

Bobonis, Gustavo J.; Cámara Fuertes, Luis R.; Schwabe, Rainer

Working Paper

The dynamic effects of information on political corruption: Theory and evidence from Puerto Rico

Working Papers, No. 2012-14

Provided in Cooperation with:

Bank of Mexico, Mexico City

Suggested Citation: Bobonis, Gustavo J.; Cámara Fuertes, Luis R.; Schwabe, Rainer (2012) : The dynamic effects of information on political corruption: Theory and evidence from Puerto Rico, Working Papers, No. 2012-14, Banco de México, Ciudad de México

This Version is available at:

<https://hdl.handle.net/10419/83736>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

Banco de México
Documentos de Investigación

Banco de México
Working Papers

N° 2012-14

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

Gustavo J. Bobonis
University of Toronto

Luis R. Cámara Fuertes
University of Puerto Rico

Rainer Schwabe
Banco de México

December 2012

La serie de Documentos de Investigación del Banco de México divulga resultados preliminares de trabajos de investigación económica realizados en el Banco de México con la finalidad de propiciar el intercambio y debate de ideas. El contenido de los Documentos de Investigación, así como las conclusiones que de ellos se derivan, son responsabilidad exclusiva de los autores y no reflejan necesariamente las del Banco de México.

The Working Papers series of Banco de México disseminates preliminary results of economic research conducted at Banco de México in order to promote the exchange and debate of ideas. The views and conclusions presented in the Working Papers are exclusively of the authors and do not necessarily reflect those of Banco de México.

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

Gustavo J. Bobonis[†]
University of Toronto

Luis R. Cámara Fuertes[‡]
University of Puerto Rico

Rainer Schwabe[§]
Banco de México

Abstract: Does the disclosure of information about corrupt activities induce a sustained reduction in corruption? We use longitudinal data on audits of municipal governments in Puerto Rico to answer this question. We find that corruption is lower in municipalities audited before an election. However, these municipalities do not exhibit decreased levels of corruption in subsequent audits. Mayors in municipalities audited preceding the previous election have higher re-election rates, suggesting that audits enable voters to select more competent politicians. We present a political agency model that rationalizes the observed short-term and dynamic effects of information on corruption and re-election rates. We conclude that audit programs must be timely, sustained, and long-term commitments in order to be effective.

Keywords: Corruption; information; political agency.

JEL Classification: D72, H41, K42, O17.

Resumen: ¿Proveer información sobre actos corruptos lleva a una reducción sostenida en la corrupción? Utilizamos datos longitudinales sobre auditorías a gobiernos municipales en Puerto Rico para responder a esta pregunta. Encontramos que la corrupción es menor en municipios que se auditan antes de una elección. Sin embargo, estos municipios no presentan niveles de corrupción menores en auditorías subsecuentes. Alcaldes en municipios que se auditaron antes de la elección anterior tienen mayores tasas de reelección, lo que sugiere que las auditorías permiten que los votantes elijan políticos más competentes. Presentamos un modelo de agencia política que racionaliza los efectos de corto y largo plazo de la información sobre la corrupción y las tasas de reelección. Concluimos que los programas de auditorías deben ser compromisos oportunos, sostenidos, y de largo plazo para ser efectivos.

Palabras Clave: Corrupción; información; problemas de agencia política.

*We are thankful to numerous colleagues and seminar participants for helpful comments and suggestions, and to the OCPR for providing access to audit reports. Research support from the Canadian Institute for Advanced Research (CIFAR), the Social Sciences and Humanities Research Council of Canada (SSHRC), and the University of Puerto Rico Social Research Center and Office of the Dean for Graduate Studies and Research is gratefully acknowledged. We are responsible for any errors.

[†] Department of Economics, University of Toronto. Email: gustavo.bobonis@utoronto.ca.

[‡] Department of Political Science, University of Puerto Rico. Email: luisraulcamara@gmail.com.

[§] Dirección General de Asuntos del Sistema Financiero. Email: rschwabe@banxico.org.mx.

I. Introduction

In a well-functioning representative democracy, citizens select competent politicians to administer public affairs and hold them accountable for their performance. To succeed in these tasks, citizens must have appropriate information about candidates' characters, abilities, and performances while in office (Manin, Przeworski, and Stokes 1999; Besley 2006). Accordingly, a growing body of research finds that voters' access to evaluations of politician performance enhances government responsiveness, reduces corruption and rent-seeking behaviors, and promotes electoral accountability in the short-run.² However, it is less well understood whether information dissemination policies can generate a *sustained* reduction in rent seeking; this depends on the nature of differences among politicians and the dynamic incentives they face. 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 opportunistic politicians, there need not be dynamic effects of information on the level of corruption.³

The central goal of this paper is to study the causal effect of the disclosure of information about politicians' corrupt actions on future levels of corruption. The requirements to study this question empirically are quite demanding. We need exogenous variation in publicly available information on politician's actions as well as longitudinal data on political corruption. We take advantage of a unique setting that provides us with the opportunity to examine such relationships.

The government of Puerto Rico has established an independent body that systematically conducts municipal government audits, the findings of which are made publicly available and disseminated to media sources. This allows us to construct a longitudinal dataset of the extent of corruption for all municipalities during the period 1987-2005. In addition, two features of the audit program allow us to exploit exogenous variation in the level of public information on politicians' corrupt activities at the time of an election. First, municipalities are audited in a pre-determined order, making the timing of audits and their dissemination exogenous. Second, audit reports released in the period leading up to an election – pre-election audits – are more likely to inform on the incumbent mayor's activities than those reports published shortly after an election due to a high independent turnover rate of politicians.⁴ We thus exploit

² For evidence regarding government responsiveness, see e.g., Besley and Burgess (2002) and Björkman and Svensson (2009); regarding corruption and rent-seeking behaviors, see e.g., Reinikka and Svensson (2005) and Olken (2007); regarding electoral accountability, see e.g., Ferraz and Finan (2008) and Banerjee et al. (2010).

³ 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.

⁴ The contrast between the pre- and post-election 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 post-election audits does reach

the exogenous ordering of municipal audits to help us establish the causal relationships of interest. Specifically, we first observe whether audit results are published before a particular election – when they are most salient to voters – or shortly after it. We then compare the governments’ levels of reported corruption across these pre-election and post-election audit groups of municipalities in subsequent terms.

We find that pre-election 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.⁵ However, in contrast to these desirable short-run consequences of the audits, municipal corruption levels in the subsequent round of audits are, on average, *the same* in municipalities audited preceding the previous election and those whose audits became publicly available afterwards.

The short-lived effect of information on the level of corruption is consistent with a broad class of pure moral hazard models of electoral politics in which all politicians are identical. However, additional evidence suggests that there is political selection at play after all. We find that incumbent re-election rates in the subsequent election are significantly higher in municipalities in which there was a pre-election audit. The presence of selection effects in observables and future re-election rates, but not in corruption, is prima-facie evidence in favor of the view that the information contained in the audits helps voters select competent but opportunistic, rather than honest or virtuous, politicians.⁶

To make sense of this particular combination of sanctioning and selection effects, we propose a model of political agency with moral hazard and heterogeneous politicians. In this model, voters decide whether to re-elect an incumbent politician but are unable to observe his type or his actions as an officeholder. Publicly disseminated audit reports provide information to voters about these actions. Because voters cannot credibly commit not to re-elect incumbents who would outperform challengers, a mayor whose reputation has improved in the past can exploit this to engage in rent-seeking activities in a later term. Because of this, mayors in municipalities whose actions, either corrupt or honest, have been made public in the past will be on average as corrupt in the next term as those whose actions have not been made public. That is, there are no dynamic effects of information on the level of political corruption.⁷

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)).

⁵ The disciplining and electoral accountability effects are consistent with the experimental findings in Olken (2007) and Ferraz and Finan (2008), respectively. As for politician selection effects, see for instance Besley (2005), Besley, Pande and Rao (2007), and Brollo et al. (2010).

⁶ For a discussion of competence and honesty as distinct dimensions of the quality of politicians, read the introduction to Caselli and Morelli (2004).

⁷ This is consistent with information dissemination on politicians’ actions leading to an increase in *ex ante* voter welfare.

Our model also predicts that information provided today induces higher re-election rates of incumbents in the following election for two reasons. First, an incumbent's expected reputation, i.e. the likelihood that he is a competent type, is better following an audited period. Because more able types are more likely to refrain from corruption, the model predicts a positive selection effect on re-election rates in the subsequent term. More interestingly, although a mayor with a better reputation should be more rent seeking, in equilibrium voters' re-election rules are less stringent so that the incumbent finds them easier to meet. Thus, both the selection and sanctioning effects induce higher re-election rates of incumbents in the following election.

Our model of electoral accountability does not capture several aspects of political life which could potentially help explain our empirical findings. In particular, we do not model lifecycle or strategic retirement effects, party-induced discipline, or political cycles. While ruling out *all* possible explanations consisting of some combination of these, or other, stories is infeasible, we have several reasons to be confident in advancing an accountability-based explanation. First, electoral accountability has been shown to be an important channel of influence for similar information dissemination programs. Second, our model provides a relatively simple mechanism relating information dissemination to a range of observable outcomes. Most importantly, the research design and the richness of the data allow us to distinguish our explanation for corrupt behavior from several of the most likely alternative interpretations.

Even though the timing of the municipal government audits is pre-determined, our results would be undermined if the actual auditing process differs systematically before and after elections. We do not, however, find any evidence that auditors were corrupt or that mayors with more political power or mayors affiliated with higher levels of government act in ways inconsistent with our main results. A second concern is that political cycles are potentially correlated with our comparison of municipalities based on the timing of the audits. However, we report evidence that the actual timing of the audited periods does not influence our results. Third, we evaluate whether changes in the national political environment – negative shocks to the popularity of political parties in particular – can generate our results, and we find no evidence to support this alternate explanation. Fourth, the effects of interest are significant only in municipalities in which elections are competitive (i.e., there is some historical alternation of parties), supporting the view that political accountability is the mechanism generating our results. Finally, we discuss evidence inconsistent with other plausible channels, such as responses from higher levels of government to audit results.

The study contributes to the growing empirical literature documenting how electoral accountability, and information provision in particular, influences political corruption.⁸ Most notably, in a series of papers Ferraz and Finan (2008; 2011) use similarly objective measures of corruption from audit reports of municipal governments in Brazil to study whether electoral accountability serves as a mechanism to align politicians' actions with voters' preferences. Specifically, they show that electoral accountability is enhanced when information about corrupt practices in audited municipalities is publicized, as well as the extent to which re-election incentives affect political corruption in the short-run.⁹ Using a randomized experiment in Indonesian villages, Olken (2007) analyzes whether different monitoring mechanisms reduce corruption in infrastructure projects, and finds that a top-down auditing scheme is effective in decreasing corruption in the short-run. Finally, Niehaus and Sukthakar (2011) 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 information revelation on political corruption. This has important policy implications because it suggests that the benefits of revealing information on politicians' actions can be short-lived.

The paper also improves our understanding of the role of information and politician heterogeneity in political agency models. While a theoretical literature has evolved in which political agency models with varying assumptions on the structure information and the nature of the differences between politicians are examined, empirical work validating these assumptions is only beginning to flourish (see Besley 2006 and Ashworth 2012 for reviews of the theoretical literature and its connection with recent empirical work). Alt et al. (2011) use variation in gubernatorial term limits across U.S. states to identify sanctioning and selection effects in fiscal policy. Gagliarducci and Nannicini (2012) use a regression discontinuity approach which exploits population-based variation in the pay of Italian mayors to tackle these questions. Both papers report evidence consistent with selection effects being the main driver of differences in fiscal policy.¹⁰ Studying legislators in Brazilian municipalities, Ferraz and Finan (2010) 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. Gordon and Huber (2007) find that judges in partisan competitive systems sentence more punitively than those in retention systems. Using

⁸ Stromberg (1999), Gentzkow (2006), Gentzkow, Glaeser and Goldin (2006) provide historical evidence of the consequences of media access on political behaviors. Besley and Burgess (2002) show that newspaper circulation affects the responsiveness of state governments in India to negative shocks to food production and flooding.

⁹ For evidence on the policy consequences of re-election incentives, see Besley and Case (1995) and List and Sturm (2006). Martínez-Bravo et al. (2007) study the consequences of the introduction of local elections for local politicians' accountability.

¹⁰ 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.

variation in judges' electoral calendars, they are able to attribute this to the incentive effects of political competition rather than selection of more punitive judges. Our work complements the literature by highlighting an alternative mechanism for distinguishing sanctioning and selection effects based on the dynamic effects of variation in voter information.

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

II. Background

II.A. Municipal Government Administration and Politics

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

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

¹¹ 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.

¹² In 1991 the legislature approved a series of laws as part of a package of municipal reforms. These municipal reforms, of which Act No. 81 was the centerpiece, greatly increased the municipal governments' autonomy vis a vis the central government and allowed them a greater role in the social and economic development, as well as the spatial planning, of their territories. Thus once the municipal reform laws became effective some municipalities began to assert a greater role in education and law enforcement, areas previously reserved for the central government. In practice, the degree of autonomy and sphere of action that each municipality has is related to its size. Large municipal governments with active mayors such as San Juan (the capital), Guaynabo,

II.B. The OCPR Municipal Government Auditing Program

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

The OCPR has been carrying out audits on municipal governments and generating and disseminating reports uninterruptedly since 1953. 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 latest audit coverage period. Following this preliminary audit, a team of approximately 10 OCPR auditors are sent to the municipality to examine these accounts and documents, as well as to inspect for the existence and quality of public work construction and delivery of public services. Auditors also interview municipality officials, members of the local community, as well as municipal council members, in order to get direct complaints about any malfeasance. Once the audit is complete, the auditing team completes a preliminary audit report. This preliminary report is shared with the municipality officials (i.e., the mayor and top management) to provide them with an opportunity to contest its findings. Once the response is received and evaluated, a final report is issued and disseminated to the public and to media sources through press conferences (more recently, reports are also being posted on the Internet). The OCPR may publish multiple reports on a municipality for one auditing period depending on the size or complexity of the municipal government.

A number of measures are taken to minimize potential biases in the conduct of the audits and in the dissemination of their findings. First, there is a constitutionally defined objective to provide the OCPR with a substantial degree of autonomy from the rest of the central government structures, in order to isolate the agency from undue external interference. To help achieve this, the Comptroller is appointed by the P.R. Governor for a ten-year term.¹³ 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, who are hired based on a competitive public examination and earn highly competitive salaries, receive extensive training prior to visiting the municipalities. Finally, in

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.

¹³ 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.

order to reduce local-level conflicts of interest, individual auditors are precluded from participating in audits of their municipality of residence.

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

All seventy-eight municipalities were audited during our period of interest (1987-2005) multiple times. The timing of the dissemination of the reports is depicted in Figure I. As can be seen, there is a tendency to publish reports at the end of the central government's fiscal year (in June), as well as a tendency to publish more reports in recent years.¹⁴ Importantly, there is no significant tendency for the OCPR to publish a disproportionate number of reports in the months preceding an election (August through October) (Figure I, Panel A). There is also no evidence of bias in the publishing of reports for municipalities in which the incumbent mayor is in the opposition to the Governor in office or to the party of the Governor who appointed the Comptroller (Panel B).

Although the reports do not provide information on the start date of an audit, they provide information on the date of dissemination of the audit reports; we can use this information to ascertain to a first approximation whether the predetermined order rule is satisfied in practice. Specifically, for each pair of audits for a municipality, we should observe 77 sets of audits in between (given that there are 78 municipalities). Panel C thus plots the distribution of the number of audit reports of other municipalities published in between two audit reports for each municipality. There are on average 76.2 (median = 75.5) reports of other municipalities between each pair of own municipality reports.¹⁵ This serves as *prima facie* evidence that the timing of the audits can be considered pre-determined and that the agency does not time the dissemination of findings to influence electoral results.

Corruption in municipal governments in Puerto Rico takes diverse forms, but corruption schemes used by local politicians and bureaucrats are based on a combination of fraud in procurement, the use of fake receipts (i.e., "phantom" firms), the illegal hiring of employees, and over-invoicing the value of products or services. In addition, the audit reports also suggest that some individuals simply divert resources for personal purposes. Since these strategies are complementary in allowing government

¹⁴ 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.

¹⁵ The distribution is also reasonably tightly distributed; the 10th percentile (90th percentile) of the distribution is 44 (108) other municipality reports. Note that as a measure of order of audit start dates this includes some (measurement) error, since there can be discrepancy between the order of audit start dates and report dissemination dates, given the size of the municipal governments and thus the complexity of the audits. This would lead to a higher variance in the reported number relative to that based on true start dates.

representatives to appropriate resources (and following the existing literature), we combine these into a single measure (see Section III.A).

Some examples will help illustrate the types of irregularities uncovered by the audits.¹⁶ In the municipality of Maunabo during February-March 1997, contracts for the pavement and maintenance of roads summing up to approximately 138K USD were partitioned into four separate projects in order to avoid having to carry out a public auction.¹⁷ 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.

As an example of corruption in the hiring of municipal employees, the case of the municipality of Toa Baja is illustrative. In a report published in June 2000, the OCPR reports the illegal hiring of 22 individuals who were relatives of the mayor and 11 individuals who were relatives of members of the Municipal Assembly. Twenty-one of these individuals, hired between September 1991 and October 1997, did not have the academic requirements or other minimum requirements to serve in their posts. These and other documented irregularities, including the excess compensation of municipal employees by approximately 262K USD, are classified as one finding of corruption in HR practices. Analogous findings in the municipalities of Cidra and Maricao are available in Appendix B.

Other examples of corruption in Maricao and Hormigueros illustrate instances of over-invoicing. In October 1998, the mayors in both municipalities formalized contracts for the collection and disposal of debris resulting from the damages caused by Hurricane Georges (in September 1998), for an estimated cost of 4.20 and 3.69 million USD (the cost per cubic yard of 28 and 26 USD), respectively. The OCPR reported evidence from the Federal Emergency Management Agency (FEMA) and the U.S. Army Corps of Engineers of over-invoicing in both cases, as the independent contractors submitted invoices for the collection and disposal of 155,157 and 51,683 cubic yards of debris, whereas it was identified that they collected 50,157 and 31,508. This represented over-invoicing by approximately 2.94 and 0.75 million USD, respectively. The OCPR referred the violations to the Department of Justice. As a consequence of the audit report, the former (two-term) mayor of Hormigueros was convicted on extortion and bribery charges for requesting and receiving 100K USD in kickbacks from the owner of the contracting firm.¹⁸ In contrast, the mayor of Maricao (in his third term) was re-elected in 2004, following the dissemination of the audit report in 2001.

News on the findings from the audit reports are routinely reported in the island-wide press – the main sources are of OCPR press conferences and releases as well as opposition candidates' campaigns.

¹⁶ For details of the audit report findings, see excerpts from these in Appendix B.

