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Essays on the Political Economy of Development

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The dissertation of Bruno Lopez Videla Mostajo is approved, and it is acceptable in quality and form for publication on microfilm and electronically.

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DEDICATION

To my family.

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ABSTRACT OF THE DISSERTATION

Essays on the Political Economy of Development

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Economic development is deeply connected with politics. Policies that foster economic growth and reduce poverty typically rely on politicians who might prefer specific groups and have career concerns. This dissertation consists of three chapters related to this issue.

Chapter 1 studies the role that political time horizons play on investments in service delivery. It leverages a policy change in Mexico that removed a ban on the consecutive re-election of local politicians in the context of staggered local elections. The results show that mayors affected by the reform make significantly more investments in the ability of their local governments to provide public goods; they also borrow less and finance their investments through less corrupt practices. The chapter presents a theoretical framework that discusses some of the key incentives

at play and proposes a simple strategy to parse out these incentives empirically. The estimated treatment effects are more pronounced in places where mayors face a higher probability of re-election, suggesting that the longer horizon effect is a key incentive driving the results.

Chapter 2 studies the role that political parties play in the allocation of credit by development banks. Using a regression discontinuity design in the context of loans to municipal governments in Mexico, it shows that municipal governments represented by a mayor of the presidential party are significantly more likely to receive new loans and more resources. These effects are driven by the allocation of credit by the largest state-owned development bank, whereas there is no effect on the allocation of credit by private banks. Municipalities where a candidate of the presidential party won spend more in debt services, yet the evidence overall indicates that they do not increase service delivery.

Chapter 3 studies whether voters reward politicians for the provision of public infrastructure. Leveraging different sources of variation in the allocation of a public infrastructure program in Mexico, it finds that voters living near the infrastructure projects do not reward municipal and federal incumbents. The evidence suggests that a mechanism driving the allocation of this program is an infrastructure-for-money mechanism.

Chapter 1

Political Time Horizons and Government Investments: Evidence from Mexico

1.1 Introduction

Improving the capacity to deliver public goods is a salient issue, particularly in the developing world (De Janvry and Dethier, 2012; Azulai et al., 2014; Page and Pande, 2018; Bandiera et al., 2019), and vast resources by development organizations are devoted to it (World Bank, 2017; Department for International Development, 2019). However, a key barrier that many developing countries face is that policies aiming to expand this capacity involve making forward-looking investments with uncertainty (Besley and Persson, 2009). In democracies, these investments are made by politicians, who typically are in power for a limited amount of time and might, therefore, focus on policies with more immediate returns. As a result, there might be underinvestments in important areas of the state, such as public security, infrastructure, and environmental services.

The extent to which time horizons affect this incentive problem has become a central question in the political economy and economic development literature: Are politicians induced to make investments to expand the capacity to provide public goods when the expected length of time they might be in power changes? There are two key mechanisms that have been studied in the theoretical literature in parallel and explain how increasing political time horizons affects these

types of investments. First, longer horizons increase the benefits of forward-looking investments, because with some probability politicians will inherit those policies that affect their future payoffs (Svensson, 1998; Acemoglu, 2005; Besley and Persson, 2009, 2010, 2011).[1] Second, extending the expected horizon of politicians increases their incentives to appear competent to voters, because they may care about re-election (Barro, 1973; Ferejohn, 1986; Besley and Case, 1995; Smart and Sturm, 2013).[2]

In this paper, I document a causal link between political time horizons and investments in the capacity to provide public goods and I uncover the underlying mechanisms. The ideal empirical setting would have an exogenous policy change that expands the amount of time some politicians can be in office but not others. I leverage a policy reform in Mexico that parallels this ideal by (i) removing a ban on the consecutive re-election of local politicians (ii) in the context of staggered local elections. The policy change and the staggered elections imply that the reform extended the amount of time mayors could be in office differentially, depending on the year in which they were elected. I exploit this feature as quasi-experimental variation in political time horizons using a difference-in-differences design.

The key functions of municipal governments in this setting are to provide public security and environmental services.[3] To answer the research question, I build a rich set of outcomes that measures the inputs used to provide these goods and services, such as the size of the public administration, the professionalization of the municipal police, and the stock of public capital. Additionally, I focus on the sources of revenue and the composition of expenditures, levels of corruption, and quality of public goods. By looking at this rich set of outcomes, I provide a thorough understanding of the effects of extending political time horizons on investments in the inputs and on the outputs of municipal governments.[4]

[1] A similar mechanism has been discussed in the models of public debt of Persson and Svensson (1989) and Alesina and Tabellini (1990), who show that governments overborrow because they don't internalize the costs of debt in terms of future spending.

[2] See Persson and Tabellini (2000) and Besley (2006) for reviews of political agency models that consider elections as an incentive mechanism that disciplines the behavior of politicians.

[3] In Mexico, public education and public health are provided by the federal and state governments.

[4] Following recent work, my definition of capacity goes beyond the ability of governments to raise revenue and focuses on the ability of governments to deliver public goods. This definition is grounded on the framework of Fukuyama (2013). The concept of "state capacity" originally referred to the ability of the state to raise revenue (see, for example, Tilly, 1985), but as Francis Fukuyama argues, "states perform a whole variety of functions, any of which can be used

I find that mayors affected by the reform expand the capacity to provide public goods and services. First, I find that municipalities with mayors facing longer horizons expand the size of the public administration and improve its human capital. I document an increase in the number of municipal employees by 8.5%, an increase in the recruitment of female police officers by 25%, and an increase in the number of police officers with higher education (undergraduate or graduate degrees) by 24%. Second, I find that municipalities with mayors facing longer horizons increase the public capital in key areas of responsibility of the local governments. They report 7% more police stations, 34% more streetlight posts, and 6.2% more waste disposal vehicles. I construct z-score indices to aggregate the treatment effects of the different measures of capacity and I find that the human capital increases by 0.07 standard deviations, the public capital by 0.1 standard deviations, and the overall capacity of the municipal governments increases by 0.1 standard deviations. These results are economically meaningful, statistically significant, and robust to various econometric specifications.

Whether this is a net gain to citizens is not assured, though, since these extra inputs have to be paid for. The investments can come either from increased borrowing—hence, increased taxes or lower service provision in the future—or increased taxes, or increased transfers from the federal government. I find that the expansions in the municipal inputs are not debt financed. In particular, the results show that municipalities with mayors facing longer horizons borrow significantly less than municipalities with mayors with shorter horizons. This finding is consistent with previous theoretical work emphasizing the role of political horizons on public debt accumulation, and provides further evidence that longer political horizons affect long-term policies by decreasing moral hazard.[5] On average, the fiscal deficit of treated municipalities is 126 pesos per capita smaller than the fiscal deficit of control municipalities, off of a prereform mean of 213 pesos per

as a proxy for state capacity as a whole." The theoretical literature has focused on investments in legal and fiscal infrastructure (Svensson, 1998; Besley and Persson, 2009, 2010, 2011) and in productive public goods, such as "roads, the state of the infrastructure, legal rules for contract enforcement, etc." (Acemoglu, 2005). The empirical literature has used broader measures of capacity, such as the number of municipal employees and local-state agencies in Colombia (Acemoglu et al., 2015), the number of post offices in the US (Acemoglu et al., 2016), biometrically authenticated payments infrastructure in India (Muralidharan et al., 2016), distance to the municipal headquarters in Mexico (Fergusson et al., 2020), and the supply of government health workers and basic health services in Uganda (Deserrano et al., 2020).

[5] In Persson and Svensson (1989) and Alesina and Tabellini (1990), a government that is guaranteed re-election fully internalizes the costs of debt in terms of future spending and does not overborrow. Following a similar line of thought, Besley and Case (1995) emphasize that the equilibrium debt with a one-term limit is higher than the equilibrium debt without term limits.

capita. Moreover, I find that treated municipalities collect, on average, 49 pesos per capita more property taxes than control municipalities, off of a prereform mean of 171 pesos per capita. There are no significant effects of the reform on other local revenue or on transfers from upper layers of government, which is consistent with the institutional fact that these transfers are mostly formula based. Overall, I find no significant changes in the total revenue of municipalities with treated mayors and in the composition of the reported expenditures.

These results combined raise an interesting question: How are mayors with longer political time horizons able to expand the capacity to provide public goods? The results show that while the overall revenue and the composition of reported expenditures do not change, mayors with longer horizons have less corrupt practices. In particular, in line with previous work on electoral accountability and corruption (Ferraz and Finan, 2011), I find that mayors with longer horizons misappropriate less public funds. I gathered official audit reports of municipal governments conducted by the Mexican Federal Auditor's Office (ASF) and constructed an objective measure of misappropriation of public funds. Specifically, I consider the fraction of audited resources that were found to be misspent. The results show that mayors with longer horizons misappropriate, on average, 11.5 percentage points less resources than control mayors, off of a prereform mean of 16%. This estimate is economically meaningful, statistically significant, and robust to various econometric specifications and estimation strategies. A back-of-the-envelope calculation suggests that treated mayors misappropriate, on average, USD 606,286 (4.3% of total income) less earmarked public funds than control mayors. While the reported composition of expenditures does not change, municipalities with treated mayors are able to expand their capacity to provide public goods partly because their mayors misappropriate less public funds.

I provide two pieces of evidence that suggest that the expansion in the municipal government's capacity improves the quality of public goods delivered. First, I focus on the quality of public security, which is the main public service delivered by local governments in my setting and has been the focus of local and national policies in the last two decades. The results indicate that while crime and violence do not increase, criminal investigations of crimes in the jurisdiction of the municipal police increase by 17% in municipalities with treated mayors. This result is

economically meaningful, especially due to the prevalence of crime and low number of criminal investigations given the low arrest rates and difficulties in the apprehension of suspects in Mexico (México Evalúa, 2012). Second, I find that the night light luminosity of municipalities with treated mayors increases by 11%. This finding is consistent with the positive treatment effects in the number of streetlight posts.

There are two key mechanisms in play, the first one being the higher incentives to internalize the future benefits of investments, and the second one being the electoral incentives. Most of the empirical literature on term limits has focused on the latter, and less attention has been put on the former. To try to parse out these mechanisms, I exploit the prevalence of strongholds in my setting. Intuitively, in places where re-election is more plausible, the longer horizon effect might be more salient. In contrast, in more competitive places incumbents might face higher electoral incentives to choose public policies that satisfy voters. I look at heterogeneous average treatment effects on the treated by strongholds, and the results show that the effects of the reform on long-lasting policies are more pronounced in these places. In particular, treated mayors in strongholds invest 0.09σ more in human and public capital and borrow 57% less than treated mayors in more competitive places. The results also show that mayors in strongholds misappropriate less resources, although this result is less precisely estimated. While strongholds may correlate with the outcomes in other ways, these results suggest that the longer horizon effect is a key determinant of long-lasting policies.

Finally, the available evidence suggests that the effects of the reform on the main outcomes of interest are not driven by the selection of politicians. A reform that extends the political horizon of mayors increases the value of holding office, and thereby may attract a different pool of candidates.[6] I find that the reform is associated with changes in some observable characteristics of mayors, with a significant increase in female representation. To understand the extent to which these changes explain the main results, I conduct a series of additional analyses. First, I find that conditional on treatment, the gender of mayors is not associated with the main outcomes. Second, I reestimate the treatment effects on a sample of mayors who selected into politics before the

[6]See Smart and Sturm (2013) for a model that considers how the introduction of term limits reduces the value of holding office differentially depending on the types of politicians.

reform. Intuitively, for this set of mayors the benefits of entering politics with a one-term limit are higher than the costs. Thus, in principle, the reform should not have affected their decision to select into politics. The results show that even when restricting the estimates to this sample of mayors, the results remain practically unchanged. Therefore, the available results suggest that the treatment effects are primarily driven by a decrease in moral hazard, meaning that the same politician changes behavior when provided with longer horizons.

The findings presented in this paper contribute to the literature that studies the political determinants of economic development. Earlier work laid out theoretical frameworks to think about the role of political time horizons on economic development (Svensson, 1998; Acemoglu, 2005; Besley and Persson, 2009, 2010, 2011). More recently, Page and Pande (2018) and Bandiera et al. (2019) have emphasized the importance of political agency for state effectiveness, underscoring the role that the incentives of political leaders play in state building and economic development. There have been important advances to understand empirically the role of government horizons on public investments (Rauch, 1994; George, 2020; Yamasaki, 2020), and recent work finds that political competition and opportunistic politicians affect investments in state capacity and in service delivery (Fergusson et al., 2020; Henn et al., 2020). I contribute to this line of work by documenting a clear causal link between political time horizons and investments in service delivery and by uncovering the underlying mechanisms. Furthermore, I provide a simple framework to think about how the incentives to invest in the ability to provide public goods change when politicians are provided with longer political time horizons.

Finally, this paper also contributes to the important literature studying the effects of elections on politicians' behavior. Previous theoretical work has studied the effects of the introduction of term limits on politicians' behavior and voters' welfare (see, for example, Smart and Sturm, 2013; Aghion and Jackson, 2016), with most of the empirical evidence relying on variation from the introduction of gubernatorial term limits in US states (Besley and Case, 1995; List and Sturm, 2006; Alt et al., 2011). Most of the empirical evidence outside the US typically compares outcomes of first-term mayors with re-election possibilities against outcomes of second-term mayors with binding term limits (Ferraz and Finan, 2011; De Janvry et al., 2012; Klein and Sakurai, 2015). The

findings presented in this paper contribute to this line of work in three important ways. First, unlike the previous studies, I compare outcomes of first-term mayors with re-election possibilities against outcomes of first-term mayors without re-election possibilities.[7] This strategy allows me to deal with the empirical concern of on-the-job training and selection of able politicians without imposing structural assumptions (Aruoba et al., 2019).[8] Second, I show that long-lasting policies are driven by the prospects of tenure in office. This relates to the work by Dal Bó and Rossi (2011) and Titiunik (2016), who show that tenure in office affects the behavior of legislators in Argentina and the US, respectively. Third, I investigate how removing term limits is associated with changes in observed characteristics of mayors and the extent to which these changes mediate public policy.

The rest of this paper is structured as follows. Section 2 provides a description of the theoretical framework. Section 3 describes the institutional background. Section 4 describes the data. Section 5 provides the description of the empirical strategy. Sections 6 and 7 present the results. Section 8 concludes.

1.2 Theoretical Framework

A reform that removes a ban on re-election changes the incentives of individual politicians in at least two ways. First, it increases the benefits of internalizing the future, because with some probability long-term policies will affect politicians' future payoffs (Persson and Svensson, 1989; Alesina and Tabellini, 1990; Svensson, 1998; Acemoglu, 2005; Besley and Persson, 2009). Second, it gives them the incentives to appear competent to voters to increase their probability of staying in office (Barro, 1973; Ferejohn, 1986; Besley and Case, 1995; Smart and Sturm, 2013).

To illustrate the role that political time horizons play on the choice of long-term policies with future rewards, Appendix 1.A lays out a two-period career concerns model with government investments, where the role of elections is to select competent politicians. As is standard in this

[7] In independent and contemporaneous work, Motolinia (fc) studies the effects of the electoral reform in Mexico on the speech of state legislators. She finds that treated legislators increase a measure of particularistic speech.

[8] Disentangling the disciplining effect of elections from the selection of able politicians has been one of the main focuses of most of the empirical work on term limits (see, for great examples, Alt et al., 2011; Ferraz and Finan, 2011). Aruoba et al. (2019) use a structural approach to disentangle the disciplining effect of elections from the selection of able politicians. Their work complements the reduced-form studies and provides additional evidence for the idea that elections discipline the behavior of politicians.

type of career concerns agency models, an agent (politician) cares about maximizing the principal's (citizens) perception of his ability (Holmström, 1999). To do that, the incumbent chooses public policy to influence citizens' perception of his ability (signal jamming). However, because some policies are long-lasting, he also cares about how these policies affect his expected future payoffs. As a result, the incumbent uses (exogenous) resources to invest and expand the capacity to deliver public goods not only because investments affect his probability of re-election, but also because investments affect his expected future rewards. This captures a key idea emphasized in the framework of Besley and Persson (2009, 2011): rulers invest in the government when they internalize the future rewards of the investments. The difference here, however, is that government investments also affect the probability of staying in office.

I compare the predictions of this model (long horizon) with the predictions of the model without re-election possibilities (short horizon). There are two main results that guide the empirical analysis. First, when the incumbent has a longer horizon, investments are higher relative to the behavior observed when the incumbent has a shorter horizon. This result is directly mapped to the empirical analysis, and the framework shows that there are two main mechanisms driving this behavior: (i) longer horizon effect and (ii) electoral incentives effect. That is, when the horizon of mayors is extended, investments should be higher, because mayors can be re-elected and might, therefore, be in office to reap the benefits. But since investments also affect voters' perception of mayors' ability, mayors also have the incentives to please voters in the current period. Second, the framework predicts that with longer horizons, private rents should be lower. This captures a key idea in political agency models: elections discipline the corrupt behavior of politicians (Persson and Tabellini, 2000; Besley, 2006). In the empirical section, I provide evidence on these predictions.

To further inform about mechanisms, the framework models strongholds as places where citizens have ideology preferences for the incumbents. As a result, in equilibrium the higher the ideology preferences for the incumbent, the more likely he will get re-elected. The framework predicts that when an incumbent with longer horizons is more likely to stay in office due to citizens' preferences, he invests more, because his perceived probability of receiving future rewards from the investments is higher. This result relates to the key idea that political stability is an important

mechanism driving policies with future costs and rewards (Persson and Svensson, 1989; Alesina and Tabellini, 1990; Svensson, 1998; Acemoglu, 2005; Besley and Persson, 2009, 2010, 2011), and shows that the "longer horizon effect" is more pronounced when the probability of remaining in office is high. I use this result in the empirical section to explore mechanisms.

This framework captures the key incentives in my setting and delivers some predictions that guide the empirical analysis, but naturally comes with a few caveats that I can address. First, here I do not consider the role of political parties, who may have longer time horizons than individual politicians. However, whether the time horizon of political parties matters in the same way as the time horizon of individual politicians is an empirical question (Besley and Case, 1995), which I explore in this paper.[9] Second, I do not consider how parties select politicians, which may imply heterogeneity in the types of politicians across parties. The empirical section addresses this issue and compares policies of short-horizon politicians against policies of long-horizon politicians within the same party.

1.3 Institutional Background

In this section, I describe the institutional setting. I briefly explain the structure of local governments in Mexico and their role in public goods provision. Then, I explain the reform and the staggered elections used in the empirical analysis.

1.3.1 Municipalities

Municipalities are the lowest level of administrative division in Mexico. They are administered by a municipal government (*Ayuntamiento*), which is divided into two bodies, the local public administration and the municipal council, both of which are led by a mayor who is elected by majority rule, typically every three years. The mayor is the highest authority of the municipal government. He has the fiscal authority to collect revenue and authorize expenditures, he is the commander in chief of the municipal police, and he is in charge of designing and implementing

[9]This argument also goes in line with Motolinia (fc), who shows that state legislators change their behavior as a result of the reform studied in this paper.

the Municipal Development Plan, which sets the goals for development in the locality and the strategies of the local government to achieve these goals (INAFED, 2004; SEDESOL, 2010b).

The local public administration has an organizational structure that can be modified by mayors, depending on their goals and policy platforms (SEDESOL, 2010a). In particular, mayors have discretion to modify the size of the public administration, including the number of directors and the number of municipal employees. However, while there is discretion regarding the organizational structure of the public administration, public workers are protected by law and wrongful dismissals represent a financial burden for local governments. Appendix 1.C shows evidence that there exist firing costs for municipal governments. Moreover, it shows that bureaucratic jobs tend to be stickier when the incumbent party wins the election.[10]

Before taking office, mayors are advised to analyze and design the organizational structure of the public administration according to their goals. Panel (a) of Figure 1.B.1 in Appendix 1.B plots the distribution of the number of directors in the local public administrations, and Panel (b) of Figure 1.B.1 in Appendix 1.B plots the distribution of the number of municipal employees. A typical public administration has 16 directors and 147 municipal employees per 10,000 inhabitants. Based on a municipal government census conducted by the National Institute of Statistics (INEGI), in 2010, the most common departments were the department of finance and the department of the *Ayuntamiento* (97.4%), followed by the department of the integral development of the family (83.2%) and the department of public security (81%).

The Mexican Constitution mandates that municipal governments are responsible for providing public security, drinking water and sewerage, street lighting, collection and final disposal of waste, public markets, cemeteries, slaughterhouses, and streets, parks, and their amenities.[11] Of these goods and services, public security is one of the most important, not only because 81% of mayors have a public security director in their public administration, but also because this service is the one citizens care the most about. According to a nationally representative survey on victimization and perceptions of public security (ENVIPE), from 2011 to 2018 public security was

[10] This evidence is consistent with recent work on Brazil. Akhtari et al. (2020) show that bureaucratic turnover in Brazil is smaller when the incumbent party wins the election.

[11] For more details about the responsibilities of the municipal government, refer to Article 115 of the Mexican Constitution at `http://www.diputados.gob.mx/LeyesBiblio/ref/cpeum.htm`.

ranked as the most important topic citizens were worried about (59.4% of citizens), followed by unemployment (43.2%) and increases in prices (34.2%). For a comparison, only 21.8% and 27.2% of citizens were worried about education and health, respectively. Finally, unlike other settings, in Mexico public education and public health are not provided by municipal governments. Instead, these services are provided by the federal or state governments.

From the revenue side, the Mexican Constitution gives municipal governments the authority to collect and manage their own resources, such as local taxes and other fees and tariffs. The most common tax in municipalities is the property tax, which accounts for 84% of the local taxes in municipalities. They also charge fees and tariffs for waste disposal and trash removal and for providing piped water, among other services. Municipalities can also borrow from traditional financial intermediaries, the bond market, and other national institutions such as suppliers. Finally, municipal governments also receive earmarked and unmarked transfers from the upper layers of government that are mostly formula based and regulated by law.[12] On average, 84% of the resources of municipal governments come from these transfers.

1.3.2 The reform and staggered elections

The 2014 reform. The research design of this paper exploits two institutional features of the Mexican institutions that date back to the 1910 Mexican Revolution and the revolts that followed. First, as a result of these events, institutions in Mexico were designed to limit re-election of elected politicians in all levels of government. Since 1933, elected mayors have been restricted to one term and for 81 years incumbents were not able to run for consecutive re-election.[13] In fact, the re-election of incumbent politicians has seemed to be unpopular among voters even in more recent years. Based on a representative survey of political attitudes and political opinions conducted by the Secretary of Government, in 2008 and in 2012, 64% and 68% of the respondents, respectively, were against the re-election of mayors. However, with the goal of improving accountability and

[12] These transfers are commonly known as *Ramo 28* and *Ramo 33*. Some municipal governments also receive discretionary earmarked transfers typically known as *Ramo 23*. These transfers are assigned at the discretion of the federal government and depend on the negotiation skills of the mayor, among other factors. See footnote 20 for more details.

[13] Mayors were allowed to run for another nonconsecutive term. However, based on a municipal government survey conducted by the Secretary of Social Development (SEDESOL), in 2000 only 7.21% of the mayors had been in office before.

the public administration and fostering projects with long-term objectives, in February of 2014 the Congress amended the constitution and removed the ban on consecutive re-election.

The constitutional amendment established that mayors and members of the council who were elected in 2015 or after could run for re-election for an additional three-year term.[14] The constitutional amendment allowed consecutive re-election only if the term was three years or less, the candidates ran under the same political party, and if the political party approved their candidacy.[15] Importantly, the reform affected candidates elected in 2015 or after, and not those who were elected before that year.[16]

Staggered elections. The second institutional feature I exploit in this paper refers to the decentralized electoral system. In Mexico, each state is in charge of managing local elections under the guidelines of the federal electoral authority. The decentralization of elections at the subnational level dates back to the Federal Electoral Law of 1946, and over time it has created a natural staggered design of local elections.[17]

Panel (a) of Figure 1.1 shows the electoral cycles for each state since 1999. The bars represent the number of states with municipal elections over time. Panel (b) shows the geographic variation of local elections after the reform. Municipalities in states with elections in 2015 or after became treated. The staggered design of local elections together with the electoral reform of 2014 provide a unique natural experiment that allows me to compare outcomes of first-term mayors with re-election possibilities against outcomes of first-term mayors without re-election possibilities. While other countries have experienced similar reforms that changed the term limits of local politicians (see Table 1.1), Mexico is a good laboratory to empirically answer the research question, because it allows me to combine the electoral reform of 2014 with the staggered design of local elections, which implies that the reform extended term limits differentially, depending on

[14]The reform also removed the ban on consecutive re-election for federal and state legislators. See Motolinia (fc) for an evaluation of the reform on the speech of state legislators.

[15]For more details about the requirements to run for re-election, see Article 115 of the Mexican Constitution at `http://www.diputados.gob.mx/LeyesBiblio/ref/cpeum.htm`.

[16]Municipalities in the states of Hidalgo and Tlaxcala do not become treated, because their legislatures did not incorporate consecutive re-election during the studied period (Magar, 2017). This group of municipalities acts as a pure control group in the empirical design.

[17]The reform of 2014 centralized the management of the state electoral authorities. However, each state is still in charge of holding elections under the guidelines of the federal electoral authority. Specifically, local authorities must follow the The General Law for Electoral Institutions and Procedures and The General Law for Political Parties (Serra, 2015).

the election cycle of the municipalities.

Discussion. Some states changed their electoral cycles before the reform was enacted. This could be a concern for the empirical analysis, if these changes were made in anticipation of the reform. However, this is unlikely for the following reasons. First, local elections follow the election cycles described above, and for the states that changed their cycle, they did it one or two elections before the reform started to be discussed. Specifically, the changes in some electoral cycles were made during the presidential term of Felipe Calderón from the PAN, while the reform was discussed in 2013 and enacted in 2014, both during the presidential term of Enrique Peña Nieto from the PRI (Serra, 2015). For example, the state of Chiapas changed its cycle in the 2010 elections, after the state governor decided to align the local elections with the federal cycle with a state reform implemented in 2009. Second, the constitutional amendment defined dates based on the federal election cycle. The federal election cycle dates back to 1934, and since then, presidential elections have been held every six years and elections for members of congress every three years. Hence, 2015 was predicted to be a federal election year even in the absence of the 2014 reform. Finally, the results are robust to excluding from the analysis municipalities in each state one at a time.

1.4 Data

The data used in this paper come from several government agencies. In this section, I describe the main features of the data and in Appendix 1.E I provide a more detailed description of the sources of the data, the cleaning procedures, and the sample of interest. The estimation sample excludes municipalities in the state of Oaxaca, because in this state the majority of the local leaders are appointed through customs and traditions. I also exclude the 16 territorial demarcations of Mexico City, which started electing mayors in 2018. Overall, I consider all of the available data of the 1,872 municipalities in the remaining 30 states of Mexico.

Human and public capital. The data on measures of the capacity to provide public goods come from biannual municipal government censuses conducted by INEGI since 2011, with information referring to the previous calendar year. These censuses report data about the municipal

governments' employees and public capital in areas related to the public administration, public security, municipal justice, piped water and sewerage, and waste disposal. Using this information, I construct measures of the capacity to provide public goods following two criteria. First, the outcomes needed to be observed before and after the reform. Second, the variables needed to be measures of human or public capital related to the main functions of municipal governments.

Following Acemoglu et al. (2015), I begin by focusing on human capital. I consider the number of directors, the total number of public workers and police officers, and the professionalization of the municipal police. These measures of human capital are important inputs for the delivery of public goods. On average, the public administrations of municipal governments have 16 directors and 147 municipality employees per 10,000 inhabitants (see Table 1.2). Moreover, on average, there are 21 police officers per 10,000 inhabitants, of whom 9% are female and 97% have basic education (less than an undergraduate degree).[18]

Then, I focus on measures of public capital. In terms of public security, I look at the number of police stations and the number of jail cells in the municipalities. On average, municipalities have 1.9 police stations and 2.6 jail cells per 10,000 inhabitants (Table 1.2). The censuses also contain information on the number of streetlight posts in the municipality for the years 2014 and 2016, before and after the implementation of the reform. I use this information to measure the public capital used to provide street lighting. I complement this information with the night light intensity in the municipality, using data from the VIIRS satellite from 2014 onward and from the DMSP satellite for years before that. On average, municipalities have 1061 streetlight posts per 10,000 inhabitants, although the distribution of this variable has a long right tail (Table 1.2).

Finally, the municipal censuses allow me to construct measures of the public capital used to provide piped water, sewerage, and waste disposal services. I identified the variables that can be followed over time and that are related to the infrastructure of the municipality used to provide these public services. These variables are the number of house connections to piped water, a dummy variable indicating whether the municipal government provides sewerage or not, and a dummy variable indicating whether the municipal government provides some chemical treatment of the sewerage water. On average, 90% of the municipalities have sewerage and 38% treat the

[18]Information about the education of police officers is only available for the years 2012, 2014, and 2016.

sewage water. I also identified a set of variables related to the ability of the municipal government to provide waste disposal services. Because waste disposal service is almost universal (99% of municipal governments provide this service), I focus on expansions in the capacity to provide this public service in the intensive margin. Specifically, I focus on the fleet of waste disposal vehicles in the municipality. On average, municipal governments have 2 waste disposal vehicles per 10,000 inhabitants.

To aggregate the information of the continuous variables into single variables, I construct equally weighted z-score indices of human and public capital following Kling et al. (2007).[19] The main idea of this approach is to aggregate the information over the multiple effects of the reform by standardizing the outcomes and averaging them. Estimates for each individual effect and for the aggregated indices are presented when discussing the results.

Public finances. Data on public finance outcomes come from the yearly balance sheets reported by each municipality to INEGI. These contain information about the main sources of revenue, such as local taxes, fees and tariffs, transfers from upper layers of government, and fiscal deficit. The summary statistics of these variables are presented in Table 1.2. Municipalities collect taxes mainly through property taxes. On average, taxes account for 4% of the annual income of a municipality, and property taxes are 84% of this revenue. They also collect local revenue through fees and tariffs. This revenue comes from fees and tariffs on some of the services provided by the municipal government or from the rents of public property. This source of revenue accounts for 5% of the total income. The bulk of the revenue of municipalities, however, are transfers from the upper layers of government. These are earmarked and unmarked transfers, and account for 85% of the total revenue of the municipality.[20] Finally, municipalities borrow from traditional financial intermediaries, the bond market, and from suppliers.[21] Typically, fiscal deficit is 4% of the annual expenditures of the municipality. The remaining revenue comes from other sources.

[19] Kling et al. (2007) consider the mean of the control group. Because I use a difference-in-differences strategy (i.e., the levels of the outcomes can be different across treatment groups), I consider the mean for all observations.

[20] These transfers are typically known as *Ramo 28* and *Ramo 33*. They are formula based and depend on the population, GDP, and the previous years' local revenue collection. Municipalities also receive discretionary transfers from the federal government, commonly known as *Ramo 23*. These are earmarked transfers, they are not formula based, and they are assigned at the discretion of the Congress. The data from INEGI do not distinguish between formula-based earmarked transfers and discretionary earmarked transfers, and this was formally verified through conversations with officials from INEGI.

[21] For instance, municipalities can borrow from firms that have procurement contracts with the local administration.

The public finance data from INEGI also report information about the expenditures of the municipality. These data are aggregated into the following categories: wages, supplies, services, subsidies and transfers, assets, public investments, and other expenditures. While these data are useful to understand the aggregated categories of expenditures, they are not consistent across municipalities for specific subcategories of expenditures. For example, in 2017 only 4% of the municipalities reported expenditures on wages of police officers. The remaining municipalities reported expenditures on wages with a higher level of aggregation. For this reason, in the empirical analysis I analyze the aggregated categories of expenditures. Finally, the data on local public finances from INEGI are only available for 85.43% of the municipalities in my sample.[22] However, in the Data Appendix 1.E I show that reporting does not vary differentially by timing of adoption of treatment.

Municipal audits. The Municipal Fund for Social Infrastructure (FISM), which is part of the earmarked federal transfers described above, is subject to audits by the independent Mexican Federal Auditor's Office (ASF). Each year, the ASF selects a group of municipalities to audit their use of FISM transfers during the previous fiscal year. The ASF selects municipalities based on their size, the financial importance of the FISM relative to the municipality's budget, whether the municipality has been audited before, and whether other audits are being undertaken in the same municipality, among other factors (Larreguy et al., 2020).[23] While these criteria are not random, in the results section I show that the probability of being selected for an audit does not vary differentially by treatment status. Moreover, the results remain unchanged when I control for characteristics of the selection criteria. Figure 1.B.2 in Appendix 1.B plots the number of audits by fiscal year. During the studied period, the number of audits conducted by the ASF ranged from 144 to 367.

The audit verifies that the FISM funds were used for the earmarked purposes, that the public infrastructure exists, that the municipality performs adequate accounting of the funds, whether the funds were transferred to other accounts, and that the municipality reports the use of the funds

[22] According to my conversations with officials from INEGI, the reasons why some municipalities don't report this information may vary across and within states and across years.

[23] The criteria to audit municipalities may vary from year to year. For example, in 2014 the ASF decided to coordinate with the states' Auditor's Offices (ESFL) to audit the 2013 fiscal year expenditures of additional municipalities.

to the federal government, among other criteria defined by the ASF. Based on these guidelines, the audit identifies the amount of FISM funds that does not satisfy these criteria, that generated probable damage to the federal public treasury, and that should be reimbursed to the federal treasury (De La O and García, 2015). Using this amount, I construct an objective measure of malfeasance spending as the fraction of recovered funds from the audited FISM transfers that were found to be misspent. These data are obtained from the annual reports of the ASF to Congress, and are publicly available on the official website of the ASF. Figure 1.E.2 in the Data Appendix 1.E shows an example of these reports. Table 1.2 shows summary statistics for this measure of malfeasance spending during the studied sample period, and Panel (b) of Figure 1.B.2 in Appendix 1.B plots the density of this measure. On average, 13% of the audited FISM funds is misspent. I use this objective measure of malfeasance spending to explore the effects of the reform on misappropriation of public funds.

Electoral and candidates data. Election dates come from the National Electoral Authority (INE). The party vote shares and political parties come from the electoral authorities of each state. Table 1.E.1 in Appendix 1.E lists the states and elections for which the election results data are available (99 elections$\times$state in total). Mayoral terms and the gender of the incumbents are obtained from the National System of Municipal Information (SNIM), which is a government agency that belongs to the Secretary of Government. Finally, I obtained the names of the candidates for mayor from the electoral authorities of each state. Using their names, I classified their gender through a classification algorithm.[24] Data on election results are available for the 30 states of my sample, whereas data on the names of candidates for mayor are available for 28 states of my sample.

Finally, I obtained other characteristics of the elected mayors from the municipal government censuses from INEGI for the years 2010, 2012, 2014, and 2016. These censuses ask local governments for information about mayors' education and their previous employment. I use the reported characteristic in each one of these calendar years and construct a data set at the municipality$\times$election level. This data set contains information about whether the mayor has

[24]To classify the gender of the candidates, I use the list of female and male elected mayors reported by SNIM. Some states reported the gender of the candidates for some election years. Using these data, I evaluate the classification error of my algorithm and find that it classifies the appropriate gender of the candidate in more than 98% of the cases.

basic education, an undergraduate or a graduate degree, and whether before being elected as a mayor, the candidate worked in another government agency, in politics, in the private sector, as a business owner, in another job, or whether it is his first job. The data for the years 2012, 2014, and 2016 are de-identified at the municipality level and I can only observe the state identifier.

1.5 Empirical Strategy

The analysis presented in this paper exploits the 2014 electoral reform and the staggered municipal elections. These two institutional features combined provide unique quasi-experimental variation in politicians' time horizons, because they extend the expected number of terms a mayor can be in office differentially, depending on whether they were elected before or after the reform. Specifically, mayors elected before the reform are term limited, whereas mayors elected in 2015 or after have a longer political time horizon.[25] I compare these two groups of municipalities before and after the reform using a difference-in-differences strategy. A nice feature of this strategy is that unlike previous work on term limits outside the US, it allows me to deal with the empirical concern of experience and selection of able politicians by comparing first-term mayors with two-term horizons against first-term mayors with one-term horizons. Figure 1.2 presents a simplified version of the empirical strategy. The treated group is formed by those municipalities with mayors elected in 2015 and who take office in or before 2016, and so on. The treated group is compared against municipalities with mayors elected before 2015.

Formally, the main analysis estimates the following regression model:

$$y_{ipt} = \lambda_i + \delta_t + \theta_p + \beta \cdot Treated_i \cdot Post_t + \sum_j \alpha_{jk} \cdot \mathbf{1}\{j = t\} \cdot X_i' + W_{it}\Gamma + \varepsilon_{ipt} \tag{1.1}$$

where y_{ipt} is an outcome of interest for municipality i, with mayor of party p, during calendar year t. The parameters λ_i are municipality fixed effects that control for fixed characteristics of the municipalities across years, whereas the parameters δ_t are calendar year fixed effects that control for common shocks within each year. The parameters θ_p are political party fixed effects that control

[25] Mayors of the municipalities in the states of Hidalgo and Tlaxcala, who had elections in 2016, did not become treated during the studied period because the state legislatures did not incorporate re-election.

for fixed characteristics of the political parties, such as their ideology or their preferences for types of politicians. $Treated_i$ takes the value of one if the mayor in municipality i during period t was elected in 2015—hence, started his first year in office in 2016—and zero otherwise. $Post_t$ takes the value of one if the calendar year t is 2016 and zero otherwise. X_i is a vector of K predetermined municipality characteristics that are interacted with time dummies, and W_{it} is a vector of time-varying characteristics, such as the mayor's margin of victory and the state GDP. Finally, ε_{ipet} is an unobserved shock that can be arbitrarily correlated within each state, and due to the small number of clusters ($G = 30$ states) I also report wild bootstrap p-values following Cameron et al. (2008). Under the standard difference-in-differences assumptions, β identifies the average treatment effect on the treated (ATT) during the first year in office of increasing term limits on the outcome of interest. Given the data availability on the measures of capacity, for ease of interpretation the main analysis restricts the sample to the years 2010, 2012, 2014, and 2016. However, Appendix 1.B presents results that exploit the staggered implementation of the reform using an event-study framework for the outcomes for which data are available on a yearly basis (i.e., public finance data and malfeasance spending).

Validity of the research design. The validity of the research design rests on three assumptions. First, in the absence of the reform, the evolution of the trends of the outcome variables should have been the same both for the treated and the comparison groups. To test this assumption, I look for differences in the evolution of the outcomes before the implementation of the reform by estimating the following dynamic specification:

$$y_{ipt} = \lambda_i + \delta_t + \theta_p + \sum_{j \neq 2010} \beta_j \cdot \mathbf{1}\{j = t\} \cdot Treated_i + \sum_j \alpha_{jk} \cdot \mathbf{1}\{j = t\} \cdot X_i' + W_{it}\Gamma + u_{ipt} \qquad (1.2)$$

where the indices are as described above. $Treated_i$ is a dummy variable that takes the value of one if municipality i had elections in 2015, and zero otherwise, and it is interacted with year dummies. The parameters β_j are the conditional mean differences in the outcome variables between the treated and the comparison group, relative to the conditional mean difference in 2010. The first assumption is likely to hold if we fail to reject the null that $\beta_j = 0$ for $j = 2012, 2014$.

To further test this assumption, I estimate the following specification:

$$y_{ipt} = \lambda_i + \delta_t + \theta_p + \phi PreTreated_{it} + \beta \cdot Treated_i \cdot Post_t + \sum_j \alpha_{jk} \cdot \mathbf{1}\{j = t\} \cdot X_i' + u_{ipt} \qquad (1.3)$$

where the indices and the outcomes are the same as above. $PreTreated_{it}$ is a dummy variable that takes the value of one if municipality i becomes treated in 2016 and year $t \in [2013, 2015]$. The parameter of interest in this specification is ϕ, which captures the average difference in the outcome between treated and control municipalities during 2013–2015 with respect to the average difference in the rest of the pre-treatment years. Failing to reject the null hypothesis would provide further evidence that the parallel trends assumption holds. Table 1.B.1 in Appendix 1.B reports the estimates of ϕ for the main outcomes considered in the analysis. Of the 35 estimates reported in Table 1.B.1, one is statistically significant at the 95% level and another one is statistically significant at the 90% level. These differences in the pre-treatment means could have occurred by chance; thereby, I interpret this as evidence that the parallel trends assumption required for identification likely holds. I discuss the validity of this assumption in more detail when I present the results.

The second assumption required for identification is that there are no anticipatory responses to the reform. As discussed in Section 1.3.2, this is unlikely, since the election cycles were determined at least one election before the announcement of the reform. The date of implementation of the reform was decided based on the federal election cycle, which was determined in 1934, and it is unlikely that it was manipulated based on the local election cycles. Finally, the estimates are not sensitive to leaving out from the estimations municipalities from a specific state, which shows that changes in the election cycle before the reform (if any) in a particular state do not confound the main treatment effects.

Finally, the third assumption required in the research design is treatment effect homogeneity by timing of adoption. This assumption states that the treatment effect estimated for municipalities with elections in 2015 is similar to the treatment effect for municipalities with elections in 2016, and so on (de Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2020). In the main results section I present results for the 2x2 difference-in-differences design, and when data allow, in Appendix 1.B I exploit the staggered implementation of the reform and estimate treatment ef-

fects for more pretreatment and posttreatment years. The results that exploit the staggered entry into treatment rely on homogeneity of treatment effects (de Chaisemartin and d'Haultfoeuille, 2020; Sun and Abraham, 2020). In Appendix 1.D, I discuss in more detail this assumption and consider the interaction-weighted estimator (IW) of Sun and Abraham (2020), which is robust to treatment heterogeneity by timing of adoption.

On-the-job training. The parameter β of equation 1.1 identifies the average treatment effect on the treated during the first year in office. This holds if the difference between the outcomes of mayors in the first year in office (treated group) and the outcomes of mayors in the second and third year in office (control group) is the same before and after the reform. Under this assumption, the difference-in-differences strategy differences out the effects of comparing outcomes while in different years of their terms. To explore this, the parameters β_j of equation 1.2 are compared relative to the difference in 2010, when mayors in the treated and control groups were in the same years in office as in the postreform period (see Figure 1.2). Moreover, in Appendix 1.B I report estimates that consider year of term fixed effects, which account for the possibility that the evolution of the outcomes may vary, depending on the year in office of the mayor.[26]

1.6 Empirical Results

This section reports the main reduced-form effects of the reform on measures of the capacity to provide public goods, local public finances, and misappropriation of public funds. The unit of observation is the municipality$\times$year and the studied period spans from 2010 to 2016. For ease of presentation, the results presented in subsections 6.1–6.3 consider data of the years 2010, 2012, 2014, and 2016, which are the years with available information for the main measures of human and public capital of the municipal governments. When the data are available for more years (public finances and misappropriation of public funds), additional results that consider all years are presented in Appendix 1.B. Moreover, the estimates for each outcome consider all the reported data, and in Appendix 1.B I present results that restrict the estimations to a balanced sample.

[26]This empirical strategy requires several pretreatment years in order to identify the year of term fixed effects.

1.6.1 Effects on public goods capacity

Human capital. This subsection reports the main findings of the effects of the reform on measures of the human capital of municipal governments. The outcomes discussed here are important inputs for public goods provision, especially for public security, which, as discussed in Section 1.3.1, is the most important public good provided by municipal governments in my setting. I begin by providing additional evidence for the validity of the parallel trends assumption. Figure 1.3 plots the results of the estimation of equation 1.2 for the number of directors, public workers, and police officers by gender. The point estimates are compared relative to the difference in means in 2010, and for all the prereform estimates, I fail to reject the null hypothesis that $\beta_j = 0$ for $j = 2012, 2014$. This provides additional evidence for the validity of the parallel trends assumption for these outcomes.

Panel A of Table 1.3 reports the results of the estimates of equation 1.1 for these outcomes. Columns 1 and 2 report the estimates for the number of municipality employees. The results show that while the number of directors in treated municipalities does not increase (column 1), there is an economically meaningful and statistically significant increase in the number of public workers (column 2). Municipalities with mayors with longer horizons increase the number of public workers by 13.86, off of a prereform mean of 164 public workers per 10,000 inhabitants. Column 3 reports the estimate for the number of police officers per 10,000 inhabitants, and columns 4 and 5 show the results by gender of the police officers. There is no significant average treatment effect on the treated in the total number of police officers, although the point estimate is positive. Most of this estimate is driven by the effect of the reform on the number of female police officers (column 5). After the reform, treated mayors increase female police recruitment by 0.52 police officers, off of a prereform mean of 2.11 per 10,000 inhabitants. This effect is statistically significant at the 99% level and economically meaningful—on average, municipalities with treated mayors increase female police recruitment by 25%. Overall, the evidence indicates that treated mayors significantly expand the size of the public administration. Columns 6 and 7 look at the effects of the reform on the education of police officers. I find no statistically significant change in the recruitment of police officers with basic education (column 6). However, I find that municipalities with treated mayors

increase the recruitment of high educated (undergraduate and graduate degree) police officers by 0.14 per 10,000 inhabitants (24% of the prereform mean). This point estimate is highly statistically significant and indicates that as a result of the reform, there is a significant improvement in the professionalization of the municipal police.

Public capital. Panel B of Table 1.3 shows the effects of the reform on measures of the public capital of municipal governments. Columns 1 and 2 look at the effects on the number of police stations per 10,000 inhabitants and the number of jail cells per 10,000 inhabitants, respectively. The results indicate that there are significant investments in the policing infrastructure by mayors with longer horizons. Specifically, municipalities with treated mayors report 0.14 more police stations per 10,000 inhabitants relative to the comparison group. This effect is statistically significant at the 95% level and represents a relative increase in the number of police stations of 7%. There are also 0.25 more jail cells in municipalities with treated mayors, off of a prereform mean of 2.7 jail cells per 10,000 inhabitants, although this last result is less precisely estimated. Overall, the findings show that mayors with longer horizons significantly expand the public capital used to provide public security.

Municipal governments are also responsible for the provision of street lighting, piped water and sewerage, and waste disposal service. Column 3 looks at the effects of the reform on the number of streetlight posts in the municipality. The outcome variable has been transformed using the inverse hyperbolic sine function to deal with outliers. This function allows the transformation of the outcome of the 2% of the municipalities reporting zeros, while transforming the data in a similar way as the logarithm function. While I cannot test the parallel trends assumption with this outcome, because it is only reported for 2014 and 2016 (before and after the implementation of the reform), Figure 1.B.3 in Appendix 1.B plots estimates of the differences in means between the treated and the comparison group in the night light intensity from 1992 to 2013 using data from the DMSP satellite (available only for those years). The figure shows that from 1992 to 2013, there were no differences in the night light intensity between the treated and the comparison groups, providing some additional evidence that the parallel trends assumption holds. Moreover, Figure 1.B.4 uses data from the VIIRS satellite for the years 2014 onward, and shows the results for

an specification that considers the staggered adoption of treatment. The conclusion is similar: the parallel trends assumption is likely to hold for outcomes of street lighting. The result from Column 3 of Panel B shows that municipalities with mayors facing longer horizons report 33.5% more streetlight posts relative to the comparison municipalities, and this point estimate is statistically significant at the 90% level when clustering at the state level and at the 95% level when computing the t-statistic using wild bootstrap. Moreover, this evidence is consistent with an 11% increase in the night light intensity observed from satellite data (Figure 1.B.4).

Columns 4–6 of Panel B report the results for the number of house connections to piped water, the probability of providing sewerage in the municipality, and for a dummy variable indicating whether the municipal government treats the sewage water. The estimates are small and statistically insignificant. The result for sewerage, however, is consistent with the findings by McIntosh et al. (2018), who study the effects of a large public investment program on urban infrastructure in Mexican neighborhoods. They find that USD 68 million in spending in urban infrastructure has a small and statistically insignificant effect on access to sewerage in treated neighborhoods, which suggests that increasing this type of public infrastructure in this setting is hard and requires large amounts of resources. Finally, column 7 reports the estimate for the number of waste disposal vehicles.[27] The result shows that municipalities with treated mayors increase the fleet of waste disposal vehicles by 6.2%.

Panel C of Table 1.3 reports the results for the z-score indices that aggregate the treatment effects reported in Panels A and B. The human capital increases by 0.07σ (column 1), whereas the public capital increases by 0.1σ (column 2). Overall, an index that aggregates all the treatment effects increases by 0.1σ (column 3). This result is statistically significant at the 95% level and economically meaningful, considering that state development requires large investments and is fundamental for economic development (Besley and Persson, 2011). Table 1.B.2 in Appendix 1.B reports the results for the standardized outcomes that are used as inputs for the indices, and Table 1.B.3 reports robustness exercises to the indices using principal component analysis. The findings are robust to this aggregation criterion.

