Overcoming Frictions in Retirement Savings with Microincentives

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Abstract

We investigate why employees undersave in 401(k) plans despite generous employer matches. In a field experiment, a personalized savings recommendation and match clarification did not increase saving despite correcting pervasive misperceptions. A $10 immediate incentive did increase saving, implying median matching gains of $583 over the remaining year and $2,000 annually. Response was pronounced among the significant share of employees who overstated contributions relative to administrative records—24 percent overall, 37 percent among nonparticipants—a rate corroborated in a national sample. The results implicate frictions of action, including enrollment status confusion, rather than frictions of intention as sources of undersaving.

JEL: D14, D15, D91, G51, J26

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I. Introduction

Automatic enrollment (AE) and employer matching contributions have become standard features of 401(k) plans, yet plan engagement remains incomplete.[2] An estimated 25 percent of employees do not contribute enough to claim the full employer match, and 18 percent of eligible workers fail to participate entirely.[3] This incomplete engagement equates to billions of dollars in unclaimed benefits each year and may threaten the long-term financial security of affected employees (Bhargava et al. 2023). Recent evidence further underscores the welfare importance of this gap by showing that existing saving incentives disproportionately benefit higher-paid workers (Choukhmane et al. 2024). The magnitude of these foregone gains is difficult to reconcile with standard economic models: employer matches typically offer returns of 50 to 100 percent on marginal contributions, yet employees exhibit limited responsiveness to variation in match generosity (Madrian 2013). At the same time, behavior appears highly sensitive to non-economic features of plan design, including defaults, simplification, minimal cues, and small elements of digital choice architecture (Madrian and Shea 2001; Beshears et al. 2018; Choi et al. 2017; Bhargava et al. 2023).

A useful way to organize these empirical puzzles is around two broad classes of barriers. The first concerns intention: employees may fail to save because they misunderstand long-run incentives, including how much they need to save and the returns created by employer matching. This possibility has motivated widespread use of financial education, savings calculators, and other informational interventions (Fernandes, Lynch, and Netemeyer 2014; Lusardi and Mitchell 2014; Kaiser et al. 2022; Ambuehl, Bernheim, and Lusardi 2022). The second concerns engagement with the decision itself. Employees may broadly recognize that they are undersaving yet fail to act because the decision is not actively confronted. These engagement failures can arise from inertia, limited attention, forgetting, or decision avoidance, and may also reflect imperfect knowledge of one's current plan status—for example, whether one is actually enrolled. While the distinction between intention and engagement is conceptually clear, it is empirically difficult to separate and is practically important for considerations of welfare and policy reform.

This paper reports results from a 401(k) field experiment designed to distinguish between these barriers among 1,137 undersaving, plan-eligible employees at a large firm whose 401(k) plan featured automatic enrollment and generous matching incentives. This population is of direct policy relevance, as it offers insight into the millions of employees who contribute little or nothing despite automatic enrollment and a substantial match. The setting is especially informative because a minimum match provision generated marginal rates of return of 100 to 367 percent for employees contributing below the match threshold, making it difficult to attribute undersaving to weak financial incentives alone. The experiment was embedded in a personalized retirement assessment that measured employee-specific saving frictions and then randomized employees to one or more interventions: a personalized savings recommendation, information clarifying the magnitude of the employer match, and a $10 immediate reward for engaging the saving decision. The first two treatments targeted barriers of intention by

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[2] Estimates of AE and match prevalence vary by source. Vanguard's How America Saves 2025, covering approximately 1,400 defined-contribution plans, reports that 86 percent of plans offer matching contributions and 61 percent feature AE, rising to 78 percent among plans with over 1,000 workers. Other sources estimate somewhat lower overall AE adoption (59 percent in Plan Sponsor Council of America 2021; 42 percent of private-sector DC participants in BLS 2022) but show a similar plan-size gradient — for example, PSCA reports 72 percent adoption among plans with 200 or more participants.

[3] Participation rates cited from Vanguard's How America Saves 2025 survey. Match participation rates cited from survey data from Empower (2025). An earlier analysis of administrative records from Voya Financial across over 800 small to midsize plans indicates a match non-claiming rate of 27%, including 30% among plans with automatic enrollment (Bhargava et al. 2023).

correcting beliefs about how much to save and the return to saving. The third tested whether a small immediate incentive could prompt engagement with the decision.

Three findings emerge. First, deficits in understanding long-run saving incentives were widespread but experimentally correcting them did not increase saving. Many employees underestimated how much they needed to save, underestimated the employer match, or scored poorly on financial-literacy questions. Yet a personalized savings recommendation improved beliefs without increasing contributions, and match clarification had no detectable effect, even among employees who initially underestimated the match. These nulls are tightly estimated and hold against a conservative pre-period benchmark. These results suggest that misunderstanding of long-run incentives, though common, was not the binding constraint in this setting.

Second, many employees misperceived the current state of the saving decision. Twenty-four percent overstated their contribution rate relative to administrative records, including 37 percent of nonparticipants who reported contributing despite not being enrolled. These discrepancies survived conservative adjustments for survey inattention and social desirability. A separate incentivized verification exercise among low-saving workers in a national sample produced a comparable, though imprecisely estimated, overstatement rate. Confusion of this magnitude is plausible against the backdrop of benefit-program complexity at large firms — new hires at our partner firm navigated twelve distinct benefit programs with varying eligibility rules and enrollment procedures. Current-status confusion therefore provides a concrete and measurable form of engagement failure, in which employees fail to act because they do not perceive that action is needed.

Third, a $10 immediate reward produced a sharply different behavioral response, increasing saving among 8 to 16 percent of employees across implementations of the reward treatment. Contribution changes persisted over subsequent months. Among employees with unclaimed matches, reward-induced increases implied median additional matching contributions of $583 over the remaining calendar year and a projected $2,000 annually if sustained. That a small immediate reward increased saving where clarification of much larger delayed matching incentives did not points to a friction tied to confronting the present decision rather than to insufficient motivation or misunderstanding of long-run returns.

We interpret the findings as showing that while barriers of intention were widespread, the binding constraints were barriers to engagement. Employees often underestimated how much they should save, but most recognized that they were undersaving. Informational interventions corrected genuine mistakes but did not change behavior. The reward, by contrast, appears to have created a moment of engagement. Status confusion is one important and measurable manifestation of these frictions, but not the entire mechanism: limited attention, forgetting, decision avoidance, and mistaken beliefs about future decision costs may also keep employees from acting even when they recognize the value of higher saving.

Whether such inaction carries clear welfare consequences depends on which friction binds. Under the most prominent class — β-δ models of present-biased procrastination (O'Donoghue and Rabin 1999; Laibson 1997), in which employees deliberately, if time-inconsistently, reweigh costs and benefits each period — the welfare consequences of default stickiness are ambiguous (Bernheim, Fradkin, and Popov 2015). Appendix 1 shows that this class of mechanisms— even allowing for significant enrollment hassle costs, an illiquidity wedge, and intermittent review— is difficult to reconcile with the joint pattern of prolonged prior inaction, intentions to defer further for months, and immediate response to a $10 reward. Under standard parameterizations, a reward-responsive employee should otherwise have been within days of enrolling. The non-deliberative frictions consistent with our data instead suggest that observed inaction need not reveal underlying preferences over retirement consumption, strengthening the welfare case for

plan features that promote engagement, particularly when the foregone match is worth thousands of dollars annually.

These findings connect evidence on the limits of financial information interventions, the behavioral importance of small incentives and cues, and passive choice in retirement saving. The contribution is to show that undersaving may persist not because employees fail to value saving, but because they fail to engage with the current saving decision. This distinction has direct policy implications. Section 113 of SECURE 2.0 permits employers to offer de minimis incentives to encourage plan participation (IRS 2024); our results suggest such incentives may work by creating a moment of engagement rather than by meaningfully changing financial returns. Conversely, our findings imply that popular engagement strategies such as personalized guidance or financial education at enrollment, or campaigns clarifying or increasing the plan match, may have limited success in increasing employee contributions—though we cannot rule out benefits outside of plan engagement or through non-informational channels such as persuasion. This conclusion stands in contrast to the emphasis on such strategies in the academic literature, policy discourse, and industry.[4] More broadly, the findings support plan designs that reduce reliance on active follow-through—including higher defaults and automatic escalation with sufficiently high caps (Thaler and Benartzi 2004)—as well as reforms that make current enrollment status and available actions more transparent.

II. Institutional Setting and Experimental Design

A. Institutional Setting

Our field partner is a major U.S. financial-services firm that, at the time of the 2016 study, employed more than 40,000 benefit-eligible workers and offered a 401(k) plan broadly representative of large automatic-enrollment plans. Beginning in 2015, new hires were automatically enrolled at a default contribution rate of 4 percent into a target-date fund. In June 2015, the firm also conducted an enrollment sweep—a common practice at large firms—automatically enrolling tenured employees contributing below 4 percent at the default rate unless they opted out. At the time of the study, approximately 10 to 15 percent of eligible employees were not participating, a rate typical of plans with automatic enrollment. Contribution changes could be made through the firm's online benefits portal and took effect at the start of the next biweekly pay cycle.

The plan offered a dollar-for-dollar employer match up to 4 percent of salary, with a guaranteed minimum annual match of $2,000 for employees contributing at or above the threshold for the calendar year. Matching contributions were immediately vested for employees hired before 2010 and vested after three years for later hires; roughly 80 percent of the experimental sample was fully vested. For lower-income employees contributing below the threshold, the minimum-match provision implied exceptionally high returns: among employees earning less than $50,000, the marginal return to the next dollar contributed ranged from 100 to 367 percent over the following calendar year. Despite these incentives, 76 percent of employees invited to the study were contributing below the full-match threshold.

