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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. We investigate mechanisms by which audits may reduce clientelism, and find that audits significantly reduce citizens' willingness to supply clientelist votes. Results also offer novel insights into audits' dynamic effects: they have more pronounced effects in the short run, especially during electoral periods.

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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: Do programs that combat corruption also lead to reduced 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

[1]Corruption is defined as the abuse of public office for private gain (Rose-Ackerman and Palifka 2016). 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.

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 non-electoral 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.

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 substantial decline of 51 percent. Across both electoral and non-electoral periods, audits caused a 3.0 percentage point decline in citizen requests for private goods from politicians — a 21 percent reduction. Furthermore, audits led to a 3.9 percentage point decline in the prevalence of requests fulfilled by politicians — a 44 percent decrease.

With respect to mechanisms, several findings suggest that anti-corruption audits reduce citizens' willingness to participate in clientelism. First, audits increase the equilibrium price of purchased votes, which is consistent with voters reducing their supply of clientelist votes. Second, audits decrease citizens' willingness to sell their votes when asked about scenarios involving clientelism. Third, audits lower citizens' assessment of politicians' honesty. Fourth, audits reduce citizens' perceptions of politicians' reciprocity in hypothetical trust games. And fifth, audits lead citizens to interact less frequently with politicians. Altogether, these results suggest that audits dampen citizens' participation in clientelism, a finding consistent with Anderson, Francois, and Kotwal's (2015) model of clientelism. As shown in the Online Appendix, if audits exogeneously worsen citizens' perceptions of clientelist candidates, then the quantity of votes sold would decrease and their price would increase. Beyond citizens, we also investigate mechanisms that focus on politicians: audits might reduce clientelism due to legal discipline, electoral discipline, or political selection effects.[3] Without denying that these mechanisms may reduce clientelism across Brazil, we do not find evidence of them within our sampled municipalities.

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., Vicente 2014; 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

[3]As discussed below, our discussion of these three mechanisms builds on Avis et al.'s (2018) model that elaborates why anti-corruption audits reduce corruption. Their study does not discuss clientelism.

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 non-electoral 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.[4] The zone's 28 million residents are disproportionately poor and rural. Most are highly vulnerable to shocks, including exposure to frequent droughts.[5] 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.[6]

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

[4] 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.

[5] 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.

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

received private goods from campaign visits to their homes, with a median value of R$ 120 (approximately USD$ 60 in 2012). Such patterns of clientelism often extend beyond election years. For example, in rural Northeast Brazil, many 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.[7] The present study focuses on politicians at the municipal level – i.e., mayors and city councilors, who are elected concurrently every four years.[8] 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

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[7] 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.

[8] 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.

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, 18.0 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." 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.[9] 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.

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

2.2 CGU Municipal Audit Program

In part to reduce corruption in federal expenditures, Brazil created the *Controladoria-Geral da União* (*CGU*, or Office of the Comptroller General) in 2003.[10] 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 whether the projects were actually completed as described (Ferraz and Finan 2008). Such forensic audits primarily covered types of corruption such as embezzlement and kickbacks in public procurement (and, importantly, excludes forms of electoral corruption including clientelism). 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

[10]Upon its 2003 creation, the *Controladoria-Geral da União* incorporated functions of its predecessor *Corregedoria-Geral da União*, which had been created in 2001.

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 nonelection year, we interviewed 3,761 respondents in November-December 2013.[11]

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 Online Appendix C.

Our discussion of potential mechanisms considers several additional outcome variables from the survey. To investigate how audits affect the price of clientelism, we employ a follow-up question that asked recipients to estimate the value of private goods they received during campaign visits in 2012. To examine how audits affect citizens' supply of clientelist votes, we consider a survey vignette that first asked respondents if they would accept a vote-buying offer of R$ 10 from a fictitious candidate, and then incrementally increased the amount until the citizen accepted an offer (up to R$ 1,000). To investigate audits' effects on citizens' perceptions of politicians, we employ survey questions that asked respondents to rate the level of honesty and competence of the top-two mayoral candidates in the 2012 election, on a four-point scale from "Very Bad" to "Very Good." In addition, we estimate continuous and binary measures of how respondents perceive their own politician's reciprocity, building on Finan and Schechter (2012). These measures 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.[12] Finally, to investigate audits' effects on clientelist relationships, we

[11]In addition to our household surveys, we also obtained census data from the Brazilian Institute of Geography and Statistics (IBGE) and political data from the Superior Electoral Court (TSE).

[12]Details about these trust games are provided in Online Appendix C. To summarize, 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

use the 2012 wave to examine: (a) whether respondents conversed at least monthly with a local politician before the 2012 electoral campaign began, and (b) whether they had such interactions *and* publicly declared their support for a candidate during the 2012 campaign (discussed below).

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.[13] Seven of these municipalities were audited during the 2009-12 mayoral administration immediately preceding the survey, and 10 were audited during prior mayoral administrations. Two of the 15 municipalities were audited during both periods.

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

a measure of altruism in order to have a measure focused on reciprocity.

[13] With regards to representativeness, our sampled municipalities are broadly similar to others in the Northeast region. Compared to outcomes from CGU audits overall, audited municipalities in our sample appear to have slightly more corruption. Specifically, outcome data are available from Avis et al. (2018) for 11 audits in our sample. Our sample shows a higher incidence of irregularities and acts of corruption, but not acts of mismanagement (analyses available upon request).

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 average treatment effect on the treated (ATT) 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}.[14] Standard errors generated by the DD-ML estimator reflect the clustered assignment of treatment across municipalities. 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).[15] Online Appendix Tables A7-A9 provide OLS specifications for all tables; as expected, DD-ML estimates are similar to OLS with greater precision.

Additional specifications provide further insights about dynamic effects. Some outcome variables were asked during both election and nonelection years (in 2012 and 2013,

[14] More specifically, the DD-ML approach used is fully linear with five folds and 200 replications using all (demeaned) covariates from Table A1. Block resampling is employed to take into account the clustered nature of the treatment (i.e., municipalities are randomly audited) and the data.

[15] All results are robust to the exclusion of this variable.

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 — a practice that legally constitutes vote buying in Brazil — 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.[16]

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 3.0 percentage point decrease in citizen requests ($p = .039$) — a 21 percent reduction in

[16] 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).

proportional terms. Given this question's inclusion in two survey waves, column 4 decomposes audits' effects between electoral and non-electoral periods. We cannot reject the hypothesis that the reduction in citizen requests is identical in 2012 and 2013 ($p = .423$), though point estimates are considerably stronger in the 2013 nonelection year. We also examine an alternative dependent variable: an indicator coded 1 if the individual requested a private good from a local politician and that good was received. 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: -4.3 and -3.5 percentage points in the 2012 electoral year and the 2013 non-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: Citizen Responses

To shed light on these results, we next explore potential mechanisms. At the outset, we emphasize that these mechanisms are by no means exhaustive, and we cannot definitively identify or rule out mechanisms with our data.

