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Do civil servants respond to behavioral interventions? A field experiment

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IDB WORKING PAPER SERIES N° IDB-WP-1218

Do Civil Servants Respond to Behavioral Interventions?

A Field Experiment

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Abstract

Introducing financial incentives to increase productivity in the public sector tends to be politically and bureaucratically cumbersome, particularly in developing countries. Behavioral interventions could be a low-cost alternative, both politically and financially, although evidence of their effectiveness remains scarce. We evaluate the effect of redesigning the notice requiring civil servants in Buenos Aires to comply with citizens' requests under Argentina's freedom of information act. The new notice, sent to the treatment group, attempts to exploit salience, deterrence, clarity, and social norms to increase adherence to deadlines. The results show an increase in the share of requests fulfilled by the second deadline, possibly because of a strong anchoring effect. These findings indicate that behavioral interventions can affect civil servants' actions. The fact that the intervention occurred at the same time as a civil service training program with sessions attended by members of both the control and treatment groups allows us to evaluate spillover effects. The evidence suggests that the time it takes a members of the treatment group to respond to a request increases with her interactions with members of the control group at the workshops. These findings have implications for policy design. First, they indicate that behavioral interventions could affect task compliance and productivity in the public sector. Second, they provide evidence that workshops may not always have the intended consequences, particularly when they increase interactions among employees with high and low incentives for task compliance.

JEL classifications: C93, D91, H11, H83

Keywords: Behavioral economics, Nudge, Civil servants, Freedom of information act, Public administration, State capacity, Incentives, Transparency, Anchoring

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1 Introduction

Countries with limited governmental capacity have access to a smaller set of public policies, face greater challenges in enhancing productivity and growth (Scartascini and Tommasi, 2010, 2014), and are at greater risk of delegitimization (Fukuyama, 2015). Raising task compliance and efficiency in the public sector, however, can improve the work culture in the public sector and promote investments in much-needed state capacities (Franco Chuaire, Scartascini, and Tommasi, 2017).¹ It can also increase trust in government institutions (Keefer and Scartascini, 2021), which is a must for improving demand for public policies and stimulating economic growth (Keefer, Scartascini, and Vlaicu, 2021).

Offering incentives to civil servants is a common way of improving performance in the public sector. However, coming up with the right incentives is not an easy task (Burgess and Ratto, 2003; Finan, Olken, and Pande, 2017). First, the public sector has unique features that often limit the implementation of pay-for-performance schemes (Weibel, Rost, and Osterloh, 2009). For example, linking compensation to performance can be challenging, as outputs are more difficult to measure than in the private sector, where pay may be based on sales performance (Dixit, 1997). Second, in large organizations, one principal must deal with multiple agents, which leads to free riding (Andolfatto and Nosal, 1997; Holmstrom, 1982). Third, civil servants in many developing countries are highly unionized, limiting administrators' ability to use financial incentives that discriminate among workers (Gindling et al., 2020).² For these reasons, efforts to improve task compliance can be politically and financially expensive.³

The use of behavioral insights could reduce the incidence of certain obstacles in implementing incentives in the public sector since most behavioral interventions have low political, financial, and administrative costs. In most cases, they do not require changes to the legal framework or union approval. They also have lower downsides, particularly that of creating negative incentives towards serving the public.⁴ There is some evidence that behavioral interventions induce employees to increase fuel efficiency in the airline industry (Gosnell, List,

¹Public sector workers represent 38% of formal wage employment globally and almost 70% in some countries (Hasnain et al., 2019). On average, they are paid more than their counterparts in the private sector, particularly in poorer nations (Panizza and Qiang, 2005; Mizala, Romaguera, and Gallegos, 2011; Hospido and Moral-Benito, 2016; Finan, Olken, and Pande, 2017; Nikolic, Rubil, and Tomić, 2017; Mancha and Enlinson, 2020) and usually enjoy more benefits and greater job security (Hasnain et al., 2019). However, fostering task compliance and productivity remains a challenge.

²Additionally, politicians tend to be averse to risk (Burgess and Ratto, 2003), and monitoring structures are likely to be embedded within the bureaucracy, leading to conflicts of interest (Bobonis, Cámara Fuertes, and Schwabe, 2016; Chu et al., 2021).

³Recent field experiments present evidence that financial incentives could positively affect hiring and performance but are not exempt from the risks mentioned above. Dal Bó, Finan, and Rossi (2013) note that higher wages attract more capable applicants and make public sector work more attractive. Exploiting this effect, however, depends on the administration's ability to screen candidates. Khan, Khwaja, and Olken (2016) show that performance-based pay for tax collectors has the potential to raise collection, but it may also increase collusion and bribes. Therefore, programs must be well designed to reduce the chances of unintended consequences (Imberman, 2015).

⁴Some cash-based incentive schemes can lead to rent extraction (Khan, Khwaja, and Olken, 2016).

and Metcalfe, 2016) and narrow the gap in compliance with school infrastructure investments (Dustan, Maldonado, and Hernandez-Agramonte, 2020). In many contexts, there are reasons to believe that nonfinancial incentives may work better than financial ones (Ashraf, Bandiera, and Jack, 2014; Khan, Khwaja, and Olken, 2019; Khan, 2020).

In this paper, we assess the effectiveness of an intervention that uses behavioral tools proven to be successful in changing behaviors. We do it in a challenging setting: boosting compliance with freedom-of-information (FOI) requests in the city of Buenos Aires, Argentina. FOI legislation establishes the right of citizens to have ready access to government records.⁵ The objective is to increase transparency and accountability, which are fundamental in a democratic society.⁶

Compliance with FOI requests is an area in which civil servants' incentives and the wishes of the people are contradictory—and this for several reasons: (i) increasing transparency and accountability could reduce civil servants' ability to shirk or cheat; (ii) fulfilling FOI requests increases workloads; (iii) civil servants must perform a task for which they were not hired, their abilities may differ from those needed to respond to the requests, and the evaluation of their performance may suffer accordingly; (iv) completion does not necessarily advance the individual interest of supervisors (Hazell and Worthy, 2010); and (v) responding to requests involves immediate costs but no immediate or future rewards.⁷

The field experiment, carried out in 2018 with the help of the municipal government, involved all requests for information submitted to all municipal agencies. The objective was to increase adherence to deadlines. We followed a stratified randomization procedure using historical data on FOI requests to assign agencies into treatment and control groups. The treatment emphasized the importance of the task and the deadlines. It also sought to lower the cost of compliance by clearly outlining the steps required to respond to requests. Finally, by displaying past compliance at the agency level, it increased the perceived cost of not complying by making the punishment more visible and signaling that monitoring was high.⁸

⁵More than a hundred countries have implemented some form of FOI legislation. In Latin America, these include Mexico (2002), Peru (2003), Chile and Uruguay (2008), Brazil (2011), Colombia and Paraguay (2014), and Argentina (2016). Buenos Aires passed its law in 1998 and revised it substantially in 2016.

⁶In the city of Buenos Aires, the law grants citizens the right to request and receive public information at no cost without disclosing the reasons behind the request. It establishes a deadline of 25 days for the city government to respond (15 days plus a 10-day extension). From January 2017 to March 2019, about half of the requests were answered by the first deadline and 78% before the second deadline. The remainder were either delayed or never addressed.

⁷Additionally, because each request is unique, and civil servants must rely on the specific features of the problem (singular information, as described by Kahneman and Tversky, 1982), it is difficult for them to predict how much time they will spend responding to a request. Furthermore, individuals may be hesitant to respond soon because they fear being assigned a second task once they finish the first one, or because it seems logical to extend the duration of the assignment to appear busy.

⁸Dusek, Pardo, and Traxler (2020) demonstrate, in the context of speeding tickets, that increasing the salience of the payment deadline is ineffective in improving compliance, but emphasizing both the deadline and the penalties increases payment rates significantly and lastingly, an effect considerably greater than that achieved by stressing only the late penalty. We anticipated that both factors would have an effect on civil servants' behavior.

The behavioral treatment was embedded in the notifications sent to public employees with citizens' requests. Those notifications incorporated salience, deterrence, clarity, and social norms.⁹ The control group continued receiving the same notice that had been used for several years.

We present evidence that the treatment affected civil servants' responses. It increased by about six percentage points the likelihood of a request being answered by the 25-day deadline. The new notifications made the deadline more salient, strengthening the anchoring effect and causing treated government officials to focus their compliance efforts on a precise day. The existence of an anchoring effect is also supported by the fact that the significant increase in requests met on the deadline was accompanied by an equally significant reduction in the number of requests answered in the days preceding the deadline. Because most of the effects come from a shift within the distribution of responses, we do not find an overall reduction in the number of days it took civil servants to respond to the requests or an improvement in compliance, as we had been expecting.

Because of the way the roll-out of the intervention took place, we can exploit the fact that treated agencies began receiving the new notifications while still having a stock of the old ones. Once the new notifications came into use, the share of requests met early went up, and the number of business days it took to meet them dropped. These results provide additional confidence in the effect of the behavioral intervention.

At the time of the intervention, the city government conducted a training program for civil servants to explain the objective and scope of the FOI law. A sample of civil servants from the treatment and control groups received this training.¹⁰ Offices whose civil servants attended training sessions were less likely to respond to FOI requests before the first deadline (by about 22 percentage points), and more likely to respond late (by 13.5 points). In other words, the training workshops seemed to *weaken* the effects of the treatment. We find a small effect of the treatment on requests received by agencies that attended workshops, and a strong effect on requests received by agencies that did not attend them. The (negative) effect of the training on response time is stronger among the control group than among the treatment group. For instance, control agencies that participated in the training workshops are less likely to answer within the first 13 days by almost 30 percentage points. The decline is not significant for the treatment group. Part of the effect of the training workshops on the response time may be direct, and part may be explained by negative spillovers from the control group to the treatment group. The workshops constitute an opportunity for interaction between treated and control individuals. The evidence indicates that a treated person is more likely to take longer to respond the more control individuals she interacts with at the workshops.

The results presented here have broad implications. First, they demonstrate that simple

⁹We opted for a kitchen sink approach that addressed all the issues identified in the diagnostic surveys and interviews with civil servants done prior to the intervention. We did not have enough power to test each insight individually.

¹⁰We show that there is no evidence of self-selection based on observables, despite the fact that training was not provided in a randomized way.

behavioral interventions can influence civil servants' conduct. The findings suggest that the stronger anchoring effect created by making deadlines more salient outweighed the expected reduction in compliance time that might have been achieved by the other factors (deterrence, clarity, and social norms). However, further research is needed to determine the exact effect of each of these mechanisms in the context of public administration, as well as to evaluate whether civil servants tend to finish on the last day purely because of the anchoring effect or as a result of greater effort to provide a higher-quality response.¹¹ Clearly, our intervention emphasized the deadlines and the relevance of meeting them, but it did not provide explicit incentives for accelerating compliance with requests.

Second, the evidence suggests that behavioral and traditional strategies to improve performance do not necessarily work as intended. Given that the sums spent on traditional training programs are several orders of magnitude larger than those required to design and implement a behavioral intervention, this result is not of minor significance. Additional research would be needed to understand the mechanisms and the reasons behind it. Importantly, the evidence seems to indicate the potential for spillovers or peer effects during the training sessions. As such, it may be relevant not only to design interventions to capture the extent of these externalities (to measure the "water-cooler effect"), but also for public officials to be aware of the consequences (positive and negative) of interactions between heterogeneous groups (e.g., those with high or low incentives) of civil servants.

The paper proceeds as follows: Section 2 describes the FOI ordinance in effect in the city of Buenos Aires. Section 3 details the intervention. Section 4 reports the data. Section 5 outlines the results of the treatment, and Section 6 those of the training program. Section 7 presents some tests used to assess the robustness of the effects. Section 8 concludes.

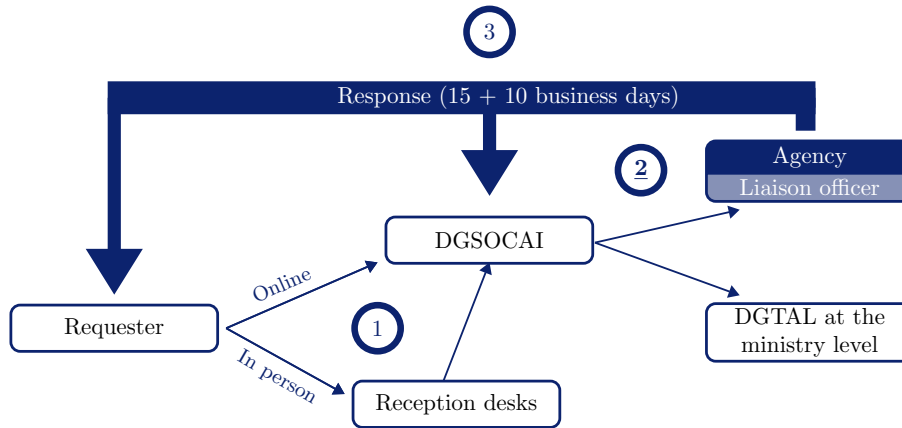
2 Freedom of Information in the City of Buenos Aires

Buenos Aires's FOI ordinance took effect on September 29, 2016, superseding previous provisions regulating access to public information in the city. It establishes that "*everyone has the right to request and receive complete, truthful, adequate, and timely information. It is not necessary to prove a subjective right, a legitimate interest, or the reasons that motivate the request to access public information.*" No fee is required, and requests may be filed online or in person at the reception desk of a city agency (*Mesas de entrada*). Filings are then transferred to the Directorate General of Monitoring of Control Bodies and Access to Public Information (*Dirección General de Seguimiento de Organismos de Control y Acceso a la Información pública—DGSOCAI*). Requests submitted online reach DGSOCAI directly, as shown in Figure 1.¹²

¹¹According to Ariely and Wertenbroch (2002), external deadlines, rather than self-imposed deadlines, encourage compliance. Thus, the following question arises: What externally imposed deadlines are most effective in increasing public servant compliance? Owing to the constraints set by the legislation, it is challenging to perform an experiment that manipulates deadlines in this setting.

¹²The form citizens must complete when submitting a request online is shown in Figure A.1. Filers must include their first and last names, an address in the city, an e-mail address at which to receive the response,

Figure 1: Procedure for processing an FOI request



Source: Authors' elaboration.

Notes: In step ①, the request for information is submitted in person or online, and it reaches DGSOCAI in both cases. Our intervention occurs in step ②. DGSOCAI sends the notice to the most competent agency in the treatment or control group to respond to the FOI request. It also sends the notice to the general administrative and legal-technical office of the relevant ministry. In the last step, step ③, the agency sends the response to the requester and DGSOCAI.

When DGSOCAI receives the citizen's request, it analyzes which agency is the most competent to respond and forwards the notification.¹³ It simultaneously forwards the claim to the general administrative and legal-technical office of the relevant ministry (DGTAL). Under the legislation, agencies are to reply within 15 business days of receipt of the request. The time limit for a response may be extended by 10 business days if the agency has trouble obtaining the requested information. Each agency has a liaison assigned by DGSOCAI to ensure that deadlines are met. In most cases, this individual is responsible for receiving, processing, and responding to requests. In others, the task is shared and may be assigned to different members of the staff depending on the content and complexity of the request.

Once the agency receives the request from DGSOCAI, it determines whether the record falls within its mandate and then begins to search for the requested information. Before the first deadline (15 business days), the agency must inform DGSOCAI and the requester whether it has a response ready, needs an extension, or proposes a discussion in which the parties can agree on a new delivery deadline beyond the official 10-day extension. Once the agency compiles the information and replies, DGSOCAI examines the record for completeness, archives it, and updates the information in the database that is uploaded to the online platform of transparency.

