention containing the reward as evaluated by either the MPL ($b = 0.50$, $p < 0.10$) or intent-to-save ($b = 0.26$, $p < 0.01$) measures. The responsiveness of these employees is substantially larger than that of employees not identified as present-focused by the MPL or intent-to-save measures. For simplicity, the reference for comparison—the experimental response to the intervention providing match clarification (and specific recommendation)—is not included in the table but these estimates, once again, suggest a nominal response and further imply that the reaction to the small reward intervention was driven by the addition of the small reward.

Ultimately, the evidence from the survey and the field speaks to a 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 two-week 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 across Frictions

To synthesize evidence across the four tested frictions we can appeal to the summary of key findings from the survey and the field offered by Table 6. Consistent with the expansive literature which motivated the present research, the table implies a moderate to high incidence, in our employee sample, of each of the candidate frictions, and, in some cases, positive correlations between the presence of frictions and adverse savings outcomes. The table offers five new insights into the factors that do, and do not, cause employees to undersave.

An initial insight is that the table presents some of the first evidence to reject any meaningful, causal, influence of low retirement literacy on employee savings. We do confirm assertions of the literature on financial literacy and education in finding pervasive evidence for the presence of low retirement literacy—nearly three-quarters of employees appear to directly, or indirectly, underestimate the minimum annual contribution required to avoid retirement poverty (this implies an even greater underestimation of how much one would need to save to ensure a comfortable retirement) and a significant share of employees scored zero on measures of financial literacy (a far larger share of

employees, roughly equivalent to the share who underestimate savings requirements, scored either zero or one). Further corroborating the literature, we find that scoring low on the financial literacy strongly predicts financial outcomes, in this case plan participation and match take-up. However, despite the prevalence of low retirement literacy (as measured through retirement-relevant beliefs and financial literacy), and the correlation between financial literacy and savings, our experiment suggests that addressing such deficits through specific savings recommendation that improves the accuracy of beliefs *does not* meaningfully increase average savings nor does it increase savings among those explicitly tagged as subject to this friction.

What might explain why improving the accuracy of retirement-relevant beliefs may not actually lead employees to increase their rate of savings? Figure 4 illustrates one possible explanation. The figure compares the perceived savings rate to the actuarially recommended rate (red line), the indirect estimate of required savings generated from a retirement calculator informed by subjective employee beliefs (blue line), and the directly elicited estimate of required savings (orange line). The figure shows that while employees underestimate savings requirements, whether directly or indirectly, such underestimation does little to close the large gap between perceived and actuarially recommended savings. Said differently, the large majority of employees—79 percent (direct measure) and 88 percent of employees (indirect measure)—appear to recognize that they are undersaving despite their biased beliefs of how much to save. Indeed, the elicited beliefs indicate that most employees recognize that they are under-saving by a fairly substantial margin. This is illustrated by a simple mean-decomposition which suggests that of the 14.1 percentage point average gap between actuarially recommended and perceived actual savings, only 3.3 percentage points, or 23.6 percent, is bridged by the direct underestimation of savings – that is, on average, employees perceive the gap between normative and actual saving rates to be 74.4 percent of its actual magnitude.[35]

One reason that deficits in retirement literacy may not causally explain a significant share of low savings is that despite biased beliefs and low financial literacy, most low saving employees both recognize their low savings and the substantial gap with normative savings targets. The survey offers one additional empirical insight to help explain why the significant inaccuracies in employee beliefs of retirement-relevant inputs lead to only moderate, but not overwhelming, underestimates of how much to save. The insight is that employee biases in retirement-relevant inputs appear to have off-setting

[35] While biased beliefs do not appear to explain much of the mean difference between actuarial recommendations and perceived actual savings, the figure also hints that variation in employee beliefs may not explain much of the in-sample variation in the degree of undersaving. While the actuarial recommendation explains only 0.001 of the variation in an employee's perceived actual savings, as indicated by adjusted R-square from a simple bivariate regression, neither the addition of the direct (adj R-squared = 0.001) or the indirect (adj R-squared = 0.016) estimate of how much to save meaningfully increases the explanatory power of a simple, additive, linear model.

implications for normative savings requirements—that is, while over-optimism in employment longevity implies a reduced need to save while over-optimism about life longevity implies a greater need to save.

A second insight from the analysis is that we offer evidence arguing against *plan confusion* about plan benefit details as contributing to undersaving. While we document substantial confusion about plan details, notably involving employees underestimating the generosity of the plan match, and while underestimation of the plan match, strongly, and significantly, predicts low participation and full match take-up, experimental clarification of the match does not result in increased average, or differential, enrollment despite successfully shifting employee beliefs. This evidence suggests that the current industry practices of investing heavily in the marketing and communication of benefit program details may have limited effect, at least through their informational channels, in leading to increases in plan engagement.

The third insight considers the role of the administrative complexity of enrollment and contribution adjustment. While we document that about one-quarter of employees in our sample perceive enrollment to require a non-trivial amount of time (i.e., 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. Given this narrow definition of complexity—that is, the time-costs, and any associated psychological hassle costs, associated with administrative enrollment/contribution—our evidence suggests that it does not play a meaningful role in deterring plan engagement at this firm.

A fourth insight highlights a factor that is largely absent from discussion by researchers, but does appear to contribute to low plan engagement. As indicated by the table, we document substantial confusion among employees about their enrollment status. Of all employees in the sample, 28 percent have inaccurate beliefs about their current contribution rate, including over one third of non-participants who report nonzero plan contribution rates. 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.

The presence of substantial plan confusion regarding enrollment status may seem more reasonable 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 corroborate 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 6, employees who overestimated their contribution rates, and who were randomized into the small reward condition, were more than three times as likely to increase their contribution ($b = 0.21$, $p < 0.01$) than their counterparts with accurate beliefs ($b = 0.06$, $p < 0.01$), ($p_{diff} < 0.01$). This pattern is consistent with the possibility that many confused employees, after learning of their actual administrative enrollment/contribution status, due to a reward-induced visit to the enrollment portal, modified their contribution to reflect their original belief.[36]

Finally, and perhaps most centrally, the table provides some of the first direct evidence, using time-varying incentives, that present focus constitutes an important barrier to plan engagement for perhaps a moderate share of employees. 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. That one-quarter to one-third of low-saving employees registering high on this friction responded to the small financial incentive is notable given the general insensitivity of this population to the (delayed) financial incentives tied to the design of the program. 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. We explore the mechanisms that might underlie the differential response to the small reward, and clarification of the match, in the next section.

6 INVESTIGATING MECHANISMS UNDERLYING PRESENT FOCUS

A striking finding of the present research is that a non-trivial share of employees increase their 401(k) contribution, often by a significant margin, after exposure to a small, but immediate, financial incentive, but not information clarifying large, delayed, matching incentives. The majority of the employees in our study neglected to fully take-up generous plan matching incentives for many months to years – a failure that similarly characterizes a significant share of 401(k) plan eligible employees across

[36] 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.

US firms more broadly. What mechanisms underlie the apparent present focus of employees in our sample and, perhaps, undersaving employees more generally? To begin this inquiry, we first evaluate whether the theoretical framework described earlier, in which a utility maximizing employee suffering from potential present bias and possibly large hassle costs associated with enrollment, can account for the observed behavior (for simplicity, and in deference to the evidence from the field study, we assume away the presence of other frictions such as low retirement literacy, plan confusion, and enrollment complexity). 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. We then outline alternative theoretical accounts of present focus and critically consider whether any offer greater promise for reconciling the present findings.

6.1. Calibrating the Beta-Delta Model of Enrollment

6.1.1. Baseline Enrollment and Match Take-up

We first investigate whether the baseline decision of employees in our sample to delay enrollment, despite the generosity of the plan match, could be rationalized by the theoretical framework presented in Section 3. Once again, we restrict our consideration to the simplified decision of an employee deciding whether to either delay enrollment or to enroll at the match threshold of 4 percent. We initially contemplate the decision of an exponential discounter and then turn to a present-biased employee with beta-delta preferences (with and without psychological hassle costs).

Standard Model Baseline. Recall that an employee governed by the standard baseline model, described above, will compare the expected presented value of future utility associated with enrolling in a savings plan against the costs, *k*, of enrollment and either enroll immediately or never:

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

To be more concrete, consider the case of a representative employee earning $50,000 annually, or approximately $25 per hour, subject to an effective marginal tax rate of $\tau_0 = \tau_R = 0.25$ both 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).[37] We conservatively assume an annual discount factor $\delta^{365} = 0.93$, corresponding to a daily discount factor of $\delta = 0.9998$.

The decision rule implies that, due to the generosity of the plan match, an employee should enroll so long as enrollment costs do not exceed $37,493, or roughly 75% of the employee's pre-tax annual

[37] 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.

income. Given that the majority of employees in the sample perceive enrollment as requiring a matter of minutes, so long as *k* reflected the time-costs of administrative enrollment, the decision rule would dictate that most employees would immediately enroll. For example, assuming 25 minutes for administrative enrollment generates an approximate $k \approx 10$ (i.e., 25/60 * $25/hour), while allowing for an additional 2 hours for deliberation as to how much to save would result in an approximate $k \approx 60$ (i.e., [120+25]/60 * $25/hour). In either case, k is unambiguously less than the foregone benefit of never enrolling.

What if the employee associates enrollment with hassle costs whose equivalent financial value exceeds administrative time-costs (e.g., Bertrand and Mullainathan 2004)? As a rough approximation of the magnitude of such costs, we appeal to recent research attempting to estimate the hassle costs associated with itemization of federal taxes, another complicated, and for many, unpleasant, financial task (Benzarti 2017). The analysis estimated that, for the typical tax-payer, aversion to itemization had a revealed price equivalent to about 4 times the economic time-cost associated with itemization. Given the decision rule above, hassle costs derived from anything close to this multiplicative factor could not rationalize the decision to never enroll.

Present-Biased Employee. We now consider the enrollment decision of an otherwise similar employee with present-biased preferences. Recall that an employee with sophistication ($\hat{\beta} = \beta < 1$) will delay enrollment for a maximum of t^* days:

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

For an employee facing costs of enrollment restricted to administrative time-costs, $k = 10$, or the time-costs of both administrative enrollment and deliberation, $k = 60$, to delay enrollment by two weeks, the length of a single pay cycle, would require $\beta = 0.11$, or $\beta = 0.42$, respectively. Either estimate indicates a degree of present bias more severe than the 0.5 to 0.9 range asserted by estimates from the field (see Dellavigna 2018) or the typical estimate of 0.9 from real effort tasks in the lab (e.g., Augenblick et al. 2015, Augenblick and Rabin forthcoming). Rationalizing a delay in enrollment of two pay cycles would require a $\beta = 0.06$ assuming $k = 10$ and $\beta = 0.26$ assuming $k = 60$.

Can present bias rationalize the decision to delay enrollment in the presence of psychological hassle costs? The first panel in Figure 6 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 either $10 (solid line), $60 (first dashed line), or $240 (second dashed line). The latter curve reflects the conservative case where hassle costs of enrollment amount to 4x the time-costs of enrollment and deliberation. The model implies that an employee with a beta of 0.5 or higher should expect to delay enrollment for no more 2 days, in the case of $k = $10, and, no more than 40 days in the extreme case of *k*

= 240. For a more plausible beta of 0.7, the calibrations, as labeled in the figure, imply a delay of no more than 1 day, in the case of k = \$10, and 17 days if k = \$240. A beta of 0.9 implies a delay of no more than 4 days even if k = \$240. Considering that most of the non-participants in our sample had failed to enroll for at least several months prior to the administration of the survey, and in some cases far longer, suggests that the presence of sophisticated present bias, as modeled here, cannot account for the baseline behavior of employees.

An alternative explanation for why employees might persistently delay enrollment is that they may be naïve to their present bias. The possibility of understanding lengthy delays in 401(k) enrollment through a traditional beta-delta framework by assuming naiveté, and/or significant hassle costs associated with enrollment, has been recognized in the literature, most notably by O'Donoghue and Rabin (1999) and DellaVigna (2018). In the envisioned scenario, employees might intend to save in the very near future but would fail to follow-through due present bias and an absence of sophistication.

Our study offers an empirical test of this possibility via two survey measures designed to measure employee intentions for future saving. The grey bars in Figure 6 depict the first of these measures. The bars depict the cumulative empirical distribution of the earliest expected time-horizon by which plan non-participants indicated that they were at least "moderately likely" to enroll. The plot reveals that only 9 percent of surveyed employees intended to save within a month while only 40 percent intended to save within 6 months. A second survey measure, not depicted in the figure, presents a similar characterization of employee intent in indicating that only 21 percent of employees were more than 50 percent likely to increase their contribution within the next three months. While the large majority of non-participating employees appear to intend to enroll eventually, most recognize that they will not be able to do for many months or longer. Overall, a model which presumes employees act in accordance with the standard economic model, but for the presence of naïve present bias and psychological hassle costs of enrollment, still has difficulty explaining the non-participation of employees who do not intend to save in the immediate future.

6.1.2. Experimental Response to the Small Reward

The actions of employees in the field experiment offer, perhaps, a more straightforward test of the beta-delta framework for understanding savings. For an employee induced to save by a small, immediate, reward, but not clarification of a much larger, but delayed, plan match, the perceived net present-value of the benefits associated with enrollment must fall within ten dollars of the perceived costs of enrollment. To illustrate, consider an employee earning \$50,000 annually with a beta of 0.9 who originally intends to enroll in six months but decides to enroll immediately after exposure to the small reward. A model of present-bias could rationalize this behavior only if an employee associates enrollment with hassle costs

whose value falls within the implausibly precise, and implausibly high, interval between $12,140 and $12,150. Permitting moderate underestimation of the benefits of enrollment, or the generosity of the match, would not help the model to plausibly account for the observed response of employees. Overall, the calibrations reject the plausibility of explaining baseline plan non-participation, or the response of employees to the experimental provision of the small reward, with the widely used beta-delta framework.

6.2 Alternative Frameworks for Understanding Present Focus

Are there alternative models of present focus that more accurately account for the observed behavior of employees in the sample? Researchers across disciplines have advanced models of inter-temporal decision making that imply a privileged treatment of the present. We briefly outline the key alternate approaches for modeling present focus informed from research in economics, psychology and decision science, and speculate as to the potential for each approach to deliver descriptively accurate accounts of employee savings behavior. We conclude by suggesting a new model of savings behavior informed by the existing literature and the findings of the present research.

Economic Models. The prevailing practice within economics for modeling present focus in the context of inter-temporal decisions involves the invocation of time-varying preferences, often implemented, as with the present research, through the assumption of hyperbolic discount rates (e.g., beta-delta time-preferences). As discussed, such models can, in theory, predict a failure to take seemingly beneficial actions if such benefits are significantly small and delayed. Economists have, however, proposed alternative approaches for modeling time-varying preferences that might similarly predict a failure to act despite the presence of delayed benefits. One approach is to assume that inter-temporal decisions involve a significant initial fixed cost (Benhabib et al. 2010). While such fixed costs could theoretically lead to inaction, our calibrations suggest to explain the non-enrollment of employees in a setting with highly generous matching incentives, the fixed costs in such a model would need to be extremely large, effectively approaching the discussed magnitude of hassle costs. Models with context-specific discount rates tied to factors such as affect (Vallacher 1993, Loewenstein 1996) or poverty (Banerjee and Mullainathan 2010) offer perhaps a more promising strategy for explaining savings delay via time-varying preferences. In such models, it is possible that employees discount dollars associated with retirement accumulation differently than those associated with the costs of enrollment or an immediate small reward in the form of a gift card.

Another established approach in economics for modeling present focus is to explicitly include distinct, competing, systems of decision-making that differ in their operative time-horizon (e.g., one system is more myopic than the other) or manner of decision-making (e.g., one system is more patient

than the other) (e.g. Shefrin and Thaler 1988 and Fudenberg and Levine 2006). Generally speaking, for models in which behavior emerges from the strategic interaction between competing selves, a delay in savings would likely imply that the preferences of the myopic, or impatient self, dominate those of the more deliberate, or patient self. One practical challenge for applying such models to explain enrollment delay is that while actual delays could be explained by the ascendency of the myopic, or impatient self, it is less clear how to explain the discrepancy between intended and actual behavior.[38]

Psychological Models. Researchers have proposed a number of psychological mechanisms that could generate dynamically inconsistent decisions that arguably reflect more complete departures from the standard economic model (see Urminsky and Zauberman 2014 for review). For example, such dynamic inconsistency could emerge from the influence of affect (Loewenstein 1996; Shiv and Fedorikhin 1999; McClure et al. 2004), differential construal of immediate versus delayed outcomes (e.g., Liberman and Trope 1998, Malkoc et al. 2005; Malkoc and Zauberman, 2006), the heightened psychological distance between present and future selves (Parfit 1984, Hershfeld et al. 2011), the expectation of greater resource flexibility in the future rather than the present (Zauberman and Lynch 2005), or failures to plan (Lynch et al. 2010). Depending on details of implementation and specific underlying assumptions, these mechanisms, or some combination of them, could predict the propensity to delay enrollment in the context of a generous match and possibly even the discrepancy between intentions and actual behavior.

6.3. An Alternative Account of Present Focus involving Anxiety and Delayed Optimism

We propose an alternative model of present focused savings that potentially offers an integrated explanation for the savings behavior of employees in the current setting and the broader set of retirement savings puzzles. The model is informed by descriptive facts from national surveys documenting the financial well-being of US households and theoretical insights from interdisciplinary research on inter-temporal decision-making. While we do not explicitly test the model experimentally in the present paper, we assess the model's ability to account for the baseline and experimental behavior of employees in our field sample, relative to the benchmark model of the prevailing beta-delta framework, and present ancillary evidence from the field and the lab to assess the motivating assumptions and behavioral predictions of the proposed model.

Two assumptions conceptually distinguish the proposed model from most other models of present focus. First, the proposed model presumes that perceived anxiety about one's financial situation is widespread and that this anxiety exerts a psychic cost on major financial decisions such as that of

[38] 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)

enrolling in a 401(k) plan. Second, the model presumes that individuals are systematically, and perhaps *overly*, optimistic about their future anxiety over intermediate, but not immediate horizons. That is, we expect that employees, particularly those currently experiencing high levels of anxiety, tend to anticipate such anxiety subsiding in the intermediate future (i.e., on the order of weeks or months) rather than immediate (i.e., on the order of days) future and we allow for the possibility that this optimism reflects a forecasting error.

