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VULNERABILITY AND CLIENTELISM

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ABSTRACT

This study argues that economic vulnerability causes citizens to participate in clientelism, a phenomenon with various pernicious consequences. We employ a randomized control trial that reduced household vulnerability through a development intervention: constructing residential water cisterns in drought-prone areas of Brazil. This intervention significantly decreased requests for private goods from politicians, especially among citizens likely to be in clientelist relationships. We also link program beneficiaries to electronic voting machines, and show the intervention decreased votes for incumbent mayors, who typically have more resources for clientelism. Findings are observed not only during the 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, 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, a large literature argues 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]

Why would citizens participate in clientelism given such potential consequences? Of the many factors posited, perhaps none has garnered more attention than poverty. An extensive theoretical literature points to the decreasing marginal utility of income as an underlying reason why impoverished citizens may 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). But while poverty focuses on the *level* of income, the *uncertainty* of income also warrants close attention. As shown by Ligon and Schechter's (2003) theoretical work, economic vulnerability – defined as encompassing both the level and uncertainty of income – has important effects on citizens' well-being.

The pervasiveness of both vulnerability and clientelism across developing countries raises two important, unexplored questions. First, is there a causal link between

[1]See Piattoni (2001); Kitschelt and Wilkinson (2007); Baland and Robinson (2008); 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.

economic vulnerability and citizens' participation in ongoing clientelist relationships? 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 in Northeast Brazil, a drought-prone region where vulnerability is prevalent. We employ a large-scale randomized control trial that exogenously decreased vulnerability through a development intervention. This intervention, which we designed in collaboration with a Brazilian NGO, constructed private rainfed water cisterns for individual households. Each cistern captures up to 16,000 liters of rainfall from a house's rooftop that can be consumed for extended periods, thereby heightening the reliability of a household's water supply and its resiliency to droughts. Analyses reveal that cisterns lead to reduced vulnerability, as measured by several prominent well-being indicators of food insecurity, depression, and self-reported health status.

In addition to randomly assigning cisterns, we conducted a novel, longitudinal survey of a large representative sample of impoverished rural households facing water insecurity. This survey not only examines vulnerability, but also measures respondents' 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 establish a set of stylized facts about the relationship between vulnerability and clientelism. For example, our survey data show that residents of drought-stricken municipalities are more likely to ask local 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 political candidates,

a mechanism of clientelism that serves as a costly signal about how they will vote.[2] As explored in this study, such evidence sheds light on the nexus between vulnerability and clientelism, in which many vulnerable citizens rely on clientelist relationships with local politicians in order to cope with negative shocks.

A key finding of our study is that in households randomly selected to receive water cisterns, citizens are significantly less likely to participate in clientelism. The intervention reduces the likelihood that citizens ask local politicians for private benefits by 3.0 percentage points, a substantial decline of 17 percent. As expected, these effects are fully concentrated among citizens who are likely to be in ongoing clientelist relationships: their requests fall by 10.9 percentage points — a remarkable 38 percent reduction in proportional terms. By contrast, we find no effect among citizens without such relationships. Furthermore, we find that the intervention decreased requests for various types of goods, beyond water requests that are directly affected by cisterns. 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 the cisterns treatment decreases citizens' requests 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 these findings, we examine whether the intervention also renders citizens less likely to vote for incumbent politicians during their reelection campaigns. Facilitating this analysis is a feature of Brazil's electoral system that provides extraordinarily granular data on voting outcomes. Our survey links individual subjects in the cisterns experiment to their specific electronic voting machines in the 2012 municipal election.

[2]Electronic voting undermines clientelist politicians' ability to monitor vote choices in Brazil, so during election campaigns, many citizens mitigate this challenge by publicly declaring support for candidates with whom they have ongoing clientelist relationships (Nichter 2018). Declarations are costly signals in part because politicians can disfavor citizens who declared for defeated candidates, when distributing various post-election benefits including health care.

In order to measure electoral responses to the cistern treatment, we can compare votes across machines — which have distinct, randomly assigned numbers of treated individuals — located in the same polling places. The cisterns treatment is estimated to decrease a citizen's probability of voting for the incumbent mayor by 29.3 percentage points. While not dispositive, these results are consistent with the argument that water cisterns make citizens less beholden to incumbent politicians, in that they may be less reliant on clientelist relationships as a risk-coping mechanism.

The findings of this study offer several important contributions to the political economy literature. Previous observational studies show correlational evidence that citizens of 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 the phenomenon, such as voters' beliefs, attitudes and preferences.[4] Our study advances the literature by providing compelling evidence that reducing vulnerability decreases citizens' participation in clientelist exchanges. Second, 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. Third, 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. Fourth, an innovative feature of our approach is that it empha-

[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.

sizes 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), which develops and tests 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 the introduction of 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 citizens' access to public and private goods should crowd out citizens' participation in clientelism. The present study is the first to corroborate this prediction with compelling evidence.

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

2 Context and Intervention

2.1 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.[5] 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 this drought-prone region, many residents of rural areas are highly vulnerable to shocks.[6] Credit and insurance markets are underdeveloped, and savings constraints often prevent citizens from procuring sufficient self-insurance. Partially due to 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 vulnerable 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).

Given their substantial vulnerability to shocks, many Brazilians request assistance from local politicians. Our longitudinal data reveal that 21.3 percent of survey respondents asked for private goods from a mayoral or councilor candidate during the 2012 election year.[7] Moreover, 8.6 percent of respondents made such requests to those politicians during the following non-election year. As shown below, these demands increase amidst adverse shocks. While not all requests involve life necessities, most do — about

[5]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.

[6]The Institute for Applied Economic Research (Instituto de Pesquisa Econômica Aplicada, or Ipea) in Brazil classified most of the Northeast region as very vulnerable according to its 2015 Index of Social Vulnerability.

[7]Local elections occur simultaneously nationwide every four years, with state and federal elections following two years later. Mayors and city 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. Voting is compulsory in Brazil, with turnout in most elections around 80 percent.

a third of requests in both years involved health care, and another quarter involved water.

Local politicians have considerable discretion and resources to fulfill citizens' requests.[8] When responding to such requests, politicians frequently mete out assistance using political criteria, as demand often exceeds available resources. In rural Northeast Brazil, mayors and city councilors often favor citizens with whom they have ongoing clientelist relationships, in which material benefits are exchanged for political support (Nichter 2018). Political clientelism and vote buying are common in much of Brazil; for example, electoral courts have ousted over a thousand politicians since 2000 for distributing private goods to citizens during political campaigns (MCCE 2012). 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). 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.[9]

In line with the literature's general consensus that clientelism tends to favor incumbents, our survey data suggest that politicians in office are more likely to engage in re-

[8] As in most Latin American countries, the provision of many public services has been decentralized to the local level (Garman, Haggard, and Willis 2001), and Brazil's government expenditures are among the most decentralized in the world (IMF 2016). Most municipalities rely 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).

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

quest fulfilling. 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 from non-incumbent politicians. During the 2012 municipal 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 2013 post-election year, 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.

2.2 Water Cisterns Intervention

In this study we employ a prospective randomized control design, in which the intervention provided rainfed water cisterns to randomly selected households. Each water cistern consists 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 2, each cistern is attached to a gutter and tube system that collects rainfall from the home's roof. The cistern is partially buried, with a manual pump on top and a small metal door providing internal access for cleaning and maintenance. While the cistern is designed to collect rainfall from a home's roof, a household can also buy water from water trucks and store it in the cistern, providing insulation from droughts. Thus, the cistern not only serves as a reliable technology for collecting rainfall, but also provides a storage device.

Cisterns cost approximately US$ 1,000 (R$ 1,500 in 2010) each to construct, and were awarded free of charge to eligible households. We show below that the intervention reduced vulnerability, but did not significantly change household wealth. Cisterns were

developed by our NGO partner *Articulação no Semi-Arido Brasileiro* (ASA, or Brazilian Semi-Arid Articulation)[10] 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, we randomized the construction of cisterns by ASA in specified municipalities, beginning in January 2012.

Since cisterns had been constructed by ASA in the region for nearly a decade, the intervention was 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, most cisterns in wealthier households had been self-built, whereas most cisterns in poorer households had been received from ASA. The cisterns we randomly assigned 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 the allocation of cisterns and does not indicate to beneficiaries that the government was in any way responsible for their receipt of cisterns.

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)

[10] See www.asabrasil.org.br.

they had physical space on their property to build a cistern, and (c) their roofs were at least 40 m^2 and composed of metal sheeting or tile (to facilitate rainfall collection).

The sample selection of households involved two steps. First, 40 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 rural households (i.e., *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 neighborhood clusters, we imposed a restriction that clusters be located at least two kilometers away from each other. Our surveys were conducted in 425 neighborhood clusters in 40 municipalities, located in all nine states of the semi-arid region. Our study population and interview sample comprise all households from the ASA cisterns program in these 40 municipalities throughout our research period. Whereas the average population of municipalities in our sample was 49,000 citizens, the average number of water cisterns built in each municipality by our intervention was just 17. Given the minute share of treated households in each municipality, it is unlikely that local politicians would shift their strategies in response to our provision of cisterns; we provide corroborating evidence below. So long as this condition holds, our estimates identify changes in citizens' behavior, rather than a combination of changes in citizens' behavior and changes in politicians' strategies.

3.2 Household surveys

We conducted a face-to-face panel survey spanning nearly three years, as shown in the timeline in Figure 3. 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 Voting data

In order to analyze whether reduced vulnerability affects incumbents' electoral performance, 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 in the same polling location. 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).[11] 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. 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.[12] 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.

To examine incumbents' electoral performance, we focus on voting outcomes in municipalities where the incumbent mayor ran for reelection. Our estimation sample is composed of 909 voting machines in polling locations where at least one survey participant was matched to any voting machine. Given that ballot secrecy requires us to use aggregate vote counts at the voting-machine level, this geographic breadth of survey respondents across so many voting machines facilitates our estimation of treatment effects by increasing statistical power. These 909 voting machines were located in 189 polling locations (primarily schools), corresponding to a mean of 4.8 machines per lo-

[11] 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.

[12] 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.

cation. On average, each machine in our sample had 334 registered voters in 2012, 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).[13]

3.4 Rainfall

To confirm the vulnerability of our household sample to droughts, we also 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.[14] 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 as the difference between the current period's rainfall and the historical (1986-2011) mean of rainfall in the municipality during identical months, divided by the municipality's historical monthly standard deviation of rainfall.[15]

4 Empirical Methodology

4.1 Research Design

Our sample consists of 1,308 households located across 425 neighborhood clusters. Randomization was performed across these neighborhood clusters (known as *logradouros* in Brazil) within municipalities. More specifically, in October 2011 clus-

[13]Further descriptive statistics for the sample are provided in Appendix Table B1.

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

[15]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 the average of historical rainfall in municipality i in period m, and σ_i is the historical standard deviation of rainfall in municipality i. The historical rainfall data cover the period 1986-2011. We then standardize this measure to have mean zero and variance equal to one in the estimating sample (following Hidalgo et al 2010). Results are robust to alternative rainfall measures, including the raw level of rainfall.

ters were stratified by municipality and randomly allocated into treatment and control arms. Households within neighborhood clusters often share water resources; thus, to avoid treatment spillovers across households, all participating households in clusters selected for treatment were assigned to receive their own individual cisterns. All participating households in clusters assigned to the control group were designated to receive nothing from our intervention throughout the study. We allocated 615 households in 189 clusters to the treatment group and 693 households in 236 clusters to the control group. The reason for the modestly larger control group was the possibility that other cistern-building entities in Northeast Brazil might provide cisterns to some control households. 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-December 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-December 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; this budget expansion led other contractors to ramp up cistern construction. It deserves emphasis that this differential take-up rate between treatment and control groups enables us to identify causal effects on our key outcome variables. Our experiment is well-powered; for example, power calculations reveal that in analy-

ses of citizen requests, it can detect a 2.5 percentage point effect at the 5% significance level.

Following the usual approach in experimental studies, we address imperfect compliance 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 LATE estimates from instrumental variable models in the appendix as detailed below.

4.1.1 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 test implies that our randomization was successful at achieving statistically similar treatment and control groups at baseline.

4.1.2 Attrition

We observe a low level of household attrition across survey rounds. Appendix 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. Overall, we find that the correlation between attrition and treatment is small, negative, and statistically indistinguishable from zero (p-value = 0.64).

4.2 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 main specification below.

4.2.1 Requests for private goods

We first estimate the overall effects of the cistern treatment on individuals' requests for private goods from local politicians. We do so by estimating equation (1), where the dependent variable is a dummy indicating whether individual i in household h, cluster c, and municipality m requested private goods from a politician during the 2012 municipal election year or during the 2013 post-election year. Our main specifications pool the data from both survey rounds and include survey wave fixed effects (γ_t). Municipal fixed effects (α_m) are also included because treatment assignment was stratified by municipality (neighborhood clusters were randomly assigned to treatment within each municipality). D_{cm} is a dummy indicating whether cluster c in municipality m was assigned to treatment. The coefficient of interest is β_1:

$$y_{ihcmt} = \alpha_m + \gamma_t + \beta_1 \cdot D_{cm} + \epsilon_{ihcmt}. \tag{1}$$

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_{ihcmt} by using clustered standard errors at the neighborhood cluster level cm.