If the citizen deems the response incomplete or unsatisfactory, or if the agency has not met the deadlines, the citizen has two options. She can file a complaint with OGDAL (*Órgano Garante del Derecho de Acceso a la Información*), which ensures that the FOI legislation

a phone number, and a detailed description of the requested information.

¹³Figure A.2 depicts the notification that civil servants would receive before the intervention (and that the control group kept receiving), as well as the redesigned notification that treated agencies began receiving.

is followed, or initiate an *amparo* in city court.¹⁴ A choice to exercise the second option precludes a contemporaneous or subsequent filing with OGDAL.

3 Intervention

The deadlines and procedures established by the enabling legislation seemed to be straightforward. What, then, was preventing civil servants from fulfilling requests on time? The city conducted a diagnostic as part of the intervention’s design, surveying and interviewing a sample of employees (liaison officers) from 15 agencies to assess their knowledge of the law and its implementation. One of the main conclusions was that many civil servants lacked a thorough understanding of the deadlines and procedures. In addition, they had not internalized the fact that complying with the law was an explicit objective of the city government. Instead, they were more aware of the risk they could face by disclosing information to citizens that their superiors might not want to become public.

The intervention was designed to address some of the issues identified in the diagnostic. Especially important were ensuring that civil servants fully understood the contents of the notification they received and the importance to the city of increasing compliance with the FOI provisions. They were to be made aware of the procedures to be followed in responding to requests, the importance of the stated deadlines, the existence of active monitoring, and the consequences of responding late. We decided that the best way to achieve these objectives was to redesign the notices that DGSOCAI sent to agencies when forwarding citizens’ requests.¹⁵

We employed a stratified randomization procedure using historical data to assign agencies into treatment and control groups.¹⁶ The treatment group agencies began receiving the redesigned notifications on September 6, 2018, while the control group agencies continued receiving the existing notifications.¹⁷

The redesigned notice included several components known from the literature to increase compliance with the law (Cabinet Office Behavioural Insights Team, 2012; L. Castro and

¹⁴An *amparo* is a judicial action that protects citizens’ constitutional rights.

¹⁵Neither of the deadlines provided under the FOI regulations (the first being 15 business days and the second 25 business days) were changed in the redesigned notice, nor was the request itself (that is, the information demanded by the citizen) ever modified in any way. The intervention changed only how civil servants were notified of citizens’ requests.

¹⁶The stratified randomization of the treatment at the agency level was based on the number of requests received between January 2, 2017, and July 26, 2018. Table B.1 shows the number of agencies in each stratum. We randomized at the agency level for two reasons. First, randomizing at the request level did not ensure compliance with treatment assignment. Second, spillovers were more likely if an agency received both treated and control requests; the effects of those spillovers would have been difficult to disentangle, whereas the strategy adopted allowed us to measure them. The number of agencies in each stratum, by treatment condition, is shown in Table B.1.

¹⁷Figure A.2 depicts the notifications that the control and the treatment groups received during the intervention.

Scartascini, 2014; Behavioral Insights Team, 2014; E. Castro and Scartascini, 2019). The first is salience, which has been proven to influence tax compliance (Chetty, Looney, and Kroft, 2009; E. Castro and Scartascini, 2019), consumer behavior (Stango and Zinman, 2014), and selection into the civil service (Ashraf et al., 2020). The urgency of responding to the request appears at the top of the redesigned notification; the word “*Urgente*” (Urgent) is emphasized, and the title of the law is displayed in white capital letters (see Figure A.2). Civil servants’ attention is drawn to this portion of the message and conveys the importance of the matter (Taylor and Thompson, 1982). The notification also prominently informs the official about the law’s content and the number of days in which the request must be answered.

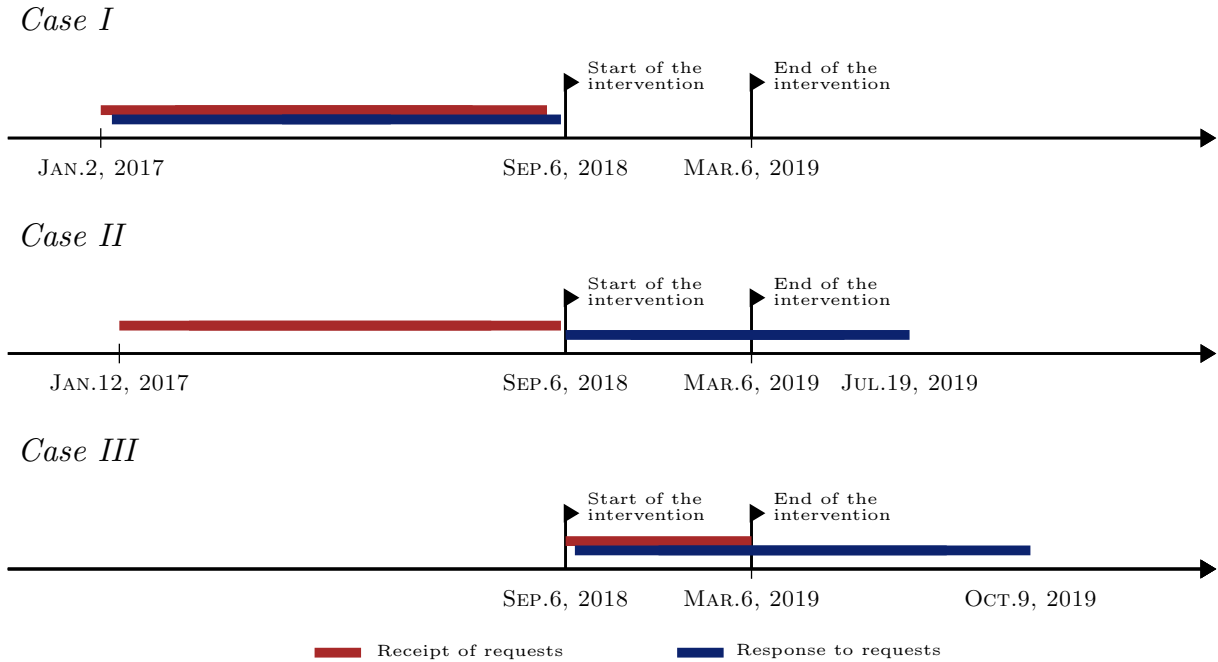
The second component is deterrence. Deterrence messages have been shown to improve compliance in the payment of speeding tickets (Dusek, Pardo, and Traxler, 2020) and taxes (Kleven et al., 2011; Pomeranz, 2015; Hallsworth et al., 2017; Chirico et al., 2019; Lopez-Luzuriaga and Scartascini, 2019). On the treatment group’s notice, deterrence takes the form of an alert sign and words setting forth the consequences of obstructing access to information or delivering an incomplete response without justification. The text emphasizes that a failure to respond or an inadequate answer may lead to punishment or termination. The notification sent to the control group also includes the consequences of noncompliance, but only in the eighth item. And it does not highlight the potential loss of income. This detail is important because the changes we incorporate in the notification sent to the treatment group do not increase the amount of information, but they are designed to trigger loss aversion.

Clarity is the third feature added to the notice. The new notification clearly details the steps that civil servants must follow to respond to a request. It also uses different colors and a new format that makes it easier for the recipient to read all the information contained in the notice. Fourth, it also includes personalized agency-level feedback on performance over the previous year.¹⁸ It shows the share of requests answered in time and the share not answered within the time limits. The way the message is framed highlights the negative outcome. This feedback provision signals that monitoring is high, which potentially increases the perception of consequences for noncompliance.

Finally, at the bottom, the notice highlights the fact that access to information is a human right, and civil servants should help ensure that right is defended. Previous research has shown that “nudging” about social norms can affect behavior (Jacobson, Mortensen, and Cialdini, 2011; McDonald and Crandall, 2015). In particular, Dustan, Maldonado, and Hernandez-Agramonte (2020) present evidence about the decisive role of social norms in inducing civil servants to submit timely expense reports. In our case, the message attempted to trigger acquiescence with an injunctive social norm, as described by Cialdini, Kallgren, and Reno (1991), on the importance of following the law.

¹⁸To our knowledge, there is no literature related to provision of feedback at aggregate levels (at the agency level in this case) in the context of public administration.

Figure 2: Timing of the intervention and requests



Note: Requests still pending on November 27, 2019 are not included.

Because the intervention was launched within the confines of an existing program, the agencies in the treatment and the control groups had been receiving citizens' requests for several years, and both used the same notifications until September 5, 2018. Once the intervention began on September 6, the treatment group received the redesigned notification, and the control group continued using the old notices. Figure 2 shows, at the request level, the three types of cases that fit within the intervention's timeline. For both treatment and control groups, some requests were received and answered before the intervention started (*Case I*); others were pending when it started (*Case II*). Only after the intervention began did the new notifications come into use (*Case III*). For some time, therefore, the treatment group had a stock of old notifications to answer while it was receiving new notifications. The variety of cases is important because it affects the way we can estimate the impact of the interventions and provides an opportunity to study spillover effects.

4 Data

We use the full public database of FOI requests submitted between January 2, 2017, and March 6, 2019.¹⁹ Individual requests are our unit of analysis. We have information on their

¹⁹The original database contained 7,568 requests. Our final dataset has 6,896 observations. We eliminated 266 denied requests and 406 other cases received by agencies that handled requests only in the pretreatment or post-treatment stages. As a result, only agencies that received requests both before and after the intervention are included in the analysis.

submission dates and subjects, the agency assigned to respond, whether the agency belongs to the treatment or the control group, and the date the request was fulfilled. There are 121 agencies included in the analysis; 62 in the control group and 59 in the treatment group. There are 3,785 observations before and 3,111 after the intervention began. (Descriptive statistics are presented in Table B.2.)²⁰ The requests were grouped into 15 categories based on the subject of the request. On average, each agency received requests related to about 4 of the 15 topics during the entire period of the analysis.²¹

One of the causes of delay mentioned in the interviews was a large backlog caused by multiple requests. Since we had the requests' submission dates, we could identify the number of requests received by an agency on a given date. It turned out that 38.4% of the requests were the only ones received by agency on that day; 18.4% were received on the same day as another request; and 43.2% arrived on a day when the agency received three requests or more. We also constructed three variables based on the share of requests the agency had fulfilled when a new one arrived. One variable is the number of closed requests over the total number of requests received until the day on which the new one arrived. The second is the share of requests still pending (but not yet late). The last is the proportion of requests that were both pending and late (backlog).²²

Considering the whole sample, before and after the intervention, when an agency received a new request, on average, 71.5% of its requests had been answered; 23.6% were still pending but not yet late; and 4.9% were already past the final deadline.²³ We include the last two percentages as controls because they could have affected the agencies' ability to respond to the new requests.

As indicated in Figure 2, there are three possible types of requests. *Case I* comprises requests received and closed before the intervention started: 3,288 submissions were received between January 2, 2017 and September 4, 2018. Agencies responded to these requests between January 6, 2017 and September 5, 2018. *Case II* comprises 497 submissions received

²⁰As of November 27, 2019, the date of the last update of the database for purposes of our analysis, 96 requests from the pretreatment stage and 104 from the post-treatment stage were still pending. All 200 had already passed the maximum number of days allowed by law (25 business days). Once we drop those requests, we are left with a sample of 120 agencies (62 in the control group and 58 in the treatment group). We have 3,689 observations before the intervention and 3,006 after it started. The additional observation that was removed belonged to the treatment agency that was dropped, because it received only one request after the intervention began, and that request was pending. Appendix B provides additional details on the dataset.

²¹The most common subjects before the intervention were permits, inspections and infrastructure (33.58%); public administration (14.95%); public spaces (8.93%); and financial accounting (7.37%). Most common after the intervention were permits, inspections and infrastructure (20.28%); public administration (19.58%); financial accounting (14.18%); labor regulations (10.67%); and public spaces (10.48%).

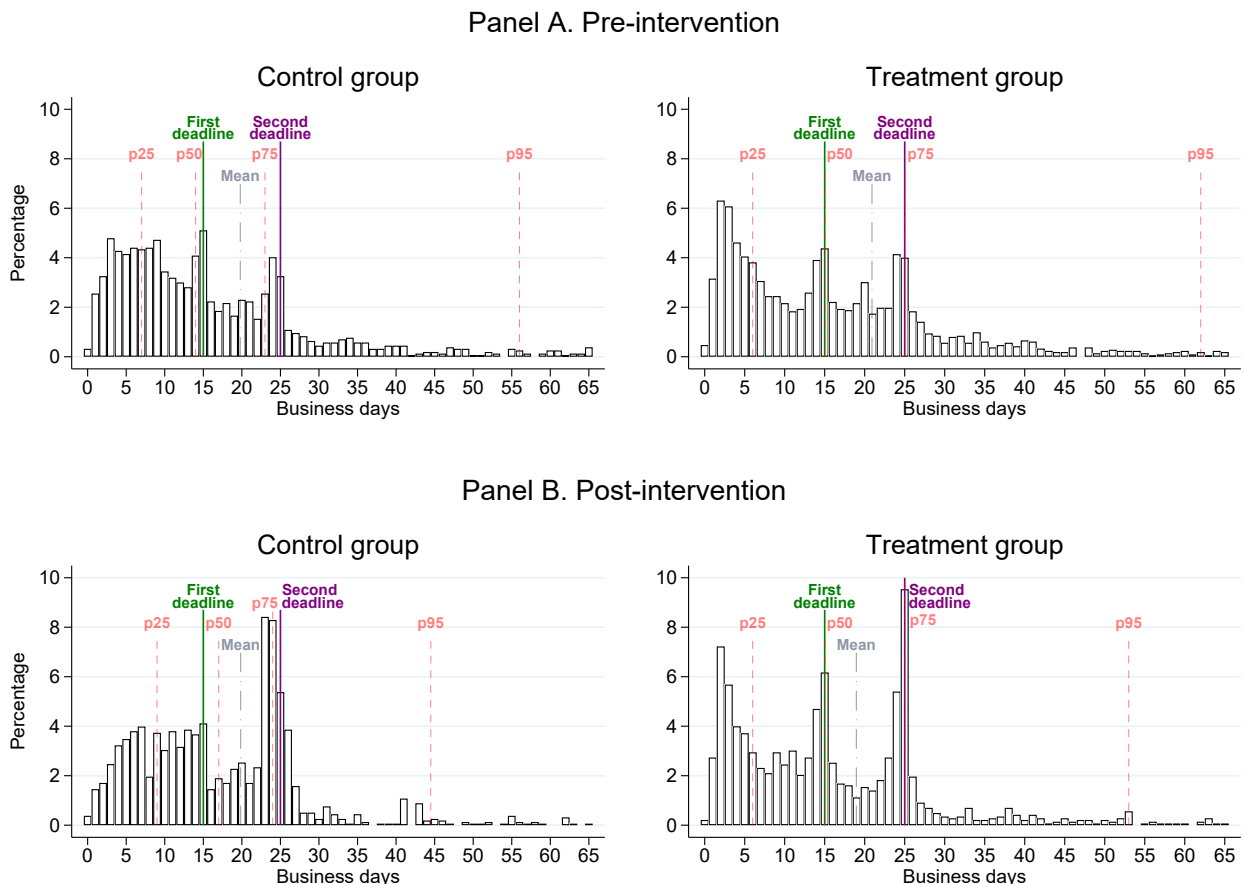
²²The percentages are based on the total number of requests received by the agency from January 2017 until the day request i was received. When an agency receives a request, it has a certain number of pending and closed requests. Those pending are either late or not. The distribution of the total number of requests received by an agency up to date d is: $\frac{Closed + Open_{<25} + Backlog_{>25}}{\text{Total number of requests up to date } d}$.

