Norms of Corruption in Politicians' Malfeasance[*]

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Abstract

We develop a political agency model in which office holders are motivated by re-election motives as well as reputation motives, and are incentivized to curb corrupt behavior through formal audits and elections, as well as norms regarding the appropriateness of corruption that prevail in their peer group. The model is meant to formalize the notion that anti-social behavior is governed by both formal rules of conduct (regulations and laws), and informal rules of conduct. We show that, while the formal institutions of audits and elections have the desired direct effect of reducing corruption, the presence of norms and their interaction with the direct incentives can have unintended effects. We examine the model's predictions in the context of Puerto Rico's anti-corruption municipal audits program over the period 1987-2014. Specifically, using a quasi-experimental identification strategy based on the exogenous timing of audits relative to elections, we show that the model helps explain peculiar patters in the data: 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 fairly large spillover effect: communities where two-thirds of adjacent jurisdictions undergo a (timely) audit experience an increase in reported corruption levels by by mayors and vice mayors of 51 percent.

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1 Introduction

Only a comparatively small number of *successful societies* have been able to develop a functional and reliable modern state (Acemoglu and Robinson (2012); Besley and Persson (2011)). Many other states — including most non-fragile states in the developing world— better 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 (Fukuyama (2014)). Notably, a sizeable number of countries in the latter group are democratic countries whose formal institutions (constitution, civil code) are well established and quite similar – if not identical – to the formal institutions in the successful group. 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[1] 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. One obvious possible explanation of this puzzle is that the latter countries have different *informal* institutions; many Latin American, Asian or African societies, for example, view patronage, nepotism and gift giving as acceptable practices to sustain relationships (Prasad et al. (2019); Köbis et al. (2018)). As in Italy or Greece, low quality bureaucracy, clientelism, and high levels of corruption are arguably connected to patrimonialism, a lack of civic community and a general distrust in the state as exemplified by socially accepted tax evasion (Fukuyama, 2014).

A central question then becomes whether and to what extent formal institutions and the law can serve to limit patronage and corrupt networks effectively and sustainably in clientelist societies in the presence, and possibly despite of, prevailing social norms. A related question is why targeted anti-corruption efforts in many of those societies have been remarkably unsuccessful (Gans-Morse et al. (2018)). The purpose of this paper is to study how legal institutions designed to curb corruption fare in a society where patters of behavior are also shaped by social norms regarding such acts. We contribute to the literature in two important ways. First, we develop a model of the interaction of formal and informal institutions, and provide evidence that can be explained by the theory. Although a sizeable body of research addresses legal structures and cultural beliefs as contributors to corruption, most of the literature studies those factors separately, and we therefore have little understanding in how they might interact. Second, our model implies that in the presence of social norms, anti-corruption efforts can have unintended (negative) spillover effects in clientilist societies where corrupt behavior is the norm and not the exception, again, a pattern that we find in the data.

Our theoretical framework is built around a multi-community political agency model in which office holders in each community are subject to formal incentives, specifically elections and audits, to limit rent seeking. At the same time, they worry about their reputation among peers, which depends on prevailing norms of corruption in neighboring communities. The model essentially combines a standard two-period careers concerns model, augmented by audits as an additional source of information for voters, with the formalization of norm-based motives developed in Benabou and Tirole (2011). In their decision making, office holders weigh the benefits from corruption against a desire to stay in office and reputations concerns. An audit in their own community unambiguously boosts an incumbent's re-election chances provided no wrongdoing was found, and thus lowers rent-seeking. How the social motives affect incentives is ambiguous, however, and determined by both the stigma of anti-social behavior and the honor of pro-social behaviour (Benabou and Tirole, 2011). Specifically, the effect of an exogenous shock to the prevalence of corruption

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[1]Most European countries are governed by a civil code originating from the Code Napoleon, and have very similar administrative states through with the code is enforced.

among an incumbent's peers depends on whether corruption is a majority or a minority phenomenon (under some mild assumptions). Audits in neighboring communities constitute shocks to the prevalence of corruption and 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 corruption are complements, amplifying the desirable direct effect of audits.

Next, we study this phenomenon in the context of Puerto Rico's municipal anti-corruption audits program. Puerto Rico is an excellent setting to examine the empirical implications of the theory because, as a U.S. territory, it shares many characteristics and formal federal institutions as states. Yet, it is considered a clientilist society with high levels of corruption and patronage (Collins et al. (2006), Pantojas-García (2015), Bobonis et al. (2016), Bobonis et al. (2022)). 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 external effects of these audits, our empirical strategy exploits the fact that audit reports released before an election— 'timely audits' (Bobonis et al., 2016) — 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. Although this allows for politicians to anticipate the timing of audits, this timing and municipalities' assignments into 'timely' and 'untimely' groups is plausibly exogenous. This setting allows us to compare levels of reported corruption across municipalities with varying levels of exposure to timely audits in neighboring jurisdictions, thus measuring the external effects of timely audits on contemporaneous levels of corruption and electoral accountability at home.

The data show 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 undesirable spillover effects of (timely) anti-corruption audits in neighboring municipalities. Exposure to timely audits in two-thirds (the sample median) of adjacent municipalities leads to an *increase* in reported corruption levels in the home municipality 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. Interestingly, our results also suggest 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 a world in which politicians are motivated by both re-election motives and social norms in several important respects. First, audits that are timed close to election do have their intended direct effect in reducing rent seeking, suggesting office holders care about their re-election. Second, there is a clear pattern of a negative spatial spillover effects of anti-corruption efforts at the municipal level, which is implied by a social motive where office holders consider the prevalence of corruption in their peer group when deciding on their own rent seeking, once we assume that the peer group is composed of their counterparts in neighboring jurisdictions. Third, since the data shows that a majority of municipal office holders are in fact corrupt, our finding that anti-corruption efforts elsewhere and corruption 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. Lastly, we find that the spillover effects – both concerning office holder behavior and re-election rates at home – are more pronounced among long-term incumbents than among first-office holders. The norm-based motives in our model provide a simple explanation for this

result since long-term incumbents care more about norms than first-time office holders under the reasonable assumption 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 are consistent with norms playing a key role in shaping corrupt behavior. At the same time, we argue that this last empirical result is inconsistent with yardstick competition among office holders – an alternative explanation of spillovers that is entirely based on formal institutions – and with other explanations such as the spatial displacement of corrupt activities.

The findings of this study offer several important contributions to the political economy literature. Previous theoretical work considers motives as to why corruption can be self-reinforcing and societies can be 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). Tirole (1996) in particular highlights the role that social norms or conventions can play in such ratchet effects. Recent contributions by Acemoglu and Jackson (2017), Besley et al. (2019) model how abruptly enacting legal reforms that are in conflict with prevailing social norms may actually result in an increase in illegal activities. Our model adapts the micro-foundation of Benabou and Tirole (2011) and Besley et al. (2019) with both individual and social motives for compliance with moral actions, and demonstrate the persistent of such norms in the context of political corruption.[2]

Most of the empirical literature on government corruption focuses on the direct effects of anti-corruption policy interventions, often audit schemes, on outcomes within a jurisdiction.[3] Most relevant to this paper is Bobonis et al. (2016) who study the same Puerto Rican municipal audit program over a subset of our time period (1987-2005). They find that (timely) audits result in a significant short-term reduction in municipal corruption levels and raise an incumbent mayors' electoral accountability. However, they find no evidence of any long term effect of audits on corruption. Avis et al. (2018) is one of many empirical studies based on the randomized municipal government anti-corruption audits in Brazil.[4] Importantly, and unlike the Puerto Rico audit program where politicians can anticipate audits because of the fixed and pre-established order, the audits in Brazil are fully random, and thus cannot be anticipated. Although they too focus primarily on the direct effect of audits, they also estimate negative spillover effects of audits in municipalities with access to local media: municipalities with AM radio experience a fall in corruption by 7.5 percent in response to each adjacent municipality having been audited in the past.

The paper is organized as follows. Section 2 provides a brief background on Puerto Rico's municipal government system and auditing program. We follow with a description of our theoretical framework and the model's predictions in Section 3. Section 4 presents a description of the data, whereas Section 5 discusses the study's empirical research design. We present the central empirical results of the paper in Section 6. In Section 7 we consider alternative theoretical explanations, and we also present our findings on government responsiveness. The paper concludes in Section 8.

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[2]Closely related to our context are studies where social norms interact with efforts aiming to reduce crime and corruption. Fisman and Miguel (2007) provide evidence that cultural norms and legal enforcement are both important determinants of corruption. Previous work in political economy has studied the effect of social norms and reputational concerns on the act of voting (Dellavigna et al. (2017)), campaign contributions (Perez-Truglia and Cruces (2017)), and the expression of xenophobic views (Bursztyn et al. (2020); see Bursztyn and Jensen (2017) for a review of this empirical literature. Köbis et al. (2018) provide an overview from a social psychology perspective on the relatively small literature of social norms of corruption.

[3]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.

[4]See, for example, Ferraz and Finan (2008), Ferraz and Finan (2011), and Zamboni and Litschig (2018).

2 Background

2.1 Municipal Government Administration, Politics, and Corruption

In Puerto Rico, municipal government consists of a mayor and a local assembly, all elected for a four-year term following the Commonwealth (and U.S. federal) government electoral cycle.[5] 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.

