

Birds of a Feather Collude Together: Subnational Alignment and Corruption

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Birds of a Feather Collude Together: Subnational Alignment and Corruption*

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Abstract

We examine how subnational partisan alignment influences corruption in clientelistic environments, focusing on the fabrication of “ghost” students to inflate education transfers to local governments in Colombia. Using a Regression Discontinuity Design, we find that partisan alignment between municipal mayors and departmental governors increases ghost students by 0.3 standard deviations, without improving genuine enrollment or student performance. Alignment also leads to more discretionary hiring, patronage-based outsourcing, and increased electoral fraud risk. The effects are strongest in municipalities with weaker institutions and entrenched clientelism. Alignment also raises the likelihood that mayors’ relatives are appointed to departmental posts and governors’ relatives to municipal posts, consistent with reciprocal patronage. These findings support the view that resource diversion benefits politicians with few benefits for local constituencies. Aligned politicians also experience better future electoral prospects, suggesting a breakdown in accountability. Our results highlight how clientelistic networks distort public service delivery, reinforcing the persistence of political corruption.

Keywords: Education, political agency, corruption, clientelism.

JEL: D7, H5, H7, I2.

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Aves del mismo plumaje coluden juntas: Alineamiento subnacional y corrupción*

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Abstract

Analizamos cómo el alineamiento partidista subnacional influye en la corrupción en contextos clientelistas, enfocándonos en la falsificación de estudiantes “fantasma” para inflar las transferencias educativas a los gobiernos locales en Colombia. Utilizando un diseño de regresión discontinua, encontramos que el alineamiento partidista entre alcaldes municipales y gobernadores departamentales incrementa los estudiantes fantasma en 0.3 desviaciones estándar, sin mejorar la matrícula real ni el desempeño académico. El alineamiento también conduce a una mayor contratación discrecional, tercerización basada en patronazgo y un aumento en el riesgo de fraude electoral. Los efectos son más fuertes en municipios con instituciones débiles y clientelismo arraigado. Asimismo, el alineamiento aumenta la probabilidad de que familiares de los alcaldes sean nombrados en cargos departamentales y que familiares de los gobernadores sean nombrados en cargos municipales, en consistencia con el patronazgo recíproco. Estos hallazgos respaldan la idea de que la desviación de recursos beneficia a los políticos, con escasos beneficios para las comunidades locales. Los políticos alineados también muestran mejores perspectivas electorales futuras, lo que sugiere una falla en la rendición de cuentas. Nuestros resultados resaltan cómo las redes clientelistas distorsionan la prestación de servicios públicos y refuerzan la persistencia de la corrupción política.

Palabras clave: Educación, agencia política, corrupción, clientelismo.

JEL: D7, H5, H7, I2.

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1 Introduction

One of democracy’s core promises is that electoral incentives discipline politicians. Classical political agency models predict that elections curb corruption by enabling voters to punish misbehavior (Barro 1973; Ferejohn 1986; Austen-Smith and Banks 1989). However, this ideal might falter for various reasons. Citizens may prioritize ethnic, partisan, or other identities and loyalties over accountability (Ashworth 2012). Voters may also fail to obtain reliable information or coordinate effectively to control corrupt politicians (De Vries and Solaz 2017; Banerjee and Pande 2007; Banerjee et al. 2014).

The breakdown of electoral control over corruption can be particularly prevalent in clientelistic environments, where politicians exchange favors for political support with voters and other politicians in their network (Fergusson, Molina, and Robinson 2022). Clientelism undermines voters’ ability to control corruption and exacerbates politicians’ incentives to engage in it. In such systems, voters remain fragmented, uninformed, and disengaged, while politicians require patronage and discretionary spending to maintain the clientelistic machinery.

This paper examines how political alignment can increase corruption in predominantly clientelistic political environments. Theoretically, we argue that resource extraction is particularly valuable to politicians with links to others in higher echelons of power for three primary reasons. First, they can more easily coordinate strategies for resource extraction. Second, providing funds for the clientelistic network may serve as a token for reciprocal political favors. These can take multiple forms, including lenient oversight from upper levels of government, the allocation of local contracts to politically connected allies, or mutual patronage through public appointments. Such exchanges between mayors and governors highlight how alignment can generate coordination for shared political gain, extending beyond the traditional focus on distributive favoritism. Finally, these advantages can translate into more resources for clientelistic vote buying, potentially insulating corrupt politicians from voter discipline and thus entrenching the clientelistic system.

To investigate these possibilities, we use an unusually precise measure of resource diversion in Colombia’s education sector, a country with a strong clientelistic tradition. Our study employs data from a nationwide audit of all students enrolled in every school in 2012, commissioned by the Ministry of Education. This censal audit followed allegations that local bureaucrats and politicians were fabricating “ghost” students to increase and divert national education transfers. The independent audit provides a school-level measure of resource diversion in the form of the proportion of ghost students.

Our analysis compares municipalities where the mayor’s party aligns with the governor’s, reflecting the partisan ties that underpin clientelistic networks in Colombia (Dávila Ladrón de Guevara 1999). Using a Regression Discontinuity Design, we show that when a municipality elects a mayor aligned with the governor’s party, the proportion of ghost students increases by approximately 0.3 standard deviations. At the same time, student performance declines in national standardized tests, with no improvement in genuine enrollment rates. An index of performance in language scores during the mayor’s tenure observes a drop of 0.23 (standardized) points.

Effects on ghosts are more prominent in municipalities with weaker institutions, including a more substantial historical prevalence of clientelism, less qualified principals, and prevalence

of a non-meritocratic hiring regime. These results align with our theoretical expectation that politicians engage more in corruption in areas with less oversight and more opportunities for discretionary diversion of resources to sustain the clientelistic network. Additionally, the adverse finding on test scores and null effect on actual service delivery suggests that a substantial portion of the diverted funds is used for political and economic gain rather than improving service quality (Fernández-Vázquez, Barberá, and Rivero 2016). We also find no evidence that aligned areas experience improvements in health or basic service delivery, suggesting that alignment does not generate additional resources that are then redirected to other locally valued priorities.

We examine how political alignment reinforces corrupt clientelistic systems. Our findings show that municipalities electing aligned mayors exhibit more and higher-value discretionary contracts in the education sector. They also increase temporary, discretionary hiring and expand outsourcing, often associated with patronage. These patterns support our first theoretical channel: intergovernmental coordination in resource extraction, primarily through clientelistic patronage and arbitrary contracting.

Exploring the second theoretical channel—that such funds partly serve as currency to pay for future political favors—presents empirical challenges, as these favors are typically unobserved. However, we provide suggestive evidence that aligned local politicians experience better future electoral prospects than their misaligned counterparts. A notable political benefit of this alignment is weaker oversight from regional and national-level agencies where upper-level politicians exert influence. We explore whether electing aligned mayors increases citizen complaints against local politicians for disciplinary violations. While we find only modest, non-significant results, these could be biased towards zero if citizens are less willing to denounce corruption where it is most prevalent. Moreover, governors with a higher share of closely elected aligned mayors also enjoy better career prospects, consistent with a mutually favorable exchange of politically valuable resources and support. Exploring favor exchange more directly and using detailed data on bureaucrats’ family networks from Riaño (2021), we show that alignment increases the likelihood of mayors’ relatives being appointed to departmental posts and governors’ relatives to municipal posts, consistent with reciprocal patronage.

Addressing our third theoretical incentive for resource diversion in clientelistic settings—extracting resources to buy votes—we confirm that aligned areas are more likely to exhibit future electoral vote-buying. This effective clientelistic vote buying helps explain why aligned politicians maintain better future electoral prospects despite diverting resources.

Fabricating public service beneficiaries to inflate transfers can stem from different motives: diverting resources to serve personal political or economic gain, or increasing funds for local services. The former motivations are more likely to thrive in environments with weak accountability, while the latter assumes some pressure for oversight. Although these possibilities are not mutually exclusive—politicians may simultaneously extract personal benefits and allocate resources to their constituencies—our findings largely support diversion for private gain, particularly in municipalities that exhibit a high prevalence of clientelism. To further assess the relevance of each possibility, we examine heterogeneous effects by local institutional quality. We find that the negative impacts of political alignment are concentrated in municipalities with weak state capacity, leading to lower math and language test scores, reduced genuine enrollment, increased risk of future electoral fraud, and greater reliance on

discretionary contracting, temporary workers, and outsourced services. These detrimental effects are typically absent or smaller in areas with stronger state capacity.

Falsification exercises evaluating covariate balance and placebo outcomes substantiate our empirical strategy for identifying the effect of electing an aligned mayor on outcomes. Importantly, our design employs a Politician-characteristic Regression Discontinuity (Marshall 2022), comparing municipalities managed by narrowly winning aligned politicians to those where non-aligned politicians narrowly win. This approach addresses critical potential sources of municipality-level selection bias and other non-municipal-level features not affected or selected by the election. However, it identifies only the effect of electing an aligned versus an unaligned candidate, which combines the effect of alignment with differences between other variables related to alignment (Bertoli and Hazlett 2025). Moreover, such differences between politicians might shift to maintain close elections, further influencing our estimates.

The set of complementary results suggests that a plausible mechanism explaining our results is indeed the partisan link between mayors and governors. Moreover, following Marshall (2022), we estimate potential biases and conclude that our estimates likely represent a lower bound of the alignment attribute, net of these “compensating differentials.” Substantively, this suggests that aligned politicians winning close elections may possess other characteristics that could mitigate their propensity to fabricate ghost students.

Also, our results demonstrate robustness to a comprehensive battery of tests, including: 1. Alterations in bandwidth size and kernel types for estimating effects; 2. Inclusion of pre-determined controls and department-level fixed effects; 3. Estimation at the municipal rather than school level; 4. Alternative transformations of the dependent variable; 5. Exclusion of exceptionally large or small schools and municipalities; and 6. Examining effects for performance changes rather than levels. We also document that neither tax revenue nor transfers significantly increase with alignment, an important result given the abundant literature on the fiscal benefits of political alignment.

Our paper contributes to several strands of literature and related policy debates, besides the main question on the effects of electoral incentives on prevailing corruption.

The literature on partisan alignment has long examined how co-partisanship between different levels of government shapes the allocation of public resources. Early work such as Grossman (1994) and Levitt and Snyder (1995) shows that aligned jurisdictions tend to receive larger federal transfers as national incumbents reward allies or consolidate support. Subsequent studies (Costa-i-Font, Rodríguez-Oreggia, and Lunapla 2003; Larcinese, Rizzo, and Testa 2006; Arulampalam et al. 2009; Berry, Burden, and Howell 2010) refine these insights across varied settings, while others highlight how institutional quality can mute distributive bias (Jones, Sanguinetti, and Tommasi 2000). More recent work shows that alignment can also influence beliefs and civic behavior (Teeselink and Melios 2025).

We build on this tradition and on studies of alignment’s effect on local governance (Solé-Ollé and Sorribas-Navarro 2008; Brollo and Nannicini 2012; Migueis 2013; Bracco et al. 2015), but focus on a dimension that has received less attention: alignment between municipal and departmental executives. This level of coordination is important in settings where the implementation of national programs depends on chains of local actors.¹

1. Curto-Grau, Solé-Ollé, and Sorribas-Navarro (2018) also study regional ties but focus on regional rather than national funds.

Our contribution is threefold. First, rather than studying transfers, we estimate the causal effect of electing an aligned mayor on a direct audit-based measure of corruption. While some recent work considers corruption (Borrella-Mas and Rode 2021; Stoecker 2022), their empirical strategies raise concerns about causal interpretation.² Second, we document welfare consequences by showing that alignment reduces the quality of public service delivery in education with no noticeable improvements in other areas. Third, we provide evidence on mechanisms by examining various heterogeneous effects, bureaucratic hiring and contracting patterns, reciprocal favor exchange across levels of government, and examining the role of partisan links relative to other traits related to alignment. This mechanism-oriented analysis distinguishes our approach from prior work that focuses primarily on distributive transfers.

Taken together, these results show that partisan alignment between local and regional executives can weaken oversight and facilitate the diversion of public funds, helping explain why alignment does not necessarily translate into improved outcomes (Asher and Novosad 2017; Bonilla-Mejía and Higuera-Mendieta 2017). This highlights the importance of studying the organizational channels through which aligned politicians coordinate, beyond the amount of resources they receive.

In the realm of corruption measurement and understanding, our study employs an objective measure of corruption and within-country variation, addressing measurement and identification limitations often present in cross-national studies relying on perception-based corruption measures (Treisman 2007; Olken and Pande 2012).

Regarding the efficiency costs of corruption (Banerjee 1997), we contribute to the ongoing debate by suggesting that ghost student fabrication leads to significant inefficiencies. This practice distorts actual education expenditure figures and impedes authorities' ability to recognize service under-provision (Olken 2007, 2009). Our findings of no local increases in service delivery or quality (as in Ferraz, Finan, and Moreira 2012), coupled with these distortions, indicate that resource diversion through ghost student fabrication incurs substantial inefficiency costs.

Our work also advances the literature on clientelism. While much research emphasizes how particularistic transfers undermine political accountability (Bates 1981; Kitschelt 2000; Stokes 2005), we reinforce the critical problem that clientelistic networks require funds for reproduction, potentially fueling corruption (e.g., Hicken 2011; Lindberg, Bue, and Sen 2022). We diverge from most literature by focusing on the incentives stemming from the network of exchanges in a clientelistic environment, particularly those between politicians, rather than solely on politician-voter exchanges through vote-buying.³

By extending the analysis of vote buying to broader, non-dyadic clientelistic relationships across multiple levels of government (as suggested by León and Wantchekon 2019), we also contribute to the literature on clientelism and decentralization. We demonstrate how clientelism can limit the advantages of political decentralization by creating conflicting accountability pressures on local politicians. We find that aligned local politicians have strong

2. Both papers estimate OLS effects of alignment on corruption. Borrella-Mas and Rode (2021) reports RD estimates in an appendix but notes they are less robust than their OLS results; their corruption measure is also based on news reports that may suffer from reporting biases and low incident rates (7% of observations).

3. In this respect, our approach is closer to Cruz and Keefer (2015), who consider how politicians in a clientelistic environment increase corruption, yet through a different mechanism: clientelistic politicians have weaker incentives to oversee executive policy implementation.

incentives for corruption, suggesting that “upward accountability” may supersede voter interests in discretionary resource allocation. These findings help inform the broader debate about the effects of fiscal decentralization on the quality of government (Enikolopov and Zhuravskaya 2007).

We also contribute to the literature on the personnel economics of the state (Finan, Olken, and Pande 2017) by demonstrating how politically aligned public officials may harm public service delivery. Our primary source of variation stems from partisan alignment between elected officials, rather than political affinity with appointed bureaucrats or frontline providers (see, for example, Colonnelli, Prem, and Teso 2020 and Xu 2018). Our findings on discretionary contracts and nepotistic hiring in aligned municipalities resonate with research on inefficient patronage and the adverse effects of politically motivated bureaucratic turnover (Akhtari, Moreira, and Trucco 2022).

Turning to the literature on education, our approach relates to the work of Reinikka and Svensson (2004) on local capture of educational transfers. Yet, we focus on variation in political incentives for fund diversion rather than school-level features affecting bargaining power. Also, our findings help explain the “learning crisis” (Sandefur, Pritchett, and Beatty 2016) by suggesting that political incentives leading to resource diversion may underlie many of the problems identified in the literature, such as unprepared teachers and insufficient school inputs. This perspective may help explain the puzzle of disappointing educational outcomes in poorer nations despite increased public spending.

Finally, we underscore the risks of incentives and fixed-rule schemes in financing public goods with weak oversight, particularly in education (Angrist et al. 2019). Our results highlight the importance of robust monitoring for quantity-based payment schemes, especially when cheating is possible (Litschig 2012; Acemoglu et al. 2020).

The rest of the paper is organized as follows. In the next section, we briefly discuss theoretical expectations on the potential effects of electing an aligned mayor. Section 3 describes the relevant institutional context for public education provision in Colombia. Section 4 outlines our data sources and the empirical strategy. Section 5 presents the main findings, while Section 6 provides evidence on the underlying mechanisms. Finally, Section 7 concludes.

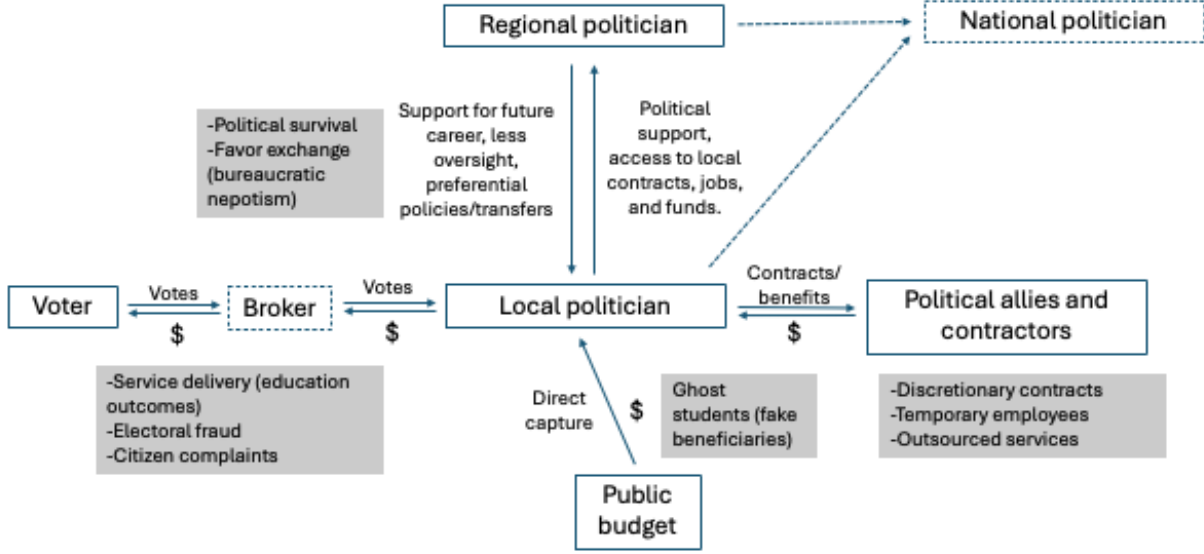
2 Theoretical discussion: clientelistic exchanges and corruption

This section examines how political alignment across government levels may influence public fund diversion in clientelistic settings. We focus on the fabrication of fake public service beneficiaries to inflate intergovernmental transfers—a widespread form of resource misallocation, including in our empirical case, Colombia.⁴ Comparable practices have been documented worldwide, from ghost students in Latin America to fictitious pension beneficiaries in Nigeria and nonexistent needy children in US pandemic relief programs.⁵

4. Similar schemes in Colombia include fabricating fake hemophilia (Contraloría General de la República 2016) and HIV/AIDS (Procuraduría General de la Nación 2018) patients to divert healthcare funds.

5. La Tercera (2015), Fallas (2013), Metro PR (2015), Fernández (2019), BBC (2021), and The United States Department of Justice (2022).

Figure 1: A simplified scheme of clientelistic political transactions



Notes: This figure presents a simplified schematic of a typical clientelistic political landscape. Solid boxes represent actors in the simplified scheme. Shaded boxes link the outcomes we examine to the relevant element of the schematic framework. Voters exchange electoral support for immediate benefits from party brokers (depicted in a dashed box since we cannot observe them). Brokers, in turn, deliver vote blocs to local politicians. These politicians finance such exchanges by exploiting public resources while in office. Additionally, aligned local and higher-level officials can trade resources and support within these networks. The figure shows that local and regional politicians may align with national-level politicians, a focus of much of the alignment literature, to the detriment of the local–regional alignment we examine.

Figure 1 depicts a simplified clientelistic political structure. At the base, voters trade votes for money and gifts from local leaders or brokers, who, in turn, secure political support for local politicians in exchange for jobs, contracts, or other benefits. To sustain these exchanges, politicians exploit public resources, whether through patronage, discretionary contracting, or direct embezzlement. The figure also highlights the role of political alignment between local clientelistic politicians and higher-level regional officials. Our focus is on how these connections facilitate corruption, rather than the more commonly studied links to national-level politicians (also shown in the figure). Finally, the figure highlights the outcomes we analyze, corresponding to the scheme’s components.

Resource diversion by fabricating beneficiaries may reflect diversion for personal political or economic gain. Political alignment may amplify these incentives through three main mechanisms:

Ease of coordination: Aligned politicians can better coordinate fraud, such as inflating enrollment figures. Through the network, an aligned local politician will have more information, ease of communication, and the ability to ensure compliance with informal agreements. Hiring and contracting become tools of clientelism, reinforcing political patronage.

Political favors: Misappropriated funds serve as currency to pay for political exchanges to higher-level partners in the clientelistic network, whether securing future support, accessing

campaign financing, or reducing oversight from upper government levels.⁶ Such reduced oversight may reflect not only reciprocal favors, but also ideological affinity, coordination advantages, or concerns about political spillovers among co-partisans; each of these channels similarly weakens monitoring and can facilitate misreporting. We expect aligned politicians to exchange favors, have stronger career trajectories and face weaker scrutiny, leading to more misbehavior potentially and increased complaints against them.

Vote-buying financing: Diverted resources—whether from public funds or allied actors (campaign donors, contractors, or political brokers)—fuel vote-buying operations sustaining the broader clientelistic exchanges depicted in Figure 1. As a result, ghost student numbers should rise more sharply in historically clientelistic areas, and aligned politicians should engage more frequently in electoral fraud.

