

The Dynamic Effects of Information on Political Corruption: Theory and Evidence from Puerto Rico*

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Abstract: Does the disclosure of information about corrupt activities induce a sustained reduction in corruption? We use publicly released routine audits of municipal governments in Puerto Rico to answer this question. We first develop a political agency model where voters re-elect incumbents based on their performance while in office. We show that, because voters cannot directly observe incumbents' actions, an incumbent whose reputation improved in the previous term is likely to engage in more rent-seeking activities in a future term. Guided by this model, we use longitudinal data on audit results to examine the long-term consequences of providing information to voters on levels of political corruption. We find that municipal corruption levels in subsequent audits are on average *the same* in municipalities audited preceding the previous election and those not audited then. In spite of this, mayors in municipalities audited preceding the previous election have higher re-election rates, suggesting that audits enable voters to select more competent politicians. We conclude that short-term information dissemination policies do not necessarily align politicians' long-term actions with voter preferences as politicians exploit their reputational gains by extracting more rents from office.

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I. Introduction

In a well-functioning representative democracy, citizens should select competent politicians to administer public affairs and hold them accountable for their performance. To succeed in these tasks, citizens must have appropriate information about candidates' characters, abilities, and performances while in office (Manin, Przeworski, and Stokes 1999; Besley 2006). Accordingly, a growing body of research finds that voters' access to evaluations of politician performance enhances government responsiveness, reduces corruption and rent-seeking behaviors, and promotes electoral accountability in the short-run.¹ However, it is less well understood whether information dissemination policies can induce a *sustained* reduction in rent seeking, aligning politicians' long-term actions with voters' preferences. These long-term consequences are particularly relevant given the dynamic incentives – re-election incentives and reputational concerns – faced by politicians.²

The central goal of this paper is to study the *long-run* corruption consequences caused by the disclosure to voters of information about politicians' corrupt actions through audit reports. To guide our empirical analysis, we first develop a simple model of political agency. In this model, voters decide whether to re-elect an incumbent politician but are unable to observe his degree of competence or actions as an officeholder. Publicly disseminated audit reports provide information to voters on these actions. If voters re-elect incumbent mayors based on their performance while in office, a mayor whose reputation has improved in the past can exploit this reputational improvement to engage in rent-seeking activities in a later term. Given these perverse incentives, we show that mayors in municipalities whose actions, either corrupt or honest, have been made public in the past will be on average *as corrupt* in the next term as those whose actions have not been made public.

The empirical content of the theory imposes demanding requirements for validation. We need exogenous variation in publicly available information on politician performance, as well as longitudinal data on political corruption. We take advantage of a unique setting that provides us with the opportunity to examine such relationships. The government of Puerto Rico has established an independent body that systematically conducts municipal government audits, the findings of which are made publicly available and disseminated to media sources. We employ a longitudinal dataset of the extent of corruption constructed from the audit reports for all municipalities during the period 1987-2005, and exploit the exogenous ordering of municipal audits to help us establish the causal relationships of interest. Specifically, we first observe whether a government is revealed to be clean or corrupt before a particular

¹ For evidence regarding government responsiveness, see e.g., Besley and Burgess (2002) and Björkman and Svensson (2009); regarding corruption and rent-seeking behaviors, see e.g., Reinikka and Svensson (2005) and Olken (2007); regarding electoral accountability, see e.g., Ferraz and Finan (2008) and Banerjee et al (2010).

² Various models of political agency predict that when information serves as a disciplining device for corruptible politicians in the current term, this allows for some corruptible politicians to 'pool' with non-corruptible types, enhancing the likelihood of corruption in future terms. See Besley (2006) for a survey and a detailed discussion.

election – when the results of the audits are most salient to voters – or after the election.³ We then compare the governments’ levels of reported corruption across these (pre-election/post-election audit) groups of municipalities, both during the audited term and in subsequent terms.

We find that pre-election audits induce a significant reduction in municipal corruption levels of approximately 67 percent, as well as an increase in incumbent mayors’ electoral accountability. We also document that negative pre-election audits lead to a significant degree of positive selection of subsequent mayors based on their pre-incumbency earnings. These findings are remarkably consistent with the *short-run* disciplining, sanctioning, and selection effects of auditing programs found in previous field experimental studies.⁴ However, in contrast to these desirable short-run consequences of the audits, municipal corruption levels in the subsequent round of audits are on average *the same* in municipalities audited preceding the previous election and those whose audits became publicly available afterwards. Thus, short-term information dissemination policies do not necessarily align politicians’ long-term actions with voter preferences as politicians exploit their reputational gains by engaging in more corrupt practices.

To further examine whether dynamic reputational concerns are at play, we test an additional prediction of the theory. Our model suggests that an incumbent’s expected reputation, i.e. the likelihood that he is a competent type, is better following an audited period. Because more able types are more likely to refrain from corruption, the model predicts a positive selection effect on re-election rates in the subsequent term. More interestingly, although a mayor with a better reputation should be more rent seeking, in equilibrium voters’ re-election rules are less stringent so that the incumbent finds them easier to meet. Thus, both the selection and sanctioning effects should induce higher re-election rates of incumbents in the following election, particularly among mayors that audits show have refrained from rent seeking in the previous audit. We find evidence of these positive, next electoral cycle re-election effects using longitudinal data on the re-election rates of incumbent mayors. These relationships also support the hypothesis that information about corruption induces an improvement in accountability in the short-run, and yield perverse incentives in the long-run.⁵

The research design and the richness of the data allow us to distinguish our explanation for corrupt behavior from a variety of alternative interpretations. First, even though the timing of the

³ The contrast between the pre- and post-election audits may have two (or more) sources. The information contained in audits may be of greater immediate interest to voters when an election is looming, so the media invests more resources in disseminating audit results and/or the information is more salient to voters. Even if information from post-election audits does reach voters, they may not use it during the subsequent election because of recency bias – the tendency for voters to place more weight on recent information (see Berry and Howell (2007), and the survey by Lewis-Beck and Paldam (2000)).

⁴ The disciplining and electoral accountability effects are consistent with the experimental findings in Olken (2007) and Ferraz and Finan (2008), respectively. As for politician selection effects, see for instance Besley (2005), Besley, Pande and Rao (2007), and Brozzo et al (2010).

⁵ This is consistent with information dissemination on politicians’ actions leading to an increase in *ex ante* voter welfare.

municipal government audits is pre-determined, our results would be undermined if the actual auditing process differs systematically before and after elections. We do not, however, find any evidence that auditors were corrupt or that mayors with more political power or mayors affiliated with higher levels of government are more likely to receive preferential audits. A second concern is that political cycles are potentially correlated with our comparison of municipalities based on the timing of the audits. However, we report evidence that the actual timing of the audited periods does not influence our results. Finally, we present evidence inconsistent with other plausible channels, such as responses from higher levels of government to audit results.

The study contributes to the growing empirical literature documenting how electoral accountability, and information provision in particular, influences political corruption. Using a randomized experiment in Indonesian villages, Olken (2007) analyzes whether different monitoring mechanisms reduce corruption in infrastructure projects, and finds that a top-down auditing scheme is effective in decreasing corruption in the short-run.⁶ Most notably, in a series of papers Ferraz and Finan (2008; 2010) use similarly objective measures of corruption from audit reports of municipal governments in Brazil to study whether electoral accountability serves as a mechanism to align politicians' actions with voters' preferences. Specifically, Ferraz and Finan (2008) show that electoral accountability is enhanced when information about corrupt practices in audited municipalities is publicized, whereas Ferraz and Finan (2010) show the extent to which re-election incentives affect political corruption in the short-run.⁷ Finally, Niehaus and Sukthankar (2011) show evidence of dynamic incentives for the corrupt behaviors of Indian bureaucrats. Our paper contributes to the literature by providing the first evidence (to our knowledge) on the diverging long and short run impacts of information revelation on political corruption.

The paper is organized as follows. Section II provides background on Puerto Rico's municipal auditing program, the municipal government system, and the national debates that influence local politics. We follow with a description of the data in Section III. Section IV presents our political agency model and discusses its main empirical implications. Section V discusses the empirical implementation of the model, the study's research design, and the main identifying assumptions. We present central empirical results of the paper and robustness evidence from the tests in Sections VI and VII. The paper concludes in Section VIII with a discussion of our work in the context of the literature on voter information and political corruption.

⁶ Stromberg (1999), Gentzkow (2006), Gentzkow, Glaeser and Goldin (2006) provide historical evidence of the consequences of media access on political behaviors. Besley and Burgess (2002) show that newspaper circulation affects the responsiveness of state governments in India to negative shocks to food production and flooding.

⁷ For evidence on the policy consequences of re-election incentives, see Besley and Case (1995) and List and Sturm (2006). Alt, Bueno de Mesquita, and Rose (2009) find that term limits affect the expected quality of incumbents. Martínez-Bravo et al (2007) study the consequences of the introduction of local elections for local politicians' accountability.

II. Background

II.A. Municipal Government Administration and Politics

Municipal governments in Puerto Rico are the level of government closest to citizens. A mayor and a local assembly govern the municipality; these officials are elected for a four-year term following the Commonwealth (and U.S. federal) government electoral cycle.⁸ Mayors and municipal council members *do not face term limits*. In fact, mayors from municipalities where their party is very dominant tend to have high re-election rates. Also, although the local assembly is usually under the control of the dominant party, the law guarantees some representation for minority parties (i.e., a small number of seats for the party that ended in second place, one seat for the party in third place). Minority assembly members usually carry out oversight work, exposing waste and corruption. The mayor appoints the top management of the municipality.

Although municipal governments possess a greater degree of autonomy than counties and cities in the United States, their sphere of influence is somewhat more limited. The bulk of the services they provide are infrastructure construction and maintenance, solid waste management, and public health services. There is heterogeneity in municipalities' fiscal autonomy, both in their ability to raise tax revenues and in their autonomy in expenditure decisions.⁹

Finally, we briefly describe the nature of political cleavages, party structure, and the degree of political participation and competition at the Commonwealth level, all of which are central to municipal politics. The Commonwealth of Puerto Rico is an unincorporated territory of the United States, and national politics are essentially shaped by the debate over P.R.'s political status relative to the United States. The three main political status alternatives are federal statehood, independence, and continuation of the current Commonwealth status. These positions shape the political party system and are the main political cleavages (Anderson 1989, 1998; Cámara Fuertes 2005). The New Progressive Party (NPP) supports federal statehood, the Popular Democratic Party (PDP) supports the Commonwealth status, and the Puerto Rican Independence Party (PIP) supports independence. The NPP and the PDP are the two main political parties; they are similar in their electoral support and regularly exchange the reigns of power. The PIP is a relatively small party, and usually receives between three and five percent of votes.

⁸ The size of the municipal assembly, which varies between 12 and 16 members, is a step function of the population that resides within its boundaries.

⁹ In 1991 the legislature approved a series of laws as part of a package of municipal reforms. These municipal reforms, of which Act No. 81 was the centerpiece, greatly increased the municipal governments' autonomy vis a vis the central government and allowed them a greater role in the social and economic development, as well as the spatial planning, of their territories. Thus once the municipal reform laws became effective some municipalities began to assert a greater role in education and law enforcement, areas previously reserved for the central government. In practice, the degree of autonomy and sphere of action that each municipality has is related to its size. Large municipal governments with active mayors such as San Juan (the capital), Guaynabo, Bayamón, and Caguas have asserted a significant degree of autonomy. Smaller municipalities with access to fewer resources are still significantly more dependent on the central government.

The intensity of the status debate supersedes all other debates, including the economic one typical of most nation states. It has been argued that, as a consequence, parties hold similar positions on many issues and the NPP and PDP have been labeled as catch-all parties (Meléndez 1998). Partisanship in Puerto Rico is high and most voters vote for the same party in the executive, legislative and municipal ballot. Thus, electoral landslides and coattail effects are common. As a general rule (with some notable exceptions), the incumbent governor’s party overwhelmingly controls both chambers of the Legislative Assembly and a majority of municipal governments. Unlike the United States—and similar to Latin American and European nations—Puerto Rico has a disciplined party system. This allows for effective partisan control of all levels of government when the same party controls all three administrative levels. Given the constitutional, and often personal, strength of the governor, his or her ideology or point of view is forcefully applied to all levels of government.

Some have argued that these distinct cleavages may yield municipal political machines that provide patronage. There is evidence of this claim in audit reports and court cases (see Section II.B). However, municipal political corruption is a phenomenon that predates the current political environment – early 20th century U.S. colonial administrations argued that this corruption was egregious even during the late Spanish colonial regime (Report of the Governor of Porto Rico 1902). In fact, the early U.S. colonial administration founded the Office of the Auditor in 1900, a precursor to the Office of the Comptroller of Puerto Rico, to address governance issues.

II.B. The OCPR Municipal Government Auditing Program

The Office of the Comptroller of Puerto Rico (“OCPR”) is an autonomous government agency created by the 1952 Constitution of the Commonwealth of Puerto Rico. Its mission is to “audit the property and public funds transactions with independence and objectivity to determine if they have been done in accordance to the law[, and] promote the effective and efficient use of the government resources [...]” (Office of the Comptroller 2009). To achieve its objectives, the OCPR periodically audits state-level government agencies and public corporations, including the legislative and judicial branches, as well as municipal governments.

The OCPR has been carrying out audits on municipal governments and generating and disseminating reports uninterruptedly since 1953. According to its constitutive legislation, municipal governments ought to be audited every other fiscal year. However, due to the OCPR’s resource constraints, there may be some delay in the timing of the audit. Importantly for our design, the order of the audits follows a routine pattern: municipalities are audited following a pre-specified order established in the 1950s. Once all municipalities have been audited, a new auditing round takes place following the same pre-specified order.

Once a municipality is identified to be audited in a fiscal year, the OCPR sends a team of auditors to gather preliminary information on a subset of activities and transactions which have taken place in the time period since the latest audit coverage period. Following this preliminary audit, a team of approximately 10 OCPR auditors are sent to the municipality to examine these accounts and documents, as well as to inspect for the existence and quality of public work construction and delivery of public services. Auditors also interview municipality officials, members of the local community, as well as municipal council members, in order to get direct complaints about any malfeasance. Once the audit is complete, the auditing team completes a preliminary audit report. This preliminary report is shared with the municipality officials (i.e., the mayor and top management) to provide them with an opportunity to contest its findings. Once the response is received and evaluated, a final report is issued and disseminated to the public and to media sources through press conferences (more recently, reports are also being posted on the Internet). Although the OCPR cannot officially classify findings as corrupt violations or not, the agency refers findings of misuse of public funds to the P.R. Department of Justice and/or to the state-level executive branch's Office of Government Ethics. The OCPR may publish multiple reports on a municipality for one auditing period depending on the size or complexity of the municipal government.

A number of measures are taken to minimize potential biases in the conduct of the audits and in the dissemination of their findings. First, there is a constitutionally defined objective to provide the OCPR with a substantial degree of autonomy from the rest of the central government structures, in order to isolate the agency from undue external interference. To help achieve this, the Comptroller is appointed by the P.R. Governor for a ten-year term.¹⁰ Second, the OCPR is technically under the state legislature. Since the agency's activities are focused on the executive branch, this gives it an additional layer of protection from undue influence. Third, the auditors, who are hired based on a competitive public examination and earn highly competitive salaries, receive extensive training prior to visiting the municipalities. Finally, in order to reduce/minimize local-level conflicts of interest, individual auditors are precluded from participating in audits of their municipality of residence.