The general phenomenon of anxiety, or stress, has been widely studied as a neurophysiological and psychological construct with a significant influence on judgement and choice. 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), researchers have associated anxiety with a number of brain regions also implicated with emotional regulation (Park et al. 2016) and other aspects of decision-making including attentional control (Eysenck et al. 2007), memory (Wolf 2009), and executive function (Arnsten 1998). While researchers have suggested several specific cognitive and motivational channels through which anxiety might influence behavior, one widely-theorized behavioral response to anxiety is avoidance of anxiogenic stimuli (e.g., Hartley and Phelps 2012). Indeed, one recent animal study claims to have documented a direct pathway between neurons in the hippocampus—a region of the brain believed to play a role in mood-related decisions—that encode anxiety-related information to regions of the hippocampus responsible for avoidant behavior suggesting that the link between anxiety and avoidance may be a hard-wired feature of our neural circuitry (Jimenez et al. 2018).[39]

Financial anxiety, or anxiety specifically associated with one's financial situation, exists as a distinct construct, at least according to one study, from general anxiety and depression (Shapiro and Burchell 2012). The prevalence of this particular form of anxiety has been documented by several US household surveys. For example, the 2018 National Financial Capability Survey, a national survey of over 25,000 individuals conducted every three years, found that 53 percent of respondents reported a 5 or higher on a 7-point scale when asked if they agreed that "thinking about my personal finances can make me feel anxious" while 44 percent reported a 5 or higher when asked if they agreed that "discussing my finances can make me heart race or make me feel stressed" (2019 NFCS). Simple bivariate correlations from the NFCS survey data further suggest that feelings of financial anxiety are not strongly correlated with linear indices of self-reported categorical income (corr = -0.23), math ability (corr = -0.15) or financial knowledge (corr = -0.26).[40] Given that self-reports of financial anxiety do correlate with implicit

[39] While research on the effects of anxiety or stress in economic domains is scarce, stress has been associated with increased risk aversion and impulsivity in the lab (Haushofer and Fehr 2014).

[40] Based on author calculations from 2018 NFCS state data extract retrieved from the FINRA Foundation website. Income was measured using an 8 point categorical scale from less than $15k to more than $150k while math ability and financial knowledge were both measured using 1 to 7 scales. Figures, and estimates, exclude the small share of respondents reporting either "don't know" or "prefer not to say".

cognitive measures of such anxiety (Shapiro and Burchell 2012), financial anxiety appears to be a theoretically distinct, widespread, and measurable phenomenon.

A consequence of high present anxiety, coupled with delayed optimism regarding future anxiety could, according to the model, compel otherwise well-informed, and rational, employees to delay plan enrollment despite the intent to save in the intermediate future. In theory, if employees are overly-optimistic about how quickly they will achieve relief from their anxiety, the model could predict lengthy delays in enrollment. For example, an employee who naively persists in thinking that relief is just a few months away, when in actuality such relief is a few years away, might delay enrollment for a lengthy duration, despite a stated intent to save at an earlier date. The framework also provides a possible explanation for the surprising responsiveness of employees to the experimental provision of a small reward (and not a far more valuable, but very delayed plan match). Research on the neural correlates of choice in the context of immediate or delayed incentives suggest that immediate rewards, particularly non-monetary ones, activate parts of the limbic system, typically associated with regulating emotion and experiences of anxiety, whereas delayed rewards engage the prefrontal cortex. This suggests that the gift card might actually motivate behavior by either relaxing financial anxiety or distracting from it.

Model Setup. We now more formally outline this alternative of anxiety-induced present focus, delayed optimism, and enrollment. We begin by adopting the simplifying initial assumptions and decision structure of the earlier theoretical framework: (i) we discuss the stylized decision of a utility-maximizing employee to enroll either now or later in a retirement savings plan resembling the 401(k) plan offered by our firm partner—a plan in which the employer matches pre-tax contributions, dollar-for-dollar, up to four percent of annual salary, (ii) we treat enrollment as involving a contribution of 4 percent for an employee earning an annual salary of $50k so that the decision to enroll effectively entails full take-up of the available plan match, (iii) we assume that the long-run discount rate equals the interest rate, and (iv) finally, we normalize the constant marginal utility of consumption to 1 so that the net utility gain from a dollar of contribution for such an employee is identical to that described in the original framework.

The innovation in the model is the inclusion of a parameter, θ_t, intended to represent the presence of financial anxiety, a phenomenon associated with potentially significant psychological costs of enrollment (and/or saving more generally). We assume that anxiety can take an either high or low value in each period, t, such that $\theta_t \in \{\theta_t^H, \theta_t^L\}$ and that the economic and psychological costs associated with enrollment, described by $f(\theta_t)$, are an increasing function of present anxiety, so that $f(\theta^L) < f(\theta^H)$. Given the focus on financially at-risk individuals, we assume that every employee experiences high financial anxiety in the initial period so that $\theta_0 = \theta^H$. Intuitively, one can think of $f(\theta_t)$ as replacing the original cost of enrollment parameter, k, in the earlier framework.

We assume that the transition from high to low anxiety is a stochastic process whereby the likelihood that an employee transitions in each period follows a geometric hazard function. Specifically, suppose t_s is a positive, discrete, random variable, that denotes the duration, in days, until the transition to low anxiety, so that $t_s \sim Geo(\lambda)$. A well-informed employee should then expect to wait an average of $\hat{t}_s = E(t_s) = 1/\lambda$ days where λ denotes the per-period transition rate conditional on having not previously transitioned. For example, if the likelihood that an employee transitions to a low state of anxiety, due to some favorable change in economic circumstances or non-economic improvement in hedonic outlook, is 1/100 each day, then the expected time until transition would be 100 days. The specific magnitude of the example would be reasonable if employees did exhibit optimism over intermediate (weeks to months) but not immediate (days) horizons.

We also acknowledge the generic possibility that many employees may be systematically *overly*-optimistic about their future hedonic state— that is, the possibility that employees, on average, underestimate how long they must wait for hedonic relief, such that for a given employee $\hat{t}_s < t_s$. There is considerable research asserting the presence of biased forecasts of hedonic outcomes since such outcomes likely emerge from intuitive, rather than deliberative, decision processes (Kahneman, 2003; Gilovich, Griffin and Kahneman, 2004). While researchers have not, to our knowledge, asserted our specific trajectory of delayed optimism, particularly with respect to financial anxiety, there is a robust literature attesting to the propensity of people to exhibit optimism, and indeed, over-optimism, about personally-relevant future outcomes (Sharot 2011).[41]

The use of a duration model, and specifically, a geometric hazard function, to describe the transition between two discrete states captures some desirable psychological features of anxiety—e.g., the non-linear influence of anxiety on behavior, the uncertainty of future financial anxiety, and an increasing likelihood of relief over time. The assumption of a constant per-period hazard rate arguably does sacrifice some psychological realism for tractability—e.g., one may expect that the likelihood of relief in a given period may increase over time or might otherwise follow some non-uniform pattern—but would not materially affect the behavioral prediction of the model so long as employees exhibit meaningful optimism about their hedonic well-being over intermediate horizons (in a subsequent section, we plot the dynamics of employee hedonic forecasts to assess the reasonableness of the assumption). To simplify how the individual engages uncertainty about future anxiety, we assume that individuals are risk neutral over the small to moderate financial stakes under consideration.

We can specify the total utility of an employee, under the specified assumptions, and after normalizing the utility of never saving to 0, as:

41 Sharot notes that the one group for which the optimism bias appears absent are those suffering from depression (2011).

$$\max_{s\in\{1,0\}} U_t = \begin{cases} -f(\theta^H) + \sum_{t=1}^{\infty} \delta^t b \quad if\ s = 1 \\ -f(\theta^L)\delta^{\hat{t}_s} + \sum_{t=\hat{t}_s}^{\infty} \delta^t b \quad if\ s = 0 \end{cases}$$

Here, *s* indicates whether an employee enrolls now (1) or delays enrollment until later (0), *b* provides the per-period utility associated with savings (identical to that specified earlier), and $\hat{t}_s$ represents an employee's expectation of the number of days until the transition to low anxiety. In contrast to the standard model baseline from earlier, which predicts that an employee who decides not to enroll immediately will never enroll, the new model predicts that an employee might rationally decide to delay enrollment until a future date when the employee expects to transition to a low anxiety state.[42]

Enrollment Decision Rule. We can simplify the decision of an employee to enroll now, or delay enrollment until the transition point, as a comparison between the expected marginal cost and benefit of delay. We can express the expected marginal cost of delay as the discounted present value of foregone benefits associated with the plan match, $\sum_{t=1}^{\hat{t}_s} \delta^t b$. The expected marginal benefit of delay can be thought of as the discounted present value of the expected reduction in anxiety-induced costs of enrollment, $[f(\theta^L)\delta^{\hat{t}_s} - f(\theta^H)]$. The resulting comparison implies that a risk-neutral employee, presently subject to high financial anxiety, will save now, rather than later, if the expected cost of delay exceeds the expected benefit of delay:

$$f(\theta^L)\delta^{\hat{t}_s} - f(\theta^H) < \sum_{t=1}^{\hat{t}_s} \delta^t b$$

An employee choosing to delay will expect to enroll in $\hat{t}_s$ days (or never). For additional tractability, we can assume δ=1 and normalize the discounted cost of enrolling later—that is, in a state of low anxiety—to zero. These simplifications imply that an employee should enroll if the following condition is satisfied, where $f(\theta^H)$ now reflects the *difference* in expected enrollment costs across states of anxiety:

$$f(\theta^H) < \hat{t}b$$

The first panel of Figure 7 provides graphical intuition for the decision rule. The figure shows that a well-informed, risk-neutral employee would delay enrollment if they expected relief from anxiety prior to the date by which the accumulated foregone benefits of enrollment, $\hat{t}b$, exceed the expected reduction in costs associated with less anxious enrollment, $f(\theta^H)$, such that $\hat{t}_s < t^*$. If an employee

[42] In theory, an employee might anticipate not transitioning to a state of low anxiety until some point after retirement in which case the employee's utility would either be 0 or the expression associated with *s=1*.

expected that relief would not occur until the threshold date, then the employee would enroll immediately. The figure also helps to depict how the presence of biased beliefs about the generosity of foregone benefits, specifically involving an underestimation of such benefits, $\hat{b} < b$, would affect the decision rule. Conceptually, any underestimation of benefits would reduce the expected marginal cost of delay leading to a flattening of the cost of delay curve and a later threshold date, $t^* < t_l^*$. As a consequence, more employees would be willing to delay enrollment because they satisfied the decision rule, $\hat{t}_s < t_l^*$.

Model Predictions. Beyond the two empirically testable assumptions that motivate the model—i.e., the widespread presence of financial anxiety and delayed optimism about future hedonic states—the model generates two testable predictions pertaining to the financial anxiety, savings, and beliefs of employees. First, the model predicts that higher levels of present financial anxiety should lead to lower levels of present engagement with a retirement savings plan. An implication is that we should expect to observe a positive correlation between self-reported levels of financial anxiety and enrollment status among the employees in our field sample. Second, the model predicts a positive correlation between the timing of an employee's optimism regarding future changes to anxiety and their intentions to save – that is, employees, not presently saving, should intend to save around the time when they anticipate relief from their financial high anxiety.

6.3. Calibrations of Anxiety and Delayed Optimism Model

As initial evidence as to the descriptive plausibility of the proposed framework, we can gauge whether the model could plausibly rationalize the savings behavior, and stated beliefs, of the employees in our sample. Of particular interest is the value of psychological costs of enrollment implied by the present model relative to the beta-delta framework. Specifically, we return to the representative case of the employee earning an annual salary of $50k facing the decision to enroll in a savings plan at a rate of contribution rate that implies full take-up of the plan match and adopt previously stated assumptions regarding marginal tax rates, marginal utility, and long-term discount rates. The second panel of Figure 7 depicts the disutility enrollment disutility implied by the anxiety and delayed-optimism model to rationalize varying delays in intended enrollment, measured in calendar days. The panel also compares the implied costs of enrollment in the new model with the beta-delta framework, assuming sophistication, and a beta of either 0.9 or 0.7.

The panel conveys the intuition that models in which employees differentially discount the immediate and more distant future can only accommodate short intended delays in savings (x-axis), unless one permits extreme disutility associated with the psychological costs of enrollment (y-axis). For example, assuming a beta of 0.9, the beta-delta model implies a disutility of enrollment of $3,471 to accommodate an intended delay of 90 calendar days (about 64 business days), and $6,943 to

accommodate a delay of six months (about 129 business days). An alternative account, in which employees delay enrollment because of high anxiety-related costs of immediate enrollment, and optimism about the likelihood of relief in the intermediate future, can accommodate intended delays with more plausible implied values of enrollment disutility (or differences in expected disutility associated with a high and low state of anxiety) – $386 for 90 days and $771 for six months. Further assuming some retirement illiteracy, such that employees underestimate the economic benefits of the plan match (i.e., $\hat{b} < b$), would reduce the slope of the enrollment cost curve and lower the implied costs of moderately delaying enrollment even further.

Conceptually, the figure conveys that to rationalize the joint presence of low savings and an intent to save in the intermediate future, with the beta-delta framework, assuming employee sophistication (the absence of near-term intentions to save would seem to be inconsistent with employee naiveté), would require an either implausibly low value of beta, or an implausibly high level of enrollment disutility. In particular, the presumption of sophistication implies that employees, particularly young ones, engage in an implausibly complicated decision process involving backwards induction across innumerous periods (or some psychological process that yield equivalent behavior). The alternative model, on the other hand, could rationalize moderate delays in savings without aggressive assumptions about high enrollment disutility, a low beta parameter, or complicated, iterative decision processes.

6.4. Evidence on Model Assumptions and Predictions

We present empirical evidence pertaining to the core assumptions and predictions of the model. An initial set of evidence helps to assess two assumptions motivating the model—whether the experience of present financial anxiety is widespread and whether individuals, particularly those with high present anxiety, systematically exhibit optimism about their intermediate, but not immediate, hedonic future. A second set of evidence addresses model predictions regarding the relationship between present anxiety, forecasts of future anxiety, and actual, and intended, savings behavior.

Evidence on Model Assumptions. To test the plausibility of model assumptions pertaining to financial anxiety, we elicited measures of present, and forecasted, financial anxiety from 575 employee respondents to the field study randomly assigned to a module focused on hedonic assessment. The module revealed that 93% of respondents reported at least "a little" anxiety (i.e., 2 or higher on a 4 point scale), while 56% reported either "a fair amount" (32%) or "a lot" (24%) of anxiety. The prevalence of at least a moderate amount of financial anxiety echoes the 53% share of moderate to high financial anxiety reported by the far larger, and more representative, sample described in the 2019 NFCS.

When, if ever, do individuals, particularly those experiencing heightened financial anxiety, expect relief from such anxiety? Our field survey asked employees who answered the original question about

present anxiety one of two, randomly assigned, follow-up questions, inviting them to forecast whether they would experience "more", "less" or the "same amount" of financial anxiety in either three or six months compared to now. The between-subject comparison indicated employees were, on average, modestly pessimistic about their hedonic prospects in three months (17% of respondents expected to experience more anxiety, while 13% expected to experience less, for a net pessimism share of 0.04), but became more optimistic about their hedonic prospects in six months (12% of respondents expected less anxiety, while 19% of respondents expected more rather, for a net pessimism share of -0.07). Among employees reporting "a lot" of present anxiety, no employees expected relief over a 3-month horizon, while 32% of respondents expected their situation to worsen, for a net pessimism share of 0.32. These employees expressed relative optimism over a lengthier horizon of six-months—15% of respondents expected relief, while 20% of respondents expected greater anxiety, reducing the net share of pessimism to 0.05. These data suggest that while few employees anticipated relief in the next few months, particularly those with high levels of present anxiety, employees were more prone to towards optimism about their hedonic states in the more intermediate term.

To generate within- rather than between-subject forecasts of financial anxiety over lengthier time-horizons from a larger, more representative sample, we surveyed several hundred employed (either part-time or full-time) US adults, aged from 25 to 55 years old, from Amazon Mechanical Turk in November 2019. The instrument was generically advertised as a short decision-making survey to limit the likelihood of selected response. The resulting sample of n = 905 featured a higher, and more representative, share of men, and a modestly lower share of full-time employees, than the field sample, but was similar in age and imputed income.[43] The survey captured experiences of present financial anxiety on a scale from 1 "not at all anxious" to 5 "extremely anxious" and then asked respondents to forecast whether their sense of anxiety would be, relative to now, higher, lower, or the same at varying dates ranging from one month to one year in the future. Specifically, all subjects were asked to forecast the relative change in their anxiety in one month and in one year and were randomized as to whether they were also asked to make a forecast for three months, six months, or both three and six months.[44]

As depicted in the first panel of Figure 8, the survey, once again, suggested the pervasiveness of at least some anxiety about their financial situation—50% of respondents indicated feeling somewhat or moderately anxious (i.e., 3 or 4 on the 5-point scale), while 16% expressed feeling extremely anxious (i.e., 5 on the 5-point scale). To characterize the net optimism of respondents over future time horizons,

[43] The survey also included random assignment to a task unrelated to the present study. Demographic sample summary: (i) gender (0.53 male, 0.47 female), (ii) age (mean: 34.7 years, sd: 7.8 years), (ii) employment status (0.86 full-time, 0.14 part-time), and (iii) estimated salary imputed from midpoints of categorical ranges with bounds of $25k and $150k ($53.8k, sd: $27.0k).
[44] We adopted this mix of between- and within-subject designs to guard against, and test for, any respondent fatigue, or scale insensitivity, that could have been generated after asking for hedonic forecasts over multiple future time-horizons.

The second panel of the figure communicates the net share of respondents forecasting an increase in anxiety (i.e., the share indicating higher anxiety less the share indicating lower anxiety) for all respondents (grey line), and separately, by level of present anxiety. For example, the grey line in the figure indicates a net share of pessimism for the 90-day outlook, across all employees, of -0.16 which reflects the 15% of employees expecting an increase in anxiety and the 31% expecting relief.

