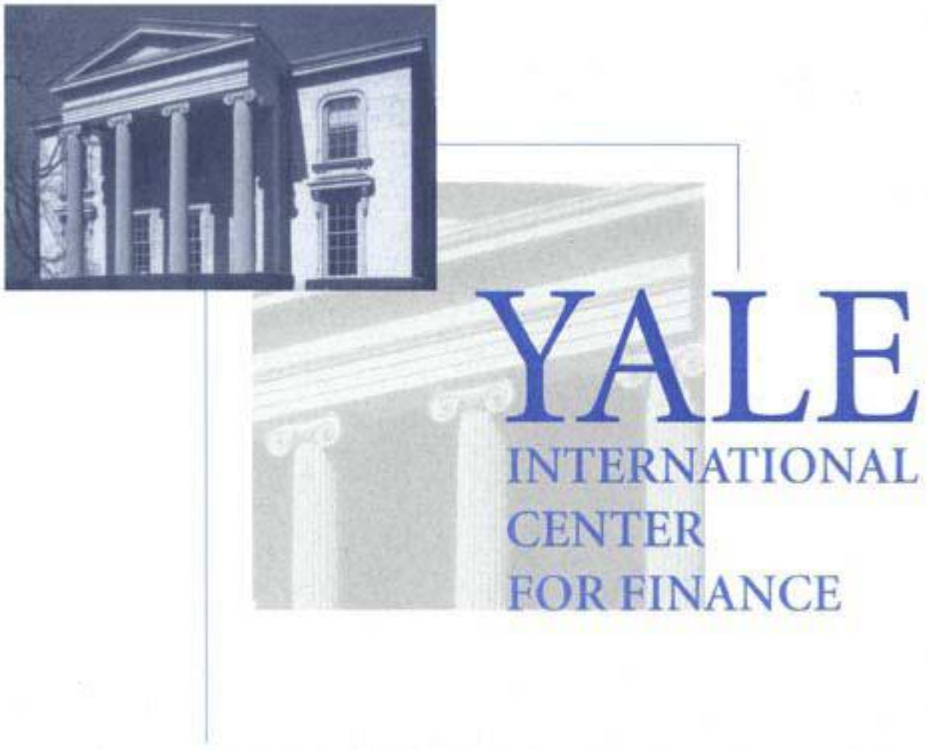


Yale ICF Working Paper No. 12-24

Small Cues Change Savings Choices

James J. Choi
Yale University and NBER

Emily Haisley
Barclays Bank PLC

Jennifer Kurkoski
Google Inc.

Cade Massey
Yale University

January 27, 2012

Small Cues Change Savings Choices

James J. Choi
Yale University and NBER

Emily Haisley
Barclays Bank PLC

Jennifer Kurkoski
Google Inc.

Cade Massey
Yale University

January 27, 2012

Abstract: In randomized field experiments, we embedded one- to two-sentence anchoring, goal-setting, or savings threshold cues in emails to employees about their 401(k) savings plan. We find that anchors increase or decrease 401(k) contribution rates by up to 1.4% of income. A high savings goal example raises contribution rates by up to 2.2% of income. Highlighting a higher savings threshold in the match incentive structure raises contributions by up to 1.5% of income relative to highlighting the lower threshold. Highlighting the maximum possible contribution rate raises contribution rates by up to 2.9% of income among low savers.

We thank David Hirshleifer, Eric Johnson, Alessandro Previtero, Victor Stango, and audiences at HBS, the Miami Behavioral Finance Conference, the NBER Household Finance Meeting, the Queen's University Behavioral Finance Conference, and Yale for helpful comments and Google for financial support. We are grateful for Minhua Wan's comments and assistance with database management. Part of the work on this paper was done while Emily Haisley was a post-doctoral associate at Yale and Jennifer Kurkoski was a doctoral student at UC Berkeley.

In this paper, we analyze field experiments where we randomly assigned employees at a large technology company to receive one of several versions of an email. Control emails reminded recipients of the employer matching contributions in their 401(k) retirement savings plan and how much they themselves had contributed so far in the calendar year. Treatment emails were identical to the control emails, except that they also included one of nine different one- to two-sentence cues. The cues were designed to trigger psychological phenomena previously documented in the psychology and behavioral economics literature. Specifically, we sought to use anchoring, goal setting, and savings-threshold salience to influence the recipient's subsequent savings choices. We find that these small cues have large effects. High numerical cues raise savings rates, and low numerical cues depress savings rates.

Psychologists have long known that the presentation of arbitrary numerical cues, or "anchors," can shift subjects' judgments, willingness to pay for goods, and hypothetical credit card payment decisions towards those anchors in laboratory experiments (Tversky and Kahneman, 1974; Johnson and Schkade, 1989; Green et al., 1998; Kahneman and Knetsch, 1993; Ariely, Loewenstein, and Prelec, 2003; Stewart, 2009).[1] However, evidence is only beginning to emerge on the importance of anchoring for economic decisions outside the laboratory (Baker, Pan, and Wurgler, forthcoming; Beggs and Graddy, 2009; Dougal et al., 2010). Our anchoring cues are one-sentence examples of 401(k) contribution rate increases that are explicitly described as containing no informational content. Anchoring should cause employees who receive the 10% or 20% contribution rate increase examples to contribute more than employees who receive the 1% or 3% contribution rate increase examples.

The next cues we test are two-sentence savings goal examples. Locke and Latham (1990, 2002, 2006) summarize a large literature showing that setting concrete goals that are difficult to achieve enhances performance relative to setting unambitious or vague "do your best" goals. A number of laboratory studies have found that behavior changes even when the goals are subconsciously primed by environmental cues rather than consciously chosen (Chartrand and Bargh, 1996; Bargh et al., 2001; Stajkovic, Locke, and Blair, 2006). Our goal treatments lie between the interventions summarized by Locke and Latham, which explicitly impose goals on

[1] Anchoring has traditionally been understood to arise from people beginning their thought process at the arbitrary anchor value and incompletely adjusting away from that starting point (Tversky and Kahneman, 1974; Epley and Gilovich, 2001). Other researchers have argued that anchoring occurs because information that is consistent with the anchor becomes more cognitively accessible (Mussweiler and Strack, 1999, 2001; Chapman and Johnson, 2002; Strack and Mussweiler, 1997).

subjects, and the interventions that subconsciously prime goals. Although email recipients can fully perceive the goal being cued, the cues do not overtly urge them to adopt a goal. A $7,000 or an $11,000 savings goal is presented as a mere example used to illustrate the matching contribution structure. The goals literature predicts that the $11,000 savings goal example will result in higher savings rates than the $7,000 savings goal example.

Our last set of cues highlights certain savings thresholds within the 401(k). Choi et al. (2002) and Benartzi and Thaler (2007) argue that many people simplify the problem of choosing a 401(k) contribution rate by using rules of thumb tied to salient savings thresholds created by the plan's structure, such as "contribute the maximum possible amount," or "contribute the minimum necessary to earn the maximum possible employer matching contributions." Our threshold cues make salient either the maximum possible contribution rate (60% of income), the annual contribution amount necessary to earn the maximum employer match ($16,500), or the annual contribution amount after which the highest marginal matching incentives stop ($3,000). Making a certain threshold salient may make an employee more likely to use it as guidance in choosing her contribution rate; a high salient threshold would increase contributions more than a low salient threshold.

We find that in the short run, low anchors decrease contribution rates relative to a control email that contains no intentional anchoring cues, but high anchors have no effect. The effect of a 1% anchor (the lowest anchor we test) initially becomes more negative over time, achieving its largest magnitude eleven weeks after the email, when it depresses contribution rates by 1.4% of income. This effect disappears seventeen weeks after the email, the pay period when many employees adjust their contribution rates in anticipation of their annual bonus payment. A 3% anchor lowers contribution rates by 0.6% of income immediately after the email, but the statistical significance of the 3% anchor effect lasts for only one two-week pay period before dissipating (although the point estimate of the treatment effect remains –0.6% for a total of six weeks). Neither a 10% nor a 20% anchor significantly changes average contribution rates for the first five months after the email. In the longer run—up to a year after the email—both low and high anchors affect contribution rates. The 1% anchor decreases contribution rates by up to 1.2% of income, the 3% anchor increases contribution rates by up to 1.2% of income, and the 10% and 20% anchors increase contribution rates by up to 1.4% of income.

We also find that the $11,000 savings goal example raises contribution rates much more

than the $7,000 savings goal example. The $11,000 goal's impact is at its apex ten weeks after the emails were sent, raising contribution rates by 2.2% of income relative to the control email. The effect slowly attenuates afterwards, but remains a statistically significant 1.1% of income even sixteen weeks after the email date. The $7,000 goal example essentially has no impact on contribution rates relative to the control email. The fact that the high goal raises savings rates in the short run whereas the high anchors do not suggests that the high-goal effect is not merely a manifestation of an anchoring effect.

Lastly, we find that highlighting high savings thresholds raises contributions relative to highlighting low savings thresholds. Making the $16,500 savings threshold salient initially results in average contribution rates similar to making the $3,000 savings threshold salient. But eighteen weeks after the email, recipients of the $3,000 threshold treatment are contributing 1.5% of income less than recipients of the $16,500 threshold treatment. It appears that recipients of the lower threshold treatment were satisfied with achieving a lower savings level and thus contributed less afterwards. Mentioning that 60% is the maximum possible contribution rate generates an even larger effect, but only for those who were contributing little prior to the email. Among those on pace before the email to contribute no more than $2,500 in 2009, receiving the 60% threshold treatment increases their contribution rate by 2.9% of income more than the control group five weeks after the email. Those on pace to contribute more than $2,500 are unaffected by the 60% threshold treatment on average. Further analysis suggests that low contribution *rates* (as a percent of income) at the time of the email, rather than low contribution dollar amounts, predicts high responsiveness to the 60% threshold treatment. Again, the pattern of these threshold effects differs from that of the anchoring effects, suggesting that some independent mechanism lies behind them.

Because many email recipients likely ignored our emails or did not read them carefully enough to notice the cues, our estimated effect sizes are closer to zero than the true effects of seeing the cues. Nevertheless, our estimates are large compared to those estimated for a conventional economic lever, employer matching contributions to a 401(k). Kusko, Poterba, and Wilcox (1998) find, at one manufacturing firm, that increasing the match rate from 25% to 150% on the first 6% of income contributed raised average 401(k) contribution rates by only 0.2% to 0.3% of income. A decrease in the match rate from 139% to 0% was accompanied by an average contribution rate fall of only 0.3% of income. Another company studied by Choi et al. (2002)

went from matching 50% of the first 5% of income contributed to matching 50% of the first 7% of income contributed for union employees, and from matching 50% of the first 6% of income contributed to matching 50% of the first 8% of income contributed for management employees. The increase in the average contribution rate from three months prior to the change to six months after the change is 0.4% of income.[2]

Due to the constraints of our field setting, we cannot establish beyond all doubt that the psychological mechanisms that motivated our cues are in fact responsible for our treatment effects. Our cues are therefore akin to automatic enrollment in retirement savings plans (Madrian and Shea, 2001; Choi et al., 2002, 2004; Beshears et al., 2008), which has large effects on savings outcomes but whose exact psychological mechanisms (candidates include procrastination, status quo bias, simplicity seeking, endorsement effects, and anchoring) are not yet precisely identified. In particular, we cannot completely exclude the possibility that our cues were effective because employees interpreted the cues as containing relevant information about their optimal savings choices.[3] If inference from cues drives our treatment effects, then our results still demonstrate a departure from the usual assumption in economics that people make highly informed financial decisions, since employees must have extremely diffuse prior beliefs about how much they should be saving in their 401(k) to be swayed by the subtle cues in our emails. Our paper's central message is that, irrespective of the exact psychological channels through which they operate, small cues of the types we have tested have large effects on savings choices. Organizations and policymakers should be cognizant of these facts when designing their communications.

Our findings are related to other research that has found that individuals' savings outcomes are strongly influenced by small changes in their decision-making environment. Many people are financially passive, so changes in the default 401(k) contribution rate and asset

[2] This result is not reported in Choi et al. (2002), but is reported here for the first time using that paper's data. The sample over which this average is calculated is restricted to those who had a positive contribution rate nine months prior to the match threshold change. Choi et al. (2002) show that the match threshold change had no effect on the probability of having a positive contribution rate.

[3] The most straightforward way to rule out the information channel would have been to show employees the number in the email being produced by a random number generator such as the wheel of fortune in Tversky and Kahneman (1974), making the uninformative nature of the cue irrefutable. However, such a demonstration would be extremely unnatural within the context of a corporate communication, creating a high risk of Hawthorne effects. In fact, even many laboratory anchoring experiments do not show the anchors being randomly generated to the subjects (e.g., Chapman and Johnson, 1994, 1999; Epley and Gilovich, 2001; Green et al., 1998; Jacowitz and Kahneman, 1995; Mussweiler and Strack, 2001; Stewart, 2009; Strack and Mussweiler, 1997; Wegener et al., 2001).

allocation change the contribution rates and asset allocations they end up with (Madrian and Shea, 2001; Choi et al., 2002, 2004; Beshears et al., 2008). Automatic contribution rate escalation causes many people's contribution rates to increase in lockstep with the automatic escalation schedule (Thaler and Benartzi, 2004; Benartzi, Peleg, and Thaler, forthcoming). Forcing people to stop being passive and actively make a savings choice before a deadline raises contribution rates relative to a regime where the default contribution rate is zero and there is no active decision deadline (Carroll et al., 2009). Simplifying the menu of 401(k) options decreases the stickiness of the default by reducing the cognitive cost of opting out (Huberman, Iyengar, and Jiang, 2004; Choi, Laibson, and Madrian, 2009b; Beshears et al., 2010a). More investment menu complexity causes employees to allocate more of their portfolio to money market and bond funds (Iyengar and Kamenica, 2010). Making the employer match's asset allocation less salient results in 401(k) participants ignoring it when choosing an asset allocation for their other 401(k) balances (Choi, Laibson, and Madrian, 2009a).

Our paper is also related to other work in economics that identifies the effect of anchoring on decisions outside the laboratory. Baker, Pan, and Wurgler (forthcoming) show that the offer price for merger targets is biased towards the target stock's trailing 52-week high, a highly salient but normatively irrelevant number. Beggs and Graddy (2009) conclude that sales prices of auctioned paintings are anchored on the painting's previous sale price because the portion of the painting's previous sale price that was due to aggregate art market conditions at the time influences the current sale price. Using a similar econometric methodology, Dougal et al. (2010) find that a firm's borrowing cost is anchored on the nominal value of its historical borrowing costs.

The remainder of our paper proceeds as follows. Section I discusses the relevant features of the company 401(k) plan. Section II describes the experimental design, and Section III describes our data. Section IV presents our experimental results. Section V concludes.

I. 401(k) plan features

Employees at the company we study can make before-tax, after-tax, or Roth contributions to their 401(k) plan. Before March 2011, employees specified three percentages: the percent of their paycheck they wanted to contribute on a before-tax, after-tax, and Roth basis. Starting in March 2011, employees had the option of specifying a dollar amount to contribute from each

paycheck to each contribution category. The sum of the contributions could not exceed 60% of income during any two-week pay period in 2009 and 2010. In 2011, employees could contribute 100% of their paycheck to the 401(k). Throughout our sample period, total before-tax plus Roth contributions during a calendar year were capped at $16,500 for employees under the age of 50, and at $22,000 for employees age 50 and over. Total 401(k) contributions including after-tax and employer matching contributions were limited to $49,000 in a calendar year.

Starting in 2007, new hires and employees who had never enrolled in the 401(k) were automatically enrolled at a 3% before-tax contribution rate unless they opted out. At the beginning of each subsequent calendar year until 2010, employees who had never actively chosen their 401(k) elections had their before-tax contribution rate automatically increased by 1 percentage point, and the default before-tax contribution rate for new hires also increased by 1 percentage point. Hence, at the time of our first emails in Fall 2009, completely passive employees were contributing 5% of their income to the plan on a before-tax basis. Employees affected by auto-escalation in 2010 started contributing 6% of their income on January 8, 2010, and all employees hired in 2010 who did not opt out were automatically enrolled at a 6% before-tax contribution rate. In 2011, the default contribution rate for new hires did not change, and seasoned employees were not subject to automatic contribution rate increases.

The company makes contributions to the 401(k) that depend upon each employee's own cumulative contributions during the calendar year. The match amount during 2009 was the greater of (1) 100% of before-tax plus Roth contributions up to $2,500, or (2) 50% of before-tax plus Roth contributions up to $16,500, resulting in a maximum possible match of $8,250. This match structure generates a 100% marginal subsidy on contributions up to $2,500, a 0% marginal subsidy on contributions between $2,501 and $5,000, and a 50% marginal subsidy on contributions between $5,001 and $16,500. In 2010, the match structure changed to be the greater of (1) 100% of before-tax plus Roth contributions up to $3,000, or (2) 50% of before-tax plus Roth contributions up to $16,500. This new match structure shifts the 0% marginal match zone to contributions between $3,001 and $6,000. Matching contributions vest immediately.

