

## Monitoring Corruptible Politicians<sup>†</sup>

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*Does monitoring corrupt activities induce a sustained reduction in corruption? Using longitudinal data on audits of municipal governments in Puerto Rico, we show corruption is considerably lower in municipalities with timely audits—before elections. However, these municipalities do not exhibit decreased levels of corruption in subsequent audits, even while mayors in these benefit from higher reelection rates. Our results suggest that audits enable voters to select responsive but corruptible politicians to office. Audit programs must disseminate results when they are most relevant for voters—shortly before an election—and ensure that these programs are sustained, long-term commitments. (JEL D72, H83, K42, O17)*

In a well-functioning representative democracy, citizens select publicly motivated and competent politicians to administer public affairs and hold them accountable for their performance. To succeed in these tasks, citizens must have appropriate information about candidates' characters, abilities, and actions while in office (Manin, Przeworski, and Stokes 1999; Besley 2006). Accordingly, a growing body of research finds that voters' access to evaluations of politician performance enhances government responsiveness, reduces corruption and rent-seeking behaviors, and promotes electoral accountability in the short run.<sup>1</sup> However, it is not well understood whether monitoring and information dissemination policies can generate a *sustained*

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<sup>1</sup>For evidence regarding government responsiveness, see, e.g., Besley and Burgess (2002) and Björkman and Svensson (2009); regarding corruption and rent-seeking behaviors, see, e.g., Reinikka and Svensson (2005) and Olken (2007); regarding electoral accountability, see, e.g., Ferraz and Finan (2008) and Banerjee et al (2011).

reduction in rent-seeking. Whether this is the case depends on the dynamic selection and incentive effects of this information. On one hand, if this information helps voters distinguish honest politicians from their corrupt counterparts, audits should lead to a sustained decrease in the level of corruption in future terms. If, in contrast, the information helps voters select competent but corruptible politicians, there need not be dynamic effects of monitoring on the level of corruption.<sup>2</sup> This paper presents strong, but context-specific, evidence that monitoring corrupt acts does not lead to a sustained reduction in corruption.

We document the effects of monitoring corrupt actions in Puerto Rico's municipal governments on short- and long-term levels of corruption. We take advantage of a unique setting that provides us with the opportunity to examine such relationships. The government of Puerto Rico has established an independent body that systematically conducts municipal government audits, the findings of which are made publicly available and disseminated to media sources. This allows us to construct a longitudinal dataset of the extent of corruption in all municipal governments during the period 1987–2005. Our empirical strategy relies on two features of the setting. First, audit reports released in the period leading up to an election—which we term “timely audits”—are more likely to inform on the incumbent mayor's activities than reports published shortly after an election due to a high independent turnover rate of politicians.<sup>3</sup> Second, municipalities are audited in a pre-established and fixed order, making the timing of audits and their assignment into timely and untimely groups plausibly exogenous. Comparing levels of reported corruption across these timely and untimely audit groups of municipalities, we measure the effect of timely audits on contemporaneous and future levels of corruption.

We find that timely and foreseeable audits induce a significant short-term reduction in municipal corruption levels of approximately 67 percent, as well as an increase in incumbent mayors' electoral accountability. These findings are remarkably consistent with the *short-run* disciplining and sanctioning effects of auditing programs found in previous field experimental studies.<sup>4</sup> However, municipal corruption levels in the subsequent round of audits are, on average, *the same* across timely and untimely audit municipalities. In contrast to this lack of effect of timely audits on future corruption, we find that incumbent reelection rates in the subsequent election are significantly higher in municipalities in which there was an earlier timely audit. All of these observed effects are strongest in municipalities in which elections

<sup>2</sup>The effects of information provision in political agency models can vary according to the assumptions made (see Besley 2006 for a survey and discussion). The claims made here contrast a pure selection model—in which politicians are either honest or corrupt—and models in which moral hazard is the main characteristic of the agency relationship. See Fearon (1999) for a defense of the pure selection view. Ferejohn (1986) is the standard reference for moral hazard models of elections.

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

<sup>4</sup>We use the terms disciplining, sanctioning, and electoral accountability effects interchangeably to refer to the change in politician behavior induced by voters punishing bad behavior and rewarding good at the ballot box. The short-term responsiveness and sanctioning effects documented in this paper are consistent with the experimental findings in Olken (2007) and Ferraz and Finan (2008), respectively.

are competitive (i.e., there is some historical alternation of parties), supporting the view that electoral accountability is an important mechanism generating our results.

Our findings reflect a combination of sanctioning and selection effects. Timely audits represent a temporary increase in monitoring relative to municipalities that receive an untimely audit. Because timely audits are foreseeable, politicians may alter their behavior in anticipation of them, fearing that they will lose the upcoming election should they fail to do so. Therefore, short-run variation in observable outcomes can be attributed to more powerful incentives to refrain from corruption—a *sanctioning effect*. However, because the improvement in monitoring is predetermined and temporary, long-run variation cannot be due to differences in monitoring.<sup>5</sup> Rather, an increase in monitoring makes some types of politician more likely to be reelected than others, altering the long-run distribution of types of incumbent politicians in timely versus untimely audit municipalities—a *selection effect*. In this light, our results suggest that while timely audits are an effective means of providing incentives to incumbent politicians to refrain from corrupt actions, they do not ensure that future incumbents will not be corruptible.

Our empirical findings are relevant for policy in several ways. Most directly, our results suggest that periodic but predictable audits are not sufficient to persistently deter corrupt behavior. Audit programs must be timely, sustained, and long-term commitments in order to be effective. Moreover, the patterns we document underscore the importance of disseminating audit results at the time when they are most relevant for voters, shortly before an election. Finally, care must be taken when altering the prevailing dynamic linkages between information and incentives; as noted by Niehaus and Sukhtankar (2013), audit programs may be effective in the short term because the prospect of future rents makes good behavior today worthwhile, so that an efficiency wage may be required to incentivize sustained good behavior (Becker and Stigler 1974). While our negative result on the long-term effects of audits on corruption need not apply to alternative (e.g., randomized) audit schemes, it demonstrates that the persistence of these effects cannot be assumed.

The study contributes to the growing empirical literature documenting how electoral accountability, and monitoring in particular, influences political corruption.<sup>6</sup> Most notably, in a series of papers, Ferraz and Finan (2008, 2011) use similar 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, they show that electoral accountability is enhanced when information about corrupt practices in audited municipalities is publicized, as well as the extent to which reelection incentives affect political corruption in the short run.<sup>7</sup> Using a randomized experiment in Indonesian villages, Olken (2007) analyzes whether different monitoring mechanisms reduce corruption

<sup>5</sup>These predetermined and temporary audits stand in contrast to settings with random audits (e.g., Olken 2007; Ferraz and Finan 2008). We discuss the distinct implications in Section IIIA (Research Design).

<sup>6</sup>Strömberg (2004), Gentzkow (2006), and Gentzkow, Glaeser, and Goldin (2006) provide historical evidence of the consequences of media access on political behaviors. Besley and Burgess (2002) show that newspaper circulation affects the responsiveness of state governments in India to negative shocks to food production and flooding.

<sup>7</sup>Recent evidence based on local and legislative elections in India, Mexico, Uganda, and Brazil is mixed (Banerjee et al. 2011; Chong et al. 2015; Humphreys and Weinstein 2012; de Figueiredo, Hidalgo, and Kasahara 2014). For a summary of the literature, see Pande (2011) and Olken and Pande (2012). For evidence on the policy consequences of reelection incentives, see Besley and Case (1995) and List and Sturm (2006).

in infrastructure projects, and finds that a top-down auditing scheme is effective in decreasing corruption. Finally, Niehaus and Sukhtankar (2013) present evidence of dynamic incentives for the corrupt behaviors of Indian bureaucrats. Our paper contributes to the literature by providing the first evidence (to our knowledge) on the diverging long- and short-run impacts of monitoring on political corruption.

Our empirical results also have intriguing implications for the theory of political agency (see Besley 2006 and Ashworth 2012 for reviews of the theoretical literature and its connection with recent empirical work). The short-lived effect of monitoring on the level of corruption which we document is consistent with a broad class of models, including pure moral hazard models of electoral politics in which all politicians are identical. However, the incumbent's advantage in the subsequent election suggests that there is political selection at play after all. Furthermore, the lack of a sustained reduction in corruption contradicts the predictions of the most commonly used models of political agency with heterogeneous politicians, in which information enables voters to reelect politicians based on their honesty, integrity, or alignment with voter preferences.<sup>8</sup> Alternative political agency models that combine selection and sanctioning are needed to interpret our empirical results. We explore one such model based on the work of Banks and Sundaram (1993) which views all politicians as being opportunistic but who differ in their ability or competence. The resulting predictions are consistent with our particular combination of sanctioning and selection effects.<sup>9</sup>

Our work is related to other empirical investigations of sanctioning and selection effects in political settings. Alt, Bueno de Mesquita, and Rose (2011) use variation in gubernatorial term limits across US states to identify sanctioning and selection effects in fiscal policy. Gagliarducci and Nannicini (2012) use a regression discontinuity approach that exploits population-based variation in the pay of Italian mayors to tackle these questions. Both papers report evidence consistent with valence-based selection effects being the main driver of differences in fiscal policy.<sup>10</sup> Studying legislators in Brazilian municipalities, Ferraz and Finan (2011) find that higher wages induce positive selection in the candidates choosing to run for office and improved performance of incumbents. They also present suggestive evidence that the effect of wages on performance comes from an incentive rather than a selection effect. Our work complements the literature by documenting sanctioning and selection effects on corruption in a novel way.

The paper is organized as follows. Section I provides background on Puerto Rico's municipal government system and auditing program. We follow with a description of the data in Section II. Section III discusses the study's research design and the main identifying assumptions. We present the central empirical results of the paper in Section IV. Section V discusses implications for theoretical models of politics, and we present robustness analysis in Sections VI and VII. The paper concludes in Section VIII.

<sup>8</sup>Some prominent examples include the models in Coate and Morris (1995), Fearon (1999), and Besley (2006).

<sup>9</sup>The model and its predictions are discussed in Section V and developed in full in online Appendix D.

<sup>10</sup>These results offer an interesting contrast with our findings, which are consistent with selection of competent but opportunistic politicians. The difference may be due to differences in social capital or norms, or different incentives for virtuous citizens to enter politics. Alternatively, at least in the case of Alt, Bueno de Mesquita, and Rose (2011), they may be due to differences in the nature of the positions studied; governors make more high-level decisions while a mayor's work is more administrative. A third possibility is that sanctioning and selection effects differ by outcome variable (e.g., fiscal policy versus corruption).

## I. Background

### A. *Municipal Government Administration and Politics*

Municipal governments in Puerto Rico are the level of government closest to citizens. A mayor and a local assembly govern the municipality; these officials are elected for a four-year term following the Commonwealth (and US federal) government electoral cycle.<sup>11</sup> Mayors and municipal council members do not face term limits. In fact, mayors from municipalities where their party is very dominant tend to have high reelection rates. Also, although the local assembly is usually under the control of the dominant party, the law guarantees some representation for minority parties (i.e., a small number of seats for the party that ended in second place, one seat for the party in third place). Minority assembly members usually carry out oversight work, exposing waste and corruption. The mayor appoints the top management of the municipality.

