Does Combating Corruption Reduce Clientelism?

Gustavo J. Bobonis, Paul J. Gertler,
Marco Gonzalez-Navarro, and Simeon Nichter*

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Abstract

Does combating corruption reduce clientelism? We examine the impact of a prominent anti-corruption program on clientelism using a novel representative survey of rural Brazilians. Randomized audits reduce politicians' provision of campaign handouts, decrease citizens' demands for private goods, and reduce requests fulfilled by politicians. With regards to mechanisms, audits undermine clientelist relationships by reducing citizens' interactions with politicians and their knowledge of incumbents. Furthermore, audits significantly deteriorate citizens' perceptions of politician reciprocity in a hypothetical trust game. Results also offer novel insights into audits' dynamic effects: they have more pronounced effects in the short run, especially during electoral periods. (*JEL D72, D73, H83*)

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*Bobonis: University of Toronto, 150 St. George St., Toronto, Canada, M5S3G7, e-mail: gustavo.bobonis@utoronto.ca. Gertler: University of California, Berkeley and NBER, 2220 Piedmont Ave. Berkeley, CA 94705 (e-mail: gertler@berkeley.edu). Gonzalez-Navarro: University of California, Berkeley, 216 Giannini Hall, Berkeley, CA, 94720 (e-mail: marcog@berkeley.edu). Nichter: University of California, San Diego, 9500 Gilman Drive, La Jolla, CA 92093 (e-mail: nichter@ucsd.edu). We thank Juliana Lins, Bárbara Magalhães, and Vânya Tsutsui for assistance during fieldwork; Márcio Thomé and the BemFam team for survey work; Tadeu Assad and the IABS team for project management. We are grateful for excellent research assistance by Joaquin Fuenzalida Bello, Fikremariam Gedefaw, Isabella Giancola-Schieda, Lisa Stockley, and Austin Zeyuan Zheng. We thank Horacio Larreguy, Fred Finan, Ernesto Dal Bó and numerous seminar participants for insightful comments. IRB approval was awarded by Brazil's *Comissão Nacional de Ética em Pesquisa* (Protocol 465/2011), the University of Toronto (Protocol 27432), and Innovations for Poverty Action (Protocol 525.11May-006). This project would not have been possible without financial support from AECID and the leadership of Pedro Flores Urbano. We also gratefully acknowledge funding from CAF, the Canadian Institute for Advanced Research, the Canada Research Chairs Program, the Social Sciences and Humanities Research Council of Canada (SSHRC) under Insight Grants 488989 and 493141, and the Ontario Work-Study program.

1 Introduction

Many countries face a syndrome of corruption and clientelism. Both phenomena have a host of pernicious consequences, ranging from inefficient policies to the under-provision of public goods (Olken and Pande 2012; Keefer 2007). These twin maladies are commonly understood to reinforce each other (e.g., Scott 1969, Robinson and Verdier 2013; Anderson et al. 2015, Leight et al. 2020).[1] For example, corrupt politicians often use illicit rents to fund vote buying during elections (della Porta 1997). In the Philippines and Thailand, incumbents frequently "plunder public coffers in order to fill war chests" used for vote buying (Aspinall et al. 2022). And in Brazil, numerous politicians have been arrested for these actions, including a mayor in Maranhão state sentenced to over nine years for stealing from the public school budget in order to fund clientelism.[2]

Such dynamics raise an important question: Does combating corruption also reduce clientelism? Given that international and domestic institutions expend substantial resources on fighting corruption, this question has key policy implications. Evidence of such externalities would suggest that the returns of anti-corruption efforts may be undervalued. Despite such potential benefits, this line of inquiry remains largely unexplored in the literature on various effects of anti-corruption interventions (Olken and Pande 2012; Gans-Morse et al. 2018).

To investigate this understudied question, the present study examines whether anti-corruption audits reduce the incidence of clientelism. We focus on the case of Brazil, where a federal agency conducted 2,241 audits by lottery, reaching a third of the nation's municipalities (Ferraz and Finan 2008; Avis et al. 2018). As shown rigorously by Avis et al. (2018), this intervention significantly reduced subsequent levels of corruption in audited municipalities. We exploit the randomization of audits to examine consequences for clientelism using a novel, longitudinal survey of impoverished rural households that we collected in Northeast Brazil. This large representative survey provides extensive evidence about clientelism during both electoral and nonelectoral periods. By coupling this survey with randomized audits, our identification strategy enables us to estimate causal effects of anti-corruption audits on subsequent levels of clientelism in sampled municipalities.

[1] Corruption is defined as the abuse of public office for private gain (Rose-Ackerman 2000). Clientelism is defined as the exchange of contingent benefits for political support (Hicken 2011; Kitschelt and Wilkinson 2007).

[2] "São Pedro de Água Branca — MPMA Obtém Condenação de Réus por Desvio de Dinheiro e Compra de Votos," Ministério Público do Estado do Maranhão, 5/22/2014. See also "Dinheiro Desviado Seria Utilizado em Campanha e Compra de Voto, Diz MPE," *G1 Piauí*, 7/15/2016 and "PF Descobre Compra de Votos com Dinheiro Público no Acre," *Acre Agora*, 5/25/2021.

Our study provides rigorous evidence suggesting that anti-corruption audits decrease important aspects of clientelism. We first consider local politicians' provision of campaign handouts, which legally constitutes vote buying in Brazil and leads to many removals from office (Nichter 2021). In the 2012 election year, audits decreased the provision of private goods during campaign visits by 3.0 percentage points — a remarkable decline of 51 percent. With respect to the demand side of clientelism, audits also caused a 2.9 percentage point decline in citizen requests for private goods from politicians — a substantial 21 percent reduction. Furthermore, audits led to a 3.9 percentage point decline in the prevalence of requests fulfilled by politicians — a striking 44 percent decrease.

The rich nature of our survey data also sheds light on mechanisms by which audits affect clientelism. Clientelist relationships are often strengthened when citizens interact frequently with politicians and know them well (Cruz et al. 2017; Duarte et al. 2019). Yet audits reduced citizens' regular interactions with politicians outside of campaigns by 5.7 percentage points, a marked 39 percent reduction. Also consistent with weakened relationships, audits led to a reduction in the share of respondents who reported knowing the incumbent group very well. Clientelism may also be undermined if citizens deem politicians to be unlikely to reciprocate with handouts after the scrutiny of audits. In line with this mechanism, we find that audits significantly deteriorated perceptions of politician reciprocity, as measured using a hypothetical trust game. While not dispositive, this evidence conforms with both related mechanisms: audits undermine clientelist relationships and perceptions of reciprocity.

The present study also offers novel insights into the dynamic effects of anti-corruption audits. We decompose effects by examining audits in the most recent vs past mayoral terms. Audits' effects on citizen requests peter out relatively quickly: whereas recent audits reduce requests by 4.9 percentage points, past audits have a small and insignificant effect. Similar but imprecisely estimated patterns are observed for politicians' provision of campaign handouts and fulfilled requests. Furthermore, we find that the effect of recent audits is amplified during electoral periods: they lead to a significantly larger decline in requests and fulfilled requests in the 2012 election year than in the 2013 nonelection year. Such findings suggest that anti-corruption audits have more pronounced effects in the short run, especially during electoral periods.