To test the hypothesis that the cisterns intervention reduces requests for private goods by citizens in clientelist relationships, we also estimate:

$$y_{ihcmt} = \alpha_m + \gamma_t + \beta_1 \cdot D_{cm} + \beta_2 \cdot R_{ihcm} + \beta_3 \cdot D_{cm} \cdot R_{ihcm} + \epsilon_{ihcmt}, \quad (2)$$

where R_{ihcm} is an indicator for the individual being in a clientelist relationship (discussed below), and the other variables remain as previously defined. With regards to equations (1) and (2), we also report results separately by survey wave, thereby distinguishing between requests made during 2012 and 2013. These specifications provide evidence about treatment effects distinguishing between electoral and non-electoral periods.

The extant literature lacks a well-established marker for whether citizens are involved in ongoing clientelist relationships, so we tackle this issue by employing two alternative measures. Our primary marker is whether the respondent conversed at least monthly with a local politician before the 2012 electoral campaign began. This measure, which is balanced across treatment and control groups (see Figure 4), builds on the intuition that such frequent interactions facilitate the face-to-face exchanges between citizens and elites that are a hallmark of ongoing clientelist relationships.[16] Of course, even though substantial fieldwork in Northeast Brazil (Nichter 2018) – as well as our own piloting – suggests that these frequent interactions typically reflect clientelist relationships, some citizens may converse regularly with local politicians for other reasons. Thus, we also show robustness to a more restrictive marker: having *both* conversed with a politician at least monthly before the 2012 electoral campaign *and* having publicly declared support for a candidate during that campaign. As discussed below, many rural Brazilians involved in contingent exchanges declare support on their homes and bodies in order to send a costly signal about how they will vote.

[16] All analyses are robust to using alternative markers of clientelism employing different frequencies of conversations with politicians.

Within our sample, 18.4 percent of survey respondents have our primary marker of being in an ongoing clientelist relationship, while 12.0 percent have the more restrictive secondary marker. We prefer our primary marker because conversations with politicians are uncorrelated with adverse shocks; by contrast, declared support increases amidst shocks such as drought (see below). As discussed in Section 5, estimates and significance of results are robust when using either marker of citizens' involvement in clientelist relationships. In the appendix, Table A4 shows that neither marker is correlated with the cisterns treatment,[17] and Appendix Tables A5 and A6 show that they do not merely serve as a proxy for economic vulnerability or other important characteristics. For example, citizens with these markers are no more (or less) vulnerable – using any of the measures discussed below – and their expenditures and wealth per capita are statistically indistinguishable from other citizens.[18]

4.2.2 Electoral outcomes

To test the hypothesis that the cisterns intervention undermines the electoral performance of incumbent mayors during their reelection campaigns, we examine the most granular data released by Brazil's electoral authorities for the 2012 municipal election. As described above, we are able to link survey respondents to the specific electronic voting machines to which they are assigned by electoral authorities. Given that voting data are publicly available at the voting machine level, we estimate:

$$y_{slm} = \alpha_{lm} + \gamma_1 \cdot TV_{slm} + \gamma_2 \cdot EV_{slm} + \gamma_3 \cdot RV_{slm} + \epsilon_{slm}, \tag{3}$$

where y_{slm} is the number of votes for the incumbent mayor in electronic voting machine (i.e., "electoral section") s, in voting location l, in municipality m. The regressor

[17] We also asked survey respondents about conversations with politicians during the 2013 non-election year. We find no effect of the cisterns treatment on these conversations, on average and even among the subset of citizens who frequently conversed with politicians before the 2012 campaign.

[18] As shown in Appendix Tables A5 and A6, citizens with these markers are more likely to vote, have all household members voting for the same candidate, and/or have campaign visits to their homes. Such behaviors are consistent with being in a clientelist relationship.

of interest is TV_{slm}, the number of treated individuals in our study assigned by electoral authorities to vote in that particular machine. Other controls in the regression are EV_{slm}, the overall number of individuals in our study assigned to that machine; α_{lm}, a voting location fixed effect to control for differential voting patterns across voting locations in a municipality; and RV_{slm}, the total number of registered voters assigned to that machine (regardless of whether they are in our study sample) to control for any possible systematic relationship between citizens' electoral behavior and the size of voting machines. 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. Furthermore, within a given polling place, citizens are assigned to a specific voting machine by electoral authorities.[19] Therefore, once we condition on the total number of individuals in the study registered to vote in the machine, we can identify γ_1 – the effect of an additional person assigned to the cisterns treatment on votes for the incumbent mayor.[20]

The analysis in Equation (3) compares electoral outcomes across voting machines within each polling location, so we test for balance in survey participant characteristics across machines within locations. Such testing provides assurance that differences in electoral outcomes correlated with the cisterns treatment are not due to the composition of voters across machines. Appendix Table B2 shows specifications in which mean survey participant characteristics (age, years of education, gender, and marker of clientelism) for a given voting machine are regressed on the number of treated individuals assigned to that machine by electoral authorities. Reassuringly, coefficients are small and statistically insignificant, suggesting that results do not stem from the composition of the electorate.

[19] Our identification strategy is robust to any influence citizens may have regarding their polling place.

[20] Additional specifications are employed to show robustness. Some employ a more recent measure of registered voters per machine: from 2012, instead of from the prior municipal election in 2008. Other specifications include an additional control variable — the change in registered 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).

Section 5.5 shows that the cisterns treatment significantly reduces votes for the incumbent mayor, without conducting any adjustments. However, further consideration is needed because specifications about electoral outcomes (but not about requests) involve aggregate data: TV_{slm} and EV_{slm} sum how many treatment and overall study participants are assigned by their voter identification cards to vote in a particular machine in a given polling location. Accurately measuring treatment effects on electoral outcomes with these aggregate data requires attention to three potential issues: (a) treatment effects on voting by non-interviewed members of treated households (as registered voters in sampled households were only interviewed if present during our home visits); (b) spillover effects on neighbors' voting behavior (e.g., due to sharing water with ineligible, neighboring households); and (c) peer effects on neighbors' voting behavior.[21] Failing to address the possible undercounting of other treated household members, as well as positive spillover and peer effects, could bias upward our estimates of treatment effects (in absolute terms). Therefore, we adjust TV_{slm} and EV_{slm} to incorporate estimates of: (a) how many non-interviewed individuals live in sampled households, (b) how many live in other households in the neighborhood cluster without a cistern at baseline (i.e., those potentially affected by spillover or peer effects of the cisterns treatment), and (c) the probabilities that these individuals are assigned by their voter registration cards to vote in the same locations and same voting machines as our interviewees. This procedure improves estimation of the magnitude of treatment effects on electoral outcomes; as mentioned, the statistical significance of findings is also robust without any such adjustments.

To conduct appropriate inference about electoral outcomes, two points require consideration. First, sampling error of the adjusted regressors must be taken into account. And second, because we allow the errors to be correlated across voting machines and lo-

[21] Studies such as Nickerson (2008), Giné and Manzuri (2018), and Fafchamps, Vaz, and Vicente (in press) find evidence of substantial positive peer effects on electoral behavior in voter education campaigns in the U.S., Pakistan, and Mozambique, respectively.

cations within a municipality, our sample is composed of 21 "clusters" – that is, municipalities in which the mayor is running for reelection. To address both points, we report p-values from a wild cluster bootstrap-t procedure. This procedure not only addresses the fact that we have a limited number of clusters (Cameron, Gelbach, and Miller 2008), but also takes into account sampling error of the adjusted regressors through bootstrapped sampling of the data used to construct estimates (Horowitz 2001). Appendix B provides a detailed description of all procedures used to construct adjusted regressors and estimate p-values.

Examining electoral outcomes at the voting-machine level provides an extraordinarily granular level of official data. However, given ballot secrecy, it is only possible to obtain a single observation per machine (i.e., total votes cast for the incumbent). This aggregation limits power in regressions of electoral outcomes compared to those of citizen requests described above. Despite this limitation, we not only examine average effects for individuals assigned to the cisterns treatment using our primary specification in Equation (3), but also analyze heterogeneity across our markers for citizens likely to be in clientelist relationships. As expected, these specifications further deteriorate the signal-to-noise ratio; for example, less than a fifth of citizens conversed frequently with politicians, and less than a tenth of voting machines have any treated citizens who meet this criterion.

5 Results

This section first examines the vulnerability and political interactions of study participants, and then implements the empirical strategy described above to investigate the cistern intervention's effects on citizens' participation in clientelism and on electoral outcomes.

5.1 Vulnerability

In order to document the vulnerability of study participants, Table 1 reports means of a set of well-being indicators and their relationship with rainfall shocks. More specifically, it reports bivariate regression coefficients of each indicator against the rainfall shock measure defined in Section 3.4. Given that the latter is calculated at the municipal level, the identification of coefficients in this descriptive exercise stems from cross-municipality variation in rainfall shocks. If rural households in our sample were not vulnerable to rainfall shocks – for example, if they could simply self-insure against droughts, or if the state provides effective social insurance – then we would expect no correlation between precipitation and each well-being indicator. But much to the contrary, bivariate regression coefficients in Table 1 suggest that citizens exhibit substantial vulnerability.

More specifically, the first 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 0.04 units, or about 0.07 standard deviations of the depression scale. The second measure is the Self-Reported Health Status (SRHS) index, which indicates how healthy respondents believed they were (higher values indicate better self-reported health). In this case, a one standard deviation decrease in rainfall decreases self-reported health on this four-point scale by 0.04 units, or about 0.08 standard deviations on the SHRS scale. The third 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). A one

standard deviation decrease in rainfall worsens children's food security by 0.06 units or about 0.07 standard deviations of child food security.

Also indicative of citizens' vulnerability to water shortages in this rural setting, negative rainfall shocks are associated with lower 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.[22] Overall, this evidence suggests that the rural Brazilians in our sample are vulnerable to rainfall shocks.

5.2 Effects of Cisterns Treatment on Vulnerability

An important first step in our analysis is to confirm that the cisterns treatment reduced vulnerability. Using measures of vulnerability collected at the household level, we estimate:

$$y_{hcm} = \alpha_m + \beta_1 \cdot D_{cm} + \epsilon_{hcm}, \tag{4}$$

where y_{hcm} is a vulnerability indicator for household h in cluster c, in municipality m. D_{cm} is a dummy indicating whether the cluster was assigned to treatment, and α_m is a municipal fixed effect. Table 2 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 0.08 units among treated households (significant at the .05 level), representing 0.14 standard deviations on the SRHS scale. In column 3, the Child Food

[22] These figures are in 2011 Brazilian Reais.

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 there is a substantial 0.13 standard deviation reduction in vulnerability caused by the cistern (significant at the .01 level; column 4). By contrast, Appendix Table A7 shows that the cistern treatment has no significant effect on wealth as measured by the value of owned durable goods, livestock, property and net liquid savings. Overall, this analysis confirms that the cisterns program reduced the vulnerability of study participants, but had no discernible effect on wealth.[23]

5.3 Vulnerability and Clientelism

Thus far, we have established the vulnerability of study participants, as well as its exogeneous decrease through our cisterns intervention. To set up our primary experimental analyses, we now provide descriptive evidence about clientelism and its link to citizens' vulnerability. Our underlying premise is that many citizens in ongoing clientelist relationships request help from local politicians in order to cope with their vulnerability.

First, Table 3 provides contextual information about citizens' interactions with politicians. Recall that our primary marker of clientelism is whether a respondent had at least monthly conversations with a local politician before the 2012 campaign began. While these frequent interactions do not definitively identify contingent exchanges, citizens exhibiting such behavior are more likely to be in clientelist relationships than those who do not. As shown, 18.4 percent of survey respondents have this marker, and citizens do not appear to form these relationships in response to negative rainfall shocks. The bivariate regression coefficients in the right column reveal no significant association between this clientelism marker and rainfall shocks.

[23] Unlike the wealth measures in Table A7, the expenditure data in Table 1 were only collected in the 2011 localization survey.

Citizens tend to interact most often with candidates for city council, and these relationships might also be expected to yield political support for a 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 exposed to negative rainfall shocks are more likely to indicate that all household members vote for the same mayoral candidate. During local electoral 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. 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 mentioned above, declared support is a key mechanism of clientelism, by which citizens transmit a costly signal that they will vote for a particular candidate. Nearly half of our survey respondents engaged in at least one form of declared support during the campaign, either on their bodies, on their homes, or at rallies. Furthermore, Table 3 reveals that citizens are more likely to engage in each form of declared support when they experience negative rainfall shocks. More specifically, a one standard deviation fall in rainfall increases the prevalence of declarations by 7.1 percentage points ($p < 0.001$). This observation is consistent with our broader argument about the link between vulnerability and citizens' participation in clientelism.