Employees receive an annual bonus that is paid each March. In 2009 and 2010, if an employee had a 5% contribution rate in effect during the pay period in which the bonus is paid, 5% of the bonus would be contributed to the 401(k) plan. As a result, many employees changed their contribution rate shortly before the bonus pay period in 2009 and 2010. Starting in 2011,

employees could choose a separate contribution election for their bonus, and this election could specify dollar amounts to be contributed rather than percentages of the bonus. Unless actively changed by the employee, the bonus contribution election was by default set equal to the election for regular paychecks. Bonuses were paid on March 6, 2009, March 5, 2010, and March 11, 2011.

II. Experimental design

On November 17, 2009, we sent emails to employees who would contribute less than $16,500 on a before-tax plus Roth basis in 2009 if they left their contribution rate elections as of November 4, 2009 unchanged. We sent a second round of emails almost a year later to employees who were on pace to contribute less than $16,500 on a before-tax plus Roth basis in 2010 if they left their contribution elections as of October 15, 2010 unchanged.[4] Most of this second round was sent on October 19, 2010, but a randomized subset of employees received their email on October 28, 2010 instead.

We randomized which email version each employee received. Figure 1 shows the template used for the 2009 emails. All 2009 emails described the matching contributions the company offered and the amount the recipient had contributed so far in 2009. Following this information was the statement, "To take greater advantage of [Company]'s 2009 match, increase your contribution rate for the remaining six weeks of 2009." The emails concluded with information on how to change one's contribution rate on the Vanguard website and was signed by the company's benefits director. The 2010 email template was identical, except that the match information was updated to reflect the new match structure, the year-to-date contribution information reflected 2010 contributions, and the statement about increasing one's contribution rate was replaced by, "To take greater advantage of [Company]'s 2010 match, increase your contribution rate soon before the year is over."

Within each year, the only difference between the control and treatment emails was that the treatment emails included one or two additional sentences between the statement about taking greater advantage of the match and the information about how to change one's

[4] We excluded from the first email campaign employees who had been hired in 2009, since they may have made contributions to a previous employer's 401(k) in 2009 (which are unobserved by us) and thus not be eligible to contribute $16,500 on a before-tax plus Roth basis to their current company's 401(k) in 2009. For the same reason, we excluded employees who had been hired in 2010 from the second email campaign.

contribution rate (the location indicated by the "Treatment text was inserted here" block in Figure 1). Table 1 summarizes the additional sentences in each treatment email. We will discuss the treatments and randomization scheme in further detail in the below subsections.

By comparing the treatment email responses to the contemporaneous control email responses, we difference out the effect of simply receiving an email with 401(k) match and year-to-date contribution information, thus identifying the effect of each cue alone. Without the control email, we can only identify the *difference* between cue treatment effects, not the absolute size of each effect. We can also cleanly identify the effect of simply receiving an email with 401(k) match and year-to-date contribution information because in 2010, we assigned some employees to receive the control email nine days later than everybody else. Comparing the choices of 2010 control email recipients to those of delayed control email recipients during that nine-day window gives us the short-run effect of receiving an email with 401(k) match and year-to-date contribution information.[5]

A. Anchoring treatments

Employees in the 1% anchor treatment received the additional sentences, "For example, you could increase your contribution rate by 1% of your income and get more of the match money for which you're eligible. (1% is just an example, and shouldn't be interpreted as advice on what the right contribution increase is for you.)" Employees in the 3%, 10%, or 20% anchor treatments were shown analogous text, but 3%, 10%, or 20% replaced the two instances of 1%.

The anchor treatments explicitly denied that the treatment text contained any information about the recipient's optimal 401(k) contribution rate. This disavowal was to make this treatment as close as possible to an arbitrary numerical anchor within a framework appropriate to a corporate communication. Although the other treatments could in principle derive all of their power from anchoring effects, the results described in Section IV seem inconsistent with this mechanism, since the non-anchoring treatments are able to *increase* contribution rates in the short run whereas the anchoring treatments are only able to *decrease* contribution rates in the short run.

[5] Assigning some employees to *never* receive an email was not considered an option because it might financially disadvantage those employees.

B. Savings-goal treatments

The savings-goal treatments were administered only in 2010. The $7,000 savings goal treatment consisted of two additional sentences added to the control email: "For example, suppose you set a goal to contribute $7,000 for the year and you attained it. You would earn $500 more in matching money this year than you're currently on pace for." The $11,000 savings goal treatment instead contained the sentences, "For example, suppose you set a goal to contribute $11,000 for the year and you attained it. You would earn $2,500 more in matching money this year than you're currently on pace for." The assignment scheme we will describe in Section II.D ensured that everybody in a goal treatment would receive the same additional match ($500 or $2,500) for attaining a $7,000 or $11,000 savings level in the 401(k).

C. Threshold-salience treatments

The three threshold-salience treatments emphasized a savings level or choice that was higher than the employee's status quo. The $3,000 and $16,500 threshold treatments were administered only in 2010, and the 60% threshold treatment only in 2009.

The $3,000 threshold treatment email included the sentence, "The next $*x* of contributions you make between now and December 31 will be matched at a 100% rate," where *x* was the difference between $3,000 and the employee's year-to-date before-tax plus Roth contributions. The $16,500 threshold treatment email instead included the sentence, "Contributing $*y* more between now and December 31 would earn you the maximum possible match," where *y* was the difference between $16,500 and the employee's year-to-date before-tax plus Roth contributions. Employees in the 60% threshold treatment had only one additional sentence added to their email relative to the control group: "You can contribute up to 60% of your income in any pay period."

D. Randomization scheme

Table 2 summarizes how the 5,622 emails sent in 2009 and the 4,555 emails sent in 2010 were allocated across experimental cells.[6] Assignments to cells in 2010 were independent of

[6] A small number of employees who were assigned to a treatment are not in our analysis (and also excluded from Table 2) because they had left the company before the first payday after the emails were sent, they had temporary Social Security numbers at the time that made matching their 401(k) transactions to subsequent Vanguard records indexed by permanent Social Security numbers difficult, or because their employment termination date was

assignments in 2009.

Employees naturally fell into three categories based on their marginal incentives to increase their contributions in the calendar year above what they would contribute if they left their contribution rate elections unchanged. The first category contains those who faced a 100% marginal match on additional contributions: employees on pace to make less than $2,500 in before-tax plus Roth contributions in 2009 and employees on pace to make less than $3,000 in before-tax plus Roth contributions in 2010. The second category contains those who faced a 0% marginal match: employees on pace to make between $2,500 and $4,999 in before-tax plus Roth contributions in 2009 and employees on pace to make between $3,000 and $5,999 in before-tax plus Roth contributions in 2010. The third category contains those who faced a 50% marginal match: employees on pace to make between $5,000 and $16,499 in before-tax plus Roth contributions in 2009 and employees on pace to make between $6,000 and $16,499 in before-tax plus Roth contributions in 2010.

Eligibility for assignment to experimental conditions depended on which category the employee was in. In 2009, employees on pace to contribute at least $5,000 could be assigned to the control, the 1% anchor treatment, the 10% anchor treatment, or the 60% threshold treatment. If an employee was on pace to contribute enough that increasing her before-tax plus Roth contribution rate by 10% of income for the remainder of 2009 would cause her 2009 before-tax plus Roth contributions to exceed $16,500, then she was not eligible to be assigned to the 10% anchor treatment. We made this exclusion because the sentence preceding the anchoring example referred to increasing one's contribution rate for the remainder of 2009 to take greater advantage of the match. The 10% anchor text could thus be interpreted as implying that it was possible to increase one's before-tax or Roth contribution rate by 10% for the remainder of 2009, and that all of those incremental dollars would be matched. Such an inference would be false for those with high enough year-to-date contributions and current contribution elections. Employees younger than 50 were not allowed to contribute more than $16,500 in 2009 on a before-tax plus Roth basis. Although older employees could exceed this cap, contributions above it would not be matched. For similar reasons, if increasing the before-tax plus Roth contribution rate by 1% of income for the remainder of 2009 would cause the employee's 2009 before-tax plus Roth

ambiguous in the data. These exclusions cause minor imbalances in the number of employees in each cell beyond the imbalances created (as explained later in the main text) by our randomization scheme.

contributions to exceed the $16,500 limit, she was ineligible to be assigned to both the 1% and the 10% anchors and could only receive either the control or the 60% threshold email.

Employees had an equal probability of being assigned to each of the treatments for which they were eligible. The eligibility criteria cause the control and 60% threshold groups to contain the most people within this projected contribution category, the 1% anchor group to contain fewer people, and the 10% anchor group to contain the fewest people. Because eligibility for the anchoring treatments depends on income and year-to-date contributions, we will control for treatment eligibility when estimating the anchoring treatment effects. Any variation remaining in treatment assignment after controlling for treatment eligibility is due to randomization, so we are able to make clean inferences about treatment effects without discarding people who were ineligible for one or more treatments.

The anchoring statements' implication that increasing one's contribution rate by the anchor amount would increase the match earned was not necessarily true for employees whose marginal match on the next dollar of contribution increase was zero. And the implication could be somewhat misleading for employees whose marginal match on the next dollar of contribution increase was 100%, because much of the increase beyond the next dollar could be in the region where the marginal match was 0%. This is why we did not administer the anchoring treatments to any employee on pace to contribute less than $5,000 in 2009. These employees had an equal chance of receiving only the control email or the 60% threshold email.

In 2010, employees on pace to make at least $6,000 in before-tax plus Roth contributions could be assigned to the control, the delayed control, the 3% anchor, the 10% anchor, or the 20% anchor. The sentence in the 2010 emails about taking greater advantage of the match did not specifically mention elevating one's contribution rate for the *entire* remainder of the year. Therefore, we used narrower criteria for excluding employees from anchoring treatments. Employees who would exceed the 2010 match cap by increasing their contribution rate by 20% for just one pay period were ineligible for the 20% anchor and delayed control. Those who would exceed the cap by increasing their contribution rate by 10% for just one pay period were also ineligible for the 10% anchor. Those who would exceed the cap by increasing their contribution rate by 3% for just one pay period were only eligible to be assigned to the control. This last group received the control email but is excluded from all our tables and analyses. Employees were equally likely to be assigned to every condition for which they were eligible.

Employees on pace to contribute between $3,000 and $5,999 on a before-tax plus Roth basis in 2010 had an equal chance of receiving the control email, the delayed control email, the $7,000 savings goal example, or the $11,000 savings goal example. Because the marginal match on before-tax plus Roth contributions between $3,001 and $6,000 was 0%, each of these employees would earn exactly $500 or $2,500 more in matching money by raising their total 2010 before-tax plus Roth contributions to $7,000 or $11,000, respectively.

Employees on pace to contribute less than $3,000 on a before-tax plus Roth basis in 2010 were equally likely to receive the $3,000 threshold treatment or the $16,500 threshold treatment. Because there were not many employees on pace to contribute less than $3,000 in 2010, we did not assign anybody in this projected contribution category to the control group. Therefore, our analysis of these treatments will just compare the $3,000 threshold treatment effect to the $16,500 threshold treatment effect.

III. Data description

We use salary and employment termination date data from personnel records and 401(k) data provided to the company by Vanguard. Vanguard data included cross-sectional snapshots of all 401(k) contribution rate elections (before-tax, after-tax, and Roth) in effect among email recipients on January 3, 2008, November 4, 2009, October 15, 2010, and every month-end from January 2010 to August 2011. We also have a record of every 401(k) contribution rate change from January 3, 2008 to August 31, 2011 and the total dollars each employee contributed to the before-tax, after-tax, and Roth accounts from January 1, 2009 to November 4, 2009 and from January 1, 2010 to October 15, 2010. Individuals in the data were assigned random identifiers; no personally identifying information was included.

We use the contribution rate data to construct a panel of 401(k) contribution rates in effect at the end of each two-week pay period.[7] Contribution rate changes submitted fewer than ten days before the next payday do not take effect until the second payday after the change, so our data allow us to identify all the contribution rates in effect up to the September 2, 2011

[7] If multiple contribution rate change transactions are recorded with the same effective date, we assign the latest contribution rate chosen before a payday to be the one that was effective on that payday. Up to February 19, 2010, we have both the date and time each change transaction was entered. After February 19, 2010, we only have the date each change transaction was entered. Therefore, if somebody entered multiple contribution rate changes on the same day, we cannot directly identify which rate was the last one entered. We can usually infer what the last rate was from the month-end contribution rate snapshots. In the rare cases where we cannot, we use the average of the contribution rates entered on that day.

payday. Because the changes file does not record the contribution rate elections employees had in effect when they first joined the 401(k), we cannot construct a complete contribution rate history between January 3, 2008 and November 3, 2009 for email recipients who were hired after January 3, 2008.

IV. Experimental results

A. Effect of getting a control email versus getting no email

Does getting an email about the 401(k) without additional cues affect savings choices? In this subsection, we assess the impact of getting a control email versus getting no email at all. Later, we will analyze how savings impact varied across email versions.

Figure 2 shows the average total contribution rate (before-tax plus after-tax plus Roth)[8] as a fraction of income at each payday for the 2009 control group. In order to avoid spurious movements in the series before November 2009 due to post-January 3, 2008 hires appearing for the first time in the data when they make their first contribution rate change, the sample in the black line is restricted to control email recipients who were already employed by the company as of January 3, 2008. The gray line includes all control email recipients from November 13, 2009 (the first payday for which we have everybody's contribution rate) onwards and is very close to the black line. Employees who leave the company after November 4, 2009 are not included in the averages after their departure date.

The impact of the company's 1% contribution auto-escalation is visible at the beginning of 2009, but it begins to be reversed immediately. By the beginning of March 2009, when the annual bonus was paid, the average total contribution rate is similar to what it was immediately prior to the auto-escalation. This strong reversal is surprising in light of the success the auto-escalation program studied by Thaler and Benartzi (2004) had at raising long-run 401(k) contribution rates. The lack of inertia at this company may be due to the bonus payment serving as a focal deadline for action. However, the magnitude of the reversal must be interpreted with caution, since only employees who were on pace to contribute less than $16,500 on a before-tax plus Roth basis in 2009 as of November 4, 2009 were sent emails (and hence included in the graph's sample). This means that some employees who maintained or increased their

[8] We use the total contribution rate because it more closely maps to total asset accumulation, which is most relevant for welfare.

contribution rates after the 2009 auto-escalation—and thus were on pace to hit the $16,500 maximum—have been excluded from the graph.

The impact of our 2009 control email appears to be large. The average total contribution rate on November 27, 2009—the first payday following the email—of control recipients employed since January 2008 is 10.7%, which is 2.3% of income higher than it was two weeks earlier. In fact, due to the ten-day lag between when a contribution rate change request is entered and when it becomes effective, the November 27 contribution rate only reflects changes that were made in response to the November 17 email on the *same* day the email was sent. Even contribution rates entered on November 18 would not be reflected until December 11. Indeed, the average total contribution rate increases further to 11.8% on December 11, 3.4 percentage points higher than it was on November 13. The average then falls slightly to 11.5% on December 24.

By comparison, during the last three pay periods of the prior year, 2008, the sample's average total contribution rate *falls* by 0.5% of income. Alternatively, if we use as the counterfactual rate of increase the 0.1% per-pay-period average contribution rate increase in the eight months prior to the experiment (March 6, 2009 to November 13, 2009), then the average contribution rate would increase by only 0.3% of income over the last three pay periods of 2009 in the absence of the control email.

The 2010 auto-escalation raises the average contribution rate to 12.1% on January 8, 2010, but the average falls quickly afterwards. On the March 5, 2010 bonus payday, the average contribution rate is 9.8%, and it falls precipitously to 8.5% on the next payday. Much of this post-bonus fall is due to some employees hitting the annual before-tax plus Roth dollar contribution limit on March 5, which forces their before-tax and Roth contribution rates to be zero for the remainder of the year.[9] The average contribution rate then falls slowly afterwards through October 2010, when our second round of emails was sent.

In October 2010, we can use a contemporaneous randomized comparison to estimate the impact of the control email. Recall that some employees on pace to contribute at least $3,000 in 2010 were randomly assigned to receive the control email nine days after everybody else. Figure 3 plots the average total contribution rate each pay period minus the total contribution rate in

[9] These employees' contribution rates are automatically restored to their previous positive level at the beginning of the next year. A portion of the average contribution rate increase at the beginning of each year is due to such employees.

effect on October 15, 2010—the last payday before the first 2010 emails were sent—for the delayed control group and the subset of the control group that was eligible to be assigned to the delayed control group. Starting in March 2011, employees could choose a specific dollar amount to contribute from each paycheck rather than being restricted to specifying a percentage of their salary to contribute, and they could specify separate contribution elections for their bonus. For all our analyses of the 2010 emails, when a dollar amount was chosen for a regular payroll contribution, we translate this choice into a percentage contribution rate using the employee's 2010 salary. Figure 3 does not show the total contribution rate in effect for the employee's 2011 bonus—as will be the case for all subsequent figures depicting the effects of the 2010 emails.