The present article advances the study of both corruption and clientelism. First, we contribute to research on corruption by underscoring an important yet unrecognized benefit of efforts to combat it. We identify this effect by studying one of the world's most prominent anti-corruption programs, which has been rigorously shown to have other significant electoral and policy effects (Ferraz and Finan 2008, Timmons and Garfias 2015). Second, we contribute to research on efforts to curb clientelism. Various studies examine anti-clientelism interventions, such as informational campaigns that seek to influence voters' perceptions about clientelism's costs (e.g., Blattman et al. 2019; Schechter and Vasudevan 2023). Our results suggest that a broader set of interventions, beyond those that primarily focus on clientelism, can also have important spillover effects on the phenomenon. Third, by providing empirical evidence of a link between corruption and clientelism, our study contributes to influential theoretical work that suggests interrelationships between the two phenomena (e.g., Robinson and Verdier 2013; Anderson et al. 2015). Fourth, unlike most existing studies (e.g., Hicken et al. 2018; Schechter and Vasudevan 2023), we offer evidence about clientelism not just during electoral but also during nonelectoral periods. Overall, this study offers important contributions to the political economy literature.

2 Context

2.1 Poverty and Political Clientelism in Northeast Brazil

The present study examines Brazil's semi-arid zone, which is predominantly located in the nation's Northeast region and covers over a million square kilometers.[3] The zone's 28 million residents are disproportionately poor and rural. Most are highly vulnerable to shocks, including exposure to frequent droughts.[4] Citizens face incomplete credit and insurance markets, and often do not have adequate savings to self-insure against shocks. Moreover, informal insurance is often insufficient, in part because rainfall shocks are spatially correlated. Many citizens are also vulnerable to adverse health shocks; most Brazilians do not have private health insurance (TCU 2014), and they often mention healthcare often as a most pressing problem in public opinion surveys.[5]

In this context with substantial poverty and vulnerability, many politicians deliver material benefits to citizens in contingent exchange for political support. The Latin American Public Opinion Project (LAPOP 2014) found that 10.7 percent of Brazilian survey respondents had been offered a benefit in exchange for their vote in the prior national election. Table 1 provides further evidence regarding the semi-arid zone, from our panel survey described below. During the 2012 municipal campaign, 5.9 percent of survey respondents received private goods from campaign visits to their homes. Such patterns of clientelism often extend beyond election years. For example, in rural Northeast Brazil, many

[3]At the time of our research, the semi-arid region included 1,133 municipalities in nine states: Alagoas, Bahia, Ceará, Minas Gerais, Paraiba, Pernambuco, Piauí, Rio Grande do Norte, and Sergipe.

[4]In its 2015 Index of Social Vulnerability, the nation's Institute for Applied Economic Research (IPEA) coded as "very vulnerable" the majority of the Northeast region.

[5]For example, see nationally representative surveys by Confederação Nacional da Indústria/CNI 2014 and the Brazilian Electoral Panel Study (2013).

citizens rely on ongoing clientelist relationships with mayors and city councilors who provide assistance during adverse shocks, in exchange for their political support (Nichter 2018). Furthermore, our survey suggests that incumbent politicians disproportionately engage in clientelism (Bobonis et al. 2022); this finding dovetails with broader research suggesting that politicians in office enjoy greater financial and organizational resources for clientelism (e.g., Gallego and Wantchekon 2012, Medina and Stokes 2007).

Clientelism is by no means a modern phenomenon in Brazil. Influential works emphasize its longstanding role in Brazilian politics (e.g., Ames 2002, Hagopian 1996, Nunes Leal 1949), and point to various reasons such as open-list proportional representation and high party fragmentation for its pervasiveness.[6] The present study focuses on politicians at the municipal level – i.e., mayors and city councilors, who are elected concurrently every four years.[7] These local politicians have substantial discretion and resources to engage in clientelism, in part due to Brazil's high degree of decentralization (IMF 2016). Over the past two decades, the Brazilian government has engaged in substantial efforts to reduce clientelism. Since 2000, electoral courts have removed over a thousand local politicians from office for distributing campaign handouts, making it the top reason why politicians are ousted in Brazil (Nichter 2021).

Citizen demands also play an important role in clientelism. Many citizens ask local politicians for assistance, especially but not only when they experience adverse shocks. Over 21 percent of survey respondents asked municipal candidates for private goods during the 2012 election year, as did 8.1 percent during the 2013 nonelection year. Most requests involve life necessities, such as healthcare and water, and they increase when exposed to negative shocks (Bobonis et al. 2022). About half of citizen requests were fulfilled by politicians. More specifically, 12.8 percent of respondents requested and received a private good in 2012, as did 4.8 percent in 2013.

In rural Northeast Brazil, politicians' ability to use clientelism is heightened by their ongoing relationships with constituents. For example, 13.4 percent of survey respondents conversed at least monthly with a local politician before the 2012 municipal campaign commenced, and 12.6 percent reported that they knew the incumbent mayor "very well."

[6]These two factors are understood to promote clientelism instead of programmatic appeals based on party platforms. Open-list proportional representation does so because it increases competition between candidates of the same party. Brazil's high party fragmentation does so because it weakens voters' ability to determine which of (many) party platforms align with their preferences.

[7]Municipal elections are held simultaneously across Brazil every four years, followed two years later by state/federal elections. Elections for mayor are by plurality, except in municipalities with more than 200,000 voters (which have second-round elections if no one receives a first-round majority). Mayors can serve up to two consecutive terms, with later reelection permissible. Elections for city councilors, who do not have term limits, are by open-list proportional representation.

In addition, 68.6 percent of citizens indicated that a mayoral candidate's representatives had visited their homes during the 2012 campaign. Voters interact more frequently with city council candidates, who are frequently clientelist brokers for allied mayoral candidates.[8] These interactions are indicative of an extensive political network, which often facilitates clientelism. Citizens' relationships with councilors can potentially influence mayoral voting, as 71.8 percent of respondents indicate they voted for a mayor and councilor from the same political coalition. Furthermore, there may be intra-household spillover effects, as 76.6 percent of citizens expressed that all family members cast a ballot for the same mayoral candidate. Beyond these interactions, clientelism is also facilitated by citizens' declared support. Although Brazil has electronic voting — which inhibits clientelist monitoring of vote choices — many voters circumvent this challenge for clientelism by providing a costly signal about how they will vote. More specifically, they declare support publicly for candidates with whom they have ongoing clientelist relationships (Nichter 2018). During the 2012 campaign, 48.4 percent of respondents declared support on their bodies, on their homes, or at rallies.

2.2 CGU Municipal Audit Program

The present study examines how anti-corruption audits affect clientelism. In part to reduce corruption in federal expenditures, Brazil created the *Controladoria-Geral da União* (*CGU*, or Office of the Comptroller General) in 2003. In its first year, the *CGU* launched an expansive audit program, entitled *Programa de Fiscalização por Sorteios Públicos* (Monitoring Program by Public Lotteries). From 2003 to 2015, this program randomly selected municipalities in televised lotteries and subjected them to audits of their expenditures of federal funds. Overall, the *CGU* conducted audits in 1,949 municipalities through 40 lotteries, which entailed scrutinizing more than R$22 billion of federal funds (Avis et al. 2018).

The *CGU* stratified lotteries for audits at the state level, so the probability of selection in a given lottery is constant across municipalities in a given state. The probability of being selected for an audit also varied over time, depending on fiscal resources available for the program. All municipalities with populations below 500,000 residents were eligible for these randomized audits. Upon selection in a given lottery, the *CGU* collected information on federal transfers to the municipal government during the previous three to four years, and randomly selected projects in specified sectors for auditing. The *CGU* then dispatched a team of 10-15 auditors to the municipality to scrutinize documents and physically inspect

[8] In Argentina and the Philippines, city councilors also play a role as brokers (Stokes et al. 2013, Ravanilla, Haim and Hicken 2022).

whether the projects were actually completed as described (Ferraz and Finan 2008). Upon completion, a report of audit results was centrally evaluated and then widely disseminated to the public.

