

Why Small Firms Fail to Adopt Profitable Opportunities*

Paul Gertler

Sean Higgins

Ulrike Malmendier

Waldo Ojeda

August 26, 2024

Abstract

Firms frequently fail to adopt new profitable opportunities even when frictions such as informational or liquidity constraints are removed. We explore three leading behavioral frictions that have been shown to explain inertia among individuals—present bias, limited memory, and lack of trust—and ask whether they help explain profit-reducing managerial behavior. In partnership with a financial technology (FinTech) payments company in Mexico, we randomly offer 33,978 firms the opportunity to switch to a contract under which they are charged a lower merchant fee. We randomly vary whether there is a deadline, reminder, and advance notice of the reminder, as well as the size of the fee reduction. The deadline does not affect take-up for the larger firms in our sample, which implies limited or no present bias for these firms, but does increase take-up by 8% for smaller firms. Reminders increase take-up by 15%, suggesting a role of memory, and announced reminders by an additional 7%. Survey data reveal trust as the likely mechanism underlying the larger effect of the announced reminder: following through with the pre-announced reminder increases firms' trust and perception of the offer's value.

*Gertler: UC Berkeley, Haas School of Business; gertler@berkeley.edu. Higgins: Northwestern University, Kellogg School of Management; sean.higgins@kellogg.northwestern.edu. Malmendier: UC Berkeley, Department of Economics and Haas School of Business; ulrike@berkeley.edu. Ojeda: Columbia University, Department of Economics; wo2198@columbia.edu. We are very grateful to our discussants Manuel Adelino, Tomomichi Amano, Jie Bai, Milo Bianchi, Miriam Bruhn, Michael Ewens, Camille Hebert, Rawley Heimer, Kai Li, Luca Lin, Song Ma, Michaela Pagel, Ryan Pratt, and Melanie Wallskog for discussing our paper. We thank conference and seminar participants at ABFER, Annual Conference on Alternative Finance, ASSA, Bank of Canada Economics of Payments Conference, Baruch College, Behavioral Industrial Organization & Marketing Symposium, Ben Gurion University, CEPR ES Conference on Financial Intermediation and Corporate Finance, Development Day at Notre Dame, Duke University, EFA, European Winter Finance Summit, Golub Capital Social Impact Lab, Harvard Business School, HBS Early Career Behavioral Economics Conference, IMF Research, IPA-GPRL Researcher Gathering, IPA SME Program, IPCDE, Jackson Hole Finance Conference, Korea Advanced Institute of Science and Technology and Korea University, Lab for Inclusive FinTech at UC Berkeley, Monash University, MWIEDC, NBER Corporate Finance Fall Meeting, NBER Organizational Economics Spring Meeting, NCDE, NFA, Northwestern University, Peking University, Queensland Corporate Finance Conference, Red Rock Finance Conference, Research in Behavioral Finance Conference, SFS Cavalcade, Toulouse School of Economics, Tulane University, University of Arizona, University of Cape Town, University of Chicago and UCEMA Joint Initiative for Latin American Experimental Economics, University of Chicago Development Faculty Meetup, University of Edinburgh Economics of Financial Technology Conference, University of Maryland, University of Melbourne, University of Sydney, University of Washington, Webinar on Entrepreneurial Finance and Innovation, and Webinar on Finance and Development for helpful comments and discussions. We thank Noah Forougi, Mohammad Atif Haidry, Miguel Angel Jimenez, César Landín, Nicolas Min, Alexandra Wall, Honghao Wang, and Tiange Ye for excellent research assistance. We are grateful for funding from the CEGA-Visa Financial Inclusion Lab and the Lab for Inclusive FinTech (LIFT) at UC Berkeley. The authors declare that they have no financial or material interests in the results of this research. IRB approvals: UC Berkeley IRB 2018-02-10796 and 2020-03-13091. AEA RCT Registry: AEARCTR-0006540.

1 Introduction

Firms often fail to adopt profitable business opportunities. Examples include cost-saving and financial technologies, management practices, and optimal pricing. The failure to take advantage of such business opportunities has been observed across many industries, including manufacturing, banking, retail, and healthcare.¹ Firms of all sizes forego substantial profits due to their failure to adopt such technologies and practices.²

Several economic frictions contribute to firms' failure to adopt profitable opportunities, including lack of information and of managerial capital, fixed costs in the presence of uncertainty and of liquidity constraints, labor constraints, and principal-agent problems.³ However, even when these standard economic frictions are removed, firms frequently still fail or are slow to adopt profitable opportunities. For example, Bloom et al. (2013) find that "even if the owners became convinced of the need to adopt a practice, they would often take several months to do so," and DellaVigna and Gentzkow (2019) and Mishra, Prabhala, and Rajan (2022) document "managerial inertia" or "stickiness in organizational structures and practices."

Why do firms exhibit such inertia and fail to take advantage of new opportunities even though these behaviors reduce their profits? In partnership with a financial technology (FinTech) company, we design a randomized controlled trial (RCT) that allows us to assess the role of three frictions that have been shown to explain inertia and lack of behavioral change in *non-managerial* settings: present bias, limited memory, and lack of trust. Present bias has been shown to affect health-related choices such as gym attendance and smoking cessation (DellaVigna and Malmendier, 2006; Giné, Karlan, and Zinman, 2010) and financial choices such as saving, borrowing, and loan repayment (Laibson, 1997; DellaVigna and Malmendier, 2004; Ashraf, Karlan, and Yin, 2006; Kuchler and Pagel, 2021). Limited memory also hampers health-related behavior, such as adherence to health-care appointments and vaccine take-up (Gurol-Urganci et al., 2013; Dai et al., 2021; Calzolari and Nardotto, 2017), as well as financial choices, such as saving and loan repayment behavior (Kar-

¹ On the failure to adopt cost-savings technologies in manufacturing, see Atkin et al. (2017). On management practices, see Bloom et al. (2013) and Giorcelli (2019) for manufacturing firms and Bruhn, Karlan, and Schoar (2018) for manufacturing, commerce, and services firms. On financial technology adoption, see Mishra, Prabhala, and Rajan (2022) for banks and Higgins (forthcoming) for retail firms. On adoption of improved care practices by healthcare firms, see Celhay, Gertler, Giovagnoli, and Vermeersch (2019). On non-optimal pricing by large retail firms, see DellaVigna and Gentzkow (2019) and Strulov-Shlain (2022).

² Microenterprises in Banerjee et al. (2023) forgo a 60% increase in profits on average; small and medium enterprises in Bruhn, Karlan, and Schoar (2018) forgo a 28% increase in productivity; medium and large firms in Bloom et al. (2013) forgo a 17% increase in productivity; large retail chains in DellaVigna and Gentzkow (2019) forgo 12% of profits; and Lyft forgoes \$160M of annual profits in List, Muir, Pope, and Sun (2023).

³ On the role of information and managerial capital, see Bloom et al. (2013), Bruhn, Karlan, and Schoar (2018), and Giorcelli (2019); on fixed costs in the presence of uncertainty and liquidity constraints, see Abel and Eberly (1994) and Banerjee, Breza, Duflo, and Kinnan (2021); on labor constraints Jagannathan, Matsa, Meier, and Tarhan (2016) and Hardy and McCasland (2023); and on principal-agent problems within firms Atkin et al. (2017) and Rigol and Roth (2021).

lan, McConnell, Mullainathan, and Zinman, 2016; Karlan, Morten, and Zinman, 2016). Finally, distrust has been shown to interfere with the same financial decisions, including saving, borrowing, and refinancing (Karlan, Mobius, Rosenblat, and Szeidl, 2009; Johnson, Meier, and Toubia, 2019; Bachas, Gertler, Higgins, and Seira, 2021). In this paper, we ask whether these determinants also explain profit-reducing *managerial* behavior and, as such, help resolve the puzzle of firms not adopting readily available business opportunities.

There are several reasons to think that these determinants may work differently for managers than for the general population. First, managers, including entrepreneurs and CEOs, differ from the general population in terms of their ability, motivation, and personality (Schoar, 2010; Kaplan and Sorensen, 2021). Entrepreneurs tend to have higher cognitive ability (de Mel, McKenzie, and Woodruff, 2010) as well as higher confidence, ambition, and expectations for their own business compared to others (Ardagna and Lusardi, 2010).⁴ Second, firms can implement management and information systems that mitigate the effects of individual biases. Firms also have partners and employees who might help reduce the effects of a manager’s biases. Finally, stakes might be higher and the adverse effects of inertia more immediate in businesses than in personal lives.

We test whether, nonetheless, these individual-level frictions affect managerial behavior and help explain managerial inertia. In our RCT, a FinTech payments provider in Mexico offered 33,978 firms that were already active users of their payments technology the opportunity to be charged a lower merchant fee for each payment they receive from customers. By adopting the new contract these firms would reduce their costs and hence increase their profits. For the median firm, which is relatively small with three employees, the expected cost savings from the reduced fee equal 3% of profits.⁵

In order to examine the effect of these three behavioral frictions, we designed the RCT to randomly vary (i) the amount of the lower fee (2.75%, 3%, or a control group that retained their current fee of 3.5–3.75%), (ii) a deadline to accept the offer, (iii) a reminder, and (iv) whether the FinTech company told the firms in advance that they would receive a reminder of the upcoming deadline (“announced reminder”). An initial message was sent to all firms receiving the offer on the first day of the experiment. The deadline was on the eighth day, and we sent reminders on the morning of the seventh day. Firms that received an announced reminder were told in the initial message on which day they would receive such a reminder.

To show how the design allows us to test for the three proposed mechanisms—present bias, limited memory, and lack of trust in other firms—we build on the model from Ericson (2017),

⁴ Relatedly, strong intra- and interpersonal skills are predictors of entrepreneurial and managerial success (Lucas, 1978; Murphy, Shleifer, and Vishny, 1991; Baumol, 1996; Gennaioli, La Porta, Lopez-de-Silanes, and Shleifer, 2013; Levine and Rubinstein, 2017).

⁵ In the subsample of firms we surveyed, the largest firm has 150 employees. The distribution of number of employees of the firms in our RCT is similar to that of 99.7% of firms in Mexico.

which studies how present bias and limited memory affect task completion. We augment the model to include the notion of trust, and apply it to managerial decision-making inside a firm, where the manager’s objective is to maximize the net present value of firm profits.⁶ The model illustrates that present bias can lead to lower adoption of a profitable opportunity because the costs to adopt are borne immediately and the benefit is in the future. Naïve present-biased agents procrastinate thinking they will adopt tomorrow, but when tomorrow arrives their present bias causes them to procrastinate again. Deadlines can help overcome present bias because, at the deadline, the manager cannot delay any longer. In our model, we show that the more present-biased a manager is, the larger the treatment effect of a deadline will be.

Managers may also have limited memory and forget about the profitable opportunity, which reminders can help overcome. Announced reminders—when managers are told in advance that they will receive a reminder—can increase their expectations about remembering the offer in the future and thus decrease initial take-up among managers who are aware that they have limited memory.

Variation in trust generates a different effect of the announced reminder: if firms lack trust in the validity of the offer, receiving a pre-announced reminder could increase trust in the FinTech firm as the firm followed through with its promise, and this can increase the perceived value of the offer. Hence, the model illustrates that cumulative take-up by firms that received an announced reminder can be higher than cumulative take-up by firms that received an unannounced reminder if the announced reminder increases the perceived value of the offer, but not otherwise.

Our RCT allows us to test these theoretical predictions.

We find that the deadline has no effect among larger firms, but does increase cumulative take-up (until the deadline) by 8.4% among smaller firms. The differential effect suggests that either decision-makers in smaller firms exhibit more present bias, or that other differences between smaller and larger firms lead the deadline to only have an effect for smaller firms. Differences in present bias across the firm size distribution might in turn reflect smaller firms being run by more present-biased individuals due to selection, or because firm growth is a function of present bias. Other differences could include smaller firms lacking organizational structures designed to mitigate the effects of the manager’s present bias.⁷ Exploiting randomized variation in the value of the offer (size of the fee reduction), we also find that the deadline has no effect for the more-valuable offer, but does have an effect for the less-valuable offer. The latter finding is in line with the model prediction that the deadline effect is decreasing in the value of the offer.

⁶ The objective function reflects that the recipient of the cost-saving offer in our RCT was the firm owner in 88.7% of cases. In most small firms, the firm owner is also the manager.

⁷ The latter mechanism would be consistent with larger firms having more formalized information and management systems (Daft, 2015) and using better management practices (Bloom et al., 2014; McKenzie and Woodruff, 2017).

We find that managers are forgetful: unannounced reminders cause a large and significant increase in adoption of the lower merchant fee by 15.2% (3.9 percentage points (pp) relative to a 25.5% baseline take-up). The higher overall take-up occurs almost entirely on the day we sent the reminder.

The announced-reminder group displays no difference in behavior relative to the unannounced-reminder group on the first day (when we sent the initial email). This null result—combined with the findings that reminders do have a large effect and that not all managers find it optimal to adopt immediately—suggests that managers are not only forgetful but also overconfident about memory.

On the day that we sent the reminder, however, announced reminders increased take-up by 2 pp (6.9%) more than unannounced reminders, and the difference in take-up persists throughout the remainder of the experiment, several months after the deadline.⁸ This result cannot be explained by a model where announced reminders only impact the probability (or perceived probability) of remembering. Instead, to cause higher take-up, the announced reminder must increase the perceived value of accepting the offer, for example by increasing trust in the offer.

To better understand the announced-reminder effect on the day of the reminder, we conduct a survey of a subsample of firms in our RCT, eliciting firms' trust and valuation of the offer. We find that, compared to managers who received an unannounced reminder, managers who received an announced reminder are 16.1 pp more likely to state that the reminder changed their perception of the offer's value (39.2% compared to 23.1% in the unannounced-reminder group). We also find evidence that the treatment effect of the announced reminder relative to the unannounced reminder is concentrated among managers who generally distrust advertised offers. The same holds when we use the number of months the firm has used the FinTech payments technology as a proxy for their trust in the FinTech firm. These results suggest that the announced reminder increases the level of trust firms have in the offer and, as a result, their perception of the offer's value. We show that alternative explanations such as different behavior induced by the announced reminder (e. g., checking the offer's profitability in preparation for the reminder) do not explain why the announced reminder group had a higher take-up rate. The result on trust could have broad implications for firms' adoption of various profitable opportunities, as these opportunities often require firm-to-firm interactions where a lack of trust may be an important barrier.

We conclude that non-standard (behavioral) mechanisms are significant determinants of managerial decision-making within small firms, above and beyond the standard economic frictions analyzed in prior literature. While there is substantial evidence about these frictions inducing inertia in terms of *non-managerial* actions, we provide evidence on how these barriers affect *man-*

⁸ Those in the no-deadline arm could still adopt after the deadline, and we show that the treatment effect of the announced reminder persists for the entire period we have data, both for the full sample and restricting to only those who did not have a deadline.

agerial decisions and prevent firms from maximizing profits. As a result, “organizational repairs” (Camerer and Malmendier, 2007) need to account for limited memory, overconfidence about one’s memory limitations, and distrust in offers affecting managers’ decision-making. To a more limited extent the same holds for present bias, albeit mostly among smaller firms and for less-valuable opportunities.

Related Literature. Individuals’ present bias and the economic costs of this bias have been extensively studied (Laibson, 1997; Madrian and Shea, 2001). Focusing on farmers, Duflo, Kremer, and Robinson (2011) find that present bias and fixed costs inhibit the adoption of newer and more efficient fertilizer. They find that small, time-limited subsidies increase adoption, especially among impatient farmers. However, evidence on the effectiveness of deadlines is mixed. In many settings deadlines do not help individuals overcome present bias. For example, individuals do not switch health plans despite a large benefit from switching and a deadline imposed by the open enrollment period (Handel, 2013; Ericson, 2014). Evidence on the effectiveness of commitment devices, which aim to help sophisticated present-biased individuals, is also mixed (Bryan, Karlan, and Nelson, 2010; Carrera et al., 2022).

Individuals’ limited memory has also been documented in a number of domains cited above. We show that limited memory also affects *managerial decisions within firms* and prevents some firms from adopting a profitable opportunity. Overconfidence also affects decision-making in a number of domains, including managerial decisions about firm entry (Camerer and Lovallo, 1999), corporate investment (Malmendier and Tate, 2005), acquisitions (Malmendier and Tate, 2008), and filing for bankruptcy (Bernstein, Colonnelli, Hoffman, and Iverson, 2023). Overconfidence about memory is less-studied even in non-managerial settings, but can lead to lower take-up of delayed payments and mail-in rebates (Ericson, 2011; Tasoff and Letzler, 2014).

Finally, a novel contribution of our paper is that we identify trust in other firms as an important friction in managerial decision-making. Prior literature has documented that a lack of trust can have significant effects on consumer decision-making. For example, distrust in banks leads individuals to avoid using banks (Guiso, Sapienza, and Zingales, 2004; Osili and Paulson, 2014), and interventions that increase trust can lead to increased savings (Bachas, Gertler, Higgins, and Seira, 2021; Mehrotra, Somville, and Vandewalle, 2021). Distrust also leads to lower stock-market participation (Guiso, Sapienza, and Zingales, 2008; Osili and Paulson, 2008) and savings deposits (D’Acunto, Prokopczuk, and Weber, 2019), makes individuals less likely to refinance their mortgage (Johnson, Meier, and Toubia, 2019), and reduces borrowing, risk pooling, and the take-up of insurance products (Karlan, Mobius, Rosenblat, and Szeidl, 2009; Feigenberg, Field, and Pande, 2013; Cole et al., 2013).

Evidence on the role of trust in *interfirm* relationships is limited. McMillan and Woodruff

(1999) find that supplier firms in Vietnam are more likely to offer trade credit to buyer firms that they trust. Banerjee and Duflo (2000) document that trust and reputation play important roles in interfirm contracting in the Indian software industry. D’Acunto, Xie, and Yao (2024) find that a reduction of interfirm trust in the US following the Enron scandal led to an increase in the number of contingencies included in contracts. Bloom, Sadun, and Van Reenen (2012) document that higher trust within multinational firms increases decentralization and raises aggregate productivity. Cai and Szeidl (2018) find that a lack of trust is a barrier to creating business partnerships in China, and randomizing regular meetings between firms increases trust. In Alfaro-Ureña, Manelici, and Vasquez (2022), local supplier firms in Costa Rica cite gaining the trust of multinational corporations as an import precursor to exporting. We find that when a firm informs other firms that they will take an action and then follows through on that action, this increases trust and adoption of a profitable opportunity.

On the effect of behavioral biases on firms, DellaVigna and Gentzkow (2019) find that managerial inertia is a key friction and define managerial inertia as “agency frictions and behavioral factors that prevent firms from implementing optimal policies even though the benefits of doing so exceed the economic costs.” There is limited evidence, however, on which behavioral factors might be driving this inertia within firms. Kremer, Lee, Robinson, and Rostapshova (2013) argue that loss aversion prevents small retail firms from stocking sufficient inventory. Beaman, Magruder, and Robinson (2014) find that limited attention prevents small retail firms from keeping sufficient small change. Selective attention appears to lead both seaweed farmers and multi-billion dollar companies to fail to attend to important features of the data (Hanna, Mullainathan, and Schwartzstein, 2014; List, Muir, Pope, and Sun, 2023). In all of these cases, these behavioral factors led to lower profits.⁹

There is also substantial evidence cited above on *other barriers* that firms face; see Verhoogen (2023) for an extensive survey on firm technology and product upgrading in developing countries, as well as the barriers that prevent firms from adopting these opportunities. Our contribution is to test whether—in addition to these barriers documented by other studies—present bias, limited memory (including distorted beliefs about memory), and a lack of trust in other firms prevent firms from adopting profitable opportunities.

2 Model

We present a simple theoretical framework that allows us to fix ideas about the potential roles of present bias, limited memory, and a lack of trust, and to generate predictions for our empirical implementation. We build on the model in Ericson (2017), which allows for present bias and

⁹ There is also the question of whether firms’ objective is to maximize profits; Banerjee et al. (2023) provide evidence that this is not the objective of vegetable market microentrepreneurs in India.

limited memory, as well as naïveté (overconfidence) about present bias and memory. We augment the model by introducing a role for trust, or lack thereof: agents might discount the value of offers or other business opportunities if they do not fully trust their business partner to follow through. The model allows us to derive predictions about the effects and interactions of these potential barriers to the adoption of profitable opportunities.

2.1 Model Assumptions

In the model, a manager decides whether or not to take advantage of a business opportunity that increases profits in the future but has an immediate cost. We consider the choice of adoption and non-adoption over T periods, from $t = 1$ (when the offer is received) to $t = T$, including the possibility $T \rightarrow \infty$. The manager at time t maximizes the net present value of firm profits, $U_t = \pi_t + \beta (\sum_{k=1}^{\infty} \delta^k \pi_{t+k})$, where δ is the standard exponential discount factor and $\beta \leq 1$ captures the possibility of present bias. The manager has beliefs $\hat{\beta} \in [\beta, 1]$, and is (partially) naïve if $\hat{\beta} > \beta$.

The model allows for managers to have imperfect memory. The parameter ρ_t measures the probability of remembering the opportunity in period t conditional on having remembered it in period $t - 1$. On the day the offer is received, we assume $\rho_1 = 1$. Managers have beliefs $\hat{\rho}_t \in [0, 1]$, and are overconfident about their memory if $\hat{\rho}_t > \rho_t$.

Adopting the offer in period τ results in an immediate cost c and a flow of future benefits y starting in period $\tau + 1$. In each period t , the manager draws a stochastic cost c_t from a known cost distribution $F(c)$ that is continuous, differentiable, and has positive density over a range $[\underline{c}, \bar{c}]$. The flow of benefits $\{y_t\}_{t=\tau+1}^{\infty}$ is the increase in profits each period after adopting the profitable opportunity. Thus for a manager adopting at time τ , the present value of future benefits as of period $\tau + 1$ is $\sum_{k=0}^{\infty} \delta^k y_{\tau+1+k} \equiv y$, and it is $\beta \delta y$ as of time τ .

We incorporate trust into the model with a parameter α_t such that the expected benefit from adopting the offer is $\alpha_t y$. Thus, α_t can be thought of as the probability that the offer is not a scam, or the probability that the FinTech company is not trying to take advantage of the firm in some way. The subscript t allows trust to change over time.

Hence, the manager decides whether to adopt based on the value function

$$V_t = \begin{cases} \beta \delta \alpha_t y - c_t & \text{if adopting in } t, \\ \hat{\rho}_{t+1} \beta \delta \mathbb{E}_t[\hat{V}_{t+1}] & \text{if not adopting in } t, \end{cases} \quad (1)$$

where $\mathbb{E}_t[\hat{V}_{t+1}]$ is the expected value (over cost draws) of the perceived continuation value of not

adopting in the current period (and potentially adopting in a future period), with

$$\hat{V}_{t+1} = \begin{cases} \hat{\beta} \delta \alpha_{t+1} y - c_{t+1} & \text{if adopting in } t+1, \\ \hat{\rho}_{t+2} \hat{\beta} \delta \mathbb{E}_{t+1}[\hat{V}_{t+2}] & \text{if not adopting in } t+1. \end{cases} \quad (2)$$

Note that V_t is a function of the present bias β , while the perceived continuation value \hat{V}_{t+1} is a function of *perceived* present bias $\hat{\beta}$. If there is a deadline, the continuation value at the deadline is zero, as is the expected value of the perceived continuation value: $\mathbb{E}_T[\hat{V}_{T+1}] = 0$.

2.2 Equilibrium Behavior

We impose the equilibrium notion of a perception-perfect equilibrium (O'Donoghue and Rabin, 1999), which requires that the action taken in each period maximizes preferences as of that period, given dynamically consistent beliefs about future strategies. We show in Appendix A that the model implies an optimal cutoff strategy where the manager adopts in period t if the offer is active and the cost draw c_t is below a threshold c_t^* . The offer is active if the manager has not already adopted before period t , has not forgotten about the offer by period t , and the deadline has not passed ($t \leq T$). The optimal cost threshold is defined recursively by the following equations:

$$c_t^* = \beta \delta (\alpha_t y - \hat{\rho}_{t+1} \mathbb{E}_t[\hat{V}_{t+1}]) \quad (3)$$

$$\mathbb{E}_t[\hat{V}_{t+1}] = \int_{\underline{c}}^{\hat{c}_{t+1}^*} (\delta \alpha_{t+1} y - c) dF(c) + (1 - F(\hat{c}_{t+1}^*)) \delta \hat{\rho}_{t+2} \mathbb{E}_{t+1}[\hat{V}_{t+2}], \quad (4)$$

where \hat{c}_{t+1}^* is what the manager at time t believes their optimal cost threshold will be at time $t+1$. The definition of \hat{c}_{t+1}^* is thus identical to that of c_{t+1}^* but replacing β with $\hat{\beta}$. Appendix A proves the existence and uniqueness of this equilibrium.

The probability of adopting at some period $t \leq T$ is the probability the offer is active (which is the product of the probability the offer is remembered and the probability the offer has not already been adopted) times the probability the cost draw c_t is below the threshold c_t^* :

$$\Pr(\text{adopt at } t) = \underbrace{\prod_{j=1}^t \rho_j}_{\Pr(\text{remember})} \underbrace{\prod_{k=1}^{t-1} (1 - F(c_k^*))}_{\Pr(\text{not adopted before } t)} F(c_t^*). \quad (5)$$

2.3 Model Predictions

The model generates several testable predictions, which we will take to the data. All proofs are in Appendix A.¹⁰

Prediction 1 (Offer Value and Take-Up). *A higher value of the offer (higher y) increases take-up.*

Prediction 2 (Offer Value and Deadline). *A higher value of the offer (higher y) implies a lower treatment effect of a deadline on cumulative take-up by the deadline.*

Prediction 2 implies that if we increase the value of the offer (as we do experimentally by randomizing the fee reduction we offer), the effect of the deadline should decrease.

Prediction 3 (Deadline and Present Bias). *The more present-biased a manager is, the larger is the treatment effect of a deadline on cumulative take-up by the deadline.*

This prediction tells us that the more present-biased the manager is, the larger the deadline effect we observe (*ceteris paribus*). The contrapositive of Prediction 3 implies limited or no present bias if a deadline does not have an effect.

We now turn to predictions about reminders.

Prediction 4 (Reminder and Memory). *If a reminder (unannounced or announced) increases the probability of remembering, it will also increase take-up.*

Next, we consider the effect of the announced relative to the unannounced reminder. We focus on the scenario where firms are forgetful and at least some firms do not adopt on the first day, e.g., because they wait for a better cost draw.

Prediction 5 (Announced Reminder and Beliefs about Memory). *The announced reminder (a) reduces take-up at $t = 1$, compared to the unannounced reminder, if managers do not believe they have perfect memory, and (b) has no differential effect on take-up at $t = 1$ if managers believe they have perfect memory.*

¹⁰ Most predictions—as well as the proof that there exists a unique equilibrium—are conditional on $\hat{\beta}$ being not too small. Our simulations in Appendix B confirm that this restriction typically rules out only unreasonably small values of $\hat{\beta}$, i.e., beliefs that one's future self will be severely present biased. Prediction 2 also requires that y is not so low that a manager without a deadline would never adopt as it is not a sufficiently valuable opportunity, and Prediction 3 requires that β is not so low that a manager without a deadline would never adopt because they always procrastinate regardless of the cost draw. In addition, Predictions 2 and 3 are conditional on ρ_t and T being not too small, which our simulations show typically excludes only extreme cases. (For example, we exclude the case of an extremely short deadline $T = 1$.) The exception is Prediction 2, which holds for most but not all reasonable combinations of $\hat{\beta}$, ρ_t , and T . The simulations in Appendix B reveal, however, that Prediction 2 does tend to hold for large $\hat{\beta}$ and ρ_t as long as $T \geq 6$ (which is the case in our experiment where $T = 8$ days).

The reason for the predicted first-day effects is that the announced reminder increases the manager’s belief about their future ability to remember and adopt. When managers know that they will receive a reminder, they do not have to worry about forgetting—so the announced reminder leads to lower take-up on day 1 if the manager thinks they have imperfect memory. If, instead, the manager is confident that they will remember, then the announced reminder will not have an effect.

Prediction 6 (Announced Reminder and Trust). *The announced reminder (a) does not affect final (cumulative) take-up, compared to the unannounced reminder, if managers inherently trust the offer; and (b) increases final (cumulative) take-up if some managers distrust the offer and their trust increases after receiving the announced reminder.*

Prediction 6 derives the trust-based (rather than memory-based) implications of an announced reminder. Variation in trust or distrust does not affect behavior on the day of the announcement. On the day the announced reminder is received, however, the recipients belief in the trustworthiness of the FinTech firm and its offer increases, and thus the expected value of adopting.

3 Institutional Context and Experimental Setting

We partnered with a FinTech payments company in Mexico to study whether these behavioral frictions—present bias, limited memory, and a lack of trust in other firms—as well as possibly distorted beliefs (overconfidence about memory) help explain firms’ failure to adopt a profitable opportunity. The FinTech company provides its customers with point-of-sale (POS) hardware and an app to accept debit and credit card payments, similar to Square in the US. For each electronic card payment that the client firms process, the FinTech company charges a merchant fee that is a percentage of the payment amount. It does not vary depending on the card network used (e.g. American Express, MasterCard, or Visa). Relative to POS terminals offered by banks, the FinTech partner’s POS terminal is less expensive to acquire and does not include a monthly fee, but the FinTech firm charges a higher merchant fee than banks. Specifically, the FinTech partner charges 199 pesos (\$9 USD) to purchase the POS terminal, no monthly fee, and a 3.5–3.75% merchant fee, while Mexico’s largest bank charges a 300 pesos (\$13) “sign-up fee” plus a 359 pesos (\$16) per month rental fee for a POS terminal, but a lower 2.15% merchant fee.¹¹

FinTech and cashless payments have expanded substantially in Mexico since the 2010s. A government program that rolled out debit cards to poor households in 2009–2012 led to an increase in small retail firms’ adoption of POS terminals (Higgins, forthcoming). Mexico passed a FinTech Law in 2018 and by 2020 had 441 FinTech startups (Finnovista, 2023) and over 50 million users

¹¹ Conversions to USD are based on market exchange rates as of September 29, 2020, the first day of our experiment. The exchange rate was near its peak at this time; using exchange rates as of January 1, 2024, the costs for the FinTech POS terminal are \$12, and for the bank POS terminal \$18 initial fee plus \$21/month rental fee.

of FinTech payment products.¹² FinTech companies offering POS terminals entered the market in 2013, and our FinTech partner estimates that in 2019, of the 3.3 million total POS terminals in use in Mexico, 1.3 million were issued by FinTech payments companies, while another 1.3 million were issued by banks and 0.6 million were issued by other issuers. Our partner had a 10% market share among FinTech-issued POS terminals, and a 4% market share among all POS terminals. Among small firms, FinTech-issued POS terminals are substantially more popular than bank-issued POS terminals: according to survey data from 2022 collected by Higgins (forthcoming), 61.1% of small retail firms with a POS terminal had a FinTech-issued POS terminal, while 21.7% had a bank-issued POS terminal and the remainder had a POS from other issuers.¹³

In focus groups we conducted with small firms using our FinTech partner's product prior to the RCT, many managers stated that prior to adopting the FinTech company's technology they did not accept card payments. While banks charge lower merchant fees, managers said that accepting card payments with our FinTech partner's technology is easier as there is less documentation needed to register, there is no need to have a bank account with the bank providing the POS terminal, and there is no minimum monthly transaction requirement to avoid extra charges. Focus group participants sought to accept electronic payments because they could increase their customer base by attracting customers who prefer to pay with debit and credit cards (consistent with empirical findings in Higgins, forthcoming). Some noted that it is convenient for them to have increased portability to process transactions anywhere as the FinTech's POS terminal is smaller than a bank-issued POS and can be connected to any mobile device. They also noted that, relative to receiving cash payments, it is convenient to have their payments deposited directly into a bank account and to have increased safety from not needing to hold as much cash.

The FinTech company's motivation for partnering with us for this experiment was two-fold. First, they were interested in testing a lower fee to increase customer retention (i.e., to lose fewer customers to competitor FinTech companies or banks). Second, they did not know the elasticity of their customer firms' card sales with respect to the fee, and thus did not know if they were charging the optimal merchant fee. On customer retention, they wanted to test whether offering a lower merchant fee would reduce customer churn, and also what messages sent to customers would increase adoption of this lower fee (and hence potentially further reduce churn). Offering to lower the merchant fee rather than automatically lowering it for all customers was necessary for administrative and technological reasons, which is what enabled us to conduct this experiment. It may also have been optimal as a form of price discrimination, as the absolute value of firms' elasticity of card sales with respect to the fee may be positively correlated with their probability of

¹² See <https://www.debate.com.mx/economia/Pagos-digitales-en-Mexico-estadisticas-y-tendencias-del-mercado-20230429-0095.html>.

¹³ The most common other type of issuer is suppliers such as Bimbo, Coca-Cola, and Grupo Modelo.

accepting the lower merchant fee.

4 Experimental Design

4.1 Intervention

Our FinTech partner randomly offered a cost-saving measure to firms that were already users of their technology. Firms were informed about the opportunity to obtain a lower merchant fee through both email and SMS text messages in order to maximize awareness of the offer. Figure 2 shows examples of the initial email that firms received.

The email had a button that linked to a short online form that firms had to fill out to activate the fee reduction, and the lower fee was generally activated within one day. The form required managers to fill in basic registration information they had previously shared with our FinTech partner: name, email, and national identification number (which is frequently used in Mexico for many types of transactions). The email informed the user that, based on the FinTech company’s best estimate, the form would only take one minute to complete.

Among the 33,978 firms in the RCT, 4,010 firms were randomly assigned to the control group that was eligible to receive an offer based on our sample selection criteria, but did not receive it. The control group size was based on institutional constraints from the FinTech partner, and the reason for including a pure control group was to measure the elasticity of card payment revenues with respect to the lower fee (Section 8). The remaining firms were assigned to one of the seven other groups combining deadlines and reminders: (i) no deadline, no reminder (4,455 firms); (ii) no deadline, announced reminder (3,671); (iii) no deadline, unannounced reminder (4,453); (iv) eight-day deadline, no reminder (4,618); (v) eight-day deadline, announced reminder (3,501); (vi) eight-day deadline, unannounced reminder (4,629); (vii) same-day deadline, no reminder (4,641). The sample size in each of these seven groups was determined based on power calculations using the results from our May 2019 randomized pilot and simulations of the model presented in Section 2.

Within each treatment group, we also experimentally varied the value of the offer by offering two levels of lower merchant fees, 3% or 2.75% (or no lower merchant fee in the control group). Prior to the experiment, firms were charged either a 3.75% or 3.5% fee for each transaction, which was a function of when they started using our FinTech partner’s technology. Thus, the fee reduction ranges from 50 basis points—for those reduced from 3.5% to 3%—to 100 basis points—for those reduced from 3.75% to 2.75%. (Part of the size of this reduction is random based on their randomized new fee offer, and part is not random based on whether they had a 3.75% or 3.5% fee before the experiment.) The lower fee lasted for six months (until March 31, 2021), after which the firm’s rate returned to their pre-intervention rate. All of this information was included in the emails they received. The reason that the fee reduction was temporary was that our FinTech part-

ner worried that firms' use of the technology might be inelastic with respect to the lower fee, in which case the FinTech company could lose a substantial amount of money by lowering the fee permanently.

We stratified our randomization by business type and quartiles of average monthly sales before our intervention, where business type is one of six categories: small retailers, professionals, beauty, clothing, restaurants, and other.

4.2 Timeline

Figure 3 shows the experiment timeline across the control group and the seven treatment groups. (The figure does not distinguish between the 2.75% vs. 3% offer, which is cross-randomized within each of the seven treatment groups).

The control group did not receive any offer and did not receive any emails related to the experiment. The no-deadline, no-reminder group received the initial email and SMS with the offer on September 29, 2020, and received no subsequent messages; this group could adopt any time after day 8 as well (Figure 2, left panel). The no-deadline, unannounced-reminder group received an initial email identical to that of the no-deadline, no-reminder group, but then also received an unannounced reminder email on October 5. The no-deadline, announced-reminder group received the initial email on September 29, and their email included an additional sentence stating that they would receive a reminder on October 5; this group then received a reminder email identical to that of the no-deadline, unannounced-reminder group's reminder email on October 5 if they had not already adopted. The deadline, no-reminder group received an initial email on September 29 that informed them of the deadline, which was October 6. The deadline, announced reminder group received an initial email on September 29 that informed them of the deadline on October 6 and that they would receive a reminder on October 5 (Figure 2, right panel). The deadline, unannounced reminder group received the same initial email on September 29 as the deadline, no-reminder group informing them of the October 6 deadline, but then received an unannounced reminder on October 5. Finally, we had a treatment arm with a same-day deadline and no-reminder (Group 8), which was informed in the initial email that the deadline was "today, September 29."

The initial emails and SMS messages were sent on September 29, 2020 at 10am Central Standard Time (CST) which is the time zone that covers most of Mexico. The groups with a deadline had until midnight on October 6, with the exception of the group with the same-day deadline, which had all of September 29 (until midnight) to take up the offer. For both the announced and unannounced reminder groups, the reminders were sent on October 5 at 10am CST regardless of whether the group had a deadline; this corresponds to one day before the deadline for groups that also had a deadline. Each of the emails was accompanied by two concurrent SMS text messages that jointly contained similar information as the email but in a condensed format. We did not ran-

domize which method of contact was used (email vs. SMS vs. both) as we wanted to maximize the probability that the manager was aware of the offer by always sending both an email and SMS.

The experiment was initially intended to launch on March 24, 2020, but was delayed due to the start of the COVID-19 pandemic. Specifically, since we could observe the electronic sales of our potential sample in administrative data, we waited until average monthly sales had recovered to pre-pandemic levels, which occurred in August 2020 (as shown in Appendix-Figure C.5). We restricted the RCT sample to the top quartile of the FinTech company’s users, using August 2020 sales. Thus, our filtering excluded firms that had closed or greatly reduced their sales due to COVID-19.

4.3 Sample

To maximize the absolute value of the offer, we selected the RCT sample to include the top quartile of the FinTech company’s over 130,000 users, as measured by their monthly sales in August 2020 (the month before our experiment launched). The sample selection—i.e., restricting to the top quartile of users—was informed by a randomized pilot we conducted with 11,755 firms in May 2019 where we offered a subset of the users who had a 3.75% fee a smaller fee reduction to 3.5%.¹⁴ The pilot included firms throughout the sales distribution. We found that the take-up rate of the lower fee was increasing in baseline sales and that the elasticity of card sales with respect to the fee was statistically significant only for the fourth quartile of baseline sales.

After filtering out firms that were included in our randomized pilot, we identified the top quartile of users (34,010 firms) as the potential RCT sample. Our FinTech partner then filtered out users that were not in good standing administratively, which resulted in a sample of 33,978 firms in the experiment. As expected by virtue of randomization, the probability of being excluded from the experiment for not being in good standing does not differ by treatment arm (Appendix-Table C.1).

5 Data

We use two main sources of data: administrative data provided by our FinTech partner and survey data that we collected on a subsample of firms in the RCT.

5.1 Administrative Data

Our main source of data is administrative data on the 33,978 firms.

Pre-experiment data and balance. Prior to the experiment, we have data on characteristics (such as manager sex, manager age, business type, and dates of registration and first transaction),

¹⁴ Other users not included in the pilot already had a 3.5% fee, depending on when they adopted the technology.

and firm \times day level data on the number of transactions and volume of pesos transacted through the FinTech payments technology from July 2019.