B. Experimental Sample and Survey Administration

On July 19, 2016, the firm emailed 5,000 prespecified employees an invitation to complete a ten- to fifteen-minute online survey about workplace benefit programs. The email described the survey as a

[4] For example, 74% of DC plans offered financial education to their employees, while 95% of plans cited education as useful for increasing engagement (2020 PLANSPONSOR survey).

collaboration between the firm and Carnegie Mellon University, designed and administered independently by the research team. Respondents were directed to a personalized Qualtrics link and had ten days to respond. To encourage participation, respondents were entered into a raffle for an Apple iPad.
The first survey module measured candidate saving frictions, including beliefs about required saving, perceived match generosity, awareness of current plan status, financial literacy, and intended timing of future savings. The second module embedded the experimental treatments within a personalized retirement preparedness assessment, allowing us to measure employee-specific frictions before randomizing interventions to address them.

The invitation sample targeted employees at risk of undersaving. The Low Saving Arm included the universe of 3,719 plan-eligible employees aged 25–55, earning below $100,000, and contributing less than 4 percent of pay, including nonparticipants. The Moderate Saving Arm was a random sample of 1,000 employees meeting the same age and income criteria but contributing between 4 and 9 percent. Twenty-eight percent of invited employees participated, a high rate for an online employee workplace survey. After excluding premature exits and employees already contributing at or above their recommended rate, 1,137 employees entered the experiment: 780 in the Low Saving Arm and 357 in the Moderate Saving Arm. Respondents closely resembled invitees in income, tenure, age, gender, and baseline contribution rates, though they were modestly more engaged with the plan (Appendix Table A1).

C. Experimental Design

The friction measures collected in the first survey module are summarized in Table 1. To characterize barriers of intention, we constructed two measures of underestimation of required saving: one direct measure asking employees to estimate the minimum persistent annual contribution rate needed for retirement security, and one indirect measure derived from employees' estimates of retirement age, retirement duration, and income replacement (the three inputs common to most saving calculators). We compared both measures to a conservative benchmark from the firm's saving calculator. We also measured financial literacy and whether employees underestimated the generosity of the employer match.

To characterize barriers to engagement, we compared self-reported contribution status to administrative records, measured underestimation of plan eligibility, and, for a subset of respondents, elicited present focus through effort-task choices with and without a front-end delay.

After completing the baseline module, employees entered a retirement preparedness assessment that delivered the experimental treatments. Assignment to an experimental arm was determined by self-reported contribution rate, and employees were randomized with equal probability to treatments within each arm. Appendix Figure A1 displays a schematic of the research design. Balance tests fail to reject equivalence across conditions (Appendix Table A2).

All employees received a personalized assessment indicating that they were not on track for retirement security, a general recommendation to increase contributions, and brief instructions with a hyperlink to the firm's benefits portal. Employees who declined to adjust their contribution were asked to reconsider, and follow-up questions elicited their saving decision and intended timing of future changes.

In the Low Saving Arm, employees were randomized to one of three treatments layered on this baseline module (see Appendix for images of treatments). The Specific Recommendation treatment replaced the general recommendation with a personalized target contribution rate, testing whether reducing underestimation of required saving increased contributions. The Match Clarification treatment added a screen emphasizing the dollar value of the employer match and illustrating that the match effectively doubled each contributed dollar, testing whether increasing perceived match generosity

increased contributions. The Small Reward treatment added an offer of a $10 Amazon gift card for engaging the enrollment decision that day, either by adjusting contributions or by reporting that the employee had considered but declined a change.

In the Moderate Saving Arm, employees were randomized to either the Generic Recommendation or the Specific Recommendation, providing the cleanest randomized test of whether personalized targets increased contributions among employees already participating. To increase power for the reward comparison, employees in either arm who initially declined to increase contributions and had not yet been offered the reward were independently re-randomized to receive either the standard reconsideration prompt or a version including the $10 reward.

D. Data, Outcomes, and Identification

The analysis links survey responses to administrative records, including employee demographics and pay-cycle-level contribution data from January through November 2016. We construct three primary outcomes: an indicator for any contribution-rate increase, including new enrollment; the change in annual contribution rate; and an indicator for an increase large enough to reach the full-match threshold.

Identification varies across contrasts. In the Moderate Saving Arm, random assignment between the Generic and Specific Recommendation directly tests whether a personalized target increases contributions relative to generic encouragement. In the Low Saving Arm, every condition included the Specific Recommendation, so its effect is not identified by within-arm randomization; we instead benchmark post-intervention changes against the two pay cycles between identification of the experimental sample in June and the July launch of the study. Within-arm randomized contrasts identify the incremental effects of match clarification and the $10 reward layered on top of the Specific Recommendation. We report unadjusted p-values throughout and note where conclusions are sensitive to Benjamini-Hochberg correction; minimum detectable effects and comparison-specific MDEs are reported in Appendix Table A3.

The layered design increases power under a fixed sample size but creates two interpretation issues. First, treatment-cell contrasts may combine component main effects with interactions among components, so Low Saving Arm estimates should be read as effects of adding match clarification or the $10 reward to an environment that already included the Specific Recommendation. The reconsideration-stage reward design is an exception: employees who initially declined to increase contributions and had not yet received the reward were independently re-randomized to a standard reconsideration prompt or one including the reward, identifying the marginal effect of the reward independent of the surrounding information environment. Second, while the pre-study comparison period was chosen to avoid sample selection and provides a reasonable control, we interpret the benchmarked Specific Recommendation estimate as an upper bound, since any response to being surveyed about retirement preparedness, independent of treatment content, is credited to the intervention.

III. Results

A. Deficits in Long-Run Incentive Understanding Are Pervasive but Do Not Explain Undersaving

Table 1 summarizes the baseline incidence of each candidate friction, its cross-sectional association with plan engagement, and whether experimentally targeting that friction increased saving. We begin with barriers of intention. Deficits in retirement-related understanding were widespread: 47 percent of employees underestimated the contribution rate needed for retirement security on a direct

assessment, 43 percent did so on an indirect assessment, and 20 percent of those administered a financial-literacy module answered neither of two questions correctly. Twenty percent underestimated the generosity of the employer match. Two of these deficits showed clear cross-sectional associations with plan behavior: employees who underestimated the match and those with zero financial-literacy scores were significantly less likely to participate and to claim the full match.

Yet correcting the corresponding beliefs experimentally did not move contributions (Figure 1, Appendix Table A4). In the Moderate Saving Arm, where the comparison is cleanly randomized, employees receiving a personalized savings target were no more likely to increase contributions than those receiving generic encouragement ($p = 0.70$). In the Low Saving Arm, the Specific Recommendation produced an estimated effect of 0.02 (se: 0.01) on any contribution increase, comparable to pre-study adjustment rates and sufficiently precise to rule out anything beyond a modest effect. Adding match clarification on top of the Specific Recommendation produced no further response. The result is virtually identical when restricted to the roughly 80% of employees whose matching contributions were vested.

This pattern is not explained by a failure of the information to shift beliefs or by a lack of scope for it to do so. The recommendation substantially raised the share of employees whose perceived required saving met or exceeded the benchmark (Appendix Table A5), confirming that beliefs moved in the intended direction. And the saving null persists within the subgroups for whom each treatment was specifically designed — those who underestimated required saving, underestimated the match, or scored poorly on the financial literacy assessment (Specific Recommendation: 0.01 among required-saving underestimators; Match Clarification: 0.00 among match underestimators), with no detectable differential relative to non-indicated employees (Table 1).

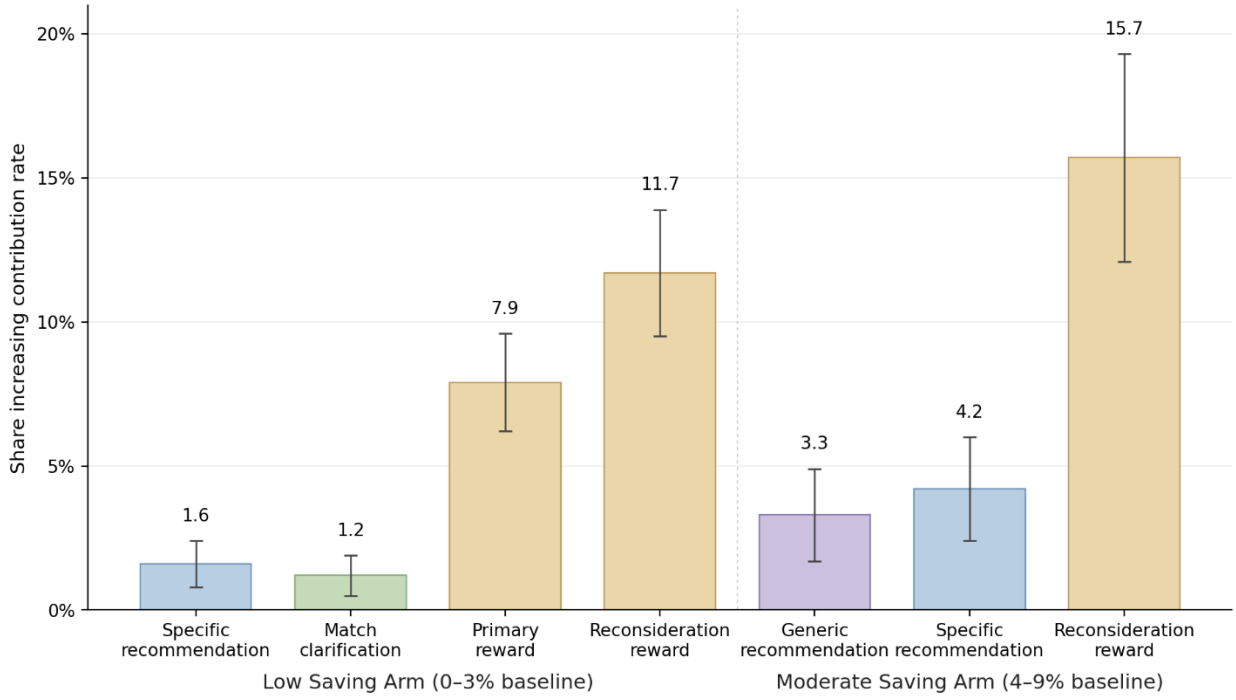

Figure 1. Increased Savings by Experimental Treatment. Bars show the share of employees who increased their 401(k) contribution rate following the intervention, separately for each treatment condition in the Low Saving Arm and the Moderate Saving Arm. Error bars are ±1 standard error.