The figure elaborates upon the pattern suggested by the field survey. Individuals, on average, appear non-optimism, or even pessimistic, over short-horizons (one to three months), but do become optimistic over intermediate and longer-run horizons (three months to a year). This pattern of delayed optimism in aggregate masks significant heterogeneity across individuals distinguished by their levels of present anxiety. As the figure depicts, those reporting high levels of present anxiety exhibit the most pronounced shift from near-term pessimism to optimism over more distant horizons. Among those reporting low anxiety, or no anxiety, in the present, employees were optimistic, on average, over all future horizons. When asked to forecast their level of financial anxiety in a year relative to now, the average respondents in every response category of present anxiety, and 46% of respondents overall, anticipated improvement in their hedonic state. In comparing forecasts of one-year and one-month, individuals were 3.8 times more likely to believe their anxiety would improve, rather than worsen, in the more distant, relative to the near-term future. This ratio of distant and near-term future escalated to 7.4 for those reporting high present anxiety. To our knowledge, this analysis offers the first empirical evidence asserting this phenomenon of delayed optimism with respect to future financial anxiety, particularly among those with high present anxiety.

Overall, the supplemental survey, which reveals a hedonic trajectory approximately consistent with the more limited data from the field sample, corroborates the model's assumption of widespread present anxiety and systematic, but delayed, optimism with respect to the future. While one might argue that this hedonic trajectory may have emerged idiosyncratically due to the particular economic circumstances during November 2019 (when we administered the survey), we note that the supplementary survey was administered two-years after the field survey. Moreover, a smaller pilot survey, conducted in November 2017, but not depicted in the figure, revealed qualitatively similar patterns of high present anxiety and delayed optimism. Collectively, these three large-sample surveys, administered at different dates, generated the same hedonic pattern.[45] Does the delayed optimism exhibited by individuals reflect a

[45] One could plausibly argue that this pattern of things getting worse before they get better might be specific to the economic environment in the US in November 2019—that is, survey respondents could, justifiably, have correlated beliefs about their economic prospects that are informed by exposure to common economic shocks. However, this sinusoidal pattern of hedonic forecasts appears surprisingly robust to the timing of the survey. As an example, in an earlier pilot survey, we canvassed US adults from Amazon Mechanical Turk (N=683) in November 2017. We observed slightly higher anxiety in the present but similar patterns of delayed optimism in the long-term relative to near-term. The survey actually captured forecasts up to 2 years in the future, by which point respondents were significantly more likely to be optimistic than pessimistic about future anxiety.

forecasting error? While our surveys do not permit longitudinal tests of (ex post) over-optimism, one can speculate that the persistence of high present anxiety across our surveys, and national household surveys, and the uniform display of (delayed) optimism across different levels of present anxiety, suggests that at least some of the delayed optimism observed reflects biased hedonic forecasts. Such bias would be consistent with the various errors routinely asserted in the broader literature on hedonic forecasts.

Evidence on Model Predictions. We now assess the accuracy of the two model predictions pertaining to retirement savings behavior—a negative correlation between present financial anxiety and plan engagement and a positive correlation between the timing of improvements to future forecasts of anxiety and intentions to save. Returning to employees in the field sample, the first panel of Figure 9 offers evidence on the positive, and non-linear, correlation between present financial anxiety and plan engagement at the outset of the study. In particular, the figure hints at the potentially debilitative effects of high financial anxiety in that those reporting no anxiety were 39% more likely to participate ($b = 0.18$, $p < 0.05$) and 108% more likely to fully take-up the match than those reporting high anxiety ($b = 0.22$, $p < 0.01$).

The second panel of Figure 9 depicts the relationship between an employee's forecasted change to future anxiety and the intent to increase savings across three future dates – one month, six months, and one year. To characterize future savings intentions, we tag employees as having a future intent to save if an employee indicated they were "moderately" or "very" likely to increase their plan savings, assuming continued employment at the firm, by each of the specified future dates. Forecasts of future anxiety were less detailed but reflected either a 3-month or 6-month horizon depending on the specific time-horizon to which the employee was randomized (recall that employees provided forecasts for only one of the two dates). An initial take-away from the plot is that the majority of employees expect to increase their savings within the subsequent year. The timing of intended future savings appears, however, to vary significantly across employees. That is, employees expecting relief from anxiety in the near future (i.e., either 3 or 6 months), relative to those expecting no relief, are only nominally, and insignificantly, more likely to express an intent to increase savings in one month ($b = 0.04$, $p = 0.93$), but substantially more likely to express an intent to save in six months ($b = 0.16$, $p < 0.01$). Indeed, more than one-half of employees expecting near-term relief in anxiety indicated an intent to increase savings in 6 months, implying an intent to save 44% higher than their counterparts. By the one-year horizon, these differences disappear, as the majority of respondents, regardless of future optimism regarding anxiety, indicate an intent to increase their savings.

Beyond this suggestive evidence from the field sample in support of the two predictions of the model, we can more formally estimate the marginal effects of present anxiety on present saving and of forecast future anxiety on intentions to save at that time through simple regressions that control for

observable demographics, and, in the case of the latter relationship, also control for present financial anxiety and present savings. These estimates yield similar conclusions – present financial anxiety strongly, and negatively predicts present savings, while optimism regarding one's financial anxiety, in the intermediate future, strongly, and positively, predicts intentions to save over a similar horizon (even controlling for present anxiety).[46]

7 CONCLUSION

We describe findings from a field experiment examining four psychological frictions as candidate explanations for under-saving, embedded within a survey of beliefs and decision-making with 401(k)-eligible employees at a large US firm. The research design allowed us to estimate average marginal effects of addressing each friction through information- and incentive-based interventions and to additionally examine whether baseline 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 empirical anomalies associated with the retirement savings of US employees. First, we corroborate existing evidence on the prevalence of low *retirement literacy*, as well as correlations between measures of financial literacy and savings, but find that the experimental provision of personalized recommendations about contribution targets does not influence saving behavior, even among employees with greater deficits in literacy. Second, in an analysis of *plan confusion*, we find that a nontrivial share of employees exhibit confusion about plan details, with one in five underestimating plan match. While match underestimates are correlated with lower baseline plan enrollment and match take-up, clarifying the generosity of the plan match does not increase engagement, even among these employees. In an unplanned analysis, we document a striking level of confusion about plan enrollment status with a conservative estimate that one-fifth of 401(k) non-participants mistakenly believed themselves to be enrolled. Consistent with the potential for a large degree of confusion about plan enrollment, employees overreporting contributions at baseline enrolled at very high rates upon prompts to observe their actual

[46] Specifically, to estimate the marginal effect of forecasted future anxiety on the future intention to save, we estimate $Pr(\text{Savings Increase in Six Months}_i) = \alpha + \gamma_1 \text{Less\_Anx}_i + \gamma_2 \text{Future\_Anx}_i + \mathbf{X}\theta + \lambda_i + \pi_i + \varepsilon_i$, where the dependent variable indicates a moderate, or greater, likelihood of saving in six months, $\mathbf{X}$ is a vector of employee demographics (a linear index in age, and dummy variables to indicate gender, age, marital status, educational category, and income category), λ_i denotes fixed effects to flexibly control for an employee's present contribution rate, and π_i denotes fixed effects to flexibly control for present financial anxiety. The coefficient of interest, $\hat{\gamma_1} = 0.22$ ($p < 0.01$), suggests that delayed optimism regarding one's future anxiety is associated with a 61% marginal increase in an employee's six-month intent to save. An analogous model estimating the relationship between present financial anxiety and present savings, excluding controls for present contribution rate and present financial anxiety, suggests that high present anxiety is associated -0.14 change in the likelihood of present plan participation ($p < 0.10$), and a -0.19 change in the likelihood of present match take-up ($p < 0.05$).

enrollment status through the small reward. Third, we find no evidence that *enrollment complexity* impedes savings—few employees perceived enrollment as overly complex administratively and simplifying enrollment did not increase savings despite reducing perceptions of how long it takes to administratively enroll. Finally, we present novel 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 valued at $10 but not to clarification of the much larger, but delayed, benefit implied by the plan match.

Through a series of calibrations, we assess whether the beta-delta framework of present-biased employees could plausibly account for the baseline reluctance of employees to enroll, their response to the experimental treatments, and their stated intentions to save in the future. The exercise suggests that, even allowing for very large psychological hassle costs of enrollment, low values of beta, and cognitively-intensive sophistication, the widely-used framework cannot account for the observed behavior of employees. Ultimately, the stated intent of employees to save in the intermediate, rather than immediate future, problematizes attempts to rehabilitate the beta-delta model by simply assuming that employees are naïve about their present bias.

We propose an alternative model, informed by research in psychology and neuroscience, in which financial anxiety, and optimism regarding future anxiety, leads employees to delay enrollment. The model specifies that financially anxious employees may delay enrollment, until they expect to transition to a less anxious state, if the anticipated benefits of delay (i.e., the reduced anxiety-costs of enrollment) exceed the opportunity costs of delay (i.e., foregone plan benefits). If one further assumes the presence of overly-optimistic hedonic forecasts, the model offers an explanation for sustained delays in plan enrollment as well as the widely-documented gap between intended and actual savings behavior. Calibrations suggest that assuming moderate disutility of enrollment, the model can account for the observed behavior and beliefs of most employees without assuming implausibly severe deficits in self-control. We present additional data from the field sample, and a supplementary sample of US employees, descriptively consistent with the central assumptions and two main predictions of the model relating current and forecast anxiety levels to current and anticipated saving behavior. Beyond offering an explanation for the under-saving of employees in the present setting, the model offers a potential unifying explanation for other empirical puzzles characterizing the savings behavior of US employees.

We highlight important limits to the present research. First, while our employee sample does seem representative of a more general population of benefit-eligible employees who are not on track for financial security in retirement, the 401(k) plan at this firm does feature a simple enrollment process and the relatively generous plan match. In particular, our findings on the role of enrollment complexity may underestimate the importance of such frictions in plans with more demanding administrative burdens of

enrollment. Second, given our access to administrative data on 401(k) plan activity spans a limited period of months, we cannot observe if employees offset increased contributions by reducing savings elsewhere, reducing plan contributions in subsequent periods, or increasing debt, particularly that tied to early-withdrawal loans (see Beshears et al. 2018). Finally, because we did not design our study to test confusion about enrollment status, our surprising findings about the degree of such confusion, and the suggested benefits of increasing enrollment transparency, should be replicated in other contexts.

In spite of these limitations, we see this paper as offering at least two practical prescriptions for the optimal design and marketing of employee savings plans to encourage higher contributions. An initial takeaway from our findings is to encourage some skepticism that the widespread practice among plan sponsors of offering decision support tools or short-term financial education alongside enrollment decisions are effective ways to influence financial outcomes. If, as our findings suggest, most employees recognize their savings shortfall even if they exhibit low retirement literacy, such decision aid strategies may be less helpful than alternatives aimed at addressing more serious barriers such as basic confusion about enrollment status and anxiety about one's proximal financial situation. For example, if employee confusion about enrollment status generalizes to many firms with similar benefit settings, we speculate that creating integrated enrollment portals and program communications across all benefits, that facilitate an employee's ability to manage many benefit decisions could reduce inadvertent non-participation. Moreover, as an alternative, or in addition, to expensive financial educational initiatives, employers could leverage immediate, non-monetary, rewards to encourage plan engagement. 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.

More centrally, our findings regarding the potential mechanisms underlying the apparent present-focus of employees offer a roadmap for the optimal design of retirement savings plans. Specifically, if high levels of financial anxiety and overly optimistic hedonic forecasts contribute to the unwillingness of employees to save, despite generous economic plan incentives, there may be justification for supplementing current 401(k) plans with a more liquid account that would accumulate assets until an employee achieves some degree of short-term financial liquidity. While dual-account designs have been proposed in recent years in various forms by academics and policy-makers as a means of addressing the absence of short-term liquidity (Beshears et al. 2014; Laibson et al. 2014; Gruber 2016; Mitchell and Lynne 2017; Beshears et al. forthcoming), our findings provide an additional psychological rationale for such reforms. The present research suggests that offering employees a highly liquid "Serenity" account with attached retirement account could encourage long-run savings and match take-up by addressing the proximal financial concerns associated with financial anxiety. If such a plan were accompanied with

additional tools, services, and education aimed at improving financial capability and wellness, then it promises an even stronger chance of eliminating a potentially critical barrier to retirement security.

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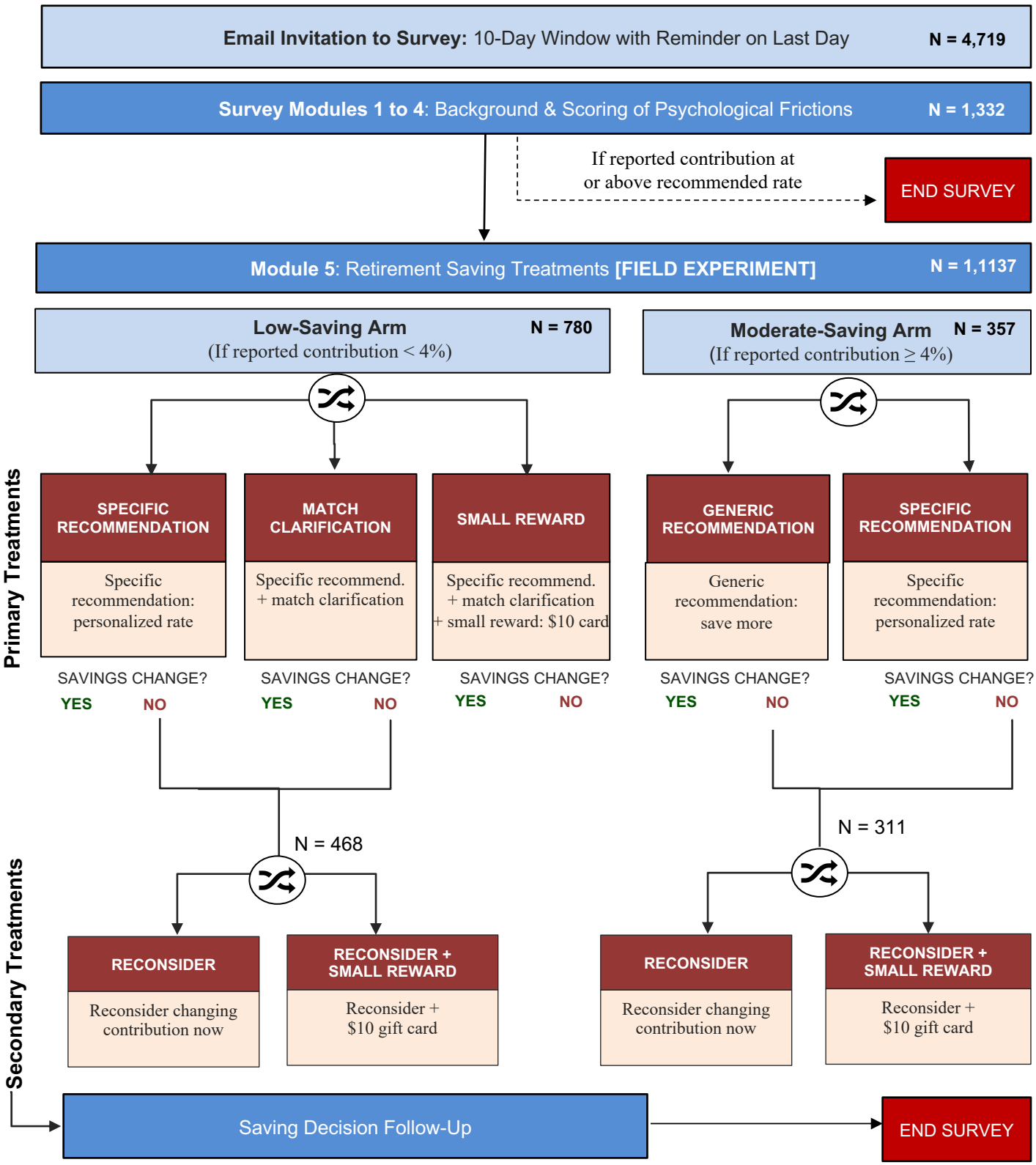
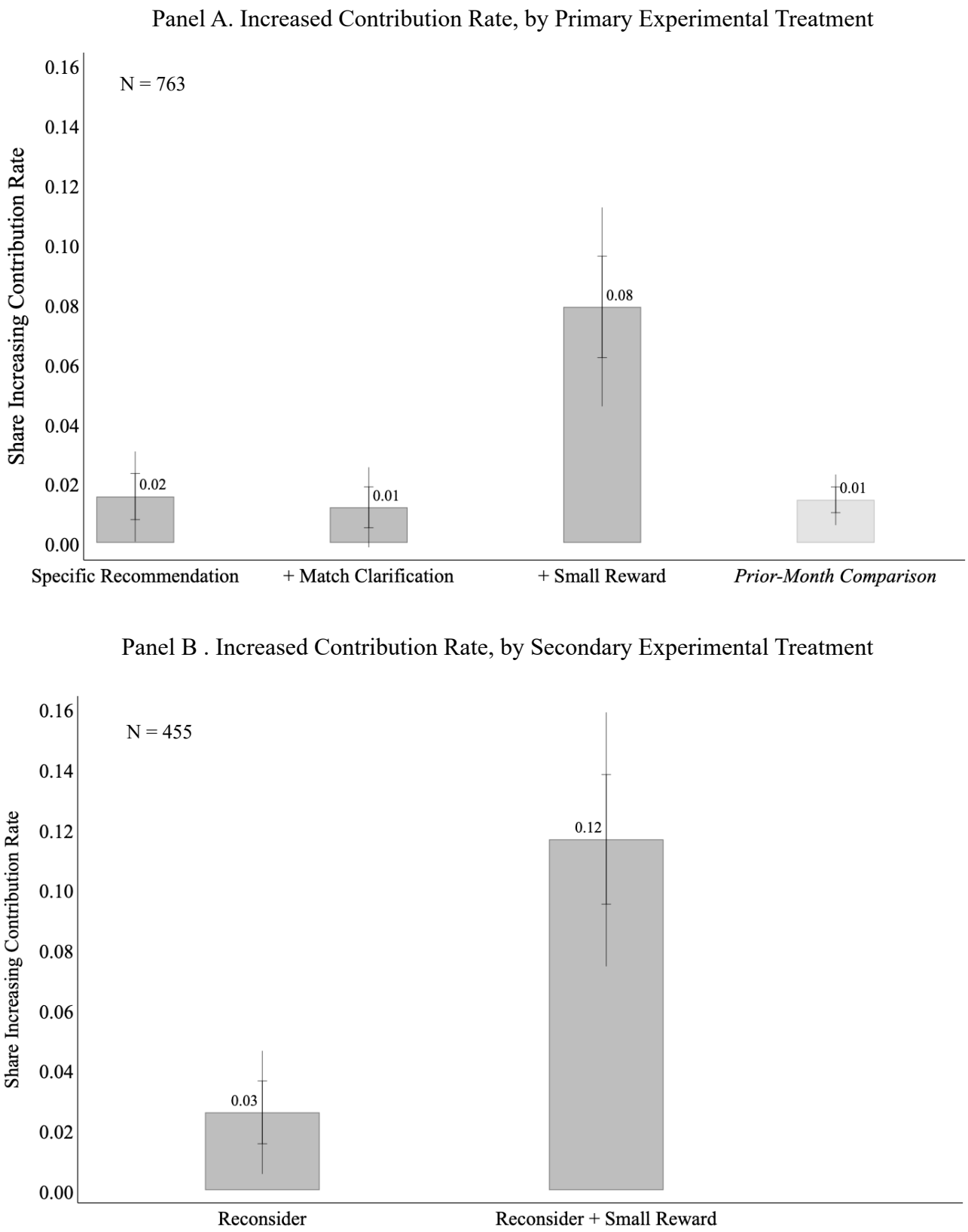
Figure 1. Research Design Schematic for Employee Survey and Field Experiment



