Why Small Firms Fail to Adopt Profitable Opportunities*

Paul Gertler   Sean Higgins   Ulrike Malmendier   Waldo Ojeda

May 26, 2023

Abstract

Why do small firms often fail to adopt new profitable opportunities, even in the absence of informational frictions, fixed costs, or misaligned incentives? We explore three potential mechanisms: present bias, memory, and trust in other firms. In partnership with a financial technology (FinTech) company in Mexico, we randomly offer 34,000 firms that are already users of the payment technology the opportunity to be charged a lower merchant fee for each payment they receive from customers. The median value of the fee reduction is 3% of profits. We randomly vary the size of the fee reduction, whether the firms face a deadline to accept the offer, whether they receive a reminder, and whether we tell them in advance that they will receive a reminder. While deadlines do not affect take-up, reminders increase take-up of the lower fee by 18%, and anticipated reminders by an additional 7%. The results point to limited memory in firms, but not present bias. Additional survey data suggests trust as the mechanism behind the significant additional effect of the anticipated reminder. Upon receiving an anticipated reminder from the FinTech company, firms value the offer more and accept it even if they generally distrust advertised offers.

---

*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: Baruch College, CUNY, Zicklin School of Business; waldo.ojeda@baruch.cuny.edu. We thank Noah Forougi, Miguel Angel Jimenez, César Landín, Alexandra Wall, and Tiange Ye for excellent research assistance. We are very grateful to 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, ASSA, 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, European Finance Association 2022 Meeting, European Winter Finance Summit, Golub Capital Social Impact Lab Winter Seminar, GSU-RFS FinTech Conference, IPA SME Program, IPA-GPRL Researcher Gathering, IPCDE, Jackson Hole Finance Conference, Korea Advanced Institute of Science and Technology and Korea University, Lab for Inclusive FinTech at UC Berkeley, Midwest International Economic Development Conference, NBER Corporate Finance Program Fall Meeting, NBER Corporate Finance Program Spring Meeting, NCDE, NFA Meeting, Northwestern University, Peking University Fintech Workshop, Red Rock Finance Conference, SFS Cavalcade, Toulouse School of Economics, Tulane University, 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 Washington, Webinar on Entrepreneurial Finance and Innovation, and Webinar on Finance and Development for helpful comments and discussions. We are grateful for funding from the CEGA-Visa Financial Inclusion Lab and the Lab for Inclusive FinTech (LIFT) at UC Berkeley. 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. This occurs across many industries —including manufacturing, banking, retail, and healthcare—and across various types of opportunities—including cost-saving technologies, financal technologies, management practices, and optimal pricing (Bloom et al., 2013; Atkin et al., 2017; Bruhn, Karlan, and Schoar, 2018; Celhay, Gertler, Giovagnoli, and Vermeersch, 2019; DellaVigna and Gentzkow, 2019; Giorcelli, 2019; Mishra, Prabhala, and Rajan, 2021; Higgins, 2022). Firms across the firm size distribution forgo substantial profits by failing to adopt these profitable opportunities. Small and medium enterprises in Bruhn, Karlan, and Schoar (2018) forgo a 28% increase in productivity on average, medium-sized firms in Bloom et al. (2013) forgo a 17% increase in productivity, and large retail chains in DellaVigna and Gentzkow (2019) forgo an increase in annual profits of about $16 million per chain, or 2% of revenue.

Several factors contribute to firms' failure to adopt profitable opportunities, including lack of information (Bloom et al., 2013; Giorcelli, 2019), fixed costs in the presence of liquidity constraints (Bruhn, Karlan, and Schoar, 2018), and misaligned incentives within firms (Atkin et al., 2017). However, even when these standard economic frictions are removed, firms are frequently still slow to adopt profitable opportunities. For example, Bloom et al. (2013) finds that "even if the owners became convinced of the need to adopt a [profitable] practice, they would often take several months to do so." Furthermore, DellaVigna and Gentzkow (2019) and Mishra, Prabhala, and Rajan (2021) find that "managerial inertia" or "stickiness in organizational structures and practices" prevent adoption of profitable opportunities.

Why do firms exhibit such inertia and fail to take advantage of new opportunities even though these behaviors reduce their profits? We analyze three sets of potential explanations: present bias, limited memory, and distrust. The first two, present bias and limited memory, have been shown to explain inertia and lack of behavioral change in *non-managerial* individual-level decision-making, including health-related choices such as healthcare appointments, vaccine take-up, or gym attendance (Gurol-Urganci et al., 2013; Dai et al., 2021; Calzolari and Nardatto, 2016; DellaVigna and Malmendier, 2006) and financial choices, such as saving, borrowing, and repayment behavior (Laibson, 1997; Karlan, McConnell, Mullainathan, and Zinman, 2016; DellaVigna and Malmendier, 2004; Karlan, Morten, and Zinman, 2016). Here, we ask whether these determinants also explain individuals' profit-reducing *managerial* behavior within small firms.

In addition, we consider the role of trust. The analysis of this third determinant is motivated by an additional finding in the literature on present bias and memory, namely, that theoretically motivated remedies, such as deadlines or reminders, can be less effective than predicted and that the effect of offering commitment devices is limited (Brune, Giné, Goldberg, and Yang, 2016; Bisin and Hyndman, 2020; Burger, Charness, and Lynham, 2011; Campos-Mercade et al., 2021).

We explore whether trust, or the lack thereof, might further contribute to the subdued response to seemingly promising opportunities. In the domain of *non-managerial* individual-level decisions, distrust has been shown to interfere with the very same decision-making situations, such as saving, borrowing, and refinancing (Karlan, Mobius, Rosenblat, and Szeidl, 2009; Johnson, Meier, and Toubia, 2019; Bachas, Gertler, Higgins, and Seira, 2021), and we introduce the analysis into the realm of *managerial* decision-making within small firms.[1] Following Anderson and Narus (1990, p. 45), we define interfirm trust as "the firm's belief that another company will perform actions that will result in positive outcomes for the firm, as well as not take unexpected actions that would result in negative outcomes for the firm." Lack of trust thus reduces the perceived expected value of the business opportunity offered to the firm.

In partnership with a financial technology (FinTech) payments provider in Mexico, we conduct a randomized controlled trial (RCT) where we offer 33,978 firms that are already active users of the payments technology the opportunity to be charged a lower merchant fee for each payment they receive from customers. The firms in our experiment are relatively small: the median number of employees is 3 and the largest firm in our experiment has 150 employees. Nevertheless, the distribution of number of employees in our experiment is similar to that of 99.7% of firms in Mexico. By adopting a lower merchant fee, these firms reduce their costs and hence increase their profits. For the median firm in our study, the expected cost savings from the reduced fee equal 2.7% of profits.

To examine the effect of these three barriers, our RCT randomly varies (i) whether we offer a lower fee to firms that are already users of the FinTech payments technology, (ii) the amount of the lower fee (and hence the value of the offer), (iii) whether they face a deadline to accept the offer, (iv) whether they receive a reminder, and (v) whether we tell them in advance that they will receive a reminder (which we refer to as an "anticipated reminder"). The design allows us to test for the three proposed mechanisms (present bias, limited memory, and lack of trust in other firms) as well as potentially distorted beliefs about present bias and memory. To show this we augment the model from Ericson (2017), which studies how present bias and limited memory affect task completion, to include the notion of trust.

Theoretically, firm owners' 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.[2] Deadlines can help overcome present bias because at the deadline period the firm owner cannot delay adopting

[1]Limited evidence exists on the role of trust on *interfirm* relationships. McMillan and Woodruff (1999) find that firms in Vietnam develop trust over time and that supplier firms 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. Cai and Szeidl (2018) find that a lack of trust is a barrier to creating business partnerships, and randomizing regular meetings between firms increases trust.

[2]We refer to firm *owners* throughout the paper for simplicity, since the owner of the firm was the recipient of our messages in 88% of cases.

the profitable opportunity anymore as it will expire. However, firm owners can also have limited memory and forget about the profitable opportunity to adopt a lower merchant fee. Reminders can help overcome limited memory as they remind firm owners who have forgotten about the offer. Anticipated reminders—when firm owners are told in advance that they will receive a reminder—can increase their expectations about remembering the offer. Thus, anticipated reminders can decrease initial take-up (before the reminder is sent), as firms know they will remember the chance to do so when the reminder arrives. On the other hand, if firms are fully overconfident about memory and think they will remember even without a reminder, the anticipated reminder would not decrease initial take-up. After the reminder is sent, theory suggests that take-up by firms that received an anticipated reminder should be no higher than take-up by firms that received an unanticipated reminder *unless* the anticipated reminder increases the perceived value of the offer (e.g., by increasing trust in the offer).

This RCT allows us to test these theoretical predictions.

We find that firms are forgetful: reminders cause a large and significant increase in adoption of the lower merchant fee. By the eighth day of our study, reminders increase adoption of the lower merchant fee by about 18%.

Firms that received a reminder are 4.7 percentage points (pp) more likely to take up the offer compared to firms that did not receive a reminder, on a base of 25.5% take-up. The higher overall take-up of the offer by firms that received a reminder is almost entirely driven by the increase in take-up on the day we sent the reminder.

We do not find evidence of present bias explaining non-adoption, as the deadline has no effect on take-up. While the point estimate of the effect of deadlines on take-up as of the date of the deadline is positive (but not statistically significant), take-up in the no-deadline group catches up to that of the deadline group within a few days after the deadline.

Firms that received an anticipated reminder had the highest overall take-up. When we sent the initial offers, the only difference between firms that would receive an anticipated reminder or an unanticipated reminder is that in the anticipated reminder group, the initial emailed informed them that they would receive a reminder and on what date they would receive it. The reminder message is the same for both groups. On the first day (when we sent the initial email), there is no difference in take-up between the anticipated- and unanticipated-reminder groups. In our theoretical framework, this result—combined with the findings that reminders do have a large effect and that not all firms find it optimal to adopt immediately—suggests that firms are not only forgetful (as shown with the deadline treatment) but also overconfident about memory. The day that we sent the reminder, anticipated reminders increased take-up of the profitable opportunity by 2 pp compared to unanticipated reminders, and the difference remains significant throughout the remainder of the experiment. This result cannot be explained by a model where anticipated reminders only impact the probability (or perceived probability) of remembering. Instead, to cause higher take-up, the anticipated reminder must increase the perceived value of accepting the offer, for example by increasing trust in the offer.

We conduct a survey of a subsample of firms in our RCT to better understand mechanisms behind the effect of the anticipated reminder relative to the unanticipated reminder on take-up. We find that, compared to firms that received an unanticipated reminder, firms that received an anticipated reminder are 16.1 pp more likely to state that the reminder changed their perception of the offer's value (39.2% relative to a base of 23.1% in the unanticipated reminder group). We also find evidence that the treatment effect of the anticipated reminder is concentrated among low firms that trust advertised offers less in general. These results suggest that the anticipated reminder increased the value of the offer by increasing the level of trust firms had in the offer. We show that alternative explanations such as different behavior induced by the anticipated reminder (e. g., checking the offer's profitability in preparation for the reminder) do not explain why the anticipated reminder group has 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 informational, cost, and incentive frictions analyzed in prior literature. While there is substantial evidence about whether these barriers prevent individuals from taking various *non-managerial* actions, there is little evidence on how these barriers affect *managerial* decisions and potentially prevent firms from maximizing profits.

Related Literature. Individuals' limited memory has been documented in a number of domains, and reminders can increase individuals' saving (Karlan, McConnell, Mullainathan, and Zinman, 2016), loan repayment (Karlan, Morten, and Zinman, 2016), gym attendance (Calzolari and Nardatto, 2016), healthcare appointment attendance (Gurol-Urganci et al., 2013), and vaccine take-up (Dai et al., 2021). 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 (e.g., Camerer and Lovallo, 1999; Malmendier and Tate, 2005), but the evidence on overconfidence about memory is limited even for non-managerial settings, with evidence from a laboratory experiment in Ericson (2011). Overconfidence about memory can exacerbate the negative effect of limited memory on completing a task (Ericson, 2017).

Individuals' present bias and the economic costs of this bias have also 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, the 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).

Lack of trust can also have significant effects on decision-making. Distrust 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), 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).

There is also substantial evidence on *other barriers* that firms face; our contribution is to test whether—in addition to these other barriers documented by other studies—limited memory, present bias, and a lack of trust, as well as beliefs about memory and present bias prevent firms from adopting profitable opportunities. A lack of information about profit-increasing management practices can prevent firms from implementing these practices (Bloom et al., 2013; Giorcelli, 2019). Even when firms have information about the existence of a profitable opportunity, these opportunities often involve fixed adoption or adjustment costs, which can prevent credit-constrained firms from adopting (Bruhn, Karlan, and Schoar, 2018; Celhay, Gertler, Giovagnoli, and Vermeersch, 2019). Firms may also be uncertain about the benefits of an opportunity and be risk or ambiguity averse (Bruhn, Karlan, and Schoar, 2018), or they may underestimate the benefits of adopting (Higgins, 2022). Incentives within firms can also be misaligned, such that new contracts need to be written for employees to act in a way that leads to an increase in profits after adopting a new technology (Atkin et al., 2017).[3]

Managerial inertia can also prevent firms from adopting practices or technologies that would increase their profits. DellaVigna and Gentzkow (2019) 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." Among potential behavioral factors, Kremer, Lee, Robinson, and Rostapshova (2013) argue that loss aversion prevents small retail firms from stocking sufficient inventory, and Beaman, Magruder, and Robinson (2014) find that limited attention prevents small firms from keeping sufficient small change; both papers document that these failures reduce firm profits.

The rest of the paper is organized as follows. Section 2 presents the theoretical framework

[3] See Verhoogen (2021) 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.

motivating the study. Section 3 describes the experimental setting. Section 4 discusses the design of the experiment including econometric specifications. Section 5 shows the impact of the unanticipated and anticipated reminders, and deadlines on take-up of a lower merchant fee offer. Section 6 provides evidence that anticipated reminders increased trust in our setting. Section 7 shows that the lower merchant fee increased usage of the electronic payment technology, and that the increase in electronic sales was large enough that lowering the merchant fee was profitable for the FinTech partner as well. Section 8 concludes.

2 Model

We use an augmented version of the model in Ericson (2017) to fix ideas about present bias, limited memory, and a lack of trust. The model also allows for naïveté (overconfidence) about them. The model allows us to derive predictions about the effects and interactions of these potential barriers to the adoption of profitable opportunities. We also structurally estimate the model [coming soon] to quantify the relative importance of these barriers.

Model assumptions. In the model, an agent makes a decision to perform a task that is beneficial in the future but has an immediate cost. The agent has potentially present-biased preferences and possibly naïveté: $U = u_0 + \beta\left(\sum_{t=1}^{\infty} \delta^t u_t\right)$, where δ is the discount factor, β is the present-bias parameter. The agent has beliefs $\hat{\beta} \in [\beta, 1]$, and is naïve if $\hat{\beta} > \beta$.

The model also incorporates imperfect memory. There is a probability of remembering the task in period t conditional on remembering it in period $t-1$, measured by the parameter ρ_t (with $\rho_0 = 1$). Agents are only be able to perform the task if they remember it. Agents have beliefs $\hat{\rho}_t \in [\rho, 1]$, and are overconfident about their memory if $\hat{\rho}_t > \rho_t$.

In each period t, the agent draws a cost c_t from a known cost distribution $F(c)$, and receives benefit y in the next period $(t+1)$ if they complete the task. We consider behavior over T periods, from $t = 1$ to $t = T$, where T is potentially infinite.

Expanding on Ericson (2017), we incorporate a trust parameter α_t. We incorporate it to the model by denoting the expected benefit from completing the task as $\alpha_t y$. 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 vary over time.

Thus, the agent decides to act based on the current value function:

$$V_t = \begin{cases} \beta\delta(\alpha_t y) - c_t, & \text{if act,} \\ \hat{\rho}_{t+1}\beta\delta E_t[\hat{V}_{t+1}], & \text{if do not act,} \end{cases}$$

where $E_t[\hat{V}_{t+1}]$ is the perceived continuation value of not completing the task in the current period (and potentially completing the task in a future period). At the deadline, the continuation value is zero as the opportunity to perform the task in future periods is removed. Note that the current value function V_t is a function of the (potential) present bias β, while the perceived continuation value $E_t[\hat{V}_{t+1}]$ is a function of the (potential) naïveté and thus $\hat{\beta}$. The indicator variable α_t indicates whether the agent trusts the offer and, if so, perceives the expected benefit from accepting the offer to be higher by an additional amount α_t.

Equilibrium behavior. By backwards induction from the deadline, the model leads to a cutoff strategy where the agent adopts in period t if the cost draw c_t is below a threshold c_t^*. This is conditional on the task being active: the agent has not already done the task before period t and has not forgotten about the task by period t. Specifically, by backwards induction we obtain a recursive set of expressions that implicitly define the cost threshold:

$$c_t^* = \beta\delta\left(\alpha_t y - \hat{\rho}_{t+1}E_t\left[\hat{V}_{t+1}\right]\right) \tag{1}$$

$$E_{t-1}\left[V_t\right] = F\left(\hat{c}_t^*\right)\left[\delta\alpha_t y - \int_0^{\hat{c}_t^*} c\, dF(c)\right] + \left(1 - F\left(\hat{c}_t^*\right)\right)\delta\hat{\rho}_{t+1}E_t\left[\hat{V}_{t+1}\right],$$

where $\int_0^{\hat{c}_t^*} c\, dF(c)$ is the expected cost draw conditional on acting. The definition of $\hat{c}_t$ is identical to that of c_t but replacing β with $\hat{\beta}$.

Then the probability of adopting at period t is the probability the task is active (which is the product of the probability the task is remembered and the probability the task has not already been adopted) times the probability the cost draw c_t is below the threshold c_t^*:

$$\Pr(\text{adopt at } t) = \underbrace{\prod_{j=1}^{t}\rho_j}_{\Pr(\text{remember})}\;\underbrace{\prod_{k=0}^{t-1}\left(1 - F\left(c_k^*\right)\right)}_{\Pr(\text{not adopted before } t)}\;F(c_t^*) \tag{2}$$

Thus, integrating over individual firms (whose i subscript is omitted above for ease of notation)—which can have heterogeneous costs—provides a set of moments, namely, the fraction of individual firms that adopt in period t, with one moment for each of the T periods for each treatment arm (where T is the period in which the deadline occurs). Our experiment thus allows us to estimate a set of moment equations of the form (2) to estimate β, $\hat{\beta}$, ρ, $\hat{\rho}$, costs, and α, where each treatment arm provides T moments.

Model predictions. The model generates several testable predictions, which we will take to the data.

Prediction 1 (Benefit). A higher expected value of the offer (higher y and/or higher α_t) increases take-up of the offer.

Prediction 2 (Reminders). Reminders increase take-up of the offer if firms are forgetful ($\rho_t < 1$).

Prediction 3 (Deadlines). (a) Deadlines increase take-up of the offer if firms are present-biased ($\beta < 1$). (b) The increase in take-up occurs immediately after receiving the initial message (at $t = 1$) rather than at the time of the deadline if firms are (partially) aware of their limited memory ($\rho \leq \hat{\rho}_t < 1$).

Note that the 'immediate effect' in part (b) occurs because some firms prefer to wait to take up the offer (either due to present bias or rationally waiting for a better cost draw); however, awareness of limited memory pushes some of these firms to adopt on the first day due to the worry about forgetting otherwise if they do not know they will receive a reminder.

Finally, consider the effect of the anticipated versus unanticipated reminder. Let's continue to consider the scenario that firms are forgetful and it is optimal for some firms to adopt not on the first day—which, again, does not necessarily require firms to be present-biased, as they could also be rationally waiting for a better cost draw, such as a day when the manager is less busy.

Prediction 4 (Anticipated Reminders and Pre-Reminder Take-Up). The anticipated reminder (a) reduces take-up on $t = 1$, compared to the unanticipated reminder, if firms are forgetful and have accurate beliefs about memory ($\hat{\rho}_t = \rho_t < 1$), and (b) has no differential effect on take-up on $t = 1$ if firms are fully overconfident about memory ($\rho_t < \hat{\rho}_t = 1$).

The reason for the predicted first-day effects is that the anticipated reminder increases the firm's belief about their future memory, i. e., their ability to remember signing up for the offer. When firms know that they will receive a reminder, they do not have to worry about forgetting—so the anticipated reminder leads to lower take-up on day 1 if firms have limited memory and are not fully overconfident about memory. If, instead firms are already fully confident that they will remember, then the anticipated reminder will not have an effect as it will not impact the belief about memory.