Compared to counties and cities in the United States, municipal governments in Puerto Rico possess a greater degree of autonomy. Although their responsibilities are primarily limited to public health services, infrastructure, and solid waste management, there is some heterogeneity in municipal abilities to raise tax revenues and manage expenditures.[6]The mayor appoints the top management of the municipality.

Similar to higher levels of government in Puerto Rico—and the rest of the United States—municipal politics are controlled by two major political parties. A large majority of mayors are members of either the Popular Democratic Party (PPD) generally on the political left on the spectrum or the New Progressive Party (PNP) generally on the political right. Bobonis et al. (2021) argue that rising political polarization is main driver of malfeasance and patronage in the municipal politics during our period of interest, and they find these patterns to be consistent with models of clientelism. The local assembly is controlled by the majority party, but the law guarantees some representation for minority parties by reserving a small number of assembly seats for members of parties ranked second or third in the election. Acting as a check on the dominant party, these minority assembly members often assume a critical oversight role, by exposing waste and corruption.

The audit reports we have access to in our data suggest that corruption in municipal governments in Puerto Rico takes diverse forms. One example is procurement fraud, which takes place though illegal auctions for contracts, over-invoicing of firms, or the use of fake receipts, possibly in exchange for votes and political favors. Government corruption can also manifested itself in illegal hiring and firing due to nepotism, discrimination, or for political patronage. Lastly, some individuals simply divert resources for personal purposes, through misappropriation of taxes or superfluous spending. Following the existing literature, we combine all of these different forms of corruption into a single comprehensive measure, which we explain in detail in Section 4.1) below.

2.2 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 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, uninterruptedly since 1953.

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[5]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.

[6]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 Puerto Rican government.

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.[7] 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.

Several measures are taken to minimize potential biases in the conduct of the audits and in the dissemination of their 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. Importantly for our research design, municipalities are audited following a pre-specified order, established in the 1950s. Once all municipalities have been audited, a new auditing round takes place following the same pre-specified order. Given that our research setting spans 28 years, all seventy-eight municipalities were audited multiple times during our period of interest.

3   Theoretical Framework

The objective of this section is to formulate a political agency model that can help to shed light on the forces that drive the empirical findings we report in Section 6 below. To this end, we first set up a general theoretical framework which is based on a career concern model of politicians' behaviour, which we augment by a social norm motive as a channel through which inter-community spillovers in corruption occur.[8]

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 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 rent positive extraction.

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[7]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.

[8]The model is flexible enough to incorporate yardstick competition, similar to Besley and Case (1995), as an alternate mechanism to norms. The predictions of the yardstick competition scenario, however, do not fit the data as well as the social norms model. An analysis of our model with yardstick competition as a source of inter-community spillovers is available from the authors upon request.

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 θ 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 $\mu_\theta = 0$. θ is known only to the politician. *Ceteris paribus*, a politician with a higher motivation θ 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 θ, i.e., their social reputation among their peer group. We assume that incumbents in different communities can 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.[9] Following Benabou and Tirole (2011), Besley et al. (2019), and Jia and Persson (2021), among others, we model a politicians' 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 of weight that a politician puts on her social motive, an office holder's preferences can thus be summarized by

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

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

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

or equivalently,

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

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 ($r = 1$) over the corrupt action ($r = 0$); we have $P(r = 1) < P(r = 0)$ and $E[\theta|r = 1] < E[\theta|r = 0]$.[10]

Voters

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[9] 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.

[10] We will verify this below. The only case where the re-election probability would not be monotonically decreasing in r would be a situation where the incumbent's strategy is non-decreasing (or non-monotone) in θ, which can be ruled out by a simple revealed preference argument.

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 voters 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 δ 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 ψ 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 δ and the expected pecuniary payoff she can generate in period 2 is larger than that of a challenger. The latter's intrinstic 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 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 $I \in \{\varnothing, r\}$, where $I = r$ if an audit took place in their community and $I = \varnothing$ otherwise. For a given anticipated strategy $\hat{r}(\cdot|\theta)$ of the incumbent, voters use their knowledge of y and I to form rational expectations of the politician's type θ. Denoting by p the probability that $s = 1$, the expected period-2 payoff of a voter when voting for the incumbent of popularity δ 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

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

By choosing r, the candidate determines the distribution of y and voter information I (if there is an audit), taking as given $\hat{r}$. 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 $P(r) = E_{y,I}\left[1 - \Phi(\underline{\delta}(y, I, \hat{r}))|r\right] \in (0,1)$, 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 revealed themselves to be corrupt ($\theta < b$) 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

$$\theta^* = b - (1 - \mu)\left(P(r = 0) - P(r = 1)\right)(R + b) - \mu\left(E\left[\theta|r = 0\right] - E\left[\theta|r = 1\right]\right), \tag{3}$$

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 θ^* 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 θ^*. At the same time, θ^* 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 $I = \varnothing$. If $y = 0$, voters know for sure that $\theta < b$. Hence, the incumbent is re-elected if and only if her popularity δ satisfies $p + \delta \geq p + (1 - p)(1 - G(b))$ or

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

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

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

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

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

where the subscript $i = N$ stand 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

$$P_N(r, \theta^*) = \begin{cases} 1 - p\Phi(\psi\underline{\delta}_N^1(\theta^*)) - (1 - p)\Phi(\psi\underline{\delta}^0) & < \frac{1}{2} \quad \text{if } r = 1 \\ 1 - \Phi(\psi\underline{\delta}_N^1(\theta^*)) & > \frac{1}{2} \qquad \text{if } r = 0. \end{cases} \tag{6}$$

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

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

confirming that considerations of electoral success motivate politicians to behave pro-socially. For future reference, note that Δ_N^R is increasing in θ^*, as $\underline{\delta}_N^1(\theta^*)$ is decreasing in θ^*. 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 θ^*, Δ_N^R is also increasing in the sensitivity ψ 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 ϕ 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 reelection 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 superscript $i = A$ indicates an audit. The probability of re-election is

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

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

Observation 1. *For all values of θ^*, $P_A(r = 1, \theta^*) < P_N(r = 1, \theta^*)$ 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

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

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 the behaving (un-)ethically, as one would expect. Again, the expected electoral gain from a non-corrupt action is increasing in θ^* — 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 ψ.

3.1.3 Social Norms

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 Benabou and Tirole (2011), we formalize this idea as follows. Let $\tilde{\theta}$ be the average cutoff θ 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 $E[\theta|\theta < \tilde{\theta}]$.[11]

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

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

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 $\Delta(\tilde{\theta})$ 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 $\Delta'(\tilde{\theta})$.

Our assumptions on $G(\theta)$ ensure that Δ^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). Now suppose $\tilde{\theta} > 0$, i.e. the majority of office holders behave unethically. In this case, the effect on the honor dominates the effect of the stigma—if 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. Hence, $\Delta^{S'} > 0$ for high values of $\tilde{\theta}$ and individual and social motives are substitutes. Intuitively, the social motive leads more politicians to reject the (corrupt) norm in order to have themselves perceived as exceptionally good citizens. Conversely, if 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, $\Delta^{S'} < 0$ for low values of $\tilde{\theta}$ and individual and social motives are complements. The social motive leads politicians to 'conform' to the norm.

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) using (10), the equilibrium cutoff θ^* 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". 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 ($\Delta^{S'} < 0$). We follow Benabou and Tirole (2011) to assume that the social multiplier is positive, which implies that social motives do not result in multiple equilibria.[12]

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[11]Recall that r is observable in the peer group, but not θ. 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.

[12]In either event, we find in our data that the majority of incumbents corrupt, so $\theta^* > 0$. This would imply $\Delta^{S'} > 0$ and

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 α_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 $i \in \{A, N\}$,

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

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 (θ_A^*, θ_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 θ^*, 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 social norms.[13]

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.[14] 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 α_A. In the short run, the effect on $\tilde{\theta}$ is simply $\partial \tilde{\theta}/\partial \alpha_A = \theta_A^* - \theta_N^* > 0$. In the long-run, θ_A^* and θ_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 the short-term because our empirical analysis only pertains to short-run responses of politicians (within two years of their electoral cycle).

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ensure uniqueness. See Section 4 for more details.

[13]As mentioned earlier, yardstick competition may be a plausible alternate channel through which spacial spillovers may occur. See Appendix for an extension of the model to allow for yardstick competition. While yardstick influences the voting behavior of voters, the social norm 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 6 for a detailed discussion.

[14]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.

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 elsewhere audits can decrease or an increase in 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 ψ.

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 ψ. 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 $\psi \to 0$, the spillover effects would be largest because only the social norm motive would operate ($\Delta_i^R \to 0$).

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.[15] 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.

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.[16]

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* reelection probabilities is, however,

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[15]We find new incumbents to be much more sensitive to the electoral effects of timely audits [see Table B.2 in the Appendix].