²³Before (after) the intervention: 70.1% (73.3%) of requests had been answered when a new request arrived at the agency; 24.3% (22.6%) were still pending but not yet overdue; and 5.6% (4.1%) were already past the final deadline.

before the intervention started, between January 12, 2017 and September 5, 2018. These requests were answered after the intervention had already begun (between September 6, 2019, and July 19, 2019). Finally, 3,111 requests received between September 6, 2018 and March 6, 2019 fall into *Case III*. Agencies responded to these requests between September 10, 2018 and October 9, 2019.

Figure 3 presents the distribution of responses by treatment group in terms of the number of business days it took agencies to respond. Panel A shows the distributions of responses for both the control and treatment groups before the intervention began. The distributions are statistically equivalent, as are the averages. Before the intervention, the control had responded to about 50% of requests by the first deadline (the *median-p50* and the *first deadline* are almost equal); the treatment group had completed the same proportion by day 15. The average number of business days (*mean*) to respond was 19.8 for the control group, and 20.9 for the treatment group.

Figure 3: Distribution of responses (in business days)



Notes: Less than 5% of requests take more than 65 business days to be answered. This graph shows only closed requests. Using the Mann-Whitney two-sample statistic, we cannot reject that the control and treatment groups are from populations with the same distribution before the intervention ($p\text{-value} = 0.2483$). However, following the intervention, the hypothesis is rejected ($p\text{-value} < 0.0001$).

Panel B displays the distributions after the intervention. As can be observed, it took

three additional days for the control group to complete half of the requests (*p50*), while the performance of the treatment group remained the same. The average number of business days (*mean*) before response remained the same for the control group, while it decreased by two days for the treatment group. The graph also shows a substantial increase in the proportion of requests met by the second deadline for both groups and a greater response rate around the first deadline for the treatment group.

Externally imposed deadlines are strongly correlated with completion times (Buehler, Griffin, and Ross, 1994; Ariely and Wertenbroch, 2002; Paola and Scoppa, 2015), which explains why, even before the intervention, we find spikes at both deadlines. Also, according to Parkinson’s Law (Parkinson, 1957), agencies will finish at the 15- and the 25-day mark since “work expands to fill the time available.” Giving people additional time, as is the case here with the extension period, affects their perceptions of the difficulty of the task (Goswami and Urminsky, 2020), and they take more time to complete it (Aronson and Gerard, 1966).²⁴

Before the intervention, the distributions for the control and treatment groups are statistically equivalent. However, following the intervention, the null hypothesis is rejected (p-value < 0.0001). Our preliminary results indicate that the intervention affected the responses of the treatment group. Additionally, we observe a relevant change in the control group’s behavior after the intervention, particularly around the second deadline. This change is likely explained by the training program, as we discuss in Section 6.

5 Empirical Analysis

To analyze the effect of the intervention on agency performance, we estimate the following regression model:

$$Y_{ij} = \alpha + \beta Treatment_j + X'_{ij}\delta + u_{ij} \quad \text{for } t = 1 \quad (1)$$

where i represents the request, j denotes the agency, and t indicates time (0 before the intervention and 1 after the intervention). We estimate equation (1) using a linear probability model and cluster the standard errors at the agency level.²⁵ Y represents six dependent variables: (i) the number of business days taken by agency j to respond to a request i ; (ii) in-time response (within the first 15 business days); (iii) response on day 15 (first deadline); (iv) response within the extension period (between 16 and 25 business days); (v) response on day 25, and (vi) late response (i.e., after day 25). The dependent variables in cases (ii) through (vi) are binary. They take the value of one if the response to the request was within the corresponding interval and zero otherwise. $Treatment_j$ equals one if the request was received by an agency j assigned to the treatment group.

X is a vector of observed characteristics of the agency and the request that vary over

²⁴In both laboratory and field settings, the association between delay, performance, and deadlines has been thoroughly examined (Peters et al., 1984; Damgaard and Nielsen, 2018).

²⁵According to Abadie et al. (2017), this is the right level at which to cluster the standard errors given that a cluster of requests is assigned to treatment.

time.²⁶ It includes the month in which the agency received the request, the topic of the request, a categorical variable for the total number of requests received by the agency on the same date as request i , the percentage of pending requests (not past the deadline), and the percentage of late requests (backlog) to that date. Importantly, as agencies reported some vexatious requesters, we incorporate a categorical variable in X to indicate the number of times the same person files a request that reaches the same agency.²⁷ Finally, u_{ij} is the error term. The main coefficient of interest is β , which accounts for the effect of the new notification on the outcome.

5.1 Official deadlines

Table 1 presents the results for the six dependent variables. The first noticeable aspect is that when we add the control variables calculated for each date and agency based on the information drawn from the interviews, the estimate of the effect of the intervention changes and becomes significant on day 25. The treatment raises by 6.1 percentage points (pp) the probability of responding to a request exactly on the second deadline (column [10]). This change represents an increase of 160% with respect to the mean in the pretreatment stage. (Before the intervention, agencies in the treatment group completed 3.86% of requests on day 25.) The table also shows that the level of compliance did not increase. If that had been the case, the coefficient in column [12] would be negative. It is important to mention that compliance with the second deadline was already high before the intervention: 78% of requests were already answered by the second deadline before the intervention started.²⁸ The results in terms of compliance may be affected by a ceiling effect. We find no significant effects for the number of business days or for the rest of the dependent variables. These results provide the first indication that the treatment increased the probability that civil servants would target a specific deadline instead of answering faster.

As expected, when individuals submit an excessive number of requests, agencies incur longer delays. Requests filed by someone who has submitted more than four take 3–4 days longer to be addressed than requests submitted by someone requesting for the first and only time. Agency performance is also correlated with the number of requests received on a given day. Table 1 shows that a request received on a day in which the agency has to process 3–10 claims is answered 2.6 business days more quickly than if it had been the only request the agency received that day. This result may reveal some non-linearities stemming from specialization and organization within agencies, though the gains disappear once the daily

²⁶Once we had analyzed the diagnostic surveys and interviews in detail, we were able to construct variables at the agency level for each date on which a request was received. We incorporated them in the regressions to increase precision. Table B.3 shows the balance test for these covariates.

²⁷In the data set, we identify “serial requesters” as those who submitted more than thirty requests between January 2017 and March 2019. Requests from serial requesters make up 41.3% of the data. Figure B.1 in the Appendix shows that one of them submitted 1,365 requests (18% of the total number of requests). Before the intervention, both groups, control and treatment, received approximately 21% of the total amount of requests from serial requesters. After the intervention, 71% of requests received by the control group came from serial requesters; 53% of those received by the treatment group came from such requesters.

²⁸The shares of responses in each interval of days before the intervention are presented in Table B.4.

requests number more than 10. The backlog and the percentage of pending requests that have not exceeded 25 business days delay the process only marginally. This delay results in a small decrease in the rate of early responses during the first 15 days.

Table 1: Agencies' response to requests (official deadlines)

	Business days		0-15		15		16-25		25		>25	
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
Treatment	-0.895 (3.591)	0.188 (1.730)	0.044 (0.134)	-0.010 (0.052)	0.017 (0.021)	-0.006 (0.010)	-0.079 (0.087)	-0.010 (0.042)	0.037 (0.032)	0.061** (0.027)	0.035 (0.063)	0.020 (0.028)
Same person: twice		0.740 (1.139)		-0.048 (0.034)		-0.001 (0.014)		0.010 (0.034)		0.036** (0.016)		0.039 (0.028)
Same person: three times		1.929 (2.200)		-0.066 (0.047)		-0.002 (0.019)		-0.016 (0.034)		0.005 (0.026)		0.083** (0.040)
Same person: 4 to 10 times		3.800** (1.486)		-0.096** (0.039)		-0.019 (0.017)		0.040 (0.031)		0.009 (0.011)		0.056** (0.027)
Same person: 11 to 20 times		2.701** (1.202)		-0.121*** (0.043)		-0.005 (0.021)		0.116*** (0.044)		0.050* (0.026)		0.004 (0.022)
Same person: > 20 times		4.151** (1.835)		-0.160*** (0.057)		-0.030 (0.021)		0.064 (0.044)		0.046 (0.029)		0.096*** (0.032)
2 requests on a single day		-0.636 (1.143)		0.030 (0.026)		0.004 (0.013)		-0.007 (0.027)		0.000 (0.014)		-0.023 (0.021)
3 to 10 requests on a single day		-2.623*** (0.986)		0.058** (0.026)		0.021 (0.018)		-0.033 (0.028)		-0.027* (0.014)		-0.025 (0.020)
> 10 requests on a single day		0.418 (2.069)		-0.017 (0.075)		-0.012 (0.020)		0.042 (0.076)		0.015 (0.055)		-0.026 (0.043)
% Pending requests		0.048 (0.042)		-0.003** (0.001)		-0.000 (0.000)		0.002* (0.001)		0.000 (0.000)		0.001 (0.001)
% Backlog		0.225 (0.188)		-0.008*** (0.002)		-0.001* (0.000)		-0.005** (0.002)		-0.003*** (0.001)		0.013*** (0.001)
Constant	19.844*** (1.570)	16.557*** (2.164)	0.474*** (0.086)	0.710*** (0.065)	0.041*** (0.010)	0.112 (0.093)	0.357*** (0.059)	0.198*** (0.055)	0.053*** (0.013)	-0.026 (0.017)	0.170*** (0.030)	0.092** (0.046)
Observations	3,006	3,006	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111
R-squared	0.000	0.082	0.002	0.194	0.002	0.055	0.007	0.113	0.005	0.054	0.002	0.128

Notes: * p < 0.10, ** p < 0.05, *** p < 0.01. Standard errors clustered by agency are shown in parentheses. The sample in the first two columns includes only requests closed by November 27, 2019 (see Tables B.2, B.3, and B.4 for further details on the sample). Control variables include: topic, month in which the agency received the request, serial requesters, number of requests received by the agency on the day request i arrived, agency backlog, and percentage of pending requests not yet overdue.

5.2 Flexible deadlines

In Table 2 and Figure 4, we explore more flexible deadlines, accepting a one-day window around the first and the second official deadlines (days 14 to 16, and 24 to 25, respectively). This seems reasonable based on the histograms presented in Figure 3 and the fact that focusing on the deadlines has a degree of randomness to it.²⁹ We include three days in the first case since agencies can always request an extension of 10 business days after the initial

²⁹Figure 3 shows a significant increase in the proportion of FOI requests fulfilled around the second deadline for the control and treatment groups after September 6, 2018. In particular, the increment seems substantial for business days 24 and 25. We also examine the days around the first deadline, because the histogram displays a rise in the proportion of requests answered by the treatment group around day 15.

deadline, and DGSOCAI usually grants it. However, a request that takes more than 25 days to be fulfilled is already considered overdue FOI.

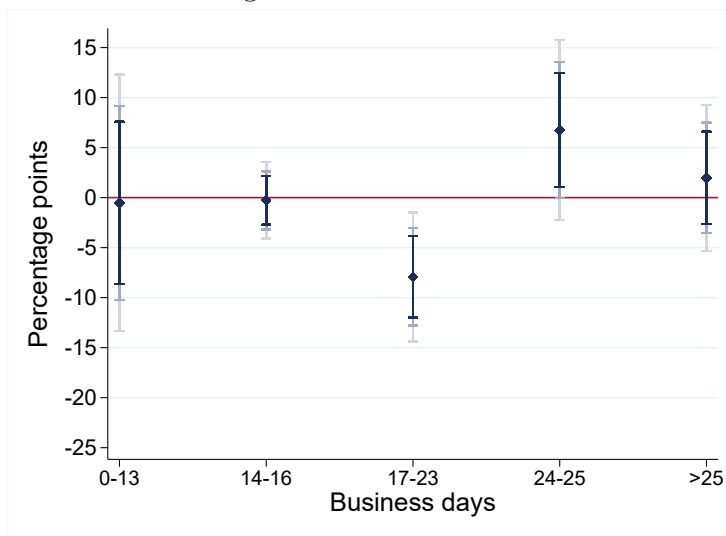
Table 2: Agencies’ response to requests (flexible deadlines)

	0-13		14-16		17-23		24-25		>25	
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]
Treatment	0.019 (0.137)	-0.005 (0.049)	0.035 (0.036)	-0.003 (0.015)	-0.094** (0.037)	-0.079*** (0.025)	0.005 (0.070)	0.068* (0.034)	0.035 (0.063)	0.020 (0.028)
Constant	0.397*** (0.073)	0.577*** (0.077)	0.091*** (0.020)	0.116 (0.094)	0.207*** (0.028)	0.257*** (0.050)	0.135** (0.056)	-0.042 (0.028)	0.170*** (0.030)	0.092** (0.046)
Observations	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111
R-squared	0.000	0.193	0.003	0.079	0.016	0.054	0.000	0.115	0.002	0.128
Controls	✗	✓	✗	✓	✗	✓	✗	✓	✗	✓

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by agency are shown in parentheses. Control variables include: topic, month in which the agency received the request, serial requesters, number of requests received by the agency on the day that request i arrived, agency backlog, and percentage of pending requests not yet overdue.

In this case, the increase in the probability of responding on the second deadline is 6.8 pp (column [8]). This finding comes from a notable change on day 25, as shown in Table 1. This result is largely explained by a lower probability (7.9 pp) of seeing a response during the extension period (business days 17–23).

Figure 4: Treatment effect



Notes: The coefficients displayed in this figure correspond to the variable *Treatment* in Table 2 when the controls are included. The color intensity of confidence intervals represents the confidence level (from darker to lighter, 90%, 95%, and 99%).

These results show that the intervention did not substantially alter the overall distribution of responses, but it did exacerbate the anchoring effect. It made the final deadline more prominent, and civil servants responded to the intervention by increasing their adherence to it. Our results are in line with the literature on nudges: average treatment effects tend to fall between 1.7 and 11.7 percentage points in interventions with similar characteristics to the one presented here (Hummel and Maedche, 2019; DellaVigna and Linos, 2020). Moreover,

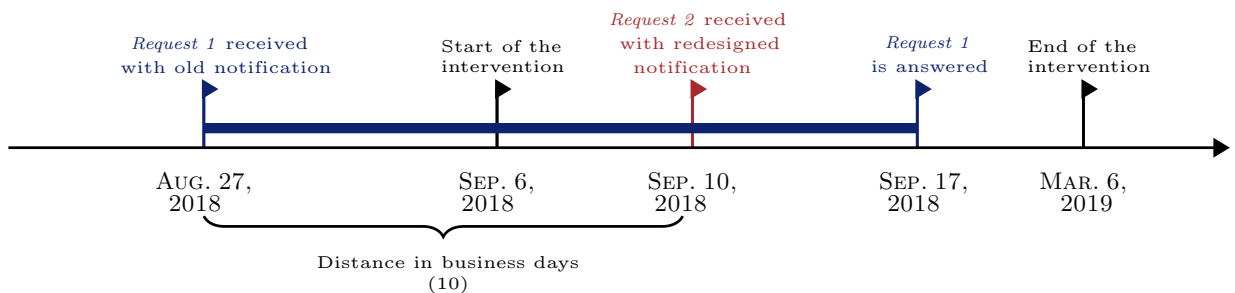
results around the lower bound are reasonable when interventions aim to improve government services (DellaVigna and Linos, 2020). Given that the randomization was done at the agency level, and the number of agencies was small (121 or 120 depending on whether pending requests are included or not), we conducted power calculations for each dependent variable considered in the study (see Appendix C). These calculations demonstrate that the sample size makes it difficult to achieve significant effects. In particular, an effect lower than 4 pp would generally not be detected, a challenging limitation for a behavioral intervention of this type.

