Vulnerability and Clientelism*

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Abstract

Political clientelism is often deemed to undermine democratic accountability and representation. This study argues that economic vulnerability causes citizens to participate in clientelism. We test this hypothesis with a randomized control trial that reduced household vulnerability through a development intervention: constructing residential water cisterns in drought-prone areas of Northeast Brazil. This exogenous reduction in vulnerability significantly decreased requests for private benefits from local politicians, especially by citizens likely to be involved in clientelist relationships. We also link program beneficiaries to granular voting outcomes, and show that this reduction in vulnerability decreased votes for incumbent mayors, who typically have more resources to engage in clientelism. Our evidence points to a persistent reduction in clientelism, given that findings are observed not only during an election campaign, but also a full year later.

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1 Introduction

Many developing countries have adopted democratic forms of government with a primary objective of heightening political representation (Acemoglu and Robinson 2006, Diamond 1999, Hagopian and Mainwaring 2005). However, democratic political institutions have often failed to provide broad representation of poor and vulnerable citizens, who are frequently the majority of constituents. Substantial research suggests that clientelism — the exchange of contingent benefits for political support (Hicken 2011, Kitschelt and Wilkinson 2007) — is an important reason why many elected politicians are neither accountable nor responsive to their constituencies (e.g., Keefer 2007, Stokes et al. 2013). Among the numerous pernicious consequences, many argue that clientelism exacerbates governmental allocative inefficiencies and undermines the functioning of democratic institutions, leading to both reduced political competition as well as the underprovision of public goods and social insurance.[1]

This study focuses on ongoing clientelist relationships in which politicians provide private benefits to citizens conditional on their political support. Why would citizens participate in such clientelist arrangements? Of the many factors posited, perhaps none has garnered more attention than poverty. An extensive theoretical literature points to the decreasing marginal utility of consumption as an underlying reason why impoverished citizens likely place relatively greater value on private consumption than on political preferences or public goods provision (e.g., Dixit and Londregan 1996, Bardhan and Mookherjee 2012). While poverty focuses on the *level* of income, the *uncertainty* of income is also important — a point underscored by Ligon and Schechter (2003), whose theoretical study defines economic vulnerability as encompassing both the level and uncertainty of income. In the present study, we investigate how economic vulnerability affects citizens' participation in clientelism and its consequences for electoral outcomes.

The pervasiveness of vulnerability and clientelism across developing countries raises two important but unexplored questions. First, does economic vulnerability have a causal effect on citizens' participation in clientelism? If so, then clientelism could — at least in prin-

[1]See Bates (1991); Kitschelt and Wilkinson (2007); Baland and Robinson (2008); Piattoni (2001); Bardhan and Mookherjee (2012); Robinson and Verdier (2013); Stokes et al. (2013); and Anderson, Francois, and Kotwal (2015) as examples of the literature characterizing clientelist politics and its consequences.

ciple — be reduced by implementing redistributive and social insurance programs. And second, if vulnerability is indeed a cause of clientelism, what are the electoral consequences of reducing vulnerability? If citizens become less reliant on elected officials as their vulnerability declines, we might expect a reduction in votes for incumbents and thereby a mitigation of any incumbency advantage. The present study advances the literature on clientelism by investigating both questions.

We focus on the interplay of vulnerability and clientelism in Northeast Brazil. This region is the largest pocket of poverty in Latin America, with many residents exposed to a high risk of recurring droughts. Between 2011 and 2013, we undertook a unique longitudinal household survey of a large representative sample of impoverished rural households, with which we measure households' interactions with local politicians before, during, and after Brazil's 2012 municipal elections. Crucially, the data reveal which individuals are likely to have ongoing clientelist relationships with local politicians, as well as important details about the nature of their interactions.

We first establish a set of stylized facts about the relationship between vulnerability and clientelism. To begin, we show that citizens living in municipalities experiencing droughts are more likely to ask politicians for private benefits, especially for water, medicine, and medical treatments. In addition, we find that citizens experiencing droughts are more likely to declare support publicly for politicians, a costly signal that they will provide votes to specific candidates. Public declarations of support are often observed in the context of clientelist relationships and are costly in part because citizens who declared for a defeated candidate may be punished with reduced benefits such as healthcare.[2] We interpret these stylized facts as *prima facie* evidence that many vulnerable citizens facing economic distress rely on their clientelist relationships with local politicians to cope with negative shocks.

Once we establish the link between economic vulnerability and clientelism, we examine whether reducing vulnerability dampens citizens' participation in clientelism. We test this hypothesis using a large-scale randomized control trial that reduced vulnerability through a development intervention. This intervention, which we designed and fielded in partnership with a Brazilian NGO, constructed and provided private, rainfed water cisterns to indi-

[2]Qualitative evidence about such patterns can be found in Nichter (2016).

vidual households. These cisterns collect and store up to 16,000 liters of water, increasing households' resiliency to droughts and enhancing the reliability of their water supply.

In our experiment, we find that citizens in households randomly selected to receive cisterns become less likely to participate in clientelism. The intervention reduces the likelihood that citizens ask local politicians for private benefits by 3.2 percentage points, a substantial decline of 15 percent. As expected, these effects are fully concentrated among citizens who are likely to be in clientelist relationships; that is, citizens who frequently conversed with politicians at least monthly before the beginning of the 2012 electoral campaign. Among such frequent interactors, we find a 11.1 percentage point reduction in citizen requests — a remarkable 32 percent reduction in proportional terms. By contrast, we find no effect among citizens who interacted sporadically if at all with politicians before the election period. A novel aspect of this study is that — unlike nearly all existing quantitative work on clientelism (e.g., Vicente 2014, Hicken et al. 2015) — it provides evidence about the phenomenon during *both* electoral and non-electoral periods. Our analyses show that reduced vulnerability decreases requests among frequent interactors not only during the election campaign, but also during the year after the election. These effects are larger when we restrict attention to municipalities in which incumbent mayors ran for reelection.

Given our finding that reduced vulnerability dampens citizens' participation in clientelism, we examine whether decreased vulnerability also renders citizens less likely to vote for the incumbent mayor during reelection campaigns. Facilitating this analysis is a feature of Brazil's electoral system that provides an extraordinarily granular level of voting data outcomes. Our survey links individual subjects in the experiment to their specific electronic voting machines in the 2012 municipal election. In order to measure electoral responses to the cistern treatment, we can thus compare votes across machines — which have distinct, randomly assigned numbers of treated individuals — located in the same polling places. Our primary estimates indicate that the cisterns treatment decreases the probability that an individual votes for the incumbent mayor by approximately 19 to 22 percentage points. This finding not only suggests that reductions in vulnerability harm incumbents electorally, but also points toward vulnerability as a first-order determinant of clientelism.

More broadly, the present paper makes several contributions to the political economy literature. First, numerous observational studies show correlational evidence that citizens with

low socioeconomic status are more likely to participate in clientelism.[3] Yet it is challenging to establish a causal relationship, in part due to the difficulty of disentangling the role of poverty and risk from those of various unobserved determinants of these practices, such as voters' beliefs, attitudes and preferences.[4] Our study advances the literature by providing compelling causal evidence that reducing vulnerability dampens citizens' participation in clientelist exchanges. Moreover, our electoral findings may be interpreted as corroborating a related hypothesis of Blattman, Emeriau, and Fiala (2017): economic independence frees the poor to express support for opposition candidates. Second, by showing how these changes in the political equilibrium are concentrated among voters in ongoing relationships, our study complements research by Finan and Schechter (2012) and Calvo and Murillo (2013), which documents how vote buying and clientelism operate through established networks based on reciprocal, partisan, or personal ties. Third, an innovative feature of our approach is that it emphasizes the important role that citizens play in clientelism, a demand-side perspective that is overlooked by most quantitative and theoretical work on the topic.

Our project is closely related to recent work by Anderson, Francois, and Kotwal (2015), who develop and test a theoretical model of clientelistic insurance. In their framework, political elites have incentives to curtail government-mandated mechanisms that help poor and vulnerable households cope with shocks, precisely because doing so enables elites to sustain clientelist arrangements. Although they do not empirically examine the effects of introducing independent risk-coping mechanisms, as we do with our water cisterns intervention, their framework has important implications that we test empirically. In particular, their model predicts that exogenous improvements in independent forms of insurance should crowd out citizens' participation in clientelism. The present study is the first to provide compelling evidence consistent with this prediction. Furthermore, our findings suggest that improving formal insurance mechanisms and implementing mandated, centralized pro-

[3]For example, based on a cross-sectional comparison of voters in Argentina following the 2001 election, Brusco, Nazareno, and Stokes (2004) and Stokes (2005) show that 12 percent of low-income respondents reported receiving a gift from a candidate or party, which is higher than the overall incidence of seven percent.

[4]For instance, Finan and Schechter (2012) argue that due to the limited enforceability of vote buying contracts, politicians and their middlemen will target individuals who are more likely to reciprocate, an individual characteristic that is generally difficult to observe.

grams — instead of allowing greater local discretion in the targeting of benefits — may help to promote changes in the de facto political power of elites.[5]

The article is organized as follows. Section 2 provides contextual information about rural Northeast Brazil. We follow with a description of our data sources in Section 3. Using these data, Section 4 presents a descriptive analysis of vulnerability and political interactions. Next, Section 5 discusses our intervention, experimental design and empirical methodology. Section 6 presents the central empirical results of our study and rules out several alternative explanations involving politician responses, citizen engagement, and credit claiming. Finally, Section 7 concludes with a discussion of findings and their broader implications.

2 Context

This study focuses on Brazil's semi-arid zone, the vast majority of which is located in the country's Northeast region. The zone spans over one million square kilometers (see Figure 1), and its population of over 28 million residents is disproportionately poor and rural.[6] It is characterized by far lower average precipitation and higher rainfall variation than the rest of Brazil. In 2012, the zone's average precipitation was just 57.2 cm, compared to 153.1 cm in the rest of the country. A fundamental source of vulnerability is the region's exposure to recurring droughts; its rainfall is temporally concentrated and evaporates quickly due to the topography and temperature (Febraban 2007, 2008).

In part due to droughts, many residents are highly vulnerable to shocks.[7] Credit and savings constraints prevent citizens from procuring sufficient self-insurance, and given the spatial correlation of rainfall shocks, the ability of rural citizens to use informal insurance to address their needs is often limited. Health shocks are another major issue, as inadequate healthcare often ranks as the top concern in opinion surveys across Brazil. Many wealthier Brazilians possess private health insurance, but impoverished citizens are particularly vul-

[5]See, for instance, de Janvry, Finan, and Sadoulet (2012) regarding evidence about discretion in the targeting of conditional cash transfer programs in Brazil and its implications for rent-seeking among local political elites. More recently, La Ferrara, Brollo, and Kaufman (2017) provide evidence that local politics can affect the enforcement of participation requirements in these programs.

[6]The semi-arid region is composed of 1,133 contiguous municipalities in nine states: Alagoas, Bahia, Ceará, Minas Gerais, Paraiba, Pernambuco, Piauí, Rio Grande do Norte, and Sergipe.

[7]In late 2015, the Institute for Applied Economic Research (Instituto de Pesquisa Econômica Aplicada, or Ipea) in Brazil released an "Index of Social Vulnerability," which indicates that vulnerability is "very high" in much of the Northeast region.

nerable to health shocks: the probability of experiencing catastrophic health expenditures is over seven times higher for the poorest quintile than it is for the richest quintile (de Barros et al. 2011).

As in most Latin American countries, many government services and expenditures have been decentralized to the local level (Garman, Haggard, and Willis 2001). Currently, Brazil's government expenditures are among the most decentralized in the world, with most municipalities relying primarily on transfers from higher levels of government to finance expenditures (IMF 2016). The service provision responsibilities of municipal governments in Brazil include aspects of healthcare, education, local infrastructure, and natural resource management (Andersson, Gordillo, and van Laerhoven 2009).

Given their substantial vulnerability to shocks, many Brazilians rely on clientelist relationships with local politicians, in particular mayors and city councilors (Nichter 2016). Vote buying and clientelism is rife throughout much of the country. For example, a 2014 survey by the Latin American Public Opinion Project (LAPOP 2014) suggests that 10.7 percent of Brazilians were offered a benefit in exchange for their vote in that year's state and federal elections. Brazil's electoral courts ousted nearly 700 politicians for delivering private goods to citizens during political campaigns between 2000 and 2008 (MCCE 2009).

Evidence from Brazil and many other countries suggests that citizens often demand clientelist benefits, even though nearly all research on clientelism focuses exclusively on politicians' offers of handouts (Nichter and Peress 2016). Our longitudinal data reveal that rural Brazilians facing shocks often turn directly to local politicians to request assistance. In the 2012 election year, 21.3 percent of survey respondents asked for private help from a mayoral or councilor candidate. Moreover, 8.3 percent of respondents made such requests to those same politicians during the following non-election year. While not all requests involve life necessities, most do —— about a third of requests in both years involved health care, and another quarter involved water. When responding to such requests, politicians often mete out assistance using political criteria, given that the number of requests often exceeds available resources. The mayor and allied councilors typically have greater access to municipal resources, so their supporters are often most likely to receive help (Nichter 2016).

Numerous factors contribute to the prevalence of clientelism in Brazil. Some evidence suggests that the electoral institution of open list proportional representation for selecting

federal deputies, state deputies and councilors fosters clientelism. By heightening intra-party competition, it tends to promote a focus on particularism rather than programmatic appeals (Hagopian 1996, Ames 2002).[8] Brazil's highly fragmented party system also weakens the ability of many politicians to employ programmatic appeals, as a large number of parties makes it more difficult for voters to ascertain which ones align with their collective interests. In addition, Brazilian politicians who aim to influence elections illicitly may find it easier to distribute contingent rewards than to engage in strategies of electoral fraud, such as registering fictitious voters or tampering with electoral returns. To reduce such fraud before voting, Brazil employs a national registration database and recurring voter registration audits. Furthermore, in part to hinder fraud after voting, it became the first country in the world to institute fully electronic voting in 2000 (Nicolau 2002; Mercuri 2002).[9]

Although electronic voting reduces the ability of politicians to manipulate electoral outcomes, the technology also exacerbates opportunistic defection that often threatens clientelist exchanges. Studies across the world have uncovered various mechanisms that reduce the probability that citizens who receive benefits will renege on their side of the bargain, such as monitoring of paper ballots (e.g., Stokes 2005) and targeting reciprocal voters (e.g., Finan and Schechter 2012). In the Brazilian context, electronic voting undermines the ability of politicians to observe vote choices, as this technological innovation undercut traditional methods such as marking paper ballots. While violating ballot secrecy is thus particularly difficult, citizens can overcome this challenge by publicly declaring support for candidates with whom they have ongoing exchange relationships (Nichter 2016). Indeed, Brazilians who receive ongoing private help from politicians often publicly declare their support during campaigns by posting flags and banners, wearing political paraphernalia, and attending rallies. Through such actions, it becomes public knowledge whom a citizen supports. Since mayors have substantial discretion in terms of local expenditures, they can condition access to local services on the provision of political support. While not all public expressions of

[8]Local elections occur simultaneously nationwide every four years, with state and federal elections following two years later. Mayors and councilors are elected concurrently in each municipality. Mayors are elected by plurality, except in municipalities with populations above 200,000, where run-off elections are held if no candidate wins an outright majority. Mayors can only hold office for two consecutive terms, but can also be reelected again in a later election. Councilors, who do not face term limits, serve in the legislative branch of the municipal government and are elected by open-list proportional representation.

[9]Fujiwara (2015) investigates how electronic voting affected political behavior and enfranchisement of Brazilians of lower socioeconomic status (see also Hidalgo 2010).

political support involve clientelism, declared support is frequently observed during local elections in rural Brazil. In our 2012 survey, 38.7 percent of respondents placed political flags or banners on their homes, 21.8 percent visibly showed their support at campaign rallies, and 18.5 percent wore campaign stickers or t-shirts (see Section 4.2). By helping politicians to identify their supporters, this mechanism facilitates clientelism amidst electronic voting.

3 Data

3.1 Study Population and Sample

Our study's population consists of rural households in Brazil's semi-arid zone without reliable access to drinking water. More specifically, households eligible for the study met the following inclusion criteria: (a) they had no piped drinking water or cistern, (b) they had physical space on their property to build a cistern, and (c) their roofs were at least $40m^2$ and composed of metal sheeting or tile (to facilitate rainfall collection).