Table 1 shows baseline summary statistics of the firms in our sample. It also shows that the randomization is balanced across treatments; the numbers in each row of the table come from a regression of each firm characteristic from the administrative data on a set of treatment dummies for the different treatments: announced reminder, unannounced reminder, deadline, and 2.75% fee. Column (1) shows the intercept (and thus the control group mean), while columns (2)–(5) show the coefficients on the treatment arm dummies. Column (6) shows the omnibus F -statistic and corresponding p -value for the regression in that row. All of the variables are balanced across treatment arms.

As shown in Panel A of Table 1, the managers who received the cost-saving offer in our RCT are 44.1% female and on average 39 years old. Panel B shows that the most common business types are *small retailers* (corner stores and prepared food vendors) at 26% and *professionals* (medical services, dentists, and veterinarians) at 23.9%. For the rest of the business types, the *beauty* category includes hair dressers, barber shops, beauty salons, and spas, *clothing* includes clothing, shoe, and accessory stores, *restaurants* are restaurants, cafes, and bars, and the *other* category includes auto shops, construction material wholesalers, and other business types. The average length of time that firms have been using our FinTech partner’s technology to accept card payments is 24 months.

Experimental data and descriptive results. From the RCT, we have data on whether and when the firm (i) opened the email, (ii) clicked on the link in the email, (iii) filled out the form to activate the lower fee, and (iv) logged into their online account, from the first day of the experiment (September 29, 2020) through the final day that the lower fee was valid (March 31, 2021). We also have firm \times day level data on the number of transactions and volume of pesos transacted through the FinTech payments technology through March 2021.

In the group with no interventions designed to increase in take-up (i.e., the no-deadline, no-reminder group), overall take-up of the profitable opportunity was 27.7%. Take-up is increasing in firm size, as measured by baseline sales through the technology, but only slightly: the smallest quintile of firms had a take-up rate of 25.7% while the largest quintile had a take-up rate of 29.1% (Appendix-Figure C.2). While these levels of take-up may appear surprisingly low for an opportunity that was worth 3% of profits for the median firm, they are substantially higher than the take-up rates of other (less-valuable) campaigns our FinTech partner had sent in the past and are comparable to estimates from studies that use letters to communicate high-value cost-saving opportunities to individuals.¹⁵

¹⁵The average click rate for campaigns sent by our FinTech partner was 1–2%. Take-up rates of high-value cost

5.2 Survey Data

We conducted a survey on a subset of firms. We were constrained by our FinTech partner in the number of surveys that we were permitted to conduct. We contacted 1,398 firms for the survey and successfully surveyed 471 firms (a 33.7% response rate). Because one of our goals in the survey was to provide evidence on the mechanism explaining the larger treatment effect of the announced reminder than the unannounced reminder, we oversampled firms in these two treatment arms, and also oversampled firms that accepted the offer. Appendix-Table C.3 shows that the survey subsample is balanced across treatment arms, and Appendix-Table C.4 shows that the surveyed subsample is comparable to the non-surveyed sample on observables.

The survey includes questions about the number of employees, profits, share of sales through the technology, questions to measure how accurately they knew the fees they were charged and their transactions through the technology (the fee they were charged prior to receiving the offer, the value of transactions made through the technology in the last week, and the amount of fees they paid in the last week), and how much they expected to save by accepting the lower fee. The latter question enables us to measure whether knowledge about the size of the fee reduction would translate well into knowledge about how much they would save, or alternatively if they had trouble translating the fee reduction into a cost savings by multiplying it by their sales (Shue and Townsend, 2021). The survey also includes questions about whether they remember receiving the email and SMS, whether they read the SMS, whether they noticed the offer had a deadline (for those assigned a deadline), and what impact the lower fee had on their business. Depending on whether and when the manager adopted, we also ask questions about why they adopted on the first day, waited and adopted on a later day, or did not adopt. Finally, the survey includes general social survey questions related to trust, procrastination, memory, and other biases, which we use for heterogeneous treatment effects analysis to understand mechanisms. On trust, survey respondents describe how much they agree with the statement “I trust advertised offers” on a 1 to 5 scale ranging from strongly disagree to strongly agree.

One variable worth highlighting here is number of employees. Figure 1 shows the distribution of the number of employees by firm. The median number of employees is 3, and the average is 3.9. Figure 1 also plots the corresponding distribution for all firms in Mexico, which we obtain from the microdata of the 2019 INEGI Economic Census. We see that the distributions look very similar except for the very largest firms in Mexico. In our RCT, 87% of firms have one to five employees, compared to 90% of all firms in Mexico. Furthermore, the largest firm in our RCT

savings opportunities for individuals include 20% take-up of mortgage refinancing that saved them over \$1,200 per year in response to a letter from the financial institution (Johnson, Meier, and Toubia, 2019) and 28% take-up of a Medicare Part D plan that covered more of the drugs a particular individual was prescribed also in response to a letter, saving them about \$100 per year (Kling et al., 2012).

has 150 employees, which corresponds to the 99.7th percentile of the distribution of number of employees across all firms in Mexico.

We also asked managers in the survey how long they expected it to take and how long it actually took to fill out the form to activate the offer. Ex ante, most firms estimated that it would take between six and ten minutes to complete; ex post, most firms report taking between one and five minutes (Appendix-Figure C.1). The expected time reported by managers likely includes the time to perform other actions before adopting the lower fee such as reading the fine print, calculating how valuable the offer was, or discussing with someone else at the firm whether to accept the offer.

Finally, we asked managers who did not adopt the profitable opportunity why they did not adopt. The most common answers (see Appendix-Figure C.13, also for wording of the questions) fall into four groups. A first group of managers “ran out of time,” which would be consistent with procrastinating due to present bias. A second group “forgot,” consistent with limited memory. For these two most-common responses, and consistent with the cost draws in our model, these managers may have been very busy, both when they got the initial email and (if applicable) when they got the reminder, and this high cost draw combined with their present bias or limited memory led them to never adopt.¹⁶ A third group thought it would take too much time, did not consider it important, or were not sure if it would benefit them. Although expected cost savings from the lower fee are equal to 3% of firm profits for the median firm, there is heterogeneity driven by (i) the random variation in whether we offered firms a 2.75% or 3% fee, (ii) the firm’s profit margins, and (iii) the percent of sales transacted through the FinTech payments technology rather than in cash (the latter heterogeneity is shown in Appendix-Figure C.10). Thus, while it is a profitable opportunity for a substantial fraction of firms in the experiment, some firms might still not consider it a sufficiently profitable given the perceived effort cost. Fourth, some firms did not trust the offer. We will analyze the roles of present bias, limited memory, and distrust in detail below.

6 Results

Our primary results use the following regression specification, estimated separately for each day of the experiment from $t = 1$ to $t = 8$:

$$y_i^t = \lambda_{s(i)} + \sum_{k=2}^K \beta_k T_i^k + \varepsilon_i, \quad (6)$$

where y_i^t is a measure of cumulative take-up, i.e., an indicator equal to 1 if firm i accepted the offer on or before day t . The specification includes randomization strata fixed effects $\lambda_{s(i)}$ (which also

¹⁶ The first bar in Appendix-Figure C.13, “opened email but did not remember doing so,” is the number of people who we observe opened the email but who did not recall opening the email when we surveyed them, so we could not ask them why they did not accept the offer.

absorb the constant). The indicator T_i^k denotes assignment of firm i to treatment arm k (where the omitted category $k = 1$ corresponds to the control group), and ε_i are heteroskedasticity-robust standard errors (not clustered since the randomization unit is the individual firm).

When presenting the results, we restate the corresponding predictions from our theoretical model. Before turning to the results, we describe how parameters in the model map to our empirical setting.

In our setting, the time period t from the model is a day. The cost draw c_t can be thought of as a measure of how busy managers are on day t , as it takes a bit of time to click the link in the email or SMS message and to fill out the short form to adopt the lower merchant fee (as well as to potentially take other actions that the manager wants to take before adopting such as checking if there is any fine print, calculating how valuable the offer is, or discussing with someone else at the firm whether to accept the offer). Survey evidence supports that how busy a manager is on a particular day indeed determines whether they adopt that day: in the survey, we asked managers who did adopt why they adopted the offer on the first day or why they delayed adopting. Among managers who adopted on the first day, 75.5% reported doing so because they had time that day, and 72.6% of managers who delayed adopting said they were too busy on the day they received the email (Appendix-Figure C.3).

The benefit y is the present value of the cost savings from a lower merchant fee. Because the lower fee was activated with a one-day delay in practice, and the FinTech company notified firms of this delay in the online form, the assumption that the cost to adopt c is borne in the current period but the benefit y is experienced with a one-period delay maps to our empirical setting.

For firms with a deadline, $T = 8$, except for the same-day deadline arm where $T = 1$; the purpose of the same-day deadline arm is to isolate variation in costs from the probability of forgetting. For treatment arms without a deadline, $T \rightarrow \infty$, or more accurately $T = 184$, the number of days between the initial offer and the fixed end date of the six-month lower fee.¹⁷ We compare cumulative take-up rates on each of the eight days prior to the deadline, and in some figures also compare take-up for six months after the RCT (since firms in treatment arms without a deadline could continue to adopt after day 8).

We assume that reminders about the task can raise the probability of remembering in the period they are sent, ρ_t . However, only an announced reminder that tells managers about a reminder they will receive in a future period τ increases the manager's current beliefs about the probability of remembering in that future period, $\hat{\rho}_\tau$.

Finally, trust in the offer α_t could potentially be affected by any of the treatments if the manager does not trust the offer inherently. We are particularly interested in the case where the manager

¹⁷In Appendix A, we formally show that we can approximate the no-deadline case by a deadline case with a sufficiently long deadline, so the distinction between no deadline and a sufficiently long but finite T is not relevant.

might trust the offer more if they are told they will receive a reminder and then receive this reminder, which would map to an increase in α_t for the period t in which the manager receives the reminder that was previously announced, as well as subsequent periods. More broadly, the t subscript on α_t allows trust to increase either upon receiving the initial message announcing that a reminder will be sent, or only upon receiving the reminder that had previously been announced.

6.1 Offer Value and Take-Up

Prediction 1 states that take-up should be higher when the offer is more valuable. To evaluate this prediction, we exploit the random variation we introduced by offering either a 2.75% or 3% fee. Since the 2.75% offer entails a larger cost reduction, it should have a higher take-up rate than the 3% offer.

Figure 4 shows the cumulative take-up rates by merchants that received a 2.75% or 3% merchant fee each day of the experiment (left panel) and the treatment effects of the 2.75% offer on cumulative take-up each day and their 95% confidence intervals (right panel). As predicted, take-up of the more profitable 2.75% offer is higher. On the first day of the experiment, it was 2 pp higher compared to a base of 19.1% take-up in the 3% offer group. By day 8, it was 3.5 pp higher compared to a base of 25.9% take-up in the 3% offer group. In relative terms, the 2.75% fee increased take-up by day 8 by 13.5%.

We also note that the higher take-up of the 2.75% offer persists after the deadline. Appendix-Figure C.6a shows take-up over six months for firms that did not have a deadline. (Those with a deadline could not adopt after the deadline, so they are excluded from this figure.) The gap in take-up between the 2.75% and 3% group persists and increases slightly to 4 pp after six months.

These results confirm Prediction 1 and validate the basic incentive design of our experiment.

6.2 Offer Value and Deadline

Prediction 2 states that the deadline effect is decreasing in the offer value. We exploit again the random variation in the offer value as well as the randomization into the deadline treatment to test this prediction.

Appendix-Figure C.7a shows the day-by-day cumulative take-up rates among merchants in the deadline versus no-deadline groups (left panel) and the treatment effects of the deadline (right panel)—in both panels separately for merchants who received the more-valuable (2.75%) versus the less-valuable (3%) offer. There is no effect of the deadline on cumulative take-up by day 8 for the more-valuable offer (2.75% fee), but for the less-valuable offer (3% fee), the deadline increases take-up by 2 pp. The difference in the estimated treatment effect of a deadline for the 2.75% and 3% fee groups is statistically significant at the 10% level ($p = 0.083$).

We also note that, for the less-valuable offer, the no-deadline group catches up to the deadline group within 2.5 weeks after the deadline (Appendix-Figure C.14). This is consistent with the model, reflecting the variance in cost draws over time. Intuitively, those with a lower value of the offer might postpone adopting if they have relatively high cost draws (are fairly busy) during the first 8 days and face no deadline. The chances of getting a sufficiently low cost draw (being sufficiently not busy) to adopt are higher with an additional 2.5 weeks of cost draws.

Overall, these results confirm that Prediction 2 holds in our experiment and validate the incentive design also within the deadline vs. no-deadline arms.

6.3 Deadline and Present Bias

We now turn to Prediction 3, which states that the more present-biased a manager is, the larger is the treatment effect of the deadline on cumulative adoption by the deadline. Vice versa, the lack of a deadline effect on cumulative adoption by the deadline implies limited or no present bias in this context.

Figure 5a shows that on day 1, take-up is lower in the deadline than in the no-deadline group, but by day 8, there is no statistically significant difference in cumulative take-up. (The point estimate of the effect of a deadline on take-up is positive but not statistically significant, at 1 pp.) Going beyond the deadline, Appendix-Figure C.6b shows that the no-deadline group catches up to the deadline group within a few days after the deadline, which is expected (and consistent with the model) if firms are experiencing different cost draws each day in terms of how busy they are. Beyond a few days after the deadline, take-up in the no-deadline group surpasses that of the deadline group, and six months after the deadline there is 2 pp higher take-up in the no-deadline group.

The lack of a statistically significant deadline effect on cumulative take-up by the deadline implies that, on average, managers display insufficient present bias for it to affect their adoption of the profitable opportunity. However, this lack of a statistically significant average treatment effect on cumulative take-up by the deadline masks substantial heterogeneity by firm size, which is one of the main two variables that we pre-specified using for heterogeneity tests in our pre-analysis plan and which we already found to be predictive of overall take up (Figure C.2).

Heterogeneity in deadline effect by firm size. Figure 6a shows that there is a statistically significant heterogeneous treatment effect of the deadline by firm size. Among larger firms, i.e., those with above-median baseline sales, the deadline has no effect, which by Prediction 3 indicates that larger firms exhibit little or no present bias. Among smaller firms, instead, the deadline increases cumulative take-up by the deadline by 2 pp. The difference in the estimated treatment effect of a deadline by firm size is statistically significant at the 5% level ($p = 0.014$).

The positive effect of a deadline for smaller firms, compared to the lack of an effect for larger firms, suggests that either decision-making in smaller firms exhibits more present bias (by Prediction 3 if other factors are similar between smaller and larger firms), or other differences between smaller and larger firms lead the deadline to only have an effect for smaller firms. As for the first possibility, differences in present bias across the firm size distribution could be due to smaller firms being run by more present-biased individuals, or it could be that managers are equally present-biased in smaller and larger firms but smaller firms lack organizational structures to mitigate the effects of the manager's present bias. Smaller firms potentially being run by more present-biased individuals could be due to selection (e.g., larger firms appoint less present-biased individuals to run the firm) or due to firm growth being a function of present bias (e.g., firms run by less present-biased individuals take advantage of more profitable opportunities and thus grow more).

As for the second possibility, other explanatory factors within the model might include differences in the relative value of the offer, in the relative opportunity cost of adopting, or in the day-to-day variance in the opportunity cost of adopting. Small and large firms could also differ in other factors that affect whether a deadline has an effect but that are not in the model.

Lack of heterogeneity in deadline effect by other factors. To assess the potential role of other explanatory factors, we conducted a large array of heterogeneity tests. First, we test for heterogeneity based on firm type—which is the other one of the two main variables to test for heterogeneity on that we pre-specified—and find no heterogeneity (Appendix-Table C.8, column 1). Second, we test for heterogeneous effects in the other variables we pre-specified, which are owner age, owner sex, and the month-over-month change in baseline sales from August to September 2020 (excluding the days of the experiment, September 29 and 30) to capture firm growth. We do not find heterogeneity in the deadline effect for any of these variables (Appendix-Table C.9). We also do not find heterogeneous effects of the deadline across the three reminder groups: announced reminder, unannounced reminder, and no reminder (Appendix-Figures C.11 and C.15).

Alternative explanations of deadline effect. We also investigated other (unmodeled) potential effects of the deadline treatment.

First, to the extent that we do estimate a deadline effect (for smaller firms and less-valuable offers), an alternative mechanism might be that deadlines create a sense of scarcity. Thus, we asked managers a survey question on why they thought the offer had a deadline. Only 11.5% of managers gave responses consistent with the deadline creating scarcity. The vast majority of managers thought that the offer had a deadline because deadlines are a usual business practice or common marketing tool (Appendix-Figure C.4, left panel). Furthermore, given that the deadline only had an effect on firms randomized into the treatment arm with *less-valuable* offers, it is

unlikely that a deadline for a less-valuable offer would create a sense of scarcity whereas a deadline for a more-valuable offer would not.

Second, to the extent that we do *not* estimate a deadline effect (for larger firms and more-valuable offers), the mechanism might be that managers did not see the sentence of the email that includes the deadline, even though “fill the form by October 6” was printed in bold red letters to make it easily noticeable (Figure 2). In the survey, we asked directly whether managers in the deadline arm noticed that the offer had a deadline, and 67.7% said yes. Furthermore, given that the deadline did have an effect for the less-valuable offer, it seems implausible that managers with a *more-valuable* offer would pay *less* attention to the email and thus be more likely to miss that it had a deadline. We also note that the announcement about a future reminder in the announced reminder arm was even further down the email (also in bold red lettering), and the announced reminder did have an effect so managers must have seen it.

6.4 Reminder and Memory

We now turn to the role of reminders and memory. From Prediction 4, a reminder (unannounced or announced) will increase take-up if managers are forgetful and the reminder increases the probability of remembering in the period it is received.

For the main results testing Prediction 4, we pool all firms that received a reminder (unannounced or announced) for increased power, since the prediction applies to both types of reminders. Figure 5b shows cumulative take-up rates for the reminder and no-reminder arms over day 1 through day 8 in the left panel, and regression coefficients and 95% confidence intervals for the effect of the reminder in the right panel. On day 1, take-up rates are close to 20% in both groups, and both groups’ take-up rates increase steadily to about 24% on day 6, the day before the reminder. There is no statistically significant difference between the reminder and no-reminder groups on any day before the reminder was sent.¹⁸ After the reminder was sent on day 7, the take-up rate for the group that received a reminder was 4.1 pp higher than take-up in the group that did not receive a reminder. On day 8, the difference in take-up is 4.7 pp. Appendix-Figure C.6c shows that—restricting to those without a deadline (since this group could still adopt after day 8)—the gap in take-up driven by the reminder persists for the six months after the experiment.¹⁹

Lack of heterogeneity in reminder effect. We fail to find any statistically significant heterogeneity of the reminder’s effect across all subgroups we consider in these (and the prior) analyses.

¹⁸ The same holds when we pool across days 1–6 for increased power (Appendix-Table C.5).

¹⁹ We also re-estimate the reminder effect restricting the sample to those receiving an unannounced reminder (as the announced reminder group received a different initial message) and again find no difference prior to the reminder and a large 3.2 pp effect after the unannounced reminder is sent on day 7 and a 3.9 pp effect on cumulative take-up by day 8 (Appendix-Figure C.8).

The effect of the reminder is similarly large across both the 2.75% and 3% offer groups (Appendix-Figure C.7b); there is no statistically significant heterogeneous treatment effect of the reminder exploiting the random variation in how valuable the offer is. The same holds when we test for heterogeneity by the expected gain from adopting the offer, which is calculated as the change in the fee times the firm’s baseline sales. As expected, firms with an above-median expected gain from adopting have higher overall take-up, but the effect of the reminder is just as large for firms with an above-median and below-median expected gain (Appendix-Figure C.9b). Both of these heterogeneity tests were pre-specified. We also have a measure of value of the offer from the survey, which is the percent of total sales that the firm transacts through the FinTech payments technology.²⁰ While there is substantial variation in this measure (Appendix-Figure C.10), we again do not find heterogeneous treatment effects of the reminder (Appendix-Table C.6, column 3).

The effect of the reminder is also equally large for smaller and larger firms, measured by below- vs. above-median baseline sales (Figure 6b). The same holds when we use below- vs. above-median number of employees from our survey data as an alternative measure of size (Table 2, column 1). Note that the heterogeneity tests by number of employees use survey data since we do not observe number of employees in the administrative data. We pre-specified the heterogeneity test using above- vs. below-median baseline sales as one of the main two variables we would test for heterogeneity on. Because one-employee firms might be different than larger firms, we also test for heterogeneous treatment effects of the reminder for firms with one employee vs. more than one employee, and again do not find statistically significant heterogeneous treatment effects (Table 2, column 2).

The reminder effect also holds irrespective of (i) whether the manager who received the email was the owner of the firm or not (Appendix-Table C.7), (ii) firm type—which was the other of the two main variables to test for heterogeneity on that we pre-specified—(Appendix-Table C.8, column 1), (iii) splits by the other manager and firm characteristics that we pre-specified, which are owner age, owner sex, and the month-over-month change in baseline sales from August to September 2020 (excluding the days of the experiment, September 29 and 30) to capture firm growth (Appendix-Table C.10), and (iv) whether or not the offer had a deadline (Appendix-Figure C.11). In sum, the reminder has a large effect on take-up across all of the types of firms in our experiment. These findings are consistent with a strong role of memory and forgetting, though they could also be attributed to alternative explanations outside of our model, which we explore next.

²⁰ No heterogeneity tests using survey measures were pre-specified, as we designed the survey after seeing the results on take-up over days 1–8 from the administrative data.

Alternative explanations for reminder effect. One possibility is that the reminder effect is not driven by managers knowing about the offer and then forgetting, but rather by them paying limited attention and thus not knowing about the offer. Indeed, limited or selective attention have been shown to lower profits in other contexts (Beaman, Magruder, and Robinson, 2014; Hanna, Mullainathan, and Schwartzstein, 2014; List, Muir, Pope, and Sun, 2023).

While we cannot rule out limited attention playing a role, we do observe whether managers opened the initial email prior to receiving the reminder. Given the design of the email with a large banner at the top showing the lower fee in large bold numbers and stating “offer to lower your merchant fee” (Figure 2), it is unlikely that managers opened the email without learning that it was an offer to lower their merchant fee. Overall, the rate of opening the initial email prior to receiving the reminder was 40.5%. This is substantially higher than the 23% open rate of marketing emails the FinTech company had sent to its users, and more than twice the 18% industry-wide open rate for retail (Mailchimp, 2023). Appendix-Figure C.12 shows that conditional on opening the email prior to receiving the reminder, the reminder still has a 5.2 pp effect on take-up. We do not observe whether managers opened the SMS message, but we asked about this in our survey and 49.4% remembered receiving the SMS; of those, 89.7% report reading the message.

Furthermore, when we asked firms why they did not accept the offer, 20.7% responded that they forgot (Appendix-Figure C.13). Another 23.4% had opened the email according to our administrative data but did not remember doing so, which could be either limited memory—forgetting not only to accept the offer but forgetting the offer even existed—or limited attention (e.g., opening the email but not reading it).

Taken together, these results suggest that limited attention cannot by itself explain the full effect of the reminder. They do not rule out, though, that limited attention played a role for some firms and that the reminder could have also eased limited attention constraints.

We also asked firms directly why they thought the FinTech company sent them a reminder. The top three reasons that firms thought the FinTech sent them a reminder were to make sure they wouldn’t forgo a valuable offer, that the FinTech company knew they would forget, or that it is a usual business practice (82.2% of firms answered one of these three responses). Only a small percentage (11.5%) were wary of the motives for being sent a reminder, answering that the reason was to increase the FinTech’s profits or to make firms fall for a scam (Appendix-Figure C.4, right panel).

6.5 Announced Reminder and Beliefs about Memory

We next analyze the differential effects of the announced and the unannounced reminder. Prediction 5 specifies that the announcement reduces pre-reminder take-up if managers do not believe they have perfect memory, but has no differential effect on pre-reminder take-up if managers be-

lieve they have perfect memory. Figure 5c shows that there is not a *negative* effect of announcing the reminder on pre-reminder take-up; the point estimate is non-significant and *positive*. In the model, the lack of a statistically significant pre-reminder effect of announcing the reminder suggests that managers are overconfident about their memory, assuming that the effect of the reminder itself indicates limited memory.

6.6 Announced Reminder and Trust

From Prediction 6, announced reminders will not increase final take-up compared to the unannounced reminder if firms inherently trust the offer, but will increase final take-up if some firms distrust the offer and their trust in the offer increases after receiving the reminder that had been pre-announced. As seen in Figure 5c, we find that take-up after the reminder is sent is nearly 2 pp higher in the announced reminder group than the unannounced reminder group (statistically significant at the 5% level). This gap in take-up between the announced and unannounced reminder groups persists over the six months after the reminder was sent (Appendix-Figure C.6d).

We note that the point estimates in Figure 5c are also positive prior to the reminder being sent, and we cannot reject that the treatment effect of the announced reminder relative to the unannounced reminder is larger on the day of the reminder (day 7) than the day before the reminder (day 6). Hence, there might be an effect of announcing the reminder when the reminder is first announced (in addition to the effect of the announced relative to unannounced reminder on the day the reminder is sent). However, when we pool across pre-reminder days and separately across post-reminder days for additional power, we *do* reject that the pre-reminder and post-reminder effects of the announced reminder are equal (Appendix-Table C.5). Specifically, the additional effect of the announced reminder once the reminder is received is 1 pp (statistically significant at the 5% level).

Lack of heterogeneous post-reminder announcement effects. We do not find statistically significant heterogeneous treatment effects of the announced reminder on cumulative take-up by day 8 along any dimensions: the size of the fee reduction (Appendix-Figure C.7c), the expected gain (Appendix-Figure C.9c), percent of total sales the firm transacts through the FinTech payments technology (Appendix-Table C.6), baseline sales (Figure 6c), number of employees and whether the firm has more than one employee (Table 2), whether the owner was the recipient of the emails (Appendix-Table C.7), whether there is a deadline (Appendix-Figure C.15b), the business type (Appendix-Table C.8), or the owner age, owner sex, or business growth in the month prior to the experiment (Appendix-Table C.10).²¹

²¹ The treatment effect of the announced reminder is not statistically significant from zero for some subgroups, likely due to a loss of power from splitting the sample. Nevertheless, we can never reject that the treatment effect of

In sum, the larger effect of the announced reminder relative to the unannounced reminder appears to hold regardless of which other treatments are included with it, and across all of the types of firms in our experiment.

The higher take-up in the announced reminder group is consistent with (some) managers not fully trusting the offer initially, but their trust increasing after the FinTech firm followed through with the reminder as it had promised in advance. As a result, the perceived value of the offer to these managers would increase and thus their take-up would increase, as outlined in Prediction 6.

We collected additional evidence to explore both the trust mechanism and potential alternative mechanisms, which we discuss in the next section.

7 Mechanisms Behind Announced Reminder Effect: The Role of Trust

In this section we explore the hypothesis that the larger effect of the announced relative to the unannounced reminder can be linked to how much managers trust the offer from the FinTech firm.

Perception of Offer Value. We first test whether the announcement and subsequent receipt of the announced reminder changed managers' perceptions of the offer. To that end, we asked managers who received a reminder, either announced or unannounced, the question: "Did receiving the reminder change your perception of the value of the offer?" The comparison of their answers in Figure 8 reveals that receiving the announced reminder caused a large 16.1 pp increase in the likelihood that the manager responded that the reminder changed their perception of the offer's value (statistically significant at the 5% level), relative to a base of 23.1% responding yes to this question in the unannounced reminder group.

In addition, we asked the open-ended follow-up question "Why did the reminder change your perception of the offer's value?" Comparing responses in the announced and unannounced reminder groups, we find that there were more responses related to trust in the announced reminder group—such as "I had doubts and didn't trust whether it was from [FinTech company]" and "[the reminder] gave it credibility."

Heterogeneity in Trust and Take-Up. Motivated by the mentions of trust in the answers to the open-ended question, we next leverage the general social survey (GSS) measures that we collected in the survey. These measure managers' general levels of trust in advertised offers, reciprocity, procrastination, memory, overconfidence about memory, and attention. Specifically, survey respondents are asked to describe how much they agree with the following statements on a 1 to 5

the announced reminder relative to the unannounced reminder is equal across groups in these heterogeneity tests.

scale, where 5 is *strongly agree*, 4 is *agree*, 3 is *neither agree nor disagree*, 2 is *disagree*, and 1 is *strongly disagree*:

[Trust in Advertised Offers]: “I trust advertised offers.”

[Reciprocity]: “I am more inclined to do business with people who live up to their promises.”

[Procrastination]: “I tend to postpone tasks, even when I know it is better to do them immediately.”

[Memory]: “I tend to have good memory about pending tasks that I have to do and complete.”

[Overconfidence about Memory]: “I tend to think my memory is better than it really is.”

[Attention]: “I can focus completely when I have to finish a task.”

We use the answers to these questions to test for heterogeneous treatment effects of the announced relative to the unannounced reminder across firms with different characteristics. For each survey measure, we create an indicator variable $\mathbb{1}(\text{High survey measure})_i$, which we set equal to 1 if the respondent agrees or strongly agrees with the respective statement. We then estimate the following regression combining the administrative and survey data on the sample that received either an announced or unannounced reminder:

$$y_i = \alpha + \beta_1 \mathbb{1}(\text{High survey measure})_i + \beta_2 \mathbb{1}(\text{Announced reminder})_i + \beta_3 \mathbb{1}(\text{High survey measure})_i \times \mathbb{1}(\text{Announced reminder})_i + \varepsilon_i, \quad (7)$$

where y_i is an indicator variable equal to one if firm i accepts the offer any time until March 31. The coefficient β_3 estimates the heterogeneous treatment effect of the announced reminder by survey measure. For example, for the first survey measure, the estimate $\hat{\beta}_3$ reveals whether the announced reminder has a differential treatment effect for firms that trust advertised offers more.

Figure 9 and Appendix-Table C.12 show the results. The upper panel of Figure 9 shows the take-up rates split in subsamples of four bars for each GSS survey measure, separately by treatment arm, $\mathbb{1}(\text{Announced reminder})_i \in \{0, 1\}$ (empty versus filled bars), and by value of the survey measure, $\mathbb{1}(\text{High survey measure})_i \in \{0, 1\}$ (blue versus red bars). Note that take-up rates in the survey are higher than in the administrative data because we oversampled firms that accepted the offer to be better powered.

The panel shows that take-up is higher among managers who received the announced reminder rather than the unannounced reminder for almost all GSS sample splits. The exception is the high-trust group. Here, take-up rates are identical and high in both the announced- and the unannounced-

reminder groups, indicating that the announced reminder did not have an effect on take-up for firms with high general trust. In contrast, comparing the two bars in the low-trust column, it is clear that the announced reminder did have a large effect on take-up for firms with low general trust.

The bottom panel of Figure 9 shows the corresponding estimated effect of announced reminders by survey measure, where the coefficients in the “Low” columns correspond to $\hat{\beta}_2$ from estimating equation (7) and the coefficients in the “High” columns correspond to $\hat{\beta}_2 + \hat{\beta}_3$. Above each pair of treatment effects we show the statistical significance of $\hat{\beta}_3$. (The $\hat{\beta}_3$ coefficient estimates and standard errors are in Appendix-Table C.12.)

For the split by high and low trust, we see that the estimated treatment effect of announced reminders is entirely concentrated among firms with low general trust. In contrast, for firms with high general trust, the treatment effect of announced reminders relative to unannounced reminders is virtually zero and not statistically significant. Appendix-Table C.12 shows that the heterogeneous treatment effect for high vs. low levels of trust is statistically significant at the 1% level. In contrast, we do not see statistically significant coefficients on heterogeneity tests for the other survey measures (with the exception of overconfidence, significant only at the 10% level with $p = 0.097$), as also shown in Appendix-Table C.12. We also note that we did not find any heterogeneous treatment effects of the announced reminder, relative to the unannounced reminder, across any of the firm and owner characteristics explored in Section 6.5.

For completeness, we confirm that there is no heterogeneous treatment effect of reminders in and of themselves (without prior announcement). When we repeat the exercise of splitting the sample by high and low survey measures, but compare the unannounced-reminder group to the *no*-reminder group, we do not find heterogeneous treatment effects among firms with high vs. low levels of general trust. Instead, as shown in Appendix-Figure C.16 and Appendix-Table C.13, the reminder is more effective for firms with high procrastination and low memory (both statistically significant at the 5% level). (The latter finding also validates the exercise of testing for heterogeneous treatment effects by general survey measures.)

These results corroborate the notion that the estimated effect of the announced reminder relative to the unannounced reminder reflects its effectiveness in generating trust that the offer represents a valuable business opportunity. Those who trusted the offer already are not affected by the announcement.

Length of Business Relationship. Finally, we probe the trust channel in the administrative data, where we have a much larger sample but no direct measure of trust. Instead, we employ the length of a firm’s business relationship with the FinTech company—i.e. the number of months the firm has been using the payment technology—as an indirect proxy, as trust may have been fostered over time through interfirm interactions.

In Figure 10, we plot take-up rates against the length of the business relationship, separately for the announced- and unannounced-reminder arms. As expected, firms that have been using the payments technology for longer have higher overall *levels* of take-up. The treatment effect of the announced reminder relative to the unannounced reminder, instead, is concentrated among firms that have been using the technology for less time as the difference between the blue and red lines indicate.

To formally test whether the treatment effect of the announced reminder relative to the unannounced reminder is decreasing in the length of a firm’s business relationship with the FinTech company, we follow a procedure described in Appendix D. Briefly, since our trust proxy, length of the business relationship, could be correlated with other factors, we control flexibly for the other covariates in the administrative data. First, we assume that take-up is a linear function of two (non-linear) polynomials: (i) a function of the length of the business relationship and treatment, where treatment here refers to the announced reminder rather than the unannounced reminder; and (ii) a function of other covariates and treatment. We then use a machine learning algorithm to estimate predicted individual treatment effects of the announced reminder. Using our estimates from this procedure, we formally test the negative relationship between the length of the business relationship and the treatment effect of the announced reminder seen in Figure 10. We use a bootstrapping procedure to test the statistical significance of this negative relationship, and find that the relationship is negative in 92.5% of the bootstrap samples.²²

Alternative Explanations. The results presented so far corroborate the hypothesized role of trust. Nevertheless, we consider three alternative mechanisms for the effect of announced reminders on take-up.

First, we consider differences in information acquisition. We ask whether the announcement of a future reminder may induce firms to check how valuable or profitable the offer is to them, knowing they can adopt when they get the reminder. For example, managers may not know their current merchant fee (which we decided not to include in the email to avoid adding confusion by including too many numbers in the email); if so, they might take the time between the initial message and the reminder to log into their account and check their current merchant fee.

We address this alternative mechanism using both survey and administrative data. In the survey, we asked managers “What was your fee with [the FinTech provider] the week before you received the offer?” We compare their response to the correct answer, and find that managers are fairly accurate (Appendix-Figure C.17): 23% of managers reported their fee precisely, and the vast

²²Because the polynomial that minimizes root mean squared error turns out to be non-linear (in particular, quadratic) in the interaction between the length of the business relationship and treatment, we evaluate whether the relationship is negative at the median value of the length of the business relationship. The relationship is also negative in 93.3%, 90.5%, and 81.3% of the bootstrap samples when evaluated at the 25th percentile, mean, and 75th percentile.

majority who were not perfectly accurate reported that their fee was 4% which could be due to rounding up the 3.5–3.75% fee. Thus, the vast majority of managers either accurately reported their fee or slightly overreported it, which if anything would lead them to think the offer was even more profitable than it was.

In addition, we use the administrative data on whether firms logged in to their accounts to check their current fee or sales. We create outcome indicator variables if anyone in the firm logged into their account or checked the amount of deposits from electronic sales in the days between when the initial offer was sent and before the reminder was sent. As shown in Appendix-Table C.14, we find that firms that were told about a future announced reminder were not more likely to check their online accounts over the course of the experiment compared to firms in the unannounced reminder group.

Finally, we return to the survey data and analyze the answers to two questions. In one question, we asked managers who did not accept the offer prior to the reminder day and received an announced reminder, “Did you do anything between receiving the initial email and receiving the reminder so that you would know whether to take up the offer when you received the reminder?” Nearly all managers (92.4%) reported not taking any particular action to evaluate the offer between the time they received the initial email and the reminder. Among the remaining 7.6% who do report taking some action between receiving the initial offer and receiving the announced reminder, only 2 managers reported calculating whether they should accept the offer.

Another question asked “Why did you wait until {days to accept} days later [to accept the offer]?”²³ As shown in Appendix-Figure C.3, only 12.3% of managers replied they needed to discuss or think about the offer first, and there is no statistically significant difference in this proportion between the announced and unannounced reminder arms ($p = 0.361$).

We conclude that the possibility of the announced reminder leading firms to take additional steps to evaluate the offer prior to the date on which they knew they would receive the reminder is not a plausible explanation for our findings.

As a second alternative mechanism, we consider the possibility that managers in the unannounced reminder group may have felt annoyed when they received the reminder or ashamed that they did not yet adopt the profitable opportunity. As a result, they may have been less likely to adopt than if they had been told in advance that they would receive the reminder. Feeling ashamed could represent an “ostrich effect” where receiving the unannounced reminder made the decision maker “stick their head in the sand” and avoid making a decision (as in Olafsson and Pagel, 2017).

To test for these or other negative responses to an unannounced reminder, we asked managers

²³ The full survey question is: “We sent you the emails and SMS to let you know about this offer on September 29, but we see that you filled the form on {activation date}. Why did you wait until {days to accept} day(s) later?” This survey question was asked to firms with a deadline, who accepted after the first day of the offer, and recalled accepting or clicking on the offer.

who received a reminder an open-ended question to tell us how they felt when they received the reminder (see Appendix-Figure C.18). Only 2.5% of managers responded that they were annoyed by the reminder, and there is no statistically significant difference between the announced and unannounced reminder groups ($p = 0.494$). Instead, the most common responses indicated that the reminder made managers feel important as a client.

A third possibility is that knowing a reminder will come on a particular day helps solve organizational frictions: for example, knowing when the reminder will come makes it easier to schedule a meeting where the decision of whether to adopt the profitable opportunity will be discussed. It is worth noting, however, that if the firm schedules a meeting about it they are unlikely to forget to adopt. The deadline also provides a particular date to make the decision by; so if this were the mechanism behind the announced reminder effect, the deadline should have a similar effect among larger firms (as organizational frictions are more likely in larger firms)—but it did not. In addition, if the mechanism were organizational frictions we would expect differential effects depending on the size of the firm (in terms of number of employees and baseline sales) and depending on whether the firm is a single-person firm or has more than one employee. However, in Section 6.5 we did not see any heterogeneous treatment effects for these variables.

In summary, inter-firm trust emerges as a plausible and important determinant of companies' willingness to adopt a profitable business opportunity with their business partner.