In terms of days, only a reduction of two days or more could be identified as significant. The estimations of the minimum detectable effect (MDE) presented here are in line with the results of DellaVigna and Linos (2020). While those author find an MDE of around 0.8 percentage points for interventions with large sample sizes and 6.3 pp for small sample sizes, we estimate an MDE of 2.3 to 5.7 pp for the nine binary dependent variables we analyze (see Table C.1). As such, our point estimates and significance levels seem to be consistent with an intervention subject to institutional constraints and small sample sizes.

5.3 Spillovers within treated agencies

On September 6, 2018, we launched the redesigned notifications. Because some agencies in the treatment group still had pending requests on that date, we can explore whether the new notices sent to them affected their behavior when dealing with old requests. To accomplish that, we create a variable that identifies the distance, measured in business days, between the date of receipt of the old request and the date on which the first request with the redesigned notification was received in the agency.

Figure 5: Example of a spillover within an agency in the treatment group



Notes: Example of the timeline of requests received before the intervention and answered after it began (Case II). Since spillovers could occur only within treated agencies, this figure corresponds to one of them.

Figure 5 shows an example in which a spillover of this type potentially exists. In this case, a treatment-group agency received *Request 1* on August 27, 2018 (on the old notification format) and responded to it on September 17, 2018. On September 10, this agency received a new request, *Request 2*, with the redesigned notification. Therefore, there are ten business days between the submission date of the (old) request, *Request 1*, and the date the agency received the first request subject to treatment, *Request 2*. We expect that a shorter distance

between these two dates will reduce the number of business days the agency takes to respond to the old request. We use different “distances” in terms of business days to identify the spillover effect of the redesigned notifications on old requests. For example, a “distance of 15 days” means that we include in the analysis only the stock of “control” notifications that are less than 15 days old by the time the first “treated” notification is received. By construction, the shorter the distance, the greater the chance of a spillover, but the smaller the sample size. Table 3 presents the results.

Table 3: Spillover analysis within agencies in the treatment group

Panel	Business days	0-15	15	16-25	25	0-13	14-16	17-23	24-25	>25
A. Distance of 15 days										
1 day closer	-1.080*	0.055*	0.010*	-0.019	-0.000	0.044	0.003	-0.045**	0.035	-0.036
	(0.537)	(0.032)	(0.006)	(0.046)	(0.016)	(0.031)	(0.024)	(0.018)	(0.027)	(0.022)
Observations	96	96	96	96	96	96	96	96	96	96
R-squared	0.759	0.512	0.274	0.588	0.428	0.705	0.334	0.766	0.491	0.822
B. Distance of 25 days										
1 day closer	-0.307	0.028**	0.000	-0.014	-0.014	0.014	0.012	-0.006	-0.006	-0.014**
	(0.369)	(0.013)	(0.001)	(0.010)	(0.010)	(0.010)	(0.009)	(0.011)	(0.011)	(0.007)
Observations	135	135	135	135	135	135	135	135	135	135
R-squared	0.603	0.519	0.227	0.575	0.489	0.660	0.336	0.635	0.452	0.699

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. We estimate the spillover effect of the redesigned notification on requests received with the old notices. Standard errors clustered by agency are shown in parentheses. Only requests that were still pending when a redesigned notification arrived at the agency are included. The sample in the first column includes only requests closed within 100 business days. Two requests with 113 and 121 days were dropped from both panels. Control variables include: topic, agency fixed effects, serial requesters, number of requests received by the agency on the day request i arrived, agency backlog, and percentage of pending requests not yet overdue.

Panel A in Table 3 reveals a significant spillover effect. It shows that an outstanding “old” request (notification identical to the control) that arrived one day closer to the day on which the first treated request was received is answered a day sooner.³⁰ That effect results in an increase of 5.5 pp in the likelihood of fulfilling the request within the first 15 business days. Expanding the window to 25 days (Panel B) yields a reduction in the number of business days and an increase in the likelihood of fulfillment before the first deadline (day 15), but not at the same magnitude or significance as Panel A. These differences are in part mechanical, because expanding the window also reduces the share of notifications that can be responded on time and potentially reduces the spillover effect, given that about 78% of all requests received before the intervention were already being fulfilled by the second deadline.

³⁰Imagine that a treatment-group agency receives two requests (on August 27 and August 28, 2018) before receiving the first redesigned request on September 10, 2018. The result indicates that the agency will take two days less to respond to the request received on August 28 than that received on August 27, because the request received on August 28 was closer to the treatment.

6 Training programs

Seminars and workshops to teach civil servants how to perform tasks in accordance with the law and how to meet their responsibilities are a common way of trying to improve compliance. However, evidence of the short-term effects of such activities, and of how they interact with other strategies, is limited. We take advantage of the context in which the redesigned notification was introduced to analyze how the behavioral and standard (training program) interventions might interact to improve task performance and productivity.

Contemporaneous with our intervention, the city government mounted a program to train civil servants on the importance of public access to government information. The program’s workshops were designed to improve public employees’ awareness of the FOI provisions of the law and to address doubts or concerns about its application. The program also served as a signal of the government’s commitment to the law.³¹ The training sessions began on August 22, 2018 and ended on December 11, 2018. During that time, DGSOCAI conducted 30 workshops.³² By the end of the training period, civil servants from 36 agencies in the control group and 32 in the treatment group had attended at least one of the 30 workshops. Although the training was not randomized among agencies or public officials, we use workshop attendance to understand the interaction between our intervention and a standard practice for fostering task productivity; that is, we examine whether civil servants’ participation in the training affected our intervention, as implied by the change in behavior of the control group in Panel B of Figure 3). We also assess whether agencies that participated in the workshops responded differently to the intervention than those that did not, and whether spillover effects emerged from the mixing of treatment and control agencies in workshop sessions.³³

While we cannot claim that workshop participation was random, neither the average number of business days taken to respond to FOI requests nor the agency’s prior performance explain the agency’s participation in at least one workshop. Regression results are presented in Table D.2. We find that agencies in the control group were more likely to attend one of

³¹There were three different types of workshops. One was aimed at liaison officers, legal advisors, and people involved in processing and responding to requests for public information within municipal agencies. Response times, good practices, transparency, and other technical aspects of implementation were discussed. A second type of workshop focused on civil servants who staffed agency desks, as they were key actors at the beginning of the process. These sessions dealt with basic notions of FOI, the right to information, and the importance of guaranteeing it. They also presented information on which agencies could receive requests, the deadlines involved, and the role of DGSOCAI. The third type of workshop was intended for any municipal civil servant interested in the topic of access to public information. These workshops offered a more general view of the right to information, the impact of new data-management technologies, and the law as an example of information management in the city of Buenos Aires. The invitation to participate appeared on the website of the Instituto Superior de la Carrera (ISC); the workshops lasted one or two days and were held at ISC headquarters.

³²Figure D.1 shows the timeline of the workshops. Only two were conducted before the intervention (on August 22 and September 4); the remaining 28 were conducted beginning on September 12). Further details are presented in Appendix D.

³³We consider that an agency participated in a workshop when at least one of its employees attended a session.

the first two workshops held before the intervention began. However, this effect vanishes as the workshops become more regular. In all scenarios, agencies in strata associated with a larger number of requests have a significantly higher likelihood of participation. These organizations probably have more employees and are inherently more likely to have someone trained at one of the workshops.³⁴ While there could be selection, the group composition in each training session (i.e., the share of control and treatment agencies) is “as good as random” given that assignment to treatment was randomized according to the number of requests received by the agencies, and this allocation was not available to the individuals in charge of the training workshops.

To account for the influence of the training workshops on handling of FOI requests, we estimate equation (1) controlling for attendance (results presented in Table D.3). The variable *workshop* takes the value of one if the agency participated in a workshop before receiving a request (or while it remained pending), and zero otherwise.³⁵ The results show that the effects of the treatment on the second deadline are about the same as those presented in Tables 1 and 2. We also find delays in the response process when civil servants attend the workshops, as the probability of providing a late response rises by 13.5 pp. These results are derived from a significant increment of 7.8 days to reply to a request. We cannot interpret this result as causal or disentangle the mechanisms at play behind it. However, one plausible conjecture (which we cannot test) is that attending the training sessions may enhance commitment to high-quality answers, causing civil servants to take longer to respond.

So far, we have examined the effect of the workshop independent of the treatment, but there are several reasons why they may reinforce each other. First, they both highlight the relevance of the right to information and the importance of complying with deadlines. Second, civil servants can use training spaces to share experiences related to task performance, ranging from information received in the notifications to practical tools for responding to requests. Therefore, we first analyze our intervention’s effect based on whether or not requests were influenced by the workshops. Second, we study the workshops’ heterogeneous impact on the control and treatment groups. To assess these effects, we use the linear probability model in equation (2). Third, we evaluate whether the group composition generated spillover effects.

$$Y_{ij} = \alpha + \beta_1 Treatment_j + \beta_2 Workshop_{ij} + \beta_3 Treatment_j \cdot Workshop_{ij} + X'_{ij}\delta + u_{ij} \text{ for } t = 1 \quad (2)$$

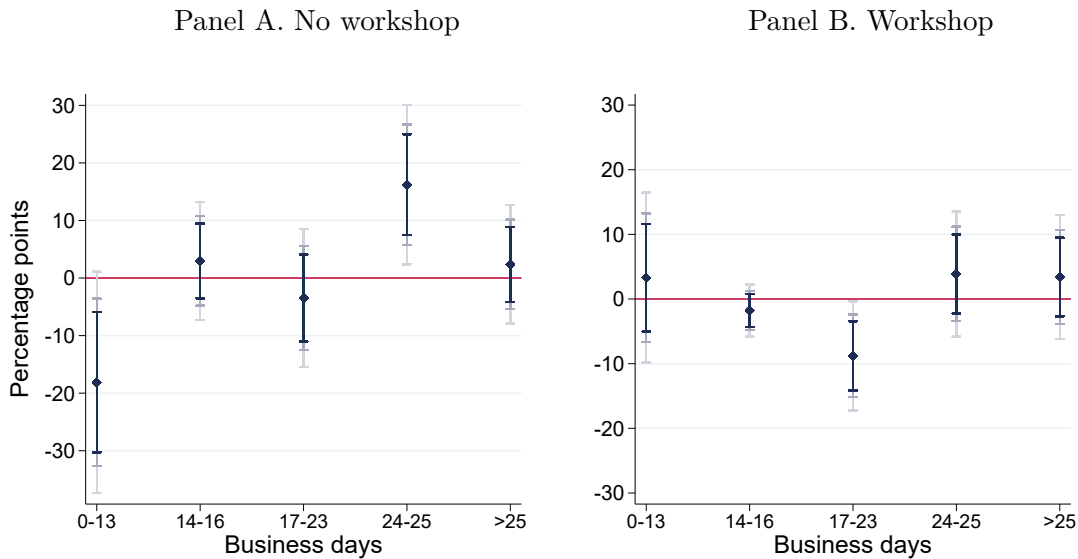
³⁴Even though we detect no selection bias in workshop participation, we recognize that the results we find cannot be considered causal since participation was not randomized.

³⁵Because the training program was carried out while our intervention was in place, a given request may or may not have been influenced by the workshops. Because we have the date of creation and completion of every request i , as well as the dates of the workshops, we calculate for each request the workshops that potentially influenced the agency response to that specific request i . If the agency that received the request attends the first workshop after it has already responded to the request, then the request cannot be influenced by the training (see Panel A in Figure D.2). However, there are two scenarios in which the workshops can influence a request (Panel B). First, the training program occurs before the request arrives at the agency or, second, the workshop takes place while the request is still pending. The variable *workshop* takes the value of one in both cases.

Figure 6 depicts the effect of the redesigned notifications according to agencies' participation in the workshops. Panel A shows that requests submitted to treatment agencies that were not influenced by a workshop have a lower probability (-16.7 pp) of being met during the first 15 days, because the responses accumulate around the second deadline (16.2 pp), compared with requests received by agencies in the control group that were not influenced by any workshop. The magnitude of the coefficient is significantly greater than the 6.8 pp increase found in Table 2.

Panel B shows that overall, requests received by treatment agencies potentially influenced by workshop attendance just had a different rate of response during the extension period compared with those received by control agencies that attended workshops. This finding is consistent with a scenario in which the workshops supply the control group with information that the treatment had previously received through the redesigned notifications.

Figure 6: Heterogeneous effect of the intervention on groups of requests (influenced or not by a workshop)



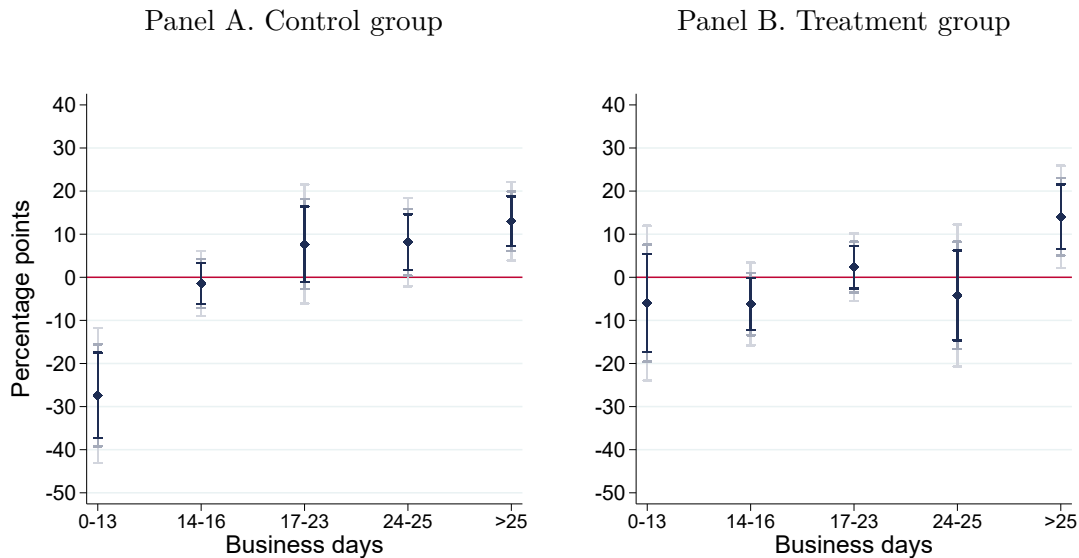
Notes: The linear combination of estimates is based on Table D.4. Panel A compares agencies in the treatment group that did not attend workshops (their requests were not influenced by the workshops) with agencies in the control group that did not attend workshops (their requests were not influenced by any workshop). The diamond corresponds to the coefficient for *Treatment*. Panel B contrasts agencies in the treatment group that attended workshops (their requests were potentially influenced by them), with agencies in the control group that attended workshops (their requests may have been influenced by them). The diamond corresponds to the linear combination of $Treatment + (Treatment \cdot Workshop)$. The color intensity of confidence intervals represents the confidence level (from darker to lighter, 90%, 95%, and 99%).

Armed with our previous results, we analyze the effect that the workshops had on the control and treatment groups. Panel A in Figure 7 shows that control agencies whose requests were influenced by the workshops behave differently from control agencies whose requests were not influenced by the workshops. They are less likely to answer within the first 13 days (-27.4 pp), but more likely to respond around the second deadline (8.1 pp) or late (13 pp).