[27] The outcome variable has been transformed using the inverse hyperbolic sine function. Only 0.4% of the observations in the sample report zero in this outcome. Furthermore, the result is almost identical if I consider a logarithmic transformation.

1.6.2 Public finances

In this subsection I report the main findings of the effects of the reform on the main sources of revenue of the municipal governments. Table 1.4 reports the estimates of equation 1.1 and Figure 1.4 plots the estimates over time and their 95% confidence intervals for fiscal deficit and property taxes. Column 1 of Table 1.4 reports the estimate for fiscal deficit. The result shows that municipalities with mayors facing re-election report 126 pesos per capita less in annual fiscal deficit, off of a prereform mean of 213 pesos per capita. This result is consistent with seminal theories of political stability and the strategic use of public debt (Persson and Svensson, 1989; Alesina and Tabellini, 1990), and with Besley and Case (1995) who show that, in equilibrium, one-term politicians overborrow relative to politicians without term limits. The finding in Column 1 contributes empirically to these theories by showing that mayors with a positive probability of staying in office borrow significantly less than mayors who will leave office for sure. Another plausible interpretation of this result is the idea that voters are sophisticated, well-informed about the budget, and "fiscal conservatives," and punish the incumbent for large deficits (Drazen and Eslava, 2010). This may also induce mayors with re-election prospects to borrow less. Section 1.6.5 sheds some light on the plausibility of this mechanism.

Column 2 of Table 1.4 shows the effect of the reform on the amount of revenue collected through property taxes. The result shows that treated municipalities collect 49.2 pesos per capita more in property taxes, off of a prereform mean of 170.6 pesos per capita. Table 1.4 also reports the point estimates for transfers from the upper layers of government, both earmarked and unmarked. In both cases, the point estimates are small relative to the prereform mean and statistically indistinguishable from zero. This is consistent with the fact that these transfers are mostly formula based.[28] Overall, these results have important implications for fiscal policy, because they show that political time horizons are an important determinant of public debt accumulation and can affect local revenue collection. As a robustness test to these findings, Table 1.B.8 and Figure 1.B.5 in Appendix 1.B show estimates that exploit the staggered implementation of the reform using an event study framework. The implications of these estimates are similar to the ones discussed in

[28] For more details on the formulas of these transfers, see the Mexican Fiscal Coordination Law.

this section.

While the aggregate budget does not change, Table 1.5 explores the effects of the reform on the composition of aggregate measures of reported public expenditures. All outcomes are divided by the annual expenditures so the point estimates across columns add up to zero. The majority of the resources are spent on wages, supplies and services, and public investments. However, the results show no evidence that municipalities with treated mayors change the composition of reported public expenditures. These results combined raise an interesting question: How are treated mayors able to finance the extra human and public capital without changing the aggregate budget and the composition of reported expenditures? To answer this question, the next subsection explores the effects of the reform on misappropriation of public funds.

1.6.3 Misappropriation of public funds

Table 1.6 reports the effects of the reform on misspent resources. Because the audits are conducted on a subset of municipalities and not every municipality is audited more than once (50% of the audited municipalities in the studied sample were audited only once), the table reports estimates of equation 1.1 with state fixed effects instead of municipality fixed effects. Note, however, that the estimate of β is still unbiased, since treatment variation is at the state level. Columns 1–3 report the estimate for the probability that a treated municipality is audited. The point estimates are positive but statistically indistinguishable from zero. This suggests that the probability that the ASF selects a municipality for an audit does not vary differentially with treatment, verifying the neutrality of this institution for conducting audits.

Columns 4–7 report the ATT on the measure of misappropriation of public funds under different specifications and Figure 1.5 plots the estimates of equation 1.2 for this outcome. The estimate is stable across the different specifications, which consider political party fixed effects (column 5), municipality characteristics (column 6), and audit characteristics (column 7), which include a dummy variable indicating whether the municipality was audited during the previous fiscal year, the fraction of municipalities audited in the state, and the percentage of the FISM funds that were audited. The result from the preferred specification (column 7) shows that mu-

nicipalities with mayors with longer horizons decrease malfeasance spending by 11.5 percentage points, off of a prereform mean of 16%. This point estimate is statistically significant at the 95% level and economically meaningful. In particular, if we extrapolate this point estimate to the rest of the municipalities and consider all the earmarked transfers received from the upper layers of government, a back-of-the-envelope calculation suggests that municipalities with longer horizons misappropriate, on average, MXN 11.66 million (USD 606,286 in 2018) less earmarked resources than mayors with shorter horizons.[29] Note that this exercise computes a lower bound for the misspent resources, if we consider that municipal governments receive revenue from other sources, such as unmarked transfers and local revenue. Overall, these findings are consistent with previous work showing the effects of electoral accountability on corruption for Brazil (Ferraz and Finan, 2011). I interpret this result as a plausible explanation for why treated municipalities are able to finance a higher stock of human and public capital.

As robustness exercises, Table 1.B.9 in Appendix 1.B reestimates the treatment effects considering group-specific pretrends. The treatment effects remain unchanged. Furthermore, Table 1.B.10 in Appendix 1.B estimates the treatment effects by exploiting the annual frequency of the data using an event study framework, and Figure 1.B.7 plots the event study estimates. The magnitudes and conclusions from the results remain practically unchanged.

1.6.4 Quality of public goods

The results presented above show that as a result of the reform, there is a significant expansion in the capacity to provide public goods. In this subsection, I present evidence on the effects of the reform on the quality of public goods delivered. I focus on measures of the quality of public security, since it is the main public good delivered by municipalities in my setting. Increasing the capacity to provide public security in a context like Mexico, however, does not necessarily imply better outcomes for citizens. For instance, it is possible that as a result of an expansion in police officer recruitment citizens start paying more bribes. Moreover, an increase in policing capacity

[29] The back-of-the-envelope calculation extrapolates the point estimate to all municipalities in the studied sample and considers the average earmarked transfers from the upper layers of government in 2016. To convert this quantity from MXN to USD, the calculation considers the average exchange rate in 2018 (MXN 19.23 per USD).

may imply more violence. In this subsection, I provide four pieces of evidence that suggest that the expansion in policing capacity led to an improvement in the quality of public security.

I begin by providing evidence that bribe payments to the municipal police did not increase. One practical limitation, however, is that obtaining data on bribes is challenging (Olken and Pande, 2012); typically, it requires surveying households (Niehaus and Sukhtankar, 2013) or having survey enumerators observing bribe payments (Olken and Barron, 2009). I overcome this challenge by using the ENVIPE. This survey asks respondents whether they think the municipal police are corrupt. If citizens in municipalities with treated mayors pay more bribes to the police, then we would expect that their perception about the corruption of the municipal police worsens. Column 1 of Panel A of Table 1.7 tests this hypothesis and shows the difference-in-differences estimate for a dummy variable that takes the value of one if the respondent thinks the municipal police are corrupt and zero otherwise. The estimate is a precisely estimated zero, and I can rule out effect sizes as small as 2.8% off the mean at the 95% level. This finding suggests that it is unlikely that bribe payments went up in municipalities with treated mayors. Using data from the same survey, column 2 of Panel A shows the difference-in-differences estimate for a dummy variable that takes the value of one if the respondent thinks his neighborhood is insecure and zero otherwise. The result is small and statistically insignificant, suggesting that crime in municipalities with treated mayors did not go up.

Next, I explore whether violence increased in treated municipalities. Column 1 of Panel B reports the difference-in-differences estimate for the number of homicides in the municipality per 10,000 inhabitants obtained from INEGI, and Figure 1.6 plots the difference-in-differences estimates over time (exploiting the availability of monthly data). The estimate is small, statistically insignificant, and does not change over time (see Panel (a) of Figure 1.6), suggesting that violence did not go up in municipalities with treated mayors. Finally, I look at the effects of the reform on criminal investigations of crimes in the jurisdiction of the municipal police. The crimes studied include burglary, personal assault, theft, rape, homicide (unrelated to drug trafficking), kidnapping, and bank robberies, among others (see Table 1.B.11 in Appendix 1.B).[30] In this setting, criminal

[30]Criminal investigations of these types of crimes are conducted by state authorities. However, the municipal police plays an important role, both in initiating investigations and during the investigations. For example, the majority of

investigations per crime are low due to the low arrest rates and difficulties in the apprehension of suspects (México Evalúa, 2012). Hence, given the null changes in the perceptions of crime, an increase in investigations would suggest an increase in arrests and apprehension of suspects per crime. The results for criminal investigations are reported in column 2 of Panel B of Table 1.7, and Figure 1.6 plots the difference-in-differences estimates over time (exploiting the availability of monthly data). The result shows that, on average, there is one more criminal investigation per 10,000 inhabitants of crimes in the jurisdiction of the municipal police, off of a prereform mean of 6. This result is highly statistically significant and economically meaningful, and the effect starts around the second quarter of the first year in office of the mayor (see Panel (b) of Figure 1.6).

1.6.5 Effects by strongholds

The results so far show evidence that political time horizons affect public policies with longer-term objectives: municipalities with treated mayors expand the public administration, improve their human capital, report more public capital for the provision of public goods, borrow significantly less, and misappropriate less public funds. As discussed in Section 1.2, there are two key mechanisms that could explain these results. On the one hand, mayors with longer political time horizons might choose different long-lasting policies, because they internalize the future costs and returns of these policies. This is consistent with the main idea stressed in seminal models of public debt (Persson and Svensson, 1989; Alesina and Tabellini, 1990), and with key ideas in models of government investments (Svensson, 1998; Acemoglu, 2005; Besley and Persson, 2009, 2010, 2011). On the other hand, it is possible that voters are sophisticated, well-informed, and forward-looking, and they reward incumbents for these policies. In turn, this may create the incentives for mayors with re-election possibilities to choose these policies. This would be consistent with models of electoral accountability arguing that rational voters reward incumbents for good policies (Barro, 1973; Ferejohn, 1986; Besley and Case, 1995; Smart and Sturm, 2013) or policies that are targeted to them (Drazen and Eslava, 2010).

One way to test the plausibility of these mechanisms is by looking at the average treatment

police interventions are made by the municipal police. According to official records from INEGI, in 2010 and in 2012, 73% and 72%, respectively, of the police interventions were made by the municipal police (INEGI, 2019).

effects on the treated by strongholds. When re-election is guaranteed, mayors have more incentives to internalize the future costs and returns of their policies.[31] In contrast, when re-election is not guaranteed, mayors might have more incentives to try to please voters through the choice of policies. The framework presented in Appendix 1.A is consistent with this argument; it shows that the horizon effect is stronger when the perceived probability of staying in office is higher due to ideology preferences for the incumbent. Hence, looking at heterogeneous treatment effects by strongholds might provide evidence on which of these mechanisms is more likely to be driving the main reduced-form effects.

I consider two criteria to classify strongholds. First, I consider a measure that captures the popularity of the mayor in the municipality. The main intuition of this criterion is to have a measure that captures whether the majority of voters support the ideology/characteristics of the mayor. To proxy for this measure, I consider the margin of victory of the mayor. Using this continuous measure, I construct a dummy variable that takes the value of one if the margin of victory in municipality i is greater than the national median (8.85%) and zero otherwise. The second criterion considers the historical popularity of the mayor's political party in the municipality. The main intuition of this criterion is that a mayor can be in a stronghold because his political party is popular in the municipality. To proxy for this measure, I compute the fraction of elections won by the postreform mayor's party using the elections from 2000 to 2017. Using this continuous measure, I construct a dummy variable that takes the value of one if this fraction is greater than the national median (40%) and zero otherwise. Then, a postreform mayor is considered to be in a stronghold if these two criteria are satisfied. In the studied sample, 27% of the municipalities satisfy both criteria. Before the reform, this measure predicts that in strongholds, the incumbent party is 30 percentage points more likely to win the next election, off of a 36% mean (Figure 1.B.8 in Appendix 1.B).

[31] In the models of public debt of Persson and Svensson (1989) and Alesina and Tabellini (1990), for example, if the incumbent is guaranteed re-election, then he does not overborrow and chooses the optimal level of public debt.

Then, using this measure of stronghold, I estimate the following specification:

$$\begin{aligned} y_{ipt} &= \lambda_i + \delta_t + \theta_p + \phi_1 Treated_i \cdot Post_t + \phi_2 Treated_i \cdot Post_t \cdot Stronghold_{it} \\ &+ \phi_3 Post_t \cdot Stronghold_{it} + \phi_4 Treated_i \cdot Stronghold_{it} + \phi_5 Stronghold_{it} \\ &+ \sum_j \alpha_{jk} \cdot \mathbf{1}\{j = t\} \cdot X_i' + W_{it}\Gamma + \epsilon_{ipt} \end{aligned} \tag{1.4}$$

where the indices are as before. The variable $Stronghold_{it}$ is a dummy variable that takes the value of one if the municipality is a stronghold and zero otherwise. The parameter of interest is ϕ_2, which identifies the heterogeneous average treatment effect of the reform on the outcomes of interest. Hence, ϕ_1 is the average treatment effect of the reform in competitive places, and $\phi_1 + \phi_2$ is the average treatment effect of the reform in strongholds.

The results of the estimation of equation 1.4 for the main outcomes of interest are presented in Table 1.8. Columns 1–3 of Table 1.8 show the results for the equally weighted z-score indices measuring the capacity to provide public goods. The results show significant differences in the ATT for treated mayors in strongholds, relative to treated mayors in more competitive places. In particular, the estimated differences in treatment effects (estimate of ϕ_2) are 0.08σ for the human capital index, 0.09σ for the public capital index, and 0.085σ for the overall index (columns 1, 2, and 3, respectively). These differences are statistically significant at the 95% level and economically meaningful, and imply that treated mayors in strongholds invest, on average, 0.15σ more in the overall capacity than control mayors in strongholds (estimate of $\phi_1 + \phi_2$ in column 3). While strongholds might be correlated with the outcomes in other ways, these results suggest that the perceived probability of re-election matters for investments, underscoring the longer horizon effect of the policy change. This is consistent with the predictions of the framework of Besley and Persson (2011), which states that political leaders invest in state capacity when their probability of staying in office is sufficiently high.

Columns 4 and 5 report the estimates of equation 1.4 for fiscal deficit and property taxes. Consistent with the idea of Persson and Svensson (1989) and Alesina and Tabellini (1990) that the higher the probability of staying in office, the higher the incentives to internalize the costs of debt in terms of future public spending, treated mayors in strongholds borrow significantly less than

treated mayors in more competitive places. I find no heterogeneous treatment effects for property tax revenue. Finally, column 6 presents the estimates for malfeasance spending. Treated mayors both in competitive places (ϕ_1) and in strongholds ($\phi_1 + \phi_2$) misappropriate less public funds than control mayors. However, the estimate is statistically significant for mayors in strongholds, while the estimate for mayors in competitive places is less precisely estimated. Moreover, the difference in treatment effects is negative (but not statistically significant). This is consistent with the idea that the prospect of future rents deters current corruption and the effect is more pronounced when the perceived probability of staying in office is high.

Altogether, the evidence presented in this subsection suggests that the longer horizon effect is a plausible mechanism driving the results: when choosing public policy, mayors care about the future costs and benefits of their policies.

1.6.6 Robustness

I run a number of robustness tests. These include (i) considering different weights in the indices; (ii) controlling for group-specific trends; (iii) restricting the estimations to a balanced sample; (iv) when data are available, using the annual frequency of the data and an empirical design that exploits the staggered entry into treatment; (v) including year of term fixed effects in the estimations; (vi) leaving out municipalities from one state at a time and reestimating the treatment effects; and (vii) robustness tests to treatment heterogeneity by timing of adoption.

The results of the robustness exercises (i) to (vi) are presented in Appendix 1.B. In Appendix 1.D, I report robustness exercises that consider the possibility of treatment heterogeneity by timing of adoption of treatment (robustness exercise vii). I use the interaction-weighted estimator (IW) proposed by Sun and Abraham (2020), which is robust to the presence of treatment heterogeneity by timing of adoption. Using their IW estimator, I reestimate the treatment effects and compare them with the treatment effects obtained from the standard two-way fixed effects model. I find that the IW estimates are similar to the two-way fixed effects model (see Tables 1.D.1 and 1.D.2).

1.7 Characteristics of mayors

By removing the ban on consecutive re-election, the reform increased the expected value of holding office. As a result, it may have also affected those who run. This opens up two questions, the first being, does it in fact change the candidates and winners in any descriptive way, and the second being trying to partial out the extent to which the reduced-form effects arise from the extensive versus the intensive margin. In this section, I shed some light on these questions.

1.7.1 Descriptive characteristics

Exploring changes in the characteristics of mayors is challenging in this setting, because they vary only by election year and stay constant during the mayor's term.[32] One way to inform whether the composition of mayors changes after the reform is by looking at the evolution of observable characteristics of the mayors before and after the reform. To that end, I estimate the following two-step specification:

$$S_{ise} = \pi_0 + \sum_{j=1}^{30} \pi_j e \cdot \mathbf{1}\{s = j\} + v_{ise} \text{ for } e < 0 \tag{1.5}$$

$$\hat{v}_{ise} = \lambda_s + \alpha PostReform_{ise} + \Gamma W_{se} + u_{ise} \tag{1.6}$$

where S_{ise} is an observable characteristic of the politician in the election relative to the reform e in municipality i in state s. $PostReform_{ise}$ takes the value of one if the election e is the postreform election and zero otherwise. λ_s are state fixed effects. In the first step, I perform a linear state-specific prediction of the outcome S using prereform elections and then residualize the outcome with the prediction for all elections (including the postreform election). In the second step, I regress the residualized outcome on the postreform dummy variable, state fixed effects, and time varying covariates. Thus, α estimates the association in the postreform period between the observed characteristic S and the reform, after controlling for prereform state-specific trends.

[32] The type, gender, and the previous employment of the mayors are characteristics that vary only by election year. For the data construction, I assume that the education of the mayor is also a fixed characteristic and does not vary while he is in office. This assumption is important for the education outcomes explored here, because the data rely on the Municipal Censuses, which can be reported during the first, second, or third year in office.

Table 1.9 reports the estimates of α for different observable characteristics of local politicians. The estimations consider data for the two elections preceding the reform and the election immediately following the reform. Columns 1 and 2 of Panel A show the estimates for the changes in the likelihood of observing female candidates and female mayors, respectively. Columns 3 to 5 of Panel A and columns 1 to 6 of Panel B use data from the municipal government censuses collected by INEGI for the years 2010, 2012, 2014, and 2016. These data are deidentified at the municipality level for the years 2012, 2014, and 2016.[33] The results are as follows. First, after the reform there is a an important increase in the fraction of female candidates and female mayors. Second, there are no significant changes in the education of mayors. Third, after the reform it is more likely that the elected mayors come from government agencies (municipal, state, or federal) or are starting their first job, and less likely that they are business owners or come from another job. Overall, these results show that the reform is associated with changes in some observed characteristics of mayors. In the next subsection, I perform two empirical exercises to try to understand the extent to which changes in the characteristics of the mayors mediate the main reduced-form effects presented in Section 1.6.

1.7.2 Understanding mechanisms

To shed some light on whether changes in the characteristics of mayors mediate the main reduced-form effects, I conduct two empirical exercises. First, I reestimate the main specifications, controlling for observed characteristics of mayors. In particular, I reestimate equation 1.1, including a dummy variable indicating whether the mayor is female or not.[34] While this is a "bad control" (Angrist and Pischke, 2008), the results from this approach illustrate the extent to which gender correlates with the main outcomes conditional on treatment. Figure 1.7 compares the main reduced-form effects reported in Tables 1.3 and 1.4 and the results from this exercise. Panel (a) plots the results for the capacity outcomes, while Panel (b) plots the results for the public

[33] While it is not possible to observe the municipality identifier, it is possible to observe the number of municipalities within each state reporting each one of the characteristics of the mayors. I use this feature of the data to build a data set with characteristics of the mayor at the municipality$\times$election level. I plan to build a richer data set and expand the analysis presented in this section with characteristics of mayors that contain municipality identifiers.

[34] I plan to expand this analysis by constructing a richer data set of characteristics of mayors with municipality identifiers.

finance outcomes. As is shown, the point estimates remain practically unchanged. This suggests that the reform is not affecting the outcomes differentially by gender and that there is no correlation between gender and the main outcomes of interest. Indeed, the point estimates for the gender coefficient in all specifications are small and statistically insignificant.[35]

While this empirical exercise illustrates the extent to which characteristics of mayors are correlated with the outcomes, it cannot inform about whether the effects are mediated by the selection of different types of politicians. The second empirical exercise tries to shed some light on this question. It consists of constructing a sample of mayors in the postreform period who were already selected into politics before the reform. Intuitively, for these mayors the expected returns of being in office are higher than the costs of entering politics, even with a one-term limit; hence, it is unlikely that the reform induced them to enter politics. I follow this idea and construct a sample of municipalities where the winning mayors in the postreform period were either already selected into politics before the reform or belong to a dynasty. Since this is an endogeneous selection, I follow the same approach for the treated and the comparison groups (municipalities that become treated later).

I identified 493 municipalities that had a mayor in the postreform period who was already selected into politics before the reform and 155 municipalities that had a mayor from a family involved in politics.[36] In Table 1.B.12 in Appendix 1.B, I compare observable characteristics of the municipalities with mayors who were involved in politics before the reform against observable characteristics of municipalities with mayors who did not run for office before the reform. Overall, municipalities with mayors who were involved in politics before the reform tend to be more marginalized and less developed. Moreover, within states, these municipalities tend to be smaller and more marginalized (column 8 of Table 1.B.12). Finally, to further explore the characteristics of these municipalities, Table 1.B.13 in Appendix 1.B restricts the sample to municipalities with mayors who were involved in politics before the reform and compares observable characteristics across the treated and control municipalities. On average, these municipalities tend to report similar demographic and sociodemographic characteristics.

[35]To save space, these point estimates are available upon request.

[36]To identify this set of municipalities, I use the candidates who have run for office in elections since 2009. In the Data Appendix 1.E I describe in more detail the procedure used to identify this set of municipalities.

I reestimate equation 1.1, restricting the sample to this set of municipalities. Figure 1.7 also compares these estimates with the main estimates in Tables 1.3 and 1.4. The point estimates for the different indices of local capacity and public finance outcomes are similar, and cannot be statistically distinguished from each other. Overall, these findings show that even for mayors who selected into politics before the reform, extending political time horizons affects long-term policies. Thus, the available evidence suggests that while the reform does indeed change some observable characteristics of mayors, the treatment effects on the political behavior of mayors are primarily driven by moral hazard, meaning that the same politician changes behavior when provided with longer political time horizons.

1.8 Conclusions

Improving the capacity to deliver public goods is a central issue in a vast number of low- and middle-income countries around the world. The decision to invest and expand this capacity, however, relies on politicians who typically are in office for a limited amount of time and might, therefore, focus on policies with more immediate returns. In this paper, I examine whether increasing the expected political horizon of local politicians affects the decision to expand this capacity. The results show that politicians with longer political time horizons improve the human capital and expand the public capital used to provide public goods. Using a unique natural experiment in Mexico and a rich data set on measures of the inputs and outputs of municipal governments, I show that first-term mayors with re-election possibilities expand the size of the public administration, increase the professionalization of the municipal police, and increase the municipal capital, relative to first-term mayors without re-election possibilities. Moreover, I find that these investments are not debt financed. Instead, using official audit reports I find that mayors with longer horizons have less corrupt expenditures.

I interpret these results through the lenses of previous theoretical work emphasizing (i) the role of horizons on long-term policies and (ii) the role of elections as an incentive mechanism. To understand which mechanism is more likely to be in play, I explore heterogeneity of the results by strongholds, which are places with more political stability and where incumbents are more

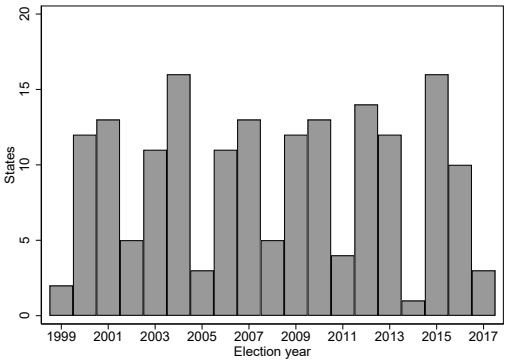
likely to earn the rewards of their investments. The results show that the effects of the reform on the main outcomes of interest are more pronounced in strongholds, underscoring the idea that the prospect of future costs and rewards of policies matter for the forward-looking choices of politicians. Finally, while the reform changed some observable characteristics of the mayors, the available evidence suggests that the treatment effects are primarily driven by a decrease in moral hazard.

Overall, the evidence presented in this paper shows that the reform induced mayors to focus more on policies with longer-term objectives, which was one of the main purposes of the policy change. However, the evidence speaks only about the immediate effects of the reform, which raises two important and connected questions. First, while the reform increased investments in human and public capital financed by less corrupt practices, it is still an open question whether citizens enjoy more consumption of public goods in the future. Second, the evidence presented here is limited in its ability to speak about the optimal number of terms for welfare. While there have been recent theoretical efforts on this matter (see, for examples, Smart and Sturm, 2013; Aghion and Jackson, 2016), providing empirical answers to these questions are avenues for future research.

1.9 Acknowledgments

This chapter, in part, is currently being prepared for submission for publication of the material. Lopez-Videla, Bruno. "Political Time Horizons and Government Investments: Evidence from Mexico." The dissertation author was the sole author of this material.

Figures



(a) Election years

(b) Postreform election year by states

Figure 1.1: Election cycles

Notes: The figure plots the election cycles by states. Panel (a) depicts the number of local elections by states and calendar year from 1999 to 2017. Panel (b) depicts geographic variation in election years across states in the postreform years. The states in the studied sample with municipal elections in 2015 are Baja California Sur, Campeche, Chiapas, Colima, Guanajuato, Guerrero, Jalisco, Michoacán de Ocampo, Morelos, México, Nuevo León, Querétaro, San Luis Potosí, Sonora, Tabasco, and Yucatán. The states with municipal elections in 2016 are Aguascalientes, Baja California, Chihuahua, Durango, Hidalgo, Quintana Roo, Sinaloa, Tamaulipas, Tlaxcala, and Zacatecas. Finally, the states with municipal elections in 2017 or after are Coahuila de Zaragoza, Nayarit, Puebla, and Veracruz de Ignacio de la Llave. Municipalities with elections in 2017 or after and those in the states of Hidalgo and Tlaxcala do not become treated during the studied period and act as a pure control group.

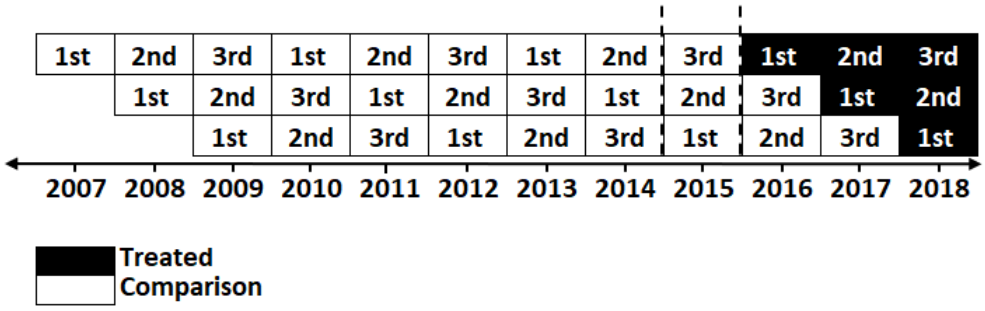
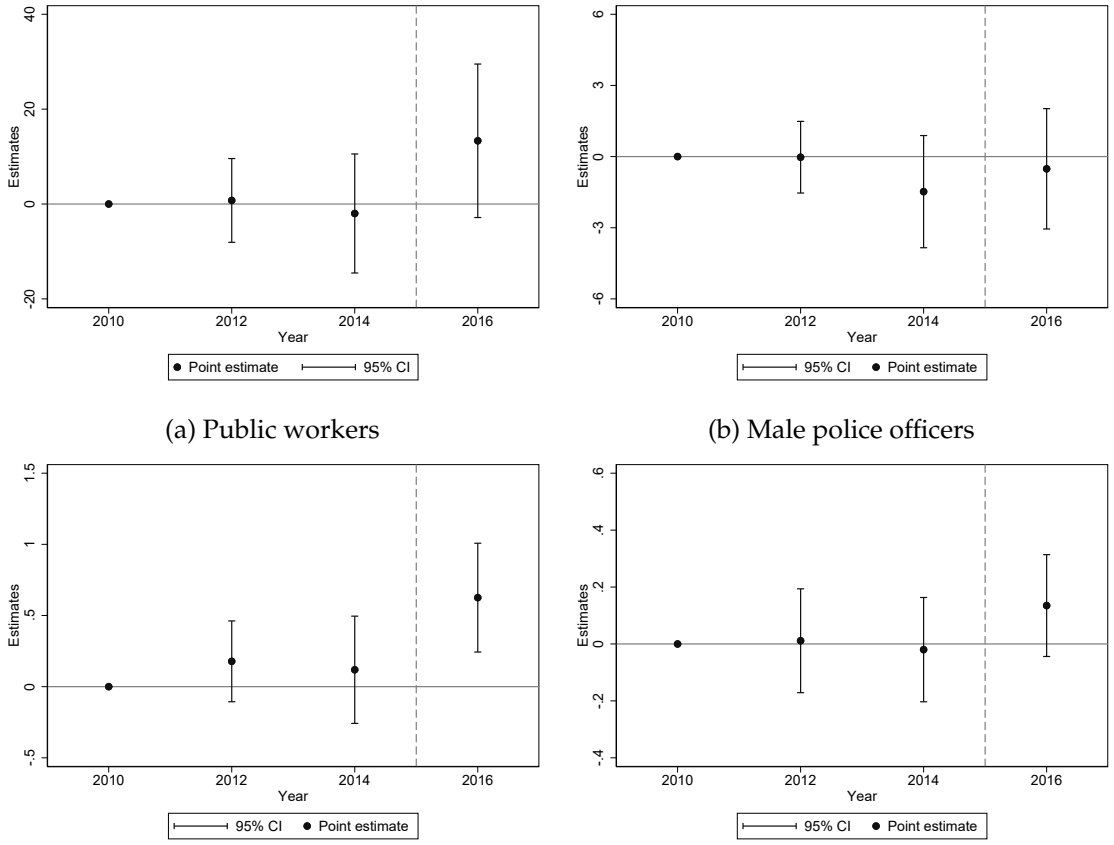


Figure 1.2: Empirical design

Notes: The figure depicts a simplified version of the empirical design. White rectangles represent the years of the terms of mayors in municipalities with elections right before the implementation of the electoral reform. Dark rectangles represent the years of the terms of mayors in municipalities with elections after the implementation of the electoral reform. The dashed vertical lines show the announcement and implementation of the reform, respectively. Depending on the state, mayors take office in the last quarter of the election year or in January of the following year.



(a) Public workers

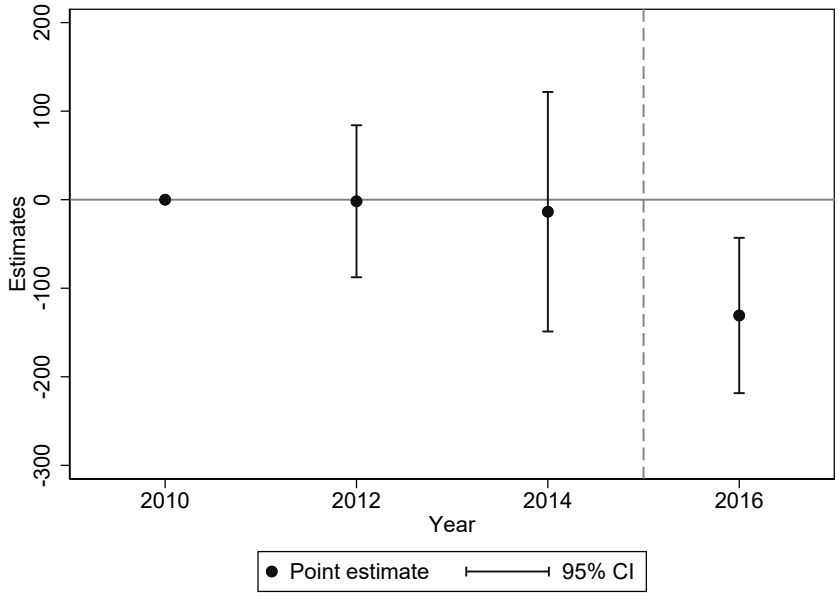
(b) Male police officers

(c) Female police officers

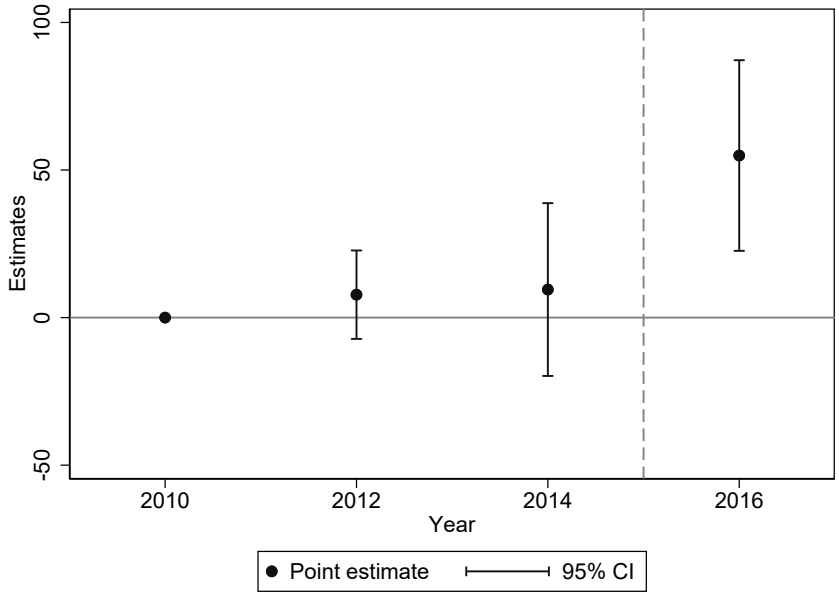
(d) Police stations

Figure 1.3: Effects on municipal employees and policing capacity

Notes: The figure plots the estimates of equation 1.2 and the 95% confidence intervals for the number of public workers, police officers by gender, and the number of police stations. The outcomes are per 10,000 inhabitants. The estimations include the log of the state GDP, the mayor's margin of victory, and the following pre-determined municipality characteristics interacted with time dummies: marginality index, average years of schooling, log of households with access to computers, and log of individuals with access to social security. The vertical dashed line corresponds to the year when the reform was implemented. Standard errors are clustered at the state level.



(a) Fiscal deficit



(b) Property taxes

Figure 1.4: Sources of revenue

Notes: The figure plots the estimates of equation 1.2 and the 95% confidence intervals for the two main sources of revenue that are under the control of local governments. Panel (a) plots the estimates for the fiscal deficit. Panel (b) plots the estimates for the property tax revenue. The estimations include the log of the state GDP, the mayor's margin of victory, and the following predetermined municipality characteristics interacted with time dummies: marginality index, average years of schooling, log of households with access to computers, and log of individuals with access to social security. Outcome variables are in pesos of 2018. The vertical dashed line corresponds to the year when the reform was implemented. Standard errors are clustered at the state level.

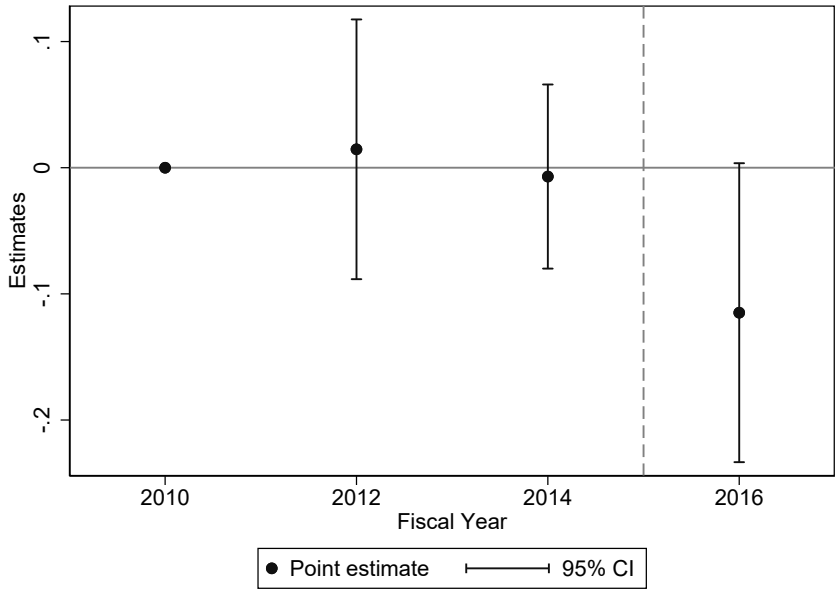
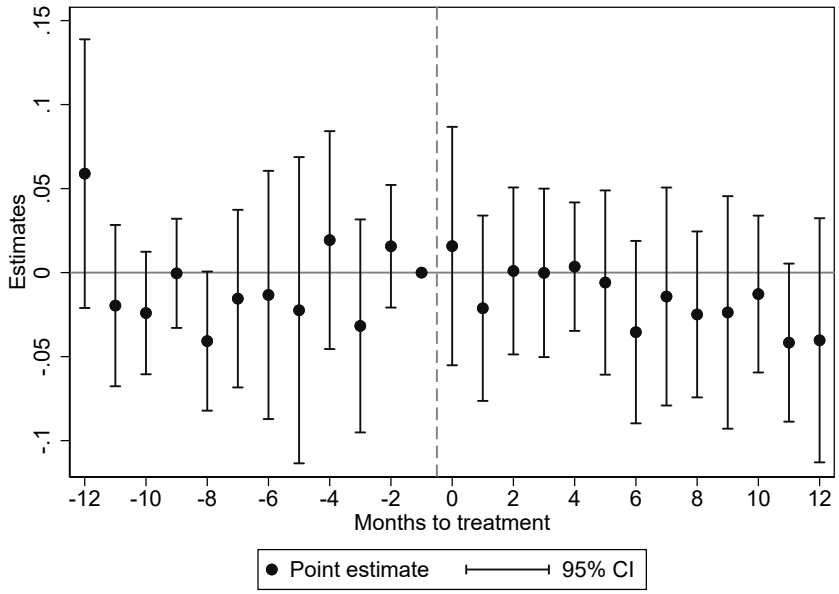
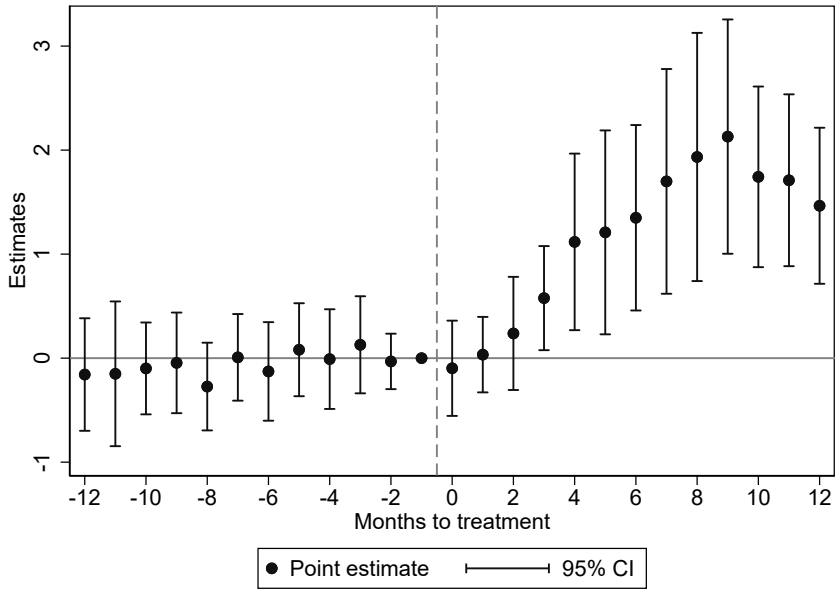


Figure 1.5: Effects on misspent resources

Notes: The figure plots the estimates of equation 1.1 and the 95% confidence intervals for the measure of malfeasance spending. The outcome variable is the fraction of FISM resources that were found to be misspent in the audit and that need to be reimbursed to the federal treasury. The estimations include the log of the state GDP, the incumbent's margin of victory, and the following predetermined municipality characteristics interacted with time dummies: marginality index, average years of schooling, log of households with access to computers, and log of individuals with access to social security. Moreover, the estimations consider state, year, and party fixed effects. The vertical dashed line corresponds to the year when the reform was implemented. Standard errors are clustered at the state level.



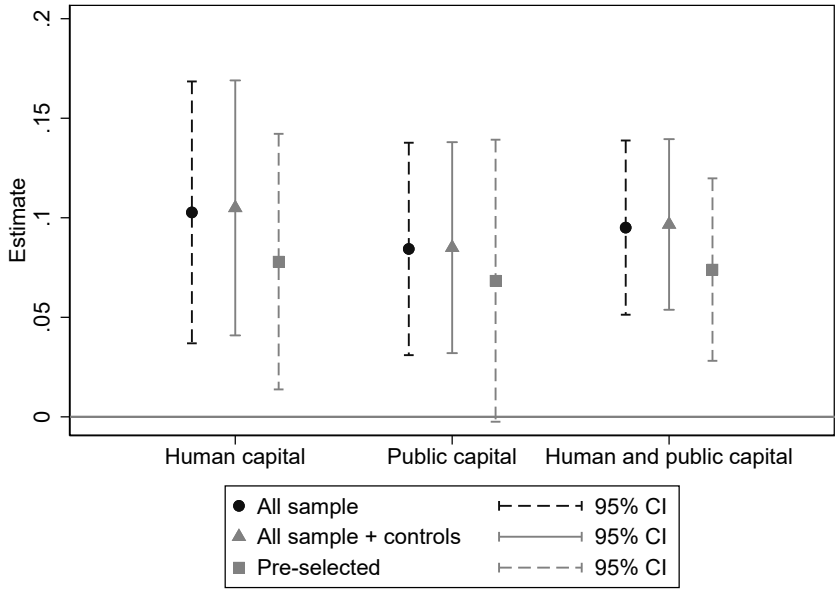
(a) Homicide rate



(b) Criminal investigations in the jurisdiction of the municipal police

Figure 1.6: Effects on delivery of public security

Notes: The figure plots difference-in-differences estimates for the homicide rate and for criminal investigations in the jurisdiction of the municipal police (i.e., common law crime) using monthly data. The criminal investigations refer to crimes such as burglary, personal assault, car theft, rape, homicide, kidnapping, and financial fraud, among other related crimes. The estimations include the log of the state GDP, the mayor's margin of victory, and the following pre-determined municipality characteristics interacted with time dummies: marginality index, average years of schooling, log of households with access to computers, and log of individuals with access to social security. The vertical dashed line corresponds to the month when the reform was implemented. Standard errors are clustered at the state level.



(a) Capacity outcomes

(b) Public finance outcomes

Figure 1.7: Estimates for the sample of municipalities with preselected mayors

Notes: The figure plots the estimates and the 95% confidence intervals for the z-score indices of local capacity and for public finance outcomes. "All sample" refers to estimates that consider all municipalities. "All sample + controls" controls for a dummy variable that takes the value of one if the mayor is female and zero otherwise. "Preselected" refers to the sample of municipalities with mayors in the postreform period that were already involved in politics before the reform. The estimates in Panel (a) have been restricted to municipalities that report the outcomes used in the construction of all indices. The y-axis is measured in standard deviations (panel (a)) and pesos per capita (panel (b)). Standard errors are clustered at the state level.

Tables

Table 1.1: Term limits of local politicians and constitutional amendments

	Term length (years)	Consecutive terms	Year of the reform	Staggered elections
	(1)	(2)	(3)	(4)
Latin America				
Bolivia	5	2	-	No
Brazil	4	2	1998	No
Chile	4	3	2020	No
Colombia	4	1	-	No
Mexico	3	2	2014	Yes
Peru	4	1	2015	No
Europe				
Italy	5	2	1993	Yes
Portugal	4	3	2005	No
Asia				
Philippines	3	3	1987	No

Notes: The table shows term limits for officials in municipal governments in different countries around the world and the year of constitutional amendments to term limits, if applicable. Data were collected from different sources of each country. In Brazil, the 1998 reform increased the number of terms from one to two. In Chile, there was unlimited re-election and the reform introduced a limit of three terms. In Peru, the 2014 reform introduced a limit of one term. In Mexico, the reform increased the number of term terms from one to two. In Italy, the 1993 reform decreased the number of terms to two. In the Philippines, the 1987 reform decreased the number of terms to three. In Portugal, the law was passed in 2005 but only became binding with the 2013 elections.

Table 1.2: Summary statistics of main variables

Variables	Mean	Std. Dev.	N	Periods
	(1)	(2)	(3)	(4)
Municipality employees (per 10,000 inhabitants)				
Directors	16.32	19.43	7026	4
Public workers	147.42	139.15	7037	4
Public security (per 10,000 inhabitants)				
Police officers	21.09	13.86	7082	4
Male police officers	19.14	13.16	7082	4
Female police officers	1.95	2.23	7082	4
Basic education police officers	20.25	13.69	5380	3
Higher education police officers	0.58	0.96	5376	3
Police stations	1.87	2.42	7267	4
Jail cells	2.57	4.12	7195	4
Street lighting (per 10,000 inhabitants)				
Streetlight posts	1034.84	794.55	3666	2
Piped water and sewerage				
House connections (per 10,000 inhabitants)	1837.85	1351.10	7034	4
Sewerage	0.90	0.30	7318	4
Sewerage water treatment	0.38	0.48	6889	4
Waste disposal				
Waste collection	0.99	0.10	7440	4
Waste treatment	0.04	0.19	7310	4
Waste disposal vehicles (per 10,000 inhabitants)	2.09	2.16	7307	4
Sources of revenue (per capita)				
Income	5355.45	3015.49	6475	4
Taxes	203.33	287.99	6475	4
Property taxes	170.60	251.79	6475	4
Fees and tariffs	148.07	168.33	6475	4
Other local revenue	118.32	259.03	6475	4
Earmarked transfers	2237.27	1499.22	6475	4
Unmarked transfers	2301.98	1977.32	6475	4
Fiscal deficit	212.72	413.09	6475	4
Initial income	62.96	259.16	6475	4
Other income	70.81	233.56	6475	4
Public expenditures (per capita)				
Wages	1602.68	1165.60	6475	4
Supplies	408.94	452.72	6475	4
Services	662.09	546.49	6475	4
Transfers and subsidies	446.47	454.61	6475	4
Assets	71.15	115.98	6475	4
Public investment	1792.64	1479.24	6475	4
Other expenditures	137.76	232.27	6475	4
Budget surplus	262.49	526.26	6475	4
Malfeasance spending				
Pr(audit=1)	0.11	0.32	7456	4
Misspent resources	0.13	0.22	858	4
Mayor characteristics				
Male candidate	0.77	0.42	22323	3
Male mayor	0.89	0.31	5345	3
Basic education	0.38	0.48	4769	3
Undergraduate	0.54	0.50	4769	3
Graduate	0.08	0.27	4769	3
Government	0.39	0.49	4603	3
Politics	0.07	0.26	4603	3
Private sector	0.06	0.24	4603	3
Business owner	0.37	0.48	4603	3
First job	0.03	0.16	4603	3
Another job	0.09	0.28	4603	3

Notes: The table shows summary statistics of the main variables of interest. The number of observations varies due to data availability. The unit of observation is the municipality×year, except for Mayor characteristics, for which the unit of observation is the municipality×election. The public finance outcomes are in pesos of 2018.

