Essays in Development Economics

Share Your Story

Essays in Development Economics

A dissertation presented

by

Laura Carolina Trucco

to

The Department of Economics

in partial fulfillment of the requirements

for the degree of

Doctor of Philosophy

in the subject of

Economics

Harvard University

Cambridge, Massachusetts

August 2015

Dissertation Advisors: *Author:*
Professor Sendhil Mullainathan   **Laura Carolina Trucco**
Professor Asim Khwaja

Essays in Development Economics

Abstract

Economic development, more often than not, is tightly connected to good governance and adequate provision of public goods and services. My dissertation examines characteristics of developing countries that are relevant to the quality of government, as they affect the interaction between citizens and government. In chapter 1, I focus on squatters and the opportunities for political intermediation associated with the lack of property rights on land. In this chapter, I study an extensive land titling program in urban Mexico and its effect on clientelism. In chapter 2, I examine the costs of party turnover for the quality of public education in Brazilian municipalities, a context where the bureaucracy is not shielded from the political process. In chapter 3, I study the feedback effect of government work on citizens' participation. To this end, I focus on maintenance work on the public space and citizens' complaints in the City of Buenos Aires.

Contents

List of Tables

List of Figures

Acknowledgments

To my parents

Introduction

Economic development, more often than not, is tightly connected to good governance and adequate provision of public goods and services. My dissertation examines characteristics of developing countries that are relevant to the quality of government, as they affect the interaction between citizens and government.

Clientelism is a common practice in developing countries, and one that often has detrimental effects for public good provision and political accountability. In chapter 1, "Can Clientelistic Ties be Broken? Evidence from an Urban Titling Program in Mexico" (with Horacio Larreguy and John Marshall), I study the determinants of clientelism by focusing on a federal land titling program in urban Mexico that reached nearly 2.16 million households in 35 years. I find that municipal incumbents, who are particularly well positioned to enforce clientelistic exchanges, are the ones most affected by the distribution of property rights on land: their vote share significantly decreases after the program was implemented in the area. At the same time, I also find that incumbent parties that were responsible for the program are rewarded by voters, especially in federal elections. I thus address an important empirical challenge in the literature, namely, teasing out the impact of reforms on clientelism from their direct effect on voters electoral preferences.

While it is expected that parties taking office will bring high level officials to the government, in developing countries the bureaucracy can also be highly exposed to the political process. In chapter 2, "Political Turnover, Bureaucratic Turnover, and the Quality of Public Services" (with Mitra Akhtari and Diana Moreira), I study the effect of party turnover on the appointment of teachers, appointment of headmasters, and the quality of education

provision in Brazil. Using a regression discontinuity design on close mayoral elections, I find that municipalities with a new party in office have higher turnover of headmasters and teachers the year after the election: 24 and 10 percentage points more positions are replaced, respectively. Party turnover also has real and persistent effects on education outcomes. In municipalities with turnover, test scores are 0.13 standard deviations lower and dropout rates are 37% higher. The decrease in performance persists three years after the election. The evidence suggests that the disruption of personnel at the school level is driving the decrease in students' performance: headmasters are less experienced in their positions and teachers' assessment of the headmaster is more negative, whereas teachers' characteristics, students' characteristics, or resources spent on education do not seem to change. Taken together, the results suggest that the disruption to the bureaucracy has a real cost for the quality of service provision.

In chapter 3, "Fixing Broken Windows: The Effect of Government Maintenance Work on Citizens' Complaints", I use a unique dataset with detailed information on citizens' complaints and government maintenance work in the public space in the City of Buenos Aires to answer the following question: do citizens participate more when the government works more? Exploiting variation of repairs over time, I estimate the effect of maintenance work in the public space on complaints filed by citizens. The main finding of this paper is that government work stimulates participation: when the government repairs problems in the public space, citizens are more likely to complain about other problems in the area. The evidence suggests that increased citizen participation reflects a change in underlying beliefs about government responsiveness rather than just a shift of attention toward problems in the public space, beliefs about state capacity, poorly executed maintenance work, or reverse causality. These findings point toward the existence of strategic complementarities between government actions and citizens' participation, which could contribute to explain the substantial variation in the quality of public goods both across and within countries.

Chapter 1

Can Clientelistic Ties be Broken? Evidence from an Urban Titling Program in Mexico [1]

1.1 Introduction

Clientelism, the exchange of personal favors for political support, is ubiquitous in many developing contexts (Kitschelt and Wilkinson 2007; Schaffer 2007). This practice has important and often detrimental implications for the provision of public goods and political accountability (Keefer 2007; Keefer and Vlaicu 2008). While a blossoming literature explains how clientelistic exchanges operate in practice—whether brokers mobilize likely supporters through turnout buying (Nichter 2008) or induce individuals to change their vote by exploiting their reciprocity (Finan and Schechter 2012; Lawson and Greene 2014), or how parties provide incentives to brokers to mitigate moral hazard and adverse selection problems (Larreguy 2013; Larreguy, Marshall and Querubin forthcoming; Stokes et al. 2013)—a lot less is known about what ultimately leads voters to enter clientelistic relationships and especially the conditions under which voters escape such relationships.

---

[1]Co-authored with Horacio Larreguy and John Marshall

Why might voters enter and remain in clientelistic interactions? One possibility is that the short-term return to voters from engaging in clientelistic exchanges is larger than the benefits associated with public goods. Since clients often live in poverty, the marginal return of immediate informal assistance from brokers may often exceed future benefits of public good provision (Diaz-Cayeros, Estévez and Magaloni forthcoming; Magaloni 2006). Even without relying on high discount rates, the ongoing distributional benefits of clientelism—made possible by lasting ties to the community that supply privileged information about individual needs—could be higher than even programmatic policies in the long-run for the subset of the population receiving such benefits (Lizzeri and Persico 2001). Alternatively, voters may be unaware of programmatic policies, and even if they are aware of such policies, promises of programmatic policies may not be regarded as credible. Many developing democracies struggle to escape equilibria where politicians that (potentially) would like to provide programmatic policies cannot commit to such provision because short-term electoral incentives lead them to break their promises in order to retain office (Keefer 2007; Keefer and Vlaicu 2008).

Understanding why voters fall into and remain in clientelistic exchanges faces major empirical challenges. In particular, the impact of any reform that may alter the marginal return of clientelistic transactions to voters or provide information about alternative political agendas like programmatic politics might also independently affect a voter's electoral behavior by altering their appraisal of the government implementing the reform. This difficulty is exemplified in the growing literature exploiting natural experiments to identify the effects of conditional cash transfer programs (CCTs). These programs, which spread across Latin America in the late 1990s and early 2000s, reduce the benefits of clientelistic transfers for voters while increasing the returns from public goods by transparently conditioning cash transfers on school attendance and regular medical checks. De La O (2013) and Diaz-Cayeros, Estévez and Magaloni (forthcoming) in Mexico, Manacorda, Miguel and Vigorito (2011) in Uruguay, and Zucco (2013) in Brazil all find that enrollment in CCTs substantially increases electoral support for the incumbent party. These findings could be taken as evidence that

CCTs are unable to undermine the conditions under which clientelistic exchanges can operate effectively. However, such a conclusion ignores the possibility that CCTs might only increase incumbent support through shorter-run mechanisms. For example, Manacorda, Miguel and Vigorito (2011) correctly note that their results are equally consistent with poorly-informed voters using policy to infer information about politicians.

In this paper we separate the effects of a major land titling program on clientelism and reward for policy implementation. Since 1973, the Mexican government has created property rights on communal lands and offered squatters the opportunity to buy those rights at highly subsidized prices. Over 35 years, the program has reached 2.16 million households. Like CCT programs, recipients of land titles experience increased access to public goods and a positive net wealth effect.[2]

There is a strong symbiotic relationship between the lack of property rights and clientelism in urban Mexico. Political brokers and government officials often offer squatters protection against eviction in exchange for political support. In addition, their inability to provide proof of residence creates a host of other opportunities for political intermediation. Consequently, the establishment of land property rights has the potential to substantially diminish the dependency of squatters upon the incumbent political parties, and thus break down clientelistic ties. However, identifying the effect of land titling on clientelism is vulnerable to the same empirical concerns as CCTs. If incumbent parties, especially at the federal level, are credited for the program's implementation, then land titling reforms may also increase political support for the incumbent among the reform's beneficiaries.

Leveraging the timing of land titling of informal urban settlements owned by ejidos and agrarian communities, we use a difference-in-differences design to identify the effect of land titling on the vote share of municipal incumbents.[3] In order to avoid comparing

---

[2]Although the short-run wealth effect is negative due to the purchase, the cost was low, and access to subsidies were facilitated. Squatters can avoid the cost altogether by choosing not to enter the program, but rarely do so.

[3]Ejidos consist of lands that were granted to communities of petitioners that never had land after the Mexican revolution. Agrarian communities instead represent the restitution of lands that were expropriated from communities of peasants during the rule of Porfirio Díaz between 1876 and 1910. Both ejidos and agrarian

locations containing squatters to those that never experienced squatters, we focus solely on electoral precincts that experienced land titling at least once as part of the CORETT (Comité para la Regularización de la Tenencia de la Tierra) program between 1980 and 2013.[4] Since the rewards from implementing the program accrue to the party executing the titling, but the ability of local politicians to engage in clientelistic practices is reduced for all future incumbents, by examining the vote share of different incumbent parties across different levels of government over time we are able to disentangle rewards for the party executing the titling from changes in the clientelistic relationship between local incumbent parties and voters.

We focus on electoral results at the precinct level—the smallest geographical electoral unit for which voting data is available—and operationalize our treatment in two ways. First, we identify the overall effect of the introduction of the titling program in the precinct across all future elections. Second, we exploit the fact that the program was implemented over several years in almost half of precincts and look at the change in titling intensity within a given precinct. We measure the effect on clientelism by looking at the vote share of municipal incumbents, since municipal incumbents are in a particularly good position to exploit the dependency of squatters upon the government. In particular, municipality governments can provide the basic infrastructure that irregular settlements lack and can condition protection from eviction and access to public good on their electoral success.

We find that land titling permanently decreases political support for municipal incumbents. The average land titling event in our sample decreases the vote share of municipal incumbents by 2.2 percentage points (or 5% of their vote tally). To alleviate the concern that these findings might be driven by differential trends in incumbent party support in areas where the titling took place, we demonstrate the validity of the identifying parallel trends assumption. First, neither changes in incumbent vote share nor changes in turnout over

---

communities were initially granted as communal lands. However, the *Programa de Certificación de Derechos Ejidales y Titulación de Solares* (PROCEDE) that started in 1992 has allocated individual land certificates since.

[4]We restrict our sample to precincts for which at least 10% of their population is part of the ejido or agrarian community that was affecting by titling by the CORETT. Our results are robust to stricter sample restrictions.

time predict the occurrence of land titling. Second, the inclusion of up to third-order lags of the corresponding treatment variable never significantly predicts electoral performance. Third, the results are robust to including state-, municipality-, and ejido-specific time trends.

We supplement these findings with further evidence consistent with a break down in clientelism driving our baseline result. First, the electoral prospects of state and federal incumbents, who are less likely to rely on weak property rights for the enforcement of their clientelistic exchanges, are not significantly harmed by the land titling program. Second, the prevalence of other sources of clientelism (in particular, government employment) ameliorates the negative impact of the land titling program on the electoral support of municipal incumbents.

However, we also rule out a series of alternative interpretations of our results. Examining the ideological leanings of municipal incumbents, we show that our findings are not explained by a shift in preferences toward more market-oriented parties (de Janvry, Gonzalez-Navarro and Sadoulet 2014). This result suggests that the findings are unlikely to be explained by a switch in political preference due to greater economic prospects (Di Tella, Galiani and Schargrodsky 2007), better labor market opportunities (de Janvry et al. forthcoming; Field 2007), or enhanced credit and investment opportunities (Field and Torero 2008; Field 2005) that could be associated with the program for its beneficiaries. We then show that the alignment of the municipal government with higher levels of government does not protect municipal incumbents from the negative electoral effect of the titling program. Given that such alignment increases the capacity of municipal governments to provide public goods, this result indicates that our findings are unlikely to reflect a lack of resources for municipal incumbents to deliver the public goods that they are obliged to provide after the allocation of property rights.

Finally, we find that voters indeed reward municipal and federal incumbents for the implementation of the program when the party in power is responsible for its implementation at the federal level. For such parties, these rewards offset the cost of breaking down clientelism at the municipal level, and entail a net gain at the federal level. We are thus able

7

to address a key empirical challenge in the literature, namely, differentiating the impact of a program on clientelism from its impact on voter appraisal of the incumbent's policy performance. Moreover, our findings help explain the puzzle of why incumbent parties implement programs and reforms that might hurt their ability to engage in clientelistic exchanges (de Janvry, Gonzalez-Navarro and Sadoulet 2014). As in the case of Mexico's CCT program, Progresa, in the late 1990s, our results suggest that a historically clientelistic government may transition toward programmatic programs in an attempt to hold onto power at the federal level.

These findings contribute to various literatures. First, our paper is naturally related to the literature that looks at the institutional changes that led to the demise of clientelism in the developed world. Stokes et al. (2013) study the collapse of machine politics in the United Kingdom and the United States. Related, Folke, Hirano and Snyder (2011*a*) and Ujhelyi (2014*a*,*b*) show how civil service reforms short-handed the ability of clientelistic parties to use government employment and resources to reward voters. We instead focus on a major developing context where clientelistic exchanges remain prevalent, and thus show how policy reforms can break down such ties in a contemporary setting.

We also reinterpret recent work highlighting the significant effect of programmatic policies on incumbent support (De La O 2013; Manacorda, Miguel and Vigorito 2011; Zucco 2013). De La O (2013) and Manacorda, Miguel and Vigorito (2011) document a short-run electoral effect, and argue that such an effect is consistent with voters updating about the incumbent's ability or willingness to deliver programmatic policies. Zucco (2013) further argues that the effect is short-lived and that programmatic policies are unlikely to induce substantial long-term realignment of voter. Our distinctive empirical strategy instead shows that incumbents implementing programmatic policies enjoy an electoral reward in both the short and the long run, but also provides evidence that the ability to engage in clientelistic exchanges is severely curtailed for future incumbents. Moreover, our analysis illuminates the relative impact of these opposing effects on the electoral performance of the incumbents implementing programmatic policies. In particular, we show that performance varies by

level of government: in the program that we study, while municipal incumbents that are more dependent on weak property rights for the enforcement of their clientelistic exchanges suffer from the breakdown of clientelism, higher levels of governments enjoy the electoral rewards of constituents pleased with the switch toward programmatic policies.

Lastly, our work is closely related to Castañeda Dower and Pfutze (2015) and de Janvry, Gonzalez-Navarro and Sadoulet (2014), who look at the effect of an agricultural land certification program on electoral behavior in Mexico. While both papers examine the same CORETT program, they focus on two different outcome variables—the vote share of the centrist National Action Party (PAN) and the clientelistic Institutional Revolutionary Party (PRI), respectively—and reach two different conclusions. While de Janvry, Gonzalez-Navarro and Sadoulet (2014) argue that the wealth effect induced by the program led to a shift in votes towards the right,[5] Castañeda Dower and Pfutze (2015) argue the program broke down clientelistic transactions. By focusing on the vote share of municipal and federal incumbent parties, and looking at heterogeneous effect by their ideological position, we are able to arbitrate between the arguments proposed by de Janvry, Gonzalez-Navarro and Sadoulet (2014) and Castañeda Dower and Pfutze (2015). In contrast to these studies, we are able to isolate the effect of property rights on clientelistic ties from the rewards associated with implementing the program.

The next section describes the use of clientelism in Mexico, its relationship to the lack of property rights, and the CORETT land titling program. Section 1.3 describes the data used in the analysis and our empirical strategy. Section 1.4 reports the results identifying the impact of the program on clientelism and rewards for the incumbent party as well as the robustness checks that we perform. Section 1.5 concludes.

---

[5]These findings are also consistent with a breakdown of clientelistic transactions, since incumbents and more clientelistic parties in Mexico are located disproportional at the left and center.

1.2 Background

We focus on informal settlements located on urban land belonging to an ejido or agrarian community in Mexico that participated in the the CORETT land titling program. These settlements are distributed across 463 municipalities (19% of all municipalities in Mexico) from all 31 Mexican states excluding the Federal District (Table 1.1).[6]

The PRI is the most common municipal incumbent in our panel (60% of precinct-elections), followed by PAN (26%), and then the Party of the Democratic Revolution or PRD (13%). The distribution of municipal incumbents resembles the distribution across all municipalities over the period, as shown in Table 1.2. Even though these areas are particularly vulnerable to clientelism, they have also experienced significant land titling reforms that could break those clientelistic ties but also generate electoral rewards for the incumbent.

1.2.1 Clientelism in urban settlements without property rights

There is a strong symbiotic relationship between clientelism and the lack of property rights in urban Mexico. First, the illegal occupation of urban land has historically been supported largely by politicians or brokers in order to establish and secure a captured base of clients.[7] Considerable evidence from focus groups we conducted and the press demonstrates that either politicians or brokers with political connections in the government have encouraged individuals to illegally take possession of land or illegally purchase land while offering protection against government intervention.[8] In return for their protection,

---

[6]We exclude the Federal District from our sample since there are no municipal governments in it, and the analogous local governing bodies have responsibilities that differ greatly from those of the municipal governments.

[7]This situation is not unique to Mexico. For example, Fox (2014) and Hansungule, Feeney and Palmer (1998) provide evidence of similar situations in Sub-Saharan Africa, while Clichevsky (2003) offers similar accounts from the Greater Buenos Aires in Argentina.

[8]For evidence from the press, see "Grandes asentamientos," El Universal, July 19th 2000; "Habitan familias en riesgo total," El Sol de Puebla, July 23rd 2009; "Vecinos trabajando," El Universal, May 19th 2013. Participants in focus groups conducted in former squatter communities that benefited from titling by the CORETT often reported that the occupation of their lands was mediated by intermediaries with connections with the municipal

Table 1.1: *Distribution of Municipalities in our Sample*

	Full Sample	Our sample	
State	Number of municipalities	Number of municipalities	% over total
Aguascalientes	11	7	63.6%
Baja California	5	4	80.0%
Baja California Sur	5	2	40.0%
Campeche	11	5	45.5%
Chiapas	118	19	16.1%
Chihuahua	67	16	23.9%
Coahuila	38	13	34.2%
Colima	10	7	70.0%
Durango	39	14	35.9%
Guanajuato	46	21	45.7%
Guerrero	81	19	23.5%
Hidalgo	84	20	23.8%
Jalisco	125	35	28.0%
México	125	29	23.2%
Michoacán	113	34	30.1%
Morelos	33	19	57.6%
Nayarit	20	7	35.0%
Nuevo León	51	6	11.8%
Oaxaca	570	11	1.9%
Puebla	217	18	8.3%
Querétaro	18	10	55.6%
Quintana Roo	10	5	50.0%
San Luis Potosí	58	12	20.7%
Sinaloa	18	15	83.3%
Sonora	72	14	19.4%
Tabasco	17	5	29.4%
Tamaulipas	43	11	25.6%
Tlaxcala	60	5	8.3%
Veracruz	212	58	27.4%
Yucatán	106	13	12.3%
Zacatecas	58	9	15.5%
Total	2441	463	19.0%

Table 1.2: *Distribution of Municipal Incumbents in our Sample, by Party*

Municipal incumbent	All Municipalities · years		Our sample Municipalities · years	
	Number	%	Number	%
PRI	190,957	56.1%	13,308	57.97%
PAN	101,994	29.9%	6,030	26.27%
PRD	38,928	11.4%	2,947	12.84%
Other	8,774	2.6%	673	2.93%

brokers sometimes charge minor fees.[9] However, the main political motive for encouraging squatting is to condition the permanence of squatters on their political support (Díaz 2008; Flores Rodríguez 2008).[10] While in some cases there are direct threats of eviction by incumbent politicians and brokers, in other cases squatters are indirectly threatened with the possibility of eviction if another party comes to power (Flores Rodríguez 2008).

Second, regardless of whether party officials or brokers offer protection or threaten eviction in exchange for political support, the possibility of a legal land title is an important political asset. Incumbent politicians and brokers often stress the importance of their continuity in power for titling to occur (Flores Rodríguez 2008). Some parties have gone even further by issuing certificates of possession free of charge to residents of several illegal settlements. Since these certificates were issued by specific political parties, squatters feared that they would be evicted if there was a change in power (see Holzner 2004 for an example from a PRI municipality). It is also common that incumbent officials explicitly condition titling opportunities on electoral support, in some cases requesting the formal affiliation of

government, who later continued to intermediate between them and the government.

[9]See "Grandes asentamientos," El Universal, July 19th 2000.

[10]Díaz (2008) also argues that often the lack of political support for the regularization of property rights originates from the fact that politicians did not want to lose the control over the voters in irregular settlements.

community members to the party (Varley 1994).[11]

Third, the illegal use of land in irregular urban settlements, together with the initial lack of provision of basic public services in such settlements, creates other opportunities for political intermediation (Vite Pérez 2001).[12] Since the municipal government is not obliged to—and probably should not—provide public services when land is occupied illegally, squatters become easy prey for political clientelism (Garcés Fierros 2009).[13] There are several accounts of public officials justifying the lack of public service provision in irregular settlements due to the lack of property rights (Varley 1994).[14] Moreover, the inability to provide formal proof of residence has prevented squatters from accessing social programs (Varley 1994).[15] The inability of squatters to legally demand that the government provide them with public services and social programs creates opportunities for political manipulation. Their precarious conditions make squatters highly dependent upon the government for social and unemployment plans and housing, which they cannot legally request because of their illegal residency (Villalón 2003). This weak position is often exploited by local politicians and political brokers who assist squatters with their demands in exchange for their votes in elections.[16]

Political intermediation takes many forms. In some cases leaders of irregular settlements are co-opted by incumbent parties, such that settlements are required to affiliate with the party in order to gain access to government benefits both for themselves and their communities (Holzner 2004). In other cases, the government promotes the creation of community

---

[11]See "Solapa Gudiño asentamientos irregulares: PAN," Imagen del Golfo, May 8th 2013.

[12]This is not unique to Mexico. Gay (1990, 1994), Burgwald (1996), Auerbach (Forthcoming) and Kuehl (2013) provide evidence that party officials intervene to direct public services to people in slums in Brazil, Ecuador, India and Peru respectively.

[13]See "Vecinos trabajando," El Universal, May 19th 2013; "Piden reubicar a dos millones; familias viven en 500 mil casas de alto riesgo," Excelsior, September 30th 2013.

[14]Varley (1994) mentions the interesting case of a public official that stated that they would not provide public services in no man's land.

[15]See "En la capital hay 8 mil familias en extrema pobreza," La Jornada Aguascalientes, August 2nd 2013.

[16]This situation is well characterized by Shami and Majid (2014) in the case of Pakistan.

associations, or exploits exiting ones by staffing their committees with individuals with close ties to the incumbent party or by directly appointing incumbent party officials (Trujeque Díaz 1997; Vite Pérez 2001). In the absence of community association leaders, intermediation is often undertaken by traditional brokers and party officials.

Regardless of whether it is because of a threat of eviction, protection, promise of land, or in exchange of public services, there is abundant evidence that people in irregular urban settlements are disproportionally subjected to political mobilization and illegal electoral practices (Holzner 2004). There is extensive evidence of squatters being mobilized to attend political rallies—often without knowing who and they are mobilizing for.[17] Importantly, individuals living in such settlements are also subject to significant turnout buying. Although *acarreo*, which involves transporting voters to polling stations, is illegal under Article 403 of the Mexican Federal Penal Code (Larreguy, Marshall and Querubin forthcoming), there are abundant newspaper accounts documenting its extensive use in irregular settlements by hired coaches and especially groups of taxi drivers.[18]

Combined, this evidence suggests that the establishment of formal property rights could substantially break down clientelistic interactions facilitated by the existence of illegal urban settlements that generate a key dependence of voters on local politicians. Some accounts suggest that the titling provided by the CORETT ended the historical clientelistic business of parties.[19]

1.2.2 CORETT and credit claiming

Due to the large number of irregular settlements spread out over social property (i.e. ejidos and agrarian communities), the administration of Luis Echeverría (1970-1976) decided to

---

[17]See "Vecinos trabajando," El Universal, May 19th 2013; "Se manifiestan vecinos de Los Volcanes en Cabildo contra comerciantes y concejal," Sistema Radiofónico Informativo, October 8th 2014.

[18]See "Cerraron gasolinerías en Cancún para evitar acarreos," Cronica, July 7th 2005; "Sustitutos de última hora en colonias irregulares," Por Esto Quintana Roo, no date.

[19]See "La situación política económica y social del estado de Baja California Norte vista desde abajo y hacia la izquierda," Rincón Rupestre, October 5th 2006.

create the Committee for the Regularization of Land Ownership (Comité para la Regularización de la Tenencia de la Tierra, CORETT) in 1973, which had limited reach and resources. Its purpose was to regularize the informal settlements located both in federal and social property. The following year the Committee was advanced to the rank of Commission and endowed with more resources and the power to expropriate land for subsequent tiling. Since 1979, the Commission has restricted its work to the regularization of social land in urban areas (Varley 1994; Díaz 2008; Flores Rodríguez 2008).

Since its conception, the CORETT has played a major role in the regularization of urban land in Mexico. By 2008, in its 35th anniversary, the CORETT had benefited 2.16 million households across Mexico. It is is estimated that 8.64 million individuals, 11.5% of the Mexican urban population, benefited from the scheme. In 35 years of work, the CORETT contributed to the titling of around 150 thousand hectares, which accounts for approximately 10% of urban land in Mexico (Carreras López 2008).

The program operates as follows. The CORETT first identifies an urban settlement located on land belonging to an ejido or agrarian community. The land is then expropriated from the ejido or community members formally owning the land in exchange for economic compensation reflecting the commercial value of the land. Once the CORETT takes possession of the land, it conducts a census of the squatters and the plots they occupy. This information then informs the (highly-subsidized) price set for squatters to acquire formal property rights over the land they occupied. Moreover, squatters are informed about the social programs that they can potentially have access to if they purchase the land. While the CORETT does not supply these social programs itself, it can channel the potential beneficiaries to the relevant institutions that provide these.[20] Lastly, the squatters have to formally request that their land be regularized, provide evidence that they indeed occupy their land, and make the necessary payments (Carreras López 2008; SEDESOL 2011).

---

[20]For example, since 2008 the Secretariat of Social Development (*Secretaría de Desarrollo Social*, SEDESOL) provides support to the squatters who are in extreme poverty through the Program to Support Settlers in Situations of Poverty to Regularize Irregular Human Settlements (*Programa de Apoyo a los Avecindados en Condiciones de Pobreza Patrimonial para Regularizar Asentamientos Humanos Irregulares*, PASPRAH).

We are currently in the process of acquiring a better understanding of the determinants of the choice of squatter settlements overtime. The rules governing the operation of the CORETT limit the scope for discretion in the titling of land across squatter settlements. The results in Table 1.3 also indicate that it is not the case that specific parties were more likely to engage in land titling over time within the precincts in our sample. Moreover, as we show later, Table 1.5 in Section 1.4 indicates that past performance of municipal and federal incumbent, as well as turnout behavior, are not associated with land titling.

The rules governing the operation of the CORETT program limit the scope for discretion in the titling of land within squatter settlements, and consequently for clientelism. However, CORETT officials make sure that all levels of government that are aligned with the party controlling the federal government receive credit for titling events. Several accounts present state governors, and even sometimes the president, delivering property rights themselves. In many cases, these efforts to enable such politicians to present the transfer of right has significantly delayed the granting of property right certificates (Varley 1994). In the absence of the president or state governor, the CORETT public officials in charge of distributing property rights repeatedly mention how instrumental the state governor and president were for the titling, as well as the sensitivity of these politicians toward the needs of poorer voters and their willingness to engage in the efforts required to help those in greatest need.[21] Moreover, it is often emphasized that access to formal property rights over their land will permit access to low-cost credit and social programs, which will contribute to the well-being of the beneficiaries.[22]

---

[21]See "Entregan Herrera Caldera y Corett títulos de propiedad a 110 familias," La Prensa, November 6th 2013; "Ramírez Marín entrega más de 5 mil títulos de propiedad en Jalisco," Secretaría de Desarrollo Agrario, Territorial y Urbano, February 25th 2014.

[22]See "Ramírez Marín entrega más de 5 mil títulos de propiedad en Jalisco," Secretaría de Desarrollo Agrario, Territorial y Urbano, February 25th 2014.

Table 1.3: *Treatment Assignment by Party Identity of Municipal Incumbent*

	Indicator of new titling at t				Share of voters that received title at t			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PRI municipal incumbent at t	-0.0023	-0.0023	0.0075	0.0123	0.0015	0.0005	0.0014	0.0017
	(0.0116)	(0.0112)	(0.0147)	(0.0146)	(0.0017)	(0.0019)	(0.0021)	(0.0022)
PAN municipal incumbent at t	-0.0076	-0.0047	0.0064	0.0119	0.0012	0.0008	0.0016	0.0018
	(0.0121)	(0.0105)	(0.0135)	(0.0134)	(0.0016)	(0.0015)	(0.0019)	(0.0020)
PRD municipal incumbent at t	0.0043	0.0062	0.0132	0.0148	-0.0005	-0.0004	-0.0002	0.0000
	(0.0103)	(0.0114)	(0.0141)	(0.0143)	(0.0015)	(0.0019)	(0.0022)	(0.0023)
Number of years with titling up to t-1	-0.0911***	-0.1062***	-0.1594***	-0.2429***				
	(0.0114)	(0.0088)	(0.0078)	(0.0085)				
Stock of voters that received title up to t-1					-0.1043***	-0.1095***	-0.1345***	-0.1714***
					(0.0051)	(0.0049)	(0.0061)	(0.0047)
Includes state trends		X				X		
Includes municipality trends			X				X	
Includes ejido trends				X				X
Observations	140,134	140,134	140,134	140,134	140,134	140,134	140,134	140,134
R-squared								
Number of precincts	4,540	4,540	4,540	4,540	4,540	4,540	4,540	4,540
Mean LHS	0.0776	0.0871	0.1199	0.1633	0.0757	0.0790	0.0958	0.1168

Notes: All regressions are at precinct level and include precinct-fixed effects. "Mean stock of voters" is a dummy that takes value 0 if the precinct hasn't received land titles before t, or the mean number of voters affected by the program at election time, where the mean is taken over all elections for which the program had already reached the precinct. "Stock of voters (t)" is share of voters in the precinct that received titles before t. Errors are clustered at municipal level. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

1.3 Data and empirical strategy

1.3.1 Data

Through several freedom of information requests, we obtained data from the CORETT on all the land owned by ejidos and agrarian communities where each CORETT titling occurred, the date when titling events started, and the number of households that benefited in each case. We matched this to the *Padrón e Historial de Núcleos Agrarios* (PHINA) of the *Registro Agrario Nacional* (RAN), which contains all the ejido and agrarian communities that were expropriated by the CORETT, including their unique RAN identifiers. For each ejido and agrarian community, we identified its geographical location using the spatial database of the *Programa de Certificación de Derechos Ejidales y Titulación de Solares* (PROCEDE). To locate beneficiaries, we use data on the spatial location of rural localities and urban blocks, together with the population in each, from the *Instituto Nacional de Estadística y Geografía* (INEGI). Finally, to link this to electoral units, we intersected this with the location of Mexico's 67,000 electoral precincts using spatial data from the *Instituto Federal Electoral* (IFE).[23]

We first retrieved the following variables from the CORETT data on the ejidos and agrarian communities: where each CORETT titling event took place, the date when those events started, as well the number of households that benefited in each case. To identify the spatial location of the beneficiaries of each titling, we used two data sources. First, we took from the *Padrón e Historial de Núcleos Agrarios* (PHINA) of the *Registro Agrario Nacional* (RAN), which contains all the ejido and agrarian communities that were expropriated by the CORETT, and the unique RAN identifiers for each of these.[24] Second, using the unique RAN identifiers for each ejido and agrarian community, we identified their geographical location in the spatial database of the *Programa de Certificación de Derechos Ejidales y Titulación de Solares* (PROCEDE).

To determine the share of voters in each precinct that benefited in each case of CORETT

---

[23]The IFE has since become the Instituto *Nacional* Electoral.

[24]The data was scrapped from http://phina.ran.gob.mx/phina2/ by Melissa Dell, who generously shared it with us.

titling, we exploited two spatial databases. First, we used data on the spatial location of rural localities and urban blocks, together with the population in each, from the *Instituto Nacional de Estadística y Geografía* (INEGI). Second, we combined this with data on the spatial location of the Mexican precincts from the *Instituto Federal Electoral* (IFE). We first intersected these two spatial databases to assign each rural locality and urban block to a precinct.[25] We then assigned each rural locality and urban block to an ejido or agrarian community. Using these two assignments, we distributed the number of households that benefited in each CORETT titling event across the precincts following population shares of each ejido and agrarian community across the precincts they overlap with. Lastly, assuming that every household that benefited in each CORETT titling had two registered voters, we computed the share of voters in each precinct that benefited from each CORETT titling.