As mentioned in section 4, we confirm that the workshop participation of civil servants from these agencies explains the considerable shift in the control group's behavior on the

second deadline. For agencies in the treatment group (Panel B), the likelihood of responding during the first 13 days does not change significantly, but the probability of replying late to a request increases by 14 pp when a workshop influences it, which is about the same than the probability observed for the control group. To sum up, the workshop decreases the percentage of responses during the first thirteen days and increases the percentage of responses around the second deadline for the control group. Additionally, the workshop increases the proportion of late responses in both the treatment and control groups. Thus, we can conclude that civil servants' participation in the training did indeed affect our intervention.

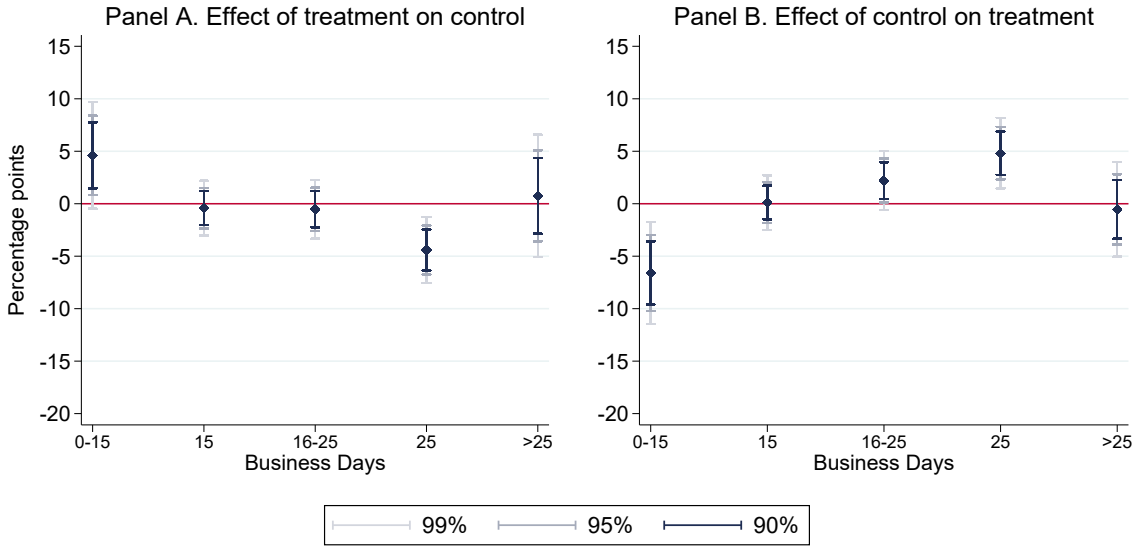
Figure 7: Heterogeneous effect of the workshops on our intervention



Notes: The linear combination of estimates for the control and treatment groups is based on Table D.4. Panel A compares agencies in the control group that attended workshops (their requests were potentially influenced by them), with agencies in the control group whose requests were not influenced by any workshop because their staff did not attend them. The diamond corresponds to the coefficient for *Workshop*. Panel B compares agencies in the treatment group that attended workshops (their requests were potentially influenced by them), with agencies in the treatment group whose requests were not influenced by any workshop because their staff did not attend them. The diamond corresponds to the linear combination of $Workshop + (Treatment \cdot Workshop)$. The color intensity of confidence intervals represents the confidence level (from darker to lighter, 90%, 95%, and 99%).

Finally, Figure 8 shows that having the control and treatment groups participating in the workshops simultaneously causes a differential effect on the two groups, which suggests spillover effects. If that were not the case, we would not observe any change in response rates due to an increase in the participation of the other group in the sessions. Panel A shows that a greater participation of the treatment group during the sessions has a positive effect on the control group, in the sense that a higher percentage of requests are answered before the first deadline. Panel B shows the opposite effect, that is, requests influenced by workshops in which the participation of the control was larger are less likely to be completed before the first deadline and more likely to finish precisely on the second deadline.

Figure 8: Spillover due to workshop participation



Notes: The coefficients displayed in Panel A correspond to the effect on the control group of an increase of 10 pp in the participation of the treatment group in the workshops. The coefficients presented in Panel B correspond to the effect on the treatment group of an increase of 10 pp in the participation of the control group in the workshops. Results in tabular form are available upon request. Control variables include: indicator variable for bad past performance, the month in which the agency received the request, serial requesters, number of requests received by the agency on the day request i arrived, agency backlog, and percentage of pending requests not yet overdue.

In summary, the behavioral intervention seems to have shifted responses towards the second deadline (day 25). That is, it made the deadline more salient and increased targeting to it. For the control group, the training sessions caused a starker drop in the first 13 days and increased the percentage of responses met by the second deadline. It also caused a significant rise in late responses for both groups. One possible reason behind this could be that public employees now provide more complete answers or answers of higher quality. Finally, it is important to note that civil servants in the control and the treatment groups shared information during the workshops, possibly creating different incentives about the timing of responding to requests.

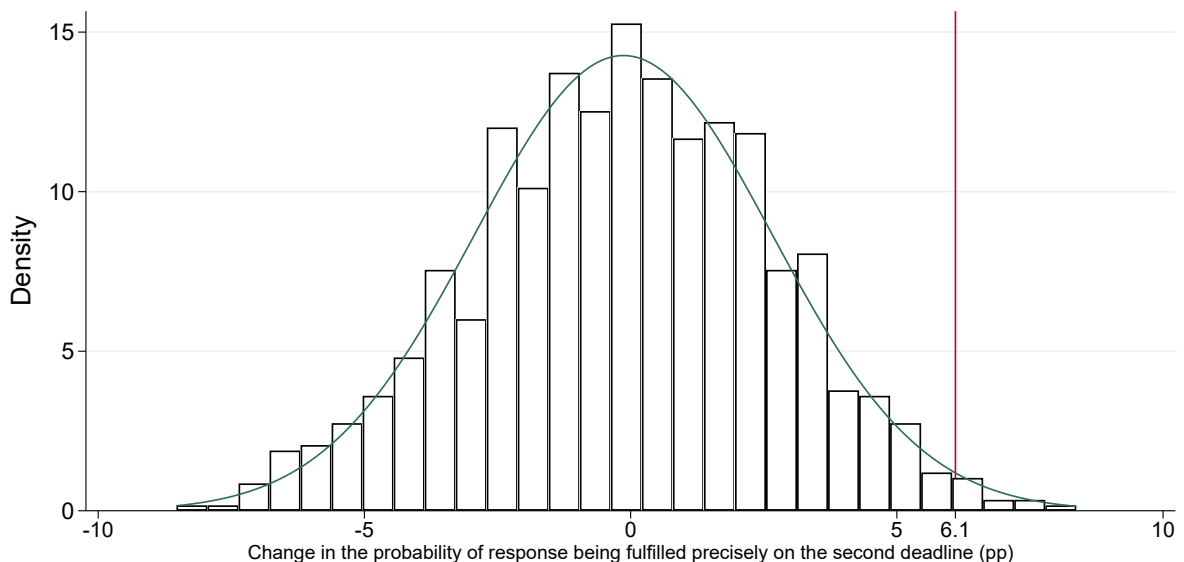
7 Robustness checks

In this section we perform some tests to assess the robustness of the effects found in Tables 1 and 2. First, we conduct a randomization inference procedure, reassigning treatment status among agencies. Second, we incorporate the pretreatment information to gain precision in the estimates. Third, we deploy a difference-in-differences model to take advantage of the structure of our data set. Subsequently, we estimate a cleaner effect by excluding from the sample requests that were received before the intervention but fulfilled after it began. Finally, we consider the number of days civil servants spent in workshops, which may have affected the promptness of their responses.

7.1 Placebo test

To prove the robustness of our results, we implement a placebo test that exploits the randomization design. We conduct a randomization inference procedure in which the treatment status of all agencies (59 treated and 62 control) is reassigned a thousand times while keeping the stratified randomization design constant. The effect of the intervention is then estimated for each treatment allocation. We expect that the distribution of results over the thousand iterations is centered at zero, since the reassignments do not correspond to the treatment allocation implemented in the intervention. The probability of obtaining the same result shown in Table 1 should be low for any alternative reassignment. Figure 9 shows the distribution of effects for the dependent variable corresponding to responses by the second deadline (25 business days) under the reassignment exercise. We see that the effect of the intervention on this dependent variable, as reported in Table 1 (column [10]), is on the far-right tail of the distribution. (Randomization inference estimates and p-values are shown in Table E.1).³⁶

Figure 9: Placebo test
Effect of the intervention on likelihood of responses occurring precisely on the second deadline (25 business days)



Notes: The distribution of the effect of the intervention comes from randomizing the treatment allocation of the agencies a thousand times, while keeping the stratified randomization design constant. The red line depicts the effect found in Table 1 on the second deadline (25 business days).

7.2 Inclusion of pretreatment information

Taking advantage of our data structure, we include pretreatment information to gain precision in the estimates. We use the following specification to identify the effect of our

³⁶Graphs of the placebo test of other dependent variables are available upon request.

intervention:

$$Y_{ijt} = \alpha + \beta_1 Post_t + \beta_2 Treatment_j \cdot Post_t + \eta_j + X'_{ijt} \delta + u_{ijt} \quad (3)$$

where $Post$ indicates that request i was received on or after September 6, 2018, and η_j is an agency-level fixed effect that captures individual heterogeneity. It captures the effect of unobserved potential explanatory factors that are time constant within each agency. In this case, β_2 is the coefficient of interest. We find a significant increase in the probability of a response occurring around the second deadline (4.6 pp), which is lower than the result reported in Table 2 (6.8 pp) but still significant (see Table E.2).

7.3 Difference-in-differences model

In this case, we estimate the effect of the redesigned notification using a standard difference-in-differences model. This approach is based on the identifying assumption that changes in response times observed in control agencies provide a good counterfactual for the changes that would have been observed in treatment agencies if they had not received the redesigned notifications.³⁷ We estimate the following model:

$$Y_{ijt} = \alpha + \beta_1 Treatment_j + \beta_2 Post_t + \beta_3 Treatment_j \cdot Post_t + X'_{ijt} \delta + u_{ijt} \quad (4)$$

Since the randomization was done at the agency level, we cannot include agency fixed effects (η_j) in this specification because the linear combination of those in the treatment group predicts exactly the variable $Treatment$. Instead, fixed effects for the strata are used. Again, we find a significant increase of 4.4 pp in the probability of a response falling precisely on the second deadline, and 3.9 pp for the days around the first deadline (see Table E.3).

7.4 Dropping *Case II* requests

As shown in Figures 2 and 5, some requests were still pending when our intervention began (*Case II*). This robustness section excludes such requests and includes only agencies that received requests in both *Cases I* and *III* (see Figure 2). We expect to find a cleaner effect with this specification, because our previous results, shown in Tables 1 and 2, did not consider the spillover within agencies presented in Section 5.3. We do not find discrepancies between the results obtained in this case and our initial results. The coefficient for the response on the second deadline is significant; its magnitude is about the same as in our main tables (6.2 pp). Similarly, there is an increase of 6.9 pp in the probability of answering around the

³⁷To examine the plausibility of this assumption, Figure E.1 shows estimates of the effect on the second deadline in months preceding and following our intervention. This graph provides evidence that the control and the treatment groups exhibited parallel trends prior to the intervention.

second deadline using flexible deadlines, 24 and 25 business days, accompanied by a drop of 8 pp in the response rate during the extension period, 17 to 23 business days (see Table E.4).

7.5 Considering days spent in workshops

Part of the effect of the training workshops could be explained by delays that take place because the person responsible for responding the requests is attending the workshop. In this section, we consider the days spent at the workshops and adjust the number of days taken by the agency to respond.³⁸ We do not find significant differences with the results obtained in Tables 1 and 2. The coefficient for the treatment is 5.4 pp on the second deadline, which is very close to the original estimate of 6.1 pp (Table 1), and the coefficient for the flexible one is also very close, 6.7 pp (see Table E.5).

Additionally, we conduct the same analysis, shown in Table D.3, in which we introduce the attendance at workshops as a control variable. The results of our intervention are robust; there is an increase of 5 pp in responses on the second deadline, and of 6.6 pp when day 24 is included in the second deadline (see Table E.6). All the effects remain similar. The workshop training causes a stark reduction in the share of responses delivered during the first 15 days, a large increase in the first days of the extension period, and a higher rate of late responses.

8 Conclusions

While financial and nonfinancial incentive programs abound in the private sector to ensure higher productivity, this is not the case in the public sector, where there are limits on the use of differential compensation. Interventions based on behavioral insight have become very common owing to their low political and economic costs. Testing whether these tools are effective in changing civil servants' behaviors is relevant, particularly for tasks in which their interests are misaligned with those of the public or government authorities. In this paper, we evaluate one of those cases in the city of Buenos Aires: the responsiveness of municipal agencies to FOI requests.

We evaluate the effect of an intervention that uses salience, deterrence, loss aversion, and other behavioral insights to enhance compliance with requests for public information. The results show that the intervention created incentives for civil servants to respond exactly on the deadlines rather than more rapidly overall. The findings are consistent with the intervention's design, which emphasized answering on time over answering faster.

³⁸We calculate the total number of workshops attended by civil servants from the agency responsible for responding to the request while it remains pending. The total number is then subtracted from the number of business days it took the agency to reply to it.

The behavioral intervention took place at the same time as a series of training workshops also aimed at increasing compliance with the FOI law. Evidence of the effect of training on public sector productivity is still scant, particularly for tasks that officials have no intrinsic motivation to perform. The suggestive evidence we present here indicates that training programs are not more effective in reducing delays. On the contrary, they seem to have increased them. Furthermore, they appear to have influenced the effectiveness of the behavioral intervention. Not only are staff members from agencies in the treatment group who participated in the workshops less likely to respond early, but their workshop interactions with relatively high numbers of staff from agencies in the control groups seems to have increased the probability that they would respond later rather than sooner.

While the results of the behavioral intervention do not reveal a significant improvement in timeliness they do demonstrate that civil servants are affected by it. The results demonstrate that behavioral insights can modify public sector performance and deadline adherence. We illustrate, in particular, how salience causes a stronger anchoring effect on deadlines. It would be interesting to test the sort of intervention we used in settings where initial compliance is lower (in this case, there was a ceiling effect, with 78% of requests already met by the second deadline prior to the intervention), or where deadlines can be explicitly adjusted. The findings also suggest that training workshops may not enhance productivity and compliance in terms of the number of days it takes to reply to a request. Future studies could delve deeper into this topic to understand the specific mechanisms that cause these delays, as well as how behavioral treatments and training interact and how they can be designed to promote compliance. Importantly, we find that the composition of the groups that participated in the training sessions matters, lending support to the long-standing literature on peer effects. Therefore, future studies should assess the conditions under which training exercises can be effective, as well as the optimal distribution of participants to maximize their benefits.