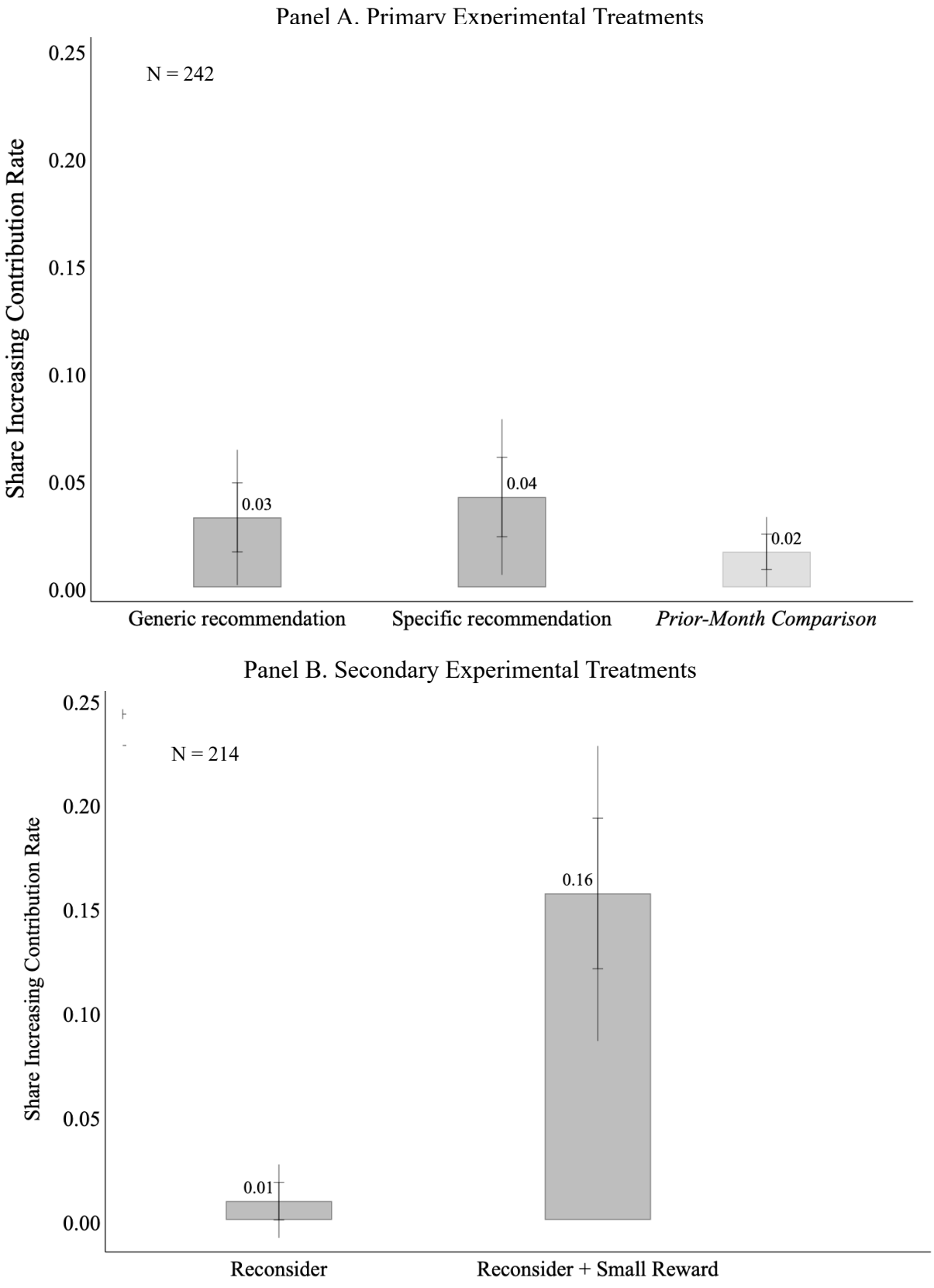
Notes: This figure depicts the research design for the employee survey and field experiment and relationships between each component. From top to bottom: The first box defines the sample of employees invited to participate based on their 401(k)-match-eligibility and contributions one month prior to the email invite, including employees not fully taking up the match and a random subsample of those contributing 4-9% of salary. The darker blue boxes denote modules of the employee survey. Survey Module 5 contains the field experiment and definition of each study arm based on baseline reported 401(k) contribution. Under each arm, orange boxes denote an experimental condition with the symbol, "🔀" denoting randomization with even probability. Below the Primary Experimental Treatments, "Savings Change?" denotes a point where employees were asked if they had made an enrollment adjustment on the linked benefit portal and arrows denote which responses led to assignment to a Secondary Experimental Treatment. Experimental responses were observed in enrollment records for the next pay date after the survey closed. See Section 4.4 for additional detail on design.

Figure 2. Share of Employees Increasing 401(k) Contribution by Experimental Treatment (Low-Saving Arm)



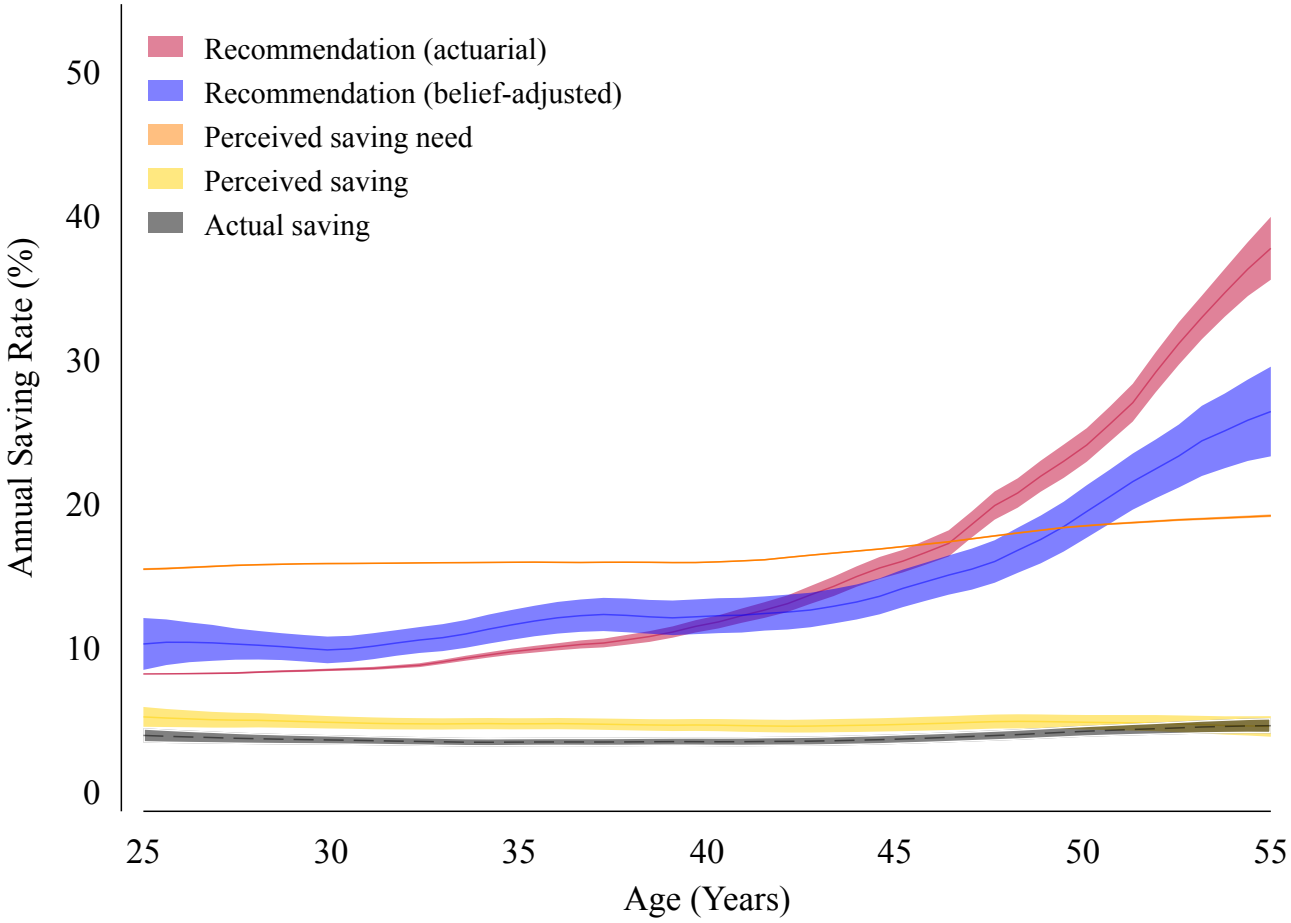
Notes: This figure depicts, for the Low-Saving Arm of the field experiment, share of employees that increased 401(k) plan contribution rate by randomly assigned primary (Panel A) and secondary (Panel B) treatments. Increased rates are identified based on enrollment at the next pay date after the employee survey closed, relative to enrollment status at the pay date preceding the survey invite, 28-day window. In addition to experimental conditions, Panel A shows, in lighter gray, an out-of-sample comparison to the same outcome over the month prior to our interventions within the sample of employee in that plot. Error bars reflect 95% confidence intervals.

Figure 3. Increased 401(k) Contribution Rate by Experimental Treatment (Moderate-Saving Arm)



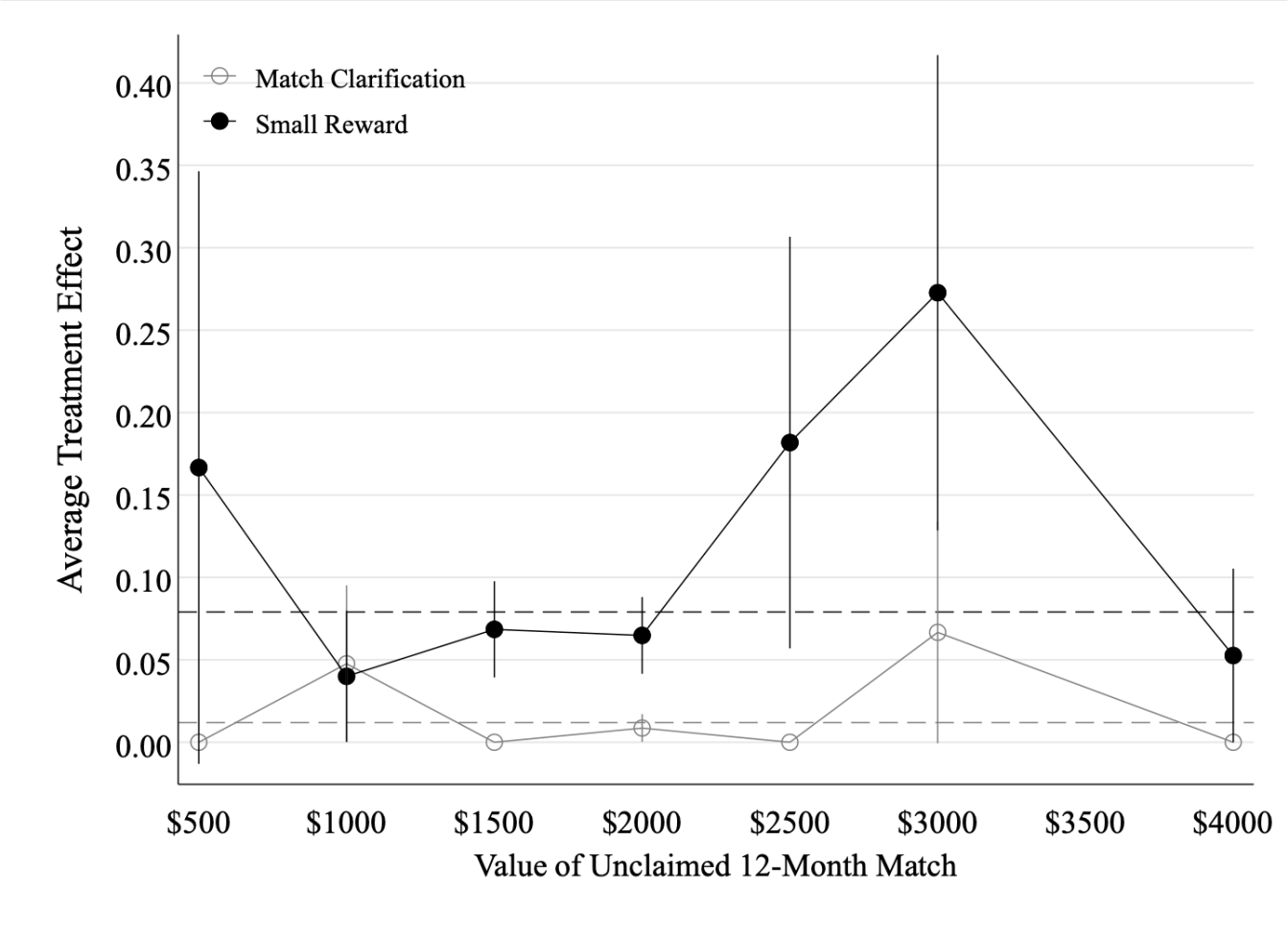
Notes: This figure depicts, for the Moderate-Saving Arm of the field experiment, share of employees that increased 401(k) plan contribution rate by randomly assigned primary (Panel A) and secondary (Panel B) treatments. Increased rates are identified based on enrollment at the next pay date after the employee survey closed, relative to enrollment status at the pay date preceding the survey invite, 28-day window. In addition to experimental conditions, Panel A shows, in lighter gray, an out-of-sample comparison to the same outcome over the month prior to our interventions within the sample of employee in that plot. Error bars reflect 95% confidence intervals.

Figure 4. Recommended, Actual & Belief-Adjusted 401(k) Saving Rates by Employee Age



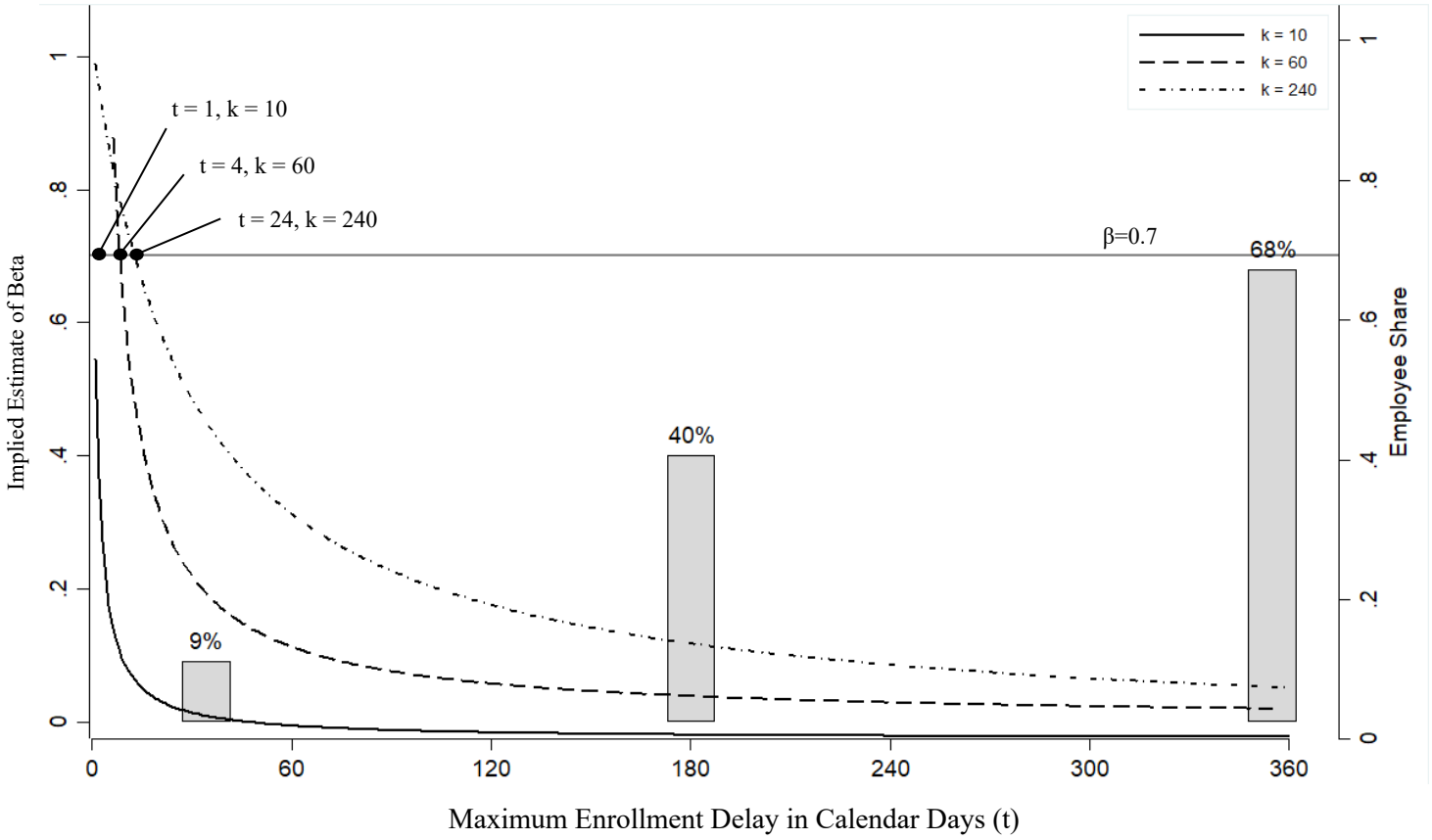
Notes: This figure depicts recommended, actual, and perceived 401(k) saving rates at the time of employee survey by employee age for all surveyed employees. The plot includes, in red: recommendations with actuarial assumptions for parameters of retirement age, years, and income replacement ratio. See Appendix Table A6 for further detail on the calculator used to form recommendations, which we take from the employee benefit web portal. In blue: analogous recommended rates adjusted to incorporate employee beliefs about these parameters. In orange: employees perceptions of the requisite rate. In gold: employees' perceptions of their current contributions. In gray: their actual saving rates at time of survey. Saving rates reflect total plan contributions (inclusive of employee and employer contributions) smoothed with moving local averages over 50 basis point bandwidth and a 95% confidence interval for all series but the perceived requisite rate (orange) so as not to obscure the other relationships this series' wider variation.

