# Electoral Integrity and Local Development: Experimental Evidence From Colombia

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**Abstract**

Does electoral integrity improve local development? Prevailing theories of democracy suggest that well-functioning elections allow citizens to select good politicians (*selection channel*) and incentivize their effort (*accountability channel*). Despite the intuitive logic of these arguments, there is limited empirical evidence supporting them. We address this gap by leveraging unique experimental variation in electoral integrity from a randomized intervention conducted during Colombia's 2019 mayoral elections, which reduced electoral irregularities. We document four findings. First, treated municipalities experienced increased access to local public services over the subsequent four years. Second, mayors in treated municipalities became more efficient and less wasteful, as measured by procurement outcomes and corruption-related judicial processes. Third, we develop a novel framework to quantify the contribution of the selection channel and find it plays a substantial role in the overall effect. Finally, we find that alternative explanations—such as increased citizen demand—are unlikely to account for the results. These findings underscore the developmental value of transparent elections.

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# 1 Introduction

While the vast majority of countries today hold elections to choose their leaders, only about half of these elections are truly free and fair (see estimates by Bishop and Hoeffler 2016).[1] In the remaining cases, elections are often undermined by irregularities, including fraud, vote buying, and violence (e.g., Callen and Long 2015; Condra et al. 2018). Moreover, the average quality of elections has declined in recent decades (Diamond 2021).

Against this backdrop, understanding whether and how electoral integrity matters for citizens' well-being has become increasingly urgent. Prevailing theories of democracy argue that well-functioning elections create mechanisms for civil society to select good politicians (*selection channel*) and to incentivize their effort (*accountability channel*) (Fearon 1999; Ferejohn 1986; Besley 2006). When elections are marred by irregularities, these mechanisms weaken: politicians are elected not for their competence but for their ability to manipulate the process, which in turn undermines the disciplinary role of elections. As a result, these theories predict that weaker electoral integrity should lead to underperforming politicians, and, ultimately lower welfare for citizens. Yet despite the intuitive logic of these arguments, systematic evidence remains scarce.

In this paper, we study this question by leveraging a unique opportunity for experimental variation in electoral integrity. Specifically, we analyze the downstream effects of a randomized intervention implemented during Colombia's 2019 local mayoral elections, designed to test whether a campaign promoting citizen oversight could reduce electoral irregularities, such as fraud, vote buying, and voter intimidation, which are common in this setting (Fergusson et al. 2018; García-Sánchez and Pantoja 2015). The intervention was randomized at the municipal-level across nearly 700 municipalities, representing more than 60% of the country's 1,123 municipalities. It consisted of a large scale social media campaign encouraging citizens to report electoral irregularities through an online channel hosted by an NGO which then passed these reports to judicial authorities for investigation. In Garbiras-Díaz and Montenegro (2022), we showed that this intervention successfully reduced electoral irregularities, as captured by multiple measures, including mentions in the media, citizen surveys and forensic tests. We also found that candidates who relied on electoral irregularities experienced a significant decrease in their vote share in treated municipalities.

We leverage the variation in electoral integrity generated by this experiment as a "first stage" to study the effect of cleaner elections on local development. We focus on dimensions of development that directly depend on the performance of local politicians in delivering public

[1] According to Varieties of Democracy (V-Dem Dataset), a project that measures democracy and its components, 82% of the countries in their database held elections in 2023 to select both the chief executive and the legislature. See Appendix Figure A1.

goods and services during their four-year mandate, which concluded in December of 2023. This setting is particularly relevant because, since the 1991 Constitution, a decentralization reform transferred responsibility for providing a limited but essential set of basic public goods and services to local governments in Colombia. These include access to sanitation—through clean drinking water, sewerage, and waste disposal—along with public lighting and road infrastructure, services that remain far from universal across the country.[2] For instance, in the municipalities in our sample, access rates to sewerage, water, and waste collection averaged only 45%, 62% and 52%, respectively. The clear assignment of responsibility for these services to local politicians in this context enables us to keep track of their performance and its impact on local development in a transparent way.

Although basic in nature, the public services under the purview of Colombian mayors are essential for fostering local development. A well-established literature has demonstrated that investment in basic sanitation infrastructure plays an important role in ensuring local populations' health, and in particular in reducing child mortality.[3] Similarly, another body of studies has shown that road infrastructure, particularly rural local roads, can improve developmental outcomes and reduce poverty.[4] Finally, public lighting has been robustly associated with reductions in crime rates (see Welsh et al. (2024) for a review).

Measuring the provision of public services presents significant challenges due to the scarcity and often limited reliability of available data. To address these issues, we combine administrative data with satellite imagery as follows. First, we use administrative data to compute the percentage of properties in each municipality that have access to sewerage, aqueduct, and waste collection services, as reported by the respective local service providers.[5] Second, to measure public lighting, we rely on nighttime light intensity as an independent and objective proxy for this public service, which is otherwise unavailable.[6]

[2]The 1991 Constitution also devolved the provision of more complex public goods, such as education and health, to a restricted group of "certified" municipalities. However, these types of municipalities are typically larger than 100,000 inhabitants, which are excluded from our experimental sample, and thus are irrelevant for our analysis.

[3]See Augsburg et al. (2025) for a recent review of this literature.

[4]For instance, Khandker et al. (2009) find that a rural road construction program in Bangladesh reduced poverty by allowing farmer to increase output prices, and decreasing transportation costs, especially for the poor. A set of papers studying the impact of a large rural road building program in India (*Gram Sadak Yojana*) report that it increases mobility into better paid urban jobs (Asher and Novosad 2020), raises enrollment of children into school (Aggarwal 2018), and improves visits to health facilities, including vaccination and antenatal care (Aggarwal 2021).

[5]To mitigate concerns regarding the self-reported nature of this data, we validate our findings using household survey data collected by the National Department of Statistics (*DANE*), which covers a broad range of municipalities and respondents.

[6]For road infrastructure, we plan to use high-resolution satellite imagery in conjunction with out-of-sample vector data on roads of varying quality to predict the presence and development of local road networks.

We begin by reporting the average treatment effects of the original intervention on local public services in the four subsequent years. Using a standardized index of access to sanitation and public lighting, we find that treated municipalities experienced an increase of approximately 0.2 standard deviations ($p < 0.01$). This improvement was experienced across the entire distribution of municipalities. Moreover, when examining the effects across types of public services, we find positive estimates on each component, with larger and more precise effects in urban areas compared to rural ones. The magnitudes of these effects correspond to roughly a 2-4 percentage point increase in the share of properties with access to sanitation services.

We also find that these effects are typically larger in municipalities assigned to a sub-treatment arm in which the main intervention was combined with a letter sent to the universe of mayoral candidates in the municipality informing them about the citizen reporting campaign. In Garbiras-Díaz and Montenegro (2022), we showed that this sub-treatment arm also experienced a larger decrease in electoral irregularities. Together, these two pieces of evidence suggest that the increase in service provision is driven by improvements in electoral integrity.

Next, we explore the intermediate actions by politicians that shape the local production of public services and may explain the observed improvements in their provision. Our analysis begins with an examination of local governments' public procurement practices—a critical stage for the provision of public services but also one where corruption and inefficiencies frequently arise during the allocation of government resources in Colombia (Gulzar et al. 2022), as well as in other developing contexts (Bosio et al. 2022). We find that local governments in treated municipalities use more competitive types of procurement, such as public tenders and auctions, and less non-competitive types, such as direct contracting, which have been associated with more politically connected firms and worse efficiency (Fazekas et al. 2025; Baltrunaite et al. 2020). We also find that local governments in treated municipalities face less corruption-related judicial processes, which is another way through which cleaner mayors might improve the use of public funds. These findings suggest that improved electoral integrity may lead to better governance practices by encouraging politicians to adopt more transparent and efficient mechanisms for resource allocation. In turn, these institutional changes may help explain the improvements observed in the provision of basic public services.

We next examine the theoretical channels through which higher electoral integrity may improve public service delivery: namely, the *selection* of better politicians and the improved *accountability* once in office. We develop an original methodology to decompose and quantify each channels' contribution to the overall effect. In this decomposition, we empirically

identify the contribution of the selection channel as the product of two components: (i) the change in the probability that a "clean" candidate (i.e., one not engaged in electoral irregularities) is elected, and (ii) the change in the provision of local public services at the municipal level given that a clean candidate is elected.

We begin by estimating the change in the probability that a "clean" candidate is elected in a municipality as a result of improvements in electoral integrity. To do so, we leverage an original pre-treatment survey, in which we asked citizens to identify which candidates running in the elections were "clean." Using this proxy, we find that the intervention increased the probability that a clean candidate was elected by 7 percentage points ($p < 0.05$). This represents a 26% increase over the control group, in which clean candidates were elected 31% of the time. We then estimate the effect of electing a clean candidate on the provision of public services using a close-election regression discontinuity design, which isolates cases in which candidate type is as good as randomly assigned while holding the accountability channel constant. Specifically, we compare access to services in municipalities governed by clean candidates to the counterfactual of those led by non-clean candidates. This exercise reveals that electing a clean candidate increases the index of public service provision by 0.7 standard deviations ($p$-value$< 0.05$). Finally, combining these estimates, we find that approximately 26% of the average treatment effect is attributable to the selection channel.[7]

While this analysis provides a transparent approach to testing the causal contribution of the selection channel, the "cleanliness" of politicians is not randomly assigned and may be associated with various underlying characteristics.[8] To assess this possibility in our context, we compiled a comprehensive dataset of politician characteristics from multiple sources, encompassing demographics, education, public and private sector experience, political experience, political alignment, and party affiliation. We find that electoral cleanliness is not strongly correlated with any of these observable characteristics. The only statistically significant relationships are weak negative correlations with age and political experience (both $\rho < 0.1$). Moreover, our results indicate that the intervention did not significantly affect the likelihood that mayors with any of these attributes were elected. Taken together, this evidence suggests that, at least in this context, the selection effect is not driven by characteristics of politicians that are readily observable in their public profiles.

[7] We argue that this estimate is likely to be a lower bound given that our measure of candidate type is noisy and thus subject to classical measurement error, which would attenuate the coefficient toward zero.

[8] The relationship between a politician's electoral cleanliness and other attributes that could influence their capacity to deliver public goods and services is theoretically ambiguous. On the one hand, cleaner candidates may be more educated or supported by more competent teams, as they rely less on electoral irregularities to secure office. On the other hand, research in other contexts has shown that politicians who engage in electoral irregularities may be better connected to the private sector and may even use such irregularities as a signal of these connections to voters (Kramon 2016).

Finally, we conduct a series of additional analyses to rule out alternative mechanisms that might explain our findings. We begin by examining data on local public revenues to assess whether treated municipalities differed in their revenue levels, which, as a key input, could potentially account for variations in public service provision. We find no significant differences in total revenues, but also in specific revenue sources, such as national transfers or locally collected taxes. This evidence suggests that the intervention likely improved the efficiency in the use of public funds rather than increasing the overall fiscal resources available to local governments.

We then test whether the effects are driven by additional support from higher levels of government. To do so, we examine public services that fall outside the mayor's mandate, namely education and healthcare. We find no effects on inputs related to these services, which are primarily provided by higher-order authorities. This result suggests that the observed improvements are unlikely to be driven by differential support of other layers of government.

Next, we investigate whether citizen demand for public goods may have shifted as a result of the intervention—a plausible mechanism, given that the social media campaign may have spurred collective action and strengthened beliefs in the effectiveness of bottom-up civic engagement. This demand-side channel contrasts with the evidence presented thus far, which emphasizes supply-side efforts initiated by politicians to improve public goods and service provision. To test this alternative explanation, we collected data on a set of variables representing the most plausible citizen actions that could influence public service outcomes in this context: (i) protests during the study period, particularly those directed at local governments with demands for improved policies and services; (ii) participation in grassroots organizations—*Juntas de Acción Comunal*, citizen organizations that, among other things, mobilize around community development; (iii) the filing of *tutelas*—a constitutional writ frequently used in Colombia to claim access to basic rights, including public goods and services (see Londoño-Vélez and Saravia (2025) for detailed context); (iv) social media users and, in particular, those interested in political and social issues, which could signal improved collective action. We find no evidence that the intervention affected any of these variables, and, indeed, find some suggestive evidence that the intervention might have *lowered* demand for public goods, which would be consistent with increased citizen satisfaction stemming from improved service provision.

**Related literature:** This paper speaks to several strands of literature. It is closely related to the papers studying the impact of democratization, as well as the quality of democracy on economic development. A well-established set of papers has studied the effects of democ-

ratization events on macroeconomic outcomes—typically per capita GDP growth—but also public good provision and taxation, using cross-country regressions and panel data methods.[9]

A closely related body of work has focused on within-country variation of democratic and electoral quality. This includes papers examining the relationship between political competition and public good provision (see Gottlieb and Kosec (2019) for a review), the effect of improved voting technology on public good provision (Fujiwara 2015), the introduction of local elections to better target local populations' preferences and public goods (Martinez-Bravo et al. 2022; Paniagua 2022) or the effect of political turnover on economic performance (Marx et al. 2024; Akhtari et al. 2022; Bazzi et al. 2025).

A growing body of work comes closest to ours by studying the relationship between different forms of electoral irregularities and economic outcomes. Most of these studies provide correlational evidence: for instance, Khemani (2015) documents a negative association between vote-buying practices and public health outcomes in the Philippines, while Anderson et al. (2015) show that vote-buying and clientelistic practices are associated with less pro-poor policies. Moving beyond descriptive correlations, Guerra and Justesen (2022) show that vote-buying reduces demand for public goods using a lab experiment in Kenya. In a similar vein to our paper, Ofosu (2019) studies an experiment in Ghana that randomized electoral observers in parliamentary elections and finds that legislators elected in more heavily monitored constituencies increased the amount of development funds spent, which the author interprets as increased politician responsiveness. Closely related to our paper, Karim (2025) studies a voter re-registration reform in Brazil aimed at curbing vote buying. Using a regression discontinuity design, he finds that the reform increased political competition and reduced incumbency advantage, consistent with Hidalgo and Nichter (2016). In the years following the reform, treated municipalities experienced higher expenditures on health and education, as well as well as improvements in related outcomes such as infant mortality.

Our paper contributes to these strands of literature by providing novel experimental estimates of the effect of increasing electoral transparency on downstream public service provision, which is inherently connected to citizen well-being. These results highlight that the benefits of democracy are not limited to initial democratization events; rather, the quality and fairness of elections, once established, are crucial determinants of this form of government's success in fostering development.

[9]Starting with the seminal work by Przeworski 2000, these relationships have been studied extensively. For a review, see Doucouliagos and Ulubaşoğlu (2008) for early contributions to this literature, and Colagrossi et al. (2020) for more recent work. While earlier papers tended to find mixed results, more recent ones find stronger and more consistent evidence linking democracy to growth as well as to human capital investments. In one of the most recent and meticulous studies in this literature, Acemoglu et al. (2019) show that one of the mechanisms through which democracy fosters growth is by increasing public goods provision—a result that aligns with our findings.