¹⁷ The 1991 Municipal Government Law (“Ley de Municipios Autónomos del Estado Libre Asociado de Puerto Rico de 1991 (Law Num. 81) establishes that for any project exceeding 40K USD, the municipal government must carry out a public audit.

¹⁸ See *El Pueblo de Puerto Rico, Apelado v. Francisco Javier Rivera Toro, Apelante*. KLAN0501622. Tribunal de Apelaciones de Puerto Rico, Región Judicial de Mayagüez. 2009 PR App. LEXIS 3664, 20 de octubre de 2009.

Although we do not have direct evidence showing that voters learned about the audit reports, anecdotal evidence suggests that the information from the audits did reach voters. For instance, an article published on September 25th 2008 (preceding the 2008 election) in a major newspaper regarding the outcome of a recent audit of the municipality of San Juan highlighted findings of mismanagement attributed to municipal employees. Specifically, the report highlighted that Jorge Santini – the mayor – and the municipality’s finance team did not appropriately administer the municipality’s finances and incurred in extravagant/unnecessary expenditures to highlight the Mayor’s image. The report was used by Ferdinand Pérez (the opposition candidate) to declare that Santini was “a disaster as an administrator”. 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 re-elected for a third term.

III. Data

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

The main data sources for the study are the municipal audit reports conducted by the OCPR. In this study we work with all municipal audit reports during the 1987-2005 period, which are relevant for the 1988 through 2004 elections. Note that there were two Comptrollers during the period for which we use the audit reports: Ileana Colón Carlo (1987-1997) and Manuel Díaz Saldaña (1997-2010).¹⁹

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 implicated individuals (if identifiable), and the reason why it is considered a violation. We generate codes from each report’s list of findings.²⁰ For each finding we coded the type of individual implicated in the finding – whether it was (i) the mayor or vice mayor, (ii) a member of the municipality’s top management such as the finance director, (iii) a rank and file employee of the municipality, or (iv) whether the individual cannot be identified.

Although the OCPR cannot officially classify findings as corrupt violations or not, the agency refers findings of misuse of public funds to the P.R. Department of Justice and/or to the state-level executive branch’s Office of Government Ethics. We created a code that specified whether the finding constituted an act of corruption or not. We operationalize corruption as an act by any municipal employee

¹⁹ Díaz Saldaña exceeded his ten-year term because the then-governor did not submit a candidate to the legislative assembly when his term expired (in 2007), and the incoming governor selected a replacement in 2010. The Constitution states that the incumbent Comptroller will continue to occupy his position until he resigns or is substituted by a new one.

²⁰ Before we began the coding process, the three (3) research assistants were given extensive training in content analysis, coding, and the details of the audit reports. We also ran tests for inter-coder and intra-coder reliability. The process continued until coder reliability was at least 0.9. The same coders worked with the reports throughout the project. Finally, a fourth research assistant examined the data to check for any errors.

that led to a *personal* financial or political benefit.²¹ Thus, the mayor receiving a bribe for a contract, or using municipal employees for his or her electoral campaign would be considered in our coding scheme as acts of corruption. On the other hand, poor bookkeeping was not (unless the report stated that it involved the cover-up of a corrupt violation).

To construct measures of corrupt violations, we follow Ferraz and Finan (2008; 2011) and combine these indicators by summing up the number of times each one of these irregularities appears. However, in contrast to their previous work, because the OCPR may publish multiple reports on a municipality during one auditing period and this depends on the size or complexity of the municipal government, we normalize our measures by the number of reports published in that auditing period. 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. This measure captures the incidence of corruption relative to overall mismanagement or waste in the municipality.²² Finally, as will be made clearer once we discuss the study's research design, we define pre-election audit reports as those whose results are published during the two years preceding the election, and post-election audit reports as those published in the two-year period following it. When municipalities have audit reports published in both periods, we aggregate only those reports published before the election and assign them to the pre-election audit group.

The empirical analysis focuses on the sample of municipalities in which the mayor is running for re-election around the time of the audit under study. This reduces the sample from 326 to 241 municipality-time observations (by 26 percent). This restriction limits the analysis to cases in which the re-election motives of interest are at play. That said the results are qualitatively and quantitatively similar using the overall sample of municipalities.

Table I, Panel A presents the means (and standard deviations) of these audit outcome variables, overall and by pre-election/post-election audit status. The audits report on average 1.3 corrupt violations per report, and there is substantial heterogeneity in the number of findings (standard deviation = 1.77). Approximately 40 percent of these findings (0.53 violations) are attributed to the mayor or vice-mayor, and 44 percent of these (0.59 violations) are referred to the Department of Justice, on average. Examining the second set of measures, we find that a non-negligible share of findings attributed to the mayor or vice-mayor or referred to the DoJ – 17 and 26 percent, respectively – are classified as corrupt. As can be expected due to their accountability effects, audits carried out and disseminated in the pre-election period show 27-61 percent less corruption than those carried out post-election (column 4). Panel B reports other

²¹ This definition is similar to the one used by the OCPR, which states that corruption is the use of government functions for private gain (Díaz Saldaña 2007). However, the OCPR does not specify whether a finding is considered a corrupt violation or not.

²² This relates to the distinction between active and passive waste highlighted in Bandiera, Prat, and Valletti (2009).

relevant characteristics of the audits, such as the number of reports resulting from the audit, as well as the start and end of the audit period (and time span) covered by the reports.

III.B. Other Data Sources

We employ additional data available from the P.R. State Electoral Commission (CEE), containing the electoral results of the municipal and statewide general elections for each municipality for election years 1988 through 2004. These data allow us to construct measures such as whether the incumbent mayor runs for re-election in the general election, whether he/she is re-elected, the vote share and win margin for the election, his/her political party affiliation, whether he/she is in the opposition to the party in power at the state level, and the terms in office.²³ As for municipal government-level variables, we use annual municipal government budget data for the fiscal years 1991-92 through 2007-08. To capture underlying variation in municipal characteristics, we rely on the 1990 and 2000 U.S. Census of Population for Puerto Rico. We use measures of the proportion of adult individuals ages 25 and older with schooling attainment levels lower than ninth grade, with a high school education or more, and with a college education or more, as well as the municipality's household median income and poverty ratio for the years 1989 and 1999. Finally, we use information on municipality-level annual unemployment rates from the P.R. Department of Labor.

Descriptive statistics of these outcome and control variables, overall and by pre-election/post-election audit status, are available in Table II. Panel A reports various electoral outcomes – whether the incumbent runs for re-election and whether the incumbent party wins (unconditionally), and the incumbent mayor's re-election rate conditional on running for re-election. Most salient is the fact that both incumbent mayor re-election 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 territorial level.

Panel B reports other political characteristics of incumbent mayors running for re-election. An approximately equal number of mayors in the sample are affiliated with the NPP or PDP, and although only 32 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 half are in the opposition to the party of the Governor who appointed the incumbent Comptroller. Incumbent mayors have been in office for approximately 1.5 terms on average. Nonetheless, these measures mask a great degree of heterogeneity in the safety of municipal seats. There is substantial variation in incumbent mayors' terms

²³ 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 this data to examine, *for this sub-sample*, whether the audits induce positive or negative selection of politicians based on their possible pre-incumbency earnings – 5 years before the relevant election. These results are more suggestive as they cover a limited sample period – see Appendix C for details.

in office; 58 percent have been in office for two or more terms and 35 percent for three or more terms.²⁴ 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 one if the party has controlled the mayoral seat for the past three terms and zero otherwise. Based on this measure, a significant share of seats (48 percent) has a strong party incumbency advantage.

In Panels C and D, we present summary statistics of municipality and municipal government pre-determined characteristics, based on the available population census and municipal government budget data. The census data suggests that municipalities receiving pre-election audits have somewhat more educated adult populations, higher median income levels, and lower poverty ratios, but the differences are small in magnitude (Panel C). We also report summary statistics of municipal government budget items for the first and second years of the incumbent mayor's term (Panel D). Although it is not completely reasonable to assume that these are pre-determined, it is comforting that these measures do not vary systematically across municipalities in the pre-election and post-election audit groups.

IV. Theoretical Framework

In this section we present a simple political agency model to help interpret our empirical findings. In brief, the story that our model tells is as follows. Voters must decide whether to re-elect an incumbent politician, but are generally unable to observe his degree of competence or his actions. Audits give voters information on the actions taken by politicians, which allows them to provide better incentives and to separate competent politicians from incompetent types (or those who are hopelessly corrupt). Thus, information reduces corruption in the short-term and enables the selection of competent politicians. Once re-elected, 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 a first-term mayor. Thus, even though there is selection, there are no dynamic effects of information 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 re-election rates.

Our model highlights an alternative mechanism for distinguishing sanctioning and selection effects based on the dynamic effects of variation in voter information. The information contained in audits enables voters to better monitor incumbents and distinguish good types from bad types. The improvement

²⁴ 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.

in monitoring is short-lived and affects incumbent behavior before the voter has had a chance to use the information for selection. Therefore, short-run differences in corruption can be attributed to a sanctioning effect. Long-run differences in corruption, however, cannot be due to differences in monitoring. Rather, they must be due to differences in the long-run distribution of incumbent types – a selection effect.²⁵

Reputation and Accountability in Repeated Elections

Consider a discrete-time, infinite horizon model of municipal politics. In each period, indexed by $t \in \{1, 2, \dots\}$, a representative voter must select a politician to administer local public affairs. Once in office, the elected politician engages in corruption $\kappa \in [0, 1]$. Political corruption is bad for voter utility $u \in \{0, 1\}$, but its effects cannot be distinguished from other negative shocks which happen with exogenous probability $1-\gamma$. Specifically, a level of corruption κ leads to low voter utility with probability κ . Thus, the voter's expected utility in the stage-game is $E(u|\kappa) = \gamma(1-\kappa)$.

The parameter γ measures the severity of the monitoring problem faced by the voter.²⁶ When $\gamma = 0$, the voter cannot tell whether the politician was corrupt or not in any given period, and can thus provide no incentives for good behavior. If $\gamma = 1$, on the other hand, the voter can perfectly distinguish politicians' actions and audits become redundant.

Politicians are one of two types – responsive or corrupt – with μ denoting the proportion of responsive types in the infinite pool of potential candidates. Responsive politicians decide how much corruption to engage in; their action set is $\kappa \in [0, 1]$. Their per-period utility while in office is $u_p(\kappa) = E + R(\kappa)$, where $E > 0$ measures ego-rents, salary, and other fixed benefits of holding office and $R(\kappa)$ are rents derived from corrupt acts. We assume that R is strictly increasing, differentiable, strictly concave and $R(0) = 0$. Payoffs outside of office are normalized to 0. In contrast, corrupt politicians always engage in all-out corruption ($\kappa = 1$).²⁷ This may be because the payoffs to corruption are too large (i.e., R is very large for them), because other interests such as organized crime have the ability to punish them if they do not extract rents, or due to incompetence or an inability to manage government funds effectively. Each politician is infinitely lived and may serve for as many periods (i.e., terms) in office as the voter asks him to. However, once replaced by a randomly selected challenger, a politician cannot return to office. Politicians and the voter share a common discount factor $\delta \in (0, 1)$.

²⁵ It is also possible that these differences are due to conditioning of continuation strategies on past audits, which is unrelated to the information revealed. However, this seems an unlikely explanation.

²⁶ It also measures the impact of political corruption on voter utility. This interpretation is less salient to the present context, however, as voters always prefer to limit corruption.

²⁷ In this setup, the voter is indifferent among politician types when responsive politicians choose full corruption ($\kappa=1$). Schwabe (2011) shows, in a more general setting, that the analysis goes through when there is a small advantage to having a responsive type in office even if he is expected to show no restraint.

The parameter μ measures the severity of the selection problem facing the voter. When $\mu = 1$, all politicians are responsive and the voter can focus all of his efforts on the moral hazard problem – providing incumbents with incentives to avoid corruption. However, as μ becomes small, incentives will rarely work and the voter will find it more important to identify and keep responsive politicians.

To help remedy the voter’s monitoring problem the OCPR conducts periodic audits in which the financial activities of the government are scrutinized and any irregularities are reported to voters. We interpret audits as making politicians’ corruption, κ , publicly observable. We write $a_t = A$ to denote an audit at time t , and $a_t = NA$ otherwise. An audit will take place before any given election with probability $p \in (0,1)$. To match the context, we assume that politicians know whether they will be audited when making their corruption decisions.²⁸ An audit in the model corresponds to a pre-election audit in our empirical framework, while $a_t = NA$ corresponds to post-election audits not observed by the voter at the time of the election.

In each period t , the voter assigns a probability μ_t that the incumbent is a responsive type; this is the politician’s *reputation*. New politicians are selected randomly (the standard approach in the literature) so that the reputation of a politician at the beginning of his first term is μ . Thereafter, the incumbent’s reputation is updated according to Bayes’ rule each time the voter observes u or κ , via a function that we denote $\hat{\mu}$.

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

When making re-election decisions, the voter has information on all past realizations of u , audits (κ), and election results, which we call a t -history h_t . A re-election strategy is a function from the set of all such possible t -histories to the incumbent’s probability of re-election: $\sigma : H \rightarrow [0,1]$. Similarly, a politician’s corruption strategy is a function from all possible histories of outcomes, as well as whether there will be an audit ($a_t \in \{A, NA\}$), to a level of corruption: $\kappa : H \times \{A, NA\} \rightarrow [0,1]$.

Equilibrium

²⁸ Theoretical results are qualitatively similar in a model with random audits. In general, the relative welfare effects of predetermined vs. random audits depends on politicians’ and voters’ preferences. With pre-determined audits, there is alternation between high and low corruption, while random audits lead to a steady level of equilibrium corruption somewhere in the middle. Politician’s risk aversion determines the relative level of corruption with random audits, while the curvature of voters’ utility functions determines the costs of variability in corruption.

We focus on perfect Bayesian equilibria. As is common in infinitely repeated games, there are many candidate equilibria. For instance, the strategy profile in which politicians always engage in all out corruption ($\kappa=1$) and the voter never re-elects them is a perfect Bayesian equilibrium. On the other hand, trigger strategies using this “bad equilibrium” as a punishment for deviation can support a variety of equilibrium behaviors. However, the credibility of equilibrium punishments that hurt both the voter and the incumbent politician is questionable – they are not renegotiation-proof. This idea is fleshed out in Schwabe (2011), where a class of equilibria meeting a stringent test of credibility while leaving room for the voter to provide incentives in a simple manner is proposed. In these reputation-dependent performance cutoff (RDC) equilibria, the voter makes re-election decisions using a performance threshold that varies with reputation, making the best-response expected level of corruption the same for incumbents of all reputations. This, in turn, makes the voter indifferent between keeping the incumbent and electing a challenger. The voter’s indifference makes his re-election strategy credible. We further restrict our attention to the RDC equilibrium yielding the highest feasible payoffs to the voter. We call these *voter-optimal* RDC equilibria.

Definition 1: A voter-optimal RDC equilibrium with value V is a perfect Bayesian equilibrium in which:

- (a) The voter’s re-election strategies depend only on the observable outcome and the incumbent’s reputation: we denote them as $\sigma_A(\mu_t, \kappa_t)$ and $\sigma_{NA}(\mu_t, u_t)$.
- (b) Politicians follow a corruption strategy $\kappa(\mu_t, a_t)$ that satisfies voter-indifference: the present discounted value of the voter’s utility equals V whenever the incumbent’s reputation is at least μ ($\mu_t \in [\mu, 1]$).
- (c) The voter’s constant per-period expected utility $(1-\delta)V$ is maximal subject to these constraints.

Point (a) states that re-election strategies will depend only on observed corruption or voter utility, depending on whether there was an audit, and the incumbent’s reputation. Point (b) states that incumbents will vary the intensity of corruption in a way that perfectly offsets the risk to the voter of having a corrupt-type incumbent. Point (c) narrows our focus to the equilibria giving the highest possible utility to the voter, subject to the constraints imposed by the first two points. Point (b) has the following key implication:

$$\mu\gamma(1 - E(\kappa(\mu, a_t))) + \delta V = \mu'\gamma(1 - E(\kappa(\mu', a_t))) + \delta V,$$

which holds if and only if the expected level of corruption is equal at reputations μ and μ' :

$$(1 - \mu) + \mu[p\kappa(\mu, A) + (1 - p)\kappa(\mu, NA)] = (1 - \mu') + \mu' [p\kappa(\mu', A) + (1 - p)\kappa(\mu', NA)] \quad (1)$$

It is clear from (1) that, in expectation, responsive incumbents with better reputations engage in more corruption than those with worse reputations.

These equilibrium selection criteria play an important role in generating predictions. Voter optimality rules out equilibria in which available incentives are not used, leading to the natural prediction that corruption will be lower during audited periods. RDC equilibrium's appeal to renegotiation-proofness leads to the prediction that the expected level of corruption does not depend on the incumbent's reputation, and thus, does not depend on whether an audit was conducted during the previous period.

Along with equilibrium selection, two restrictions on the model's parameters will allow us to present a clean analysis. Specifically, we will assume that both the monitoring and selection problems are economically important (i.e. γ and μ sufficiently below one), in a way that will be specified below.

Proposition 1 describes the voter's re-election strategy in a voter-optimal RDC equilibrium when the selection problem is significant. The voter displays no tolerance of bad outcomes for first-term incumbents (the reputation- μ incentive constraints are binding). This greatly simplifies the analysis as incumbents of only two reputations, μ and 1, will hold office in equilibrium. During audited periods, the incumbent's action is perfectly observed, so incumbents are re-elected when observed corruption equals equilibrium corruption for responsive types. During non-audited periods, incumbents are re-elected when voter utility is high, although the voter will occasionally re-elect a high-reputation incumbent who does not deliver high utility.

Proposition 1: There exists $\mu^* \in (0,1]$ such that if $\mu < \mu^*$, in a voter-optimal RDC equilibrium, the voter's re-election strategy is of the form:²⁹

a) When a reputation- μ incumbent is audited:

$$\sigma_A(\mu, \kappa_t) = \begin{cases} 1 & \text{if } \kappa_t \geq \kappa(\mu, A) \\ 0 & \text{otherwise} \end{cases}$$

b) When a reputation- μ incumbent is not audited:

$$\sigma_{NA}(\mu, u_t) = \begin{cases} 1 & \text{if } u_t = 1 \\ 0 & \text{otherwise} \end{cases}$$

c) When a reputation-1 incumbent is audited:

$$\sigma_A(1, \kappa_t) = \begin{cases} 1 & \text{if } \kappa_t \geq \kappa(1, A) \\ 0 & \text{otherwise} \end{cases}$$

d) When a reputation-1 incumbent is not audited:

$$\sigma_{NA}(1, u_t) = \begin{cases} 1 & \text{if } u_t = 1 \\ k & \text{if } u_t = 0 \end{cases}$$

where $k \geq 0$.