Prediction 5. (Anticipated Reminders and Post-Reminder Take-Up) Anticipated reminders (a) do not affect post-reminder take-up, compared to the unanticipated reminder arm if firms inherently trust the offer ($\alpha_t = 1$ regardless of treatment arm); and (b) increase post-reminder take-up if some firms distrust the offer, and their trust in the offer increases after receiving the reminder as announced in advance.

3 Experimental Setting

We partnered with a FinTech company in Mexico to study the effects of present bias, limited memory, and a lack of trust in other firms, plus beliefs about them on the probability of accepting a profitable opportunity. The FinTech company provides its clients with point-of-sale (POS) hardware and an app to accept debit and credit card payments, similar to Square in the US. The POS terminal is available for purchase in retail stores and online platforms. The user can start accepting electronic payments after registering their user information and linking to their bank account. For each electronic payment their clients process, the FinTech company charges a merchant fee that is a percentage of the payment. The merchant fee rate does not vary depending on the card network used. Relative to POS terminals offered by banks, the FinTech partner's POS terminal is less expensive and does not include a monthly fee, but the FinTech partner charges a higher percent transaction fee for each card payment than banks do on the POS terminals they issue.

In focus groups conducted with our FinTech partner's users prior to this study, many users stated that with our partner technology they were able to accept electronic payments for the first time. While banks charge lower merchant fees, users say accepting electronic payments with our FinTech partner is easier as there is less documentation needed to register, there is no need to have a bank account with same the bank providing electronic payments, 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 that wanted to pay with debit and credit cards. Some noted that it is convenient for them to have increased portability to process transactions anywhere without carrying cash as the FinTech's POS terminal is smaller and can be connected to any mobile device. They also noted that 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 increasing customer retention (i.e., losing fewer customers to competitor FinTech companies or banks). Second, they did not know what their customers' elasticity of card revenues was 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 modifications to the messages they sent would increase customer 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 firms' elasticity of card revenues with respect to the fee may be positively correlated with their probability of accepting the lower merchant fee.

4 Experimental Design

Our study sample consists of 33,978 firms that are *already* active users of the FinTech payments provider. To maximize the absolute value of the offer, we selected the sample to include only the top quartile of the FinTech company's users based on average monthly sales in the previous six months. Prior to the experiment, firms in our sample paid 3.75% or 3.50% in merchant fees per payment they received from customers. We offer firms the opportunity to lower their merchant fee. The core of our RCT consists of a $2 \times 3 \times 2 +$ control group design, where we interact whether we send the offer with a deadline or without a deadline; with an unanticipated reminder, anticipated reminder, or no reminder; and whether we offer to reduce the fee to 3.00% or 2.75%. Our control group consists of firms were eligible to receive an offer based on our selection criteria but were not sent an offer. For the deadline groups, we set the deadline at the end of the eighth day, and firms were told the date of the deadline in the initial message they received. We sent reminders at the beginning of the seventh day. Firms that received an anticipated reminder were told in the initial message that they would receive a reminder, and they were told on what day they would receive the reminder. We have an additional two treatment groups that receive the offer with a one-day deadline, no reminder and either a fee reduction to 3.00% or 2.75%.

4.1 Sample

To define our sampling frame for the experiment, we restricted the set of all users of the FinTech company's payment technology to the top quartile of users based on August 2020 sales. The decision to restrict to the top quartile was to ensure that the offer was sufficiently valuable; the precise use of the top quartile was based on a randomized pilot we conducted with 11,755 firms in May 2019 where we offered a smaller fee reduction from 3.75% to 3.50%. In that pilot, we found that the take-up rate of the lower fee was increasing in baseline sales and that the elasticity of card payment revenues with respect to the fee was statistically significant only for the fourth quartile of baseline sales starting. We also filtered to users what were in good standing administratively with the FinTech partner at the time of the study implementation. The sampling frame was made up of 33,978 users.

We stratified our randomization by business type across six categories: beauty, clothing, professionals, restaurants, small retailers, and other. Table 1 shows summary statistics of the firms in our sample. It also shows that the randomization is balanced across treatments, using a regression of the respective sample characteristics on treatment indicators. Firms in our sample are 44.1% female-owned, and the most common business types are *Small retailers* (at 26%) and *Professionals* (at 23.9%). *Small retailers* include corner stores and prepared foods, while the *Professionals* business type includes medical services, dentists and veterinarians. For the rest of the business types, the *Beauty* category includes beauty salons and spas, *Clothing* includes clothing and accessory stores, *Restaurants* includes restaurants, cafes and bars, and the *Other* category includes auto shops and sale of construction materials.

In addition to the descriptive characteristics provided to us by the FinTech partner, we also elicited further information in the survey we ran on a subsample of the firms (cf. Section 6). One variable worth highlighting here is firm size. The vast majority of firms in our sample (87%) have 1 to 5 employees. Figure 1 shows the distribution of the number of employees by firm. The median firm has 3 employees, and the largest firm in our sample has 150 employees. The firm size distribution in our survey sample looks similar to the firm size distribution of all firms in Mexico, where 90% of all firms have 1–5 employees. We also cover the majority of the distribution with the firms in our survey sample: 99.7% have equal or less than 150 employees.

4.2 Intervention

We randomly offered a cost-saving measure to firms who were already users of our FinTech partner's technology to process electronic payments by debit and credit card. Through the FinTech company that processes their payments, we offered to lower the merchant fee they were charged for each sale they made through the technology. This fee reduction intervention was offered through an email and SMS text message campaign. The offer had a link to a short online form that firms could complete to obtain the fee reduction. The form required firms to fill 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 the form would only take one minute to complete. We asked in the survey how long firms expected and actually took to complete the offer. Ex-ante, most firms estimated that it would take between 6 and 10 minutes to complete the offer; ex-post, most firms report taking between 1 and 5 minutes (Appendix Figure **??**). This expectation likely takes into account the time firms thought it would take to read the fine print, calculate whether the offer is valuable, and fill out the form. Figure 2 shows examples of the email that firms received.

Among the 33,978 firms in the study sample, 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.

The remaining firms were assigned to one of the fourteen other groups. First, the firms were randomly assigned to one of seven groups combining deadlines and reminders: (i) no deadline, no reminder; (ii) no deadline, anticipated reminder; (iii) no deadline, unanticipated reminder; (iv) one-week deadline, no reminder; (v) one-week deadline, anticipated reminder; (vi) one-week deadline, unanticipated reminder; (vii) one-day deadline, no reminder. The sample size in each of these seven groups was determined based on power calculations using the results from our May 2019 randomized pilot.

We use the deadline treatment to test whether firms are present-biased. However, the deadline could have effects for other reasons: for example, by creating a sense of scarcity. We asked firms in a survey question why they thought the offer had a deadline, and only 11.5% of firms replied "Because opportunities were not available for all users." (Appendix Figure B.3, left panel). For the vast majority of firms, deadlines are a usual business practice or common marketing tool.

Similarly, we asked firms why they thought they had been sent a reminder (Appendix Figure B.3, right panel). 82.2% of firms replied with at least one of the top three reasons, indicating they believed the reminder was a usual business practice so that businesses wouldn't forget to adopt a valuable offer. 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.

Within each treatment group, we also experimentally varied the value of the offer by offering two levels of lower merchant fees. Merchants were currently charged either a 3.75% or 3.50% fee for each transaction, measured as a percent of the sale amount. We randomized the offer to be either 3.00% or 2.75%. (Thus, the fee reduction ranges from 50 basis points—for those reduced from 3.50% to 3.00%—to 100 basis points—for those reduced from 3.75% to 2.75%. Part of this reduction is randomized based on their new fee offer, and part is endogenous based on whether they currently had a 3.75% or 3.50% fee before the experiment.) The lower fee lasted for six months, after which point the firm's rate returned to their current (pre-intervention) rate. All of this information was included in the e-mails they received. The reason that the fee reduction was temporary was that our FinTech partner 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.

4.3 Mapping to Model

Mapping the model assumptions to our experimental setting, the benefit y is the cost savings from a lower merchant fee and the costly task is clicking the link in the email and filling out a short form. The time period t is a day. The cost draw c_t can be thought of as a measure of how busy firms are on that day. We have survey evidence that supports this as a factor in whether firms adopt. In two distinct survey questions, we ask businesses why they adopted the offer on the first day or why they did not adopt on the first day (Appendix Figure B.1). 75.5% of firms that adopted on the first day reported doing so because they had time that day, and 72.6% of firms that delayed adopting cited time constraints as the reason for their delay.

For firms assigned to a treatment arm with an anticipated or unanticipated reminder, the reminder is sent on the morning of day 7 (which is one day before the deadline for the treatment arm that also includes a deadline). Reminders about the task raise the probability of remembering ρ_t in the period they are sent. However, only an anticipated reminder that tells agents about a reminder they will receive in a future period τ increases the agent's current expectation of remembering in that future period τ, $\hat{\rho}_\tau$.

We set our deadline to be in one week (midnight on day 8 of the experiment, $T = 8$) for all treatment arms that have a deadline, except one, where we set it to be one day ($T = 1$) in order to isolate variation in costs from the probability of forgetting when structurally estimating the model. For treatment arms without a deadline, $T = \infty$. For all treatment arms except the one-day deadline arm, we compare take-up rates for eight periods; these take-up rates are also used as moments to estimate forgetfulness and present-bias parameters.

The original Ericson (2017) model does not include distrust (which is nested in our augmented version of the model by setting $\alpha_t = 1$ for all firms). Any of the treatments could potentially increase trust, α_t. For example, α_t may increase when the firm owner is told they will receive a reminder in the future, and it may also increase once that reminder is sent if the firm owner was told that they would receive a reminder in the future. Consider the case where a firm owner does *not* trust the offer inherently, but does trust the offer if they are told they will receive a reminder and then receive this reminder. In this case, the difference between the cost thresholds is:

$$c^*_{t,\text{anticipated}} - c^*_{t,\text{unanticipated}} = \beta\delta y.$$

This means that the anticipated-reminder group has a higher cost threshold in any period for the agent to decide to act. This leads to higher take-up of the offer compared to the groups that did not receive an anticipated reminder. The t subscript on α_t allows trust to increase either upon receiving the initial message ($\alpha_t > 0$ for all t) or only upon receiving the anticipated reminder ($\alpha_t = 0$ for $t < t_{\text{reminder}}$ and $\alpha_t > 0$ for $t \geq t_{\text{reminder}}$, where t_{reminder} is the period in which the reminder arrives.

4.4 Timeline

Figure 3 shows the experiment timeline for the different types of treatments. 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 group with a one-day deadline had all of September 29 (until midnight) to take up the offer. The group with a one-week deadline had until midnight on October 6. The anticipated reminder group were told in the initial offer sent on September 29 that they would receive a reminder on October 5. For both the groups with an anticipated and with an unanticipated reminder, the reminders were sent on October 5 at 10am CST, i. e., one day before the deadline for groups that also had a deadline. Each of the emails was accompanied by two SMS text messages that contained similar information in a condensed format.

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 (as shown in Figure 4) and applied the filtering criteria to August 2020 sales to exclude firms that had closed or greatly reduced their electronic sales due to COVID-19.

4.5 Specifications

With our experimental design we estimate in reduced form the effects of a reminder or deadline on the probability of taking up the lower fee. Our primary results use the following regression:

$$y_i = \lambda_{s(i)} + \sum_{k=2}^{K} \beta_k T_i^k + \varepsilon_i \tag{3}$$

where y_i is the outcome of interest (take-up on day 1, take-up on days 7 and 8, and overall take-up over the course of the experiment), $\lambda_{s(i)}$ are strata fixed effects (which also absorb the constant), T_i is a vector of indicator variables T_i^k denoting assignment to one of the K treatments (with the omitted category $k = 1$ for the control group), and ε_i are heteroskedasticity-robust standard errors (not clustered since the randomization unit is the individual firm).

To more precisely detect effects over time and focus on specific treatment comparisons, we also estimate equations where the outcome is daily take-up during the study timeline or take-up before or after the reminder is sent. To estimate the effects over time we use the following regression:

$$y_{it} = \gamma_i + \delta_t + \sum_{t'=1}^{8} \sum_{k=2}^{K} \beta_{t'k} T_i^k \mathbb{1}(t' = t) + \varepsilon_{it} \tag{4}$$

where y_{it} is the outcome of interest for each day t, γ_i are firm fixed effects (which also absorb the constant), δ_t are time fixed effects, T_i is a vector of indicator variables denoting treatment assignment, and ε_{it} are standard errors clustered at the firm level. When we compare effects between fewer treatment groups, K will be less than the eight total treatment groups in the study. We also estimate effects collapsing the eight time periods into pre and post-reminder periods. In this case, days 1 through 6 are the pre-reminder period and days 7 and 8 are the post-reminder period.

5 Results

Take-up of the profitable opportunity is low: among firms that did not receive the deadline or reminder treatments, take-up was 27.7%. While take-up is higher by larger firms—measured by baseline sales through the technology (as we only observe number of employees in the survey subsample)—even among the top quintile of baseline sales, take-up is only 29.1% (Appendix Figure B.4).

Our baseline Prediction 1 states that a lower merchant fee offer (i. e., larger cost reduction and hence more valuable offer) should generate to a higher take-up rate. Figure 5 shows the take-up rates by merchants that received a 2.75% or 3.00% merchant fee.

Receiving the more profitable 2.75% merchant fee offer increased take-up by 4.2 pp (from 26.4% to 30.6%). This shows that when merchants received a more valuable offer, they increased their take-up. We will distinguish between the objective change in value and the subjective change in expectations about the value of the offer (due to trust) in the test of Prediction 5 below.

Next we consider the overall effect of reminders. Prediction 2 states that reminders will increase take-up if firms are forgetful. Figure 6 shows the take-up rate of the lower merchant fee offer by October 6. For the groups that had a deadline, October 6 was the last day on which they could activate the lower merchant fee. The top three bars in different orange tones show the take-up rates for groups that had a deadline. The bottom three bars in different blue tones show the take-up rates for groups that did not have a deadline. Across the deadline and no deadline groups, the top two (darker-shaded) bars show that the reminders increased lower merchant fee take-up. The pattern is similar in both groups: receiving a reminder, whether anticipated or unanticipated, increases take-up by about 5 pp compared to the groups that received no reminder, regardless of whether the offer also had a deadline.

To more precisely show take-up rates by treatment group, Table 2 provides the regression coefficients from estimating model specification (3). The omitted group is the control group, which had 0% take-up. When there was no deadline and no reminder, 25.4% of firms accepted the offer. When there was no deadline, an anticipated reminder increased take-up to 30.5% and an unanticipated reminder increased take-up to 29%. When there was a deadline and no reminder, 25.6% of firms accepted the offer. When there was a deadline, an anticipated reminder increased take-up to 31.8% and an unanticipated reminder increased take-up to 31.8%. In other words, we recover the substantial reminder effect of about 5 pp.

Zooming in to the timing of the reminder effect, Figure 7 shows raw take-up rates (upper panel) and regression coefficients and 95% confidence intervals (lower panel), comparing treatment groups that received a reminder and those that did not. On day 1, take-up rates start close to 20% both sets of treatment groups. On all days before the reminder was sent, there is no statistically significant difference between the group that would eventually receive a reminder and those that would not. Both their take-up rates increase steadily over the next five days until day 6. When the reminder is sent on day 7, the take-up rate for the group that received a reminder increases by 4.1 pp compared to the group that did not receive a reminder. This effect is robust to controlling for time and firm fixed effects. On day 8, the difference in take-up is 4.7 pp. The effect of the reminder remains across both lower merchant fee offers (Appendix Figure B.5).

We also find that the effect of the reminder persists after the deadline (Appendix Figure B.6). Furthermore, the effect of the reminder is not driven by people not seeing the initial message but seeing the reminder: we observe whether people opened the initial email, and Appendix Figure B.7 shows that the effect holds conditional on opening the initial email before receiving the reminder.

Overall, we estimate a significant and large reminder effect, which occurs on the day the reminder is sent and persists afterwards. This finding implies that imperfect memory plays a significant role in explaining firms' failure to accept the profitable offer of a lower merchant fee.

Turning to Prediction 3, we estimate the effect of imposing a deadline. Figure 8 shows that on day 1, take-up in the deadline groups is *lower* than that in the no-deadline groups; and by day 8, there is no statistically significant difference in take-up. In addition, although the point estimate on cumulative take-up by day 8 is positive (but not statistically significant), Appendix Figure B.8 shows that take-up in the no-deadline groups catches up within a few days after the deadline, and that over a six-month time horizon after the deadline, there is about 2 pp higher take-up in the no deadline group.

In summary, we find that the deadline did not cause an increase in take-up of the offer. Based on Prediction 3, the lack of an effect of the deadline suggests that present bias does not explain firms' failure to accept the profitable lower-fee offer.

Returning to the reminder effect, we next analyze the differential effects of the anticipated and the unanticipated reminder. We start from the differential effect on day 1 of the experiment, which is the focus of Prediction 4. Figure 9 provides again both the overall take-up rates and the coefficient estimates of the difference, separately for each day. We observe that the take-up among firms in the groups with the anticipated reminder is consistently higher than the take-up among firms who received an unanticipated reminder. However, the difference on day 1 is not statistically significant. Based on Prediction 4, the lack of a significantly *negative* difference in take-up on day 1 suggests that firms are overconfident about memory.

Next, we focus on the difference in take-up after firms received the reminder. The lower panel reveals that the positive difference in take-up rates between the anticipated and unanticipated reminder groups becomes significant after the reminder has been sent, on days 7 and 8. We formally test whether the effect of the anticipated reminder comes before or at the time of the reminder in Appendix Table A.3. We cannot reject that the effect of the anticipated reminder prior to the reminder is 0. We find an increase in take-up due to the anticipated reminder after firms receive the reminder when compared to the unanticipated reminder. We also find that the higher take-up in the anticipated reminder group persists after the deadline (Appendix Figure B.9).

The higher take-up beginning on day 7 in the anticipated reminder group suggests that, as outlined in Prediction 5, (some) firms might not have fully trusted the offer initially, and that receiving a reminder that they had been told in advance they would receive increased their trust in the provider and, as a result, the perceived value in the offer. We will explore the proposed interpretation with additional survey evidence in Section 6.

Overall, we have found evidence of limited memory as well as overconfidence about memory, and a possibly role for trust in explaining firms' take-up behavior. We do not detect an influence of present bias in firm decision-making.

6 Mechanisms Behind Anticipated Reminder Effect

6.1 Anticipated Reminders Increase Trust in the Offer

Working with our FinTech partner, we conducted a survey with a subsample of firms in our study. Our FinTech partner surveyed 471 firms by phone. The survey included questions on firm characteristics and usage of the FinTech payments technology that our partner's administrative data does not contain. We asked questions about respondents' perceptions about the offer from our study to further understand mechanisms behind the effect of the anticipated reminder.

In Appendix Table A.1, we show that our survey sample is comparable to our overall sample we compare means of business characteristics across both groups. In Appendix Table A.2 we show that, within the survey sample, firms are similar across business characteristics when we compare firm that had and did not have a deadline, firms that had no reminder and an unanticipated reminder, and firms that had an unanticipated reminder and an anticipated reminder, respectively.

We are interested in testing whether the announcement and then receipt of a reminder increased firms' perceptions of the offer's value (relative to an unanticipated reminder). For firms that received a reminder, either anticipated or unanticipated, we asked them to respond yes or no to the question "Did receiving the reminder change your perception of the value of the offer?" Figure 10 shows that receiving the anticipated reminder is associated with an 16.1 pp increase the likelihood that the firm responded that the reminder changed their perception of the offer's value (statistically significant at the 5% level). We also asked an open-ended follow-up question, "Why did the reminder change your perception of the offer's value?" Comparing responses in the anticipated and unanticipated reminder groups, there were more responses related to trust in the anticipated reminder group—such as "I had doubts and didn't trust whether it was from [FinTech company]" and "[the reminder] gave it credibility."