8 Elasticity of Electronic Sales

For completeness, we also evaluate the impact of a lower merchant fee on electronic sales through the payment technology. We use the following estimating equation:

$$y_{it} = \beta \cdot Treated_i \times Post_t + \gamma_i + \delta_t + \varepsilon_{it}, \quad (8)$$

where y_{it} is an outcome measuring sales using the payments technology, i denotes a firm, t denotes a month, γ_i are firm fixed effects, and δ_t are time fixed effects. Standard errors are clustered at the firm level. $Treated_i$ is an indicator for a firm that received a lower merchant fee offer, i.e., a firm in any treatment arm except the control group, and $Post_t$ is an indicator that equals one during any time period after we sent the offers. Our main coefficient of interest β measures the intent-to-treat (ITT) effect of receiving an offer on use of the FinTech payments technology. To estimate the treatment on the treated (TOT), i.e., the effect on the firms that adopted the lower merchant fee, we replace $Treated_i$ with $Accepted_i$ in specification (8) and instrument $Accepted_i$ with $Treated_i$.

Panel A of Table 3 shows the ITT effect of the lower merchant fee on electronic sales through the payment technology. The first two columns of Panel A show regression results with intensive

measures of payment usage: log sales volume in pesos and log number of transactions (both plus 1). Firms that received the offer increased the average sales volume and number of payments they transacted with the payment technology by 10.7% and 3%, respectively.²⁴ The third column of Panel A shows the regression results with the extensive measure of payment usage: an indicator if the firm made at least one transaction on or after the current month. Firms that received the offer increased their probability of continuing to use the payment technology by 1.3 pp.

Panel B of the same table shows the TOT effect of the lower merchant fee on electronic sales through the payment technology. Firms that accepted the offer increased the sales volume and number of payments they transacted with the payment technology by 42% and 10.8%, respectively. Firms that accepted the offer also increased their probability of continuing to use the payment technology by 4.3 pp. The control mean of the probability of continuing to use the payment technology is 84.7%. This means that firms that accepted the offer were, in relative terms, 5.1% more likely to continue to use the payment technology on or after a given month compared to the control mean. Because the increase in sales processed through the FinTech technology by firms that accepted the lower merchant fee (42%) was larger than the decrease in our FinTech partner's revenues from these firms paying a lower fee on sales they would have made anyway (up to $(3.75 - 2.75)/3.75 = 27\%$), offering the lower merchant fee turned out to increase the profits of our FinTech partner.

Our findings suggest that lowering merchant fees can increase payment usage on both the intensive and extensive margins. This raises the question of how firms encouraged customers to use cards more, or discouraged them from using cards before the treatment. It is possible that some firms previously preferred cash payments due to the cost of accepting card payments and used various methods to steer consumers towards cash transactions. For example, firms can surcharge a percentage or fixed amount to card-paying customers, set a minimum threshold for paying by card, hide the POS terminal in a drawer and only bring it out if they perceive that they will lose a sale by not accepting card payments, or say that the POS terminal is not working. Cash has various indirect costs, however (Alvarez, Argente, Jimenez, and Lippi, 2022; Bachas, Higgins, and Jensen, 2020), so with a lower fee per transaction, some firms may have switched from preferring cash payments to preferring card payments.

One way for a firm to incentivize more card payments by customers is to eliminate surcharges. Surcharging is the practice of passing through the merchant fee only to customers who pay by card, and it is prevalent in this context: Higgins (forthcoming) finds that 63% of small retailers with POS terminals surcharge their customers who pay by card in Mexico. To explore the impact of lower fees on firms, we asked those who accepted the lower fee to respond to the open-ended question, "Is this offer working for your business? What impact has it had?" Despite the open-

²⁴ These percent changes are calculated as $(\exp(\beta) - 1) \times 100\%$.

ended nature of this question, 24.1% of managers replied that prior to receiving the lower fee, they would surcharge customers who paid by card, while after receiving the lower fee they stopped surcharging and absorbed the cost (Appendix-Figure C.19). For example, one manager replied, “[The effect is] very good, [we] don’t charge the fee to clients anymore.”

9 Conclusion

The analyses in this paper reveal a significant role of non-standard determinants in explaining firm decision-making. Well-known behavioral determinants of individuals failing to act in non-managerial settings appear to also bind in the context of managerial decision-making within firms. In particular, imperfect memory and distorted beliefs about the probability of remembering in the future emerge as significant determinants in our setting, while present bias does not appear to play an important role for larger firms, though it potentially does play a role for smaller firms. Beyond those three factors, we provide evidence of distrust as a key explanatory variable. Distrust is likely an important friction in many firm-to-firm relationships, and we provide evidence on an intervention that increased trust in a particular firm-to-firm interaction, and that thereby led to increased adoption of a profitable opportunity.

While the role of these determinants in inhibiting decision-making has been much discussed in the consumer-level literature, they have received less attention when studying firms and impediments to their profit maximization and growth. Our findings suggest that the analysis of firms’ adoption of profitable opportunities would benefit from researchers considering mechanisms beyond the traditional economic frictions that explain non-adoption.

References

- Abel, Andrew and Janice Eberly (1994). “A Unified Model of Investment Under Uncertainty.” *American Economic Review* 84(5), 1369–1384.
- Alfaro-Ureña, Alonso, Isabela Manelici, and Jose P Vasquez (2022). “The Effects of Joining Multi-national Supply Chains: New Evidence from Firm-to-Firm Linkages.” *Quarterly Journal of Economics* 137(3), 1495–1552.
- Alvarez, Fernando, David Argente, Rafael Jimenez, and Francesco Lippi (2022). “Cash: A Blessing or a Curse?” *Journal of Monetary Economics* 125, 85–128.
- Ardagna, Silvia and Annamaria Lusardi (2010). “Explaining International Differences in Entrepreneurship: The Role of Individual Characteristics and Regulatory Constraints.” *International Differences in Entrepreneurship*. Ed. by Josh Lerner and Antoinette Schoar. University of Chicago Press.
- Ashraf, N., D. Karlan, and W. Yin (2006). “Tying Odysseus to the Mast: Evidence From a Commitment Savings Product in the Philippines.” *The Quarterly Journal of Economics* 121(2), 635–672.
- Atkin, David, Azam Chaudhry, Shamyala Chaudry, Amit K Khandelwal, and Eric Verhoogen (2017). “Organizational Barriers to Technology Adoption: Evidence from Soccer-Ball Producers in Pakistan.” *Quarterly Journal of Economics* 132(3), 1101–1164.
- Bachas, Pierre, Paul Gertler, Sean Higgins, and Enrique Seira (2021). “How Debit Cards Enable the Poor to Save More.” *Journal of Finance* 76(4), 1913–1957.
- Bachas, Pierre, Sean Higgins, and Anders Jensen (2020). “Towards a Cashless Economy? Evidence from the Elasticity of Cash Deposits of Mexican Firms.”
- Banerjee, Abhijit, Emily Breza, Esther Duflo, and Cynthia Kinnan (2021). “Can Microfinance Unlock a Poverty Trap for Some Entrepreneurs?” *Working Paper*.
- Banerjee, Abhijit, Greg Fischer, Dean Karlan, Matt Lowe, and Benjamin N Roth (2023). “Do Microenterprises Maximize Profits? A Vegetable Market Experiment in India.”
- Banerjee, Abhijit V. and Esther Duflo (2000). “Reputation Effects and the Limits of Contracting: A Study of the Indian Software Industry.” *Quarterly Journal of Economics* 115(3), 989–1017.
- Baumol, William J. (1996). “Entrepreneurship: Productive, Unproductive, and Destructive.” *Journal of Business Venturing* 11(1), 3–22.
- Beaman, Lori, Jeremy Magruder, and Jonathan Robinson (2014). “Minding Small Change among Small Firms in Kenya.” *Journal of Development Economics* 108, 69–86.
- Bernstein, Shai, Emanuele Colonnelli, Mitchell Hoffman, and Benjamin Iverson (2023). “Life After Death: A Field Experiment with Small Businesses on Information Frictions, Stigma, and Bankruptcy.”

- Bloom, Nicholas, Benn Eifert, Aprajit Mahajan, David McKenzie, and John Roberts (2013). “Does Management Matter? Evidence from India.” *Quarterly Journal of Economics* 128(1), 1–51.
- Bloom, Nicholas, Renata Lemos, Raffaella Sadun, Daniela Scur, and John Van Reenen (2014). “The New Empirical Empirics of Management.” *Journal of the European Economic Association* 12(4), 835–876.
- Bloom, Nicholas, Raffaella Sadun, and John Van Reenen (2012). “The Organization of Firms Across Countries.” *The Quarterly Journal of Economics* 127(4), 1663–1705.
- Bruhn, Miriam, Dean Karlan, and Antoinette Schoar (2018). “The Impact of Consulting Services on Small and Medium Enterprises: Evidence from a Randomized Trial in Mexico.” *Journal of Political Economy* 126(2), 635–687.
- Bryan, Gharad, Dean Karlan, and Scott Nelson (2010). “Commitment Devices.” *Annual Review of Economics* 2(1), 671–698.
- Cai, Jing and Adam Szeidl (2018). “Interfirm Relationships and Business Performance.” *Quarterly Journal of Economics* 133(3), 1229–1282.
- Calzolari, Giacomo and Mattia Nardotto (2017). “Effective Reminders.” *Management Science* 63(9), 2915–2932.
- Camerer, Colin and Dan Lovallo (1999). “Overconfidence and Excess Entry: An Experimental Approach.”
- Camerer, Colin and Ulrike Malmendier (2007). “Behavioral Economics of Organizations.” *Behavioral Economics and Its Applications*. Ed. by Peter Diamond and Hannu Vartiainen. Princeton University Press.
- Carrera, Mariana, Heather Royer, Mark Stehr, Justin Sydnor, and Dmitry Taubinsky (2022). “Who Chooses Commitment? Evidence and Welfare Implications.” *The Review of Economic Studies* 89(3), 1205–1244.
- Cellhay, Pablo A., Paul J. Gertler, Paula Giovagnoli, and Christel Vermeersch (2019). “Long-Run Effects of Temporary Incentives on Medical Care Productivity.” *American Economic Journal: Applied Economics* 11(3), 92–127.
- Cole, Shawn, Xavier Giné, Jeremy Tobacman, Petia Topalova, Robert Townsend, and James Vickery (2013). “Barriers to Household Risk Management: Evidence from India.” *American Economic Journal: Applied Economics* 5(1), 104–135.
- D’Acunto, Francesco, Marcel Prokopcuk, and Michael Weber (2019). “Historical Antisemitism, Ethnic Specialization, and Financial Development.” *The Review of Economic Studies* 86(3), 1170–1206.
- D’Acunto, Francesco, Jin Xie, and Jiaquan Yao (2024). “Trust and Contracts: Empirical Evidence.”
- Daft, Richard L. (2015). *Organization Theory and Design*. 12th edition. Boston, MA: Cengage Learning.

- Dai, Hengchen, Silvia Saccardo, Maria A. Han, Lily Roh, Naveen Raja, Sitaram Vangala, Hardikumar Modi, Shital Pandya, Michael Sloyan, and Daniel M. Croymans (2021). “Behavioural Nudges Increase COVID-19 Vaccinations.” *Nature* 597(7876), 404–409.
- de Mel, Suresh, David McKenzie, and Christopher Woodruff (2010). “Who Are the Microentrepreneur Owners? Evidence from Sri Lanka on Tokman versus De Soto.” *International Differences in Entrepreneurship*. Ed. by Josh Lerner and Antoinette Schoar. University of Chicago Press.
- DellaVigna, Stefano and Matthew Gentzkow (2019). “Uniform Pricing in U.S. Retail Chains.” *Quarterly Journal of Economics* 134(4), 2011–2084.
- DellaVigna, Stefano and Ulrike Malmendier (2004). “Contract Design and Self-Control: Theory and Evidence.” *The Quarterly Journal of Economics* 119(2), 353–402.
- DellaVigna, Stefano and Ulrike Malmendier (2006). “Paying Not to Go to the Gym.” *American Economic Review* 96(3), 694–719.
- Duflo, Esther, Michael Kremer, and Jonathan Robinson (2011). “Nudging Farmers to Use Fertilizer: Theory and Experimental Evidence from Kenya.” *American Economic Review* 101(6), 2350–2390.
- Ericson, Keith M Marzilli (2014). “Consumer Inertia and Firm Pricing in the Medicare Part D Prescription Drug Insurance Exchange.”
- Ericson, Keith M. Marzilli (2011). “Forgetting We Forget: Overconfidence and Memory.” *Journal of the European Economic Association* 9(1), 43–60.
- Ericson, Keith Marzilli (2017). “On the Interaction of Memory and Procrastination: Implications for Reminders, Deadlines, and Empirical Estimation.” *Journal of the European Economic Association* 15(3), 692–719.
- Feigenberg, B., E. Field, and R. Pande (2013). “The Economic Returns to Social Interaction: Experimental Evidence from Microfinance.” *The Review of Economic Studies* 80(4), 1459–1483.
- Finnovista (2023). “FinTech Radar Mexico.” <https://www.finnovista.com/en/radar/fintech-radar-mexico-23-eng/>.
- Gennaioli, Nicola, Rafael La Porta, Florencio Lopez-de-Silanes, and Andrei Shleifer (2013). “Human Capital and Regional Development.” *The Quarterly Journal of Economics* 128(1), 105–164.
- Giné, Xavier, Dean Karlan, and Jonathan Zinman (2010). “Put Your Money Where Your Butt Is: A Commitment Contract for Smoking Cessation.” *American Economic Journal: Applied Economics* 2(4), 213–235.
- Giorcelli, Michela (2019). “The Long-Term Effects of Management and Technology Transfers.” *American Economic Review* 109(1), 121–152.

- Guiso, Luigi, Paola Sapienza, and Luigi Zingales (2004). "The Role of Social Capital in Financial Development." 94(3).
- Guiso, Luigi, Paola Sapienza, and Luigi Zingales (2008). "Trusting the Stock Market." *The Journal of Finance* 63(6), 2557–2600.
- Gurol-Urganci, Ipek, Thyra De Jongh, Vlasta Vodopivec-Jamsek, Rifat Atun, and Josip Car (2013). "Mobile Phone Messaging Reminders for Attendance at Healthcare Appointments." *Cochrane Database of Systematic Reviews*. Ed. by Cochrane Consumers and Communication Group.
- Handel, Benjamin R (2013). "Adverse Selection and Inertia in Health Insurance Markets: When Nudging Hurts." *American Economic Review* 103(7), 2643–2682.
- Hanna, Rema, Sendhil Mullainathan, and Joshua Schwartzstein (2014). "Learning Through Noticing: Theory and Evidence from a Field Experiment." *Quarterly Journal of Economics* 129(3), 1311–1353.
- Hardy, Morgan and Jamie McCasland (2023). "Are Small Firms Labor Constrained? Experimental Evidence from Ghana." *American Economic Journal: Applied Economics* 15(2), 253–284.
- Higgins, Sean (forthcoming). "Financial Technology Adoption: Network Externalities of Cashless Payments in Mexico." *American Economic Review*.
- Jagannathan, Ravi, David A. Matsa, Iwan Meier, and Vefa Tarhan (2016). "Why Do Firms Use High Discount Rates?" *Journal of Financial Economics* 120(3), 445–463.
- Johnson, Eric J, Stephan Meier, and Olivier Toubia (2019). "What's the Catch? Suspicion of Bank Motives and Sluggish Refinancing." *The Review of Financial Studies* 32(2), 467–495.
- Kaplan, Steven N. and Morten Sorensen (2021). "Are CEOs Different?" *The Journal of Finance* 76(4), 1773–1811.
- Karlan, Dean, Margaret McConnell, Sendhil Mullainathan, and Jonathan Zinman (2016). "Getting to the Top of Mind: How Reminders Increase Saving." *Management Science* 62(12), 3393–3411.
- Karlan, Dean, Markus Mobius, Tanya Rosenblat, and Adam Szeidl (2009). "Trust and Social Collateral."
- Karlan, Dean, Melanie Morten, and Jonathan Zinman (2016). "A Personal Touch in Text Messaging Can Improve Microloan Repayment." *behavioral science*.
- Kling, J. R., S. Mullainathan, E. Shafir, L. C. Vermeulen, and M. V. Wrobel (2012). "Comparison Friction: Experimental Evidence from Medicare Drug Plans." *The Quarterly Journal of Economics* 127(1), 199–235.
- Kremer, Michael, Jean Lee, Jonathan Robinson, and Olga Rostapshova (2013). "Behavioral Biases and Firm Behavior: Evidence from Kenyan Retail Shops." *American Economic Review* 103(3), 362–368.

- Kuchler, Theresa and Michaela Pagel (2021). “Sticking to Your Plan: The Role of Present Bias for Credit Card Paydown.” *Journal of Financial Economics* 139(2), 359–388.
- Laibson, D. (1997). “Golden Eggs and Hyperbolic Discounting.” *The Quarterly Journal of Economics* 112(2), 443–478.
- Levine, Ross and Yona Rubinstein (2017). “Smart and Illicit: Who Becomes an Entrepreneur and Do They Earn More?” *The Quarterly Journal of Economics* 132(2), 963–1018.
- List, John A, Ian Muir, Devin Pope, and Gregory Sun (2023). “Left-Digit Bias at Lyft.” *Review of Economic Studies*, rdad014.
- Lucas, Robert E. (1978). “On the Size Distribution of Business Firms.” *The Bell Journal of Economics* 9(2), 508.
- Madrian, B. C. and D. F. Shea (2001). “The Power of Suggestion: Inertia in 401(k) Participation and Savings Behavior.” *Quarterly Journal of Economics* 116(4), 1149–1187.
- Mailchimp (2023). “Email Marketing Statistics and Benchmarks by Industry.” Tech. rep.
- Malmendier, Ulrike and Geoffrey Tate (2005). “CEO Overconfidence and Corporate Investment.” *The Journal of Finance* 60(6), 2661–2700.
- Malmendier, Ulrike and Geoffrey Tate (2008). “Who Makes Acquisitions? CEO Overconfidence and the Market’s Reaction.” *Journal of Financial Economics*.
- McKenzie, David and Christopher Woodruff (2017). “Business Practices in Small Firms in Developing Countries.” *Management Science* 63(9), 2967–2981.
- McMillan, John and Christopher Woodruff (1999). “Interfirm Relationships and Informal Credit in Vietnam.” *Quarterly Journal of Economics*.
- Mehrotra, Rahul, Vincent Somville, and Lore Vandewalle (2021). “Increasing Trust in Bankers to Enhance Savings: Experimental Evidence from India.” *Economic Development and Cultural Change* 69(2), 623–644.
- Mishra, Prachi, Nagpurnanand Prabhala, and Raghuram G Rajan (2022). “The Relationship Dilemma: Why Do Banks Differ in the Pace at Which They Adopt New Technology?” *Review of Financial Studies* 35(7), 3418–3466.
- Murphy, Kevin M., Andrei Shleifer, and Robert W. Vishny (1991). “The Allocation of Talent: Implications for Growth.” *The Quarterly Journal of Economics* 106(2), 503.
- O’Donoghue, Ted and Matthew Rabin (1999). “Doing It Now or Later.” *American Economic Review* 89(1).
- Olafsson, Arna and Michaela Pagel (2017). “The Ostrich in Us: Selective Attention to Financial Accounts, Income, Spending, and Liquidity.” Tech. rep. w23945. Cambridge, MA: National Bureau of Economic Research, w23945.
- Osili, Una Okonkwo and Anna Paulson (2014). “Crises and Confidence: Systemic Banking Crises and Depositor Behavior.” *Journal of Financial Economics* 111(3), 646–660.

- Osili, Una Okonkwo and Anna L. Paulson (2008). “Institutions and Financial Development: Evidence from International Migrants in the United States.” *Review of Economics and Statistics* 90(3), 498–517.
- Rigol, Natalia and Benjamin Roth (2021). “Loan Officers Impede Graduation from Microfinance: Strategic Disclosure in a Large Microfinance Institution.” Tech. rep. w29427. Cambridge, MA: National Bureau of Economic Research, w29427.
- Schoar, Antoinette (2010). “The Divide between Subsistence and Transformational Entrepreneurship.” *Innovation Policy and the Economy* 10, 57–81.
- Shue, Kelly and Richard R. Townsend (2021). “Can the Market Multiply and Divide? Non-Proportional Thinking in Financial Markets.” *Journal of Finance* 76(5), 2307–2357.
- Strulov-Shlain, Avner (2022). “More Than a Penny’s Worth: Left-Digit Bias and Firm Pricing.” *Review of Economic Studies*, rdac082.
- Tasoff, Joshua and Robert Letzler (2014). “Everyone Believes in Redemption: Nudges and Overoptimism in Costly Task Completion.” *Journal of Economic Behavior & Organization* 107, 107–122.
- Verhoogen, Eric (2023). “Firm-Level Upgrading in Developing Countries.” *Journal of Economic Literature* 61(4), 1410–1464.

Table 1: Baseline Treatment Balance

	Intercept	Deadline	Announced reminder	Unannounced reminder	2.75% Fee	Joint test <i>F</i> -stat
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Manager characteristics						
Female	0.440*** (0.005)	-0.002 (0.006)	0.003 (0.007)	-0.001 (0.007)	0.002 (0.006)	0.256 [0.937]
Age	39.457*** (0.12)	-0.039 (0.14)	0.244 (0.17)	0.186 (0.16)	-0.011 (0.13)	1.051 [0.385]
Panel B: Business characteristics						
<i>Business type</i>						
Small retailers	0.259*** (0.005)	0.001 (0.005)	-0.001 (0.007)	-0.001 (0.006)	0.000 (0.005)	0.037 [0.999]
Professionals	0.239*** (0.004)	0.001 (0.005)	-0.001 (0.006)	-0.001 (0.006)	0.000 (0.005)	0.014 [1.000]
Beauty	0.087*** (0.003)	0.002 (0.003)	0.000 (0.004)	0.000 (0.004)	0.000 (0.003)	0.065 [0.997]
Clothing	0.090*** (0.003)	0.000 (0.003)	0.000 (0.004)	0.000 (0.004)	0.000 (0.003)	0.027 [1.000]
Restaurants	0.122*** (0.003)	0.000 (0.004)	0.001 (0.005)	0.002 (0.005)	-0.001 (0.004)	0.038 [0.999]
Other	0.203*** (0.004)	-0.003 (0.005)	0.002 (0.006)	0.000 (0.006)	0.001 (0.004)	0.115 [0.989]
<i>Pre-treatment sales variables</i>						
Months since first transaction	24.101*** (0.17)	-0.074 (0.20)	0.101 (0.25)	0.120 (0.24)	0.120 (0.19)	0.173 [0.973]
% months business made sales	0.817*** (0.002)	0.001 (0.003)	0.001 (0.003)	-0.001 (0.003)	0.001 (0.003)	0.210 [0.958]
Log monthly card sales volume	8.783*** (0.012)	0.012 (0.013)	-0.011 (0.016)	0.014 (0.015)	-0.002 (0.012)	0.806 [0.545]
Log monthly card transactions	2.057*** (0.015)	0.003 (0.017)	-0.010 (0.021)	-0.003 (0.020)	0.006 (0.016)	0.137 [0.984]

This table reports differences in characteristics of the manager (typically the firm owner) and of the business by treatment group. The unit of observation is a firm, and each regression includes all firms in the experiment ($N = 33,978$). Each row shows coefficients from a regression of that row's characteristic on an intercept (column 1) and indicator variables for deadline (column 2), announced reminder (column 3), unannounced reminder (column 4), and 2.75% fee (column 5). Column (6) shows the F -statistic and corresponding p -value from an omnibus F -test of the coefficients on all treatment group dummies in that row's regression. Manager and business characteristics are defined when the user signs up for the technology. Pre-treatment sales variables include only card sales and are an average over all months from July 2019 to August 2020. Heteroskedasticity-robust standard errors are included in parentheses, and p -values for the F -statistics are in square brackets.

Table 2: Heterogeneous Treatment Effects by Number of Employees

	Firm accepted offer			
	(1)	(2)	(3)	(4)
Intercept	0.478*** (0.105)	0.571*** (0.133)	0.494*** (0.053)	0.486*** (0.083)
Above-median # of employees	0.022 (0.150)		0.068 (0.071)	
More than 1 employee		-0.120 (0.160)		0.056 (0.091)
Reminder	0.100 (0.111)	0.006 (0.145)		
Above-median # of employees × Reminder	0.065 (0.157)			
More than 1 employee × Reminder		0.181 (0.173)		
Announced reminder			0.169** (0.073)	0.190 (0.115)
Above-median # of employees × Announced reminder			0.025 (0.095)	
More than 1 employee × Announced reminder				-0.007 (0.126)
Number of firms	462	462	417	417
Mean heterogeneity variable	0.565	0.816	0.573	0.83

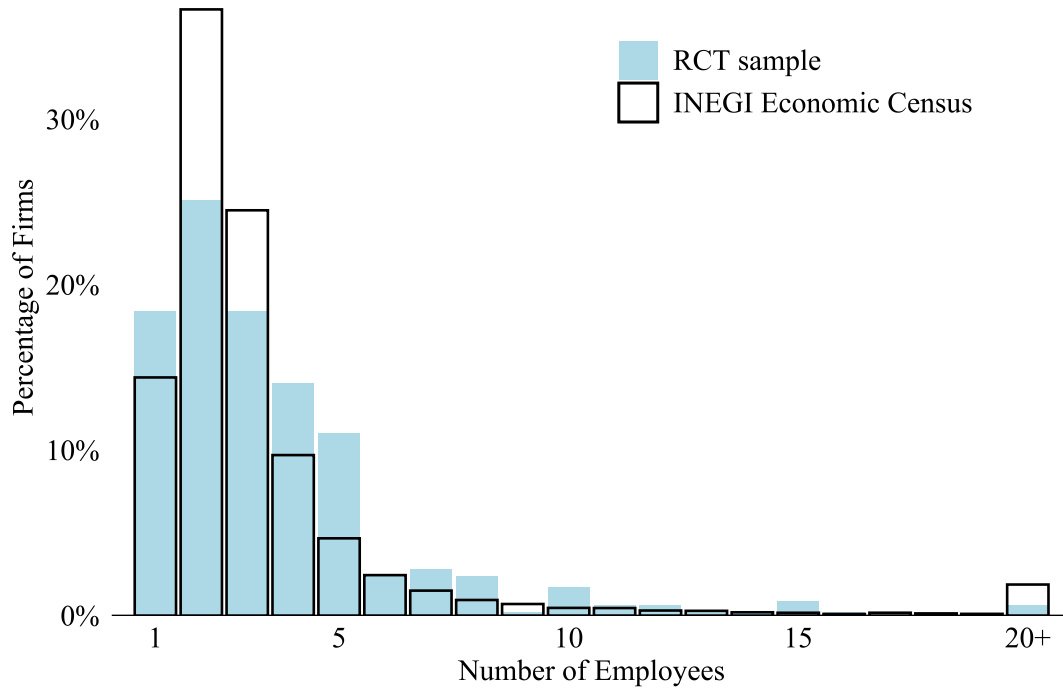
This table reports heterogeneous treatment effects of the reminder and announced reminder by number of employees. We use two measures for the number of employees, an indicator for above-median number of employees, defined as firms with ≥ 3 employees, and an indicator for more than one employee. The unit of observation is a firm. Data comes from survey conducted on a random sample of firms in the experiment ($N = 471$) and includes take-up from September 29 to March 31. All 471 firms in the survey were asked: “How many employees work in your business, including yourself?” 9 firms that did not answer the question and 1 firm from the control group were excluded from the sample. In the survey sample, the average number of employees is 3.9, the median is 3, and the standard deviation is 7.4. Column (1) includes all firms that provided an answer to the number-of-employees question, and column (2) includes only firms that received a reminder. Robust standard errors are included in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Monthly Sales Elasticity: Intent to Treat and Treatment on the Treated

	Log(sales + 1) (1)	Log(# transactions + 1) (2)	Continued using technology (3)
<u>Panel A: Intent to Treat</u>			
Post * Treated	0.101** (0.046)	0.030* (0.016)	0.013** (0.005)
<u>Panel B: Treatment on the Treated</u>			
Post * Accepted	0.351** (0.161)	0.102* (0.055)	0.043** (0.017)
Number of observations	662,162	662,162	662,162
Number of firms	33,978	33,978	33,978
Cluster std. errors	Firm	Firm	Firm
Fixed effects	Firm & month	Firm & month	Firm & month
Control mean (levels)	24,471	30.02	0.847
Control mean (levels, winsorized)	12,178	19.52	0.847

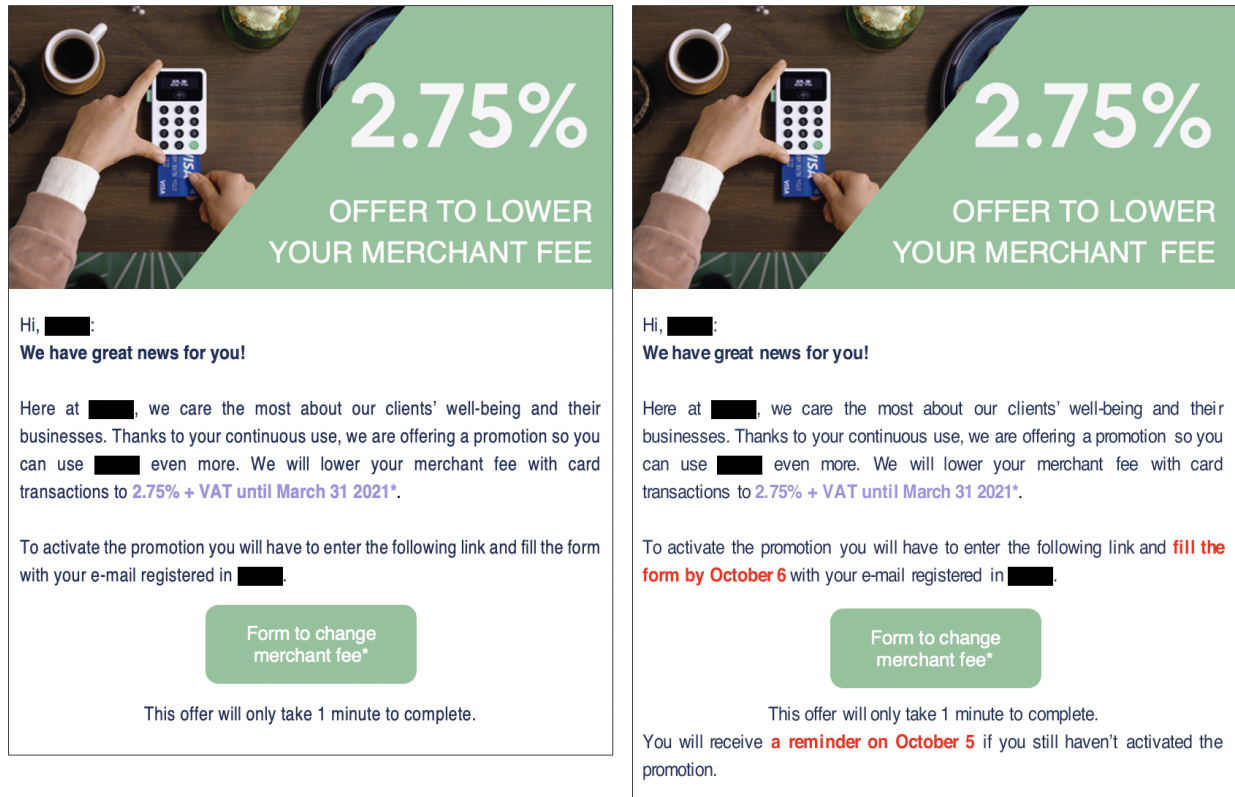
This table reports sales elasticities of the treated group (who received the offer) in Panel A and of the compliers (who accepted the offer) in Panel B. Data is from July 2019 to March 2021, includes Sep 29 and Sep 30 as part of October, and contains all firms in the experiment. The unit of observation is a firm-month. Post * Treated is an interaction term of Post and Treated, where 'Post' is an indicator for the time period is after the treated firms received the offer and 'Treated' is an indicator for firms who received the offer. Post * Accepted is an interaction term of Post and Adopted, where 'Accepted' is an indicator for firms that accepted the offer. Post * Accepted is instrumented by Post * Treated. Control means include data from the treatment period. Log average monthly sales volume and log average monthly transactions transform sales volume and transactions after winsorizing at the 95th percentile. Regressions include firm and month fixed effects. Clustered standard errors at the firm level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1: Number of Employees



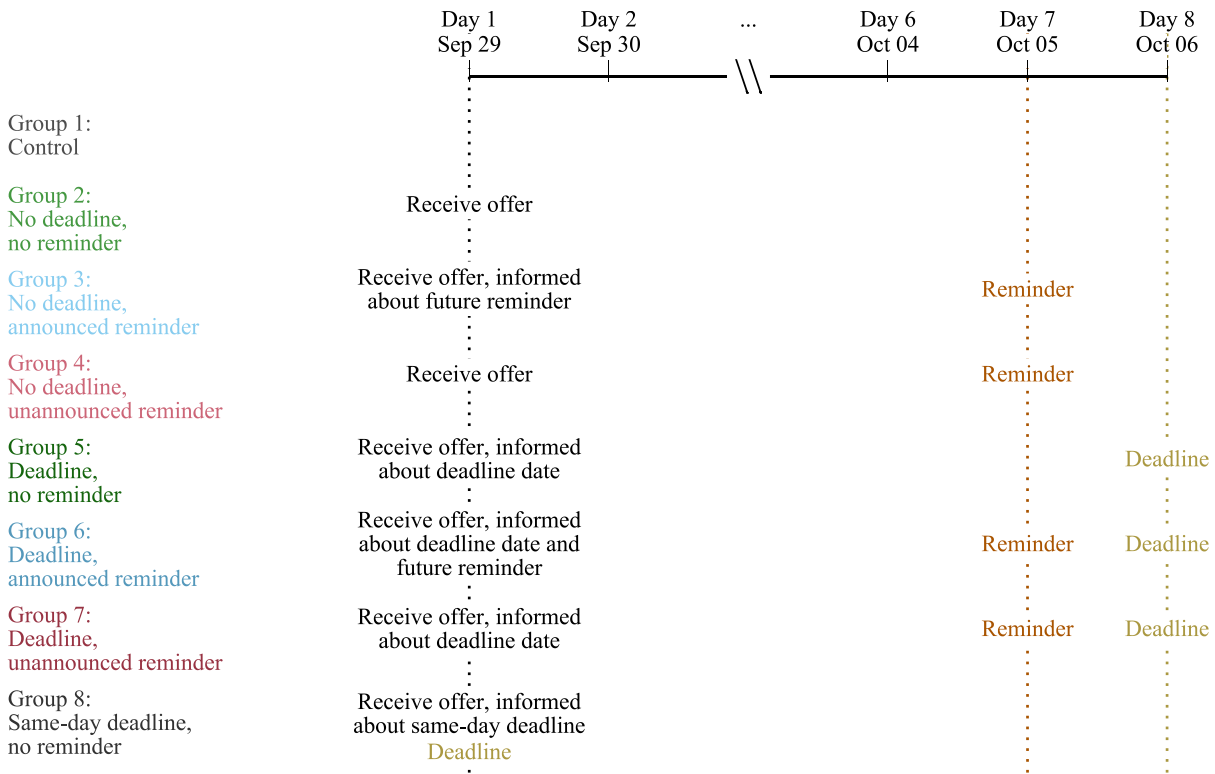
This figure shows the histograms of the number of employees by firm from our survey ($N = 462$) and from the 2019 INEGI Economic Census ($N = 5,360,215$). Our survey was conducted on a random sample of firms in the experiment ($N = 471$) and included the question: “*How many employees work in your business, including yourself?*” 9 firms that did not answer the survey question are excluded from the figure. We top-code the figure at 20 employees. 99.8% of firms in our sample and 98.2% of firms in the Economic Census have ≤ 20 employees. In the RCT subsample, the average number of employees is 3.9, the median is 3, the maximum 150, and the standard deviation 7.4.

Figure 2: Sample Emails with Lower Rate Offers



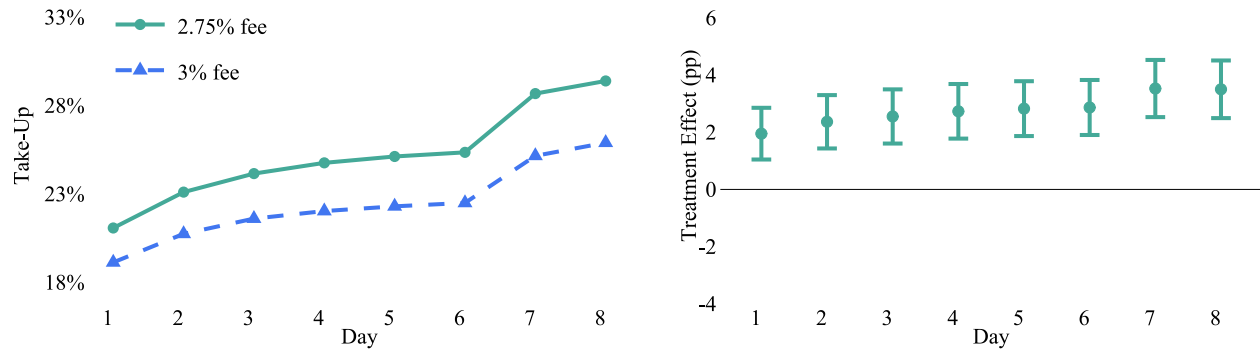
This figure shows screenshots of the emails sent to different treatment arms. The left panel shows the initial email sent to treatment groups with no deadline and either no or an unannounced reminder. The right panel shows the initial email sent to the deadline, announced-reminder arm. Additional versions of the email for other combinations of treatments were designed accordingly: for example, the no-deadline, announced-reminder arm would exclude the “fill the form by October 6” sentence but include the “reminder on October 5” sentence from the right panel. The text is translated from the original Spanish into English. Asterisks at the end of the purple text refer to the fine print at the bottom of the email, which is not included in the screenshots. The fine print reads: “By filling out the form you authorize [redacted] to change the fee on your [redacted] account to a 2.75% + VAT fee per successful card payment transaction until March 31, 2021. Starting April 1, 2021, the fee will revert back to the fee you had before activating this promotion. Terms and conditions apply.” The underlined text was a link that redirected to the FinTech company’s overall terms and conditions website, as there were no additional terms and conditions for the offer itself; nevertheless the company’s legal department insisted that a “terms and conditions” link had to be included in the email. Only 1.2% of firms that opened the email clicked on the “terms and conditions” link.