In order to cope with vulnerability, citizens often turn to politicians for assistance. As shown in Table 4, during the 2012 election year, 21.3 percent of survey respondents asked for private benefits from a mayoral or councilor candidate, and 8.6 percent made requests of those politicians during the 2013 non-election year. The composition of de-

mands during both years reveals that many requests are motivated by vital needs such as medicine, medical treatments, and water. Just as analyses in Section 5.1 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. Nearly 60 percent of this increase in requests involves water (2.3 percentage points), and nearly 30 percent involves medicine or medical treatments (1.1 percentage points). Table 4 also shows that politicians fulfill approximately half of such requests, and are more responsive to demands for water and health care than to demands for construction materials. When asked about hypothetical scenarios involving urgent needs, about a quarter of all respondents in 2012 indicated they would first turn to a local politician for help, even before family, friends and municipal offices. During our fieldwork, numerous local politicians explained that councilor candidates often serve as brokers for allied mayoral candidates, and thus fulfill requests on their behalf.[24]

If such patterns are orthogonal to citizens' relationships with politicians, they may merely reflect constituency service based exclusively on need, rather than clientelism. But citizens with either marker of clientelism described in Section 4.2.1 are disproportionately likely to ask for private goods and to have fulfilled requests. First, consider our primary clientelism marker among respondents in the control group. Approximately 28.5 percent of citizens with our primary marker requested private benefits from politicians in 2012 or 2013, versus only 15.3 percent of those without the marker, a difference of 13.2 percentage points (standard error = 0.028). In addition, citizens with the marker were more than twice as likely as those without the marker to receive benefits by request (16.7 vs. 7.6 percent) in either year, a difference of 9.0 percentage points (standard error = 0.021). Similar patterns are observed when employing our more restrictive marker of clientelism – having at least monthly conversations with a politician before the 2012

[24]Beyond Brazil, evidence suggests that city councilors also serve as brokers in Argentina and the Philippines (Stokes et al. 2013, Ravanilla, Haim and Hicken 2017).

electoral campaign *and* having declared support publicly for a candidate during that campaign.[25]

5.4 Effects of Cistern Treatment on Requests

Turning to our primary experimental analyses, we now explore whether cisterns reduced citizens' requests for assistance from politicians, and inquire whether this effect was concentrated among citizens who are likely to be in ongoing clientelist relationships. Following the empirical strategy in Section 4.2, Table 5 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, employs as its dependent variable a dummy for whether the respondent requested any private good from a politician. It shows that the intervention reduced the likelihood that citizens requested such benefits by 3.0 percentage points (17 percent of the control group mean; significant at the 5 percent level). Most strikingly, column 2 shows that these effects are fully concentrated among citizens with our primary marker of being in a clientelist relationship — having at least monthly conversations with a politician before the 2012 electoral campaign began. Among such citizens, we estimate a 10.9 percentage point (38 percent) reduction in requests (significant at the 1 percent level). By contrast, among respondents without this marker, we estimate an insignificant 1.2 percentage point reduction in requests (column 2). Columns 3 and 4 of Table 5 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 significant at the 10 percent level in 2012, and significant at the 5 percent level in 2013. Importantly, we cannot reject the hypothesis that the effects for both years are equivalent. Across citizens likely to be in clientelist relationships, the treatment effect is significant at the 1 percent level and remarkably similar during both years (10.2 and

[25]Citizens with this more restrictive marker were significantly more likely to request private goods (28.1 vs. 16.4 percent, $p < 0.001$) and have fulfilled requests (16.9 vs. 8.3 percent, $p < 0.001$), when compared to citizens without the marker.

11.8 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.[26]

In Table 5, columns 5-8 show analogous regressions ignoring requests for water, which are expected to be directly affected by the cistern treatment. Results are broadly similar in significance, though estimated coefficients are mechanically smaller given the exclusion of water requests. Appendix Table A9 disaggregates findings by the type of good requested, revealing that 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).

In order to heighten comparability with analyses of electoral outcomes in Section 5.5, we also estimate the aforementioned models using only the subsample of municipalities in which the incumbent mayor runs for reelection (Table 6). We find that citizen requests fall by 4.4 percentage points across both waves (significant at the 1 percent level). As above, coefficients are of similar magnitude when estimated separately by year (column 3), and effects are substantial and concentrated among the subsample of citizens with markers of being in ongoing clientelist relationships (columns 2 and 4).[27]

Another important line of inquiry involves the cistern treatment's impact on the fulfillment of requests. Appendix Table A12 employs as the dependent variable a dummy coded 1 if the individual requested a private good from a local politician *and* that good was received. Individuals with our primary marker of being in a clientelist relationship

[26] Appendix Table A8 also shows that results are robust to the inclusion of basic individual characteristics such as age, gender and education, as well as controlling for the only variable unbalanced at baseline (whether neighbors had a cistern).

[27] 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 A10 and A11.

are 6.1 percentage points less likely to ask and receive any private good (significant at the 5 percent level). When estimating separately by year, the corresponding coefficients are significant at the 5 percent level for 2012 and 10 percent level for 2013 (column 4). Again, treatment effects are similar when exclusively examining municipalities with incumbent mayors running for reelection (Appendix Table A13).[28]

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 7 presents estimates that employ requests for public goods as the outcome variable. We do not find evidence of a substitution of requests towards public goods. The estimated coefficients are very small and cannot be distinguished from zero.[29]

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. As shown in Appendix Tables A17 - A21, all findings in this section are robust when employing our more restrictive marker for clientelism – having at least

[28] For completeness, Table A14 reports results on request fulfillment by type of good requested that was actually received. In addition, given that some members of the control group ended up obtaining cisterns from other sources, we investigated whether there are heterogeneous effects based on the cross-municipality degree of non-compliance in the control group. Appendix Table A16 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. As expected, negative effects are no greater in municipalities with above median non-compliance.

[29] Results are similar in Appendix Table A15, in which the dependent variable is an indicator for having requested and received a public good.

monthly conversations with a politician before the 2012 electoral campaign *and* having declared support publicly for a candidate during that campaign.

5.5 Effects of Cistern Treatment 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 incumbents' greater access to resources to engage in clientelism, we now follow the empirical strategy in Section 4.2.2 to investigate whether the cisterns treatment undercut the performance of incumbent mayors during their reelection campaigns. Recall that Brazil provides extraordinarily granular data on electoral outcomes at the electronic voting machine level, and our survey links individual subjects in the cisterns experiment to their specific voting machine in the 2012 municipal election. 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.

Table 8 presents our main results estimating the effect of the cisterns intervention on incumbents' votes and other electoral outcomes. Panel A provides unadjusted estimates and reveals that the cisterns treatment significantly reduces votes for the incumbent mayor. Given the important considerations elaborated in Section 4.2.2, Panel B employs adjusted regressors to improve estimation of the magnitude of treatment effects. These adjusted regressors account for potential upward bias of estimated treatment effects resulting from voting by non-interviewed members of treated households, as well as from spillover and peer effects on voting by neighbors excluded from the study. The discussion of results below focuses on our preferred estimates in Panel B; as mentioned, Panel A shows that the statistical significance of findings is also robust without any such adjustments.

More specifically, columns 1 and 2 of Panel B report primary findings about the cisterns treatment's effects on electoral outcomes. First, column 1 reveals that for every

additional respondent assigned to the treatment condition, the incumbent receives 0.29 fewer votes (bootstrap p-value = 0.038).[30] By contrast, the overall number of individuals in our study assigned to the voting machine has no effect statistically distinguishable from zero; the coefficient on this control variable is positive and small in magnitude. These findings are consistent with our argument that reducing vulnerability through the cisterns intervention significantly reduces votes for the incumbent mayor.

Next, column 2 examines heterogeneity across our primary marker for citizens likely to be in clientelist relationships. While estimates are statistically indistinguishable from zero, patterns observed are consistent with our broader argument. The estimated coefficient on treated individuals with the clientelism marker is -0.47, over two times larger than the coefficient on treated individuals without the clientelism marker (-0.19).[31] To be sure, this specification is hindered by low power, given that it employs variation in the shares of citizens in clientelist relationships across voting machines. Nevertheless, the finding corroborates our overall argument, as the reduction in votes for incumbents appears to be primarily from effects among citizens in such relationships.[32]

We next investigate whether treatment effects, which suggest a fall in incumbent votes, translate to an increase in votes for mayors' challengers. Adapting our preferred specification (column 1 in Panel B), we examine as the dependent variable the total number of votes received by any challenger in the 2012 mayoral race. As shown in column 3, we estimate a coefficient of very close 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 0.20 (p-value = 0.138). We also report treatment effects on voter turnout (column 4), as well as blank and null votes (column 5). Although imprecisely estimated, our estimate on the treatment effect

[30] The coefficient in Panel A is larger but does not account, for example, for treatment effects on voting by non-interviewed members of treated households.

[31] Again, the coefficient in Panel A is larger but does not account for non-interviewed individuals plausibly affected by the cisterns treatment.

[32] Appendix Table A22 shows comparable results employing the alternative clientelism marker.

on voter turnout – a reduction of 0.09 citizens casting a ballot per additional respondent assigned to treatment – is consistent with the possibility that the intervention reduces turnout. In contrast, the effect on blank and null votes is both small and statistically indistinguishable from zero.

5.6 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.

5.6.1 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., Hicken 2011, Nichter 2008, Vicente 2014).

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. As mentioned, 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 9 examines whether any differences can be detected between politicians' actions towards citizens assigned to the treatment versus control groups. 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 citizens likely to be in clientelist relationships 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.

5.6.2 Citizen Engagement

Our main findings show that citizens likely to be in clientelist relationships with politicians were more responsive to the cisterns treatment. We employed frequent conversations with a local politician before the 2012 campaign as our primary marker for clientelistic relationships. As discussed in Section 4.2.1, this marker is 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, as discussed above, Appendix Tables A17 - A22 show that all findings are robust to a more restrictive marker of being in a clientelist relationship: having *both* conversed with a politician at least monthly outside of campaign season *and* having declared support for a candidate during the campaign. Given that declared support is a mechanism employed to sustain ongoing clientelist relationships in Brazil, citizens who meet both criteria are particularly likely to be in clientelist relationships. As a second approach, 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 A23 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 Table 5 (column 2). 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 2). These findings suggest that controlling for community engagement measures does not significantly change our results.

5.6.3 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 Trabalhadores*, or PT). As shown in Appendix Table A24, 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 (see Appendix Tables A25 and A26). 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.

5.6.4 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 3.4 and estimate a fully interacted triple differences specification. These results are shown in Table A27. Columns 1 and 4 include a *TreatmentXRainfall* control. Columns 2 and 5 add the triple interaction between treatment, rainfall and our primary clientelism marker, whereas columns 3 and 6 show the fully saturated model by also including the *RainfallXClientelist Relationship* 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 A27 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 A28, 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.[33] By serving as a water storage device, cisterns can thus reduce vulnerability even in the absence of rainfall.

6 Conclusion

This paper has investigated the relationship between economic vulnerability and 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 examines granular electronic voting machine outcomes, reveals that the cisterns treatment undercut the number of votes received by incumbent mayors during their

[33] 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.

reelection campaigns. Overall, these findings are consistent with 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 may be 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. Further research should explore whether reduced vulnerability leads citizens to abandon these ties altogether, as well as whether centrally mandated insurance mechanisms can therefore curb clientelism in developing countries. Another important avenue for research is how the scaling up of such policies might affect the behavior of both citizens and politicians in clientelist relationships.

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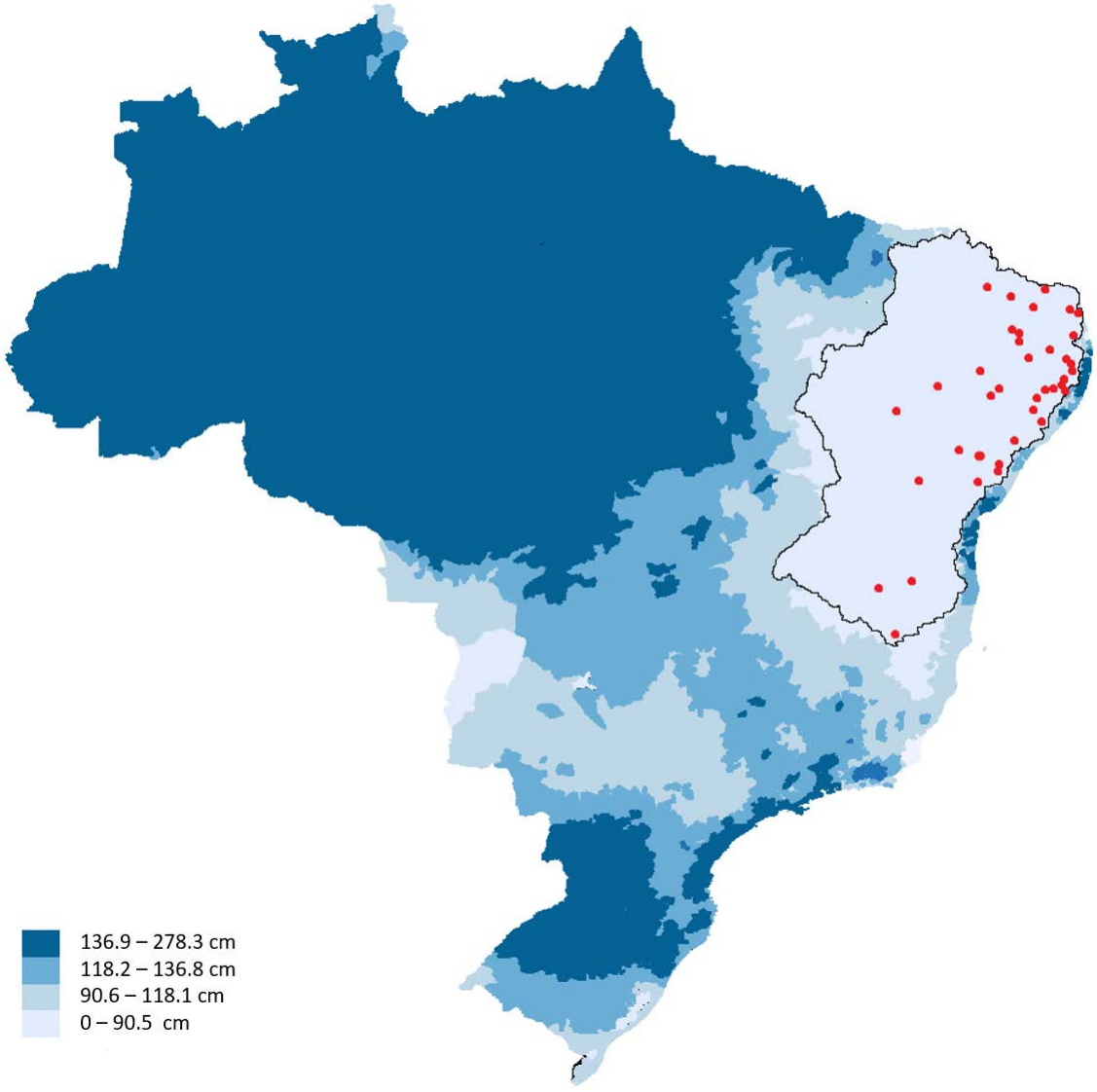
Figures and Tables