A second strand of related literature examines the selection and accountability channels, which, as mentioned before, are the main proposed mechanisms through which democracy promotes citizen well-being (Fearon 1999; Ferejohn 1986; Besley 2006). The selection channel has typically been tested by studying whether information about candidates' past performance or qualifications affects voters' choices (e.g., Ferraz and Finan 2008; Dunning et al. 2019; Casey et al. 2021), and by examining whether candidate characteristics—such as gender or pro-sociality—influence their policies (e.g., Chattopadhyay and Duflo 2004; Beath et al. 2016; Gulzar and Khan 2024). Similarly, several papers test accountability channel by exploiting variation in term limits to assess how incentives affect politician performance (e.g., Ferraz and Finan 2011; Coviello and Gagliarducci 2017). Our paper contributes to this literature by introducing a simple methodology to quantitatively decompose the selection and accountability channels, which can be readily used in other settings.

Finally, this paper contributes to the broader literature on the determinants of local public goods provision and infrastructure, as well as their downstream effects on citizen welfare. A growing body of research has emphasized the role of public sector employees and civil servants, focusing, in particular, on the impact of incentives and technological innovations on the efficiency of service delivery (see Finan et al. (2017) for a review). Another strand documents how investments in basic infrastructure—such as sanitation (Alsan and Goldin 2019; McCulloch et al. 2025; Bancalari 2024), electrification (Allcott et al. 2016; Lipscomb et al. 2013), and local or interregional road construction (Asher and Novosad 2020; Ghani et al. 2016; Jedwab and Storeygard 2021)—can significantly improve downstream outcomes for citizens, including income, health, and economic activity. Overall, our findings underscore the importance of electoral quality as a critical driver of improved public goods and service provision and more efficient government spending.

The remainder of the paper is organized as follows. Section 2 describes the context, including the 2019 intervention and the institutions regulating local politicians' responsibilities in Colombia. Section 3 outlines the data sources, the construction of variables, and the empirical strategy. Section 4 presents the main results on public service provision and explores both the intermediate mechanisms driving these results and alternative explanations. Section 5 presents our methodology to decompose the main effect into selection and accountability channels, along with the results of this exercise. Section 6 concludes.

# 2 The Colombian Context

Colombia offers an ideal setting to study the relationship between electoral integrity and local development. Although the country has maintained formal democratic institutions for most of its history—interrupted only by a brief period of military rule between 1953 and 1957—elections continue to be marred by widespread irregularities (MOE 2023). Clientelism, in particular, remains pervasive: recent surveys indicate that about 18% of respondents have "sold" their vote at least once, and around 7% report coercion or intimidation to vote in a particular way (Fergusson et al. 2018; García-Sánchez and Pantoja 2015). These rates are substantially higher among rural and lower-income populations.

Against this backdrop, online reporting of electoral irregularities has become an increasingly important oversight mechanism. The *Misión de Observación Electoral* (MOE), a prominent domestic NGO specializing in election monitoring, operates the country's most widely used online platform, which allows citizens to *anonymously* report instances of vote buying, coercion, and other forms of electoral misdeeds. These reports are forwarded to the relevant national oversight bodies—most notably the Attorney General's Office and the Inspector General's Office—for formal investigation. This collaboration between the MOE and national watchdogs makes online reporting a consequential instrument of accountability: between 2010 and 2018, approximately 15% of reports resulted in formal investigations and 2.5% in judicial decisions such as sanctions or acquittals (see Garbiras-Díaz and Montenegro 2022).[10]

A second relevant feature of the Colombian context is the limited and uneven access to basic public goods and services. In 2019, just before our intervention, median property access rates for aqueduct, sewerage and waste collection were 61%, 39% and 45%, respectively. Moreover, access varies widely across municipalities—for example, aqueduct coverage ranges from 98% (90th percentile) to 23% (10th percentile)—and within municipalities, where rural access often represents only 20%-50% of urban access. While gaps are partially mitigated by non-conventional or individualized solutions (e.g., latrines, rainwater collection), disparities remain substantial.

[10] Even when investigations do not lead to formal penalties, they often impose substantial reputational costs on politicians, as the proceedings and their outcomes are publicly accessible.

## 2.1 Mayoral elections

Mayors in Colombia are elected for four-year, non-renewable terms by simple plurality.[11] Elections are held concurrently with those for governors, municipal councils, and departmental assemblies. Candidates may run under a party banner or as independents, supported by citizen signatures.

Although immediate reelection is prohibited, several mechanisms ensure that mayors remain responsive to their constituents and accountable for their performance in office. First, formal oversight institutions—including the *Procuraduría General de la Nación*, the *Contraloría General de la Nación* or the *Fiscalía de la Nación*—regularly audit and investigate potential cases of corruption, embezzlement of public funds, irregularities in procurement, or any type of mismanagement by municipal governments, and can impose sanctions when necessary. Civil society organizations may also initiate recall processes or escalate concerns to national oversight bodies.

Second, career incentives sustain performance despite term limits. Local office frequently serves as a stepping stone to future political positions. To assess the extent of electoral ambition among Colombian mayors, we analyzed the universe of candidates running for local and legislative office between 1997 and 2023, using administrative records compiled by CEDE at the University of Los Andes.[12] We find that over 40% of elected mayors run again in a later election—local or legislative—and around 30% rerun in their own municipality after the mandatory cooling-off period. These patterns indicate strong mayors' electoral ambition that provide a strong reason to exert effort in office even without the prospect of immediate reelection.

Finally, informal political incentives further reinforce accountability. Many mayors belong to parties or local family political dynasties that depend on well-performing representatives to maintain influence. These actors can reward or punish performance through internal promotions, endorsements for higher-level positions, or the withdrawal of future opportunities (Caputo-Leyva 2022). Even in the absence of party or dynasty pressures, mayors frequently pursue political or professional careers that hinge on their reputation as competent and effective leaders, much of which is shaped during their term in office.

[11] An exception applies in Bogotá, where a runoff is held if no candidate obtains at least 40% of the vote and the leading candidate does not win by a margin of at least 10 percentage points.

[12] We matched elected mayors to candidates in subsequent elections using a phonetic algorithm based on full names and two last names. To reduce false positives, we restricted to individuals with two last names and required exact municipality matches for reruns in the same locality. These choices reduce misidentification risk but may omit some true matches. Appendix Figure A2 presents the resulting estimates.

## 2.2 Local Service Provision and Mayoral Responsibilities

Mayors serve as the highest executive authority at the municipal level, responsible for implementing the local budget, ensuring the provision of basic public services, and coordinating with national and departmental authorities. As in other Latin American countries, these responsibilities have been gradually strengthened through a process of decentralization over the past decades, particularly following the 1991 Constitution (Falleti 2010).

The Constitution, together with key pieces of legislation, entrusts mayors with a well-defined set of service delivery responsibilities not shared with other levels of government. This institutional arrangement allows for a transparent evaluation of mayoral performance by tracking progress within this specific domain, thereby reducing attribution problems. Although some of these public services are not straightforward to measure, as discussed in the next section, we propose a series of strategies to capture them comprehensively. This approach helps mitigate the concern that strong performance in one observable area might be offset by underperformance in another, less easily observed dimension.

The main public services under mayors' responsibility are the following: (i) aqueduct and (ii) sewer network connectivity, (iii) waste collection, (iv) public lighting, and (v) local road infrastructure. The first three of these services are known as "residential public services" (*servicios públicos domiciliarios*), and are regulated by *Law 142 of 1994*. The latter two are classified as "non-residential services" and are regulated, mainly, by *Decree 2424 of 2006* and *Law 136 of 1994*. While road infrastructure and public lighting that connect more than one municipality may be provided or supported by higher-level governments, it is the explicit responsibility of mayors to ensure the provision of these services when they only involve their municipality.

Mayors are tasked with ensuring that these services are provided broadly, efficiently, continuously, and with adequate quality. The tools they are endowed with to do so are several. First, they are in charge of formulating development plans (*Planes de Desarrollo Municipal* and *Planes de Ordenamiento Territorial*) to set service provision goals and allocate the budget intended to cover these expenditures. Municipal revenue comes from its own collected revenue (e.g., property taxes, fees and service charges), as well as national transfers, the most important of which comes from the *Sistema General de Participaciones*, which is often earmarked for specific purposes, including locally provided services.

Second, they decide *how* these services are provided. They may do so directly through publicly constituted companies—particularly in small or underserved areas—or by contracting their provision to private or mixed companies through procurement processes.[13] Procure-

[13] Municipalities may provide services directly only when no private providers are willing to do so, or when they can demonstrate that they can deliver the service more cost-effectively than private providers. As a

ment is a central component of the production function of public services and remains vulnerable to various types of inefficiencies. In this context, as in other under-institutionalized settings, it has been shown to be affected by rent-seeking, collusion, and political favoritism, especially at the subnational level (Gulzar et al. 2022). Thus, mayoral supervision and involvement in this process plays an important role in improving service provision and quality.

Third, mayors are responsible for monitoring the provision of these services by local providers. Although they do not hold direct sanctioning authority, they can escalate concerns to national-level entities—most importantly, the *Superintendencia de Servicios Públicos Domiciliarios*—which is empowered to investigate providers, impose sanctions, and, in extreme cases, intervene in their operations. This oversight role allows mayors to influence service quality indirectly by ensuring that performance failures or irregularities are formally reported to the competent regulatory authority.

**Other public services.** Contrary to other countries in the region, such as Brazil, Colombian mayors are typically not responsible for health or education services.[14] In these sectors, mayors play primarily a coordinating role, while the main responsibility and fiscal burden fall on departmental and national authorities. Finally, although mayors are formally designated as the highest security authority at the local level, their role in practice is limited. Security enforcement is carried out mainly by the National Police and the military. Mayors coordinate activities with these institutions and may contribute to initiatives such as surveillance systems; however, priorities and resources for personnel, equipment, and operations are set and managed at the national government.

## 2.3 The 2019 Experiment

In our original intervention, we implemented a large-scale field experiment around the 2019 local elections with the explicit goal of addressing two shortcomings of conventional strategies to combat electoral irregularities: limited scalability and low cost-effectiveness. To do so, we tested whether a citizen-based election monitoring campaign—disseminated online and implemented in coordination with enforcement authorities—could serve as a scalable and cost-efficient tool for deterring electoral irregularities. While the full details of the intervention and its results are presented in Garbiras-Díaz and Montenegro (2022), we briefly

result, direct public provision is relatively rare; in 2023 only about 9% of providers were public (Superservicios 2024).

[14] Departmental capitals and some municipalities with populations above 100,000 that demonstrate high financial and administrative capacity can be certified to provide these services (*Law 715 of 2001*). Given that the selection criteria for municipalities in our intervention had a relatively low population threshold, very few of them are certified in our sample.

summarize here the main features of the experimental design and its short-term effects on irregularities and electoral outcomes.

**Experimental Design.** The unit of randomization was the municipality, the administrative level at which mayors are elected. Our sample comprised 698 municipalities.

A randomly selected two-thirds of these municipalities were assigned to the main treatment, in which we encouraged citizens to report electoral irregularities through an online platform called "*Pilas con el voto*" ("Watch out for the vote"), operated by the MOE. To do so, we implemented a large-scale social media campaign delivered via Facebook and Instagram ads. The campaign ran for five days—from October 24 to October 28, just before and during the October 27 elections—and targeted all adults (18 years and older) residing in treated municipalities. This timing allowed citizens to report misdeeds in the days leading up to the election, on election day, and the day after.[15] The campaign reached 4.4 million users—approximately one-third of the targeted population.

The main ad shown in treated municipalities included two components: (i) an information message indicating citizens where to report, thereby lowering the cost of reporting; (ii) a call-to-action message emphasizing the urgency of reducing irregularities.[16] To net out any potential effects of exposure to political ads, control group municipalities received a "placebo" ad reminding citizens about the election date.

In a second treatment arm, we coordinated with the Attorney General's Office to send formal letters to the universe of mayoral candidates in a random subset of municipalities treated with the online campaign. A first letter was mailed on September 6, after the candidate registration deadline, followed by a reminder email three weeks before the election. This component was designed to capture candidates' reactions to the increased monitoring.[17]

**Results on Irregularities.** Because electoral manipulation is inherently covert, irregularities are difficult to observe directly. To overcome this challenge, in Garbiras-Díaz and

[15] Electoral crimes are defined in the Colombian Criminal Code. Misdeeds occurring before election day include campaigning by public officials and voter registration fraud. Election-day violations include electoral fraud and illicit political advertising, which is prohibited on election day. A full list is available at https://www.cne.gov.co/delitos-electorales.

[16] In practice we randomized whether municipalities received both components or only one. For conciseness, and to maximize power, we pool these treatment arms in the main analyses.

[17] Among municipalities receiving the letter, we also randomized the version of the letter to account for the possibility of signal jamming—for instance, candidates might use the information about the reporting site to systematically attack the opposition through false reports, thereby rendering the platform uninformative. To that end, we created a "full knowledge" version of the letter that explicitly informed candidates about the website used in the citizen campaign. In our main analyses, we pool these subtreatments and focus on the overall campaign and the letter arm.

Montenegro (2022), we relied on two complementary approaches. First, we used mentions of electoral irregularities in local and national media—including radio, television, print, and online outlets—collected by an independent media-monitoring firm. Second, we constructed a forensic measure using fine-grained vote-tally data at the voting booth level (*mesa de votación*), applying standard tests from the criminology literature, such as second-digit Benford's Law, to detect statistical anomalies consistent with manipulation.

Across both measures, we find that the social media intervention reduced the incidence of electoral irregularities. Treated municipalities were 6 percentage points less likely to be mentioned in media reports documenting electoral misconduct. Using our forensic measure, treated municipalities were 9 percentage points less likely to fail tests for statistical anomalies. We obtain qualitatively similar results using continuous versions of both measures.[18]

For the second treatment arm—the letters sent to candidates—most effects appear to be driven by municipalities where candidates were explicitly informed about the campaign, suggesting a potential deterrence mechanism behind the observed reduction in irregularities. However, the estimated difference in coefficients is not statistically significant.

**Results on Elections.** We then examined whether the intervention affected electoral outcomes. We begin by testing if candidates who relied more heavily on electoral malpractice saw their vote share reduced in municipalities exposed to the intervention. To identify these candidates, we administered a pre-treatment survey three weeks before the intervention, asking citizens to evaluate each mayoral candidate's perceived propensity for electoral misconduct. We find that candidates scoring above average on the irregularity measure experienced a 2.5 percentage point reduction in vote share within treatment municipalities. Other than these effects on candidate vote shares, we found no effects on other election outcomes, such as turnout or electoral competition.

# 3 Data and Empirical Specification

## 3.1 Measuring Local Public Service Access

Analyzing locally provided public services presents several challenges. Data about these services is typically scarce, not always systematically collected, and, in cases when it is available, it might not be subject to the most rigorous quality controls. We address these challenges combining different sources depending on the type of public service studied.

[18] The original article also presents results for additional outcomes and a series of robustness checks.

**Administrative Data.** We collect data on municipal access to aqueduct, sewerage and waste collection from the *Superintendencia de Servicios Públicos Domiciliarios* (SSPD), a national public agency overseeing the provision of these "domiciliary" public services. By law, municipalities must report these data annually, with the help of providers, to a unified system managed by the SSPD (Law 1176 of 2007). The main indicators reported are the percentage of properties in a given municipality that have access to each of the mentioned public services.

We implement two data-cleaning steps on the 2016-2024 available data. First, we interpolate to fill missing variables. Second, we clean for outliers by replacing observations at time $t$ with the average of the values at $t-1$ and $t+1$, when the value at $t$ differs by more than three standard deviations from this average.

