Why Small Firms Fail to Adopt Profitable Opportunities*

Paul Gertler Sean Higgins Ulrike Malmendier Waldo Ojeda

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Abstract

Why do small firms often fail to adopt new profitable opportunities? We explore a setting in which standard economic frictions are removed but many firms still do not adopt a profitable opportunity, and study three other frictions: limited memory, present bias, and a lack of 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 total firm profits. We randomly vary the size of the fee reduction, a deadline, a reminder, and advance notice of the reminder. While deadlines do not affect take-up, reminders increase take-up by 15%, and announced reminders by an additional 7%. Survey data help identify trust as the likely underlying mechanism behind the larger effect of the announced reminder: receiving an announced reminder increases firms' perceptions of the offer's value, and its treatment effect on take-up is stronger among firms that 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, EFA, European Winter Finance Summit, Golub Capital Social Impact Lab, 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, MWIEDC, NBER Corporate Finance Fall Meeting, NBER Organizational Economics Spring Meeting, NCDE, NFA, Northwestern University, Peking University, 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. The authors declare that they have no financial or material interests in the results of this research. IRB approvals: UC Berkeley IRB 2018-02-10796 and 2020-03-13091. AEA RCT Registry: AEARCTR-0006540.

1 Introduction

Firms often fail to adopt profitable business opportunities. This occurs across many industries —including manufacturing, banking, retail, and healthcare—and across various types of opportunities—including cost-saving technologies, financial technologies, management practices, and optimal pricing.[1] Firms of all sizes forgo substantial profits by failing to adopt profitable opportunities. Small and medium enterprises in Bruhn, Karlan, and Schoar (2018) were forgoing a 28% increase in productivity on average, medium and large firms in Bloom et al. (2013) were forgoing a 17% increase in productivity, large retail chains in DellaVigna and Gentzkow (2019) were forgoing $16M in annual profits, and Lyft was forgoing $160M in annual profits (List, Muir, Pope, and Sun, 2023).

Several factors contribute to firms' failure to adopt profitable opportunities, including lack of information or managerial capital (Bloom et al., 2013; Bruhn, Karlan, and Schoar, 2018; Giorcelli, 2019), fixed costs in the presence of uncertainty or liquidity constraints (Abel and Eberly, 1994; Banerjee, Breza, Duflo, and Kinnan, 2021), labor constraints (Jagannathan, Matsa, Meier, and Tarhan, 2016; Hardy and McCasland, 2023), and principal-agent problems 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) find that "even if the owners became convinced of the need to adopt a practice, they would often take several months to do so." Furthermore, DellaVigna and Gentzkow (2019) and Mishra, Prabhala, and Rajan (2022) 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: limited memory, present bias, and distrust. All three of these determinants have been shown to explain inertia and lack of behavioral change in *non-managerial* individual-level decision-making. First, limited memory affects health-related choices such as adherence to healthcare appointments and vaccine take-up (Gurol-Urganci et al., 2013; Dai et al., 2021; Calzolari and Nardatto, 2016) and financial choices, such as saving and loan repayment behavior (Karlan, McConnell, Mullainathan, and Zinman, 2016; Karlan, Morten, and Zinman, 2016). Second, present bias similarly affects health-related choices such as gym attendance and smoking cessation (Della Vigna and Malmendier, 2006; Giné, Karlan, and Zinman, 2010) and financial choices such as saving, borrowing, and loan

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

repayment (Laibson, 1997; DellaVigna and Malmendier, 2004; Ashraf, Karlan, and Yin, 2006; Kuchler and Pagel, 2021). Third, distrust has been shown to interfere with the very same financial decisions, such as saving, borrowing, and refinancing (Karlan, Mobius, Rosenblat, and Szeidl, 2009; Johnson, Meier, and Toubia, 2019; Bachas, Gertler, Higgins, and Seira, 2021). Our main contribution is to ask whether these determinants also explain individuals' profit-reducing *managerial* behavior within small firms.

In partnership with a financial technology (FinTech) payments provider in Mexico, we conducted a randomized controlled trial (RCT) where the FinTech offered 33,978 firms that were 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 are relatively small: the median number of employees is 3 and the largest firm has 150 employees. Nevertheless, the distribution of number of employees is similar to that of 99.7% of firms in Mexico. By adopting a lower merchant fee, these firms would reduce their costs and hence increase their profits. For the median firm, the expected cost savings from the reduced fee equal 3% of profits.

To examine the effect of these three barriers, our RCT randomly varied (i) the amount of the lower fee (2.75%, 3%, or a control group that retained their current fee of 3.5–3.75%), (ii) a deadline to accept the offer, (iii) a reminder, and (iv) whether the FinTech company told the firms in advance that they would receive a reminder (which we refer to as an "announced reminder"). The design allows us to test for the three proposed mechanisms—limited memory, present bias, and lack of trust in other firms—as well as potentially distorted beliefs about memory and present bias.

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; thus, the firm owner procrastinates thinking they will adopt tomorrow, but when tomorrow arrives their present bias causes them to procrastinate again.[2] Deadlines can help overcome present bias because at the deadline period the firm owner cannot delay adopting the profitable opportunity any longer as it will expire. However, firm owners can also have limited memory and forget about the profitable opportunity. Reminders can help overcome limited memory. Announced reminders—when firm owners are told in advance that they will receive a reminder—can increase their expectations about remembering the offer. Thus, announced reminders can decrease initial take-up (before the reminder is sent), as firm owners know they will remember the chance to do so when the reminder arrives. On the other hand, if firm owners are overconfident about memory and think they will remember even without a reminder, the

[2]We refer to firm owners throughout the paper since the firm owner was the recipient of our messages in 88.7% of cases. In most of these small firms, the firm owner is also the manager.

announced reminder would not decrease initial take-up. After the reminder is sent, theory suggests that cumulative take-up by firms that received an announced reminder should be no higher than cumulative take-up by firms that received an unannounced reminder *unless* the announced 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 firm owners are forgetful: unannounced reminders cause a large and significant increase in adoption of the lower merchant fee. By the eighth day of our study, unannounced reminders increase adoption of the lower merchant fee by 15%. Firm owners who received an unannounced reminder are 3.9 percentage points (pp) more likely to take up the offer compared to firm owners that did not receive a reminder, on a base of 25.5% take-up. The higher overall take-up 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 by the deadline. 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.

Firm owners who received an announced reminder had the highest overall take-up. When we sent the initial offers, the only difference between the announced and unannounced reminder groups was that in the announced reminder group, the initial email informed them that they would receive a reminder and on what date they would receive it. The reminder message was the same for both groups. On the first day (when we sent the initial email), there is no difference in take-up between the announced- and unannounced-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 reminder treatment) but also overconfident about memory. The day that we sent the reminder, announced reminders increased take-up of the profitable opportunity by 2 pp compared to unannounced reminders, and the difference remains significant throughout the remainder of the experiment.

This result cannot be explained by a model where announced reminders only impact the probability (or perceived probability) of remembering. Instead, to cause higher take-up, the announced reminder must increase the perceived value of accepting the offer, for example by increasing trust in the offer.

We conducted a survey of a subsample of firms in our RCT to better understand mechanisms behind the effect of the announced reminder relative to the unannounced reminder on take-up. We find that, compared to firms that received an unannounced reminder, firms that received an announced reminder are 16.1 pp more likely to state that the reminder changed their perception of the offer's value (39.2% relative to a base of 23.1% in the unannounced reminder group). We also find evidence that the treatment effect of the announced reminder relative to the unannounced reminder is concentrated among firm owners who generally distrust advertised offers. We find similar evidence in the administrative data using the number of months the firm has used the FinTech payments technology as a proxy for their trust in the FinTech firm. These results suggest that the announced reminder 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 announced reminder (e. g., checking the offer's profitability in preparation for the reminder) do not explain why the announced reminder group had a higher take-up rate. The result on trust could have broad implications for firms' adoption of various profitable opportunities, as these opportunities often require firm-to-firm interactions where a lack of trust may be an important barrier.

We conclude that non-standard (behavioral) mechanisms are significant determinants of managerial decision-making within small firms, above and beyond the standard economic frictions analyzed in prior literature. While there is substantial evidence about 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 cited above. We show that limited memory also affects *managerial decisions within firms* and prevents some firms from adopting a profitable opportunity. Overconfidence also affects decision-making in a number of domains (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). Theoretically, 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). Evidence on the effectiveness of commitment devices, which aim to help sophisticated present-biased individuals, is also mixed (Bryan, Karlan, and Nelson, 2010).

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 cited above on *other barriers* that firms face; 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. 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.

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

Finally, we find that trust in other firms is an important friction. Evidence on the role of trust in *interfirm* relationships is limited and mostly descriptive. 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. Bloom, Sadun, and Van Reenen (2012) document that higher trust within multinational firms increases decentralization and raises aggregate productivity. Cai and Szeidl (2018) find that a lack of trust is a barrier to creating business partnerships, and randomizing regular meetings between firms increases trust. Alfaro-Ureña, Manelici, and Vasquez (2022) find that local supplier firms cite gaining the trust of multinational corporations as an import precursor to exporting. We find that when a firm informs other firms that they will take an action and then follows through on that action, this increases trust and adoption of a profitable opportunity.

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 present bias and memory. The model allows us to derive predictions about the effects and interactions of these potential barriers to the adoption of profitable opportunities.

2.1 Model Assumptions

In the model, a firm owner makes a decision to adopt a profitable opportunity that is beneficial in the future but has an immediate cost. The firm owner has potential present bias around future firm profits preferences and possibly naïveté: $U = \pi_0 + \beta\left(\sum_{t=1}^{\infty} \delta^t \pi_t\right)$, where δ is the discount factor and β is the present-bias parameter. The firm owner has beliefs $\hat{\beta} \in [0,1]$, and is naïve if $\hat{\beta} > \beta$.

The model also incorporates imperfect memory. There is a probability of remembering the opportunity in period t conditional on remembering it in period $t-1$, measured by the parameter ρ_t (with $\rho_0 = 1$). Firm owners are only able to adopt the opportunity if they remember it. Firm owners have beliefs $\hat{\rho}_t \in [0,1]$, and are overconfident about their memory if $\hat{\rho}_t > \rho_t$.

In each period t, the firm owner 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 adopting the offer 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 firm owner decides to adopt based on the current value function:

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

where $E_t[\hat{V}_{t+1}]$ is the perceived continuation value of not adopting in the current period (and potentially adopting in a future period). At the deadline, the continuation value is zero as the opportunity to adopt 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 *perceived* present bias, and hence a function of (potential) naïveté and thus $\hat{\beta}$.

2.2 Equilibrium Behavior

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

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

$$E_t\left[\hat{V}_{t+1}\right] = F\left(\hat{c}_{t+1}^*\right)\left[\delta\alpha_{t+1}y - \int_0^{\hat{c}_{t+1}^*} c\,dF(c)\right] + \left(1 - F\left(\hat{c}_{t+1}^*\right)\right)\delta\hat{\rho}_{t+2}E_{t+1}\left[\hat{V}_{t+2}\right], \tag{2}$$

where $\int_0^{\hat{c}_{t+1}^*} c\,dF(c)$ is the expected cost draw conditional on acting next period. 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^*:

$$\Pr(\text{adopt at } t) = \underbrace{\prod_{j=1}^{t}\rho_j}_{\Pr(\text{remember})} \underbrace{\prod_{k=0}^{t-1}\left(1 - F\left(c_k^*\right)\right)}_{\Pr(\text{not adopted before } t)} F(c_t^*). \tag{3}$$

2.3 Model Predictions

The model generates several testable predictions, which we will take to the data. The proofs of these model predictions are in Appendix B.

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

Prediction 2 (Reminder). A reminder increases take-up if firm owners are forgetful ($\rho_t < 1$).

Prediction 3 (Deadline). (a) A deadline increases take-up if firm owners 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 firm owners believe they have limited memory ($\hat{\rho}_t < 1$).

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 announced versus unannounced reminder. We 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 (Announced Reminder and Initial Pre-Reminder Take-Up). The announced reminder (a) reduces take-up at $t = 1$, compared to the unannounced reminder, if firm owners do not believe they have perfect memory ($\hat{\rho}_t < 1$), and (b) has no differential effect on take-up at $t = 1$ if firm owners believe they have perfect memory ($\hat{\rho}_t = 1$).

The reason for the predicted first-day effects is that the announced 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 announced 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 announced reminder will not have an effect as it will not impact the belief about memory.

Prediction 5. (Announced Reminder and Final Take-Up). The announced reminder (a) does not affect final (cumulative) take-up, compared to the unannounced reminder, if firms inherently trust the offer ($\alpha_t = 1$); and (b) increases final (cumulative) take-up if some firms distrust the offer, and their trust in the offer increases after receiving the announced reminder.

3 Institutional Context and Experimental Setting

We partnered with a FinTech payments company in Mexico to study the effects of limited memory, present bias, and a lack of trust in other firms, plus beliefs about memory and present bias, on small firms' adoption of a profitable opportunity. The FinTech company provides its clients, which are small firms, with point-of-sale (POS) hardware and an app to accept debit and credit card payments, similar to Square in the US. For each electronic card payment that the small firms process, the FinTech company charges a merchant fee that is a percentage of the payment amount. The merchant fee rate does not vary depending on the card network used (e.g. American Express, MasterCard, or Visa). Relative to POS terminals offered by banks, the FinTech partner's POS terminal is less expensive to acquire and does not include a monthly fee, but the FinTech partner charges a higher merchant fee than banks. Specifically, the FinTech partner charges 199 pesos to purchase the POS terminal, no monthly fee, and a 3.5–3.75% merchant fee, while Mexico's largest bank charges a 300 pesos "sign-up fee" plus a 359 pesos per month rental fee for a POS terminal, but a lower 2.15% merchant fee.

FinTech and cashless payments have expanded substantially in Mexico in recent years. A government program that rolled out debit cards to poor households in 2009–2012 led to an increase in small retail firms' adoption of POS terminals (Higgins, 2022). Mexico passed a FinTech Law in 2018 and by 2020 had 441 FinTech startups (Finnovista, 2023) and over 50 million users of FinTech payment products.[3] FinTech companies offering POS terminals entered the market in

[3] See https://www.debate.com.mx/economia/Pagos-digitales-en-Mexico-estadisticas-y-tendencias-del-mercado-20230429-0095.html.

2013, and our FinTech partner estimates that in 2019, of the 3.3 million total POS terminals in use in Mexico, 1.3 million were issued by FinTech payments companies, while another 1.3 million were issued by banks and 0.6 million were issued by other issuers. Our partner had a 10% market share among FinTech-issued POS terminals, and a 4% market share among all POS terminals. Among small firms, FinTech-issued POS terminals are substantially more popular than bank-issued POS terminals: according to survey data from 2021 collected by Higgins (2022), 61.1% of small retail firms with a POS terminal had a FinTech-issued POS terminal, while 21.7% had a bank-issued POS terminal and the remainder had a POS from other issuers.[4]

In focus groups we conducted with our FinTech partner's users prior to the RCT, many users stated that prior to adopting the FinTech company's technology they did not accept card payments. While banks charge lower merchant fees, users said that accepting card payments with our FinTech partner's technology is easier as there is less documentation needed to register, there is no need to have a bank account with the same bank providing the POS terminal, and there is no minimum monthly transaction requirement to avoid extra charges. Focus group participants sought to accept electronic payments because they could increase their customer base by attracting customers who prefer to pay with debit and credit cards (consistent with empirical findings in Higgins, 2022). Some noted that it is convenient for them to have increased portability to process transactions anywhere as the FinTech's POS terminal is smaller than a bank-issued POS and can be connected to any mobile device. They also noted that, relative to receiving cash payments, it is convenient to have their payments deposited directly into a bank account and to have increased safety from not needing to hold as much cash.

The FinTech company's motivation for partnering with us for this experiment was two-fold. First, they were interested in testing a lower fee to increase customer retention (i.e., to lose fewer customers to competitor FinTech companies or banks). Second, they did not know 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] The most common other type of issuer is suppliers—such as Bimbo, Coca-Cola, and Grupo Modelo—which now also offer POS terminals directly to retail firms (Higgins, 2022).

4 Experimental Design

Our RCT sample consists of 33,978 firms that were *already* active users of the FinTech payments technology. Prior to the experiment, firms in our sample paid 3.75% or 3.5% in merchant fees per card payment they received from customers. In our RCT, the FinTech company offered 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; whether we send an unannounced reminder, announced reminder, or no reminder; and whether we offer to reduce the fee to 3% or 2.75%. The pure control group consists of firms that are part of the RCT sample and hence eligible to receive an offer based on our selection criteria, but that were randomly assigned to not receive any 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 in the morning of the seventh day. Firms that received an announced 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 two additional treatment arms (in addition to the $2 \times 3 \times 2 +$ control group) that receive the offer with a same-day deadline and no reminder, with a fee reduction to 3% or 2.75%.

4.1 Sample

To maximize the absolute value of the offer, we selected the RCT sample to include only the top quartile of the FinTech company's over 130,000 users, based on monthly sales in August 2020 (the month before our experiment launched). The decision to restrict to the top quartile of baseline sales 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 a 3.75% to 3.5% fee to a subset of the users who had a 3.75% fee.[5] In that pilot, we included firms throughout the baseline sales distribution and 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.

Prior to conducting the RCT, we filtered out firms that were included in our randomized pilot, then restricted to the top quartile of users based on baseline (August 2020) sales. This resulted in a potential RCT sample of 34,010 firms. Our FinTech partner then filtered out users that were not in good standing administratively with the FinTech partner at the time of the study implementation, which resulted in a sample of 33,978 users in the experiment.

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; the numbers in

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

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

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 food vendors, while *professionals* include 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, construction material wholesalers, and other business types.