²⁹ Only reputation 1 and μ are ever in office on the equilibrium path. For completeness, we specify voter re-election strategies off the equilibrium path as: $\sigma_A(\mu', \kappa_t) = \sigma_{NA}(\mu', u_t) = 0$ when $\mu' \neq 1$ or μ .

Proof: See Online Appendix A. ■

Short-Run Accountability Effects of the Audits

Because audits provide additional information about a politician's actions, they should enable the voter to punish high corruption and reward restraint more accurately, making incentives more effective. Thus, we should expect that corruption be lower during audited periods than during non-audited periods. The following proposition supports this intuition, when the monitoring problem is significant.³⁰

Proposition 2: There exists $\gamma^* \in (0,1]$ such that, in a voter-optimal RDC equilibrium, $\gamma < \gamma^*$ and $\mu < \mu^*$ imply that there is less corruption during audited periods than during non-audited periods: $\kappa(\mu_b, NA) > \kappa(\mu_b, A)$.

Proof: See Online Appendix A. ■

Effects of the Audits on Political Corruption in Future Periods

In equilibrium, politicians of all reputations will perform equally well (or poorly) in expectation so that the voter is indifferent between re-electing them and electing a new politician with reputation μ .³¹ This implies that politicians with a high reputation will pocket the benefits of their accumulated reputation by engaging in more corruption than responsive politicians of lower reputation. Interestingly, this implies that reported corruption from future audits should be, on average, constant across municipalities that faced an audit in an earlier period and those that did not.

Proposition 3: In the voter-optimal RDC equilibrium, a period t audit has no effect on period $t+1$ expected corruption: $E[\kappa_{t+1}/a_t=A] = E[\kappa_{t+1}/a_t=NA]$.

Proof: Corruption strategies are functions of reputation and a_t . Audits are determined independently each period. By equation (1), expected corruption is not affected by expected reputation. Therefore, $E[\kappa_{t+1}/a_t=A] = E[\kappa_{t+1}/a_t=NA]$. ■

Effects on Electoral Outcomes and Politician Selection

³⁰ By ensuring that reputation- μ incentive constraints are binding, Proposition 1 ensures that first-term incumbents will engage in more corruption during non-audited periods than during audited periods. However, it leaves open the possibility that the reverse is true for incumbents with high reputation. Indeed, because more corruption is allowed of high reputation incumbents there is slack in their incentive constraints and, for some parameter values, it may be optimal for the voter to be more lenient during audited periods in order to increase re-election rates and the value of holding office. However, this makes little sense if the monitoring problem is significant, considerably reducing the implementable level of restraint during non-audited periods.

³¹ This type of voter indifference is a part of any renegotiation proof equilibrium. See Proposition 3 in Schwabe (2011).

We can use the model's predictions about re-election rates (summarized in Table III)³² to draw conclusions about the effect of audits on politician selection. Specifically, incumbents are more likely to be responsive types following an audited period compared to a non-audited period.³³ This leads to the empirical implication that re-election rates will be higher in periods following a pre-election audit. Denote by $q_{t+1|A}$ and $q_{t+1|NA}$ the re-election probability of the incumbent in period $t+1$ given an audit and no audit in period t , respectively.

Proposition 4: Assume $\mu < \mu^*$ and $\gamma < \gamma^*$. In the voter-optimal RDC equilibrium $q_{t+1|A} > q_{t+1|NA}$.

Proof: See Online Appendix A. ■

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

From theory to empirics

The formal model provides four key predictions to be taken to the data:

- (i) the expected dissemination of the audit reports should decrease the number of corrupt violations by incumbent politicians in the short-run (Proposition 2);
- (ii) re-election rates at time t should be negatively correlated with the number of corruption findings (Proposition 1);
- (iii) politicians in power in the next term will engage *on average in the same level of corrupt violations* irrespective of the municipality being audited preceding or after the election (Proposition 3); and,
- (iv) on average, re-election rates at time $t+1$ should be higher in municipalities that experienced a pre-election audit at time t relative to those that did not (Proposition 4).

³² The model predicts that re-election rates will be higher after audited periods than non-audited periods. However, this prediction depends on our assumption that restraint may still lead to low voter utility, but all-out corruption will never lead to high voter utility. This assumption is made for convenience as it limits the number of cases that must be addressed (only reputation 1 incumbents are re-elected). Therefore, this particular prediction should not be taken literally.

³³ It is also worth noting that the probability of having a responsive type in office during period $t+1$ is higher when there is a responsive type in office during period t . This means that the selection effects of audits are persistent: for any integer n , the probability of having a responsive type in office during period $t+n$ is higher if there was an audit during period t than if there was not.

Predictions (i-ii) can be generated by multiple political agency models. In contrast, the second set (iii-iv, or Propositions 3-4) cannot (to our knowledge) be jointly explained by existing theoretical work. These are the main testable predictions that we take to the data.

V. Effects of the Audits on Corruption and Short-Term Electoral Accountability

V.A. Empirical Methodology

We compare the outcomes for municipalities whose audit reports were disseminated in the two-year period before each election to those whose audit reports were disseminated in the two-year period following each election, for the election years 1988 through 2000. These comparisons are illustrated in Figure II. In the following paragraphs, we present the empirical specifications used to test hypotheses (i)-(iv).

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

$$c_{m,t} = \theta A_{m,t} + \beta X_{m,t} + \gamma_t + \alpha_m + \varepsilon_{m,t}, \quad (2)$$

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 or not the municipality audit report was published in the two-year period preceding election year t . $X_{m,t}$ is a vector of municipality and mayor characteristics that influence the municipality's level of corruption.³⁴ The terms α_m and γ_t represent municipality and election intercepts, respectively, and $\varepsilon_{m,t}$ denotes unobserved characteristics that determine corruption at time t . Under the assumption that $A_{m,t}$ is strictly exogenous, the coefficient θ provides a consistent estimate of the average effect of the audit dissemination on rent-seeking in municipal governments. Standard models of political agency predict and existing evidence shows that $\theta < 0$, arguably due to the short-run disciplining effects of information on politicians' rent seeking decisions.

To examine the longer-term consequences of providing information to voters from the audits on the rent-seeking behaviors in local governments, we compare the outcomes of the subsequent audit (disseminated around election in period $t+4$) across municipalities whose earlier audit reports were disseminated in the two-year period before election t to those whose audit reports were disseminated in

³⁴ 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; incumbent's party has won in previous 3+ elections; and the incumbent's number of terms in office.

the two-year period following election t (see Figure II). Note that a municipality that is audited pre-election during election period t may be audited pre- or post-election during election period $t+4$.³⁵

We estimate the average effect of the audits and their dissemination in term t on the reported rent-seeking levels in the subsequent audit:

$$c_{m,t+1} = \theta_{p1}A_{m,t} + \theta_{p2}A_{m,t+1} + \beta X_{m,t} + \gamma_{t+1} + \alpha_m + \varepsilon_{m,t+1}, \quad (3)$$

where $c_{m,t+1}$ denotes the number of corrupt violations per report in municipality m in the subsequent audit, A_{mt} is the indicator for whether or not the municipality audit report was published in the two-year period preceding election year t , and $\varepsilon_{m,t+1}$ denotes unobserved characteristics that determine corruption at time $t+1$. In all longer-term effects specifications, we also include a control for the timing of the next audit ($A_{m,t+1}$, an indicator for whether or not the municipality audit report was published in the two-year period preceding the next election year).³⁶ Parameter θ_{p1} captures whether the pre-election dissemination of the first audit generates a *longer-term* reduction in rent seeking. Whether we find a significant reduction or not is an empirical question; it depends on both the nature of heterogeneity in politicians' characters and the dynamic incentives they face.

We can interpret parameter θ_{p1} as the effect of the pre-election dissemination of the (first) audit under the assumption that audit reports released in the period leading up to an election are more likely to inform on the existing incumbent mayor's activities than those reports published shortly after an election. This assumption is reasonable, at least in this context, for various reasons. First, due to the low overall re-election rate of mayors in the post-election audit group (32 percent); second, because 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. And last, even if the information from post-election audits does reach voters, they may not use it during the subsequent election because of recency bias – the tendency for voters to place more weight on recent information.³⁷ This assumption has the testable implication that the information contained in post-election 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 VI below).

To verify that the effects estimated in equations (2) and (3) are due to the mayor's electoral accountability, we estimate heterogeneous effects of audits by the competitiveness of the mayoral seats

³⁵ In our sample of municipalities with consecutive audits, 73 percent of municipalities audited pre-election were audited pre-election the following election period, whereas 54 percent of those audited post-election have a pre-election audit during election period $t+4$. 28 percent of our municipality-period observations are not audited in the subsequent term.

³⁶ 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.

³⁷ 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.

(using our summary measure of incumbency advantage), and by the identity of the agent identified in the report as committing the corrupt violation – the mayor or vice-mayor, or other municipal employees. We also check for heterogeneous effects by the incumbent mayor’s tenure in office as voter learning about the incumbent’s characteristics should be more pronounced in earlier terms, possibly leading to a shift in the accountability relationship.

The setting also allows us to test whether the audit program leads to short-term electoral accountability (sanctioning) similar to those found in the existing literature. We estimate a model analogous to equation (2) that uses as dependent variable an indicator for the re-election of the incumbent mayor in election year t (denoted $e_{m,t}$). The model captures the average effects of the audits and their dissemination on the incumbent mayor’s electoral accountability. We also test whether the dissemination of the audits increases the likelihood of re-election among politicians in municipalities with zero reported corruption and whether it decreases the likelihood of re-election of those mayors shown to have engaged in corruption. Therefore, following Ferraz and Finan (2008), we estimate the model:

$$e_{m,t} = \theta_{E1}A_{m,t} + \theta_{E2}A_{m,t}c_{m,t} + \beta_{E1}c_{m,t} + \beta_{E2}X_{m,t} + \gamma_t + \alpha_m + \varepsilon_{m,t}, \quad (4)$$

Again, standard political agency models predict that $\theta_{E2} < 0$. Additionally, since the information on post-election audits are (by definition) not available at the time of the election, the content of these audits should have no effect on the probability of re-election of the incumbent mayor. Therefore, an ancillary prediction in the empirical model is that $\beta_{E1} = 0$.

V.B. Results

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

There are no discernible differences in the levels of reported corruption across these two groups of municipalities in earlier audits – the mean number of violations per report revolves around 1.7 and those attributed to the mayor or vice-mayor around 0.80 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 among municipalities facing a pre-election audit, relative to those facing a post-election audit. A similar pattern holds for the number of violations attributed to the municipality-level executives (0.56 = 0.85 – 0.29). 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 to 0.08 (= 1.90 – 1.82) violations per report and is statistically indistinguishable from zero. Again, we find a similar pattern for the number of violations by the municipal executives during this later audit (0.006 = 0.804–0.798). The graphical 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 the corrupt behaviors of incumbent politicians and other municipal employees (Table IV). Estimates of the average effects of the pre-election audit show a systematic reduction in the number of corrupt violations in the municipality. There are 1.32 (64 percent) fewer reported corrupt violations in the pre-election audit municipalities relative to those audited post-election (Panel A, column 1). We also find 0.57 (67 percent) fewer corrupt violations per report by the mayor or vice-mayor (column 2), which suggests that there is a very limited (if any) shift in corrupt violations charges – actual or reported – between mayors and other municipality employees. This estimate suggests that the disciplining effects are not concentrated strictly among elected officials of the municipality.³⁸ 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 P.R. Department of Justice; the point estimate indicates 0.54 (61 percent) fewer violations per report among municipalities that were audited prior to the elections relative to those that were audited afterwards (column 3). The share of findings classified as corrupt also decreases by approximately 47-50 percent (columns 4-5). These relationships are stable and robust to controls (not reported) and to using the overall sample of municipalities (not reported).

As expected given the heterogeneity in political competition across municipalities, the short-term disciplining effects are concentrated among municipalities with competitive elections. The estimated impacts in competitive seat municipalities imply reductions in rent-seeking levels in the 73-110 percent range in proportional terms, whereas the estimated effects among municipalities with a significant party incumbency advantage are significantly smaller and statistically indistinguishable from zero (Panel B).³⁹ We also examine whether the disciplining effects vary by the tenure of the politician, as suggested by theory (specifications with interaction of pre-election audit and the number of terms in office of the politician). Although the point estimates suggest that higher tenure incumbents tend to be less disciplined by the pre-election audits, the estimated differential effects are small and statistically insignificant from zero (Panel C).⁴⁰

³⁸ 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).

³⁹ Among the municipalities with post-election audits, those with competitive mayoral seats report 29-54 percent lower corruption levels than those with non-competitive elections (not reported in the tables). This evidence is also consistent with rent-seeking levels being lower in jurisdictions with greater electoral competition.

⁴⁰ The model also predicts that the lower effectiveness of audits in reducing corruption as incumbents face more terms in office should be concentrated among responsive types. We can evaluate this prediction by restricting our sample to those mayors who have been re-elected at least once, and the point estimates suggest that the relationship is indeed stronger among first term

Parametric estimates of the effects of the pre-election audits on the number of corrupt violations in the subsequent term allow us to formally test for the audit's longer-term effects (Table V). The point estimate from the average effects model (equation (3)) with municipality and election-specific intercepts as well as municipality and mayor controls indicates a (statistically insignificant) increase of 0.25 of a violation per report (13.9 percent) among the pre-election audit municipalities (column 1). The relationship remains unchanged when focusing on the number of violations by the mayor or vice-mayor (column 2). 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 0.52 violations (80 percent). Moreover, in this case we can reject a decrease in corruption greater than 0.034 violations (5.1 percent) with 95 percent confidence.⁴¹ Again, the point estimates from specifications that use the alternative measure of corruption suggest an increase in corruption, although these are also statistically indistinguishable from zero (columns 4 and 5). These relationships are stable and robust to controls (not reported) and to using the overall sample of municipalities (not reported in the tables).

We also examine whether there is heterogeneity in the relationship between the audits and longer-term levels of corruption levels, based on characteristics of the municipality or of the (originally incumbent) mayor (Table VI). We find moderate or substantial increases in corruption levels among municipalities with competitive mayoral seats (Panel A, row 1), and no varying corruption levels among those with lopsided elections (Panel A, row 2). This effect heterogeneity may be due to stronger party discipline in non-competitive municipalities; parties are likely to have more control over politicians where nomination rather than election is the gateway to office. Consistent with the theory, the point estimates show no significant differences in the long-term effects of pre-election audits across politicians with different levels of experience (Panel B).⁴²

We now focus on the short-run effects of the audit program on electoral accountability – i.e., incumbent mayors' re-election rates. We again start the discussion with a graphical analysis to shed light on the patterns in the data. Figure IV depicts incumbent mayors' re-election rates as a function of the reported corrupt violations per report in the municipality, distinguishing between municipalities whose

mayors who are re-elected. 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 re-election. Estimates are available upon request.

⁴¹ 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 authors upon request).

⁴² The model predicts a positive relation between experience and corruption among responsive types. We can evaluate this prediction by restricting our sample to those mayors who have been re-elected at least once, and the point estimates are consistent with this hypothesis. 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 re-election. Estimates are available upon request.

audit reports were published in the two-year period prior to the election (represented by a solid red line) and those whose reports were published in the two-year period following each election (represented by a dashed green line).⁴³ Incumbent mayors in municipalities whose reports were published pre-election exhibit a clear downward-sloping trend between successful re-election rates and the number of corrupt violations per report. Among the municipalities with no reported violations, re-election rates are 50 percent, decrease to approximately 30 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; re-election rates are similar at 52 percent among those administrations with favorable audits, and decrease at a slower rate to 28 percent and 27 percent for administrations with moderate and high corruption levels, respectively. The contrast of these two relationships suggests that voters do care about corruption, and hold corrupt politicians accountable when informed. This evidence is consistent with previous work on municipal audit programs and electoral accountability, as shown by Ferraz and Finan (2008) for mayors in Brazil.

Parametric linear probability estimates of the reduced-form relationship following empirical models (2) and (4) capture the results depicted above (Table VII). The pre-election audits have no significant effect on incumbent mayor's re-election rates (column 1). However, incumbent mayors' re-election rates are significantly correlated with the number of corrupt violations among pre-election audit municipalities. The point estimate indicates that the probability of a successful re-election is 6.0 percentage points (19 percent) lower for each additional finding per report (column 2). In contrast (and as expected), the information on the post-election audits has no effects on the probability of re-election of the incumbent mayor; the β_{EI} point estimate implies a 0.7 percentage point (2 percent) reduction in the incumbent's re-election rate and is statistically indistinguishable from zero.⁴⁴ Overall, the estimated relationships support the hypothesis that information about corrupt violations induces an improvement in electoral accountability.

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 pre-election audits on incumbent re-election rates among municipalities with competitive elections, whereas re-election rates are significantly lower in municipalities with a large party incumbency advantage (column 3). This result can be rationalized by the significantly (23-33 percentage points) lower proportion of incumbent mayors who run

⁴³ The reported differences between pre-election and post-election audit municipalities are regression-adjusted for election period fixed effects. The graphical relationship and parametric estimates is qualitatively similar for the overall sample of municipalities (including mayors that do not run for re-election).

⁴⁴ Using an alternative measure also suggests that voters punish politicians found to engage in corruption – the point estimate implies that a one standard deviation (0.27) increase in the share of findings classified as corrupt leads to a 4 percentage point (12 percent) lower re-election rate, but it is not precisely estimated (column 3).

for re-election in these municipalities (not reported in the tables) and potential selection as to who chooses to run. We similarly find lower re-election rates among mayors with more experience in office (see column 4), which can also be rationalized by a lower share of these incumbents choosing to run for re-election (not reported in the tables).

The evidence presented in this sub-section supports the hypotheses that information about corrupt violations induces short-run disciplining and electoral accountability (sanctioning), but that the effects of the pre-election audits on rent-seeking levels are short-lived. This is consistent with the idea that politicians in power in the next term will engage on average in the same level of rent-seeking after an audited period than after a non-audited period, because their increased reputation for competence allows them to engage in greater rent-seeking. 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 sub-section we examine other predictions of our theory to evaluate in more detail whether political selection is playing a role.

VI. Politician Selection and Long-Term Electoral Performance Effects

VI.A. Empirical Methodology

To examine the longer-term consequences of providing information to voters on next period incumbent's electoral performance, we employ models analogous to equation (3) to estimate the average effect of the audits and their dissemination in term t on re-election of the incumbent mayor in election year $t+1$ (denoted $e_{m,t+1}$).

$$e_{m,t+1} = \theta_{E'1}A_{m,t} + \beta_{E'1}A_{m,t+1} + \beta_{E'2}X_{m,t} + \gamma_{t+1} + \alpha_m + \varepsilon_{m,t+1}, \quad (5)$$

Parameter $\theta_{E'1}$ captures whether the pre-election dissemination of the first audit generates a *longer-term* improvement in electoral accountability. Proposition 4 implies that $\theta_{E'1} > 0$.