A first step in investigating mechanisms is to consider whether audits are primarily a supply-side or demand-side shock. For example, audits may reduce citizens' supply of clientelist votes if they increase their disgust toward illegal activities, or audits may reduce politicians' demand for clientelist votes if they heighten legal risks. While audits may well have both supply-side and demand-side effects, we test which side dominates by examining the impact of audits on the price of clientelist handouts. As shown in Online Appendix Figure

A1, if audits lead citizens to reduce their supply of clientelist votes, the equilibrium price of votes would be expected to increase. By contrast, if audits lead politicians to reduce their demand for clientelist votes, the equilibrium price of votes would be expected to decrease. In both cases, we would expect a decreased equilibrium quantity of purchased votes, as observed in Table 2 above. To test the effect of audits on the price of clientelism, we examine a follow-up question that asked recipients to estimate the value of the handout they received during the 2012 electoral campaign.[17] As shown in Table 3, audits led to a 39 percent increase in the value of handouts among this subset of recipients in the 2012 campaign (column 1, p = .026). Without denying the possibility that both effects coexist, this increase in prices suggests that audits' effects on voters' supply of clientelist votes dominate any effects of politicians' demand for clientelist votes. Beyond this price increase among recipients, we also examine if the average amount of money received across *all* respondents falls, given the finding in Table 2 that fewer citizens receive handouts. As expected, column 2 shows that audits led to a 23 percent decrease in the average amount received across the overall sample (i.e., including recipients and non-recipients).

To investigate further whether audits reduce citizens' supply of clientelist votes, we also consider a vignette in our survey. We first asked respondents if they would accept a vote-buying offer of R$ 10 from a fictitious candidate, increasing the offer until acceptance. As shown in column 3 of Table 3, audits reduced the share of citizens who would sell their vote for R$ 100 or below by 3.8 percentage points (p = .046). We focus on this price point because the median value of actual campaign handouts reported in our sample was R$ 120 (approximately USD$ 60 in 2012). Online Appendix Table A2 shows that point estimates are similar, though significance falls to the 10 percent level, when examining audits' effect on the cumulative share of citizens who agreed to vote-buying offers up to R$ 200, R$ 500 or R$ 1,000. By contrast, audits did not affect the share of citizens willing to accept offers

[17] As described in Online Appendix C, respondents were asked this follow-up if they answered affirmatively to the question analyzed in column 1 of Table 2.

of R$ 10, R$ 25, or R$ 50; however, only 24% of actual campaign handouts reported by our respondents were valued at R$ 50 or less. Taken together, this vignette and the increased price of actual handouts suggest that audits reduced citizens' willingness to sell their votes.

In the Online Appendix, we show that this evidence is consistent with Anderson, Francois, and Kotwal's (2015) model of clientelism. Our adaptation of their model suggests that if audits worsen citizens' perceptions of valence characteristics of clientelist candidates, then the quantity of votes sold would decrease and their price would increase. To investigate this potential mechanism further, we examine the effect of audits on citizens' perceptions of politicians' honesty and competence, two valence characteristics often examined in the literature on corruption and political economy more broadly (Dal Bó and Finan 2018). Our survey asked respondents to rate the level of honesty and competence of the top-two mayoral candidates in the 2012 election, on a four-point scale from "Very Bad" to "Very Good." As shown in column 4, audits lead to a reduction of 0.11 points in the average assessment of candidates' honesty. This finding is significant at the 1 percent level and equivalent to 0.22 standard deviations of the honesty scale. Moreover, audits lead to a reduction of 0.09 points in the average assessment of candidates' competence, equivalent to 0.19 standard deviations of the competence scale. This evidence suggests audits lead citizens to view politicians as less honest and competent; as noted, these worsened perceptions are expected to render contingent exchanges more costly for politicians and to decrease equilibrium clientelism levels.

We also explore whether citizens in audited municipalities perceive their local politicians as less reciprocal, which may lead citizens to believe politicians are likely to renege on clientelist agreements. Such beliefs might similarly be expected to make it more costly for politicians to buy votes and decrease its prevalence. To explore whether audits affect citizens' perceptions of politician reciprocity, we employ hypothetical trust games described in Section 3.2. Table 3 shows that audits 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). Thus, as with honesty and competence, audits reduce the perceived reciprocity of politicians.

Furthermore, audits reduce citizens' interactions with politicians, which may contribute to worsening perceptions of politicians just discussed, and may also undermine clientelism more generally. As discussed in Bobonis et al. (2022), frequent interactions often facilitate ongoing clientelist relationships, which tend to involve face-to-face exchanges between voters and elites. Audits may reduce these interactions by making citizens reluctant to engage with politicians, or by decreasing candidates' efforts to cultivate relationships with voters. To investigate the effect of audits, column 8 examines whether respondents conversed at least monthly with a local politician before the 2012 electoral campaign began. Audits caused monthly interactions with politicians to fall by 8.3 percentage points ($p < .001$). This effect represents a marked 42 percent decline, given that 19.7 percent of respondents in unaudited municipalities reported monthly interactions.[18] Column 9 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 ($p < .001$).

While not dispositive, these results are consistent with audits reducing voters' supply of clientelist votes. Findings in Table 3 suggest that citizens in audited municipalities become less willing to sell their votes; they have worsened perceptions of politicians' honesty, competence and reciprocity; and they interact less frequently with politicians.

[18]Online Appendix Table A2 shows that similarly, the share of respondents having weekly interactions with politicians fell by 5.0 percentage points ($p < .001$) — an even larger 58 percent decline in proportional terms.

5.3 Mechanisms: Politician Responses

We next turn to another reason why anti-corruption audits might reduce clientelism: they may also dampen politicians' demand for clientelism (e.g., if audits heighten legal risks). To this end, we draw on insights from Avis et al. (2018) — a study that focuses on corruption and does not mention clientelism. Their model with Bayesian learning suggests that audits cause mayors to extract fewer corrupt rents because of legal discipline, electoral discipline, and political selection effects. First, consider legal discipline — in which audits increase politicians' expected cost of facing legal action for corrupt acts — which they find accounts for 72 percent of the reduction in corrupt rents caused by randomized audits across Brazil (p. 1959). One might also expect this legal discipline mechanism to reduce clientelism: politicians who extract fewer corrupt rents may consequently have less resources available for clientelism, and they may likewise expect higher costs of legal action for distributing illicit campaign handouts. Without denying that this mechanism may reduce clientelism across Brazil, we do not find evidence of it within our sampled municipalities. Using data in Avis et al. (2018), we find that audited municipalities are not more likely to have crackdowns, legal actions, or convictions than unaudited municipalities in our sample (see Online Appendix Table A3). We also found no mention of clientelism in any of the audit reports released by the CGU for our sample.