Table 1.
Synthesis of Survey and Field Evidence on Psychological Frictions and 401(k) Plan Engagement

		Cross-Sectional Difference in Engagement by Indicator			Experimental Response [I(Increased Contribution Rate)]			
Friction Indicator	Baseline Incidence	E(ΔParticipation)	E(ΔMatch Claim)	Treatment [Low Arm if unspecified]	All	Friction Not Indicated	Friction Indicated	Difference Test (p-value)
1. Knowledge of Long-Term Incentives (Intention)								
Direct Underestimation of Required Savings	0.47	0.04	-0.03	Specific Recommendation	0.02	0.02	0.01	0.48
Indirect Underestimation of Required Savings	0.43	-0.03	-0.11***	Specific Recommendation	0.02	0.01	0.02	0.95
Financial Literacy - Zero Score on 2-Item Assessment	0.20	-0.13*	-0.14**	Specific Recommendation	0.02	0.02	0.00	0.72
Underestimation of Plan Match	0.20	-0.16***	-0.14***	Match Clarification	0.01	0.02	0.00	0.55
2. Knowledge of Current Plan Status (Action)								
Overestimation of Plan Contribution Rate	0.24	-0.29***	-0.24***	Small Reward	0.08	0.06	0.21	0.00
Overestimation of Plan Enrollment | Non-Participant	0.38	-	-	Small Reward	0.08	0.06	0.20	0.00
3. Present Focus (Action)								
Present Focus Implied by Effort Allocation Choice	0.10	0.01	-0.04	Small Reward	0.08	0.04	0.14	0.17

Note: This table synthesizes evidence from the survey and field for the three candidate psychological frictions. For each dichotomous friction indicator, the first column reports the baseline prevalence, while the next set of columns summarizes the cross-sectional difference in plan engagement conditioned on whether the friction is indicated or not. The final set of columns report the overall share of employees increasing their contribution rate in response to the specified treatment and the differential experimental response across employees for whom the friction was and was not indicated (estimated from a single pooled regression). Robust standard errors are displayed parenthetically (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Figure 2, which plots perceived, actual and recommended savings rates by employee age, helps explain why correcting beliefs did not shift behavior. Although many employees underestimated required saving—especially those beginning to save later in working life—this underestimation accounted for little of the gap between actual saving and the benchmark. A decomposition of mean differences indicates that replacing the benchmark saving rate with an employee's direct beliefs about how much to save fails to meaningfully reduce the 9.7 percentage point saving gap, while variation in perceived saving requirements does not explain much of the variation in undersaving.[5] Most employees already recognized the direction of the problem: 88 percent understood that they were undersaving, often by substantial margins, and only 5.6 percent of non-responsive Low Saving Arm employees perceived themselves as adequately prepared for retirement. More precise information about the savings target mattered little when employees already knew they were (often, substantially) below that target.

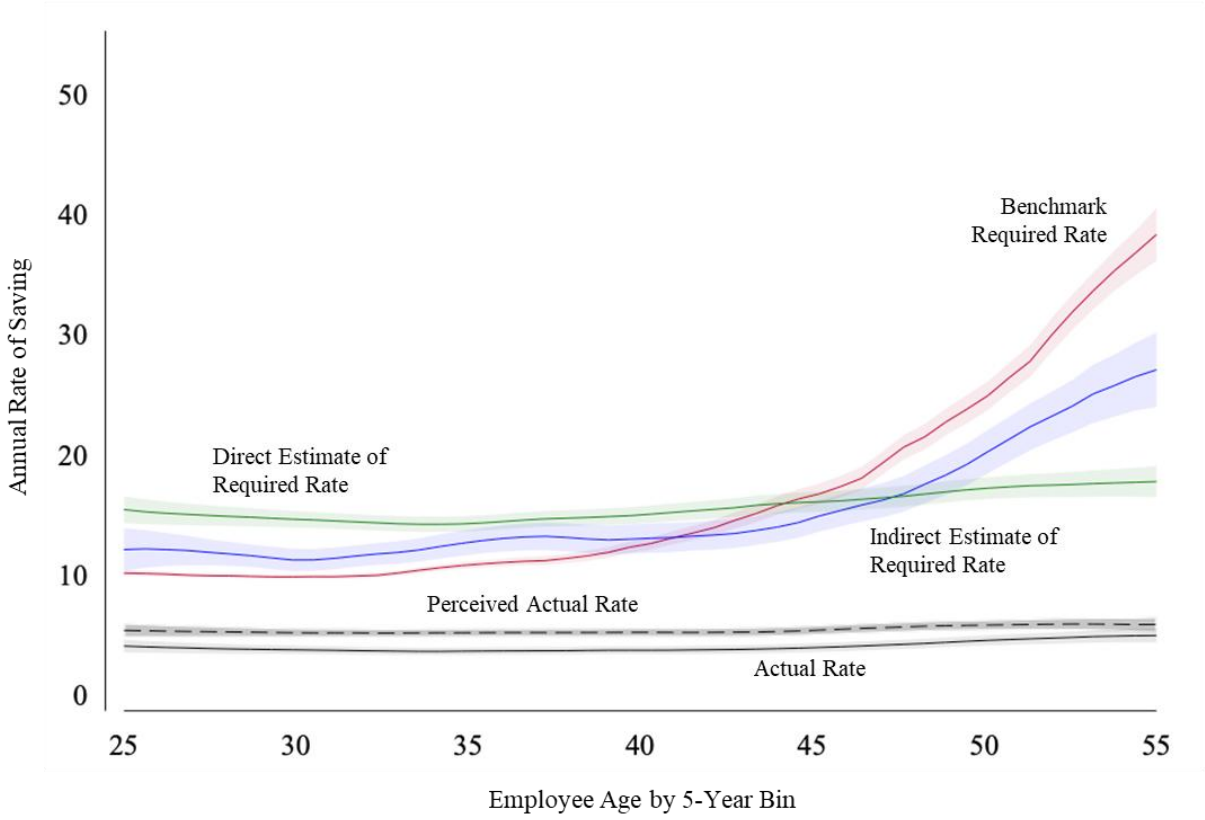

Figure 2. Perceived, Actual, and Recommended Savings Rate by Employee Age. This figure summarizes recommended, actual, and perceived 401(k) annual saving rates by age for surveyed employees. Specifically, the plot depicts the local moving average of the actuarially recommended rate of annual required saving (red), the direct estimate of the required annual saving rate (green), and the indirect estimate of the required annual saving rate (purple) by employee age. The plot also depicts the actual (black) and perceived actual (dashed black) annual saving rates by employee age. Each linear smoother is estimated using a bandwidth of 50 basis points and the shaded region reflects the 95 percent confidence interval for the local mean. Saving rates reflect total 401(k) plan contributions (inclusive of the plan match). Please refer to the text for details underlying the calculation of the actuarial recommendation.

[5] A bivariate regression of benchmark and perceived actual saving yields an adjusted R-squared of -0.007; including employee beliefs on how much to save (direct, indirect, or both) does not increase the model's explanatory power.

Employees were not well informed, but misinformation about long-run saving incentives was not behaviorally pivotal in this setting. The pattern points away from barriers of intention and toward frictions that prevent employees from acting on intentions they already broadly hold.

B. Current-Status Misunderstanding

Although misunderstandings of long-run incentives were common, the survey revealed a more immediate form of confusion: many employees misperceived their current saving status. Table 2 compares self-reported and administratively recorded 401(k) contributions. Twenty-four percent of employees overstated their contribution rate relative to administrative records. Among nonparticipants, 37 percent reported making a positive contribution despite records showing no enrollment, and 26 percent reported contributing enough to claim the full match despite not being in the plan.

Several features suggest these discrepancies are not merely survey noise or social desirability bias. Errors were strongly asymmetric, consisting overwhelmingly of overstatement rather than understatement, and they bunched at salient values, especially the 4 percent match threshold. Conservative exclusions for respondents whose other answers suggested inattention or exaggeration reduced the discrepancy rate among nonparticipants from 0.37 to 0.21 for participation and from 0.24 to 0.14 for full-match take-up; joint adjustment did not reduce these estimates further (Appendix Table A6).

This misunderstanding is behaviorally distinct from misperceiving the match or underestimating required saving. An employee who underestimates the match may still know that she is not fully claiming it. An employee who mistakenly believes she is already enrolled may not perceive any action to be required. Current-status misunderstanding therefore represents a concrete form of engagement failure: passivity may persist not because the employee prefers low saving, but because she misperceives the current state of the decision problem.

This interpretation differs from prior work documenting discrepancies between self-reported and administrative pension records, which typically treats such gaps as measurement error (e.g., Mitchell 1988; Gustman and Steinmeier 2004; Chan and Stevens 2008; Dushi and Iams 2010; Dushi and Honig 2015). In our setting, employees reported their status before being asked whether to change contributions, so incorrect beliefs entered the decision they faced. In the next section, we connect these discrepancies to behavior: employees who overstated their contributions were more than three times as likely to increase saving when offered the small reward.

Corroboration in a National Sample. To assess whether plan-status confusion extends beyond the partner firm, we surveyed 923 full- or part-time employees aged 25–55 on Amazon Mechanical Turk in December 2023. Respondents had incomes below $100,000 and self-reported enrollment in a defined-contribution plan at a contribution rate of 8 percent or less; plans were not restricted to those with automatic enrollment. Eighty-seven percent of respondents reported access to matching contributions. The sample resembled field respondents in age, gender, tenure, and income (Appendix Table A7). The survey did not disclose qualification criteria in advance and included questions on multiple benefit programs to reduce demand effects and strategic reporting.

Plan-status uncertainty was widespread. Nearly half of employees who reported being enrolled were not fully certain of their enrollment status, and 13 percent were unsure whether their plan featured automatic enrollment. This uncertainty was significantly higher among lower-income employees, controlling for demographics and tenure ($b = 0.168$, $p < 0.001$). To corroborate the field overstatement finding, we offered a stratified subset of 569 respondents a $15 gift card for anonymized screenshots of their plan status. Sixty-nine submitted verifiable images, yielding an adjusted overstatement rate of 21

percent (95% CI: 0.11 to 0.31). Although imprecise, this estimate is close to the 24 percent rate in the field data, and roughly half of overestimating employees mistakenly believed they were enrolled when they were not. The exercise is not intended to provide a precise national prevalence estimate, but to show that current-status confusion is not unique to the partner firm or its automatic-enrollment plan.