In addition to the descriptive characteristics from administrative data provided to us by the FinTech partner, we also elicited further information in a survey we ran on a subsample of the firms (see Section 5.2). One variable worth highlighting here is number of employees. Figure 1 shows the distribution of the number of employees by firm. The median number of employees is 3 and the average is 3.9. The distribution of number of employees in our RCT sample looks similar to that of all firms except the very largest in Mexico; we obtain this distribution using microdata from the 2019 INEGI Economic Census. In our RCT, 87% of firms have one to five employees, compared to 90% of all firms in Mexico. Furthermore, the largest firm in our RCT has 150 employees, which corresponds to the 99.7th percentile of the distribution of number of employees across all firms in Mexico.

4.2 Intervention

Our FinTech partner randomly offered a cost-saving measure to firms that were already users of their technology to process electronic payments by debit and credit card. The FinTech company offered to lower the merchant fee these firms were charged for each sale they made through the technology. This fee reduction intervention was offered through email and SMS text messages—all firms received both an email and SMS version of each message in order to maximize the probability they would be aware of the offer. Figure 2 shows examples of the initial email that firms received.

The offer had a link to a short online form that firms had to fill out to activate the fee reduction, and the new lower fee was generally activated within one day. The form required firm owners to fill in basic registration information they had previously shared with our FinTech partner: name, email and national identification number (which is frequently used in Mexico for many types of transactions). The email informed the user that the form would only take one minute to complete, based on the FinTech company's best estimate of the time it would take to fill out the form. We asked firm owners in the survey how long they expected it to take and how long it actually took to fill out the form to activate the offer. Ex ante, most firms estimated that it would take between six and ten minutes to complete the offer; ex post, most firms report taking between one and five minutes (Figure A.1). The time reported by firm owners likely includes the time to perform other actions before activating the lower fee such as reading the fine print, calculating how valuable the offer was, or discussing with someone else at the firm whether to accept the offer.

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

Within each treatment group, we also experimentally varied the value of the offer by offering two levels of lower merchant fees (or no lower merchant fee in the control group). Prior to the experiment, firms were charged either a 3.75% or 3.50% fee for each transaction, measured as a percent of the sale amount; whether they were charged 3.75% or 3.5% was a function of when they started using our FinTech partner's technology. We randomized the offer to be either 3% or 2.75%. (Thus, the fee reduction ranges from 50 basis points—for those reduced from 3.5% to 3%—to 100 basis points—for those reduced from 3.75% to 2.75%. Part of this reduction is random based on their randomized new fee offer, and part is not random based on whether they had a 3.75% or 3.5% fee before the experiment.) The lower fee lasted for six months (until March 31, 2021), after which the firm's rate returned to their pre-intervention rate. All of this information was included in the 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 to adopt the lower merchant fee. The time period t is a day. The cost draw c_t can be thought of as a measure of how busy firm owners are on day t. Survey evidence supports that how busy a firm owner is on a particular day indeed determines whether they adopt that day: in two distinct survey questions, we asked firm owners why they adopted the offer on the first day or why they did not adopt on the first day (Figure A.2). Among firm owners who adopted on the first day, 75.5% reported doing so because they had time that day, and 72.6% of firms that delayed adopting said they were too busy on the day they received the email.

We set the deadline to be after eight days ($T = 8$) for all treatment arms that have a deadline, except for the same-day deadline arm ($T = 1$); the purpose of the same-day deadline arm is to isolate variation in costs from the probability of forgetting. For treatment arms without a deadline, $T = \infty$. We compare cumulative take-up rates on each of the eight days prior to the deadline, and in some specifications also compare take-up for six months after the RCT (since firms in treatment arms without a deadline could continue to adopt after day 8).

We 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. To understand whether the mechanism behind a potential deadline effect maps to the model, we asked firms a survey question on why they thought the offer had a deadline. Only 11.5% of firms gave responses consistent with the deadline creating scarcity. The vast majority of firm owners thought that the offer had a deadline because deadlines are a usual business practice or common marketing tool (Figure A.3, left panel).

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

We also asked firms why they thought the FinTech company sent them a reminder. The top three reasons that firms thought the FinTech sent them a reminder were to make sure they wouldn't forgo a valuable offer, that the FinTech company knew they would forget, or that it is a usual business practice (82.2% of firms answered one of these three responses). Only a small percentage (11.5%) were wary of the motives for being sent a reminder, answering that the reason was to increase the FinTech's profits or to make firms fall for a scam (Figure A.3, right panel).

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* fully trust the offer inherently, but trusts the offer more if they are told they will receive a reminder and then receive this reminder. In this case, denoting the increase in trust caused by treatment as $\Delta\alpha$, the difference between the cost thresholds—for a period $t = \tau$ after receiving the reminder—is:

$$c^*_{\tau,\text{announced}} - c^*_{\tau,\text{unannounced}} = \beta\delta\Delta\alpha y.$$

This means that in the post-reminder periods, the firm will have a higher cost threshold if it is assigned to the announced reminder group rather than the unannounced reminder group. This leads to higher take-up. More broadly, the t subscript on α_t allows trust to increase either upon receiving the initial message or only upon receiving the announced reminder.

4.4 Timeline

Figure 3 shows the experiment timeline across the seven treatment groups plus control group. The control group (Group 1) did not receive any offer and did not receive any emails related to the experiment. The no deadline, no reminder Group 2 received the initial email and SMS with the offer on September 29, and received no subsequent messages; this group could adopt any time after day 8 as well (Figure 2, left panel). The no deadline, announced reminder Group 3 received the initial email on September 29, and their email included an additional sentence stating that they would receive a reminder on October 5. The no deadline, unannounced reminder Group 4 received an initial email identical to that of the no deadline, no reminder Group 2, then received an unannounced reminder email on October 5. The deadline, no reminder Group 5 received an initial email on September 29 that informed them of the deadline, which was October 6. The deadline, announced reminder Group 6 received an initial email on September 29 that informed them of the deadline on October 6 and that they would receive a reminder on October 5 (Figure 2, right panel). The deadline, unannounced reminder Group 7 received the same initial email on September 29 as the deadline, no reminder Group 5 informing them of the October 6 deadline, but then received an unannounced reminder on October 5. Finally, we had a treatment arm with a same-day deadline and no reminder (Group 8), which was informed in the initial email that the deadline was "today, September 29."

The initial emails and SMS messages were sent on September 29, 2020 at 10am Central Standard Time (CST) which is the time zone that covers most of Mexico. The group with the same-day deadline had all of September 29 (until midnight) to take up the offer. The group with a regular deadline had until midnight on October 6. For both the announced and unannounced reminder arms, 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 concurrent SMS text messages that jointly 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 A.4) and applied the filtering criteria using August 2020 sales to exclude firms that had closed or greatly reduced their sales due to COVID-19.

5 Data

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

5.1 Administrative Data

Our main source of data is administrative data on the 33,978 firms. These data include baseline characteristics (such as owner sex, owner age, business type, and dates of registration and first transaction), and both pre- and post-experiment firm $\times$ day level data on the number of transactions and volume of pesos transacted through the FinTech payments technology from July 2019 through March 2021. From the RCT, we have data on whether and when the firm (i) opened the email, (ii) clicked on the link in the email, (iii) filled out the form to activate the lower fee, and (iv) logged into their online account, from the first day of the experiment (September 29, 2020) through the final day that the lower fee was valid (March 31, 2021).

5.2 Survey Data

We conducted a survey on a subset of firms. We were constrained by our FinTech partner in the number of surveys that we were permitted to conduct. We contacted 1,398 firms for the survey and successfully surveyed 471 firms (a 33.7% response rate). Because one of our goals in the survey was to understand why the announced reminder had a larger treatment effect than the unannounced reminder, we oversampled firms in these two treatment arms, and also oversampled firms that accepted the offer. Table A.2 shows that the survey subsample is balanced across treatment arms, and Table A.3 shows that the surveyed subsample is comparable to the non-surveyed sample on observables.

The survey includes questions about number of employees, profits, share of sales through the technology, questions to measure how accurately they knew the fees they were charged and their transactions through the technology (the fee they were charged prior to receiving the offer, the value of transactions made through the technology in the last week, and the amount of fees they paid in the last week), and how much they expected to save by accepting the lower fee. It also includes questions about whether they remember receiving the email and SMS, whether they read the SMS, whether they noticed the offer had a deadline (for those assigned a deadline), and what impact the lower fee had on their business. Depending on whether the firm owner adopted and when, we also ask questions about why they adopted on the first day, waited and adopted on a later day, or did not adopt. Finally, the survey includes general social survey questions that we use for heterogeneous treatment effects analysis.

6 Results

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

Our primary results use the following regression specification separately for each day from day 1 through day 8 of the experiment:

$$y_i = \lambda_{s(i)} + \sum_{k=2}^{K} \beta_k T_i^k + \varepsilon_i, \tag{4}$$

where y_i is a measure of cumulative take-up, i.e., a dummy variable equal to 1 if firm i accepted the offer on or before that day. The specification also includes randomization strata fixed effects $\lambda_{s(i)}$ (which also absorb the constant), T_i^k denotes assignment to treatment arm k (where the omitted category $k = 1$ corresponds to the control group), and ε_i are heteroskedasticity-robust standard errors (not clustered since the randomization unit is the individual firm).

6.1 Benefit

From Prediction 1 in Section 2.3, firms offered a 2.75% fee should have higher take-up than firms offered a 3% fee, since the 2.75% fee implies a larger cost reduction and hence a more valuable offer. Figure 5 shows the cumulative take-up rates by merchants that received a 2.75% or 3% merchant fee (left panel) and the treatment effects of the 2.75% fee offer and their 95% confidence intervals (right panel).

Receiving the more profitable 2.75% merchant fee offer increased take-up. On the first day of the experiment, take-up was 2 pp higher in the 2.75% offer group compared to a base of 19.1% take-up in the 3% offer group. By day 8, take-up was 3.5 pp higher in the 2.75% offer group compared to a base of 25.9% take-up in the 3% offer group. In relative terms, the 2.75% fee increased take-up by 13.5%.

The higher take-up by firms that received a 2.75% offer persists after the deadline: Figure A.5a shows take-up over six months for firms that did not have a deadline (since those with a deadline could not adopt after the deadline). The gap in take-up between the 2.75% and 3% group persists and increases slightly to 4 pp after six months.

6.2 Reminder

From Prediction 2, a reminder will increase take-up if firm owners are forgetful. Figure 6 shows the take-up rate of the lower merchant fee offer by day 8 (October 6, 2020) across treatment arms. We show adoption by day 8 because for the groups that had a deadline, day 8 was the last day on which they could accept the lower merchant fee. Among firms without a deadline (comparing the first two bars of the figure), the unannounced reminder increased take-up by 3.6 pp (14.2%), from 25.4% to 29%. Among firms with a deadline, the unannounced reminder increased take-up by 4.1 pp (16.1%), from 25.6% to 29.8%. The announced reminder increases take-up even further, to 30.5% among firms without a deadline and 31.8% among firms with a deadline.[6]

Zooming in to the timing of the reminder effect (pooling across all firms that received a reminder), Figure 7a shows cumulative take-up rates for the no reminder and reminder arms over day 1 through day 8 in the left panel, and regression coefficients and 95% confidence intervals for the effect of the reminder in the right panel. On day 1, take-up rates were close to 20% in both the reminder and no reminder groups, and both groups' take-up rates increased steadily to about 24% on day 6, the day before the reminder. On all days before the reminder was sent, there is no statistically significant difference between the reminder and no reminder groups, as expected since both groups received an identical initial email.[7] After the reminder was sent on day 7, the take-up rate for the group that received a reminder was 3.2 pp higher than take-up in the group that did not receive a reminder. On day 8, the difference in take-up is 4.7 pp. Figure A.5b shows that—restricting to those without a deadline (since this group could still adopt after day 8)—the gap in take-up driven by the reminder persists for the six months after the experiment.

Heterogeneous effects of reminder. The effect of the reminder is similarly large across both the 2.75% and 3% offer groups (Figure A.6a); there is no statistically significant heterogeneous treatment effect of the reminder exploiting the random variation in how valuable the offer is. We also test for heterogeneity by the expected gain from adopting the offer, which is calculated as the change in the fee times the firm's baseline sales. As expected, firms with an above-median expected gain from adopting have higher overall take-up, but the effect of the reminder is just as large for firms with an above-median and below-median expected gain (Figure A.7a). Both of these heterogeneity tests were prespecified in our pre-analysis plan (PAP). We also have a measure of value of the offer from the survey, which is the percent of total sales that the firm transacts through the FinTech payments technology.[8] While there is substantial variation in this measure (Figure A.8), we again do not find heterogeneous treatment effects of the reminder (Table A.5,

[6] The regression coefficients from specification (4) for take-up by day 8 are shown in Table A.1.

[7] Table A.4 shows that there is also no statistically significant effect of the reminder prior to the reminder being sent, pooling across days 1–6 for increased power.

[8] No heterogeneity tests using survey measures were prespecified, as we designed the survey after seeing the results from days 1–8 in the RCT.

column 2).

The effect of the reminder was also equally large for smaller and larger firms, measured both by below- vs. above-median baseline sales (Figure A.9a) and by below- vs. above-median number of employees (Table 2, column 1). Note that the heterogeneity tests by number of employees use survey data since we do not observe number of employees in the administrative data. We prespecified the heterogeneity test using above- vs. below-median baseline sales as one of the main two variables we would test for heterogeneity on. Because one-employee firms might be different than larger firms, we also test for heterogeneous treatment effects of the reminder for firms with one employee vs. more than one employee, and again do not find statistically significant heterogeneous treatment effects (Table 2, column 2). Finally, we test for heterogeneity by whether the owner of the firm was the one who received the email, and again do not find heterogeneous treatment effects (Table A.6).

We next test whether the reminder is more effective when the offer has a deadline, but find that it is equally effective regardless of whether there is a deadline (Figure A.10). We also test for heterogeneity based on firm type—which was the other of the two main variables to test for heterogeneity on that we pre-specified—and find no heterogeneity (Table A.7, column 1). Finally, we test for heterogeneity on the other variables we pre-specified, which are dummies for above-median owner age, owner sex, and above-median change in baseline sales from August to September 2020 (excluding the days of the experiment, September 29 and 30) to capture firm growth. We do not find heterogeneity on these variables (Table A.8).

In sum, the reminder has a large effect on take-up across all of the types of firms in our experiment.

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

While we cannot rule out limited attention playing a role, we do observe whether firm owners opened the initial email prior to receiving the reminder. Although opening the email is not a perfect measure of knowing about the offer, given the design of the email with a large banner at the top showing the lower fee in large bold numbers and stating "offer to lower your merchant fee" (Figure 2), it is unlikely that firm owners opened the email without knowing it was an offer to lower their merchant fee. Overall, the rate of opening the initial email prior to receiving the reminder was 40.5%. This is substantially higher than the 23% open rate of marketing emails the FinTech company had sent to its users, and more than twice the 18% industry-wide open rate for retail (Mailchimp, 2023). Figure A.13 shows that conditional on opening the email prior to receiving the reminder, the reminder still has a 5.2 pp effect on take-up. We do not observe whether firm owners opened the SMS message, but we asked about this in our survey and 49.4% remembered receiving the SMS; of those, 89.7% report reading the message.

Furthermore, when we asked firms why they did not accept the offer, 20.7% responded that they forgot, which establishes that a substantial fraction of firms that did not accept the offer did know about it and forgot (Figure A.11). Another 23.4% had opened the email according to our administrative data but did not remember doing so, which could be either limited memory—forgetting not only to accept the offer but forgetting the offer even existed—or limited attention (e.g., opening the email but not reading it).

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

6.3 Deadline

Turning to Prediction 3, we estimate the effect of imposing a deadline. Figure 7b 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. Prediction 3 states that if a firm owner is present-biased, there will be an effect of the deadline. The contrapositive of this is that no deadline effect implies that firm owners are not present-biased in this context. In addition, although the point estimate on cumulative take-up by day 8 is positive (but not statistically significant), if this were due to present bias that our RCT was underpowered to detect, we would expect the gap in take-up to persist after the deadline, as the present-biased firm owners would continue procrastinating. On the other hand, if the (not statistically significant) gap in take-up by day 8 were due to firms in the no deadline arm rationally waiting for a better cost draw—i.e., a day they were less busy—we would expect to see take-up quickly catch up in the no deadline arm after the deadline. Figure A.5c shows that take-up in the no deadline group indeed catches up within a few days after the deadline, casting further doubt on the possibility that present bias prevents adoption of this profitable opportunity. Take-up in the no deadline group then surpasses that of the deadline group, and six months after the deadline there is 2 pp higher take-up in the no deadline group.

Heterogeneous effects of deadline. Figures A.6b, A.7b, and A.9b show that there *is* a statistically significant heterogeneous treatment effect of deadlines by the value of the offer (3% vs. 2.75% fee), the size of the gain (Figure A.7b), and the size of the firm (above vs. below-median baseline sales). The deadline *does* have a statistically significant effect on take-up by the day of the deadline for firms with the less-valuable 3% offer and for firms with below-median baseline sales. Nevertheless, in both of these cases take-up in the no deadline arm catches up to take-up in the deadline arm relatively quickly within 2–3 weeks after the deadline (Figure A.18).

We do not find heterogeneous effects of the deadline across the three reminder groups: announced reminder, unannounced reminder, and no reminder (Figures A.10 and A.12c). We also do not find heterogeneous treatment effects by other firm characteristics, including business type (Table A.7, column 2) and owner age, owner sex, or business growth (Table A.10).

Alternative explanations of lack of deadline effect. The deadline would not have an effect if people did not see the sentence of the email that includes the deadline, despite putting "fill the form by October 6" in bold red letters to increase the probability that the firm owner would see it (Figure 2). One reason that it is unlikely that people did not see this line of the email is that the announced reminder was even further down the email also in bold red lettering, and the announced reminder did have an effect so firm owners must have seen it. In the survey, we asked directly whether firms in the deadline treatment groups noticed that the offer had a deadline, and 67.7% said yes.

6.4 Announced Reminders

We next analyze the differential effects of the announced and the unannounced reminder. We start with the pre-reminder effect of announcing the reminder, which is the focus of Prediction 4. Prediction 4 specifies that the announced reminder reduces pre-reminder take-up if firm owners do not believe they have perfect memory and has no differential effect on pre-reminder take-up if firm owners believe they have perfect memory. Figure 7c shows that there is not a *negative* effect of announcing the reminder on pre-reminder take-up: the point estimate is *positive* and not statistically significant. In the model, this suggests that firm owners are overconfident about their memory.