All seventy-eight municipalities were audited during our period of interest (1987-2005) multiple times. The timing of the dissemination of the reports is depicted in Figure I. As can be seen, there is a tendency to publish reports at the end of the central government's fiscal year (i.e., in June) (as well as a tendency to publish more reports in recent years).¹¹ Importantly, there is no insignificant tendency for the OCPR to publish a disproportionate number of reports in the months preceding an election (i.e. August

¹⁰ The appointment requires the advice and consent of the members of both legislative chambers. In addition, the person can only be removed from office while serving the term by an impeachment procedure. Third Article, Section 22 of the Constitution of the Commonwealth of Puerto Rico.

¹¹ For the 1997-2000 and 2001-2004 terms, almost all the municipalities were audited at least once. José M. Díaz Saldaña, the Comptroller appointed in October 1997, made a point to audit all municipalities at the beginning of his term, a fact clearly shown by the data.

through October) (Figure I, Panel A). There is also no evidence of bias in the publishing of reports for municipalities in which the incumbent mayor is in the opposition to the Governor in office or to the party of the Governor who appointed the Comptroller (Panel B). This serves as *prima facie* evidence that the agency does not time the dissemination of findings to influence electoral results.

Each report contains, among other information, the period covered by the audit and, most importantly, an itemized list describing each irregularity. Based on our evaluation of the reports, we classified irregularities into those associated with corruption and those that represent waste and/or poor administration. A more stringent classification involves using only those irregularities referred to the Department of Justice.¹² As expected, corruption in municipal governments in Puerto Rico takes diverse forms, but corruption schemes used by local politicians and bureaucrats are based on a combination of fraud in procurement, the use of fake receipts (i.e., “phantom” firms), the illegal hiring of employees, and over-invoicing the value of products or services. In addition, the audit reports also suggest that some individuals simply divert resources for personal purposes. Since these strategies are complementary in allowing government representatives to appropriate resources (and following the existing literature), we combine these into a single measure (see Section III.A).

Some examples will help illustrate the types of irregularities uncovered by the audits.¹³ In the municipality of Maunabo during February-March 1997, contracts for the pavement and maintenance of roads summing up to approximately 138K USD were partitioned into four separate projects in order to avoid having to carry out a public auction.¹⁴ Moreover, the auditors were unable to confirm the authenticity of other quotes submitted for the projects. We classified this finding as an instance of fraud in procurement. Second, in the municipality of Vieques during October 1995, the municipal Auction Board carried out an auction for the construction of four classrooms in the school at Barrio Playa Grande. The Board did not adjudicate the contract, in spite of there being valid bids by two independent contractors for 225K and 340K USD, respectively. In a second auction in November 2005, the second contractor submitted the only bid for the project, for 325K USD. In December 2005, the mayor signed the contract for the project with the second contractor for 285K USD. These actions by the municipal managers caused the municipality to pay 60K USD in excess for the completion of the project. We classified this second finding as a case of auction fraud in procurement and over-invoicing.

As an example of corruption in the hiring of municipal employees, the case of the municipality of Toa Baja is illustrative. In a report published in June 2000, the OCPR reports the illegal hiring of 22 individuals who were relatives of the mayor and 11 individuals who were relatives of members of the

¹² The reports also contain information on the area of government in which the irregularity took place (e.g., public infrastructure, law enforcement), as well as – if available - the misappropriated amount and the date(s) of the act.

¹³ For details of the audit report findings, see excerpts from these in Appendix B.

¹⁴ The 1991 Municipal Government Law (“Ley de Municipios Autónomos del Estado Libre Asociado de Puerto Rico de 1991 (Law Num. 81) establishes that for any project exceeding 40K USD, the municipal government must carry out a public audit.

Municipal Assembly. Twenty-one of these individuals, hired between September 1991 and October 1997, did not have the academic requirements or other minimum requirements to serve in their posts. These and other documented irregularities, including the excess compensation of municipal employees by approximately 262K USD, are classified as one finding of corruption in HR practices. Analogous findings in the municipalities of Cidra and Maricao are available in Appendix B.

Other examples of corruption in Maricao and Hormigueros illustrate instances of over-invoicing. In October 1998, the mayors in both municipalities formalized contracts for the collection and disposal of debris resulting from the damages caused by Hurricane Georges (in September 1998), for an estimated cost of 4.20 and 3.69 million USD (the cost per cubic yard of 28 and 26 USD), respectively. The OCPR reported evidence from the Federal Emergency Management Agency (FEMA) and the U.S. Army Corps of Engineers of over-invoicing in both cases, as the independent contractors submitted invoices for the collection and disposal of 155,157 and 31,508 cubic yards of debris, whereas it was identified that they collected 50,157 and 51,683. This represented over-invoicing by approximately 2.94 and 0.75 million USD, respectively. The OCPR referred the violations to the Department of Justice. As a consequence of the audit report, the former (two-term) mayor of Hormigueros was convicted on extortion and bribery charges for requesting and receiving 100K USD in kickbacks from the owner of the contracting firm.¹⁵ In contrast, the mayor of Maricao (in his third term) was re-elected in 2004, following the dissemination of the audit report in 2001.

News on the findings from the audit reports are routinely reported in the island-wide press – the main sources are of OCPR press conferences and releases as well as opposition candidates’ campaigns. Although we do not have direct evidence showing that voters learned about the audit reports, anecdotal evidence suggests that the information from the audits did reach voters. For instance, an article published on September 25th 2008 (preceding the 2008 election) in a major newspaper regarding the outcome of a recent audit of the municipality of San Juan highlighted findings of mismanagement attributed to municipal employees. Specifically, the report highlighted that Jorge Santini – the mayor – and the municipality’s finance team did not appropriately administer the municipality’s finances and incurred in extravagant/unnecessary expenditures to highlight the Mayor’s image. The report was used by Ferdinand Pérez (the opposition candidate) to declare that Santini was “a disaster as an administrator”; and the statement was later challenged by the incumbent (Hopgood Dávila 2008). In spite of this finding of plausible misuse of funds, Santini – a mayor in his second term – was re-elected for a third term.

III. Data

¹⁵ See *El Pueblo de Puerto Rico, Apelado v. Francisco Javier Rivera Toro, Apelante*. KLAN0501622. Tribunal de Apelaciones de Puerto Rico, Región Judicial de Mayagüez. 2009 PR App. LEXIS 3664, 20 de octubre de 2009.

III.A. Measures of Corruption based on the Audit Reports

The main data sources for the study are the municipal audit reports conducted by the OCPR. In this study we work with all municipal audit reports during the 1987-2005 period, which are relevant for the 1988 through 2004 elections. Note that there were two Comptrollers during the period for which we use the audit reports: Ileana Colón Carlo (1987-1997) and Manuel Díaz Saldaña (1997-2010).¹⁶

Each report contains a list of findings and a detailed description of each. These are classified as main and secondary findings. Main findings are actions that have substantive consequences, while secondary findings are those considered by the OCPR not to have serious consequences. Each reported finding consists of a detailed explanation of a situation, the implicated individuals (if identifiable), and the reason why it is considered a violation or irregularity. We generate codes from each report's list of findings.¹⁷ For each finding we coded the type of individual implicated in the finding – whether it was (i) the mayor or vice mayor, (ii) a member of the municipality's top management such as the finance director, (iii) a rank and file employee of the municipality, or (iv) whether the individual cannot be identified.

The research team also classified the findings based on the type of act. Although corruption in municipal governments in Puerto Rico takes diverse forms, most corruption schemes used by local politicians and bureaucrats to appropriate resources are based on a combination of fraud in procurement, the use of fake receipts, “phantom” firms, or “phantom” employees, and over-invoicing the value of products or services. In addition, the audit reports also suggest that some individuals simply divert resources for personal purposes. We also coded the area of government activity in which the act took place (e.g., public infrastructure, law enforcement), the misappropriated amount (if stated), the date(s) of the act, and whether the finding was referred to the P.R. Department of Justice. Most importantly, we created a code that specified whether the finding constituted an act of corruption or not. We operationalize corruption as an act by any municipal employee that led to a *personal* financial or political benefit.¹⁸ Thus, the mayor receiving a bribe for a contract, or using municipal employees for his or her electoral campaign would be considered in our coding scheme as acts of corruption. On the other hand, poor bookkeeping was not (unless the report stated that it directly involved the cover-up of a corrupt violation).

¹⁶ Díaz Saldaña exceeded his ten-year term because the then-governor did not submit a candidate to the legislative assembly when his term expired (in 2007), and the incoming governor selected a replacement in 2010. The Constitution states that the incumbent Comptroller will continue to occupy his position until he resigns or is substituted by a new one.

¹⁷ Before we began the coding process, the three (3) research assistants were given extensive training in content analysis, coding, and the details of the audit reports. We also ran tests for inter-coder and intra-coder reliability. The process continued until coder reliability was at least 0.9. The same coders worked with the reports throughout the project. Finally, a fourth research assistant examined the data to check for any errors.

¹⁸ This definition is similar to the one used by the OCPR, which states that corruption is the use of government functions for private gain (Díaz Saldaña 2007). However, the OCPR does not specify whether a finding is considered a corrupt violation or not.

To construct measures of corrupt violations, we follow Ferraz and Finan (2008; 2010) and combine these indicators by summing up the number of times each one of these irregularities appear, overall and by category. However, in contrast to their previous work, because the OCPR may publish multiple reports on a municipality during one auditing period and this depends on the size or complexity of the municipal government, we normalize our measures by the number of reports published in that auditing period.¹⁹ Finally, as will be made clearer once we discuss the study's research design, we define the time periods preceding each election as the two years preceding the election, and the post-election audit reports as those published in the two-year period following it. To take into account the fact that a subset of the municipalities has audit reports published in both periods, for these we aggregate only those reports published before the election and assign them to the pre-election audit group.

III.B. Other Data Sources

We employ two additional datasets available from the P.R. State Electoral Commission (CEE). The first comprises the electoral results of the municipal and statewide general elections for each municipality for election years 1988 through 2004. These data allow us to construct measures such as whether the incumbent mayor runs for re-election in the general election, whether he/she is re-elected, the vote share and win margin for the election, his/her political party affiliation, whether he/she is in the opposition to the incumbent party in power at the state level, and the terms in office. The second dataset was compiled from publicly available state-level income tax returns for the four year period preceding each of the 2000 and 2004 elections. All candidates are required by law to submit these documents to the CEE in order to be certified, and they subsequently become part of the public record.

As for municipal government-level outcomes that may be influenced by incumbent politicians, we use annual municipal government budget data for the fiscal years 1991-92 through 2007-08. Finally, to capture underlying variation in municipal characteristics, we rely on the 1990 and 2000 U.S. Census of Population for Puerto Rico. We use measures of the proportion of adult individuals ages 25 and older with schooling attainment levels lower than ninth grade, with a high school education or more, and with a college education or more, as well as the municipality's household median income and poverty ratio for the years 1989 and 1999. Finally, we use information on municipality-level annual unemployment rates from the P.R. Department of Labor. Descriptive statistics of these variables are available in Table I.

IV. Theoretical Framework

¹⁹ The mean number of reports per audited municipality during this period ranged from 1.2 for the 1988 period to 2.1 for the 2000 period.

In this section, we present a simple model that helps interpret our findings. We utilize the political agency framework studied by Schwabe (2010) and others, whereby voters decide whether to re-elect an incumbent politician but are unable to observe his degree of competence or his actions. If voters re-elect incumbent mayors based on their performance in office, a mayor whose reputation has improved in the past can exploit this reputational improvement to engage in rent-seeking activities, leaving voters indifferent between re-electing him and electing an unknown challenger. Given these perverse reputation incentives, the model predicts that re-elected mayors who have been shown to have either refrained from or engaged in rent-seeking activities in the past will be *on average* as corrupt in future terms than mayors whose levels of corruption have not been exposed.

Reputation and Accountability in Repeated Elections

Consider a discrete-time, infinite horizon model of municipal politics. In each period (indexed by $t \in \{1, 2, \dots\}$), a representative voter must select a politician to administer local public affairs. Following each election, the elected politician chooses a level of effort $e \in [0, 1]$ that influences, but does not perfectly determine, the level of a public good that is provided to voters, $g \in \{0, 1\}$. Specifically, the probability with which the public good is provided is equal to e : $\Pr(g = 1|e) = e$. The (representative) voter values only the public good; thus $E[g] = e$ is also the voter's expected utility.

Politicians are one of two types – normal or corrupt – with μ denoting the proportion of normal types in the infinite pool of potential candidates. Normal types may choose to work to avoid corruption and mishandling of public funds by exerting costly effort e . Their per-period utility while in office is $u(e) = R - c(e)$, where R ($R > 0$) are ego-rents, salary, and other fixed benefits of holding office. The cost of effort is increasing and convex in its level ($c'(e) > 0$, $c''(e) > 0$ for all $e > 0$), as well as satisfying the conditions for an interior solution ($c(0) = c'(0) = 0$, and $c(1) > \frac{\partial R}{(1-\delta(1-p))}$).²⁰ Payoffs outside of office are normalized to 0. In contrast, corrupt politicians always choose to exert no effort ($e = 0$). This may be because effort is too costly for them for it to be worth exerting (i.e., $c'(e)$ is very large) or due to incompetence. Politicians and the voter have a common discount factor $\delta \in (0, 1)$.

Each politician is infinitely lived and may serve for as many periods (i.e., terms) in office as the voter asks him to. However, once replaced by a randomly selected challenger, a politician cannot return to office. Finally, we assume that a politician's type and action (e) are private information of the politician – not observable by voters. Thus, voters must infer incumbents' type and action from their performance.

To help remedy this monitoring problem, and to help voters keep politicians in line, the OCPR conducts periodic audits in which the financial activities of the government are scrutinized and any

²⁰ The lower bound on c that we impose may be more restrictive than is necessary for an interior solution. The fully specified condition, its motivation, and implications are discussed in detail in Online Appendix A.

irregularities are reported to voters. We interpret audits as making politicians' effort, e , publicly observable and we write $a_t = 1$ to denote an audit at time t (and $a_t = 0$ otherwise)²¹. An audit will take place before any given election with probability $p \in (0,1)$. To match the context, we assume that politicians know whether they will be audited when making corruption/effort decisions.

The voter assigns in each period t a probability μ_t that the incumbent is a normal type; this is the politician's *reputation*. Because, following the literature, we assume that new politicians are selected randomly, the reputation of a politician at the beginning of his first term is μ . Thereafter, the incumbent's reputation is updated according to Bayes' rule each time the voter observes g or e , via a function that we denote $\hat{\mu}$.

The timing of the infinitely repeated stage game is as follows. At the beginning of each period, voters decide whether to re-elect the incumbent or select a challenger who has been drawn at random from the pool of potential politicians. Then, the OCPD announces whether there will be an audit during the current period. Taking this into account, the politician makes an effort choice, after which voters observe their payoffs and audit results when available, and update their beliefs regarding the incumbent's type.