Figure 5. Employee Response to Match Clarification and Small Reward by Foregone Match Value



Notes: This figure depicts the experimental response to the small reward (black line) and the match clarification (grey line) treatments by the estimated 12-month financial value of an employee's foregone plan match (estimated $500-bins). The dashed lines depict the average response associated with the treatments. All data is restricted to employees contributing below the match at the time of the study. See text for details describing the calculation of the potential 12-month financial value of the plan match.

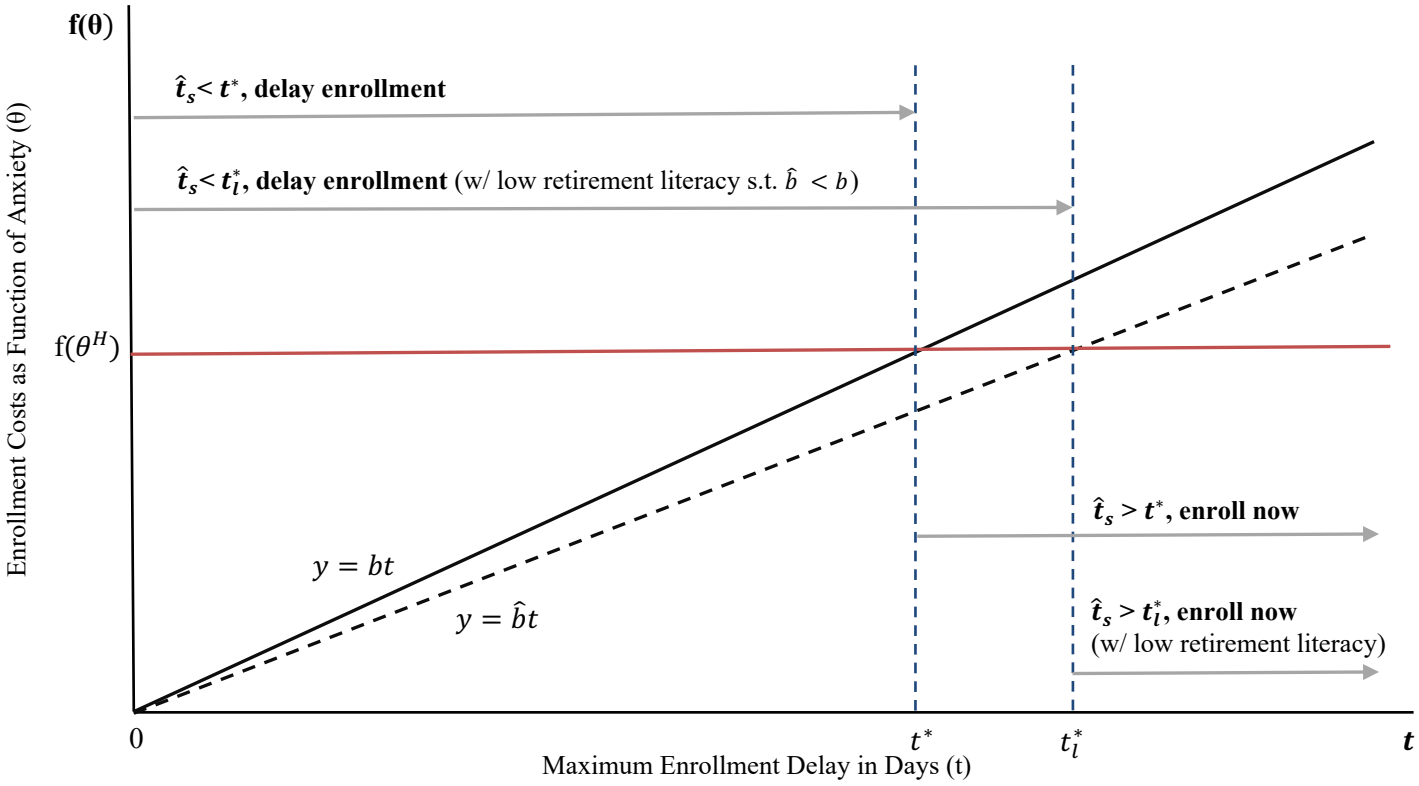
Figure 6. Implied Beta for Beta-Delta Present Bias Model by Enrollment Delay
For an employee earning $50k annual salary and plan enrollment at a contribution rate of 4 percent)



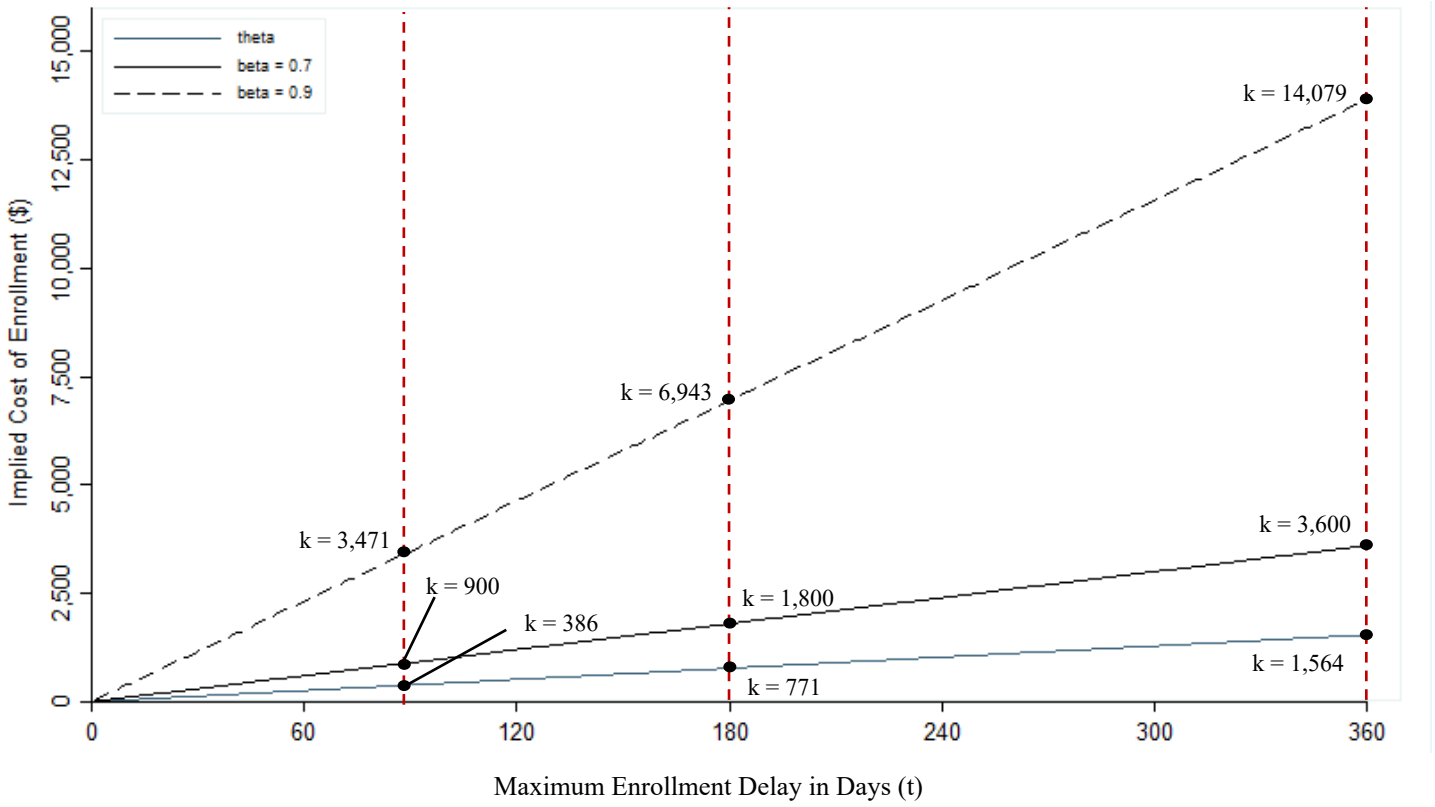
Notes: The figure reports the implied estimate of the beta parameter from the beta-delta model as a function of the maximum delay in match take-up for a sophisticated present-biased employee as specified by the beta-delta model, and initial assumptions, detailed in the text. Estimates are separately displayed for enrollment costs of k = 10, 60, and 240. The figure also depicts the maximum enrollment delay in calendar days (t) that can be rationalized for each value of k, assuming a beta of 0.7. The cumulative empirical distribution (light gray bars) reflects the timing of future intentions to enroll among self-reported non-participants. Employees are tagged as intending to participate at the earliest time-horizon—given 1 month, 6 month, or 12 month options—for which they report being "moderately" or "very" likely to participate.

Figure 7. Anxiety and Delayed Optimism Model and Enrollment Delay

Panel A. Stylized Costs and Benefits of Enrollment Delay

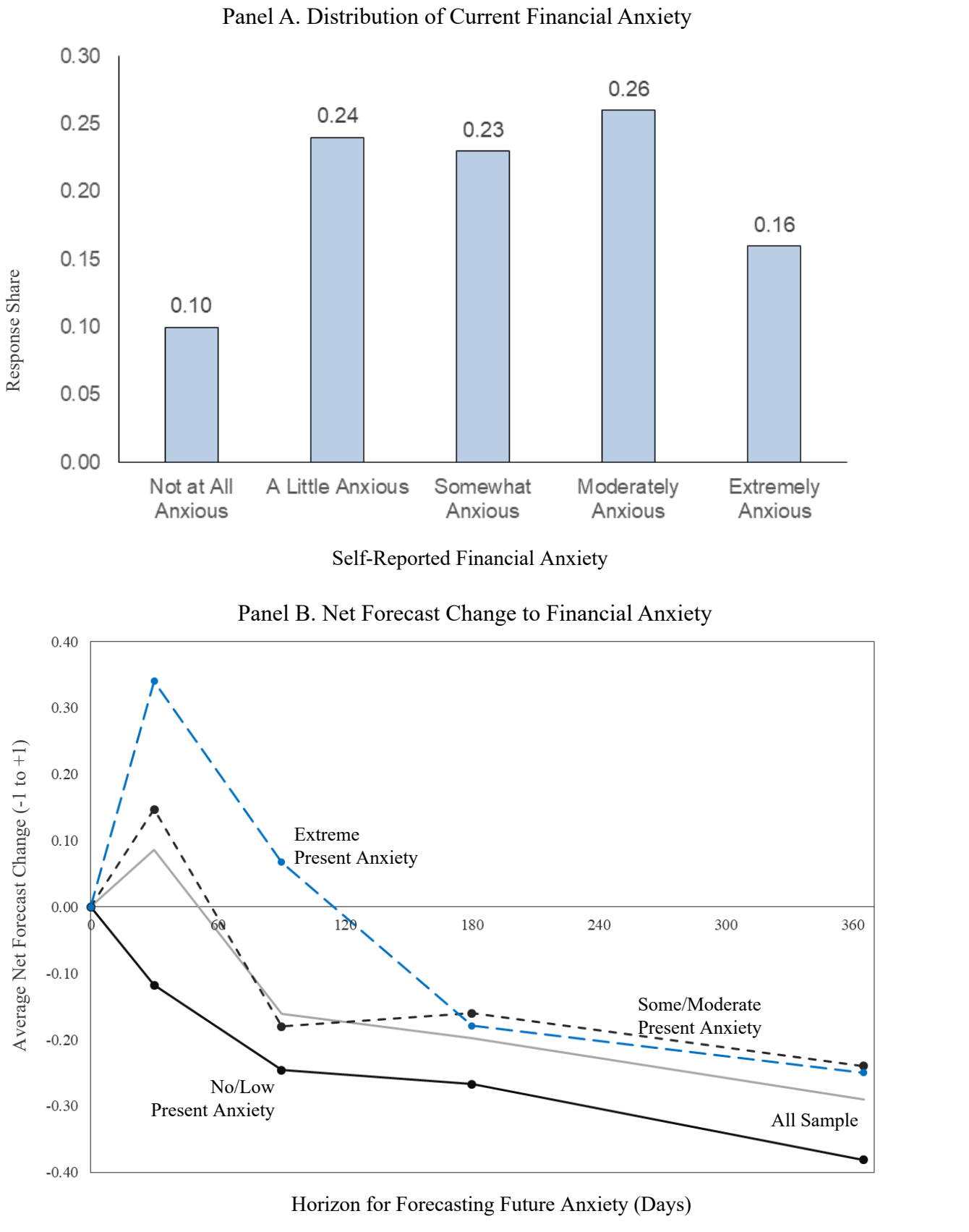


Panel B. Implied Enrollment Costs by Enrollment Delay for Beta-Delta and Anxiety Models



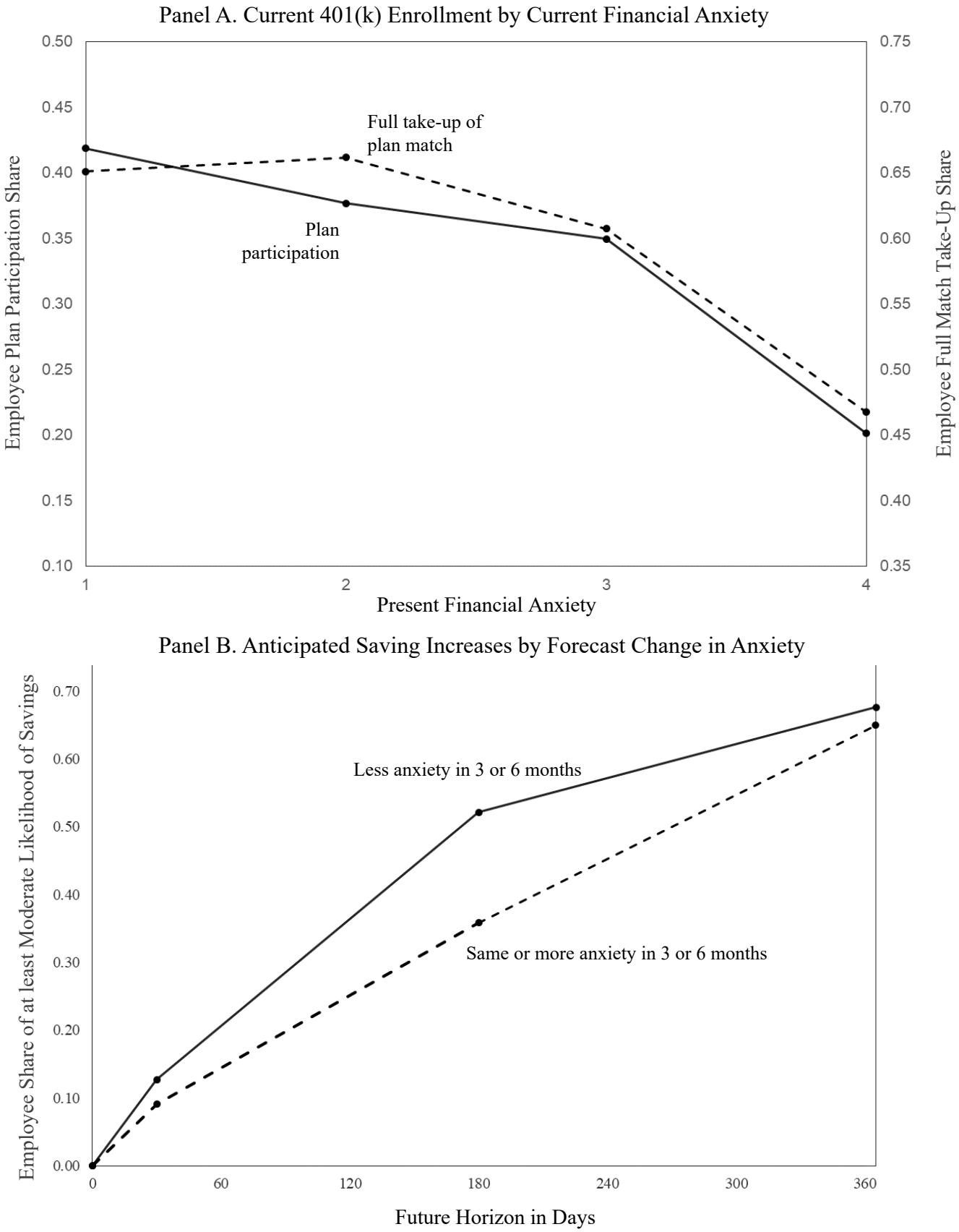
Notes: Panel A depicts the marginal costs and benefits of enrollment delay in the anxiety and delayed-optimism model and indicates the conditions under which an employee will delay enrollment. The panel separately shows the marginal costs of delay, via foregone plan benefits, assuming accurate (solid line) or downward-biased perceptions (dashed line). Panel B depicts the enrollment cost curves implied by the beta-delta model, for beta of 0.9 (dashed black line) and 0.7 (solid black line), and the anxiety and delayed-optimism model (blue line) for varying durations of delay.