Although municipal governments possess a greater degree of autonomy than counties and cities in the United States, their sphere of influence is somewhat more limited. The bulk of the services they provide are the construction and maintenance of infrastructure, solid waste management, and public health services. There is heterogeneity in municipalities' fiscal autonomy, both in their ability to raise tax revenues and in their expenditure decisions.<sup>12</sup>

### B. *The OCPR Municipal Government Auditing Program*

The Office of the Comptroller of Puerto Rico (OCPR) is an autonomous government agency created by the 1952 Constitution of the Commonwealth of Puerto Rico. Its mission is to "audit the property and public funds transactions with independence and objectivity to determine if they have been done in accordance to the law[, and] promote the effective and efficient use of the government resources [...]" (Office of the Comptroller 2003). To achieve its objectives, the OCPR periodically audits state-level government agencies and public corporations, including the legislative and judicial branches, as well as municipal governments.

The OCPR has been carrying out audits on municipal governments and generating and disseminating reports uninterruptedly since 1953. Once a municipality is to be audited, the OCPR sends a team of auditors to gather preliminary information on a subset of activities and transactions that have taken place in the time period since the previous audit. Following this preliminary audit, a team of approximately ten OCPR auditors is sent to the municipality to examine these accounts

<sup>11</sup> 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.

<sup>12</sup> 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-à-vis the central government and allowed them a greater role in the social and economic development, as well as the spatial planning, of their territories. Thus, once the municipal reform laws became effective some municipalities began to assert a greater role in education and law enforcement, areas previously reserved for the central government. In practice, the degree of autonomy and sphere of action that each municipality has is related to its size. Large municipal governments with active mayors such as San Juan (the capital), Guaynabo, Bayamón, and Caguas have asserted a significant degree of autonomy. Smaller municipalities with access to fewer resources are still significantly more dependent on the central government.

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 team prepares a preliminary report. This report is shared with municipality officials (i.e., the mayor and top management) to provide them with an opportunity to contest its findings. Once the response is received and evaluated, a final report is issued and disseminated to the public and to the media through press conferences. More recently, reports are also being posted on the Internet. The OCPR may publish multiple reports on a municipality for one auditing period depending on the size and complexity of the municipal government.

A number of measures are taken to minimize potential biases in the conduct of the audits and in the dissemination of their findings. First, there is a constitutionally defined objective to provide the OCPR with a substantial degree of autonomy from the rest of the central government in order to isolate the agency from undue external interference. To help achieve this, the comptroller is appointed by the PR governor for a ten-year term.<sup>13</sup> Second, the OCPR is accountable to the state legislature. Since the agency's activities are focused on the executive branch, this gives it an additional layer of protection from undue influence. Third, the auditors are hired based on a competitive public examination, earn highly competitive salaries, and receive extensive training. Finally, in order to reduce conflicts of interest, individual auditors are precluded from participating in audits of their municipality of residence.

According to the OCPR's constitutive legislation, municipal governments ought to be audited every other fiscal year. However, due to the OCPR's resource constraints, there may be some delay in the timing of the audit. Importantly for our design, municipalities are audited following a prespecified order (established in the 1950s). Once all municipalities have been audited, a new auditing round takes place following the same prespecified order.

All 78 municipalities were audited multiple times during our period of interest (1987–2005). The timing of the dissemination of the reports is depicted in Figure 1. There is a tendency to publish reports at the end of the central government's fiscal year (in June), as well as a tendency to publish more reports in recent years.<sup>14</sup> There is no significant tendency for the OCPR to publish a disproportionate number of reports in the months preceding an election (August through October) (panel A of Figure 1). There is also no evidence of bias in the publication dates of reports for municipalities in which the incumbent mayor is in the opposition to the governor in office or to the party of the governor who appointed the comptroller (panel B). This serves as *prima facie* evidence that the timing of the audits can be considered predetermined and that the agency does not time the dissemination of findings to influence electoral results.

<sup>13</sup>The appointment requires the advice and consent of the members of both legislative chambers. In addition, the person can only be removed from office while serving the term by an impeachment procedure: Third Article, Section 22 of the Constitution of the Commonwealth of Puerto Rico.

<sup>14</sup>For the 1997–2000 and 2001–2004 terms, almost all the municipalities were audited at least once. José M. Díaz Saldaña, the comptroller appointed in October 1997, made a point to audit all municipalities at the beginning of his term, a fact clearly shown by the data.

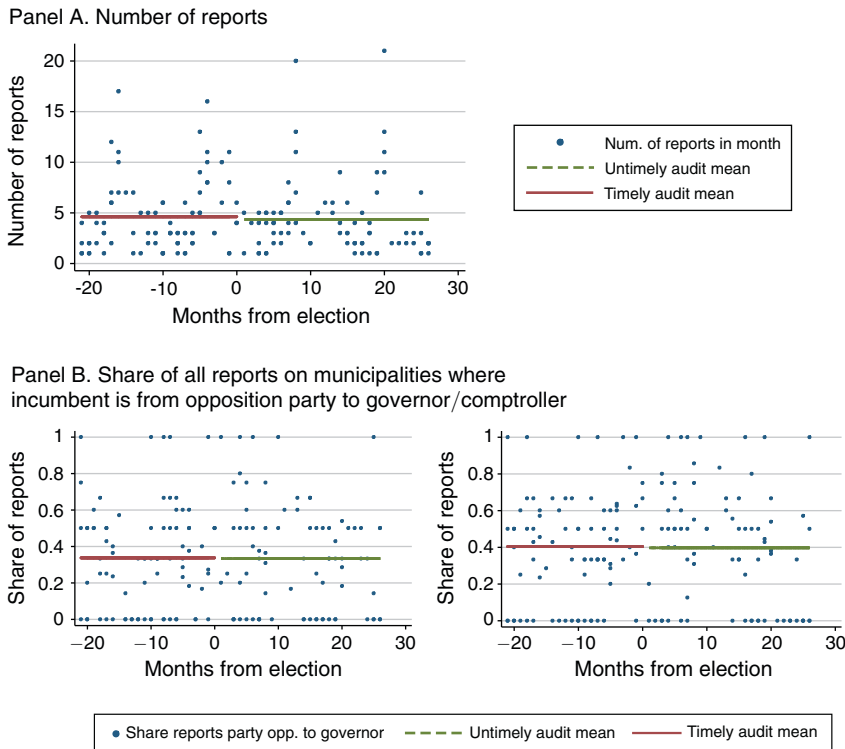


FIGURE 1. PUBLICATION OF AUDIT REPORTS, TIMING AND OUTCOMES, 1987–2005

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

Corruption in municipal governments in Puerto Rico takes diverse forms. Corruption schemes are typically based on a combination of fraud in procurement, the use of fake receipts (i.e., “phantom” firms), the illegal hiring of employees, and overinvoicing the value of products or services. In addition, the audit reports suggest that some individuals simply divert resources for personal purposes. Following the existing literature, we combine these into a single measure (see Section IIA).

Some examples will help illustrate the types of irregularities uncovered by the audits.<sup>15</sup> In a report on the municipality of Maunabo during February to March 1997, contracts for the pavement and maintenance of roads summing up to approximately US\$138,000 were partitioned into four separate projects in order to avoid having to carry out a public auction.<sup>16</sup> Moreover, the auditors were unable to confirm the authenticity of other quotes submitted for the projects. We classified this finding as an instance of fraud in procurement.

<sup>15</sup>For additional examples and details of the audit report findings, see excerpts from these in online Appendix A.

<sup>16</sup>The 1991 Municipal Government Law (*Ley de Municipios Autónomos del Estado Libre Asociado de Puerto Rico de 1991*, Law No. 81) establishes that for any project exceeding US\$40,000, the municipal government must carry out a public auction.

Other examples of corruption in Maricao and Hormigueros illustrate instances of overinvoicing. In October 1998, the mayors in both municipalities formalized contracts for the collection and disposal of debris resulting from the damages caused by Hurricane Georges (in September 1998) for an estimated cost of US\$4.20 million and US\$3.69 million, respectively. The OCPR reported evidence from the Federal Emergency Management Agency (FEMA) and the US Army Corps of Engineers of overinvoicing in both cases. This represented overinvoicing by approximately US\$2.94 million and US\$0.75 million, respectively. The OCPR referred the violations to the PR Department of Justice (DOJ). As a consequence, the former (two-term) mayor of Hormigueros was convicted on extortion and bribery charges for requesting and receiving US\$100,000 in kickbacks from the owner of the contracting firm. In contrast, the mayor of Maricao, in his third term, was reelected in 2004 following the dissemination of the (untimely) audit report in 2001.

News on the findings from the audit reports is routinely reported in the island-wide press. The main sources are OCPR press conferences and releases as well as opposition candidates' campaigns. Although we do not have direct evidence showing that voters learned about the audit reports, anecdotal evidence suggests that the information from the audits did reach voters. For instance, an article published on September 25, 2008 (40 days preceding the 2008 election) in a major newspaper reported on the outcome of a recent audit of the municipality of San Juan. Specifically, the report highlighted that Jorge Santini (the mayor) and the municipality's finance team did not appropriately administer the municipality's finances and incurred extravagant/unnecessary expenditures to boost the mayor's image. The report was used by Ferdinand Pérez (the opposition candidate) to declare that Santini was "a disaster as an administrator." The statement was later challenged by the incumbent mayor (Hopgood Dávila 2008). In spite of this finding of plausible misuse of funds (not classified as corrupt), Santini—a mayor in his second term—was reelected for a third term.

## II. Data

### *A. Measures of Corruption Based on the Audit Reports*

The main data sources for the study are the municipal audit reports published by the OCPR between 1987 and 2005.<sup>17</sup> Each report contains a list of findings and a detailed description of each. Each reported finding consists of a detailed explanation of a situation, the individuals involved (if identifiable), and the reason why it is considered a finding.

Because the OCPR cannot officially classify findings as instances of corruption, we created a code that specified whether the finding constituted an act of corruption.<sup>18</sup>

<sup>17</sup>The structure of the audits and the audit reports are consistent starting from the mid-1980s onward. Therefore, we restrict our sample to jurisdictions with audit reports starting in 1987. There were two comptrollers during this period: Ileana Colón Carlo (1987–1997) and Manuel Díaz Saldaña (1997–2010).

<sup>18</sup>Before we began the coding process, the three research assistants were given extensive training in content analysis, coding, and the details of the audit reports. We also ran tests for inter-coder and intra-coder reliability. The process continued until coder reliability was at least 0.9. The same coders worked with the reports throughout the project. Finally, a fourth research assistant examined the data to check for any errors.



We operationalize corruption as an act by any municipal employee which led to a *personal* financial or political benefit.<sup>19</sup> 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 would not (unless the report stated that it involved the cover-up of a corrupt violation).