When making re-election decisions, the voter has information on all past realizations of g , audits, and election results, which we call a t -history h_t . Thus, a re-election strategy is a function from the set of all such possible t -histories to the incumbent's probability of re-election: $\sigma : H \rightarrow [0,1]$. Similarly, a politician's effort strategy is a function from all possible histories of outcomes, as well as whether there will be an audit ($a_t \in \{0,1\}$) during the current period, to an effort choice: $e : H \times \{0,1\} \rightarrow [0,1]$. Given strategies and beliefs, we can write the voter's value function, before it is known whether there will be an audit, recursively:

$$V(\sigma, e, \hat{\mu}; h_t) = \hat{\mu}(h_t)[pe(h_t, a=1) + (1-p)e(h_t, a=0)] + \delta E[V(\sigma, e, \hat{\mu}; h_{t+1}(g, a))] \quad (1)$$

where $p \in (0,1)$ denotes the probability of an audit, and the expectation is taken over the level of public goods as well as whether there is an audit and, if so, its results. Similarly, we denote the value function of a normal incumbent politician $Q(\sigma, e, \hat{\mu}; h_t)$:

$$Q(\sigma, e, \hat{\mu}; h_t) = pu(e(h_t, a=1)) + (1-p)u(e(h_t, a=0)) + \delta E[Q(\sigma, e, \hat{\mu}; h_{t+1}(g, a))]. \quad (2)$$

As in most infinitely repeated games, there are many candidate equilibria. Following Schwabe (2010), we argue that a class of perfect public equilibria of this game – reputation-dependent performance cutoffs (RDC) equilibria – are particularly convincing because they meet a stringent test of credibility on

²¹ The event $a_t = 1$ in the model corresponds to a pre-election audit in the data. The event $a_t = 0$ corresponds to a post-election audit whose results will not be disseminated.

the part of the voter. In RDC equilibria, incumbents are re-elected only if their observed performance exceeds a *cutoff that varies with the incumbent's reputation*.²² Crucially, these performance cutoffs vary in such a way as to make it incentive compatible for politicians to exert just enough effort to leave the voter indifferent between re-electing the incumbent and electing a challenger, thus making the voter's payoffs (i.e., value function) constant across reputations. If this is the case, voters face no commitment problem when making re-election decisions because they will be indifferent between having the incumbent or a (randomly selected) challenger in office.²³ We further restrict our attention to the RDC equilibrium yielding the highest feasible payoffs to the voter. We call this equilibrium the *voter-optimal* RDC equilibrium.

Definition

A *voter-optimal* RDC equilibrium with value V is an equilibrium in which:

- Politicians follow an effort strategy $e(\mu_t, a)$ that satisfies voter-indifference: $V(\mu_t) = V$ for all politicians with reputation $\mu_t \in [\mu, 1]$.
- The voter follows a reputation-dependent performance threshold re-election strategy:

$$\sigma(e_t, \mu_t, a_t = 1) = \begin{cases} 1 & \text{if } e_t \geq e(\mu_t) \\ 0 & \text{otherwise} \end{cases}$$

$$\sigma(g_t, \mu_t, a_t = 0) = \begin{cases} 1 & \text{if } g_t = 1 \\ r(\mu_t) & \text{if } g_t = 0 \end{cases}$$

- The voter's constant per-period expected utility $V(1-\delta)$ is maximized subject to these constraints.

In the voter-optimal RDC equilibrium of this model politicians are only re-elected if they have revealed themselves to be normal. Thus, there are only two relevant levels of incumbent reputation: μ and 1. The only other factor that may affect the level of effort exerted by a politician is whether there will be an audit during the current period. We refer to the states of the world in which the incumbent has reputation $\mu_t = \mu$ as states $\{\mu, a=1\}, \{\mu, a=0\}$ and the states in which the incumbent has reputation $\mu_t = 1$ as $\{1, a=1\}$ and $\{1, a=0\}$. Correspondingly, let $e_{\mu, a=1}$, $e_{\mu, a=0}$, $e_{1, a=1}$, and $e_{1, a=0}$ denote the equilibrium levels of effort in these states, respectively. In order to lighten notation, we will denote $e_s = pe_{s, a=1} + (1-p)e_{s, a=0}$ where $s \in (\mu, 1)$, for ex-ante expected levels of effort.

²² This stands in contrast to equilibria in which voters use performance standards to make re-election decisions without being responsive to an incumbent's reputation. In these equilibria the voter's continuation payoffs vary systematically with the incumbent's reputation, and the voters will be expected to throw incumbents out of office who would normally outperform challengers. That is, the voters face a commitment problem which undermines the credibility of their re-election strategy. Formally, these equilibria are not weakly renegotiation-proof (WRP, Farrell and Maskin 1989).

²³ Because these equilibria depend on the history of play h_t only through the incumbent's current reputation μ_t , we write value functions and strategies as functions of reputation. To lighten notation, we also drop the dependence of value functions on equilibrium strategies.

Theorem 1: In the voter-optimal RDC equilibrium, the voter uses the following re-election strategy:

a) Following a politician's first term in office, given an audit:

$$\sigma(\mu_t = \mu; a = 1) = \begin{cases} 1 & \text{if } e \geq e_{\mu, a=1} \\ 0 & \text{otherwise} \end{cases}$$

b) Following a politician's first term in office, given no audit:

$$\sigma(\mu_t = \mu; a = 0) = \begin{cases} 1 & \text{if } g_t = 1 \\ 0 & \text{otherwise} \end{cases}$$

c) Following a politician's second or higher term in office, given an audit:

$$\sigma(\mu_t = 1; a = 1) = \begin{cases} 1 & \text{if } e \geq e_{1, a=1} \\ 0 & \text{otherwise} \end{cases}$$

d) Following a politician's second or higher term in office, given no audit:

$$\sigma(\mu_t = 1; a = 0) = \begin{cases} 1 & \text{if } g_t = 1 \\ r & \text{if } g_t = 0 \end{cases}$$

where $r \geq 0$.

Proof: See Online Appendix A.

When there is an audit, the effort level is observed and perfectly reveals the incumbent's type as well as any deviation from the equilibrium level of effort. The voter's re-election rule keeps an incumbent in office as long as he has behaved as a normal politician is expected to. Without an audit, the only credible signal of high effort is a high level of the public good ($g=1$). Incentives are at their strongest when failure to deliver the public good means that the incumbent will be thrown out of office, and this is what the voter does in state $\{\mu, a=0\}$.

Short-Run Accountability Effects of the Audits

Because audits provide additional information about a politician's actions, they enable the voter to punish high corruption and reward restraint more accurately, making incentives more effective. We thus expect that corruption will be lower during audited periods than during non-audited periods. Our first propositions support this intuition.

Proposition 1: In the voter-optimal RDC equilibrium, $e_{\mu, a=1} > e_{\mu, a=0}$ (first term politicians - reputation μ).

Proof: See Online Appendix A.

The second proposition states conditions under which there are positive disciplining effects of the audits for second or higher term incumbents.

Proposition 2: In the voter-optimal RDC equilibrium, $e_{l,a=1} > e_{l,a=0}$ if $\mu e_\mu > e_{\mu,a=0}$. Sufficient conditions for $\mu e_\mu > e_{\mu,a=0}$ are:

- (a) $\mu \geq \tilde{\mu}$, where $\tilde{\mu} \in (0,1)$ large enough; or,
- (b) $c''(e) > K$ for all e and some constant $K > 0$ large enough.

Proof: See Online Appendix A.

In general, it is plausible that among second or higher term incumbents, effort levels may be lower during audited periods than during non-audited periods. This is because incumbents with high reputations are expected to exert a lower average level of effort than first term incumbents. We thus identify conditions in which effort levels are higher – when competition from challengers is stark (i.e., a sufficiently high initial reputation level μ), or when the politician’s marginal cost of effort is high, driving a wedge between the effort levels incumbents are willing to exert during audited and non-audited periods.

Effects on Short-Run Electoral Outcomes

Again, because audits provide additional information about a politician’s actions, they enable the voter to punish high corruption and reward restraint more accurately, improving the degree of electoral accountability. In particular, the model predicts that the unconditional probability of re-election is *higher* during audited than during non-audited periods, but that this varies depending on the outcome of the audit, supporting our intuition.²⁴ Denote by $q_{t|a=1}$ and $q_{t|a=0}$ the re-election probability of the incumbent in period t given an audit and no audit in period t , respectively.

Proposition 3: In the voter-optimal RDC equilibrium, $q_{t|a=1} > q_{t|a=0}$.

Proof: See Online Appendix A.

The intuition is as follows: two things must happen in order for an incumbent to be re-elected: he must be a normal type, and the voter must see proof of this. While proof of a politician’s type is assured

²⁴ We may also look at re-election rates conditional on corruption levels. Here again, the model predicts that re-election rates will be higher during audited periods in which there is low corruption. However, there is no difference in re-election rates when there is high corruption as these incumbents are always thrown out of office. When we take this prediction to the data, we must keep in mind the possibility that the number of corruption findings corresponding to $e = 0$ may vary among municipalities, so that the proportion falling into the high corruption category may be increasing with the level of reported corruption.

during an audited period, it is only received probabilistically when there is no audit. Therefore, re-election rates unconditional on corruption levels should be higher during audited than during non-audited periods.

Effects of the Audits on Politician Effort in Future Periods

In equilibrium, the voter must be indifferent between having an incumbent with a high reputation and a new incumbent (with reputation μ). Therefore, politicians of all reputations will perform equally well (or poorly) in expectation so that the voter is indifferent between re-electing them and electing a challenger.²⁵ This implies that politicians with high reputations will pocket the benefits of their accumulated reputation by being more corrupt than normal politicians of lower reputations. Interestingly, the model predicts that reported corruption from future audits should be, on average, constant across municipalities that faced an audit in an earlier period and those that did not.

Proposition 4: In the voter-optimal RDC equilibrium, the ex-ante expected level of effort will be constant across reputations, $\mu e_\mu = e_1 = \bar{e}$, so that the voter's expected stage-game utility is $\mu e_0 + (1 - \mu)0 = e_1$ in both states.

Proof: See Online Appendix A.

Another intriguing prediction of RDC equilibrium involves the dynamics of corruption choices. As we argued above, politicians reap the benefits of their accumulated reputations by being more corrupt. This means that audits showing very low corruption will typically involve normal politicians, early in their career, who are showing great restraint in order to build up their reputation. Good audit results mean that voters will update their beliefs about the incumbent upwards. Perversely, this then allows politicians to be more corrupt in future periods. Formally, because in RDC equilibrium ex-ante expected effort is constant, $E(e_{t+1} | h_t) = \bar{e}$, the expected change in effort is simply the difference between today's effort level and \bar{e} or:

$$E(e_{t+1} | h_t) - e_t = \bar{e} - e_t. \quad (3)$$

Thus, audits showing low corruption (high effort) will be followed, on average, by audits showing moderate corruption (moderate effort). This increase in corruption will be larger when current audit results are cleaner.

Effects on Politician Selection and Electoral Outcomes in Future Periods

²⁵ This type of voter indifference is a part of any renegotiation proof equilibrium. See Proposition 3 in Schwabe (2010).

We can use the model's predictions about re-election rates to draw conclusions about the politician selection effects of audits. Specifically, are incumbents more likely to be normal following an audited period compared to a non-audited period? The answer to this question is affirmative. For incumbents in their first period, only normal types are re-elected, and they are re-elected with higher probability during audited periods. Specifically, the probability of having a normal incumbent during period $t+1$ conditional on having a first term incumbent during period t is: $\mu + (1 - \mu)\mu$ if there was an audit conducted during period t , and $\mu e_{\mu,a=0} + (1 - \mu e_{\mu,a=0})\mu$ if there was not. Similarly, incumbents in their second or higher terms are re-elected with probability 1 following audited periods, and only with probability $e_{I,a=0} + (I - e_{I,a=0})(r + (I - r)\mu)$ following non-audited periods.²⁶

Finally, the model predicts that both the selection and sanctioning effects should induce higher re-election rates of incumbents in the following election, particularly among mayors that audits show have refrained from rent seeking in the previous audit. Denote by $q_{t+1|a=1}$ ($q_{t+1|a=0}$) the re-election probability of the incumbent in period $t+1$ given an audit (no audit) in period t .

Proposition 5: In the voter-optimal RDC equilibrium, $q_{t+1|a=1} > q_{t+1|a=0}$.

Proof: See Online Appendix A.

The proposition formalizes the following logic: conducting an audit means that voters will be more likely to re-elect normal politicians, and normal politicians are more likely to do well enough to get re-elected in subsequent periods – there is a selection effect on re-election rates. Moreover, although higher reputation implies lower effort by the incumbent, in equilibrium voter re-election thresholds (r) are lower and thus easier to meet. Thus, both selection and sanctioning effects influence period $t+1$ re-election rates in the same direction.

V. Empirical Framework

We are interested in examining the dynamic consequences of providing information to voters from audit reports on rent-seeking behaviors in local governments. In particular, our objective is to test the following predictions of our political agency model:

- (i) the expected dissemination of the audit reports should decrease the number of corrupt violations by incumbent politicians in the short-run (Propositions 1 and 2);

²⁶ It is also worth noting that the probability of having a normal type in office during period $t+1$ is higher when there is a normal type in office during period t . This means that the selection effects of audits are persistent: for any integer n , the probability of having a normal type in office during period $t+n$ is higher if there was an audit during period t than if there was not.

- (ii) the incumbent’s likelihood of re-election should be higher *on average* during periods audited pre-election than during those audited post-election (Proposition 3); the probability of re-election should be negatively correlated with the number of corruption findings;
- (iii) politicians in power in the next term will engage *on average in the same level of corrupt violations* irrespective of the municipality being audited preceding or after the election (Proposition 4); and,
- (iv) on average, re-election rates at time $t+1$ should be higher in municipalities that experienced a pre-election audit at time t relative to those that did not (Proposition 5).

These are the main testable predictions that we take to the data.

Our research design exploits the pre-determined routine nature of the publicly released audit reports and the timing of the municipal elections. We compare the outcomes for municipalities whose audit reports were disseminated in the two-year period before each election, relative to those whose audit reports were disseminated in the two-year period following each election, for the election years 1988 through 2000. Although municipalities are not audited at random, we can examine whether this comparison presents problems of identification in various ways. In the following paragraphs we present the empirical specifications used to test these hypotheses, and discuss potential threats to validity.

We estimate the average effect of the expected dissemination of the audits on short-term rent-seeking levels using the following reduced-form specification:

$$c_{mt} = \theta A_{mt} + \beta X_{mt} + \gamma_t + \alpha_m + \varepsilon_{mt}, \quad (4)$$

where c_{mt} denotes the number of corrupt violations per report in municipality m around election year t , and A_{mt} is an indicator for whether or not the municipality audit report was published in the two-year period preceding election year t . X_{mt} is a vector of municipality and mayor characteristics that influence the municipality’s level of corruption.²⁷ The terms α_m and γ_t represent municipality and election intercepts, respectively, and ε_{mt} denotes unobserved characteristics that determine corruption at time t . Under the assumption that A_{mt} is strictly exogenous, the coefficient θ provides a consistent estimate of the average effect of the audit dissemination on rent-seeking in municipal governments at time t , capturing both the effect of expecting to be audited and the public release of this information. Our model (as well as other models of political agency) predicts that $\theta < 0$.²⁸

²⁷ We use as controls the number of municipality government reports, the number of municipal public corporation or consortium reports; indicators for the mayor’s membership in the NPP, for the incumbent being in the opposition party to the state-level executive government, and for the incumbent being in the opposition party to the governor who appointed the Comptroller; the vote share for the incumbent in the previous election; and the incumbent’s number of terms in office.