With regards to electoral discipline in Avis et al.'s (2018) model, audits can induce first-term mayors — who are eligible for reelection in Brazil — to refrain from corrupt acts because they are concerned voters will detect them and punish them electorally. The authors find that electoral discipline accounts for 28 percent of the reduction in corrupt rents caused by Brazil's randomized audits (p. 1959). This mechanism may also reduce clientelism: first-term politicians who extract fewer corrupt rents may have less funds available for clientelism during their reelection campaigns, and audits may heighten their concern that voters will detect that they are distributing campaign handouts to others and punish them electorally. To examine this mechanism, we build on Avis et al.'s (2018) test of the electoral discipline mechanism: whether audits' effects vary across first-term and second-term mayors. Although this interaction is weakly powered in our sample, we observe no significant differences (see Online Appendix Table A4). While this mechanism may well hold in Brazil more broadly, we thus do not find evidence of electoral discipline within our sample.

Another mechanism explored by Avis et al. (2018) is that audits might reduce corruption by improving the quality of candidates who enter politics or win reelection. If audits had such political selection effects, one might also expect that higher-quality candidates would engage in less clientelism. Yet Avis et al. argue that political selection accounts for less than one percent of audits' effects on corruption across Brazil — and find that audits did not affect characteristics of the candidate pool or elected politicians, or of the competitiveness of mayoral elections. Similarly, we do not observe significant differences between audited and unaudited municipalities in our sample when investigating this potential mechanism (see Online Appendix Tables A5 and A6). In the 2012 election, the education, gender, and age of mayoral candidates — as well as of elected mayors — are statistically indistinguishable between our sample's audited and unaudited municipalities. Likewise, we do not observe significant differences for the number of candidates, vote margin, or campaign expenses. Also belying the political selection mechanism, we do not find evidence of strategic dropout or lower reelection rates due to audits within our sample. In audited municipalities, all first-term mayors ran for reelection in 2012, and 40 percent were reelected. In unaudited municipalities, 68 percent of first-term mayors ran for reelection, and 38 percent of those running were reelected.[19] Overall, while we cannot definitively rule out this potential mechanism, we do not find evidence that political selection explains our results.

Regarding politicians' demand for clientelist votes, we thus do not find evidence that the legal discipline, electoral discipline, or political selection mechanisms explain why audits reduce clientelism, but such mechanisms may well play a role beyond our sampled municipalities. In sum, without denying the possibility that both effects coexist, findings

[19] Online Appendix Table A5 shows these results adjusting for state fixed effects.

from our sample suggest that audits' effects on voters' supply of clientelist votes dominated any effects of politicians' demand for clientelist votes.

5.4 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 ($p = .204$). 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 1.2 percentage points. In this case, we can reject the null hypothesis that short and longer-term effects are equivalent ($p = .023$). 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 ($p = .028$), 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 also examine effects on fulfilled requests. 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 ($p = .175$). 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 ($p = .010$), 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.

6 Conclusion

This study provides novel evidence that interventions aimed at reducing corruption can also decrease clientelism. We exploit Brazil's randomized 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 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, our findings are most consistent with audits reducing citizens' supply of clientelist votes. Citizens in audited municipalities become less willing to sell their votes; they have worsened perceptions of politicians' honesty, competence, and reciprocity; and they interact less frequently with politicians. Regarding politicians' demand for votes, we do not find evidence of the legal discipline, electoral discipline, or political selection mechanisms; however, we cannot definitively rule out such mechanisms. Without denying the possibility that both effects coexist, findings suggest that audits' effects on voters' supply of clientelist votes dominated any effects of politicians' demand for clientelist votes in our sampled municipalities.

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.[20] Yet citizen welfare could also arguably be undermined if misappropriated funds from corruption largely flow

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

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.[21] An important direction 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 (e.g., Ramos et al. 2022).

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 effects on politicians' demand for clientelist votes, 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.

[21] Avis et al. (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 points), and their reported average population size of 23,599.

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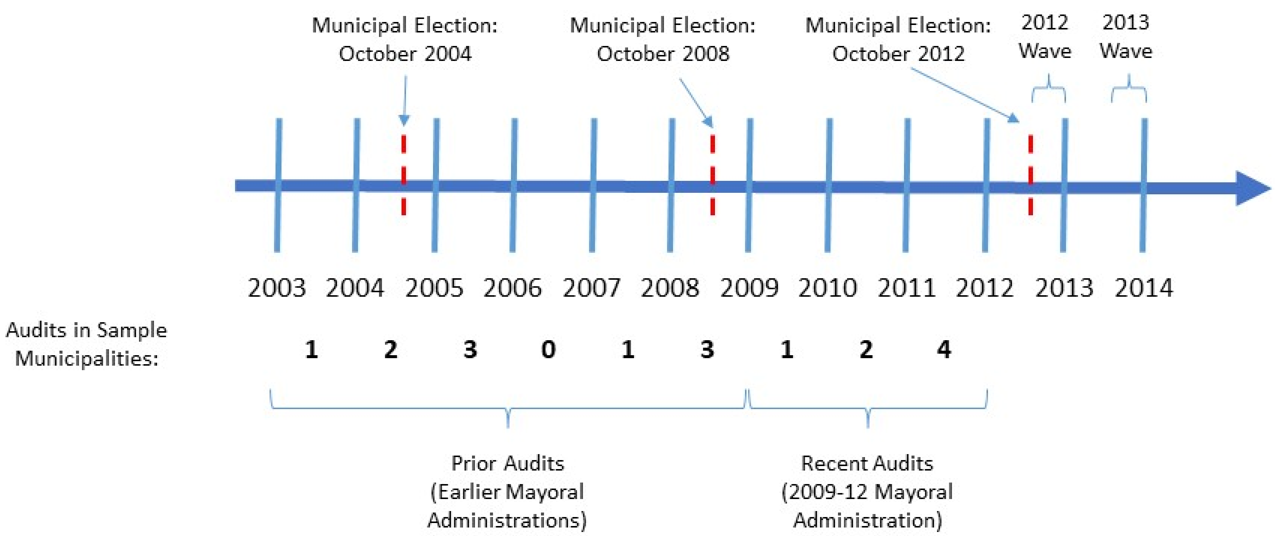
Figures

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)	0.180	0.385	3,664
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

Note: See Online Appendix C for definitions and coding of key variables.

Table 2: Audits' Effects 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	-0.030**		-0.039***	
	(0.013)	(0.019)	(0.014)		(0.010)	
β_2: Audit × 2012				-0.021		-0.043***
				(0.021)		(0.015)
β_3: Audit × 2013				-0.038***		-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 their 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 survey questions examined in this table are provided in Online Appendix C.