We also test whether the audits-induced 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:

$$e_{m,t+1} = \theta_{E'1}A_{m,t} + \theta_{E'2}A_{m,t}c_{m,t} + \beta_{E'1}A_{m,t+1} + \beta_{E'2}c_{m,t} + \beta_{E'3}X_{m,t} + \gamma_{t+1} + \alpha_m + \varepsilon_{m,t+1}, \quad (6)$$

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, as we argue that recency bias, lower media dissemination, or low re-election rates are relevant factors, information contained in the post-election audits should have weaker or no effects on the electoral outcomes of the incumbent mayor in the following election; that is, $\beta_{E'2} = 0$.

VI.B. Results

To examine long-term electoral performance effects of, we start with a graphical analysis analogous to that for short-term electoral accountability. Figure V depicts the next-term mayors' re-election rates (in election year $t+4$) as a function of the reported corrupt violations per report in the municipality at time t , distinguishing between municipalities whose audit reports were published in the two-year period prior to the election (represented by a solid red line) and those whose reports were published in the two-year period following each election (represented by a dashed green line).⁴⁵ It is based on a measure of the mayor's successful re-election or otherwise (i.e., not run for re-election, or lose in primary or general election).

Incumbent mayors in municipalities whose reports were published pre-election exhibit a clear downward-sloping trend between successful re-election rates and the number of corrupt violations per report. In stark contrast, there is no relationship among municipalities whose reports were published following the election. The graph shows evidence of a reward for mayors receiving favorable audits or audits with moderate corruption levels at time t , in spite of their substantial increases in corruption in the following audit. The difference among those municipalities with low (zero reported) corruption suggests a 24 percentage points (= $0.44-0.20$) higher longer-term re-election rate of the incumbent, whereas among those with moderate corruption, the next incumbent experiences an electoral reward of 9 percentage points (= $0.28-0.19$). Interestingly, there is still a large penalty of 17 percentage points (= $0.09-0.26$) among those incumbents in municipalities with high levels of reported corruption at time t , as there is no modification in the corrupt actions among this group. This is consistent with a flexible interpretation of the model's results. Incumbents who are audited pre-election t and exhibit low levels of corruption are likely to be re-elected and will have above-average reputation, leading to above average re-election rates at election $t+1$. Those who are audited pre-election and are shown to have engaged in high levels of corruption are unlikely to be re-elected, and their successors will have below average reputations and re-election rates.

Parametric estimates of the reduced-form relationship again capture the results depicted above (Table VII). Although incumbent mayors' overall successful re-election rates (in election $t+4$) are not statistically significantly correlated with the incidence of a pre-election audit, both on average and by the number of corrupt violations among pre-election audit municipalities, the point estimates suggest a positive re-election effect (columns 5-6). Moreover, we find evidence of large positive re-election effects (in the range of 19-30.6 percentage points) among the subset of municipalities with competitive mayoral seats and first term mayors, respectively (columns 7-8). In contrast, negative information on the post-

⁴⁵ Again, the reported differences between pre-election and post-election audit municipalities are regression-adjusted for election period fixed effects.

election audits has no effect on the probability of re-election of the incumbent mayor. The β_{E2} point estimate implies a 0.4 percentage point (2.1 percent) decrease in the incumbent's re-election rate and is statistically indistinguishable from zero (column 6, row 3).

We also decompose the successful election effects by their components – running for re-election vs. being re-elected conditional on running for office. Although our theoretical framework does not consider strategic retirement, and thus the choice to run for office, we would obtain a stronger validation of the model's prediction if effects were concentrated on voters' electoral decisions. The estimates show that among mayors who run for re-election in the next term there is a positive shift in their electoral performance, on average (16.5 percentage points; column 9) and among the previously shown to be relevant subsets – those in municipalities with initially favorable audits (17.9 percentage points; col. 10), those in competitive races (26.7 percentage points; col. 11), and those in which the mayor was initially in his first term in office (24.6 percentage points; col. 12).⁴⁶ In summary, these relationships support the hypothesis that timely audit information leads to a politician selection effect that increases re-election rates in the longer-term.

VII. Testing for Alternative Explanations

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

If the actual initial audits were manipulated, then we might expect mayors who were politically affiliated with the party in power in the state government or with the party who appointed the Comptroller to receive more favorable audit reports. To assess this possibility, we estimate specifications that allow for heterogeneous 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 (an interaction of this indicator variable with the pre-election audit indicator) (Table VIII, Panel A). Although the estimates of these heterogeneous responses suggest that municipalities in the opposition to the party of the governor

⁴⁶ Although the point estimates suggest that a component of the longer-term electoral performance effects are driven by the incumbent mayor's decision to not run for office, these are statistically indistinguishable from zero (see Appendix Table I, columns 5-8).

who appointed the Comptroller receive somewhat less favorable audit outcomes (see columns 3 and 5), even among aligned municipalities we find no evidence of a sustained reduction in rent-seeking levels (Panel A, row 1).⁴⁷

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 in the tables). The estimates indicate no evidence of selective auditing, or of differential intensity of auditing, as measured by the number of reports.

Political cycles: A second concern is that political cycles are potentially correlated with our comparison of municipalities based on the timing of the audits. Municipalities receiving pre-election audits do cover time periods farther away from the election relative to those receiving later audits (see Table I, Panel A), which could affect the comparability of the audit outcomes across these groups. We examine whether this issue affects our results by controlling for the actual timing of the audited periods (i.e., the start of the audit period, and the time span of the audit period), and find no influence upon any of our results (not reported in the tables – these are available from the authors upon request).

We also examine whether another aspect of political 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 depreciated popularity of the political party, these 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 in an expected competitive election this might allow them to garner an advantage over the challenger. To the extent that the former effect dominates, we would observe higher rent-seeking levels among these incumbents facing negative popularity shocks. To evaluate this, we use as a proxy for negative popularity shock an indicator for whether the period t incumbent’s party loses the following gubernatorial election (at time $t+4$), and estimate specifications that allow for heterogeneous long-term effects for municipalities in which mayors face or do not face these shocks (Table VIII, Panel B). The estimates for municipalities in which mayors do not face these shocks imply no sustained reduction in rent-seeking levels (Panel B, row 1), and the effects among those facing the popularity shock suggest that the increased competitiveness leads to a disciplining effect (row 2).

⁴⁷ Another possibility previously raised in the literature is that incumbents who won by narrow margins in the previous election have a greater incentive to bribe OCPD auditors to receive more favorable reports. To examine this threat to validity, we extend the baseline model to control for the incumbent’s margin of victory in the previous election and its interaction with the pre-election audit indicator. We do not find evidence that a mayor’s previous level of political support influenced the audit process and including these additional controls do not affect the main short-term or long-term responses of the pre-election audits (estimates available upon request). This heterogeneity is correlated with that reported based on the degree of competitiveness of mayoral seats (Tables III and V).

Transfers from Central Government: It is plausible that the central government may have increased the level of transfers to municipalities after favorable audits (and reduced the flow of funds to municipalities after instances of corruption were exposed in those jurisdictions). 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 2009; Brollo 2010). To examine this hypothesis, we use the data on municipal government income statements, which provides us with the following additional revenue information: property tax, licensing, waste disposal services, transfers and other government revenues. We estimate the relationship between the pre-election audits and the fiscal year-specific revenues by source. To the extent that the available data allows us to assess this alternate explanation, we find no evidence of this channel in the data in this context (not reported in the tables; estimates available upon request).

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

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

VIII. Conclusion

The central goal of this paper is to study the causal effect of the disclosure of information about politicians' corrupt actions on future levels of corruption. We develop a model of political agency and

reputation building and show that, in equilibrium, a politician whose reputation has improved in the past exploits information asymmetries to engage in rent-seeking, leaving voters indifferent between re-electing him and electing an unknown challenger. Given these reputation-contingent incentives, re-elected mayors whose rent-seeking activities have been exposed will, on average, be *as corrupt* in the next term as mayors whose levels of corruption have not been exposed. We then use unique longitudinal data on municipal government audits in Puerto Rico to study this relationship empirically. We find that audits lead to a significant short-term reduction in municipal corruption, as well as an increase in incumbent mayors' electoral accountability. However, municipal corruption levels in the subsequent round of audits are on average *the same* in municipalities audited preceding the previous election and those whose audits became publicly available afterwards.

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

Our empirical findings, and their implications for political agency theory, are relevant for public policy in several ways. Most directly, our results confirm the potential effectiveness of government audit programs and the importance of electoral accountability as a disciplining mechanism. The difference in the effect of pre- and post-election audits we document underscores the importance of disseminating audit results at the time when they are most relevant for voters – shortly before an election. Furthermore, the null dynamic effect of audits on the level of corruption leads us to conclude that audit programs must be timely, sustained, and long-term commitments in order to be effective. Our conclusion that municipal 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 (2010), Fisman et al. (2012), Gagliarducci and Naniccini (2012), Dal Bó et al. (2012) and others, is needed to develop a better understanding of these issues and their appropriate policy responses.

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

Acknowledgments:

This is a substantially revised version of an earlier draft circulated with the title “Does Exposing Corrupt Politicians Reduce Corruption?” We thank Victor Aguirregabiria, Tim Besley, Matilde Bombardini, Leah Brooks, Adriana Camacho, Steve Coate, Taryn Dinkelman, Leo Feler, Claudio Ferraz, Fred Finan, Steven Haider, Anke Kessler, Monica Martínez-Bravo, Ben Nyblade, Torsten Persson, Nancy Qian, Imran Rasul, Aloysius Siow, Alessandro Tarozzi, Duncan Thomas, Pedro Vicente, as well as other seminar and conference participants at Banco de México, Carleton, Cornell, the CIFAR IOG Program Spring 2009 Meetings, Dalhousie, Duke, Michigan State, McGill, Northwestern–Kellogg, Princeton University Conference on Political Agency and Institutions, NEUDC 2010, SAIS, Simon Fraser, University of British Columbia, UC Berkeley, University College London, Università Bocconi, Toronto, and Yale for helpful comments and suggestions. We are especially grateful to Hon. José M. Díaz Saldaña, Comptroller of Puerto Rico, for providing us access to the municipal government audit reports of the Office of the Comptroller of Puerto Rico, and to Fred Finan for invaluable help and advice in the construction of the audit reports data. Aileen Cardona, Laura Delgado, María del Mar Ortiz, Zorimar Rivera, Vilma López, Adriana Robertson, and Julie Wilson provided superb research assistance. Research support from the Canadian Institute for Advanced Research (CIFAR), the Social Sciences and Humanities Research Council of Canada (SSHRC), and the University of Puerto Rico Social Research Center and Office of the Dean for Graduate Studies and Research is gratefully acknowledged. We are responsible for any errors.

References

- Alt, James, Ethan Bueno de Mesquita, and Shanna Rose (2011). “Disentangling Accountability and Competence in Elections: Theory and Evidence from U.S. Term Limits.” *Journal of Politics*, 73: 171-186.
- Ashworth, Scott (2012). “Electoral Accountability: Recent Theoretical and Empirical Work.” *Annual Review of Political Science*, 15: 183-201.
- Bandiera, Oriana, Andrea Prat, and Tommaso Valletti (2009). “Active and Passive Waste in Government Spending: Evidence from a Policy Experiment.” *American Economic Review*, 99: 1278-1308.
- Banerjee, Abhijit, Selvan Kumar, Rohini Pande, and Felix Su (2010). “Voter Information and Electoral Accountability: Experimental Evidence from Urban India.”, unpublished manuscript, Harvard University.
- Berry, Christopher R., and William G. Howell (2007). “Accountability and Local Elections: Rethinking Retrospective Voting.” *Journal of Politics*, 69(3), 844-58.
- Besley, Timothy J. (2004). “Paying Politicians: Theory and Evidence.” *Journal of the European Economic Association*, 2:193–215.
- _____ (2005). “Political Selection”, *Journal of Economic Perspectives Symposium on Institutions*, 19(3), 43-60.
- _____ (2006). *Principled Agents? The Political Economy of Good Government*. Oxford: Oxford University Press.
- Besley, Timothy, and Robin Burgess (2002). “The Political Economy of Government Responsiveness: Theory and Evidence From India.” *Quarterly Journal of Economics*, 117, 1415-51.
- Besley, Timothy J., and Anne Case (1995). “Does Electoral Accountability Affect Economic Policy Choices? Evidence from Gubernatorial Term Limits.” *Quarterly Journal of Economics*, 110(3), 769-798.
- Besley, Timothy, Rohini Pande, and Vijayendra Rao (2007). “Just Rewards? Local Politics and Public Resource Allocation in South India.” Unpublished manuscript, Harvard University.
- Björkman, Martina, and Jakob Svensson (2009). “Power to the People: Evidence from a Randomized Field Experiment on Community-Based Monitoring in Uganda.” *Quarterly Journal of Economics*, 124(2), 735-769.
- Brollo, Fernanda (2010). “Who is Punishing Corrupt Politicians – Voters or the Central Government? Evidence from the Brazilian Anti-Corruption Program.” Unpublished manuscript, Università Bocconi.
- Brollo, Fernanda, Tommaso Nannicini, Roberto Perotti, and Guido Tabellini (2010). “The Political Resource Curse”, Unpublished manuscript, Università Bocconi.
- Caselli, Francesco, and Massimo Morelli (2004). “Bad Politicians.” *Journal of Public Economics*, 88, 759-82.
- Dal Bó, Ernesto, Federico Finan, and Martín Rossi (2012). “Strengthening State Capabilities: The Role of Financial Incentives in the Call to Public Service.” NBER Working Paper 18156.
- Di Tella, Rafael, and Raymond Fisman (2004). “Are Politicians Really Paid Like Bureaucrats?” *Journal of Law and Economics*, 47(2), 477-513.
- Diermeier, Daniel, Michael Keane, and Antonio Merlo (2005). “A Political Economy Model of Congressional Careers.” *American Economic Review*, 95: 347–373.

- Fearon, James (1999). "Electoral accountability and the control of politicians: selecting good types versus sanctioning poor performance," in Preszeworski, A., S. Stokes, and B. Manin (eds), *Democracy, Accountability and Representation*, Cambridge University Press, pp. 55-97.
- Ferejohn, John (1986). "Incumbent Performance and Electoral Control", *Public Choice*, 50: 5-25.
- Ferraz, Claudio, and Frederico Finan (2008). "Exposing Corrupt Politicians: The Effects of Brazil's Publicly Released Audits on Electoral Outcomes", *Quarterly Journal of Economics*, 123, 703-45.
- _____ (2010). "Motivating Politicians: The Impacts of Monetary Incentives on Quality and Performance." Unpublished manuscript, UC Berkeley.
- _____ (2011). "Electoral Accountability and Corruption: Evidence from the Audit Reports of Local Governments." *American Economic Review*, 101, 1274-1311.
- Fisman, Raymond, and Edward Miguel (2007). "Corruption, Norms, and Legal Enforcement: Evidence from Diplomatic Parking Tickets." *Journal of Political Economy*, 115(6), 1020-48.
- Fisman, Raymond, Nikolaj Harmon, Emir Kamenica, and Inger Munk (2012). "Labor Supply of Politicians." Unpublished manuscript, Columbia University.
- Gagliarducci, Stefano, and Tommaso Nannicini (2012). "Do Better Paid Politicians Perform Better? Disentangling Incentives from Selection." *Journal of the European Economic Association*, forthcoming.
- Gentzkow, Matthew A. (2006). "Television and Voter Turnout." *Quarterly Journal of Economics*, 121(3), 931-972.
- Gentzkow, Matthew A., Edward L. Glaeser and Claudia Goldin (2006). "The Rise of the Fourth Estate: How Newspapers Became Informative and Why It Mattered." In Glaeser, E.L., and C. Goldin (eds.) *Corruption and Reform: Lessons from American History*. National Bureau of Economic Research.
- Gordon, Sanford C., and Gregory A. Huber (2007). "The Effect of Electoral Competitiveness on Incumbent Behavior." *Quarterly Journal of Political Science* 2(2): 107-138.
- Hopgood Dávila, Eugenio (2008). "Contralor le da mala nota a Santini (Comptroller gives Santini a bad grade)", *El Nuevo Día*, September 25th.
http://www.elnuevodia.com/diario/noticia/puertorico/hoy/noticias/contralor_le_da_mala_not_a_santini/463146 (accessed September 25, 2008).
- Lewis-Beck, Michael S., and Martin Paldam (2000). "Economic voting: an introduction." *Electoral Studies*, 19, 113-21.
- List, John A., and Daniel M. Sturm (2006). "How Elections Matter: Theory and Evidence from Environmental Policy." *Quarterly Journal of Economics*, 121(4), 1249-1281.
- Litschig, Stephan, and K. Morrison (2009). "Electoral Effects of Fiscal Transfers: Quasi-Experimental Evidence from Local Executive Elections in Brazil, 1982-1988." Unpublished manuscript, Universitat Pompeu Fabra.
- Manin, Bernard, Adam Przeworski, and Susan Stokes (1999). *Democracy, Accountability and Representation*. New York: Cambridge University Press.
- Martínez-Bravo, Monica, Gerard Padro i Miquel, Nancy Qian, and Yang Yao (2007). "Do Local Elections in Non-Democracies Increase Accountability? Evidence from Rural China." Unpublished manuscript, Yale University.

Niehaus, Paul, and Sandip Sukthankar (2011). "Corruption Dynamics: The Golden Goose Effect." Unpublished manuscript, UC San Diego and Dartmouth College.

Office of the Comptroller, Commonwealth of Puerto Rico. "Misión, Visión y Valores." http://www.ocpr.gov.pr/OCPR_mision.htm (accessed April 9, 2009).

Olken, Benjamin (2007). "Monitoring Corruption: Evidence from a Field Experiment in Indonesia", *Journal of Political Economy*, 115(2), 200-49.

Pande, Rohini (2007). "Understanding Political Corruption in Low Income Countries". In *Handbook of Development Economics*, Volume 4, T. Schultz & J. Strauss, 3155-3184.

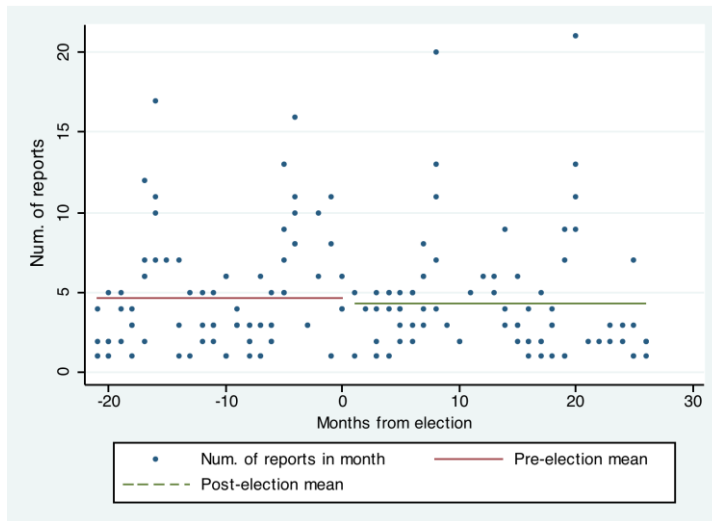
Reinikka, Ritva, and Jakob Svensson (2005). "Fighting corruption to improve schooling: Evidence from a newspaper campaign in Uganda", *Journal of the European Economic Association*, 3 (2-3), 259-67.