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: Cistern

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

Figure 3: Timeline

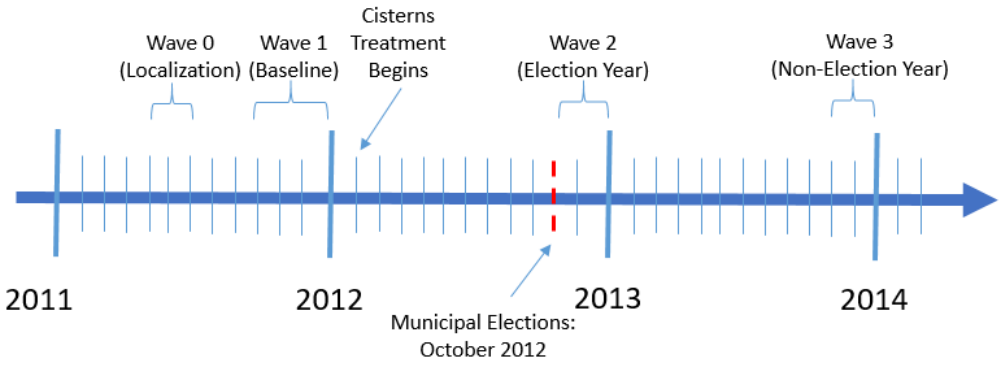


Figure 4: Frequency of Conversations before 2012 Election Campaign, by Treatment Status

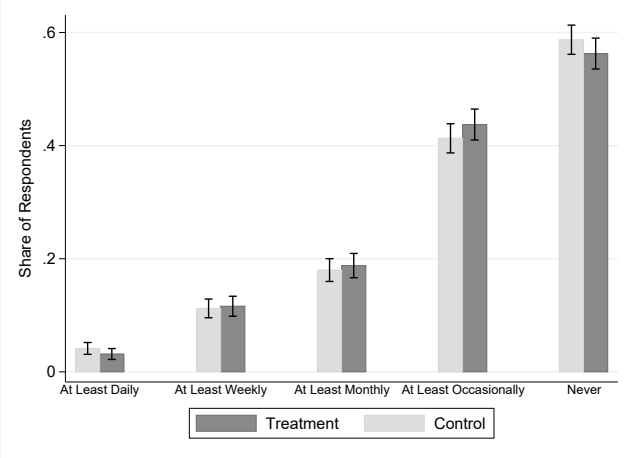


Table 1: Vulnerability and Rainfall Shocks

Variable	Mean	Relationship with Rainfall Shocks
-(CES-D Scale)	3.331	0.044***
	[0.642]	(0.016)
Self-Reported Health Status (SRHS) Index	2.828	0.039**
	[0.531]	(0.017)
Child Food Security Index	-0.309	0.060**
	[0.914]	(0.025)
Total Household Expenditure	367.845	24.398***
	[200.070]	(6.671)
Total Household Food Expenditure	239.146	13.331***
	[133.477]	(4.507)
Total Household Non-Food Expenditure	133.619	11.543***
	[130.256]	(3.694)

Notes: Column 1 presents the mean of each vulnerability measure and its standard deviation in brackets. Column 2 reports coefficients from regressing each vulnerability measure on standardized rainfall shocks, with standard errors clustered at the neighborhood level in parentheses. As defined in Section 3.4, 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 five 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. Sample sizes are 1128, 1052, 1128, 1281, 1299, and 1306, respectively, and vary due to item non-response. * 10%, ** 5%, *** 1% significance levels.

Table 2: Vulnerability and Assignment to Treatment (2013)

	-(CES-D Scale)	SRHS Index	Child Food Security Index	Overall Index
	(1)	(2)	(3)	(4)
Treatment	0.092**	0.075**	0.084	0.126***
	(0.037)	(0.033)	(0.054)	(0.043)
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Observations	1128	1052	1128	1128
Mean of Dependent Variable	3.331	2.828	-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 five items. The Self-Reported Health Status (SRHS) Index employs a scale of 1-4, in which higher values indicate better perceived health. 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 Overall Vulnerability Index is the unweighted mean of standardized values of all of the above indexes. * 10%, ** 5%, *** 1% significance levels.

Table 3: Interactions with Politicians (2012)

Variable	Mean	Relationship with Rainfall Shocks
Clientelist Relationship (Marker 1)	0.184	-0.011
	[0.387]	(0.009)
Clientelist Relationship (Marker 2)	0.120	-0.012
	[0.325]	(0.007)
Voted for mayor/councilor of same coalition	0.718	-0.015
	[0.450]	(0.014)
All household members voted 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)
Declared support on body (sticker, shirt)	0.185	-0.023**
	[0.388]	(0.010)
Declared support on house (flag, banner, wall painting)	0.387	-0.063***
	[0.487]	(0.017)
Declared support at rally (Attended and displayed paraphernalia)	0.218	-0.039***
	[0.413]	(0.011)

Notes: Column 1 presents the mean of each variable and its standard deviation in brackets. Column 2 reports coefficients from regressing each vulnerability measure on standardized rainfall shocks (as defined in Section 3.4). Markers for clientelist relationships are discussed in Section 4.2.1. Marker 1 is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign. Marker 2 is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign *and* publicly declared support for a candidate during that campaign. 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. Sample sizes are 2667, 2678, 1742, 2339, 2680, 2678, 2678, 2678, and 2678, respectively, and vary due to item non-response. Sample size for "voted for mayor/councilor of same coalition" is different since this question was asked in 2013. * 10%, ** 5%, *** 1% significance levels.

Table 4: Citizen Requests for Private Goods, and Relationship with Rainfall (2012 and 2013)

	Mean (2012) (1)	Mean (2013) (2)	Relationship with Rainfall Shocks (2012) (3)	Relationship with Rainfall Shocks (2013) (4)	P-Value from Pooling Test (5)
Panel A: Ask for private goods from any politician					
Any	0.213	0.086	-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)	
Medicine 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)	
Observations			2667	1943	
Panel B: Ask for and receive private goods from any politician					
Any	0.124	0.035	-0.023***	0.000	0.015
	[0.330]	[0.184]	(0.008)	(0.005)	
Water	0.034	0.004	-0.011***	-0.001	0.096
	[0.182]	[0.064]	(0.006)	(0.001)	
Medicine 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)	
Observations			2665	1943	

Notes: Columns 1-2 present the mean of each request for private goods measure; standard deviations are reported in brackets. Columns 3-4 report coefficients from regressing each request for private goods 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: Requests for Private Goods (2012 and 2013)

	All Requests				All but Water			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1: Treatment	-0.030**	-0.012			-0.026**	-0.016		
	(0.013)	(0.013)			(0.012)	(0.012)		
β_2: Clientelist Relationship		0.119***				0.082***		
		(0.026)				(0.025)		
β_3: Treatment X Clientelist Relationship		-0.097***				-0.057*		
		(0.034)				(0.032)		
β_4: Treatment X 2012			-0.029*	-0.012			-0.025	-0.016
			(0.017)	(0.018)			(0.016)	(0.017)
β_5: Treatment X 2013			-0.031**	-0.012			-0.027*	-0.016
			(0.016)	(0.015)			(0.014)	(0.013)
β_6: Clientelist Relationship X 2012				0.134***				0.094***
				(0.033)				(0.031)
β_7: Clientelist Relationship X 2013				0.092***				0.060**
				(0.031)				(0.029)
β_8: Clientelist Relationship X Treatment X 2012				-0.090**				-0.052
				(0.044)				(0.043)
β_9: Clientelist Relationship X Treatment X 2013				-0.106***				-0.063*
				(0.040)				(0.037)
$\beta_1 + \beta_3$		-0.109***				-0.073**		
		(0.032)				(0.031)		
$\beta_4 + \beta_8$				-0.102**				-0.068*
				(0.042)				(0.041)
$\beta_5 + \beta_9$				-0.118***				-0.079**
				(0.040)				(0.037)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4288	4288	4288	4288	4288	4288	4288	4288
Mean of Y : Overall		0.164		0.164		0.164		0.133
Mean of Y : Control Group		0.177		0.177		0.145		0.145
Mean of Y : Clientelist Relationship in Control Group		0.285		0.285		0.225		0.225

Notes: Outcome variable is coded 1 if respondent reported requesting a private good from a local politician in 2012 or 2013; 0 otherwise. Specifications employ pooled data to 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. Marker for clientelist relationship is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see discussion in Section 4.2.1 and robustness to alternative marker in appendix). Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table 6: Requests for Private Goods - Municipalities where Incumbent Ran for Re-Election (2012 and 2013)

	All Requests				All but Water			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1: Treatment	-0.044***	-0.023			-0.030**	-0.017		
	(0.016)	(0.016)			(0.015)	(0.015)		
β_2: Clientelist Relationship		0.142***				0.103***		
		(0.033)				(0.030)		
β_3: Treatment X Clientelist Relationship		-0.109**				-0.067*		
		(0.042)				(0.040)		
β_4: Treatment X 2012			-0.041*	-0.024			-0.026	-0.017
			(0.022)	(0.022)			(0.021)	(0.021)
β_5: Treatment X 2013			-0.048**	-0.021			-0.037**	-0.018
			(0.020)	(0.019)			(0.018)	(0.017)
β_6: Clientelist Relationship X 2012				0.158***				0.112***
				(0.040)				(0.039)
β_7: Clientelist Relationship X 2013				0.114***				0.087**
				(0.044)				(0.039)
β_8: Clientelist Relationship X Treatment X 2012				-0.086				-0.048
				(0.058)				(0.058)
β_9: Clientelist Relationship X Treatment X 2013				-0.143***				-0.097*
				(0.054)				(0.049)
$\beta_1 + \beta_3$		-0.131***				-0.085**		
		(0.040)				(0.039)		
$\beta_4 + \beta_8$				-0.110**				-0.065
				(0.055)				(0.056)
$\beta_5 + \beta_9$				-0.164***				-0.115**
				(0.053)				(0.048)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2193	2193	2193	2193	2193	2193	2193	2193
Mean of Y : Overall		0.148		0.148		0.148		0.119
Mean of Y : Control Group		0.166		0.166		0.166		0.132
Mean of Y : Clientelist Relationship in Control Group		0.294		0.294		0.294		0.225

Notes: Outcome variable is coded 1 if respondent reported requesting a private good from a local politician in 2012 or 2013; 0 otherwise. Specifications employ pooled data to 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. Marker for clientelist relationship is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see discussion in Section 4.2.1 and robustness to alternative marker in appendix). Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table 7: Requests for Public Goods (2012 and 2013)

	All Municipalities				Municipalities with Incumbent Mayors Running for Re-Election			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1: Treatment	-0.004	-0.003			-0.002	0.001		
	(0.005)	(0.005)			(0.006)	(0.006)		
β_2: Clientelist Relationship With Politician		0.032***				0.035**		
		(0.012)				(0.015)		
β_3: Treatment X Clientelist Relationship		-0.003				-0.012		
		(0.015)				(0.020)		
β_4: Treatment X 2012			-0.003	-0.006			0.001	-0.002
			(0.007)	(0.007)			(0.009)	(0.009)
β_5: Treatment X 2013			-0.005	0.000			-0.006	0.004
			(0.006)	(0.005)			(0.007)	(0.007)
β_6: Clientelist Relationship X 2012				0.030*				0.031
				(0.016)				(0.020)
β_7: Clientelist Relationship X 2013				0.034**				0.044*
				(0.017)				(0.025)
β_8: Clientelist Relationship X Treatment X 2012				0.013				0.013
				(0.022)				(0.029)
β_9: Clientelist Relationship X Treatment X 2013				-0.028				-0.053**
				(0.020)				(0.026)
$\beta_1 + \beta_3$		-0.006				-0.011		
		(0.015)				(0.019)		
$\beta_4 + \beta_8$				0.007				0.011
				(0.021)				(0.028)
$\beta_5 + \beta_9$				-0.028				-0.049
				(0.020)				(0.025)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4292	4292	4292	4292	2197	2197	2197	2197
Mean of Y : Overall		0.024		0.024		0.025		0.025
Mean of Y : Control Group		0.026		0.026		0.027		0.027
Mean of Y : Clientelist Relationship in Control Group		0.051		0.051		0.055		0.055

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. Marker for clientelist relationship is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see discussion in Section 4.2.1 and robustness to alternative marker in appendix). Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table 8: Votes for Incumbent and Other Electoral Outcomes (2012)

	Votes for Incumbent Mayor		Votes for Challenger Candidates	Turnout	Blank and Null Votes
	(1)	(2)	(3)	(4)	(5)
Panel A: Unadjusted Regressors:					
Treated Respondents	-1.423**		1.040	-0.442	-0.059
	[0.042]		[0.120]	[0.532]	[0.872]
Respondents	0.361		-0.348	0.127	0.114
	[0.360]		[0.478]	[0.812]	[0.546]
Treated Respondents with Clientelist Relationship		-2.674			
		[0.176]			
Treated Respondents without Clientelist Relationship		-1.081			
		[0.322]			
Respondents with Clientelist Relationship		0.552			
		[0.634]			
Respondents without Clientelist Relationship		0.289			
		[0.552]			
Panel B: Adjusted Regressors:					
Treated Respondents	-0.293**		0.202	-0.087	0.004
	[0.038]		[0.138]	[0.634]	[0.940]
Respondents	0.074		0.043	0.085	-0.032
	[0.104]		[0.594]	[0.450]	[0.488]
Treated Respondents with Clientelist Relationship		-0.467			
		[0.404]			
Treated Respondents without Clientelist Relationship		-0.187			
		[0.304]			
Respondents with Clientelist Relationship		0.070			
		[0.934]			
Respondents without Clientelist Relationship		0.105			
		[0.278]			
Registered Voters	Yes	Yes	Yes	Yes	Yes
Location Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	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. Marker for clientelist relationship is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see discussion in Section 4.2.1 and robustness to alternative marker in appendix). * 10%, ** 5%, *** 1% significance levels.

