

# Norms of Corruption in Politicians' Malfeasance\*

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## Abstract

To what extent can audits serve to limit patronage and corrupt networks effectively and sustainably in clientelist societies with a prevailing norm of corruption? We develop a political agency model in which office holders are motivated to reduce rent seeking behavior through re-election incentives operating via elections and audits (formal institutions), but also through reputational concerns that are influenced by the prevailing norm on corruption in their peer group (informal institutions). We show that, while the formal institutions of audits and elections have the desired direct effect of reducing corruption, they also affect informal rules of conduct, which can have unintended effects. We then apply this theoretical framework to evidence from Puerto Rico's anti-corruption municipal audits program over the period 1987-2014, and argue that the interaction of elections, audits, and norms can help explain a peculiar pattern in the data. Using a quasi-experimental design based on the exogenous timing of audits relative to elections, we find that mayors respond positively to audits in their own community, but negatively to audits - and the corresponding reduction in corruption - in neighboring municipalities. Our estimates suggest a large negative spillover effect: communities where two-thirds of adjacent jurisdictions undergo a (timely) audit experience a 30 percent increase in reported corruption levels.

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# 1 Introduction

Most countries — including most non-fragile states in the developing world — fit the description of *clientelist societies*, characterized by a strong presence of patronage networks that facilitate the capture of government by elites and special interest groups along with dysfunctional bureaucracies (Acemoglu and Robinson, 2012; Besley and Persson, 2011; Fukuyama, 2017). Notably, a sizeable number of countries in this group are democracies, with well established formal institutions (e.g., constitution, civil codes) similar to those of developed countries.<sup>1</sup> One possible explanation of this puzzle is that the clientelist societies have different norms, cultures and traditions, or *informal* institutions; across many Latin American, Asian or African societies, for example, patronage, nepotism, and gift giving are viewed as acceptable practices to sustain relationships (Nunn, 2012, 2022; Prasad et al., 2019; Köbis et al., 2018). Clientelism and high levels of corruption are arguably connected to patrimonialism, low quality bureaucracies, and a general distrust in the state (Fukuyama, 2017; Robinson and Verdier, 2013; Anderson et al., 2015b).

A central question becomes whether and to what extent formal institutions and the law can serve to limit patronage and corrupt networks effectively and sustainably in the presence of, and possibly despite of, prevailing social norms.<sup>2</sup> In this paper, we study how legal institutions designed to curb corruption fare in a society where patterns of behavior are also shaped by social norms. We develop a model where office holders are motivated through formal incentives (elections and anti-corruption measures), intrinsic benefits, and social reputation. The theory implies that anti-corruption efforts can have negative spillover effects in clientelist societies where corrupt behavior is the norm, which dampens the overall success of those measures. We then provide robust evidence of such externalities in empirical data from Puerto Rico, and argue that our theoretical framework can help explain these patterns.

Our theory is built on a standard political agency model in which office holders differ in their intrinsic motivation (or private benefit of rent-seeking) and are limited in their anti-social behavior by formal institutions, specifically elections and audits. Audits provide a supplementary source of information for voters, which can amplify the role of elections incentivizing politicians. In addition, we posit that office holders are concerned about their reputation among peers (or, alternatively, their self-image), following the formalization of social motives developed in Bénabou and Tirole (2006, 2011). In their decision making, office holders weigh the benefits from corruption against a desire to stay in office and the loss of reputation or self-image. We show that an audit in their own community unambiguously boosts an incumbent’s re-election chances provided no wrongdoing was found, and lowers rent-seeking. How the social motives affect rent-seeking behavior is ambiguous, however, and primarily depends on the extent to which anti-social behavior carries a stigma and pro-social behavior provides honour (Bénabou and Tirole, 2011), which in turn is determined by the prevailing social norm, i.e., whether corruption is a majority or a minority phenomenon. Assuming that the relevant peer group of office holders are their counterparts in neighbouring communities, audits in those communities constitute shocks to the prevalence of corruption and thus induce spillover effects, the sign of which depends on whether most office holders are corrupt or not. In a society where corruption is “the norm”, the spillover is *negative*, meaning that anti-corruption efforts elsewhere *increase* corruption at home – and thus the effects of audits are dampened. In a society where corruption is not the norm, anti-corruption efforts affecting others in the peer group have a *positive* spillover effect – own and peer

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<sup>1</sup>Examples that easily come to mind are Italy or Greece, two wealthy industrialized countries who share their basic civil code with European neighbours such as Germany. In other parts of the world, Brazil, Mexico, and India are examples of clientelist societies, despite adequate administrative and political institutions and ongoing efforts to curb corruption and political patronage. Most of these countries are governed by a civil code originating from the Code Napoleon, and have very similar administrative states through which the code is enforced.

<sup>2</sup>A related question is why targeted anti-corruption efforts in many of those societies have been remarkably unsuccessful, as highlighted in the survey by Gans-Morse et al. (2018).

corruption are complements, amplifying the desirable direct effect of audits.

We study these predictions in the context of Puerto Rico’s municipal anti-corruption audits program. Puerto Rico is an excellent candidate to apply our model because, as a U.S. territory, it shares many characteristics and formal federal institutions with U.S. states. In particular, elections are fair and conducted according to international standards, and institutions aimed at curbing corruption are very well designed. Yet, it is considered a clientelist society with high levels of corruption and patronage among elected officials.<sup>3</sup> We construct a longitudinal and spatially linked dataset of anti-corruption audits in all municipal governments during the period 1987-2014. To credibly measure the causal effects of these audits, our empirical strategy exploits the fact that audit reports released before an election — ‘timely audits’ — are more likely to matter for re-election chances than reports published after an election, and that municipalities are audited in a pre-established and fixed order.<sup>4</sup> Whether any given audit is ‘timely’ or ‘untimely’ is thus exogenous and the assignment of municipalities into timely and untimely groups is plausibly quasi-random. Because politicians anticipate the audit schedule, we can identify their behavioral responses to a timely audit. More importantly, the setting allows us to identify reported corruption in municipalities that are heterogeneous with regard to how many of their neighbouring communities undergo a timely audit, thereby measuring the *spillover effects of audits elsewhere* on contemporaneous levels of corruption at home.

Our empirical results confirm that timely audits have their intended direct effect in that they are associated with a significant reduction in reports of corrupt violations in the home community. At the same time, however, we find substantial spillover effects of timely anti-corruption audits in neighboring municipalities. In particular, our estimates suggest that if timely audits take place in two-thirds of adjacent municipalities (the sample median), reported corruption levels in the home community *increase* by 10 percent. The effects are considerably larger—34 percent—for findings of corruption attributed to the municipal government leadership (mayor or vice-mayor), and are even more concentrated among experienced politicians who have been in office for more than one term: we find a substantial 51 percent increase in reported corruption attributed to long-term incumbent mayors and vice-mayors. At the same time, our results indicate that the increased corruption driven by these spillovers is not associated with an increase in the incumbent mayors’ electoral accountability, i.e., a corresponding decrease in re-election prospects.

These findings are consistent with the idea that politicians are motivated by both re-election and image-based concerns and our specific theoretical framework in several important respects. First, audits that are timed close to an election do have their intended direct effect in reducing rent seeking, suggesting office holders care about their re-election and thus worry about the information that audits provide to voters. Second, the clear pattern of negative spatial spillover effects of anti-corruption efforts is consistent with a social motive where office holders’ reputation or self image depends on the prevailing norm of corruption in their peer group. Third, since the data shows that a majority of municipal office holders are in fact corrupt, our finding that anti-corruption efforts elsewhere and pro-social behavior at home are substitutes, not complements, is consistent with the theoretical prediction of the sign of the spillover being determined by the prevalence of corruption. This finding constitutes evidence in favor of Bénabou and Tirole (2011)’s theory of how prosocial or antisocial actions can be complements or substitutes depending on whether stigma or honour dominates the reputational concern.<sup>5</sup> Third, we find that the spillover effects concerning office

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<sup>3</sup>For a summary of the growing literature on corruption and clientelism in Puerto Rico, see e.g. Atilés et al. (2022), and Bobonis et al. (2022).

<sup>4</sup>See (Bobonis et al., 2016). Since all municipalities are audited, we do not observe corruption in the counterfactual world where no audit takes place.

<sup>5</sup>The social (or self-) image of office holders endogenously depends on the social norm here: while corruption is always antisocial and abstaining from corruption thus increases one’s reputation (or self-image), the extent to which this happens depends on the prevailing norm, i.e., how others in the peer group behave. The idea that social norms and reputational concerns are intertwined is not new, and has been supported empirically (e.g. Kimbrough and Vostroknutov, 2016; Howe et al.,

holder behavior are concentrated primarily among long-term incumbents, not among first-office holders. The (self-)image-based motive in our model offers a simple explanation for this result since long-term incumbents naturally will put relatively less weight on re-election incentives than first-time office holders under the reasonable assumption (which we confirm in the data) that the re-election chances of the former group are less sensitive to the findings of audits than those of the latter group. Thus, the data support the notion that motives other than re-election prospects play a key role in shaping corrupt behavior. Crucially, this last empirical result is inconsistent with yardstick competition among office holders – a phenomenon that could provide an alternative explanation of our observed spillovers that is entirely based on formal institutions – as well as with other explanations such as the spatial displacement of corrupt activities.

This paper contributes to several strands of research. First, our findings on the extent to which social motives dampen formal incentives when the prevailing norm is corruption tie into previous theoretical work on why corruption can be self-enforcing and societies could become stuck in a high corruption equilibrium, once a critical level of malfeasance is reached (e.g. Andvig and Moene, 1990; Bardhan, 1997; Mishra, 2006; Tirole, 1996; Mauro, 2004). Similarly, we contribute to existing research on the extent to which unethical behavior is shaped by legal institutions on the one hand, and cultural norms or beliefs on the other hand. The bulk of this literature considers those factors separately, however, and we therefore have a limited understanding of their interaction.<sup>6</sup> Exceptions are recent contributions by Acemoglu and Jackson (2017) and Besley et al. (2019) that examine how abruptly enacting legal reforms that are in conflict with prevailing social norms can paradoxically increase illegal activities.

Second, we add to the literature on the effectiveness of anti-corruption strategies by providing an explanation of – and empirical evidence on – why targeted anti-corruption efforts in clientelist societies are often remarkably unsuccessful. Specifically, although anti-corruption audit programs appear to be effective at reducing corruption by public officials in the short-run, the evidence regarding the sustainability of these efforts is mixed (e.g. Bobonis et al., 2016; Finan et al., 2017; Avis et al., 2018; Finan and Mazzocco, 2021).<sup>7</sup> At the same time, the negative externality we identify stands in contrast with a small literature documenting that newly implemented anti-corruption audit programs can have positive spillovers, due to salience and learning (Avis et al. (2018) and Zamboni and Litschig (2018)). We discuss our findings in relationship to this literature in Section 6.<sup>8</sup>

Lastly, since our empirical results provide indirect evidence on image-based motivation of politicians, they underscore the importance of reputation (or self-image) as a powerful determinant in prosocial or antisocial behavior, as put forward by e.g. by Andreoni and Bernheim (2009), Bénabou and Tirole (2006, 2011) or Hoffman et al. (2000) in the economics field, and a large literature in evolutionary biology (e.g. Nowak and Sigmund, 2005; Chudek and Henrich, 2011) and social psychology (see Anderson et al. (2015a) for a review). Here, our model is most closely related to the recent contributions of Besley et al. (2019), Jia and Persson (2020) and Graf et al. (2023) who like us use the Benabou-Tirole framework to better understand how the interconnection of formal incentives, intrinsic motivation and reputational concerns can help explain behavioural patterns they find in the data.<sup>9</sup> More generally, our empirical findings add to the

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<sup>6</sup>See Finan et al. (2017) for a detailed review of these arguments and a survey of the empirical evidence. Köbis et al. (2018) provide an overview from a social psychology perspective on the relatively small literature of social norms of corruption.

<sup>7</sup>Gans-Morse et al. (2018), Olken and Pande (2012), and Prasad et al. (2019) provide thorough and interdisciplinary reviews of empirical studies on corruption and anti-corruption policies.

<sup>8</sup>Since we identify unintended consequences of anti-corruption measures when officials are motivated by concerns for reputation, our work is also related to Hanming Fang and Zh (2022) who show that in order to avoid being stereotyped as corrupt, city official in China may be favoring state-owned land developers at the expense of efficient competing private developers, which could explain the recent rise in state-owned enterprises in the country.

<sup>9</sup>While Besley et al. (2019) use the framework to explain the observed dynamics of property-tax evasion in the United Kingdom, Jia and Persson (2020) study the ethnic choice of children in ethnically mixed marriages in China. Graf et al.

growing empirical research on how social motives as well as legal enforcement measures influence citizen-state relationships (e.g. Fisman and Miguel, 2007; Perez-Truglia and Cruces, 2017; Dellavigna et al., 2017).

The paper is organized as follows. Section 2 provides a brief background on Puerto Rico’s municipal government structure and auditing program. We follow with a description of our theoretical framework and the model’s predictions in Section 3. Section 4.1 discusses the data and our empirical design. We present the central empirical results of the paper in Section 5. In Section 6 we consider alternative theoretical explanations. The paper concludes in Section 7.

## 2 Background

### Municipal Government Administration, Politics, and Corruption

Patronage, clientelism, and corruption have been salient features of Puerto Rico (PR)’s modern history: the country is considered a quintessential example of a clientelist society, both among scholars and the general public (Pantojas-García, 2015; Atilas et al., 2022). Corruption in local governments takes diverse forms and is typically based on procurement fraud, such as illegalities in auctions and in other forms of contracting, as well as over-invoicing. Public corruption can also manifest itself in the illegal hiring and firing of employees due to political patronage or nepotism. In addition, the information we have from the audit reports suggests that some individuals simply divert resources for personal purposes. For more details on these forms of corruption as well as examples, see Bobonis et al. (2016).

Like higher levels of government, municipal politics in PR are controlled by two political parties, Partido Popular Democrático (PPD) and the pro-statehood Partido Nuevo Progresista (PNP). The elected positions consist of the mayor and members of the local assembly, all elected for a four-year term following the Commonwealth (and U.S. federal) government electoral cycle.<sup>10</sup> The elected mayor appoints the top management of the municipality. There are no term limits for mayors nor for members of the municipal assembly. As a consequence, in municipalities where one party heavily dominates the political landscape, incumbent mayors tend to remain in office for long periods of time.

Similar to the United States, municipal governments possess a certain degree of autonomy. Although their responsibilities are generally limited to public health services, infrastructure, and solid waste management, there is some heterogeneity in governments’ capacity to raise tax revenue and manage expenditures.<sup>11</sup> Despite the high prevalence of malfeasance – 64 percent of the audits in our sample reveal evidence of corruption – technocratic anti-corruption efforts via the Office of the Comptroller of Puerto Rico (“OCPR”)’s anti-corruption audit program, are reasonably effective. This is particularly the case in more politically competitive jurisdictions (Bobonis et al., 2016). Moreover, and as shown in this earlier work, information regarding corruption is processed across ideological loyalties in Puerto Rico in a manner that enables voters to sanction and hold corrupt officials accountable.

### 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 stated mission is to “audit the property and public funds transactions with independence and objectivity to determine if they have been done in

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(2023) extend the Benabou-Tirole model to allow for social norms governing the perception of extrinsic motivation, and use the framework to explain heterogeneity in blood donation patterns across Europe.

<sup>10</sup>The size of the municipal assembly, which varies between 12 and 16 members, is a step function of the population that resides within its boundaries.

<sup>11</sup>Large municipal governments with more active mayors such as San Juan (the capital), Guaynabo, Bayamón, and Caguas assert a significant degree of autonomy. Conversely, smaller municipalities have access to fewer resources, and thus depend more on the central government.

accordance to the law[, and] promote the effective and efficient use of the government resources [...]” (Office of the Comptroller 2009). The OCPR has been carrying out audits on municipal governments, producing and disseminating reports on corrupt activities, uninterrupted since 1953.

The audit process is thorough; Bobonis et al. (2016) provides a detailed description. In addition to looking for discrepancies within government documents and accounts, highly trained auditors perform inspections for the existence and quality of public construction work and the delivery of public services, and they also conduct interviews with municipality officials and local community members.<sup>12</sup> At the end of the process, a final report is issued and disseminated to the media through press conferences. Findings of corruption have also been publicized as part of the political campaigns of opposition candidates, and more recently, reports are posted online as well. Depending on the size and complexity of the municipal government, the OCPR may publish multiple reports on a municipality for one auditing period.

Importantly, several measures are taken to minimize potential biases in the conduct of the audits and in the dissemination of findings. First, the OCPR is provided with a substantial degree of autonomy from the rest of the central government in the constitution, with the Comptroller appointed by the governor of Puerto Rico for a ten-year term. Second, the OCPR is accountable to both legislative chambers, not the executive branch. Third, auditors are selected through a public process, earn highly competitive salaries, and receive extensive training. Lastly, in order to reduce individual conflicts of interest, auditors are prohibited from participating in audits of their municipality of residence.