In addition, we asked a number of general survey questions to measure firms' overall levels of trust in advertised offers, reciprocity, procrastination, memory, overconfidence about memory, and attention. Using a scale from 1 to 5, where 5 is "Strongly agree," 4 is "Agree," 3 is "Neither agree nor disagree," 2 is "Disagree," and 1 is "Strongly disagree," we asked the following questions:

> [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.*

These questions allow us to test whether those who accepted the offer in the anticipated reminder group differ in characteristics than those who accepted the offer in the unanticipated reminder group. For each characteristic we create a dummy variable $\mathbb{1}(\textit{Survey measure})$, which we set equal to 1 if the respondent agrees or strongly agrees with the question. We characterize it as "High" when it is equal to 1 and as "Low" when it is equal to 0. We estimate the following regression combining the administrative and survey data and restricting to the sample that received either an anticipated or unanticipated reminder:

$$\begin{aligned} Accepted_i = \alpha + \beta_1 \mathbb{1}(Survey\ measure)_i + \beta_2 \mathbb{1}(Anticipated\ reminder)_i \\ + \beta_3 \mathbb{1}(Survey\ measure)_i \times \mathbb{1}(Anticipated\ reminder)_i + \varepsilon_i, \end{aligned} \tag{5}$$

where $Accepted_i$ is an indicator variable equal to one if the firm accepted the offer. The coefficient β_3 gives the heterogeneous treatment effect of the anticipated reminder by survey characteristics. For example, for the first survey question, β_3 reveals whether firms that trust advertised offers more have a differential treatment effect of anticipated reminder.

Figure 11 summarizes the results, comparing firms that received anticipated versus unanticipated reminders. [4] The bottom panel shows the take-up rates for firms by their survey measure high or low status, and if they received an anticipated or unanticipated reminder. The top panel shows the effect of anticipated reminders split by level of the survey measure response. Both treatment effects correspond to combinations of coefficients from Equation 5: the low survey response treatment effect corresponds to β_2, while the high survey response treatment effect corresponds to $\beta_2 + \beta_3$. Above each pair of treatment effects we show the statistical significance of β_3 from the same equation.

Anticipated reminders increase take-up for firms with low trust as shown in Figure 11. Within the trust measure results in the bottom panel, the take-up of low trust firms (blue) increased when they received an anticipated reminder (solid fill) compared to when they received an unanticipated reminder (empty fill). The top panel shows the coefficient on the anticipated reminder variable is positive when restricting the sample to low trust firms. There is no difference in take-up for firms with high trust (red) due to the anticipated reminder compared to the unanticipated one. The coefficient on the interaction term for high trust and anticipated reminder is negative and

[4] Appendix Table A.4 shows the complete regression results, comparing firms who received anticipated versus non-anticipated reminders.

statistically significant at the 1% level when we estimated Equation 5. This indicates that the anticipated reminder is concentrated among low-trust firms, while the anticipated reminder has no effect for high-trust firms.

Among other survey measures, the coefficients on the interaction term is generally insignificant as seen in Table A.4. Beyond trust, there is no other survey measure across low and high that had a higher take-up rate due to the anticipated reminder. In other words, the anticipated reminder was helpful in allowing firms to overcome their initial level of distrust and not for any other survey measure.

It is possible that the reminder effect is also concentrated among low-trusting firms. To test this hypothesis, we repeat the same exercise comparing the unanticipated-reminder groups to groups with *no* reminder and show the results in Appendix Table A.5. We find that the reminder effect is concentrated among firms that are more likely to procrastinate and firms with low memory (both statistically significant at the 5% level).

Figure 12 presents additional evidence that the anticipated reminder effect is concentrated among the less-trusting firms in the sample. We proxy trust with experience using the technology by using administrative data on months using the technology from the entire sample that received reminders. We plot experience using the technology against offer take-up rate. As experience could be correlated with other factors, we residualize take-up and months using the technology with baseline firm characteristics selected using double lasso selection. The figure shows a broadly positive relationship between residualized months using the technology and the take-up rate for firms that received reminders. Firms that have used the technology for a shorter time likely have lower trust in the FinTech company. It is for these less-experienced firms that the anticipated reminder has a statistically significant positive effect.

6.2 Alternative Explanations for Anticipated Reminder Effect

While the survey results so far speak directly to the hypothesized role of trust, we consider three alternative mechanisms for the effect of anticipated reminders on take up.

6.2.1 Anticipated Reminder Increases Knowledge of Offer Profitability

First, we ask whether the announcement of a future reminder may induce firms check the offer's profitability or worth to them, knowing they can adopt when they get the reminder. For example, firms 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 the first potential alternative mechanism using both survey and administrative data.

In the survey, we asked firms "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 firms are fairly accurate (Figure 13). 23% of firms report their fee precisely, and the vast majority who are not perfectly accurate report that their fee is 4% which could be due to rounding. Thus, the vast majority of firms either accurately report their fee or slightly overreport it, which if anything would lead them to think the offer is even more profitable than it is.

In addition, we use administrative data on whether firms log in to their accounts to check their current fee or sales. We create outcome indicator variables if the firm ever logged into their account or checked the amount of deposits from electronic sales in the days between when the initial offer is sent and before the reminder is sent. As shown in Table 3, we find that firms that were told about a future anticipated reminder are not more likely to check their online accounts in the days after we sent the initial offers compared to other firms in our study.

We also note that, in the survey, we ask firms that received an anticipated reminder and accepted on or after the day of the reminder or did not accept the offer, "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?" 92.4% of the firms report not taking any particular action to evaluate the offer. Among the remaining 7.6% who do report taking some action between receiving the initial offer and receiving the anticipated reminder, only 2 out of 8 firms reported calculating whether they should accept the offer.

Furthermore, we also highlight that in answers to another question ("Why did you wait until {days to accept} days later?", see Appendix Figure B.1), only 12.3% of firms replied they needed to discuss or think about the offer first.[5] Although more firms mentioned they had to discuss or think about the offer in the anticipated reminder group (14.6%) than in the unanticipated group (5.1%), this difference is not statistically significant ($p = 0.361$). We conclude that additional steps to evaluate the offer due to the announcement of the reminder are not a plausible explanation for our findings.

6.2.2 Negative Effect of Unanticipated Reminder

As a second alternative mechanism, we consider the possibility that firms in the unanticipated-reminder group may feel annoyed when they receive the reminder or ashamed that they did not yet adopt the profitable opportunity. As a result, they may be 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 unanticipated reminder makes the decision maker feel ashamed and

[5] Full 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 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.

thus makes them "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 unanticipated reminder, we asked firms that received a reminder an open-ended question to tell us how they felt when they received the reminder (see Appendix Figure B.11). Only 2.5% of firms responded that they were annoyed by the reminder, 2.7% in the anticipated reminder and 2.4% in the unanticipated reminder groups. This difference is close to 0 and not statistically significant ($p = 0.494$). Instead, the most common responses indicated that the reminder made firms feel important as a client.

6.2.3 Anticipated Reminder Effects are Driven by Larger Firms

As a third alternative mechanism, we address if our results are driven by larger firms. Larger firms could have more workers and hence more aware of the reminder to accept the offer. In addition, larger firms may receive the initial offer but still delay to accept the offer until they discuss with the firm decision-maker. With the anticipated reminder, they are given a concrete day to decide by and hence could be more likely to set in the firm agenda, discuss the offer and to accept.

To address potential differential reminder effects by firm size on offer take-up, we test for heterogeneity in the effects by firms' number of workers using the survey sample and by firms' baseline sales measured with administrative data using the whole sample. Using the survey sample, the first two columns in Table 4 we test for an additional reminder effect among firms with a number of employees above the median or above one employee. There is no statistically significant additional effect of the reminder among larger firms with more employees as observed in the coefficient estimated for the interaction variable of the reminder with the firm size survey sample proxy variables. When we restrict the sample to only firms that received a reminder, columns 3 and 4 show that there is no statistically significant additional effect of an anticipated reminder among larger firms surveyed as measured by their number of workers.[6]

Using the whole sample, Figure 14 shows anticipated reminders increase take-up regardless of baseline sales. We do not find a statistically significant difference in take-up due to the anticipated reminder by firm size as measured by baseline sales. The lack of heterogeneous effects by number of employees and baseline sales do not support the hypothesis that reminder and anticipated reminder effects are driven by larger firms in our sample.

We also don't find heterogeneous treatment effects by other firm characteristics. Using administrative data, Appendix Tables A.7, A.8, A.9, and A.10 test for an additional effect of the reminder, deadline, and anticipated reminder by baseline firm characteristics: owner age, owner sex, baseline

[6]Using the survey sample, we also find no differential effects of the reminder or anticipated reminder in accepting the offer by the percent of sales the firm makes through the technology. Appedix Table A.6 presents the results similar to Table 4. There is no statistical significance of the coefficient on the interaction variable between the reminder or anticipated reminder with an indicator of the firm being in the top median of percentage sales using the technology.

change in sales, and business type. There is no statistically significant differential effect of each of the three treatments by baseline firm characteristics. Additionally, we test whether there is an additional effect of reminder or anticipated reminder in firms where the owner is the person in the firm who received the emails (Appendix Table A.11). We don't find evidence to support this hypothesis.

6.3 Accounting for Low Take-Up Rates

Why then does take-up of the offer remain far below 100%, even with an anticipated reminder and despite being a profitable opportunity? There are a number of potential reasons. Some firms likely do not trust the offer even with the anticipated reminder, and the anticipated reminder effect does not change depending on firm characteristics. Other firms may have been very busy both when they got the initial email and when they got the reminder, and then forgot. Some firms may not have seen the messages. Finally, although cost savings from the lower fee are equal to 2.7% of profits for the median firm (based on survey data on expected benefits and total profits), there is heterogeneity driven by (i) the random variation in whether we offered firms a 2.75% or 3.00% fee, (ii) the firm's profit margins, and (iii) the percent of sales transacted through the FinTech payments technology rather than in cash. Appendix Figure B.12 shows, based on a survey question, that there is indeed substantial heterogeneity in the percentage of sales made through the FinTech payments technology.

7 Elasticity of Card Payment Revenues

Firms that adopted the lower merchant fee increased their usage of the payment technology. To test the impact of a lower merchant fee on payment usage we use the following regression:

$$y_{it} = \beta \cdot Treated_i \times Post_t + \gamma_i + \delta_t + \varepsilon_{it}, \tag{6}$$

where y_{it} is a payment-technology usage outcome, i denotes a firm, t denotes a month, γ_i are firm fixed effects and δ_t are time fixed effects. Our payment usage outcome variables are the log of sales volume (in pesos) plus one, the log of the number of transactions plus one, and an indicator for whether the firm continued making transactions through the payment technology on or after the current month. 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 (6) and instrument $Accepted_i$ with $Treated_i$.

Panel A of Table 5 shows the ITT effect of the lower merchant fee on payment usage. 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. Firms that received the offer increased the average sales volume and number of payments they transacted with the payment technology by 10.7% and 30%, respectively.[7] The third column (Panel A) shows the regression results with the extensive measure of payment usage: an indicator if the firm made any number of transactions on or after the current month. Firms that received the offer increased their probability of continuing to use the payment technology by 1.3pp.

Panel B of the same table shows the TOT effect of the lower merchant fee on payment usage. Firms that accepted the offer increased the sales volume and number of payments they transacted with the payment technology by about 42% and 10.8%, respectively. Firms that accepted the offer also increased their probability of using the payment technology by 4.3pp. The control mean of the probability of continuing to use the payment technology is 95%. This means that firms that accepted the offer were, in relative terms, 4.5% (=4.3/10) 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 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. 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. Firms can surcharge a percentage or fixed amount to card-paying customers, set a minimum threshold for paying with card, or even mention that the terminal does not work at the time of payment. With a lower fee per transaction, firms could have increased their relative preference for card payments.

One way to incentivize more card payments is to eliminate surcharges. To explore the impact of lower fees on firms, we asked those who accepted the lower fee and recalled accepting or clicking on the offer to respond to the open-ended question "Is this offer working for your business? What impact has it had?" 24.1% of firms replied that the lower fee helped them stop surcharging customers who paid with cards (Appendix Figure B.13). For example, one firm replied, "(The effect is) very good, (we) don't charge commission to clients anymore". Additionally, 9.1% of firms answered that they began giving clients discounts on products due to the lower commission. For example, one firm answered "It works when charging clients, (and) giving them discounts".

In total, 26.5% of firms said they stopped surcharging or began giving discounts. Among the

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

group of firms that stopped surcharging or began giving discounts, 74.1% report that they increased their profits or the number of card transactions. This suggests that accepting the lower offer led firms to absorb the commission either directly or indirectly through discounts, and that this led to an increase in sales and greater profitability.

8 Conclusion

We find that limited memory, overconfidence about memory, and a lack of trust in other firms partly explain why firms are slow to adopt profitable opportunities. We sent firms an offer to lower the merchant fee they pay for every electronic card payment they accept from customers. We find that when the offer included a reminder it had a large effect on taking up a profitable opportunity. Reminders increased take-up of the lower fee by 18%, suggesting that firms are forgetful about adopting profitable opportunities. We find that firms do not procrastinate, and hence the deadline does not have an effect. Anticipated reminders increased the lower merchant fee adoption by an additional 7% on top of an unanticipated reminder. Through a survey with a subsample of the firms in the study, we find that the anticipated reminder increased trust: it increased firms' perceptions of the offer's value and increased take-up by firms that trust advertised offers less.

Our findings suggest that the analysis of slow adoption within firms benefits from researchers considering mechanisms beyond the traditional economic explanations of non-adoption, such as information frictions, fixed costs, and incentive misalignment. Well-known behavioral determinants of individuals failing to adopt and take advantage of a new opportunity appear to be valid also in the firm context. In particular, imperfect memory and distorted beliefs about future failures to remember emerge as significant determinants in our setting, while present bias does not appear to play an important role. Beyond those two factors, which have been much discussed in the consumer-level literature, we provide evidence of trust as a key explanatory variable, which has received less attention so far.

References

Anderson, James C. and James A. Narus (1990). "A Model of Distributor Firm and Manufacturer Firm Working Partnerships". In: *Journal of Marketing* 54.1, pp. 42–58.

Anderson, Stephen J. and David McKenzie (Jan. 1, 2022). "Improving Business Practices and the Boundary of the Entrepreneur: A Randomized Experiment Comparing Training, Consulting, Insourcing, and Outsourcing". In: *Journal of Political Economy* 130.1, pp. 157–209.

Atkin, David, Azam Chaudhry, Shamyla Chaudry, Amit K Khandelwal, and Eric Verhoogen (2017). "Organizational Barriers to Technology Adoption: Evidence from Soccer-ball Producers in Pakistan". In: *The Quarterly Journal of Economics* 132.3, pp. 1101–1164.

Bachas, Pierre, Paul Gertler, Sean Higgins, and Enrique Seira (2021). "How Debit Cards Enable the Poor to Save More". In: *Journal of Finance* 76, pp. 1913–1957.

Banerjee, Abhijit V. and Esther Duflo (2000). "Reputation Effects and the Limits of Contracting: A Study of the Indian Software Industry". In: *Quarterly Journal of Economics* 115.3, pp. 989–1017.

Beaman, Lori, Jeremy Magruder, and Jonathan Robinson (2014). "Minding Small Change Among Small Firms in Kenya". In: *Journal of Development Economics* 108, pp. 69–86.

Bisin, Alberto and Kyle Hyndman (2020). "Present-bias, procrastination and deadlines in a field experiment". In: *Games and Economic Behavior* 119, pp. 339–357.

Bloom, Nicholas, Benn Eifert, Aprajit Mahajan, David McKenzie, and John Roberts (2013). "Does Management Matter? Evidence from India". In: *The Quarterly Journal of Economics* 128.1, pp. 1–51.

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". In: *Journal of Political Economy* 126.2, pp. 635–687.

Brune, Lasse, Xavier Giné, Jessica Goldberg, and Dean Yang (2016). "Facilitating Savings for Agriculture: Field Experimental Evidence From Malawi". In: *Economic Development and Cultural Change* 64 (2), pp. 187–220.

Burger, Nicholas, Gary Charness, and John Lynham (2011). "Field and Online Experiments in Self-Control". In: *Journal of Economic Behavior & Organization* (77), pp. 393–404.

Cai, Jing and Adam Szeidl (Dec. 2018). "Interfirm Relationships and Business Performance*". In: *The Quarterly Journal of Economics* 133.3, pp. 1229–1282.

Calzolari, Giacomo and Mattia Nardatto (2016). "Effective Reminders". In: *Management Science* 63 (9), pp. 2915–2932.

Camerer, Colin and Dan Lovallo (1999). "Overconfidence and Excess Entry: An Experimental Approach". In: *American Economic Review* 89 (1), pp. 306–318.

Campos-Mercade, Pol, Armando N. Meier, Florian H. Schneider, Stephan Meier, Devin Pope, and Erik Wengström (2021). “Monetary incentives increase COVID-19 vaccinations”. In: *Science* 374.6569, pp. 879–882.

Celhay, Pablo A, Paul J Gertler, Paula Giovagnoli, and Christel Vermeersch (2019). “Long-Run Effects of Temporary Incentives on Medical Care Productivity”. In: *American Economic Journal: Applied Economics* 11.3, pp. 92–127.

Cole, Shawn, Xavier Giné, Jeremy Tobacman, Peta Topalova, Robert Townsend, and James Vickery (2013). “Barriers to Household Risk Management: Evidence From India”. In: *American Economic Journal: Applied Economics* 5 (1), pp. 104–135.

Dai, Hengchen, Silvia Saccardo, Maria A. Han, Lily Roh, Naveen Raja, Sitaram Vangala, Hardikkumar Modi, Shital Pandya, Michael Sloyan, and Daniel M. Croymans (2021). “Behavioural Nudges Increase COVID-19 Vaccinations”. In: *Nature* 597, pp. 404–409.

DellaVigna, Stefano and Matthew Gentzkow (June 2019). “Uniform Pricing in U.S. Retail Chains”. In: *The Quarterly Journal of Economics* 134.4, pp. 2011–2084.

DellaVigna, Stefano and Ulrike Malmendier (2004). “Contract Design and Self-Control: Theory and Evidence”. In: *Quarterly Journal of Economics* 119 (2), pp. 353–402.

— (June 2006). “Paying Not to Go to the Gym”. In: *American Economic Review* 96.3, pp. 694–719.

Duflo, Esther, Michael Kremer, and Jonathan Robinson (2011). “Nudging Farmers to Use Fertilizer: Theory and Experimental Evidence from Kenya”. In: *American Economic Review* 101.6, pp. 2350–90.

Ericson, Keith M. Marzilli (2011). “Forgetting We Forget: Overconfidence and Memory”. In: *Journal of the European Economic Association* 9 (2), pp. 43–60.

— (2014). “Consumer Inertia and Firm Pricing in the Medicare Part D Prescription Drug Insurance Exchange”. In: *American Economic Association: Economic Policy* 6 (1), pp. 38–64.

Ericson, Keith Marzilli (2017). “On the interaction of memory and procrastination: Implications for reminders, deadlines, and empirical estimation”. In: *Journal of the European Economic Association* 15.3. ISBN: 1542-4766 Publisher: Oxford University Press, pp. 692–719.

Feigenberg, Benjamin, Erica Field, and Rohini Pande (2013). “The Economic Returns to Social Interaction: Experimental Evidence from Microfinance”. In: *Review of Economic Studies* 80, pp. 1459–1483.

Giorcelli, Michela (Jan. 2019). “The Long-Term Effects of Management and Technology Transfers”. In: *American Economic Review* 109.1, pp. 121–52.

Guiso, Luigi, Paola Sapienza, and Luigi Zingales (2004). “The Role of Social Capital in Financial Development”. In: *American Economic Review* 49 (3), pp. 526–556.

— (2008). “Trusting the Stock Market”. In: *Journal of Finance* 63 (6), pp. 2557–2600.

Gurol-Urganci, I, T de Jongh, V Vodopivec-Jamsek, R Atun, and J Car (2013). "Mobile Phone Messaging Reminders for Attendance at Healthcare Appointments (Review)". In: *Cochrane Database of Systematic Reviews* 2013, pp. 1–43.

Handel, Benjamin R. (Dec. 2013). "Adverse Selection and Inertia in Health Insurance Markets: When Nudging Hurts". In: *American Economic Review* 103.7, pp. 2643–82.

Higgins, Sean (2022). "Financial Technology Adoption: Network Externalities of Cashless Payments in Mexico". In: *Conditionally accepted, American Economic Review*.

Johnson, Eric J., Stephan Meier, and Olivier Toubia (2019). "What's the Catch? Suspicion of Bank Motives and Sluggish Refinancing". In: *Review of Financial Studies* 32 (2), pp. 467–495.

Karlan, Dean, Margaret McConnell, Sendhil Mullainathan, and Jonathan Zinman (2016). "Getting to the Top of Mind: How Reminders Increase Saving". In: *Management Science* 62.12, pp. 3393–3411.