As shown rigorously by Avis et al. (2018), the *CGU*'s random audits significantly reduced corruption in subsequent years. Leveraging the fact that some municipalities were selected in multiple lotteries, they find that experiencing an audit decreased future corruption by 8 percent. A key reason for this reduction in corruption is that when audits reveal corruption, politicians are subjected to both electoral and legal punishments (Ferraz and Finan 2008; Avis et al. 2018).

3 Data

3.1 Study Population and Sample

The study population is the set of rural households in Brazil's semi-arid zone that lack reliable access to drinking water. The present study employs an expanded sample of survey data that we originally collected as part of a randomized control trial involving the allocation of water cisterns (Bobonis et al. 2022). Sample selection of households involved two steps, using the federal government's *Cadastro Único* dataset as a sampling frame. First, we randomly selected municipalities in the semi-arid zone employing weights proportional to the number of households lacking water access. Second, we randomly selected clusters of neighboring households (i.e., *bairros logradouros* in the *Cadastro Único*) in these municipalities. Clusters were required to be at least two kilometers away from each other, and up to six eligible households were interviewed per cluster. Overall, we fielded the panel survey in 654 rural neighborhood clusters in 40 municipalities, across all nine states of the semi-arid region.

3.2 Household Surveys

Our panel survey, which conducted face-to-face interviews over nearly three years, is one of the first to examine clientelism during both election and nonelection years. In 2011, we conducted localization and baseline waves of households heads, providing detailed household information used for balance tests and covariate adjustment. The next two waves, which provide outcome variables discussed below, involved individual-level surveys of all present household members at least 18 years of age. To study clientelism during an election year, we interviewed 3,685 respondents immediately after the October 2012 municipal elections (in November-December 2012). And to examine a non-election year, we interviewed 3,761 respondents in November-December 2013.

The present study examines several outcome variables from the panel survey, which were briefly introduced above. Using the 2012 election-year wave, we examine whether respondents reported receiving private goods from campaign visits to their homes. We also investigate if respondents were promised private goods during campaign visits in 2012. Moreover, we examine whether respondents requested private goods from local politicians at any time in 2012 or 2013, and whether such requests were fulfilled by politicians. All questions examined in this study are provided in Appendix A2.

We explore potential mechanisms by considering additional outcome variables from the survey. To investigate effects on clientelist relationships, we first use the 2012 wave to examine whether: (a) respondents conversed at least monthly with a local politician before the 2012 electoral campaign began, (b) they had such interactions at least weekly, and (c) they had such interactions *and* publicly declared their support for a candidate during the 2012 campaign (discussed below). In addition, we analyze a survey question in our 2012 wave about how well respondents report knowing the mayoral candidate of the incumbent group. For 39 of 40 municipalities, we identified this candidate as meeting one of the following criteria: (i) was the incumbent mayor; (ii) was vice-mayor in the incumbent mayor's administration; (iii) was a copartisan of the incumbent mayor; or (iv) was a member of a party listed in the incumbent mayor's coalition.[9] We also estimate effects for the restricted sample of 21 municipalities in which the mayor ran for reelection. Finally, to investigate effects on perceptions of reciprocity, we estimate continuous and binary measures of how respondents perceive their own politician's reciprocity, building on Finan and Schechter (2012). We employ a series of hypothetical trust games in our 2013 survey, which asked respondents about how they expected the councilor for whom they voted to behave.[10]

3.3 Audits in Sampled Municipalities

With respect to our panel survey, 15 of the 40 municipalities (38 percent) in the sample were randomly audited through the *CGU* program described above. Figure 1 shows the distribution of these audits over time.[11] Seven of these municipalities were audited during

[9]In the remaining municipality, electoral authorities revoked the candidacy of a copartisan of the term-limited incumbent mayor.

[10]In each trust game, citizens were asked how they expected the councilor candidate they voted for to play as the second player of the game. We measure the citizen's perception of the councilor's reciprocity by calculating the average share that would be returned if the councilor received more than half of the first mover's endowment minus the share returned if he/she received less than half of the first player's endowment. In this way, we subtract a measure of altruism in order to have a measure focused on reciprocity.

[11]The timing and outcomes of audits in our study sample are similar to those in the broader sample of audited municipalities.

the 2009-12 mayoral administration immediately preceding the survey, and 10 were audited during prior mayoral administrations.[12] No municipalities in our sample were selected for audits after the 2012 municipal election.

4 Empirical Methods

Our identification strategy exploits the randomization of anti-corruption audits across municipalities and time. Given random assignment, we employ the following specifications to compare individual survey responses of residents from audited vs. unaudited municipalities:

$$y_{ihcmst} = \theta \text{Audit}_{ms} + \beta \text{X}_{ihcmst} + \alpha_s + \delta_t + \varepsilon_{ihcmst} \tag{1}$$

where y_{ihcmst} represents an outcome of interest for individual i in household h, neighborhood cluster c, municipality m and state s at time t. $Audit_{ms}$ is an indicator coded as 1 if the municipality had ever been audited through the *CGU* anti-corruption program before the 2012 election. The vector α_s is a set of state fixed effects, which are included as the *CGU* stratified audit lotteries at the state level. Specifications pool data from two survey rounds and include survey wave fixed effects (δ_t).

Given the random assignment of audits across municipalities within states and over time, θ in Equation 1 captures the intent-to-treat (ITT) effect of municipal audits on outcomes of interest. Consistent with a broad literature demonstrating that the *CGU* audits program is randomly assigned across municipalities, we find evidence of considerable balance between treatment and control groups. Results in Appendix Table A1 show that 34 of 38 variables are statistically indistinguishable between audited and unaudited municipalities (at the 5 percent level); this modest degree of imbalance may be expected given that our survey was implemented in 40 municipalities.

In order to minimize potential bias and improve precision of estimates of θ, we follow recent advances in the empirical literature by employing the double/debiased machine learning (DD-ML) technique (Chernozhukov et al. 2017, 2018). Building on a traditional Lasso approach, DD-ML systematically selects controls from many potential covariates to reduce bias and increase efficiency. DD-ML thereby improves estimates of θ by optimally selecting a set of control variables represented by vector X_{ihcmst}.[13] Standard errors generated by the DD-ML estimator reflect the clustered assignment of treatment across municipalities.

[12]Two of the 15 municipalities were audited during both terms.

[13]More specifically, the DD-ML approach used is fully linear with five folds and 500 replications using demeaned covariates. Block resampling is employed to take into account the clustered nature of the treatment (i.e., municipalities are randomly audited) and the data.

Specifications also include an indicator coded as 1 if the respondent was randomly assigned to receive a water cistern in an accompanying experiment (see Bobonis et al. 2022).[14]

Additional specifications provide further insights about dynamic effects. Some outcome variables were asked during both election and non-election years (in 2012 and 2013, respectively). For such variables, we decompose audits' effects between electoral and non-electoral periods by interacting the $Audit_{ms}$ indicator in Equation 1 with indicators for whether the response corresponds to the 2012 or 2013 survey wave. Moreover, we can distinguish between the short and longer-term effects of audits for all outcomes of interest. To this end, we estimate models in which $Audit_{ms}$ in Equation 1 is replaced with indicators for whether the municipality experienced an audit during the 2009-12 mayoral administration (i.e., *Recent Audit*) or during any prior mayoral administration since the program's 2003 initiation (i.e., *Past Audit*).

