

## All Eyes on Them: A Field Experiment on Citizen Oversight and Electoral Integrity<sup>†</sup>

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*Can information and communication technologies help citizens monitor their elections? We analyze a large-scale field experiment designed to answer this question in Colombia. We leveraged Facebook advertisements sent to over 4 million potential voters to encourage citizen reporting of electoral irregularities. We also cross-randomized whether candidates were informed about the campaign in a subset of municipalities. Total reports, and evidence-backed ones, experienced a large increase. Across a wide array of measures, electoral irregularities decreased. Finally, the reporting campaign reduced the vote share of candidates dependent on irregularities. This light-touch intervention is more cost-effective than monitoring efforts traditionally used by policymakers. (JEL C93, D12, D72, D83, O17)*

Clientelism, voter intimidation, and electoral fraud are part of the long list of electoral irregularities that persistently threaten democratic institutions in the developing world (World Bank 2017). Politicians draw on these different strategies, often combining several of them, as a way of distorting elections to their advantage. Beyond the direct consequences of undermining fair elections and eroding political accountability (Stokes 2005; Hicken 2011), a growing amount of evidence has shown that different types of electoral irregularities also harm the economic and political well-being of countries.<sup>1</sup>

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<sup>1</sup>By increasing the political returns of targeted transfers, clientelism leads to the underprovision of public goods and it generates inefficiencies (Khemani 2015; Baland and Robinson 2007; Vicente and Wantchekon 2009). Indirectly, it is also correlated to fiscal corruption (Singer 2009), which in turn might cause inefficiencies for firms

Despite systematic efforts by governments and the international community to reduce the prevalence of electoral irregularities, these remain pervasive across the developing world. The most widely used and studied measures to deter electoral irregularities, such as deploying electoral observers or auditors, require high state capacity, coordination, and large investments (including, for instance, the training of several hundreds of in situ observers), which are scarce in these contexts, and particularly in remote regions where these issues are more pronounced. At the same time, both the digitization and the information and communication technology (ICT) revolutions have provided a large number of tools that are cost-effective, scalable, and thus able to overcome those hurdles, which have only recently begun to be exploited to curb electoral irregularities.

In this paper, we study whether crowdsourcing the oversight of elections—i.e., outsourcing this task to a large group of people through online platforms—can be effective in the fight against electoral irregularities.<sup>2</sup> We analyze a massive online campaign conducted through Facebook that had the goal to encourage citizen monitoring of elections around the 2019 mayoral elections in Colombia and ask whether it was effective at both generating citizen reporting and reducing electoral irregularities.

This context provides an ideal setting to study this question for three reasons. First, electoral irregularities remain highly pervasive in Colombia despite the presence of domestic and international observers and auditors. Second, in recent years, there have been initiatives by both the Colombian government and NGOs to increase civil society's engagement in overseeing elections through online reporting websites and applications. Finally, internet access as well as social media usage have been on the rise throughout the country, with 69 percent of the population connected and over 60 percent of them registered as Facebook users.

Leveraging the features of this setting, we launched a large-scale Facebook advertisement campaign, reaching more than 4.4 million citizens, which represents a third of the targeted population. We randomized a subset of our sample of 698 municipalities (more than half in the country) to receive advertisements encouraging citizen reports of irregularities through a pre-existing website hosted by the Misión de Observación Electoral (MOE), a prominent local NGO.”

In designing the advertisements we sent to citizens, we targeted two underlying reasons why citizens might shy away from reporting. As emphasized in the literature studying persuasion (DellaVigna and Gentzkow 2010), media campaigns make a direct emotional appeal to viewers, which in our setting might prime intrinsic motivations to spur citizens to report. Alternatively, despite being encouraged to report, citizens may face a “last-mile problem” due to the difficulty of reporting and acquiring the information to do so. To understand the role of these different channels, we designed three versions of the advertisements, containing either (i) a message informing citizens about the MOE's reporting website and providing them with a link to their online form, (ii) a call-to-action message urging citizens to act against

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and governments alike (Olken and Pande 2012). Furthermore, voter intimidation might also help to perpetuate violence in weak states (Acemoglu et al. 2013; Robinson and Torvik 2009).

<sup>2</sup>Although crowdsourcing has been previously used by governments and nongovernment organizations (NGOs) to monitor elections, experimental studies testing for its effectiveness are still scant. A well-known example is the case of *Uchaguzi*, an election monitoring digital platform used in Kenya, which has been credited with reducing electoral violence (World Bank 2016).

irregularities by reporting them (but without any information about how and where to do so), or (iii) a combination of both.

In addition to understanding citizens' responses to the reporting campaign, we also wanted to study how candidates reacted to it. To do so, we further cross-randomized whether candidates running for mayor in certain municipalities, as well as their staff, received a letter informing them about the reporting campaign or not. This second treatment arm allows us to study the general equilibrium effect of these interventions to the extent that, aside from citizens, candidates also became aware of the reporting campaign and had time to react to it. Ours is one of the few studies that experimentally varies candidates' and citizens' involvement separately when examining interventions around elections.

We find that citizens in the municipalities exposed to the reporting campaign were significantly more likely to file reports, and when they did so they were also more likely to include information useful for prosecuting the instigators of electoral irregularities. In particular, being exposed to the reporting campaign increases the proportion of municipalities with at least one report by about 10.6 percentage points, and those with evidence-backed reports with evidence-backed reports by 8.8 percentage points (corresponding to an increase of 37 percent and 55 percent, respectively, from the control mean). We further find significant differences in exposing citizens to variations in the content of the ads. In particular, providing information about the reporting website generated more reports than the ads containing only messages encouraging citizens to report. This indicates that an important barrier citizens face to reporting in this context is a lack of information about where to do so.

This first set of results suggests that the reporting campaign did increase citizens' monitoring of elections. But did this discourage candidates from engaging in electoral malpractice? The main challenge in answering this question is that measuring electoral irregularities is difficult given their illicit and opaque nature. Moreover, in our context, this is complicated since commonly used measures of irregularities, such as survey-based measures and reports, might confuse reporting behavior with the actual occurrence of irregularities. To overcome these difficulties, we constructed two mutually complementary measures that circumvent them. First, we created an original database of mentions of irregularities in the news, coming from local and national media outlets in a wide array of formats, including written, TV, and radio news, both online and offline. Second, drawing from the literature on electoral forensics, we constructed a measure of electoral irregularities based on administrative data. In particular, we leverage fine-grained voting record data to proxy electoral manipulation as deviations in the distribution of the tallies' second digit obtained by each candidate from Benford's second digit law, a popular measure in this literature (Hicken and Mebane 2017).

Results using both of these measures suggest that the increase in citizen monitoring significantly deterred irregularities, affecting both municipalities where candidates were notified about the campaign and where they were not. Municipalities exposed to the reporting campaign experienced a reduction of 5.5 percentage points in the likelihood of having an irregularity mentioned in the media, and it decreased the probability that the voting counts deviated from Benford's second digit law by approximately 8.7 percentage points. In examining the types of irregularities affected, we find that the largest decline comes from instances of vote buying reported in the media, which is a common type of irregularity in this context.

Next, we study whether this reduction in electoral irregularities might have affected the electoral prospects of candidates whose votes depended on electoral malpractice. To identify such candidates from the large set of those running in the mayoral elections, we conducted a pretreatment survey three weeks before the intervention. For every candidate running in their municipality, we asked citizens to assess each candidate's perceived likelihood of engaging in electoral irregularities. We then validated this variable against objective measures of candidates' engagement in illegal activities. The results indicate that the reporting campaign reduced the vote share of these candidates substantially. For instance, we find that candidates who rated above average in the survey-based measure of electoral irregularities experienced a 2.5 percentage point decrease in their vote share in municipalities exposed to the reporting campaign. We further provide evidence showing that this effect is not driven by a difference in the popularity of candidates identified as more prone to irregularities.

The detected drop in electoral irregularities due to the reporting campaign could explain the reduction in the vote share of those candidates who would have likely engaged in such irregularities. However, a potential alternative channel could be a change in citizens' social norms about irregularities and their preferences for different types of candidates. To approximate the relative importance of the first mechanism, we perform a set of bounding exercises that indicate that at least 28 percent, and potentially all of the effect on candidates' vote share, is due to the drop in irregularities.

Finally, we show that the reporting campaign was very cost-effective compared to other interventions studied in the literature. A simple back-of-the-envelope calculation suggests that the intervention reduced by one percentage point per polling station the vote share of the candidates most prone to engage in electoral irregularities, at a cost of US\$0.70. As a benchmark, the more traditional strategy of deploying electoral observers costs several orders of magnitude more (over US\$500 also for a 1 percentage point change in vote share per polling station, in our most "optimistic" calculations), and this is true even for more recent innovations, which use ICTs to enhance electoral monitoring. Thus, by informing on how to innovate existing practices, this paper promises to contribute to ongoing endeavors of governments and democracy-promotion organizations to curb electoral irregularities.

This paper makes several contributions and builds on at least three strands of literature. First, it speaks to extant work on ways to fight electoral irregularities. Table 1 provides a summary of the interventions studied in this literature and their findings. Most studies have focused on either top-down methods to monitor elections—e.g., the use of domestic and international observers—or on bottom-up approaches to educating civil society about the negative consequences of electoral malpractice. We contribute to this scholarship by analyzing the effectiveness of a citizen monitoring campaign that combines elements from the mobilization and the monitoring strategies, which constitutes a third understudied approach.<sup>3</sup> Furthermore, we show that

<sup>3</sup>Four papers study interventions that included electoral reporting components, but they do so tangentially and/or their focus does not fully align with ours. (i) Ferree et al. (2017) show that monetary incentives provided through several ICT channels increase different forms of political participation, including volunteering as citizen electoral observers, but do not study the effects on electoral irregularities. (ii) Driscoll and Hidalgo (2014) study an education campaign informing citizens how to file complaints about electoral irregularities in Georgia and find

TABLE 1—EXPERIMENTAL AND QUASI-EXPERIMENTAL INTERVENTIONS TO CURB ELECTORAL IRREGULARITIES

Paper	Context	Type of intervention	N. observations/ treated	Direct measures of irregularities	Other related outcomes
<i>Panel A. Election observers</i>					
Asunka et al. (2019)	Ghana/2012	Domestic observers	1,794 polling stations/1,230	• Reports of violence/intimidation (from surveys): -7 pp	• Turnout: -4.5pp • Abnormal turnout <sup>a</sup> (= 1): -3.3 to -4.7 pp
Buzin, Brondum, and Robertson (2016)	Russia/2011	Domestic observers	7,123 polling stations/768	NA	• Turnout, incumbent's and challengers' vote share: null <sup>†</sup>
Enikolopov et al. (2013)	Russia/2011	Domestic observers	3,164 polling stations/156	NA	• Turnout: -6.5 pp • Incumbent's vote share: -10.8 pp • Challengers' vote share: 1.7 - 3.5 pp
Hyde (2007)	Armenia/2003	International observers	1,763 polling stations/1,008	NA	• Incumbent's vote share: -2 pp - -5.9 pp
Hyde (2010)	Indonesia/2004	International observers	1,822 villages/482	NA	• Incumbent's vote share: 6.5 pp
Ichino and Schündeln (2012)	Ghana/2008	Domestic observers	868 electoral areas/276	• % change in registered voters: -3.5	NA
Leeffers and Vicente (2019)	Mozambique/2009	Domestic observers	8,394 polling stations/989	NA	• Turnout: -2 pp • Blank votes: 1.2 pp • Incumbent's vote share: null <sup>†</sup> • Challenger's vote share: null <sup>†</sup>
		International observers	1,812 polling stations/67 <sup>b</sup>	NA	• Turnout: null • Blank votes: null • Incumbent's vote share: null • Challenger's vote share: null <sup>†</sup>
<i>Panel B. Top-down ICT monitoring</i>					
Callen and Long (2015)	Afghanistan/2010	Announcement of photo quick count	471 polling centers/238	• Aggregation fraud (number of votes): ‡ -18,790 (politicians connected to aggregators)	• Votes for most politically connected candidate: -5.5 pp • Damaged election material (= 1): -10.8 pp
Callen et al. (2016)	Uganda/2011	Announcement of photo quick count <sup>c</sup>	1,001 polling centers/681	• Missing voting tallies (= 1): -5.6 pp • Adjacent two last digits in winning candidate's voting tally (= 1): -7.9 pp	• Incumbent's votes: null

(Continued)

this approach is more cost-effective than the previously-studied alternatives, and we highlight its potential to be fully scalable.