According to the OCPR’s constitutive legislation, municipal governments are to be audited every other fiscal year. However, due to the OCPR’s resource constraints, in reality these audits generally take place three to six years apart. Critically for our research design, municipalities are audited following a pre-specified order and are thus *anticipated*, which allows us to causally link behavioral responses. Once all municipalities have been audited, a new auditing round takes place following the same pre-specified order. Given that our data-set spans three decades, all seventy-eight municipalities were audited multiple times during our period of interest. We discuss additional details of the audits and data in Section 4.1 below.

### 3 Theoretical Framework

In this section, we formulate a general theoretical framework based on a career concerns agency model of politicians’ behaviour, which we augment by elements of the model of Bénabou and Tirole (2006) to generate a reputation-based motive through which inter-community spillovers in corruption occur. The model formalizes the notion that pro- and antisocial behaviors among the political class are governed by both formal and informal rules of conduct. The model helps to shed light on the forces that drive the empirical findings we report in Section 5 below.

#### 3.1 A Model of Corruption and Inter-community Norm Spillovers

##### 3.1.1 Basic Framework

Consider an economy with a large number of identical communities populated by a continuum of voters of size (measure) one. Time lasts two periods denoted by  $t = 1, 2$ . In each period, voters in a representative community elect a politician to hold office. The elected politician, henceforth incumbent, receives ego rents

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<sup>12</sup>Bobonis et al. (2016) indicate several features of the audit program that work to minimize potential biases in the conduct of audits: the OCPR functions autonomously from the central government; focused on the activities of the executive branch, the OCPR is accountable to the state legislature; and individual auditors undergo a highly competitive selection process, are extensively trained, earn high salaries, and are prohibited from participating in audits in their home municipality.

$R$  from holding office period, and decides whether or not to divert local resources for personal gain. Let  $r \in \{0, 1\}$  be the official's decision, with  $r = 1$  denoting positive rent extraction.

### Office holders

In choosing whether or not to extract rents, office holders weigh individual motives and social motives; in particular, a politician's decision varies with their intrinsic motivation of advancing public interests, the chances of being re-elected, and the social reputation gain (or loss) with respect to her peers. We describe each of these aspects separately below.

In each period, an incumbent politician receives a material payoff  $R + br + \theta(1 - r)$ , where  $b$  is a fixed (monetary) benefit from rent extraction and  $\theta$  denotes the intrinsic motivation to act pro-socially, which is randomly drawn from a continuously differentiable and symmetric uni-modal distribution  $G(\theta)$  with mean and median  $\mu_\theta = 0$ .  $\theta$  is known only to the politician. *Ceteris paribus*, a politician with a higher motivation  $\theta$  has less incentive to take a corrupt action.

For simplicity, we assume that politicians care only about the material payoff in the second period. Period-2 incumbents therefore extract rents if and only if  $\theta < b$ . There is no discounting. Denoting by  $P(r)$  the probability that an incumbent is re-elected after the first period given his first period decision  $r$ , the period-1 individual gain of office holders from being re-elected in the second period is

$$P(r)(R + \max\{\theta, b\}).$$

In addition, period-1 incumbents are concerned about how other politicians view their intrinsic motivation  $\theta$ , i.e., their social reputation among their peer group (the formalization below also admits self-image as an alternative interpretation). We assume that incumbents in different communities observe each others' decisions  $r$  in period 1. This assumption captures the fact that fellow politicians have better knowledge than voters on whether an incumbent of a community is acting to advance public interests.<sup>13</sup> Following Bénabou and Tirole (2006), Besley et al. (2019), and Jia and Persson (2020), among others, we model a politician's reputation as her 'expected type'  $E[\theta|r]$ , which is the average type of politicians in adjacent communities who make the same choice as the incumbent.

Denoting by  $\mu \in [0, 1]$  the relative weight that a politician puts on her social motive, an office holder's preferences can thus be summarized by

$$u(r|\theta) = R + br + \theta(1 - r) + (1 - \mu)P(r)(R + \max\{\theta, b\}) + \mu E[\theta|r], \quad (1)$$

and she chooses  $r = 0$  if and only if

$$u(r = 0|\theta) \geq u(r = 1|\theta),$$

or equivalently,

$$\theta \geq b - (1 - \mu)(P(r = 0) - P(r = 1))(R + \max\{\theta, b\}) - \mu(E[\theta|r = 0] - E[\theta|r = 1]). \quad (2)$$

As will become clear shortly, the right-hand side is less than  $b$  since, both re-election chances (the second term) and reputation (the third term) increase in equilibrium if the incumbent chooses the pro-social action ( $r = 0$ ) over the corrupt action ( $r = 1$ ); we have  $P(r = 1) < P(r = 0)$  and  $E[\theta|r = 1] < E[\theta|r = 0]$ .<sup>14</sup>

<sup>13</sup>We can easily allow for only partial observations of  $r$ , similar to Besley et al. (2019). Also recall that one period would usually cover multiple years of office until re-election, so while the actions of other politicians may be observed with a lag, it would still be within the same electoral period under consideration.

<sup>14</sup>We will verify this below. The only case where the re-election probability would not be monotonically decreasing in  $r$  would

## Voters

In each period, the decision of the incumbent  $r \in \{0, 1\}$  and the state of the economy  $s \in \{0, 1\}$  jointly determine public good consumption  $y$  for a representative voter in a community,

$$y = 1 - r \cdot (1 - s).$$

Public good consumption  $y$  is publicly observable, but the state of the economy  $s$  is not. We assume that the incumbent's choice of  $r$  is revealed to voters only through an audit. A favourable state  $s = 1$  will therefore 'mask' bad behavior in a community without an audit. Conversely, voters will know whether the office holder took the corrupt action if the state is unfavorable,  $s = 0$ , or if there was an audit. Other than through a change in voter behavior, however, there are no penalties associated with corruption, that is, the only punitive effect that audits can have work through re-election chances. We also assume—in line with actual circumstances in our data—that incumbents are aware of whether they will have to undergo an audit or not.

We model elections with a simple probabilistic voting model. Voters care about their public good consumption as specified above. In addition, they experience a random popularity shock  $\delta$  in favor of the incumbent at the end of period 1, which is drawn from a normal distribution  $N(0, 1/\psi^2)$ . The parameter  $\psi$  measures the sensitivity of the incumbent's re-election chances to choice of  $r$ , and will serve later to distinguish between short-term and long-term incumbents. The voters thus re-elect the incumbent if and only if the sum of his popularity  $\delta$  and the expected pecuniary payoff she can generate in period 2 is larger than that of a challenger. The latter's intrinsic motivation is drawn from the same distribution  $G(\theta)$  as that of the incumbent, ensuring that differences in expected period-2 behavior between incumbent and challenger are solely due to voters having accumulated more information on the incumbent's type at the end of period 1.

Specifically, at the end of period 1, voters observe the level of public goods  $y$ . They also have information  $I \in \{\emptyset, r\}$ , where  $I = r$  if an audit took place in their community and  $I = \emptyset$  otherwise. For a given anticipated strategy  $\hat{r}(\theta)$  of the incumbent, voters use their knowledge of  $y$  and  $I$  to form rational expectations of the politician's type  $\theta$ . Denoting by  $p$  the probability that  $s = 1$ , the expected period-2 payoff of a voter when voting for the incumbent of popularity  $\delta$  is

$$p + (1 - p)\Pr(\theta \geq b|y, I, \hat{r}) + \delta.$$

Comparing this with the expected payoff from voting for the challenger,  $p + (1 - p)(1 - G(b))$ , and assuming that voters favor the incumbent if they are indifferent, the incumbent wins the election if and only if

$$\delta \geq \underline{\delta}(y, I, \hat{r}) \equiv (1 - p)[(1 - G(b)) - \Pr(\theta \geq b|y, I, \hat{r})].$$

By choosing  $r$ , the candidate determines the distribution of  $y$  and voter information  $I$  (if there is an audit), taking  $\hat{r}$  as given. From her perspective, the probability of re-election as a function of  $r$  is thus the probability that  $\delta \geq \underline{\delta}(y, I, \hat{r})$  or equivalently,  $P(r) = E_{y, I}[1 - \Phi(\underline{\delta}(y, I, \hat{r}))|r] \in (0, 1)$ , where  $\Phi(\cdot)$  denotes the cdf of a normal distribution.

## Equilibrium

Elections in this economy serve both a disciplining and a sorting role: voters can oust candidates who have

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be a situation where the incumbent's strategy is non-decreasing (or non-monotone) in  $\theta$ , which can be ruled out by a simple revealed preference argument.



revealed themselves to be corrupt while at the same time, candidates have an incentive to behave pro-socially to manipulate voter's beliefs about their type through their choice of  $r$  and the corresponding public good level  $y$  if there is no audit. Audits improve voters' ability to discipline incumbents and select good candidates by increasing the information available to voters.

Recall that incumbents with types  $\theta \geq b$  always choose the non-corrupt action: they not only follow their intrinsic motivation but also increase their chances of being re-elected and their social reputation. An incumbent with  $\theta < b$  on the other hand may choose  $r = 1$  over  $r = 0$  if her intrinsic payoff from the corrupt action is sufficiently high. From (2), there is a cut-off value  $\theta^* < b$  defined by

$$\theta^* = b - (1 - \mu)(P(r = 0) - P(r = 1))(R + b) - \mu(E[\theta|r = 0] - E[\theta|r = 1]), \quad (3)$$

such that the office holder's equilibrium choice of  $r(\theta)$  is  $r = 0$  if  $\theta \geq \theta^*$  and  $r = 1$  otherwise. In equilibrium, voters correctly infer the threshold value  $\theta^*$  and  $\hat{r}(\theta) = r(\theta)$ . Voters' beliefs  $\Pr(\theta \geq b|y, I, \hat{r})$  and, hence, the re-election probability of the incumbent thus depend on  $\theta^*$ . At the same time,  $\theta^*$  determines how many peers are corrupt in equilibrium, and therefore also influences the reputation gain (loss) from the pro-social (anti-social) action. We examine each of these channels in turn.

### 3.1.2 Electoral Discipline and Audits at Home

To begin with, we examine how the re-election chances affect the incentives of the incumbent to take the pro-social action, and how the incentives are shaped by a (timely) audit in their own community. Consider first a community with no audit, so  $I = \emptyset$ . If voters observe  $y = 0$ , they know for sure that  $\theta < b$ . Hence, the incumbent is re-elected if and only if her popularity  $\delta$  satisfies  $p + \delta \geq p + (1 - p)(1 - G(b))$  or

$$\delta \geq \underline{\delta}^0 \equiv (1 - p)(1 - G(b)) > 0. \quad (4)$$

If voters observe  $y = 1$ , given the office holders (correctly anticipated) strategy that  $r(\theta) = 0$  if and only if  $\theta \geq \theta^*$ , the conditional probability of  $\theta \geq b$  is

$$\Pr(\theta \geq b|y = 1, \emptyset, \theta^*) = \frac{1 - G(b)}{p + (1 - p)(1 - G(\theta^*))}.$$

The incumbent is re-elected if and only if  $[p + (1 - p)\Pr(\theta \geq b|y = 1, \emptyset, \theta^*)] + \delta \geq p + (1 - p)(1 - G(b))$  or, equivalently,

$$\delta \geq \underline{\delta}_N^1(\theta^*) \equiv -(1 - p)^2 \frac{(1 - G(b))G(\theta^*)}{p + (1 - p)(1 - G(\theta^*))} < 0, \quad (5)$$

where the subscript  $N$  stands for the no-audit case. Recalling that corrupt actions are disguised by a good state of economy  $s = 1$  with probability  $1 - p$ , and that non-corrupt actions always result in  $y = 1$ , so the probability of re-election for the incumbent is

$$P_N(r, \theta^*) = \begin{cases} 1 - p\Phi(\psi\underline{\delta}_N^1(\theta^*)) - (1 - p)\Phi(\psi\underline{\delta}^0) & \text{if } r = 1 \\ 1 - \Phi(\psi\underline{\delta}_N^1(\theta^*)) & \text{if } r = 0. \end{cases} \quad (6)$$

with  $\underline{\delta}_0$  and  $\underline{\delta}_N^1$  defined by (4) and (5), respectively. The change in probabilities that the incumbent is re-elected when he takes  $r = 0$  as opposed to  $r = 1$ , namely the re-election motive in no-audit communities,

can be written as

$$\Delta_N^R(\theta^*) \equiv P_N(r = 0, \theta^*) - P_N(r = 1, \theta^*) = (1 - p)(\Phi(\psi \underline{\delta}^0) - \Phi(\psi \underline{\delta}_N^1)) > 0, \quad (7)$$

confirming that considerations of electoral success motivate politicians to behave pro-socially. For future reference, note that  $\Delta_N^R$  is increasing in  $\theta^*$ , as  $\underline{\delta}_N^1(\theta^*)$  is decreasing in  $\theta^*$ . Intuitively, as voters expect fewer incumbents to behave ethically, they take a  $y = 1$  as stronger evidence of pro-social behavior and are more likely to re-elect an incumbent who delivered a good outcome. For a given  $\theta^*$ ,  $\Delta_N^R$  is also increasing in the sensitivity  $\psi$  of voters' to office holder behavior, as

$$\frac{\partial \Delta_N^R(\theta^*)}{\partial \psi} = (1 - p)\psi \cdot (\phi(\psi \underline{\delta}^0) \underline{\delta}^0 - \phi(\psi \underline{\delta}_N^1) \underline{\delta}_N^1) > 0,$$

where  $\phi$  denotes the normal density function.

We next investigate how audits (the main source of external variation in our empirical analysis) affect incentives. It is easy to show that audits have a further disciplinary effect by decreasing the chances of re-election for  $r = 1$  and increasing the chances of re-election for  $r = 0$ . To see this, suppose the incumbent chooses  $r = 1$ . Since an audit always discloses  $r$ ,  $I = 1$  and voters know for sure that the incumbent's type is  $\theta \leq \theta^* < b$ . As a result, her re-election chances are identical to the case where no audit takes place but a public good level of  $y = 0$  discloses corruption. Now suppose the incumbent chooses  $r = 0$ , which ensures that the public good outcome of the community is always  $y = 1$ . When there is no audit, the voters observe  $y = 1$ , but are unsure whether the outcome resulted from ethical behaviour or a favorable state of economy, or both. An audit, in contrast, allows voters to observe  $I = r = 0$  directly. Knowing that the incumbent's type is  $\theta \geq \theta^*$  with certainty, they will re-elect her if  $p + (1 - p)(1 - G(b))/(1 - G(\theta^*)) + \delta \geq p + (1 - p)(1 - G(b))$  or

$$\delta \geq \underline{\delta}_A^1(\theta^*) \equiv -\frac{(1 - p)(1 - G(b))G(\theta^*)}{1 - G(\theta^*)},$$

where the subscript  $A$  indicates an audit. The probability of re-election is

$$P_A(r, \theta^*) = \begin{cases} 1 - \Phi(\psi \underline{\delta}^0) & \text{if } r = 1 \\ 1 - \Phi(\psi \underline{\delta}_A^1(\theta^*)) & \text{if } r = 0, \end{cases} \quad (8)$$

As is easily verified,  $\underline{\delta}_A^1(\theta^*) < \underline{\delta}_N^1(\theta^*)$  for all values of  $\theta^*$ . Thus, we have the following observation.

**Observation 1.** *For all values of  $\theta^*$ ,  $P_A(r = 1, \theta^*) < P_N(r = 1, \theta^*)$  and  $P_A(r = 0, \theta^*) > P_N(r = 0, \theta^*)$ . That is, (non)corrupt incumbents' election chances fall (rise) as a result of a home-community audit.*

Letting

$$\Delta_A^R(\theta^*) \equiv P_A(r = 0, \theta^*) - P_A(r = 1, \theta^*) = \Phi(\psi \underline{\delta}^0) - \Phi(\psi \underline{\delta}_A^1(\theta^*)), \quad (9)$$

and noting that  $\Delta_A^R(\theta^*) > \Delta_N^R(\theta^*)$ , we can conclude that audits unambiguously increase the gain (loss) in expected electoral success resulting from behaving (un-)ethically, as one would expect. Again, the expected electoral gain from a non-corrupt action is increasing in  $\theta^*$  — the fewer incumbents are hiding their true type with a pro-social action in the first period, the more confident voters are that a re-elected first-period incumbent will not act corrupt in the second period — and amplified by  $\psi$ .