The sample selection of households involved two steps. First, municipalities were randomly selected using weights proportional to the number of households without access to piped water and cisterns, according to the most recent administrative data from the federal government's *Cadastro Único*. In the second step, clusters of neighboring households (i.e., *bairros logradouros* in the *Cadastro Único*) were selected at random within the sample municipalities. Up to six eligible households were interviewed in each cluster. In order to ensure independence of observations across household clusters, we imposed a restriction that clusters be located at least two kilometers away from each other. Our surveys were conducted in 425 rural neighborhood clusters in 40 municipalities, located in all nine states of the semi-arid region.

3.2 Household surveys

We conducted a face-to-face panel survey spanning nearly three years, as shown in the timeline in Figure 2. In the localization effort for study recruitment (May-July 2011), we identified 1,308 water-vulnerable households (i.e., households eligible for participation) in the randomly selected neighborhood clusters. Once households had been located, we conducted an in-depth baseline household survey of 1,189 household heads in October-

December 2011, gathering detailed household characteristics as well as information about individual family members. This first survey wave — which predated the cistern treatment — provides a rich set of household and individual-level characteristics such as water access, education, health, depression, labor supply, and food insecurity.

The next two waves, which enable us to capture effects of the cistern treatment, involved individual-level surveys of all present household members at least 18 years of age. These waves not only repeated many earlier questions to gather post-treatment data on household and individual characteristics, but also provide one of the first longitudinal surveys ever fielded investigating clientelism during both election and non-election years. In order to study political interactions around the campaign season, the second wave was fielded in November-December 2012, immediately after the October 2012 municipal elections. This wave successfully contacted 1,238 households in the sample. Given that all adults present in these households were interviewed, this second wave totaled 2,680 individual interviews. To capture effects during a non-election period, the third wave was fielded in November-December 2013. This wave successfully reached 1,119 households in the sample, with a total of 1,944 individuals interviewed.

3.3 Rainfall

We gathered monthly precipitation data at the municipal level for the past quarter century (1986-2013) from the Climate Hazards Group Infrared Precipitation with Station (CHIRPS) database.[10] On average, municipalities in our sample had 40.9 cm of rainfall in 2012 and 69.3 cm in 2013. To ensure meaningful comparisons across municipalities with differing climatic conditions, rainfall shocks are measured in analyses below as the difference between the current period's rainfall and the historical mean of rainfall in the municipality during identical months, divided by the municipality's historical monthly standard deviation of rainfall.[11]

[10]Site: http://chg.geog.ucsb.edu/data/chirps/.

[11]More specifically, our standardized rainfall shock measure is defined as $Standardized\ \ Rain_{imy} = (Rain_{imy} - \overline{Rain_{im}})/\sigma_i$, where $Rain_{imy}$ refers to rainfall in municipality i in period m (a set of calendar months) in year y, and $\overline{Rain_{im}}$ refers to average historical rainfall in municipality i in period m, and σ_i is historical standard deviation of rainfall in municipality i. Historical data based on 1986-2011 rainfall. This measure is advantageous over standardizing with σ_{im}: the latter approach is extremely sensitive to deviations in rainfall in months with historically low levels and variation of rainfall. We then standardize this measure so that it has mean zero and variance one in the estimating sample. Findings are robust to alternative rainfall measures, including the use of raw rainfall.

3.4 Voting data

In order to analyze survey respondents' electoral outcomes, we gathered the most granular voting data released by Brazil's Superior Electoral Court (*Tribunal Superior Eleitoral*, or TSE) for the 2012 municipal election. These data provide electoral returns for each electronic voting machine in surveyed municipalities. We also submitted information requests to the TSE to obtain the precise geographic location of each voting machine, enabling comparisons of votes received by mayoral candidates across different machines located in the same polling place. Of the 40 municipalities in our sample, 27 mayors were in their first term and thus eligible to run for reelection in 2012. Of these 27 mayors, 21 (77.8 percent) chose to run again, and eight were reelected (i.e., 38.1 percent of those who ran).[12] On average, the 21 incumbent mayors in our sample vying for reelection in 2012 received 46.9 percent of the votes cast, whereas their top challenger received 49.1 percent of the votes cast. This difference of just 2.2 percentage points is consistent with the competitiveness of many local elections in Brazil. In our sample, 1,355 respondents resided in municipalities where the incumbent ran for reelection in 2012.

To examine the impact of the cistern treatment on electoral results, we matched survey respondents to their voting machines. This task involved asking respondents in Wave 2 for their electoral section number (*seção eleitoral*), an identification number that Brazilians provide on various official documents (e.g., when applying for *Bolsa Família*). Each section number corresponds to a unique voting machine in a municipality.[13] Enumerators recorded respondents' section numbers twice to ensure accuracy and asked respondents to show their voter identification cards to confirm their section number. We were able to collect this information for 85 percent of all respondents in the 2012 survey wave. Note that in Brazil, voters are assigned to a specific voting machine by electoral authorities, and absentee voting is generally prohibited. In addition, voting is compulsory for all literate Brazilians between their 18th and 70th birthdays.

[12] In comparison, across Brazil in 2012, 74.8 percent of eligible mayors chose to run again, and those who ran experienced a reelection rate of 55.0 percent. See: "Mais da Metade dos Atuais Prefeitos que Disputaram o Segundo Mandato foram Eleitos," *Agência Brasil*, October 13, 2012.

[13] More specifically, it corresponds to a unique voting machine in an electoral zone, which usually (but not always) corresponds to a municipality. Our matching process incorporates this point: we asked respondents not only their voting machine number but also the name of their voting location, and thus could cross-check with official TSE records about respondents' electoral zones.

In municipalities where the incumbent mayor ran for reelection, we linked survey respondents to 909 voting machines across 189 voting locations (with a mean of 4.8 machines per location). On average, each of these machines had 334 eligible voters, of which 257 cast a valid ballot for a candidate, 19 cast blank or invalid votes, and 58 abstained. Of all votes cast in these machines, the incumbent candidate received an average of 117 votes (45.5 percent), and the challenger received 140 votes (54.5 percent) —– a vote margin of 23 votes (9.0 percentage points).

4 Descriptive analysis

4.1 Vulnerability

We first establish that households in our sample are indeed vulnerable. Aside from reporting means of welfare indicators, we can provide additional insights by examining their relationship with rainfall shocks. Table 1 reports bivariate regression coefficients of a set of vulnerability indicators against the rainfall shock measure defined in Section 3.3. Given that the rainfall shock measure is defined at the municipal level, the identification of coefficients is obtained from cross-municipality variation in rainfall shocks. If rural households could simply self-insure against rainfall shocks, or if the state provides effective social insurance, then we would expect no correlation between precipitation and vulnerability. But much to the contrary, bivariate regression coefficients in Table 1 suggest that negative rainfall shocks significantly increase several markers of vulnerability. The first vulnerability measure is based on the conventional CES-D scale (Radloff 1977), which is employed internationally to identify symptoms of depression using self-reported questions. The five-item scale reflects an average across items regarding how often respondents experienced five depressive symptoms and is coded here such that lower values correspond to more depression (to facilitate comparisons with other measures). A one standard deviation decrease in rainfall increases depression by 0.05 units, or about 0.1 standard deviations (σ) of the depression scale. The second vulnerability measure is the Child Food Security Index, a five-point scale summing binary responses from five questions about whether any child in the household encountered limited food over the past three months. Lower measures correspond to less food security, and hence, greater vulnerability. A one standard deviation decrease in rainfall worsens chil-

dren's food security by 0.05 units or about 0.05σ. The third vulnerability measure is the Self-Reported Health Status (SRHS) index, which indicates how healthy respondents believed they were (higher values indicate better self-reported health). A one standard deviation decrease in rainfall decreases self-reported health on this four-point scale by 0.04 units or 0.075 standard deviations.

Also indicative of the link between water and vulnerability in this rural setting, low rainfall decreased the level of household expenditures over the 30 days preceding the survey. A one standard deviation decrease in rainfall reduces household expenditures by R$ 24.40 (representing about 7 percent of average household expenditures) — more specifically, it cuts R$ 13.33 from expenditures on food and R$ 11.54 from other expenditures such as health, gas, and electricity.[14] Overall, the strong relationship between rainfall shocks and these indicators underscores the vulnerability of citizens in our sample.

4.2 Political interactions

Given their vulnerability to shocks, many citizens in rural Northeast Brazil rely on ongoing clientelist relationships with politicians for assistance. We provide contextual information about citizens' interactions with politicians in Table 2. In the first half of 2012 — before that year's election campaign officially began in July —18.4 percent of survey respondents talked at least monthly with a local politician. While these citizens most often conversed with a single councilor, their relationships might also be expected to yield political support for that councilor's allied mayoral candidate: 71.8 percent of respondents reported voting for a mayor and councilor of the same political group or coalition. In addition, there are likely to be spillover effects of such relationships on voting behavior within households, as 77.3 percent of respondents report that all family members vote for the same mayoral candidate. Citizens do not appear to form these relationships as a response to negative shocks. As shown by bivariate regression coefficients in the right column, there is no significant association of the first two measures with rainfall shocks earlier in the year. By contrast, citizens exposed to negative rainfall shocks are more likely to indicate that all household members vote for the same mayoral candidate.

[14]These figures are in 2011 Brazilian Reais.

During local political campaigns, mayoral candidates employ an extensive network of operatives to canvass citizens' homes. Over the course of the 2012 municipal campaign, 69.6 percent of respondents reported receiving at least one home visit from representatives of a mayoral candidate, a figure uncorrelated with rainfall shocks. While operatives' reasons for such visits are often multifaceted, their reach to so many poor, isolated households suggests the presence of an extensive political network, which is typically a prerequisite for clientelism.

As discussed above, declared support is a key mechanism by which politicians can obtain information about the trustworthiness of their clients. Nearly half of respondents engaged in at least one form of declared support, either on their bodies, on their homes, or at rallies. Table 2 also reveals that citizens are more likely to engage in each form of declared support when they experience negative rainfall shocks. This observation is consistent with our broader argument that vulnerability causes citizens to participate in clientelism.

We also examine the characteristics of respondents who conversed with local politicians at least monthly before the 2012 electoral campaign began. While clientelism is not the only reason for such conversations, citizens who interact so frequently with politicians outside of campaign periods are especially likely to be in clientelist relationships. For this reason, we employ these monthly interactions as a marker for citizens likely involved in ongoing clientelist relationships. As explained below, this marker plays an important role in analyses because we expect such citizens to respond differently to our experimental treatment. Of course, one might be concerned that these frequent interactions are merely a proxy for respondents' level of economic vulnerability or other important characteristics. For example, perhaps only the poorest citizens in our sample are motivated to interact frequently with politicians, given their needs. Table 3 suggests that contrary to this hypothesis, frequent interactors do not have significantly lower (or higher) expenditures or wealth on a per capita basis than respondents who did not regularly converse with politicians before the campaign began. In addition, they are not significantly different with respect to age, education, or homeownership. However, frequent interactors are more likely to be male and live in a larger household that is headed by a male. Moreover, as might be expected, their political behavior also differs from infrequent interactors. Based on our 2012 wave, frequent interactors are significantly more likely to: (a) turn out to vote, (b) report that all house-

hold members voted for the same mayoral candidate, (c) publicly declare support, and (d) receive campaign visits. However, they are not more likely to vote for a mayoral and councilor candidate of the same political group. Overall, citizens in ongoing relationships with politicians do not differ markedly from other households in our sample with respect to their socioeconomic characteristics, but they do tend to be more politically engaged.

Our longitudinal data also reveal that many rural Brazilians turn directly to local politicians to request assistance. Table 4 provides a closer examination of descriptive evidence introduced in Section 2 as well as summary statistics of our main clientelism indicators. As shown, during the 2012 election year, 21.3 percent of survey respondents asked for private help from a mayoral or councilor candidate, and 8.3 percent made requests of those politicians during the following non-election year. The composition of demands during both years reveals that citizens' requests are motivated by vital needs such as medicine, medical treatments, and water. Just as analyses in Section 4.1 suggest that rainfall shocks increase vulnerability, Table 4 also shows that rainfall shocks increase requests for assistance from politicians. Bivariate regression coefficients suggest that a one standard deviation decrease in rainfall increases overall requests by 3.9 percentage points in 2012. Approximately 60 percent of this increase in demands involves water (2.3 percentage points), and about a quarter involve medicine or medical treatment (1.1 percentage points). As shown, politicians fulfill approximately half of such requests and are more responsive to demands for water and healthcare than for construction materials.

Our data also corroborate the general consensus in the literature that clientelism tends to favor incumbents. Incumbents usually have greater financial and organizational resources to engage in clientelism, not least because they can more easily access government coffers, programs, and employees (e.g., Gallego and Wantchekon 2012, Stokes 2009). Studies suggest that the ability to control public programs and employment helps incumbents' electoral performance (Schady 2000, Folke, Hirano and Snyder 2011), and experimental evidence suggests that clientelism is more effective for incumbent candidates (Wantchekon 2003). In our study's control group, respondents were more likely to have received private benefits from incumbent than non-incumbent politicians. During the 2012 election year, 7.0 percent of respondents had requests fulfilled by incumbent candidates, versus 5.7 percent by challenger candidates. The disparity is even starker during the year after the 2012 election, reaching an

order of magnitude: whereas 3.6 percent of respondents had requests fulfilled by politicians in office, only 0.36 percent had requests fulfilled by politicians out of office.

5 Empirical methodology

5.1 Research design

5.1.1 Intervention

The experimental treatment employed in this study involves rain-fed water cisterns. The cisterns were developed by our NGO partner *Articulação no Semi-Arido Brasileiro* (ASA, or Brazilian Semi-Arid Articulation)[15] as a strategy to help poor rural households cope with irregular rainfall. Prior to our experiment, ASA had built cisterns in Northeast Brazil since 2003. As described below, our project randomized the construction of cisterns by ASA, beginning in January 2012. These water cisterns consist of an enclosed structure made of reinforced concrete, capable of holding up to 16,000 liters of water (about the size of a small room). As shown in Figure 3, each cistern is attached to a gutter and tube system that collects rainfall from the home's roof. The cistern is partially buried, so that a manual pump on top is located at hip-level height. A small metal door provides internal access for cleaning and maintenance.

A cistern is an important asset for the household, because it serves as a reliable technology for collecting and storing water. While cisterns are designed to collect rainfall from a home's roof, households can also buy water from a water truck and store it in the cistern, insulating themselves from droughts. Thus, the cistern not only collects rainfall, but also serves as a storage device. Each cistern cost approximately US$ 1,000 (R$ 1,500 in 2010) to construct. Cisterns were awarded free of charge to eligible households.

Since cisterns had been constructed by ASA in the region for nearly a decade, the intervention was rather well-known by the population. As such, there were no concerns about whether households would accept cisterns or know how to use and maintain them. With respect to existing cisterns in the region, wealthier households tended to have self-built cisterns, whereas poorer households tended to have received them from ASA. The cisterns

[15] ASA is an umbrella organization of over 3,000 civil society entities. See www.asabrasil.org.br.

randomly assigned during our intervention were financed by an international development agency, but implemented through ASA. Only one minor attribute differed between our intervention's cisterns and those previously constructed by ASA: each cistern's usual plaque that displayed various logos also included the development agency's logo. In our study, local politicians had no input whatsoever regarding which households were selected to participate or receive cisterns. Moreover, as a longstanding practice, ASA does not consult with local politicians regarding cisterns and did not indicate to beneficiaries that the government was in any way responsible for their receipt of cisterns.

5.1.2 Experimental design

In October 2011, household clusters were stratified by municipality and randomly allocated into treatment and control arms. Randomization was performed across neighborhood clusters (i.e., *bairro logradouros*) within municipalities. Households within neighborhood clusters often share water resources; thus, to avoid treatment contamination across households, all participating households in clusters selected for treatment were assigned to receive their own individual cisterns. Our sample consists of 615 households in 189 treatment clusters and 693 households in 236 control group clusters. A larger share of households was assigned to the control group given the possibility that some cisterns might be built in control households by other cistern-building entities. For ethical reasons, we would not inhibit households from obtaining cisterns by other means.

Experimental compliance is shown in Appendix Table A1. In Wave 2 of the survey in November 2012, 67.5 percent of households assigned to treatment had received a cistern. This percentage increased to 90.8 percent by Wave 3 in November 2013. Some of the noncompliance stems from the fact that our partner, ASA, is an umbrella NGO coordinating many small associations at the municipal level or below. In some cases, we learned ex-post that certain local associations had less human resources to organize construction than initially expected.

With regards to compliance among households assigned to the control group, 20.2 percent of households had a cistern by Wave 2, which increased to 65.3 percent by Wave 3. Treatment among those assigned to the control group mainly resulted from an unforeseen expansion of federal funds for cistern construction after our study was designed and fielded.