Figure 8. Present Financial Anxiety and Forecast Changes to Anxiety – Supplementary Sample



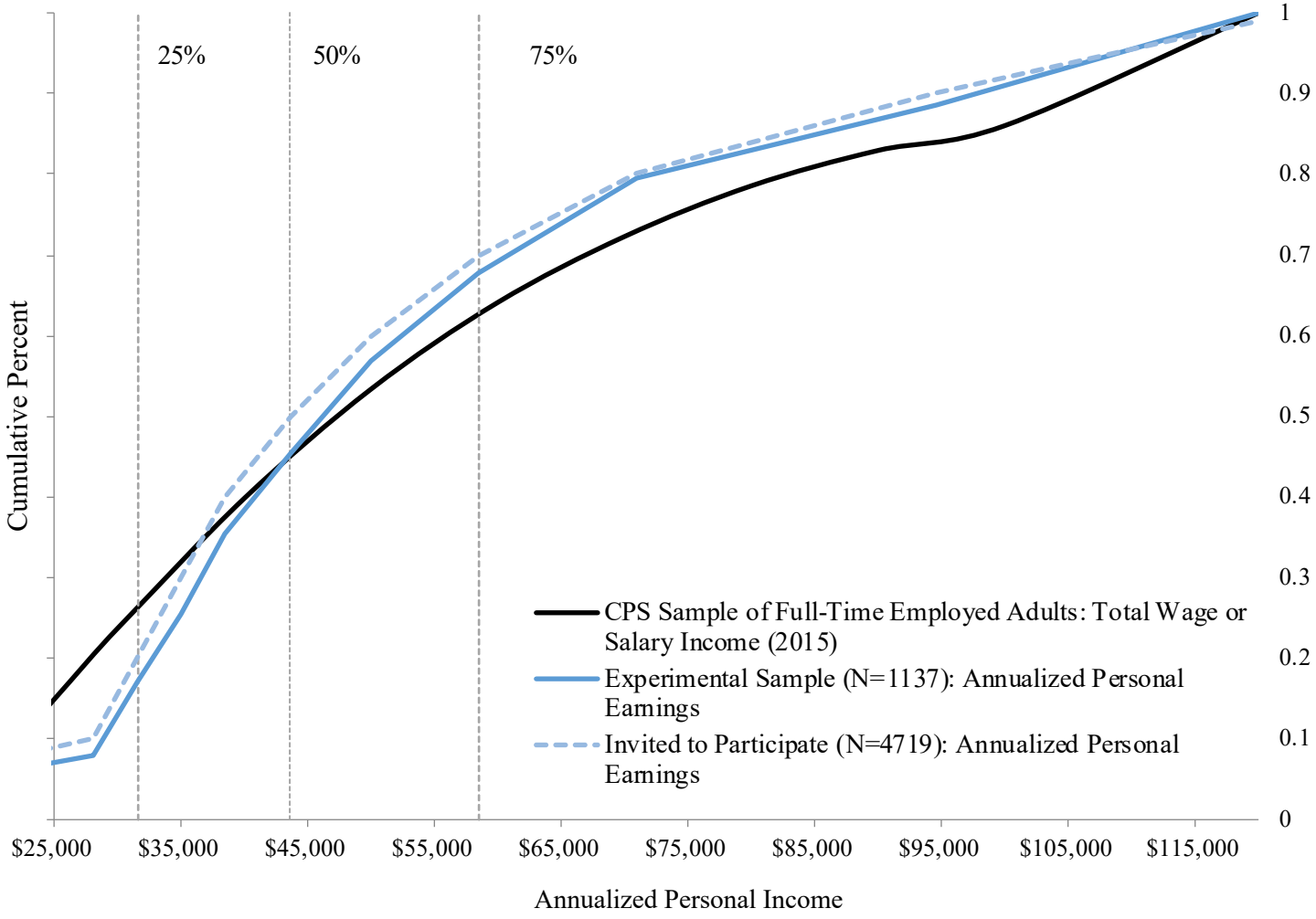
Notes: Panel A depicts the distribution of self-reported present financial anxiety among a supplementary survey sample of US employees. Panel B reports the average net forecasted change in future anxiety for the same sample across varying future horizons. We calculated the forecasted change measure by first scoring each employee's forecast as +1 (increase in anxiety), 0 (no change to anxiety), or -1 (decrease in anxiety) and then averaging these scores for each of horizon. The panel presents this averaged forecasted change measure for the entire sample (grey solid line) and separately by level of present anxiety. Because respondents were assigned to produce forecasts over a random subset of future horizons, there are compositional differences across each horizon.

Figure 9. Financial Anxiety and Employee 401(k) Saving Behavior



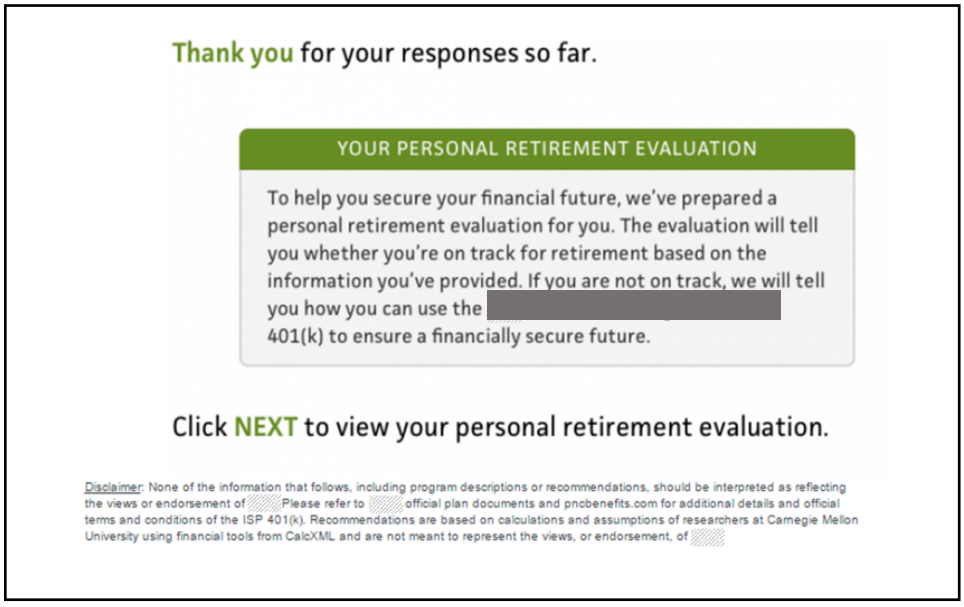
Notes: Panel A depicts the share of employees in the field study who did not participate in the plan and did not fully take-up the plan match by self-reported level of present financial anxiety (1 = "None", 2 = "Very Little", 3 = "Fair Amount", 4 "A Lot"). Panel B reports the share of employees expressing at least a moderate likelihood of increasing their savings across varying future time horizons for employees anticipating less financial anxiety in the intermediate future (either 3 or 6 months) (solid line) or more/the same amount of financial anxiety in the intermediate future (dashed line). Data is restricted to the respondent sub-samples answering each question.

Appendix Figure A1. Income Distributions in Field Sample and Nationally Representative Sample of Full-Time Employed Adults

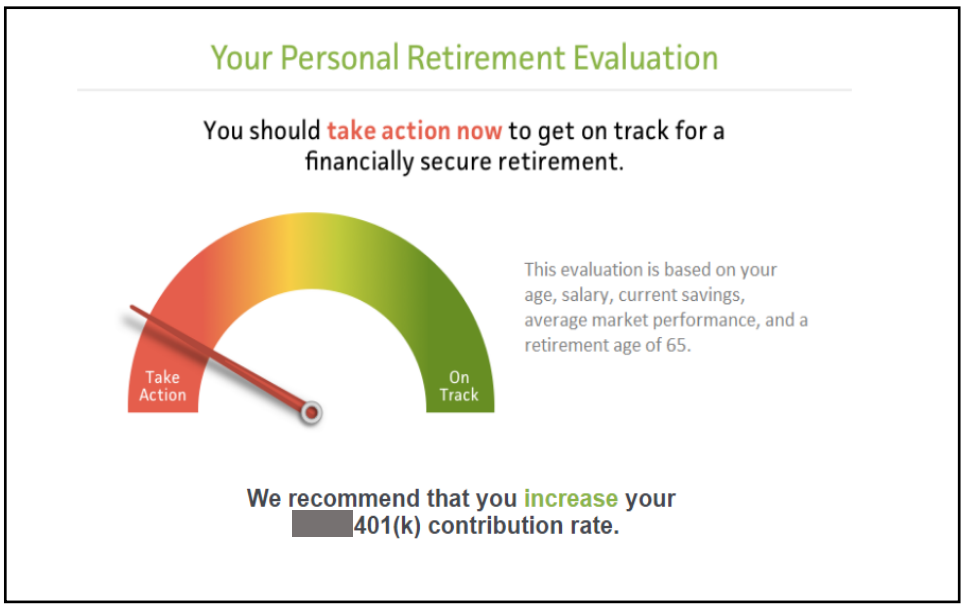


Notes: This figure depicts the annual income distribution of our main study sample ("Experimental Sample") in comparison to the income distribution of all employees we invited to participate in the study ("Invited to Participate") and to the 2015 income distribution of a nationally representative sample of full-time employed adults in the U.S. from the Current Population Survey ("CPS Sample of Full-Time Employed Adults"). The dashed vertical lines denote the 25th, 50th, and 75th percentiles of the distribution of employees invited to participate.

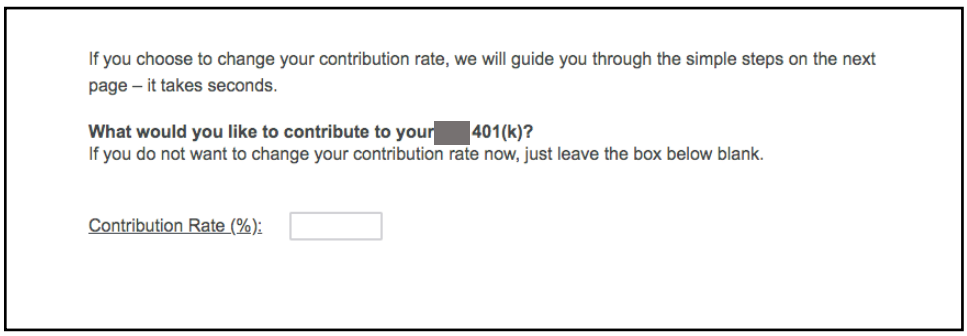
Appendix Figure A2.
Screenshots of Field Experiment Web-Flow – Base Condition (Generic Recommendation)



Introductory Screen

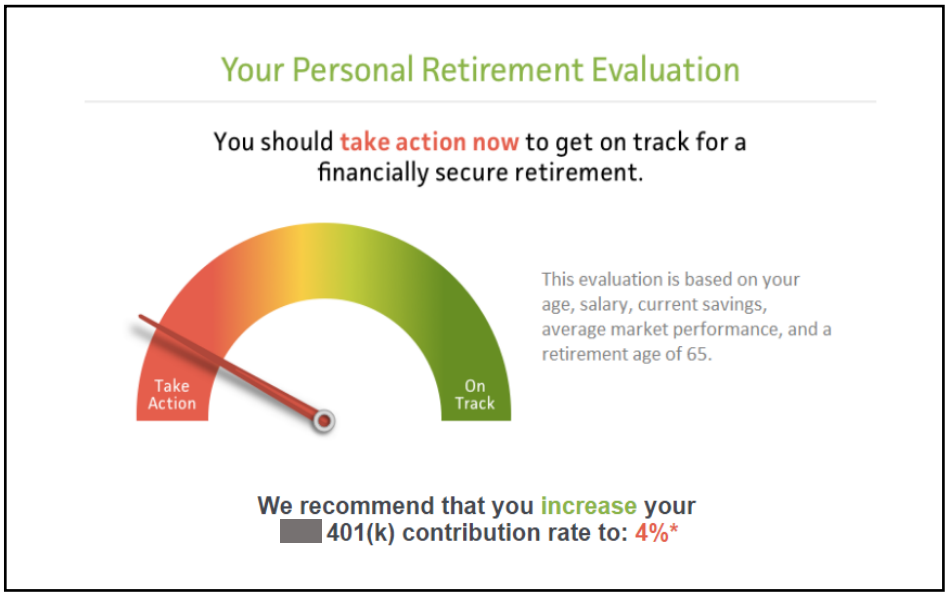


Retirement Assessment
(Generic Recommendation)

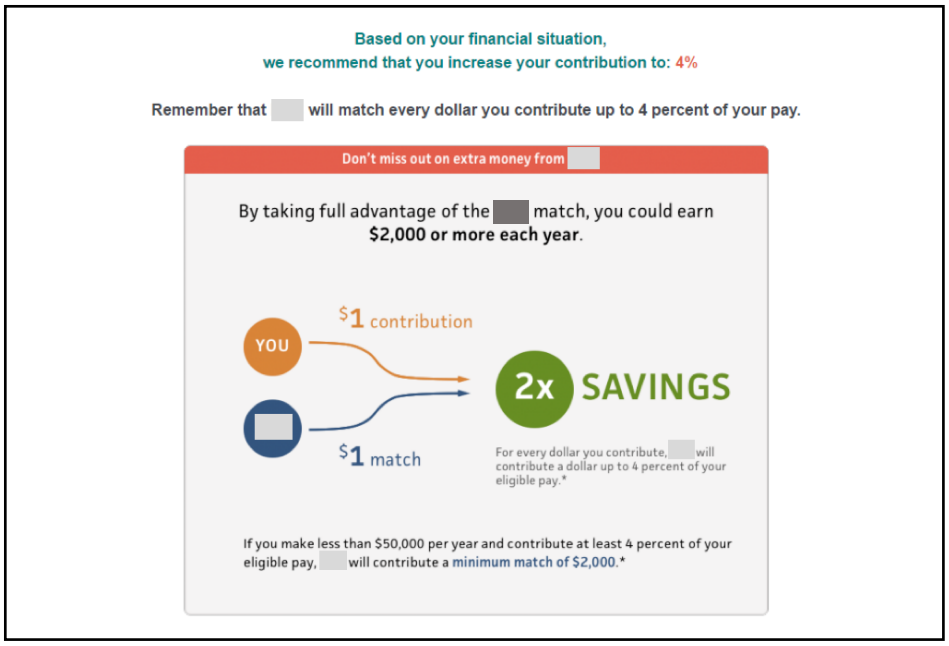


Contribution Prompt
(Prior to Benefit Portal Link)

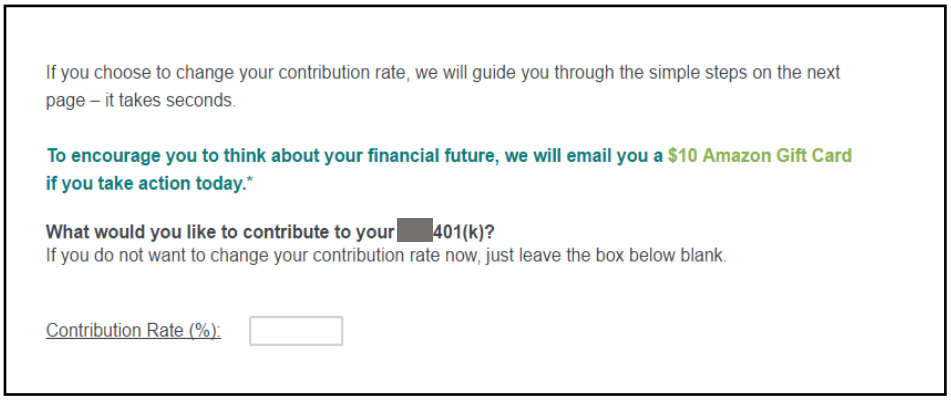
Appendix Figure A3.
Screenshots of Field Experiment Web-Flow – Experimental Treatments