²⁸ The empirical model captures the idea of “recency bias” – that voters take into account more recent conditions in making electoral decisions, and this thus influence the equilibrium behavior of incumbents. See Berry and Howell (2007), and the survey by Lewis-Beck and Paldam (2000).

An analogous model that uses as dependent variable an indicator for the re-election of the incumbent mayor in election year t (denoted e_{mt}) captures the average effects of the audits and their actual dissemination on the incumbent's electoral accountability. We also test whether the dissemination of the audits increases the likelihood of re-election among politicians exerting high effort (zero reported corruption), and decreases the likelihood of re-election of those mayors shown to have engaged in corruption. Therefore, following Ferraz and Finan (2008), we estimate the model:

$$e_{mt} = \theta_{E1}A_{mt} + \theta_{E2}A_{mt}c_{m,t} + \beta_{E1}c_{m,t} + \beta_{E2}X_{mt} + \gamma_t + \alpha_m + \varepsilon_{mt}, \quad (5)$$

Our model (as well as other models of political agency) predicts that $\theta_{E1} > 0$ and $\theta_{E2} < 0$.

To examine the dynamic consequences of providing information to voters from the audits on the rent-seeking behaviors in local governments, we estimate the average effect of the audits (and their dissemination) in term t on the reported rent-seeking levels in the subsequent audit:

$$c_{m,t+1} = \theta_P A_{mt} + \beta X_{mt} + \gamma_{t+1} + \alpha_m + \varepsilon_{m,t+1}, \quad (6)$$

where $c_{m,t+1}$ denotes the number of corrupt violations per report in municipality m in the subsequent audit, A_{mt} is the indicator for whether or not the municipality audit report was published in the two-year period preceding election year t , and $\varepsilon_{m,t+1}$ denotes unobserved characteristics that determine corruption at time $t+1$. The theory predicts that $\theta_P = 0$ as the incumbent will engage on average in the same level of corruption after an audited period than after a non-audited period. In all longer-term effects specifications, we also include a control for the timing of the next audit ($A_{m,t+1}$, an indicator for whether or not the municipality audit report was published in the two-year period preceding the next election year).²⁹

We further decompose the effects of the pre-election audit by the identity of the agent – the mayor or vice-mayor, or another employee of the municipality – identified in the report as committing the corrupt violation. This distinction may be informative, as it allows us to assess whether voters respond differently to direct violations by mayors and those by other municipal employees.

The overall comparison of municipalities does not capture the possibility that the outcome of the publicly released audit (at time t) contains information about the corrupt behaviors of the (possibly prior) incumbent mayor and other municipal government employees. Specifically, we expect the incumbent politician in the next term to engage in more [less] corrupt activities the more [less] favorable the outcome of the previous period audit. However, this pattern of “mean reversion” should be constant in municipalities receiving a pre-election versus post-election audits (since $E(e_{t+1}) - c_t = \bar{e} - c_t$, irrespective of the outcome of the audit at time t). To test for these possibly heterogeneous patterns, we estimate

²⁹ The results are qualitatively and quantitatively similar irrespective of the inclusion of the future audit timing control. Estimates are available from the authors upon request.

dynamic panel models that include an interaction of the pre-election status of the audit report with the level of corruption reported in the audit at time t :

$$c_{m,t+1} = \theta_{P1}A_{mt} + \theta_{P2}A_{mt}c_{m,t} + \beta_1c_{m,t} + \beta_2X_{mt} + \gamma_{t+1} + \alpha_m + \varepsilon_{m,t+1}, \quad (7)$$

where all variables are defined as above. The θ_{P1} parameter estimate captures the effect of the pre-election audit on subsequent rent seeking activities given a favorable review in the period t audit (i.e., $c_{m,t} = 0$), whereas θ_{P2} captures the differential effect of the pre-election audit given a less favorable outcomes of the previous audit (i.e., an additional finding of corruption in the preceding audit). We will test whether $\theta_{P1} = \theta_{P2} = 0$.

Since equation (7) is a dynamic panel data model, it is well known that, even if c_{mt} and $\varepsilon_{m,t+1}$ are not correlated, for small t then estimation of the fixed effects model using either a within groups or a first differences estimator is not consistent (e.g., Nickell 1981, Arellano and Bond 1991). Specifically, taking first differences of equation (7):

$$c_{m,t+1} - c_{m,t} = \theta_{P1}(A_{m,t} - A_{m,t-1}) + \theta_{P2}(A_{m,t}c_{m,t} - A_{m,t-1}c_{m,t-1}) + \beta_1(c_{m,t} - c_{m,t-1}) + \beta_2(X_{m,t} - X_{m,t-1}) + (\gamma_{t+1} - \gamma_t) + (\varepsilon_{m,t+1} - \varepsilon_{m,t}), \quad (7')$$

and since $E[c_{m,t} \varepsilon_{m,t}] \neq 0$, the inclusion of the lagged dependent variable will generate bias in the OLS estimates of the coefficients of interest (θ_{P1} and θ_{P2}), even under the assumption that $A_{m,t}$ is strictly exogenous. We show in Appendix C that the OLS estimates of coefficients θ_{P1} and θ_{P2} in equation (7') are biased towards zero, in favor of finding the hypothesized relationship of interest.³⁰

An IV estimator for the first-differenced panel data model is based on the one first proposed by Anderson and Hsiao (1981, 1982). It uses the second lag of the dependent variable ($c_{m,t-2}$) and its interaction with the second lag of the audit variable ($A_{m,t-2}c_{m,t-2}$) – variables uncorrelated with the first-differenced error term – as IVs for ($A_{m,t}c_{m,t} - A_{m,t-1}c_{m,t-1}$) and ($c_{m,t} - c_{m,t-1}$), the variables that are correlated with the error term. Under the assumption of strict exogeneity of the audits and no serial correlation in the error terms, and given the robustness of the instruments, this IV estimator provides consistent estimates of coefficients θ_{P1} and θ_{P2} .

VI. Results

VI.A. Short-Run Accountability and Politician Selection Effects

³⁰ The OLS estimate of equation (11') leads to a downward bias in the coefficient θ_{P1} and an upward bias in the coefficient on θ_{P2} . That is, against finding the inverse relationship of interest – an increase in corruption as a response to a more favorable audit in the earlier period and a decrease in corruption as a response to a less favorable audit in the earlier period. See Appendix B for details.

We first present evidence of the short-run effects of the audit program on the corrupt behaviors of incumbent politicians and other municipal employees (Table II). Estimates of the average effects of the pre-election audit show a systematic reduction in the number of corrupt violations in the municipality. There are 1.43 (66 percent) fewer reported corrupt violations in the pre-election audit municipalities relative to those audited post-election (column 1). We also find 0.63 (67 percent) fewer corrupt violations per report by the mayor or vice-mayor (column 2), which suggests that there is a very limited (if any) shift in the corrupt violations charges – in reality or found by the auditors – between mayors and other municipality employees. This estimate suggests that the disciplining effects are not concentrated strictly among elected officials of the municipality. In particular, the estimated reductions are of similar magnitude (in proportional terms) across top management, rank and file employees, and unidentified municipality employees (not reported in the tables). We find comparable effects using a more stringent measure of corruption – the number of findings (per report) of misuse of public funds referred to the P.R. Department of Justice; the point estimate indicates 0.65 (66 percent) fewer violations per report among municipalities that were audited prior to the elections relative to those that were audited afterwards (column 3). Importantly, these relationships are stable and robust to controls (not reported) and to focusing on the subset of municipalities in which the incumbent runs for re-election (columns 6-7, 9).

We also examine whether the disciplining effects vary by the tenure of the politician, as suggested by the theory (specifications with interaction of pre-election audit and the number of terms in office of the politician). Although the point estimates suggest that higher tenure incumbents tend to be less disciplined by the pre-election audits, the estimated differential effects are small and statistically insignificant from zero (columns 3, 5). Again, these results are robust to focusing on municipalities in which the incumbent runs for re-election (columns 8, 10).

We now focus on the short-run effects of the audit program on electoral accountability at the municipality level – i.e., incumbent mayors' re-election rates. We start the discussion with a graphical analysis to shed light on the patterns in the data. Figure II depicts incumbent mayors' successful re-election rates as a function of the reported corrupt violations per report in the municipality, distinguishing between municipalities whose audit reports were published in the two-year period prior to the election (represented by a solid red line) and those whose reports were published in the two-year period following each election (represented by a dashed green line).³¹ Panel A is based on a measure of the mayor's successful re-election or otherwise (i.e. not run for re-election, or lose in primary or general election), whereas Panel B uses a measure of the incumbent's re-election rate conditional on running for re-election.

³¹ The reported differences between pre-election and post-election audit municipalities are regression-adjusted for election period fixed effects.

Incumbent mayors in municipalities whose reports were published pre-election exhibit a clear downward-sloping trend between successful re-election rates and the number of corrupt violations per report. Among the municipalities with no reported violations, re-election rates are 39 percent, and reduce consistently to approximately 20 percent among incumbent administrations charged with up to two violations (moderate corruption), and to 9 percent among administrations charged with more than two violations (high corruption). In contrast, the relationship among municipalities whose reports were published following the election is less stark; re-election rates are similar at 40 percent among those administrations with favorable audits, and decrease at a slower rate to 22 percent and 18 percent for administrations with moderate and high corruption levels, respectively. The relationship among incumbent mayors who run for re-election in the general election is even starker. It also shows no evidence of a reward for mayors receiving favorable or moderately unfavorable audits, but a large penalty of 17 percentage points among those administering municipalities with high levels of reported corruption. The contrast of these two relationships suggests that voters do care about corruption, and hold corrupt politicians accountable when informed. This evidence is consistent with previous work on municipal audit programs and electoral accountability, as shown by Ferraz and Finan (2008) for mayors in Brazil.

Parametric linear probability estimates of the reduced-form relationship following empirical model (4) capture the results depicted above (see Table III, columns 1-4). Although incumbent mayors' overall successful re-election rates are not significantly correlated with the number of corrupt violations among pre-election audit municipalities (column 1-2), the relationship is strongly negative among those incumbent mayors running for re-election in the general election. The point estimate indicates that the probability of a successful re-election is 6.0 percentage points (19 percent) lower for each additional finding per report (column 4). Overall, the estimated relationships support the hypothesis that information about corrupt violations induces an improvement in electoral accountability.

Finally, we present evidence on the effects of the audits on the selection of politicians in this election (see Table III, columns 5-8). Following the theoretical literature on politician's wages and politician selection, we use the elected mayor's household per capita earnings five years preceding the respective election year as a plausible measure of the competence of the politician elected into office. The point estimates of the average effects of the pre-election audit show a slight degree of positive selection of higher earnings politicians in the municipality, overall and among the subset of municipalities in which incumbent mayors ran for re-election (columns 1 and 3). These suggest that on average elected mayors following pre-election audits have earned an additional \$5,720 to \$6,680 USD per capita (13-15 percent), respectively; however, these effects are imprecisely estimated. However, there is a positive earnings-selection effect among municipalities with non-zero levels of corruption, as captured by parametric estimates of the reduced-form relationship following empirical model (7). The point estimate indicates

that those (re-)elected mayors have earned an additional \$11,780 USD per capita (27 percent) for each additional finding per report (column 2). The degree of earnings-based selection is similar among the subsample of municipalities where the incumbent runs for re-election, at \$10,910 USD per capita (25 percent) per additional violation (column 4). Overall, the estimates support the hypothesis that information about corrupt violations induces a degree of pre-incumbency earnings-based selection.

VI.B. Average Effects of the Audits on Subsequent Rent-Seeking Levels

We start by examining trends in the level of corruption for municipalities being audited in the two years before election period t , compared to those audited in the following two years. Figure III plots the average number of corrupt violations per report from audits one and two terms before election t , around the election in year t , and in the following audit. We show the trends separately for municipalities with a pre-election t audit (represented by the solid red line) and for municipalities with a post-election t audit (represented by the dashed green line). Panel A is based on the total number of violations per report in the audit, whereas Panel B uses only the number of violations attributed to the mayor or vice-mayor.

There are no discernible differences in the levels of reported corruption across these two groups of municipalities in earlier audits – the mean number of violations per report revolves around 1.6 and those attributed to the mayor or vice-mayor around 0.70 and the differences are statistically indistinguishable from zero. In contrast, for audits around election t there is a stark decrease of 1.37 (= $|0.80 - 2.17|$) violations per report among municipalities facing a pre-election audit, relative to those facing a post-election audit. A similar pattern holds for the number of violations attributed to the municipality-level executives ($0.64 = |0.31 - 0.95|$). This difference is consistent with the regression-based results above showing a substantial short-run disciplining effect by incumbents facing greater scrutiny (see Section VI.A). Finally, comparing these groups of municipalities in the next round of audits (around election $t+4$), we find that the difference in corruption levels decreases substantially to 0.37 (= $|1.25 - 1.62|$) violations per report and is statistically indistinguishable from zero. Again, we find a similar pattern for the number of violations by the municipal executives during this later audit ($0.18 = |0.54 - 0.72|$). The graphical evidence strongly suggests that the disciplining effects of the pre-election audits are short-lived.

Parametric estimates of the longer-run effects of the pre-election audits on the number of corrupt violations in the subsequent term allow us to formally test for these effects (Table IV). The point estimate from the average effects model (equation [6]) with municipality and election-specific intercepts (as well as municipality and mayor controls) indicates a (statistically insignificant) decrease of 0.16 of a corrupt violation per report (10 percent) among the pre-election audit municipalities (column 1). The relationship remains unchanged when focusing on the number of violations by the mayor or vice-mayor (column 2).

The point estimate from this specification implies a small decrease in rent-seeking of 0.09 of a violation (13 percent). Using the more stringent measure of corruption – the number of findings referred to the Department of Justice – gives even starker results (column 3). The point estimate implies no difference in the number of violations. Moreover, the results are robust to examining the sub-sample of municipalities in which the incumbent ran for re-election at time t (columns 4-6). In fact, the point estimates from these specifications suggest an increase in corruption, although these are statistically indistinguishable from zero. The specification using the number of “DoJ-referred” findings suggests an increase in approximately one third of a violation; moreover, we can reject a decrease in corruption greater than 0.065 violations (12 percent) with 95 percent confidence.³²

We also examine whether there is heterogeneity in the relationship between the audits and longer-term levels of corruption levels, based on characteristics of the municipality or of the (originally incumbent) mayor (Table V). Consistent with the theory, the next period’s effects of the audits induce greater discipline among incumbents with fewer terms in office than among those with more experience, as these mayors are more likely to respond to the auditing scheme to build up their reputation (columns 1-3). We also expect the following term’s levels of corruption to be lower in jurisdictions where competition from challengers is stark (e.g., a sufficiently high initial reputation level μ). Although we cannot observe the baseline reputation of opposition candidates, we operationalize this idea by examining jurisdictions where the incumbent won in the previous election (period $t - 4$) with a varying win margin (columns 4-6). Although imprecisely estimated, our results are consistent with this idea – in competitive jurisdictions, we observe moderately lower levels of corruption in future periods, and this long-term discipline effect is decreasing in the degree of lop-sidedness of the jurisdiction. Finally, we test whether the disciplining effects are more likely to take place in jurisdictions with a higher proportion of well-educated individuals – we interact the pre-election audit with the proportion of adults with a university (or post-graduate) degree in the municipality as measured in the preceding census (columns 7-9). Again, our results are consistent with the idea that a more-educated populace is better able to select and/or discipline politicians in the long term – although these patterns are somewhat imprecisely estimated.