Figure 3: Timeline



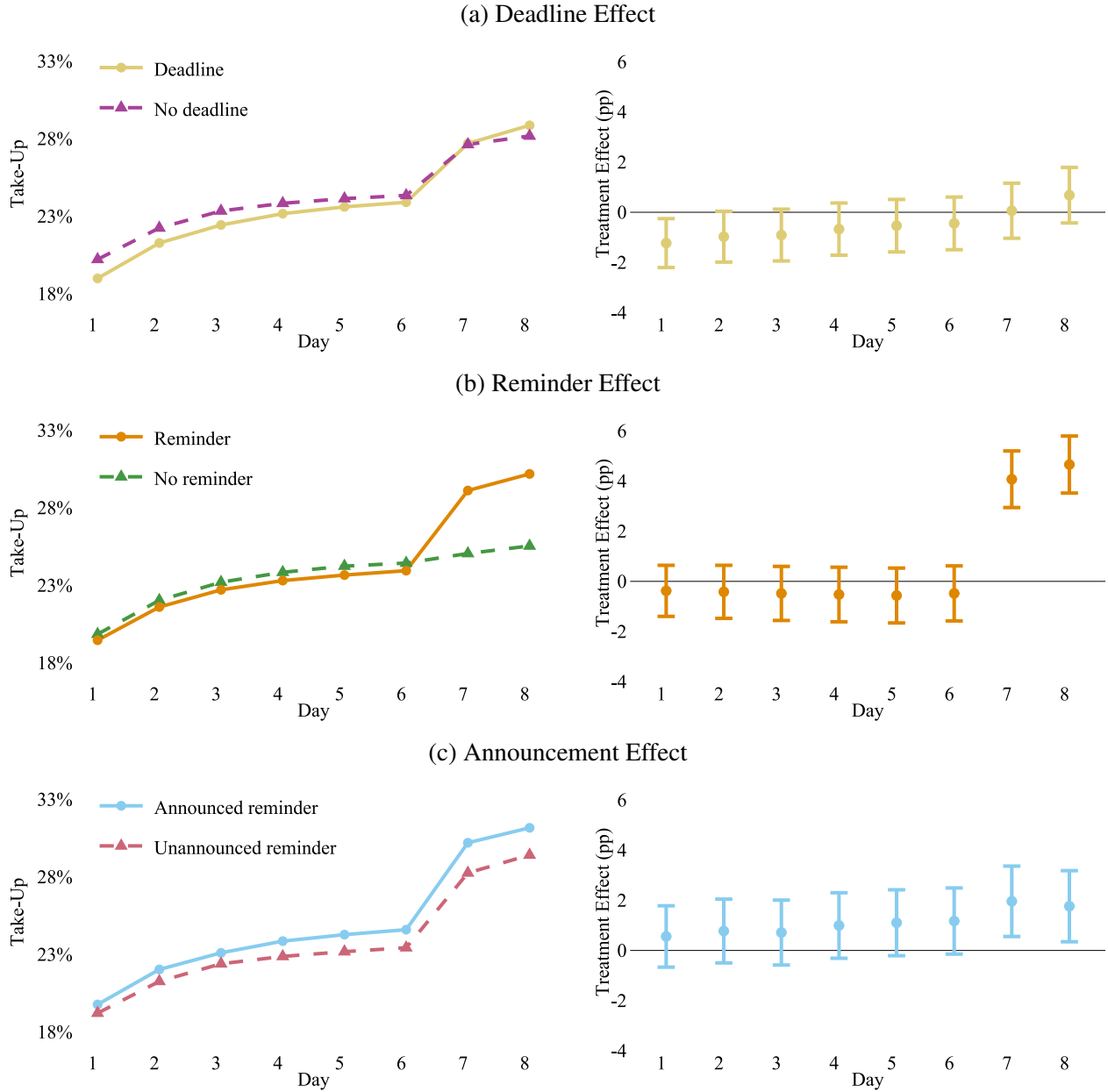
This figure shows the timing of the initial email, reminder, and deadline for each treatment arm. Days 3–5 (October 1–3, 2020) are omitted from the timeline to simplify the figure.

Figure 4: Effect of Offer Value on Take-up



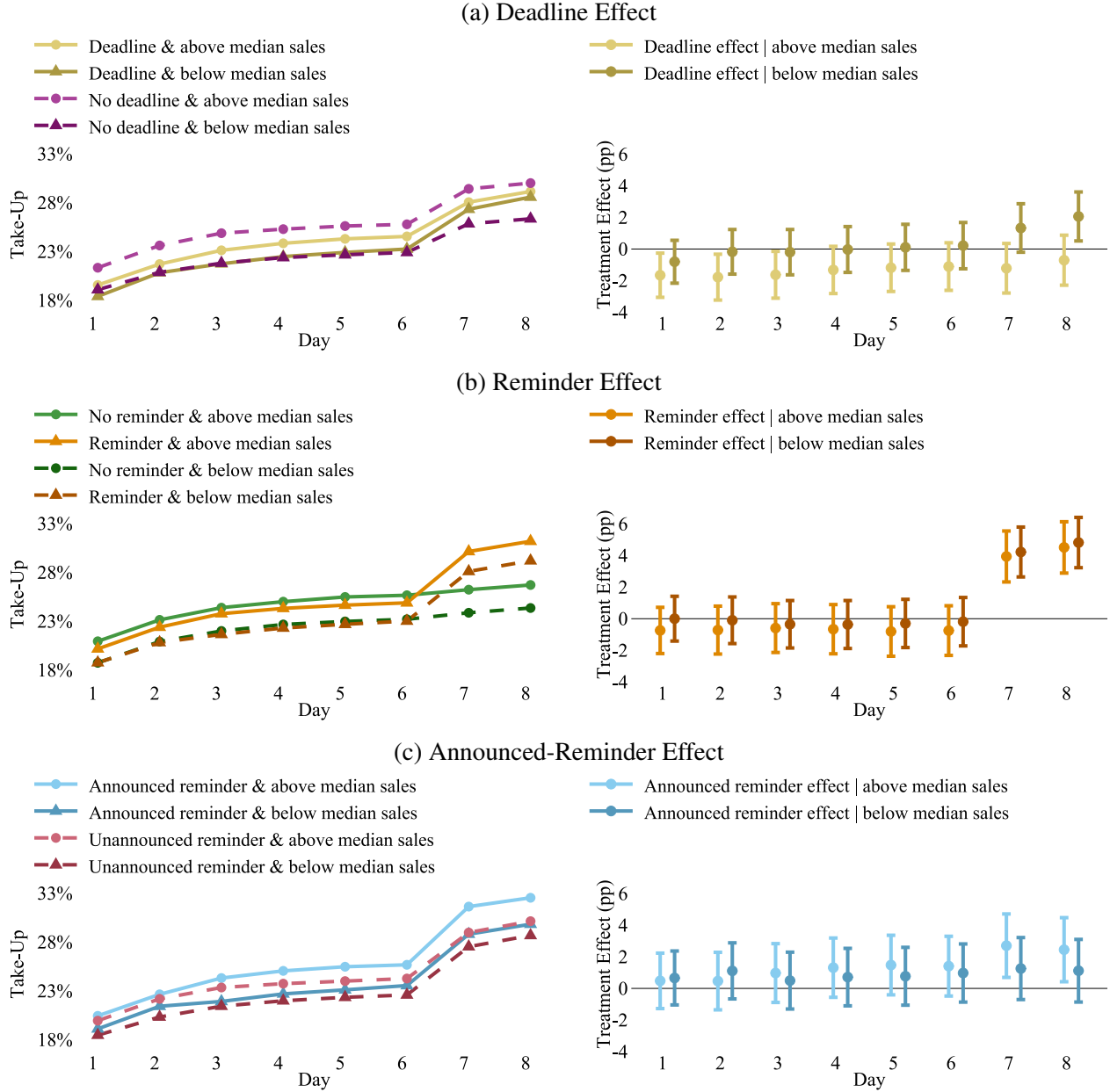
The line graph on the left shows average take-up rates for the 2.75% and 3% fee groups. The coefficient graph on the right shows the corresponding coefficient estimates for the effect of a more-valuable offer (2.75% rather than 3%) in percentage points (pp), separately for each day of the experiment. The unit of observation is a firm. Coefficient estimates and 95% confidence intervals come from daily regressions of cumulative take-up on an indicator of the fee group, with strata fixed effects. Data include take-up from September 29 to October 6 (the day of the deadline), among 29,968 firms with 2.75% and 3.00% offers.

Figure 5: Effect of Treatment on Take-Up



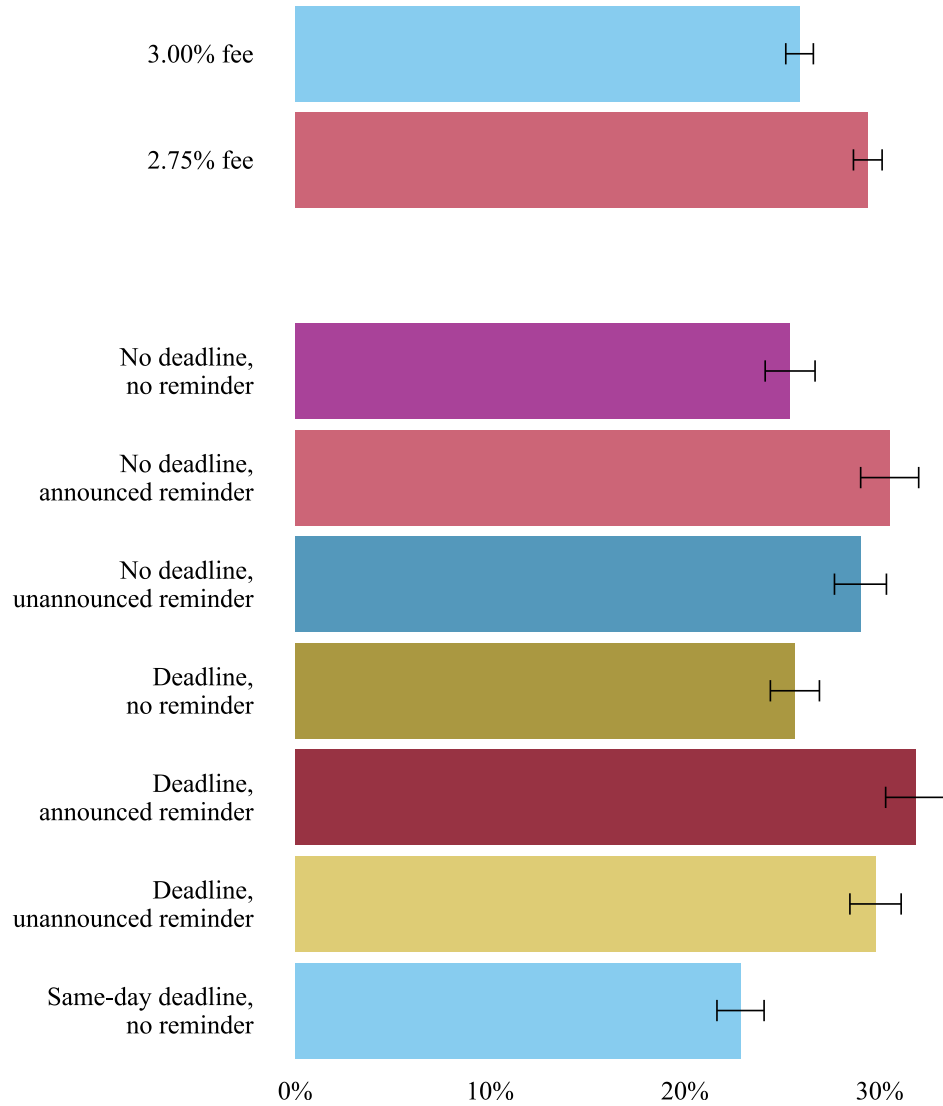
This figure shows take-up rates and treatment effects - Fixed by Mohammad] of the offer by deadline, reminder, and announcement. The unit of observation is a firm. Line graphs show average take-up rates by treatment group. Coefficient graphs show the corresponding coefficient estimates for the differential take-up of the groups, separately for each day of the experiment. Coefficient estimates and 95% confidence intervals come from daily regressions of cumulative take-up on treatment. Regressions include strata fixed effects. Data include take-up from September 29 to October 6 (the day of the deadline). Panel (a) includes 25,327 firms with and without deadlines, excluding the same-day deadline group. Panel (b) includes 18,155 firms with unannounced reminders and no reminders, excluding the same-day deadline group. Panel (c) includes 16,254 firms with announced and unannounced reminders, excluding firms without reminders.

Figure 6: Heterogeneous Treatment Effects by Baseline Sales



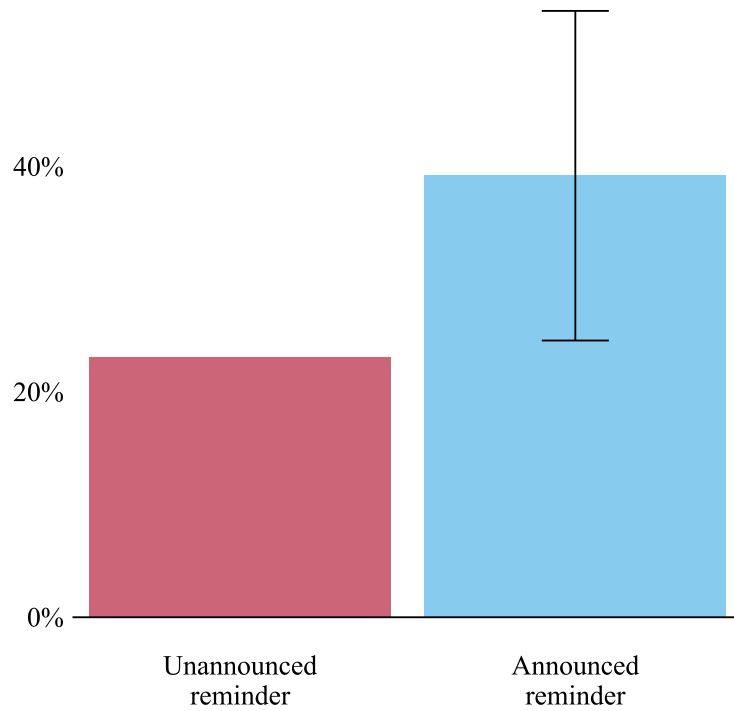
This figure shows take-up and heterogeneous treatment effects of the deadline, unannounced reminder and announced reminder, separately for the subsamples with above and below-median baseline sales. The unit of observation is a firm. Line graphs show average take-up rates by treatment and baseline-sales group. Coefficient graphs show the corresponding coefficient estimates for the differential take up of the reminder groups, separately for each day of the experiment. Coefficient estimates and 95% confidence intervals come from daily regressions of cumulative take-up on treatment, a dummy indicating above-median baseline sales, and the interaction between treatment and the above-median baseline sales dummy, controlling for strata fixed effects. Data includes take-up from September 29 to October 6 (the day of the deadline). Panel (a) includes 25,327 firms with and without deadlines, excluding the same-day deadline group. Panel (b) includes 25,327 firms with reminders and no reminders, excluding the same-day deadline group. Panel (c) includes 16,254 firms with announced and unannounced reminders, excluding firms without reminders.

Figure 7: Take-up by Treatment Arm



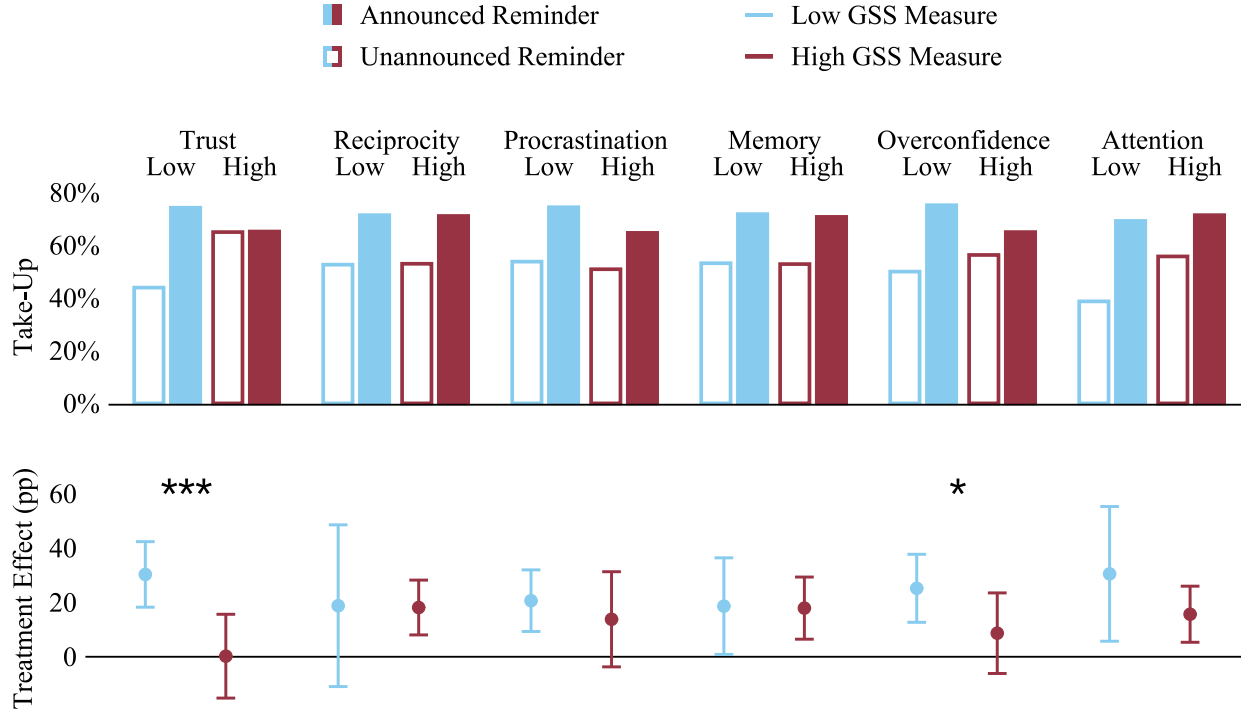
This figure shows cumulative take-up by day 8 (October 6), which is the day of the deadline for firms that had a deadline, separately for each treatment arm. The two fee arms in the upper panel of the figure are cross-randomized with the deadline and reminder arms in the lower panel. The coefficients and 95% confidence intervals are estimated using a version of model (6) where we regress cumulative take-up by day 8 on strata fixed effects and, in the upper panel, dummies for the 2.75% and 3% fee arms or, in the lower panel, dummies for the various deadline and no-deadline treatment arms along with the same-day deadline arm. (In both panels, the control group that did not receive an offer is the omitted dummy.) The figure uses data from the full sample of firms ($N = 33,978$).

Figure 8: Effect of Announced Reminder on Perceived Offer Value



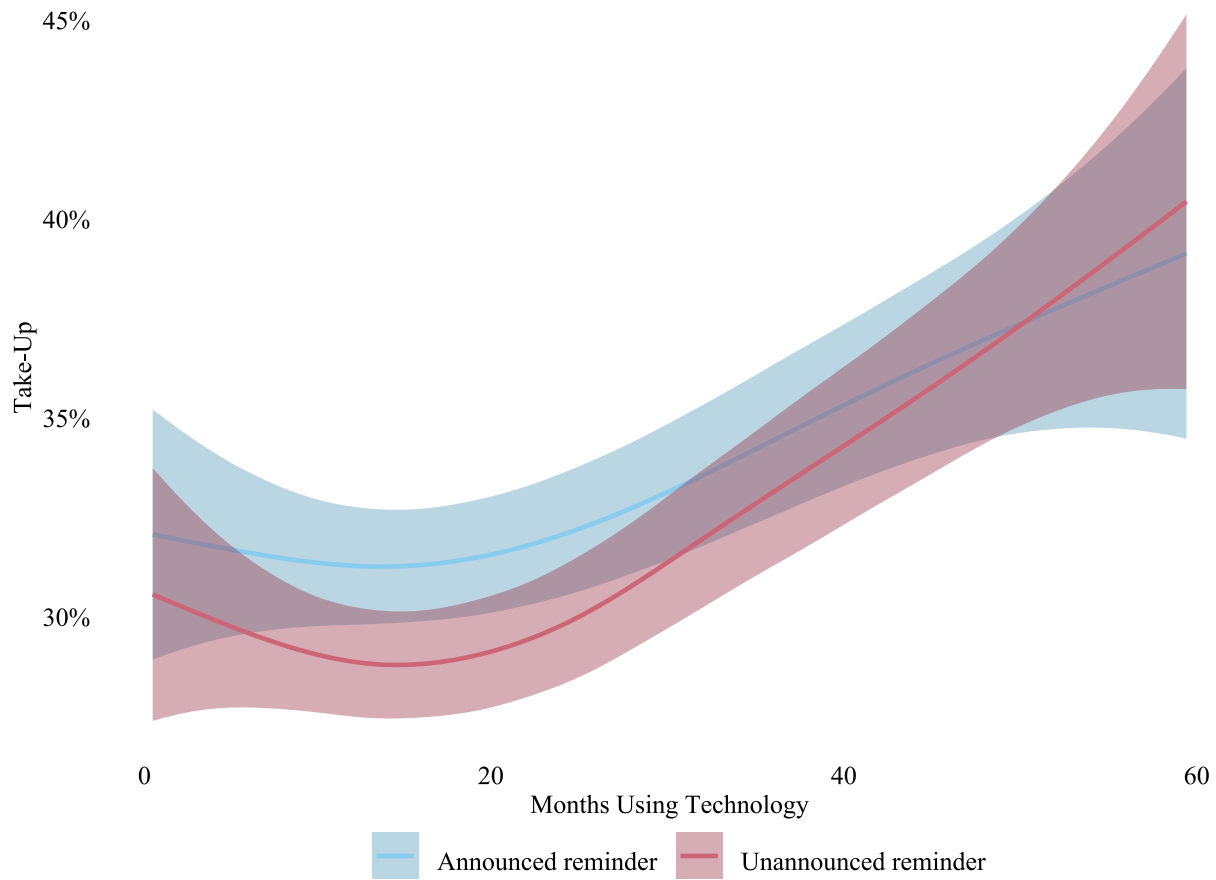
This figure contains a barplot with the percentage of firms that answered yes to the survey question: “*Did the reminder change your perception of the offer’s value?*”, separately by reminder type. Data comes from survey conducted on a random sample of firms in the experiment ($N = 471$), with this question asked to the 157 firms that recall receiving the first email or SMS, recall receiving a reminder, received an offer with a reminder, and accepted the offer after receiving the reminder or did not accept the offer. 5 firms that did not know the answer to the question were excluded from the sample. Coefficient estimates and 95% confidence intervals come from a regression of offer value change on an indicator for announced reminder, with heteroskedasticity-robust standard errors.

Figure 9: Heterogeneous Effects of Announced Reminder by GSS Measures



This figure shows heterogeneous take-up rates of the unannounced and announced reminder, and treatment effects (in pp) of the announced relative to the unannounced reminder, by each of the six GSS survey sample splits. The survey question asked respondents whether they agreed or disagreed with the following six statements: (1) *Trust*: I trust advertised offers. (2) *Reciprocity*: I am more inclined to do business with people who live up to their promises. (3) *Procrastination*: I tend to postpone tasks, even when I know it is better to do them immediately. (4) *Memory*: I tend to have good memory about pending tasks that I have to do and complete. (5) *Overconfidence*: I tend to think my memory is better than it really is. (6) *Attention*: I can focus completely when I have to finish a task. The scale of these responses is 1 to 5, where 5 is highest level of agreement and 1 highest level of disagreement. Binary measure variables were created from these responses, coding 4 and 5 (agree and completely agree) as 1 and 1–3 (completely disagree, disagree and neither agree nor disagree) as 0. The upper graph plots cumulative take-up rates (in %) from September 29 to March 31 separately by treatment arm, $\mathbb{1}(\text{Announced reminder})_i \in \{0, 1\}$ (empty versus filled bars), and by value of the survey measure, $\mathbb{1}(\text{High survey measure})_i \in \{0, 1\}$ (blue versus red bars). Note that take-up rates in the survey are higher than in the administrative data because we oversampled firms that accepted the offer to be better powered. The lower graph shows the corresponding estimated effect of announced reminders by survey measure, where the coefficients in the “Low” columns correspond to $\hat{\beta}_2$ from estimating equation (7) and the coefficients in the “High” columns correspond to $\hat{\beta}_2 + \hat{\beta}_3$. Above each pair of treatment effects we show the statistical significance of $\hat{\beta}_3$. (The $\hat{\beta}_3$ coefficient estimates and standard errors are in Appendix-Table C.12.) The unit of observation is a firm. Data include firms with announced and unannounced reminders in the survey sample. 43 firms that did not answer the question were excluded from the sample. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 10: Effect of Announced Reminders by Length of Business Relationship



This figure plots take-up rates of the offer against the number months firms have been using the FinTech technology, split by reminder type. The data include firms from the announced- and the unannounced-reminder groups, and take-up is measured from September 29 to March 31. Colored lines are local polynomial regression fits, and shaded polygons are 95% confidence intervals. The blue line shows take-up in the announced-reminder arm and the red line shows take-up in the unannounced-reminder arm. Thus, the difference between the blue and red lines is the treatment effect of the announced reminder relative to the unannounced reminder. Firms above the 95th percentile of months using the technology were omitted from the graph for legibility.

Internet Appendix

A Model Proofs

This appendix includes proofs of some properties of the equilibrium described in Section 2.2, as well as the model predictions in Section 2.3.

A.1 Equilibrium Properties

Proposition 1 (Optimal Strategy). *The optimal strategy is to adopt in period t if and only if the cost draw is below c_t^* as defined by equations (3) and (4).*

Proof. Denote the agent's decision of whether to adopt in period t as s_t and their perceived future strategies (as well as their perception of their successor's future strategies) as of time t as $\{\hat{s}_{t+k}^t\}_{k=1}^\infty$. We use $u_\tau^t(\hat{s}_\tau^t)$ to denote the instantaneous utility the firm owner believes as of time t to receive for taking the action they perceive they are going to take at time τ , \hat{s}_τ^t . Note that for $\tau > t$, this differs from the instantaneous utility they would actually receive by taking action \hat{s}_τ^t in period τ if they are present biased, hence the need for the superscripts in u_τ^t . We can then write the agent's expected utility of the perceived actions at time t as:

$$V_t = u_t^t(s_t) + \beta \mathbb{E}_t \left[\sum_{k=1}^\infty \left(\prod_{j=1}^k \hat{\rho}_{t+j} \right) \delta^k u_{t+k}^t(\hat{s}_{t+k}^t) \right]. \quad (9)$$

In a perception-perfect equilibrium, the agent chooses the optimal s_t to maximize V_t under a dynamically consistent belief that $\{\hat{s}_{t+k}^t\}_{k=1}^\infty$ is the perceived strategies of the future selves at time t , i.e., there exists a sequence of future beliefs $\{\{\hat{s}_{t+k}^t\}_{k=1}^\infty\}_{\tau > t}$ such that they are:

1. internally consistent, i.e., for all $\tau > t$,

$$\hat{s}_\tau^t \in \arg \max_a u_\tau^\tau(a) + \hat{\beta} \mathbb{E}_t \left[\sum_{k=1}^\infty \left(\prod_{j=1}^k \hat{\rho}_{t+j} \right) \delta^k u_{\tau+k}^\tau(\hat{s}_{\tau+k}^t) \right],$$

where $\hat{\beta}$ enters instead of β since the agent believes at time t that all future selves in $\tau > t$ will have present bias level $\hat{\beta}$; and

2. externally consistent, i.e., for all $t < t' < \tau$, $\hat{s}_\tau^t = \hat{s}_\tau^{t'}$.

The external consistency allows us to drop the superscript in the notation \hat{s}_{t+k}^t , so we now simply write \hat{s}_{t+k} . Similarly, note that the instantaneous utility functions are also externally consistent,

i.e., $u_\tau^t = u_\tau^{t'}$ for all $t < t' < \tau$, so we may also drop the superscripts on u_τ^t for $t < \tau$ and write it as \hat{u}_τ , where we add a hat to denote the perception as of time t about future instantaneous utility. For $\tau = t$, we may also suppress the superscript and write $u_\tau^t = u_\tau^\tau$ as u_τ since it is the instantaneous utility in the current period.

The internal consistency condition exhibits a recursive structure. To see this, note that we can write the continuation value as

$$\hat{V}_{t+1} = \hat{u}_{t+1}(\hat{s}_{t+1}) + \mathbb{E}_{t+1} \left[\sum_{k=1}^{\infty} \left(\prod_{j=1}^k \hat{\rho}_{t+1+j} \right) \delta^k \hat{u}_{t+1+k}(\hat{s}_{t+1+k}) \right],$$

where β and $\hat{\beta}$ do not enter because $t+1$ is already in the future. Then we can rewrite the internal consistency condition as:

$$\hat{s}_t \in \arg \max_a u_t(a) + \hat{\beta} \hat{\rho}_{t+1} \delta \mathbb{E}_t[\hat{V}_{t+1}].$$

Plugging in the utility form, this condition states that the future self at time $\tau > t$ is *perceived* to receive a utility of $\hat{\beta} \delta \alpha_\tau y - c_\tau$ if she adopts, and $\hat{\beta} \delta \hat{\rho}_{\tau+1} \mathbb{E}_\tau[\hat{V}_{\tau+1}]$ otherwise, and to act optimally. Therefore, the optimal threshold in the perceived strategy of the future self in period $\tau > t$ is

$$\hat{c}_\tau^* = \hat{\beta} \delta (\alpha_\tau y - \hat{\rho}_{\tau+1} \mathbb{E}_\tau[\hat{V}_{\tau+1}]), \quad (10)$$

which has a hat on \hat{c}_τ^* since it is a direct function of $\hat{\beta}$.

Similarly, the optimality condition for the agent at time t is

$$s_t \in \arg \max_a u_t(a) + \beta \hat{\rho}_{t+1} \delta \mathbb{E}_t[\hat{V}_{t+1}].$$

At time t , the agent receives a utility of $\beta \delta \alpha_t y - c_t$ if she adopts, and $\beta \hat{\rho}_{t+1} \delta \mathbb{E}_t[\hat{V}_{t+1}]$ otherwise. Therefore, the optimal threshold for the agent at time t deciding whether to adopt is

$$c_t^* = \beta \delta (\alpha_t y - \hat{\rho}_{t+1} \mathbb{E}_t[\hat{V}_{t+1}]),$$

which has no hat on the c_t^* since it is a direct function of β rather than $\hat{\beta}$.

As of time t , we can then write the continuation value for period $t+1$ as follows (where β and $\hat{\beta}$ do not enter because the future payoffs are already being discounted by β by the present self at time $t' \leq t$):

$$\hat{V}_{t+1} = \begin{cases} \delta \alpha_{t+1} y - c_{t+1} & \text{if } c_{t+1} \leq \hat{c}_{t+1}^*, \\ \delta \hat{\rho}_{t+2} \mathbb{E}_{t+1}[\hat{V}_{t+2}] & \text{if } c_{t+1} > \hat{c}_{t+1}^*. \end{cases} \quad (11)$$

Taking expectations of equation (11) over cost draws,

$$\mathbb{E}_t[\hat{V}_{t+1}] = \int_{\underline{c}}^{\hat{c}_{t+1}^*} (\delta\alpha_{t+1}y - c) dF(c) + \int_{\hat{c}_{t+1}^*}^{\bar{c}} \delta\hat{\rho}_{t+2} \mathbb{E}_{t+1}[\hat{V}_{t+2}] dF(c), \quad (12)$$

which is equivalent to equation (4) by the fundamental theorem of calculus. \square

The following lemma will be useful in many of the subsequent proofs. For ease of notation, from now on we assume a constant α and $\hat{\rho}$ over time, but allow ρ_t to vary over time as the probability of remembering in a particular period will be affected by receiving a reminder in that period when we turn to the model predictions.

Lemma 1 (Monotonicity of Continuation Values). *For a sufficiently large $\hat{\beta}$, a higher continuation value in period $t + 1$ implies a higher continuation value in period t .*

Proof. Taking the partial derivative of equation (4) with respect to $\mathbb{E}_{t+1}[\hat{V}_{t+2}]$,

$$\begin{aligned} \frac{\partial \mathbb{E}_t[\hat{V}_{t+1}]}{\partial \mathbb{E}_{t+1}[\hat{V}_{t+2}]} &= (\delta\alpha y - \hat{c}_{t+1}^*) f(\hat{c}_{t+1}^*) \frac{d\hat{c}_{t+1}^*}{d\mathbb{E}_{t+1}[\hat{V}_{t+2}]} - f(\hat{c}_{t+1}^*) \frac{d\hat{c}_{t+1}^*}{d\mathbb{E}_{t+1}[\hat{V}_{t+2}]} \delta\hat{\rho} \mathbb{E}_{t+1}[\hat{V}_{t+2}] \\ &\quad + (1 - F(\hat{c}_{t+1}^*)) \delta\hat{\rho}. \end{aligned} \quad (13)$$

Next, taking derivatives of equation (10) for $\tau = t + 1$ with respect to $\mathbb{E}_{t+1}[\hat{V}_{t+2}]$, we have that $d\hat{c}_{t+1}^*/d\mathbb{E}_{t+1}[\hat{V}_{t+2}] = -\beta\delta\hat{\rho}$. Rearranging equation (10) with $\tau = t + 1$ to express $\mathbb{E}_{t+1}[\hat{V}_{t+2}]$ as a function of \hat{c}_{t+1}^* , then plugging this and $d\hat{c}_{t+1}^*/d\mathbb{E}_{t+1}[\hat{V}_{t+2}] = -\beta\delta\hat{\rho}$ into the right-hand side of equation (13) and rearranging terms:

$$\frac{\partial \mathbb{E}_t[\hat{V}_{t+1}]}{\partial \mathbb{E}_{t+1}[\hat{V}_{t+2}]} = \left[(1 - F(\hat{c}_{t+1}^*)) - (1 - \hat{\beta}) \hat{c}_{t+1}^* f(\hat{c}_{t+1}^*) \right] \delta\hat{\rho}. \quad (14)$$

Thus $\partial \mathbb{E}_t[\hat{V}_{t+1}]/\partial \mathbb{E}_{t+1}[\hat{V}_{t+2}] \geq 0$ if $\kappa \equiv (1 - F(\hat{c}_{t+1}^*)) - (1 - \hat{\beta}) \hat{c}_{t+1}^* f(\hat{c}_{t+1}^*) \geq 0$. This holds for $\hat{\beta} = 1$, and by continuity it also holds for sufficiently large $\hat{\beta} < 1$. \square

Proposition 2 (Existence and Uniqueness). *An equilibrium of the model uniquely exists (for $\hat{\beta}$ sufficiently large that monotonicity of continuation values holds).*

Proof. By Proposition 1, an optimal strategy of the future self does not deviate from an optimal threshold strategy at a positive probability, and thus it suffices to only consider the threshold strategies for the perceived actions of future selves. Now, the claim to be proven is that an optimal threshold strategy, characterized by the sequence of optimal thresholds $\{c_t^*\}$ with $\hat{c}_t^* \in [\underline{c}, \bar{c}]$ for each t uniquely exists for each future self.

We first construct such a sequence of thresholds by approximating the given no-deadline problem with problems of sufficiently long deadlines and then prove that the corresponding actions satisfy the given dynamic and satisfy it uniquely. Formally, denote $\{\mathbb{E}_t[\hat{V}_{t+1}^T]\}_{t \leq T}$ as the sequence of continuation values defined by equation (12) when the deadline is at date T , with the terminal condition that $\hat{\rho}_{T+1}\mathbb{E}_T[\hat{V}_{T+1}^T] = 0$. Denote the optimal cost thresholds induced by these values as \hat{c}_t^T and c_t^T ; the superscripts denote that the deadline is at date T .

As $T \rightarrow \infty$, the continuation values $\mathbb{E}_t[V_{t+1}^T]$ converge. This is because for a given date t and sufficiently large $T_1 < T_2$, it holds that

$$\left| \mathbb{E}_t[\hat{V}_{t+1}^{T_1}] - \mathbb{E}_t[\hat{V}_{t+1}^{T_2}] \right| \leq \delta \left| \mathbb{E}_{t+1}[\hat{V}_{t+2}^{T_1}] - \mathbb{E}_{t+1}[\hat{V}_{t+2}^{T_2}] \right| \leq \delta^{T_1-t-1} \left| \mathbb{E}_{T_1}[\hat{V}_{T_1+1}^{T_2}] \right|,$$

which can be arbitrarily small since the last expectation is bounded by δy . Note that the first inequality is based on the assumption that $\hat{\beta}$ is sufficiently large that monotonicity of continuation values holds (Lemma 1), so that

$$0 \leq \frac{\partial \mathbb{E}_t[\hat{V}_{t+1}]}{\partial \mathbb{E}_{t+1}[\hat{V}_{t+2}]} = \left(1 - F(\hat{c}_{t+1}^*) - (1 - \hat{\beta})\hat{c}_{t+1}^* f(\hat{c}_{t+1}^*) \right) \delta \hat{\rho}_{t+2} \leq \delta.$$

Now, by Cauchy's sequence criterion, the sequence $\{\mathbb{E}_t[\hat{V}_{t+1}^T]\}$ converges, and so do the sequences $\{\hat{c}_t^T\}$ and $\{c_t^T\}$. Therefore, we can take their limits as $\{\mathbb{E}_t[\hat{V}_{t+1}]\}$, $\{\hat{c}_t^*\}$, and $\{c_t^*\}$.

Note that both sides of equations (10) and (12) are continuous functions. Therefore, it follows that $\{\mathbb{E}_t[\hat{V}_{t+1}]\}$, $\{\hat{c}_t^*\}$, and $\{c_t^*\}$ satisfy (10) and (12) by taking the limits of each side. \square

Corollary 1. *As the deadline increases to infinity, the equilibrium thresholds will converge to the no-deadline case.*

This corollary implies that we can approximate the no-deadline case by a deadline case with a sufficiently long deadline.

A.2 Proofs of Model Predictions

A sufficiently large $\hat{\beta}$ such that monotonicity of continuation values holds (Lemma 1) is a condition for all predictions except Prediction 4. For conciseness we do not write out this condition in the statement of each prediction.

Prediction 1 (Offer Value and Take-Up). *A higher value of the offer (higher y) increases take-up.*

Proof. Take-up is higher if the optimal cost threshold is higher in each period, so we seek to prove that $dc_t^*/dy > 0$.

Taking derivatives of equation (3) with respect to y ,

$$\frac{dc_t^*}{dy} = \beta \delta \left(\alpha - \hat{\rho} \frac{d\mathbb{E}_t[\hat{V}_{t+1}]}{dy} \right). \quad (15)$$

Thus a sufficient condition for $dc_t^*/dy > 0$ is that $d\mathbb{E}_t[\hat{V}_{t+1}]/dy < \alpha/\hat{\rho}$, which is what we will show.