5 Results

5.1 Audits' Effects on Clientelism

Following this empirical strategy, Table 2 estimates the effects of anti-corruption audits on clientelism using several key outcome variables. As shown in column 1, audits decreased politicians' provision of private goods during the 2012 electoral campaign by 3.0 percentage points (p = .020). This effect represents a remarkable 51 percent decline in handouts, compared to the prevalence of 5.9 percent reported in unaudited municipalities. One might postulate that politicians reduce their distribution of campaign handouts because they no longer promise voters such benefits. Belying this argument, column 2 shows that anti-corruption audits had no effect on politicians' promises during the 2012 campaign.[15] Another possibility is that anti-corruption audits lead citizens to demand fewer private benefits from local politicians. Indeed, column 3 estimates that audits caused a 2.9 percentage point decrease in citizen requests ($p = .040$) — a 21 percent reduction in proportional terms. Given this question's inclusion in two survey waves, column 4 decomposes audits' effects between electoral and nonelectoral periods. We cannot reject the hypothesis that the reduction in citizen requests is identical in 2012 and 2013 (p = .423), though point estimates are

[14] All results are robust to the exclusion of this variable. In addition, findings hold when the DD-ML estimator is forced to include unbalanced baseline covariates shown in Appendix Table A1.

[15] Following a common strategy in the corruption literature to mitigate social desirability bias (e.g., Johnson et al. 2000; Svensson 2003), we also considered a dependent variable in which respondents were asked if they knew anyone who received handouts during the 2012 campaign. Findings in column 1 remain significant when using this third-person dependent variable (not shown).

considerably stronger in the 2013 nonelection year.

Why might audits reduce citizen demands for private benefits? One potential reason is that citizens believe politicians are less likely to fulfill such requests after audits (e.g., due to heightened risks of providing benefits in exchange for political support). Consistent with this possibility, column 5 estimates that citizens in audited municipalities are 3.9 percentage points less likely to have fulfilled requests for private goods from politicians ($p < .001$) — a substantial 44 percent reduction in proportional terms. Once again, we cannot reject that the magnitude of this causal effect is identical across both waves ($p = .594$). Column 6 shows that the effect is large and precisely estimated in both years: -4.4 and -3.5 percentage points in the 2012 electoral year and the 2013 post-electoral year, respectively.

Overall, Table 2 provides evidence that anti-corruption audits reduce clientelism during both election and nonelection years. In audited municipalities, politicians distribute fewer campaign handouts, though their promises continue unabated. Furthermore, audits reduce citizens' demands for private benefits from politicians, and they decrease the prevalence of fulfilled requests.

5.2 Mechanisms

To shed light on these results, we next explore mechanisms. One potential mechanism is that audits undermine ongoing clientelist relationships between voters and local politicians. Clientelist relationships are often strengthened when citizens interact frequently with politicians and know them well. Table 3 shows that audits significantly reduced citizens' interactions with politicians in early 2012 (i.e., before the 2012 electoral campaign began). Monthly interactions with politicians fell by 5.7 percentage points (column 1, $p = .001$). This effect represents a marked 39 percent decline, given that 14.7 percent of respondents in unaudited municipalities reported monthly interactions. Similarly, column 2 shows that the share of respondents having weekly interactions with politicians fell by 5.1 percentage points ($p < .001$) — an even larger 59 percent decline in proportional terms. Column 3 considers a more restrictive coding of citizens' interactions with politicians, which is coded 1 only if a respondent had such interactions *and* publicly declared support for a candidate during the 2012 campaign. Declared support is a mechanism commonly employed in clientelism to overcome ballot secrecy (Nichter 2018): citizens involved in clientelist relationships put up signs and banners on their homes, wear political paraphernalia, and attend rallies to signal their support for a politician publicly. This measure is likewise reduced by 5.7 percentage points ($p = .001$).

Also consistent with weakened clientelist relationships, respondents in audited municipalities report less knowledge of incumbents (who typically have more resources for clientelism). As shown in column 4, audits lead to a 4.4 percentage point reduction ($p = .011$)

in the share of citizens reporting that they know the mayoral candidate from the incumbent group "very well." The point estimate is comparable (albeit imprecisely estimated) when restricting the sample to the 21 of 40 municipalities in which the incumbent mayor ran for reelection: audits lead to a 4.2 percentage point decrease in knowing that candidate "very well." Taken together, these findings about citizens' interactions and political knowledge are consonant with a mechanism in which audits undermine clientelist relationships.

Another potential mechanism is that audits weaken clientelism by affecting perceptions of reciprocity. For example, contingent exchanges may be undermined if citizens deem politicians to be unlikely to reciprocate with handouts after the scrutiny of audits. Table 3 examines this possibility using the hypothetical trust games described in Section 3.2, and finds that audits indeed deteriorate perceptions of politician reciprocity. In audited municipalities, citizens perceive the local councilor candidate for whom they voted in 2012 to be significantly less reciprocal, using both continuous and binary measures of politician reciprocity (columns 6 and 7, respectively). Also consistent with this mechanism is the finding above that citizens in audited municipalities are less likely to request private goods in audited municipalities. Overall, this evidence conforms with both related mechanisms: audits undermine clientelist relationships as well as perceptions of reciprocity.

5.3 Dynamic Effects of Audits

We next explore the dynamic effects of Brazil's anti-corruption audits on clientelism. To this end, Table 4 decomposes effects into *Recent Audits* experienced during the most recent mayoral administration (2009-12), and *Past Audits* experienced during any prior mayoral administration since the program's 2003 initiation. Dependent variables mirror those employed in Table 2.

Estimates show that recent audits reduce politicians' provision of private goods during the 2012 electoral campaign by 3.7 percentage points ($p = .013$), whereas past audits have no effect on such transfers (column 1). However, we cannot reject that both effects are statistically equivalent ($p = .210$). Although inconclusive, these findings suggest that audits' dampening of clientelism fades over time. As before, evidence in column 2 points away from the possibility that politicians reduce their distribution of campaign handouts because they no longer promise voters such benefits. Both short and longer-term effects on promises are statistically indistinguishable from zero, and neither coefficient is substantial in magnitude.

Column 3 decomposes audit effects on citizens' demands for private benefits from local politicians. Whereas recent audits reduce citizens' requests by 4.9 percentage points, past audits reduce them by only 1.3 percentage points. In this case, we can reject the null hypothesis that short and longer-term effects are equivalent ($p = .036$). This finding suggests that audits' effects on the demand side of clientelism peter out relatively quickly. Given this question's inclusion in two survey waves, column 4 decomposes audits' short-term effects on voter demands into electoral vs. non-electoral periods. Estimates reveal that recent audits reduce citizens' requests for private goods sharply during the 2012 election year — by 7.3 percentage points ($p = .001$). In contrast, they have a statistically insignificant effect on requests in the 2013 nonelection year. We can reject the hypothesis that the effect is identical in both years ($p = .029$), which partly reflects differing levels of requests across years. In this specification, the effect of past audits again remains statistically insignificant.

As in Section 5.1, we next examine if the evidence is consistent with the possibility that audits reduce such demands because citizens believe politicians are less likely to fulfill such requests after audits. Column 5 reports that both recent and past audits render it significantly less likely that citizens have fulfilled requests for private goods from politicians. The decrease is 4.4 percentage points for recent audits ($p = .001$) and 2.5 percentage points for past audits ($p = .014$); we cannot reject the equality of these effects ($p = .159$). Column 6 decomposes audits' short-term effects on request fulfillment into electoral vs. non-electoral periods. Recent audits reduce fulfilled requests by 6.6 percentage points ($p < .001$) in the 2012 election year, and by 2.2 percentage points ($p = .090$) in the 2013 nonelection year. We can reject the hypothesis that these effects are identical ($p = .009$), which partly reflects different levels of request fulfillment in electoral vs non-electoral periods. The effect of past audits remains virtually unchanged in this specification, in terms of both its magnitude and statistical significance.