The average total contribution rate of the control group (the thin black line) on October 29 is 1.5% of income higher than it was on October 15, whereas the delayed control group's average total contribution rate (the thick black line) rises by only 0.1% of income during the same period. The difference is significant at the 1% level (t = 4.94). Figure 3 subsequently shows, however, that the delayed control group makes up for lost time, contributing more than the control group on November 26 and December 10. Both groups end up contributing about the same total amount out of non-bonus paychecks. Using as the dependent variable each employee's total contribution rate in excess of his October 15, 2010 total contribution rate averaged across non-bonus paydays,[10] the contribution rate in effect from October 29 to December 23 is 2.4% of income higher than the October 15 contribution rate for the control group and 2.2% of income higher for the delayed control group; the difference is not significant, with a p-value of 0.523. (All p-values reported in this paper are two-sided.) Over the longer horizon of October 29 to September 2 of the following year, the delayed control group has a 0.06% of income (p = 0.843) higher average contribution rate than the control group.

The delay also has no effect on the contribution rate chosen for the bonus. Because we do not have information on each employee's bonus size, if an employee chose to contribute a certain dollar amount out of his bonus (rather than a percentage), we cannot translate that choice into a percentage election. We therefore do not include employees who chose a dollar amount for their bonus contribution in any of our analyses of the 2011 bonus. Only 4.5% of employees across all the 2010 mailings chose a dollar amount for their bonus contribution, so the sample loss is small.

[10] In this and subsequent regressions where we average across paydays, if an employee leaves the company before the end of the period over which we are averaging, the employee's average contribution rate during the periods she was present at the company is used as the dependent variable.

We find that the delay reduced the contribution out of the bonus by only 0.02% of the bonus amount ($p = 0.974$).

Both campaigns generated positive comments from recipients. Many expressed generic thanks, such as: "Kudos and thanks to the benefits team for this very helpful and timely reminder." A few specifically mentioned the usefulness of the information included in the email, suggesting that a sizable fraction of employees have only a noisy sense of how much they are saving and the company match structure. For example, one employee noted, "I had no idea I was putting so little away!" Another said, "I had thought my % was high enough to take advantage of all of [Company]'s 401k matching but hadn't had time to double check. This e-mail is one of the examples of why I like working at [Company]."

Collectively, these results indicate that reminding employees about their 401(k) match, informing them of their year-to-date contribution amount, and making salient the year-end date had a large effect on contribution behavior.[11] However, small changes in the timing of the email relative to the salient reference date did not seem to matter for total accumulation, as employees responded more strongly the closer the email was to the reference date. Because we did not vary the presence of the reminder, the year-to-date contribution amount, and the salience of the year-end date, we cannot separately identify each component's contribution to the effect we measure.

B. Effect of the anchors

The first treatment effects we discuss are those of the 1% and 10% anchors in the 2009 emails. Figure 4 plots, separately by treatment condition, the average total contribution rate each pay period minus the total contribution rate the recipient had in effect on November 13, 2009, the last payday before the 2009 emails. The average contribution rate of the control (excluding employees ineligible to be assigned to both the 1% and 10% anchors), 1% anchor, and 10% anchor groups all rise during the first two pay periods before beginning to fall, but the 1% anchor group's average is persistently below the other two averages until March 5, 2010, when all three converge as bonuses are paid. The three series diverge from each other after March 5. The 1% anchor group again contributes less than the control group. The 10% anchor group, which had an

[11] Karlan et al. (2011) and Cadena and Schoar (2011) find that reminders affect financial behaviors in developing country settings. Carroll et al. (2009) find no effect from a reminder in the U.S. One key difference may be that the Karlan et al. (2011) and Cadena and Schoar (2011) reminders were associated with a deadline, whereas the Carroll et al. (2009) reminder was not. The emails we analyze in this paper are thus closer to the reminders that have previously been found to be effective.

average contribution rate close to the control group's average until March 5, has a higher contribution rate after March 5.

Panel A of Table 3 shows the regression-adjusted anchoring effects on average contribution rates in each pay period from November 27, 2009 to October 15, 2010. The dependent variable is the recipient's total contribution rate minus the recipient's total contribution rate on November 13, 2009. We control for a 1% anchor treatment dummy, a 10% anchor treatment dummy, and a dummy for whether the recipient was ineligible to receive the 10% anchor because his projected 2009 contributions were too high. We again exclude employees ineligible to be assigned to both anchors.

The point estimate of the 1% anchor treatment effect on the average contribution rate is consistently negative until March 5, 2010, but surprisingly—given our prior expectation that anchoring effects would be strongest immediately after the email was sent—its point estimate initially grows in magnitude over time and is not statistically significant at the 10% level until January 8. From January 22 to February 19, the 1% anchor decreases average total contribution rates by between 1.1% and 1.4% of income at the 5% significance level or better. The negative 1% anchor effect disappears when bonuses are paid on March 5, but re-emerges (insignificantly) the next pay period. It achieves 10% significance from April 30 to May 14, 5% significance from May 28 to July 23, and is again significant at the 10% level on September 17 and the 5% level from October 1 to 15. During the significant post-bonus periods, the 1% anchor depresses contribution rates by about 1% of income. Using as the dependent variable each employee's total contribution rate in excess of his November 13, 2009 total contribution rate averaged across paydays and controlling for treatment eligibility (not shown in exhibits), the 1% anchor decreases contribution rates by 0.8% of income ($p = 0.041$) during the seven paydays between November 27 and February 19, increases contribution rates by 0.05% ($p = 0.933$) of income during the March 5 bonus payday, and decreases contribution rates by 0.9% of income ($p = 0.039$) during the sixteen paydays from March 19 to October 15. Because we do not know how large each employee's bonus was, we do not know how each of these three averages should be weighted to construct the 1% anchor effect on total contributions as a percent of total compensation across all 24 paydays after the email was sent.

The delayed reaction of the average contribution rate to the 1% anchor may be due to employees re-reading the email weeks after it had been sent in order to remind themselves of the

instructions on how to change their contribution rate. The delayed reaction of the average is not caused by employees who react to the email with greater delay being more susceptible to anchors; the average contribution rate among employees who changed their contribution rate between the email send date and year-end 2009 also exhibits a growing divergence between the 1% anchor and control groups in January, an attenuation of the anchor effect on the bonus payday, and a re-emergence of the anchor effect after the bonus (not shown in exhibits). The fact that the 1% anchor had no significant effect on average contribution rates in 2009 does not mean it had no effect at all that year. A linear probability regression with the same controls as above (not shown in a table) reveals that 1% anchor recipients were 1.5 percentage points more likely ($p = 0.018$) to have a contribution rate exactly 1% of income higher than their November 13, 2009 contribution rate during at least one pay period between November 27 and December 24, 2009. This effect represents a doubling relative to the control group.[12]

In contrast, consistent with the visual impression in Figure 4, the 10% anchor never has a significant effect on the average contribution rate before the 2010 bonus (Table 3, Panel A) or the likelihood of a 10% contribution rate increase before year-end 2009 (not shown in a table). On the bonus payday, the point estimate of the 10% anchor effect becomes positive for the first time and remains consistently positive until October 15. The 10% anchor effect's point estimate post-bonus for the most part is about 0.5% of income and not statistically significant, but it grows to about 0.9% of income for three pay periods from August 20 to September 17 and achieves 10% significance. Controlling for treatment eligibility, the 10% anchor on average decreases contribution rates by 0.3% of income ($p = 0.429$) during the seven paydays before the bonus, increases contribution rates by 0.1% of income ($p = 0.864$) on the bonus payday, and increases contribution rates by 0.5% of income ($p = 0.260$) during the sixteen paydays after the bonus. Like the 1% anchor, the point estimates of the 10% anchor's average effect follow a similar pattern if we restrict the sample to employees who changed their contribution rates between the email send date and year-end 2009.

Although the 1% anchor results in lower average contribution rate increases, does it generate more equitable outcomes by encouraging a larger fraction of recipients to make small contribution rate increases? Changing the magnitude of a cue might have different impacts on the likelihood of inducing change versus the size of the change conditional on it occurring. Panel B

[12] More precisely, the constant term in the regression is also 1.5%.

of Table 3 replaces the dependent variable in Panel A's regressions with a dummy for a recipient's total contribution rate during a given pay period being different from her November 13, 2009 total contribution rate. In other words, these regressions model the probability of any change, regardless of size. We find no strong evidence that the anchors affected the probability of action. Neither the 1% nor 10% anchor treatment dummies is significant at the 5% level on any payday. The 1% anchor has one payday, January 8, where it has a positive 3.8 percentage point effect that is significant at the 10% level.

The relatively weak effect of the 10% anchor could be due to one of three reasons. First, 10% may be close to the average recipient's implicit reference point, or spontaneous anchor, when thinking about contribution increases, so the 10% anchor had little marginal effect on average. This story predicts that an even higher anchor would increase contribution rates more. Second, most recipients might consider a 10% contribution rate increase to be wildly implausible. Wegener et al. (2001) find that extreme anchors can have weaker effects than moderate anchors. This story implies that higher anchors would not have a greater effect. Third, if people place a high weight on immediate gratification (Laibson, 1997; McClure et al., 2004), then it may be harder for anchors to decrease current consumption (i.e. raise the savings rate) than to increase it. This explanation has no clear prediction about the effect of higher anchors, but may be able account for why the 10% anchor has a positive effect after bonuses are paid if immediate gratification is less tempting after a post-bonus consumption binge (Baumeister et al., 1998).

The second email campaign tested the effect of the 20% anchor treatment. In addition, we tested a less extreme low anchor, 3%, and re-tested the 10% anchor. Figure 5 shows the subsequent average total contribution rates in excess of the October 15, 2010 averages.

The visual appearance of the 10% anchor effect in the 2010 mailing replicates that from the 2009 mailing: the 10% anchor group hews closely to the control group before the March 11 bonus and contributes substantially more than the control group after the bonus. The other anchor groups' contribution rates relative to the 10% anchor group are ordered in a manner consistent with anchoring through the end of 2010: the 20% anchor group's contribution rates are higher than the 10% anchor group's, which are in turn higher than the 3% anchor group's. However, even the 20% anchor group does not consistently contribute more than the control group. In 2011, the 3% and 10% anchor groups contribute similar amounts through the end of the

sample period, while the 20% anchor group starts contributing more than the 10% anchor group from the end of April until the end of the sample period.

Panel A of Table 4 shows the regression-adjusted treatment effect estimates on the average total contribution rate in excess of the October 15, 2010 total contribution rate. Through March 18, only the 3% anchor has a significant effect relative to the control at the 10% level, depressing the average contribution rate by 0.6% of income in the very first pay period after the email. The statistical significance of the 3% treatment effect dissipates afterwards, but its point estimate remains –0.6% of income for another month. Despite the monotonic relationship between anchor size and treatment effect point estimate for most of 2010, we cannot in any pay period reject the hypothesis that all three anchoring effects are equal. In untabulated regressions, we find that none of the anchors in the second email campaign increased the probability that the recipient's contribution rate was exactly 3%, 10%, or 20% higher than her October 15, 2010 contribution rate in a subsequent pay period before year-end 2010.

Starting on April 1, all three anchor groups contribute significantly more than the control group. Unlike in the 2009 email campaign, the statistical significance of the 10% anchor effect is strong and appears shortly after the bonus: a 1.2% increase that is significant at the 5% level on April 1, a 1.4% increase that is significant at the 1% level on April 15, and a 0.9% increase that is significant at the 10% level on April 29. However, contrary to the visual impression from Figure 5, the 20% anchor group does not generally have a higher treatment effect than the other two anchor groups once we control for treatment eligibility. The 20% anchor effect point estimate is in fact mostly smaller than the 3% anchor effect point estimate, which surprisingly is usually the largest of the three and has the most persistent statistical significance.[13]

Averaging across paydays and controlling for treatment eligibility, the 3%, 10%, and 20% anchors decrease contribution rates by 0.3% of income (p = 0.473), 0.3% of income (p = 0.487), and 0.1% of income (p = 0.779), respectively, during the ten paydays between October 29 and March 4. They respectively increase bonus contributions by 0.2% (p = 0.759), 0.8% (p = 0.360), and 0.5% (p = 0.555) of the bonus. During the thirteen paydays from March 18 to

[13] Because of the treatment eligibility criteria, the population over which each of the anchoring treatments is identified differs slightly. If treatment effect heterogeneity is correlated with treatment eligibility, then a true monotonic treatment effect ordering within a fixed population could be obscured by our regression pooling procedure. However, running regressions using only employees eligible for all three anchors does not reveal treatment effect monotonicity after the bonus, while the monotonicity of the point estimates right after the email was sent remains.

September 2, the 3%, 10%, and 20% anchors increase contribution rates by 0.9% (p = 0.022), 0.6% (p = 0.139), and 0.9% (p = 0.048) of income, respectively.

In Panel B, we see that the anchors affected the probability of changing one's contribution rate in quite a different fashion from the way they did in the first email campaign. All three anchors *decreased* the probability of action relative to the control by as much as 7 percentage points in a given pay period, with no consistent relationship between anchor magnitude and action probability. This different reaction may be driven by the fact that the anchor examples in the second email campaign were couched in a more vague time frame than in the first email campaign ("soon before the year is over" versus "for the remaining six weeks of 2009"). The difference does not appear to be due to the fact that many employees were seeing the anchors for a second time; restricting the sample to employees who did not receive a 2009 email (which reduces the sample size considerably to 548) also yields negative treatment point estimates through January 7 that are sometimes significant.[14]

In summary, we find that in the short run, the lower two anchors we tested (1% and 3%) depress average contribution rates, but the higher two anchors (10% and 20%) do not increase average contribution rates relative to the no-anchor control group. After the bonus is paid, the lowest anchor we tested (1%) decreases contribution rates and the three highest anchors (3%, 10%, and 20%) increase contribution rates, but there is no significant difference among the high anchor treatment effect magnitudes. The fact that the 20% anchor effect is indistinguishable from the 10% anchor effect indicates that the relative weakness of the 10% anchor is not due to its being close to the average employee's reference contribution increase. The 3% anchor is an interesting intermediate case that decreases contribution rates in the short run but increases them in the long run. The evidence on how anchors affect the probability of making a contribution rate change is mixed; this effect may depend on the way the timing of action in the anchoring text is understood.

C. Effect of savings goal examples

Figure 6A shows how average total contribution rates evolve following the dissemination of the $7,000 and $11,000 savings goal examples. Through March 4, 2011, the $11,000 goal

[14] After January 7, the treatment effect point estimates in this subsample turn mostly positive, but the only significant coefficient is for the 3% anchor effect on April 15, when it achieves 10% significance.

group has persistently higher average contribution rates than the control group, with the gap peaking at 2.2% of income at year-end 2010. The ordering then reverses; the $11,000 goal group contributes less than the control group from March 18 to September 2. The $7,000 goal group oscillates above and below the control group, but is consistently below the $11,000 goal group before April 1 and above it afterwards.

Panel A of Table 5 contains the regression output analogue of Figure 6A. We see that the $11,000 goal treatment effect on the average contribution rate is increasing through year-end 2010, achieving statistical significance at the 10% level on November 26 and significance at the 5% level on December 23. The treatment effect remains significant at the 10% level as late as February 4. Starting on April 1 through the end of the sample period, the treatment effect point estimate is negative but insignificant. The $7,000 goal treatment effect is significant only once at the 10% level on May 27, when its point estimate is a positive 0.8% of income. Averaging across paydays, the $11,000 goal increases contribution rates by 1.1% of income ($p = 0.044$) before the bonus, increases the bonus contribution by 0.1% of the bonus ($p = 0.893$), and decreases contribution rates by 0.3% of income ($p = 0.369$) after the bonus. The $7,000 goal increases contributions by 0.008% of income ($p = 0.989$) before the bonus, decreases the bonus contribution by 1.5% of the bonus ($p = 0.145$), and increases contributions by 0.040% of income ($p = 0.912$) after the bonus.

Although the later negative point estimates of the $11,000 goal treatment effect might indicate that the $11,000 goal's positive effect on contributions reverses in the long run, the reversal could be an artifact of the annual dollar cap on before-tax plus Roth contributions. Because the $11,000 goal group contributed more early in 2011, they were more likely to hit the cap midway through the year, forcing their before-tax and Roth contribution rates to be zero for the remainder of 2011.[15]

We cannot identify precisely which employees were constrained by the cap because we have no information on the size of each employee's 2011 bonus, so we do not know the dollars contributed out of the bonus for the 95% of employees who had a percentage contribution election for their bonus. However, an employee who reduces his contribution rate to zero for the remainder of 2011 is likely to have done so because he hit the cap. We therefore construct an

[15] They would still be able to contribute on an after-tax basis, but after-tax contributions are not matched and have a less favorable tax treatment.

alternative contribution rate series that replaces any unbroken string of 0% total contribution rates that ends on September 2, 2011 (the end of our sample period) with the last positive total contribution rate the employee had in 2011. The difference between this alternative series and the actual series shows how much of the drop in each group's average contribution rate is likely to be driven by the contribution cap.

Figure 6B plots the resulting average contribution rates. In this graph, the $11,000 goal group never contributes less than the control or $7,000 goal groups, suggesting that the reversal in Figure 6A is due to the contribution cap. Consistent with our assumption that permanent moves to a 0% contribution rate are due to the cap, the three contribution rate series in Figure 6B are indistinguishable from those in Figure 6A early in the year—when people are less likely to have hit the cap—and begin to deviate only after the March 11 bonus is paid.