Table 1.3: Effects on the capacity to provide public goods

	Municipality employees		Police officers				
	Directors	Public workers	Total	Male	Female	Basic education	Higher education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Human capital							
Treated x Postreform	0.921	13.86**	0.580	0.0589	0.521***	0.370	0.144***
	(0.858)	(6.061)	(0.875)	(0.794)	(0.151)	(0.729)	(0.0450)
	[0.37]	[0.07]	[0.55]	[0.96]	[0.00]	[0.66]	[0.01]
Dep. Var. Mean	15.58	163.90	23.70	21.59	2.11	22.65	0.61
Observations	7026	7037	7082	7082	7082	5380	5376
R^2	0.05	0.08	0.06	0.09	0.09	0.06	0.04

	Public security		Lighting	Piped water and sewerage			Waste disposal
	Police stations	Jail cells	asinh(Street light posts)	House connections	Sewerage	Sewage treatment	asinh (vehicles)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel B. Public capital							
Treated x Postreform	0.138**	0.247	0.335*	84.30	0.00583	0.00444	0.0620**
	(0.0653)	(0.208)	(0.179)	(104.1)	(0.00704)	(0.0491)	(0.0285)
	[0.08]	[0.36]	[0.04]	[0.52]	[0.48]	[0.95]	[0.11]
Dep. Var. Mean	1.96	2.70	7.37	1839.10	0.88	0.41	1.96
Observations	7267	7195	3666	7034	7318	6889	7307
R^2	0.01	0.02	0.04	0.13	0.02	0.03	0.02

	Human capital	Public capital	Human and public capital
	(1)	(2)	(3)
Panel C. Aggregated outcomes (z-score indices)			
Treated x Postreform	0.0733**	0.103**	0.0951***
	(0.0308)	(0.0408)	(0.0223)
	[0.04]	[0.00]	[0.00]
Observations	5264	3459	3316
R^2	0.04	0.04	0.08
Municipality FEs	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes
Party FEs	Yes	Yes	Yes
Covariates	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows difference-in-differences estimates for measures of the stock of human and public capital used to provide goods and services. Panel A shows the results for human capital, whereas Panel B shows the results for public capital. Panel C shows the results for z-score indices that aggregate the information reported in Panels A and B. The human capital index aggregates the information from columns 1–7 of Panel A. The public capital index aggregates the continuous variables of Panel B, which correspond to columns 1–4 and column 7. Finally, the human and public capital index aggregates the information used in the other two indices. The number of observations varies across columns due to data availability and Appendix 1.B presents results for a balanced sample. Basic education aggregates no education, primary, secondary, high school, and technical education, whereas higher education aggregates undergraduate and graduate education. The covariates include the log of the state GDP, the mayor's margin of victory, and predetermined municipality characteristics interacted with time dummies. Standard errors clustered at the state level are shown in parentheses and wild bootstrap p-values clustered at the state level are shown in brackets.

Table 1.4: Effects on sources of revenue

				Transfers	
	Fiscal deficit	Property taxes	Fees and tariffs	Earmarked	Unmarked
	(1)	(2)	(3)	(4)	(5)
Treated x Postreform	-125.6***	49.21***	0.906	20.40	2.363
	(44.25)	(13.59)	(10.10)	(124.8)	(121.4)
	[0.01]	[0.00]	[0.93]	[0.91]	[0.98]
Dep. Var. Mean	212.7	170.6	148.1	2237.3	2302.0
Observations	6475	6475	6475	6475	6475
R^2	0.03	0.07	0.04	0.17	0.20
Municipality FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Party FEs	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows difference-in-differences estimates for outcomes related to the sources of revenue of the municipality. All outcomes are per capita and in pesos of 2018. The mean of the dependent variable is computed using the prereform period. See Table 1.3 for a description of the covariates. Standard errors clustered at the state level are shown in parentheses and wild bootstrap p-values clustered at the state level are shown in brackets.

Table 1.5: Effects on the composition of public expenditures

	Wages	Supplies and services	Subsidies and transfers	Properties	Public investment	Other	Budget surplus
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated x Postreform	-0.0155	-0.00303	0.0256	-0.00285	-0.00602	0.00770	-0.00594
	(0.0174)	(0.0176)	(0.0211)	(0.00344)	(0.0417)	(0.00651)	(0.0147)
	[0.47]	[0.87]	[0.29]	[0.46]	[0.90]	[0.28]	[0.76]
Dep. Var. Mean	0.30	0.19	0.08	0.01	0.33	0.03	0.05
Observations	6474	6474	6474	6474	6474	6474	6474
R^2	0.06	0.17	0.15	0.04	0.09	0.08	0.04
Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Party FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows difference-in-differences estimates for the composition of the public expenditures of the municipal governments. Wages refers to expenditures in wages, benefits, and bonuses, among other expenditures related to municipal workers' compensation. Supplies and services refer to the procurement of supplies and services required for the day-to-day activities of the public administration. Subsidies and transfers refers to expenditures related to subsidies to the local economic activity and transfers to the municipal government branches and social programs. Properties refers to expenditures related to the acquisition of properties. Public investment refers to the procurement of infrastructure projects. Other expenditures refers to financial investments, transfers to other municipalities, debt services, and other expenditures that cannot be grouped in these categories. Finally, Budget surplus is the difference between the annual income and the public expenditures. See Table 1.3 for a description of the covariates. The mean of the dependent variable is computed using the prereform period. Standard errors clustered at the state level are shown in parentheses and wild bootstrap p-values clustered at the state level are shown in brackets.

Table 1.6: Effects on misappropriation of public funds

	Pr(audit=1)			Misspent resources			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated x Postreform	0.00886	0.0105	0.0181	-0.112**	-0.107**	-0.113**	-0.115**
	(0.0171)	(0.0175)	(0.0151)	(0.0494)	(0.0491)	(0.0501)	(0.0461)
	[0.59]	[0.53]	[0.24]	[0.03]	[0.04]	[0.04]	[0.03]
Dep. Var. Mean	0.13	0.13	0.13	0.16	0.16	0.16	0.16
Observations	7488	7481	7456	861	860	858	858
R^2	0.02	0.03	0.29	0.06	0.07	0.12	0.14
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Party FEs	No	Yes	Yes	No	Yes	Yes	Yes
Municipality characteristics	No	No	Yes	No	No	Yes	Yes
Audit characteristics	No	No	No	No	No	No	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows difference-in-differences estimates for the probability that the FISM funds of a municipality are being audited (columns 1–3) and for the fraction of FISM funds that are misspent (columns 4–6). Municipality characteristics are the state GDP, the mayor's margin of victory, and predetermined municipality characteristics interacted with year dummies. Audit characteristics are a dummy variable indicating whether the municipality was audited the year before, the fraction of municipalities audited in the state, and the fraction of FISM funds audited from the universe of FISM funds received by the municipality in the fiscal year. The mean of the dependent variable is computed using the prereform period. Standard errors clustered at the state level are shown in parentheses and wild bootstrap *p*-values clustered at the state level are shown in brackets.

Table 1.7: Effects on delivery of public security

	Corrupt police	Insecure neighborhood
	(1)	(2)
Panel A. Perceptions of corruption and insecurity		
Treated x Postreform	-0.00247	0.0197
	(0.0107)	(0.0178)
	[0.85]	[0.33]
Dep. Var. Mean	0.75	0.45
Observations	294,056	456,541
R^2	0.06	0.09
	Homicides	Investigations
	(1)	(2)
Panel B. Homicides and criminal investigations		
Treated x Postreform	-0.00479	1.019***
	(0.0230)	(0.293)
	[0.87]	[0.01]
Dep. Var. Mean	0.16	6.04
Observations	116,496	116,496
R^2	0.004	0.04
Municipality FEs	Yes	Yes
Year FEs	Yes	Yes
Party FEs	Yes	Yes
Covariates	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows difference-in-differences estimates for perceptions of corruption and insecurity, homicides, and criminal investigations. Panel A considers data from ENVIPE (available from 2011 onward). Column 1 reports the estimate for a dummy variable that takes the value of one if the respondent thinks the municipal police are corrupt and zero otherwise. Column 2 reports the estimate for a dummy variable that takes the value of one if the respondent thinks his/her neighborhood is insecure and zero otherwise. The estimations reported in Panel A consider the survey weights. Panel B uses administrative data. Column 1 reports the estimate for the number of homicides per 10,000 inhabitants. Column 2 reports the estimate for the number of common law criminal investigations per 10,000 inhabitants. The criminal investigations refer to crimes such as burglary, personal assault, car theft, rape, homicide, kidnapping, and financial fraud, among other related crimes that are in the jurisdiction of the municipal police. The data on homicides come from INEGI and the data on criminal investigations come from the Secretary of Public Security (available from 2011 onward). The data in Panel A are at the individual×year level, whereas the data in Panel B are at the municipality×month level. The period considered in both panels spans from 2011–2016. Finally, the estimates in Panel B consider the sample of municipalities that report data to the Secretary of Public Security. All estimations include the log of the state GDP, the mayor's margin of victory, and the following predetermined municipality characteristics interacted with time dummies: marginality index, average years of schooling, log of households with access to computers, and log of individuals with access to social security. Standard errors clustered at the state level are shown in parentheses and wild bootstrap p-values clustered at the state level are shown in brackets.

Table 1.8: Effects by strongholds

	Human capital index	Public capital index	Human and public capital index	Public Finance		Misspent resources
				Fiscal deficit	Property taxes	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated x Postreform	0.0743**	0.0582*	0.0676**	-90.824**	47.57***	-0.0779
	(0.0351)	(0.0315)	(0.0263)	(41.03)	(12.86)	(0.0585)
	[0.07]	[0.10]	[0.04]	[0.03]	[0.01]	[0.24]
Treated x Post x Stronghold	0.0820**	0.0893**	0.0850***	-119.23**	6.51	-0.1068
	(0.0352)	(0.0377)	(0.0303)	(49.52)	(19.77)	(0.0893)
	[0.04]	[0.04]	[0.02]	[0.04]	[0.74]	[0.24]
Stronghold	-0.0202	-0.0162	-0.0185	-38.50	9.36	0.0045
	(0.0453)	(0.0272)	(0.0289)	(32.61)	(11.86)	(0.0254)
	[0.72]	[0.64]	[0.63]	[0.25]	[0.54]	[0.86]
Treated x Postreform + Treated x Post x Stronghold	0.156***	0.148***	0.153***	-210.06***	54.09**	-0.1847**
	(0.0383)	(0.0340)	(0.0238)	(62.11)	(22.92)	(0.0702)
	[0.00]	[0.00]	[0.00]	[0.01]	[0.03]	[0.01]
Observations	3316	3316	3316	6475	6475	858
R^2	0.07	0.04	0.09	0.03	0.07	0.15
Municipality FEs	Yes	Yes	Yes	Yes	Yes	No
State FEs	No	No	No	No	No	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Party FEs	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows estimates of the heterogeneous average treatment effects on the treated on the equally weighted z-score indices of human and public capital (columns 1–3), on public finance outcomes (columns 4–5), and on the measure of misappropriation of public funds (column 6). The estimations in columns 1–2 have been restricted to the municipalities reporting information for all the outcomes used in both indices. The covariates in column 6 include the audit characteristics. Standard errors clustered at the state level are shown in parentheses and wild bootstrap *p*-values clustered at the state level are shown in brackets.

Table 1.9: Characteristics of mayors after the reform

Panel A. Gender and education of politicians

	Candidates	Elected candidates			
	Female	Female	Basic educ.	Undergraduate	Graduate
	(1)	(2)	(3)	(4)	(5)
Postreform election	0.153***	0.103***	-0.0185	0.0165	-0.0130
	(0.0444)	(0.0306)	(0.0325)	(0.0329)	(0.0126)
	[0.01]	[0.01]	[0.59]	[0.62]	[0.32]
Dep. Var. Mean	0.23	0.11	0.38	0.54	0.08
Observations	22323	5345	4769	4769	4769
R^2	0.06	0.03	0.01	0.01	0.006

Panel B. Previous employment of elected candidates

	Government	Politics	Private sector	Business owner	First job	Another job
	(1)	(2)	(3)	(4)	(5)	(6)
Postreform election	0.0532**	0.0111	-0.0103	-0.0680**	0.0465**	-0.0324*
	(0.0246)	(0.0144)	(0.0109)	(0.0293)	(0.0183)	(0.0159)
	[0.05]	[0.45]	[0.45]	[0.05]	[0.00]	[0.07]
Dep. Var. Mean	0.39	0.07	0.06	0.37	0.03	0.09
Observations	4603	4603	4603	4603	4603	4603
R^2	0.007	0.01	0.006	0.01	0.02	0.007
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
State Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows associations between the postreform elections and different observable characteristics of the candidates and elected mayors. Column 1 of Panel A refers to estimates for the pool of candidates. Columns 2–5 of Panel A and 1–6 of Panel B show estimates for characteristics of the elected mayors. The number of observations varies across group of characteristics due to data availability. Each dependent variable takes the value of one if the mayor satisfies the specified characteristic and zero otherwise. Basic education groups mayors with no education up to technical education; undergraduate groups mayors with incomplete and complete undergraduate degree; finally, graduate groups mayors with incomplete and complete graduate degrees. Postreform election takes the value of one if the election is postreform and zero otherwise. The estimation procedure consists of two steps: (i) regressing the outcome on state-specific election trends, and (ii) regressing the residuals from step (i) on the postreform election dummy, state fixed effects, and log of GDP. Clustered standard errors at the state level are shown in parentheses and wild bootstrap p-values clustered at the state level are shown in brackets.

1.A Appendix: A Career Concerns Model with Investments

This section lays out a career concerns model where the role of elections is to select competent politicians. As is standard in career concerns models, the agent (politician) wants to maximize the principal's (citizens) perception of his ability (Holmström, 1999). In the model, the incumbent chooses public policy to appear competent to voters (signal jamming incentives). However, because some policies are long-lasting, he also cares about how these policies affect his expected future payoffs. The career concerns framework was first introduced to political agency by Persson and Tabellini (2000), and extended by Alesina and Tabellini (2007, 2008), Bonfiglioli and Gancia (2013), Brollo et al. (2013), and Martinez (2020). This section builds on the work of Persson and Tabellini (2000) and Bonfiglioli and Gancia (2013), and complements the career concerns framework in political agency by studying the role of political time horizons on the choice of long-lasting policies.

1.A.1 Model setup

The economy and the government. Consider an economy with two periods $t = \{1, 2\}$. The economy consists of a mass 1 of citizens who live in both periods, discount the future by $\beta \in (0, 1]$, and derive utility from the consumption of public goods. The expected utility of the representative citizen is

$$W = \mathbb{E}(g_1 + \beta g_2)$$

In both periods, there is an exogenous source of government revenue of size τ.[37] In the first period, the revenue can be allocated to rents r_1, government investments i, savings s, or public goods g_1. In the second period, the government inherits the investments i and savings s and allocates the resources to the provision of public goods g_2 or rents r_2. The technologies for

[37] While in the Mexican context mayors can collect local revenue through property taxes, as explained in Section 3 of the main text, the majority of resources of the municipal governments come from fiscal transfers from the upper layers of government.

public goods provision in periods 1 and 2 take the form:

$$g_1 = \theta + f(i) + \tau - r_1 - \omega i - s \tag{1.A.1}$$

$$g_2 = \theta + f(i) + \tau + h(s) - r_2 \tag{1.A.2}$$

where $\theta > 0$ is the ability of the incumbent to provide public goods, which is fixed across periods.[38] A higher value of θ reflects a more competent politician, because the same resources yield more public goods.[39] f and h are continuous, concave functions with $f' > 0$, $f'' < 0$, $f'(0) = 0$, $h' > 0$, $h'' < 0$, and $h'(0) = 0$. The parameter $\omega > 0$ is the unit cost of government investments. The incumbent can make investments at the beginning of period 1 to increase the provision of public goods at the ends of both periods. These types of investments are the ones I consider in the empirical analysis, which increase the stock of municipality employees and public capital used to provide public goods. Moreover, they are characterized for delivering some rewards in the short run but, importantly, have a long-lasting impact. Alternatively, one could think of government investments with rewards only in the long run, and the predictions of the model would be similar. However, the current formulation allows the incumbent to increase government investments to get rewarded in the ballot.

Politicians and rents. At the beginning of period 1, a politician is randomly drawn from the pool of citizens, and before the elections (which will be described below), an opponent is drawn at random from the same pool of citizens. Politicians have ability θ that is drawn at random uniformly from a distribution with mean 1 and density ξ. The realization of θ is unknown to politicians and citizens, but both have common knowledge about its distribution and that ability is a permanent feature of politicians.

As is standard in these types of models, a politician cares about rents. However, I also consider the possibility that he cares about social welfare, but derives utility only when he is the one delivering public goods and not his opponent. This reflects the idea that politicians may also

[38] The linearity assumption in these types of models has been used by other authors as well (see, for example, Alesina and Tabellini (2007)). The linearity makes the algebra more tractable.

[39] Alternatively, one could think of the ability of the politician as a quality that reduces the unit cost of public goods. Besley and Smart (2007) use this formulation in a model of the political economy of public finance to reflect the idea that the unit cost of providing public goods is decreasing in the ability of politicians.

care about reputation building while in office and they do so by delivering public goods. The expected utility of a politician is

$$V = \mathbb{E}(g_1) + R + H(r_1) + p\beta(R + H(r_2) + \mathbb{E}(g_2)) \tag{1.A.3}$$

where p is the perceived probability of the incumbent of getting re-elected. With probability p, the incumbent will stay in office and deliver public goods in period 2. R are wages and other nonpecuniary benefits of holding office, and $H(\cdot)$ is a continuous, well-behaved concave function with $H' > 0$ and $H'' < 0$.

Voting. Citizens have ideology preferences δ for the incumbent. The parameter δ is drawn at random uniformly from $[-\eta, \eta]$, where η is sufficiently high enough such that the public goods delivered by the incumbent are always positive and the probability of re-election does not exceed 1 (which is defined below). A representative citizen is indifferent between $\mathbb{E}[g_2] + \delta = \mathbb{E}[g_2^o]$, where $\mathbb{E}[g_2^o]$ is the expected public goods that would be delivered by the opponent running against the incumbent in the elections at the end of period 1. I assume that before choosing policies, the incumbent observes the realization of δ, which determines the election. Unlike in the standard probabilistic voting model, when choosing policies the incumbent has no uncertainty about the realization of δ. This assumption allows me to model strongholds—which are places where citizens have ideology preferences for the incumbent—and derive comparative statics that guide the empirical analysis.

Timing. The timing of events is as follows:

1. At the beginning of period 1, δ is realized. Without knowing his level of competence, the incumbent chooses r_1, i, and s.

2. θ is realized and the level of public good g_1 is determined according to A.1. Citizens observe g_1, but cannot observe θ, r_1, i, and s.

3. Elections are held at the end of period 1. If the incumbent is re-elected, his level of competence remains constant. If he is not re-elected, then the level of competence of the winner is drawn at random from the same distribution.

4. In period 2, the winner of the elections inherits i and s, chooses rents r_2, and delivers public goods according to A.2.

1.A.2 Equilibrium

Given the timing of events, the model can be solved by backward induction. In period 2, whoever is in office inherits investments i and savings s, and solves the following problem:

$$\begin{aligned} \max_{r_2} \quad & R + H(r_2) + g_2 \\ \text{s.t.} \quad & g_2 = \theta + f(i) + \tau + h(s) - r_2 \end{aligned} \tag{1.A.4}$$

Assuming an interior solution, the first order condition of this problem leads to $H'(r_2) = 1$. Let r_2^* be the level of rents in period 2 satisfying this equality. Then, public goods g_2 are residually determined according to A.2: $g_2^* = \theta + f(i) + \tau + h(s) - r_2^*$. This behavior is observed irrespective of who is in office in the second period.

On expectation, an opponent running against the incumbent has a level of competence equal to 1. Therefore, given the ideology preferences δ, in period 2 citizens are better off electing the incumbent if and only if $\theta + f(i) + \tau + h(s) - r_2^* + \delta \geq 1 + f(i) + \tau + h(s) - r_2^*$. Thus, in the elections at the end of period 1, citizens elect the incumbent if and only if:

$$E[\theta|g_1] \geq 1 - \delta \tag{1.A.5}$$

Using equation A.1, it is possible to express $E[\theta|g_1] = g_1 - f(i^e) - \tau + r_1^e + \omega i^e + s^e = \theta + f(i) - f(i^e) + (r_1^e - r_1) + (\omega i^e - \omega i) + (s^e - s)$, where r_1^e is the expectation of citizens about the rents extracted by the incumbent in period 1, i^e is their expectation about the investments made by the incumbent, and s^e is their expectation about savings. Using this, the incumbent's probability of

re-election is

$$\begin{aligned} p &= Pr[\theta + f(i) - f(i^e) + (r_1^e - r_1) + (\omega i^e - \omega i) + (s^e - s) \geq 1 - \delta] \\ &= 1 - Pr[\theta < 1 - \delta + f(i^e) - f(i) + (r_1 - r_1^e) + (\omega i - \omega i^e) + (s - s^e)] \\ &= \frac{1}{2} + \xi - \xi[1 - \delta + f(i^e) - f(i) + (r_1 - r_1^e) + (\omega i - \omega i^e) + (s - s^e)] \end{aligned} \tag{1.A.6}$$

where the third equality follows from the properties of the uniform distribution.

Note that the probability of re-election is a decreasing function of rents and savings. This is because these policies decrease the available resources for public goods, from which citizens infer the ability of the incumbent. In contrast, the probability of re-election can be an increasing or decreasing function of investments, depending on the size of the marginal product of investments. This is because while investments allow the incumbent to provide more public goods, they also reduce the available resources for public goods. If $f'(i) > \omega$, then it is increasing; otherwise, it is decreasing. Specifically, we have

$$\frac{\partial p}{\partial r_1} = \frac{\partial p}{\partial s} = -\xi < 0 \tag{1.A.7}$$

$$\frac{\partial p}{\partial i} = -\xi[\omega - f'(i)] \tag{1.A.8}$$

Having determined the optimal voting strategy, in period 1 the incumbent will set policies by solving the following problem:

$$\begin{aligned} \max_{r_1, i, s} \ & 1 + f(i) + \tau - r_1 - \omega i - s + R + H(r_1) + p\beta[R + H(r_2^*) + 1 + f(i) + \tau + h(s) - r_2^*] \\ \text{s.t.} \ \ p &= \frac{1}{2} + \xi - \xi[1 - \lambda(\delta) + f(i^e) - f(i) + (r_1 - r_1^e) + (\omega i - \omega i^e) + (s - s^e)] \end{aligned}$$

Assuming an interior solution, the first order condition with respect to rents is

$$H'(r_1) = 1 - \frac{\partial p}{\partial r_1}\beta[R + H(r_2^*) + 1 + f(i) + \tau + h(s) - r_2^*] \tag{1.A.9}$$

The left-hand side of equation A.9 denotes the marginal utility of a unit increase in private rents. The right-hand side is the marginal cost of private rents, which has two parts. The first one is

the marginal social cost due to the foregone resources, and the second one is the marginal private cost, which is composed of the "electoral threshold" $\frac{\partial p}{\partial r_1}$ and the "value of holding office" in the next period $[R + H(r_2^*) + 1 + f(i) + \tau + h(s) - r_2^*]$.

Similarly, the first order condition with respect to investments is

$$(1 + p\beta)f'(i) \ = \omega - \frac{\partial p}{\partial i}\beta[R + H(r_2^*) + 1 + f(i) + \tau + h(s) - r_2^*] \tag{1.A.10}$$

The left-hand side of equation A.10 is the marginal benefit of investments, which comes from the marginal increase in public goods in period 1 and the discounted marginal increase in public goods in period 2. The latter is the "longer horizon effect," which is the marginal benefit of the inherited investments in the next period discounted by the probability of staying in office. The right-hand side of equation A.10 is equal to the marginal cost of investments, which has two parts. The first one is the marginal social cost in terms of current public goods. The second part is the marginal private cost, which is composed of the "electoral threshold" in terms of investments and the "value of holding office."

Finally, the first order condition with respect to savings is

$$p\beta h'(s) = 1 - \frac{\partial p}{\partial s}\beta[R + H(r_2^*) + f(i) + \tau + h(s) - r_2^*)] \tag{1.A.11}$$

The left-hand side of equation A.11 is the discounted marginal benefit of savings, i.e., the benefits from savings due to the "long-horizon effect." The right-hand side is the marginal cost of savings, which like equations A.9 and A.10 has two parts: the marginal social cost in terms of foregone public goods in period 1 and the marginal private costs.

Equations A.9–A.11 characterize the best responses of the incumbent as functions of citizens' expectations of rents, investments, and savings. In equilibrium citizens are not fooled, so we can impose $r_1 = r_1^e$, $i = i^e$ and $s = s^e$.[40] Imposing this equilibrium condition, from A.6 the probability of re-election becomes $p = \frac{1}{2} + \xi\delta$. Notice that the equilibrium probability depends on the ideology preferences for the incumbent: the more popular the incumbent, the higher the likeli-

[40] This comes from the fact that the equilibrium has to be a fixed point.

hood that he will get re-elected. Moreover, while in equilibrium the probability of re-election does not depend on the choice variables, off the equilibrium path the incumbent can choose policies to try to "fool" citizens and increase his likelihood of re-election.

Using the equilibrium condition, equations A.9-A.11 become

$$H'(r_1) = 1 + \xi\beta[R + H(r_2^*) + 1 + f(i) + \tau + h(s) - r_2^*] \tag{1.A.12}$$

$$\Big[1 + \Big(\frac{1}{2} + \xi\delta\Big)\beta\Big] f'(i) = \omega + \xi[\omega - f'(i)]\beta[R + H(r_2^*) + 1 + f(i) + \tau + h(s) - r_2^*] \tag{1.A.13}$$

$$\Big[\frac{1}{2} + \xi\delta\Big]\beta h'(s) = 1 + \xi\beta[R + H(r_2^*) + 1 + f(i) + \tau + h(s) - r_2^*] \tag{1.A.14}$$

Equations A.12–A.14 characterize the solution to the incumbent's problem.

1.A.3 The model without re-election

Now consider a model where mayors are lame ducks and have no prospect of re-election due to an institutional one-term limit. The economy and the government are as before, with the incumbent in each term facing the technologies for public goods A.1 and A.2. Politicians, citizens, and the information set are the same as before. The difference, however, is that with term limits, $p = \frac{\partial p}{\partial r_1} = \frac{\partial p}{\partial i} = \frac{\partial p}{\partial s} = 0$.

Hence, the solution to the incumbent's problem in period 1 is characterized by the following equations:

$$H'(r_1^{TL}) = 1 \tag{1.A.15}$$

$$f'(i^{TL}) = \omega \tag{1.A.16}$$

$$s^{TL} = 0 \tag{1.A.17}$$

Public goods are residually determined according to A.1.

Proposition 1. *a) The equilibrium rents with re-election possibilities are strictly smaller than the equilibrium rents with a one-term limit: $r_1 < r_1^{TL}$. b) The equilibrium investments with re-election possibilities are strictly higher than the equilibrium investments with a one-term limit: $i > i^{TL}$. c) The*

equilibrium savings with re-election possibilities are strictly higher than the equilibrium savings with a one-term limit: $s > s^{TL}$

Proof. To proof part a) of the proposition, first note that $\xi\beta[R + H(r_2^*) + 1 + f(i) + \tau + h(s) - r_2^*] > 0$. By comparing equation A.12 with equation A.15, and due to the concavity of $H(\cdot)$, the result is apparent.

To proof part b) of the proposition, first note that due to the concavity of $f(\cdot)$, part b) holds iff $f'(i) < f'(i^{TL})$. After some algebra, we can rewrite equation A.13 as follows:

$$f'(i) \;=\; \frac{\omega + \xi\omega\beta[R + H(r_2^*) + 1 + f(i) + \tau + h(s) - r_2^*]}{1 + \frac{1}{2}\beta + \xi\beta[R + H(r_2^*) + 1 + f(i) + \tau + h(s) - r_2^*]} \tag{1.A.18}$$

Thus, comparing A.16 with A.18, we get

$$\begin{aligned} \frac{\omega + \xi\omega\beta[R + H(r_2^*) + 1 + f(i) + \tau + h(s) - r_2^*]}{1 + \frac{1}{2}\beta + \xi\beta[R + H(r_2^*) + 1 + f(i) + \tau + h(s) - r_2^*]} &< \omega \\ 0 &< \frac{1}{2}\beta\omega \end{aligned} \tag{1.A.19}$$

The last inequality implies that $f'(i) < \omega = f'(i^{TL})$, from which the result is apparent.

Finally, part c) of the proposition holds by noting that for A.14 to hold, savings s need to be greater than zero. □

Part a) of Proposition 1 is consistent with the main idea in political agency models: elections discipline the corrupt behavior of politicians (Persson and Tabellini, 2000; Besley, 2006). Intuitively, with re-election prospects the incumbent tries to appear competent to citizens to get rewarded in the ballot. Hence, he reduces rent extraction and delivers more public goods. Parts b) and c) of Proposition 1 are consistent with the idea that with longer political time horizons, politicians invest and save more, because with some probability they will stay in office and enjoy the future rewards of these policies.

1.A.4 Public policy and ideology preferences δ

In this subsection, I derive predictions with respect to the ideology preferences. For simplicity, I consider that the incumbent can only choose rents or investments and cannot save. Under this assumption, $s = 0$ and $h(s) = 0$ in equations A.12 and A.13.

Proposition 2. *In equilibrium, investments are an increasing function of the ideology preferences:* $\frac{\partial i}{\partial \delta} > 0$, *and rents are a decreasing function of the ideology preferences:* $\frac{\partial r_1}{\partial \delta} < 0$.

Proof. Using the implicit function theorem and equation A.13, we have

$$\begin{aligned} \frac{\partial i}{\partial \delta} \;=\; -\underbrace{[\xi\beta f(i)]}_{>0} \times \Bigg[& \underbrace{\big(1 + (1/2 + \xi\delta)\beta\big) f''(i)}_{<0} \\ & + \underbrace{\xi f''(i)\beta[R + H(r_2^*) + 1 + f(i) + \tau + h(s) - r_2^*]}_{<0} \\ & - \xi[\omega - f'(i)]\beta f'(i) \Bigg]^{-1} \end{aligned} \tag{1.A.20}$$

From part b) of Proposition 1, we know that $f'(i) < \omega$. Using this result in equation A.20, we get $-\xi[\omega - f'(i)]\beta f'(i) < 0$. Hence, $\frac{\partial i}{\partial \delta} > 0$.

Finally, using the implicit function theorem and equation A.12, we have

$$\frac{\partial r_1}{\partial \delta} \;=\; \underbrace{\xi\beta f'(i)\frac{\partial i}{\partial \delta}}_{>0}\underbrace{[H''(r_1)]}_{<0}^{-1} < 0 \tag{1.A.21}$$

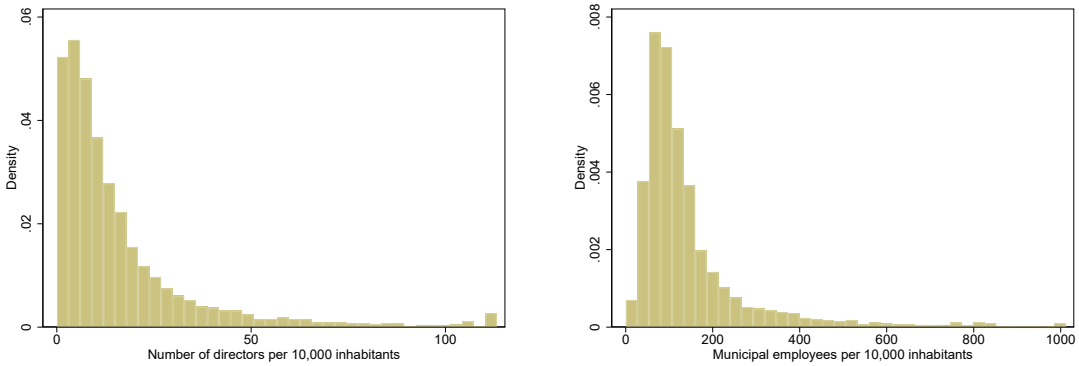
□

The first part of Proposition 2 is intuitive. Investments are higher when the incumbent has a higher perceived probability of staying in office. This is because it is more likely that he will inherit the investments and enjoy their future returns. The second part of Proposition 2 shows that when the value of holding office goes up (through the positive changes in investments), the incumbent reduces rents to increase the probability of remaining in office.

1.A.5 Discussion

This model highlights the main incentives in my setting, but it comes with some caveats that we can address. First, I do not model the role of political parties. It has been argued that in party-centered systems like Mexico, political parties play a key role in the types of policies chosen by politicians. For instance, political parties may have longer time horizons than individual politicians and might, therefore, discipline their behavior. Ultimately, whether the time horizon of political parties matters more or less than the time horizon of the individual politician is an empirical question (Besley and Case, 1995), which I answer in this paper. If the time horizon of political parties matters more than the time horizon of individual politicians, then we shouldn't expect to see any effect of extending term limits on the incumbents' behavior. Second, this model does not consider how parties select politicians or who decides to enter politics. In the empirical section I address these two issues: (i) I compare outcomes of first-term mayors with long horizons against outcomes of first-term mayors with short horizons within the same party, and (ii) I re-estimate the treatment effects for mayors who plausibly were not induced to enter politics by the reform.

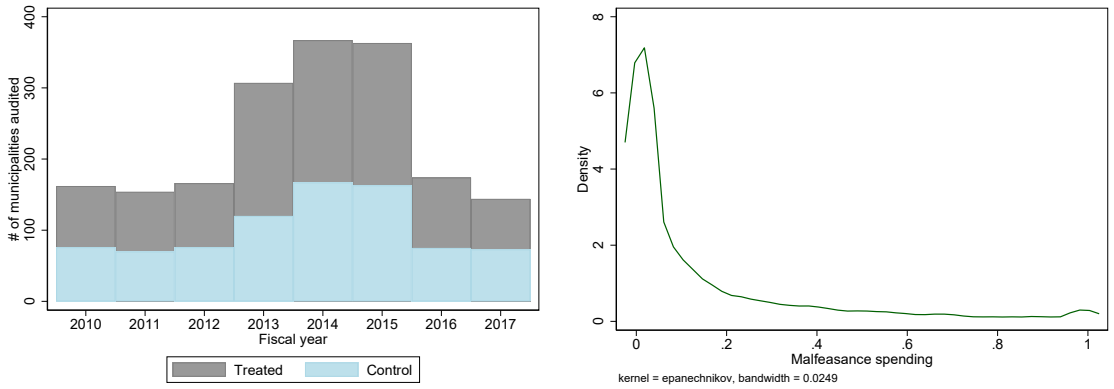
1.B Appendix: Additional Figures and Tables



(a) Directors (b) Public workers

Figure 1.B.1: Public administration

Notes: The figure plots distributions of the staff in the municipal public administration. Panel (a) plots the distribution of the number of senior directors in the public administration. Panel (b) plots the size of the number of workers in the public administration per 10,000 inhabitants. The data come from the Municipal Government Census conducted by the National Institute of Statistics (INEGI) in 2011, 2013, 2015 and 2017, with the information referring to the previous calendar years. The variables have been winsorized to the 99% percentile to deal with outliers.



(a) Audited municipalities (b) Misspent resources: Non-parametric density

Figure 1.B.2: Audits by the ASF

Notes: The figure plots the number of municipalities audited by fiscal year (Panel (a)) and the density of the measure of malfeasance spending for the fiscal years 2010–2017 (Panel (b)). The treated group in Panel (a) refers to municipalities with elections in 2015, whereas the control group refers to municipalities with elections in 2016 or after. There are 7 audits that report the misspent resources fraction greater than one; this is due to the financial returns (interest) that the FISM should have generated if they would not have been spent. The value of these 7 observations are winsorized to one.

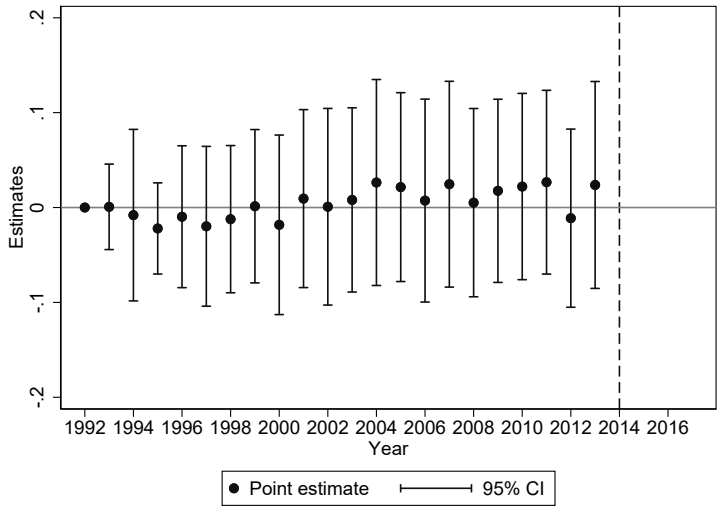


Figure 1.B.3: Differences in the night lights intensity

Notes: The figure plots the differences in night lights from 1993 to 2013 relative to 1992 between municipalities with elections in 2015 and municipalities with elections after that year. The outcome variable is the log of the sum of the median night light pixels in the municipality. Standard errors are clustered at the state level.

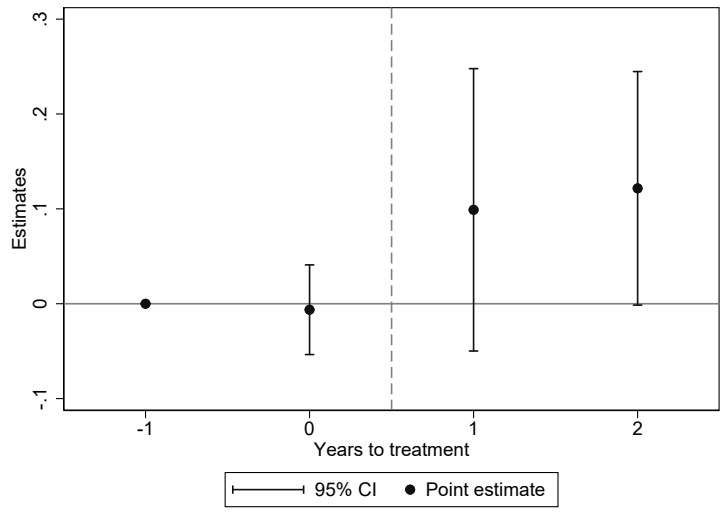
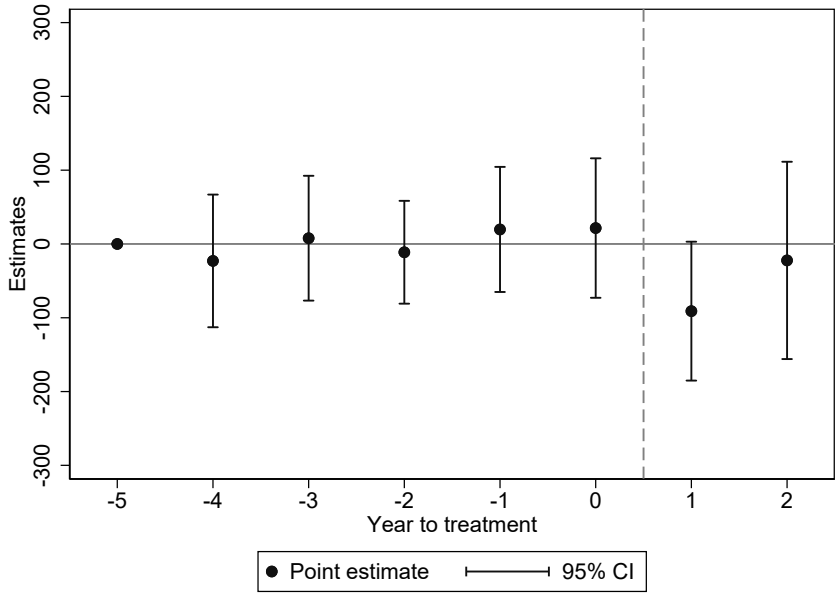
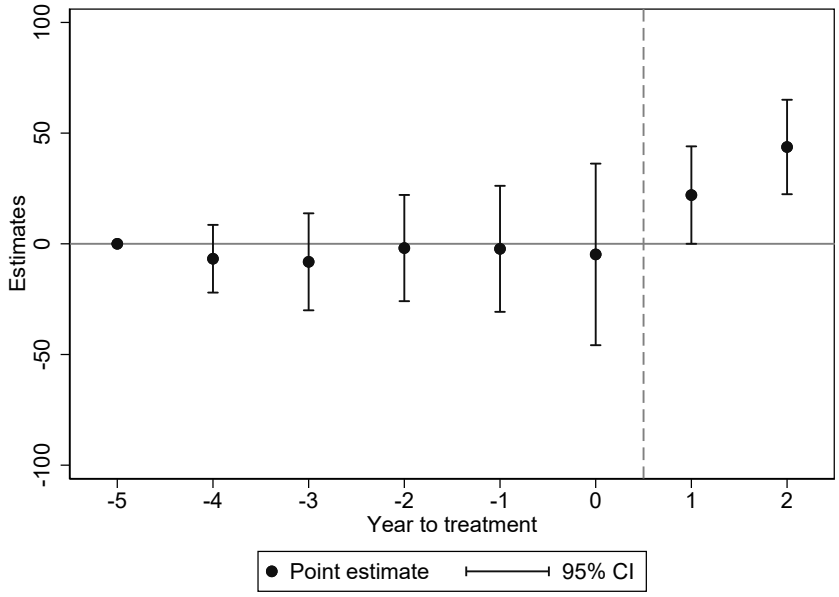


Figure 1.B.4: Night lights intensity

Notes: The figure plots the estimates of the effect of re-election on the log of the median night light per pixel within a year in each municipality. The model exploits the staggered adoption of treatment and uses data from 2014 to 2017. The VIIRS satellite reports data starting from 2014. Standard errors are clustered at the state level.



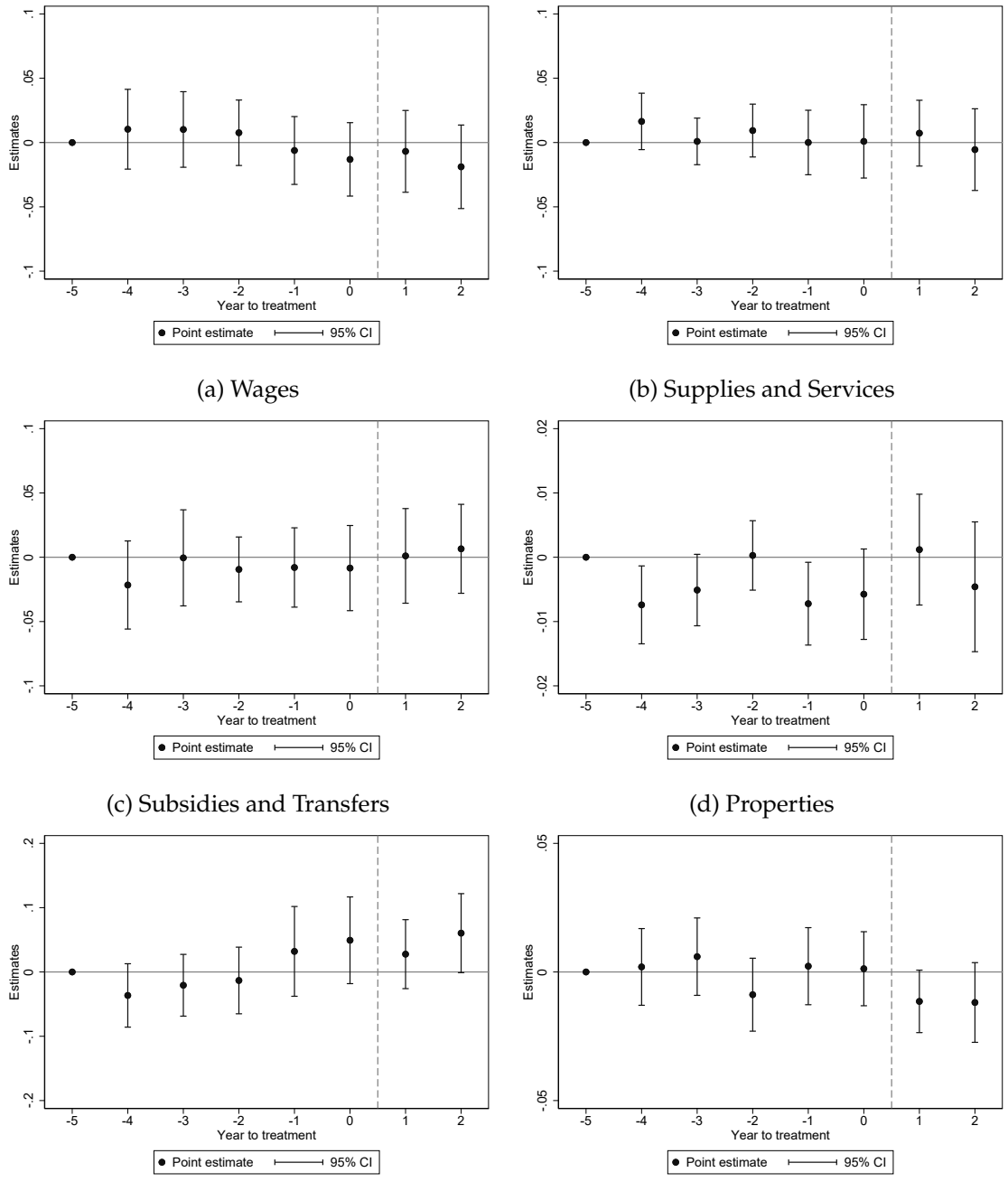
(a) Fiscal Deficit



(b) Property Taxes

Figure 1.B.5: Sources of revenue

Notes: The figure plots the estimates of an event-study specification that considers the staggered adoption of treatment, and the 95% confidence intervals for the two main sources of revenue that are in control of local governments. Panel (a) plots the estimates for the fiscal deficit. Panel (b) plots the estimates for the property tax revenue. The estimations include the log of the state GDP, the mayor's margin of victory, and the following pre-determined municipality characteristics interacted with time dummies: marginality index, average years of schooling, log of households with access to computers and log of individuals with access to social security. Outcome variables are in pesos of 2018. The vertical dashed line corresponds to the year when the reform was implemented. Standard errors are clustered at the state level.



(a) Wages

(b) Supplies and Services

(c) Subsidies and Transfers

(d) Properties

(e) Public Investment

(f) Other

Figure 1.B.6: Composition of expenditures

Notes: The figure plots the estimates of an event-study specification that considers the staggered adoption of treatment, and the 95% confidence intervals for aggregate measures of expenditures as a fraction of total expenditures. The estimations include the log of the state GDP, the mayor's margin of victory, and the following pre-determined municipality characteristics interacted with time dummies: marginality index, average years of schooling, log of households with access to computers and log of individuals with access to social security. Standard errors are clustered at the state level.

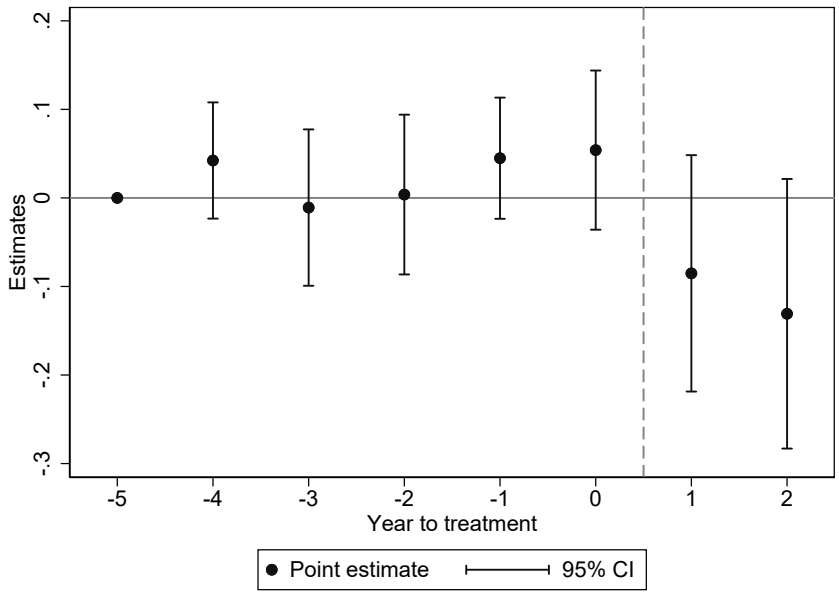


Figure 1.B.7: Effects on misspent resources:
exploiting the staggered adoption of treatment

Notes: The figure plots difference-in-differences estimates and its 95% confidence intervals of the effects of the reform on misspent resources that exploit the staggered adoption of treatment. The specification controls for state, year and political party fixed effects. In addition, it controls for time varying and pre-determined municipality characteristics, with the pre-determined municipality characteristics interacted with year dummies. See Table 1.3 for a description of the covariates. The standard errors are clustered at the state level.