We use electoral returns from the IFE and State Electoral Institutes for every available precinct in each municipal and presidential elections between 1994 and 2013.[26] We focus on precincts that were reached by the land titling program, which leaves us with 4,277 unique precincts. Figure 1.1 shows the length of our panel on municipal elections for the precincts in our sample.

There is extensive variation in the timing of the land titling implementation, as depicted in Figure 1.2.

Moreover, there are two sources of variation in treatment intensity. First, ejidos (or agrarian communities) and precincts do not fully coincide. An ejido can cover parts of several precincts, and precincts can intersect more than one ejido, which generates variation in the exposure—the proportion of voters affected—of each precinct to the titling program. Figure 1.3 shows the distribution of surface area that is covered by ejidos that were subject to land titling at some point. Second, precincts vary in the number of times that they received

---

[25]At the end of this procedure we only keep precincts where at least 10% of their population is part of the ejido or agrarian community that was effected by titling by the CORETT. Our results are robust to stricter sample restrictions.

[26]For federal elections, we use data from the 1994, 2000, 2006, and 2012 presidential elections. Municipal and state-level elections are often not held concurrently.

Figure 1.1: *Length of Panel for Precincts in our Sample*
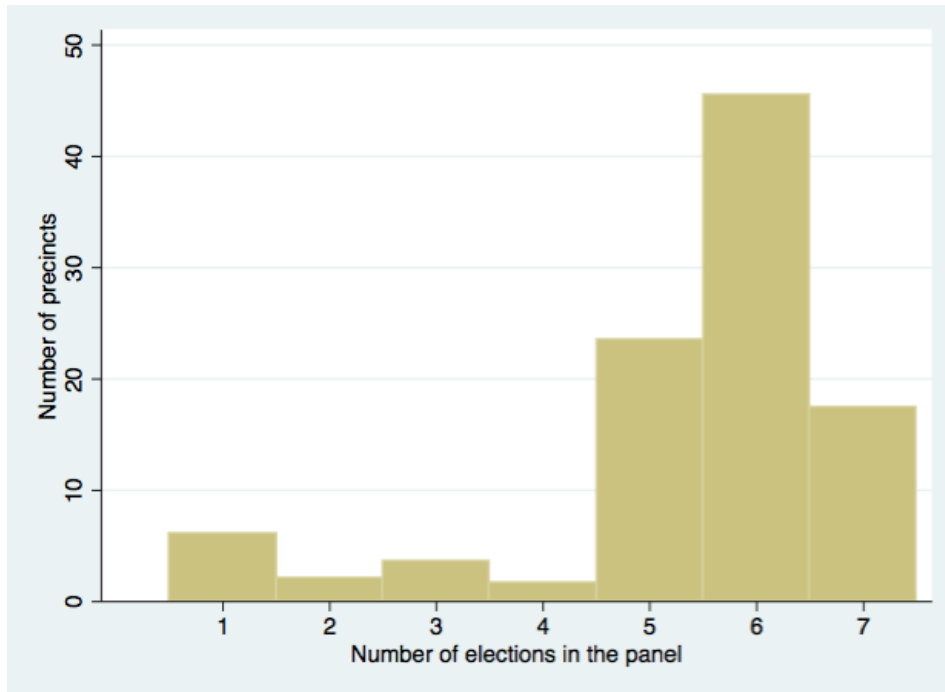
titles. Figure 1.4 shows that in almost half of the precincts, the program was implemented over several (far from consecutive) years.

1.3.2 Empirical Strategy

Municipal incumbents are in a particularly good position to exploit the dependency of squatters upon the government, as they have the closest connections with the community, provide the public goods to squatters lack the most, and can condition the permanence of the squatter in the lands they occupy and public good access on their electoral success. Accordingly, we assess the effect of property rights on clientelism by looking at changes in the vote share of municipal incumbent parties after the land titling program is implemented by the CORETT. Since our regressions do not track one particular party but hold for municipal incumbents regardless of their affiliation, our estimates are designed not to capture a shift in voters' political preferences over parties, but a change in the clientelistic relationship between incumbent parties and voters.

Figure 1.2: *Distribution of Land Titling over Time*

Note: In 26 cases there were two titlings at t for a precinct, corresponding to two different ejidos
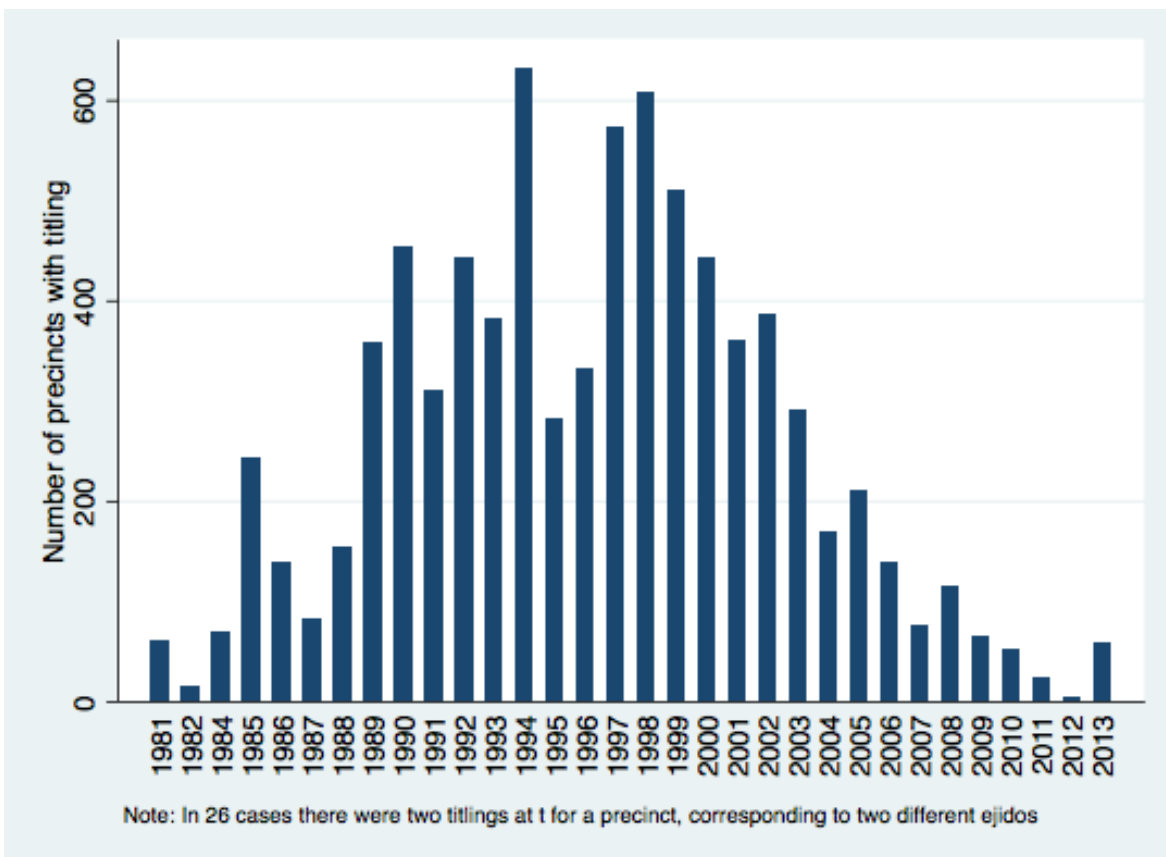
Figure 1.3: *Share of Precinct Land Affected by Land Titling*
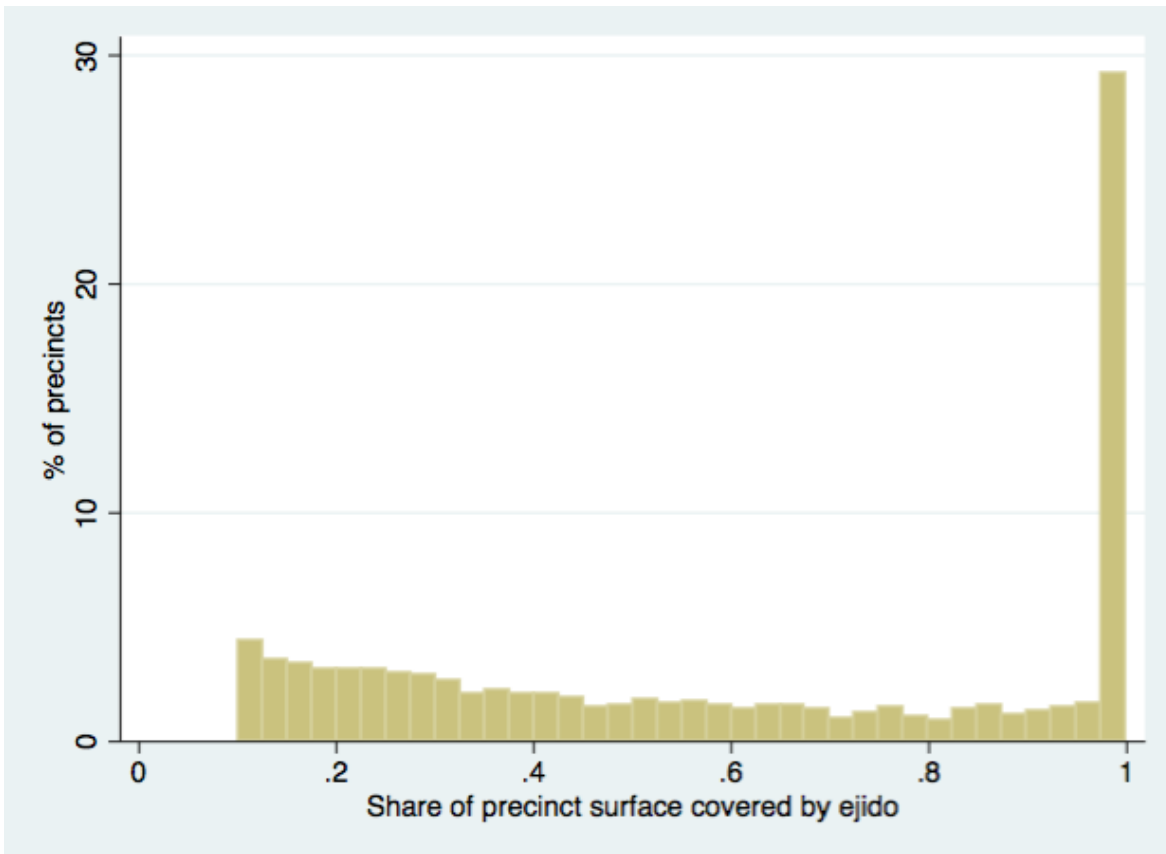
Notes: To minimize measurement error in the spatial assignment of precincts to ejidos, we censor our sample at 10% of the share of precinct land that intersects with an ejido affected by land titling.

22

Figure 1.4: *Number of Times Land Titles Were Distributed in a Precinct*
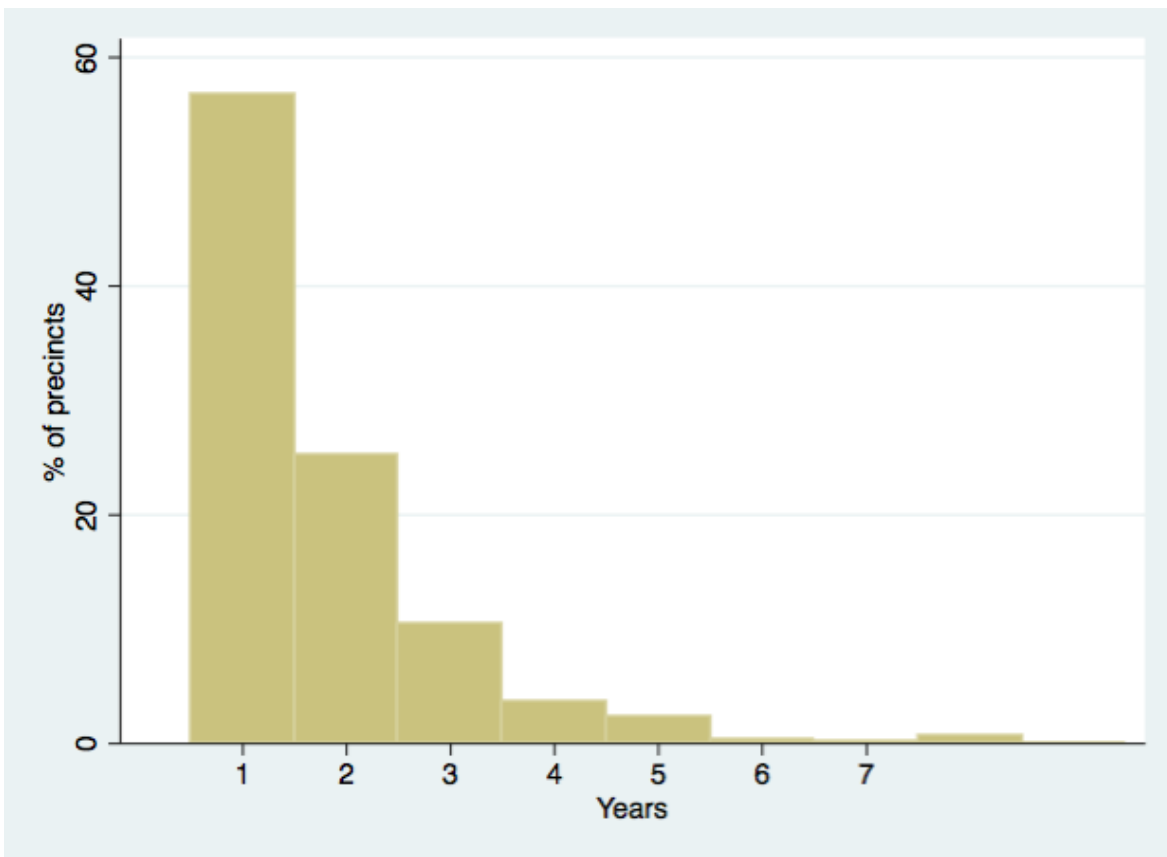
We use a difference-in-differences design to identify the effect of the land titling program. In particular, we exploit within-precinct variation in the exposure to the program to measure its impact on the municipal incumbent's vote share. This allows us to circumvent potential concerns about the correlation between unobservable but fixed characteristics of the precincts and the allocation of the program.

We use two measures of land titling. First, we consider whether the program had already reached the precinct by the time of an election. To take into account the fact that the program had different intensity in different precincts, our variable takes value 0 before the start of the program, and the average share of voters that had received a title by election time, where the average is calculated over all post-program elections in our panel for that precinct. Importantly, this variable does not change if a precinct experiences a second round of land titling. We thus estimate the following regression using our measure of existing program intensity, D_{pt}:

$$Y_{pt} = \beta D_{pt} + \delta_t + \eta_p + \varepsilon_{pt}, \tag{1.1}$$

where Y_{pt} is the outcome of interest in precinct p at election year t, δ_t are election year fixed effects, and η_p are precinct fixed effects. The election year fixed effects control for common trends in our outcomes of interest. Throughout, we cluster errors by municipality.

Our second measure of land titling captures the effect of an increase in the intensity of the program over time in a given precinct. In almost half of the precincts, the program was implemented over several (not necessarily consecutive) years. We use the following specification to measure the impact of an additional increment in the share of voters that receive a title:

$$Y_{pt} = \beta S_{pt} + \gamma S_{pt-1} + \delta_t + \eta_p + \varepsilon_{pt}, \tag{1.2}$$

where S_{pt} is the share of voters that had received a title before election year t in precinct p. Equation (2) allows us to exploit significantly more variation in the treatment as the roll out of the land titling program started before our panel for municipal elections and only 9.5%

of the precinct-year observations are not affected by the program in our data.

Our design relies on the assumption that the timing of land titlings is not correlated with changes in incumbent electoral support for any other reason than the treatment of interest. The main threat to our identification assumption is the strategic allocation of the program to areas where the outcome is trending in a particular direction. We start addressing the validity of our empirical strategy by showing no association between the program allocation and recent changes in incumbent electoral support. To further test our identifying assumption, we then test for the significance of up to three lags of the corresponding treatment variables. Through our different specifications we also show the robustness of our results to the inclusion of state-, municipality-, and ejido-specific time trends.

Programs or reforms that affect the return of engaging in clientelistic transactions might also have direct effects on voters' electoral behavior. This makes it difficult to isolate its impact on clientelism. The land titling program provides an ideal setting to separate the two effects because rewards (or punishments) are likely to be targeted towards the specific incumbent party executing the titling, but the ability to engage in clientelistic practices is affected for all future local incumbents.

For each election and each precinct, we compute $IncTitling_{pt}$ as the share of years when the program was executed that the current municipal incumbent party was the federal incumbent at the time. As noted above, the federal government is responsible for the program. We thus separate the such credit claiming effects from the effects working through the potential breakdown of clientelistic ties using the following specifications:

$$Y_{pt} = \alpha D_{pt} + \beta IncTitling_{pt} + \gamma \left(D_{pt} \times IncTitling_{pt} \right) + \delta_t + \eta_p + \varepsilon_{pt}, \qquad (1.3)$$

$$Y_{pt} = \alpha S_{pt} + \beta IncTitling_{pt} + \gamma \left(S_{pt} \times IncTitling_{pt} \right) + \delta S_{pt-1} + \kappa IncTitling_{pt-1}$$
$$+ \rho \left(S_{pt-1} \times IncTitling_{pt-1} \right) + \delta_t + \eta_p + \varepsilon_{pt}, \qquad (1.4)$$

where the coefficients on $D_{pt} \times IncTitling_{pt}$ in equation (3) and on $S_{pt} \times IncTitling_{pt}$ in equation (4) capture the rewards or punishment incurred by the municipal incumbent due

25

to previous titling in the precinct. To further assess the credit claiming channel, we use the same approach to examine how titling differentially affects the federal incumbent's vote share in federal elections when it presided over past land titling.

1.4   Results

1.4.1   Clientelism breakdown and robustness checks

As argued above, the benefits from clientelistic practices whose enforcement rely on weak property rights mostly accrue to municipal incumbents. Consequently, any change in the underlying costs and benefits of clientelism should be mostly reflected in the vote share of municipal incumbents in municipal elections. Table 1.4 assesses this claim, reporting the effect of land titling on the vote share of the municipal incumbent party in municipal elections. Column (1), which corresponds to equation (1), shows the effect of first exposure to the program, taking into account the overall reach of the program in the precinct. For a precinct with average initial exposure to the program, the land titling program decreases municipal incumbent's vote share by 2.2 percentage points.[27] That is, in a precinct with average exposure to the program at election time, we expect an average decrease of 2.2 percentage points in the vote share of future municipal incumbents. This represents approximately a 5% decrease in the vote share for the average municipal incumbent. Columns (2), (3), and (4) consecutively demonstrate the robustness of this result to the inclusion of trends at the state, municipality, and ejido levels respectively. The negative effect remains significant despite the loss of precision, and statistically indistinguishable from the estimate in column (1).

Column (5), which corresponds to equation (2), reports the results of an increase in the intensity of the program over time. The results show that an average increase in land titling intensity leads to an additional 0.93 percentage points loss of vote share for municipal

---

[27]The average stock of voters exposed across municipalities is 20.5%. We calculate the effect of the program as 0.205* $\times$ (-0.1077) = -0.022.

Table 1.4: *Effect on Vote Share of Municipal Incumbent in Municipal Elections*

	Vote share of municipal incumbent in municipal elections							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean stock of voters with title	-0.1077***	-0.0843***	-0.0739**	-0.0707*				
	(0.0267)	(0.0267)	(0.0301)	(0.0373)				
Stock of voters with title (t)					-0.0654***	-0.0517**	-0.0497*	-0.0547*
					(0.0245)	(0.0259)	(0.0264)	(0.0320)
Stock of voters with title (t-1)					-0.0100	-0.0162	-0.0083	-0.0169
					(0.0246)	(0.0238)	(0.0245)	(0.0281)
Includes state trends		X				X		
Includes municipality trends			X				X	
Includes ejido trends				X				X
Observations	22,477	22,477	22,477	22,477	22,477	22,477	22,477	22,477
R-squared	0.0446	0.0892	0.2333	0.2487	0.0436	0.0888	0.2331	0.2487
Number of precincts	4,202	4,202	4,202	4,202	4,202	4,202	4,202	4,202
Mean LHS	0.436	0.436	0.436	0.436	0.436	0.436	0.436	0.436

Notes: All regressions are at precinct level and include precinct-fixed effects. "Mean stock of voters" is a dummy that takes value 0 if the precinct hasn't received land titles before t, or the mean number of voters affected by the program at election time, where the mean is taken over all elections for which the program had already reached the precinct. "Stock of voters (t)" is share of voters in the precinct that received titles before t. Errors are clustered at municipal level. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

incumbents, or 2.13% of their votes.[28] Columns (6), (7), and (8) similarly find no significant changes in this result once state, municipality, or ejido trends are considered. In sum, and consistent with breaking down clientelistic ties, we find that the establishment of property rights on land substantially decreases the electoral support of future municipal incumbents.

Rather than break down clientelistic ties, a potential concern is that the decrease in the municipal incumbent's vote share could simply reflect strategic allocation of the CORETT program to areas where support for the municipal incumbent is declining. If this were the case, our identifying parallel trends assumption would be violated. To address this concern, we first test whether electoral trends at the precinct level predict the allocation of the program by running the following specification:

$$N_{pt,t+1} = \beta C_{pt} + \delta_t + \eta_p + \varepsilon_{pt} \qquad (1.5)$$

where C_{pt} is a measure of electoral trends between the previous election at time $t-1$ and the election at t in precinct p, and $N_{pt,t+1}$ is a measure of land titling allocations in precinct p for the subsequent period $(t, t+1]$. We employ three measures of to capture electoral pre-trends: first, we use the change in the municipal vote share to check whether the program responds to municipal interests; second, we examine the change in the municipal turnout rate to test whether titling aims to (re) mobilize to voters; and third, we also look at the change in the federal vote share obtained by the federal incumbent at t, to check whether the program responds to federal interests. We check the predictive power of those electoral changes for two different measures of program allocation: an indicator that captures whether land titles were distributed at all between t and $t+1$, and the share of voters that received a title between t and $t+1$.

Table 1.5 presents the results. Each cell corresponds to one combination of our measures of electoral pre-trends and program allocation. None of the electoral trends significantly predicts the allocation of the program, lending support to our identifying assumption.

---

[28]The average increase in the share of voters treated by the program is 14.3 percentage points. The effect of the program is calculated as 0.143 × (-0.0654) = -0.093.

These results also challenge the hypothesis that the federal or municipal governments are promising (and delivering) future land titles in exchange for current support from voters.

To further validate our design, we test for the existence of differential trends by including lags of our treatment variables. Significant lags would suggest differential trends across precincts that vary in the number of voters receiving land titles. Tables 1.6 and 1.7 report the results of specifications including one, two and three lags. Across all specifications, the coefficient on the lags are generally small and statistically insignificant. This lends additional support to our empirical strategy. Moreover, the coefficients on the effect of the titling program on municipal incumbent's voter share remain stable and statistically indistinguishable from those obtained in our baseline estimations.

Together, the results in Tables 1.5-1.7, in conjunction with the robustness of our baselines estimates in Table 1.4 to the inclusion of state-, municipality- and ejido-level trends, suggest that our estimates are unlikely to be driven by differential trends.

1.4.2   Evidence of clientelism breakdown

While the robust results in Table 1.4 are indicative that land titling in irregular urban settlements led to a breakdown in clientelistic exchanges between municipal incumbents and voters, we provide further supporting evidence to cement this link. First, as suggested above, since municipal governments are more likely to benefit from clientelistic practices whose enforcement relies on weak property rights, such incumbents should disproportionately suffer from land titling relatively to federal and state incumbents. To test this, we replicate our estimates from Table 1.4 instead considering as an outcome the municipal vote share of the state governor and federal incumbents, as well as the federal vote share of the federal incumbent.

Tables 1.8, 1.9 and 1.10 respectively present the effect of the land titling program for state and federal incumbents in municipal elections, and for federal incumbent in federal elections. They suggest that the allocation of property rights by the CORETT program reduces support for state and federal incumbents in municipal and federal elections, although the effect is

Table 1.5: *Predicting Titling*

	Indicator new titling in the precinct (t,t+1)				Stock of voters reached by new titling (t,t+1)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A								
Δ vote share for municipal incumbent t	0.0722	0.0783	0.0344	0.0373	0.0068	0.0074*	0.0063	0.0059
(municipal elections)	(0.0591)	(0.0509)	(0.0450)	(0.0461)	(0.0042)	(0.0044)	(0.0050)	(0.0052)
Observations	18,063	18,063	18,063	18,063	18,063	18,063	18,063	18,063
R-squared	0.1316	0.1776	0.3208	0.3986	0.0692	0.0836	0.2097	0.3130
Number of precincts	4,016	4,016	4,016	4,016	4,016	4,016	4,016	4,016
Panel B								
Δ precinct turnout t	0.0077	0.0448	0.0562	0.0480	0.0044	0.0052	0.0071	0.0059
	(0.0427)	(0.0434)	(0.0478)	(0.0484)	(0.0057)	(0.0053)	(0.0053)	(0.0054)
Observations	17,387	17,387	17,387	17,387	17,387	17,387	17,387	17,387
R-squared	0.1071	0.1518	0.3238	0.4098	0.0622	0.0745	0.2286	0.3347
Number of precincts	4,016	4,016	4,016	4,016	4,016	4,016	4,016	4,016
Panel C								
Δ Vote share for federal incumbent t	0.1867	0.0417	-0.0768	-0.0841	-0.0108	0.0050	-0.0045	0.0002
(federal elections)	(0.1972)	(0.1209)	(0.0914)	(0.0955)	(0.0136)	(0.0142)	(0.0164)	(0.0162)
Observations	8,449	8,449	8,449	8,449	8,449	8,449	8,449	8,449
R-squared	0.2267	0.2996	0.5005	0.6539	0.0983	0.1807	0.3993	0.5643
Number of precincts	3,909	3,909	3,909	3,909	3,909	3,909	3,909	3,909
Includes state trends		X				X		
Includes municipality trends			X				X	
Includes ejido trends				X				X

Notes: All regressions are at precinct level and include precinct-fixed effects. "Δ vote share for municipal incumbent t" is the change in vote share between $t-1$ and t for the party that is the municipal incumbent at t. "Δ vote share for federal incumbent t" is defined in the same way but for federal incumbent and in federal elections. "Δ precinct turnout t" is the change in turnout between $t-1$ and t for the precinct. "Indicator new titling in the precinct" takes value 1 land titles were given between t (not included) and $t+1$ (included), and 0 otherwise. "Stock of voters reached by new titling" equals to the share of voters in the precinct that received a title. Errors are clustered at municipal level. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

30

Table 1.6: *Robustness Check: Effect on Vote Share of Municipal Incumbent in Municipal Elections, Up to 3 Lags. (Dummy Mean Stock)*

	Vote share of municipal incumbent in municipal elections			
	(1)	(2)	(3)	(4)
PANEL A: One lag				
Dummy mean stock of voters with title (t)	-0.0930***	-0.0736**	-0.0720**	-0.0679*
	(0.0282)	(0.0293)	(0.0323)	(0.0383)
Dummy mean stock of voters with title (t-1)	-0.0246	-0.0183	-0.0051	-0.0110
	(0.0235)	(0.0231)	(0.0246)	(0.0277)
PANEL B: Two lags				
Dummy mean stock of voters with title (t)	-0.0895***	-0.0708**	-0.0674**	-0.0632*
	(0.0286)	(0.0294)	(0.0319)	(0.0379)
Dummy mean stock of voters with title (t-1)	-0.0397*	-0.0308	-0.0146	-0.0163
	(0.0235)	(0.0235)	(0.0260)	(0.0283)
Dummy mean stock of voters with title (t-2)	0.0262	0.0228	0.0246	0.0198
	(0.0207)	(0.0207)	(0.0217)	(0.0242)
PANEL C: Three lags				
Dummy mean stock of voters with title (t)	-0.0833***	-0.0668**	-0.0640**	-0.0613
	(0.0292)	(0.0296)	(0.0316)	(0.0373)
Dummy mean stock of voters with title (t-1)	-0.0347	-0.0274	-0.0115	-0.0144
	(0.0234)	(0.0234)	(0.0257)	(0.0281)
Dummy mean stock of voters with title (t-2)	0.0077	0.0108	0.0185	0.0176
	(0.0214)	(0.0213)	(0.0239)	(0.0257)
Dummy mean stock of voters with title (t-3)	0.0363*	0.0252	0.0175	0.0093
	(0.0220)	(0.0233)	(0.0192)	(0.0218)
Includes state trends		X		
Includes municipality trends			X	
Includes ejido trends				X
Observations	22,477	22,477	22,477	22,477
Number of precincts	4,202	4,202	4,202	4,202
Mean LHS	0.436	0.436	0.436	0.436

Notes: All regressions are at precinct level and include precinct fixed effects. "Dummy mean stock of voters (t)" is a dummy that takes vaue 0 if the precinct hasn't received land titles before t, or the mean number of voters affected by the program at election time, where the mean is taken over all elections for which the program had already reached the precinct. Errors are clustered at muincipal level.

Table 1.7: *Robustness Check: Effect on Vote Share of Municipal Incumbent in Municipal Elections, Up to 3 Lags. (Stock of Voters)*

	Vote share of municipal incumbent in municipal elections			
	(1)	(2)	(3)	(4)
PANEL A: Two lags				
Stock of voters with title (t)	-0.0632***	-0.0502*	-0.0476*	-0.0514
	(0.0244)	(0.0257)	(0.0260)	(0.0313)
Stock of voters with title (t-1)	-0.0357	-0.0373	-0.0236	-0.0251
	(0.0239)	(0.0241)	(0.0267)	(0.0293)
Stock of voters with title (t-2)	0.0434*	0.0370	0.0346	0.0261
	(0.0256)	(0.0263)	(0.0250)	(0.0275)
PANEL B: Three lags				
Stock of voters with title (t)	-0.0612**	-0.0493*	-0.0472*	-0.0508
	(0.0245)	(0.0257)	(0.0259)	(0.0311)
Stock of voters with title (t-1)	-0.0341	-0.0365	-0.0225	-0.0235
	(0.0237)	(0.0239)	(0.0264)	(0.0289)
Stock of voters with title (t-2)	0.0224	0.0258	0.0251	0.0209
	(0.0270)	(0.0278)	(0.0281)	(0.0295)
Stock of voters with title (t-3)	0.0363	0.0203	0.0222	0.0176
	(0.0252)	(0.0240)	(0.0212)	(0.0229)
Includes state trends		X		
Includes municipality trends			X	
Includes ejido trends				X
Observations	22,477	22,477	22,477	22,477
R-squared	0.0445	0.0894	0.2336	0.2490
Number of precincts	4,202	4,202	4,202	4,202
Mean LHS	0.436	0.436	0.436	0.436

Notes: All regressions are at precinct level and include precinct fixed effects. "Stock of voters (t)" is the number of voters in the precinct that received titles before t. Errors are clustered at muincipal level.

smaller in magnitude and never statistically significant. In addition to further supporting our baseline estimates indicating that property rights significantly weaken the scope for clientelistic exchanges, these estimates suggest that state and federal incumbents do not rely on weak property rights to enforce their clientelistic transactions. Rather, they are more likely to depend on the significant budget they are able to allocate to social programs and public employment (Larreguy 2013).

Second, the lack of property rights on land is not the only opportunity for political intermediation and vote buying. Another potentially important way of obtaining electoral support is through public employment in municipal government. Although state and federal employees outnumber municipal employees, such employment nevertheless represents an effective local source of patronage in Mexico. Municipal incumbents can allocate positions in the local government in exchange for voter mobilization, and municipal government employees might perceive continuity in their position to be conditional upon the continuity of the municipal incumbent party (Robinson and Verdier 2013*a*). As a result, we expect municipal incumbents in municipalities with larger government employment to be less susceptible to the effects of the land titling program. To test this argument, we interact our two measures of land titling with the share of the municipal population working in the government.[29] The results in Table 1.11 show that, in precincts that belong to municipalities with a larger share of government employees, the vote share of municipal incumbents is less affected by the land titling program. This finding provides further support for our claim that the titling program affects municipal incumbents' vote share by weakening their ability to enforce clientelistic exchanges. On the other hand, the results also suggest that clientelistic ties may only be effectively severed when reforms are able target multiple tools for facilitating exchanges.

---

[29]The data originates from the INEGI census conducted every decade (1980, 1990, 2000 and 2010).