### 3.1.3 Social Motive

The social component of an incumbent's preferences affects the behavior of the incumbent through (implicit) comparison to the behavior of all her fellow politicians. Building on Bénabou and Tirole (2006), we formalize this idea as follows. Let  $\tilde{\theta}$  be the average cutoff  $\theta$  in adjacent communities, which measures the total share of office holders in the incumbent's peer group who are corrupt. The social payoff for a given choice of  $r$  is  $E[\theta|r, \tilde{\theta}]$ , resulting in a reputation gain from choosing  $r = 0$  of  $E[\theta|\theta \geq \tilde{\theta}]$  while  $r = 1$  yields a lower payoff  $E[\theta|\theta < \tilde{\theta}]$ .<sup>15</sup>

The social motive channel through which the behavior of office holders in adjacent communities impacts the choices of an incumbent in the home community can then be subsumed by

$$\Delta^S(\tilde{\theta}) \equiv E[\theta|\theta \geq \tilde{\theta}] - E[\theta|\theta < \tilde{\theta}] > 0. \quad (10)$$

The first term in this expression can be interpreted as the 'honor' of pro-social choice and the second term as the 'stigma' of anti-social choice. Note that both conditional means are monotone and increasing in  $\tilde{\theta}$ . How the gain in social reputation from pro-social behavior  $\Delta^S(\tilde{\theta})$  changes with  $\tilde{\theta}$  therefore depends on whether the 'honor' effect dominates the 'stigma' effect or vice-versa. Consider for instance an exogenous increase in corruption among communities, which corresponds to a rise in  $\tilde{\theta}$ . As fewer peers are behaving ethically, more honour can be gained from a pro-social choice, but the stigma from corruption falls at the same time. Whether or not the office holder in the home community will respond with reduced or increased corruption in adjacent communities thus depends on the sign of  $\Delta^{S'}(\tilde{\theta})$ .

We use Figure 1 to illustrate the two possibilities about the sign of  $\Delta^{S'}(\tilde{\theta})$ . When  $\tilde{\theta} = \hat{\theta} > 0$ , only a small number of politicians behave ethically, a drop in the number of honorable incumbents (increasing  $\tilde{\theta}$ ) increases the honour of being pro-social by more than it lowers the stigma of corruption, namely the effect on the honor dominates the effect of the stigma. Hence,  $\Delta^{S'} > 0$ , and individual and social motives are substitutes. The social motive leads more politicians to reject the (corrupt) norm in order to have themselves perceived as exceptionally good citizens. Conversely, when  $\tilde{\theta} = -\hat{\theta} < 0$ , few peers are corrupt, a further increase in corruption does little to the honour of being pro-social but significantly reduces the stigma of corruption. Hence,  $\Delta^{S'} < 0$ , and individual and social motives are complements. The social motive leads politicians to 'conform' to the norm.

Our assumptions on  $G(\theta)$  ensure that  $\Delta^S$  has a unique interior minimum, which is located at  $\tilde{\theta} = 0$  due to the symmetry of  $G(\theta)$  around  $\theta = 0$  (Jewitt, 2004). This implies that for all  $\tilde{\theta} > 0$ , i.e., the majority of office holders behave unethically, we have  $\Delta^{S'}(\tilde{\theta}) > 0$ , namely the effect on the honor dominates the effect of the stigma; for all  $\tilde{\theta} < 0$ , i.e., the majority of office holders behave ethically, we have  $\Delta^{S'}(\tilde{\theta}) < 0$ . We illustrate this in Figure 2 using a  $t$ -distribution (with the degree of freedom being 5). Panel (2a) shows that the honor of being pro-social and the stigma of being corrupt are both increasing in  $\tilde{\theta}$ . The difference between these two effects, which is exactly  $\Delta^S(\tilde{\theta})$ , is plotted in Panel (2b). Clearly,  $\Delta^S(\tilde{\theta})$  is decreasing everywhere for  $\tilde{\theta} < 0$  and increasing everywhere for  $\tilde{\theta} > 0$ . Moreover, for this distribution, we have  $d^2\Delta^S(\tilde{\theta})/d\tilde{\theta}^2 > 0$ .

To build the intuition of how social motives affect equilibrium corruption in a simplified setting, let us ignore the re-election motive for the moment ( $\mu = 1$ ) and consider an increase in the direct benefit from corrupt behavior,  $b$ . Noting that audits play no role if behavior is solely determined through social motives, and so all communities are *de facto* identical, we have  $\tilde{\theta} = \theta^*$ . From (3) and using (10), the equilibrium

<sup>15</sup>Recall that  $r$  is observable in the peer group, but not  $\theta$ . This formulation also assumes that peers care about the average motivation, and one's reputation does not vary with whether or not one's community is audited.

cutoff  $\theta^*$  is determined by

$$\theta^* = b - \Delta^S(\theta^*),$$

with

$$\frac{d\theta^*}{db} = \frac{1}{1 + \Delta^{S'}}.$$

The term  $1/(1 + \Delta^{S'})$  is a “social multiplier”. We follow Bénabou and Tirole (2011) to assume that the social multiplier is positive, which implies that social motives do not result in multiple equilibria.<sup>16</sup> If individual and social motives are substitutes ( $\Delta^{S'} > 0$ ), the social multiplier is less than one and individual incentives are dampened by the social motive. Similarly, the social multiplier is greater than one and individual incentives are amplified through social channels if individual and social motives are complements ( $\Delta^{S'} < 0$ ).<sup>17</sup>

### 3.2 The Effect of Audits and Social Motives on Equilibrium Corruption

If re-election motives play a role, audited communities will behave differently from communities that are not audited, which implies different cutoffs  $\theta_A^* \neq \theta_N^*$  in general whenever  $\mu < 1$ . Denoting by  $\alpha_i$  the share of communities with a timely audit ( $i = A$ ) and no timely audit ( $i = N$ ), respectively, the average cutoff among neighboring communities is  $\tilde{\theta} = \alpha_A \theta_A^* + \alpha_N \theta_N^*$ , with  $\alpha_N = 1 - \alpha_A$ . The equilibrium condition (3) can be written as, for  $i \in \{A, N\}$ ,

$$\theta_i^* + \mu \Delta^S(\tilde{\theta}) = b - (1 - \mu) \Delta_i^R(\theta_i^*)(R + b), \quad (11)$$

where  $\Delta_i^R(\cdot)$  is given by (8) and (6), respectively, and  $\Delta^S(\theta_i^*)$  is given by (10). Under mild conditions, given our assumption above that the social multiplier  $1/(1 + \Delta^{S'})$  is positive, we can prove the existence of equilibrium cutoffs  $(\theta_A^*, \theta_N^*)$  defined by (11) such that the incumbent chooses

$$r(\theta) = \begin{cases} 1 & \text{if } \theta \geq \theta_i^* \\ 0 & \text{otherwise.} \end{cases}$$

We relegate the proof to the appendix. Let us first confirm the effect of a (timely) audit on the home community in equilibrium. From (11) it is immediate that due to  $\Delta_A^R(\theta^*) > \Delta_N^R(\theta^*)$  for all  $\theta^*$ , we must have  $\theta_A^* < \theta_N^*$ . Hence,

**Proposition 1 (Equilibrium Corruption and Audits at Home).** *Equilibrium corruption in audited communities is lower on average than in non-audited communities,  $\theta_A^* < \theta_N^*$ .*

As explained in the introduction, the main purpose of our analysis is to gain some understanding of (spatial) spillovers and their empirical relevance. The channel that connects outcomes in neighboring communities is reputational concerns and social norms.<sup>18</sup>

<sup>16</sup>Given the distribution of  $\theta$ , it is possible to ensure  $1/(1 + \Delta^{S'}) > 0$  when the value of  $b$  is properly specified. For example, if  $G(\theta)$  is the  $t$ -distribution with the degree of freedom being 5, which is the distribution considered in Figure 2, when the value of  $b$  ensures  $\tilde{\theta}$  to be larger than  $-1.5$ , then  $1/(1 + \Delta^{S'}) > 0$ .

<sup>17</sup>In either event, we find in our data that the majority of incumbents corrupt, so  $\theta^* > 0$ . This would imply  $\Delta^{S'} > 0$  and ensure uniqueness. See Section 4.1 for more details.

<sup>18</sup>Our model is flexible enough to incorporate yardstick competition, similar to Besley and Case (1995), as an alternate mechanism to norms. Yardstick competition may be a plausible alternative channel through which spacial spillovers may occur. We have developed an extension of the model that includes yardstick competition, which is available upon request. While

Our next step is therefore to derive comparative static predictions of the model for the case where the number of audits elsewhere in the economy changes.<sup>19</sup> In doing so, we consider the effect of an external shock (more audits elsewhere) on the equilibrium behavior of home incumbents. Since the social motive does not depend on voters' behavior and re-election probabilities, the only factor through which an office holder's social norms payoff affects her choice of  $r$  is the difference in the average (or expected) type of politician who behaves pro-socially compared to the corresponding average type of corrupt politician,  $\tilde{\theta}$ .

Consider a small relative increase in the number of audits in adjacent communities, corresponding to an increase in  $\alpha_A$ . In the short run, when the incumbents in neighbouring communities have changed their behavior, the effect on  $\tilde{\theta}$  is simply  $\partial\tilde{\theta}/\partial\alpha_A = \theta_A^* - \theta_N^* > 0$ . In the long-run,  $\theta_A^*$  and  $\theta_N^*$  will adjust to new equilibrium values because all incumbents in all neighboring communities will experience a decrease in their peers' corruption. In what follows, we present results for both cases but, generally speaking, we are more interested in the short-term because our empirical analysis only pertains to short-run responses of politicians (within two years of their electoral cycle).

**Proposition 2 (Spillovers in Corruption through Social Norms).** *The effect of reduced corruption through audits in adjacent communities depends on the social norm  $\tilde{\theta}$ :*

- a) *If the norm is that the majority of politicians are corrupt, namely  $\tilde{\theta} > 0$ , then relatively more audits elsewhere increase corruption at home in the short run and the long run – neighboring and own corruption are substitutes.*
- b) *If the norm is that the majority of politicians behave pro-socially, namely  $\tilde{\theta} < 0$ , then more audits elsewhere will lead to a decrease in corruption in the home community in the short run – neighboring and own corruption are complements. In the long run, the spillover effect of audits is ambiguous: more audits elsewhere can decrease or increase corruption in the home community.*

*Moreover, the short run spillover effects of corruption through social norms are more pronounced (in either direction), the less sensitive re-election chances are to office holder behavior, i.e. the smaller  $\psi$ .*

Thus, when a majority of politicians is corrupt, the norm motive causes the spillover effect of audits to be negative: more corruption-reducing measures in adjacent communities causes home politicians to become *more corrupt* because it diminishes the honor of behaving pro-socially. When a majority of politicians are not corrupt, in contrast, the spillover effect is positive and audits elsewhere *reinforce good behavior* at home. The last part of the proposition states the magnitude of the spillover effect is decreasing in the sensitivity of voters to incumbent behavior  $\psi$ . This is intuitive, as an office holder whose re-election chances are very sensitive to his behavior will put relatively less weight on the re-election motive. In the extreme case where  $\psi \rightarrow 0$ , the spillover effects would be largest because only the social norm motive would operate ( $\Delta_i^R \rightarrow 0$ ).

Since our data contain information on how many terms a politician has served in office, we can use incumbency as a proxy for the sensitivity of electoral prospects to behavior.<sup>20</sup> Specifically, long-term incumbents plausibly would be politicians whose re-election chances are not very sensitive to their behavior in the previous period. In contrast, voters should be more sensitive to behavior of incumbents who are yet unknown to them because they are in their first term in office.

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yardstick influences the voting behavior of voters, the social motive effect works through changing directly the behavior of incumbents. From a theoretical point of view, both channels are equally compelling; there is no *a priori* reason to favor one over the other. The data, however, are more consistent with norms than with yardstick. See Section 5 for a detailed discussion.

<sup>19</sup>We are not interested in strategic interactions, and thus want to think of each community as being very small. Incumbents and voters in the home community are taking (expected) outcomes and politicians' behavior in adjacent communities as given.

<sup>20</sup>We find new incumbents to be more sensitive to the electoral effects of timely audits (see Table 4).

**Observation 2.** *Short run spillover effects of audits in neighboring communities, operating through social norms, should be larger for long term incumbents than for first-time office holders.*

As we will see, we do find this effect in our empirical analysis. First term office holders react less to changes in corruption among neighbouring communities than politicians who have been in office longer.<sup>21</sup>

### 3.3 The Effect of Audits and Social Motives on Equilibrium Re-election Probabilities

In this subsection, we briefly analyze how audits in the home community affect the office holder's equilibrium chances of re-election. The relationship is of interest because we have information on re-election rates in our data. We already established in Section 3.1.2 that audits lower the chances of electoral success for corrupt incumbents and increase the chances of electoral success for pro-social incumbents (Observation 1), thereby mitigating equilibrium corruption. The overall effect on *ex ante* re-election probabilities is, however, indeterminate. Formally, using the fact that  $r(\theta) = 1$  if and only if  $\theta \leq \theta_i^*$ , the ex ante re-election probability of an office holder can be written as

$$E[P_i(r, \theta_i^*)] = G(\theta_i^*)P_i(r = 1, \theta_i^*) + (1 - G(\theta_i^*))P_i(r = 0, \theta_i^*) \quad (12)$$

And therefore, after some manipulations,

$$E[P_A(r, \theta_A^*)] - E[P_N(r, \theta_N^*)] = (E[P_A(r, \theta_N^*)] - E[P_N(r, \theta_N^*)]) + (E[P_A(r, \theta_A^*)] - E[P_A(r, \theta_N^*)]).$$

The first term  $E[P_A(r, \theta_N^*)] - E[P_N(r, \theta_N^*)]$  on the right-hand side represents the sorting effect of (home community) audits, while the second term  $E[P_A(r, \theta_A^*)] - E[P_A(r, \theta_N^*)]$  captures the disciplinary effect of audits. The signs of both effects are indeterminate. To see this consider first the sorting effect. On the one hand, politicians who remain corrupt even in the face of an audit are more likely to be ousted from office when voters can observe  $r$ . On the other hand, politicians that took the pro-social choice are now more likely to be re-elected because voters are more confident on their type. The disciplinary effect is equally ambiguous: while audits cause corruption to go down ( $\theta_A^* < \theta_N^*$ ), which generally improves ex-ante re-election rates, voter confidence on electing a pro-social type drops as more incumbents take the non-corrupt action, which tends to diminish electoral chances. It is therefore not possible to make any general statements on how audits affect observed re-election probabilities.

It remains to examine how audits elsewhere spill over into re-election chances at home. We already saw in Proposition 2 that the impact of more audits in adjacent communities on home community corruption,  $d\theta_i^*/d\alpha_A$ , depends on the sign of  $\Delta^{S'}$ . Note that there is no effect of audits elsewhere on re-election rates other than through their effect on the equilibrium level of corruption at home,  $\theta_i^*$ .<sup>22</sup> To see how this translates into re-election rates, we can take derivatives of (12) with respect to  $\theta_i^*$ :

$$\frac{dE[P_i(r, \theta_i^*)]}{d\theta_i^*} = -g(\theta_i^*)\Delta_i^R(\theta_i^*) - \psi [pG(\theta_i^*) \cdot \mathbb{I}_{i=N} + (1 - G(\theta_i^*))] \phi(\psi\delta_i^1) \frac{d\delta_i^1}{d\theta_i^*}, \quad (13)$$

where  $\mathbb{I}_{i=N}$  is the indicator function for  $i = N$ , i.e.,  $\mathbb{I}_{i=N} = 1$  if  $i = N$  and  $\mathbb{I}_{i=N} = 0$  if  $i = A$ . The first term of  $dE[P_i(r, \theta_i^*)]/d\theta_i^*$  is always negative, and represents the selection effect of elections. Higher levels of

<sup>21</sup>This is one of our findings that is difficult to reconcile with a yardstick competition model. Since spillover effects operates through re-election chances in such a model, one would expect first time office holders to react more strongly to changes in outcomes elsewhere.

<sup>22</sup>This is different in a yardstick competition model, where the the levels of corruption elsewhere directly influence the quality of information available to voters at home.

corruption reduce the expected probability of office holders winning re-election because a corrupt incumbent is re-elected less often than a pro-social incumbent. The second term captures an information effect, i.e. how average electoral fortunes change as voters take increased corruption into account when evaluating public good outcomes and interpret positive information as better evidence of pro-social behavior. The sign of this effect is always positive, regardless of whether (i) the community is not audited and the favorable information is conveyed by good public good outcomes or (ii) the community is audited and the favorable information is that no evidence of corrupt behavior was found. In either event, the fewer incumbents act pro-socially, the more precise is a positive signal and the more confident voters will be that favorable evidence signifies a pro-social type. Hence,  $\partial \delta_i^1 / \partial \theta_i^* < 0$ ,  $i = N, A$ .<sup>23</sup> Thus, the overall effect on re-election probabilities is again ambiguous. Irrespective of the sign of  $dE(P_i(r, \theta_i^*)) / d\theta_i^*$ , however, we will show that the (absolute value of the) effect on re-election probabilities is amplified by  $\psi$ : the more sensitive voters are to observed outcomes (or incumbent behavior), the larger the effect on re-election rates. Given our interpretation of incumbency as a proxy for  $\psi$ , the result implies that when compared to those of first-term incumbents, long-term incumbents' re-election chances are less affected by audits in adjacent communities.