At the beginning of our study, ASA was the predominant builder of cisterns in the region, but this budget expansion led other contractors to ramp up cistern construction.

Following the usual approach in experimental studies, we address such complications by focusing on intention-to-treat effects (ITT). That is, analyses compare those we intended to treat (respondents assigned to the treatment group) to those we intended not to treat (respondents assigned to the control group). In addition, we provide instrumental variable estimations in the appendix as detailed below.

5.1.3 Baseline balance

Baseline balance is presented in Appendix Table A2. Mean values for the treatment and control groups are shown, as well as differences in means and standard errors of these differences. Slightly over half of individuals in our sample are female. On average, respondents are 37 years old and have six years of education (i.e., they completed primary school). Household size is just over four members, and about 63 percent of households have at least one neighbor with a cistern. Only the latter characteristic had a small but significant difference of 6 percentage points between the treatment and control groups.

The table also shows balance between the two groups for various other indicators, including: expenditures and wealth per capita, age of the household head, homeownership, electricity, migration, land ownership, land size, number of children and political participation. An F-test reported in the last row of the table fails to reject the joint hypothesis that all coefficients are zero. This finding implies that our randomization was successful at achieving statistically similar treatment and control groups at baseline.

5.1.4 Attrition

We observe a low level of household attrition across survey rounds. Table A3 shows that from the 1,308 households identified for study participation, 9.1 percent were not successfully interviewed during the baseline survey (Wave 1). During the election year survey (Wave 2), the attrition rate was lower, at 5.4 percent of households identified for study participation. In the post-election survey (Wave 3), attrition increased to 14.5 percent of households identified for study participation. Furthermore, the attrition of households is uncorrelated with treatment status, as shown in the last row of the table. The correlation with treatment is small and negative, and statistically indistinguishable from zero (p-value=0.64).

5.2 Empirical strategy

Our main empirical analyses focus on outcomes obtained from household surveys as well as from official electoral results. The type of data informs the regression models used in each analysis. We describe each specification below.

5.2.1 Household vulnerability

We first establish the effects of the cistern treatment on different vulnerability indicators. We do so by estimating:

$$y_{ij} = \alpha_j + \beta_1 \cdot D_{ij} + \epsilon_{ij}, \tag{1}$$

where y_{ij} is a vulnerability indicator for household i in municipality j, D_{ij} is a dummy indicating whether household i in municipality j was assigned to treatment, and α_j is a municipal fixed effect. We include municipality fixed effects since treatment assignment was stratified at the municipality level; neighborhood clusters were randomly assigned to treatment within a municipality. Because households within a given cluster are neighbors and may share common shocks, we allow for arbitrary intra-cluster correlation of the error term ϵ_{ij} by using clustered standard errors at the neighborhood cluster (i.e., *bairro logradouro*) level.

5.2.2 Requests for private help

We next estimate the overall effects of the cistern treatment on individuals' requests for private help (and in separate specifications, whether such requests were fulfilled). We do so by estimating equation (1) using as the dependent variable a dummy indicating whether individual i in municipality j requested private goods from a politician in either 2012 or 2013. Our primary analyses employ a pooled data specification, which stacks both survey rounds and includes survey wave fixed effects.

To test the hypothesis that the cisterns intervention reduces requests for private goods among citizens in clientelist relationships, we also employ individual-level data to estimate:

$$y_{ijt} = \alpha_j + \gamma_t + \beta_1 \cdot D_{ij} + \beta_2 \cdot F_{ij} + \beta_3 \cdot D_{ij} \cdot F_{ij} + \epsilon_{ijt}, \tag{2}$$

where y_{ijt} is the dummy outcome variable defined above; F_{ij} is an indicator for the person being a frequent interactor *before* the electoral campaign (as defined in Section 4.2); and the other variables are as defined above. In addition, we report results separately by survey

wave, thereby distinguishing between requests made during 2012 and 2013. These specifications provide evidence about treatment effects during *both* electoral and non-electoral periods.

5.2.3 Electoral outcomes

To test the hypothesis that the cisterns intervention undermines the electoral performance of mayors during their reelection campaigns, we examine data from Brazil's electoral authorities. As described above, we are able to link survey respondents to the specific electronic voting machines in which they cast votes. We aggregate the data at the voting machine level and estimate:

$$y_{msj} = \alpha_{sj} + \gamma_1 \cdot TV_{msj} + \gamma_2 \cdot CV_{msj} + \gamma_3 \cdot EV_{msj} + \epsilon_{msj}, \tag{3}$$

where y_{msj} is the number of votes for the incumbent mayor in voting machine m, in voting location s, in municipality j. The regressor of interest is TV_{msj}, which is the number of participants assigned to the treatment group who are registered to vote in that particular voting machine. Other controls in the regression are CV_{msj}, the number of individuals in our study assigned to the control group in the voting machine; α_{sj}, a voting location fixed effect to control for differential voting patterns across voting locations in a municipality; and EV_{msj}, the total number of citizens registered to vote in the machine during the prior municipal election (in 2008). Recall that for a given voting machine, the proportion of voters from the experimental sample who are assigned to the treatment condition is assigned randomly. Therefore, once we condition on the number of control individuals in the study registered to vote in the machine, we can identify the effect of an additional person assigned to the cisterns treatment on votes for the incumbent mayor.[16]

As mentioned above, Brazil releases electoral results at the voting machine level, so we aggregate the counts of treated and control individuals in our experimental sample for each voting machine to construct explanatory variables. Accurately measuring treatment effects with these aggregate data poses a challenge, because non-interviewed individuals may also

[16] Additional specifications are employed to show robustness. Some employ a more recent measure of eligible voters per machine (from 2012). Other specifications include an additional control variable — the change in eligible voters between 2008 and 2012 — which could influence the number of votes received by an incumbent. More generally, this latter design is similar to those used to measure spatial (direct and external) treatment effects, as in Miguel and Kremer (2004).

have been treated. In particular, eligible voters in sampled households were only interviewed if present during our home visits, and treated households may have shared water from their cisterns with ineligible, neighboring households. Failing to address this undercounting of potentially treated individuals could bias our estimates of treatment effects upwards in absolute terms. We thus adjust both the number of treated and control voters regressors (TV_{msj} and CV_{msj}, respectively) to incorporate estimates of the number of non-interviewed individuals within (a) households in our sample, and (b) households in the neighborhood cluster with no cistern at baseline (i.e., those who potentially received shared water), as well as the probabilities that those individuals vote in the same locations and voting machines as our interviewees. This procedure improves estimation of the magnitude of treatment effects on electoral outcomes; the statistical significance of findings is also robust without any such adjustments.[17]

To conduct appropriate inference, we must take into account two separate considerations. First, we need to address the fact that the adjusted regressors are subject to sampling error. Second, because we allow the errors to be correlated across voting machines and locations within a municipality, our sample is composed of 21 "clusters," or municipalities in which the mayor is running for reelection. To address both points, we report p-values from a wild cluster bootstrap procedure (Cameron, Gelbach, and Miller 2008). This procedure also takes into account sampling error in the construction of the adjusted regressors through bootstrapped sampling of the data used to construct estimates (Horowitz 2001). Appendix B thoroughly explains the procedures used to construct adjustment factors and to conduct appropriate inference.

While extremely granular, a limitation of using voting-machine-level outcomes as the dependent variable — instead of individual-level outcomes as in the previous subsection — is that we only have a single observation per machine (*i.e.*, total number of votes for the incumbent). This aggregation reduces the power in regressions of electoral outcomes. In addition to the primary specification above, which obtains average effects for individuals assigned to the cisterns treatment, we also examine heterogeneity in these effects by frequency of interactions with politicians. This regression further deteriorates the signal-to-noise ratio, given

[17] Even without adjustment, the specification above reveals that the cisterns treatment significantly reduces votes for the incumbent mayor. Unadjusted regressions are shown in Appendix Table B1.

that less than one-fifth of citizens are frequent interactors and less than one-tenth of voting machines have any treated frequent interactors.

6 Results

6.1 Effects on Household Vulnerability

This study argues that introducing water cisterns reduces vulnerability, which in turn decreases clientelist requests. As such, the first step of our empirical analysis is to establish that the cisterns treatment indeed reduced vulnerability. To this end, Table 5 provides estimates of the intervention's effect on various measures of household vulnerability. As shown in column 1, with respect to the adapted CES-D scale of depressive symptoms described above, survey respondents experience an improvement of 0.09 units in 2013. This finding is significant at the .05 level and equivalent to 0.14 standard deviations in the CES-D scale. Column 2 shows that another measure of vulnerability described above, Self-Reported Health Status, also improves by 0.075 units among treated households (significant at the .05 level), representing 0.14 standard deviations on the SRHS scale. In column 3, the Child Food Security Index also shows an improvement of similar magnitude (0.08), though this estimate is imprecisely estimated. An overall index that standardizes and adds these three components as in Kling, Liebman and Katz (2007) suggests that there is a substantial 0.13σ reduction in vulnerability caused by the cisterns program (significant at the .01 level; column 4). Overall, this analysis confirms that the cisterns program had first-order intended effects in reducing the vulnerability of these households.

6.2 Effects on Clientelism

Given that the cisterns treatment lowered vulnerability, we next show that it also reduced requests, especially by citizens likely to be in clientelist relationships. Table 6 presents estimates of the causal impacts of the cistern intervention on citizen requests for private goods from local politicians. Column 1, which pools data across survey waves, shows that the intervention reduced the likelihood that citizens requested such benefits by 3.2 percentage points (15.0 percent). This finding is significant at the .05 level. Most strikingly, column 2 shows that these effects are fully concentrated among citizens who are likely to be involved

in clientelist relationships — those having at least monthly conversations with a politician before the 2012 electoral campaign began. Among this group of frequent interactors, we estimate an 11.1 percentage point (32.2 percent) reduction in requests (significant at the .01 level). By contrast, among other respondents, we estimate an insignificant 1.4 percentage point reduction in requests (p-value=0.86; column 2). Decomposing these effects by type of good requested reveals that the treatment effect for frequent interactors is negative across good types: requests for water fall by 3.9 percentage points, requests for construction materials fall by 3.7 percentage points, and requests for medicine or medical treatments fall by 2.3 percentage points, though the latter is imprecisely estimated (reported in Appendix Table A4).

Columns 3-6 of Table 6 show that similar patterns hold when estimating the specification separately for the 2012 electoral year and the 2013 post-electoral year. Across all citizens, the treatment effect on requests is only significant at the .11 level in 2012, but is significant at the .05 level in 2013. Importantly, we cannot reject the hypothesis that the coefficients for both years are identical. Across frequent interactors, the treatment effect is significant at the .01 level and remarkably similar during both years (10.3 and 10.6 percentage points, respectively). The fact that this reduction in requests is of the same magnitude outside of the electoral period suggests that the effect is persistent and has longer term effects on relationships between citizens and politicians, rather than just short-term effects around campaigns.

In order to heighten comparability with analyses of individuals' voting behavior (see Section 6.3 below), we also estimate the aforementioned models using only the subsample of municipalities in which the incumbent mayor runs for reelection (columns 7-12). We find that citizen requests fall by 4.5 percentage points across both waves, with similar magnitudes when estimated separately; these findings are all significant at the .01 or .05 level (columns 7, 9, and 11). Again, the effects are substantial and concentrated among the subsample of frequent interactors (columns 8, 10, and 12).[18]

Whereas the above specifications focus on whether the cisterns treatment affects citizens' requests for private assistance, it is also of interest whether the cisterns treatment leads to an

[18]For completeness, we also estimated these models using an instrumental variable approach in which assignment to treatment is employed as an instrument for actually receiving a cistern. As expected, the estimated coefficients are amplified in proportion to the degree of compliance. The statistical significance remains unchanged from our main results. See Appendix Tables A5 and A6.

actual reduction in the receipt of benefits. To examine this question, Table 7 employs as a dependent variable whether the respondent's request for a private good was fulfilled by a local politician.[19] Column 1 shows that the cisterns intervention does not have an overall impact across survey waves on the equilibrium probability of fulfilled requests (point estimate = -0.007). However, we observe a substantial reduction of 6.2 percentage points among frequent interactors (significant at the .05 level; column 2). This effect is significantly different from zero in 2012 but not in 2013 (columns 4 and 6).[20] Again, the effects are similar when exclusively examining municipalities with incumbent mayors running for reelection (columns 7-12); for this sample, the reductions in benefits received by frequent interactors are statistically significant when pooling across both waves (at the .05 level), as well as during and after the election (at the 0.10 level) when estimated separately.[21]

While these analyses have focused on requests for private goods, they leave open the question of whether individuals substituted requests of private goods for that of public goods. To investigate further, we also consider requests for public goods. More specifically, we classify requests as involving public goods if they ask for community water infrastructure, investments in public roads, improvements to local health clinics, improvements to local schools, or improvements to the electricity infrastructure (e.g., public lighting). Analogous to analyses for private goods, Table 8 presents estimates that employ requests for public goods as the outcome variable. We do not find evidence of a substitution of requests

[19]Specifically, we use a question about whether the respondent requested a private good or service from a politician, in conjunction with a follow-up question. The follow-up question was whether the respondent had received what he or she had requested.

[20]Appendix Table A7 presents pooled results across both years for different types of goods that were requested and received by citizens. We find evidence of negative effects across all types of goods, although these coefficients tend to be less statistically significant than for our primary dependent variable of interest examined above, citizen requests.

[21]One might be concerned about the degree of non-compliance in the control group. For instance, perhaps cisterns obtained by those in the control group were different, in that they involved clientelistic behavior on behalf of recipients. In that case, the negative effects observed from assignment to treatment could be partially attributed to increased clientelism among those in the control group. We investigate this possibility by examining whether there are heterogeneous effects based on the cross-municipality degree of non-compliance among those assigned to the control group. Appendix Table A9 shows findings for requests using a triple interaction regression which adds a fully interacted dummy variable for whether the municipality had above median non-compliance levels among those assigned to control. Belying this alternative explanation, negative effects are no greater in municipalities with above median non-compliance.

towards public goods. The estimated coefficients are very small and cannot be distinguished from zero.[22]

Altogether, these analyses indicate that the cisterns treatment reduced requests of local politicians by citizens likely to be in clientelist relationships, during both election and non-election years. Moreover, this decrease in requests actually contributed to a fall in the prevalence of private benefits delivered to citizens by local politicians. In addition, the reduction in requests was observed across various private goods that citizens typically request.

6.3 Effects on Electoral Outcomes

Thus far, results suggest that the cisterns intervention reduced vulnerability and clientelism, during both electoral and non-electoral years. Given these findings and the fact that incumbents generally have more resources for clientelism, we also seek to determine whether the cisterns treatment undercut the performance of incumbent mayors during their reelection campaigns. As explained above, we link survey respondents to the electronic voting machines in which they voted in the 2012 election. This approach enables us to examine how our intervention affected electoral outcomes. Table 9 presents our main results about the effect of cisterns on incumbents' votes and other electoral outcomes. Specifications in columns 1-3 show that the cisterns treatment reduced the number of votes received by incumbent mayors. Column 1 controls for the number of voters assigned to each voting machine in 2012. Column 2 instead controls for the number of voters assigned to each machine during the prior mayoral election in 2008 (to account for endogenous changes in voter registration). By contrast, column 3 controls for both the number of voters assigned to each machine in 2008 and the change in assigned voters per machine between 2008 and 2012. Across all three specifications, the estimated coefficient on the number of treatment individuals is remarkably stable. We find that for every additional respondent assigned to the treatment condition, the incumbent receives 0.22, 0.19, and 0.21 fewer votes (bootstrap p-values are 0.040, 0.034 and 0.030, respectively). In contrast, the estimated effect of individuals assigned to the control group on incumbent votes is positive, but small and statistically indistinguishable from zero in all but one specification.

[22]Results are similar in Appendix Table A8, in which the dependent variable is an indicator for having requested and received a public good.

Next, we estimate heterogenous treatment effects by the number of frequent and infrequent interactors per voting machine using the preferred set of controls from column 3.[23] Although estimates in column 4 are statistically indistinguishable from zero, the patterns observed are consistent with preceding analyses of citizen requests. The estimated coefficient on treated individuals who frequently interacted with politicians is -0.36, over four times larger than the coefficient on treated individuals who infrequently interacted with politicians (-0.08). In contrast, among control individuals the estimated coefficients are small and positive : 0.09 among frequent interactors and 0.11 among infrequent interactors. To be sure, this analysis is hindered by low power, given that it employs variation in the shares of frequent and infrequent interactors across voting machines. Nevertheless, its findings corroborate our overall argument, as the reduction in votes for incumbents appears to be primarily from effects among frequent interactors.