Table 1.8: *Effect on Municipal Vote Share of State Incumbent*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Vote share of state incumbent in municipal elections							
Mean stock of voters with title	-0.0069	-0.0086	-0.0104	-0.0198				
	(0.0310)	(0.0263)	(0.0320)	(0.0389)				
Stock of voters with title (t)					0.0031	0.0103	-0.0020	-0.0079
					(0.0255)	(0.0244)	(0.0266)	(0.0314)
Stock of voters with title (t-1)					-0.0009	-0.0107	-0.0214	-0.0272
					(0.0274)	(0.0244)	(0.0251)	(0.0307)
Includes state trends		X				X		
Includes municipality trends			X				X	
Includes ejido trends				X				X
Observations	23,117	23,117	23,117	23,117	23,117	23,117	23,117	23,117
R-squared	0.0442	0.1220	0.2643	0.2825	0.0442	0.1221	0.2644	0.2827
Number of precincts	4,277	4,277	4,277	4,277	4,277	4,277	4,277	4,277
Mean LHS	0.429	0.429	0.429	0.429	0.429	0.429	0.429	0.429

Notes: All regressions are at precinct level and include precinct-fixed effects. "Mean stock of voters" is a dummy that takes value 0 if the precinct hasn't received land titles before t, or the mean number of voters affected by the program at election time, where the mean is taken over all elections for which the program had already reached the precinct. "Stock of voters (t)" is share of voters in the precinct that received titles before t. Errors are clustered at municipal level. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table 1.9: *Effect on Municipal Vote Share of Federal Incumbent*

	Vote share of federal incumbent in municipal elections							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean stock of voters with title	-0.0530	-0.0456	-0.0477	-0.0478				
	(0.0331)	(0.0295)	(0.0290)	(0.0347)				
Stock of voters with title (t)					-0.0172	-0.0107	-0.0294	-0.0244
					(0.0314)	(0.0284)	(0.0287)	(0.0331)
Stock of voters with title (t-1)					-0.0068	-0.0135	0.0031	0.0071
					(0.0263)	(0.0244)	(0.0240)	(0.0268)
Includes state trends		X				X		
Includes municipality trends			X				X	
Includes ejido trends				X				X
Observations	23,117	23,117	23,117	23,117	23,117	23,117	23,117	23,117
R-squared	0.2521	0.3091	0.4010	0.4151	0.2518	0.3089	0.4009	0.4149
Number of precincts	4,277	4,277	4,277	4,277	4,277	4,277	4,277	4,277
Mean LHS	0.392	0.392	0.392	0.392	0.392	0.392	0.392	0.392

Notes: All regressions are at precinct level and include precinct-fixed effects. "Mean stock of voters" is a dummy that takes value 0 if the precinct hasn't received land titles before t, or the mean number of voters affected by the program at election time, where the mean is taken over all elections for which the program had already reached the precinct. "Stock of voters (t)" is share of voters in the precinct that received titles before t. Errors are clustered at municipal level. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table 1.10: *Effect on Federal Vote Share of Federal Incumbent*

				Vote share for federal incumbent in federal elections				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean stock of voters with title	-0.0197	-0.0128	-0.0225	0.0133				
	(0.0344)	(0.0318)	(0.0356)	(0.0463)				
Stock of voters with title (t)					-0.0076	-0.0125	-0.0140	0.0342
					(0.0336)	(0.0273)	(0.0288)	(0.0347)
Stock of voters with title (t-1)					-0.0505	-0.0558	-0.0501	-0.0371
					(0.0457)	(0.0399)	(0.0487)	(0.0555)
Includes state trends		X				X		
Includes municipality trends			X				X	
Includes ejido trends				X				X
Observations	12,602	12,602	12,602	12,602	12,602	12,602	12,602	12,602
R-squared	0.4413	0.5235	0.6091	0.6292	0.4422	0.5248	0.6098	0.6302
Number of Precincts	4,153	4,153	4,153	4,153	4,153	4,153	4,153	4,153
Mean LHS	0.509	0.509	0.509	0.509	0.509	0.509	0.509	0.509

Notes: All regressions are at precinct level and include precinct-fixed effects. "Mean stock of voters" is a dummy that takes value 0 if the precinct hasn't received land titles before t, or the mean number of voters affected by the program at election time, where the mean is taken over all elections for which the program had already reached the precinct. "Stock of voters (t)" is share of voters in the precinct that received titles before t. Errors are clustered at municipal level. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table 1.11: *Heterogeneous Effects by Share of Government Employees*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Vote share of municipal incumbent in municipal elections					
Mean stock of voters	-0.2093***	-0.1338***	-0.1133*	-0.1122				
	(0.0496)	(0.0452)	(0.0593)	(0.0690)				
Mean stock of voters × Share govt. employees	9.1843**	4.4420	3.1150	3.5226				
	(3.6862)	(3.0651)	(3.6595)	(4.6390)				
Stock of voters with title					-0.1784***	-0.1461***	-0.1525***	-0.1582**
					(0.0473)	(0.0492)	(0.0560)	(0.0623)
Stock of voters with title×Share govt. employees					8.4298***	6.9881**	7.2432**	7.6144**
					(3.1635)	(2.9908)	(3.1749)	(3.6485)
Stock of voters with title (t-1)					0.0041	0.0037	0.0273	0.0242
					(0.0433)	(0.0426)	(0.0452)	(0.0515)
Stock of voters with title t-1×Share govt. employees					-1.0722	-1.5256	-2.4892	-2.7190
					(2.2912)	(2.2425)	(2.1798)	(2.6962)
Includes state trends		X				X		
Includes municipality trends			X				X	
Includes ejido trends				X				X
Observations	22,164	22,164	22,164	22,164	22,164	22,164	22,164	22,164
R-squared	0.0457	0.0888	0.2311	0.2467	0.0451	0.0891	0.2315	0.2472
Number of precincts	4,088	4,088	4,088	4,088	4,088	4,088	4,088	4,088
F-statistic	19.38	10.51	5.885	3.795	13.68	7.405	6.566	5.307
P-value	1.34e-05	0.00128	0.0157	0.0520	0.000243	0.00676	0.0107	0.0217
Mean LHS	0.437	0.437	0.437	0.437	0.437	0.437	0.437	0.437

Notes: All regressions are at precinct level and include precinct-fixed effects. "Mean stock of voters" is a dummy that takes value 0 if the precinct hasn't received land titles before t, or the mean number of voters affected by the program at election time, where the mean is taken over all elections for which the program had already reached the precinct. "Stock of voters (t)" is share of voters in the precinct that received titles before t. Errors are clustered at municipal level. The F-statistic and P-value correspond to a test of(Mean stock of voters) + (Mean stock of voters)*(Share govt. employees)* Standard Deviation(Share govt. employees), where Standard Deviation(Share govt. employees) is the sample standard deviation of the share of government employees. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

1.4.3 Alternative explanations

Our empirical analysis focuses on the electoral outcomes of incumbent parties as opposed to the outcomes of particular parties to avoid the concern that our results are, in line with the positive wealth effect of the program, confounded by a shift in voters preferences toward more right-wing parties (de Janvry, Gonzalez-Navarro and Sadoulet 2014). However, such concern remains if, as suggested by Table 1.2, incumbents and more clientelistic parties in Mexico are disproportionately leftist or centrist. This concern is supported by work suggesting that the granting of property rights improves expectations over economic prospects and leads to better labor market, credit and investment opportunities (de Janvry et al. forthcoming; Di Tella, Galiani and Schargrodsky 2007; Field 2005, 2007; Field and Torero 2008), as well as by talks with beneficiaries of the CORETT's titling program during the focus groups that we conducted.

To address this concern, we explore how the effect of land titling in our baseline specification in Table 1.4 vary with municipal incumbent party ideology. We consider two different specifications. First, we estimate heterogeneous effects using a continuum measure of ideology that takes value -1 if the incumbent is a left-wing party (mainly the PRD, the Labor Party or PT, and the Citizen Movement Party or MC), 0 if it is a centrist party (mainly the PRI, the Mexican Green Party or PVEM, and the New Alliance Party or PANAL), and 1 if it is a right-wing party (mainly the PAN). Second, we estimate heterogeneous effects using an indicator of incumbent ideology—left and right—with the excluded ideology—center—being the comparison group. Tables 1.12 and 1.13 present the corresponding results, which both fail to support the possibility that our baseline estimates are driven by a rightward shift in the preferences of the voters benefited by the CORETT titling program. If anything, the results in 1.13 suggest that the allocation of property rights hurt right-wing incumbent parties.

Another concern is that our baseline results, and the differences in the results that rise when focusing on different levels of incumbency, reflect the possibility that the worse electoral performance of municipal incumbents is due to their lack of resources to deliver

Table 1.12: *Heterogeneous Effects by Ideology of Municipal Incumbent. (Continuous version)*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Vote share of municipal incumbent in municipal elections							
Dummy mean stock of voters with title t	-0.1060***	-0.0821***	-0.0716**	-0.0681*				
	(0.0263)	(0.0264)	(0.0302)	(0.0375)				
(Dummy mean stock of voters with title t)*Ideology	-0.0089	-0.0185	-0.0175	-0.0184				
	(0.0247)	(0.0233)	(0.0222)	(0.0236)				
Stock of people with title t					-0.0679***	-0.0526**	-0.0479*	-0.0526
					(0.0251)	(0.0264)	(0.0275)	(0.0337)
(Stock of people with title t)*Ideology					0.0253	0.0124	-0.0226	-0.0211
					(0.0534)	(0.0522)	(0.0521)	(0.0563)
Includes state trends		X				X		
Includes municipality trends			X				X	
Includes ejido trends				X				X
Observations	22,477	22,477	22,477	22,477	22,477	22,477	22,477	22,477
R-squared	0.0456	0.0902	0.2338	0.2492	0.0449	0.0901	0.2337	0.2493
Number of precincts	4,202	4,202	4,202	4,202	4,202	4,202	4,202	4,202
Mean LHS	0.436	0.436	0.436	0.436	0.436	0.436	0.436	0.436

Notes: All regressions are at precinct level and include precinct-fixed effects. "Mean stock of voters" is a dummy that takes value 0 if the precinct hasn't received land titles before t, or the mean number of voters affected by the program at election time, where the mean is taken over all elections for which the program had already reached the precinct. "Stock of voters (t)" is share of voters in the precinct that received titles before t. Errors are clustered at municipal level. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

Table 1.13: *Heterogeneous Effects by Ideology of Municipal Incumbent. (Categorical version)*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Vote share of municipal incumbent in municipal elections				
Dummy mean stock of voters with title t	-0.1009***	-0.0750**	-0.0634*	-0.0609				
	(0.0294)	(0.0295)	(0.0325)	(0.0388)				
(Dummy mean stock of voters with title t)*Right	-0.0438	-0.0535**	-0.0511*	-0.0520*				
	(0.0275)	(0.0261)	(0.0285)	(0.0308)				
(Dummy mean stock of voters with title t)*Left	-0.0237	-0.0209	-0.0228	-0.0216				
	(0.0379)	(0.0373)	(0.0355)	(0.0367)				
Stock of people with title t					-0.0754***	-0.0637**	-0.0602**	-0.0657*
					(0.0266)	(0.0280)	(0.0301)	(0.0349)
(Stock of people with title t)*Right					0.0104	0.0004	-0.0210	-0.0186
					(0.0516)	(0.0529)	(0.0600)	(0.0627)
(Stock of people with title t)*Left					0.0334	0.0366	0.0662	0.0697
					(0.1021)	(0.1003)	(0.1007)	(0.1124)
Includes state trends		X				X		
Includes municipality trends			X				X	
Includes ejido trends				X				X
Observations	22,477	22,477	22,477	22,477	22,477	22,477	22,477	22,477
R-squared	0.0783	0.1151	0.2467	0.2621	0.0774	0.1149	0.2470	0.2625
Number of precincts	4,202	4,202	4,202	4,202	4,202	4,202	4,202	4,202
Mean LHS	0.436	0.436	0.436	0.436	0.436	0.436	0.436	0.436

Notes: All regressions are at precinct level and include precinct-fixed effects. "Mean stock of voters" is a dummy that takes value 0 if the precinct hasn't received land titles before t, or the mean number of voters affected by the program at election time, where the mean is taken over all elections for which the program had already reached the precinct. "Stock of voters (t)" is share of voters in the precinct that received titles before t. Errors are clustered at municipal level. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

the public goods that they are obliged to provide after the allocation of property rights. As we show in section 1.2, in the absence of property rights, municipal incumbents can neglect communities of squatters on the grounds that their illegal occupation prevents them from being entitled to public goods. However, this ceases to be the case when the CORETT's program reaches those communities. To test this alternative explanation, we exploit the fact that, under such explanation, municipal governments that are aligned with either the state or federal incumbents—and consequently enjoy increased access to their resources—should be affected less by the allocation of property rights. Tables 1.14 and 1.15 report the results of specifications that respectively explore the heterogeneous effects of land titling by the alignment of municipal incumbents with the state and federal governments. The absence of significant differences provides no evidence that this competing explanation drives our results.

1.4.4 Rewards to program implementing parties

Our estimates so far reflect the overall impact of the program on the electoral support for the municipal incumbents. However, voters could also reward the parties directly involved in the land titling program (De La O 2013; Manacorda, Miguel and Vigorito 2011; Zucco 2013). Importantly, because the CORETT program is administered at the federal level, this credit claiming channel is more likely to change the conclusions of our analysis regarding the effect of the CORETT's titling program on the electoral support of state and federal incumbents. To the extent that this also operates at the municipal level, it suggests that our current estimates understate the impact of land titling on the breakdown of clientelism. In this section, we explicitly account for such effects. Since we expect that any reward or punishment for implementing the program would primarily affect the party responsible for the titling at the federal level, but the ability to engage in clientelistic practices is affects all future municipal incumbents, we can separate the two effects by exploiting variation in the extent to which parties are responsible for past titling in different precincts.

Since qualitative accounts suggest that voters know that the federal government leads the

Table 1.14: *Heterogeneous Effects by Alignment with State Government*

	Vote share of municipal incumbent in municipal elections							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean stock of voters	-0.1248***	-0.1054***	-0.0924***	-0.0934**				
	(0.0285)	(0.0297)	(0.0322)	(0.0399)				
Mean stock of voters×Alignment mun-state t	0.0206	0.0303	0.0278	0.0340				
	(0.0183)	(0.0189)	(0.0214)	(0.0228)				
Stock of voters with title					-0.0821*	-0.0765	-0.0752	-0.0852
					(0.0480)	(0.0486)	(0.0513)	(0.0583)
Stock of voters with title×Alignment mun-state t					0.0155	0.0233	0.0286	0.0365
					(0.0519)	(0.0509)	(0.0563)	(0.0602)
Stock of voters with title (t-1)					-0.0037	-0.0072	0.0034	-0.0022
					(0.0477)	(0.0480)	(0.0500)	(0.0534)
Stock of voters with title t-1×Alignment mun-state t					0.0038	0.0049	-0.0026	-0.0043
					(0.0523)	(0.0525)	(0.0554)	(0.0589)
Includes state trends			X				X	
Includes municipality trends		X				X		
Includes ejido trends				X				X
Observations	22,477	22,477	22,477	22,477	22,477	22,477	22,477	22,477
R-squared	0.0772	0.1122	0.2452	0.2607	0.0761	0.1117	0.2450	0.2607
Number of precincts	4,202	4,202	4,202	4,202	4,202	4,202	4,202	4,202
F-statistic	13.64	6.889	3.996	2.357	5.816	3.724	2.585	2.221
P-value	0.000247	0.00896	0.0462	0.125	0.0163	0.0542	0.109	0.137
Mean LHS	0.436	0.436	0.436	0.436	0.436	0.436	0.436	0.436

Notes: All regressions are at precinct level and include precinct-fixed effects. "Mean stock of voters" is a dummy that takes value 0 if the precinct hasn't received land titles before t, or the mean number of voters affected by the program at election time, where the mean is taken over all elections for which the program had already reached the precinct. "Stock of voters (t)" is share of voters in the precinct that received titles before t. Errors are clustered at municipal level. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

42

Table 1.15: *Heterogeneous Effects by Alignment with Federal Government*

	Vote share of municipal incumbent in municipal elections							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean stock of voters	-0.1071***	-0.0899***	-0.0687**	-0.0618				
	(0.0274)	(0.0272)	(0.0310)	(0.0382)				
Mean stock of voters×Alignment mun-fed t	0.0044	0.0188	-0.0034	-0.0106				
	(0.0194)	(0.0203)	(0.0226)	(0.0248)				
Stock of voters with title					-0.0839***	-0.0740**	-0.0586*	-0.0618
					(0.0323)	(0.0334)	(0.0329)	(0.0382)
Stock of voters with title×Alignment mun-fed t					0.0449	0.0565	0.0343	0.0334
					(0.0424)	(0.0416)	(0.0427)	(0.0458)
Stock of voters with title (t-1)					0.0134	0.0066	0.0095	0.0030
					(0.0357)	(0.0360)	(0.0360)	(0.0399)
Stock of voters with title t-1×Alignment mun-fed t					-0.0497	-0.0479	-0.0445	-0.0513
					(0.0487)	(0.0471)	(0.0500)	(0.0536)
Includes state trends		X				X		
Includes municipality trends			X				X	
Includes ejido trends				X				X
Observations	22,477	22,477	22,477	22,477	22,477	22,477	22,477	22,477
R-squared	0.0460	0.0919	0.2407	0.2561	0.0453	0.0917	0.2406	0.2564
Number of precincts	4,202	4,202	4,202	4,202	4,202	4,202	4,202	4,202
F-statistic	12.06	5.797	4.425	2.984	1.500	0.300	0.484	0.486
P-value	0.000564	0.0164	0.0359	0.0847	0.221	0.584	0.487	0.486
Mean LHS	0.436	0.436	0.436	0.436	0.436	0.436	0.436	0.436

Notes: All regressions are at precinct level and include precinct-fixed effects. "Mean stock of voters" is a dummy that takes value 0 if the precinct hasn't received land titles before t, or the mean number of voters affected by the program at election time, where the mean is taken over all elections for which the program had already reached the precinct. "Stock of voters (t)" is share of voters in the precinct that received titles before t. Errors are clustered at municipal level. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

43

program, the party that was in power at the federal level when land titles were distributed in the precinct should be credited for its implementation.[30] Table 1.16 reports the effect of the program on the vote share for the municipal incumbent in municipal elections according to the responsibility of the party for previous titling in the precinct.

The results indicate that voters reward parties involved in the program: municipal incumbents with more involvement in the distribution of property rights on land receiver greater political support at the municipal level. The results in column (1) indicate that a municipal incumbent with average responsibility over titling suffers a 24.4% reduction in the negative impact of the program on its vote share.[31] Even though voters reward involvement in the program, the net effect of being exposed to the program continues to be negative for municipal incumbents (as indicated by the test at the bottom the table). Controlling for state, municipality, or ejido trends does not affect this conclusion. Column (5) reports similar results for an increase in the share of voters that received a title. For a municipal incumbent with average responsibility over previous titling, the negative effect on vote share due to an increase in the intensity of the program is 48% lower.[32] Moreover, the net effect of an increase in the intensity of the program, while sill negative, stops being statistically different from zero at the 95% confidence level.

Table 1.17 shows instead the effect of the land titling program on the vote share received by the federal incumbent in federal elections. We find that the program brings substantial positive rewards for the federal incumbents responsible for the implementation of the program. In our sample, federal incumbents have an average share of participation in previous titling of 87.6%. The estimates in column (1) indicate that, in a precinct with average exposure, the rewards to a federal incumbent with an average level of involvement in the program is approximately 2.5 percentage points, or a 5% increase in the vote share of

---

[30]Omitted regressions where we run a race between all levels of incumbency also confirm this claim.

[31]The average share of previous titling that were responsibility of a municipal incumbent in our panel is 0.86 (among those incumbents that participated at all). Therefore, the average increase in vote share is $0.054 \times 0.86 = 0.464$, which is approximately 24.4% of the level effect of the program.

[32]The average increase in vote share is $0.071 \times 0.86 = 0.061$ which is approximately 48.2% of the level effect of the program.

Table 1.16: *Heterogeneous Effects of Municipal Incumbent Being the Federal Incumbent at Time of Titling*

	Vote share of municipal incumbent in municipal elections							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean stock of voters	-0.1903***	-0.1576***	-0.1257***	-0.1237***				
	(0.0278)	(0.0267)	(0.0300)	(0.0380)				
Mean stock of voters	0.0542**	0.0485**	0.0477**	0.0500**				
×Mun incumbent was fed incumbent at titling	(0.0225)	(0.0224)	(0.0196)	(0.0202)				
Stock of voters with title					-0.1264***	-0.1075***	-0.0953***	-0.1002***
					(0.0289)	(0.0292)	(0.0288)	(0.0340)
Stock of voters with title					0.0710***	0.0604***	0.0541***	0.0550**
× Mun incumbent was fed incumbent at titling					(0.0229)	(0.0227)	(0.0205)	(0.0226)
Stock of voters with title (t-1)					0.0065	0.0035	0.0225	0.0142
					(0.0274)	(0.0265)	(0.0275)	(0.0309)
Stock of voters with title t-1					-0.0088	-0.0118	-0.0225	-0.0233
× Mun incumbent was fed incumbent at titling t-1					(0.0209)	(0.0197)	(0.0231)	(0.0268)
Mun incumbent was fed incumbent at titling	0.0393***	0.0363***	0.0224**	0.0237**	0.0317**	0.0306**	0.0174	0.0196
	(0.0128)	(0.0118)	(0.0104)	(0.0110)	(0.0134)	(0.0125)	(0.0123)	(0.0142)
Mun incumbent was fed incumbent at titling t-1					0.0025	0.0023	-0.0046	-0.0027
					(0.0108)	(0.0103)	(0.0122)	(0.0146)
Includes state trends				X				X
Includes municipality trends		X				X		
Includes ejido trends			X				X	
Observations	22,477	22,477	22,477	22,477	22,429	22,429	22,429	22,429
R-squared	0.0679	0.1080	0.2416	0.2576	0.0652	0.1066	0.2417	0.2579
Number of precincts	4,202	4,202	4,202	4,202	4,202	4,202	4,202	4,202
F-statistic	18.66	12.09	5.824	3.367	2.561	1.809	1.541	1.280
P-value	1.91e-05	0.000556	0.0162	0.0671	0.110	0.179	0.215	0.259
Mean LHS	0.436	0.436	0.436	0.436	0.436	0.436	0.436	0.436

Notes: All regressions are at precinct level and include precinct-fixed effects. "Mean stock of voters" is a dummy that takes value 0 if the precinct hasn't received land titles before t, or the mean number of voters affected by the program at election time, where the mean is taken over all elections for which the program had already reached the precinct. "Stock of voters (t)" is share of voters in the precinct that received titles before t. Errors are clustered at municipal level. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

the federal incumbent.[33] An average increase in the share of voters that received a land title entails an extra 2.2 percentage points or a 4.3% increase in the vote share for incumbents with average level of involvement in the program, as reported in column (5).[34] Columns (2)-(4), and (6)-(8) show that the estimates of the electoral return of the titling program for federal incumbents is robust to the inclusion of trends.

These important findings highlight the need to disentangle the effect of programmatic policies on parties' ability to enforce clientelistic exchanges from their effect on voters' perceptions about parties' ability and willingness to implement programmatic policies. Had we restricted our analysis to the specification in Table 1.10, where there is no significant effect of the CORETT's titling program on the federal incumbent's vote share, the ultimate conclusion of this study would have been significantly different.

In sum, we find that voters reward municipal incumbents involved in the land titling program, but those rewards do not completely offset the negative impact from a break down of clientelistic ties. However, for federal incumbents responsible for the implementation of the program the gains are larger and the overall impact of the program is positive. These findings thus help to explain why federal incumbent parties implement programs and reforms that might hurt their ability to enforce clientelistic exchanges (de Janvry, Gonzalez-Navarro and Sadoulet 2014).

1.5 Conclusion

In this paper, we show that programmatic policies can simultaneously break down clientelistic ties whilst generating rewards for party responsible for the policy. In contrast with previous studies focusing on a single level of government, our analysis across local government—where clientelistic relationship can be most easily sustained—and the federal government—which was primarily responsible for the implementation of the program—

---

[33]The average stock of voters exposed across municipalities is 20.9%. Thus 0.876×0.1344×0.209 = 0.0246

[34]Using that the average increase in the stock of voters is 18.4%, we obtain that 0.876×0.1345×0.184 = 0.0217

Table 1.17: *Heterogeneous Effects of Federal Incumbents Being the Federal Incumbent at Time of Titling*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Vote share of federal incumbent in federal elections				
Mean stock of voters	-0.0702*	-0.0457	-0.0435	-0.0121				
	(0.0404)	(0.0313)	(0.0316)	(0.0520)				
Mean stock of voters	0.1344***	0.1239***	0.1053***	0.1119*				
× Fed incumbent was fed incumbent at titling	(0.0374)	(0.0321)	(0.0405)	(0.0575)				
Stock of people with title					-0.0193	-0.0093	0.0132	0.0774
					(0.0428)	(0.0294)	(0.0358)	(0.0632)
Stock of people with title					0.1345***	0.1184***	0.0938*	0.0974
×Fed incumbent was fed incumbent at titling					(0.0426)	(0.0385)	(0.0485)	(0.0685)
Stock of people with title (t-1)					0.0018	-0.0061	-0.0014	0.0190
					(0.0380)	(0.0347)	(0.0385)	(0.0454)
Stock of people with title t-1					-0.0479	-0.0577	-0.0772*	-0.0828
×Fed incumbent was fed incumbent at titling t-1					(0.0425)	(0.0416)	(0.0458)	(0.0526)
Fed incumbent was fed incumbent at titling	-0.0352***	-0.0412***	-0.0416***	-0.0513**	-0.0402***	-0.0432***	-0.0453***	-0.0598***
	(0.0129)	(0.0117)	(0.0141)	(0.0250)	(0.0121)	(0.0108)	(0.0121)	(0.0194)
Fed incumbent was fed incumbent at titling t-1					0.0155	0.0158	0.0195	0.0171
					(0.0237)	(0.0230)	(0.0249)	(0.0278)
Includes state trends		X				X		
Includes municipality trends			X				X	
Includes ejido trends				X				X
Observations	12,602	12,602	12,602	12,602	12,602	12,602	12,602	12,602
R-squared	0.4489	0.5305	0.6133	0.6333	0.4514	0.5331	0.6166	0.6372
Number of Precincts	4,153	4,153	4,153	4,153	4,153	4,153	4,153	4,153
F-statistic	3.487	7.056	4.412	8.007	8.495	9.244	6.551	11.00
P-value	0.0625	0.00817	0.0362	0.00486	0.00373	0.00250	0.0108	0.000983
Mean LHS	0.509	0.509	0.509	0.509	0.509	0.509	0.509	0.509

Notes: All regressions are at precinct level and include precinct-fixed effects. "Mean stock of voters" is a dummy that takes value 0 if the precinct hasn't received land titles before t, or the mean number of voters affected by the program at election time, where the mean is taken over all elections for which the program had already reached the precinct. "Stock of voters (t)" is share of voters in the precinct that received titles before t. Errors are clustered at municipal level. * denotes $p < 0.1$, ** denotes $p < 0.05$, *** denotes $p < 0.01$.

is able to differentiate clientelistic from credit claiming forces associated with a major land titling program. In particular, we identify persistent losses among future municipal incumbents, especially among those lacking alternative resources to support clientelistic transfers. Alongside the break down of clientelistic ties, we also find evidence of effective credit-claiming by the federal incumbent. While the losses associated with a decline in clientelistic capacity of a municipal incumbent are partially offset by alignment with the federal incumbent, we find clear evidence that the federal incumbent is in fact rewarded for its part in implementing the program. In addition to showing how the provision of property rights can break down clientelistic ties, which represent a major challenge to democratic and economic development across the developing world, our findings also explain why federal incumbents may wish to implement popular programmatic policies that nevertheless reduce their capacity to harvest votes by exploiting the dependence of voters on local government.

Chapter 2

Political Turnover, Bureaucratic Turnover, and the Quality of Public Services[1]

2.1 Introduction

Party turnover entails not only a change of politicians in office, but also a change of officials within the government. These officials typically hold strategic positions, which allows politicians to gain control over the bureaucracy and facilitate the implementation of new policies. However, party turnover can also permeate and potentially disrupt the appointment process at lower levels of the bureaucracy.

In fact, the perception that the bureaucracy may be inefficiently exposed to political factors is one of the reasons for the widespread adoption of civil service reforms in developed countries, and for its advocacy in developing countries. Such reforms are thought to improve the quality of public goods and services by insulating the bureaucracy from political pressures, encouraging its professionalization, and fostering the use of meritocratic appointments. However, an independent bureaucracy can also become entrenched and

---

develop weak incentives to perform. Hence, the ability of politicians to appoint and dismiss government employees can not only smooth the implementation of new policies, but can also strengthen the accountability of bureaucrats, potentially improving the delivery of public goods and services. Overall, the effect of the intervention of politicians in the bureaucracy is *ex ante* ambiguous.

This paper uses a regression discontinuity design on close mayoral elections in Brazil to identify the effect of party turnover on teachers' and headmasters' appointments and the consequences for students' performance. Several reasons make education an ideal public service to focus on. First, education provision is a crucial government function and includes one of the largest numbers of employees in Brazilian municipalities. Second, education quality has a precise and meaningful measure, namely, students' test scores and dropout rate. Third, the quality of education is particularly sensitive to the rotation of teachers and headmasters, the public servants in charge of its delivery.

We find that teachers' and headmasters' appointments are affected by the political process. When a new party takes office, there is a 10 percentage point increase in the share of teachers who are replaced, and a 24 percentage point increase in the share of headmasters who are replaced. The majority of the incoming teachers are hired using short-term contracts and have not gone through the formal civil service screening process. Likewise, most of the appointments for headmasters do not follow strict meritocratic rules but are designated by politicians. This is consistent with anecdotal evidence about the use of school positions for electoral purposes.

Party turnover also has real and persistent effects on education outcomes: in municipalities with a new party, the year after the election tests scores of fourth graders are 0.13 standard deviations lower and students' dropout rate is 37% higher. The deterioration of educational outcomes seems to persist. Three years after the election, test scores are still lower and dropout rates are still higher in municipalities with party turnover. This suggests that even if the new party intended to improve the quality of education, the overall effect of party turnover on education is systematically negative, at least up to three years after

the election. We believe that a more likely explanation is that the deterioration in education quality is an unintended consequence of the disruption to the school-level bureaucracy that obeys political reasons.

Several pieces of evidence suggest that party turnover affects students' educational attainment through the turnover of teachers and headmasters. First, we do not find evidence that political turnover changes the resources received by the municipality or the allocation of funds to education. This is evidence against the hypothesis that political turnover lowers educational outcomes because a new political party faces difficulties in securing funds for education provision. Second, we do not find evidence of differences in teachers' characteristics, which suggests that the results are not driven by a change in their composition. Moreover, it also suggests that the appointment of teachers by the new political party is not an attempt to draw in better-qualified teachers, at least on observable characteristics. Third, students' characteristics are similar as well; thus, sorting of students into (or out of) municipal schools does not seem to explain the results. However, we do find evidence that points to the existence of organizational maladjustments that may affect the functioning of the schools: newly-appointed headmasters in municipalities with political turnover have less experience in that role, and teachers' reports about the headmaster are more likely to be negative.

We are likely capturing the causal effect of political turnover. Treatment and control municipalities are remarkably balanced on observables. Moreover, we do not detect manipulation of electoral outcomes: the McCrary test does not find a significant discontinuity in the distribution of municipalities around the cutoff. It may still be the case that voters punish incumbent parties in municipalities where the quality of education is falling. However, students' performance in treatment and control municipalities is similar in levels and trends prior to the election. Finally, results are robust to the inclusion of covariates, to the use of a bandwidth smaller than optimal, and to controls for party identity or ideology.

We also find that the disruption of the bureaucracy is not circumscribed to education services. We find that when a new party takes office, there is a significant increase in the

total number of municipal employees: the share of new employees during the first year is 11 percentage points higher. This increase is concentrated during the first months after the new party takes office, and is not compensated with a corresponding increase in the share of employees leaving the municipal government, at least for the two years following the election. Therefore, a change in parties inflates the bureaucracy. We thus believe that the increase in turnover at the school level is part of a more general phenomenon; the main difference is that the number of positions at the school level is naturally restricted by the number of schools managed by the municipality.

Our work contributes to a growing empirical literature on the relationship between bureaucrats and politicians and the relevance of the structure of the bureaucracy for government performance. Iyer and Mani (2012) document how politicians in India use frequent reassignments to control bureaucrats with career concerns, and they find evidence that political transfers of bureaucrats is associated with increases in a poverty index. Rasul and Rogger (2014) find that more autonomous bureaucrats are more likely to successfully complete projects in Nigeria. Their finding is consistent with cross-country evidence on the relevance of bureaucrats' autonomy. Rauch (1995) and Rauch and Evans (2000) find a significant relationship between the level of "weberianism" of the bureaucracy[2] and different measures of government performance. Interestingly, they find that meritocratic recruitment is particularly predictive of positive bureaucratic performance, which is consistent with our results using microeconomic data on education.