Heath, Larrick, and Wu (1999) argue that goals very far from the status quo create a "starting problem," where individuals find it difficult to motivate themselves to start a task. The linear probability regressions in Panel B of Table 5 show no evidence that our seemingly ambitious $11,000 goal generated a starting problem. The probability of having a contribution rate different than one's October 15, 2010 contribution rate is between 1.5 and 5.9 percentage points higher among $11,000 goal recipients than control email recipients, depending on the pay period, and this difference is significant at the 10% level on December 23, 2010. The $7,000 goal group is also more likely to act than the control group, with the difference in probabilities being significant at the 5% level on January 7 and at the 10% level on February 18, March 4, and the bonus payday. On these significant days, the $7,000 goal group has an 8 to 9% higher probability of having a different contribution rate. There is no evidence that the probability of action is lower for the $11,000 goal group than for the $7,000 goal group. The absolute value of the *t*-statistic in a test of the difference between the two groups never exceeds the 1.39 ($p = 0.165$) it attains on December 23, when the $11,000 goal group is *more* likely to have acted than the $7,000 goal group.

D. Effect of $3,000 and $16,500 savings threshold salience

We begin our analysis of making the $3,000 and $16,500 savings thresholds salient by examining a histogram of total 2010 before-tax plus Roth contribution amounts by treatment

group.[16] Figure 7 shows that those who received the $3,000 threshold treatment appear more likely than those who received the $16,500 threshold treatment to end up with 2010 before-tax plus Roth contributions clustered around $3,000. Specifically, the $3,000 threshold treatment recipients were 5.0 percentage points more likely to end up with 2010 contributions between $2,700 and $2,999, 0.8 percentage points more likely to end up with 2010 contributions between $3,000 and $3,299, and 0.4 percentage points more likely to end up with 2010 contributions between $3,300 and $3,599. The 6.2 percentage point increase in the probability of having 2010 contributions totaling between $2,700 and $3,599 is not statistically significant, however (p = 0.113).

Despite there being hints that the $3,000 threshold treatment affected 2010 contributions relative to the $16,500 threshold treatment, this effect does not appear in average total contribution rates. Figure 8 shows that the average total contribution rates of the two groups are quite similar through year-end 2010. But a large gap opens up in 2011, as $3,000 threshold treatment recipients drop their contribution rate much more than $16,500 threshold treatment recipients. Panel A of Table 6 shows that the difference between the two groups' average total contribution rates peaks at 1.5% of income on February 18, when it also achieves statistical significance at the 5% level. The difference is also significant at the 10% level on January 21, March 4, and April 1 through May 13, and completely disappears by July 22. Averaging across the January 7 through July 8 non-bonus paydays, the $16,500 threshold group on average contributed 0.9% of income (p = 0.043) more than the $3,000 threshold group. The $16,500 threshold group also contributed 0.7% more of their bonus (p = 0.359). Panel B of Table 6 indicates, however, that the threshold highlights did not have differential effects on the probability of action.

E. Effect of 60% contribution rate threshold salience

We analyze the effect of making the 60% contribution rate threshold salient separately for recipients who were on pace to contribute less than $2,500, between $2,500 and $4,999, and between $5,000 and $16,499 in 2009, since each of these groups faced different marginal matching incentives.

[16] We examine before-tax plus Roth contributions instead of total contributions in the histogram because the thresholds in the treatments were linked to the match, which was only earned on before-tax and Roth contributions.

Figure 9 plots over time the average total contribution rate in excess of the November 13, 2009 total contribution rate. Recipients of the 60% threshold treatment who were projected to contribute less than $2,500 in 2009 immediately raise their contribution rate by 2.5% of income more than the control group, and this gap grows to 2.9% of income on December 24 before attenuating to less than 1% of income from January 22 to October 15. Table 7 shows the corresponding regression results through the March 5 bonus payday only for the sake of brevity. Panel A indicates that the treatment effects are statistically significant at the 5% level or better through January 8, 2010 and insignificant afterwards. Averaging across paydays, the 60% threshold treatment increased contribution rates by 1.8% of income (p = 0.012) before the bonus, 1.4% of income (p = 0.153) on the bonus payday, and 0.7% of income (p = 0.366) after the bonus through October 15, 2010.

On the other hand, the bottom two graphs in Figure 9 and Panels B and C of Table 7 indicate that there is no significant 60% threshold treatment effect for recipients who were on pace to contribute at least $2,500 in 2009.

In untabulated regressions, we examine whether the 60% highlight caused recipients to contribute exactly 60% of their income in any pay period between November 27, 2009 and October 15, 2010.[17] These regressions show that the 60% threshold treatment made contributing at 60% more likely only for recipients who were previously on pace to contribute less than $2,500 in 2009. The effect for these recipients was a 5.7 percentage point increase (p = 0.020) in the probability of contributing 60%, up from a baseline probability of 5.4% in the control group. The 60% threshold treatment effect's point estimate declines to an insignificant 1.1% (p = 0.411) for recipients on pace to contribute between $2,500 and $4,999 in 2009, and declines even further to an insignificant –1.0% (p = 0.461) effect for recipients on pace to contribute between $5,000 and $16,499 in 2009.

Is the 60% threshold treatment effect on low contributors due to their learning from it that the plan's maximum contribution rate is 60%? According to this explanation, employees in the control group chose smaller contribution increases than they otherwise would have because they falsely believed they were not allowed to contribute more. Table 8 presents evidence against this explanation. The coefficients are from a regression where the dependent variable is a dummy for

[17] The results are qualitatively similar if we only consider the period from November 27, 2009 to December 24, 2009.

having a higher total contribution rate than one had in effect on November 13, 2009, and the explanatory variable is a dummy for receiving the 60% threshold treatment.[18] The regressions in Panel A show that among low contributors, those who received a 60% highlight were between 5.7 and 13.5 percentage points more likely to make an increase of any size between November 27 and March 5, whereas the information story would predict that both groups would be equally likely to make a contribution increase (albeit of different sizes). These results also indicate that making salient the very high maximum possible contribution rate did not induce inertia due to demotivation among low savers.

Panels B and C show that among those on pace to contribute more than $2,500, there is no effect of the 60% threshold treatment on the probability of increasing contributions, consistent with the previous null effects within these projected contribution categories on average contribution rates and the probability of contributing exactly 60%.

Table 9 explores further a theme that emerges from the analysis so far: The 60% threshold treatment has a larger effect on people contributing little at the time the email was sent. The table shows that low contribution *rates*, not low contribution dollar amounts, predict susceptibility to the 60% threshold treatment, even within the population on pace to contribute less than $2,500 in 2009. The dependent variable in the regressions, which are run separately for each projected contribution category, is the difference between that pay period's total contribution rate and the November 13, 2009 total contribution rate. The explanatory variables are a 60% threshold treatment dummy, a dummy for the November 13, 2009 total contribution rate being 0% or 1%, and an interaction between these two dummies.[19]

For those projected to contribute less than $2,500 (Panel A), the interaction is 3.9% and significant at the 5% level on November 27. In contrast, the coefficient on the uninteracted treatment dummy is only 1.0% and insignificant, indicating that almost all of the 60% threshold treatment effect in this contribution category is concentrated among employees with contributions of 0 to 1%. The interaction loses statistical significance by December 24 and attenuates, but the point estimate remains sizable, never falling below 1.3%.

[18] Untabulated regressions where the dependent variable is a dummy for having a contribution rate that is *either* higher or lower than the November 13, 2009 value yield similar results.

[19] We have chosen a dummy for the total November 13 contribution rate being 0% or 1% because in untabulated regressions of November 27 contribution rates minus November 13 contribution rates that control for dummies for each November 13 contribution rate from 0% to 5% and interactions of those dummies with the treatment effect, the 0% and 1% interactions are large and the other interactions are small or negative.

Even though the 60% threshold treatment's average effect on employees projected to contribute $2,500 to $4,999 and those projected to contribute $5,000 or more was small and insignificant in Table 7, Panels B and C of Table 9 show that there is a strong positive treatment effect among employees in these projected contribution categories who were contributing 0 to 1% at the time of the email. The treatment interaction among recipients projected to contribute $2,500 to $4,999 is significant and much more persistent than the interaction among those projected to contribute less than $2,500. The interaction starts at 3.5% but grows to 9.1% by January 8 and remains large (6.8%) and significant through March 5. Adding the treatment and interaction coefficients together yields a treatment effect for 0 to 1% contributors in this projected contribution category of 3.2% to 8.9% of income. The treatment interaction pattern for recipients projected to contribute more than $5,000 is closer to that of the first projected contribution category; the interaction is large (6.4%) and significant on the first payday after the email, but loses statistical significance immediately afterwards while retaining a large positive point estimate (between 0.8% and 5.0%) until February 19. In that first payday, the treatment effect for 0 to 1% contributors is 6.3% of income.

Beshears et al. (2010b) present evidence that low-income employees are more strongly influenced by the default contribution rate in retirement savings plans. However, the strength of the 60% threshold treatment effect among 0 to 1% contributors does not seem to be explained by a general negative correlation between income and susceptibility to "nudges." The average salary of those contributing 0 to 1% immediately prior to the email is 41% higher than that of those contributing at a higher rate among employees on pace to contribute less than $2,500, 61% higher among employees on pace to contribute between $2,500 and $4,999, and 5% lower among employees on pace to contribute more than $5,000.

Our leading hypothesis is that employees with low contribution rates were particularly motivated by the 60% contribution rate cue because of the especially large gap between their current contribution rate and the cue. There is some weak evidence that the savings goal cues were also especially effective for people whose year-to-date dollar contributions were far below the cue. In untabulated regressions, we find that the point estimates of the interactions between the goal treatment dummies and total 2010 dollar contributions prior to the second email are usually negative through March 4, 2011 (8 out of 10 pay periods for the $7,000 goal cue and 9 out of 10 pay periods for the $11,000 goal cue), and the interaction with the $11,000 goal

treatment is negative and significant at the 10% level on February 4, 2011.[20]

V. Conclusion

This paper documents that small numerical cues can influence decisions as economically significant and familiar as retirement savings plan contributions. Low cues decrease contribution rates by up to 1.5% of income, and high cues increase contribution rates by up to 2.9% of income. Cues have large effects even when the surrounding communication explicitly denies that the cue has informational content, implying that even incidental numbers contained in a communication can have large unintended consequences on subsequent choices.

In many ways this was an unlikely setting for these kinds of cues to wield much influence. The communication was the kind of administrative email many employees ignore. For those who did read it, the cues were only a very small part of a relatively information-rich note. Finally, acting on the cue required multiple steps beyond reading the email (logging into the 401(k) administrator's site, etc.). Finding an economically meaningful influence on decisions in this setting underscores the potential importance of these cues. Moreover, the impact of these cues was long-lasting—in some conditions, for up to a year.

Our treatment effects are estimated on a particular sample of employees—people who are generally highly educated, technology savvy, accustomed to making changes in their 401(k), and on good terms with the company management. However, we believe that cues can be effective in populations that are quite different from our study company's population. In a paper released after the first working paper draft of our own study, Goda, Manchester, and Sojourner (2011) describe a field experiment they ran using hard-copy mailings to University of Minnesota employees. Cues are not the main focus of their experiment; they are primarily interested in the effect that providing projections of asset balances and income to employees has on retirement savings plan contributions. But they did randomly vary the graphs used to communicate these projections. One set of graphs showed asset and income projections for the cases where the employees increased their savings by \$0, \$50, \$100, or \$250 per pay period. The other set of graphs showed these projections for the cases where the employees increased their savings by

[20] Because employees assigned to the \$3,000 and \$16,500 threshold treatments were not eligible to be assigned to a no-cue control group, we can only test whether the rate at which the \$3,000 threshold effect changes with year-to-date contributions is faster than the rate at which the \$16,500 threshold effect changes with year-to-date contributions. We cannot test whether any single threshold effect's absolute size changes with year-to-date contributions.

$0, $100, $200, or $500 per pay period. Employees receiving the graphs with the higher savings examples had a contribution rate six months after the mailing that was on average 0.19% of income higher than that of those who received the graphs with the lower savings examples.[21]

Our findings provide both an opportunity and a warning for organizations and policy makers. The kinds of cues we investigate could be intentionally used to influence saving behavior more efficiently than through the use of more costly interventions, such as financial education or increases in matching incentives. But unintentional cues buried in mundane communications can also affect behavior. Thus, organizations and policymakers should take responsibility for the cues they disseminate and wield them mindfully.

References

Ariely, Dan, George Loewenstein, and Drazen Prelec, 2003. "'Coherent arbitrariness': Stable demand curves without stable preferences." *Quarterly Journal of Economics* 118, pp. 73-105.

Baker, Malcolm, Xin Pan, and Jeffrey Wurgler, forthcoming. "A reference point theory of mergers and acquisitions." *Journal of Financial Economics*.

Bargh, John A., Peter M. Gollwitzer, Annette Lee-Chai, Kimberly Barndollar, and Roman Trötschel, 2001. "The automated will: Nonconscious activation and pursuit of behavioral goals." *Journal of Personality and Social Psychology* 81, pp. 1014-1027.

Baumeister, Roy F., Ellen Bratslavsky, Mark Muraven, and Dianne M. Tice, 1998. "Ego depletion: Is the active self a limited resource?" *Journal of Personality and Social Psychology* 74, pp. 1252-1265.

Beggs, Alan, and Kathryn Graddy, 2009. "Anchoring effects: Evidence from art auctions." *American Economic Review* 99, pp. 1027-1039.

Benartzi, Shlomo, Ehud Peleg, and Richard H. Thaler, forthcoming. "Choice architecture and retirement savings plans." In Eldar Shafir, ed., *The Behavioral Foundations of Policy*. Princeton, NJ: Princeton University Press.

Benartzi, Shlomo, and Richard H. Thaler, 2007. "Heuristics and biases in retirement savings behavior." *Journal of Economic Perspectives* 21, pp. 81-104.

Beshears, John, James J. Choi, David Laibson, and Brigitte C. Madrian, 2008. "The importance of default options for retirement savings outcomes: Evidence from the United States." In Stephen J. Kay and Tapen Sinha, editors, *Lessons from Pension Reform in the Americas*, pp. 59-87. Oxford: Oxford University Press.

[21] The magnitude of this treatment effect cannot be directly compared to ours because Goda, Manchester, and Sojourner's graphs were distributed in a paper mailing and were not prominently featured, but appeared on the second page of a four-page brochure.

Beshears, John, James J. Choi, David Laibson, and Brigitte C. Madrian, 2010a. "Simplification and saving." NBER Working Paper 12659.

Beshears, John, James J. Choi, David Laibson, and Brigitte C. Madrian, 2010b. "The limitations of defaults." Mimeo, Harvard University.

Cadena, Ximena, and Antoinette Schoar, 2011. "Remembering to pay? Reminders vs. financial incentives for loan payments." NBER Working Paper 17020.

Carroll, Gabriel D., James J. Choi, David Laibson, Brigitte C. Madrian, and Andrew Metrick, 2009. "Optimal Defaults and Active Decisions." *Quarterly Journal of Economics* 124, pp. 1639-1674.

Chapman, Gretchen B., and Eric J. Johnson, 1994. "The limits of anchoring." *Journal of Behavioral Decision Making* 7, pp. 223-242.

Chapman, Gretchen B., and Eric J. Johnson, 1999. "Anchoring, activation, and the construction of values." *Organizational Behavior and Human Decision Processes* 79, pp. 115-153.

Chapman, Gretchen B., and Eric J. Johnson, 2002. "Incorporating the irrelevant: Anchors in judgments of belief and value." In Thomas Gilovich, Dale Griffin, Daniel Kahneman, eds., *Heuristics and Biases: The Psychology of Intuitive Judgment.* New York: Cambridge University Press, pp. 120-138.

Chartrand, Tanya L., and John A. Bargh, 1996. "Automatic activation of impression formation and memorization goals: Nonconscious goal priming reproduces effects of explicit task instructions." *Journal of Personality and Social Psychology* 71, pp. 464-478.

Choi, James J., David Laibson, Brigitte C. Madrian, 2009a. "Mental Accounting in Portfolio Choice: Evidence from a Flypaper Effect." *American Economic Review* 99, pp. 2085-2095.

Choi, James J., David Laibson, and Brigitte C. Madrian, 2009b. "Reducing the Complexity Costs of 401(k) Participation: The Case of Quick Enrollment." In David A. Wise, editor, *Developments in the Economics of Aging*, pp. 57-82. Chicago: University of Chicago Press.

Choi, James J., David Laibson, Brigitte C. Madrian, and Andrew Metrick, 2002. "Defined Contribution Pensions: Plan Rules, Participant Decisions, and the Path of Least Resistance." In James Poterba, ed., *Tax Policy and the Economy* 16, pp. 67-114.

Choi, James J., David Laibson, Brigitte C. Madrian, and Andrew Metrick, 2004. "For Better or For Worse: Default Effects and 401(k) Savings Behavior." In David Wise, ed., *Perspectives in the Economics of Aging*. Chicago: University of Chicago Press, pp. 81-121.

Dougal, Casey, Joseph Engelberg, Christopher A. Parsons, and Edward D. Van Wesep, 2010. "Anchoring and the cost of capital." Mimeo, University of North Carolina at Chapel Hill.

Epley, Nicholas, and Thomas Gilovich, 2001. "Putting adjustment back in the anchoring and adjustment heuristic: Differential processing of self-generated and experimenter-provided anchors." *Psychological Science* 12, pp. 391-396.