Summarizing the discussion above and noting that corruption  $\theta_i^*$  in the home community depends among other things on average corruption in neighboring communities, we can state:

**Proposition 3.** *Although average corruption unambiguously drops as a result of a home audit, equilibrium re-election rates in audited communities may be higher or lower than in communities that are not audited,*

$$E[P_A(r, \theta_A^*)] - E[P_N(r, \theta_N^*)] \geq 0.$$

*That is, audits and the associated lower levels of corruption do not necessarily translate into elevated chances of electoral success for incumbents.*

*The spillover effects of audits elsewhere are ambiguous as well, and will depend on i) whether individual and social motives are substitutes or complements (the sign of  $\Delta^{S'}$ ), and (ii) whether the selection effect of elections dominates the information effect.*

Consider for example the case of norms as substitutes,  $\Delta^{S'} > 0$ , which is relevant in our data. In this situation, the external effect of audits in neighbouring communities is negative: corruption at home increases. This elevated corruption will lower equilibrium re-election rates if the likelihood that corrupt politicians are ousted is larger than the increase in the likelihood that 'lucky' (but corrupt) incumbents win re-election due to increased information reliability. Conversely, if the latter information effect dominates, re-election rates will rise ex ante *despite the fact that any given incumbent is more likely to be corrupt.*

How the magnitude of any short-run spillover effect will depend on incumbency is generally also ambiguous: on the one hand, long-term incumbents will react more strongly to any given drop in corruption in neighbouring community as a result of more audits; on the other hand, their re-election chances are less sensitive to their behaviour. For cases where voters are highly insensitive to behavior, the latter will dominate of course: as  $\psi \rightarrow 0$ , even a significant change in corruption as a result of norm spillover will not alter re-election rates. We will see that, indeed, the data show this pattern: although we observe stronger reactions to norm spillovers from long-term incumbents – consistent with our Observation 2 – their re-election probabilities vary less with the spillovers than those of first-term office holders, justifying again our usage of incumbency as a proxy of voter sensitivity.

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<sup>23</sup>See Appendix A for a formal derivation.

### 3.4 Empirical Implications

To summarize the empirical implications of the theoretical results above, consider first the impact of home audit on the outcome of home community:

1. Conditional on (non-)corrupt actions, the re-election probability should go (up) down as result of a home-community audit. (Observation 1)
2. In equilibrium, the effect of a home audit on the corrupt behavior of the home incumbent is negative. (Proposition 1)
3. In equilibrium, the effect of a home audit on the home re-election rate is indeterminate. (Proposition 3)

Turning to the implications of spillover effects through the social motive, we have:<sup>24</sup>

4. If the majority of politicians in the peer group is corrupt, then individual and social motives are substitutes and the spillover effect is negative: more audits elsewhere will elevate corruption at home. The converse is true if the majority of politicians are non-corrupt, at least in the short run. Moreover, the short-run spillover effect is stronger (weaker) for incumbents whose re-election chances are less (more) sensitive to their behavior/observable outcomes while in office. (Proposition 2)
5. The spillover effect of audits elsewhere on re-election rates at home is ambiguous and depends both on how individual and social motives interact, and on whether the selection effect dominates the information precision effect or vice versa. (Proposition 1)

## 4 Data and Methodology

### 4.1 Data

Our population is comprised of all 78 municipalities in Puerto Rico, over 7 election years from 1988 through 2012. Once we restrict the sample to observations with adequate audit data to measure instances of corruption, our final sample consists of 470 distinct observations of municipalities around each election. For each observation, we have information on municipal audit reports, election outcomes, mayor, and municipality characteristics.

#### Municipal Audit Data

Our central data sources are the municipal audit reports published by the OCPR between 1987 and 2014.<sup>25</sup> Municipalities are audited following a pre-specified order. According to the OCPR's constitutive legislation, municipal governments are to be audited every other fiscal year. However, due to the OCPR's resource constraints, in practice these audits generally take place three to six years apart. Given that the data span almost three decades, all seventy-eight municipalities were audited multiple times during our period of interest. The data capture information for roughly 86 percent of all municipalities-by-electoral term units of analysis.

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<sup>24</sup>These spillover results hold regardless of whether the home incumbent is corrupt or not, that is, even if as a result of the change in voter's re-election decisions, the incumbent changes his equilibrium behaviour

<sup>25</sup>The structure of the audits and the audit reports are only consistent starting from the mid-1980s onwards. We therefore restrict our sample to jurisdictions with audit reports starting in 1987.



Each report contains a list of findings identified as the misuse of public funds, a detailed description of each finding, the individuals involved (if identifiable), and the reason(s) why it is considered a finding. Officially, the OCPR does not classify findings as instances of corruption. We undertook a comprehensive coding process to specify whether a finding constitutes an act of corruption, defined as “an act by any municipal employee that led to a *personal* financial or political benefit. See Bobonis et al. (2016) for a more detailed description of this coding process. Corrupt acts identified through the audits are typically based on a combination of procurement fraud, 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.

Following the existing literature and earlier work (Ferraz and Finan, 2008, 2011; Bobonis et al., 2016), we combine these into a single measure by summing up the number of times each one of these irregularities appears. To account for the fact that larger, more complex municipalities often require multiple reports in a given auditing period, the total number of corrupt violations is normalized by dividing the count of violations by the number of reports issued. We also construct a separate index of violations per report attributed exclusively to the municipal government leadership: the mayor or vice-mayor. These are the two main measures of corruption (in a given municipality and auditing period) we use for our empirical analysis.

We present descriptive statistics of these audit outcome variables in Table 1, Panel A. Sixty-four percent of audit reports contain at least one corrupt violation. Thus, the majority of municipalities experience corruption. We observe on average 1.18 corrupt violations per report; however, there is substantial variation (the standard deviation is 1.64). Approximately 38 percent of these findings (0.45 violations) are attributed directly to the mayor or vice-mayor. Panel B reports other relevant characteristics, such as the number of reports from the audit, the time span of the audits, and our measure of adjacent timely audit exposure. The reports span an auditing period of 4.76 years on average.

### Other Data Sources

To connect audits to electoral outcomes, we obtain election data from the P.R. State Electoral Commission (CEE) containing the results of municipal and statewide ballots for the 1988-2012 elections. With this data, we observe whether the incumbent mayor runs for re-election in the general election, whether she is re-elected, the vote share and win margin for the election, her political party affiliation, whether she is in the opposition to the party in power at the state level, and her number of terms in office. We obtain basic socioeconomic municipality-level characteristics from the 1990 U.S. Census of Population for Puerto Rico. We use measures of the proportion of adults (ages 25 and over) with schooling attainment levels of high school education or more, with a college education or more, as well as the municipality’s household median income and poverty ratio for 1989. Finally, we obtain information on municipality-level annual unemployment rates from the U.S. Department of Labor Local Area Unemployment Statistics.

Table 2 contains descriptive statistics for these electoral and socioeconomic characteristics. From the electoral outcomes in Panel A, we note that although 78 percent of incumbent mayors run for re-election, both incumbent mayor re-election rates (conditional on running) and overall party success rates are quite low, at 28 and 34 percent, respectively. Panel B reports other characteristics of incumbent mayors. Incumbents are split roughly equally between the two major parties, PNP and PPD. 36 percent of mayors are affiliated with the party in opposition to the party of the governor in office at the time, and approximately 44 percent are in opposition to the party of the governor who appointed the current Comptroller. Incumbent mayors remain in office for just over one term, on average, but there is substantial variation: 41 percent of incumbents have been in office for two or more terms and 20 percent for three or more terms.<sup>26</sup> Moreover, the average

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<sup>26</sup>Several mayors in the sample (e.g., in the municipalities of Bayamón, Carolina, and Manatí) have been in office for four or

incumbent mayor’s winning margin in the previous election is 3 percentage points. We capture 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 considerable proportion of seats (57 percent) have a strong party incumbency advantage. In Panel C, we present summary statistics for municipality characteristics based on the population census and municipal unemployment rates during the first two years of the incumbent’s term.

## 4.2 Methodology

### Research Design

We exploit the disciplining effects induced by the timing of municipal audits and their geographic location in the network of municipalities to help us establish the direct and spillover effects of the audits on corruption. Following Bobonis et al. (2016), we define *timely* audits as audits that are disseminated in the two-year period before the relevant election. Conversely, *untimely* audits are those disseminated in the two-year period following the election. Because municipalities are audited in a pre-established and fixed order, the timing of audits is exogenous and their assignment into timely and untimely groups is plausibly quasi-random.<sup>27</sup> We expect audit reports released in the period leading up to an election to be more likely to inform on the incumbent mayor’s activities than those reports published shortly after an election.<sup>28</sup> Therefore, timely versus untimely audits measure variation in anticipated monitoring that enables us to measure its disciplining effects on each municipality’s level of corruption.

To illustrate the incentive effects of audits at home, Figure 3 plots the average number of corrupt violations (per report) attributed to the mayor or vice-mayor during each of the previous two electoral terms, and in the following electoral term, for municipalities in election year  $t$ . We show the trends separately for municipalities with timely (red line) and untimely (green line) audits. There are no discernible differences in the levels of reported corruption across these two groups of municipalities in earlier years—the mean number of violations per report revolves around 0.56—and the differences are statistically indistinguishable from zero. In contrast, around election year  $t$  there is a stark difference of 0.56 ( $= 0.80 - 0.24$ ) violations per report. In following year  $t + 1$  the difference decreases again. In summary, timely audits strongly mitigate corruption in the short-run, but the effects seem to be short-lived.<sup>29</sup>

We perform balance tests of municipalities with timely audit assignment for a set of predetermined characteristics (adjusting for municipality and election fixed effects) to further examine the validity of the design. Municipalities under timely and untimely audits do not differ significantly in a large set of incumbent

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more terms.

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

<sup>28</sup>This assumption is reasonable for various reasons in this context. First, the information from untimely audits may be of less relevance in future elections due to the low overall re-election rate of mayors (34 percent). Second, the media may invest more resources in disseminating audit results or the information may be more salient to voters because the information contained in audits may be of greater immediate interest to voters when an election is looming. Relatedly, even if the information from untimely audits does reach voters, they may not use it during the subsequent election because of recency bias – the tendency for voters to place more weight on recent information (Berry and Howell, 2007; Lewis-Beck and Stegmaier, 2000). Our partition of mayors’ terms into two-year windows is also supported by patterns in incumbent’s corrupt behavior. For a more detailed empirical examination of this partitioning and further discussion, see Bobonis et al. (2016).

<sup>29</sup>In ancillary analyses (see Appendix Table A2) we show that the short-run effects are significantly stronger in municipalities in which local elections reflect a historical alternation of parties – there is no strong party incumbency advantage – a finding that supports the view that electoral accountability is an important explanatory mechanism.

mayor, political, and socio-economic characteristics (see Table 2, Panel B).<sup>30</sup>

In order to measure cross-municipal external effects of timely audits, we exploit variation in the share of neighboring municipalities that are subject to a timely audit during an incumbent’s term in office—adjacent timely audit exposure. Since timely audits elsewhere reduce contemporaneous levels of corruptions in their communities, timely audits in neighboring municipalities result in an *exogenous decrease in neighboring corruption levels from the point of view of the home municipality*. This exogenous reduction in neighboring corruption levels, observed by the home incumbent, generates the shock to corruption levels in neighboring jurisdictions that we observe and that we aim to explain in our formal model.

In column (5) of Tables 1 and 2, we verify that the share of timely audits in neighboring jurisdictions is orthogonal to own municipality characteristics. We find no significant relationships with audit characteristics—including whether an own municipality audit is timely or untimely – as well as with incumbent mayor and municipality-level political characteristics. With respect to socio-economic characteristics, the spillover exposure measure has a small correlation with greater population education levels, greater median incomes, and lower poverty rate at baseline; these differences should lead to minimal or no bias in our spillover effects estimates.<sup>32</sup> In overall terms, these diagnostics support the identifying assumptions for the empirical analysis.

### Estimation

We estimate the average direct and external effects of the timely dissemination of audits on short-term levels of municipal government corruption using the following specification:

$$c_{m,t} = \beta \mathbf{A}_{m,t}^{(-m)} + \alpha A_{m,t} + \lambda X_{m,t} + \gamma_t + \rho_m + \epsilon_{m,t} \quad (14)$$

where  $c_{m,t}$  denotes the number of corrupt violations per report in municipality  $m$  around election year  $t$ .  $\mathbf{A}_{m,t}^{(-m)}$  measures the share of municipalities neighboring municipality  $m$  that receive a timely audit preceding election year  $t$ , and  $A_{m,t}$  indicates whether or not the audit within municipality  $m$  was timely.  $X_{m,t}$  is a vector of municipality and mayor characteristics that influence the municipality’s level of corruption.<sup>33</sup> The terms  $\rho_m$  and  $\gamma_t$  represent municipality and election fixed effects respectively, and  $\epsilon_{m,t}$  denotes unobserved characteristics that determine our measure of corruption at time  $t$ . Under the assumption that  $\{A_{m,t}, \mathbf{A}_{m,t}^{(-m)}, X_{m,t}\}$  are strictly exogenous, the coefficients  $\beta$  and  $\alpha$  are consistent estimates of the average external and direct effects, respectively, of timely audits on contemporaneous levels of reported corruption. We cluster standard errors at the municipality level.

Recall from our theoretical model that the direct effect of a timely audit in the home municipality should be negative:  $\alpha < 0$ . Moreover, the external effect of timely audits in neighboring municipalities—now represented by  $\beta$ —depends on the prevailing social norm: whether the majority of politicians in the

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<sup>30</sup>Notably, there is balance in the proportion of municipalities whose incumbent is member of the party in opposition to the governor or of the party that appointed the current comptroller (rows 2-3); in whether the incumbent is in the first term (row 4); in the degree of competition in the previous election as measured by the incumbent mayor’s win margin in the previous election (row 6); and in the lack of party turnover at the mayoral level (party incumbency advantage, row 7). There is also balance in municipal baseline socio-economic characteristics (Panel C), such as the population’s educational attainment levels, household median income and poverty rates, and in the municipalities’ unemployment rate in the first and second year of the incumbent mayor’s term.<sup>31</sup> This evidence supports the assumption that the timing of the audits can be considered exogenous and plausibly uncorrelated with unobserved determinants of municipal corruption levels.

<sup>32</sup>The correlations are small in magnitude in terms of normalized differences Imbens and Rubin (2015) and the variables are very weakly correlated with *lower* corruption levels (which would imply downward bias in absolute magnitude of the spillover effects). Moreover, our specifications include municipal fixed effects which control for such fixed differences in municipality characteristics.

<sup>33</sup>We use as controls the number of municipality government reports, the number of municipal public corporation or consortium reports; indicators for the mayor’s membership in the PNP, for the incumbent being in the opposition party to the state-level executive government, and for the incumbent being in the opposition party to the governor who appointed the Comptroller; the vote share for the incumbent in the previous election; and whether the incumbent was in their first electoral term.

incumbent’s peer group is corrupt or not. Based on the average levels of municipal government corruption observed from our audit data (Table 1, Panel A), politicians in the majority of municipalities do engage in corrupt behavior. According to our social norms model, when the majority is corrupt, a decrease in average peer corruption (as a result of timely audits in neighboring municipalities) would imply  $\beta > 0$  due to the overall decreased social benefit of ethical behavior by the incumbent.

In addition to our baseline specification, we allow for external effects to differ by (a) whether the home municipality faces a timely audit or not, and (b) whether the incumbent mayor is in their first electoral term (new incumbent) or is a more experienced politician. To do this, we augment the model (14) respectively by interacting the spillover exposure measure  $A_{m,t}^{(-m)}$  with the own municipality timely audit indicator ( $A_{m,t}$ ) or with  $N_{m,t}$ , where the latter is an indicator for whether the mayor is a new incumbent and is in the set of controls  $X_{m,t}$ . Following the predictions discussed above, we expect spillovers to be weaker if the home municipality experiences a timely audit itself. Furthermore, because new incumbents face more competitive elections, they should be more responsive to electoral incentives relative to reputational (social) motives. Thus, in both cases we expect the coefficient on such interaction terms,  $\beta_2$ , to be negative.

Lastly, the identification strategy also allows us to examine the audit program’s direct and external effects on mayoral incumbents’ re-election rates and corruption in the subsequent electoral term. Specifically, we estimate model (14) using  $e_{m,t}$ , an indicator for the re-election of the incumbent mayor in election year  $t$ , and  $c_{m,t+1}$ , the number of corrupt violations per report in municipality  $m$  around election year  $t + 1$ , as the dependent variables.<sup>34</sup>

## 5 Results

### 5.1 Direct and Spillover Effects of Timely Audits on Home Corruption

Table 3 presents the main empirical results. We separate all results according to whether the dependent variable is corrupt violations of the mayor and vice-mayor only (left-hand panel) or of all elected municipal officials (right-hand panel). The baseline specification in columns (1) and (4) report the direct effects as well as the spillover effects from timely audits in adjacent municipalities on the number of corrupt violations in the home municipality. The estimates of the direct effects  $\alpha_1$  document a systematic reduction in the number of corrupt violations in the municipality as a result of timely audits. For mayors and vice-mayors, the point estimate is 0.56, which represents a 70 percent drop compared to the 0.8 violations per audit report published after the election.<sup>35</sup> The estimated direct effect for all elected officials implies 1.34 fewer reported corrupt violations in municipalities with timely audits compared to those whose audit reports were published after the election. The corresponding reduction in reported corruption is 67 percent, suggesting little difference in how mayors and other municipality officials respond to the audits.

