Does Exposing Corrupt Politicians Reduce Corruption?*

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Abstract: Does the disclosure of information about corruption practices induce a sustained reduction in corruption levels? We use publicly-released routine audit reports to study this question. The government of Puerto Rico has established a mechanism to routinely conduct municipal government audits, whose findings are then made publicly available and disseminated to media sources. Using a longitudinal dataset of corrupt violations constructed from the audit reports during the period 1987-2006, we compare the subsequent term governments’ levels of reported corruption for municipalities audited at different points in time around an election. The pre-election release of the audit reports led to significant short-term reductions in municipal corruption levels and an increase in incumbent mayors’ electoral accountability. However, municipal corruption levels in the subsequent term are the same in municipalities audited preceding the previous election and are higher among municipalities shown to have refrained from rent-seeking activities in the first audit. These findings are consistent with a political agency model of reputation dynamics in which rent seeking is increasing in the incumbent’s reputation.

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I. Introduction

In a well-functioning representative democracy, citizens should select competent politicians to administer public affairs and hold these accountable for their performance. A precondition for these aims of democratic government is that citizens have appropriate information about candidates’ character, abilities, and performance while in office (Manin, Przeworski, and Stokes 1999; Besley 2006). Accordingly, a growing literature recognizes that voters having access to information to evaluate politicians’ performance enhances government responsiveness, reduces corruption and rent-seeking behaviors in the short-run (e.g., Besley and Burgess 2002; Reinikka and Svensson 2005; Olken 2007) and promotes electoral accountability (e.g., Ferraz and Finan 2008; Banerjee et al 2010). However, whether providing information to voters about the corrupt practices of politicians induces a sustained improvement in government administration and a reduction in rent-seeking – desirable qualities of good government in the longer run – is less well understood.

This paper studies the effects of the disclosure of local government corruption practices on the longer-run levels of corruption of municipal governments. It overcomes previous limitations by using data from a unique setting: routine publicly released audit reports of municipal government activities in Puerto Rico. The government of Puerto Rico has established a systematic mechanism to conduct municipal government audits, whose findings are then made publicly available and disseminated to media sources. Our research design exploits the exogenous ordering in which municipalities are routinely audited over time, a rule which helps us establish the effects of this auditing and information dissemination program on the municipalities’ subsequent levels of corruption. Specifically, we employ a unique longitudinal dataset of corruption findings constructed from the audit reports for municipalities during the period 1987-2006 to compare (i) the short-term levels of reported corruption of municipalities audited before versus after each election, as well as (ii) the subsequent audit’s levels of reported corruption and other municipal outcomes.

Our first set of results confirms previous evidence that the audits reduced municipal corruption levels by 60 percent, consistent with there being a substantial short-run disciplining effect of the audits in the municipality. We also show that the release of the audit reports improved electoral accountability, reducing the re-election rates of incumbent mayors by 6 percentage points (25 percent) in municipalities where up to two corrupt violations were reported, and by 12 percentage points in those where more than two corrupt violations were reported. These findings are consistent with short-term findings from similar auditing programs (Olken 2007; Ferraz and Finan 2008).1

1 Ferraz and Finan (2008) find that the electoral performance of incumbent mayors audited before the elections was not significantly different from that of mayors whose municipalities were audited after the election, on average. When accounting for the level of corruption that was revealed in the audit, they find re-election effects of very similar magnitude.
In contrast to these previously identified desirable short-run consequences of the audits, we find that observed municipal corruption levels in the subsequent audit are equivalent in municipalities audited preceding the previous election relative to those whose reports were disseminated following the previous election. Moreover, the perverse rent-seeking effects are concentrated among municipalities whose incumbent mayors were shown to have refrained from rent-seeking activities in the first audit. Our preferred estimates indicate that rent-seeking activities increase by 0.55 to 1 standard deviation among municipalities with favorable outcomes in the previous audit, whereas there is no discernible change in corruption in municipalities whose incumbent mayors were shown to have engaged in corruption in the first place. The findings lend support to the argument that short-term information dissemination policies do not necessarily align politicians’ actions with voters’ preferences in the longer-run.

Our study highlights the role that information plays in enhancing political accountability in the short-run but its plausible negative consequences for politician behavior in the longer-run. In particular, the findings are consistent with a political agency model of reputation dynamics in which in equilibrium rent seeking is increasing in the incumbent’s reputation (Schwabe 2010). In this model, if voters re-elect incumbent mayors based on their reputation-based performance in office, a mayor whose reputation has improved in the past can exploit this information asymmetry to engage in rent-seeking activities, leaving voters indifferent between reelecting him and electing an inexperienced challenger. Given these perverse reputation incentives, reelected mayors who have been shown to refrain from rent-seeking activities in the past will on average be more corrupt in future terms than mayors who do not. Importantly, this is consistent with information dissemination on politicians’ actions leading to an increase in ex ante voter welfare, as the short-run positive disciplining effect can dominate the negative consequences of imperfectly screening a corruptible politician who then serves in a subsequent term.

The research design and the richness of the data also allow us to distinguish our explanation for corruption outcomes from various alternative interpretations. First, even though the timing of the municipal government audits is pre-determined, the actual auditing process could have differed systematically before and after elections. We do not however find any evidence that auditors were corrupt or that mayors with more political power or those affiliated with higher levels of government receive preferential audits. A second concern is that political cycles are potentially consistent with the variation in the effects of the timing of the audits on the observed levels of corruption. However, we show that the

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2 We also find that the less favorable pre-election audits led to a significant degree of selection of subsequent mayors based on (higher) pre-incumbency earnings.
3 Our findings are also consistent with a political agency model of electoral accountability in which information serves as a disciplining device for corruptible politicians in the current term, but by allowing for some corruptible politicians to ‘pool’ with non-corruptible types, enhances the likelihood of corruption in future terms (Besley 2006). We are currently working on theoretically identifying possible distinctions between these two arguments.
actual timing of the acts of corruption, as measured by the dates in which acts were committed, is not correlated with the timing of the audit.

The study contributes to the growing empirical literature documenting how electoral accountability, and information provision in particular, influences political behavior. Besley and Burgess (2002) show that newspaper circulation affects the responsiveness of state governments in India to negative shocks to food production and flooding. 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. 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 corruption practices in these municipalities is publicized, whereas Ferraz and Finan (2010) show the extent to which re-election incentives affect political corruption in the short-run. Our paper contributes to the literature by providing for the first time (to our knowledge) evidence of the divergent inter-temporal consequences on political corruption of voters having information about the corrupt practices of politicians.

The paper is organized as follows: Section II provides a background on Puerto Rico’s municipal audit program, the municipal government system, and the national political debate which influences local politics. Section III discusses the main empirical implications of the political agency model under consideration. Section IV provides a description of the data used in the analysis, followed in Section V by the empirical implementation of the model, the study’s research design, and the main identifying assumptions. The central empirical results of the paper and robustness evidence from the tests are presented in Section VI. The paper concludes in Section VII with a discussion toward the reconciliation of the existing evidence.

II. Background

II.A. 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 and implemented by Act No. 9 of July 24, 1952. The agency’s mission is to “audit the property and public funds transactions with

5 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.
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 for the benefit of our people” (Office of the Comptroller 2009). The OCPR, thus, can examine virtually every (central or municipal) government financial transaction. To do this, the OCPR periodically audits the 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 as to 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 as to be audited in a fiscal year, the OCPR sends a team of auditors, accompanied by a supervisor, 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 then shared with the municipality officials (i.e. mayor and top management) to provide these 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. 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 executive branch’s Office of Government Ethics. When an audit report implicates an important mayor or describes a particularly outrageous corruption scheme it can generate substantial press coverage. Finally, note that the OCPR may publish multiple reports on a municipality for one auditing period; this depends on the size or complexity of the municipal government.

Based on our tabulation of the data, all seventy eight municipalities were audited during our period of interest (1987-2006) multiple times. The timing of the dissemination of the reports is depicted in Figure 1. As can be seen, there is a tendency to publish reports in the month of June, at the end of the central government’s fiscal year, although this tendency has diminished over time. There is a non-extreme tendency for the OCPR to publish reports in the two months preceding an election (lines in red) (i.e.
September and October), and a tendency to publish more reports in recent years. For the 1996 and 2000 electoral periods, almost all or all of the municipalities were audited at least once (see Figure II). 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.

A number of measures are taken to minimize potential biases in the conduct of the audits and in the dissemination of their findings. First, the Comptroller, who is appointed by the governor with the advice and consent of the majority of the members that make up each legislative chamber, serves for a ten-year term (until its successor is named and takes possession); he can only be removed from his/her position by an impeachment procedure. The intent is that the OCPR is given a substantial degree of autonomy from the rest of the central government structures, to isolate him or her from undue external interference. Also, the OCPR is technically a part of the Legislative Branch (as it is constituted under Article 3 of the Constitution, the article that establishes and rules the Legislative Assembly). Since most of the OCPR’s activities are focused on the Executive Branch and the municipalities, this guarantees the office an additional layer of protection. Second, the auditors, who are hired based on a competitive public examination and earn highly competitive salaries, receive extensive training prior to visiting the municipalities. Also, in order to reduce/minimize local-level conflicts of interest, individual auditors are precluded from participating in audits of their municipality of residence.