Specific Recommendation

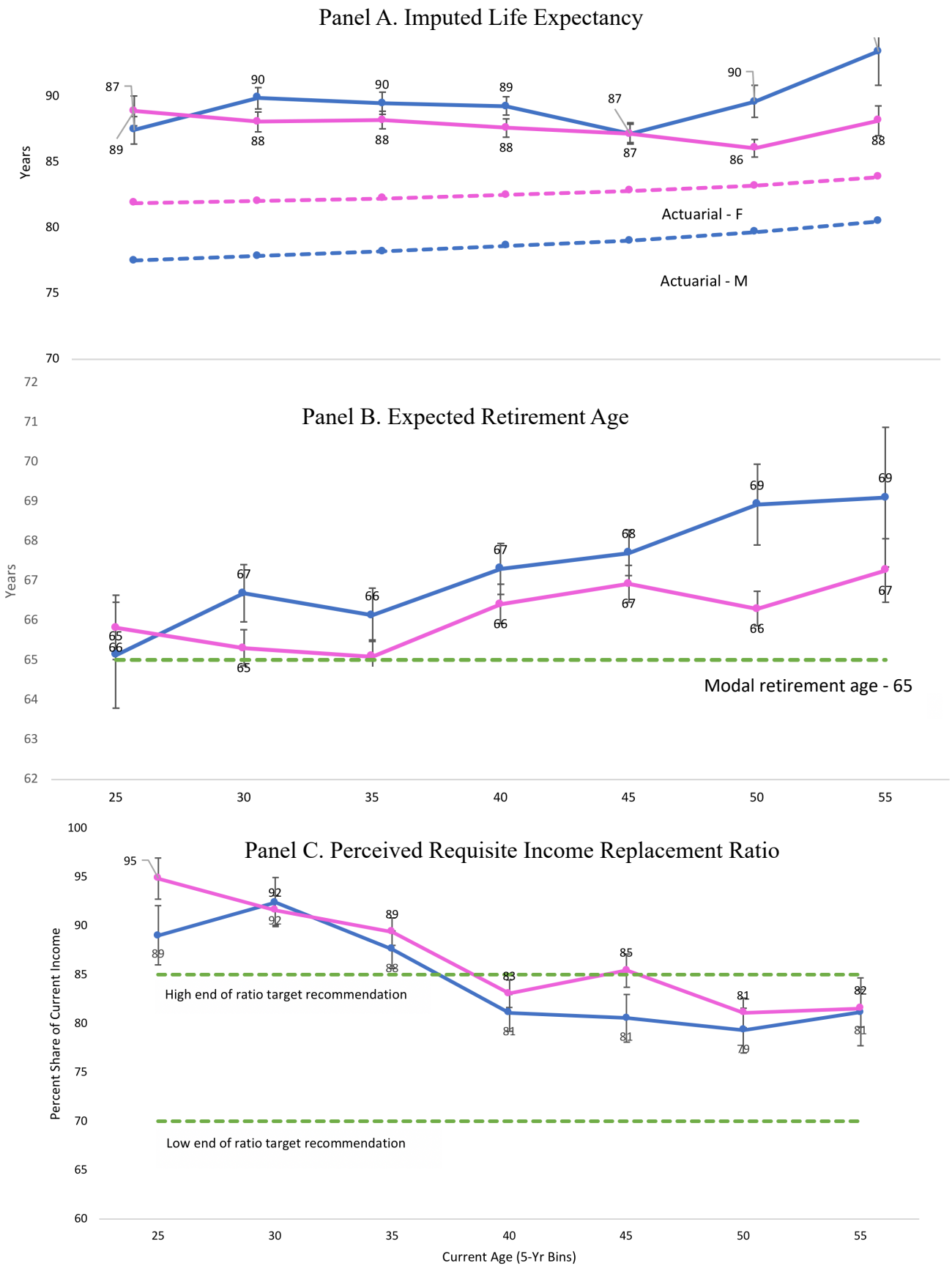


Match Clarification



Small Reward

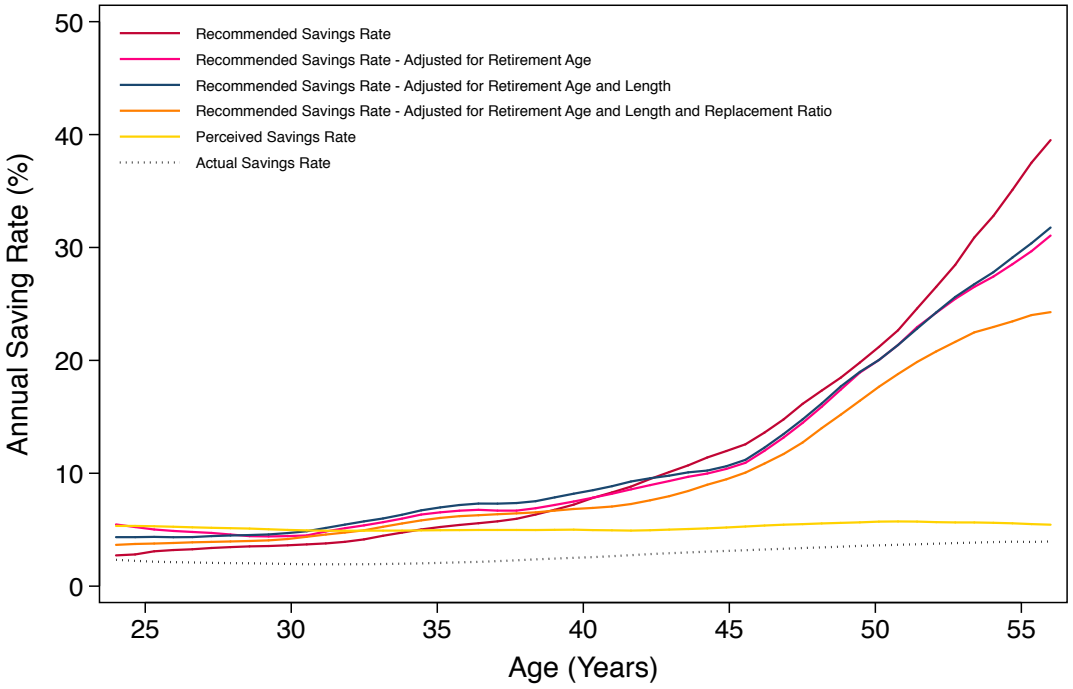
Appendix Figure A4. Employee Beliefs Regarding Three Key Retirement Inputs



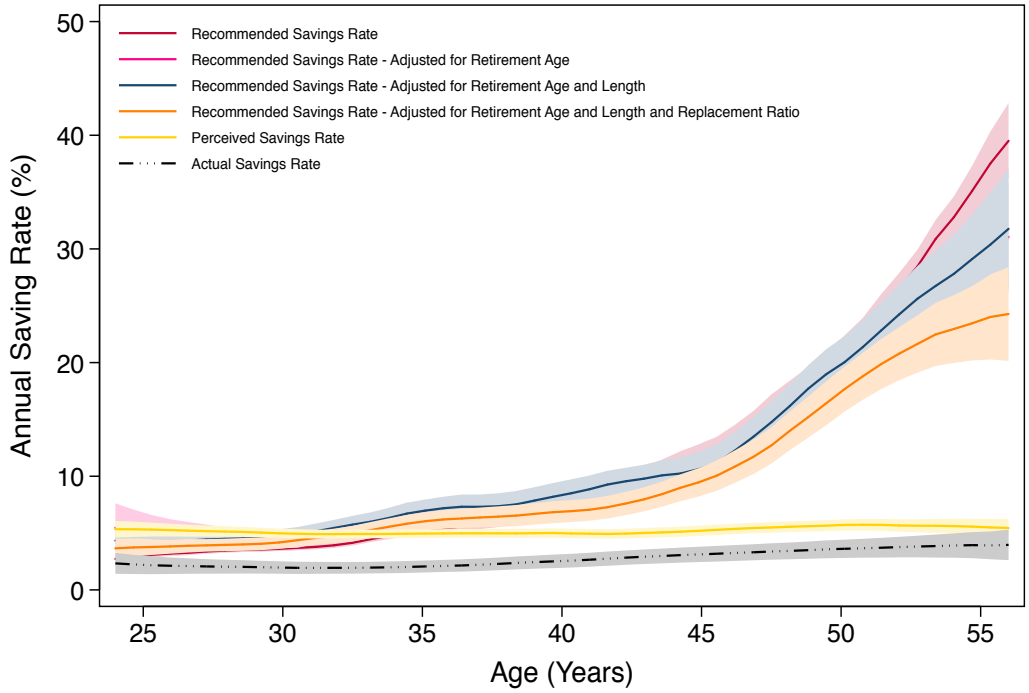
Notes: This figure depicts distributions of reported retirement parameters by current age for men (blue) and women (pink). Panel A shows imputed life expectancy with the benchmark of actuarial predictions by age and gender. Life expectancy is imputed by adding expected retirement age and anticipated retirement length. Panel B shows expected retirement age with the benchmark of the modal current retirement age of 65 (Median age is 63). Panel C shows employees' stated minimum income replacement ratios with benchmarks demarcating a range of minimum income replacement ratios typically suggested in the personal finance industry from 70% of 85%.

Appendix Figure A5.
Recommended Savings Rates Adjusted by Retirement Beliefs and Actual Saving

Panel A. Local Mean Smoothing



Panel B. Local Mean Smoothing with 95% Confidence Intervals



Notes: This figure displays local-mean smoothers of employees' recommended savings rates adjusted for different beliefs about retirement, self-reported savings rates, and actual savings rates observed in administrative data across current age. The local-mean smoother is calculated at every 50 points and is displayed with a 95% confidence interval. Panel A. presents the local-mean smoothers without confidence intervals and Panel B. presents the same plots with 95% confidence intervals. For the assumptions used to calculate the four different recommendations, see Appendix Table A3, Inventory of Saving Recommendation Rates and Parameter Changes.

Table 1.
Summary of Employee Demographics and Savings

	All Sample		401(k) Non-Participants		401(k) Participants		Difference Test
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	(p-value)
Panel A. Invited Employee Sample							
Employee Characteristics							
N =	4,719	-	2,261	-	2,458	-	-
Male [1,0]	0.35	0.48	0.36	0.48	0.34	0.47	0.35
Age [Yrs]	38.8	8.34	38.49	8.2	39.0	8.46	0.05
Tenure [Yrs]	7.8	6.96	7.38	6.64	8.1	7.22	0.00
Imputed income [$ thousands]	51.0	21.31	49.23	20.59	52.63	21.83	0.00
401(k) Savings Behavior							
Participation [1,0]	0.52	0.50	0.0	-	1.0	0	-
Contribution Rate [% annual pay]	1.7	2.39	0.0	-	3.2	2.45	-
Savings Rate (inclusive of match) [est. % annual pay]	3.2	3.98	0.0	-	6.0	3.59	-
Full Match Take-Up [1,0]	0.24	0.43	0.0	-	0.46	0.50	-
Panel B. Respondent Employee Sample							
Employee Characteristics							
N =	1,332	-	559		773	-	-
Male [1,0]	0.33	0.47	0.35	0.48	0.32	0.47	0.28
Age [Yrs]	39.5	8.3	39.1	8.0	39.8	8.4	0.11
Tenure [Yrs]	8.4	7.4	7.93	6.9	8.8	7.7	0.03
Imputed income [$ thousands]	52.4	21.5	50.0	20.6	54.0	22.0	0.00
Married [1,0]	0.58	0.49	0.55	0.50	0.60	0.49	0.09
Any Children [1,0]	0.69	0.46	0.71	0.46	0.67	0.47	0.17
Non-white [1,0]	0.29	0.46	0.33	0.47	0.27	0.44	0.01
College Degree [1, 0]	0.53	0.50	0.52	0.50	0.54	0.50	0.33
Accumulated Savings							
Less than $10k [1,0]	0.53	0.50	0.59	0.49	0.48	0.50	0.00
$10k - $75k [1,0]	0.31	0.46	0.26	0.44	0.36	0.48	0.00
$75k or more [1,0]	0.16	0.36	0.14	0.35	0.16	0.37	0.37
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 < 3-Month Expenses [1,0]	0.68	0.03	0.75	0.04	0.63	0.04	0.06
401(k) Savings Behavior							
Participation [1,0]	0.58	0.49	0.00	-	1.00	-	-
Contribution Rate [% annual pay]	1.9	2.3	0.0	-	3.3	2.1	-
Self-Reported Contribution Rate [% annual pay]	2.4	2.2	1.3	1.9	3.3	1.9	-
Savings Rate (inclusive of match) [est. % annual pay]	3.6	4.0	0.0	-	6.2	3.3	-
Full Match Take-Up [1,0]	0.28	0.45	0.00	-	0.48	0.50	-

Note: This table summarizes observable demographic and plan participation details for two analytic samples. Panel A describes employees invited to participate in the field study in July 2016, and Panel B describes employees who participated in the survey, excluding those who exited before reporting age and income. All employees from this survey sample who self-reported contributing less than 10% of their salary and less than our calculated recommendation were assigned to an experimental intervention. Imputed incomes are calculated using midpoints of salary deciles within the invited sample and used to calculate eligibility for the minimum match and corresponding estimates of total contributions.

Table 2.
Candidate Psychological Frictions and Empirical Savings Puzzles

		Can Friction Account for Savings Puzzle?			
Candidate Psychological Friction	Selected Research	Undersaving	Match Neglect	Default Efficacy	Default Inertia
1. Low Retirement Literacy		**Yes**	**No**	**Yes**	**Yes**
Exponential Growth Bias	(Stango & Zinman 2009, Goda et al. 2015)	Yes	No	No	No
Overconfidence about Earnings	(EBRI 2014)	Yes	No	No	No
Low Financial Literacy	(Lusardi & Mitchell 2006, 2007; Choi et al. 2004)	Yes	No	Yes	Yes
2. Plan Confusion		**No**	**Yes**	**Yes**	**Yes**
Inattention to Plan Default	(Choi et al. 2011, Agnew et al. 2012)	No	No	Yes	Yes
Inattention to or Underestimation of Plan Match	(Choi et al. 2011)	No	Yes	Yes	Yes
3. 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
4. Present Focus					
Present Bias (Beta-Delta)	(Madrian & Shea 2001; Brown et al. 2016; Benartzi & Thaler 2004; Goda et al. 2015)	**Yes**	**Yes**	**Yes**	**Yes**

Note: This table summarizes mechanisms for undersaving suggested by the literature into four general categories, key citations from the literature corresponding to each mechanism, and our predictions 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.
Survey Evidence on Prevalence of Psychological Frictions in Full Sample and by 401(k) Plan Engagement

	Full Sample		by Plan Participation		by Full Match Take-Up		Difference Tests (p)	
Binary Indicator of Psychological Friction	Mean	(N)	Contrib = 0%	Contrib > 0%	Contrib < 4%	Contrib ≥ 4%	Participation	Full Match
Panel A. Low Retirement Literacy								
1. Retirement Beliefs								
Direct Underestimation of Required Savings	0.47	(1321)	0.45	0.49	0.48	0.45	0.18	0.26
Indirect Underestimation of Required Savings	0.43	(1332)	0.45	0.42	0.47	0.33	0.27	0.00
2. Financial Literacy								
Two-Item Financial Literacy Score Equals Zero	0.20	(305)	0.24	0.16	0.23	0.11	0.06	0.02
Panel B. Plan Confusion								
3. Confusion about Plan Detail								
Underestimation of Plan Match	0.31	(1332)	0.35	0.29	0.33	0.27	0.02	0.04
4. Confusion about Plan Status								
Overestimation of Plan Contribution	0.23	(1306)	0.36	0.13	0.30	0.09	0.00	0.00
Erroneous Belief in Plan Enrollment	0.15	(1332)	0.37	0.00	0.00	0.00	0.00	0.00
Panel C. Enrollment Complexity								
5. Estimated Enrollment/Adjustment Time								
Overestimation of Time Required to Enroll/Adjust	0.23	(577)	0.26	0.21	0.22	0.24	0.18	0.74
Panel D. Present Focus								
6. Intertemporal Effort Allocation in Hypothetical Choice								
Present Focus Identified in Choices	0.09	(305)	0.10	0.08	0.09	0.10	0.65	0.80
7. Proximal Intent to Increase Savings								
Likelihood of Savings Increase in 3 Months ≥ 50%	0.21	(577)	0.25	0.18	0.25	0.13	0.07	0.00

Note: This table summarizes correlational evidence from the employee survey relating 401(k) participation and full match take-up to the four categories of psychological frictions laid out as candidate explanations for saving puzzles in Table 2. Each panel presents survey measures relating to one category of frictions with binary indicators reflecting responses consistent with the friction. From left to right each row summarizes the mean of the specified measure for the full employee survey sample, then subgroups split by plan participation and by full match take-up at the last payroll date preceding the survey (0% contributors, nonzero contributors, 0, 1, 2, or 3% contributors, and contributors at 4% or higher). The last two columns show p-values for two-sided t-tests comparing outcomes across participation and full match take-up status.

Table 4.
Overview of Employee 401(k) Enrollment Responses by Experimental Treatment

	Dependent Variable				
	Increase to Contribution Rate (1,0)				Full Match (1,0)
	Low Arm			Moderate Arm	Low Arm
Experimental Treatment	All	Contrib = 0%	Contrib = 1 to 3%	Contrib ≥ 4%	Contrib < 4%
Panel A. Primary Treatments					
Generic Recommendation [GR]	--	--	--	0.03**	--
				(0.02)	
Specific Recommendation [SR]	0.02	0.01	0.02	0.04**	0.00
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Match Clarification [+SR]	0.01*	0.01	0.01	--	0.01
	(0.01)	(0.01)	(0.01)		(0.01)
Small Reward [+MC]	0.08***	0.08***	0.08***	--	0.04***
	(0.01)	(0.02)	(0.03)		(0.01)
N	767	409	354	242	767
F-Tests of Coefficient Equality (p-value)					
SR v. GR	--	--	--	0.70	--
Match Clarification v. SR	0.74	0.99	0.60	--	0.45
Small Reward v. Match Clarification	0.00	0.01	0.01	--	0.03
Non-Experimental Comparisons					
Experimental Sample Pre-Period	0.014	0.015	0.014	0.025	0.000
	(0.004)	(0.006)	(0.006)	(0.008)	-
Panel B. Secondary Treatments					
Savings Reconsideration [SavRecon]	0.03**	0.02*	0.03	0.01	0.01*
	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)
Small Reward [+SavRecon]	0.12***	0.11***	0.12***	0.16***	0.04***
	(0.02)	(0.03)	(0.03)	(0.04)	(0.01)
N	459	232	223	213	459
F-Test of Coefficient Equality (p-value)					
Small Reward v. SavRecon	0.00	0.01	0.01	0.00	0.07

Note: This table summarizes employee 401(k) contribution changes in response to experimentally assigned treatments through a series of OLS regressions predicting whether an employee increased their contribution rate by any amount (columns 1-4) or whether they increased their contribution rate to or above the full employer match limit of 4% (column 5) for the sample described in each column header. Left to right, the first three and fifth column of Panel A describe the Low Arm of the experiment—employees initially contributing less than the 4% of salary match limit—while the fourth column describes the Moderate Arm of the study: employees initially contributing from 4% to 9% of salary. Panel A summarizes regressions of each outcome on indicators for the three potential primary treatments with a suppressed constant. For comparison, the last row of the panel reports the outcome variable for the out-of-sample comparison of contribution changes by these employees in the two pay-period time prior to the study. The bottom three rows of this panel report p-values from F-tests of coefficient equality isolating the marginal effects of the match clarification and small reward for the Low Arm, and of the Specific Recommendation for the Moderate Arm. Panel B summarizes analogous regressions of each outcome on indicators for the two experimentally-assigned secondary treatments. The bottom row of this panel reports the result of F-tests isolating the marginal effect of the small reward on top of the reconsideration prompt. Robust standard errors in parentheses. (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Table 5.
Change in Retirement Literacy and Perceived Enrollment Complexity Due to Experimental Treatments

	Retirement Literacy Δ I(perceived required savings ≥ recommendation)		Enrollment Complexity Δ I(perceived time-cost of enrollment = minutes)	
Experimental Treatment	Low Arm	Moderate arm	Low Arm	Moderate Arm
Generic Recommendation	--	0.08*** (0.02)	--	0.05** (0.02)
Specific Recommendation	0.24*** (0.03)	0.23*** (0.03)	0.03* (0.01)	0.07*** (0.03)
Match Clarification	0.20*** (0.03)	--	0.07*** (0.02)	
Small Reward	0.19*** (0.03)	--	0.07*** (0.02)	
Pre-Study Base Rate	0.50	0.40	0.78	0.77
N	708	330	330	165

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-Saving arm and Panel B for the Moderate-Saving arm. Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.10.