From Prediction 5, announced reminders do not increase final take-up compared to the unannounced reminder if firms inherently trust the offer, but increase final take-up if some firms distrust the offer and their trust in the offer increases after receiving the announced reminder. Take-up after the reminder is sent is 2 pp higher in the no reminder group than the reminder group (statistically significant at the 5% level). This gap in take-up between the announced and unannounced reminder groups persists over six months (Figure A.5d).

Because the point estimates are also positive prior to the reminder being sent, it is likely that some of the announced reminder effect comes from announcing the reminders prior to sending the reminder. While we cannot reject that the treatment effect of the announced reminder relative to the unannounced reminder is larger on the day of the reminder (day 7) than the day before the reminder (day 6), when we pool across pre-reminder and post-reminder days for additional power, we *do* reject that the pre-reminder and post-reminder effects of the announced reminder are equal (Table A.4). Specifically, the effect of announcing the reminder is 1 pp.

The higher take-up in the announced 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 offer and, as a result, the perceived value in the offer. We reached the conclusion that trust was the underlying mechanism based on evidence that we present in Section 7; we also explore potential alternative mechanisms in that section.

Heterogeneous effects of announced reminder. We do not find statistically significant heterogeneous treatment effects of the announced reminder on any dimensions: the size of the fee reduction (Figure A.6c), the expected gain (Figure A.7c), percent of total sales the firm transacts through the FinTech payments technology (Table A.5), baseline sales (Figure A.9c), number of employees and whether the firm has more than one employee (Table 2), whether the owner was the recipient of the emails (Table A.6), whether there is a deadline (Figure A.12c), the business type (Table A.7), or the onwer age, owner sex, or business growth in the month prior to the experiment (Table A.8).[9]

In sum, the larger effect of the announced reminder relative to the unannounced reminder appears to hold across all of the types of firms in our experiment.

7 Mechanisms Behind Announced Reminder Effect

In this section we explore a number of hypotheses about why the announced reminder had a larger effect than the unannounced reminder.

7.1 Announced Reminders Increase Perception of Offer's Value

We first test whether the announcement and then receipt of an announced reminder increased firms' perceptions of the offer's value (relative to an unannounced reminder). For firms that received a reminder, either announced or unannounced, 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 8 shows that receiving the announced reminder caused a 16.1 pp increase in the likelihood that the firm responded that the reminder changed their perception of the offer's value (statistically significant at the 5% level), relative to a base of 23.1% responding yes to this question in the unannounced reminder group. 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 announced and unannounced reminder groups, there were more responses related to trust in the announced reminder group—such as "I had doubts and didn't trust whether it was from [FinTech company]" and "[the reminder] gave it credibility."

[9]The treatment effect of the announced reminder is not statistically significant from zero for some subgroups, likely due to a loss of power from splitting the sample. Nevertheless, we can never reject that the treatment effect of the announced reminder relative to the unannounced reminder is equal across groups in these heterogeneity tests.

7.2 Announced Reminder Effect Concentrated Among Less-Trusting Firms

Next, we test for heterogeneous treatment effects of the anticipated reminder across general social survey measures that we collected in the survey. These measure firm owner's general levels of trust in advertised offers, reciprocity, procrastination, memory, overconfidence about memory, and attention. Specifically, we ask the survey respondents to describe how much they agree with the following statements on a 1 to 5 scale, where 5 is *strongly agree*, 4 is *agree*, 3 is *neither agree nor disagree* 2 is *disagree*, and 1 is *strongly disagree*:

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

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

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

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

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

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

These questions allow us to test whether the effect of the announced reminder relative to the unannounced reminder is larger for firms with various characteristics, such as firms that generally distrust advertised offers. It is worth noting that across all characteristics on which we tested for heterogeneous treatment effects in Section 6.4, we did not find evidence of heterogeneous treatment effects. For each characteristic we create a dummy variable $\mathbb{1}(High\ survey\ measure)$, which we set equal to 1 if the respondent agrees or strongly agrees with the question.We estimate the following regression combining the administrative and survey data and restricting to the sample that received either an announced or unannounced reminder:

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

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 announced 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 announced reminder.

Figure 9 and Table A.11 show the results. The bottom panel of Figure 9 shows the take-up rates for firms by treatment group, $\mathbb{1}(Announced\ reminder)_i$, and high vs. low values of the survey measure, $\mathbb{1}(High\ survey\ measure)_i$; take-up rates in the survey are higher than in the administrative data because we oversampled firms that accepted the offer to be better powered. Focusing on the take-up by low vs. high trust, as expected the overall level of take-up among the unannounced reminder group is higher among firm owners who generally trust advertised offers.

The top panel of Figure 9 shows the effect of announced reminders by $\mathbb{1}(High\ survey\ measure)_i$, where the coefficients in the "Low" columns correspond to β_2 from (5) and the coefficients in the "High" columns correspond to $\beta_2 + \beta_3$. Above each pair of treatment effects we show the statistical significance of β_3; the β_3 coefficient and its standard error are in Table A.11. The treatment effect of anticipated reminders is entirely concentrated among firms with low general trust. In contrast, for firms with high general trust, the treatment effect of announced reminders relative to unannounced reminders is very close to zero and not statistically significant. The heterogeneous treatment effect for high vs. low levels of trust is statistically significant at the 1% level (Table A.11). This indicates that the announced reminder is concentrated among low-trust firms, while the announced reminder has no effect for high-trust firms. In contrast, we generally do not see any statistically significant coefficients on heterogeneity tests for the other survey measures (with the exception of overconfidence, significant only at the 10% level).

We next test whether the *unannounced* reminder also has heterogeneous treatment effects across firms that have high vs. low levels of general trust, as even an unannounced reminder could increase trust. To test this hypothesis, we repeat the same exercise comparing the unannounced reminder groups to groups with *no* reminder and show the results in Figure A.17 and Table A.12. We do not find heterogeneous treatment effects of the reminder among firms with high vs. low levels of general trust, but we do find that the reminder is more effective for firms with high procrastination and low memory (both statistically significant at the 5% level).

Finally, we return to the administrative data where we have a much larger sample but do not have a direct measure of trust. Nevertheless, how long the firm has been using the technology is likely a proxy for how much they trust the FinTech payments company, as trust may have been fostered over time through interfirm interactions. In Figure 10, we plot take-up rates against the number of months that the firm has been using the technology for the announced and unannounced reminder arms. As experience could be correlated with other factors, we residualize both axes with baseline firm characteristics selected using double LASSO selection. The figure shows that, as expected, firms that have been using the technology for longer have higher overall *levels* of take-up. Furthermore, the treatment effect of the announced reminder relative to the unannounced reminder (i.e., the difference between the blue line and the red line) is concentrated among firms that have been using the technology for less time. The treatment effect of the anticipated reminder vs. the unanticipated reminder is statistically significant at the 5% level for the bottom tercile of months using the technology.

7.3 Alternative Explanations for Announced 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 announced reminders on take-up.

Announced reminder increases knowledge of offer profitability. First, we ask whether the announcement of a future reminder may induce firms to check how valuable or profitable the offer is to them, knowing they can adopt when they get the reminder. For example, 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 A.16): 23% of firms reported their fee precisely, and the vast majority who were not perfectly accurate reported that their fee was 4% which could be due to rounding up the 3.5–3.75% fee. Thus, the vast majority of firms either accurately reported their fee or slightly overreported it, which if anything would lead them to think the offer was even more profitable than it was.

In addition, we use administrative data on whether firms logged in to their accounts to check their current fee or sales. We create outcome indicator variables if the firm 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 announced reminder were not more likely to check their online accounts over the course of the experiment compared to firms in the unannounced reminder group.

In addition, we asked firms that did not accept the offer prior to the reminder day and received an announced reminder, "Did you do anything between receiving the initial email and receiving the reminder so that you would know whether to take up the offer when you received the reminder?" Nearly all firms (92.4%) reported not taking any particular action to evaluate the offer between the time they received the initial email and the reminder. Among the remaining 7.6% who do report taking some action between receiving the initial offer and receiving the announced reminder, only 2 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 Figure A.2), only 12.3% of firms replied they needed to discuss or think about the offer first, and there is no statistically significant difference in this proportion between the announced and unannounced reminder arms ($p = 0.361$).[10] We conclude that the

[10] The full survey question is: "We sent you the emails and SMS to let you know about this offer on September 29,

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

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

To test for these or other negative responses to an unannounced reminder, we asked firms that received a reminder an open-ended question to tell us how they felt when they received the reminder (see Figure A.14). Only 2.5% of firms responded that they were annoyed by the reminder, and there is no statistically significant difference between the announced and unannounced reminder groups ($p = 0.494$). Instead, the most common responses indicated that the reminder made firms feel important as a client.

Announced reminder solves organizational frictions. Another possibility is that knowing a reminder will come on a particular day helps solve organizational frictions: for example, knowing when the reminder will come makes it easier to schedule a meeting where the decision of whether to adopt the profitable opportunity will be discussed. It is worth noting, however, that if the firm schedules a meeting about it they are unlikely to forget to adopt, and the deadline also provides a particular date to make the decision by so if this were the mechanism behind the announced reminder effect, the deadline should have a similar effect. In addition, if the mechanism were organizational frictions we would expect differential effects depending on the size of the firm (in terms of number of employees and baseline sales) and depending on whether the firm is a single-person firm or has more than one employee. However, in Section 6.4 we did not see any heterogeneous treatment effects for these variables.

7.4 Accounting for Low Take-Up Rates

Why then does take-up remain far below 100%, even with an announced reminder and despite being a profitable opportunity? There are a number of potential reasons, which we list in the approximate order of their prevalence in response to our survey question that directly asked firms why they did not adopt (Figure A.11). First, firm owners may have been very busy both when they

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.

got the initial email and when they got the reminder, and then forgot, which would be consistent with the top two most common responses of "ran out of time" and "forgot".[11] Others thought it would take too much time, did not consider it important, or were not sure if it would benefit them. Although expected cost savings from the lower fee are equal to 3% of firm profits for the median firm, there is heterogeneity driven by (i) the random variation in whether we offered firms a 2.75% or 3% fee, (ii) the firm's profit margins, and (iii) the percent of sales transacted through the FinTech payments technology rather than in cash. Thus, while it is a profitable opportunity for a substantial fraction of firms in the experiment, some firms might still not consider it a sufficiently profitable opportunity. Some firms likely do not trust the offer even with the announced reminder, and the announced reminder effect does not change depending on firm characteristics.

8 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 an outcome measuring use of the payments technology, 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 4 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 3%, respectively.[12] The third column (Panel A) shows the regression results with the

[11] The first bar in Figure A.11, "opened email but did not remember doing so," is the number of people who we observe opened the email but who did not recall opening the email when we surveyed them, so we could not ask them why they did not accept the offer.

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

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 1.3 pp.

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 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 84.7%. This means that firms that accepted the offer were, in relative terms, 5.1% (=4.3/0.8) 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. Cash also has various indirect costs, however, so with a lower fee per transaction, some firms may 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 (Figure A.15). For example, one firm replied, "(The effect is) very good, (we) don't charge commission to clients anymore."

9 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. Unannounced reminders increased take-up of the lower fee by 15%, 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. Announced reminders increased the lower merchant fee adoption by an additional 7% on top of an unannounced reminder. Through a survey with a subsample of the firms in the study, we find that the announced 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. Well-known behavioral determinants of individuals failing to adopt and take advantage of a new opportunity appear to also bind in the context of firms. 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, we provide evidence of distrust as a key explanatory variable. While the role of these determinants in inhibiting decision making has been much discussed in the consumer-level literature, they have received less attention when studying firms and impediments to their growth.

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Table 1: Baseline Treatment Balance

	Intercept (1)	Announced reminder (2)	Unannounced 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	39.402***	0.290*	0.231	-0.006	-0.025	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	24.107***	0.096	0.115	-0.078	0.122	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]

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

Table 2: Heterogeneous Treatment Effects by Number of Employees

	Firm accepted offer			
	(1)	(2)	(3)	(4)
Intercept	0.478***	0.571***	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)		
Announced reminder			0.169**	0.190
			(0.073)	(0.115)
Above median # of employees			0.025	
× Announced reminder			(0.095)	
More than 1 employee				-0.007
× Announced reminder				(0.126)
Number of firms	462	462	417	417
Mean heterogeneity variable	0.565	0.816	0.573	0.83

This table reports heterogeneous treatment effects of the reminder and announced reminder by number of employees. Two measures of number of employees are used: above-median number of employees and more than one employee. 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 31. 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 3: Account Log ins by Treatment

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

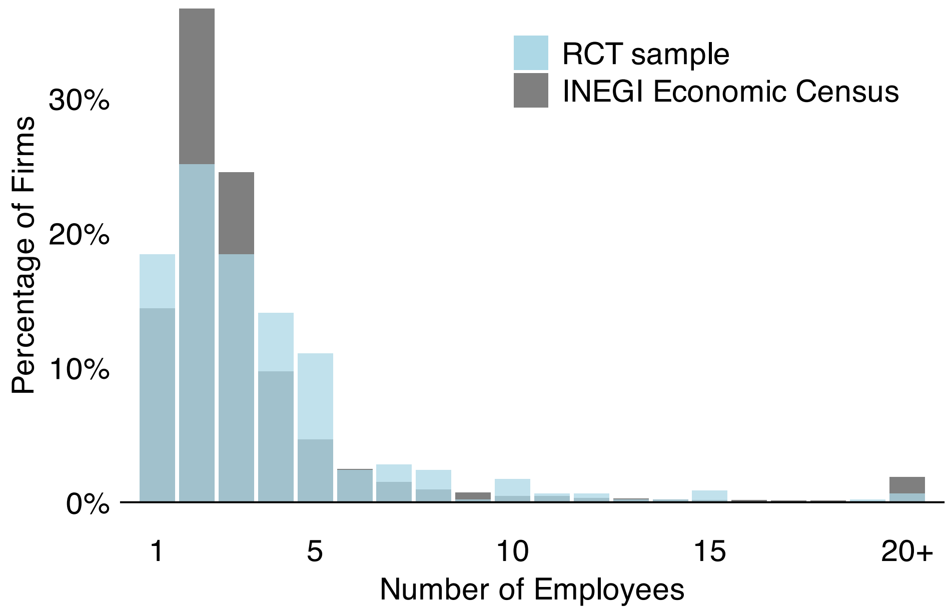
Note: This table reports differences in the probability a firm logged into the FinTech platform and viewed deposits by treatment. The unit of observation is at the firm level. The coefficients come from the regression of the outcome on treatment dummies. Column (1) shows the effect on the probability that a firm logged into the FinTech platform, and column (2) shows the effect on the probability that a firm logged into the FinTech platform and viewed its deposits. Administrative data on the FinTech platform includes all firms in the experiment (N = 33,978) and consists in observations from days 1 to 8 of the experiment (until the one-week deadline). Robust standard errors are included in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: 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	0.847
Control mean (levels, winsorized)	12,178	19.52	0.847

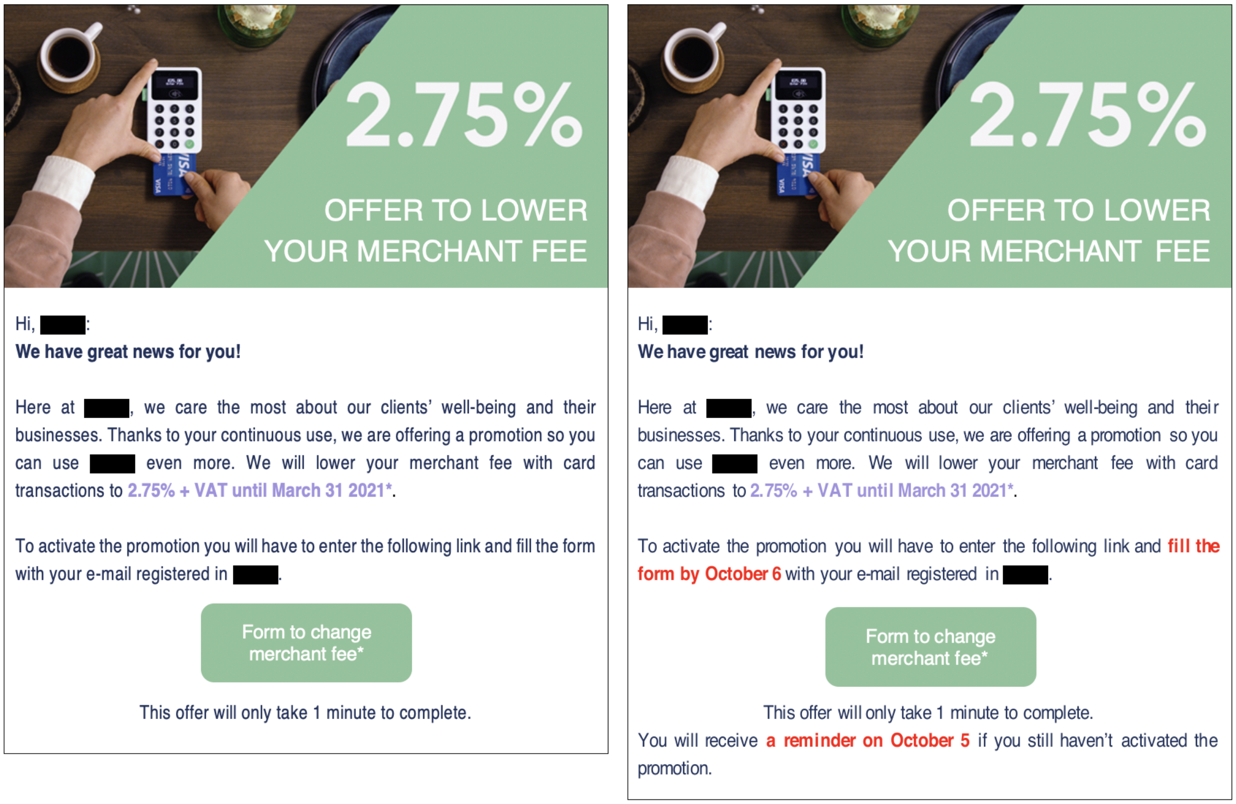
This table reports sales elasticities of the treated group (being offered the lower fee) and of the adopted group (adopting the lower fee). Data is from July 2019 to March 2021, includes Sep 29 and Sep 30 as part of October, and contains all firms in the experiment. The unit of observation is at the 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