Table 3: Audits' Effects on Handout Value, Willingness to Sell Votes, Perceptions and Interactions

	Log Value of Campaign Handouts (2012)		Willingness to Sell Vote (2012)	Perceptions of Mayoral Candidates (2012)		Opinion about Own Politician Reciprocity (2013)		Interactions with Politicians, Before Campaign (2012)	
	Handout Recipients	Overall Sample	For R$100	Honesty	Competence	Continuous Measure	Binary Measure	Monthly	Monthly & Declared
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
β_1: Audit	0.393**	-0.227**	-0.038**	-0.109***	-0.093***	-0.006**	-0.041**	-0.083***	-0.057***
	(0.176)	(0.109)	(0.019)	(0.027)	(0.035)	(0.003)	(0.017)	(0.020)	(0.017)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	195	3,657	3,643	2,817	3,085	3,656	3,656	3,664	3,663
Mean of Y: Unaudited	226.923	12.100	0.157	2.852	2.824	0.025	0.211	0.197	0.129

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 is the log of the value of campaign handouts received during the 2012 campaign + 0.01; the top 5% of observations are winsorized. Column 1 restricts analysis to respondents who reported receiving a handout and column 2 reports results for the full sample. For these two columns, "Mean of Y: Unaudited" reports levels not logs. In column 3, the dependent variable is an indicator variable coded 1 for respondents who answered they would be willing to sell their vote in a vignette for R$ 100 or less (see Section 3.2 for details). In columns 4-5, the dependent variable is a four-point scale (from "Very Bad" to "Very Good") in which the respondent rated the honesty and competence of each of the top-two mayoral candidates in the 2012 election; each respondent's ratings are averaged across the two candidates. 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. In column 8, the dependent variable refers to whether the respondent had conversations with a politician at least monthly before the campaign began. In column 9, it refers to whether the respondent had monthly interactions with a politician before the campaign *and* publicly declared support for a candidate during the 2012 election.

Table 4: Audits' Effects 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**	-0.014	-0.049***		-0.044***	
	(0.015)	(0.022)	(0.015)		(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	0.024	-0.012	-0.014	-0.025**	-0.027***
	(0.013)	(0.022)	(0.014)	(0.014)	(0.010)	(0.010)
Test of homogeneous effects (p-value):						
(a) $H_0 : \beta_1 = \beta_4$	0.204	0.178	0.023		0.175	
(b) $H_0 : \beta_2 = \beta_3$				0.028		0.010
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 their 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). Recent Audit refers to audits during the 2009-2012 mayoral administration; Past Audit refers to earlier audits (i.e., between 2003-2008).

Online Appendix for:

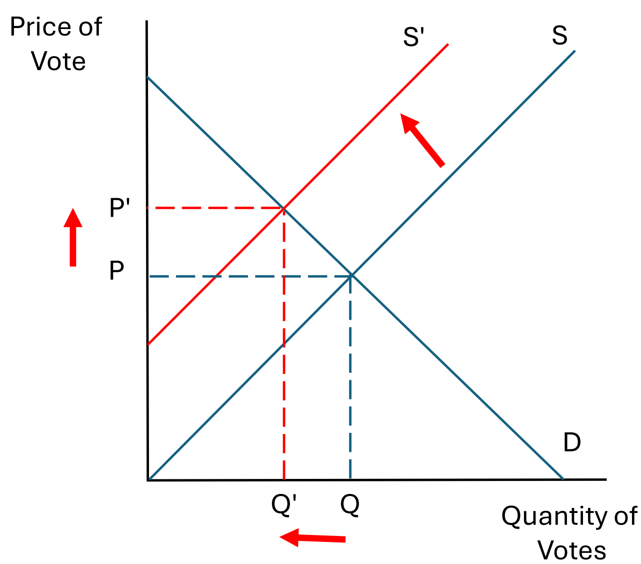
Does Combating Corruption Reduce Clientelism?

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

Appendix A: Figures and Tables

Figure A1: Comparison of Decreased Supply of Votes vs. Decreased Demand for Votes

Decrease in Supply
of Votes



Decrease in Demand
for Votes

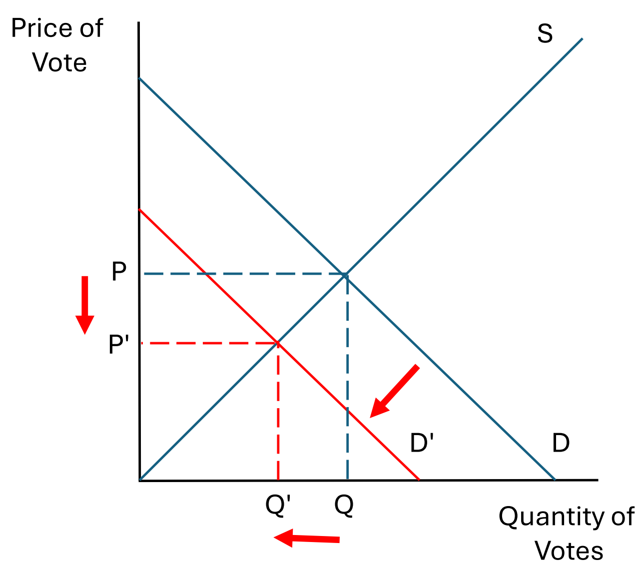


Table A1: Balance Tests (2012 and 2013)

	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: Audits' Effects on Willingness to Sell Votes and Citizen Interactions (Additional Specifications)

	Willingness to Sell Vote (2012)							Interactions with Politicians Before Campaign (2012)
	For R$10	For R$25	For R$50	For R$100	For R$200	For R$500	For R$1000	Weekly
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1: Audit	0.005	0.002	-0.019	-0.039**	-0.038*	-0.039*	-0.042*	-0.050***
	(0.011)	(0.014)	(0.017)	(0.019)	(0.021)	(0.023)	(0.024)	(0.013)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	3,645	3,649	3,644	3,643	3,642	3,638	3,634	3,664
Mean of Y: Unaudited Group	0.045	0.067	0.106	0.157	0.190	0.257	0.329	0.086

Note: This table provides additional specifications for Table 3 (for columns 3 and 8). 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 1-7, the dependent variable is an indicator variable coded 1 for respondents who answered they would be willing to sell their vote in a vignette for the amount shown in the column header or less (see Section 3.2 for details). In column 8, the dependent variable is coded 1 if the respondent reported having conversations with a politician or their representative at least weekly before the campaign began.

Table A3: Audits' Effects on Crackdowns, Convictions, Legal Actions

	Crackdowns	Convictions	Legal Actions
	(1)	(2)	(3)
β_1: Audit	-0.001	-0.038	-0.038
	(0.003)	(0.030)	(0.030)
Municipality Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
N	400	400	400
Mean of Y: Unaudited Group	0.003	0.032	0.035

Note: This table adapts columns 1, 3 and 5 in Table 4 of Avis et al. (2018), by restricting their panel dataset to include only our sampled municipalities through 2012. Specifications include municipality and year fixed effects. Robust standard errors are clustered by municipality and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. As Avis et al. (2018) explain, in column 1 "the dependent variable is whether a police crackdown on political corruption was conducted in the municipality in a given year." In column 2, "the dependent variable is whether a mayor was prosecuted for corruption in a given year." In column 3, "the dependent variable is whether a police investigation or a conviction occurred."