Table 9: Politician Responses (2012)

	Campaign Visits		Campaign Visit Handouts		Offered Handout		Received Handout	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1: Treatment	0.001	-0.006	0.009	0.019	-0.003	-0.009	-0.005	-0.008
	(0.022)	(0.025)	(0.012)	(0.012)	(0.015)	(0.016)	(0.010)	(0.010)
β_2: Clientelist Relationship		0.079***		0.077***		0.010		0.002
		(0.030)		(0.022)		(0.023)		(0.017)
β_3: Treatment X Clientelist Relationship		0.042		-0.053		0.041		0.019
		(0.042)		(0.033)		(0.035)		(0.025)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2680	2667	2308	2305	1634	1626	1624	1616
Mean of Y : Overall		0.699		0.069		0.077		0.033
Mean of Y : Control Group		0.694		0.067		0.077		0.036
Mean of Y : Clientelist Relationship in Control Group		0.782		0.126		0.076		0.035
P-Value of $\beta_1 + \beta_3$		0.342		0.256		0.347		0.626

Notes: Dependent variables are listed in column headers. Treatment is coded 1 if respondent belongs to a participating household in a neighborhood cluster selected for treatment; 0 otherwise. Marker for clientelist relationship is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see discussion in Section 4.2.1). Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

7 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 (1)	Control Group (2)	Difference (3)	Standard Error of Difference (4)	Observations (5)
Individual Characteristics					
Age	36.587	37.393	-0.345	(0.642)	2988
Female	0.518	0.535	-0.016	(0.011)	2990
Current Student	0.139	0.126	0.005	(0.013)	2972
Years of Education	5.903	5.728	0.006	(0.193)	2931
P-Value of Joint F-Test			0.601		
Household Characteristics					
Household Size	4.288	4.221	0.054	(0.119)	1308
Number of Total Neighbors	17.658	15.959	1.997	(1.377)	1283
Neighbor has Cistern	0.664	0.598	0.060***	(0.035)	1237
Bolsa Familia Amount Received	91.954	85.915	4.945	(4.327)	1290
Household Wealth Per Member	18,955.48	20,256.44	-1,187.8	(992.416)	1299
Household Expenditure Per Member	100.324	109.276	-7.745	(4.776)	1281
Age of Household Head	43.899	44.840	-0.555	(0.937)	1307
Household Head Education	5.734	5.830	-0.241	(0.250)	961
Household Head is Female	0.182	0.182	0.007	(0.019)	1308
Owns House	0.863	0.873	-0.016	(0.021)	1308
Number of Rooms in House	5.266	5.331	-0.082	(0.079)	1294
Has Access to Electricity	0.883	0.905	-0.018	(0.018)	1308
Migrated Recently	0.111	0.107	0.006	(0.017)	1303
Owns Land	0.483	0.465	-0.004	(0.030)	1307
Land Size	3.413	3.554	-0.218	(0.684)	1308
Children in Household 0-6 Months	0.047	0.058	-0.015	(0.013)	1308
Children in Household 6 Months - 5 Years	0.631	0.612	-0.001	(0.038)	1308
Household Members 5 Years - 64 Years	3.397	3.316	0.099	(0.112)	1308
Household Members Over 64 Years	0.213	0.235	-0.029	(0.028)	1308
Voted in 2008 Municipality Election	0.891	0.865	0.020	(0.019)	1290
P-Value of Joint F-Test			0.588		

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 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	-0.02			
Standard Error	(0.017)			

Table A4: Clientelism Markers and Assignment to Treatment

	Clientelist Relationship	Clientelist Relationship*
Treatment	-0.00231	0.00875
	(0.0169)	(0.0153)
Municipality Fixed Effects	Yes	Yes
Observations	2667	2666
Mean of Dependent Variable	0.184	0.120

Notes: Outcome variables are markers for clientelist relationships discussed in Section 4.2.1. In column 1, the primary marker (Clientelist Relationship) is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise. In column 2, the alternative marker (Clientelist Relationship*) is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign *and* publicly declared support for a candidate during that campaign; 0 otherwise. Treatment is coded 1 if respondent belongs to a participating household in a neighborhood cluster selected for treatment; 0 otherwise. Standard errors are clustered at the neighborhood level and reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A5: Characteristics of Respondents with Primary Marker of Clientelist Relationships (2011 and 2012)

Variable	Individuals with Primary Clientelism Marker (1)	Individuals without Primary Clientelism Marker (2)	Difference (3)
Individual Characteristics			
Age	37.445	37.377	0.208
			(0.856)
Years of Education	6.105	5.746	0.274
			(0.228)
Female	0.451	0.558	-0.114***
			(0.025)
Household Characteristics			
Household Wealth Per Member	5894.113	5641.094	175.033
			(387.676)
Household Expenditure Per Member	103.230	104.900	-0.640
			(4.752)
Household Head Education	5.882	5.688	0.059
			(0.279)
Household Head is Female	0.150	0.194	-0.063**
			(0.025)
Owns House	0.881	0.858	0.024
			(0.022)
Household Size	4.539	4.187	0.383***
			(0.136)
Household Vulnerability Indicators			
-(CES-D Scale)	0.043	-0.011	0.040
			(0.059)
SRHS Index	-0.000	-0.003	0.003
			(0.078)
Child Food Security Index	0.054	-0.027	0.035
			(0.057)
Overall Vulnerability Index	0.049	-0.003	0.034
			(0.041)
Political Activities			
Voted in 2008 Municipal Election	0.916	0.871	0.043**
			(0.019)
Voted for mayor/councilor of same coalition	0.732	0.719	0.006
			(0.033)
All household members voted for same mayoral candidate	0.819	0.761	0.051**
			(0.023)
Received visit from 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 survey repondents with versus without the primary clientelism marker. As discussed in Section 4.2.1, this marker is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise. 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 A6: Characteristics of Respondents with Alternative Marker of Clientelist Relationships (2011 and 2012)

Variable	Individuals with Alternative Clientelism Marker (1)	Individuals without Alternative Clientelism Marker (2)	Difference (3)
Individual Characteristics			
Age	36.956	37.476	-0.259
			(1.041)
Years of Education	6.122	5.755	0.302
			(0.269)
Female	0.430	0.554	-0.129***
			(0.030)
Household Characteristics			
Household Wealth Per Member	5984.550	5698.861	46.703
			(557.115)
Household Expenditure Per Member	105.880	105.460	2.741
			(5.725)
Household Head Education	6.046	5.738	0.125
			(0.340)
Household Head is Female	0.170	0.187	-0.020
			(0.029)
Owns House	0.878	0.861	0.016
			(0.027)
Household Size	4.557	4.209	0.343**
			(0.167)
Household Vulnerability Indicators			
-(CES-D Scale)	0.063	-0.005	0.031
			(0.073)
SRHS Index	0.046	-0.022	0.078
			(0.085)
Child Food Security Index	0.046	0.001	0.035
			(0.072)
Overall Vulnerability Index	0.064	0.006	0.048
			(0.050)
Political Activities			
Voted in 2008 Municipal Election	0.908	0.880	0.024
			(0.023)
Voted for mayor/councilor of same coalition	0.767	0.716	0.041
			(0.040)
All household members voted for same mayoral candidate	0.843	0.762	0.070***
			(0.024)
Received visit from any mayoral candidate	0.794	0.683	0.088***
			(0.027)
Any declared support	1.000	0.415	0.550***
			(0.020)

Notes: Columns 1-2 present the mean of each variable for survey repondents with versus without the alternative clientelism marker. As discussed in Section 4.2.1, this marker is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign *and* publicly declared support for a candidate during that campaign; 0 otherwise. 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 A7: Wealth and Assignment to Treatment (2013)

	Value of Durables	Value of Livestock	Value of Property	Net Savings	Total Wealth
	(1)	(2)	(3)	(4)	(5)
Treatment	-41.238	-110.374	-1012.764	261.782	-902.594
	(308.755)	(342.491)	(1120.759)	(195.161)	(1242.988)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	1128	1128	1128	1128	1128
Mean of Dependent Variable	3717.210	2137.855	25678.445	-684.039	30849.470

Notes: Each column reports the coefficient from regressing each wealth measure on treatment, with municipality fixed effects. Durables in column 1 defined as the sum of the estimated values of: cars, trucks, motorcycles, refrigerator, stove, washing machine, sewing machine, television, DVD player, cell phone, computer, and satellite television. Livestock in column 2 defined as the sum of the estimated values of: cows, steer, calves, horses, donkeys, female goats, male goats, young sheep, adult sheep, pigs, chickens, ducks, turkeys, geese, rabbits, fish, and other animals. Value of property in column 3 defined as the sum of the estimated values of residential property and agricultural plots. Net savings in column 4 defined as total liquid assets minus total loans, in which total liquid assets include liquid savings, cash, loaned money and seed stock. Total loans include loans from family, loan sharks, banks, stores, government, and other sources. Total wealth in column 5 is defined as the sum of the measures in columns 1-4. Standard errors are clustered at the neighborhood level and reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A8: Requests for Private Goods - Inclusion of Additional Controls (2012 and 2013)

	All Requests				All but Water			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1: Treatment	-0.030**	-0.013			-0.026**	-0.017		
	(0.012)	(0.013)			(0.012)	(0.012)		
β_2: Clientelist Relationship		0.125***				0.087***		
		(0.026)				(0.024)		
β_3: Treatment X Clientelist Relationship		-0.092***				-0.052		
		(0.034)				(0.032)		
β_4: Treatment X 2012			-0.028*	-0.013			-0.025	-0.016
			(0.017)	(0.018)			(0.016)	(0.017)
β_5: Treatment X 2013			-0.032**	-0.014			-0.028**	-0.018
			(0.016)	(0.015)			(0.014)	(0.013)
β_6: Clientelist Relationship X 2012				0.140***				0.099***
				(0.032)				(0.031)
β_7: Clientelist Relationship X 2013				0.098***				0.066**
				(0.031)				(0.028)
β_8: Clientelist Relationship X Treatment X 2012				-0.086*				-0.048
				(0.044)				(0.043)
β_9: Clientelist Relationship X Treatment X 2013				-0.100**				-0.057
				(0.040)				(0.037)
β_{10}: Neighbor Has Cistern	-0.000	-0.000	-0.000	-0.000	-0.000	-0.000*	-0.000	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
β_{11}: Female	0.047***	0.052***	0.047***	0.052***	0.045***	0.049***	0.045***	0.049***
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
β_{12}: Years of Schooling	-0.006***	-0.006***	-0.006***	-0.006***	-0.006***	-0.006***	-0.006***	-0.006***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
β_{13}: Age	-0.001*	-0.001*	-0.001*	-0.001*	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
$\beta_1 + \beta_3$		-0.105***				-0.069**		
		(0.032)				(0.031)		
$\beta_4 + \beta_8$				-0.099**				-0.065
				(0.041)				(0.040)
$\beta_5 + \beta_9$				-0.114***				-0.075**
				(0.040)				(0.037)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4288	4288	4288	4288	4288	4288	4288	4288
Mean of Y : Overall		0.164		0.164		0.164		0.133
Mean of Y : Control Group		0.177		0.177		0.145		0.145
Mean of Y : Clientelist Relationship in Control Group		0.285		0.285		0.225		0.225

Notes: Outcome variable is coded 1 if respondent reported requesting a private good from a local politician in 2012 or 2013; 0 otherwise. Specifications employ pooled data to 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. Marker for clientelist relationship is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see discussion in Section 4.2.1). Specifications show robustness of results to the inclusion of individual characteristics and controlling for the only variable unbalanced at baseline (whether neighbors had a cistern). Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A9: Requests for Private Goods, by Primary Type (2012 and 2013)

	All Municipalities			Municipalites with Incumbent Mayors Running for Re-Election		
	Water	Construction	Medical Treatment/Medicine	Water	Construction	Medical Treatment/Medicine
	(1)	(2)	(3)	(4)	(5)	(6)
β_1: Treatment	0.004	-0.001	-0.013	-0.006	0.001	-0.020**
	(0.006)	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)
β_2: Clientelist Relationship	0.042***	0.042**	0.012	0.043**	0.037*	0.036
	(0.013)	(0.017)	(0.017)	(0.017)	(0.020)	(0.023)
β_3: Treatment X Clientelist Relationship	-0.042***	-0.037*	-0.010	-0.033	-0.032	-0.017
	(0.016)	(0.021)	(0.021)	(0.021)	(0.026)	(0.029)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4292	4292	4292	2197	2197	2197
Mean of Dependent Variable	0.0380	0.0403	0.0531	0.0341	0.0355	0.0478
P-Value of $\beta_1 + \beta_3$	0.0133	0.0658	0.228	0.0619	0.210	0.160