Our estimates of the spillover effects of timely audits in neighbouring jurisdictions are also sizeable. The point estimate of  $\beta_1$  for mayors and vice-mayors is 0.4, suggesting that their reported corruption would be 50 percent higher if all adjacent neighbours underwent a timely audit as compared to a situation where none

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<sup>34</sup>When the dependent variable is  $c_{m,t+1}$ , we also control for whether there was a timely audit within the municipality in the subsequent electoral term, the share of adjacent municipalities with timely audits in the next election, as well as the relevant interactions with timely audit in the own municipality (in the next term) and new incumbent.

<sup>35</sup>To check whether the effects are due to the mayor’s electoral accountability, we estimate heterogeneous effects of audits by the competitiveness of the mayoral seats, using our summary measure of party incumbency advantage; see estimates in Appendix Table A.3 As expected, 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 80-95 percent range (in proportional terms), whereas the estimated effects among municipalities with a significant party incumbency advantage are in the 48-57 percent range.

did. To further illustrate the magnitude of the spillovers, we can compare municipalities in which the median share of neighboring jurisdictions have a timely audit (approximately 0.67) to those where no neighboring municipalities do so. We report these estimates in the bottom panel of the table. The point estimate of the spillover effect implies that exposure to timely audits in the median share of adjacent municipalities leads to an increase in reported corruption levels by 0.27 violations ( $= 0.40 \times 0.67$ ) in the home municipality, or 34 percent relative to the control group mean. This estimated spillover is large and precisely estimated when we focus on the violations attributed to the municipal government leadership.<sup>36</sup> For all officials, the point estimates of the average spillover effects imply a weaker spillover effect, which moreover is not statistically significant (column 4).

The specifications in columns (2) and (5) examine heterogeneous spillover effects for home municipalities with untimely versus timely audits. We find that the spillover increase in home corruption is mitigated by timely audits at home: the point estimate is negative for both mayor/vice-major and all elected officials. These results are consistent with our theory of spillover effects through social motives, because the incentive effects of home audits work through home elections and not through reputational concerns and are thus separated from the former. However, spillover effects among municipalities experiencing untimely audits and among those with experienced incumbents are large. Interestingly, in the latter group the estimated combined effect ( $\beta_1 + \beta_2$ ) is zero, suggesting that the negative spillover is eradicated by home audits. This also explains why we do not necessarily find a significant effect if we do not allow the slope to vary depending on whether or not the home municipality is audited in a timely fashion: it appears that the spillover effect is present — 0.84 or 42 percent higher than baseline — among all officials as well, provided there is no timely home audit.

Columns (3) and (6) document how the spillover effects vary by level of incumbency of the office holder. Note first that estimated corruption among new office holders overall is higher than among experienced incumbents; our estimate of  $\lambda$  is positive and statistically different from zero those specifications. Next, consider the interaction term for  $\beta_3$  which estimates the spillover for municipalities with first-term incumbent mayors vs. those with long-term incumbents. For mayors and vice-mayors, the spillover effect point estimates suggest an increase of 0.40 corrupt violations, or 50 percent relative to the control group mean, among municipalities with more experienced incumbents (bottom panel). Although the differences are not statistically significant, the point estimates imply smaller increases of 0.09 corrupt violations (11 percent) among municipalities with new incumbents; the latter estimates are statistically indistinguishable from zero. Similar results hold for violations of all officials. Here, the point estimate is an increase of 0.46 corrupt violations (23 percent) relative to the control group mean among municipalities with more experienced incumbents. In contrast, we find evidence of no spillover effects in municipalities with new incumbents. This observed heterogeneity is supports the theoretical predictions of the model, which imply a relatively greater weight on social motives for long term incumbents who face an electorate that is less sensitive to their behavior in the current legislative period (including any information on this behavior revealed through audits).

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<sup>36</sup>In an ancillary analysis, we evaluate whether the linear structure of the spillover effects represents a good approximation. In Appendix Figure A.1, we show a bin scatter-plot of the data (residuals adjusting for municipality and electoral term fixed effects) showing the relationship between exposure to timely audits in neighboring jurisdictions and the number of violations by the political leadership in the home municipality. The semi-parametric relationship is appropriately summarized by a linear model, indicating that it is not variation in the extensive margin, any neighboring municipality having a timely audit, that is driving the results.

## 5.2 Direct and Spillover Effects of Timely Audits on Home Re-election Rates and Long Term Corruption

Turning to short term electoral accountability (incumbent mayors' re-election rates) and longer term corruption levels, Table 4 reports estimates from a linear probability model estimates of equation (14) using  $e_{m,t}$ , an indicator for the re-election of the incumbent mayor in election year  $t$ , as the dependent variable. We keep our three specifications from Table 3. Throughout all specifications, the point estimate on the effect of timely audits on incumbent mayors' re-election rates is very small and not statistically different from zero (column 1). Recalling that officials adjust their behavior as in response to timely audits, this result is consistent with the theory. Note, however, that new incumbents have significantly lower re-election rates and that the effect is quite sizeable. Again, this is not surprising. After all, experienced mayors have already been re-elected once: being positively selected, they have safer seats. In addition, we found earlier that new incumbents tend to be more corrupt, and are thus more likely to be punished at the ballot box.

Our point estimates of the spillover effects indicate that less corruption elsewhere is associated with an increase in the incumbent mayors' probability of re-election, but as column (3) shows, this positive relation is primarily driven by new incumbents who we found *not* to be increasing their corrupt behavior from spillovers. Median exposure to adjacent timely audits leads a substantial rise in the re-election probability of the new incumbents by 14.4 percentage points, a 57 percent increase relative to the control group mean. It is important to note that this finding is not inconsistent with the theory: specifically, we know from Table 3 that audits elsewhere increase corruption among experienced incumbents. This should decrease their electoral chances, *ceteris paribus*. At the same time, though, the electorate is now more confident about re-electing mayors for whom their posterior believe indicates a pro-social type and those incumbents will be re-elected more often. *Ex ante*, we can therefore not make unambiguous statements about how re-election rates are affected in general; however, if we have a subgroup of mayors (new incumbents) which we suspect to react less than the average incumbent to the spillover, we would expect their re-election rates to be higher than average due to the latter effect. The finding also re-enforces the idea that the length of incumbency serves as a stratifying measure for politicians' sensitivity to re-election concerns: long-term office holders tend to be safer in office and we expect their electoral chances to be less sensitive to behavioral changes triggered by audits at home or elsewhere, whereas the opposite to be the case for new incumbents.

Our estimates of the effects of timely audits on subsequent corrupt violations are reported in columns (4)–(6). Although the sign of the point estimates for both the direct and external effects across all specifications are generally similar than in short-term analysis, they are considerably smaller and are imprecisely estimated.<sup>37</sup> This finding is also consistent with theory: the forces of electoral accountability and political selection, in conjunction with the short-run norms motives, lead to muted spillover effects of timely audits on political corruption.

## 6 Alternative Mechanisms

In summary, our empirical work above documents a consistent positive direct effect of timely audits on contemporaneous reports of corrupt violations, but also provides evidence of a robust and negative spillover effect of exposure to timely audits in adjacent municipalities – suggesting that efforts to curb corruption not as effective as they would be in a world without spatial spillovers. All of those results hold only in the

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<sup>37</sup>Bobonis et al. (2016) already show for a smaller sample that the direct effect of timely audits is short lived. We confirm this and show that the spillover effects exhibit similar fading out patterns. Analogous results hold when we examine all corrupt violations in the municipality, not just political corruption by the leadership.

short-term. At the same time, while experienced incumbents face no electoral penalty for their increased corruption due to the spillover, new incumbents' electoral prospects appear to rise as a result of more audits and correspondingly lower corruption in adjacent municipalities. The central predictions of our theory fit those findings well, and our theoretical framework thus provides a plausible explanation for the spatial negative spillover effects of audits in neighbouring communities: lower corruption in the peer group decreases the incentives to be pro-social for office holders who worry about their reputation, because obtaining from corruption becomes relatively less honourable. Notably, the spillover effect in the model is more pronounced the less sensitive the electorate reacts to information related to office holder behavior because the trade-off between electoral motives and social motives endogenously shifts toward the latter in this case. Since politicians who have been re-elected already once are naturally safer in office, this theoretical prediction also bears out in the data, where we find the negative spillovers are primarily concentrated among long-term incumbents.

In this section, we argue that three prominent alternative explanations of the patterns we observe in the data, namely yardstick competition, the displacement of corrupt activities, and informational channels are unlikely to drive our empirical results. We also seek to rule out models of politician learning and behavioral models of salience.

*Yardstick Competition* Yardstick competition models also imply that politicians' behavior is spatially linked, which may provide an alternative way to think about the spillovers we identify. The main idea of yardstick competition is that the electorate is able to observe outcomes or policies from neighbouring jurisdictions and can use this information to better hold their home office holders accountable. Translated to our context with corruption, consider a the simplest possible yardstick scenario where voters observe public good levels in neighbouring communities and can use this information – because the underlying determinants of public good levels are correlated across communities (e.g. economic growth) – in their own assessment of how likely it is that a high (respectively, low) level of public good provision at home indicates prosocial (respectively, anti-social) behavior of the incumbent office holder. There are two key properties of this model that do not fit our data. First, we know that audits in neighboring communities will decrease corruption there, which will tend to increase the level of public goods provided in those communities. Voters observing better outcomes elsewhere would rationally update their beliefs and be *less lenient* in the home community, assuming public good levels are positively correlated. This would put additional pressure on office holders to curb corrupt behavior and thus lead to a decrease in corruption. Put differently, in this situation audits elsewhere would re-enforce pro-social behavior at home rather than dampen it. Second, and equally important, the channel through which yardstick competition – an other models that have voters as the central mechanism of spatial spillovers – works is the electorate. This implies that the spillover effects should be more pronounced for new incumbents, who are plausibly facing a more sensitive electorate than long-term incumbents. Again, the data here suggest the opposite, indicating that electoral channels are unlikely to be the key to understand the patterns we find in the data.

*Displacement of Corrupt Activities.* Another possible and simple explanation for our results could lie in the spacial displacement of corrupt activities.<sup>38</sup> Corruption and patronage networks in this context are characterized by a highly polarized political equilibrium commonly found in highly divided societies (Magaloni, 2006; Stokes et al., 2013; Padró-i-Miquel, 2007). These are evident in intense allegiances to the territory's traditional political parties, and to a distribution of benefits to constituents largely based on such allegiances (Pantojas-García, 2015; Atilés et al., 2022; Bobonis et al., 2022). It is plausible that corruption

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<sup>38</sup>Such an explanation is common in the context of studying the effects of policing and monitoring on crime. See, for instance, Dell (2015) and Blattman et al. (2021).

networks could shift such activities across municipal borders, particularly in those under the control of the same political party, to avoid detection in those experiencing timely audits.

To evaluate the relevance of this alternative explanation, we examine heterogeneity in spillovers within and across political party networks. If this mechanism were to play a prominent role, we should observe the spillover effects to be concentrated solely among incumbents belonging to the same political party network as those in neighboring jurisdictions. In contrast, if spillovers are also present across political party lines, the evidence would be supportive of the norms mechanism.<sup>39</sup> We estimate an empirical model that allows for the spillover effects of timely audits to vary by own-party versus opposing-party networks:

$$c_{m,t} = \beta_1 \mathbf{A}_{m,t}^{p(-m)} + \beta_2 \mathbf{A}_{m,t}^{o(-m)} + \alpha A_{m,t} + \lambda X_{m,t} + \gamma_t + \rho_m + \epsilon_{m,t} \quad (15)$$

where  $\mathbf{A}_{m,t}^{p(-m)}$  measures the share of adjacent party-aligned municipalities with a timely audit, and likewise with  $\mathbf{A}_{m,t}^{o(-m)}$  for municipalities where the incumbent is of the opposing party. All other variables are defined as in model 14. As usual, we also estimate models allowing for heterogeneity in the spillover effects by the timeliness of the home audit and by the experience of the home incumbent.

We report these results in Table 5. Our findings of positive spillover effects generally hold within and across political party networks. The point estimates imply that the median exposure to adjacent timely audits leads to a 21 percent increase in corruption among same party networks and 11 percent among municipalities crossing political party lines (column 1). We cannot reject the null hypothesis that such spillover effects are equivalent (p-value = 0.31). Among municipalities experiencing untimely audits, the point estimates suggest stronger spillovers within political party networks than across these (32 vs. 11 percent), but we cannot reject that these are systematically different (p-value = 0.27) (column 2). Finally, in regards to heterogeneity across experienced home incumbents, the group among whom we expect social norms to play a relatively stronger role, we find robust evidence that the spillover effects are quantitatively similar within and across political party networks. The point estimates indicate similarly and precisely estimated robust spillover effects (26 vs. 20 percent); we fail to reject the null of no heterogeneity (p-value = 0.64) (column 3).<sup>40</sup> In sum, we do not observe strong evidence of heterogeneity according to party networks, supporting the hypothesis that displacement is not the main driver of the spillover effects.

*More information about corrupt opportunities as a result of audits elsewhere.* One can imagine that the spillover rise in home corruption is partially reflective of audits elsewhere revealing more information regarding corrupt practices in reference localities, and not necessarily because the reference group has systematically lower corruption levels. To investigate and distinguish this mechanism empirically, we pursue an empirical strategy whereby different incumbents observe exogenously high or low levels of corruption in neighboring municipalities' timely audits.<sup>41</sup> To do so, we leverage variation in the degree of electoral competition, the historical alternation of parties, in office in neighboring jurisdictions to generate such heterogeneity in corruption levels. We find that the negative externalities of timely audits are strongest when adjacent municipalities experience party alternation; that is, when timely audits reveal significantly lower levels of corruption. In contrast, the point estimates of the external effects from adjacent municipalities with party incumbency advantage are remarkably small and statistically indistinguishable from zero; this

<sup>39</sup>One caveat of such a test is that the social norms could be heterogeneous across political party networks, and evidence of heterogeneity is also consistent with this more nuanced interpretation of the norms model.

<sup>40</sup>In Appendix Table A.4, we explore whether the spillover effects differ across the two main parties, PPD and PNP. We do not find evidence that there is heterogeneity across these.

<sup>41</sup>We cannot simply use the observed level of corruption from timely audits in neighboring jurisdictions, as it is plausible that the unobserved determinants of corrupt activities are correlated across such jurisdictions. This unobserved heterogeneity would lead to bias in our estimates of such spillover effects.



suggests that no externalities are at play when more timely information regarding higher levels of corruption is revealed. We take this pattern of external effects as evidence that the level of corruption, and not just the additional information, is the driving force of the negative spillover effects. See Appendix B.1 for details.

*Politician Learning and Salience.* Lastly, one could imagine that the spillover effects reflect office holders' learning about the audit technology and its electoral or judicial consequences. From the observation of timely audits in nearby jurisdictions, incumbents home could update their beliefs regarding either the probability of having a timely audit, the probability of detection of malfeasance, or the possible electoral or judicial repercussions of such audits. Relatedly, neighboring audits may make the program more salient to incumbent politicians.

In an influential study examining the introduction of the municipal anti-corruption audits program in Brazil, Avis et al. (2018) show evidence consistent with politicians updating their beliefs on possible consequences of own audits and those in neighboring jurisdictions, with the result that politicians engaged in *less* corruption. Our main finding on the sign of spillovers contradict this mechanism: because neighboring audits did not decrease political corruption, the evidence we find does not support the positive updating of beliefs regarding detection or on electoral or judicial accountability. Moreover, one would expect any belief updating due to learning to be stronger among new incumbents vis-a-vis more experienced ones, as the latter may have learned about such processes during previous incumbencies. The concentration of negative spillovers among experienced incumbents also counters such mechanisms.

Another possibility is that politicians update their beliefs regarding the probability of experiencing a timely audit *downwards*. We have already found there to be no discernible relationship between the probability of timely audits in neighboring jurisdictions and home timely audits (see Appendix Table A.5), but another possibility is that politicians perceive their own audit to be less likely to be scrutinizing if a lot of their peers are being audited as well. Using the number of separate audit reports issued for each municipal audit as an indicator of intensity, Table A.3 in the Appendix documents that there is no positive or negative correlation between adjacent audit exposure and home audit intensity.

Finally, regarding the plausibility of a behavioral mechanism of salience: the odds of having a neighboring municipality timely audited among Puerto Rico's 78 municipalities are very large. Thus, neighboring timely audits are not rare occurrences, making it also less plausible that behavioral arguments of audit salience apply in our setting. Overall, our findings do not point to the channel of politician learning or salience as drivers of the observed spillover effects.

## 7 Conclusion

In this paper, we argue that social norms can play a key role in shaping rent extraction and other corrupt activities. We develop a formal model to allow for social motives in the form of reputations concerns which are influenced by prevailing norms and study the interaction of those motives with anti-corruption efforts through formal rules and institutions, specifically, the disciplinary incentives public officials are subjected to through elections and audits.

Our empirical analysis is conducted in the context of Puerto Rico's municipal anti-corruption audit program. The fact that audits are carried out in pre-determined and fixed order provides the foundation of our empirical strategy, which exploits the exogeneity of timely audits. Our results on the direct effect on corruption are optimistic and confirm earlier findings: timely audits succeed in reducing observed corrupt violations, at least in the short run. This reduction in corruption within the municipality is strongest in municipalities with higher (lower) levels of electoral competition (incumbency advantage).