Table 6.
Synthesis of Survey and Field Evidence on Psychological Frictions and 401(k) Savings

		Cross-Sectional Difference			Differential Response to Treatment		
Psychological Friction	Baseline Incidence	E(Δ Part | Friction)	E(Δ Match TU | Friction)	Treatment [Low Arm if unspecified]	Low Friction	High Friction	High ≥ Low (p-value)
1. Retirement Literacy							
Direct Underestimation of Required Savings	0.47	-0.04	0.03	Specific Recommendation	0.02	0.01	0.75
Two-Item Low Financial Literacy Equals Zero	0.12	0.05	0.06	Specific Recommendation	0.02	0.00	0.64
2. Plan Confusion							
Underestimation of Plan Match	0.31	0.06**	0.06**	Match Clarification	0.01	0.01	0.49
Overestimation of Plan Contribution	0.23	0.22***	0.24***	Small Reward	0.06	0.21	0.00
3. Enrollment Complexity							
Overestimation of Time Required to Enroll/Adjust	0.23	0.05	-0.01	Generic Recommendation [Moderate Arm]	0.02	0.00	0.64
4. Present Focus							
Dynamic Inconsistency in Choice	0.09	0.02	-0.01	Small Reward	0.04	0.50	0.00
Likelihood Saving Increase in 3 Months ≥ 50%	0.21	0.06*	0.11***	Small Reward	0.03	0.26	0.00

Note: This table synthesizes evidence from our employee sample role of each of the four candidate psychological frictions we study (Top to bottom: retirement literacy, confusion, enrollment complexity, and present focus). Under each friction, a row presents our associated survey measures indicating lower or Moderate incidence for a given employee. Across the table left to right we present evidence on the baseline sample incidence, baseline correlational evidence on association with plan participation and full match take-up, the experimental treatment designed to target that friction and contribution responses to that treatment. The fourth from right column describes experimental responses to the relevant experimental treatment from OLS regressions of whether an employee increased their contribution rate on indicators for all possible experimental treatments. The next two columns present estimates from an analogous regression with separate indicators for the experimental treatment on employees with the lower vs. the Moderate value of the survey measure in that row. The final column presents a p-value from F-test of whether the estimated response for the Moderate friction group exceeds the response for the lower friction group. Regressions are estimated with robust standard errors and a suppressed constant. (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Table 7.
Discrepant Employee Reports of 401(k) Plan Contribution

Type of Discrepancy	All Sample	401(k) Non-Participants	401(k) Participants	Difference (p-value)
Discrepant Contribution [1,0]	0.28	0.37	0.22	<0.001
Self-Report Contribution Higher than Actual [1,0]	0.24	0.37	0.15	<0.001
Self-Report Contribution Lower than Actual [1,0]	0.04	--	0.07	--
Discrepant Self-Report of Participation [1,0]	0.38	0.38	--	--
Discrepant Self-Report of Non-Participation [1,0]	0.01	--	0.01	--
Discrepant Self-Report of Full Match Take-Up [1,0]	0.19	0.26	0.10	<0.001
Discrepant Self-Report of Less Than Full Match Take-Up [1,0]	0.04	--	0.04	--
Average (Self-Report Rate - Actual Rate | Discrepant Overreport) [%]	3.17	3.81	2.03	<0.001

Note: This table summarizes the average outcome for a series of measures capturing confusion among all employees, 401(k) non-participants, and 401(k) participants. The final column presents the results of F-tests of equal prevalence of plan confusion among non-participants and participants.

Table 8.
Discrepant Enrollment Reports Adjusted for Potential Inattention and Exaggeration

	Discrepant Report Type	
	Any Participation	Full Match TU
Discrepant Reporting Share of Non-Participants	0.38	0.26
Panel A. Inattention Adjustment		
Discrepant Report Share | Passing Attention Check	0.35	0.24
Panel B. Exaggeration Adjustment		
Discrepant Report Share, Adjusted for Exaggeration by:		
Over-Reported Salary	0.34	0.24
Reported highest contribution option (10% or more)	0.37	0.25
Reported highest accumulated savings option (75k or more)	0.33	0.23
Reported highest education option (Graduate school)	0.35	0.23
Any of the above	0.24	0.16
Panel C. Confusion		
Discrepant Report Share Adjusted for Exaggeration | Attention Check	0.23	0.15

Note: This table assesses inattention and exaggeration as potential reasons for discrepant reports of plan participation and full match take-up among plan non-participants in our field study sample. We identify non-participation and discrepancies based on administrative enrollment records at the last paydate prior to the study. Panel A shows adjustments to conservatively rule out careless errors resulting from task inattention. We characterize responses as potentially reflecting inattention if the employee failed a fairly strict attention check in the survey which required them to follow the instruction at the top of one screen of the survey to skip the next question. Panel B shows adjustments to rule out potential exaggerators from the share of employees misreporting by adjusting the shares of misreports to exclude employees who gave other responses consistent with exaggeration (reporting a higher salary category than their salary decile in administrative data or reporting the highest option on the three other ordinal survey questions asked of the full sample regarding contribution, accumulated savings, and education). The last panel shows the share of mistaken reports after adjusting for all of the above exaggeration measures and conditioning on attention check compliance.

Table A1.
Balancing Tests for Experimental Assignment

	Low-Saving Arm			Chi-squared or F-statistic	Moderate-Saving Arm		Chi-squared or F-statistic
	Rec	Rec + Match	Rec + Match + $10	(p-value)	Generic Guidance	Rec	(p-value)
Employee Characteristics							
N =	262	262	256	-	179	178	-
Male [1,0]	0.33	0.34	0.33	0.89	0.32	0.33	0.79
	(0.03)	(0.03)	(0.03)		(0.03)	(0.04)	
Age [Yrs]	39.1	38.3	38.32	0.45	43.1	43.2	0.88
	(0.50)	(0.52)	(0.52)		(0.53)	(0.51)	
Tenure [Yrs]	9.3	8.8	9.4	0.62	12.0	11.8	0.89
	(0.48)	(0.42)	(0.48)		(0.63)	(0.68)	
Imputed income [$ thousands]	47.3	45.9	46.1	0.94	54.0	54.2	0.60
	(1.02)	(1.02)	(1.01)		(1.28)	(1.33)	
401(k) Savings Behavior							
Participation [1,0]	0.55	0.53	0.52	0.72	1.00	1.00	-
	(0.03)	(0.03)	(0.03)		-	-	
Contribution Rate [% annual pay]	0.84	0.86	0.78	0.21	3.50	3.56	0.60
	(0.06)	(0.07)	(0.06)		(0.18)	(0.19)	

Note: This table summarizes the characteristics of the employees in our study's experimental treatment groups, separately for the Low-Saving arm and Moderate-Saving arm by mean with standard errors displayed in parentheses. We also report test statistics (chi-squared statistic for binary variables and F-statistics for all others) for the null hypothesis that the outcome variable is distributed equally across the treatment groups in the relevant experimental arm.

Appendix Table A2.
Survey Measures of Psychological Frictions by 401(k) Enrollment Status - Extended

	Full Sample		Plan Participation		Full Match Take-Up		Difference Test (p-value)	
Measure of Psychological Friction	N	Mean	Contrib = 0%	Contrib ≥ 0%	Contrib < 4%	Contrib ≥ 4%	Participation	Full Match
Panel A. Low Retirement Literacy								
1. Retirement Beliefs								
Retirement Age [Years]	1332	66.4	67	65.9	66.6	65.7	0.00	0.01
Imputed Life Expectancy [Years]	1332	88.2	88.3	88.1	87.9	88.9	0.70	0.05
Income Replacment Ratio [%]	1330	86.2	88	84.9	87.1	83.8	0.00	0.00
Perceived Minimal Sufficient Savings Rate [%]	1321	13.9	15	13	14.1	13.3	0.02	0.42
Direct Underestimation of Required Savings	**1321**	**0.47**	**0.45**	**0.49**	**0.48**	**0.45**	**0.18**	**0.26**
Indirect Underestimation of Required Savings	**1332**	**0.43**	**0.45**	**0.42**	**0.47**	**0.33**	**0.27**	**0.00**
2. Financial Literacy								
Financial Literacy: Interest Q [1,0]	305	0.52	0.57	0.48	0.5	0.57	0.11	0.33
Financial Literacy: Inflation Q [1,0]	305	0.62	0.52	0.7	0.58	0.73	0.00	0.02
Financial Literacy 2-Item Score [0-2]	305	1.14	1.09	1.18	1.08	1.3	0.31	0.02
Expected 20-Yr Annual Return [%]	300	7.47	8.02	7.01	8.01	5.99	0.27	0.05
Two-Item Financial Literacy Score = 0 [1,0]	**305**	**0.2**	**0.24**	**0.16**	**0.23**	**0.11**	**0.06**	**0.02**
Panel B. Plan Confusion								
3. Confusion about Plan Detail								
Aware of Plan Eligibility [1,0]	1332	0.98	0.97	0.99	0.98	0.99	0.10	0.43
Incorrect Match Limit [1,0]	1332	0.4	0.44	0.38	0.43	0.33	0.02	0.00
Underestimates Match Limit [1,0]	**1332**	**0.31**	**0.35**	**0.29**	**0.33**	**0.27**	**0.02**	**0.04**
4. Confusion about Plan Status								
Overestimates Own Participation [1,0]	559	0.38	0.38	-	0.38	-	-	-
Overestimates Own Match Take-Up [1,0]	937	0.19	0.26	0.1	0.19	-	0.00	-
Overestimates Own Contribution [1,0]	**1306**	**0.24**	**0.37**	**0.15**	**0.3**	**0.09**	**0.00**	**0.00**
Panel C. Enrollment Complexity								
5. Estimated Time to Implement Desired Enrollment								
Perceived Enrollment Change Time: Minutes [1,0]	577	0.77	0.74	0.79	0.78	0.76	0.18	0.74
Perceived Enrollment Change Time: Few Hours [1,0]	577	0.11	0.11	0.12	0.11	0.13	0.82	0.47
Perceived Enrollment Change Time: > Few Hours [1,0]	577	0.11	0.14	0.09	0.12	0.11	0.04	0.76
Overestimates Enrollment Change Time [1,0]	**577**	**0.23**	**0.26**	**0.21**	**0.22**	**0.24**	**0.18**	**0.74**
Panel D. Present Focus								
6. Intertemporal Effort Allocation in Hypothetical Choice								
Present-Focus Not Ruled Out by MPL [1,0]	305	0.84	0.86	0.82	0.84	0.83	0.28	0.73
Present Focus Implied by Choice	**305**	**0.09**	**0.1**	**0.08**	**0.09**	**0.1**	**0.65**	**0.80**
7. Proximal Intent to Increase Savings								
3-Month Contribution Increase Likelihood [%]	577	19.1	21.15	17.69	21.46	14.17	0.13	0.00
3-Month Contribution Increase Likelihood ≥ 50% [1,0]	**577**	**0.21**	**0.25**	**0.18**	**0.25**	**0.13**	**0.07**	**0.00**

Note: This table summarizes correlational evidence relating baseline 401(k) participation and full match take-up to survey measures of the four categories of psychological frictions laid out as candidate explanations for saving puzzles in Table 2. This is an extended version of Table 3 that adds raw survey responses used to create the main binary indicators of each friction (bolded rows). Each panel presents measures relating to one category of frictions. From left to right each row summarizes the mean of the specified measure for the full employee survey sample, then subgroups split by plan participation and by full match take-up at the last payroll date preceding the survey (0% contributors, nonzero contributors, 0, 1, 2, or 3% contributors, and contributors at 4% or higher). The last two columns show p-values for two-sided t-tests comparing outcomes across participation and full match take-up status.

Appendix Table A3.
Employee 401(k) Contribution Responses by Experimental Treatment in Low-Saving Study Arm

	Increase in Contribution Rate [1,0]			Change to Full Match Take-Up [1,0]		
	All Low Arm	Split by Baseline Participation		All Low Arm	Split by Baseline Participation	
Experimental Treatment	$Contrib_{pre} < 4\%$	$Contrib_{pre} = 0\%$	$Contrib_{pre} = 1, 2, 3\%$	$Contrib_{pre} < 4\%$	$Contrib_{pre} = 0\%$	$Contrib_{pre} = 1, 2, 3\%$
Panel A. Primary Treatments						
Specific 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)
" " + Match Clarification	0.01*	0.01	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
" " + Small Reward	0.08***	0.08***	0.08***	0.04***	0.03**	0.04**
	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)
N	767	409	354	763	409	354
Out-of-Sample Comparison (Pre-Study)						
Prior-Month Behavior:	0.014	0.015	0.014	0.000	0.000	0.000
	(0.004)	(0.006)	(0.006)	(0.003)	(0.004)	(0.005)
F-Tests of Coefficient Equality (p-value)						
Specific Rec. v. Prior-Month Comparison	0.90	0.99	0.89			
Match Clarification v. Specific Recommendation	0.74	0.99	0.60	0.157	0.318	0.318
Small Reward v. Match Clarification	< 0.001	0.01	0.01	0.033	0.182	0.096
Panel B. Secondary Treatments						
Reconsider	0.03**	0.02*	0.03	0.01*	0.02	0.01
	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
"" + Small Reward	0.12***	0.11***	0.12***	0.04***	0.05**	0.03**
	(0.02)	(0.03)	(0.03)	(0.01)	(0.02)	(0.02)
N	455	232	223	455	232	223
F-Test of Coefficient Equality (p-value)						
Small Reward v. Reconsider	< 0.001	0.01	0.01	0.071	0.179	0.207

Note: This table presents the main effects of our experimental interventions on savings outcomes for the Low-Saving 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).

Appendix Table A4.

Employee 401(k) Contribution Responses by Experimental Treatment in Moderate-Saving Study Arm

	Increase in Contribution Rate [1,0]		
	All	Split by Baseline Contribution	
Experimental Treatment	$Contrib_{pre} \geq 4\%$	$Contrib_{pre} = 4\%$	$Contrib_{pre} > 4\%$
Panel A. Primary Treatments			
Generic Recommendation	0.03**	0.03	0.04
	(0.02)	(0.02)	(0.03)
" " + Specific Recommendation	0.04**	0.03	0.06*
	(0.02)	(0.02)	(0.04)
N	242	150	92
Out-of-Sample Comparison (Pre-Study)			
Prior-Month Behavior:	0.017**	0.007	0.033*
	(0.008)	(0.007)	(0.019)
F-Tests of Coefficient Equality (p-value)			
Generic Recommendation v. Out-of-Sample Comparison	0.98	0.58	0.74
Generic vs. Specific Recommendation	0.70	0.94	0.69
Panel B. Secondary Treatments			
Reconsider	0.01	0.00	0.02
	(0.01)	(0.03)	(0.02)
"" + Small Reward	0.16***	0.14***	0.19***
	(0.04)	(0.04)	(0.07)
N	213	134	79
F-Tests of Coefficient Equality (p-value)			
Small Reward v. Reconsider	0.000	0.002	0.020

Note: This table presents the main effects of our experimental interventions on savings outcomes for the Moderate-Saving 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.

Appendix Table A5.
Differential Employee Response to Reward and Match Clarification by Financial Anxiety

	Measure of Financial Anxiety					
	Self-Reported Anxiety (Total N = 328, High Friction N = 207)			Self-Reported Anxiety and Forecast Reduction (Total N = 215, High Friction N = 23)		
Experimental Treatment	Low	High	Difference (p-value)	Low	High	Difference (p-value)
Specific Rec. + Match Clarification	0.02	0	0.32	0.01	-0.08	0.32
	(0.02)	-0.02		(0.01)	(0.03)	
"" + Small Reward	0.05*	0.10***	0.28	0.08***	0.25***	0.43
	(0.03)	(0.04)		(0.03)	(0.22)	
Reconsider	0.03	0.01	0.72	0.02	0	0.16
	(0.02)	(0.01)		(0.01)	-0.09	
"" + Small Reward	0.09***	0.08***	0.84	0.07***	0.17***	0.41
	(0.04)	-0.03		(0.03)	(0.11)	

Notes: This table summarizes heterogeneity in responses to experimental assignment of the $10 gift card in the Low Arm by measures of self-reported financial anxiety. Each row summarizes predicted share of employees increasing contributions among treated by the intervention identified in row header and categorized as "Low" or "High" on the described measure of financial anxiety. "Self-Reported Anxiety" uses a median split by self-reported financial anxiety levels. "Self-Reported Anxiety with Predicted Drop" compares the subset of above-median anxiety employees who also predict future reductions in anxiety. Estimates come from OLS regressions of whether employees increased contributions on separate indicators for "Low" and "High" anxiety employees treated by each intervention. The last column displays results of F-tests of coefficient equality for the low and high anxiety groups and the intervention described in each row. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A6.
Inventory of Saving Recommendation Rates and Parameter Changes

Recommendation	Participant characteristics			Parameters On Which We Elicited Beliefs or Intentions				Fixed Parameters		
	Age	Income	Accumulated Savings	Retirement Age	Retirement Length	Replacement Ratio	Annual Rate of Return	Inflation	Include SS?	SS Marital status
Experimental Treatment (Specific Recommendation)	Reported decade	Reported income category	Assumed $0 if under 50 or	65	20	Income-Based Assumption*	0.08	0.03	Yes	Single
Actuarial	Actual age	Income decile (of invited	Self-reported bracket	65	20	Income-Based Assumption*	0.08	0.03	Yes	Single
Belief-Adjusted for Retirement Age	Actual age	Income decile	Self-reported bracket	Belief	20	Income-Based Assumption*	0.08	0.03	Yes	Single
Belief-Adjusted for Retirement Length	Actual age	Income decile	Self-reported bracket	65	Belief	Income-Based Assumption*	0.08	0.03	Yes	Single
Belief-Adjusted for Replacement Ratio	Actual age	Income decile	Self-reported bracket	65	20	Income-Based Assumption*	0.08	0.03	Yes	Single
Belief-Adjusted for Return	Actual age	Income decile	Self-reported bracket	65	20	Income-Based Assumption*	Rate implied by survey	0.03	Yes	Single
Belief-Adjusted for Return, Ratio, Age and Ret. Length	Actual age	Income decile	Self-reported bracket	Belief	Belief	Belief	Rate implied by survey	0.03	Yes	Single

Notes: This table reports the parameters assumed for different versions of Saving Recommendation rates generated by a third-party online calculator which the firm hosted for participants on its benefit website.
* The Income-Based Assumption that detrmines our income replacmeent ration assumption is 125% for employees earning under $25k, 100% for employees earning $25-$55k, 80% for employees earning $55k or higher.