The field and survey evidence point to plan-status confusion as a potentially important and underappreciated barrier to 401(k) saving. Given the economic stakes of enrollment, confusion of this magnitude may seem surprising. But it is more understandable in the context of the broader complexity of workplace benefits: for example, new hires at our partner firm were asked to make enrollment decisions across as many as twelve benefit programs, with varying rules governing eligibility, default enrollment, and employee action.

Table 2.
Discrepant Employee Reports of 401(k) Plan Engagement

Type of Discrepancy	All Sample	Plan Participation		Difference Tests
		No	Yes	(p-value)
Discrepant Contribution [1,0]				
Any Discrepant Contribution	0.28	0.37	0.22	0.00
Self-Reported Contribution Rate > Actual Rate	0.24	0.37	0.15	0.00
Self-Reported Contribution Rate < Actual Rate	0.04	--	0.07	--
Discrepant Enrollment Status [1,0]				
Discrepant Self-Report of Participation	0.37	0.37	--	--
Discrepant Self-Report of Non-Participation	0.01	--	0.01	--
Discrepant Self-Report of Full Match Take-Up	0.19	0.26	0.10	0.00
Discrepant Self-Report of Less Than Full Match Take-Up	0.04	--	0.04	--
Average Rate Discrepancy | Discrepant Overreport [%]	3.17	3.81	2.03	0.00

Note: This table summarizes discrepancies between the self-reported and administrative 401(k) plan engagement of employees. Each row reports the average discrepancy associated with the indicated measure for the entire employee sample and for subsamples distinguished by administrative participation. The final column reports a p-value from a t-test of mean differences in discrepancy across plan participation. We identify participation and discrepancies based on administrative records as of the last pay date prior to the survey invitation.

C. Microincentives Increased Saving

The $10 reward produced a sharply different pattern of response from information (Figure 1, Appendix Table A4). In the Low Saving Arm, adding the reward to match clarification increased the probability of any contribution-rate increase by 8 percentage points, increased the average contribution rate by 15 basis points of salary, and increased full match take-up by 4 percentage points. All three contrasts are statistically significant and survive Benjamini-Hochberg adjustment. Similar effects appear in the reconsideration-stage randomizations in both arms. Across implementations, 8 to 16 percent of employees increased saving in response to the reward, and 10.5 percent of all employees offered the reward increased contributions.

The primary Low Saving Arm treatment layers the $10 offer onto an environment that already clarified the match. The reconsideration-stage re-randomization — which held the survey environment fixed and varied only the $10 offer among employees who initially declined to act — produced comparable effects, indicating that this layering does not drive the result. The key finding is therefore not

simply that information was insufficient, but that adding a small immediate incentive to engage the decision generated behavior change where information about long-run incentives alone did not.

The economic magnitude of the response was large relative to the reward. For employees with unclaimed matches, reward-induced contribution changes implied median additional employer contributions of $583 (maximum $1,583) over the remainder of the calendar year. If contribution rates are sustained, implied annual matching gains approach $2,000 (maximum $3,800), though we observe behavior only through November. Several features suggest the response reflects neither timing nor a temporary attempt to claim the gift card: most contribution changes exceeded one percentage point, many reached the full-match threshold, and no systematic reversal appeared in the pay cycles we observed.

The reward response is difficult to reconcile with standard economic explanations for prior inaction. If employees failed to increase contributions because fixed enrollment costs exceeded the value of the match, or because liquidity constraints made higher contributions infeasible, a $10 reward should not have induced persistent contribution increases whose per-paycheck costs exceeded the reward. Consistent with this interpretation, in the subsample for whom we elicited financial circumstances, 61 percent reported emergency savings sufficient to cover at least three months of expenses, and the reward effect did not differ meaningfully between liquidity-constrained and unconstrained employees. These patterns suggest that many employees recognized the value of saving but failed to translate that recognition into action.

The evidence instead points to barriers to engagement with the present decision.[6] A microincentive (a small, immediate reward) may create an active-decision moment analogous to active-choice enrollment designs (Carroll et al. 2009). The structure of stated intentions reinforces this interpretation. Among employees not currently saving at the recommended rate, a substantial majority reported planning to increase contributions within the coming year, yet most anticipated doing so only after a delay of at least one month. These employees were not reporting an absence of motivation; they were reporting a plan to defer. The gap between those stated intentions and prior behavior—many had left matching contributions unclaimed for months or years—suggests that delay itself was part of the friction.

Status confusion appears to be one important, but not exclusive, channel through which the reward operated. As Figure 3 depicts, employees who overstated their current contributions were more than three times as likely to increase contributions in response to the reward as employees who correctly reported their status (0.21 vs. 0.06, $p < 0.01$). This heterogeneity is reward-specific: the confused-minus-unconfused response gap was -1.8 percentage points under the specific recommendation and +2.2 percentage points under match clarification, compared with +15.2 percentage points under the reward, and a joint test rejects equality of the reward gap with the two informational-treatment gaps ($p = 0.038$). Confused employees were therefore not generically more susceptible to any nudge, though Figure 3 also shows a meaningful reward response among unconfused employees, consistent with the reward addressing engagement frictions beyond status confusion alone.

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[6] Two alternative interpretations of the reward effect merit consideration. First, the reward could have operated through experimenter demand or employer signaling. Because all conditions informed employees that they were not on track and prompted them to consider increasing contributions, demand would predict elevated response across conditions rather than the differential concentrated in the reward arm that we observe. The reconsideration-stage re-randomization — which held the survey environment fixed and varied only the $10 offer — produced comparable effects, further inconsistent with demand as the primary channel. Second, the reward could have operated through reciprocity or gift exchange. The threefold differential response among employees who overstated their current contributions is difficult to reconcile with reciprocity as the primary channel, since gift-exchange motives should not vary systematically with prior misperception of plan status.

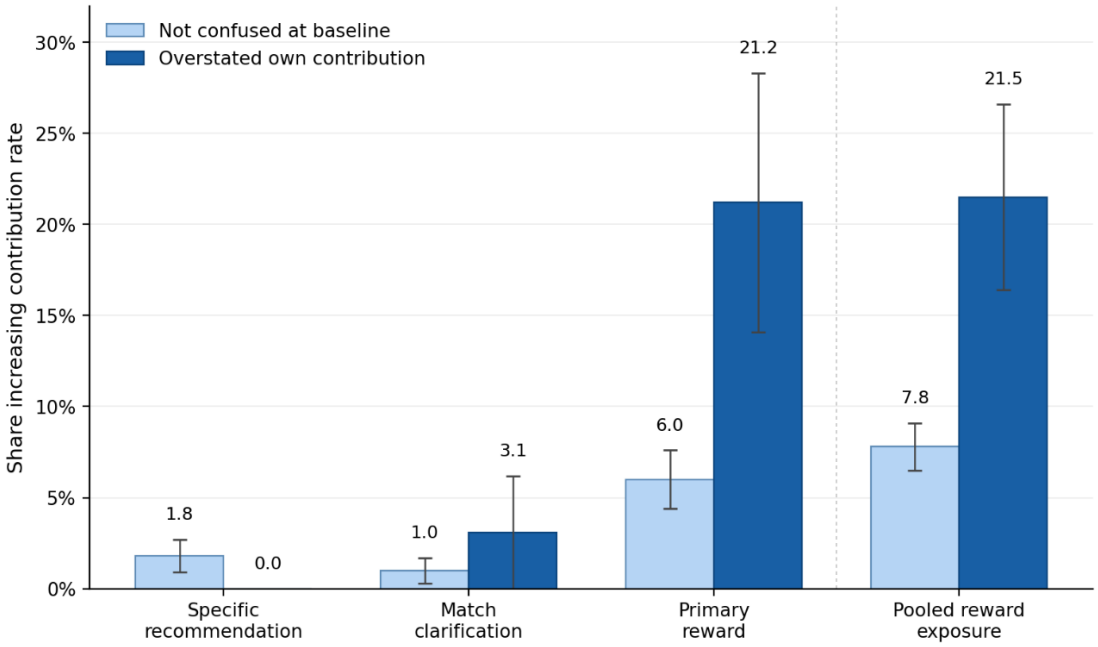

Figure 3. Increased Savings by Treatment and Baseline Confusion Status in the Low Saving Arm. Bars show the share of employees who increased their 401(k) contribution rate following the intervention, separately for employees whose self-reported contribution rate matched administrative records (light blue), and those who overstated it (dark blue). "Pooled reward exposure" combines the primary-stage reward arm with the reconsideration-stage reward re-randomization. Error bars are ±1 standard error. Cell Ns: Specific Recommendation (252 / 220 / 32), Match Clarification (241 / 209 / 32), Primary Reward (249 / 216 / 33), Pooled Reward Exposure (465 / 400 / 65).

This pattern bears on the welfare interpretation of our findings. Prolonged inaction in the face of large financial stakes can be rationalized by a class of explanations in which employees actively engage with the saving decision but deliberately defer it — most prominently β–δ models of present-biased procrastination. Within this class, the welfare consequences of inaction are ambiguous: time-inconsistent deferral can still reveal stable underlying preferences over retirement consumption (Bernheim, Fradkin, and Popov 2015). Appendix 1 formalizes why the full pattern observed here — sustained prior inaction, an abrupt response to a $10 reward, and stated intent to continue delaying — is difficult to reconcile with this class. Under standard parameterizations, an employee induced to act by a $10 reward must have been close to the enrollment margin and, absent the reward, should have acted within days. That prediction is hard to square with the years of prior inaction or the delayed stated intentions in our data. The evidence therefore points less to deliberate, repeated postponement than to frictions that prevent employees from actively confronting the saving decision: limited attention, forgetting, decision avoidance, mistaken beliefs about future decision costs, or imperfect knowledge of current status.