Some of these predictions, however, change if politicians fabricate beneficiaries to secure additional funds for genuinely improving public services.⁷ In this case, political alignment may still encourage beneficiary fabrication by well-intentioned politicians, if alignment improves coordinating resources and claiming credit for service improvements, boosting their electoral prospects. However, if these motivations are prevalent, alignment should not lead to widespread patronage, discretionary hiring, or arbitrary contracting, as these would undermine service quality. Likewise, it should not result in increased citizen complaints. Instead, we should observe tangible service improvements in education or other areas. Additionally, ghost beneficiary manipulation should not be more prevalent in historically clientelistic areas, nor should aligned politicians engage more frequently in electoral fraud or favor exchange.⁸

Both private-gain and service-delivery motives may coexist, and assessing their relative weight and impact is precisely what motivates our empirical analysis.

3 Background

To help interpret our data and findings, this section examines clientelism and corruption in Colombia’s political system and the institutional framework of education financing and provision.

3.1 Parties, political clientelism and corruption

Colombia’s party system changed markedly over the past three decades. The 1991 Constitution and later electoral reforms weakened the Liberal–Conservative duopoly and enabled the entry of new partisan organizations. The system became increasingly fragmented and

6. The oversight mechanism aligns with Montenegro (2020), who finds that decentralized auditing institutions in Colombia perform worse than centralized ones, highlighting how weak local accountability can enable discretion in aligned settings.

7. Despite a less venal motivation, such a possibility still constitutes corruption in its basic sense of “dishonest or illegal behavior by powerful people, (such as government officials or police officers)”, Corruption, Merriam-Webster Dictionary, <https://www.merriam-webster.com/dictionary/corruption>, last accessed September 17, 2024.

8. Appendix Table B-1 summarizes the contrasting expectations under two extreme motivations for producing fake beneficiaries: personal gain versus community benefit. The table also reports our empirical results, which align with the predictions under the private-gain motive.

volatile, especially from the late 1990s through the 2010s. The number of parties contesting municipal elections rose from about two in 1997 to more than four in 2011, and the average number of candidates increased from 3.4 to just above 4. During the same period, the traditional parties' municipal win rate fell from 77% to 41%.

This fragmentation did not weaken clientelistic coordination. Parties continue to function as networks of brokers and officeholders that mobilize support through public employment, contracting, and access to discretionary resources (Archer 1990; Dávila Ladrón de Guevara 1999; Guerrero García 2014). Even as partisan labels multiplied, parties remained important channels connecting local politicians to departmental and national leaders. This continuity is reflected in patterns of partisan alignment: although the share of municipalities with a mayor aligned to the governor declined from 48% in 1997 to 30% in 2011, such alignment still occurred in roughly one in three municipalities. Alignment in 2011 therefore remains an informative marker of shared political networks in a fragmented system.

Consistent with widespread clientelism, vote-buying is common in Colombia. Fergusson, Molina, and Riaño (2018) show that approximately 20% of voters base their decisions on gifts or favors from politicians or brokers. Notably, respondents readily admit to this behavior, suggesting its social acceptability.

Vote buying is typically part of a broader network of clientelistic exchanges, involving voters, brokers, politicians, campaign donors, and contractors. These patterns are well-documented in academic literature and journalistic investigations. Huertas and Osorio (2018) describe the market for local leaders or brokers in the capital city of Bogotá, where they charge “70,000 Colombian pesos to secure a vote” and “often work for more than one candidate.” The exchange also often involves patronage, with leaders telling candidates: “I help you with the votes, and [when] you are elected, you help me with jobs for ten people.” Discussing the Northern region of Colombia, Ardila (2015) describes brokers as the “professionals of vote buying”, receiving patronage, government contracts, and cash for securing votes.

The resulting interactions between politicians at different government levels and the implications for corruption are well documented. The governor of the Department of Córdoba and friendly mayors received campaign support from a health service provider later involved in the fake hemophilia patients scandal (Cantillo 2017). Drawing from more systematic data, Ruiz (2017) suggests that donor-funded candidates reciprocate with contracts and Bonilla-Mejía and Higuera-Mendieta (2017) demonstrate the clientelistic ties between local and national politicians, emphasizing the electoral gains local mayors aligned with incoming presidential coalitions offer to congressional candidates and the resulting additional discretionary transfers they receive.

Focusing on education specifically, in the impoverished port of Buenaventura, fake students were linked to the mayor Bartolo Valencia's group (“the group of 100”) involving school principals and politicians to misuse education resources (El País 2014; Bermudez 2015). School principals contributed to the political campaign in exchange for more educational transfers. Other donors and supporters received other benefits: Pedro Marino Barahona won public contracts worth over \$2,100 million pesos, and local leader Rodrigo Mina controlled jobs in the Secretary of Education. The Office of the General Attorney found evidence of ghost students and the hiring of shell companies to instruct false students. Buenaventura's share of ghost students is 4.24%, approximately 1.5 standard deviations more

than the mean.

The critical role of school principals and school jobs in the network of clientelistic exchanges and vote buying extends beyond Buenaventura. [Molano-Jimeno \(2020\)](#) describes the details in one of the country’s “epicenters” of vote buying: the municipality of Soledad in the Northern department of Atlántico featuring the “most sophisticated and perverse method” of vote buying, “supported on the schools”. A source describes:

The thing works like this: Soledad Mayor’s Office or the Atlántico Governor’s Office contracts private schools because public institutions cannot cover the demand. The agreement finances student quotas and professors’ salaries. With this, they have two forms of corruption: one that works by collecting a fraction of the wages of the teachers and employees, and another that is converting the families of the scholarship recipients and the school workers into their voters.

A teacher adds that if one is unemployed, one can get a job at a school in Soledad so long as one is willing to accept a share of the salary while reporting it as if one were receiving it in full. “They also asked us [teachers, employees, and families of the scholarship recipients] to vote and made us register the school as a polling station,” he concludes.

In sum, in this context of prevalent clientelism and corruption, alignment between governors and mayors strengthens incentives and opportunities to produce ghost students, matching the simple scheme of Figure 1. Schools may lie at the center of the vote-buying operation and fuel other clientelistic exchanges. Their jobs offer opportunities for political patronage, their students’ families a captive electoral base, and the contracts offer opportunities to capture resources and reward political supporters. A mayor with access to the departmental governor will likely face a more robust demand for (and supply of) siphoning opportunities. An aligned clientelistic governor, who might have helped the mayor during the campaign, will expect the mayor to use part of the resources at their disposal clientelistically, contributing to the governor’s personal or political gain. The mayor may also expect access to other funds in office in return and support for his future political ambitions. Finally, the governor may look the other way when this occurs in aligned relative to non-aligned municipalities. We next discuss the institutional details determining the funding and provision of education and how it interacts with these political incentives.

3.2 Public education in Colombia: institutional details

The Secretary of Education monitors, supports, and helps administer Educational Institutions (schools) in Colombia. It implements education policy plans, monitors funds managed by schools in their jurisdiction, and guarantees quality standards. More importantly, it hires, promotes, transfers, and fires public school principals and teachers.

Mayors are the chief executive authorities in municipalities. They oversee local spending and the implementation of education programs. Administrative staff in the education secretariat manage day-to-day execution of education funds and contracts.⁹ School principals

9. Municipal councilors are elected legislators who approve the municipal budget and exercise political oversight. In principle, a council with strong opposition representation could constrain mismanagement, but

manage schools (together with an academic and an executive committee) and have broad responsibilities.¹⁰ Because Secretaries of Education not only administer resources but also supervise expenditures and ensure compliance with quality standards, they can exercise discretion in oversight intensity. In aligned municipalities, this discretion may translate into greater tolerance for irregularities or less stringent monitoring, consistent with the reciprocal favor mechanisms described earlier.

Open calls must fill teaching positions. But when they fail, temporary direct hiring is allowed. Open calls are frequently alleged to be intentionally designed to fail to facilitate discretionary temporary hiring. Payroll, especially direct provisional hiring, has long been linked to electoral support for local politicians, particularly mayors, in a classic clientelistic patronage fashion (as illustrated in the previous section). Ayala-Guerrero (2017) argue that provisional, temporary hiring rewards political favors with a detrimental effect on quality. Anecdotal evidence from news stories also reveals politician control of school contracts and school jobs, especially of provisional hiring (for instance, [El Tiempo 2018](#) and [Azuelo and Leon 2015](#)).

Each Secretary of Education ascribes to a *Entidad Territorial Certificada* (ETC, Certified Territorial Entity), certified to have the adequate institutional capacity to manage the regional or local education system. All departments (32 in total) are ETCs and thus have a Secretary of Education.¹¹ In addition, 58 municipalities were certified as ETCs (and thus have an independent Secretary of Education).¹² Furthermore, given their strategic importance, five were designated as certified special districts, thus becoming ETCs with their own Secretary.

The system for financing education is also essential for interpreting our findings. Most of the money spent on education comes from central government transfers (88%), while regional governments contribute 3% of the total, and local government sources complete the remaining 9% (Melo-Becerra et al. 2016).

The rules for central government funds are summarized in Table 1. The table highlights the key role that student enrollment plays in allocating national resources to different areas.

measuring this systematically is difficult: council size varies widely, councilors often run on local coalitions rather than national party labels, and no reliable mapping of coalitions to partisan blocs exists at scale. Because of these data limitations, we do not examine heterogeneity by council composition here, though this remains a promising direction for future work.

10. Principal’s duties span five domains: (i) strategic management (e.g., aligning the school’s mission and vision); (ii) human resource management (e.g., teaching staff assignment across branches and grades); (iii) leading pedagogical and academic processes to promote quality (e.g., developing specific study plans that are relevant and appropriate for the community and context); (iv) strengthening relationship with the community (e.g., leading community engagement activities); and (v) administering financial and physical resources (e.g., executing the school’s budget and investment plans).

11. The largest administrative division in Colombia is the department, each headed by a publicly-elected Governor.

12. Municipalities are the second largest administrative division. Of the 58 certified municipalities, 13 experienced close elections. We retain them in the main analysis because they represent less than 5% of the sample and, as discussed with public officials, political alignment may still influence outcomes even where municipalities enjoy greater autonomy. Certified municipalities manage larger budgets and exercise greater discretion in contracting and school administration, which can heighten corruption risks (Borrelli-Mas and Rode 2021). As a former departmental Secretary of Education remarked, “A certified municipality can be a channel for good or bad things—but you can certainly take a lot of advantage.”

Table 1: Types of education transfers to local governments in Colombia

Account	Percent of total resources	Transferred to	Distribution criteria
Payroll	90%	Paid directly by Ministry of Education	Number of Teachers, itself a function of <i>student enrollment</i> .
Quality-enrollment (<i>calidad-matricula</i>)	5%	Regional Secretary of Education	Performance, poverty, and <i>student enrollment</i> .
Quality-access (<i>calidad-gratuidad</i>)	5%	Schools	<i>Student enrollment</i> .

Notes: This table presents the different types of education transfers from the central to local governments in Colombia and highlights how each type depends on student enrollment figures.

Government funds are divided into three accounts: payroll (*nómina*), quality-enrollment (*calidad-matricula*), quality-access (*calidad-gratuidad*). The majority of government funds (90%) come through the payroll account, with the remaining 10% split equally in the two “quality” accounts. The amount transferred by the Ministry of Education through the payroll account is a direct function of the number of teachers, which in turn is a function of student enrollment.¹³ The quality-enrollment account is transferred to the regional and local Secretaries of Education and responds to enrollment, performance (in student dropout and grade repetition), and poverty indices. These funds can be used for infrastructure, teaching materials, utilities, teacher training, student transportation, and student meals. Finally, funds from the quality-access account are transferred directly to schools as a function of enrolled students. They can cover teaching materials, infrastructure, office supplies, utilities, student travel expenses, subcontracting professional and technical services, pedagogical activities, transportation, and academic and non-academic activities.

In short, this subsection reveals that a *de jure* meritocratic system to fill school vacancies is plagued with political patronage and provisional hires to reward political favors. It also confirms that artificially increasing the number of enrolled students implies receiving more national transfers, the primary source of resources.

13. Specifically, the norm (Decree 3020 of 2002) stipulates that allocating teachers must respect a minimal pupil-teacher ratio of 32:1 in urban areas and 22:1 in rural areas. The rationale for establishing a lower bound on the student-teacher ratio is to distribute scarce teachers better.

4 Data and empirical strategy

4.1 Data

Ghost students and other outcomes

We build on the 2012 audit study financed and contracted by the National Ministry of Education. Fieldwork started in September and ended in the second week of December, the last day of classes. Audit firms implementing the study were competitively selected, and Ministry functionaries protected the audit from cooptation, for instance, by avoiding local auditors.¹⁴ In addition to the auditing firms, a separate set of firms was hired to inspect the auditing process.

The audit intended to reach all schools in the country, auditing the students reported on enrollment lists by June 30, 2012. The goal was narrowly missed because of a few distant and rural schools: 8,167,051 out of the 8,679,035 students in the Ministry’s information system records were audited (94.10%).

Auditors conducted detailed, in-person verification by physically visiting each school. If a student was found to be missing, the auditors required supplementary documentary evidence to account for their absence, such as notes on the reasons for missing school, grade records, and exams presented by the student. Additionally, Secretaries of Education and school staff were given just two days to respond and clarify any alleged mistakes by the auditing firm (as per *Circular 28 of 2012*). This short timeline left little room to fabricate supporting evidence. Furthermore, while the specific audit dates were announced shortly before the visits, these announcements came after the enrollment lists had been compiled. As a result, it was impossible for schools to retroactively adjust their reported enrollment figures in anticipation of the audits. The rigorous documentation required to justify a student’s inclusion on the enrollment lists also minimized opportunities to create fraudulent evidence.

The audit identified a total of 148,410 ghost students.¹⁵

Using the auditors’ final report, we construct our main dependent variable: the share of ghost students per school in each municipality. To contextualize the magnitude of this figure, we estimate the total cost based on government transfers per student, arriving at approximately \$110 million USD—equivalent to about 3.5% of the payroll account.¹⁶ Of course, this figure is just the direct cost of transferring money to nonexistent students. As Section 3 revealed, several additional opportunities for resource diversion may compound

14. Previous audits of smaller scale were conducted during 2010 and 2011, covering 762,431 and 732,073 students, respectively (as opposed to over eight million in 2012). These audits targeted problematic municipalities with signs of mismanagement and corruption. To address concerns that differential behavior in previously audited places contaminates our estimates, we verified that political alignment does not predict prior audits or pre-existing measures of municipal corruption and inefficiency. Appendix Figure C-3 shows that alignment does not predict ghosts found on previous audits nor pre-existing levels of municipal state capacity or citizen complaints against functionaries.

15. When the ghost student scandal emerged, some press reports—citing government sources—mistakenly inflated the figures (reporting up to three times the actual number) by conflating audited inexistent students with the routine year-to-year turnover of enrolled schoolchildren. To ensure accuracy, we verified the audit data with government officials, confirming that our figures align with final official government reports; see for instance [MEN \(2012\)](#).

16. The USD value of ghosts uses the average exchange rate in 2012, COP \$1,800 per dollar.

the financial burden of creating fake students.

We collect data on several additional variables to explore the mechanisms that shed light on the effects of electing an aligned mayor, which we discuss in detail in Appendix Table A-1.

Electoral data

We use electoral data compiled by Pachón and Sánchez-Torres (2014) from the Colombian national electoral authority. While our dataset covers local elections from 1997, 2000, 2003, 2007, and 2011, our main analysis focuses on the 2011 election, held on October 30. This election selected mayors in 1,099 municipalities and governors in 32 departments, with terms running from January 1, 2012, to December 31, 2015. Both mayors and governors were elected by a simple plurality rule at their respective levels.

4.2 Empirical strategy

We rely on partisan alignment between the mayor and the governor for our key explanatory variable. We leverage the quasi-random variation in partisan alignment generated by close municipal elections to identify causal effects. Our Regression Discontinuity (RD) Design compares the share of ghost students (or other outcomes) in municipalities where a mayoral candidate from the governor’s party narrowly won versus those where they narrowly lost (Lee 2008; Lee and Lemieux 2010).

To measure alignment between municipal and departmental officials, we focus on the 2011 local elections for mayors and governors. Once elected, governors and mayors quickly appoint their cabinets, including Secretaries of Education, who play a key role in allocating school principals and staff. This timing allows us to assess the impact of political alignment on ghost student numbers, as the nationwide audit was conducted eight months after the new officials took office. Our final sample includes 4,383 schools across 332 municipalities where a candidate from the governor’s party and an opponent from another party competed in the mayoral election. These municipalities represent about one-third of the 1,099 that held elections in 2011.

To illustrate the variation in our data, Figure 2 displays the geographical distribution of the key variables in the analysis across Colombia. Darker colors represent a higher proportion of ghost students in the municipality. The squares denote places where a governor’s party candidate competed and lost by a narrow margin (using a 13% vote threshold), while the triangles show where the governor’s party candidate won. This map demonstrates significant geographic variation in ghost student proportions, as well as a wide dispersal of competitive races involving governor’s party candidates (both winners and losers).¹⁷

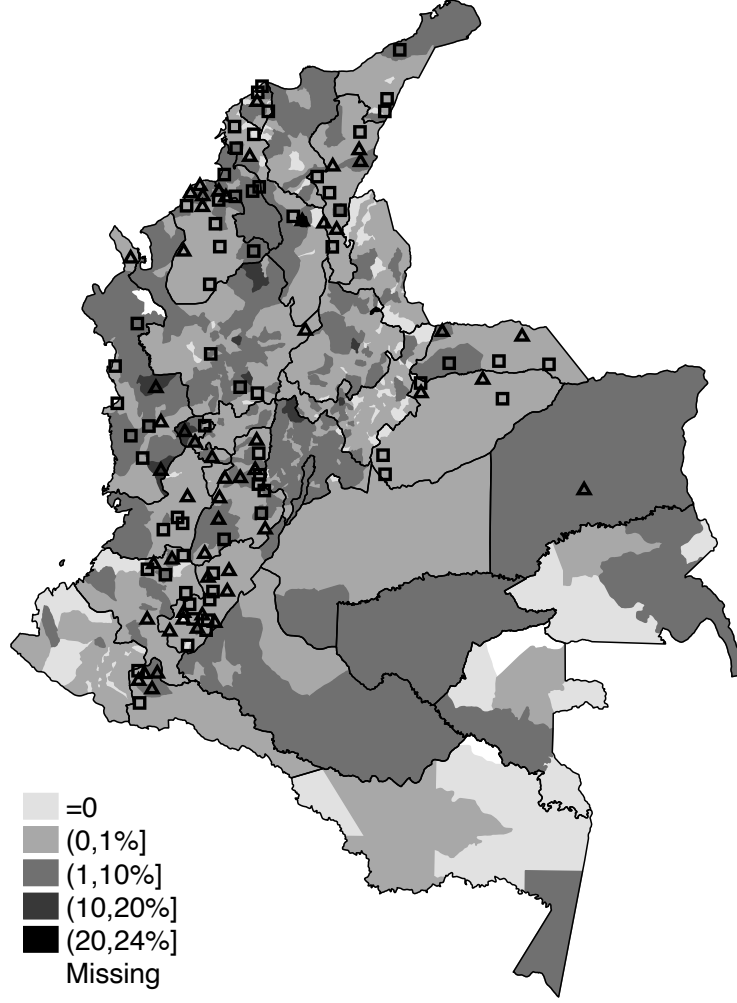
Our baseline RD specification is

$$y_{im} = \alpha + \tau \text{Alignment}_m + f(\text{margin}_m) + Z'_{im} \Gamma + \varepsilon_{im}, \quad (1)$$

where i indexes schools, m denotes municipalities, and y_{im} is the outcome of interest (e.g., the share of ghost students in school i located in municipality m). Alignment_m is an indi-

17. There are much fewer but larger municipalities in the sparsely populated areas of the Eastern Planes (on the east of the map) and the Amazon (towards the south and south-east).

Figure 2: Ghost students in the Colombian territory



Notes: This figure shows the fraction of ghost students per municipality (darker=more ghosts). Squares (triangles) are places where a candidate of the elected governor competed in a close race, defined using the baseline bandwidth of 13%, and lost (won).

cator variable equal to one if municipality m elected an aligned mayor, $f(\text{margin}_m)$ denotes a polynomial that controls for a smooth function of the margin of victory of the mayor, and Z_{im} represents a vector of covariates that includes a set of pre-determined school- and municipal-level controls. Finally, ε_{im} denotes an error term capturing all omitted factors. Since Alignment_m is defined at the municipality level, we cluster standard errors accordingly to account for heteroscedasticity.

The coefficient of interest, τ , represents the local average treatment effect (LATE) for municipalities electing an aligned mayor.

We estimate this equation non-parametrically, using bias-corrected estimates within the optimal bandwidth following Calonico et al. (2019). The bandwidth minimizes the mean squared error (MSE) of the RD treatment effect, achieving the best trade-off between bias and precision. Because this choice affects inference, we apply the robust bias-corrected inference

framework proposed by Calonico, Cattaneo, and Titiunik (2014). Our baseline specification employs a triangular kernel weighting scheme with a linear polynomial estimated separately on each side of the threshold, and we assess robustness to alternative kernel functions and polynomial specifications.

Additionally, we estimate equation (1) at the municipal level, using one observation per municipality instead of per school when outcomes are measured at that level.