Goda, Gopi Shah, Colleen Flaherty Manchester, and Aaron Sojourner, 2011. "What's my account really worth? The effect of lifetime income disclosure on retirement savings." Mimeo, University of Minnesota.

Green, Donald, Karen E. Jacowitz, Daniel Kahneman, and Daniel McFadden, 1998. "Referendum contingent valuation, anchoring, and willingness to pay for public goods." *Resources and Energy Economics* 20, pp. 85-116.

Heath, Chip, Richard P. Larrick, and George Wu, 1999. "Goals as reference points." *Cognitive Psychology* 38, pp. 79-109.

Huberman, Gur, Sheena S. Iyengar, and Wei Jiang, 2004. "How much choice is too much: Determinants of individual contributions in 401K retirement plans." In Olivia S. Mitchell and Steven P. Utkus, eds., *Pension Design and Structure: New Lessons from Behavioral Finance*, pp. 83-96. Oxford: Oxford University Press.

Iyengar, Sheena S., and Emir Kamenica, 2010. "Choice proliferation, simplicity seeking, and asset allocation." *Journal of Public Economics* 94, pp. 530-539.

Jacowitz, Karen E., and Daniel Kahneman, 1995. "Measures of anchoring in estimation tasks." *Personality and Social Psychology Bulletin* 21, pp. 1161-1166.

Johnson, Eric, and David A. Schkade, 1989. "Bias in utility assessments: Further evidence and explanations." *Management Science* 35, pp. 406-424.

Karlan, Dean, Margaret McConnell, Sendhil Mullainathan, and Jonathan Zinman, 2011. "Getting to the top of mind: How reminders increase saving." NBER Working Paper 16205.

Kahneman, Daniel, and Jack Knetsch, 1993. "Anchoring or shallow inferences: The effect of format." Mimeo, University of California, Berkeley.

Kusko, Andrea, James Poterba, and David Wilcox, 1998. "Employee Decisions with Respect to 401(k) Plans." In Olivia Mitchell and Sylvester Schieber, eds., *Living with Defined Contribution Pensions: Remaking Responsibility for Retirement.* Philadelphia: University of Pennsylvania Press, pp. 98-112.

Laibson, David, 1997. "Golden eggs and hyperbolic discounting." *Quarterly Journal of Economics* 112, pp. 443-477.

Locke, Edwin A., and Gary P. Latham, 1990. *A Theory of Goal Setting and Task Performance*. Englewood Cliffs, NJ: Prentice-Hall.

Locke, Edwin A., and Gary P. Latham, 2002. "Building a practically useful theory of goal setting and task motivation: A 35-year odyssey." *American Psychologist* 57, pp. 705–717.

Locke, Edwin A., and Gary P. Latham, 2006. "New directions in goal-setting theory." *Current Directions in Psychological Science* 15, 265-268.

Madrian, Brigitte C., and Dennis F. Shea, 2001. "The power of suggestion: Inertia in 401(k) participation and savings behavior." *Quarterly Journal of Economics* 116, pp. 1149-1187.

McClure, Samuel M., David I. Laibson, George Loewenstein, and Jonathan D. Cohen, 2004. "Separate neural systems value immediate and delayed monetary rewards." *Science* 306, pp. 503-507.

Mussweiler, Thomas, and Fritz Strack, 1999. "Hypothesis-consistent testing and semantic priming in the anchoring paradigm: A selective accessibility model." *Journal of Experimental and Social Psychology* 35, pp. 136-164.

Mussweiler, Thomas, and Fritz Strack, 2001. "Considering the impossible: Explaining the effects of implausible anchors." *Social Cognition* 19, pp. 145-160.

Stajkovic, Alexander D., Edwin A. Locke, and Eden S. Blair, 2006. "A first examination of the relationships between primed subconscious goals, assigned conscious goals, and task performance." *Journal of Applied Psychology* 91, pp. 1172-1180.

Stewart, Neil, 2009. "The cost of anchoring on credit-card minimum repayments." *Psychological Science* 20, pp. 39-41.

Strack, Frtiz, and Thomas Mussweiler, 1997. "Explaining the enigmatic anchoring effect: Mechanisms of selective accessibility." *Journal of Personality and Social Psychology* 73, pp. 437-446.

Thaler, Richard H., and Shlomo Benartzi, 2004. "Save More Tomorrow™: Using behavioral economics to increase employee saving." *Journal of Political Economy* 112, pp. S164-S187.

Tversky, Amos, and Daniel Kahneman, 1974. "Judgment under uncertainty: Heuristics and biases." *Science* 185, pp. 1124-1131.

Wegener, Duane T., Richard E. Petty, Brian T. Detweiler-Bedell, and W. Blair G. Jarvis, 2001. "Implications of attitude change theories for numerical anchoring: Anchor plausibility and the limits of anchor effectiveness." *Journal of Experimental Social Psychology* 37, pp. 62-69.

Table 1. Cue text

This table lists the text that was inserted into the emails in each cue treatment at the point indicated in Figure 1.

Cue type	Treatment	Additional email text
Anchor	1% anchor	For example, you could increase your contribution rate by 1% of your income and get more of the match money for which you're eligible. (1% is just an example, and shouldn't be interpreted as advice on what the right contribution increase is for you.)
	3% anchor	For example, you could increase your contribution rate by 3% of your income and get more of the match money for which you're eligible. (3% is just an example, and shouldn't be interpreted as advice on what the right contribution increase is for you.)
	10% anchor	For example, you could increase your contribution rate by 10% of your income and get more of the match money for which you're eligible. (10% is just an example, and shouldn't be interpreted as advice on what the right contribution increase is for you.)
	20% anchor	For example, you could increase your contribution rate by 20% of your income and get more of the match money for which you're eligible. (20% is just an example, and shouldn't be interpreted as advice on what the right contribution increase is for you.)
Savings goal	$7,000 goal	For example, suppose you set a goal to contribute $7,000 for the year and you attained it. You would earn $500 more in matching money this year than you're currently on pace for.
	$11,000 goal	For example, suppose you set a goal to contribute $11,000 for the year and you attained it. You would earn $2,500 more in matching money this year than you're currently on pace for.
Savings threshold	$3,000 threshold	The next $*x* of contributions you make between now and December 31 will be matched at a 100% rate. *[x is the difference between $3,000 and the recipient's year-to-date match-eligible contributions]*
	$16,500 threshold	Contributing $*y* more between now and December 31 would earn you the maximum possible match. *[y is the difference between $16,500 and the recipient's year-to-date match-eligible contributions]*
	60% threshold	You can contribute up to 60% of your income in any pay period.

Table 2. Subjects per experimental cell

This table shows the number of employees who received each version of the 401(k) email in 2009 and 2010. The numbers are reported separately by projected contribution category. Projected contributions are the total before-tax plus Roth contributions to the 401(k) an employee would have ended up with in 2009 or 2010 if the contribution rates effective immediately prior to email receipt remained unchanged for the remainder of the calendar year.

Panel A: 2009 email campaign			
	Projected 2009 before-tax + Roth contributions		
	$0 - $2,499	$2,500 - $4,999	$5,000 - $16,499
Control	257	651	1,008
1% anchor	0	0	968
10% anchor	0	0	829
60% threshold	252	651	1,006
Panel B: 2010 email campaign			
	Projected 2010 before-tax + Roth contributions		
	$0 - $2,999	$3,000 - $5,999	$6,000 - $16,499
Control	0	263	660
Delayed control	0	260	561
3% anchor	0	0	660
10% anchor	0	0	609
20% anchor	0	0	565
$7,000 savings goal	0	264	0
$11,000 savings goal	0	262	0
$3,000 threshold	226	0	0
$16,500 threshold	225	0	0

Table 3. Effect of 1% and 10% anchors in 2009 emails

Within each panel, a separate regression is run for each column. The sample is employees who were on pace to contribute at least $5,000 in before-tax plus Roth contributions in 2009 if they left the contribution rates in effect on November 4, 2009 unchanged for the remainder of 2009. We exclude employees ineligible to be assigned to both anchors. In Panel A, the dependent variable is the difference between the total (before-tax plus after-tax plus Roth) 401(k) contribution rate effective on the column's pay day and the total contribution rate effective on November 13, 2009. In Panel B, the dependent variable is a dummy for whether the total contribution rate on the column's payday differs from the total contribution rate on November 13, 2009. The control variables are dummies for whether the employee received the 1% anchor, the 10% anchor, and whether the employee was ineligible to receive the 10% anchor. Standard errors are in parentheses below the point estimates.

Panel A: Contribution rate relative to 11/13/2009 contribution rate

	11/27/09	12/11/09	12/24/09	1/8/10	1/22/10	2/5/10	2/19/10	3/5/10	3/19/10	4/2/10	4/16/10	4/30/10
1% anchor	-0.573	-0.174	-0.326	-0.962^{+}	-1.306**	-1.395**	-1.103*	0.054	-0.492	-0.752	-0.724	-0.782^{+}
	(0.450)	(0.556)	(0.601)	(0.563)	(0.501)	(0.485)	(0.485)	(0.639)	(0.482)	(0.486)	(0.467)	(0.468)
10% anchor	-0.422	-0.368	-0.174	-0.628	-0.220	-0.319	-0.282	0.116	0.451	0.438	0.319	0.611
	(0.477)	(0.590)	(0.638)	(0.597)	(0.532)	(0.515)	(0.516)	(0.678)	(0.513)	(0.517)	(0.500)	(0.498)
No 10% assignment	-0.671	-2.629**	-4.508**	-1.976*	-0.904	-0.873	-0.430	-1.403	-1.989**	-1.352^{+}	-1.002	-0.789
	(0.642)	(0.794)	(0.859)	(0.804)	(0.714)	(0.692)	(0.692)	(0.911)	(0.688)	(0.694)	(0.666)	(0.667)
Constant	3.092**	4.337**	3.955**	4.702**	2.751**	2.335**	1.871**	2.140**	-0.369	-0.162	-0.235	-0.464
	(0.331)	(0.409)	(0.442)	(0.415)	(0.369)	(0.357)	(0.357)	(0.470)	(0.356)	(0.358)	(0.344)	(0.345)

	5/14/10	5/28/10	6/11/10	6/25/10	7/9/10	7/23/10	8/6/10	8/20/10	9/3/10	9/17/10	10/1/10	10/15/10
1% anchor	-0.906^{+}	-0.955*	-1.190*	-1.029*	-0.927*	-0.966*	-0.770	-0.711	-0.633	-0.909^{+}	-1.063*	-1.171*
	(0.470)	(0.468)	(0.467)	(0.475)	(0.468)	(0.472)	(0.485)	(0.489)	(0.490)	(0.498)	(0.509)	(0.519)
10% anchor	0.561	0.363	0.303	0.410	0.572	0.467	0.725	0.877^{+}	0.998^{+}	0.946^{+}	0.712	0.759
	(0.500)	(0.497)	(0.496)	(0.504)	(0.498)	(0.502)	(0.516)	(0.519)	(0.520)	(0.529)	(0.540)	(0.551)
No 10% assignment	-0.951	-1.249^{+}	-1.846**	-2.263**	-2.475**	-2.565**	-2.815**	-2.983**	-3.501**	-3.566**	-3.684**	-3.983**
	(0.670)	(0.668)	(0.667)	(0.677)	(0.666)	(0.672)	(0.692)	(0.699)	(0.698)	(0.712)	(0.726)	(0.741)
Constant	-0.566	-0.480	-0.637^{+}	-0.764*	-1.066**	-1.190**	-1.576**	-1.738**	-2.056**	-3.566**	-2.155**	-2.244**
	(0.346)	(0.345)	(0.345)	(0.350)	(0.346)	(0.348)	(0.358)	(0.360)	(0.361)	(0.712)	(0.376)	(0.383)

Panel B: Probability of contribution rate different than 11/13/2009 contribution rate

	11/27/09	12/11/09	12/24/09	1/8/10	1/22/10	2/5/10	2/19/10	3/5/10	3/19/10	4/2/10	4/16/10	4/30/10
1% anchor	0.012	0.018	0.017	0.038^{+}	0.031	0.035	0.032	0.002	0.025	0.024	0.021	0.020
	(0.017)	(0.020)	(0.021)	(0.022)	(0.022)	(0.022)	(0.022)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
10% anchor	0.008	0.011	0.011	0.034	0.015	0.020	0.012	0.006	0.017	0.008	0.005	-0.002
	(0.018)	(0.021)	(0.022)	(0.023)	(0.023)	(0.023)	(0.023)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
No 10% assignment	0.084**	0.143**	0.138**	-0.142**	-0.143**	-0.113**	-0.115**	-0.099**	-0.102**	-0.085**	-0.087**	-0.099**
	(0.024)	(0.028)	(0.029)	(0.031)	(0.031)	(0.031)	(0.031)	(0.029)	(0.030)	(0.030)	(0.030)	(0.030)
Constant	0.147**	0.227**	0.264**	0.611**	0.626**	0.627**	0.640**	0.727**	0.699**	0.696**	0.697**	0.705**
	(0.012)	(0.014)	(0.015)	(0.016)	(0.016)	(0.016)	(0.016)	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)

	5/14/10	5/28/10	6/11/10	6/25/10	7/9/10	7/23/10	8/6/10	8/20/10	9/3/10	9/17/10	10/1/10	10/15/10
1% anchor	0.025	0.021	0.019	0.016	0.009	0.011	0.008	0.010	0.012	0.010	0.008	0.007
	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
10% anchor	0.003	-0.003	-0.006	-0.007	-0.011	-0.015	-0.015	-0.014	-0.007	-0.009	-0.008	-0.002
	(0.022)	(0.023)	(0.023)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
No 10% assignment	-0.092**	-0.092**	-0.080**	-0.077*	-0.064*	-0.068*	-0.058^{+}	-0.049	-0.046	-0.026	-0.020	-0.005
	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.029)
Constant	0.703**	0.705**	0.709**	0.714**	0.720**	0.724**	0.731**	0.733**	0.736**	0.742**	0.745**	0.748**
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)

$^{+}$ Significant at the 10% level. * Significant at the 5% level. ** Significant at the 1% level.

Table 4. Effect of 3%, 10%, and 20% anchors in 2010 emails

Within each panel, a separate regression is run for each column. The sample is employees who were not assigned to the delayed control group and were on pace to contribute at least $6,000 in before-tax plus Roth contributions in 2010 if they left the contribution rates in effect on October 15, 2010 unchanged for the remainder of 2010. In Panel A, the dependent variable is the difference between the total (before-tax plus after-tax plus Roth) 401(k) contribution rate effective on the column's pay day and the total contribution rate effective on October 15, 2010. In Panel B, the dependent variable is a dummy for whether the total contribution rate on the column's payday differs from the total contribution rate on October 15, 2010. The columns labeled "Bonus" uses the contribution rate elections in effect for the annual bonus to construct the dependent variable. The control variables are dummies for whether the employee received the 3% anchor, 10% anchor, or 20% anchor, and whether the employee was ineligible to receive the 10% or 20% anchors. We exclude employees ineligible to be assigned to all three anchors. Standard errors are in parentheses below the point estimates.