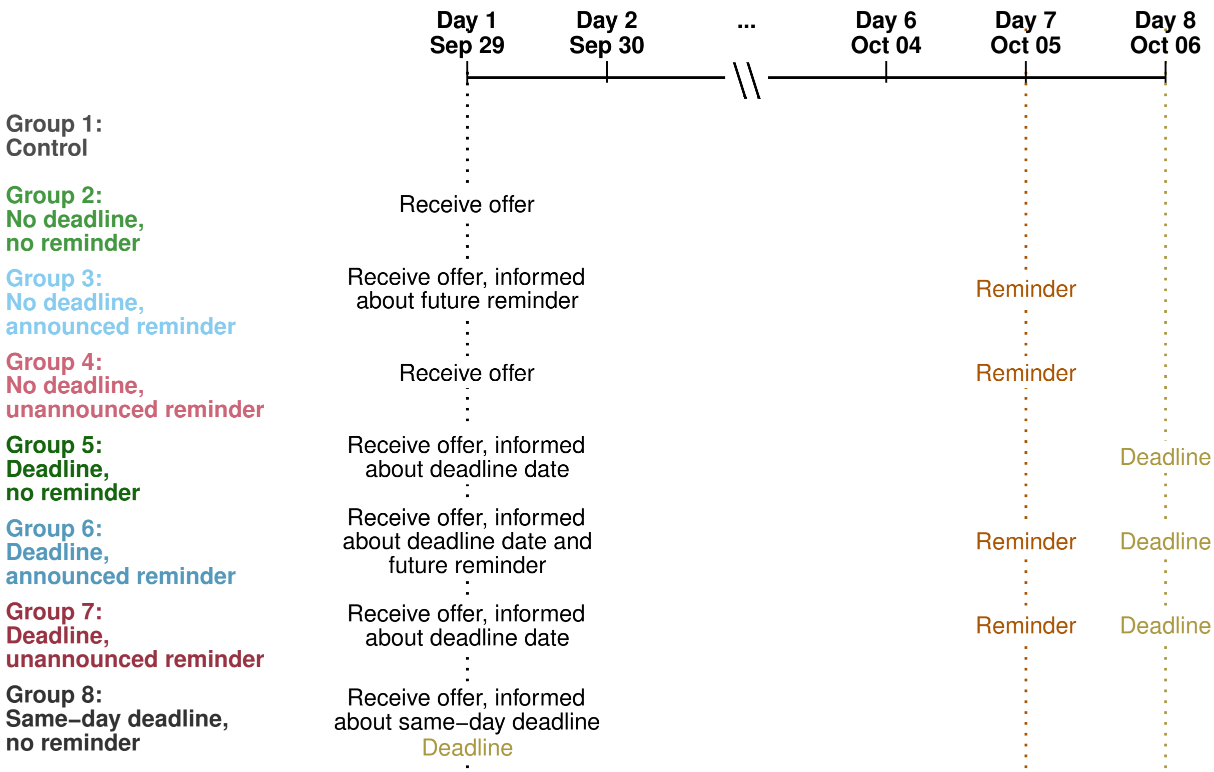
This figure shows a histogram of the number of employees by firm from the subsample of firms in our RCT included in our survey ($N = 462$) and the 2019 INEGI Economic Census ($N = 5,360,215$). All 471 firms in the survey were asked the following survey question: "How many employees work in your business, including yourself?" 9 firms that did not answer the survey question are excluded from the figure. We top-code the figure at 20 employees, so the rightmost bin for the histogram marked "20+" includes firms with ≥ 20 employees. 99.8% of firms in our sample and 98.2% of firms in the Economic Census have ≤ 20 employees. For the RCT sample, mean number of employees = 3.9, median = 3, maximum = 150, and standard deviation = 7.4.

Figure 2: Sample Emails with Lower Rate Offers



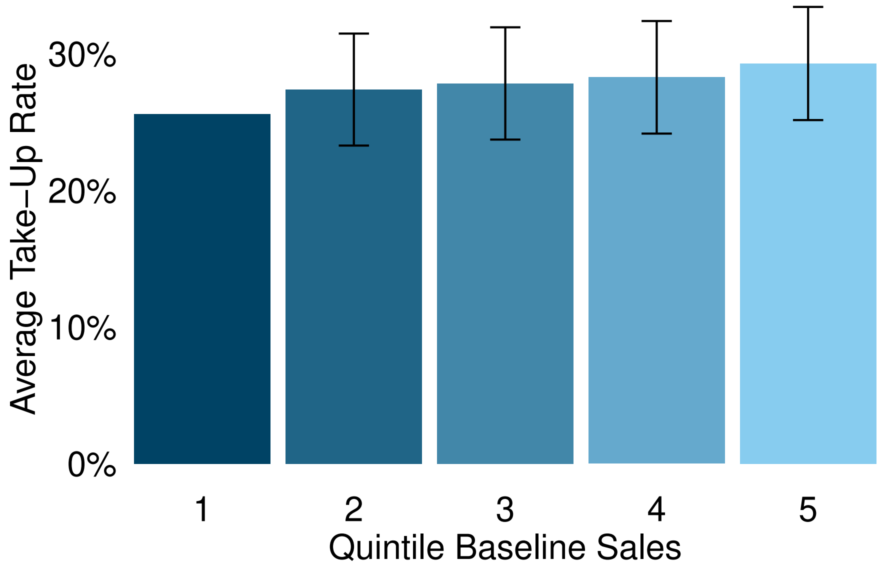
This figure shows screenshots of the emails sent to different treatment arms. The left panel shows the email sent to treatment groups with no deadline and either no reminder or an unannounced reminder (since the initial email would be identical for these groups). The right panel shows an offer sent to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

Figure 3: Study Timeline



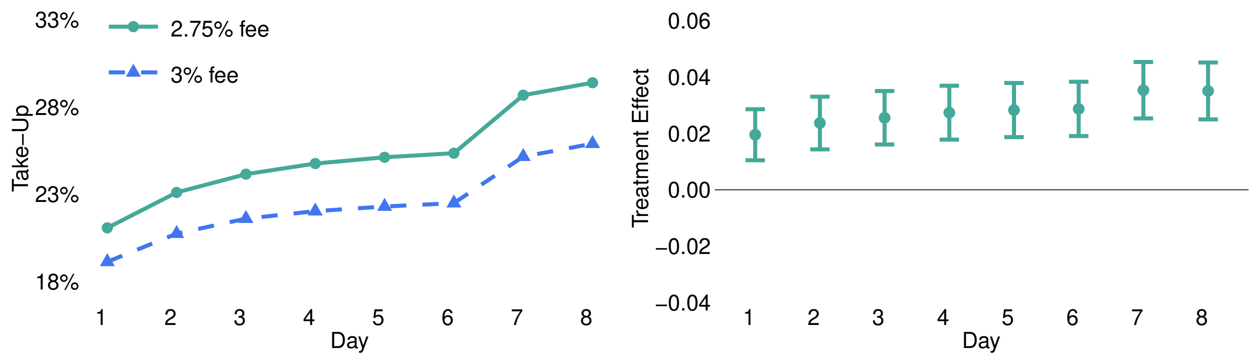
This figure shows the timing of the initial email, reminder, and deadline for each treatment arm. Days 3-5 (October 1–3, 2020) were omitted from the timeline to simplify the figure.

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



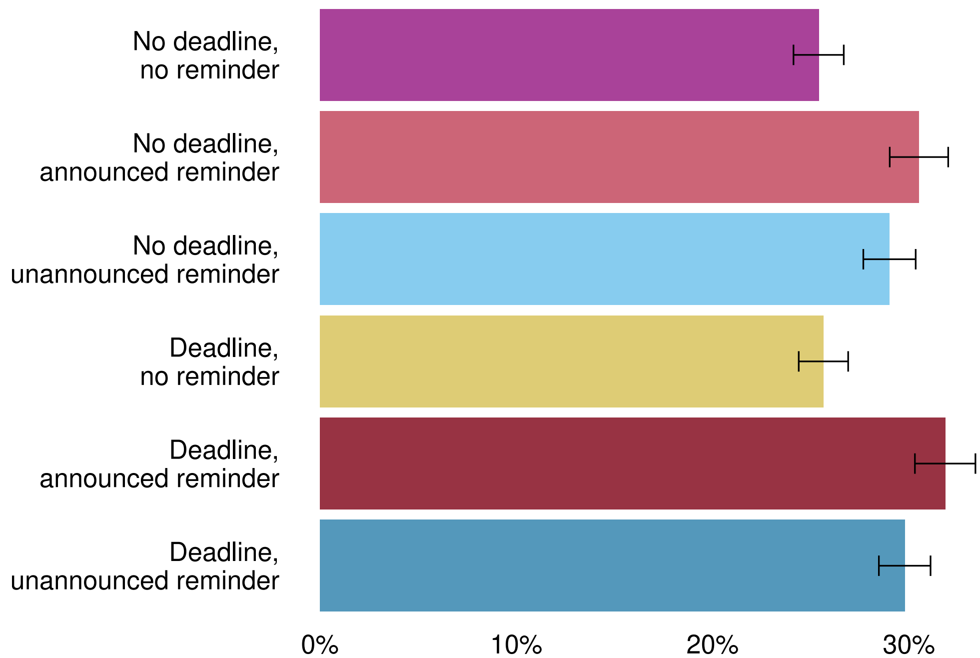
This figure shows the average take-up rate by quintile of baseline sales for firms in the no deadline, no reminder group ($N = 4,455$). Data includes take-up from September 29 to March 31. Baseline sales is defined as the winsorized average monthly sales volume from from Sepember 2019 to August 2020. 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 ($p = 0.080$).

Figure 5: Effect of Lower Fee on Take-up



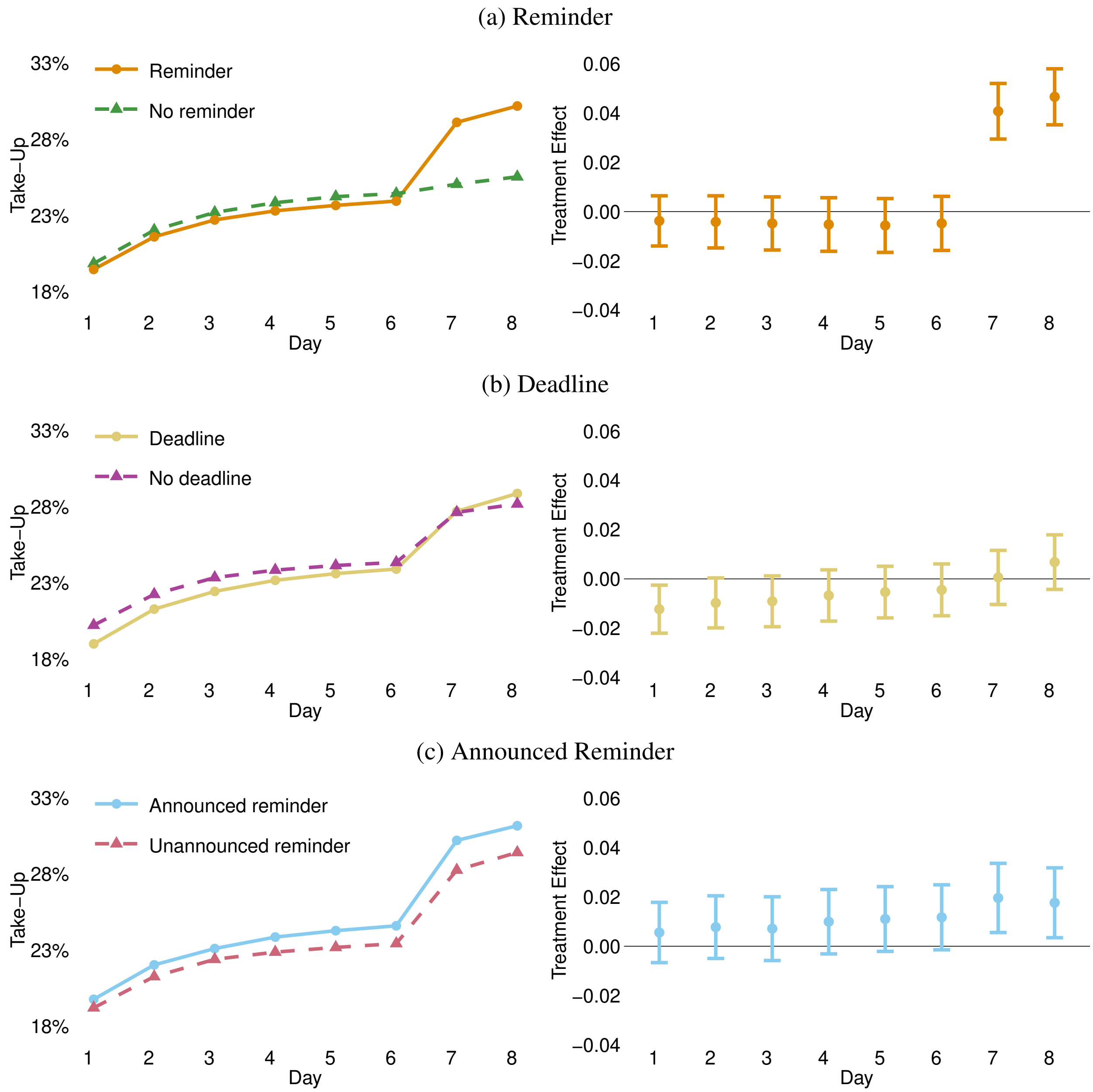
This figure shows take-up and treatment effects of the fee group. The unit of observation is at the firm level. The line graph shows average take-up rates by fee group. The coefficient graph shows the corresponding coefficient estimates for the fee effect, separately for each day of the experiment. Coefficient estimates and 95% confidence intervals come from daily regressions of take-up on a dummy of the fee group, including strata fixed effects. Data includes take-up from September 29 to October 6 (the day of the deadline), with 29,968 firms with 2.75% and 3.00% offers.

Figure 6: Take-up by Treatment Arm



This figure shows take-up by day 8 by treatment group, where day 8 is the day of the deadline for firms that had a deadline. The coefficients and 95% confidence intervals come from a regression of take-up by day 8 on treatment group dummies, using specification (4). The figure uses data from $N = 25,327$ firms, excluding the control group (Group 1) and the same-day deadline, no reminder group (Group 8) from the full sample of 33,978 firms. Take-up is from Day 1 to Day 8, September 29 to October 6 (the day of the deadline).

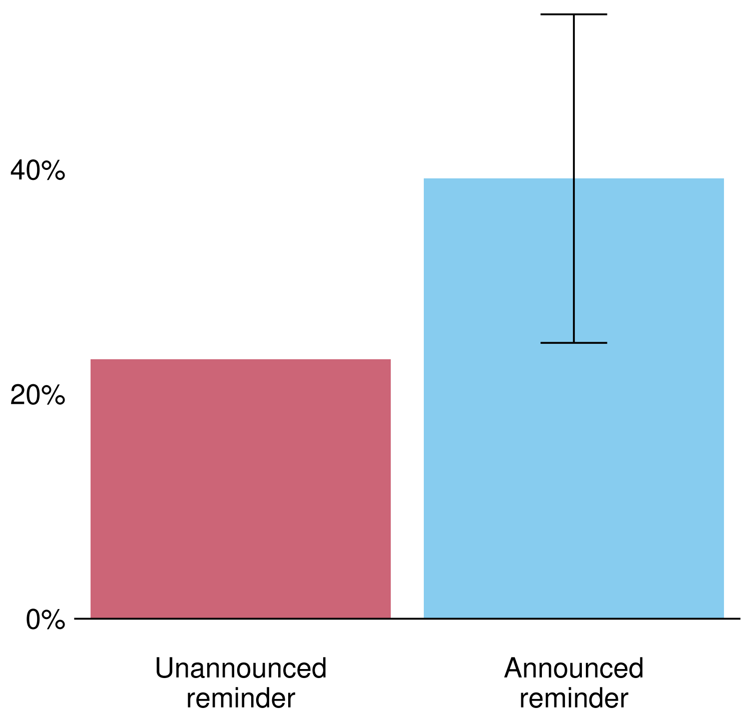
Figure 7: Effect of Treatment on Take-Up



Note: This figure shows take-up and treatment effects of the lower fee offer by unannounced reminder, deadline and announced reminder groups. The unit of observation is at the firm level. Line graphs show average take-up rates by treatment group. Coefficient graphs show the corresponding coefficient estimates for the differential take up of the reminder groups, separately for each day of the experiment. Coefficient estimates and 95% confidence intervals come from daily regressions of take-up on treatment. Regressions include strata fixed effects.