**Satellite Data.** Data on public lighting and local road infrastructure is not systematically collected by the central government in our setting. To overcome this lack of information, we use historic satellite-based proxies of these types of public goods. We use yearly nighttime light data from the *Visible Infrared Imaging Radiometer Suite* (VIIRS). This data has been used extensively by NASA as well as by researchers for different uses, including to proxy for electrification efforts in developing contexts (e.g., see Gibson et al. (2020) for a review). The yearly public version of this data aggregates monthly cloud-free radiance across pixels of approximately a 500x500 meters. We use the median radiance aggregation across months to minimize the risk that outliers introduce noise into the estimates.

**Survey Data.** The National Department of Statistics (DANE, by its acronym in Spanish) collects household-level information on access to aqueduct, sewerage, and waste collection services through the yearly Qaulity of Life Survey (*Encuesta de Calidad de Vida*, or ECV). While this survey is not representative at the municipal level, it provides a external validation of the administrative data, which are self-reported and may be subject to concerns about data quality. We therefore use these survey responses as a robustness check.

We accessed the microdata through DANE's secure access facilities and pooled the ECV datasets for the years 2022 and 2023 to maximize coverage. The resulting sample includes approximately 103,000 respondents across 469 municipalities. We conduct the analysis at the individual level, allowing us to control for fixed respondent-level characteristics such as age, gender, and whether the residence is located in a rural or urban area. This approach helps to substantially reduce residual variance in the outcome variables.

**Public Service Index.** We aggregate the administrative and satellite-based indicators described above into a single index. This composite measure offers a transparent and parsimonious way to assess whether the intervention had an overall effect on public goods provision. Additionally, it helps mitigate concerns related to multiple hypothesis testing, given the relatively large number of individual outcomes analyzed (Anderson 2008). As a complement, we also report results for each type of public good separately.

We build our main index in three steps. First, we measure the change in each of the measures of specific public goods:

$$\Delta y^j = y^j_{post} - y^j_{pre}$$

where $y^j_{post}$ is the measure of public good of type $j \in \{\text{aqueduct, sewerage, waste, lighting, roads}\}$ in a post period, after the intervention, while $y^j_{pre}$ is measured before the intervention. In our main analysis, the pre-intervention period is defined as the average across the years 2018 and 2019, while the post-intervention period corresponds to the average across 2022 and 2023. The choice of this post period reflects the notion that expanding the stock of public goods requires time and is therefore best captured toward the end of the mayors' terms. We also analyze the dynamics of public goods provision separately, which confirms this intuition.

Our main analysis uses the *changes* in public good provision in the construction of the public service index, as described above, rather than the *levels*. This approach improves the precision of the estimates by accounting for pre-existing municipal-level differences in public goods. As a robustness check, we also report results based on the levels of public goods provision and provide evidence that there are no significant imbalances in these variables during the pre-intervention period.

Second, we standardize each of these variables using the mean and standard deviation of the municipalities of the control group.

$$z\left(\Delta y^j\right) = \frac{\Delta y^j - \mu_{\Delta y^j, control}}{\sigma_{\Delta y^j, control}}$$

Third, we add these standardized variables into an index:

$$I = \sum_j z\left(\Delta y^j\right)$$

Finally, we standardize this index using again the mean and standard deviation of the

control group:

$$z\left(I\right) = \frac{I - \mu_{I,control}}{\sigma_{I,control}}$$

## 3.2 Additional Variables of Interest

**Mechanisms.** We collected data on four main groups of intermediate variables through which the intervention may have affected the provision of local public goods. First, we gathered information related to the efficiency of public procurement and corruption processes within municipal governments, drawing on two primary sources. The first is SECOP, a publicly available dataset that records all public procurement contracts executed by all levels of government. This dataset includes information on the contracting government agency, the final provider of the good or service, the total value of each contract, and a description of the contract's purpose.

Critically, SECOP also reports the modality of the procurement process—that is, whether the contract was awarded through a competitive selection process or assigned directly to a provider without competition. We use this variable to calculate, for each municipality, the share of total contract value awarded competitively versus directly over the 2020–2023 period.[19]

The information in SECOP is imputed by the procurement offices of government agencies, so errors and typos are common. To reduce the influence of (extreme) outliers, we drop contracts whose value exceeds the $99.99^{th}$ percentile in their category of contracted goods and services.[20]

We collect data on judicial corruption cases involving local governments from the *Sistema Penal Oral Acusatorio* (SPOA) dataset, Colombia's national criminal case management system, which is maintained by the Office of the Attorney General (*Fiscalía General de la Nación*). It contains all processes due to crimes by type of government agency. We restrict our attention to municipal governments and study the process associated with corruption crimes.[21]

The second group of intermediate variables we examine relates to municipal governments' revenue collection. We obtain these data from the *Departamento Nacional de Planeación*

[19] We define competitive selection process to be those indicating public tenders (*licitaciones*, merit contests (*concurso de méritos* and auctions (*subastas*).

[20] These categories include, for instance, consulting, service contracts, or public works, which might include very different average values, which warrants doing a category-specific outlier-removal process. We manually confirm that each of the dropped contracts seemed to have a type in its reported value.

[21] We consider corruption cases to be those labeled as having to do with administrative, judicial, tax, electoral or private corruption crimes.

(DNP), which compiles detailed annual information on municipal revenues, which are reported directly by these local governments through forms (FURAG) in a yearly frequency. Using the official categorization of revenue sources, we analyze differences across municipalities in the composition of revenues, including national government transfers, locally collected taxes, and other income sources.

The third group of intermediate variables we examine consists of education and healthcare inputs. For the former, we rely on data collected by the Colombian National Statistics Office in coordination with the Ministry of Education through the *Censo Anual de Educación Formal*. We use information on the number of facilities, teachers, administrative staff and students in each municipality, focusing on primary and secondary education. We compute pre- and post-intervention changes using data from 2019 and 2022 (the most recent available wave), respectively. For healthcare inputs, we rely on data from the Colombian Ministry of Health. We draw on multiple sources, including the *Registro Especial de Prestadores de Servicios de Salud* and the *Registro de Talento Humano en Salud*. We construct indicators for the number of facilities, physicians, nurses, beds, offices, and other inputs. We compute changes between the pre- and post-intervention periods using 2019 and 2023 data.

Finally, the last group of intermediate variables considered relates to citizens' demand for local public goods. We consider four types of outcomes that capture the primary ways in which citizens in this context may either express their desire for improved public goods or organize collectively to provide them directly. The first of these variables measures instances of citizen protests during the study period (2020–2023). We construct this variable using data from the protest database compiled by the *Centro de Investigación y Educación Popular* (CINEP), a well-regarded local NGO that documents and studies social movements and human rights violations in the region. Their protest dataset ("*Base de luchas sociales*") is assembled through manual review of mentions of protest events across multiple sources, including the "(...) written press, radio, television, internet, social media and communications by organizations" (according to their website).

The second variable used to capture citizen demand for public goods is the number of, and membership in, *Juntas de Acción Comunal* (JACs), which are legally regulated grassroots organizations that operate at the local level and play a key role in civic participation and local development (Law 743 of 2002). These organizations serve as intermediaries between citizens and local governments, advocating for community needs and priorities. They also occasionally implement small-scale infrastructure projects, particularly in areas such as sanitation and local road construction. With over six million members nationwide, JACs constitute the largest grassroots organization in Colombia dedicated to local development. We obtained data on the number of JACs and their membership per organization from the

Ministry of the Interior, the institution responsible for regulating their operations.

The third intermediate variable related to citizen demand for public goods is the number of submitted *tutelas*, which are constitutional writs filed by individuals seeking protection of their fundamental rights before the state. These often include demands for access to basic public goods, given that such access is constitutionally protected in Colombia. The *tutela* process is designed to be accessible: it does not require legal representation, and judges are required to issue an initial ruling within ten business days (Law 393 of 1997). As a result, tutelas have become a widely used mechanism for citizens to defend their constitutional rights (see Londoño-Vélez and Saravia (2025) for further context). We use aggregate data from the Constitutional Court on the number of tutelas filed in each municipality between 2020 and 2023, along with information on the specific rights invoked in each case.

The final set of intermediate variables related to citizen demand for public goods is the number of social media users in each municipality and, in particular, the subset of them who are interested in political and social issues. We collect these data using Meta's Marketing API which provides lower and upper bounds on the number of active monthly users in a given targeted population. Our used measure is the average of both of these bounds. We measure the total number of users per municipalities of age 18 and higher, as well as the subset of these that express interest in political and social issues, according to Meta's classification.[22]Unfortunately, this data is not available retrospectively. We collected it in March 2026, and is thus only a proxy of number of users in 2023.

**Candidate Characteristics.** We also compile a comprehensive set of characteristics for mayors elected in 2019 to investigate how the intervention may have influenced candidate selection. Candidate age, sex, and party affiliation are obtained directly from the information provided upon registering their candidacy with the *Registraduría Nacional*, the primary electoral authority. To estimate political experience, we count the number of times each candidate has been elected to municipal councils, Mayors' Offices, the Senate, or the House of Representatives since 1992, using election data collected and cleaned by the *Centro de Estudios sobre Desarrollo Económico* (CEDE) at the *Universidad de los Andes*. Additionally, we gather information on mayors' education and their experience in the public and private sectors from the biographical statements included in their candidacy manifestos, which are submitted to and publicly posted by the *Registraduría Nacional*.[23] Finally, we collect data

[22]More concretely, we define these users as those interest in "Politics and social issues", "Politics", "Community issues" or "Volunteering".

[23]Although submission of manifestos is legally required, only 462 mayors—approximately two thirds of our experimental sample—include biographical information. In our main analysis, we treat missing bios as missing data; however, results remain robust when coding these cases as zeros (i.e., indicating no education

on mayors' business ownership by scraping records from the *Registro Mercantil*, Colombia's official business registry.

## 3.3 Empirical Specification

We use a simple linear specification to estimate the average effects of the intervention:

$$y_m = \mathbf{T}_m'\beta + X_m'\gamma + \epsilon_m \tag{1}$$

where $y_m$ is the outcome variable in municipality $m$; $\mathbf{T}_m$ is a vector of indicators for different aggregations of treatment arms in the intervention; $X_m$ is a set of municipal-level covariates; and $\epsilon_m$ is the error term.[24]

In our main specifications, we estimate this equation using two different aggregations of the treatment arms. First, we pool all treatment arms into a single combined treatment indicator. Second, we distinguish between treated municipalities in which we sent letters to mayoral candidates and those in which we did not. We focus on these partitions because, as reported in Garbiras-Díaz and Montenegro (2022), they capture the main differences in electoral irregularities—municipalities where candidates were sent a letter experienced the largest reductions in such irregularities, as discussed above.

Following Garbiras-Díaz and Montenegro (2022), we select the covariates included in $X_m$ using the double-post-Lasso covariate selection method proposed by Chernozhukov et al. (2015) and Belloni et al. (2014). The set of potential covariates for this method corresponds to those used in our original paper, with the addition of a baseline measure of the outcome variable when available. We also report estimates without control variables as a robustness check.

Finally, all tables include randomization inference p-values (Athey and Imbens 2017; Young 2018) alongside heteroskedasticity-robust standard errors.

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or public/private sector experience). Importantly, the submission of bios is well balanced across treatment arms.

[24] While our strategy to examine the effects of electoral integrity on public service provision can be understood as through the lens of an instrumental variable framework, in which the effects of the experimental intervention on electoral irregularities represent a "first stage" relationship, we refrain from actually computing IV estimates for two main reasons. First, irregularities are measured with substantial noise given the difficulty in measuring them. As a result, using our intervention as an instrument for irregularities would yield a first stage that is severely attenuated by measurement error. This would generate weak-instrument problems—such as highly unstable and potentially inflated IV estimates—and thus provide little information about the local average treatment effect (Staiger and Stock 1997). Second, the exclusion restriction might potentially fail to hold, further complicating the interpretation of the IV estimate. For these reasons, we interpret our estimates as reduced-form effects of cleaner elections rather than the causal effect of irregularities per se.

# 4 Main Results

In this section, we present and discuss the estimated effects of our intervention on public service provision. We then examine additional outcome variables that might mediate this relationship, and finally, we explore alternative explanations.

## 4.1 The Effects of Electoral Integrity on Public Service Provision

We begin by examining the effect of the intervention on better local public service provision in the four years after the ad campaign. As explained in Section 2, in this setting, mayors are primarily in charge of providing these services, and thus the progress they make on this front represents a transparent measure of their performance in office.

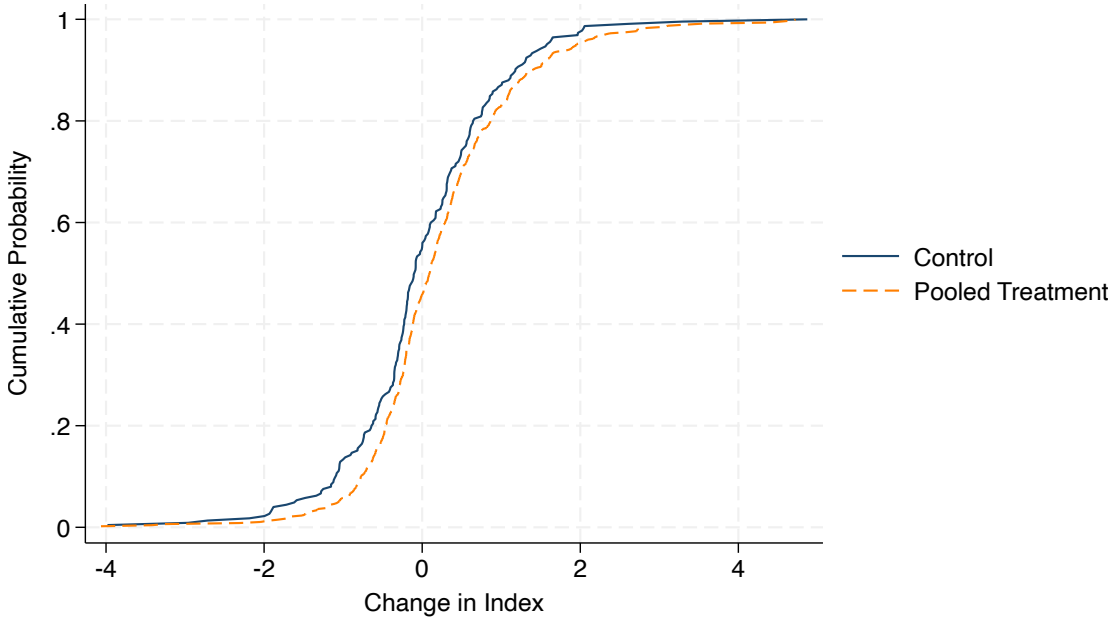
To assess the potential effect of the intervention across the distribution, in Figure 1 we plot the cumulative density function of the the Public Service Index, described in Section 3.1, by treatment condition. This figure reveals that there is clear rightward shift along the whole distribution of this index in municipalities exposed to the intervention. This suggests that the intervention caused an improvement in the provision of public services, and that this effect was experienced by all municipalities, regardless of their level of access to public services.

We next examine the average effect of the intervention by estimating the model in equation (1). Column (1) of Table 1 displays the point coefficients of this exercise. These results suggest that treated municipalities exhibit better service provision four years later, relative to their pre-intervention levels. Specifically, we estimate an increase of 0.19 standard deviations in this index. While these estimates are based on pre-post changes, the results are very similar when examining levels directly (see Appendix Table A2).