Table A4: Audits' Effects on Private Goods Provision, Requests and Promises (First vs. Second-Term Mayors)

	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)
β_1: Recent Audit	-0.044**	0.009	-0.060**	-0.063***
	(0.021)	(0.034)	(0.024)	(0.020)
β_2: Recent Audit × First-Term Mayor	0.012	-0.036	0.020	0.027
	(0.024)	(0.042)	(0.029)	(0.025)
β_3: First-Term Mayor	-0.016	0.011	-0.028	-0.009
	(0.016)	(0.027)	(0.018)	(0.013)
β_4: Past Audit	-0.024*	0.029	-0.021	-0.029**
	(0.014)	(0.025)	(0.016)	(0.011)
State Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
N	3,681	3,681	7,416	7,408
Mean of Y: Unaudited Group	0.059	0.196	0.140	0.089

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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Specifications examine whether effects on each dependent variable are heterogeneous across mayors who were in their first vs. second terms in 2009-2012. First-term mayors are eligible for immediate reelection in Brazil; second-term mayors are not. In column 1, the dependent variable refers to whether the respondent reported receiving a private good from a campaign visit to their home in 2012. In column 2, it refers to whether the respondent reported receiving a promise of private goods during such campaign visits. In column 3, it refers to whether the respondent requested a private good from a politician (in 2012 or 2013). In columns 4, it refers to whether the respondent reported receiving a private good requested from a politician (in 2012 or 2013). Recent Audit refers to audits during the 2009-2012 mayoral administration; Past Audit refers to earlier audits (i.e., between 2003-2008).

Table A5: Characteristics of 2012 Mayoral Election (Audited vs. Unaudited Municipalities)

	Number of Candidates	Vote Margin	Log Campaign Expenses	Incumbent Runs for Reelection	Incumbent Wins Reelection
	(1)	(2)	(3)	(4)	(5)
β_1: Audit	0.089	-0.057	-0.208	0.326*	-0.053
	(0.362)	(0.034)	(0.396)	(0.177)	(0.315)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
N	40	40	116	27	21
Mean of Y: Unaudited Group	2.800	0.142	12.881	0.684	0.385

Note: This table employs OLS specifications to examine characteristics of the 2012 mayoral election; Chernozhukov et al.'s (2018) double/debiased machine learning technique is not employed given the sample size. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. In column 1, the dependent variable is the number of mayoral candidates. In column 2, it is the vote margin between the top two mayoral candidates (as a share of valid votes). In column 3, it is the log of campaign expenses for each of the mayoral candidates in the 2012 election. Column 4 is coded 1 if the municipality has a first-term mayor who runs for reelection, and is coded 0 if the first-term mayor does not run for reelection. Column 5 is coded 1 if the municipality has a first-term mayor who wins reelection, and is coded 0 if the first-term mayor runs for reelection but loses.

Table A6: Candidate Characteristics in 2012 Election (Audited vs. Unaudited Municipalities)

	All Candidates			Elected Candidates		
	HS Graduate	Female	Age	HS Graduate	Female	Age
	(1)	(2)	(3)	(4)	(5)	(6)
β_1: Audit	0.052	0.051	0.822	-0.0091	-0.0864	4.552
	(0.054)	(0.091)	(2.477)	(0.080)	(0.130)	(4.514)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
N	116	116	116	40	40	40
Mean of Y: Unaudited Group	0.900	0.143	45.957	0.960	0.120	44.440

Note: This table employs OLS specifications to examine candidate characteristics in the 2012 mayoral election; Chernozhukov et al.'s (2018) double/debiased machine learning technique is not employed given the sample size. Robust standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Columns 1-3 include all mayoral candidates; columns 4-6 include only elected mayoral candidates. In columns 1 and 4, the dependent variable is coded 1 if the candidate is a high school graduate; 0 otherwise. In columns 2 and 5, it is coded 1 if the candidate is female; 0 if male. In columns 3 and 6, the dependent variable is the candidate's age in years.

Table A7: Effects of Audits on Private Goods Provision, Requests, and Promises (OLS Version of Table 2)

	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	-0.029*	0.011	-0.029		-0.040***	
	(0.017)	(0.022)	(0.023)		(0.014)	
β_2: Audit × 2012				-0.021		-0.045*
				(0.034)		(0.023)
β_3: Audit × 2013				-0.037		-0.036***
				(0.022)		(0.012)
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: This table is identical to Table 2, except that it uses OLS specifications instead of 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 their 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 survey questions examined in this table are provided in Online Appendix C.

Table A8: Audits' Effects on Handout Value, Willingness to Sell Votes, Perceptions and Interactions (OLS Version of Table 3)

	Log Value of Campaign Handouts (2012)		Willingness to Sell Vote (2012)	Perceptions of Mayoral Candidates (2012)		Opinion about Own Politician Reciprocity (2013)		Interactions with Politicians, Before Campaign (2012)	
	Handout Recipients	Overall Sample	For R$100	Honesty	Competence	Continuous Measure	Binary Measure	Monthly	Monthly & Declared
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
β_1: Audit	0.417**	-0.223	-0.038	-0.110***	-0.094	-0.006**	-0.041*	-0.084**	-0.058**
	(0.158)	(0.134)	(0.025)	(0.038)	(0.061)	(0.003)	(0.022)	(0.031)	(0.023)
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	195	3,657	3,643	2,817	3,085	3,656	3,656	3,664	3,663
Mean of Y: Unaudited	226.923	12.100	0.157	2.852	2.824	0.025	0.211	0.197	0.129

Note: This table is identical to Table 3, except that it uses OLS specifications instead of 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 is the log of the value of campaign handouts received during the 2012 campaign + 0.01; the top 5% of observations are winsorized. Column 1 restricts analysis to respondents who reported receiving a handout and column 2 reports results for the full sample. For these two columns, "Mean of Y: Unaudited" reports levels not logs. In column 3, the dependent variable is an indicator variable coded 1 for respondents who answered they would be willing to sell their vote in a vignette for R$ 100 or less (see Section 3.2 for details). In columns 4-5, the dependent variable is a four-point scale (from "Very Bad" to "Very Good") in which the respondent rated the honesty and competence of each of the top-two mayoral candidates in the 2012 election; each respondent's ratings are averaged across the two candidates. 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. In column 8, the dependent variable refers to whether the respondent had conversations with a politician or their representative at least monthly before the campaign began. In column 9, it refers to whether the respondent had monthly interactions with a politician before the campaign *and* publicly declared support for a candidate during the 2012 election.