We next investigate whether treatment effects, which suggest a fall in incumbent votes, translate to an increase in votes for mayors' challengers. Based on our preferred specification (column 3), we examine as the dependent variable the total number of votes received by any challenger in the 2012 mayoral race. As shown in column 5, we estimate a coefficient of almost identical magnitude — but with the opposite sign — as the estimate for incumbents' votes. For every additional respondent assigned to the treatment condition, votes for challenger candidates increase by 0.254 (p-value = 0.072). We also report treatment effects on voter turnout (column 5), as well as blank and null votes (column 5), which are both small and statistically indistinguishable from zero. The former result helps to rule out potential turnout buying as an explanation for electoral responses to the cisterns intervention.[24]

Overall, these findings provide novel, credible evidence that reduced vulnerability not only leads to a reduction in clientelist requests, but also undercuts the performance of incumbent mayors. More broadly, it corroborates our argument that vulnerability — in a context where formal mechanisms of social insurance are largely absent — can help deter clientelist politics by decreasing support for incumbent politicians who disproportionately engage in such arrangements.

[23] Appendix table B2 presents heterogenous treatment effects for all outcomes and control variables used in table 9 for completeness.

[24] See e.g., Nichter (2008) and Larreguy, Marshall, and Querubín (2016) regarding turnout buying.

6.4 Robustness Checks

Thus far, the findings of this study provide substantial evidence that the cisterns intervention reduced citizen requests, especially by citizens likely to be in clientelist relationships. Furthermore, the intervention undercut the performance of incumbent mayors during their reelection campaigns. We now conduct additional analyses to confirm the robustness of these findings and to rule out several potential alternative explanations.

6.4.1 Politician Responses

Our interpretation of the experimental findings is that exogenously allocating cisterns caused a decline in citizens' requests for private goods from local politicians. However, one might be concerned that this decline in requests may be partially reflective of local politicians changing their clientelist strategies in response to the assignment of cisterns. After all, the literature on clientelism suggests that elites have a wide arsenal of strategies in their toolkit, such as vote buying and turnout buying (e.g., Vicente 2014, Hicken et al. 2015, Larreguy, Marshall and Querubin 2016).

At the outset, it should be emphasized that even though our intervention substantially reduced the vulnerability of recipient households, it was minuscule in the context of the overall municipality. Whereas the population of the 40 municipalities in our sample averaged 49,000 citizens, our intervention constructed an average of only 17 cisterns in each municipality. Although such a limited intervention makes it unlikely that local politicians would adapt their municipal-level strategies, it is still worth investigating whether households with cisterns were approached differently than those without cisterns. Such findings would change how we interpret our primary results.

Table 10 examines whether respondents in households assigned to receive the cisterns treatment report any differences in politicians' actions towards them. Columns 1 and 2 show that politicians and their representatives were no more or less likely to visit the homes of treated subjects during the 2012 political campaign. Column 3 suggests that during those visits, handouts were not significantly more or less likely to be distributed to households assigned to the treatment condition, when compared to those assigned to the control condition. Furthermore, column 4 shows no significant difference in such handouts received by frequent interactors who were assigned to treatment versus those assigned to control.

We also inquired of all respondents whether a politician had offered them a handout in exchange for their votes, and if so, whether they had accepted that offer. Columns 5-8 show that respondents assigned to the cisterns treatment were not more or less likely than those assigned to the control group to answer affirmatively to either question. More broadly, we find no evidence that politicians responded differently to citizens depending on their treatment assignment, corroborating our interpretation that findings reflect citizens' (rather than politicians') responses to the cistern intervention. To be clear, we do not claim that politicians' strategies would necessarily remain unchanged when overall vulnerability in their districts declines. Rather, we argue that our intervention was so small in the context of the overall municipality that it was unlikely to have changed politicians' strategies. The data are consistent with this argument.

6.4.2 Citizen Engagement

Our main findings show that citizens who conversed frequently with politicians before the 2012 election campaign commenced were more responsive to the cisterns treatment. We have employed these frequent interactions as markers for clientelistic relationships, and showed in Section 4.2 that they are not associated with various socioeconomic characteristics. One might be concerned, however, that these frequent interactions could potentially reflect citizens' general engagement with politics rather than their clientelist relationships with specific politicians. To counter this alternative explanation, we undertake a two-pronged approach. First, we directly control for measures of citizen engagement and their interactions with treatment. More specifically, these measures are: (a) whether the respondent is a member of a community association, (b) whether the respondent is the president of a community association, and (c) whether the respondent voted in the 2008 municipal election. Table A10 reports our main clientelism specification controlling for these different community engagement measures separately (columns 1-3) as well as jointly (column 4). The estimated coefficients on the interaction term (β_3) are practically unchanged from the corresponding coefficient in column 2 of Table 6. Similarly, columns 5-8 repeat this exercise limiting attention to the subset of municipalities in which incumbent mayors ran for reelection. Again, the coefficients on the interaction are practically identical to the corresponding coefficient

in Table 6 (column 7). These findings suggest that controlling for community engagement measures does not significantly change our results.

Our second approach is to show that findings are not sensitive to the particular marker for clientelism employed. To this end, we replicate our analysis using a more restrictive measure of whether a respondent is in a clientelist relationship. In Tables A11 and A12, respondents are coded as being in a clientelist relationship only if they conversed frequently with a politician before the 2012 election campaign commenced *and* they publicly declared support for a candidate during the 2012 campaign. Recall from Section 4.2 that declared support is a mechanism commonly employed in clientelism to overcome ballot secrecy: 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. Overall, specifications in Tables A11 and A12 reveal that estimates and significance are nearly identical when using this alternative measure. These findings belie the alternative explanation: it is not merely politically active citizens, but rather citizens in clientelist relationships, who are especially responsive to the cisterns treatment.[25]

6.4.3 Credit Claiming and Political Alignment

Another potential concern involves credit claiming. Even though our intervention randomly assigned cisterns with no input from politicians, one possibility is that incumbent mayors claimed credit for respondents' receipt of cisterns and that such behavior affected electoral outcomes. Our main results counter such an interpretation: the cisterns intervention does not increase, but rather *decreases* votes for the incumbent mayor. However, another form of credit claiming could potentially involve political alignment with higher levels of government. After all, numerous studies have emphasized the effects of political alignment across different levels of government (e.g., Brollo and Nannicini 2012; Dell 2015; Durante and Gutierrez 2015). Perhaps mayoral candidates who were copartisans with Brazil's then-president Dilma Rousseff were especially likely to engage in credit claiming behavior — or otherwise benefit electorally — from the cisterns treatment. To consider this possibility, we examined whether treatment effects on electoral outcomes differ between mayoral candidates who were and were not affiliated with Rousseff's Workers' Party (*Partido dos Tra-*

[25]We do not employ the measure described here as the primary marker of being in a clientelist relationship because, in principle, declared support during the campaign can be endogenous.

balhadores, or PT). We find no evidence of such differences. Furthermore, treatment effects on requests and fulfilled requests do not differ between municipalities with and without PT mayors. These findings are shown in Appendix Tables A13-A15. These results are unsurprising, given that the cisterns intervention involved in this study was financed by an international development agency and not the federal government. Overall, our findings do not point to credit claiming or misattribution.

6.4.4 The Role of Rainfall

Given that rainfall provides a source of water for the cisterns, we also investigate whether precipitation reinforces the effects we documented for clientelism. To this end, we employ the rainfall shock variable defined in Section 3.3 and estimate a fully interacted triple differences specification. These results are shown in Table A16. Columns 1 and 4 include a *TreatmentXRainfall* control. Columns 2 and 5 add the triple interaction between Treatment, Rainfall and Frequent Interactor, whereas columns 3 and 6 show the fully saturated model by also including the *RainfallXFrequent Interactor* regressor. Across specifications, the coefficient on *TreatmentXRainfall* is negative as expected, but the coefficient is insufficiently large to be statistically significant. The lower portion of Table A16 shows that the treatment effect is estimated to be significantly different from zero and of similar magnitude as in our main specification even if the rainfall shock is zero (i.e., under normal rainfall conditions). A similar pattern emerges in Appendix Table A17, which reports findings for fulfilled requests for private goods from local politicians. It thus appears that overall rainfall plays the expected role — it amplifies the reduction in requests when a household has a cistern — but effects are not particularly strong. A likely reason is revealed by ultrasonic sensors we installed in a subsample of constructed cisterns. Approximately half of the water that flowed into cisterns was not from rainfall, but instead from water truck deliveries.[26] By serving as a water storage device, cisterns can thus reduce vulnerability even in the absence of rainfall.

[26]Rainfall appears in the cisterns' water level data as relatively gradual increases, whereas water truck fillings appear as a rapid surge in the cisterns' water level.

7 Conclusion

This paper has investigated whether reducing economic vulnerability has a causal effect on citizens' participation in clientelism. It is based on a dedicated longitudinal dataset of a representative sample of impoverished rural households in Northeast Brazil. Unlike previous studies, this panel survey enables the measurement of multiple dimensions of vulnerability as well as interactions with local politicians over a three-year period. We combine these data with a large-scale randomized control trial of a development intervention which reduced household vulnerability through the construction of private water cisterns. The experiment yields several important findings. First, the cisterns treatment decreased citizens' demands for private benefits, especially among respondents who are likely to be in clientelist relationships. Second, we show evidence of the persistence of treatment effects, given that findings are observed not only during the election campaign, but also a full year later. Third, our analysis of election results, which examine granular electronic voting machine outcomes, reveals that the cisterns treatment undercut the number of votes received by incumbent mayors during their reelection campaigns. Overall, these findings support the argument that cisterns — by reducing vulnerability — undermine clientelist relationships and thereby impinge on the electoral performance of incumbents. More broadly, our results also suggest that vulnerability is a first-order determinant of clientelism in contexts with limited formal mechanisms of social insurance.

The findings of this study are relevant for policy, especially because they can inform efforts to reduce clientelism. Numerous studies explore anti-clientelism campaigns, which often attempt to dampen citizens' acceptance of vote-buying offers. Such research provides various insights, but often suggests mixed results of these campaigns (e.g., Vicente 2014; Hicken et al. 2015). While further investigation is needed in other contexts, our study contributes by underscoring another modality to fight clientelism. The experimental results provide rigorous evidence that improving citizens' livelihoods can undercut their willingness to participate in contingent exchanges. Our findings are thus consistent with the view that centrally mandated insurance mechanisms can be a powerful tool to curb clientelism in developing countries.

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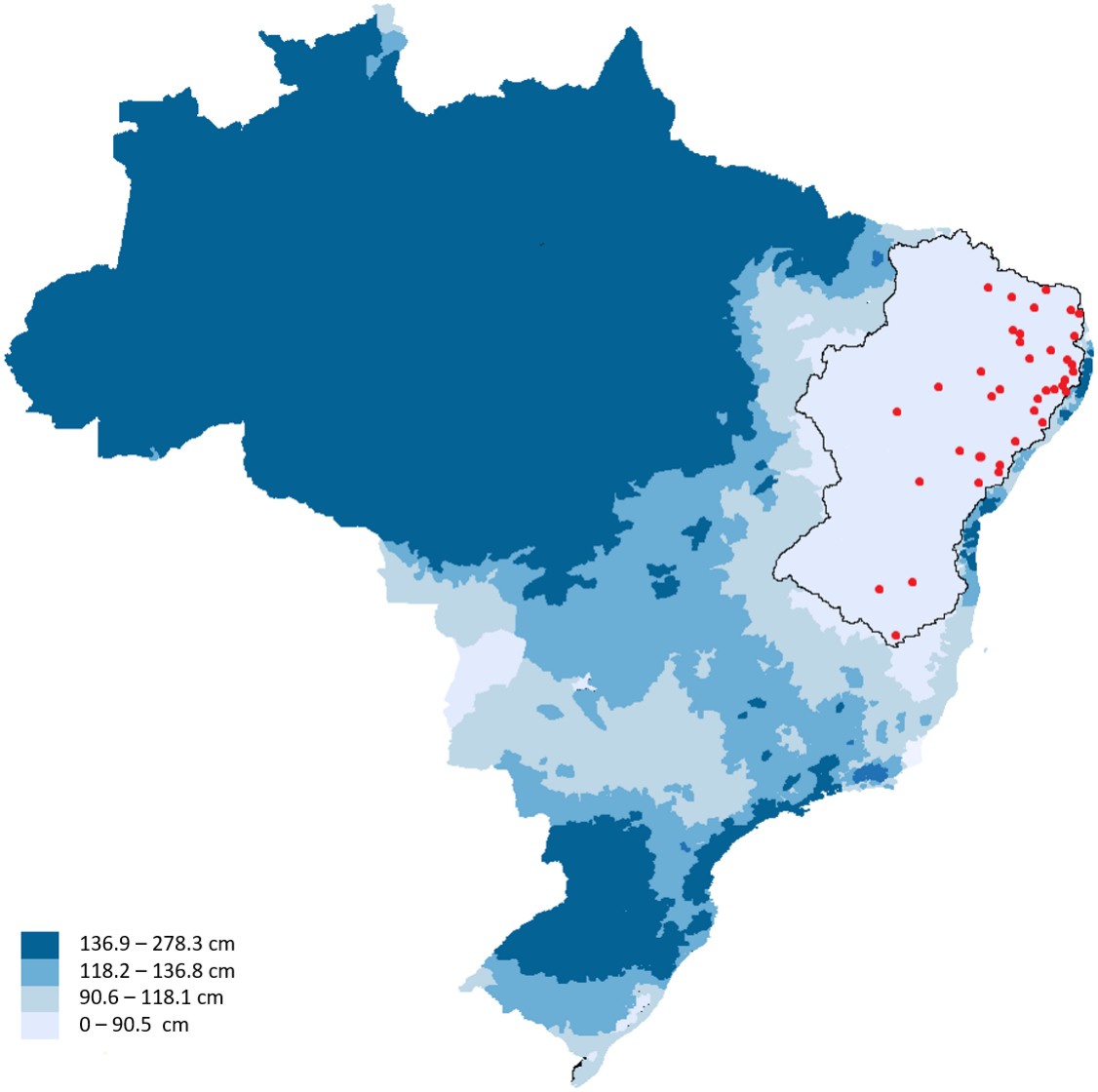
Figures and Tables

Figure 1: Brazil's Semi-Arid Region, Sample Municipalities, and Rainfall Levels



Notes: Brazil's semi-arid region consists of 1,133 municipalities in 9 states, as circumscribed by a black line in the figure. Red dots indicate the location of the 40 sample municipalities. Background colors reflect average rainfall levels (1986-2013) specified in the legend (darker colors represent more rainfall).

Figure 2: Timeline

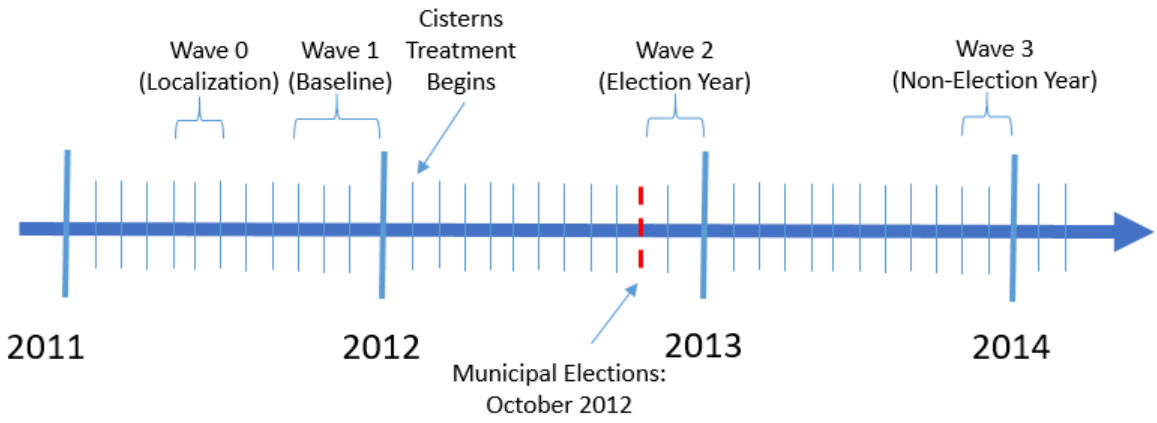


Figure 3: Cistern

Notes: The ASA cistern, shown on left, stores up to 16,000 liters of water and is made of reinforced concrete.