To construct measures of corruption, we follow Ferraz and Finan (2008, 2011) and combine these indicators by summing up the number of times each one of these irregularities appears. Because the OCPR may publish multiple reports on a municipality during one auditing period depending on the size or complexity of the municipal government, we normalize our measures by the number of reports published in that auditing period. In addition to the total number of corrupt violations, we look at corrupt violations by the mayor or vice-mayor and those referred to the PR DOJ. We also construct a second set of measures: the proportion of findings attributed to the mayor/vice-mayor or referred to the DOJ that are classified as corrupt, relative to the total number of findings of corruption or mismanagement in the reports. These measures capture the incidence of corruption relative to overall mismanagement or waste in the municipality.<sup>20</sup>

We use the complete sample of municipalities with available audit data across periods in the empirical analysis. Panel A of Table 1 presents the means (and standard deviations) of these audit outcome variables, overall and by timely/untimely audit status. Sixty-nine percent of audit reports contain at least one corrupt violation. The audits report on average 1.38 corrupt violations per report, and there is substantial heterogeneity in the number of instances of corruption (standard deviation = 1.79). Approximately 42 percent of these findings (0.58 violations) are attributed to the mayor or vice-mayor, and 47 percent (0.65 violations) are referred to the DOJ, on average. Examining the second set of measures, we find that a nonnegligible share of findings attributed to the mayor or vice-mayor or referred to the DOJ—19 and 27 percent, respectively—are classified as corrupt. Panel B reports other relevant characteristics of the audits, such as the number of reports resulting from the audit, the start and end of the audit period (and time span) covered by the reports.

To measure the extent to which the OCPR follows the prespecified order of audits, we construct a set of variables that capture deviations from the expected order. To determine this, we would ideally use information on the start date of each audit. Since the reports do not provide this information, we use instead the date of dissemination of each audit report. We can use this to ascertain to a first approximation whether the OCPR follows the predetermined order in practice. Since there are 78 municipalities, we should observe 77 audits conducted in between each pair of audits for a given municipality.

We estimate that there are on average 76.8 reports of other municipalities between each pair of own municipality reports. Specifically, we construct the average difference from the expected number of other municipality reports (77) between each pair of audits for each municipality (for a sample of 220 sequential

<sup>19</sup>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.

<sup>20</sup>This relates to the distinction between active and passive waste highlighted in Bandiera, Prat, and Valletti (2009).

TABLE 1—CHARACTERISTICS OF THE AUDIT REPORTS

	All (1)	Timely audit (2)	Untimely audit (3)	Difference (adjusted) (4)	Observations (5)
<i>Panel A. Audit outcomes</i>					
Any corrupt violation [1/0]	0.69 [0.46]	0.62 [0.49]	0.78 [0.41]	−0.13 (0.05)	326
Number of all corrupt violations per report	1.38 [1.79]	0.80 [0.99]	2.17 [2.26]	−1.36 (0.21)	326
Number of violations by mayor/vice-mayor	0.58 [0.99]	0.31 [0.57]	0.95 [1.28]	−0.60 (0.12)	326
Number of violations referred to Dept. of Justice	0.65 [1.20]	0.40 [0.81]	0.99 [1.52]	−0.67 (0.16)	326
Share of findings, attributed to mayor/ vice-mayor, classified as corrupt	0.19 [0.28]	0.16 [0.29]	0.22 [0.26]	−0.06 (0.04)	326
Share of findings referred to Dept. of Justice classified as corrupt	0.27 [0.37]	0.22 [0.35]	0.34 [0.39]	−0.11 (0.05)	326
<i>Panel B. Other audit characteristics</i>					
Number of audit reports	1.88 [1.15]	2.16 [1.32]	1.49 [0.72]	0.77 (0.13)	326
Start of audit period in reports (years from election)	6.31 [2.59]	6.84 [2.70]	5.59 [2.25]	1.22 (0.25)	326
End of audit period in reports (years from election)	1.24 [1.60]	1.56 [1.57]	0.80 [1.54]	0.43 (0.16)	326
Time span of audited period (years)	5.08 [2.59]	5.29 [2.76]	4.79 [2.32]	0.78 (0.24)	326
Deviation from predetermined order of reports					
Mean of deviation from expected number of reports	−0.16 [16.91]	1.04 [18.24]	−1.48 [15.28]	3.38 (2.64)	220
Mean of absolute value of deviation from expected number of reports	14.30 [10.88]	15.79 [11.03]	12.66 [10.52]	0.70 (1.88)	220

Notes: Standard deviations of variables are reported in brackets. Differences estimated in ordinary least squares (OLS) regression models, regression-adjusted for municipality and electoral term fixed effects. Robust standard errors of mean differences are reported in parentheses.

municipal audit pairs).<sup>21</sup> The mean of this variable is −0.16, which suggests that the rule is followed closely, on average (panel B of Table 1). To measure the extent of the deviation from the rule, we construct the average of the *absolute value* of the deviation from the expected number of audits from other municipalities between a pair of audits for each municipality (mean = 14.3 reports).<sup>22</sup> There is however measurement error in this measure of variability, since there can be some discrepancy between the order of audit start dates and report dissemination dates due to the size of the municipal governments and thus the complexity of the audits—which can lead us to overestimate the true deviation from the rule.<sup>23</sup> We evaluate whether these deviations are a threat to our identification strategy in Section IIIA.

<sup>21</sup> Specifically, our first measure is defined as  $Dev_{mt} = (N_{mt}^{oth\ reports} - 77)$ , where  $N_{mt}^{oth\ reports}$  denotes the number of audits of other municipalities disseminated between two audits for municipality  $m$  in election cycle  $t$ . In the case that there are multiple audits in one cycle, we take the average of this measure across each pair of audits.

<sup>22</sup> Our second measure is defined as  $AbsDev_{mt} = |N_{mt}^{oth\ reports} - 77|$ , where  $N_{mt}^{oth\ reports}$  is defined as above.

<sup>23</sup> The results are robust to using an alternative set of measures based on the maximum instead of the average of the deviations.

## B. Other Data Sources

We employ additional data available from the PR State Electoral Commission (CEE) containing the results of municipal and statewide ballots for the 1988, 1992, 1996, 2000, and 2004 elections. These data allow us to construct measures such as whether the incumbent mayor runs for reelection in the general election, whether he/she is reelected, the vote share and win margin for the election, his/her political party affiliation, whether he/she is in the opposition to the party in power at the state level, and his/her terms in office.<sup>24</sup> We also use annual municipal government budget data for the fiscal years 1991–1992 through 2007–2008 from the PR Office of the Commissioner of Municipal Affairs. To capture underlying variation in municipal characteristics, we rely on the 1990 US census of population for Puerto Rico. We use measures of the proportion of adults (ages 25 and over) with schooling attainment levels of high school education or more, and with a college education or more, as well as the municipality's household median income and poverty ratio for 1989. Finally, we use information on municipality-level annual unemployment rates from the PR Department of Labor.

We present descriptive statistics for these outcome and control variables in Table 2. Panel A reports various electoral outcomes: whether the incumbent runs for reelection and whether the incumbent party wins (unconditionally), and the incumbent mayor's reelection rate conditional on running for reelection. Most salient is the fact that both incumbent mayor reelection rates (conditional on running) and overall party success rates are low in this context, at 35 and 33 percent, respectively. This is arguably due to strong party popularity effects at the state level.

Panel B reports other political characteristics of incumbent mayors running for reelection. An approximately equal number of mayors in the sample are affiliated with the New Progressive Party (NPP) or Popular Democratic Party (PDP), and although only 33 percent on average are affiliated to the party in opposition to that of the state-level executive (again suggestive of significant party popularity/coattail effects), approximately 44 percent are in the opposition to the party of the governor who appointed the incumbent comptroller. Incumbent mayors have been in office for approximately 1.15 terms on average. Nonetheless, these measures mask a great deal of heterogeneity in the safety of municipal seats. There is substantial variation in incumbent mayors' terms in office: 58 percent have been in office for two or more terms and 35 percent for three or more terms.<sup>25</sup> Moreover, the incumbent mayor's win margin in the previous election is on average 11 percentage points. We capture this heterogeneity in seat safety by constructing a summary measure of party incumbency advantage: an indicator variable equal to 1 if the party has controlled the mayoral seat for the past three terms and zero otherwise. Based on this measure, a significant proportion of seats (55 percent) have a strong party incumbency advantage. In panels C and D, we present summary statistics for municipality and

<sup>24</sup>We compiled a dataset of incumbent mayors' publicly available state-level income tax returns for the four-year period preceding each of the 2000 and 2004 elections. All candidates are required by law to submit these documents to the CEE in order to be certified, and they subsequently become part of the public record. We use these data to examine, *for this subsample*, whether the audits induce positive or negative selection of politicians based on their possible pre-incumbency earnings—five years before the relevant election. These results are more suggestive as they cover a limited sample period—see online Appendix C for details.

<sup>25</sup>A number of mayors in the sample (e.g., in the municipalities of Bayamón, Carolina, and Manatí) are known for having been in office for five or more terms.

TABLE 2—CHARACTERISTICS OF THE MUNICIPALITIES

	All (1)	Timely audit (2)	Untimely audit (3)	Difference (adjusted) (4)	Observations (5)
<i>Panel A. Electoral outcomes</i>					
Incumbent runs for reelection (1/0)	0.74 [0.44]	0.72 [0.45]	0.76 [0.43]	-0.03 (0.06)	326
Incumbent party wins (1/0)	0.33 [0.47]	0.35 [0.48]	0.31 [0.46]	-0.05 (0.05)	326
Incumbent mayor wins l running (1/0)	0.35 [0.48]	0.37 [0.48]	0.32 [0.47]	-0.11 (0.06)	241
<i>Panel B. Incumbent mayor characteristics</i>					
Mayor, member of PNP (1/0)	0.51 [0.50]	0.52 [0.50]	0.49 [0.50]	0.09 (0.06)	326
Member of opposition party to governor (1/0)	0.33 [0.47]	0.35 [0.48]	0.30 [0.46]	0.04 (0.06)	326
Member of opp. party to governor appointing comptroller (1/0)	0.44 [0.50]	0.47 [0.50]	0.41 [0.50]	0.06 (0.07)	326
Terms in office	1.15 [1.24]	1.19 [1.36]	1.09 [1.07]	0.07 (0.14)	326
Mayor's win margin in previous election	0.11 [0.09]	0.11 [0.09]	0.10 [0.09]	0.00 (0.01)	326
Party incumbency advantage (1/0)	0.55 [0.50]	0.57 [0.50]	0.52 [0.50]	0.04 (0.06)	326
<i>Panel C. Pre-audit municipality characteristics</i>					
Share of pop. high school education or more (1990)	0.44 [0.08]	0.44 [0.07]	0.44 [0.08]	0.01 (0.01)	326
Share of pop. college education or more (1990)	0.10 [0.04]	0.11 [0.04]	0.10 [0.04]	0.00 (0.01)	326
Household median income (US\$1,000s) (1990)	8.22 [1.75]	8.26 [1.75]	8.16 [1.77]	0.12 (0.24)	326
Poverty rate (1990)	0.61 [0.10]	0.61 [0.10]	0.62 [0.09]	-0.01 (0.01)	326
Unemployment rate (first year of mayor's term)	0.17 [0.05]	0.16 [0.05]	0.18 [0.06]	0.00 (0.00)	216
Unemployment rate (second year of mayor's term)	0.16 [0.05]	0.16 [0.05]	0.17 [0.05]	0.00 (0.00)	273

(Continued)

municipal government characteristics based on the population census and municipal government budget data during the first two years of the incumbent's term.