Notes: Outcome variable is coded 1 if respondent reported requesting the specific private good denoted by the column header from a local politician in 2012 or 2013; 0 otherwise. Specifications employ pooled data to 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. Marker for clientelist relationship is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see discussion in Section 4.2.1). Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A10: Requests for Private Goods (Instrumental Variables Estimation, 2012 and 2013)

	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: Clientelist Relationship		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 Clientelist Relationship		-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
Year Fixed Effects	Yes	Yes	No	No	No	No	Yes	Yes	No	No	No	No
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 : Clientelist Relationship 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 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. Marker for clientelist relationship is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see discussion in Section 4.2.1). Observations for 2013 employ the subset of individuals for which this marker was measured in a prior wave. Owns a Cistern is coded 1 if the household had a cistern in the relevant year; 0 otherwise. This variable is instrumented by Assignment to Treatment. Owns a Cistern X Clientelist Relationship is instrumented by Assignment to Treatment X Clientelist Relationship. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A11: Ask for and Receive Private Goods (Instrumental Variables Estimation, 2012 and 2013)

	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: Clientelist Relationship		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 Clientelist Relationship		-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
Year Fixed Effects	Yes	Yes	No	No	No	No	Yes	Yes	No	No	No	No
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 : Clientelist Relationship 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 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. Marker for clientelist relationship is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see discussion in Section 4.2.1). Observations for 2013 employ the subset of individuals for which this marker was measured in a prior wave. Owns a Cistern is coded 1 if the household had a cistern in the relevant year; 0 otherwise. This variable is instrumented by Assignment to Treatment. Owns a Cistern X Clientelist Relationship is instrumented by Assignment to Treatment X Clientelist Relationship. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A12: Ask for and Receive Private Goods (2012 and 2013)

	All Requests				All but Water			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1: Treatment	-0.005	0.007			-0.011	-0.006		
	(0.010)	(0.010)			(0.009)	(0.009)		
β_2: Clientelist Relationship		0.076***				0.043**		
		(0.020)				(0.017)		
β_3: Treatment X Clientelist Relationship		-0.068***				-0.029		
		(0.025)				(0.023)		
β_4: Treatment X 2012			-0.003	0.012			-0.012	-0.006
			(0.014)	(0.014)			(0.012)	(0.013)
β_5: Treatment X 2013			-0.009	-0.001			-0.009	-0.006
			(0.010)	(0.010)			(0.009)	(0.009)
β_6: Clientelist Relationship X 2012				0.099***				0.060**
				(0.027)				(0.025)
β_7: Clientelist Relationship X 2013				0.036*				0.013
				(0.020)				(0.018)
β_8: Clientelist Relationship X Treatment X 2012				-0.081**				-0.036
				(0.035)				(0.033)
β_9: Clientelist Relationship X Treatment X 2013				-0.043				-0.016
				(0.027)				(0.025)
$\beta_1 + \beta_3$		-0.061**				-0.035		
		(0.024)				(0.022)		
$\beta_4 + \beta_8$				-0.069**				-0.041
				(0.033)				(0.031)
$\beta_5 + \beta_9$				-0.044*				-0.022
				(0.026)				(0.024)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4284	4284	4284	4284	4284	4284	4284	4284
Mean of Y : Overall		0.090		0.090		0.071		0.071
Mean of Y : Control Group		0.092		0.092		0.077		0.077
Mean of Y : Clientelist Relationship in Control Group		0.167		0.167		0.124		0.124

Notes: Outcome variable is coded 1 if respondent reported requesting and receiving a private good from a local politician in 2012 or 2013; 0 otherwise. Specifications employ pooled data to 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. Marker for clientelist relationship is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see discussion in Section 4.2.1). Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A13: Ask for and Receive Private Goods – Municipalities where Incumbent Ran for Reelection (2012 and 2013)

	All Requests				All but Water			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1: Treatment	-0.015	-0.002			-0.013	-0.003		
	(0.013)	(0.013)			(0.011)	(0.011)		
β_2: Clientelist Relationship		0.079***				0.056**		
		(0.025)				(0.022)		
β_3: Treatment X Clientelist Relationship		-0.068**				-0.048*		
		(0.031)				(0.028)		
β_4: Treatment X 2012			-0.014	-0.000			-0.011	-0.001
			(0.018)	(0.018)			(0.016)	(0.016)
β_5: Treatment X 2013			-0.018	-0.006			-0.014	-0.007
			(0.013)	(0.013)			(0.012)	(0.011)
β_6: Clientelist Relationship X 2012				0.098***				0.066**
				(0.035)				(0.033)
β_7: Clientelist Relationship X 2013				0.046				0.038
				(0.028)				(0.025)
β_8: Clientelist Relationship X Treatment X 2012				-0.069				-0.053
				(0.045)				(0.042)
β_9: Clientelist Relationship X Treatment X 2013				-0.063*				-0.038
				(0.036)				(0.032)
$\beta_1 + \beta_3$		-0.070**				-0.051*		
		(0.030)				(0.027)		
$\beta_4 + \beta_8$				-0.069				-0.054
				(0.042)				(0.040)
$\beta_5 + \beta_9$				-0.069*				-0.045
				(0.035)				(0.032)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2192	2192	2192	2192	2192	2192	2192	2192
Mean of Y : Overall		0.083		0.083		0.064		0.064
Mean of Y : Control Group		0.088		0.088		0.070		0.070
Mean of Y : Clientelist Relationship in Control Group		0.165		0.165		0.124		0.124

Notes: Outcome variable is coded 1 if respondent reported requesting and receiving a private good from a local politician in 2012 or 2013; 0 otherwise. Specifications employ pooled data to 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. Marker for clientelist relationship is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see discussion in Section 4.2.1). Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A14: Ask for and Receive Private Goods, by Primary Type (2012 and 2013)

	All Municipalities			Municipalites with Incumbent Mayors Running for Re-Election		
	Water	Construction	Medical Treatment/Medicine	Water	Construction	Medical Treatment/Medicine
	(1)	(2)	(3)	(4)	(5)	(6)
β_1: Treatment	0.0136**	0.00217	-0.00975	0.00237	0.00573	-0.0167**
	(0.00550)	(0.00389)	(0.00658)	(0.00692)	(0.00484)	(0.00775)
β_2: Clientelist Relationship	0.0336***	0.0104	0.0114	0.0217*	0.00600	0.0231
	(0.0106)	(0.00871)	(0.0146)	(0.0114)	(0.0103)	(0.0174)
β_3: Treatment X Clientelist Relationship	-0.0434***	-0.00526	-0.0134	-0.0159	-0.00569	-0.0207
	(0.0134)	(0.0121)	(0.0175)	(0.0165)	(0.0146)	(0.0212)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4288	4288	4288	2196	2196	2196
Mean of Dependent Variable	0.0226	0.0142	0.0354	0.0209	0.0118	0.0323
P-Value of $\beta_1 + \beta_3$	0.0232	0.792	0.144	0.448	0.998	0.0558

Notes: Outcome variable is coded 1 if respondent reported requesting and receiving a private good specified in the column header from a local politician in 2012 or 2013; 0 otherwise. Specifications employ pooled data to 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. Marker for clientelist relationship is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see discussion in Section 4.2.1). Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A15: Ask for and Receive Public Goods (2012 and 2013)

	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	-0.0011	0.0030	0.0003	0.0001	0.0015	0.0038	0.0036	0.0077	-0.0018	-0.0022
	(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: Clientelistic Relationship		0.0208**		0.0338**		-0.0018		0.0236**		0.0391**		-0.0021
		(0.0086)		(0.0137)		(0.0014)		(0.0114)		(0.0185)		(0.0022)
β_3: Treatment X Clientelistic Relationship		-0.0135		-0.0216		0.0012		-0.0118		-0.0201		0.00260
		(0.0107)		(0.0171)		(0.0015)		(0.0155)		(0.025)		(0.0026)
$\beta_1 + \beta_3$:		-0.0115		-0.0186		0.00134		-0.00796		-0.0124		0.000338
		(0.0103)		(0.0164)		(0.0012)		(0.0147)		(0.0234)		(0.0009)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year 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		0.00793		0.0120		0.00123		0.00956		0.0149		0.00117
Mean of Y : Control Group		0.00851		0.0129		0.00120		0.00933		0.0138		0.0154
Mean of Y : Clientelistic Relationship 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. Marker for clientelist relationship is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see discussion in Section 4.2.1). Observations for 2013 employ the subset of individuals for which this marker was measured in a prior wave. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A16: Requests for Private Goods (Robustness to Above Median Noncompliance)

	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: Clientelist Relationship		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 Clientelist Relationship		-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: Clientelist Relationship 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 Clientelist Relationship 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
Year Fixed Effects	Yes	Yes	No	No	No	No	Yes	Yes	No	No	No	No
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 : Clientelist Relationship 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 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. Marker for clientelist relationship is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see discussion in Section 4.2.1). Observations for 2013 employ the subset of individuals for which this marker was measured in a prior wave. 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 A17: Requests for Private Goods - Alternative Clientelism Marker (2012 and 2013)

	All Requests				All but Water			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1: Treatment	-0.030**	-0.021*			-0.026**	-0.020*		
	(0.013)	(0.012)			(0.012)	(0.012)		
β_2: Clientelist Relationship*		0.108***				0.078**		
		(0.033)				(0.031)		
β_3: Treatment X Clientelist Relationship*		-0.085**				-0.056		
		(0.042)				(0.040)		
β_4: Treatment X 2012			-0.029*	-0.022			-0.025	-0.023
			(0.017)	(0.017)			(0.016)	(0.016)
β_5: Treatment X 2013			-0.031**	-0.019			-0.027*	-0.017
			(0.016)	(0.015)			(0.014)	(0.014)
β_6: Clientelist Relationship* X 2012				0.110***				0.067*
				(0.040)				(0.037)
β_7: Clientelist Relationship* X 2013				0.103**				0.100**
				(0.044)				(0.043)
β_8: Clientelist Relationship* X Treatment X 2012				-0.068				-0.028
				(0.055)				(0.051)
β_9: Clientelist Relationship* X Treatment X 2013				-0.113**				-0.104**
				(0.054)				(0.053)
$\beta_1 + \beta_3$		-0.106**				-0.076*		
		(0.041)				(0.040)		
$\beta_4 + \beta_8$				-0.089*				-0.051
				(0.053)				(0.050)
$\beta_5 + \beta_9$				-0.133**				-0.121**
				(0.054)				(0.052)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4288	4286	4288	4286	4288	4286	4288	4286
Mean of Y : Overall		0.164		0.164		0.164		0.133
Mean of Y : Control Group		0.177		0.177		0.145		0.145
Mean of Y : Clientelist Relationship* in Control Group		0.281		0.281		0.223		0.223

Notes: Outcome variable is coded 1 if respondent reported requesting a private good from a local politician in 2012 or 2013; 0 otherwise. Specifications employ pooled data to 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. Clientelist Relationship* is the alternative marker of clientelist relationships discussed in Section 4.2.1. This marker is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign *and* publicly declared support for a candidate during that campaign; 0 otherwise. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A18: Requests for Private Goods - Municipalities where Incumbent Ran for Re-Election - Alternative Clientelism Marker (2012 and 2013)

	All Requests				All but Water			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1: Treatment	-0.044***	-0.031*			-0.030**	-0.022		
	(0.016)	(0.016)			(0.015)	(0.015)		
β_2: Clientelist Relationship*		0.149***				0.124***		
		(0.039)				(0.035)		
β_3: Treatment X Clientelist Relationship*		-0.116**				-0.080*		
		(0.050)				(0.048)		
β_4: Treatment X 2012			-0.041*	-0.031			-0.026	-0.021
			(0.022)	(0.022)			(0.021)	(0.020)
β_5: Treatment X 2013			-0.048**	-0.031			-0.037**	-0.022
			(0.020)	(0.019)			(0.018)	(0.017)
β_6: Clientelist Relationship* X 2012				0.164***				0.129***
				(0.052)				(0.048)
β_7: Clientelist Relationship* X 2013				0.120**				0.114**
				(0.058)				(0.056)
β_8: Clientelist Relationship* X Treatment X 2012				-0.098				-0.054
				(0.072)				(0.070)
β_9: Clientelist Relationship* X Treatment X 2013				-0.145**				-0.124*
				(0.071)				(0.068)
$\beta_1 + \beta_3$		-0.147***				-0.102**		
		(0.050)				(0.048)		
$\beta_4 + \beta_8$				-0.129*				-0.076
				(0.070)				(0.069)
$\beta_5 + \beta_9$				-0.176**				-0.146**
				(0.071)				(0.067)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2193	2193	2193	2193	2193	2193	2193	2193
Mean of Y : Overall		0.148		0.148		0.148		0.119
Mean of Y : Control Group		0.166		0.166		0.166		0.132
Mean of Y : Clientelist Relationship* in Control Group		0.305		0.305		0.305		0.244

Notes: Outcome variable is coded 1 if respondent reported requesting a private good from a local politician in 2012 or 2013; 0 otherwise. Specifications employ pooled data to 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. Clientelist Relationship* is the alternative marker of clientelist relationships discussed in Section 4.2.1. This marker is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign *and* publicly declared support for a candidate during that campaign; 0 otherwise. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A19: Requests for Public Goods - Alternative Clientelism Marker (2012 and 2013)