Table A9: Effects of Audits on Private Goods Provision, Requests and Promises – By Audit Term (OLS Version of Table 4)

	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	-0.036*	-0.012	-0.049**		-0.052***	
	(0.019)	(0.023)	(0.021)		(0.016)	
β_2: Recent Audit × 2012 wave				-0.072**		-0.074***
				(0.027)		(0.023)
β_3: Recent Audit × 2013 wave				-0.024		-0.029**
				(0.023)		(0.013)
β_4: Past Audit	-0.018	0.024	-0.012	-0.014	-0.022	-0.023*
	(0.016)	(0.031)	(0.022)	(0.022)	(0.013)	(0.013)
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: This table is identical to Table 4, except that it uses OLS specifications instead of 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 their 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).

Appendix B: Model of Clientelism

As discussed in the Introduction and Section 5.2, we formalize our results by adapting Anderson, Francois, and Kotwal's (2015) [henceforth AFK] theoretical model of clientelism. The following discussion extends our Online Appendix in Bobonis et al. (2022) by presenting comparative statics for an additional parameter and adapting the exposition to the topic of the present paper. Other aspects below include substantial verbatim text from our Online Appendix in Bobonis et al. (2022).

In AFK's model, clientelist politicians undermine policies for poor and vulnerable households, so that they can facilitate clientelist arrangements. These clientelist arrangements involve informal insurance transfers — more specifically, in contingent exchange for votes, clientelist politicians provide transfers to particular citizens if they experience negative shocks. Clientelist politicians make such arrangements in order to increase the likelihood that they win election, and they provide lower levels of public goods while in office to extract rents that can be partially used for these clientelist transfers. We adapt AFK's model to examine implications of the introduction of an intervention — such as anti-corruption audits — that affects citizens' perceptions of valence characteristics of clientelist candidates (e.g., their honesty and competence). As shown below, if audits exogenously reduce such perceptions, the model predicts a decrease in votes traded in exchange for state-contingent clientelist transfers. Moreover, for those exchanges that do occur, the model predicts an increase in the value of equilibrium transfers.

The Model

Setup

Each individual l is either a citizen i (i in M) or a politician j (j in P); there are 2n citizens and a number of politicians normalized to size 1 in the municipality, where $1 << n$. Each individual has a type regarding clientelism (denoted c_l), either $c_l = C$, or $c_l = N$, denoting clientelist and non-clientelist types (respectively). Each agent is thus identified by their political class (M, P) and clientelism type (C, N). Citizens own negligible land or capital and make private good consumption decisions from an exogenous source of state-contingent income (y_s), where $s \in \{g, b\}$ respectively denote the good and bad states of the world; the latter occurs with probability $\mu \in (0, 1)$. They also enjoy utility from the consumption of a public good (G) provided by the government. Clientelist citizens ($c_i = C$) tend to have stronger relationships with clientelist politicians ($c_j = C$) than do non-clientelist citizens ($c_i = N$); this will be formally specified below.

Officeholders are tasked with providing pro-poor public goods to citizens. There are two coalitions of politicians: incumbents and challengers. The incumbent coalition has access to existing government revenue from federal transfers (T). Following AFK, we assume that all politicians in the incumbent coalition are clientelist types who expend this exogenous revenue stream on public goods (G) and pecuniary rents (R). All politicians enjoy ego rents (E) from office. We also assume that clientelist types — unlike non-clientelist types — extract pecuniary rents R while in office in part to fund clientelist transactions described below. Also following AFK, we assume that when in office, the clientelist coalition's expenditure on public goods ($\tilde{G}$) is strictly lower than the non-clientelist coalition's expenditure on public goods (G). We assume that the challenging coalition is composed of non-clientelist types.[22]

[22]This assumption is a simplification of a more complex scenario in which challengers could be clientelist or non-clientelist, with their type drawn at random from the pool of potential politicians. In this alternative scenario, clientelist opposition candidates may engage in vote trading with citizens via similar insurance

Citizens have additively-separable preferences over the consumption of the private good consumed from state-contingent income (y_s) and from S_i^j, a possible insurance transfer from clientelist politician j to citizen i, the aforementioned public good, and idiosyncratic preferences for the incumbent coalition:

$$U_{ik}(C_{jk}) = v(\widetilde{G}) + \mu u(y_b + S_i^j) + (1-\mu)u(y_g) + \phi_k, \tag{2}$$

where U_{ik} denotes the expected utility outcome corresponding to the coalition in parentheses controlling the municipal government, in this case a clientelist government. Citizens exhibit decreasing marginal utility (and risk aversion) over the consumption of the private good ($u' > 0$, $u'' < 0$) and the public good ($v' > 0$, $v'' < 0$). The ϕ_k term, drawn from distribution $g(\phi_k)$, represents the citizens' idiosyncratic preferences for the incumbent coalition j in municipality k. Citizens' perceptions of higher-quality valence characteristics of clientelist candidates (e.g., their honesty and competence) would imply an increase in ϕ_k. Inversely, their perceptions of lower-quality valence characteristics of such candidates would imply a decrease in ϕ_k.

Politicians are risk neutral and seek to maximize the expected value of office, net of informal insurance arrangements they have promised to clientelist citizens in contingent exchange for electoral support. Through these arrangements, politicians trade informal insurance — which provides transfers during a state of need (i.e., the bad state) — for votes. Such informal insurance transfers would be needed to cover, for example, medical expenses for health shocks to a household member, loss or damage to a household asset such as the dwelling, as well as basic needs (e.g., water). An insurance promise is a commitment by the politician to a transfer when needed by the citizen. We assume that the need state is observable to both politicians and citizens but is unenforceable by formal/legal mechanisms. As mentioned above, S_i^j denotes the value of the insurance transfer from clientelist politician j to individual i, where the magnitude of S_i^j depends on the extent of the insurance commitment.

To maintain power, the incumbent coalition must ensure they receive at least n votes in order to win the election. To this end, members of the incumbent coalition divide vote trading responsibilities symmetrically. Each politician has an incentive to free-ride on the vote-trading of his colleagues; to overcome this, they impose sanctions on individuals who renege in their obligations. Following AFK, we assume that a clientelist politician j receives a punishment X_C imposed by all the other clientelist politicians if he reneges on his promise to citizen i. In contrast, no clientelist insurance agreements take place between opposition candidates and citizens; thus, the punishment clientelist politicians impose on each other is greater than the punishment non-clientelist politicians would impose $(X_N), or X_C \geq X_N = 0$. In addition, clientelist citizens can impose non-pecuniary punishments $X \geq 0$ on politicians who renege on the insurance obligation in the case of need; it is equivalent to (and can be interpreted as) the utility loss to the politician from a breakdown of a relationship with a clientelist citizen.

Finally, in addition to the costs or punishments common to all individuals of a particular type, we follow AFK and allow for each politician-citizen pair to share a common idiosyncratic history that generates utility loss (x_i^j) to the politician if he reneges on the promise of an insurance transfer to citizen i in the state of need. This captures (in a reduced-form manner) the loss to the politician of the continuation value of the relationship with the citizen. Consistent with the literature characterizing the structure and value of relationships in social networks (e.g., Jackson and

promises. If their types are known to citizens, this complicates the analysis in the model but does not affect the theoretical results. While such modeling assumptions do not precisely match reality, our survey data suggest that incumbent candidates are indeed more clientelistic than challengers (see Bobonis et al. 2022).