### III. Methodology

#### A. Research Design

We exploit the ordering of municipal audits to help us establish the dynamic effects of the audits on corruption and incumbent reelection rates. We compare the outcomes for municipalities whose audit reports were *timely*, i.e., disseminated in the two-year period before the relevant election, to those whose audit reports were *untimely*, i.e., disseminated in the two-year period following the election, for

TABLE 2—CHARACTERISTICS OF THE MUNICIPALITIES (*Continued*)

	All (1)	Timely audit (2)	Untimely audit (3)	Difference (adjusted) (4)	Observations (5)
<i>Panel D. Municipal government budget</i>					
Total expenditures (first and second year of mayor's term)	11.76 [23.32]	13.62 [27.60]	8.27 [10.96]	-0.21 (0.28)	216
Capital improvements	0.22 [0.43]	0.22 [0.42]	0.22 [0.45]	0.00 (0.02)	212
Salaries and benefits	4.82 [5.38]	4.99 [5.32]	4.51 [5.53]	-0.07 (0.24)	212
Social assistance	0.23 [0.38]	0.22 [0.36]	0.25 [0.41]	0.04 (0.04)	212
Other expenditures	4.01 [5.60]	4.39 [5.90]	3.33 [4.97]	0.14 (0.20)	212
Property tax	3.25 [5.50]	3.49 [5.87]	2.82 [4.77]	0.26 (0.21)	212
Licensing	1.95 [3.31]	2.08 [3.23]	1.71 [3.47]	-0.13 (0.09)	212
Waste disposal	0.22 [0.79]	0.27 [0.95]	0.12 [0.33]	-0.01 (0.03)	212
Transfers and other revenue	3.01 [2.15]	3.02 [1.97]	2.99 [2.48]	-0.21 (0.33)	216

*Notes:* Standard deviations of variables are reported in brackets. Differences estimated in OLS regression models, regression-adjusted for municipality and electoral term fixed effects, ( $a =$  except pre-audit municipality characteristics differences (data available from 1990 decennial census)). Robust standard errors of mean differences are reported in parentheses.

the election years 1988 through 2000.<sup>26</sup> Because municipalities are audited in a pre-established and fixed order, the timing of audits and their assignment into timely and untimely groups is plausibly exogenous.<sup>27</sup> These comparisons are illustrated in Figure 2.

We expect audit reports released in the period leading up to an election to be more likely to inform on the incumbent mayor's activities than those reports published shortly after an election. This assumption is reasonable for various reasons, at least in this context. First, due to the low overall reelection rate of mayors (35 percent), the information from untimely audits may be of less relevance in future elections. Second, because the information contained in audits may be of greater immediate interest to voters when an election is looming, the media may invest more resources in disseminating audit results or the information may be more salient to voters. Relatedly, even if the information from untimely audits does reach voters, they may not use it during the subsequent election because of recency bias—the tendency

<sup>26</sup>When municipalities have audit reports published in both periods, we aggregate only those reports published before the election and assign them to the timely audit group. This is to analyze the effects of the information available to voters at the time of the election on incumbent politician's electoral outcomes. Results on subsequent audits use all available reports and the results are robust to alternate aggregation methodologies.

<sup>27</sup>More precisely, our methodology resembles a fuzzy regression discontinuity (RD) design in a panel datasetting, where the running variable is calendar time, the threshold of interest is the exact day the election was held, and the bandwidth is the 24-month bandwidth period around each election. However, because we only observe the actual timing of dissemination of the report and not the planned one, we cannot implement this RD design. That said, we show that there is no evidence of selection on observable characteristics across the timely and untimely audit groups (see Sections IIIA and VI).

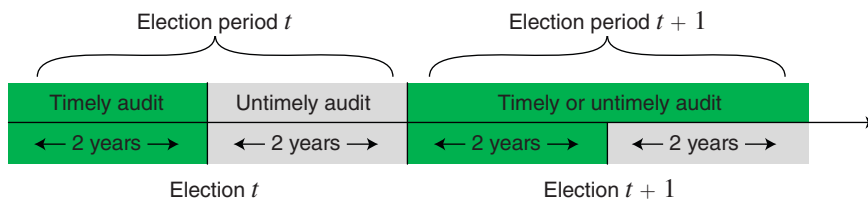


FIGURE 2. EMPIRICAL FRAMEWORK

Notes: A municipality with a timely audit during election period  $t$  may have a timely or untimely audit during election period  $t + 1$ . In our sample, 73 percent of municipalities with a timely audit at time  $t$  received a timely audit the following election period ( $t + 1$ ), whereas 54 percent of those with an untimely audit have a timely audit during election period  $t + 1$ .

for voters to place more weight on recent information.<sup>28</sup> This hypothesis has the testable implication that the information contained in untimely audits should have weaker (or no) effects on the electoral outcomes of the incumbent mayor in the following election—a prediction that we also examine (see Section IVB below).

Our partition of mayors' terms into two-year windows is supported by patterns in incumbent's corrupt behavior. Panel A of Figure 3 shows the differences in the number of findings classified as corrupt in audit reports by the year of publication within an incumbent mayor's term, relative to the first year of the term. We find no significant differences in the reported number of corrupt violations in audits published during the second year of a mayor's term (relative to the first year). In contrast, the reported number of corrupt violations drops significantly during the last two years of the term, by 2.28 and 1.60 findings per report. Interestingly, this pattern is not present in the number of noncorrupt findings (panel B). This disciplining effect of the audits during an incumbent mayor's last two years in a term fits our definition of timeliness of audits well.

The presumed exogeneity of the timing of audits enables us to give our empirical results a causal interpretation. To evaluate the validity of this assumption, we note that the number of audit reports covering a same audit period is somewhat greater for timely audits (panel B of Table 1, row 1). In addition, the time span of the audited period is 0.78 years longer (panel B, rows 2–4).<sup>29</sup> However, timely audits are as likely as untimely audits to follow the predetermined audit order rule (rows 5–6). That is, the OCPD does not seem to break the predetermined order rule disproportionately for audits disseminated in a timely manner. In Appendix B we document correlates of our measures of deviation from the audit order rule based on our data on audit characteristics, municipal socioeconomic and political characteristics, and characteristics of mayors. In summary, the deviation is correlated with certain political variables of interest, but it is *not* correlated with the audit being timely or not.

Moreover, municipalities under timely and untimely audits do not differ significantly in a large set of incumbent mayor, political, and socioeconomic characteristics

<sup>28</sup>Recency bias—that voters take into account more recent conditions in making electoral decisions—should influence the equilibrium behavior of incumbents. See Berry and Howell (2007) and the survey by Lewis-Beck and Paldam (2000) for a detailed discussion.

<sup>29</sup>While we do not have an explanation for this, the greater time span covered by timely audits would mechanically bias results against finding short-term effects of on the number of findings of corruption.

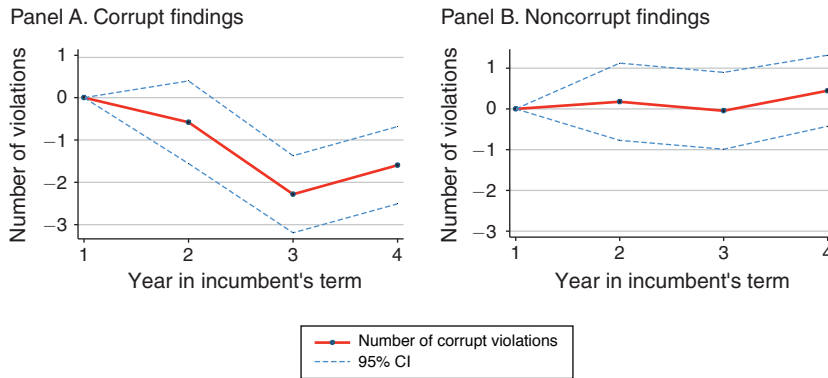


FIGURE 3. NUMBER OF FINDINGS IN REPORTS BY YEAR OF PUBLICATION DURING INCUMBENT'S TERM

*Notes:* Panel A shows the differences in the number of findings classified as corrupt in audit reports by the year of publication within an incumbent mayor's term, relative to the first year of the term, together with 95 percent confidence intervals (regression-adjusted for election cycle fixed effects). Panel B shows the analogous relationship for findings classified as not corrupt.

(see panels B, C, and D of Table 2). There is balance in the proportion of municipalities whose incumbent is member of the party in opposition to the governor (panel B, row 2); in the degree of competition in previous elections as measured by the incumbent mayor's win margin in the previous election (row 5); and in the lack of party turnover at the mayoral level (party incumbency advantage, row 6). We observe a similar balance in the population's educational attainment levels, household median income and poverty rates, and in the municipalities' unemployment rates. This evidence supports the assumption that the timing of the audits can be considered predetermined and that the agency does not time the dissemination of findings to influence electoral or other results.<sup>30</sup>

While our comparison of timely and untimely audits is reminiscent of the comparison of municipalities audited pre- and post-election in settings with random audits (e.g., Olken 2007; Ferraz and Finan 2008), it is important to note some key differences. In our setting, audit timing is exogenous because the order in which municipalities are audited is predetermined. Because audits are not randomly assigned though, politicians anticipate them and adapt their behavior accordingly. Therefore, our dataset includes cross-sectional variation in anticipated monitoring that enables us to measure its effect on corruption. As such, Puerto Rico's distinct institutional arrangements mean that the empirical strategy employed and the questions addressed in this paper differ from those in the existing literature.

<sup>30</sup>We also find balance in the municipal government budget measures (panel D of Table 2). Although it is not completely reasonable to assume that these are predetermined, it is comforting that these measures do not vary systematically across municipalities in the timely and untimely audit groups.

### B. Econometric Methodology

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

$$(1) \quad c_{m,t} = \theta A_{m,t} + \beta \mathbf{X}_{m,t} + \gamma_t + \alpha_m + \varepsilon_{m,t},$$

where  $c_{m,t}$  denotes the number of corrupt violations per report in municipality  $m$  around election year  $t$ , and  $A_{m,t}$  is an indicator for whether the municipality audit report was published in the two-year period preceding election year  $t$ .  $\mathbf{X}_{m,t}$  is a vector of municipality and mayor characteristics that influence the municipality's level of corruption.<sup>31</sup> The terms  $\alpha_m$  and  $\gamma_t$  represent municipality and election intercepts, respectively, and  $\varepsilon_{m,t}$  denotes unobserved characteristics that determine our measure of corruption at time  $t$ . Under the assumption that  $\{A_{m,t}, \mathbf{X}_{m,t}\}$  are strictly exogenous, the coefficient  $\theta$  provides a consistent estimate of the average effect of the audit dissemination on rent-seeking in municipal governments. We thus use variation in the timeliness of audits within municipalities over time to identify the effects of interest. Standard models of political agency predict and existing evidence shows that  $\theta < 0$ , due to the short-run disciplining effects of monitoring on politicians' rent-seeking decisions.