	Pooled		2012		2013		Pooled		2012		2013	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
β_1: Treatment	-0.00391	-0.00356	-0.00233	-0.00288	-0.00676	-0.00482	-0.00167	-0.00159	0.00305	0.00225	-0.00916	-0.00751
	(0.00485)	(0.00483)	(0.00669)	(0.00676)	(0.00504)	(0.00486)	(0.00594)	(0.00609)	(0.00916)	(0.00931)	(0.00615)	(0.00616)
β_2: Clientelist Relationship*		0.0341**		0.0397*		0.0234		0.0228		0.0335		0.00326
		(0.0162)		(0.0213)		(0.0217)		(0.0187)		(0.0266)		(0.0233)
β_3: Treatment X Clientelist Relationship*		-0.00670		0.00150		-0.0203		-0.00340		0.00145		-0.0140
		(0.0201)		(0.0279)		(0.0237)		(0.0238)		(0.0352)		(0.0228)
$\beta_1 + \beta_3$		-0.0103		-0.00138		-0.0251		-0.00499		0.00371		-0.0215
		(0.0197)		(0.0269)		(0.0234)		(0.0228)		(0.0338)		(0.0224)
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4292	4290	2667	2666	1625	1624	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 : Clientelistic Relationship* in Control Group		0.0537		0.0633		0.0357		0.0458		0.0588		0.0217

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. Clientelist Relationship* is the alternative marker of clientelist relationships discussed in Section 4.2.1. This marker is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign *and* publicly declared support for a candidate during that campaign; 0 otherwise. Observations for 2013 employ the subset of individuals for which this marker was measured in a prior wave. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A20: Ask for and Receive Private Goods - Alternative Clientelism Marker (2012 and 2013)

	All Requests				All but Water			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1: Treatment	-0.005	0.003			-0.011	-0.007		
	(0.010)	(0.010)			(0.009)	(0.009)		
β_2: Clientelist Relationship*		0.070***				0.043*		
		(0.026)				(0.024)		
β_3: Treatment X Clientelist Relationship*		-0.073**				-0.036		
		(0.032)				(0.030)		
β_4: Treatment X 2012			-0.003	0.006			-0.012	-0.009
			(0.014)	(0.014)			(0.012)	(0.012)
β_5: Treatment X 2013			-0.009	-0.004			-0.009	-0.004
			(0.010)	(0.010)			(0.009)	(0.009)
β_6: Clientelist Relationship* X 2012				0.086**				0.043
				(0.035)				(0.030)
β_7: Clientelist Relationship* X 2013				0.042				0.045
				(0.031)				(0.030)
β_8: Clientelist Relationship* X Treatment X 2012				-0.084*				-0.027
				(0.044)				(0.040)
β_9: Clientelist Relationship* X Treatment X 2013				-0.050				-0.051
				(0.039)				(0.037)
$\beta_1 + \beta_3$		-0.070**				-0.043		
		(0.032)				(0.030)		
$\beta_4 + \beta_8$				-0.078**				-0.036
				(0.043)				(0.039)
$\beta_5 + \beta_9$				-0.054				-0.054
				(0.037)				(0.035)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4284	4282	4284	4282	4284	4282	4284	4282
Mean of Y : Overall		0.090		0.090		0.071		0.071
Mean of Y : Control Group		0.093		0.093		0.077		0.077
Mean of Y : Clientelist Relationship* in Control Group		0.169		0.169		0.128		0.128

Notes: Outcome variable is coded 1 if respondent reported requesting and receiving a private good from a local politician in 2012 or 2013; 0 otherwise. Specifications employ pooled data to 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. Clientelist Relationship* is the alternative marker of clientelist relationships discussed in Section 4.2.1. This marker is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign *and* publicly declared support for a candidate during that campaign; 0 otherwise. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A21: Ask for and Receive Private Goods in Municipalities where Incumbent Ran for Reelection - Alternative Clientelism Marker (2012 and 2013)

	All Requests				All but Water			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1: Treatment	-0.015	-0.005			-0.013	-0.005		
	(0.013)	(0.013)			(0.011)	(0.011)		
β_2: Clientelist Relationship*		0.090***				0.074**		
		(0.032)				(0.029)		
β_3: Treatment X Clientelist Relationship*		-0.091**				-0.068*		
		(0.039)				(0.038)		
β_4: Treatment X 2012			-0.014	-0.001			-0.011	-0.002
			(0.018)	(0.018)			(0.016)	(0.016)
β_5: Treatment X 2013			-0.018	-0.011			-0.014	-0.009
			(0.013)	(0.013)			(0.012)	(0.012)
β_6: Clientelist Relationship* X 2012				0.109**				0.081**
				(0.044)				(0.040)
β_7: Clientelist Relationship* X 2013				0.054				0.062
				(0.045)				(0.044)
β_8: Clientelist Relationship* X Treatment X 2012				-0.107*				-0.075
				(0.056)				(0.052)
β_9: Clientelist Relationship* X Treatment X 2013				-0.063				-0.054
				(0.055)				(0.053)
$\beta_1 + \beta_3$		-0.096**				-0.072**		
		(0.039)				(0.038)		
$\beta_4 + \beta_8$				-0.108**				-0.077*
				(0.054)				(0.051)
$\beta_5 + \beta_9$				-0.073				-0.063
				(0.052)				(0.051)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2192	2192	2192	2192	2192	2192	2192	2192
Mean of Y : Overall		0.083		0.083		0.064		0.064
Mean of Y : Control Group		0.088		0.088		0.070		0.070
Mean of Y : Clientelist Relationship* in Control Group		0.183		0.183		0.145		0.145

Notes: Outcome variable is coded 1 if respondent reported requesting and receiving a private good from a local politician in 2012 or 2013; 0 otherwise. Specifications employ pooled data to 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. Clientelist Relationship* is the alternative marker of clientelist relationships discussed in Section 4.2.1. This marker is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign *and* publicly declared support for a candidate during that campaign; 0 otherwise.Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A22: Votes for Incumbent and Other Electoral Outcomes (2012) - Alternative Clientelism Marker

	Votes for Incumbent Mayor		Votes for Challenger Candidates	Turnout	Blank and Null Votes
	(1)	(2)	(3)	(4)	(5)
Panel A: Unadjusted Regressors:					
Treated Respondents	-1.423**		1.040	-0.442	-0.059
	[0.042]		[0.120]	[0.532]	[0.872]
Respondents	0.361		-0.348	0.127	0.114
	[0.360]		[0.478]	[0.812]	[0.546]
Treated Respondents with Clientelist Relationship*		-2.523			
		[0.222]			
Treated Respondents without Clientelist Relationship*		-1.327			
		[0.116]			
Respondents with Clientelist Relationship*		1.976			
		[0.166]			
Respondents without Clientelist Relationship*		0.215			
		[0.600]			
Panel B: Adjusted Regressors:					
Treated Respondents	-0.293**		0.202	-0.087	0.004
	[0.038]		[0.138]	[0.634]	[0.940]
Respondents	0.074		0.043	0.085	-0.032
	[0.104]		[0.594]	[0.450]	[0.488]
Treated Respondents with Clientelist Relationship*		-1.229			
		[0.186]			
Treated Respondents without Clientelist Relationship*		-.154			
		[0.374]			
Respondents with Clientelist Relationship*		0.745			
		[0.39]			
Respondents without Clientelist Relationship*		0.065			
		[0.544]			
Eligible Voters	Yes	Yes	Yes	Yes	Yes
Location Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	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. Clientelist Relationship* is the alternative marker of clientelist relationships discussed in Section 4.2.1. This marker is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign *and* publicly declared support for a candidate during that campaign; 0 otherwise. * 10%, ** 5%, *** 1% significance levels.

Table A23: Requests for Private Goods, Controlling for Citizen Engagement (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: Clientelist Relationship	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 Clientelist Relationship	-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
Year 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 : Clientelist Relationship 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 good from a local politician in 2012 or 2013; 0 otherwise. Specifications employ pooled data to 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. Marker for clientelist relationship is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see discussion in Section 4.2.1). Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A24: Effect on Votes for Workers' Party (PT) Mayoral Candidate (2012)

	Votes for PT Candidate	
	(1)	(2)
Panel A: Unadjusted Regressors:		
Number of Treated Individuals	-0.603	
	[0.476]	
Number of Respondents	0.314	
	[0.478]	
Number of Treated Individuals in a Clientelist Relationship		-0.197
		[0.942]
Number of Treated Individuals Not in a Clientelist Relationship		-0.749
		[0.502]
Number of Respondents in a Clientelist Relationship		0.726
		[0.622]
Number of Respondents Not in a Clientelist Relationship		0.255
		[0.644]
Panel B: Adjusted Regressors:		
Number of Treated Individuals	-0.0332	
	[0.734]	
Number of Respondents	0.0307	
	[0.768]	
Number of Treated Individuals in a Clientelist Relationship		-0.451
		[0.322]
Number of Treated Individuals Not in a Clientelist Relationship		-0.0681
		[0.754]
Number of Respondents in a Clientelist Relationship		0.227
		[0.484]
Number of Respondents Not in a Clientelist Relationship		0.0744
		[0.756]
Registered Voters	Yes	Yes
Location Fixed Effects	Yes	Yes
Observations	867	867

Notes: Outcome variable is votes for candidate from the PT party. *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 A25: Requests for Private Goods from PT Mayors (2012 and 2013)

	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	-0.00633	-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 PT Mayor	0.0170	0.00240	0.00607	-0.0164	0.0254	0.0241	0.0166	0.0162	0.0157	0.00694	0.00152	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: Clientelist Relationship		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 Clientelist Relationship		-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: Clientelist Relationship X PT Mayor		-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 Clientelist Relationship X PT Mayor		0.0720		0.118		0.000409		-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
Year Fixed Effects	Yes	Yes	No	No	No	No	Yes	Yes	No	No	No	No
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 : Clientelist Relationship in Control Group		0.285		0.345		0.181		0.294		0.353		0.190

Notes: Outcome variable is coded 1 if respondent reported requesting a private good from a local politician in 2012 or 2013; 0 otherwise. Specifications employ pooled data to 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. Marker for clientelist relationship is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see discussion in Section 4.2.1). Observations for 2013 employ the subset of individuals for which this marker was measured in a prior wave. PT Mayor is coded 1 if a member of the Worker's Party; 0 otherwise. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A26: Ask for and Receive Private Goods from PT Mayors (2012 and 2013)

	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	-0.00317	0.0102	0.00235	0.0190	-0.0109	-0.00418	-0.0194	-0.00540	-0.0194	-0.00528	-0.0154	-0.00383
	(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 PT Mayor	-0.00986	-0.0151	-0.0275	-0.0313	0.0133	0.00538	0.0156	0.0114	0.00857	0.00961	0.0163	0.00217
	(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: Clientelist Relationship		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 Clientelist Relationship		-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: Clientelist Relationship X PT Mayor		-0.0361		0.000121		-0.0843***		-0.0157		0.0405		-0.0990**
		(0.0455)		(0.0718)		(0.0290)		(0.0591)		(0.0908)		(0.0405)
β_6: Treatment X Clientelist Relationship X PT Mayor		0.0172		-0.00347		0.0419		0.00548		-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
Year Fixed Effects	Yes	Yes	No	No	No	No	Yes	Yes	No	No	No	No
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 : Clientelist Relationship in Control Group		0.167		0.218		0.0764		0.165		0.216		0.190

Notes: Outcome variable is coded 1 if respondent reported requesting and receiving a private good from a local politician in 2012 or 2013; 0 otherwise. Specifications employ pooled data to 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. Marker for clientelist relationship is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see discussion in Section 4.2.1). Observations for 2013 employ the subset of individuals for which this marker was measured in a prior wave. PT Mayor is coded 1 if a member of the Worker's Party; 0 otherwise. Standard errors clustered at the neighborhood cluster level reported in parentheses. * 10%, ** 5%, *** 1% significance levels.

Table A27: Requests for Private Goods, with Rainfall (2012 and 2013)

	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: Clientelist Relationship	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 Clientelist Relationship	-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 × Clientelist Relationship		-0.00727	0.00789		-0.00816	0.0227
		(0.0217)	(0.0320)		(0.0313)	(0.0472)
β_7: Rainfall × Clientelist Relationship			-0.0153			-0.0310
			(0.0239)			(0.0359)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4288	4288	4288	2193	2193	2193
Mean of Dependent Variable			0.164			0.148

Notes: Outcome variable is coded 1 if respondent reported requesting a private good 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. Marker for clientelist relationship is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see discussion in Section 4.2.1). 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 A28: Ask For and Receive Private Good, with Rainfall (2012 and 2013)

	All Municipalities			Municipalites with Incumbent Mayors Running for Re-Election		
	(1)	(2)	(3)	(4)	(5)	(6)
β_1: Treatment	0.00757	0.00707	0.00771	-0.000351	-0.000675	0.00153
	(0.0102)	(0.0103)	(0.0103)	(0.0135)	(0.0135)	(0.0138)
β_2: Clientelist Relationship	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	-0.00824	-0.00804	-0.00232	-0.00304	-0.00301	0.00377
	(0.00935)	(0.00937)	(0.00967)	(0.0144)	(0.0144)	(0.0142)
β_4: Treatment X Clientelist Relationship	-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	-0.00510	-0.00194	-0.00640	-0.00636	-0.00544	-0.0124
	(0.0102)	(0.0106)	(0.0105)	(0.0148)	(0.0151)	(0.0150)
β_6: Treatment × Rainfall × Clientelist Relationship		-0.0169	0.00814		-0.00426	0.0344
		(0.0157)	(0.0242)		(0.0209)	(0.0349)
β_7: Rainfall × Clientelist Relationship			-0.0253			-0.0388
			(0.0189)			(0.0285)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4284	4284	4284	2192	2192	2192
Mean of Dependent Variable			0.0903			0.0826

Notes: Outcome variable is coded 1 if respondent reported requesting and receiving a private good 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. Marker for clientelist relationship is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see discussion in Section 4.2.1). 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: Cisterns Treatment and Electoral Outcomes – Adjustment of Regressors and Procedures for Inference

For analyses of the cistern intervention's effects on electoral outcomes, this appendix discusses the adjustment of regressors as well as procedures to conduct appropriate inference. As emphasized in Section 4.2.2, the statistical significance of findings is robust without any adjustments (see also Table 8). However, the procedure described below improves estimation of the magnitude of treatment effects on electoral outcomes.