This paper also contributes to a body of work concerned with the relevance of teachers' and headmaster' stability for students' outcomes. Within this literature, Miranda and Pazello (2014) is close to our work. They are interested in measuring the impact of headmaster turnover on students' performance in Brazilian municipalities, and instrument for headmaster turnover with party turnover. We are more focused on the political economy side of the story: we believe that the disruption of the bureaucracy is interesting on its own

---

[2]In his classic *Economy and Society* (1922), Weber argues that a clear separation between the bureaucracy and politicians facilitates capitalist growth. Concretely, this separation includes the use of meritocratic recruitment and rules that ensure predictable career paths within the bureaucracy.

right, and we explore it at length. Whereas our results suggest that the disruption at the school level has real costs for educational attainment, there is some work that suggests that teachers' job protection can also be harmful. In India, teacher unionization is associated with lower student achievement (Kingdon and Teal 2010). Along the same lines, the use of contract teachers and the increase in the flexibility of appointments improves teachers' effort and students' performance (Muralidharan and Sundararaman 2013; Jacob 2010). Even though this literature suggests that teachers' entrenchment is a real problem, our results indicate that political appointments may not be the right way of providing incentives to public servants in the school.

Our findings suggest that positions at the school level in Brazil are used as electoral patronage. From a theoretical standpoint, it is argued that patronage can indeed be electorally advantageous as it solves a commitment problem between candidates and voters (Robinson and Verdier 2013*b*). Consistent with this view, the incumbency advantage in US states during the 20th century was more pronounced under patronage systems than civil service systems, potentially contributing to party entrenchment (Folke, Hirano and Snyder 2011*b*). Furthermore, the shift towards civil service systems has been found to have real effects on government outcomes. Rauch (1995) studies the adoption of civil service in US cities during the Progressive Era, and finds that the lengthening of bureaucratic tenure led to an increase in the pursuit of longer-gestation projects. Whereas these papers compare outcomes under different institutional arrangements, we do so for different political situations within a given institutional context.

More broadly, our paper also contributes to an extensive literature on the costs and benefits of institutions that promote political turnover. Term limits are one prominent institutional feature along these lines. There are theoretical reasons as to why term limits are beneficial (Smart and Sturm 2013); in practice, however, term limits have been found to have some costs for government performance. For instance, Ferraz and Finan (2011) find that term-limited incumbents are associated with an increase in corruption in Brazil and Besley and Case (1995) find that term limits affect policy variables, in particular they increase taxes

and government expenditures. On the other hand, Besley, Persson and Reynal-Querol (2013) argue that, from a theoretical point of view, high turnover can lead incumbents to adopt stronger executive constraints. These papers focus mainly on the effects associated with the expectation of turnover, while we are concerned with the effects of the execution of turnover. Moreover, a large literature studies the effect of a change in party identity on the characteristics of adopted policies, mostly in developed countries. As an example, Ferreira and Gyourko (2009) use close elections to study partisanship in US local politics. They find that the party identity of the politician does not affect the size of city government, the allocation of local public spending, or crime rates. In contrast, our results point to the existence of a cost that, in principle, is unrelated to the identity of the party, but hinges instead on the actual execution of political turnover.

The rest of the paper is organized as follows. Section 2 describes the institutional context and the data. Section 3, discusses the empirical strategy. Section 4 shows results on the effect of party turnover on turnover at the school level. Section 5 reports the results for the quality of education provision. Section 6 discusses potential mechanisms that could account for the results found in section 5. Section 7 presents further robustness checks for our results at the school level. Section 8 looks at the impact of party turnover on municipal employees more generally. Section 9 concludes.

2.2 Institutional context and data

2.2.1 Institutional context

This section provides information about Brazilian municipalities and mayoral elections, as well as the education sector in Brazil.

There are 5,570 municipalities in Brazil. Municipal elections are held in October every four years on the same day across the country. Mayors take office in January the following year. Mayors can hold office for only two consecutive terms; they have the possibility to return after a one-term hiatus. Municipal governments are highly decentralized and

autonomous. Furthermore, municipal employment is a large part of public sector employment: municipal employment was 47% of public employment in 2002 and 52.6% of public employment in 2010. Therefore understanding how the political process affects municipal employment is very important in this context.

The education sector in Brazil is the ideal setting for studying the "transition costs" of turnover for several reasons. First, education is the main responsibility of local governments. They are mandated to allocate 25% of their revenue to education (this is in addition to any funding the municipality gets for education from the federal government). Additionally, local governments control a sizable portion of public schools: in 2007, 80% of all public schools were managed by municipal governments. The municipality is responsible for all decisions regarding the daily operations of the school: distribution of school lunches, providing school transportation, and the hiring, paying, and training of teachers. Mayors are required by law to hire teachers according to very strict procedures: potential teaching candidates participate in a widely advertised exam and those with the highest rank are hired as teachers and granted job security. However, in practice, mayors exert considerable discretion in hiring temporary teachers using short-term contracts.[3] School principals often report that dealing with a new team every year is a frequent and major challenge.[4] Mayors are also responsible for hiring headmasters in some municipalities. In 6.3% of municipalities, headmasters are hired using a public exam, in 43% of municipalities, they are elected by the public, and in 45% of municipalities, they are nominated by politicians (*Associa Nacional de Pradua e Pesquisa em Educa*). The school headmaster is often a teacher from the same school who is promoted or elected to the headmaster position. The second reason for focusing on education is due to how education is funded in Brazil. Most of the funds for education,

---

[3]The mayor of Areia, Paraiba, was prosecuted for hiring numerous short-term teachers without going through official procedures (http://www.liberdade96fm.com.br; accessed on October 20, 2014).

[4]Principal and school staff report that around 35% of their teachers turn over every year. They feel managing the school under these conditions is very challenging. As the Secretary of Education of Saulo has put it: "High rotation rate transforms the school into a space without soul, where a lot of people circulate with no commitment to the school collective." (http://gestaoescolar.abril.com.br/formacao/rotatividade-professores-483054.shtml; accessed October 20, 2014).

especially those funds that ensure the daily operations of the schools, such as funds for teacher wages and textbooks, come from a federal fund called FUNDEF, a non-discretionary fund that pays a fixed rate per student enrolled in municipal schools.[5] Thus the funding of the daily operations of the education sector is less likely to be affected by political cycles and political alliances. The third reason for focusing on education goes beyond the institutional context of Brazil and is related to the production function of education. The quality of education is more likely to depend on inter-personal relationships and organizational norms, as compared to, for example, road construction. Therefore education, in any country, is precisely where the costs of turnover of employees can have pronounced effects. Finally, contract teachers are becoming increasingly common in developing countries (Muralidharan and Sundararaman 2013) which renders education provision susceptible to political factors.

2.2.2 Data

Our empirical analysis combines four Brazilian data sources. First, we use data on electoral outcomes of local government executive bodies (e.g. mayor position in each municipality). Second, we use data on formal workers in Brazil. In particular, we focus on employees in municipal governments. Third, we use data on several aspects of education provision including measures of educational quality, such as test scores and dropout rates, and key inputs in education, such as student's socio-economic background, teachers' characteristics and teachers' (re)assignment. Fourth, we complement these data with municipalities' socioeconomic characteristics.

The election results come from the *Tribunal Superior Eleitoral* which oversees all local, state, and federal elections in Brazil. We use this data to construct the vote margin of the candidate with the highest votes compared to the vote margin of the incumbent party for the 2008 election.[6]

---

[5]See Menezes-Filho and Pazello (2007) for a description of FUNDEF.

[6]We do not consider earlier elections because some of our outcome variables are missing for previous election cycles. Soon outcome variables for 2013 will be released and we will be able to incorporate the 2012 election cycle into our empirical analysis.

The data on Brazilian public workers comes from the Ministry of Work and Employment of Brazil. This dataset has an annual frequency, and follows all employees in the formal sector. It contains information about the position, wage, and basic demographic characteristics of employees. We use the data that corresponds to the period 2007 to 2009.

The data on education outcomes are from two different sources. The first is the *Censo Escolar* which is the annual survey filled by every school in Brazil. A large share of the educational budget is determined based on the enrollment figures in this census, and in recent years, the government has begun auditing the information from this census; thus misreporting can have consequences. Therefore we believe this survey is accurate and reliable. We use *Censo Escolar* from three years, 2007, 2009 and 2011, to construct the following outcomes: teachers' turnover (percent and number of teachers who departed and entered a school between 2007 and 2009, or between 2007 and 2011) and characteristics of movers (age and years of schooling of teachers who departed and entered a school between 2007 and 2009, or between 2007 and 2011). The survey also asks for information on the schools' facilities and infrastructure: whether the school is located in an urban or rural area, whether the principal has an office, whether there are administrative and/or educational computers, etc. We use the information on school facilities and infrastructure as controls and to check for balance between treatment and control municipal schools. The reference date of the survey is May 31st of every year. So the survey should be thought of as an assessment of the school five months after the mayor has been in power. The second source for education outcomes is *Prova Brasil,* a standardized national exam taken by all public school attendees, in 4th and 8th grade, with 20 or more students. This exam is conducted every two years. We use four years of data (2005, 2007, 2009, and 2011) to construct our test score measures. *Prova Brasil* also conducts surveys about all students who take the exam, the associated teacher, and the associated headmaster. We use the headmaster survey to construct our measure of headmaster turnover. Moreover, these surveys from *Prova Brasil* provide us with demographic and socioeconomic characteristics of students, teachers, and headmasters. The exam is taken around mid-November and should be thought as an assessment of the

school eleven months after the mayor has been in power. Figure 2.1 illustrates the timing of election, *Censo Escolar*, and *Prova Brasil*.

Figure 2.1: *Timeline of Election and Data Collection*
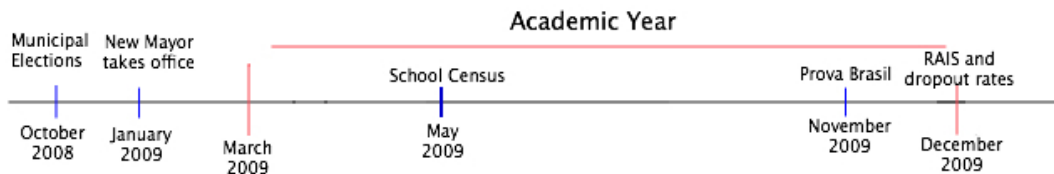
The last source of data is *Instituto Brasileiro de Geografia e Estatistica* (IBGE). We use this source to gather information such as population, GDP per capital, and average years of schooling in the municipality.

2.3 Empirical strategy

To estimate the effect of party turnover[7] on bureaucratic turnover and other outcomes, one cannot compare the outcomes of interest in municipalities that experience a change in the ruling party with municipalities that do not experience a change in the ruling party. A municipality with an incompetent ruling party is likely to have bad outcomes across numerous measures, and the constituency may be more likely to vote for a change in the next election cycle. On the other hand, constituents of a municipality with an effective local government and good provision of public goods and services may be more likely to vote to keep the same party in the next election cycle. Therefore, comparing outcomes of these two sets of municipalities could result in a correlation between party turnover and outcomes that does not reflect the causal effect of interest. In order to identify the impact of a change in the ruling party, we compare municipalities where the incumbent party barely

---

[7]We have also analyzed the effect of incumbent turnover (not reported here). Results are similar but somewhat weaker.

lost (treatment municipalities) with municipalities where the incumbent party barely won (control municipalities). That is, we use a sharp regression discontinuity design on close elections.

Our main specification is a local linear regression. For our main outcomes of interest (headmaster turnover, teachers turnover, test scores, and dropout rates) we have calculated the optimal bandwidth using both the procedures proposed by Imbens and Kalyanaraman (2012) and by Calonico, Cattaneo and Titiunik (2014). In order to keep the sample consistent across different estimations and to minimize bias concerns from using a larger-than-optimal bandwidth , we fix the bandwidth to be 5%, the smallest of all the optimal bandwidths estimated. We define close elections as those where the difference between the vote share of the incumbent party and the vote share of the best opponent falls within that range. Or main specification is as follows:

$$Y = \alpha + \beta D_m + \gamma \times Margin_m + \delta D_m \times Margin_m + \varphi X + \varepsilon; \qquad (2.1)$$

where Y is the outcome variable of interest (bureaucratic turnover, students' test scores, teacher and headmaster turnover, etc), the variable D_m is a dummy variable equal to one if the incumbent party loses, *i.e.* there is party turnover; $Margin_m$, is the difference between the vote share of the incumbent party and the vote share of the best opponent party; X is a set of controls that includes the baseline value of the dependent variable, municipal characteristics, school characteristics, and students' characteristics (whenever appropriate). Given that our treatment is at the municipality level, we cluster all standard errors at the municipality level.

As a robustness check, we also report the results of the following specification:

$$Y = \alpha + \beta D_m + \varphi X + \varepsilon; \qquad (2.2)$$

for municipalities where the margin of win is less than 3%. Since the number of municipalities in this sample is small, we do a comparison of means, as opposed to controlling for margin of win, in order to avoid overfitting problems. Furthermore, we

estimate a cubic specification using the sample with 25% margin of win.

The identification assumption is that outcomes are continuous around the cutoff, thus any discontinuity is the result of the treatment assignment. While it is not possible to fully test this assumption, Table 2.1 shows that municipalities are remarkably similar in terms of observables prior to the election. We do not find evidence of differences in economic or demographic characteristics of the municipalities, schools, or students therein; this lends support to our identification assumption.

Table 2.1: *Balance Check*

Sample: 5% window		Uncond. Mean		Balance		
	N	**Control**	**Treat**	**Diff**	**S.E.**	**P-val**
Municipality Level Characteristics						
Average years of schooling 2004	585	4.213	4.254	-0.133	0.192	0.489
GDP per capita 2007	594	5.160	6.076	1.087	0.838	0.195
Pop share w/Bolsa Familia 2007	594	0.0793	0.0788	0.00686	0.00725	0.345
Unemployment rate 2000	593	9.795	10.34	0.798	0.989	0.420
Population 2007	594	18834	19609	891.2	4,752	0.851
Public sector/GDP	594	0.260	0.244	-0.0157	0.0215	0.466
Federal transfers/GDP	572	2.256	1.987	-1.760	1.246	0.158
State transfers/GDP	572	0.688	1.321	2.876	2.729	0.292
Education expend/Total expend	580	0.465	0.468	0.0196	0.0250	0.433
Share of right wing parties 2007	594	0.00623	0.0183	0.0356	0.0241	0.140
Share of centrist parties 2007	594	0.729	0.755	0.0476	0.0767	0.535
Share of left wing parties 2007	594	0.265	0.227	-0.0832	0.0748	0.267
Bureaucracy Characteristics						
Number of bureaucrats 2007	594	423.3	482.6	22.28	108.7	0.838
Quantity hired 2007	594	107.6	127.1	-40.28	39.75	0.311
Quantity left 2007	594	73.28	85.75	-9.560	24.31	0.694
Share hired 2007	593	0.360	0.318	0.202	0.122	0.0995
Share left 2007	593	0.384	0.340	0.121	0.148	0.412
Average age 2007	536	39.70	39.69	-0.304	0.413	0.463
Primary education 2007	594	151.0	171.9	22.96	40.36	0.570
Highschool education 2007	594	182.7	204.3	4.379	53.28	0.935
College+ education 2007	594	123.9	147.7	-35.78	47.37	0.450
Wage rate (% of min wage) 2007	536	2.118	2.181	-0.0405	0.109	0.711
Seniority 2007	536	98.91	97.55	-3.303	5.802	0.569

Table 2.1: *Balance Check (continued)*

Sample: 5% window		Uncond. Mean		Balance		
	N	Control	Treat	Diff	S.E.	P-val
Hours worked 2007	536	36.82	36.53	-0.531	0.915	0.562
Under contract 2007	594	98.58	125.3	27.05	51.09	0.597
Has tenure 2007	594	345.1	378.8	-27.82	91.28	0.761
Under temporary contract 2007	594	13.88	19.84	-7.682	12.29	0.532
Municipal School Characteristics						
Average number of teachers 2007	2,752	17.79	18	-1.110	1.644	0.500
% urban	2,706	1.256	1.291	0.0759	0.0770	0.324
% pipe water 2007	2,705	0.787	0.798	-0.116	0.0846	0.169
% pipe sewer 2007	2,705	0.368	0.410	0.00347	0.0953	0.971
% headmaster has office 2007	2,706	0.792	0.785	-0.0769	0.0574	0.181
% has computer lab	2,706	0.243	0.286	-0.0115	0.0668	0.863
% bathroom inside 2007	2,706	0.961	0.956	-0.0546	0.0302	0.0711
Dropout rate 2007	2,465	3.735	3.579	-0.0978	1.0325	0.925
Head politically appointed 2007	1,835	0.333	0.394	0.0336	0.0812	0.680
Share of tenured teachers 2007	1,876	0.674	0.690	-0.00614	0.0798	0.939
Baseline Scores						
Math 4th grade (standardized)	1,963	-0.0553	0.0437	0.164	0.221	0.458
Portuguese 4th grade (stand)	1,963	-0.0466	0.0306	0.172	0.216	0.426
Math 8th grade(standardized)	717	-0.0425	0.0687	-0.248	0.268	0.355
Portuguese 8th grade (stand)	717	-0.0532	0.0791	-0.0929	0.274	0.735
Students Characteristics						
% with computer	2,760	0.198	0.208	-0.0171	0.0364	0.638
% lives with both parents	2,557	0.693	0.699	0.0164	0.0250	0.511
% works	2,715	0.199	0.183	-0.0190	0.0216	0.378
% has car	2,747	0.326	0.348	-0.0281	0.0546	0.607
% sees mother reading	2,738	0.844	0.854	0.0232	0.0273	0.395
% male	2,735	0.508	0.509	0.00512	0.0217	0.813
Age	2,754	11.87	11.79	-0.0421	0.237	0.859

Notes: All calculations are made over the sample within the 5% bandwidth. The unconditional means are just the sample means for the treatment and control municipalities. The values reported in the balance correspond to the variable "political turnover" in the local linear regression that is our main specification throughout the paper. Errors are robust for municipality level characteristics, and bureaucracy characteristics. Errors are clustered at

the municipality level for school characteristics, scores, and students characteristics.

Another threat to the validity of the empirical strategy is the possibility of manipulation of vote shares in close elections, in a way that correlates with other characteristics that affect our outcomes of interest. For instance, more corrupt incumbents may be both more likely to commit fraud in the election and more likely to provide ineffective public goods and services. Figure 2.2 shows the distribution of municipalities according to the vote margin of the incumbent party. The distribution is fairly smooth around the cutoff, and the McCrary test fails to reject the null hypothesis of no manipulation of the running variable.

Figure 2.2: *McCrary Test*
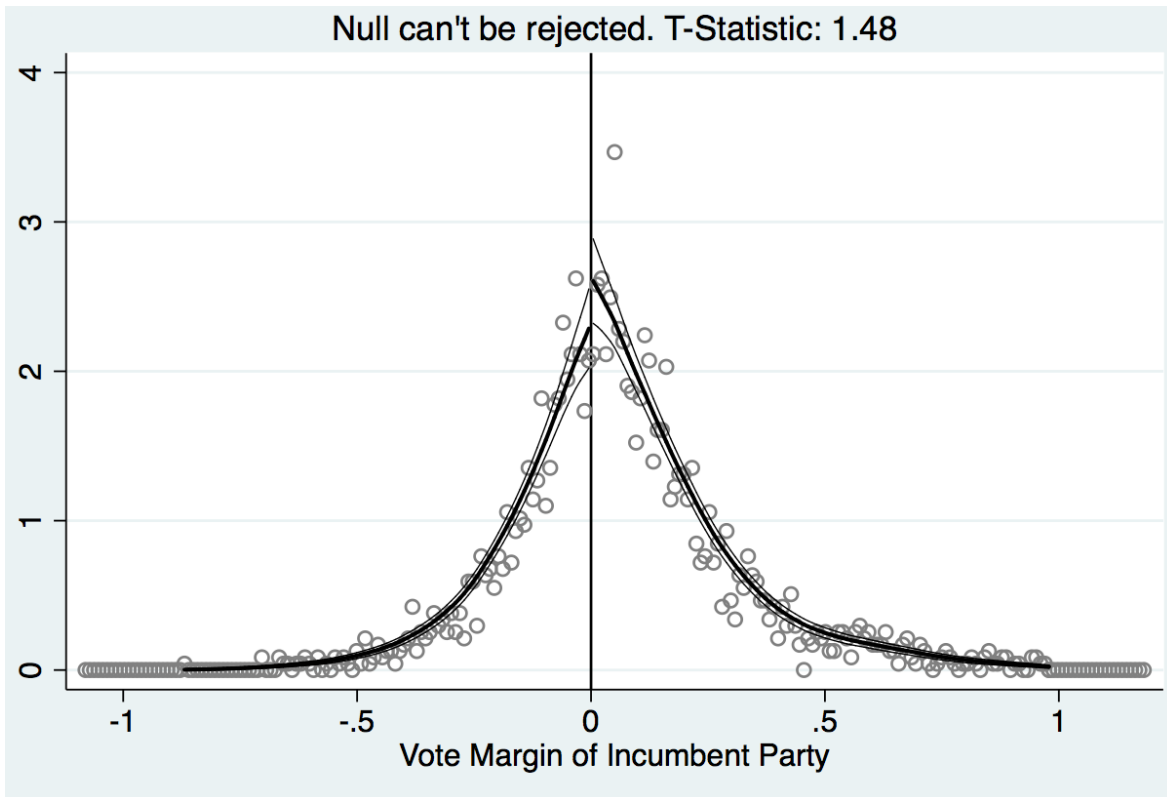
Notes: This figure reports the density of municipalities on the running variable, i.e. vote margin of incumbent party.

In sum, we do not find evidence of differences in characteristics across control and treatment municipalities or sorting around the cutoff, which suggests that the identification assumption for our RDD strategy holds.

2.3.1 Our sample

A number of steps restrict the sample to be used in our estimations. First, our running variable is defined only for municipalities where the incumbent party is running for reelection. Of the 5,570 municipalities in Brazil, we identified 2,659 (48%) that meet this criteria. This number is relatively low because in many cases the incumbent party supports another party in the 2008 elections (coalitions are a common feature of local politics in Brazil). Second, we focus on municipalities where the party taking office was chosen in the first round. This means we exclude 18 municipalities where the incumbent party ran for reelection and the election went to the second round: Second-round elections occur in municipalities with more than 200,000 inhabitants where no candidate reaches the 50% threshold for an outright win. The sample also excludes municipalities that had supplementary elections, which happens when null votes are enough to change the result of the election. This is the case for 70 municipalities. Of the remaining 2,572 municipalities, close elections (5% margin) account for 594 municipalities, which is our final sample. Moreover, since *Prova Brasil* is taken only when 20 or more students are present in the classroom, our sample contains the largest 36% of primary municipal schools in those municipalities.

In other words, our results are representative of municipalities where the incumbent party ran for reelection and the winner was defined in the first round, and where elections were close. These municipalities are smaller, slightly richer, with relatively smaller schools, lower dropout rates and approximately 6% more of the headmaster positions appointed by politicians.

2.4 Effect of party turnover on teachers and headmasters

Headmasters play a key role in the implementation of government policies, disciplining of teachers, overseeing the academic curriculum, and providing cohesion within the school.

Headmasters are more likely to be replaced when a new party takes office. Figure 2.3 shows the share of headmasters that have been in that position (within the same school) for less than two years as of November 2009, as a function of the margin of win of the incumbent party. There is a clear discontinuity in the share of new headmasters with turnover: approximately 24 percentage points more headmasters are new.

The estimates in Table 2.2 confirm this result. Moreover, the result is robust to the inclusion of controls, and to a narrower bandwidth, lending support to the causal interpretation of our finding. In Table 2.2 we also see that the rate of change of headmasters in the control group is high: even without party turnover, approximately 40% of the headmasters change. This is the rate accumulated over 2 years, which partially accounts for its high level; even so, it is consistent with public's perception and anecdotal accounts of a pervasive problem of high turnover in the education system in Brazil.

The effect of party turnover is not restricted to headmasters. Figure 2.4 shows the existence of a discontinuity in the share of teachers entering and exiting the school. The outcome variable is calculated as the share of

Figure 2.3: *Effect of Party Turnover on Headmaster Turnover*
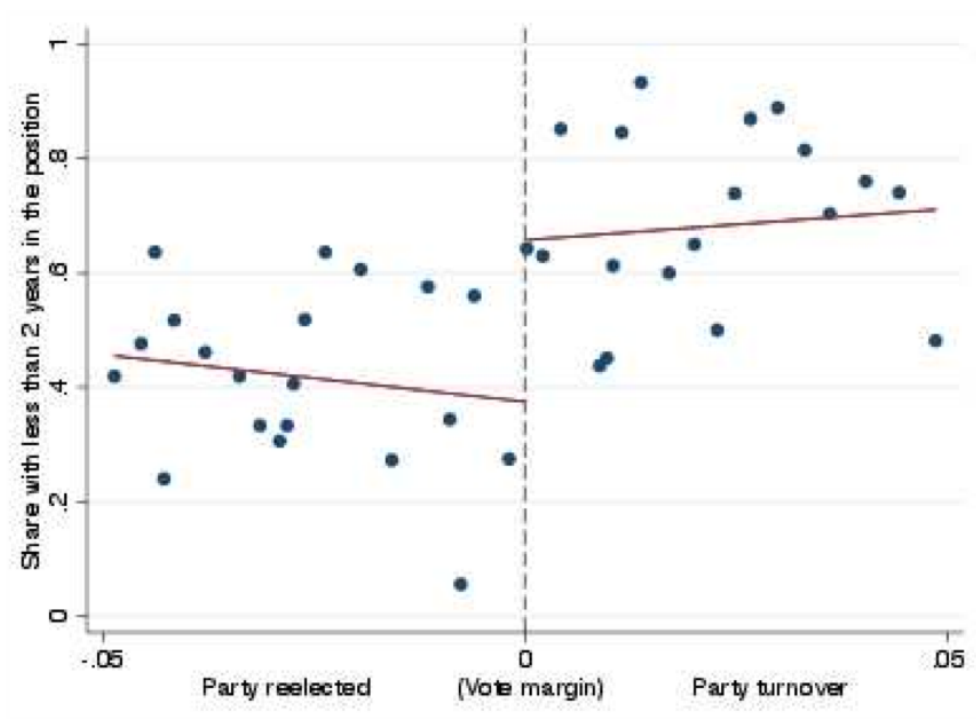
Notes: This figure displays the average share of headmasters in that position for less than two years (in 2009) by vote margin. Only headmasters of primary municipal schools that took Prova Brasil were included. No covariates were included.

Table 2.2: *Effect of Party Turnover on Headmaster Turnover*

Dependent variable: Less than 2 years as headmaster in this school as of 2009						
	5% window		3% window		Cubic, 25% window	
Party turnover	0.2489***	0.2967***	0.2204***	0.2443***	0.2024**	0.2102**
	(0.0813)	(0.0793)	(0.0603)	(0.0558)	(0.0812)	(0.0849)
Observations	2,290	1,872	1,443	1,204	8,849	7,247
Clusters	481	454	280	266	1632	1539
Control group mean	0.418	0.418	0.415	0.415	0.408	0.408
Controls	NO	YES	NO	YES	NO	YES

Notes: Only headmasters of primary municipal schools that took Prova Brasil were included. Controls are: years of schooling at the municipal level, per capita GDP in 2007, share of population that received Bolsa Familia in 2007, unemployment rate in 2007, whether the school is in an urban or rural area, number of teachers in the school in 2007, whether the school counts with water pipes, sewer pipes, office for the principal, computer lab, and bathroom inside the school, proportion of students that have a computer, that live with both parents, that work, that have a car, that observe their mother reading. Errors are clustered at the municipality level.

teachers that are new to the school and the share of teachers that have left the school in 2009 with respect to those in 2007. Table 2.3 shows that schools in municipalities with turnover see a 16 percentage point increase in teachers entering the school and a 9 percentage point increase in teachers leaving the school. These results are robust to the inclusion of controls and to narrowing the bandwidth. Moreover, the overall number of teachers at the municipality level is not significantly different, although the point estimate is positive. This is likely the case because the number of schools restricts the number of teaching positions, and municipalities typically face a shortage in the supply of teachers (at least in the short run).

The increase in turnover, both of teachers and headmasters, could reflect an attempt to improve the quality of education. However, anecdotal evidence points to widespread use of teachers and headmaster positions for electoral purposes. Teachers that support a given party within the community, receive a promotion to headmaster in exchange if the party wins[8].

Moreover, most action comes from political appointments. Column (1) and (2) of Table 2.4 conditions the effect of party turnover on whether the headmaster was politically appointed prior to the election. We find that a large part of the turnover of headmaster is concentrated on schools that were already "politicized". It should

---

[8]For example, the following two articles discuss survey evidence on the prevalence of politically appointed school principals, noting that "the political [appointment of headmasters] tends to discourage students, faculty, and staff, because they do not see the [headmaster] as a person with support" from colleagues and the community: http://www1.folha.uol.com.br/fsp/cotidian/ff1010200702.htm and http://www1.folha.uol.com.br/fsp/cotidian/ff1010200701.htm (accessed on February 18,2015).

Table 2.3: *Effect of Party Turnover on Teacher Turnover*

Dependent Variable:	(1)		(2)		(3)	
5% window	New (share)		Left (share)		Net change	
Party turnover	0.1638**	0.0596	0.0966***	0.0740**	0.0672	-0.0144
	(0.0667)	(0.0563)	(0.0328)	(0.0313)	(0.0580)	(0.0432)
Observations	2,705	2,267	2,705	2,267	2,705	2,267
Clusters	557	530	557	530	557	530
Control group mean	0.636	0.636	0.461	0.461	0.175	0.175
Controls	NO	YES	NO	YES	NO	YES
3% window						
Party turnover	0.1214***	0.0864**	0.0802***	0.0666***	0.0413	0.0198
	(0.0396)	(0.0376)	(0.0212)	(0.0216)	(0.0306)	(0.0262)
Observations	1,702	1,453	1,702	1,453	1,702	1,453
Clusters	325	312	325	312	325	312
Control group mean	*0.592*	*0.592*	*0.458*	*0.458*	*0.134*	*0.134*
Controls	NO	YES	NO	YES	NO	YES
Cubic, 25% window						
Party turnover	0.1060*	0.0736	0.0836**	0.0720**	0.0224	0.0017
	(0.0568)	(0.0597)	(0.0325)	(0.0323)	(0.0449)	(0.0445)
Observations	10,480	8,782	10,480	8,782	10,480	8,782
Clusters	1864	1775	1864	1775	1864	1775
Control group mean	0.636	0.636	0.459	0.459	0.177	0.177
Controls	NO	YES	NO	YES	NO	YES

Notes: The share of new teachers and teachers that left the position were calculated over the total number of teachers in 2007. The outcome variable of column 3 is the sum of all teachers that are new at the municipal level minus the sum of all teachers that left a position at the municipal level, over the total number of teachers in the municipality in 2007. Controls are: years of schooling at the municipal level, per capita GDP in 2007, share of population that received Bolsa Familia in 2007, unemployment rate in 2007, whether the school is in an urban or rural area, number of teachers in the school in 2007, whether the school counts with water pipes, sewer pipes, office for the principal, computer lab, and bathroom inside the school, proportion of students that have a computer, that live with both parents, that work, that have a car, that observe their mother reading. Errors are clustered at the municipality level.

Figure 2.4: *Effect of Party Turnover on Teacher Turnover*
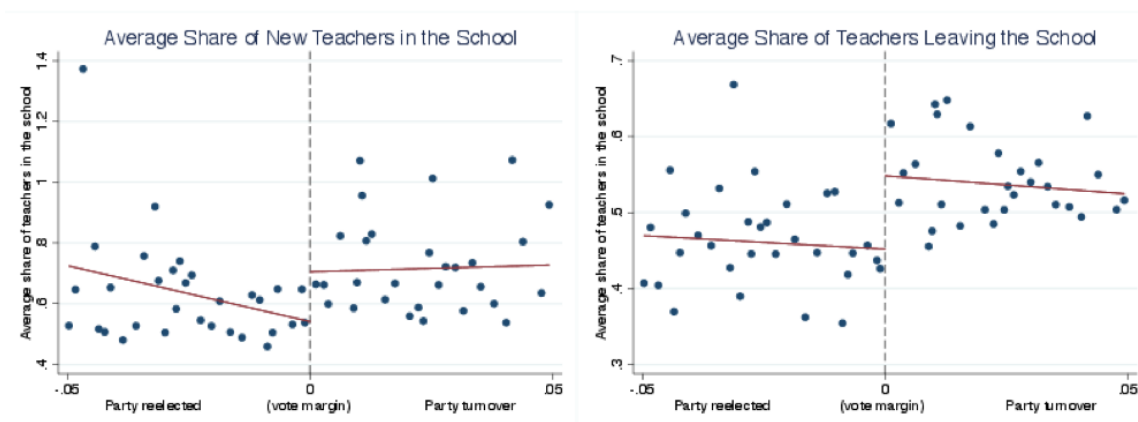
Notes: These figures display the average share of teachers that are new or exited in 2009, by vote margin. The denominator is the number of teachers in 2007. Only primary municipal schools that took Prova Brasil were included. No covariates were included.

be noted that many schools do not report type of appointment in 2007, and so this result is not conclusive but only suggestive. It could still be the case that the new party is trying to replace headmasters with the "wrong" incentives with better headmasters. However, we find that the incoming headmasters are mostly politically appointed as well. The dependent variable for column (3) and (4) is a dummy that takes value 1 if the school has a new headmaster in 2009 and this headmaster is politically appointed, and 0 otherwise. We see that political turnover significantly increases the appointment of these type of headmasters. At the same time, we do not find a similar effect for other types of appointments. Column (5) and (6) reports the results for new headmasters with other types of appointments. The estimates are small and not significant in this case.