Taking derivatives of equation (4) with respect to y ,

$$\begin{aligned} \frac{d\mathbb{E}_t[\hat{V}_{t+1}]}{dy} &= (\delta \alpha y - \hat{c}_{t+1}^*) f(\hat{c}_{t+1}^*) \frac{d\hat{c}_{t+1}^*}{dy} + \delta \alpha F(\hat{c}_{t+1}^*) \\ &\quad - f(\hat{c}_{t+1}^*) \frac{d\hat{c}_{t+1}^*}{dy} \delta \hat{\rho} \mathbb{E}_{t+1}[\hat{V}_{t+2}] + (1 - F(\hat{c}_{t+1}^*)) \delta \hat{\rho} \frac{d\mathbb{E}_{t+1}[\hat{V}_{t+2}]}{dy}. \end{aligned} \quad (16)$$

Rearranging equation (10) with $\tau = t + 1$ to express $\mathbb{E}_{t+1}[\hat{V}_{t+2}]$ as a function of \hat{c}_{t+1}^* , plugging this into equation (16), and rearranging terms:

$$\frac{d\mathbb{E}_t[\hat{V}_{t+1}]}{dy} = \left(\hat{\beta}^{-1} - 1 \right) \frac{d\hat{c}_{t+1}^*}{dy} f(\hat{c}_{t+1}^*) \hat{c}_{t+1}^* + \delta \alpha F(\hat{c}_{t+1}^*) + (1 - F(\hat{c}_{t+1}^*)) \delta \hat{\rho} \frac{d\mathbb{E}_{t+1}[\hat{V}_{t+2}]}{dy}. \quad (17)$$

Taking derivatives of equation (10) for $\tau = t + 1$ with respect to y , and plugging $d\hat{c}_{t+1}^*/dy$ into equation (17):

$$\begin{aligned} \frac{d\mathbb{E}_t[\hat{V}_{t+1}]}{dy} &= \left(\hat{\beta}^{-1} - 1 \right) \left(\hat{\beta} \delta \left(\alpha - \hat{\rho} \frac{d\mathbb{E}_{t+1}[\hat{V}_{t+2}]}{dy} \right) \right) f(\hat{c}_{t+1}^*) \hat{c}_{t+1}^* \\ &\quad + \delta \alpha F(\hat{c}_{t+1}^*) + (1 - F(\hat{c}_{t+1}^*)) \delta \hat{\rho} \frac{d\mathbb{E}_{t+1}[\hat{V}_{t+2}]}{dy} \\ &= \left(F(\hat{c}_{t+1}^*) + (1 - \hat{\beta}) \hat{c}_{t+1}^* f(\hat{c}_{t+1}^*) \right) \delta \alpha \\ &\quad + \left(1 - F(\hat{c}_{t+1}^*) - (1 - \hat{\beta}) \hat{c}_{t+1}^* f(\hat{c}_{t+1}^*) \right) \delta \hat{\rho} \frac{d\mathbb{E}_{t+1}[\hat{V}_{t+2}]}{dy} \\ &= (1 - \kappa) \delta \alpha + \kappa \delta \hat{\rho} \frac{d\mathbb{E}_{t+1}[\hat{V}_{t+2}]}{dy}, \end{aligned} \quad (18)$$

where $\kappa \equiv 1 - F(\hat{c}_{t+1}^*) - (1 - \hat{\beta}) \hat{c}_{t+1}^* f(\hat{c}_{t+1}^*)$. Note that $\kappa \leq 1$ always, and for sufficiently large $\hat{\beta}$ such that monotonicity of continuation values holds (Lemma 1), $0 \leq \kappa \leq 1$. At the deadline $t = T$, $d\mathbb{E}_T[\hat{V}_{T+1}]/dy = 0$, as there is no continuation value since the task expires. Plugging this into the right side of equation (18) for $t = T - 1$, $d\mathbb{E}_{T-1}[\hat{V}_T]/dy = (1 - \kappa) \delta \alpha \leq \delta \alpha$. By induction, we can

prove that $d\mathbb{E}_t[\hat{V}_{t+1}]/dy \leq \delta\alpha$:

$$\begin{aligned}\frac{d\mathbb{E}_t[\hat{V}_{t+1}]}{dy} &= (1 - \kappa)\delta\alpha + \kappa\delta\hat{\rho}\frac{d\mathbb{E}_{t+1}[\hat{V}_{t+2}]}{dy} \\ &\leq (1 - \kappa)\delta\alpha + \kappa\delta\hat{\rho}\delta\alpha \\ &\leq (1 - \kappa)\delta\alpha + \kappa\delta\alpha = \delta\alpha.\end{aligned}$$

Thus we have:

$$\hat{\rho}\frac{d\mathbb{E}_t[\hat{V}_{t+1}]}{dy} \leq \hat{\rho}\delta\alpha \leq \delta\alpha \leq \alpha,$$

so $d\mathbb{E}_t[\hat{V}_{t+1}]/dy \leq \alpha/\hat{\rho}$, with strict inequality as long as at least one of $\delta < 1$, $\hat{\rho} < 1$, or $\kappa > 0$ holds strictly. \square

Prediction 2 (Offer Value and Deadline). *A higher value of the offer (higher y) implies a lower treatment effect of a deadline on cumulative adoption by the deadline (for y sufficiently large that the firm owner without a deadline still adopts with positive probability, and ρ_t and T sufficiently large).*

Proof. Denote $\mathbb{E}_{t-1}[V_t]$ by v_t . Equation (12) can then be written as

$$v_t = \int_{\underline{c}}^{\hat{c}_t^*} (\delta\alpha y - c) dF(c) + \int_{\hat{c}_t^*}^{\bar{c}} \delta\hat{\rho}v_{t+1} dF(c),$$

where

$$\hat{c}_t^* = \hat{\beta}\delta(\alpha y - \hat{\rho}v_{t+1}).$$

Assume that $\rho_t = 1$; for cases with ρ_t sufficiently large (i.e., sufficiently close to 1), the following arguments hold by continuity in ρ_t . In this special case with $\rho_t = 1$, the cumulative adoption rate by period T is

$$P(\{c_t^*\}_{t=1}^T) = 1 - \prod_{t=1}^T (1 - F(c_t^*)),$$

where

$$c_t^* = \beta\delta(\alpha y - \hat{\rho}v_{t+1}).$$

We now introduce the notation of a tilde indicating the no-deadline case. When there is a deadline at date T , $v_{T+1} = 0$, while in general, without a deadline, $\tilde{v}_{T+1} > 0$. The deadline effect can be written as

$$P|_{v_{T+1}=0} - P|_{v_{T+1}=\tilde{v}_{T+1}} = - \int_0^{\tilde{v}_{T+1}} \frac{\partial P}{\partial v_{T+1}} dv_{T+1}.$$

We are thus interested in the sign of

$$\frac{\partial}{\partial y} (P|_{v_{T+1}=0} - P|_{v_{T+1}=\bar{v}_{T+1}}) = - \int_0^{\bar{v}_{T+1}} \frac{\partial^2 P}{\partial y \partial v_{T+1}} dv_{T+1}. \quad (19)$$

To this aim, we seek to determine the sign of $\partial^2 P / \partial y \partial v_{T+1}$. Define $R \equiv \ln(1 - P)$, or equivalently $P = 1 - e^R$. Then,

$$\begin{aligned} \frac{\partial^2 P}{\partial y \partial v_{T+1}} &= \frac{\partial}{\partial y} \left(\frac{\partial P}{\partial v_{T+1}} \right) \\ &= \frac{\partial}{\partial y} \left(-e^R \frac{\partial R}{\partial v_{T+1}} \right) \\ &= -e^R \left(\frac{\partial R}{\partial y} \frac{\partial R}{\partial v_{T+1}} + \frac{\partial^2 R}{\partial y \partial v_{T+1}} \right). \end{aligned} \quad (20)$$

Next, define $\phi(c) \equiv \ln(1 - F(c))$. We have that

$$R = \sum_{t=1}^T \ln(1 - F(c_t^*)) = \sum_{t=1}^T \phi(c_t^*).$$

Thus,

$$\begin{aligned} \frac{\partial R}{\partial y} &= \sum_{t=1}^T \phi'(c_t^*) \frac{\partial c_t^*}{\partial y}, \\ \frac{\partial R}{\partial v_{T+1}} &= \sum_{t=1}^T \phi'(c_t^*) \frac{\partial c_t^*}{\partial v_{T+1}}, \\ \frac{\partial^2 R}{\partial y \partial v_{T+1}} &= \sum_{t=1}^T \left(\phi''(c_t^*) \frac{\partial c_t^*}{\partial y} \frac{\partial c_t^*}{\partial v_{T+1}} + \phi'(c_t^*) \frac{\partial^2 c_t^*}{\partial y \partial v_{T+1}} \right), \end{aligned}$$

where

$$\begin{aligned} \phi'(c) &= -\frac{f(c)}{1 - F(c)}, \\ \phi''(c) &= \phi'(c) \left(\frac{f'(c)}{f(c)} + \frac{f(c)}{1 - F(c)} \right) \end{aligned}$$

from the definition of $\phi(c)$. Define $\lambda(c) \equiv F(c) + (1 - \hat{\beta})cf(c)$. Then, we have the following

derivatives:

$$\begin{aligned}
\frac{\partial v_t}{\partial v_{t+1}} &= (\delta \alpha y - \hat{c}_t^*) f(\hat{c}_t^*) \left(-\hat{\beta} \delta \hat{\rho} \right) - \hat{\rho} \delta v_{t+1} f(\hat{c}_t^*) \left(-\hat{\beta} \delta \hat{\rho} \right) + (1 - F(\hat{c}_t^*)) \delta \hat{\rho}_{t+1} \\
&= \left(1 - F(\hat{c}_t^*) - (1 - \hat{\beta}) \hat{c}_t^* f(\hat{c}_t^*) \right) \delta \hat{\rho} \\
&= (1 - \lambda(\hat{c}_t^*)) \delta \hat{\rho}, \\
\frac{\partial v_t}{\partial y} &= (\delta \alpha y - \hat{c}_t^*) f(\hat{c}_t^*) \hat{\beta} \delta \alpha - \hat{\rho} v_{t+1} f(\hat{c}_t^*) \hat{\beta} \delta \alpha + \delta \alpha F(\hat{c}_t^*) \\
&= \left(F(\hat{c}_t^*) + (1 - \hat{\beta}) \hat{c}_t^* f(\hat{c}_t^*) \right) \delta \alpha \\
&= \lambda(\hat{c}_t^*) \delta \alpha.
\end{aligned}$$

Therefore, recursively we can write that

$$\begin{aligned}
dv_t &= \frac{\partial v_t}{\partial v_{t+1}} dv_{t+1} + \frac{\partial v_t}{\partial y} dy \\
&= (1 - \lambda(\hat{c}_t^*)) \delta \hat{\rho} dv_{t+1} + \lambda(\hat{c}_t^*) \delta \alpha dy \\
&= (1 - \lambda(\hat{c}_t^*)) \delta \hat{\rho} (1 - \lambda(\hat{c}_{t+1}^*)) \delta \hat{\rho} dv_{t+2} + (1 - \lambda(\hat{c}_t^*)) \delta \hat{\rho} \lambda(\hat{c}_{t+1}^*) \delta \alpha dy + \lambda(\hat{c}_t^*) \delta \alpha dy \\
&= \left((\delta \hat{\rho})^{T-t+1} \prod_{s=t}^T (1 - \lambda(\hat{c}_s^*)) \right) dv_{T+1} + \delta \alpha \sum_{s=t}^T \left((\delta \hat{\rho})^{s-t} \prod_{u=t}^{s-1} (1 - \lambda(\hat{c}_u^*)) \right) \lambda(\hat{c}_s^*) dy.
\end{aligned}$$

Consequently, the partial derivatives of v_t with respect to v_{T+1} and y are

$$\begin{aligned}
\frac{\partial v_t}{\partial v_{T+1}} &= (\delta \hat{\rho})^{T-t+1} \prod_{s=t}^T (1 - \lambda(\hat{c}_s^*)), \\
\frac{\partial v_t}{\partial y} &= \delta \alpha \sum_{s=t}^T \left((\delta \hat{\rho})^{s-t} \prod_{u=t}^{s-1} (1 - \lambda(\hat{c}_u^*)) \right) \lambda(\hat{c}_s^*).
\end{aligned}$$

The cross-partial derivative of interest can then be found using logarithmic differentiation as

$$\begin{aligned}
\frac{\partial^2 v_t}{\partial y \partial v_{T+1}} &= \frac{\partial}{\partial y} \left(\frac{\partial v_t}{\partial v_{T+1}} \right) \\
&= \frac{\partial v_t}{\partial v_{T+1}} \sum_{s=t}^T \frac{\partial}{\partial y} \ln(1 - \lambda(\hat{c}_s^*)) \\
&= \frac{\partial v_t}{\partial v_{T+1}} \sum_{s=t}^T \frac{-\lambda'(\hat{c}_s^*)}{1 - \lambda(\hat{c}_s^*)} \frac{\partial \hat{c}_s^*}{\partial y}.
\end{aligned}$$

Also note that

$$\begin{aligned}\frac{\partial c_t^*}{\partial y} &= \beta \delta \alpha - \beta \delta \hat{\rho} \frac{\partial v_{t+1}}{\partial y}, \\ \frac{\partial c_t^*}{\partial v_{T+1}} &= -\beta \delta \hat{\rho} \frac{\partial v_{t+1}}{\partial v_{T+1}}, \\ \frac{\partial^2 c_t^*}{\partial y \partial v_{T+1}} &= -\beta \delta \hat{\rho} \frac{\partial^2 v_{t+1}}{\partial y \partial v_{T+1}}.\end{aligned}$$

Plugging in the partial derivatives of v_{t+1} leads to

$$\begin{aligned}\frac{\partial c_t^*}{\partial y} &= \beta \delta \alpha \left(1 - \delta \hat{\rho} \sum_{s=t+1}^T \left((\delta \hat{\rho})^{s-t-1} \prod_{u=t+1}^{s-1} (1 - \lambda(\hat{c}_u^*)) \right) \lambda(\hat{c}_s^*) \right), \\ \frac{\partial c_t^*}{\partial v_{T+1}} &= -\beta (\delta \hat{\rho})^{T-t+1} \prod_{s=t+1}^T (1 - \lambda(\hat{c}_s^*)), \\ \frac{\partial^2 c_t^*}{\partial y \partial v_{T+1}} &= -\beta \delta \hat{\rho} \frac{\partial v_{t+1}}{\partial v_{T+1}} \sum_{s=t+1}^T \frac{-\lambda'(\hat{c}_s^*)}{1 - \lambda(\hat{c}_s^*)} \frac{\partial \hat{c}_s^*}{\partial y} \\ &= \frac{\partial c_t^*}{\partial v_{T+1}} \sum_{s=t+1}^T \frac{-\lambda'(\hat{c}_s^*)}{1 - \lambda(\hat{c}_s^*)} \frac{\partial \hat{c}_s^*}{\partial y}.\end{aligned}$$

Thus, we find that

$$\begin{aligned}\frac{\partial^2 R}{\partial y \partial v_{T+1}} &= \sum_{t=1}^T \phi''(c_t^*) \frac{\partial c_t^*}{\partial y} \frac{\partial c_t^*}{\partial v_{T+1}} + \phi'(c_t^*) \frac{\partial^2 c_t^*}{\partial y \partial v_{T+1}} \\ &= \sum_{t=1}^T \phi''(c_t^*) \frac{\partial c_t^*}{\partial y} \frac{\partial c_t^*}{\partial v_{T+1}} + \phi'(c_t^*) \frac{\partial c_t^*}{\partial v_{T+1}} \sum_{s=t+1}^T \frac{-\lambda'(\hat{c}_s^*)}{1 - \lambda(\hat{c}_s^*)} \frac{\partial \hat{c}_s^*}{\partial y}.\end{aligned}$$

Next, we seek to control the size of this cross-partial derivative. The first step is to assume that there is some \underline{y} such that for every $y \geq \underline{y}$ and $v_{T+1} \in [0, \tilde{v}_{T+1}]$, agents always adopt at a positive probability, i.e., $c_t \in [\underline{c}, \bar{c}] \subset (\underline{c}, \bar{c})$. Therefore, $\lambda(c_t) \in [\underline{\lambda}, \bar{\lambda}]$. For sufficiently large $\hat{\beta}$, it also holds that $\bar{\lambda} < 1$. To find the lower bound of $\partial c_t^* / \partial y$, note that

$$\begin{aligned}& 1 - \sum_{s=t+1}^T \left((\delta \hat{\rho})^{s-t-1} \prod_{u=t+1}^{s-1} (1 - \lambda(\hat{c}_u^*)) \right) \lambda(\hat{c}_s^*) \\ & \geq 1 - \sum_{s=t+1}^T \left(\prod_{u=t+1}^{s-1} (1 - \lambda(\hat{c}_u^*)) \right) \lambda(\hat{c}_s^*) \\ & = \prod_{s=t+1}^T (1 - \lambda(\hat{c}_s^*)) > 0.\end{aligned}$$

Therefore, for each $t > 0$,

$$\frac{\partial c_t^*}{\partial y} \geq \beta \delta \alpha (1 - \delta \hat{\rho}) \equiv \underline{\omega} > 0.$$

The upper bound can be obtained as

$$\frac{\partial c_t^*}{\partial y} \leq \beta \delta \alpha \equiv \bar{\omega} < 1.$$

We conclude that $\partial c_t^* / \partial y \in [\underline{\omega}, \bar{\omega}]$. On the other hand,

$$-\beta \delta \hat{\rho} (\delta \hat{\rho} (1 - \underline{\lambda}))^{T-t} \leq \frac{\partial c_t^*}{\partial v_{T+1}} \leq -\beta \delta \hat{\rho} (\delta \hat{\rho} (1 - \bar{\lambda}))^{T-t}.$$

Together these imply that

$$\begin{aligned} \left| \frac{\partial^2 R}{\partial y \partial v_{T+1}} \right| &\leq \sum_{t=1}^T |\phi''(c_t^*)| \left| \frac{\partial c_t^*}{\partial y} \right| \left| \frac{\partial c_t^*}{\partial v_{T+1}} \right| + |\phi'(c_t^*)| \left| \frac{\partial c_t^*}{\partial v_{T+1}} \right| \sum_{s=t+1}^T \frac{|\lambda'(\hat{c}_s^*)|}{|1 - \lambda(\hat{c}_s^*)|} \left| \frac{\partial \hat{c}_s^*}{\partial y} \right| \\ &\leq \sum_{t=1}^T M_2 \bar{\omega} \beta \delta \hat{\rho} (\delta \hat{\rho} (1 - \underline{\lambda}))^{T-t} + M_1 \beta \delta \hat{\rho} (\delta \hat{\rho} (1 - \underline{\lambda}))^{T-t} \sum_{s=t+1}^T \frac{M_\lambda}{1 - \bar{\lambda}} \bar{\omega} \\ &\leq M_2 \bar{\omega} \beta \delta \hat{\rho} \left(\sum_{t=1}^T (\delta \hat{\rho} (1 - \underline{\lambda}))^{T-t} \right) + M_1 \beta \delta \hat{\rho} \frac{M_\lambda}{1 - \bar{\lambda}} \bar{\omega} \left(\sum_{t=1}^T (T-t) (\delta \hat{\rho} (1 - \underline{\lambda}))^{T-t} \right) \\ &\leq \frac{M_2 \bar{\omega} \beta \delta \hat{\rho}}{1 - \delta \hat{\rho} (1 - \underline{\lambda})} + M_1 \beta \delta \hat{\rho} \frac{M_\lambda}{1 - \bar{\lambda}} \bar{\omega} \frac{\delta \hat{\rho} (1 - \underline{\lambda})}{(1 - \delta \hat{\rho} (1 - \underline{\lambda}))^2} \equiv M, \end{aligned}$$

where $M_1 \equiv \max_{c \in [\underline{c}, \bar{c}]} |\phi'(c)|$, $M_2 \equiv \max_{c \in [\underline{c}, \bar{c}]} |\phi''(c)|$, and $M_\lambda \equiv \max_{c \in [\underline{c}, \bar{c}]} |\lambda'(c)|$. They exist since all the functions are continuous over the given compact interval. This result indicates that as $T \rightarrow \infty$, the cross-partial derivative is bounded from above. At the same time, if we define $L \equiv \min_{c \in [\underline{c}, \bar{c}]} |\phi'(c)| > 0$, then we have $\phi(c_t^*) < -L$ and thus

$$\begin{aligned} \frac{\partial R}{\partial y} &= \sum_{t=1}^T \phi'(c_t^*) \frac{\partial c_t^*}{\partial y} \leq - \sum_{t=1}^T L \underline{\omega} = -TL \underline{\omega} < 0, \\ \frac{\partial R}{\partial v_{T+1}} &= \sum_{t=1}^T \phi'(c_t^*) \frac{\partial c_t^*}{\partial v_{T+1}} \geq \sum_{t=1}^T L \beta \delta \hat{\rho} (\delta \hat{\rho} (1 - \bar{\lambda}))^{T-t} \geq L \beta \delta \hat{\rho} > 0. \end{aligned}$$

Therefore,

$$\frac{\partial R}{\partial y} \frac{\partial R}{\partial v_{T+1}} + \frac{\partial^2 R}{\partial y \partial v_{T+1}} \leq -TL \underline{\omega} L \beta \delta \hat{\rho} + M.$$

For sufficiently large T , this is negative, so from equation (20),

$$\frac{\partial^2 P}{\partial y \partial v_{T+1}} > 0.$$

Thus the sign of $\partial(P|_{v_{T+1}=0} - P|_{v_{T+1}=\bar{v}_{T+1}})/\partial y$ in equation (19) is negative. In conclusion, the treatment effect of a deadline is decreasing in y under the conditions stated in the prediction. \square

Prediction 3 (Deadline and Present Bias). *The more present-biased a firm owner is, the larger is the treatment effect of a deadline on cumulative adoption by the deadline (for β sufficiently large that the firm owner without a deadline still adopts with positive probability, and ρ_t and T sufficiently large).*

Proof. Denote the optimal cost thresholds for the deadline and no-deadline groups as c_t^* and \tilde{c}_t^* , respectively. Then let $\{c_t^*\}_{t=1}^T$ denote the optimal thresholds at each point in time up to the deadline period T in the deadline case and $\{\tilde{c}_t^*\}_{t=1}^T$ denote those in the no-deadline case (still up to the deadline case's deadline in period T). In the deadline period $t = T$, $\tilde{c}_T^* < c_T^*$ since $\mathbb{E}_T[\hat{V}_{T+1}] = 0$ in the deadline group and $\mathbb{E}_T[\hat{V}_{T+1}] > 0$ in the no-deadline group. It will also hold that $\tilde{c}_t^* < c_t^*$ for all t if $\hat{\beta}$ is sufficiently large that monotonicity of continuation values holds (Lemma 1).

Define the cumulative adoption probability at time T as

$$P(\{c_t^*\}_{t=1}^T) = \sum_{t=1}^T F(c_t^*) \prod_{j=1}^t \rho_j (1 - F(c_{j-1}^*)).$$

Formally, we then want to prove that the gap in cumulative adoption rates by time T ,

$$P(\{c_t^*\}_{t=1}^T) - P(\{\tilde{c}_t^*\}_{t=1}^T),$$

is weakly decreasing in β (since higher values of β indicate less present bias) over an interval such that $0 < F(\tilde{c}_t^*) < F(c_t^*) < 1$. We will show this for $\rho \in [\rho_0, 1]$ and $T > T_0$, where ρ_0 and T_0 are constants.

The proposition to be proven is thus

$$\frac{\partial}{\partial \beta} (P(\{c_t^*\}_{t=1}^T) - P(\{\tilde{c}_t^*\}_{t=1}^T)) \leq 0.$$

First, define that

$$\gamma_t^* \equiv c_t^*/\beta = \delta(\alpha_t y - \hat{\rho} \mathbb{E}_t[\hat{V}_{t+1}]),$$

which is independent of β (as $\mathbb{E}_t[\hat{V}_{t+1}]$ is a function of $\hat{\beta}$, not β) and, similarly, $\tilde{\gamma}_t^* \equiv \tilde{c}_t^*/\beta$. With these definitions, we can write $P(\{c_t^*\}_{t=1}^T)$ as $\bar{P}(\beta, \{\gamma_t^*\}_{t=1}^T)$. Moreover, since $\tilde{c}_t^* < c_t^*$ for all t , it

must hold that $\tilde{\gamma}_t^* < \gamma_t^*$, so the difference in cumulative take-up can be written as a path integral:

$$\frac{\partial}{\partial \beta} (P(\{c_t^*\}_{t=1}^T) - P(\{\tilde{c}_t^*\}_{t=1}^T)) = \sum_{t=1}^T \int_{\gamma_t^*}^{\tilde{\gamma}_t^*} \frac{\partial^2}{\partial \beta \partial \gamma_t} \bar{P}(\beta, \gamma_1^*, \dots, \gamma_{t-1}^*, \gamma_t, \tilde{\gamma}_{t+1}^*, \dots, \tilde{\gamma}_T^*) d\gamma_t.$$

Therefore, it suffices to show that

$$\frac{\partial^2 \bar{P}}{\partial \beta \partial \gamma_t} \leq 0$$

whenever $0 < F(c_t) < 1$ for all t .

By continuity, it suffices to prove this for $\rho_t = 1$ as it follows that it holds for sufficiently large ρ_t . When $\rho_t = 1$, we can rewrite the cumulative adoption rate by period T as

$$P(\{c_t^*\}_{t=1}^T) = 1 - \prod_{j=1}^T (1 - F(c_j^*)). \quad (21)$$

We want to calculate $\partial^2 \bar{P} / \partial \beta \partial \gamma_t$. First, we have that

$$\frac{\partial \bar{P}}{\partial \gamma_t} = \beta f(c_t^*) \prod_{j \neq t} (1 - F(c_j^*)), \quad (22)$$

since the only factor in the second term on the right-hand side of equation (21) that contains γ_t is $(1 - F(c_t^*))$, and since $\partial c_t^* / \partial \gamma_t = \beta$. Next, taking logarithms of both sides of equation (22), then taking the partial derivative with respect to β , rearranging, and plugging back in $\partial \bar{P} / \partial \gamma_t$ for the term on the right-hand side of equation (22),

$$\begin{aligned} \frac{\partial^2 \bar{P}}{\partial \beta \partial \gamma_t} &= \frac{\partial}{\partial \beta} \left(\frac{\partial \bar{P}}{\partial \gamma_t} \right) = \frac{\partial \bar{P}}{\partial \gamma_t} \frac{\partial}{\partial \beta} \left(\ln \frac{\partial \bar{P}}{\partial \gamma_t} \right) \\ &= \frac{\partial \bar{P}}{\partial \gamma_t} \frac{\partial}{\partial \beta} \left(\ln \beta + \ln f(c_t^*) + \sum_{j \neq t} \ln (1 - F(c_j^*)) \right) \\ &= \frac{\partial \bar{P}}{\partial \gamma_t} \left(\frac{1}{\beta} + \frac{c_t^*}{\beta} \frac{f'(c_t^*)}{f(c_t^*)} - \sum_{j \neq t} \frac{c_j^*}{\beta} \frac{f(c_j^*)}{1 - F(c_j^*)} \right) \\ &= f(c_t^*) \prod_{j \neq t} (1 - F(c_j^*)) \left(1 + c_t^* \frac{f'(c_t^*)}{f(c_t^*)} - \sum_{j \neq t} \frac{c_j^* f(c_j^*)}{1 - F(c_j^*)} \right) \\ &= - \left(\prod_{j=1}^T (1 - F(c_j^*)) \right) \frac{f(c_t^*)}{1 - F(c_t^*)} \left(\sum_{j \neq t} \frac{c_j^* f(c_j^*)}{1 - F(c_j^*)} - 1 - c_t^* \frac{f'(c_t^*)}{f(c_t^*)} \right), \end{aligned} \quad (23)$$

where we have used the fact that $\partial c_t^* / \partial \beta = c_t^* / \beta = \gamma_t^*$ because c_t^* is linear in β .

The right hand side of (23) has three components and a preceding negative sign. The first two components are positive, so a sufficient condition for $\partial^2 \bar{P} / \partial \beta \partial \gamma_t \leq 0$ is that the third component is positive. Since $F(c)$ has a continuously differentiable positive density function over its support $[\underline{c}, \bar{c}]$ and $0 < F(c_j) < 1$ by assumption, it must hold that for every $j \neq t$, the continuous function

$$\frac{c_j^* f(c_j^*)}{1 - F(c_j^*)} \leq \min_x \frac{x f(x)}{1 - F(x)} \equiv M$$

has a strictly positive minimum value that we define as M . Similarly, the function

$$1 + c_t^* \frac{f'(c_t^*)}{f(c_t^*)} \leq L$$

is uniformly bounded from above. Therefore, we can bound the third component on the right-hand side of (23):

$$\sum_{j \neq t} \frac{c_j^* f(c_j^*)}{1 - F(c_j^*)} - 1 - c_t^* \frac{f'(c_t^*)}{f(c_t^*)} \geq (T - 1)M - L, \quad (24)$$

since the first term on the left-hand side of (24) sums over all periods except t . Thus, for large enough $T \geq T_0 = (L/M) + 1$, the third component on the right-hand side of (23) is positive, in which case $\partial^2 \bar{P} / \partial \beta \partial \gamma_t \leq 0$ for all t . \square

Prediction 4 (Reminder and Memory). *If a reminder (unannounced or announced) increases the probability of remembering, it will also increase take-up.*

Proof. From equation (3), c_t^* only depends on $\hat{\rho}_\ell$ for $\ell > t$ and not on ρ_t . Hence ρ_t impacts the probability of adopting at time t only through its impact on the probability of the task being active in period t . For any period t ,

$$\Pr(\text{task active at } t) = \prod_{j=1}^t \rho_j \prod_{k=1}^{t-1} (1 - F(c_k^*)),$$

so $d \Pr(\text{task active at } t) / d \rho_t > 0$. By assumption, an unannounced reminder received in period t increases ρ_t and therefore increase the probability of the task being active and the probability of adopting. \square

Prediction 5 (Announced Reminder and Beliefs about Memory). *The announced reminder (a) reduces take-up at $t = 1$, compared to the unannounced reminder, if managers do not believe they have perfect memory, ($\hat{\rho}_t < 1$ for $t > 1$), and (b) has no differential effect on take-up at $t = 1$ if managers believe they have perfect memory ($\hat{\rho}_t = 1$ for $t > 1$).*

Proof. Take-up at $t = 1$ will be lower if the optimal cost threshold is lower, so we seek to show

that $dc_t^*/d\hat{\rho} \leq 0$. Taking derivatives of equation (3) with respect to $\hat{\rho}$,

$$\frac{dc_t^*}{d\hat{\rho}} = -\beta \delta \mathbb{E}_t[\hat{V}_{t+1}] - \beta \delta \hat{\rho} \frac{d\mathbb{E}_t[\hat{V}_{t+1}]}{d\hat{\rho}}. \quad (25)$$

Thus a sufficient condition for $dc_t^*/d\hat{\rho} < 0$ is $d\mathbb{E}_t[\hat{V}_{t+1}]/d\hat{\rho} > 0$.

Taking derivatives of equation (4) with respect to $\hat{\rho}$,

$$\begin{aligned} \frac{d\mathbb{E}_t[\hat{V}_{t+1}]}{d\hat{\rho}} &= (\delta \alpha y - \hat{c}_{t+1}^*) f(\hat{c}_{t+1}^*) \frac{d\hat{c}_{t+1}^*}{d\hat{\rho}} - f(\hat{c}_{t+1}^*) \frac{d\hat{c}_{t+1}^*}{d\hat{\rho}} \delta \hat{\rho} \mathbb{E}_{t+1}[\hat{V}_{t+2}] \\ &\quad + (1 - F(\hat{c}_{t+1}^*)) \delta \mathbb{E}_{t+1}[\hat{V}_{t+2}] + (1 - F(\hat{c}_{t+1}^*)) \delta \hat{\rho} \frac{d\mathbb{E}_{t+1}[\hat{V}_{t+2}]}{d\hat{\rho}}. \end{aligned} \quad (26)$$

Rearranging equation (10) with $\tau = t + 1$ to express $\mathbb{E}_{t+1}[\hat{V}_{t+2}]$ as a function of \hat{c}_{t+1}^* and plugging this into the second term on the right-hand side of (26) (but leaving $\mathbb{E}_{t+1}[\hat{V}_{t+2}]$ in the third term), and rearranging terms:

$$\begin{aligned} \frac{d\mathbb{E}_t[\hat{V}_{t+1}]}{d\hat{\rho}} &= (\hat{\beta}^{-1} - 1) \frac{d\hat{c}_{t+1}^*}{d\hat{\rho}} \hat{c}_{t+1}^* f(\hat{c}_{t+1}^*) \\ &\quad + (1 - F(\hat{c}_{t+1}^*)) \delta \mathbb{E}_{t+1}[\hat{V}_{t+2}] + (1 - F(\hat{c}_{t+1}^*)) \delta \hat{\rho} \frac{d\mathbb{E}_{t+1}[\hat{V}_{t+2}]}{d\hat{\rho}}. \end{aligned} \quad (27)$$

Taking derivatives of equation (10) for $\tau = t + 1$ with respect to $\hat{\rho}$, plugging in $d\hat{c}_{t+1}^*/d\hat{\rho}$ in equation (27), and rearranging terms:

$$\begin{aligned} \frac{d\mathbb{E}_t[\hat{V}_{t+1}]}{d\hat{\rho}} &= (\hat{\beta}^{-1} - 1) \left[-\hat{\beta} \delta \left(\mathbb{E}_{t+1}[\hat{V}_{t+2}] + \hat{\rho} \frac{d\mathbb{E}_{t+1}[\hat{V}_{t+2}]}{d\hat{\rho}} \right) \right] \hat{c}_{t+1}^* f(\hat{c}_{t+1}^*) \\ &\quad + (1 - F(\hat{c}_{t+1}^*)) \delta \mathbb{E}_{t+1}[\hat{V}_{t+2}] + (1 - F(\hat{c}_{t+1}^*)) \delta \hat{\rho} \frac{d\mathbb{E}_{t+1}[\hat{V}_{t+2}]}{d\hat{\rho}} \\ &= \left(1 - F(\hat{c}_{t+1}^*) - (1 - \hat{\beta}) \hat{c}_{t+1}^* f(\hat{c}_{t+1}^*) \right) \delta \left(\mathbb{E}_{t+1}[\hat{V}_{t+2}] + \hat{\rho} \frac{d\mathbb{E}_{t+1}[\hat{V}_{t+2}]}{d\hat{\rho}} \right) \\ &= \kappa \delta \left(\mathbb{E}_{t+1}[\hat{V}_{t+2}] + \hat{\rho} \frac{d\mathbb{E}_{t+1}[\hat{V}_{t+2}]}{d\hat{\rho}} \right), \end{aligned} \quad (28)$$

where $\kappa \equiv 1 - F(\hat{c}_{t+1}^*) - (1 - \hat{\beta}) \hat{c}_{t+1}^* f(\hat{c}_{t+1}^*)$. Note that $\kappa \leq 1$ always, and for sufficiently large $\hat{\beta}$ such that monotonicity of continuation values holds (Lemma 1), $0 \leq \kappa \leq 1$. At the deadline $t = T$, $d\mathbb{E}_T[\hat{V}_{T+1}]/d\hat{\rho} = 0$ as there is no continuation value since the task expires. Plugging this into the right-hand side of equation (28) for $t = T - 1$, the second term inside the parentheses will equal 0. The first term inside the parentheses is positive, so $d\mathbb{E}_{T-1}[\hat{V}_T]/d\hat{\rho} \geq 0$. Recursively, for all $t < T$, $d\mathbb{E}_t[\hat{V}_{t+1}]/d\hat{\rho} \geq 0$ from equation (28), which is what we sought to prove.

Announced reminders increase $\hat{\rho}_\tau$ for the period τ in which the firm owner is told the reminder will arrive, as long as $\hat{\rho}_\tau < 1$. On the other hand, if $\hat{\rho}_\tau = 1$ even in the absence of the announced reminder, $\hat{\rho}_\tau$ cannot be increased. Thus, (a) if firm owners do not believe they have perfect memory, the announced reminder lowers the cost threshold c_t^* since $dc_t^*/d\hat{\rho} < 0$, and (b) if firm owners believe they have perfect memory, $\hat{\rho}_\tau$ does not increase and hence there is no change in c_t^* or take-up. \square

Prediction 6 (Announced Reminder and Trust). *The announced reminder (a) does not affect final (cumulative) take-up, compared to the unannounced reminder, if firms inherently trust the offer ($\alpha_t = 1$); and (b) increases final (cumulative) take-up if some firms distrust the offer ($\alpha_t < 1$) and their trust increases after receiving the announced reminder.*

Proof. The proof that $dc_t^*/d\alpha > 0$ is analogous to the proof from Prediction 1 that $dc_t^*/dy > 0$, given the way that $\alpha_t y$ enters equations (3) and (4). If (a) $\alpha_t = 1$ inherently, then α_t cannot increase from receiving the announced reminder, and hence there is no change in c_t^* and no difference in take-up between receiving an announced or an unannounced reminder. If (b) $\alpha_t < 1$ and the announced reminder increases α_t by increasing trust in the offer, then the threshold c_t^* increases. The higher optimal cost threshold after receiving the reminder leads to an increase in take-up. \square

B Model Simulations

In this Appendix, we simulate the model to test when the formal restrictions required for the model to hold are binding. For simplicity, we assume that memory and trust are constant over time and thus drop the t subscript on α , ρ , and $\hat{\rho}$. In all simulations, we set the mean value of the offer y to 1 (with $y = 1$ always in some simulations, while allowing y to vary around this mean in others), $F(c)$ to be a uniform distribution over $[0.4, 0.6]$, and $\delta = 0.99$.

Prediction 1 only has the restriction that $\hat{\beta}$ is sufficiently large such that monotonicity of continuation values holds (Lemma 1). To simulate this prediction, we allow ρ , $\hat{\rho}$, and β to vary over $[0, 1]$ in increments of 0.01, and we allow $\hat{\beta}$ to vary over $[\beta, 1]$ in increments of 0.01. We set the other parameters as described above, set $\alpha_t = 1$ for all t , and set $T = 8$ as in the deadline group in our experimental setting. Within each test we then allow y to vary from 0.5 to 1.5 in increments of 0.005 and test whether cumulative take-up of the offer by T is monotonically increasing as we increase y over that range.

Appendix-Figure B.1 shows that in these simulations, Prediction 1 nearly always holds regardless of the level of $\hat{\beta}$. The figure plots, for each value of $\hat{\beta}$, the fraction of simulations (combinations of the parameters ρ , $\hat{\rho}$, and β) in which take-up is increasing in y . The lowest fraction of simulations in which the prediction holds occurs when $\hat{\beta} = X$, but even then the prediction holds in $Y\%$ of simulations.

Predictions 2 and 3 have additional restrictions in addition to $\hat{\beta}$ being sufficiently large that monotonicity of continuation values holds. Specifically, the proof imposes the following additional restrictions: (i) parameter values must be such that managers in the no deadline arm adopt with positive probability, (ii) ρ_t must be sufficiently large, and (iii) T must be sufficiently large. We again test how binding these restrictions are in simulations of the model.

For Prediction 3, we again allow ρ , $\hat{\rho}$, and β to vary over $[0, 1]$, and $\hat{\beta}$ to vary over $[\beta, 1]$, all in increments of 0.01. We now fix $y = 1$ and $\alpha_t = 1$ for all t , and set the other parameters as described above. We now allow for a deadline arm with $T = 8$ and a no-deadline arm with $T = 183$ which matches our experimental setting where the benefit only lasted for six months. (By Corollary 1, the no deadline case can be approximated by a sufficiently long deadline.) Within a fixed set of parameter values for ρ , $\hat{\rho}$, and $\hat{\beta}$, we allow β to vary between 0 and 1 and test whether the difference in cumulative take-up by $t = 8$ between the deadline and no-deadline arms decreases in β . We then plot a heatmap where we show the fraction of simulations, for a given ρ and $\hat{\beta}$ (since these are the parameters with restrictions for the proposition to hold), where the prediction holds.

Appendix-Figure B.2 plots the simulation results. For values of $\hat{\beta} < 0.4$, which also implies $\beta < 0.4$ due to the restriction that $\hat{\beta} \geq \beta$, the first restriction that the parameters are such that the manager without a deadline adopts with positive probability no longer holds. For larger values of $\hat{\beta}$, we see that Prediction 3 nearly always holds, except when ρ is very low; even for very low values of ρ (e.g., $\rho < 0.2$), the prediction holds in the vast majority of simulations when $\hat{\beta} \geq 0.8$.

To test when the restriction that T is sufficiently large binds for Prediction 3, we repeat the same process for various values of T . Appendix-Figure B.3 shows the same simulation results for $T = 1$, $T = 2$, $T = 4$, and $T = 6$. The results that the prediction holds for the vast majority of ρ and $\hat{\beta}$ values is true as long as $T \geq 2$.