This mechanistic distinction sharpens the case for intervention. When inaction reflects non-engagement or misunderstanding of current status rather than deliberate, preference-revealing deferral, observed behavior is unlikely to capture employees' true preferences over retirement consumption — and the welfare case for plan features that promote engagement or transparency is correspondingly stronger, particularly when the foregone employer match is worth hundreds or thousands of dollars per year.

IV. Conclusion

Three findings emerge from a field experiment among undersaving employees at a large firm with automatic enrollment and generous matching incentives. First, deficits in understanding long-run saving incentives were widespread: nearly half of employees underestimated the contribution rate needed for retirement security, and one in five underestimated the employer match. Yet experimentally correcting these beliefs did not increase contributions. Information improved belief accuracy but did not change behavior, suggesting that misunderstanding of long-run incentives, while common, was not the primary binding constraint in this setting.

Second, the study uncovered a consequential form of current-status misunderstanding. A substantial share of employees, including 37 percent of plan nonparticipants, overstated their contributions relative to administrative records. A national corroboration exercise produced a comparable, though imprecisely estimated, overstatement rate among low-saving workers and documented widespread uncertainty about plan status, especially among lower-income employees. These findings suggest that some employees may fail to act not because they reject saving, but because they misperceive the current state of the decision problem.

Third, a $10 immediate reward increased saving where clarification of much larger delayed incentives did not. Among employees with unclaimed matches, reward-induced contribution increases implied median additional matching contributions of $583 over the remaining calendar year and roughly $2,000 annually if sustained. The reward was roughly three times more effective among employees who overstated their contributions, consistent with the incentive prompting some workers to discover their true enrollment status. This asymmetry points to a friction tied to engagement with the present decision rather than to insufficient motivation or misunderstanding of long-run returns.

Collectively, the evidence implicates frictions that prevent employees from actively confronting the saving decision. Current-status misunderstanding is one measurable manifestation of these frictions, but limited attention, forgetting, inertia, and decision avoidance may also prevent employees from acting on intentions they already broadly hold. A calibration exercise shows that the combination of prolonged prior inaction, delayed stated intentions, and immediate reward response is difficult to reconcile with repeated conscious deferral under standard β–δ parameterizations.

Several limitations are worth noting. The field evidence comes from a single firm and a targeted population of undersaving employees, and the experiment was conducted in 2016. While benefit environments have evolved, the national survey suggests that uncertainty about current plan status remains widespread. In addition, the randomized contrasts identify treatment effects, but the mapping from those effects to mechanisms relies partly on observed associations: current-status misunderstanding is measured rather than experimentally manipulated, and the national verification sample is modest. These considerations limit precise statements about prevalence and channels. Still, the core empirical pattern, a sharp behavioral response to a small immediate incentive alongside insensitivity to large delayed incentives, is difficult to reconcile with models based solely on information deficits or standard cost-based explanations.

In the near term, the findings support small immediate incentives to prompt plan engagement, as recently permitted under Section 113 of SECURE 2.0, and suggest that such incentives may work less by altering financial tradeoffs than by creating a moment of engagement with the saving decision. Conversely, strategies emphasizing personalized guidance, financial education, or match clarification may have limited success in raising contributions, at least through the engagement channels typically envisioned. More broadly, the results reinforce the value of plan-design features that reduce reliance on

active employee follow-through, including higher default contribution rates, automatic escalation with sufficiently high caps, simplified benefit portals, and personalized notifications showing employees their current contribution rate, any unclaimed match, and the steps needed to capture it. Policies to increase retirement saving should address not only what employees understand about the future, but whether they perceive and act on the decision they face today.

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Supplemental Appendix for

Overcoming Frictions in Retirement Savings with Micro-incentives

Saurabh Bhargava & Lynn Conell-Price

April 2026

1. Calibration of β-δ Models of Delayed Saving

This appendix evaluates whether a benchmark class of models based on β-δ present-biased procrastination (O'Donoghue and Rabin 1999b; Laibson 1997) can rationalize the joint pattern documented in the field experiment. In this class of models, employees correctly understand the long-run return to saving but may delay enrollment because of fixed adjustment costs, the illiquidity of retirement contributions, and, when $\beta < 1$, present-biased preferences.

The calibration asks whether plausible parameter values can generate three empirical patterns observed in the data: (i) prolonged prior inaction, as many employees had left employer matching contributions unclaimed for months or years; (ii) delayed stated intentions, as most employees who planned to save more anticipated doing so only after a delay of months rather than days; and (iii) in the reward subsample, immediate response to a one-time $10 incentive that increased saving.

The first two patterns characterize the broad sample of undersavers, while the third applies only to the subset randomized to the reward treatment. We adapt the calibration framework from the longer working paper (Bhargava and Conell-Price 2021), treating beliefs about the benefits of saving as approximately correct, an assumption motivated by the experimental evidence showing that improving beliefs about saving needs and match generosity did not increase contributions.

A. Setup

An employee chooses whether to increase contributions to the threshold required for full-match take-up. Periods are indexed in business days. Utility is

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

where $\delta \in (0,1)$ is the standard discount factor and $\beta \in (0,1]$ captures present bias. When $\beta = 1$, the model reduces to the standard exponential-discounting framework.

Let k denote the one-time utility cost of increasing the contribution rate, inclusive of time and hassle. A wage-based calibration with a Benzarti (2020)-style hassle multiplier suggests a plausible range of roughly $50 to $200, corresponding to 30 minutes to two hours valued at about four times the wage-based opportunity cost.

Let b denote the per-period utility gain from contributing at the match threshold rather than remaining below it. For a representative employee earning $50,000 annually, contributing at the 4 percent threshold, and facing equal current and retirement tax rates of 25 percent, the daily net benefit is approximately $6.

To allow for the possibility that retirement contributions are less valuable than current consumption due to withdrawal restrictions, illiquidity, or the inability to smooth consumption against near-term shocks, we introduce a reduced-form liquidity wedge $\lambda \in (0,1]$, so that the effective per-period benefit becomes λb. When $\lambda = 1$, retirement dollars are valued one-for-one. A value of $\lambda = 0.50$ implies that employees value retirement resources at half their face value, which accommodates substantial consumption-smoothing

concerns or the combined effect of withdrawal restrictions, penalties, and taxation. We treat $\lambda = 0.50$ as a conservative benchmark, noting that even substantially lower values do not qualitatively change the conclusions.

The benchmark model assumes employees reconsider the enrollment decision each business day. Because financial decisions may instead be revisited only intermittently, due to limited attention, forgetting, or infrequent account monitoring, we also consider a generalization in which the employee reviews the decision only with probability p on a given business day. Conditional on review, the employee solves the same β-δ decision problem described above. The benchmark case corresponds to $p = 1$.

B. Maximum Rationalized Delay

For a sophisticated β-δ decision-maker who anticipates future procrastination (O'Donoghue and Rabin 1999b), the maximum delay before enrollment occurs when the present-discounted cost of enrolling today equals the discounted stream of benefits from saving. Under daily review ($p = 1$), the maximum delay is

$$T^* = \frac{k(1 - \delta\beta)}{\beta\lambda b}.$$

Standard model ($\beta = 1$). When preferences are time-consistent, the model predicts either immediate enrollment or no enrollment at all, depending on whether k falls below or above $\lambda b/(1 - \delta)$. Under our parameterization ($b = \$6$, $\delta = 0.9997$), this threshold is approximately \$19,994 when $\lambda = 1$ and \$9,997 when $\lambda = 0.50$. Both values far exceed plausible enrollment costs. The standard model therefore cannot rationalize prolonged non-enrollment under stable conditions: it predicts that employees should either enroll immediately or never but not delay for months while intending to act in the future.

Present bias with daily review ($p = 1$). Allowing $\beta < 1$ introduces the possibility of deliberate repeated deferral. But the maximum delays the model can sustain remain short relative to the data. With $\beta = 0.7$ and enrollment costs ranging from $k = \$50$ to \$200, the model rationalizes maximum delays of only 1 to 14 business days under daily review without the liquidity wedge. When combined with a liquidity wedge of $\lambda = 0.50$, the maximum delay increases to about 29 business days.

Present bias with intermittent review ($p < 1$). If employees reconsider the decision only intermittently, enrollment opportunities occur less frequently in calendar time. Let p denote the probability that the employee reviews the saving decision on a given business day. Because, on average, $1/p$ business days elapse between successive reviews, the maximum delay in days is approximately $T_p^* \approx \frac{T^*}{p}$.

This extension nests mechanisms emphasized in the literature on financial decision-making, including limited attention, forgetting, and infrequent account monitoring.

Appendix Table A8 reports the implied maximum delays under representative parameter values, maintaining the conservative liquidity-wedge benchmark $\lambda = 0.50$ throughout. Under daily decision review ($p = 1$), the model generates delays of 2 to 29 days, depending on the degree of present bias and the assumed enrollment cost. Allowing for intermittent review substantially increases the maximum

delay: for example, when $\beta = 0.7$ and $k = \$200$, the maximum delay rises from 29 days under daily review to 71 days under twice-weekly review ($p = 0.40$) and 143 days under weekly review ($p = 0.20$).

While intermittent review can reconcile the model with longer intended delays, it fundamentally changes the interpretation of inaction. Delay no longer reflects deliberate, repeated deferral, the hallmark of β-δ procrastination, but rather infrequent engagement with the saving decision. Under this interpretation, the model's ability to generate long delays comes not from the present-bias mechanism but from the assumption that employees rarely think about saving, which is itself a form of limited attention rather than preference-based procrastination.

Naive agents. The preceding analysis assumes sophistication: the employee correctly anticipates her future procrastination. Under naivete (O'Donoghue and Rabin 1999b), the employee incorrectly believes she will act in the next period and therefore never strategically adjusts her enrollment timing. A fully naive β-δ agent can procrastinate indefinitely, since she always expects to act tomorrow, and this version of the model can therefore rationalize arbitrarily long delays, seemingly resolving the tension between the model and the observed delays in enrollment.

However, the survey evidence on stated intentions argues against full naivete as a characterization of our sample. A fully naive agent should report an intention to act immediately: she believes, incorrectly, that she will follow through tomorrow. In contrast, most undersaving employees who planned to increase contributions reported intending to do so not immediately but after a delay of several months to a year.