In summary, we conclude that the disciplining effects of the pre-election audits are short-lived. This is consistent with the idea that politicians in power in the next term will engage on average in the same level of rent-seeking after an audited period than after a non-audited period, because their increased reputation for competence allows them to engage in greater rent-seeking. Thus in the next sub-section, we

³² We generally have sufficient precision to reject moderately sized reductions in the number of violations. For instance, the lower 95 percent confidence interval from specification (4) is -0.48; that is, a 32 percent reduction in the number of violations. The results are robust to exploring the extensive margin only – indicator variables for whether there is reported corruption in the audit. Estimates are available in Appendix Table A1.

examine other predictions of our theory to evaluate in more detail whether these dynamic incentives are playing a role.

VI.C. Dynamic Effects of the Audits on Subsequent Rent-Seeking Levels

In this section we present preliminary graphical evidence on the short-term reported and the subsequent corruption levels; that is, the relationship between reported rent seeking levels over time for municipalities audited before and after the election at time t . Figure IV depicts this relationship: residual corruption levels after removing municipality and election fixed effects for municipalities audited preceding an election, relative to those whose audit reports were published following the election.

These figures provide some suggestive evidence of the model's predictions on dynamics of corruption. First, the (expected) dissemination of the audit reports preceding an election decrease the short-run rent seeking behaviors of incumbent politicians in the municipalities, as the distribution of audit findings is compressed towards observing fewer corrupt violations. Second, there is no significant difference in reported rent seeking in the subsequent audit after an audited period than after a non-audited period, as the average of the residuals is approximately the same for municipal governments that experienced an audit in the preceding term and those that did not.

Finally, there is no statistically significant relationship between unfavorable audit outcomes in a given audit and those in the subsequent audit among local governments that were not audited preceding the first election. Interestingly, this is also the case among governments that were audited preceding the first election. This is *prima facie* evidence that the change in corruption level is simply going to be the difference between the short-term corruption level and the expected level of corruption. The following analysis provides formal tests of this prediction.

IV estimates from empirical models of equation (7') show that there is a significant increase in corruption among municipalities with no reported findings of corruption in the previous audit (Table VI). The point estimate of θ_{p1} from a specification with a linear interaction term in the preceding audit findings (and using all violations as dependent variable) indicates that the overall number of corrupt violations in the subsequent audit decrease by 0.06 of a violation among municipalities with a favorable outcome of the previous audit (column 2). Moreover, the estimate of θ_{p2} implies a relative decrease of 0.06 of a corrupt finding per each additional finding of corruption per report in the preceding audit. These estimates are small and statistically indistinguishable from zero. The resulting total effect given non-zero corrupt findings in the preceding audit are *also* small and not significantly indistinguishable from zero at conventional confidence levels. The same result holds when we focus on the outcomes for mayors with limited experience (those in their first term during the first audit – a specification with an interaction of the pre-election (t) audit and the incumbent's terms in office in period t) (column 3).

Since the theory makes predictions regarding the corrupt activities of the municipal government executive, we estimate specifications using as dependent variable the number of corrupt violations by the mayor or vice-mayor in the subsequent audit (columns 4-7). We also allow for heterogeneous responses by the number of violations attributed to the municipal executive (i.e., mayor or vice-mayor) versus other municipal employees (columns 6-7). The relationship remains unchanged when focusing on the number of violations by the mayor or vice-mayor, or using the more as our outcome measure the number of findings referred to the Department of Justice (columns 8-10). Moreover, the estimates from the sub-sample of municipalities in which mayors ran for re-election at time t following the initial audit are very similar (see Appendix Table II).³³

In sum, these robustness checks further indicate that reputation-dependent performance strategies are quite prevalent among incumbent politicians, allowing them to engage in greater rent seeking while in office. Thus in the next sub-section, we examine the final predictions of the theory regarding the dynamic/longer-term consequences for incumbents' re-election prospects.

VI.D. Effects of the Audits on Long-Term Electoral Performance

As the theory suggests, conducting an audit enables voters to re-elect normal politicians, and normal politicians are more likely to do well enough to get re-elected in subsequent periods – there is a selection effect on re-election rates. Moreover, although higher reputation implies lower effort by the incumbent, in equilibrium voter re-election thresholds (r) are lower and thus easier to meet. Thus, both selection and sanctioning effects influence future period re-election rates. Therefore, a final test of the model is that incumbents in the next term whose municipalities experienced a pre-election audit at time t , will be expected, on average, to have higher re-election rates in the following election (at time $t+4$).

We conduct a graphical analysis analogous to that for the incumbents' short-term electoral accountability to shed light on the patterns in the data. Figure V depicts the next incumbent mayors' successful re-election rates (at time $t+4$) as a function of the reported corrupt violations per report in the municipality at time t , distinguishing between municipalities whose audit reports were published in the two-year period prior to the election (represented by a solid red line) and those whose reports were published in the two-year period following each election (represented by a dashed green line).³⁴ Panel A is based on a measure of the mayor's successful re-election or otherwise (i.e. not run for re-election, or lose in primary or general election), whereas Panel B uses a measure of the incumbent's re-election rate conditional on running for re-election.

³³ We also generate OLS estimates of equation (7') for purposes of comparing these with the analogous IV estimates. Consistent with the hypothesized direction of bias due to the nature of the dynamic panel model, the OLS estimate of coefficient θ_{p1} is smaller than its IV analog, whereas that of coefficient θ_{p2} is larger (estimates available upon request).

³⁴ Again, the reported differences between pre-election and post-election audit municipalities are regression-adjusted for election period fixed effects.

Incumbent mayors in municipalities whose reports were published pre-election exhibit a clear downward-sloping trend between successful re-election rates and the number of corrupt violations per report. In stark contrast, there is no relationship among municipalities whose reports were published following the election. The graph shows evidence of a reward for mayors receiving favorable audits or audits with moderate corruption levels at time t , in spite of their substantial increases in corruption in the following audit. The difference among those municipalities with low (zero reported) corruption suggests a 24 percentage point ($= |0.44 - 0.20|$) higher longer-term re-election rate of the incumbent, whereas among those with moderate corruption, the next incumbent experiences an electoral reward of 9 percentage points ($= |0.28 - 0.19|$). Interestingly, there is still a large penalty of 17 percentage points ($= |0.09 - 0.26|$) among those incumbents in municipalities with high levels of reported corruption at time t , as there is no modification in the corrupt actions among this group. This evidence strongly suggests that, if not indifferent, voters reward incumbents in spite of their increased rent-seeking in later periods.

Parametric linear probability estimates of the reduced-form relationship again capture the results depicted above (Table VII). Although incumbent mayors' overall successful re-election rates (in election $t+4$) are not significantly correlated with the number of corrupt violations among pre-election audit municipalities (columns 1-4), the estimates among mayors who run for re-election in the next term experience a positive shift in their electoral performance (columns 5-8). Overall, these estimated relationships support the hypothesis that information about corrupt violations induces an improvement in electoral accountability in the short-run, while having perverse incentives in the longer-run.

VII. Testing for Alternative Explanations

Manipulation of audits: The validity of our research design relies on three important conditions: (i) the exogenous timing of the audits, (ii) the fixed timing of municipal elections, and (iii) the comparability of the audit process across municipalities and across time. Even though we have shown that the timing of the audits is uncorrelated with observable characteristics of the municipality, one potential concern could lie in the actual audit process. Specifically, if the audits conducted in the two-year period before elections differed systematically from those conducted after elections, then our empirical strategy would be invalidated. An example of this type of concern is that the auditors themselves might have been corrupted. We thus follow Ferraz and Finan (2008) and assess multiple reasons for potential biases in the actual audit processes.

If the actual initial audits were manipulated, then we might expect mayors who were politically affiliated with the party in power in the state government or with the party who appointed the Comptroller to receive more favorable audit reports. To assess this possibility, we estimate specifications that include as controls indicator variables for the incumbent being from a political party (i) in the opposition to the

incumbent governor and (ii) in the opposition to the party of the governor who appointed the Comptroller, as well as their interaction with the pre-election audit indicator (Table VIII—columns 1, 3, 5). Including these additional controls do not affect the main responses of the pre-election audits on the level of reported violations. Moreover, estimates of these heterogeneous responses suggest that municipalities in the opposition to the party of the governor who appointed the Comptroller receive more favorable audit report outcomes, placating this concern.

Another possibility previously raised in the literature is that incumbents who won by narrow margins in the previous election have a greater incentive to bribe OCPR auditors to receive more favorable reports. To examine this threat to validity, we extend the baseline model to control for the incumbent’s margin of victory in the previous election and its interaction with the pre-election audit indicator (Table VIII, columns 2, 4, 6). Again, we do not find substantial evidence that a mayor’s previous level of political support influenced the audit process and including these additional controls do not affect the main short-run responses of the pre-election audits.

We also evaluate whether the extent of subsequent auditing varied significantly across municipalities of different types. To do so, we estimate specifications using as dependent variables (i) an indicator for the existence of a subsequent audit report, and (ii) the number of reports from the subsequent audit (Table VIII, columns 7-12). The estimates indicate no evidence of selective auditing, or of differential intensity of auditing, as measured by the number of reports.³⁵

Finally, we examine whether there is evidence of manipulation of the next term audits, again based on the ideas that (i) mayors who were politically affiliated with the party in power in the state government or (ii) with the party who appointed the Comptroller, or (iii) who won by narrow margins in the previous election, to receive more favorable audit reports. To assess this possibility, we estimate analogous longer-term effects specifications that include the same control variables as discussed above. (Table IX). Again, we find that including these additional controls do not affect the main responses of the pre-election audits on the level of reported violations in the future term.³⁶

Political cycles: A second concern is that political cycles are potentially correlated with our comparison of municipalities based on the timing of the audits. Municipalities receiving pre-election audits do cover time periods farther away from the election relative to those receiving later audits (see Table I, Panel A), which could affect the comparability of the audit outcomes across these groups. We examine whether this issue affects our results by controlling for the actual timing of the audited periods (i.e., the start of the

³⁵ Analogous estimates for the sample of municipalities in which mayors choose to run for re-election in period t are available in Appendix Table III.

³⁶ We also carry out placebo tests, showing that the pre-election audit effects are uncorrelated with predetermined characteristics of the municipality, such as the preceding election win margin. Estimates are available from the authors upon request.

audit period, and the time span of the audit period), and it influences none of our results (not reported in the tables – these are available from the authors upon request).

Transfers from Central Government: It is plausible that the central government may have increased the level of transfers to municipalities after favorable audits (and reduced the flow of funds to municipalities after instances of corruption were exposed in those jurisdictions) (Brollo 2010). If voters reward politicians for obtaining more resources from higher levels of government, an increase in transfers by the central government could provide an incumbency advantage to the mayor, allowing him to engage in rent seeking activities in the future with lower risk of removal from office.³⁷

To examine this hypothesis, we use the data on municipal government income statements, which provides us with the following additional revenue information: property tax, licensing, waste disposal services, transfers and other government revenues. We estimate the relationship between the pre-election audits and the fiscal year-specific revenues by source. To the extent that the available data allows us to assess this alternate explanation, we find no evidence of this channel in the data, at least in this context (estimates available upon request).

Mayor's Political Experience: If engaging in corrupt practices involves learning (by doing) or if it takes time to establish the networks that enable individuals to engage in corrupt practices, then the increase in corruption in municipalities could be the result of having more experienced mayors in office in a future term. On the other hand, experience could allow mayors to learn to engage in corrupt practices while reducing the likelihood of getting caught, leading to a downward bias in the estimated increases in corrupt practices in municipalities with previously favorable (pre-election) audits. In any case, note that because short-run re-election rates do not differ among municipalities with favorable pre-election vs. post-election audits, there is no prima facie evidence of selection based on experience, on average. Therefore, to the extent that the available re-election data allows us to assess this explanation, the evidence is inconsistent with mayor experience driving our results.

Strategic Challenger Entry: Is the reputation building that may take place simply a result of the observed performance of incumbent politicians, or do strategic actions by a more diverse group of agents in the political sphere (i.e., competing parties) can help inform voters about the characteristics of candidates in competition? We believe that these additional strategic interactions compound the effects discussed in the paper. For instance, political parties can strategically choose to field candidates as a response to information voters receive about the corrupt violations by incumbents. Distinguishing the relative magnitudes of the incumbent's own reputation from these additional interactions remains important work.

³⁷ For evidence on the electoral consequences of fiscal transfers from higher levels of government, see Brollo et al. (2010) and Litschig and Morrison (2009).

VIII. Conclusion

The central goal of this paper is to study the *long-run* corruption consequences caused by the disclosure to voters of information about politicians' corrupt actions. We develop a model of political agency and reputation building, and show that a politician whose reputation has improved in the past can exploit these information asymmetries to engage in rent-seeking activities, leaving voters indifferent between re-electing him and electing an unknown challenger. Given these perverse reputation incentives, re-elected mayors who have been shown to have refrained from or engaged in rent-seeking activities in the past will be on average *as corrupt* in the next term as mayors whose levels of corruption have not been exposed. We then use unique longitudinal data on municipal government audits in Puerto Rico to study this relationship empirically. We find that audits lead to a significant short-term reduction in municipal corruption, as well as an increase in incumbent mayors' electoral accountability. However, municipal corruption levels in the subsequent round of audits are on average *the same* in municipalities audited preceding the previous election and those whose audits became publicly available afterwards. Thus, short-term information dissemination policies do not necessarily align politicians' long-term actions with voter preferences as politicians exploit their reputational gains by extracting additional rents from office.

Our paper contributes to the ongoing debate regarding the nature of the differences among politicians, and the type of qualities that voters evaluate in their representatives (see, for instance, Fearon 1999 and Besley 2005). One view is that some politicians are virtuous or honest and will do all they can to serve voters, while others are opportunistic and seek office primarily to extract rents from office. Another, possibly complementary, view holds that all politicians are opportunistic but differ in their ability or competence. The two positions have different implications for public policy as well as for our understanding of democracy. If we believe that some politicians are virtuous, we must also believe that policies that enable voters to evaluate politicians' character can be just as effective as those which help voters evaluate their policies and rent-seeking activities. Furthermore, in this case, helping voters better select their politicians will have long-lasting effects on the quality of government as virtuous politicians will continue to govern well even when they have no signaling motive. On the other hand, if politicians differ mostly in their competence, the most effective policies are those that provide information to voters about incumbents' actions in office, and the effects of these policies will be short-lived as opportunistic politicians take advantage of situations in which voters have less information about their actions in office. Our results provide strong, if context-specific, evidence for the second view. We find that, although the informational content of audits lowers corruption in the short-term and enables voters to re-elect better politicians, audits have no lasting effect on the level of corruption.

Finally, our work follows the view that corrupt behavior is a choice made by policymakers and is a rational response to the structure of the political-economic environment, such as political institutions and (the inadequacy of) information (Pande 2007). It does not exclude, however, the possibility that the rational behavior of politicians in democratic governments can generate or perpetuate “norms” or “cultures” of corruption, as it can induce citizens to have “self-fulfilling prophecies” regarding the corrupt behavior of politicians. While institutional innovations such as audit programs can improve voter welfare – and the theoretical and empirical results that we present are consistent with voters taking full advantage of information in pre-election audits – it is possible for a society to remain in a sub-optimal equilibrium in which these innovations are ineffectual. This speaks to the debate in the literature on governance and political corruption on whether corruption is a social norm or habit that is pervasive in low- and middle-income countries, or whether it strictly responds to structure. These general queries regarding the determinants of good governance remain important questions for future research.