The fact that most action happens through political appointments is consistent with the use of school positions for patronage practices. However, it could still just reflect that the incoming mayors are genuinely trying to improve the quality of education but face institutional constraints to execute their decisions. In the next section, we discuss the effect on students outcomes.

2.5 Quantifying the impact of party turnover on education quality

We have established that party turnover affects the appointment of teachers and headmasters. Does this matter for the quality of education? As mentioned previously, there are numerous reasons to focus on education when studying the impact of party turnover on the quality of public services. For example, teachers and headmasters are key in the production function of eduction and contract teachers are becoming more common across the

Table 2.4: *Headmaster Turnover and Political Appointments*

	Dependent variable: Less than 2 years as headmaster in 2009					
	All		And pol. appoint.		And other appoint.	
	(1)	(2)	(3)	(4)	(5)	(6)
Party turnover	0.1366*	0.1565**	0.2272***	0.2589***	0.0243	0.0385
	(0.0827)	(0.0702)	(0.0702)	(0.0730)	(0.0576)	(0.0730)
Pol appoint before election	0.0442	0.0018				
	(0.0502)	(0.0523)				
Pol appoint before election*Party turnover	0.2362**	0.2237***				
	(0.0919)	(0.0812)				
Observations	978	974	2,251	1,848	2,251	1,848
Clusters	253	250	480	450	480	450
Control group mean	0.4	0.4	0.12	0.12	0.29	0.29
Controls	NO	YES	NO	YES	NO	YES

Notes: Only headmasters of primary municipal schools that took Prova Brasil were included. The dependent variable in Column (1) and (2) is an indicator that takes value 1 if the headmaster is new in the school after the election, and 0 otherwise. The dependent variable in column (3) and (4) is an indicator that takes value 1 if the headmaster is new in the school after the election and it was politically appointed, and 0 otherwise. The dependent variable in column (5) and (6) is an indicator that takes value 1 if the headmaster is new in the school after the election and it was not politically appointed, and 0 otherwise. "Political appointment before the election" is a dummy that takes value 1 if the headmaster in 2007 reported that he/she had been politically appointed to the school. Controls are: years of schooling at the municipal level, per capita GDP in 2007, share of population that received Bolsa Familia in 2007, unemployment rate in 2007, whether the school is in an urban or rural area, number of teachers in the school in 2007, whether the school counts with water pipes, sewer pipes, office for the principal, computer lab, and bathroom inside the school, proportion of students that have a computer, that live with both parents, that work, that have a car, that observe their mother reading. Errors are clustered at the municipality level.

developing world (Muralidharan and Sundararaman, 2013) which renders education provision susceptible to political factors. Additionally, test scores are a direct measure of service quality and are unlikely to be differentially manipulated in places with and without political turnover. Moreover, test scores directly relate to welfare at the individual level. In this section, we look at the impact of party turnover on education outcomes, and in the next section we investigate the relationship with the increase in turnover of teachers and headmasters.

2.5.1 Effect on test scores and dropout rates the year after the election

In order to assess the effect of party turnover on the quality of education, we focus on two of the most important measures of student performance: test scores and dropout rates of primary school students. Specifically, we use math and Portuguese test scores for 4th and 8th graders and dropout rate at the school level in 2009, the first year in office for the (sometimes new) party. We combine math and Portuguese into a single score at the student level, and express it in units of standard deviation of the control group. Figure 2.5 shows the effect of party turnover on test scores. When a new party takes office, test scores of both 4th graders and 8th graders decrease. As Table 2.5 repots, we estimate a decrease between 0.095 and 0.133 standard deviations for 4th graders, and between 0.087 and 0.079 for 8th graders. Even though the decrease is statistically not different for 4th and 8th graders, it is significant only for 4th graders. This is likely to be the case because federal and state governments control the majority of schools that offer later grades. This means that our sample of municipal schools that offer 8th grade classes is substantially smaller than our sample of municipal schools that offer 4th grade classes and, hence, so is the number of 8th grade students in our sample.

Figure 2.5: *Effect of Party Turnover on Test Scores*
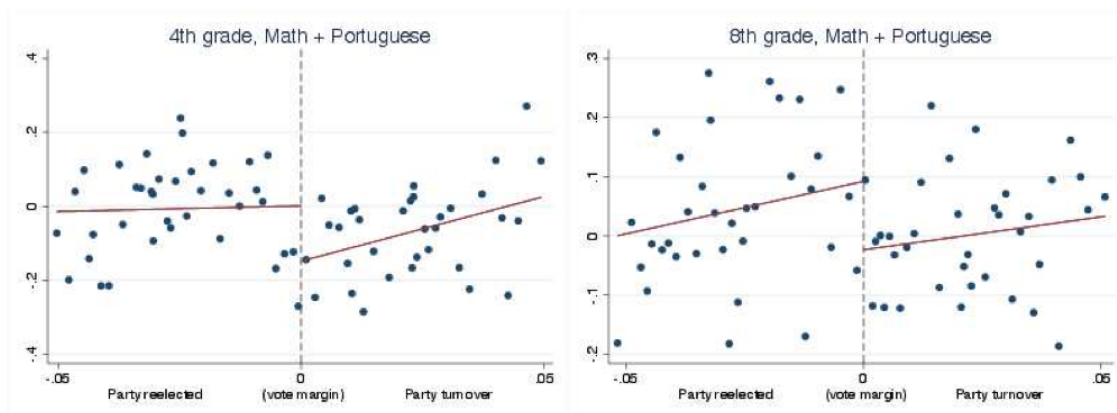
Notes: Scores are expressed in terms of standard deviations of the control group in 2009. The lag of the outcome variable in 2007 is included as a control, but no other covariates are included. Only students in municipal schools are included.

69

Table 2.5: *Effect of Party Turnover on Test Scores*

PANEL A: 4th grade

	Dependent variable: Total score 4th graders in 2009					
	5% window		3% window		Cubic, 25% window	
Party turnover	-0.1338**	-0.0958**	-0.0925***	-0.1016***	-0.0911	-0.0973**
	(0.0581)	(0.0461)	(0.0348)	(0.0272)	(0.0556)	(0.0473)
Observations	107,016	106,769	68,542	68,412	428,198	425,583
Clusters	499	493	291	288	1668	1653
Controls	NO	YES	NO	YES	NO	YES

PANEL B: 8th grade

	Dependent variable: Total score 8th graders in 2009					
	5% window		3% window		Cubic, 25% window	
Party turnover	-0.0876	-0.0792	-0.0450	-0.0443	0.0223	0.0196
	(0.0608)	(0.0649)	(0.0387)	(0.0393)	(0.0599)	(0.0607)
Observations	35,459	35,322	21,983	21,933	148,530	147,949
Clusters	292	287	169	167	977	967
Controls	NO	YES	NO	YES	NO	YES

Notes: Scores are expressed in terms of standard deviations of the control group in 2009. Only municipal schools are included. The lag of the outcome variable in 2007 is included as a control. Controls are: years of schooling at the municipal level, per capita GDP in 2007, share of population that received Bolsa Familia in 2007, unemployment rate in 2007, whether the school is in an urban or rural area, number of teachers in the school in 2007, whether the school counts with water pipes, sewer pipes, office for the principal, computer lab, and bathroom inside the school, proportion of students that have a computer, that live with both parents, that work, that have a car, that observe their mother reading. Errors are clustered at the municipality level.

The difference between municipalities with and without turnover could reflect differences in performance prior to 2009, that could have led to the outcome of the election. However, not only were test scores similar in control and treatment municipalities, as shown in Figure 2.6, but so were the pre-trends in test scores.

Party turnover is not significant when we estimate our main specification for test scores of 4th graders in 2007,[9] controlling for the corresponding scores in 2005. Table 2.6 reports this estimation. Importantly, the point estimates are small and not robust across different specifications.

Party turnover also affects the dropout rates, as shown in Figure 2.7. The year after the election, the overall

---

[9]*Prova Brasil* was taken for the first time in 2005, and only results for 4th graders are available.

Figure 2.6: *Test Scores Before the Election*
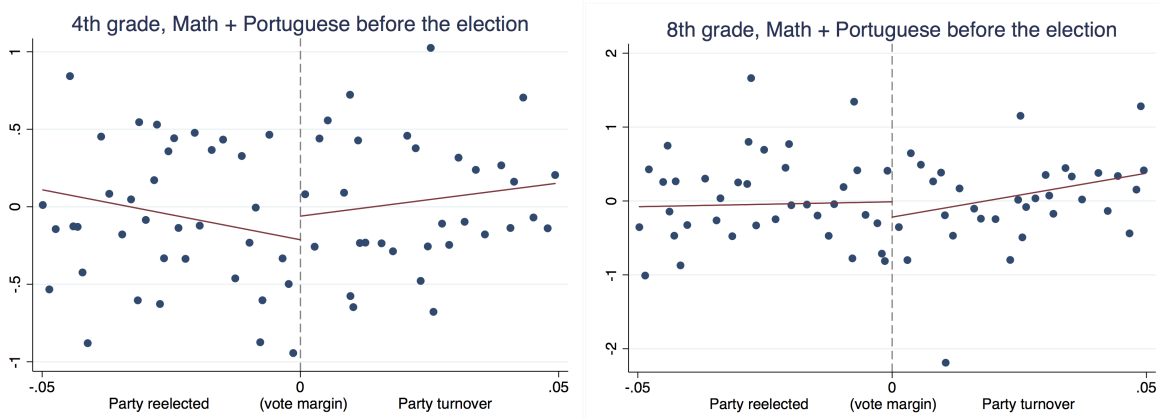
Notes: Scores are expressed in terms of standard deviations of the entire sample. No controls are included. Only students in municipal schools are included.

Table 2.6: *No Pre-Trend in Test Scores*

| | Dependent variable: Total score 4th graders in 2007 | | |
	5% window	3% window	Cubic, 25% window
Party turnover	0.0225	-0.0502	-0.0144
	(0.1125)	(0.0757)	(0.1078)
Total test score in 2005	0.7555***	0.7581***	0.7442***
	(0.0245)	(0.0275)	(0.0228)
Observations	1,327	868	5,378
Clusters	440	253	1474
Controls	NO	NO	NO

Notes: This table shows our main specifications for scores of 4th graders in 2007, controlling for scores of 4th graders in 2005. Only municipal schools are included. 8th graders are not included because we do not have test scores for them in 2005. Errors are clustered at the municipality level.

dropout rate increases by 1 percentage point in primary schools managed by the municipality (Table 2.7) in municipalities that have a new party in power. Since the dropout rate is 2.7% for control schools, this means that a party change leads to a 37% increase in students' dropout. This finding is consistent with the fact that performance in school is a determinant of retention. Moreover, preexisting differences do not seem to explain the result on dropout rates either: before the election (i.e. 2007), schools were similar in municipalities with and without party turnover. The absence of discontinuities can be seen in Figure 2.8.

Figure 2.7: *Effect of Party Turnover on Dropout Rate*
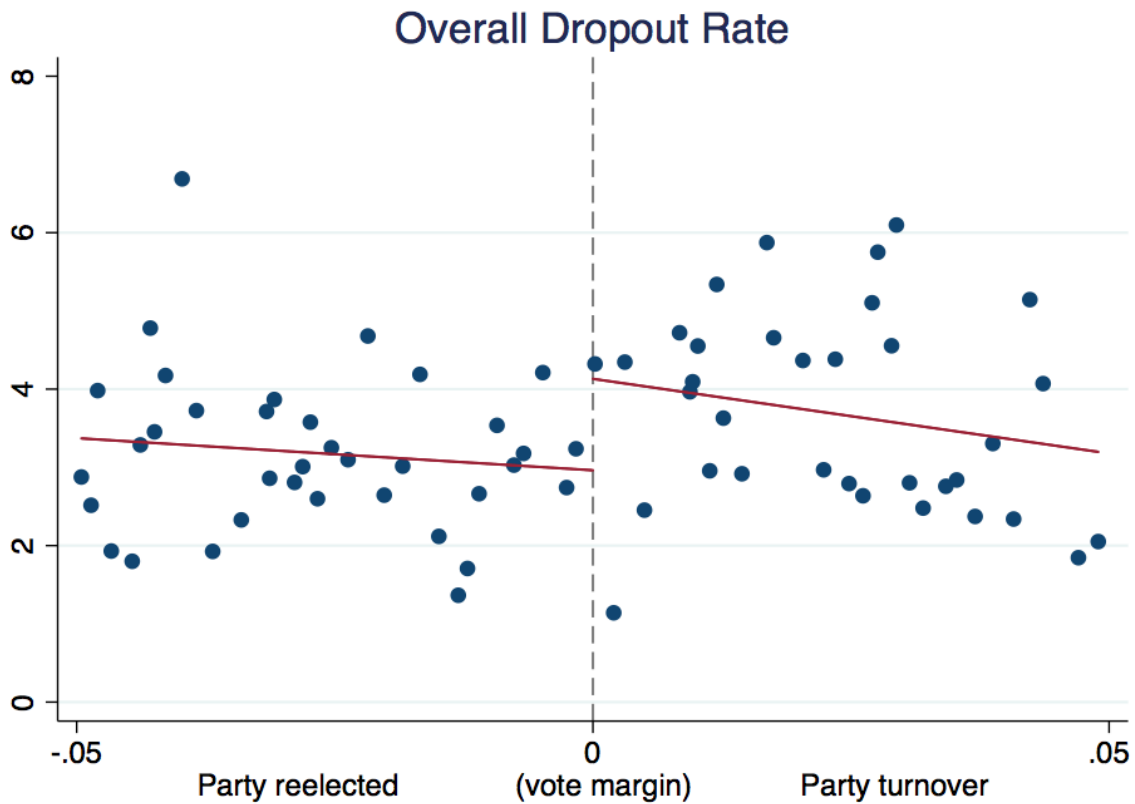
Notes: The outcome variable is the percentage of students in the school that dropped out during 2009. The lag of the outcome variable in 2007 is included as a control, but no other covariates are included. Only municipal schools are included.

2.5.2 Effect on test scores and dropout rates three years after the election

Do the negative effects persist over time? This is an important question not only from a welfare perspective, but also in order to interpret the results found so far. It is possible that the negative effect during the year after the

Table 2.7: *Effect of Party Turnover on Dropout Rate*

	5% window		3% window		Cubic, 25% window	
	Dependent variable: Overall dropout rate in 2009					
Party turnover	0.9971**	0.7991**	0.6847***	0.5638***	0.4479	0.3205
	(0.4514)	(0.3983)	(0.2455)	(0.1847)	(0.4477)	(0.4272)
Observations	2,447	2,447	1,550	1,550	9,278	9,278
Clusters	539	539	316	316	1794	1794
Control group mean	2.693	2.693	2.572	2.572	2.722	2.722
Controls	NO	YES	NO	YES	NO	YES

Notes: The outcome variable is the percentage of students in the school that dropped out during 2009. The lag of the outcome variable in 2007 is included as a control. Controls are: years of schooling at the municipal level, per capita GDP in 2007, share of population that received Bolsa Familia in 2007, unemployment rate in 2007, whether the school is in an urban or rural area, number of teachers in the school in 2007, whether the school counts with water pipes, sewer pipes, office for the principal, computer lab, and bathroom inside the school, proportion of students that have a computer, that live with both parents, that work, that have a car, that observe their mother reading. Only municipal schools are included. Errors are clustered at the municipality level.

Figure 2.8: *Dropout Rate Before the Election*
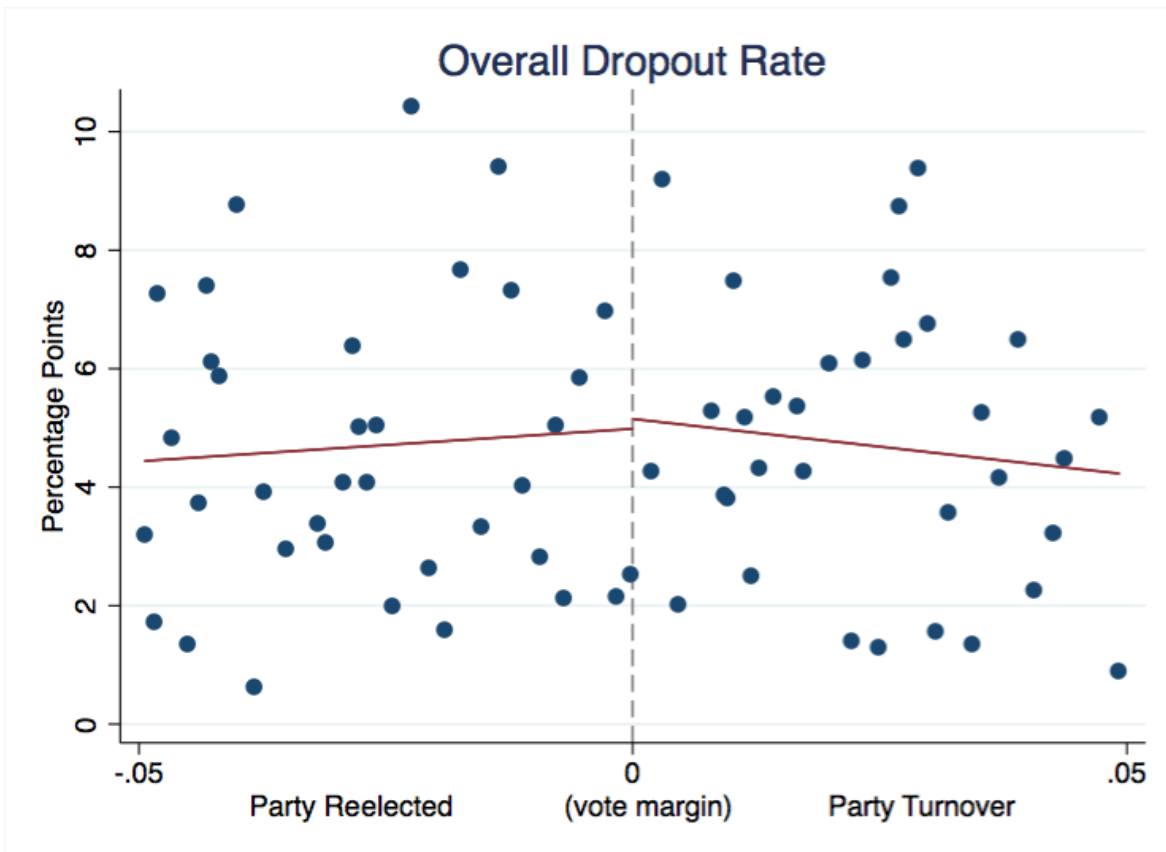
Notes: The outcome variable is the percentage of students in the school that dropped out during 2007. Only municipal schools are included. No controls were included.

election is compensated in the long run. This could be the case if the incoming political party is sincerely trying to improve the quality of education: it makes some changes that are initially disruptive but that improve the quality of schools and test scores in the longer run. We use test scores and dropout rates in 2011, three years after the election, to check this hypothesis.

Table 2.8 reports the effect on test scores of the cohorts that are 4th and 8th graders in 2011 (three years after the election), who were 2nd and 6th graders in 2009 (the year after the election). The estimates indicate that three years after the election, test scores are still lower in municipalities with party turnover, particularly so for the older cohort. It is important to emphasize that we do not have a panel: we cannot track the same students over time to see what happens to their outcomes three years after they have experienced political turnover.

Table 2.8: *Effect of Party Turnover on Test Scores 3 Years After Election*

PANEL A: 4th grade

	Dependent variable: Total score 4th graders in 2011					
	5% window		3% window		Cubic, 25% window	
Party turnover	-0.1071	-0.0328	-0.0684	-0.0427	-0.0808	-0.0305
	(0.0758)	(0.0567)	(0.0470)	(0.0342)	(0.0775)	(0.0610)
Observations	80,443	79,913	53,110	52,995	294,723	292,585
Clusters	465	463	268	267	1559	1545
Controls	NO	YES	NO	YES	NO	YES

PANEL B: 8th grade

	Dependent variable: Total score 8th graders in 2011					
	5% window		3% window		Cubic, 25% window	
Party turnover	-0.1163	-0.1669***	-0.0696	-0.1050**	-0.0501	-0.0205
	(0.0837)	(0.0641)	(0.0513)	(0.0410)	(0.0744)	(0.0603)
Observations	32,195	32,157	20,073	20,073	118,136	118,098
Clusters	285	284	163	163	946	945
Controls	NO	YES	NO	YES	NO	YES

Notes: Scores are expressed in terms of standard deviations of the control group in 2011. Only municipal schools are included. The lag of the outcome variable in 2007 is included as a control. Controls are: years of schooling at the municipal level, per capita GDP in 2007, share of population that received Bolsa Familia in 2007, unemployment rate in 2007, whether the school is in an urban or rural area, number of teachers in the school in 2007, whether the school counts with water pipes, sewer pipes, office for the principal, computer lab, and bathroom inside the school, proportion of students that have a computer, that live with both parents, that work, that have a car, that observe their mother reading. Errors are clustered at the municipal level.

Table 2.8 points out that party turnover has effects on students one year after the shock and on (a different group of) students three years after the shock. Similarly, dropout rates in municipalities with a new party in power are still higher in 2011, as shown in Table 2.9.

Table 2.9: *Effect of Party Turnover on Dropout Rate 3 Years After Election*

| | Dependent variable: Overall dropout rate in 2011 | | | | | |
	5% window		3% window		Cubic, 25% window	
Party turnover	0.9074**	0.8460**	0.5817**	0.6029***	0.7615*	0.7181*
	(0.4015)	(0.3931)	(0.2246)	(0.1981)	(0.3974)	(0.3940)
Observations	2,427	2,427	1,539	1,539	9,198	9,198
Clusters	538	538	316	316	1789	1789
Control group mean	2.195	2.195	1.999	1.999	2.193	2.193
Controls	NO	YES	NO	YES	NO	YES

Notes: The outcome variable is the percentage of students in the school that dropped out during 2011. The lag of the outcome variable in 2007 is included as a control. Controls are: years of schooling at the municipal level, per capita GDP in 2007, share of population that received Bolsa Familia in 2007, unemployment rate in 2007, whether the school is in an urban or rural area, number of teachers in the school in 2007, whether the school counts with water pipes, sewer pipes, office for the principal, computer lab, and bathroom inside the school, proportion of students that have a computer, that live with both parents, that work, that have a car, that observe their mother reading. Only municipal schools are included. Errors are clustered at the municipality level.

This does not mean that education in Brazil is spiraling down; in fact, performance of students in Brazil as a whole has improved during the 2000's according to IDEB (dice de Desenvolvimento da Educa Bca, measured by INEP). It may well be the case that overall education quality is improving in Brazil over time; however, within the subset of municipalities with close elections, party turnover decreases students' performance, when compared to municipalities without party turnover.

Students in Brazil spend at least 8 years in primary school.[10] This means that during their time in school, they will be exposed to a post-election year from 1 to 3 times. Our results show that most students in the municipal system are likely to experience a decrease in the quality of their education at some point. These results work against the hypothesis that the new party is generating changes in the education system with the objective of improving the quality of education, unless one is willing to believe that these changes systematically fail to generate benefits during most of the mandate. Alternatively, the improvements could take more than

---

[10]The length of primary school varies between 8 and 9 years.

three years to be realized. Given that elections are held every four years, one wonders whether those benefits realize at all. We believe that a more plausible explanation is that the negative effect on students' performance is a by-product of the use of school positions for patronage.

2.6 Mechanisms

So far we have shown that party turnover leads to higher turnover of teachers, higher turnover of headmasters, and lower performance of students. It could be argued that party turnover is affecting the quality of education for reasons other than the increase in turnover of school employees. In this section we test for other mechanisms that could account for the decrease in students' performance. We do not find evidence supporting that these other mechanisms are driving our results; instead, we find evidence suggesting that political turnover creates malfunctions within the school that are related to teachers and headmasters.

2.6.1 Teachers' characteristics do not change

Teacher turnover may affect test scores and dropout rates not through the disruption that this change entails, but because the new teachers are worse teachers. To assess the likelihood of this explanation, we test for differences in teachers' characteristics that are related to their ability to teach effectively. Table 2.10 shows the effect of turnover on teachers' characteristics such as age, education, and experience. The final pool of teachers is similar, at least in terms of observables: none of the characteristics are statistically different. Therefore, this explanation for the decrease in education quality is unlikely.

The increase in teacher turnover could have been the result of an attempt to improve the quality of education in the longer run. For example, the new mayor could have promoted the replacement of some teachers for more qualified ones even though, at the end of the day, it did not translate into better educational outcomes. Since research shows that identifying good teachers at the hiring stage is not straightforward (Staiger and Rockoff 2010; Rockoff et al. 2011), characteristics such as education level or experience are often used by officials as proxies for quality. Therefore, an attempt to improve the quality of municipal education would be reflected in a change of observable characteristics. The fact that teachers are similar in terms of observables suggests that other reasons drive the increase in teachers' turnover.

2.6.2 Student composition does not change

It is possible that the deterioration of education quality is purely driven by new political parties systematically broadening education access to students from disadvantageous backgrounds. If that is indeed the case, the decrease in test score should not be interpreted as a decrease in the quality of education but as reflecting an underlying change in the composition of students. We test in Table 2.11 whether the socioeconomic composition

Table 2.10: *Effect of Party Turnover on Teachers' Characteristics*

| | | | Characteristics of Final Pool of Teachers | | | |
| | Age | Educ | Female | Experience | 1 school | Tenured |
5% window	(1)	(2)	(3)	(4)	(5)	(6)
Party turnover	-0.0278	-0.0300	-0.0189*	0.0197	0.0499	0.0055
	(0.3310)	(0.0536)	(0.0100)	(0.0296)	(0.0384)	(0.0589)
Observations	2,705	2,705	2,705	1,743	1,738	1,733
Clusters	557	557	557	489	487	488
Control group mean	37.29	13.23	0.854	0.170	0.585	0.663
Controls	NO	NO	NO	NO	NO	NO
Party turnover	-0.0418	-0.0176	-0.0188**	0.0065	0.0537	0.0057
	(0.3045)	(0.0494)	(0.0094)	(0.0297)	(0.0362)	(0.0526)
Observations	2,296	2,296	2,296	1,735	1,730	1,725
Clusters	532	532	532	482	480	481
Control group mean	37.57	13.28	0.863	0.170	0.585	0.662
Controls	YES	YES	YES	YES	YES	YES

Notes: This table reports the coefficient of "party turnover" from our main specifications. Teachers' characteristics are measured in 2009. Columns 1-3 compare the characteristics of teachers new in 2009 (with respect to 2007). Columns 4-6 compare the characteristics of teachers that left in 2009 (with respect to 2007). Columns 7-12 compare the characteristics of the final pool of teachers in 2009. The lag of the outcome variable in 2007 is included as a control for column 7-12. Controls are: years of schooling at the municipal level, per capita GDP in 2007, share of population that received Bolsa Familia in 2007, unemployment rate in 2007, whether the school is in an urban or rural area, number of teachers in the school in 2007, whether the school counts with water pipes, sewer pipes, office for the principal, computer lab, and bathroom inside the school, proportion of students that have a computer, that live with both parents, that work, that have a car, that observe their mother reading. Only teachers in municipal schools are included. Errors are clustered at the municipality level.

of students taking the exam is different in 2009.

Students are slightly more likely to be male, which would work against our findings since female students are found to do better in general. All other coefficients are statistically insignificant, suggesting that the decrease in test scores is not driven by a policy that broadens the access to education or any other force that would change the composition of students. Moreover, this result also works against the hypothesis that the increase in dropout rates leads to a different pool of students, which in turn explains the decrease in test scores.

2.6.3 Resources dedicated to education do not change

When the new party takes office, it may go through a learning process regarding the management of the municipality, the acquisition of funds from the federal and state governments, or the generation of revenue from its tax base. These adjustments could impact the amount of resources dedicated to education, and thus generate the observed decrease in test scores and the increase in dropout rates. However, we see in Table 2.12 that the share of resources allocated to education is similar with turnover and reelection. Moreover, we do not find differences in the transfers received from the national or state government, which would signal differences in the ability of the new party to raise funds.

2.6.4 Malfunctions within the school

The turnover of teachers and headmaster could be generating management problems within the school. Table 2.13 reports the effect of turnover on the characteristics of headmasters. Indeed, consistent with this management story, we see that headmasters are less likely to have experience in the position of headmaster (in any school), but are otherwise similar.

According to anecdotal accounts, headmasters are often teachers that have been in the education system for a long time, and are now being promoted or elected to headmaster positions. Furthermore, we observe more negative reports from teachers in regards to their respective headmasters. Table 2.14 shows the effect of party turnover on the response of teachers to questions about the headmaster, such as "From 1 to 5 (where 1 means to strongly disagree, and 5 means to strongly agree), how much do you trust the headmaster?" We find that in municipalities with turnover, the perception of teachers about the headmaster are generally worse. Recall that teachers also have higher turnover in treated municipalities, thus this change in perception could be a combination of the opinion of teachers about the new headmaster and the opinion of new teachers about the headmaster of the school where they were transferred. Either way, this result suggests that the turnover of teachers and headmasters is hurting the relationship between them and, hence, their performance and effectiveness in the school.

79

Table 2.11: *Effect of Party Turnover on Students' Characteristics*

	Students	Male	Black	Age	Car	Computer	Bath	Mother reads	Works
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
5% window									
Party turnover	-17.3385	0.0106*	0.0411	0.0661	-0.0351	-0.0105	-0.0077	0.0150	-0.0084
	(14.0062)	(0.0056)	(0.0431)	(0.2560)	(0.0611)	(0.0525)	(0.0238)	(0.0192)	(0.0176)
Observations	163,621	171,568	167,730	173,436	172,557	173,747	170,872	167,764	164,437
Clusters	416	557	557	557	557	557	557	557	557
Control group mean	166.8	0.501	0.642	12.16	0.360	0.313	0.203	0.850	0.178
Controls	NO	NO	NO	NO	NO	NO	NO	NO	NO
Party turnover	-12.0586	0.0086	0.0061	-0.0343	-0.0001	-0.0010	-0.0098	0.0121**	0.0018
	(11.1840)	(0.0062)	(0.0245)	(0.1937)	(0.0079)	(0.0083)	(0.0115)	(0.0057)	(0.0072)
Observations	152,844	157,985	154,619	159,784	159,304	160,162	157,459	155,347	152,656
Clusters	412	548	548	548	548	548	548	548	548
Control group mean	167.6	0.503	0.641	12.13	0.365	0.318	0.205	0.853	0.176
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: This table reports the coefficient of "party turnover" from our main specifications. Students' characteristics are measured in 2009. The lag of the outcome variable in 2007 is included as a control. Controls are: years of schooling at the municipal level, per capita GDP in 2007, share of population that received Bolsa Familia in 2007, unemployment rate in 2007, whether the school is in an urban or rural area, number of teachers in the school in 2007, whether the school counts with water pipes, sewer pipes, office for the principal, computer lab, and bathroom inside the school, proportion of students that have a computer, that live with both parents, that work, that have a car, that observe their mother reading. Only students in municipal schools are included. Errors are clustered at the municipality level.

Table 2.12: *Effect of Party Turnover on Municipal Resources*

5% window	(1)		(2)		(3)	
	Share of budget spent on education		**Federal transfers capita**		**State transfers per capita**	
Party turnover	0.0206	0.0103	1.4220	1.2247	-2.1456	-2.0975
	(0.0237)	(0.0216)	(3.2847)	(3.3300)	(1.8920)	(2.5497)
Observations	6,415	1,311	6,247	1,281	6,247	1,281
Clusters	517	364	510	360	510	360
Control group mean	0.572	0.572	2.348	3.485	2.091	2.431
Controls	NO	YES	NO	YES	NO	YES

Notes: This table reports the coefficient of "party turnover" from our main specifications. Outcomes are measured in 2009. The lag of the outcome variable in 2007 is included as a control. Controls are: years of schooling at the municipal level, per capita GDP in 2007, share of population that received Bolsa Familia in 2007, unemployment rate in 2007, whether the school is in an urban or rural area, number of teachers in the school in 2007, whether the school counts with water pipes, sewer pipes, office for the principal, computer lab, and bathroom inside the school, proportion of students that have a computer, that live with both parents, that work, that have a car, that observe their mother reading. Errors are robust.