We do not yet have direct evidence showing that voters learned about the audit reports (data collection is in progress), but some anecdotal evidence suggests that the information from the audits did reach voters. For instance, an article published on September 25th 2008 (before the 2008 elections) in a major newspaper regarding the outcomes of a recent audit of the municipality of San Juan highlighted findings of mismanagement attributed to municipal employees. Specifically, the report highlighted that the 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 (Hopgood Dávila 2008). This report was used by the PDP opposition candidate for mayor in the 2008 election, Ferdinand Pérez, to declare that Santini was “a disaster as an administrator”, and was also publicly challenged by the incumbent subsequently (Hopgood Dávila 2008).

II.B. Municipal Government Activities and Political System

The seventy eight municipal governments in Puerto Rico constitute the level of government closest to citizens. There is no jurisdictional distinction between the cities and the municipalities in which they lie. In terms of its organization, a municipality is governed by a mayor and a municipal assembly, officials who are elected for a four-year term following the central (and U.S. federal) government

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6 Third Article, Section 22 of the Constitution of the Commonwealth of Puerto Rico.
electoral cycle. 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. Mayors and municipal council members do not face term limits; they can be reelected as many times as their constituents allow them that privilege. In fact, mayors from municipalities where their party is very dominant tend to be reelected almost indefinitely, until they retire or death permanently terminates their political career. Also, although the dominant party usually has a significant majority of the members of the assembly, the law guarantees some representation for political minorities: a small number of seats for the party that ended in second place and one seat for the party in third place. Given their generally small size, municipal assembly members in the minority usually carry out an oversight work, exposing waste and corruption. The mayor appoints the top management of the municipality.

Although municipal governments possess some degree of autonomy, compared, for example, to counties and cities in the United States, their sphere of influence is somewhat more limited. The bulk of the services they provide are related to infrastructure construction and maintenance, solid waste management, public health services, and the like. 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, for example, 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.7

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.

A final description concerns the nature of political cleavages, the party structure, and the degree of political participation and competition at the national level, which greatly influence municipal politics. The Commonwealth of Puerto Rico is an unincorporated territory of the United States. Annexed to the U.S. following the Spanish American War by virtue of the Treaty of Paris of 1898, it progressively gained an increasing degree of autonomy until the establishment of the Commonwealth of Puerto Rico ("Estado Libre Asociado"—ELA) in 1952. However, even with the establishment of the commonwealth status, Puerto Rico is still under the territorial clause of the U.S. Constitution. Federal sovereignty and law apply

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7 The municipal reform also created two new agencies to further strengthen municipal autonomy: the Office of the Commissioner for Municipal Affairs ("Oficina del Comisionado de Auntos Municipales"—OCAM) and the Center for the Collection of Municipal Revenue ("Centro para el Recaudo de Ingresos Municipales"—CRIM). OCAM is the agency in charge of defending and implementing municipal autonomy. CRIM is the agency responsible for collecting resources such as property taxes for the municipalities. Its board of directors is composed of mayors.
to Puerto Rico, and both the U.S. Department of Justice and the federal judiciary operate in the Island to enforce federal-level laws.\textsuperscript{8} Puerto Rico’s degree of autonomy to the Federal government is similar to that of a U.S. State except in two important ways: residents of Puerto Rico do not have Congressional representation with voting rights and do not participate in the U.S. presidential elections. In addition, a number of federal laws do not apply or apply differently than to the states, when specified in federal legislation.

National politics are essentially shaped by the debate over P.R.’s political status or relationship between the island and the United States. The three main political status alternatives: federal statehood, independence, and continuation of the current Commonwealth status, shape the political party system; it is the main political cleavage (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 the independent status. The NPP and the PDP are the two main political parties; they are similar in size in terms of electoral support and interchange regularly in power. The PIP is a relatively small third party, usually receiving around three (3) percent of the national electoral vote.

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 party of the incumbent governor is the same that overwhelmingly controls both chambers of the Legislative Assembly and municipal governments. Contrary to the United States—and more in common with 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 the 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.

III. Theoretical Framework

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 and actions. If voters re-elect incumbents mayors based on their reputation-based performance in office, a mayor whose reputation has

\textsuperscript{8}The fact that federal laws apply to Puerto Rico is important in the context of this investigation, since several mayors have gone to jail for racketeering with federal money.
improved in the past can exploit this information asymmetry to engage in rent-seeking activities, leaving voters indifferent between reelecting him and electing an inexperienced challenger. Given these perverse reputation incentives, the model predicts that reelected mayors who have been shown to have refrained from (engaged in) rent-seeking activities in the past will on average be more (less) corrupt in future terms than mayors who do not.

**Reputation and Accountability in Repeated Elections**

Consider a discrete-time, infinite horizon model of municipal politics. In each period (indexed by \( t \in \{1, 2, \ldots \} \)), a representative voter must select a politician to administer local public affairs. Following each election, the elected politician chooses a level of effort \( e \in [0, \infty) \) that influences, but not perfectly determines, the level of a public good that is provided to voters:

\[
g = e + \varepsilon,
\]

where \( \varepsilon \) is a normally distributed noise term with mean 0 and variance \( \rho^2 \). The (representative) voter values the level of public good linearly; thus \( E[g] = e \) is also the voter’s expected utility.

Politicians are of two types – normal or corrupt – with \( \mu_0 \) denoting the proportion of normal types in the 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 \( u(e) \), where \( u'(e) < 0 \) for all \( e \), and 0 otherwise Additionally, we assume that \( u \) is sufficiently concave for the politician’s best response effort level to be uniquely determined by the first order condition to his utility maximization problem, a condition we call strict concavity and which we formally define in the appendix. 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., \( u'(0) \) is very large) or due to incompetence. Politicians and voters 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 irregularities are reported to voters. We interpret audits as providing additional information about politician’s effort:

\[
c = e + \zeta,
\]
where \( \zeta \) is a normally distributed, zero-mean noise term with variance \( \theta^2 \). To match the context, we assume that politicians know whether they will be audited when making corruption/effort decisions. The voter may compute a composite signal:

\[
s = \lambda g + (1 - \lambda)c, \quad \text{where} \quad \lambda = \frac{\theta^2}{\rho^2 + \theta^2},
\]

which is information-equivalent (that is, it generates the same posterior beliefs about the politician’s type) to using the public good and audit signals separately.\(^9\) We make the assumption that audits are informative relative to the politician’s observed performance while in office, or \( \theta^2 < \rho^2(1 - \delta)/\delta \).

The voter assigns in each period a probability \( \mu_j \) that politician \( j \) 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 \( \mu_0 \). Thereafter, his reputation is updated according to Bayes’ rule each time the voter observes \( g \) or \( c \), via a function which we denote \( \mu_j \).

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

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

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 \( Q(\sigma, e, \tilde{\mu}; h_t) \):

\[
Q(\sigma, e, \tilde{\mu}; h_t) = p e(h_t, a = 1) + (1 - p)e(h_t, a = 0) + \delta E[Q(\sigma, e, \tilde{\mu}; h_{t+1}(g, c))].
\]

The timing of the infinitely repeated stage game is as follows. A politician is elected at the beginning of each period; if newly elected, nature reveals his type. The politician then makes the policy/effort choice, after which voters observe their payoffs, update their beliefs regarding the incumbent’s reputation, and decide whether or not to re-elect the incumbent or select a challenger who has been drawn at random from the pool of potential politicians.

\(^9\) This composite signal \( s \) is also normally distributed with variance \((\rho^2\theta^2)/(\rho^2 + \theta^2) < \rho^2\).
As in most infinitely repeated games, there are many candidate equilibria. Following Schwabe (2010), we argue that a class of sequential equilibria of this game – reputation-dependent performance cutoffs (RDC) equilibria – are particularly convincing because they avoid commitment problems from the part of the voter. In RDC equilibria, incumbents are reelected only if their observed performance exceeds a cutoff that varies with the incumbent’s reputation.\textsuperscript{10} 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 reelecting 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.\textsuperscript{11}

The voter indifference condition, central to RDC equilibrium, implies that the voter’s value function is constant across reputations:

\[
V = \mu[p e(h_i, a = 1) + (1 - p) e(h_i, a = 0)] + \delta V
\]

This, in turn, means that the ex-ante expected level of effort (and rent-seeking) will be constant across reputations \(\mu\):

\[
\bar{e} = \mu[p e(h_i, a = 1) + (1 - p) e(h_i, a = 0)] = (1 - \delta) V.
\]

This means that a politician with a high reputation (high \(\mu\)) will pocket the benefits of his accumulated reputation by being more corrupt than a normal politician of a lower reputation. 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.

The most 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 corruption is constant, \(E(e_{t+1}) = \bar{e}\), the expected change in effort is simply the difference between today’s corruption level and \(\bar{e}\) or:

\textsuperscript{10} This stands in contrast to equilibria in which voters use performance standards to make reelection 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 reelection strategy. Formally, these equilibria are not weakly renegotiation-proof (WRP, Farrell and Maskin 1989).

\textsuperscript{11} This class of equilibria satisfy weakly renegotiation-proofness (Farrell and Maskin 1989) and challenger stationarity (Banks and Sundaram 1993); that is, the value to voters of electing an inexperienced politician is history-independent.
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.

IV. Data

IV.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 1987-2006 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-present).