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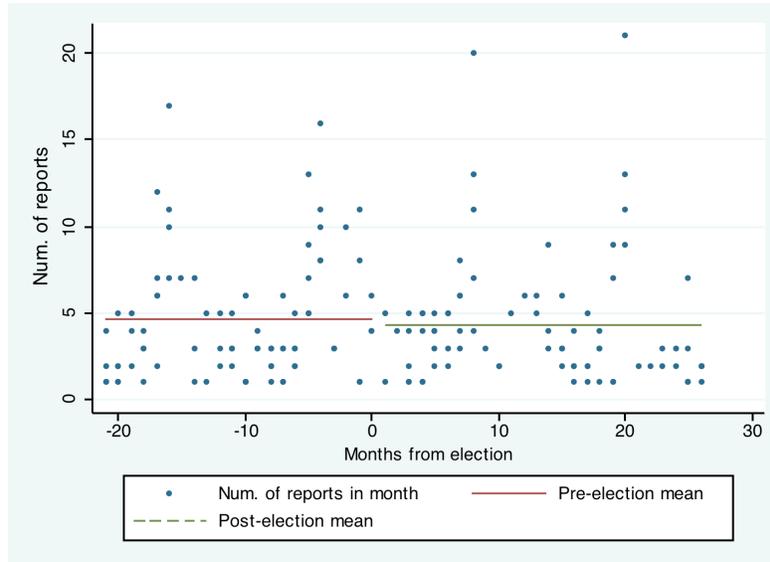
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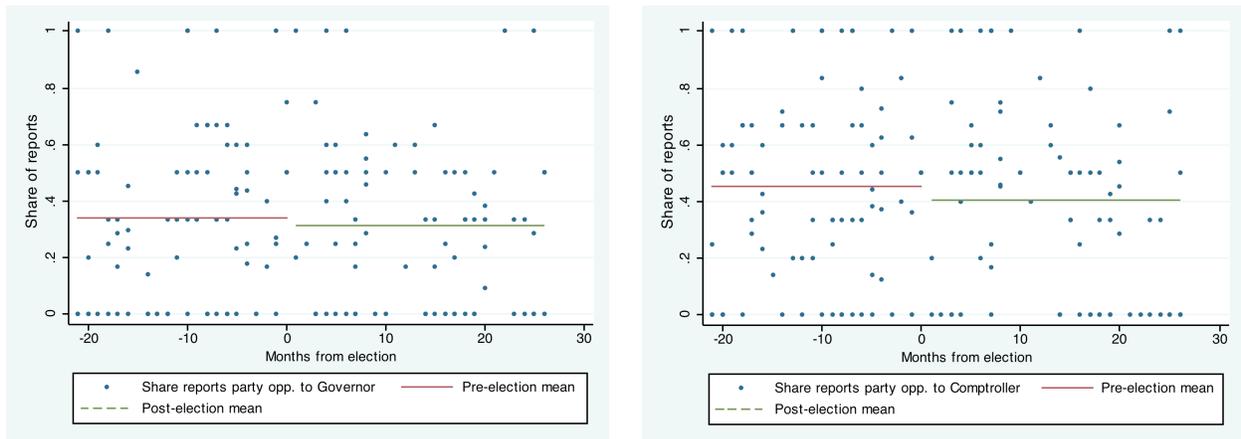
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FIGURE I: TIMING OF PUBLICATION OF AUDIT REPORTS, 1985-2005

PANEL A: NUMBER OF REPORTS



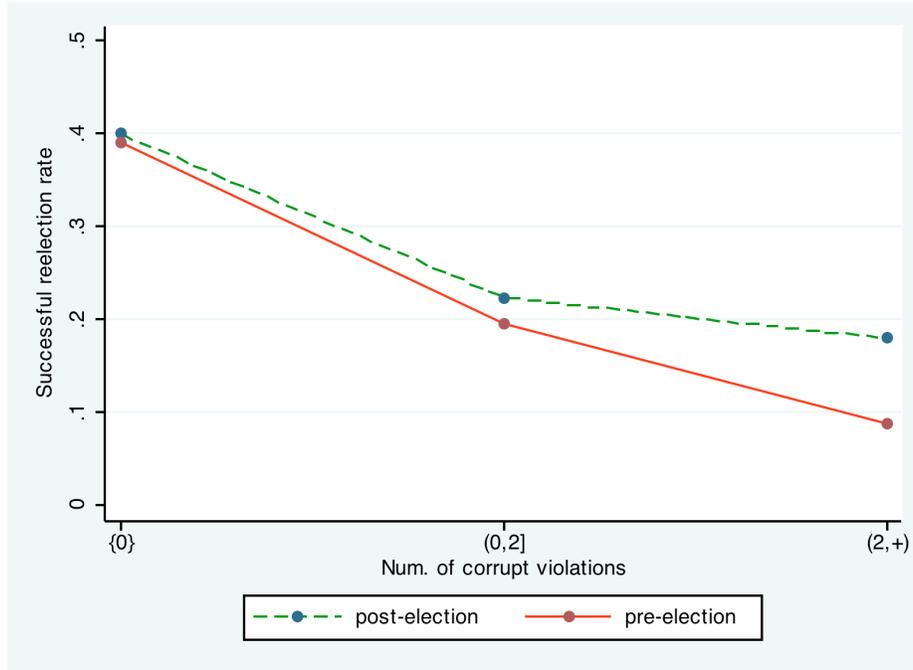
PANEL B: SHARE OF ALL REPORTS ON MUNICIPALITIES WHERE INCUMBENT IS FROM OPPOSITION PARTY TO GOVERNOR / COMPTROLLER



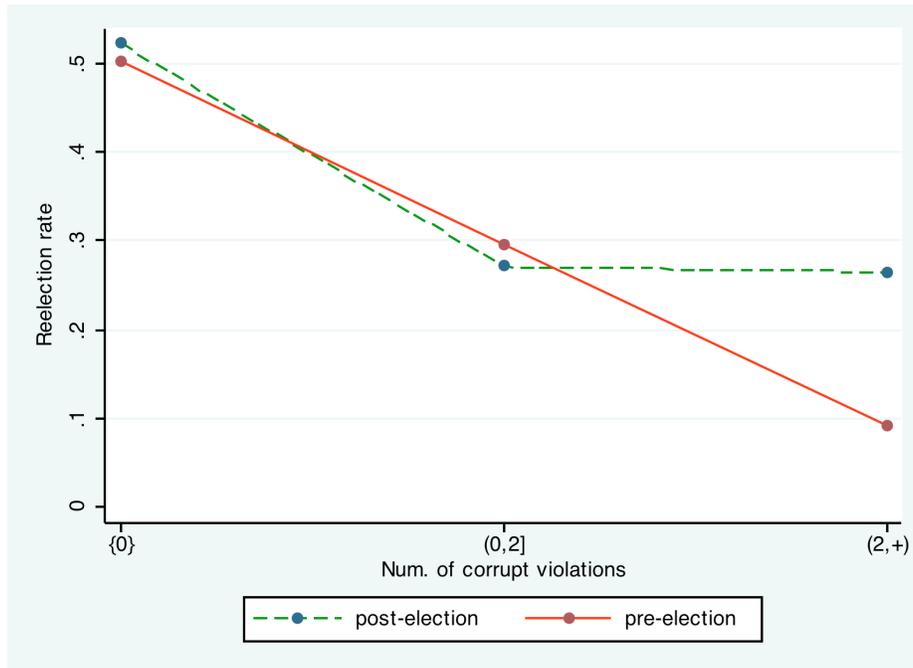
Notes: Panel A shows the timing of release of the number of reports by month in the four-year period around each election (in Nov. 1988, 1992, 1996, 2000, and 2004). Panel B presents the share of published reports of municipalities in which the incumbent is in the opposition party to the Governor in office or to the Governor who appointed the Comptroller in office, in each month. The red line in each figure demarcates the mean for the 22 months before the November election; the green lines demarcate the mean for the 26 months following an election.

FIGURE II:
 RELATIONSHIP BETWEEN REPORTED CORRUPTION LEVELS AND ELECTORAL
 ACCOUNTABILITY FOR MUNICIPALITIES AUDITED BEFORE AND AFTER ELECTIONS

PANEL A: INCUMBENT RUNS FOR & WIN RE-ELECTION



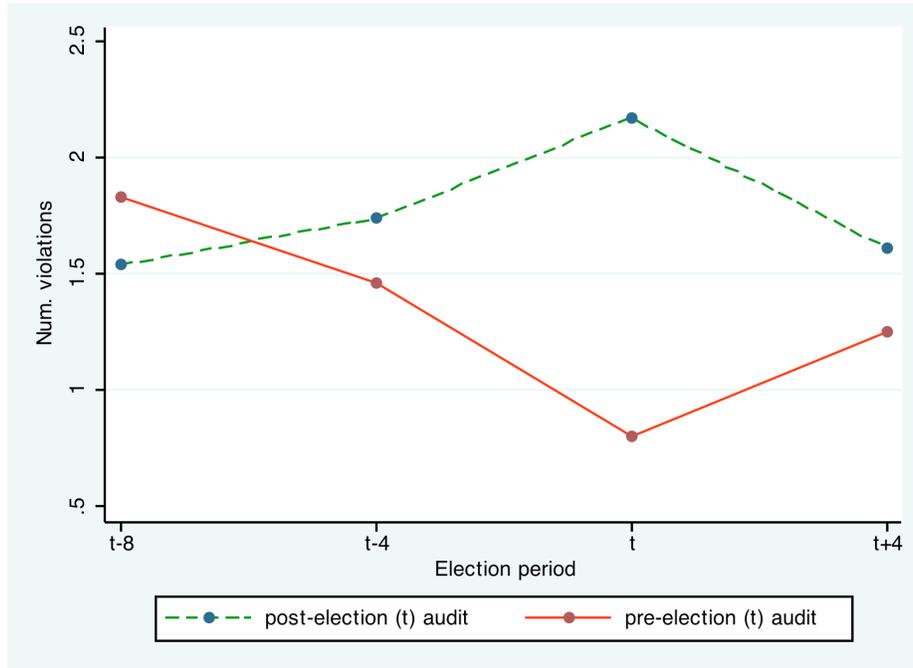
PANEL B: INCUMBENT WINS RE-ELECTION | RUNNING



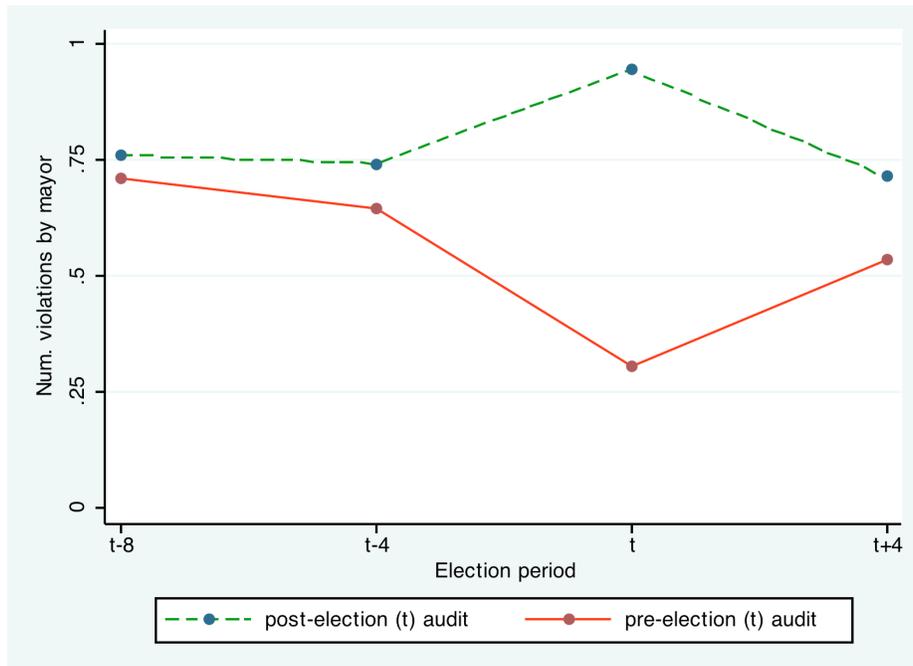
Notes: The figures show the adjusted (by election intercepts) relationship between the mayors who were successfully re-elected in the election and the number of corrupt violations per report in the audits for municipalities audited before and after the elections.

FIGURE III:
 NUMBER OF VIOLATIONS ACROSS TIME, BY PRE-ELECTION AUDIT IN ELECTION (t)

PANEL A: ALL VIOLATIONS



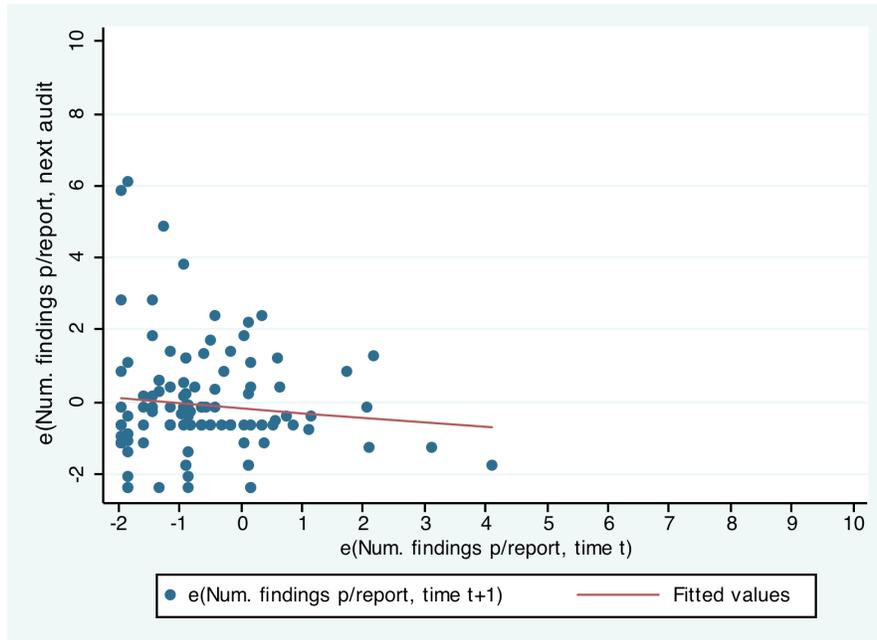
PANEL B: VIOLATIONS BY MAYOR OR VICE-MAYOR



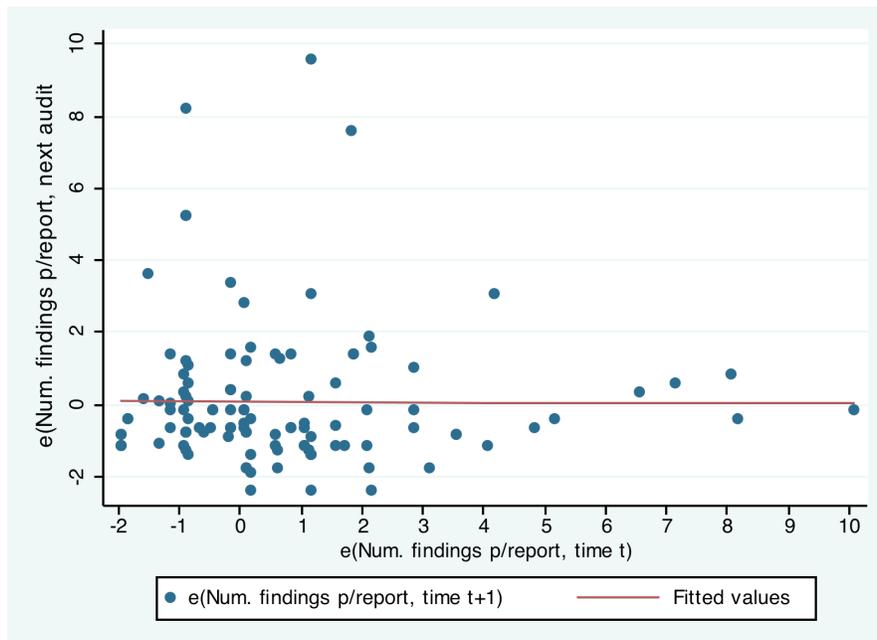
Notes: The figures show the unadjusted relationship between the number of corrupt violations per report in each audit, for municipalities audited before and after the election at time (t).