5 Results

5.1 Descriptive statistics

Appendix Tables B-2 and B-3 present descriptive statistics for the key variables at the school and municipality levels, respectively.

On average, 1.3% of reported students are ghost students, though there is substantial variation. About half of the schools in our sample reported enrollment figures accurately, while the top 1% of schools fabricated 20% or more of their reported students.

The average school has 655 students and 23 teachers. Among 100 students, 52 are male, 27 are in high school, 98 are minors, and 73 live in rural areas. Nearly all come from low-income backgrounds (strata 0, 1, or 2). Additionally, one student has disabilities, 36 attend a double shift, and 10 have third-party-hired teachers. Postgraduate education is rare among teachers (1.8%) and principals (1.7%).

A candidate from the governor’s party wins 30% of the time, and mayors are predominantly male (93%) and white (78%), with 30% affiliated with the two traditional parties. Baseline local state capacity is weak. For instance, electoral fraud risk is widespread—nearly half of the municipalities exhibited anomalies historically. Additionally, municipalities recorded an average of two citizen complaints per 10,000 inhabitants against public officials.

Crucially, the raw data reveal a clear pattern: schools in aligned municipalities report 1.5% ghost students, compared to 1.1% in unaligned municipalities—a one-third increase. The difference is even larger at the municipal level: an average of 1.7% ghost students in aligned areas, nearly doubling the 0.9% in unaligned municipalities. We now formally examine this effect.

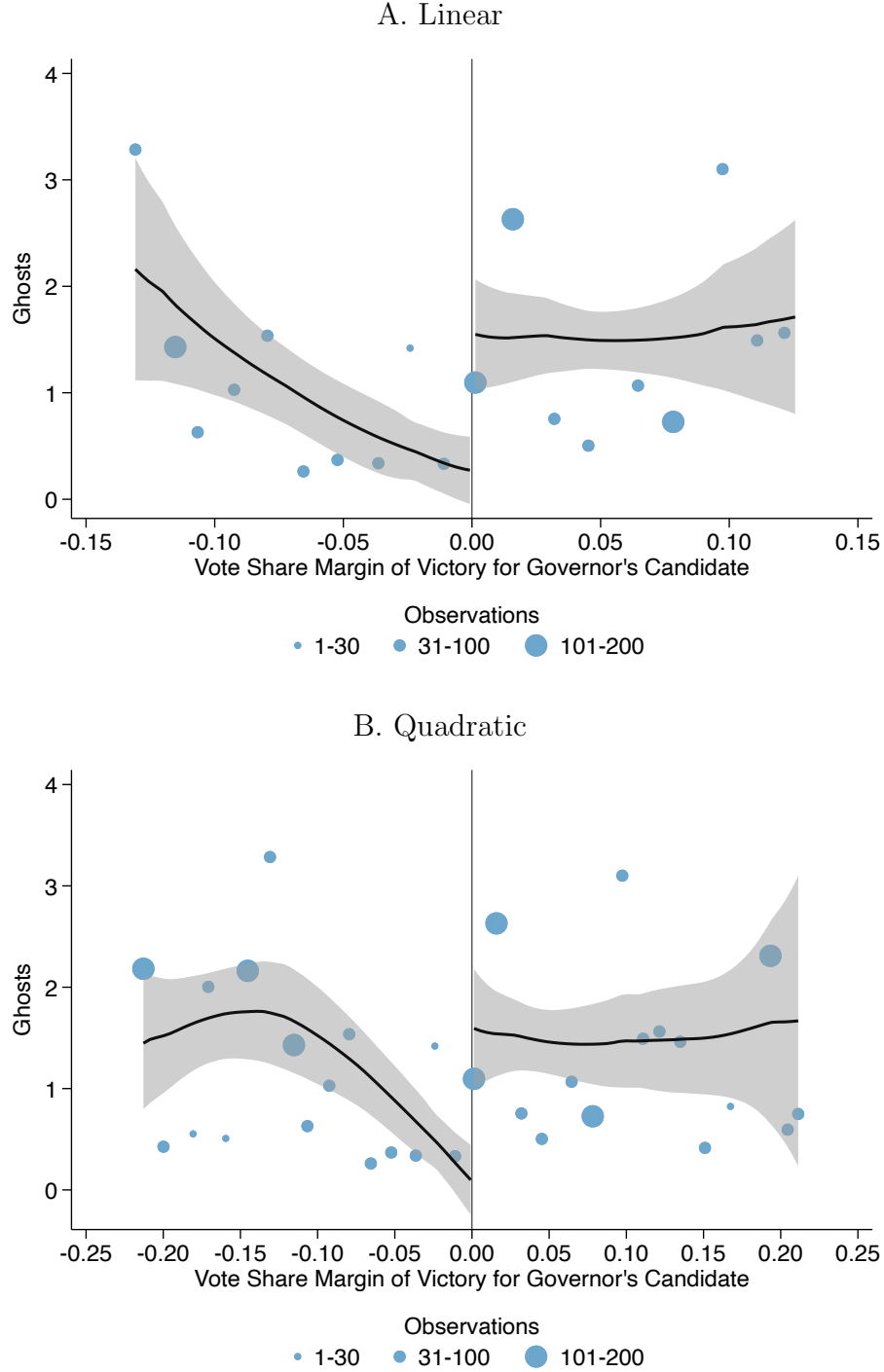
5.2 Political alignment and ghost students

Main effects

Figure 3 presents our main results graphically. There is a sizable and significant increase in the share of ghosts in municipalities where the mayor is aligned with the governor. This result is robust to using linear or quadratic local polynomials.

Panel A of Table 2 reports the point estimates from equation (1) across different kernel types (Triangular, Epanechnikov, and Uniform) and polynomial specifications (linear or quadratic) without controls. In Panel B, we instead use a triangular kernel and a linear polynomial and assess the robustness of our estimates to different sets of pre-determined controls. Column 1 includes school-level controls, Column 2 adds demographic and geographic controls, and Column 3 introduces electoral controls, including fixed effects for major parties

Figure 3: The effect of political alignment on ghost students



Notes: This figure shows the fraction of ghost students (in %) as a function of the vote share margin of victory of the mayoral candidate from the governor's party, relative to the highest-ranked candidate from a different party. All estimates are bias-corrected, non-parametrically estimated and computed within the optimal bandwidth following Calonico et al. (2019) using a triangular kernel and a linear and quadratic polynomial in Panels A and B, respectively. Bins are selected to mimic the variance (evenly spaced using spacing estimators). The shaded areas show 95 percent confidence intervals based on standard errors clustered at the municipality level and robust to heteroskedasticity. Observations: 4,383.

to distinguish the impact of political alignment from broader party influences. Column 4 includes all municipality-level controls, combining those in Columns 2 and 3, while Column 5 includes the complete set of school- and municipality-level controls. Finally, Column 6 further controls for department-level fixed effects. A complete list and definitions of school- and municipality-level control variables are provided in Appendix Table A-1.

The pattern revealed by Table 2 is clear: electing an aligned mayor leads to a statistically significant increase in the share of ghost students. These patterns remain highly stable when including different sets of controls and across our various estimating equations.

Table 2: RD estimates of the effect of political alignment on ghost students

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable is ghost students per school (in %)						
<i>A. No controls</i>						
Alignment	1.402 (0.692)	1.272 (0.708)	1.213 (0.726)	1.588 (0.765)	1.510 (0.783)	1.172 (0.888)
Observations	4,383	4,383	4,383	4,383	4,383	4,383
Obs. in bandwidth	1,338	1,249	1,099	2,090	1,926	1,455
Bandwidth	0.130	0.118	0.107	0.215	0.197	0.144
Kernel	Trian.	Epan.	Unif.	Trian.	Epan.	Unif.
Local polynomial order	1	1	1	2	2	2
<i>B. Robustness to including controls</i>						
Alignment	1.317 (0.606)	1.313 (0.706)	1.738 (0.497)	1.568 (0.571)	1.696 (0.550)	1.564 (0.236)
Observations	3,939	4,383	4,383	4,383	3,939	3,939
Obs. in bandwidth	1,428	844	948	768	720	673
Bandwidth	0.150	0.082	0.091	0.080	0.081	0.072
School controls	Yes	No	No	No	Yes	Yes
Geographic/demographic controls	No	Yes	No	Yes	Yes	Yes
Electoral controls	No	No	Yes	Yes	Yes	Yes
Department fixed effects	No	No	No	No	No	Yes

Notes: This table reports RD estimates of the effect of electing a politically aligned mayor on the fraction of fabricated students in schools in the mayor’s municipality. The dependent variable is the school-level ratio of fake students to total students in 2012. *Alignment* is a binary indicator equal to one if mayoral candidate of the same party as the elected governor won the election. All results are based on equation (1), which is estimated at the school level. Panel A reports estimates using different kernels and polynomials with no controls. In contrast, Panel B reports estimates with a triangular kernel and a linear polynomial, incorporating a set of school-level, geographic/demographic-level, electoral-level, and/or department-level controls as indicated in each column. We do not report the coefficients for these additional controls, because, unlike traditional linear regression, the non-parametric estimators implemented do not directly estimate coefficients for the control variables (see Appendix D). See Appendix Table A-1 for the list of control variables within each group. All estimates are bias-corrected, non-parametrically estimated, are computed within the optimal bandwidth following Calonico et al. (2019). Standard errors are clustered at the municipality level and are robust to heteroskedasticity.

The impact is also quantitatively meaningful, with an increase of 1.2 to 1.7 percentage

points in the share of ghost students, equivalent to up to a third of a standard deviation. In schools where mayors are unaligned with the governor’s party, the fraction of ghost students is 1.12% of the total reported enrollment (see Appendix Table B-2). When the mayor is aligned, this figure doubles or triples, implying an increase in fabricated students per school from 8.4 to 21.5. Given an average of 13.2 schools per municipality, and a per-student transfer of \$742 USD, electing an aligned mayor leads to an estimated misallocation of \$129,000 USD in 2012 (approximately \$180,000 USD in 2024)—a substantial amount relative to local budgets.¹⁸

This incentive is especially strong considering that the estimated economic loss per aligned mayor is nearly six times the average annual mayoral salary in 2012.¹⁹ Later in the paper, we show that municipalities with politically aligned mayors underperform on other governance and service delivery outcomes, suggesting that this represents only a lower bound of the total economic cost.

Naturally, the RDD strategy uncovers a local effect, and the results herein may not be externally valid in other contexts, particularly in non-competitive races. One conjecture is that aligned politicians will be even more able to produce fake students for personal gain with little accountability costs in less competitive places. If so, the estimates here are a lower bound. Alternatively, corruption may be rampant without competition, so competitive places observe more significant marginal impacts of alignment, or mayors in close races might quickly extract rents, anticipating possible electoral defeat (of their parties, since they are term-limited). Descriptive statistics in Appendix Table B-4 are inconsistent with this last conjecture since ghost students are very similar in close and non-close races between aligned and unaligned mayors.²⁰

In any case, our findings remain relevant for at least two reasons. First, the results help elucidate the general theoretical motivations underlying ghost students. Second, the set of close races underlying our empirical strategy is not negligible in scope. In our baseline model, 332 mayoral races involved an aligned versus unaligned candidate, with 119 being decided by a narrow margin. Moreover, these competitive races occurred across 21 (of 32) departments,

18. Our data capture ghost enrollment early in each mayor’s term, when the audit campaign took place. Because follow-up audits were limited and non-random, we cannot study whether fabrication rises or falls later. If anything, incentives to inflate enrollment are strongest early, when doing so yields benefits over the entire term. Still, we acknowledge that potential dynamic adjustments with opposing impacts (e.g., experience, changed perceptions of oversight) cannot be assessed with the available data, and we note this as a limitation for external validity.

19. Colombian municipalities are classified into seven categories based on population and income, with legally mandated salary caps for mayors. Based on the classifications in our sample, the average maximum salary for a mayor in 2012 was approximately \$20,939 USD (or \$37,689,638 COP).

20. The pattern in Figure 3 suggests that non-aligned mayors who win narrowly may report fewer ghost students than those who win by larger margins. However, this pattern away from the cutoff is not causal, since municipalities further from the threshold differ in ways we cannot observe. The RD design identifies the difference at the threshold: aligned and non-aligned mayors who both narrowly win should face similar direct electoral incentives to limit corruption. If alignment provides a degree of political safety to narrow winners and this leads to more ghost students, this is consistent with our interpretation that alignment enables relative misbehavior. To examine whether treatment effects vary beyond the cutoff, we implemented the local method of Dong and Lewbel (2015), which estimates the Treatment Effect Derivative (TED) at the threshold. This approach provides a test of how the effect evolves as the running variable moves away from the cutoff, without imposing global functional forms or requiring conditional independence as in Angrist and Rokkanen (2015). Across specifications, the estimated derivatives are positive but imprecise, with confidence intervals including zero. Detailed results are available upon request.

with at least one close race observed in 18.

Thus, the close races captured by our empirical approach are meaningful in terms of both scale and geographic distribution.²¹

In Figure 4, we estimate quantile treatment effects (Frandsen, Frölich, and Melly 2012). The results show that the largest impact of political alignment on ghost students occurs at relatively high levels of ghost student prevalence. There are a few potential explanations for this pattern. First, the reputational risk for those involved may be greater when ghost students are less prevalent. Second, setting up such a system may be more costly and risky in settings where the infrastructure for manufacturing fake beneficiaries is less established or pervasive. Where these fraudulent practices are less common, education officials and auditors may also be more likely to detect and crack down on them. This result also suggests that tackling entrenched corruption in public service delivery may require targeted strategies that address the underlying conditions enabling such practices to take root and flourish.

Robustness

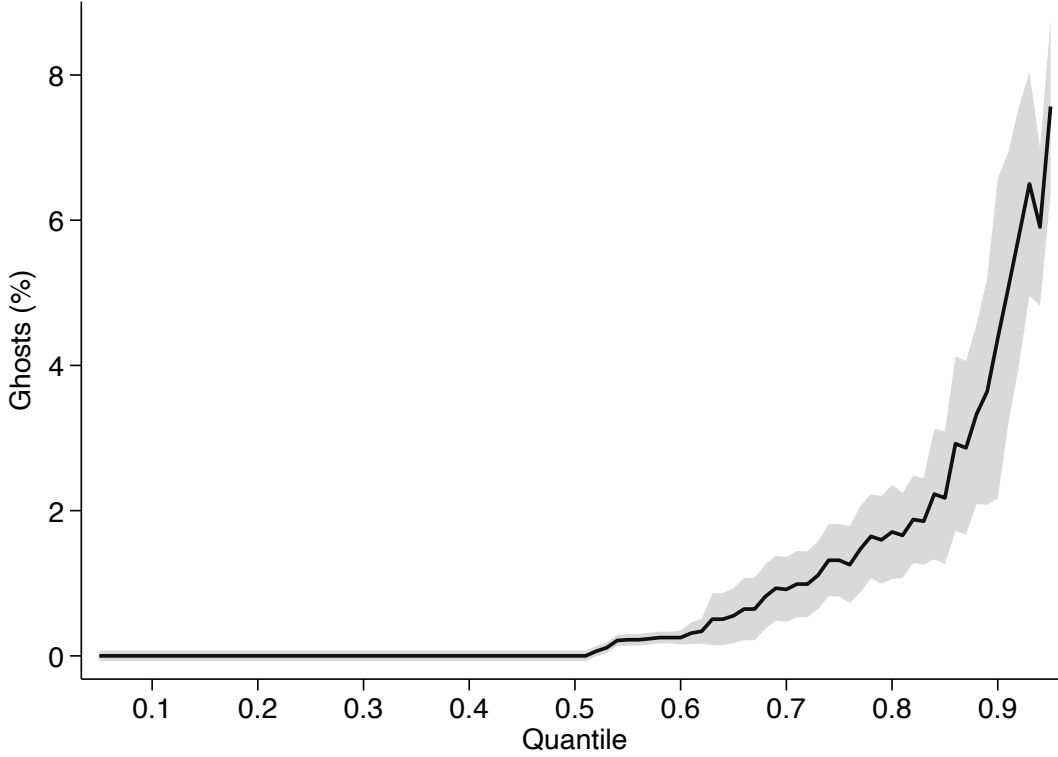
As noted, our baseline specifications are based on the optimal bandwidth from Calonico et al. (2019). We also verify the robustness of the results to the bandwidth choice in Appendix Figure C-1. This figure shows the estimated treatment effect and confidence bands and the number of observations as we vary the bandwidth from 50% to 150% of the optimal bandwidth (for the baseline linear polynomial bias-corrected coefficient with a triangular kernel). The coefficient is stable, changing only slightly and smoothly as we alter the window. Also, we only lose conventional statistical significance with bandwidths that are 60-70% as large as the optimal. Even then, the changes are more in the estimates' precision than in the estimated effects' size.

Appendix Table B-5 tests the robustness of our main results to different transformations of the dependent variable in Columns 1 to 4. When using a simple dummy for ghost incidence, the coefficient is positive (0.088) but has a large standard error of 0.057. Also, regarding raw descriptives, the median for a ghost dummy is one when the municipality is aligned and zero when not. In short, some differences are also apparent in the extensive margin, though these are not as conclusive as those in the intensive margin. This finding is also consistent with the quantile treatment effects. Considering the log of ghosts, the inverse hyperbolic sine of ghosts, or the raw level of the variable in Columns 2, 3, and 4, respectively, we again find sizable and precisely estimated positive effects of alignment on ghosts. The estimated coefficients are 0.30 (standard error: 0.10), 0.36 (standard error: 0.12), and 19.4 (standard error: 7.5), respectively.

Our school-level analysis, while allowing for direct control of school-level characteristics, overweights municipalities with more schools. As expected, the school distribution is skewed: there are 13 schools per municipality on average, but the median is 8, with an interquartile range (IQR) of 4 to 15. Similarly, student enrollment within municipalities is also skewed,

21. To further assess the representativeness of our sample, Appendix Table B-4 compares several characteristics of the schools and municipalities included in our analysis to those outside of it. This comparison reveals that no municipal-level variables statistically differ between the close and non-close races categories, while only 3 out of 13 school-level variables are different at the 10% level. Moreover, as noted, our main results are robust to controlling for these observable differences.

Figure 4: Quantile treatment effects



Notes: This figure shows the nonparametric estimator and 95% confidence intervals for local quantile treatment effects (see Frandsen, Frölich, and Melly 2012) of the effect of electing a politically aligned mayor on the fraction of fabricated students in schools located in the mayor’s municipality. All estimates are computed inside the optimal bandwidth. Observations: 4,383.

with 655 students per school on average, but a median of 315 and an IQR of 59 to 934. Because we use the fraction of students as a ratio, our estimates over-represent fake student numbers in smaller schools. While the dependent variable transformation in Column 4 helps mitigate this concern, we systematically assess the role of school and municipality size in Appendix Table B-5. These columns report our main estimates on the fraction of fabricated students, dropping small (Columns 5–7) or large (Columns 8–10) municipalities, and dropping small (Columns 11–13) or large (Columns 14–16) schools. Notably, the results remain positive, significant, and remarkably stable across all specifications, confirming that municipality and school size do not drive our findings.

Moreover, in Appendix Table B-6, we assess the robustness of our results by estimating the regressions at the municipality level, weighted by the number of schools in each municipality. The baseline coefficients range from 1.1 to 2.5 (0.39%–0.86% of one standard deviation in the share of ghost students), which is roughly in line with—if not slightly higher than—our baseline estimates.

Even though variation in our independent variable occurs at the municipality level, we take the school as the relevant unit of analysis because, as discussed in Section 3, enrollment decisions are made at that level. This choice is also more meaningful from a policy per-

spective than a municipality-level comparison that gives equal weight to all municipalities, given the substantial heterogeneity in their size and administrative scope.²² In Appendix Table B-7, we show that the main estimate remains positive and, if anything, slightly larger under alternative weighting schemes in the municipality-level regressions (using total student enrollment or total population) and in unweighted specifications.

5.3 Validation: placebo treatments, balance on covariates, manipulation, compensating differentials, and intergovernmental transfers

Our RD approach assumes that other factors besides our treatment variable vary smoothly at the threshold between an aligned candidate’s win or loss. Thus, any discontinuous change in the proportion of ghost students is only attributable to electing a mayor aligned with the governor.

Appendix Figures C-2 and C-3 present a series of falsification tests that help validate our identification assumption. Panel A of Appendix Figure C-2 estimates our standard RD analysis. The dependent variable is ghost students from the 2012 census, and the treatment variable is alignment between the mayor and governor party in each election year from 1997 to 2011. The results reveal that only links in the 2011 election, and *not* in precedent terms (1997, 2000, 2003, or 2007), predict fake students in 2012.

Panel B shows balance in a critical predetermined variable: political alignment in previous races. In particular, we look at whether alignment in the 2011 election can predict the success of aligned candidates in past elections. Reassuringly, we find no discernible robust differences between treatment and control municipalities at the threshold. The figure thus provides evidence that concurrent alignment (not prior alignment or some other omitted correlated variable) explains ghosts.

Appendix Figure C-3 looks more systematically at balance across the win-loss threshold in predetermined observables. Panel A shows the (standardized) RD coefficients for the effect of selecting an aligned mayor on predetermined school characteristics, Panel B repeats the exercise for municipal characteristics, Panel C explores baseline state capacity variables, and Panel D looks at available outcome variables measured pre-treatment. In general, we find no apparent differences in these covariates, and the point estimates are close to zero (in a few cases, there is considerable imprecision, so even though the point estimates are not significant at conventional levels, we cannot rule out relatively large differences). Recall also that controlling for observables only confirms our conclusions, increasing the precision of our estimates without substantially affecting their magnitude. Importantly, just as there are no significant differences in pre-existing learning outcomes, we also find no differences in baseline values of the set of outcomes we examine to understand the motivations for the increase in ghost students (including contracted service students, citizen complaints, and temporary school staff, among others).