In Panel B we examine how this effect varied between municipalities receiving the letter to politicians for those who did not receive it. The estimates reveal that the effects are stronger among the municipalities receiving the letter to candidates (although this difference is not significant). This result echoes the finding in Garbiras-Díaz and Montenegro (2022) that this treatment arm was particularly effective in reducing electoral irregularities.

We next disaggregate the index to identify the components driving these effects (Figure 2). We find that improvements in water supply and sewerage are concentrated in urban areas, while gains in waste management are driven by improvements in rural areas. Effect sizes range from 0.13 to 0.17 standard deviations ($p$-value $< 0.05$ for all estimated effects). In level terms (i.e., post-intervention measures), these effects correspond to increases of between 2 and 4 percentage points in properties with access to the studied services. These are sizeable effects, representing increases of approximately 2-3% compared to the control group mean

Figure 1: Estimated Cumulative Density Function for Index by Treatment Condition




**Note:** This figure presents the cumulative probability of our Public Service Index, by treatment condition. The treatment group, identified in dashed lines, pools together all municipalities receiving the social media campaign encouraging citizens to report irregularities.

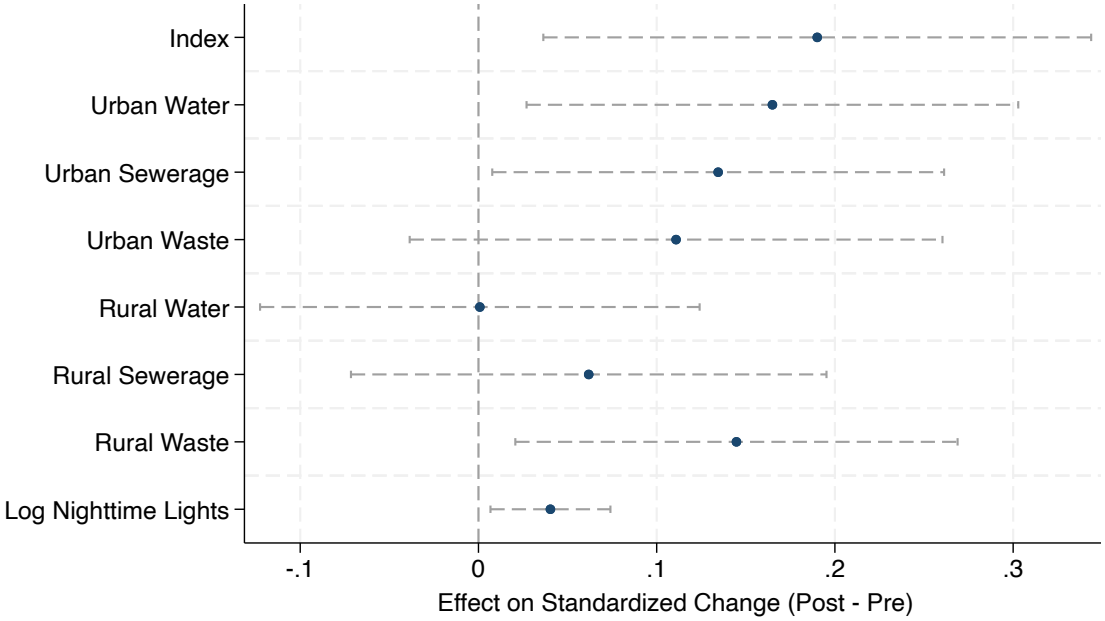
in urban services, and up to 16% in the case of waste collection in rural settings.

Turning to nighttime lights, which we log-transform to account for the skewed distribution of the original variable, we observe a 0.04 standard deviation increase in the change between pre- and post-treatment values ($p$-value $< 0.05$). When measured in levels, this effect corresponds to a 0.036% ($p$-value $< 0.01$).

One potential concern in using nighttime lights as a proxy for public lighting provision is that changes in nighttime luminosity may capture changes in economic activity, which has been documented in the development literature (e.g., Gibson et al. (2020)). To assess this possibility, we first examine whether the intervention affected GDP and find no evidence that it did (column (1) in Table A4). We then test whether the intervention only affected the component of nighttime lights unrelated to economic activity. Specifically, we residualize nighttime lights with respect to GDP and show that the intervention impacts only this residual component (columns (2) and (3) in Table A4).

We then examine whether these effects are driven by differences in public service access in urban or rural parts of municipalities. Insofar as expanding infrastructure might be cheaper in urban areas due to economies of scale, we would expect mayors to focus there efforts on them. These areas might be more attractive for mayors in terms of political gains, given

Figure 2: Effects on Public Service Index and Its Components (Pre–Post)




**Note:** This figure presents the estimated effects of the intervention on the components of the Public Service Index, measured in levels. These results correspond to Table A1. All variables have been standardized as described in Section 3.1.

that there are higher density of voters in these areas. However, as mentioned in Section 2, access to public services is lower in rural parts, which would suggest that efforts might be more impactful in these areas.

To study this question, we compute public service indices independently for urban and rural areas of municipalities.[25] These results suggest that effects are stronger in urban areas ($\hat{\beta}$ = 0.214, $p$-value < 0.01). The effect is also positive for rural areas, but its magnitude is roughly half the size of that in urban areas and is not significant. Again, we find stronger and more robust results for those municipalities where candidates where notified about the intervention (see Table 1).

The evidence presented thus far relies on administrative records that, while available for the universe of municipalities in our experimental sample, are subject to the data quality limitations discussed earlier. As a robustness check, we next turn to evidence from direct beneficiaries. For this purpose, we draw on the household survey data described in Section 3. The results, presented in Appendix Table A3, validate our previous findings: respondents in treated municipalities report higher access to public services, as measured by an index that

[25]The administrative data on access to the aqueduct, sewerage and waste collection differentiates between properties in each of the types of locations. For nighttime lights, we use satellite imagery to differentiate by these same types of locations.

Table 1: Effect on Public Service Index, by Area (Pre-Post)

| | Index | Index Urban | Index Rural |
|---|---|---|---|
| **Panel A. Pooled Treatment Effects** | | | |
| [$T$] Any treatment | 0.190** | 0.214*** | 0.112 |
| | (0.078) | (0.080) | (0.077) |
| | [0.003] | [0.000] | [0.143] |
| **Panel B. Letter - No Letter** | | | |
| [$NL$] No Letter - Any Ad | 0.166* | 0.186* | 0.115 |
| | (0.097) | (0.096) | (0.095) |
| | [0.080] | [0.060] | [0.227] |
| [$L$] Letter - Any Ad | 0.202** | 0.229*** | 0.110 |
| | (0.085) | (0.088) | (0.084) |
| | [0.017] | [0.007] | [0.227] |
| Test $NL = L$, p-value | 0.69 | 0.64 | 0.95 |
| Control Mean | 0.000 | 0.000 | 0.000 |
| Sample Size | 697 | 697 | 696 |

**Notes:** This table presents the effects of the intervention on the public service index, by area of coverage. All specifications include the covariates selected using the method described in Chernozhukov et al. (2015) and Belloni et al. (2014). Robust standard errors are shown in parentheses, and randomization inference p-values are shown in square brackets. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

aggregates across the available measures. The estimated effect suggests that the intervention increased access to public services by 0.08-standard deviations ($p < 0.01$).

Together, these results show that the intervention, which promoted cleaner elections, led to improved access to public services, both as measured in a combined index and separately for the different types of services under the responsibility of mayors.

## 4.2 Mechanisms and Alternative Explanations

### 4.2.1 Procurement and Corruption

Our reduced form findings indicate that public service provision increased significantly in municipalities where we implemented the campaign in 2019, relative to the control group. We now explore potential mechanisms that could explain these observed improvements in local service delivery.

One key mechanism related to how municipal governments procure goods and services. In the Colombian context, public procurement has long been recognized as an area particularly prone to corruption and inefficiencies (Gulzar et al. 2022). As such, either better selected politicians or more accountable ones might engage in more efficient or transparent procurement practices, which might then result in a better provision of public services procured. To assess whether the intervention influenced how public resources were allocated, we examine

two sets of outcomes related to procurement practices and local administrative integrity.

First, we explore the degree to which local governments engage in less competitive types of procurement, which have been used previously in the literature to proxy for corruption and inefficiency (Fazekas et al. 2025; Baltrunaite et al. 2020). In Section 3, we provide further details of the sources and how we build these variables.

The estimates are displayed in Table A5 in the Appendix. The results suggest that treated municipalities significantly increased their use of competitive procurement methods—such as public tenders and open auctions—and decreased their reliance on non-competitive procedures, particularly direct contracting. More concretely, governments in treated municipalities spent over 2 percentage points more of their budget on competitive contracts and approximately 3 percent less on non-competitive, direct contracts. Moreover, these results are robust to whether the outcome variable is defined as the log absolute value. Reassuringly, we again find that these effects are stronger for the group of municipalities where candidates received the letter informing them about the intervention. These shifts suggest greater transparency and efficiency in the use of public funds, potentially reducing opportunities for clientelism and rent-seeking.

Second, we use data from SPOA (described above) to test whether treated municipal governments are less likely to be involved in corruption investigations. Results are shown in Appendix Table A6. They suggest that there was a reduction in corruption-related judicial processes against mayoral offices in treated municipalities. Treated municipalities had 0.5 corruption processes on SPOA, which represents a 26% compared to the control group mean. Interestingly, we observe no such effect for judiciary processes that do not have to do with corruption. On the extensive margin, treated municipalities also experience a decrease in the probability of having at least one corruption-related process, but this is not significant (probably because the baseline mean is already very high, with over 87% of municipalities having one such process). Since these legal proceedings are typically triggered by irregularities in contract execution and administrative practices, their decline provides further evidence of improved governance following the intervention.

#### 4.2.2 Long-Term Election Monitoring

We next examine whether the intervention produced lasting improvements in citizen oversight of elections. This is an essential precondition for sustained political *accountability*, which will be further formalized in Section 5. We use a natural outcome to measure this: the reporting activity of citizens during the 2023 elections, four years after the original intervention. In Table 2 we explore the effects of the intervention on the number of reports per thousand citizens, the inverse asymptotic sine (asinh) transformation of the number of

reports, and an indicator for whether any reports were issued from a given municipality.

We find that treated municipalities saw a continued increase in reporting activity according to all of these measures. On the intensive margin, municipalities exposed to any of the treatments experienced an increase of 0.025 reports per thousand citizens ($p < 0.05$), or a 12% increase when using the asinh transformation ($p < 0.1$). These numbers represent, respectively, a 25% and 10.5% increase compared to the control group mean of each variable. As expected, this effect is smaller than the one found in 2019 of a 67% increase compared to the control group mean, as reported in Garbiras-Díaz and Montenegro (2022). On the extensive margin, we find a positive but not statistically significant effect of the intervention on the probability of filing at least one report. These null results might be driven by the fact that, in 2023, 73% of municipalities filed at least one report, while this was only 29% in 2019. As before, we also find that the effects on all of this outcomes are larger, and statistically different in some cases, for municipalities where candidates were informed of the campaigned to those where they were not informed.

Table 2: Effects on Reports to MOE in 2023 Elections

| | Reports per 1000 citizens | Asinh Transform Count | Any report = 1 |
|---|---|---|---|
| **Panel A. Pooled Treatment Effects** | | | |
| [$T$] Any treatment | 0.025** | 0.122* | 0.045 |
| | (0.011) | (0.071) | (0.035) |
| | [0.027] | [0.097] | [0.180] |
| **Panel B. Letter vs. No Letter** | | | |
| [$NL$] No Letter - Any Ad | 0.014 | 0.025 | -0.017 |
| | (0.013) | (0.094) | (0.046) |
| | [0.300] | [0.790] | [0.727] |
| [$L$] Letter - Any Ad | 0.030** | 0.172** | 0.078** |
| | (0.012) | (0.076) | (0.037) |
| | [0.010] | [0.047] | [0.050] |
| Test $NL = L$, p-value | 0.24 | 0.10 | 0.02 |
| Control Mean | 0.099 | 1.159 | 0.707 |
| Sample Size | 698 | 698 | 698 |

**Notes:** This table presents the effect of the intervention on citizen reports to the MOE around the 2023 elections. Column (1) shows the number of reports per 1,000 citizens, column (2) shows the inverse hyperbolic sine transformation of the total number of reports in 2023 (which accounts for the large number of zeros), and column (3) shows an indicator that takes the value of 1 if the latter variable is positive. All specifications include the covariates selected using the method described in Chernozhukov et al. (2015) and Belloni et al. (2014). Robust standard errors are shown in parentheses and random inference p-values are shown in square brackets; *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

### 4.2.3 Alternative Explanations

We now examine the three most plausible alternative mechanisms that could explain how the intervention impacted public service provision.

**1. Local Government Revenue and National Support.** The intervention might have increased the revenue of local governments though several different channels, which have then improved the resources used to increase public service provision. First, it might have affected how efficient mayors might have been in collecting locally-collected revenue (mainly through property taxes and fines). Second, it might have also improved the efforts of mayors to obtain national-level transfers or support from the higher-order governments.

We begin by using administrative data on municipal revenues to test these hypothesis. Table A7 in the Appendix displays the results. Overall, we find no significant differences in local government revenue between treated and control municipalities. Moreover, this is true also when we look at either locally-collected tax revenue, or and inter-governmental transfers, which is the largest component of municipal revenue.

**2. Services Outside the Mayor's Purview.** Next, we examine whether the intervention affected services provided primarily by higher levels of government. As explained in Section 2.2, education and healthcare services in the municipalities studied are largely provided by departmental authorities. Examining these services, therefore, provides an indirect test of whether observed improvements in public service provision were driven by differential support from higher-order authorities. We test this possibility by examining differences in educational and healthcare inputs.[26]

We begin with education inputs, examining the number of facilities, teachers, administrative staff, and students. The results (reported in Appendix Table A8) show no differences in any of these variables, independently of whether we consider all schools in municipalities or just public ones.

We then examine healthcare inputs, including the number of facilities, physicians, nurses and assistants, stretches, beds, ambulances, offices and consultation rooms. The results (reported in Appendix Table A9) again show no effect of our intervention in any of these inputs.

Together, the evidence from subsections 1 and 2 suggests that the improvements in local pubic service provision generated by the intervention are more likely attributable to more

[26] We focus on inputs to capture outcomes that lie directly outside the mayor's purview. Outputs could reflect downstream effects of improvements in municipal services, such as sanitation or infrastructure.

efficient use of existing resources rather than to an increase in fiscal capacity or additional support from higher-order levels of government.

**3. Citizen Demand for Public Services.** We next explore whether citizen demand for public services might explain differences in access to these services. This might have occurred because the ad campaign had an explicit objective of making citizens mobilize for a the particular cause of reporting irregularities, but which might have extended to other causes by improving collective action more generally.

There is no unique objective way of measuring citizen demand for public services. However, we examine the four most plausible ways through which this demand could materialize into actual differences in public service provision in this setting.

First, we examine whether treated municipalities experienced more protests, which might be a vehicle to demand more public services and government actions. The estimates of this exercise are displayed in Table A10. We find no evidence that the intervention affected the occurence of protests per 10 thousand inhabitants. Moreover, when we focus on only protests with a municipal scope, demanding changes in public policies and directed against the municipal executive government, we find that the municipalities where candidates received a letter informing them about the campaign actually saw a *decrease* in protests. This would be the opposite of what would happen if citizen demand for public services increased. Instead, it could be the result of the fact that supply of these services increased, which would decrease citizens' need to demand them through protests.