Overall, Table 4 provides novel insights into the dynamic effects of anti-corruption audits. Evidence suggests that audits cause a relatively stronger fall on some aspects of clientelism in the short term. The decline in citizen demands is significantly larger for recent audits than for past audits; similar but statistically indistinguishable patterns are observed for politicians' provision of campaign handouts and fulfilled requests. Furthermore, the impact of recent audits is amplified during electoral periods: they lead to a significantly larger decline in requests and fulfilled requests during the 2012 election year.[16]

6 Conclusion

This study provides novel evidence that interventions aimed at reducing corruption can also decrease clientelism. We exploit Brazil's randomized anti-corruption audit program as a source of exogenous variation in top-down anti-corruption efforts. To measure audits' effects, we employ a unique panel survey of rural Brazilian households that provides detailed

[16]For all mechanisms in Table 2, we cannot reject the hypothesis of equivalent effects for recent vs. past audits (see Appendix Table A2).

information on clientelism during both electoral and non-electoral years. Our empirical strategy leverages the fact that 38 percent of sampled municipalities had experienced an anti-corruption audit.

Analyses yield numerous important findings. Audits halve the reported incidence of campaign handouts distributed by politicians, an illegal practice that constitutes vote buying. This stark decrease in the provision of private goods during an electoral campaign does not stem from a reduction in politicians promising such goods. Instead, audits reduce citizens' demands for private benefits from politicians, and they decrease the prevalence of fulfilled requests. Evidence suggests that anti-corruption audits affect clientelism not only during the election campaign, but during the following nonelection year. Results also shed light on dynamic effects: audits have more pronounced effects in the short run. In recently audited municipalities, election-year requests and fulfilled requests fall especially sharply.

With respect to mechanisms, evidence suggests that audits undermine clientelist relationships as well as perceptions of reciprocity. Clientelist relationships are often strengthened when citizens interact frequently with politicians and know them well — yet both fall substantially in audited municipalities. Audits significantly reduce citizens' interactions with politicians outside of the electoral period, and also decrease the share of citizens reporting that they know incumbents "very well." Furthermore, audits deteriorate perceptions of politician reciprocity. Using hypothetical trust games, we find that citizens in audited municipalities perceive the local councilor for whom they voted to be significantly less reciprocal.

These results have important implications and suggest avenues for future research. One key question involves the multifaceted ways in which this reform may affect citizen welfare. We show that one of the world's most prominent anti-corruption programs substantially undercut clientelism, at least in the short run. This reduction likely benefits citizen welfare, given that a broad literature argues that clientelism exacerbates governmental allocative inefficiencies and contributes to the underprovision of public goods.[17] Yet citizen welfare could also arguably be undermined if misappropriated funds from corruption largely flow to citizens as clientelist handouts. Pointing against this possibility, a back-of-the-envelope calculation suggests that audits' effects on the value of reduced corruption is over four times that of audits' effects on the value of reduced clientelist transfers.[18] An important direction

[17]Baland and Robinson (2008); Hicken (2011); Bardhan and Mookherjee (2012); Robinson and Verdier (2013); and Anderson, Francois, and Kotwal (2015).

[18]Avis, Ferraz, and Finan (2018) estimate that audits' effects on the value of reduced corruption is R$355,000 per year per municipality. By contrast, we estimate the aggregate value of reduced clientelist transfers to be approximately R$85,000 per year per municipality. We generate this estimate by applying the median value of handouts in our sample (R$120) to the effect size in Table 2, column 1 (3.0 percentage

for future research is to examine various effects of anti-corruption interventions on citizen welfare (e.g., Finan and Mazzocco 2021), including impacts on public goods provision.

Furthermore, empirical research is warranted to explore if our findings are observed beyond Brazil, and if some types of anti-corruption interventions have lasting effects on patterns of clientelism. From a theoretical standpoint, this paper corroborates influential studies that suggest a link between corruption and clientelism (e.g., Robinson and Verdier 2013; Anderson et al. 2015). Further work can shed additional light on the mechanisms by which reduced corruption decreases clientelism. For example, it may predict conditions under which anti-corruption efforts induce relatively greater supply-side vs. demand-side responses (i.e., politician-initiated vs. citizen-initiated responses), as well as heterogeneity by factors such as socioeconomic and political characteristics.

Such empirical and theoretical work also offers the strong potential to contribute to policy debates. Across the globe, policymakers expend significant resources to combat corruption. Our findings suggest the tantalizing possibility that anti-corruption efforts may also be effective at reducing clientelism. Given that policymakers rarely consider clientelism when designing or evaluating anti-corruption programs, the existence of such spillovers would suggest that the returns of these interventions may be undervalued. To inform policy makers about potential externalities, it is important to advance our understanding of how and why different interventions may play this complementary role.

points), and their reported average population size of 23,599.

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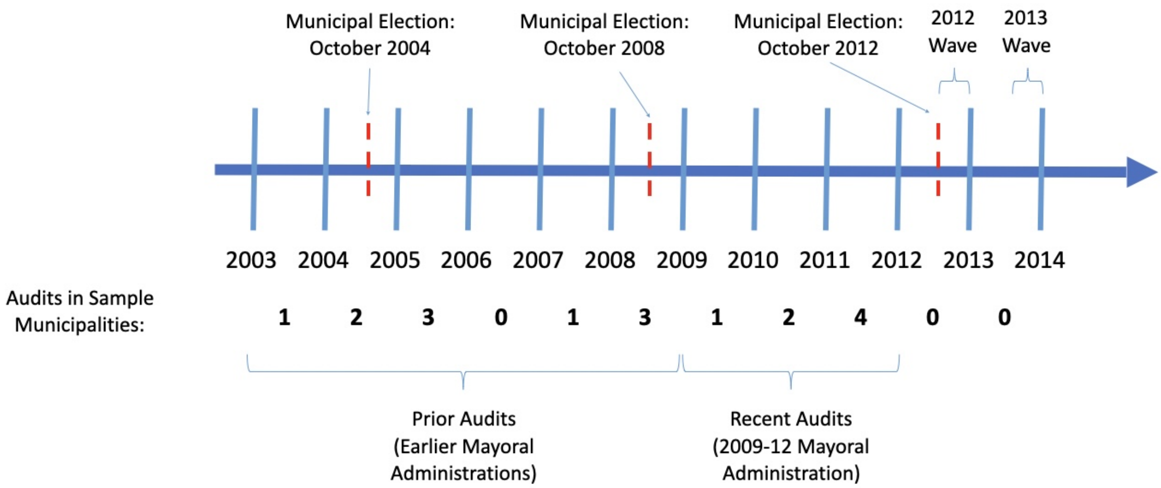
Figures