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. 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 or to the Office of Government Ethics. 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

\[ E(c_{it}) - c = \bar{c} - c_i. \]  

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12 Before we began the coding process, the research assistants were given extensive training in content analysis, coding, and the details of the audit reports. Then they coded several reports to familiarize themselves with their format. We then ran tests for intercoder and intracoder reliability. The process continued until coder reliability was at least 0.9. The same coders worked with the reports throughout the project. The data was then sent to another research assistant who went over the complete dataset to catch and correct any errors.
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 covering up of a corrupt violation).

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. 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.

IV.B. Other Data Sources

We employ two additional datasets available from the P.R. State Electoral Commission (“Comisión Estatal de Elecciones”—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. These documents are required by law of all candidates to submit the CEE in order to be certified and 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

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13 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.

14 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.
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.

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 the political agency model:

(i) the expected dissemination of the audit reports should decrease the short-run rent seeking behaviors of incumbent politicians in the municipalities, and their actual dissemination the incumbent politician’s electoral accountability;

(ii) 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; and,

(iii) the incumbent politician will be expected, on average, to engage in more (less) corrupt activities the more (less) favorable the outcome of the previous period audit.

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 subsequently 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:

\[ c_{mt} = \theta A_{mt} + \beta X_{mt} + \gamma_t + \alpha_m + \epsilon_{mt}, \]  

(9)

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. The terms \( \alpha_m \) and \( \gamma_t \) represent state and election intercepts, respectively, and \( \epsilon_{mt} \) denotes unobserved characteristics that determine corruption at time \( t \). Under the assumption that \( A_{mt} \) is strictly exogenous, the coefficient \( \theta \) 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. Models of political agency predict that \( \theta < 0 \). An analogous model that uses as dependent variable an indicator for the re-election of the incumbent mayor in election year \( t \) (denoted \( e_m \)) captures the effects of the audits and their actual dissemination on the incumbent’s electoral accountability.

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:

\[
c_{m,t+1} = \theta_{PA}A_{mt} + \beta X_{mt} + \gamma_{t+1} + \alpha_m + \epsilon_{m,t+1},
\]

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 \( \epsilon_{m,t+1} \) 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 rent-seeking after an audited period than after a non-audited period.

The overall comparison of municipalities does not capture the possibility that the outcome of the audit being publicly released at time \( t \) contains information about the corrupt behaviors of the incumbent mayor and other municipal government employees. Specifically, we expect the incumbent politician in the next term to engage in more corrupt activities the more favorable the outcome of the previous period audit, and to engage in less corrupt activities the less favorable the outcome of the previous audit. To test for these possibly heterogeneous effects, we estimate 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 \( t \):

\[
c_{m,t+1} = \theta_{P1}A_{mt} + \theta_{P2}A_{mt}c_{m,t} + \beta_1c_{m,t} + \beta_2X_{mt} + \gamma_{t+1} + \alpha_m + \epsilon_{m,t+1},
\]

where all variables are defined as above. The \( \theta_{P1} \) 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 \( \theta_{P2} \) 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). Since the incumbent politician will be expected, on average, to engage in more (less) corrupt activities the more (less) favorable the outcome of the previous period audit, we will test whether \( \theta_{P1} > 0 \) and \( \theta_{P2} < 0 \).

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. To test this hypothesis,
we allow for mayor/vice-mayor-specific and other employee-specific measures of corruption in empirical model (11).

Since equation (11) is a dynamic panel data model, it is well known that, even if \( c_{mt} \) and \( e_{m,t+1} \) are not correlated, if \( t \) does not approach infinity 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 (11):

\[
c_{m,t+1} - c_{m,t} = \theta_{P1}(A_{m,t} - A_{m,t-1}) + \theta_{P2}(A_{m,t}c_{m,t} - A_{m,t-1}c_{m,t-1}) + \beta_1(c_{m,t} - c_{m,t-1}) \\
+ \beta_2(X_{m,t} - X_{m,t-1}) + (y_{t+1} - y_t) + \epsilon_{m,t+1} - \epsilon_{m,t},
\]

(11')

and since \( E[c_{m,t} \epsilon_{m,t}] \neq 0 \), the inclusion of the lagged dependent variable will generate bias in the OLS estimates of the coefficients of interest (\( \theta_{P1} \) and \( \theta_{P2} \)), even under the assumption that \( A_{m,t} \) is strictly exogenous. We show in Appendix A that the OLS estimates of coefficients \( \theta_{P1} \) and \( \theta_{P2} \) in equation (3') are biased towards zero, against finding the hypothesized relationship of interest.\(^{15}\)

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 \( \theta_{P1} \) and \( \theta_{P2} \).

VI. Results

VI.A. Short-Run Discipline and Electoral Accountability Effects

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 among

\(^{15}\) The OLS estimate of equation (11') leads to a downward bias in the coefficient \( \theta_{P1} \) and an upward bias in the coefficient on \( \theta_{P2} \). 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.
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 for investigation 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, this relationship is stable and robust to controls (not reported) and focusing on the subset of municipalities in which the incumbent runs for re-election (columns 4-6).

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).\textsuperscript{16} 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 reelection rate conditional on running for reelection.

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 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.

\textsuperscript{16} The reported differences between pre-election and post-election audit municipalities are regression-adjusted for election period fixed effects.
Parametric linear probability estimates of the reduced-form relationship following empirical model (9) capture the results depicted above (see Table II, columns 7-10). Although incumbent mayors’ overall successful re-election rates are not significantly correlated with the number of corrupt violations among pre-election audit municipalities (column 7-8), 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 10). Overall, these estimated relationships support the hypothesis that information about corrupt violations induces an improvement in electoral accountability.

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

We start by examining the relationship between the level of corruption for municipalities audited in the two years before election period $t$, and those audited in the following two years. Figure III plots the number of corrupt violations per report from audits one and two terms before election $t$, around election in year $t$ and in the following audit 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.

For audits preceding the one around election $t$, there are no discernible differences in the levels of reported corruption across municipalities – 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).

Although the previous results support the hypotheses that information about corrupt violations induces greater accountability, comparing audits in the next term (around election $t+4$) the difference in subsequent corruption levels across these groups of municipalities 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 ($0.18 = |0.54 – 0.72|$). This graphical evidence strongly suggests that the disciplining effects of the pre-election audits are short-lived.
Parametric estimates of the reduced-form longer-run effects of the pre-election audits on the number of corrupt violations in subsequent audits allow us to formally test for these effects (Table III). The point estimate from the average effects model (equation [10]) with municipality and election-specific intercepts (as well as municipality and mayor controls) indicates an (statistically insignificant) increase of 0.15 of a corrupt violation per report (9 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) or using the more stringent measure of corruption – the number of findings referred to the Department of Justice (column 3). The point estimates from these specifications imply small increases in rent-seeking of 0.06 of a violation (8 percent) and 0.12 of a violation (19 percent), respectively. 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) and those in which the incumbent was initially in his/her first or second term in office (columns 7-9), mayors likely to respond to the auditing scheme to build up their reputation.17

In summary, 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. In the next sub-section, we examine 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 reported corruption levels and 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 (see Section VI.A). Second, there is a small increase 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 slightly larger for municipal governments that

17 We have sufficient precision to reject moderately-sized reductions in the number of violations. For instance, the lower 95 percent confidence interval from specification (1) is -0.364; that is, a 23 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 from the authors upon request.
experienced an audit in the preceding term (see Section VI.B). Finally, there is a negative relationship between unfavorable audit outcomes in a given audit and those in the subsequent audit among local governments that were audited preceding the first election. In contrast, there is no relationship in these outcomes among governments that were not audited preceding the first election. The evidence is also consistent with the fact that the incumbent politician will be expected, on average, to engage in more corrupt activities the more favorable the outcome of the previous period audit, conditional on there being an audit. The following analysis provides formal tests of these predictions.

IV estimates from empirical models of equation (11') show that there is a significant increase in corruption among municipalities with no reported findings of corruption in the previous audit (Table IV). The point estimate of $\theta_{P1}$ from a specification with a linear interaction term in the preceding audit findings (and excluding covariates) indicates that the overall number of corrupt violations in the subsequent audit increase by 1.07 (43 percent of a standard deviation) among municipalities with a favorable outcome of the previous audit (column 3). Moreover, the model implies a relative decrease of 0.48 of a corrupt finding per each additional finding of corruption per report in the preceding audit (20 percent of a standard deviation; significant at 95 percent confidence). The resulting total effects given non-zero corrupt findings in the preceding audit are small and not significantly indistinguishable from zero at conventional confidence levels (see bottom of the table). The estimated effects are unchanged when we include covariates.