Karlan, Dean, Markus Mobius, Tanya Rosenblat, and Adam Szeidl (2009). "Trust and Social Collateral". In: *Quarterly Journal of Economics* 124 (3), pp. 1307–1361.

Karlan, Dean, Melanie Morten, and Jonathan Zinman (2016). "A Personal Touch in Text Messaging Can Improve Microloan Repayment". In: *Behavioral Science & Policy* 1 (2), pp. 31–39.

Kremer, Michael, Jean Lee, Jonathan Robinson, and Olga Rostapshova (May 2013). "Behavioral Biases and Firm Behavior: Evidence from Kenyan Retail Shops". In: *American Economic Review* 103.3, pp. 362–68.

Laibson, David (1997). "Golden Eggs and Hyperbolic Discounting". In: *Quarterly Journal of Economics* 112 (2), pp. 443–477.

Madrian, Brigitte C. and Dennis F. Shea (2001). "The Power of Suggestion: Inertia in 401(k) Participation and Savings Behavior". In: *Quarterly Journal of Economics* 116 (4), pp. 1149–1187.

Malmendier, Ulrike and Geoffrey Tate (2005). "CEO Overconfidence and Corporate Investment". In: *Journal of Finance* 60 (6), pp. 2661–2700.

McMillan, John and Christopher Woodruff (1999). "Interfirm Relationships and Informal Credit in Vietnam". In: *Quarterly Journal of Economics* 114 (4), pp. 1285–1320.

Mehrotra, Rahul, Vincent Somville, and Lore Vandewalle (2021). "Increasing trust in bankers to enhance savings: Experimental evidence from India". In: *Economic Development and Cultural Change* 69.2, pp. 623–644.

Mishra, Prachi, Nagpurnanand Prabhala, and Raghuram G Rajan (Oct. 2021). "The Relationship Dilemma: Why Do Banks Differ in the Pace at Which They Adopt New Technology?" In: *The Review of Financial Studies*. hhab118.

Olafsson, Arna and Michaela Pagel (Oct. 2017). *The Ostrich in Us: Selective Attention to Financial Accounts, Income, Spending, and Liquidity*. Working Paper 23945. National Bureau of Economic Research.

Osili, Una Okonkwo and Anna Paulson (2008). “Institutions and Financial Development: Evidence from International Migrants in the United States”. In: *Review of Economics and Statistics* 90 (3), pp. 498–517.

— (2014). “Crises and Confidence: Systemic Banking Crises and Depositor Behavior”. In: *Journal of Financial Economics* 111.3, pp. 646–660.

Verhoogen, Eric (2021). “Firm-Level Upgrading in Developing Countries”. In: *NBER Working Paper* 29461.

Table 1: Baseline Treatment Balance

	Intercept (1)	Anticipated reminder (2)	Unanticipated reminder (3)	Deadline (4)	2.75% Fee (5)	Joint test F-stat (6)
Panel A: Firm owner characteristics						
Owner sex female	0.442***	0.002	-0.003	-0.003	0.002	0.224
	(0.004)	(0.007)	(0.007)	(0.006)	(0.005)	[0.925]
Owner age	39.40***	0.29*	0.23	-0.01	-0.03	1.075
	(0.10)	(0.16)	(0.15)	(0.13)	(0.13)	[0.367]
Panel B: Business characteristics						
Business type						
Beauty	0.087***	0.000	0.000	0.002	0.000	0.081
	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	[0.988]
Clothing	0.089***	0.000	0.001	0.000	0.000	0.007
	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)	[1.000]
Professionals	0.239***	-0.001	-0.001	0.001	0.000	0.017
	(0.004)	(0.006)	(0.006)	(0.005)	(0.005)	[0.999]
Restaurants	0.123***	0.001	0.002	0.000	-0.001	0.046
	(0.003)	(0.005)	(0.004)	(0.004)	(0.004)	[0.996]
Small retailers	0.260***	-0.001	-0.001	0.001	0.000	0.019
	(0.004)	(0.006)	(0.006)	(0.005)	(0.005)	[0.999]
Other	0.202***	0.002	0.000	-0.003	0.001	0.136
	(0.004)	(0.006)	(0.005)	(0.005)	(0.004)	[0.969]
Pre-treatment sales variables						
Months since first transaction	24.11***	0.10	0.11	-0.08	0.12	0.215
	(0.15)	(0.24)	(0.23)	(0.20)	(0.19)	[0.930]
% months business made sales	0.818***	0.001	-0.001	0.001	0.001	0.164
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	[0.957]
Log average monthly sales volume	8.789***	-0.017	0.009	0.008	0.000	0.635
	(0.010)	(0.016)	(0.015)	(0.013)	(0.012)	[0.637]
Log average monthly transactions	2.053***	-0.007	0.001	0.005	0.005	0.086
	(0.013)	(0.020)	(0.019)	(0.016)	(0.016)	[0.987]

Note: This table reports differences in firm owner characteristics, business characteristics, and pre-treatment sales variables by treatment group. The unit of observation is at the firm level. Columns (1)-(5) contain coefficients from the regression of each outcome on an intercept and dummies for anticipated reminder, unanticipated reminder, deadline, and 2.75% fee treatment groups. Column (6) contains the F-statistic and corresponding p-value from a joint F-test of all coefficients in the regression. Log average monthly sales volume and log average monthly transactions transform winsorized sales volume and transactions at the 95th percentile. Data is from 07/2019 to 08/2020 and includes all firms in experiment ($N = 33,978$). Average baseline winsorized monthly sales volume is $541.9 USD, and average baseline winsorized number of transactions is 18.51. Standard errors are in parentheses and p-values for the F-statistics are in square brackets.

Table 2: Main Regression Results

	Accepted Offer
Group 2: No deadline, no reminder	0.254***
	(0.011)
Group 3: No deadline, anticipated reminder	0.305***
	(0.014)
Group 4: No deadline, unanticipated reminder	0.290***
	(0.015)
Group 5: Deadline, no reminder	0.256***
	(0.015)
Group 6: Deadline, anticipated reminder	0.318***
	(0.017)
Group 7: Deadline, unanticipated reminder	0.298***
	(0.017)
Group 8: 24-hour deadline, no reminder	0.229***
	(0.012)
Num.Obs.	33978
R2	0.055
R2 Adj.	0.054
Cluster Std. Errors	Strata
Fixed Effects	Strata

Note: This regression reports the effect of being assigned to a treatment group on the probability of accepting the offer. The unit of observation is at the firm level. Omitted Group 1 is Control. Regressions include strata fixed effects. Clustered standard errors at the strata level are included in parentheses.

Table 3: Account Log-ins by Reminder Type

	Log in (1)	Viewed deposits (2)
Intercept	0.095***	0.037***
	(0.003)	(0.002)
Anticipated reminder	-0.003	0.000
	(0.005)	(0.003)
Number of firms	16,254	16,254

Note: This table reports differences in account log-ins by reminder type. The data consists of observations from days 1 to 8 of the experiment (until the deadline) from the anticipated and unanticipated reminder treatment groups. The unit of observation is at the firm level. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Heterogeneous Treatment Effects by Number of Employees

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

Note: This table reports heterogeneous treatment effects by number of employees. The unit of observation is at the firm level. The coefficients come from the regression of lower fee offer take-up on treatment, the heterogeneity variable and the interaction between treatment and the heterogeneity variable. Above median # of employees is defined as firms with ≥ 3 employees.

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 17. All 471 firms in the survey were asked this particular question. Survey question: *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. Survey sample number of employees mean = 3.9 , median = 3 , standard deviation = 7.4.

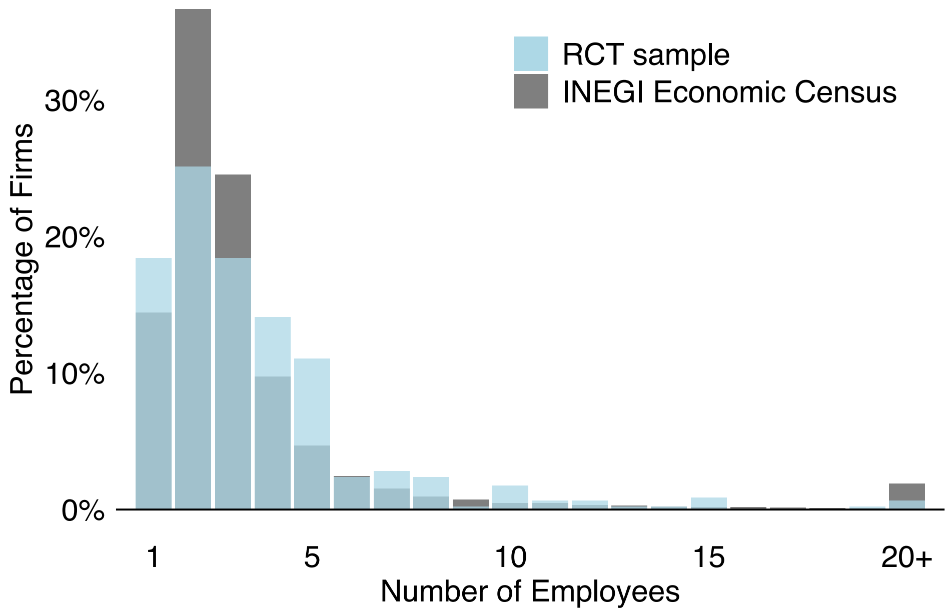
Column (1) includes all firms that provided an answer to the number of employees question, and column (2) includes only firms that received a reminder. Robust standard errors are included in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Monthly Sales Elasticity: Intent to Treat and Treatment on the Treated

	Log(sales + 1) (1)	Log(# transactions + 1) (2)	Continued using technology (3)
Panel A: Intent to Treat			
Post * Treated	0.101**	0.030*	0.013**
	(0.046)	(0.016)	(0.005)
Panel B: Treatment on the Treated			
Post * Adopted	0.351**	0.102*	0.043**
	(0.161)	(0.055)	(0.017)
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	0.850
Control mean (levels, winsorized)	12,178	19.52	0.850

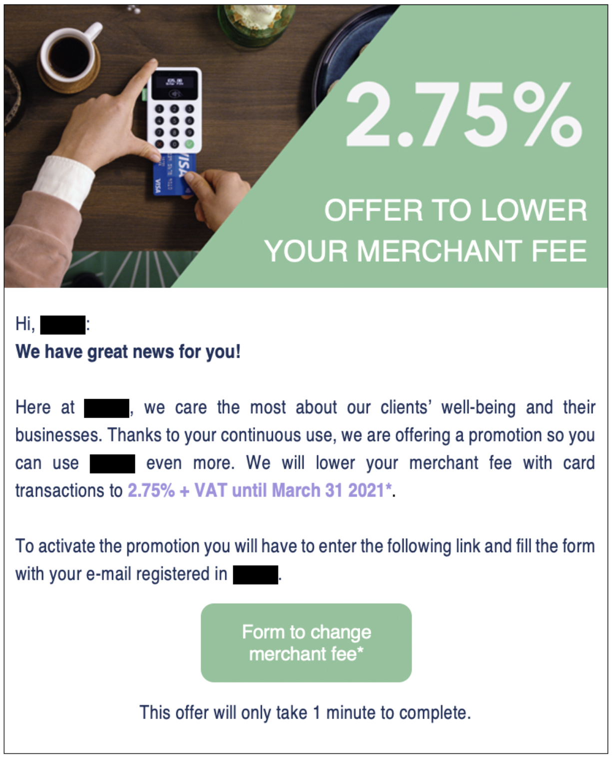
Note: This table reports sales elasticities of the treated group (being offered the lower fee) and of the adopted group (adopting the lower fee). Data is from July 2019 to March 2021, includes Sep 29 and Sep 30 as part of October, and contains all firms in the experiment. The unit of observation is at the firm-month level. Post * Treated is an interaction term of Post and Treated. ‘Post‘ is equal to 1 if the time period is after the firm received the lower fee and ‘Treated‘ is an indicator for if the firm was offered the lower fee. Post * Adopted is an interaction term of Post and Adopted. ‘Post‘ is equal to 1 if the time period is after the firm received the lower fee and ‘Adopted‘ is an indicator for if the firm accepted the lower fee. Post * Adopted is instrumented by Post * Treated, where Treated = 1 if the firm received the offer. Control means include data from the treatment period. Log average monthly sales volume and log average monthly transactions transform winsorized sales volume and transactions at the 95th percentile. Regressions include firm and month fixed effects. Clustered standard errors at the firm level are included in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

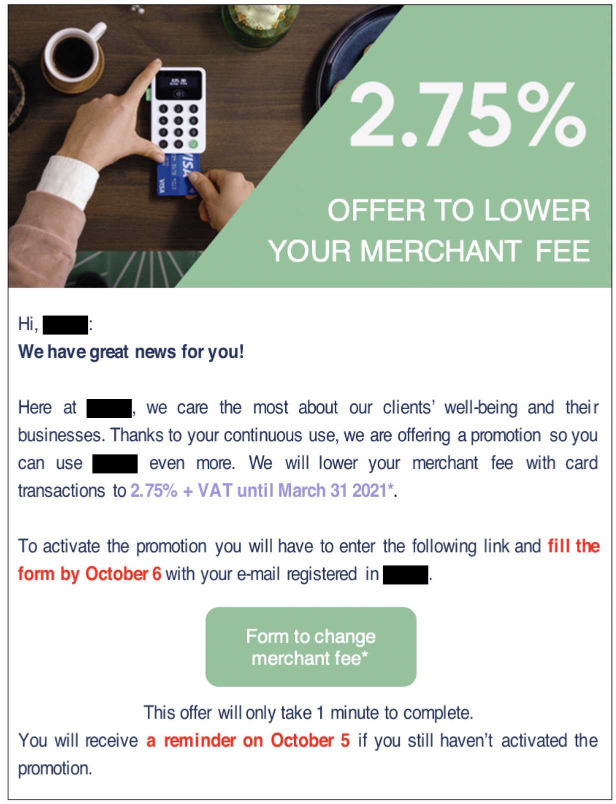
Figure 1: Number of Employees



Note: This figure contains a histogram of the number of employees by firm. Data comes from survey conducted on a random sample of firms in the experiment (N = 471) and the 2019 INEGI Economic Census (N = 5,360,215). All 471 firms in the survey were asked this particular question. Survey question: *How many employees work in your business, including yourself?* 9 firms that did not answer the question were excluded from the sample. We right-censor the figure at 20, so the rightmost bin for the histogram includes firms with $\geq$ 20 employees. 99.8% of firms in our sample and 98.2% of firms in the Economic Census have $\leq$ 20 employees. Survey sample number of employees mean = 3.9, median = 3, standard deviation = 7.4.

Figure 2: Sample Emails with Lower Rate Offers





Note: Left figure shows an offer sent to treatment groups without reminders or with anticipated reminders and no deadline. Right figure shows an offer sent to treatment groups with anticipated reminder and deadlines. The text is translated from the original Spanish into English.

Asterisks at the end of the purple text refer to the fine print. The fine print read: *By filling out the form you authorize (the FinTech) to change the commission on your (FinTech) account to 2.75% + VAT commission per successful card payment transaction until March 31, 2021. Starting April 1st 2021 the fee will revert back to the fee you had before activating this promotion. Terms and conditions apply*. The underlined text redirected to the FinTech's overall terms and conditions website, and only 1.2% of firms that opened the email clicked on this link.

Figure 3: Study Timeline

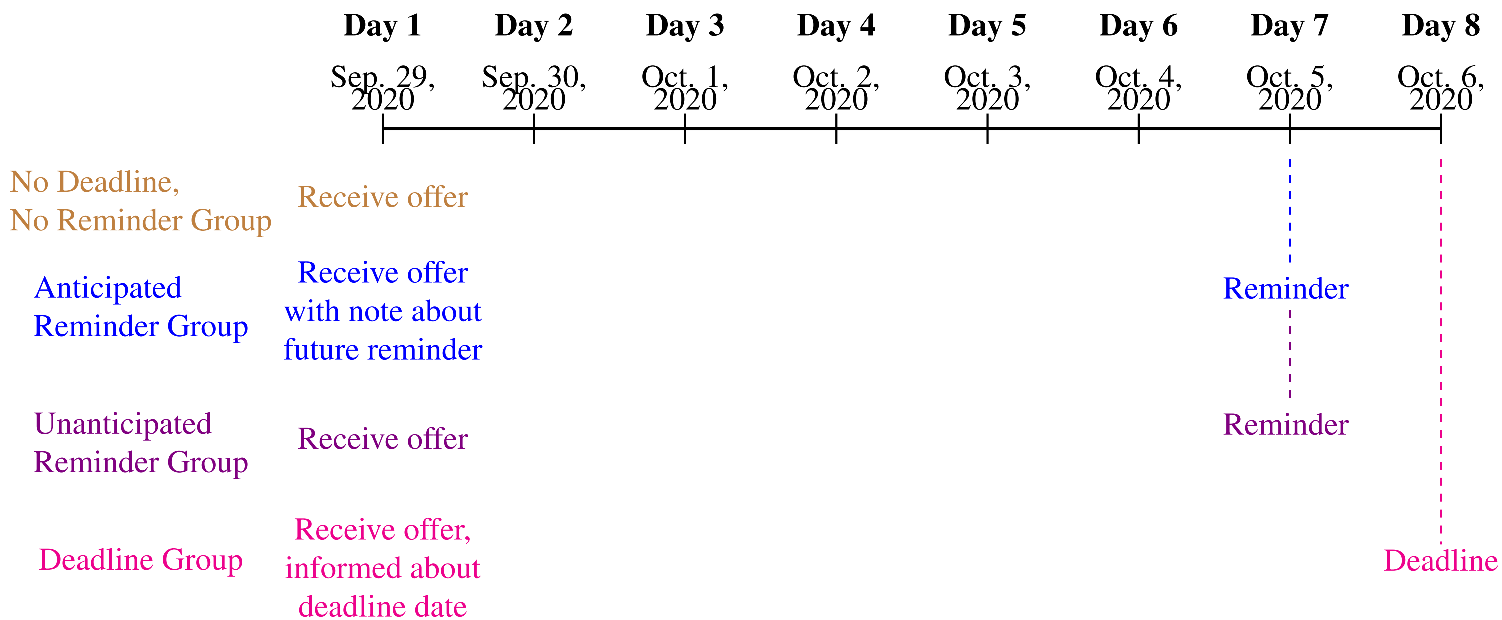
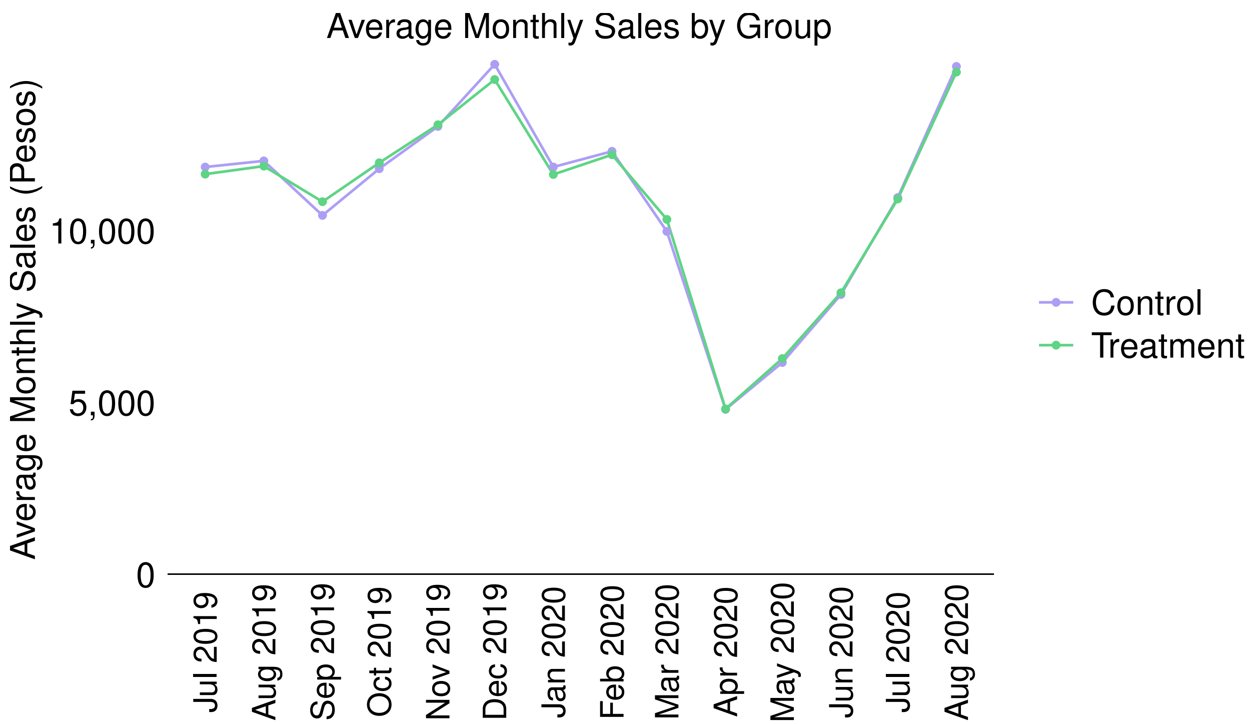
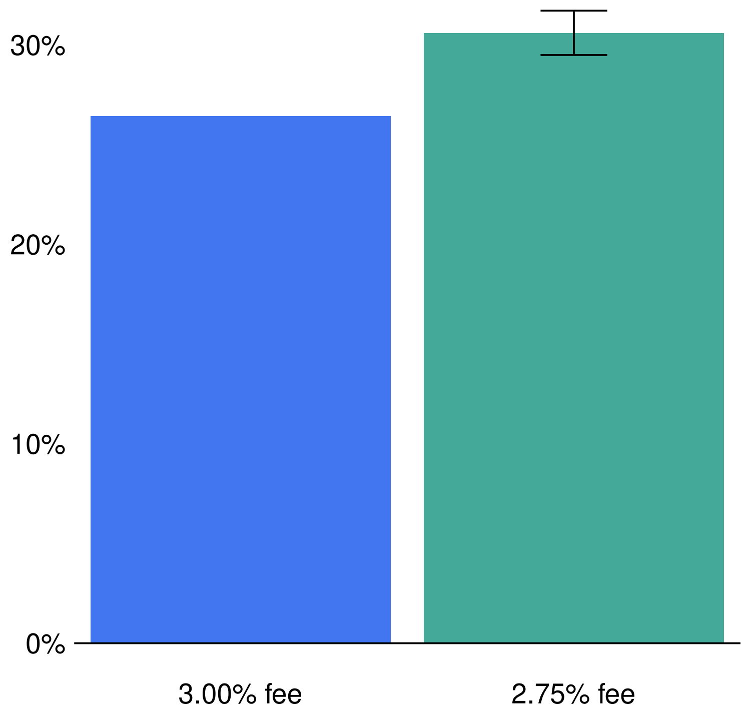