Second, we contribute to the growing literature examining the use of bottom-up technologies to improve governments' accountability and the delivery of public goods and services. Spurred by the World Bank's 2004 *World Development Report*, governments and NGOs alike have heeded the call to use these types of technology

that it increased electoral irregularity reports but depressed turnout. They interpret these results as a consequence of citizens' belief that they were being monitored by either the regime or researchers and that retaliation might ensue. (iii) Aker, Collier, and Vicente (2017) study the effect of sending SMS messages inviting citizens to report electoral problems to a hotline around elections in Mozambique. Although they find no effects on the occurrence of election problems, they do find increased turnout and knowledge about elections. (iv) Finally, Gonzalez (2021) finds that areas in Afghanistan with access to cellphone coverage present less electoral fraud and argues that this is due to greater use of an electoral irregularity reporting hotline.

TABLE 1—EXPERIMENTAL AND QUASI-EXPERIMENTAL INTERVENTIONS TO CURB ELECTORAL IRREGULARITIES (CONTINUED)

Paper	Context	Type of intervention	N. observations/ treated	Direct measures of irregularities	Other related outcomes
<i>Panel C. Voter education campaigns</i>					
Blattman et al. (2019)	Uganda/2016	Anti-vote-buying in-person campaign	2,796 villages/1,427	Survey data: • Perception about others' likelihood of selling vote: -0.061 SD • Vote-buying: null for incumbents and 0.062 SD for challengers	• Turnout: null <sup>†</sup> • Incumbent's vote share: -0.063 SD (identical but opposite sign for challenger's vote share)
Collier and Vicente (2014)	Nigeria/2007	Anti-violence campaign	24 enumeration areas (50 inds. per area)/12	• Violence events reported by journalists (= 1): -46.8 pp • Perceptions of politically motivated violence: -0.23 SD	• Turnout: 11.1pp • Incumbent's vote share: 8-12.8 pp • Challengers' vote share: -7 pp
Hicken et al. (2018)	Philippines/2013	T1: Promise not to sell vote, T2: Promise to vote in good conscientiousness	883 voters/592	Vote switching as proxy for vote-selling: • T1: -9.53 • T2: null	NA
Schechter and Vasudevan (2021)	India/2014	Anti-vote-buying radio campaign	60 radio stations/30	• Vote share for clientelistic parties: -7.14 pp	• Turnout: null • Incumbent's vote share: null
Vicente (2014)	São Tomé and Príncipe/2006	Anti-vote-buying in-person campaign	50 enumeration areas/40	• Perception that voting is affected by vote-buying: -0.42 - -0.46 SD • Perception that voting was conducted in good conscience: 0.32-0.48 SD • Frequency of vote-buying: -0.17 - -0.22 SD	• Turnout: -2.5 - -6.4 pp • Incumbent's vote share: 3.4 pp • Challengers' vote share: -3.5 pp
<i>Panel D. Bottom-up monitoring</i>					
Aker, Collier, and Vicente (2017)	Mozambique/2009	T1: Education campaign; T2: SMS w/info. to reporting hotline; T3: newspapers w/info. to hotline + T1	161 polling stations/120	• Number of electoral problems reported by election observers (T1/T2/T3): null/null/-0.588	• Turnout: 5.3/5.3/5.4 pp • Incumbent's vote share: 4.6/null/4.1pp • Challengers' vote share: -3.2/null/-1.4 pp
Driscoll and Hidalgo (2014)	Georgia/2008	Education campaign to file electoral complaints	84 precincts/42	NA	• Complaints (= 1): 12 pp • Number of complaints: 0.26 • Turnout: -5.44pp • Incumbent's vote share: null
Gonzalez (2021)	Afghanistan/2009	Cellphone access	1,074-2,039 polling stations	• Abnormal votes: <sup>d</sup> -4 pp • Abnormal polling centers: -7.7 pp	• Complaints to reporting hotline: 0.144-0.257 • Number of insurgent attacks: null

Notes: This table summarizes the literature on experimental and quasi-experimental interventions to curb electoral irregularities. We chose for each paper the most relevant results related to electoral irregularities and other related outcomes. In some cases, this meant leaving aside the results of some papers that do not fully fit the objectives of this table.

<sup>†</sup> Effects are statistically significant for some subgroups.

<sup>‡</sup> Only for estimated lower Lee bound.

<sup>a</sup> Abnormal turnout is measured using different indicators of high turnout relative to the constituency mean or median.

<sup>b</sup> The authors indicate that 67 international observers were sent to several polling stations, but do not indicate the exact number.

<sup>c</sup> The authors study different types of announcement letter, but we only report the overall effect.

<sup>d</sup> The author's measure of abnormal votes combines outliers in turnout, votes for the winning candidate, and complaints about irregularities.

in areas as diverse as education, health, public works and elections. Most of the papers in this strand of the literature<sup>4</sup> have focused on studying offline interventions that do not exploit the potential of ICTs and social media to create engagement and to facilitate the spread of information to increase citizen oversight effectively. Despite recent calls to study this “second generation” of bottom-up technologies (Peixoto and Fox 2016), our paper is one of the few studies to do so using field experiments.<sup>5</sup>

Related to this last point, this paper also speaks to the literature on e-governance and ICT interventions designed to increase the accountability and the efficiency of public services. While most of this literature has focused on studying top-down efforts to increase data availability or decrease leakages,<sup>6</sup> this paper focuses on crowdsourcing, which is an understudied technology. Importantly, our findings provide a rare example of how social media can be used to enhance democracy and transparency, in contrast to the preponderant view that has focused on its negative effects; for a review, see Zhuravskaya, Petroya, and Enikolopov (2020).

The remainder of this paper is organized as follows. Section I provides background information about the intervention’s context, including a discussion of electoral irregularities in Colombia, an overview of online electoral reporting, and the elections around which the experiment was conducted. Section II provides a description of the experimental design, the study sample and the data used. Section III presents and discusses the main results, and Section IV provides a cost-benefit analysis of the intervention. Finally, Section V considers the relevance of the findings from a policy perspective and concludes.

## I. Context

### A. Electoral Irregularities in Colombia

Electoral irregularities take many forms and permeate every election in Colombia’s democracy. Most existing studies have explored their extent and their main features drawing largely on qualitative accounts.<sup>7</sup> However, a few studies have documented the incidence of the problem quantitatively. Fergusson, Molina, and Riaño (2017) use a list experiment to estimate Colombians’ engagement in clientelistic practices—broadly defined as receiving particularistic benefits in exchange for political support—and find that approximately 18 percent do so at some point in their lives.<sup>8</sup> Using this same method, García-Sánchez and Pantoja (2015) show that about 7 percent of voters were intimidated to vote in a particular way in the 2014 presidential elections.

<sup>4</sup>See Fox (2015) for a review of this vast literature.

<sup>5</sup>Recent examples include Aker, Collier, and Vicente (2017); Aker and Ksoll (2020); and Muralidharan et al. (2021).

<sup>6</sup>See Muralidharan, Niehaud, and Sukhtankar (2016); Lewis-Faupel et al. (2016); Banerjee et al. (2020); Callen et al. (2020) for recent examples.

<sup>7</sup>See, for instance, Leal and Dávila (1990); Holland and Palmer-Rubin (2015).

<sup>8</sup>Other studies find similar estimates in other countries in the region (Gonzalez-Ocantos et al. 2012).

We use the term “electoral irregularities” to refer to any conduct affecting elections penalized by Colombian law. Apart from vote buying and voter intimidation, common irregularities include:<sup>9</sup>

**Illicit Political Advertising:** Political advertisement is forbidden on election day and on public infrastructure.

**Campaigning by Public Servants:** When public servants use their position to interfere in elections by either trying to favor or harm a particular candidate or party.

**Fraud in Voter Registration:** When citizens register to vote in a polling station in a location different from their place of residence in order to obtain an illicit profit or to alter electoral results. This is usually done as a way to facilitate vote buying as explained below.

**Election Disruption:** When someone disturbs or prevents the normal development of elections or vote counting by deceit or force. In practice it frequently occurs through riots led by citizens or candidates.

**Electoral Fraud:** Other illicit forms of altering electoral results, such as ballot stuffing.

The organizational details about how electoral irregularities occur vary according to the type of irregularity. As reported and studied in diverse contexts (Stokes 2005; Stokes et al. 2013), in Colombia vote buying and other forms of clientelism are carried out via local brokers mediating between political organizations and voters. These brokers play the important role of providing political organizations with the information required to target and recruit potential voters into clientelistic relationships, as well as ensuring that these voters actually vote in the intended way. A common form of monitoring compliance is by registering voters in polling stations outside of their place of residence so that brokers can control their clients’ vote choice (Rueda 2017).<sup>10</sup>

Other types of electoral irregularities, such as illicit political advertising and campaigning by public servants, are commonly done by or with the complicity of local politicians, who collude with candidates to return political favors (Arenas 2018). Voter intimidation is commonly performed by armed actors such as guerrillas, paramilitaries, criminal gangs, or even the military, in collusion with local or national politicians (Acemoglu, Robinson, and Santos-Villagran 2013), but also by nonarmed actors, such as employers who threaten their employees with job loss if they fail to vote in a particular way.

<sup>9</sup>We approximate how common these irregularities are by the number of reports gathered about them. Online Appendix Figure A1 displays the number of reports made to the MOE in the 2015 elections, using the same window of time around election day used in our intervention.

<sup>10</sup>Evidence from other contexts shows that reciprocity (Finan and Schechter 2012) or, alternatively, targeting voters likely to sympathize with the candidate supported by the broker (Nichter 2008) are also prevalent methods to ensure compliance in vote-buying.



### B. Electoral Oversight and Reporting in Colombia

Several governmental agencies and NGOs run online electoral reporting platforms in Colombia. The MOE, an independent, nonpartisan NGO and one of the largest devoted to promoting electoral integrity,<sup>11</sup> hosts one of the most important of these websites, called *Pilas con el voto* (roughly translated, “keep an eye on your vote”). This website allows users to submit reports anonymously, requiring them to specify only the date and municipality of the reported irregularity. Additionally, it asks the users to describe the reported event in a free-form field, which is then classified by the MOE’s staff into one (or several) electoral irregularities. Depending on the amount of evidence and facts (places, names, and proof such as videos) provided about the reported irregularities, they also rank the report’s “trustworthiness” in three categories: high, medium, and low.

As an NGO, the MOE does not have the power to directly investigate and take legal action about the reports they receive through *Pilas con el Voto*; yet, these are consequential. The MOE prepares official reports based on the information provided by citizens, which it disseminates widely among the general public and the government agencies in charge of investigating and sanctioning electoral irregularities. Furthermore, it shares information with these watchdog entities based on citizen reports and other sources resulting from their oversight activities.<sup>12</sup> This partnership has proven successful. For instance, during the 2019 local elections, after receiving several reports from citizens in El Rosario, a municipality in the southwest of the country, the MOE alerted the competent authorities about about potential electoral irregularities taking place there. Less than a year later (in May 2020), it resulted in the arrest and conviction of a mayoral candidate and his campaign chief of staff, both accused of vote buying. A similar case occurred in the northern municipality of Manaure, where the MOE raised concerns about systematic reporting of “identity theft” during the voter registration process and on the day of the election. In 2020, eight people involved in this case were convicted.