Schwabe, Rainer (2011). "Reputation and Accountability in Repeated Elections." Unpublished manuscript, Banco de México.

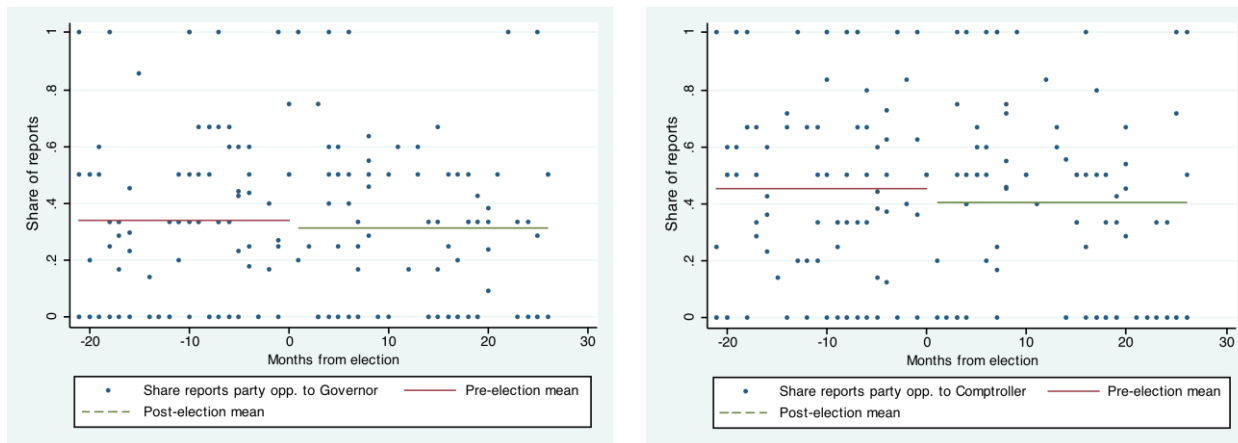
Stromberg, David (2004). "Radio's Impact on Public Spending." *Quarterly Journal of Economics*, 119(1), 189-221.

FIGURE I: TIMING OF PUBLICATION OF AUDIT REPORTS, 1987-2005

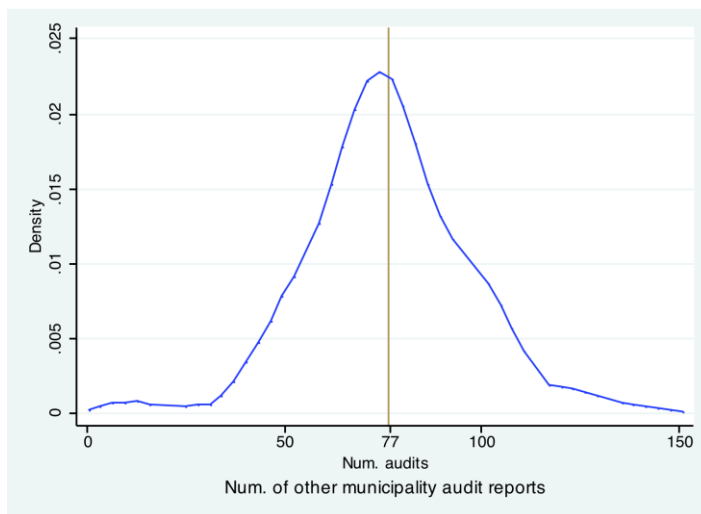
PANEL A: NUMBER OF REPORTS



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

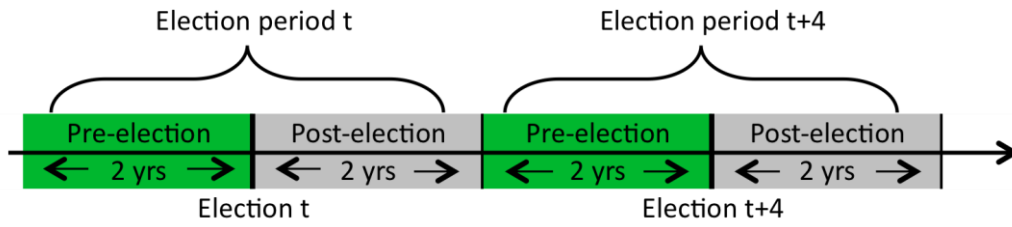


PANEL C: DISTRIBUTION OF NUMBER OF OTHER MUNICIPALITY REPORTS



Notes: Panel A shows the timing of release of the number of reports by month in the four-year period around each election (in Nov. 1988, 1992, 1996, 2000, and 2004). Panel B presents the share of published reports of municipalities in which the incumbent is in the opposition party to the Governor in office or to the Governor who appointed the Comptroller in office, in each month. The red line in each figure demarcates the mean for the 22 months before the November election; the green lines demarcate the mean for the 26 months following an election. Panel C presents an estimated density of the number of reports of other municipal governments published in between each pair of reports for every municipality. The straight (gray) line in the figure represents the mean of the distribution (= 76.2 reports).

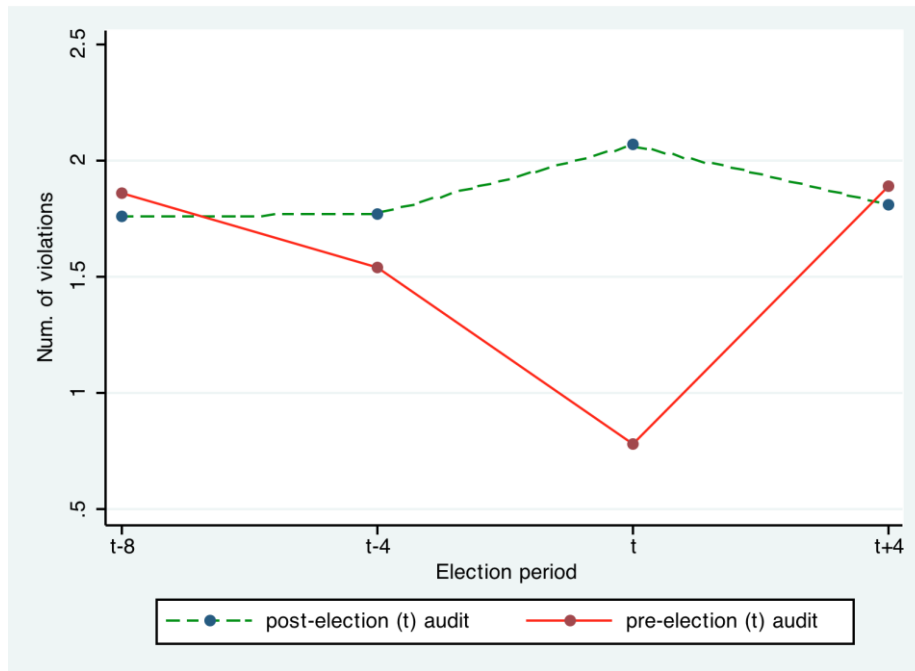
FIGURE II:
EMPIRICAL FRAMEWORK



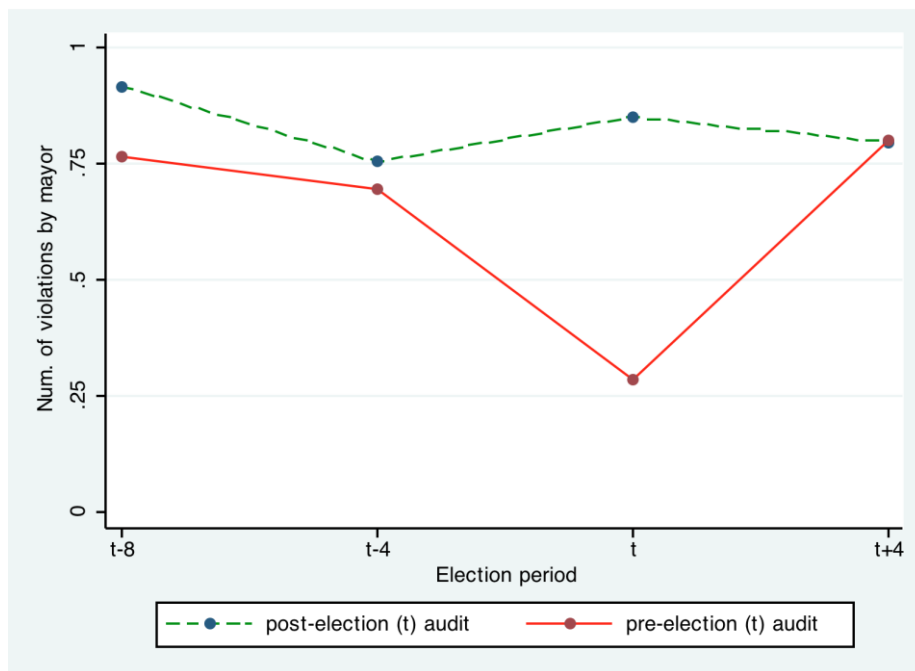
Notes: A municipality audited pre-election during election period t may be audited pre- or post-election during election period $t+4$. In our sample, 73 percent of municipalities audited pre-election were audited pre-election the following election period, whereas 54 percent of those audited post-election have a pre-election audit during election period $t+4$.

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

PANEL A: ALL VIOLATIONS

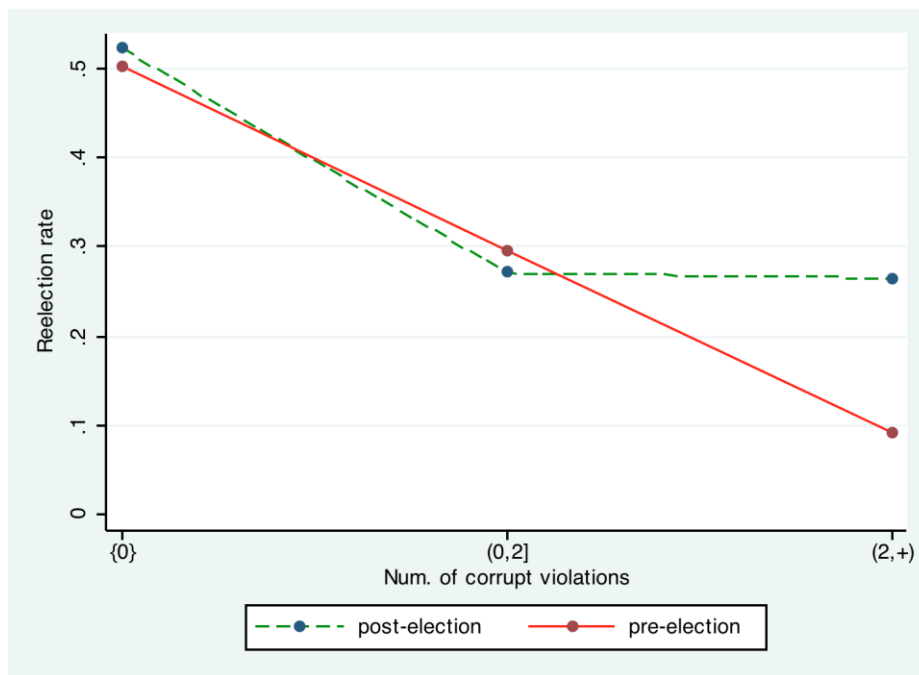


PANEL B: VIOLATIONS BY MAYOR OR VICE-MAYOR



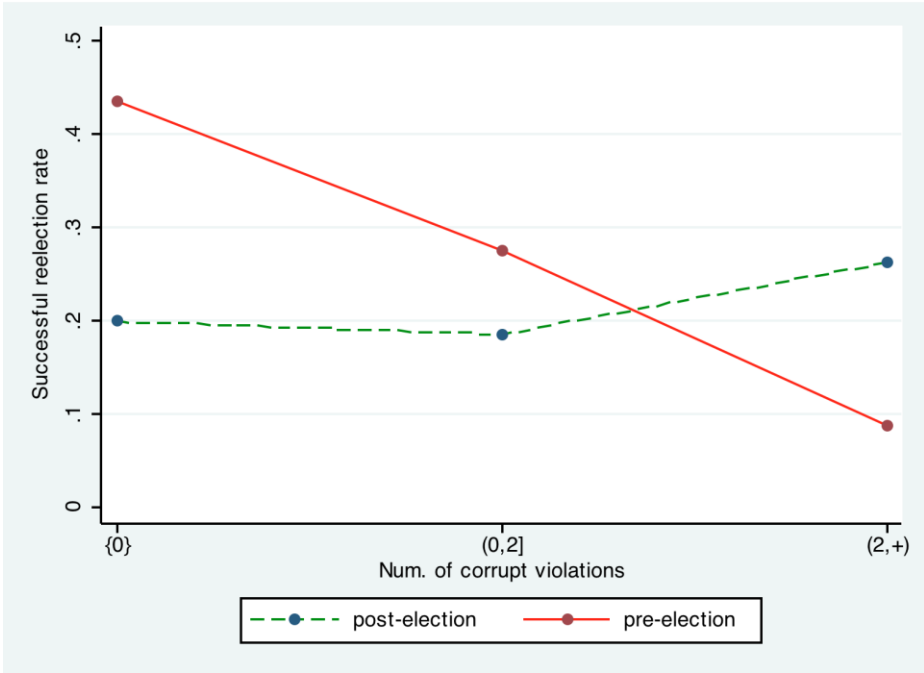
Notes: The figures show the unadjusted relationship between the number of corrupt violations per report in each audit, for municipalities audited before and after the election at time (t).

FIGURE IV:
 RELATIONSHIP BETWEEN REPORTED CORRUPTION LEVELS AND ELECTORAL
 ACCOUNTABILITY (INCUMBENT WINS RE-ELECTION | RUNNING) FOR MUNICIPALITIES
 AUDITED BEFORE AND AFTER ELECTIONS



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

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



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

TABLE I: CHARACTERISTICS OF THE AUDIT REPORTS

	All	Pre-election audit	Post-election audit	Difference (Adjusted)	N
	(1)	(2)	(3)	(4)	(5)
<u>Panel A: Audit outcomes</u>					
Number of all corrupt violations per report	1.34 [1.77]	0.79 [1.03]	2.07 [2.22]	-1.12 *** (0.22)	241
Number of violations by mayor/vice-mayor	0.53 [0.88]	0.29 [0.58]	0.85 [1.08]	-0.52 *** (0.12)	241
Number of violations referred to Dept. of Justice	0.59 [1.15]	0.36 [0.79]	0.89 [1.45]	-0.50 *** (0.17)	241
Share of findings, attributed to mayor/vice-mayor, classified as corrupt	0.17 [0.27]	0.15 [0.27]	0.21 [0.25]	-0.06 * (0.03)	241
Share of findings referred to Dept. of Justice classified as corrupt	0.26 [0.37]	0.21 [0.36]	0.33 [0.38]	-0.09 * (0.05)	241
<u>Panel B: Other audit characteristics</u>					
Number of audit reports	1.83 [1.15]	2.15 [1.21]	1.42 [0.63]	0.78 *** (0.11)	241
Start of audit period in reports (years from election)	6.18 [2.70]	6.72 [2.85]	5.49 [2.25]	1.64 *** (0.27)	241
End of audit period in reports (years from election)	1.18 [1.53]	1.50 [1.55]	0.76 [1.41]	0.57 *** (0.17)	241
Time span of audited period (years)	5.01 [2.67]	5.22 [2.85]	4.74 [2.40]	1.07 *** (0.26)	241

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

TABLE II: CHARACTERISTICS OF THE MUNICIPALITIES

	All	Pre-election	Post-election	Difference	N
	(1)	audit	audit	(Adjusted)	(5)
	(1)	(2)	(3)	(4)	(5)
<u>Panel A: Electoral outcomes</u>					
Incumbent runs for re-election (1/0)	0.74 [0.44]	0.72 [0.45]	0.76 [0.43]	-0.04 (0.05)	326
Incumbent party wins (1/0)	0.33 [0.47]	0.35 [0.48]	0.31 [0.46]	0.01 (0.06)	326
Incumbent mayor wins running (1/0)	0.35 [0.48]	0.37 [0.48]	0.32 [0.47]	0.00 (0.07)	241
Elected mayor's earnings (000's) (5 years before election) [2000 and 2004 elections]	44.79 [22.97]	45.12 [19.01]	43.97 [30.95]	-1.75 (7.71)	96
<u>Panel B: Incumbent mayor characteristics</u>					
Mayor, member of PNP (1/0)	0.52 [0.50]	0.51 [0.50]	0.54 [0.50]	0.00 (0.07)	241
Member of opposition party to Governor (1/0)	0.32 [0.47]	0.32 [0.47]	0.32 [0.47]	0.00 (0.07)	241
Member of opp. party to Governor appointing Comptroller (1/0)	0.53 [0.50]	0.57 [0.50]	0.48 [0.50]	0.11 * (0.06)	241
Terms in office	1.54 [1.21]	1.62 [1.36]	1.44 [1.00]	0.18 (0.16)	241
Mayor's win margin in previous election	0.11 [0.09]	0.12 [0.09]	0.10 [0.09]	0.01 (0.01)	241
Party incumbency advantage (1/0)	0.48 [0.50]	0.51 [0.09]	0.44 [0.50]	0.09 (0.07)	241
<u>Panel C: Pre-audit municipality characteristics</u>					
High school education or more (%)	0.46 [0.09]	0.48 [0.09]	0.44 [0.08]	0.03 *** (0.01)	241
College or more (%)	0.11 [0.04]	0.12 [0.04]	0.10 [0.05]	0.01 ** (0.01)	241
Household median income (1,000 USD)	9.30 [2.98]	9.92 [3.16]	8.51 [2.51]	0.99 ** (0.38)	241
Poverty rate	0.59 [0.10]	0.57 [0.10]	0.61 [0.09]	-0.03 ** (0.01)	241
Unemployment rate (first year of mayor's term)	0.17 [0.06]	0.16 [0.05]	0.18 [0.06]	0.00 (0.01)	167
Unemployment rate (second year of mayor's term)	0.16 [0.05]	0.16 [0.05]	0.17 [0.05]	-0.01 (0.01)	209