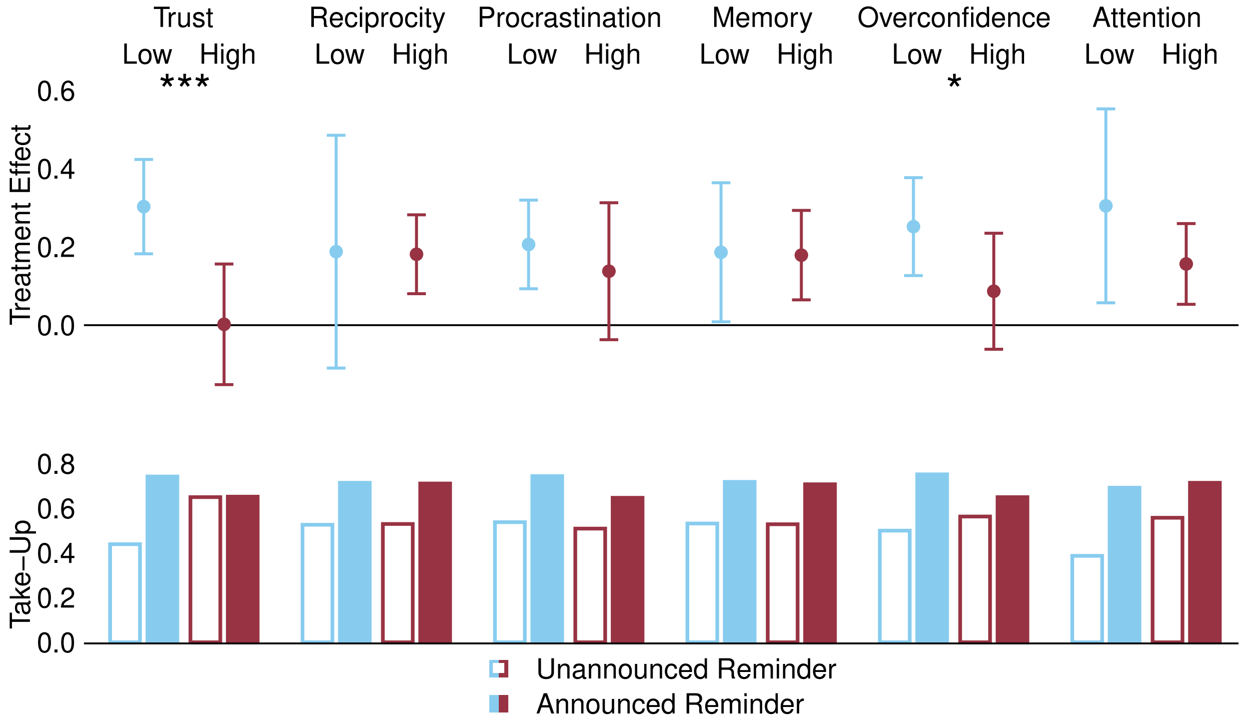
Data includes take-up from September 29 to October 6 (the day of the deadline). Panel (a) includes 18,155 firms with unannounced reminders and no reminders, excluding the same-day deadline group; Panel (b) includes 25,327 firms with deadlines and no deadlines, excluding the same-day deadline group; Panel (c) includes 16,254 firms with announced and unannounced reminders, excluding firms without reminders.

Figure 8: Percentage of Firms for Which Reminder Changed Perception of Offer Value



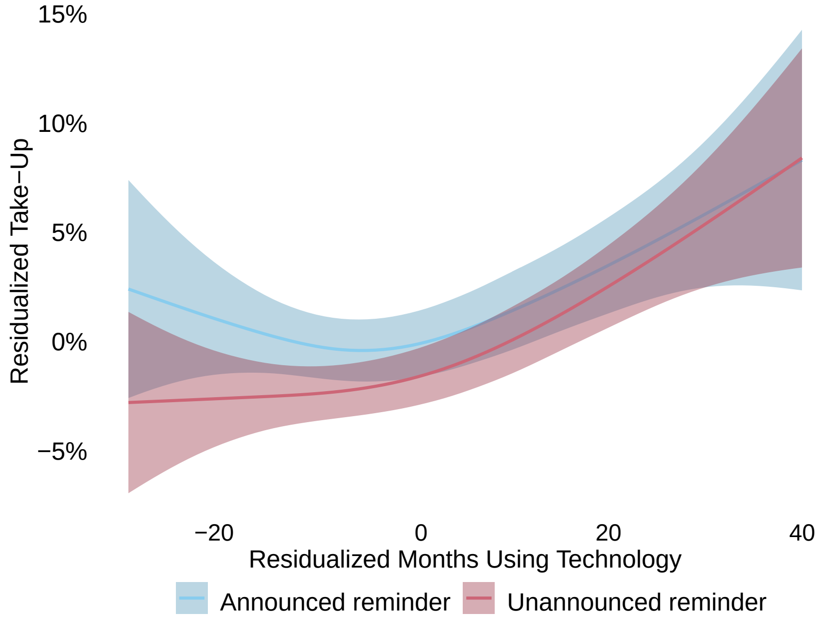
This figure contains a barplot with the percentage of firms that said the reminder changed their perception of the offer's value, by reminder type. Data comes from survey conducted on a random sample of firms in the experiment ($N = 471$), with 157 firms asked this particular question. This question 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 announced reminder, with heteroskedasticity-robust standard errors.

Figure 9: Heterogeneous Effect of Announced Reminders by Survey Measures



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 announced reminder, the survey measure,and the interaction between announced reminder and the survey measure. Data includes firms with announced and unannounced reminders in survey sample, and includes take-up from September 29 to March 31. All firms in the survey were asked these questions. The survey question asked respondents whether they agreed or disagreed with the following six statements: (1) *Trust*: I trust advertised offers. (2) *Reciprocity*: I am more inclined to do business with people who live up to their promises. (3) *Procrastination*: I tend to postpone tasks, even when I know it is better to do them immediately. (4) *Memory*: I tend to have good memory about pending tasks that I have to do and complete. (5) *Overconfidence*: I tend to think my memory is better than it really is. (6) *Attention*: I can focus completely when I have to finish a task. The scale of these responses is 1 to 5, where 5 is highest level of agreement and 1 highest level of disagreement. Binary measure variables were created from these responses, coding 4 and 5 (agree and completely agree) as 1 and 1-3 (completely disagree, disagree and neither agree nor disagree) as 0. 43 firms that did not answer the question were excluded from the sample. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 10: Effect of Announced Reminders by Months Using the Technology



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 announced reminder and unannounced reminder groups, and includes take-up from September 29 to March 31. 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 above the 95th percentile of months using the technology were omitted from the graph for legibility.

Internet Appendix

A Tables and Figures

Table A.1: Main Regression Results

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

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. The coefficients come from the regression of lower fee offer take-up on treatment dummies, with Group 1: Control omitted from the regression. Data includes all firms in the experiment (N = 33,978) and includes take-up from September 29 to October 6 (the day of the one-week deadline). Regressions include strata fixed effects. Clustered standard errors at the strata level are included in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.2: Survey Baseline Treatment Balance

	Intercept	Announced reminder	Unannounced reminder	Deadline	Joint test F-stat
	(1)	(2)	(3)	(4)	(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	40.518***	-1.236	-0.760	0.227	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	20.852***	1.060	2.561	2.631	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 announced reminder, unannounced 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.

Table A.3: 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	39.512	39.732	0.220	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	24.182	23.804	-0.378	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.

Table A.4: Announced Reminder and Reminder Effect Timing

	Firm accepted offer	
	(1)	(2)
Reminder	-0.005	
	(0.005)	
Reminder × Post reminder	0.048***	
	(0.002)	
Announced reminder		0.009
		(0.006)
Announced reminder × Post reminder		0.010**
		(0.004)
Num. Obs.	202,616	130,032
Num. Firms	25,327	16,254
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 announced reminder groups, comparing take-up on days 1-6 (before the lower fee offer reminder was sent) against take-up on days 7-8 (until the deadline). The unit of observation is at the firm-day level. Column (1) uses data from 25,327 firms, excluding the Control and same-day deadline, no reminder groups from the full sample of 33,978 firms. Column (2) uses data from 7,172 firms, including only firms that received a reminder. 'Post reminder' is 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.5: 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)	
Announced reminder			0.201**
			(0.087)
Above median % sales using technology			-0.039
× Announced reminder			(0.116)
Number of firms	306	306	273

Note: This table reports heterogeneous treatment effects of the reminder and announced 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$10% of their total sales using the technology. Data comes from survey conducted on a random sample of firms in the experiment (N = 471), and includes take-up from September 29 to March 31. The question *What share of your total pesos of sales did you make through the technology in the past week?* was asked to all users that reported they obtained income using the technology in the past week ($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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: 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 × Reminder		0.011	
		(0.200)	
Announced reminder			0.318**
			(0.130)
Owner was recipient of emails × Announced reminder			-0.163
			(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% of firms, the owner receives the email. Data comes from survey conducted on a random sample of firms in the experiment (N = 471), and includes take-up from September 29 to March 31. 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.7: Heterogeneous Treatment Effects by Firm Business Type

	Firm accepted offer		
	Reminder	Deadline	Announced 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 31. Columns (1) and (2) exclude the Control and same-day deadline, no reminder groups from the full sample of 33,978 firms. Column (3) keeps only firms from the announced and unannounced reminder groups. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: 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	-0.001		
× Reminder	(0.012)		
Owner sex female		0.009	
× Reminder		(0.012)	
Above median baseline change in sales			0.015
× Reminder			(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 31, excluding the Control and same-day 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.9: 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 31, excluding the Control and same-day 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Heterogeneous Treatment Effects of Announced Reminder by Firm Baseline Characteristics

	Firm accepted offer		
	(1)	(2)	(3)
Announced 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		
× Announced reminder	(0.015)		
Owner sex female		0.021	
× Announced reminder		(0.015)	
Above median baseline change in sales			-0.008
× Announced reminder			(0.015)
Number of firms	15,138	15,141	16,254

Note: This table reports heterogeneous treatment effects of announced 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 announced reminder, the heterogeneity variable and the interaction between announced 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 31, from the announced and unannounced 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.11: Treatment Effect of Announced 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)
Announced 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
× Announced 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

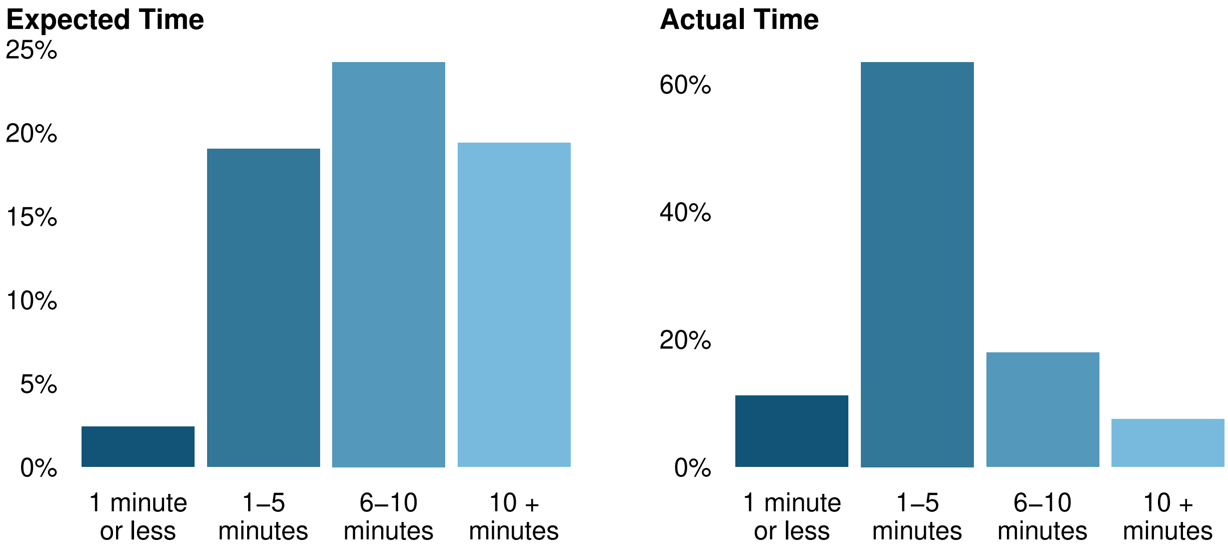
Note: This table reports heterogeneous treatment effects by survey measure. The unit of observation is at the firm level. The coefficients come from the regression of lower fee offer take-up on announced reminder, the survey measure,and the interaction between announced reminder and the survey measure. Data includes firms with announced and unannounced reminders in survey sample, and includes take-up from September 29 to March 31. All firms in the survey were asked these questions. The survey question asked respondents whether they agreed or disagreed with the following six statements: (1) *Trust*: I trust advertised offers. (2) *Reciprocity*: I am more inclined to do business with people who live up to their promises. (3) *Procrastination*: I tend to postpone tasks, even when I know it is better to do them immediately. (4) *Memory*: I tend to have good memory about pending tasks that I have to do and complete. (5) *Overconfidence*: I tend to think my memory is better than it really is. (6) *Attention*: I can focus completely when I have to finish a task. The scale of these responses is 1 to 5, where 5 is highest level of agreement and 1 highest level of disagreement. Binary measure variables were created from these responses, coding 4 and 5 (agree and completely agree) as 1 and 1-3 (completely disagree, disagree and neither agree nor disagree) as 0. 43 firms that did not answer the question were excluded from the sample. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A.12: Heterogeneous Treatment Effects of Unannounced 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)
Unannounced 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
× Unannounced 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

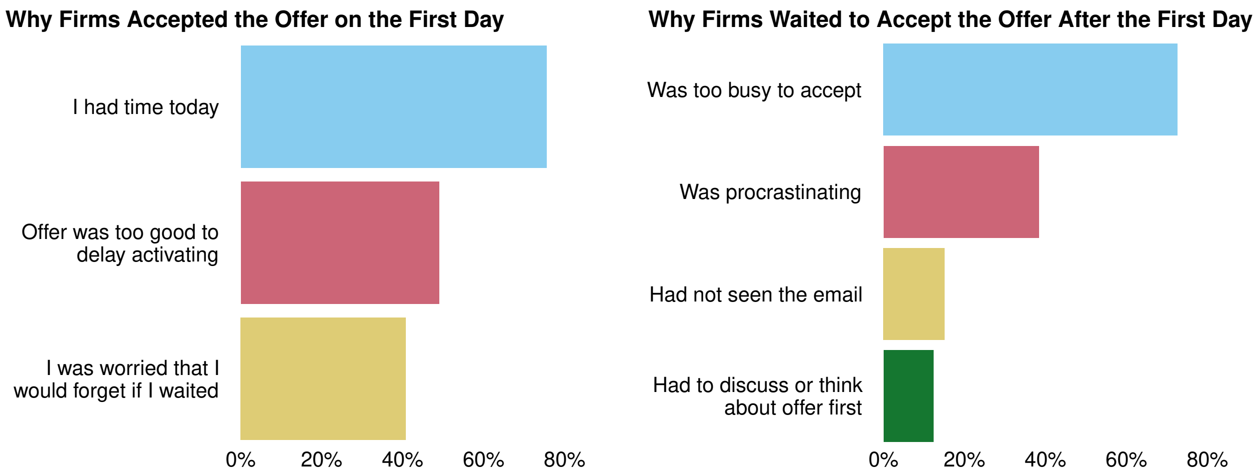
Note: This table reports heterogeneous treatment effects by survey measure. The unit of observation is at the firm level. The coefficients come from the regression of lower fee offer take-up on unannounced reminder, the survey measure,and the interaction between unannounced reminder and the survey measure.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

Figure A.1: Self-Reported Time to Accept Offer



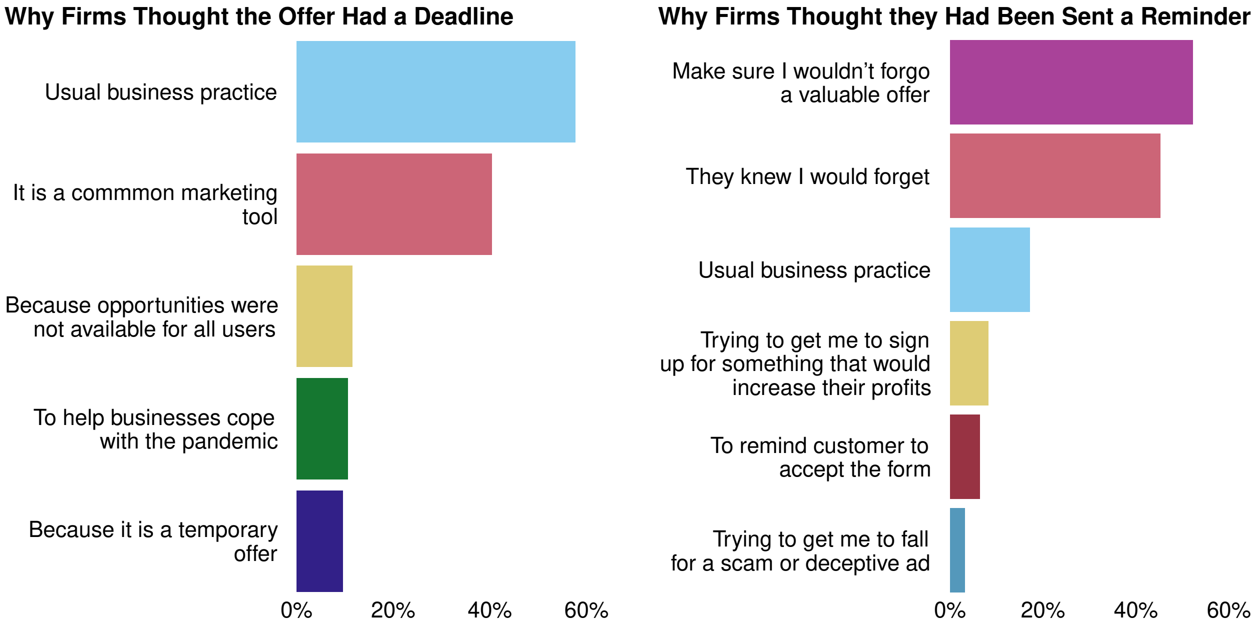
This figure shows how long firms expected it would take them to fill out the form to accept the offer (left panel) and how long it actually took them (right panel). The left panel shows responses to the following survey question: "How long did you expect completing the form to activate the lower fee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