For Prediction 2, we follow the same procedure as for Prediction 3, but we now allow y to vary over $[0.5, 1.5]$ in increments of 0.005 and test whether—for each combination of values of ρ , $\hat{\rho}$, β , and $\hat{\beta}$ —the treatment effect of the deadline on adoption by $t = 8$ (the deadline period for those in the deadline arm) is decreasing in y . Appendix-Figure B.4 shows that the restrictions for this prediction to hold are more binding: the prediction still holds in the majority of cases when $\hat{\beta}$ and ρ are both sufficiently large, but there are a number of cases in which the prediction does not hold. We next test the restriction that T must be sufficiently large in Appendix-Figure B.5, and find that results are similar to the $T = 8$ case when $T = 6$, but that the prediction tends to fail to hold for lower values of T .

The proof of Prediction 4 does not impose any restrictions on the parameters of the model, so we do not include simulations for that prediction.

For Predictions 5 and 6, the only restriction imposed on the proofs is a sufficiently large $\hat{\beta}$ that monotonicity of continuation values holds (Lemma 1). Appendix-Figures B.6 and B.7 show that

the predictions nearly always hold; specifically, for any value of $\hat{\beta}$ they hold in a minimum of $Z\%$ of cases.

Figure B.1: Fraction of Simulations where Prediction 1 Holds

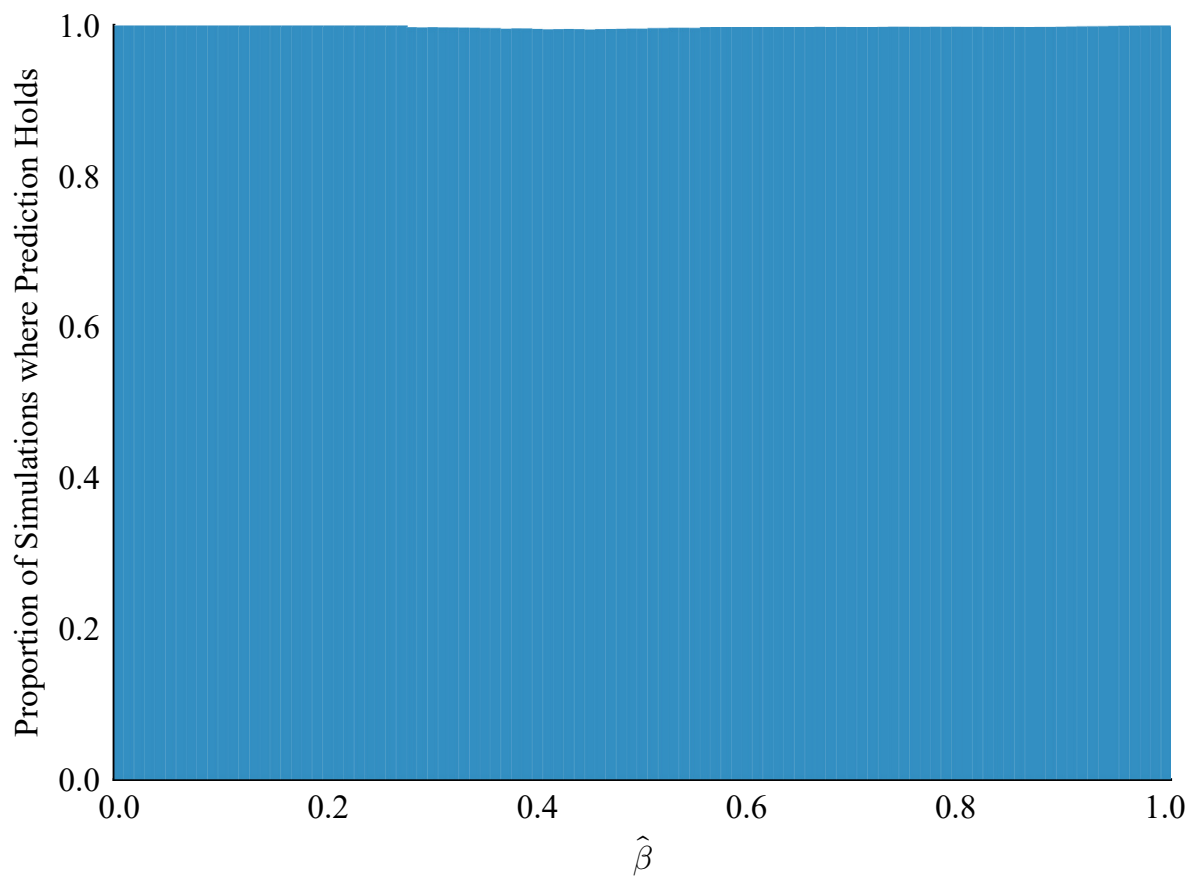


Figure B.2: Fraction of Simulations where Prediction 3 Holds for $T = 8$ in Deadline Arm

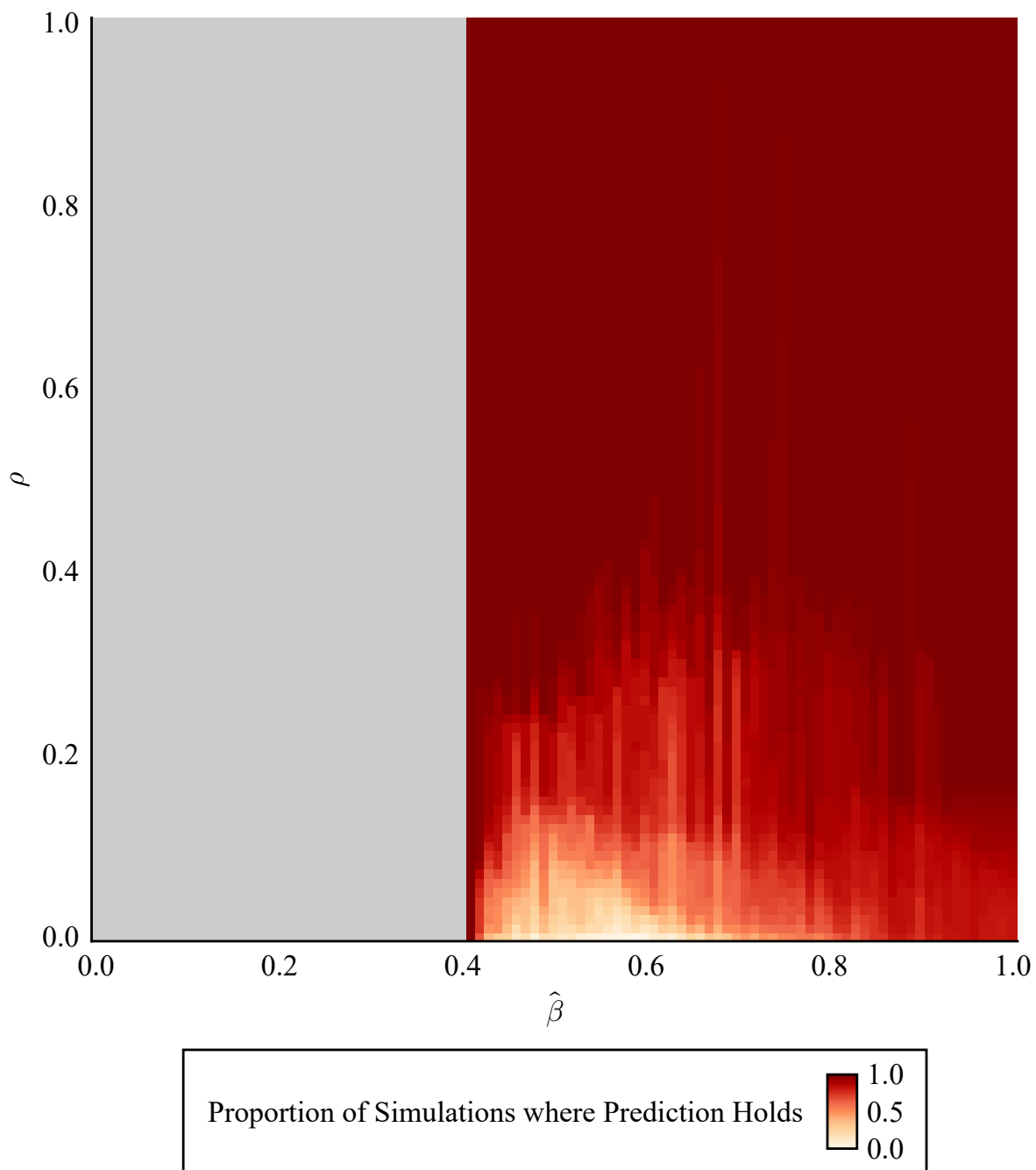


Figure B.3: Fraction of Simulations where Prediction 3 Holds for Various T in Deadline Arm

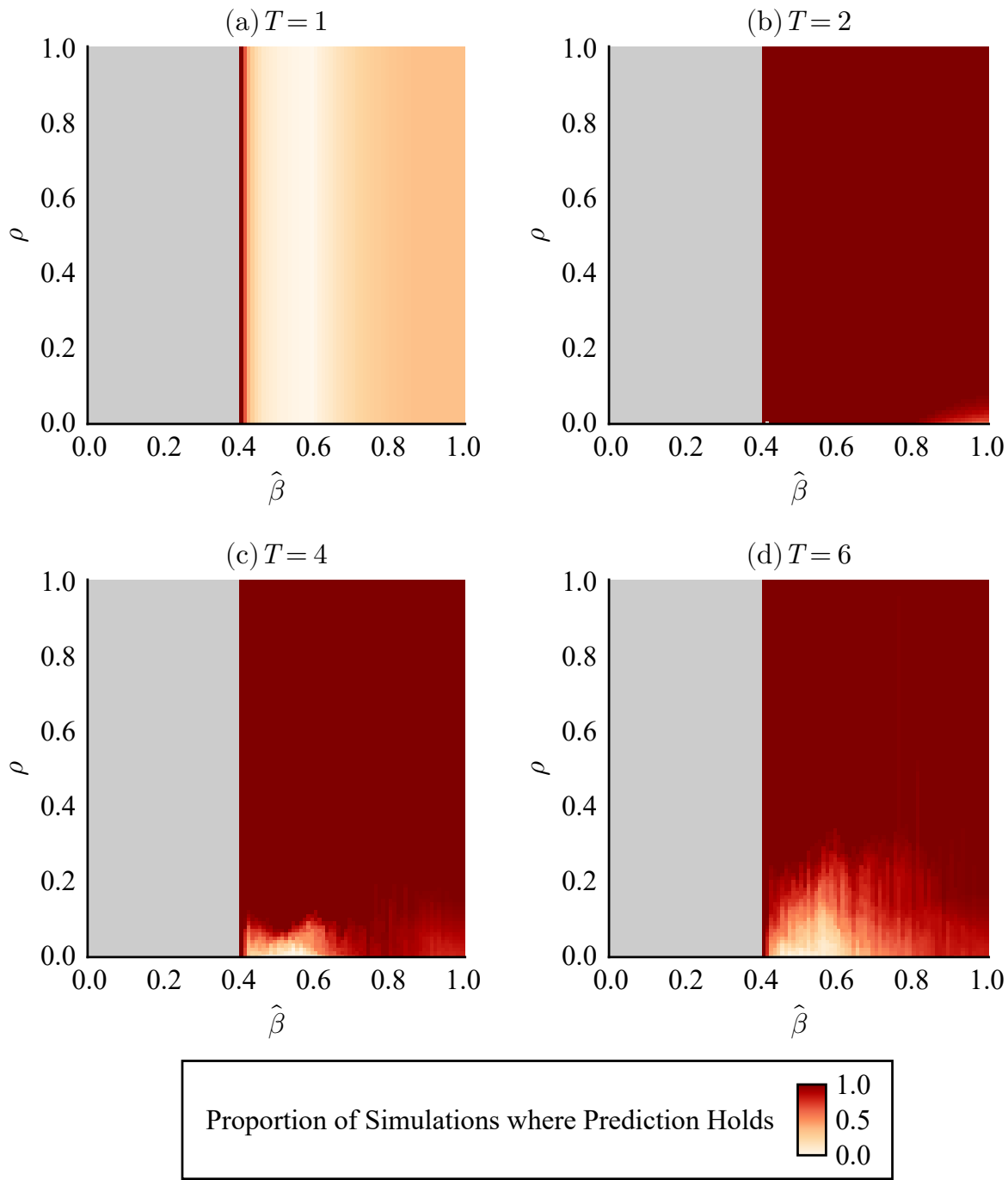


Figure B.4: Fraction of Simulations where Prediction 2 Holds with $T = 8$ in Deadline Arm

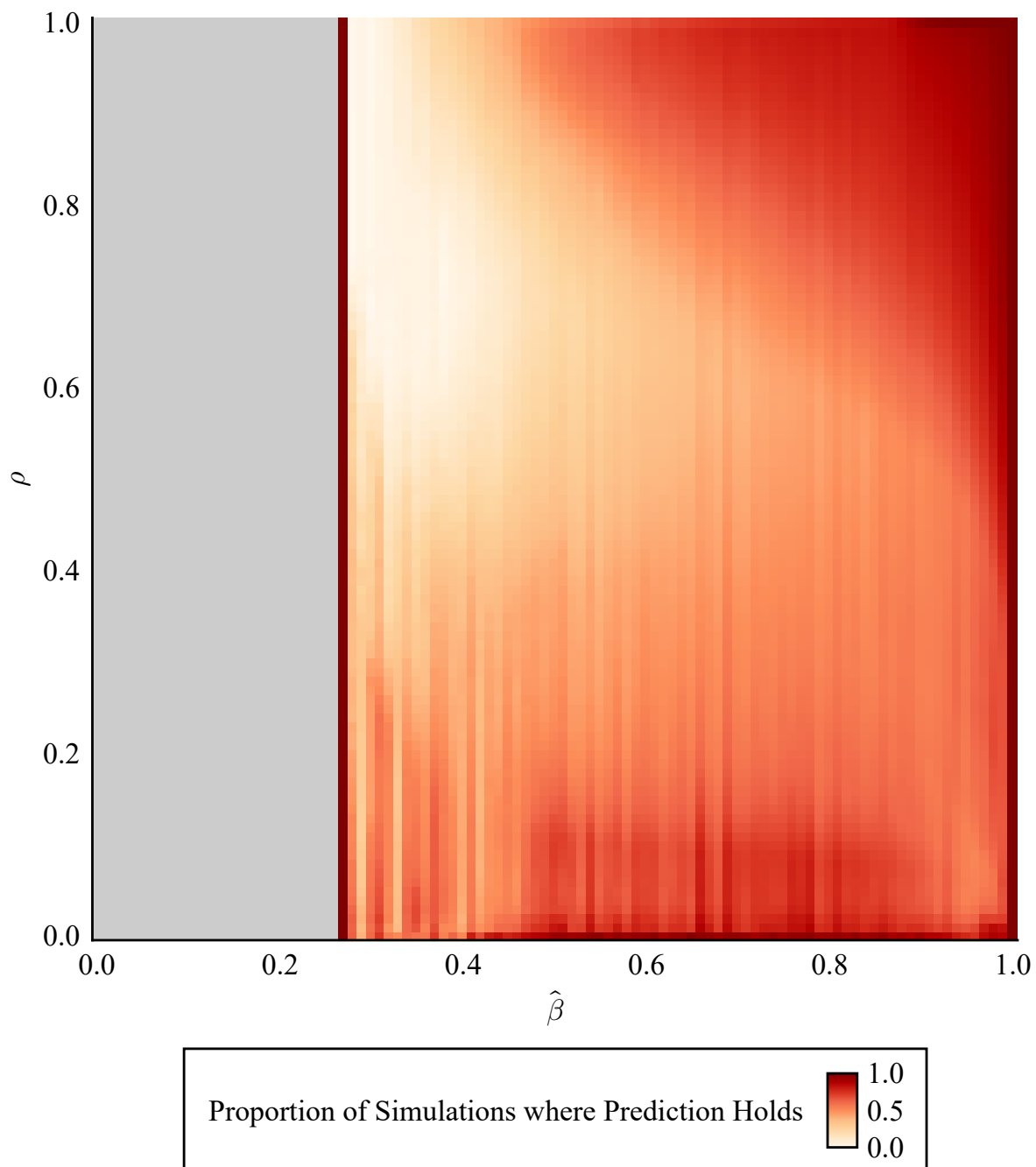


Figure B.5: Fraction of Simulations where Prediction 2 Holds for Various T in Deadline Arm

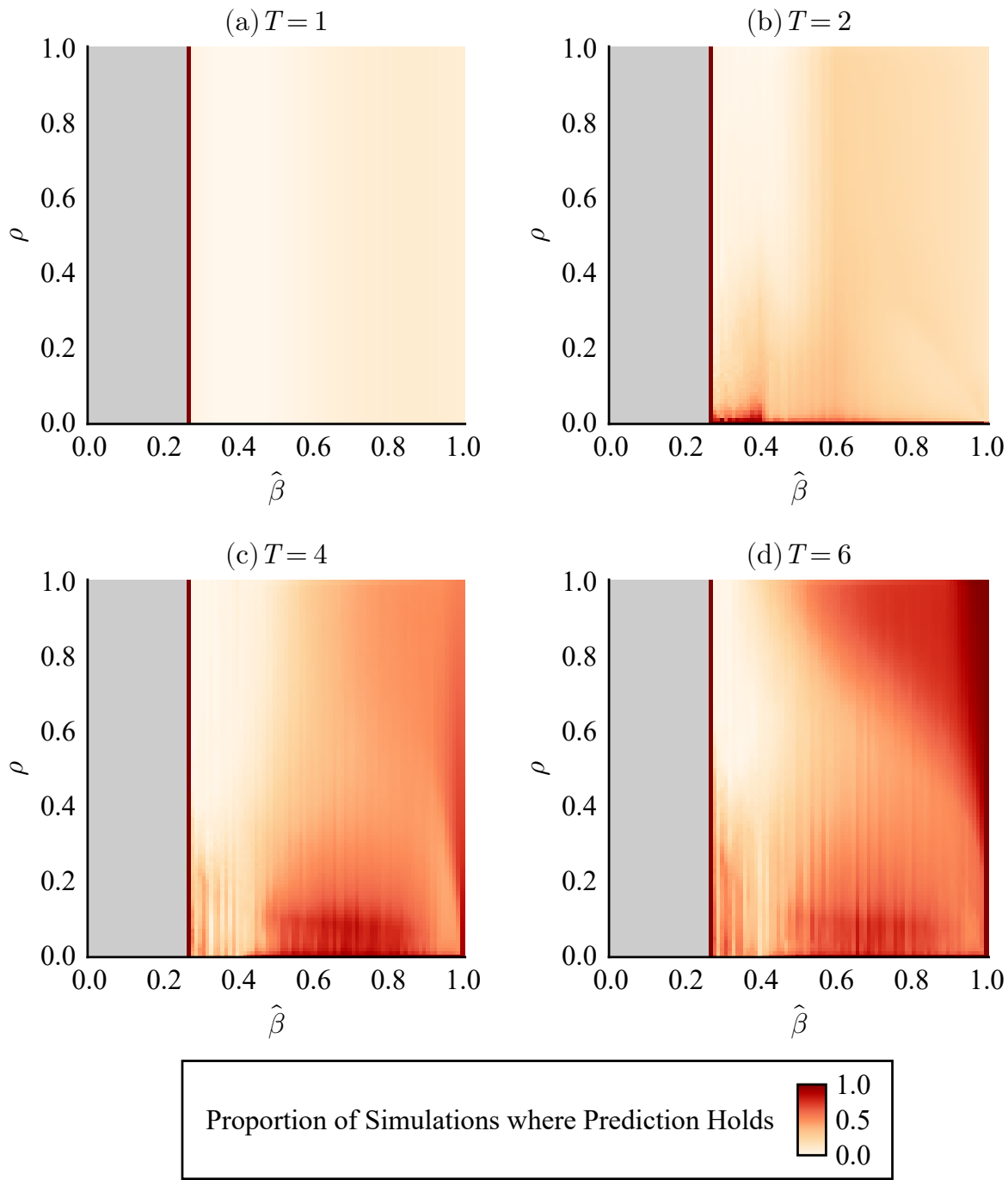


Figure B.6: Fraction of Simulations where Prediction 5 Holds

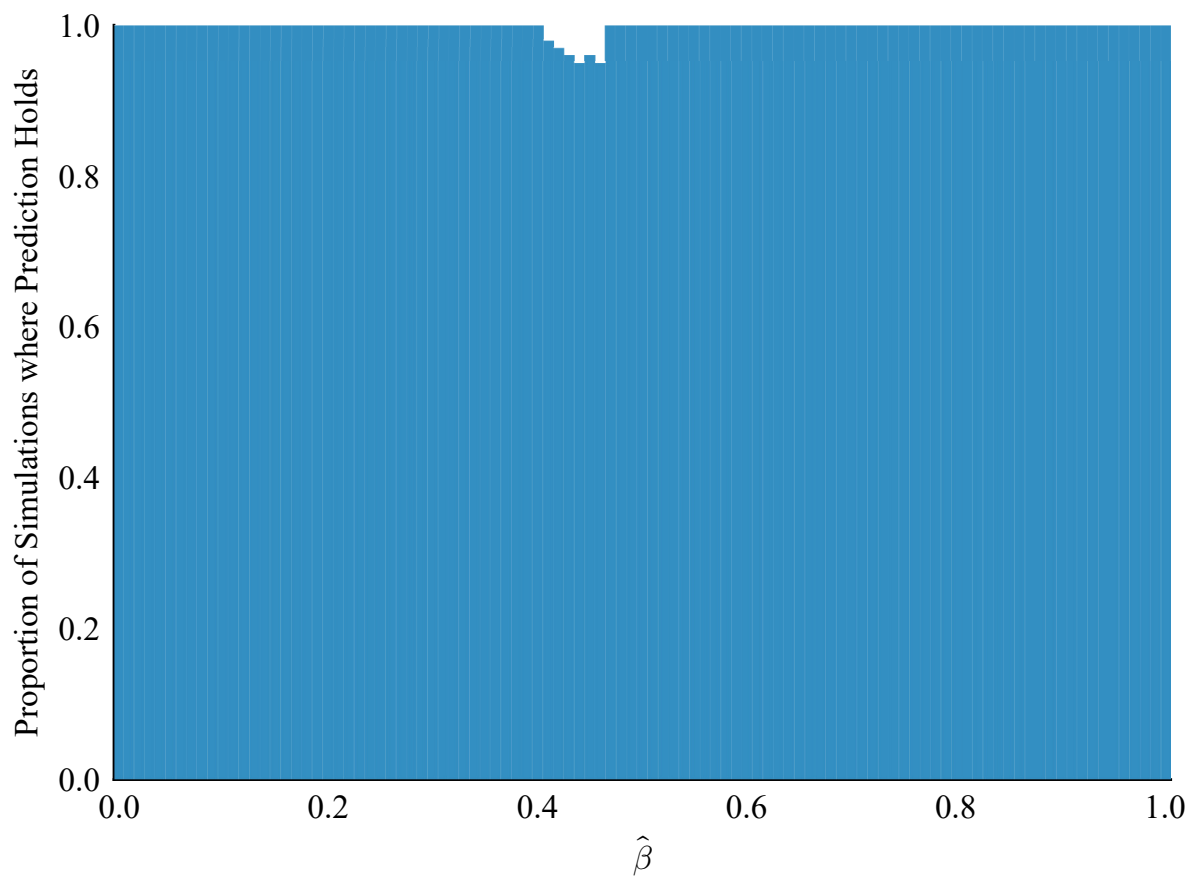
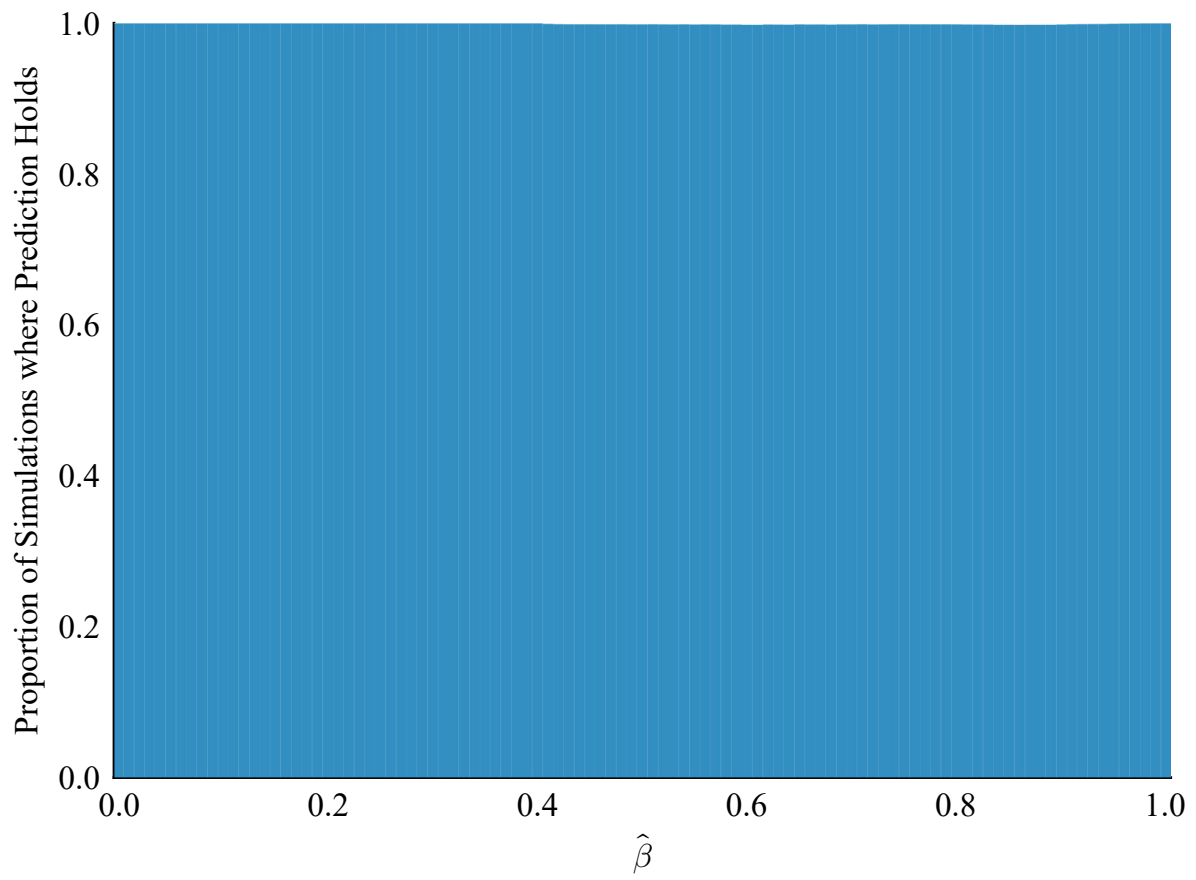


Figure B.7: Fraction of Simulations where Prediction 6 Holds



C Tables and Figures

Table C.1: Treatment Balance (Attrition Test)

	Attrition
Intercept	0.00076*** (0.00025)
Announced reminder	0.00084 (0.00053)
Unannounced reminder	-0.00017 (0.00036)
Deadline	0.00020 (0.00037)
2.75% Fee	-0.00006 (0.00035)
Number of observations	34,010

This table tests for differential attrition by treatment arm. The dependent variable equals 1 if the firm was excluded from the RCT sample due to the FinTech partner's filtering of firms that were not in good standing administratively. The unit of observation is a firm, and the regression includes all firms ($N = 34,010$) prior to exclusions. Standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.2: Main Regression Results: Average Treatment Effects

	Accepted Offer
Group 2: No deadline, no reminder	0.254*** (0.011)
Group 3: No deadline, announced reminder	0.305*** (0.014)
Group 4: No deadline, unannounced reminder	0.290*** (0.015)
Group 5: Deadline, no reminder	0.256*** (0.015)
Group 6: Deadline, announced reminder	0.318*** (0.017)
Group 7: Deadline, unannounced reminder	0.298*** (0.017)
Group 8: Same-day deadline, no reminder	0.229*** (0.012)
Number of observations	33,978

This table reports the estimated effect of being assigned to a treatment group on the probability of take-up. The unit of observation is a firm. The control group (Group 1) is omitted from the regression. Data includes all firms in the experiment ($N = 33,978$), and the dependent variable is an indicator equal to 1 if a firm accepts the offer between September 29 to October 6 (the day of the one-week deadline). The estimation includes strata fixed-effects. Clustered standard errors at the strata level are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.3: Survey Baseline Treatment Balance

	Intercept	Deadline	Announced reminder	Unannounced reminder	2.75% Fee	Joint test <i>F</i> -stat
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Manager characteristics						
Female	0.396*** (0.083)	0.060 (0.046)	-0.053 (0.083)	-0.029 (0.084)	0.103* (0.046)	1.832 [0.121]
Age	41.067*** (1.48)	0.193 (1.00)	-1.216 (1.53)	-0.698 (1.58)	-1.084 (1.00)	0.456 [0.768]
Panel B: Business characteristics						
<i>Business type</i>						
Small retailers	0.339*** (0.077)	0.012 (0.041)	-0.126 (0.077)	-0.103 (0.078)	0.050 (0.041)	1.151 [0.332]
Professionals	0.234*** (0.075)	-0.001 (0.042)	0.013 (0.073)	0.049 (0.074)	0.057 (0.042)	0.694 [0.597]
Beauty	0.133** (0.053)	-0.025 (0.023)	-0.078 (0.052)	-0.069 (0.053)	0.023 (0.023)	1.483 [0.206]
Clothing	0.032 (0.029)	0.001 (0.025)	0.061* (0.029)	0.068* (0.030)	-0.023 (0.025)	0.789 [0.533]
Restaurants	0.121*** (0.045)	-0.006 (0.029)	0.028 (0.047)	0.030 (0.048)	-0.064* (0.030)	1.283 [0.276]
Other	0.141** (0.056)	0.020 (0.036)	0.103* (0.059)	0.025 (0.057)	-0.044 (0.036)	1.814 [0.125]
<i>Pre-treatment sales variables</i>						
Months since first transaction	21.257*** (2.37)	2.617 (1.64)	1.091 (2.41)	2.620 (2.47)	-0.833 (1.64)	1.021 [0.396]
% months business made sales	0.866*** (0.029)	0.004 (0.020)	-0.039 (0.030)	-0.036 (0.031)	-0.014 (0.020)	0.449 [0.773]
Log monthly card sales volume	8.616*** (0.168)	-0.066 (0.100)	0.120 (0.169)	0.178 (0.172)	0.078 (0.100)	0.554 [0.696]
Log monthly card transactions	2.043*** (0.200)	0.083 (0.127)	-0.082 (0.203)	0.037 (0.206)	0.007 (0.128)	0.320 [0.864]

This table reports differences in characteristics of the manager (typically the firm owner) and of the business by treatment group. The unit of observation is a firm, and each regression includes all firms in the survey sample ($N = 471$). Each row shows coefficients from a regression of that row's characteristic on an intercept (column 1) and indicator variables for deadline (column 2), announced reminder (column 3), unannounced reminder (column 4), and 2.75% fee (column 5). Column (6) shows the F -statistic and corresponding p -value from an omnibus F -test of all coefficient estimates in the regression. Manager and business characteristics are defined when the user signs up for the technology. Pre-treatment sales variables include only card sales and are an average over all months from July 2019 to August 2020. Heteroskedasticity-robust standard errors are in parentheses, and p -values for the F -statistics are in square brackets.

Table C.4: Full-Sample vs Survey-Sample Balance

	Non-survey sample (1)	Survey sample (2)	Difference (3)	<i>p</i> -value (4)
Panel A: Manager characteristics				
Female	0.441 (0.497)	0.444 (0.497)	0.003 (0.023)	[0.907]
Age	39.509 (11.03)	39.732 (10.44)	0.223 (0.50)	[0.656]
Panel B: Business characteristics				
<i>Business type</i>				
Small retailers	0.260 (0.438)	0.268 (0.443)	0.008 (0.021)	[0.705]
Professionals	0.238 (0.426)	0.291 (0.455)	0.053 (0.021)	[0.012]**
Beauty	0.088 (0.283)	0.066 (0.248)	-0.022 (0.012)	[0.059]*
Clothing	0.090 (0.285)	0.079 (0.269)	-0.011 (0.012)	[0.381]
Restaurants	0.123 (0.329)	0.110 (0.314)	-0.013 (0.015)	[0.379]
Other	0.202 (0.402)	0.187 (0.390)	-0.015 (0.018)	[0.400]
<i>Pre-treatment sales variables</i>				
Months since first transaction	24.187 (16.94)	23.804 (17.75)	-0.383 (0.82)	[0.641]
% months business made sales	0.818 (0.227)	0.826 (0.220)	0.008 (0.010)	[0.446]
Log monthly card sales volume	8.791 (1.113)	8.757 (1.077)	-0.034 (0.050)	[0.492]
Log monthly card transactions	2.055 (1.423)	2.066 (1.379)	0.011 (0.064)	[0.863]
<i>F</i> -stat of joint test			1.09	[0.365]
Number of observations	33,507	471	33,978	33,978

This table reports differences in characteristics of the manager (typically the firm owner) and of the business by sample. The unit of observation is a firm. Column (1) contains the average mean and standard deviation of the non-survey sample, and column (2) of the survey sample. Column (3) shows the difference between columns (2) and (1) and standard error, and column (4) reports the associated *p*-value of the difference test. Data is from 07/2019 to 08/2020 and includes all firms in the experiment ($N = 33,978$).

Table C.5: Deadline, Reminder and Announced-Reminder Effect – Timing

	Firm accepted offer		
	(1)	(2)	(3)
Deadline	-0.008 (0.005)		
Deadline \times Post reminder	0.012*** (0.003)		
Reminder		-0.005 (0.005)	
Reminder \times Post reminder		0.048*** (0.002)	
Announced reminder			0.009 (0.006)
Announced reminder \times Post reminder			0.010** (0.004)
Number of observations	202,616	202,616	130,032
Number of firms	25,327	25,327	16,254
Cluster std. errors	Firm	Firm	Firm
Fixed effects	Day	Day	Day
Control mean take-up on day 6	0.243	0.244	0.234

This table reports treatment effects of the deadline, reminder and announced reminder groups, comparing take-up on days 1-6 (before the lower fee offer reminder was sent) against take-up on days 7-8 (until the deadline). The unit of observation is a firm-day. Column (1) uses data from 25,327 firms, excluding the control and same-day deadline, no-reminder groups from the full sample of 33,978 firms. Column (2) uses data from 7,172 firms, including only firms that received a reminder. *Post reminder* is an indicator equal to 1 if an observation comes from the time period after the firm received the reminder. Regressions include firm fixed effects. Clustered standard errors at the firm level are included in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.6: Heterogeneous Treatment Effects by Percent Sales Using Technology

	Firm accepted offer			
	(1)	(2)	(3)	(4)
Intercept	0.563*** (0.040)	0.608*** (0.055)	0.476*** (0.110)	0.466*** (0.066)
Above-median % sales using technology	0.108* (0.055)	0.103 (0.076)	0.190 (0.175)	0.134 (0.086)
Deadline		-0.094 (0.081)		
Above-median % sales using technology × Deadline		0.016 (0.111)		
Reminder			0.101 (0.118)	
Above-median % sales using technology × Reminder			-0.096 (0.185)	
Announced reminder				0.201** (0.087)
Above-median % sales using technology × Announced reminder				-0.039 (0.116)
Number of observations	306	306	306	273

This table reports heterogeneous treatment effects of the deadline, reminder and announced reminder groups by the percentage of sales generated using the technology. The unit of observation is a firm. *Above-median % sales using the technology* is an indicator for firms with $\geq 10\%$ of their total sales using the technology. Data comes from survey conducted on a random sample of firms in the experiment ($N = 471$), and includes take-up from September 29, 2020, to March 31, 2021. We construct the variable “% sales using technology” for this table using two questions from the survey. First, managers were asked “*What is the approximate value of all transactions you have made through the technology in the past week?*” 227 managers responded with a positive peso amount, 94 answered 0, 80 said “didn’t know”, 59 refused to answer, and 11 responses were missing. The 227 managers who answered with a positive peso amount were then asked the question “*What share of your total pesos of sales did you make through the technology in the past week?*” 212 provided a valid answer, 10 said “don’t know”, and 5 refused to answer. To construct the variable, out of the 212 valid answers to the latter question were considered and the 94 managers who answered 0 to the former question had their responses coded as 0% ($N = 306$). Columns (1), (2) and (3) include all firms that provided an answer to the percentage sales question, and column (3) includes only firms that received an announced reminder. Robust standard errors are included in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.7: Heterogeneous Treatment Effects by Owner Receiving Emails

	Firm accepted offer			
	(1)	(2)	(3)	(4)
Intercept	0.679*** (0.064)	0.679*** (0.089)	0.556*** (0.166)	0.545*** (0.107)
Owner was recipient of emails	-0.074 (0.069)	-0.065 (0.095)	-0.096 (0.186)	-0.007 (0.113)
Deadline		0.001 (0.129)		
Owner was recipient of emails × Deadline		-0.018 (0.138)		
Reminder			0.149 (0.180)	
Owner was recipient of emails × Reminder			0.011 (0.200)	
Announced reminder				0.318** (0.130)
Owner was recipient of emails × Announced reminder				-0.163 (0.139)
Number of observations	471	471	471	425

This table reports heterogeneous treatment effects of the deadline, reminder and announced-reminder groups by email recipient, namely, an indicator for whether the owner is receiving the corresponding emails. The unit of observation is a firm. *Owner was recipient of emails* is an indicator constructed from answers to the survey question “*Is the account owner the person that receives emails from the Fintech firm?*”, which was asked to all users that took part in the survey. The survey was conducted on a random sample of firms in the experiment ($N = 471$), and includes take-up from September 29, 2020, to March 31, 2021. The answers indicated that the owner receives the email in 88.7% of firms. Columns (1) and (2) include all firms in the survey, and column (3) includes only firms that received an announced reminder. Robust standard errors are included in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.8: Heterogeneous Treatment Effects by Business Type

<i>Treatment:</i>	Firm accepted offer		
	Deadline	Reminder	Announced reminder
	(1)	(2)	(3)
Treatment	-0.014 (0.013)	0.038*** (0.013)	0.018 (0.016)
Small retailers	0.010 (0.012)	-0.003 (0.014)	0.002 (0.014)
Professionals	0.087*** (0.013)	0.077*** (0.014)	0.097*** (0.015)
Beauty	-0.009 (0.016)	-0.038** (0.018)	-0.005 (0.019)
Clothing	0.011 (0.016)	-0.010 (0.018)	0.007 (0.019)
Restaurants	0.012 (0.015)	-0.023 (0.016)	0.004 (0.017)
Small retailers × Treatment	-0.008 (0.017)	0.013 (0.017)	0.019 (0.021)
Professionals × Treatment	0.006 (0.018)	0.020 (0.018)	0.001 (0.023)
Beauty × Treatment	-0.023 (0.022)	0.027 (0.023)	-0.014 (0.029)
Clothing × Treatment	-0.021 (0.023)	0.015 (0.023)	-0.005 (0.029)
Restaurants × Treatment	-0.040* (0.020)	0.023 (0.021)	-0.009 (0.026)
Number of observations	25,327	25,327	16,254

This table reports heterogeneous treatment effects by business type of the client firm. The omitted business-type category is “Other.” The unit of observation is a firm. Data include take-up from September 29 to March 31. Columns (1) and (2) exclude the control and same-day deadline, no-reminder groups from the full sample of 33,978 firms. Column (3) keeps only firms from the announced- and unannounced-reminder groups. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.9: Heterogeneous Treatment Effects of Deadline by Baseline Characteristics