Empirical models that allow for a quadratic term to capture heterogeneous responses given the outcome of the preceding audit seem more appropriate at fitting the data; the quadratic term implies a substantial degree of curvature in the relationship and is precisely estimated (columns 4-5). Estimates from a specification with no additional controls indicate that among municipalities with no reported findings of corruption in the previous audit the overall number of corrupt violations in the subsequent audit increase by 2.38 (97 percent of a standard deviation), on average (column 4). Among municipalities with one and two reported findings of corruption per report in the previous audit, the estimates respectively suggest an average increase of 0.50 corrupt findings (20 percent) and a decrease of 0.67 corrupt findings (27 percent) in the following audit, although neither is statistically distinguishable from zero. An analogous specification with additional municipality and mayoral controls gives similar results: a significant increase of 2.46 corrupt findings (101 percent) among municipalities with previous favorable audit outcomes, and smaller or negative but statistically insignificant effects on corruption among municipalities with less favorable audit outcomes in the previous round of audits (column 5). Thus the information about corrupt violations made available to the public before an election induces a perverse increase in rent seeking in municipalities with previously favorable audit outcomes, and no modification in rent seeking among municipalities with previously unfavorable audit outcomes.
We also report the OLS estimates of equation (11') for purposes of comparing these with the analogous IV estimates. Note that, consistent with the hypothesized direction of the bias due to the nature of the dynamic panel model, the OLS estimate of coefficient $\theta_{p1}$ from equation (3') is smaller than its IV analog, whereas that of coefficient $\theta_{p2}$ is larger (column 6; see Appendix A for details). We find analogous differences in the heterogeneous treatment effects models when comparing the OLS and IV quadratic interaction effects specifications (column 7).

As a first set of robustness tests, we estimate the models for the sample of municipalities in which mayors run for re-election in the current term, and find quantitatively similar results (Table V, columns 1-3). The point estimate from the average effects model with controls suggests an average increase of 0.39 of a corrupt violation per report among the pre-election audit municipalities, but the relationship is not precisely estimated (column 1). The effect is again stronger among municipalities with no reported findings of corruption in the previous audit. The point estimate from a specification with the linear interaction term indicates that the overall number of corrupt violations in the subsequent audit increase by 0.85 (35 percent) among municipalities with a favorable outcome of the previous audit (column 2). The model again implies a relative decrease of 0.51 corrupt findings per each additional finding of corruption per report in the preceding audit (21 percent of a standard deviation; significant at 90 percent confidence), with resulting total effects given non-zero corrupt findings in the preceding audit small and statistically indistinguishable from zero. Qualitatively similar results are found in the specifications with quadratic interactions with the level of corruption in the first audit (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. 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 (Table V, columns 4-7). The average effect specification shows an insignificant increase in corrupt violations by the incumbent executive (column 4), whereas the (quadratic) heterogeneous response model shows a significant increase of 0.842 violations (55 percent) by the local executive among municipalities with previous favorable audit outcomes (columns 7). Moreover, among municipalities with non-zero reported findings of corruption per report committed by the local executive in the previous audit, the estimates suggest no significant effect in the number of violations in the following audit. The relationship is similar but less precisely estimated with the linear heterogeneous response model (column 5). The estimates from the sub-sample of municipalities in which mayors were running for re-election following the initial audit are very similar (columns 8-11).

The theoretical framework also predicts that the learning regarding the incumbent’s type will take place early in a politician’s career. We thus estimate the models for the sub-sample of municipalities in
which mayors are in their first or second term in office, as the results should be concentrated among this
group of incumbents (Table VI, columns 1-3).\textsuperscript{18} Although we cannot statistically distinguish the effects
for this sub-sample relative to the overall sample of municipalities, the estimates show that the
relationship holds robustly for this sub-sample. The average effect is positive and imprecisely estimated
(column 1), whereas the heterogeneous effects models indicate a significant increase of 1.19 violations
(48 percent) among municipalities with earlier favorable audits and no effects among municipalities with
previously unfavorable audit report outcomes (columns 2-3). Note however that our sample sizes do not
permit us to estimate the models for municipalities whose mayors are in their third or higher terms in
office (the sample size in first differences for this latter subgroup is 47 observations), as the robustness of
the instrumental variable(s) is lost for this subgroup of observations.

We also present a series of models similar to those reported in Table IV but estimating the effects
of the policy on a more stringent measure of corruption – the number of findings (per report) of misuse of
public funds referred for investigation to the P.R. Department of Justice (Table VI, columns 4-7). Overall
the results tell a similar story. For instance, the estimates in column (5) imply that an initial favorable
audit increases the future incumbent mayor’s reported “DoJ-referred” corruption by 0.62 findings per
report, or 37 percent of a standard deviation, whereas the reporting of an additional corrupt violation in
the initial audit reduced the future incumbent mayor’s reported “DoJ-referred” corruption by 0.29
findings per report, or 17 percent of a standard deviation, among municipalities that were audited prior to
the elections relative to those that were audited afterwards.

Additional Specification Checks: 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.

First, we 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

\textsuperscript{18} An additional identification concern that this specification addresses is that the results may be driven by last term effects of
politicians who have been in office for a significant number of terms leading to sample selection bias.
audit (Table VII, columns 1-4). The estimates indicate no evidence of selective auditing, or of differential intensity of auditing, as measured by the number of reports.

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. Including these additional controls do not affect the main heterogeneous responses of the pre-election audits on subsequently reported violations (columns 5 and 9).

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. Again, we do not find any evidence that a mayor’s previous level of political support influenced the audit process and including these additional controls do not affect the main heterogeneous responses of the pre-election audits (columns 6 and 10).

The remaining columns of Table VII provide further evidence of the robustness of our results. Specifically, we also test for whether our corruption measure is simply capturing a differential effect by the seniority of the mayor or some other characteristic of the municipality such as the educational attainment of the population. After allowing for differential effects in these margins, our point estimates on the main effects remain essentially unchanged (columns 7-8, 11-12).19

This evidence allows us to conclude that the information about corrupt violations induces a certainly perverse increase in rent seeking activities that is concentrated among municipalities with previous favorable audit outcomes. We next explore whether the provision of information to voters induces modifications in other actions of municipal executive officeholders, and whether it affects the selection of politicians into office.

VI.D. Effects of the Audits on Politician Selection, Longer-Term Electoral Performance

Political agency models with information dissemination (ours and others) make clear predictions regarding the effects of providing voters with information about the politician’s actions with respect to political selection – mainly, the competence of the (re-)elected politicians should increase, and the quality

19 We also carry out placebo tests, showing that the heterogeneous 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.
of government should improve, unless the pool of potential candidates is composed of a substantial proportion of bad politicians (e.g., Caselli and Morelli 2004; Besley 2006). In this sub-section, we present evidence that tests these predictions of the models with respect to the degree of politician selection in the following electoral term.

We present evidence on the reduced-form effects of the pre-election audits on the selection of mayors following the election (Table VIII). Following the theoretical literature on politician’s wages and politician selection, we use the (re-)elected mayor’s household per capita earnings in the fourth year preceding the respective election year as a plausible measure of the competence of the politician elected into office. Estimates of the average effects of the pre-election audit show no systematic (positive or negative) selection of higher earnings politicians in the municipality, even among the subset of municipalities in which incumbent mayors ran for re-election (columns 1 and 3). 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 (10). The point estimate indicates that those (re-)elected mayors have on average $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 sub-sample 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.

An additional characteristic of the model is that if voters re-elect incumbent mayors based on their reputation-based performance in office, a mayor whose reputation has improved in the past can exploit this information asymmetry to engage in rent-seeking activities, leaving voters indifferent between reelecting him and electing an inexperienced challenger. Therefore, an additional test of the model is that re-election rates of incumbents in the next term (in the election at time t+4) should not vary significantly among municipalities that received an earlier pre-election audit and those receiving a later audit.

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).20 Panel A is based on a measure of the mayor’s successful re-election or otherwise (i.e. not run for re-election, or

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20 Again, the reported differences between pre-election and post-election audit municipalities are regression-adjusted for election period fixed effects.
lose in primary or general election), whereas Panel B uses a measure of the incumbent’s reelection rate conditional on running for reelection.

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 reelection 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 following empirical model (9) again capture the results depicted above (see Table VIII, columns 5-12). As expected, 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, the estimates among mayors who run for re-election in the next term experience a positive shift in their electoral performance (columns 9-12). 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.

**VI.E. Effects of the Audits on Other Measures of Municipal Government Performance**

If mayors respond to the information being provided to voters given their re-election incentives, they will not only refrain from engaging in corrupt practices, but will also exert effort along other performance dimensions that should provide them with an electoral advantage. In this subsection, we assess whether greater scrutiny due to a pre-election audit induces mayors to exert effort in the raising of local government revenues, and whether this results in greater expenditures in the municipality. If mayors under greater scrutiny and who as a result engage in no (or less) rent seeking may also raise more local government revenue and carry out expenditures in public goods or other activities attractive to voters, whereas those who are not disciplined should not.