Figure 4: Sales by Month Prior to Experiment Start



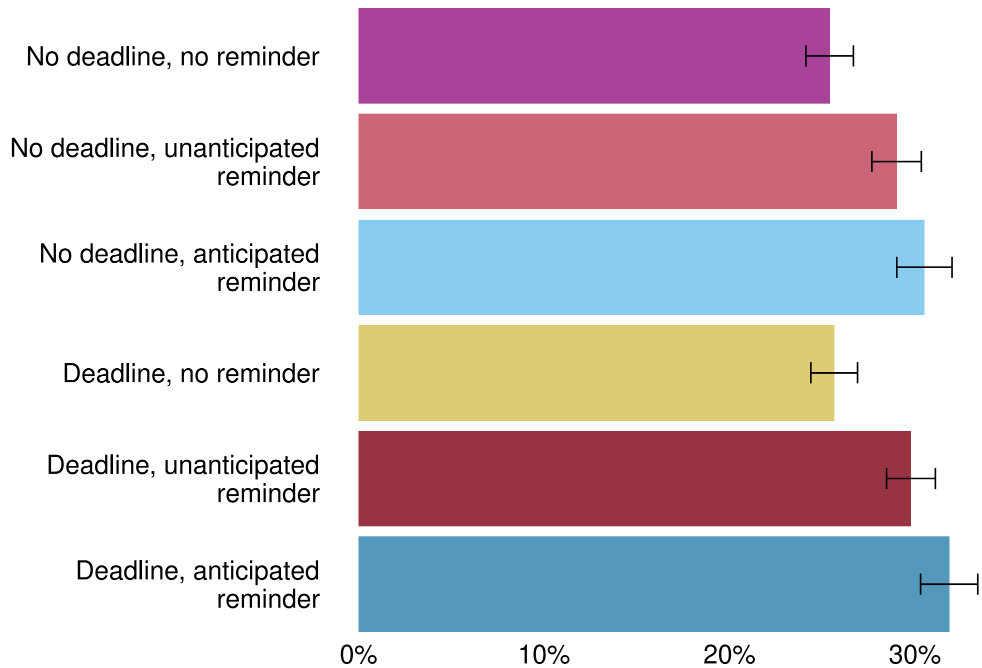
Note: This figure shows average monthly sales for pooled treatment and control groups. Lower fee offers were sent to businesses when sales were back to pre-pandemic levels.

Figure 5: Take-up by Fee



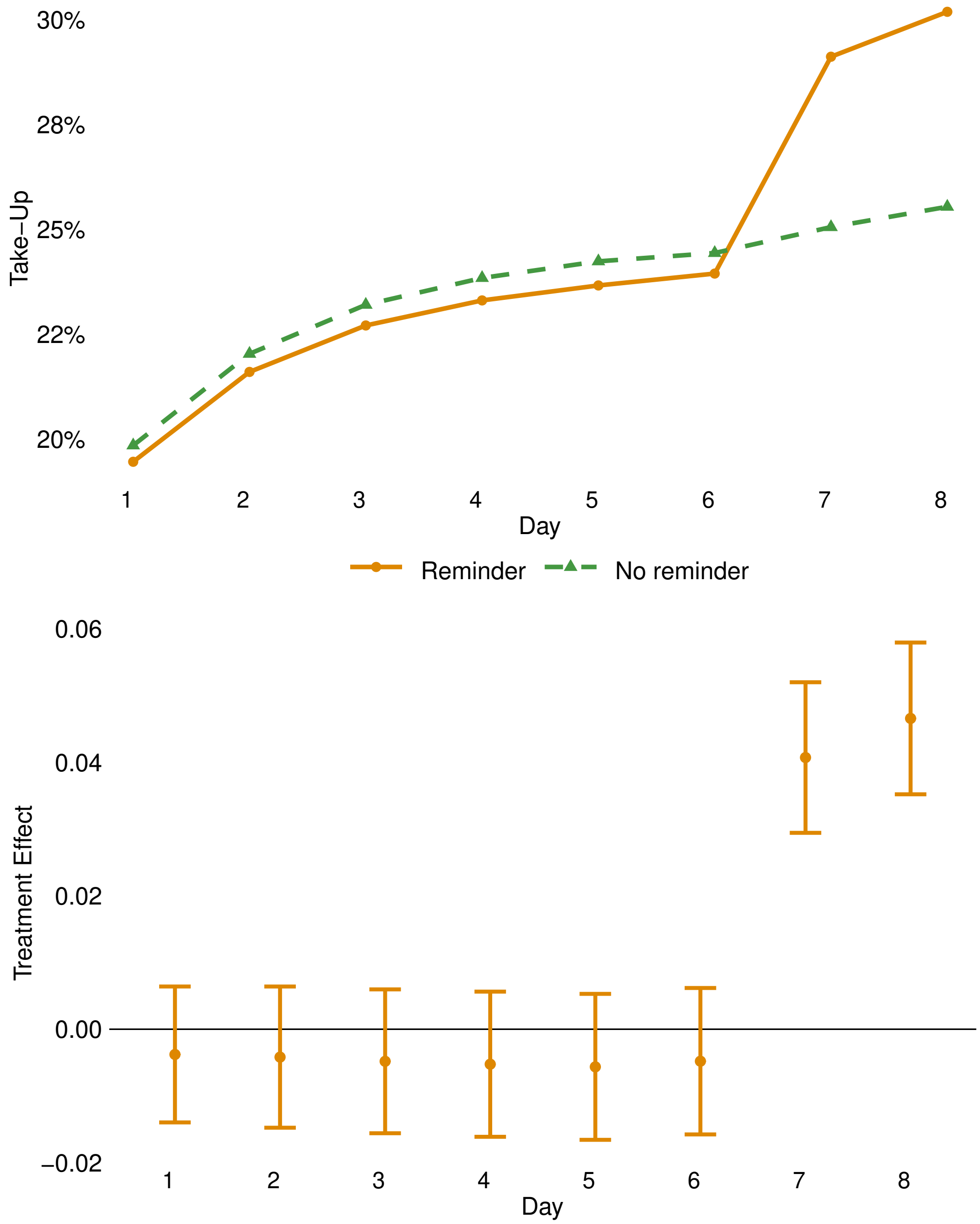
Note: This figure contains a barplot with lower fee offer takeup by the fee offered. The unit of observation is at the firm level.The coefficients and 95% confidence intervals come from a regression of takeup by the deadline on a dummy of getting the 2.75% fee. Data includes 25,327 businesses, excluding the Control and 24-hour deadline, no reminder groups from the full sample of 33,978 firms. Take-up is from September 29 to October 6 (the day of the deadline). Average take-up is 26.4% for the 3.00% fee group, and 30.6% for the 2.75% fee group. Robust standard errors are included in parentheses.

Figure 6: Take-up by Treatment Arm



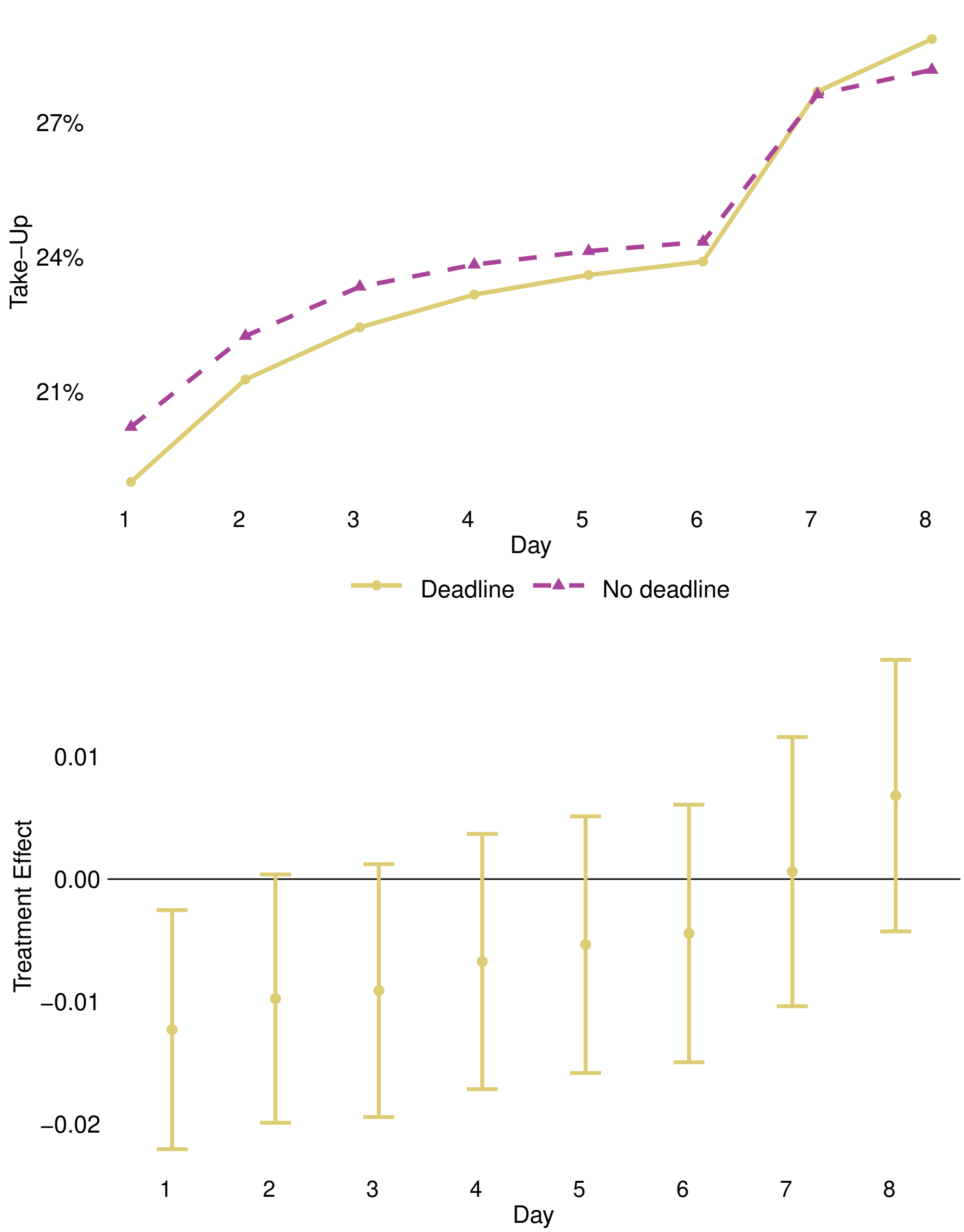
Note: This figure contains a barplot with lower fee offer takeup by the deadline by treatment group. The unit of observation is at the firm level.The coefficients and 95% confidence intervals come from a regression of takeup by the deadline on treatment group dummies. Data includes 25,327 businesses, excluding the Control and 24-hour deadline, no reminder groups from the full sample of 33,978 firms. Take-up is from September 29 to October 6 (the day of the deadline). Robust standard errors are included in parentheses.

Figure 7: Effect of Reminder on Take-up



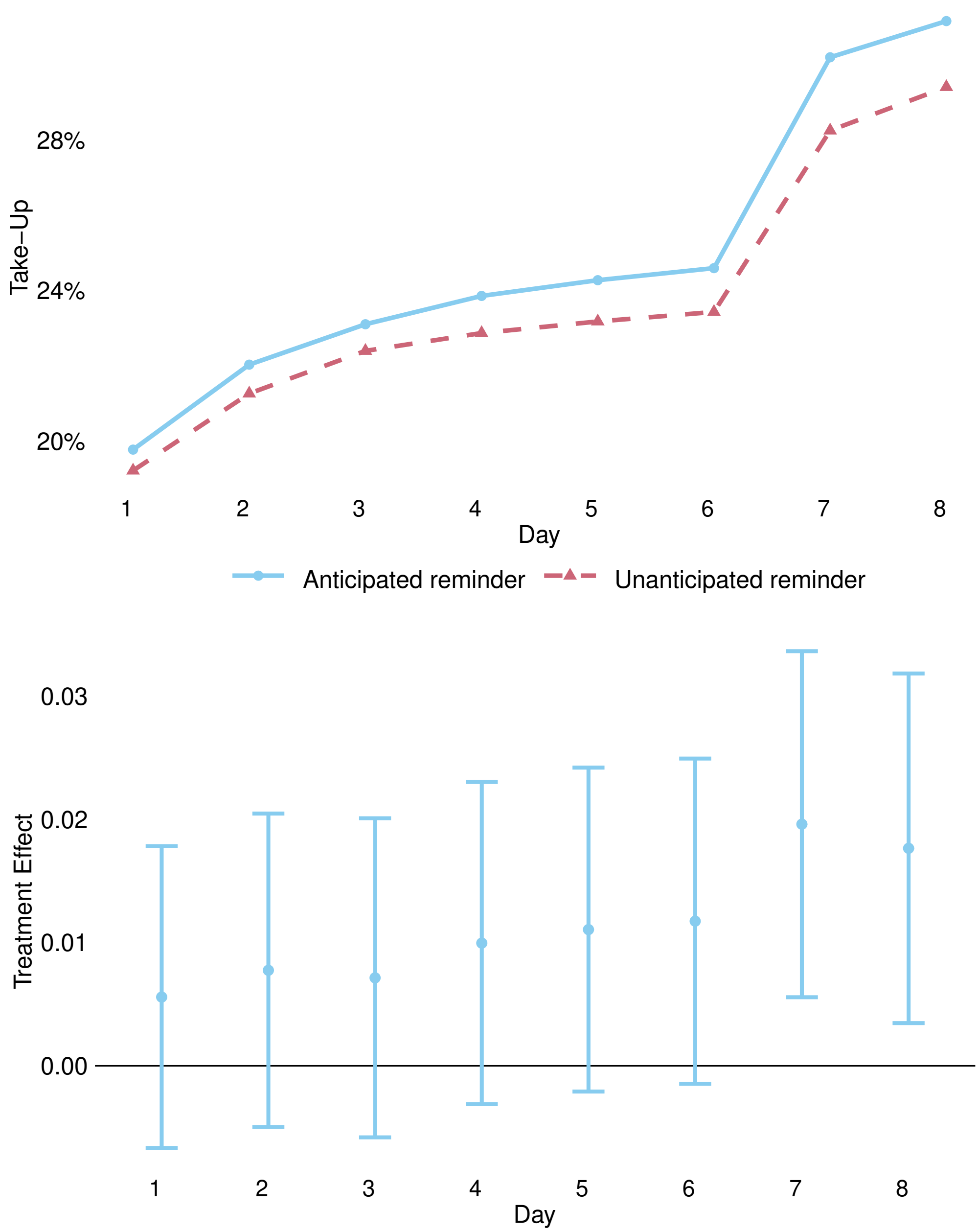
Note: The upper panel shows take-up rates of the reminder and no reminder groupsThe bottom panel shows the corresponding coefficient estimates for the differential take up of the reminder groups, separately for each day of the experiment.

Figure 8: Effect of Deadline on Take-up



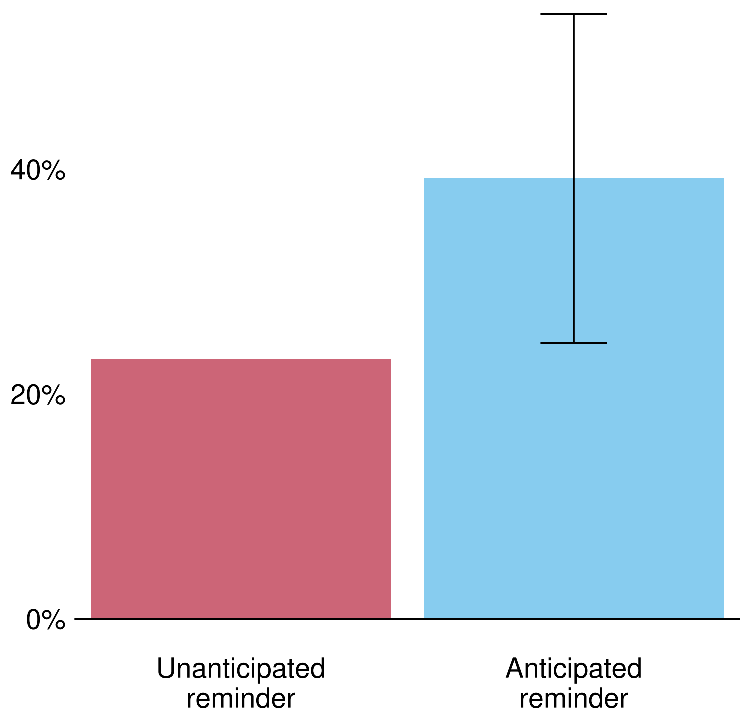
Note: This figure shows short-term take-up and coefficient estimates for deadline and no deadline groups.