One of the main agencies involved in both investigating and sanctioning the electoral misbehavior reported to the MOE is the attorney-inspector general’s office (henceforth, AG).<sup>13</sup> The AG is an independent institution that oversees the correct conduct of public servants through both preventive and sanctioning powers. During the electoral process, it ensures candidates abide by the law and are in good standing to run for office. Additionally, it closely monitors the electoral process.

How effective are watchdog institutions in investigating and prosecuting the irregularities reported? Answering this question is complicated because a significant fraction of reports do not contain enough evidence for the electoral watchdogs to start a judicial case. For instance, only 13 percent of reports submitted to the MOE were deemed to have high-level “trustworthiness” (meaning they contain hard evidence about the facts reported). Notwithstanding this difficulty, a brief glance at the AG’s database of historical reports in the 2010–2018 period suggests two important facts: (i) 2.5 percent

<sup>11</sup>The MOE has offices in more than half of the municipalities in the country in all 32 departments.

<sup>12</sup>This collaboration has been institutionalized, as in the case of the *Eje Temático de Protección a los Mecanismos de Participación Democrática*, a special unit within the attorney general’s office, created in 2014 to investigate cases related to electoral irregularities.

<sup>13</sup>The Spanish name for this institution is the *Procuraduría General de la Nación*.

of reports ultimately reached a judicial decision, such as sanction or acquittal, and (ii) an additional 12.4 percent of reports were being investigated by the AG at the end of the period. Since investigations are publicly conducted, and thus might harm the reputation of candidates and parties, these statistics reflect that reporting does entail substantial costs for candidates accused of committing irregularities.

### C. *The 2019 Mayoral Elections*

In Colombia, local authorities—such as mayors, governors and municipal council members—are elected every four years in a single round. For this study, we focus on the 2019 local elections and, particularly, on mayoral races.<sup>14</sup> Reelection is not allowed, which implies that there are no incumbents in any of the races. Furthermore, given the country’s weakly institutionalized party system, mapping candidates to parties to make a case for party-wise reelection is not straightforward. This lack of party discipline is reflected in the large number of candidates participating in each race (e.g, the average number is five in our sample of municipalities).

## II. Research Design

### A. *Experimental Design*

The main intervention was a large-scale Facebook advertisement campaign designed to encourage citizen reporting of electoral irregularities. The campaign lasted for five days and targeted all Facebook users of 18 years or older. It started on October 24, three days before the elections, and it ended on the night of October 28, one day after the elections. This extra day allowed citizens who had witnessed electoral irregularities but had not reported them on election day to report them the next day through the reporting website.

We designed four versions of ads to flesh out the underlying mechanisms through which the campaign could potentially affect reporting. Namely, municipalities were randomized to receive Facebook ads with the following messages:

*C. Placebo Control Group.*—A message reminding viewers about the coming elections, “*Don’t forget that local elections will take place on Sunday, October 27.*”

*I. Information Message.*—A message informing viewers about the MOE’s reporting website, “*The MOE has the following website where you can report electoral irregularities: [LINK]. Don’t forget that local elections will take place on Sunday, October 27.*”

<sup>14</sup>We focus on mayors since this post is assigned at the municipal level, corresponding to the level at which we randomized our intervention. Additionally, the number of mayoral candidates is tractable compared to the council members, which often had several dozen candidates.

*CA. Call-to-Action Message.*—A message with a call-to-action to report irregularities and act against them, “*In these elections let’s stop electoral irregularities. Report them! Don’t forget that local elections will take place on Sunday, October 27.*”

*I + CA. Information + Call-to-Action Message.*—A message containing both TI and TC.

The rationale for each of these experimental groups is the following. We included a placebo message in the control group to net out the effect of politically-oriented advertisements on citizens’ behavior. Treatments I, CA, and I + CA, sought to separate two mechanisms through which the ads could have impacted citizens’ propensity to monitor elections, either (i) by reducing the cost of reporting by informing citizens about the reporting website or (ii) by highlighting the urgency to take action against electoral irregularities.

In each of these treatment groups, the corresponding text is featured as the header of the advertisement. Additionally, a short video in a slide-show format was shown beneath the header highlighting the main points of each message. Figure 1 depicts the slides used for the different ads.<sup>15</sup>

By targeting ads to the universe of (adult) Facebook users in a municipality, both citizens and members of the candidates’ campaigns might have become aware of the monitoring campaign directly. Moreover, they might have also learned about the campaign indirectly, by hearing about it through others. As such, the effects of treatments I, CA, and I + CA combine both citizens’ and candidates’ responses. However, it is important to note that candidates’ responses were limited by the fact that we sent the ads only three days before the elections.

To fully understand how candidates would react to this campaign with more foresight, we further cross-randomized whether candidates were informed about the intervention two months before its occurrence through letters. In particular, we randomized municipalities in groups I, CA, and I + CA<sup>16</sup> to the following conditions:<sup>17</sup>

*TL. Letter to Candidates.*—All of the candidates running for Mayor in the municipalities in this group were informed about the monitoring campaign.

*TNL. No Letter.*—None of the candidates running for Mayor in the municipalities in this group were informed about the monitoring campaign.

<sup>15</sup>The background in these slides was white so that it did not reflect any of the colors associated with particular candidates, and the main image shows a ballot box with a text urging to vote, which was the message transmitted through the placebo message.

<sup>16</sup>We did not allow the ad control group to be included in this second treatment arm to avoid deception—i.e., informing candidates about a campaign that was not taking place in that municipality.

<sup>17</sup>We further randomized two versions of these letters, with a slight change in the text. In the first version, which we call the “full knowledge letter,” candidates were informed about the campaign and the website promoted for the ad campaign. In the second version, which we call the “partial knowledge letter,” they were told that the campaign would take place, but the specific website was not mentioned. Since the difference between the treatment effects of these two groups are not statistically significant in most analyses, we do not report their results separately in the main text. See footnote 43 for their results.

Slide A



"Report electoral irregularities!"

Slide B



"Reporting website: Pilas con el voto"

Slide C



"Sunday, October 27"

Slide D



"Next local elections"

FIGURE 1. AD SLIDESHOW

*Notes:* This figure shows the four possible slides that appear in the ad interventions. Below each slide is an English translation of the text contained on the slides. The placebo control group was shown only slides C and D. The group with the information message was shown slides B, C, and D. The group with the call-to-action message was shown slides A, C, and D. Finally, the group with call-to-action and information message was shown all the slides, A–D.

Online Appendix Figure A2 displays an example of the letters sent. We partnered with the AG in sending these letters, for two main reasons. First, in Colombia, candidates' and their staffs' contact information is not public. Therefore, the AG helped by providing this information. Second, sending the letters on behalf of this entity maximized the chances that candidates would actually read them. To boost the effectiveness of these interventions, we sent reminder letters three weeks before the elections.<sup>18</sup> We sent both physical letters and emails to maximize the chances of getting the candidates' attention.<sup>19</sup>

<sup>18</sup> As discussed in Section IA, the planning and execution of electoral irregularities can take place several weeks and even months before the election, so this additional forewarning time would potentially have an important deterrent effect over candidates.

<sup>19</sup> Notably, the deadline to register for candidacy occurred more than a month before we sent the first of these letters, so this treatment did not generate any differential effect on the selection of candidates.

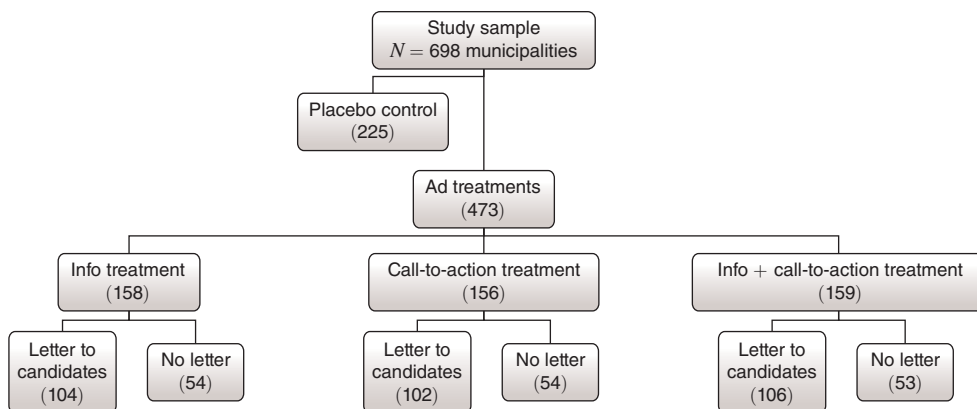


FIGURE 2. RANDOMIZATION DESIGN

Notes: This figure illustrates the experimental design of the experiment. The sample size within each treatment group is shown in parentheses.

Figure 2 summarizes the full factorial design of our experiment, and Figure 3 shows the timeline of the different interventions and electoral milestones.

We intentionally designed all interventions not to affect any particular candidate or party: all ads were nonpartisan, and we sent the letter to the universe of the mayoral candidates running in the treated municipalities. Thus, we do not have a priori reasons to believe that we intervened in the political outcomes of interest in ways different than potentially dampening the electoral prospects of those politicians likely to engage in electoral irregularities. Furthermore, we did not disclose the sample of municipalities nor their treatment assignment to our implementing partners (i.e., the MOE and the AG) to avoid potential biases.

### B. Study Sample

The study sample consisted of 698 municipalities coming from every Colombian department (see online Appendix Figure A3), containing approximately 19 million inhabitants, almost 40 percent of the population in the country.<sup>20</sup> Table A1 in the online Appendix presents the summary statistics for a selected set of variables for the municipalities included in the study sample and compares them to the universe of municipalities in the country. As expected, given the selection criteria, the average population is smaller in our sample (27,000) than in the whole country (43,000). The municipalities in our sample also have a relatively lower Facebook

<sup>20</sup>There are two reasons why we chose to use municipalities as the units of randomization. First, because it is the finest geographical level at which systematic data on reports exist. Second, we opted to treat the universe of mayoral candidates running in a municipality to minimize the risk of favoring a particular party or candidate. The sample was chosen following population-based criteria. Specifically, we only included municipalities with at least 5,000 and no more than 97,000 inhabitants. These cutoffs followed two principles. First, we used a lower bound because Facebook’s application programming interface (API) does not allow targeting populations with too few users. Second, we chose the upper bound to keep the costs of the ads within our budget, while ensuring a high-impact treatment able to reach at least 30 percent of the population. Additionally, we did not consider a few municipalities where we had run a pilot and ads had not reached more than 5 percent of users.

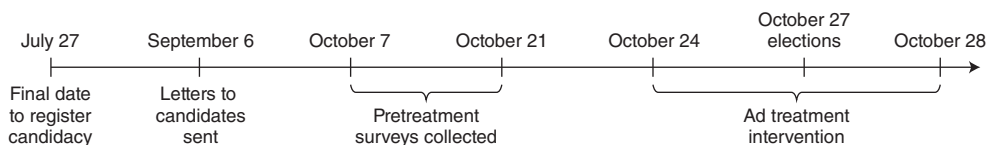


FIGURE 3. TIMELINE OF THE INTERVENTION

*Notes:* This figure shows the timeline of the interventions performed in the study. Note that the timeline is not drawn to scale.

penetration rate, with an average of approximately 40 percent of the population reported as active users by Facebook, while the average penetration rate across the country is 61 percent. However, apart from these differences, the experimental sample is fairly similar to the country's average across other characteristics, such as GDP per capita, rurality rates, poverty rates and previous reporting behavior.

### C. Randomization in Practice: Stratification and Balance Checks

To increase the balance on potential confounders across treatment conditions, we conducted a stratified randomization. We defined strata by the intersection of bins partitioning the sample in three ways: (i) by the fiftieth and eighty-fifth percentiles of the population over the age of 18, (ii) by the twentieth and eightieth percentiles of voter turnout in the first round of presidential elections in 2018, and (iii) by whether the municipalities filed reports through the MOE's website around the congressional elections of 2018 above or below the median.