To examine the longer-term consequences of audits on rent-seeking behavior, we compare the outcomes of the subsequent audit (disseminated around the next election four years later at  $t + 1$ ) across municipalities whose earlier (period  $t$ ) audit reports were disseminated in a timely manner relative to those whose audit reports were untimely. Note that a municipality whose period  $t$  audit is timely may be subject to a timely or untimely audit during election period  $t + 1$ .<sup>32</sup> We estimate the average effect of the audits and their dissemination in term  $t$  on the reported rent-seeking levels in the subsequent audit using the following model:

$$(2) \quad c_{m,t+1} = \theta_{P1} A_{m,t} + \theta_{P2} A_{m,t+1} + \beta \mathbf{X}_{m,t} + \gamma_{t+1} + \alpha_m + \varepsilon_{m,t+1},$$

where  $c_{m,t+1}$  denotes the number of corrupt violations per report in municipality  $m$  in the subsequent audit. In all longer-term effects specifications, we also include a control for the timeliness of the next audit ( $A_{m,t+1}$ ).<sup>33</sup> The parameter  $\theta_{P1}$  captures the effect of timely audits on corruption levels in the *next term*. Whether we find a significant reduction will depend on whether voters are able to use the information in audits to select "better" politicians, the nature of heterogeneity among politicians, and the dynamic incentives politicians face (see Section V).

<sup>31</sup> We use as controls the number of municipality government reports, the number of municipal public corporation or consortium reports; indicators for the mayor's membership in the NPP, for the incumbent being in the opposition party to the state-level executive government, and for the incumbent being in the opposition party to the governor who appointed the comptroller; the vote share for the incumbent in the previous election; and the incumbent's number of terms in office.

<sup>32</sup> In our sample of municipalities with consecutive audits, 73 percent of timely audit municipalities received an untimely audit the following election period, whereas 54 percent of those with untimely audits have a timely audit during election period  $t + 1$ . Twenty-eight percent of our municipality period observations are not audited in the subsequent term.

<sup>33</sup> The results are qualitatively and quantitatively similar irrespective of the inclusion of the future audit timing control. Estimates are available from the authors upon request.



The setting also allows us to examine the equilibrium relationship between the timely audit program and short-term incumbents' reelection rates, similar to that found in the existing literature. Following Ferraz and Finan (2008), we estimate the model

$$(3) \quad e_{m,t} = \theta_{E1}A_{m,t} + \theta_{E2}A_{m,t}c_{m,t} + \beta_{E1}c_{m,t} \\ + \beta_{E2}\mathbf{X}_{m,t} + \gamma_t + \alpha_m + \varepsilon_{m,t},$$

where  $e_{m,t}$  is an indicator for the reelection of the incumbent mayor in election year  $t$ . Again, standard political agency models predict that  $\theta_{E2} < 0$ . The estimates of  $\{\theta_{E1} \theta_{E2}\}$  are not interpretable as the causal effects of the timely audit content on incumbent reelection rates (as in Ferraz and Finan 2008, for instance) due to the endogeneity of the audit outcomes ( $c_{mt}$ ) with respect to the timing of the audit ( $A_{mt}$ ) as a result of the responsiveness effect.

To examine the consequences of timely monitoring on next-period incumbent electoral performance, we employ models analogous to equation (2) to estimate the average effect of timely audits in term  $t$  on the incumbent mayor's probability of reelection in election year  $t + 1$  (denoted  $e_{m,t+1}$ ),

$$(4) \quad e_{m,t+1} = \theta_{E'1}A_{m,t} + \beta_{E'1}A_{m,t+1} + \beta_{E'2}\mathbf{X}_{m,t} \\ + \gamma_{t+1} + \alpha_m + \varepsilon_{m,t+1}.$$

Parameter  $\theta_{E'1}$  captures the effect of the timely dissemination of the first audit on *longer-term* reelection rates. We also test whether the audit-induced shift in long-term electoral performance is heterogeneous across municipalities with zero reported corruption and among those whose executives were shown to have engaged in corruption in the first audit, with the following model of heterogeneous effects:

$$(5) \quad e_{m,t+1} = \theta_{E'1}A_{m,t} + \theta_{E'2}A_{m,t}c_{m,t} + \beta_{E'1}A_{m,t+1} \\ + \beta_{E'2}c_{m,t} + \beta_{E'3}\mathbf{X}_{m,t} + \gamma_{t+1} + \alpha_m + \varepsilon_{m,t+1}.$$

This empirical model is also useful because it allows us to verify whether the content of audits disseminated in the two years following the year  $t$  election are used by voters to sanction (or reward) incumbents in the next term. If recency bias, lower media dissemination, or low reelection rates are relevant factors, information contained in untimely audits should have weak or no effects on electoral outcomes in the following election; that is,  $\beta_{E'2} = 0$ .

## IV. Results

### A. Effects of the Audits on Corruption

We start by examining the level of corruption for municipalities with timely audits and those with untimely ones around election period  $t$ . Figure 4 plots the

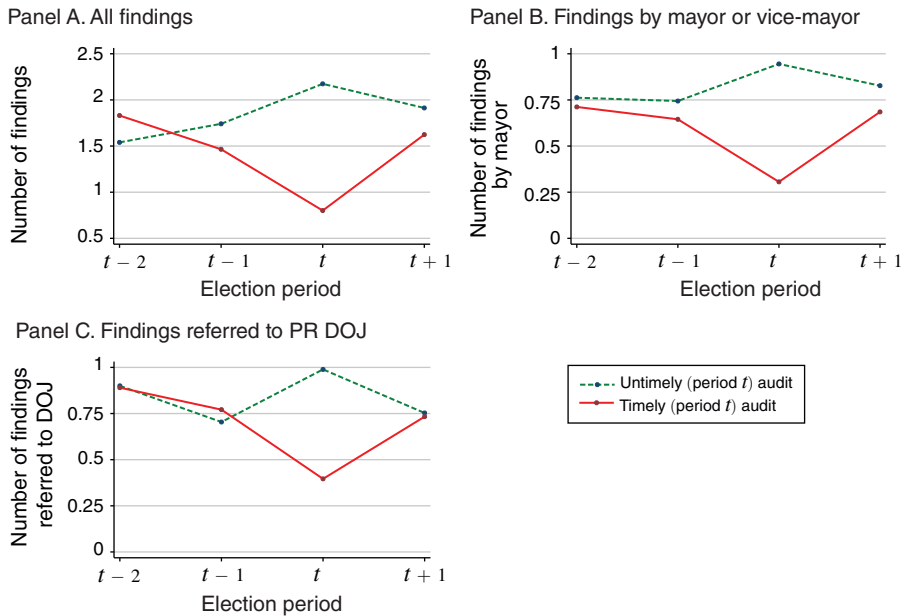


FIGURE 4. NUMBER OF FINDINGS ACROSS TIME, BY TIMELINESS OF AUDIT IN ELECTION ( $t$ )

average number of corrupt violations per report from audits in each of the previous two terms, around the election term  $t$ , and in the following audit. We show the trends separately for municipalities with timely (solid line) and untimely (dashed line) audits. 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, and panel C reports the findings referred to the PR DOJ.

There are no discernible differences in the levels of reported corruption across these two groups of municipalities in earlier audits—the mean number of violations per report revolves around 1.7 and those attributed to the mayor or vice-mayor around 0.75, and the differences are statistically indistinguishable from zero. In contrast, for audits around election  $t$  there is a stark difference of 1.28 ( $= 0.79 - 2.07$ ) violations per report. A similar pattern holds for the number of violations attributed to municipality-level executives ( $0.56 = 0.85 - 0.29$ ) and those referred to the DOJ ( $0.53 = 0.89 - 0.36$ ). Finally, comparing these groups of municipalities in the next round of audits (around election year  $t + 4$ ), we find that the difference in corruption levels decreases dramatically and is statistically indistinguishable from zero. This evidence strongly suggests that the disciplining effects of the audits are short lived.

Next, we present regression-based evidence of the short-run effects of the audit program on corruption (Table 3). The estimates show a systematic reduction in the number of corrupt violations in the municipality. There are 1.43 (66 percent) fewer reported corrupt violations in municipalities with timely audits relative to those whose audit reports were published after the election (panel A, column 1). We also find 0.63 (67 percent) fewer corrupt violations per report by the mayor or

TABLE 3—EFFECTS OF THE (*Timing of the*) AUDITS  
ON THE NUMBER OF CORRUPT VIOLATIONS IN THE CURRENT AUDIT

	Number of corrupt violations, per report			Share of findings classified as corrupt violations, findings	
	All OLS (1)	By mayor/ vice-mayor OLS (2)	Referred to DOJ OLS (3)	By mayor/ vice-mayor OLS (4)	Referred to DOJ OLS (5)
<i>Panel A. Average effects</i>					
Timely audit	-1.43 (0.22)	-0.63 (0.14)	-0.65 (0.16)	-0.082 (0.040)	-0.160 (0.055)
Municipality controls	Yes	Yes	Yes	Yes	Yes
Election year and municipality FEs	Yes	Yes	Yes	Yes	Yes
<i>Panel B. Effects by party advantage</i>					
Timely audit	-1.90 (0.32)	-0.92 (0.17)	-1.12 (0.25)	-0.174 (0.045)	-0.229 (0.069)
Timely audit × incumbent's party has won in previous 3+ elections	0.96 (0.41)	0.61 (0.24)	0.97 (0.32)	0.191 (0.061)	0.153 (0.106)
Municipality controls	Yes	Yes	Yes	Yes	Yes
Election year and municipality FEs	Yes	Yes	Yes	Yes	Yes
Observations	326	326	326	326	326
Mean of dep. variable (untimely audits)	2.17	0.95	0.99	0.22	0.34

*Notes:* Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. 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 ( $t - 1$ ); and the incumbent's number of terms in office (at time  $t$ ). The sample is composed of all municipalities in which mayors are running for reelection that had a first audit during 1987 to 2002.

vice-mayor (column 2), which suggests that there is a very limited (if any) shift in corrupt violations charges between mayors and other municipality employees.<sup>34</sup> We find comparable effects using the more stringent measure of corruption—the number of findings (per report) of misuse of public funds referred to the PR 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 afterward (column 3). The share of findings classified as corrupt also decreases by approximately 37–47 percent (columns 4–5). These relationships are stable and robust to controls and to using the sample of municipalities in which mayors are running for reelection (not reported).

To check whether the effects are due to the mayor's electoral accountability, we estimate heterogeneous effects of audits by the competitiveness of the mayoral seats (using our summary measure of incumbency advantage). As expected, the short-term disciplining effects are concentrated among municipalities with

<sup>34</sup>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). This suggests that mayors are able to discipline their employees effectively.