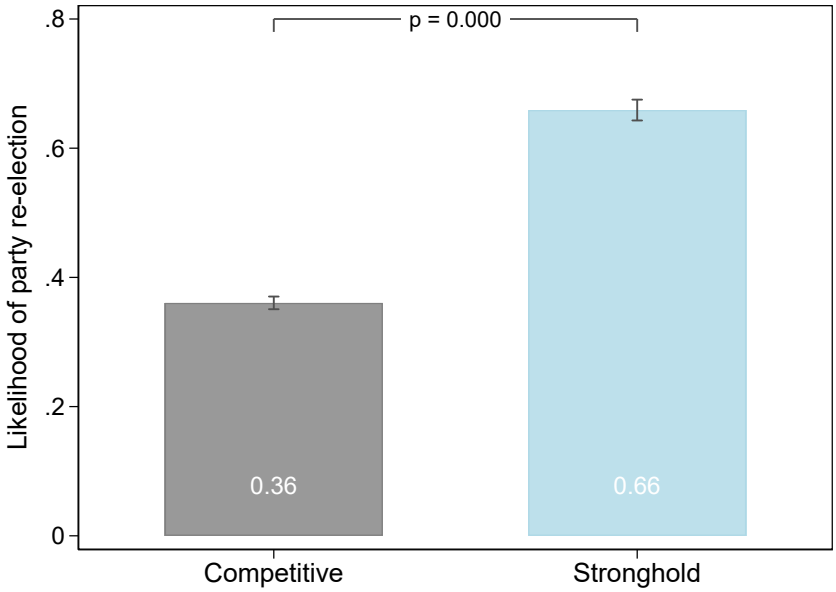
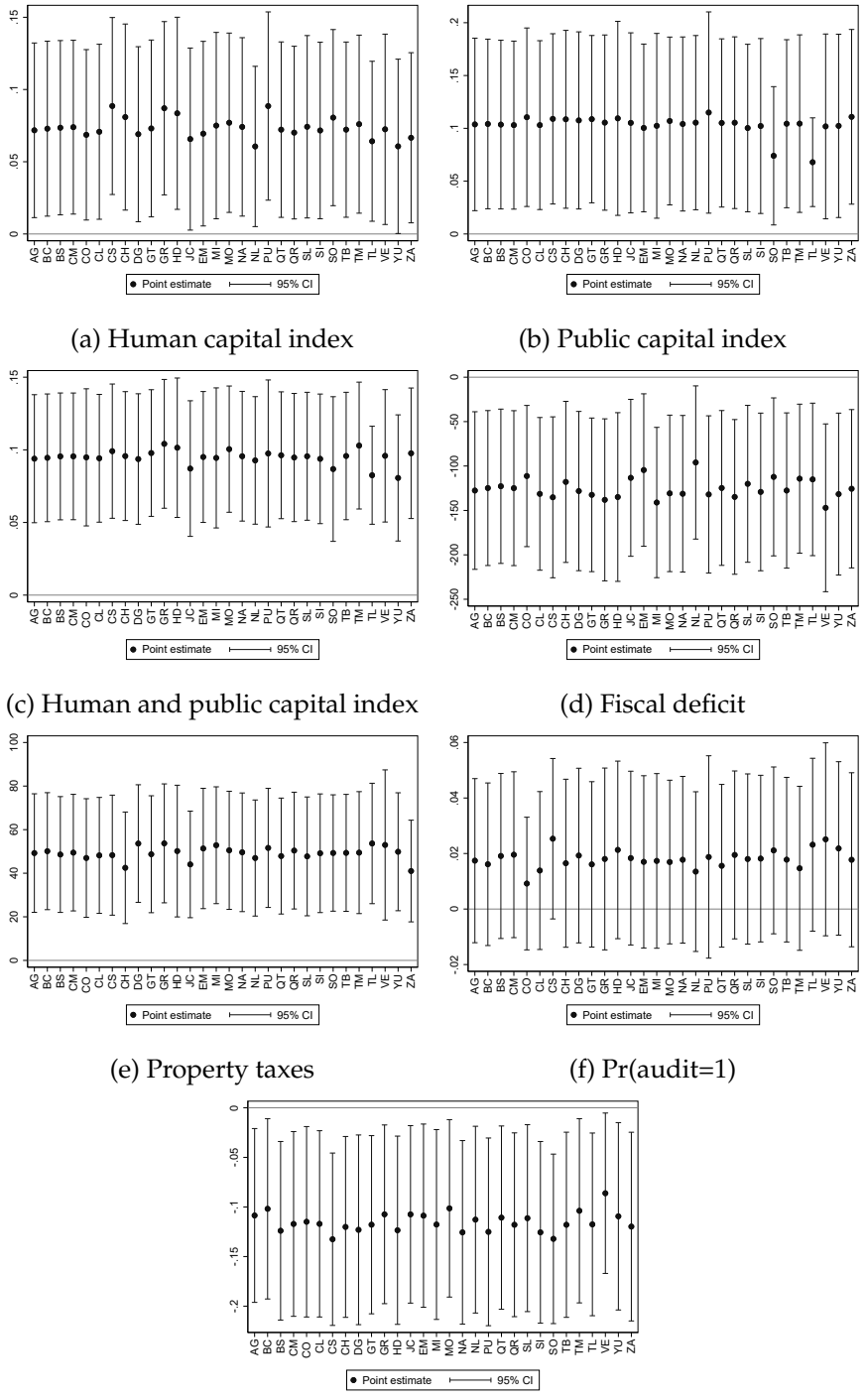


Figure 1.B.8: Prereform incumbent party re-election

Notes: The figure plots the likelihood of party re-election in competitive places and in strongholds before the 2014 reform was enacted. Standard errors are robust.



(a) Human capital index

(b) Public capital index

(c) Human and public capital index

(d) Fiscal deficit

(e) Property taxes

(f) Pr(audit=1)

(g) Misspent resources

Figure 1.B.9: Leave-one-out robustness exercises

Notes: The figure plots the estimates of equation 1.2 and its 95% confidence intervals for the human capital index (Panel (a)), public capital index (Panel (b)), human and public capital index (Panel (c)), the fiscal deficit (Panel (d)), the property tax revenue (Panel (e)), the probability that a municipality is audited (Panel (f)), and misspent resources (Panel (g)), by excluding from the estimations the municipalities in the state denoted in the x-axis. The estimations from panels a-e consider municipality fixed effects, whereas the estimations from panels f and g consider state fixed effects. Standard errors are clustered at the state level.

Table 1.B.1: Estimates of the differences in pre-trends

Variables	Estimate of ϕ	Std. Err.	Wild p-value
	(1)	(2)	(3)
Municipality employees			
Directors	-1.29	0.98	0.30
Public workers	-2.41	5.85	0.74
Public security			
Police officers	-1.44	1.06	0.25
Male police officers	-1.46	0.96	0.19
Female police officers	0.02	0.14	0.90
Basic education police officers	-1.42	0.98	0.23
Higher education police officers	0.06	0.06	0.41
Police stations	-0.03	0.08	0.79
Jail cells	-0.26	0.11	0.03
Piped water and sewerage			
House connections	9.24	145.54	0.95
Sewerage	0.00	0.01	0.73
Sewerage water treatment	-0.03	0.05	0.58
Waste disposal			
Waste collection vehicles	-0.01	0.02	0.62
Sources of revenue			
Income	288.79	150.5	0.16
Fiscal deficit	-6.71	32.45	0.88
Taxes	2.36	10.82	0.85
Property taxes	12.65	9.30	0.22
Fees and tariffs	-12.82	9.23	0.20
Other local revenue	-73.1	61.97	0.37
Earmarked transfers	311.13	112.46	0.06
Unmarked transfers	25.51	74.91	0.77
Initial income	63.52	39.58	0.21
Other income	-21.1	41.93	0.79
Public expenditures			
Wages	-0.01	0.01	0.55
Supplies and services	-0.01	0.01	0.39
Transfers and subsidies	0.00	0.02	0.88
Assets	0.00	0.00	0.89
Public investment	0.04	0.03	0.21
Other expenditures	0.00	0.00	0.73
Budget surplus	-0.02	0.01	0.26
Municipal audits			
Pr(audit=1)	0.00	0.03	0.99
Pr(audit previous year=1)	0.01	0.02	0.61
Fraction of munis. audited in the state	0.02	0.03	0.60
Sample of resources audited	0.01	0.04	0.89
Malfeasance spending	-0.01	0.04	0.77

Notes: The table shows estimates of ϕ of equation 1.3, its standard error and its wild bootstrap p-value, for the main outcomes considered in this paper. Column 1 reports the estimate of ϕ, column 2 reports the estimate for its standard error, and column 3 reports the wild bootstrap p-value clustered at the state level. The estimations that consider the outcomes reported on a yearly basis (sources of revenue, public expenditures, and municipal audits) consider data for 2010–2016, whereas estimations for the other outcomes consider data for 2010, 2012, 2014, and 2016. Standard errors in parentheses are clustered at the state level. The estimations control for year, municipality, party fixed effects and municipality characteristics interacted with year dummies. See Table 1.3 for a description of the covariates.

Table 1.B.2: Effects on standardized outcomes

	Municipality Employees		Police Officers				
	Directors (1)	Public Workers (2)	Total (3)	Male (4)	Female (5)	Basic Education (6)	Higher Education (7)
Panel A. Human Capital							
Treated x Postreform	0.0472	0.0991**	0.0418	0.00446	0.233***	0.0270	0.149***
	(0.0440)	(0.0434)	(0.0630)	(0.0601)	(0.0675)	(0.0531)	(0.0467)
	[0.37]	[0.07]	[0.55]	[0.96]	[0.00]	[0.66]	[0.01]
Observations	7026	7037	7082	7082	7082	5380	5376
R^2	0.0519	0.0798	0.0645	0.0900	0.0867	0.0592	0.0370

	Public security		Lighting	Piped water and Sewerage	Waste disposal
	Police Stations (1)	Jail Cells (2)	asinh(Street light posts) (3)	House connections (4)	asinh (Vehicles) (5)
Panel B. Public Capital					
Treated x Postreform	0.0572**	0.0600	0.249*	0.0624	0.0616**
	(0.0270)	(0.0505)	(0.133)	(0.0770)	(0.0284)
	[0.08]	[0.36]	[0.04]	[0.52]	[0.11]
Observations	7267	7195	3666	7034	7307
R^2	0.0100	0.0171	0.0431	0.131	0.0238
Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Party FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows differences-in-differences estimates for the standardized outcomes considered in the indices of Table 1.3. See the notes on Table 1.3 for a description of the outcomes and the covariates. Standard errors clustered at the state level are shown in parentheses and wild bootstrap p-values clustered at the state level are shown in brackets.

Table 1.B.3: Effects on indices of capacity

	Human Capital		Public Capital		Human and Capital	
	z-score	PCA	z-score	PCA	z-score	PCA
	(1)	(2)	(3)	(4)	(5)	(6)
Treated x Postreform	0.0733**	0.0593	0.103**	0.109**	0.0951***	0.107***
	(0.0308)	(0.0411)	(0.0408)	(0.0502)	(0.0223)	(0.0289)
	[0.04]	[0.20]	[0.00]	[0.01]	[0.00]	[0.00]
Observations	5264	5264	3459	3459	3316	3316
R^2	0.0403	0.0466	0.0416	0.0309	0.0784	0.0579
Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes
Party FEs	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows difference-in-differences estimates for the indices considered in Table 1.3. The z-score index is the average of the treatment effects of the reform on the standardized outcomes. The PCA index weights the standardized outcomes using the eigenvector of the first component of a principal component analysis, and it has been standardized for ease of interpretation. The inputs for the indices are the standardized outcomes used in Table 1.B.2. See the notes on Table 1.3 for a description of the outcomes and the covariates. Standard errors clustered at the state level are shown in parentheses and wild bootstrap p-values clustered at the state level are shown in brackets.

Table 1.B.4: Effects on the capacity to provide public goods:
Balanced panel I

	Municipality Employees		Police Officers				
	Directors	Public Workers	Total	Male	Female	Basic Education	Higher Education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Human Capital							
Treated x Postreform	0.779	14.26**	0.740	0.172	0.568***	0.357	0.144***
	(0.994)	(6.586)	(0.862)	(0.769)	(0.166)	(0.740)	(0.0475)
	[0.51]	[0.08]	[0.43]	[0.84]	[0.00]	[0.67]	[0.01]
Dep. Var. Mean	16.04	166.2	23.74	21.65	2.100	22.79	0.620
Observations	5909	5952	6175	6175	6175	5109	5097
R^2	0.0542	0.0852	0.0671	0.0942	0.0885	0.0599	0.0359

	Public security		Lighting	Piped water and Sewerage			Waste disposal
	Police Stations	Jail Cells	asinh(Street light posts)	House connections	Sewerage	Sewage treatment	asinh (Vehicles)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel B. Public Capital							
Treated x Postreform	0.154**	0.256	0.335*	93.71	0.00323	0.00844	0.0538*
	(0.0657)	(0.226)	(0.179)	(98.36)	(0.00650)	(0.0573)	(0.0279)
	[0.05]	[0.42]	[0.04]	[0.45]	[0.65]	[0.91]	[0.14]
Dep. Var. Mean	1.970	2.740	7.370	1855.5	0.880	0.430	1.960
Observations	6771	6570	3608	6605	7132	6166	6954
R^2	0.0102	0.0173	0.0431	0.122	0.0189	0.0298	0.0253
Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Party FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows differences-in-differences estimates for the outcomes considered in Table 1.3. Each estimation restricts the sample to municipalities that report the corresponding outcome in all years for which I can observe data. See the notes in Table 1.3 for a description of the outcomes and the covariates. Standard errors clustered at the state level are shown in parentheses and wild bootstrap p-values clustered at the state level are shown in brackets.

Table 1.B.5: Effects on the capacity to provide public goods:
Balanced panel II

	Municipality Employees		Police Officers				
	Directors	Public Workers	Total	Male	Female	Basic Education	Higher Education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Human Capital							
Treated x Postreform	2.095**	16.69***	0.922	0.458	0.464***	0.900	0.114*
	(1.009)	(5.039)	(0.562)	(0.482)	(0.153)	(0.633)	(0.0607)
	[0.11]	[0.02]	[0.17]	[0.41]	[0.01]	[0.26]	[0.14]
Dep. Var. Mean	16.23	166.7	22.16	19.75	2.410	21.33	0.640
Observations	3316	3316	3316	3316	3316	3316	3316
R^2	0.0329	0.0692	0.0319	0.0335	0.0749	0.0286	0.0333
	Public security		Lighting	Piped water and Sewerage			Waste disposal
	Police Stations	Jail Cells	asinh(Street light posts)	House connections	Sewerage	Sewage treatment	asinh (Vehicles)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel B. Public Capital							
Treated x Postreform	0.148	0.420*	0.236**	28.97	-0.00215	0.0364	0.0620**
	(0.0979)	(0.231)	(0.110)	(54.81)	(0.00391)	(0.0473)	(0.0289)
	[0.21]	[0.09]	[0.04]	[0.61]	[0.63]	[0.51]	[0.08]
Dep. Var. Mean	1.950	2.670	7.380	1950.6	0.870	0.430	2.020
Observations	3316	3316	3316	3316	3312	3151	3316
R^2	0.0108	0.0371	0.0312	0.0799	0.0242	0.0168	0.0271
Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Party FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows differences-in-differences estimates for the outcomes considered in Table 1.3. Each estimation restricts the sample to municipalities that report data in all outcomes used to construct the indices. See the notes in Table 1.3 for a description of the outcomes and the covariates. Standard errors clustered at the state level are shown in parentheses and wild bootstrap *p*-values clustered at the state level are shown in brackets.

Table 1.B.6: Effects on the capacity to provide public goods: group-specific pre-trends

	Municipality Employees		Police Officers				
	Directors (1)	Public Workers (2)	Total (3)	Male (4)	Female (5)	Basic Education (6)	Higher Education (7)
Panel A. Human Capital							
Treated x Postreform	2.581**	15.85***	1.910*	1.497*	0.414*	2.439**	0.0629
	(1.211)	(5.319)	(0.985)	(0.830)	(0.222)	(1.117)	(0.0853)
	[0.09]	[0.02]	[0.07]	[0.09]	[0.11]	[0.07]	[0.53]
Dep. Var. Mean	15.58	163.9	23.70	21.59	2.110	22.65	0.610
Observations	7026	7037	7082	7082	7082	5380	5376
R^2	0.0551	0.0799	0.0661	0.0920	0.0869	0.0620	0.0373
	Public security		Piped water and Sewerage			Waste disposal	
	Police Stations (1)	Jail Cells (2)	House connections (3)	Sewerage (4)	Sewage treatment (5)	asinh (Vehicles) (6)	
Panel B. Public Capital							
Treated x Postreform	0.158	0.587**	40.11	0.0178***	0.0101	0.100**	
	(0.120)	(0.281)	(181.4)	(0.00529)	(0.0535)	(0.0376)	
	[0.27]	[0.02]	[0.85]	[0.00]	[0.87]	[0.05]	
Dep. Var. Mean	1.960	2.700	1839.1	0.880	0.410	1.960	
Observations	7267	7195	7034	7318	6889	7307	
R^2	0.0100	0.0189	0.132	0.0204	0.0275	0.0243	
Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Party FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows differences-in-differences estimates for the outcomes considered in Table 1.3. Each estimation includes group-specific trends. See the notes in Table 1.3 for a description of the outcomes and the covariates. Standard errors clustered at the state level are shown in parentheses and wild bootstrap p-values clustered at the state level are shown in brackets.

Table 1.B.7: Effects on sources of revenue:
Balanced panel

				Transfers	
	Fiscal deficit	Property Taxes	Fees and Tariffs	Earmarked	Unmarked
	(1)	(2)	(3)	(4)	(5)
Treated x Postreform	-159.2***	50.57***	-12.46	212.2	84.16
	(47.31)	(17.34)	(9.605)	(187.1)	(173.3)
	[0.02]	[0.02]	[0.21]	[0.43]	[0.68]
Dep. Var. Mean	212.9	180.8	149.2	2196.6	2311.5
Observations	5615	5615	5615	5615	5615
Municipalities	1408	1408	1408	1408	1408
R^2	0.03	0.06	0.04	0.18	0.22
Municipality FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Party FEs	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows difference-in-difference estimates for outcomes related to the sources of revenue of the municipality. All outcomes are per capita and in pesos of 2018. The mean of the dependent variable is computed using the prereform period. The estimation sample is restricted to municipalities that report public finance data in all years. Standard errors clustered at the state level are shown in parentheses and wild bootstrap *p*-values clustered at the state level are shown in brackets.

Table 1.B.8: Effects on sources of revenue:
staggered adoption of treatment

				Transfers	
	Fiscal deficit	Property Taxes	Fees and Tariffs	Earmarked	Unmarked
	(1)	(2)	(3)	(4)	(5)
Treated x Postreform	-93.33***	30.95***	-10.62	88.51	16.79
	(31.47)	(10.75)	(8.132)	(131.8)	(94.24)
	[0.01]	[0.01]	[0.25]	[0.63]	[0.89]
Dep. Var. Mean	208.6	173.9	152.2	2273.4	2336.3
Observations	12762	12762	12762	12762	12762
R^2	0.02	0.04	0.03	0.16	0.20
Municipality FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Year of term FEs	Yes	Yes	Yes	Yes	Yes
Party FEs	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows the results of estimating the following specification:

$$y_{ipet} = \lambda_i + \delta_t + \theta_p + \gamma_e + \beta \cdot Treated_{it} + \sum_j \alpha_{jk} \cdot \mathbf{1}\{j = t\} \cdot X_i' + W_{it}\Gamma + \varepsilon_{ipet}$$

where $Treated_{it}$ takes the value of one if the municipality becomes treated in year t, and zero otherwise. The parameter γ_e are year of term fixed effects. The rest of the parameters are as described in the main text. This specification uses annual data from 2010 to 2017, and exploits the staggered implementation of the reform. All outcomes are per capita and in pesos of 2018. The mean of the dependent variable is computed using the prereform period. Standard errors clustered at the state level are shown in parentheses and wild bootstrap p-values clustered at the state level are shown in brackets.

Table 1.B.9: Effects on misappropriation of public funds: group-specific pre-trends

	Pr(audit=1)			Misspent resources			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated x Postreform	-0.00335	0.00251	0.0301	-0.118*	-0.103	-0.103*	-0.106**
	(0.0631)	(0.0616)	(0.0567)	(0.0637)	(0.0620)	(0.0550)	(0.0439)
	[0.95]	[0.97]	[0.69]	[0.10]	[0.15]	[0.09]	[0.02]
Dep. Var. Mean	0.13	0.13	0.13	0.16	0.16	0.16	0.16
Observations	7488	7481	7456	861	860	858	858
R^2	0.02	0.03	0.29	0.06	0.07	0.12	0.14
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Party FEs	No	Yes	Yes	No	Yes	Yes	Yes
Municipality characteristics	No	No	Yes	No	No	Yes	Yes
Audit characteristics	No	No	No	No	No	No	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows difference-in-difference estimates for the probability that the FISM funds of a municipality are being audited (columns 1–3) and for the fraction of FISM funds that are misspent resources (columns 4–6). The estimations control for group-specific trends. Refer to the notes in Table 1.6 for a description of the variables. Standard errors clustered at the state level are shown in parentheses and wild bootstrap *p*-values clustered at the state level are shown in brackets.

Table 1.B.10: Effects on misappropriation of public funds: exploiting the staggered adoption of treatment

	Pr(audit=1)			Misspent resources				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat x Post	-0.00530	-0.00450	0.000182	-0.124***	-0.121***	-0.129***	-0.133***	-0.104*
	(0.0210)	(0.0212)	(0.0231)	(0.0414)	(0.0419)	(0.0462)	(0.0451)	(0.0562)
	[0.83]	[0.86]	[0.99]	[0.01]	[0.01]	[0.01]	[0.01]	[0.17]
Dep. Var. Mean	0.13	0.13	0.13	0.17	0.17	0.17	0.17	0.17
Observations	14976	14963	14911	1819	1818	1814	1814	1814
R^2	0.02	0.03	0.26	0.08	0.09	0.17	0.17	0.17
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of term FEs	No	No	No	No	No	No	No	Yes
Party FEs	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Municipality characteristics	No	No	Yes	No	No	Yes	Yes	Yes
Audit characteristics	No	No	No	No	No	No	Yes	Yes

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows the results of estimating the following specification:

$$y_{ipet} = \lambda_i + \delta_t + \theta_p + \gamma_e + \beta \cdot Treated_{it} + \sum_j \alpha_{jk} \cdot \mathbf{1}\{j = t\} \cdot X_i' + W_{it}\Gamma + \varepsilon_{ipet}$$

where $Treated_{it}$ takes the value of one if the municipality becomes treated in year t, and zero otherwise. The parameter γ_e are year of term fixed effects. The rest of the parameters are as described in the main text. Refer to Table 1.6 for additional notes. Standard errors clustered at the state level are shown in parentheses and wild bootstrap *p*-values clustered at the state level are shown in brackets.

Table 1.B.11: Frequency of criminal investigations: 2011–2017

Crimes	Frequency	Percentage
Thefts	4508129	38.72
Other crimes	3763229	32.32
Burglary	1605097	13.78
Personal assault	1329599	11.42
Homicides	251795	2.16
Rape	93503	0.80
Cattle raiding	44995	0.39
Theft on roads	24488	0.21
Thefts in banking institutions	14581	0.13
Kidnapping	8709	0.07

Notes: The table shows the frequency and the percentage of criminal investigations from 2011 to 2017 by type of crime. The data come from the Secretary of Security and Civilian Protection and are publicly available online.

Table 1.B.12: Municipalities with mayors that were in politics and new politicians

	New in politics			Mayor was in politics				Conditional mean	
	N (1)	Mean (2)	Std. Dev. (3)	N (4)	Mean (5)	Std. Dev. (6)	t-stat (7)	Difference (8)	t-stat (9)
Demographic characteristics									
log(population)	1222	9.83	1.38	648	9.83	1.25	-0.03	-0.15	-2.46
log(male)	1222	9.12	1.37	648	9.12	1.24	0.01	-0.15	-2.5
log(female)	1222	9.16	1.39	648	9.15	1.26	-0.07	-0.15	-2.42
log(houses)	1222	8.44	1.37	648	8.44	1.23	-0.05	-0.17	-2.72
Socioeconomic characteristics									
Marginality Index	1222	-0.22	0.93	648	-0.11	0.99	2.39	0.13	3.34
log(HH with TV)	1222	8.26	1.42	648	8.24	1.28	-0.36	-0.2	-3.02
log(HH with computer)	1220	6.14	1.97	647	6.07	1.76	-0.77	-0.34	-3.98
Avg. years of schooling	1222	6.94	1.44	648	6.77	1.36	-2.57	-0.23	-3.71
log(HH with social security)	1222	9.33	1.45	648	9.36	1.26	0.56	-0.17	-2.76
Electoral characteristics									
Margin of victory	1221	0.12	0.12	646	0.11	0.11	-1.84	-0.02	-3.13
PAN	1221	0.25	0.43	646	0.24	0.43	-0.51	0.03	1.5
PRI	1221	0.52	0.50	646	0.53	0.50	0.45	-0.03	-1.08
PRD	1221	0.09	0.29	646	0.12	0.33	1.73	0.00	-0.18
Other party	1221	0.14	0.34	646	0.11	0.31	-1.71	0.00	-0.09

Notes: The table shows summary statistics of characteristics of the municipality during the election inmediatelly preceding the reform for the group of municipalities with mayors new in politics and the group of municipalities with mayors that were either already in politics or belong to a family involved in politics. Column 7 reports the t-statistic for the difference between columns 5 and 2. Column 8 reports the difference in means conditional on state fixed effects and column 9 reports the t-statistic of this difference.

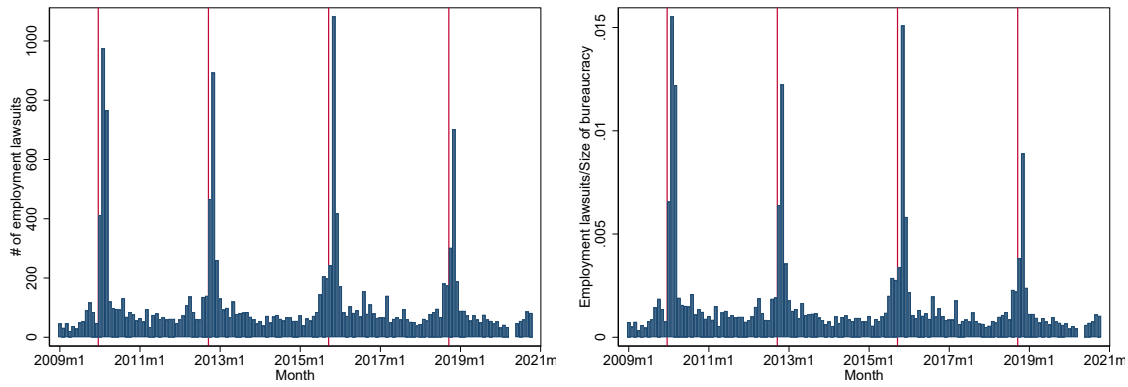
Table 1.B.13: Municipalities with mayors that were involved in politics before the reform

	Late adopter			Early adopter			
	N	Mean	Std. Dev.	N	Mean	Std. Dev.	t-stat
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Demographic characteristics							
log(population)	253	9.79	1.14	395	9.86	1.31	0.78
log(male)	253	9.08	1.13	395	9.15	1.30	0.78
log(female)	253	9.11	1.15	395	9.18	1.32	0.79
log(houses)	253	8.42	1.14	395	8.45	1.28	0.34
Socioeconomic characteristics							
Marginality Index	253	-0.14	0.90	395	-0.09	1.04	0.54
log(HH with TV)	253	8.24	1.19	395	8.24	1.33	0.07
log(HH with computer)	253	6.07	1.68	394	6.07	1.82	0.06
Avg. years of schooling	253	6.76	1.30	395	6.77	1.41	0.1
log(HH with social security)	253	9.30	1.19	395	9.40	1.31	0.99
Electoral characteristics							
Margin of victory	251	0.13	0.14	395	0.10	0.09	-2.49
PAN	251	0.20	0.40	395	0.26	0.44	1.78
PRI	251	0.57	0.50	395	0.50	0.50	-1.74
PRD	251	0.08	0.27	395	0.15	0.35	2.72
Other party	251	0.14	0.35	395	0.09	0.28	-2.18

Notes: The table shows summary statistics of characteristics of the municipality during the election inmediatelly preceding the reform for the group of municipalities with mayors that were in politics before the reform. The sample is divided into municipalities with elections in 2015 (i.e. early adopters) and municipalities with elections in 2016 or after (late adopters). Column 7 reports the t-statistic for the difference between columns 5 and 2.

1.C Appendix: Political Turnover and Bureaucratic Turnover

This section presents evidence that there exist firing costs for municipal governments and that bureaucratic jobs are relatively sticky when the incumbent party remains in office. I focus on the third largest state in Mexico in terms of population, the state of Jalisco, for which I have data on bureaucratic turnover. There are two key takeaways from the evidence presented in this section. First, there is a significant bureaucratic turnover every time a new mayor comes into office. Figure 1.C.1 shows the counts and employment lawsuits for wrongful dismissals as fraction of the size of the bureaucracy in the state of Jalisco. The Figure shows that there is a sharp increase in the number of lawsuits against the municipal governments in the three months after a mayor takes office. Second, when the incumbent party wins the election, bureaucratic jobs are more sticky. Figure 1.C.2 shows that the probability of receiving lawsuits for wrongful dismissals and the number of lawsuits are significantly higher for municipalities where the incumbent party lost, compared to municipalities where the incumbent party won.



(a) Counts (b) % of the size of bureaucracy

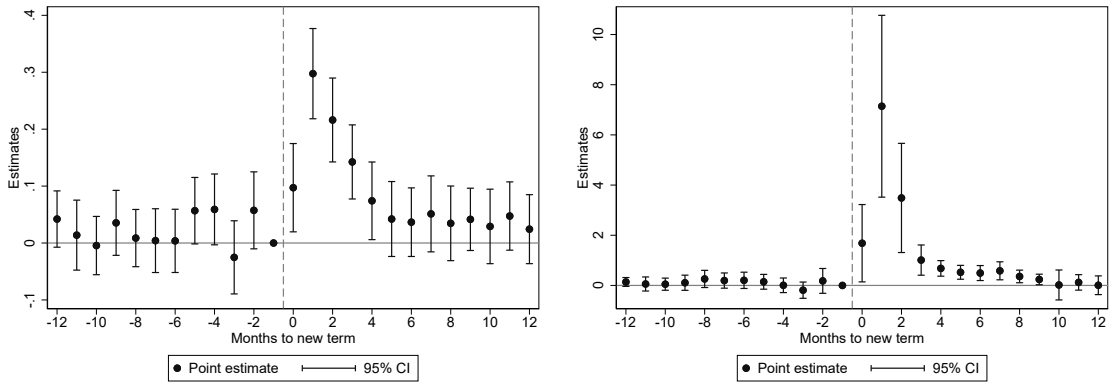
Figure 1.C.1: Employment lawsuits for wrongful dismissals

Notes: Panel (a) plots the counts of employment lawsuits against municipal governments in the state of Jalisco by month. Panel (b) plots the number of employment lawsuits as a fraction of the size of the bureaucracy. The size of the bureaucracy was obtained from the Municipal Government Census conducted by INEGI. The size of the bureaucracy for 2009 considers the data reported for 2010 due to data availability, so the fractions for that year should be interpreted with caution. The vertical lines denote the beginning of a new mayoral term.

Figure 1.C.2 plots the estimates of the following specification:

$$y_{it} = \lambda_i + \delta_t + \theta_p + \sum_{j=-12, j\neq -1}^{12} \alpha_j IncumbentLost_{it} \times NewTerm_{ij(t)} + \varepsilon_{it} \tag{1.C.1}$$

where y_{it} is a dummy variable indicating whether municipality i got an employment lawsuit for wrongful dismissal in month t and zero otherwise, or the number of employment lawsuits. $IncumbentLost_{it}$ is a dummy variable that takes the value of one if the incumbent party lost the elections and zero otherwise. $NewTerm_{ij(t)}$ is a dummy variable that takes the value of one if the mayor began his term j months ago. The parameters λ_i, δ_t, and θ_p are municipality, month, and political party fixed effects, respectively. It is important to note that α_j measures associations, since the incumbent party could have lost for reasons that are correlated with the outcome variables. I will extend this analysis by focusing on close races to deal with endogeneity.



Dep. Var. Mean: 0.20

(a) $Pr(\#lawsuits > 0)$

Dep. Var. Mean: 0.63

(b) # of lawsuits

Figure 1.C.2: Political turnover and employment lawsuits

Notes: The figure plots the estimates of equation 1.C.1 for a dummy variable that takes the value of one if the municipal government received an employment lawsuit and zero otherwise (panel a), and for the number of employment lawsuits received by the municipal government (panel b). The dependent variable means are computed using the 12 months leading to the new term and for the group of municipalities where the incumbent party lost. Standard errors are clustered at the municipality level.

1.D Appendix: Treatment Homogeneity by Timing of Adoption

Figures 1.B.5, 1.B.6, and 1.B.7 report the estimates of the following specification:

$$y_{ipt} = \lambda_i + \delta_t + \theta_p + \sum_{l=-T, l\neq -5}^{T} \beta_l \cdot Treated_{ilt} + \sum_j \alpha_{jk} \cdot \mathbf{1}\{j = t\} \cdot X_i' + W_{it}\Gamma + \varepsilon_{ipt} \tag{1.D.1}$$

where $Treated_{ilt}$ takes the value of one if municipality i is l periods away from treatment and zero otherwise. The rest of the parameters and indices are as described in Section 1.5. de Chaisemartin and d'Haultfoeuille (2020) and Sun and Abraham (2020) show that the estimates of the ATT that exploit this type of staggered adoption of treatment are the weighted sum of the ATTs in each group and period. In addition to the standard difference-in-differences assumptions, identification under this design requires treatment effect homogeneity. That is, the treatment effects in each relative period do not depend on the timing of adoption. If this assumption does not hold, then the weights can take negative values.

To test whether the treatment effects are contaminated by effects from other periods other than l, I follow Sun and Abraham (2020) and compare the estimates of equation 1.D.1 with the interaction-weighted estimator (IW) proposed by these authors, which is robust to treatment heterogeneity by timing of adoption. Specifically, I estimate a weighted average of the cohort-specific average treatment effects on the treated ($CATT_{cl}$), where c denotes the cohort (defined by the timing of adoption of treatment) and l refers to the year relative to treatment as defined in equation 1.D.1. In the setting studied in this paper, there are two cohorts: (i) municipalities with elections in 2015 and (ii) municipalities with elections in 2016. In the estimations I consider a never-treated group, which are the municipalities with elections after 2016 and municipalities in the states of Hidalgo and Tlaxcala, which do not become treated during the studied period. This last group of municipalities allows me to identify the calendar year fixed effects.

The estimation procedure is as follows:

Step 1: Estimate the $CATT_{cl}$ using the following specification:

$$\begin{aligned} y_{ipt} &= \lambda_i + \delta_t + \theta_p + \sum_{c \notin C} \sum_{l \neq -5} \alpha_{cl} \cdot (\mathbf{1}\{C_i = c\} \cdot Treated_{ilt}) \\ &+ \sum_j \alpha_{jk} \cdot \mathbf{1}\{j = t\} \cdot X_i' + W_{it}\Gamma + \epsilon_{ipt} \end{aligned} \tag{1.D.2}$$

Step 2: Estimate the weights $Pr\{C_i = c | C_i \in [-l, T-l]\}$ by sample shares of each cohort.

Step 3: Estimate the IW as follows:

$$\hat{v}_l = \sum_{l \in g} \sum_c \hat{\alpha}_{cl} \hat{Pr}\{C_i = c | E_i \in [-l, T-l]\} \tag{1.D.3}$$

where $\hat{\alpha}_{cl}$ is obtained from Step 1 and $\hat{Pr}\{C_i = c | C_i \in [-l, T-l]\}$ is obtained from Step 2.

I estimate equation 1.D.1 and compare the estimates of β_l with the estimates of v_l of equation 1.D.3. I focus on outcomes that I can observe on a yearly basis, because they allow me to exploit the staggered design. These outcomes are (i) fiscal deficit and property taxes and (ii) malfeasance spending. The results for the public finance outcomes are reported in Table 1.D.1, while the results for malfeasance spending are reported in Table 1.D.2. Column 1 reports the estimates of equation 1.D.1. Column 2 reports the estimates of equation 1.D.3. Columns 3 and 4 report the estimates of the $CATT_{el}$ from equation 1.D.2. For the case of fiscal deficit and malfeasance spending, the results show that the treatment effects are similar for each treated cohort (columns 3 and 4). Moreover, for all outcomes, the point estimates $\hat{\beta}_1$ and $\hat{\beta}_2$ are statistically indistinguishable from $\hat{v}_1$ and $\hat{v}_2$. This suggests that for these outcomes, the standard two-way fixed effect model is robust to treatment heterogeneity by timing of adoption.

Table 1.D.1: Heterogeneity by timing of adoption: public finance outcomes

Year relative to treatment	FE Estimates	IW Estimates	Estimates for $CATT_{e,l}$ $\hat{\alpha}_{1,l}$	$\hat{\alpha}_{2,l}$
	(1)	(2)	(3)	(4)
Panel A. Public debt per capita				
-6	-0.34	1.42	-	1.42
	(45.72)	(81.77)	-	(81.77)
-5	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
-4	-22.96	-28.21	-10.38	-84.48
	(43.95)	(37.45)	(48.67)	(59.58)
-3	7.80	-20.45	-33.09	21.18
	(41.35)	(33.26)	(39.42)	(94.3)
-2	-11.22	-17.53	-8.64	-49.98
	(34.07)	(38)	(49.79)	(50)
-1	19.69	-5.60	-23.59	52.92
	(41.44)	(49.58)	(65.73)	(55.42)
0	21.55	22.65	23.26	20.67
	(46.21)	(40.32)	(47.12)	(75.77)
1	-91.02	-99.96	-105.44	-83.29
	(46.01)	(43.05)	(43.14)	(100.4)
2	-22.28	-27.57	-27.57	-
	(65.36)	(60.61)	(60.61)	-
Panel B. Property taxes per capita				
-6	21.64	6.84	-	6.84
	(6.65)	(7.71)	-	(7.71)
-5	0.00	0.00	0.00	0.00
	(0)	(0)	(0)	(0)
-4	-6.74	-0.86	-2.19	3.33
	(7.48)	(5.99)	(7.34)	(13.19)
-3	-8.14	3.40	6.26	-6.00
	(10.71)	(10.39)	(11.1)	(15.11)
-2	-1.92	6.42	13.18	-18.25
	(11.73)	(6.77)	(8.44)	(20.8)
-1	-2.27	1.25	-1.37	9.78
	(13.92)	(14.91)	(17.31)	(13.36)
0	-4.79	13.21	20.89	-11.80
	(20.05)	(12.57)	(13.86)	(30.63)
1	22.01	30.23	49.43	-28.20
	(10.76)	(10.21)	(11.04)	(21.97)
2	43.73	34.74	34.74	-
	(10.43)	(10.6)	(10.6)	-

Notes: The table shows estimates of the treatment effects on public debt per capita (Panel A) and property taxes per capita (Panel B). Column (1) shows the estimates of equation 1.D.1, column (2) shows the estimates of equation 1.D.3, whereas columns (3) and (4) show the estimates of equation 1.D.2. Standard errors in columns (1), (3) and (4) are clustered at the state level, whereas standard errors in column (2) are computed analytically as in Sun and Abraham (2020).

Table 1.D.2: Heterogeneity by timing of adoption: misspent resources

Year relative to treatment	FE Estimates	IW Estimates	Estimates for $CATT_{e,l}$ $\hat{\alpha}_{1,l}$	$\hat{\alpha}_{2,l}$
	(1)	(2)	(3)	(4)
-6	0.02	0.03	-	0.03
	(0.04)	(0.07)	-	(0.07)
-5	0.00	0.00	0.00	0.00
	(0)	(0)	(0)	(0)
-4	0.04	0.05	0.05	0.05
	(0.03)	(0.04)	(0.07)	(0.09)
-3	-0.01	-0.04	0.04	-0.19
	(0.04)	(0.05)	(0.06)	(0.11)
-2	0.00	-0.10	-0.14	0.03
	(0.04)	(0.06)	(0.09)	(0.05)
-1	0.04	0.01	0.00	0.03
	(0.03)	(0.04)	(0.05)	(0.05)
0	0.05	0.02	0.00	0.09
	(0.04)	(0.04)	(0.04)	(0.11)
1	-0.09	-0.08	-0.07	-0.14
	(0.07)	(0.07)	(0.08)	(0.08)
2	-0.13	-0.19	-0.19	-
	(0.07)	(0.06)	(0.06)	-

Notes: The table shows estimates of the treatment effects on the fraction of misspent resources. Column (1) shows the estimates of equation 1.D.1, column (2) shows the estimates of equation 1.D.3, whereas columns (3) and (4) show the estimates of equation 1.D.2. Standard errors in columns (1), (3) and (4) are clustered at the state level, whereas standard errors in column (2) are computed analytically as in Sun and Abraham (2020).

1.E Appendix: Data

This paper uses data from different government agencies. To combine all of these data sets, I created a crosswalk that maps all of them using the 2016 municipal identifiers reported by the National Institute of Statistics (INEGI). The main data sets are (i) panel of municipality employees and public capital in the municipality; (ii) panel of local public finance outcomes; (iii) panel of audit reports; (iv) panel of criminal investigations and homicides; (v) observable characteristics of the candidates and elected politicians in municipal elections; and (vi) municipality characteristics. In this appendix, I describe the sources of the data and how I construct the samples of interest. All data sets exclude information from the state of Oaxaca and from Mexico City.

1.E.1 Municipality employees and public capital

The measures of municipality employees and public capital come from the municipal government census conducted every two years by INEGI since 2011. The data come from a collection of surveys of municipal government officials about information of the local government during the previous calendar year. The surveys ask for information about the *Ayuntamiento*, the municipal governments' public administration, public security, municipal justice, drinking water and sewerage, and waste disposal. To guarantee the quality of the information provided in these censuses, INEGI asks the director of each department of the municipal government that is providing the information to sign the questionnaire. These censuses are publicly available at `https://www.inegi.org.mx/`. To deal with outliers, the outcome variables have been winsorized to the 99th percentile.

1.E.2 Public Finance data

Public finance outcomes come from the balance sheets of each municipality reported annually to INEGI. These data contain information about revenue and expenses of approximately 86% of the municipalities in Mexico. The sample of interest considers data reported from 2010 to 2017. All variables are per capita and expressed in MXN pesos of 2018. Figure 1.E.1 tests whether reporting public finance data to INEGI varies differentially by timing of adoption of treatment.

Panel (a) includes municipalities in all states, whereas Panel (b) excludes municipalities in the state of Puebla. In this state, only 4% of the municipalities reported data in the studied period (INEGI, 2017). These data are publicly available at `https://www.inegi.org.mx/`. To deal with outliers, the outcome variables have been winsorized to the 99th percentile.

1.E.3 Audit reports

The data on misspent resources come from official annual audit reports by Mexico's Federal Auditor's Office (ASF) to Congress. The reports are publicly available at `https://www.asf.gob.mx/Section/58_Informes_de_auditoria` and can be downloaded for the years 2000 to 2018 (as of July 14 of 2020). To construct the measure of misspent resources, I use the value of unauthorized spending as a fraction of the total audited FISM resources. The unauthorized spending is identified as the *Recuperaciones Totales*, which denote the amount of resources that the audit considers damaged the Federal Public Treasury and that need to be reimbursed to the federal treasury.

1.E.4 Criminal investigations and homicides

The data on criminal investigations come from the Secretary of Security and Civilian Protection. These investigations contain information about the *Common Law Crimes* committed in each municipality that are being investigated in the local (state) courts. These types of crimes are the crimes in the jurisdiction of the local police. The data contain the counts for each type of crime on a monthly basis and the municipality identifier. The data are reported from 2011 onward, and are publicly available at `https://www.gob.mx/sesnsp`. I also use the homicides reported by INEGI on a monthly basis, which can be accessed at `https://www.inegi.org.mx/`.

1.E.5 Electoral and candidate data

Data on elections and characteristics of the mayors and candidates come from several sources. Election dates come from the National Electoral Authority (INE). The candidate vote shares come from the electoral authorities of each state. The data have to be cleaned for each indi-

vidual election, because typically the votes for coalitions are not added. Importantly, coalitions are defined before elections, which deals with possible concerns of strategic behavior of candidates postelection. Table 1.E.1 lists the states and year of elections for which the votes for each party or coalition were collected.

The names of the candidates come from the electoral authorities of each state. I was able to collect the information for 28 states out of 30 states of my sample.[41] I obtained the gender of the candidate for mayor through a classification algorithm based on the names and genders of 12,970 local politicians from 2000 to 2017. I obtained these names from the National System of Municipal Information (SNIM), which reports the names and genders of the elected mayors.

I used the names of the candidates to identify the postreform mayors who were involved in politics before the reform (i.e., they ran for office before). With the list of postreform mayors, I performed a fuzzy merge with the names of candidates in the two immediate prereform elections based on the first name, middle name, and last names of the candidate, restricting the merge to an exact match with the municipality. When the name of the mayor was not found in the previous elections, I used his two last names (father's and mother's last names) to identify whether a sibling of the mayor ran for a political position before the reform. This last group of mayors was classified as a dynasty.

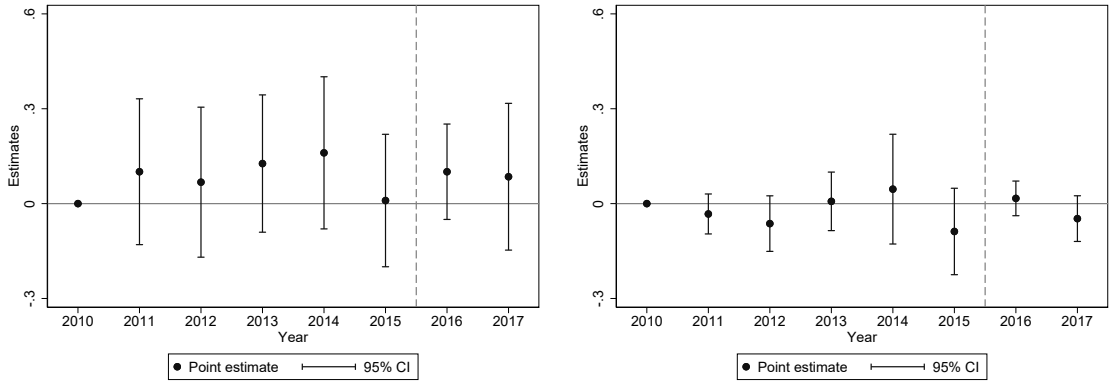
Finally, data on characteristics of the elected mayors were obtained from the municipal government censuses conducted by INEGI. This data are available for the years 2010, 2012, 2014, and 2016. The data for the years 2012 onward are deidentified at the municipality level and I can only observe the state identifier. Using these data, I built a panel data set at the municipality$\times$election level containing information about the education of the mayor and his previous employment. If for each election a municipality is observed twice (because for the same term there are two census waves), I used the information reported in the last year. These data are publicly available at `https://www.inegi.org.mx/`.

[41] The missing states are Tlaxcala and Puebla.

1.E.6 Municipality characteristics

The estimations presented in this paper control for characteristics of the municipalities, some of which are fixed over time and others of which vary on a yearly basis or on a quinquennial basis. The estimations consider the logarithm of the households in the municipality with access to computers, the logarithm of households in the municipality with access to social security, and the average years of schooling in the municipality. These variables come from the 2010 population census conducted by INEGI and can be downloaded from SNIM (`http://www.snim.rami.gob.mx/`). I also consider an index of the degree of marginalization of the municipality, which is constructed by CONAPO and can be downloaded from SNIM (`http://www.snim.rami.gob.mx/`). This covariate varies every five years, because it is computed using the 2010 population census of INEGI and the 2015 *Conteo* of INEGI. The estimations also control for the state GDP, which varies annually and can be downloaded from INEGI (`https://www.inegi.org.mx/`). Finally, the majority of the outcome variables are normalized by the 2010 or 2015 population in the municipality. These data come from the 2010 population census and from the 2015 *Conteo* and were downloaded from SNIM (`http://www.snim.rami.gob.mx/`).