Table 1: Vulnerability and Rainfall Shocks

Variable	Mean	Relationship with Rainfall Shocks
-(CES-D Scale), 2013	3.33	0.046***
	(0.64)	(0.016)
Child Food Security Index, 2013	-0.31	0.046**
	(0.91)	(0.026)
Self Reported Health Status (SRHS) Index, 2013	2.83	0.039**
	(0.53)	(0.017)
Total Household Expenditure, 2011	367.85	24.398***
	(200.07)	(6.671)
Total Household Food Expenditure, 2011	239.15	13.331***
	(133.48)	(4.507)
Total Household Non-Food Expenditure, 2011	133.62	11.543***
	(130.26)	(3.69)

Notes: Column 1 presents the mean of each vulnerability measure and its standard deviation in parentheses, while column 2 reports coefficients from regressing each vulnerability measure on standardized rainfall shocks (as defined in Section 3.3). Standard errors are clustered at the neighborhood level and reported in parentheses in Column 2. Rainfall is measured in standard deviations of rainfall deviations during January-September of the relevant year from the historic average rainfall during 1986-2011. The -(CES-D) scale is a 5-item self-reported scale designed to measure depressive symptomatology in the general population. Each item ranges from 1 to 4 with higher values representing less depression, and the scale reported for each individual is the average across the 5 items. The Child Food Security Index is a sum of Yes/No (1/0) responses to whether in last three months any child skipped a meal, ate less than they should, was hungry but did not eat, did not have varied consumption, or had only limited types of food. All responses enter negatively, which means a higher Child Food Security Index indicates better food security for children. The Self-Reported Health Status (SRHS) Index measures responses on a 4-point scale regarding how good respondents believed their health is. Higher SRHS values indicate better reported health. Expenditures expressed in 2011 Brazilian reais. Non-food household expenditure includes rent, clothing, health, gas, electricity and other expenses. * 10%, ** 5%, *** 1% significance levels.

Table 2: Interactions with Politicians (2012)

Variable	Mean	Relationship with Rainfall Shocks
Interact at least monthly with a politician, before electoral campaign	0.184	-0.011
	(0.387)	(0.009)
Voting for the same group/coalition	0.718	-0.015
	(0.450)	(0.014)
All household members voting for the same mayoral candidate	0.773	-0.024**
	(0.419)	(0.012)
Received visit from representatives of any mayoral candidate	0.696	0.016
	(0.460)	(0.012)
Any declared support	0.485	-0.071***
	(0.500)	(0.017)
Declaration on person's body (sticker, shirt)	0.185	-0.023**
	(0.388)	(0.010)
Declaration on person's house (flag, banner, painting)	0.387	-0.063***
	(0.487)	(0.017)
Declaration at rally (attend rally, wear sticker/show support in rally)	0.218	-0.039***
	(0.413)	(0.011)

Notes: Column 1 presents the mean of each variable and its standard deviation are in parentheses, while column 2 reports coefficients from regressing each vulnerability measure on standardized rainfall shocks (as defined in Section 3.3). Standard errors are clustered at the neighborhood level and reported in parentheses in Column 2. Rainfall is measured in standard deviations of rainfall deviations during January-September of 2012 from the historic average rainfall during 1986-2011. * 10%, ** 5%, *** 1% significance levels.

Table 3: Frequent and Infrequent Interactors: Characteristics (2011 and 2012)

Variable	Frequent Interactors	Infrequent Interactors	Difference
Individual Characteristics (2011)			
Age	37.45	37.38	0.21
			(0.86)
Years of Education	6.11	5.75	0.27
			(0.23)
Female	0.451	0.558	-0.114***
			(0.025)
Household Characteristics (2011)			
Household Wealth Per Member	5894.1	5641.1	175.03
			(387.68)
Household Expenditure Per Member	103.2	104.9	-0.64
			(4.75)
Household Head Education	5.88	5.69	0.06
			(0.28)
Household Head is Female	0.150	0.194	-0.063**
			(0.025)
Owns House	0.881	0.858	0.024
			(0.022)
Household Size	4.54	4.19	0.38***
			(0.14)
Political Activities (2012)			
Voted in 2008 Municipal Election	0.916	0.871	0.043**
			(0.019)
Voting for the same group/coalition	0.732	0.719	0.006
			(0.033)
All household members voting for the same mayoral candidate	0.819	0.761	0.051**
			(0.023)
Received visit from representative of any mayoral candidate	0.802	0.676	0.099***
			(0.021)
Any declared support	0.655	0.448	0.187***
			(0.026)

Notes: Columns 1-2 present the mean of each variable for frequent and infrequent interactors, respectively. Frequent interactors are respondents who interacted with either the mayor, a councilor or their representative at least monthly before the 2012 election campaign commenced. Column 3 reports differences estimated in an OLS regression model with municipality fixed effects. Standard errors are clustered at the neighborhood level and reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table 4: Clientelist Relationships (2012 and 2013)

	Mean (2012)	Mean (2013)	Relationship with Rainfall Shocks (2012)	Relationship with Rainfall Shocks (2013)	P-Value from Pooling Test
Panel A: Ask for private help from any politician					
Any	0.213	0.083	-0.039***	-0.003	0.003
	(0.409)	(0.280)	(0.010)	(0.007)	
Water	0.055	0.010	-0.023***	0.001	0.001
	(0.228)	(0.098)	(0.007)	(0.002)	
Medicines or Medical Treatment	0.071	0.021	-0.011**	0.003	0.033
	(0.257)	(0.142)	(0.005)	(0.004)	
Construction Materials	0.057	0.016	-0.009	-0.003	0.356
	(0.233)	(0.125)	(0.006)	(0.003)	
Panel B: Ask for and receive private help from any politician					
Any	0.124	0.032	-0.023***	0.001	0.011
	(0.330)	(0.176)	(0.008)	(0.005)	
Water	0.034	0.004	-0.011***	-0.001	0.096
	(0.182)	(0.064)	(0.006)	(0.001)	
Medicines or Medical Treatment	0.051	0.010	-0.008***	0.002	0.070
	(0.219)	(0.098)	(0.005)	(0.003)	
Construction Materials	0.022	0.002	-0.005	-0.001***	0.314
	(0.146)	(0.045)	(0.004)	(0.001)	

Notes: Columns 1-2 present the mean of each clientelism measure; standard deviations are reported in parentheses. Columns 3-4 report coefficients from regressing each clientelism measure on rainfall shocks. Standard errors are clustered at the neighborhood level and reported in parentheses. Rainfall shocks are measured by the standard deviations of rainfall during January-September of the relevant year from the historic average rainfall during 1986-2011. Column 5 presents the p-value from the *F*-test of whether coefficients reported in columns 3 and 4 are equal. * 10%, ** 5%, *** 1% significance levels.

Table 5: Vulnerability and Assignment to Treatment (2013)

	-(CES-D Scale)	SRHS Index	Child Food Security Index	Overall
Treatment	0.093**	0.075**	0.084	0.126***
	(0.038)	(0.033)	(0.054)	(0.043)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Observations	1128	1052	1128	1128
Mean of Dependent Variable	3.331	2.830	-0.309	0.001

Notes: Each column reports the coefficient from regressing each vulnerability measure on treatment, with municipality fixed effects. Standard errors are clustered at the neighborhood level and reported in parentheses. The -(CES-D) scale is a 5-item self-reported scale designed to measure depressive symptomatology in the general population. Each item ranges from 1 to 4 with higher values representing less depression, and the scale reported for each individual is the average across the 5 items. The Child Food Security Index is a sum of Yes/No (1/0) responses to whether in the last three months any child skipped a meal, ate less than they should, was hungry but did not eat, did not have varied consumption, or had only limited types of food. All responses enter negatively, such that a higher Child Food Security Index indicates better food security for children. The Self-Reported Health Status (SRHS) Index employs a scale of 1-4, in which higher values indicate better perceived health. The Overall Vulnerability Index is the unweighted mean of standardized values of all of the above indexes. * 10%, ** 5%, *** 1% significance levels.

Table 6: Private Help Requests (2012 and 2013)

	Ask for private help from any politician											
	All Municipalities						Municipalities with Incumbent Mayors Running for Re-election					
	Pooled		2012		2013		Pooled		2012		2013	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
β_1: Treatment	-0.032**	-0.014	-0.027	-0.010	-0.033**	-0.018	-0.045***	-0.024	-0.044**	-0.026	-0.044**	-0.021
	(0.013)	(0.013)	(0.017)	(0.018)	(0.015)	(0.014)	(0.016)	(0.016)	(0.021)	(0.022)	(0.019)	(0.019)
β_2: Frequent Interactor With Politician		0.120***		0.133***		0.085***		0.145***		0.148***		0.117***
		(0.027)		(0.033)		(0.032)		(0.033)		(0.040)		(0.044)
β_3: Treatment X Frequent Interactor With Politician		-0.097***		-0.093**		-0.089**		-0.111**		-0.087		-0.123**
		(0.034)		(0.044)		(0.040)		(0.043)		(0.058)		(0.052)
$\beta_1 + \beta_3$:		-0.111***		-0.103**		-0.106***		-0.134***		-0.113**		-0.144***
		(0.032)		(0.041)		(0.039)		(0.04)		(0.054)		(0.051)
P-Value of $\beta_1 + \beta_3$		0.014		0.007		0.001		0.037		0.005		0.001
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2667	2667	1621	1621	4288	4288	1345	1345	848	848	2193	2193
Mean of Y : Overall		0.213		0.083		0.164		0.196		0.072		0.148
Mean of Y : Control Group		0.224		0.098		0.177		0.211		0.093		0.166
Mean of Y : Frequent Interactors in Control Group		0.345		0.181		0.285		0.353		0.190		0.294
P-Value from pooling test:	0.301	0.908					0.790	0.232				

Notes: Outcome variable is coded 1 if respondent reported requesting a private benefit from a local politician in 2012 or 2013; 0 otherwise. Pooled regressions examine requests in either year. Treatment is coded 1 if respondent belongs to a participating household in a neighborhood cluster selected for treatment; 0 otherwise. Frequent Interactor with Politician is coded 1 if respondent reported talking at least monthly to a local politician before the 2012 electoral campaign commenced; 0 otherwise. Pooling test reports the p-value on the joint test of equality of the regression coefficients in years 2012 and 2013. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table 7: Ask for and Receive Private Help (2012 and 2013)

	Ask for and receive private help from any politician											
	All Municipalities						Municipalities with Incumbent Mayors Running for Re-election					
	Pooled		2012		2013		Pooled		2012		2013	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
β_1: Treatment	-0.007	0.005	-0.004	0.012	-0.008	-0.003	-0.016	-0.003	-0.017	-0.003	-0.012	-0.003
	(0.010)	(0.010)	(0.013)	(0.014)	(0.009)	(0.009)	(0.013)	(0.013)	(0.018)	(0.019)	(0.011)	(0.011)
β_2: Frequent Interactor With Politician		0.077***		0.097***		0.032		0.081***		0.090**		0.050*
		(0.020)		(0.027)		(0.020)		(0.025)		(0.035)		(0.028)
β_3: Treatment X Frequent Interactor With Politician		-0.068***		-0.084**		-0.032		-0.069**		-0.071		-0.050
		(0.025)		(0.035)		(0.026)		(0.032)		(0.044)		(0.032)
$\beta_1 + \beta_3$:		-0.062**		-0.072**		-0.034		-0.072**		-0.074*		-0.052*
		(0.024)		(0.033)		(0.024)		(0.031)		(0.041)		(0.031)
P-Value of $\beta_1 + \beta_3$		0.029		0.168		0.011		0.075		0.097		0.020
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4284	4284	2663	2663	1621	1621	2192	2192	1344	1344	848	848
Mean of Y : Overall		0.090		0.124		0.035		0.083		0.118		0.027
Mean of Y : Control Group		0.092		0.124		0.039		0.088		0.122		0.033
Mean of Y : Frequent Interactors in Control Group		0.167		0.218		0.076		0.165		0.216		0.076
P-Value from pooling test:	0.717	0.146					0.829	0.318				

Notes: Outcome variable is coded 1 if respondent reported requesting and receiving a private benefit from a local politician in 2012 or 2013; 0 otherwise. Pooled regressions examine requests in either year. Treatment is coded 1 if respondent belongs to a participating household in a neighborhood cluster selected for treatment; 0 otherwise. Frequent Interactor with Politician is coded 1 if respondent reported talking at least monthly to a local politician before the 2012 electoral campaign commenced; 0 otherwise. Pooling test reports the p-value on the joint test of equality of the regression coefficients in years 2012 and 2013. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table 8: Public Good Requests (2012 and 2013)

	Ask for public goods from any politician											
	All Municipalities						Municipalities with Incumbent Mayors Running for Re-election					
	Pooled		2012		2013		Pooled		2012		2013	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
β_1: Treatment	-0.0039	-0.0034	-0.0023	-0.0043	-0.0068	-0.0026	-0.0017	0.0007	0.0031	0.00132	-0.0092	-0.0007
	(0.0049)	(0.0048)	(0.0067)	(0.0066)	(0.0050)	(0.0051)	(0.0059)	(0.0062)	(0.0092)	(0.0091)	(0.0062)	(0.0065)
β_2: Frequent Interactor With Politician		0.0317***		0.0310*		0.0324*		0.0350**		0.0323		0.0390
		(0.0118)		(0.0158)		(0.0167)		(0.0153)		(0.0202)		(0.0241)
β_3: Treatment X Frequent Interactor With Politician		-0.0027		0.0111		-0.0246		-0.0122		0.0095		-0.0468*
		(0.0154)		(0.0217)		(0.0199)		(0.0202)		(0.0288)		(0.0246)
$\beta_1 + \beta_3$		-0.0062		0.0068		-0.0272		-0.0115		0.0108		-0.0475
		(0.0149)		(0.0208)		(0.0187)		(0.0187)		(0.0273)		(0.0228)
P-Value of $\beta_1 + \beta_3$		0.679		0.744		0.147		0.539		0.692		0.0389
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4292	4292	2667	2667	1625	1625	2197	2197	1345	1345	852	852
Mean of Y : Overall		0.0235		0.0304		0.0123		0.0246		0.0335		0.0106
Mean of Y : Control Group		0.0264		0.0329		0.0156		0.0271		0.0345		0.0154
Mean of Y : Frequent Interactors in Control Group		0.0505		0.0556		0.0417		0.0550		0.0576		0.0506

Notes: Outcome variable is coded 1 if respondent reported requesting a public good from a local politician in 2012 or 2013; 0 otherwise. Pooled regressions examine requests in either year. Treatment is coded 1 if respondent belongs to a participating household in a neighborhood cluster selected for treatment; 0 otherwise. Frequent Interactor with Politician is coded 1 if respondent reported talking at least monthly to a local politician before the 2012 electoral campaign commenced; 0 otherwise. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table 9: Effects on Votes For Incumbent and Other Electoral Outcomes (2012)

	Votes for Incumbent Mayor				Votes for Challengers	Turnout	Blank and Null Votes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Number of Treated Individuals	-0.219**	-0.190**	-0.210**		0.254*	0.008	-0.035
	[0.040]	[0.034]	[0.030]		[0.072]	[0.962]	[0.610]
Number of Treated Individuals who are Frequent Interactors				-0.361			
				[0.282]			
Number of Treated Individuals who are Infrequent Interactors				-0.080			
				[0.410]			
Number of Control Individuals	0.074	0.096**	0.075		0.044	0.086	-0.033
	[0.104]	[0.016]	[0.106]		[0.584]	[0.450]	[0.480]
Number of Control Individuals who are Frequent Interactors				0.085			
				[0.936]			
Number of Control Individuals who are Infrequent Interactors				0.106			
				[0.276]			
Total Eligible Voters (2012)	Yes	No	No	No	No	No	No
Total Eligible Voters (2008)	No	Yes	Yes	Yes	Yes	Yes	Yes
Change in Eligible Voters (2008-2012)	No	No	Yes	Yes	Yes	Yes	Yes
Location Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	909	909	909	909	909	909	909

Notes: Outcome variables denoted by column headers. p-values using wild clustered bootstrap in brackets. The estimation sample consists of 21 municipalities in which the incumbent mayor was running for reelection in 2012. We allow standard errors to be correlated within municipalities. * 10%, ** 5%, *** 1% significance levels.