We assess whether electoral manipulation—such as aligned mayors fraudulently securing narrow victories—could bias our identification strategy. If so, the sample of narrow win-

22. For example, Bogotá—the capital and largest municipality—has a population comparable to that of the smallest 70% of municipalities combined.

ners may not be comparable to narrow losers, violating our identification assumption. To test for this, Panel A of Appendix Figure C-4 presents the McCrary test (McCrary 2008), which examines the distribution of the running variable around the winning threshold. The estimated discontinuity is 0.027 with a standard error of 0.265, a precise zero, providing no evidence of a discontinuity at the threshold.²³ Panel B further validates this assumption using the test proposed by Cattaneo, Jansson, and Ma (2018), which again finds no evidence of manipulation, with a difference of -0.164 and standard error of 0.471. Similarly, the test by Bugni and Canay (2021) fails to reject the null of no manipulation at the threshold (p-value = 0.749).

In short, standard diagnostics show no manipulation or imbalance in predetermined covariates, supporting the assumption of continuity of potential outcomes at the win–loss threshold. We also emphasize that although 13% may appear wide, these are competitive, low-information races. Overall, the evidence suggests that municipalities on either side of the cutoff are comparable.

6 What drives ghosts?

So far, we have established that municipalities with mayors aligned with higher-level politicians exhibit more ghost students than those without such connections. However, aligned politicians may inflate enrollment numbers for private gain or for public benefit. In this section, we present evidence highlighting the former mechanism’s prominence.

6.1 Heterogeneous effects by school or municipal characteristics

Table 3 reports heterogeneous effects across different types of schools and municipalities.²⁴ Columns 1 to 5 explore how corruption opportunities shape the results, based on the premise that fabricating ghost students should be easier in municipalities with weaker and less transparent institutions. In Column 1, we use an index comprising several components of state capacity.²⁵ Columns 2 to 5 examine each component separately: an open government index measuring the amount of information reported by municipal governments and their standards of public management (Column 2); an integral performance index also evaluating public management standards and decision making with public funds (Column 3); a municipal transparency index that assesses the mechanisms used by the municipalities to guide and

23. There are fewer observations to the right than to the left of the threshold, which reflects the multi-party nature of Colombian elections—many unaligned candidates can narrowly defeat an aligned contender, but only one aligned candidate can win. The key validity condition remains the balance of observations near the cutoff.

24. Standard errors are sometimes smaller than in the full sample despite the reduced sample size. Notably, they decrease in one subgroup but increase in the other. This likely reflects that the former group of municipalities is more homogeneous and responds more similarly to alignment, reducing residual variance and thus yielding smaller standard errors.

25. A municipality has “high capacity” if it ranks above the sample median in *each* of its four components; otherwise it is coded “low.” This intersection rule avoids arbitrary weighting and ensures that the “high” category reflects consistently strong performance across all dimensions.

strengthen the relationship between the citizens and the State (Column 4); and an electoral security measure capturing the risk of electoral fraud (Column 5).

The results indicate that stronger institutions mitigate the impact of political alignment on ghost student numbers. All coefficients in Panel A (weaker institutions) are substantially larger than in Panel B (stronger institutions). In fact, most coefficients in Panel B are negative, and the composite index even suggests that alignment may reduce ghost student prevalence in more institutionalized municipalities. These patterns imply that the baseline RD estimates are driven mainly by schools in municipalities with weaker and less transparent institutions. Paying particular attention to those predictions that help identify corruption motives driving the results, Column 5 is noteworthy, as it focuses on whether the impacts are stronger where the clientelistic nature of political exchange is more prevalent.²⁶ The key effect is only present for municipalities with above-median risk, which aligns with the expectation of a privately-driven motivation.

Consistent with this, fake students are lower in better-managed schools within municipalities. While we lack direct measures of school-level governance, we have data on school managers' education and teacher hiring practices. In Panel B of Column 6, we show that alignment does not increase the share of fabricated students in schools managed by qualified managers (with a graduate degree). By contrast, the coefficient in Panel A is statistically significant, suggesting alignment increases ghosts under the leadership of less educated officials.

In Column 7, we test whether schools with managers in an "old regime" with no meritocratic schemes and more potential direct appointments by politicians, amplify the effect. An "old" regime (dating back to 1979) automatically promotes teachers based on tenure, education, and training. Teachers remain employed until retirement, and mayors or governors can directly appoint school principals. The "new" regime (introduced in 2002) introduces merit-based advancement, requiring teachers and aspiring principals to pass competitive exams and meet stringent service and educational criteria to advance their careers. Schools with managers of the non-meritocratic system drive the impact of alignment on ghost students. Overall, results in Columns 6 and 7 align with the idea that more accountable schools attracting the best personnel help avoid fraudulent fabrication of students.

26. While we have good measures of vote buying as one useful proxy from Fergusson, Molina, and Riaño (2018), this is only available for a subset of municipalities. Therefore, we use the risk of electoral fraud indicator of the elections of 2007 produced by the *Misión de Observación Electoral* (MOE, Mission of Electoral Oversight), the leading independent organization overseeing electoral processes in Colombia. This risk uses six dimensions of anomalies in the election: atypical level of participation, abrupt changes in participation, unusual null votes and unmarked ballots, electoral dominance, and *electoral transhumance* (the practice of illegally registering in a district other than the district where the voter lives).

Table 3: RD estimates of the effect of political alignment on ghost students – heterogeneity by baseline state capacity and school quality

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable is ghost students per school (in %)							
	Heterogeneous effect by:						
	State capacity					School quality	
	Index	Openness	Performance	Transparency	Electoral security	Principals' educ.	Merit-based
<i>A. Low</i>							
Alignment	5.160 (0.484)	2.073 (0.919)	2.046 (1.128)	2.869 (0.925)	4.191 (0.549)	2.320 (0.891)	1.631 (0.832)
Observations	1,932	1,965	1,906	2,040	2,185	1,886	2,237
Obs. in bandwidth	629	819	634	784	698	688	794
Bandwidth	0.130	0.130	0.130	0.130	0.130	0.130	0.130
<i>B. High</i>							
Alignment	-1.104 (0.590)	-0.256 (0.345)	-0.033 (0.209)	0.111 (0.285)	-1.152 (1.091)	-1.161 (0.352)	-1.552 (0.885)
Observations	2,007	1,974	2,033	1,899	1,754	2,053	285
Obs. in bandwidth	612	422	607	457	543	553	111
Bandwidth	0.130	0.130	0.130	0.130	0.130	0.130	0.130

Notes: This table reports the heterogeneous effects of electing an aligned mayor on ghost students, separately for schools with low (Panel A) and high (Panel B) levels of state capacity and school quality metrics. These metrics include an aggregated index of state capacity at the municipality level (Column 1), which is the average of four components: (i) state openness (Column 2), (ii) state performance (Column 3), (iii) state transparency (Column 4), and (iv) electoral security (Column 5). Additionally, the table includes two measures of school quality: the fraction of principals with a graduate degree (Column 6) and a binary variable equals to one if the school implemented a new regime that assigned school employees based on meritocratic schemes, as opposed to an “old” regime that allowed direct appointments by politicians (Column 7). High and low levels are defined as values above or below the variable’s median. When the variable is binary, we split the sample by each dummy value. The dependent variable is the school-level ratio of fake students to total students in 2012. *Alignment* is a binary indicator equal to one if mayoral candidate of the same party as the elected governor won the election. All results are based on equation (1), which is estimated at the school level, and includes a set of school-level, student- and teacher-level, geographic/demographic-level and electoral-level controls. See Appendix Table A-1 for the list of control variables within each group. We do not report the coefficients for these additional controls, because, unlike traditional linear regression, the non-parametric estimators implemented do not directly estimate coefficients for the control variables (see Appendix D). All estimates are bias-corrected, non-parametrically estimated, are computed within the optimal bandwidth following Calonico et al. (2019) using a triangular kernel and a linear polynomial. Standard errors are clustered at the municipality level and are robust to heteroskedasticity.

These findings suggest a privately driven motivation driving the increase in ghost students in aligned areas. However, a politician concerned for the community could still be more willing to fabricate beneficiaries in municipalities and schools where corruption is more prevalent, as this reduces the probability of receiving sanctions. Moreover, such features of municipalities and schools may be correlated with poverty and poor learning outcomes, increasing the need to inflate transfers to improve service.

6.2 Public benefit? Effects on public service quantity and quality

We now examine the implications for other relevant outcomes that help us distinguish the mechanisms motivating corruption. We first ask whether the students receive better or worse (and less or more) education. If the net effect of alignment on public service provision is positive, it suggests that some diversion of resources is for public gain. Columns 1 to 10 of Panels A and B in Table 4 examine whether students in aligned municipalities achieve better or worse academic results. Columns 2 to 4 assess if students in aligned municipalities improve the SABER 5 exams, which measure academic proficiency by the 5th grade. In columns 5-7, we examine SABER 9 test scores to assess potential impacts on middle school performance. Finally, Columns 8 to 10 focus on SABER 11, the college-level entry exam (equivalent to the US SAT). We consider scores in 2012, 2013, and 2014 (when the mayor's tenure ends). Panel A analyzes math scores, while Panel B looks at language scores. Column 1 aggregates the indicators in all Columns (which are standardized) into a single index. Using the same structure, we shift from quality to quantity in Panel C, examining the municipal enrollment rate (students enrolled in schools as a proportion of those who should be attending) as the dependent variable.

Table 4: RD estimates of the effect of political alignment on school performance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
<i>A. The dependent variable is the average math test score of students in each school on the national standardized exam</i>										
	Index average	Grade 5 (elementary)			Grade 9 (middle school)			Grade 11 (high school)		
		2012	2013	2014	2012	2013	2014	2012	2013	2014
Alignment	-0.047 (0.091)	0.014 (0.114)	-0.173 (0.094)	-0.328 (0.115)	0.193 (0.124)	-0.110 (0.106)	-0.032 (0.114)	-0.107 (0.149)	0.246 (0.126)	0.114 (0.106)
Observations	2,844	2,470	2,388	2,527	1,900	1,858	1,923	1,883	1,932	1,930
Obs. in bandwidth	441	419	406	382	296	413	428	259	347	294
Bandwidth	0.066	0.072	0.078	0.065	0.063	0.084	0.082	0.056	0.075	0.060
Mean dependent	-0.486	-0.501	-0.488	-0.486	-0.520	-0.382	-0.367	-0.287	-0.220	-0.272
S.D. dependent	0.682	0.808	0.712	0.829	0.695	0.690	0.688	0.917	0.814	0.862
<i>B. The dependent variable is the average language test score of students in each school on the national standardized exam</i>										
	Index average	Grade 5 (elementary)			Grade 9 (middle school)			Grade 11 (high school)		
		2012	2013	2014	2012	2013	2014	2012	2013	2014
Alignment	-0.226 (0.079)	-0.139 (0.098)	-0.141 (0.112)	-0.223 (0.096)	-0.123 (0.085)	-0.207 (0.072)	-0.105 (0.068)	-0.175 (0.125)	-0.284 (0.136)	-0.034 (0.126)
Observations	2,859	2,464	2,407	2,535	1,905	1,884	2,000	1,883	1,932	1,930
Obs. in bandwidth	436	406	372	400	283	312	246	303	286	322
Bandwidth	0.065	0.070	0.070	0.068	0.057	0.068	0.050	0.068	0.060	0.072
Mean dependent	-0.550	-0.567	-0.536	-0.592	-0.516	-0.550	-0.567	-0.309	-0.217	-0.349
S.D. dependent	0.686	0.711	0.687	0.747	0.785	0.698	0.720	0.923	0.905	0.948
<i>C. The dependent variable is the students' enrollment rate in each municipality</i>										
	Index average	Grade 1-5 (elementary)			Grades 6-9 (middle school)			Grades 10-11 (high school)		
		2012	2013	2014	2012	2013	2014	2012	2013	2014
Alignment	-0.079 (0.078)	-0.007 (0.069)	-0.003 (0.076)	-0.036 (0.092)	-0.093 (0.094)	-0.099 (0.101)	-0.086 (0.109)	-0.080 (0.062)	-0.098 (0.068)	-0.110 (0.072)
Observations	329	329	329	329	329	329	329	329	329	329
Obs. in bandwidth	125	137	159	140	119	117	124	114	121	125
Bandwidth	0.138	0.148	0.174	0.156	0.131	0.126	0.135	0.125	0.131	0.136
Mean dependent	0.635	0.897	0.888	0.892	0.652	0.676	0.691	0.325	0.336	0.340
S.D. dependent	0.174	0.199	0.209	0.235	0.216	0.221	0.230	0.145	0.155	0.155

Notes: This table reports RD estimates of the effect of electing a politically aligned mayor on school quality, measured by school performance and enrollment. School performance is measured using the national standardized exam that all students must take at the end of grades 5, 9, and 11. Panels A and B report the effect on the school-level average of these scores in math and language, respectively, for each grade and year. Panel C reports the impact on the enrollment rate (the fraction of people in age to be students that are actual students), which is measured at the municipality level, for grades 1-5 (elementary school), 6-9 (middle school) and 10-11 (high school) and for 2012, 2013 and 2014. Column 1 reports the effects of an aggregated index that takes the average across metric (either performance or enrollment) and years, while the rest focus on a specific metric/year. *Alignment* is a binary indicator equal to one if mayoral candidate of the same party as the elected governor won the election. All results are based on equation (1). In Panels A and B, this equation is estimated at the school level and includes controls at the school, student, teacher, geographic/demographic, and electoral levels. In Panel C, this equation is estimated at the municipal level and incorporates geographic/demographic-level and electoral-level controls. We do not report the coefficients for these additional controls, because, unlike traditional linear regression, the non-parametric estimators implemented do not directly estimate coefficients for the control variables (see Appendix D). See Appendix Table A-1 for the list of control variables within each group. All estimates are bias-corrected, non-parametrically estimated, are computed within the optimal bandwidth following Calonico et al. (2019) using a triangular kernel and a linear polynomial. Standard errors are clustered at the municipality level and are robust to heteroskedasticity.

Table 5: RD estimates of the effect of political alignment – heterogeneity by baseline state capacity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable is...	Students' performance math	Students' performance language	Students' enrollment	Electoral fraud mayor	Electoral fraud governors	Citizens' complaints	Discretionary contracts total number	Discretionary contracts value	Temporary employees	Outsourced services
<i>A. Schools in municipalities with below median state capacity</i>										
Alignment	-0.041 (0.045)	0.205 (0.190)	-0.324 (0.102)	35.490 (17.846)	23.159 (22.345)	0.030 (0.044)	426.143 (35.402)	512.639 (33.746)	11.389 (3.256)	10.712 (2.769)
Observations	1,172	1,185	166	166	166	166	78	78	1,082	1,642
Obs. in bandwidth	181	134	56	49	57	51	19	19	212	317
Bandwidth	0.057	0.050	0.138	0.123	0.140	0.127	0.107	0.110	0.083	0.070
<i>B. Schools in municipalities with above median state capacity</i>										
Alignment	-0.478 (0.071)	-1.400 (0.167)	-0.030 (0.046)	35.013 (24.451)	9.451 (24.886)	-0.240 (0.173)	21.397 (6.269)	28.093 (5.197)	9.254 (2.998)	-4.312 (1.387)
Observations	1,124	1,128	163	163	163	163	90	90	958	1,701
Obs. in bandwidth	147	130	69	62	70	66	25	28	215	226
Bandwidth	0.057	0.050	0.138	0.123	0.140	0.127	0.107	0.110	0.083	0.070

Notes: This table reports the heterogeneous effects of electing an aligned mayor on the main outcomes from Tables 1-7, separately for schools in municipalities below and above the median of our state capacity index (in Panel A and B, respectively). These metrics include the students' performance indices in math and language (Columns 1 and 2) and the enrollment rate (Column 3); the probability (in %) of electoral fraud in the subsequent election of mayors (Column 4) and governors (Column 5); the number of citizens' complaints per one thousand inhabitants involving mayors and school managers in 2012, which were presented to the general inspector's office (Column 6); the fraction (in %), both in number and value (Columns 7 and 8, respectively), of education contracts awarded through a selection method with no competition among bidders between 2012 and 2015; and indices on the fraction (in %) of temporary employees hired (Column 9) and the fraction (in %) of students taught by teachers hired through a third party (Column 10). *Alignment* is a binary indicator equal to one if mayoral candidate of the same party as the elected governor won the election. All results are based on equation (1). In Columns 1, 2, 3, 9, and 10 this equation is estimated at the school level and incorporates a set of school-level, student- and teacher-level, geographic/demographic-level, and electoral-level controls. In Columns 4-8, this equation is estimated at the municipal-level and incorporates geographic/demographic-level and electoral-level controls. We do not report the coefficients for these additional controls, because, unlike traditional linear regression, the non-parametric estimators implemented do not directly estimate coefficients for the control variables (see Appendix D). See Appendix Table A-1 for the list of control variables within each group. All estimates are bias-corrected, non-parametrically estimated, are computed within the optimal bandwidth following Calonico et al. (2019) using a triangular kernel and a linear polynomial. Standard errors are clustered at the municipality level and are robust to heteroskedasticity.

The results suggest that political alignment does not improve education—and may even harm it. Most coefficients for “Alignment” in Table 4 are negative, indicating weaker academic performance in aligned municipalities. Some individual effects are significant, and for the composite index, the decline in performance is significant for language scores, with a 0.3 standard deviation decrease.²⁷ Similarly, all enrollment coefficients are negative, though they are typically not statistically significant, individually or in the aggregate index.

Overall, Table 4 reveals a pattern of lower—or at best unchanged—educational outcomes in aligned municipalities. Political alignment does not enhance learning or school access; this aligns with private-gain motivations for corruption rather than with diversion that benefits the public.²⁸

Furthermore, the heterogeneity across levels of state capacity in Columns 1 to 3 of Table 5 further rejects the possibility of community gains from political alignment: exploring effects below (Panel A) and above (Panel B) the median level of our composite index of state capacity, we find that areas with low state capacity—where alignment lead to a higher incidence of reported fake students—drive the adverse effect on school performance to a greater extent.

A possible alternative interpretation is that aligned mayors may reallocate resources from education to other public services that constituents value more. Boas, Hidalgo, and Toral (2021), for example, show that Brazilian voters may prefer that mayors invest less in education relative to other areas. If such a mechanism were operating here, resource diversion could still produce welfare gains outside the education sector. This seems unlikely, given that central government transfers are earmarked for the education sector, thereby imposing obstacles to expenditure relocation. However, to examine this possibility, we analyze changes in health and basic service provision between the pre-alignment period (2008–2011) and the mayoral term (2012–2015). Appendix Table B-11 reports the percent change in subsidized health coverage, infant mortality, and the number of health facilities, as well as changes in piped water coverage, electricity users, and urban electricity coverage.

We find no evidence that alignment improves health outcomes or the provision of various basic services. The estimated effects on subsidized health coverage, infant mortality, and health facilities are small and not statistically different from zero. For piped water and electricity users, the point estimates are negative and large in magnitude but imprecisely estimated, while the effect on urban electricity coverage is close to zero. Overall, the pattern of estimates does not suggest that aligned municipalities compensated for misreported enrollment by expanding other valued public services.

These results are inconsistent with the interpretation that alignment-induced resource diversion generates welfare improvements outside the education sector. Taken together with the evidence of lower educational performance, the findings support the view that alignment primarily facilitates private political gain rather than reallocation toward public benefit.

27. Appendix Table B-9 reports similar findings when analyzing levels rather than changes.

28. For reference, Appendix Table B-10 shows that our baseline results on the effect of political alignment on ghost students are not specific to any particular grade level. In general, we find similarly sized effects across elementary, middle, and high schools.

6.3 Effects on fraud, citizen complaints, and contracting patterns

Politicians using public office for private gain may be more willing to engage in electoral fraud and clientelistic exchanges with voters later, investing in anticipation of future rents by manipulating elections or offering voters private rewards in exchange for political support. In Columns 1 and 2 of Table 6, we evaluate an additional dependent variable: the risk of electoral fraud in the 2015 elections. This analysis, at the municipality level, reveals that aligned municipalities are more likely to feature a higher risk of future electoral fraud. The Alignment coefficient is 45 (with a standard error of 19) for the mayor’s race and 31 (with a standard error of 19) for the governor’s election. The increase is roughly 70% of the mean for mayors and half the mean for governors.

Recall also that if a clientelistic machine captures public service delivery in aligned municipalities, we should observe more complaints involving disciplinary problems against public functionaries in these places. We explore this in Column 3 of Table 6, using data on total complaints against all functionaries in a given municipality, from the Office of the Inspector General (*Procuraduría*). Results show no additional complaints against functionaries.²⁹

In Columns 4 and 5, we study the effects of alignment on discretionary public contracts awarded through a direct selection method with no competition among bidders. Although discretionary spending is not illegal or inconvenient *per se*, it generates more opportunities for wrongdoing and might lead to more corruption (Gallego, Prem, and Vargas 2020). The coefficient in Column 4 indicates that political alignment increases the share of discretionary contracts in the education sector by 15.4 percentage points—approximately twice the mean and close to one standard deviation. Similarly, Column 5 shows that alignment raises the share of discretionary contract value by 18.7 percentage points.