Figure 1: Timeline of Audits, Elections and Survey Waves



Tables

Table 1: Summary Statistics

Variable	Mean (1)	Std. Dev. (2)	N (3)
Politicians Provide Private Goods in Campaign, 2012	0.059	0.237	3,681
Request Private Good from Politician, 2012	0.215	0.411	3,664
Request Private Good from Politician, 2013	0.081	0.273	3,752
Request and Receive Private Good from Politician, 2012	0.128	0.334	3,656
Request and Receive Private Good from Politician, 2013	0.048	0.214	3,752
Politicians Promise Private Goods, 2012	0.207	0.405	3,681
Frequent Interactions with Local Politician, 2012 (Outside of Campaign)	0.134	0.340	3,664
Knows Incumbent Very Well (Mayor)	0.126	0.333	1,257
Opinion About Own Politician Reciprocity (Continuous)	0.022	0.061	3,656
Received Visit from Representatives of Any Mayoral Candidate	0.686	0.464	3,681
All Household Members Voting for the Same Mayoral Candidate	0.766	0.423	3,195
Any Declared Support	0.484	0.500	3,679

Table 2: Effects of Audits on Private Goods Provision, Requests, and Promises

	Politicians Provide Private Goods in Campaign (2012)	Politicians Promise Private Goods in Campaign (2012)	Citizens Request Private Goods (2012-13)		Politicians Provide Requested Private Goods (2012-13)	
	(1)	(2)	(3)	(4)	(5)	(6)
β_1: Audit	-0.030**	0.010	-0.029**		-0.039***	
	(0.013)	(0.019)	(0.014)		(0.010)	
β_2: Audit $\times$ 2012				-0.021		-0.044***
				(0.021)		(0.015)
β_3: Audit $\times$ 2013				-0.037***		-0.035***
				(0.014)		(0.010)
Test of homogeneous effects (p-value):						
(a) $H_0 : \beta_2 = \beta_3$				0.423		0.594
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	No	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	3,681	3,681	7,416	7,416	7,408	7,408
Mean of Y: Unaudited Group	0.059	0.196	0.140	0.140	0.089	0.089
Mean of Y: Unaudited Group in 2012	0.059	0.196	0.205	0.205	0.132	0.132
Mean of Y: Unaudited Group in 2013	–	–	0.081	0.081	0.049	0.049

Note: Regressions use Chernozhukov et al.'s (2018) double/debiased machine learning technique (see section 4). Robust standard errors are clustered by municipality and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In column (1), the dependent variable refers to whether the respondent reported receiving a private good from a campaign visit to his or her home in 2012. In column (2), it refers to whether the respondent reported receiving a promise of private goods during such campaign visits. In columns (3)-(4), it refers to whether the respondent requested a private good from a politician (in 2012 or 2013). In columns (5)-(6), it refers to whether the respondent reported receiving a private good requested from a politician (in 2012 or 2013). All questions examined in this study are provided in Appendix A2.

Table 3: Effects of Audits on Citizens' Interactions and Perceptions vis-à-vis Politicians

	Interactions with Politicians, Before Campaign (2012)			Knows Incumbent Very Well (2012)		Opinion about Own Politician Reciprocity (2013)	
	Monthly	Weekly	Monthly + Public Support	Incumbent (Any)	Incumbent (Mayor)	Continuous Measure	Binary Measure
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
β_1: Audit	-0.057***	-0.051***	-0.057***	-0.044**	-0.042	-0.006**	-0.043**
	(0.017)	(0.013)	(0.017)	(0.017)	(0.037)	(0.003)	(0.018)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	3,664	3,664	3,663	2,432	1,257	3,656	3,656
Mean of Y: Unaudited Group	0.147	0.086	0.129	0.109	0.134	0.025	0.211

Note: Regressions use Chernozhukov et al.'s (2018) double/debiased machine learning technique (see section 4). Robust standard errors are clustered by municipality and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In columns (1)-(2), the dependent variable refers to whether the respondent conversed at least monthly or weekly (respectively) with a local politician before the 2012 electoral campaign began. In column (3), it refers to whether the respondent had such conversations at least monthly *and* publicly declared support for a candidate before the 2012 election. In column (4), the dependent variable refers to whether the respondent knows very well a candidate who meets one of the following criteria: (i) is the incumbent mayor; (ii) is vice-mayor in the incumbent mayor's administration; (iii) is a copartisan of the incumbent mayor; or (iv) is a member of a party listed in the incumbent mayor's coalition; the sample is restricted to include municipalities where such a candidate ran for election, and respondents who vote in the municipality. In column (5), the dependent variable refers to whether the respondent knows the incumbent mayor very well; the sample is restricted to include municipalities where the incumbent mayor ran for election, and respondents who vote in the municipality. The dependent variable in column (6) uses Finan and Schechter (2012)'s continuous measure of reciprocity, and the dependent variable in column (7) is an indicator for whether the continuous measure of reciprocity is strictly positive. These variables measure how reciprocal respondents think the city councilor candidate they voted for would be in a hypothetical game.

Table 4: Effects of Audits on Private Goods Provision, Requests and Promises – By Audit Term

	Politicians Provide Private Goods in Campaign (2012)	Politicians Promise Private Goods in Campaign (2012)	Citizens Request Private Goods (2012-13)		Politicians Provide Requested Private Goods (2012-13)	
	(1)	(2)	(3)	(4)	(5)	(6)
β_1: Recent Audit	-0.037**	-0.013	-0.049***		-0.044***	
	(0.015)	(0.023)	(0.016)		(0.013)	
β_2: Recent Audit × 2012 wave				-0.073***		-0.066***
				(0.021)		(0.018)
β_3: Recent Audit × 2013 wave				-0.025		-0.022*
				(0.016)		(0.012)
β_4: Past Audit	-0.018	0.025	-0.013	-0.014	-0.025**	-0.027***
	(0.013)	(0.021)	(0.014)	(0.014)	(0.010)	(0.010)
Test of homogeneous effects (p-value):						
(a) $H_0 : \beta_1 = \beta_4$	0.210	0.193	0.036		0.159	
(b) $H_0 : \beta_2 = \beta_3$				0.029		0.009
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	No	No	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	3,681	3,681	7,416	7,416	7,408	7,408
Mean of Y: Unaudited group	0.059	0.196	0.140	0.140	0.089	0.089
Mean of Y: Unaudited group in 2012	0.059	0.196	0.205	0.205	0.132	0.132
Mean of Y: Unaudited group in 2013	–	–	0.081	0.081	0.049	0.049

Note: Regressions use Chernozhukov et al.'s (2018) double/debiased machine learning technique (see section 4). Robust standard errors are clustered by municipality and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In column (1), the dependent variable refers to whether the respondent reported receiving a private good from a campaign visit to his or her home in 2012. In column (2), it refers to whether the respondent reported receiving a promise of private goods during such campaign visits. In columns (3)-(4), it refers to whether the respondent requested a private good from a politician (in 2012 or 2013). In columns (5)-(6), it refers to whether the respondent reported receiving a private good requested from a politician (in 2012 or 2013).