FIGURE IV:
RELATIONSHIP BETWEEN REPORTED CORRUPTION LEVELS AND SUBSEQUENT
CORRUPTION LEVELS FOR MUNICIPALITIES AUDITED BEFORE AND AFTER ELECTIONS

PANEL A: PRE-ELECTION AUDITS



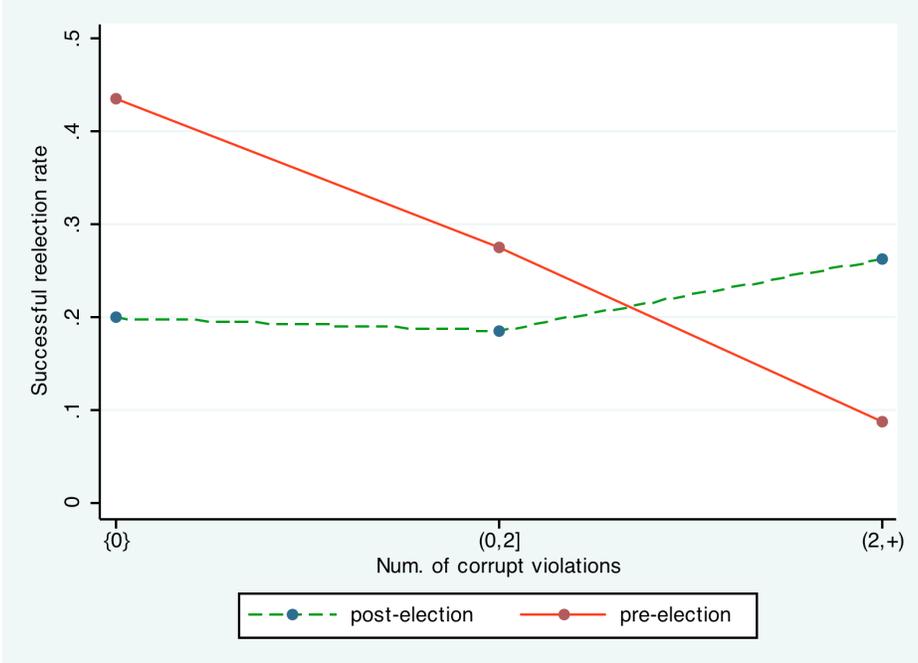
PANEL B: POST-ELECTION AUDITS



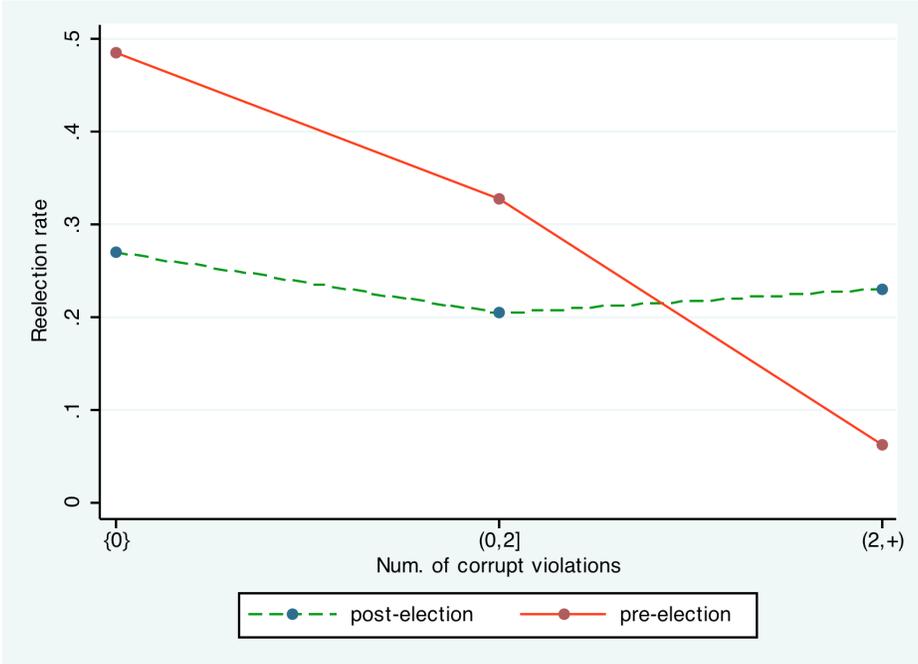
Notes: The figures plot residuals from a set of regressions of the number of findings of corruption per report in periods t and $t+1$, on election and municipality fixed effects.

FIGURE V: RELATIONSHIP BETWEEN REPORTED CORRUPTION LEVELS AND LONG-TERM ELECTORAL ACCOUNTABILITY (IN ELECTION AT TIME [t+4]) (FOR MUNICIPALITIES AUDITED BEFORE AND AFTER ELECTION (AT TIME [t]))

PANEL A: INCUMBENT RUNS FOR & WINS RE-ELECTION



PANEL B: INCUMBENT WINS RE-ELECTION | RUNNING



Notes: The figures show the adjusted (by election intercepts) relationship between the mayors who were successfully re-elected in election at time (t+4) and the number of corrupt violations per report in the audits for municipalities audited before and after election at time (t).

TABLE I: CHARACTERISTICS OF THE MUNICIPALITIES

Sample	All Municipalities					Municipalities with subsequent audit				
	All (1)	Pre-election audit (2)	Post-election audit (3)	Difference (Adjusted) (4)	N (5)	All (6)	Pre-election audit (7)	Post-election audit (8)	Difference (Adjusted) (9)	N (10)
<u>Panel A: Audit reports</u>										
Number of all corrupt violations per report	1.38 [1.70]	0.80 [0.99]	2.17 [2.26]	-1.24 (0.19)	326	1.58 [1.89]	0.93 [1.07]	2.24 [2.28]	-1.30 (0.24)	232
Number of violations by mayor/vice-mayor	0.58 [0.99]	0.31 [0.57]	0.95 [1.29]	-0.58 (0.10)	326	0.68 [1.12]	0.33 [0.62]	1.04 [1.40]	-0.70 (0.13)	232
Number of violations referred to Dept. of Justice	0.65 [1.20]	0.40 [0.81]	0.99 [1.52]	-0.58 (0.14)	326	0.74 [1.28]	0.50 [0.91]	1.00 [1.53]	-0.45 (0.17)	232
Number of audit reports	1.88 [1.15]	2.16 [1.32]	1.49 [0.72]	0.69 (0.10)	326	1.88 [1.22]	2.30 [1.47]	1.46 [0.68]	0.71 (0.13)	232
Start of audit period in reports (years from electi.	6.31 [2.59]	6.84 [2.70]	5.59 [2.25]	1.61 (0.22)	326	6.84 [2.66]	7.85 [2.58]	5.80 [2.33]	2.59 (0.25)	232
Time span of audited period (years)	5.08 [2.59]	5.29 [2.76]	4.79 [2.32]	0.98 (0.21)	326	5.71 [2.52]	6.40 [2.46]	5.00 [2.39]	1.67 (0.26)	232
<u>Panel B: Electoral outcomes</u>										
Incumbent runs for re-election (1/0)	0.74 [0.44]	0.72 [0.45]	0.76 [0.43]	-0.04 (0.05)	326	0.75 [0.44]	0.69 [0.46]	0.80 [0.40]	-0.13 (0.05)	232
Incumbent mayor wins running (1/0)	0.35 [0.48]	0.37 [0.48]	0.32 [0.47]	0.00 (0.07)	241	0.32 [0.47]	0.28 [0.45]	0.35 [0.48]	-0.02 (0.08)	173
Incumbent party wins (1/0)	0.33 [0.47]	0.35 [0.48]	0.31 [0.46]	0.01 (0.06)	326	0.33 [0.47]	0.31 [0.46]	0.35 [0.48]	-0.01 (0.07)	232
<u>Panel C: Pre-audit incumbent mayor characteristics</u>										
Mayor, member of PNP	0.51 [0.50]	0.52 [0.50]	0.49 [0.50]	0.03 (0.06)	326	0.52 [0.50]	0.56 [0.50]	0.47 [0.50]	0.03 (0.07)	232
Member of opposition party to Governor	0.33 [0.47]	0.35 [0.48]	0.30 [0.46]	0.05 (0.05)	326	0.30 [0.46]	0.31 [0.46]	0.30 [0.46]	0.00 (0.06)	232
Member of opp. party to Gov. appointing Comptroller	0.44 [0.50]	0.47 [0.50]	0.41 [0.49]	0.09 (0.06)	326	0.42 [0.50]	0.42 [0.50]	0.43 [0.50]	-0.02 (0.06)	232
Terms in office	2.15 [1.24]	2.19 [1.36]	2.09 [1.07]	0.10 (0.14)	326	2.15 [1.18]	2.17 [1.33]	2.12 [1.01]	0.04 (0.15)	232
Mayor's win margin in previous election	0.11 [0.09]	0.11 [0.09]	0.10 [0.09]	0.01 (0.01)	326	0.11 [0.09]	0.11 [0.09]	0.10 [0.10]	0.01 (0.01)	232

TABLE I: CHARACTERISTICS OF THE MUNICIPALITIES (CONT'D)

Sample	All Municipalities				Municipalities with subsequent audit					
	All (1)	Pre-election audit (2)	Post-election audit (3)	Difference (Adjusted) (4)	N (5)	All (6)	Pre-election audit (7)	Post-election audit (8)	Difference (Adjusted) (9)	N (10)
<u>Panel D:</u> Pre-audit municipality characteristics										
High school education or more (%)	0.46 [0.09]	0.48 [0.09]	0.45 [0.08]	0.025 (0.010)	326	0.44 [0.08]	0.45 [0.08]	0.44 [0.07]	0.011 (0.011)	232
College or more (%)	0.11 [0.04]	0.12 [0.04]	0.11 [0.05]	0.010 (0.006)	326	0.10 [0.04]	0.11 [0.04]	0.10 [0.04]	0.009 (0.006)	232
Household median income (1,000 USD)	9.17 [2.84]	9.65 [3.05]	8.51 [2.40]	0.885 (0.342)	326	8.25 [1.77]	8.34 [1.84]	8.16 [1.70]	0.203 (0.257)	232
Poverty rate	0.60 [0.10]	0.58 [0.10]	0.61 [0.09]	-0.024 (0.012)	326	0.61 [0.09]	0.60 [0.10]	0.62 [0.09]	-0.015 (0.013)	232
<u>Panel E:</u> Other municipality characteristics										
Government budget	10.06 [18.22]	11.61 [21.77]	7.63 [10.2]	3.05 (2.44)	266	9.31 [15.42]	11.39 [19.29]	6.93 [8.79]	4.16 (2.41)	188
Capital improvements	2.28 [4.35]	0.24 [0.44]	0.21 [0.43]	0.05 (0.07)	265	0.25 [0.47]	0.29 [0.48]	0.20 [0.45]	0.08 (0.08)	188
Salaries & benefits	4.96 [7.44]	5.48 [8.59]	4.15 [5.14]	1.25 (1.11)	265	4.97 [8.23]	5.95 [10.34]	3.85 [4.63]	1.97 (1.25)	188
Social assistance	2.33 [3.91]	0.22 [0.34]	0.26 [0.46]	-0.01 (0.06)	265	0.27 [0.45]	0.27 [0.40]	0.28 [0.49]	0.01 (0.07)	188
Other expenditures	3.94 [6.64]	4.54 [7.62]	3.02 [4.62]	1.28 (0.99)	265	3.82 [6.98]	4.89 [8.84]	2.60 [3.62]	2.10 (1.12)	188
Property tax	3.22 [6.75]	3.70 [7.79]	2.47 [4.65]	1.13 (1.08)	265	3.25 [7.26]	4.15 [9.06]	2.23 [4.24]	1.74 (1.11)	188
Licensing	2.00 [4.08]	2.30 [4.50]	1.53 [3.28]	0.60 (0.67)	265	1.92 [4.27]	2.46 [5.19]	1.31 [2.80]	1.03 (0.64)	188
Waste disposal	0.20 [0.72]	0.25 [0.89]	0.11 [0.33]	0.14 (0.12)	265	0.20 [0.71]	0.29 [0.92]	0.10 [0.30]	0.17 (0.14)	188
Transfers & other revenue	3.11 [3.98]	3.28 [4.71]	2.85 [2.46]	0.44 (0.35)	273	3.13 [4.46]	3.57 [5.90]	2.65 [1.72]	0.91 (0.68)	193
Unemployment rate	0.157 [0.051]	0.150 [0.046]	0.168 [0.057]	-0.011 (0.007)	273	0.161 [0.052]	0.153 [0.047]	0.171 [0.056]	-0.014 (0.008)	193

Notes: Standard deviations of variables are reported in parentheses. Differences estimated in OLS regression models, regression-adjusted for electoral term fixed effects. Robust standard errors of mean differences are reported in parentheses. The sample is composed of all municipalities that had a first audit during 1987-20

TABLE II: EFFECTS OF THE (TIMING OF) THE AUDITS ON THE NUMBER OF CORRUPT VIOLATIONS IN CURRENT AUDIT

Sample	Dependent variables:									
	Number of corrupt violations					Number of corrupt violations				
	All	by Mayor / Vice-mayor	All Municipalities	Referred to Dept. of Justice	All	by Mayor / Vice-mayor	All	by Mayor / Vice-mayor	Referred to Dept. of Justice	Mayors who run for re-election
	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)	OLS (7)	OLS (8)	OLS (9)	OLS (10)
Pre-election audit	-1.43*** (0.22)	-0.63*** (0.13)	-0.77** (0.19)	-0.65*** (0.16)	-0.71** (0.21)	-1.32*** (0.30)	-0.57*** (0.16)	-0.71** (0.18)	-0.54*** (0.20)	-0.54 (0.28)
Pre-election audit × Terms in office		0.13 (0.12)		0.06 (0.12)				0.10 (0.10)		-0.01 (0.13)
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election Year & Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	326	326	326	326	326	241	241	241	241	241
Mean of dep. variable (controls)	2.17	0.95	0.95	0.99	0.99	2.07	0.85	0.85	0.89	0.89

Notes: Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at (*) 90%; (**) 95%; (***) 99% confidence levels, respectively. Controls are the number of municipality government reports, the number of municipal public corporation or consortium reports; indicators for New Progressive Party membership, for incumbent in the opposition party to the state-level executive government, and for incumbent in the opposition party to the governor who appointed Comptroller; the vote share for the incumbent in the previous election (t-4); and the incumbent's number of terms in office (at time t). The sample is composed of all municipalities that had a first audit during 1987-2002.