Our results on the external effect of corruption provide reasons for concern, however: decreased corruption in adjacent municipalities—brought about by timely audits—*increase* levels of reported corruption in the

home municipality. The spillover effect is worse in municipalities with more experienced incumbent mayors, where exposure to timely audits in the median number of timely audits in adjacent municipalities (two-thirds) results in a 23.5 percent additional increase in reported corruption. Moreover, unlike the direct effect of the audits strengthening electoral accountability (Bobonis et al. (2016), Ferraz and Finan (2008)) this increase in corruption from the spillover effect is not punished by voters at the ballot box.

We show that the patterns we uncover in the data are consistent with norms as a driver of behavior, where spillovers originate from an inter-dependency of incentives through formal institutions (elections and audits) and informal institutions (social norms). In our context, these external effects are spatial and can be explained by norms provided the relevant peer group for municipal officials are their neighbouring counterparts. Through several robustness checks, we are able to rule out alternative explanations of yardstick competition, displacement through party connections, and politician learning, as the main mechanism at play. A model with social norms then implies that the prevalence of corruption is key to nature of the externality we observe. Specifically, in societies with high levels of corruption such as Puerto Rico, when corruption falls in the peer group, the honour of pro-social actions falls by more than the associated rise in stigma of corrupt action: pro-social behavior of others and oneself are substitutes.

We would thus expect anti-corruption efforts to have *negative* spillover effects and, as a consequence, the overall success rate of any initiative may well be dampened. Conversely, in societies with low levels of corruption such as the United States, a setting with very similar formal institutions to Puerto Rico, we would expect pro-social behaviors to reinforce each other and, thus, anti-corruption efforts to have more desirable spillover effects. This distinction is clearly relevant to any policy aiming at a *sustainable* check on government corruption. Taking our study as providing sufficient evidence of the importance of the role of spillover effects, our argument would imply, for example, that slow and gradual changes to the social norm, recommended by Acemoglu and Jackson (2017), may not be succeed in the long run. Indeed, perhaps a sweeping jolt aimed at drastically curbing the social norm of corruption—enough to bring society out of a "corruption trap"—may be better positioned to succeed in bringing about a society where honorable behavior can spread and reinforce itself to become the social norm.

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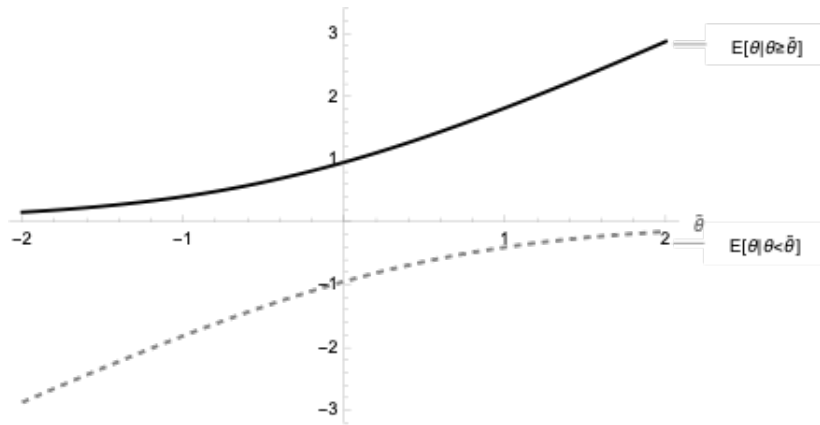
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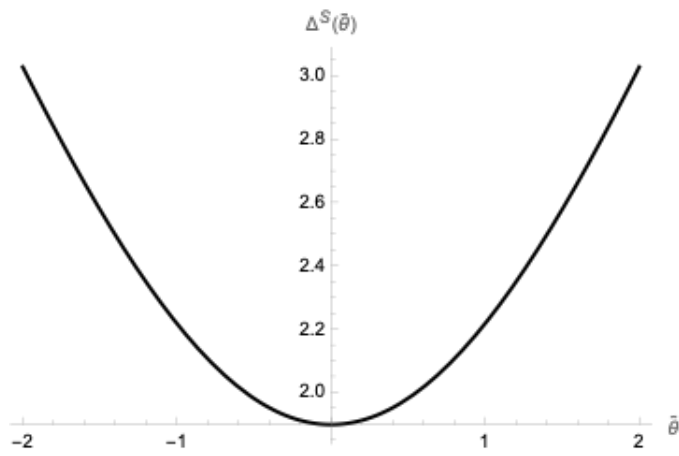
**Figure 1:** Illustration of Social Norm



**Figure 2:** Illustration of Social Motives



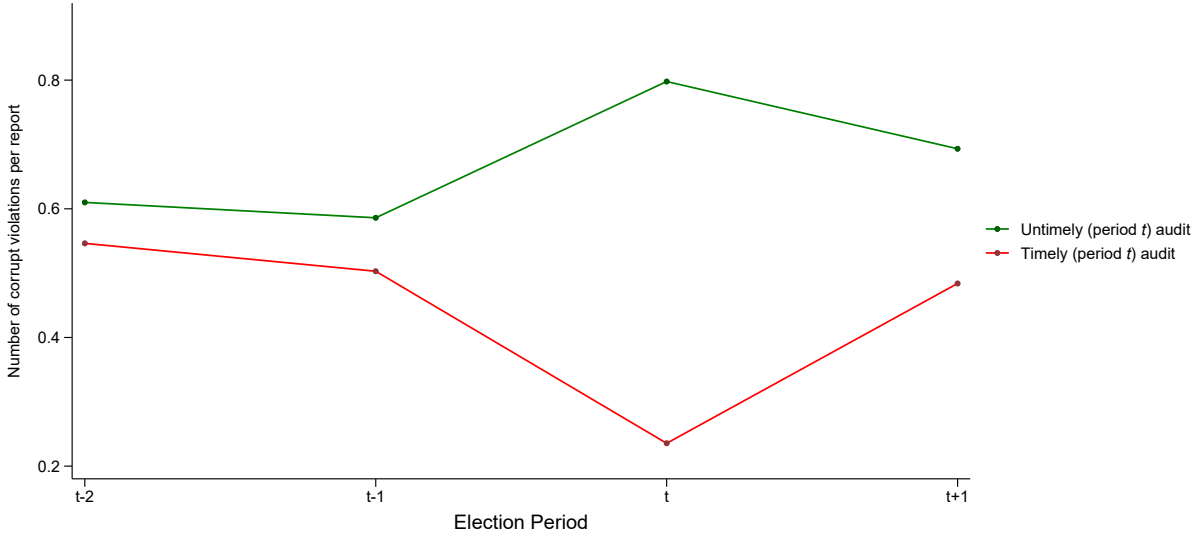
(a) Honor and Stigma Effects and Their Relation to  $\tilde{\theta}$



(b) Gain in Social Reputation



**Figure 3:** Number of Corrupt Violations across Time by Timeliness of Audit in Election ( $t$ )



*Note:* The figure shows the unadjusted relationship between the number of corrupt violations per report in each audit, for municipalities with timely and untimely audits around election time ( $t$ ).

**Table 1: Characteristics of the Audit Reports**

	All (1)	Timely audit (2)	Untimely audit (3)	Difference (adjusted) (4)	Share of adj. municipalities w/ timely audit (5)	Observations (6)
<i>Panel A: Audit outcomes</i>						
Any corrupt violation	0.64 [0.48]	0.57 [0.50]	0.75 [0.44]	-0.15*** (0.06)	-0.07 (0.11)	470
Number of all corrupt violations per report	1.18 [1.64]	0.66 [0.89]	2.00 [2.14]	-1.27*** (0.17)	0.40 (0.36)	470
Number of violations by mayor/ vice-mayor	0.45 [0.89]	0.24 [0.50]	0.80 [1.21]	-0.54*** (0.10)	0.42** (0.19)	470
<i>Panel B: Other audit characteristics</i>						
Number of audit reports	1.97 [1.23]	2.28 [1.37]	1.49 [0.74]	0.69*** (0.12)	0.02 (0.29)	470
Start of audit period in reports (years from election)	5.98 [2.34]	6.50 [2.35]	5.16 [2.07]	2.17*** (0.18)	-0.00 (0.42)	470
End of audit period in reports (years from election)	1.22 [1.43]	1.47 [1.41]	0.83 [1.38]	0.64*** (0.13)	-0.32 (0.36)	470
Time span of audited period (years)	4.76 [2.39]	5.04 [2.48]	4.33 [2.18]	1.52*** (0.19)	0.32 (0.50)	470
Share of adjacent municipalities with timely audit	0.60 [0.29]	0.63 [0.28]	0.56 [0.30]	-0.03 (0.03)	1.00 (0.00)	470
Observations	470	288	182	470	470	

*Note:* Standard deviations of variables in each category are reported in brackets. Differences in the means of the variables between municipalities with timely and untimely audits (column 4) and the correlation with the share of adjacent municipalities with timely audits (column 5) are estimated via ordinary least squares (OLS) regression models, regression-adjusted for municipality and electoral term fixed effects. Robust standard errors are clustered by municipality and reported in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table 2: Characteristics of the Municipalities**

	All (1)	Timely audit (2)	Untimely audit (3)	Difference (adjusted) (4)	Share of adj. municipalities w/ timely audit (5)	Observations (6)
<i>Panel A: Electoral Outcomes</i>						
Incumbent mayor runs for reelection	0.77 [0.42]	0.78 [0.42]	0.77 [0.42]	-0.02 (0.05)	0.01 (0.11)	470
Incumbent party wins	0.34 [0.48]	0.36 [0.48]	0.31 [0.47]	-0.02 (0.04)	0.03 (0.09)	470
Incumbent mayor wins   running	0.28 [0.45]	0.30 [0.46]	0.25 [0.44]	-0.03 (0.05)	0.08 (0.09)	470
<i>Panel B: Incumbent Mayor Characteristics</i>						
Mayor, member of PNP	0.52 [0.50]	0.53 [0.50]	0.51 [0.50]	0.09* (0.05)	0.15 (0.09)	470
Member of opposition party to governor	0.36 [0.48]	0.38 [0.49]	0.34 [0.48]	-0.02 (0.06)	-0.12 (0.11)	470
Member of opposition party to governor appointing comptroller	0.44 [0.50]	0.46 [0.50]	0.40 [0.49]	-0.02 (0.06)	-0.02 (0.13)	470
New incumbent	0.37 [0.48]	0.37 [0.48]	0.38 [0.49]	0.02 (0.06)	-0.14 (0.13)	470
Terms in office	1.35 [1.38]	1.47 [1.49]	1.16 [1.18]	0.25* (0.14)	0.06 (0.29)	470
Mayor's win margin in previous election	0.03 [0.04]	0.03 [0.04]	0.02 [0.03]	0.00 (0.00)	0.00 (0.01)	470
Party incumbency advantage	0.57 [0.49]	0.57 [0.50]	0.58 [0.50]	0.01 (0.05)	0.01 (0.14)	470
Observations	470	288	182	470	470	

*Note:* Standard deviations of variables in each category are reported in brackets. Differences in the means of the variables between municipalities with timely and untimely audits (column 4) and the correlation with the share of adjacent municipalities with timely audits (column 5) are estimated via ordinary least squares (OLS) regression models, regression-adjusted for electoral term fixed effects. Robust standard errors are clustered by municipality and reported in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table 2: Characteristics of the Municipalities (*continued*)**

	All (1)	Timely audit (2)	Untimely audit (3)	Difference (adjusted) (4)	Share of adj. municipalities w/ timely audit (5)	Observations (6)
<i>Panel C: Pre-Audit Municipality Characteristics</i>						
Share of population with high school education or more (1990) <sup>a</sup>	0.440 [0.076]	0.443 [0.074]	0.436 [0.079]	0.007 (0.009)	0.034* (0.018)	470
Share of population with college education or more (1990) <sup>a</sup>	0.105 [0.040]	0.107 [0.038]	0.103 [0.043]	0.004 (0.005)	0.015 (0.010)	470
Household median income (USD1,000s) (1990) <sup>a</sup>	8.220 [1.769]	8.231 [1.729]	8.203 [1.837]	0.030 (0.216)	0.992** (0.407)	470
Poverty rate (1990) <sup>a</sup>	0.613 [0.096]	0.610 [0.097]	0.617 [0.095]	-0.007 (0.010)	-0.050** (0.020)	470
Unemployment rate (first year of mayor's term)	0.161 [0.047]	0.157 [0.044]	0.169 [0.052]	0.002 (0.003)	-0.008 (0.010)	362
Unemployment rate (second year of mayor's term)	0.159 [0.046]	0.154 [0.044]	0.167 [0.049]	-0.001 (0.003)	-0.000 (0.007)	418
Observations	470	288	182	470	470	

*Note:* Standard deviations of variables in each category are reported in brackets. Differences in the means of the variables between municipalities with timely and untimely audits (column 4) and the correlation with the share of adjacent municipalities with timely audits (column 5) are estimated via ordinary least squares (OLS) regression models, regression-adjusted for electoral term fixed effects. Robust standard errors are clustered by municipality and reported in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$  \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table 3: Direct and Spillover Effects of Timely Audits on Number of Corrupt Violations in the Current Audit**

	Dependent variables: Number of corrupt violations per report					
	Mayor/Vice-Mayor			All		
	(1)	(2)	(3)	(4)	(5)	(6)
$\beta_1$ : Share of adjacent municipalities with timely audit	0.40** (0.17)	0.56** (0.27)	0.60*** (0.18)	0.30 (0.31)	0.84** (0.40)	0.68* (0.37)
$\beta_2$ : Share of adjacent municipalities with timely audit $\times$ Timely audit		-0.28 (0.32)			-0.95* (0.48)	
$\beta_3$ : Share of adjacent municipalities with timely audit $\times$ New incumbent			-0.46 (0.31)			-0.88* (0.53)
$\alpha_1$ : Timely audit	-0.56*** (0.10)	-0.39* (0.21)	-0.49*** (0.13)	-1.34*** (0.18)	-0.78** (0.32)	-1.27*** (0.24)
$\lambda_1$ : New incumbent	0.21* (0.11)	0.21* (0.11)	0.60** (0.23)	0.34** (0.16)	0.31** (0.15)	1.00** (0.40)
Municipality Controls ( $\lambda$ )	Yes	Yes	Yes	Yes	Yes	Yes
Election Year and Municipality FE's	Yes	Yes	Yes	Yes	Yes	Yes
<i>Spillover Effects Estimates:</i>						
Share of adjacent municipalities with timely audit among interacted group ( $\beta_1 + \beta_k$ )		0.28 (0.19)	0.14 (0.27)		-0.11 (0.36)	-0.19 (0.44)
$\Delta$ Outcome   0 to median share of adjacent municipalities with timely audit						
(a) average or among reference group ( $\beta_1 \times \text{median}(\text{share})$ )	0.27** (0.11)	0.37** (0.18)	0.40*** (0.12)	0.20 (0.20)	0.56** (0.27)	0.46* (0.25)
(b) among interacted group ( $(\beta_1 + \beta_k) \times \text{median}(\text{share})$ )		0.19 (0.12)	0.09 (0.18)		-0.07 (0.24)	-0.13 (0.29)
Observations	470	470	470	470	470	470
Mean of dep. variable (untimely audits)	0.80	0.80	0.80	2.00	2.00	2.00

*Note:* : Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$  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-1); and the incumbent's number of terms in office (at time t). The specifications in columns 3 and 6 include an interaction term between timely audit (1/0) and being a new incumbent. To illustrate the magnitude of the estimated spillover effects, we compare municipalities in which the median share of neighboring jurisdictions have a timely audit (approximately 0.67) to those where no neighboring municipalities do so [ $\beta_1 \times 0.67$ ].

**Table 4: Direct and Spillover Effects of Timely Audits on Re-Election and Number of Corrupt Violations in the Subsequent Audit**

	Dependent variables:					
	Successful re-election of incumbent mayor			Number of corrupt violations per report ( $t+4$ ) Mayor/Vice-Mayor		
	(1)	(2)	(3)	(4)	(5)	(6)
$\beta_1$ : Share of adjacent municipalities with timely audit	0.097* (0.052)	0.087 (0.075)	0.009 (0.055)	0.259 (0.326)	0.527 (0.514)	0.326 (0.452)
$\beta_2$ : Share of adjacent municipalities with timely audit $\times$ Timely audit		0.018 (0.083)			-0.525 (0.484)	
$\beta_3$ : Share of adjacent municipalities with timely audit $\times$ New incumbent			0.207** (0.091)			-0.195 (0.402)
$\alpha_1$ : Timely audit	0.031 (0.027)	0.021 (0.055)	-0.002 (0.034)	-0.051 (0.096)	0.248 (0.288)	-0.038 (0.164)
$\lambda_1$ : New incumbent	-0.458*** (0.039)	-0.457*** (0.040)	-0.636*** (0.066)	-0.151 (0.129)	-0.149 (0.131)	-0.624 (0.434)
Municipality Controls ( $\lambda$ )	Yes	Yes	Yes	Yes	Yes	Yes
Election Year and Municipality FE's	Yes	Yes	Yes	Yes	Yes	Yes
<i>Spillover Effects Estimates:</i>						
Share of adjacent municipalities with timely audit among interacted group ( $\beta_1 + \beta_k$ )		0.105* (0.060)	0.216*** (0.080)		0.00 (0.25)	0.13 (0.26)
$\Delta$ Outcome   0 to median share of adjacent municipalities with timely audit						
(a) average or among reference group ( $\beta_1 \times \text{median}(\text{share})$ )	0.065* (0.035)	0.058 (0.050)	0.006 (0.036)	0.17 (0.22)	0.35 (0.34)	0.22 (0.30)
(b) among interacted group ( $(\beta_1 + \beta_k) \times \text{median}(\text{share})$ )		0.070* (0.040)	0.144*** (0.054)		0.00 (0.17)	0.09 (0.17)
Observations	470	470	470	366	366	366
Mean of dep. variable (untimely audits)	0.253	0.253	0.253	0.69	0.69	0.69

*Note:* Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$  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 ( $t-1$ ); and the incumbent's number of terms in office (at time  $t$ ). The specifications in columns 3 and 6 include an interaction term between timely audit (1/0) and being a new incumbent. The specifications in columns 4-6 include controls for the share of adjacent municipalities with timely audits in the next election, as well as the relevant interactions with timely audit in the own municipality (in the next term) and new incumbent in columns 5 and 6, respectively. To illustrate the magnitude of the estimated spillover effects, we compare municipalities in which the median share of neighboring jurisdictions have a timely audit (approximately 0.67) to those where no neighboring municipalities do so [beta  $\times$  0.67].