Figure A.2: Why Firms Accepted the Offer on or After the First Day



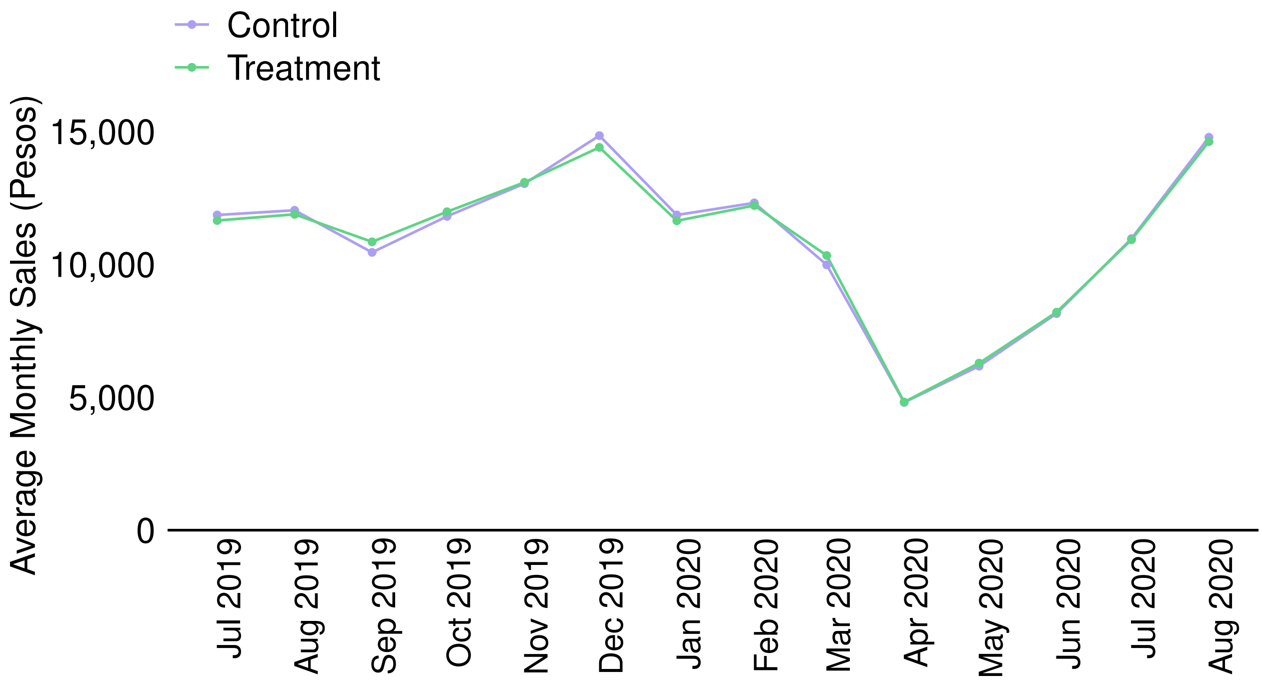
This figure shows why firms accepted the offer on the day they received the initial email and SMS (left panel) or after that day (right panel). Respondents could provide more than one response, so totals add up to more than 100%. The left panel shows responses to the following survey question: "Our records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

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



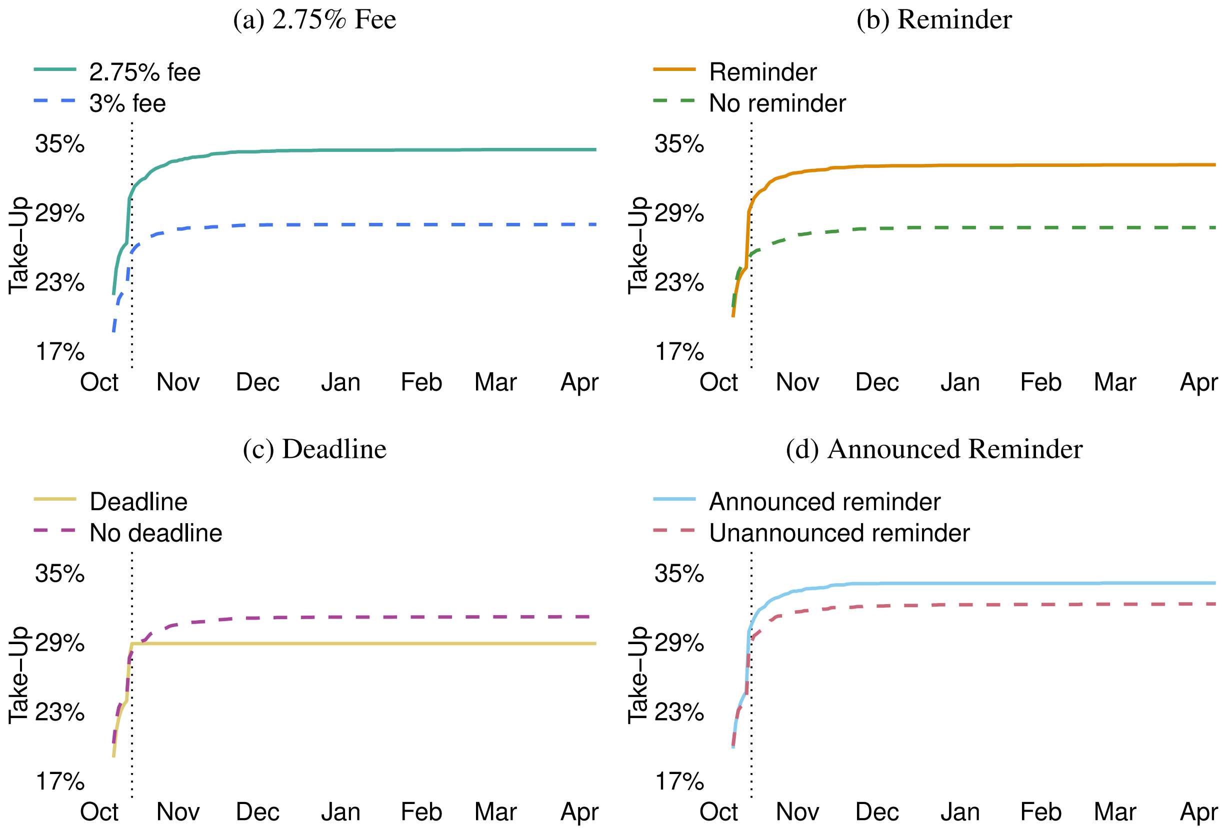
This figure shows why firms thought the offer had a deadline (left panel) or reminder (right panel). Respondents could provide more than one response, so totals add up to more than 100%. The left panel shows responses to the following survey question: "Why do you think the offer had a deadline?" This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

Figure A.4: Sales by Month Prior to Experiment



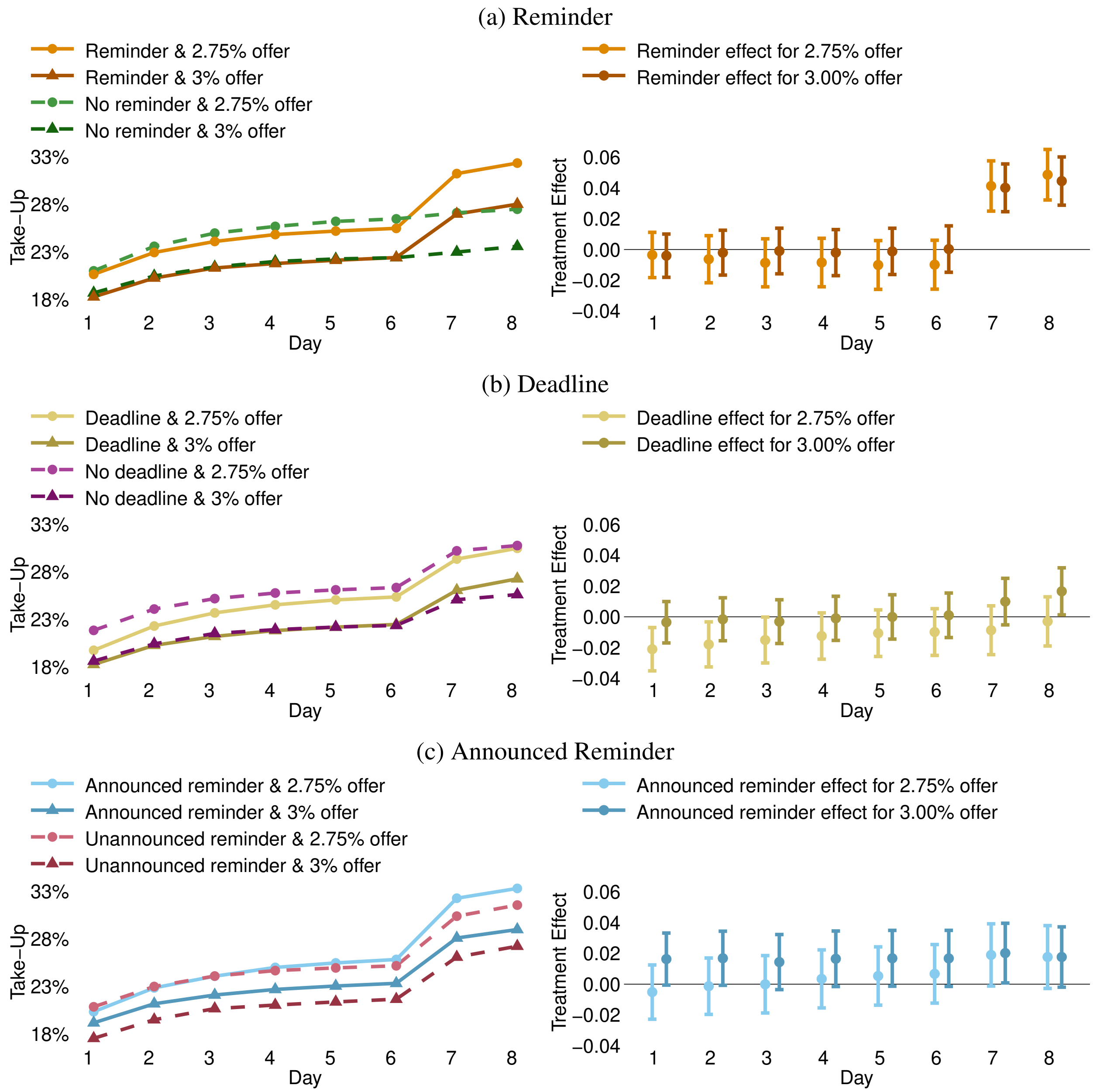
Note: This figure shows average winsorized monthly sales for the control group (Group 1) and the pooled treatment groups (Groups 2 through 8). Sales fell in March and April 2020 due to the COVID-19 pandemic, which delayed the planned start of our experiment. We waited to launch the experiment until sales had recovered to pre-pandemic levels, which occurred in August 2020. To determine the final RCT sample, we applied the filtering criteria described in Section 4.4 using August 2020 sales to exclude firms that had closed or greatly reduced their sales due to COVID-19. Monthly sales are winsorized at the 95th percentile within each treatment group and month. Data includes all firms in the experiment (N = 33,978) and includes take-up from July 2019 to August 2020 (14 months of data per firm).

Figure A.5: Long-Term Take-Up by Treatment



Note: This figure shows long-term take-up of the lower fee offer by 2.75% fee, reminder, deadline and announced reminder groups. The unit of observation is at the firm level. The dotted line indicates the day of the deadline. Data includes take-up from September 29 to March 31. Panel (a) includes 17,220 firms with 2.75% and 3.00% offers and no deadline; Panel (b) includes 12,579 firms with reminders and no reminders and no deadline; Panel (c) includes 25,327 firms with deadlines and no deadlines, excluding the same-day deadline group; Panel (d) includes 8,124 firms with anticipated and unanticipated reminders and no deadline.

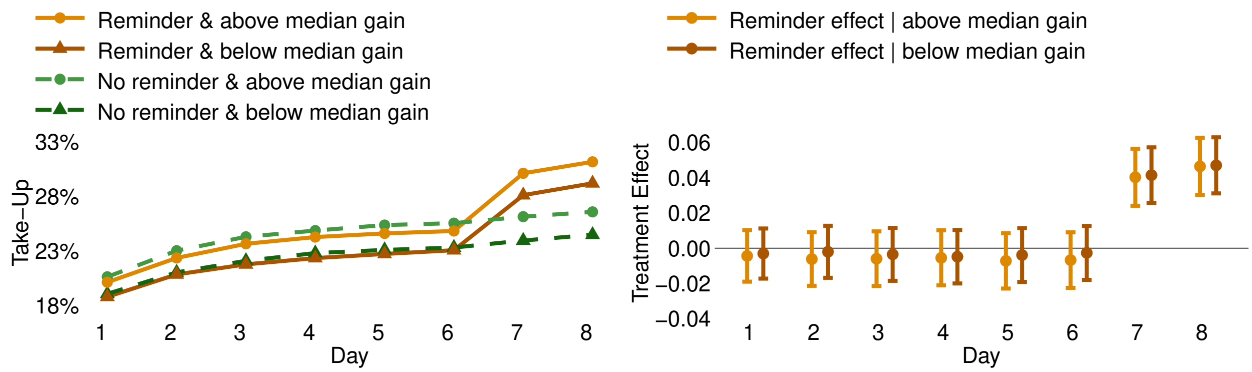
Figure A.6: Heterogeneous Treatment Effects by Offer Value



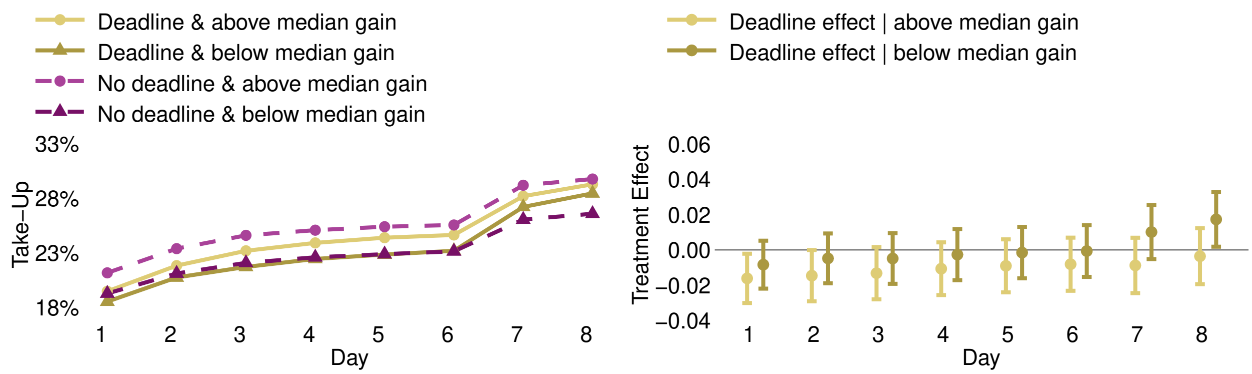
Note: This figure shows take-up and heterogeneous treatment effects by fee level of the lower fee offer by reminder, deadline and announced reminder groups. The unit of observation is at the firm level. Line graphs show average take-up rates by treatment and fee level group. Coefficient graphs show the corresponding coefficient estimates for the differential take up of the reminder groups, separately for each day of the experiment. Coefficient estimates and 95% confidence intervals come from daily regressions of take-up on treatment, a dummy indicating 2.7% fee, and the interaction between treatment and the 2.7% fee dummy. Regressions include strata fixed effects. Data includes take-up from September 29 to October 6 (the day of the deadline). Panel (a) includes 25,327 firms with reminders and no reminders, excluding the same-day deadline group; Panel (b) includes 25,327 firms with deadlines and no deadlines, excluding the same-day deadline group; Panel (c) includes 16,254 firms with announced and unannounced reminders, excluding firms without reminders.