TABLE III: EFFECTS OF THE (TIMING OF) THE AUDITS ON SHORT-TERM ELECTORAL OUTCOMES, POLITICIAN SELECTION

Sample	Dependent variables:															
	Incumbent runs for & wins re-election		Incumbent wins re-election running		Elected mayor's earnings (000's) (5 years before election) [2000 and 2004 elections]		Mayors who run for re-election									
	All Municipalities	OLS (1)	All Municipalities	OLS (2)	All Municipalities	OLS (3)	All Municipalities	OLS (4)	All Municipalities	OLS (5)	All Municipalities	OLS (6)	All Municipalities	OLS (7)	All Municipalities	OLS (8)
Pre-election audit		-0.043 (0.040)		-0.019 (0.056)		-0.059 (0.046)		-0.010 (0.056)		5.72 (9.56)		-5.36 (12.17)		6.68 (11.80)		-4.63 (13.67)
Pre-election audit × Num. violations		-0.035 (0.029)		-0.035 (0.029)		-0.060** (0.028)		-0.060** (0.028)				11.78** (5.81)				10.91* (5.74)
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election Year & Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-election audits F-statistic [p-value]		1.75 [0.18]		3.75 [0.03]		2.66 [0.08]		2.18 [0.12]								
Observations	326	326	241	241	121	121	96	96								
Mean of dep. variable (controls)	0.25	0.25	0.31	0.31	42.4	42.4	44.0	44.0								

Notes: Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at (*) 90%; (**) 95%; (***) 99% confidence levels, respectively. Controls are the number of municipality government reports, the number of municipal public corporation or consortium reports; indicators for New Progressive Party membership, for incumbent in the opposition party to the state-level executive government, and for incumbent in the opposition party to the governor who appointed Comptroller; the vote share for the incumbent in the previous election (t-4); and the incumbent's number of terms in office (at time t). The sample is composed of all municipalities that had a first audit during 1987-2002. The reported "Pre-election audits F-statistic" refers to a test of joint significance on the Pre-election audit and its interaction with the number of violations (p-value in brackets).

TABLE IV: THE EFFECTS OF THE AUDITS ON THE NUMBER OF CORRUPT VIOLATIONS IN THE SUBSEQUENT AUDIT (TERM)

		Dependent variables: Number of corrupt violations in subsequent audit (term)					
Sample		by Mayor / Vice-mayor		Referred to Dept. of Justice		Referred to Dept. of Justice	
		All	All Municipalities	All	All Municipalities	All	All Municipalities
		OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)
Pre-election audit		-0.16 (0.25)	-0.09 (0.15)	0.00 (0.18)	0.15 (0.32)	0.07 (0.21)	0.32 (0.23)
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election Year & Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations		232	232	232	173	173	173
Mean of dep. variable (controls)		1.62	0.72	0.64	1.51	0.70	0.55

Notes: Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at (*) 90%; (**) 95%; (***) 99% confidence levels, respectively. Controls are the number of municipality government reports, the number of municipal public corporation or consortium reports; indicators for New Progressive Party membership, for incumbent in the opposition party to the state-level executive government, and for incumbent in the opposition party to the governor who appointed Comptroller; the vote share for the incumbent in the previous election (t-4); and the incumbent's number of terms in office (at time t). The sample is composed of all municipalities that had a first audit during 1987-2002 and a subsequent one in the 1991-2005 period.

TABLE V:
HETEROGENEOUS EFFECTS OF THE AUDITS ON THE NUMBER OF CORRUPT VIOLATIONS IN THE SUBSEQUENT AUDIT (TERM)

Sample	Number of all corrupt violations per report in subsequent audit (term)								
	Referred to			Referred to			Referred to		
	All	by Mayor / Vice-mayor	Dept. of Justice	All	by Mayor / Vice-mayor	Dept. of Justice	All	by Mayor / Vice-mayor	Dept. of Justice
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	
Pre-election audit	-0.54* (0.32)	-0.21 (0.20)	-0.27 (0.22)	-0.60 (0.40)	-0.27 (0.22)	-0.20 (0.32)	0.34 (0.62)	-0.02 (0.46)	0.83 (0.50)
Pre-election audit × Terms in office	0.39** (0.18)	0.13 (0.11)	0.29** (0.13)						
× Win margin in (t-4) election				4.25* (2.48)	1.83 (1.60)	1.98 (2.07)			
× Proportion B/A degree or higher							-5.08 (5.84)	-0.66 (4.14)	-8.43* (4.59)
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Δ Violations (Δ I.S.D. of Interacted V	0.46	0.15	0.34	0.39	0.17	0.18	-0.20	-0.03	-0.34
N	232	232	232	232	232	232	232	232	232
Mean of dep. variable (controls)	1.62	0.72	0.64	1.62	0.72	0.64	1.62	0.72	0.64

Notes: Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at (*) 90%, (**) 95%; (***) 99% confidence levels, respectively. Controls are the number of municipality government reports, the number of municipal public corporation or consortium reports; indicators for New Progressive Party membership, for incumbent in the opposition party to the state-level executive government, and for incumbent in the opposition party to the governor who appointed Comptroller; the vote share for the incumbent in the previous election (t-4); and the incumbent's number of terms in office (at time t). The sample is composed of all municipalities that had a first audit during 1987-2002 and a subsequent one in the 1991-2005 period.

TABLE VI: ROBUSTNESS TESTS - EFFECTS OF THE AUDITS ON THE NUMBER OF CORRUPT VIOLATIONS IN THE SUBSEQUENT AUDIT (TERM)

Sample	Dep. variable: Number of corrupt violations in subsequent audit (term)														
	All Violations					Violations by Mayor or Vice-Mayor					Violations referred to DoJ				
	All	FD/OLS	FD/IV	FD/IV	FD/IV	All	FD/OLS	FD/IV	FD/IV	FD/IV	All	FD/OLS	FD/IV	FD/IV	FD/IV
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)						
Pre-election audit (t)	-0.08 (0.31)	-0.06 (0.36)	-0.42 (0.41)	-0.02 (0.19)	-0.01 (0.24)	-0.03 (0.21)	-0.18 (0.31)	-0.04 (0.21)	0.09 (0.30)	-0.05 (0.32)					
Pre-election audit (t) × Num. violations	-0.06 (0.21)	-0.06 (0.21)	-0.06 (0.21)	-0.06 (0.21)	-0.06 (0.21)	-0.06 (0.21)	-0.06 (0.21)	-0.06 (0.21)	-0.06 (0.15)	-0.06 (0.15)					
Pre-election audit (t) × Num. violations - mayor					0.01 (0.31)	0.01 (0.30)	0.03 (0.30)								
Pre-election audit (t) × Num. violations - others					-0.01 (0.19)										
Pre-election audit (t) × Terms in office (t)			0.38** (0.18)			0.17 (0.14)				0.15 (0.11)					
Pre-election audit (t+4)	-1.381*** (0.37)	-1.368*** (0.36)	-1.417*** (0.35)	-0.475** (0.22)	-0.484** (0.21)	-0.491** (0.22)	-0.505** (0.20)	-0.652** (0.25)	-0.651** (0.25)	-0.671*** (0.25)					
Number of Violations (t)	No	Yes	Yes	No	Yes	Yes	Yes	No	Yes	Yes					
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes					
Election Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes					
Weak identification F-statistic	-	104.2	99.4	-	170.5	56.8	161.1	-	104.2	99.4					
Pre-election F-statistic [p-value]	-	0.24 [0.88]	1.78 [0.41]	-	0.00 [1.00]	0.03 [0.99]	0.35 [0.84]	-	0.17 [0.92]	0.39 [0.82]					
N	153	153	153	153	153	153	153	153	153	153					
Mean of dep. variable (level)	1.43	1.43	1.43	0.63	0.63	0.63	0.63	0.58	0.58	0.58					

Notes: Coefficient estimates and standard errors from (first-differenced) OLS and IV regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at (*) 90%; (**) 95%; (***) 99% confidence levels, respectively. For IVs, controls, samples, and reported statistics – see notes to table IV. The reported “Pre-election audits F-statistic” refers to a test of joint significance on the Pre-election audit and its interaction with the number of violations (p-value in brackets).

TABLE VII: THE EFFECTS OF THE AUDITS ON LONG-TERM ELECTORAL OUTCOMES

Sample	Incumbent runs for & wins re-election (period t+4)			Incumbent wins re-election running (period t+4)		
	All Municipalities	Mayors who run for re-election (t)	All Municipalities	All Municipalities	Mayors who run for re-election (t)	All Municipalities
Pre-election audit	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)
	0.041 (0.055)	0.080 (0.066)	0.102 (0.073)	0.116 (0.076)	0.101* (0.054)	0.151** (0.069)
Pre-election audit × Num. violations		-0.043 (0.031)		-0.030 (0.038)		-0.060 (0.036)
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes
Election Year & Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes
Pre-election audits F-statistic [p-value]		1.07 [0.35]		1.17 [0.31]		2.39 [0.10]
Observations	326	326	241	241	262	262
Mean of dep. variable (controls)	0.20	0.20	0.19	0.19	0.24	0.24
						0.23
						0.179** (0.080)
						-0.047 (0.040)
						0.165** (0.068)
						188
						0.23

Notes: Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at (*) 90%; (**) 95%; (***) 99% confidence levels, respectively. Controls are the number of municipality government reports, the number of municipal public corporation or consortium reports; indicators for New Progressive Party membership, for incumbent in the opposition party to the state-level executive government, and for incumbent in the opposition party to the governor who appointed Comptroller; the vote share for the incumbent in the previous election (t-4), and the incumbent's number of terms in office (at time t). The sample is composed of all municipalities that had a first audit during 1987-2002. The reported "Pre-election audits F-statistic" refers to a test of joint significance on the Pre-election audit and its interaction with the number of violations (p-value in brackets).

TABLE VIII:
TESTING FOR MANIPULATION OF THE AUDITING PROCESS

<i>Sample / Model</i>	Num. of corrupt violations per report in audit (t)															
	All			by Mayor / Vice-mayor			Referred to Dept. of Justice		Audit in subsequent term (1/0)		Num. of reports in subsequent audit					
	All / OLS	(2)	(3)	All / OLS	(4)	(5)	All / OLS	(6)	All / OLS	(7)	(8)	(9)	All / OLS	(10)	(11)	(12)
Pre-election audit	-1.39*** (0.26)	-1.34*** (0.31)	-0.72*** (0.16)	-0.36 (0.25)	-0.58** (0.20)	-0.64** (0.23)	-0.096* (0.056)	-0.031 (0.086)	-0.100 (0.075)	-0.159 (0.137)	-0.017 (0.161)	0.117 (0.186)				
<i>Additional Controls</i> : Pre-audit ×																
× Opposition party to Gov.	1.047*** (0.379)		0.623** (0.294)		0.441 (0.283)			-0.103 (0.092)					0.224 (0.302)			
× Opposition party to Gov. appointing Comptroller	-0.916* (0.530)		-0.265 (0.273)		-0.509 (0.376)			-0.078 (0.105)					-0.165 (0.249)			
× Win margin in (t-4) election	-0.942 (2.901)		-2.818 (2.545)		-0.131 (2.328)			0.040 (0.480)					-2.723 (1.638)			
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	326	326	326	326	326	326	326	326	326	326	326	326	326	326	326	326
Mean of dep. variable (controls)	2.17	2.17	0.95	0.95	0.99	0.99	0.83	0.83	0.83	0.83	0.83	0.83	1.94	1.94	1.94	1.94

Notes: Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at (*) 90%, (**) 95%, (***) 99% confidence levels, respectively. Controls are the number of municipality government reports, the number of municipal public corporation or consortium reports; indicators for New Progressive Party membership, for incumbent in the opposition party to the state-level executive government, and for incumbent in the opposition party to the governor who appointed Comptroller; the vote share for the incumbent in the previous election (t-4); and the incumbent's number of terms in office (at time t). The sample is composed of all municipalities that had a first audit during 1987-2002.

TABLE IX:
TESTING FOR MANIPULATION OF THE AUDITING PROCESS (CONT.)

Sample Model	Num. of corrupt violations per report in subsequent audit/term (t+4)																																					
	All			by Mayor / Vice-mayor			Referred to Dept. of Justice			All			by Mayor / Vice-mayor			Referred to Dept. of Justice																						
	OLS	IV/FD	(1)	OLS	IV/FD	(2)	OLS	IV/FD	(3)	OLS	IV/FD	(4)	OLS	IV/FD	(5)	OLS	IV/FD	(6)	OLS	IV/FD	(7)	OLS	IV/FD	(8)	OLS	IV/FD	(9)	OLS	IV/FD	(10)	OLS	IV/FD	(11)	OLS	IV/FD	(12)		
Pre-election audit	0.02	0.31	(0.30)	-0.10	-0.10	(0.19)	-0.04	0.18	(0.21)	-0.04	0.18	(0.30)	0.43	0.25	(0.33)	0.43	0.25	(0.47)	0.11	-0.11	(0.25)	0.11	-0.11	(0.25)	0.26	0.23	(0.33)	-0.12	-0.12	(0.23)								
Pre-election audit × Num. violations		-0.17	(0.24)					-0.16	(0.19)					-0.15	(0.31)																							
Pre-election audit × Num. violations by mayor/vice-mayor					-0.04	(0.33)																																
<i>Additional Controls: Pre-audit ×</i>																																						
× Opposition party to Gov.	-1.046*	-1.403**	(0.618)	-0.011	-0.018	(0.382)	-0.365	-0.697	(0.462)	-0.365	-0.697	(0.493)	-0.491	-1.257*	(0.878)	-0.491	-1.257*	(0.753)	0.120	0.073	(0.640)	0.120	0.073	(0.375)	-0.409	-0.724	(0.553)											
× Opposition party to Gov. appointing Comptroller	0.283	0.433	(0.498)	0.059	0.318	(0.300)	0.388	0.634	(0.317)	0.388	0.634	(0.448)	-0.356	0.357	(0.612)	-0.356	0.357	(0.648)	-0.177	0.229	(0.372)	-0.177	0.229	(0.422)	0.397	0.908*	(0.527)											
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Election Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
Weak identification F-statistic	-	68.9		-	118.4		-	68.9		-	68.9		-	61.1		-	61.1		-	139.7		-	139.7		-	61.1												
N	232	153		232	153		232	153		232	153		173	119		173	119		173	119		173	119		173	119												
Mean of dep. variable (controls)	1.62	1.62		0.72	0.72		0.64	0.64		0.64	0.64		1.51	1.51		1.51	1.51		0.70	0.70		0.70	0.70		0.55	0.55												

Notes: Coefficient estimates and standard errors from level OLS and (first-differenced) IV regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at (*) 90%; (**) 95%; (***) 99% confidence levels, respectively. For IVs, controls, samples, and reported statistics – see notes to table IV.