Table A2 in the online Appendix reports balance checks for the different treatment arms using five sets of covariates, and online Appendix Figure A4 displays the  $p$ -values for a subset of the treatment arms. The first set of covariates includes measures of past reporting through the MOE's website. The second one has socioeconomic covariates, such as population or per capita income. The third set includes political covariates, such as past turnout and election outcomes for different parties, as well as past values of the forensic tests we use as outcome variables. The fourth set consists of region dummies. Finally, we also include covariates of interest such as the share of the adult population reached by the intervention ads and the responses to a pretreatment survey we discuss in the next sections.

The results suggest that municipalities are well balanced across treatment arms. Only 16 differences in means out of 264 comparisons in Table A2 are statistically significant at a 10 percent level or less. While these imbalances might have arisen by chance, this justifies including covariates in our main specifications (as explained in Section IIF).

### D. Ad Campaign Scale and Engagement

Table 2 provides summary statistics of measures reflecting the scale of the ad campaign (i) aggregated across all of the municipalities in the sample, (ii) per

municipality, and (iii) per population age 18 years and above (i.e., those eligible to vote).<sup>21</sup>

Overall, the ad campaign was successful in reaching a large population in the targeted municipalities. Approximately 4.4 million Facebook users saw their treatment condition ad an average of  $\approx 3$  times. In each municipality, the ad reached, on average, 6,245 people, which represents 34 percent of the adult population. Additionally, the ads generated substantial engagement by users: more than 23,000 people clicked on the link to the MOE's reporting website—i.e., an average of 73.9 people per municipality—at a cost of approximately US\$0.23 per link click.<sup>22</sup> In terms of other types of engagement, in the average municipality, 14 people “reacted” to the ads (i.e., by “liking” it, “loving” it, etc.), 6.5 of them shared them, and 0.63 of them left a comment.

While these statistics indicate that the ad campaign was successful in producing engagement and reaching a wide audience across the *average* municipality, it did not do so in some cases. For instance, in 21 municipalities there were absolutely no viewers. The reasons for these differences in ad viewers might have included many external factors, such as problems with internet connectivity in different areas or errors in Facebook's geolocation of some municipalities. However, these differences do not represent a threat to the identification strategy, given that they are well balanced across treatment arms: panel E of online Appendix Table A2 shows that there is balance in terms of municipalities with no viewers, in the number of viewers and also in the percentage of the population who viewed the ads.

### E. Data

*Outcome Variables.*—Our main outcomes of interest are the reports submitted by citizens to the MOE's reporting website and (proxies) for the actual occurrence of electoral irregularities. Additionally, we analyze the impact of the interventions on electoral outcomes. We measure these outcomes combining administrative and originally collected data. Below, we describe in detail how we measure each of them.

**1. Reporting:** Our main outcome variable to assess whether this campaign was successful in getting citizens to report electoral irregularities is the number of reports per municipality collected through the MOE's website. For our main measure, we only consider reports submitted on the dates of the intervention, i.e., from October 24, when the campaign started, to October 28, when it ended. In addition to analyzing the total number of reports, we also disaggregate them by their “trustworthiness,” assessed and classified by the MOE as either high, medium, or low, depending on the evidence and the information contained in the reports, as explained in Section I.

<sup>21</sup>The information contained in this table comes from Facebook's marketing API. For ease of interpretation, we altered the original name of some of the variables. In particular, “viewers of the ad” and “times the ad appeared on a screen” correspond to Facebook's “unique reach” and “impressions” variables.

<sup>22</sup>The implied “click rate”—i.e., the number of clicks by 10,000 impressions—is 0.4 percent. This is in the ballpark of related interventions. For instance, Brookman and Green (2014) use Facebook ads to promote campaigns of US politicians and find a click rate of 0.02 percent. Similarly, Enríquez et al. (2021) use Facebook ads to spread information about the performance of Mexican politicians, and find a click rate of 1–2 percent.

TABLE 2—SCALE OF AD CAMPAIGN

	Total	Per municipality	Per population > 18 years
Viewers of the ad	4,358,870	6,244.80	0.34
Times the ad appeared on a screen	12,886,430	18,461.93	1.01
People clicking on the MOE's link <sup>a</sup>	23,418	73.87	4.02 (per 1,000)
Users reacting to ad	9,623	13.79	0.76 (per 1,000)
Post shares	4,531	6.49	0.36 (per 1,000)
Comments on ad	437	0.63	0.03 (per 1,000)

*Notes:* This table reports several measures of the scale of the Facebook advertisement campaign, as well as metrics of the engagement of users with the ads. The variables reported in this table are defined as follows. “Viewers of the ad” is the number of distinct individuals who saw the ads at least once. “Times the ad appeared on a screen” is the number of times the ads appeared on any screen. “People clicking on MOE’s link” is the number of distinct individuals who clicked on the link landing on MOE’s reporting website. “Users reacting to ad” is the number of distinct individuals who reacted to the ad by clicking on one of the available Facebook reactions (i.e., like, love, laugh, etc ...). “Post shares” is the number of times people shared the ad in their own timeline, in other friends’ timelines or in groups. “Comments on ad” is the number of comments made on the ads.

<sup>a</sup>For this metric, we only considered the municipalities actually receiving the link to MOE’s website (i.e., the ones receiving the information message) when computing the measures by municipality and population 18 or older.

We define reports as “high quality” if these are classified as either of a medium or high level of trustworthiness.<sup>23</sup>

**2. Electoral Irregularities:** We use two different types of measures to proxy for the actual occurrence of electoral irregularities.

First, we capture irregularities by their mention in the news. There are two main challenges in using this type of measure to proxy for actual electoral irregularities. First, municipalities included in our sample are small and thus do not attract as much attention from major news outlets as bigger ones. Second, some news might originate from citizen reports through the MOE, which could bias our estimates in the direction of finding more news about irregularities in places receiving the treatment.

We overcome these difficulties by constructing an original database of electoral irregularities covering both local and national media from a large set of media types, such as TV, radio, and print and online newspapers. This focus on various news types allows us to maximize the chances of including news about the municipalities in our sample. Furthermore, we purge from our measure any news that originated from citizens’ reports to the MOE to avoid confounding both phenomena, although we alternatively use the full set of news as a robustness check. To avoid any type of researcher bias in the coding of these data, we outsourced the collection of this information to third parties. Finally, in using these data, we aggregate all of the irregularities in the news occurring in each municipality, but we also report the effects on specific types of irregularities. Section B of the online Appendix provides further details about how we built this dataset.

To construct our second measure of electoral irregularities, we borrow from the electoral forensics literature, which uses data-driven methods to detect electoral

<sup>23</sup>We do not distinguish medium and high trustworthiness reports, since there are very few reports of the latter type in our sample (under 10 percent of municipalities in the sample submit such reports).



irregularities. In Section IIIB, we discuss the specific tests we use and their interpretation. In constructing these tests we use the official voting records provided by the *Registraduría Nacional del Estado Civil*, Colombia's electoral office, at the voting booth level.

**3. Electoral Outcomes:** We also examine the effect of our intervention on election results. Specifically, we study whether it affected turnout, the vote share of particular candidates, or electoral competition. We compute all of these outcomes at the municipal level using official voting records from the *Registraduría Nacional del Estado Civil*.

*Pretreatment Survey.*—We administered a pretreatment survey to gather additional information that was not available from existing external sources.<sup>24</sup> We conducted this survey between October 7, three weeks before election day, until October 21, two days before the advertisement campaign began (see Figure 3). We recruited respondents through Facebook advertisements targeting users in the municipalities in the sample, inviting them to participate in the survey.<sup>25</sup> As with the main campaign, we displayed the survey recruitment ads to all users of age 18 or older in the sample of municipalities. Take-up was incentivized by including those who completed the survey in a raffle for several Samsung tablets (valued at US\$120).

The survey took approximately 10–15 minutes to answer, and the recruitment ad did not refer to its content or to the upcoming elections to avoid biased responses.<sup>26</sup> The main goal of this survey was to collect data on voters' perceptions about the mayoral candidates running in their municipalities, which we used in the analyses that we discuss later in detail in Section IIIC. The number of complete surveys is 6,121, coming from 630 municipalities, so the average number of responses per municipality is approximately 10. Our final sample is balanced across treatment conditions in terms of respondents' sociodemographic characteristics (see online Appendix Table A3), as well as in terms of the characteristics of the municipalities from which we obtained responses (see online Appendix Table A12).

*Other Covariates.*—We collected a rich set of municipal-level covariates to conduct balance checks and to include as controls in the main specifications. We mentioned these variables in Section IIC when we described the balance checks performed. In addition, we also collected candidate-level covariates, such as sex, age, party and type of electoral platform (i.e., single party, coalition of parties, or independent). In Section C of the online Appendix, we describe all these variables in more detail and indicate their sources.

<sup>24</sup> See Garbiras-Díaz and Montenegro (2022b) for access to this original survey.

<sup>25</sup> Using Facebook ads as a survey recruitment strategy has been studied in both developed and developing contexts. Samuels and Zucco (2013) show it is effective at reaching populations that are costly to reach through conventional survey methods, and Zhang et al. (2018) show that it approximates the representativeness of common recruitment methods such as phone surveys.

<sup>26</sup> The main header in these ads read, "Your opinion counts! Take our survey and participate in the raffle of three Samsung tablets. It will not take more than 10 minutes."

### F. Empirical Analysis

Our main specification to estimate the average treatment effect of the different interventions is the following:

$$(1) \quad y_m = \mathbf{T}'_m \beta + X'_m \gamma + \epsilon_m$$

where  $y_m$  is the outcome variable for municipality  $m$ ;  $\mathbf{T}_m$  is a vector of indicators for the different treatment arms;  $X_m$  is a set of municipal covariates, including a set of fixed effects for the strata used in the randomization; and  $\epsilon_m$  is the error term.

We estimate equation (1) using three different partitions of the treatment arms to study the effects of different subtreatments.<sup>27</sup> First, at the coarser level, we take  $\mathbf{T}_m$  to simply include an indicator variable that takes the value of one if municipality  $m$  is in any of the treatment arms receiving ads and zero if it is in the placebo control group. Second, we study the differences between the specific messages featured in the ads by including separate indicators for whether municipality  $m$  received the information message, the call-to-action message or both, in vector  $\mathbf{T}_m$ . Third, we analyze the effect of the letter sent to politicians by including indicators for whether municipality  $m$  received any of the reporting ads and either (i) we did not send a letter to candidates, or (ii) we did send it.

We report Huber-White standard errors for estimates of equation (1), along with randomization inference  $p$ -values to allow for inference that does not depend on distributional assumptions or asymptotic theory (Athey and Imbens 2017; Young 2018).

Instead of specifying the covariates to be included in  $X_m$ , we use the double-post-lasso covariate selection method proposed by Chernozhukov, Hansen, and Spindler (2015) and Belloni, Chernozhukov, and Hansen (2014) to choose them. This method increases the precision of the estimates without running into overfitting issues. Crucially, it includes covariates for which there is imbalance across treatment arms as well as important predictors of the outcome variables considered. Unless otherwise specified, the set of covariates in panels A–D in online Appendix Table A2 along with the number of responses to the pretreatment survey and the set of strata fixed effects are the ones used when relying on this method. As detailed in Section III E, we also report estimates without control variables as a robustness check.

## III. Main Results

### A. Results on Reporting

We begin by examining whether the reporting campaign was successful in inducing citizens to report irregularities and, in particular, high-quality reports with hard evidence that could be useful to prosecute offenders. Table 3 presents the point

<sup>27</sup>This approach allows us to study the effect of different treatment conditions without sacrificing the statistical power needed to estimate the full model with nine different treatment arm combinations. However, we also report the results of the “long model,” including all nine dummies as a robustness check.

estimates from equation (1), while Figure 4 provides a visual representation of a subset of the estimates.