[16]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.

indeterminate. Formally, using the fact that $r(\theta) = 1$ if and only if $\theta \le \theta_i^*$, the ex ante re-election probability of an office holder can be written as

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

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 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 a pro-social type drops as more incumbents take the non-corrupt action make which tends to diminish electoral chances. It 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, θ_i^*.[17] To see how this translates into re-election rates, we can take derivatives of (12) with respect to θ_i^*:

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

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 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, $\partial\underline{\delta}_i^1/\partial\theta_i^* < 0$, $i = N, A$.[18] 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 ψ: the more sensitive voters are to observed outcomes (or incumbent behavior), the larger the effect on re-election rates. Given our interpretation of

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[17]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.

[18]See Appendix A for a formal derivation.

incumbency as a proxy for ψ, 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 θ_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,*

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

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 $\psi \to 0$, 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.

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 social norms, we have.[19]

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

Our sample is comprised of all 78 municipalities in Puerto Rico, over 7 election years from 1998 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 a municipality in a given election year. For each observation, we have information on municipal audit reports, election outcomes, mayor characteristics, municipality characteristics.

4.1 Municipal Audit Data

Our central data sources are the municipal audit reports published by the OCPR between 1987 and 2014.[20] Each report contains a list of findings and a description of each finding, which entails a detailed explanation of a situation, the individuals involved (if identifiable), and the reason why it is considered a finding. Officially, the OCPR cannot classify findings as instances of corruption, however.

Bobonis et al. (2016) undertake a comprehensive coding process to specify whether a finding constituted corruption, defined as "an act by any municipal employee that led to a *personal* financial or political benefit."[21] 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 it by the number of reports issued. They also construct a separate index of corrupt 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.

However, not every municipality is audited in the four year period around an election in every cycle, especially prior to the 2000s. As a result, there are cases where we have no data on outcomes of corrupt violations. Our data captures roughly 86 percent of all municipalities over this period of time that received an audit within the relevant time frame. We distinguish between two channels through which audits affect municipalities. First, because we have data on the publication date of each audit report, we are able to tell whether or not each municipality itself underwent a 'timely' audit, i.e. in the two year period prior to an election. Our focus on timely audits is explained in detail below. In addition to this binary indicator of direct exposure to audits, we also construct a measure that allows us to capture any external effects of

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[19]These spillover results hold regardless of whether 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, the comparative statics still hold

[20]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.

[21]See their paper for a more detailed description of the coding process.

audits. Using spacial location of municipalities, we define "adjacent timely audit exposure" as the share of municipalities adjacent to the home municipality that experience a timely audit.

We present descriptive statistics of these audit outcome variables in Table 1, overall and by timely/untimely audit status. Sixty-four percent of audit reports contain at least one corrupt violation. Thus, the majority of municipalities seem to experience corruption. We measure 1.18 corrupt violations per report on average with substantial variation (standard deviation = 1.64). Approximately 38 percent of these findings (0.45 violations) are attributed directly to the mayor or vice-mayor. These estimated differences in Column 4 are consistent with the previous literature and our theoretical model: municipalities with timely audits seem to exhibit lower levels of corruption. 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. On average, a municipality should expect to see 60 percent of its adjacent municipalities undergoing a timely audit. The median of this measure is approximately two-thirds. Interestingly, we observe substantial differences across timely and untimely audits in terms of the scale of these audits; audits appear to be longer and consist of more individual reports when they are timely. However, as Bobonis et al. (2016) argue, these differences would only bias our estimates in the opposite direction.

4.2   Other Data Sources

To connect audits to electoral success, 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 know 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. To account for underlying variation in municipal characteristics, we rely on 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, and with a college education or more, as well as the municipality's household median income and poverty ratio for 1989. Finally, we use information on municipality-level annual unemployment rates from the P.R. Department of Labor.

Table 2 contains descriptive statistics for these electoral outcome and municipal control variables. From the electoral outcomes in Panel A, we note the interesting fact that both incumbent mayor re-election rates (conditional on running) and overall party success rates are quite low, at 28 and 34 percent, respectively. This may due to strong party popularity effects at play in state level politics. Yet, once we account for several cases of mayors who govern for very long periods of time (recall that there are no term limits), these statistics seem more indicative of a great heterogeneity in turnover rates of incumbent mayors and political parties.

Panel B reports other political characteristics of incumbent mayors running for re-election. Our sample of mayors is split 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 current governor 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.[22] Moreover, the average incumbent mayor's win margin in the previous election is 3 percentage points. We capture this heterogeneity in seat

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[22]Several mayors in the sample (e.g., in the municipalities of Bayamón, Carolina, and Manatí) have been in office for five or more terms.

safety by constructing a summary measure of party incumbency advantage: an indicator variable equal to one if the party has controlled the mayoral seat for the past three terms and zero otherwise. Based on this measure, a significant proportion of seats (57 percent) have a strong party incumbency advantage.

5 Methodology

5.1 Research Design

We exploit the disciplining effects induced by the variation in timeliness of municipal audits and their geographic location in the network of municipalities to help us establish the direct and external effects of the audits on corruption and incumbent re-election rates. In doing so, we draw on previous work by Bobonis et al. (2016), who 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.[23] Because municipalities are audited in a fixed order, established long ago in the past, their timing is plausibly exogenous, although politicians are able to anticipate them and adapt their behavior accordingly. Therefore, timely vs. untimely audits measure variation in *anticipated* monitoring that enables us to measure its disciplining effects on each municipality's level of corruption. Standard models of political agency predict—and existing evidence shows—that the short-run disciplining effects of monitoring lead to large decreases in municipality-level corruption (Bobonis et al. (2016)).

To illustrate these short-run disciplining effects of audits, Figure 1 plots the average number of corrupt violations per report during each of the previous two election years, and in the following election year, for municipalities audited around election year t. We show the trends separately for municipalities with timely (solid red line) and untimely (dashed green line) audits. Panel A is based on the total number of violations per report in the audit, whereas Panel B uses only the number of violations attributed to the mayor or vice-mayor. 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 1.35 and those attributed to the mayor or vice-mayor around 0.56—and the differences are statistically indistinguishable from zero. In contrast, around election year t there is a stark difference of 1.34 ($= 2.00 - 0.66$) violations per report. A similar pattern holds for the number of violations attributed to municipality-level executives ($0.56 = 0.80$–0.24). Finally, comparing these groups of municipalities in the following election year, $t+1$, we find that the difference in corruption levels decreases dramatically. Although this difference for mayor/vice-mayor corruption is still statistically significant, when we measure all violations the difference between the these two groups is statistically indistinguishable from zero, suggesting that the direct disciplining effects of the audits are short-lived.

In order to measure cross-municipal external effects of timely audits, we exploit variation in the share of neighboring municipalities who are subject to a timely audit during an incumbent's term in office—adjacent timely audit exposure. Since timely audits reduce contemporaneous levels of corruptions, 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 spillover effects that we observe and that we explain in our formal model

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[23]For a rationale of the two year period, see Bobonis et al. (2016). When municipalities have audit reports published in both periods, we aggregate only those reports published before the election and assign them to the timely audit group. This is to analyze the effects of the information available to voters strictly at the time of the election on incumbent politician's electoral outcomes. Results on subsequent audits use all available reports and the results are robust to alternate aggregation methodologies.

through changes in the social norm of corruption among neighboring municipal governments.

5.2 Econometric Methodology

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:

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

where $c_{m,t}$ denotes the number of corrupt violations per report in municipality m around election year t. $\boldsymbol{A}_{m,t}^{(-m)}$ 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.[24] The terms ρ_m and γ_t represent municipality and election intercepts respectively, and $\epsilon_{m,t}$ denotes unobserved characteristics that determine our measure of corruption at time t. Under the assumption that $\{A_{m,t}, \boldsymbol{A}_{m,t}^{(-m)}, X_{m,t}\}$ are strictly exogenous, the coefficients β and α are consistent estimates of the average external and direct effects, respectively, of timely audit dissemination on contemporaneous levels of reported corruption. This exogeneity also gives our empirical results a causal interpretation. We thus use variation in the timeliness of audits within sets of municipalities over time to identify the effects of interest.

Recall from our theoretical model that the direct effect of a timely audit in the home municipality is negative: $\alpha < 0$. Moreover, the external effect of timely audits in neighboring municipalities—now represented by β—depends on the prevailing social norm: whether the majority of politicians in the incumbent's peer group is corrupt or not. Based on the general pervasiveness of clientelism in Puerto Rican politics (Bobonis et al. (2021)), as well as average levels of municipal government corruption observed from our audit data (Table 1, Panel A), it seems most likely that the majority of politicians 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.

We further investigate heterogeneity in external effects by whether the incumbent mayor was in their first electoral term (new incumbent). To do this, we augment model 14 with an interaction term:

$$c_{m,t} = \beta_1 \boldsymbol{A}_{m,t}^{(-m)} + \beta_2 (\boldsymbol{A}_{m,t}^{(-m)} \cdot N_{m,t}) + \alpha A_{m,t} + \lambda X_{m,t} + \gamma_t + \rho_m + \epsilon_{m,t} \tag{15}$$

where $N_{m,t}$ is an indicator for whether the mayor is a new incumbent and is part of controls $X_{m,t}$. Following our theoretical model, new incumbents are likely more responsive to electoral concerns over concerns of social norms. Thus, the model predicts $\beta_2 < 0$.