To examine this hypothesis, we use data on municipal government income statements for the fiscal years 1991-92 to 2006-07 that provides us with the following information: (i) the total municipal
government budget, as well as (ii) capital improvements expenditures, (iii) total salaries (and benefits) of municipal employees, (iv) social assistance expenditures, and (v) other expenditures. We take advantage of the annual nature of the data and estimate the relationship between the pre-election audits and the fiscal year-specific expenditures. Specifically, we estimate variants of the following model:

$$
\Delta \text{gm}_{s,t} = \theta P_{1,s}(A_{m,t} - A_{m,t-4}) + \theta P_{2,s}(A_{m,t},c_{m,t} - A_{m,t-1},c_{m,t-1}) + \beta_{1,s}(c_{m,t} - c_{m,t-1}) + \beta_{2,s}(X_{m,t} - X_{m,t-1}) + (\gamma_{1,s} - \gamma_{2,s}) + (\epsilon_{m,s} - \epsilon_{m,s-4}),
$$

where $$\Delta \text{gm}_{s,t}$$ denotes one of the following differences in the municipal expenditures variables mentioned above in fiscal year $$s$$ around election year $$e$$, for the two fiscal years preceding each election ($$s \in \{e-1, e\}$$) and the three following each ($$s \in \{e+1, e+2, e+3\}$$):

$$
\Delta \text{gm}_{s,t} = \text{gm}_{s,t} - \text{gm}_{s-4,t} \text{ if } s \in \{e-1, e\}, \text{ and}
\Delta \text{gm}_{s,t} = \text{gm}_{s,t} - \text{gm}_{s-8,t} \text{ if } s \in \{e+1, e+2, e+3\}.
$$

If mayors who are under greater scrutiny and as a result engage in no (or less) rent seeking also carry out expenditures in public goods or other activities attractive to voters, then we expect a positive coefficient on $$\theta P_{1,s}$$ ($$\theta P_{1,s} > 0$$), in particular in the fiscal years just preceding an election ($$s \in \{e-1, e\}$$). In contrast, among those mayors who are not disciplined as a result of the audit, we should observe no response in municipal expenditures, or ($$\theta P_{2,s} < 0$$) such that ($$\theta P_{1,s} + \theta P_{2,s} = 0$$).

Panel A of Figure VI illustrates the fiscal year-specific relevant correlation estimates ($$\theta P_{1,s}$$ and $$\theta P_{2,s}$$) from equation (12') (with 90 percent confidence intervals in dashed lines) for the total government budget responses to a pre-election audit. Estimates of $$\theta P_{1,s}$$ for the two fiscal years preceding the election show a significant increase in the government budget among municipalities receiving a favorable pre-election audit; this effect diminishes in size and becomes statistically indistinguishable from zero in the fiscal years following the election. The point estimate indicates a 716.7 USD thousands (24 percent of a standard deviation) increase in government expenditures in the fiscal year preceding the election (significant at 90 percent confidence).

The coefficient estimates for the differential response in municipalities with an additional violation ($$\theta P_{2,s}$$) show a significant differential decrease in governmental expenditures in the two fiscal years preceding the election that also gradually decreases in magnitude and becomes indistinguishable from zero by the second fiscal year following the election (Figure VI, Panel A). The analogous point estimate indicates a 1,992.4 USD thousands (66 percent of a standard deviation) relative decrease in government expenditures in the fiscal year preceding the election (significant at 95 percent confidence).

21 We take an 8-fiscal years difference for the three fiscal years following the election due to the potential endogeneity of the $$g_{m,s-4}$$ term in the four year difference, as this can result from the actions of the incumbent mayor during the term of the first audit.
The resulting overall effect for municipalities with one or two corrupt reported violations by the mayor is statistically indistinguishable from zero.

We also find that municipalities whose mayors under greater scrutiny refrain from corruption also concentrate their expenditures in capital improvements and in compensation to municipality employees – both fairly visible forms of public goods provision and government intervention in the municipality. Again, the increase takes place principally in the fiscal year of the election. Panels B and C of Figure VI illustrate the fiscal year-specific relevant correlation estimates ($\theta_{P1,s}$ and $\theta_{P2,s}$) from equation (12’) (with 90 percent confidence intervals in dashed lines) for these specific budget responses to a pre-election audit. Estimates of $\theta_{P1,s}$ for the election fiscal year show a significant increase in capital improvement expenditures among municipalities receiving a favorable pre-election audit; this effect diminishes in size and becomes statistically indistinguishable from zero in the fiscal years following the election. The point estimate indicates an 89.5 USD thousands (29 percent of a standard deviation) increase in government expenditures in the fiscal year preceding the election (significant at 95 percent confidence). The coefficient estimates for the differential response in municipalities with an additional violation ($\theta_{P2,s}$) again show a significant differential decrease in governmental expenditures in the fiscal year of the election that also gradually decreases in magnitude and becomes indistinguishable from zero in the fiscal years following the election (Figure VI, Panel B). The analogous point estimate indicates a 126.6 USD thousands (41 percent of a standard deviation) relative decrease in government expenditures in the fiscal year preceding the election (significant at 99 percent confidence). The resulting overall effect for municipalities with one or two corrupt reported violations by the mayor is again statistically indistinguishable from zero. Similar patterns to those of overall expenditures hold for personnel expenditures (salaries and benefits) (Figure VI, Panel C).

In contrast, we find that the greater scrutiny from the audits does not significantly modify patterns of social assistance and other expenditures (Panels D and E). Municipalities with favorable pre-election audits do not increase their expenditures in these areas in fiscal years preceding the election. We also find very little evidence of municipal expenditure changes among those municipalities with unfavorable pre-election audits. If these other expenditure areas are less visible forms of public goods provision or government intervention in the municipality, thus less likely to enter into a voter’s calculus when making a voting decision, mayors should not modify expenditures in these areas to the same extent as they would in budget areas that are more visible to voters.

VII. Testing for Alternative Explanations

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

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 again estimate the relationship between the pre-election audits and the fiscal year-specific revenues by source based on model (12'). The increase in revenue available to local governments is mainly through local sources. Among those municipalities under scrutiny with favorable corruption outcomes, we find an average increase in property tax revenues of 351.6 USD thousand (33 percent of a standard deviation), and an average increase in licensing revenues of 242.0 USD thousands (33 percent of a standard deviation). In contrast, we find no evidence of a significant increase in higher-level transfers and other revenues; the point estimate suggests an average reduction of 61.4 USD thousands from this source (37 percent of a standard deviation; row 4, column 7). Therefore, to the extent that the available data allows us to assess this alternate explanation, the evidence is inconsistent with the aforementioned central government transfer responses causing these responses among incumbent mayors, at least in this context.

**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 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. 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 field candidates strategically as a response to information voters receive about the corrupt violations of incumbent politicians. Distinguishing the relative magnitudes of the own reputation building by the incumbent from these additional interactions remains important future work.

VII. Conclusion

Does the disclosure of information about corruption practices induce a sustained reduction in corruption levels? We use publicly released routine audit reports to study this question. The government of Puerto Rico has established a mechanism to routinely conduct municipal government audits, whose findings are then made publicly available and disseminated to media sources. Using a longitudinal dataset of corrupt violations constructed from the audit reports during the period 1987-2006, we compare the subsequent term governments’ levels of reported corruption for municipalities audited at different points in time around an election. The pre-election release of the audit reports led to significant short-term reductions in municipal corruption levels and an increase in incumbent mayors’ electoral accountability. However, municipal corruption levels in the subsequent term are as high in municipalities audited preceding and following the previous election and these are higher concentrated among municipalities shown to have refrained from rent-seeking activities in the first audit. These findings are consistent with a political agency model of reputation dynamics in which rent seeking is increasing in the incumbent’s reputation in equilibrium. Our study thus highlights the role that information plays in enhancing political accountability in the short-run but its plausible negative consequences for politician behavior in the longer-run. In particular, the findings lend empirical support to the argument that, although this may lead to greater social (i.e., voter) welfare, short-term information dissemination policies do not necessarily align politicians’ actions with voters’ preferences in the longer-run.

Our work follows the view that corrupt behavior is a choice made by individuals who make policies and is a rational response to the structure of the political and economic environment, such as political institutions, (the inadequacy of) information, and the structure of networks in a polity (Pande 2007). More generally, it suggests that the rational behavior of politicians in democratic governments is such that it can generate or perpetuate “generalized beliefs”, “norms” or “cultures” of corruption, as it can induce citizens to generate “self-fulfilling prophecies” regarding the corrupt behavior of politicians. This speaks to the debate in the literature on governance and political corruption on whether corruption is a social norm or habit that is much pervasive in low- and middle-income countries, or whether it strictly responds to structure. This and other general queries regarding the determinants of good governance remain important questions for future research.
References


Pande, Rohini (2007). “Understanding Political Corruption in Low Income Countries”. In Handbook of


Przeworski, Adam, Susan Stokes, and Bernard Manin (1999). Democracy, Accountability and


manuscript, Northwestern University.

119(1), 189-221.
FIGURE I: TIMING OF PUBLICATION OF AUDIT REPORTS, 1985-2005
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

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.

PANEL B: INCUMBENT WINS RE-ELECTION | RUNNING
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

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.