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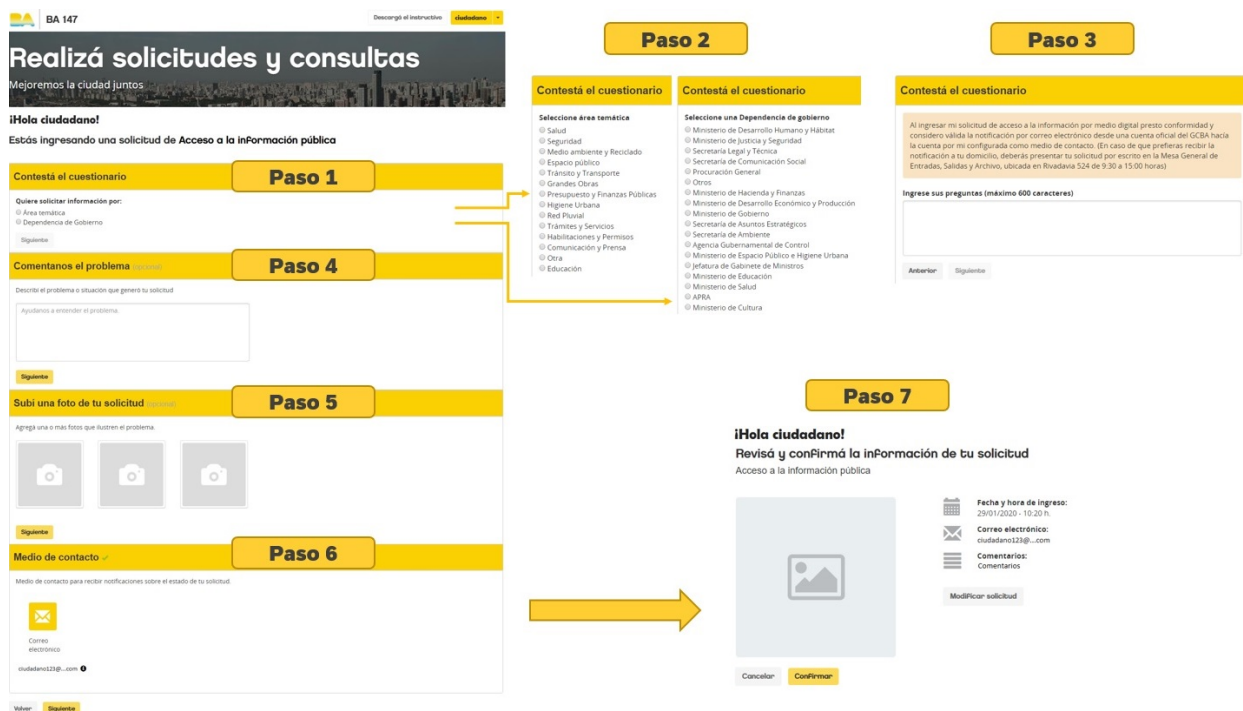
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Appendix

A Submission of requests and notices

This appendix includes Figures A.1 and A.2. The first is the form citizens must complete when filing requests online. It describes the process of submission step by step. The second figure presents the notification that all civil servants received before the intervention (and which the control group continued to receive during the intervention) and the new notification designed for the treatment group agencies.

Figure A.1: Submission of online requests



Notes: This figure presents the procedure citizens must follow to submit their request online. They must first log in to this website: <https://gestioncolaborativa.buenosaires.gov.ar/confirmacion/1468525201056>, and then proceed through each step. In step ①, the citizen chooses between theme areas or Government units. Step ② is when she selects one of them, and step ③ is where she writes down the request. Step ④ is optional, and is used for citizens to describe the situation. Step ⑤ then allows the user to upload pictures that illustrate the problem. In step ⑥ the citizen enters their contact information, and finally, in step ⑦ she confirms the request for information.

Figure A.2: Notifications

Panel A. Control

DG [.....]

[.....]

SEÑOR/A DIRECTOR/A GENERAL

S / D

1. El/la vecino/a [.....] realizó un pedido de información pública en el marco de la Ley 104.
2. La Ley 104 consagra el derecho de toda persona a solicitar y a recibir información completa, veraz, adecuada y oportuna de cualquier órgano del Gobierno, la que deberá ser brindada **dentro del plazo de quince (15) días hábiles**.
3. La respuesta al pedido de información pública deberá elaborarse dentro de un **Informe (IF) producido en GEDO** y ser remitida al vecino/a por la dependencia a su cargo mediante **notificación fehaciente** a la dirección que figura en la carátula del presente. Se solicita, en caso de corresponder, adjuntar al interesado copia de la documentación necesaria.
4. Se deberá vincular al Expediente Electrónico (a) la respuesta redactada en un **Informe (IF)** y (b) **documentos respaldatorios** y (c) **copia de la notificación fehaciente firmada** y realizar el pase a esta DG (usuario DGSOCAI-03).
5. La **denegatoria** al acceso a la información solo procede en aquellos casos que (a) la información no exista y el funcionario **no esté obligado a producirla** y (b) cuando se produce algunas de las **excepciones** prevista en el artículo 6° de la Ley 104. En caso que se encuadre dentro de estos supuestos, es necesario **detallar los elementos y las razones al vecino/a**.
6. Si **no contara con la información requerida por no ser de su competencia**, se solicita girar el Expediente a esta DG (DGSOCAI-03) a la brevedad.
7. En caso de hacer uso de la **prórroga única y excepcional** de diez (10) días, deberá comunicarlo al vecino/a mediante notificación fehaciente **antes del vencimiento del plazo** y a esta DG mediante Comunicación Oficial (CCOO). La mentada notificación debe constar en el Expediente como informe gráfico (IFGRA).
8. Por último, le recordamos que el funcionario o agente responsable que en forma arbitraria obstruya el acceso a la información, la suministre en forma incompleta – sin estar justificado - u obstaculice de cualquier modo el cumplimiento de la Ley 104, comete una **falta grave sin perjuicio de las responsabilidades civiles o penales que pueden corresponder**. La falta de respuesta o la respuesta inadecuada puede conducir a su **judicialización**.
9. Si tiene alguna consulta, no dude en comunicarse con esta DG a dgsocai@buenosaires.gov.ar o al 5091-7301/7298.

Sin otro particular, aprovecho la oportunidad para saludarlo/a atentamente.-

Panel B. Treatment

Notes: These two images depict examples of the notices sent to agencies in the control group (Panel A), and the redesigned notices sent to agencies in the treatment group (Panel B).

B Descriptive statistics

This appendix provides additional information on the data set. Because we conducted a stratified randomization using historical data to assign agencies to treatment and control groups, Table B.1 shows the number of agencies in each stratum by treatment assignment.

Table B.1: Stratification of agencies

Stratum	Number of requests	Number of agencies in sample		
		Control	Treatment	Total
I	1	12	7	19
II	2 — 10	22	26	48
III	11 — 100	24	20	44
IV	> 100	4	6	10
	Total	62	59	121

Notes: The strata were created based on the number of requests that each agency received between January 2, 2017, and July 26, 2018.

We use the last update available of the data set on the website of the city of Buenos Aires as of November 27, 2019. On that day, the database contained 7,568 requests. We eliminate 266 denied requests and 406 other cases received by agencies that handled petitions only in the pretreatment or the post-treatment stages. As of November 27, 200 of the 6,896 outstanding requests were still unresolved; 96 had been submitted prior to the intervention; 104 after it began.

We specify four subsamples for the analyses of this paper. Sample 1 includes all pending and closed requests. Sample 2 excludes those still pending on November 27, 2019. In our primary specification, (Tables 1 and 2), we use Sample 1 to analyze the effect of the treatment on all binary variables that indicate periods of business days taken to respond (3,111 observations). To evaluate the impact of the treatment on the number of days, we use only closed requests (Sample 2, 3,006 observations). It seemed reasonable to exclude the 104 requests that were still pending after the intervention began because they had already exceeded the maximum number of business days allowed by the law (25), and we could not anticipate the number of days agencies would take to respond to them. Applying that decision left us with 120 agencies instead of 121. We removed one agency in the treatment group because it received only one request after the intervention began, and that request was pending.

We use samples 3 and 4 in Section 7.4. These samples do not include requests that were received before the intervention and processed after it began. They include only the 114 agencies that received requests in both Cases *I* and *III* (See Figure 2). Sample 3 includes pending requests and is thus utilized to analyze the effect of the treatment on indicator variables. Sample 4 contains only closed requests and is used to estimate the effect of the treatment on the number of days it takes to respond to a request.

Table B.2 presents the descriptive statistics of the four subsamples. It shows the number

of agencies in the control and treatment groups, the number of requests received before and during our intervention, the average number of requests received per agency (including denied requests), and the minimum and maximum number of requests received by an agency. Table B.3 shows the covariate balance tests.

Table B.2: Descriptive statistics

Sample 1 - Main sample						Sample 2					
Condition	Number of		Average	N. Requests		Condition	Number of		Average	N. Requests	
	Agencies	Requests		(Min)	(Max)		Agencies	Requests		(Min)	(Max)
Before (<i>Cases I & II</i>)						Before (<i>Cases I & II</i>)					
Control	62	1,584	26.4	1	515	Control	62	1,566	26.4	1	515
Treatment	59	2,201	37.8	1	665	Treatment	58	2,123	38.5	1	665
Total	121	3,785	32	1	665	Total	120	3,689	32.2	1	665
After (<i>Case III</i>)						After (<i>Case III</i>)					
Control	62	1,596	26.3	1	329	Control	62	1,580	26.3	1	329
Treatment	59	1,515	28.2	1	377	Treatment	58	1,426	28.7	1	377
Total	121	3,111	27.2	1	377	Total	120	3,006	27.4	1	377

Sample 3						Sample 4					
Condition	Number of		Average	N. Requests		Condition	Number of		Average	N. Requests	
	Agencies	Requests		(Min)	(Max)		Agencies	Requests		(Min)	(Max)
Before (<i>Case I</i>)						Before (<i>Case I</i>)					
Control	56	1,371	29	1	515	Control	56	1,371	29	1	515
Treatment	58	1,917	38.5	1	665	Treatment	58	1,917	38.5	1	665
Total	114	3,288	33.8	1	665	Total	114	3,288	33.8	1	665
After (<i>Case III</i>)						After (<i>Case III</i>)					
Control	56	1,557	28.4	1	329	Control	56	1,543	28.4	1	329
Treatment	58	1,514	28.7	1	377	Treatment	58	1,426	28.7	1	377
Total	114	3,071	28.5	1	377	Total	114	2,969	28.5	1	377

Notes: Samples 1 and 2 pool all observations from *Cases I & II* in the pretreatment stage. Samples 3 and 4 consider only *Case I* for the pretreatment stage. The average, minimum, and maximum number of requests per agency consider all requests received by the agency, even those not included in the sample, since even these represent workload. We ensure that we have the same agencies before and after the intervention in each sample.

Table B.3: Balance test - Covariates (pre-intervention stage)

	All	Control	Treatment	Difference	p-value
N. times the same person submits a request to the same agency	1.980 (1.408)	1.867 (1.323)	2.062 (1.460)	0.194	0.337
Number of requests per day	2.965 (3.500)	2.748 (3.644)	3.120 (3.384)	0.372	0.380
Pending requests: Percentage of pending requests not yet overdue	24.336 (25.314)	25.460 (26.091)	23.526 (24.715)	-1.934	0.419
Backlog: Percentage of overdue pending requests over total number of requests received	5.564 (7.678)	3.957 (5.394)	6.721 (8.790)	2.764	0.005

Notes: The first three columns present the mean for each variable. Standard deviations are shown in parentheses. The last column shows the p-values that result from a regression of the treatment on all covariates included in the main specification. Month fixed effects and topic fixed effects are not shown.

Table B.4 displays, for each sample in the pretreatment stage, the average number of business days it took agencies to reply to requests by group (all agencies, agencies in the control group, and agencies in the treatment group). It also shows the proportion of responses for each interval of days.

Table B.4: Balance tests dependent variables (pre-intervention stage)

Panel A - Sample 1 - Before (*Cases I and II*)

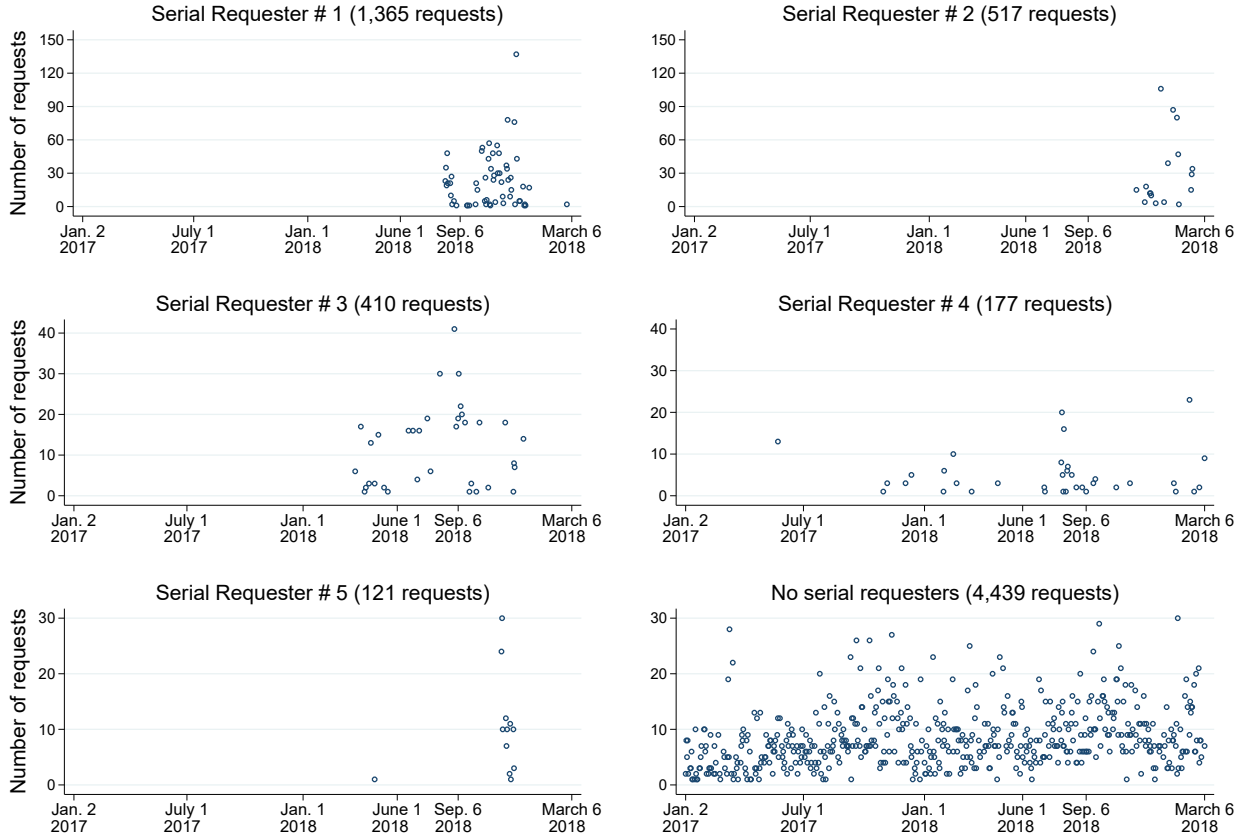
Variable	All	Control	Treatment	Obs.
Business days*	20.433 (30.094)	19.789 (30.479)	20.908 (29.804)	3,689
First official deadline (≤ 15 business days)	0.542 (0.498)	0.582 (0.493)	0.514 (0.500)	3,785
15 business days	0.046 (0.209)	0.051 (0.219)	0.042 (0.201)	3,785
Extension period (16-25 business days)	0.239 (0.427)	0.235 (0.424)	0.242 (0.428)	3,785
25 business days	0.036 (0.186)	0.032 (0.177)	0.039 (0.193)	3,785
Late response (>25 business days)	0.218 (0.413)	0.182 (0.386)	0.244 (0.430)	3,785

Panel B - Samples 3 and 4 - Before (*Case I*)

Variable	All	Control	Treatment	Obs.
Business days	18.213 (21.873)	17.726 (21.830)	18.561 (21.902)	3,288
First official deadline (≤ 15 business days)	0.590 (0.492)	0.624 (0.485)	0.567 (0.496)	3,288
15 business days	0.047 (0.211)	0.052 (0.222)	0.043 (0.204)	3,288
Extension period (16-25 business days)	0.221 (0.415)	0.206 (0.405)	0.232 (0.422)	3,288
25 business days	0.031 (0.173)	0.031 (0.170)	0.031 (0.174)	3,288
Late response (>25 business days)	0.189 (0.391)	0.170 (0.376)	0.202 (0.402)	3,288

Notes: The table shows the average number of days agencies took to respond to a request, and the proportion of claims answered within each interval of days. Standard deviations are shown in parentheses. *The statistics in Panel A are for sample 1, except for business days, which includes only closed requests (sample 2). Samples 3 and 4 have the same observations in the pre-intervention stage as in Table B.2.