Panel A: Contribution rate relative to 10/15/2010 contribution rate

	10/29/10	11/12/10	11/26/10	12/10/10	12/23/10	1/7/11	1/21/11	2/4/11	2/18/11	3/4/11	Bonus	3/18/11
3% anchor	-0.649^{+}	-0.639	-0.615	-0.068	-0.593	-0.429	-0.161	-0.116	0.006	0.219	0.247	0.641
	(0.373)	(0.484)	(0.576)	(0.584)	(0.592)	(0.583)	(0.527)	(0.500)	(0.576)	(0.525)	(0.805)	(0.485)
10% anchor	-0.601	-0.468	-0.233	0.233	-0.634	-0.855	-0.392	-0.602	-0.078	0.107	0.762	0.648
	(0.384)	(0.499)	(0.593)	(0.602)	(0.610)	(0.601)	(0.544)	(0.516)	(0.594)	(0.542)	(0.830)	(0.501)
20% anchor	-0.419	-0.296	-0.196	0.307	0.220	-0.046	-0.077	-0.447	-0.448	-0.154	0.503	0.445
	(0.394)	(0.511)	(0.608)	(0.617)	(0.625)	(0.616)	(0.557)	(0.528)	(0.609)	(0.555)	(0.852)	(0.513)
No 20%	-0.537	-1.205	-2.061*	-2.888**	-3.322**	-1.887*	-1.623^{+}	-1.941*	-2.423**	-2.272**	-2.429^{+}	-2.302**
assignment	(0.600)	(0.781)	(0.928)	(0.943)	(0.955)	(0.940)	(0.849)	(0.805)	(0.926)	(0.845)	(1.298)	(0.782)
No 10% or	-1.320^{+}	-1.753^{+}	-2.411*	-3.034**	-5.509**	-1.875^{+}	-1.458	-1.092	0.669	-0.517	-0.079	-2.950**
20% assign	(0.695)	(0.905)	(1.080)	(1.094)	(1.108)	(1.091)	(0.985)	(0.934)	(1.075)	(0.980)	(1.546)	(0.904)
Constant	2.069**	3.027**	3.680**	2.909**	2.826**	3.509**	2.338**	2.184**	1.336**	1.053**	1.528**	0.351
	(0.273)	(0.354)	(0.421)	(0.428)	(0.433)	(0.427)	(0.386)	(0.366)	(0.422)	(0.385)	(0.589)	(0.355)
	4/1/11	4/15/11	4/29/11	5/13/11	5/27/11	6/10/11	6/24/11	7/8/11	7/22/11	8/5/11	8/19/22	9/2/11
3% anchor	1.199**	1.228**	1.060*	0.919^{+}	1.013*	0.880^{+}	0.845^{+}	0.930^{+}	0.880^{+}	0.930*	0.801	0.876^{+}
	(0.463)	(0.466)	(0.463)	(0.473)	(0.458)	(0.470)	(0.487)	(0.496)	(0.478)	(0.474)	(0.492)	(0.504)
10% anchor	1.224*	1.380**	0.937^{+}	0.803	0.754	0.491	0.348	0.231	0.168	0.329	0.589	0.590
	(0.478)	(0.482)	(0.479)	(0.489)	(0.473)	(0.485)	(0.503)	(0.512)	(0.493)	(0.490)	(0.509)	(0.521)
20% anchor	0.927^{+}	1.150*	1.008*	0.898^{+}	0.916^{+}	0.824^{+}	0.812	0.845	0.951^{+}	0.946^{+}	0.828	0.692
	(0.490)	(0.495)	(0.491)	(0.501)	(0.485)	(0.498)	(0.518)	(0.526)	(0.507)	(0.503)	(0.522)	(0.535)
No 20%	-2.509**	-2.613**	-2.191**	-1.953*	-2.710**	-2.907**	-3.149**	-3.221**	-2.880**	-3.238**	-3.975**	-4.881**
assignment	(0.745)	(0.750)	(0.746)	(0.760)	(0.734)	(0.752)	(0.779)	(0.793)	(0.762)	(0.758)	(0.788)	(0.805)
No 10% or	-2.726**	-2.710**	-2.862**	-3.764**	-4.586**	-4.120**	-4.532**	-4.556**	-4.594**	-5.348**	-6.052**	-5.982**
20% assign	(0.865)	(0.876)	(0.870)	(0.891)	(0.861)	(0.881)	(0.913)	(0.930)	(0.895)	(0.886)	(0.919)	(0.943)
Constant	-1.023**	-1.233**	-1.260**	-1.146**	-1.493**	-1.525**	-1.658**	-1.802**	-2.068**	-2.163**	-2.312**	-2.683**
	(0.339)	(0.342)	(0.340)	(0.347)	(0.336)	(0.345)	(0.357)	(0.363)	(0.350)	(0.348)	(0.361)	(0.370)

Panel B: Probability of contribution rate different than rate effective 10/15/2010

	10/29/10	11/12/10	11/26/10	12/10/10	12/23/10	1/7/11	1/21/11	2/4/11	2/18/11	3/4/11	Bonus	3/18/11
3% anchor	-0.033^{+}	-0.038	-0.038	-0.055*	-0.064*	-0.047	-0.044	-0.031	-0.009	-0.008	0.007	-0.005
	(0.018)	(0.022)	(0.024)	(0.024)	(0.025)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
10% anchor	-0.027	-0.025	-0.030	-0.039	-0.056*	-0.073*	-0.061*	-0.061*	-0.039	-0.040	-0.026	-0.035
	(0.019)	(0.022)	(0.024)	(0.025)	(0.026)	(0.029)	(0.029)	(0.029)	(0.028)	(0.028)	(0.029)	(0.028)
20% anchor	-0.035^{+}	-0.046*	-0.053*	-0.053*	-0.061*	-0.068*	-0.065*	-0.068*	-0.038	-0.037	-0.027	-0.036
	(0.019)	(0.023)	(0.025)	(0.026)	(0.026)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.030)	(0.029)
No 20%	0.028	0.038	0.013	0.067^{+}	0.069^{+}	0.052	0.054	0.032	0.064	0.063	0.082^{+}	0.060
assignment	(0.029)	(0.035)	(0.038)	(0.040)	(0.040)	(0.045)	(0.045)	(0.045)	(0.044)	(0.044)	(0.045)	(0.044)
No 10% or	-0.012	-0.013	-0.031	0.036	0.062	0.007	0.042	0.066	0.113*	0.112*	0.041	0.105*
20% assign	(0.034)	(0.041)	(0.044)	(0.046)	(0.047)	(0.052)	(0.052)	(0.052)	(0.051)	(0.051)	(0.054)	(0.051)
Constant	0.145**	0.218**	0.267**	0.298**	0.326**	0.521**	0.544**	0.553**	0.574**	0.574**	0.586**	0.580**
	(0.013)	(0.016)	(0.017)	(0.018)	(0.018)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)

	4/1/11	4/15/11	4/29/11	5/13/11	5/27/11	6/10/11	6/24/11	7/8/11	7/22/11	8/5/11	8/19/22	9/2/11
3% anchor	-0.010	-0.007	-0.008	-0.012	-0.008	-0.008	-0.006	-0.012	-0.018	-0.027	-0.018	-0.019
	(0.028)	(0.027)	(0.027)	(0.027)	(0.027)	(0.028)	(0.027)	(0.028)	(0.028)	(0.028)	(0.027)	(0.027)
10% anchor	-0.032	-0.044	-0.042	-0.047	-0.046	-0.046	-0.039	-0.043	-0.039	-0.035	-0.027	-0.020
	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
20% anchor	-0.040	-0.045	-0.040	-0.044	-0.047	-0.047	-0.047	-0.046	-0.042	-0.041	-0.034	-0.031
	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)	(0.029)
No 20%	0.053	0.057	0.069	0.064	0.066	0.067	0.062	0.064	0.054	0.065	0.098*	0.151**
assignment	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)
No 10% or	0.079	0.079	0.086^{+}	0.078	0.082	0.103*	0.101^{+}	0.115*	0.135**	0.178**	0.176**	0.189**
20% assign	(0.051)	(0.052)	(0.052)	(0.052)	(0.052)	(0.052)	(0.052)	(0.052)	(0.052)	(0.051)	(0.051)	(0.051)
Constant	0.596**	0.601**	0.605**	0.612**	0.616**	0.615**	0.617**	0.623**	0.627**	0.629**	0.627**	0.632**
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)

$^{+}$ Significant at the 10% level. * Significant at the 5% level. ** Significant at the 1% level.

Table 5. Effect of goal examples in 2010 emails

Within each panel, a separate regression is run for each column. The sample is employees who were not assigned to the delayed control group and were on pace to contribute between $3,000 and $5,999 in before-tax plus Roth contributions in 2010 if they left the contribution rates in effect on October 15, 2010 unchanged for the remainder of 2010. In Panel A, the dependent variable is the difference between the total (before-tax plus after-tax plus Roth) 401(k) contribution rate effective on the column's pay day and the total contribution rate effective on October 15, 2010. In Panel B, the dependent variable is a dummy for whether the total contribution rate on the column's payday differs from the total contribution rate on October 15, 2010. The columns labeled "Bonus" uses the contribution rate elections in effect for the annual bonus to construct the dependent variable. The control variables are dummies for whether the employee received the $7,000 savings goal example or the $11,000 savings goal example. Standard errors are in parentheses below the point estimates.

Panel A: Contribution rate relative to 10/15/2010 contribution rate

	10/29/10	11/12/10	11/26/10	12/10/10	12/23/10	1/7/11	1/21/11	2/4/11	2/18/11	3/4/11	Bonus	3/18/11
$7,000 goal	-0.437	0.783	0.761	0.699	0.100	0.046	-0.403	-0.194	-0.624	-0.671	-1.482	-0.654
	(0.476)	(0.656)	(0.876)	(0.953)	(0.932)	(0.775)	(0.647)	(0.604)	(0.711)	(0.711)	(1.017)	(0.680)
$11,000 goal	0.678	0.835	1.505^{+}	1.762^{+}	2.234*	1.388^{+}	0.570	1.102^{+}	0.560	0.536	0.137	0.038
	(0.477)	(0.656)	(0.878)	(0.956)	(0.935)	(0.777)	(0.649)	(0.606)	(0.712)	(0.712)	(1.021)	(0.682)
Constant	0.608^{+}	0.726	1.414*	1.736*	1.704**	1.784**	1.996**	1.709**	1.808**	1.831**	2.736**	1.935**
	(0.337)	(0.464)	(0.620)	(0.674)	(0.659)	(0.548)	(0.457)	(0.427)	(0.502)	(0.502)	(0.717)	(0.481)
	4/1/11	4/15/11	4/29/11	5/13/11	5/27/11	6/10/11	6/24/11	7/8/11	7/22/11	8/5/11	8/19/22	9/2/11
$7,000 goal	-0.135	-0.178	0.008	0.139	0.758^{+}	0.658	0.460	0.274	0.221	0.250	-0.015	0.010
	(0.592)	(0.579)	(0.612)	(0.617)	(0.308)	(0.430)	(0.361)	(0.363)	(0.364)	(0.371)	(0.392)	(0.392)
$11,000 goal	-0.358	-0.589	-0.720	-0.637	-0.002	-0.090	-0.249	-0.333	-0.393	-0.159	-0.360	-0.287
	(0.595)	(0.582)	(0.615)	(0.619)	(0.437)	(0.431)	(0.612)	(0.364)	(0.365)	(0.371)	(0.393)	(0.393)
Constant	1.500**	1.600**	1.675**	1.563**	0.738*	0.733*	0.662**	0.748**	0.780**	0.638*	0.564*	0.495^{+}
	(0.419)	(0.409)	(0.432)	(0.435)	(0.308)	(0.304)	(0.255)	(0.257)	(0.257)	(0.262)	(0.278)	(0.279)

Panel B: Probability of contribution rate different than rate effective 10/15/2010

	10/29/10	11/12/10	11/26/10	12/10/10	12/23/10	1/7/11	1/21/11	2/4/11	2/18/11	3/4/11	Bonus	3/18/11
$7,000 goal	0.008	0.023	0.033	0.024	0.012	0.090*	0.062	0.068	0.078+	0.078+	0.077+	0.055
	(0.020)	(0.025)	(0.029)	(0.031)	(0.033)	(0.043)	(0.044)	(0.044)	(0.044)	(0.044)	(0.045)	(0.044)
$11,000 goal	0.015	0.019	0.035	0.041	0.057+	0.059	0.034	0.034	0.035	0.031	0.037	0.022
	(0.020)	(0.025)	(0.030)	(0.032)	(0.033)	(0.043)	(0.044)	(0.044)	(0.044)	(0.044)	(0.045)	(0.044)
Constant	0.046**	0.076**	0.107**	0.130**	0.142**	0.347**	0.403**	0.415**	0.469**	0.473**	0.488**	0.492**
	(0.014)	(0.018)	(0.021)	(0.022)	(0.023)	(0.030)	(0.031)	(0.031)	(0.031)	(0.031)	(0.032)	(0.031)
	4/1/11	4/15/11	4/29/11	5/13/11	5/27/11	6/10/11	6/24/11	7/8/11	7/22/11	8/5/11	8/19/22	9/2/11
$7,000 goal	0.047	0.038	0.044	0.050	0.046	0.052	0.049	0.039	0.035	0.045	0.038	0.048
	(0.044)	(0.044)	(0.044)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)
$11,000 goal	0.022	0.024	0.035	0.041	0.033	0.041	0.037	0.029	0.033	0.037	0.029	0.024
	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)	(0.045)
Constant	0.498**	0.504**	0.508**	0.502**	0.510**	0.508**	0.516**	0.528**	0.534**	0.529**	0.539**	0.540**
	(0.031)	(0.031)	(0.031)	(0.031)	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)

+ Significant at the 10% level. * Significant at the 5% level. ** Significant at the 1% level.

Table 6. Effect of highlighting $3,000 and $16,500 thresholds in 2010 emails

Within each panel, a separate regression is run for each column. The sample is employees who were on pace to contribute less than $3,000 in before-tax plus Roth contributions in 2010 if they left the contribution rates in effect on October 15, 2010 unchanged for the remainder of 2010. In Panel A, the dependent variable is the difference between the total (before-tax plus after-tax plus Roth) 401(k) contribution rate effective on the column's pay day and the total contribution rate effective on October 15, 2010. In Panel B, the dependent variable is a dummy for whether the total contribution rate on the column's payday differs from the total contribution rate on October 15, 2010. The columns labeled "Bonus" uses the contribution rate elections in effect for the annual bonus to construct the dependent variable. The control variable is a dummy for whether the employee received the $16,500 contribution threshold treatment. Standard errors are in parentheses below the point estimates.

Panel A: Contribution rate relative to 10/15/2010 contribution rate												
	10/29/10	11/12/10	11/26/10	12/10/10	12/23/10	1/7/11	1/21/11	2/4/11	2/18/11	3/4/11	Bonus	3/18/11
$16,500 threshold	0.218	0.231	0.106	0.223	0.031	0.312	1.440+	1.070	1.536*	1.041+	0.690	0.841
	(0.686)	(1.005)	(1.199)	(1.297)	(1.274)	(1.052)	(0.793)	(0.698)	(0.755)	(0.600)	(0.752)	(0.545)
Constant	1.066*	2.597**	3.658**	4.209**	4.280**	3.284**	1.665**	1.815**	1.480**	1.386**	1.511**	1.330**
	(0.484)	(0.707)	(0.843)	(0.912)	(0.895)	(0.738)	(0.557)	(0.490)	(0.527)	(0.420)	(0.524)	(0.382)
	4/1/11	4/15/11	4/29/11	5/13/11	5/27/11	6/10/11	6/24/11	7/8/11	7/22/11	8/5/11	8/19/22	9/2/11
$16,500 threshold	1.350+	1.374+	1.184+	1.122+	0.499	0.501	0.447	0.257	-0.002	-0.357	0.104	0.212
	(0.702)	(0.710)	(0.669)	(0.669)	(0.441)	(0.451)	(0.453)	(0.472)	(0.643)	(0.617)	(0.435)	(0.440)
Constant	1.270**	1.284*	1.201*	1.220**	1.010**	1.079**	1.119**	1.302**	1.614**	1.762**	1.185**	0.990**
	(0.493)	(0.498)	(0.469)	(0.469)	(0.309)	(0.317)	(0.319)	(0.330)	(0.450)	(0.433)	(0.306)	(0.310)
Panel B: Probability of contribution rate different than rate effective 10/15/2010												
	10/29/10	11/12/10	11/26/10	12/10/10	12/23/10	1/7/11	1/21/11	2/4/11	2/18/11	3/4/11	Bonus	3/18/11
$16,500 threshold	-0.004	-0.005	-0.036	-0.053	-0.052	-0.060	-0.048	-0.066	-0.024	-0.021	-0.024	-0.025
	(0.028)	(0.035)	(0.038)	(0.040)	(0.040)	(0.046)	(0.046)	(0.047)	(0.048)	(0.048)	(0.048)	(0.048)
Constant	0.102**	0.164**	0.222**	0.258**	0.265**	0.404**	0.411**	0.438**	0.453**	0.450**	0.433**	0.468**
	(0.020)	(0.025)	(0.027)	(0.028)	(0.029)	(0.032)	(0.033)	(0.033)	(0.033)	(0.033)	(0.034)	(0.034)
	4/1/11	4/15/11	4/29/11	5/13/11	5/27/11	6/10/11	6/24/11	7/8/11	7/22/11	8/5/11	8/19/22	9/2/11
$16,500 threshold	-0.033	-0.022	-0.011	-0.020	-0.031	-0.017	-0.012	-0.009	-0.005	0.012	0.005	0.005
	(0.048)	(0.048)	(0.048)	(0.048)	(0.048)	(0.049)	(0.049)	(0.049)	(0.049)	(0.049)	(0.049)	(0.049)
Constant	0.486**	0.477**	0.472**	0.477**	0.493**	0.493**	0.502**	0.512**	0.507**	0.509**	0.512**	0.517**
	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)	(0.034)

+ Significant at the 10% level. * Significant at the 5% level. ** Significant at the 1% level.

Table 7. Effect of 60% contribution rate threshold treatment in 2009 emails on average contribution rate change

Each panel contains a different sample of employees, divided according to how much they would contribute on a before-tax plus Roth basis to the 401(k) in 2009 if they left the contribution rates in effect on November 13, 2009 unchanged for the remainder of 2009. We exclude employees assigned to an anchor condition. Within each panel, a separate regression is run for each column. The dependent variable is the difference between the total (before-tax plus after-tax plus Roth) 401(k) contribution rate effective on the column's payday and the total contribution rate effective on November 13, 2009. The control variable is a dummy for whether the employee received the 60% contribution rate threshold treatment. Standard errors are in parentheses below the point estimates.