Our second measure of citizen demand are *tutelas*, which, as mentioned above (Section 3) are a popular legal tool used by Colombian citizens to ensure their fundamental rights before the state. In practice, a common use is to use them to demand access to basic public services to the national government, since these services are a pre-condition to protect citizen's life, which is their main fundamental right. In Table A11 in the Appendix we explore the effects of the intervention on the number of *tutelas* per thousand inhabitants in treated municipalities. Overall, we find no evidence that there are more tutelas filed in general or specifically by type of demand, as a consequence of the intervention. In particular, the effect of *tutelas* filed demanding access to public works are not significant and small.

Next, we alternatively measure citizen demand through the presence and membership of *Juntas de Acción Comunal* (JACs), which are the most popular grassroots organizations in this context, and which often are involved in supporting the provision of public services at the local level. In Table A12 we display the estimates on the number of JACs and on their membership per capita.[27] Across these different measures we find small and insignificant

[27]For reference we also include these outcomes for all grassroot associations monitored by the Ministry

estimates, which suggest that JAC presence and membership are unlikely to be explaining the results.

Finally, we examine whether the intervention might have facilitated collective action by measuring it's effect on the number of social media users and, in particular, on those who express interest in political and social issues, which could shift the demand for local public services. We measure these variables as the log number of Meta users, as well as the number of users as a percentage of the population in each municipality. The estimates reported in Table A13 are small and insignificant, suggesting that the intervention did not affect thisproxy for citizen collective action and behavior either.

Despite the fact that we only have a limited number of variables proxying citizen demand for local public services, we believe that they are the ones more likely to reflect changes in citizen behavior that could have impacted the provision of these public services. Therefore, we interpret the findings of null results as compelling evidence that this alternative explanation does not play an important role in explaining our main results.

# 5 Selection and Accountability Channels

In this section, we develop a simple framework to decompose the observed effect of the intervention on public good provision into: (i) the extent to which it is driven by the selection of politicians who are less involved in electoral irregularities, and (ii) the extent to which it results from improved accountability among incumbent politicians, holding constant any changes in their underlying characteristics. Our focus on these two specific mechanisms—selection and accountability—reflects the traditional distinction emphasized in the literature on the welfare-enhancing features of democracy (Fearon 1999; Ferejohn 1986; Besley 2006).

We begin by assuming that the public good provision in municipality $m$ when politician $i$ is in office ($Y_{mi}$) is a function of three elements: (i) the type of mayor in power $\phi_i \in \{clean, dirty\}$, (ii) this mayor's effort in providing these goods, $e_i$, and (iii) an extra term $\xi_m$, which captures all other factors determining public goods provision that do not directly depend on the local politician in power, such as national programs and transfers to provide public goods or citizen-led actions to provide these goods, among others. We further assume that this function is linear, so that it simplifies to the following expression:

$$Y_{mi} = \phi_i + e_i + \xi_m \tag{2}$$

Denoting the average treatment effect of the intervention as $\Delta Y_{mi} \equiv E\left[Y_{mi} \mid D = 1\right] -$

of the Interior, but the vast majority of them are JACs, so the estimates barely change.

$E[Y_{mi} \mid D = 0]$, we can decompose it into three terms:

$$\underbrace{\Delta Y_{mi}}_{\text{ATE}} = \underbrace{\Delta \phi_i}_{\text{Selection Channel}} + \underbrace{\Delta e_i}_{\text{Accountability Channel}} + \underbrace{\Delta \xi_m}_{\text{Alternative Channels}} \tag{3}$$

The first of these terms is the selection channel, the second is the accountability channel, and the last one captures other potential channels. The selection channel operates on the premise that the intervention, by making elections cleaner, may have altered the type of politician who is elected. In turn, the characteristics of politicians elected under cleaner electoral conditions can influence the provision of local public goods. Formally, the selection effect can be expressed as the product of the change in the probability that a clean candidate is elected and the expected impact of electing such a candidate on public good provision:

$$\Delta \phi_i = \Delta Pr[i = \text{clean}] \times (E[Y \mid i = \text{clean}] - E[Y \mid i = \text{dirty}]) \tag{4}$$

According to this expression, we can obtain an estimate of the overall selection channel by separately estimating the two terms in (4). Moreover, having estimated this channel, we can get an estimate of the accountability channel under the stronger assumption that $\Delta \xi_m = 0$ by computing $\Delta e_i = \Delta Y_{mi} - \Delta \phi_i$. In Section 4.2, we provide supporting evidence for this assumption by examining the main alternative channels that could explain changes in public goods provision independently of local politicians, and showing that these channels do not appear to be driving results in this context. However, we acknowledge that this constitutes only suggestive evidence, and therefore, any estimate of the accountability channel should be interpreted with appropriate caution.

Another important caveat to keep in mind is that the proposed estimate of the selection channel will also capture changes in politicians' effort that are driven by their underlying type. This may be somewhat confusing, as effort is modeled as a separate term from politician type in equation (2). In this sense, the estimate of the accountability channel will only reflect changes in effort that are not attributable to changes in type. This does not undermine the validity of the exercise, but rather clarifies an important aspect of the interpretation of the proposed decomposition.

We now proceed to empirically obtain estimates of the selection and accountability channels in four main steps.

**Step 1. Obtaining estimates of politician types.** Prior to the intervention in 2019, we conducted an online pre-treatment survey to collect citizens' perceptions regarding which candidates were likely to engage in electoral irregularities in the upcoming elections. These

responses allow us to construct empirical proxies for categorizing mayors by type, as outlined in the previously described framework. Full details of the survey and methodology are provided in Garbiras-Díaz and Montenegro (2022); here, we summarize only the key elements of this measurement approach.

In this survey, we asked respondents to state whether each candidate would engage in the main types of electoral irregularities identified in this context (e.g., vote-buying, illicit advertisements, voter intimidation, etc.). We then aggregated these answers at the candidate level by computing the percentage of respondents that state a candidate was going to engage in any electoral irregularity. We denote this $Z_{mc}$, for candidate $c$ in municipality $m$. In Garbiras-Díaz and Montenegro (2022) we validate this measure by showing that it is positively correlated ($\rho \approx 0.4$) with an independent measure of candidates' past malfeasance performed by the local NGO *Fundación Paz y Reconciliación* (PARES) in a subset of 48 municipalities in our sample.

To maximize the use of information, we then created an indicator of candidate cleanliness by adding up the standardized version of (1) $Z_{mc}$, and (2) an indicator for whether candidate $c$ was *not* included in PARES' list of candidates with past malfeasance. We then created an indicator of whether this index was above average among the candidates in this municipality. This last variable is our main measure of candidate cleanliness.

**Step 2. Estimating $\Delta Pr\left[i = \textbf{clean}\right]$.** Using the categorization of clean and dirty politicians we then obtain an estimate of $\Delta Pr\left[i = \text{clean}\right]$ by using the indicator for clean mayors as an outcome in equation (1).

Column (1) of Appendix Table A14 shows the main results. While 31% of municipalities in the control group elected clean candidates, we see that this increased by 7 percentage points ($p < 0.1$) to 38% in the municipalities exposed to the intervention. This represents a large increase of 26% over the control group mean. Moreover, we see that this effect is larger and more precise in municipalities in which politicians received a letter informing them of the monitoring campaign, which is also what we find in Garbiras-Díaz and Montenegro (2022) with respect to the effects on electoral irregularities.

In the remaining columns of Table A14, we furthermore see that this finding is robust to different measures of candidate cleanliness—namely, when using $Z_{mc}$ or an indicator for whether $Z_{mc}$ is above the average in the municipality.

**Step 3. Estimating $E\left[Y \mid i = \textbf{clean}\right] - E\left[Y \mid i = \textbf{dirty}\right]$.** To estimate the second term in equation (4), we employ a close-election quasi-experiment in which the type of candidate is randomly varied, while the accountability channel—as well as the other mechanisms

described in equation (3)—is held constant.[28]

In practice, we implement a regression discontinuity design (RDD), using the margin of victory of the most successful clean candidate (as previously defined) as the running variable. This is done in elections where the two front-runners include one clean and one dirty candidate, which amounts to 311 municipalities in our sample. More specifically, we estimate regressions of the following form:

$$y_m = \alpha \mathbb{1}\{R_m \leq 0\} + f(R_m) + e_m \tag{5}$$

where $R_m$ is the vote margin for the clean candidate, and $f(R_m)$ is a polynomial of $R_m$ which varies above and below the cutoff of $R_m = 0$. This regression is estimated for observations that fall within a given bandwidth $h$ of the cutoff. In our main specifications we use the optimal bandwidth proposed by Calonico et al. (2019). We use a uniform kernel and HC3 standard errors to account for the relatively small sample used, but also report the p-value of the polynomial bias robust standard errors proposed by Calonico et al. (2014).

The key identification assumption underlying the interpretation of coefficient $\alpha$ as the causal effect of electing a clean candidate is the continuity of potential outcomes at the cutoff of the running variable. This assumption implies that the election of a clean candidate is as good as random in close races. We provide two main pieces of evidence in support of this assumption.[29] First, we demonstrate that the distribution of the running variable is continuous at the cutoff, suggesting the absence of manipulation in the vote share of clean candidates. Figure A7 in the Appendix presents the continuity test following Cattaneo et al. (2018). As shown in the figure, we fail to reject the null hypothesis of continuity at the threshold ($p$-value $\approx 0.92$).

Second, we find no evidence of any systematic discontinuity in municipal covariates at the cutoff. To assess this, we estimate equation (5) using the baseline (2018–2019) values of the public service index, along with other economic and political variables. Appendix Figure A8 presents the estimates of standardized covariates and their corresponding confidence

[28] While the RD estimate is, by design, a local effect to a particular set of municipalities (e.g. the ones with barely winning/loosing clean candidates), and is thus not representative of the more general effect of a clean candidate, this local effect is arguably the one of interest for this setting. The reason is that we are interested in knowing the effect of the intervention on the selection channel, which will arguably operate only in municipalities with competitive elections, where clean candidates were marginally elected due to the treatment.

[29] These two tests provide ex-post supporting evidence for the identification assumption. A further, ex-post, theoretical argument supporting the assumption is that even though in this context dirty candidates might manipulate elections, they cannot perfectly predict the non-manipulated voting counts, which means that the probability that they win is still as good as random when sufficiently close around the threshold, even though in the overall elections it is not.

intervals. Reassuringly, the point estimate for the public service index is very close to zero and statistically insignificant. Moreover, the estimates for the other covariates are also not statistically significant.

These findings lend support to the validity of the identification assumption underpinning the regression discontinuity design. We now turn to the estimation results for equation (5), using our main outcome of interest—the change in the Public Service Index over the relevant period.

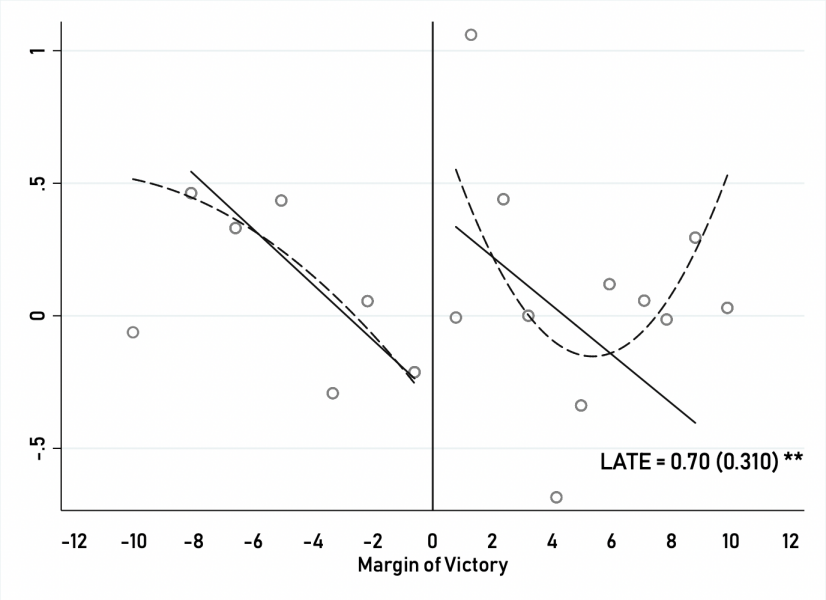
Figure 3 presents the main RDD results. Both linear and quadratic polynomials are fitted within their respective optimal bandwidths. The results consistently indicate that the election of a clean candidate leads to an increase in public service provision, regardless of the polynomial specification. The point estimate from the linear specification suggests that electing a clean candidate, as opposed to a dirty one, increases the public service index by a substantial 0.7 standard deviations ($p$-value $< 0.05$).

Table A15 presents the main point estimate, along with results from alternative specifications that vary the bandwidth by ±30% around the optimal value and employ both first- and second-order polynomials. Across all specifications, the estimated effects remain positive and statistically significant, reinforcing the robustness of the main findings. Notably, the primary estimate of 0.70 standard deviations lies within the range of the alternative estimates, which vary between 1.2 and 0.48 standard deviations, depending on the specification.

One concern with these estimates is that they are constructed using the experimental sample, part of which was directly affected by the intervention. This could potentially introduce noise into the estimates, since public service provision was itself affected by the intervention, and it could also mask potential heterogeneous effects in the close-election RD estimates between treatment and control municipalities. We address these concerns in two different ways. First, we control for treatment assignment in the baseline RD specification. Second, we re-estimate the RD models separately for the subsamples of control or treatment municipalities. These exercises are presented in Table A16. Reassuringly, the results suggest that RD estimates do not depend on the treatment status: the point estimates are very similar across restricted samples, although they lose significance given the loss of power, and are also unchanged when controlling for treatment assignment.

**Step 4. Estimating the overall selection channel.** Finally, we plug the estimates obtained in steps 2 and 3 above into equation (4) to get an estimate of the selection channel and its contribution to overall effect of the intervention. This suggests that the selection channel accounts for a change of $\approx 0.049$ ($0.073 \times 0.701$) standard deviations in the provision of public goods, which is equivalent to approximately 26% of the overall estimated effect in

Figure 3: RDD Estimating the Effect of Electing a Clean Candidate




**Notes:** This figure displays the binned average public service index according to the margin of victory of clean candidates. A linear and quadratic polynomials are displayed for observations in their respective bandwidths of, approximately, 7 and 13. The estimated effect displayed corresponds to the one obtained with the linear polynomial.

Table A1 (Column 1, Panel A).

As noted above, under the assumption that the intervention did not affect public service provision through alternative channels unrelated to politicians, the results suggest that the remaining 74% of the intervention's effect can be attributed to the *accountability channel*. However, this interpretation requires qualification due to potential measurement error in the candidate cleanliness proxy. If this is classical measurement error, the estimates obtained in steps 2 and 3 are likely to be attenuated. As a result, the contribution of the selection channel may be underestimated, leading to an overestimation of the accountability channel. Given these considerations, we interpret the reported estimates as a *lower* bound for the selection channel and an *upper* bound for the accountability channel.

**Selection and Other Candidate Characteristics.** In the previous exercise, we focused on examining how the intervention impacted the selection of clean mayors. However, "cleanliness" is only one of several mayoral characteristics that might have been affected by the intervention. In fact, some characteristics predictive of our outcomes of interest might be correlated with cleanliness. This point has been recently formalized by Marshall (2024) in the context of regression discontinuity designs studying the effect of barely elected candidates

with a certain characteristic of interest. The author argues that these designs risk bundling the attribute of interest with other potentially correlated characteristics, thereby limiting the interpretability of the results.