TABLE 4—THE EFFECTS OF THE AUDITS ON THE NUMBER OF CORRUPT VIOLATIONS IN THE SUBSEQUENT AUDIT (*Term*)

	Number of corrupt violations per report in next audit			Share of findings classified as corrupt violations in next audit	
	All findings OLS (1)	By mayor/vice-mayor OLS (2)	Referred to DOJ OLS (3)	Findings by mayor/vice-mayor OLS (4)	Findings referred to DOJ OLS (5)
<i>Panel A. All municipalities</i>					
Pre-election audit	-0.10 (0.32)	-0.07 (0.16)	0.16 (0.24)	0.058 (0.055)	0.032 (0.067)
Municipality controls	Yes	Yes	Yes	Yes	Yes
Election year and municipality FEs	Yes	Yes	Yes	Yes	Yes
Observations	232	232	232	232	232
Mean of dep. variable (post-election)	1.91	0.83	0.75	0.21	0.36

*Notes:* Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. 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 ( $t - 1$ ); and the incumbent's number of terms in office (at time  $t$ ). The sample is composed of all municipalities in which mayors are running for reelection that had a first audit during the period 1987–2002.

competitive elections. The estimated impacts in competitive seat municipalities imply reductions in rent-seeking levels in the 60–95 percent range (in proportional terms), whereas the estimated effects among municipalities with a significant party incumbency advantage are very small and statistically indistinguishable from zero (panel B).<sup>35</sup>

Estimates of the effects of timely audits on the number of corrupt violations in the subsequent term allow us to formally test for the audits' longer-term effects (Table 4). The point estimate from the average effects model (equation (2)) suggests a (statistically insignificant) decrease of 0.10 of a violation per report (5.4 percent) among timely audit municipalities (column 1). The relationship remains unchanged when focusing on the number of violations by the mayor or vice-mayor (column 2). The point estimate from this specification implies a small increase in rent-seeking of 0.07 of a violation (8.1 percent). Using the more stringent measure of corruption—the number of findings referred to the Department of Justice—gives even starker results (column 3). The point estimate implies an increase of 0.16 violations (21 percent). We can reject a decrease in corruption greater than 38–43 percent with 95 percent confidence.<sup>36</sup> Again, the point estimates from specifications that use the share of violations classified as corrupt suggest an increase in rent-seeking, although

<sup>35</sup> Among untimely audit municipalities, those with competitive mayoral seats report 29–54 percent lower corruption than those with noncompetitive elections (not reported in the tables). This evidence is also consistent with rent-seeking levels being lower in jurisdictions with greater electoral competition.

<sup>36</sup> We generally have sufficient precision to reject moderately sized reductions in the number of violations. The results are also robust to exploring the extensive margin only—indicator variables for whether there is reported corruption in the audit (available from the authors upon request).

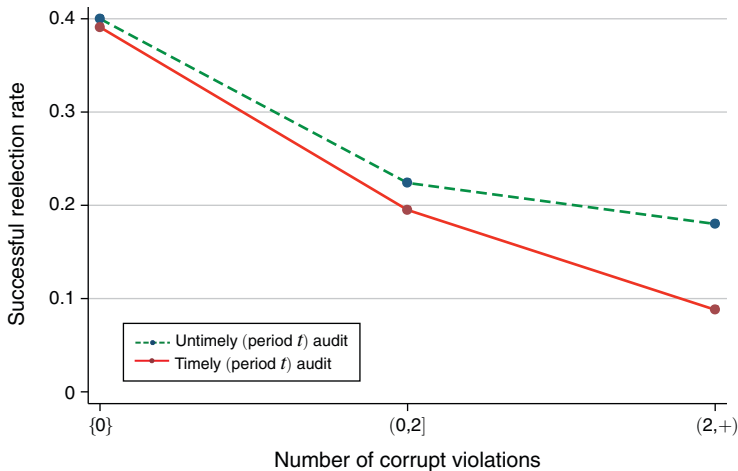


FIGURE 5. RELATIONSHIP BETWEEN REPORTED CORRUPTION LEVELS AND INCUMBENT MAYOR'S REELECTION RATE FOR MUNICIPALITIES WITH TIMELY AND UNTIMELY AUDITS

*Note:* The figure shows the adjusted (by election intercepts) relationship between the mayors who were successfully reelected in the election and the number of corrupt violations per report in the audits for municipalities with a timely and an untimely audit around the period  $t$  election.

these are also statistically indistinguishable from zero (columns 4 and 5). These relationships are stable and robust to controls and to using the sample of municipalities in which mayors are running for reelection (not reported).

### B. Audits and Short-Term Electoral Accountability

We now focus on the short-run relationship between the audit program and electoral accountability, i.e., incumbent mayors' reelection rates. We again start the discussion with a graphical analysis to shed light on the patterns in the data. Figure 5 depicts incumbent mayors' reelection rates as a function of reported corrupt violations per report in the municipality, distinguishing between municipalities with timely audits (solid line) and those with untimely ones (dashed line).<sup>37</sup> Incumbent mayors in timely audit municipalities exhibit a clear downward-sloping trend between reelection rates and the number of corrupt violations per report. Among municipalities with no reported violations, reelection rates are 39 percent, decrease to approximately 20 percent among incumbent administrations charged with up to two violations per report, and to 9 percent among administrations charged with more than two violations. In contrast, the relationship among municipalities whose reports were published following the election is less stark. Reelection rates are similar at 40 percent among those administrations with favorable audits, and decrease at a slower rate to 22 percent and 20 percent for administrations with moderate and high corruption levels, respectively. The contrast of these two relationships suggests that voters hold corrupt politicians accountable when informed.

<sup>37</sup>The reported differences between timely and untimely audit municipalities are regression-adjusted for election period fixed effects.

TABLE 5—THE EFFECTS OF THE AUDITS ON SHORT-TERM AND LONG-TERM ELECTORAL OUTCOMES

	Incumbent runs for and wins reelection (period $t$ )				Incumbent runs for reelection (period $t$ )	Incumbent wins reelection   running (period $t$ )
	OLS	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)	(6)
Timely audit	-0.043 (0.040)	-0.019 (0.056)	-0.002 (0.052)	0.031 (0.053)	-0.011 (0.065)	-0.010 (0.056)
Timely audit $\times$ num. violations		-0.035 (0.029)			-0.068 (0.042)	-0.060 (0.028)
Num. of violations		0.000 (0.014)			-0.013 (0.014)	-0.007 (0.013)
Timely audit $\times$ incumbent's ( $t$ ) party has won in previous 3+ elections			-0.076 (0.080)			
Timely audit $\times$ terms in office				-0.070 (0.034)		
Municipality controls	Yes	Yes	Yes	Yes	Yes	Yes
Election year and municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes
Incumbent mayor ( $t$ ) FEs	No	No	No	No	No	No
Timely audits $F$ -statistic [ $p$ -value]		1.74 [0.18]	0.79 [0.46]	2.68 [0.075]	1.92 [0.15]	3.75 [0.03]
Observations	326	326	326	326	326	241
Mean of dep. variable (controls)	0.25	0.25	0.25	0.25	0.76	0.31

(Continued)

This evidence, although consistent with previous work on the effects of municipal audit programs on electoral accountability (i.e., Ferraz and Finan 2008), is not interpretable in a causal manner but only as an equilibrium relationship due to the endogeneity of the audit outcomes with respect to the timing of the audit as a result of the responsiveness effect.

Linear probability estimates of the reduced-form relationship following empirical model (3) captures the results depicted above (Table 5). Timely audits have no significant effect on incumbent mayors' reelection rates (column 1). Also, reelection rates are not significantly correlated with the number of corrupt violations among timely audit municipalities. The point estimate indicates that the probability of a successful reelection is 3.5 percentage points (14 percent) lower for each additional finding per report (column 2). This imprecisely estimated effect masks some heterogeneity, in that incumbent mayors in municipalities with negative timely audits are less likely to run for reelection (column 5) and, among those running, their reelection probability is weaker (column 6). Interestingly, the outcomes of untimely audits have no relationship with the probability of reelection. The  $\beta_{E1}$  point estimate implies no reduction in the incumbent's reelection rate (overall and conditioning on a mayor running for reelection). Overall, the estimated relationships are consistent

TABLE 5—THE EFFECTS OF THE AUDITS ON SHORT-TERM AND LONG-TERM ELECTORAL OUTCOMES (*Continued*)

	Incumbent runs for and wins reelection (period $t + 1$ )					
	OLS (7)	OLS (8)	OLS (9)	OLS (10)	OLS (11)	OLS (12)
Timely audit	0.041 (0.055)	0.080 (0.066)	0.133 (0.072)	0.128 (0.072)	0.050 (0.068)	0.202 (0.074)
Timely audit $\times$ num. violations		-0.043 (0.031)				
Num. of violations		0.007 (0.018)				
Timely audit $\times$ incumbent's ( $t$ ) party has won in previous 3+ elections			-0.179 (0.099)		-0.027 (0.092)	
Timely audit $\times$ terms in office				-0.083 (0.039)		-0.131 (0.034)
Municipality controls	Yes	Yes	Yes	Yes	Yes	Yes
Election year and municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes
Incumbent mayor ( $t$ ) FEs	No	No	No	No	Yes	Yes
Timely audits $F$ -statistic [ $p$ -value]		1.07 [0.35]	2.03 [0.14]	2.38 [0.10]	0.27 [0.77]	7.48 [0.001]
Observations	326	326	326	326	326	326
Mean of dep. variable (controls)	0.20	0.20	0.20	0.20	0.20	0.20

*Notes:* Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. 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 ( $t - 1$ ); 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 the period 1987–2002. The reported timely audits  $F$ -statistic refers to a test of joint significance on the timely audit and its interactions with the relevant explanatory variable ( $p$ -value in brackets).

with the hypothesis that monitoring incumbent behavior induces an improvement in electoral accountability.<sup>38</sup>

The evidence presented up to this point supports the hypotheses that monitoring incumbent behavior induces short-run disciplining and electoral accountability, but that the effects of timely audits on the level of corruption are short lived. This is consistent with the idea that politicians in power in the next term will, on average, engage in the same level of rent-seeking after an audited period than after a nonaudited period, because their improved reputation allows greater leeway. However, as highlighted earlier this is also consistent with a broad class of pure moral hazard models of electoral politics in which all politicians are identical. Thus, in the next subsection we examine other predictions of our theory to evaluate in more detail whether political selection is playing a role.

<sup>38</sup>For the sake of completeness, we also report estimates from models examining heterogeneous effects by seat competitiveness and the incumbent mayor's terms in office. We find no effects of timely audits on incumbent reelection rates among municipalities with competitive elections, whereas reelection rates are significantly lower in municipalities with a large party incumbency advantage (column 3). We similarly find lower reelection rates among mayors with more experience in office (see column 4). Note however that theories of political agency do not have strong predictions regarding this short-term relationship.

### C. Politician Selection and Long-Term Electoral Performance Effects

Parametric estimates of the reduced-form relationship show that, although incumbent mayors' overall reelection rates (in election  $t + 1$ ) are not statistically significantly correlated with the incidence of a timely audit—on average or by the number of corrupt violations among timely audit municipalities—the point estimates suggest a positive reelection effect (Table 6, columns 7–8). However, we find evidence of a large positive reelection effect (13.3 percentage points, or 67 percent) among the subset of municipalities with competitive mayoral seats (column 9) and among first-term mayors (12.8 percentage points, or 64 percent; column 10).