PANEL B: POST-ELECTION AUDITS
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 & WIN RE-ELECTION

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 \((t)\).
FIGURE VI:
RELATIONSHIP BETWEEN REPORTED CORRUPTION LEVELS AND MUNICIPAL EXPENDITURES FOR MUNICIPALITIES AUDITED BEFORE AND AFTER ELECTIONS

Pre-Election Audit Coefficient Estimates

Panel A: Government Budget

Panel B: Capital Improvements

Panel C: Salaries & Benefits
FIGURE VI (CONT’D):
RELATIONSHIP BETWEEN REPORTED CORRUPTION LEVELS AND MUNICIPAL EXPENDITURES FOR MUNICIPALITIES AUDITED BEFORE AND AFTER ELECTIONS

Pre-Election Audit Coefficient Estimates
Pre-Election Audit × Violations by Mayor per report Coefficient Estimates

Panel D: Social Assistance

Panel E: Other Expenditures
<table>
<thead>
<tr>
<th>Sample</th>
<th>All Municipalities</th>
<th>Panel A: Number of corruption violations per report</th>
<th>Panel B: Election outcomes</th>
<th>Panel C: Pre-suit incumbent mayor characteristics</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>N</td>
<td>Pre-election audit</td>
<td>Post-election audit</td>
<td>Difference</td>
<td>Pre-election audit</td>
</tr>
<tr>
<td>All violations</td>
<td>2.17 (0.97)</td>
<td>2.17 (0.97)</td>
<td>0.00</td>
<td>2.17 (0.97)</td>
</tr>
<tr>
<td>Background characteristics</td>
<td>2.17 (0.97)</td>
<td>2.17 (0.97)</td>
<td>0.00</td>
<td>2.17 (0.97)</td>
</tr>
<tr>
<td>Mayors who ran for re-election</td>
<td>2.17 (0.97)</td>
<td>2.17 (0.97)</td>
<td>0.00</td>
<td>2.17 (0.97)</td>
</tr>
</tbody>
</table>

TABLE I: CHARACTERISTICS OF THE MUNICIPALITIES
### TABLE I: CHARACTERISTICS OF THE MUNICIPALITIES (CONT’D)

| Sample | All Municipalities | | | | | | Mayors who run for re-election (t) | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Pre-election audit | Post-election audit | Difference (Adjusted) | N | Pre-election audit | Post-election audit | Difference (Adjusted) | N |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| **Panel D: Pre-audit municipality characteristics** | | | | | | | | | | |
| High school education or more (%) | 0.46 | 0.48 | 0.45 | 0.025 | 326 | 0.44 | 0.45 | 0.44 | 0.011 | 232 |
| | [0.09] | [0.09] | [0.08] | (0.010) | | [0.08] | [0.08] | [0.07] | (0.011) | |
| College or more (%) | 0.11 | 0.12 | 0.11 | 0.010 | 326 | 0.10 | 0.11 | 0.10 | 0.009 | 232 |
| | [0.04] | [0.04] | [0.05] | (0.006) | | [0.04] | [0.04] | [0.04] | (0.006) | |
| Household median income (1,000 USD) | 9.17 | 9.65 | 8.51 | 0.885 | 326 | 8.25 | 8.34 | 8.16 | 0.203 | 232 |
| | [2.84] | [3.05] | [2.40] | (0.342) | | [1.77] | [1.84] | [1.70] | (0.257) | |
| Poverty rate | 0.60 | 0.58 | 0.61 | -0.024 | 326 | 0.61 | 0.60 | 0.62 | -0.015 | 232 |
| | [0.10] | [0.10] | [0.09] | (0.012) | | [0.09] | [0.10] | [0.09] | (0.013) | |

**Panel E: Other municipality characteristics**

| | | | | | | | |
| Government budget | 10.06 | 11.61 | 7.63 | 3.05 | 266 | 9.31 | 11.39 | 6.93 | 4.16 | 188 |
| | [18.22] | [21.77] | [10.2] | (2.44) | | [15.42] | [19.29] | [8.79] | (2.41) | |
| Capital improvements | 2.28 | 0.24 | 0.21 | 0.05 | 265 | 0.25 | 0.29 | 0.29 | 0.08 | 200 |
| | [4.35] | [0.44] | [0.43] | (0.07) | | [0.47] | [0.48] | [0.45] | (0.08) | |
| Salaries & benefits | 4.96 | 5.48 | 4.15 | 1.25 | 265 | 4.97 | 5.95 | 3.85 | 1.97 | 188 |
| | [7.44] | [8.59] | [5.14] | (1.11) | | [8.23] | [10.34] | [4.63] | (1.25) | |
| Social assistance | 2.33 | 0.22 | 0.26 | -0.01 | 265 | 0.27 | 0.27 | 0.28 | 0.01 | 98 |
| | [3.91] | [0.34] | [0.46] | (0.06) | | [0.45] | [0.40] | [0.49] | (0.07) | |
| Other expenditures | 3.94 | 4.54 | 3.02 | 1.28 | 265 | 3.82 | 4.89 | 2.60 | 2.10 | 188 |
| | [6.64] | [7.62] | [4.62] | (0.99) | | [6.98] | [8.84] | [3.62] | (1.12) | |
| Property tax | 3.22 | 3.70 | 2.47 | 1.13 | 265 | 3.25 | 4.15 | 2.23 | 1.74 | 188 |
| | [6.75] | [7.79] | [4.65] | (1.08) | | [7.26] | [9.06] | [4.24] | (1.11) | |
| Licensing | 2.00 | 2.30 | 1.53 | 0.60 | 265 | 1.92 | 2.46 | 1.31 | 1.03 | 200 |
| | [4.08] | [4.50] | [3.28] | (0.67) | | [4.27] | [5.19] | [2.80] | (0.64) | |
| Waste disposal | 0.20 | 0.25 | 0.11 | 0.14 | 265 | 0.20 | 0.29 | 0.10 | 0.17 | 188 |
| | [0.72] | [0.89] | [0.33] | (0.12) | | [0.71] | [0.92] | [0.30] | (0.14) | |
| Transfers & other revenue | 3.11 | 3.28 | 2.85 | 0.44 | 273 | 3.13 | 3.57 | 2.65 | 0.91 | 193 |
| | [3.98] | [4.71] | [2.46] | (0.35) | | [4.46] | [5.90] | [1.72] | (0.68) | |
| Unemployment rate | 0.157 | 0.150 | 0.168 | -0.011 | 273 | 0.161 | 0.153 | 0.171 | -0.014 | 193 |
| | [0.051] | [0.046] | [0.057] | (0.007) | | [0.052] | [0.047] | [0.056] | (0.008) | |
### TABLE II:
EFFECTS OF THE (TIMING OF) THE AUDITS ON THE NUMBER OF CORRUPT VIOLATIONS IN CURRENT AUDIT, ELECTORAL OUTCOMES

<table>
<thead>
<tr>
<th>Sample</th>
<th>All Municipalities</th>
<th>Mayors who run for re-election</th>
<th>All Municipalities</th>
<th>Mayors who run for re-election</th>
</tr>
</thead>
<tbody>
<tr>
<td>Pre-election audit</td>
<td>OLS (1)</td>
<td>OLS (2)</td>
<td>OLS (3)</td>
<td>OLS (4)</td>
</tr>
<tr>
<td>Pre-election audit × Num. violations</td>
<td>-1.43*** (0.22)</td>
<td>-0.63*** (0.13)</td>
<td>-0.65*** (0.16)</td>
<td>-1.32*** (0.30)</td>
</tr>
<tr>
<td>Municipality Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Election Year &amp; Municipality FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Pre-election audits F-statistic [p-value]</td>
<td>1.75 [0.18]</td>
<td>3.75 [0.03]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>326</td>
<td>326</td>
<td>326</td>
<td>241</td>
</tr>
<tr>
<td>Mean of dep. variable (controls)</td>
<td>2.17</td>
<td>0.95</td>
<td>0.99</td>
<td>2.07</td>
</tr>
</tbody>
</table>

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

<table>
<thead>
<tr>
<th>Sample</th>
<th>Dependent variables: Number of corrupt violations in subsequent audit (term)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>All Municipalities</td>
</tr>
<tr>
<td></td>
<td>All</td>
</tr>
<tr>
<td></td>
<td>by Mayor /</td>
</tr>
<tr>
<td></td>
<td>Vice-mayor</td>
</tr>
<tr>
<td></td>
<td>OLS (1)</td>
</tr>
<tr>
<td>Pre-election audit</td>
<td>0.15</td>
</tr>
<tr>
<td></td>
<td>(0.26)</td>
</tr>
<tr>
<td>Municipality Controls</td>
<td>Yes</td>
</tr>
<tr>
<td>Election Year &amp; Municipality FEs</td>
<td>Yes</td>
</tr>
<tr>
<td>Mean of dep. variable (controls)</td>
<td>1.62</td>
</tr>
</tbody>
</table>

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 1991-2006 period.
## TABLE IV: HETEROGENEOUS EFFECTS OF THE AUDITS ON THE NUMBER OF CORRUPT VIOLATIONS IN THE SUBSEQUENT AUDIT (TERM)

| Dep. variable: Number of corrupt violations in subsequent audit (term) | All agents |
|-----------------------------|-----------------|-----------------|-----------------|-----------------|-----------------|-----------------|-----------------|-----------------|
|                             | FD/IV (1)       | FD/IV (2)       | FD/IV (3)       | FD/IV (4)       | FD/IV (5)       | FD/IV (6)       | FD/OLS (7)      | FD/OLS (8)      |
| Pre-election audit          | 0.57** (0.281)  | 0.48 (0.32)     | 1.07*** (0.35)  | 1.05*** (0.41)  | 2.38*** (0.92)  | 2.46** (0.983)  | -0.461* (0.258) | -0.494 (0.381)  |
| Pre-election audit × Num. violations | -0.48** (0.21)  | -0.55** (0.23)  | -2.24** (1.04)  | -2.63** (1.100) |                |                | 0.055 (0.148)  | 0.227 (0.475)   |
| Pre-election audit × Num. violations² |               | 0.36* (0.19)    | 0.43** (0.207)  |                |                |                | 0.100 (0.120)  |                |