The setting 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. We estimate models 14 and 15 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.[25]

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

[25]When the dependent variable is $c_{m,t+1}$, we also control for whether there was a timely audit within the municipality in the

Our causal interpretation of these effects rests on the presumed exogeneity of timely audits. Our estimates would be biased if mayoral and municipal characteristics varied significantly between timely and untimely audited municipalities. The estimated differences in our measures of these variables across timely and untimely municipalities (Column 4 of Table 2) provide evidence of balance on a large majority of these variables. However, we do find more timely audits to take place in municipalities governed by a PNP mayor and where the incumbent mayor is more experienced. Similarly, we check for correlations of these variables with our measure of adjacent timely audit exposure in Column 5. While we find no correlation with any of our measures of mayor characteristics, we do observe significant correlations with several of our socio-economic variables. Generally, municipalities with better socio-economic conditions are likely to have more exposure to adjacent municipalities with a timely audit. However, in ancillary analyses, we find none of these variables to be significant determinants of corruption (not reported). Nevertheless, we ensure to include all of these variables as controls throughout our empirical analysis.

Perhaps more important for our analysis of spillover effects is the question of whether audits are significantly spatially correlated. If this were the case, then it would be more difficult to discern the root cause of our observed spillover effects. While they could be caused by changes in neighboring corruption levels, it would also be possible that they are simply driven by correlation with home audits. However, this does not seem to be the case. We observe no significant correlation of these audit characteristics with the share of adjacent municipalities with a timely audit (Panel B of Table 1).[26]

6 Results

6.1 Effects of Timely Audits on Corruption

Table 3 presents our estimation results of equation (14). All specifications include our set of municipality controls as well as election year and municipality fixed effects.

The baseline specification in Column (1) reports the spillover effects from timely audits in adjacent municipalities on the number of corrupt violations in the home municipality, as well as the direct effects of timely audits within the municipality. Column (4) reports analogous estimates for the number of corrupt violations attributed to the mayor or vice-mayor. Our results are also robust to using the sub-sample of municipalities in which mayors are running for re-election (not reported).

Consistent with our model and expectations, the estimates of the direct effects document a systematic reduction in the number of corrupt violations in the municipality as a result of timely audits. Column (1) shows an estimated 1.34 (67 percent) fewer reported corrupt violations in municipalities with timely audits compared to those whose audit reports were published after the election. Similarly, we estimate 0.56 (70 percent) fewer corrupt violations per report by the mayor or vice-mayor [Column (4)], indicating little difference in how mayors and other municipality employees react to an anticipated audit. These direct effects are precisely estimated and stable across all specifications.

Our estimates of the spillover effects suggest substantial positive external effects of exposure to timely audits in neighboring municipalities on the level of corruption at home, although those are not as precisely estimated. To illustrate the size of the externality, compare a municipality where no neighbouring municipalities undergo a timely audit to a municipality where the median share of neighbours $\left(\frac{2}{3}\right)$ have

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subsequent electoral term.

[26]Moreover, in Appendix Table B.1 we confirm that adjacent audits seem to affect neither the presence of home audits nor the number of reports.

a timely audit. The point estimate of the spillover effect on the overall number of corrupt violations – although imprecisely estimated – implies that exposure to timely audits in the median share of adjacent municipalities leads to an increase in reported corruption levels by 0.20 violations ($= 0.30 \times \frac{2}{3}$), or 10 percent relative to the control group mean [Column (1)]. The spillover is larger and more precisely estimated when we focus on the violations attributed to municipal government leaders: an analogous increase in the exposure to timely audits in neighboring municipalities leads to an increase in 0.27 corrupt violations ($= 0.40 \times \frac{2}{3}$), or 33.8 percent relative to the control group mean [(Column (4)]. Thus, the estimated increase in corrupt behavior at home in response to lower corruption among neighbouring municipalities (triggered by audits) is three times larger when we restrict attention exclusively to the municipal government's leadership. The above finding of no such differences in the estimated responses to a home audit suggests that the spillover effect is concentrated among mayors and vice mayors. Since the social motive involves a comparison to one's direct peers, we would expect the municipal leadership to be more sensitive to the behavior of their counterparts in other municipalities than the rest of the government. We therefore view this piece of evidence as consistent with the notion that social norm play a role in the decisions of officials in leadership positions.

In addition to the baseline specifications, we investigate whether the spillover effect from lower corruption in adjacent municipalities differs between home municipalities with first-term incumbent mayors and those with long-term incumbents. Columns (2) and (5) report estimates of equation 15. We find some evidence that municipalities with politicians who have been in office for more than one term are affected more by the spillovers: in those municipalities, a change from zero to median exposure to adjacent timely audits leads to an increase in reported corruption levels by 0.47 violations ($= 0.71 \times \frac{2}{3}$), a 23.5 percent increase relative to the control group mean [(Column (2)]. As before, the point estimate is larger and more precisely estimated if we only look at corrupt violations attributed to the municipal government leadership: the point estimate implies an increase in 0.41 corrupt violations, or 51.3 percent relative to the control group mean [(Column (5)]. In contrast, the point estimates on the interaction term for new incumbents imply a smaller and insignificant spillover effect in corruption. These results are robust to allowing the direct effect to vary by incumbency as well [Columns (3) and (6)]. Indeed, if we look at all violations, the point estimate is negative, suggesting a positive relationship between corruption elsewhere and at home for first-term mayors.[27] This observed heterogeneity is consistent with the theoretical model, which predicts a greater responsiveness to social norms for long term incumbents who are less sensitive to electoral concerns.

6.2 Effects of Timely Audits on Re-election and Subsequent Corruption

We now turn to the short-run relationship between the audit program and electoral accountability – i.e., incumbent mayors' re-election rates. Recall that the theoretical trade-off between the selection effect and the incentive effect of audits prevents unambiguous predictions for this outcome variable in our model; this is true both with respect to the direct effects of audits and the associated lower levels of corruption, and with respect to any spillover-effects.

In Table 4, we report 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. The point estimate on the effect of timely audits on incumbent mayors' re-election rates is positive but not significantly different from zero [Column (1)]. We also find that the increased corruption driven by the spillover effect is not associated with an increase in the incumbent mayors' electoral accountability. If anything, our results suggest the opposite: the point estimate of the spillover effect implies that median exposure to adjacent timely audits

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[27]We will discuss this finding further in Section 8 below.

leads to an *increase* in the incumbent mayor's re-election rate by 6.5 percentage points ($= 0.097 \times \frac{2}{3}$), or 25.7 percent relative to the control group mean.

We also explore the heterogeneity in the spillover effects of the timely audits by degree of incumbency of the mayor in office. The theoretical model has no clear-cut implications for this case either: while long-term office holders are predicted to react more strongly to the spillover generated through the social norms channel, they are by definition safer in office and we would expect their electoral chances to be less sensitive to behavioral changes triggered by audits at home or elsewhere. The opposite is true for new incumbents. In the specification of Column (2), we interact the spillover exposure measure with an indicator for whether the mayor is a newly elected incumbent. We find that incumbent mayors don't experience any change in electoral success rates in response to spillovers, although we saw earlier that their corrupt activity increases [Table 3]. This finding is consistent with the fact that we use the length of incumbency as a measure to proxy for the sensitivity of electoral chances to politicians' behavior. In contrast, we observe an increase in electoral chances positive rates in municipalities whose incumbent mayors are in their first term in office: median exposure to adjacent timely audits leads to a substantial increase in the re-election probability of new incumbents by 14.5 percentage points, a 57.3 percent increase relative to the control group mean [Column (2)]. We also note that this increase could be the driving force behind the point estimate in Column (1), and will discuss the finding further below.

We now briefly turn to our estimates of the effects of timely audits on subsequent corrupt violations in Columns (3) through (6). Although the sign of our point estimates for both the direct and external effects across all specifications are positive, none are precisely estimated. While previous work (Bobonis et al. (2016)) has already shown the direct effect to be short lived, it now seems that the spillover effect is short-lived as well.

In summary, we find not only a consistent negative direct effect of timely audits on contemporaneous reports of corrupt violations, but also substantial evidence of an external effect of exposure to timely audits in adjacent municipalities – suggesting that efforts to curb corruption have spill-over effects. If we restrict ourselves to corrupt behavior among the leadership, our point estimates for these spillovers are positive, i.e. corruption increases at home. All of those results hold only in the short-term, however. Further, we find that the spillovers are primarily observed for long-term incumbents, who we posit to pay relatively less attention to how their behavior influences re-election chances and, by extension, pay relatively more attention to social norms. Finally, while experienced incumbents face no electoral penalty for their increased corruption, new incumbents' electoral prospects appear to increase as a result of more audits and correspondingly lower corruption in adjacent municipalities.

7 Robustness Checks and Alternatives to Social Norms

In this section, we present some additional estimations designed to explore alternative theoretical explanations of the patters we observe in the data, notably yardstick competition and displacement of corrupt activities. We also seek to rule out models of politician learning and behavioral models of salience.