We examine heterogeneity across levels of state capacity for electoral risk and complaints in Columns 4 to 6 of Table 5. The detrimental effects on mayoral elections fraud risk are similar below (Panel A) and above (Panel B) the median level of our capacity index. Nonetheless, we observe more detrimental effects in low-state capacity settings for electoral risk in government elections and citizens’ complaints. This adds to the results that align with the privately-driven intentions for corruption.

6.4 Patronage and correlated resource diversion? Effects on hiring patterns and outsourcing service

29. A critical caveat is that some areas may have lower reporting rates of public officials than others. Indeed, the most obvious expectation is that citizens will be less engaged in places with more corruption or that their complaints will be dismissed. If so, we would expect the coefficient on Alignment to be biased downwards. Note, nonetheless, that even for this result, as for the remaining outcomes, heterogeneity by levels of state capacity supports the notion that corruption does not benefit the community.

Table 6: RD estimates of the effect of political alignment on electoral fraud, complaints, and discretionary contracts

	(1)	(2)	(3)	(4)	(5)
Dependent variable is...					
	Electoral fraud		Citizens'	Discretionary contracts	
	mayors	governors	complaints	number	value
Alignment	35.321 (20.043)	26.457 (19.455)	0.006 (0.139)	17.051 (5.648)	17.261 (6.717)
Observations	309	309	309	154	154
Obs. in bandwidth	110	115	105	40	58
Bandwidth	0.125	0.131	0.119	0.108	0.140
Mean dependent	62.727	63.478	0.158	8.137	12.618
S.D. dependent	48.574	48.360	0.259	15.689	18.711

Notes: This table reports RD estimates of the effect of electing a politically aligned mayor on the probability (in %) of electoral fraud in the subsequent election of mayors (Column 1) and governors (Column 2); the number of citizen complaints per one thousand inhabitants involving mayors and school managers in 2012, which were presented to the general inspector's office (Column 3); and the fraction (in %), both in number and value (Columns 4 and 5, respectively), of education contracts awarded through a selection method with no competition among bidders between 2012 and 2015. *Alignment* is a binary indicator equal to one if mayoral candidate of the same party as the elected governor won the election. All results are based on equation (1), estimated at the municipality level, and includes a set of geographic/demographic-level and electoral-level controls. We do not report the coefficients for these additional controls, because, unlike traditional linear regression, the non-parametric estimators implemented do not directly estimate coefficients for the control variables (see Appendix D). See Appendix Table A-1 for the list of control variables within each group. All estimates are bias-corrected, non-parametrically estimated, are computed within the optimal bandwidth following Calonico et al. (2019) using a triangular kernel and a linear polynomial. Standard errors are clustered at the municipality level and are robust to heteroskedasticity.

Table 7: RD estimates of the effect of political alignment on personal hiring and outsourced services

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable is...									
			Temporary employees				Outsourced services		
	Index	Teachers 2012	Teachers 2013	Principals 2012	Principals 2013	Index	2012	2013	2014
Alignment	6.821 (2.579)	6.615 (5.990)	10.858 (6.176)	0.567 (0.307)	0.901 (0.236)	3.347 (1.537)	1.232 (1.133)	1.990 (2.109)	4.911 (1.636)
Observations	2,040	3,266	3,131	1,984	1,903	3,343	3,343	3,214	2,843
Obs. in bandwidth	427	453	428	316	300	543	583	458	392
Bandwidth	0.083	0.059	0.058	0.064	0.064	0.070	0.075	0.071	0.071
Mean dependent	11.202	31.360	33.504	0.871	0.893	5.948	5.466	7.052	7.436
S.D. dependent	13.000	36.476	36.217	3.752	3.691	13.514	13.281	15.142	16.649

Notes: This table reports RD estimates of the effect of electing a politically aligned mayor on the fraction (in %) of temporary employees hired (Columns 1–5) and the fraction (in %) of students whose education is provided by a private contractor but financed by the central government (Columns 6–9). Columns 1 and 6 report effects on aggregated indices, which take the average of the fraction of temporary employment and outsourced services, respectively, across post-electoral years (and separately for teachers and principals in the case of temporary employment), while the rest of the columns focus on each of these components. *Alignment* is a binary indicator equal to one if mayoral candidate of the same party as the elected governor won the election. All results are based on equation (1), which is estimated at school level, and includes a set of school-level, student- and teacher-level, geographic/demographic-level, and electoral-level controls. We do not report the coefficients for these additional controls, because, unlike traditional linear regression, the non-parametric estimators implemented do not directly estimate coefficients for the control variables (see Appendix D). See Appendix Table A-1 for the list of control variables within each group. All estimates are bias-corrected, non-parametrically estimated, are computed within the optimal bandwidth following Calonico et al. (2019) using a triangular kernel and a linear polynomial. Standard errors are clustered at the municipality level and are robust to heteroskedasticity.

We next investigate how political alignment shapes different contracting approaches, including patronage and opportunities for misallocating school resources. We first look at temporary employees, using an index in Column 1 of Table 7 and examining its components in Columns 2 to 5: teachers in 2012 and 2013 (Columns 2 and 3) and principals in 2012 and 2013 (Columns 3 and 4).³⁰ We see a consistent increase, with all components experiencing an increase that results in a rise of about half the mean value in the composite index.

Columns 6 to 9 instead examine outsourced services using a composite index for 2012, 2013, and 2014. Again, we observe an appreciable increase that doubles the composite index’s mean. Since temporary employees are often directly hired and contracted services are also linked to patronage, this evidence again concurs with the private-benefit motives of fake public service beneficiaries. When exploring heterogeneity by baseline levels of state capacity, Column 9 of Table 5 reports no differences for temporary employees. However, Column 10 finds that low-capacity environments drive the effect on outsourced services.

In summary, the findings on hiring patterns and service outsourcing further support the possibility that corruption is unlikely to benefit constituencies at large but rather serves only involved politicians and their allies. This adds to the findings on effective service delivery, stronger effects in areas with high electoral fraud and low state capacity, and subsequent electoral fraud.

6.5 Patronage as favor exchange

We next examine whether alignment facilitates reciprocal patronage between mayors and governors. If aligned politicians simply coordinate more effectively or share ideological goals, we would expect no systematic exchange of bureaucratic appointments. In contrast, if alignment relaxes oversight and enables mutual benefit, we should see family members of aligned politicians obtaining public positions across levels of government. Given existing evidence that bureaucratic nepotism in Colombia is associated with lower qualifications and weaker performance (Riaño 2021), such a pattern would also imply adverse consequences for public administration.

To study this, we use data on family networks among public employees and elected officials from Riaño (2021). Kinship links are drawn from Colombia’s official human resources registry (SIGEP), where public servants must disclose immediate family members. The network’s connected components identify extended kin, allowing us to trace relatives beyond the nuclear family.

We construct three binary indicators of bureaucratic nepotism. First, we measure whether mayors’ relatives hold positions within their own municipal administrations. Second, we capture whether mayors’ relatives obtain posts in the departmental government. Third, we measure whether governors’ relatives hold positions in municipal governments. For each outcome, we create year-specific indicators (2012–2015) and an indicator capturing any occurrence during the mayoral term.

Table 8 reports the results. Panel A shows that alignment increases the probability that a mayor hires a relative in the municipal administration by 19.5 percentage points, from a mean of 6.4 percent. The year-by-year estimates indicate that this pattern persists across

30. Data is not available in 2014.

the mayoral term and is particularly intense in 2013.

Nepotistic hiring by aligned mayors within their own municipal administrations underscores the broader pattern of appointments that generate private rents with likely costs for citizens. More importantly, when we look at vertical ties, Panel B shows cross-level patronage from governors to mayors. Alignment increases the likelihood that a mayor’s relative is employed in the departmental bureaucracy by 31.7 percentage points relative to a baseline of 9.6 percent. This effect is large and stable across years, with coefficients ranging from 25.0 to 38.1 percentage points.

Panel C shows the reciprocal pattern: alignment increases the probability that a governor’s relative is employed in the municipal administration by 5.0 percentage points, relative to a mean of 3.3 percent. Although smaller in magnitude, this effect is statistically significant and again persistent across years.

Taken together, these findings indicate coordinated patronage between aligned mayors and governors. The consistent cross-placement of relatives supports the idea that alignment facilitates mutual benefit rather than merely cooperation for service delivery. Combined with evidence from Riaño (2021) that such nepotism is associated with weaker bureaucratic performance, the results suggest that alignment undermines administrative quality.

6.6 Political survival: future party and politician prospects

Our findings point to the entrenchment of a corrupt clientelistic machine that captures and distorts public service delivery. The evidence on electoral fraud is especially relevant because, ideally, a well-functioning democracy would punish corrupt politicians and functionaries by not re-electing them. However, with clientelism and electoral corruption, these types may persist in power by targeting key voters or through outright electoral fraud. In particular, they might also have more resources to invest in future campaigns, given their greater appropriation of resources relative to non-aligned politicians. This discussion leads to the one remaining ingredient of our theoretical predictions that we have yet to examine: the future electoral prospects of aligned mayors. While aligned mayors should outperform unaligned ones, whether the corrupt fabrication is for a politician’s private benefit or to favor the community, revising this issue is particularly important to determine whether these corrupt politicians can persist in power.

In Table 9, Panel A, we present a stacked, close-election RD (PCRD) at the municipality–party–election level. The running variable is the aligned candidate’s vote margin in election at cycle t and outcomes are for the same municipality–party in the next cycle $t + 1$ (party runs; party wins; wins conditional on running). We use local linear regressions with a triangular kernel, MSE-optimal bandwidths, robust bias-corrected inference, and include municipality and election-cycle fixed effects. Alignment has no detectable effect on the party’s unconditional probability of winning the next election. However, conditional on running, we find an increase in win probability of close to 7 percentage points (coefficient of 0.0724, standard error 0.0359) from a base mean of 23.2%. Given that parties in aligned municipalities field more candidates in subsequent elections, this could reflect the entrance of marginal, lower-qualified candidates, and hence the importance of conditioning on running. Also, the results with party vote share as the dependent variable (not shown in the Table) produce an Alignment coefficient of 0.0979 (standard error 0.0131). Taken together, these

results suggest that alignment helps sustain party presence in local electoral competition and translates into improved electoral performance at the ballot box.

In Panel B, we switch attention from parties to politicians. We present RD regressions tracing individuals involved in close mayoral elections from 1997 to 2011 and compare winners' and losers' subsequent electoral careers (in other offices). The dependent variables are dummies for participating, winning, and winning conditional on running in any future local, regional, or national election. While this specification no longer exploits exogenous variation in alignment, it still allows us to explore the differential career trajectories of aligned versus unaligned close winners. In each case, we report the effect of a win on unaligned and aligned winners. The findings in Columns 1 and 2 reveal that candidates in close elections have a baseline probability of running for elected offices in the future of 40%. Unaligned winners are likelier to run again (coefficient 0.06, standard error 0.03). The magnitude for aligned winners is much smaller, negative, and not significantly different from zero (coefficient -0.014, standard error 0.05). While this may suggest that aligned winners are not more successful than unaligned ones, Columns 3 to 6 reveal the opposite: out of a baseline unconditional probability of winning future elections for narrow competitors of 17.9%, aligned winners can increase it sizably, by 5.4 percentage points (coefficient 0.054, standard error 0.033) while unaligned winners observe no change (coefficient -0.003 , standard error 0.022). Consistent with these findings, aligned winners are much more likely to win conditional on running (Column 6), whereas unaligned ones are not (Column 5).

In Panel C, the analysis focuses on the governors' career perspectives. For incumbent governors (elected in 1997, 2000, 2003, 2007, and 2011), we examine their performance in subsequent elections, whether at the municipal, regional, or national level, following the next regional election. We next test whether the triumph of aligned mayoral candidates helps predict governors' electoral success. To capture random variation in political alignment, the main explanatory variable measures the number of close races won by mayoral candidates aligned with the governor in their department. The regressions include period-of-governor fixed effects. To ensure that the overall electoral competitiveness or alignment patterns between candidates and governors do not drive the results, we also include the departmental percentage of total and close municipal elections involving aligned and non-aligned candidates. Although the results are not always statistically significant at conventional levels due to the small sample, all coefficients are positive and suggest the potential influence of political networks and local support on a governor's future electoral success. Specifically, the coefficient of 0.018 (standard error 0.01) suggests that when governors have the luck that an aligned additional mayor wins a close election in their department, they increase their winning chances in subsequent contests by 1.8 percentage points, highlighting the importance of local support and political networks in shaping political outcomes. Winning conditional on running also increases, by 2.4 percentage points over a mean of 0.56 (standard error 0.015).

Table 9 thus raises concerns about electoral accountability, since corrupt politicians are more likely to remain in power, reinforcing a cycle of corruption. In municipalities with weak institutions, aligned mayors illegally capture more central government resources, causing net harm to the local economy and society. Yet, despite these negative consequences, they enjoy better long-term political prospects, further entrenching corruption and clientelism.

6.7 Partisan links versus other traits of aligned types

As noted in the introduction, our design is a Politician-characteristic RD (PCRD, Marshall 2022), which does not compare winners to losers (say, outcomes for the politician who wins the election relative to outcomes of his runner-up). Instead, it compares municipalities managed by winners of different types: an aligned narrow winner or an unaligned narrow winner. This comparison thus identifies the effect of electing the type “aligned politician.” Our complementary outcomes suggest that such a partisan link likely plays a role in the effects we observe. Yet, these may also reflect other differences between aligned and non-aligned politicians. Also, Marshall (2022) notes that these candidate-level characteristics may be “compensated” precisely to ensure elections remain close between candidates that differ in alignment. For instance, the sample of aligned politicians facing close elections may have relatively low talent or motivation.

Estimating the *sole* effect of alignment net of these “compensating differentials” with a PCRD requires strong assumptions: either that alignment does not affect candidate vote shares or that no other compensating differentials affect the outcome. Also, interpreting the balancing tests on candidate characteristics is not straightforward since these differences respond to restricting to a close election.

In Appendix Figure C-5, we follow Marshall (2022) to bound and correct the magnitudes of the estimated effect using the observable compensating differentials. Marshall (2022) proposes a corrected estimator given by:

$$\hat{\tau}^{\text{corr}} = \hat{\tau} - \sum_k \hat{\gamma}_k \hat{\delta}_k,$$

where $\hat{\tau}$ is the uncorrected (baseline) estimate, $\hat{\delta}_k$ is the RD estimator for the effect of Alignment on each observable compensating differential k – candidate-level characteristics, theoretically distinct from mayor-governor partisan affinity, that guarantee closed elections –, and $\hat{\gamma}_k$ is the estimated LATE of each k on ghost students at the discontinuity. Since we lack identification strategies for our compensating differentials, to capture plausible values of $\hat{\gamma}_k$, we estimate a regression of ghost students on potential compensated differentials and use these OLS estimators to create a grid of plausible values for each $\hat{\gamma}_k$. In Panel A, we show the distribution of the corrected coefficients, $\hat{\tau}^{\text{corr}}$, generated by scaling each estimated γ_k by 0.5, 1, and 1.5.³¹ The distribution of corrected coefficients is centered at 1.44, slightly larger than our baseline estimate of 1.40, suggesting that our baseline results are very similar—and, if anything, somewhat *downwardly* biased—when accounting for the observable compensating differentials. In the top portion of Panel B, for reference, we display the values of $\hat{\gamma}_k$ estimated by OLS. In the lower part, we calculate the value of each $\hat{\gamma}_k$ that would nullify the corrected estimate of the Alignment effect on ghost students. Most of the coefficients would have to be implausibly large and, in some cases, change signs to make $\hat{\tau}^{\text{corr}}$ equal to zero.

Finally, mayor-governor alignment could affect many policies beyond those in the education sector. Our estimates should be interpreted with this caveat in mind. We already documented no effects on health outcomes and various public services. Given the extensive

31. Since we have 11 observable compensated differentials and three possible values, this procedure amounts to $3^{11} = 177,147$ ways of estimating $\hat{\tau}^{\text{corr}}$.

literature on political alignment, a crucial question is whether alignment leads to significant changes in subnational transfers. Crucially, Colombia’s fiscal decentralization gives a limited role to direct transfers from departments to municipalities. This feature appears in Appendix Table B-8, where we report the mean and standard deviations of average municipal tax and transfer revenue (in logs and as a percentage of total revenue). While national transfers to municipalities averaged 78% of total municipal revenue, all non-national transfers accounted for, on average, only 0.98%. Moreover, we do not find significant changes when examining whether municipal-departmental alignment influences these transfers. Thus, while we cannot rule out that our effects respond to other issues besides educational policies that vary when local mayors align with governors, the potential role of the main confounder, intergovernmental transfers, is minimal.

Table 8: RD estimates of the effect of political alignment on patronage and mayor-governor family ties

Dependent variable is...	By year				
	Any (2012–2015)	2012	2013	2014	2015
	(1)	(2)	(3)	(4)	(5)
<i>A: Mayor’s family member in own mayor’s office</i>					
Alignment	0.195 (0.0893)	0.0651 (0.0339)	0.208 (0.0853)	0.0633 (0.0372)	0.0660 (0.0382)
Observations	329	329	329	329	329
Obs. in bandwidth	109	85	96	92	96
Bandwidth	0.119	0.096	0.108	0.102	0.108
Mean dependent	0.064	0.012	0.021	0.011	0.010
S.D. dependent	0.246	0.108	0.144	0.104	0.102
<i>B: Mayor’s family member in governor’s office</i>					
Alignment	0.317 (0.145)	0.381 (0.138)	0.259 (0.128)	0.254 (0.128)	0.250 (0.125)
Observations	329	329	329	329	329
Obs. in bandwidth	94	75	109	110	117
Bandwidth	0.105	0.083	0.117	0.121	0.126
Mean dependent	0.096	0.080	0.064	0.064	0.103
S.D. dependent	0.296	0.273	0.246	0.245	0.305
<i>C: Governor’s family member in mayor’s office</i>					
Alignment	0.0498 (0.0229)	0.0301 (0.0177)	0.0651 (0.0372)	0.0651 (0.0372)	0.0129 (0.0150)
Observations	329	329	329	329	329
Obs. in bandwidth	92	94	92	92	83
Bandwidth	0.103	0.105	0.103	0.103	0.091
Mean dependent	0.033	0.011	0.011	0.011	0.024
S.D. dependent	0.179	0.103	0.104	0.104	0.154

Notes: This table reports RD estimates of the effect of electing a politically aligned mayor on the probability of observed family ties in public offices during their term. The dependent variables are an indicator equal to 1 if at least one family member of the mayor is employed or contracted in the municipality’s mayor’s office (Panel A); an indicator equal to 1 if at least one family member of the mayor is employed or contracted in the department governor’s office (Panel B); and an indicator equal to 1 if at least one family member of the governor is employed or contracted in the municipality’s mayor’s office (Panel C). Columns present the overall post-election effect and its year-by-year breakdown: “Any (2012–2015)” reports the effect when the indicator occurs in any post-election year, and the four subsequent columns report the estimates for 2012, 2013, 2014, and 2015 separately. *Alignment* is a binary indicator equal to one if mayoral candidate of the same party as the elected governor won the election. All results are based on equation (1), estimated at the municipality level, and includes a set of geographic/demographic-level and electoral-level controls. We do not report the coefficients for these additional controls, because, unlike traditional linear regression, the non-parametric estimators implemented do not directly estimate coefficients for the control variables (see Appendix D). See Appendix Table A-1 for the list of control variables within each group. All estimates are bias-corrected, non-parametrically estimated, are computed within the optimal bandwidth following Calonico et al. (2019) using a triangular kernel and a linear polynomial. Standard errors are clustered at the municipality level and are robust to heteroskedasticity.

Table 9: Party and politicians' prospects – municipal incumbency advantage analysis

Dependent variable is...	Run in next election		Win in next election		Win in next election/Run	
	(1)	(2)	(3)	(4)	(5)	(6)
<i>A: Party future prospects</i>						
Alignment	0.115 (0.0363)		0.00103 (0.0292)		0.0724 (0.0359)	
Observations	1,882		1,882		1,113	
Obs. in bandwidth	552		588		297	
Bandwidth	0.091		0.098		0.076	
Mean dependent	0.643		0.228		0.232	
S.D. dependent	0.480		0.420		0.423	
<i>B: Candidate future prospects</i>						
	unaligned	aligned	unaligned	aligned	unaligned	aligned
Win	0.064 (0.030)	-0.014 (0.047)	-0.003 (0.022)	0.054 (0.033)	-0.023 (0.034)	0.199 (0.039)
Observations	15,448	2,988	15,448	2,988	5,567	1,208
Obs. in bandwidth	3,094	853	3,271	849	1,958	381
Bandwidth	0.051	0.064	0.055	0.064	0.090	0.069
Mean dependent	0.399	0.417	0.164	0.179	0.411	0.412
S.D. dependent	0.490	0.493	0.370	0.384	0.492	0.493
<i>C: Governor future prospects</i>						
Governor's Alignments	0.012 (0.011)		0.018 (0.010)		0.024 (0.015)	
Observations	152		152		62	
Mean dependent	0.409		0.227		0.556	
S.D. dependent	0.493		0.420		0.501	

Notes: This table summarizes the party and politicians' future prospects for all local elections in Colombia between 1997 and 2011 (i.e., 1997, 2000, 2003, 2007, and 2011) using nationwide data on three outcomes: running in the subsequent election, winning the next election, and winning the next election conditional on running. Panel A reports RD estimates of the effect of electing a politically aligned mayor in all mayoral elections. *Alignment* is a binary indicator equal to one if mayoral candidate of the same party as the elected governor won the election. Panel B estimates the incumbency advantage at the candidate level in all mayoral elections. Incumbency advantage represents the effect of (barely) winning the (initial) election. *Win* is a binary indicator equal to one if the party or candidate won the election. These estimates are calculated separately for cases where the party does not coincide with the governor's party (odd columns) and for cases where it does (even columns). Panel C estimates the incumbency advantage at the gubernatorial candidate level across all departmental elections. *Governor's Alignment* is defined as the number of mayoral candidates within the department who win close elections and belong to the same party as the elected governor. Panels A and B estimates are bias-corrected, non-parametrically estimated using a triangular kernel and a linear polynomial, and computed within the optimal bandwidth following Calonico et al. (2019). In Panel A and B, the regression also includes a complete set of municipality and year fixed effects. We do not report the coefficients for these additional controls, because, unlike traditional linear regression, the non-parametric estimators implemented do not directly estimate coefficients for the control variables (see Appendix D). Estimates in Panel C are based on a linear regression that includes year fixed effects and controls for both the departmental percentage of total and close municipal elections involving aligned (and non-aligned) candidates. We report the coefficients associated to the extended set of controls in these linear regressions in Appendix Table D-1. Standard errors in Panels A and B are clustered at the municipality level and are robust to heteroskedasticity. Standard errors in Panel C are robust to heteroskedasticity.