To further shed light on the interpretation of our results, we now assess whether a candidate's cleanliness is correlated with other observable characteristics that might themselves affect outcomes. To be sure, ex-ante, it is hard to theorize about the direction of these correlations. For instance, the literature has shown that candidates more likely to engage in clientelism may be more effective at delivering services due to their embeddedness in patronage networks (Kramon 2016), or that candidates with criminal backgrounds may exhibit distinct patterns of behavior once in office (Khemka 2023).

To systematically assess this possibility, we conduct two types of tests. First, we compile a rich set of candidate characteristics—both sociodemographic and political—using multiple sources. Most of the data come from administrative records of candidate registrations, while others are hand-coded. For example, we code candidates' education levels and prior private sector experience using their CVs, which are included in campaign manifestos.[30] Second, beyond examining statistical correlations, we test whether our intervention affected the characteristics of elected mayors, beyond their cleanliness, in the 2019 elections.

Appendix Table A17 presents a correlation matrix between two alternative measures of cleanliness and a large set of candidate characteristics, including demographic characteristics, education, private, public and political experience, as well as party affiliation. Surprisingly, the evidence indicates that correlations are extremely low ($\rho < 0.1$) and not statistically significant, with the exception of age, political experience, and affiliation with a historical party.[31] Specifically, cleaner candidates tend to be slightly younger, are less likely to have prior political experience, and are less likely to run under the ticket of a historical party. Notably, these traits are consistent with those documented in the literature on outsider candidates in Latin America (Serra 2018; Hollyer et al. 2022).

Appendix Table A18 additionally reports the estimated effect of our intervention on the same set of characteristics of elected mayors as before. This provides for another way of testing how the experiment influenced the selection of candidates with characteristics other than their cleanliness. The results suggest that the intervention did not affect the selection of mayors along any of these characteristics. In addition to the variables explored here—and crucial to our identification—in Garbiras-Díaz and Montenegro (2022) we also show that the intervention did not affect the likelihood of electing the most popular candidate, as measured

[30] Colombian law mandates candidates to publish these documents online on a designated website, from which we scrape the data.

[31] These are traditional parties founded in the 19th century, when Colombia gained independence—namely, the Liberal and Conservative Parties.

by our pre-treatment citizen survey conducted in 2019.

Altogether, these results suggest that, in this context, the selection effect is unlikely to be driven by the characteristics examined here. Importantly, this implies that selection of clean candidates affects the provision of public services through other channels than just through its correlation with these alternative characteristics.

# 6 Concluding Remarks

This paper provides novel experimental evidence on the causal relationship between electoral integrity and citizen welfare. By leveraging a randomized intervention that reduced electoral irregularities in Colombia's 2019 local mayoral elections, we demonstrate that improvements in electoral quality generate substantial benefits for public service provision.

We make several important contributions to the literature on democratic accountability. First, we provide the first experimental evidence on the causal effect of electoral integrity on public good provision—a key developmental outcome that directly affects citizen well-being. Second, we develop a novel decomposition methodology to quantify the relative contributions of the selection and accountability channels posited in the literature as mechanisms through which elections enhance societal welfare. Third, we show that improvements in electoral integrity translate into better governance practices, as reflected in more competitive procurement processes and a reduction in corruption-related judicial proceedings.

Our findings carry significant policy implications. Most importantly, they highlight the importance of investing in mechanisms that guarantee free and fair elections as a means to promote local development. They bridge a long-standing "big picture" perspective that emphasizes the role of institutional quality in generating development endogenously, with the view advocated, among others, by Duflo (2017), that relatively small but targeted interventions can unlock meaningful developmental gains. More concretely, the success of our low-cost social media intervention in producing substantial improvements suggests that citizen oversight mechanisms can be powerful tools for both strengthening democratic accountability and increasing downstream public good and service provision.

As democracies around the world face growing challenges to electoral quality—not only in the form of irregularities, but also in terms of institutional legitimacy and the rise of populism—our findings offer encouraging evidence that targeted interventions to strengthen democratic institutions can yield substantial social benefits and improve citizens' lives.

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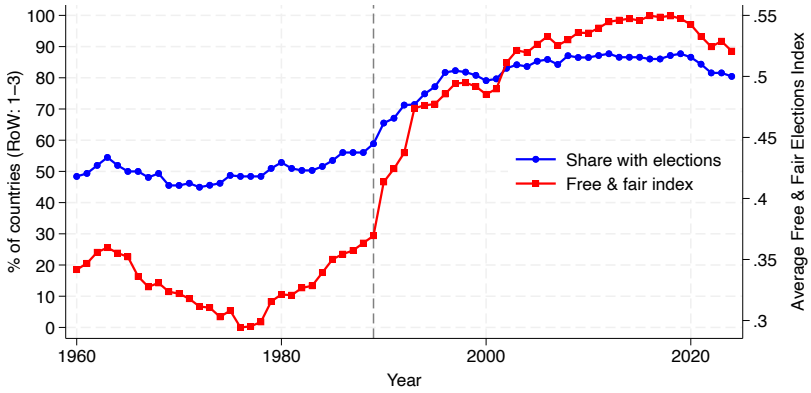
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# Supplementary Data and Appendix

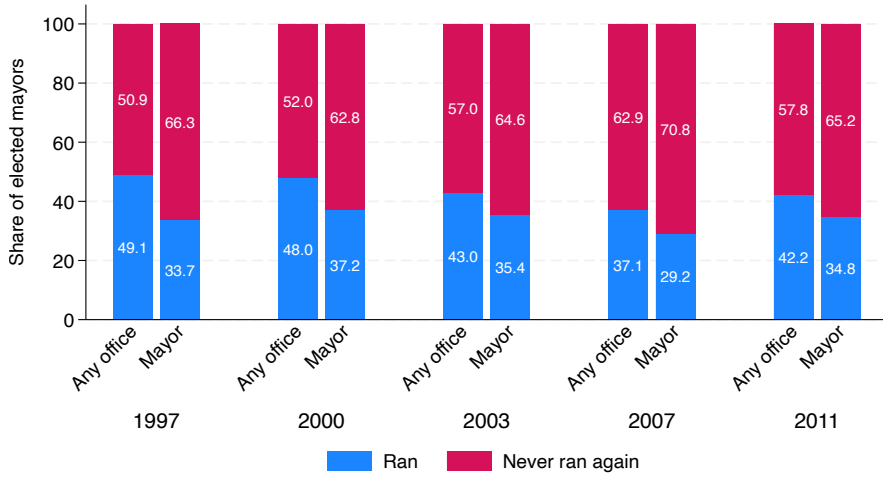
# A Context

Figure A1: Prevalence and Quality of Elections by Year (1960–present)




**Note:** The figure plots the share of countries holding national elections (left axis) and the average quality of elections (right axis), using data from the Varieties of Democracy Project (version 15). The share of countries with elections includes those classified as electoral autocracies or democracies (`v2x_regime` $\in \{1, 2, 3\}$). The quality of elections is measured by the Free and Fair Elections Index (`v2xel_frefair`).

Figure A2: Elected mayors' subsequent candidacies by election cohort




**Note:** This figure shows the percentage of mayors elected in a given year who later ran in subsequent elections. These subsequent elections include local and national-level contests—excluding presidential elections—held between 2000 and 2023. Local elections in Colombia, conducted every four years in odd-numbered years, include races for mayor, municipal council, governor, and departmental assembly. National elections, held every four years in even-numbered years, include contests for the Senate and the House of Representatives. The data were collected by the Universidad de los Andes using original records from Colombia's electoral office (*Registraduría Nacional del Estado Civil*). *Any office* indicates mayors who ran for any subsequent elected position, whereas *Mayor* includes only those who ran again for mayor in the same municipality in non-consecutive terms.

# B Descriptive Statistics

| Variable | Mean Pre-treatment (Control) | SD Pre-treatment (Control) |
|---|---|---|
| Water Urban | 88.47 | 17.54 |
| Sewerage Urban | 80.5 | 26.06 |
| Waste Collection Urban | 87.22 | 17.53 |
| Water Rural | 34.89 | 32.36 |
| Sewerage Rural | 14.39 | 23.02 |
| Waste Collection Rural | 19.42 | 26.99 |
| Log Mean Nightlights | -.85 | .78 |

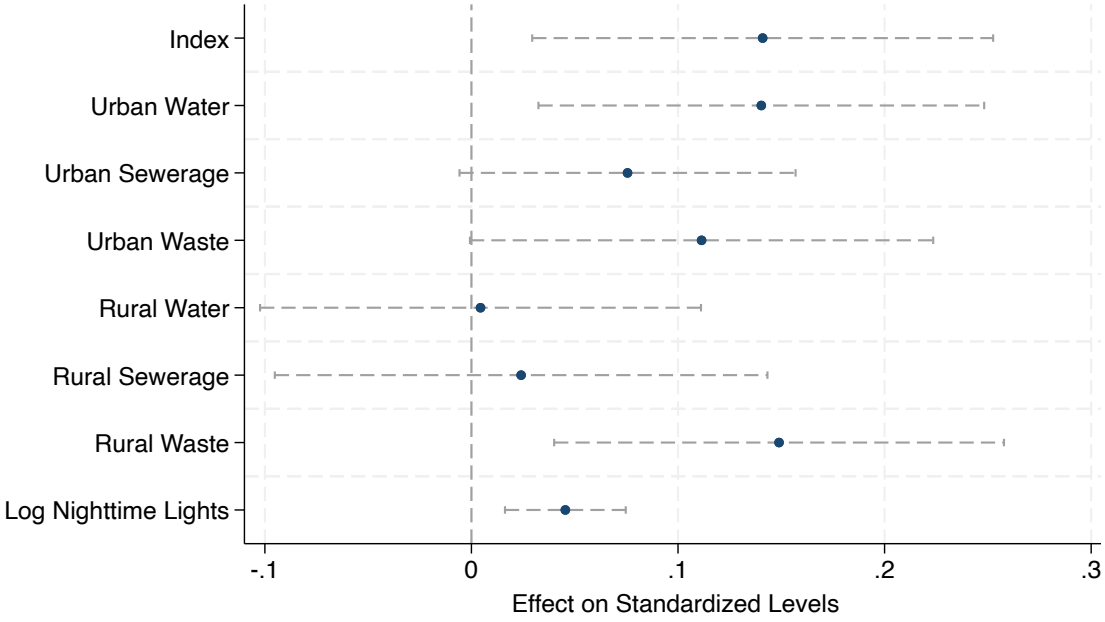
# C Additional Figures and Tables for Public Service Index

Table A1: Effects on Components of Public Service Index (Pre-Post)

| | Index | Water Urban | Sewerage Urban | Waste Urban | Water Rural | Sewerage Rural | Waste Rural | Log Nighttime Light |
|---|---|---|---|---|---|---|---|---|
| **Panel A. Pooled Treatment Effects** | | | | | | | | |
| [$T$] Any treatment | 0.190** | 0.165** | 0.134** | 0.111 | 0.001 | 0.062 | 0.145** | 0.040** |
| | (0.078) | (0.070) | (0.065) | (0.076) | (0.063) | (0.068) | (0.063) | (0.017) |
| | [0.013] | [0.007] | [0.060] | [0.147] | [0.990] | [0.390] | [0.013] | [0.037] |
| **Panel B. Letter vs. No Letter** | | | | | | | | |
| [$NL$] No Letter - Any Ad | 0.166* | 0.196** | 0.070 | 0.066 | -0.015 | 0.051 | 0.185** | 0.046 |
| | (0.097) | (0.087) | (0.081) | (0.087) | (0.079) | (0.085) | (0.076) | (0.028) |
| | [0.080] | [0.020] | [0.440] | [0.463] | [0.887] | [0.563] | [0.017] | [0.093] |
| [$L$] Letter - Any Ad | 0.202** | 0.149** | 0.168** | 0.134 | 0.009 | 0.067 | 0.124* | 0.038** |
| | (0.085) | (0.076) | (0.070) | (0.084) | (0.069) | (0.073) | (0.071) | (0.017) |
| | [0.007] | [0.037] | [0.017] | [0.127] | [0.893] | [0.383] | [0.067] | [0.020] |
| Test $NL = L$, p-value | 0.69 | 0.56 | 0.20 | 0.39 | 0.76 | 0.84 | 0.42 | 0.77 |
| Control Mean | 0.000 | -0.116 | 0.085 | -0.009 | 0.285 | 0.340 | 0.194 | 0.403 |
| Control PreSTD Mean | | 88.47 | 80.5 | 87.22 | 34.89 | 14.39 | 19.42 | -0.85 |
| Control PreSTD SD | | 17.54 | 26.06 | 17.53 | 32.36 | 23.02 | 26.99 | 0.78 |
| Sample Size | 697 | 698 | 697 | 697 | 698 | 697 | 697 | 698 |

**Notes:** This table presents the effect of the intervention on the change of index components. All specifications include the covariates selected using the method described in Chernozhukov et al. (2015) and Belloni et al. (2014). Robust standard errors are shown in parentheses and randomization inference p-values are shown in square brackets; *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

Figure A3: Effects on Public Service Index and its Components — Levels




**Note:** This figure presents the estimated effects of the intervention on the components of the public service index, measured in levels. These results correspond to Table A2. All variables have been standardized as described in Section 3.1.