7.1 Heterogeneous Effects of Audits by Electoral Competitiveness

According to our theoretical model of social norms, we expect the spillover effect of timely audits to be more pronounced for incumbents who place less weight on (are less sensitive to) re-election concerns. At the same time, the direct effect of home audits should be smaller for those politicians. Above, we test these hypotheses by separating new and old incumbents. As a robustness check, however, similar effects should

also be visible for office holders in municipalities where electoral competition is less intense because the ruling party enjoys an incumbency advantage. Using party incumbency advantage as an alternative proxy for electoral sensitivity to office holder behavior, we estimate:

$$c_{m,t} = \beta_1 \boldsymbol{A}_{m,t}^{(-m)} + \beta_2 (\boldsymbol{A}_{m,t}^{(-m)} \cdot I_{m,t}) + \alpha_1 A_{m,t} + \alpha_2 (A_{m,t} \cdot I_{m,t}) + \kappa I_{m,t} + \lambda X_{m,t} + \gamma_t + \rho_m + \epsilon_{m,t} \qquad (16)$$

where $I_{m,t}$ is an indicator for whether the incumbent's party won the previous 3+ elections in municipality m as of election year t. All other variables are defined as in model 14.

The corresponding coefficients are reported in Table 5 and are too imprecisely estimated to draw any definite conclusions. However, our findings are consistent with the predictions, suggesting that the reduction in corruption negative as a direct effect of timely audits is smaller in municipalities with party incumbency advantage (lower electoral competition). Although imprecisely estimated, the point estimates suggest the direct effect of timely audits on corrupt violations in municipalities with incumbency advantage are 28 percent smaller than the estimated effect in those without incumbency advantage. This difference is even wider—51 percent—and much more precisely estimated for corrupt violations attributed to the the mayor or vice mayor.With regard to the estimated spill-over effect of audits elsewhere, we continue to find a positive coefficient (corruption increase at home), though we do not observe the size of the spillover to vary with party incumbency.

The above specification also provides us with a distinct opportunity to test the validity of our social norms model against a model of yardstick competition (Besley and Case (1995)), however. Specifically, we can check whether timely audits in neighboring municipalities influence the incumbent politician's behavior through their effects on *voter information* (yardstick), or through observed changes through the incumbent's peer group (the social norm). From the results in Table 5, we observe that timely audits have a stronger direct effect on corruption—especially mayoral corruption—when there is no incumbency advantage. Thus, timely audits within these particular municipalities should have a stronger influence on the social norm. Therefore, to test for distinct responses to the social norm channel, we can examine separately the effects of timely audits in adjacent municipalities with and without incumbency advantage. If social norms is the principal mechanism behind these spillover effects, then we should observe a stronger response to audits in adjacent municipalities with no incumbency advantage. Conversely, if our main results were driven chiefly by yardstick competition, then this heterogeneity of direct effects of timely audits on corruption in neighboring municipalities should not matter.

We examine how the external effects of corruption vary based on the level of electoral competition in neighboring municipalities with model 17:

$$c_{m,t} = \beta_1 \widetilde{\boldsymbol{A}}_{m,t}^{(-m)} + \beta_3 \widehat{\boldsymbol{A}}_{m,t}^{(-m)} + \alpha A_{m,t} + \lambda X_{m,t} + \gamma_t + \rho_m + \epsilon_{m,t} \qquad (17)$$

where $\widetilde{\boldsymbol{A}}_{m,t}^{(-m)}$ represents the number of neighboring municipalities with timely audits and party alternation: essentially the opposite of incumbency advantage, this is an indicator for whether the incumbent's party lost at least one of the previous 3 elections in municipality m as of election year t. $\widehat{\boldsymbol{A}}_{m,t}^{(-m)}$ represents the number of neighboring municipalities with timely audits and party incumbency advantage. In other words, β_1 represents the spillover effect of adjacent timely audits that have a greater effect on the social norm, and β_3 represents the spillover effect of adjacent timely audits that have a lesser effect on the social norm. It is important to note how this specification differs from previous models in that we use the number of timely audits in certain adjacent municipalities as opposed to, for instance, shares of adjacent municipalities with a timely audit that do or do not have party alternation. This is to avoid dividing by zero in cases

where no adjacent municipalities experience a timely audit. Thus, in addition to the municipality and mayor characteristics defined in model 16, controls $X_{m,t}$ include the number of neighboring municipalities with party alternation $(X_{m,t}^1)$ and the number of neighboring municipalities with party incumbency advantage $(X_{m,t}^3)$, effectively normalizing our main variables of interest. All other variables are defined as in model 14.

In addition, we are still interested in whether the previously observed heterogeneity in external effects by home incumbency still holds in this analysis. Thus, in model 18 we allow these external effects to vary based on both the level of electoral competition in neighboring municipalities and whether the own-municipality mayor was a new incumbent:

$$
\begin{aligned}
c_{m,t} = {} & \beta_1 \widetilde{\boldsymbol{A}}_{m,t}^{(-m)} + \beta_2 (\widetilde{\boldsymbol{A}}_{m,t}^{(-m)} \cdot N_{m,t}) + \beta_3 \widehat{\boldsymbol{A}}_{m,t}^{(-m)} + \beta_4 (\widehat{\boldsymbol{A}}_{m,t}^{(-m)} \cdot N_{m,t}) \\
& + \alpha A_{m,t} + \lambda X_{m,t} + \gamma_t + \rho_m + \epsilon_{m,t}
\end{aligned}
\tag{18}
$$

where $N_{m,t}$ is an indicator for whether the own-municipality mayor was a new incumbent. In addition to the controls defined in model 17, $X_{m,t}$ also includes interaction terms $(X_{m,t}^1 \cdot N_{m,t})$ and $(X_{m,t}^3 \cdot N_{m,t})$.

We report the results of equations 17 and 18 in Table 6.[28] Across all specifications, we find the external effect of timely audits to be strongest when the adjacent municipality experiences party alternation. Conversely, the point estimates of the external effects from adjacent municipalities with incumbency advantage are remarkably small. With p-values of 0.041 for all violations (Column 1) and 0.126 for violations from municipal leadership (Column 2), we can reject the null hypothesis of homogeneous effects by level of neighboring electoral competition. When we allow for further heterogeneity by home incumbency, however, our estimates become too noisy to draw any confident statements. Interestingly, for mayor/vice mayor violations in Column 4, the heterogeneity by levels of neighboring electoral competition only exists for new incumbent mayors (p-value = 0.080). [29] Overall, we take this concentration of external effects in neighboring municipalities with party alternation—where timely audits have a greater influence on the social norm of corruption—as suggestive evidence that social norms, not political yardstick competition, is indeed the chief force driving our main results.

7.2 Heterogeneous Effects of Audits by Party Networks

Another possible explanation for our results lies in the spacial 'displacement' of corrupt activities.[30] Specifically, it could be the case that corruption is spilling over to adjacent municipalities because corrupt agents move from audited communities to un-audited communities in order to avoid detection. To account for displacement, a natural test is to check how the spillover effect varies with the presence of a home audit: there should be less displacement of corruption when the home municipality faces a timely audit as well. Unfortunately, such a test would also inevitably capture the disciplinary effect of increased monitoring in the home municipality. An alternative way to analyze displacement – which is also perhaps more appropriate in our political context – is to focus on displacement based on party networks. For instance, one form of corruption is the overcharging of private firms by the municipal government as a form of patronage in exchange for political favors from that political party. If the incumbent politician in one municipality knows that she is to be subject to a timely audit, she is more likely to avoid engaging in this corrupt behavior with the firm. Thus, in an effort to obtain the same political favors from the party, the firm

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[28]We report full results including coefficients on the controls for number of audits $(X_{m,t}^1, X_{m,t}^2, \text{etc.})$ in Appendix Table B.3.

[29]In Appendix Table B.4 we also find these effects to be dampened by the presence of a timely audit in the home municipality.

[30]Such an explanation is common in the context of studying the effects of policing and "hot spot" monitoring on crime. See, for instance, Dell (2015) and Blattman et al. (2021).

turns to adjacent municipalities—where the incumbent is from that same party—to engage in these corrupt activities. Therefore, according to this displacement theory, we should observe the spillover effect to be strongest when the home incumbent is of the same party as the neighboring incumbent facing an audit.

Accordingly, we examine how the spillover effects vary based on party networks by examining separately the effect of timely audits based on own-party versus opposing-party networks in model 19:

$$c_{m,t} = \beta_1 \boldsymbol{A}_{m,t}^{p(-m)} + \beta_3 \boldsymbol{A}_{m,t}^{o(-m)} + \alpha A_{m,t} + \lambda X_{m,t} + \gamma_t + \rho_m + \epsilon_{m,t} \tag{19}$$

where $\boldsymbol{A}_{m,t}^{p(-m)}$ measures the share of adjacent party-aligned municipalities with a timely audit, and likewise with $\boldsymbol{A}_{m,t}^{o(-m)}$ for municipalities where the incumbent is of a different party. All other variables are defined as in model 14. As usual, we also run a regression interacting these two terms with an indicator for new home incumbent.