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

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

	All	Pre-election	Post-election	Difference	N
	(1)	audit	audit	(Adjusted)	(5)
		(2)	(3)	(4)	
<u>Panel D: Municipal government budget</u>					
Total expenditures (1st & 2nd year of mayor's term)	10.67 [18.60]	12.24 [21.89]	7.85 [9.98]	2.98 (2.73)	167
Capital improvements	0.22 [0.42]	0.23 [0.43]	0.20 [0.41]	0.03 (0.10)	165
Salaries & benefits	4.91 [5.40]	5.24 [5.55]	4.33 [5.11]	0.07 (1.22)	165
Social assistance	0.24 [0.39]	0.23 [0.38]	0.25 [0.42]	0.01 (0.09)	165
Other expenditures	4.06 [5.59]	4.60 [6.12]	3.10 [4.40]	1.02 (1.20)	165
Property tax	3.32 [5.60]	3.72 [6.12]	2.63 [4.52]	0.96 (1.27)	165
Licensing	2.05 [3.37]	2.26 [3.34]	1.68 [3.42]	0.28 (0.80)	165
Waste disposal	0.24 [0.86]	0.31 [1.04]	0.12 [0.33]	0.20 (0.17)	165
Transfers & other revenue	2.98 [2.00]	3.07 [2.08]	2.82 [1.86]	0.16 (0.34)	167

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

TABLE III: EQUILIBRIUM RE-ELECTION RATES

$q_{t/i,j}$	Incumbent politician's reputation	
	μ	1
Audit ($a_t = A$)	μ	1
No audit ($a_t = NA$)	$\mu\gamma(1 - \kappa(\mu, NA))$	$\frac{\gamma(1 - \kappa(1, NA)) + (1 - \gamma(1 - \kappa(1, NA)))\sigma_{NA}(1,0)}{1}$

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

	Dependent variables:				
	Number of corrupt violations, per report,			Share of findings classified as corrupt violations, findings	
	All	by Mayor/ Vice-mayor	referred to DOJ	by Mayor/ Vice-mayor	referred to DOJ
	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)
Panel A: Average Effects					
Pre-election audit	-1.32*** (0.30)	-0.57*** (0.16)	-0.54*** (0.20)	-0.097** (0.041)	-0.163** (0.068)
Municipality Controls	Yes	Yes	Yes	Yes	Yes
Election Year & Municipality FEs	Yes	Yes	Yes	Yes	Yes
Panel B: Effects by Party Advantage					
Pre-election audit	-1.82*** (0.38)	-0.78*** (0.20)	-0.98*** (0.26)	-0.152*** (0.051)	-0.256*** (0.077)
Pre-election audit × Incumbent's party has won in previous 3+ elections	1.25** (0.57)	0.56* (0.29)	1.11*** (0.38)	0.144 (0.088)	0.240** (0.113)
Municipality Controls	Yes	Yes	Yes	Yes	Yes
Election Year & Municipality FEs	Yes	Yes	Yes	Yes	Yes
Panel C: Effects by Mayor's Tenure					
Pre-election audit	-1.65*** (0.42)	-0.71*** (0.18)	-0.54* (0.28)	-0.216*** (0.064)	-0.209** (0.097)
Pre-election audit × terms in office	0.23 (0.20)	0.10 (0.10)	-0.01 (0.13)	0.083** (0.033)	0.032 (0.044)
Municipality Controls	Yes	Yes	Yes	Yes	Yes
Election Year & Municipality FEs	Yes	Yes	Yes	Yes	Yes
Observations	241	241	241	241	241
Mean of dep. variable (post-election)	2.07	0.85	0.89	0.21	0.33

Notes: Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at (*) 90%; (**) 95%; (***) 99% confidence levels, respectively. Controls are the number of municipality government reports, the number of municipal public corporation or consortium reports; indicators for New Progressive Party membership, for incumbent in the opposition party to the state-level executive government, and for incumbent in the opposition party to the governor who appointed Comptroller; the vote share for the incumbent in the previous election (t-4); incumbent's party has won in previous 3+ elections; 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 re-election that had a first audit during 1987-2002.

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

	Dependent variables:				
	Number of corrupt violations per report in next audit,			Share of findings classified as corrupt violations in next audit,	
	All findings	by Mayor/ Vice-mayor	referred to DOJ	findings by Mayor/ Vice-mayor	findings referred to DOJ
	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)
Pre-election audit	0.25 (0.39)	0.07 (0.23)	0.52* (0.28)	0.087 (0.087)	0.055 (0.090)
Municipality Controls	Yes	Yes	Yes	Yes	Yes
Election Year & Municipality FEs	Yes	Yes	Yes	Yes	Yes
Observations	173	173	173	173	173
Mean of dep. variable (post-election)	1.82	0.80	0.67	0.20	0.35

Notes: Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at (*) 90%; (**) 95%; (***) 99% confidence levels, respectively. Controls are the number of municipality government reports, the number of municipal public corporation or consortium reports; indicators for New Progressive Party membership, for incumbent in the opposition party to the state-level executive government, and for incumbent in the opposition party to the governor who appointed Comptroller; the vote share for the incumbent in the previous election (t-4); incumbent's party has won in previous 3+ elections; 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 re-election that had a first audit during 1987-2002.

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

	Dependent variables:				
	Number of corrupt violations per report in next audit,			Share of findings classified as corrupt violations in next audit,	
	All findings	by Mayor/ Vice-mayor	referred to DOJ	findings by Mayor/ Vice-mayor	findings referred to DOJ
	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)
<u>Panel A: Effects by Party Advantage</u>					
Pre-election audit	0.63 (0.54)	0.17 (0.27)	0.95** (0.40)	0.186** (0.090)	0.202* (0.121)
Pre-election audit × Incumbent's (t) party has won in previous 3+ elections	-0.66 (0.72)	-0.19 (0.36)	-0.89* (0.50)	-0.240* (0.134)	-0.340* (0.174)
Municipality Controls	Yes	Yes	Yes	Yes	Yes
Election Year & Municipality FEs	Yes	Yes	Yes	Yes	Yes
<u>Panel B: Effects by Mayor's Tenure</u>					
Pre-election audit	0.08 (0.52)	-0.06 (0.30)	0.18 (0.37)	0.122 (0.102)	-0.051 (0.133)
Pre-election audit × terms in office	0.14 (0.24)	0.10 (0.18)	0.27 (0.17)	-0.029 (0.044)	0.087 (0.080)
Municipality Controls	Yes	Yes	Yes	Yes	Yes
Election Year & Municipality FEs	Yes	Yes	Yes	Yes	Yes
Observations	173	173	173	173	173
Mean of dep. variable (post-election)	1.82	0.80	0.67	0.20	0.35

Notes: Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at (*) 90%; (**) 95%; (***) 99% confidence levels, respectively. Controls are the number of municipality government reports, the number of municipal public corporation or consortium reports; indicators for New Progressive Party membership, for incumbent in the opposition party to the state-level executive government, and for incumbent in the opposition party to the governor who appointed Comptroller; the vote share for the incumbent in the previous election (t-4); incumbent's party has won in previous 3+ elections; and the incumbent's number of terms in office (at time t). The sample is composed of all municipalities that had a first audit during 1987-2002.

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

Dependent variables:	Incumbent wins re-election (period t)				Incumbent runs for & wins re-election (period t+4)				Incumbent wins re-election running (period t+4)			
	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)	OLS (7)	OLS (8)	OLS (9)	OLS (10)	OLS (11)	OLS (12)
Pre-election audit	-0.059 (0.046)	-0.010 (0.056)	0.015 (0.056)	0.055 (0.076)	0.102 (0.073)	0.116 (0.076)	0.194** (0.089)	0.306*** (0.103)	0.165** (0.068)	0.179** (0.080)	0.267*** (0.099)	0.246** (0.109)
Pre-election audit × Num. violations		-0.060** (0.028)				-0.030 (0.038)				-0.047 (0.040)		
Num. of violations		-0.007 (0.013)				-0.004 (0.021)				-0.014 (0.021)		
Pre-election audit × Incumbent's (t) party has won in previous 3+ elections			-0.161** (0.073)				-0.223 ⁺ (0.139)				-0.263* (0.146)	
Pre-election audit × terms in office				-0.080** (0.035)				-0.143*** (0.050)				-0.065 (0.050)
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election Year & Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-election audits F-statistic [p-value]		3.75 [0.03]	2.94 [0.06]	4.27 [0.02]		1.17 [0.31]	2.47 [0.09]	4.84 [0.01]		2.50 [0.09]	3.76 [0.03]	3.13 [0.05]
Observations	241	241	241	241	241	241	241	241	188	188	188	188
Mean of dep. variable (controls)	0.31	0.31	0.31	0.31	0.19	0.19	0.19	0.19	0.23	0.23	0.23	0.23

Notes: Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at (*) 90%; (**) 95%; (***) 99% confidence levels, respectively. Controls are the number of municipality government reports, the number of municipal public corporation or consortium reports; indicators for New Progressive Party membership, for incumbent in the opposition party to the state-level executive government, and for incumbent in the opposition party to the governor who appointed Comptroller; the vote share for the incumbent in the previous election (t-4); incumbent's party has won in previous 3+ elections; and the incumbent's number of terms in office (at time t). The sample is composed of all municipalities that had a first audit during 1987-2002. The reported "Pre-election audits F-statistic" refers to a test of joint significance on the Pre-election audit and its interactions (p-value in brackets).

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

	Dependent variables:				
	Number of corrupt violations per report in next audit,			Share of findings classified as corrupt violations in next audit, findings	
	All findings	by Mayor/ Vice-mayor	referred to DOJ	by Mayor/ Vice-mayor	referred to DOJ
	OLS	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)	(5)
<u>Panel A: Robustness to Comptroller Bias</u>					
Pre-election audit	0.18 (0.40)	-0.04 (0.25)	0.13 (0.25)	0.06 (0.10)	-0.08 (0.10)
Pre-election audit × Incumbent (period t) from opposition party to Governor appointing Comptroller	0.18 (0.56)	0.24 (0.32)	0.91* (0.47)	0.08 (0.09)	0.31* (0.16)
Municipality Controls	Yes	Yes	Yes	Yes	Yes
Election Year & Municipality FEs	Yes	Yes	Yes	Yes	Yes
<u>Panel B: Robustness to Shocks to Party Popularity</u>					
Pre-election audit	0.90 (0.57)	0.50 (0.31)	0.90** (0.44)	0.20* (0.10)	0.19 (0.15)
Pre-election audit × Shock (period t+4) to popularity of incumbent mayor's (period t) party	-0.96* (0.57)	-0.64* (0.36)	-0.57 (0.45)	-0.16 (0.11)	-0.20 (0.16)
Shock (period t+4) to popularity of incumbent mayor's (period t) party	-0.13 (0.51)	0.13 (0.40)	-0.04 (0.37)	-0.05 (0.08)	0.04 (0.15)
Municipality Controls	Yes	Yes	Yes	Yes	Yes
Election Year & Municipality FEs	Yes	Yes	Yes	Yes	Yes
Observations	173	173	173	173	173
Mean of dep. variable (post-election)	1.82	0.80	0.67	0.20	0.35

Notes: Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at (*) 90%; (**) 95%; (***) 99% confidence levels, respectively. Controls are the number of municipality government reports, the number of municipal public corporation or consortium reports; indicators for New Progressive Party membership, for incumbent in the opposition party to the state-level executive government, and for incumbent in the opposition party to the governor who appointed Comptroller; the vote share for the incumbent in the previous election (t-4); incumbent's party has won in previous 3+ elections; and the incumbent's number of terms in office (at time t). The sample is composed of all municipalities that had a first audit during 1987-2002.

Online Appendix for:
The Dynamic Effects of Information on Political Corruption:
Theory and Evidence from Puerto Rico

Gustavo J. Bobonis [†]

Luis R. Cámara Fuertes [‡]

Rainer Schwabe [§]

[†] Associate Professor, Department of Economics, University of Toronto, BREAD, and CIFAR. Address: 150 St. George St., Toronto, Ontario, M5S 3G7, Canada. Tel: 416-946-5299. Fax: 416-978-6713. E-mail: gustavo.bobonis@utoronto.ca.

[‡] Professor, Department of Political Science, University of Puerto Rico-Río Piedras. Address: P.O. Box 23445, University of Puerto Rico, Río Piedras Campus, San Juan, PR 00931, USA. Tel: 787-764-0000, ext. 4332. E-mail: luisraulcamara@gmail.com.

[§] Financial Researcher, Mexican Central Bank. Address: Av. 5 de Mayo no. 1, México D.F., 06059, México. E-mail: rschwabe@banxico.org.mx.

Appendix A: Proofs – Model of Reputation and Accountability in Repeated Elections

In this Appendix we analyze the theoretical model described in Section IV of the paper and prove the results contained therein. Throughout, we make reference to the model set-up, definitions, and propositions contained in the body of the paper.

The proof of Proposition 1, which describes equilibrium re-election strategies, begins with some preliminary analysis of the model in which we describe the incumbent's problem, derive an expression for his reputation- I value function, and describe the relevant incentive constraints. In the process, we draw conclusions about what the voter's equilibrium re-election strategy looks like. The final piece of the proof comes in the form of Lemma 1, which shows that, when the selection problem is significant, reputation- μ incentive constraints will be binding.

The proof of Proposition 2, which shows that corruption is lower during audited periods, builds on Lemma 1. Proposition 3, on the null dynamic effects of audits on corruption, is proven in the body of the paper and is not discussed here. We end the appendix with a discussion of equilibrium re-election rates and the proof of Proposition 4.

Proof of Proposition 1

Proposition 1: There exists $\mu^* \in (0,1]$ such that if $\mu < \mu^*$, in a voter-optimal RDC equilibrium, the voter's re-election strategy is of the form:

- a) When a reputation- μ incumbent is audited:

$$\sigma_A(\mu, \kappa_t) = \begin{cases} 1 & \text{if } \kappa_t \geq \kappa(\mu, A) \\ 0 & \text{otherwise} \end{cases}$$

- b) When a reputation- μ incumbent is not audited:

$$\sigma_{NA}(\mu, u_t) = \begin{cases} 1 & \text{if } u_t = 1 \\ 0 & \text{otherwise} \end{cases}$$

- c) When a reputation- I incumbent is audited:

$$\sigma_A(1, \kappa_t) = \begin{cases} 1 & \text{if } \kappa_t \geq \kappa(1, A) \\ 0 & \text{otherwise} \end{cases}$$

- d) When a reputation- I incumbent is not audited:

$$\sigma_{NA}(1, u_t) = \begin{cases} 1 & \text{if } u_t = 1 \\ k & \text{if } u_t = 0 \end{cases}$$

where $k \geq 0$.

Proposition 1 describes re-election strategies for incumbents with reputations which will be observed on the equilibrium path (Lemma 1 below ensures that only reputation- μ and I incumbents are ever in office). Off the equilibrium path, strategies are $\sigma_A(\mu', \kappa_t) = \sigma_{NA}(\mu', u_t) = 0$ and $\kappa(\mu', A) = \kappa(\mu', NA) = 1$ when $\mu' \neq 1$ or μ .

Any positive outcome observed by the voter, be it restraint from corruption ($\kappa < 1$) or high voter utility ($u = 1$), will fully reveal the incumbent as a responsive type. Because of this, the politician's motivation for limiting the extent of his corruption will be based on the value of being re-elected with a good reputation ($\mu_t = 1$). For a given strategy profile (σ, κ) , this value can be written recursively as:

$$Q = E + pR(\kappa(1, A)) + (1-p)R(\kappa(1, NA)) + (p\sigma_A(1, \kappa(1, A)) + (1-p)[\gamma(1-\kappa(1, NA))\sigma_M(1,1) + (1-\gamma(1-\kappa(1, NA)))\sigma_M(1,0)])\delta Q,$$

where the first three terms represent the expected utility in the stage game, and the term in parenthesis is the ex-ante probability of re-election.

Because politicians' motivation for abstaining from corruption is based on the value of staying in office (Q), voter-optimality requires that Q be maximized subject to the level of expected voter utility provided. This insight allows us to narrow the set of strategies under consideration by noting that not re-electing incumbents who deliver high voter utility is an inefficient way to dampen incentives (the same can be achieved by re-electing incumbents who *do not* deliver high voter utility, but this increases rather than decreases Q), so that $\sigma_{NA}(1,1) = 1$. Similarly, not re-electing incumbents who extract rents in the expected quantities lowers the value of holding office without any additional benefit, so $\sigma_A(\mu_b, \kappa(1,A)) = 1$. Using these observations, and solving for Q , we have:

$$Q = \frac{E + pR(\kappa(1, A)) + (1-p)R(\kappa(1, NA))}{1 - (p + (1-p)[\gamma(1-\kappa(1, NA)) + (1-\gamma(1-\kappa(1, NA)))\sigma_{NA}(1,0)])} \quad (A1)$$

When there is an audit, the incumbent's *incentive compatibility constraint* is:

$$E + R(\kappa(\mu_b, A)) + \delta Q \geq E + R(1) \quad (A2)$$

The incumbent avoids absolute corruption whenever rents forgone are less than the value of expected future office-related benefits:**

$$R(1) - R(\kappa(\mu_b, A)) \leq \delta Q \quad (IC-A)$$

During periods when there is no audit, the incumbent politician must trade off marginal increases in rents against marginal decreases in the probability of re-election. The incumbent's problem is:

$$\max_x E + R(x) + \gamma(1-x)\delta Q + (1-\gamma(1-x))\sigma_M(\mu_t, 0)\delta Q'$$

where $Q' > 0$ is the value of holding office when reputation is $\hat{\mu}(\mu_t, u_t = 0)$. At the optimum ($x = \kappa(\mu_b, NA)$), the following first order condition holds:††

$$R'(\kappa(\mu_b, NA)) = \delta\gamma(Q - \sigma_{NA}(\mu_b, 0)Q') \quad (IC-NA)$$

The negative effect of the monitoring problem on incentives is evident in the appearance of the parameter γ on the right-hand-side of the equality. By lowering the expected value of avoiding corruption, it raises the minimum implementable level of corruption, which is achieved when $\sigma_{NA}(\mu_b, 0) = 0$.

The preceding arguments establish the basic structure of re-election strategies in the voter-optimal RDC equilibrium. It remains to be proven, however, that the reputation- μ incentive constraints are binding. We do that in the following lemma, which completes the proof of Proposition 1.

Lemma 1: There exists a $\mu^* \in (0,1]$ such that, in a voter-optimal RDC equilibrium, $\mu < \mu^*$ implies $\sigma_{NA}(\mu, 0) = 0$ and (IC-A) holds with equality when the incumbent's reputation is $\mu_t = \mu$.

** The assumptions that $R(0) = 0$, R continuous and $E > 0$ ensure that incumbents can show some restraint during audited periods.

†† The expression is $R'(\kappa(\mu_b, NA)) \geq \delta\gamma(Q - \sigma_{NA}(\mu_b, 0)Q')$ for corner solutions at $\kappa(\mu_b, NA) = 1$, and $R'(\kappa(\mu_b, NA)) \leq \delta\gamma(Q - \sigma_{NA}(\mu_b, 0)Q')$ for corner solutions at $\kappa(\mu_b, NA) = 0$. Strict concavity of R ensures that the FOC identifies a global maximum.