	11/27/09	12/11/09	12/24/09	1/8/10	1/22/10	2/5/10	2/19/10	3/5/10
Panel A: $0 - $2,499 projected 2009 contributions								
60% threshold	2.484**	2.749**	2.867*	2.536*	0.563	0.812	0.648	1.353
	(0.877)	(1.010)	(1.160)	(1.058)	(0.811)	(0.728)	(0.731)	(0.946)
Constant	1.004	1.763*	2.512**	2.424**	1.437*	0.680	0.779	0.631
	(0.617)	(0.710)	(0.816)	(0.743)	(0.569)	(0.511)	(0.513)	(0.665)
Panel B: $2,500 - $4,999 projected 2009 contributions								
60% threshold	-0.084	-0.288	-0.093	0.252	0.565	0.186	0.237	0.696
	(0.408)	(0.511)	(0.524)	(0.445)	(0.383)	(0.355)	(0.344)	(0.578)
Constant	1.363**	2.278**	2.295**	2.289**	1.805**	1.934**	1.814**	2.344**
	(0.288)	(0.361)	(0.371)	(0.316)	(0.271)	(0.251)	(0.244)	(0.409)
Panel C: $5,000 - $16,499 projected 2009 contributions								
60% threshold	-0.094	0.312	0.273	-0.475	-0.276	-0.402	-0.304	-0.723
	(0.460)	(0.551)	(0.588)	(0.552)	(0.485)	(0.466)	(0.458)	(0.583)
Constant	2.925**	3.885**	3.194**	4.390**	2.689**	2.251**	1.847**	1.878**
	(0.325)	(0.390)	(0.417)	(0.391)	(0.344)	(0.330)	(0.324)	(0.413)

[+] Significant at the 10% level. * Significant at the 5% level. ** Significant at the 1% level.

Table 8. Effect of 60% contribution rate threshold treatment in 2009 emails on probability of a contribution rate increase

Each panel contains a different sample of employees, divided according to how many dollars they would contribute on a before-tax plus Roth basis to the 401(k) in 2009 if they left the contribution rates in effect on November 13, 2009 unchanged for the remainder of 2009. We exclude employees assigned to an anchor condition. Within each panel, a separate regression is run for each column. The dependent variable is a dummy for whether the total contribution rate on the column's payday is higher than the total contribution rate on November 13, 2009. The control variable is a dummy for whether the employee received the 60% contribution rate threshold treatment. Standard errors are in parentheses below the point estimates.

Panel A: $0 - $2,499 projected 2009 contributions								
	11/27/09	12/11/09	12/24/09	1/8/10	1/22/10	2/5/10	2/19/10	3/5/10
60% threshold	0.057*	0.066*	0.071*	0.115**	0.123**	0.123**	0.119**	0.135**
	(0.027)	(0.033)	(0.036)	(0.044)	(0.044)	(0.044)	(0.044)	(0.044)
Constant	0.078**	0.128**	0.164**	0.490**	0.476**	0.470**	0.470**	0.377**
	(0.019)	(0.022)	(0.025)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)
Panel B: $2,500 - $4,999 projected 2009 contributions								
	11/27/09	12/11/09	12/24/09	1/8/10	1/22/10	2/5/10	2/19/10	3/5/10
60% threshold	-0.003	-0.009	-0.009	-0.032	-0.033	-0.034	-0.044	-0.006
	(0.015)	(0.019)	(0.020)	(0.026)	(0.026)	(0.026)	(0.026)	(0.027)
Constant	0.081**	0.135**	0.156**	0.688**	0.686**	0.693**	0.697**	0.621**
	(0.011)	(0.013)	(0.014)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
Panel C: $5,000 - $16,499 projected 2009 contributions								
	11/27/09	12/11/09	12/24/09	1/8/10	1/22/10	2/5/10	2/19/10	3/5/10
60% threshold	0.001	0.012	0.007	0.020	0.012	0.011	0.004	-0.010
	(0.016)	(0.019)	(0.019)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
Constant	0.150**	0.215**	0.217**	0.556**	0.543**	0.533**	0.530**	0.508**
	(0.011)	(0.013)	(0.013)	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)

[+] Significant at the 10% level. * Significant at the 5% level. ** Significant at the 1% level.

Table 9. Interaction of pre-email contribution rate with 60% contribution rate threshold treatment effect on subsequent contribution rate change

Each panel contains a different sample of employees, divided according to how many dollars they would contribute to the 401(k) in 2009 if they left the contribution rates in effect on November 13, 2009 unchanged for the remainder of 2009. We exclude employees assigned to an anchor condition. Within each panel, a separate regression is run for each column. The dependent variable is the difference between the total (before-tax plus after-tax plus Roth) 401(k) contribution rate effective on the column's payday and the total contribution rate effective on November 13, 2009. The control variables are dummies for whether the employee received the 60% contribution rate threshold treatment and whether her total contribution rate on November 13, 2009 was 0% or 1%, and the interaction of these two dummies. Standard errors are in parentheses below the point estimates.

Panel A: $0 - $2,499 projected 2009 contributions								
	11/27/09	12/11/09	12/24/09	1/8/10	1/22/10	2/5/10	2/19/10	3/5/10
60% threshold	0.956	1.400	1.715	1.523	0.127	0.420	0.312	0.821
	(1.172)	(1.344)	(1.541)	(1.408)	(1.080)	(0.972)	(0.975)	(1.267)
60% threshold ×	3.929*	3.748^{+}	3.460	3.099	1.577	1.390	1.300	1.843
0-1% rate	(1.747)	(2.003)	(2.297)	(2.106)	(1.615)	(1.451)	(1.455)	(1.888)
0-1% rate	0.911	2.135	3.275*	2.599^{+}	2.321*	1.977^{+}	2.193*	2.431^{+}
	(1.219)	(1.397)	(1.603)	(1.464)	(1.124)	(1.010)	(1.012)	(1.315)
Constant	0.554	0.708	0.900	1.140	0.295	-0.297	-0.305	-0.575
	(0.857)	(0.982)	(1.125)	(1.029)	(0.788)	(0.710)	(0.712)	(0.926)
Panel B: $2,500 - $4,999 projected 2009 contributions								
	11/27/09	12/11/09	12/24/09	1/8/10	1/22/10	2/5/10	2/19/10	3/5/10
60% threshold	-0.275	-0.568	-0.371	-0.178	0.151	-0.163	-0.046	0.354
	(0.415)	(0.520)	(0.533)	(0.450)	(0.384)	(0.357)	(0.348)	(0.587)
60% threshold ×	3.519^{+}	5.656*	5.615*	9.083**	8.320**	7.032**	5.665**	6.840*
0-1% rate	(2.099)	(2.630)	(2.694)	(2.263)	(1.931)	(1.819)	(1.770)	(2.988)
0-1% rate	1.052	-0.568	0.089	-1.234	0.005	-0.087	0.037	0.057
	(1.626)	(0.520)	(2.086)	(1.752)	(1.495)	(1.422)	(1.384)	(2.335)
Constant	1.329**	2.275**	2.292**	2.330**	1.805**	1.937**	1.813**	2.343**
	(0.292)	(0.366)	(0.376)	(0.317)	(0.271)	(0.252)	(0.245)	(0.414)

Panel C: $5,000 - $16,499 projected 2009 contributions								
	11/27/09	12/11/09	12/24/09	1/8/10	1/22/10	2/5/10	2/19/10	3/5/10
60% threshold	-0.191	0.264	0.185	-0.408	-0.253	-0.330	-0.137	-0.491
	(0.462)	(0.558)	(0.596)	(0.556)	(0.480)	(0.458)	(0.447)	(0.576)
60% threshold ×	6.446*	3.734	5.045	0.881	4.270	3.738	0.750	-1.155
0-1% rate	(2.765)	(3.333)	(3.554)	(3.315)	(2.856)	(2.756)	(2.690)	(3.468)
0-1% rate	5.679**	4.046^{+}	3.270	8.640**	-0.253	14.085**	16.452**	18.308**
	(1.784)	(2.150)	(2.293)	(2.139)	(0.480)	(3.738)	(1.715)	(2.211)
Constant	2.733**	3.748**	3.083**	4.096**	2.271**	1.768**	1.283**	1.251**
	(0.328)	(0.396)	(0.423)	(0.395)	(0.341)	(0.325)	(0.317)	(0.409)

$^{+}$ Significant at the 10% level. * Significant at the 5% level. ** Significant at the 1% level.

Figure 1. 2009 email text

Dear [Employee],

We want to remind you that [Company] matches your qualified contributions (pre-tax and Roth) to the [Company] 401(k) Plan. In other words, [Company] will give you free money for saving in your 401(k).

What is the [Company] match?
[Company]'s matching contribution is the greater of: (a) 100% of your qualified 2009 401(k) contributions up to $2,500; or (b) 50% of your qualified 2009 contributions up to $16,500 for a total possible match of $8,250.*

Where am I at right now?
You've made $X,XXX in qualified payroll contributions to the [Company] 401(k) Plan as of November 1, 2009.

To take greater advantage of [Company]'s 2009 match, increase your contribution rate for the remaining six weeks of 2009. **Treatment text was inserted here.**

See this calendar for deadlines for making contribution changes. **

How do I increase my contribution?
To change your contribution rate, follow these steps:
1. Log in to Vanguard, our 401(k) vendor. (If you've never logged in before, you will need the [Company] Plan number, [######].)

2. Click on "Change paycheck deductions" under the "I want to. . ." menu

3. Adjust your percentages in the boxes.

4. Click "continue" and follow directions until you see the confirmation page. A confirmation will also be emailed or mailed to you.

Happy saving!
- [Director of Benefits]

** Must be employed at last day of the plan year in order to receive the maximum match. See URL for more details.*

*** The actual amount you can contribute is subject to other IRS limits. See Plan Specific Limitations for details.*

Figure 2. Average total contribution rate among November 2009 control email recipients

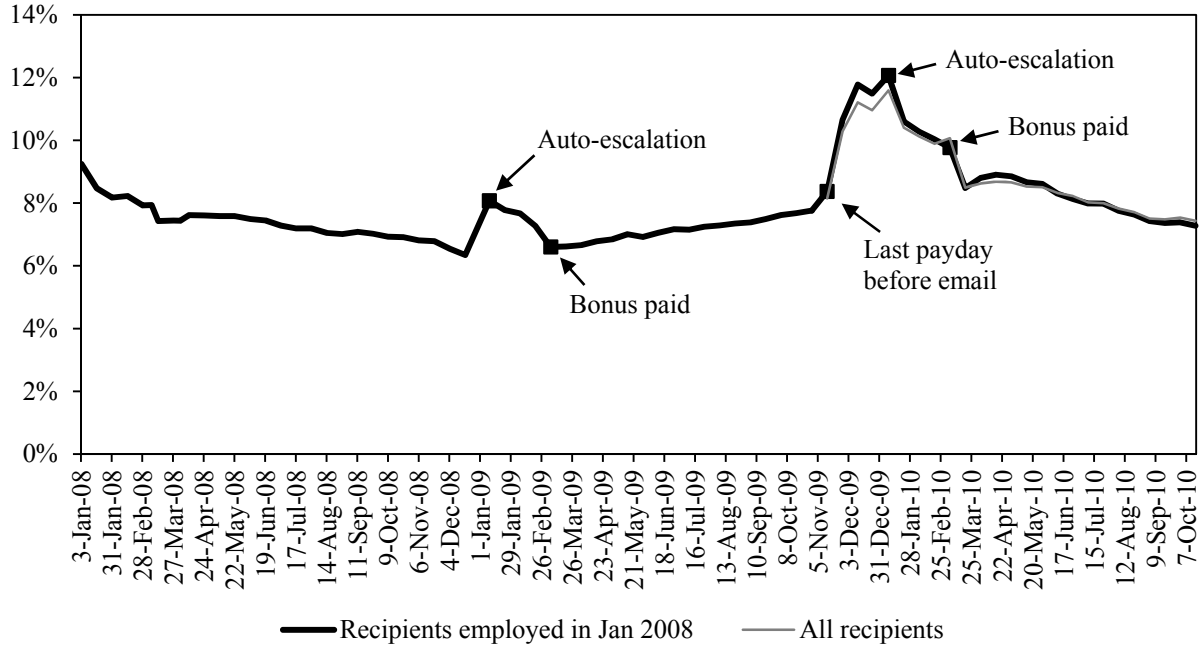


Figure 3. Average total contribution rate in excess of October 15, 2010 total contribution rate, email recipients on pace to contribute $3,000 or more in 2010

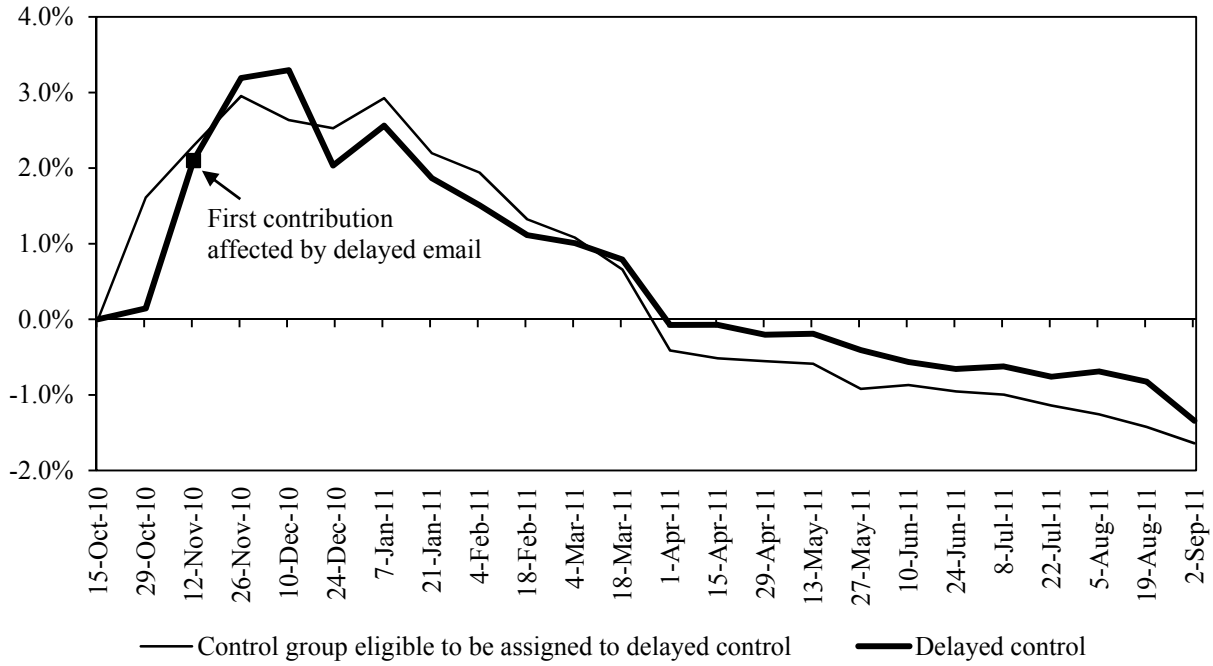


Figure 4. Average total contribution rate in excess of November 13, 2009 total contribution rate, email recipients projected to contribute $5,000 to $16,499 in 2009

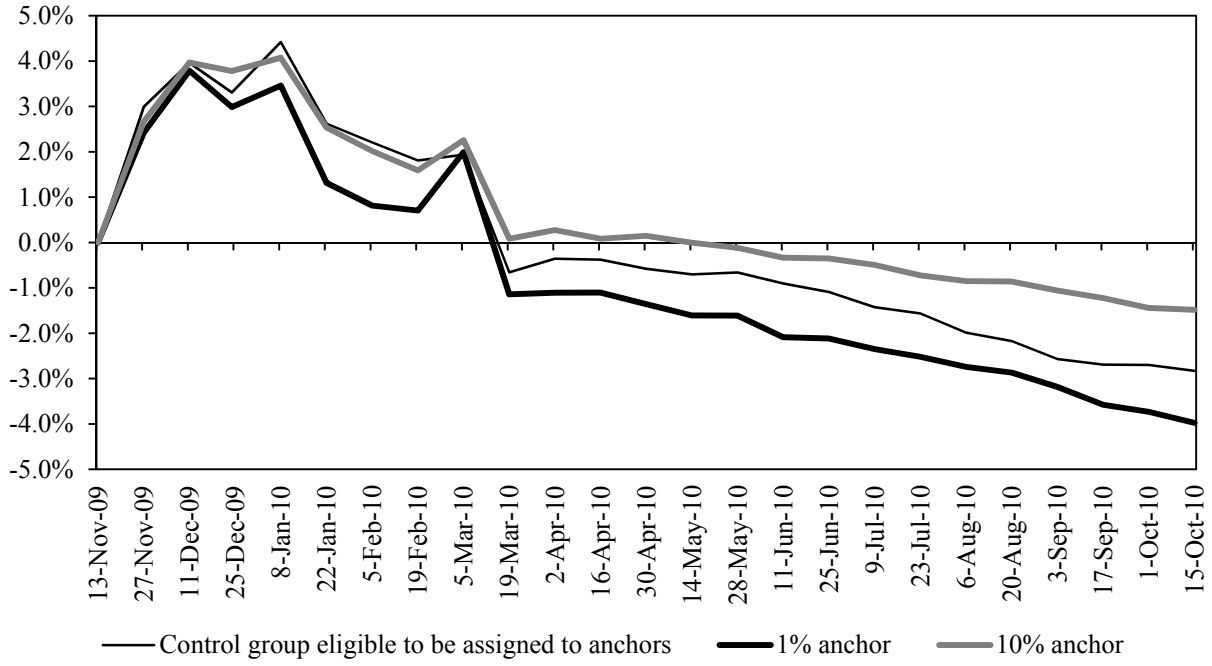


Figure 5. Average total contribution rate in excess of October 15, 2010 total contribution rate, email recipients projected to contribute $6,000 to $16,499 in 2010