Overall, the evidence indicates that the reporting campaign increased reporting substantially, both on the extensive as well as on the intensive margin. First, we find that receiving any of the treatments increased the probability that a report was filed from a municipality by 10.6 percentage points ( $p \approx 0.01$ ), which corresponds to an increase of 37 percent compared to the control mean. Similarly, the number of reports increased in these municipalities by about 0.37 ( $p < 0.01$ ), a 67 percent increase compared to the control.<sup>28</sup>

Next, we examine if the intervention affected the subset of higher quality reports. Municipalities receiving any of the treatments were 8.8 percentage points more likely to submit a report deemed as high quality by the MOE than control ones ( $p < 0.01$ ), and they increased the number of these types of report by 0.19 ( $p < 0.01$ ), which represent, respectively, a 55 and 94 percent increase compared to the control group.

We further explore *how* the ads' messages might have impacted citizens' incentives to report. To do so, we separately estimate the effect of each version of the ad (panel B of Table 3). We start by pointing out that across outcomes, the ads containing the information of where individuals could report—i.e., the link to the MOE's website—seem to be the ones driving the positive treatment effects. In particular, the “call-to-action” message does not change citizen reporting in a statistically significant way, while the municipalities receiving the information message saw an increase in the probability of filing reports by approximately 14.5 percentage points and the number of reports by 0.46 ( $p < 0.01$ ). Also note that there are no statistically significant differences between the “information” and the “information + call-to-action” messages, rejecting the hypothesis that the interaction of both versions could boost the incentives to report even further than the information message alone.

Next, we study whether the letters sent to candidates had differential effects on reporting. Interestingly, the effects are larger when no letter is sent, although this difference is only significant at the extensive but not the intensive margin. We interpret this finding in light of the results reported in the following section, which suggest that the letter sent to candidates might have had an *extra* deterrent effect over candidates' decisions to engage in electoral irregularities, which in turn might have reduced reporting by citizens.

### B. Effects on Electoral Irregularities

We now examine whether the intervention reduced the occurrence of electoral irregularities. To the extent that the reporting campaign was public, it is possible that campaign staff and candidates were informed about it and they changed their behavior in response to the threat of being reported. However, as mentioned in Section IIA,

<sup>28</sup>One potential concern with our main reporting outcomes is that they only capture reports filed during the period of the intervention. Thus, if the treatments shift the timing of reporting from the postintervention into the intervention period, the main estimates might mechanically overstate the overall effect. However, as reported in online Appendix Table A4, the opposite is true: the treatments caused a positive (but mostly insignificant) increase in reporting in the postintervention period, suggesting that effects persist in the short run.

TABLE 3—IMPACTS ON REPORTS

	Reports (= 1) (1)	N. reports (2)	High quality reports (= 1) (3)	High quality n. reports (4)
<i>Panel A. Pooled treatment</i>				
[T] Any treatment	0.106 (0.035) [0.010]	0.366 (0.100) [0.005]	0.088 (0.030) [0.005]	0.188 (0.053) [0.003]
<i>Panel B. Subtreatments by types of ads</i>				
[IA] Information ad	0.145 (0.048) [0.004]	0.456 (0.147) [0.000]	0.076 (0.040) [0.059]	0.131 (0.063) [0.027]
[CA] Call-to-action ad	0.016 (0.045) [0.708]	0.164 (0.136) [0.200]	0.074 (0.040) [0.060]	0.161 (0.082) [0.022]
[I + CA] Information + call-to-action ad	0.157 (0.046) [0.000]	0.472 (0.147) [0.000]	0.112 (0.040) [0.007]	0.270 (0.085) [0.000]
Test IA = CA, <i>p</i> -value	0.01	0.10	0.97	0.74
Test IA = I + CA, <i>p</i> -value	0.83	0.93	0.44	0.14
Test CA = I + CA, <i>p</i> -value	0.01	0.08	0.42	0.31
<i>Panel C. Subtreatments by letter/no letter</i>				
[NL] No letter: any ad	0.220 (0.047) [0.000]	0.515 (0.135) [0.000]	0.149 (0.043) [0.001]	0.267 (0.083) [0.000]
[L] Letter: any ad	0.048 (0.038) [0.207]	0.290 (0.115) [0.013]	0.057 (0.032) [0.079]	0.147 (0.057) [0.015]
Test NL = L, <i>p</i> -value	0.00	0.13	0.03	0.17
Control mean	0.29	0.55	0.16	0.20
Sample size	698	698	698	698

*Notes:* The outcome in column 1 is an indicator for whether any report was issued to MOE from each municipality. In column 2 it is the number of such reports. In columns 3 and 4 the same definitions are used on the subset of reports of a high quality (see Section I for a discussion about how quality of reports is assessed by MOE). All specifications include the covariates selected using the method described in Chernozhukov, Hansen, and Spindler (2015) and Belloni, Chernozhukov, and Hansen (2014). Robust standard errors are shown in parentheses and random inference *p*-values are shown in brackets.

candidates' knowledge about the reporting campaign and their subsequent reaction to it would have been substantially constrained by the fact that the advertising campaign started only three days before election day, which would have given them relatively little time to adjust accordingly. We thus expect that the letters sent to candidates informing them of the reporting campaign—which were sent almost two months before election day—would generate a larger behavioral response on candidates and, in particular, a larger deterrent effect on irregularities.

Given the illicit nature of electoral irregularities, measuring them has been one of the main challenges in the literature. A popular strategy has been to use citizen reports to infer the occurrence of different types of irregularities. However, in our context, this is not possible given that reporting is directly affected by the intervention in potentially opposite ways to its effect on irregularities: i.e., while the campaign increases reporting, as shown in the last section, it might have reduced their

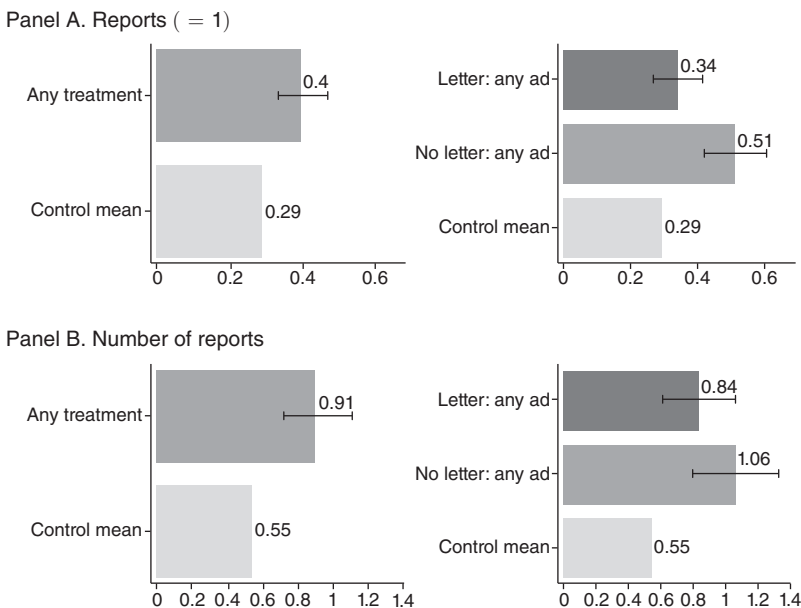


FIGURE 4. IMPACTS ON REPORTS

Notes: This figure reports the effects of the intervention on report outcomes. The outcome in panel A is an indicator for whether any report was issued to MOE from each municipality. In panel B it is the number of such reports. The control group mean is shown in each plot, while the remaining bars report the control group mean plus the effect for each treatment arm estimated using specification (1). All specifications include the covariates selected using the method described in Chernozhukov, Hansen, and Spindler (2015) and Belloni, Chernozhukov, and Hansen (2014). Ninety-five percent confidence intervals are reported.

actual occurrence. We develop two strategies to measure electoral irregularities that overcome this difficulty, which we describe next. Additionally, in online Appendix F, we describe the construction of irregularity proxies using the responses to a postintervention online survey. We prefer the outcomes reported in the main text since they rely on external sources, less subject to the biases from citizens’ perceptions of irregularities, which might have been affected by the advertisement campaign. The results across different variables are consistent qualitative and quantitatively.

*Media-Based Measure of Electoral Irregularities.*—Our first measure comes from an original database of electoral irregularities covered by the media, described in detail in Section IIE. We construct two outcome measures using this dataset: an indicator for whether any irregularity was mentioned in the news in a given municipality, and the number of such irregularities.

Figure 5 depicts the main treatment effects on the first of these variables, while Table 4 shows the estimates of the different treatment arms on both variables. These results indicate that the campaign had a large negative effect on electoral irregularities, at both the extensive and intensive margins. Receiving any of the treatments reduced the probability of irregularities occurring by 5.5 percentage points, and the number of irregularities by 0.08 ( $p < 0.05$ ). These effects are particularly large given that they represent reductions of approximately 34 percent and 41 percent, respectively, compared to the control group mean.

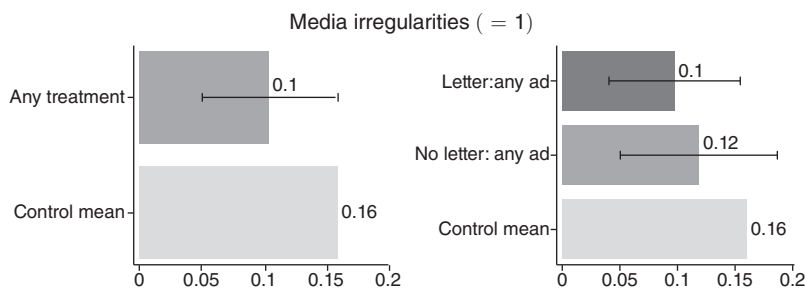


FIGURE 5. IMPACTS ON MEDIA-BASED IRREGULARITIES

*Notes:* This figure reports the effects of the intervention on a media-based measure of electoral irregularities. The outcome is an indicator for whether any irregularity was reported in the media in a particular municipality. The control group mean is shown in each plot, while the remaining bars report the control group mean plus the effect for each treatment arm estimated using specification (1). All specifications include the covariates selected using the method described in Chernozhukov, Hansen, and Spindler (2015) and Belloni, Chernozhukov, and Hansen (2014). Ninety-five percent confidence intervals are reported.

When examining the effect of the different ad messages, we observe that the ads containing information about the reporting website are the main driving force behind these treatment effects. The estimates for the call-to-action message are also negative, but they are not statistically significant.<sup>29</sup>

In line with our previous discussion about the short period that candidates had to react to the reporting campaign, we find that the letters sent to candidates generated larger and more precisely estimated reductions in the probability and the number of irregularities (see panel C of the same table). Yet, in drawing these comparisons, note that the difference between the municipalities receiving the letter and those that did not is not statistically significant.

As explained in Section IIE, to avoid confounding reporting and actual irregularities, we purge our main measure of electoral irregularities from all news that arises from citizen reports to the MOE. One potential problem in excluding report-related news is that we might be mechanically ignoring (actual) irregularities in municipalities treated by the reporting campaign since news about irregularities might be more likely to mention the MOE in those municipalities. As a robustness check, we thus consider an alternative measure containing *all* irregularities mentioned in the news, whether they mention reports made to the MOE or not, and we report the results in Table A5 of the online Appendix. We see that, although the precision of some of the estimated effects drop slightly, the main results hold, and the estimates remain statistically significant.

In addition to considering the effect of the interventions on all types of electoral irregularities, in online Appendix Table A6 we also examine this effect on particular cases. Although the estimates are noisy given that some types of irregularities have a substantially low probability of occurrence, we see that the reduction of irregularities seems to be concentrated on vote buying, whose likelihood drops by 4 percentage points in municipalities exposed to the reporting campaign. Even

<sup>29</sup> However, the difference between these treatment arms is not statistically significant (see panel B, Table 4).