We report these results in Table 7.[31] Our findings of positive spillover effects generally hold irrespective of party connections. While our point estimates seem to be larger and more precisely estimated when neighboring incumbents are of the same party, the estimated effect on the complementary group is too imprecise for us to discern any statistically significant difference in effects when we consider all sources of corruption (Columns 1 and 2). When we focus on corrupt violation by the mayor/vice mayor, our estimates for municipalities of a different party do become more precise, yet the lowest p-value we observe in our test of homogeneous effects is still very high: 0.31 (Column 3). In regards to heterogeneity across new and experienced home incumbents, although only precisely estimated in one instance (Column 2), the signs of our point estimates still suggest the spillover effects to be most concentrated among experienced incumbents. When we allow for this further dimension of heterogeneity, we still do not observe significant differences in effects by party connection for either new mayors or experienced mayors. Because we observe no significant heterogeneity according to party connection across any of these specifications, we confidently reject the displacement argument as a compelling explanation of our main results.

7.3   Politician Learning and Salience

As mentioned in the Introduction, our results of positive spillover effects run counter the findings of two empirical studies on anti-corruption audits in Brazil (Avis et al. (2018), Lichand et al. (2016)). These studies explain their negative spillovers through models of politician learning and salience of the audit program gleaned from neighboring audits. While we have already found there to be no concrete relation between neighboring and home timely audits (Table B.1), we also contend that such models are not applicable to our research setting. First, unlike those studied in Brazil, the Puerto Rican audits are not randomized but instead follow a fixed order. Each municipality has been audited in the past, which makes an environment with learning less likely. Moreover, each municipality knows when they are to be audited, knows if the upcoming audit will be timely, and knows the approximate time period between audits. Thus, there is little additional information about the audit program to be gained from the presence of neighboring timely audits. Second, compared to Brazil–with over 5,500 municipalities–the odds of having a neighboring municipality timely audited among Puerto Rico's 78 municipalities are much larger. Thus, neighboring timely audits are not as rare an occurrence, making it also less plausible that behavioral arguments of audit salience apply in our setting.

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[31]In Appendix Table B.5, we explore how the spillover effect differs by the two main parties, PPD and PNP. We find these spillover effects to be much stronger for the PPD. Anecdotally, we can attribute this to a lower sensitivity to social norm concerns (i.e. a lower μ) for incumbent politicians from the PNP.

8   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 and displacement through party connections 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 country 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 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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9 Figures and Tables

Figure 1: Number of Corrupt Violations Across Time by Timeliness of Audit in Election (t)
Note: The figures show the unadjusted relationship between the number of corrupt violations per report in each audit, for municipalities with timely and untimely audits around election at time (t).

Table 1: Audit Reports Characteristics

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

Note: Standard deviations of variables are reported in brackets. Differences estimated in ordinary least squares (OLS) regression models, regression-adjusted for municipality and electoral term fixed effects. Robust standard errors are clustered by municipality and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ See Table 3 for full direct and spillover effects of timely audits on audit outcomes.

Table 2: Municipality Characteristics

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

Note: Standard deviations of variables are reported in brackets. Differences estimated in ordinary least squares (OLS) regression models, regression-adjusted for municipality and electoral term fixed effects. Robust standard errors are clustered by municipality and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	All (1)	Timely audit (2)	Untimely audit (3)	Difference (adjusted) (4)	Correlation w/ adjacent timely audit exposure (5)	Observations (6)
Panel C: Pre-Audit Municipality Characteristics						
Share of population with high school education or more (1990)[a]	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)[a]	0.105 [0.040]	0.107 [0.038]	0.103 [0.043]	0.004 (0.005)	0.015 (0.010)	470
Household median income (US$1,000s) (1990)[a]	8.220 [1.769]	8.231 [1.729]	8.203 [1.837]	0.030 (0.216)	0.992** (0.407)	470
Poverty rate (1990)[a]	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			

Note: Standard deviations of variables are reported in brackets. Differences estimated in ordinary least squares (OLS) regression models, regression-adjusted for municipality and electoral term fixed effects (*a* = except for cross-sectional variables, which are regression-adjusted only 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 3: Direct and Spillover Effects of Timely Audits on Contemporaneous Corruption

| | Dependent variables: Number of corrupt violations per report | | | | | |
| | All | | | Mayor/Vice-Mayor | | |
	(1)	(2)	(3)	(4)	(5)	(6)
β_1: Adjacent timely audit exposure	0.30	0.71*	0.68*	0.40**	0.62***	0.60***
	(0.31)	(0.37)	(0.37)	(0.17)	(0.19)	(0.18)
β_2: Adjacent timely audit exposure ×		-0.91*	-0.88*		-0.49	-0.46
New incumbent		(0.51)	(0.53)		(0.30)	(0.31)
α_1: Timely audit	-1.34***	-1.37***	-1.27***	-0.56***	-0.57***	-0.49***
	(0.18)	(0.18)	(0.24)	(0.10)	(0.10)	(0.13)
α_2: Timely audit × New incumbent			-0.23			-0.19
			(0.33)			(0.21)
λ_1: New incumbent	0.34**	0.88**	1.00**	0.21*	0.51***	0.60**
	(0.16)	(0.36)	(0.40)	(0.11)	(0.19)	(0.23)
Municipality Controls (λ)	Yes	Yes	Yes	Yes	Yes	Yes
Election Year and Municipality FE's	Yes	Yes	Yes	Yes	Yes	Yes
Spillover Effects Estimates:						
Adjacent timely audit exposure		-0.20	-0.19		0.13	0.14
among new incumbents ($\beta_1 + \beta_2$)		(0.43)	(0.44)		(0.26)	(0.27)
Δ Outcome	0 to median adjacent timely audit exposure						
(a) among all or experienced mayors	0.20	0.47*	0.46*	0.27**	0.41***	0.40***
(β_1 × median(exposure))	(0.20)	(0.25)	(0.25)	(0.11)	(0.13)	(0.12)
(b) among new incumbent mayors		-0.13	-0.13		0.09	0.09
(($\beta_1 + \beta_2$) × median(exposure))		(0.29)	(0.29)		(0.17)	(0.18)
Observations	470	470	470	470	470	470
Mean of dep. variable (untimely audits)	2.00	2.00	2.00	0.80	0.80	0.80

Note: OLS regressions. Robust standard errors are clustered by municipality and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Direct and Spillover Effects of Timely Audits on Re-Election Rates and Subsequent Corruption

	Successful re-election of incumbent mayor		Number of corrupt violations per report $(t+1)$ All		Mayor/Vice-Mayor	
	(1)	(2)	(3)	(4)	(5)	(6)
β_1: Adjacent timely audit exposure	0.097*	-0.001	0.18	0.05	0.23	0.21
	(0.052)	(0.056)	(0.47)	(0.67)	(0.33)	(0.45)
β_2: Adjacent timely audit exposure × New incumbent		0.219**		0.29		0.06
		(0.091)		(0.77)		(0.45)
α_1: Timely audit	0.031	0.037	0.16	0.17	-0.05	-0.05
	(0.027)	(0.027)	(0.21)	(0.23)	(0.10)	(0.11)
α_2: Timely audit in next electoral term			-0.64***	-0.64***	-0.33**	-0.33**
			(0.24)	(0.23)	(0.14)	(0.14)
λ_1: New incumbent	-0.458***	-0.589***	-0.52**	-0.69	-0.16	-0.19
	(0.039)	(0.063)	(0.21)	(0.50)	(0.14)	(0.31)
Municipality Controls (λ)	Yes	Yes	Yes	Yes	Yes	Yes
Election Year and Municipality FE's	Yes	Yes	Yes	Yes	Yes	Yes
Spillover Effects Estimates:						
Adjacent timely audit exposure among new incumbents $(\beta_1 + \beta_2)$		0.217***		0.34		0.27
		(0.081)		(0.52)		(0.33)
Δ Outcome | 0 to median adjacent timely audit exposure						
(a) among all or experienced mayors ($\beta_1 \times$ median(exposure))	0.065*	-0.001	0.12	0.03	0.16	0.14
	(0.035)	(0.037)	(0.31)	(0.45)	(0.22)	(0.30)
(b) among new incumbent mayors (($\beta_1 + \beta_2$) × median(exposure))		0.145***		0.23		0.18
		(0.054)		(0.35)		(0.22)
Observations	470	470	366	366	366	366
Mean of dep. variable (untimely audits)	0.253	0.253	1.71	1.71	0.69	0.69

Note: OLS regressions. Robust standard errors are clustered by municipality and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Heterogeneous Direct and Spillover Effects of Timely Audits by Level of Electoral Competition

	Dependent variables:			
	Number of corrupt violations per report			
	All		Mayor/Vice-Mayor	
	(1)	(2)	(3)	(4)
α_1: Timely audit	-1.59***	-1.62***	-0.77***	-0.78***
	(0.24)	(0.24)	(0.12)	(0.13)
α_2: Timely audit ×	0.45	0.48	0.39**	0.39**
Party incumbency advantage	(0.29)	(0.30)	(0.15)	(0.16)
β_1: Adjacent timely audit exposure	0.29	0.55	0.41**	0.47*
	(0.30)	(0.39)	(0.16)	(0.24)
β_2: Adjacent timely audit exposure ×		-0.46		-0.10
Party incumbency advantage		(0.48)		(0.30)
κ: Party incumbency advantage	0.06	0.33	-0.23	-0.17
	(0.27)	(0.32)	(0.14)	(0.20)
Municipality Controls (λ)	Yes	Yes	Yes	Yes
Election Year and Municipality FE's	Yes	Yes	Yes	Yes
Direct and Spillover Effects Estimates:				
Timely audits among municipalities with	-1.14***	-1.14***	-0.38***	-0.38***
party incumbency advantage ($\alpha_1 + \alpha_2$)	(0.22)	(0.22)	(0.13)	(0.13)
Adjacent timely audit exposure among				
municipalities with party		0.09		0.37*
incumbency advantage ($\beta_1 + \beta_2$)		(0.38)		(0.21)
Δ Outcome | 0 to median adjacent				
timely audit exposure				
(a) among all or mun.'s with alternation	0.19	0.36	0.28**	0.31*
($\beta_1 \times$ median(share))	(0.20)	(0.26)	(0.11)	(0.16)
(b) among mun.'s with party incumb. adv.		0.06		0.25*
(($\beta_1 + \beta_2) \times$ median(share))		(0.25)		(0.14)
Observations	470	470	470	470
Mean of dep. variable (untimely audits)	2.00	2.00	0.80	0.80

Note: OLS regressions. Robust standard errors are clustered by municipality and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Party incumbency advantage is a dummy variable for whether the incumbent's party won the previous 3+ elections in the municipality.