Table 10: Politician responses (2012)

	Campaign Visits		Campaign Visit Handouts		Offered Handout		Received Handout	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1: Treatment	0.0166	0.0109	0.0140	0.0225*	-0.0029	-0.0085	-0.0046	-0.0078
	(0.0199)	(0.0222)	(0.0110)	(0.0118)	(0.0152)	(0.0159)	(0.0098)	(0.0105)
β_2: Frequent Interactor With Politician		0.0575**		0.0662***		0.0099		0.0025
		(0.0258)		(0.0205)		(0.0233)		(0.0172)
β_3: Treatment X Frequent Interactor With Politician		0.0269		-0.0426		0.0406		0.0191
		(0.0360)		(0.0318)		(0.0353)		(0.0247)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2274	2271	2308	2305	1634	1626	1624	1616
Mean of Y : Overall		0.821		0.0625		0.0769		0.0334
Mean of Y : Control Group		0.809		0.0576		0.0766		0.0362
Mean of Y : Frequent Interactors in Control Group		0.876		0.109		0.0764		0.0350
P-Value of $\beta_1 + \beta_3$		0.243		0.492		0.347		0.626

Notes: Dependent variable is listed in the column header. Treatment is coded 1 if respondent belongs to a participating household in a neighborhood cluster selected for treatment; 0 otherwise. Frequent Interactor with Politician is coded 1 if respondent reported talking at least monthly to a local politician before the 2012 electoral campaign commenced; 0 otherwise. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Online Appendix (Not for Publication)

Appendix A: Additional Figures and Tables

Table A1: Compliance

	Households	Cisterns in November 2012	Cisterns in November 2013
Assigned to Treatment	615	67.45%	90.78%
Assigned to Control	693	20.23%	65.30%
Total	1308		

Table A2: Baseline Characteristics of Treatment and Control Groups

Variable	Treatment Group	Control Group	Difference	Standard Error of Difference
Individual Characteristics				
Age	36.587	37.393	-0.345	(0.642)
Female	0.518	0.535	-0.016	(0.011)
Current Student	0.139	0.126	0.005	(0.013)
Years of Education	5.903	5.728	0.006	(0.193)
Household Characteristics				
Household Size	4.288	4.221	0.054	(0.119)
Number of Total Neighbors	17.658	15.959	1.997	(1.377)
Neighbor has Cistern	0.664	0.598	0.060***	(0.035)
Bolsa Familia Amount Received	91.954	85.915	4.945	(4.327)
Total Household Expenditure	367.149	376.861	-6.454	(12.636)
Household Wealth Per Member	18,955.48	20,256.44	-1,187.8	(992.416)
Household Expenditure Per Member	100.324	109.276	-7.745	(4.776)
Age of Household Head	43.899	44.840	-0.555	(0.937)
Household Head Education	5.734	5.830	-0.241	(0.250)
Household Head is Female	0.182	0.182	0.007	(0.019)
Owns House	0.863	0.873	-0.016	(0.021)
Number of Room in House	5.266	5.331	-0.082	(0.079)
Has Access to Electricity	0.883	0.905	-0.018	(0.018)
Migrated Recently	0.111	0.107	0.006	(0.017)
Owns Land	0.483	0.465	-0.004	(0.030)
Land Size	3.413	3.554	-0.218	(0.684)
Household Members 0-6 Months	0.047	0.058	-0.015	(0.013)
Household Members 6 Months - 5 Years	0.631	0.612	-0.001	(0.038)
Household Members 5 Years - 64 Years	3.397	3.316	0.099	(0.112)
Household Members Older than 64 Years	0.213	0.235	-0.029	(0.028)
Voted in 2008 Municipal Election	0.891	0.865	0.020	(0.019)
P-Value of Joint F-Test			0.647	

Notes: Columns 1-2 present the mean of each variable for the treatment and control group, respectively. Column 3 reports differences estimated in OLS regression model with municipality fixed effects. Column 4 reports the standard errors of the differences, which are clustered at the neighborhood level and reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A3: Attrition

	Wave 0 (Localization)	Wave 1 (Baseline)	Wave 2 (Election Year)	Wave 3 (Non-election Year)
Households	1,308	1,189	1,238	1,119
Rate of Attrition from Wave 0		9.10%	5.35%	14.45%
Correlation with Treatment Status	- 0.007			
Standard Error	(0.015)			

Table A4: Specific Private Help Requests (Pooled Data, 2012 and 2013)

	Ask for private help from any politician					
	All Municipalities			Municipalites with Incumbent Mayors Running for Re-Election		
	Water	Construction	Medical Treatment/Medicine	Water	Construction	Medical Treatment/Medicine
β_1: Treatment	0.0037	-0.0007	-0.0131	-0.0062	0.0009	-0.0200**
	(0.0064)	(0.0073)	(0.0081)	(0.00769)	(0.0087)	(0.0093)
β_2: Frequent Interactor With Politician	0.0420***	0.0418**	0.0123	0.0430**	0.0375*	0.0356
	(0.0131)	(0.0168)	(0.0172)	(0.0173)	(0.0205)	(0.0235)
β_3: Treatment X Frequent Interactor With Politician	-0.0422***	-0.0367*	-0.0100	-0.0332	-0.0320	-0.0173
	(0.0163)	(0.0211)	(0.0214)	(0.0213)	(0.0264)	(0.0294)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4292	4292	4292	2197	2197	2197
Mean of Dep Variable	0.0380	0.0403	0.0531	0.0341	0.0355	0.0478
P-Value of $\beta_1 + \beta_3$	0.0133	0.0659	0.228	0.0617	0.210	0.160

Notes: Outcome variable is coded 1 if respondent reported requesting a specific private good denoted by the column header from a local politician in 2012 or 2013; 0 otherwise. Treatment is coded 1 if respondent belongs to a participating household in a neighborhood cluster selected for treatment; 0 otherwise. Frequent Interactor with Politician is coded 1 if respondent reported talking at least monthly to a local politician before the 2012 electoral campaign commenced; 0 otherwise. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A5: Private Help Requests (Instrumental Variables Estimation, 2012 and 2013)

	Ask for private help from any politician											
	All Municipalities						Municipalities with Incumbent Mayors Running for Re-election					
	Pooled		2012		2013		Pooled		2012		2013	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
β_1: Owns a Cistern	-0.085***	-0.043	-0.058	-0.022	-0.068**	-0.037	-0.111***	-0.065*	-0.084**	-0.052	-0.076**	-0.039
	(0.032)	(0.033)	(0.036)	(0.037)	(0.030)	(0.029)	(0.038)	(0.038)	(0.041)	(0.042)	(0.033)	(0.032)
β_2: Frequent Interactor With Politician		0.196***		0.170***		0.114**		0.215***		0.178***		0.143**
		(0.053)		(0.049)		(0.045)		(0.062)		(0.057)		(0.058)
β_3: Owns a Cistern X Frequent Interactor With Politician		-0.227***		-0.199**		-0.167**		-0.237**		-0.178		-0.203**
		(0.086)		(0.098)		(0.079)		(0.096)		(0.117)		(0.091)
$\beta_1 + \beta_3$		-0.270***		-0.221**		-0.204***		-0.302***		-0.229**		-0.242***
		(0.082)		(0.093)		(0.078)		(0.092)		(0.112)		(0.089)
P-Value of $\beta_1 + \beta_3$		0.001		0.018		0.009		0.001		0.040		0.007
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4090	4090	2663	2663	1617	1617	2083	2083	1343	1343	846	846
Mean of Y : Overall		0.168		0.213		0.083		0.153		0.196		0.072
Mean of Y : Control Group		0.184		0.225		0.098		0.175		0.211		0.093
Mean of Y : Frequent Interactors in Control Group		0.295		0.345		0.181		0.308		0.353		0.190
Cragg-Donald F-Stat	932.9	440.91	937.58	385.38	617.17	272.52	632.87	467.19	663.81	219.5	534.93	222.65

Notes: Outcome variable is coded 1 if respondent reported requesting a private benefit from a local politician in 2012 or 2013; 0 otherwise. Pooled regressions examine requests in either year. Treatment is coded 1 if respondent belongs to a participating household in a neighborhood cluster selected for treatment; 0 otherwise. Frequent Interactor with Politician is coded 1 if respondent reported talking at least monthly to a local politician before the 2012 electoral campaign commenced; 0 otherwise. Owns a Cistern is coded 1 if the household had a cistern in the relevant year. It is instrumented by Assignment to Treatment. Owns a Cistern X Frequent Interactor with Politician is instrumented by Assignment to Treatment X Frequent Interactor with Politician. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A6: Ask for and Receive Private Help Requests (Instrumental Variables Estimation, 2012 and 2013)

	Ask for and receive private help from any politician											
	All Municipalities						Municipalities with Incumbent Mayors Running for Re-election					
	Pooled		2012		2013		Pooled		2012		2013	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
β_1: Owns a Cistern	-0.020	0.010	-0.008	0.024	-0.017	-0.005	-0.037	-0.010	-0.033	-0.007	-0.020	-0.005
	(0.024)	(0.025)	(0.028)	(0.030)	(0.018)	(0.019)	(0.029)	(0.029)	(0.035)	(0.036)	(0.019)	(0.019)
β_2: Frequent Interactor With Politician		0.131***		0.131***		0.043		0.121**		0.115**		0.061*
		(0.040)		(0.040)		(0.028)		(0.048)		(0.048)		(0.036)
β_3: Owns a Cistern X Frequent Interactor With Politician		-0.159**		-0.179**		-0.060		-0.142*		-0.144		-0.083
		(0.065)		(0.078)		(0.051)		(0.073)		(0.091)		(0.057)
$\beta_1 + \beta_3$		-0.149**		-0.155**		-0.066		-0.151**		-0.150*		-0.088
		(0.063)		(0.075)		(0.048)		(0.072)		(0.879)		(0.055)
P-Value of $\beta_1 + \beta_3$		0.018		0.038		0.175		0.037		0.087		0.107
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4090	4090	2663	2663	1617	1617	2083	2083	1343	1343	846	846
Mean of Y : Overall		0.092		0.124		0.035		0.084		0.118		0.027
Mean of Y : Control Group		0.096		0.125		0.039		0.090		0.122		0.033
Mean of Y : Frequent Interactors in Control Group		0.172		0.218		0.076		0.169		0.216		0.076
Cragg-Donald F-Stat	932.9	440.91	937.58	385.38	617.17	272.52	632.87	467.19	663.81	219.5	534.93	222.65

Notes: Outcome variable is coded 1 if respondent reported requesting and receiving a private benefit from a local politician in 2012 or 2013; 0 otherwise. Pooled regressions examine requests in either year. Treatment is coded 1 if respondent belongs to a participating household in a neighborhood cluster selected for treatment; 0 otherwise. Frequent Interactor with Politician is coded 1 if respondent reported talking at least monthly to a local politician before the 2012 electoral campaign commenced; 0 otherwise. Owns a Cistern is coded 1 if the household had a cistern in the relevant year. It is instrumented by Assignment to Treatment. Owns a Cistern X Frequent Interactor with Politician is instrumented by Assignment to Treatment X Frequent Interactor with Politician. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A7: Ask for and Receive Specific Private Help (Pooled Data, 2012 and 2013)

	Ask for and receive private help from any politician					
	All Municipalities			Municipalites with Incumbent Mayors Running for Re-Election		
	Water	Construction	Medical Treatment/Medicine	Water	Construction	Medical Treatment/Medicine
β_1: Treatment	0.0135**	0.0022	-0.0098	0.0023	0.0058	-0.0167**
	(0.0055)	(0.0039)	(0.0066)	(0.0069)	(0.0048)	(0.0078)
β_2: Frequent Interactor With Politician	0.0336***	0.0104	0.0114	0.0216*	0.0061	0.0230
	(0.0106)	(0.0087)	(0.0146)	(0.0114)	(0.0103)	(0.0173)
β_3: Treatment X Frequent Interactor With Politician	-0.0434***	-0.0053	-0.0134	-0.0159	-0.0057	-0.0207
	(0.0134)	(0.0121)	(0.0175)	(0.0165)	(0.0146)	(0.0212)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4288	4288	4288	2196	2196	2196
Mean of Dep Variable	0.0226	0.0142	0.0354	0.0209	0.0118	0.0323
P-Value of $\beta_1 + \beta_3$	0.0231	0.792	0.144	0.448	0.997	0.0558

Notes: Outcome variable is coded 1 if respondent reported requesting and receiving a specific private good denoted by the column header from a local politician in 2012 or 2013; 0 otherwise. Treatment is coded 1 if respondent belongs to a participating household in a neighborhood cluster selected for treatment; 0 otherwise. Frequent Interactor with Politician is coded 1 if respondent reported talking at least monthly to a local politician before the 2012 electoral campaign commenced; 0 otherwise. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A8: Ask for and Receive Public Goods (2012 and 2013)

	Ask for and receive public goods from any politician											
	All Municipalities						Municipalities with Incumbent Mayors Running for Re-election					
	Pooled		2012		2013		Pooled		2012		2013	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
β_1: Treatment	-0.0004	0.0020	-0.00107	0.0029	0.00035	0.0001	0.0015	0.0038	0.0036	0.00774	-0.0018	-0.0023
	(0.0026)	(0.0024)	(0.0042)	(0.0039)	(0.0014)	(0.0015)	(0.0040)	(0.0038)	(0.0066)	(0.0063)	(0.0017)	(0.0021)
β_2: Frequent Interactor With Politician		0.0208**		0.0338**		-0.0016		0.0236**		0.0391**		-0.0021
		(0.0086)		(0.0137)		(0.0014)		(0.0114)		(0.0185)		(0.0022)
β_3: Treatment X Frequent Interactor With Politician		-0.0135		-0.0216		0.0012		-0.0118		-0.0201		0.0026
		(0.0107)		(0.0171)		(0.0015)		(0.0155)		(0.0248)		(0.0026)
$\beta_1 + \beta_3$:		-0.0115		-0.0186		0.0013		-0.0080		-0.0124		0.0003
		(0.0103)		(0.0164)		(0.0012)		(0.0147)		(0.0234)		(0.0009)
P-Value of $\beta_1 + \beta_3$		0.264		0.255		0.260		0.589		0.599		0.692
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4288	4288	2663	2663	1625	1625	2196	2196	1344	1344	852	852
Mean of Y : Overall		0.0079		0.0120		0.0012		0.0096		0.0149		0.0012
Mean of Y : Control Group		0.0085		0.0129		0.0012		0.0093		0.0138		0.0154
Mean of Y : Frequent Interactors in Control Group		0.0253		0.0397		0		0.0275		0.0432		0.0380

Notes: Outcome variable is coded 1 if respondent reported requesting and receiving a public good from a local politician in 2012 or 2013; 0 otherwise. Pooled regressions examine requests in either year. Treatment is coded 1 if respondent belongs to a participating household in a neighborhood cluster selected for treatment; 0 otherwise. Frequent Interactor with Politician is coded 1 if respondent reported talking at least monthly to a local politician before the 2012 electoral campaign commenced; 0 otherwise. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A9: Robustness to Above Median Non-Compliance for Private Help Requests

	Ask for private help from any politician											
	All Municipalities						Municipalities with Incumbent Mayors Running for Re-election					
	Pooled		2012		2013		Pooled		2012		2013	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
β_1: Treatment	-0.0414**	-0.0306*	-0.0380*	-0.0300	-0.0493**	-0.0358*	-0.0589***	-0.0465**	-0.0586**	-0.0485*	-0.0610**	-0.0471**
	(0.0180)	(0.0175)	(0.0219)	(0.0224)	(0.0225)	(0.0200)	(0.0205)	(0.0207)	(0.0252)	(0.0264)	(0.0263)	(0.0234)
β_2: Above Median Noncompliance in Control Group	-0.0597	-0.0504	-0.116	-0.115	0.0146	0.0437*	-0.0640	-0.0877	0.116	0.0806	0.00320	0.0188
	(0.0409)	(0.0404)	(0.123)	(0.132)	(0.0163)	(0.0248)	(0.101)	(0.104)	(0.0901)	(0.0933)	(0.00684)	(0.0232)
β_3: Treatment X Above Median Noncompliance	0.0235	0.0378	0.0220	0.0416	0.0320	0.0368	0.0409	0.0675**	0.0408	0.0662	0.0474	0.0723*
	(0.0250)	(0.0260)	(0.0335)	(0.0357)	(0.0294)	(0.0284)	(0.0331)	(0.0337)	(0.0461)	(0.0470)	(0.0371)	(0.0388)
β_4: Frequent Interactor With Politician		0.112***		0.0993**		0.127**		0.114***		0.105*		0.117**
		(0.0370)		(0.0473)		(0.0498)		(0.0392)		(0.0536)		(0.0584)
β_5: Treatment X Frequent Interactor With Politician		-0.0715		-0.0540		-0.0902		-0.0800		-0.0667		-0.0890
		(0.0481)		(0.0644)		(0.0604)		(0.0520)		(0.0741)		(0.0688)
β_6: Frequent Interactor X Above Median Noncompliance		0.0143		0.0634		-0.0736		0.0703		0.106		0.00510
		(0.0527)		(0.0662)		(0.0648)		(0.0681)		(0.0813)		(0.0908)
β_7: Treatment X Frequent Interactor X Above Median Noncompliance		-0.0507		-0.0744		-0.00450		-0.0743		-0.0450		-0.0969
		(0.0676)		(0.0885)		(0.0799)		(0.0869)		(0.117)		(0.103)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4288	4288	2667	2667	1621	1621	2193	2193	1345	1345	848	848
Mean of Y : Overall		0.164		0.213		0.0827		0.148		0.196		0.0719
Mean of Y : Control Group		0.177		0.224		0.0976		0.166		0.211		0.0931
Mean of Y : Frequent Interactors in Control Group		0.285		0.345		0.181		0.294		0.353		0.190
P-Value of $\beta_1 + \beta_3$		0.0323		0.171		0.0476		0.0109		0.0919		0.0603