### Municipality Controls

<table>
<thead>
<tr>
<th>Election Year Fixed Effects</th>
<th>No</th>
<th>Yes</th>
<th>No</th>
<th>Yes</th>
<th>No</th>
<th>Yes</th>
<th>Yes</th>
<th>Yes</th>
</tr>
</thead>
<tbody>
<tr>
<td>Weak identification test</td>
<td>278.4</td>
<td>268.4</td>
<td>77.4</td>
<td>90.0</td>
<td>10.0</td>
<td>9.0</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Pre-election audits F-statistic</td>
<td>9.98</td>
<td>8.05</td>
<td>6.69</td>
<td>6.63</td>
<td>1.81</td>
<td>2.31</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>[p-value]</td>
<td>[0.01]</td>
<td>[0.02]</td>
<td>[0.08]</td>
<td>[0.08]</td>
<td>[0.17]</td>
<td>[0.11]</td>
<td>-</td>
<td>-</td>
</tr>
</tbody>
</table>

### Implied Effects [p-value]

<table>
<thead>
<tr>
<th>Num. violations = 0</th>
<th>1.07</th>
<th>1.05</th>
<th>2.38</th>
<th>2.46</th>
<th>-0.46</th>
<th>-0.49</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>[0.01]</td>
<td>[0.02]</td>
<td>[0.01]</td>
<td>[0.01]</td>
<td>[0.08]</td>
<td>[0.20]</td>
</tr>
<tr>
<td>Num. violations = 1</td>
<td>0.59</td>
<td>0.50</td>
<td>0.50</td>
<td>0.27</td>
<td>-0.41</td>
<td>-0.62</td>
</tr>
<tr>
<td></td>
<td>[0.04]</td>
<td>[0.14]</td>
<td>[0.23]</td>
<td>[0.62]</td>
<td>[0.07]</td>
<td>[0.04]</td>
</tr>
<tr>
<td>Num. violations = 2</td>
<td>0.11</td>
<td>-0.06</td>
<td>-0.67</td>
<td>-1.06</td>
<td>-0.35</td>
<td>-0.55</td>
</tr>
<tr>
<td></td>
<td>[0.76]</td>
<td>[0.89]</td>
<td>[0.28]</td>
<td>[0.14]</td>
<td>[0.20]</td>
<td>[0.13]</td>
</tr>
</tbody>
</table>

### Observations

<table>
<thead>
<tr>
<th>Observations</th>
<th>153</th>
<th>153</th>
<th>153</th>
<th>153</th>
<th>153</th>
<th>153</th>
</tr>
</thead>
</table>

### Std. deviation of dependent variable

| Std. deviation of dependent variable | 2.45 | 2.45 | 2.45 | 2.45 | 2.45 | 2.45 |

### Notes:

Coefficient estimates and standard errors from (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. IVs are the second lag of the dependent variable and its interaction with a pre-election audit in period (t-4). 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 1991-2006 period. 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 V: ROBUSTNESS TESTS - EFFECTS OF THE AUDITS ON THE NUMBER OF CORRUPT VIOLATIONS IN THE SUBSEQUENT AUDIT (TERM)

<table>
<thead>
<tr>
<th>Sample</th>
<th>Dep. variable: Number of corrupt violations in subsequent audit (term)</th>
<th>All Violations</th>
<th>Violations by Mayor or Vice-Mayor</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>FD/IV (1)</td>
<td>FD/IV (2)</td>
<td>FD/IV (3)</td>
</tr>
<tr>
<td>Pre-election audit</td>
<td>0.39</td>
<td>0.85**</td>
<td>2.30**</td>
</tr>
<tr>
<td></td>
<td>(0.35)</td>
<td>(0.43)</td>
<td>(1.05)</td>
</tr>
<tr>
<td>Pre-election audit ×</td>
<td>-0.51*</td>
<td>-2.52*</td>
<td></td>
</tr>
<tr>
<td>Num. violations</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.30)</td>
<td>(1.29)</td>
<td></td>
</tr>
<tr>
<td>Pre-election audit ×</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Num. violations - mayor</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pre-election audit ×</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(Num. violations - mayor)^2</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pre-election audit ×</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Num. violations - others</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pre-election audit ×</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>(Num. violations - others)^2</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

### Notes:
Coefficient estimates and standard errors from (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.
**TABLE VI: ROBUSTNESS TESTS - EFFECTS OF THE AUDITS ON THE NUMBER OF CORRUPT VIOLATIONS IN THE SUBSEQUENT AUDIT (CONT.)**

<table>
<thead>
<tr>
<th></th>
<th>Dep. variable: Number of corrupt violations in subsequent audit (term)</th>
<th>All Violations</th>
<th>Violations referred to Dept. of Justice</th>
<th>Terms in office ≤ 2</th>
<th>All</th>
<th>Mayors who run for re-election</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>FD/IV (1)</td>
<td>FD/IV (2)</td>
<td>FD/IV (3)</td>
<td>FD/IV (4)</td>
<td>FD/IV (5)</td>
<td>FD/IV (6)</td>
</tr>
<tr>
<td>Pre-election audit</td>
<td>0.41</td>
<td>1.19*</td>
<td>2.43*</td>
<td>0.32</td>
<td>0.62*</td>
<td>0.44*</td>
</tr>
<tr>
<td></td>
<td>(0.40)</td>
<td>(0.65)</td>
<td>(1.27)</td>
<td>(0.26)</td>
<td>(0.33)</td>
<td>(0.27)</td>
</tr>
<tr>
<td>Pre-election audit × Num. violations</td>
<td>-0.66**</td>
<td>-2.36*</td>
<td>-0.29**</td>
<td>-0.21</td>
<td>(0.29)</td>
<td>(1.29)</td>
</tr>
<tr>
<td>Pre-election audit × Num. violations^2</td>
<td></td>
<td></td>
<td></td>
<td>0.34</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(0.23)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Municipality Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Election Year Fixed Effects</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Weak identification F-statistic</td>
<td>84.9</td>
<td>23.4</td>
<td>6.3</td>
<td>268.4</td>
<td>91.0</td>
<td>339.8</td>
</tr>
<tr>
<td>Pre-election audits F-statistic [p-value]</td>
<td>5.2</td>
<td>4.1</td>
<td>4.7</td>
<td>3.7</td>
<td>[0.08]</td>
<td>[0.25]</td>
</tr>
<tr>
<td>Implied Effects [p-value]</td>
<td>Num. violations = 0</td>
<td>1.19</td>
<td>2.43</td>
<td>0.62</td>
<td>0.63</td>
<td>Num. violations = 1</td>
</tr>
<tr>
<td></td>
<td>[0.07]</td>
<td>[0.06]</td>
<td>[0.06]</td>
<td>[0.05]</td>
<td>[0.27]</td>
<td>[0.54]</td>
</tr>
<tr>
<td></td>
<td>[0.77]</td>
<td>[0.28]</td>
<td>[0.88]</td>
<td>[0.47]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>106</td>
<td>106</td>
<td>106</td>
<td>153</td>
<td>153</td>
<td>119</td>
</tr>
<tr>
<td>Std. Dev. of dep. variable</td>
<td>2.45</td>
<td>2.45</td>
<td>2.45</td>
<td>1.66</td>
<td>1.66</td>
<td>1.69</td>
</tr>
</tbody>
</table>

**Notes:** Coefficient estimates and standard errors from (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.
## TABLE VII: TESTING FOR MANIPULATION OF THE AUDITING PROCESS

<table>
<thead>
<tr>
<th>Dependent variables:</th>
<th>Audit (1/0)</th>
<th>Num. of reports</th>
<th>Number of all corrupt violations per report in subsequent audit (term)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sample / Model</td>
<td>All / OLS (FD)</td>
<td>All / OLS (FD)</td>
<td>All / IV (FD)</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td><strong>Panel A</strong>: Average Effects</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pre-election audit</td>
<td>-0.096*</td>
<td>-0.159</td>
<td>-0.16</td>
</tr>
<tr>
<td></td>
<td>(0.056)</td>
<td>(0.137)</td>
<td>(0.31)</td>
</tr>
<tr>
<td><strong>Panel B</strong>: Heterogeneous Effects</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Pre-election audit</td>
<td>-0.069</td>
<td>-0.072</td>
<td>-0.187</td>
</tr>
<tr>
<td>(Num. violations)</td>
<td>(0.061)</td>
<td>(0.074)</td>
<td>(0.174)</td>
</tr>
<tr>
<td>Pre-election audit</td>
<td>-0.007</td>
<td>0.070</td>
<td>-0.005</td>
</tr>
<tr>
<td>(Num. violations)²</td>
<td>(0.034)</td>
<td>(0.075)</td>
<td>(0.076)</td>
</tr>
<tr>
<td>Pre-election audit</td>
<td>-0.029</td>
<td>-0.012</td>
<td>0.51**</td>
</tr>
<tr>
<td>(Num. violations)²</td>
<td>(0.019)</td>
<td>(0.061)</td>
<td>(0.21)</td>
</tr>
<tr>
<td><strong>Additional Controls</strong>: Pre-audit ×</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>× Opposition party to Gov.</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
</tr>
<tr>
<td>× Opposition party to Gov. appointing Comptroller</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
</tr>
<tr>
<td>× Win margin in (t-4) election</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
</tr>
<tr>
<td>× Terms in office</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
</tr>
<tr>
<td>× Percent BA degree or higher</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
</tr>
<tr>
<td><strong>Municipality Controls</strong>:</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td><strong>Election Year Fixed Effects</strong>:</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Weak identification F-statistic</td>
<td>-</td>
<td>-</td>
<td>66.8</td>
</tr>
<tr>
<td>Pre-election audits F-statistic</td>
<td>1.12</td>
<td>1.76</td>
<td>0.79</td>
</tr>
<tr>
<td>[p-value]</td>
<td>[0.33]</td>
<td>[0.16]</td>
<td>[0.46]</td>
</tr>
<tr>
<td>Std. dev. of dependent variable</td>
<td>0.71*</td>
<td>0.71*</td>
<td>1.42</td>
</tr>
</tbody>
</table>