Table 6: Heterogeneous Spillover Effects of Timely Audits by Level of Electoral Competition in Neighboring Municipalities

	Dependent variables:			
	Number of corrupt violations per report			
	All		Mayor/Vice-Mayor	
	(1)	(2)	(3)	(4)
β_1: Number of timely audits in adjacent municipalities with party alternation	0.24**	0.33***	0.18***	0.19**
	(0.10)	(0.12)	(0.06)	(0.07)
β_2: Number of timely audits in adjacent municipalities with party alternation $\times$ New incumbent		-0.20		0.00
		(0.18)		(0.12)
β_3: Number of timely audits in adjacent municipalities with party incumb. adv.	-0.05	-0.03	0.04	0.10
	(0.11)	(0.14)	(0.06)	(0.07)
β_4: Number of timely audits in adjacent municipalities with party incumb. adv. $\times$ New incumbent		-0.03		-0.13
		(0.20)		(0.11)
Number of Audits in Adjacent Mun. Controls	Yes	Yes	Yes	Yes
Own Timely Audit Control	Yes	Yes	Yes	Yes
Municipality Controls (λ)	Yes	Yes	Yes	Yes
Election Year and Municipality FE's	Yes	Yes	Yes	Yes
Spillover Effects Estimates:				
Number of timely audits in adjacent municipalities with party alternation among new incumbents ($\beta_1 + \beta_2$)		0.13		0.19*
		(0.14)		(0.10)
Number of timely audits in adjacent municipalities with party incumb. adv. among new incumbents ($\beta_3 + \beta_4$)		-0.05		-0.03
		(0.17)		(0.09)
Test of homogeneous effects [p-value]:				
(a) among all or experienced incumbents (H_0: $\beta_1 = \beta_3$)	0.041	0.079	0.126	0.463
(b) among new incumbents (H_0: $\beta_1 + \beta_2 = \beta_3 + \beta_4$)		0.291		0.080
Observations	470	470	470	470
Mean of dep. variable (untimely audits)	2.00	2.00	0.80	0.80

Note: OLS regressions. Robust standard errors are clustered by municipality and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Party incumbency advantage is a dummy variable for whether the incumbent's party won the previous 3+ elections in the municipality. Controls: The number of audits in adjacent municipalities controls in columns 1-4 include (a) the number of all audits in adjacent municipalities with party alternation and (b) the number of all audits in adjacent municipalities with party incumbency advantage. Columns 3-4 also include interactions of these numbers of audits with new incumbent mayor indicators. See Appendix Table B.3 for covariate coefficients.

Table 7: Heterogeneous Spillover Effects of Timely Audits by Party Connection

	All		Mayor/Vice-Mayor	
	Dependent variables: Number of corrupt violations per report			
	(1)	(2)	(3)	(4)
β_1: Same party adjacent timely audit exposure	0.36* (0.20)	0.59** (0.26)	0.33*** (0.09)	0.42*** (0.13)
β_2: Same party adjacent timely audit exposure × New incumbent		-0.63* (0.38)		-0.23 (0.26)
β_3: Opposition party adjacent timely audit exposure	0.19 (0.24)	0.29 (0.31)	0.19* (0.11)	0.32** (0.15)
β_4: Opposition party adjacent timely audit exposure × New incumbent		-0.20 (0.36)		-0.29 (0.26)
Own Timely Audit Control	Yes	Yes	Yes	Yes
Municipality Controls (λ)	Yes	Yes	Yes	Yes
Election Year and Municipality FE's	Yes	Yes	Yes	Yes
Spillover Effects Estimates:				
Test of homogeneous effects [p-value]:				
(a) among all or experienced incumbents (H_0: $\beta_1 = \beta_3$)	0.48	0.37	0.31	0.64
(b) among new incumbents (H_0: $\beta_1 + \beta_2 = \beta_3 + \beta_4$)		0.75		0.63
Δ Outcome | 0 to median adjacent timely audit exposure				
(a) mun.'s of the same party, all or experienced inc.'s ($\beta_1 \times$ med(exposure))	0.18* (0.10)	0.29** (0.13)	0.17*** (0.05)	0.21*** (0.06)
(b) mun.'s of the same party, new incumbents (($\beta_1 + \beta_2$) × med(exposure))		-0.02 (0.14)		0.10 (0.10)
(c) mun.'s of the opposite party, all or experienced inc.'s ($\beta_3 \times$ med(exposure))	0.09 (0.12)	0.14 (0.15)	0.09* (0.05)	0.16** (0.08)
(d) mun.'s of the opposite party, new incumbents (($\beta_3 + \beta_4$) × med(exposure))		0.04 (0.15)		0.02 (0.10)
Observations	470	470	470	470
Mean of dep. variable (untimely audits)	2.00	2.00	0.80	0.80

Note: OLS regressions. Robust standard errors are clustered by municipality and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

10 Appendix A: Omitted Proofs

Existence of An Equilibrium for System (11)

We show that if

$$\lim_{\theta \to -\infty} \theta + \mu \Delta^S(\theta) < b - (1-\mu)(R+b), \tag{20}$$

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 (20) holds. We first show that a solution to the system (11) is within a non-empty, convex, and compact set. Note that since Δ^S and Δ_i^R are positive, if (θ_A^*, θ_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 (20) 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 (θ_A^*, θ_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 θ_i^* and the right-hand side is decreasing in θ_i^*, the equation (11) implicitly defines θ_i^* as a function of θ_{-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 α_A. From equilibrium condition (11), we have, for $i = A, N$,

$$\frac{d\theta_i^*}{d\alpha_A} \cdot \left[1 + (1-\mu)\frac{d\Delta_i^R(\theta_i^*)}{d\theta_i^*}(R+b)\right] = -\mu \Delta^{S'}(\tilde{\theta})\frac{d\tilde{\theta}}{d\alpha_A}. \tag{21}$$

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

$$\frac{d\Delta_N^R(\theta_N^*)}{d\theta_N^*} = -(1-p)\psi \cdot \phi(\psi\underline{\delta}_N^1)\frac{d\underline{\delta}_N^1}{d\theta_N^*} > 0, \quad \frac{d\Delta_A^R(\theta_A^*)}{d\theta_A^*} = -\psi \cdot \phi(\psi\underline{\delta}_A^1)\frac{d\underline{\delta}_A^1}{d\theta_A^*} > 0. \tag{22}$$

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 (21), 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) $\frac{d\theta_A^*}{d\alpha_A} > 0$, $\quad \frac{d\theta_N^*}{d\alpha_A} > 0$, and $\quad \frac{d\tilde{\theta}}{d\alpha_A} > 0$;

2) $\frac{d\theta_A^*}{d\alpha_A} < 0$, $\quad \frac{d\theta_N^*}{d\alpha_A} < 0$, and $\quad \frac{d\tilde{\theta}}{d\alpha_A} < 0$,

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 ψ 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 ψ in the home community is negligible to the incumbents of other communities. This implies that the right-hand side of equation (21) is independent of the home community's ψ, 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 ψ in opposite directions.

For the purpose of our study, we compare the values of $d\theta_i^*/d\alpha_A$ in the cases of $\psi \to 0$ and $\psi > 0$ only. The case $\psi \to 0$ 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 $\psi \to 0$, according to (22), we have $d\Delta_i^R(\theta_i^*)/d\theta_i^* \to 0$, because

$$\frac{d\underline{\delta}_N^1}{d\theta_N^*} = -\frac{p(1-p)^2(1-G(b))g(\theta_N^*)}{[p + (1-p)(1-G(\theta_N^*))]^2}, \qquad \frac{d\underline{\delta}_A^1}{d\theta_A^*} = -\frac{(1-p)(1-G(b))g(\theta_A^*)}{(1-G(\theta_A^*)^2}. \tag{23}$$

are both bounded. Therefore,

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

when $\psi \to 0$. 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

$$\left|\frac{d\theta_i^*}{d\alpha_A}\right|_{\psi\to0} > \left|\frac{d\theta_i^*}{d\alpha_A}\right|_{\psi>0}.$$

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 θ_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 (23).