**Table 5: Spillover Effects of Timely Audits on Corruption  
by Incumbent Party in Adjacent Municipalities**

	Dependent variables: Number of Corrupt violations per report by Mayor/Vice-Mayor		
	(1)	(2)	(3)
$\beta_1$ : Share of adjacent municipalities of the same party with timely audit	0.33*** (0.09)	0.52** (0.21)	0.42*** (0.13)
$\beta_2$ : Share of adjacent municipalities of the same party with timely audit × Timely audit		-0.31 (0.24)	
$\beta_3$ : Share of adjacent municipalities of the same party with timely audit × New incumbent			-0.23 (0.26)
$\beta_4$ : Share of adjacent municipalities of opposition party with timely audit	0.19* (0.11)	0.19 (0.18)	0.32** (0.15)
$\beta_5$ : Share of adjacent municipalities of opposition party with timely audit × Timely audit		-0.01 (0.21)	
$\beta_6$ : Share of adjacent municipalities of opposition party with timely audit × New incumbent			-0.29 (0.26)
Own Timely Audit Control	Yes	Yes	Yes
Municipality Controls ( $\lambda$ )	Yes	Yes	Yes
Election Year and Municipality FE's	Yes	Yes	Yes
<i>Spillover Effects Estimates:</i>			
Test of homogeneous effects [p-value]:			
(a) average or among reference group incumbents ( $H_0: \beta_1 = \beta_4$ )	0.31	0.27	0.64
(b) among interacted group ( $H_0: \beta_1 + \beta_k = \beta_4 + \beta_j$ )		0.79	0.63
$\Delta$ Outcome   0 to median share of adjacent municipalities with timely audit			
(a) mun.'s of the same party, all or reference group ( $\beta_1 \times \text{med}(\text{share})$ )	0.17*** (0.05)	0.26** (0.10)	0.21*** (0.06)
(b) mun.'s of the same party, interacted group ( $(\beta_1 + \beta_k) \times \text{med}(\text{share})$ )		0.11** (0.05)	0.10 (0.10)
(c) mun.'s of the opposite party, all or reference group ( $\beta_4 \times \text{med}(\text{share})$ )	0.09* (0.05)	0.09 (0.09)	0.16** (0.08)
(d) mun.'s of the opposite party, interacted group ( $(\beta_4 + \beta_j) \times \text{med}(\text{share})$ )		0.09 (0.06)	0.02 (0.10)
Observations	470	470	470
Mean of dep. variable (untimely audits)	0.80	0.80	0.80

*Note:* Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$  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-1); and the incumbent's number of terms in office (at time t). To illustrate the magnitude of the estimated spillover effects, we compare municipalities in which the median share of neighboring jurisdictions have a timely audit (approximately 0.67) to those where no neighboring municipalities do so [ $\beta \times 0.67$ ].

## 8 Appendix A: Omitted Proofs

### Existence of An Equilibrium for System (11)

We show that if

$$\lim_{\theta \rightarrow -\infty} \theta + \mu \Delta^S(\theta) < b - (1 - \mu)(R + b), \quad (16)$$

then according to Brouwer fixed-point theorem, the system (11) has a solution, which is an equilibrium to the model of social norms.

Suppose that condition (16) holds. We first show that a solution to the system (11) is within a non-empty, convex, and compact set. Note that since  $\Delta^S$  and  $\Delta_i^R$  are positive, if  $(\theta_A^*, \theta_N^*)$  solves (11), then there should be  $\theta_i^* \leq b$ ,  $i \in \{A, N\}$ . And since  $\Delta_i^R(\theta_i) \leq 1$  and condition (16) holds, there exists a finite  $\underline{\theta}_i$  such that for all  $\theta_i^* < \underline{\theta}_i$ , we have

$$\theta_i^* + \mu \Delta^S(\alpha_A \theta_A^* + \alpha_N \theta_N^*) < b - (1 - \mu)(R + b) \leq b - (1 - \mu) \Delta_i^R(\theta_i^*)(R + b),$$

for all  $\theta_{-i}^* \leq b$ . Thus, there should be  $\theta_i^* \geq \underline{\theta}_i$ . We restrict the choice of  $(\theta_A^*, \theta_N^*)$  to the set  $[\underline{\theta}_A, b] \times [\underline{\theta}_N, b]$  in the rest of the proof.

Given that the left-hand side of (11) is increasing in  $\theta_i^*$  and the right-hand side is decreasing in  $\theta_i^*$ , the equation (11) implicitly defines  $\theta_i^*$  as a function of  $\theta_{-i}^*$ . Let  $f_i(\theta_{-i}^*)$  be the implicit function. The assumption that  $G(\theta)$  is continuously differentiable implies that  $f_i(\theta_{-i}^*)$  is continuous, thus the vector function  $(f_A(\theta_N^*), f_N(\theta_A^*))$  is continuous on  $[\underline{\theta}_A, b] \times [\underline{\theta}_N, b]$ . According to Brouwer fixed-point theorem, the vector function  $(f_A(\theta_N^*), f_N(\theta_A^*))$  has a fixed point, which is an equilibrium to the model of social norms.

### Proof of Proposition 2

The results in parts a) and b) are about the impact of increasing  $\alpha_A$ . From equilibrium condition (11), we have, for  $i = A, N$ ,

$$\frac{d\theta_i^*}{d\alpha_A} \cdot \left[ 1 + (1 - \mu) \frac{d\Delta_i^R(\theta_i^*)}{d\theta_i^*} (R + b) \right] = -\mu \Delta^{S'}(\tilde{\theta}) \frac{d\tilde{\theta}}{d\alpha_A}. \quad (17)$$

Note that in the short run, we have

$$\frac{d\tilde{\theta}}{d\alpha_A} = (\theta_A^* - \theta_N^*),$$

while in the long run,

$$\frac{d\tilde{\theta}}{d\alpha_A} = (\theta_A^* - \theta_N^*) + \alpha_A \frac{d\theta_A^*}{d\alpha_A} + \alpha_N \frac{d\theta_N^*}{d\alpha_A}.$$

From the definition of  $\Delta_i^R(\theta_i^*)$ , we have

$$\frac{d\Delta_N^R(\theta_N^*)}{d\theta_N^*} = -(1 - p)\psi \cdot \phi(\psi \delta_N^1) \frac{d\delta_N^1}{d\theta_N^*} > 0, \quad \frac{d\Delta_A^R(\theta_A^*)}{d\theta_A^*} = -\psi \cdot \phi(\psi \delta_A^1) \frac{d\delta_A^1}{d\theta_A^*} > 0. \quad (18)$$

Thus,  $d\theta_i^*/d\alpha_A$  has the same sign as  $-\Delta^{S'}(\tilde{\theta})d\tilde{\theta}/d\alpha_A$ , and  $d\theta_A^*/d\alpha_A$  and  $d\theta_N^*/d\alpha_N$  always have the same sign.



We first look at the case of long run. Consider the situation in result a). Since  $\tilde{\theta} > 0$ , we have  $\Delta^{S'}(\tilde{\theta}) > 0$ , given our assumption on  $G(\theta)$ . Then it must be the case that

$$\frac{d\theta_A^*}{d\alpha_A} > 0, \quad \frac{d\theta_N^*}{d\alpha_A} > 0, \quad \text{and} \quad \frac{d\tilde{\theta}}{d\alpha_A} < 0.$$

This is because if  $d\theta_A^*/d\alpha_A \leq 0$  and  $d\theta_N^*/d\alpha_A \leq 0$ , there should be  $d\tilde{\theta}/d\alpha_A \geq 0$  according to (17), a contradiction to  $d\tilde{\theta}/d\alpha_A = (\theta_A^* - \theta_N^*) + \alpha_A d\theta_A^*/d\alpha_A + \alpha_N d\theta_N^*/d\alpha_A < 0$ .

Consider the situation in result b). Since  $\tilde{\theta} < 0$ , we have  $\Delta^{S'}(\tilde{\theta}) < 0$ . Then  $d\theta_A^*/d\alpha_A$ ,  $d\theta_N^*/d\alpha_A$ , and  $d\tilde{\theta}/d\alpha_A$  all have the same sign. In this situation, either of the following cases can possibly happen,

- 1)  $\frac{d\theta_A^*}{d\alpha_A} > 0$ ,  $\frac{d\theta_N^*}{d\alpha_A} > 0$ , and  $\frac{d\tilde{\theta}}{d\alpha_A} > 0$ ;
- 2)  $\frac{d\theta_A^*}{d\alpha_A} < 0$ ,  $\frac{d\theta_N^*}{d\alpha_A} < 0$ , and  $\frac{d\tilde{\theta}}{d\alpha_A} < 0$ ,

depending on the parameters.

If, however, we are in the case of short run, that is,  $d\tilde{\theta}/d\alpha_A = (\theta_A^* - \theta_N^*) < 0$ , then it is easy to verify that in the situation of result a), there must be that

$$\frac{d\theta_A^*}{d\alpha_A} > 0, \quad \frac{d\theta_N^*}{d\alpha_A} > 0,$$

which is the same as what happens in the long run. In the situation of result b), there must be that

$$\frac{d\theta_A^*}{d\alpha_A} < 0, \quad \frac{d\theta_N^*}{d\alpha_A} < 0.$$

Now we examine the impact of the home community's  $\psi$  on the value of  $d\theta_i^*/d\alpha_A$  in this community. Note that we assume that there is a large number of communities, so the change of  $\psi$  in the home community is negligible to the incumbents of other communities. This implies that the right-hand side of equation (17) is independent of the home community's  $\psi$ , regardless of whether it is short run or long run. Therefore, the absolute value  $|d\theta_i^*/d\alpha_A|$  of  $d\theta_i^*/d\alpha_A$  and the value of  $d\Delta_i^R(\theta_i^*)/d\theta_i^*$  change with  $\psi$  in opposite directions.

For the purpose of our study, we compare the values of  $d\theta_i^*/d\alpha_A$  in the cases of  $\psi \rightarrow 0$  and  $\psi > 0$  only. The case  $\psi \rightarrow 0$  corresponds to a long-term incumbent whose re-election chances no longer depend on his behavior. The case  $\psi > 0$  corresponds to a short-term office holder whose probability of re-election is sensitive to his behavior. When  $\psi \rightarrow 0$ , according to (18), we have  $d\Delta_i^R(\theta_i^*)/d\theta_i^* \rightarrow 0$ , because

$$\frac{d\delta_N^1}{d\theta_N^*} = -\frac{p(1-p)^2(1-G(b))g(\theta_N^*)}{[p+(1-p)(1-G(\theta_N^*))]^2}, \quad \frac{d\delta_A^1}{d\theta_A^*} = -\frac{(1-p)(1-G(b))g(\theta_A^*)}{(1-G(\theta_A^*))^2}. \quad (19)$$

are both bounded. Therefore,

$$1 + (1-\mu)\frac{d\Delta_i^R(\theta_i^*)}{d\theta_i^*}(R+b) \rightarrow 1,$$

when  $\psi \rightarrow 0$ . When  $\psi > 0$  (and is also sufficiently small so that the cutoffs  $\underline{\delta}^0$  and  $\underline{\delta}_i^1$  are interior solutions), we have

$$1 + (1-\mu)\frac{d\Delta_i^R(\theta_i^*)}{d\theta_i^*}(R+b) > 1.$$

Therefore, we have

$$\left| \frac{d\theta_i^*}{d\alpha_A} \right|_{\psi \rightarrow 0} > \left| \frac{d\theta_i^*}{d\alpha_A} \right|_{\psi > 0}.$$

That is, the spillover effect is more pronounced when the incumbent is a long-term incumbent, instead of a new office holder.

### Proof of Proposition 3

The first part of this proposition regarding the impact of home audits has been proved in Section 3.3.

For result a), we only need to prove that the sign of  $dE[P_i(r, \theta_i^*)]/d\theta_i^*$  is ambiguous. According to the discussion below equation (13), to show that the effect of  $\theta_i^*$  on the ex ante probability of re-election is ambiguous, we only need to show that  $d\underline{\delta}_i^1/d\theta_i^*$  is negative. This is true according to (19).

To prove result b), we first take the derivative of  $E[P_i(r, \theta_i^*)]$  with respect to  $\alpha_A$  and obtain

$$\begin{aligned} \frac{dE[P_i(r, \theta_i^*)]}{d\alpha_A} &= \frac{dE[P_i(r, \theta_i^*)]}{d\theta_i^*} \frac{d\theta_i^*}{d\alpha_A} \\ &= \left\{ -g(\theta_i^*)\Delta_i^R(\theta_i^*) - \psi [pG(\theta_i^*) \cdot \mathbb{I}_{i=N} + (1 - G(\theta_i^*)) \phi(\psi \underline{\delta}_i^1)] \frac{d\underline{\delta}_i^1}{d\theta_i^*} \right\} \frac{d\theta_i^*}{d\alpha_A} \end{aligned}$$

We compare the absolute values of  $dE[P_i(r, \theta_i^*)]/d\alpha_A$  in the cases of  $\psi \rightarrow 0$  and  $\psi > 0$  only. When  $\psi \rightarrow 0$ , since  $\Delta_i^R(\theta_i^*) \rightarrow 0$  and  $d\underline{\delta}_i^1/d\theta_i^*$  is bounded, we have

$$\left| \frac{dE[P_i(r, \theta_i^*)]}{d\alpha_A} \right| \rightarrow 0.$$

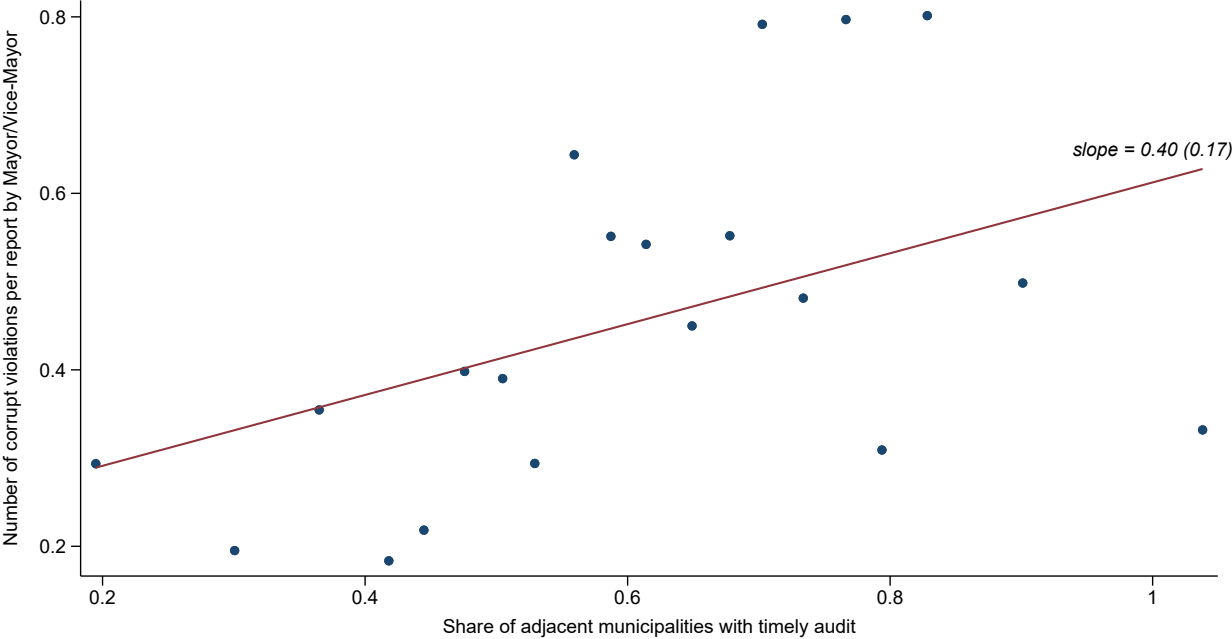
When  $\psi > 0$ , *generically* we have

$$\left| \frac{dE[P_i(r, \theta_i^*)]}{d\alpha_A} \right| > 0.$$

Therefore, the magnitude of the spillover effect is stronger if the incumbent is a short-term incumbent, whose re-election chances are sensitive to his behavior, rather than a long-term office holder. This completes the proof.