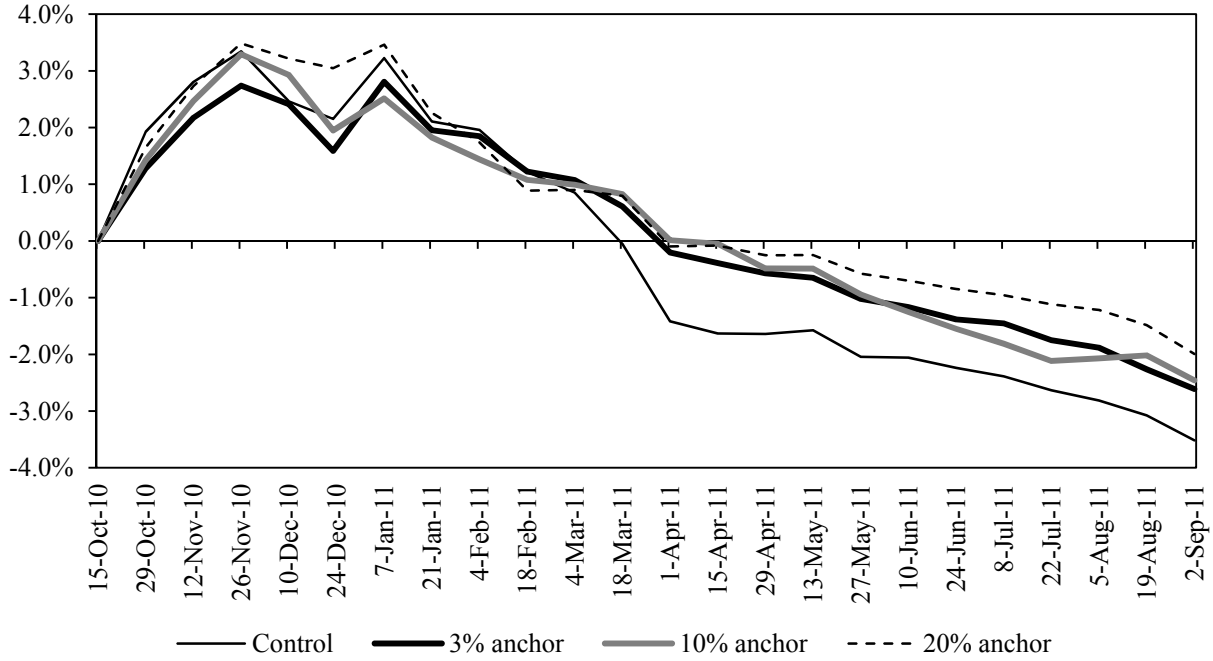


Figure 6A. Average total contribution rate in excess of October 15, 2010 total contribution rate, email recipients projected to contribute $3,000 to $5,999 in 2010

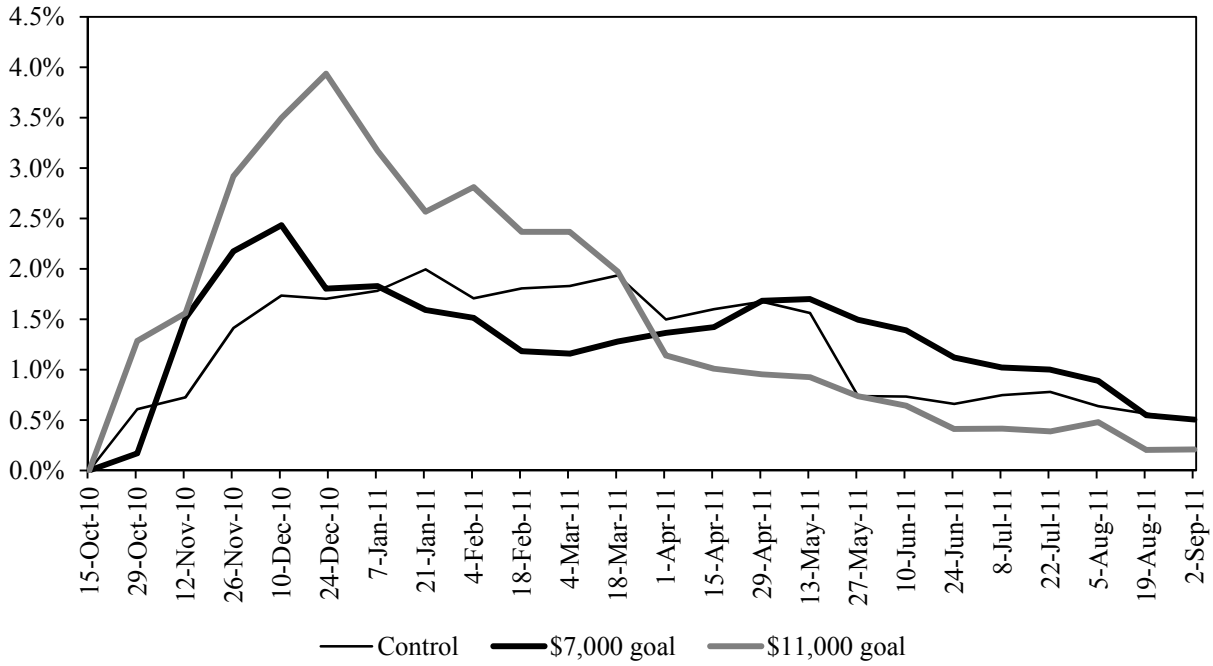


Figure 6B. Adjusted average total contribution rate in excess of October 15, 2010 total contribution rate, email recipients projected to contribute $3,000 to $5,999 in 2010

Any contiguous sequence of 0% contribution rates that begins after January 7, 2011 and ends on September 2, 2011 is replaced by the last positive contribution rate in 2011 observed for the employee.

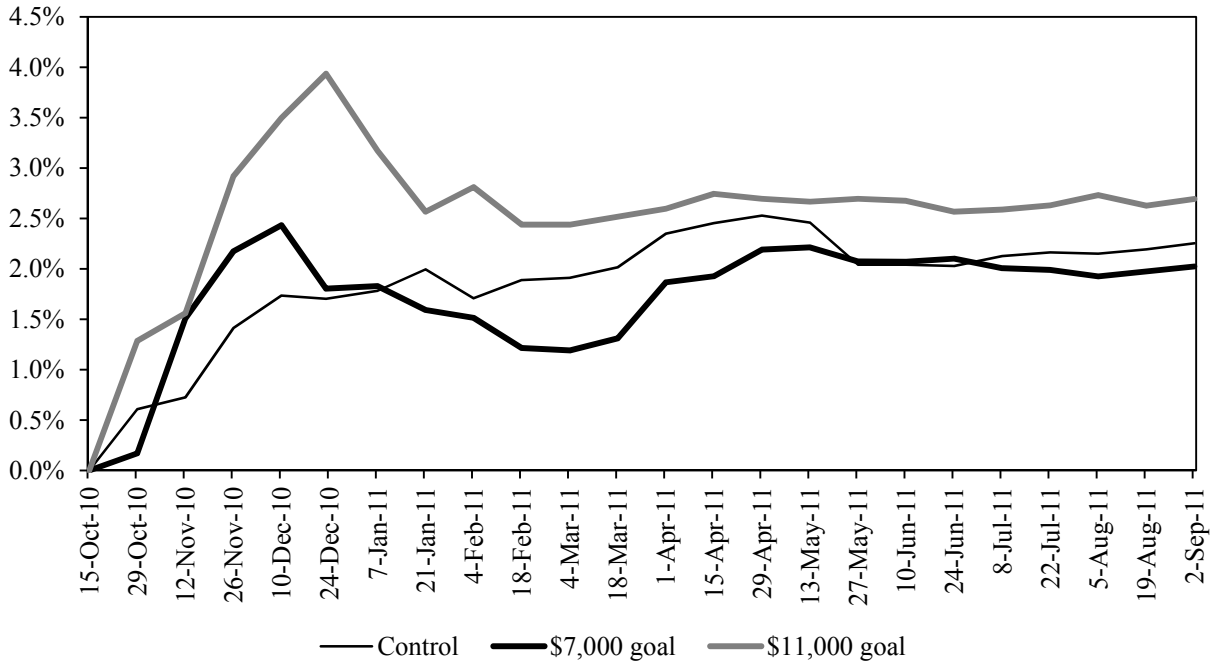


Figure 7. Histogram of total before-tax plus Roth 2010 contributions, email recipients projected to contribute less than $3,000 in 2010

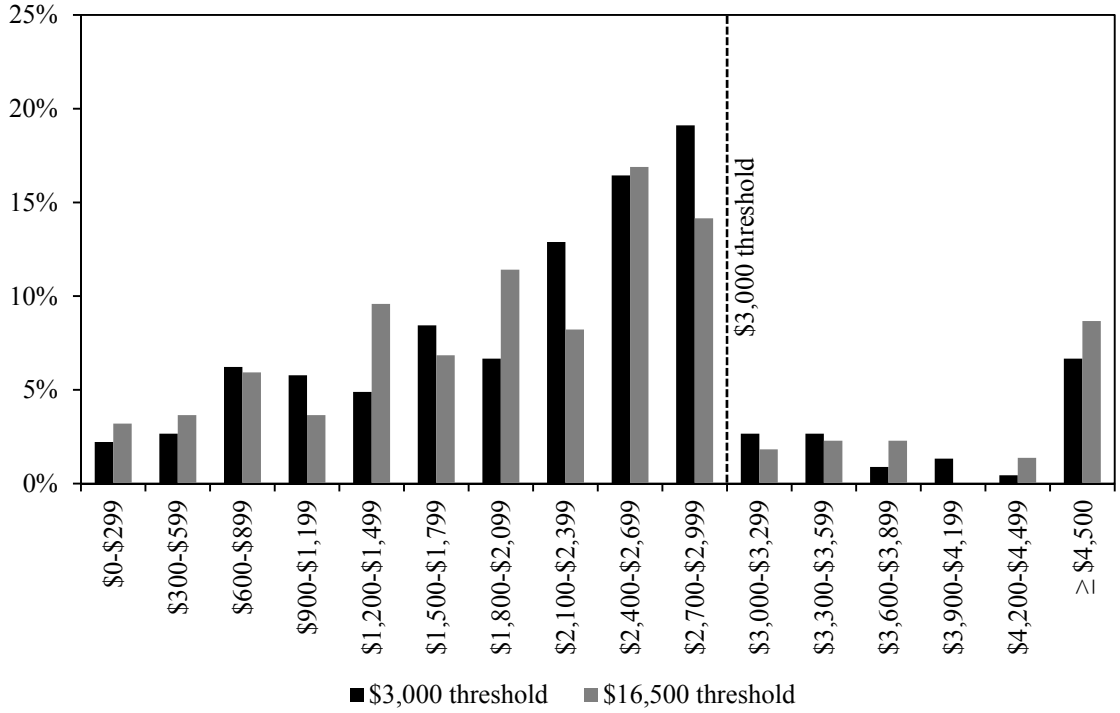


Figure 8. Average total contribution rate in excess of October 15, 2010 total contribution rate, email recipients projected to contribute less than $3,000 in 2010

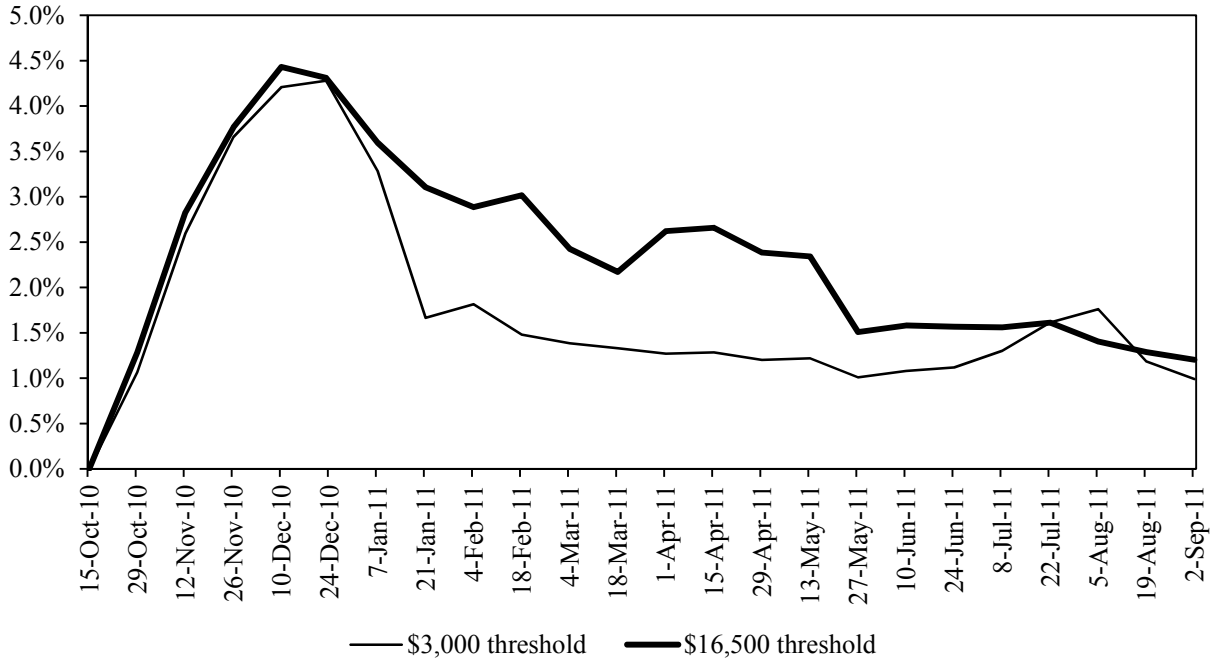
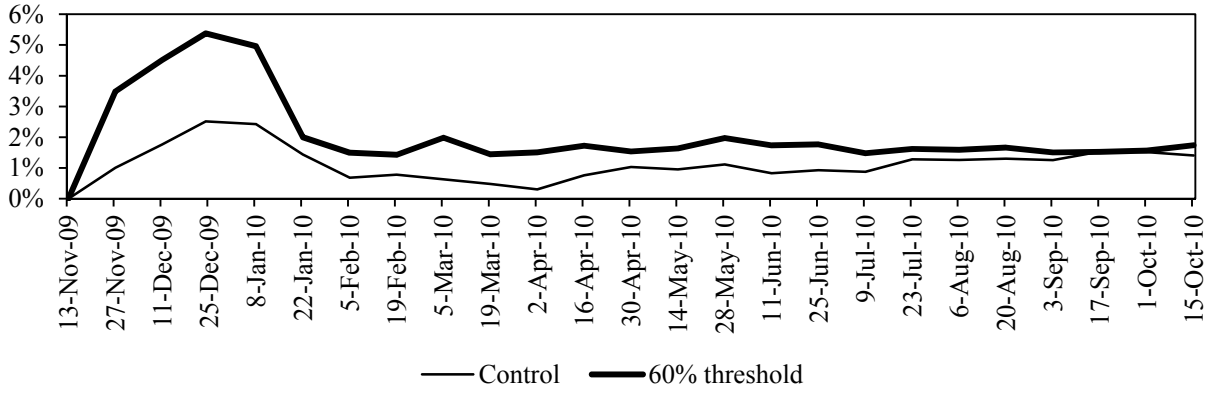
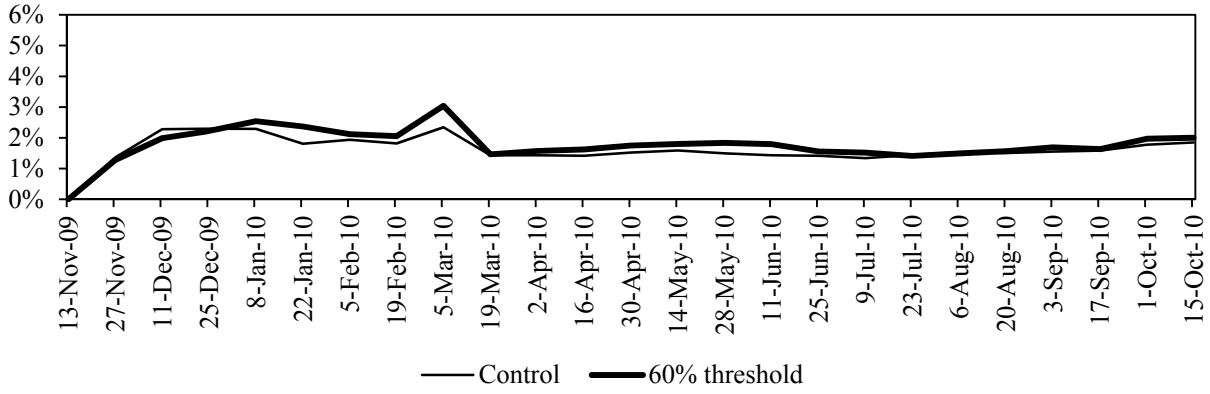


Figure 9. Average total contribution rate in excess of November 13, 2009 total contribution rate

Projected 2009 contributions: $0 - $2,499



Projected 2009 contributions: $2,500 - $4,999



Projected 2009 contributions: $5,000 - $16,499