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

$$\frac{dE[P_i(r,\theta_i^*)]}{d\alpha_A} = \frac{dE[P_i(r,\theta_i^*)]}{d\theta_i^*}\frac{d\theta_i^*}{d\alpha_A}$$

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

We compare the absolute values of $dE[P_i(r,\theta_i^*)]/d\alpha_A$ in the cases of $\psi\to0$ and $\psi>0$ only. When $\psi\to0$, since $\Delta_i^R(\theta^*)\to0$ and $d\underline{\delta}_i^1/d\theta_i^*$ is bounded, we have

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

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 Tables

Table B.1: Spillover Effects of Timely Audits on Home Audits

	Timely Audit		Number of audit reports	
	(1)	(2)	(3)	(4)
β_1: Adjacent timely audit exposure	-0.13	0.00	0.01	0.04
	(0.10)	(0.13)	(0.27)	(0.29)
β_2: Adjacent timely audit exposure		-0.29		-0.07
$\times$ New incumbent		(0.18)		(0.36)
α_1: Timely audit			0.70***	0.70***
			(0.11)	(0.11)
λ_1: New incumbent	0.02	0.19*	-0.20*	-0.15
	(0.05)	(0.11)	(0.10)	(0.21)
Municipality Controls (λ)	Yes	Yes	Yes	Yes
Election Year and Municipality FE's	Yes	Yes	Yes	Yes
Spillover Effects Estimates:				
Share of adjacent municipalities with timely		-0.26		-0.03
audit among new incumbents ($\beta_1 + \beta_2$)		(0.14)		(0.36)
Δ Outcome | 0 to median adjacent timely audit exposure				
(a) among all or experienced mayors	-0.08	-0.00	0.01	0.03
($\beta_1 \times$ median(share))	(0.06)	(0.08)	(0.18)	(0.19)
(b) among new incumbent mayors		-0.17		-0.02
(($\beta_1 + \beta_2$) $\times$ median(share))		(0.10)		(0.24)
Observations	470	470	470	470
Mean of dep. variable	0.61	0.61	1.49	1.49

Note: OLS regressions. Robust standard errors are clustered by municipality and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ The number of municipality reports and the number of consortium reports are not included as controls. Mean of dependent variable in Columns (3) and (4) is conditional on untimely audit.

Table B.2: Heterogeneous Direct Effects of Timely Audits on Re-Election by Incumbency

	Dependent variable: Successful re-election of incumbent mayor	
	(1)	(2)
α_1: Timely audit	-0.013	-0.011
	(0.036)	(0.036)
α_2: Timely audit $\times$ New incumbent	0.097	0.102*
	(0.059)	(0.059)
β_1: Adjacent timely audit exposure		0.103**
		(0.051)
λ_1: New incumbent	-0.518***	-0.518***
	(0.044)	(0.044)
Municipality Controls (λ)	Yes	Yes
Election Year and Municipality FE's	Yes	Yes
Observations	470	470
Mean of dep. variable (untimely audits)	0.253	0.253

Note: OLS regressions. Robust standard errors are clustered by municipality and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.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			
	All (1)	Mayor/ Vice-Mayor (2)	All (3)	Mayor/ Vice-Mayor (4)
β_1: Number of timely audits in adjacent municipalities with party alternation	0.24** (0.10)	0.18*** (0.06)	0.33*** (0.12)	0.19** (0.07)
β_2: Number of timely audits in adjacent municipalities with party alternation $\times$ New incumbent			-0.20 (0.18)	0.00 (0.12)
β_3: Number of timely audits in adjacent municipalities with party incumb. adv.	-0.05 (0.11)	0.04 (0.06)	-0.03 (0.14)	0.10 (0.07)
β_4: Number of timely audits in adjacent municipalities with party incumb. adv. $\times$ New incumbent			-0.03 (0.20)	-0.13 (0.11)
λ_1: Number of audits in adjacent municipalities with party alternation	-0.14 (0.15)	-0.04 (0.09)	-0.10 (0.15)	0.01 (0.09)
λ_2: Number of audits in adjacent municipalities with party alternation $\times$ New incumbent			-0.06 (0.15)	-0.12 (0.11)
λ_3: Number of audits in adjacent municipalities with party incumb. adv.	0.03 (0.16)	-0.01 (0.08)	0.01 (0.18)	-0.02 (0.09)
λ_4: Number of audits in adjacent municipalities with party incumb. adv. $\times$ New incumbent			0.06 (0.19)	0.05 (0.08)
λ_5: New incumbent	0.34** (0.16)	0.21* (0.11)	0.59 (0.42)	0.51* (0.27)
α_1: Timely audit	-1.36*** (0.18)	-0.57*** (0.10)	-1.40*** (0.18)	-0.59*** (0.10)
Municipality Controls (λ)	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)	2.00	0.80	2.00	0.80

Note: OLS regressions. Robust standard errors are clustered by municipality and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Party incumbency advantage is a dummy variable for whether the incumbent's party won the previous 3+ elections in the municipality. See Table 6 in main text for postestimation values.

Table B.4: Heterogeneous Spillover Effects of Timely Audits by Neighboring Electoral Competition and Own Timely Audit

	All		Mayor/Vice-Mayor	
	(1)	(2)	(3)	(4)
β_1: Number of timely audits in adjacent municipalities with party alternation	0.37** (0.14)	0.52*** (0.17)	0.27*** (0.10)	0.30*** (0.11)
β_2: Number of timely audits in adjacent municipalities with party alternation $\times$ New incumbent		-0.28 (0.19)		-0.04 (0.12)
β_3: Number of timely audits in adjacent municipalities with party incumb. adv.	0.03 (0.14)	0.05 (0.15)	0.03 (0.08)	0.08 (0.09)
β_4: Number of timely audits in adjacent municipalities with party incumb. adv. $\times$ New incumbent		-0.02 (0.21)		-0.15 (0.11)
μ_1: Number of timely audits in adjacent municipalities with party alternation $\times$ Timely audit	-0.22* (0.13)	-0.27* (0.14)	-0.14* (0.08)	-0.16* (0.08)
μ_2: Number of timely audits in adjacent municipalities with party incumb. adv $\times$ Timely Audit	-0.11 (0.15)	-0.11 (0.15)	0.04 (0.08)	0.06 (0.08)
Number of Audits in Adjacent Mun. Controls	Yes	Yes	Yes	Yes
Own Timely Audit Control	Yes	Yes	Yes	Yes
Municipality Controls (λ)	Yes	Yes	Yes	Yes
Election Year and Municipality FE's	Yes	Yes	Yes	Yes
Spillover Effects Estimates:				
Number of timely audits in adjacent municipalities with party alternation, with own timely audit ($\beta_1 + \mu_1$)	0.15 (0.10)	0.25 (0.12)	0.13 (0.05)	0.14 (0.07)
Number of timely audits in adjacent municipalities with party incumb. adv., with own timely audit ($\beta_3 + \mu_2$)	-0.08 (0.13)	-0.06 (0.16)	0.06 (0.07)	0.13 (0.08)
Test of homogeneous effects [p-value]: (a) with untimely own audit ($\beta_1 = \beta_3$)	0.114	0.072	0.072	0.158
(b) with timely own audit ($\beta_1 + \mu_1 = \beta_3 + \mu_2$)	0.094	0.127	0.411	0.955
Observations	470	470	470	470
Mean of dep. variable (untimely audits)	2.00	2.00	0.80	0.80

Note: OLS regressions. Robust standard errors are clustered by municipality and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Party incumbency advantage is a dummy variable for whether the incumbent's party won the previous 3+ elections in the municipality. Controls: The number of audits in adjacent municipalities controls in columns 1-4 include (a) the number of all audits in adjacent municipalities with party alternation and (b) the number of all audits in adjacent municipalities with party incumbency advantage.

Table B.5: Spillover Effects of Timely Audits on Number of Corrupt Violations by Incumbent Party in Own Municipality

	Dependent variables:			
	Number of corrupt violations per report			
	All		Mayor/Vice-Mayor	
	(1)	(2)	(3)	(4)
β_1: Adjacent timely audit exposure	0.48	0.96**	0.45**	0.72***
	(0.37)	(0.44)	(0.20)	(0.21)
β_2: Adjacent timely audit exposure $\times$	-0.38	-0.58*	-0.10	-0.25
PNP incumbent mayor	(0.27)	(0.32)	(0.17)	(0.22)
β_3: Adjacent timely audit exposure $\times$		0.49		0.35
PNP incumbent mayor $\times$ New incumbent		(0.48)		(0.31)
β_4: Adjacent timely audit exposure $\times$		-1.12*		-0.59
New incumbent		(0.61)		(0.37)
λ_1: PNP incumbent mayor	0.54***	0.61**	0.18*	0.31**
	(0.19)	(0.26)	(0.10)	(0.15)
λ_2: PNP incumbent mayor $\times$ New incumbent		-0.04		-0.22
		(0.34)		(0.24)
λ_3: New incumbent	0.35**	0.99**	0.22*	0.66***
	(0.16)	(0.40)	(0.11)	(0.20)
Own Timely Audit Control	Yes	Yes	Yes	Yes
Municipality Controls	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)	2.00	2.00	0.80	0.80

Note: OLS regressions. Robust standard errors are clustered by municipality and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$