This pattern is more consistent with partial sophistication, or with employees recognizing that some friction will impede near-term action, even if they are mistaken about its nature or persistence, than with the prediction of naive present bias.

C. Response to the $10 Reward

The reward provides an additional calibration target. Under the β-δ model with daily review and the liquidity-wedge benchmark $\lambda = 0.50$, an employee who enrolls immediately in response to a $10 reward but would not otherwise enroll must occupy a very narrow region of parameter space: with $\beta = 0.7$, such an employee must have $k \in [7,17]$ and, absent the reward, would have delayed by no more than about 2.4 business days. With $\beta = 0.9$, the employee must have $k \in [27,37]$ and would have delayed by no more than about 1.4 business days.

Allowing intermittent review can lengthen delays, but it clarifies rather than overturns the calibration logic. If the reward operates as a pure incentive, it can change behavior only on a day when the employee reviews the saving decision; conditional on review, the β-δ enrollment threshold remains narrow, so a respondent who enrolls because of the $10 reward must have been close to enrolling already. That implication is difficult to reconcile with the observed data: many responsive employees had left the match unclaimed for months or years, and a significant portion of undersaving employees who intended to save more reported plans to wait several months before doing so.

Alternatively, the reward may have worked by inducing review itself: the email, deadline, or small payment may have prompted employees to pay attention to a decision they otherwise would not have considered. But this interpretation points away from β-δ procrastination as a sufficient explanation. In that

case, the reward changes behavior not because it materially improves the return to saving, but because it serves as a cue that brings the saving decision to mind or makes the need to act salient.

D. Interpretation

The calibrations suggest that a benchmark class of models, in which employees hold approximately correct beliefs about the return to saving and delay solely because of fixed costs, illiquidity, and present bias, is quantitatively strained by the data. The β-δ model formalizes a specific class of action barriers in which the employee is aware of her current status, recognizes the need to act, and makes a deliberate, if time-inconsistent, decision each period to defer. Under plausible parameters, even allowing for substantial illiquidity and intermittent decision review, this framework struggles to jointly rationalize months-long prior inaction and immediate response to a small reward.

Importantly, the calibration does not rule out, and is broadly consistent with, several alternative classes of action barriers that lie outside the β-δ framework. These include:

Current-status confusion: the employee does not perceive a need to act because she mistakenly believes she is already enrolled or contributing at a sufficient rate.

Limited attention or forgetting: the saving decision never reaches consciousness on a given day, consistent with reminder-based models (e.g., Ericson 2017; Karlan et al. 2016) in which small prompts trigger action by making the decision salient.

Decision avoidance: the employee avoids engaging with the decision entirely rather than weighing costs and benefits and deliberately postponing action.

Belief-based procrastination: the employee expects acting to feel easier in the future due to mistaken beliefs about future decision costs.

What unifies these alternatives, and distinguishes them from β-δ procrastination, is that inaction is sustained by mistakes or lack of engagement rather than by a deliberate intertemporal tradeoff. The calibration therefore reinforces the interpretation offered in the main text: the relevant action barriers appear to be those that remove the perceived need to act or prevent the decision from arising, rather than those that merely raise the perceived cost of acting today relative to tomorrow.

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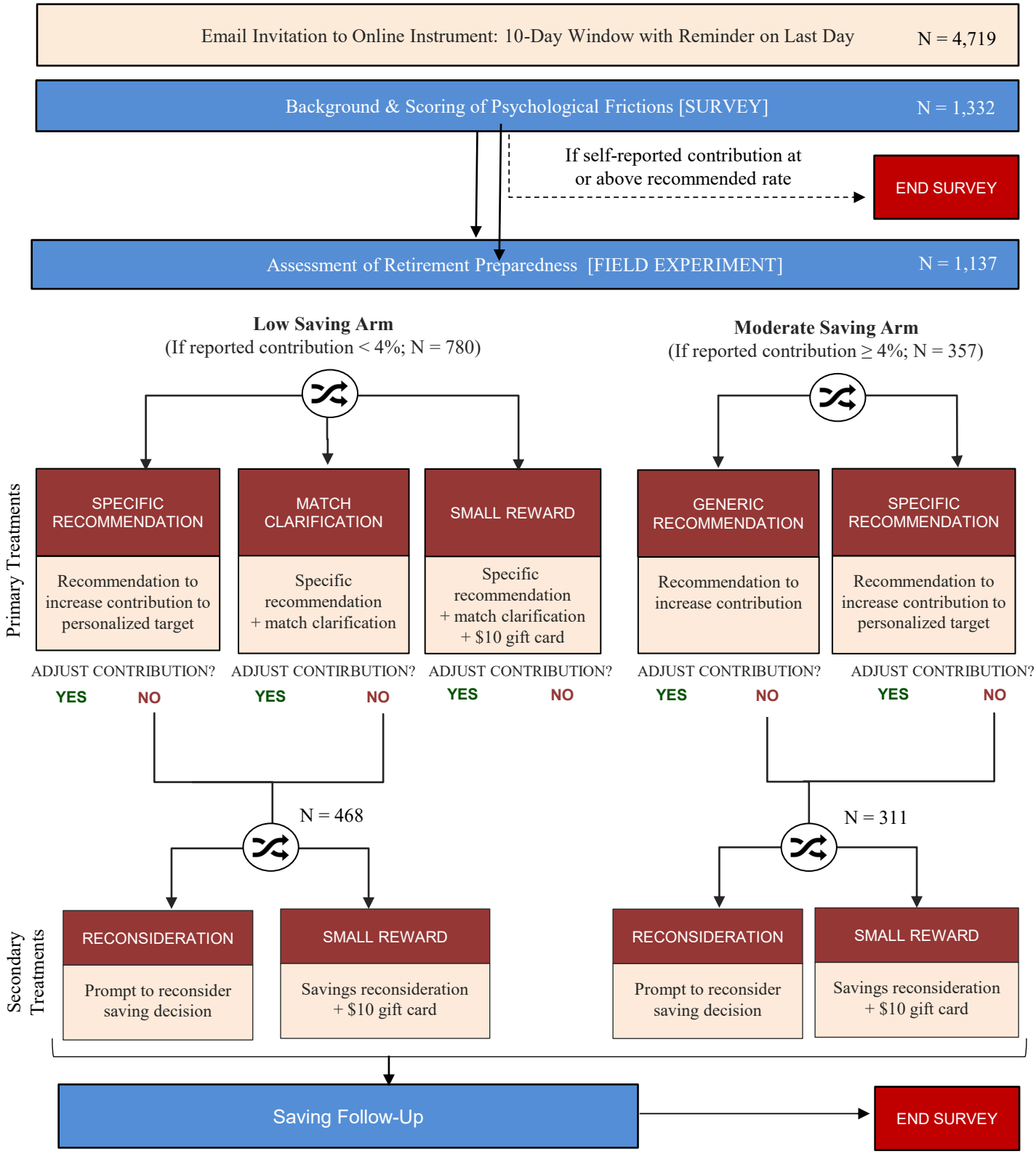
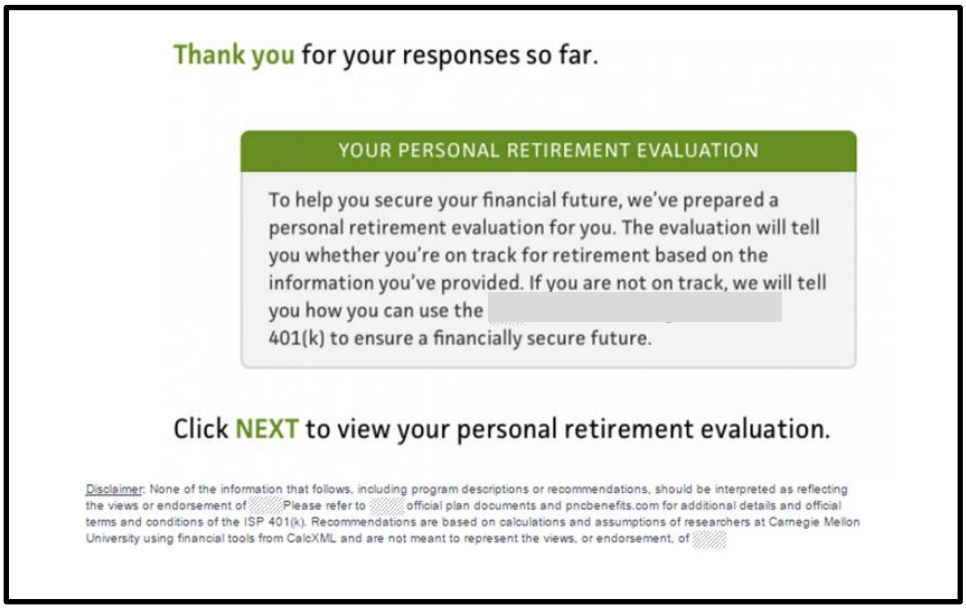
Appendix Figure A1