Figure 9: Effect of Anticipated Reminder on Take-up



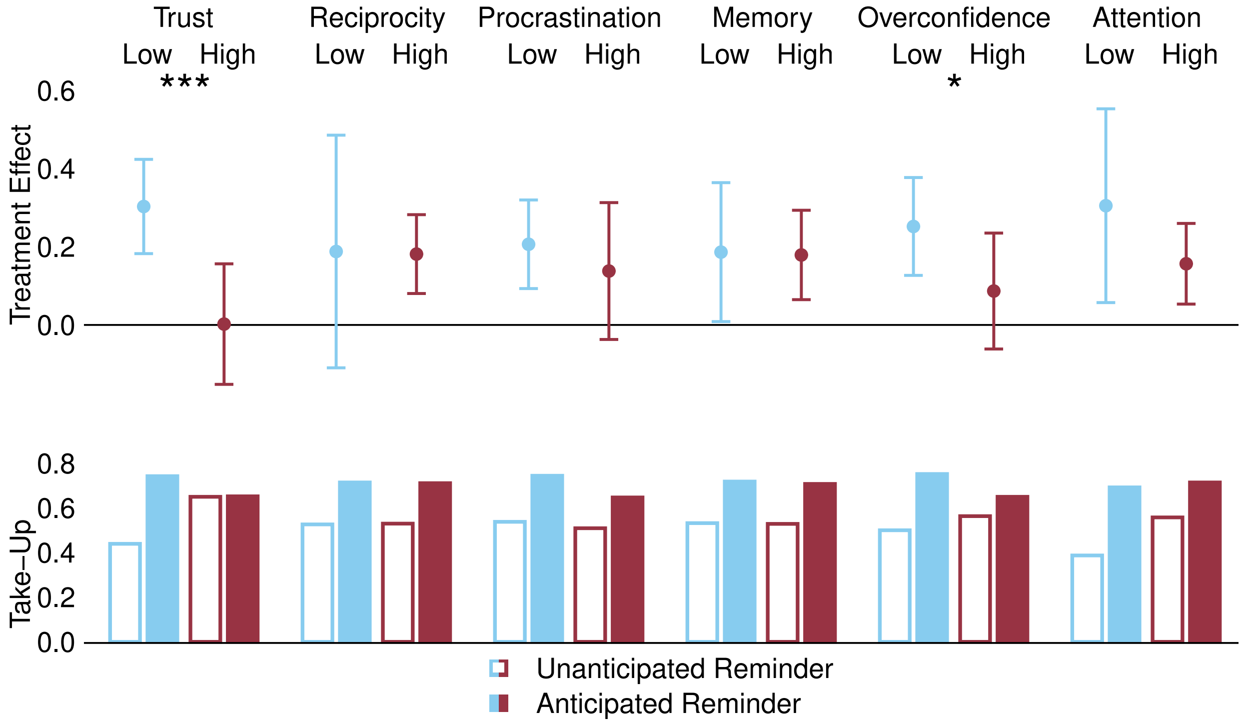
Note: This figure shows short-term take-up and coefficient estimates for anticipated and unanticipated reminder groups.

Figure 10: Percentage of Firms for Which Reminder Changed Perception of Offer Value



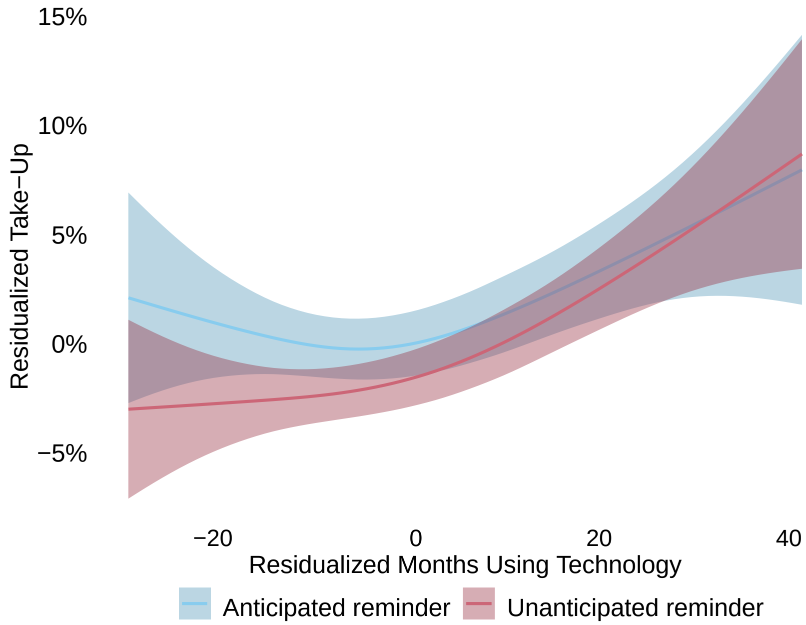
Note: This figure contains a barplot with the percentage of firms that said the reminder changed their perception of the offer's value, by reminder type. Data comes from survey conducted on a random sample of firms in the experiment (*N* = 471), with 157 firms asked this particular question. This question was asked to 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. Survey question: *Did the reminder change your perception of the offer's value?* 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 a dummy of anticipated reminder, with heteroskedasticity-robust standard errors.

Figure 11: Heterogeneous Effect of Anticipated Reminders by Survey Measures



Note: This table reports heterogeneous treatment effects by survey measure. The unit of observation is at the firm level. The coefficients come from the regression of lower fee offer take-up on anticipated reminder, the survey measure,and the interaction between anticipated reminder and the survey measure. Data includes firms with anticipated and unanticipated reminders in survey sample, and includes take-up from September 29 to March 17. 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. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 12: Heterogeneous Treatment Effect of Anticipated Reminders by Firm Experience with Technology

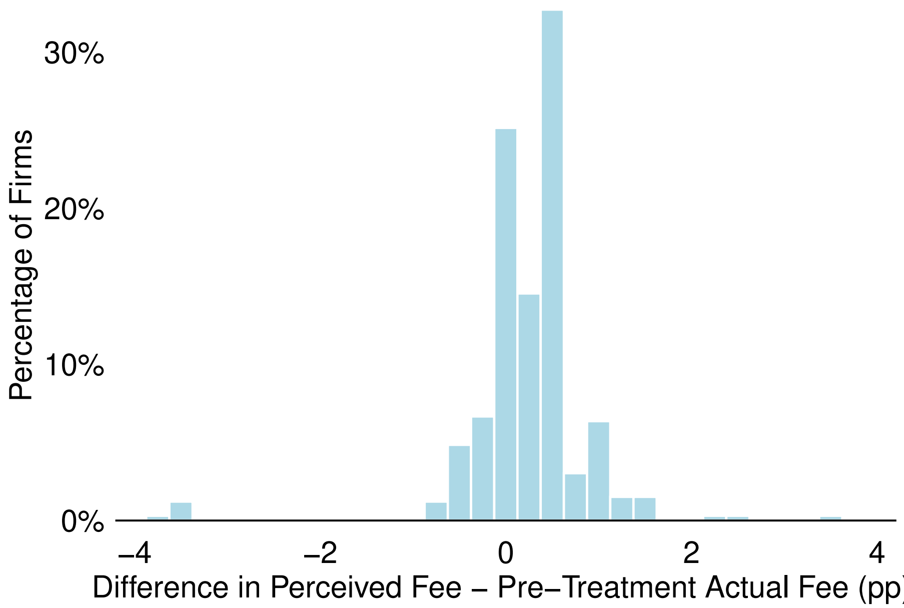


Note: This figure shows a correlation of months using the technology with lower fee offer take-up, split by reminder type. Data contains firms from the anticipated reminder and unanticipated reminder groups, and includes take-up from September 29 to March 17. 7%% of observations had missing values for the variable *Owner age*. We replace these missings with 0 and include a dummy indicating missing owner age, following Anderson and McKenzie (2022). The unit of observation is at the firm level.

Lower fee offer take-up and months using the technology were residualized using baseline firm characteristics. We used double lasso selection to pick the set of variables used for the residualization, from the universe of baseline firm characteristics and their interactions. Double lasso selection was performed separately on months using the technology and lower-fee take-up. We used the union of the two resulting sets and the dummy indicating missing owner age to residualize months using the technology. The residualization consisted in running regressions of take-up and months using the technology on (1) $\mathbb{1}$(Business type: Beauty), (2) $\mathbb{1}$(Business type: Clothing), (3) $\mathbb{1}$(Business type: Professionals) $\times$ Log average pre-treatment monthly # transactions, (4) $\mathbb{1}$(Business type: Professionals) $\times$ Proportion of pre-treatment months business made sales, (5) $\mathbb{1}$(Business type: Restaurants), (6) $\mathbb{1}$(Business type: Small retailers), (7) $\mathbb{1}$(Business type: Small retailers) $\times$ Log average pre-treatment monthly # transactions, (8) $\mathbb{1}$(Missing owner age), (9) Owner age, (10) Owner age $\times$ Proportion of pre-treatment months business made sales, (11) Owner age $\times$ $\mathbb{1}$(Business type: Professionals), (12) $\mathbb{1}$(Owner sex female), and (13) Proportion of pre-treatment months business made sales, and extracting the residuals.

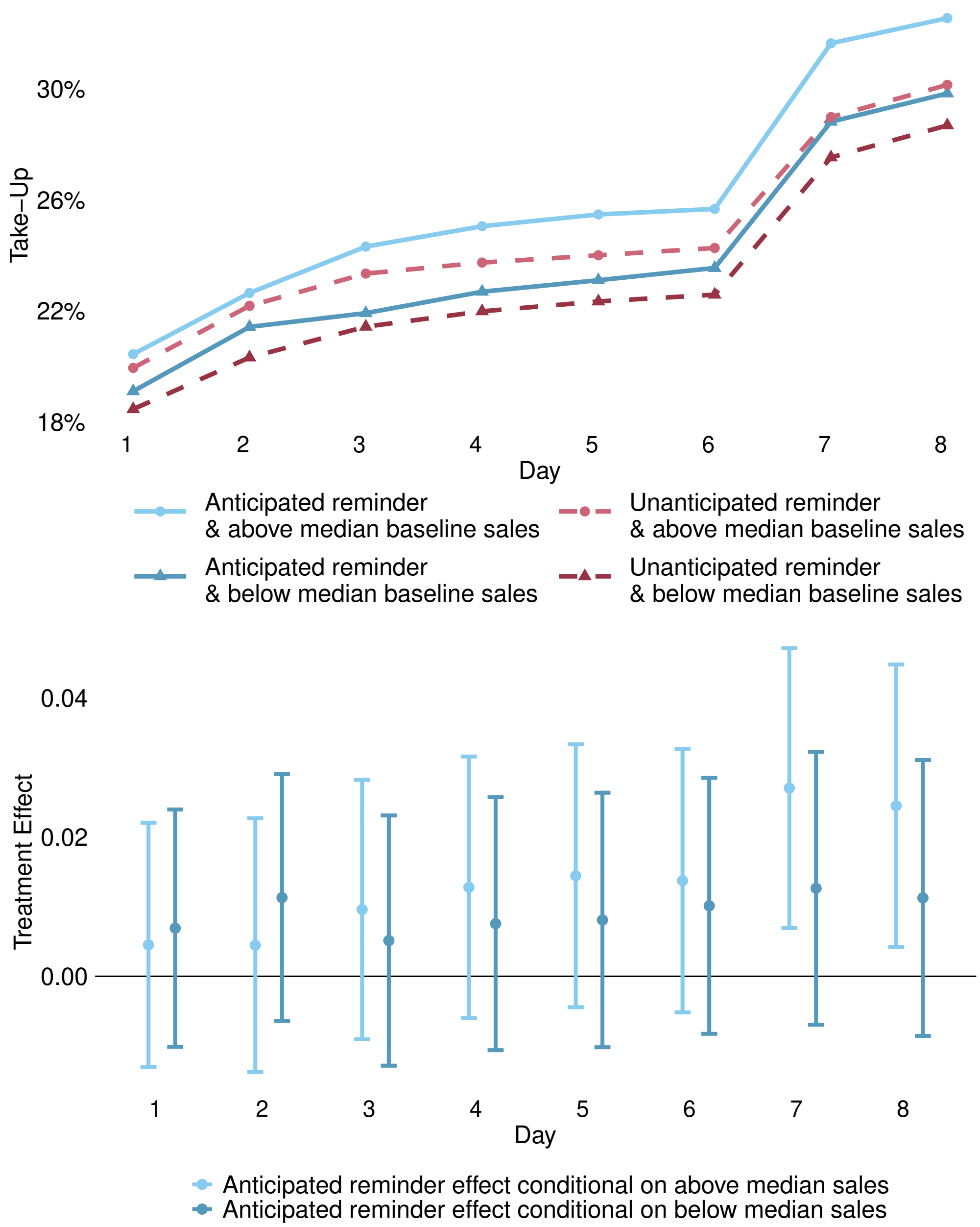
Colored lines are local polynomial regression fits, and shaded polygons are 95% confidence intervals. Firms in the top 5 percentile of months using the technology were omitted from the graph.

Figure 13: Difference in Pre-Treatment Actual Fee and Perceived Fee



Note: This figure contains a histogram of differences in the pre-treatment fee and the fee firms perceive. 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 did not know the answer to the question, and 23 firms that did not answer the question. Differences in actual and perceived fee mean = 0.3, median = 0.2, standard deviation = 0.7.

Figure 14: Effect of Anticipated Reminder on Take-up by Baseline Sales



Note: This figure shows short-term take-up and coefficient estimates for anticipated and unanticipated reminder groups by baseline sales.

APPENDIX

A Appendix Tables

Appendix Table A.1: Survey Balance: Full Sample vs Survey Sample

	Full sample (1)	Survey sample (2)	Difference (3)	P-value (4)
Panel A: Firm owner characteristics				
Owner sex female	0.441	0.444	0.003	0.908
	(0.497)	(0.497)	[0.023]	
Owner age	39.51	39.73	0.22	0.660
	(11.02)	(10.44)	[0.50]	
Panel B: Business characteristics				
Business type				
Beauty	0.087	0.066	-0.022	0.062*
	(0.282)	(0.248)	[0.012]	
Clothing	0.089	0.079	-0.011	0.388
	(0.285)	(0.269)	[0.012]	
Professionals	0.239	0.291	0.052	0.013**
	(0.426)	(0.455)	[0.021]	
Restaurants	0.123	0.110	-0.013	0.386
	(0.328)	(0.314)	[0.015]	
Small retailers	0.260	0.268	0.008	0.709
	(0.439)	(0.443)	[0.021]	
Other	0.202	0.187	-0.015	0.406
	(0.401)	(0.390)	[0.018]	
Pre-treatment sales variables				
Months since first transaction	24.18	23.80	-0.38	0.646
	(16.95)	(17.75)	[0.82]	
% months business made sales	0.818	0.826	0.008	0.452
	(0.227)	(0.220)	[0.010]	
Log average monthly sales volume	8.791	8.757	-0.034	0.498
	(1.112)	(1.077)	[0.050]	
Log average monthly transactions	2.056	2.066	0.011	0.865
	(1.422)	(1.379)	[0.064]	
Number of observations	33,978	471	34,449	34,449
F-stat of joint test			1.06	0.389

Note: This table reports differences in firm owner characteristics, business characteristics, and pre-treatment sales variables by sample. The unit of observation is at the firm level. Column (1) contains the full sample mean and standard deviation, column (2) the mean and standard deviation of the sample of firms surveyed from the full sample, column (3) the difference between columns (2) and (1) and standard error, and column (4) reports the associated p-value of the difference test. Columns (3) and (4) come from the regression of each baseline variable on Log average monthly sales volume and log average monthly transactions transform winsorized sales volume and transactions at the 95th percentile. Data is from 07/2019 to 08/2020 and includes all firms in experiment.

Appendix Table A.2: Survey Baseline Treatment Balance

	Intercept (1)	Anticipated reminder (2)	Unanticipated reminder (3)	Deadline (4)	Joint test F-stat (5)
Panel A: Firm owner characteristics					
Owner sex female	0.446***	-0.050	-0.022	0.059	0.756
	(0.079)	(0.082)	(0.082)	(0.046)	[0.519]
Owner age	40.52***	-1.24	-0.76	0.23	0.216
	(1.45)	(1.53)	(1.58)	(1.00)	[0.886]
Panel B: Business characteristics					
Business type					
Beauty	0.144***	-0.078	-0.067	-0.026	1.626
	(0.053)	(0.052)	(0.052)	(0.023)	[0.183]
Clothing	0.021	0.060*	0.067*	0.001	0.778
	(0.028)	(0.029)	(0.029)	(0.025)	[0.507]
Professionals	0.262***	0.015	0.053	-0.002	0.316
	(0.069)	(0.072)	(0.073)	(0.042)	[0.813]
Restaurants	0.090*	0.026	0.026	-0.005	0.104
	(0.043)	(0.047)	(0.047)	(0.029)	[0.958]
Small retailers	0.364***	-0.125	-0.100	0.011	1.039
	(0.074)	(0.077)	(0.078)	(0.041)	[0.375]
Other	0.119*	0.101*	0.022	0.020	1.916
	(0.054)	(0.058)	(0.056)	(0.036)	[0.126]
Pre-treatment sales variables					
Months since first transaction	20.85***	1.06	2.56	2.63	1.277
	(2.24)	(2.41)	(2.47)	(1.64)	[0.282]
% months business made sales	0.859***	-0.039	-0.037	0.004	0.440
	(0.028)	(0.030)	(0.031)	(0.020)	[0.725]
Log average monthly sales volume	8.654***	0.123	0.183	-0.067	0.533
	(0.164)	(0.168)	(0.170)	(0.100)	[0.660]
Log average monthly transactions	2.046***	-0.082	0.037	0.083	0.427
	(0.193)	(0.202)	(0.205)	(0.127)	[0.734]

Note: This table reports differences in firm owner characteristics, business characteristics, and pre-treatment sales variables by treatment group. The unit of observation is at the firm level. Columns (1)-(4) contain coefficients from the regression of each outcome on an intercept and dummies for anticipated reminder, unanticipated reminder and deadline treatment groups. Column (5) contains the F-statistic and corresponding p-value from a joint F-test of all coefficients in the regression. Log average monthly sales volume and log average monthly transactions transform winsorized sales volume and transactions at the 95th percentile. Data is from 07/2019 to 08/2020 and includes all firms in the survey sample ($N = 471$). Standard errors are in parentheses and p-values for the F-statistics are in square brackets.

Appendix Table A.3: Anticipated Reminder and Reminder Effect Timing

	Firm accepted offer	
	(1)	(2)
Reminder	-0.005	
	(0.005)	
Reminder × Post reminder	0.048***	
	(0.002)	
Anticipated reminder		0.009
		(0.006)
Anticipated reminder × Post reminder		0.010**
		(0.004)
Num. Obs.	202,616	130,032
Num. Firms	25,327	7,172
Cluster Std. Errors	Firm	Firm
Fixed Effects	Day	Day
Mean Control Take-Up on Day 6	0.244	0.234

Note: This table reports treatment effects of the reminder and anticipated 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 at the firm-day level. Column (1) uses data from 25,327 firms, excluding the Control and 24-hour 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 equal to 1 if the time period is after the firm received the reminder. Regressions include firm fixed effects. Clustered standard errors at the firm level are included in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A.4: Treatment Effect of Anticipated Reminder Concentrated Among Low Trust Firms

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

Note: This table reports heterogeneous treatment effects by survey measure. The unit of observation is at the firm level. The coefficients come from the regression of lower fee offer take-up on anticipated reminder, the survey measure,and the interaction between anticipated reminder and the survey measure. Data includes firms with anticipated and unanticipated reminders in survey sample, and includes take-up from September 29 to March 17. 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. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A.5: Heterogeneous Treatment Effects of Unanticipated Reminder by Survey Measures

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

Note: This table reports heterogeneous treatment effects by survey measure. The unit of observation is at the firm level. The coefficients come from the regression of lower fee offer take-up on unanticipated reminder, the survey measure,and the interaction between unanticipated reminder and the survey measure. Data includes firms with unanticipated reminders and no reminders in survey sample, and includes take-up from September 29 to March 17. 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. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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

	Firm accepted offer		
	(1)	(2)	(3)
Intercept	0.563***	0.476***	0.466***
	(0.040)	(0.110)	(0.066)
Above median % sales using technology	0.108*	0.190	0.134
	(0.055)	(0.175)	(0.086)
Reminder		0.101	
		(0.118)	
Above median % sales using technology		-0.096	
× Reminder		(0.185)	
Anticipated reminder			0.201**
			(0.087)
Above median % sales using technology			-0.039
× Anticipated reminder			(0.116)
Number of firms	306	306	273

Note: This table reports heterogeneous treatment effects of the reminder and anticipated reminder groups and percentage of sales using the technology on take-up. The unit of observation is at the firm level. The coefficients come from the regression of lower fee offer take-up on treatment, a dummy indicating above-median percentage sales using the technology, and the interaction between treatment and the above-median percentage sales variable. Above median percentage sales using the technology is defined as 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 to March 17. The question *What share of your total pesos of sales did you make through the technology in the past week?* was asked to all users that reported they obtained income using the technology in the past week ($N = 306$). 16 firms were excluded from the sample, including 1 firm from the Control group, 10 firms that did not know the answer to the question, and 5 firms that did not answer the question. Additionally, firms that responded that they had obtained 0 income using the technology were coded as conducting 0% of their sales through the technology.