TABLE 4—IMPACTS ON IRREGULARITY MEASURES

	Media-based irregularities		Deviations from Benford's 2nd digit law	
	Media irregularities (= 1) (1)	Number of media irregularities (2)	Index of all forensic test stats (z-score) (3)	Any <i>p</i> -value from Forensic tests < 0.05 (= 1) (4)
<i>Panel A. Pooled treatment</i>				
[T] Any treatment	-0.055 (0.027) [0.040]	-0.082 (0.037) [0.015]	-0.142 (0.068) [0.029]	-0.087 (0.038) [0.030]
<i>Panel B. Subtreatments by types of ads</i>				
[IA] Information ad	-0.060 (0.035) [0.100]	-0.092 (0.044) [0.051]	-0.297 (0.084) [0.001]	-0.121 (0.048) [0.016]
[CA] Call-to-action ad	-0.039 (0.035) [0.270]	-0.063 (0.046) [0.181]	-0.094 (0.085) [0.273]	-0.071 (0.051) [0.146]
[I + CA] Information + call-to-action ad	-0.067 (0.033) [0.050]	-0.089 (0.044) [0.054]	-0.039 (0.090) [0.668]	-0.070 (0.048) [0.137]
Test $IA = CA$ , <i>p</i> -value	0.56	0.50	0.02	0.36
Test $IA = I + CA$ , <i>p</i> -value	0.83	0.94	0.01	0.32
Test $CA = I + CA$ , <i>p</i> -value	0.42	0.56	0.56	0.99
<i>Panel C. Subtreatments by letter/no letter</i>				
[NL] No letter: any ad	-0.041 (0.035) [0.242]	-0.058 (0.046) [0.232]	-0.084 (0.088) [0.349]	-0.078 (0.050) [0.114]
[L] Letter: any ad	-0.062 (0.029) [0.022]	-0.094 (0.039) [0.009]	-0.173 (0.073) [0.017]	-0.093 (0.041) [0.019]
Test $NL = L$ , <i>p</i> -value	0.50	0.35	0.28	0.75
Control mean	0.16	0.20	0.00	0.52
Sample size	698	698	698	698

*Notes:* The outcome in column 1 is an indicator for whether any irregularity was reported in the media in a particular municipality. In column 2 it is the number of different irregularities. In column 3 it is the index of the  $\chi^2$ , Kolmogorov-Smirnov and Kuiper test statistics testing for Benford's second digit law, described in Section IIIB. In column 4 it is an indicator that takes the value of one if the *p*-value of any of these tests leads to rejection of the null hypothesis with less than a 5 percent significance level. All specifications include the covariates selected using the method described in Chernozhukov, Hansen, and Spindler (2015) and Belloni, Chernozhukov, and Hansen (2014). Robust standard errors are shown in parentheses and random inference *p*-values are shown in brackets.

though this type of irregularity seems to be the main driver of the effect, most types of irregularities also experience a decrease in their probability of occurrence. The direction of the estimates are preserved when excluding types of irregularities one at a time from our main measure (see online Appendix Table A7).

In Section F of the online Appendix, we report the results using alternative survey-based measures of irregularities. These probably capture types of irregularities that might be underreported by the media. Notably, we find that the intervention reduced the occurrence of illicit advertising and campaigning by public servants using this measure (see online Appendix Table A30). This further supports that treatment effects are not driven by any single type of irregularity.

*Forensic Measure of Electoral Irregularities.*—Our second measure of electoral irregularities comes from the election forensics literature, which uses “anomalies” in the administrative voting data to infer the occurrence of irregularities; see Hicken and Mebane (2017) for a review. In particular, we test for deviations of the second digit of voting counts from Benford’s second digit law, a “natural” distribution of second digits.<sup>30</sup>

Although this type of test remains one of the most popular tools in the forensics literature,<sup>31</sup> some have cautioned against interpreting it as a *sufficient* proof of electoral irregularities (Mebane 2011; Deckert, Myagkov, and Ordeshook 2011). In our setting, these concerns are alleviated by the fact that we will focus on comparing the relative adherence to Benford’s law of municipalities in our treatment arms as proxies of electoral irregularities, instead of considering this test as an absolute fail/pass measure of the occurrence of irregularities. Moreover, we use this measure as *complementary* to our news-based measure in assessing the occurrence of electoral irregularities, as advised by Hicken and Mebane (2017). Benford’s law complements it by detecting less conspicuous electoral irregularities, which might not be covered by the news but still leave a trace in administrative records. As shown in online Appendix Table A8, both types of measures are positively correlated ( $\rho \approx 0.1$ ), but this correlation is not perfect, as expected from this discussion. However, the correlation with the individual types of irregularities reported in the media (online Appendix Table A9) suggests that the forensic measures are strongly correlated to those suggesting fraud, such as registration fraud and electoral fraud.<sup>32</sup>

We use three of the most popular tests to verify compliance with Benford’s second digit law: Pearson’s  $\chi^2$ , Kolmogorov-Smirnov, and Kuiper’s test. We describe the respective test statistics, our construction of the main variables, and discuss their relative strengths and weaknesses in online Appendix D. For ease of interpretation, we synthesize the results of these tests into two main outcome variables. First, we construct a standardized index of all three test statistics, so that larger values reflect larger deviations from Benford’s second digit law. Second, we use an indicator variable which takes the value of one if any of the tests rejects the null hypothesis—i.e., there is statistical evidence of deviations—with  $p$ -value  $< 0.05$ .

Figure 6 presents a visual representation of the estimated effects of the interventions on the latter outcome, while the last two columns of Table 4 report the point estimates for the full set of treatments on both outcomes. Across measures, we find that the municipalities exposed to the reporting campaign experienced fewer deviations from Benford’s distribution, suggesting that fewer electoral irregularities took place in these locations. In general, rejection of the null hypothesis was substantial in the study sample, with 52 percent of the municipalities rejecting it across any of the tests. Being exposed to any of the interventions in the reporting campaign

<sup>30</sup>Jara et al. (2011) use this method on a long panel of elections in Colombia, and show that elections that fail Benford’s law coincide with those held as fraudulent by public opinion and academic studies.

<sup>31</sup>A related first digit law has also been proposed, but the evidence indicates that it is not suited to detect fraud in contexts, such as ours, in which the maximum number of vote counts per voting booth are capped at relatively low numbers (300 in our setting) since these caps distort the natural distribution of the first digit (Pericchi and Torres 2011).

<sup>32</sup>Interestingly, it is also positively correlated with cases of riots reported in the media, which might occur because many of these riots arise from allegations of fraud.



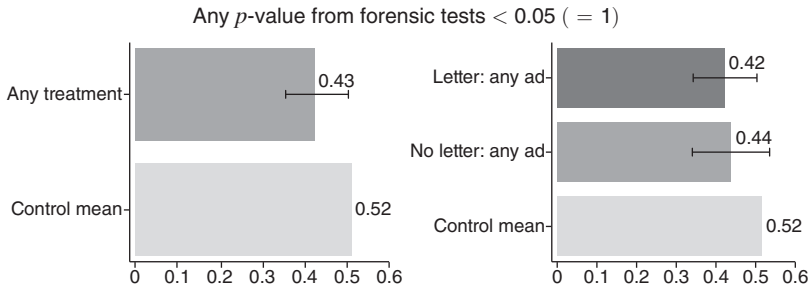


FIGURE 6. IMPACTS ON DEVIATIONS FROM BENFORD'S SECOND DIGIT LAW

*Notes:* This figure reports the effects of the intervention on a forensic measure of electoral irregularities. The outcome is an indicator that takes the value of one if the  $p$ -value of any of the tests described in Section IIIB leads to rejection of the null hypothesis with less than a 5 percent significance level. The control group mean is shown in each plot, while the remaining bars report the control group mean plus the effect for each treatment arm estimated using specification (1). All specifications include the covariates selected using the method described in Chernozhukov, Hansen, and Spindler (2015) and Belloni, Chernozhukov, and Hansen (2014). Ninety-five percent confidence intervals are reported.

reduced this substantially, by 8.7 percentage points, while, in the intensive margin, the test statistics decreased by 0.14 standard deviations ( $p < 0.05$ ).

Consistent with our findings using the media-based measures, we also find that (i) municipalities receiving the information message had a larger drop in the deviation from Benford's distribution than those receiving the call-to-action message (although this difference is not statistically significant in the extensive margin), and (ii) the letter sent to candidates had an "extra" effect in terms of the magnitude of the reduction in the deviation from Benford's second digit law (although the difference is not statistically different).

As a robustness check, in Table A10 in the online Appendix we report the results when using each of the three tests separately. Consistent with the fact that the Pearson  $\chi^2$  test is less powered to reject the null hypothesis in small samples (see the online Appendix for a discussion), only 28 percent of municipalities in the control group reject the null under this test with a significance level of 5 percent, while 42 percent and 34 percent of them do so when using the Kolmogorov-Smirnov and the Kuiper test, respectively. The main results remain qualitatively unchanged by the choice of tests, although the effects are not statistically significant in the case of the Pearson  $\chi^2$  test, while they are mostly significant and larger in the case of the two remaining tests.

Lastly, in Section E of the online Appendix, we report and confirm the robustness of these results to using the tests suggested by Beber and Scacco (2012)—an alternative set of forensic tests commonly used in the literature—as outcome variables.

### C. Effects on Election Outcomes

The evidence presented in the last sections shows that the reporting campaign not only increased citizen reporting but also had a robust and substantial effect deterring electoral irregularities. In so far as some candidates' electoral prospects might have depended on engaging in irregularities (e.g., the votes they might have bought or the

voters they might have intimidated to vote for them), we may expect the reporting campaign to also reduce their vote share. In this section, we begin by studying this possibility and then examine if the intervention affected other electoral outcomes, such as turnout. In the next section, we discuss alternative channels through which the reporting campaign might have affected electoral outcomes and also provide estimates of how much of the changes can be accounted for by the observed reduction in electoral irregularities.

*Identifying the Candidates Likely to Engage in Irregularities.*—While the reporting campaign might have depressed the vote share for candidates whose success depended on electoral irregularities, it might not have affected other candidates. In our setting, the main challenge in studying these differences is identifying which candidates could have been involved in irregularities had our intervention not occurred.<sup>33</sup> To overcome this difficulty, we use citizens' responses in the pretreatment survey we described in Section IIE to identify which candidates were more likely to engage in electoral irregularities in each municipality. In particular, we asked respondents to state whether each candidate would engage in different types of electoral irregularities.<sup>34</sup>

We then aggregated respondents' answers to these questions to construct three different variables at the candidate level. First, we computed the percentage of respondents that state a candidate was going to engage in any electoral irregularity. Denote this first variable as  $Z_{cm}^1$ , for candidate  $c$  in municipality  $m$ . Second, we created a within-municipality version of this variable by subtracting the municipality's mean from  $Z_{cm}^1$ , so that positive values indicate that the candidate is more likely than the average one to engage in electoral irregularities. Thus, if  $\bar{Z}_c$  is the municipality's mean, our second variable is defined as  $Z_{cm}^2 = Z_{cm}^1 - \bar{Z}_m$ . Finally, we also created an indicator of whether  $Z_{cm}^1$  was above average, so that  $Z_{cm}^3 = 1\{Z_{cm}^1 \geq \bar{Z}_m\}$ . While the first variable captures the full candidate-level variation in the likelihood to engage in irregularities that we aim to test, the latter two variables have the advantage of netting out municipal-level factors that could influence responses in ways unrelated to candidates' behavior.<sup>35</sup>

A potential concern in using these survey-based measures is that citizen views about candidates might not be good indicators about the actual behavior of candidates in general, especially when it comes to hard-to-observe behavior such as engagement in electoral misbehavior. In order to validate our measures vis-à-vis this concern, we contrast them with an external measure that does not rely on citizen input for its construction. Specifically, we take advantage of the fact that the *Fundación Paz y Reconciliación* (PARES), a well-known NGO in Colombia, conducted a background check of the candidates in 48 of the municipalities in our sample

<sup>33</sup>This is especially difficult given the large number of candidates running in the mayoral elections in each municipality and the scarce information about most candidates' backgrounds. Upon registering their candidacy, candidates are subject to a legal background check by the government. Thus, there is virtually no variation in candidates' legal history, which could otherwise be used for this purpose.

<sup>34</sup>We did this for the most common irregularities described in Section I: vote buying, illicit advertisement, campaigning by public servants, voter intimidation, fraud in voter registration, and electoral fraud.