7 Conclusion

This paper investigates a particularly prevalent form of corruption in the public sector: the fabrication of fake beneficiaries to increase government transfers for potential diversion. Our focus is on the impact of alignment with higher-level politicians on local politicians' incentives to engage in this practice. In settings where political alignment is highly clientelistic, politicians are likely to inflate beneficiary lists to divert resources for personal economic or political gain. Instead, when there is pressure for accountability, politicians may fabricate beneficiaries to secure more funds and better serve the actual beneficiaries.

These two motivations have distinct observable predictions: while both agree in predicting that aligned politicians should produce more “ghost” public service users and should enjoy better future electoral prospects, predictions on the use of patronage and discretionary contracting, favor exchange, citizen complaints, impacts on effective service delivery, subsequent electoral fraud, and prevalence in areas with a history of clientelistic features help distinguish the dominant motivation.

This distinction is critical for designing effective anti-corruption policies and assessing efficiency and equity implications. The privately driven case suggests significant inefficiency costs, as diverted resources could otherwise yield substantial social returns. The community-driven case is more nuanced and, in extreme cases, may resemble corruption as grease on the wheels (Banerjee 1997), where misappropriated funds ultimately reach intended beneficiaries.

Reality likely lies between these two extremes. Politicians may be partly privately driven and partly community driven, and both motivations may be complementary. Indeed, local politicians and bureaucrats might seize or misallocate part of the extra money for personal economic or political gain, while also using some resources for beneficiaries. This approach could satisfy voters and make it easier to evade corruption.

Focusing on the case of ghost students in Colombia, a country exhibiting prevalent clientelism, we find evidence consistent with private-gain motivations driving this form of corruption. Politicians do not break the rules to improve their constituencies' welfare. Indeed, when estimating the impacts of electing a local municipal mayor aligned to a regional departmental governor, we observe contracting practices consistent with patronage and favor exchange, no evidence of improvement in service delivery, increases in subsequent electoral fraud, and more substantial effects in areas with a history of clientelistic practices and other measures of institutional weakness.

These findings reveal a substantial failure of electoral accountability, as political incentives fail to effectively constrain the diversion of public resources for personal gain. Moreover, the harmful impacts of alignment are most pronounced where institutions are weaker — precisely the places that would benefit most from greater accountability. There is also no indication these practices will erode, as aligned politicians enjoy improved electoral prospects. The results thus uncover a key and persistent source of widening inequalities.

Our findings are relevant to the policy debate on improving educational outcomes in developing countries, where substantial money has increased enrollment with only meager impacts on learning. Since the 1980s, spending has doubled on average in Latin America and Sub-Saharan Africa, tripled in the Middle East, increased more than five times in East Asia, and risen almost eight times in South Asia (Glewwe et al. 2011). Consequently, formal schooling has experienced a dramatic expansion globally, achieving nearly universal coverage

of primary education and showing a significant increase in post-primary enrollment.³² However, the expansion and reach of formal education systems have not been accompanied by proportional progress in education quality (Mbiti 2016), especially outside richer countries and households.³³ Our paper emphasizes that deep-rooted political incentives may explain such disappointing outcomes.³⁴

32. The data collected in the World Bank's 2018 World Development Report (The World Bank 2018) shows that, between 1980 and 2010, net enrollment rates in primary education reached more than 90% in East Asia & Pacific (99.6%), Latin America & Caribbean (99.5%), Middle East & North Africa (98.0%), South Asia (97.5%), Sub-Saharan Africa (90.1%). Moreover, during the same period, enrollment in secondary education more than doubled in all of these developing regions: in East Asia & Pacific, net secondary enrollment rates jumped from 39.8% to 80.6%, in Latin America & Caribbean from 28.1% to 81.9%, in the Middle East & North Africa from 34.2% to 66.8%, in South Asia from 22.5% to 57.1%, and in Sub-Saharan Africa from 15.9% to 41.9%.

33. According to the results from the OECD's Programme for International Student Assessment (PISA), students' median score in 23 of the 45 countries in the 2015 study performed below the established minimum proficiency threshold in mathematics (Pisa 2016). More importantly, this cross-country assessment shows that learning is substantially lower in low- and middle-income countries and for children from most vulnerable households (The World Bank 2018).

34. See also Callen et al. (2023) for the case of health services.

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Online Appendix

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A Variable definition and sources

Table A-1: Variables and sources

Variable	Description	Source
Main Variables		
Ghosts	Ratio of false students to total students in 2012.	Ministry of Education, 2012 Audit
Alignment	Binary variable equals one if mayoral candidate of the same party as the elected governor won the election in 2011.	Pachón and Sánchez-Torres (2014)
Other school-level outcomes		
Outsourced service	Fraction of students whose education is provided by a private contractor but financed by the central government. Data used for the period 2010-2014.	Ministry of Education
School performance	Average math and language score of students in each school on the national standardized exam. Data used for the period 2010-2014.	Colombian Institute for the Evaluation of Education (<i>ICFES</i>)
Temporary employees	Fraction of teachers (or principals) hired under provisional appointment. Data used for the period 2011-2013.	Ministry of Education
Municipal-level outcomes		
Citizen's complaints	Total complaints per capita against public servants. Data used for the period 2011-2012.	Inspector General (<i>Procuraduría</i>)
Discretionary contracts	Fraction, both in number and value, of education contracts awarded through a selection method with no competition among bidders. Following Gallego, Prem, and Vargas (2020), we define contracts as those that are awarded using public tenders, auctions, selection based on merits, or a special regime and focus only in municipal contracts. Data used for the period 2012-2015.	SECOP (<i>Sistema Electrónico para la Contratación Pública</i>)
Electoral fraud	Binary variable equals one if municipality had risk of electoral fraud in the mayoral or gubernatorial elections. Electoral risk is based on five dimensions of anomalies in the election: atypical level of participation, abrupt changes in participation, unusual null votes and unmarked ballots, and electoral dominance. Data used for the 2007 and 2015 local elections.	Misión de Observación Electoral (MOE) (2018)
School enrollment	Fraction of students enrolled in schools as a proportion of the population of school-age children. Data used for the period 2012-2014.	Ministry of Education
Secondary outcomes		
Tax revenue	Municipality tax revenue per capita expressed either in logs or as a fraction of total revenue. Data used for the period 2011-2014.	National Planning Department (<i>Departamento Nacional de Planeación</i>)

Continued on next page

Table A-1 – Variable definition and sources (continues from previous page)

Transfers	Transfer per capita to the municipality, expressed either in logs or as a fraction of total revenue. We use total transfers, transfers from the national government, and transfers from non-national entities (<i>i.e.</i> , departmental transfers). Data used for the period 2011-2014.	National Planning Department (<i>Departamento Nacional de Planeación</i>)
School-level controls		
Employees tertiary	Share of teachers (or principals) in 2011 with tertiary education.	Ministry of Education
Students disabilities	Share of students in 2011 with physical disabilities.	Ministry of Education
Students full time	Share of students in 2011 enrolled in full-time school.	Ministry of Education
Students high school	Share of students in 2011 in high-school (grades 8-11).	Ministry of Education
Students low income	Share of students in 2011 in strata 0, 1, and 2. Neighborhoods are classified into six strata, ranging from zero to six, with six being the wealthiest and zero the poorest.	Ministry of Education
Students men	Share of male students in 2011.	Ministry of Education
Students minors	Share of students in 2011 who are underage.	Ministry of Education
Students outsourced	Share of students in 2011 whose education is provided by a private contractor but financed by the central government.	Ministry of Education
Students rural	Share of students in 2011 living in rural areas.	Ministry of Education
Total students	Total number of students registered in the school in 2011.	Ministry of Education
Total teachers	Total number of teachers registered in the school in 2011.	Ministry of Education
Geographic/demographic controls		
Area (km2)	Area of the municipality in square kilometers.	Acevedo, Bornacelly Olivella, et al. (2014)
Dist. to Bogota	Distance in kilometres from the centroid of a municipality to Bogota, the capital city of Colombia.	Acevedo, Bornacelly Olivella, et al. (2014)
Dist. to dep. capital	Distance in kilometres from a municipality centroid to the capital of the respective department.	Acevedo, Bornacelly Olivella, et al. (2014)
Elevation	Average geographical elevation of the municipality in meters above the sea level.	Acevedo, Bornacelly Olivella, et al. (2014)
Notaries	Number of notaries in the municipality.	Acevedo, Bornacelly Olivella, et al. (2014)
Total Population	Municipality's total population.	Acevedo, Bornacelly Olivella, et al. (2014)
Urban Population	Municipality's total urban population.	Acevedo, Bornacelly Olivella, et al. (2014)
Electoral controls		

Continued on next page

Table A-1 – Variable definition and sources (continues from previous page)

Council votes	Share of votes for the governor's party in the 2011 municipal council.	Pachón and Sánchez-Torres (2014)
Mayor characteristics	Binary variable equals one if the elected mayor in 2011 is White, Black, Asian, or female.	Ruiz (2017)
Mayor party	Binary variables equal one if the mayor's party belongs to one of the top five parties with the most elected mayors in 2011: <i>Partido Liberal</i> , <i>Partido Verde</i> , <i>Partido de la U</i> , <i>Partido Conservador</i> , and <i>Movimiento ASI</i> .	Pachón and Sánchez-Torres (2014)
Baseline state capacity		
Electoral security	One minus <i>Electoral fraud</i> in 2007. The MOE <i>Electoral fraud</i> index aggregates indicators of atypical electoral behavior that signal potential integrity risks such as unusual participation patterns, anomalies in invalid or unmarked ballots, and patterns consistent with restricted competition or dominance. Taking one minus this score yields a higher value for safer electoral environments.	Misión de Observación Electoral (MOE) (2018)
State capacity index	Binary variable equals one if the average of <i>Electoral security</i> , <i>State openness</i> , <i>State performance</i> , and <i>State transparency</i> is above its median.	
State openness	<i>Indice de gobierno abierto</i> . It measures the amount of information reported by municipal governments and the status in the implementation of standards that seek to promote better public management. The index is measured in 2009 and based on four components: systems of internal accountability, information management, visibility in contracting, and transparency in the accountability process. It captures the extent to which municipal governments comply with open-government and anti-corruption standards by disclosing information and enabling oversight.	Inspector General (<i>Procuraduría</i>)
State performance	<i>Indice de desempeño integral</i> . It evaluates four (equally-weighted) components of public management and decision making in the use of public resources in 2009: (i) <i>efficacy</i> —progress toward development goals; (ii) <i>efficiency</i> —outputs achieved relative to inputs in core services (education, health, water and sanitation); (iii) <i>legal compliance</i> —observance of statutory rules for execution and reporting of intergovernmental transfers (SGP); and (iv) <i>administrative and fiscal management</i> —organizational capacity and fiscal performance.	National Planning Department (<i>Departamento Nacional de Planeación</i>)
State transparency	Component of <i>State openness</i> that evaluates the mechanisms used by municipalities to guide and strengthen the relationship between citizens and the state. It evaluates whether the municipal administration has procedures that guide and strengthen the relationship between citizens and the state and whether it reports to the community, regularly and with adequate quality, on plans, execution, contracting, and results.	Inspector General (<i>Procuraduría</i>)
Baseline school quality		
Merit-based	Binary variable equals one if the school implemented a new regime that assigned school employees based on meritocratic schemes, as opposed to the “old” regime that allowed direct appointments by politicians following Decree 2277 of year 1979.	Ministry of Education
Principals' educ.	Binary variable equals one if the fraction of principals in the school with graduate education is above its median in 2011.	Ministry of Education

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Table A-1 – Variable definition and sources (continues from previous page)

Incumbency-advantage analysis		
Run in next election	Binary variable equals one if the party or candidate participated in an election.	Pachón and Sánchez-Torres (2014)
Vote share in next election	Vote share for the party as a proportion of total votes in the next election. If the party is not running, the vote share is set to zero.	Pachón and Sánchez-Torres (2014)
Win in next election	Binary variable equals one if a candidate won the next election.	Pachón and Sánchez-Torres (2014)
Win in next election Run	Binary variable that equals one if a candidate won the next election, in the sample of observations where the respective candidate participated in the election.	Pachón and Sánchez-Torres (2014)

B Appendix tables

Table B-1: Privately-driven versus community-driven cases of fake public service beneficiaries

Privately-driven case	Community-driven case		
<u>A. Core assumption</u>			
Politicians fabricate beneficiaries for personal gain	Politicians fabricate beneficiaries for public benefit		
<u>B. effects of electing an aligned mayor</u>			
1. Improved coordination of necessary actions	1. Improved coordination of necessary actions		
2. Funds are token for exchange of political favors	2. Improved credit-claiming		
3. Funds for clientelistic vote buying	3. Funds for better service		
<u>C. Observable predictions</u>			
Variable	Privately- driven case	Community- driven case	Finding
Fake beneficiaries	↑	↑	↑
1. Patronage and discretionary contracting	↑	=	↑
2. Electoral prospects	↑	↑	↑
2. Citizen complaints	↑	=/↓	=
2. Favor exchange	↑	=/↓	↑
3. Effective service	= / ↓	↑	↓
3. Fake beneficiaries in high electoral fraud areas	↑	=	↑
3. Electoral fraud	↑	=	↑

Notes: This table provides a structured summary of the core assumptions, effects of electing an aligned mayor, and expected observable outcomes under two extreme assumptions about the motivations of politicians to fabricate beneficiaries. While some predictions—such as an increase in fake beneficiaries and improved electoral prospects—are common to both, key differences help distinguish them. The privately-driven case leads to greater public sector patronage, more citizen complaints, more favor exchange, and higher electoral fraud, without improving service delivery. In contrast, the community-driven case predicts better service provision, with fewer signs of political misconduct. ↑, =, ↓ summarizes what each case predicts and the evidence the paper finds (an increase, no change, or decrease in the corresponding observable phenomena).

Table B-2: Summary statistics – School level variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Any race			Aligned close races			Non-aligned close races		
	Obs.	Mean	S.D.	Obs.	Mean	S.D.	Obs.	Mean	S.D.
<i>A. Main variables</i>									
Alignment	4,383	0.31	0.46	783	1.00	0.00	555	0.00	0.00
Ghost	4,383	1.35	4.77	783	1.52	4.53	555	1.12	4.74
Outsourced services	4,383	8.81	18.26	783	6.44	16.43	555	5.88	12.72
School perf. language	3,195	-0.53	0.70	564	-0.48	0.74	489	-0.63	0.61
School perf. math	3,183	-0.48	0.70	564	-0.41	0.74	484	-0.54	0.62
Temporary employees	2,680	9.60	11.48	512	8.86	11.93	442	7.88	9.58
<i>B. Controls</i>									
Principals tertiary	3,945	1.67	3.33	732	1.80	2.95	513	1.73	2.31
Students disabilities	4,344	1.42	4.05	771	1.74	5.16	552	1.86	5.02
Students full time	4,344	35.62	46.88	771	20.79	39.35	552	23.59	41.02
Students high school	4,344	27.48	24.87	771	29.18	25.55	552	32.27	22.34
Students low income	4,344	97.86	6.51	771	97.91	6.85	552	98.49	4.51
Students men	4,344	52.11	8.07	771	51.87	8.67	552	52.02	6.93
Students minors	4,344	93.89	8.92	771	92.81	10.07	552	92.83	8.22
Students outsourced	4,344	10.01	25.43	771	5.12	17.06	552	7.81	19.45
Students rural	4,344	73.07	43.42	771	73.15	43.41	552	73.96	42.53
Teachers tertiary	3,945	1.78	8.40	732	2.31	11.77	513	1.87	8.10
Total students	4,344	654.99	830.23	771	725.28	881.39	552	742.79	750.15
Total teachers	3,945	23.41	27.75	732	24.64	28.00	513	27.72	25.82
<i>C. Baseline state capacity and school quality</i>									
Electoral security	4,383	0.43	0.49	783	0.30	0.46	555	0.60	0.49
Merit-based	2,619	0.11	0.32	510	0.15	0.35	431	0.10	0.30
Openness	4,383	0.50	0.50	783	0.27	0.44	555	0.43	0.49
Performance	4,383	0.50	0.50	783	0.56	0.50	555	0.41	0.49
Principals' educ.	2,619	0.62	0.49	510	0.68	0.47	431	0.58	0.49
State capacity index	4,383	0.51	0.50	783	0.40	0.49	555	0.59	0.49
Transparency	4,383	0.50	0.50	783	0.35	0.48	555	0.39	0.49

Notes: The table reports summary statistics for school-level variables for schools in municipalities where there is a race between a candidate from the same party as the governor and a candidate from a different party (Columns 1–3), for schools in municipalities with close races where the mayor belongs to *the same* party as the governor (Columns 4–6), and for schools in municipalities with close races where the mayor belongs to *a different* party than the governor (Columns 7–9). Panel A reports the main independent variable, *Alignment*, which is a binary variable that equals one if the mayoral candidate from the same party as the elected governor won the election, along with the outcome variables, including *Ghosts*, which represents the fraction (in %) of reported students that are fake. Panel B reports the set of pre-determined school-level controls. Panel C reports baseline measures of state capacity and school quality. Close races are defined using the optimal bandwidth in our baseline specification (13.0%). See the text and Appendix Table A-1 for more details and definitions.

Table B-3: Summary statistics – Municipality level variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Any race			Aligned close races			Non-aligned close races		
	Obs.	Mean	S.D.	Obs.	Mean	S.D.	Obs.	Mean	S.D.
<i>A. Main variables</i>									
Alignment	332	0.30	0.46	48	1.00	0.00	61	0.00	0.00
Ghost	332	1.27	2.35	48	1.78	4.35	61	0.85	1.22
Citizens' complaints	332	0.19	0.31	48	0.14	0.29	61	0.16	0.23
Disc. contracts number	170	19.25	23.30	24	10.01	18.08	29	11.34	16.75
Disc. contracts value	170	16.85	21.65	24	12.00	21.02	29	9.20	15.43
Electoral fraud governors	332	64.76	47.84	48	60.42	49.42	61	65.57	47.91
Electoral fraud mayors	332	68.67	46.45	48	60.42	49.42	61	62.30	48.87
School coverage	329	0.62	0.16	48	0.61	0.15	61	0.64	0.19
<i>B. Geographic/demographic-level controls</i>									
Area (km2)	332	1,155.60	3,916.22	48	2,229.48	9,397.88	61	975.16	1,801.61
Dist. to Bogota	332	391.13	190.09	48	371.68	162.69	61	419.08	213.76
Dist. to dep. capital	332	82.01	61.55	48	84.74	63.52	61	74.65	46.26
Elevation	332	709.66	1,518.97	48	768.60	713.19	61	470.57	543.21
Notaries	329	0.75	0.77	48	0.90	1.29	61	0.61	0.49
Total population	332	36.31	62.79	48	45.05	97.94	61	24.79	21.38
Urban population	332	23.13	57.17	48	29.87	89.73	61	13.40	15.89
<i>C. Electoral-level controls</i>									
Council votes	332	0.18	0.12	48	0.23	0.10	61	0.22	0.09
Mayor asian	332	0.13	0.33	48	0.10	0.31	61	0.16	0.37
Mayor black	332	0.09	0.29	48	0.08	0.28	61	0.07	0.25
Mayor female	332	0.07	0.25	48	0.04	0.20	61	0.07	0.25
Mayor party ASI	332	0.18	0.39	48	0.08	0.28	61	0.05	0.22
Mayor party Conservador	332	0.04	0.19	48	0.10	0.31	61	0.03	0.18
Mayor party Liberal	332	0.26	0.44	48	0.23	0.42	61	0.28	0.45
Mayor party U	332	0.24	0.43	48	0.38	0.49	61	0.34	0.48
Mayor party Verde	332	0.09	0.28	48	0.08	0.28	61	0.05	0.22
Mayor white	332	0.78	0.41	48	0.81	0.39	61	0.77	0.42
<i>D. Baseline state capacity</i>									
Electoral security	332	0.51	0.50	48	0.46	0.50	61	0.64	0.48
Openness	332	0.50	0.50	48	0.44	0.50	61	0.46	0.50
Performance	332	0.50	0.50	48	0.48	0.50	61	0.49	0.50
State capacity index	332	0.50	0.50	48	0.48	0.50	61	0.62	0.49
Transparency	332	0.50	0.50	48	0.58	0.50	61	0.41	0.50

Notes: The table reports summary statistics for school-level variables for schools in municipalities where there is a race between a candidate from the same party as the governor and a candidate from a different party (Columns 1–3), for municipalities with close races where the mayor belongs to *the same* party as the governor (Columns 4–6), and for municipalities with close races where the mayor belongs to *a different* party than the governor (Columns 7–9). Panel A reports the main independent variable, *Alignment*, which is a binary indicator equal to one if the mayoral candidate from the same party as the elected governor won the election, along with the outcome variables, including *Ghosts*, which represents the fraction (in %) of reported students that are fake. Panels B and C report the set of pre-determined geographic/demographic-level and electoral-level controls, respectively. Panel D reports baseline measures of state capacity. Close races are defined using the optimal bandwidth in our baseline specification (13.0%). See the text and Appendix Table A-1 for more details and definitions.