In contrast, negative information from untimely audits has no effect on the probability of reelection. The  $\beta_{E2}$  point estimate implies a 0.4 percentage point (2.1 percent) decrease in the incumbent's reelection rate and is statistically indistinguishable from zero, consistent with the importance of an audit being timely (column 8, row 3). In summary, these relationships support the hypothesis that timely audits lead to a politician selection effect that increases reelection rates in the longer term.

## V. Implications for the Theory of Political Agency

In the preceding section we have presented evidence on the short- and long-term effects of audits on corruption and reelection rates. The information contained in audits enables voters to better monitor incumbents and to (partially) distinguish good politicians from bad. The improvement in monitoring is short lived and affects incumbent behavior before the voter has had a chance to use the information to increase the probability of having a good politician in office. Therefore, short-run variation in observable outcomes can be attributed to a *sanctioning effect*. Long-run variation, however, cannot be due to differences in monitoring. Rather, it must be due to differences in the long-run distribution of incumbent types—a *selection effect*.

The short- and long-term effects of audits we have documented are inconsistent with several of the most commonly used models of political agency. Positive long-term effects of audits on reelection rates differ from the standard predictions of models with no heterogeneity (e.g., Barro 1973; Ferejohn 1986). Moreover, our finding of nil long-term effects on the level of corruption contrasts with the predictions of models based on the most commonly used type space in which some politicians are intrinsically motivated to act in accordance with voter-welfare while the rest are self-interested (e.g., Coate and Morris 1995, Fearon 1999).<sup>39</sup> Because such *good* politicians would limit their corruption regardless of whether they are being audited, these models generally predict that audits, which enable the voter to select *good* politicians with higher probability, have long-lasting effects on the level of corruption.

<sup>39</sup>These types are called *good* in Coate and Morris (1995) and Fearon (1999); *congruent* in Besley (2006, ch. 3.3); and *noncorrupt* in Ferraz and Finan (2008). Other models, in which candidate *quality* (Lohman 1998), ability (Caselli and Morelli 2004), or *competence* (Ashworth 2005) are modeled as increasing voter utility for any given incumbent action, have similar implications. As shown in Schwabe (2011), these types of politicians can be incorporated into a model that generates equilibrium predictions consistent with our empirical findings as long as they are sufficiently rare.



TABLE 6—ROBUSTNESS TESTS: MANIPULATION OF AUDITS

	Number of corrupt violations per report referred to DOJ, in current audit ( $t$ )				Number of corrupt violations per report referred to DOJ, in next audit ( $t + 1$ )			
	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)	OLS (7)	OLS (8)
Timely audit (period $t$ )	-0.71 (0.24)	-0.73 (0.24)	-0.48 (0.18)	-0.64 (0.23)	0.16 (0.23)	0.13 (0.25)	-0.09 (0.22)	0.34 (0.47)
Mean deviation from expected num. of reports (period $t$ )	0.001 (0.004)				-0.011 (0.008)			
Mean of abs. value of deviation from expected num. reports ( $t$ )		-0.014 (0.013)				0.001 (0.017)		
Timely audit $\times$ incumbent (period $t$ ) from opp. party to governor appointing comptroller			-0.41 (0.37)				0.69 (0.33)	
Timely audit $\times$ mayor's win margin in previous election, period ( $t - 1$ )				-0.13 (2.33)				-1.72 (3.07)
Municipality controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election year and municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	220	220	326	326	198	198	232	232
Mean of dep. variable (untimely audits)	0.99	0.99	0.99	0.99	0.71	0.71	0.75	0.75

*Notes:* Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. 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 ( $t - 1$ ); 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 the period 1987–2002.

By ruling out some of the most widely used models of political agency, our findings contribute to the ongoing debate regarding the nature of the differences among politicians, and the type of qualities that voters evaluate in their representatives (see, for instance, Fearon 1999 and Besley 2005). As we have seen, one view is that some politicians are virtuous or honest and will do all they can to serve voters, while others are opportunistic and seek office primarily to extract rents from office. Another, possibly complementary, view holds that all politicians are opportunistic but differ in their ability or competence (see, for instance, Banks and Sundaram 1993). The two positions have different implications for the effectiveness of anticorruption audit programs as well as for our understanding of democracy.

We formulate a model that builds on the second view and is consistent with our empirical findings. In the model, which we present in online Appendix D, politicians are one of two types: responsive or corrupt. Responsive politicians may choose to moderate their rent-seeking, while corrupt types always engage in all-out corruption.<sup>40</sup> Each period, voters must decide whether to reelect an incumbent politician, but are generally unable to observe his type or actions in office. Audits give voters

<sup>40</sup>In the Banks and Sundaram (1993) framework, corrupt types have an infinitely high cost of effort (restraint from corruption). Effort is costly for responsive types, but not so costly that it cannot be outweighed by the benefits of staying in office.

information on the actions taken by politicians, which allows them to provide stronger incentives and to separate responsive from corrupt types. Thus, audits reduce corruption in the short term and lead to the selection of responsive politicians. Once reelected, however, politicians who survived an audit have a good reputation, which gives them an advantage over unknown challengers. High-reputation incumbents take advantage of this by engaging in as much corruption as voters will tolerate: the level expected from an unknown first-term mayor. Thus, even though there is selection, there are no dynamic effects of audits on corruption. However, politicians who survived an earlier audit are able and willing to adapt their behavior to voter standards, as they have shown in the past. This, along with the lower standards that the voters can credibly hold them to, translates into higher reelection rates. A full description of the model and its equilibria, along with the formal statement and proof of all relevant results, is in the online Appendix D.

The model provides four key predictions consistent with the data:

- (i) The expected timely dissemination of the audit reports decreases the number of corrupt violations by incumbent politicians in the short run;
- (ii) Reelection rates at time  $t$  are negatively correlated with the number of corrupt violations;
- (iii) Politicians in office at time  $t + 1$  will engage in *the same number of corrupt violations*, on average, irrespective of whether the municipality's audit at time  $t$  was timely or untimely; and,
- (iv) On average, reelection rates at time  $t + 1$  will be higher in municipalities that experienced a timely audit at time  $t$  relative to those that did not.

Predictions (i–ii) can be generated by a variety of political agency models. In contrast, existing canonical models cannot (to our knowledge) jointly explain the second set (iii–iv) of predictions.<sup>41</sup>

## VI. Robustness 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 timely audits differed systematically from untimely ones, 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 assess multiple potential biases in the actual audit processes. For all robustness checks, we

<sup>41</sup> Given the complexity of dynamic political agency models that incorporate both sanctioning and selection (Duggan and Martinelli 2015), a characterization of models consistent with our empirical findings is beyond the scope of this paper.

report estimates from our preferred measure of findings referred to the PR DOJ, but all results are robust to the choice of outcome measure.

We estimate models in which we control for our measures of deviation from the predetermined audit order, among the subsample of observations for which we can construct these measures, to examine whether deviations from the rule affect our estimates of the effects of timely audits (Table 6, columns 1–2, 5–6). Controlling for these measures does not affect our estimates of short-run and longer-run effects of timely audits. In addition, the implied relationship between the deviation measures and the findings of corruption is small. For instance, a one standard deviation increase in the measure of dispersion from the gap predicts a 0.15 ( $= 10.9 \times -0.014$ ) decrease in the number of findings referred to the DOJ in the short run, and a 0.011 ( $= 10.9 \times 0.13$ ) increase in the longer run. Comparable estimates for all other measures are reported in online Appendix Tables 3 and 4.

In addition, if the actual audits were manipulated 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 allow for heterogeneous short-term and long-term effects for municipalities in which the incumbent is from the same party or from the opposition to the party of the governor who appointed the comptroller.<sup>42</sup> The estimates of these heterogeneous responses suggest that municipalities in the opposition to the party of the governor who appointed the comptroller receive no less favorable audit outcomes in the short term (column 3). In the longer run, we observe the inverse relationship with the period  $t$  incumbent's party. Given the low reelection rates in this context, this likely reflects the fact that the period  $t + 1$  incumbent is from the opposition party, and thus receives a similarly unfavorable treatment by the audit authorities as seen in the short run (column 7). Even among aligned municipalities, we find no evidence of a sustained reduction in rent-seeking levels in the longer run.

Another possibility is that incumbents who won by narrow margins in the previous election have a greater incentive to bribe 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 timely audit indicator. We do not find evidence that a mayor's previous level of political support influenced the audit process and including these additional controls does not affect the main short-term or long-term responses to timely audits (columns 4, 8).

We also evaluate whether the extent of subsequent auditing varied significantly across municipalities of different types. To do so, we estimate specifications using as dependent variables (i) an indicator for the existence of a subsequent audit report, and (ii) the number of reports from the subsequent audit (not reported). The estimates indicate no evidence of selective auditing, or of differential intensity of

<sup>42</sup>We also check for heterogeneous effects for municipalities in which the mayor is from the opposition to the incumbent governor. Point estimates are not significantly different from zero at conventional levels, but suggest that these incumbents receive slightly more favorable audits than those from the governor's party (available from the authors upon request).

auditing, as measured by the number of reports.<sup>43</sup> These tests show, to the best of our ability, that the manipulation of audits is unlikely and that it does not affect our main findings.

## VII. Tests for Alternative Explanations

Thus far, we have interpreted our empirical results within a theoretical framework derived from political agency models in which timely audits affect outcomes through their sanctioning and selection effects. In what follows, we discuss several alternative interpretations, but find little support for them in the data.

*Selection versus Incentives and Endogenous Sorting by Type.*—We have argued that the short-term effects of timely audits are driven mainly by a sanctioning or incentive effect, while their long-term effect is primarily due to sorting of incumbent politician types that is a consequence of the election that follows a timely audit (see Section V and online Appendix D). It is possible, however, that the effects we document reflect the entry and exit decisions of politicians who respond to the foreseeable possibility of timely audits in a given municipality. For example, strategic candidate entry considerations can lead bad types to choose not to run for office (in period  $t - 1$ ) if they can predict a timely audit in the coming term, such that the differences in corruption we observe between timely and untimely audit groups of municipalities are partly driven by an *ex ante* incumbent selection effect. In principle, one can test this alternative explanation by including incumbent fixed effects in models analogous to equations (1) and (2), and verifying whether the observed patterns of within-incumbent corruption are consistent with the conceptual framework we have presented.<sup>44</sup> In the following paragraphs, we discuss the results of this exercise.

We can isolate the impacts of timely audits due to incentive effects (among those incumbents in office) *if* we assume that the potential sorting of politicians based on their type can be captured by an incumbent fixed effect in a model analogous to equation (1). The estimate from this specification implies that there are 0.42 fewer findings of corruption per report in the short run (Table 7, column 1). This suggests that the short-run effects of timely audits are the result of a sanctioning effect.