Proof: Equations (IC-A) and (IC-NA) show that incentives for both reputation- μ and I incumbents are derived from the reputation- I value function Q , as politicians are revealed as responsive types when they are shown to have avoided corruption. Thus, feasible levels of voter utility as being linked to Q .

Recall the connection between corruption strategies and Q in equation (A1). We write:

$$Q(x) = \max_{\hat{\kappa}(1,A), \hat{\kappa}(1,NA), \hat{\sigma}_M(1,0)} \frac{E + pR(\hat{\kappa}(1,A)) + (1-p)R(\hat{\kappa}(1,NA))}{1 - (p + (1-p)[\gamma(1 - \hat{\kappa}(1,NA)) + (1 - \gamma(1 - \hat{\kappa}(1,NA)))\hat{\sigma}_M(1,0)]}$$

such that

$$\gamma[p(1 - \hat{\kappa}(1,A)) + (1-p)(1 - \hat{\kappa}(1,NA))] = x, \text{ (IC-A), and (IC-NA) hold, and } \hat{\kappa}(1,A), \hat{\kappa}(1,NA), \hat{\sigma}_M(1,0) \in [0,1]$$

The function $Q(x)$ traces the reputation- I value function derived from optimal allocation of effort across audit and no-audit states, for a given level of expected voter utility x . Its domain X is implicitly defined as the set of voter utility levels for which $Q(x)$ is well-defined; at some level of x the decrease in the politician's continuation value due to the lower rents he expects to extract makes implementing higher voter utility infeasible. Thus, there is an upper bound on feasible reputation- I voter utilities $x = \max\{X\}$.

A second constraint on the level of voter utility comes from the restraint implementable for reputation- μ incumbents. If the highest feasible voter utility when $Q = Q(x)$ and the incumbent's reputation is μ is weakly lower than x , then the reputation- μ incentive constraints must be binding at the voter optimal RDC equilibrium. That is, if:

$$x \geq \gamma\mu[p(1 - \hat{\kappa}(\mu,A)) + (1-p)(1 - \hat{\kappa}(\mu,NA))] \quad (\text{A3})$$

where $R(1) - R(\hat{\kappa}(\mu,A)) = \delta Q(x)$; $R'(\hat{\kappa}(\mu,NA)) = \gamma\delta Q(x)$ (or the corner solution conditions described in footnote †† hold); and $\hat{\kappa}(\mu,A), \hat{\kappa}(\mu,NA) \in [0,1]$, then, we must have $\sigma_{NA}(\mu,0) = 0$ and (IC-A) holding with equality at the voter optimal RDC equilibrium. Because μ enters multiplicatively in the expression for expected voter utility when the incumbent's reputation is μ , there exists a μ^* such that inequality (A3) holds for all $\mu < \mu^*$. ■

An implication of Lemma 1 is that corruption is lower during audited periods than during non-audited periods when the incumbent's reputation is μ . This is because incentive constraints are binding in this situation, and available incentives are stronger when there is an audit. To see this, note that a necessary condition for (IC-NA) to hold is $R(1) - R(\kappa(\mu,NA)) \leq \delta\gamma Q$. This is identical to (IC-A), except for the presence of γ on the right-hand-side. Thus, we have that $R(1) - R(\kappa(\mu,NA)) \leq R(1) - R(\kappa(\mu,A)) = \delta Q$, or $R(\kappa(\mu,NA)) \geq R(\kappa(\mu,A))$, which implies $\kappa(\mu,NA) \geq \kappa(\mu,A)$ since R is strictly increasing.

Proof of Proposition 2

Proposition 2: There exists $\gamma^* \in (0,1]$ such that, in a voter-optimal RDC equilibrium, $\gamma < \gamma^*$ and $\mu < \mu^*$ imply that there is less corruption during audited periods than during non-audited periods: $\kappa(\mu_b,NA) > \kappa(\mu_b,A)$.

Proof: Lemma 1 proves that $\kappa(\mu,NA) > \kappa(\mu,A)$. The RDC equilibrium refinement demands that $\mu[p(1 - \kappa(\mu,A)) + (1-p)(1 - \kappa(\mu,NA))] = p(1 - \kappa(1,A)) + (1-p)(1 - \kappa(1,NA))$. Using Lemma 1 and (IC-NA), we assert that $\kappa(1,NA) \geq \kappa(\mu,NA)$. Therefore, if $\mu[p(1 - \kappa(\mu,A)) + (1-p)(1 - \kappa(\mu,NA))] > 1 - \kappa(\mu,NA)$, it must be that $\kappa(1,NA) > \kappa(1,A)$. This sufficient condition can be rewritten as $\frac{1 - \kappa(\mu,NA)}{1 - \kappa(\mu,A)} < \frac{\mu p}{1 - \mu(1-p)}$. Examination of equation (IC-NA) reveals that $\lim_{\gamma \rightarrow 0} \kappa(\mu,NA) = 1$, while, given $Q > 0$, γ does not affect feasible restraint during

audited periods. Therefore, $\lim_{\gamma \rightarrow 0} \frac{1-\kappa(\mu,NA)}{1-\kappa(\mu,A)} = 0 < \frac{\mu p}{1-\mu(1-p)}$ and we may find a γ^* such that $\gamma < \gamma^*$ implies that corruption is lower during audited periods. ■

Proof of Proposition 4

We now turn our attention to the model's predictions about re-election rates, derived from Proposition 1. We use $q_{t|i,j}$ to denote the re-election rate during period t when incumbent reputation is i and $a_t = j$, and drop the i or j subscript when we average across its possible values. For instance, $q_{t|\mu} = p\mu + (1-p)\mu\gamma(1-\kappa(\mu,NA))$ averages the values in the first column of the table below. In order to study the dynamic effects of a period t audit, Proposition 4 looks at average re-election rates the following period $q_{t+1|j}$.

In state μ , if an audit is conducted, the incumbent will be re-elected with probability $q_{t|\mu,A}=\mu$; all responsive politicians are re-elected because their restraint from corruption reveals them to be responsive. When there is no audit, the incumbent's re-election rate is $q_{t|\mu,NA} = \mu\gamma(1 - \kappa(\mu,NA))$; the voter must experience a good outcome in order to re-elect the incumbent. In state 1, when an audit is conducted, there-election rate is $q_{t|1,A}=1$. When there is no audit, the re-election rate is $q_{t|1,NA} = \gamma(1 - \kappa(1,NA)) + (1 - \gamma(1 - \kappa(1,NA)))\sigma_{NA}(1,0)$; the voter re-elects incumbents who deliver high utility, but also occasionally re-elects an incumbent who does not. The following table summarizes these results.

$q_{t i,j}$	Incumbent politician's reputation	
	μ	1
Audit ($a_t = A$)	μ	1
No audit ($a_t = NA$)	$\mu\gamma(1 - \kappa(\mu,NA))$	$\gamma(1 - \kappa(1,NA)) + (1 - \gamma(1 - \kappa(1,NA)))\sigma_{NA}(1,0)$

For incumbents in their first period, only responsive types are re-elected, and they are re-elected with higher probability during audited periods. Specifically, the probability of having a responsive incumbent during period $t + 1$, conditional on having a reputation- μ incumbent during period t is $\mu + (1 - \mu)\mu$ if there was an audit conducted during period t , and $\mu\gamma(1 - \kappa(\mu,NA)) + (1 - \mu\gamma[1 - \kappa(\mu,NA)])\mu$ if there was not. Similarly, there will be a responsive incumbent at $t + 1$ with probability 1 following audited periods, and only with probability $\gamma(1 - \kappa(1,NA)) + (1 - \gamma(1 - \kappa(1,NA)))\sigma_{NA}(1,0) + \mu(1 - \sigma_{NA}(1,0))$ following non-audited periods.

Proposition 4: Assume $\mu < \mu^*$ and $\gamma < \gamma^*$. In the voter-optimal RDC equilibrium $q_{t+1|A} > q_{t+1|NA}$.

Proof: We argue in the text above that the probability of having a high reputation incumbent is higher after an audited period: $Pr(\mu_{t+1} = 1/a_t = A) > Pr(\mu_{t+1} = 1/a_t = NA)$. Thus, we need only show that high reputation incumbents are re-elected more often. This is the case if the following inequality holds:

$$p(1 - \mu) + (1 - p)[\gamma(1 - \kappa(1,NA)) + (1 - \gamma(1 - \kappa(1,NA)))\sigma_{NA}(1,0) - \mu\gamma(1 - \kappa(\mu,NA))] > 0 \quad (A4)$$

To derive a sufficient condition for this inequality, set $\sigma_{NA}(1,0) = 0$. Rearranging equation (1) in the body of the paper:

$$\gamma(1 - p)[(1 - \kappa(1,NA)) - \mu(1 - \kappa(\mu,NA))] = \gamma p[\mu(1 - \kappa(\mu,A)) - (1 - \kappa(1,A))]$$

Substituting into inequality (A4) and simplifying:

$$p(1 - \mu) > \gamma p[(1 - \kappa(1,A)) - \mu(1 - \kappa(\mu,A))],$$

which holds if and only if:

$$(1 - \mu)(1 - \gamma) > \gamma(\mu\kappa(\mu,A) - \kappa(1,A))$$

By Lemma 1 and (IC-A), we know that $\kappa(\mu,A) \leq \kappa(1,A)$. Therefore, the inequality holds. ■

Appendix B: Examples – Content of Audit Reports

AUCTIONS

Report Number: M-00-08
Municipality: Maunabo
Unit: 4049
Audited Period: July 1st, 1996 – December 31st, 1998
Authorized by: Manuel Díaz Saldaña
Report Date: October 28th, 1999
Press Release Date: November 1st, 1999

Report pages: 12-16

“Hallazgo 1 – Cotizaciones cuya autenticidad no se pudo corroborar; contratación de obras sin subastas; compras y servicios sin solicitar cotizaciones, y compras en mercado abierto sin la autorización de la Asamblea Municipal

a. No se pudo corroborar la autenticidad de cuatro cotizaciones sometidas a nombre de dos contratistas. Dichas cotizaciones fueron consideradas para la adjudicación de cuatro contratos para la repavimentación de calles y caminos en varios sectores por \$137,689 entre febrero y marzo de 1997 (véase el Apartado b). En las cotizaciones se indicaban direcciones y números de teléfono que no correspondían a los contratistas a nombre de quienes se sometieron las mismas. Los funcionarios municipales tampoco pudieron ofrecer información sobre el particular que nos permitiera corroborar la autenticidad de dichas cotizaciones.

[...]

Esta situación nos impidió verificar la corrección y legalidad de dichas cotizaciones. Además, propicia la comisión de irregularidades con las mismas. El Alcalde no cumplió con las disposiciones citadas ni protegió adecuadamente los intereses del Municipio.

b. El Alcalde fraccionó los costos de la repavimentación de caminos y calles en varios sectores del Municipio por \$137,689 en cuatro contratos formalizados con dos contratistas en febrero y marzo de 1997 (véase el Apartado a). Esto evitó que el costo individual de éstos excediera de \$40,000 y se obvió la celebración de las subastas públicas requeridas. A continuación presentamos los detalles:

<u>Descripción</u>	<u>Fecha del contrato</u>	<u>Importe</u>
Varios caminos en el Sector Baerga del Barrio Palo Seco	24 feb 97	\$39,945
Varios caminos en el Sector Los Montaña	”	39,800
Camino Antonio Rodríguez	13 mar 97	22,742
Caminos Alejo Torres y José L. García	”35,202
	\$137,689

Una situación similar se comentó en el informe de auditoría anterior.”

Report Number:M-00-06

Municipality: Vieques
Unit: 4075
Audited Period: July 1st, 1994 – June 30th, 1998
Authorized by: Manuel Díaz Saldaña
Report Date: September 30th, 1999
Press Release Date: October 5th, 1999

Report pages: 16, 18-20

“Hallazgo 1 – Compras, servicios y obras sin celebrar subastas y sin solicitar cotizaciones; perjuicio ocasionado al Municipio por el rechazo de las ofertas en una subasta; subastas adjudicadas a licitadores que no presentaron las mejores ofertas, y compras en mercado abierto sin la autorización de la Asamblea Municipal

[...]

e. En octubre de 1995 la Junta de Subastas celebró una subasta para la construcción de cuatro salones en la Escuela de Playa Grande. A dicha subasta comparecieron dos contratistas con ofertas por \$225,000 y \$340,000, las cuales cumplían con las especificaciones requeridas. La Junta de Subastas no adjudicó dicha subasta. En las actas de ese organismo no se indicaron las razones, si algunas, para dicha decisión.

En noviembre de 1995 la Junta de Subastas celebró una segunda subasta para la adjudicación de dicha obra con las mismas especificaciones de la subasta anterior. A esta subasta solamente concurrió el contratista que cotizó \$340,000 en la subasta anterior, pero en esta ocasión con una oferta por \$325,000. Esta oferta excedió por \$100,000 la presentada por el otro contratista en la subasta de octubre de 1995. La Junta de Subastas adjudicó la segunda subasta por \$325,000 al único licitador.

En diciembre de 1995 la Alcaldesa formalizó el contrato para la construcción de la obra por \$285,000. Dicho importe era \$40,000 menor que el monto por el cual se adjudicó la obra. Para dicha diferencia no se ofrecieron las razones, si algunas. Por otra parte, el importe contratado excedía por \$60,000 el importe cotizado por el licitador que ofreció la cotización más baja en la subasta celebrada en octubre de 1995.

Esta situación ocasionó que el Municipio pagara en exceso \$60,000 en la construcción de la referida obra.

f. En abril de 1997 y mayo de 1998 la Junta de Subastas adjudicó dos subastas para la compra de cinco vehículos por \$134,082 a tres licitadores cuyas ofertas excedieron por \$25,081 las presentadas por otros licitadores que cumplieron con las especificaciones establecidas en las subastas. A continuación presentamos el detalle:

<u>Descripción</u>	<u>Adjudicada</u>	<u>Oferta más baja</u>	<u>Exceso</u>
Vehículo 4 x 4	\$28,188	\$25,456	\$2,732
"Pick-Up" 150 (2)	34,216	32,322	1,894
"Pick-Up" pequeña	15,751	13,725	2,026
Ambulancia Categoría II	<u>55,927</u>	<u>37,498</u>	<u>18,429</u>
	\$134,082	\$109,001	\$25,081

En las actas de la Junta de Subastas no se indicaron las razones que justificaban dichas adjudicaciones.

Esta situación ocasionó que el Municipio pagara en exceso \$25,081 en la adquisición de dichos vehículos, recursos que pudieron utilizarse para atender otras necesidades.

La Alcaldesa y la Junta de Subastas no cumplieron con la disposición citada ni protegieron los intereses del Municipio.”

PATRONAGE JOBS

Report Number: M-00-25
Municipality: Cidra
Unit: 4021
Audited Period: July 1st, 1995 – June 30th, 1998
Authorized by: Manuel Díaz Saldaña
Report Date: March 16th, 2000
Press Release Date: March 17th, 2000

Report pages: 18-21

“Hallazgo 3 – Nombramiento de un familiar del Alcalde y de otro funcionario a puestos de carrera y de confianza sin cumplir éstos con los requisitos establecidos ni el Municipio cumplir con otros requisitos

a. En septiembre de 1993 el Alcalde emitió un nombramiento a un familiar suyo para ocupar el puesto de carrera de Supervisor de Forestación y Ornato con un sueldo de \$1,222 mensuales. Para dicho nombramiento no se cumplió con los siguientes requisitos básicos: la publicación de una convocatoria, el suministro de examen y el establecimiento de un Registro de Elegibles. Además, los documentos sobre la Convocatoria y el Registro de Elegible relacionados con el referido nombramiento, tenían fechas de un año después de que se le emitió el mismo.

[...]

Las situaciones mencionadas en los apartados "a" al "c", no permite una administración adecuada y de excelencia del personal de carrera y de confianza sobre las bases del sistema de mérito.

El Alcalde, la Asamblea Municipal y el Director de Recursos Humanos no cumplieron con las disposiciones de ley citadas.”

Report Number: M-00-40
Municipality: Toa Baja
Unit: 4070
Audited Period: January 1st, 1996 – December 31st, 1998
Authorized by: Manuel Díaz Saldaña
Report Date: June 14th, 2000
Press Release Date: June 16th, 2000

Report pages: 15-21

“Hallazgo 1 – Nombramientos de familiares del Alcalde y de legisladores municipales que no tenían la preparación académica y demás requisitos mínimos requeridos para ocupar los puestos, y sueldos pagados ilegalmente a otros funcionarios y empleados en exceso de la retribución máxima establecida

(1) De febrero de 1985 a octubre de 1997 el Alcalde nombró en el Municipio a 22 empleados que eran sus parientes. También nombró a 11 empleados que eran parientes de 5 legisladores municipales. De estos 11 empleados, 4 eran parientes de la Presidenta de la Asamblea Municipal.

(2) Veintiuno de los empleados parientes del Alcalde y de legisladores municipales, nombrados de septiembre de 1991 a octubre de 1997 no poseían la preparación académica y los requisitos mínimos exigidos para la clase de puesto en los cuales fueron nombrados.

(3) De agosto de 1994 a septiembre de 1996 el Alcalde reclasificó en dos ocasiones a dos empleadas familiares de éste, a puestos para los cuales éstas no cumplían con la preparación académica y los requisitos mínimos de los puestos.

(4) De abril de 1990 a enero de 1997 el Alcalde aprobó 17 reclasificaciones de puestos y concedió 27 aumentos de sueldo a 2 funcionarios y a 9 empleados de los que se indican en el Apartado (1), cuyos nuevos sueldos asignados a los puestos que ocupaban excedieron de \$39 a \$1,051 la retribución máxima fijada en el Plan de Clasificación y Retribución Uniforme del Municipio. Como consecuencia, de abril de 1990 a julio de 1997 el Municipio pagó sueldos en exceso e ilegales por \$128,434 a dichos funcionarios y empleados parientes del Alcalde y de legisladores municipales.

[...]

Las situaciones comentadas en los apartados "a" y "b" se las informamos al Director Ejecutivo de la Oficina de Ética Gubernamental por carta del 25 de junio de 1999.

[...]

En el Artículo 6-A del Reglamento de Ética Gubernamental se establece, entre otras cosas, que todo servidor público deberá evitar tomar acción, esté o no específicamente prohibida por este Reglamento, que pueda resultar en o crear la apariencia de:

- Dar trato preferencial a cualquier persona, salvo justa causa.
- Perder su completa independencia o imparcialidad.
- Afectar adversamente la confianza del público en la integridad y honestidad de las instituciones gubernamentales.
- Promover una acción oficial sin observar los procedimientos establecidos.

[...]

El nepotismo es contrario al sistema democrático, el cual supone que todos aquéllos que reúnan los requisitos compitan en igualdad de condiciones al momento de optar por un empleo gubernamental. Los procedimientos de selección de personal tienen que ser imparciales para lograr reclutar el mejor talento disponible. El nepotismo derrota estos principios.