Table A2: Effects on Components of Public Service Index (Levels)

| | Index | Water Urban | Sewerage Urban | Waste Urban | Water Rural | Sewerage Rural | Waste Rural | Log Nighttime Light |
|---|---|---|---|---|---|---|---|---|
| **Panel A. Pooled Treatment Effects** | | | | | | | | |
| [$T$] Any treatment | 0.141** | 2.461** | 2.011* | 1.953* | 0.142 | 0.554 | 4.045*** | 0.036*** |
| | (0.057) | (0.964) | (1.077) | (1.001) | (1.759) | (1.398) | (1.507) | (0.012) |
| | [0.020] | [0.013] | [0.060] | [0.043] | [0.957] | [0.710] | [0.003] | [0.000] |
| **Panel B. Letter vs. No Letter** | | | | | | | | |
| [$NL$] No Letter - Any Ad | 0.093 | 2.328** | 0.721 | 1.108 | -0.152 | -0.155 | 4.430** | 0.035* |
| | (0.073) | (1.180) | (1.418) | (1.250) | (2.235) | (1.799) | (1.885) | (0.018) |
| | [0.223] | [0.083] | [0.630] | [0.357] | [0.940] | [0.920] | [0.020] | [0.023] |
| [$L$] Letter - Any Ad | 0.166*** | 2.529** | 2.675** | 2.391** | 0.294 | 0.913 | 3.846** | 0.036*** |
| | (0.062) | (1.027) | (1.153) | (1.056) | (1.922) | (1.522) | (1.682) | (0.012) |
| | [0.010] | [0.010] | [0.007] | [0.023] | [0.907] | [0.563] | [0.033] | [0.000] |
| Test $NL = L$, p-value | 0.29 | 0.85 | 0.14 | 0.24 | 0.84 | 0.54 | 0.76 | 0.96 |
| Control Mean | 0.000 | 86.438 | 82.711 | 87.062 | 44.111 | 22.216 | 24.657 | -0.533 |
| Sample Size | 697 | 698 | 697 | 697 | 698 | 697 | 697 | 698 |

**Notes:** This table presents the effects of the intervention on the components of the Public Service Index, measured in levels. All specifications include the covariates selected using the method described in Chernozhukov et al. (2015) and Belloni et al. (2014). Robust standard errors are shown in parentheses, and randomization inference p-values are shown in square brackets. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

Table A3: Effects on Access to Services Using Household Surveys

| | Index | $PCA_1$ | Sewerage | Waste Collection | Water |
|---|---|---|---|---|---|
| **Panel A. Pooled Treatment** | | | | | |
| Any treatment | 0.080*** | 0.079*** | 0.016 | 0.042*** | 0.036** |
| | (0.03) | (0.03) | (0.02) | (0.01) | (0.02) |
| **Panel B. Subtreatments by Letter - No Letter** | | | | | |
| No Letter - Any Ad | 0.051 | 0.049 | 0.002 | 0.023 | 0.031 |
| | (0.04) | (0.04) | (0.02) | (0.02) | (0.02) |
| Letter - Any Ad | 0.092*** | 0.092*** | 0.022 | 0.052*** | 0.038** |
| | (0.03) | (0.03) | (0.02) | (0.02) | (0.02) |
| Control Mean | 0.00 | 0.00 | 0.40 | 0.54 | 0.69 |
| Sample Size | 103,040 | 103,040 | 103,040 | 103,040 | 103,040 |
| Municipalities | 469 | 469 | 469 | 469 | 469 |

**Note:** Estimates are based on the ECV for 2022 and 2023. All variables are standardized. `Index` corresponds to an additive index of the standardized values for access to sewerage, waste collection, and aqueduct. $PCA_1$ corresponds to the first principal component from a principal components analysis (PCA) of the same three variables. Columns (3)–(5) correspond to responses to the question "Which of the following utilities does the household have?" (2) Sewerage: 1 = "Yes"; (3) Waste collection: 1 = "Yes"; (4) Water: 1 = "Yes". All estimates use sampling weights provided by DANE. All models include double-post-lasso selected controls. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

Table A4: Effects on GDP and Components of Nighttime Lights

| | Log GDP (1) | Residual Log Nighttime Lights (2) | Predicted Log Nighttime Lights By GDP (3) |
|---|---|---|---|
| **Panel A. Pooled Treatment Effects** | | | |
| [$T$] Any treatment | -0.015 | 0.033*** | -0.006 |
| | (0.016) | (0.013) | (0.006) |
| | [0.317] | [0.013] | [0.347] |
| **Panel B. Letter vs. No Letter** | | | |
| [$NL$] No Letter - Any Ad | -0.027 | 0.037** | -0.011 |
| | (0.021) | (0.018) | (0.008) |
| | [0.183] | [0.030] | [0.170] |
| [$L$] Letter - Any Ad | -0.008 | 0.031** | -0.003 |
| | (0.017) | (0.013) | (0.007) |
| | [0.637] | [0.023] | [0.677] |
| Test $NL = L$, p-value | 0.30 | 0.76 | 0.30 |
| Control Mean | 6.01 | -0.06 | -0.47 |
| Sample Size | 698 | 698 | 698 |

**Notes:** This table presents the effects of the intervention on the GDP of municipalities, as well as the components of the night time lights predicted and non-predicted by GDP. The outcome in column (1) is the (log) average GDP in 2022 and 2023. In column (2) it is the (log) night time light average in 2022 and 2023 once it has been residualized in a linear regression from the (log) average GDP in 2022 and 2023. In column (3) it is the (log) night time light average in 2022 and 2023 predicted in a linear regression using the (log) average GDP in 2022 and 2023 as an explanatory variable. All specifications include the covariates selected using the method described in Chernozhukov et al. (2015) and Belloni et al. (2014). Robust standard errors are shown in parentheses, and randomization inference p-values are shown in square brackets. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

Figure A4: Effects on Index, by Year (Pre-Post)

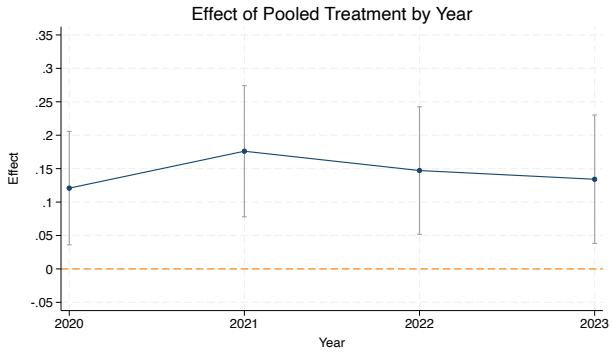



Figure A5: Index all components

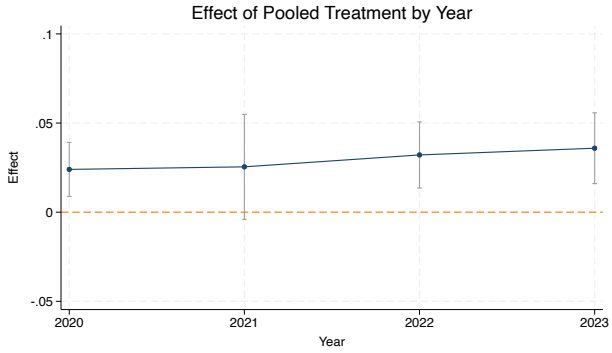



Figure A6: (Log) Nighttime lights

# D Mediating Variables and Alternative Explanations

Table A5: Effects on Procurement Outcomes

| | Log Value Total Contracts 2020-2023 | Log Value Competitive Contracts 2020-2023 | % Value Competitive Contracts 2020-2023 | Log Value Direct Contracts 2020-2023 | % Value Direct Contracts 2020-2023 |
|---|---|---|---|---|---|
| **Pooled Treatment Effects** | | | | | |
| [$T$] Any treatment | 0.057 | 0.180** | 2.264* | -0.033 | -3.119** |
| | (0.057) | (0.074) | (1.292) | (0.056) | (1.314) |
| | [0.367] | [0.020] | [0.063] | [0.530] | [0.030] |
| **Letter vs. No Letter** | | | | | |
| [$NL$] No Letter - Any Ad | 0.062 | 0.186** | 1.062 | 0.010 | -2.422 |
| | (0.066) | (0.087) | (1.657) | (0.064) | (1.617) |
| | [0.353] | [0.033] | [0.497] | [0.883] | [0.167] |
| [$L$] Letter - Any Ad | 0.054 | 0.177** | 2.886** | -0.054 | -3.482** |
| | (0.063) | (0.081) | (1.400) | (0.064) | (1.441) |
| | [0.343] | [0.023] | [0.053] | [0.433] | [0.023] |
| Test $NL = L$, p-value | 0.90 | 0.91 | 0.25 | 0.32 | 0.49 |
| Control Mean | 24.60 | 23.64 | 43.59 | 23.64 | 42.18 |
| Sample Size | 698 | 692 | 696 | 697 | 696 |

Table A6: Effects on Judicial Processes Against Mayors' Offices

| | Intensive Margin | | | Extensive Margin | | |
|---|---|---|---|---|---|---|
| | All Processes | Corruption | Non-corruption | All Processes | Corruption | Non-corruption |
| **Panel A. Pooled Treatment Effects** | | | | | | |
| [$T$] Any treatment | -0.427 | -0.473** | 0.035 | 0.028 | -0.014 | 0.019 |
| | (0.261) | (0.215) | (0.082) | (0.037) | (0.038) | (0.034) |
| | [0.080] | [0.017] | [0.740] | [0.457] | [0.727] | [0.600] |
| **Panel B. Letter vs. No Letter** | | | | | | |
| [$NL$] No Letter - Any Ad | -0.656** | -0.607*** | -0.032 | 0.019 | -0.009 | -0.002 |
| | (0.279) | (0.233) | (0.086) | (0.047) | (0.048) | (0.042) |
| | [0.027] | [0.010] | [0.763] | [0.660] | [0.813] | [1.000] |
| [$L$] Letter - Any Ad | -0.308 | -0.404* | 0.069 | 0.033 | -0.016 | 0.030 |
| | (0.295) | (0.238) | (0.099) | (0.040) | (0.041) | (0.037) |
| | [0.293] | [0.073] | [0.557] | [0.397] | [0.667] | [0.450] |
| Test $NL = L$, p-value | 0.19 | 0.32 | 0.32 | 0.76 | 0.87 | 0.42 |
| Control Mean | 2.218 | 1.844 | 0.373 | 0.538 | 0.511 | 0.213 |
| Sample Size | 698 | 698 | 698 | 698 | 698 | 698 |

## Table A7: Local Public Revenue

| | Total Revenue (log) | Taxes (log) | Transfers (log) | Other Sources of Revenue (log) |
|---|---|---|---|---|
| **Panel A. Pooled Treatment Effects** | | | | |
| [$T$] Any treatment | 0.019 | -0.020 | -0.017 | 0.029 |
| | (0.017) | (0.025) | (0.018) | (0.040) |
| | [0.223] | [0.433] | [0.367] | [0.500] |
| **Panel B. Letter vs. No Letter** | | | | |
| [$NL$] No Letter - Any Ad | 0.006 | -0.016 | -0.002 | -0.028 |
| | (0.021) | (0.030) | (0.023) | (0.051) |
| | [0.787] | [0.607] | [0.890] | [0.607] |
| [$L$] Letter - Any Ad | 0.025 | -0.021 | -0.024 | 0.058 |
| | (0.019) | (0.027) | (0.020) | (0.045) |
| | [0.147] | [0.400] | [0.200] | [0.233] |
| Test $NL = L$, p-value | 0.30 | 0.86 | 0.31 | 0.09 |
| Control Mean | 25.664 | 23.610 | 25.172 | 23.946 |
| Sample Size | 673 | 673 | 673 | 672 |

## Table A8: Effects on Education

| | Facilities | | Teachers | | Students | | Admin. Staff | |
|---|---|---|---|---|---|---|---|---|
| | All | Public | All | Public | All | Public | All | Public |
| **Panel A. Pooled Treatment Effects** | | | | | | | | |
| [$T$] Any treatment | -0.005 | -0.007 | -0.003 | -0.003 | -0.003 | -0.002 | -0.001 | -0.018 |
| | (0.006) | (0.005) | (0.006) | (0.006) | (0.008) | (0.008) | (0.017) | (0.017) |
| | [0.350] | [0.217] | [0.597] | [0.613] | [0.747] | [0.830] | [0.957] | [0.263] |
| **Panel B. Letter vs. No Letter** | | | | | | | | |
| [$NL$] No Letter - Any Ad | -0.011 | -0.010 | -0.002 | -0.000 | 0.003 | 0.003 | -0.003 | -0.015 |
| | (0.008) | (0.008) | (0.007) | (0.007) | (0.010) | (0.011) | (0.020) | (0.020) |
| | [0.153] | [0.143] | [0.713] | [0.927] | [0.740] | [0.730] | [0.890] | [0.487] |
| [$L$] Letter - Any Ad | -0.002 | -0.006 | -0.004 | -0.004 | -0.005 | -0.005 | 0.000 | -0.019 |
| | (0.006) | (0.006) | (0.007) | (0.007) | (0.008) | (0.008) | (0.019) | (0.018) |
| | [0.750] | [0.297] | [0.657] | [0.523] | [0.500] | [0.577] | [0.987] | [0.293] |
| Test $NL = L$, p-value | 0.26 | 0.55 | 0.85 | 0.57 | 0.41 | 0.42 | 0.87 | 0.83 |
| Control Mean | 3.69 | 3.60 | 5.37 | 5.27 | 8.06 | 8.00 | 3.77 | 3.58 |
| Sample Size | 698 | 698 | 698 | 698 | 698 | 698 | 698 | 698 |

**Notes:** This table presents the effects of the intervention on education outcomes. All outcomes are log-transformed. All specifications include covariates selected using the method described in Chernozhukov et al. (2015) and Belloni et al. (2014), as well as the lagged outcome variable measured in 2019. Robust standard errors are reported in parentheses, and randomization inference p-values are reported in square brackets. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

## Table A9: Effects on Healthcare

| | Facilities per 10k citizens | Physicians per 10k citizens | Nurses and assistants per 10k citizens | Stretchers per 10k citizens | Beds per 10k citizens | Ambulances per 10k citizens | Offices per 10k citizens | Rooms per 10k citizens |
|---|---|---|---|---|---|---|---|---|
| **Panel A. Pooled Treatment Effects** | | | | | | | | |
| [$T$] Any treatment | 0.018 | 0.175 | 1.646 | -0.169 | -0.105 | 0.006 | 0.195 | -0.032 |
| | (0.024) | (0.779) | (4.470) | (0.185) | (0.494) | (0.066) | (0.264) | (0.070) |
| | [0.637] | [0.883] | [0.843] | [0.383] | [0.830] | [0.907] | [0.513] | [0.667] |
| **Panel B. Letter vs. No Letter** | | | | | | | | |
| [$NL$] No Letter - Any Ad | 0.007 | 0.828 | 3.350 | 0.044 | 1.134 | -0.005 | 0.222 | -0.010 |
| | (0.024) | (1.035) | (3.935) | (0.260) | (0.921) | (0.087) | (0.355) | (0.092) |
| | [0.777] | [0.403] | [0.420] | [0.843] | [0.217] | [0.953] | [0.510] | [0.883] |
| [$L$] Letter - Any Ad | 0.024 | -0.162 | 0.767 | -0.279 | -0.744 | 0.012 | 0.182 | -0.043 |
| | (0.031) | (0.975) | (5.985) | (0.198) | (0.468) | (0.072) | (0.290) | (0.076) |
| | [0.547] | [0.920] | [0.953] | [0.163] | [0.080] | [0.870] | [0.577] | [0.540] |
| Test $NL = L$, p-value | 0.63 | 0.45 | 0.68 | 0.21 | 0.04 | 0.85 | 0.91 | 0.71 |
| Control Mean | 0.058 | 7.584 | 41.019 | 3.924 | 5.933 | 1.543 | 6.398 | 1.313 |
| Sample Size | 698 | 698 | 698 | 698 | 698 | 698 | 698 | 698 |

**Notes:** This table presents the effects of the intervention on health provison outcomes. All outcomes come from official statistics published by Colombia's Ministry of Health. Outcomes in columns (1)-(3) are measured in 2023, while the remaining ones are measured in 2026. All specifications include covariates selected using the method described in Chernozhukov et al. (2015) and Belloni et al. (2014). Robust standard errors are reported in parentheses, and randomization inference p-values are reported in square brackets. *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