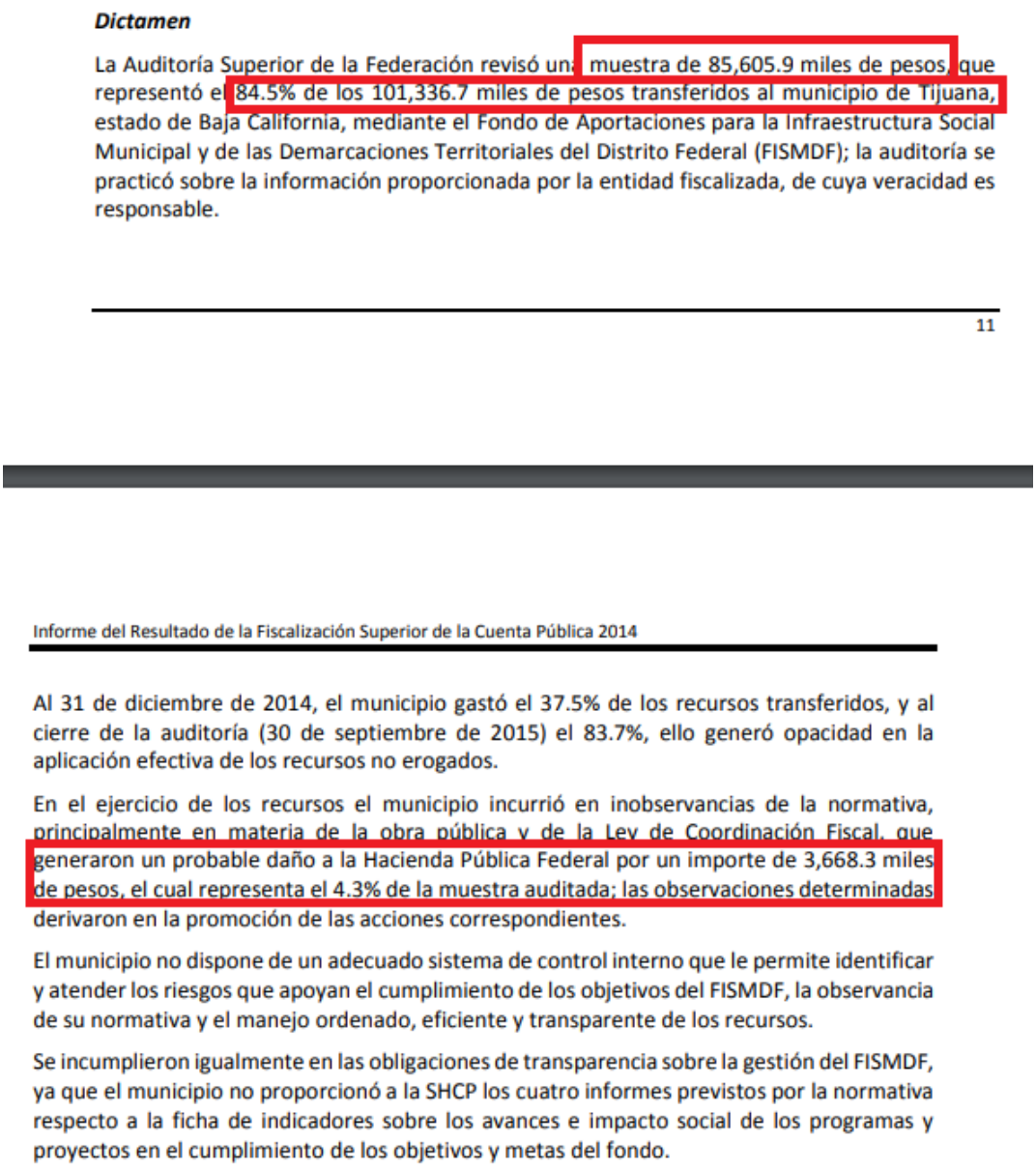
1.E.7 Data Appendix supplemental material



(a) All states (b) Excluding Puebla

Figure 1.E.1: Probability of reporting public finance data to INEGI

Notes: The figure plots the differences in means by year for the probability that a municipality reports public finance data to INEGI between municipalities that become treated in the 2015 elections and the rest of municipalities. Panel (a) reports the estimates for all states whereas Panel (b) reports the results for estimates that exclude the state of Puebla. The vertical dashed line corresponds to the year when the reform was implemented. Standard errors are clustered at the state level.



Dictamen

La Auditoría Superior de la Federación revisó una muestra de 85,605.9 miles de pesos, que representó el 84.5% de los 101,336.7 miles de pesos transferidos al municipio de Tijuana, estado de Baja California, mediante el Fondo de Aportaciones para la Infraestructura Social Municipal y de las Demarcaciones Territoriales del Distrito Federal (FISMDF); la auditoría se practicó sobre la información proporcionada por la entidad fiscalizada, de cuya veracidad es responsable.

11

Informe del Resultado de la Fiscalización Superior de la Cuenta Pública 2014

Al 31 de diciembre de 2014, el municipio gastó el 37.5% de los recursos transferidos, y al cierre de la auditoría (30 de septiembre de 2015) el 83.7%, ello generó opacidad en la aplicación efectiva de los recursos no erogados.

En el ejercicio de los recursos el municipio incurrió en inobservancias de la normativa, principalmente en materia de la obra pública y de la Ley de Coordinación Fiscal, que generaron un probable daño a la Hacienda Pública Federal por un importe de 3,668.3 miles de pesos, el cual representa el 4.3% de la muestra auditada; las observaciones determinadas derivaron en la promoción de las acciones correspondientes.

El municipio no dispone de un adecuado sistema de control interno que le permite identificar y atender los riesgos que apoyan el cumplimiento de los objetivos del FISMDF, la observancia de su normativa y el manejo ordenado, eficiente y transparente de los recursos.

Se incumplieron igualmente en las obligaciones de transparencia sobre la gestión del FISMDF, ya que el municipio no proporcionó a la SHCP los cuatro informes previstos por la normativa respecto a la ficha de indicadores sobre los avances e impacto social de los programas y proyectos en el cumplimiento de los objetivos y metas del fondo.

Figure 1.E.2: Audit report for Tijuana, Baja California: 2014 fiscal year

Notes: The figure shows the summary of the findings of the audit conducted by the ASF to the municipality of Tijuana, Baja California. The audit was conducted in 2015 to funds used in the 2014 fiscal year. The first red square highlights the total FISM funds audited. The second red square highlights the total FISM funds received by the municipality during the 2014 fiscal year. Finally, the third red square highlights the amount of audited resources that were found to be misspent.

Table 1.E.1: States with municipal elections by year

State	2007	2008	2009	2010	2011	2012	2013	2014	2015	2016
Aguascalientes	x			x			x			x
Baja California	x			x			x			x
Baja California Sur		x			x				x	
Campeche			x			x			x	
Chiapas	x			x		x			x	
Chihuahua	x			x			x			x
Coahuila de Zaragoza			x				x			
Colima			x			x			x	
Durango	x			x			x			x
Guanajuato			x			x			x	
Guerrero		x				x			x	
Hidalgo		x			x					x
Jalisco			x			x			x	
Michoacán de Ocampo	x				x				x	
Morelos			x			x			x	
México			x			x			x	
Nayarit					x			x		
Nuevo León			x			x			x	
Puebla	x			x			x			
Querétaro			x			x			x	
Quintana Roo		x		x			x			x
San Luis Potosí			x			x			x	
Sinaloa	x			x			x			x
Sonora			x			x			x	
Tabasco			x			x			x	
Tamaulipas	x			x			x			x
Tlaxcala	x			x			x			x
Veracruz de Ignacio de la LLave	x			x			x			
Yucatán	x			x		x			x	
Zacatecas	x			x			x			x

Notes: The table shows the states and election years for which data on election results were collected. An "x" indicates that the state had elections in that year and that the data were collected.

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Chapter 2

The Effects of Political Alignment on the Allocation of Public Credit

2.1 Introduction

Development banks are an important source of financing of public infrastructure and other productive public goods, and many developing countries use them as a tool to foster economic development and reach the poor (United Nations, 2015). While in principle their allocation rules are well defined, in practice they can be subject to political manipulation (Cole, 2009; Carvalho, 2014; Ru, 2018) and be used as an additional way to provide resources to regions sharing the same political interests as those of the banks. Because this source of financing has become more salient across the globe and is used to increase the provision of public goods, understanding which political factors affect their allocation can inform about their effectiveness for economic development and their contribution to public debt accumulation.[1]

In this paper, we study whether having a mayor of the presidential party affects the allocation of credit by development banks at the subnational level. We study this question in the context of credit to municipal governments in Mexico during the period of 2009 to 2015. This setting represents a good laboratory to empirically answer this question for a number of reasons. First, in

[1] See Alesina and Passalacqua (2016) on the importance of political economy forces for public debt accumulation.

Mexico local governments get credit mainly from traditional financial intermediaries. This credit is allocated by a state-owned development bank, the National Bank of Public Works and Services (Banobras), and by private banks. On average, during the period studied, 94% of local public debt was owed to one of these two sources (CEFP, 2017). This allows us to explore whether government credit is differentially allocated to municipalities depending on their political alignment status, and then we can compare this allocation with the allocation of credit by private banks. This is unique in the literature, since it has mostly studied the allocation of resources to local governments by upper levels of government, and not the allocation of resources to local governments by private agents. Second, during the period studied the presidential party switched from the PAN to the PRI. This allows us to explore whether the allocation of subnational credit varies depending on which political party is in power. Finally, by studying the allocation of credit to local governments, we can exploit local variation holding national institutions constant.

We use a close elections regression discontinuity design and compare the allocation of credit to municipalities where a candidate of the presidential party barely won against the allocation of credit to municipalities where a candidate of the presidential party barely lost. Under certain conditions, this strategy provides quasi-experimental variation in the political party of mayors, allowing us to identify the main effects of interest. We use close elections during the years 2009–2013 and combine several sources of administrative data. First, we use detailed credit records that contain individual data on every loan allocated by Banobras and by private banks to municipal governments. Our first set of results looks at the effects of being politically aligned to the presidential party on the extensive margin of credit and on the size of the loans. We find that municipalities where a candidate of the presidential party won by a narrow margin are, on average, 4.2 to 4.6 percentage points more likely to receive new loans, off of a mean of 17%. Second, we find that politically aligned municipalities receive 18.5–19.6 pesos per capita more in loans, which represents a relative increase in the amount of resources allocated of about 33.5%–35%. These effects are driven by the allocation of credit by Banobras, and we find small and statistically insignificant discontinuities in the allocation of credit by private banks. We also exploit the fact that during the period studied, the presidential party switched from the PAN to the PRI, and look

at whether the first set of results varies by the political party in power. We find that regardless of which party controls the federal government, municipalities politically aligned to the presidential party are more likely to receive credit from the state-owned development bank, while we do not find significant differences in the allocation of credit by private banks under the two different political parties.

There are important fiscal policy implications of these results. Unlike fiscal transfers from upper levels of the government, often studied in the political economy literature, resources obtained through credit have to be repaid, which can reduce future consumption of public and private goods (Clemens and Miran, 2012) and restrict future policy choices (Persson and Svensson, 1989; Alesina and Tabellini, 1990). To understand the fiscal policy implications, we test whether politically aligned municipalities end up paying more for the loans. Because debt payments depend on the price and maturity of the loans, we begin by exploring if these contract terms vary around the winning threshold and find that politically aligned municipalities receive loans that are 4% cheaper, although this estimate is marginally significant at the 10% level. Then, using the monthly installments of the loans, we find that aligned municipalities end up paying more in debt services, which we interpret as a mechanical effect due to the larger loan sizes received. Specifically, we find that politically aligned municipalities pay 23–24.2 pesos per capita more in debt services, which implies an interest amount of about 4.5 pesos per capita more. These extra payments come from payments to the state-owned development bank and we find small and statistically insignificant discontinuities in the total payments to private banks.

Whether or not these extra payments imply better outcomes for citizens in politically aligned municipalities is an empirical question. Using data on municipal annual balance sheets, we test whether aligned municipalities report higher public expenditures. We find that politically aligned municipalities increase the procurement of supplies used to deliver goods and services and used in the performance of the day-to-day administrative activities. Additionally, consistent with the results found for debt payments, we find that politically aligned municipalities report higher expenditures in debt services. Specifically, the estimated effect shows that these municipalities pay annually 29% more in debt services than politically unaligned municipalities. We do not

find evidence that these municipalities increase expenditures in public investments or in acquiring assets for the municipal government.

In our setting, loans to municipal governments are intended to increase the provision of public infrastructure and productive public goods. We construct different measures of the public infrastructure used to provide the goods and services of responsibilities of local governments in our setting and explore whether politically aligned municipalities report higher levels in these measures. We find no evidence that this is the case. Finally, we find that politically aligned municipalities do not increase the night light intensity relative to politically unaligned municipalities.

Altogether, these results contribute to the idea that despite receiving more resources, politically connected municipalities may not improve the quality of service delivery (Callen et al., 2020). Previous work has also emphasized that aid flows to governments can be subject to political manipulation (Alesina and Dollar, 2000; Barro and Lee, 2005; Faye and Niehaus, 2012), and that political leaders play an important role for economic development (Jones and Olken, 2005). We contribute to this literature by providing causal empirical evidence on the extent to which the political party of local leaders affects the allocation of credit by development banks and ultimately the quality of service delivery. Additionally, by studying local governments we exploit local variation in political parties holding national institutions constant.

We also contribute to the literature that has studied the role of political economy forces on public debt accumulation. Earlier work laid out theoretical frameworks to think about the importance of political incentives on public debt accumulation (Persson and Tabellini, 1999; Alesina and Tabellini, 1990; Aghion and Bolton, 1990; Yared, 2010; Song et al., 2012).[2] Yet, most of the empirical evidence on this topic looks at cross-country comparisons that may suffer from reverse causality (Alesina and Passalacqua, 2016). For the case of Mexico, Hernández-Trillo and Smith-Ramírez (2009) find that the population size of municipalities and mayors' political parties correlate with the credit ratings of the municipal governments. This paper contributes to this literature by providing evidence on the causal link between political economy forces and public debt accumulation.

Finally, we contribute to the growing literature studying how the political party affiliation

[2]See Yared (2019) for a recent review of the literature on the political economy of public debt.

of local politicians affects the allocation of resources by upper levels of government. For example, politically aligned mayors in Brazil and Spain receive more transfers from upper levels of government (Brollo and Nannicini (2012) and Curto-Grau et al. (2018), respectively), while aligned politicians receive more federal grants in US states (Ansolabehere and Snyder, 2006). Similarly, places sharing the same ethnicity as the president in Kenya receive more expenditures on roads (Burgess et al., 2015), while local governments in India represented by a member of the ruling party get more mining permits and license more areas for mining (Asher and Novosad, 2017). We contribute to this literature by studying the allocation of credit to politically aligned municipalities by state-owned and private banks. Unlike in the previous work, studying the allocation of credit by private agents gives us a unique benchmark that can inform about whether or not the political party affiliation of politicians matters for their allocation decisions.

The remainder of this paper is structured as follows: Section 2 describes the institutional background, the sources of data, and the main sample of interest. Section 3 describes the empirical strategy. Section 4 reports the main results on the allocation of credit to municipalities. Section 5 reports the results on the effects on public expenditures, state capacity, and public infrastructure. Section 6 discusses possible explanations of the results and finally we conclude.

2.2 Institutional Background and Data

In this section, we first describe some stylized facts regarding municipal debt in Mexico. We then introduce our definition of political alignment and list the data used throughout the paper. The period of study spans from 2009 to 2015.

2.2.1 Municipal debt

Local public debt in Mexico has increased significantly in the past decade. From the last quarter of 2005 to the last quarter of 2016, the stock of local public debt increased in real terms by 151.26% (Figure 2.A.1a), and the fraction of municipalities holding some debt increased from 19% to 30%, with some periods having almost 40% of municipalities holding debt (Figure 2.A.1b in the Appendix). This debt came from several sources, including state-owned banks, private banks,

and the bond market. From 2009 to 2015, on average 94% of municipalities' debt came from loans by traditional financial intermediaries, of which 49% was granted by state-owned banks (SHCP, 2019).

Government credit to municipalities is allocated through the National Bank of Public Works and Services (Banobras), which is a state-owned development bank that serves primarily to development ventures of state and municipal governments. The bank belongs to the Secretary of Finance and Public Credit and its director is appointed by the president. Created in 1933, Banobras has served as a major financier of infrastructure and public services projects led by different levels of the government, including states and municipalities. Banobras allocates direct credit for infrastructure projects and public services such as roads, sidewalks, water and energy provision, and security and justice, among others (Banobras, 2015). The bank also allocates indirect or induced credit by providing financial guarantees to municipalities, which improves their credit worthiness and allows them to access credit markets with better credit scores. This paper focuses on the allocation of direct credit from Banobras, which we can observe through the credit registry as we will describe below in Section 2.2.3. We also look at credit from private banks and see if political alignment plays a role in their allocation decisions.

2.2.2 Political alignment

During the period of our study, Mexico had a multiparty system dominated by the National Action Party (PAN), the Institutional Revolutionary Party (PRI), and the Party of the Democratic Revolution (PRD). Presidential and gubernatorial elections are held every six years while municipal elections are typically held every three years. Importantly, during the period of our study there were no incumbents at any level of the government, so neither the president, state governors, nor municipal mayors could run for reelection.

We define aligned municipalities as those municipalities that are represented by a mayor belonging to the presidential party coalition. Meanwhile, we define unaligned municipalities as those municipalities that are represented by a mayor of any other political party coalition. During the period studied, there were two political parties in the presidency: from December 2006 to

November 2012, the PAN (President Felipe Calderón) was in power, while from December 2012 to December 2018, the PRI (President Enrique Peña Nieto) was in power. Thus, from January 2009 to November 2012, a municipality is politically aligned to the federal government if its mayor belongs to the PAN while it is politically unaligned if its mayor belongs to another political party coalition. Similarly, from December 2012 to December 2018, a municipality is politically aligned if its mayor belongs to the PRI, while it is politically unaligned if its mayor belongs to any other political party coalition.

In Mexico, mayoral terms typically last three years and elections are staggered across states. In this paper we use municipal elections from 2009 to 2013. Our estimation sample excludes the territorial demarcations of Mexico City and the municipalities in the state of Oaxaca. We exclude municipalities in the state of Oaxaca because the majority of its leaders are selected through customs and traditions and not through elections, mainly due to its large fraction of indigenous population. In Figure 2.1, we show the timeline and the total number of municipal elections per year considered, and in Table 2.B.1 in the Appendix 2.B.1 we show the states with municipal elections per year. Overall, we use close municipal elections from a total of 3,398 elections.

2.2.3 Data

Credit data

The main analysis uses data on loans to municipalities from traditional financial intermediaries from July 2009 to May 2015.[3] The data consist of monthly reports by Banobras and private banks to the National Banking and Securities Commission (CNBV) on every loan to municipal governments. The reports contain the authorized loan size, interest rates, maturity, and default rates, among other characteristics of the loans. Importantly, reports include the period in which each loan was originated, which we use to match loans with the term of a mayor.

We restrict our sample to new loans from Banobras and from private banks granted to municipal governments between July 2009 and May 2015, and we consider the contract terms when

[3]We use this period because the methodology used to collect credit records changed in July 2009, and we got access to data spanning until May 2015.

the loan was originated. We use these loan characteristics to study the differences in the allocation of credit across aligned and unaligned municipalities. Furthermore, to deal with outliers, we winsorize observations with loans in the top one percent of the loan size distribution. In the Appendix, we give a more detailed description of how we built the sample of interest and the sources of the data.

In Panel A of Table 2.1 we show summary statistics for the main characteristics of the loans in municipalities where the presidential party's margin of victory was less than +/-10 percentage points. Columns 1–3 show summary statistics for unaligned municipalities, whereas columns 4–6 show summary statistics for aligned municipalities. Column 7 reports the t-statistic for the difference in means across the two groups of municipalities. On average, each year 19% of aligned municipalities have at least one new loan from a traditional financial intermediary, while 17% of unaligned municipalities have one. Moreover, aligned municipalities receive on average 13 pesos more in loans per capita than unaligned municipalities and slightly cheaper loans. When we split the loans by their source, we observe that the differences explained above are driven by the loans allocated by Banobras, while we do not observe statistically significant differences in means for the loans allocated by private banks.

Electoral data

Electoral data from 2009 to 2013 were obtained from the electoral authorities in each of Mexico's states. Figure 2.A.2 in the Appendix shows the geographic variation of elections during the 2009–2011 and 2012–2013 periods where the margin of victory of the presidential party was smaller than the absolute value of +/-10%. While the geographic size and number of municipalities vary across states, during the period studied in this paper there were several close elections all around the country. Table 2.E.3 in the Appendix tests for systematic differences in the characteristics of the municipalities where the presidential party won or lost by less than a 10% margin against municipalities where the presidential party won or lost by a larger margin. Overall, we do not find systematic differences across years between municipalities with close elections and municipalities with less competitive elections.

In Panel B of Table 2.1 we show summary statistics of some predetermined characteristics of aligned and unaligned municipalities with competitive elections (i.e., +/- 10% margin of victory). On average, politically aligned municipalities are slightly less marginalized and have 7% fewer inhabitants, both differences statistically significant only at the 90% level. We find no statistically significant differences in other characteristics such as GDP, years of schooling, and population with social security.

2.2.4 Expenditures and public infrastructure

In this paper, we also aim at exploring whether aligned municipalities improve service delivery. To this end, first we use annual municipal public finance data that contain information about the sources of revenue and annual expenditures of the municipalities. These data are collected by the National Institute of Statistics and Geography (INEGI) and contain public finance information for about 89.66% of the municipalities in our sample. While we cannot observe data on all municipalities, we would be concerned if the fraction of municipalities that report information was discontinuous across the winning threshold. In Figure 2.G.1 in the Appendix we test the latter and show that this fraction varies smoothly across the winning threshold.

Panel C of Table 2.1 reports summary statistics for the different types of annual expenditures of municipal governments. On average, aligned municipalities spend more on wages and on the procurement of supplies used in day-to-day municipal activities as well as for the provision of goods and services. There are no differences in means across the two groups of municipalities in the procurement of services, subsidies and social transfers, the acquisition of assets for the municipal government, and in public investments. Interestingly, aligned municipalities do spend more on debt services.

The data on public infrastructure and service delivery come from municipal government censuses conducted by INEGI every two years since 2011. The data refer to the previous calendar year and contain information about the number of police stations, the number of jail cells, the number of streetlight posts, and whether the municipal government collects waste, the number of waste disposal vehicles, whether the municipality has sewerage, among other characteristics of

the municipal government administration. We focus on the 2013 and 2015 modules with information referring to 2012 and 2014, respectively. The summary statistics of these outcomes are shown in Panel D of Table 2.1. We do not find differences in means in any of the outcomes considered for aligned and unaligned municipalities. We also complement the information on streetlight posts using the night light intensity in each municipality captured by satellite imagery. The night light intensity data for the years 2010–2013 come from the DMSP satellite, whereas the data for the years 2014–2015 come from the VIIRS satellite.

2.3 Empirical Strategy

To study the effects of being represented by a mayor of the ruling party on credit and public goods, one could imagine an experiment that randomly assigns mayors of the ruling party to a set of municipalities and mayors from different political parties to the remaining municipalities, generating experimental variation in municipalities' political alignment status. This design would ensure that municipalities with unaligned mayors are, on average, a reasonable counterfactual for municipalities with aligned mayors. While this ideal experiment is infeasible, it is possible to generate such variation quasi-experimentally through a close elections regression discontinuity design by comparing municipalities where the presidential party barely won against municipalities where the presidential party barely lost. Under certain conditions, municipalities where the presidential party lost by a narrow margin are a reasonable counterfactual for municipalities where the presidential party won by a narrow margin.[4] The running variable of the design is the margin of victory of the presidential party, defined as

$$margin_{ie} = share_{ie}^{pr} - share_{ie}^{op}$$

where $share_{ie}^{pr}$ is the share of votes obtained by the presidential party in municipality i during elections e, and $share_{ie}^{op}$ is the highest share of votes obtained by any other political party coalition during the same election. Thus, $margin_{ie}$ is the winning or losing margin obtained by the presi-

[4]The underlying assumption behind this approach is that municipalities with close elections are similar in observed and unobserved characteristics correlated with the dependent variable (Lee and Lemieux, 2010).

dential party coalition in municipality i during elections e. If $margin_{ie} > 0$, then the municipality is politically aligned, whereas if $margin_{ie} < 0$, then it is politically unaligned. The baseline analysis estimates the following equation:

$$\begin{aligned} y_{iet} = \beta_0 + \beta \mathbf{1}(margin_{ie} > 0) + f(margin_{ie}) \\ + h(margin_{ie})\mathbf{1}(margin_{ie} > 0) + \mathbf{X}_i\mathbf{\Gamma} + \theta_t + \varepsilon_{iet} \end{aligned} \tag{2.1}$$

where y_{iet} is an outcome of interest for a municipality during each t year of the mayor elected in elections e and $\mathbf{1}(margin_{ie} > 0)$ is an indicator function that takes the value of one if a municipality is politically aligned and zero otherwise. $f(\cdot)$ and $h(\cdot)$ are polynomial functions of order 1 or 2 and $\mathbf{X}_i$ is a vector of predetermined municipality economic and sociodemographic characteristics, such as GDP per capita, average years of schooling, degree of marginalization, human development index, population, number of houses, and population with social security. θ_t are calendar year fixed effects and ε_{iet} is an error term. The parameter of interest β_1 identifies the local average treatment effect (LATE) of being represented by a member of the presidential party on the outcomes of interest in the vicinity of 0. Note that $\mathbf{X}_i$ and θ_t are not required for identification, and including covariates in the estimations leads to unbiased estimates of β_1 as long as they are predetermined and balanced around the winning threshold (Cattaneo et al., 2018). In the next section, we show that all the covariates included in the vector $\mathbf{X}_i$ are balanced around the winning threshold. Furthermore, all our point estimates are robust to excluding the set of covariates and calendar year fixed effects. In our baseline specifications, we estimate equation 2.1 using a local linear regression with a 10% bandwidth and a local polynomial regression of degree 2 with a 20% bandwidth.[5] Additionally, all our baseline specifications consider a triangular kernel.[6]

[5]The bandwidth obtained from applying the optimal bandwidth selection described in Calonico et al. (2014) to the main outcome of interest is 13.4%. Table 2.D.1 reports the optimal bandwidth for each of the outcomes considered in this paper; the optimal bandwidths vary from 9.2% to 19.54%.

[6]Our results are robust to different specifications, bandwidths and kernels. For example, we also present results using triangular and rectangular kernels varying the bandwidth from 2% to 100%.

2.3.1 Validity of the research design

The validity of our research design rests on two main assumptions. First, we require that voters do not behave strategically around the winning threshold.[7] Second, we require that relevant factors that could affect outcomes vary smoothly around the winning threshold.[8] The first assumption can be tested by performing a manipulation test of the running variable (McCrary, 2008), under the null that the distribution of the margin of victory varies smoothly around the winning threshold. In Figure 2.2 we show the nonparametric distribution of the margin of victory and perform the manipulation test. In this test, we fail to reject the null that there is no discontinuity of the running variable around the winning threshold, providing some empirical evidence that in the 2009–2013 municipal elections, voters did not behave strategically in the vicinity of the winning threshold.[9] Furthermore, Figures 2.C.1b and 2.C.1d in the Appendix perform the manipulation test for the PAN and the PRI margins of victory, respectively. In both cases, we fail to reject the null hypothesis that the distributions of the margins of victories vary smoothly around the winning threshold.

The second assumption requires that there are no other discontinuities besides treatment of relevant factors that are correlated with the outcomes of interest. This assumption ensures that municipalities where the presidential party won by a small margin and municipalities where the presidential party lost by a small margin have, on average, similar characteristics. While we cannot test this assumption for unobserved characteristics, it is possible to do it for a set of observable characteristics. In Table 2.C.1 in the Appendix we test whether several economic and demographic characteristics of the municipalities measured in 2005 vary smoothly around the winning threshold. We report discontinuity estimates and their standard errors for competitive

[7] We would be concerned if voters in municipalities with close elections vote for the party in power, because they know their municipality would be favored by the allocation of resources. A positive discontinuity in the distribution of the margin of victory at the winning threshold would suggest this scenario.

[8] We also need that party incumbency is not correlated with victory in very close elections, which could invalidate our design (Caughey and Sekhon, 2011). The latter is also recognized by other studies using the same design for Mexico (e.g., Dell (2015)). However, using municipal elections from 1970 to 2009, Eggers et al. (2015) have shown for Mexico that party incumbency is not correlated with victory in very close elections, which provides evidence that incumbency effects are not much of a concern in our setting.

[9] In this study we use ex-post close elections and rely on the assumption that they are as good as random. While we cannot know whether ex-post elections were ex-ante close, we provide estimations considering margins of victory from 2% to 100%, and all results are robust to the selection of the bandwidth.

elections. We consider 10% and 20% bandwidths and use local linear regressions with triangular weights. Furthermore, we cluster our standard errors at the municipality level. In all the 16 point estimates reported, we fail to reject the null that municipalities' observable characteristics vary smoothly around the winning threshold. Thus, municipalities with close elections are balanced in predetermined observable characteristics, suggesting that our second assumption required for identification is likely to hold.

2.4 Empirical Results

In this section, we present the results on credit to municipalities. First, we present the results for estimates of the causal effect of being aligned to the presidential party on the probability of having credit and on the size of the loans granted by Banobras and by private banks. Then, we present results for the impact of the alignment status of a municipality on other characteristics of the loans. The unit of observation is a municipality$\times$mayor$\times$year. Furthermore, standard errors in all specifications are clustered at the municipality level to account for arbitrary correlations in the error structure within municipalities.[10]

2.4.1 Credit outcomes

We begin by exploring the effects of political alignment on the probability of having new loans during each year of a mayor's three-year term and on the loan's size. The baseline specifications consider close elections in 2009–2013, and outcomes of loans granted from July 2009 to May 2015.

Figure 2.3 examines the probability that a municipality receives at least one loan from a traditional financial intermediary in a given year. By studying this outcome we can investigate whether the alignment status of a municipality has an effect on the extensive margin of granting credit to municipalities. The figure plots the probability of having new loans against the margin of victory of the presidential party. Each dot represents the average probability of having new

[10] It is possible that standard errors are correlated cross-sectionally across municipalities in the same state, if receiving credit is correlated with the amount of loans received by neighboring municipalities, for example. Clustering at the state by year level to allow for such correlation hardly affects the inference of our estimates.

loans within a two-percentages-point bin, while the solid lines represent a linear or quadratic prediction estimated separately on each side of the threshold and within the 10% or 20% bandwidth, respectively. Furthermore, the predictions are weighted using triangular kernels. A positive margin corresponds to aligned municipalities, whereas a negative margin corresponds to unaligned municipalities.

In Figure 2.3a we analyze the effects of the alignment status of a municipality on the probability of having new loans from any source. We report the estimated discontinuities in Panel A of Table 2.2. In column 1 we consider a polynomial of degree 1 and a bandwidth of 10%, while in column 2 we consider a polynomial of degree 2 and a bandwidth of 20%. We find that municipalities where the presidential party won by a small margin are 4.2 to 4.6 percentage points more likely to receive credit from any source, compared to municipalities where the presidential party lost by a small margin. These magnitudes correspond to a relative increase in the probability of having credit from any source of 25%–27%, respectively.

In Figure 2.3b we explore the effect of the alignment status of a municipality on the probability of having at least one new loan from Banobras. The magnitude of the discontinuities is similar across the different specifications. Municipalities where the presidential party barely won are 4.4 to 4.8 percentage points more likely to receive a new loan from Banobras, compared to municipalities where the presidential party barely lost (columns 3 and 4 of Panel A in Table 2.2, respectively). These estimates are economically meaningful. Being politically aligned to the presidential party increases the probability of having a new loan from Banobras from 44% to 53%.

Our empirical strategy relies on the fact that municipalities where the presidential party barely lost are a reasonable counterfactual of municipalities where the presidential party barely won. Therefore, to interpret these results, note that the only difference between the two groups of municipalities around the winning threshold is their alignment status. This means that the state-owned bank is differentially allocating more loans to politically aligned municipalities. This result holds even if we move further away from the winning threshold and look at municipalities where the presidential party won by a larger margin (Figure 2.3b), although this is only suggestive, since we are only identified when the margin of victory is sufficiently close to zero.

Next, we turn to analyzing the results for credit from private banks. Whether the political alignment of a municipality should affect the allocation of credit by private banks or not is ambiguous. On the one side, if private banks perceive that the federal government is more likely to bail out aligned municipalities, then we would expect that these municipalities would receive more private credit. On the other side, if credit from Banobras substitutes credit from private banks, we would expect that aligned municipalities would get less private credit. Given this ambiguity, this is an empirical question. In Figure 2.3c we examine the effects of the alignment status of a municipality on the probability of having new loans from private banks. Columns 5 and 6 of Panel A in Table 2.2 report the estimated discontinuities when we consider a polynomial of degree 1 or 2 and bandwidths of 10% or 20%, respectively. In both specifications, the magnitudes are close to zero, insignificant, and not economically meaningful. Hence, in contrast with the results found for credit from the state-owned bank, the political alignment of the municipality does not affect the allocation of credit by private banks.

We also examine whether political alignment affects the amount of resources allocated to municipalities. In Figure 2.4 we plot the total loan size per capita, loan size per capita from Banobras, and loan size per capita from private banks.[11] As before, the figure plots the average loan size and a linear or quadratic prediction on either side of the winning threshold. The estimated discontinuities are reported in Panel B of Table 2.2. We find that aligned municipalities receive in loans 18.5 to 19.6 pesos per capita more than unaligned municipalities. These effects are mostly driven by the allocation of new loans from Banobras. On average, aligned municipalities receive 16.6–18 pesos per capita more from Banobras than do unaligned municipalities. These estimates are economically meaningful. On average, aligned municipalities receive 52.4%–58.4% more in resources from the state-owned bank. Similar to what we find for the extensive margin of credit, the estimated discontinuities for credit from private banks are small and statistically insignificant (columns 5 and 6 of Panel B of Table 2.2).

All of the results discussed so far are robust to the bandwidth selection and choice of kernels. Figure 2.D.1 shows estimates for discontinuities in the probability of having new loans

[11] In all cases, loan size is divided by the population observed during 2005 in each municipality. For simplicity, we will use loan size and loan size per capita interchangeably from now on.

from Banobras using different bandwidths and kernels. Similarly, Figure 2.D.2 shows estimates for discontinuities in the size of the loans from Banobras. As it is shown, results are not sensitive to the bandwidth selection or choice of kernel, and as expected, statistical power increases as we increase the bandwidth.[12] Figures 2.D.3 and 2.D.4 show the results for private credit. The estimates are close to zero and as expected, they become more precise as we increase the bandwidth.

2.4.2 Party preferences

In this section, we examine the effects on the allocation of credit by ruling party. As we described in Section 2.2, during the period of our study the presidential party switched from the PAN to the PRI, and along with this switch the director of Banobras changed from a PAN member to a PRI member. This setting provides variation in the presidential party that can be used to test whether public credit is allocated differentially depending on the policy preferences of each party. To test the latter, we look at the effects of having a PAN mayor during the PAN period and during the PRI period. Similarly, we also look at the effects of having a PRI mayor during the PAN period and during the PRI period. If both parties allocate more credit to municipalities represented by one of their members, then we would expect that PAN mayors receive more loans from Banobras during the PAN period and fewer loans during the PRI period, and vice versa.

We estimate the following model:

$$\begin{aligned} y_{it} = {} & \alpha_0 + \alpha_1 \mathbf{1}(PANmargin_{it} > 0) + \alpha_2 \mathbf{1}(PANmargin_{it} > 0) \times PRIperiod_t + \alpha_3 PRIperiod_t \\ & + \alpha_4 PANmargin_{it} + \alpha_5 PANmargin_{it} \times \mathbf{1}(PANmargin_{it} > 0) + \alpha_6 PANmargin_{it} \times PRIperiod_t \\ & + \alpha_7 PANmargin_{it} \times \mathbf{1}(PANmargin_{it} > 0) \times PRIperiod_t + u_{it} \end{aligned} \tag{2.2}$$

where $PANmargin_{it}$ is the margin of victory of the PAN in elections in municipality i immediately preceding year t. $PRIperiod_t$ is a dummy variable that takes the value of one if t is between December 2012 and May 2015 and zero otherwise. Note that we are not splitting municipalities across subgroups, but rather we are looking at the effects in different time periods. However, we

[12] To save space, similar results for other outcomes are available upon request.

allow for differential slopes across periods on either side of the winning threshold. Thus, α_1 is the effect of having a PAN mayor during the PAN period on the outcome of interest, whereas $\alpha_1 + \alpha_2$ is the effect of having a PAN mayor during the PRI period.[13] A similar specification is estimated when looking at the effects of having a PRI mayor on the outcome of interest, but instead we consider the PRI margin of victory.

Figure 2.5 shows point estimates and their 90% confidence intervals for the effects of being aligned to the PAN and the PRI during the PAN and PRI periods on the likelihood of getting new loans from Banobras and on their size. The estimated discontinuities are reported in column 1, Panels A and B of Table 2.3. All coefficients are estimated using local linear regression, triangular weights, a 10% bandwidth, and controlling for predetermined municipality characteristics and calendar year fixed effects. We observe that during the PAN period, municipalities with PAN mayors were 4.4 percentage points more likely to receive credit from Banobras, while during the PRI period they were 3.8 percentage points less likely to get credit (Figure 2.5a and Panel A of Table 2.3). A symmetric effect is observed for municipalities with PRI mayors. During the PAN period, municipalities with PRI mayors were 4.3 percentage points less likely to obtain credit from Banobras, while during the PRI period they were 3.3 percentage points more likely to get credit from Banobras (Figure 2.5b and Panel B of Table 2.3).[14] Note that since we are splitting the sample into two periods (PAN and PRI periods), standard errors are larger by approximately $\sqrt{2}$.

A similar pattern is observed when looking at the size of the loans. During the PAN period, municipalities where the PAN won by a small margin received from Banobras 23.4 pesos more, compared to municipalities where the PAN candidates lost by a small margin. In contrast, during the PRI period, they received 16.8 pesos less, compared to municipalities where PAN candidates lost by a small margin (Figure 2.5c and column 3 of Panel A in Table 2.3). The results are symmetric for PRI mayors. That is, municipalities where the PRI won by a small margin received 22.2 pesos less from Banobras during the PAN period, while they received 9.5 pesos more during the PRI period, compared to municipalities where the PRI lost by a small margin (Figure 2.5d and column

[13] As before, they are only identified at the vicinity of zero.

[14] Note that the effects are not perfectly symmetric, because Mexico has a multiparty system, meaning that a loss by the PRI does not necessarily imply a win by the PAN, and vice versa.

3 of Panel B in Table 2.3).[15]

Figure 2.6 and columns 2 and 4 of Table 2.3 document the effects of PAN and PRI alignment on the likelihood of having credit from private banks and on its size during both presidential terms. Unlike credit from Banobras, the allocation of credit by private banks is not affected by which political party is in power around the winning threshold.

2.4.3 Heterogeneity by state alignment

While government credit can be used to allocate resources to municipalities with more political proximity to the federal government, the amount of resources sent to municipal governments may vary by whether the state government is aligned or not (Curto-Grau et al., 2018). To explore the latter, we interact $\mathbf{1}(margin_{it} > 0)$ in equation 2.1 with a dummy variable that takes the value of one if the state government of municipality i is aligned and zero otherwise.[16] One important limitation of this approach is that state elections are not random, and thus it is possible that the interaction term is correlated with unobserved characteristics that explain the outcome variable of interest, invalidating the causal interpretation of the heterogeneous LATE (Becker et al., 2013). Below, we present results for our main sample, and in Table 2.B.2 in the Appendix we restrict our sample to municipal elections that were held after state elections (56% of the original sample), so that the alignment status of the state government is predetermined.[17] The main implications of the results are similar across both samples.

Results are reported in Table 2.4. We find that aligned municipalities in aligned states are 6 percentage points more likely to receive credit from Banobras than aligned municipalities in unaligned states. A similar pattern is observed when looking at the amount of resources in column 3, although the estimated difference in the discontinuity is not statistically significant. Columns 2 and 4 report the results for loans from private banks and we do not find differences in the discontinuities by the alignment status of the state. In Table 2.B.2, we show the results

[15] Note again that since the sample is divided into two periods, the standard errors grow by approximately $\sqrt{2}$.

[16] We define the alignment status of a state government by whether the governor belongs to the presidential party or not.

[17] In Mexico, state elections are every six years, whereas municipal elections are every three years. This setting allows us to hold fixed the alignment status of a state government and look at close municipal elections at the midterm of the governor.

when we consider the sample of municipalities in states that had elections before the municipal elections. For this subsample, we find a similar pattern, but with larger point estimates and more precisely estimated differences in the discontinuities.

2.4.4 Do citizens end up paying more?

In this section, we explore the additional payments by aligned municipalities given the allocated loans. Because these payments depend on the size of the loans and other contract characteristics, we begin by estimating discontinuities in the interest rate and the loan maturity.[18] Table 2.B.3 in the Appendix 2.B shows the results. We find that the state-owned bank allocates credit to aligned municipalities that is 4% cheaper, although this estimate is significant only at the 10% level. We do not find discontinuities in the loan maturity and the contract terms of private loans.[19]

Then, to determine whether aligned municipalities end up paying more for the loans, we compute the total payments of the loans by considering the initial size, price, and maturity agreed upon in the initial loan contract, and then explore discontinuities around the winning threshold.[20] Columns 1 and 2 of Table 2.5 show the estimated discontinuities for the overall payments. Regardless of the specification, aligned municipalities pay mechanically more for the allocated loans, compared to unaligned municipalities. Columns 3 and 4 show the estimated discontinuities for payments to Banobras, whereas columns 5 and 6 show the estimated discontinuities for payments to private banks.

The results show that aligned municipalities pay 23–24.2 pesos per capita more in debt services and these extra payments are entirely driven by payments to the state-owned bank. These

[18] These measures are typically used in the literature that studies the allocation of credit by state-owned banks. See, for example, Khwaja and Mian (2008) and Cole (2009).

[19] We conduct several robustness tests of these results. First, in the Appendix we provide discontinuity estimates for the interest rate from Banobras considering different bandwidths and kernels. Additionally, to deal with selection into the credit market, Table 2.E.4 in the Appendix reports estimates for Lee bounds considering a 1% bandwidth. While conservative, these bounds provide best- and worst-case scenarios supported by the data of the treatment effects for municipalities that are always selected into the credit market.

[20] Section 2.F in the Appendix describes in more detail the amortization formula and the assumptions used to compute these payments. We use calculated annuities instead of the flow of payments, because in our sample period we cannot observe the full set of payments (i.e., many loans are due after our sample period). It is also important to mention that few loans have repayment issues in our setting, because the federal government earmarks formula-based transfers for debt services.

estimates and the estimates reported in Table 2.2 imply that citizens in aligned municipalities pay, on average, 4.5 pesos more in interest for the allocated loans.

2.5 Public investment

So far, this paper documents that municipalities politically aligned with the federal government receive more credit, and these effects are entirely driven by the allocation of the state-owned development bank. Previous empirical work has shown for municipalities in the US that financial constraints can affect local employment and growth (Adelino et al., 2017). In this section, we ask whether politically aligned municipalities increase public investment or not, in particular investment in public infrastructure.[21]

2.5.1 Strategy

Our goal is to test whether more credit has an effect on public investment or not. A simple model that regresses measures of public investment on credit would lead to biased estimates, if credit is correlated with unobservable characteristics that explain public investment. While close municipal elections can cleanly identify the LATE of the alignment status of a municipality on access to credit, they cannot be used to instrument credit to explain public investment, because political alignment affects other outcomes that can be correlated with public investment, invalidating the exclusion restriction.[22] Given that close elections cannot be used to instrument credit, in this section we present reduced form effects of estimations of equation 2.1, considering the outcomes described in this section. Reduced form estimates inform about the overall effects of political alignment on public investment, considering that one of the mediators of the effects is more public credit.

[21] In this section, the unit of observation is a municipality$\times$year.

[22] See, for example, Brollo and Nannicini (2012), who use a similar empirical strategy to study if political alignment affects the allocation of fiscal transfers to Brazilian municipalities. In their study, the authors find that municipalities where the presidential party had a close win receive one-third more resources through discretionary transfers. Also, Asher and Novosad (2017) find that localities aligned to the federal government in India receive more mining permits.

2.5.2 Effects on public expenditures

We begin by exploring the reduced form effects of close elections on the main components of municipalities' annual expenditures. As we described in Section 2.2, these data reports the aggregate expenditures of the municipality on wages, supplies, procured services, subsidies and transfers, assets acquisitions, public investments, and debt services, among other expenditures. We consider these outcomes and estimate equation 2.1 with a 10% bandwidth, triangular weights, and a polynomial of degree 1.

Table 2.6 presents the main results. We find a positive and statistically significant discontinuity in expenditures on supplies. Specifically, municipalities aligned to the federal government spend 69.5 pesos per capita more on supplies than unaligned municipalities, off of a mean of 326 pesos per capita (Column 2). These types of expenditures are those related to the procurement of supplies used by local governments for the provision of goods and services and for the performance of day-to-day administrative activities. In the Appendix we show that these estimates are robust to the bandwidth selection and to the weights considered. We also find in Column 7 that aligned municipalities pay 29.7 pesos per capita more for debt services than unaligned municipalities, off of a mean of 104 pesos per capita. This estimate is statistically significant at the 10% level, but we show in the Appendix that as we increase the bandwidth, the precision of this estimate increases and becomes significant at the 5% level. These expenditures refer to debt services related to loans and other inherited debts from previous administrations. This result is consistent with the results found for the total payments of allocated loans in Table 2.5, providing additional evidence for the idea that aligned municipalities end up paying more for their loans.

We do not find evidence that aligned municipalities increase or decrease expenditures in other categories. However, the estimates for the other categories of public expenditures are imprecisely estimated. For example, while we do find a positive discontinuity in public investments in Column 6 (80.64), its standard error is large (161.8). With this standard error, we can only detect effect sizes higher than 22% of the mean. We also estimate negative discontinuities in other expenditures. Specifically, aligned municipalities report spending less in subsidies and transfers and in asset acquisitions (Columns 4 and 5, respectively). Yet, as described before, these estimates

are imprecisely estimated.

2.5.3 Investments in service delivery

As mentioned in the previous section, the data on public expenditures are aggregated at higher levels of expenditures. This limits our ability to understand whether aligned municipalities change the composition of expenditures within each category. To overcome this limitation, in this section we focus on different measures of the public infrastructure used to deliver goods and services. In our setting, municipalities are mandated by the Constitution to provide the following goods and services: public security, drinking water and sewerage, street lighting, collection and final disposal of waste, public markets, cemeteries, slaughterhouses, and streets, parks, and other amenities. We focus on measures that allow the municipal government to increase the provision of some of these goods and services and estimate the reduced form effects of political alignment on these outcomes using data for 2012 and 2014. Specifically, we estimate the effects on the number of police stations, the number of jail cells, the number of streetlight posts, a dummy variable indicating whether the municipality collects waste, the number of waste disposal vehicles, the number of water sources in the municipality, and a dummy variable indicating whether the municipality provides sewerage or not. The variables measured in quantities are per 10,000 inhabitants and are transformed using the inverse hyperbolic sine transformation, since there are few municipalities that report zeros and this transformation allows us to deal with some outliers. Additionally, we complement this information with the logarithm of the night light intensity in the municipality. For 2012, we use data from the DMSP satellite, whereas for 2014 we use data from the VIIRS satellite. Finally, we construct a standardized index using the first component of a principal component analysis to summarize the information of all of these variables.[23]

The results are shown in Table 2.7. We do not find statistically significant discontinuities in any of these measures and the estimates for some outcomes are positive while those for others are negative. When we look at the estimate for the index, we find a negative discontinuity of 0.07σ, albeit the estimated standard error is large and we cannot reject the null that this estimate is

[23] We exclude streetlight posts from this index, because we can only observe this variable for 2014.

different from zero. Taken together, we interpret these results as showing no evidence that aligned municipalities report improvements in service delivery.

2.6 Discussion

The results shown in Section 2.4 establish that municipal governments politically aligned to the federal government receive more public credit from the state-owned development bank. There are several hypotheses that could explain these results. First, it is possible that the state-owned development bank allocates more resources to aligned municipalities because their mayors can help the presidential party implement its policy platform. This would be true if preferences for public spending between aligned and unaligned municipalities differ systematically. While we cannot directly test these hypothesis, the results presented in Table 2.6 and Table 2.7 suggest that this is not the case. We only find an increase in expenditures on supplies and debt services, the latter being consistent with the fact that aligned municipalities receive more credit. We also don't find significant differences in different measures of investments in service delivery. Taken together, these results suggest, in the aggregate, that there are no systematic differences in spending preferences between aligned and unaligned municipalities.[24]

Another possible explanation of the main results is that the presidential party uses public credit to favor municipalities that share its ideology. Under the assumption that at the winning threshold the demand for credit is similar for both aligned and unaligned municipalities, then our results would suggest this hypothesis. However, mayors who belong to the presidential party may borrow more because they face a softer budget constraint. For instance, they may receive more discretionary transfers from the federal government (Brollo and Nannicini, 2012). In Mexico, the majority of the revenue of the municipal government comes from transfers from upper levels of government (83% in our study period). These are mostly formula-based and depend on

[24]We also tested in Table 2.E.2 in the Appendix whether the state-owned bank allocates more resources to places with a high concentration of firms that each presidential party considers strategic for development. If the state-owned bank allocates more credit to aligned places with a high concentration of firms that each president considers strategic, then we would expect heterogeneous local average treatment effects by this dimension. We do not find evidence for this hypothesis.

the population in the municipality, the local GDP, and previous local revenue collection.[25] Some municipalities also receive additional earmarked transfers which are not formula-based and are allocated at the discretion of the federal government.[26] An important limitation of the data from INEGI is that it doesn't allow us to distinguish between formula-based and discretionary transfers.