Online Appendix for:

Does Combating Corruption Reduce Clientelism?

by Gustavo J. Bobonis, Paul J. Gertler,
Marco Gonzalez-Navarro, and Simeon Nichter

Table A1: Balance tests (2012 and 2013 survey waves)

	Any Audit (1)	Past Audit (2)	Recent Audit (3)	No Audit (4)	Difference (1) - (4) (5)	Difference (2) - (4) (6)	Difference (3) - (4) (7)	Joint test p-value (8)	Obs. (9)
Panel A: Individual Characteristics									
Age	37.142	37.518	36.286	37.513	-0.753	-0.212	-1.606	0.411	4,882
	[17.142]	[17.070]	[17.196]	[16.903]	(1.163)	(1.101)	(1.670)		
Female	0.491	0.483	0.501	0.507	-0.026*	-0.030*	-0.024	0.150	5,241
	[0.500]	[0.500]	[0.500]	[0.500]	(0.015)	(0.016)	(0.020)		
N	2,188	1,408	929	3,053					
Panel B: Household Characteristics									
Household size	4.296	4.285	4.320	4.218	0.179	0.182	0.304	0.406	1,728
	[2.070]	[1.780]	[2.360]	[1.973]	(0.179)	(0.174)	(0.295)		
Children 0-6 months	0.057	0.063	0.044	0.059	0.007	0.012	0.002	0.673	2,313
	[0.236]	[0.249]	[0.206]	[0.237]	(0.016)	(0.017)	(0.019)		
Children 6 months - 5 years	0.603	0.560	0.689	0.608	-0.017	-0.065	0.090	0.134	2,313
	[0.715]	[0.715]	[0.719]	[0.738]	(0.083)	(0.080)	(0.098)		
Household members 5-64 years	3.375	3.401	3.331	3.434	0.074	0.104	0.131	0.694	2,313
	[2.217]	[2.115]	[2.293]	[1.930]	(0.150)	(0.141)	(0.278)		
Household members $\geq$ 65 years	0.258	0.262	0.252	0.221	0.008	0.011	0.007	0.879	2,313
	[0.595]	[0.611]	[0.572]	[0.532]	(0.026)	(0.025)	(0.042)		
Age of household head	43.603	44.487	42.447	45.293	-1.224	-0.123	-2.110	0.156	1,727
	[17.192]	[16.528]	[17.855]	[16.084]	(1.184)	(1.095)	(1.742)		
Household head education	5.786	5.596	6.031	5.925	-0.028	-0.238	0.121	0.387	1,292
	[3.563]	[3.465]	[3.736]	[3.732]	(0.337)	(0.271)	(0.524)		
Household head is female	0.176	0.133	0.240	0.190	-0.027	-0.066**	0.055	0.005	1,728
	[0.381]	[0.340]	[0.428]	[0.393]	(0.030)	(0.026)	(0.042)		

Note: Standard deviations of variables are reported in brackets. Robust standard errors are clustered by municipality and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Differences are adjusted for state fixed effects. Past audits refer to 2003-08 audits and recent audits refer to 2009-12 audits. Column (8) regresses each dependent variable on dummies for whether a municipality experienced a past audit and whether a municipality experienced a recent audit, and tests the hypothesis that the coefficients on both dummies are jointly equal to 0.

Table A1: Balance tests (continued)

	Any Audit (1)	Past Audit (2)	Recent Audit (3)	No Audit (4)	Difference (1) - (4) (5)	Difference (2) - (4) (6)	Difference (3) - (4) (7)	Joint test p-value (8)	Obs. (9)
Panel B: Household Characteristics (continued)									
Agricultural household	0.487	0.540	0.405	0.471	0.101	0.155*	0.052	0.187	2,312
	[0.500]	[0.499]	[0.491]	[0.499]	(0.078)	(0.085)	(0.120)		
Owns house	0.889	0.882	0.883	0.832	0.038	0.035	0.033	0.668	2,343
	[0.315]	[0.323]	[0.321]	[0.374]	(0.030)	(0.034)	(0.040)		
Number of rooms in house	5.254	5.405	5.003	5.414	-0.126	0.010	-0.280*	0.022	2,287
	[1.302]	[1.312]	[1.258]	[1.411]	(0.131)	(0.138)	(0.165)		
Owns land	0.477	0.464	0.465	0.489	0.025	0.015	0.036	0.994	2,308
	[0.500]	[0.499]	[0.499]	[0.500]	(0.059)	(0.068)	(0.090)		
Has access to electricity	0.981	0.980	0.980	0.970	0.001	0.003	-0.009	0.709	2,313
	[0.138]	[0.141]	[0.139]	[0.172]	(0.008)	(0.010)	(0.007)		
Has bathroom in house	0.484	0.464	0.494	0.550	-0.126**	-0.142**	-0.073	0.046	2,313
	[0.500]	[0.499]	[0.501]	[0.498]	(0.055)	(0.057)	(0.098)		
Has bathroom outside house	0.132	0.132	0.141	0.214	-0.041	-0.042	-0.045	0.374	2,313
	[0.338]	[0.339]	[0.348]	[0.410]	(0.026)	(0.031)	(0.027)		
Has sewerage	0.012	0.010	0.012	0.009	0.008	0.005	0.013	0.510	2,313
	[0.108]	[0.100]	[0.111]	[0.093]	(0.006)	(0.007)	(0.009)		
Has disabled household member	0.114	0.115	0.119	0.107	-0.012	-0.009	-0.004	0.969	2,309
	[0.318]	[0.320]	[0.324]	[0.310]	(0.018)	(0.021)	(0.023)		
Log miles to state capital	4.970	5.127	4.766	4.763	0.493**	0.614***	0.187	0.024	2,313
	[0.518]	[0.417]	[0.558]	[0.619]	(0.195)	(0.213)	(0.292)		
Log miles to municipal seat	1.898	1.966	1.719	1.991	0.342*	0.344	0.258	0.242	2,313
	[0.826]	[0.899]	[0.810]	[0.763]	(0.177)	(0.205)	(0.217)		
Log miles to polling station	1.121	1.448	0.785	0.732	0.155	0.538*	-0.454	0.134	988
	[1.117]	[0.884]	[1.219]	[1.240]	(0.299)	(0.309)	(0.271)		
N	943	594	412	1,400					

Note: Standard deviations of variables are reported in brackets. Robust standard errors are clustered by municipality and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Differences are adjusted for state fixed effects. Past audits refer to 2003-08 audits and recent audits refer to 2009-12 audits. Column (8) regresses each dependent variable on dummies for whether a municipality experienced a past audit and whether a municipality experienced a recent audit, and tests the hypothesis that the coefficients on both dummies are jointly equal to 0.

Table A1: Balance tests (continued)

	Any Audit (1)	Past Audit (2)	Recent Audit (3)	No Audit (4)	Difference (1) - (4) (5)	Difference (2) - (4) (6)	Difference (3) - (4) (7)	Joint test p-value (8)	Obs. (9)
Panel C: Municipality Characteristics									
Population (1000's) in 2000	46.999	37.362	56.410	41.296	10.378	-0.235	26.045	0.756	40
	[60.507]	[22.188]	[88.627]	[49.278]	(16.334)	(9.888)	(34.936)		
Share urban in 2000	0.526	0.541	0.482	0.494	0.023	0.031	-0.007	0.908	40
	[0.209]	[0.178]	[0.275]	[0.218]	(0.083)	(0.086)	(0.131)		
Income per capita in 2000	189.458	184.509	185.494	193.754	-10.395	-18.372	-7.785	0.651	40
	[85.391]	[56.967]	[120.139]	[71.770]	(25.638)	(23.008)	(48.279)		
Gini index in 2000	0.578	0.581	0.591	0.577	0.010	0.010	0.014	0.702	40
	[0.048]	[0.054]	[0.057]	[0.054]	(0.021)	(0.023)	(0.029)		
Share poor in 2000	0.601	0.602	0.620	0.602	0.018	0.025	0.023	0.788	40
	[0.138]	[0.098]	[0.196]	[0.114]	(0.044)	(0.042)	(0.082)		
Share of illiterate adults in 2000	0.382	0.382	0.397	0.394	-0.024	-0.015	-0.025	0.923	40
	[0.074]	[0.067]	[0.098]	[0.084]	(0.027)	(0.032)	(0.047)		
Log rainfall (2012 survey)	3.275	3.238	3.308	3.626	-0.317***	-0.337**	-0.266**	0.063	40
	[0.235]	[0.245]	[0.244]	[0.351]	(0.110)	(0.131)	(0.121)		
N	15	10	7	25					

Note: Standard deviations of variables are reported in brackets. Robust standard errors are clustered by municipality and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Differences are adjusted for state fixed effects. Past audits refer to 2003-08 audits and recent audits refer to 2009-12 audits. Column (8) regresses each dependent variable on dummies for whether a municipality experienced a past audit and whether a municipality experienced a recent audit, and tests the hypothesis that the coefficients on both dummies are jointly equal to 0. Log rainfall (2012 survey) refers to log of mean monthly rainfall in the 12 months before the 2012 survey was conducted (Dec 2011 - Nov 2012).