Table 2.13: *Effect of Party Turnover on Headmasters' Characteristics*

5% window	(1) Younger than 40		(2) Years of Schooling		(3) Experience in education		(4) Experience as headmaster	
Party turnover	0.0392	0.0315	-0.0188	-0.0114	-0.0325	-0.0894	-2.0059***	-2.0697***
	(0.0387)	(0.0400)	(0.0234)	(0.0243)	(0.4010)	(0.3642)	(0.5610)	(0.5480)
Observations	1,554	1,543	1,554	1,543	1,541	1,530	1,540	1,530
Clusters	439	431	439	431	439	431	436	429
Control group mean	0.106	0.106	0.931	0.930	14.60	14.60	5.920	5.912
Controls	NO	YES	NO	YES	NO	YES	NO	YES

Notes: This table reports the coefficient of "party turnover" from our main specifications. Headmasters' characteristics are measured in 2009. The lag of the outcome variable in 2007 is included as a control. Controls are: years of schooling at the municipal level, per capita GDP in 2007, share of population that received Bolsa Familia in 2007, unemployment rate in 2007, whether the school is in an urban or rural area, number of teachers in the school in 2007, whether the school counts with water pipes, sewer pipes, office for the principal, computer lab, and bathroom inside the school, proportion of students that have a computer, that live with both parents, that work, that have a car, that observe their mother reading. Only headmasters in municipal schools are included. Errors are clustered at the municipality level.

Table 2.14: *Effect of Party Turnover on Perception of Teachers about Headmasters*

	(1) Motivates me	(2) I trust I trust	(3) Commits teachers	(4) Promotes innovation	(5) Attention to students	(6) Attention to adm norms	(7) Attention to maintanence	(8) I feel respected
				Reports of teachers regarding the headmaster				
5% window								
Party turnover	-0.0823	-0.1213*	-0.1128	-0.1524*	-0.0368	-0.0221	-0.0513	-0.0455
	(0.0736)	(0.0736)	(0.0722)	(0.0787)	(0.0841)	(0.0737)	(0.0742)	(0.0682)
Observations	1,743	1,730	1,738	1,738	1,646	1,635	1,652	1,647
Clusters	490	481	488	488	477	476	480	475
Control group mean	4.029	4.182	4.102	4.104	4.127	4.263	4.289	4.385
Controls	NO	NO	NO	NO	NO	NO	NO	NO
3% window								
Party turnover	-0.0758*	-0.0997**	-0.1181***	-0.1204***	-0.0558	0.0202	-0.0312	-0.0238
	(0.0451)	(0.0460)	(0.0424)	(0.0454)	(0.0507)	(0.0465)	(0.0434)	(0.0419)
Observations	1,118	1,115	1,115	1,115	1,049	1,046	1,052	1,054
Clusters	284	283	283	283	277	274	279	277
Control group mean	3.999	4.155	4.075	4.090	4.095	4.236	4.257	4.357
Controls	NO	NO	NO	NO	NO	NO	NO	NO

Notes: This table reports the assessment of teachers in 2009 regarding the headmaster. Teachers were asked to indicate the level of agreement with each of the statements indicated in the table, from 1 to 5. 1 corresponds to a high degree of disagreement, and 5 to a high degree of agreement. Only reports for municipal schools are included. Errors are clustered at the municipality level.

2.7   Robustness checks

We have already shown that the results are robust to controlling for characteristics of the municipalities, schools, and students. We have also shown that results are robust to the use of a 3% window, or a cubic specification over the 25% window. It could still be argued that the identity of the winning parties accounts for the effects observed. The 2008 municipal elections were particularly good for the *Partido dos Trabalhadores* (PT, left wing) and the *Partido do Movimento Democrco Brasileiro* (PMDB, centrist). The PT saw a 36.1% increase in the number of municipalities under their control (from 410 to 558), and the increase was 13.8% for PMDB (from 1060 to 1207). On the flip side, the *Democractas* (DEM, right wing) and *Partido da Social Democracia Brasileira* (PSDB, centrist) lost 36.9% (from 794 to 501) and 9.4% (from 870 to 788) of the municipalities, respectively. More generally, Brazilian municipalities experienced a shift to the left. In order to check whether we are capturing effects of the identity of the parties involved or their ideologies, we re-estimate our main specification for all our outcomes with different controls for party identity and ideology. Table 2.15 shows the coefficient of party turnover on our main outcome variables, after including fixed effects for party identity of the winner or the incumbent, ideology (left, center, right) of the winner or the incumbent party, a dummy for whether the incumbent or winning party runs at the national level, and a measure of the ideological distance between the party in power before and the party in power after the election. In all cases our results remain significant, and their magnitudes are statistically similar to the ones reported before.[11]Therefore, our turnover variable does not seem to be capturing ideology or party effects.

Table 2.15: *Robustness of the Results to Party Identity and Ideology*

	Fixed Effects on:						
5% window							
Coef.	Party identity		Ideology		Runs at national level		Ideological change
Party Turn.:	Winner	Incumbent	Winner	Incumbent	Winner	Incumbent	
Head turnover	0.2135***	0.2388***	0.2460***	0.2473***	0.2482***	0.2547***	0.2536***
	(0.0800)	(0.0810)	(0.0829)	(0.0820)	(0.0817)	(0.0792)	(0.0805)
Obs.	2,290	2,290	2,290	2,290	2,290	2,290	2,290
Clusters	481	481	481	481	481	481	481
Teacher exit	0.0920***	0.0975***	0.0991***	0.0954***	0.0896***	0.0993***	0.0995***
	(0.0336)	(0.0300)	(0.0344)	(0.0319)	(0.0321)	(0.0321)	(0.0328)
Obs.	2,705	2,705	2,705	2,705	2,705	2,705	2,705
Clusters	557	557	557	557	557	557	557
Teacher hired	0.1273**	0.1480**	0.1561**	0.1637**	0.1536**	0.1688**	0.1495**
	(0.0611)	(0.0664)	(0.0732)	(0.0664)	(0.0617)	(0.0686)	(0.0714)
Obs.	2,705	2,705	2,705	2,705	2,705	2,705	2,705
Clusters	557	557	557	557	557	557	557

[11]Including municipality, schools, and students controls do not change these results.

Table 2.15: *Robustness of the Results to Party Identity and Ideology (continued)*

5% window Coef. Party Turn.:	Party identity		Ideology		Runs at national level		Ideological change
	Winner	**Incumbent**	**Winner**	**Incumbent**	**Winner**	**Incumbent**	
Net change	0.0353	0.0505	0.0570	0.0683	0.0640	0.0696	0.0501
	(0.0536)	(0.0600)	(0.0640)	(0.0582)	(0.0533)	(0.0610)	(0.0618)
Obs.	2,705	2,705	2,705	2,705	2,705	2,705	2,705
Clusters	557	557	557	557	557	557	557
Scores 4th gr	-0.1394***	-0.1128*	-0.1512**	-0.1390**	-0.1364**	-0.1338**	-0.1327**
	(0.0515)	(0.0574)	(0.0606)	(0.0573)	(0.0589)	(0.0579)	(0.0582)
Obs.	107,016	107,016	107,016	107,016	107,016	107,016	107,016
Clusters	499	499	499	499	499	499	499
Scores 8th gr	-0.1236**	-0.0797	-0.1164*	-0.0893	-0.0899	-0.0875	-0.0874
	(0.0559)	(0.0550)	(0.0614)	(0.0591)	(0.0634)	(0.0608)	(0.0613)
Obs.	35,459	35,459	35,459	35,459	35,459	35,459	35,459
Clusters	292	292	292	292	292	292	292
Dropout rate	0.9553**	0.7382*	0.9157**	0.8573*	0.8549*	0.8916**	0.8657**
	(0.4072)	(0.4205)	(0.4113)	(0.4422)	(0.4355)	(0.4413)	(0.4207)
Obs.	2,452	2,452	2,452	2,452	2,452	2,452	2,452
Clusters	536	536	536	536	536	536	536

Notes: This table reports the coefficient of party turnover on our main local linear specification after including different sets of controls for party identity and ideology. Each column has a different set of controls: fixed effects for party identity, party ideology (left, center, right), and whether the party runs at the national level, for incumbent or winning party. The last column controls for ideological distance between winning party and incumbent party. This distance can take values between -2 and 2, where -2 corresponds to the change from right to left and 2 corresponds to the change from left to right; milder changes take intermediate values (e.g. from center to left takes value -1). The lag of the corresponding outcome variable in 2007 is included as a control. Errors are clustered at the municipality level.

2.8 Effect of party turnover on all municipal employees

In this section, we broaden the scope and look for the effect of party turnover on appointments at the municipal government as a whole. If new parties use government positions to reward supporters, this is likely to happen in other sectors as well. Table 2.16 shows the effect of party turnover on the share of employees that entered and exited the bureaucracy in 2009 (defined with respect to the total number of employees in 2007, since 2008 is

an election year).[12]

Table 2.16: *Effect of Party Turnover on Bureaucrats Turnover*

	Total from Pooling January - December 2009					
5% window	(1)		(2)		(3)	
	Bureaucrats new in the position (share)		**Bureaucrats that left the position (share)**		**Net change**	
Party turnover	0.1164***	0.1146**	-0.0085	-0.0044	0.1292***	0.1225***
	(0.0450)	(0.0453)	(0.0308)	(0.0308)	(0.0451)	(0.0455)
Observations	587	587	587	587	587	587
Control group mean	0.416	0.416	0.315	0.315	0.0519	0.0519
Controls	NO	YES	NO	YES	NO	YES
3% window						
Party turnover	0.0879***	0.0844***	-0.0172	-0.0162	0.0968***	0.0915***
	(0.0283)	(0.0291)	(0.0188)	(0.0198)	(0.0279)	(0.0287)
Observations	344	344	344	344	344	344
Control group mean	0.390	0.390	0.300	0.300	0.0496	0.0496
Controls	NO	YES	NO	YES	NO	YES
Cubic, 25% window						
Party turnover	0.0882**	0.0857**	-0.0173	-0.0205	0.1170***	0.1144***
	(0.0426)	(0.0423)	(0.0295)	(0.0294)	(0.0420)	(0.0420)
Observations	1954	1952	1954	1,975	1954	1,976
Control group mean	0.398	0.398	0.322	0.322	0.0386	0.0386
Controls	NO	YES	NO	YES	NO	YES

Notes: The outcome in column 1 is the share of bureaucrats that start working in a new position during 2009, where the denominator is the total number of bureaucrats in 2007. The outcome in column 2 is the same, but for bureaucrats that left a position during 2009. The outcome of column 3 is just the share that entered minus the share that left a position in the municipality. The lag of the outcome variable (in 2007) is included as a control. Controls are: years of schooling at the municipal level, per capita GDP in 2007, share of population that received Bolsa Familia in 2007, unemployment rate in 2007. Errors are robust.

Indeed, hiring is larger in municipalities where a new party took office: the share of new employees

[12]Using the log of share of employees who enter and exit the bureaucracy yields similar results.

is 11 percentage points higher. There is no corresponding increase in the number of employees leaving the government, leading to a net increase in the size of the bureaucracy. For the average municipality, this is an increase of approximately 60 employees.

One could argue that bureaucrats who leave may do so before the new party takes office. We estimate (not reported) the effect of turnover on accumulated hiring and exit since November 2008 (the month after the election), and results are not different. We also do not find differences in the results if we estimate the effect for 2009 and 2010, which means that the number of bureaucrats does not bounce back for at least two years. Figure 2.9 shows the discontinuity in hiring that takes place when there is party turnover, corroborating the causal interpretation of our results. Moreover, the increase in hiring happens right after the new party takes office, which indicates that it is indeed driven by the occurrence of party turnover.

**Figure 2.9:** *Effect of Party Turnover on Bureaucrats Hired*
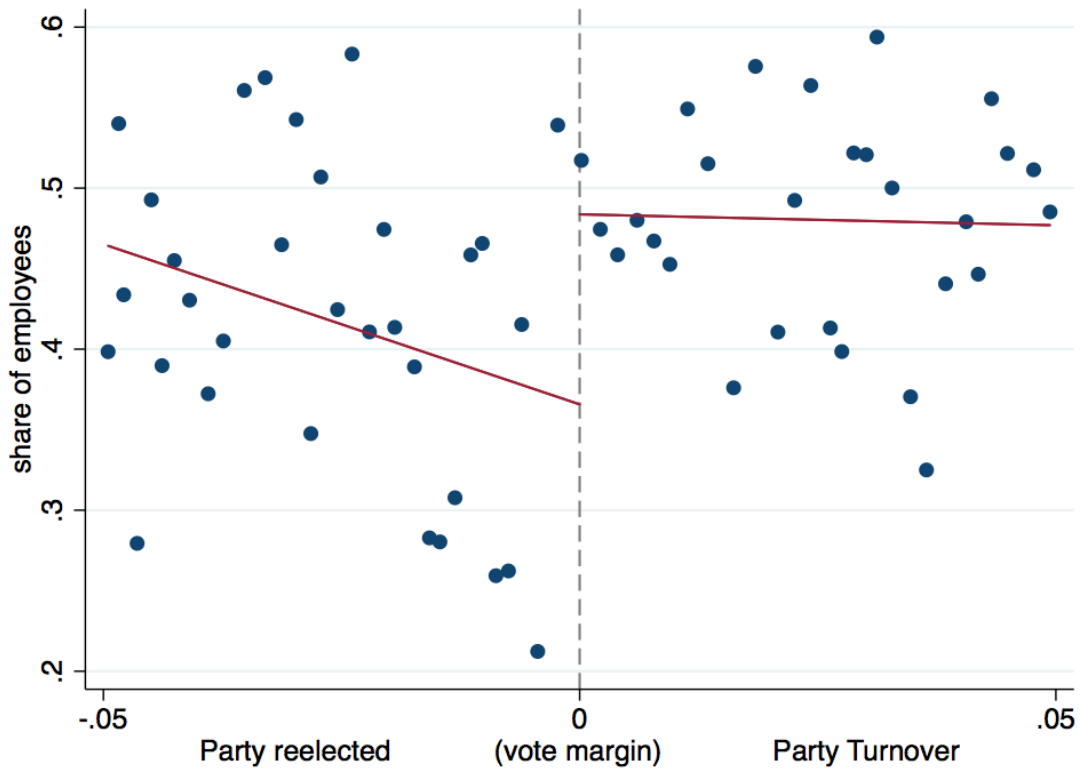
Notes: The share of employees hired was calculated over the total number of municipal employees in 2007. The share of employees hired in 2007 was used as a control.

In Figure 2.10 we separately estimate our main specification month by month, and plot the estimated coefficients on party turnover. The estimates from before the election took place serve as a placebo, and show

that we are not capturing pre-trends or seasonal effects. The estimated increase of the bureaucracy is consistent with calculations made by IPEA (Instituto de Pesquisa Econmica Aplicada), which report a 35% growth of municipal employment between 2003 and 2010.

Figure 2.10: *Bureaucrats Hired Over Time*
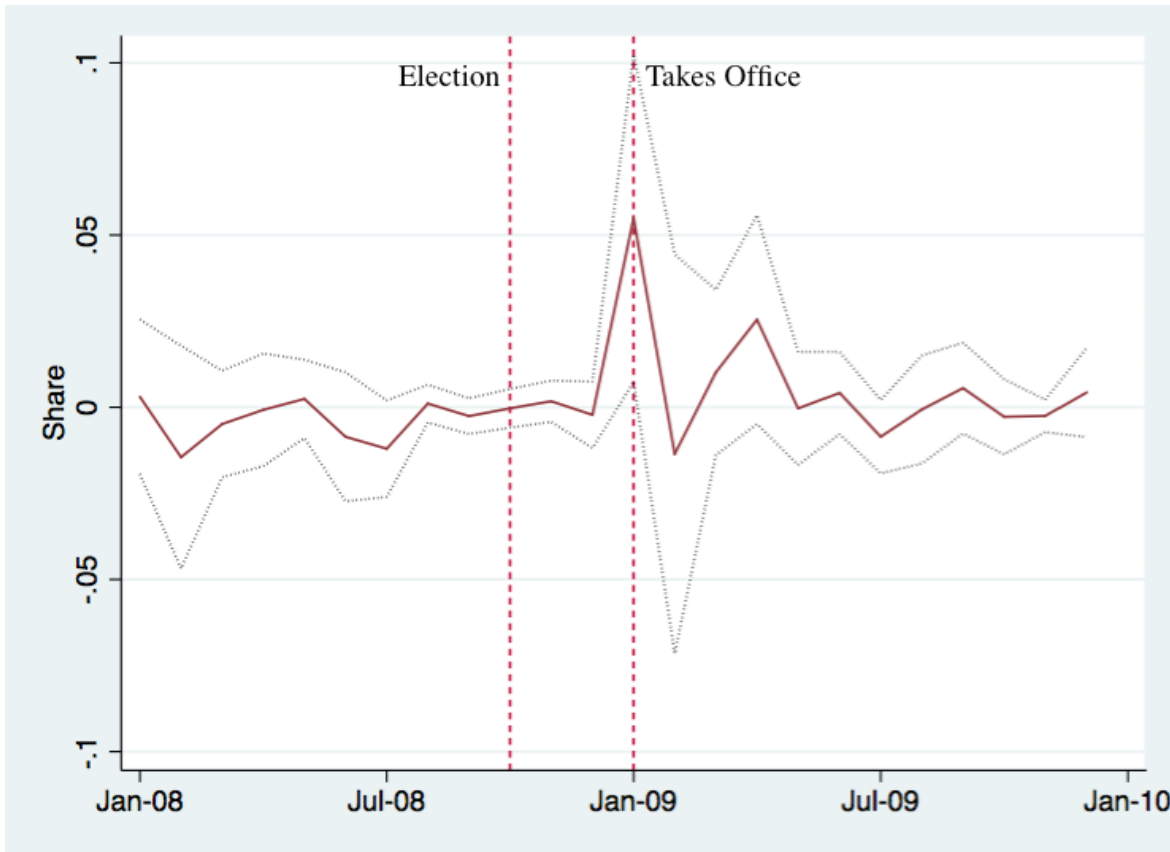
Notes: In order to construct this figure, we estimated our main specification separately for the share of bureaucrats hired month by month. The figure reports the corresponding estimated coefficients and 95% confidence intervals. The lag of the outcome variable (in 2007) is included as a control. Errors are robust.

In sum, we find that party turnover not only affects teachers and headmasters but also disrupts the municipal bureaucracy more generally. We thus believe that the increase in turnover at the school level is a reflection of a more general phenomenon occurring in municipal governments, in which new parties bring new people to work in the government; the main difference is that the number of positions at the school level is naturally restricted (e.g. there cannot be more than one headmaster per school), which constrains the margin of increase.

2.9 Conclusion

The objective of this paper is to study the effect of party turnover on bureaucrats' turnover and the consequences for the delivery of public services. To this end, we focus on close municipal elections in Brazil. We find that party turnover leads to an increase in turnover of teachers and headmasters and, more generally, an increase in the size of the bureaucracy, consistent with patronage practices. For the case of education, we find that test scores are lower and dropout rates are higher in municipalities where a new party takes office. Competing explanations such as changes in the characteristics of teachers or students, or changes in resources allocated to schools do not seem to account for our results. Instead, we find that headmasters are less experienced and teachers are more negative about the performance of the headmaster. Therefore, the evidence suggests that the disruption to the bureaucracy is not innocuous.

Elections are a tool for citizens to remove politicians that do not serve them well. While the threat of removal may be good for performance, this paper shows that its execution may bring costs that these political institutions were intending to avoid. However, our results do not imply that term limits or political turnover are intrinsically harmful for the economy. They rather emphasize one reason for coupling them with proper protection of civil servants, in order to isolate basic government functions from political transitions. Protection of civil servants has traditionally been achieved by the use of tenure regimes, which poses a serious dilemma for the design of public institutions, since tenure deprives workers from incentives to perform. We believe that a better understanding of this tension, and of potential solutionst for it, is key for our understanding of the limits and determinants of state capacity.

Chapter 3

Fixing Broken Windows: The Effect of Government Maintenance Work on Citizens' Complaints

3.1 Introduction

Quality of government is key for economic development. Governments are central in generating the conditions for bottom-up economic growth and non-monetary welfare through the provision of public goods and services. Citizens' political participation is widely regarded to be an important determinant of government representativeness and effectiveness (World Development Report 2004). However, less is known about the direct feedback effect from quality of government to citizens' participation. This paper is concerned with the following question: do citizens participate more when they face a better government?

I provide evidence on the impact of quality of government on citizens' political participation in a particular setting: government maintenance work of public space and citizens' complaints. I use a unique dataset with detailed information on citizens' complaints about and government maintenance of sidewalks, streets, trees, and streetlights in the City of Buenos Aires. The information was recorded at the city block-level between November 2012 and July 2013. The question is whether citizens are more likely to complain about problems in a given block when they observe the government working nearby. I argue that the contingencies associated with the execution of maintenance work generates plausible exogenous variation of repairs over time, and thus exploit within-block variation in order to identify the effect of repairs on complaints. I present several test for the plausibility of this assumption.

There are three reasons why this context is nicely suited to study the relevance of government effectiveness on participation. First, information asymmetries are not a constraint for citizens: residents are better informed than the government about the condition of the public space. Second, and related, it is easy for residents to observe this type of government work. Third, the cost of filing a complaint is unlikely to constitute a significant barrier to participation.

I find that government work stimulates citizens' participation. For a given block, the existence of government work nearby increases the likelihood of new complaints between 1.2 and 1.5 percentage points (4.6% to 5.7% with respect to blocks without repairs nearby).[1] Several pieces of evidence suggest that differential pre-trends or reverse causality do not drive this result: first, results are robust to including area and neighborhood trends; second, leads for repairs nearby are not significant predictors of complaints at time t; third, repairs that address notifications other than contemporaneous complaints still have an effect on current complaints. Fourth, repairs during month t are positively associated with complaints filed during the last week of month t but not with complaints filed during the first week of that month. Finally, complaints solved during month t predict complaints during the first week of month $t+1$. Reassuringly, visibility of repairs (both in terms of size of the work and public good involved) also predicts new complaints, and there also are spillovers from repairs in one public good to complaints about other public goods.

The increase in participation seems to be driven by a change in fundamental parameters of the decision-making process, as opposed to merely reflecting a shift in attention toward problems in the public space. I use repairs performed by utility service companies (water, electricity, etc.), which typically involve interventions in sidewalks and streets. Contrary to previous results, complaints do not respond to these interventions in the public space, although they would also drive citizens' attention to problems in the area. The relevant parameter seems to be an updated perception of government responsiveness: repairs that address past reports from citizens or citizens' representatives (as opposed to those coming from other sources) have the largest impact. This result still holds after adjusting for a measure of the size of the problem.

A growing literature on the determinants of citizens' participation and, more broadly, social capital has found individual, community, and institutional characteristics that predict the level of participation in community and political life. At the individual level, more educated citizens are more likely to be active (Verba and Nie 1987; Verba et al. 1995; Besley, Pande and Rao 2005; Blais and Dobrzynska 1998). Botero, Ponce and Shleifer (2013) argue that the positive relationship between education and quality of government is mediated by the fact that more educated citizens complain more, which in turn incentivizes the government to perform. Gender is also relevant for participation (Bratton 1999; Besley, Pande and Rao 2005; Giné and Mansuri 2011), and social capital is higher for citizens with more income (Glaeser, Laibson and Sacerdote 2002). At the community level, cultural aspect such as family ties predict the degree of civic engagement and political participation

---

[1] I focus on blocks that do not have repairs on a given month, in order to avoid direct effects of those repairs on complaints

(Alesina and Giuliano 2011). Moreover, in the US, Alesina and La Ferrara (2000) find that inequality and ethnic fragmentation are negatively related to participation in associational activities. Consistent with this result, Banerjee et al. (N.d.) finds that informational campaigns discouraging voting along caste lines increase voter turnout.

This paper is closest to the work that focuses on the impact of political institutions and their functioning. The institutional context can directly determine the costs and benefits from participation. Political violence is a deterrent of participation (Collier and Vicente 2014), whereas fines for abstention and vote buying seem to encourage it (León 2013; Vicente 2014). The functioning of institutions can also set expectations about the efficacy of taking actions. Information about political corruption is found to push voters to withdraw from the political process (Chong et al. 2012), whereas information about the performance and qualifications of politicians, programatic campaigns coupled with voter feedback on platforms, and campaigns to raise awareness about the relevance of the electoral process for policy increases voter turnout (Banerjee et al. 2011; Wantchekon 2003; Giné and Mansuri 2011). A more direct measure of the relevance of expectations comes from Botero, Ponce and Shleifer (2013), who find that citizens' perceived likelihood of punishment for a policeman violating the law is a strong predictor of their probability to complain about officials' misconduct.

This paper addresses the impact of actual government actions and responsiveness on participation, and provides evidence that is suggestive of concurrent changes in citizens' expectations. Taken together with the literature that documents the relevance of social capital for government quality and responsiveness (Putnam, Leonardi and Nanetti 1994; Goldin and Katz 1999; Knack 2002; Banfield 1967; Björkman, Svensson et al. 2009; Martinez-Bravo et al. 2011; Banerjee, Iyer and Somanathan 2007; Nannicini et al. 2013), it suggests that a framework that considers the co-determination of culture and institutions may be more appropriate in order to understand both government performance and citizens' political engagement.[2]

Lastly, the results in this paper are akin to the broken windows theory (Wilson and Kelling 1982), which states that physical manifestations of disorder (i.e., broken windows) promote behavior conducive to more disorder. In this case, when the government fixes problems, behavior that is conducive to more repairs is encouraged.

The remainder of the paper is organized as follows. Section 2 describes the context and the data used. Section 3 discusses the empirical strategy. Section 4 presents the main results and possible mechanisms at play. Section 5 concludes.

---

[2]Other papers that looks at the interdependence of culture and institutions are: Carlin, Dorobantu and Viswanathan (2009); Alesina et al. (2015); Alesina, Cozzi and Mantovan (2012); Alesina and Angeletos (2005); Acemoglu and Jackson (2015); Aghion et al. (2010).

3.2 Context and Data

With 203 km^2, 3 million residents, and approximately 4 more million people commuting every day, the City of Buenos Aires is one of the largest metropolitan areas of South America. As in most big cities, the condition of the public space is in constant change. Even though citizens can complain to the government about a range of problems, from corruption to streetlights to school infrastructure, most complaints are related to problems in the public space. In this paper, I will focus on complaints and maintenance work on streets, sidewalks, trees, and streetlights, since the work executed for those public goods has high volume and is better recorded.

Traditionally, citizens filed complaints directly to the corresponding office (e.g., a request for tree pruning would be filed at the tree maintenance office), where it was decided whether to address them or dismiss them. Information about complaints was not shared with other areas, resulting in low accountability within the government regarding the degree of responsiveness to citizens. The administration that won the elections in 2007 decided to give a larger role to citizens' requests in the determination of government work in the public space. A process of centralization and digitization of all complaints started in 2007, which dramatically increase their visibility within the government. In 2007, the government set up a call center to receive all complaints, in 2009 it added a website for citizens to file complaints online, and in 2012 it also incorporated mobile applications. All the complaints obtained through these channels are first registered in a single dataset, and then redirected to the corresponding government area.

Figure 3.1 depicts the increased focus of government work on complaints between November 2012 and July 2013. Even though the total number of repairs does not have any clear trend during this period, the shift of repairs toward problems reported by citizens is apparent. Within seven months, the share of repairs that were linked to a complaint went from 15% to 65%, that is, it more than quadruplicate. This increase reflects a change in the choices about what work to do and also a better registration of the match between work and existing complaints, both reflecting the growing interest in addressing citizens' complaints.

The shift in attention toward citizens' complaints was met with a growing demand for government work. Figure 3.2 shows the upward trend for the number of complaints: in two years, from the beginning of 2012 to the end of 2013, it almost duplicated. Although the government lowered the cost to complain by opening new (online) channels of communication, most of this increase came from the intensification of complaints over the phone, consistent with an increasingly demanding citizenry.

3.2.1 Organization of government work

The City of Buenos Aires is geographically divided into fifteen administrative areas, and the maintenance of each public good is managed by a separate government office. The government does not typically execute the work, but it has contractors for each public good and area. For sidewalks, streets, and trees, each of these contractors have contracts that determine an overall budget, and the prices for each possible task that they could

Figure 3.1: *Problems Fixed Over Time*
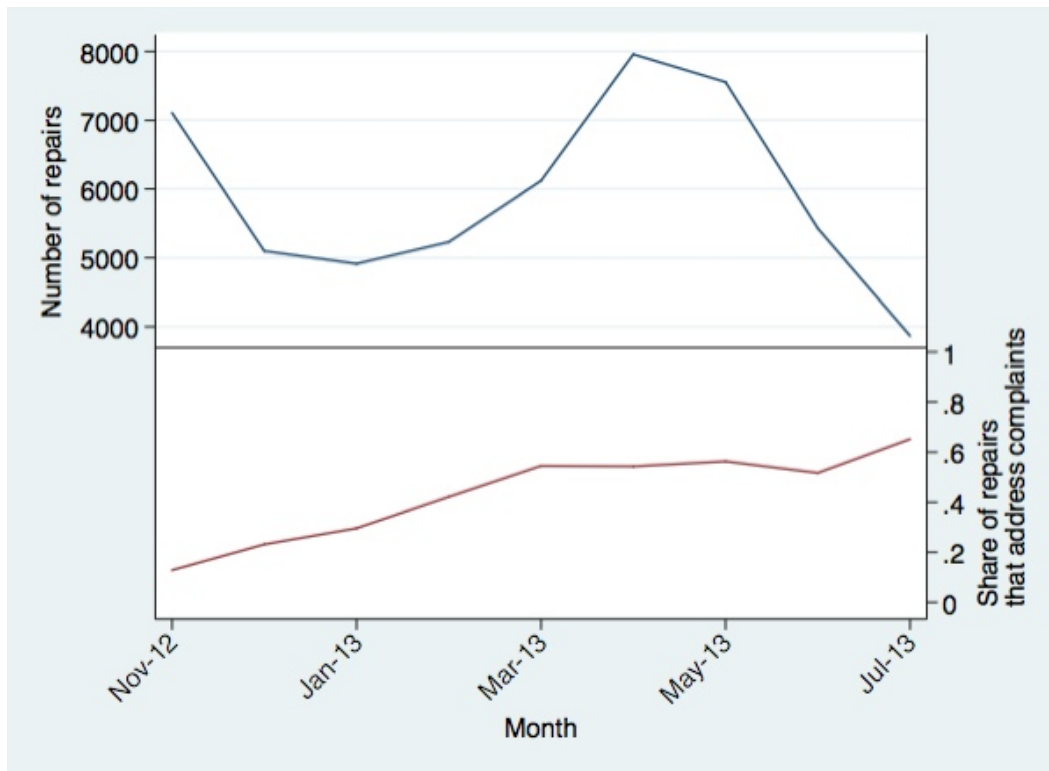
Notes: Above the horizontal line, the figure displays the total number of repairs in the City of Buenos Aires per month in the panel. Only repairs about street, sidewalks, trees, and streetlights are included. Below the horizontal line, the figure displays the share of those repairs that are addressing a complaint, that is: (complaints repaired)/(total repairs)

Figure 3.2: *Complaints Over Time*
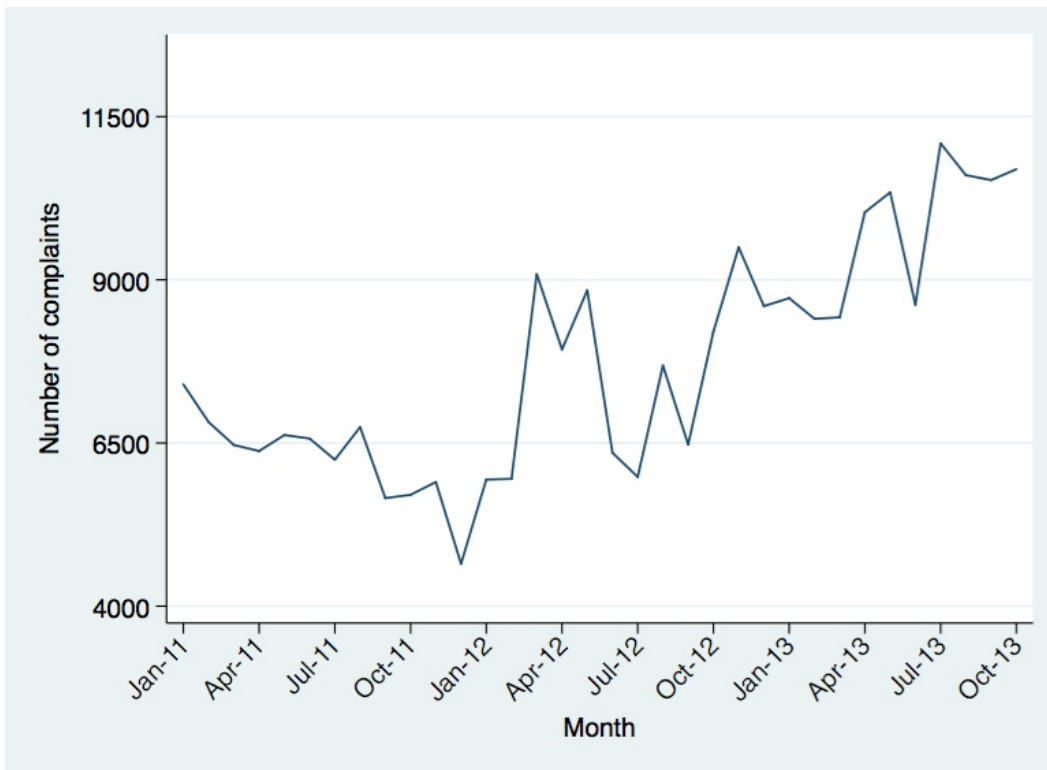
Notes: This figure displays the total number of complaints per month, between January 2011 and October 2013. Only complaints about street, sidewalks, trees, and streetlights are included.

perform. For streetlights, the contract stipulates a fix price per public light maintained.