The point estimate from a specification of equation (2) that incorporates an incumbent fixed effect implies that there are 0.64 more findings of corruption in the longer run (significant at 90 percent confidence) (column 5). The difference between the timely audit effects estimates across the specifications with and without incumbent mayor fixed effects is 0.48 ( $= 0.16 - 0.64$ ) ( $p$ -value = 0.019).<sup>45</sup> This

<sup>43</sup>If the OCPD or auditors were to strategically manipulate results closer to the election, we should find different corruption levels for reports published during this period. However, the number of reported corrupt violations published in the year of the election is not significantly different from those published during the third year of the term (see panel A of Figure 3).

<sup>44</sup>Specifications with incumbent mayor fixed effects are vulnerable to endogeneity concerns due to the relation between audit results, electoral outcomes, and strategic retirement.

<sup>45</sup>We estimate the two equations as a system via seemingly unrelated regression (SUR) with cluster-robust standard errors at the municipality level. Inference is based on a test of equality of the timely audit coefficient estimates across equations. The patterns are qualitatively similar across outcome variables, but larger and more precisely estimated for our preferred measures of findings referred to the DOJ. Estimates available upon request.

TABLE 7—ADDITIONAL ROBUSTNESS TESTS

	Number of corrupt violations per report referred to DOJ, in current audit ( $t$ )				
	OLS (1)	OLS (2)	OLS (3)	OLS (4)	
Timely audit (period $t$ )	-0.42 (0.19)	-0.71 (0.21)	-0.61 (0.25)	-0.81 (0.18)	
Timely audit $\times$ terms in office		0.060 (0.12)			
Timely audit, period ( $t - 1$ )			0.04 (0.18)		
Audit period start date				0.02 (0.03)	
Audit period end date				0.16 (0.07)	
Timely audit $\times$ incumbent's party has won previous 3+ elections					
Incumbent mayor ( $t$ ) FEs	Yes	No	No	No	
Municipality controls	Yes	Yes	Yes	Yes	
Election year and municipality FEs	Yes	Yes	Yes	Yes	
Observations	326	326	326	326	
Mean of dep. variable (untimely a.)	0.99	0.99	0.99	0.99	
	Number of corrupt violations per report referred to DOJ, in next audit ( $t + 1$ )				
	OLS (5)	OLS (6)	OLS (7)	OLS (8)	OLS (9)
Timely audit (period $t$ )	0.64 (0.36)	0.60 (0.48)	-0.21 (0.57)	0.29 (0.23)	0.84 (0.36)
Timely audit $\times$ shock (period $t + 1$ ) to popularity of party of (period $t$ ) incumbent mayor		-0.67 (0.47)			
Timely audit $\times$ incumbent (period $t + 1$ ) runs for reelection			0.40 (0.63)		
Audit period start date				0.10 (0.06)	
Audit period end date				-0.29 (0.11)	
Timely audit $\times$ incumbent's party has won previous 3+ elections					-1.43 (0.53)
Incumbent mayor ( $t$ ) FEs	Yes	Yes	No	No	No
Municipality controls	Yes	Yes	Yes	Yes	Yes
Election year and municipality FEs	Yes	Yes	Yes	Yes	Yes
Observations	232	232	232	232	232
Mean of dep. variable (untimely a.)	0.75	0.75	0.75	0.75	0.75

Notes: Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. 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 ( $t - 1$ ); 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 the period 1987–2002.

within-incumbent increase in corruption can be interpreted as a selection effect in the following sense: if incumbents who were reelected following a timely audit faced the same incentives in future terms as they did during their first term, they would have engaged in 0.48 fewer corrupt acts than incumbents in other municipalities that received a timely audit during period  $t$ . However, consistent with our model of political agency, weaker incentives during the following term induced more rent-seeking in equilibrium, offsetting this selection effect.

Finally, we can also isolate the period  $t + 1$ 's election incumbency advantage in a modified version of equation (4) that similarly incorporates incumbent fixed effects. We report heterogeneous effects estimates by competitive mayoral seats (Table 5, column 11) and by mayors' terms in office (column 12). The estimates suggest that the incumbency advantage among mayors in competitive seats is largely due to a selection effect, as expected. However, (period  $t$ ) first-term mayors retain an incumbency advantage at  $t + 1$  even after accounting for selection effects. This could plausibly be explained by additional restraint from corruption by first-term mayors due to a continued desire to signal their type to voters.

*Mayor's Tenure, Political Experience, and Retirement.*—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 an increase in corruption 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 change in corrupt practices in municipalities with previously favorable (timely) audits. In any case, because the number of terms in office of mayors at baseline is equivalent across municipalities with timely and untimely audits, and short-run reelection rates do not differ among municipalities with favorable timely versus untimely audits, there is no *prima facie* evidence of selection based on experience. Furthermore, among those elected into office at time  $t$ , the difference in the number of terms in office is also small ( $-0.15$  years) and insignificant (standard error = 0.26). Therefore, to the extent that the available data allow us to assess this explanation, the evidence is inconsistent with experience driving our results.

We also examine whether the disciplining effects vary by the tenure of the politician, as voter learning about the incumbent's characteristics should be more pronounced in earlier terms, possibly leading to a shift in the accountability relationship. We thus estimate specifications with the interaction of a timely audit and the number of terms in office of the politician. Although the point estimates suggest that higher tenure incumbents tend to be less disciplined by timely audits, the estimated differential effects are small and statistically indistinguishable from zero (Table 7, column 2).<sup>46</sup>

<sup>46</sup>The model also predicts that audits will be less effective in reducing corruption among incumbents who have spent more terms in office. We can evaluate this prediction by restricting our sample to those mayors who have been reelected at least once. Point estimates suggest that the relationship is indeed stronger among first-term mayors who are reelected. These estimates are less credible due to the lower precision (given the smaller sample size,  $N = 84$ ) and the potential sample selection problem due to endogenous reelection. Estimates are available upon request.

We also examine whether another aspect of career cycles—negative party popularity shocks—matters for the incumbent’s behavior in a manner that can help explain our divergent short-term and long-term corruption results. If incumbent mayors in a future term expect more competitive elections due to a decline in the popularity of their political party, they may, on one hand, engage in more rent-seeking activities if their career as mayor is less likely to continue (a last-term effect) or, on the other, engage in less corruption if this might give them an edge over the challenger. To the extent that the former effect dominates, we would observe higher rent-seeking levels among incumbents facing negative popularity shocks. To evaluate this, we use an indicator for whether the period  $t$  incumbent’s party loses the following gubernatorial election (at time  $t + 1$ ) as a proxy for negative popularity shocks, and estimate specifications that allow for heterogeneous long-term effects for municipalities in which mayors face or do not face these shocks (Table 7, column 6). The estimates for municipalities in which mayors do not face these shocks imply no sustained reduction in rent-seeking levels, and the effects among those facing the popularity shock suggest that the increased competitiveness leads to a (statistically insignificant) disciplining effect.

Finally, we examine whether exit or retirement of politicians in period  $t + 1$  can help explain the absence of longer-term corruption effects. In spite of the fact that there are no term limits, the time  $t + 1$  incumbent’s planned exit from office could induce greater rent-seeking given that the incumbent would not face reelection incentives. To evaluate this explanation, we estimate a heterogeneous effects model similar to those described above, using an indicator variable for the incumbent’s running for reelection in period  $t + 1$  (Table 7, column 7). We find no evidence that corruption increases disproportionately in municipalities in which mayors do not run for reelection. Because the reelection decision is potentially endogenous, we take these results with caution.

*Political Cycles.*—An additional concern is that political cycles are potentially correlated with our comparison of municipalities based on the timing of the audits. Municipalities receiving timely audits do cover time periods further away from the election relative to those receiving later audits (see panel A of Table 1), which could affect the comparability of the audit outcomes across these groups. We examine whether this issue affects our results by controlling for the actual timing of the audited periods (i.e., the start of the audit period, and the time span of the audit period) (Table 7, columns 4, 8). We find no influence upon any of our results.

*Transfers from Central Government.*—It is plausible that the central government may have increased the level of transfers to municipalities after favorable audits (and vice-versa). 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 (Litschig and Morrison 2013; Brollo 2010). To examine this hypothesis, we use the data on municipal government income statements, which provide us with the following additional revenue information: property tax, licensing, waste disposal services, transfers, and other government revenues. We estimate the relationship between the timely audits and

the fiscal year-specific revenues by source. To the extent that the available data allow us to assess this alternate explanation, we find no evidence of this channel in the data in this context (not reported, available from the authors upon request).

*Effects by Degree of Competition.*—We estimate heterogeneous effects of audits by the competitiveness of the mayoral seats using our summary measure of incumbency advantage. Consistent with the short-term disciplining effects being concentrated among municipalities with competitive elections, the longer-term impacts in competitive seat municipalities imply an increase in rent-seeking levels of 0.84 findings per report (60–95 percent), whereas the estimated effects among municipalities with a significant party incumbency advantage are statistically indistinguishable from zero (Table 7, column 9).<sup>47</sup>

*Strategic Challenger Entry.*—Is the reputation building that may take place 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) help inform voters about the characteristics of candidates? These additional strategic interactions could compound the effects discussed in the paper (e.g., political parties can strategically choose to field candidates as a response to audit results). In complementary work, we try to distinguishing the relative magnitudes of the incumbent's own reputation from these additional interactions, and find no evidence in support of this channel (Cámara Fuertes and Bobonis 2015).

### VIII. Conclusion

The central goal of this paper is to study the causal effect of monitoring politicians' corrupt actions on future levels of corruption. We use unique longitudinal data on municipal government audits in Puerto Rico to study this relationship empirically. We find that audits lead to a significant short-term reduction in municipal corruption, as well as an increase in incumbent mayors' electoral accountability. However, municipal corruption levels in the subsequent round of audits are on average *the same* in municipalities audited preceding the previous election and those whose audits became publicly available afterward.

Our empirical findings are relevant for policy in several ways. Most directly, our results suggest that periodic and predictable audits are not sufficient to persistently deter corrupt behavior. Audit programs must be timely, sustained, and long-term commitments in order to be effective. Moreover, the patterns we document underscore the importance of disseminating audit results at the time when they are most relevant for voters: shortly before an election. While our negative result on the long-term effects of audits on corruption need not apply to alternative audit schemes such as the randomized audit program studied by Ferraz and Finan (2008, 2011), it demonstrates that the persistence of these effects cannot be assumed.

<sup>47</sup> Among untimely audit municipalities, those with competitive mayoral seats report 29 to 54 percent lower corruption than those with noncompetitive elections (not reported in the tables). This evidence is also consistent with rent-seeking levels being lower in jurisdictions with greater electoral competition.



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

Finally, our conclusion that local administrations (in Puerto Rico) are prone to opportunistic rent-seeking may be somewhat troubling from a normative standpoint. Public policies addressing politicians' wages and public image could help improve the quality of candidates. More research on the determinants of politician characteristics, along the lines of Caselli and Morelli (2004); Besley (2004); Ferraz and Finan (2011); Fisman et al. (2015); Gagliarducci and Nannicini (2012); and others, is needed to develop a better understanding of these issues and their appropriate policy responses.

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