	Firm accepted offer		
	(1)	(2)	(3)
Deadline	-0.028*** (0.008)	-0.026*** (0.008)	-0.015* (0.008)
Above-median manager age	-0.005 (0.009)		
Above-median manager age × Deadline	0.012 (0.012)		
Female manager		-0.027*** (0.009)	
Female manager × Deadline		0.008 (0.012)	
Above-median baseline change in sales			0.042*** (0.008)
Above-median baseline change in sales × Deadline			-0.017 (0.012)
Number of observations	23,614	23,617	25,327

This table reports heterogeneous treatment effects of the deadline by baseline characteristics of the firm. The unit of observation is a firm. *Above-median manager age* is defined as $\text{age} \geq \text{median manager age (37.23)}$. *Female* is equal to 1 for 51% of the sample. *Above-median change in sales* is defined as a percentage change in sales between August and September 2020 (winsorized at the 95th percentile) \geq the median change in sales (-25%). Data includes take-up from September 29, 2020, to March 31, 2021, excluding the control and the same-day deadline, no-reminder groups from the full sample of 33,978 firms. Column (1) includes all firms for which we can identify manager age; column (2) includes all firms for which we can identify manager sex; and column (3) includes all firms in the data sample. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.10: Heterogeneous Treatment Effects of Reminder by Baseline Characteristics

	Firm accepted offer		
	(1)	(2)	(3)
Reminder	0.054*** (0.009)	0.049*** (0.009)	0.045*** (0.008)
Above-median manager age	0.001 (0.010)		
Above-median manager age × Reminder	-0.001 (0.012)		
Female manager		-0.029*** (0.010)	
Female manager × Reminder		0.009 (0.012)	
Above-median baseline change in sales			0.024** (0.009)
Above-median baseline change in sales × Reminder			0.015 (0.012)
Number of observations	23,614	23,617	25,327

This table reports heterogeneous treatment effects of the reminder by baseline characteristics of the firm. The unit of observation is a firm. *Above-median manager age* is defined as $\text{age} \geq \text{median manager age (37.23)}$. *Female* is equal to 1 for 51% of the sample. *Above-median change in sales* is defined as a percentage change in sales between August and September 2020 (winsorized at the 95th percentile) \geq the median change in sales (-25%). Data includes take-up from September 29, 2020, to March 31, 2021, excluding the control and the same-day deadline, no-reminder groups from the full sample of 33,978 firms. Column (1) includes all firms for which we can identify manager age; column (2) includes all firms for which we can identify manager sex; and column (3) includes all firms in the data sample. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.11: Heterogeneous Treatment Effects of Announced Reminder by Baseline Characteristics

	Firm accepted offer		
	(1)	(2)	(3)
Announced reminder	0.012 (0.011)	0.012 (0.011)	0.024** (0.010)
Above-median manager age	-0.009 (0.010)		
Above-median manager age × Announced reminder	0.020 (0.015)		
Female manager		-0.030*** (0.010)	
Female manager × Announced reminder		0.021 (0.015)	
Above-median baseline change in sales			0.042*** (0.010)
Above-median baseline change in sales × Announced reminder			-0.008 (0.015)
Number of observations	15,138	15,141	16,254

This table reports heterogeneous treatment effects of announced reminder by baseline characteristics of the firm. The unit of observation is a firm. *Above-median manager age* is defined as $\text{age} \geq \text{median manager age}$ (37.23). *Female* is equal to 1 for 51% of the sample. *Above-median change in sales* is defined as a percentage change in sales between August and September 2020 (winsorized at the 95th percentile) \geq the median change in sales (-25%). Data includes take-up from September 29, 2020, to March 31, 2021, from the announced- and unannounced-reminder groups. Column (1) includes all firms for which we can identify manager age; column (2) includes all firms for which we can identify manager sex; and column (3) includes all firms in the data sample. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.12: Treatment Effect of Announced Reminder Concentrated Among Low Trust Firms

	Firm accepted offer					
	Trust (1)	Reciprocity (2)	Procrastination (3)	Memory (4)	Overconfidence (5)	Attention (6)
Intercept	0.439*** (0.048)	0.526*** (0.115)	0.538*** (0.044)	0.532*** (0.073)	0.500*** (0.050)	0.387*** (0.088)
Survey measure	0.211*** (0.072)	0.003 (0.121)	-0.029 (0.081)	-0.003 (0.085)	0.063 (0.073)	0.171* (0.097)
Announced reminder	0.303*** (0.062)	0.188 (0.152)	0.206*** (0.058)	0.186** (0.091)	0.252*** (0.064)	0.305** (0.127)
Survey measure × Announced reminder	-0.301*** (0.100)	-0.007 (0.160)	-0.069 (0.106)	-0.007 (0.108)	-0.165 (0.099)	-0.149 (0.137)
Number of firms	388	388	388	388	388	388
Prop. survey measure = 1	0.367	0.895	0.313	0.682	0.421	0.841

This table reports heterogeneous treatment effects of the announced reminder by general social survey (GSS) measures collected in our survey. The survey questions asked respondents how much they agree on a scale from 1 to 5 with the following six statements: (1) *Trust*: I trust advertised offers. (2) *Reciprocity*: I am more inclined to do business with people who live up to their promises. (3) *Procrastination*: I tend to postpone tasks, even when I know it is better to do them immediately. (4) *Memory*: I tend to have good memory about pending tasks that I have to do and complete. (5) *Overconfidence*: I tend to think my memory is better than it really is. (6) *Attention*: I can focus completely when I have to finish a task. The indicator *High survey measures* codes responses of 4 and 5 (agree and completely agree) as 1 and 1-3 (completely disagree, disagree and neither agree nor disagree) as 0. The unit of observation is a firm. Data includes firms with announced and unannounced reminders in the survey sample and take-up from September 29, 2020, to March 31, 2021. All firms in the survey were asked these questions. 43 firms that did not answer the question were excluded from the sample. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.13: Heterogeneous Treatment Effects of Unannounced Reminder by Survey Measures

	Firm accepted offer					
	Trust (1)	Reciprocity (2)	Procrastination (3)	Memory (4)	Overconfidence (5)	Attention (6)
Intercept	0.406*** (0.088)	0.600*** (0.221)	0.586*** (0.092)	0.278*** (0.107)	0.370*** (0.094)	0.273** (0.135)
Unannounced reminder	0.033 (0.100)	-0.074 (0.249)	-0.048 (0.102)	0.254* (0.129)	0.130 (0.106)	0.114 (0.162)
High survey measure	0.344* (0.178)	-0.143 (0.237)	-0.404*** (0.149)	0.359** (0.149)	0.322** (0.160)	0.279* (0.164)
High survey measure × Unannounced reminder	-0.133 (0.192)	0.146 (0.266)	0.376** (0.170)	-0.362** (0.171)	-0.259 (0.176)	-0.108 (0.191)
Number of observations	227	227	227	227	227	227
Prop. high survey measure = 1	0.367	0.895	0.313	0.682	0.421	0.841

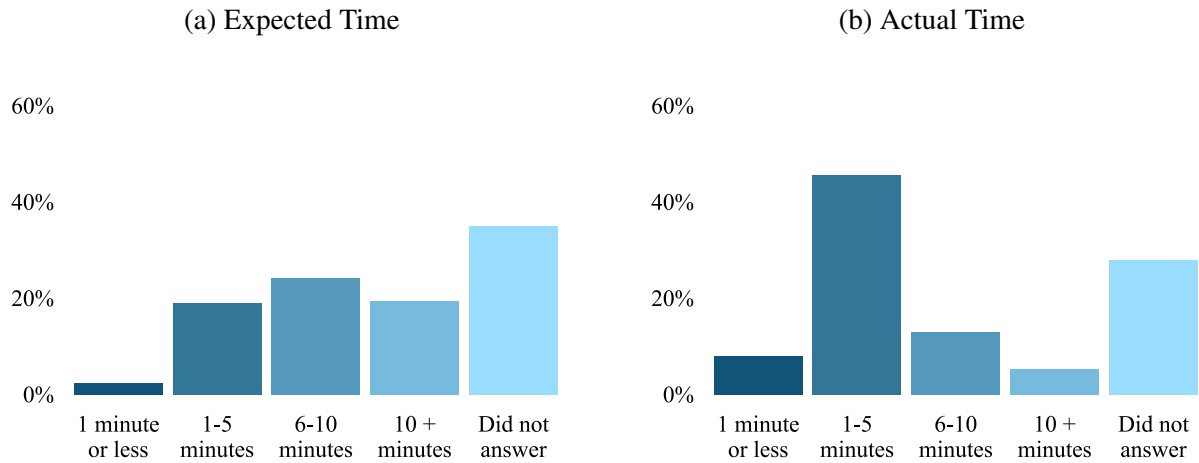
This table reports heterogeneous treatment effects of the unannounced reminder by general social survey (GSS) measures collected in our survey. The survey questions asked respondents how much they agree on a scale from 1 to 5 with the following six statements: (1) *Trust*: I trust advertised offers. (2) *Reciprocity*: I am more inclined to do business with people who live up to their promises. (3) *Procrastination*: I tend to postpone tasks, even when I know it is better to do them immediately. (4) *Memory*: I tend to have good memory about pending tasks that I have to do and complete. (5) *Overconfidence*: I tend to think my memory is better than it really is. (6) *Attention*: I can focus completely when I have to finish a task. The indicator *High survey measures* codes responses of 4 and 5 (agree and completely agree) as 1 and 1-3 (completely disagree, disagree and neither agree nor disagree) as 0. The unit of observation is a firm. Data includes firms with unannounced and no reminders in the survey sample and take-up from September 29, 2020, to March 31, 2021. All firms in the survey were asked these questions. 43 firms that did not answer the question were excluded from the sample. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C.14: Account Log-ins by Treatment

	Firm logged in (1)	Firm viewed deposits (2)
Intercept	0.092*** (0.003)	0.037*** (0.002)
Announced reminder	-0.003 (0.004)	0.000 (0.003)
Unannounced reminder	0.000 (0.004)	0.000 (0.003)
Same-day deadline	0.000 (0.005)	0.002 (0.003)
One-week deadline	-0.001 (0.003)	0.000 (0.002)
2.75% offer	0.006* (0.003)	-0.001 (0.002)
Number of observations	33,978	33,978

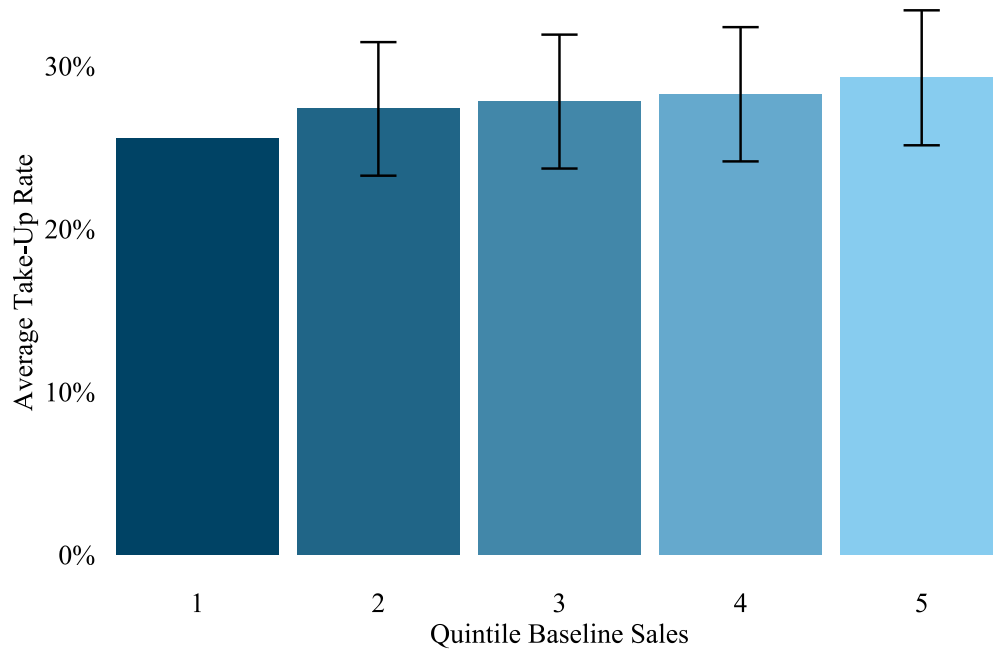
This table reports differences in the probability of a firm logging into the FinTech platform (column 1) and viewing deposits by treatment (column 2), measured from day 1 to day 8 of the experiment (until the one-week deadline). The unit of observation is a firm. The sample includes all firms in the experiment ($N = 33,978$). Robust standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure C.1: Self-Reported Time Cost of Accepting the Offer



The figure shows how long firms expected it would take them to fill out the form to accept the offer (left panel) and how long it actually took them (right panel). The left panel shows responses to the survey question: *“How long did you expect completing the form to activate the lower fee would take you?”* This question was asked to users who recall receiving the first email or SMS ($N = 289$). The right panel shows responses to the survey question: *“How long did it take you to fill out the offer?”* This question was asked to respondents who recalled receiving the first email or SMS and recalled accepting the offer or clicking on the link in the email to accept the offer ($N = 186$).

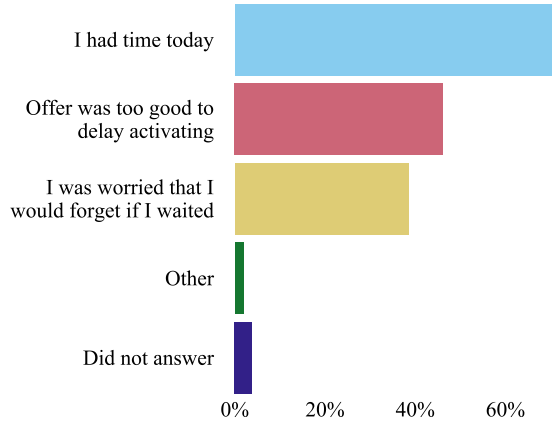
Figure C.2: Take-Up by Baseline Sales Quintiles for No-Deadline, No-Reminder Group



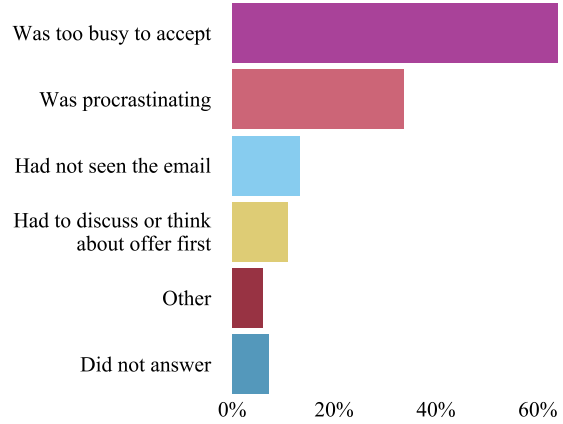
This figure shows the average take-up rate by quintile of baseline sales for firms in the no-deadline, no-reminder group ($N = 4,455$). Data include take-up from September 29, 2020, to March 31, 2021. Baseline sales is defined as the winsorized average monthly sales volume from September 2019 to August 2020. Coefficient estimates and 95% confidence intervals come from a regression of take-up on quintile dummies, with heteroskedasticity-robust standard errors. Average take-up rate for the no-deadline, no-reminder group is 27.7%. The difference in take-up rates between the fifth quintile (29.1%) and the first quintile (25.7%) is statistically significant at the 10% level ($p = 0.080$).

Figure C.3: Why Firms Accepted the Offer On or After the First Day

(a) Why Firms Accepted the Offer on the First Day



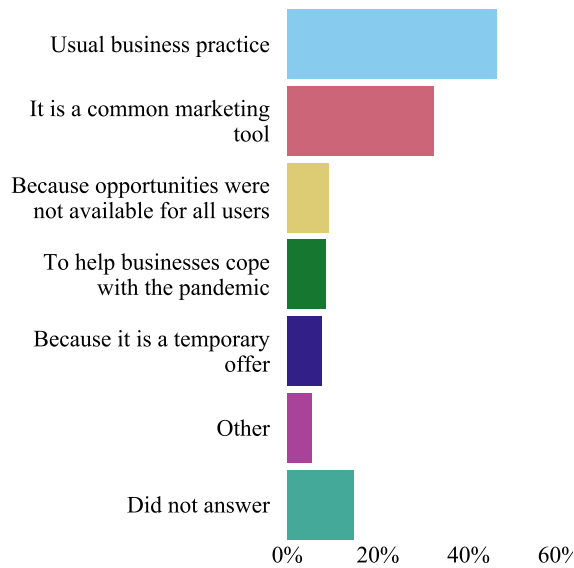
(b) Why Firms Accepted the Offer After the First Day



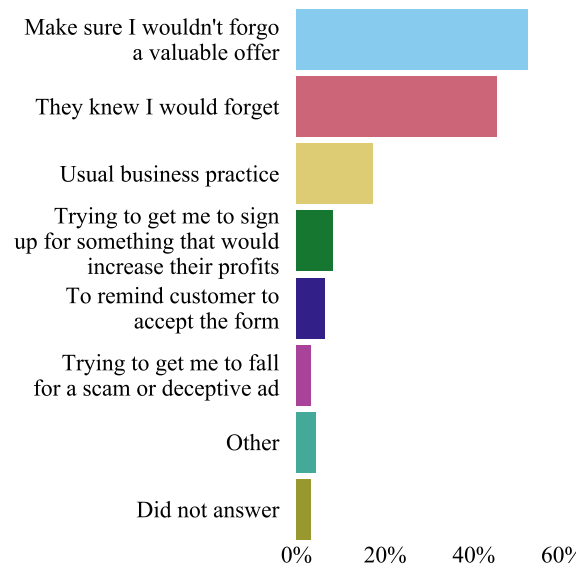
The left panel shows responses to the survey question: “Our records show that you activated the offer on September 29, even though your deadline to activate the offer was not until October 6. Why did you activate the offer on September 29?” This question was asked to firms that recall receiving the first email or SMS, received a deadline, and accepted the offer on day 1 ($N = 52$). The right panel shows responses to the survey question: “We sent you the emails and SMS to let you know about this offer on September 29, but we see that you filled the form on {activation date}. Why did you wait until {days to accept} day(s) later?” This question was asked to firms that recalled receiving the first email or SMS, had a deadline, accepted the offer after day 1, and recalled accepting the offer or clicking the link to accept the offer ($N = 83$). Respondents could provide more than one response, so totals add up to more than 100%.

Figure C.4: Why Firms Thought the Offer Had a Deadline and Reminder

(a) Why Firms Thought the Offer Had a Deadline

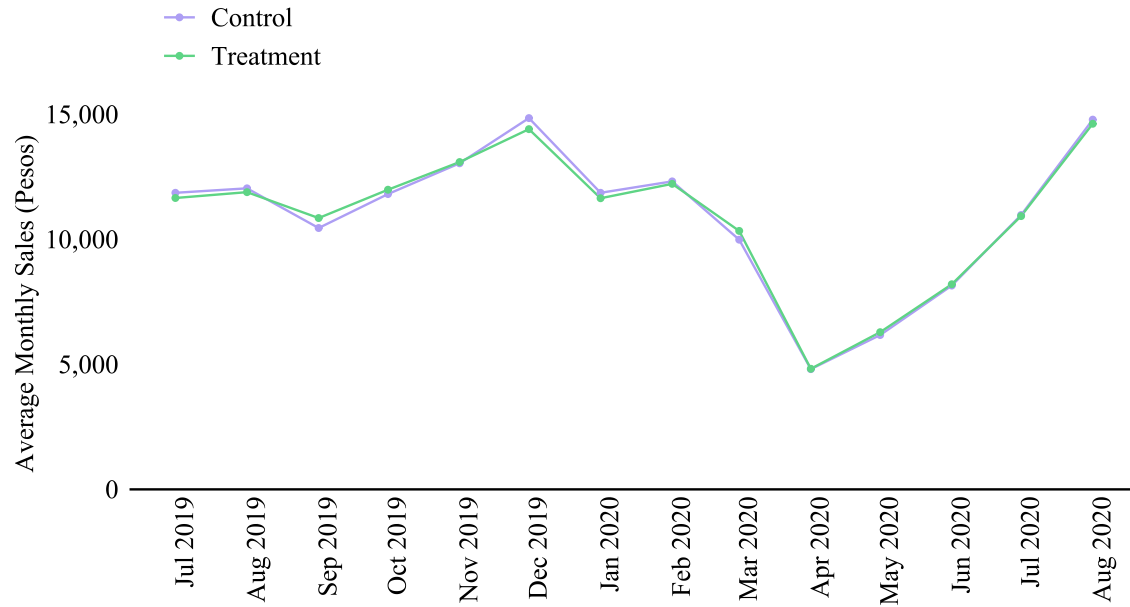


(b) Why Firms Thought they Had Been Sent a Reminder



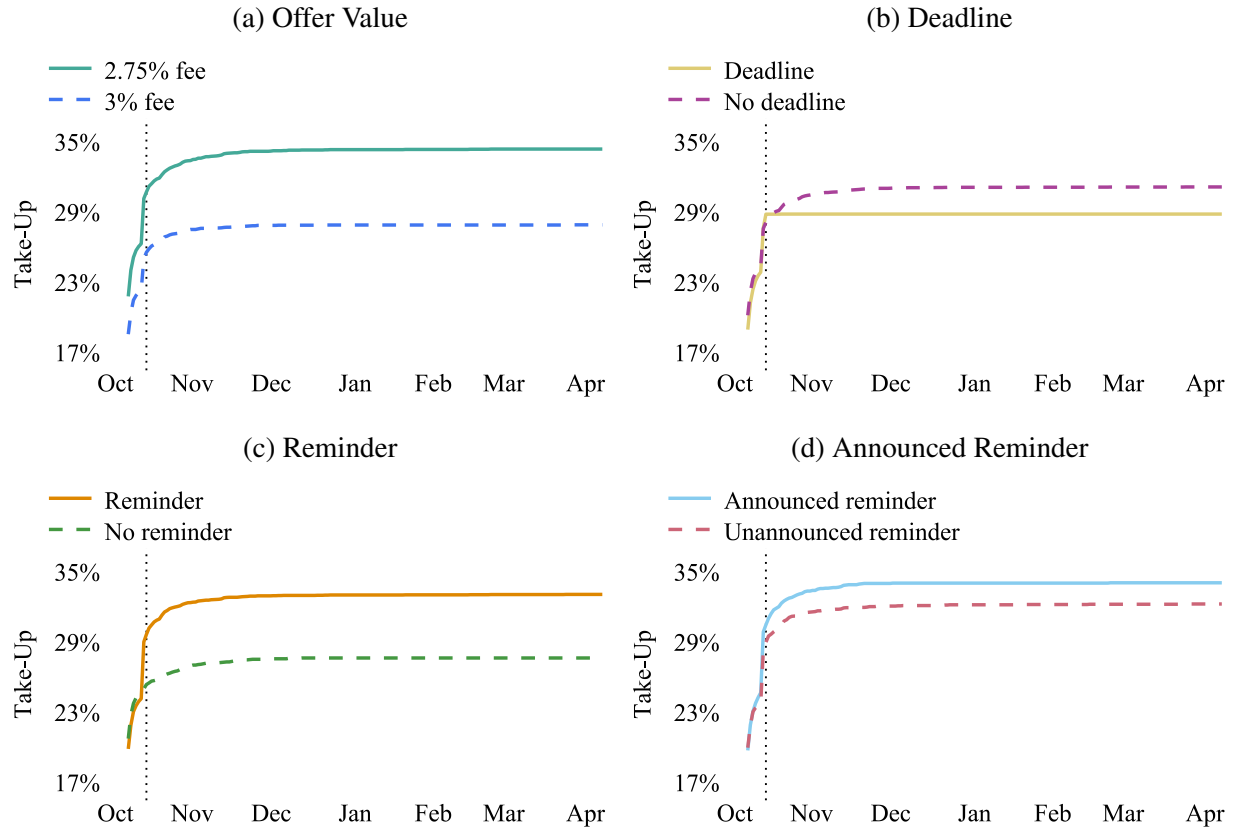
The left panel shows responses to the survey question: “*Why do you think the offer had a deadline?*” This question was asked to firms that recalled receiving the first email or SMS, had a deadline, and recalled that the offer had a deadline ($N = 130$). The right panel shows responses to the question: “*Why do you think we sent you a reminder?*” This question was asked to firms that recalled receiving the first email or SMS, were assigned to receive a reminder, did not accept the offer prior to the reminder, and recalled receiving the reminder ($N = 157$). Respondents could provide more than one response, so totals add up to more than 100%.

Figure C.5: Pre-Treatment Sales



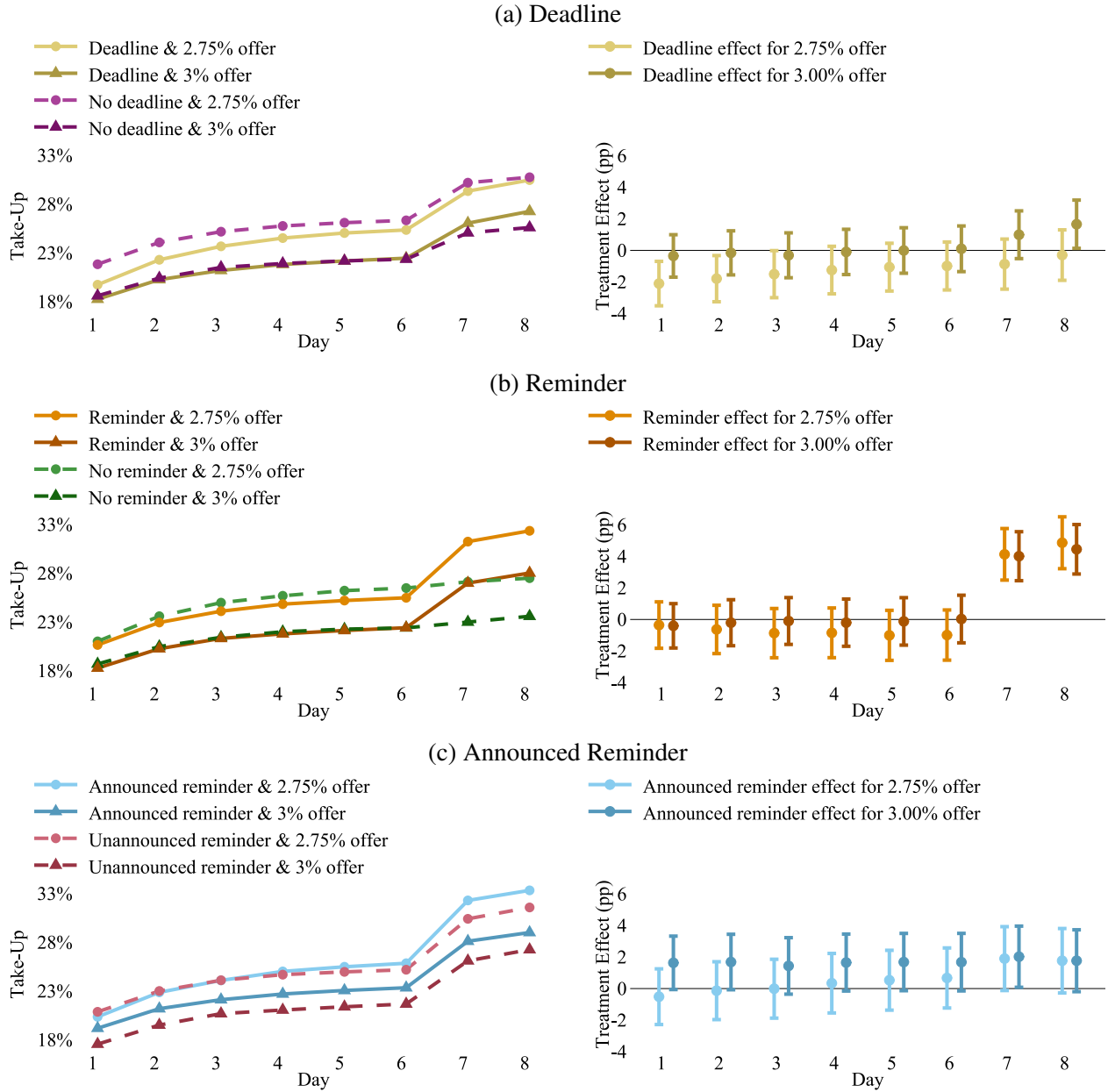
The figure shows average winsorized monthly sales for the control group (Group 1) and the pooled treatment groups (Groups 2 through 8). Sales fell in March and April 2020 due to the COVID-19 pandemic and recovered to pre-pandemic levels in August 2020. Monthly sales are winsorized at the 95th percentile within each treatment group and month. Data includes all firms in the experiment ($N = 33,978$) from July 2019 to August 2020 (14 months of data per firm).

Figure C.6: Long-Term Take-Up by Treatment Arm



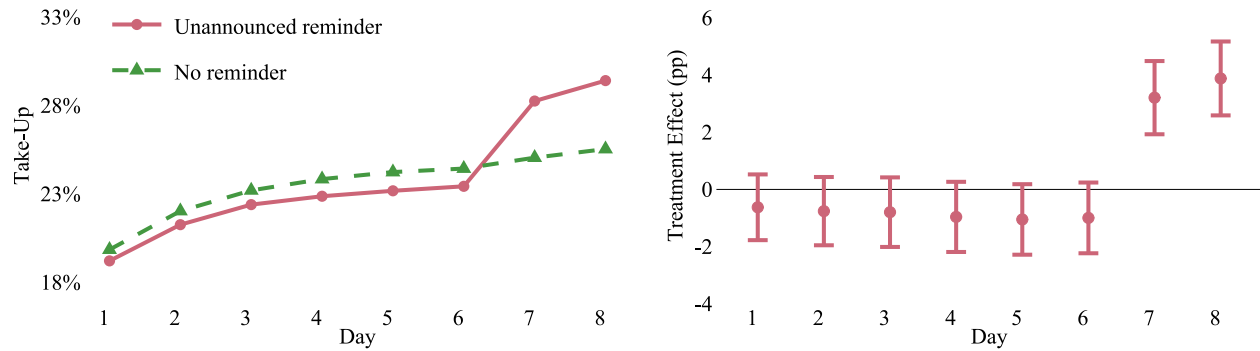
The four panels show long-term take-up by 2.75% fee - Fixed by Mohammad], deadline, reminder, and announced reminder groups. The unit of observation is a firm. The dotted vertical line indicates the day of the deadline. Data includes take-up from September 29, 2020, to March 31, 2021. Panel (a) includes 17,220 firms with 2.75% and 3.00% offers and no deadline. Panel (b) includes 25,327 firms with and without a deadline, excluding the same-day deadline group. Panel (c) includes 12,579 firms with and without reminders, restricted to firms without a deadline. Panel (d) includes 8,124 firms with announced or unannounced reminders, restricted to firms without a deadline.

Figure C.7: Heterogeneous Treatment Effects by Offer Value



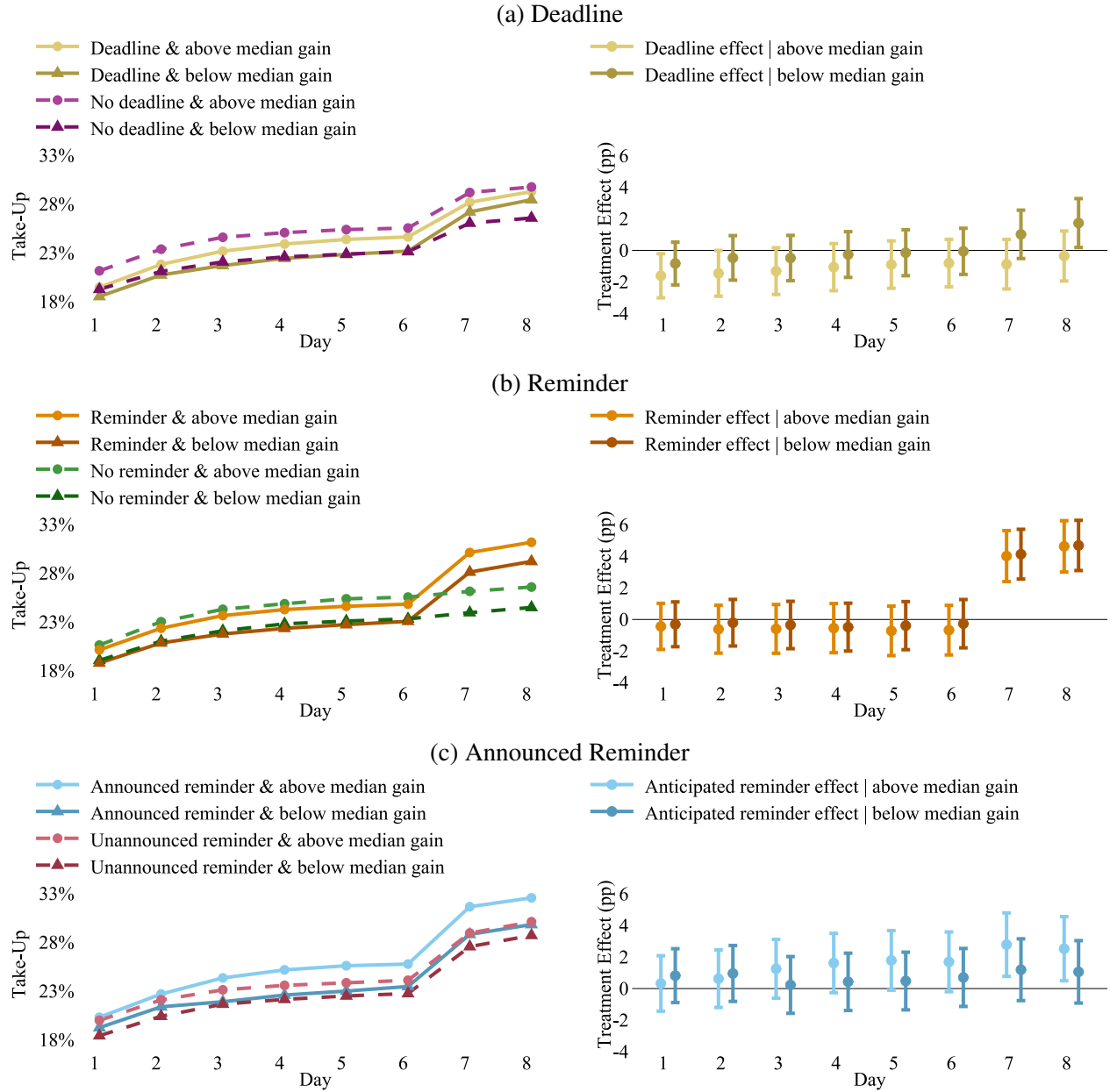
The figure shows average take-up rates (line graphs on the left) and heterogeneous treatment effects (coefficient graphs on the right) by fee level of the offer and, respectively, by reminder, deadline and announced reminder groups, and separately for each day of the experiment. The unit of observation is a firm. Coefficient estimates and 95% confidence intervals come from daily regressions of take-up on treatment, a dummy indicating 2.75% fee, and the interaction between treatment and the 2.75% fee dummy, controlling for strata fixed effects. Data includes take-up from September 29 to October 6 (the day of the deadline). Panel (a) includes 25,327 firms with and without deadlines, excluding the same-day deadline group. Panel (b) includes 25,327 firms with and without reminders, excluding the same-day deadline group. Panel (c) includes 16,254 firms with announced and unannounced reminders, excluding firms without reminders.

Figure C.8: Treatment Effect of Unannounced vs. No Reminder



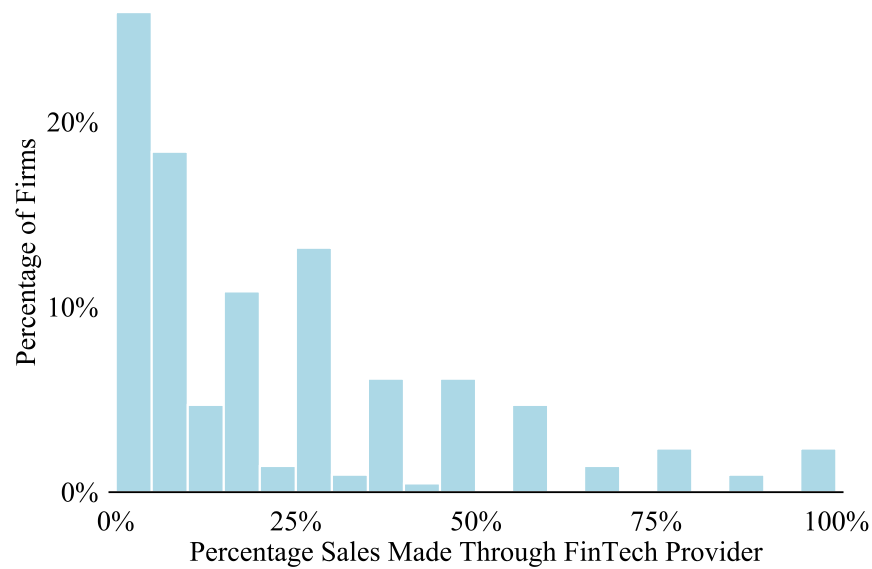
The figure shows average take-up rates (line graphs on the left) and treatment effects (coefficient graphs on the right) for the unannounced-reminder versus no-reminder groups, separately for each day of the experiment. The unit of observation is a firm. Coefficient estimates and 95% confidence intervals come from daily regressions of take-up on the unannounced-reminder treatment, controlling for strata fixed effects. Data include take-up from September 29 to October 6 (the day of the deadline). The figure includes 18,155 firms with unannounced reminders and no reminders, excluding the same-day deadline group.