Figure 3.3 describes the main steps for how the government organizes the maintenance work. First, the government receives and creates notifications. Citizens' complaints account for 40% of the notifications; other sources include government officials, requests from other institutions, requests from citizens' representatives, and repairs following the work of utility service companies. The corresponding government office generates work orders based on these notifications and sends them to the contractors, who execute the work orders and report back to the government office.

Figure 3.3: *Organization of Maintenance Work*
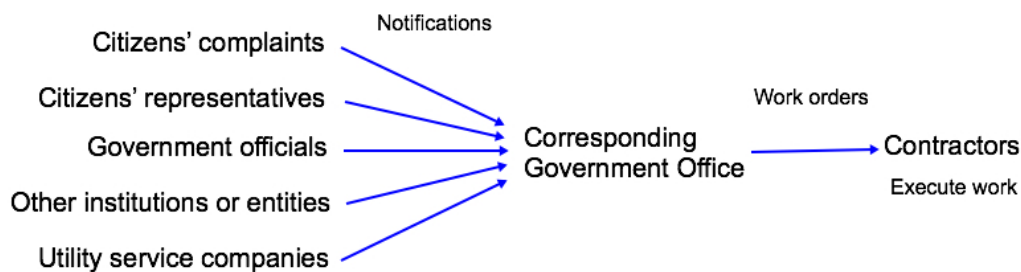
Even though the geographic distribution of work is affected by complaints, several other factors that do not depend on the government determine the timing at which the work is executed. First, contractors traditionally had a great deal of control over the allocation decision and the timing. Even though this has been changing over time, during this period contractors still retained a good degree of autonomy to decide what to execute and when. In fact, not all notifications became work orders, and not all work orders were executed: 30% of notifications created between November and December of 2012 had work orders by July 2013, of which contractors had only completed 50%. Second, maintenance work is also subjected to weather conditions. No work can be done with rain, street repairs are constrained by low temperatures, and tree pruning depends on the season and the species. Third, the sudden emergence of urgent problems (those that pose a threat to citizens) and the arrival of judicial orders delays the processing of all other work. Lastly, the available budget determines how much work is performed and its distributions over time. The budget available is affected by the time of the year and the existence of large infrastructure projects in the City. All in all, the government faced a complex work process that limited its ability to control the timing at which work would be executed.

3.2.2 Data

I count with panel data for 22,315 blocks over 9 months (November 2012 to July 2013). This data contains three pieces of information:

- Anonymous information about complaints filed during that period regarding sidewalks, streets, street-

lights, and trees. Among other things, this data contains the type of problem, the address of the problem, the day of the report, and the number of reiterations of the complaint. This data was obtained upon request to the government.

- Information about maintenance work for those public goods during the same period. Among other things, this data contains other notifications besides complaints, the type of problem reported, location, dates in the maintenance process (e.g., when the work order was created), and types of tasks executed,. Even though the exact time of the execution of the work is not registered in this data, I can recover the month of execution based on the payment to contractors for the work. This data was obtained upon request to the government.

- Information about the use of land in the City. That is, the number of buildings in each block, the type of building (house, apartments, commercial, offices, school, etc.), and the number of floors per building. This data was obtained from the City Government website.

Figure 3.4 shows the distribution of work over the blocks in the sample during the period for which I have data. Most blocks do not receive repairs, or receive a small number of repairs.

Only 35% of the blocks have 3 or more repairs during this period. The different government offices and their corresponding contractors normally do not coordinate their work, as reflected in Table 3.1 that shows the (low) correlation between repairs on different types of public goods on a given month.

Table 3.1: *Correlation of Work Across Public Goods on a Given Month*

Repairs at t for block b:				
	Sidewalks	Trees	Streetlights	Streets
Sidewalks	1			
Trees	0.0201	1		
Streetlights	0.00410	-0.0130	1	
Streets	0.00840	0.181	0.00320	1

Notes: This table shows the pairwise correlation across repairs for sidewalks, trees, streets, and streetlights during the same month and in the same block.

Approximately 10% of the blocks do not register complaints during the period considered, as shown in Figure 3.5, and the median block registers four complaints during this period.

For each block I compute the share of parcels that have residential buildings (houses or apartments), the share of parcels with commercial buildings (stores or offices), the share of parcels with other buildings (churches,

Figure 3.4: *Distribution of Repairs Across Blocks*
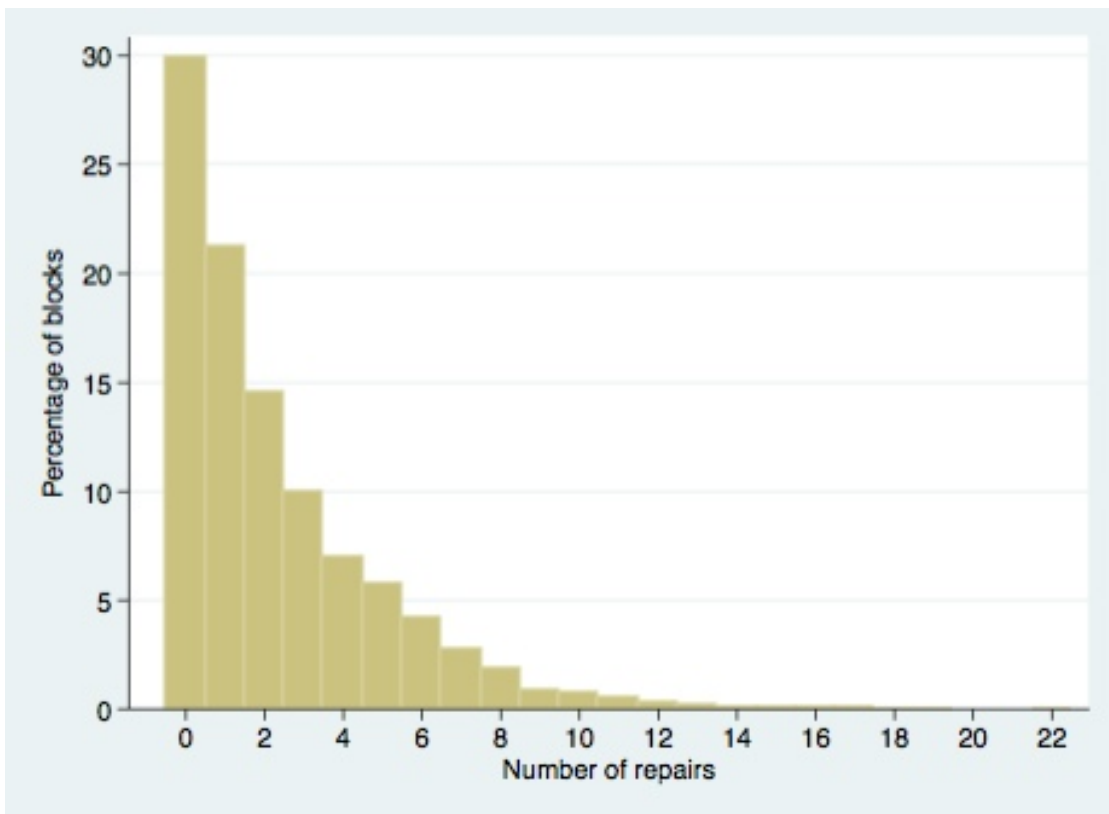
Notes: To create this figure, I first counted the number of repairs during the panel for each block. The figure shows the distribution of blocks according to the total number of repairs received.

Figure 3.5: *Distribution of Complaints Across Blocks*
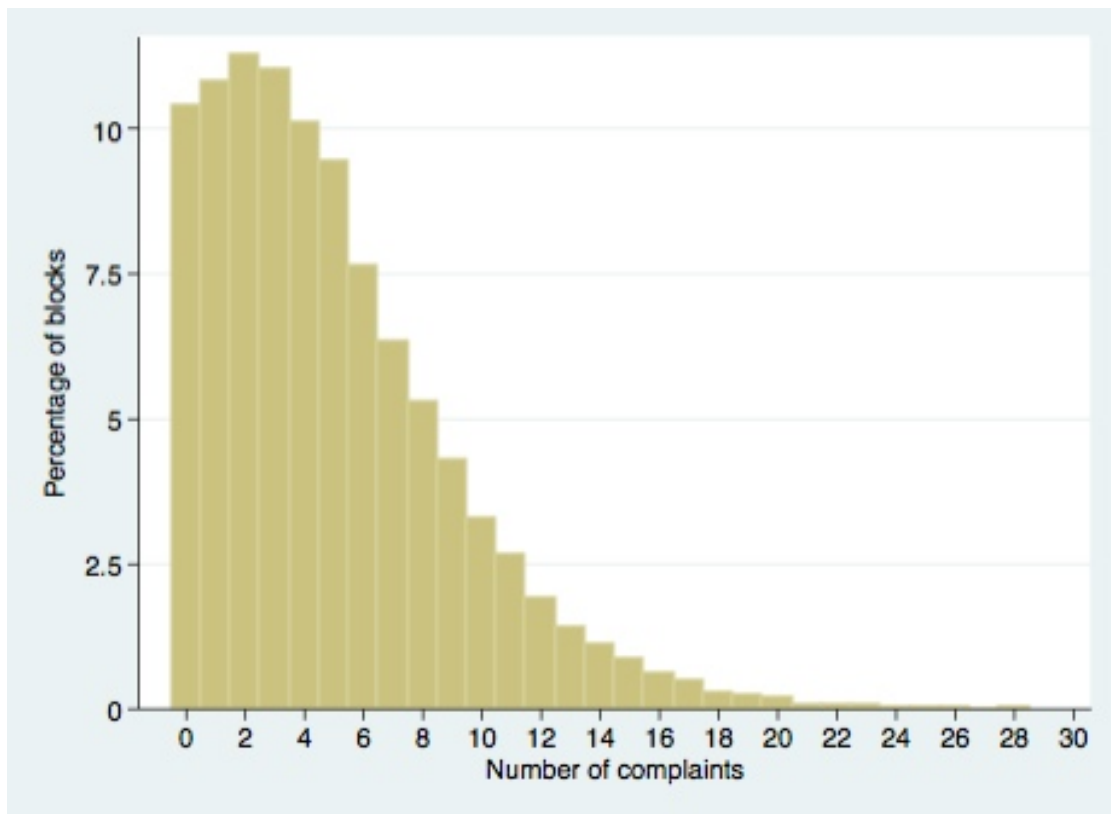
Notes: To create this figure, I first counted the number of complaints during the panel for each block. The figure shows the distribution of blocks according to the total number of complaints filed.

government offices, schools, etc), and the total number of floors in the block, as a measure of the circulation of people in the block. Table 3.2 describes the characteristics of the blocks based on whether they had complaints, repairs, and complaints repaired.

Table 3.2: *Characteristics of Blocks by Existence of Complaints, Repairs, and Complaints Repaired*

	Complaints		Repairs		Complaints s repaired	
	0	1+	0	1+	0	1+
Share residential buildings	0.481	0.626	0.591	0.624	0.591	0.638
	(0.359)	(0.266)	(0.315)	(0.261)	(0.297)	(0.255)
Share comercial buildings	0.313	0.286	0.285	0.289	0.295	0.281
	(0.314)	(0.235)	(0.269)	(0.231)	(0.256)	(0.227)
Share other buildings	0.205	0.0881	0.124	0.0874	0.114	0.0813
	(0.308)	(0.141)	(0.218)	(0.136)	(0.192)	(0.127)
Density (total number of floors)	15.83	34.30	27.02	35.03	30.04	35.58
	(19.34)	(26.63)	(25.54)	(26.66)	(26.40)	(26.50)
Block in high income area	0.398	0.502	0.464	0.502	0.497	0.484
	(0.490)	(0.500)	(0.499)	(0.500)	(0.500)	(0.500)
Previous complaints not solved	1.643	5.255	3.397	5.513	3.878	5.950
	(2.162)	(4.107)	(3.470)	(4.185)	(3.638)	(4.293)

Notes: This table reports characteristics of the blocks, according to whether they have received at least one complaint, at least one repair, and at least one complain repaired on a given month. The first three characteristics are the share of parcels in the block occupied with residential buildings (houses or apartments), comercial buildings (shops or offices), and other buildings (government buildings, churches, empty parcels, sport clubs, etc). Density is a variable that counts the total number of floors in the block. That is, if the block has 5 building with 3 floors and 5 buildings with 8 floors, this variable takes value 5*3+5*8 = 55. Block in high income area takes value 1 if the block is in an area that has income above the median for the city, and 0 otherwise. Previous complaints not solved is the sum of all complaints opened in the block between January 1st 2012 and the last month, that do not have a repair associated up to that point. The table displays the average and standard deviation (in parenthesis) for each of these characteristics, for each of the subgroups described at the beginning of this note.

Complaints are more common in higher income areas (blocks with complaints are in high income areas 50% of the time and blocks without complaints are in high income areas 39.8% of the time), in areas with residential buildings (an average of 62% of the block is residential, versus 48% in cases without complaints), in blocks with less predominance of other buildings (an average of 8.8% of the block has other buildings, versus 20.5% in cases without complaints) and in blocks with more circulation of people (an average of 34.4 floors in the block, versus

15.8 floors in cases without complaints). Moreover, complaints on a given month are more common in blocks with a larger stock of previous complaints unsolved. Blocks with at least one repair or at least one complaint repaired have similar characteristics.

3.3 Empirical Strategy

The objective is to estimate the causal effect of government maintenance work in the public space on the number of complaints by citizens. In order to avoid unobservable characteristics of the block that could correlate repairs and complaints, such as severity of the problems or neighbors' propensity to complain, I exploit variation over time at the block level. Let N_b be the pair of blocks contiguous to block b. That is, if b = {Astrada 2200-2299} (where Astrada is the name of the street), then N_b = {Astrada 2100-2199; Astrada 2300-2399}[3]. The main specification is:

$$C_{bt} = \alpha + \beta R_{N_b t} + block.dummies + time.dummies + \varepsilon_{bt} \tag{3.1}$$

where C_{bt} is an indicator that takes value 1 if there are new complaints at time t for problems in block b, $R_{N_b t}$ is an indicator that takes value 1 if there are repairs on N_b at time t, and β is the coefficient of interest. I look at repairs on N_b because those blocks are the most visible ones from block b, and focus on indicators of repairs and complaints (as opposed to quantities) because this specification is more robust to any underlying heterogeneity or misspecification.[4] Government work can affect complaints in several ways: first, the more repairs, the fewer the remaining problems to complain about; second, the repair itself may be faulty, generating new problems and complaints; and third, a repair can convey information to citizens about the government. Since I am interested in the third channel, I focus on the effect of repairs nearby in order to isolate the effect of observing repairs, while keeping constant the stock of problems to complain about at block b.

However, β could capture differential trends for blocks that observed and did not observe repairs at time t. Figure 3.6 compares the trends for these two groups. The dashed line shows the evolution of the likelihood of complaints for blocks that had repairs nearby, with the horizontal axis redefined to be centered around the time of the repair. The solid line is a weighted average of the likelihood of complaints in blocks without repairs nearby, where the weights replicate the relevance of each month t for the construction of the treatment averages. In all cases, blocks experiencing repairs at $t = 0$ are excluded in order to avoid the direct effects of the repairs described before.

Figure 3.6 shows that repairs are allocated to areas where complaining is generally higher, in line with the

---

[3]Blocks in the City of Buenos Aires are typically 100 meters long, comprise 100 numbers, and start at the multiples of 100.

[4]Results using number of repairs and/or number of complaints yields similar results

Figure 3.6: *Pre-Trends in Likelihood of Complaints*
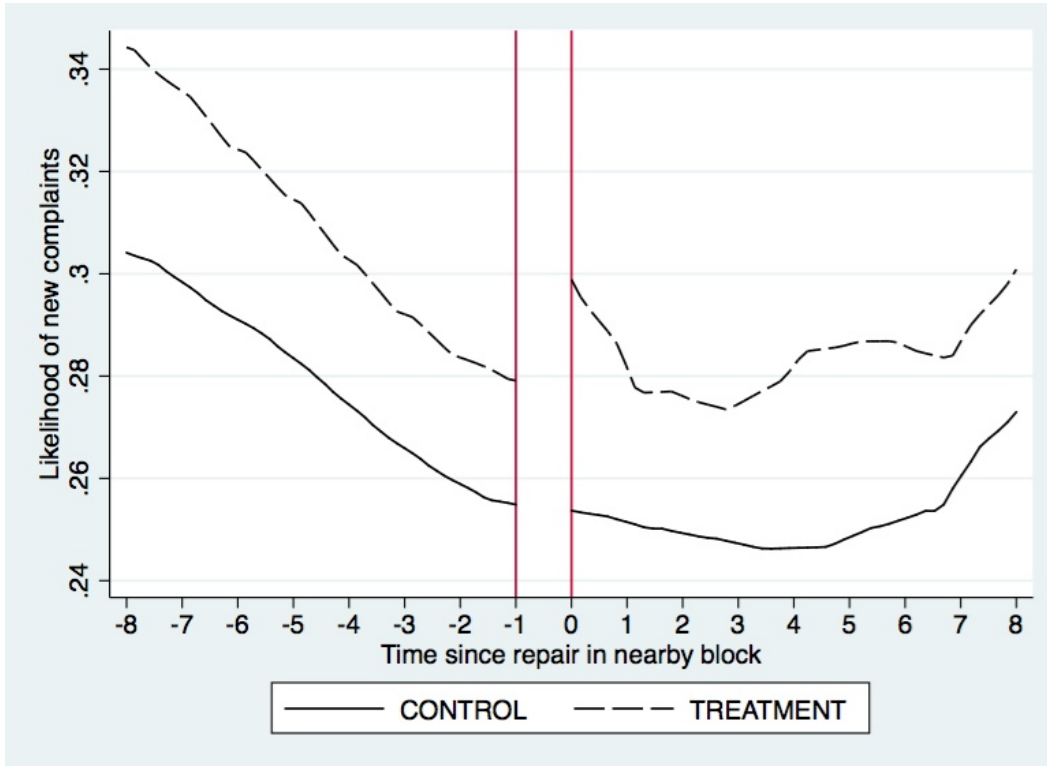
Notes: This figure compares the likelihood of new complaints on a given month, for blocks that had repairs nearby with those that didn't have repairs nearby. For each block that observed repairs nearby more than once, I take each occurrence as a separate observation. In order to construct the treatment line, I define t=0 as the month where the repair nearby happens, and keep only the blocks that didn't experience a repair themselves during that month. For each t, I calculate the share of those blocks that had at least one new complaint, which is the dashed line. In order to construct the control line, I calculate for each month, the share of blocks that had at least one complaint, among those that did not have repairs nearby that month. I also calculate for each t (as defined in this figure, after re-centering) the proportion of observations used for the treatment line that correspond to each month in the data. I then use these proportions to do a weighted average of the calculated share. This is the control line.

attempt of the government to be more responsive. At the same time, the timing does not seem to respond to a differential increase in complaints in preceding months. That is, blocks with nearby repairs at time t have the same pre-trend in complaints as the control blocks, which suggests that the fixed effects estimation is unlikely to pick up pre-existent differences. I also report results for the main specification with up to two leads and lags of the treatment variable. As a further check, in the regressions I also include controls for linear, quadratic, and cubic trends at the area and the neighborhood level.

Figure 3.6 also shows that the likelihood of complaints in block b increases during the month of the repair in N_b, which is consistent with an increase in participation when citizens observe the government working nearby. However, reverse causality could also account for this finding: in month t the government targets repairs to some blocks (but not others) in areas that are experiencing a current spike in complaints. There are two possible sources for a concurrent spike of complaints in nearby blocks. First, the existence of spatial correlation of problems. Even though contiguous blocks are likely to be similar, it is unlikely that the same type of problem will suddenly arise on both blocks during the same month. For instance, the lifespan of LED streetlights is between 50,000 and 100,000 hours; even if they are installed at the same time, the chances are small that streetlights on contiguous blocks will break during the same month. Moreover, problems that evolve slowly (such as growing trees) do not provide a natural anchor related to the problem that would drive together complaints across blocks.

A second possibility is the existence of a change in neighbors' propensity to complain. I thus empirically address the reverse causality concern in several ways: first, I use that the date of the complaint is perfectly recorded to check whether the repairs in month t are associated with complaints at the beginning or at the end of the month. The existence of reverse causality would predict that the association is stronger in the first case, and an increase in citizens' participation as a result of repairs would predict that the association is stronger in the second case. Second, I divide the effect of repairs into those that address contemporaneous complaints (that is, complaints filed during the same month) and all other repairs. If reverse causality drives the results, repairs that do not address contemporaneous complaints will not be relevant.

Third, I check whether the visibility of the problem matters. I use the number of task executed as a measure of the visibility of the repairs, and I also look at the effect of repairs for public goods with different degrees of visibility. Moreover, I look for spillovers across different public goods. That is, I check whether (for instance) a street repair in N_b at time t is associated with a higher likelihood of complaints about sidewalks in b at time t. As described in the previous section, each type of job has its own set of providers and is managed by different groups of government officials. For reverse causality to generate any relationship across types of work, spikes of complaints about one type of work in one block need to be frequently paired with spikes about work 2 in a nearby block, and the later be quickly solved. However, even in the same block the correlation of complaints for different types of problems is not very high (although they are positive), as shown in table 3.3.

Lastly, reverse causality could operate over time: citizens complained about problems in N_b (and potentially

103

Table 3.3: *Correlation of Complaints Across Public Goods on a Given Month*

Complaints at t for block b:

	Sidewalks	Trees	Streetlights	Streets
Sidewalks	1			
Trees	0.165	1		
Streetlights	0.0443	0.0722	1	
Streets	0.0729	0.0832	0.0247	1

Notes: This table shows the pairwise correlation across complaints for sidewalks, trees, streets, and streetlights during the same month and in the same block.

b) at $t-1$ and then continue to complain (at least) about b at time t. If the government replied to the complaints at time $t-1$ by fixing them at time t, then we would observe a positive association between complaints and nearby repairs at t. I thus check how sensitive are the results to including complaints in b and N_b at time $t-1$ as a control.

3.4 Results

This section presents the main results. Section 4.1 examines the effect of government work on citizens' participation and robustness checks. Section 4.2 discusses the interpretation of the results found.

3.4.1 Government work increases citizens' participation

Table 3.4 presents results for the main specification. The existence of at least one repair one block away increases the likelihood of complaints in block b during the same period in 1.2 percentage points, which corresponds to an increase of 4.6% with respect to the blocks without repairs nearby (column (1)). This result does not reflect direct effects of the repairs, as it would be the case if deficient repairs were generating further complaints, because blocks with contemporaneous repairs are excluded. Past actions from citizens or the government could potentially introduce a bias in the coefficient of interest, if they generate both complaints in b and repairs in N_b. For instance, complaints in N_b at time $t-1$ could be associated both with complaints in b and repairs in N_b at time t. Therefore, column (2) controls for the number of complaints and repairs during the previous month, both in block b and in N_b. Results are not significantly affected, suggesting that the positive relationship between complaints and repairs at t is not driven by past behavior.

Given the rather conservative definition of N_b, these estimates are likely to underestimate the real effect. In

Table 3.4: *Effect of Repairs on Complaints, Fixed Effects Estimation*

	Dummy new complaints in block b at time t							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dummy repairs Nb, t	0.0123***	0.0126***	0.0152***	0.0151***	0.0156***	0.0154***	0.0157***	0.0155***
	(0.0030)	(0.0032)	(0.0031)	(0.0031)	(0.0032)	(0.0032)	(0.0034)	(0.0033)
No repairs own block, t	x	x	x	x	x	x	x	x
Lag complaints repairs b, Nb		x	x	x	x	x	x	x
Area trends			linear		quadratic		cubic	
Neighborhood trends				linear		quadratic		cubic
Observations	169,757	151,231	151,231	151,201	151,231	151,201	151,231	151,201
R-squared	0.0038	0.0051	0.0064	0.0070	0.0068	0.0079	0.0071	0.0090
Number of blocks	22,221	22,221	22,221	22,217	22,221	22,217	22,221	22,217
Mean control	0.262	0.256	0.256	0.256	0.256	0.256	0.256	0.256

Notes: All regressions include month and block fixed effects. "Dummy new complaints in block b at time t" is a dummy that takes value 1 if there was a new complaint filed in block b at during month t, and 0 otherwise. "Dummy repairs Nb, t" is a dummy that takes value 1 if there was a repair in a block near block b during month t. All regressions exclude blocks that are experiencing repairs at t. "Lag complaints repairs b, Nb" includes as controls the number of repairs and complaints at t-1 for both block b and nearby blocks Nb. Linear trends fit a different linear trend per area/neighborhood, including their own intercept. Quadratic and cubic trends are similarly defined. Errors are clustered at area level.

all these estimations, only contiguous blocks that are on the same street as the repair are considered as treated because the repairs are most visible from those blocks. In reality, other blocks that are currently considered control may be partially treated. Moreover, a repair in a given block will increase the likelihood of complaints on both blocks nearby, and so the impact of the repair is twice this estimation.

Since the source of identification comes from variation over time, Column (3) to (9) include area-specific trends[5] and neighborhood-specific trends. Including linear, quadratic, and cubic trends at either level does not affect the results, and slightly increase the point estimate to 1.5 percentage points (5.7%). Moreover, Table 3.5 reports results including up to two leads and lags of the treatment variable. Column (1) and (2) show that neither leads nor lags are significantly associated with new complaints in block b at time t, and that their inclusion does not affect the coefficient of interest. Leads not only are not significant, but also have a small point estimate, which provides evidence against the existence of differential pre-trends.

Even if there are no differential pre-trends, this result could simply reflect that the government replies quickly to areas that experience a spike in complaints. In other words, reverse causality (combined with spatial correlation) could potentially account for the positive association between repairs in N_b and complaints in b. To test for the existence of the causal link of interest, I split the sample into repairs that are addressing complaints opened during the same month and all other repairs. Table 3.6 reports the results of the fixed effects estimation with repairs split in those two groups.

Reassuringly, repairs that are not addressing contemporaneous complaints significantly predict complaints during the current month, which points to the existence of a causal link from repairs to complaints. At the same time, repairs that address contemporaneous complaints in N_b are also associated with an increase in complaints in b. In principle, this could indicate that both causal links are at play, although a government that addresses complaints and does so in a very short period of time could have a large impact on citizens' perceptions. In order to further explore the existence of reverse causality, I make use of the fact that the date of the complaints is perfectly recorded and exploit its variation within a month. That is, I separately estimate the impact of repairs during month t on complaints filed at the beginning and at the end of the month (Panel A, Table 3.7).

Column (1) to (4) reports results for complaints filed during the first week of the month. In all cases the effect is not significant, and the point estimate is close to zero. At the same time, complaints filed during the last week of the month are significantly higher when a repair was executed nearby. Column (5) to (8) shows this result, and its robustness to controls. In the last four columns, I check for within-month variation in complaints. That is, the dependent variable is an indicator that takes value 1 if the number of complaints filed during the last week of the month is strictly higher than those filed during the first week, and 0 otherwise. The evidence indicates that when there are repairs nearby, an increase in complaints in block b during the month is more likely to happen.

---

[5]These are the 15 administrative areas described in section 2.

Table 3.5: *Fixed Effects Estimation Including Leads and Lags*

	Dummy new complaints in block b at time t		Number new complaints in block b at time t	
	(1)	(2)	(3)	(4)
Dummy repairs Nb, t+2		-0.0048		-0.0085
		(0.0053)		(0.0097)
Dummy repairs Nb, t+1	0.0011	-0.0056	0.0083	-0.0026
	(0.0033)	(0.0051)	(0.0075)	(0.0092)
Dummy repairs Nb, t	0.0145***	0.0159**	0.0299***	0.0294**
	(0.0045)	(0.0062)	(0.0084)	(0.0111)
Dummy repairs Nb, t-1	-0.0011	-0.0006	-0.0020	-0.0022
	(0.0033)	(0.0054)	(0.0061)	(0.0086)
Dummy repairs Nb, t-2		-0.0014		-0.0037
		(0.0048)		(0.0068)
No repairs own block, t	X	X	X	X
Observations	131,393	92,598	131,393	92,598
R-squared	0.0024	0.0017	0.0032	0.0023
Number of blocks	22,219	22,198	22,219	22,198
Mean control	0.252	0.249	0.364	0.358

Notes: All regressions include month and block fixed effects. "Dummy new complaints in block b at time t" is a dummy that takes value 1 if there was a new complaint filed in block b at during month t, and 0 otherwise. "Number new complaints in block b at time t" is the number of complaints opened during month t for block b. "Dummy repairs Nb, t" is a dummy that takes value 1 if there was a repair in a block near block b during month t. All regressions exclude blocks that are experiencing repairs at t. Errors are clustered at area level.

Table 3.6: *Effect of Contemporaneous Complaints Repaired vs Other Repairs*

	Dummy new complaints in block b, time t			
	(1)	(2)	(3)	(4)
Dummy contemp. complaints repaired Nb, t	0.0133**	0.0129**	0.0176***	0.0175***
	(0.0048)	(0.0053)	(0.0058)	(0.0058)
Dummy any other repair Nb, t	0.0102**	0.0102**	0.0110**	0.0109**
	(0.0039)	(0.0048)	(0.0046)	(0.0047)
No repairs own block, t	x	x	x	x
Lag complaints repairs b, Nb		x	x	x
Area trends			linear	
Neighborhood trends				linear
Observations	169,757	151,231	151,231	151,201
R-squared	0.0038	0.0055	0.0068	0.0074
Number of blocks	22,221	22,221	22,221	22,217

Notes: All regressions include month and block fixed effects. "Dummy new complaints in block b at time t" is a dummy that takes value 1 if there was a new complaint filed in block b at during month t, and 0 otherwise. "Dummy contemporaneous complaints repaired Nb,t " is a dummy that takes value 1 when there was at least one repair nearby at t that was solving a complaint opened during t, and 0 otherwise. "Dummy any other repair Nb, t" is a dummy that takes value 1 if there was at least one repair nearby at t, that is not solving a complaint opened during t, and 0 otherwise. All regressions exclude blocks that are experiencing repairs at t. "Lag complaints repairs b, Nb" includes as controls the number of repairs and complaints at t-1 for both block b and nearby blocks Nb. Linear trends fit a different linear trend per area/neighborhood, including their own intercept. Errors are clustered at area level.

Table 3.7: *Effect on Complaints Filed During First Week vs Last Week*

	Dummy new compl. block b, first week of t				Dummy new compl. block b, last week of t			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PANEL A								
Dummy repairs Nb, t	0.0022	0.0019	0.0026	0.0025	0.0063***	0.0057***	0.0068***	0.0066***
	(0.0017)	(0.0020)	(0.0021)	(0.0021)	(0.0013)	(0.0013)	(0.0012)	(0.0012)
No repairs own block, t	x	x	x	x	x	x	x	x
Lag complaints repairs b, Nb		x	x	x		x	x	x
Area trends			linear				linear	
Neighborhood trends				linear				linear
Observations	169,757	151,231	151,231	151,201	169,757	151,231	151,231	151,201
R-squared	0.0038	0.0033	0.0038	0.0041	0.0032	0.0049	0.0055	0.0060
Number of blocks	22,221	22,221	22,221	22,217	22,221	22,221	22,221	22,217
PANEL B								
Dummy complaints repaired Nb, t	0.0018	0.0006	0.0015	0.0015	0.0091***	0.0087***	0.0100***	0.0098***
	(0.0027)	(0.0026)	(0.0026)	(0.0025)	(0.0026)	(0.0024)	(0.0021)	(0.0021)
No repairs own block, t	x	x	x	x	x	x	x	x
Lag complaints repairs b, Nb		x	x	x		x	x	x
Area trends			linear				linear	
Neighborhood trends				linear				linear
Observations	169,757	151,231	151,231	151,201	169,757	151,231	151,231	151,201
R-squared	0.0038	0.0032	0.0038	0.0041	0.0033	0.0050	0.0055	0.0061
Number of blocks	22,221	22,221	22,221	22,217	22,221	22,221	22,221	22,217

Notes: All regressions include month and block fixed effects. "Dummy repairs Nb, t" is a dummy that takes value 1 if there was a repair in a block near block b during month t. "Dummy complaints repaired Nb, t" is dummy that takes value 1 if there was a complaint repaired in a block near block b during month t. In column (1) to (4), the dependent variable is a dummy that takes value 1 if there was at least one complaint open in block b during the first 7 days of month t, and 0 otherwise. In column (5) to (8), the dependent variable is similarly defined but for the last 7 days. All regressions exclude blocks that are experiencing repairs at t. "Lag complaints repairs b, Nb" includes as controls the number of repairs and complaints at t-1 for both block b and nearby blocks Nb. Linear trends fit a different linear trend per area/neighborhood, including their own intercept. Errors are clustered at area level.