Wolinsky 1996; Johnson and Gilles 2000), most ties tend to be relatively weak and socially distant. We thus assume that the distribution of these relationship values is randomly and independently drawn from a cumulative distribution $F(x_i^j)$ with unimodal and decreasing density.[23]

Therefore, clientelist politicians will choose the structure of insurance commitments to maximize their payoff:

$$Max_{S_i^j}\ P_{win|VT}(k)[E+R] - \mu n S_i^j, \tag{3}$$

where $P_{win|VT}(k)$ denotes the probability that the incumbent politicians win reelection under clientelism (i.e., vote trading), subject to the government budget constraint and a set of individual rationality and incentive compatibility constraints. Specifically, insurance transfers between each politician j and citizen i must satisfy the incentive compatibility condition that the value of the transfer should not be greater than the cost to the politician of reneging on the promise, or:

$$S_i^j \leq (X_i + I_i^j X + x_i^j), \tag{4}$$

where I_i^j is an indicator variable equal to one if both the citizen and the politician are clientelist types ($c_i = C$ and $c_j = C$), and $I_i^j = 0$ otherwise. The informal insurance arrangement must also satisfy politicians' individual rationality constraint, or

$$P_{win|VT}(k)[E+R] - \mu n S_i^j \geq P_{win|NVT}(k)[E+R], \tag{5}$$

where $P_{win|NVT}(k)$ denotes the probability that incumbent politicians win the election if they refrain from engaging in clientelism. Finally, the scheme requires each incumbent politician j's actions to be compatible with the citizen's decision to enter the informal contract with him. That is, the citizen's expected utility from voting for a clientelist government must be greater than or equal to his expected utility from voting for the challenging coalition (the citizens' IR constraint), or $U_{ik}(C_j) \geq U_{ik}(N)$. In the absence of clientelist insurance, the non-clientelist opposition politicians (N) would win the election, and in that case the citizen's utility is:

$$U_{ik}(N) = v(G) + \mu u(y_b) + (1-\mu)u(y_g). \tag{6}$$

Timing

The timing of the model is as follows: (1) Incumbent politicians, and citizens, can make clientelist insurance arrangements. Each arrangement specifies a transfer S_i^j from an incumbent politician to a citizen if in the state of need (i.e., the bad state), in exchange for the citizen's vote. (2) The state is revealed to both parties. (3) Each politician chooses the transfer level if the bad state arises. (4) Elections occur. If the need state occurred and the transfer received by citizen i is (at least) S_i^j, he casts his vote for the incumbent politician with whom he made a clientelist arrangement. If the bad state occurred and the transfer received is less than S_i^j, he casts his vote against the incumbent politician. Sanctions by other clientelist politicians and citizens are

[23]For example, this structure is satisfied by assuming that $F(x_i^j)$ follows a Pareto distribution with minimum $x_m > 0$ and scale parameter $\alpha > 0$. This assumption regarding the shape of the distributional of these relationship values is consistent with the empirical observation across multiple contexts that most ties tend to be relatively weak and socially distant (e.g., Banerjee et al. 2013; Cruz 2019; Duarte et al. 2019) and with the role of close relationships in the self-enforcement of informal contracts or arrangements (Chandrasekhar, Kinnan, and Larreguy 2018). We make the independence assumption for purposes of tractability.

imposed on any reneging incumbent politician. If the bad state does not arise, citizens in clientelist arrangements vote for incumbent politicians as promised.

Characterization of Equilibrium

Following AFK, we first present the conditions under which a clientelistic relationship produces a surplus of a given citizen-politician pair. That is, clientelist vote trading is both individually rational and incentive compatible for a citizen (i)-politician (j) pair if and only if:

$$x_i^j \geq u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)) - y_b - X_i - I_i^j X, \tag{7}$$

where $\triangle v(\widetilde{G}) = v(G) - v(\widetilde{G})$ represents the gap in the citizen's utility value of the public good offered by the non-clientelist and clientelist politicians. Specifically, the clientelist insurance arrangement takes place if and only if:

$$x_i^j \geq \begin{cases} u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)) - y_b - (X_C + X) \; for \; c_i = C \; and \; c_j = C \\ u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)) - y_b - X_C \; for \; c_i = N \; and \; c_j = C \end{cases}. \tag{8}$$

Proofs of all results are presented at the end of this Appendix.

A high value of the incumbent coalition's valence characteristics (ϕ_k) makes it less costly for citizens to vote for the clientelist candidates, and their individual rationality easier to satisfy. A high value of the idiosyncratic utility loss (x_i^j) to the politician makes reneging on a promised transfer a more costly action, and hence supports a greater range of incentive compatible transfers from them in return for citizens' votes. When citizens and politicians are in a clientelist relationship ($c_i = C$ and $c_j = C$) (condition (a) in equation 8), this sustains higher punishments, X, and hence makes higher transfers incentive compatible. Because citizens who do not have a relationship with a clientelist politician cannot punish him, the citizen-induced punishment X term disappears in equation (8) condition (b), and so only other clientelist politicians can punish (X_C) the reneging politician; this limits the range of incentive compatible transfers to non-clientelist citizens.

Defining the Likelihood of Clientelist Insurance and Transfer Levels

The probability of clientelist insurance (and thus of individual vote trading) for clientelist and non-clientelist citizen types can be defined respectively as:

$$P_{VT}[k|c_i = C] = 1 - F[u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)) - y_b - (X_C + X)], \tag{9}$$

and

$$P_{VT}[k|c_i = N] = 1 - F[u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)) - y_b - (X_C)]. \tag{10}$$

We next consider the relationship between these individual conditions and the likelihood of clientelism and vote trading in aggregate. Because no single clientelist politician can manage to independently control all clientelist insurance arrangements, the group must be able to contract votes from a sufficiently large number of citizens to ensure a majority in the election. Following AFK, we assume that if and only if a majority of politicians find it individually rational to accept incentive-compatible transfer arrangements, then vote trading occurs and clientelist politicians can exert control. It is equivalent to assuming that politicians have the capability to act in their collective interests; if there are sufficient gains to be made from engaging in clientelism, we assume that it occurs. If, however, the votes that can be feasibly traded by clientelist politicians are not sufficient for them to gain control of the municipal government, they do not engage in the practice.