In Table 2.B.4 in the Appendix we present discontinuity estimates for the total earmarked and unmarked transfers. We find small and statistically insignificant discontinuities for both types of transfers, although we can only detect effect sizes higher than 11.2% of the mean. The point estimates are positive and small relative to the mean, but the data do not allow us to determine with precision whether politically aligned municipalities face a softer budget constraint. The fact that the point estimates are positive, however, suggest that mayors that belong to the presidential party face a softer budget constraint, leaving this hypothesis as a plausible interpretation of the results.

2.7 Conclusion

This paper provides causal evidence on the effects of being politically aligned to the federal government on the allocation of public credit to Mexican municipalities. Using quasi-experimental variation in mayoral elections, this paper shows that, during the period 2009–2015, municipalities politically aligned to the federal government were more likely to receive credit from the state-owned development bank, both in the extensive and intensive margins. We also find that private banks do not allocate credit to municipalities differentially depending on the party affiliation of the local politicians.

We then explore whether politically aligned municipalities increase investments in service delivery. Using different measures related to the main public goods and services of responsibility of local governments in our setting, we do not find evidence of this. However, this paper is limited in its ability to test the direct effects of public credit on public investment, and only speaks to the overall effect of political alignment on the outcomes of interest, considering that one of the

[25] These transfers are commonly known as *Ramo 28* and *Ramo 33*.
[26] These transfers are commonly known as *Ramo 23*.

mediators is more credit from the state-owned development bank. We believe that exploring this direct relationship is an avenue for future research.

2.8 Acknowledgments

This chapter, in full, is currently being prepared for submission for publication of the material. de la Garza, Adrian; Lopez-Videla, Bruno. "The Effects of Political Alignment on the Allocation of Public Credit." The dissertation author was a primary investigator and author of this material.

Figures

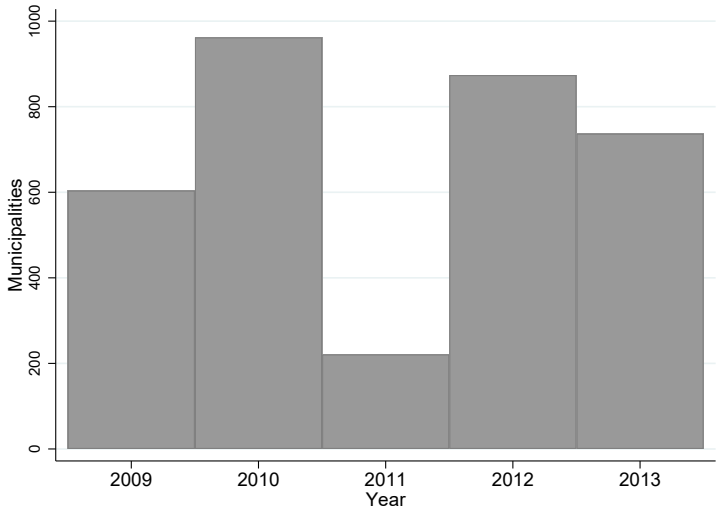
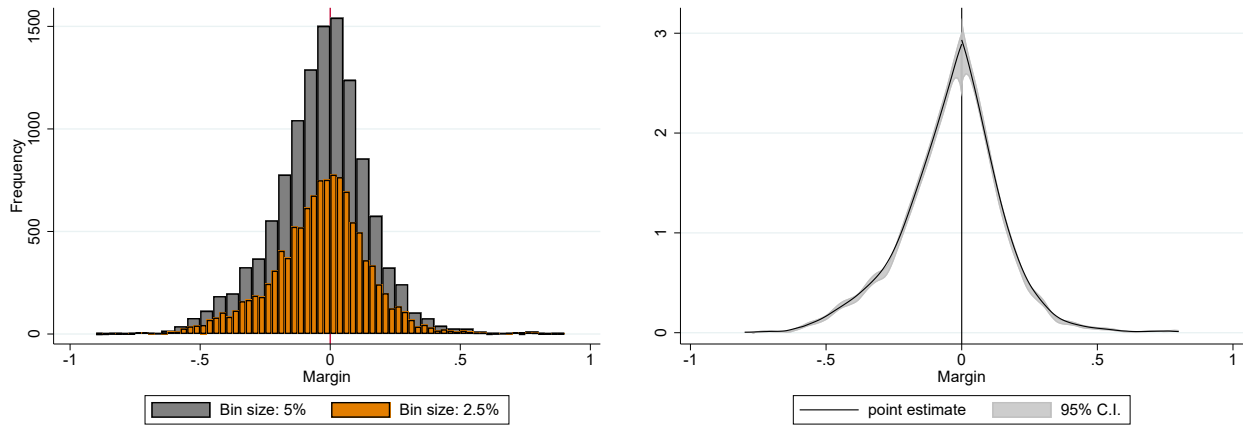


Figure 2.1: Municipal elections over time

Notes: The figure shows the timeline of the 3,398 municipal elections considered in our study. In December of 2012, the presidential party switched from the PAN from Felipe Calderón to the PRI from Enrique Peña Nieto. We exclude elections in Mexico City and in the state of Oaxaca. Data on election years come from the national electoral authority (INE).

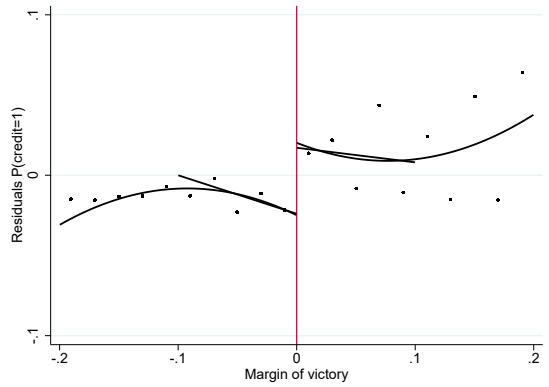


(a) Histogram

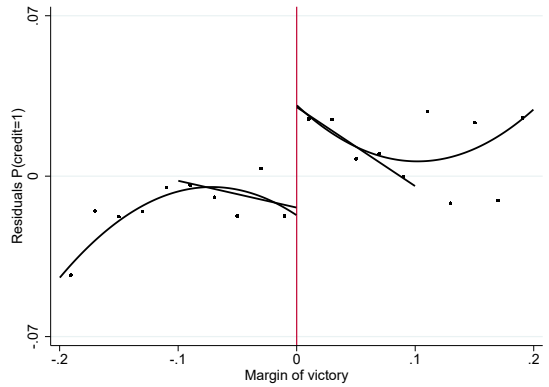
(b) Manipulation test

Figure 2.2: Testing for manipulation of the running variable

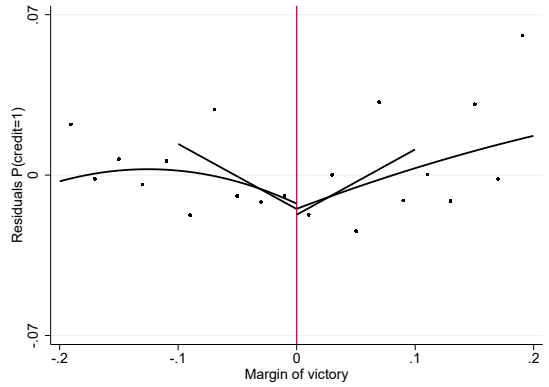
Notes: The figure tests for manipulation of municipal elections from 2009 to 2013. In panel (a) we show the histogram of the margin of victory for +/-100% points range with 2.5 and 5 percentage points bins. In panel (b) we test for manipulation of the running variable (McCrary, 2008) and we fail to reject the null hypothesis of no discontinuity around the winning threshold (p-val=0.62).



(a) Total



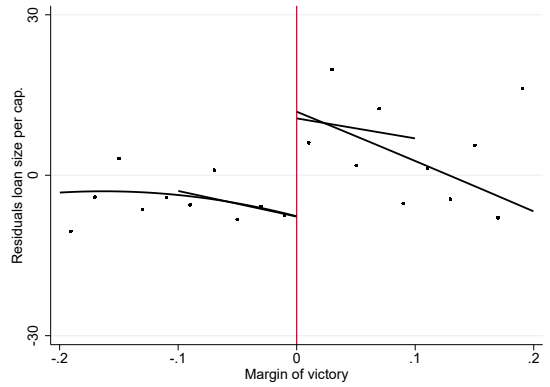
(b) State-owned



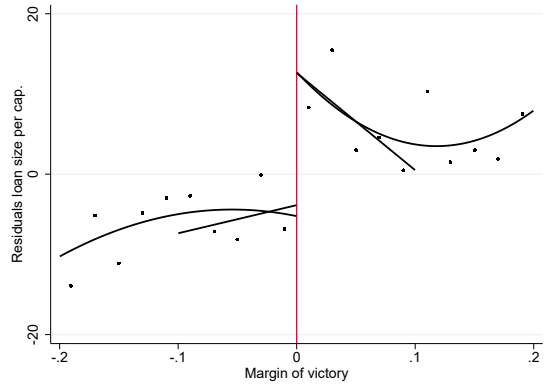
(c) Private Banks

Figure 2.3: Probability of Having Credit RD Figures

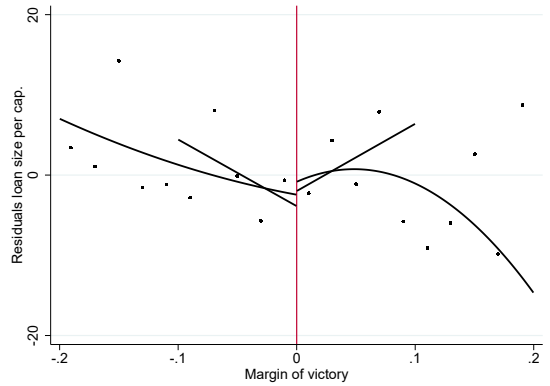
Notes: The figure plots the probability of having new credit against the presidential party margin of victory. Negative values of the margin of victory indicate municipalities where the presidential party lost, whereas positive values indicate municipalities where the presidential party won. Each dot corresponds to the average value of the outcome variable within two percentages points bin. The solid lines plot the predicted values of a linear or quadratic regression estimated separately on either side of the winning threshold within the 10% or 20% bandwidth, respectively. Finally, the outcomes are residualized considering predetermined municipality characteristics and calendar year fixed effects.



(a) Total



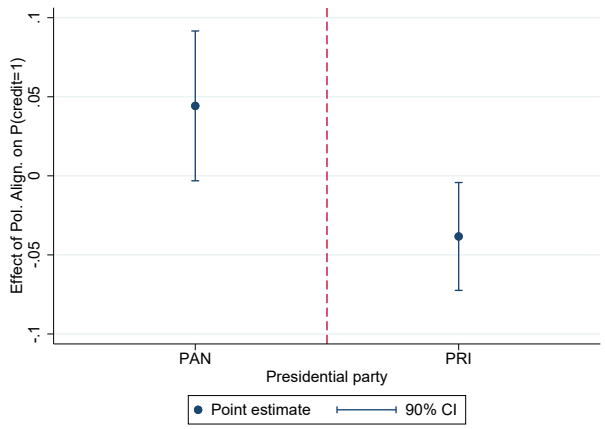
(b) State-owned



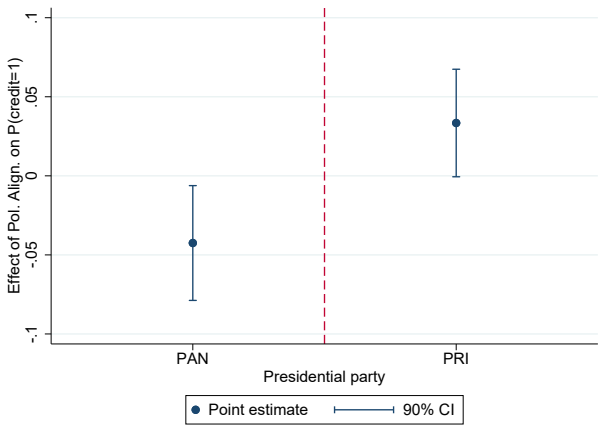
(c) Private Banks

Figure 2.4: Loan Size per Capita RD Figures

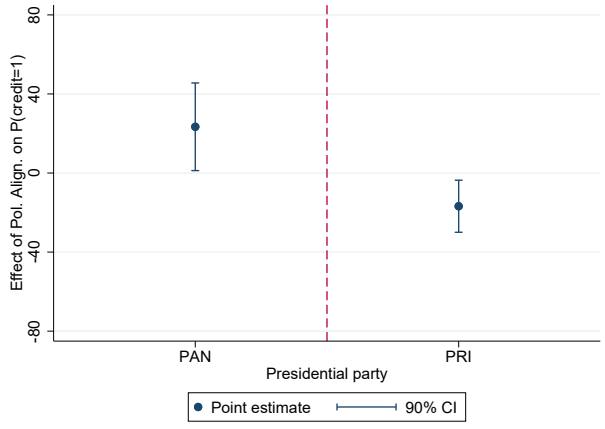
Notes: The figure plots loan size per capita against the presidential party margin of victory. Negative values of the margin of victory indicate municipalities where the presidential party lost, whereas positive values indicate municipalities where the presidential party won. Each dot corresponds to the average value of the outcome variable within two percentages points bin. The solid lines plot the predicted values of a linear or quadratic regression estimated separately on either side of the winning threshold within the 10% or 20% bandwidth, respectively. Finally, the outcomes are residualized considering predetermined municipality characteristics and calendar year fixed effects.



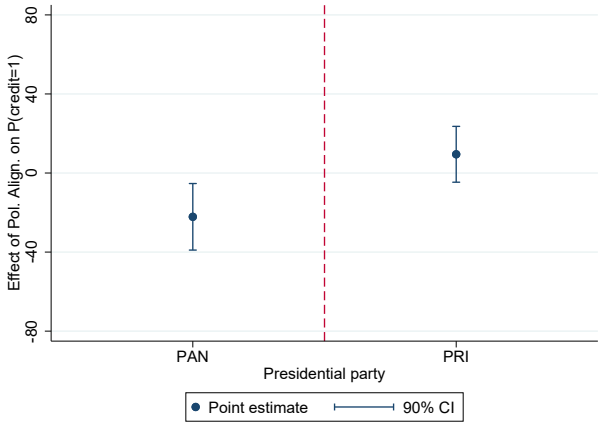
(a) PAN margin of victory
P(credit=1)



(b) PRI margin of victory
P(credit=1)



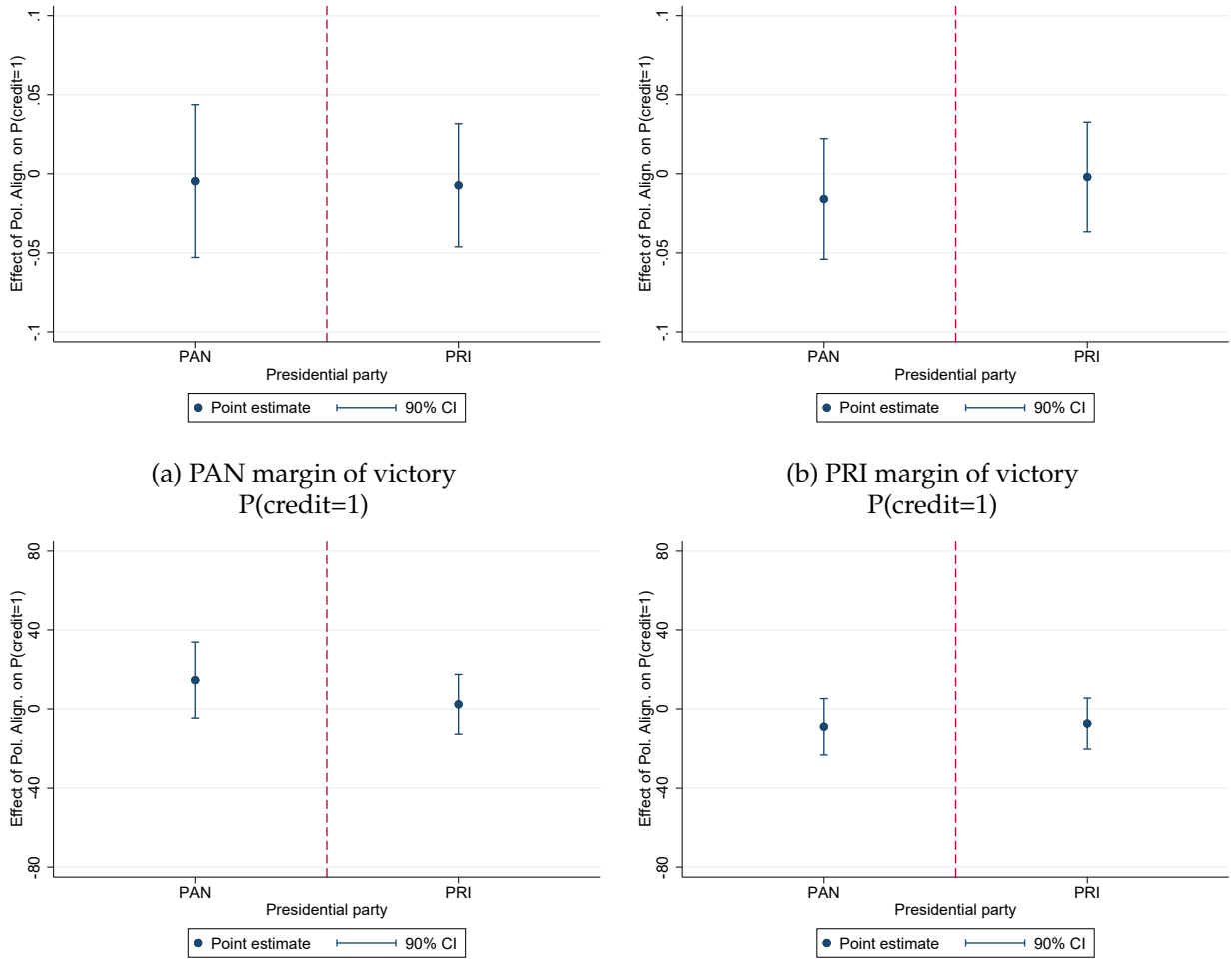
(c) PAN margin of victory
Loan size per capita



(d) PRI margin of victory
Loan size per capita

Figure 2.5: Effect by political party: state-owned

Notes: The figure plots point estimates and its 90% confidence interval for the discontinuity in the probability of having new credit (figures (a) and (b)) and the loan size per capita (figures (c) and (d)) from the state-owned bank during the PAN and the PRI presidential terms. Figures (a) and (c) consider the PAN margin of victory whereas figures (b) and (d) consider the PRI margin of victory. Each discontinuity is estimated using a local polynomial regression of degree 1, a bandwidth of 10%, triangular Kernels, year fixed effects and predetermined municipal covariates. Standard errors are clustered at the municipality level.



(a) PAN margin of victory P(credit=1)

(b) PRI margin of victory P(credit=1)

(c) PAN margin of victory Loan size per capita

(d) PRI margin of victory Loan size per capita

Figure 2.6: Effect by political party: private banks

Notes: The figure plots point estimates and its 90% confidence interval for the discontinuity in the probability of having new credit (figures (a) and (b)) and the loan size per capita (figures (c) and (d)) from private banks during the PAN and the PRI presidential terms. Figures (a) and (c) consider the PAN margin of victory whereas figures (b) and (d) consider the PRI margin of victory. Each discontinuity is estimated using a local polynomial regression of degree 1, a bandwidth of 10%, triangular Kernels, year fixed effects and predetermined municipal covariates. Standard errors are clustered at the municipality level.

Table 2.1: Summary Statistics of Main Variables

	Unaligned Municipalities			Aligned Municipalities			
	Mean (1)	Std. Dev. (2)	N (3)	Mean (4)	Std. Dev. (5)	N (6)	t-stat (7)
Panel A. Credit characteristics							
Total credit							
Pr(credit=1)	0.17	0.37	2717	0.19	0.39	2745	2.54
Size per cap.	55.12	167.18	2717	67.86	190.51	2745	2.63
Loan price	7.60	2.15	468	7.35	2.04	543	-1.92
Maturity	3.24	4.30	462	3.44	4.50	541	0.74
State-owned bank							
Pr(credit=1)	0.10	0.30	2717	0.12	0.32	2745	2.7
Size per cap.	31.64	120.89	2717	43.08	146.43	2745	3.15
Loan price	6.71	0.96	270	6.59	1.12	331	-1.4
Maturity	4.57	5.42	270	4.59	5.00	331	0.03
Private banks							
Pr(credit=1)	0.09	0.28	2717	0.08	0.28	2745	-0.07
Size per cap.	23.48	109.55	2717	24.77	120.22	2745	0.41
Loan price	8.56	2.62	244	8.35	2.70	245	-0.85
Maturity	1.89	3.32	238	1.94	3.42	242	0.15
Panel B. Municipality characteristics							
Economic characteristics							
log(GDP per cap.)	8.67	0.43	2717	8.69	0.43	2745	1.51
Avg. years of schooling	6.28	1.41	2717	6.32	1.38	2745	1.06
Marginality Index	-0.20	0.89	2717	-0.24	0.89	2745	-1.7
Human Development Index	0.76	0.06	2717	0.76	0.06	2745	1.4
Sociodemographic characteristics							
log(population)	9.71	1.35	2717	9.64	1.32	2745	-1.76
log(number of houses)	8.25	1.33	2717	8.19	1.29	2745	-1.67
log(pop. with social security)	8.37	1.85	2717	8.37	1.76	2745	-0.06
Panel C. Public expenditures							
Wages	1355.52	963.51	1857	1443.04	1251.84	1939	2.42
Supplies	325.95	315.12	1857	357.82	378.43	1939	2.82
Services	553.10	400.25	1857	574.73	507.50	1939	1.46
Subsidies and Transfers	383.27	385.63	1857	382.00	429.55	1939	-0.1
Assets	67.18	380.41	1857	62.65	143.83	1939	-0.48
Public Investment	1459.63	1578.26	1857	1430.72	1403.48	1939	-0.6
Debt Services	103.88	180.46	1857	120.89	252.01	1939	2.4
Other	18.60	200.91	1857	27.52	238.74	1939	1.25
Panel D. Capacity and public infrastructure							
Police Stations	2.11	2.91	813	2.85	21.87	860	0.59
Jail Cells	3.12	5.33	799	3.11	7.21	850	0.21
Street lighting	1098.15	887.81	450	1122.04	934.28	477	0.77
Collects waste	0.99	0.11	857	0.99	0.09	881	-0.76
Waste disposal vehicles	2.39	2.83	820	2.47	3.34	861	0.94
Water Sources	6.13	11.49	826	5.76	8.52	859	-0.53
Sewerage	0.88	0.32	850	0.88	0.33	868	0.17

Notes: This table shows summary statistics of the main variables for aligned and unaligned municipalities where the presidential party won or lost, respectively, by less than 10 percentage points. Columns 1-3 show summary statistics for unaligned municipalities whereas columns 4-6 show summary statistics for aligned municipalities. Column (7) shows the t-statistic for difference in means across the two groups of municipalities. Municipality characteristics are measured in 2005, while variables in pesos are all measured per capita and in Mexican pesos of 2010. Police stations, jail cells, street lighting, waste disposal vehicles and water sources are per 10,000 inhabitants. The unit of observation in panels A and B is municipality×mayor×year whereas the unit of observation in panels C and D is municipality×year because this data are reported for calendar years.

Table 2.2: Effects of political alignment on credit

	Total		State-owned		Private banks	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. P(credit=1)						
Aligned	0.0422**	0.0457**	0.0442***	0.0481***	-0.00170	-0.00180
	(0.0214)	(0.0228)	(0.0165)	(0.0176)	(0.0182)	(0.0194)
Dep. Var. mean	0.17	0.17	0.10	0.09	0.09	0.09
Observations	5462	8656	5462	8656	5462	8656
R^2	0.126	0.125	0.068	0.068	0.093	0.093
Panel B. Loan size per capita						
Aligned	18.49*	19.63*	16.57**	17.97**	1.913	1.662
	(9.738)	(10.41)	(7.513)	(8.004)	(6.221)	(6.646)
Dep. Var. mean	55.12	55.98	31.64	30.77	23.48	25.21
Observations	5462	8656	5462	8656	5462	8656
R^2	0.078	0.075	0.042	0.040	0.079	0.074
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	10%	20%	10%	20%	10%	20%
Kernel	Triangle	Triangle	Triangle	Triangle	Triangle	Triangle
Polynomial degree	1	2	1	2	1	2

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows discontinuity estimates for the probability of having new credit and the size of the loans per capita. Regressions consider local polynomial regression with a 10% bandwidth and polynomials of degrees 1 (columns 1, 3 and 5) and 2 (columns 2, 4 and 6). All regressions include municipality predetermined characteristics and year fixed effects. In addition, all regressions consider a triangle Kernel. Dependent variable means are computed for unaligned municipalities within the 10 percentage points range. Standard errors shown in parentheses are clustered at the municipality level.

Table 2.3: Effects by party coalition

	Pr(credit=1)		Loan size per cap.	
	(1)	(2)	(3)	(4)
	State-owned	Private banks	State-owned	Private banks
Panel A. PAN margin of victory				
PAN x PAN period	0.0442	-0.00461	23.39*	14.61
	(0.0288)	(0.0294)	(13.48)	(11.69)
PAN x PRI period	-0.0383*	-0.00725	-16.80**	2.392
	(0.0207)	(0.0237)	(8.006)	(9.189)
Difference	-0.0826***	-0.00264	-40.20***	-12.22
	(0.0299)	(0.0316)	(13.98)	(11.62)
Dep. Var. Mean	0.11	0.10	38.12	30.22
Observations	4627	4627	4627	4627
R^2	0.0713	0.105	0.0438	0.0867
Panel B. PRI margin of victory				
PRI x PAN period	-0.0425*	-0.0159	-22.16**	-8.948
	(0.0221)	(0.0232)	(10.23)	(8.661)
PRI x PRI period	0.0334	-0.00202	9.475	-7.345
	(0.0207)	(0.0211)	(8.588)	(7.833)
Difference	0.0759***	0.0139	31.63***	1.603
	(0.0250)	(0.0260)	(11.10)	(8.661)
Dep. Var. Mean	0.10	0.07	36.28	17.9
Observations	5994	5994	5994	5994
R^2	0.0653	0.0944	0.0431	0.0777
Covariates	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Bandwidth	10%	10%	10%	10%
Kernel	Triangle	Triangle	Triangle	Triangle
Polynomial degree	1	1	1	1

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows discontinuity estimates for the probability of having credit and the loan size per capita from the state-owned bank and from private banks. Panel A considers the PAN margin of victory while Panel B considers the PRI margin of victory. PAN period refers to the period when the presidential party was the PAN, whereas PRI period refers to the period when the presidential party was the PRI. Columns 1 and 2 report discontinuity estimates for the probability of having new loans from Banobras and private banks, respectively. Columns 3 and 4 report discontinuity estimates for the loan size of new loans from Banobras and private banks, respectively. All regressions include municipality predetermined characteristics and year fixed effects. In addition, all regressions consider a triangle Kernel. Dependent variable means are computed for municipalities at the left of the threshold within the 10 percentage points range. Standard errors shown in parentheses are clustered at the municipality level.

Table 2.4: Effects by alignment of the state government

	P(credit=1)		Loan size per cap.	
	(1) State-owned	(2) Private banks	(3) State-owned	(4) Private banks
Aligned	0.0136	-0.0121	6.676	-0.613
	(0.0246)	(0.0292)	(10.56)	(10.59)
Aligned x Aligned State	0.0607*	0.0184	19.67	4.927
	(0.0362)	(0.0403)	(15.44)	(16.10)
Sum of effects				
Aligned +	0.0744***	0.00631	26.35**	4.314
Aligned x Aligned State	(0.0240)	(0.0257)	(10.74)	(9.926)
Dep. Var. Mean	0.10	0.09	31.64	23.48
Observations	5462	5462	5462	5462
R^2	0.0694	0.0940	0.0430	0.0793
Covariates	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Bandwidth	10%	10%	10%	10%
Kernel	Triangle	Triangle	Triangle	Triangle
Polynomial degree	1	1	1	1

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows heterogeneity of the effects of political alignment on credit by the political alignment status of the state. We report regressions with triangular weights considering a 10% bandwidth, covariates and differential splines across subgroups at either side of the winning threshold. Standard errors clustered at the municipality level are shown in parentheses.

Table 2.5: Effects on total payments

	Total		State-owned		Private banks	
	(1)	(2)	(3)	(4)	(5)	(6)
Aligned	22.95**	24.21**	21.19**	22.77**	1.763	1.439
	(11.10)	(11.89)	(9.028)	(9.642)	(6.422)	(6.868)
Dep. Var. Mean	62.45	63.29	37.20	36.23	25.25	27.07
Observations	5462	8656	5462	8656	5462	8656
R^2	0.077	0.074	0.040	0.039	0.081	0.076
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	10%	20%	10%	20%	10%	20%
Kernel	Triangle	Triangle	Triangle	Triangle	Triangle	Triangle
Polynomial degree	1	2	1	2	1	2

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows discontinuity estimates for total loan payments. Loan payments were computed at the time the loan was granted, assuming a fixed interest rate and monthly installments. Regressions consider local polynomial regression with a 10% bandwidth and polynomials of degrees 1 (columns 1, 3 and 5) and 2 (columns 2, 4 and 6). All regressions include municipality predetermined characteristics and year fixed effects. In addition, all regressions consider a triangle Kernel. Dependent variable means are computed for unaligned municipalities withing the 10 percentage points range. Standard errors shown in parentheses are clustered at the municipality level.

Table 2.6: Effects on public expenditures

	Wages	Supplies	Services	Subsidies and Transfers	Assets	Public Investment	Debt Services	Other
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Aligned	124.4	69.49**	58.60	-37.55	-18.31	80.64	29.72*	11.19
	(148.3)	(31.99)	(50.31)	(38.84)	(19.22)	(161.8)	(17.80)	(16.41)
Dep. Var. Mean	1355.5	325.9	553.1	383.3	67.18	1459.6	103.9	18.60
Observations	3796	3796	3796	3796	3796	3796	3796	3796
R^2	0.341	0.354	0.378	0.0840	0.0105	0.170	0.0358	0.00917
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	10%	10%	10%	10%	10%	10%	10%	10%
Kernel	Triangle	Triangle	Triangle	Triangle	Triangle	Triangle	Triangle	Triangle
Polynomial degree	1	1	1	1	1	1	1	1

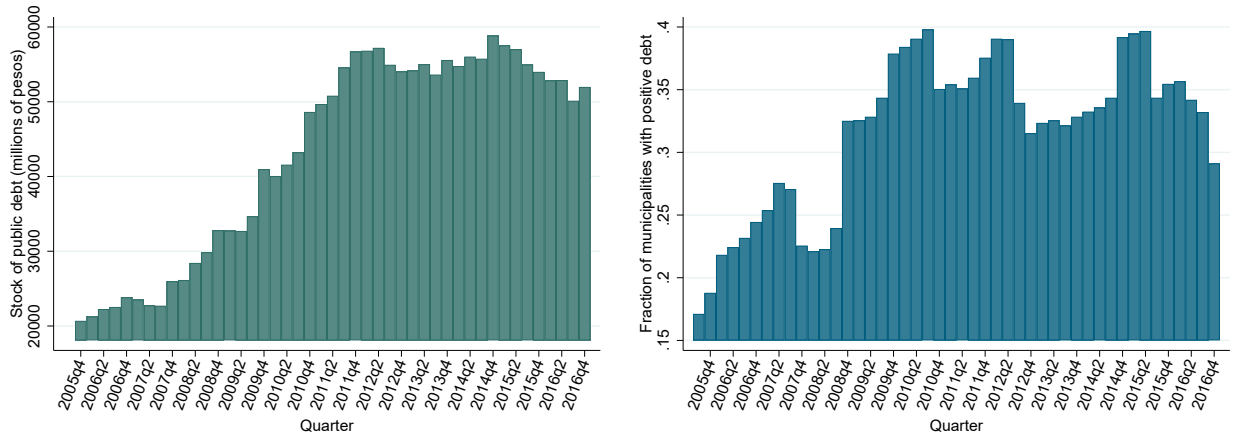
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows discontinuity estimates for municipal government's expenditures. All outcome variables are per capita. Regressions consider local polynomial regression with a 10% bandwidth and polynomials of degree 1. All regressions include municipality predetermined characteristics and calendar year fixed effects. In addition, all regressions are weighted using a triangle Kernel. Dependent variable means are computed for unaligned municipalities within the 10 percentage points range. Standard errors shown in parentheses are clustered at the municipality level.

Table 2.7: Effects on public infrastructure

	Police stations	Jail Cells	Street lighting	Collects waste	Waste disposal vehicles	Water sources	Sewerage	log(DMSP)	log(VIIRS)	Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Aligned	-0.0606	-0.0581	0.130	0.00186	-0.0220	-0.0649	0.0411	-0.0846	-0.0155	-0.0765
	(0.0709)	(0.0607)	(0.0952)	(0.0110)	(0.0449)	(0.0855)	(0.0322)	(0.0676)	(0.0822)	(0.0630)
Observations	1673	1649	927	1702	1681	1685	1684	709	959	1581
R^2	0.275	0.232	0.678	0.0319	0.756	0.331	0.125	0.679	0.774	0.648
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	10%	10%	10%	10%	10%	10%	10%	10%	10%	10%
Kernel	Triangle	Triangle	Triangle	Triangle	Triangle	Triangle	Triangle	Triangle	Triangle	Triangle
Polynomial degree	1	1	1	1	1	1	1	1	1	1

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows discontinuity estimates for outcomes related to the municipal government's capacity to provide public goods and other measures of public infrastructure in 2012 and 2014. The outcomes in columns 1, 2, 3, 5 and 6 have been transformed using the inverse hyperbolic sine transformation. The outcomes in columns 4 and 7 are dummy variables that take the value of one if the municipal government collects waste or provides sewerage, respectively. The outcome in column 8 is the log of the nightlight intensity in the municipality measured by the DMSP satellite in 2012, whereas the outcome in column 9 is the log of the nightlight intensity in the municipality measured by the VIIRS satellite. Column 10 considers an standardized index constructed with the first component of a principal component analysis using the variables in columns 1, 2, and 4-7. We do not consider Street lighting because this information is only available for the year 2014. Standard errors shown in parentheses are clustered at the municipality level.

2.A Appendix: Additional Figures

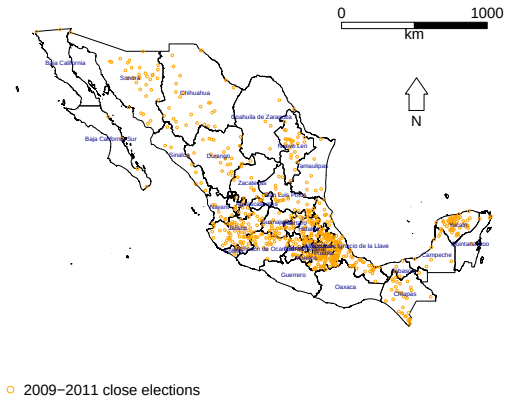


(a) Stock of Public Debt

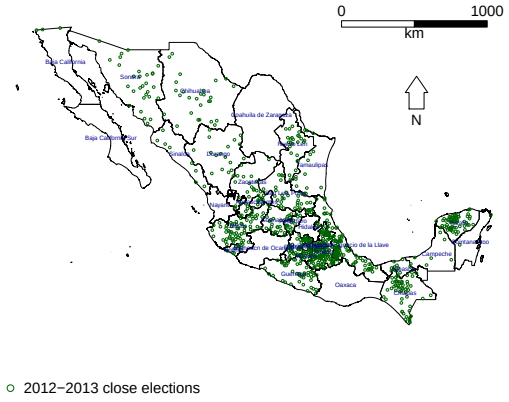
(b) Municipalities with Debt

Figure 2.A.1: Local Public Debt

Notes: Panel (a) shows the stock of local public debt per quarter. Panel (b) shows the fraction of municipalities with public debt in each quarter. The data on the stock of debt are in real pesos and come from the Secretary of Finance and Public Credit.



(a) 2009–2011 elections



(b) 2012–2013 elections

Figure 2.A.2: Geographic variation of competitive elections

Notes: The figure shows municipal elections with a margin of victory of +/-10%. Panel (a) shows close municipal elections from 2009-2011 while panel (b) shows close municipal elections from 2012-2013. Each dot corresponds to the capital of the municipality.

2.B Appendix: Additional Tables

Table 2.B.1: Municipal Elections per State and Year

States	2009	2010	2011	2012	2013
Aguascalientes		x			x
Baja California		x			x
Baja California Sur			x		
Campeche	x			x	
Chiapas		x		x	
Chihuahua		x			x
Coahuila de Zaragoza	x				o
Colima	x			x	
Mexico City	o			o	
Durango		x			x
Guanajuato	x			x	
Guerrero				x	
Hidalgo			x		
Jalisco	x			x	
Michoacan de Ocampo			x		
Morelos	x			x	
Mexico	x			x	
Nayarit			x		
Nuevo Leon	x			x	
Oaxaca		o			o
Puebla		x			x
Queretaro	x			x	
Quintana Roo		x			x
San Luis Potosi	x			x	
Sinaloa		x			x
Sonora	x			x	
Tabasco	x			x	
Tamaulipas		x			x
Tlaxcala		x			x
Veracruz de Ignacio de la Llave		x			x
Yucatan		x		x	
Zacatecas		x			x

Notes: The table shows the states with municipal elections during each year. Elections in 2009, 2010, 2011 and 2013 were held in different periods, whereas elections in 2012 were all held in July along with the presidential elections. We consider elections in states and years marked with an "x".

Table 2.B.2: Effects by alignment of the state government

	P(credit=1)		Loan size per cap.	
	(1) State-owned	(2) Private banks	(3) State-owned	(4) Private banks
Aligned	-0.00306	0.0195	3.247	7.320
	(0.0292)	(0.0425)	(12.81)	(17.15)
Aligned x Aligned State	0.128***	-0.0204	47.69**	9.753
	(0.0470)	(0.0583)	(23.03)	(23.13)
Sum of effects				
Aligned + Aligned x Aligned State	0.125***	-0.000877	50.94***	17.07
	(0.0363)	(0.0407)	(18.35)	(14.88)
Dep. Var. Mean	0.10	0.11	34.04	29.84
Observations	3040	3040	3040	3040
R^2	0.121	0.105	0.0807	0.0948
Covariates	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Bandwidth	10%	10%	10%	10%
Kernel	Triangle	Triangle	Triangle	Triangle
Polynomial degree	1	1	1	1

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows heterogeneity of the effects of political alignment on credit by the political alignment status of the state. The sample is restricted to municipal elections held after state elections, so that the alignment status of the state government is predetermined. We report regressions with triangular weights considering a 10% bandwidth, covariates and differential splines across subgroups at either side of the winning threshold. Standard errors clustered at the municipality level are shown in parentheses.

Table 2.B.3: Political alignment and credit: loan contract terms

	Total		State-owned		Private banks	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Loan price						
Aligned	-0.372	-0.360	-0.251*	-0.242*	0.313	0.281
	(0.305)	(0.325)	(0.134)	(0.143)	(0.511)	(0.558)
Dep. Var. mean	7.63	7.60	6.73	6.76	8.61	8.5
Observations	979	1583	591	921	465	796
R^2	0.259	0.265	0.517	0.520	0.408	0.403
Panel B. Maturity						
Aligned	0.338	0.168	-0.680	-0.899	0.564	0.450
	(0.559)	(0.602)	(0.709)	(0.773)	(0.580)	(0.628)
Dep. Var. mean	3.27	3.17	4.53	4.19	1.96	2.18
Observations	973	1577	591	921	458	789
R^2	0.100	0.090	0.223	0.214	0.088	0.081
Covariates	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	10%	20%	10%	20%	10%	20%
Kernel	Triangle	Triangle	Triangle	Triangle	Triangle	Triangle
Polynomial degree	1	2	1	2	1	2

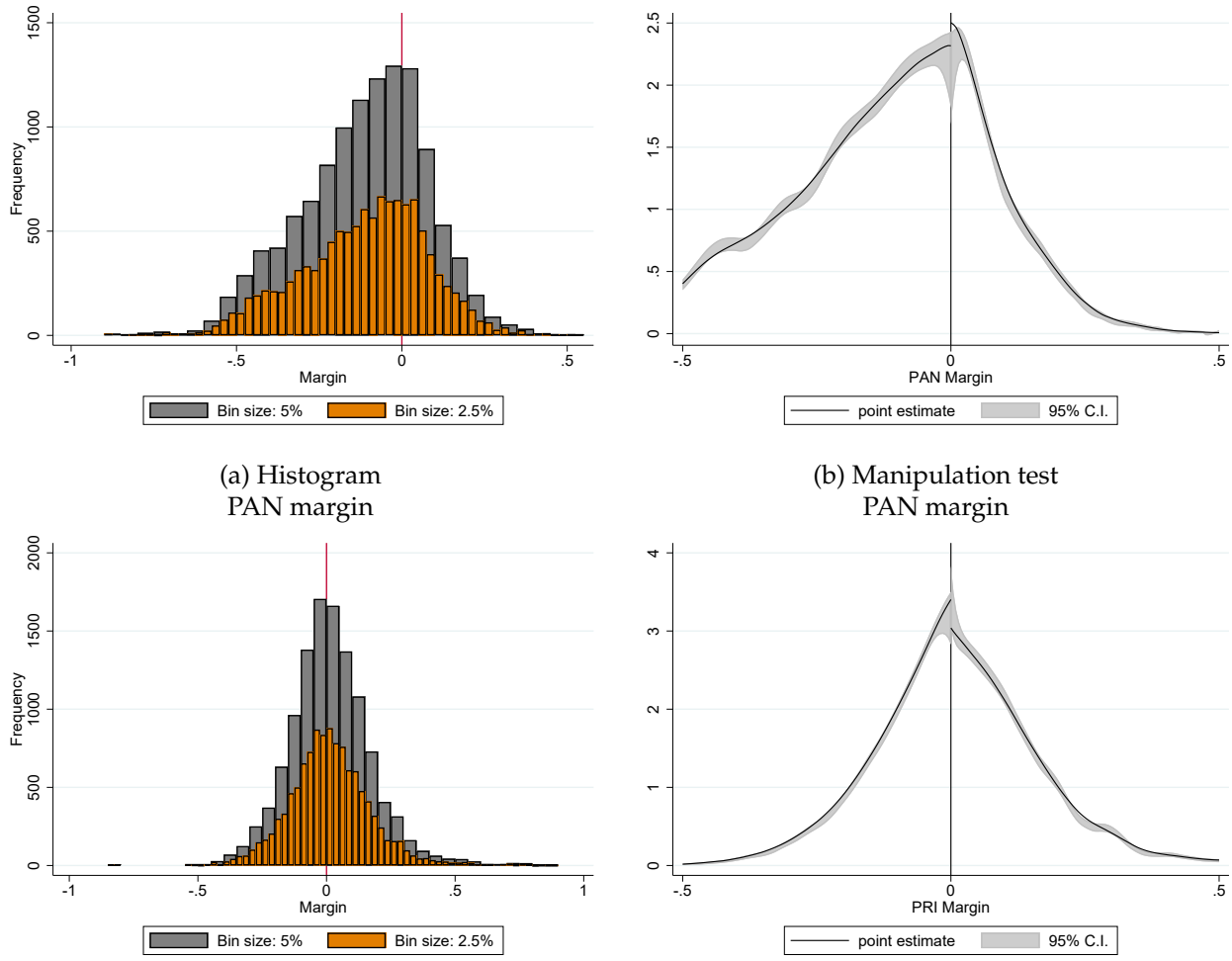
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows discontinuity estimates for characteristics of the loans from the state-owned bank and private banks. Regressions consider local polynomial regression with a 10% bandwidth and polynomials of degrees 1 (columns 1, 3 and 5) and 2 (columns 2, 4 and 6). All regressions include municipality predetermined characteristics and year fixed effects. In addition, all regressions consider a triangle Kernel. Dependent variable means are computed for unaligned municipalities withing the 10 percentage points range. Standard errors shown in parentheses are clustered at the municipality level.

Table 2.B.4: Effects on federal and state transfers

	Earmarked	Unmarked
	(1)	(2)
Aligned	26.92	99.76
	(103.6)	(110.4)
Dep. Var. Mean	1812.4	1924.5
Observations	3796	3796
R^2	0.257	0.528
Covariates	Yes	Yes
Year FEs	Yes	Yes
Bandwidth	10%	10%
Kernel	Triangle	Triangle
Polynomial degree	1	1

Notes: The table shows discontinuity estimates for federal and state transfers. We consider the aggregate values for earmarked and unmarked transfers reported by INEGI. Standard errors shown in parentheses are clustered at the municipality level.

2.C Appendix: Validity of the Research Design



(a) Histogram PAN margin

(b) Manipulation test PAN margin

(c) Histogram PRI margin

(d) Manipulation test PRI margin

Figure 2.C.1: Testing for manipulation of the running variable

Notes: The figure tests for manipulation of municipal elections from 2009 to 2013. In panels (a) and (c) we show the histogram of the PAN and PRI margins of victory for +/-100% points range with 2.5 and 5 percentage points bins, respectively. In panels (b) and (d) we test for manipulation of the running variable (McCrary, 2008) and in both cases we fail to reject the null hypothesis of no discontinuity around the winning threshold (p-val=0.0.77 and p-val=0.32, respectively).

Table 2.C.1: Smoothness of baseline characteristics

	All elections	
	(1)	(2)
Economic characteristics		
log(GDP)	0.0857	-0.0177
	(0.160)	(0.119)
log(GDP cap.)	-0.00156	0.0219
	(0.0438)	(0.0335)
Avg. years of schooling	0.0882	0.0769
	(0.151)	(0.112)
Marginality Index	-0.0356	-0.0410
	(0.0973)	(0.0729)
Human Development Index	0.00261	0.00446
	(0.007)	(0.00514)
Sociodemographic characteristics		
log(Population)	0.0872	-0.0396
	(0.140)	(0.103)
log(Number of houses)	0.0828	-0.0395
	(0.138)	(0.102)
log(Pop. with social security)	0.157	0.0362
	(0.186)	(0.139)
Observations	1560	2490
Bandwidth	10%	20%
Kernel	Triangle	Triangle
Polynomial degree	1	1

Notes: The table shows discontinuity estimates for economic and demographic characteristics of the municipalities measured in 2005. The regressions consider local linear regressions and triangular Kernels. Column (1) reports the results when considering a 10% bandwidth and column (2) reports the results when considering a 20% bandwidth. Municipality characteristics were obtained from Sistema Nacional de Informacion Municipal from the Mexican Secretary of Government. Standard errors shown in parentheses are clustered at the municipality level.

2.D Appendix: Robustness of the Main Results

2.D.1 Optimal Bandwidths by Outcome

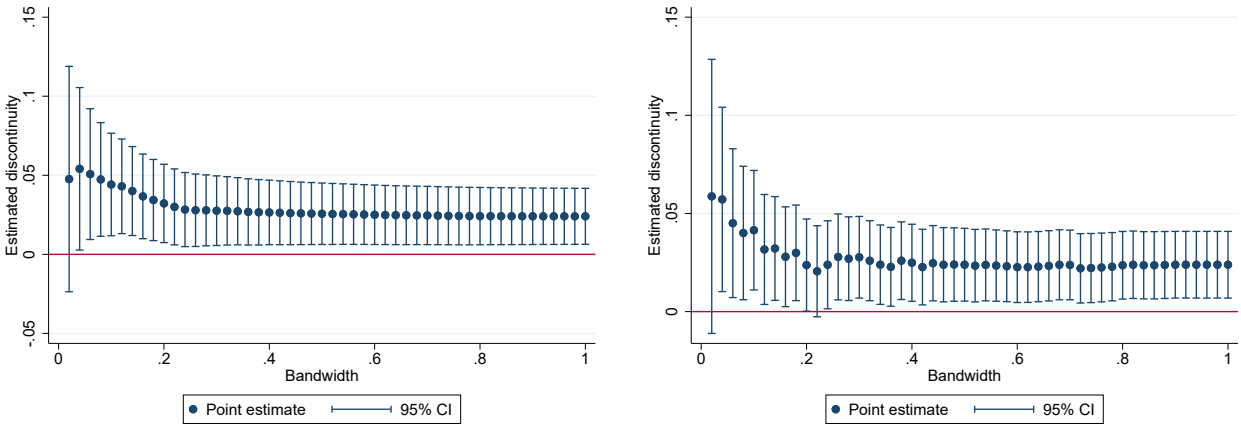
Table 2.D.1: Optimal Bandwidths by Outcome

Outcome variable	Bandwidth
	(1)
Pr(credit=1)	0.134
Pr(state-owned=1)	0.174
Pr(private banks=1)	0.107
Size per cap.	0.129
State-owned size per cap.	0.126
Private banks size per cap.	0.098
Loan price	0.117
State-owned loan price	0.123
Private banks loan price	0.136
Maturity	0.137
State-owned maturity	0.146
Private banks maturity	0.105
Paid	0.129
Paid (state-owned)	0.125
Paid (private banks)	0.099
Wages	0.135
Supplies	0.092
Services	0.153
Subsidies and Transfers	0.125
Assets	0.146
Public Investment	0.160
Debt Services	0.195
Other expenses	0.137
Police Stations	0.194
Jail Cells	0.148
Street lighting	0.103
Collects waste	0.148
Waste disposal vehicles	0.141
Water Sources	0.155
Sewerage	0.188

Notes: The table presents the optimal bandwidth for each outcome variable. Optimal bandwidths were calculated using the optimal bandwidth selection method described in Calonico et al. (2014). The reported values consider a triangular Kernel, a common bandwidth across each side of the threshold and no covariates.