Figure A.7: Heterogeneous Treatment Effects by Expected Gain from Adopting

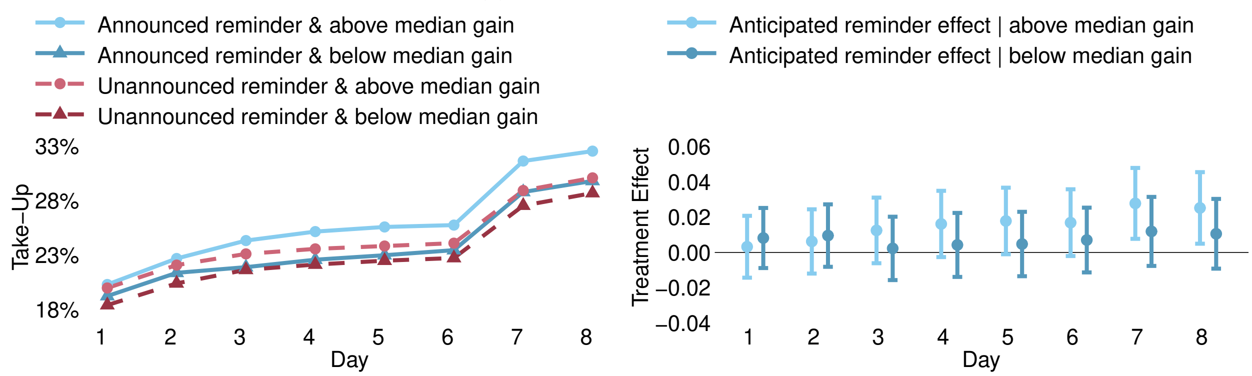
(a) Reminder



(b) Deadline

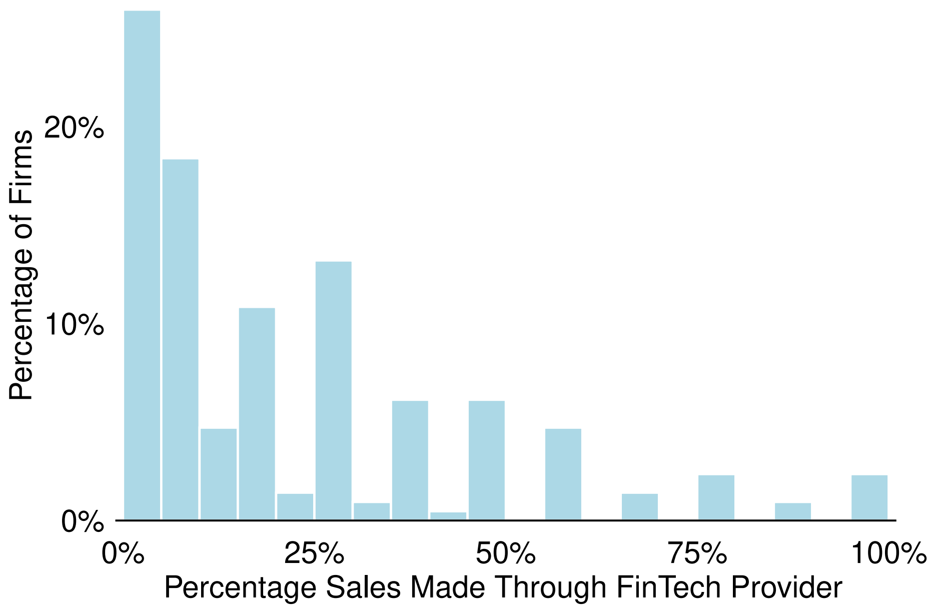


(c) Announced Reminder



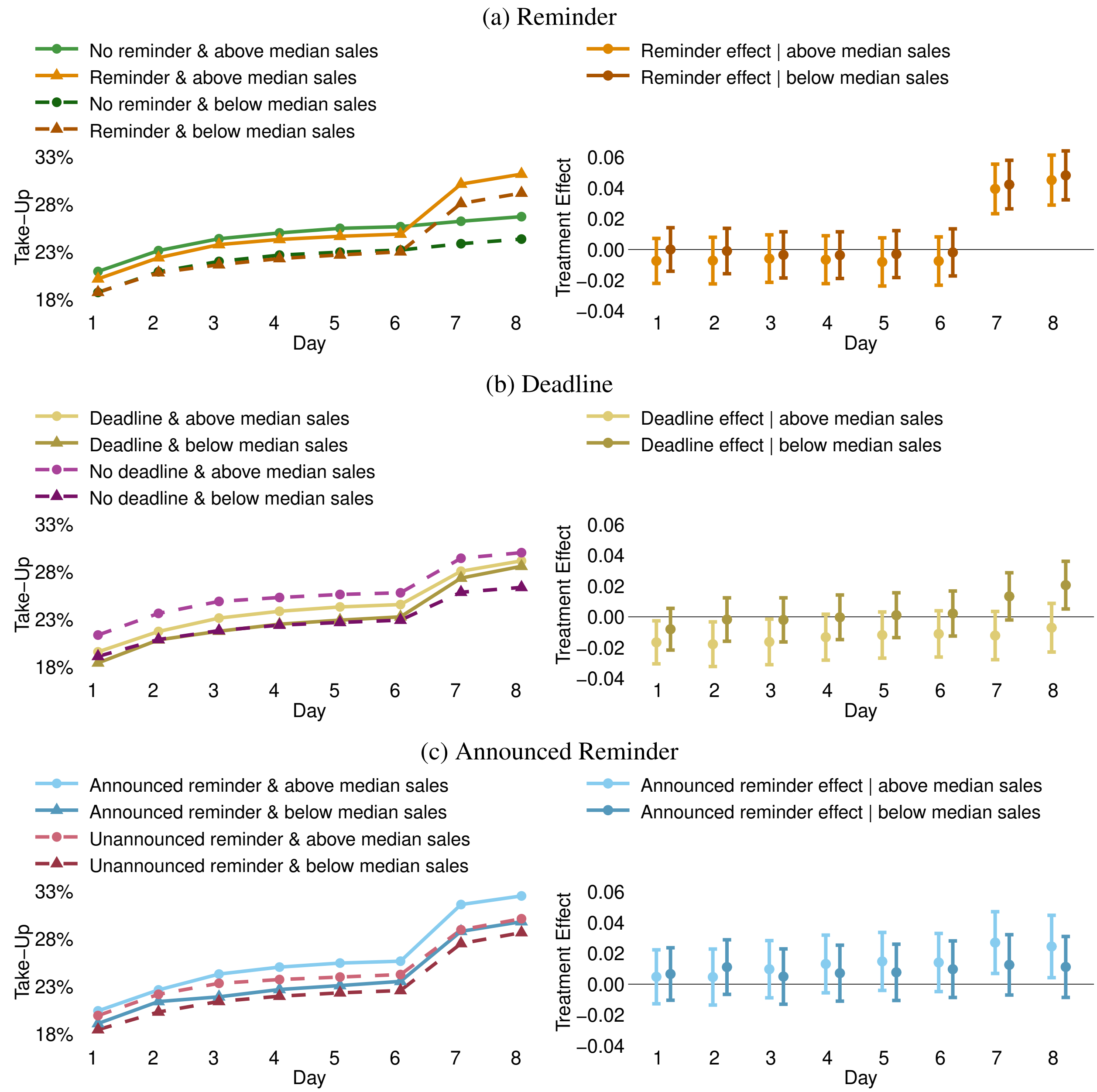
Note: This figure shows take-up and heterogeneous treatment effects by expected gain by reminder, deadline and announced reminder groups. The unit of observation is at the firm level. Line graphs show average take-up rates by treatment and expected gain group. Coefficient graphs show the corresponding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

Figure A.8: Percent of Sales Made Through FinTech Provider Last Week



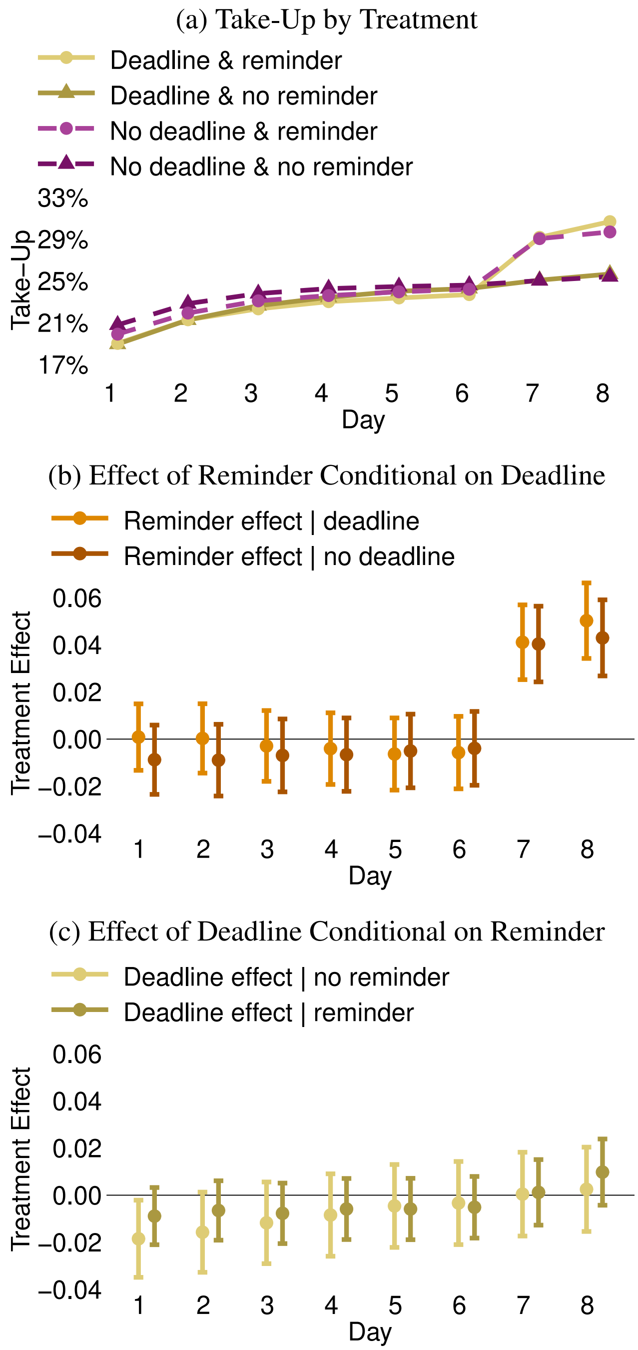
Note: This figure contains a histogram of the percentage of weekly sales made through the FinTech provider. Data comes from survey conducted on a random sample of firms in the experiment (N = 471) with 227 firms asked what their previous fee was. Survey question: *What share of your total pesos of sales 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.

Figure A.9: Heterogeneous Treatment Effects by Baseline Sales



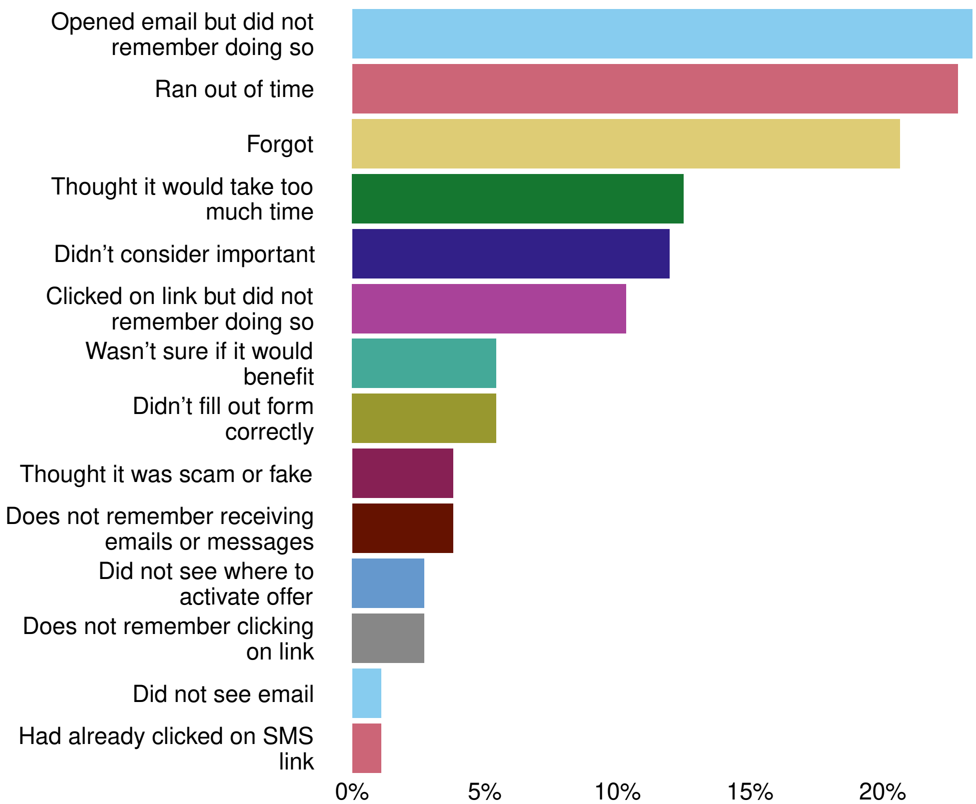
Note: This figure shows take-up and heterogeneous treatment effects by baseline sales by reminder, deadline and announced reminder groups. The unit of observation is at the firm level. Line graphs show average take-up rates by treatment and baseline sales group. Coefficient graphs show the corresponding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

Figure A.10: Effect of Reminder Interacted With Deadline



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

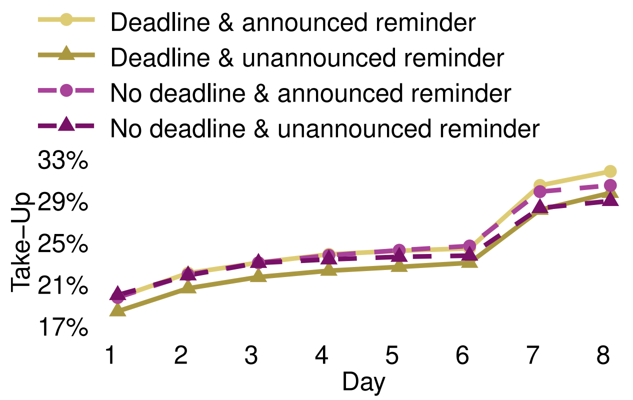
Figure A.11: Reasons Why Firms Did Not Adopt Offer



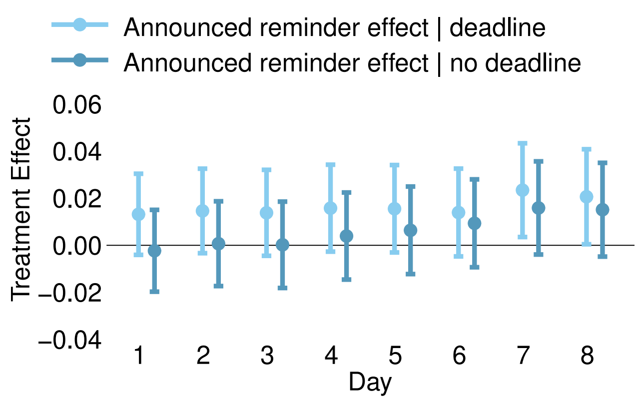
Note: This figure contains a barplot with the reasons given by firms for not adopting the offer. Data comes from survey conducted on a random sample of firms in the experiment ($N = 471$), and includes 169 firms that did not adopt the offer. The categories "Other" (7 firms) and "Did not answer" (8 firms) were omitted from the plot. Survey questions: *3.1. Our records show that you did not open the email. Why not?*; *4.1. What inconvenience did you have to click the link and finish the form?*; and *5.1. We observe that you did not complete the form after clicking the offer. Why did you not complete the form?*.

Figure A.12: Effect of Deadlines Interacted With Announced Reminder

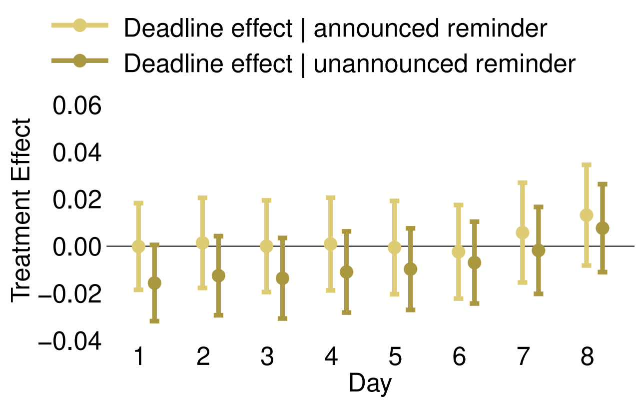
(a) Take-Up by Treatment



(b) Effect of Announced Reminder Conditional on Deadline

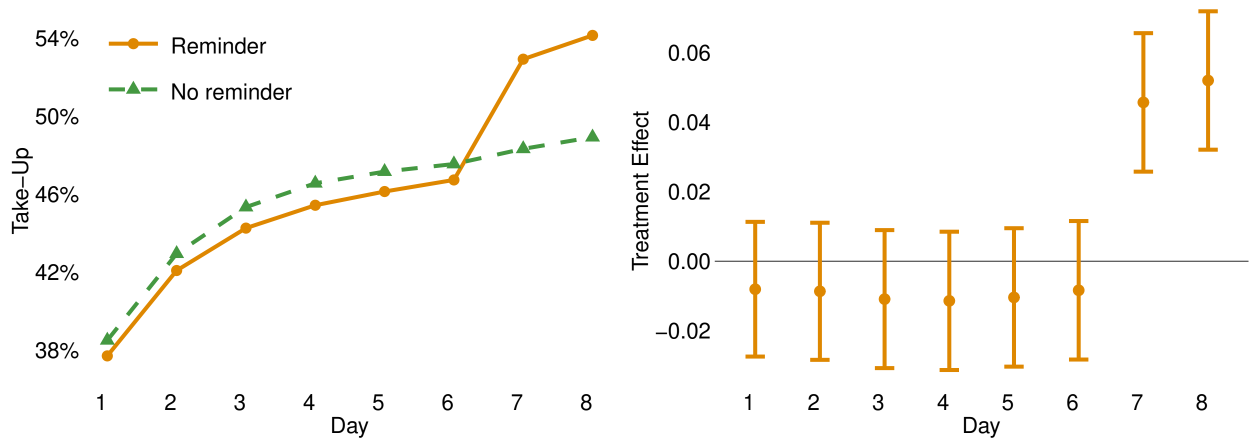


(c) Effect of Deadline Conditional on Announced Reminder



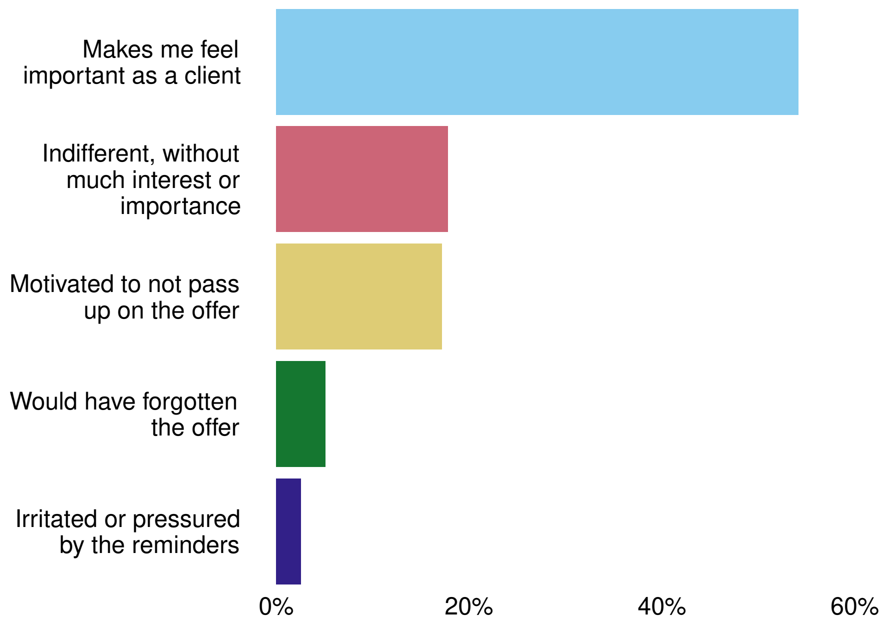
Note: This figure shows take-up and heterogeneous treatment effects by reminder and announced reminder groups. The unit of observation is at the firm level. Panel (a) shows average take-up rates by treatment group. Coefficient graphs show the corresponding coefficient estimates for the differential take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