Panel B of Table 3.7 replicates the previous exercise, but focusing on repairs that are originated by complaints, since those are the repairs most likely[6] to drive the reverse causality link. Results are similar, providing further assurance for the interpretation of the previous evidence. Table 3.8 further supports that repairs that address complaints have a causal effect on further complaints. In this case, I look at the effect of those repairs at time t on complaints the first week of the month $t+1$ and find a positive link: a complaint repaired at time t in N_b increases between 6.4 and 9.7 percentage points the likelihood of complaints in block b during the first week of $t+1$.

Table 3.8: *Effect on Complaints Filed During the First Week of Following Month*

	Dummy new complaints in b, first week t+1			
	(1)	(2)	(3)	(4)
Dummy complaints repaired Nb, t	0.0976***	0.0857***	0.0643***	0.0644***
	(0.0033)	(0.0037)	(0.0020)	(0.0020)
No repairs own block, t	x	x	x	x
Lag complaints repairs b, Nb		x	x	x
Area trends			linear	
Neighborhood trends				linear
Observations	149,919	131,393	131,393	131,367
R-squared	0.0024	0.0049	0.0054	0.0057
Number of blocks	22,219	22,219	22,219	22,215

Notes: All regressions include month and block fixed effects. "Dummy complaints repaired Nb, t" is dummy that takes value 1 if there was a complaint repaired in a block near block b during month t. "Dummy new complaints in block b, first week t+1" is a dummy that takes value 1 if there was at least one complaint open in block b during the first 7 days of month t+1, and 0 otherwise. All regressions exclude blocks that are experiencing repairs at t. "Lag complaints repairs b, Nb" includes as controls the number of repairs and complaints at t-1 for both block b and nearby blocks Nb. Linear trends fit a different linear trend per area/neighborhood, including their own intercept. Errors are clustered at area level.

In addition, more visible repairs have a larger effect. Each repairs is subdivided into tasks that are pre-defined in the contract signed by the provider, and are the same for all providers involved. For instance,

---

[6]Since some of the administrative systems used by the government were relatively new, sometimes repairs that addressed citizens' reports were not directly linked to the corresponding complaint but with a new internal notification.

repairing a sidewalk could include the tasks of "construction of subfloor with 12cm of thickness", "construction of internal planter", and "tile laying". A given repair specifies which tasks were executed, and how many times. Therefore, the number of tasks is a measure of the visibility of the repair that combines both its size and its length of execution. Table 3.9 includes the number of tasks executed, and finds that not only the existence of repairs matters, but the visibility of the job (defined as above) also has an impact on complaints.

Table 3.9: *Effect of Visibility of Repairs, Defined as Number of Tasks Executed*

	Dummy new complaints in block b at time t			
	(1)	(2)	(3)	(4)
Dummy repairs Nb, t	0.0078**	0.0080**	0.0110**	0.0109**
	(0.0036)	(0.0037)	(0.0038)	(0.0038)
Number of tasks Nb, t	0.0008**	0.0009**	0.0008**	0.0008*
	(0.0003)	(0.0004)	(0.0004)	(0.0004)
No repairs own block, t	x	x	x	x
Lag complaints repairs b, Nb		x	x	x
Area trends			linear	
Neighborhood trends				linear
Observations	169,757	151,231	151,231	151,201
R-squared	0.0038	0.0052	0.0064	0.0070
Number of blocks	22,221	22,221	22,221	22,217

Notes: All regressions include month and block fixed effects. "Dummy repairs Nb, t" is dummy that takes value 1 if there was a repair in a block near block b during month t. "Number of tasks Nb, t" is the number of tasks executed during month t in blocks near block b. "Dummy new complaints in block b at time t" is a dummy that takes value 1 if there was at least one complaint open in block b during month t, and 0 otherwise. All regressions exclude blocks that are experiencing repairs at t. "Lag complaints repairs b, Nb" includes as controls the number of repairs and complaints at t-1 for both block b and nearby blocks Nb. Linear trends fit a different linear trend per area/neighborhood, including their own intercept. Errors are clustered at area level.

Another way of measuring the relevance of the visibility of government work is related to the type of public good being repaired. The dataset contains complaints and repairs for the four most important public goods maintained by the city government: trees, sidewalks, streets, and streetlights. Changes in sidewalks and streetlights are typically more noticeable for residents than changes in trees or streets. Table 3.10 separates the effect of repairs by type of public good involved Column (1) shows that the increase in complaints is driven

111

by fixing sidewalks and streetlights, but streets and trees do not have a significant effect, consistent with the previous result. Fixing sidewalks increases the likelihood of complaints by approximately 3.1 percentage points (11.9%), and fixing streetlights by approximately 0.95 percentage points (3.6%). Column (2) to (5) focus on complaints for each public good separately. Repairs of one public good are not necessarily associated with more complaints for that same public good (which is the most direct implication of reverse causality), but oftentimes with complaints about other public goods.

Table 3.10: *Effect of Repairs, by Type of Public Good*

	Dummy new complaints in block b at time t				
	All	Trees	Sidewalks	Streets	Streetlights
	(1)	(2)	(3)	(4)	(5)
Dummy repairs trees Nb, t	-0.0001	0.0029	0.0008	-0.0014	-0.0005
	(0.0058)	(0.0039)	(0.0030)	(0.0023)	(0.0026)
Dummy repairs sidewalks Nb, t	0.0312***	0.0226***	0.0327***	0.0054	-0.0006
	(0.0071)	(0.0045)	(0.0065)	(0.0033)	(0.0020)
Dummy repair street Nb, t	0.0109	0.0053	0.0002	0.0074**	0.0001
	(0.0071)	(0.0036)	(0.0044)	(0.0030)	(0.0047)
Dummy repairs streetlights Nb, t	0.0095**	0.0084*	0.0014	0.0059***	-0.0052
	(0.0043)	(0.0045)	(0.0025)	(0.0015)	(0.0038)
No repairs own block, t	x	x	x	x	x
Observations	169,757	169,757	169,757	169,757	169,757
R-squared	0.0039	0.0015	0.0020	0.0008	0.0186
Number of blocks	22,221	22,221	22,221	22,221	22,221
Mean control	0.262	0.125	0.0859	0.0402	0.0717

Notes: All regressions include month and block fixed effects. "Dummy new complaints in block b at time t" is a dummy that takes value 1 if there was a new complaint filed in block b during month t, and 0 otherwise. Column (1) looks at any complaint, whereas column (2) to (5) only look at complaints regarding trees, sidewalks, streets, and streetlights respectively. "Dummy repairs trees Nb, t" is a dummy that takes value 1 if there was a repair of a tree in a block near block b during month t, and 0 otherwise. Variables for sidewalks, street, and streetlights are defined in the same way. All regressions exclude blocks that are experiencing any repair at t. "Lag complaints repairs b, Nb" includes as controls the number of repairs and complaints at t-1 for both block b and nearby blocks Nb, separately for each public good. Errors are clustered at area level.

In sum, the evidence indicates that government work does stimulate citizens participation: repairing

problems in a block generates an increase in citizens' complaints in nearby blocks. Does this imply a change in citizens' perception about the government, or it simply captures a shift in attention towards problems in the public space? In the next section, I present evidence that aims at distinguishing among potential stories behind this result.

3.4.2 Interpretation

There are two big categories of explanations for the result found so far. On the one hand, citizens may become more active because the repairs convey information about the expected value of complaining. On the other hand, repairs may just draw citizens' attention toward problems in the public space. In this section I present results that suggest that government work increases citizens' participation by improving citizens' expectations about government responsiveness.

Not only the government intervenes in the public space. Another important actor are utility service companies that provide gas, electricity, and water. Whenever there is a problem with their service, or maintenance work to perform more generally, companies notify the government and proceed to execute the work. The type of work they perform many times involves opening a hole in the sidewalk (and sometimes in the street), which usually remains open for at least a few days. In general, their employees are quite distinguishable from government employees: they wear identifying uniforms and their vehicles have the name of the company plotted . Therefore, their work is visible for citizens, and normally not attributed to the government. If government repairs were driving attention to problems in the public space, it could be expected that work performed by utility service companies would have the same effect. I use information about the location and time of interventions by companies to test whether this is the case. Table 3.11 present the results for the main specification using the work done by providers.

Columns (1) to (4) show the estimated effect of companies' work in N_b on citizens' complaints in b at time t, separated by whether the work was announced to the government with anticipation (planned work) or spontaneous (emergency). In all cases the point estimates are small and not significant. Column (5) to (8) combines both types of work into one indicator for any work performed by companies, and the lack of effect still remains. Therefore, citizens do not seem to react to interventions in the public space by service utility service companies, which provides evidence against the hypothesis that saliency is what drives the main result.

Alternatively, repairs may convey information about the government, which in turn could affect the expected payoff of complaining. In its simplest form, a citizen will complain if:

$$Value(solution) * [P(solution|complaint) - P(solution|no.complaint)]cost(complaint) \qquad (3.2)$$

Repairs could be taken as a signal of state capacity or of government responsiveness, both with a pos-

Table 3.11: *Effect of Work by Utility Service Companies*

	Dummy new complaints in block b at time t							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dummy emergency work Nb, t	0.0011	-0.0010	-0.0008	-0.0017				
	(0.0022)	(0.0022)	(0.0022)	(0.0021)				
Dummy planned work Nb, t	0.0043	0.0090	0.0097	0.0093				
	(0.0051)	(0.0066)	(0.0066)	(0.0067)				
Dummy any work Nb, t					0.0026	0.0016	0.0018	0.0010
					(0.0024)	(0.0024)	(0.0024)	(0.0023)
No repairs own block, t	x	x	x	x	x	x	x	x
lag own block		x	x	x		x	x	x
area trends			linear				linear	
neighborhood trends				linear				linear
Observations	169,757	151,231	151,231	151,201	169,757	151,231	151,231	151,201
R-squared	0.0037	0.0050	0.0062	0.0069	0.0037	0.0050	0.0062	0.0068
Number of blocks	22,221	22,221	22,221	22,217	22,221	22,221	22,221	22,217
Mean control	0.262	0.256	0.256	0.256	0.262	0.256	0.256	0.256

Notes: All regressions include month and block fixed effects. "Dummy emergency work Nb, t" is dummy that takes value 1 if there was an emergency work opened by a utility service company in a block near block b during month t. "Dummy planned work Nb, t" is dummy that takes value 1 if there was planned work opened by a utility service company in a block near block b during month t. "Dummy any work Nb, t" is dummy that takes value 1 if there was either planned or emergency work opened by a utility service company in a block near block b during month t. "Dummy new complaints in block b at time t" is a dummy that takes value 1 if there was at least one complaint open in block b during month t, and 0 otherwise. All regressions exclude blocks that are experiencing repairs at t. "Lag complaints repairs b, Nb" includes as controls the number of repairs and complaints at t-1 for both block b and nearby blocks Nb. Linear trends fit a different linear trend per area/neighborhood, including their own intercept. Errors are clustered at area level.

114

itive effect on the perceived effectiveness of complaints, $[P(solution|complaint) - P(solution|no.complaint)]$.[7] Whereas state capacity is related to the execution of work in general, work that was requested by citizens is more tightly related to government responsiveness. Therefore, in order to distinguish between these two explanations, I separately identify the effect of repairs that come from citizens' requests from those that come from other sources.

Table 3.12 disaggregates the effect of repairs into these five sources: citizens' complaints, requests by citizens' representatives, work decided internally by the government, work requested to the government by judges or other institutions, and government repairs related to previous work done by utility service companies. I find that the existence of repairs addressing citizens' requests (directly or through representatives) have the largest impact. However, problems reported by citizens may be different from problems notified by other sources, accounting for the differential impact. In Table 3.13 I thus focus on the number of tasks performed in order to solve the problem, as a measure of its size and complexity. Results show that underlying differences across problems are not driving the differential impact between repairs from different sources, at least under this measure of their characteristics. All in all, the result found suggests that the perception of government responsiveness may be the main explanation for an increase in citizens' participation.

3.5 Discussion

Does government effectiveness affect citizens' participation? I examine this question in the context of work in the public space and citizens? complaints in the City of Buenos Aires. This context is particularly well suited to capture changes in beliefs, since barriers to participation are relatively low and work is easily observable.

This paper gives a positive answer: when citizens observe the government working in the public space, they are more likely to complain about other problems in the public space. A variety of tests suggest that reverse causality or differential pre-trends do not drive this result, but an updated perception of government responsiveness is what stimulates participation.

In developing countries, access to public goods is not only low overall, but also quite heterogeneous, both across countries and within countries (Banerjee, Iyer and Somanathan 2007). This heterogeneity and its persistence could potentially be explained by the existence of multiple equilbria: in some areas, good provision leads citizens to hold higher expectations about the government, which in turn stimulates political actions that reinforce good provision; whereas in some other places, citizens' expectation about the government are very low, which in turn leads to low participation and hence worse outcomes. This paper contributes by providing

---

[7]In both cases, there could be a negative second order effect. If citizens perceive that state capacity is higher, they may believe that a solution will ensue regardless of their complaints, which in turn would disincentive their participation. Moreover, if responsiveness is perceived to be higher, citizens may prefer to free ride from neighbors, as their participation may be believed to be enough now to obtain a solution.

Table 3.12: *Effect of Repairs, by Source of Notification*

	Dummy new complaints in block b at time t			
	(1)	(2)	(3)	(4)
Dummy repairs Nb at t, from:				
Complaints	0.0151***	0.0139***	0.0175***	0.0173***
	(0.0044)	(0.0045)	(0.0046)	(0.0046)
Representatives	0.0254**	0.0411**	0.0418**	0.0404**
	(0.0102)	(0.0141)	(0.0145)	(0.0147)
Government officials	0.0027	0.0031	0.0026	0.0026
	(0.0045)	(0.0054)	(0.0055)	(0.0055)
Other institutions	0.0274	0.0208	0.0213	0.0206
	(0.0202)	(0.0264)	(0.0261)	(0.0262)
Utility service companies	0.0097	0.0079	0.0088	0.0093
	(0.0092)	(0.0093)	(0.0092)	(0.0094)
No repairs own block, t	x	x	x	x
Lag complaints repairs b, Nb		x	x	x
Area trends			linear	
Neighborhood trends				linear
Observations	169,757	151,231	151,231	151,201
R-squared	0.0039	0.0054	0.0067	0.0074
Number of blocks	22,221	22,221	22,221	22,217

Notes: All regressions include month and block fixed effects. "Dummy new complaints in block b at time t" is a dummy that takes value 1 if there was a new complaint filed in block b during month t, and 0 otherwise. "Dummy repairs Nb at t, from complaints" is a dummy that takes value 1 if there was at least one repair in a block near block b during month t that was originated by a complaint, and 0 otherwise. Variables for representatives, government officials, other institutions, and utility service companies are defined in the same way. All regressions exclude blocks that are experiencing any repair at t. "Lag complaints repairs b, Nb" includes as controls the number of repairs and complaints at t-1 for both block b and nearby blocks Nb, separately for each source of notification. Errors are clustered at area level.

Table 3.13: *Effect of Repairs, by Source of Notification, Adjusting by Size of Problem*

	Dummy new complaints in block b at time t			
	(1)	(2)	(3)	(4)
Number of tasks performed Nb at t, from:				
Complaints	0.0040***	0.0039***	0.0044***	0.0044***
	(0.0008)	(0.0008)	(0.0008)	(0.0008)
Representatives	0.0059**	0.0064**	0.0067**	0.0065**
	(0.0021)	(0.0026)	(0.0026)	(0.0026)
Government officials	-0.0000	0.0001	0.0000	0.0000
	(0.0003)	(0.0004)	(0.0004)	(0.0004)
Other institutions	0.0033*	0.0025	0.0025	0.0024
	(0.0017)	(0.0020)	(0.0020)	(0.0020)
Utility service companies	0.0006	0.0007	0.0007	0.0008
	(0.0012)	(0.0012)	(0.0012)	(0.0012)
No repairs own block, t	x	x	x	x
Lag complaints repairs b, Nb		x	x	x
Area trends			linear	
Neighborhood trends				linear
Observations	169,757	151,231	151,231	151,201
R-squared	0.0040	0.0056	0.0069	0.0075
Number of blocks	22,221	22,221	22,221	22,217

Notes: All regressions include month and block fixed effects. "Dummy new complaints in block b at time t" is a dummy that takes value 1 if there was a new complaint filed in block b during month t, and 0 otherwise. "Number of tasks performed Nb at t, from complaints" is the number of tasks executes in blocks near block b during month t that were originated by a complaint. Variables for representatives, government officials, other institutions, and utility service companies are defined in the same way. All regressions exclude blocks that are experiencing any repair at t. "Lag complaints repairs b, Nb" includes as controls the number of repairs and complaints at t-1 for both block b and nearby blocks Nb, separately for each source of notification. Errors are clustered at area level.

evidence for the causal effect of quality of government on participation, which complements previous work on the causal effect of participation on quality of government.

References

Acemoglu, Daron and Matthew O Jackson. 2015. "History, expectations, and leadership in the evolution of social norms." *The Review of Economic Studies* 82(2):423–456.

Aghion, Philippe, Yann Algan, Pierre Cahuc and Andrei Shleifer. 2010. "Regulation and distrust." *Quarterly Journal of Economics* 125(3).

Alesina, Alberto and Eliana La Ferrara. 2000. "Participation in Heterogeneous Communities." *Quarterly Journal of Economics* 115.

Alesina, Alberto and George-Marios Angeletos. 2005. "Fairness and Redistribution." *The American Economic Review* 95(4):960.

Alesina, Alberto, Guido Cozzi and Noemi Mantovan. 2012. "The Evolution of Ideology, Fairness and Redistribution*." *The Economic Journal* 122(565):1244–1261.

Alesina, Alberto and Paola Giuliano. 2011. "Family ties and political participation." *Journal of the European Economic Association* 9(5):817–839.

Alesina, Alberto, Yann Algan, Pierre Cahuc and Paola Giuliano. 2015. "Family values and the regulation of labor." *Journal of the European Economic Association* .

Auerbach, Adam. Forthcoming. "Clients and Communities: The Political Economy of Party Network Organization and Development in India's Urban Slums." *World Politics* .

Banerjee, Abhijit, Donald Green, Jennifer Green and Rohini Pande. N.d. Can voters be primed to choose better legislators? Experimental evidence from rural India. Citeseer.

Banerjee, Abhijit, Lakshmi Iyer and Rohini Somanathan. 2007. "Public action for public goods." *Handbook of development economics* 4:3117–3154.

Banerjee, Abhijit V, Selvan Kumar, Rohini Pande and Felix Su. 2011. "Do Informed Voters Make Better Choices? Experimental Evidence from Urban India.".

Banfield, Edward C. 1967. "The moral basis of a backward society.".

Besley, Timothy and Anne Case. 1995. "Does Electoral Accountability Affect Economic Policy Choices? Evidence from Gubernatorial Term Limits." *The Quarterly Journal of Economics* pp. 769–798.

Besley, Timothy, Rohini Pande and Vijayendra Rao. 2005. "Participatory democracy in action: Survey evidence from South India." *Journal of the European Economic Association* 3(2-3):648–657.

Besley, Timothy, Torsten Persson and Marta Reynal-Querol. 2013. "Political Instability and Institutional Reform: Theory and Evidence.".

Björkman, M, J Svensson et al. 2009. "Power to the people: evidence from a randomized field experiment on community-based monitoring in Uganda." *Quarterly Journal of Economics* 124(2):735–769.

Blais, André and Agnieszka Dobrzynska. 1998. "Turnout in electoral democracies." *European Journal of Political Research* 33(2):239–262.

Botero, Juan, Alejandro Ponce and Andrei Shleifer. 2013. "Education, Complaints, and Accountability." *Journal of Law and Economics* 56(4):959–996.

Bratton, Michael. 1999. "Political participation in a new democracy institutional considerations from Zambia." *Comparative Political Studies* 32(5):549–588.

Burgwald, Gerrit. 1996. *Struggle of the Poor: Neighborhood Organization and Clientelist Practice in a Quito Squatter Settlement*. Amsterdam: CEDLA.

Calonico, Sebastian, Matias D Cattaneo and Rocio Titiunik. 2014. "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs." *Econometrica* 82(6):2295–2326.

Carlin, Bruce Ian, Florin Dorobantu and S Viswanathan. 2009. "Public trust, the law, and financial investment." *Journal of Financial Economics* 92(3):321–341.

Carreras López, Juan Manuel. 2008. *Estudios Agrarios* 38:7–21.

Castañeda Dower, Paul and Tobias Pfutze. 2015. "Vote Suppression and Insecure Property Rights." *Journal of Development Economics* 114:1–19.

Chong, Alberto, O De La, L Ana, Dean Karlan and Leonard Wantchekon. 2012. Looking beyond the incumbent: The effects of exposing corruption on electoral outcomes. Technical report National Bureau of Economic Research.

Clichevsky, Nora. 2003. "Pobreza y Acceso al Suelo Urbano. Algunas Interrogantes sobre las Políticas de Regularización en América Latina." *Naciones Unidas* .

Collier, Paul and Pedro C Vicente. 2014. "Votes and violence: evidence from a field experiment in Nigeria." *The Economic Journal* 124(574):F327–F355.

de Janvry, Alain, Kyle Emerick, Marco Gonzalez-Navarro and Elisabeth Sadoulet. forthcoming. "Delinking Land Rights from Land Use: Certification and Migration in Mexico." *American Economic Review* .

de Janvry, Alain, Marco Gonzalez-Navarro and Elisabeth Sadoulet. 2014. "Are Land Reforms Granting Complete Property Rights Politically Risky? Electoral Eutcomes of Mexico's Certification Program." *Journal of Development Economics* 110:216–225.

De La O, Ana L. 2013. "Do Conditional Cash Transfers Affect Electoral Behavior? Evidence from a Randomized Experiment in Mexico." *American Journal of Political Science* 57(1):1–14.

Di Tella, Rafael, Sebastian Galiani and Ernesto Schargrodsky. 2007. "The Formation of Beliefs: Evidence from the Allocation of Land Titles to Squatters." *Quarterly Journal of Economics* 122(1):209–241.

Diaz-Cayeros, Alberto, Federico Estévez and Beatrix Magaloni. forthcoming. *Strategies of Vote Buying: Democracy, Clientelism, and Poverty Relief in Mexico*. New York: Cambridge University Press.

Díaz, Rodrigo. 2008. "City Growth and Community-owned Land in Mexico City." *MIT Thesis* .

Ferraz, Claudio and Frederico Finan. 2011. "Electoral Accountability and Corruption: Evidence from the Audits of Local Governments." *The American Economic Review* 101(4):1274.

Ferreira, Fernando and Joseph Gyourko. 2009. "Do Political Parties Matter? Evidence from US Cities-super." *The Quarterly Journal of Economics* 124(1):399–422.

Field, Erica. 2005. "Property Rights and Investment in Urban Slums." *Journal of the European Economic Association Papers and Proceedings* 3(2-3):279–290.

Field, Erica. 2007. "Entitled to Work: Urban Property Rights and Labor Supply in Peru Erica Field." *The Quarterly Journal of Economics* 122(4):1561–1602.

Field, Erica and Maximo Torero. 2008. "Do Property Titles Increase Credit Access Among the Urban Poor? Evidence from a Nationwide Titling Program.".

Finan, Frederico and Laura Schechter. 2012. "Vote-Buying and Reciprocity." *Econometrica* 80(2):863–881.

Flores Rodríguez, Carlos E. 2008. "Suelo Ejidal en México. Un Acercamiento al Origen y Destino del Suelo Ejidal en México." *Cuaderno de Investigación Urbanística* 57:1–86.

Folke, Olle, Shigeo Hirano and James M. Snyder. 2011*a*. "Patronage and Elections in U.S. States." *American Political Science Review* 105(3):567–585.

Folke, Olle, Shigeo Hirano and James M Snyder. 2011*b*. "Patronage and elections in US states." *American Political Science Review* 105(03):567–585.

Fox, Sean. 2014. "The Political Economy of Slums: Theory and Evidence from Sub-Saharan Africa." *World Development* 54:191–203.

Garcés Fierros, César. 2009. *Suelo Urbano para la Población Pobre de México*. México, Distrito Federal: pp. 87–102.

Gay, Robert. 1990. "Community Organization and Clientelist Politics in Contemporary Brazil: A Case Study from Suburban Rio de Janeiro." *International Journal of Urban and Regional Research* 14:648?–666.

Gay, Robert. 1994. *Popular Organization and Democracy in Rio do Janeiro: A Tale of Two Favelas*. Philadelphia: Temple University Press.

Giné, Xavier and Ghazala Mansuri. 2011. "Together we will: evidence from a field experiment on female voter turnout in Pakistan." *World Bank Policy Research Working Paper* 5692.

Glaeser, Edward L, David Laibson and Bruce Sacerdote. 2002. "An economic approach to social capital*." *The Economic Journal* 112(483):F437–F458.

Goldin, Claudia and Lawrence F Katz. 1999. "Human capital and social capital: the rise of secondary schooling in America, 1910–1940." *Journal of interdisciplinary history* 29(4):683–723.

Hansungule, Michelo, Patricia Feeney and Robin Palmer. 1998. "Report on the Land Tenure Insecurity on the Zambian Copperbelt.".

Holzner, Claudio A. 2004. "The End of Clientelism? Strong and Weak Networks in a Mexican Squatter Movement." *Mobilization* 9(3):223–240.

Imbens, Guido and Karthik Kalyanaraman. 2012. "Optimal Bandwidth Choice for the Regression Discontinuity Estimator." *Review of Economic Studies* 79(3):933–959.

Iyer, Lakshmi and Anandi Mani. 2012. "Traveling agents: political change and bureaucratic turnover in India." *Review of Economics and Statistics* 94(3):723–739.

Jacob, Brian A. 2010. "The Effect of Employment Protection on Worker Effort: Evidence from Public Schooling.".

Keefer, Philip. 2007. "Clientelism, Credibility, and the Policy Choices of Young Democracies." *American Journal of Political Science* 51(4):804–821.

Keefer, Philip and Razvan Vlaicu. 2008. "Democracy, Credibility, and Clientelism." *Journal of Law, Economics, and Organization* 24(2):371–406.

Kingdon, Geeta and Francis Teal. 2010. "Teacher unions, teacher pay and student performance in India: A pupil fixed effects approach." *Journal of Development Economics* 91(2):278–288.

Kitschelt, Herbert and Steven I. Wilkinson. 2007. *Patrons, Clients, and Policies: Patterns of Democratic Accountability and Political Competition*. Cambridge, UK: Cambridge University Press.

Knack, Stephen. 2002. "Social capital and the quality of government: Evidence from the states." *American Journal of Political Science* pp. 772–785.

Kuehl, Alexander L. 2013. "Urbanization and Social Control. Divergent State Policies in Squatter Societies." *Policy Analysis* 4(2):18–33.

Larreguy, Horacio. 2013. "Monitoring Political Brokers: Evidence from Clientelistic Networks in Mexico." Working paper.

Larreguy, Horacio, John Marshall and Pablo Querubin. forthcoming. "Parties, Brokers and Voter Mobilization: How Turnout Buying Depends Upon the Party's Capacity to Monitor Brokers." *American Political Science Review* .

Lawson, Chappell and Kenneth F. Greene. 2014. "Making Clientelism Work: How Norms of Reciprocity Increase Voter Compliance." *Comparative Politics* 47(1):61–85.

León, Gianmarco. 2013. "Turnout, political preferences and information: Experimental evidence from Perú." Working paper.

Lizzeri, Alessandro and Nicola Persico. 2001. "The Provision of Public Goods under Alternative Electoral Incentives." *American Economic Review* 91(1):225–239.

Magaloni, Beatriz. 2006. *Voting for Autocracy: Hegemonic Party Survival and its Demise in Mexico*. New York: Cambridge University Press.

Manacorda, Marco, Edward Miguel and Andrea Vigorito. 2011. "Government Transfers and Political Support." *American Economic Journal: Applied Economics* 3(3):1?–28.

Martinez-Bravo, Monica, Nancy Qian, Yang Yao et al. 2011. Do local elections in non-democracies increase accountability? Evidence from rural China. Technical report National Bureau of Economic Research.

Menezes-Filho, Naércio and Elaine Pazello. 2007. "Do teachers? wages matter for proficiency? Evidence from a funding reform in Brazil." *Economics of Education Review* 26(6):660–672.

Miranda, Jéssica and Elaine Pazello. 2014. "Rotatividade de Diretores e Desempenho da Escola.".

Muralidharan, Karthik and Venkatesh Sundararaman. 2013. Contract teachers: Experimental evidence from India. Technical report National Bureau of Economic Research.

Nannicini, Tommaso, Andrea Stella, Guido Tabellini and Ugo Troiano. 2013. "Social capital and political accountability." *American Economic Journal: Economic Policy* 5(2):222–250.

Nichter, Simeon. 2008. "Vote Buying or Turnout Buying? Machine Politics and the Secret Ballot." *American Political Science Review* 102(1):19–31.

Putnam, Robert D, Robert Leonardi and Raffaella Y Nanetti. 1994. *Making democracy work: Civic traditions in modern Italy*. Princeton university press.

Rasul, Imran and Daniel Rogger. 2014. "Management of Bureaucrats and Public Service Delivery: Evidence from the Nigerian Civil Service.".

Rauch, James E. 1995. "Bureaucracy, infrastructure, and economic growth: Evidence from US cities during the Progressive Era." *The American Economic Review* 85(4):968.

Rauch, James E and Peter B Evans. 2000. "Bureaucratic structure and bureaucratic performance in less developed countries." *Journal of public economics* 75(1):49–71.

Robinson, James A. and Thierry Verdier. 2013*a*. "The Political Economy of Clientelism." *The Scandinavian Journal of Economics* 115(2):260–291.

Robinson, James A. and Thierry Verdier. 2013*b*. "The Political Economy of Clientelism." *Scandinavian Journal of Economics* 115(2):260–291.

Rockoff, Jonah E, Brian A Jacob, Thomas J Kane and Douglas O Staiger. 2011. "Can you recognize an effective teacher when you recruit one?" *Education* 6(1):43–74.

Schaffer, Frederic C. 2007. *Elections for Sale: The Causes and Consequences of Vote Buying*. Boulder, CO: Lynne Rienner Publishers.

SEDESOL. 2011. "Manual Ciudadano 2011: Sedesol a los Ojos de Todos." *Secretaría de Desarrollo Social* .

Shami, Mahvish and Hadia Majid. 2014. "The Political Economy of Public Goods Provision in Slums.".

Smart, Michael and Daniel M Sturm. 2013. "Term limits and electoral accountability." *Journal of Public Economics* 107:93–102.

Staiger, Douglas O and Jonah E Rockoff. 2010. "Searching for effective teachers with imperfect information." *The Journal of Economic Perspectives* 24(3):97–117.

Stokes, Susan C., Thad Dunning, Marcelo Nazareno and Valeria Brusco. 2013. *Brokers, Voters, and Clientelism: The Puzzle of Distributive Politics*. Cambridge University Press.

Trujeque Díaz, José Antonio. 1997. "Dinámicas Política y Social de la Urbanización Popular en Nuevo Laredo (1990-1995)." *Estudios Demográficos y Urbanos* 12(3):521–548.

Ujhelyi, Gergely. 2014*a*. "Civil Service Reform." *Journal of Public Economics* 118:15–?25.

Ujhelyi, Gergely. 2014*b*. "Civil Service Rules and Policy Choices: Evidence from US State Governments." *American Economic Journal: Economic Policy* 6(2):338–380.

Varley, Ann. 1994. *Revista Mexicana de Sociología* 56(4):135–164.

Verba, Sidney, Kay Lehman Schlozman, Henry E Brady and Henry E Brady. 1995. *Voice and equality: Civic voluntarism in American politics*. Vol. 4 Cambridge Univ Press.

Verba, Sidney and Norman H Nie. 1987. *Participation in America: Political democracy and social equality*. University of Chicago Press.

Vicente, Pedro C. 2014. "Is vote buying effective? Evidence from a field experiment in West Africa." *Economic Journal* 124(February):356–387.

Villalón, Roberta. 2003. "Navigating Clientelist Networks: The Beginnings of a Journey, Neuquen, Argentina 2003.".

Vite Pérez, Miguel Ángel. 2001. "Clientelismo Político y Exclusión Social: el Caso de Cuautepec." *Sociológica* 16(47):199–238.

Wantchekon, Leonard. 2003. "Clientelism and Voting Behavior: Evidence from a Field Experiment in Benin." *World Politics* 55(3):399–422.

Wilson, James Q and George L Kelling. 1982. "Broken windows." *Atlantic monthly* 249(3):29–38.

Zucco, Cesar. 2013. "When Payouts Pay Off: Conditional Cash Transfers and Voting Behavior in Brazil 2002-10." *American Journal of Political Science* 57(4):810?–822.