Notes: Outcome variable is coded 1 if respondent reported requesting a private benefit from a local politician in 2012 or 2013; 0 otherwise. Pooled regressions examine requests in either year. Treatment is coded 1 if respondent belongs to a participating household in a neighborhood cluster selected for treatment; 0 otherwise. Frequent Interactor with Politician is coded 1 if respondent reported talking at least monthly to a local politician before the 2012 electoral campaign commenced; 0 otherwise. Above Median Noncompliance partitions sample municipalities at the median of control group noncompliance. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A10: Private Help Requests Controlling for Citizen Engagement (Pooled Data, 2012 and 2013)

	All Municipalities				Municipalities with Incumbent Mayors Running for Re-Election			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1: Treatment	-0.006	-0.015	0.050	0.057	-0.020	-0.024	0.001	0.005
	(0.014)	(0.013)	(0.037)	(0.038)	(0.017)	(0.016)	(0.054)	(0.055)
β_2: Frequent Interactor With Politician	0.117***	0.120***	0.121***	0.118***	0.142***	0.146***	0.144***	0.142***
	(0.027)	(0.027)	(0.027)	(0.027)	(0.034)	(0.033)	(0.033)	(0.033)
β_3: Treatment X Frequent Interactor With Politician	-0.093***	-0.098***	-0.100***	-0.097***	-0.106**	-0.112***	-0.112***	-0.110**
	(0.034)	(0.034)	(0.034)	(0.034)	(0.043)	(0.043)	(0.043)	(0.043)
Member of a Community Association	Yes	No	No	Yes	Yes	No	No	Yes
Member of a Community Association X Treatment	Yes	No	No	Yes	Yes	No	No	Yes
President of a Community Association	No	Yes	No	Yes	No	Yes	No	Yes
President of a Community Association X Treatment	No	Yes	No	Yes	No	Yes	No	Yes
Voted in 2008 Municipal Election	No	No	Yes	Yes	No	No	Yes	Yes
Voted in 2008 Municipal Election X Treatment	No	No	Yes	Yes	No	No	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4288	4288	4231	4231	2193	2193	2153	2153
Mean of Y : Overall	0.164	0.164	0.164	0.164	0.148	0.148	0.146	0.146
Mean of Y : Control Group	0.177	0.177	0.177	0.177	0.166	0.166	0.164	0.164
Mean of Y : Frequent Interactors in Control Group	0.285	0.285	0.286	0.286	0.294	0.294	0.290	0.290

Notes: Outcome variable is coded 1 if respondent reported requesting a private benefit from a local politician in 2012 or 2013; 0 otherwise. Treatment is coded 1 if respondent belongs to a participating household in a neighborhood cluster selected for treatment; 0 otherwise. Frequent Interactor with Politician is coded 1 if respondent reported talking at least monthly to a local politician before the 2012 electoral campaign commenced; 0 otherwise. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A11: Private Help Requests - Frequent Interactors who Declare Public Support (2012 and 2013)

	Ask for private help from any politician																	
	Panel A: All Municipalities									Panel B: Municipalities with Incumbent Mayors Running for Reelection								
	Pooled			2012			2013			Pooled			2012			2013		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
β_1: Treatment	-0.0296**	-0.0207*	-0.0124	-0.0271	-0.0194	-0.0103	-0.0330**	-0.0236	-0.0175	-0.0440***	-0.0307*	-0.0228	-0.0438**	-0.0326	-0.0259	-0.0437**	-0.0297	-0.0211
	(0.0125)	(0.0125)	(0.0130)	(0.0167)	(0.0171)	(0.0177)	(0.0147)	(0.0144)	(0.0143)	(0.0162)	(0.0157)	(0.0164)	(0.0213)	(0.0215)	(0.0220)	(0.0193)	(0.0190)	(0.0187)
β_2: Declared Frequent Interactor		0.108***	0.118***		0.108***	0.120***		0.0990**	0.104**		0.148***	0.158***		0.151***	0.162***		0.127**	0.135**
		(0.0327)	(0.0331)		(0.0396)	(0.0399)		(0.0438)	(0.0437)		(0.0391)	(0.0398)		(0.0512)	(0.0515)		(0.0579)	(0.0579)
β_3: Treatment X Declared Frequent Interactor		-0.0854**	-0.0934**		-0.0729	-0.0813		-0.0932*	-0.0992*		-0.117**	-0.124**		-0.0983	-0.104		-0.128*	-0.136**
		(0.0419)	(0.0422)		(0.0539)	(0.0540)		(0.0531)	(0.0530)		(0.0496)	(0.0502)		(0.0707)	(0.0710)		(0.0673)	(0.0674)
β_4: Undeclared Frequent Interactor			0.120***			0.153***			0.0585			0.117**			0.125**			0.0927
			(0.0373)			(0.0478)			(0.0496)			(0.0484)			(0.0550)			(0.0766)
β_5: Treatment X Undeclared Frequent Interactor			-0.103**			-0.110			-0.0814			-0.0872			-0.0585			-0.107
			(0.0499)			(0.0695)			(0.0583)			(0.0678)			(0.0928)			(0.0835)
$\beta_1 + \beta_3$:		-0.106**	-0.106**		-0.0923*	-0.0916*		-0.117**	-0.117**		-0.147***	-0.146***		-0.131*	-0.130*		-0.158**	-0.157**
		(0.0414)	(0.0414)		(0.0523)	(0.0523)		(0.0531)	(0.0532)		(0.0496)	(0.0496)		(0.0682)	(0.0683)		(0.0670)	(0.0673)
P-Value of $\beta_1 + \beta_3$		0.0108	0.0111		0.0785	0.0808		0.0283	0.0287		0.0033	0.0035		0.0563	0.0591		0.0195	0.0204
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4288	4286	4286	2667	2666	2666	1621	1620	1620	2193	2193	2193	1345	1345	1345	848	848	848
Mean of Y : Overall		0.164			0.213			0.0827			0.148			0.196			0.0719	
Mean of Y : Control Group		0.177			0.224			0.0977			0.166			0.211			0.0931	
Mean of Y : Undeclared Infrequent Interactors in Control Group		0.165			0.211			0.0872			0.148			0.191			0.0815	

Notes: Outcome variable is coded 1 if respondent reported requesting a private benefit from a local politician in 2012 or 2013; 0 otherwise. Pooled regressions examine effects in either year. Treatment is coded 1 if respondent belongs to a participating household in a neighborhood cluster selected for treatment; 0 otherwise. Declared Frequent Interactor is coded 1 if respondent reported talking at least monthly to a local politician before the 2012 electoral campaign commenced and engaged in at least one form of declared support; 0 otherwise. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A12: Ask for and Receive Private Help - Frequent Interactors who Declare Public Support (2012 and 2013)

	Ask for and receive private help from any politician																	
	Panel A: All Municipalities									Panel B: Municipalities with Incumbent Mayors Running for Reelection								
	Pooled			2012			2013			Pooled			2012			2013		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)
β_1: Treatment	-0.00730	0.0026	0.0067	-0.0036	0.0067	0.0116	-0.0081	-0.0049	-0.0026	-0.0155	-0.0047	-0.0025	-0.0171	-0.0032	-0.0029	-0.0117	-0.0071	-0.0023
	(0.0097)	(0.0098)	(0.0099)	(0.0134)	(0.0140)	(0.0142)	(0.0091)	(0.0096)	(0.0093)	(0.0127)	(0.0127)	(0.0127)	(0.0180)	(0.0188)	(0.0190)	(0.0114)	(0.0121)	(0.0113)
β_2: Declared Frequent Interactor		0.0703***	0.0766***		0.0847**	0.0934***		0.0368	0.0387		0.0896***	0.0942***		0.0998**	0.105**		0.0581	0.0610
		(0.0259)	(0.0260)		(0.0345)	(0.0346)		(0.0312)	(0.0307)		(0.0316)	(0.0322)		(0.0435)	(0.0440)		(0.0458)	(0.0453)
β_3: Treatment X Declared Frequent Interactor		-0.0727**	-0.0766**		-0.0916**	-0.0960**		-0.0326	-0.0348		-0.0914**	-0.0931**		-0.111**	-0.111**		-0.0434	-0.0481
		(0.0324)	(0.0325)		(0.0438)	(0.0439)		(0.0373)	(0.0370)		(0.0393)	(0.0395)		(0.0540)	(0.0540)		(0.0509)	(0.0504)
β_4: Undeclared Frequent Interactor			0.0732***			0.103***			0.0229			0.0549			0.0648			0.0347
			(0.0268)			(0.0374)			(0.0306)			(0.0345)			(0.0458)			(0.0438)
β_5: Treatment X Undeclared Frequent Interactor			-0.0481			-0.0555			-0.0302			-0.0197			0.0172			-0.0611
			(0.0374)			(0.0549)			(0.0396)			(0.0503)			(0.0752)			(0.0463)
$\beta_1 + \beta_3$:		-0.0701**	-0.0699**		-0.0848**	-0.0844**		-0.0374	-0.0374		-0.0961**	-0.0956**		-0.115**	-0.114**		-0.0505	-0.0503
		(0.0317)	(0.0317)		(0.0420)	(0.0420)		(0.0353)	(0.0354)		(0.0389)	(0.0390)		(0.0516)	(0.0516)		(0.0477)	(0.0479)
P-Value of $\beta_1 + \beta_3$		0.0275	0.0282		0.0440	0.0453		0.290	0.291		0.0145	0.0150		0.0276	0.0287		0.292	0.294
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4284	4282	4282	2663	2662	2662	1621	1620	1620	2192	2192	2192	1344	1344	1344	848	848	848
Mean of Y : Overall		0.0904			0.124			0.0346			0.0826			0.118			0.0271	
Mean of Y : Control Group		0.0925			0.124			0.0386			0.0877			0.122			0.0333	
Mean of Y : Undeclared Infrequent Interactors in Control Group		0.0831			0.113			0.0336			0.0757			0.106			0.0272	

Notes: Outcome variable is coded 1 if respondent reported requesting and receiving a private benefit from a local politician in 2012 or 2013; 0 otherwise. Pooled regressions examine effects in either year. Treatment is coded 1 if respondent belongs to a participating household in a neighborhood cluster selected for treatment; 0 otherwise. Declared Frequent Interactor is coded 1 if respondent reported talking at least monthly to a local politician before the 2012 electoral campaign commenced and engaged in at least one form of declared support; 0 otherwise. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A13: Private Help Requests from PT Mayors

	Ask for private help from any politician											
	All Municipalities						Municipalities with Incumbent Mayors Running for Re-election					
	Pooled		2012		2013		Pooled		2012		2013	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
β_1: Treatment	-0.0333**	-0.0128	-0.0285	-0.0063	-0.0384**	-0.0230	-0.0481**	-0.0275	-0.0479*	-0.0281	-0.0441**	-0.0254
	(0.0150)	(0.0154)	(0.0202)	(0.0214)	(0.0166)	(0.0160)	(0.0203)	(0.0207)	(0.0276)	(0.0287)	(0.0219)	(0.0216)
β_2: Treatment X Mayor is PT	0.0170	0.0024	0.0060	-0.0164	0.0254	0.0241	0.0166	0.0162	0.0157	0.00694	0.0015	0.0178
	(0.0251)	(0.0271)	(0.0320)	(0.0357)	(0.0354)	(0.0354)	(0.0287)	(0.0301)	(0.0361)	(0.0389)	(0.0464)	(0.0441)
β_3: Frequent Interactor With Politician		0.132***		0.150***		0.0895**		0.148***		0.167***		0.102**
		(0.0291)		(0.0366)		(0.0351)		(0.0368)		(0.0472)		(0.0469)
β_4: Treatment X Frequent Interactor With Politician		-0.109***		-0.113**		-0.0881*		-0.101**		-0.0867		-0.102*
		(0.0378)		(0.0491)		(0.0448)		(0.0481)		(0.0678)		(0.0567)
β_5: Frequent Interactor with Politician X Mayor is PT		-0.0729		-0.100		-0.0232		-0.0381		-0.104		0.0766
		(0.0666)		(0.0793)		(0.0822)		(0.0841)		(0.0857)		(0.125)
β_6: Treatment X Frequent Interactor X Mayor is PT		0.0720		0.118		0.0004		-0.0300		0.0115		-0.103
		(0.0834)		(0.108)		(0.0948)		(0.0998)		(0.120)		(0.132)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4288	4288	2667	2667	1621	1621	2193	2193	1345	1345	848	848
Mean of Y : Overall		0.164		0.213		0.0827		0.148		0.196		0.0719
Mean of Y : Control Group		0.177		0.224		0.0976		0.166		0.211		0.0931
Mean of Y : Frequent Interactors in Control Group		0.285		0.345		0.181		0.294		0.353		0.190
P-Value of $\beta_1 + \beta_4$		0.0008		0.0104		0.0130		0.0046		0.0672		0.0236

Notes: Outcome variable is coded 1 if respondent reported requesting a private benefit from a local politician in 2012 or 2013; 0 otherwise. Pooled regressions examine requests in either year. Treatment is coded 1 if respondent belongs to a participating household in a neighborhood cluster selected for treatment; 0 otherwise. Frequent Interactor with Politician is coded 1 if respondent reported talking at least monthly to a local politician before the 2012 electoral campaign commenced; 0 otherwise. Mayor is PT is coded 1 if the Worker's Party is part of the Mayor's coalition; 0 otherwise. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A14: Ask for and Receive Private Help from PT Mayors

	Ask for and receive private help from any politician											
	All Municipalities						Municipalities with Incumbent Mayors Running for Re-election					
	Pooled		2012		2013		Pooled		2012		2013	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
β_1: Treatment	-0.0032	0.0102	0.0024	0.0190	-0.0109	-0.0042	-0.0194	-0.0054	-0.0194	-0.00528	-0.0154	-0.0038
	(0.0113)	(0.0116)	(0.0156)	(0.0167)	(0.0105)	(0.0106)	(0.0161)	(0.0162)	(0.0231)	(0.0246)	(0.0133)	(0.0130)
β_2: Treatment X Mayor is PT	-0.0098	-0.0151	-0.0275	-0.0313	0.0133	0.0054	0.0156	0.0114	0.0086	0.00961	0.0163	0.0022
	(0.0217)	(0.0225)	(0.0297)	(0.0310)	(0.0209)	(0.0227)	(0.0220)	(0.0234)	(0.0317)	(0.0348)	(0.0249)	(0.0259)
β_3: Frequent Interactor With Politician		0.0822***		0.0979***		0.0484**		0.0814***		0.0821**		0.0692**
		(0.0222)		(0.0297)		(0.0242)		(0.0288)		(0.0379)		(0.0345)
β_4: Treatment X Frequent Interactor With Politician		-0.0712**		-0.0852**		-0.0391		-0.0677*		-0.0626		-0.0637
		(0.0290)		(0.0391)		(0.0319)		(0.0355)		(0.0484)		(0.0400)
β_5: Frequent Interactor with Politician X Mayor is PT		-0.0361		0.0001		-0.0843***		-0.0157		0.0405		-0.0990**
		(0.0455)		(0.0718)		(0.0290)		(0.0591)		(0.0908)		(0.0405)
β_6: Treatment X Frequent Interactor X Mayor is PT		0.0172		-0.0035		0.0419		0.0055		-0.0406		0.0723*
		(0.0556)		(0.0886)		(0.0362)		(0.0748)		(0.116)		(0.0418)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4284	4284	2663	2663	1621	1621	2192	2192	1344	1344	848	848
Mean of Y : Overall		0.0903		0.124		0.0345		0.0826		0.118		0.0271
Mean of Y : Control Group		0.0924		0.124		0.0386		0.0877		0.122		0.131
Mean of Y : Frequent Interactors in Control Group		0.167		0.218		0.0764		0.165		0.216		0.190
P-Value of $\beta_1 + \beta_4$		0.0280		0.0710		0.154		0.0364		0.136		0.0811

Notes: Outcome variable is coded 1 if respondent reported requesting and receiving a private benefit from a local politician in 2012 or 2013; 0 otherwise. Pooled regressions examine requests in either year. Treatment is coded 1 if respondent belongs to a participating household in a neighborhood cluster selected for treatment; 0 otherwise. Frequent Interactor with Politician is coded 1 if respondent reported talking at least monthly to a local politician before the 2012 electoral campaign commenced; 0 otherwise. Mayor is PT is coded 1 if the Worker's Party is part of the Mayor's coalition; 0 otherwise. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A15: Effect on Votes for Workers' Party (PT), Year 2012

	(1)	(2)	(3)
Number of Treated Individuals in Machine	-0.003	0.036	0.013
	[0.986]	[0.808]	[0.933]
Number of Control Individuals in Machine	0.031	0.053	0.032
	[0.790]	[0.668]	[0.776]
Total Eligible Voters at Machine	0.346***		
	[0.000]		
Total Eligible Voters in Machine in 2008		-0.002	0.319***
		[0.940]	[0.000]
Change in Eligible Voters			0.363***
			[0.000]
Location Fixed Effects	Yes	Yes	Yes
Observations	867	867	867

Notes: Outcome variable is votes for candidate from either the PT or a coalition that includes PT. *p*-values in square brackets obtained using wild clustered bootstrap. The estimation sample consists of 21 municipalities in which the incumbent mayor was running for reelection in 2012. We allow standard errors to be correlated within municipalities. * 10%, ** 5%, *** 1% significance levels.