Figure B.1: Number of requests submitted by serial requesters



Notes: This figure depicts the top five requesters, the overall number of requests they submitted, and the distribution of requests over time. A serial requester is someone who filed at least 30 requests between January 2, 2017 and March 6, 2019. Each circle indicates the number of requests submitted on a specific date. This figure excludes the nine serial requesters who filed the remaining 539 requests. They each filed between 32 and 95 requests.

Figure B.1 depicts the top five serial requesters, the overall number of claims they submitted, and the distribution of requests over time. A serial requester is someone who filed at least 30 requests between January 2, 2017 and March 6, 2019. In total, serial requesters submitted 3,129 of the 7,568 FOI requests in the data set (42.3%). This figure shows that Serial Requester # 1 submitted 1,365 requests between August 13, 2018 and February 26, 2019, with several of them being filed on the same day. On one particular day, this requester submitted 137.

C Power calculation

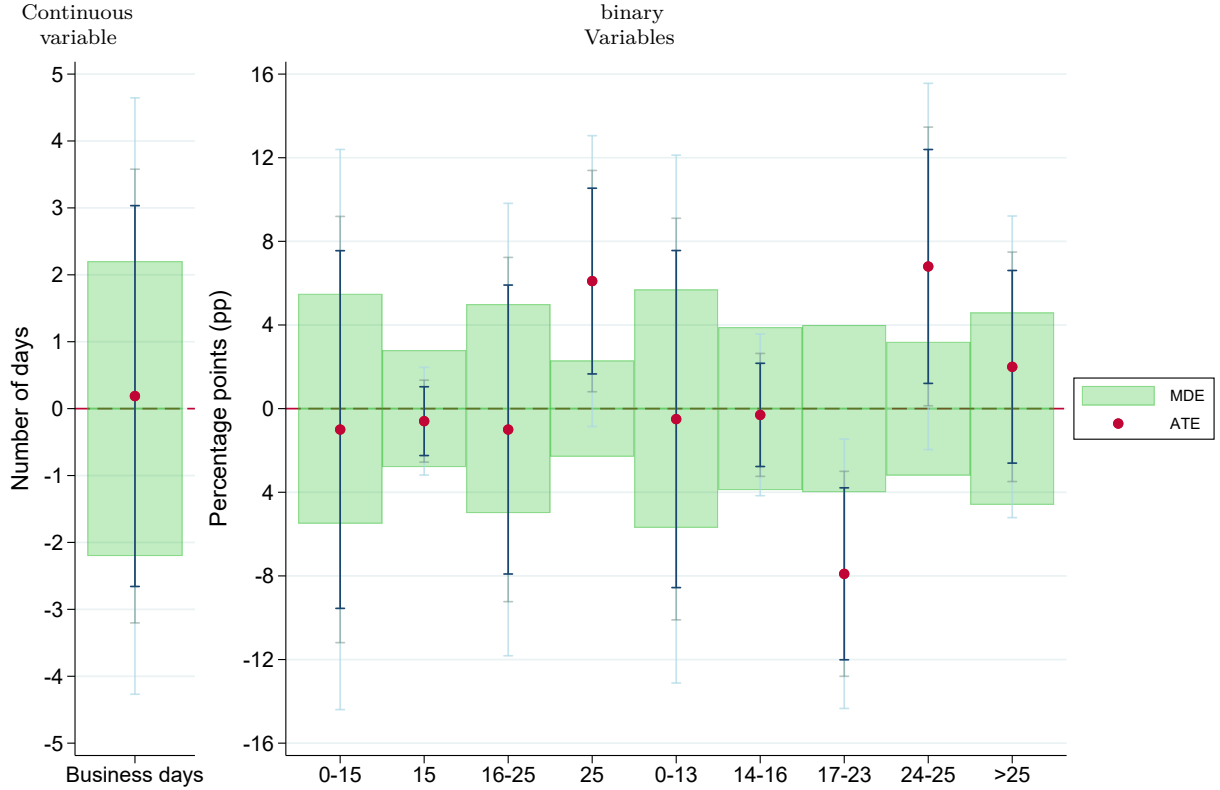
Because the randomization was conducted at the agency level and the number of agencies included in the study is small (121 or 120 depending on whether we include pending requests), we present the power calculations for each dependent variable. Table C.1 and Figure C.1 show the minimum detectable effect (MDE) considering (i) the baseline correlation as in Hemming and Taljaard (2016), (ii) cluster design with power set at 80%, and (iii) a significance level of 5%. We also specify a comparison of proportions to carry out power estimations for all binary dependent variables. This method, proposed by Hemming and Marsh (2013), uses a normal approximation without a continuity correction. Table C.1 shows that an effect lower than 4 pp would not be detected on average (average MDE for the nine binary dependent variables).

Table C.1: Power calculation

	Business days	0-15	15	16-25	25	0-13	14-16	17-23	24-25	>25
Minimum detectable effect	2.204	0.055	0.028	0.050	0.023	0.057	0.039	0.040	0.032	0.046
Control mean	19.789	0.582	0.051	0.235	0.032	0.491	0.113	0.141	0.072	0.182
Control standard deviation	30.479	0.493	0.219	0.424	0.177	0.500	0.317	0.349	0.259	0.386
Intra-cluster correlation	0.002	0.225	0.130	0.140	0.129	0.171	0.151	0.064	0.124	0.114
Baseline correlation	0.050	0.892	0.809	0.823	0.808	0.855	0.836	0.661	0.802	0.786

Notes: All power estimations incorporate cluster design. We conducted a stratified randomization over 121 agencies (62 assigned to the treatment group and 59 to the control group). Power is set at 80% and the significance level at 5%. The table shows the means and standard deviations of the control group before the intervention. The power estimations are conducted over the sample of requests after the intervention started. However, we control for the baseline correlation (correlation between the outcome at baseline and follow-up) as suggested by Hemming and Taljaard (2016). We specify a comparison of proportions for the power estimations, following Hemming and Marsh (2013) for all the binary dependent variables.

Figure C.1: Power calculation



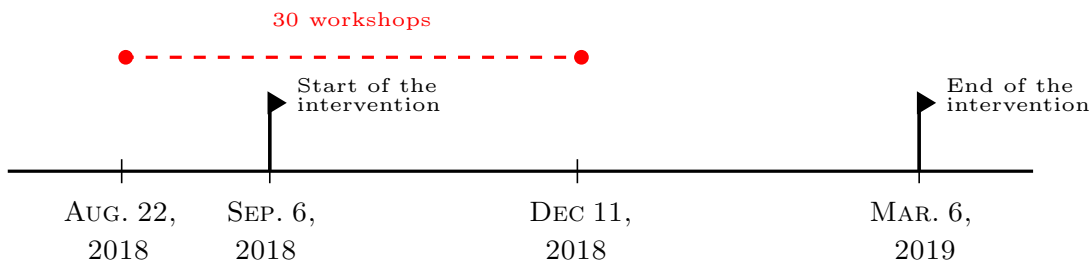
Notes: MDE denotes the minimum detectable effect, and the red circle is the average treatment effect found in Tables 1 and 2. The color intensity of confidence intervals represents the confidence level (from darker to lighter, 90%, 95%, and 99%). All power estimations incorporate a cluster design at the agency level. We conducted a stratified randomization over 121 agencies (62 assigned to the treatment group and 59 to the control group). Power is set at 80% and the significance level at 5%. We consider baseline correlation as in Hemming and Taljaard (2016). We specify a comparison of proportions for the power estimations of the binary dependent variables. Table C.1 presents more details on the estimations.

D Workshops

Here we provide further information about the workshops conducted by the city government of Buenos Aires. The figures in Section D.1 show the workshop timeline and the interaction with requests received by agencies that participated in them. Section D.1 also includes Table D.1, which describes the number of agencies that participated in at least one workshop over various time periods. Section D.2 tests whether agencies self-select for training over time. Finally, Section D.3 shows the results of the main specification, including the workshop as a control. It also presents the results on the interaction of both types of interventions.

D.1 Timeline of workshops and agencies' participation

Figure D.1: Timeline of the workshops



Source: Authors' elaboration.

Figure D.1 shows that the city government carried out 30 training sessions (workshops) between August 22, 2018 and December 11, 2018 for the purpose of improving civil servants' compliance with requests for public information. There is an overlap of about three months between our intervention and the workshop initiative.

Table D.1 presents the number of agencies that participated in training sessions during various periods. First, it shows that between August 22 and September 5, 2018, eight agencies in the control group and three in the treatment group attended at least one of the two workshops offered during that period. Between September 6, 2018, when our intervention with the redesigned notice began, and December 13, 2018, the number of participating agencies grew significantly for both groups. By the end of the workshop program, 56% of the agencies had attended at least one workshop.

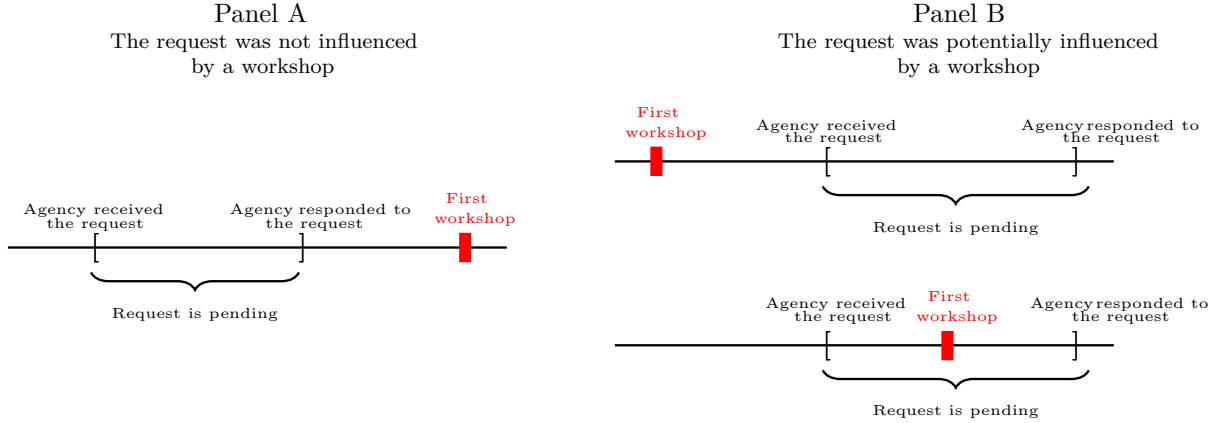
Table D.1: Agencies' participation in workshops

Participated in at least one workshop	Aug. 22 — Sep. 5 (Pre)		Sep. 6 — Dec. 13 (Post)		Aug. 22 — Dec. 13 (All)		Total
	No	Yes	No	Yes	No	Yes	
Control	54	8	30	32	26	36	62
Treatment	56	3	28	31	27	32	59
Total	110	11	58	63	53	68	121

Notes: This table indicates in the “Yes” column the number of agencies that participated in at least one workshop during that period and had at least one request influenced by it. Two agencies participated but did not receive any requests following their participation. Since we cannot observe the impact of the workshops on these institutions' response, these agencies are placed in the “No” columns.

Figure D.2 shows two cases of interaction between requests and agencies' participation in workshops: one in which request i is not influenced by a workshop, even if a civil servant from the agency attends one (Panel A), and the other in which the request is potentially influenced by a civil servant's attendance (Panel B). A workshop has no effect on a request that is open and closed before an agency staff member attends it. However, a workshop can affect disposition of a request when at least one of the agency's staff attends one workshop, either before the request is received or while it is still pending.

Figure D.2: Influence of the workshops at the request level



Notes: Panel A illustrates the situation in which a workshop does not influence a request. Panel B depicts two scenarios in which a workshop may influence a request. The case at the top illustrates a request influenced by a workshop because a civil servant from the agency attended it before the request arrived at the agency. The case at the bottom depicts a request that was influenced by a workshop because a civil servant from the agency attended it while the request was pending. The term “first workshop” refers to the first training session attended by any of the agency’s staff.

D.2 Workshop selection

Table D.2: Determinants of agency participation in at least one workshop

	Participation in a workshop					
	Before Sept. 6, 2018 (1)	2	After Sept. 6, 2018 (3)	(4)	Any time (5)	(6)
Business days	0.003 (0.003)		0.003 (0.003)		0.005 (0.003)	
Bad past performance (more than 30% of requests answered late)		-0.003 (0.066)		-0.020 (0.115)		-0.009 (0.114)
Treatment	-0.107** (0.052)	-0.098** (0.048)	-0.014 (0.087)	-0.000 (0.094)	-0.078 (0.082)	-0.062 (0.091)
Stratum = 2, ($2 \leq X \leq 10$ requests)	0.048 (0.043)	0.079** (0.036)	0.032 (0.148)	0.067 (0.146)	0.061 (0.150)	0.116 (0.146)
Stratum = 3, ($11 \leq X \leq 100$ requests)	0.086 (0.060)	0.117** (0.054)	0.435*** (0.147)	0.469*** (0.143)	0.460*** (0.143)	0.513*** (0.139)
Stratum = 4, (>100 requests)	0.292* (0.148)	0.318** (0.150)	0.591*** (0.163)	0.620*** (0.164)	0.683*** (0.127)	0.727*** (0.126)
Constant	0.018 (0.041)	0.042* (0.024)	0.264* (0.137)	0.286** (0.134)	0.271** (0.136)	0.312** (0.132)
Observations	114	114	114	114	114	114
R-squared	0.102	0.088	0.211	0.207	0.260	0.245

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors are shown in parentheses. The analysis considers only requests that were already closed before the agency participated in a workshop and before the intervention began on September 6, 2018. Seven agencies are not included since all of their requests were closed after September 6. Bad past performance indicates that the agency responded late more than 30% of all requests (i.e., later than 25 business days). The dependent variable in Columns 1 and 2 indicates the agency’s attendance at one or both of the workshops offered by DGSOCAI on August 22 and September 4, 2018. Columns 3 and 4 consider the participation of the agency after the treatment started on September 6. Finally, the dependent variable in columns 5 and 6 is the agency’s participation in a workshop at any stage.

Table D.2 examines agencies’ self-selection into training. We test whether an agency’s participation in at least one workshop depended on the average number of business days it took

to respond to a request, its previous performance, or its assignment to the control or treatment group. We use requests that were closed before the agency attended a workshop and treatment began. To control for the agency’s previous performance, we create the binary variable *Bad past performance*. This variable is equal to one if the agency responded late to more than 30% of the total number of requests it received before participating in a workshop and before the intervention began.