In order to move from individual-level measures of the likelihood of clientelist insurance and electoral support for the incumbent coalition, we aggregate in the following way to municipal-level outcomes. Denote $P_{win|VT}[k]$ as the proportion of citizens who enter clientelist arrangements, and hence vote for the incumbent group, in municipality k:

$$P_{win|VT}[k] = \sigma_{CC,k} P_{VT}[k|c_i = C] + \sigma_{NC,k} P_{VT}[k|c_i = N] \tag{11}$$

where σ_{ij} are the frequencies of citizen i and politician j pairs in municipality k. Similarly, the transfer level required to ensure citizens agree to vote trade must satisfy the condition that citizens are willing to vote for the incumbent group, or:

$$v(\widetilde{G}) + \mu u(y_b + S_i^j) + (1-\mu)u(y_g) + \phi_k \geq v(G) + \mu u(y_b) + (1-\mu)u(y_g). \tag{12}$$

This implies that the level of transfers must satisfy the following condition:

$$S_i^{j*} \geq u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)) - y_b. \tag{13}$$

Comparative Statics

Whereas the Online Appendix of Bobonis et al. (2022) derives other comparative statics, the present discussion extends that analysis with a specific focus on anti-corruption audits. To that end, we examine the implications of the introduction of an information intervention — such as the audits studied here — that affects citizens' perceptions of the valence characteristics of clientelist candidates (e.g., their honesty and competence). Specifically, we examine: (a) effects on the likelihood that citizens and politicians enter a clientelist insurance arrangement after audits reveal predominantly negative information about politicians, and (b) the value of equilibrium transfers among those who do enter such relationships.

Result 1: An improvement (decline) in citizens' perceptions of valence characteristics (ϕ_k) of clientelist candidates increases (reduces) the probability that both clientelist and non-clientelist citizens will engage in vote trading: $\frac{\partial P_{VT}[k|c_i=C]}{\partial \phi_k} > 0$ and $\frac{\partial P_{VT}[k|c_i=N]}{\partial \phi_k} > 0$.

Result 2: An improvement (decline) in citizens' perceptions of valence characteristics (ϕ_k) of clientelist candidates decreases (increases) the value of equilibrium transfers among those who do enter such relationships: $\frac{\partial S_i^{j*}}{\partial \phi_k} < 0$.

We now clarify why these results provide theoretical motivation for our study. Recall that our treatment examines the effects of anti-corruption audits, which involve a monitoring and information intervention. In the model, we interpret that negative information about corruption released by audits leads citizens to update their perceptions of the valence characteristics of clientelist candidates (e.g., their honesty and competence). As discussed above, citizens' perceptions of lower-quality valence characteristics of such candidates would imply a decrease in ϕ_k. Given Results 1 and 2 above, if audits provide damaging information that leads citizens to perceive politicians as lower-quality in this regard, it renders clientelist arrangements more costly and less prevalent — as it is harder to satisfy citizens' individual rationality constraints.

Proofs of Propositions

Proof of Conditions for Vote Trading to Satisfy IC and IR Constraints:

We first present the conditions under which a clientelist relationship produces a surplus for a given citizen-politician pair. The citizen's IR constraint is:

$$v(\widetilde{G}) + \mu u(y_b + S_i^j) + (1-\mu)u(y_g) + \phi_k \geq v(G) + \mu u(y_b) + (1-\mu)u(y_g) \tag{14}$$

Following AFK's characterization of equilibrium, we assume the politician's incentive compatibility (IC) constraint (condition (9)) is binding. Substituting this IC constraint into the citizen's IR constraint above and rearranging results in the condition:

$$x_i^j \geq u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)) - y_b - X_i - I_i^j X \tag{15}$$

shown as condition (12) above.

Proof of Result 1:

In the case of clientelist citizens, from equation (9) it is the case that, $\frac{\partial P_{VT}[k|c_i=C]}{\partial \phi_k} = (1/\mu)f[u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)) - y_b - (X_C + X)][u^{-1'}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b))]$. Since $[u^{-1'}(((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b))] = \frac{u'(y_b)}{u'(u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b))}$, and the term in the denominator equals the citizen's marginal utility of consumption in the bad state given the minimum transfer level $\underline{S}_i^j$ that satisfies the citizen's IR constraint ($u'(u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b))) = u'(y_b + \underline{S}_i^j)$), then $\frac{u'(y_b)}{u'(u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)))} > 1$ for any $S_i^{j*} > 0$. Therefore, $\frac{\partial P_{VT}[k|c_i=C]}{\partial \phi_k} > 0$. Following the same logic for non-clientelist citizens, based on equation (10) it is the case that $\frac{\partial P_{VT}[k|c_i=N]}{\partial \phi_k} > 0$.

Proof of Result 2:

Since the IR constraint (14) for citizens in a clientelist arrangement binds, it follows that: $\frac{\partial S_i^{j*}}{\partial \phi_k} = (-1/\mu)[u^{-1'}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)) - y_b]$. Following the same argument regarding the sign of the term in brackets for any $S_i^{j*} > 0$ (as in the proof of Result 1 above), it is the case that $\frac{\partial S_i^{j*}}{\partial \phi_k} < 0$.

Appendix C: 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?"
  - If yes: "What was the value in money, more or less, of the help you received?"

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: *Value of Campaign Handouts (asked in 2012).*

- See description of the following variable above: *Politicians Promise Private Goods in Campaign (asked in 2012).*

Variable: *Willingness to Sell Vote (asked in 2012).*

- Definition: Respondent reports whether they would accept a vote-buying offer from a fictitious city councilor candidate. We first asked if they would accept an offer of R$ 10, and then incrementally increased the amount until acceptance (up to R$ 1,000).
- "If a city councilor candidate offers you R$ 10 to vote for him, would you accept it?"
- If no: "If a city councilor candidate offers you R$ 25 to vote for him, would you accept it?"
- If no: "If a city councilor candidate offers you R$ 50 to vote for him, would you accept it?"
- If no: "If a city councilor candidate offers you R$ 100 to vote for him, would you accept it?"
- If no: "If a city councilor candidate offers you R$ 200 to vote for him, would you accept it?"
- If no: "If a city councilor candidate offers you R$ 500 to vote for him, would you accept it?"

- If no: "If a city councilor candidate offers you R$ 1000 to vote for him, would you accept it?"

Variable: *Perceptions of Mayoral Candidates (asked in 2012).*

- Definition: Respondent rates the 2012 mayoral candidates with respect to their honesty and competence. Question was asked about the top-two mayoral candidates in the 2012 election.
- Respondents were first asked if they knew of each candidate:
- "Would you say you know of [NAME]?" Answer choices: "Very Well, Well, Little, Very Little, Never Heard of the Candidate."
- Unless indicating they never heard of the candidate, respondents were then asked:
- I am going to ask you about these politicians, whether they are good or not, your opinion about them. I would just like to remind you that I have no connection with any of them and that your answers will be kept secret. What do you think of [NAME] in relation to:"
- "Their honesty?" Answer choices: "Very Good, Good, Bad, Very Bad."
- "Capacity to do things?" Answer choices: "Very Good, Good, Bad, Very Bad."
- By respondent, ratings for a characteristic are averaged across the two candidates. If a respondent only rates one candidate, that candidate's ratings are used instead of the average.

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."

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)?"