# Appendix B: Additional Results, Tables and Figures

**Figure A.1:** Relationship between the Share of Adjacent Municipalities with Timely Audits and Corruption in Own Municipality



*Note:* This figure shows a bin scatter-plot of the data (residuals adjusting for municipality and electoral term fixed effects) showing the relationship between exposure to timely audits in neighboring jurisdictions and the number of violations by the political leadership in the home municipality.

## Appendix B.1: More information about corrupt opportunities as a result of audits elsewhere

One can imagine that the spillover rise in home corruption is partially reflective of audits elsewhere revealing more information regarding corrupt practices in reference localities, and not necessarily because the reference group has systematically lower corruption levels. To investigate and distinguish this mechanism empirically, we need to pursue an empirical strategy whereby different incumbents observe exogenously high or low levels of corruption in neighboring municipalities' timely audits.<sup>42</sup> To do so, we leverage variation in the degree of electoral competition, the historical alternation of parties, in office in neighboring jurisdictions to generate such heterogeneity in corruption levels. Recall that the reduction in corruption from timely audits is significantly stronger among municipalities with such stronger electoral accountability (see Appendix Table A.3). We thus examine how the external effects of timely audits vary based on the level of electoral competition in neighboring municipalities.

We use the following empirical model (20):

$$c_{m,t} = \beta_1 \mathbf{A}_{m,t}^{(-m,alt)} + \beta_2 \mathbf{A}_{m,t}^{(-m,adv)} + \kappa_1 \mathbf{ALL}_{m,t}^{(-m,alt)} + \kappa_2 \mathbf{ALL}_{m,t}^{(-m,adv)} + \alpha A_{m,t} + \lambda X_{m,t} + \gamma_t + \rho_m + \epsilon_{m,t} \quad (20)$$

where  $\mathbf{A}_{m,t}^{(-m,alt)}$  represents the neighboring municipalities with party alternation (the converse of our party incumbency advantage indicator) that have timely audits around municipality  $m$  and election  $t$ ;  $\mathbf{A}_{m,t}^{(-m,adv)}$  represents the analogous measure for municipalities with party incumbency advantage. We also control for  $\mathbf{ALL}_{m,t}^{(-m,alt)}$  and  $\mathbf{ALL}_{m,t}^{(-m,adv)}$ , the total number of neighboring municipalities with party alternation and with party incumbency advantage, respectively, with audits (timely or untimely) around election period  $t$ . All other variables are defined as in empirical model (14). Since the number of timely audits in neighboring jurisdictions is quasi-random conditional on the total number of audits in such reference groups,  $\beta_1$  and  $\beta_2$  capture the heterogeneous spillover effects of each adjacent timely audits for municipalities from each respective group.<sup>43</sup> We also examine heterogeneity in such spillovers by the timeliness of the own audit and by incumbent experience, by allowing interactions of the spillover measures above with the heterogeneity indicators described earlier.

We report the results of equations 20 in Table A.1.<sup>44</sup> Across all specifications, we find that the negative externalities of timely audits are strongest when adjacent municipalities experience party alternation; that is, when timely audits reveal significantly lower levels of corruption. In contrast, the point estimates of the external effects from adjacent municipalities with party incumbency advantage are remarkably small and statistically indistinguishable from zero; this suggests that no externalities are at play when more timely information regarding higher levels of corruption is revealed. Although we cannot reject the null hypothesis of homogeneous spillover effects by level of neighboring electoral competition (p-value = 0.236 for violations among the municipal leadership), there are starker differences when focusing on the spillover effects among

<sup>42</sup>We cannot simply use the observed level of corruption from timely audits in neighboring jurisdictions, as it is plausible that the unobserved determinants of corrupt activities are correlated across such jurisdictions. This unobserved heterogeneity would lead to bias in our estimates of such spillover effects.

<sup>43</sup>This specification follows an extensive literature estimating spatial spillovers (e.g. Miguel and Kremer, 2004; Bobba and Gignoux, 2019). We specify the model using the number of timely audits instead of the parsimonious specification using shares of municipalities with timely audits for one main reason: to avoid non-randomly losing observations when dividing by zero in cases where no adjacent municipalities experience an audit (timely or untimely) around election  $t$ . All main empirical results in the paper are robust to using this alternative specification.

<sup>44</sup>We report full results including coefficients on the controls for number of audits ( $X_{m,t}^1$ ,  $X_{m,t}^2$ , etc.) in Appendix Table A.3.

**Table A.1: Heterogeneous Spillover Effects of Timely Audits on Corruption:  
By Party Alternation v.s. Party Incumbency Advantage in Adjacent Municipalities**

	Dependent variables: Number of Corrupt Violations by Mayor/Vice-Mayor		
	(1)	(2)	(3)
$\beta_1$ : Number of timely audits in adjacent municipalities with party alternation	0.18*** (0.06)	0.28*** (0.10)	0.19** (0.07)
$\beta_2$ : Number of timely audits in adjacent municipalities with party alternation × Own timely audit		-0.15* (0.08)	
$\beta_3$ : Number of timely audits in adjacent municipalities with party alternation × New incumbent			0.00 (0.12)
$\beta_4$ : Number of timely audits in adjacent municipalities with party incumb. adv.	0.04 (0.06)	0.03 (0.08)	0.10 (0.07)
$\beta_5$ : Number of timely audits in adjacent municipalities with party incumb. adv. × Own timely audit		0.04 (0.08)	
$\beta_6$ : Number of timely audits in adjacent municipalities with party incumb. adv. × New incumbent			-0.13 (0.11)
Number of Audits in Adjacent Mun. Controls	Yes	Yes	Yes
Own Timely Audit Control	Yes	Yes	Yes
Municipality Controls ( $\lambda$ )	Yes	Yes	Yes
Election Year and Municipality FE's	Yes	Yes	Yes
<i>Spillover Effects Estimates:</i>			
Number of timely audits in adjacent municipalities with party alternation among interacted group ( $\beta_1 + \beta_2$ ) or ( $\beta_1 + \beta_3$ )		0.13** (0.06)	0.19* (0.10)
Number of timely audits in adjacent municipalities with party incumb. adv. among interacted group ( $\beta_4 + \beta_5$ ) or ( $\beta_4 + \beta_6$ )		0.07 (0.07)	-0.03 (0.09)
Test of homogeneous effects [p-value]:			
(a) average or among reference group ( $H_0: \beta_1 = \beta_4$ )	0.126	0.067	0.463
(b) among interacted group ( $H_0: \beta_1 + \beta_k = \beta_4 + \beta_j$ )		0.519	0.080
Observations	470	470	470
Mean of dep. variable (untimely audits)	0.80	0.80	0.80

*Note:* OLS regressions. Robust standard errors are clustered by municipality and reported in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$  Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$  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-1); and the incumbent's number of terms in office (at time t). To illustrate the magnitude of the estimated spillover effects, we compare municipalities in which the median share of neighboring jurisdictions have a timely audit (approximately 0.67) to those where no neighboring municipalities do so [ $\beta_1 \times 0.67$ ].

municipalities who themselves experience untimely audits (p-value = 0.067).<sup>45</sup> When we allow for further heterogeneity by home incumbency, however, our estimates become too noisy to draw any conclusive evidence. Overall, we take this pattern of external effects in neighboring municipalities with party alternation—where timely audits have a greater effect in decreasing corruption—as evidence that the level of corruption, and not just the additional information, is the driving force of the negative spillover effects.

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<sup>45</sup>When examining overall corruption levels in the municipality, we have more precision to distinguish such heterogeneous effects; p-value = 0.041 for all violations, results available upon request.

**Table A.2: Effects of Timely Audits on Corruption,  
by Party Alternation vs. Party Incumbency Advantage**

	Dependent variables:			
	Number of corrupt violations per report			
	Mayor/Vice-Mayor		All	
	(1)	(2)	(3)	(4)
$\alpha_1$ : Timely audit	-0.77*** (0.12)	-0.78*** (0.13)	-1.59*** (0.24)	-1.62*** (0.24)
$\alpha_2$ : Timely audit $\times$ Party incumbency advantage	0.39** (0.15)	0.39** (0.16)	0.45 (0.29)	0.48 (0.30)
$\beta_1$ : Share of adjacent municipalities with timely audit	0.41** (0.16)	0.47* (0.24)	0.29 (0.30)	0.55 (0.39)
$\beta_2$ : Share of adjacent municipalities with timely audit $\times$ Party incumb. adv.		-0.10 (0.30)		-0.46 (0.48)
$\lambda_1$ : Party incumbency advantage	-0.23 (0.14)	-0.17 (0.20)	0.06 (0.27)	0.33 (0.32)
Municipality Controls ( $\lambda$ )	Yes	Yes	Yes	Yes
Election Year and Municipality FE's	Yes	Yes	Yes	Yes
Timely audits among municipalities with party incumbency advantage ( $\alpha_1 + \alpha_2$ )	-0.38*** (0.13)	-0.38*** (0.13)	-1.14*** (0.22)	-1.14*** (0.22)
Observations	470	470	470	470
Mean of dep. variable (untimely audits)	0.80	0.80	2.00	2.00

*Note:* Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$  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 ( $t-1$ ); and the incumbent's number of terms in office (at time  $t$ ). Party incumbency advantage is a dummy variable for whether the incumbent's party won the previous 3+ elections in the municipality. To illustrate the magnitude of the estimated spillover effects, we compare municipalities in which the median share of neighboring jurisdictions have a timely audit (approximately 0.67) to those where no neighboring municipalities do so [ $\beta \times 0.67$ ].

**Table A.3: Heterogeneous Spillover Effects of Timely Audits on Number of Corrupt Violations by Levels of Corruption in Adjacent Municipalities**

	Dependent variables: Number of corrupt violations per report			
	Mayor/ Vice-Mayor	All	Mayor/ Vice-Mayor	All
	(1)	(2)	(3)	(4)
$\beta_1$ : Number of timely audits in adjacent municipalities with party alternation	0.18*** (0.06)	0.24** (0.10)	0.19** (0.07)	0.33*** (0.12)
$\beta_2$ : Number of timely audits in adjacent municipalities with party alternation × New Incumbent			0.00 (0.12)	-0.20 (0.18)
$\beta_3$ : Number of timely audits in adjacent municipalities with party incumb. adv.	0.04 (0.06)	-0.05 (0.11)	0.10 (0.07)	-0.03 (0.14)
$\beta_4$ : Number of timely audits in adjacent municipalities with party incumb. adv. × New Incumbent			-0.13 (0.11)	-0.03 (0.20)
$\lambda_1$ : Number of audits in adjacent municipalities with party alternation	-0.04 (0.09)	-0.14 (0.15)	0.01 (0.09)	-0.10 (0.15)
$\lambda_2$ : Number of audits in adjacent municipalities with party alternation × New Incumbent			-0.12 (0.11)	-0.06 (0.15)
$\lambda_3$ : Number of audits in adjacent municipalities with party incumb. adv.	-0.01 (0.08)	0.03 (0.16)	-0.02 (0.09)	0.01 (0.18)
$\lambda_4$ : Number of audits in adjacent municipalities with party incumb. adv. × New Incumbent			0.05 (0.08)	0.06 (0.19)
$\lambda_5$ : New incumbent	0.21* (0.11)	0.34** (0.16)	0.51* (0.27)	0.59 (0.42)
$\alpha_1$ : Timely audit	-0.57*** (0.10)	-1.36*** (0.18)	-0.59*** (0.10)	-1.40*** (0.18)
Municipality Controls ( $\lambda$ )	Yes	Yes	Yes	Yes
Election Year and Municipality FE's	Yes	Yes	Yes	Yes
Observations	470	470	470	470
Mean of dep. variable (untimely audits)	0.80	2.00	0.80	2.00

*Note:* Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$  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-1); and the incumbent's number of terms in office (at time t). Party incumbency advantage is a dummy variable for whether the incumbent's party won the previous 3+ elections in the municipality. To illustrate the magnitude of the estimated spillover effects, we compare municipalities in which the median share of neighboring jurisdictions have a timely audit (approximately 0.67) to those where no neighboring municipalities do so [ $\beta_1 \times 0.67$ ].



**Table A.4: Spillover Effects of Timely Audits on Number of Corrupt Violations by Incumbent Party in Own Municipality**

	Dependent variables:					
	Number of corrupt violations per report					
	Mayor/Vice-Mayor			All		
	(1)	(2)	(3)	(4)	(5)	(6)
$\beta_1$ : Share of adjacent municipalities with timely audit	0.45** (0.20)	0.61** (0.30)	0.72*** (0.21)	0.48 (0.37)	1.02** (0.45)	0.96** (0.44)
$\beta_2$ : Share of adjacent municipalities with timely audit $\times$ Timely audit		-0.28 (0.31)			-0.95** (0.47)	
$\beta_3$ : Share of adjacent municipalities with timely audit $\times$ PNP incumbent	-0.10 (0.17)	-0.10 (0.17)	-0.25 (0.22)	-0.38 (0.27)	-0.38 (0.27)	-0.58* (0.32)
$\beta_4$ : Share of adjacent municipalities with timely audit $\times$ PNP incumbent $\times$ New incumbent			0.35 (0.31)			0.49 (0.48)
$\beta_5$ : Share of adjacent municipalities with timely audit $\times$ New incumbent			-0.59 (0.37)			-1.12* (0.61)
$\lambda_1$ : PNP incumbent mayor	0.18* (0.10)	0.17 (0.10)	0.31** (0.15)	0.54*** (0.19)	0.52*** (0.18)	0.61** (0.26)
$\lambda_2$ : PNP incumbent mayor $\times$ New incumbent			-0.22 (0.24)			-0.04 (0.34)
$\lambda_3$ : New incumbent	0.22* (0.11)	0.21* (0.11)	0.66*** (0.20)	0.35** (0.16)	0.32** (0.15)	0.99** (0.40)
Own Timely Audit Control	Yes	Yes	Yes	Yes	Yes	Yes
Municipality Controls	Yes	Yes	Yes	Yes	Yes	Yes
Election Year and Municipality FE's	Yes	Yes	Yes	Yes	Yes	Yes
Observations	470	470	470	470	470	470
Mean of dep. variable (untimely audits)	0.80	0.80	0.80	2.00	2.00	2.00

*Note:* Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$  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-1); and the incumbent's number of terms in office (at time t). To illustrate the magnitude of the estimated spillover effects, we compare municipalities in which the median share of neighboring jurisdictions have a timely audit (approximately 0.67) to those where no neighboring municipalities do so [ $\beta_1 \times 0.67$ ].

**Table A.5: Estimates of Spillover Effects of Timely Audits on the Number of Audit Reports**

	Dependent variable: Number of audit reports		
	(1)	(2)	(3)
$\beta_1$ : Share of adjacent municipalities with timely audit	0.01 (0.27)	-0.09 (0.21)	0.04 (0.29)
$\beta_2$ : Share of adjacent municipalities with timely audit $\times$ Timely audit		0.17 (0.34)	
$\beta_3$ : Share of adjacent municipalities with timely audit $\times$ New incumbent			-0.07 (0.36)
$\alpha_1$ : Timely audit	0.70*** (0.11)	0.60*** (0.21)	0.70*** (0.11)
$\lambda_1$ : New incumbent	-0.20* (0.10)	-0.19* (0.10)	-0.15 (0.21)
Municipality Controls ( $\lambda$ )	Yes	Yes	Yes
Election Year and Municipality FE's	Yes	Yes	Yes
Spillover Effects Estimates:			
Share of adjacent municipalities with timely audit among new incumbents ( $\beta_1 + \beta_k$ )		0.08 (0.38)	-0.03 (0.36)
$\Delta$ Outcome   0 to median share of adjacent municipalities with timely audit			
(a) among all or experienced mayors ( $\beta_1 \times \text{median}(\text{share})$ )	0.01 (0.18)	-0.06 (0.14)	0.03 (0.19)
(b) among new incumbent mayors ( $(\beta_1 + \beta_k) \times \text{median}(\text{share})$ )		0.06 (0.25)	-0.02 (0.24)
Observations	470	470	470
Mean of dep. variable (untimely audits)	1.49	1.49	1.49

*Note:* Coefficient estimates and standard errors from OLS regressions are presented; disturbance terms are clustered at the municipality level. Coefficient estimates statistically significant at \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$  levels, respectively. Controls are indicators for New Progressive Party membership, for incumbent in the opposition party to the state-level executive government, and for incumbent in the opposition party to the governor who appointed Comptroller; the vote share for the incumbent in the previous election (t-1); and the incumbent's number of terms in office (at time t). To illustrate the magnitude of the estimated spillover effects, we compare municipalities in which the median share of neighboring jurisdictions have a timely audit (approximately 0.67) to those where no neighboring municipalities do so [ $\beta_1 \times 0.67$ ].