Schematic Research Design for Survey and Field Study

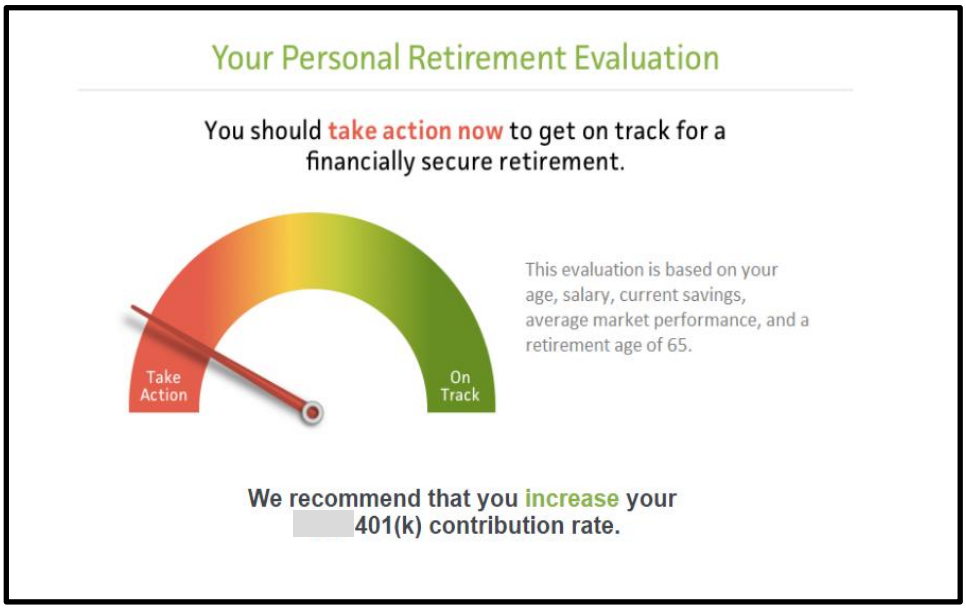


Note: This figure schematically depicts the research design and general procedure for the survey and field study. The figure charts the progression of qualified employees from an email invitation to participate in the survey to an initial module designed to collect background information and score each respondent on measures associated with the candidate frictions. The figure then describes a second module of the survey which provides respondents with an assessment of their retirement preparedness in the context of several randomized elements that constitute the field study (randomization denoted by pictograph). While the module informed all respondents as to their lack of preparedness, respondents proceeded through a subsequent web-flow determined by their study arm and assignment to a primary experimental treatment as shown in the figure. Respondents who did not report changing their plan contribution after the initial set of treatments were then asked to reconsider their decision in the context of a secondary set of treatments. Finally, the survey presented respondents with follow-up questions about their savings decision and future intentions to save. Inferences about any change in employee contribution, in response to the field study, rely on administrative data from the pay dates following the end of the survey period and preceding the survey invitation.

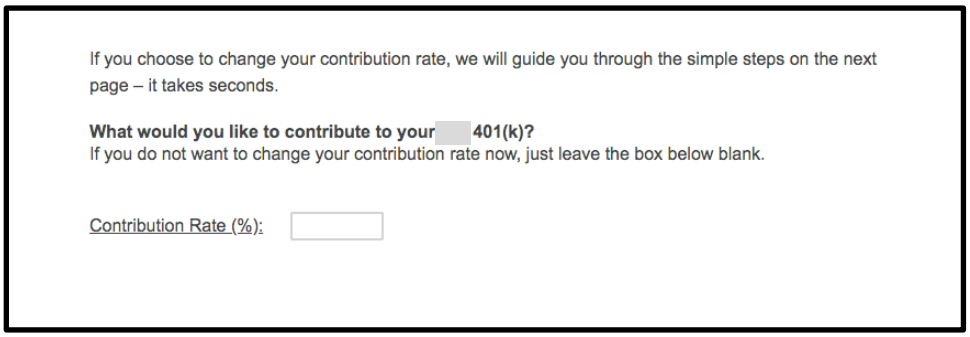
Appendix Figure A2
Screenshots of Baseline Retirement Assessment Web-Flow from Field Study
(Generic Recommendation)



Introductory Screen



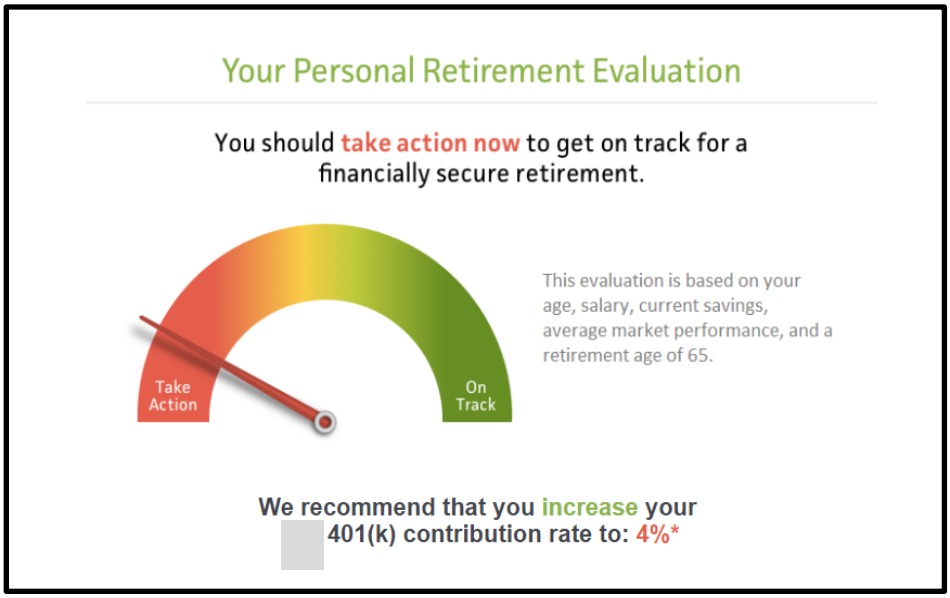
Retirement Assessment
(Generic Recommendation)



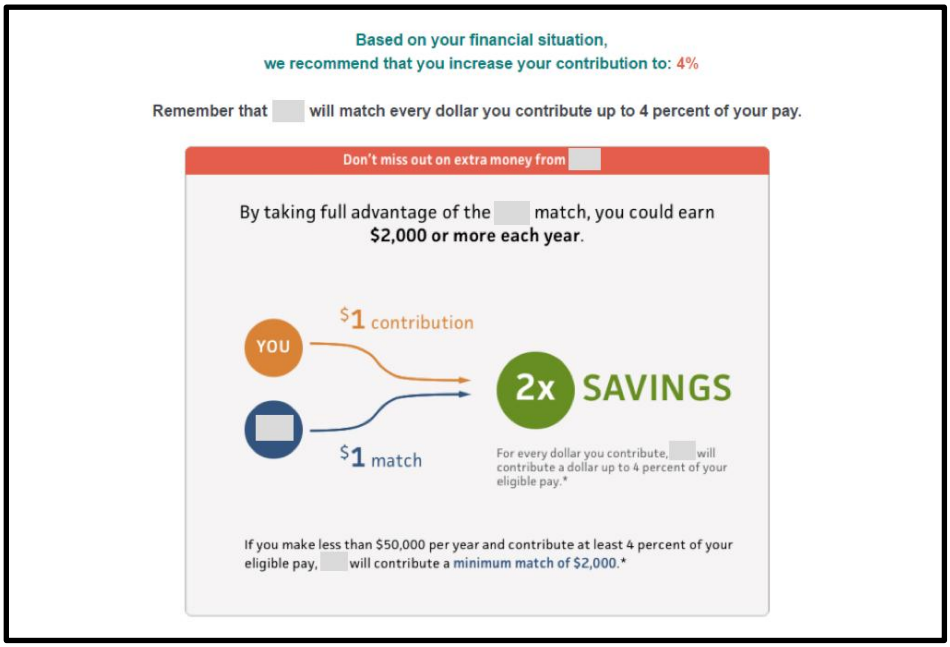
Saving Prompt
(Prior to Benefit Portal Link)

Appendix Figure A3

Screenshots of Experimental Variation in Retirement Assessment Web-Flow from Field Study



Specific Recommendation



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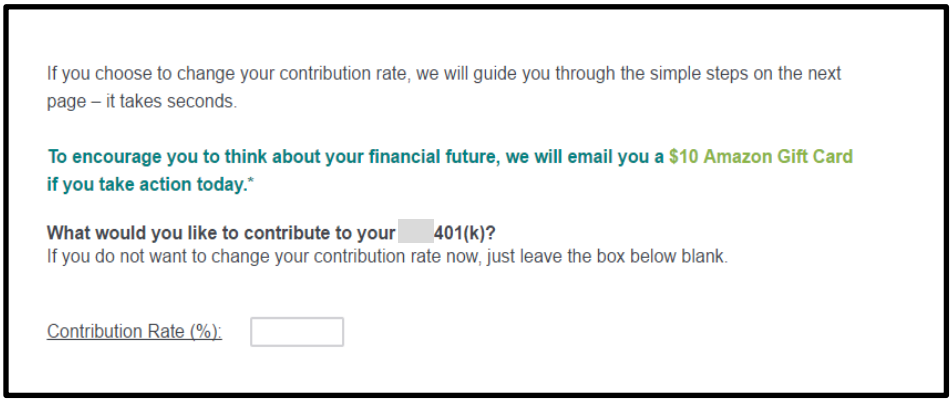

Small Reward

Appendix Table A1.
Summary of Employee Demographics and 401(k) Plan Engagement

	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
Income (imputed) [$ thousands]	51.0	21.31	49.23	20.59	52.63	21.83	0.00
401(k) Saving Behavior							
Participation [1,0]	0.52	0.50	0.0	-	1.0	0	-
Contribution Rate [% annual pay]	1.7	2.4	0.0	-	3.2	2.5	-
Saving Rate (inclusive of plan match) [% annual pay]	3.3	4.2	0.0	-	6.3	3.8	-
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
Income (imputed) [$ 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
Financial Liquidity (N = 227)							
Emergency Savings < 3-Month Expenses [1,0]	0.68	0.47	0.75	0.43	0.63	0.48	0.05
Emergency Liquidity < 3-Month Expenses [1,0]	0.39	0.49	0.48	0.50	0.32	0.47	0.01
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]	2.6	2.4	1.4	2.2	3.5	2.1	-
Saving Rate (including plan match) [% annual pay]	3.8	4.2	0.00	-	6.48	3.54	-
Full Match Take-Up [1,0]	0.28	0.45	0.00	-	0.48	0.50	-

Note: This table summarizes available demographic, financial, and plan engagement details for employees in two analytic samples. Panel A describes employees invited to participate in the field study as of July 2016 while Panel B describes employees who responded to the invitation and at least partially completed the online survey (see text for inclusion criteria). The varying sample sizes across measures reflect the random assignment of respondents to select survey modules (e.g., financial liquidity). We imputed income from administrative data on salary decile and used the imputed income to calculate matching contributions for any employee presumed to be eligible for the minimum match.