Table A2: Effects of Audits on Citizens' Interactions and Perceptions vis-à-vis Politicians – By Audit Term

	Interactions with Politicians, Before Campaign (2012)			Knows Incumbent Very Well (2012)		Opinion about Own Politician Reciprocity (2013)	
	Monthly	Weekly	Monthly + Public Support	Incumbent (Any)	Incumbent (Mayor)	Continuous Measure	Binary Measure
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
β_2: Audit, current term (2009-12)	-0.038	-0.046***	-0.036*	-0.012	-0.021	-0.006*	-0.026
	(0.023)	(0.016)	(0.020)	(0.025)	(0.037)	(0.003)	(0.022)
β_3: Audit, past terms (2003-08)	-0.051***	-0.039***	-0.048***	-0.048***	-0.067**	-0.006**	-0.052***
	(0.017)	(0.013)	(0.016)	(0.016)	(0.033)	(0.003)	(0.017)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Test of homogeneous effects (p-value)							
(a) $H_0 : \beta_2 = \beta_3$	0.534	0.638	0.504	0.143	0.234	0.941	0.295
N	3,664	3,664	3,663	2,432	1,257	3,656	3,656
Mean of Y: Control group	0.147	0.086	0.129	0.109	0.134	0.025	0.211

Note: Regressions use Chernozhukov et al.'s (2018) double/debiased machine learning technique (see section 4). Robust standard errors are clustered by municipality and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In columns (1) and (2), the dependent variable refers to whether the respondent conversed at least monthly or weekly (respectively) with a local politician before the 2012 electoral campaign began. In column (3), it refers to whether the respondent had such conversations at least monthly *and* publicly declared support for a candidate before the 2012 election. In column (4), the dependent variable refers to whether the respondent knows very well a candidate who meets one of the following criteria: (i) is the incumbent mayor; (ii) is vice-mayor in the incumbent mayor's administration; (iii) is a copartisan of the incumbent mayor; or (iv) is a member of a party listed in the incumbent mayor's coalition; the sample is restricted to include municipalities where such a candidate ran for election, and respondents who vote in the municipality. In column (5), the dependent variable refers to whether the respondent knows the incumbent mayor very well; the sample is restricted to include municipalities where the incumbent mayor ran for election, and respondents who vote in the municipality. The dependent variable in column (6) uses Finan and Schechter (2012)'s continuous measure of reciprocity, and the dependent variable in column (7) is an indicator for whether the continuous measure of reciprocity is strictly positive. These variables measure how reciprocal respondents think the city councilor candidate they voted for would be in a hypothetical game.

A1 Survey Questions for Key Variables

Variable: *Politicians Provide Private Goods in Campaign (asked in 2012).*

- Definition: Respondent reported receiving private benefits or services from a campaign visit in 2012.
- Coded 1 if answered yes to having received a private benefit or service during or through promises made in a campaign visit; 0 otherwise.
- Question in 2012, asked during module about campaign visits by representatives of politicians:
  - "Did you receive any help? For example, help can be goods (like bricks), services (like medical exams), money, food or beverages"
  - If yes: "What help was this?"

Variable: *Politicians Promise Private Goods in Campaign (asked in 2012).*

- Definition: Respondent reported that private benefits or services were promised during a campaign visit in 2012.
- Coded 1 if answered yes to having been promised a private benefit or service during a campaign visit; 0 otherwise.
- Question in 2012, asked during module about campaign visits by representatives of politicians:
  - "Did they promise any help? For example, help can be goods (like bricks), services (like medical exams), money, food or beverages"

Variable: *Citizens Request Private Goods (asked in 2012 and 2013).*

- Definition: Respondent requested private good from a local politician.
- Coded 1 if answered yes to requesting from politician, unless specifying that the request was for a non-private benefit; 0 otherwise.
- Questions used in 2012 wave to define this variable:
  - (a) "This year, did you ask a city councilor candidate for help?";
  - (b) [If yes:] "What did you ask for?";
  - (c) "This year, did you ask a mayor candidate for help?";
  - (d) [If yes:] "What did you ask for?"
- Identical questions were asked in 2013, first inquiring about requests of candidates who won the election, and then inquiring about requests of candidates who lost the election.

Variable: *Politicians Provide Requested Private Goods (asked in 2012 and 2013).*

- Definition: Respondent reported receiving private good requested from a politician.
- Coded 1 if answered yes to receiving a requested private good; 0 otherwise.
- This variable is generated from a question asked directly after *Request* variable described above. Question: "Did you receive it?"

Variable: *Interactions with Local Politician, Before Campaign (asked in 2012).*

- Definition: Respondent reports conversing with a political candidate at least monthly before the 2012 campaign began.
- Coded 1 if answered yes to having spoken with politician at least monthly; 0 otherwise.
- Questions:
  - (a) "This year, did you speak with any city councilor candidate?";
  - (b) [If yes:] "How often before the political campaign (before June)?";
  - (c) "This year, did you speak with any mayor candidate?";
  - (d) [If yes:] "How often before the political campaign (before June)?"

Variable: *Knows Incumbent Mayor Very Well (asked in 2012).*

- Definition: Respondent reported knowing the incumbent mayor very well.
- Coded 1 if answered "Very Well" when asked about how well they know the incumbent mayor; 0 otherwise.
  - "Would you say that you know [incumbent mayor's name]: Very Well, Well, Little, Very Little, or Never Heard of?"
- See Section 3.2 for coding details for mayoral candidate of the incumbent group (using the same question format)

Variable: *Opinion About Own Politician Reciprocity (from hypothetical trust games in 2013).*

- We employ hypothetical trust games to estimate binary and continuous measures of reciprocity.
- Following Finan and Schechter (2012), we measure reciprocity by calculating the average share returned when the individual receives more than half of the first mover's endowment minus the share returned when receiving less than half of the first player's endowment. (We implicitly assume that when the first mover sends at least half, the second mover thinks that she has been treated well. On the other hand, if the first mover sends less than half, then it is assumed that the second mover thinks she has been treated poorly.)
- In this way, we subtract a measure of altruism in order to have a measure focused on reciprocity. We also create a binary variable based on this continuous measure, using an indicator equal to 1 if the difference in the shares returned to player 1 described above is positive.
- The hypothetical trust games were played in 2013, and examined how much the respondent expects his or her own councilor to return when playing a random citizen.
- Responses were recorded for four rounds asked consecutively, in which the citizen sends R$2, R$4, R$6, and R$8 (respectively) to the councilor.
- For example, for the R$2 round we asked: "Think of the councilor candidate that you voted for in the last election. Our team doesn't want to know the name of that candidate and is not going to talk to any politician from here. Suppose that someone named *Fulano* is chosen to be your councilor's partner in the game. Suppose that our team gives R$10 to *Fulano*.

If *Fulano* gives R$2 to your candidate, we give R$4 more, so now your candidate gets R$6. Of that R$6, how much money (or nothing) do you think your candidate will send back to *Fulano*? Your candidate will never know who *Fulano* is or where he lives."