## Table A10: Effects on Protests

| | | | | Only Municipal Protests | | | |
|---|---|---|---|---|---|---|---|
| | | By Geography | | By Reason | | By Adversary | |
| | All Protests (per 10K pop.) | Municipality | Rest | Policies and Public SS | Rest | Executive Govmt. | Rest |
| **Panel A. Pooled Treatment Effects** | | | | | | | |
| [$T$] Any treatment | -0.090 | -0.024 | -0.046 | -0.030 | 0.005 | -0.025 | 0.001 |
| | (0.122) | (0.019) | (0.120) | (0.019) | (0.015) | (0.016) | (0.010) |
| | [0.440] | [0.183] | [0.730] | [0.137] | [0.780] | [0.093] | [0.927] |
| **Panel B. Letter vs. No Letter** | | | | | | | |
| [$NL$] No Letter - Any Ad | 0.048 | -0.008 | 0.116 | -0.006 | -0.003 | -0.014 | 0.006 |
| | (0.161) | (0.027) | (0.153) | (0.033) | (0.023) | (0.021) | (0.014) |
| | [0.773] | [0.753] | [0.407] | [0.900] | [0.867] | [0.523] | [0.713] |
| [$L$] Letter - Any Ad | -0.161 | -0.033* | -0.130 | -0.042** | 0.009 | -0.031* | -0.002 |
| | (0.129) | (0.019) | (0.128) | (0.017) | (0.015) | (0.016) | (0.010) |
| | [0.180] | [0.057] | [0.320] | [0.000] | [0.517] | [0.053] | [0.857] |
| Test $NL = L$, p-value | 0.16 | 0.29 | 0.08 | 0.23 | 0.56 | 0.34 | 0.56 |
| Control Mean | 1.544 | 0.083 | 1.461 | 0.062 | 0.021 | 0.059 | 0.024 |
| Sample Size | 698 | 698 | 698 | 698 | 698 | 698 | 698 |

Table A11: Effects on Citizen Writs (*Tutelas*)

| | All Writs (per 1K pop.) | By Right Being Claimed (per 1K pop.) | | | | | |
|---|---|---|---|---|---|---|---|
| | | Public Works | Social Services | Health | FOIA | Justice | Labor |
| **Panel A. Pooled Treatment Effects** | | | | | | | |
| [$T$] Any treatment | -0.019 | 0.004 | -0.024** | 0.006 | 0.008 | -0.020 | -0.001 |
| | (0.102) | (0.003) | (0.011) | (0.041) | (0.038) | (0.013) | (0.003) |
| | [0.870] | [0.363] | [0.017] | [0.927] | [0.857] | [0.200] | [0.617] |
| **Panel B. Letter vs. No Letter** | | | | | | | |
| [$NL$] No Letter - Any Ad | 0.034 | 0.010 | -0.018 | 0.048 | 0.022 | -0.027* | -0.001 |
| | (0.131) | (0.007) | (0.014) | (0.043) | (0.045) | (0.015) | (0.003) |
| | [0.770] | [0.127] | [0.200] | [0.360] | [0.630] | [0.087] | [0.797] |
| [$L$] Letter - Any Ad | -0.046 | 0.002 | -0.027** | -0.016 | 0.001 | -0.016 | -0.001 |
| | (0.108) | (0.003) | (0.011) | (0.046) | (0.044) | (0.016) | (0.003) |
| | [0.690] | [0.603] | [0.010] | [0.717] | [0.977] | [0.353] | [0.627] |
| Test $NL = L$, p-value | 0.50 | 0.25 | 0.42 | 0.09 | 0.68 | 0.59 | 0.91 |
| Control Mean | 2.24 | 0.03 | 0.16 | 0.74 | 0.62 | 0.23 | 0.05 |
| Sample Size | 698 | 698 | 698 | 698 | 698 | 698 | 698 |

Table A12: Effects on Grassroot Citizen Associations

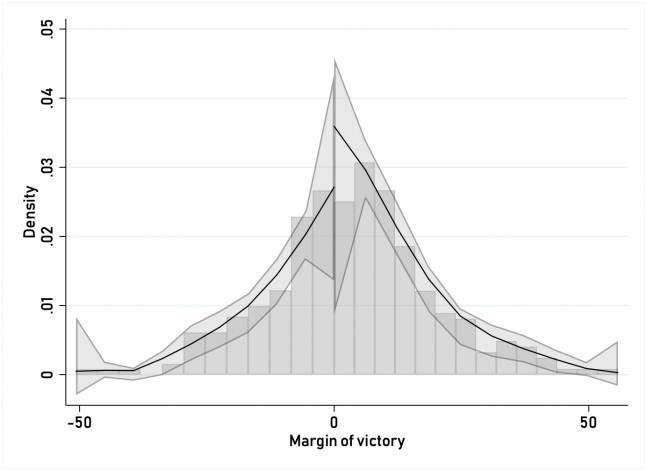
| | Number of All Associations | Number of JACs | Members of All Associations (% population) | Members JACs (% population) |
|---|---|---|---|---|
| **Panel A. Pooled Treatment Effects** | | | | |
| [$T$] Any treatment | 0.346 | 0.496 | 0.764 | 0.554 |
| | (2.139) | (2.059) | (0.736) | (0.713) |
| | [0.860] | [0.843] | [0.317] | [0.447] |
| **Panel B. Letter vs. No Letter** | | | | |
| [$NL$] No Letter - Any Ad | -0.258 | -0.055 | 0.755 | 0.474 |
| | (2.578) | (2.493) | (0.932) | (0.905) |
| | [0.923] | [0.983] | [0.400] | [0.593] |
| [$L$] Letter - Any Ad | 0.660 | 0.781 | 0.769 | 0.596 |
| | (2.364) | (2.277) | (0.823) | (0.790) |
| | [0.787] | [0.673] | [0.373] | [0.497] |
| Test $NL = L$, p-value | 0.71 | 0.73 | 0.99 | 0.89 |
| Control Mean | 37.471 | 36.044 | 12.463 | 12.155 |
| Sample Size | 698 | 698 | 698 | 698 |

Table A13: Effects on Active Social Media Users

| | Log Number of Meta Users | Percent Meta Users in Population | Log Number of Meta Users - Interested in Political and Social Issues | Percent Meta Users in Population - Interested in Political and Social Issues |
|---|---|---|---|---|
| **Panel A. Pooled Treatment Effects** | | | | |
| [$T$] Any treatment | -0.033 | -0.615 | -0.030 | 0.077 |
| | (0.036) | (2.421) | (0.033) | (1.015) |
| | [0.387] | [0.783] | [0.333] | [0.950] |
| **Panel B. Letter vs. No Letter** | | | | |
| [$NL$] No Letter - Any Ad | -0.014 | -1.431 | -0.001 | -0.169 |
| | (0.042) | (2.717) | (0.039) | (1.158) |
| | [0.737] | [0.600] | [0.990] | [0.887] |
| [$L$] Letter - Any Ad | -0.043 | -0.199 | -0.045 | 0.202 |
| | (0.040) | (2.737) | (0.036) | (1.142) |
| | [0.237] | [0.937] | [0.210] | [0.887] |
| Test $NL = L$, p-value | 0.47 | 0.64 | 0.24 | 0.74 |
| Control Mean | 9.589 | 77.256 | 8.690 | 31.985 |
| Sample Size | 698 | 698 | 698 | 698 |

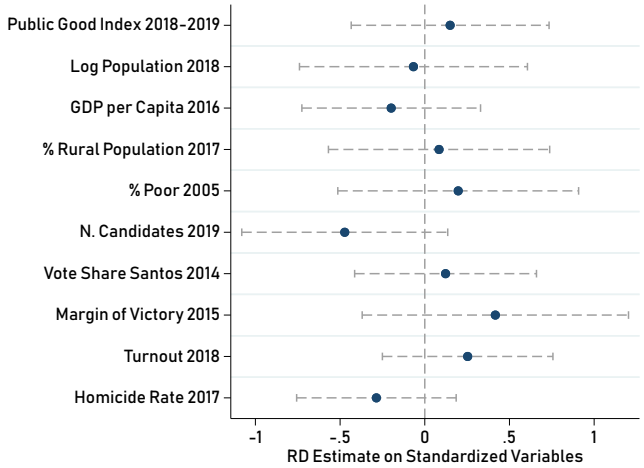
# E Decomposition

Figure A7: RD Manipulation Test




**Notes:** This figure displays the test for the continuity of distribution of the running variable in equation (5) proposed by Cattaneo et al. (2018).

Figure A8: RD Continuity of Covariates at Threshold




**Notes:** This figure displays the estimates of equation (5) using different covariates as outcome variables.

Table A14: Effects on Politicians' Type

| | Above Average Cleanliness Index (=1) | Above Average Will Perform Irregularities (% Responses) | Will Perform Irregularities (% Responses) |
|---|---|---|---|
| **Panel A. Pooled Treatment Effect** | | | |
| [$T$] Any treatment | 0.073* | 0.080* | -0.045* |
| | (0.040) | (0.041) | (0.024) |
| | [0.070] | [0.067] | [0.073] |
| **Panel B. Subtreatments by Letter - No Letter** | | | |
| [$NL$] No Letter - Any Ad | 0.051 | 0.067 | -0.035 |
| | (0.052) | (0.053) | (0.030) |
| | [0.347] | [0.187] | [0.237] |
| [$L$] Letter - Any Ad | 0.084* | 0.086* | -0.049* |
| | (0.044) | (0.044) | (0.025) |
| | [0.067] | [0.067] | [0.063] |
| Test $NL = L$, p-value | 0.51 | 0.71 | 0.62 |
| Control Mean | 0.31 | 0.31 | 0.62 |
| Sample Size | 595 | 592 | 620 |

**Notes:** This table presents the effect of the intervention on different measures of the elected mayors' types. The outcome in column (1) is an indicator of whether the elected mayor had a score above the average in her respective municipality on the cleanliness index described in the main text. In column (2) it is an indicator for whether $Z_{mc}$, described in the main text, is above average. In column (3), it is $Z_{mc}$. All specifications include the covariates selected using the method described in Chernozhukov et al. (2015) and Belloni et al. (2014). Robust standard errors are shown in parentheses and randomization inference p-values are shown in square brackets; *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

Table A15: RD Estimates with Different Bandwidths and Polynomials

| | Change in Index | | |
|---|---|---|---|
| **Panel A. First Order Polynomial** | | | |
| RD Estimate | 0.701** | 1.006*** | 0.482* |
| | (0.310) | (0.381) | (0.272) |
| Bandwidth selection | Optimal | 0.7 × Optimal | 1.3 × Optimal |
| Bandwidth | 8.82 | 6.18 | 11.47 |
| Conventional p-value | 0.02 | 0.01 | 0.08 |
| Bias-Robust p-value | 0.02 | 0.20 | 0.01 |
| Sample Size | 144 | 102 | 176 |
| **Panel B. Second Order Polynomial** | | | |
| RD Estimate | 0.952** | 1.209** | 0.905** |
| | (0.403) | (0.543) | (0.365) |
| Bandwidth selection | Optimal | 0.7 × Optimal | 1.3 × Optimal |
| Bandwidth | 11.89 | 8.32 | 15.46 |
| Conventional p-value | 0.02 | 0.03 | 0.01 |
| Bias-Robust p-value | 0.02 | 0.48 | 0.02 |
| Sample Size | 182 | 136 | 210 |

**Notes:** This table presents the point estimates of equation (5) using different bandwidths and polynomials on the change of the public service index. HC3 standard errors are shown in parentheses; *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

Table A16: RD Robustness to Experimental Treatment Arms

| | Original Estimate (1) | Controlling For Treatment Dummy (2) | Only Control Group (3) | Only Treated Group (4) |
|---|---|---|---|---|
| RD Estimate | 0.701** | 0.693** | 0.751 | 0.694 |
| | (0.310) | (0.310) | (0.527) | (0.461) |
| Bandwidth selection | Optimal | Optimal | Optimal | Optimal |
| Bandwidth | 8.82 | 8.82 | 8.82 | 8.82 |
| Conventional p-value | 0.02 | 0.03 | 0.15 | 0.13 |
| Bias-Robust p-value | 0.02 | 0.03 | 0.36 | 0.10 |
| Sample Size | 144 | 144 | 53 | 91 |

**Notes:** This table presents the robustness of the RD estimates to the different experimental treatment arms. Column (1) presents the main estimate from Table A15. Column (2) controls for a treatment indicator. Columns (3) and (4) display the estimated effect only using the control and the pooled treatment group samples, respectively. HC3 standard errors are shown in parentheses; *** p-value < 0.01, ** p-value < 0.05, * p-value < 0.1.

Table A17: Correlation Matrix of Candidate Characteristics

| | Log Age | Female (=1) | Undergraduate or More (=1) | Business Owner (=1) | Any Public Sector Experience (=1) | Any Private Sector Experience (=1) | Any Political Experience (=1) |
|---|---|---|---|---|---|---|---|
| $\rho$ - Above Average Clean. Index (=1) | -0.099** | 0.035 | -0.036 | -0.031 | -0.051 | 0.038 | -0.086** |
| | [0.015] | [0.410] | [0.470] | [0.450] | [0.325] | [0.451] | [0.034] |
| $\rho$ - Will Perform Irregularities (% Responses) | 0.051 | -0.010 | 0.004 | -0.008 | -0.008 | 0.004 | 0.042 |
| | [0.208] | [0.799] | [0.932] | [0.845] | [0.868] | [0.928] | [0.294] |

| | N. Political Posts | Incumbent (=1) | Incumbent Loose Def. (=1) | Coalition of Parties (=1) | Independent Candidate (=1) | Historical Party (=1) | Institutional Left Party (=1) |
|---|---|---|---|---|---|---|---|
| $\rho$ - Above Average Clean. Index (=1) | -0.047 | -0.006 | -0.004 | 0.027 | -0.054 | 0.069 | 0.047 |
| | [0.252] | [0.877] | [0.923] | [0.518] | [0.144] | [0.104] | [0.275] |
| $\rho$ - Will Perform Irregularities (% Responses) | 0.046 | -0.044 | -0.016 | -0.006 | 0.045 | -0.091** | -0.012 |
| | [0.258] | [0.272] | [0.691] | [0.890] | [0.261] | [0.023] | [0.761] |

Table A18: Effects on Characteristics of Elected Mayor: Socio-demographics and Experience

| | Log Age | Female (=1) | Undergraduate or More (=1) | Any Political Experience (=1) | N. Political Posts | Business Owner (=1) | Any Public Sector Experience (=1) | Any Private Sector Experience (=1) |
|---|---|---|---|---|---|---|---|---|
| **Pooled Treatment Effects** | | | | | | | | |
| [$T$] Any treatment | -0.019 | -0.010 | 0.076 | -0.067 | -0.039 | -0.032 | -0.039 | 0.003 |
| | (0.016) | (0.026) | (0.049) | (0.076) | (0.040) | (0.040) | (0.030) | (0.050) |
| | [0.253] | [0.723] | [0.127] | [0.360] | [0.330] | [0.413] | [0.217] | [0.967] |
| **Letter vs. No Letter** | | | | | | | | |
| [$NL$] No Letter - Any Ad | -0.025 | -0.012 | 0.019 | -0.070 | -0.058 | -0.039 | -0.026 | 0.008 |
| | (0.021) | (0.033) | (0.062) | (0.098) | (0.051) | (0.051) | (0.039) | (0.063) |
| | [0.247] | [0.793] | [0.743] | [0.533] | [0.260] | [0.460] | [0.540] | [0.880] |
| [$L$] Letter - Any Ad | -0.016 | -0.009 | 0.106** | -0.065 | -0.030 | -0.028 | -0.046 | 0.001 |
| | (0.017) | (0.029) | (0.053) | (0.081) | (0.044) | (0.043) | (0.034) | (0.054) |
| | [0.357] | [0.813] | [0.030] | [0.453] | [0.540] | [0.523] | [0.203] | [1.000] |
| Test $NL = L$, p-value | 0.64 | 0.93 | 0.13 | 0.96 | 0.55 | 0.81 | 0.61 | 0.90 |
| Control Mean | 3.78 | 0.12 | 0.47 | 0.72 | 0.46 | 0.43 | 0.91 | 0.47 |
| Sample Size | 697 | 697 | 462 | 698 | 698 | 695 | 462 | 462 |