Columns (1) and (2) include all firms that provided an answer to the percentage sales question, and column (3) includes only firms that received a reminder. Robust standard errors are included in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A.7: Heterogeneous Treatment Effects of Reminder by Firm Baseline Characteristics

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

Note: This table reports heterogeneous treatment effects of reminder by firm baseline characteristics. The unit of observation is at the firm level. The coefficients come from the regression of lower fee offer take-up on reminder, the heterogeneity variable and the interaction between reminder and heterogeneity variable. Above median owner age is defined as firms with owner age $\geq$ median owner age (37.23). Change in sales is defined as the percentage change in sales between August and September 2020, and was winsorized at the 95th percentile. Above median change in sales as change in sales $\geq$ median change in sales (-25%). 49% of the sample is male. Data includes take-up from September 29 to March 17, excluding the Control and 24-hour deadline, no reminder groups from the full sample of 33,978 firms. Column (1) includes all firms for which we can identify owner age, column (2) includes all firms for which we can identify owner sex, and column (3) includes all firms in the experiment. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A.8: Heterogeneous Treatment Effects of Deadline by Firm Baseline Characteristics

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

Note: This table reports heterogeneous treatment effects of deadline by firm baseline characteristics. The unit of observation is at the firm level. The coefficients come from the regression of lower fee offer take-up on deadline, the heterogeneity variable and the interaction between deadline and heterogeneity variable. Above median owner age is defined as firms with owner age $\geq$ median owner age (37.23). Change in sales is defined as the percentage change in sales between August and September 2020, and was winsorized at the 95th percentile. Above median change in sales as change in sales $\geq$ median change in sales (-25%). 49% of the sample is male. Data includes take-up from September 29 to March 17, excluding the Control and 24-hour deadline, no reminder groups from the full sample of 33,978 firms. Column (1) includes all firms for which we can identify owner age, column (2) includes all firms for which we can identify owner sex, and column (3) includes all firms in the experiment. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A.9: Heterogeneous Treatment Effects of Anticipated Reminder by Firm Baseline Characteristics

	Firm accepted offer		
	(1)	(2)	(3)
Anticipated reminder	0.012	0.012	0.024**
	(0.011)	(0.011)	(0.010)
Above median owner age	-0.009		
	(0.010)		
Owner sex female		-0.030***	
		(0.010)	
Above median baseline change in sales			0.042***
			(0.010)
Above median owner age	0.020		
× Anticipated reminder	(0.015)		
Owner sex female		0.021	
× Anticipated reminder		(0.015)	
Above median baseline change in sales			-0.008
× Anticipated reminder			(0.015)
Number of firms	15,138	15,141	16,254

Note: This table reports heterogeneous treatment effects of anticipated reminder by firm baseline characteristics. The unit of observation is at the firm level. The coefficients come from the regression of lower fee offer take-up on anticipated reminder, the heterogeneity variable and the interaction between anticipated reminder and heterogeneity variable. Above median owner age is defined as firms with owner age $\geq$ median owner age (37.23). Change in sales is defined as the percentage change in sales between August and September 2020, and was winsorized at the 95th percentile. Above median change in sales as change in sales $\geq$ median change in sales (-25%). 49% of the sample is male. Data includes take-up from September 29 to March 17, from the anticipated and unanticipated reminder groups. Column (1) includes all firms for which we can identify owner age, column (2) includes all firms for which we can identify owner sex, and column (3) includes all firms in the experiment. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A.10: Heterogeneous Treatment Effects by Firm Business Type

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

Note: This table reports heterogeneous treatment effects by firm business type. The unit of observation is at the firm level. The coefficients come from the regression of lower fee offer take-up on treatment, business type dummies and the interaction between treatment and business type dummies. The omited category is ”Other” business type. Data includes take-up from September 29 to March 17. Columns (1) and (2) exclude the Control and 24-hour deadline, no reminder groups from the full sample of 33,978 firms. Column (3) keeps only firms from the anticipated and unanticipated reminder groups. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Table A.11: Heterogeneous Treatment Effects by Owner Receiving Emails

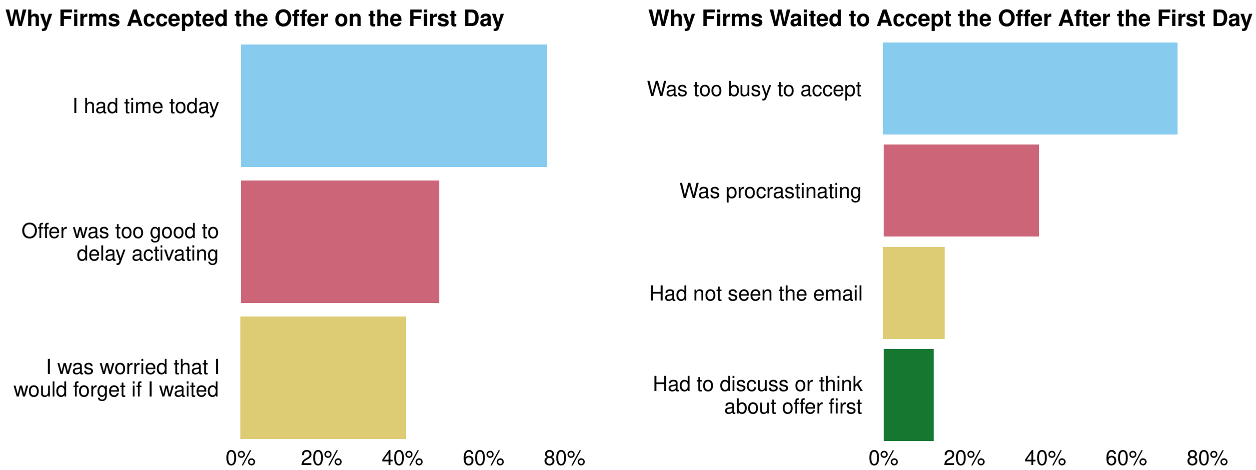
	Firm accepted offer		
	(1)	(2)	(3)
Intercept	0.679***	0.556***	0.545***
	(0.064)	(0.166)	(0.107)
Owner was recipient of emails	-0.074	-0.096	-0.007
	(0.069)	(0.186)	(0.113)
Reminder		0.149	
		(0.180)	
Owner was recipient of emails		0.011	
$\times$ Reminder		(0.200)	
Anticipated reminder			0.318**
			(0.130)
Owner was recipient of emails			-0.163
$\times$ Anticipated reminder			(0.139)
Number of firms	471	471	425

Note: This table reports heterogeneous treatment effects by owner receiving emails. The unit of observation is at the firm level. The coefficients come from the regression of lower fee offer take-up on treatment, a dummy indicating that the firm owner receives the FinTech's emails, and the interaction between treatment and the owner receiving emails variable. In 88.7 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 17. The question *Is the account owner the person that receives emails from iZettle?* was asked to all users that took part in the survey.

Columns (1) and (2) include all firms in the survey, and column (3) includes only firms that receved a reminder. Robust standard errors are included in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

B Appendix Figures

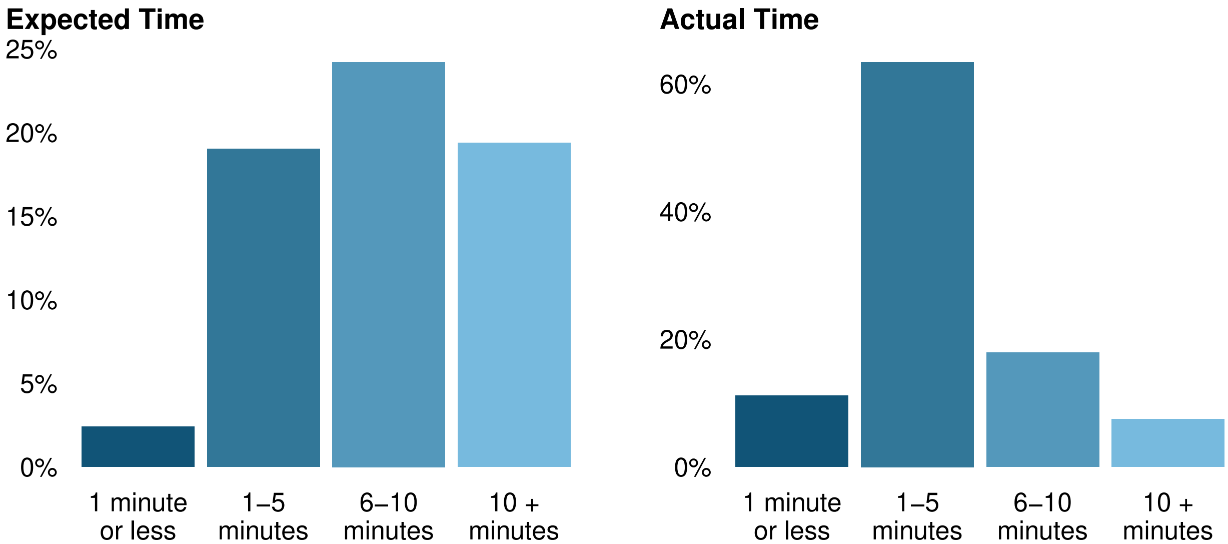
Appendix Figure B.1: Why Firms Accepted the Offer on the First Day or Delayed Activating



Note: The left panel contains a barplot with reasons given by firms for accepting the offer on the first day. Data comes from survey conducted on a random sample of firms in the experiment ($N = 471$), with 52 firms asked this particular question. This question was asked to firms that recall receiving the first email or SMS, received a deadline, and accepted on the first day of the offer. 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?* Firms could provide more than one response, so totals add up to more than 100%. 3 firms were excluded from the sample, including 1 firm giving other responses, and 2 firms that did not know the answer to the question.

The right panel contains a barplot with reasons given by firms for not accepting the offer on the first day. Data comes from survey conducted on a random sample of firms in the experiment ($N = 471$), with 83 firms asked this particular question. This question was asked to firms that recall receiving the first email or SMS, with a deadline, who accepted after the first day of the offer, and recalled accepting or clicking on the offer. 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?* Firms could provide more than one reason for not accepting the offer on the first day, so totals add up to more than 100%. 10 firms were excluded from the sample, including 4 firms giving other responses, 1 firm that did not know the answer to the question, and 5 firm that did not answer the question.

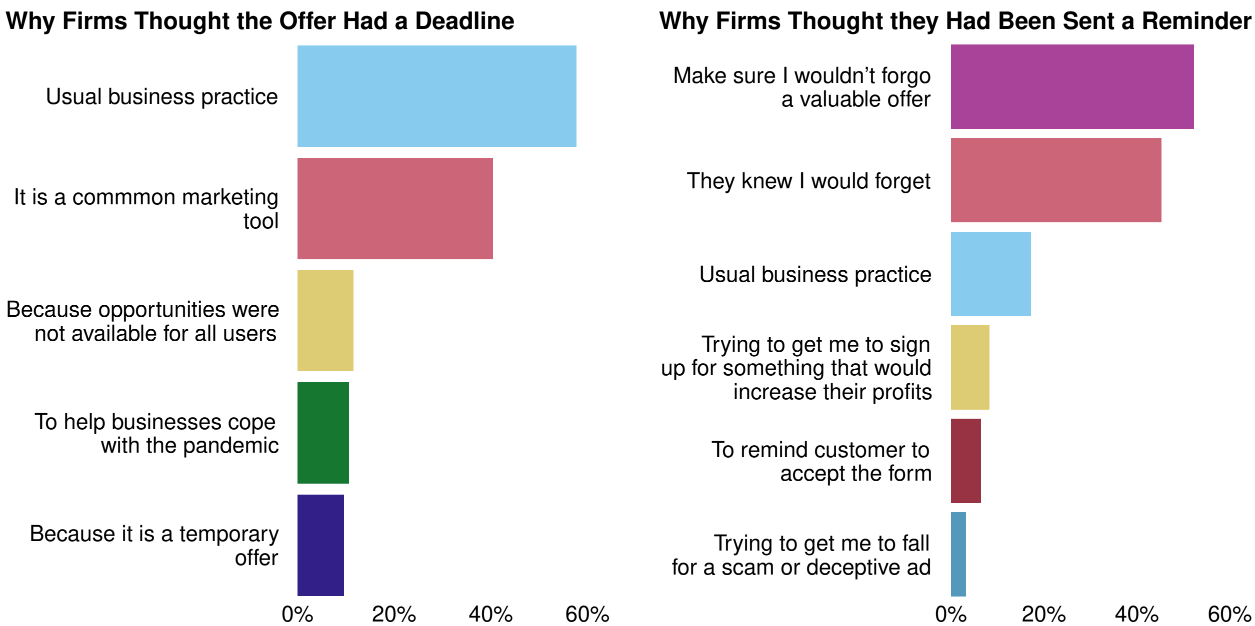
Appendix Figure B.2: Self-Reported Time to Accept Offer



Note: The left panel contains a barplot with the time firms expected it took them to fill out the offer. Data comes from survey conducted on a random sample of firms in the experiment ($N = 471$), with 289 firms asked this particular question. This question was asked to users who recall receiving the first email or SMS. Survey question: *How long did you expect completing the form to activate the lower fee would take you?* This figure combines two versions of the survey question asked to separate groups of firms. In the first version, the third category respondents can select is "5+ minutes". In the second version, the third category respondents can select is "1-5 minutes", and there is a fourth category "10+ minutes".We combined "1-5 minutes" and "10+ minutes" into "5+ minutes"and merged both versions of the question. 101 firms were excluded from the sample, including 69 firms that did not know the answer to the question, and 32 firms that did not answer to the question.

The right panel contains a barplot with the time firms report it took them to fill out the offer. Data comes from survey conducted on a random sample of firms in the experiment ($N = 471$), with 186 firms asked this particular question. This question was asked to firms that recall receiving the first email or SMS, accepted the survey, and recalled accepting or clicking on the offer. Survey question: *How long did it take you to fill out the offer?* This figure omits responses to a previous version of the question which categorized responses in three categories: 1 minute or less, betwen 1 and 5 minutes, and 5+ minutes. 52 firms were excluded from the sample, including 23 firms that did not know the answer to the question, and 29 firms that did not answer to the question.

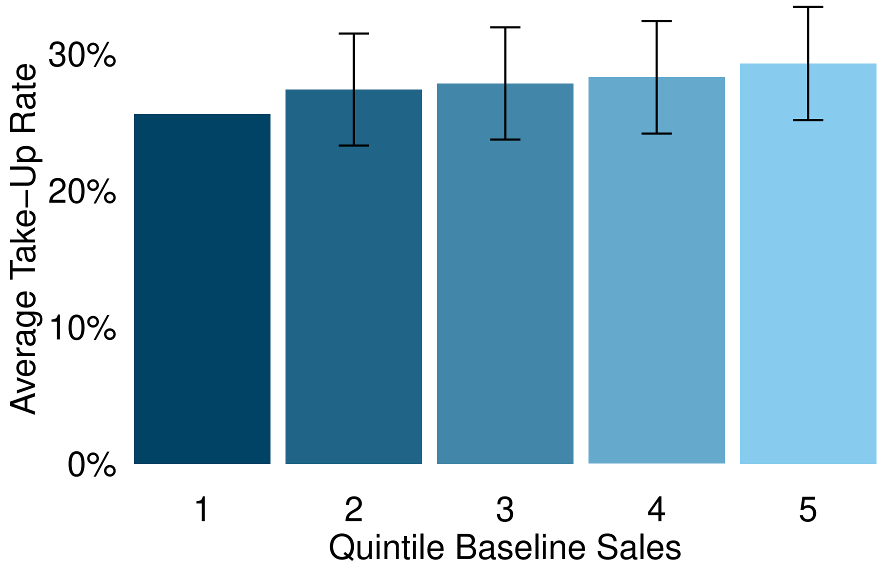
Appendix Figure B.3: Why Firms Thought the Offer Had a Deadline and Reminder



Note: The left panel contains a barplot showing why firms thought the offer had a deadline. Data comes from survey conducted on a random sample of firms in the experiment ($N = 471$), with 130 firms asked this particular question. This question was asked to firms that recall receiving the first email or SMS, received a deadline, and noticed the offer had a deadline. Survey question: *Why do you think the offer had a deadline?* Firms could provide more than one response, so totals add up to more than 100%. 26 firms were excluded from the sample, including 7 firms giving other responses, 18 firms that did not know the answer to the question, and 1 firm that did not answer the question.

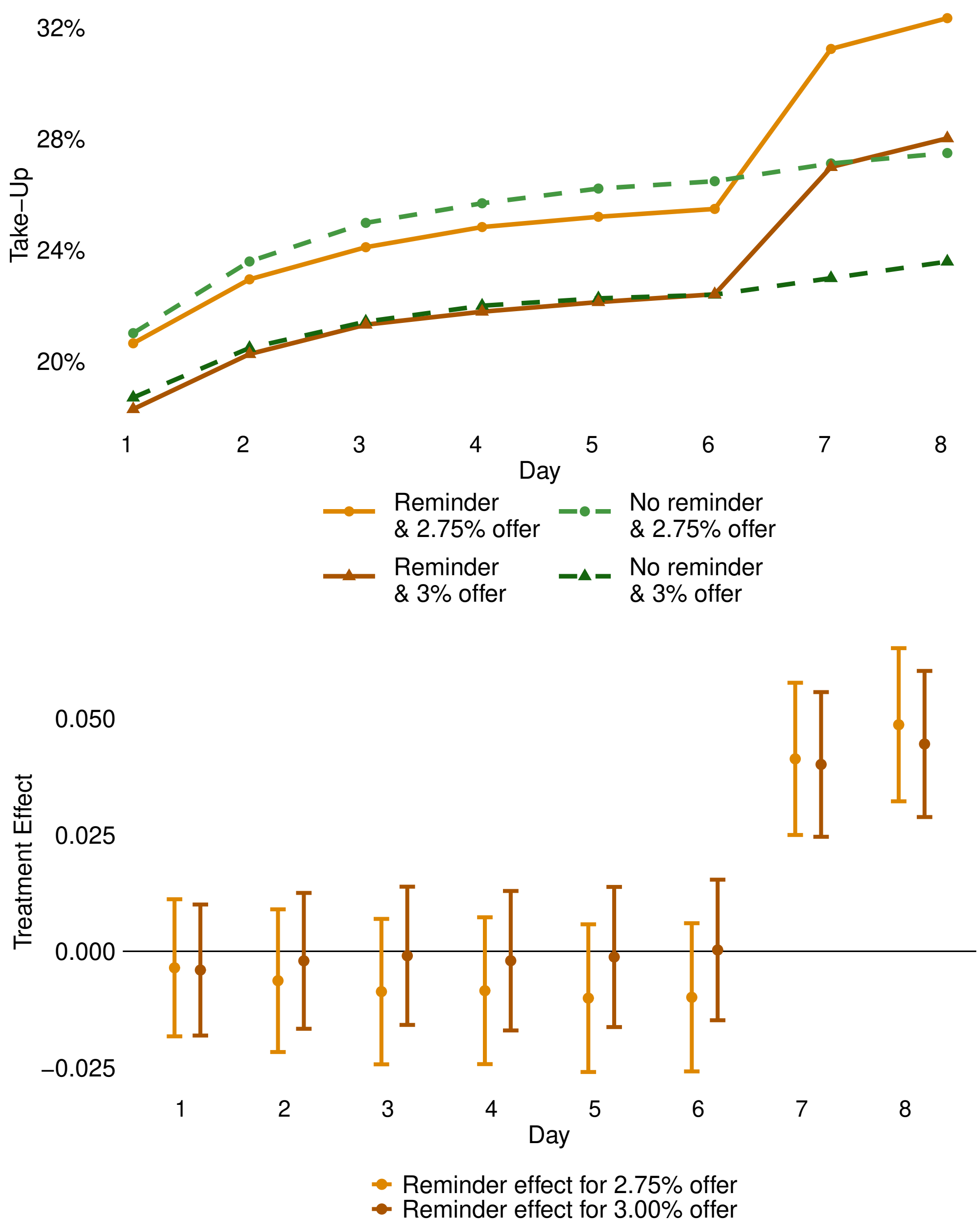
The right panel contains a barplot showing why firms thought they had been sent a reminder. Data comes from survey conducted on a random sample of firms in the experiment ($N = 471$), with 157 firms asked this particular question. This question was asked to firms that recall receiving the first email 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. Survey question: *Why do you think we sent you a reminder?* Firms could provide more than one response, so totals add up to more than 100%. 12 firms were excluded from the sample, including 7 firms giving other responses, and 5 firms that did not know the answer to the question.