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.
TABLE VIII: THE EFFECTS OF THE AUDITS ON POLITICIAN SELECTION, LONG-TERM ELECTORAL OUTCOMES

<table>
<thead>
<tr>
<th>Sample</th>
<th>Elected mayor's earnings (000's) (5 years before election) [2000 and 2004 elections]</th>
<th>Incumbent runs for &amp; wins re-election (period t+4)</th>
<th>Incumbent wins re-election</th>
<th>running (period t+4)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>All Municipalities</td>
<td>Mayors who run for re-election (t)</td>
<td>All Municipalities</td>
<td>Mayors who run for re-election (t)</td>
</tr>
<tr>
<td></td>
<td>OLS (1)</td>
<td>OLS (2)</td>
<td>OLS (3)</td>
<td>OLS (4)</td>
</tr>
<tr>
<td>Pre-election audit</td>
<td>5.72 (9.56)</td>
<td>-5.36 (12.17)</td>
<td>6.68 (11.80)</td>
<td>-4.63 (13.67)</td>
</tr>
<tr>
<td>Pre-election audit × Num. violations</td>
<td>11.78** (5.81)</td>
<td>10.91* (5.74)</td>
<td>-0.043 (0.031)</td>
<td>-0.030 (0.038)</td>
</tr>
<tr>
<td>Municipality Controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Election Year &amp; Municipality FE</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Pre-election audits F-statistic [p-value]</td>
<td>2.66 [0.08]</td>
<td>2.18 [0.12]</td>
<td>1.07 [0.35]</td>
<td>1.17 [0.31]</td>
</tr>
<tr>
<td>Implied Effects [p-value]</td>
<td>-5.4 [0.66]</td>
<td>-4.6 [0.74]</td>
<td>0.08 [0.23]</td>
<td>0.12 [0.13]</td>
</tr>
<tr>
<td>Num. violations = 0</td>
<td>0.13</td>
<td>6.4 [0.51]</td>
<td>6.3 [0.59]</td>
<td>0.04 [0.49]</td>
</tr>
<tr>
<td>Num. violations = 1</td>
<td>3.26 [0.08]</td>
<td>18.2 [0.08]</td>
<td>17.2 [0.16]</td>
<td>0.00 [0.93]</td>
</tr>
<tr>
<td>Num. violations = 2</td>
<td>188</td>
<td>188</td>
<td>188</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>121</td>
<td>121</td>
<td>96</td>
<td>96</td>
</tr>
</tbody>
</table>

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 rackets).
Appendix B: Bias in OLS Estimates of $\theta_{p1}$ and $\theta_{p2}$ in the First-Differenced Dynamic Panel Data Model

Taking first differences of equation (3):

$$c_{m,t+1} - c_{m,t} = \theta_{p1}(A_{m,t} - A_{m,t-1}) + \theta_{p2}(A_{m,t}c_{m,t} - A_{m,t-1}c_{m,t-1}) + \beta_1(c_{m,t} - c_{m,t-1}) + \beta_2(X_{m,t} - X_{m,t-1}) + (\gamma_{t+1} - \gamma_t) + (\epsilon_{m,t+1} - \epsilon_{m,t}),$$

and under the assumption that $A_{m,t}$ is strictly exogenous ($E[A_{m,t}\epsilon_{m,k} | X_{m,t}, \gamma_{t+1}, \alpha_{m}] = 0 \forall k \in \{1,2,\ldots,T\}$), standard arguments imply that:

$$p\lim \theta_{p1,OLS} = \theta_{p1} + \frac{\text{cov}(\Delta \eta_{m,t}^A, \Delta \epsilon_{m,t+1})}{\text{var}(\Delta \eta_{m,t}^A)}$$

and

$$p\lim \theta_{p2,OLS} = \theta_{p2} + \frac{\text{cov}(\Delta \eta_{m,t}^{hc}, \Delta \epsilon_{m,t+1})}{\text{var}(\Delta \eta_{m,t}^{hc})}$$

where $\Delta \eta_{m,t}^A$ and $\Delta \eta_{m,t}^{hc}$ are the residuals from the auxiliary equations,

$$\Delta A_{m,t} = \pi_{10} + \pi_{11}(A_{m,t}c_{m,t}) + \pi_{12}\Delta c_{m,t} + \pi_{13}\Delta X_{m,t} + \Delta \gamma_{t+1} + \Delta \eta_{m,t}^A,$$

and

$$\Delta (A_{m,t}c_{m,t}) = \pi_{20} + \pi_{21}(A_{m,t} - \Delta A_{m,t}) + \pi_{22}\Delta c_{m,t} + \pi_{23}\Delta X_{m,t} + \Delta \gamma_{t+1}^{hc} + \Delta \eta_{m,t}^{hc}.$$  

Since the residual covariances in the asymptotic bias terms above (ignoring the additional regressors) can be expressed as:

$$\text{cov}(\Delta \eta_{m,t}^A, \Delta \epsilon_{m,t+1}) = \text{cov}(\Delta A_{m,t} - \pi_{10} - \pi_{11}(A_{m,t}c_{m,t}) - \pi_{12}\Delta c_{m,t} - \Delta \gamma_{t+1} + \Delta \eta_{m,t}^A)$$

and

$$\text{cov}(\Delta \eta_{m,t}^{hc}, \Delta \epsilon_{m,t}) = \text{cov}(\Delta A_{m,t}c_{m,t} - \pi_{20} - \pi_{21}(A_{m,t} - \Delta A_{m,t}) - \Delta \gamma_{t+1}^{hc} + \Delta \eta_{m,t}^{hc})$$

then:

$$p\lim \theta_{p1,OLS} = \theta_{p1} - \pi_{12} \frac{\text{cov}(\Delta c_{m,t}, \Delta \epsilon_{m,t+1})}{\text{var}(\Delta \eta_{m,t}^A)}$$

and

$$p\lim \theta_{p2,OLS} = \theta_{p2} - \pi_{22} \frac{\text{cov}(\Delta c_{m,t}, \Delta \epsilon_{m,t+1})}{\text{var}(\Delta \eta_{m,t}^{hc})}.$$  

To determine the signs of $\pi_{12}$ and $\pi_{22}$, we can rewrite the first auxiliary regression as:

$$\Delta (A_{m,t}c_{m,t}) = -\frac{\pi_{10}}{\pi_{11}} + \frac{1}{\pi_{11}} \Delta A_{m,t} + \frac{\pi_{12}}{\pi_{11}} \Delta c_{m,t} - \frac{1}{\pi_{11}} \Delta \eta_{m,t}^A,$$

and since

$$\Delta (A_{m,t}c_{m,t}) = (\Delta A_{m,t})c_{m,t} + (\Delta c_{m,t})A_{m,t-1}$$

$$= (\Delta A_{m,t})c_{m,t-1} + (\Delta c_{m,t})A_{m,t-1} + (\Delta A_{m,t})(\Delta c_{m,t}),$$
we can characterize the signs of the projection coefficients on the asymptotic bias terms. Specifically, since $A_{m,t-1} \geq 0$ and $c_{m,t-1} \geq 0$ by the definition of the variables, then:

$$
\pi_{12} = \frac{-A_{m,t-1}}{c_{m,t-1}} \leq 0 \quad \forall \; c_{m,t-1} > 0,
$$

and

$$
\pi_{22} = A_{m,t-1} \geq 0.
$$

In combination with the fact that $\text{cov}(\Delta c_{m,t}, \Delta e_{m,t+1}) < 0$, this implies that:

(A2)

$$
\begin{align*}
\text{plim} \theta_{p1, OLS} &= \theta_{p1} - \pi_{12} \frac{\text{cov}(\Delta c_{m,t}, \Delta e_{m,t+1})}{\text{var}(\Delta \eta_{m,t}^A)} < \theta_{p1} \\
\text{plim} \theta_{p2, OLS} &= \theta_{p2} - \pi_{22} \frac{\text{cov}(\Delta c_{m,t}, \Delta e_{m,t+1})}{\text{var}(\Delta \eta_{m,t}^A)} > \theta_{p2}.
\end{align*}
$$

The $\theta_{p1}$ 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 $\theta_{p2}$ 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), and the theory predicts that $\theta_{p1} > 0$ and $\theta_{p2} < 0$. Therefore, the OLS estimate of equation (3′) gives a downward biased estimate of the $\theta_{p1}$ coefficient and an upward biased estimate of the $\theta_{p2}$ coefficient.