Figure C.9: Heterogeneous Treatment Effects by Expected Gain from Take-Up



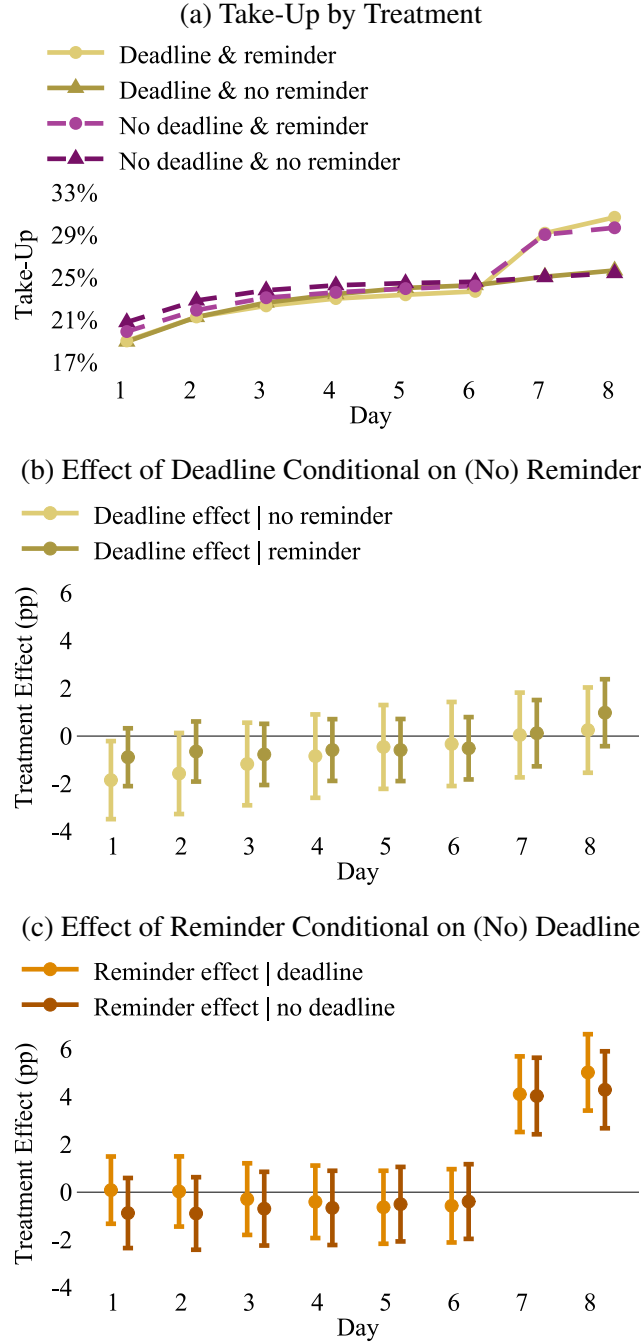
The figure shows average take-up rates (line graphs on the left) and heterogeneous treatment effects (coefficient graphs on the right) by expected gain from take-up and, respectively, by reminder, deadline and announced reminder groups, and separately for each day of the experiment. The unit of observation is a firm. Coefficient estimates and 95% confidence intervals come from daily regressions of take-up on treatment, a dummy indicating above-median expected gain, and the interaction between treatment and the above-median expected gain dummy, controlling for strata fixed effects. Data includes take-up from September 29 to October 6 (the day of the deadline). Panel (a) includes 25,327 firms with and without deadlines, excluding the same-day deadline group. Panel (b) includes 25,327 firms with and without reminders, excluding the same-day deadline group. Panel (c) includes 16,254 firms with announced and unannounced reminders, excluding firms without reminders.

Figure C.10: Percent of Sales Made Through FinTech Provider in Prior Week



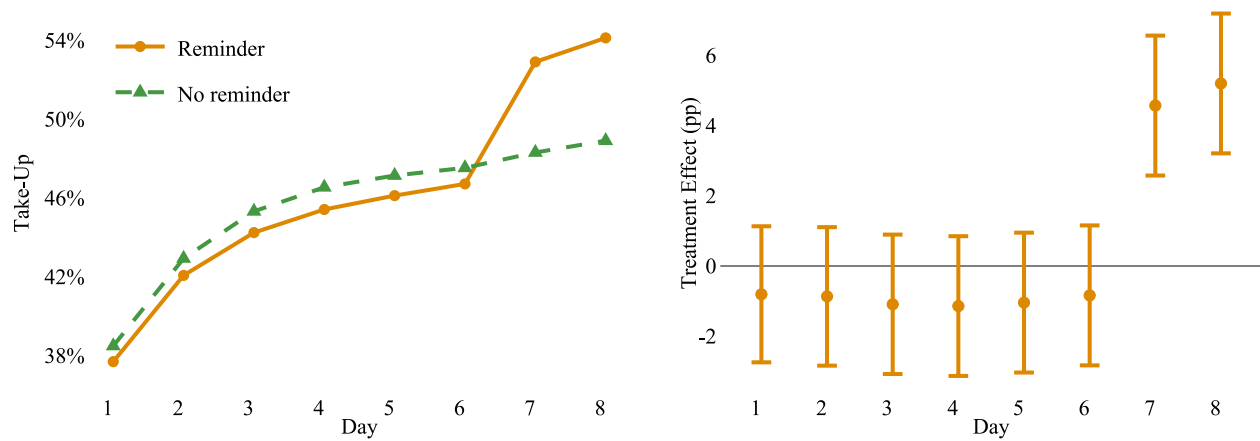
Note: This figure contains a histogram of the percentage of weekly sales made through the FinTech provider, constructed from the survey question “What share of your total pesos of sales did you make through (provider) in the past week?” The survey was conducted on a random sample of firms in the experiment ($N = 471$), with 227 firms asked what their previous fee was. 10 firms that indicated that they did not know the answer to the question and 5 firms that did not answer the question were excluded from the sample. Percentage sales mean = 24.9, median = 20, standard deviation = 23.7.

Figure C.11: Interaction Effects of Reminder and Deadline



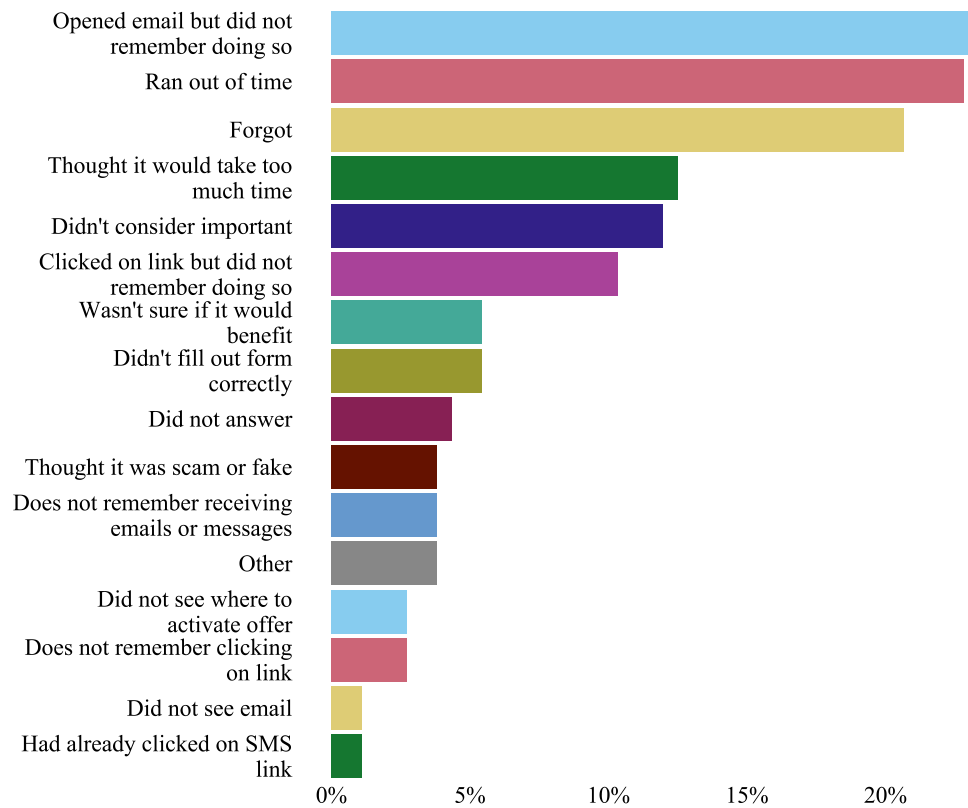
The figure shows average take-up rates (line graphs in panel (a)) and heterogeneous treatment effects (coefficient graphs in panels (b) and (c)) by reminder and deadline groups. The unit of observation is a firm. Coefficient estimates and 95% confidence intervals come from daily regressions of take-up on treatment, a deadline dummy, a reminder dummy and the interaction between the deadline and reminder dummies: $Accepted_i = \alpha_{s(i)} + \beta_1 \mathbb{1}(Deadline)_i + \beta_2 \mathbb{1}(Reminder)_i + \beta_3 \mathbb{1}(Deadline)_i \times \mathbb{1}(Reminder)_i + \varepsilon_i$. Regressions include strata fixed effects. Panel (b) compares $\beta_1 + \beta_3$ against β_1 , while Panel (c) compares $\beta_2 + \beta_3$ against β_2 . Data includes take-up from September 29 to October 6 (the day of the deadline), with 25,327 firms with reminders and no reminders and deadlines and no deadlines, excluding the same-day deadline group.

Figure C.12: Effect of Reminder on Take-Up Conditional on Opening Email



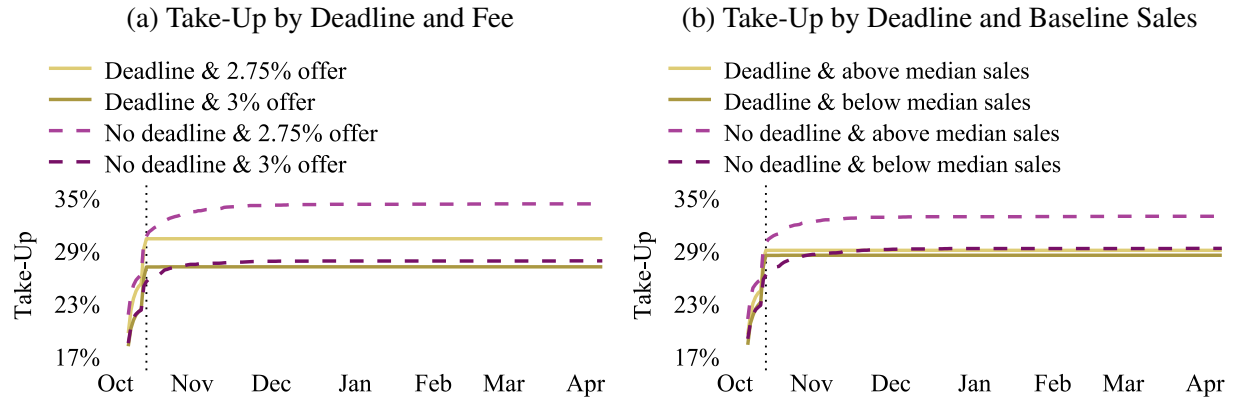
The figure shows take-up rates (line graphs on the left) and treatment effects (coefficient graphs on the right) of the unannounced reminder, separately for each day of the experiment, for firms that opened the email before the day of the reminder. The unit of observation is a firm. Coefficient estimates and 95% confidence intervals come from daily regressions of take-up on a dummy of the unannounced reminder group, including strata fixed effects. Data includes take-up from September 29 to October 6 (the day of the deadline), with 10,246 firms with unannounced reminders and no reminders that opened the email before the reminder.

Figure C.13: Reasons Why Firms Did Not Adopt Offer



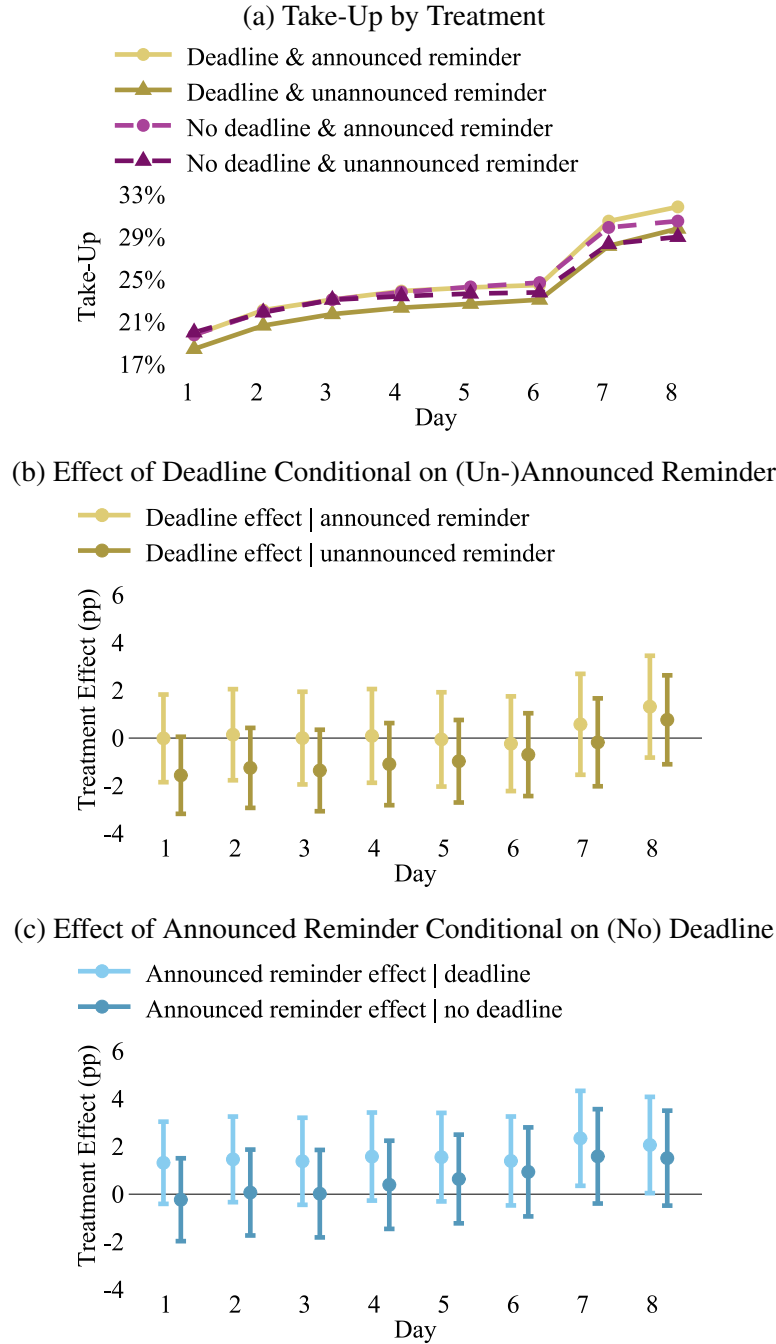
The figure shows a barplot with the reasons given by firms for not adopting the offer. The survey was fielded on a random subsample of $N = 471$ firms, and this figure includes 169 firms that did not adopt the offer. The figure combines responses from the following survey questions: If our administrative data show that the respondent did not open the email, we ask “*Our records show that you did not open the email. Why not?*” If our administrative data show that the respondent opened the email but did not click the link, we ask “*What prevented you from clicking the link and filling out the form?*” If our administrative data show that the respondent clicked the link but did not complete the form, we ask “*We observe that you did not complete the form after clicking the offer. Why did you not complete the form?*” The categories “Other” (7 firms) and “Did not answer” (8 firms) are omitted from the plot.

Figure C.14: Long-Term Take-up by Deadline, Offer Value and Firm Size



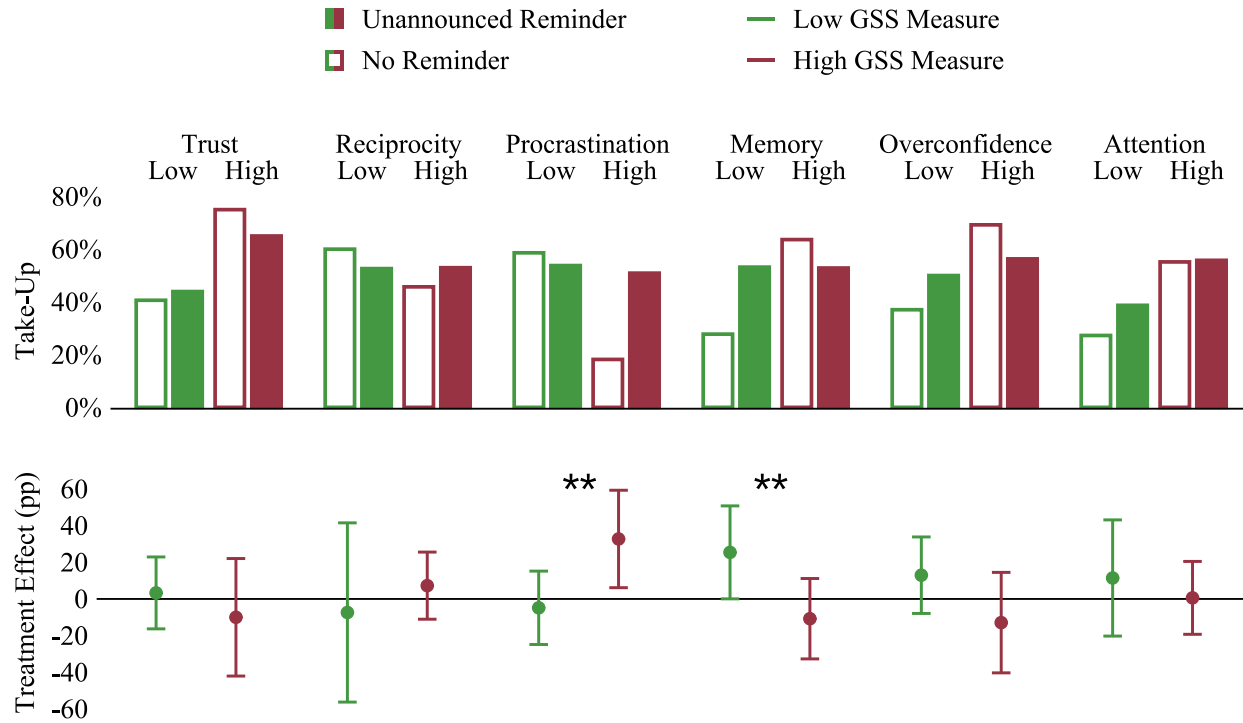
The figure shows long-term take-up by deadline, offer value and firm size. The unit of observation is a firm. The dotted vertical line indicates the day of the deadline. Data includes take-up from September 29 to March 31 by 25,327 firms with and without a one-week deadline.

Figure C.15: Interaction Effects of Deadline and (Un-)Announced Reminder



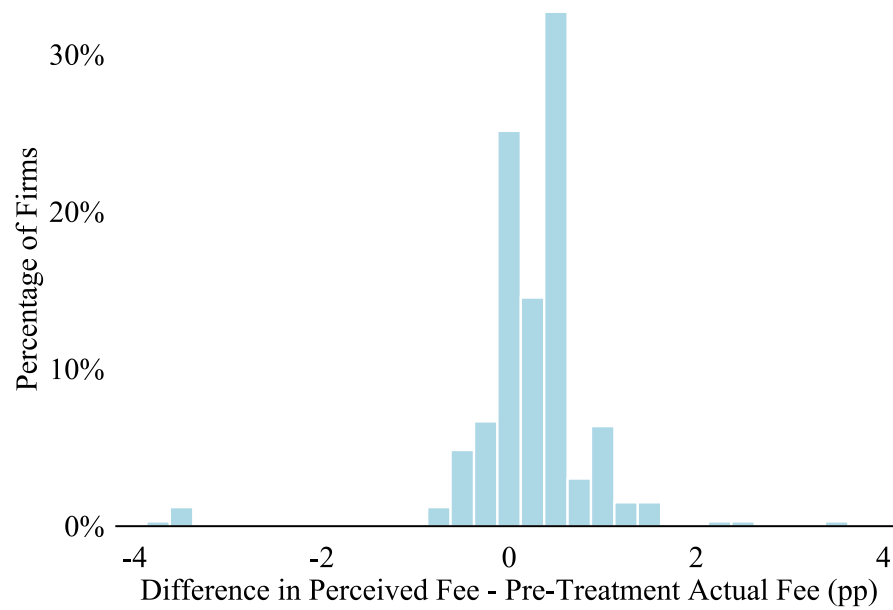
The figure shows take-up and heterogeneous treatment effects by reminder and announced reminder groups. The unit of observation is a firm. Panel (a) shows average take-up rates by treatment group. Panels (b) and (c) show coefficient estimates and 95% confidence intervals come from daily regressions of take-up on treatment, a deadline dummy, a announced reminder dummy and the interaction between the deadline and announced reminder dummies: $Accepted_i = \alpha_{s(i)} + \beta_1 \mathbb{1}(Deadline)_i + \beta_2 \mathbb{1}(Announced\ reminder)_i + \beta_3 \mathbb{1}(Deadline)_i \times \mathbb{1}(Announced\ reminder)_i + \varepsilon_i$. Regressions include strata fixed effects. Panel (b) compares $\beta_1 + \beta_3$ against β_1 , while panel (c) compares $\beta_2 + \beta_3$ against β_2 . Data includes take-up from September 29 to October 6 (the day of the deadline), with 16,254 firms with announced and unannounced reminders, excluding firms without reminders.

Figure C.16: Heterogeneous Effect of Unannounced Reminder by Survey Measures



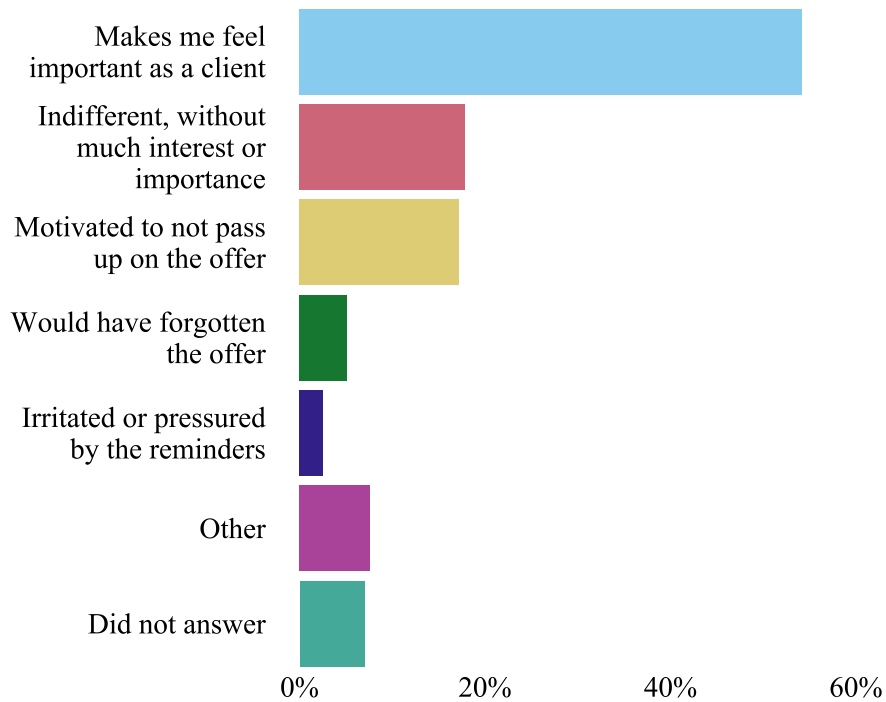
The table reports heterogeneous treatment effects by survey measure. The unit of observation is a firm. The coefficients come from the regression of take-up on unannounced reminder, the survey measure, and the interaction between unannounced reminder and the survey measure. Data includes firms with unannounced reminders and no reminders in survey sample, and includes take-up from September 29 to March 31. All firms in the survey were asked these questions. The survey question asked respondents whether they agreed or disagreed with the following six statements: (1) *Trust*: I trust advertised offers. (2) *Reciprocity*: I am more inclined to do business with people who live up to their promises. (3) *Procrastination*: I tend to postpone tasks, even when I know it is better to do them immediately. (4) *Memory*: I tend to have good memory about pending tasks that I have to do and complete. (5) *Overconfidence*: I tend to think my memory is better than it really is. (6) *Attention*: I can focus completely when I have to finish a task. The scale of these responses is 1 to 5, where 5 is highest level of agreement and 1 highest level of disagreement. Binary measure variables were created from these responses, coding 4 and 5 (agree and completely agree) as 1 and 1-3 (completely disagree, disagree and neither agree nor disagree) as 0. 43 firms that did not answer the question were excluded from the sample. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure C.17: Difference in Pre-Treatment Actual Fee and Perceived Fee



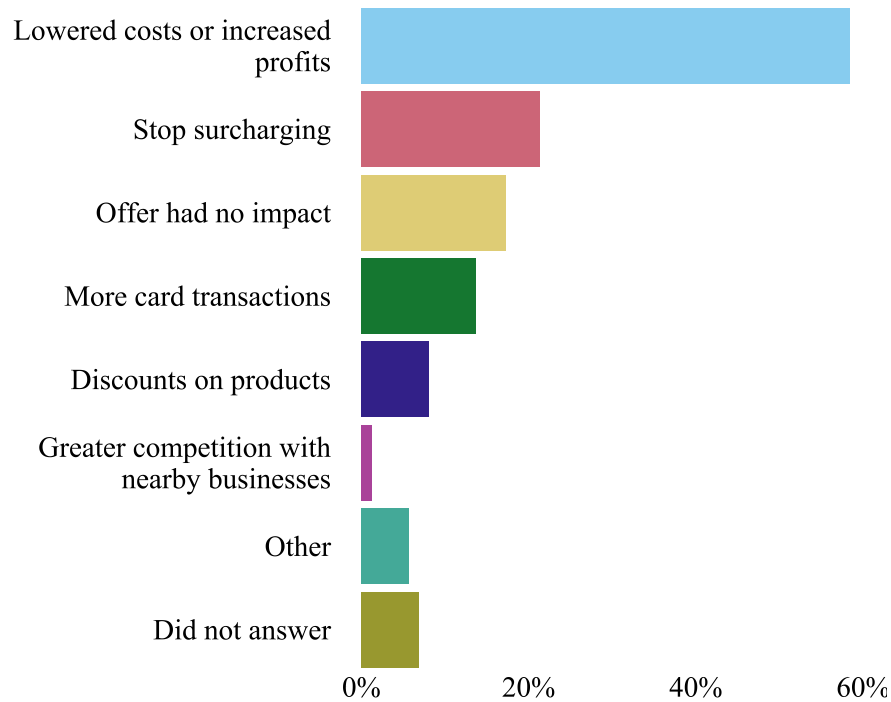
This figure shows a histogram of the differences in firms' actual and perceived pre-treatment fees, with a mean of 0.3, median of 0.2, and standard deviation of 0.7. Data comes from survey conducted on a random sample of firms in the experiment ($N = 471$) with 471 firms asked what their previous fee was. Survey question: *What was your commission with (provider) the week before you received the offer?* 141 firms were excluded from the sample, including 118 firms that answered that they did not know, and 23 firms that did not answer the question.

Figure C.18: How Firms Felt About Receiving Reminder



The figure contains a barplot showing how firms felt when receiving the reminder. Data comes from survey conducted on a random sample of firms in the experiment ($N = 471$), with 157 firms asked this question. This question was asked to firm owners assigned to the announced or unannounced reminder arms who recalled receiving the first email or SMS, did not adopt prior to the reminder, and recalled receiving a reminder, received an offer with a reminder, and accepted the offer after receiving the reminder or did not accept the offer. Survey question: *How did receiving the reminder make you feel?* Respondents could provide more than one response, so totals add up to more than 100%. 23 firms were excluded from the sample, including 12 firms giving other responses, and 11 firms that did not know the answer to the question.

Figure C.19: Impact of Take-Up on Business Outcomes



The figure contains a barplot with the impact the lower fee offer had on firms. Data comes from survey conducted on a random sample of firms in the experiment ($N = 471$). with 248 firms asked this question. This question was asked to users who accepted the survey, and recalled accepting or clicking on the offer. Survey question: *Is this offer working for your business? What impact has it had?* Respondents could provide more than one reason for not accepting the offer on the first day, so totals add up to more than 100%. 28 firms were excluded from the sample, including 11 firms giving other responses, 5 firms that did not know the answer to the question, and 12 firms that did not answer to the question.

D Heterogeneous Effect of Announced Reminder by Length of Business Relationship

In this appendix, we detail our methodology for testing whether the treatment effect of the announced reminder relative to the unannounced reminder is decreasing in the length of a firm's business relationship with the FinTech company. We find evidence that the effect of announced reminders is decreasing in the length of the business relationship.

D.1 Framework for Estimating Heterogeneous Announced Reminder Effect

We restrict the sample to managers who were randomized to receive either an announced reminder or an unannounced reminder. We express expected take-up as a conditional expectation, conditional on covariates and “treatment,” i.e., whether the manager received an announced reminder. We assume this conditional expectation is a sum of two polynomials: (i) f , a function of length of business relationship and the announced reminder dummy; and (ii) g , a function of covariates and the announced reminder dummy. That is,

$$\mathbb{E}[Y | X, T] = \mathbb{E}[Y | X_1, X_{-1}, T] = f(X_1, T) + g(X_{-1}, T), \quad (29)$$

where $Y \in \{0, 1\}$ represents take-up of the profitable opportunity; $T \in \{0, 1\}$ represents whether the reminder was unannounced or announced; and $X \in \mathbb{R}^p$ represents a p -dimensional vector of all the observable characteristics. We divide X into $X_1 \in \mathbb{R}$, which represents the length of business relationship, and $X_{-1} \in \mathbb{R}^{p-1}$, which represents a $(p - 1)$ -dimensional vector of observable characteristics, excluding the length of business relationship.

Following Imbens and Rubin (2015), we write the conditional average treatment effect (CATE) as

$$\tau(x) \equiv \mathbb{E}[Y(T = 1) - Y(T = 0) | X = x]. \quad (30)$$

Taking the stable unit treatment values assumption (SUTVA) and the overlap assumption (i.e., $0 < \Pr(T = 1 | X) < 1$) as given and noting that the announced reminder (T) is randomly assigned, we can rewrite equation (30) as

$$\tau(x) = \mathbb{E}[Y | X_1, X_{-1}, T = 1] - \mathbb{E}[Y | X_1, X_{-1}, T = 0]. \quad (31)$$

Plugging equation (29) into equation (31), we can rewrite the CATE as

$$\tau(x) = [f(X_1, T = 1) + g(X_{-1}, T = 1)] - [f(X_1, T = 0) + g(X_{-1}, T = 0)].$$

Hence, we can predict the CATE:

$$\hat{\tau}(x) = [\hat{f}(X_1, T = 1) + \hat{g}(X_{-1}, T = 1)] - [\hat{f}(X_1, T = 0) + \hat{g}(X_{-1}, T = 0)]. \quad (32)$$

D.2 Model Specification

We use a penalized regression model, ridge regression, to estimate heterogeneous treatment effects of the announced reminder. We use the *glmnet* package (Friedman, Hastie, and Tibshirani, 2010) with 10-fold cross-validation (CV) to select the penalty parameter that minimizes the mean squared error. The reason we use ridge regression is that we need to extract coefficient estimates from our model to test whether the treatment effect is decreasing in the length of business relationship. Hence, although alternative approaches to estimate heterogeneous treatment effects exist, such as incorporating neural networks (Farrell, Liang, and Misra, 2021), we intentionally refrain from using these more complex models.

We consider various potential model specifications, where we vary the following: (i) the degree of the polynomials f and g , which can each vary from degree-one to degree-four; (ii) winsorization of the top 5% of the continuous covariates; and (iii) standardization of the covariates.²⁵ Henceforth, a notation like $(2, 1)$ represents a model configuration where the polynomial degree of f is 2, and that of g is 1. We evaluate which of these potential models performs best using the out-of-sample root mean squared error (OOS RMSE).

Specifically, we calculate the OOS RMSE from 64 distinct ridge regression models (where $64 = 4$ potential degrees for $f \times 4$ potential degrees for $g \times 2$ winsorized or non-winsorized $\times 2$ standardized or non-standardized). For each model, this involves a four-step process:

1. Divide our dataset into a training set and a hold-out test set using a 9:1 ratio.
2. Train our model on the training set, including performing 10-fold cross-validation within the training set.
3. Use the trained model to predict the outcome (take-up) on the test set.
4. Compute the OOS RMSE by comparing predicted take-up to observed take-up for each observation in the test set.

To compare the 64 models and account for randomness in the process of splitting the data, we repeat the above process using 300 distinct seeds, which determine 300 distinct random splits

²⁵We do not consider polynomials higher than fourth-order because the fourth-degree polynomials already have substantially worse out-of-sample fit than lower-order polynomials due to overfitting. We do not winsorize the bottom 5% of the continuous covariates as they are all bounded below by 0. Standardization converts each covariate to a Z-score by subtracting the sample mean and then dividing the demeaned variable by the standard deviation.

of the training and testing sets.²⁶ We then compare the percentage of instances (across the 300 seeds) in which each model outperforms—i.e., has a lower OOS RMSE than—the benchmark non-winsorized and non-standardized (1, 1) model.

Figure D.1 shows the results of this comparison. We find that models incorporating higher-degree polynomials, such as the $(\cdot, 4)$ models, generally exhibit poorer performance. This occurs to an even greater extent for the standardized models and is likely due to overfitting. Among the 64 models, we observe that the (2, 2) model with no winsorization or standardization yields the best fit on average; thus, we select this model for the remainder of the analysis.

D.3 Results

Using the (2, 2) ridge regression model without winsorization or standardization, we estimate equation (32). We test whether the treatment effect of the announced reminder relative to the unannounced reminder is decreasing in the length of business relationship. In other words, we test whether $\hat{\tau}(x)$ is decreasing in X_1 . Specifically, we test whether

$$\frac{\partial \hat{\tau}(x)}{\partial X_1} = \frac{\partial \hat{f}(X_1, T = 1)}{\partial X_1} - \frac{\partial \hat{f}(X_1, T = 0)}{\partial X_1} \quad (33)$$

is negative at various values of X_1 , including the 25th percentile (11.8 months), median (20 months), mean (24.2 months), and 75th percentile (33.1 months).

To formally test whether (33) is negative, we employ a bootstrapping method with 1,000 iterations, and document the percentage of bootstrap iterations in which (33) is negative. Algorithm 1 describes our exact bootstrapping procedure.

Algorithm 1 Bootstrapping Algorithm

Require: Original data set \mathcal{D} of size $|\mathcal{D}|$ and number of resamples $B = 1000$

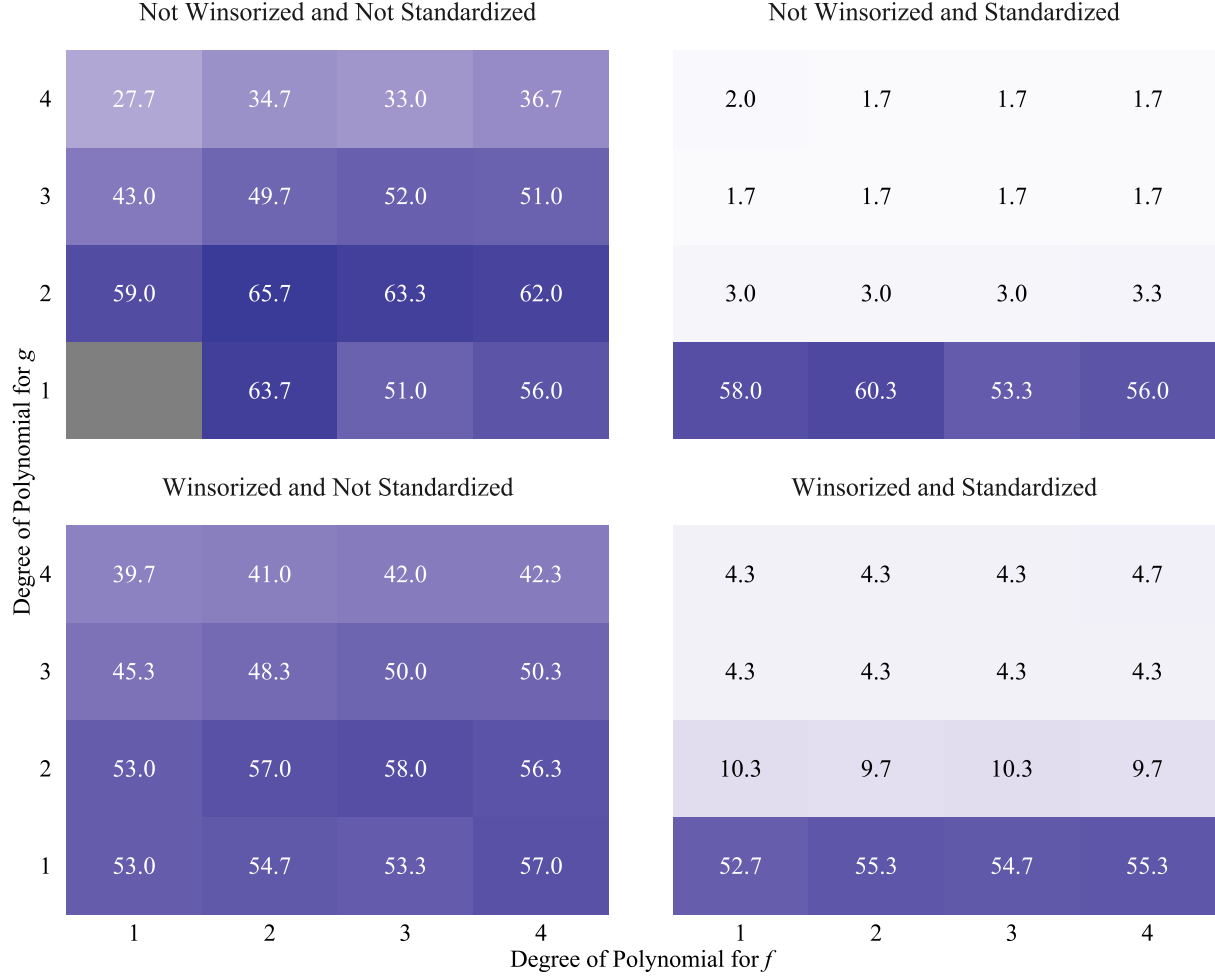
- 1: **for** $b = 1$ to B **do**
 - 2: Sample with replacement from \mathcal{D} to get \mathcal{D}_b of size $|\mathcal{D}_b| = |\mathcal{D}|$.
 - 3: Estimate $\hat{f}(X_1, T = 1)$ and $\hat{f}(X_1, T = 0)$ in equation (32) from \mathcal{D}_b .
 - 4: Compute $\partial \hat{\tau}(x) / \partial X_1$ using equation (33).
 - 5: Evaluate $\partial \hat{\tau}(x) / \partial X_1$ at the 25th percentile, median, mean, and 75th percentile of X_1 in \mathcal{D} .
 - 6: **end for**
 - 7: Compute the percentage of bootstrap iterations out of $B = 1000$ total bootstrap iterations in which $\partial \hat{\tau}(x) / \partial X_1 < 0$, evaluated at the 25th percentile, median, mean, and 75th percentile of X_1 in \mathcal{D} .
-

We find that the derivative is negative for 93.3%, 92.5%, 90.5%, and 81.3% of the iterations at the 25th percentile, median, mean, and 75th percentile of X_1 , respectively. We conclude that the

²⁶The additional splits of the training data done during 10-fold cross-validation are also affected by the seed.

effect of the announced reminder on take-up of the profitable opportunity is higher for managers who have been using the FinTech payments technology for less time and thus likely have less trust in the FinTech company.

Figure D.1: Percent of OOS RMSEs lower than not winsorized or standardized (1, 1) OOS RMSE



This figure shows the percentage of out-of-sample root mean squared errors (OOS RMSEs) for the given models that are lower than those of the benchmark non-winsorized and non-standardized (1,1) model. Using 300 distinct randomization seeds, which determine 300 different training and test set splits, we compute the corresponding 300 OOS RMSEs for each model. We then compare the 300 OOS RMSEs of a given model with the 300 OOS RMSEs from the benchmark model and calculate the percent of times, out of 300, that the OOS RMSE of the given model is smaller than that of the benchmark model. For example, the 57.0% for the winsorized and not standardized (4, 1) model indicates that this model has a lower OOS RMSE than the benchmark model for 171 distinct randomization seeds out of the 300 total seeds.

Appendix References

- Farrell, Max H., Tengyuan Liang, and Sanjog Misra (2021). “Deep Neural Networks for Estimation and Inference.” *Econometrica* 89(1), 181–213.
- Friedman, Jerome, Trevor Hastie, and Robert Tibshirani (2010). “Regularization Paths for Generalized Linear Models via Coordinate Descent.” *Journal of Statistical Software* 33(1).
- Imbens, Guido and Donald B. Rubin (2015). *Causal Inference for Statistics, Social, and Biomedical Sciences: An Introduction*. New York, NY: Cambridge University Press.