Estas situaciones ocasionaron lo siguiente:

- Crean una percepción negativa de la Administración Municipal de Toa Baja.
- Se utilizó el sistema de personal para favoritismos.
- Se pagaron indebidamente sueldos por \$262,194,
- Se pudo propiciar la comisión de irregularidades porque se debilitan los controles internos al nombrarse a parientes en los puestos.
- Se pudo afectar el Municipio, ya que al nombrar en los puestos a familiares del Alcalde y de legisladores municipales se corre el riesgo de que surjan conflictos y situaciones que

podieran degenerar en actos de corrupción.

El Alcalde y la Presidenta de la Asamblea Municipal se aprovecharon de sus cargos para beneficiar a familiares allegados a éstos, en perjuicio de otras personas cualificadas que estuvieran interesadas de participar en las funciones públicas del Municipio.”

Report Number: M-01-50
Municipality: Maricao
Unit: 4048
Audited Period: January 1st, 1997 – December 31st, 1999
Authorized by: Manuel Díaz Saldaña
Report Date: June 14th, 2001
Press Release Date: June 15th, 2001

Report pages: 37-40

“Hallazgo 6. – Puesto ocupado ilegalmente por un pariente del Alcalde

a. En enero de 1997 el Alcalde nombró a un pariente suyo en el puesto de Director de Recreación y Deportes. En julio de 1997 el Alcalde sometió el nombramiento de dicho funcionario a la Asamblea Municipal para su confirmación. A esta fecha había transcurrido el período permitido por ley para someter a la Asamblea Municipal el nombramiento. Dicho organismo rechazó el nombramiento porque la persona no poseía la preparación académica requerida para el puesto. En enero y marzo de 1998 el Alcalde sometió nuevamente el nombramiento de dicha persona a la Asamblea Municipal para su confirmación, pero dicho organismo lo rechazó en esas ocasiones por la misma razón. A pesar de la determinación de la Asamblea Municipal, el referido funcionario continuó en el puesto y en abril de 2000 renunció al mismo. A continuación presentamos el detalle de los períodos en que dicho funcionario ocupó ilegalmente el puesto y los sueldos por \$55,519 pagados:

PERÍODO	SUELDOS	RAZÓN
abr. a jul. 97	\$5,236	No se sometió el nombramiento a confirmación dentro de los 90 días siguientes al mismo
sep. 97 a ene 98	7,975	Nombramiento rechazado por la Asamblea Municipal, pero continuo en el puesto
abr. 98 a abr. 00	<u>42,308</u>	”
TOTAL	<u>\$55,519</u>	

[...]

El nepotismo es contrario al sistema democrático, el cual supone que todos aquéllos que reúnan los requisitos compitan en igualdad de condiciones al momento de optar por un empleo gubernamental.

Los procedimientos de selección de personal tienen que ser imparciales para lograr reclutar el mejor talento disponible. El nepotismo derrota estos principios.

Esta situación resulta perjudicial al Municipio, ya que el funcionario indicado ocupó el puesto sin cumplir con los requisitos del mismo. Además, los actos de nepotismo crean una percepción negativa de favoritismo. Por otra parte, son ilegales los sueldos pagados por \$55,519.

El Alcalde y el funcionario indicado no cumplieron con las disposiciones citadas y actuaron en perjuicio del Municipio.”

OVERINVOICING

Report Number: M-01-50
Municipality: Maricao
Unit: 4048
Audited Period: January 1st, 1997 – December 30th, 1999
Authorized by: Manuel Díaz Saldaña
Report Date: June 14th, 2001
Press Release Date: June 15th, 2001

Report pages: 22-27

“Hallazgo 1 - Facturación de recogido de escombros en exceso de los estimados de FEMA; contrato enviado con tardanza a la Oficina del Contralor, y otras deficiencias relacionadas con la contratación de estos servicios

a. En octubre de 1998 el Alcalde formalizó con un contratista un contrato por \$4,200,000 para el recogido de escombros, corte de árboles, disposición de éstos y el barrido de calles y aceras, como consecuencia del paso del Huracán Georges. El contrato se pagaría con fondos provenientes de la *Federal Emergency Management Administration* (FEMA). En el contrato se estableció que la tarifa por yarda cúbica recogida sería de \$28. Además, se acordó en el contrato que el Municipio se hacía responsable del pago total del contrato en caso de que FEMA no realizara los desembolsos correspondientes.

De noviembre de 1998 a enero de 1999 el contratista facturó al Municipio, mediante certificaciones de trabajos realizados, \$4,344,603 por 155,164 yardas cúbicas de escombros recogidos. Las referidas certificaciones estaban firmadas por empleados del Municipio como que los servicios se recibieron de conformidad. A diciembre de 1999 el Municipio había pagado \$2,598,595 de dicho importe por 92,807 yardas cúbicas. Las facturas sometidas y pagadas al contratista estaban firmadas como correctas por el Alcalde. Del importe pagado, \$1,027,831 provinieron de fondos aportados por FEMA, \$67,500 de fondos administrativos de programas federales y los restantes \$1,503,264 de fondos del **Programa CDBG** asignados para la construcción de 10 proyectos.

FEMA determinó que el contratista recogió y depositó en los centros de acopio 50,157 yardas cúbicas de escombros, en lugar de las 155,164 facturadas al Municipio. A diciembre de 1999 FEMA había aprobado fondos al Municipio por \$1,404,396 para el recogido de escombros, correspondientes a las 50,157 yardas cúbicas de escombros recogidos. Esto representó una diferencia entre lo facturado al Municipio por el contratista y lo aprobado por FEMA de \$2,940,207 (105,007 yardas cúbicas). También representó un pago en exceso de \$1,194,199 (42,650 yardas cúbicas) respecto al importe aprobado por FEMA y lo desembolsado por el Municipio al contratista. El Municipio apeló la determinación de FEMA. A junio de 2000 la reclamación estaba pendiente de resolución por FEMA. El Municipio no había pagado los restantes \$1,746,008 facturados por el contratista.

[...]

Nuestro examen de los viajes realizados por 15 camiones reveló que el contratista facturó en exceso \$261,884. Esto, porque la capacidad de los camiones figurada en los informes excedía las medidas de éstos, según nos informaron los dueños de los mismos. Además, se incluyeron camiones como prestando servicios, lo cual era incorrecto según la evidencia obtenida.

[...]

El Alcalde y los demás funcionarios concernientes no cumplieron con las disposiciones citadas ni protegieron los intereses del Municipio respecto a la situación señalada.”

Report Number: M-02-29
Municipality: Hormigueros
Unit: 4035
Audited Period: July 1st, 1998 – June 30th, 2000
Authorized by: Manuel Díaz Saldaña
Report Date: December 26th, 2001
Press Release Date: January 15th, 2002

Report pages: 20-24

“Hallazgo 1. – Facturación y pagos en exceso por servicios de recogido de escombros por los daños ocasionados por el Huracán Georges

a. En octubre de 1998 el Alcalde formalizó un contrato por un costo estimado de \$3,692,000 con una persona para el recogido de escombros causados por el paso del Huracán *Georges*, el 21 de septiembre de dicho año. [Véase el **Hallazgo 2**] El contrato se pagaría con fondos provenientes de la *Federal Emergency Management Agency (FEMA)*. En dicho contrato se estimó el recogido de escombros hasta un máximo de 142,000 yardas cúbicas a una tarifa de \$26 la yarda cúbica.

De noviembre de 1998 a marzo de 1999 el contratista facturó al Municipio, mediante certificaciones de trabajos realizados, \$1,676,818 por 64,493 yardas cúbicas de escombros recogidas. Los funcionarios municipales certificaron como correctas dichas certificaciones. El desglose de las certificaciones es como sigue: \$256,360 correspondían a 9,860 yardas cúbicas depositadas en el Vertedero del Municipio de Mayagüez, \$1,343,758 por 51,683 yardas cúbicas depositadas en un centro de acopio ubicado en el Municipio de Añasco el cual tenía la autorización del Cuerpo de Ingenieros del Ejército de los Estados Unidos, y los restantes \$76,700 por 2,950 yardas cúbicas que se transportaron al referido centro de acopio, pero que no fueron depositadas en el mismo porque estaban contaminadas. Éstas fueron depositadas en un vertedero.

A mayo de 1999 el Municipio había pagado \$1,151,317 del importe facturado por 44,282 yardas cúbicas de fondos de **FEMA**. De dicho importe, \$818,257 correspondían a 31,508 yardas de escombros recogidos en el centro de acopio ubicado en el Municipio de Añasco y los restantes \$333,060 a las 12,810 yardas depositadas en el Vertedero del Municipio de Mayagüez y los escombros contaminados. A la fecha de nuestra auditoría, febrero de 2001, el Municipio no le había pagado al contratista los restantes \$525,501 correspondientes a 20,175 yardas cúbicas de escombros.

El Cuerpo de Ingenieros del Ejército de los Estados Unidos certificó que las yardas cúbicas de escombros recogidos en el centro de acopio ubicado en el Municipio de Añasco fueron de 22,983^{**}, en

^{**} Se depositaron 16,776 yardas cúbicas, pero se le adicionó un 37 por ciento por el asentamiento y compactación de los escombros una vez depositados en el Vertedero.

lugar de las 51,683 facturadas al Municipio. A base de dicha certificación, a mayo de 1999 **FEMA** había aprobado fondos al Municipio por \$597,558 correspondientes a ese renglón. Las yardas cúbicas facturadas al Municipio por el contratista en ese renglón excedían por 28,700 a las certificadas por el Cuerpo de Ingenieros. Esto representa una facturación en exceso de \$746,200. Con relación al importe pagado, representa un pago en exceso al contratista de \$221,635. A febrero de 2001 el Municipio no había pagado los restantes \$525,501 facturados en exceso por el contratista.

b. Con relación a los procesos de supervisión y facturación de dichos servicios se determinó lo siguiente:

- 1) El Municipio no realizó una supervisión adecuada de los trabajos contratados para asegurarse que se habían rendido en las fechas y por las cantidades facturadas. A estos efectos se determinó que el contratista facturó que los camiones realizaron 2,502 viajes, pero el Cuerpo de Ingenieros certificó que se realizaron 917 viajes. Esto representa un exceso de 1,585 viajes, lo cual forma parte del importe facturado en exceso.
- 2) En las facturas sometidas al Municipio por el contratista éste certificó los viajes de los camiones a base de la capacidad máxima en yardas cúbicas de los mismos, y no por la cantidad real de yardas cúbicas de escombros depositados por cada camión.

El 5 de noviembre de 2001 le entregamos a los funcionarios del Departamento de Justicia, a solicitud de éstos, toda la evidencia obtenida relacionada con los hechos señalados. En ese mismo mes la Secretaria de Justicia solicitó al Panel del Fiscal Especial Independiente la designación de un Fiscal Especial Independiente (FEI) para investigar estos hechos y los señalados en el **Hallazgo 3**, entre otros, y la radicación de los cargos criminales correspondientes contra el Alcalde.

[...]

Esta situación propició la comisión de irregularidades y de pagos indebidos. Además, puede afectar al Municipio, ya que existe la posibilidad de que tenga que asumir algunos costos si **FEMA** decide no reembolsar los fondos.

El Alcalde y los demás jefes de las dependencias municipales concernientes no protegieron los intereses del Municipio respecto a la situación señalada.”

Appendix C: Politician Selection Effects based on Elected Mayors' Pre-Candidacy Earnings

C.1. Data and Empirical Methodology

We employ an additional dataset compiled from publicly available state-level income tax returns for the four year period preceding each of the 2000 and 2004 elections (available from the P.R. State Electoral Commission (CEE)). All candidates were 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 this data to examine, for this sub-sample, whether the audits induce positive selection of politicians based on their pre-incumbency earnings – 5 years before the relevant election.

To identify whether the pre-election audit dissemination generates selection in the types of politicians who win office, we estimate a model analogous to equation (2) that uses as dependent variable the household per capita earnings five years before the election of the mayor elected in period t (denoted y_{mt}). The model captures the average effects of the audits and their dissemination on a measure of income/socio-economic status of the elected mayor (whether it is the re-elected mayor or the challenger).^{§§} To the extent that pre-determined income is correlated with competence, managerial or campaigning ability, finding evidence of a correlation would represent evidence of information inducing politician selection. We also test whether the audit-induced politician selection is heterogeneous across municipalities with zero reported corruption and among those whose executives were shown to have engaged in corruption. We thus estimate models (analogous to equation (4)) to uncover this potential heterogeneity in politician selection.

$$y_{mt} = \theta_{Y1}A_{mt} + \theta_{Y2}A_{mt}c_{m,t} + \beta_{Y1}c_{m,t} + \beta_{Y2}X_{mt} + \gamma_t + \alpha_m + \varepsilon_{mt}, \quad (5)$$

Our model predicts that $\theta_{Y1} > 0$ and $\theta_{Y2} < 0$. Additionally, since the information on post-election audits are (by definition) not available at the time of the election, the content of these audits should have no effect on the probability of re-election of the incumbent mayor. Therefore, an ancillary prediction in the empirical model is that $\beta_{Y1} = 0$.

C.2. Results

We start the discussion with a graphical analysis. Appendix Figure I depicts the elected mayor's household per capita earnings five years preceding the respective election as a function of the reported corrupt violations per report in the municipality, again distinguishing between municipalities whose audit reports were published in the two-year period prior to the election (represented by a solid red line) and those whose reports were published in the two-year period following each election (represented by a dashed green line).^{***} Elected mayors in municipalities whose reports were published pre-election exhibit a clear downward-sloping trend between successful re-election rates and the number of corrupt violations per report. Among the municipalities with no reported violations or with moderate corruption (up to two violations per report), the data suggests that for pre-election audit municipalities elected mayors have lower pre-candidacy earnings than those with no audit, although (not statistically significant). In contrast, we see a reversal in politician selection among municipalities with high corruption (more than two violations per report). We find a difference of approximately \$30K USD in the pre-candidacy earnings of individuals across municipalities with and without a pre-election audit. This is indicative of significant

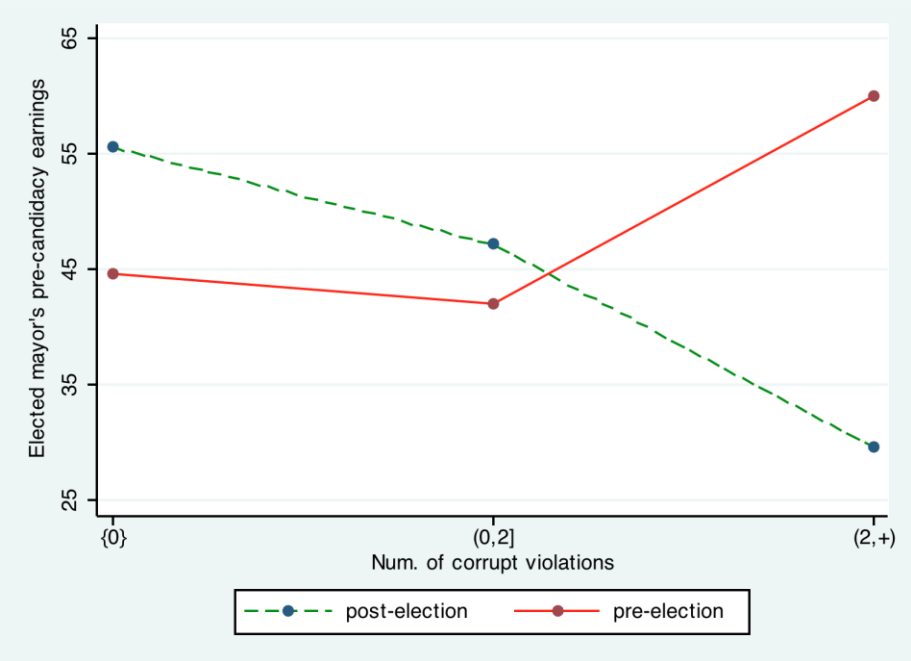
^{§§} This analysis is analogous to estimates of policies and institutions on politician selection based on the educational attainment of candidates and elected politicians, as in Besley (2004), Diermeier, Keane, and Merlo (2005), Ferraz and Finan (2010), Fisman et al. (2012) and Gagliarducci and Nannicini (2012).

^{***} The reported differences between pre-election and post-election audit municipalities are regression-adjusted for election period fixed effects. The graphical relationship and parametric estimates are qualitatively similar for the overall sample of municipalities (including municipalities in which mayors do not run for re-election).

positive politician selection among municipalities where voters receive information about negative audit outcomes.

The point estimate from a parametric empirical model of the average effect of the pre-election audit shows a slight degree of positive selection of politicians with previously higher earnings; it suggests that on average elected mayors following pre-election audits have earned an additional \$6,680 USD per capita (15 percent), although it is imprecisely estimated (Appendix Table I, column 1). However, there is a positive earnings-selection effect among municipalities with non-zero levels of corruption, as captured by parametric estimates of the reduced-form relationship following empirical model (5). The point estimate indicates that those (newly elected or re-elected) mayors have earned an additional \$10,910 USD per capita (25 percent) for each additional finding per report (column 2). Overall, the estimates support the hypothesis that information about corrupt violations induces a degree of pre-incumbency earnings-based selection..

APPENDIX FIGURE I:
 RELATIONSHIP BETWEEN REPORTED CORRUPTION LEVELS AND ELECTED MAYOR'S
 PRE-CANDIDACY EARNINGS FOR MUNICIPALITIES AUDITED BEFORE AND AFTER
 ELECTIONS



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

APPENDIX TABLE I:
THE EFFECTS OF THE AUDITS ON POLITICIAN SELECTION, LONG-TERM ELECTORAL OUTCOMES

Sample	Dependent variables:							
	Elected mayor's earnings (000's) (5 years before election) [2000 and 2004 elections]				Incumbent runs for re-election (period t+4)			
	Mayors running for re-election (period t)							
	OLS (1)	OLS (2)	OLS (3)	OLS (4)	OLS (5)	OLS (6)	OLS (7)	OLS (8)
Pre-election audit	6.68 (11.80)	-4.63 (13.67)	0.57 (11.25)	3.31 (11.22)	-0.083 (0.068)	-0.077 (0.085)	-0.094 (0.085)	0.052 (0.088)
Pre-election audit × Num. violations		10.91* (5.74)				0.015 (0.044)		
Num. of violations		-3.67 (3.35)				0.012 (0.017)		
Pre-election audit × Incumbent's party has won in previous 3+ elections			27.85* (15.48)				0.028 (0.153)	
Pre-election audit × terms in office				2.24 (6.92)				-0.094* (0.048)
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election Year & Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-election audits F-statistic [p-value]	-	2.18 [0.12]	3.11 [0.05]	0.18 [0.84]	-	0.43 [0.66]	0.77 [0.47]	2.51 [0.09]
Observations	96	96	96	96	241	241	241	241
Mean of dep. variable (controls)	44.0	44.0	44.0	44.0	0.82	0.82	0.82	0.82

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