The dependent variable is an indicator variable of workshop participation—that is, participation in one of the workshops by at least one agency staff member. The dependent variable in the first two columns is one if the agency attended at least one of the workshops carried out on August 22 and September 4, 2018, and zero otherwise. Participation in the third and fourth columns is only counted after the intervention began; in other words, agencies whose staff participated in workshops beginning on September 12, 2018. Finally, columns 5 and 6 pool all the data. In this case, the dependent variable indicates the agency’s participation in at least one workshop at any point in time.

D.3 Results with workshop attendance

Table D.3 presents the results of estimating Equation 1 with the additional control of workshop attendance. It shows that, on average, a civil servant’s attendance at at least one workshop significantly delays responses to requests. There is a substantial decrease in the proportion of requests addressed within the first 13 business days and a notable increase in the proportion answered during the extension period or later. The coefficients for the treatment are similar to those reported in Tables 1 and 2. These results serve as evidence that participation in the workshop was random and did not affect our estimation of the intervention’s effect.

Table D.3: Agencies’ response to requests, controlling for workshop attendance

	Business days	0-15	15	16-25	25	0-13	14-16	17-23	24-25	>25
Treatment	0.869 (1.745)	-0.029 (0.053)	-0.009 (0.009)	-0.003 (0.043)	0.059** (0.025)	-0.020 (0.050)	-0.006 (0.014)	-0.075*** (0.025)	0.069** (0.034)	0.032 (0.027)
Workshop	7.794*** (1.766)	-0.216*** (0.067)	-0.037* (0.020)	0.081 (0.059)	-0.018 (0.040)	-0.168*** (0.061)	-0.038 (0.024)	0.050 (0.032)	0.020 (0.047)	0.135*** (0.027)
Constant	11.831*** (2.681)	0.843*** (0.077)	0.134 (0.090)	0.148** (0.065)	-0.015 (0.031)	0.681*** (0.101)	0.139 (0.092)	0.226*** (0.053)	-0.054 (0.042)	0.009 (0.052)
Observations	3,006	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111
R-squared	0.101	0.218	0.058	0.117	0.055	0.208	0.081	0.056	0.116	0.143

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by agency are shown in parentheses. The sample in the first column includes only requests closed by November 27, 2019. The workshop variable takes the value of one if the agency had participated in a workshop before receiving the request or while the request was still pending, and zero otherwise. Control variables include: topic, the month in which the agency received the request, serial requesters, number of requests received by the agency on the day that request i arrived, agency backlog, and percentage of pending request not yet overdue.

Table D.4 presents the results of the linear probability model in Equation 2 to assess the workshops’ heterogeneous impact on the control and treatment groups following the intervention (Section 6).

Table D.4: Heterogeneous effects of the workshops

	Business days	0-15	15	16-25	25	0-13	14-16	17-23	24-25	>25
Treatment	3.582 (2.208)	-0.167** (0.081)	0.000 (0.028)	0.143** (0.070)	0.117** (0.045)	-0.181** (0.073)	0.030 (0.039)	-0.035 (0.046)	0.162*** (0.053)	0.024 (0.039)
Workshop	9.565*** (1.515)	-0.308*** (0.068)	-0.031 (0.019)	0.177*** (0.054)	0.020 (0.026)	-0.274*** (0.060)	-0.014 (0.029)	0.077 (0.053)	0.081** (0.039)	0.130*** (0.035)
Treatment x Workshop	-3.600 (3.127)	0.184* (0.098)	-0.012 (0.030)	-0.195** (0.085)	-0.077 (0.048)	0.214** (0.084)	-0.048 (0.043)	-0.053 (0.062)	-0.123* (0.063)	0.010 (0.059)
Constant	9.922*** (2.566)	0.942*** (0.080)	0.128 (0.091)	0.044 (0.065)	-0.056** (0.027)	0.795*** (0.103)	0.113 (0.094)	0.197*** (0.061)	-0.120*** (0.042)	0.014 (0.054)
Observations	3,006	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111
R-squared	0.103	0.223	0.058	0.124	0.058	0.216	0.082	0.057	0.121	0.143

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by agency are shown in parentheses. The sample in the first column includes only closed requests. Control variables include: the month in which the agency received the request, serial requester, number of requests received by the agency on the day that request i arrived, agency backlog, and percentage of pending requests not yet overdue.

E Robustness checks

The figures and tables in this appendix correspond to those mentioned in Section 7. Section E.1 shows the coefficients and p-values from a randomization inference procedure in which we randomly reassigned treatment status across agencies to assess the significance of the results reported in Tables 1 and 2. Section E.2 presents the results achieved after adding the pretreatment information to raise the precision in the estimates (Equation 3). Section E.3 includes the figure in which we test the parallel trend assumption and the results of the difference-in-differences model (Equation 4). Section E.4 presents the results of Equation 1, excluding the requests received before the intervention but answered only after it began (*Case II*). Last, Section E.5 presents the results, taking into account that civil servants spent one to two days in the workshops, which may have affected their ability to complete the assignment on time.

E.1 Placebo test

Table E.1: Permutation analysis - Randomization inference

Variable	Business days	0-15	15	16-25	25	0-13	14-16	17-23	24-25	>25
Treatment	0.188 [0.700]	-0.010 [0.926]	-0.006 [0.460]	-0.010 [0.174]	0.061 [0.000]	-0.005 [0.942]	-0.003 [0.822]	-0.079 [0.000]	0.068 [0.000]	0.020 [0.068]
Observations	3,006	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111

Notes: The coefficients from Tables 1 and 2 are displayed in this table. The p-values shown in brackets are obtained from a thousand Montecarlo simulations. The sample in the first column considers only requests closed by November 27, 2019, while the others take into account both pending and closed requests. Topic, month in which the agency received the request, serial requesters, number of requests received by the agency on the day that request i arrived, agency backlog, and percentage of pending requests (not yet overdue) are all control variables.

E.2 Inclusion of pretreatment information

Table E.2: Agencies' response to requests (inclusion of pretreatment information)

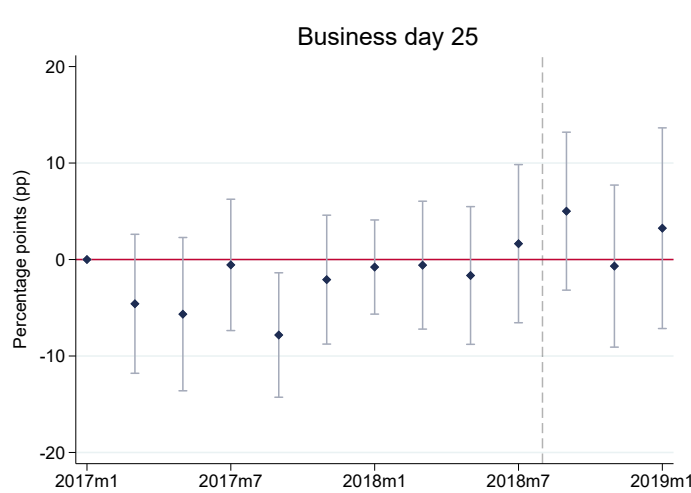
	Business days	0-15	15	16-25	25	0-13	14-16	17-23	24-25	>25
Post	-0.174 (1.492)	0.005 (0.024)	-0.003 (0.014)	0.022 (0.027)	-0.004 (0.013)	-0.002 (0.033)	0.008 (0.018)	0.035 (0.025)	-0.014 (0.019)	-0.027 (0.030)
Treatment x Post	-0.462 (2.268)	0.006 (0.032)	0.026 (0.018)	0.021 (0.027)	0.026 (0.016)	-0.026 (0.036)	0.038 (0.024)	-0.032 (0.023)	0.046** (0.021)	-0.027 (0.036)
Constant	17.960*** (5.978)	0.709*** (0.104)	0.337*** (0.042)	0.224*** (0.081)	-0.042 (0.026)	0.329*** (0.104)	0.410*** (0.059)	0.209** (0.087)	-0.016 (0.045)	0.067 (0.068)
Observations	6,695	6,896	6,896	6,896	6,896	6,896	6,896	6,896	6,896	6,896
R-squared	0.125	0.279	0.066	0.184	0.127	0.295	0.083	0.098	0.188	0.169

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by agency are shown in parentheses. The sample in the first column includes only requests closed by November 27, 2019. Control variables include: topic, agency and month fixed effects, serial requesters, number of requests received by the agency on the day that request i arrived, agency backlog, and percentage of pending requests not yet overdue.

E.3 Difference-in-Differences

To assess whether the parallel trends assumption holds, Figure E.1 plots the $treatment \cdot Post$ coefficients in Equation 4 for periods of time of two months. As displayed in the results in Table E.3, this figure shows a small jump after September 2018 around the second deadline.

Figure E.1: Response on the second deadline - Bimonthly



Notes: Each diamond represents β_3 in Equation 4 grouping data bimonthly. The 95% confidence interval is displayed. Control variables include: topic, strata, month in which the agency received the request, serial requesters, number of requests received by the agency on the day that request i arrived, agency backlog, and percentage of pending requests not yet overdue.

Table E.3: Agencies' response to requests (difference-in-differences model)

	Business days	0-15	15	16-25	25	0-13	14-16	17-23	24-25	>25
Treatment	-1.119 (2.551)	0.006 (0.068)	-0.016* (0.009)	-0.033 (0.035)	0.009 (0.012)	0.030 (0.071)	-0.033** (0.013)	-0.032 (0.024)	0.009 (0.018)	0.027 (0.042)
Post	0.325 (1.488)	-0.023 (0.039)	0.001 (0.014)	0.046 (0.030)	-0.011 (0.015)	-0.029 (0.048)	0.003 (0.018)	0.057** (0.027)	-0.007 (0.020)	-0.023 (0.030)
Treatment \times Post	1.036 (2.329)	-0.008 (0.052)	0.021 (0.015)	0.009 (0.036)	0.044* (0.022)	-0.037 (0.054)	0.039* (0.020)	-0.048* (0.025)	0.046 (0.029)	-0.001 (0.039)
Constant	14.770** (5.841)	0.700*** (0.132)	0.037 (0.051)	0.214** (0.107)	-0.009 (0.030)	0.607*** (0.137)	0.158** (0.065)	0.133 (0.101)	0.016 (0.072)	0.086 (0.078)
Observations	6,695	6,896	6,896	6,896	6,896	6,896	6,896	6,896	6,896	6,896
R-squared	0.080	0.170	0.023	0.094	0.040	0.193	0.043	0.048	0.089	0.114

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by agency are shown in parentheses. The sample in the first column includes only requests closed by November 27, 2019. Control variables include: topic, strata, month in which the agency received the request, serial requesters, number of requests received by the agency on the day that request i arrived, agency backlog, and percentage of pending requests not yet overdue.

E.4 Dropping requests received before the intervention started and answered only after it began

Requests pending when our intervention started on September 6, 2018 (*Case II*) might be subject to spillover effects, as shown in Section 5.3. This type of request is not included in the samples in Table E.4. Furthermore, these samples retain only agencies that had requests both in Cases *I* and *III* (see Figure 2). As in Tables 1 and 2, we estimate the effect in the postintervention period.

Table E.4: Agencies' response to requests (omitting requests received before and answered after the intervention started)

	Business days	0-15	15	16-25	25	0-13	14-16	17-23	24-25	>25
Treatment	0.134 (1.721)	-0.011 (0.053)	-0.006 (0.010)	-0.008 (0.043)	0.062** (0.027)	-0.005 (0.049)	-0.003 (0.015)	-0.080*** (0.025)	0.069** (0.035)	0.019 (0.028)
Constant	16.597*** (2.168)	0.721*** (0.065)	0.111 (0.093)	0.180*** (0.055)	-0.028 (0.018)	0.590*** (0.075)	0.113 (0.094)	0.249*** (0.051)	-0.051* (0.029)	0.099** (0.046)
Observations	2,969	3,071	3,071	3,071	3,071	3,071	3,071	3,071	3,071	3,071
R-squared	0.083	0.200	0.054	0.122	0.056	0.199	0.080	0.056	0.122	0.130

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by agency are shown in parentheses. The sample in the first column includes only requests that were closed by November 27, 2019. Control variables include: topic, month in which the agency received the request, serial requesters, number of requests received by the agency on the day that request i arrived, agency backlog, and percentage of pending requests not yet overdue.

E.5 Considering days spent in workshops

In this case, we account for civil servants’ attendance at workshops and adjust the number of days it took the agency to reply to the requests. Table E.5 shows the results when Equation 1 is estimated.

Table E.5: Agencies’ response to requests (considering days spent at workshops)

	Business Days	0-15	15	16-25	25	0-13	14-16	17-23	24-25	>25
Treatment	0.134 (1.720)	-0.013 (0.052)	-0.013 (0.010)	-0.003 (0.044)	0.054** (0.026)	-0.005 (0.049)	-0.005 (0.015)	-0.073*** (0.026)	0.067** (0.032)	0.016 (0.028)
Constant	16.376*** (2.154)	0.719*** (0.065)	0.085 (0.093)	0.184*** (0.055)	-0.018 (0.015)	0.597*** (0.078)	0.103 (0.094)	0.235*** (0.049)	-0.033 (0.025)	0.097** (0.046)
Observations	3,006	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111
R-squared	0.081	0.193	0.032	0.111	0.061	0.191	0.086	0.060	0.130	0.127

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by agency are shown in parentheses. The sample in the first column includes only requests closed by November 27, 2019. All dependent variables were adjusted for the number of days that civil servants spent in workshops. We subtract the number of days spent in workshops from the number of business days taken by the agency to respond to the request. Then, we reclassify the response time into the intervals used throughout the paper. Control variables include: topic, month in which the request was received by the agency, serial requesters, number of requests received by the agency on the day that request i arrived, agency backlog, and percentage of pending requests not yet overdue.

Last, Table E.6, shows the results when we control for attendance at workshops while the request was pending.

Table E.6: Agencies’ response to requests controlling for attendance at workshops

	Business days	0-15	15	16-25	25	0-13	14-16	17-23	24-25	>25
Treatment	0.773 (1.741)	-0.031 (0.052)	-0.017* (0.009)	0.004 (0.044)	0.050** (0.023)	-0.018 (0.049)	-0.009 (0.015)	-0.065** (0.026)	0.066** (0.031)	0.026 (0.027)
Workshop	7.316*** (1.748)	-0.203*** (0.066)	-0.049** (0.020)	0.083 (0.059)	-0.043 (0.038)	-0.146** (0.062)	-0.055** (0.024)	0.084** (0.033)	-0.003 (0.042)	0.120*** (0.027)
Constant	11.940*** (2.649)	0.844*** (0.077)	0.115 (0.090)	0.133** (0.065)	0.008 (0.029)	0.687*** (0.100)	0.137 (0.089)	0.184*** (0.053)	-0.031 (0.037)	0.023 (0.051)
Observations	3,006	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111	3,111
R-squared	0.098	0.214	0.040	0.115	0.065	0.202	0.090	0.066	0.130	0.140

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered by agency are shown in parentheses. The sample in the first column includes only requests closed by November 27, 2019. The workshop variable takes the value of one if the agency had participated in a workshop before it received the request or while the claim was still pending, and zero otherwise. Control variables include: topic, month in which the agency received the request, serial requesters, number of requests received by the agency on the day that request i arrived, agency backlog, and percentage of pending requests not yet overdue.

Appendix References

- Hemming, Karla, and Jen Marsh. 2013. “A menu-driven facility for sample-size calculations in cluster randomized controlled trials.” *The Stata Journal* 13 (1): 114–35.
- Hemming, Karla, and Monica Taljaard. 2016. “Sample size calculations for stepped wedge and cluster randomised trials: a unified approach.” *Journal of Clinical Epidemiology* 69:137–46.