Table B-4: Summary statistics – In-sample and out-of-sample differences in school and municipality variables

	(1)	(2)	(3)	(4)	(5)	(6)
	Mean of variable in sample of municipality with:			p-value difference		
	No	Any race		in columns:		
	race	All	Close races	Non-close races	(1)-(2)	(3)-(4)
<i>A. School-level variables</i>						
Ghost	1.183	1.348	1.356	1.344	0.466	0.966
Students rural	64.040	73.070	73.485	72.889	0.140	0.917
Students full time	31.413	35.623	21.958	41.607	0.382	0.005
Students outsourced	17.125	10.013	6.240	11.665	0.054	0.097
Total students	636.327	654.990	732.586	621.008	0.824	0.287
Total teachers	24.946	23.406	25.908	22.252	0.670	0.302
Principals tertiary	1.865	1.675	1.772	1.630	0.402	0.641
Students men	52.141	52.105	51.933	52.180	0.867	0.431
Students minors	94.651	93.891	92.821	94.360	0.137	0.069
Teachers tertiary	1.991	1.782	2.128	1.623	0.633	0.468
Students high school	28.693	27.480	30.471	26.170	0.611	0.208
Students disabilities	1.457	1.416	1.788	1.253	0.854	0.250
Students low income	94.850	97.855	98.151	97.725	0.014	0.591
<i>B. Municipality-level variables</i>						
Ghost	1.159	1.266	1.257	1.272	0.466	0.962
Total population	43.130	36.314	33.161	38.075	0.543	0.500
Urban population	34.525	23.125	20.194	24.763	0.303	0.490
Notaries	0.781	0.748	0.739	0.752	0.783	0.892
Elevation	1,324.537	709.663	600.571	770.610	0.000	0.221
Dist. to Bogota	291.641	391.128	394.839	389.055	0.000	0.792
Area (km2)	953.313	1,155.599	1,489.908	968.826	0.395	0.363
Dist. to dep. capital	80.839	82.007	79.546	83.381	0.770	0.566
Mayor female	0.112	0.066	0.059	0.070	0.010	0.678
Mayor asian	0.099	0.127	0.143	0.117	0.197	0.514
Mayor black	0.028	0.090	0.067	0.103	0.000	0.247
Mayor white	0.873	0.783	0.790	0.779	0.001	0.822

Notes: This table reports the school-level (Panel A) and municipality-level (Panel B) means of the variables listed across different samples. Column 1 reports the mean of the listed variable for observations in 767 municipalities that did not race a candidate from the governor’s party and one from a different party in the mayoral election (i.e., at least one of them did not run). Column 2 reports the mean of the listed variable for the remaining 332 municipalities with such a race. Columns 3 and 4 report the mean of the listed variable for observations in municipalities with close and non-close races, respectively, where a close race is defined based on our optimal bandwidth (13%). Column 5 reports the p-value of the difference in means between observations in municipalities with no race and those with any race. In contrast, Column 6 reports the p-value of the difference in means between observations in municipalities with close and non-close races. See the text and Appendix Table A-1 for more details and definitions.

Table B-5: Additional robustness checks on the effect of political alignment on ghost students

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Dependent variable is the alternative transformation				Dependent variable is ghost students (in %)											
	Any ghost	Log of ghosts	asinh of ghosts	level ghosts	Dropping municipalities with population in bottom			Dropping municipalities with population in top			Dropping schools with number of students in bottom			Dropping schools with number of students in top		
					1 %	5 %	10 %	1 %	5 %	10 %	1 %	5 %	10 %	1 %	5 %	10 %
Alignment	0.088 (0.057)	0.295 (0.098)	0.361 (0.122)	19.389 (7.490)	1.696 (0.550)	1.696 (0.550)	1.694 (0.550)	1.634 (0.747)	1.660 (0.804)	1.908 (0.718)	1.696 (0.550)	1.677 (0.585)	1.664 (0.598)	1.764 (0.557)	1.727 (0.564)	1.270 (0.501)
Observations	3,939	3,939	3,939	3,939	3,939	3,937	3,931	3,730	3,346	3,069	3,939	3,835	3,677	3,909	3,740	3,532
Obs. in bandwidth	720	720	720	720	720	720	720	597	597	571	720	703	685	715	697	669
Bandwidth	0.081	0.081	0.081	0.081	0.081	0.081	0.081	0.081	0.081	0.081	0.081	0.081	0.081	0.081	0.081	0.081
Mean dependent	0.488	0.417	0.525	7.121	1.170	1.171	1.171	1.179	1.179	1.210	1.170	1.198	1.196	1.171	1.172	1.113
S.D. dependent	0.500	0.682	0.865	34.033	3.177	3.179	3.179	3.356	3.356	3.426	3.177	3.210	3.206	3.197	3.224	3.052

Notes: This table explores additional robustness checks of our main RD estimates on the effect of electing a politically aligned mayor on the number of fabricated students in schools within the mayor's municipality. The first four columns report estimates based on alternative transformations of the dependent variable: a dummy indicating whether there is any fabricated student in the school (Column 1), the log of (one plus) the number of fabricated students (Column 2), the hyperbolic sine of the number of fabricated students (Column 3), and the raw number of fabricated students (Column 4). Columns 5 to 16 report estimates on the fraction (in %) of fabricated students, restricting the sample along several dimensions: dropping schools in populations with a small (Columns 8-10) or large (Columns 11-13) number of inhabitants, or dropping schools with a small (Columns 11-13) or large (Columns 14-16) number of students. *Alignment* is a binary indicator equal to one if mayoral candidate of the same party as the elected governor won the election. All results are based on equation (1), which is estimated at school level, and includes a set of school-level, student- and teacher-level, geographic/demographic-level, and electoral-level controls. We do not report the coefficients for these additional controls, because, unlike traditional linear regression, the non-parametric estimators implemented do not directly estimate coefficients for the control variables (see Appendix D). See Appendix Table A-1 for the list of control variables within each group. All estimates are bias-corrected, non-parametrically estimated, are computed within the optimal bandwidth following Calonico et al. (2019) using a triangular kernel and a linear polynomial. Standard errors are clustered at the municipality level and are robust to heteroskedasticity.

Table B-6: Municipality-level RD estimates of the effect of political alignment on ghost students

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable is ghost students per municipality (in %)									
Alignment	1.525 (0.609)	1.478 (0.633)	1.215 (0.689)	1.804 (0.702)	1.688 (0.691)	1.429 (0.731)	1.161 (0.499)	1.415 (0.472)	2.524 (0.554)
Observations	332	332	332	332	332	332	329	332	329
Obs. in bandwidth	139	130	117	165	179	158	109	129	90
Bandwidth	0.154	0.143	0.126	0.189	0.210	0.170	0.119	0.142	0.101
Kernel	Triangular	Epanechnikov	Uniform	Triangular	Epanechnikov	Uniform	Triangular	Triangular	Triangular
Local polynomial Order	1	1	1	2	2	2	1	1	1
Geographic/demographic controls							✓		✓
Electoral controls								✓	✓

Notes: This table reports municipality-level RD estimates of the effect of electing a politically aligned mayor on the fraction of fabricated students in schools in the mayor's municipality. The dependent variable is the school-level ratio of fake students to total students in 2012. *Alignment* is a binary indicator equal to one if mayoral candidate of the same party as the elected governor won the election. All results are based on equation (1), which is estimated at the municipality level. Columns 1-6 report estimates using different kernels and polynomials with no controls, while Columns 7-9 report estimates with a triangular kernel and a linear polynomial, incorporating a set of geographic/demographic-level, and/or electoral-level as indicated in each column. We do not report the coefficients for these additional controls, because, unlike traditional linear regression, the non-parametric estimators implemented do not directly estimate coefficients for the control variables (see Appendix D). See Appendix Table A-1 for the list of control variables within each group. All estimates are bias-corrected, non-parametrically estimated, are computed within the optimal bandwidth following Calonico et al. (2019). Standard errors are clustered at the municipality level and are robust to heteroskedasticity.

Table B-7: RD estimates of the effect of political alignment on ghost students, school- versus municipality-level results and the role of weights

	(1)	(2)	(3)	(4)	(5)
Dependent variable is ghost students per school (in %)					
	School baseline	Municipality school weights	Municipality student weights	Municipality population weights	Municipality no weights
Alignment	1.696 (0.550)	2.524 (0.554)	2.190 (0.590)	2.491 (0.618)	2.664 (1.722)
Observations	3,939	329	329	329	329
Obs. in bandwidth	720	90	109	94	111
Bandwidth	0.081	0.101	0.119	0.104	0.124
Mean dependent	1.170	1.163	1.260	1.140	1.282
S.D. dependent	3.177	2.870	3.044	2.814	3.030

Notes: This table reports RD estimates of the effect of electing a politically aligned mayor on the fraction of fabricated students. Column (1) reports the baseline specification at the school level. Column (2) reports the results at the municipality-level weighted by the number of schools. Column (3) presents municipality-level results weighted by student enrollment in 2011. Column (4) presents municipality-level results weighted by population. Column (5) presents municipality-level results with no additional weights. The dependent variable is the school-level or municipality-level ratio of fake students to total students in 2012. *Alignment* is a binary indicator equal to one if mayoral candidate of the same party as the elected governor won the election. All results are based on equation (1), which is estimated with the baseline set of controls. In the school-level specification, it includes a set of school-level, student- and teacher-level, geographic/demographic-level, and electoral-level controls. In the municipality-level specification, it includes a set of geographic/demographic-level and electoral-level controls). We do not report the coefficients for these additional controls, because, unlike traditional linear regression, the non-parametric estimators implemented do not directly estimate coefficients for the control variables (see Appendix D). See Appendix Table A-1 for the list of control variables within each group. All estimates are bias-corrected, non-parametrically estimated, are computed within the optimal bandwidth following Calonico et al. (2019) using a triangular kernel and a linear polynomial. Standard errors are clustered at the municipality level and are robust to heteroskedasticity.

Table B-8: RD estimates of the effect of political alignment on fiscal performance – tax revenue and transfers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable is...								
	Tax Revenue		Transfers					
			Total		national government		non-national entities	
	log per capita	% of total revenue	log per capita	% of total revenue	log per capita	% of total revenue	log per capita	% of total revenue
Alignment	-0.195 (0.310)	0.985 (3.241)	-0.302 (0.174)	-0.243 (4.808)	-0.163 (0.121)	5.516 (5.961)	-0.963 (0.671)	-4.068 (3.269)
Observations	329	329	329	329	329	329	318	329
Obs. in bandwidth	125	125	109	127	95	122	105	186
Bandwidth	0.138	0.137	0.117	0.140	0.106	0.134	0.121	0.228
Mean dependent	4.500	12.012	6.674	78.778	6.672	77.268	1.258	0.940
S.D. dependent	0.901	10.996	0.439	12.735	0.379	13.809	1.622	4.714

Notes: This table reports RD estimates of the effect of electing a politically aligned mayor on the fiscal performance. Fiscal performance is measured using collected tax revenue (Columns 1–2), total received transfers (Columns 3–4), transfers received from the national government (Columns 5–6), and transfers received from non-national entities (Columns 7–9). Odd-numbered columns present the logarithm of these variables, while even-numbered columns present them as a percentage of total revenue. *Alignment* is a binary indicator equal to one if mayoral candidate of the same party as the elected governor won the election. All results are based on equation (1), which is estimated at the municipality level, and includes a set of geographic/demographic-level and electoral-level controls. We do not report the coefficients for these additional controls, because, unlike traditional linear regression, the non-parametric estimators implemented do not directly estimate coefficients for the control variables (see Appendix D). See Appendix Table A-1 for the list of control variables within each group. All estimates are bias-corrected, non-parametrically estimated, are computed within the optimal bandwidth following Calonico et al. (2019) using a triangular kernel and a linear polynomial. Standard errors are clustered at the municipality level and are robust to heteroskedasticity.

Table B-9: Robustness checks on the effect of political alignment on changes in school performance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Grade 5 (elementary)			Grade 9 (middle school)			Grade 11 (high school)		
	Δ 2012-2009	Δ 2013-2009	Δ 2014-2009	Δ 2012-2009	Δ 2013-2009	Δ 2014-2009	Δ 2012-2010	Δ 2013-2010	Δ 2014-2010
<i>A. The dependent variable is the change in the average math score of students in each school on the nationals standardized exam</i>									
Alignment	-0.146 (0.090)	-0.391 (0.089)	-0.601 (0.153)	0.129 (0.137)	-0.253 (0.121)	-0.135 (0.154)	-0.270 (0.162)	0.150 (0.150)	-0.201 (0.111)
Observations	2,277	2,161	2,275	1,623	1,544	1,587	1,727	1,715	1,682
Obs. in bandwidth	391	358	334	305	267	297	278	310	290
Bandwidth	0.073	0.074	0.064	0.078	0.074	0.079	0.070	0.079	0.075
Mean dependent	0.028	0.030	0.072	-0.144	-0.050	0.027	0.079	0.075	0.097
S.D. dependent	0.755	0.771	0.857	0.712	0.716	0.786	0.720	0.790	0.672
<i>B. The dependent variable is the change in the average language score of students in each school on the nationals standardized exam</i>									
Alignment	-0.026 (0.054)	-0.179 (0.108)	-0.137 (0.100)	-0.032 (0.098)	-0.394 (0.096)	-0.257 (0.133)	0.049 (0.167)	-0.141 (0.160)	-0.107 (0.105)
Observations	2,269	2,164	2,269	1,617	1,559	1,639	1,727	1,715	1,682
Obs. in bandwidth	376	389	358	265	254	250	380	389	369
Bandwidth	0.072	0.079	0.068	0.068	0.069	0.060	0.084	0.088	0.085
Mean dependent	-0.019	-0.032	-0.009	-0.102	-0.051	-0.025	-0.003	0.014	-0.034
S.D. dependent	0.658	0.679	0.728	0.667	0.613	0.710	0.818	0.783	0.777

Notes: This table reports RD estimates of the effect of electing a politically aligned mayor on school quality, measured by school performance changes. School performance is measured using the national standardized exam that all students must take at the end of grades 5, 9, and 11. Panels A and B report the effect on the change in the school-level average of these scores in math and language, respectively, for each grade and the changes over the years indicated in the corresponding column. *Alignment* is a binary indicator equal to one if mayoral candidate of the same party as the elected governor won the election. All results are based on equation (1), which is estimated at the school level, and includes a set of school-level, student- and teacher-level, geographic/demographic-level, and electoral-level controls. We do not report the coefficients for these additional controls, because, unlike traditional linear regression, the non-parametric estimators implemented do not directly estimate coefficients for the control variables (see Appendix D). See Appendix Table A-1 for the list of control variables within each group. All estimates are bias-corrected, non-parametrically estimated, are computed within the optimal bandwidth following Calonico et al. (2019) using a triangular kernel and a linear polynomial. Standard errors are clustered at the municipality level and are robust to heteroskedasticity.

Table B-10: RD estimates of the effect of political alignment on ghosts students by grade

	(1)	(2)	(3)
Dependent variable is ghost students per school (in %)			
	Grades 1-5 (elementary)	Grades 6-9 (middle school)	Grades 10-11 (high school)
Alignment	1.582 (0.751)	1.161 (0.683)	1.424 (0.735)
Observations	4,336	2,798	2,212
Obs. in bandwidth	1,427	860	784
Bandwidth	0.142	0.121	0.135
Mean dependent	1.364	1.325	1.240
S.D. dependent	4.725	5.809	5.167
Fraction of ghost by school level (%)	39.877	44.673	15.450
Fraction of students by school level (%)	49.144	37.483	13.372

Notes: This table reports RD estimates of the effect of electing a politically aligned mayor on the fraction of fabricated students in schools in the mayor’s municipality. The dependent variable is the school-level ratio of fake students to total students in 2012: for grades 1–5 in Column 1, grades 6–9 in Column 2, and grades 10–11 in Column 3. *Alignment* is a binary indicator equal to one if mayoral candidate of the same party as the elected governor won the election. All results are based on equation (1), which is estimated at the school level, and includes a set of school-level, student- and teacher-level, geographic/demographic-level, and electoral-level controls. We do not report the coefficients for these additional controls, because, unlike traditional linear regression, the non-parametric estimators implemented do not directly estimate coefficients for the control variables (see Appendix D). See Appendix Table A-1 for the list of control variables within each group. All estimates are bias-corrected, non-parametrically estimated, are computed within the optimal bandwidth following Calonico et al. (2019) using a triangular kernel and a linear polynomial. Standard errors are clustered at the municipality level and are robust to heteroskedasticity. The figure also reports the fraction of ghost and total students in the respective school level.

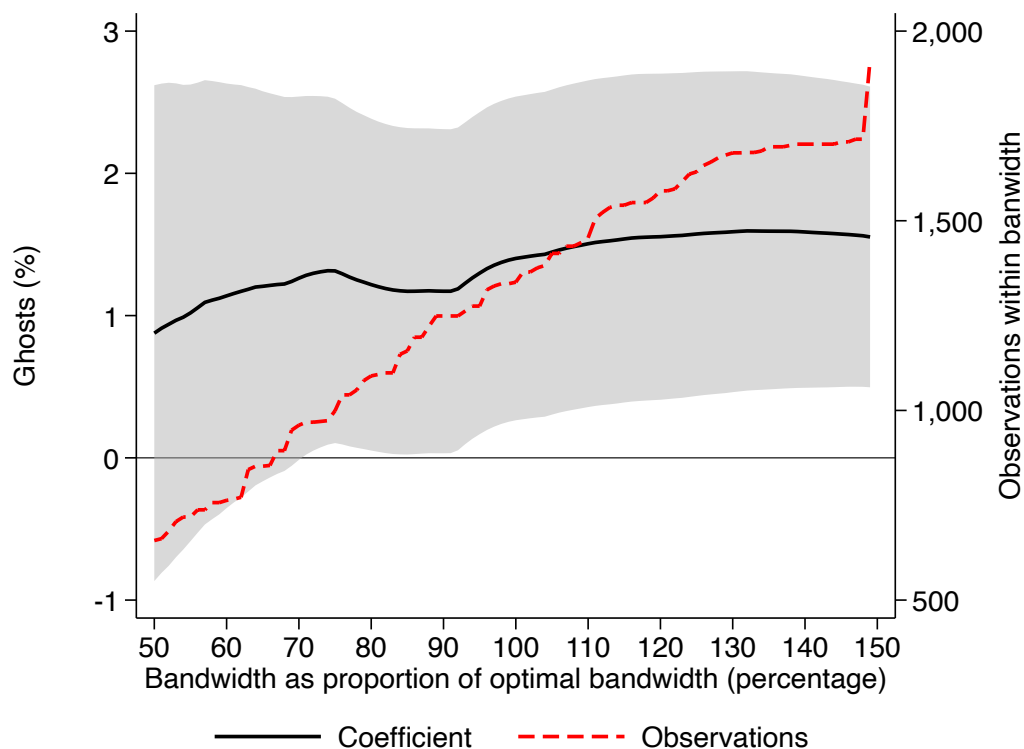
Table B-11: RD estimates of the effect of political alignment on health outcomes and public services

Dependent variable: percent change from 2008–2011 to 2012–2015						
	Health Outcomes			Public Services		
	Subsidized Health Coverage	Infant Mortality Rate	Health Facilities	Piped water coverage	Electricity users	Electricity coverage
	(1)	(2)	(3)	(4)	(5)	(6)
Alignment	0.0230 (0.0314)	-0.0154 (0.0161)	-0.0671 (0.123)	-0.327 (0.205)	-0.488 (0.529)	0.0311 (0.0602)
Observations	329	329	329	319	329	329
Obs. in bandwidth	121	153	95	163	151	110
Bandwidth	0.131	0.166	0.107	0.199	0.164	0.121
Mean dependent	0.059	-0.106	0.039	0.074	0.262	0.027
S.D. dependent	0.082	0.052	0.469	0.502	0.920	0.157

Notes: This table reports RD estimates of the effect of electing a politically aligned mayor on municipality-level changes in health and public-service outcomes. The dependent variable in each column is the percent change from 2008–2011 to 2012–2015. Subsidized health coverage measures the share of residents enrolled in the subsidized health insurance regime. The infant mortality rate (IMR) is the number of deaths under age one per 1,000 live births. Health facilities is the number of public health provider sites. Piped water coverage measures the share of the population served by the municipal aqueduct. Electricity users is the total number of electricity customers. Electricity coverage is an index measuring electricity service coverage in the municipality's urban center. *Alignment* is a binary indicator equal to one if mayoral candidate of the same party as the elected governor won the election. All results are based on equation (1), which is estimated at the municipality level, and includes a set of geographic/demographic-level and electoral-level controls. We do not report the coefficients for these additional controls, because, unlike traditional linear regression, the non-parametric estimators implemented do not directly estimate coefficients for the control variables (see Appendix D). See Appendix Table A-1 for the list of control variables within each group. All estimates are bias-corrected, non-parametrically estimated, are computed within the optimal bandwidth following Calonico et al. (2019) using a triangular kernel and a linear polynomial. Standard errors are clustered at the municipality level and are robust to heteroskedasticity.