Appendix Table A2.
Tests of Covariate Balance across Experimental Treatments

	Low-Saving Arm				Moderate-Saving Arm		
	Specific Recommendation	Match Clarification	Small Reward	Difference Test (p-value)	Generic Recommendation	Specific Recommendation	Difference Test (p-value)
Employee Characteristics							
N =	262	262	256	-	179	178	-
Male [1,0]	0.33	0.35	0.33	0.89	0.31	0.34	0.63
	(0.03)	(0.03)	(0.03)		(0.03)	(0.04)	
Age [Yrs]	39.6	38.8	38.8	0.39	43.63	43.7	0.94
	(0.50)	(0.51)	(0.51)		(0.52)	(0.51)	
Tenure [Yrs]	8.1	7.8	8.2	0.82	10.9	10.5	0.66
	(0.47)	(0.39)	(0.45)		(0.64)	(0.65)	
Income (imputed) [$ thousands]	50.1	48.7	49.2	0.73	59.5	59.1	0.87
	(1.21)	(1.22)	(1.25)		(1.71)	(1.77)	
401(k) Savings Behavior							
Participation [1,0]	0.49	0.48	0.46	0.76	0.78	0.75	0.52
	(0.03)	(0.03)	(0.03)		(0.03)	(0.03)	
Contribution Rate [% annual pay]	0.88	0.91	0.81	0.59	3.49	3.49	0.99
	(0.07)	(0.08)	(0.06)		(0.18)	(0.19)	

Note: This table summarizes the characteristics of the employees across assigned 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. The sample described here includes all employees in the Low-Saving or Moderate-Saving Arm assigned based on self-reported contribution rate, including 132 employees with discrepant self-reported contribution rates who would have been assigned to the other arm based on contribution rates observed in administrative data at the last pay date before the study.

Appendix Table A3.
Minimum Detectable Effect Sizes

Intervention and Comparison Context	MDE (pp)
Specific Recommendation	
Pooled	1.94
Low Arm (Pre-study Control)	2.12
Moderate Arm	5.45
Match Clarification	3.09
Small Reward	
Pooled	1.91
Low Arm - Primary	2.19
Low Arm - Reconsideration	3.98
Moderate Arm - Reconsideration	3.39

Note: This table reports minimum detectable effect sizes (MDEs) for the binary outcome indicating any increase in 401(k) contribution rate. MDEs are reported in percentage points and are computed using one-sided tests (alpha = 0.05) with 80 percent power, based on comparison-specific sample sizes and control-group outcome moments. “Pooled” rows combine the relevant intervention contrasts (as described in the text). The Low-Arm pre-study comparison uses the pre-survey pay-cycle benchmark and is not a contemporaneous randomized control comparison.

Appendix Table A4.
Marginal Effect of Experimental Treatments on 401(k) Plan Engagement

	Low-Saving Arm			Moderate-Saving Arm	
	Dependent Variable			Dependent Variable	
Experimental Treatment	Contribution Rate Increase (1,0)	Δ Contribution Rate	Δ Full Match Take-Up (1,0)	Contribution Rate Increase (1,0)	Δ Contribution Rate
Panel A. Primary Treatments					
Generic Recommendation [GR]	--		--	0.03**	0.09
				(0.02)	(0.05)
Specific Recommendation [SR]	0.02	0.02*	0.00	0.04**	0.07**
	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)
Match Clarification + SR [MC]	0.01*	0.02	0.01	--	--
	(0.01)	(0.02)	(0.01)		
Small Reward + MC [Reward]	0.08***	0.15***	0.04***	--	--
	(0.02)	(0.04)	(0.01)		
N	763	763	763	242	242
F-Tests of Coefficient Equality (p-value)					
SR v. GR	--		--	0.70	0.722
MC v. SR	0.73	0.822	0.157	--	--
Reward v. MC	0.00	0.004	0.033	--	--
Pre-Study Comparison	0.014	-0.037	0.000	0.017	-0.029
	(0.004)	(0.017)	--	(0.008)	(0.031)
Panel B. Secondary Treatments					
Reconsideration [Recon]	0.03**	0.03	0.01*	0.01	-0.03
	(0.01)	(0.03)	(0.01)	(0.01)	(0.04)
Small Reward + Reconsideration [Reward]	0.12***	0.18***	0.04***	0.16***	0.19***
	(0.02)	(0.06)	(0.01)	(0.04)	(0.05)
N	455	455	455	213	213
F-Test of Coefficient Equality (p-value)					
Reward v. Reconsideration	0.00	0.00	0.07	0.00	0.00

Note: This table summarizes marginal changes in plan contributions in response to the experimental treatments as estimated through a series of regressions predicting saving outcomes—contribution rate increase (1,0), contribution rate change (percent of salary), and contribution rate increase resulting in full match take-up (1,0)—as a function of indicators for treatment assignment, with a suppressed constant. Panel A summarizes the response of employees to the primary treatments while Panel B summarizes response to the secondary treatments. To facilitate comparisons between treatments, both panels report p-values from pairwise F-tests of coefficient equality, and Panel A additionally reports an out-of-sample reference of plan contribution changes by in-sample employees during the period prior to the study. The first three columns report the share of increased plan engagement among employees in the Low Arm while the final two columns describe the increase in plan engagement for employees in the Moderate Arm. Inferences about any change in employee contribution rates in response to the field study rely on administrative data from the pay dates following the end of the survey period and preceding the survey invitation. Robust standard errors are displayed parenthetically (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Appendix Table A5.
Marginal Effect of Experimental Treatments on Retirement Literacy

	Retirement Literacy Δ I(perceived required savings ≥ recommendation)	
Experimental Treatment	Low Arm	Moderate Arm
Generic Recommendation	--	0.09*** (0.03)
Specific Recommendation	0.24*** (0.03)	0.17*** (0.04)
Match Clarification	0.20*** (0.03)	--
Small Reward	0.19*** (0.03)	--
Base Rate Prior to Interventions	0.50	0.40
N	704	228

Note: This table summarizes changes in employee retirement literacy before and after experimental treatments as estimated through a series of linear probability models (with suppressed constants). The first column estimates the change in the share of employees who perceive a required rate of annual savings at or above the recommended rate for the Low Arm's primary experimental treatment. The next column presents estimates corresponding to the Moderate Arm primary treatments. The table also reports base values of beliefs prior to the interventions in the second to last row. The varying sample sizes across measures reflect the random assignment of respondents to select survey modules. Robust standard errors are displayed parenthetically (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$).

Appendix Table A6.
Discrepant Employee Reports of 401(k) Plan Engagement Adjusted for Inattention and Exaggeration

	Discrepant Report Type	
	Participation	Full Match
Discrepant Reporting Share of Non-Participants	0.37	0.26
Panel A. Inattention Adjustment		
Discrepant Report Share | Passing Attention Check	0.34	0.24
Panel B. Exaggeration Adjustment		
Discrepant Report Share, Adjusted for Exaggeration by:		
Over-Reported Salary	0.33	0.24
Reported highest salary option ($75k or above)	0.30	0.20
Reported highest contribution option (10% or more)	0.36	0.25
Reported highest accumulated savings option ($75k or more)	0.32	0.23
Reported highest education option (Graduate school)	0.33	0.23
Reported highest confidence in retirement preparation	0.35	0.25
Any of the above	0.21	0.14
Panel C. Inattention and Exaggeration Adjustment		
Discrepant Report Share Adjusted for Exaggeration | Attention Check	0.20	0.13

Note: This table assesses the potential role of inattention and exaggeration in explaining discrepant employee reports of 401(k) plan participation and full match take-up. Panel A adjusts for inattention by reporting the rate of discrepancies for highly attentive employees as indicated by whether an employee passed an "attention check" within the survey (see Section 4 for details). Panel B adjusts for deliberate exaggeration by reporting the discrepancy rate after excluding employees whose response elsewhere in the survey indicated potential exaggeration. Specifically, the panel reports discrepancy rates after excluding employees (i) whose self-reported salary range was inconsistent with administrative records, (ii) whose response reflected the most socially desirable item on a response menu for each of the five questions for which one could reasonably identify the most socially desirable response (i.e., questions regarding salary, 401(k) contribution, accumulated savings, educational attainment, and confidence in retirement preparation), or (iii) who satisfied any of the six aforementioned exclusion screens. Panel C reports the residual discrepancy rates after adjusting for both inattention and exaggeration—i.e., discrepancies due to potential employee confusion—by reassigning employees satisfying any of the exaggeration screens and conditioning on passing the attention check.

Appendix Table A7.
Sample Comparison across Field Experiment and National Corroboration

Demographic Characteristics	Field Experiment Respondents	National Invited	National Verified
Age [Yrs]	39.5	37.6	36.0
Male [1,0]	0.33	0.48	0.41
Tenure			
Less than 4 years	0.30	0.34	0.39
4 or more years	0.70	0.66	0.61
Income (Estimated) [$ thousands]	52.4	53.9	60.3

Note: This table compares characteristics of invited and verified individuals from the national corrboration sample with respondents in the field experiment. Tenure and income were elicited by category in the national verification sample and income estimates reflect an average of categorical midpoints with the exception of "less than $25k" which was assigned as $20k.

Appendix Table A8.
Maximum Rationalized Savings Delay Under Beta-Delta Calibration w/ Liquidity Wedge

		Probability of Reviewing Savings Decision		
β	*k* ($)	Daily Review	Twice-weekly Review	Weekly Review
0.9	$50	2	5	9
0.9	$200	7	19	37
0.7	$50	7	18	36
0.7	$200	29	71	143

Notes: This table reports the maximum delay, in business days, before increasing retirement contributions becomes optimal under the β-δ procrastination model described in Appendix 1. Each cell reports the maximum rationalized delay implied by the calibration formula for T* for the indicated parameter values. Rows vary the present-bias parameter β and the one-time enrollment cost k, while columns vary the probability p that an employee reviews the savings decision on a given business day. The benchmark model assumes daily review (p=1); the twice-weekly and weekly review columns correspond to p=0.40 and p=0.20, respectively. The calibration assumes a daily benefit from saving of b=$6, discount factor δ=0.9997, and liquidity wedge λ=0.50. Values represent upper bounds on the delays that can be rationalized under the indicated parameter values.