2.D.2 Robustness of the results

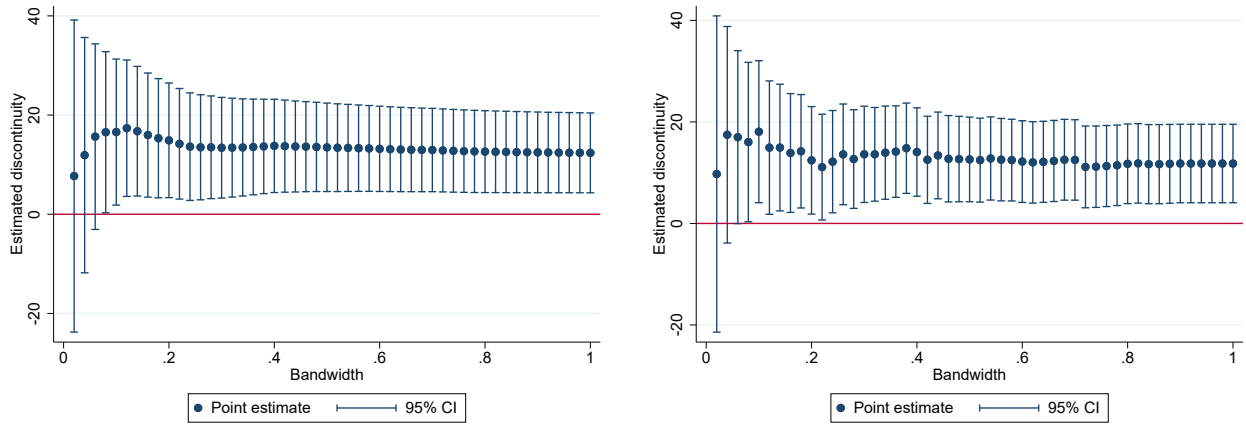
In this section we present robustness of the results to the bandwidth and Kernel selection.



(a) Triangular (b) Rectangular

Figure 2.D.1: Probability of having credit from the state-owned bank

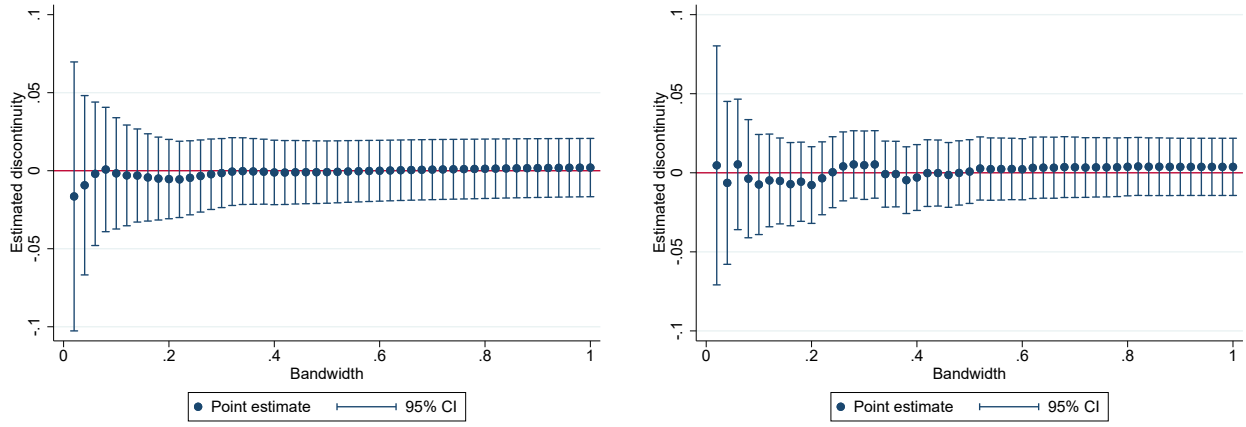
Notes: This figure shows discontinuity estimates with different bandwidths and Kernels for the probability of having new credit from the state-owned bank. Each figure plots the point estimate and its 95% confidence interval from a local polynomial regression of degree 1 against each of the bandwidths considered in the estimation. The bandwidth varies in two percentage points from 2% to 100%. Panel (a) considers a triangular Kernel whereas panel (b) considers a rectangular Kernel.



(a) Triangular (b) Rectangular

Figure 2.D.2: Loan size per capita from the state-owned bank

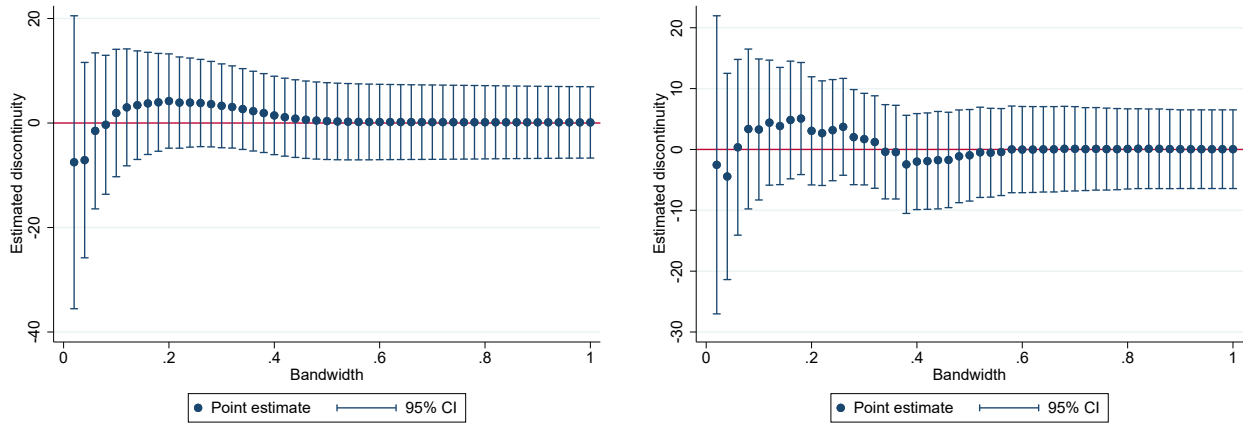
Notes: This figure shows discontinuity estimates with different bandwidths and Kernels for the loan size per capita from the state-owned bank. Each figure plots the point estimate and its 95% confidence interval from a local polynomial regression of degree 1 against each of the bandwidths considered in the estimation. The bandwidth varies in two percentage points from 2% to 100%. Panel (a) considers a triangular Kernel whereas panel (b) considers a rectangular Kernel.



(a) Triangular (b) Rectangular

Figure 2.D.3: Probability of having credit from private banks

Notes: This figure shows discontinuity estimates with different bandwidths and Kernels for the probability of having new credit from private banks. Each figure plots the point estimate and its 95% confidence interval from a local polynomial regression of degree 1 against each of the bandwidths considered in the estimation. The bandwidth varies in two percentage points from 2% to 100%. Panel (a) considers a triangular Kernel whereas panel (b) considers a rectangular Kernel.



(a) Triangular

(b) Rectangular

Figure 2.D.4: Loan size per capita from private banks

Notes: This figure shows discontinuity estimates with different bandwidths and Kernels for the loan size per capita from private banks. Each figure plots the point estimate and its 95% confidence interval from a local polynomial regression of degree 1 against each of the bandwidths considered in the estimation. The bandwidth varies in two percentage points from 2% to 100%. Panel (a) considers a triangular Kernel whereas panel (b) considers a rectangular Kernel.

2.E Appendix: Additional Results

2.E.1 Years in office

To explore the dynamics of the effects by the number of years the mayor has been in office, we interact $\mathbf{1}(margin_{it} > 0)$ in equation 2.1 of the main text with two dummy variables indicating whether the mayor is in his second or third year in office, respectively, allowing for differential slopes across years in office and at either side of the winning threshold. Our specifications consider a polynomial of degree 1, a 10% bandwidth, are weighted using a triangular Kernel and control for municipal characteristics and calendar year fixed effects.

Results are shown in Table 2.E.1. Columns 1 and 3 report the effects for credit from Banobras while columns 2 and 4 report the effects for credit from private banks. While the effects for credit from the state-owned bank are larger during the third year in office, we cannot reject that they are statistically different from those observed during the first year in office. This holds for both the probability of getting credit and for the size of the loans. In contrast, consistent with the results presented above, we find small and statistically insignificant effects for credit from private banks during all years the mayor has been in office. Overall, these results suggest that unlike previous studies have shown (e.g. Cole (2009) and Carvalho (2014)), we do not find robust evidence that supports the idea that in our setting government credit to local governments is used to influence elections.[27]

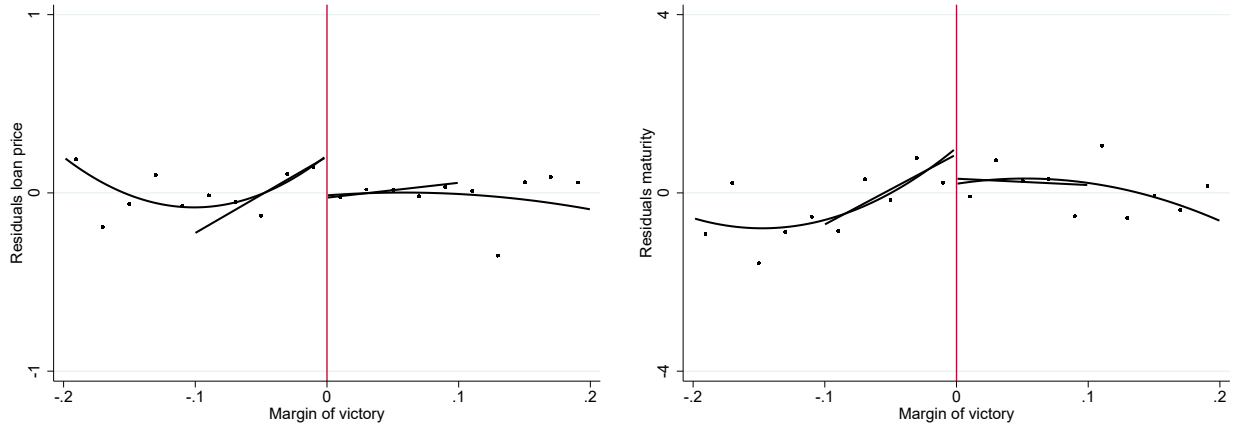
[27] Note, however, that Cole (2009) looks at the allocation of credit to farmers in India, while Carvalho (2014) looks at the allocation of credit to firms in Brazil. Instead, in this paper we look at the allocation of credit to local governments, which has different implications as discussed in this paper.

Table 2.E.1: Effects by years in office

	Pr(credit=1)		Loan size per cap.	
	(1)	(2)	(3)	(4)
	State-owned	Private banks	State-owned	Private banks
Aligned	0.0249	0.00659	9.132	2.286
	(0.0313)	(0.0258)	(13.22)	(8.459)
Aligned x second year	0.0193	-0.00980	6.129	10.14
	(0.0495)	(0.0273)	(22.10)	(8.318)
Aligned x third year	0.0280	-0.0161	11.43	-9.693
	(0.0416)	(0.0348)	(16.11)	(14.20)
Sum of effects				
Aligned +	0.0443	-0.00321	15.26	12.43
Aligned x second year	(0.0349)	(0.0304)	(17.01)	(10.80)
Aligned +	0.0529**	-0.0095	20.56**	-7.407
Aligned x third year	(0.0256)	(0.0229)	(9.840)	(9.472)
Dep. Var. Mean	0.0968	0.0854	31.64	23.48
Observations	5462	5462	5462	5462
R^2	0.0307	0.0786	0.0212	0.0746
Covariates	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Bandwidth	10%	10%	10%	10%
Kernel	Triangle	Triangle	Triangle	Triangle
Polynomial degree	1	1	1	1

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows discontinuity estimates for the probability of having credit and the loan size per capita from the state-owned bank and from private banks by the number of years a mayor has been in office. All regressions include municipality predetermined characteristics and year fixed effects. In addition, all regressions consider a triangle Kernel and a 10% bandwidth. Dependent variable means are computed for municipalities at the left of the threshold within the 10 percentage points range. Standard errors shown in parentheses are clustered at the municipality level.

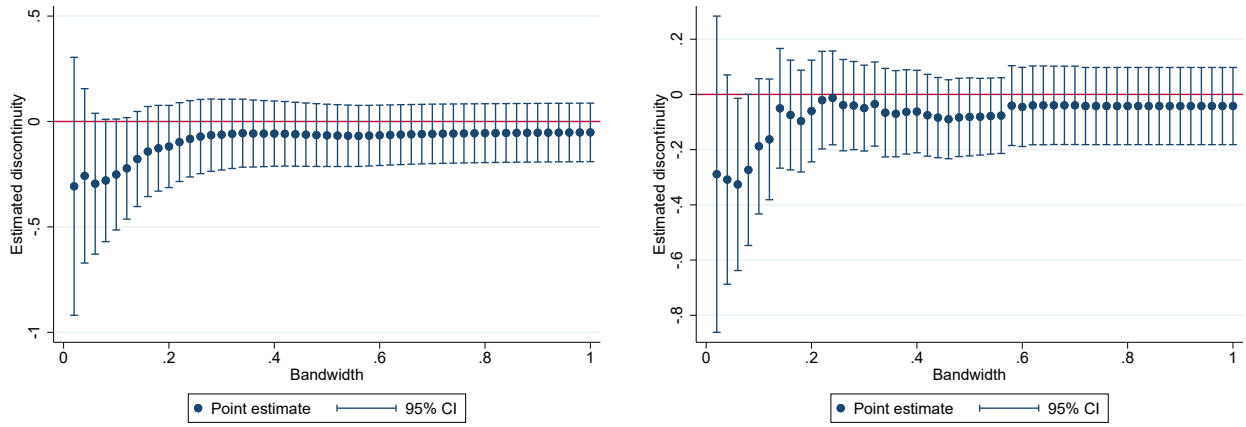
2.E.2 Other results



(a) Loan price (b) Maturity

Figure 2.E.1: Loan contract terms: state-owned bank

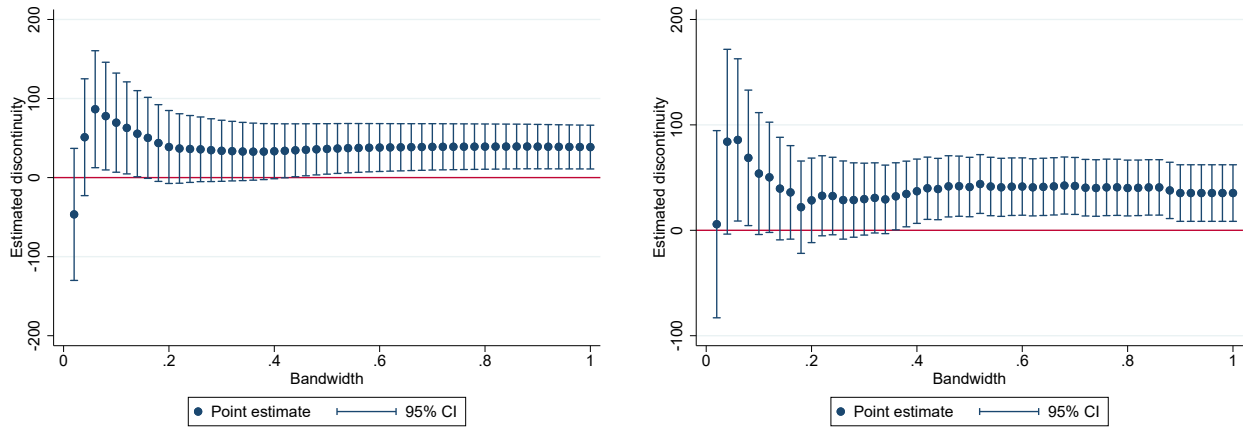
Notes: The figure plots contract terms of new credit from the state-owned bank against the presidential party margin of victory. Negative values of the margin of victory indicate municipalities where the presidential party lost, whereas positive values indicate municipalities where the presidential party won. Each dot corresponds to the average value of the outcome variable within two percentages points bin. The solid lines plot the predicted values of a linear or quadratic regression estimated separately on either side of the winning threshold within the 10% or 20% bandwidth, respectively. Finally, the outcomes are residualized considering predetermined municipality characteristics and calendar year fixed effects.



(a) Triangular (b) Rectangular

Figure 2.E.2: Loan price from the state-owned bank

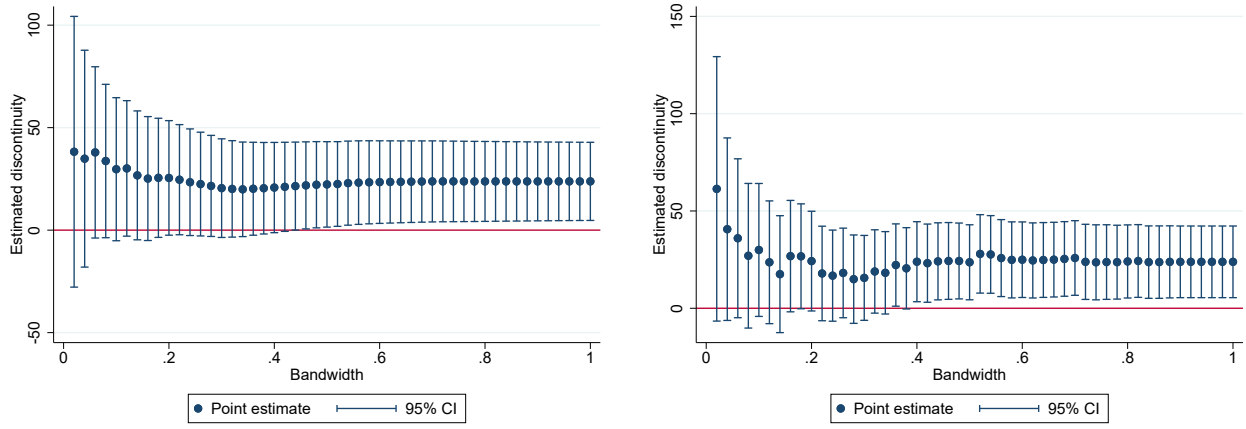
Notes: This figure shows discontinuity estimates with different bandwidths and Kernels for the weighted average loan price from the state-owned bank. Each figure plots the point estimate and its 95% confidence interval from a local polynomial regression of degree 2 against each the bandwidth considered in the estimation. The bandwidth varies in two percentage points from 2% to 100%. Panel (a) considers a triangular Kernel whereas panel (b) considers a rectangular Kernel.



(a) Triangular (b) Rectangular

Figure 2.E.3: Public expenditures in supplies

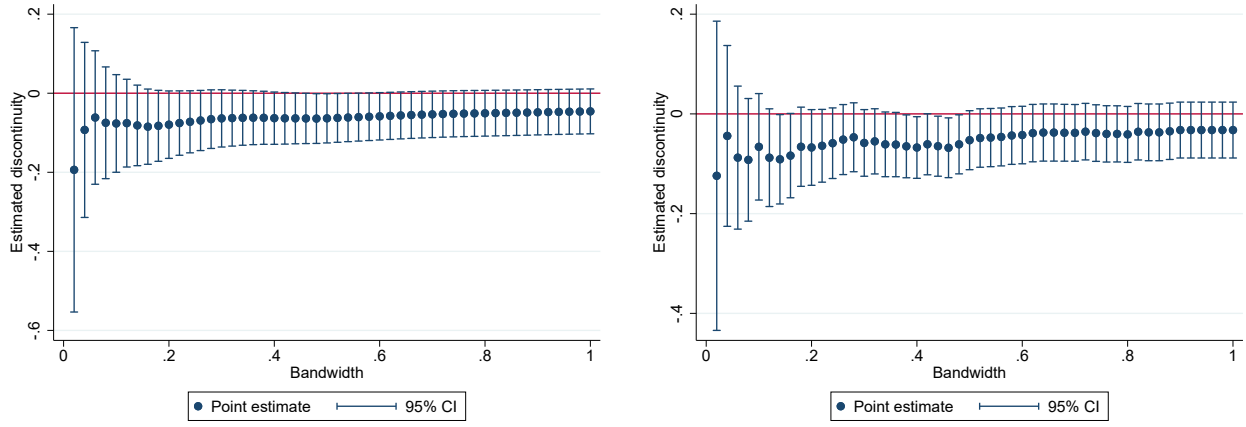
Notes: This figure shows discontinuity estimates with different bandwidths and Kernels for public expenditures in supplies. Each figure plots the point estimate and its 95% confidence interval from a local polynomial regression of degree 1 against each the bandwidth considered in the estimation. The bandwidth varies in two percentage points from 2% to 100%. Panel (a) considers a triangular Kernel whereas panel (b) considers a rectangular Kernel.



(a) Triangular (b) Rectangular

Figure 2.E.4: Public expenditures in debt services

Notes: This figure shows discontinuity estimates with different bandwidths and Kernels for debt services. Each figure plots the point estimate and its 95% confidence interval from a local polynomial regression of degree 1 against each the bandwidth considered in the estimation. The bandwidth varies in two percentage points from 2% to 100%. Panel (a) considers a triangular Kernel whereas panel (b) considers a rectangular Kernel.



(a) Triangular (b) Rectangular

Figure 2.E.5: Public infrastructure index

Notes: This figure shows discontinuity estimates with different bandwidths and Kernels for the public infrastructure index. Each figure plots the point estimate and its 95% confidence interval from a local polynomial regression of degree 1 against each the bandwidth considered in the estimation. The bandwidth varies in two percentage points from 2% to 100%. Panel (a) considers a triangular Kernel whereas panel (b) considers a rectangular Kernel.

Table 2.E.2: The National Development Plan and Public Credit

	P(credit=1)		Loan size per cap.	
	(1) State-owned	(2) Private banks	(3) State-owned	(4) Private banks
Aligned	0.0355**	-0.00174	18.79**	5.495
	(0.0172)	(0.0177)	(7.590)	(5.255)
Aligned x Strategic sector	-0.00991	-0.00824	-9.315	-3.275
	(0.0253)	(0.0266)	(11.74)	(9.528)
Sum of effects				
Aligned +	0.0256	-0.00999	9.477	2.220
Aligned x Strategic sector	(0.0185)	(0.0194)	(8.977)	(7.775)
Dep. Var. Mean	0.0900	0.0900	30.77	25.21
Observations	8656	8656	8656	8656
R^2	0.0691	0.0933	0.0421	0.0746
Covariates	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
Bandwidth	10%	10%	10%	10%
Kernel	Triangle	Triangle	Triangle	Triangle
Polynomial degree	1	1	1	1

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows heterogeneity of the effects of political alignment on credit in places with a high fraction of firm establishments from the strategic sectors defined by each presidential party. For the PAN period, firms in the primary sector, tourism, and housing (SCIAN=11,72,531,532) are considered as strategic sector. For the PRI period, firms in the primary, mining, transport, tourism and patents (SCIAN11,21,48,49,72,533) are considered as strategic sector. We then compute the fraction of establishments in the municipality belonging to these sectors and create a dummy variable indicating if the municipality is above the national median, and zero otherwise. The results report the interaction of this dummy with the RD specification that considers triangular weights, a 20% bandwidth, covariates and differential splines across subgroups at either side of the winning threshold. Standard errors clustered at the municipality level are shown in parentheses.

Interpretation: Political alignment could increase the allocation of credit because mayors are aligned with the ideology preferences of the presidential party. To test this hypothesis, we identified the strategic sectors defined by each presidential party at the beginning of their terms in the National Development Plan. This plan sets the goals and strategies of the presidential party for its six years term. The PAN from Felipe Calderón considered the following sectors from the SCIAN classification as strategic for the development of Mexico: primary sector, tourism and housing. The PRI from Enrique Peña Nieto considered the following sectors from the SCIAN classification as strategic for the development of Mexico: primary sector, mining, transport, tourism and patents.

We identified the firm establishments in these sectors in each municipality using the National Statistical Directory of Economic Units with data based on the 2009 economic census from INEGI.

If political alignment increases the allocation of credit due to an alignment of ideology preferences in regards to the development of Mexico, then we would expect that the state-owned bank allocates more credit to those aligned municipalities with a higher fraction of firms in the sectors considered as strategic for the development of Mexico. We do not find evidence that supports this hypothesis.

Table 2.E.3: Characteristics of municipalities with close elections

Election years	2009-2011	2012-2013
	(1)	(2)
Economic characteristics		
log(GDP)	-0.174**	-0.0513
	(0.0744)	(0.0785)
log(GDP cap.)	-0.00643	-0.0126
	(0.0209)	(0.0222)
Avg. years of schooling	-0.0561	-0.0194
	(0.0692)	(0.0744)
Marginality index	0.0246	0.00846
	(0.0433)	(0.0486)
Human development index	-0.00108	0.000244
	(0.00305)	(0.00335)
Sociodemographic characteristics		
log(Population)	-0.167***	-0.0388
	(0.0646)	(0.0684)
log(Number of houses)	-0.162**	-0.0435
	(0.0638)	(0.0675)
log(Pop. with social security)	-0.142	0.0147
	(0.0888)	(0.0948)

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table tests for systematic differences in characteristics of municipalities with close elections where the presidential party won or lost by a margin of +/-10% against municipalities where the presidential party won or lost by a higher margin. All characteristics are measured in 2005. The table reports coefficients estimated separately by regressing the municipality characteristic on a dummy variable that takes the value of one if the municipality had a close election, and zero otherwise. Column 1 reports estimates for municipal elections in 2009-2011 while column 2 reports estimates for municipal elections in 2012-2013. Robust standard errors are shown in parentheses.

Table 2.E.4: Loan contract terms
Lee bounds for treatment effects

	Total		State-owned		Private banks	
	LB	UB	LB	UB	LB	UB
Panel A. Loan price						
Aligned	-1.2*	-0.05	-0.88**	0.46	-0.99	0.85
	(0.63)	(0.61)	(0.36)	(0.34)	(1.40)	(1.15)
	$[-2.44, 1.14]$		$[-1.59, 1.13]$		$[-3.74, 3.11]$	
Panel B. Duration						
Aligned	-1.74*	1.57	-3.41*	2.45	-0.15	0.57
	(0.73)	(1.2)	(1.4)	(2.34)	(0.36)	(0.57)
	$[-3.16, 3.92]$		$[-6.07, 7.04]$		$[-0.84, 1.7]$	
Bandwidth	1%	1%	1%	1%	1%	1%
Kernel	Triangle	Triangle	Triangle	Triangle	Triangle	Triangle

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The table shows Lee bounds for treatment effects of political alignment on loan contract terms for municipalities always selected into the credit market. LB=lower bound while UB=upper bound. All estimations consider a bandwidth of 0.01 and are weighted with a triangle Kernel. Standard errors in parentheses are computed analytically as in Lee (2009). The confidence range is reported in brackets.

Interpretation: Given that municipalities with credit are a selected sample, for each parameter estimated in section 2.4.4, we provide Lee bounds (Lee, 2009) which under random assignment of treatment and a monotonicity assumption, will inform about the range of the treatment effects supported by the data for municipalities always selected into the credit market.

Formally, in our setting Lee bounds require that close elections occur at random and that political alignment increases the likelihood of selecting into the credit market for all municipalities. We tested the first assumption in Section 2.3, whereas the second assumption is likely to hold given the results found in Section 2.4.1.

2.F Appendix: Loan Payments

Since we can observe the contract terms when the loan was originated (i.e. size, price and the maturity), we can compute the total payments used in Table 2.5 as follows:

1. Compute annuities using the following formula:

$$A = LoanSize \cdot \frac{r(1+r)^N}{(1+r)^N - 1}$$

   where $LoanSize$ is the observed loan size, r is the interest rate, and N is the loan maturity.

2. Since there are N payments of amount A, the total amount paid is $A \times N$.

It is important to highlight that to compute the amount paid, we have assumed that loans have monthly installments and that the interest rate is fixed. In our sample, 95.57% of the loans have monthly installments, while 55.86% have a fixed rate. Nevertheless, during the period of our study, the risk free rate set by the central bank varied few times (4.5 from July 2009 to February 2014, and 4 to 3 from March 2013 to May 2015). The implication of our results, however, varies little if we restrict our sample to loans with monthly installments and fixed rate only.

2.G Appendix: Data

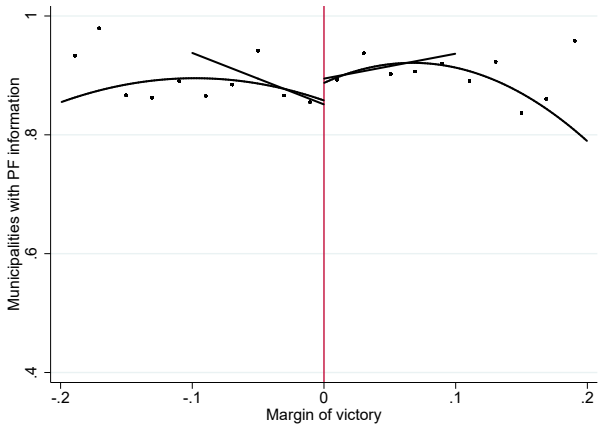


Figure 2.G.1: Municipalities with public finance information
Notes: The figure plots the fraction of municipalities with public finance information against the presidential party margin of victory. Negative values of the margin of victory indicate municipalities where the presidential party lost, whereas positive values indicate municipalities where the presidential party won. Each dot corresponds to the average value of the outcome variable within two percentages points bin. The solid lines plot the predicted values of a linear or quadratic regression estimated separately on either side of the winning threshold within the 10% or 20% bandwidth, respectively.

Interpretation: The figure tests for discontinuities in the fraction of municipalities with reported public finance information around the winning threshold. We code as one if the municipality reported public finance information at least once during the period of our study, and zero otherwise. Our sample excludes municipalities in the state of Oaxaca and in Mexico City. In both specifications, we fail to reject the null hypothesis that the fraction of municipalities reporting public information varies smoothly around the threshold. The estimated discontinuities are 0.044 (se=0.0397) and 0.03 (se=0.0424) when considering the linear and quadratic specifications, respectively.

2.G.1 Credit data

Data on credit outcomes were obtained from Banco de México (Mexico's Central Bank). The data are the credit registry collected on a monthly basis by the National Banking and Securities Commission (CNBV). The data contain all outstanding loans granted by Banobras and private banks to municipal governments. Our sample of loans include all new loans granted by Banobras and private banks to municipal governments during the period of our study. We aggregate the

data at the municipality×mayor×year level, so we consider only the loans granted to a municipality while the elected mayor was in office. The monthly frequency of the data allows us to identify the loans allocated to municipalities during the PAN and the PRI periods, so we can study the effects of alignment on credit by party affiliation as shown in Table 2.3. We winsorize the top 1% loan sizes to deal with outliers and all variables measured in pesos are per capita and in pesos of 2010.

These data are not publicly available. To apply for the data, researchers can submit a research proposal to the the Research Department at Banco de México.

2.G.2 Electoral data

Election results come from the official websites of the electoral authorities of each state. Election dates were obtained from the National Electoral Institute (INE). Mayoral terms were obtained from the National System of Municipal Information (SNIM) "`http://www.snim.rami.gob.mx/`".

2.G.3 Municipality characteristics

Municipality characteristics reported in Table 2.C.1 are all measured in 2005, and were downloaded from the National System of Municipal Information (SNIM) "`http://www.snim.rami.gob.mx/`". The economic characteristics are: GDP, GDP per capita, average years of schooling, marginality index, human development index, and federal transfers. The demographic characteristics are: population, population between 18 and 24 years old, population between 25 and 59 years old, number of houses, population with social security, and population in 2000.

2.G.4 Annual expenses and revenue

The data on annual expenditures and annual revenue were obtained from Municipal Public Finance statistics collected by the Mexican National Institute of Statistics and Geography (INEGI). These data can be obtained from "`https://www.inegi.org.mx/programas/finanzas/`".

2.G.5 Public infrastructure

The data on public infrastructure come from the modules of the municipal government census conducted by INEGI every two years since 2011: i) Public security module, ii) Drinking water and sewerage module, and iii) Solid urban waste module. We use the data referring to the years 2012 and 2014. These data can be downloaded from "`https://www.inegi.org.mx`".

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Chapter 3

Infrastructure for Votes? Experimental and Quasi-Experimental Evidence from Mexico

3.1 Introduction

Do voters reward the provision of infrastructure? As the developing world rapidly urbanizes, better infrastructure is key not only for helping cities realize the agglomeration dividend, but also to remain livable despite huge projected growth Marx et al. (2013); Gollin et al. (2016); Bryan et al. (2020). The provision of high-quality infrastructure has been shown to deliver a large array of benefits, including substantial health improvements Kremer et al. (2011); Galiani et al. (2017), an increase in property values and private investment Gonzalez-Navarro and Quintana-Domeque (2016); McIntosh et al. (2018), and lower market transaction costs Casaburi et al. (2013). Given these benefits and because urban infrastructure is a local public good that requires public provision, it is important to understand the political incentives that exist to shape its supply. The empirical evidence on the electoral returns to infrastructure, however, is surprisingly scant, partly because these investments are rarely allocated exogenously.

In seminal models of political agency and distributive politics, elections structure policy

responsiveness and the allocation of benefits when citizens are able to observe policy outcomes to decide their vote Dixit and Londregan (1996); Fearon (1999); Besley (2006). A large empirical literature across the world supports this assumption about voter behavior for a range of types of government discretionary spending Golden and Min (2013). For the case of local infrastructure provision, however, the existing evidence is either based on observational data or is indirect, with a focus on the political determinants, rather than on the electoral returns, of infrastructure projects. Moreover, experimental evidence suggests that programmatic policies – where incumbents have little to no discretion in delivering benefits due to public and well known distribution rules – induce no electoral reward to incumbents Imai et al. (2020). In the absence of direct political incentives to benefit citizens with programmatic infrastructure investments, incumbents may seek other ways to improve their electoral performance – such as misusing infrastructure funds to facilitate clientelistic and patronage politics. In turn, this misallocation may prevent governments from making the right public investments that lead to faster growth.

In this article, we study the electoral effects of Hábitat, a Mexican urban infrastructure improvement program, in both its experimental evaluation phase and its at-scale endogenous allocation. Mexico is an important place to study urban infrastructure: with 80% of its population living in cities, Latin America is the world's most urbanized region, and its urban challenges today will be those of other rapidly urbanizing regions tomorrow UN Habitat (2012). The Hábitat program provides federal funding to poor urban neighborhoods to fund public works like street paving, piped water, and sewage – local public goods that are the standard currency of retail politics.[1] From 2009–2011, the program was randomized and evaluated at the neighborhood level in 60 municipalities; it increased property values and crowded in private investment McIntosh et al. (2018). After the experimental period, the program was scaled up and allocated at the discretion of a federal agency and the participating municipal governments.

We measure how the program affected electoral outcomes at the level of both the electoral precinct and the municipality, and measure the program's political determinants in the endogenous allocation period. Using the neighborhood-level randomization of the program's evaluation,

[1] An example of the prevalence of this received wisdom is that of Al D'Amato, US Senator known for focusing on constituents' quotidian amenities, who earned the nickname "Senator Pothole." Lurie (1994)

we map the blocks of the study onto electoral precincts and compare electoral outcomes in treated and control areas. To study electoral effects in the non-experimental period, we leverage the staggered scale-up of the program at the municipal level in a difference-in-differences framework. To learn about the determinants of program allocation in the non-experimental period, we use a close-election regression discontinuity which provides exogenous variation in partisan alignment between federal and municipal governments.

We find that the program had different electoral effects depending on the level of granularity of measurement. First, we use the experimental variation from the RCT to study the program's effects on electoral outcomes at the local precinct level, where residents were directly benefitted by the investments.[2] Although the program raised turnout slightly, we estimate a precise zero effect of the infrastructure program on precinct-level vote share, for both federal and municipal incumbent politicians. We rule out reasonably small electoral benefits for incumbents even in the most densely program-saturated precincts, and even in the municipalities where the program represented the largest shock to municipal infrastructure budgets.

We then examine the electoral effect at the higher *municipal* level, during the infrastructure program's post-experimental scale-up, using a difference-in-differences approach. Here we identify large, positive electoral effects of the program for municipal candidates of the nationally-ruling PRI party. These effects are concentrated among PRI incumbents. Our estimates on the electoral effect for incumbents in general is positive but not statistically significant. These results suggest a puzzle: how does the program produce electoral benefits at the municipal level if not by persuading the directly benefitted voters in the precincts where it is implemented?

Our regression discontinuity results shed further light on this apparent puzzle. During the program's post-experimental scale-up, the PRI was the party in power at the federal level, and hence was responsible for allocation of Hábitat. We find that municipalities where PRI mayoral candidates barely won were 3 to 6.5 percentage points more likely to receive Hábitat funding (a large increase from a base of 8.9%). How does the party convert this funding into the documented electoral municipal victories? The experimental evidence suggests it is not through simply im-

[2]The unit of analysis in Mexico is the *sección electoral*. We translate this into English term "precinct" because this is the word often used for comparable units in English-speaking countries.

proving infrastructure at the most granular level. We consider the possibility that Hábitat provides cover for federal funds to be used for municipal campaigns, e.g. through over-invoicing or diversion of funds. The regression discontinuity results show that not only does partisan alignment increased Hábitat allocation; it also correlates with a higher amount of misused funds recovered by an independent federal audit agency from those municipalities receiving the program.

These results are consistent with PRI-aligned municipal governments misusing federal Hábitat funds for campaigning or engaging in clientelistic politics, and thus coveting the program despite its negligible direct electoral returns. The overall pattern that emerges is reminiscent of the electoral politics of pork-barrel spending in other settings (e.g., Stein and Bickers, 1994). In Brazil, for example, despite a weak relationship between pork spending and electoral success, incumbent legislators still trade this type of particularistic spending for campaign contributions, which flexibly allows them to secure votes for reelection or to pursue another office (Samuels, 2002).

Despite the suggestive evidence of an infrastructure-for-money mechanism, however, we note that alternative explanations exist. Politicians might, for example, be better than at identifying blocks where infrastructure investment is likely to deliver votes than a random allocation. If so, that could explain why the estimates from the precinct-level analysis reveal a null effect but the municipality-level results indicate positive and large electoral returns. We are currently gathering data to test whether electoral rewards exist at the precinct level in the post-experimental, endogenous allocation period.

These results help to fill a gap in the available evidence on the electoral returns to infrastructure. Existing work is based on observational data (e.g., Magaloni, 2006; Briggs, 2012; Harding, 2015; Cruz and Schneider, 2016; Voigtlaender and Voight, 2019), or presents only indirect evidence that focuses on the political causes of infrastructure provision (e.g., Burgess et al., 2005; Castells and Solé-Ollé, 2005; Blimpo et al., 2013; Boas et al., 2014; Min, 2015; Lehne et al., 2018; Harris and Posner, 2019; Thomas Bohlken, 2021). This includes a large literature on political buget cycles (e.g., Khemani, 2004; Drazen and Eslava, 2010; Pierskalla and Sacks, 2018; Bostashvili and Ujhelyi, 2019). Here, in addition to presenting experimental evidence of the electoral impact of urban

infrastructure, we also measure these returns at various levels of granularity to present a more complete political dynamic at play.

This paper also contributes to our understanding of electoral accountability in contexts of widespread corruption. A key assumption in models of political agency is that voters are able to observe policy outcomes and update their beliefs about politicians. The existing evidence on the role of voter information, however, is mixed. In some settings, disclosed information on politician performance leads to a voter response at the ballot box (Ferraz and Finan, 2008; Chong et al., 2015; Banerjee et al., 2020); by contrast, in many others there is no evidence that disclosed performance affects survey-reported turnout or vote choice (Dunning et al., 2019). Unlike other policies, public works are a highly visible form of local spending that are easily attributable, and Hábitat specifically has been shown to substantially increase access to basic infrastructure McIntosh et al. (2018). Using the original evaluation survey, we show that the random allocation of the program indeed increases the knowlege of Hábitat by almost 8 percentage points. As such, the estimated null electoral returns are not driven by a poor understanding of the local infrastructure projects, nor by incorrect attribution: we measure no impact of infrastructure on either local or federal incumbent party vote shares. Instead, these results suggest that the information revealed by Hábitat does not change voters' prior beliefs about the incumbent party. This may not be surprising if voters recognize that the evaluation of the program was implemented programmatically – leaving local politicians with little discretion over implementation – or if, alternatively, they expect the program to be plundered for funds – which is consistent with our evidence for Hábitat's post-experimental scale-up.

Our results also cast the interpretation of past studies on the determinants of infrastructure provision in a different light. While some studies assume that politicians deliver public works that benefit citizens to secure those citizens' votes, our experimental evidence opens the possibility that these projects provide different political benefits to politicians, such as funds that can be repurposed to influence voters who were not directly benefited by infrastructure investments. Similarly, our findings suggests that it is possible that the electoral returns that have been identified at even moderately high levels of aggregation in past studies may be explained by an infrastructure-

for-money mechanism, rather than by direct voter responsiveness. Thus, this research sounds a note of caution: Hábitat might look like an example of effective democratic accountability if one focused only on the municipal level. By combining this analysis with the granular precinct-level experimental results and the RD results on determinants of program allocation, we show that even programmatic policies have the potential to be turned to clientelistic ends in some political contexts. Politicians seeking electoral advantage are not necessarily limited to hoping voters straightforwardly reward them for a program that works as intended.

The rest of this article proceeds as follows: Section 3.2 gives further detail on the context and the Hábitat program. Section 3.3 describes the data. Section 3.4 lays out the empirical design and results from our precinct-level analysis of the Hábitat RCT. Section 3.5 outlines our identification strategy and results for the municipality-level difference-in-differences results in the post-experimental scale-up period. Section 3.6 describes our close-election discontinuity and results on political determinants of Hábitat allocation in the post-experimental scale-up period. Section 3.7 wraps up and synthesizes our various results.

3.2 Background

Hábitat was created in 2003 by Mexico's Social Development Secretariat (SEDESOL). The program, which was subsequently transferred to another agency, the Urban, Territorial, and Agrarian Development Secretariat (SEDATU) in 2013, lasted until 2018. It provided federal funding to municipalities for urban infrastructure improvements like street paving, illumination, electrification, piped water, and sewage in low-income neighborhoods. Individual infrastructure projects were decided in close consultation with the community, and funding was split mostly between federal, state, and municipal governments. During the program's evaluation, half of the funding came from the federal government, while local governments provided the other half. The program was characterized for having centralized targeting and spending rules with decentralized implementation.

For neighborhoods to be eligible for the program, they had to consist of inhabited households in marginalized urban areas – settlements with 15,000 people or more – with high levels of

poverty, poor urban infrastructure and services, and no active conflict over land tenure. The experimental evaluation of Hábitat was implemented from 2009–2011, and consisted of randomizing neighborhoods into the program, from a sample of 60 participating municipalities.[3] It was found to increase property values and private property investment McIntosh et al. (2018). In the years following the experimental evaluation, the program was scaled up and allocated endogenously by federal authorities to 610 municipalities across all Mexican states (see Figure 3.1).

Figure 3.2 depicts the timeline of events considered in our analysis. We begin by using the experimental variation from 2009–2011 to study the effects on the 2012–2013 municipal elections.[4] Then, we use the endogenous scale up of the program from 2013–2017 to study the effects on the 2014–2019 municipal elections.

3.3 Data

The data used in this paper come from several government agencies. In this section, we describe the sources, the samples of interest, and the main features of the data.

Electoral data. Electoral data for municipal elections is under the jurisdiction of the state electoral authorities. We obtained election results from the 2009–2017 elections at the municipality level from the electoral authorities of each state. At the precinct level, we have obtained the municipal election data from the six states with the most Hábitat polygons: Baja California, Mexico City, Guanajuato, Jalisco, Mexico, and Puebla. These states together account for 57.6% of all Hábitat polygons in the experiment.

Our analysis of the Hábitat RCT's precinct-level electoral effects focuses on municipal elections immediately after the 2009–2011 Hábitat experiment – 2012 and 2013. Municipal election years are staggered across states. This means some municipal elections are held in the same years as federal elections, while others are not. When states hold their own elections in the same year as federal elections (every three years), they typically use the federal electoral geographies for the

[3]The program defined neighborhoods as "polygons," a geographical unit corresponding only to the Hábitat program and no other government administrative function. The median polygon consisted of 29 blocks.

[4]The municipal electoral cycles in Mexico are staggered across states. For the analysis that exploits on the experimental variation, we focus on the municipal elections inmediatelly after the experimental evaluation period.

state and municipal elections as well. Otherwise, they may or may not use the federal election geography from the preceding federal election – and ascertaining which geography was used is difficult. Four of the states for which we have municipal election data held their elections in 2012, alongside the federal elections that year (Mexico, Jalisco, Mexico City, and Guanajuato). The other two held their municipal elections the following year, in 2013 (Baja California and Puebla).

Table 3.1 lists the states which contained Hábitat polygons during the experimental period. We also note which states had elections alongside the federal elections in 2012 and which we have collected the data for. Below, we present analyses which include all states for which we have data as well as analyses limited to states with 2012 municipal elections, where we are more confident in the electoral geography.

Our difference-in-differences analysis of the endogenous scale-up of the program, from 2013–2017, is at the municipal-election year level, and uses municipality-level electoral results from this period as outcome variables. Finally, the regression discontinuity estimates use the margin of victory of the presidential party, which we obtained from the municipality-level electoral results.

Hábitat data. We obtained data on the allocation of Hábitat during the 2013–2017 period from SEDATU. We requested the data through the National Transparency Platform (PNT). These data contain information about the universe of Hábitat polygons in each year, including the number of treated blocks, number of houses, and size of the population living in each polygon. The data also contain the goereferenced polygons, which we use to match the precinct-level electoral data. On average, each year around 9% of the municipalities receive the program, and 3% of the municipal population and blocks benefit from it (see Table 3.2).

Corruption data. We use audit reports from Mexico's Federal Auditors Office (ASF) from 2013 to 2017 to construct an objective measure of corruption. Each year, the ASF selects a group of municipalities to audit the use of the Municipal Fund for Social Infrastructure (FISM)–which is part of the federal earmarked transfers to municipalities–during the previous calendar year. The ASF selects municipalities based on their size, the financial importance of the FISM funds relative to the municipality's budget, whether the municipality has been audited before, and whether

other neighboring municipalities are being audited in that year, among other things. On a typical year during the studied period, 12% of the municipalities become audited (Table 3.2).

The audits verify that the funds were used for its earmarked purposes, that the municipality conducts appropriate accounting of the funds, whether the infrastructure exists, whether the resources were transferred to other accounts, and whether the municipality reports to the federal government how the resources were spent, among other criteria defined by the ASF. Each audit reports the amount of resources that were found to be misspent, if any, and that have to be reimbursed to the federal treasury. We use this amount as a fraction of the audited resources as our measure of corruption. On average, during our studied period 14% of the FISM funds were found to be misspent (see Table 3.2).

3.4 RCT: precinct-level electoral returns

3.4.1 Empirical strategy

In this section, we leverage the polygon-level randomization of the RCT evaluation of the Hábitat urban infrastructure program to identify electoral impacts at the precinct level. The experiment followed a two-stage randomized saturation design. In the first stage, the saturation of treatment was randomized across 60 participating municipalities. In the second stage, assignment of treatment was randomized across eligible polygons.

As described in Section 3.2, the allocation of Hábitat is defined and administered at the level of polygons, which are smaller than municipalities and consist of a small number of blocks.[5] In the experimental evaluation of the program, the median polygon contained 24.5 blocks. The electoral outcomes, however, are available at the electoral precinct level. Precincts also consist of a small number of blocks, but do not correspond one-to-one with the Hábitat polygons. The median precinct in our sample consists of 34 blocks.