C Appendix figures

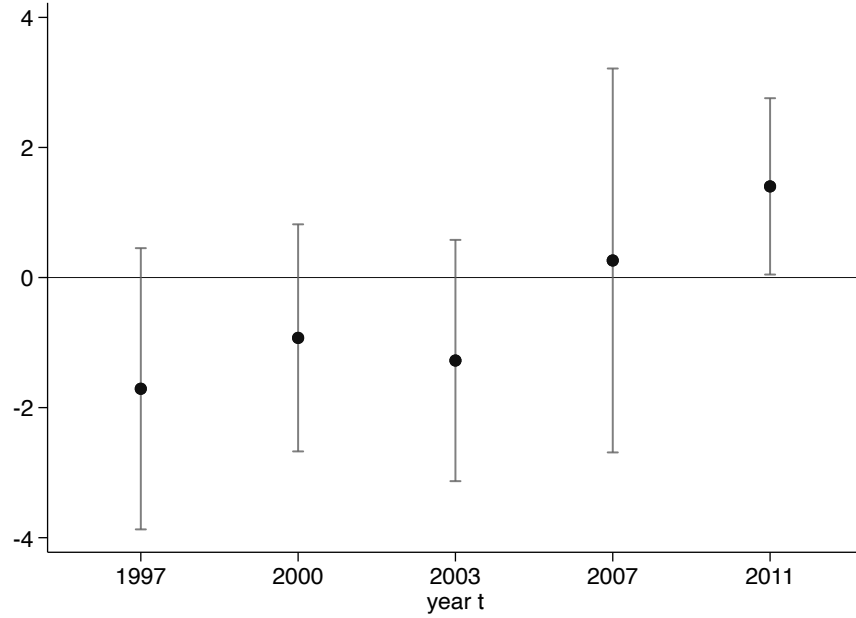
Figure C-1: RD estimates of the effect of political alignment on ghost students
– robustness to bandwidth choice



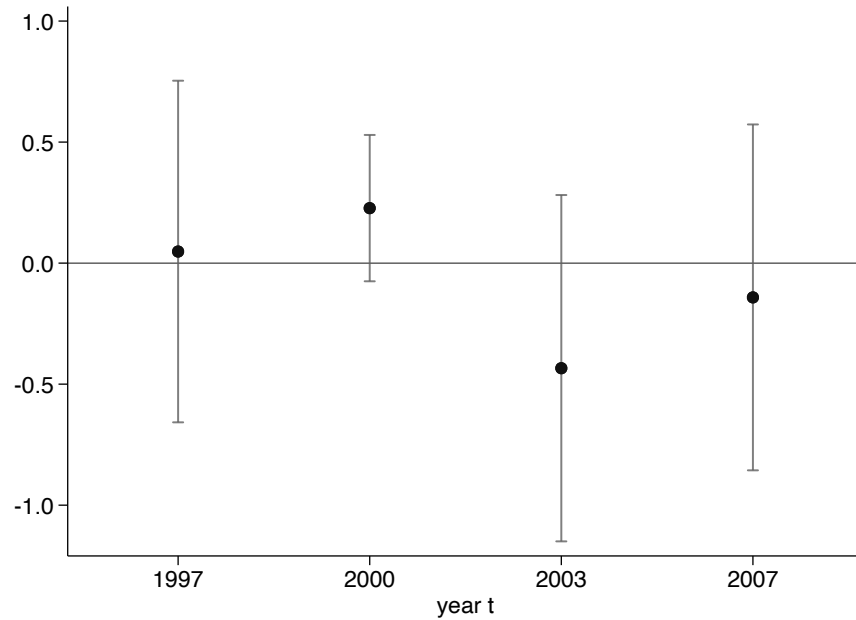
Notes: This figure shows the robustness of our main RD estimates (see Table 2) across different bandwidth values, ranging from 50% to 150% of the optimal bandwidth. The dependent variable is the school-level ratio of fake students to total students in 2012. *Alignment* is a binary indicator equal to one if mayoral candidate of the same party as the elected governor won the election. All results are based on equation (1), which is estimated at the school level and includes a set of school-level, geographic/demographic-level and electoral-level controls. All estimates are bias-corrected, non-parametrically estimated following Calonico et al. (2019). Standard errors are clustered at the municipality level and are robust to heteroskedasticity. Observations: 4,383.

Figure C-2: Falsification tests

A. Predicting ghost students in 2012 with political alignment in year t

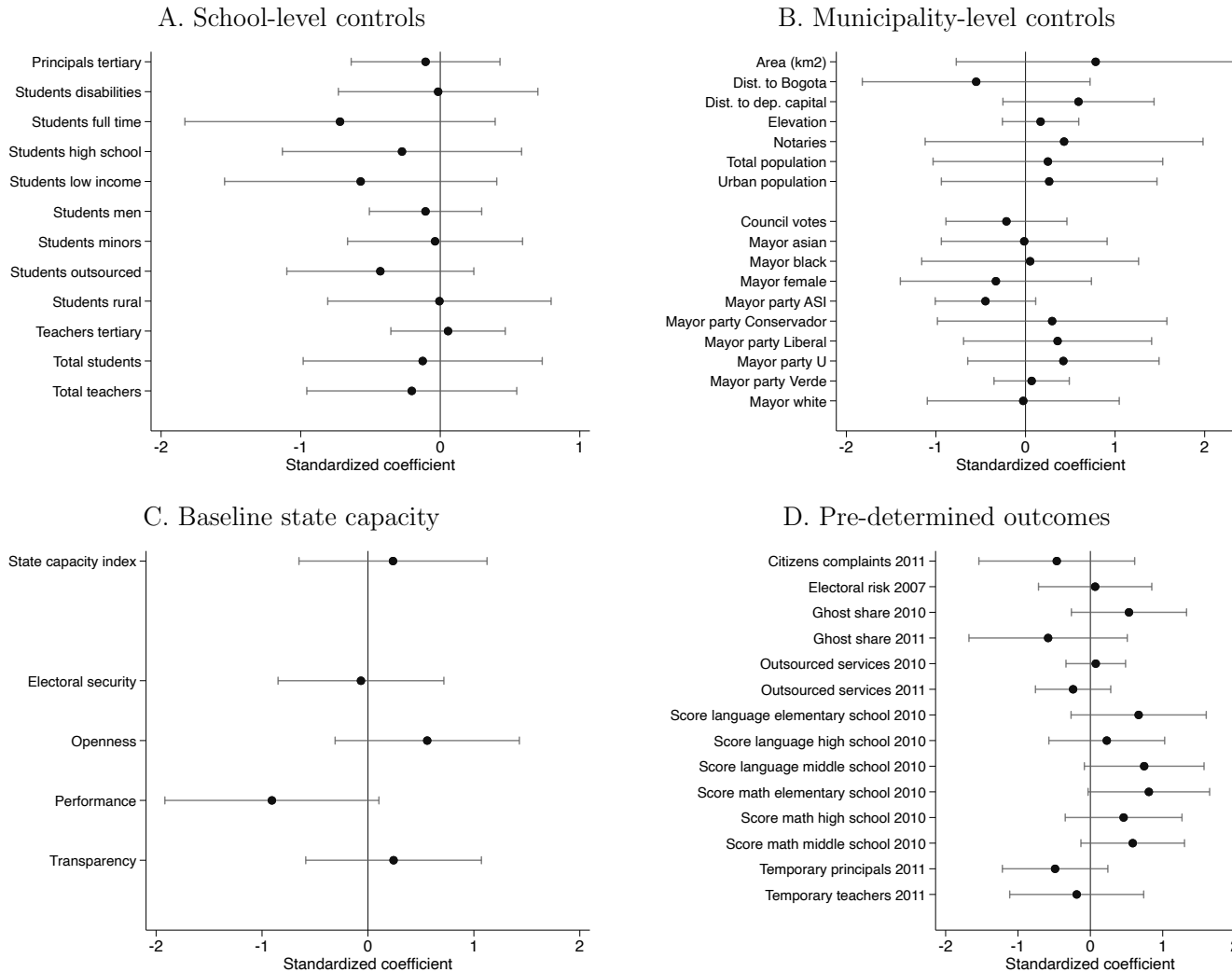


B. Predicting political alignment in year t with political alignment in 2011



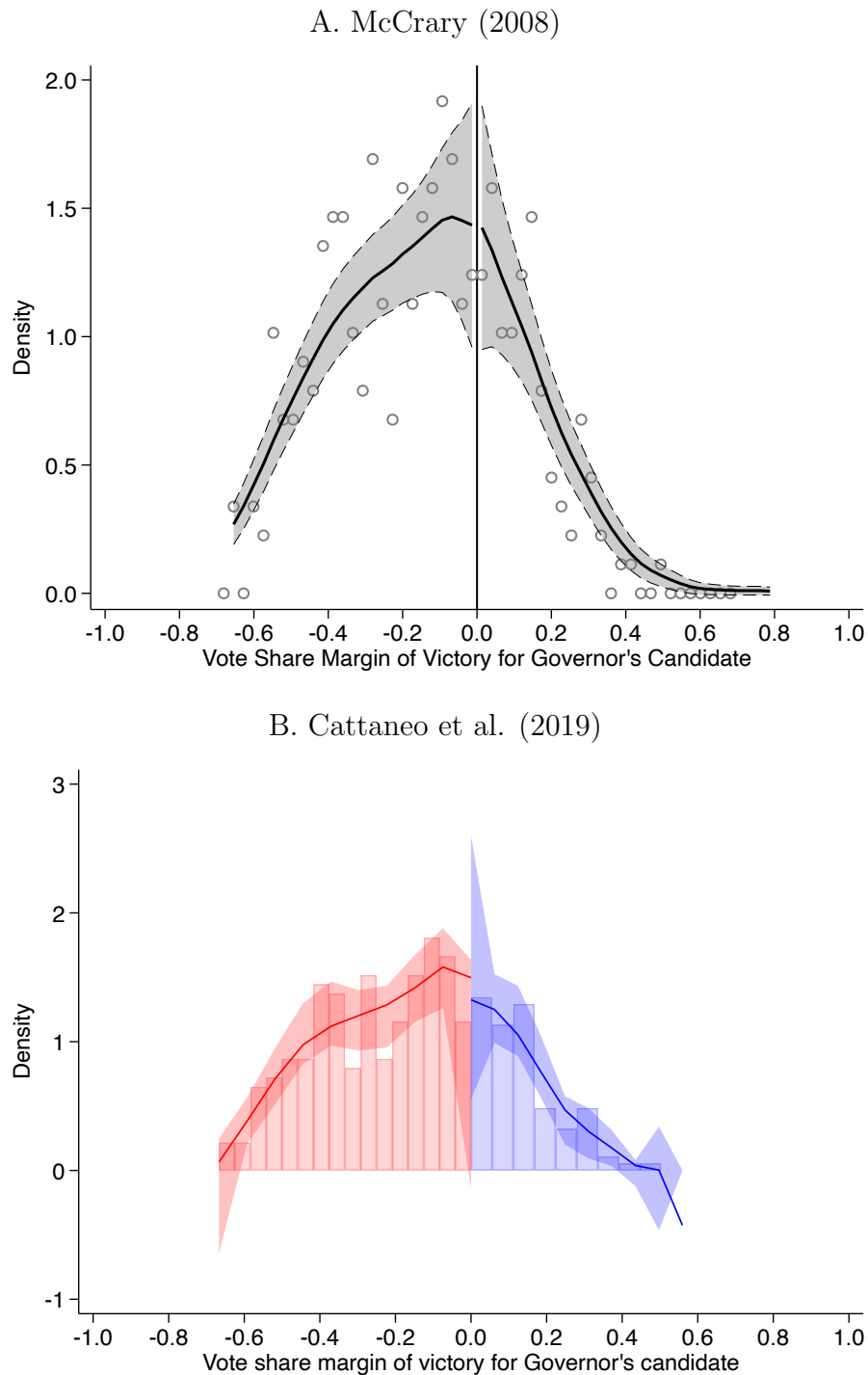
Notes: This table reports two sets of falsification tests. Panel A presents RD estimates of the effect of having a politically aligned mayor in year $t \forall t \in \{1997, 2000, 2003, 2007, 2011\}$ on the ratio of fake students to total students in 2012. Panel B presents RD estimates of the effect of having a politically aligned mayor in year $t \forall t \in \{1997, 2000, 2003, 2007\}$ on the probability of having a politically aligned mayor in 2011. All results are based on equation (1). All estimates are bias-corrected, non-parametrically estimated, are computed within the optimal bandwidth following Calonico et al. (2019). Standard errors are clustered at the municipality level and are robust to heteroskedasticity. In Panel A, the number of observations is 5,489, 5,904, 4,994, 7,552 and 4,383 for each year respectively (1997, 2000, 2003, 2007 and 2011). In Panel B, the number of observations is 4,383.

Figure C-3: Balance on pre-determined characteristics



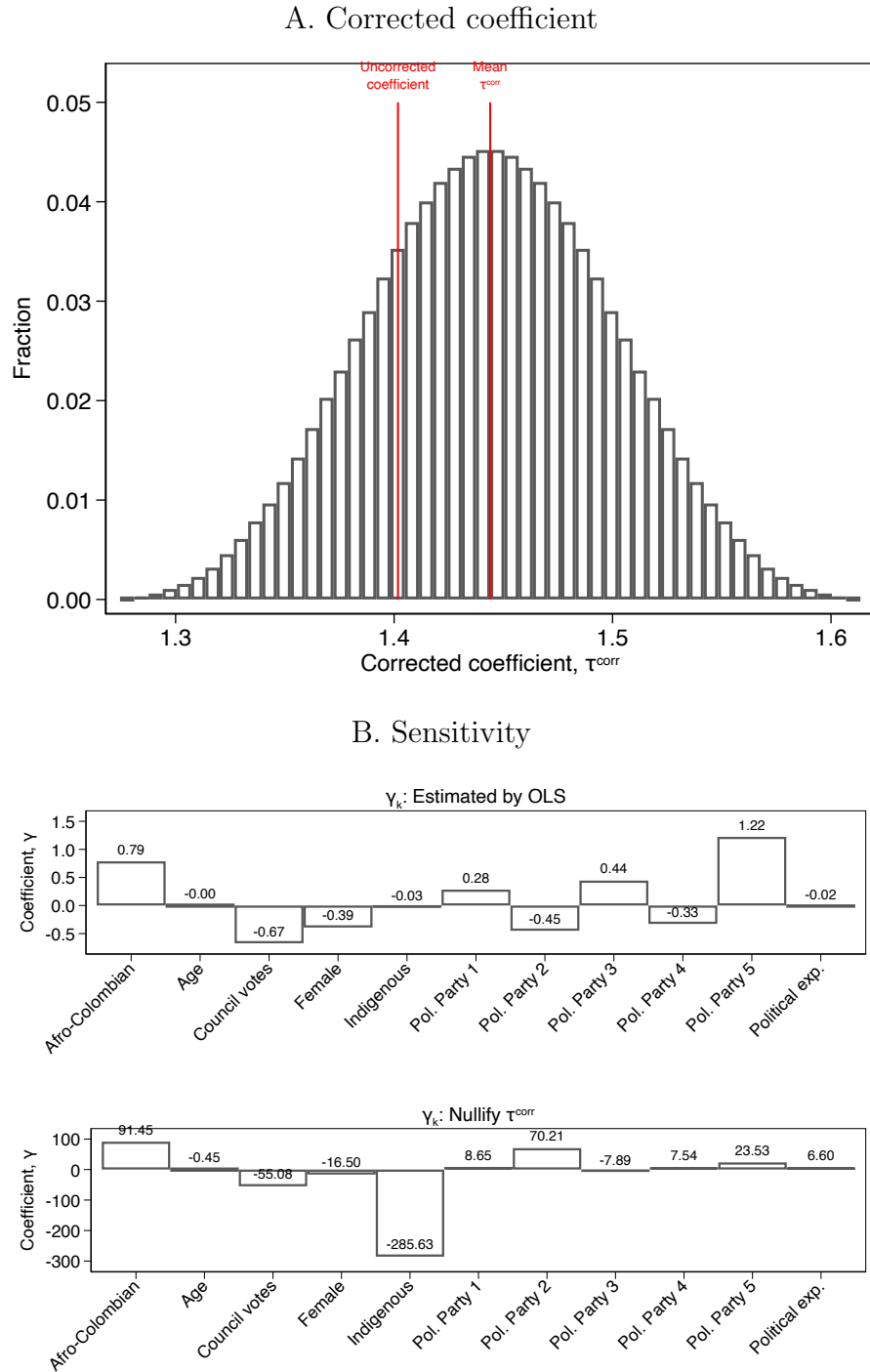
Notes: This figure reports RD estimates of the effect of electing a politically aligned mayor on pre-determined characteristics, including school-level controls (Panel A), municipality-level controls (Panel B), baseline state capacity measures (Panel C), and pre-determined values of the main outcomes when available (Panel D). All coefficients are standardized (beta coefficients). *Alignment* is a binary indicator equal to one if mayoral candidate of the same party as the elected governor won the election. All estimates are bias-corrected, non-parametrically estimated, are computed within the optimal bandwidth following Calonico et al. (2019). The whisker areas show 95 percent confidence intervals based on standard errors clustered at the municipality level and robust to heteroskedasticity. The number of observations is 332 and 4,338 for the municipality- and school-level regressions, respectively.

Figure C-4: Manipulation density test for running variable



Notes: This figure displays the distribution of the vote share margin of victory for the mayoral candidate from the governor's party, relative to the highest-ranked candidate from a different party, and presents estimates of the discontinuity of this function around zero. In Panel A, the density is estimated based on McCrary (2008), with an estimated discontinuity at zero of 0.0268 and a standard error of 0.2647. Panel B presents the estimate based on Calonico et al. (2019), with an estimated discontinuity at zero of -0.164 and a standard error of 0.471. The number of observations is 332.

Figure C-5: Assessing possible politician characteristic biases (PCRD)



Notes: Panel A plots distribution of Marshall (2022) corrected estimator, $\hat{\tau}^{\text{corr}} = \hat{\tau} - \sum_k \hat{\gamma}_k \hat{\delta}_k$, generated by scaling each estimated γ_k by 0.5, 1, and 1.5. The top portion of Panel B displays the values of $\hat{\gamma}_k$ estimated by OLS. The lower portion of panel B calculates the value of each $\hat{\gamma}_k$ that would nullify the mean corrected estimated effect. See the text for more details. The number of observations is 4,383.

D Estimates with full set of covariates when applicable

Our main estimates follow the non-parametric, bias-corrected regression-discontinuity approach of Calonico et al. 2019. This method allows for covariate adjustment to improve precision, though standard practice does not involve reporting covariate coefficients. We use a parametric (linear) regression only in Panel C of Table 9 to analyze governor performance. Unlike mayors, who can be aligned to only one governor, governors may be aligned with multiple mayoral candidates, each competing in separate municipal races. Table D-1 presents the estimated coefficients for all covariates in this specification.

Table D-1: Party and politicians' prospects – incumbency advantage for governors

	(1)	(2)	(3)
<i>C: Governor future prospects</i>			
	Run in next election	Win in next election	Win in next election/Run
Governor's Alignments	0.012 (0.011)	0.018 (0.010)	0.024 (0.015)
Fraction of close elections involving connected candidates	-0.359 (0.238)	-0.017 (0.139)	0.398 (0.326)
Fraction of elections involving connected candidates	0.063 (0.215)	-0.183 (0.118)	-0.493 (0.240)
Year= 2000	-0.073 (0.127)	-0.159 (0.115)	-0.238 (0.192)
Year= 2003	-0.309 (0.121)	-0.191 (0.114)	0.006 (0.209)
Year= 2007	-0.248 (0.130)	-0.165 (0.117)	-0.077 (0.212)
Year= 2011	-0.149 (0.133)	-0.128 (0.120)	-0.116 (0.192)
Constant	0.631 (0.112)	0.391 (0.108)	0.649 (0.160)
Observations	152	152	62
Mean dependent	0.409	0.227	0.556
S.D. dependent	0.493	0.420	0.501

Notes: This table summarizes the incumbency advantage reported in Panel C of Table 9 in the main paper. It includes all local elections in Colombia between 1997 and 2011 (i.e., 1997, 2000, 2003, 2007, and 2011) using nationwide data on three outcomes: running in the subsequent election, winning the next election, and winning the next election conditional on running. *Governor's Alignment* is defined as the number of mayoral candidates within the department who win close elections and belong to the same party as the elected governor. Estimates are based on a linear regression that includes year fixed effects and controls for both the departmental percentage of total and close municipal elections involving aligned (and non-aligned) candidates. Standard errors are robust to heteroskedasticity.