Figure A.13: Effect of Reminder on Take-Up Conditional on Opening Email



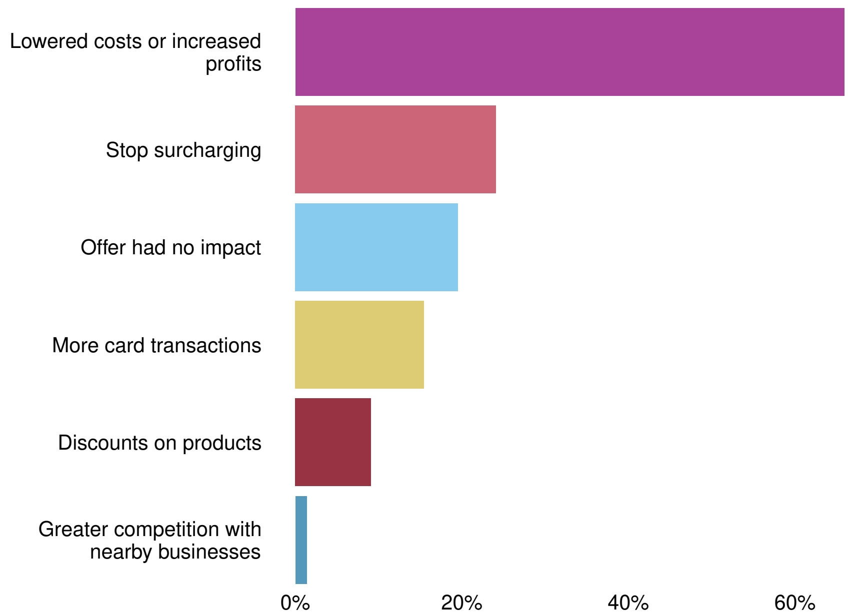
Note: This figure shows take-up and treatment effects of the unannounced reminder group for firms that opened the email before the day of the reminder. The unit of observation is at the firm level. The line graph shows average take-up rates by unannounced reminder group. The coefficient graph shows the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

Figure A.14: How Firms Felt About Receiving Reminder



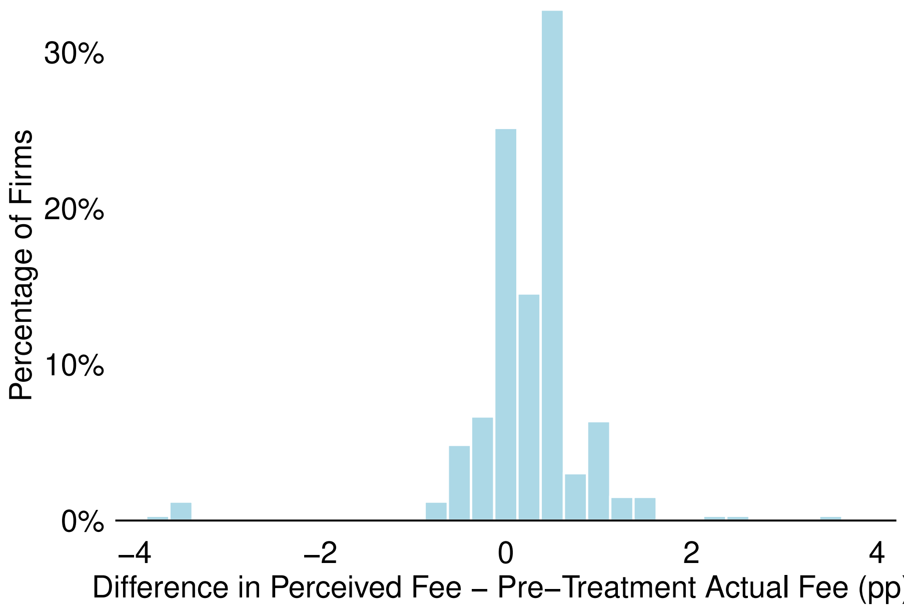
Note: This figure contains a barplot showing how firms felt when receiving the reminder. Data comes from survey conducted on a random sample of firms in the experiment ($N = 471$), with 157 firms asked this question. This question was asked to firms that recall receiving the first email or SMS, recall 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?* Respondents could provide more than one response, so totals add up to more than 100%. 23 firms were excluded from the sample, including 12 firms giving other responses, and 11 firms that did not know the answer to the question.

Figure A.15: Impact of Lower Fee Offer



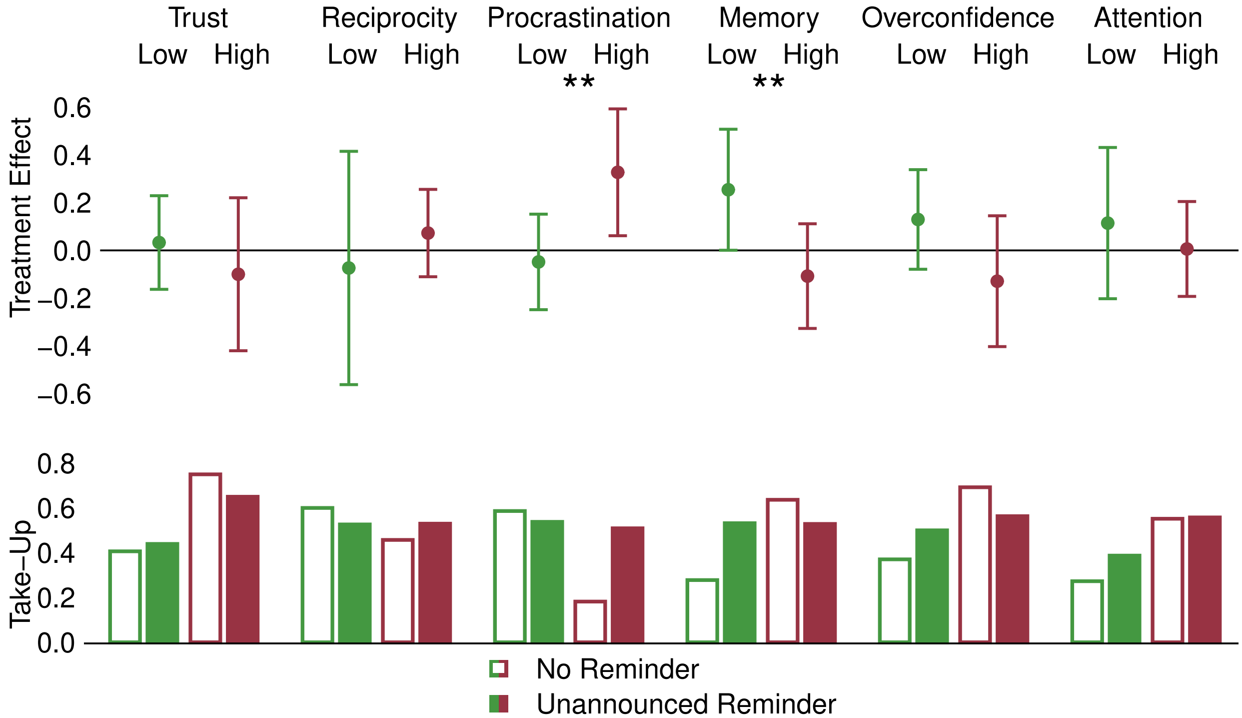
Note: This figure contains a barplot with the impact the lower fee offer had on firms. Data comes from survey conducted on a random sample of firms in the experiment ($N = 471$). with 248 firms asked this question. This question was asked to users who accepted the survey, and recalled accepting or clicking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

Figure A.16: Difference in Pre-Treatment Actual Fee and Perceived Fee



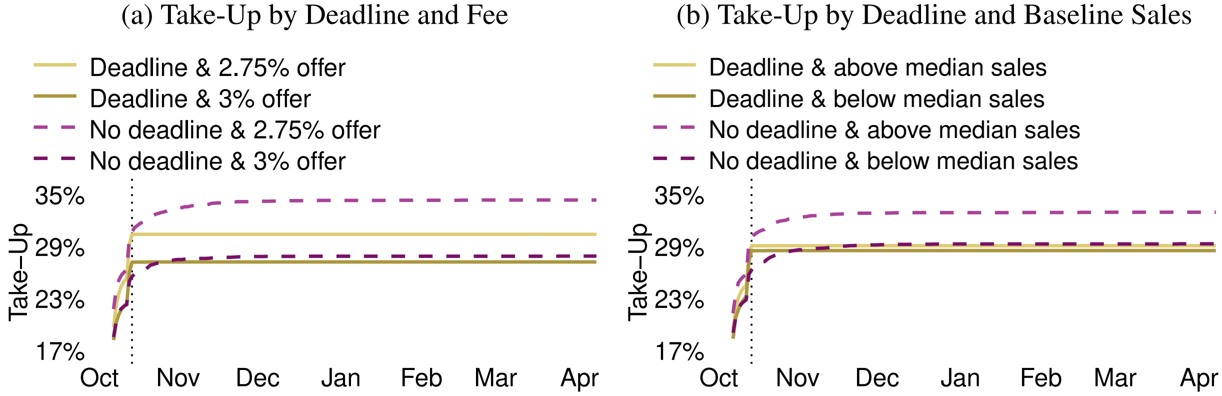
Note: This figure contains a histogram of differences in the pre-treatment fee and the fee firms perceive. Data comes from survey conducted on a random sample of firms in the experiment (N = 471) with 471 firms asked what their previous fee was. Survey question: *What was your commission with (provider) 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 A.17: Heterogeneous Effect of Unannounced 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 take-up on unannounced reminder, the survey measure, and the interaction between unannounced reminder and the survey measure. Data includes firms with unannounced reminders and no reminders in survey sample, and includes take-up from September 29 to March 31. All firms in the survey were asked these questions. The survey question asked respondents whether they agreed or disagreed with the following six statements: (1) *Trust*: I trust advertised offers. (2) *Reciprocity*: I am more inclined to do business with people who live up to their promises. (3) *Procrastination*: I tend to postpone tasks, even when I know it is better to do them immediately. (4) *Memory*: I tend to have good memory about pending tasks that I have to do and complete. (5) *Overconfidence*: I tend to think my memory is better than it really is. (6) *Attention*: I can focus completely when I have to finish a task. The scale of these responses is 1 to 5, where 5 is highest level of agreement and 1 highest level of disagreement. Binary measure variables were created from these responses, coding 4 and 5 (agree and completely agree) as 1 and 1-3 (completely disagree, disagree and neither agree nor disagree) as 0. 43 firms that did not answer the question were excluded from the sample. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure A.18: Long-Term Take-up by Deadline, Offer Value and Firm Size



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

B Proofs of Model Predictions

This appendix includes proofs of the model predictions in Section 2.3.

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

Proof. Taking derivatives of equation (1) with respect to α_t,

$$\frac{dc_t^*}{d\alpha_t} = \beta\delta y > 0, \tag{7}$$

so raising α_t increases threshold c_t^* and hence take-up. Taking derivatives of equation (1) with respect to y,

$$\frac{dc_t^*}{dy} = \beta\delta\left(\alpha_t - \hat{\rho}\frac{dE_t\left[\hat{V}_{t+1}\right]}{dy}\right). \tag{8}$$

To determine its sign, rewrite equation (2) using the fundamental theorem of calculus as

$$E_t\left[\hat{V}_{t+1}\right] = \int_0^{c_{t+1}^*}(\delta\alpha_{t+1}y - c)\,f(c)\,dc + (1-F(c_{t+1}^*))\delta\hat{\rho}E_t\left[\hat{V}_{t+2}\right]. \tag{9}$$

Taking derivatives of equation (9) with respect to y,

$$\begin{aligned}\frac{dE_t\left[\hat{V}_{t+1}\right]}{dy} =& \left(\delta\alpha_{t+1}y - c_{t+1}^*\right)f(c_{t+1}^*)\frac{dc_{t+1}^*}{dy} - f(c_{t+1}^*)\frac{dc_{t+1}^*}{dy}\delta\hat{\rho}E_{t+1}\left[\hat{V}_{t+2}\right] + \\ &(1-F(c_{t+1}^*))\delta\hat{\rho}\frac{dE_{t+1}\left[\hat{V}_{t+2}\right]}{dy}.\end{aligned}$$

The first two terms are zero by the envelope theorem, so

$$\frac{dE_t\left[\hat{V}_{t+1}\right]}{dy} = (1-F(c_{t+1}^*))\delta\hat{\rho}\frac{dE_{t+1}\left[\hat{V}_{t+2}\right]}{dy}. \tag{10}$$

Note that at the deadline $t = T$, $dE_T\left[\hat{V}_{T+1}\right]/dy = 0$, as there is no continuation value since the task expires. Plugging this into the right side of equation (10) for $t = T-1$, $dE_{T-1}\left[\hat{V}_T\right]/dy = 0$. Recursively, for all t, $dE_t\left[\hat{V}_{t+1}\right]/dy = 0$. Therefore, plugging this into (8),

$$\frac{dc_t^*}{dy} = \beta\delta\alpha_t > 0.$$

Thus, raising y or raising α_t increases the threshold c_t^* and hence increases take-up.

Prediction 2 (Reminder). A reminder increases take-up if firm owners are forgetful ($\rho_t < 1$).

Proof. Note that from (1), c_t^* only depends on $\hat{\rho}_\ell$ for $\ell > t$ and not on ρ_t. Hence ρ_t impacts the probability of adopting at time t only through its impact on the probability of the task being active in period t. For any period t,

$$\Pr(\text{task active at } t) = \prod_{j=1}^{t} \rho_j \prod_{k=0}^{t-1} (1 - F(c_k^*)),$$

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

Prediction 3 (Deadline). [In progress.]

Prediction 4 (Announced Reminder and Initial Pre-Reminder Take-Up). The announced reminder (a) reduces take-up at $t = 1$, compared to the unannounced reminder, if firm owners do not believe they have perfect memory ($\hat{\rho}_t < 1$), and (b) has no differential effect on take-up at $t = 1$ if firm owners believe they have perfect memory ($\hat{\rho}_t = 1$).

Proof. Taking derivatives of equation (1) with respect to $\hat{\rho}_t$,

$$\frac{dc_t^*}{d\hat{\rho}_t} = -\beta\delta E_t\left[\hat{V}_{t+1}\right] - \beta\delta\hat{\rho}_t \frac{dE_t\left[\hat{V}_{t+1}\right]}{d\hat{\rho}_t}.$$

Taking derivatives of equation (9) with respect to $\hat{\rho}_t$,

$$\begin{aligned}\frac{dE_t\left[\hat{V}_{t+1}\right]}{d\hat{\rho}_t} = &\left(\delta\alpha_{t+1}y - c_{t+1}^*\right) f(c_{t+1}^*)\frac{dc_{t+1}^*}{d\hat{\rho}_t} - f(c_{t+1}^*)\frac{dc_{t+1}^*}{d\hat{\rho}_t}\delta\hat{\rho}_t E_t\left[\hat{V}_{t+2}\right] + \\ &\left(1 - F(c_{t+1}^*)\right)\delta E_{t+1}\left[\hat{V}_{t+2}\right] + \left(1 - F(c_{t+1}^*)\right)\delta\hat{\rho}_t \frac{dE_{t+1}\left[\hat{V}_{t+2}\right]}{d\hat{\rho}_t}.\end{aligned}$$

The first two terms are zero by the envelope theorem, so

$$\frac{dE_t\left[\hat{V}_{t+1}\right]}{d\hat{\rho}_t} = \left(1 - F(c_{t+1}^*)\right)\delta E_{t+1}\left[\hat{V}_{t+2}\right] + \left(1 - F(c_{t+1}^*)\right)\delta\hat{\rho}_t \frac{dE_{t+1}\left[\hat{V}_{t+2}\right]}{d\hat{\rho}_t}. \tag{11}$$

Note that at the deadline $t = T$, $dE_T\left[\hat{V}_{T+1}\right]/d\hat{\rho}_t = 0$ as there is no continuation value since the task expires. Plugging this into the right-hand side of (11)for $t = T - 1$, $dE_{T-2}\left[\hat{V}_{T-1}\right]/d\hat{\rho}_t \geq 0$, since the first term on the right-hand side of (11) is (weakly) positive by definition of each component. Recursively, for all $t < T$, $\frac{dE_t\left[\hat{V}_{t+1}\right]}{d\hat{\rho}_t} \geq 0$. Therefore,

$$\frac{dc_t^*}{d\hat{\rho}_t} = -\beta\delta E_t\left[\hat{V}_{t+1}\right] - \beta\delta\hat{\rho}_t \frac{dE_t\left[\hat{V}_{t+1}\right]}{d\hat{\rho}_t} < 0. \tag{12}$$

Announced reminders increase $\hat{\rho_\tau}$ for the period τ in which the firm owner is told the reminder will arrive, as long as $\hat{\rho}_\tau < 1$. On the other hand, if $\hat{\rho_\tau} = 1$ even in the absence of the announced reminder, $\hat{\rho_\tau}$ cannot be increased. Thus, (a) if firm owners do not believe they have perfect memory, the announced reminder lowers the cost threshold c_t^* by equation (12), and (b) if firm owners believe they have perfect memory, $\hat{\rho}_\tau$ does not increase and hence there is no change in c_t^* or take-up.

Prediction 5. (Announced Reminder and Final Take-Up). The announced reminder (a) does not affect final (cumulative) take-up, compared to the unannounced reminder, if firms inherently trust the offer ($\alpha_t = 1$); and (b) increases final (cumulative) take-up if some firms distrust the offer, and their trust in the offer increases after receiving the announced reminder.

Proof. From Proposition 1,

$$\frac{dc_t^*}{d\alpha_t} = \beta\delta y > 0$$

so raising α_t increases the threshold c_t^* and hence take-up.

If (a) $\alpha_t = 1$ inherently, then α_t cannot increase from receiving the announced reminder, and hence there is no change in c_t^* and no difference in take-up between receiving an announced or an unannounced reminder. If (b) $\alpha_t < 1$ and the announced reminder increases α_t by increasing trust in the offer, then the threshold c_t^* increases. The higher threshold leads to an increase in post-reminder take-up.