We define treatment at the precinct level as the fraction of the Habitat RCT blocks within the precinct which were part of a Hábitat treatment polygon. In practice, due in part to the low density of Hábitat-eligible polygons within municipalities, over 95% of precincts contain only

[5] A block is a well-defined geographic administrative unit and corresponds to a city block with a street on each side.

treatment or control blocks.[6] Figure 3.3 illustrates our geographic randomization, mapping treatment and control precincts and polygons in the municipality of Toluca, State of Mexico, the country's most populous state and that which contains the most Hábitat polygons in the experiment. Our analysis also includes the states of Puebla, Jalisco, Guanajuato, and Baja California, as well as Mexico City, but we omit these maps in the interest of space.

Equation 3.1 expresses our baseline regression specification:

$$Y_{im} = \beta \cdot Treatment_{im} + \alpha X_{im} + \gamma_m + \varepsilon_{im} \tag{3.1}$$

where Y_{im} represents electoral outcomes in precinct i in municipality m in the election immediately following the 2009–2011 Hábitat experiment (2012 or 2013 municipal elections, depending on the electoral cycle, and 2012 federal elections). The outcomes we focus on are incumbent vote share at the municipal and federal level, municipal margin of victory, and municipal turnout. $Treatment_{im}$ measures the fraction of precinct i's study blocks which are treated. γ_m are municipality fixed effects. X_{im} is a vector of pre-determined precinct-level controls, such as the log of population, average years of schooling, and log of people without access to health services. Following the original randomized design, we allow the error term ε_{im} to be arbitrarily correlated within municipalities. Under this regression model, the parameter of interest β identifies the effect of having all blocks treated in the precinct.

3.4.2 Main results

Table 3.3 shows the average effect of the Hábitat treatment on four relevant electoral outcomes: the vote share of the incumbent mayor (columns 1 and 2), the winning mayoral candidate's margin of victory (columns 3 and 4), turnout in the municipal elections (columns 5 and 6), and the precinct-level vote share for the incumbent presidential party (columns 7 and 8). For each outcome, we estimate results with and without including controls. Because the original RCT randomized treatment assignment within municipalities, we include municipality fixed effects in

[6] We also report results that consider a dummy variable for whether the precinct had at least one treated block.

all regressions.

We identify a precise null effect of receiving the program on the vote share of both municipal and federal incumbents (columns 1–2 and 7–8, respectively). In the case of vote share for the municipal incumbent, we can rule out effect sizes as small as 3% off the mean with 95% confidence. For federal incumbents, we can rule out effect sizes of 5% off the mean with 95% confidence. The program increased turnout by about 1 percentage point (columns 5–6), albeit the estimate is marginally statistically significant when we include controls. This result could indicate that public works can increase citizen engagement, even if not by straightforwardly raising incumbent vote share. The program also seems to have made elections more competitive as measured by reduced margins of victory (though these estimates are less precisely estimated).

This null result on incumbent vote share is surprising. McIntosh et al. (2018) showed that the policy produced many of its desired effects – beneficiaries seem to have noticed the program, insofar as their property values went up and they invested more in their property. Moreover, the program was jointly funded by federal and local governments, creating many actors who could plausibly have claimed credit.

We conduct a number of robustness exercises to the main results. First, in Table 3.A.1 in the Appendix 3.A we limit the analysis to states whose elections are in the same years as the federal elections, to address the possibility that the election geographies may vary across election years. The results are broadly qualitatively similar. Second, in Table 3.A.2 we restrict our estimates to a sample of precincts that had 25% or more of its blocks in the Hábitat experiment (either treatment or control), to account for the possibility that the program might have been too thinly dispersed for voters to notice. The conclusions from our findings remain practically unchanged. Third, in Table 3.A.3 we code treatment as one if there is at least one treated block in the precinct and zero otherwise. The results remain practically unchanged. Fourth, we leverage the randomized saturation design of the experiment to test for the presence of spillover effects. The results of this exercise are presented in Table 3.A.4. The evidence overall indicates that there are not spillover effects of receiving the program on the main outcomes studied here, although some of the estimates are less precisely estimated.

Finally, we examine whether treatment had a differential effect in municipalities where the Hábitat program represented a relatively large fraction of investments, measured by the fraction of total municipal infrastructure spending and by the total population benefiting from the program. Based on these measures, we identify those municipalities above the median and conduct an heterogeneity analysis. The results of this analysis are shown in Table 3.4. We find that even in places with high levels of infrastructure investments, the program does not lead to stronger electoral effects.

3.4.3 Voter information and political opinions

Next, we turn to analyze whether the null effects can be explained by lack of information about the program or by changes in voters' perceptions about politics. To explore this, we leverage the baseline and endline survey rounds from the Hábitat experiment, which contain information about whether voters' knew about the program and several questions regarding their trust in political institutions. The results of this analysis are reported in Table 3.5. In column 1, we report the estimate for a dummy variable taking the value of one if citizens knew about the program and zero otherwise. The estimate shows that citizens living in treated polygons were 7.6 percentage points more likely to respond that they knew about the program, relative to citizens living in control polygons. The point estimate represents a relative increase in knowledge about the program of 42%, and is highly statistically significant. This result indicates that the null effects reported above are not driven by a poor understanding of the local infrastructure projects.

Columns 2–6 of Table 3.5 report the estimates for different questions regarding citizens' perceptions of political institutions, such as whether they know of any local neighborhood leader (column 2), if a local leader helps to solve problems (column 3), if they report having high trust for local leaders (column 4), for public officials (column 5), and for political parties (column 6). The estimates are small and statistically significant, although in some cases they are less precisely estimated. We interpret these findings as evidence that the information revealed by Hábitat did not change voters' prior beliefs about the incumbent party.

Overall, our precinct-level results strongly indicate that despite increasing property val-

ues and crowding in private investment, the Hábitat infrastructure program had approximately no effect on the electoral fortunes of any of the politicians who could have claimed credit for it. This null result together with the results on voters' opinions about politics, challenge the electoral accountability framework. To interrogate it further, we turn to the scale up of the program and explore whether these effects remain when politicians are allowed to allocate the program endogenously.

3.5 DiD: municipality-level electoral returns

After the Hábitat program's experimental phase in 2009–2011, it was scaled up from the period 2013-2017. In this scale-up period, it was no longer subject to random assignment, but was allocated by federal authorities in coordination with municipal governments. During this period, the PRI was in power at the federal level. This gives us a rare opportunity to separately measure the program's electoral effects when randomized and when allocated endogenously.

Without randomization in the scale-up period, we rely on quasi-experimental difference-in-differences estimates to measure the program's electoral effect. Due to data limitations, we can currently identify the effect of Hábitat in this period only at the municipal level, not at the more granular precinct level we used for the RCT analysis. Comparing precinct-level electoral effects under random and endogenous allocation would be unique in the literature, and we are currently working to obtain the data necessary to make this comparison. For now, we focus on the electoral effects of the program at a more aggregate level in the 2013–2017 time window.

3.5.1 Empirical strategy

We compare electoral outcomes in municipalities before and after receiving Hábitat funding, including municipality and election year fixed effects. Equations 3.2 and 3.3 express our difference-in-differences specifications:

$$Y_{mt} = \beta_1 Habitat_{mt} + X_{mt}\Gamma + \gamma_m + \mu_t + \varepsilon_{mt} \tag{3.2}$$

$$Y_{mt} = \beta_1 Habitat_{mt} + \beta_2 PRI\_incumbent_{mt} + \beta_3 Habitat_{mt} \times PRI\_incumbent_{mt} + X_{mt}\Gamma + \gamma_m + \mu_t + \varepsilon_{mt} \tag{3.3}$$

where Y_{mt} represent electoral outcomes in municipality m in election year t. The outcomes we focus on are municipal incumbent vote share and municipal PRI candidate vote share. $Habitat_{mt}$ is a dummy for whether municipality m had received Hábitat by year t. $PRI\_incumbent_{mt}$ is a dummy for whether municipality m's incumbent was a member of the PRI in year t. X_m is a vector of municipality-level controls which we include in some specifications (2010 municipality population, as well as its interaction with year dummies). ε_{mt} is a municipality-year-specific error term which is allowed to be arbitrarily correlated within municipalities. γ_m and μ_t are municipality and election year fixed effects, respectively. Election year fixed effects capture variation from time shocks common to all municipalities, and municipality fixed effects capture time-invariant municipality-level variation, such that we are identified off variation in Hábitat allocation over time *within* municipality. Under the assumption that municipality-level Habitat receipt is not temporally correlated with other important unobservables, β_1 and β_3 allow us to identify the average effect of Hábitat and the effect of Hábitat for municipalities with incumbents from the PRI, respectively.

3.5.2 Results

Table 3.6 shows the estimates of equations 3.2 and 3.3. We identify positive coefficients of Habitat on the likelihood that municipal incumbents win in general (columns 1 and 2), though these are not statistically significant. However, in the last four columns, the table shows large and reasonably significant effects on the likelihood of a win by the PRI candidate – especially when she is an incumbent. We note that the inclusion of controls diminishes the magnitude of the coefficient but not the estimate of its standard error, decreasing its statistical significance (columns 3 and 4). Overall, we interpret these estimates to mean that Hábitat receipt helped PRI candidates win municipal elections, especially when they were incumbents (columns 5 and 6).

At first glance, this result seems to be in some tension with the results from the RCT analysis. Why does Hábitat produce electoral returns for the PRI at the municipal level, but not for

incumbents at the precinct level? We consider a number of potential explanations. It is possible that even though the average precinct is unaffected by the infrastructure program, politicians are able to identify the neighborhoods most likely to exhibit treatment effects and make sure the program goes there. We plan to test this hypothesis using neighborhood-level Hábitat allocation data in the *post-experimental* endogenous allocation period, contingent upon data access.

In the meantime, we consider the alternative hypothesis that electoral benefits can come through a channel other than the persuasion of voters who experience the program's intended effects. The party in charge of allocating the program may find other ways to help co-partisans using program funding. This is the subject of our next section.

3.6 RD: political determinants of allocation and corruption

In this section, we look not at the effects but the causes of Hábitat, using a close election regression discontinuity design to test whether partisan alignment with the federal government increased program receipt at the municipality level. We focus on the years 2013–2017, for which we have the post-experimental Hábitat allocation data.

Studying the political determinants of Hábitat allocation complements our study of its electoral effects, providing a kind of test of the party's revealed preference. If politicians at the federal level do not believe the program creates electoral benefits, we might expect to see no special effort to steer its allocation toward their municipal co-partisans. By contrast, evidence that partisanship affects program allocation would suggest (though not prove) that party leaders believe the program creates electoral advantage.

The results from Section 3.4 imply that any electoral advantage of the program does not come from persuading voters who experience the infrastructure improvements in their own neighborhoods. One potential channel through which politicians can gain advantage is through corruption. For instance, PRI-aligned municipal mayors may be able to siphon off some of the federal funds intended for infrastructure into campaign expenditures (e.g. through no-bid contracts or other procurement irregularities). We measure this using the amount of municipal funding marked as misappropriated by Mexico's independent federal audit agency ASF, as explained in

Section 3.3.

3.6.1 Empirical strategy

Our baseline specification estimates the following regression model:

$$Y_{mt} = \alpha_0 + \alpha_1 Aligned_{mt} + \mathbb{1}(margin_{mt} > 0) \times h(margin_{mt}) + f(margin_{mt}) + X_{mt}\Gamma + u_{mt} \quad (3.4)$$

where Y_{mt} is an outcome of interest for municipality m during year t; $margin_{mt}$ is the margin of victory of the PRI party in municipality m during the elections inmediatelly preceding year t; $Aligned_{mt}$ takes the value of one if the margin of victory of the PRI party is greater than zero, and zero otherwise; $h(\cdot)$ and $f(\cdot)$ are unknown polynomial functions; X_{mt} is a vector of municipality characteristics, and u_{mt} is an error term that is allowed to be arbitrarily correlated within the municipality. We estimate equation 3.4 using local linear regression, a triangular Kernel, and a bandwidth of 10%. Appendix 3.B presents the results of robustness exercises that consider a different degree of the polynomial of $h(\cdot)$ and $f(\cdot)$ and different bandwidths. We also report the estimates that consider the optimal bandwidth selection criteria proposed by Calonico et al. (2014).

3.6.2 Hábitat allocation

Figure 3.4 shows the RD plots of Hábitat receipt variables around the discontinuity. We restrict the window to a margin of victory for the PRI candidate of +/- 10 percentage points. The point estimates of equation 3.4 are reported in Table 3.7 considering linear and quadratic polynomial fits in each side of the discontinuity. We find that places where a candidate of the PRI barely won are 3 to 6.5 percentage points more likely to receive at least one Hábitat polygon, relative to municipalities where a candidate of the PRI barely lost (columns 1 and 2). Columns 3–6 also show that partisan alignment affects the intensive margin as well, both in terms of the total population affected by the program and in terms of the total blocks receiving the program.

Appendix 3.B checks the robustness of the estimates of equation 3.4, including plots which substitute linear for quadratic polynomial fits on either side of the threshold, and plots of estimated coefficients for various choices of bandwidth including the optimal bandwidth according

to Calonico et al. (2014).

3.6.3 Corruption

This subsection uses the same empirical method to look at our measure of corruption based on the audit reports by the ASF. While there are many channels and sources of municipal funding in Mexico, following De La O and García (2015) and Larreguy et al. (2020), we contend that the ASF audits provide one reasonable measure of corruption at the municipal level. Columns 1 and 2 of Table 3.8 show the discontinuity estimates considering linear and quadratic fits, respectively. The point estimates are positive, although the standard errors are large.

To further investigate our hypothesis, columns 3–4 and columns 5–6 report the estimates by splitting the sample by whether the municipality received the program or not, respectively. While these selection of the sample is endogenous, it informs whether partisan alignment correlates with corruption when a municipality received the program. Interestingly, we find a strong correlation between partisan alignment and corruption when a municipality receives the program while we don't find such correlation when a municipality does not receive the program.

These last results provide some suggestive evidence of our hypothesis with some caveats. Specifically, evidence that partisan alignment correlates with corruption when municipalities receive the program does not categorically prove that federal Hábitat funds are being siphoned off to support the campaigns of municipal PRI candidates. However, it is consistent with that interpretation. This coincident evidence that partisan alignment drives both Hábitat allocation and correlates with municipal corruption when municipalities receive the program merits further examination. We are currently in the process of obtaining and preparing data from other sources that can shed further light on the potential misuse of Hábitat transfers.

3.7 Conclusion

The benefits to urban infrastructure are well-documented, and the developing world needs much more of it to deal with the challenges of increasing urbanization in the coming decades. Because public infrastructure is typically provided by politicians, understanding the political drivers

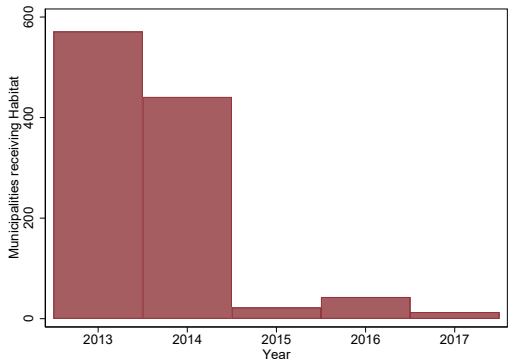
of its supply is important, especially in contexts with high corruption. In this paper, we show that a large urban infrastructure program produced no discernible electoral rewards when it was randomized at the neighborhood level. After it was scaled up, it does seem to have helped local incumbents from the nationally ruling party to get elected at the municipal level – although regression discontinuity evidence suggests this may have been due to the party diverting infrastructure funding into campaign uses.

Our paper highlights the need for further inquiry not only into which policies can aid growth, but who has incentives to champion those policies. It may be the case that politicians will compete over policy only after (enforced) laws make it too costly to compete over patronage.

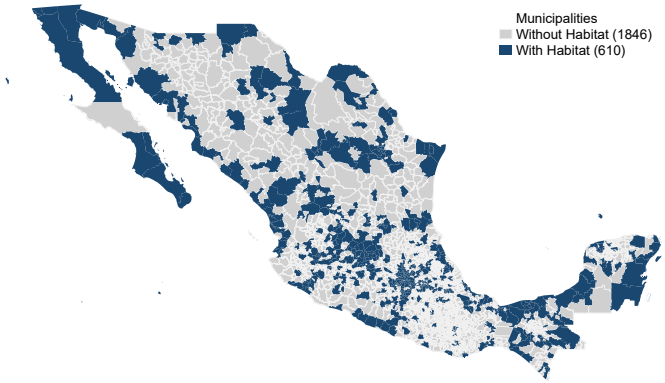
3.8 Acknowledgments

This chapter, in part, is currently being prepared for submission for publication of the material. Garfias, Francisco; Lopez-Videla, Bruno; Sandholtz, Wayne Aaron. "Infrastructure for votes? Experimental and Quasi-Experimental Evidence from Mexico." The dissertation author was a primary investigator and author of this material.

Figures



(a) Municipalities with new Hábitat polygons



(b) Geographic variation in Hábitat allocation

Figure 3.1: 2013–2017 Hábitat allocation

Notes: Panel (a) plots the number of municipalities receiving new Hábitat polygons per year from 2013 to 2017. Panel (b) plots the geographic variation in the allocation of Hábitat across municipalities in the 2013–2017 time window.

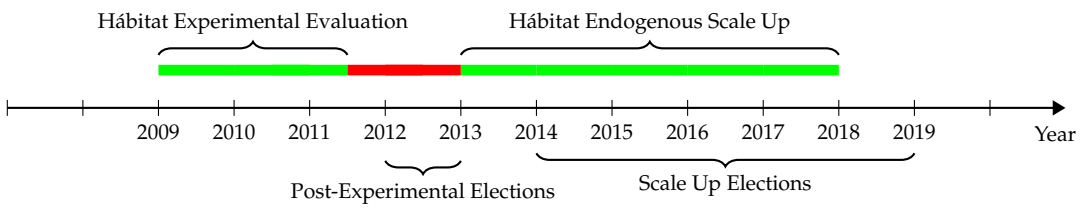


Figure 3.2: Timeline of Events

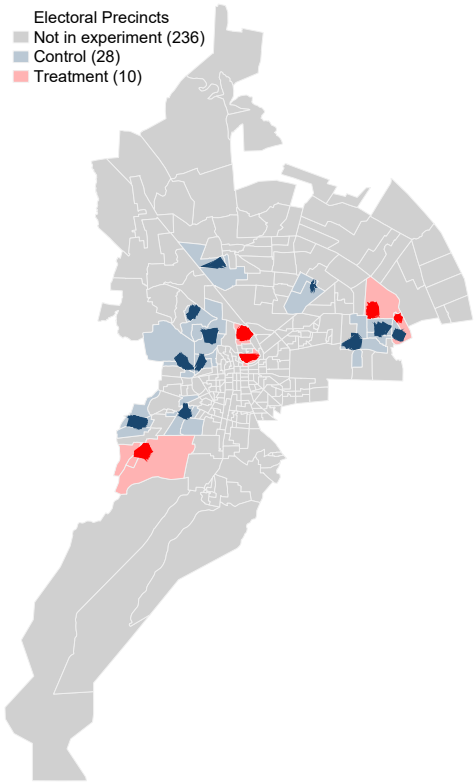
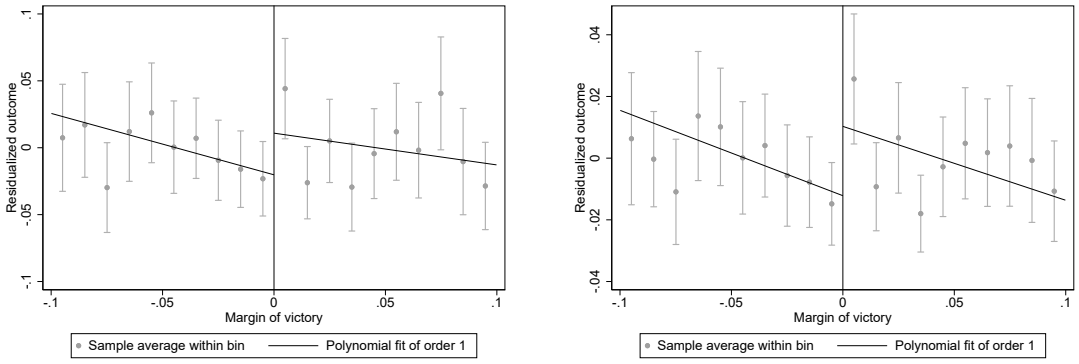


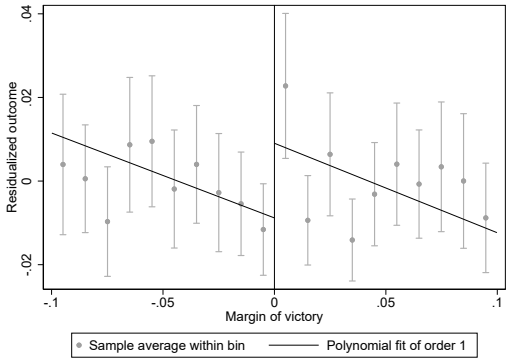
Figure 3.3: Treatment and control polygons and precincts

Notes: Map showing location of treatment and control Hábitat polygons and electoral precincts in the municipality of Toluca, State of Mexico. Precincts containing any Hábitat-treated block are labeled "treatment" in this map. Hábitat polygons (shown in darker red and blue) are superimposed to the electoral precincts in the analysis.



(a) Pr(Habitat=1)

(b) Habitat population



(c) Habitat blocks

Figure 3.4: RDD Figures: Linear fit

Notes: The running variable is the PRI margin of victory. Habitat population is normalized by the urban population in the municipality in 2010. Habitat blocks is normalized by the number of urban blocks in the municipality in 2010. The outcome variables are residualized after controlling for the gender of the mayor, municipality population, number of blocks in the municipality, and an index from CONAPO that measures the degree of marginalization of the municipality.

Tables

Table 3.1: Number of experiment polygons, by state

State	Election in 2012	In analysis	Polygons	Study pop.
	(1)	(2)	(3)	(4)
Mexico	✓	✓	74	193630
Puebla		✓	39	64309
Jalisco	✓	✓	24	44137
Distrito Federal	✓	✓	20	51842
Guanajuato	✓	✓	20	40693
Baja California		✓	20	40481
Tamaulipas			20	22393
Michoacan	✓		19	27451
Guerrero	✓		16	17005
Veracruz			14	19850
Quintana Roo			13	25787
Sinaloa			13	15936
Chihuahua			11	21592
Sonora	✓		7	9872
Morelos	✓		7	15484
Nuevo Leon	✓		7	11000
Coahuila			6	6865
Yucatan	✓		5	6332
Campeche	✓		4	7827
Chiapas	✓		3	5383

Table 3.2: Summary statistics

	Mean	Std. Dev.	N
	(1)	(2)	(3)
Experimental evaluation			
Municipal incumbent share	0.44	0.10	598
Margin of victory	0.12	0.17	598
Turnout	0.52	0.12	598
Federal incumbetn share	0.22	0.10	598
Post-experimental rollout			
Hábitat	0.09	0.28	12280
Population with Hábitat	0.03	0.13	12280
Blocks with Hábitat	0.03	0.11	12280
Corruption data			
Audit	0.12	0.32	12280
Misspent resources	0.14	0.24	1426

Notes: The table shows summary statistics of the the main outcomes. Experimental evaluation corresponds to the sample of post-experimental elections. The unit of observation for this sample is the electoral precinct. Post-experimental rollout and corruption data consider the years 2013–2017. The unit of observation is the municipality-year.

Table 3.3: Effect of infrastructure improvement on electoral outcomes

	Municipal incumbent share		Margin of victory		Turnout		Federal incumbent share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-0.008	-0.009	-0.024	-0.024	0.009	0.012*	0.004	0.005
	(0.009)	(0.008)	(0.017)	(0.015)	(0.006)	(0.007)	(0.005)	(0.005)
Mean (control)	0.441	0.441	0.132	0.132	0.531	0.531	0.210	0.210
Observations	598	598	598	598	598	598	598	598
Adj. R^2	0.467	0.491	0.413	0.442	0.839	0.861	0.782	0.793
Controls		✓		✓		✓		✓

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The table shows estimates of the effects of infrastructure investment on electoral outcomes at the municipality and federal level. Municipal incumbent share and margin of victory correspond to the electoral outcomes for the incumbent party at the municipality level. Turnout and federal incumbent share correspond to the 2012 presidential elections. All regressions include municipality fixed effects. Controls: log of population, Avg. years of schooling, log of people without access to health services.. States included in the estimations: Baja California, Distrito Federal, Guanajuato, Jalisco, Mexico, Puebla. Standard errors clustered at the municipality level are shown in parentheses.

Table 3.4: Heterogeneity analysis by intensity of Hábitat investments

	Municipal incumbent share		Margin of victory		Turnout		Federal incumbent share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-0.016	-0.009	-0.036**	-0.022	0.010	0.012	0.009	0.011
	(0.010)	(0.010)	(0.015)	(0.017)	(0.009)	(0.009)	(0.007)	(0.008)
High investment × Treatment	0.012		0.016		-0.004		-0.011	
	(0.019)		(0.035)		(0.012)		(0.011)	
High population × Treatment		0.001		-0.003		-0.007		-0.014
		(0.018)		(0.034)		(0.012)		(0.010)
Mean (control)	0.442	0.441	0.133	0.132	0.533	0.531	0.209	0.210
Observations	585	598	585	598	585	598	585	598
Adj. R^2	0.470	0.466	0.417	0.412	0.838	0.839	0.782	0.783
Controls	N	N	N	N	N	N	N	N

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The table shows heterogeneity analysis on the main outcomes of interest by the intensity of Habitat Investments. *High investment* describes municipalities for which Habitat investment as a fraction of infrastructure spending is above the median (.074). *High population* describes municipalities for which the population treated with Habitat is above the median (18909). All regressions include municipality fixed effects. States included in the estimations: Baja California, Distrito Federal, Guanajuato, Jalisco, Mexico, Puebla. Standard errors clustered at the municipality level are shown in parentheses.

Table 3.5: Effects on knowledge about Hábitat and political opinions

	Knowledge of Hábitat	Any Local Leader	Local Leader is Useful	Trust in Local Leader	Trust in Public Officials	Trust in Political Parties
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.0761***	0.00939	-0.0371	0.00652	-0.000823	0.0136
	(0.0269)	(0.0327)	(0.141)	(0.0139)	(0.0136)	(0.0127)
Round 2	-0.0990***	0.0171	-0.00399	-0.0246**	-0.0226**	-0.0220*
	(0.0290)	(0.0320)	(0.115)	(0.0116)	(0.00939)	(0.0124)
Mean (control)	0.181	0.264	0.516	0.0565	0.0521	0.0333
Observations	684	684	557	684	684	684
R^2	0.136	0.00734	0.00286	0.0481	0.0375	0.0435

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The table shows estimates of the effects of receiving the program on dummy variables that take the value of one if the respondents knew about Hábitat (column 1); if they know of any local neigborhood leader (column 2); if a local leader helps to solve problems (column 3); if they report high trust for the local leader (column 4), for public officials (column 5), and for political parties (column 6). The analysis exploits the baseline and endline surveys from McIntosh et al. (2018) and uses population weights to be representative of the population living in the polygons. Standard errors clustered at the municipality level are shown in parentheses.

Table 3.6: Effects of the program on electoral outcomes at the municipality level

	Incumbent win		PRI win			
	(1)	(2)	(3)	(4)	(5)	(6)
PRI Incumbent					-0.49***	-0.49***
					(0.033)	(0.033)
Habitat	0.078	0.036	0.17**	0.12	0.078	0.019
	(0.095)	(0.099)	(0.075)	(0.079)	(0.058)	(0.062)
Habitat × PRI Incumbent					0.15**	0.12*
					(0.058)	(0.060)
Habitat + Habitat × PRI Incumbent					0.22***	0.14*
					(0.07)	(0.08)
Controls		✓		✓		✓
Within-District Mean of DV	0.41	0.41	0.35	0.35	0.35	0.35
Within-District SD of DV	0.33	0.33	0.28	0.28	0.28	0.28
R^2	0.60	0.61	0.68	0.68	0.76	0.77
Observations	3452	3446	3452	3446	3451	3446
Number of Municipios	2035	2033	2035	2033	2035	2033

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The table shows difference-in-difference estimates of the effects of receiving the program on electoral outcomes at the municipality level. The studied sample considers outcomes observed during the 2014–2017 municipal elections. All regressions include municipality and election-year FE. Controls include log of 2010 municipality population, alone and interacted with election year dummies, as well as the margin of victory in the last election. Standard errors clustered at municipality level are shown in parentheses.

Table 3.7: Effects of political alignment on the allocation of Hábitat

	Pr(Habitat=1)		Habitat Population		Habitat Blocks	
	(1)	(2)	(3)	(4)	(5)	(6)
RD estimate	0.0320*	0.0653**	0.0224**	0.0441***	0.0178**	0.0359***
	(0.0177)	(0.0262)	(0.00991)	(0.0148)	(0.00800)	(0.0120)
Dep. Var. Mean	0.0888	0.0888	0.0334	0.0334	0.0263	0.0263
Observations	10094	10094	10094	10094	10094	10094
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Bandwidth	10%	10%	10%	10%	10%	10%
Polynomial	1	2	1	2	1	2

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The table shows estimates of the effects of having a mayor from the PRI on different measures of the allocation of the Hábitat program. All estimations control for municipality population, number of blocks in the municipality, and an index from CONAPO that measures the degree of marginalization of the municipality. Standard errors clustered at the municipality level are shown in parentheses.

Table 3.8: Effects of political alignment on corruption

	Full sample		Without Hábitat		With Hábitat	
	(1)	(2)	(3)	(4)	(5)	(6)
RD estimate	0.0192	0.0369	-0.0213	-0.0185	0.138***	0.188***
	(0.0404)	(0.0596)	(0.0473)	(0.0716)	(0.0520)	(0.0702)
Dep. Var. Mean	0.151	0.151	0.151	0.151	0.151	0.151
Observations	1411	1411	1005	1005	406	406
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular
Bandwidth	10%	10%	10%	10%	10%	10%
Polynomial	1	2	1	2	1	2

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The table shows estimates of the effects of having a mayor from the PRI on the misspent resources as a fraction of the audited funds. All estimations control for municipality population, number of blocks in the municipality, and an index from CONAPO that measures the degree of marginalization of the municipality. Standard errors clustered at the municipality level are shown in parentheses.

3.A Appendix: Additional Tables

Table 3.A.1: Effect of infrastructure improvement on electoral outcomes – State elections on the federal elections schedule

	Municipal incumbent share		Margin of victory		Turnout		Federal incumbent share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-0.014	-0.014	-0.034	-0.033*	0.012*	0.015*	0.006	0.007
	(0.011)	(0.010)	(0.020)	(0.018)	(0.007)	(0.009)	(0.006)	(0.005)
Mean (control)	0.437	0.437	0.164	0.164	0.568	0.568	0.188	0.188
Observations	440	440	440	440	440	440	440	440
Adj. R^2	0.454	0.516	0.403	0.466	0.820	0.872	0.818	0.858
Controls		✓		✓		✓		✓

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The table shows estimates of the effects of infrastructure investment on electoral outcomes at the municipality and federal level. Municipal incumbent share and margin of victory correspond to the electoral outcomes for the incumbent party at the municipality level. Turnout and federal incumbent share correspond to the 2012 presidential elections. All regressions include municipality fixed effects. Controls: log of population, Avg. years of schooling, log of people without access to health services.. States included in the estimations: Distrito Federal, Guanajuato, Jalisco, Mexico. Standard errors clustered at the municipality level are shown in parentheses.

Table 3.A.2: Effect of infrastructure improvement on electoral outcomes, limited to electoral sections with a high fraction of blocks in the study ($> 25\%$)

	Municipal incumbent share		Margin of victory		Turnout		Federal incumbent share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	-0.004	-0.006	-0.020	-0.020	0.008	0.009	-0.004	-0.004
	(0.013)	(0.014)	(0.027)	(0.027)	(0.007)	(0.008)	(0.007)	(0.008)
Mean (control)	0.441	0.441	0.136	0.136	0.514	0.514	0.210	0.210
Observations	296	296	296	296	296	296	296	296
Adj. R^2	0.540	0.541	0.510	0.511	0.869	0.875	0.783	0.785
Controls		✓		✓		✓		✓

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The table shows estimates of the effects of infrastructure investment on electoral outcomes at the municipality and federal level. Municipal incumbent share and margin of victory correspond to the electoral outcomes for the incumbent party at the municipality level. Turnout and federal incumbent share correspond to the 2012 presidential elections. All regressions include municipality fixed effects. Controls: log of population, Avg. years of schooling, log of people without access to health services.. States included in the estimations: Baja California, Distrito Federal, Guanajuato, Jalisco, Mexico, Puebla. Standard errors clustered at the municipality level are shown in parentheses.

Table 3.A.3: Effect of infrastructure improvement on electoral outcome

	Municipal incumbent share		Margin of victory		Turnout		Federal incumbent share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ITT	-0.005	-0.005	-0.020	-0.019	0.004	0.008	0.005	0.005
	(0.009)	(0.008)	(0.017)	(0.015)	(0.006)	(0.007)	(0.005)	(0.005)
Mean (control)	0.441	0.441	0.132	0.132	0.531	0.531	0.210	0.210
Observations	598	598	598	598	598	598	598	598
Adj. R^2	0.466	0.490	0.412	0.441	0.838	0.860	0.782	0.793
Controls		✓		✓		✓		✓

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The table shows estimates of the effects of infrastructure investment on electoral outcomes at the municipality and federal level. Municipal incumbent share and margin of victory correspond to the electoral outcomes for the incumbent party at the municipality level. Turnout and federal incumbent share correspond to the 2012 presidential elections. The treatment variable takes the value of one if at least one block in the electoral precinct has been treated and zero otherwise. All regressions include municipality fixed effects. Controls: log of population, Avg. years of schooling, log of people without access to health services.. States included in the estimations: Baja California, Distrito Federal, Guanajuato, Jalisco, Mexico, Puebla. Standard errors clustered at the municipality level are shown in parentheses.

Table 3.A.4: Treatment and spillover effects of infrastructure investment on electoral outcomes

	Municipal incumbent share		Margin of victory		Turnout		Federal incumbent share	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment x Saturation	0.008	-0.021	-0.020	-0.038	-0.065	-0.053	0.043	0.009
	(0.045)	(0.041)	(0.074)	(0.074)	(0.106)	(0.101)	(0.069)	(0.060)
Control x Saturation	-0.016	-0.037	0.011	-0.005	-0.038	-0.030	0.003	-0.011
	(0.049)	(0.045)	(0.081)	(0.081)	(0.107)	(0.103)	(0.066)	(0.063)
Mean (control)	0.441	0.441	0.132	0.132	0.531	0.531	0.210	0.210
Observations	598	598	598	598	598	598	598	598
Adj. R^2	0.000	0.038	-0.001	0.002	0.020	0.042	0.020	0.133
Controls		✓		✓		✓		✓

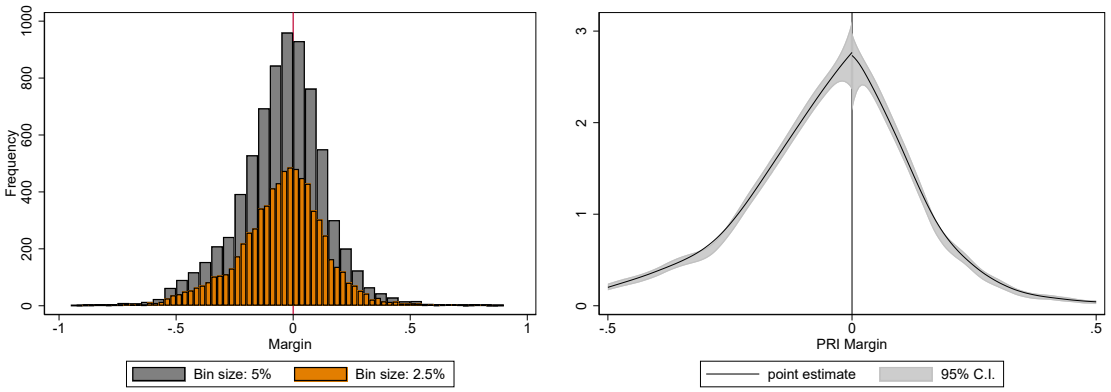
Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The table shows estimates of the treatment and spillover effects of infrastructure investment on electoral outcomes at the municipality and federal level. Municipal incumbent share and margin of victory correspond to the electoral outcomes for the incumbent party at the municipality level. Turnout and federal incumbent share correspond to the 2012 presidential elections. The treatment variable takes the value of one if at least one block in the electoral precinct has been treated and zero otherwise. Controls: log of population, Avg. years of schooling, log of people without access to health services.. States included in the estimations: Baja California, Distrito Federal, Guanajuato, Jalisco, Mexico, Puebla. Standard errors clustered at the municipality level are shown in parentheses.

3.B Appendix: Empirical Strategy

3.B.1 Validity of the regression discontinuity design

The validity of our regression discontinuity design rests on two important assumptions. First, the running variable should not be manipulated at the winning threshold. One way to test this assumption is by looking at discontinuities in the density of the running variable at the winning threshold. Figure 3.B.1 displays a histogram and a density plot of the margin of victory of the presidential party. As it is shown, there are no discontinuities in the density of the running variable at the winning threshold, providing some evidence that our first assumption required for identification holds.

The second assumption for identification requires that observed and unobserved predetermined characteristics of the municipalities vary smoothly at the winning threshold. While the smoothness of unobserved characteristics is fundamentally untestable, Figure 3.B.2 shows evidence of this assumption for observed characteristics of the municipalities. Overall, the evidence presented in this subsection shows that the assumptions required for the validity of the close elections regression discontinuity design hold. This is in line with previous empirical work that have



(a) Histogram (b) Density

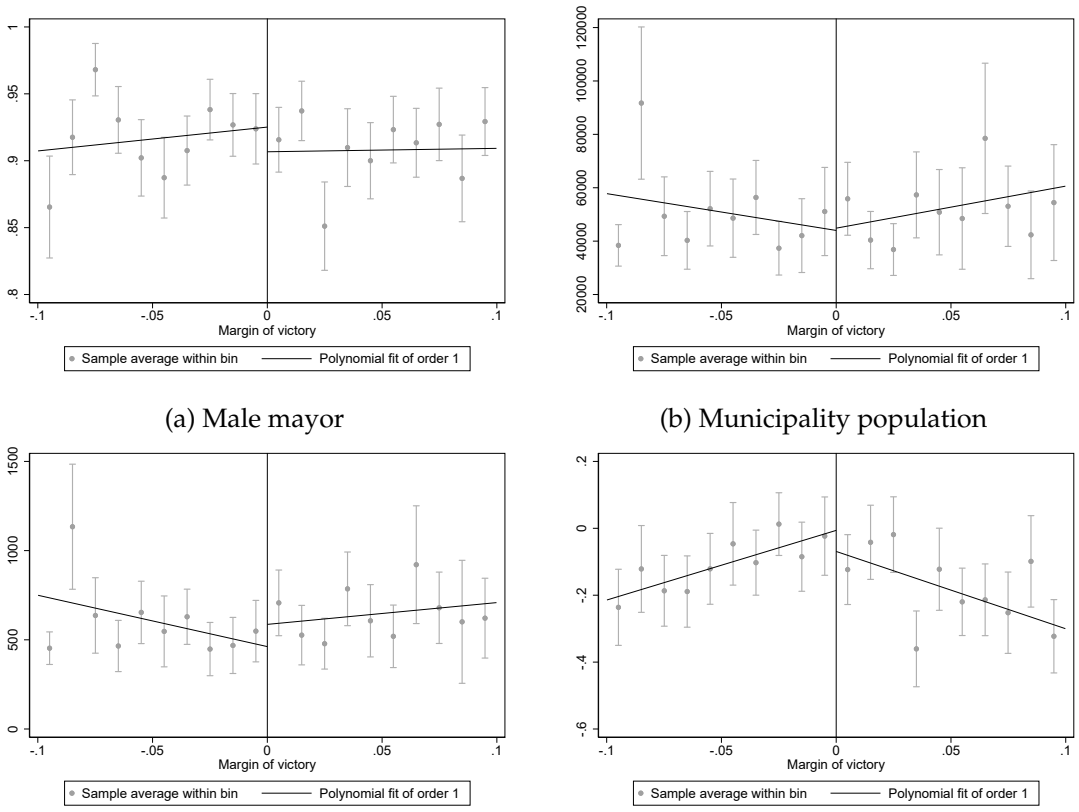
Figure 3.B.1: Testing for manipulation of the running variable

Notes: The figure tests for manipulation of the margin of victory of the presidential party. Panel (a) plots the histogram of the running variable with bin sizes of 2.5% and 5%. Panel (b) plots the non-parametric density of the running variable.

used the similar empirical design in this setting (e.g., Dell, 2015).

3.B.2 Political determinants of Hábitat allocation

This section considers alternative regression discontinuity specifications for the analyses in Section 3.6. We show plots that fit quadratic polynomials around the discontinuity, and we show plots for our estimated treatment effects under various bandwidth choices for both the linear fit and the quadratic fit.



(a) Male mayor

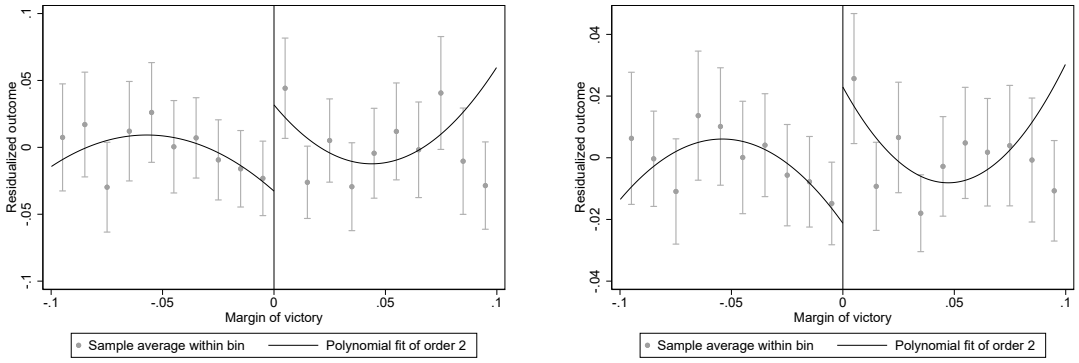
(b) Municipality population

(c) Number of blocks

(d) Marginality index

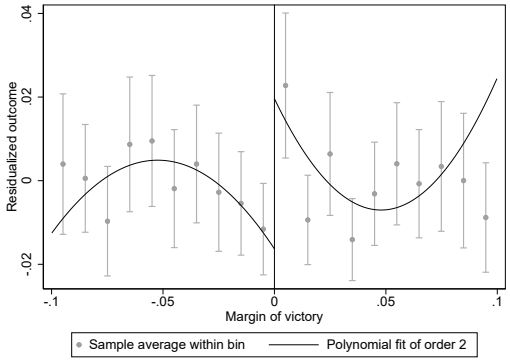
Figure 3.B.2: Smoothness of covariates

Notes: The figure plots RD figures for different observable characteristics of the municipality. Panel (a) considers a dummy variable that takes the value of one if the mayor is male and zero otherwise. Panel (b) considers the municipality population obtained from the 2010 population census and the 2015 population count. Panel (c) considers the number of blocks in the municipality obtained from the population census. Finally, panel (d) considers the marginality index constructed by CONAPO.



(a) Pr(Habitat=1)

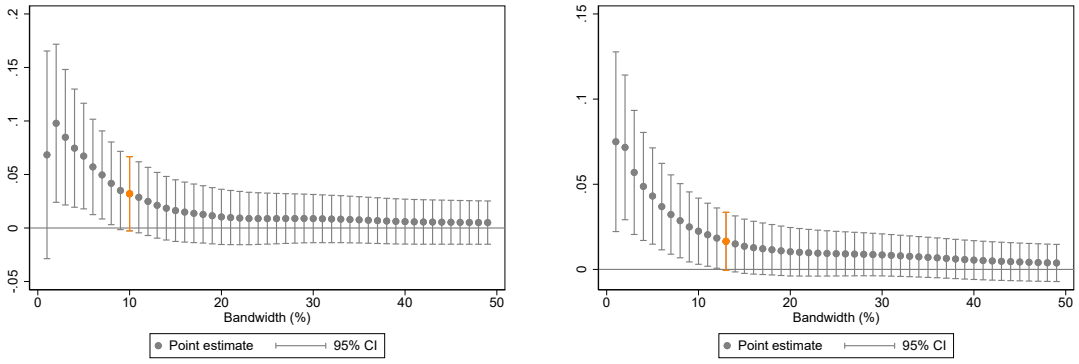
(b) Habitat population



(c) Habitat blocks

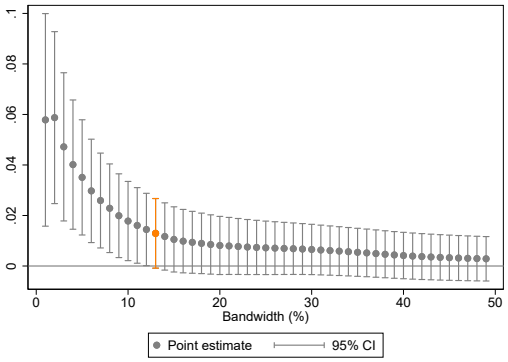
Figure 3.B.3: RDD Figures: Quadratic fit

Notes: The running variable is the PRI margin of victory. Habitat population is normalized by the urban population in the municipality in 2010. Habitat blocks is normalized by the number of urban blocks in the municipality in 2010. The outcome variables are residualized after controlling for the gender of the mayor and the pre-determined urban population in the municipality.



(a) Pr(Habitat=1)

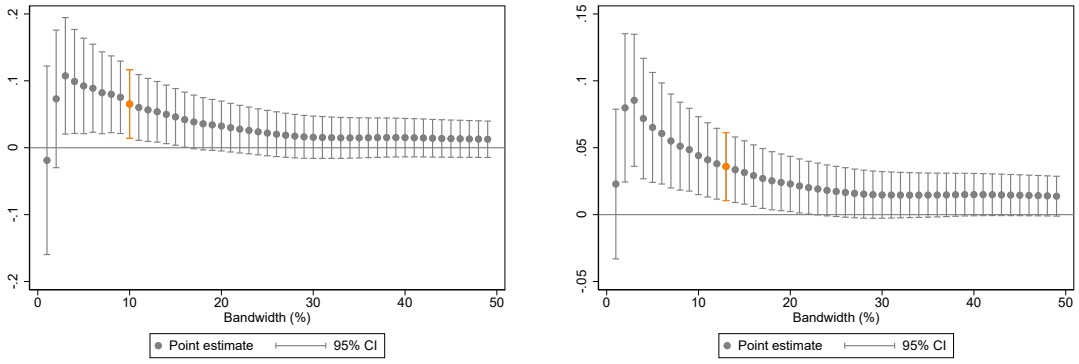
(b) Habitat population



(c) Habitat blocks

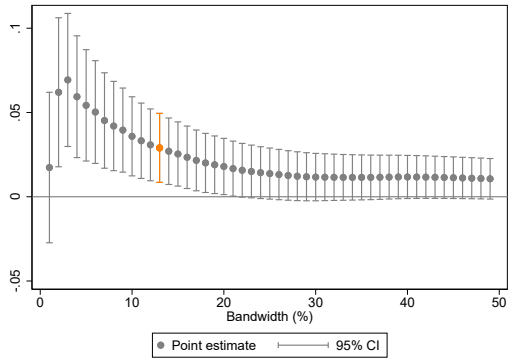
Figure 3.B.4: Robustness to the bandwidth selection: Linear fit

Notes: The figure plots RDD estimates using different bandwidths. The point estimate using the optimal bandwidth selection criteria proposed by Cattaneo et al. is in orange. The running variable is the PRI margin of victory. Habitat population is normalized by the urban population in the municipality in 2010. Habitat blocks is normalized by the number of urban blocks in the municipality in 2010. The outcome variables are residualized after controlling for the gender of the mayor and the pre-determined urban population in the municipality.



(a) Pr(Habitat=1)

(b) Habitat population



(c) Habitat blocks

Figure 3.B.5: Robustness to the bandwidth selection: Quadratic fit

Notes: The figure plots RDD estimates using different bandwidths. The point estimate using the optimal bandwidth selection criteria proposed by Cattaneo et al. is in orange. The running variable is the PRI margin of victory. Habitat population is normalized by the urban population in the municipality in 2010. Habitat blocks is normalized by the number of urban blocks in the municipality in 2010. The outcome variables are residualized after controlling for the gender of the mayor and the pre-determined urban population in the municipality.

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