Table A16: Private Help Requests (Pooled Data with Rainfall, 2012 and 2013)

	Ask for private help from any politician					
	All Municipalities			Municipalites with Incumbent Mayors Running for Re-Election		
	(1)	(2)	(3)	(4)	(5)	(6)
β_1: Treatment	-0.0115	-0.0117	-0.0113	-0.0205	-0.0211	-0.0194
	(0.0131)	(0.0131)	(0.0132)	(0.0167)	(0.0166)	(0.0166)
β_2: Frequent Interactor With Politician	0.119***	0.119***	0.120***	0.142***	0.142***	0.145***
	(0.0264)	(0.0264)	(0.0267)	(0.0329)	(0.0329)	(0.0345)
β_3: Rainfall	-0.0210*	-0.0209*	-0.0174	-0.00990	-0.00984	-0.00444
	(0.0114)	(0.0114)	(0.0121)	(0.0176)	(0.0176)	(0.0169)
β_4: Treatment X Frequent Interactor With Politician	-0.0964***	-0.0953***	-0.0965***	-0.109***	-0.107**	-0.110**
	(0.0338)	(0.0340)	(0.0342)	(0.0420)	(0.0415)	(0.0428)
β_5: Treatment × Rainfall	-0.00546	-0.00410	-0.00680	-0.00769	-0.00593	-0.0115
	(0.0122)	(0.0129)	(0.0127)	(0.0189)	(0.0203)	(0.0197)
β_6: Treatment × Rainfall × Frequent Interactor		-0.00727	0.00789		-0.00816	0.0227
		(0.0217)	(0.0320)		(0.0313)	(0.0472)
β_7: Rainfall × Frequent Interactor			-0.0153			-0.0310
			(0.0239)			(0.0359)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4288	4288	4288	2193	2193	2193
Mean of Dep Variable			0.164			0.148
P-Value of $\beta_1 + \beta_4 + \beta_5$	0.0007			0.0011		
P-Value of $\beta_1 + \beta_4 + \beta_5 + \beta_6$		0.0010	0.0093		0.0026	0.0253

Notes: Outcome variable is coded 1 if respondent reported requesting a private benefit from a local politician in 2012 or 2013; 0 otherwise. Treatment is coded 1 if respondent belongs to a participating household in a neighborhood cluster selected for treatment; 0 otherwise. Frequent Interactor with Politician is coded 1 if respondent reported talking at least monthly to a local politician before the 2012 electoral campaign commenced; 0 otherwise. Standardized rainfall shocks are measured by the deviation of rainfall in the municipality during January-September of 2012 (or 2013) from the historic average rainfall during January-September of 1986-2011. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A17: Ask For and Receive Any Private Help (Pooled Data with Rainfall, 2012 and 2013)

	Ask for and receive private help from any politician					
	All Municipalities			Municipalites with Incumbent Mayors Running for Re-Election		
	(1)	(2)	(3)	(4)	(5)	(6)
β_1: Treatment	0.0076	0.0071	0.0077	-0.0004	-0.0008	0.0015
	(0.0102)	(0.0103)	(0.0103)	(0.0135)	(0.0135)	(0.0138)
β_2: Frequent Interactor With Politician	0.0756***	0.0756***	0.0775***	0.0793***	0.0793***	0.0833***
	(0.0196)	(0.0196)	(0.0200)	(0.0252)	(0.0252)	(0.0271)
β_3: Rainfall	-0.0082	-0.0080	-0.0023	-0.0030	-0.0030	0.0038
	(0.00935)	(0.00937)	(0.00967)	(0.0144)	(0.0144)	(0.0142)
β_4: Treatment X Frequent Interactor With Politician	-0.0676***	-0.0649**	-0.0669***	-0.0683**	-0.0670**	-0.0712**
	(0.0252)	(0.0254)	(0.0258)	(0.0312)	(0.0310)	(0.0326)
β_5: Treatment × Rainfall	-0.0051	-0.0019	-0.0064	-0.0064	-0.0054	-0.0124
	(0.0102)	(0.0106)	(0.0105)	(0.0148)	(0.0151)	(0.0150)
β_6: Treatment × Rainfall × Frequent Interactor		-0.0169	0.0081		-0.0043	0.0344
		(0.0157)	(0.0242)		(0.0209)	(0.0349)
β_7: Rainfall × Frequent Interactor			-0.0253			-0.0388
			(0.0189)			(0.0285)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4284	4284	4284	2192	2192	2192
Mean of Dep Variable			0.0903			0.0826
P-Value of $\beta_1 + \beta_4 + \beta_5$	0.0094			0.0235		
P-Value of $\beta_1 + \beta_4 + \beta_5 + \beta_6$		0.0045	0.0551		0.0359	0.232

Notes: Outcome variable is coded 1 if respondent reported requesting and receiving a private benefit from a local politician in 2012 or 2013; 0 otherwise. Treatment is coded 1 if respondent belongs to a participating household in a neighborhood cluster selected for treatment; 0 otherwise. Frequent Interactor with Politician is coded 1 if respondent reported talking at least monthly to a local politician before the 2012 electoral campaign commenced; 0 otherwise. Standardized rainfall shocks are measured by the deviation of rainfall in the municipality during January-September of 2012 (or 2013) from the historic average rainfall during January-September of 1986-2011. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Appendix B: Adjustment of Treatment and Control Individuals Regressors

The equation for estimating voting outcomes for the incumbent at a given machine in a given location in a given municipality is as follows:

$$y_{msj} = \alpha_{sj} + \gamma_1 \cdot TV_{msj} + \gamma_2 \cdot CV_{msj} + \gamma_3 \cdot EV_{msj} + \epsilon_{msj}, \tag{4}$$

where y_{msj} is the number of votes for the incumbent mayor in voting machine m, in voting location s, in municipality j. The regressor of interest is TV_{msj}, the total number of treated people assigned to vote in that particular machine. As mentioned in Section 5.2.3, we did not interview all adult eligible voters in our sample households as well as those in other ineligible households in the neighborhood cluster without a cistern at baseline (who potentially share water from the cisterns). Failing to address this undercounting of individuals in the treatment and control groups could bias our estimates of treatment effects upwards in magnitude. We thus adjust both the number of treated and control voters regressors (TV_{msj} and CV_{msj}, respectively), to incorporate estimates of the number of non-interviewed individuals in the following manner.

The regressor of interest, TV_{msj}, can be expressed as follows:

$$TV_{msj} = \sum_c TV_{cmsj}, \tag{5}$$

where TV_{cmsj} is the total number of treated voters in neighborhood cluster c assigned to vote in machine m, in voting location s, in municipality j. This can be further decomposed into the following expression:

$$TV_{msj} = \sum_c \left[TV_{I,h,cmsj} + TV_{NI,h,cmsj} + TV_{NI,h\_,cmsj} \right], \tag{6}$$

where $TV_{I,h,cmsj}$ denotes voters who were interviewed (denoted by the subscript I) from household h in cluster c and voted in the machine denoted by msj. $TV_{NI,h,cmsj}$ refers to voters from the same household h who were not interviewed by us (denoted by the subscript NI), and $TV_{NI,h\_,cmsj}$ denotes all voters from households other than h (i.e. households that were

not part of our survey) belonging to cluster c and voting in this particular machine. This can also be expressed as:

$$TV_{msj} = \sum_{c} \left[TV_{I,h,cmsj} + TV_{NI,h,cj}\theta_{NI,h,cms} + TV_{NI,h_-,cj}\theta_{NI,h_-,cms} \right], \quad (7)$$

where $TV_{NI,h,cj}$ is the total number of voters not interviewed in household h, $TV_{NI,h,cj}$ is the total number of voters belonging to all other households, $\theta_{NI,h,cms}$ and $\theta_{NI,h_-,cms}$ are the proportions of each of these two groups of voters assigned to vote in machine m in location s. We obtain or estimate these quantities in the following manner:

(a) obtain $TV_{NI,h,cj}$ from the baseline household survey data;

(b) estimate $TV_{NI,h_-,cj}$ by taking the median of the number of neighboring households from the same neighborhood cluster without a cistern at baseline reported by households in the localization survey and the median number of eligible voters per household in the baseline survey; and

(c) estimate the proportions $\theta_{NI,h,cms}$ and $\theta_{NI,h_-,cms}$ using information from the assignment of interviewed individuals (denoted as $\theta_{I,h,ms}$) across voting machines and locations.

We estimate that (i) 90.5 percent of eligible adults vote in the same municipality they are interviewed in; (ii) among these, 86.9 percent of interviewed individuals in a neighborhood cluster are assigned to vote in the same voting location s; and (iii) among those assigned to vote in the same location, 40.2 percent are assigned to vote in the same voting machine m**.** We assign the counted/estimated number of non-interviewed individuals in the neighborhood cluster in the following manner:

(1) 31.6 percent (= 90.5 percent x 86.9 percent x 40.2 percent) are assigned in equal proportion to the voting machines in which interviewed individuals are assigned to vote;

(2) 47.0 percent (= 90.5 percent x (86.9 percent - 34.8 percent)) of these individuals are assigned in equal proportion to the remaining set of voting machines of the locations in which interviewed individuals are assigned to vote;

(3) 11.9 percent (= 90.5 percent x (100 percent - 86.9 percent)) of these individuals are assigned to vote in other voting locations in the municipality; and

(4) 9.5 percent (= 100 percent - 90.5 percent) of these individuals are assigned to vote in other municipalities.[27]

To conduct appropriate inference, we must take into account two separate considerations. First, we need to address the fact that the adjusted regressors are subject to sampling error. Second, because we allow the errors to be correlated across voting machines and locations within a municipality, our sample is composed of 21 "clusters," or municipalities in which the mayor is running for reelection. To take the sampling error into account, we bootstrap the entire quantification exercise 1,000 times. In each replication, we draw a random sample of neighborhood clusters (sampling with replacement); and estimate each of the number of neighboring households, the number of eligible voters per neighboring household, and the proportion of individuals assigned to vote across locations and machines. This nonparametric bootstrap exercise allows us to construct p-values of the test of no impact of the treatment on electoral outcomes ($\gamma_1 = 0$) (Horowitz 2001). We carry out an analogous procedure to adjust the number of control voters regressor (CV_{msj}) and the p-value of the $\gamma_2 = 0$ statistical test. To address the small number of municipalities issue, we implement a wild cluster bootstrap procedure in each of the bootstrap samples above to generate replicate estimates of the Wald statistics for the $\gamma_1 = 0$ and $\gamma_2 = 0$ statistical tests (Cameron, Gelbach, and Miller 2008).

This procedure leads us to adjust our estimates of the treatment effects of the intervention on electoral outcomes downwards and makes our inference regarding the presence of treatment effects more conservative. Panel A of Appendix Table B1 reports the point estimates from the specification with the adjusted regressors of interest together with p-values from the non-parametric and wild cluster bootstrap procedure. Panel B of Appendix Table B1 shows that the qualitative relationship between the treatment and electoral results is robust to using a non-adjusted regressor; in this case, we report the p-value from a wild cluster bootstrap procedure. Finally, for purposes of comparability, we report in Panel A p-values from a standard wild cluster bootstrap procedure that does not take into account sampling error in the construction of the adjusted regressors. While the adjustment allows us to gain confidence in the appropriate magnitude of the treatment effects of the intervention, this

[27]Because we restrict the analysis to the voting locations where interviewed individuals are assigned to vote, we effectively exclude individuals in categories 3 and 4 for purposes of the adjustment.

indicates that the relationship and the degree of precision of our inference is driven by the underlying data and not by the adjustment procedure.

Table B1: Effects on Votes for Incumbent and Other Electoral Outcomes

	Votes for Incumbent Mayor			Votes for Challenger Candidates	Turnout	Blank and Null Votes
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Adjusted Regressors						
Number of Treated Individuals	-0.219**	-0.190**	-0.210**	0.254*	0.008	-0.035
Non-parametric adjustment and wild cluster bootstrap p-value	[0.040]	[0.034]	[0.030]	[0.072]	[0.962]	[0.610]
Wild cluster bootstrap p-value	[0.014]	[0.022]	[0.030]	[0.010]	[0.848]	[0.592]
Number of Control Individuals	0.074	0.096**	0.075	0.044	0.086	-0.033
Non-parametric adjustment and wild cluster bootstrap p-value	[0.104]	[0.016]	[0.106]	[0.584]	[0.450]	[0.480]
Wild cluster bootstrap p-value	[0.190]	[0.222]	[0.170]	[0.588]	[0.672]	[0.334]
Panel B: Undjusted Regressors						
Number of Treated Individuals	-1.062**	-1.298**	-0.995**	0.775**	-0.223	-0.003
Wild cluster bootstrap p-value	[0.014]	[0.012]	[0.032]	[0.020]	[0.572]	[0.976]
Number of Control Individuals	0.361	0.152	0.391	-0.310	0.169	0.088
Wild cluster bootstrap p-value	[0.360]	[0.900]	[0.328]	[0.556]	[0.668]	[0.658]
Total Eligible Voters (2012)	Yes	No	No	No	No	No
Total Eligible Voters (2008)	No	Yes	Yes	Yes	Yes	Yes
Change in Eligible Voters (2008-2012)	No	No	Yes	Yes	Yes	Yes
Location Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	909	909	909	909	909	909

Notes: p-values using wild clustered boostrap in brackets. The estimation sample consists of 21 municipalities in which the incumbent mayor was running for reelection in 2012. We allow standard errors to be correlated within municipalities. * 10%, ** 5%, *** 1% significance levels.

Table B2: Effects on Votes for Incumbent and Other Electoral Outcomes

	Votes for Incumbent Mayor			Votes for Challenger Candidates	Turnout	Blank and Null Votes
	(1)	(2)	(3)	(4)	(5)	(6)
Number of Treated Individuals who are Frequent Interactors	-0.396	-0.325	-0.361	0.195	-0.058	0.108
	[0.246]	[0.318]	[0.282]	[0.412]	[0.740]	[0.390]
Number of Treated Individuals who are Infrequent Interactors	-0.082	-0.092	-0.080	0.072	-0.033	-0.026
	[0.402]	[0.410]	[0.410]	[0.330]	[0.710]	[0.218]
Number of Control Individuals who are Frequent Interactors	0.070	0.076	0.085	0.474	0.563	0.005
	[0.402]	[0.960]	[0.936]	[0.304]	[0.194]	[0.776]
Number of Control Individuals who are Infrequent Interactors	0.105	0.013	0.106	-0.154	-0.035	0.013
	[0.934]	[0.846]	[0.276]	[0.342]	[0.814]	[0.882]
Total Eligible Voters (2012)	Yes	No	No	No	No	No
Total Eligible Voters (2008)	No	Yes	Yes	Yes	Yes	Yes
Change in Eligible Voters (2008-2012)	No	No	Yes	Yes	Yes	Yes
Location Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	909	909	909	909	909	909

Notes: p-values using wild clustered boostrap in brackets. The estimation sample consists of 21 municipalities in which the incumbent mayor was running for reelection in 2012. We allow standard errors to be correlated within municipalities. * 10%, ** 5%, *** 1% significance levels.