<sup>35</sup>For instance, in "pessimistic" municipalities, most respondents might say that candidates will engage in irregularities regardless of whether they indeed will. The second and third measures will alleviate this concern by focusing on the within-municipality variation.

and constructed a dataset indicating if each candidate had a history of past malfeasance, such as involvement in corruption or association with violent organizations.<sup>36</sup>

Despite the fact that the sample of municipalities investigated by PARES is relatively small and is not representative of the municipalities in our sample,<sup>37</sup> this outcome allows us to study how this “objective” measure compares to our survey-based method. As reported in Table A13 in the online Appendix, having a history of malfeasance according to PARES is strongly and significantly correlated with all of our survey-based measures and, in particular, with the within-municipality measures, which have correlations of  $\approx 0.4$ .

*Candidate-Level Regressions.*—We use our candidate-specific measures of the likelihood of engaging in electoral irregularities to estimate the following candidate-level regressions:

$$(2) \quad \text{Vote Share}_{cm} = \mathbf{T}'_m \beta + \psi Z_{cm} + Z_{cm} \times \mathbf{T}'_m \delta + X'_{cm} \gamma + \epsilon_{cm},$$

where  $\text{Vote Share}_{cm}$  is the vote share obtained by candidate  $c$  running in municipality  $m$ ,  $\mathbf{T}_m$  is a vector including different indicators for partitions of the treatment groups as before,  $Z_{cm}$  is one of the candidate-specific measures of the likelihood of engagement in electoral irregularities, and  $X_{cm}$  is a set of municipal and candidate covariates. Given our previous discussion, we expect that  $\delta < 0$ , which would suggest that candidates more engaged in electoral irregularities would experience a greater drop in their vote share.

Following our randomization strategy, we cluster the standard errors at the municipal level. However, these standard errors fail to incorporate the uncertainty generated by the fact that our survey-based measures ( $Z_{cm}$ ) are estimates themselves. To account for this extra uncertainty, we report the  $p$ -values from a two-step clustered bootstrap procedure, which incorporates the variation coming from estimating  $Z_{cm}$  and then equation (2). We give a detailed description of this procedure in Section G of the online Appendix.

Given that the pretreatment survey responses came from a subset of municipalities in the original sample, we recalculate the balance checks for these regressions and report the results in online Appendix Table A12. Results in this table show that the municipalities continue to be well balanced in this subsample across both municipal and candidate covariates. As before, we use the double-post-lasso covariate selection method to choose the municipal and candidate-level covariates to be included in the regressions.

*Results from the Candidate-Level Regressions.*—Table 5 presents the results of estimating equation (2) using the three different survey-based measures of candidates’ likelihood to engage in irregularities. Across measures, we find that candidates more likely to engage in electoral irregularities experienced a large decrease in

<sup>36</sup>The full report and dataset can be accessed here: <https://pares.com.co/2019/10/11/informe-completo-ii-candidatos-cuestionados-2019/>.

<sup>37</sup>In conversations with representatives of PARES they explained that the main selection criteria were to choose municipalities where there were high threats to electoral integrity due to a history of violence, corruption or mining interests.

TABLE 5—IMPACTS ON VOTE SHARE OF CANDIDATES LIKELY TO ENGAGE IN IRREGULARITIES

Interaction term Z:	Vote share (%)			
	Candidate will engage in irregularities (fraction of respondents) (1)	Demeaned candidate will engage in irregularities (fraction of respondents) (2)	Above average candidate will engage in irregularities (= 1) (3)	Past malfeasance (= 1) (4)
<i>Panel A. Pooled treatment</i>				
[T × Z] Any treatment × Z	−3.543 (1.547) [0.016] {0.021}	−3.002 (1.771) [0.088] {0.101}	−2.456 (0.865) [0.007] {0.007}	−5.081 (3.156) [0.124]
<i>Panel B. Subtreatments by types of ads</i>				
[IA × Z] Information ad × Z	−5.403 (1.863) [0.003] {0.004}	−4.908 (2.125) [0.028] {0.021}	−2.362 (1.114) [0.032] {0.034}	−9.161 (3.121) [0.024]
[CA × Z] Call-to-action ad × Z	−1.782 (1.984) [0.360] {0.363}	0.120 (2.398) [0.965] {0.960}	−1.841 (1.113) [0.099] {0.099}	−2.662 (4.923) [0.571]
[I + CA × Z] Information + call-to-action ad × Z	−3.056 (2.075) [0.146] {0.141}	−3.921 (2.542) [0.102] {0.120}	−3.177 (1.167) [0.002] {0.007}	−1.479 (4.403) [0.739]
Test IA × Z = CA × Z, <i>p</i> -value	0.08	0.05	0.67	0.18
Test IA × Z = I + CA × Z, <i>p</i> -value	0.27	0.71	0.52	0.06
Test CA × Z = I + CA × Z, <i>p</i> -value	0.57	0.17	0.30	0.83
<i>Panel C. Subtreatments by letter/no letter</i>				
[NL × Z] No letter: any ad × Z	−4.553 (1.944) [0.021] {0.021}	−3.303 (2.297) [0.119] {0.156}	−2.475 (1.101) [0.019] {0.029}	−3.860 (3.700) [0.328]
[L × Z] Letter: any ad × Z	−2.979 (1.670) [0.069] {0.072}	−2.832 (1.968) [0.165] {0.150}	−2.436 (0.948) [0.012] {0.012}	−5.961 (3.672) [0.135]
Test NL × Z = L × Z, <i>p</i> -value	0.39	0.84	0.97	0.59
Control mean	20.01	20.01	20.01	20.01
Sample size	2,989	2,989	2,989	263
Number of municipalities	630	630	630	48

*Notes:* The outcome in all columns is the vote share of each candidate, expressed as a percentage of total valid votes. In each of these columns, a different measure of the likelihood that a candidate commits irregularities is used to compute the candidate-level heterogeneous effects. In column 1 it is the proportion of respondents from the pretreatment survey that say the candidate might commit at least one type of electoral irregularity. In column 2 the outcome is this same variable, demeaned using the municipality-level mean. In column 3 it is an indicator that takes the value of one if this variable is above the municipal-level mean. Finally, in column 4 it is an indicator for whether a candidate was found to be involved in malfeasance in the past according to the investigation by the NGO PARES. All specifications include the covariates selected using the method described in Chernozhukov, Hansen, and Spindler (2015) and Belloni, Chernozhukov, and Hansen (2014). Clustered standard errors at the municipal-level are shown in parentheses, randomization inference *p*-values are shown in brackets, and clustered wild-bootstrap *p*-values correcting for the variance in estimating Z are shown in braces.

their vote share when they were running in municipalities included in the reporting campaign interventions. The reporting campaign reduced the vote share of candidates for which 100 percent of respondents say they will be engaged in electoral irregularities by 3.5 percentage points ( $p < 0.05$ ), by 3 for the demeaned version of this variable ( $p \approx 0.1$ ), and by 2.5 for candidates above the municipal average according to this variable ( $p < 0.01$ ). Notice that, although large, these estimates are in the “ballpark” of other interventions to curb electoral irregularities.<sup>38</sup>

Mimicking the results for electoral irregularities, we see that the effect on these candidates’ vote share is largest for municipalities receiving the information message, yet, the difference is not always statistically significant. Likewise, the effect difference between municipalities where the candidates received a letter or not depends on the outcome used.

In the last column of Table 5 we alternatively estimate equation (2) using an indicator variable that takes the value of one if the candidate was found to have a history of past malfeasance according to the investigation conducted by the NGO PARES. Despite this regression being limited to the few municipalities they investigated, we find estimates consistent with our results using the survey-based measures.

As a robustness check, we verify whether, rather than measuring the propensity to engage in electoral irregularities, our measures capture candidates’ popularity.<sup>39</sup> To test this hypothesis, we estimated equation (2) interacting different measures of the popularity of candidates with the treatment indicators. In particular, we use two different proxies of candidates’ popularity: (i) the proportion of respondents in the pretreatment survey who say that the candidate will win the election, and (ii) an indicator for whether the candidate is running with an incumbent party or not. The results, presented in online Appendix Table A15, show that the heterogeneity with respect to popularity is not significant, which suggests that this is not driving previous results.

*Additional Electoral Outcomes.*—We now examine if the reporting campaign also impacted other electoral outcomes, such as turnout or the margin of victory. As seen in online Appendix Table A16, the intervention did not affect either of these outcomes. In particular, the fact that turnout was not significantly affected suggests that the decrease in the vote share for candidates more likely to engage in irregularities must have come with a parallel increase in the vote share for “cleaner” candidates.

#### D. *Interpreting the Effects of the Intervention*

The evidence presented in the previous sections shows that the reporting campaign reduced electoral irregularities as well as the vote share of candidates likely

<sup>38</sup>For instance, in the context of Russia, Enikolopov et al. (2013) find that polling stations where electoral observers were assigned experienced a decrease of 11 percentage points in the vote share for the incumbent party. Similarly, Blattman et al. (2019) find that an anti-vote-buying campaign in Uganda decreased the vote share of incumbents by approximately 3 percentage points. Finally, Callen and Long (2015) find that announcing the implementation of a photo quick count to detect aggregation fraud in Afghanistan reduced the vote share of the most politically connected candidates by 5.5 percentage points.

<sup>39</sup>As reported in online Appendix Table A14, the tested measures are indeed correlated with the popularity of candidates.

to engage in them. In this section, we discuss the possible mechanisms underlying these results. We begin by analyzing the decrease in irregularities. This could have occurred through several mechanisms, including (i) a reduction in candidates' willingness to engage in irregularities and (ii) a reduction in voters' willingness to sell their votes (e.g., out of the fear of being reported or a change in social norms linked to irregularities). Importantly, both mechanisms operate in the same direction. While we cannot fully disentangle these mechanisms, the letter sent to candidates helps us shed light on the first one since it elicits candidates' reactions to the reporting campaign. The fact that municipalities receiving these letters experienced a larger reduction than those without them (i.e., they experienced reductions 50 percent larger or more) suggests that candidate reactions are an important factor driving the effects. Yet, the lack of statistical significance in some differences undermines our ability to draw strong conclusions from this evidence.

We now turn to the effects on election outcomes. These effects might have occurred because of the decline in electoral irregularities that would, in tandem, reduce the vote share of candidates relying on them to boost their electoral prospects. Alternatively, it is plausible that the reporting campaign could have raised the salience of electoral irregularities to voters by exposing them to this issue through Facebook advertisements. In turn, this might have changed their social norms about electoral malpractice, which might affect their decision to vote against the candidates they perceived to be at the source of irregularities, an interpretation that would be consistent with our findings. Moreover, these channels have distinct practical implications. According to the "salience channel," voters' (potentially inaccurate) perceptions about different candidates determine their votes whereas, under the "electoral irregularity channel," actual irregularities are what drive the reduction in the electoral support for candidates likely to engage in irregularities.

Observationally, both the drop in electoral irregularities and the increased issue salience might have determined the overall effect reported in the previous section.<sup>40</sup> One feature of our experimental design that allows us to take a first step in figuring the relative contribution of both channels in determining the overall effect is the presence of the "call-to-action" message treatment condition. Municipalities in this group were exposed to the increased salience about irregularities, but, as we have seen in the previous sections, they experienced no significant changes in reporting, or in the occurrence of electoral irregularities. Thus, they provide a group in which most (if not all) of the changes in electoral outcomes would have come from the "salience channel" and not the "electoral irregularity channel." As seen in Table 5, the effect of the call-to-action message on the vote share of candidates more likely to engage in electoral irregularities is not statistically significant and its magnitude corresponds to 0–72 percent of the reduction estimated for the group receiving only the information message (depending on the specification).<sup>41</sup> Assuming the effect of the call-to-action treatment captures the full extent of the salience channel, this

<sup>40</sup> Another channel that might have affected candidates' vote share is substituting the resources devoted to electoral irregularities for legal campaigning efforts. However, we do not emphasize this channel since it is likely that it might have dampened the drop in the vote share of candidates more likely to engage in irregularities rather than explain the effect we find.