Appendix Figure B.4: Take-Up by Baseline Sales Quintiles for No Deadline, No Reminder Group



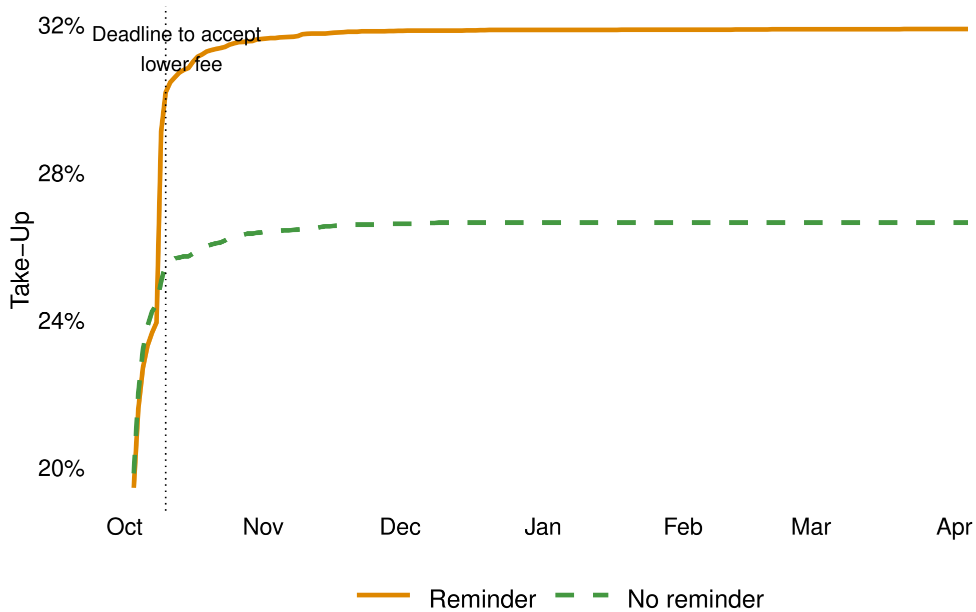
Note: This figure contains a bar plot with the average take-up rate by quintile of baseline sales for firms from the no deadline, no reminder group ($N = 4,455$). Data includes take-up from September 29 to March 17. Baseline sales is defined as the winsorized average monthly sales volume from from Sepember 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 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 ($p = 0.080$).

Appendix Figure B.5: Effect of Reminder on Take-Up by Offer Value



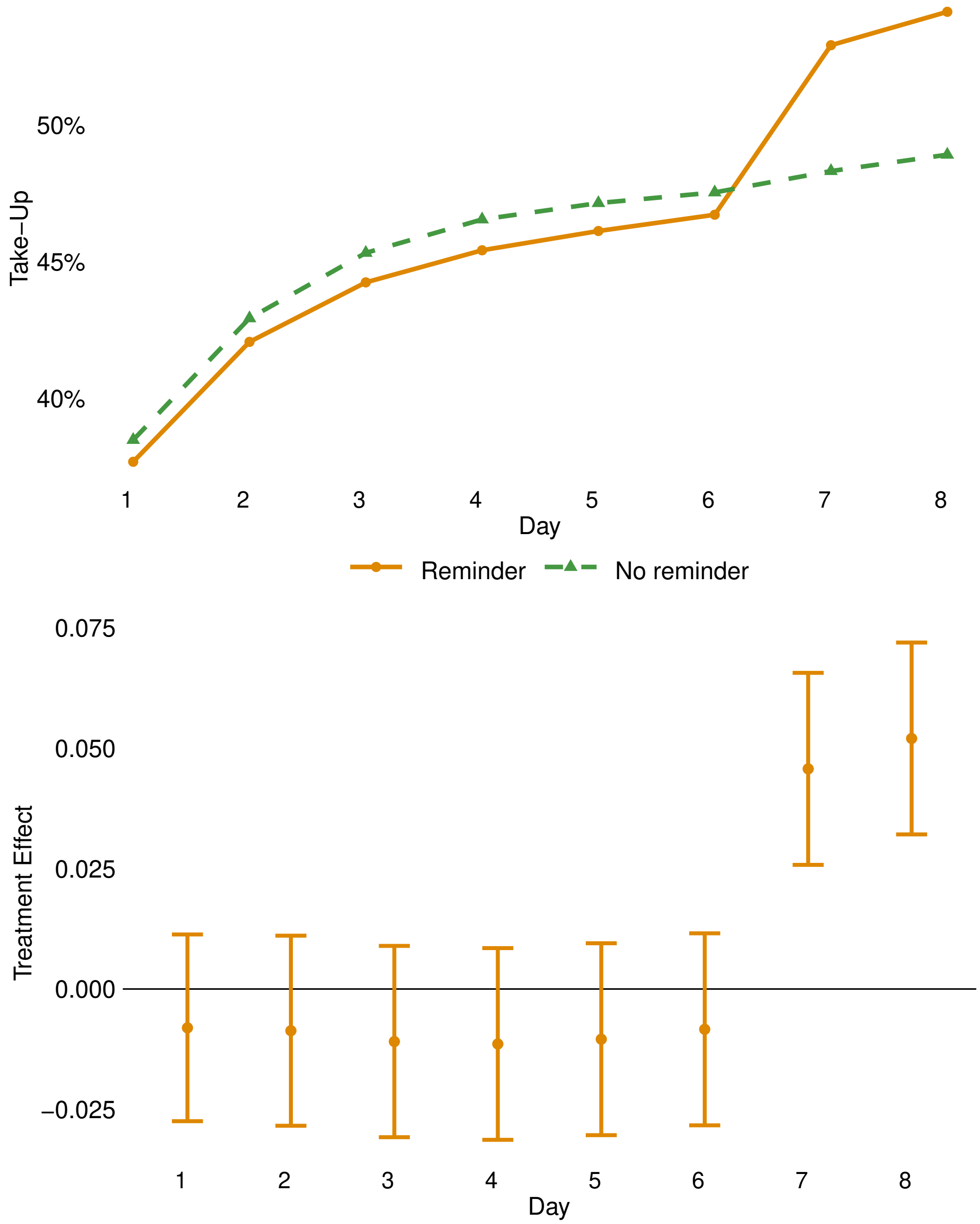
Note: This figure shows short-term take-up and coefficient estimates for reminder and no reminder groups by the lower fee offered.

Appendix Figure B.6: Reminder and No Reminder Take-up Beyond Deadline



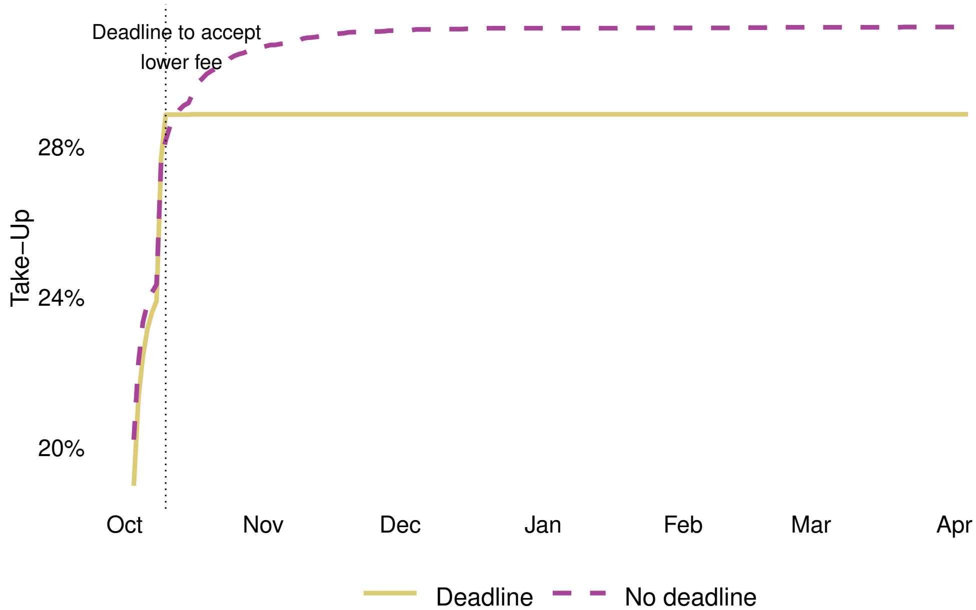
Note: This figure shows long-term take-up of reminder and no reminder groups, conditional on businesses opening the first email with the lower fee offer. Last date is last take-up of the offer by a business.

Appendix Figure B.7: Effect of Reminder on Take-Up Conditional on Opening Email



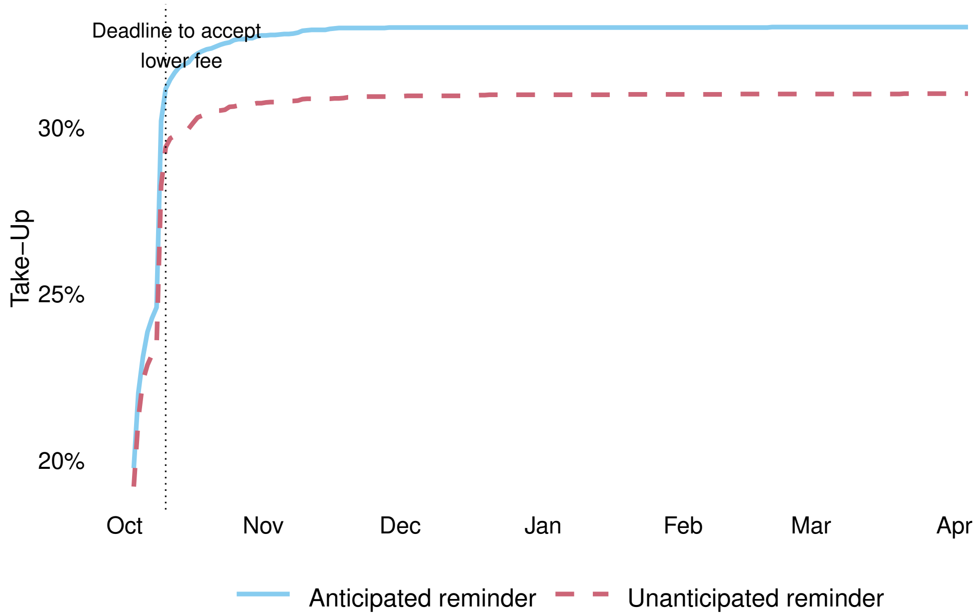
Note: This figure shows short-term take-up and coefficient estimates for reminder and no reminder groups, conditional on businesses opening the first email with the lower fee offer.

Appendix Figure B.8: Deadline and No Deadline Take-up Beyond Deadline



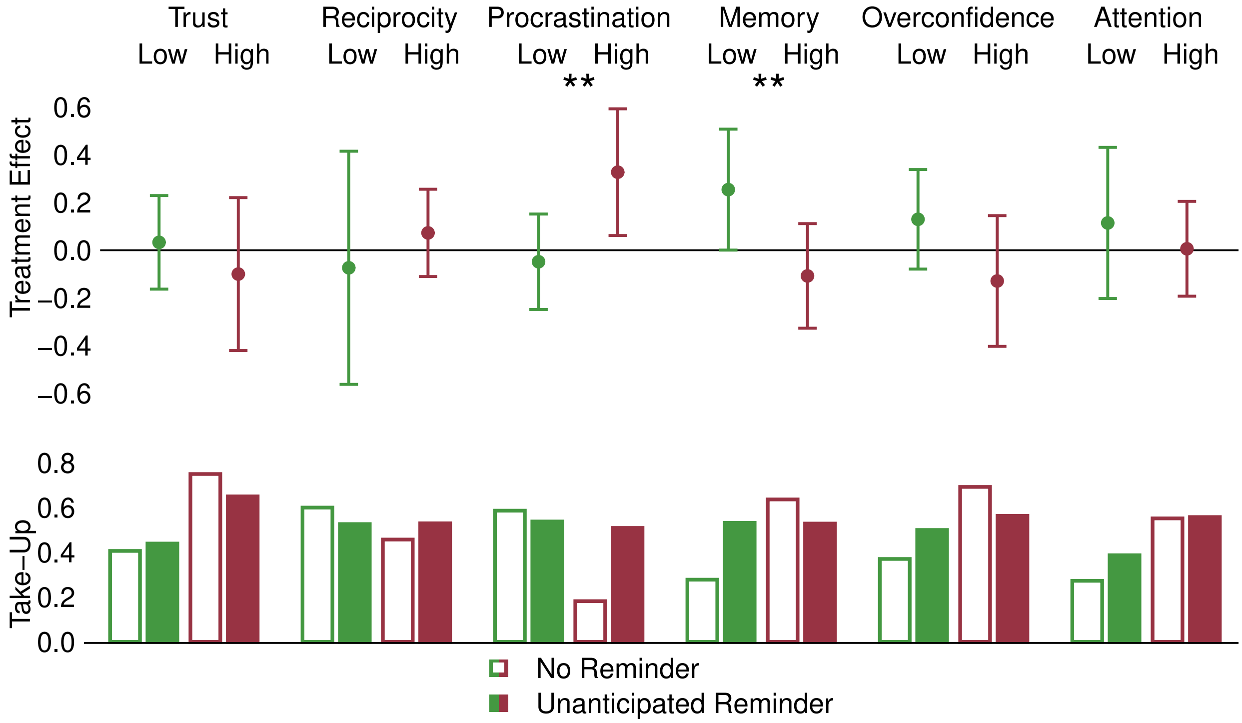
Note: This figure shows long-term take-up of deadline and no deadline groups, conditional on businesses opening the first email with the lower fee offer. Last date is last take-up of the offer by a business.

Appendix Figure B.9: Anticipated and Unanticipated Reminder Take-up Beyond Deadline



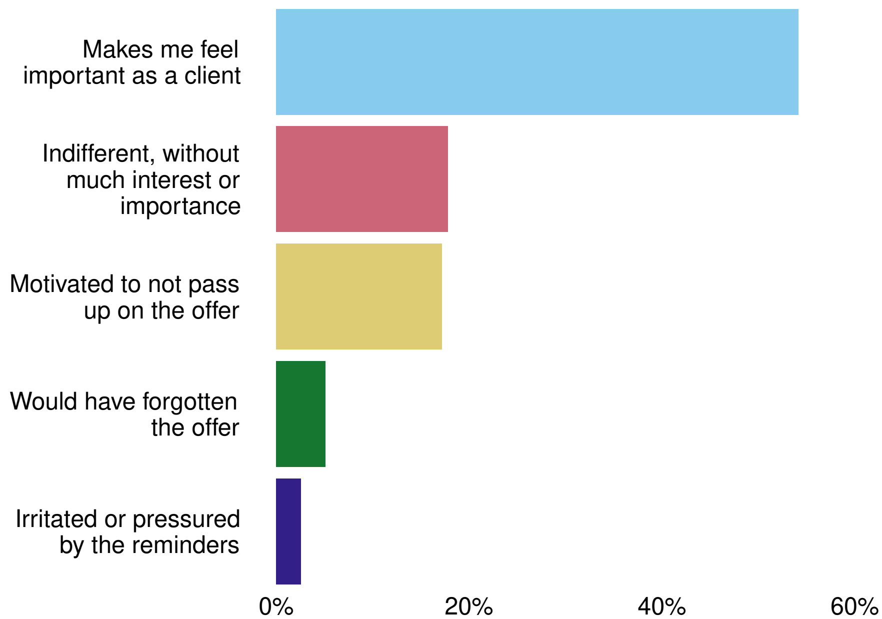
Note: This figure shows long-term take-up of anticipated and unanticipated reminder groups, conditional on businesses opening the first email with the lower fee offer. Last date is last take-up of the offer by a business.

Appendix Figure B.10: Heterogeneous Effect of Unanticipated Reminder by Survey Measures



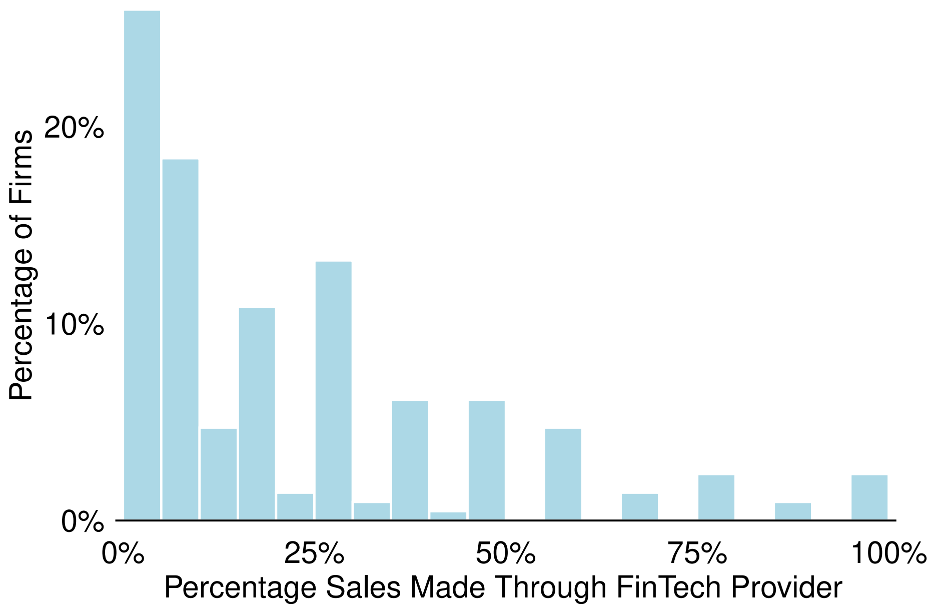
Note: This table reports heterogeneous treatment effects by survey measure. The unit of observation is at the firm level. The coefficients come from the regression of lower fee offer take-up on unanticipated reminder, the survey measure,and the interaction between unanticipated reminder and the survey measure. Data includes firms with unanticipated reminders and no reminders in survey sample, and includes take-up from September 29 to March 17. 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. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix Figure B.11: How Firms Felt About Receiving Reminder



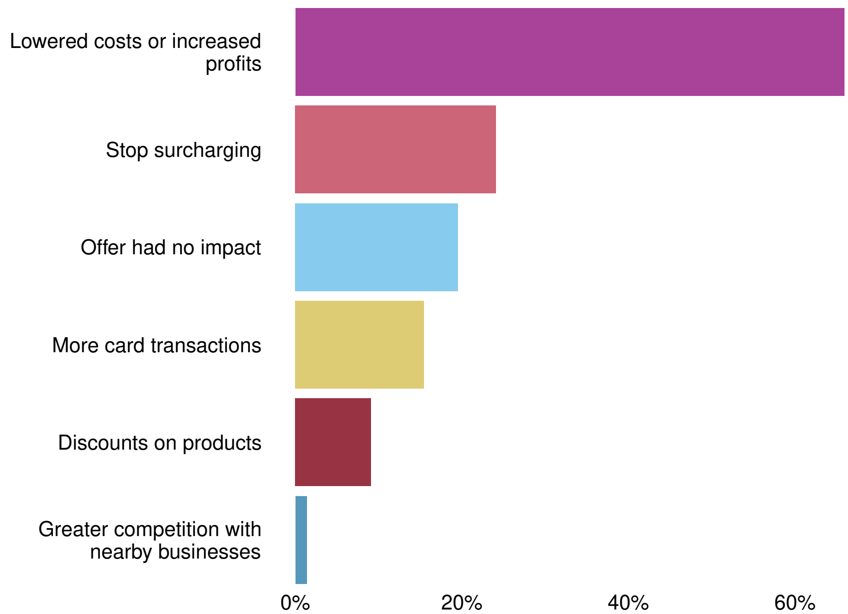
Note: This figure contains a barplot showing how firms felt when receiving the reminder. Data comes from survey conducted on a random sample of firms in the experiment (N = 471), with 157 firms asked this particular question. This question was asked to firms that recall receiving the first email or SMS, recall receiving a reminder, received an offer with a reminder, and accepted the offer after receiving the reminder or did not accept the offer. Survey question: *How did receiving the reminder make you feel?* Firms 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.

Appendix Figure B.12: Percent of Sales Made Through FinTech Provider Last Week



Note: This figure contains a histogram of the percentage of weekly sales made through the FinTech provider. Data comes from survey conducted on a random sample of firms in the experiment (N = 471) with 227 firms asked what their previous fee was. Survey question: *What share of your total pesos of sales did you make through (provider) in the past week?* 15 firms were excluded from the sample, including 10 firms that did not know the answer to the question, and 5 firms that did not answer the question. Percentage sales mean = 24.9, median = 20, standard deviation = 23.7.

Appendix Figure B.13: Impact of Lower Fee Offer



Note: This 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 particular 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?* Firms 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.