<sup>41</sup> Notice that the difference between both treatments is only significant at conventional levels in the specification in columns 1 and 2, although it is close to being significant in column 4.

would imply that between 28 percent and 100 percent of the total decrease in the vote share of candidates likely to engage in irregularities is due to the electoral irregularity channel.

As an alternative way of quantifying the contribution of this channel, we formalize the previous discussion with a simple model. Suppose the vote share obtained by candidate  $c$ , conditional on her likelihood of engaging in irregularities ( $Z_c$ ), is given by a function  $V_c = V(I(T), O(T); Z_c)$ , where  $I(T)$  are the electoral irregularities that candidate  $c$  engages in, given the treatment status  $T$ ;  $O(T)$  are other factors influencing the vote share of the candidate  $c$ —including the salience of irregularities—which depend on  $T$ . Given this expression, the treatment effect on the vote share of  $c$  can be decomposed as

$$(3) \quad \frac{dV_c}{dT} = \frac{\partial V_c}{\partial I} \frac{dI}{dT} + \frac{\partial V_c}{\partial O} \frac{dO}{dT}.$$

We are interested in the first term of equation (3): the effect of irregularities on candidate  $c$ 's vote share, which depends on treatment assignment  $T$ . We have direct estimates of  $\frac{dI}{dT}$  from the results presented in the previous sections. On the other hand, we do not have causal estimates for  $\frac{\partial V_c}{\partial I}$ , but we can approximate this by using correlations in the control group.<sup>42</sup> Using these estimates, we can then approximate the percentage of the total effect on vote share due to the change in electoral irregularities by computing  $\left(\frac{\partial V_c}{\partial I} \frac{dI}{dT}\right) \times 100 / \left(\frac{dV_c}{dT}\right)$ .

In online Appendix Table A17, we report the results of performing this exercise using different combinations of the proxies used for electoral irregularities ( $I_m$ ) and  $Z_c$  in the previous analysis. The results range between 12 percent and 54 percent, with a mean of 31 percent, which is similar to estimates comparing the effects of the call-to-action and information message treatment arms. While this exercise is far from perfect, it provides complementary evidence of the relative importance of the “electoral irregularity channel” in determining the drop in the vote share of candidates likely to engage in electoral irregularities.

### E. Additional Robustness

In this section, we show that our results are robust to different methodological decisions in estimating the treatment effects of the interventions. First, we study the robustness with respect to excluding controls in the estimation, except for fixed effects for the strata used in randomization. We report the redoing of the main results using these specifications in online Appendix Tables A18–A20. As expected, across outcomes, the magnitude of the treatment effects is virtually unaffected, but these are less precisely estimated. Despite this, we see that most of the treatment effects discussed in the previous sections remain significant, and the main conclusions remain unaltered.

<sup>42</sup>In practice, we get an estimate for  $\frac{dV_c}{dI}$  by estimating coefficient  $\alpha_3$  in the following regression:

$$\text{Vote Share}_{cm} = \alpha_1 I_m + \alpha_2 Z_{cm} + \alpha_3 Z_{cm} \times I_m + u_{cm}$$

using different proxies for irregularities ( $I_m$ ) and candidates' propensity to engage in these ( $Z_{cm}$ ).

Second, we study the sensitivity of the results to using a “long model”—i.e., including all possible interactions of the treatment arms—as recommended by Muralidharan, Romero, and Wüthrich (2019). We report these alternative specifications in online Appendix Tables A21–A23. Despite the sizable loss in power when using the long specifications, we see that virtually all of the estimated treatment effects preserve the same sign as the short models, and that many continue to be significant. Additionally, we see that in most of the estimations, we cannot reject the null that effects are equal within the aggregated treatment arms used in the short models.<sup>43</sup>

#### IV. Cost-Benefit Analysis

To better appreciate the effectiveness of the reporting campaign, we compare its cost-effectiveness to other similar strategies to reduce electoral irregularities studied in the literature. In the following, we limit ourselves to comparisons to the few papers that report costs and we compare their cost-effectiveness, as measured by their impact on votes for candidates benefiting from irregularities, which is a measure provided by most papers. We focus on interventions studying (i) electoral observers, (ii) top-down ICT technologies, and (iii) voter-education campaigns against irregularities, which are the most common interventions studied in the literature. In online Appendix H, we provide the details of the following computations and provide a summary of the estimates in online Appendix Table A27.

We use two main benchmark metrics for the cost-benefit analysis of our intervention. First, a back-of-the-envelope calculation suggests that our intervention reduced by one percentage point the vote share of candidates above average in the proportion of people who say they will engage in electoral irregularities at the cost of US\$0.70 per polling station. Second, a similar calculation suggests that a single dollar spent in the reporting campaign reduced by 21 the votes for these same candidates.

*1. Comparison to Electoral Observers.*—Despite being the strategy most used and studied to curb electoral irregularities, virtually no paper examining the effects of electoral observers reports their cost. In a discussion of their cost-effectiveness, Callen et al. (2016) estimate that the European Union spends about US\$6,000–20,000 per polling station deploying electoral observers in developing countries. These large costs undermine the cost-effectiveness of this strategy, even if we consider the most “optimistic” evaluations of their impact. For instance, Enikolopov et al. (2013) find that the random deployment of electoral observers around elections in Russia generated a drop of 11 percentage points per polling station of the vote share of the allegedly corrupt incumbent party, which is the largest effect reported for these types of interventions that we know of. Taking the range of costs per polling station mentioned before, this would imply that a one percentage point decrease in the vote

<sup>43</sup>In online Appendix Tables A24–A26, we further present the estimates of equations (1) and (2) using separate indicators for the two versions of the letters sent to candidates described in footnote 17. The results indicate that, in most cases, there is no significant difference between these treatment arms, except when analyzing the candidate-level regressions, when there seems to be an extra effect of the partial knowledge letter.



share costs between US\$545 and US\$1,818 per polling station, which is several orders of magnitude higher than the costs of our intervention.<sup>44</sup>

2. *Comparison to Top-Down ICT Monitoring.*—Two papers report the cost-effectiveness of ICT monitoring interventions. Both of them study the impact of announcing the adoption of a technology that takes pictures of electoral tallies before aggregating vote counts, as a way of reducing aggregation fraud. In the first paper, Callen and Long (2015) report that deploying this intervention in Afghanistan costs approximately US\$210 per polling station and reduces by six the votes for candidates “connected” to electoral authorities (which are the ones most able to benefit from fraud). This would imply that a single dollar reduced by 0.03 the votes for these candidates. In the second paper, Callen et al. (2016) report a cost of US\$40 per polling station in Uganda. Their intervention decreased the vote share of the incumbent candidate by 3 percentage points, which implies that reducing a single percentage point costs approximately US\$13. Although these strategies are substantially more cost-effective than deploying electoral observers, they are considerably less so than the reporting campaign we study, mainly because they require sending staff to take pictures of voting tallies. Crowdsourcing this task to decrease these costs, in a spirit similar to our intervention, might be a promising avenue for future research.

3. *Comparison to Voter-Education Interventions.*—The lion’s share of the voter education campaigns studied in the literature involves the training and mobilization of staff to convey messages and develop activities related to reducing electoral irregularities (Collier and Vicente 2014; Vicente 2014; Hicken et al. 2018; Blattman et al. 2019; Schechter and Vasudevan 2021). Although we can legitimately suspect such efforts probably involve large costs relative to their reported effects on irregularities, virtually none of these papers report the costs of their studied interventions. One exception is Schechter and Vasudevan (2021), who, instead of relying on in-person campaigns, explore the effect of a radio campaign informing citizens of the economic consequences of vote-buying in India. They find that the intervention was extremely cost-effective in reducing the votes of candidates running with vote-buying parties, with a one-dollar investment translating into 109 fewer votes for these candidates, approximately five times as cost-effective as the reporting campaign we deployed. Importantly, however, the experimental design of this intervention ruled out possible reactions of candidates, which might have muted its effect.<sup>45</sup> This implies that the practical implementation of this intervention, which would entail the full knowledge and reaction of candidates, could presumably lead to smaller cost-effectiveness estimates. However, since this intervention produces estimates in the same order of magnitudes as ours, it highlights the great promise of using media to enhance electoral integrity.

<sup>44</sup>These estimates might be overstated given that the costs reported by Callen et al. (2016) refer to less developed countries than Russia. However, even if real costs were 10 percent of the considered ones, this intervention would still be more than two orders of magnitude costlier than the one we consider.

<sup>45</sup>Schechter and Vasudevan (2021) argue that since the radio campaign occurred three days before the elections when electioneering is prohibited and most vote buying occurs in this context, candidates would not have the chance to react to it.

As these comparisons show, the intervention considered in this paper leads to substantial cost reductions compared to other alternatives studied in the literature. However, we note some of the prerequisites necessary to make this intervention work: (i) electoral watchdogs willing and capable to punish authors of electoral irregularities, (ii) a well-functioning (user-friendly) reporting platform that guarantees reporter anonymity, (iii) simple and attractive social media advertisements to popularize the use of the reporting platform, and (iv) widespread internet access. Before adopting this strategy in different contexts, it is thus necessary to check whether these conditions are in place in the particular setting.

## V. Conclusion

The recent trend of “democratic backsliding” around the world—i.e., the deterioration in the quality of political institutions among democracies—underscores the need for innovative improvements in several dimensions of democracy assistance (Haggard and Kaufman 2021; Bermeo 2016). Despite substantial efforts by both governments and international agencies to fight electoral irregularities, these remain an important issue that hampers accountability and development in a large part of the world. In this paper, we provide evidence that ICTs incorporating civil society in the oversight of elections are an effective way to promote electoral integrity in the context of widespread irregularities.

The policy implications of these findings are threefold. First, we find that using a social media campaign to induce citizens to report is an effective way to spur high-quality, evidence-backed reports that can be used by competent authorities to prosecute the authors of electoral irregularities. Second, we find that this intervention deters electoral irregularities in a more cost-effective way than other strategies traditionally used by governments, NGOs, and international organizations—e.g., election observers and different education campaigns. The reason for this is that hiring, recruiting, and mobilizing specialized personnel to oversee elections involves substantial costs compared to the simple idea of recruiting citizens to watch over their own elections through inexpensive ICT channels, such as Facebook advertisements. These cost-saving strategies should inform current endeavors to increase electoral integrity, and be adapted to local conditions to exploit the relative advantages of this approach. Finally, by relying on social media to disseminate our reporting campaign, we believe this type of strategy can easily be scaled up in contexts where there is enough internet and social media penetration. Moreover, given the rapid expansion of the internet throughout the developing world, these types of strategies will be increasingly more suitable for the most remote regions, where alternative options may be forbiddingly costly, and government accountability is worse (World Bank 2017).

The findings in this paper also open several important avenues for future research. To begin with, how do these types of interventions affect citizens’ views and trust about democracy and the government more generally?<sup>46</sup> Although we do not find effects on turnout, access to voice channels and increased accountability might

<sup>46</sup>Recent work studying this are Ofosu (2019); Berman et al. (2019); Acemoglu et al. (2020).

improve citizen attitudes toward democratic institutions. Yet, this probably requires sustained exposure for the mechanisms to emerge. Second, what are the downstream effects of this type of intervention on the accountability of governments and the provision of public goods and services? By decreasing the popular support of candidates more engaged in electoral irregularities, these interventions might generate positive effects on the selection and agency of candidates. Third, we have shown the potential of a social network like Facebook to amplify the scope and effect of campaigns that encourage citizen monitoring of elections. However, our experimental design does not allow us to fully test for their efficacy compared to other dissemination vehicles, including traditional media or other technologies which represents an important gap in our understanding about how to optimally design communication campaigns in developing settings.<sup>47</sup>

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<sup>47</sup> See Erlich et al. (2018) for a discussion of the practical advantages and disadvantages of using different communication channels in a similar setup.

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