Before further discussion, recall that the equation for estimating voting outcomes for the incumbent mayoral candidate at a given electronic voting machine in a given voting location (in a given municipality) is as follows:

$$y_{slm} = \alpha_{lm} + \gamma_1 \cdot TV_{slm} + \gamma_2 \cdot EV_{slm} + \gamma_3 \cdot RV_{slm} + \epsilon_{slm}, \tag{5}$$

where y_{slm} is the number of votes for the incumbent mayor in electronic voting machine (i.e., "electoral section") s, in voting location l, in municipality m. The regressor of interest is TV_{slm}, the number of treated individuals in our study assigned by electoral authorities to vote in that particular machine. Other controls in the regression are EV_{slm}, the overall number of individuals in our study assigned to that machine; α_{lm}, a voting location fixed effect to control for differential voting patterns across voting locations in a municipality; and RV_{slm}, the total number of registered voters assigned to that machine (regardless of whether they are in our study sample). 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. Furthermore, within a given polling place, citizens are assigned to a specific voting machine by electoral authorities.[34] Therefore, once we condition on the total number of individuals in the study registered to vote

[34]Our identification strategy is robust to any influence citizens may have regarding their polling place.

in the machine, we can identify γ_1 – the effect of an additional person assigned to the cisterns treatment on votes for the incumbent mayor.

Adjustment of Regressors

Although our study shows that the cisterns treatment significantly reduces votes for the incumbent mayor – without conducting any adjustments – further consideration is needed because specifications about electoral outcomes (but not about requests) involve aggregate data. As mentioned in Section 4.2.2, Brazil releases electoral results at the electronic voting machine level. Thus, for analyses of electoral results, we aggregate the counts of treated and control individuals in our experimental sample for each machine to construct explanatory variables. TV_{slm} and EV_{slm} sum how many treatment and overall study participants were assigned by electoral authorities to vote in a particular machine in a given polling location. Accurately measuring treatment effects on electoral outcomes with these aggregate data requires attention to three potential issues: (a) treatment effects on voting by non-interviewed members of treated households (as registered voters in sampled households were only interviewed if present during our home visits); (b) spillover effects on neighbors' voting behavior (e.g., due to sharing water with ineligible, neighboring households); and (c) peer effects on neighbors' voting behavior. Failing to address the possible undercounting of other treated household members, as well as positive spillover and peer effects, could bias upward our estimates of treatment effects (in absolute terms). Therefore, we adjust TV_{slm} and EV_{slm} to incorporate estimates of: (a) how many non-interviewed individuals live in sampled households, (b) how many live in other households in the neighborhood cluster without a cistern at baseline (i.e., those potentially affected by spillover or peer effects of the cisterns treatment), and (c) the probabilities that these individuals are assigned by their voter registration cards to vote in the same locations and same voting machines as our interviewees.

The following discussion explains the procedure for adjusting the number of treated individuals (TV_{slm}); we follow an analogous procedure to adjust the total number of experimental subjects regressor (EV_{slm}).

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

$$TV_{slm} = \sum_{c} TV_{cslm}, \tag{6}$$

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

$$TV_{slm} = \sum_{c} \left[TV_{cslm}^{I,h} + TV_{cslm}^{NI,h} + TV_{cslm}^{NI,h_-} \right], \tag{7}$$

where $TV_{cslm}^{I,h}$ denotes voters who were interviewed (denoted by the superscript I) from household h in cluster c and who are assigned (as specified by electoral authorities on the respondent's voting identification card) to vote in the machine denoted by slm. $TV_{cslm}^{NI,h}$ refers to voters from the same household h who were not interviewed (denoted by the superscript NI), and TV_{cslm}^{NI,h_-} denotes all voters from households other than h (i.e. households that were not part of our survey) from cluster c who are assigned to vote in the machine denoted by slm. This can also be expressed as:

$$TV_{slm} = \sum_{c} \left[TV_{cslm}^{I,h} + TV_{cm}^{NI,h} \pi_{cslm}^{NI,h} + TV_{cm}^{NI,h_-} \pi_{cslm}^{NI,h_-} \right], \tag{8}$$

where $TV_{cm}^{NI,h}$ is the total number of voters not interviewed in household h and $\pi_{cslm}^{NI,h}$ is the proportion of these voters who are assigned to vote in machine s in location l. Analogously, TV_{cm}^{NI,h_-} is the total number of voters belonging to all other households in the neighborhood cluster and π_{cslm}^{NI,h_-} is the proportion of these voters who are assigned to vote in machine s in location l.

To estimate TV_{slm}, we obtain each of the quantities on the right side of Equation 4 in the following manner:

(a) We obtain $TV_{cslm}^{I,h}$ directly from a question in our survey, which inquires about which electronic voting machine the respondent is assigned to vote (as specified by electoral authorities on the respondent's voting identification card).

(b) We obtain $TV_{cm}^{NI,h}$ directly from a question in the baseline survey, which includes information about all household members.

(c) We estimate $TV_{cm}^{NI,h-}$ from responses to two questions in our surveys. The first question (in our localization survey) asked how many neighboring households do not have water cisterns. The second question (in the baseline survey) provides information about how many household members are of voting age. With these data by cluster, we estimate $TV_{cm}^{NI,h-}$ by using the median of households' responses about the number of neighboring households without cisterns $\times$ the median number of household members of voting age.

(d) To estimate $\pi_{cslm}^{NI,h}$ and $\pi_{cslm}^{NI,h-}$, we assume these proportions are equivalent and do not vary across electronic voting machines and voting locations in our sample. We then estimate $\pi_{cslm}^{NI,h}$ and $\pi_{cslm}^{NI,h-}$ by assigning non-interviewed individuals probabilistically (by neighborhood cluster) across machines and locations. To do so, we undertake several steps described below.

First, we estimate the probability that survey respondents vote in the same municipalities, voting locations, and electronic voting machines as others (i.e., as uninterviewed voters in sampled households and as voters in other households in sampled clusters); it is assumed these probabilities are equal across neighborhood clusters. In a second step, we assign non-interviewed voters probabilistically to the machines and locations in which individuals within and outside our sample of respondents are designated to vote (as specified by electoral authorities on voting identification cards). More

precisely, we estimate $\pi^{NI,h}_{cslm}$ and π^{NI,h_-}_{cslm} as follows:

$$\pi^{NI,h}_{cslm} = \pi^{NI,h_-}_{cslm}, = \begin{cases} \theta^{Ihslm}.\left(TV^{I,h}_{cslm}/\sum_s TV^{I,h}_{cslm}\right) & if\ TV^{I,h}_{cslm} > 0 \\ \theta^{Ih,s_-,lm}.\left(RV^{I,h}_{slm}/\sum_s RV^{I,h}_{slm}\right) & if\ TV^{I,h}_{cslm} = 0\ \&\ \sum_s TV^{I,h}_{cslm} > 0 \\ (\theta^{Ih,s_-,l_-,m} + \theta^{Ih,s_-,l_-,m_-})0 & if\ \sum_s TV^{I,h}_{cslm} = 0 \end{cases} \tag{9}$$

where:

θ^{Ihslm} is the probability an individual outside our sample is assigned to vote in the same voting machine as respondents in our sample. That is, $\theta^{Ihslm} = P(Sample\ Municipality) \cdot P(Sample\ Location \mid Sample\ Municipality) \cdot P(Sample\ Section \mid Sample\ Location)$;

$\theta^{Ih,s_-,lm}$ is the probability an individual outside our sample is assigned to vote in another voting machine – but in the same voting location – as respondents in our sample. That is, $\theta^{Ih,s_-,lm} = P(Sample\ Municipality) \cdot P(Sample\ Location \mid Sample\ Municipality) \cdot (1 - P(Sample\ Section \mid Sample\ Location))$;

$\theta^{Ih,s_-,l_-,m}$ is the probability an individual outside our sample is assigned to vote in another voting location in the municipality. That is, $\theta^{Ih,s_-,l_-,m} = P(Sample\ Municipality) \cdot (1 - P(Sample\ Location \mid Sample\ Municipality))$; and

θ^{Ih,s_-,l_-,m_-} is the probability an individual outside our sample is assigned to vote in another municipality. That is, $\theta^{Ih,s_-,l_-,m_-} = 1 - P(Sample\ Municipality)$.

In order to estimate $\pi^{NI,h}_{cslm}$ and π^{NI,h_-}_{cslm} in Equation (9), the next step is to estimate each θ just described. To this end, we use the following estimates from our surveys:
(i) $P(Sample\ Municipality)$ = 90.5 percent of respondents are registered to vote in the municipality where their household cluster is located (as specified by electoral authorities on voting identification cards);
(ii) among the subet of voters in (i), $P(Sample\ Location \mid Sample\ Municipality)$ = 86.9 percent are registered to vote in the same voting location; and
(iii) among those registered to vote in the same location, $P(Sample\ Section \mid Sample\ Location)$ = 40.2 percent are registered to voting machines in our sample of machines with respon-

dents assigned to these ($TV^{I,h}_{cslm} > 0$).

Using this information, we estimate each θ in Equation (9) as follows:

(1) 31.6 percent (= 0.905 $\times$ 0.869 $\times$ 0.402) of non-respondents are registered to vote in the same sets of electronic voting machines as those of our respondents. That is, we assign θ^{Ihslm} = 31.6 percent in equal proportion to the distribution across machines in which respondents from the neighborhood cluster registered to vote;

(2) 47.0 percent (= 0.905 $\times$0.869 $\times$ (1 - 0.402)) of non-respondents are registered to vote in other machines in the same voting locations as those of our repondents. That is, we assign $\theta^{Ih,s_-,lm}$ = 47.0 percent in equal proportion to the distribution of registered voters across the remaining machines of the voting locations in which our respondents are registered to vote;

(3) The remaining 21.4 percent of non-respondents are registered to vote outside electronic voting machines in which our respondents are registered. More specifically, $\theta^{Ih,s_-,l_-,m} = P(Sample\ Municipality) \cdot (1 - P(Sample\ Location \mid Sample\ Municipality))$ (= 0.905 $\times$ (1 - 0.869) = 11.9 percent) of non-respondents are assigned to other voting locations in the municipality, whereas $\theta^{Ih,s_-,l_-,m_-} = 1 - P(Sample\ Municipality)$ (= (1 - 0.905) = 9.5 percent) are assigned to vote in other municipalities. Both of these groups are outside our scope of analysis.[35]

Inference

To conduct appropriate inference about electoral outcomes (but not citizen requests), recall that two points require consideration. First, sampling error of the adjusted regressors must be taken into account. And second, because we allow the errors to be correlated across voting machines and locations within a municipality, our sample is composed of 21 "clusters" – that is, municipalities in which the mayor is running for reelection. To address both points, we report p-values from a wild cluster bootstrap-t procedure. This procedure not only addresses the fact that we have a limited number of

[35] Because we restrict the analysis to the voting locations where interviewed individuals are assigned to vote, we effectively exclude individuals in this category for purposes of the adjustment.

clusters (Cameron, Gelbach, and Miller 2008), but also takes into account sampling error of the adjusted regressors through bootstrapped sampling of the data used to construct estimates (Horowitz 2001). More specifically, 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 following: the number of neighboring households, the number of registered 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 EV_{slm} control (i.e., the overall number of individuals in our study assigned to a given machine) 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, making our inferences about treatment effects more conservative. Panel B of Table 8 reports the point estimates from the specification with the adjusted regressors of interest along with p-values from the nonparametric and 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 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: Summary Statistics for Voting Outcomes

Variable	All Voting Locations in Municipality	Locations in Estimation Sample
Number of Voting Locations per Municipality	32.4 (35.8)	9.1 (6.0)
Number of Voting Machines per Municipality	133.4 (164.3)	43.3 (22.2)
Number of Registered Voters per Municipality	43241.0 (54150.1)	14633.7 (8342.8)
Number of Registered Voters per Machine	323.4 (33.2)	330.2 (43.3)
Number of Votes for Incumbent Mayor per Machine	116.2 (31.2)	118.1 (34.1)
Number of Votes for Challenging Mayoral Candidate per Machine	135.6 (30.6)	138.5 (30.9)
Number of Blank/Invalid Votes in Mayoral Election per Machine	18.4 (4.2)	18.5 (3.7)
Turnout per Machine	270.2 (28.7)	275.1 (32.0)
Number of Respondents per Machine	- -	1.05 (1.68)
Number of Treated Respondents per Machine	- -	0.50 (1.14)
Number of Control Respondents per Machine	- -	0.55 (1.16)

Notes: The table displays means as well as standard deviations in parenthesis.

Table B2: Voting Outcomes: Balance Across Machines in Voters' Characteristics

	Age	Schooling Years	Clientelist Relationship	Female Respondents
	(1)	(2)	(3)	(4)
Number of Treated Individuals	-0.129	0.004	0.006	-0.005
	[0.416]	[0.808]	[0.294]	[0.128]
Number of Respondents	-0.020	0.003	-0.001	0.004
	[0.634]	[0.708]	[0.582]	[0.216]
Total Registered Voters	Yes	Yes	Yes	Yes
Location Fixed Effects	Yes	Yes	Yes	Yes
Observations	909	909	909	909
Mean of Dependent Variable	39.2	6.4	0.346	0.547

Notes: Bootstrapped p-values using wild clustered bootstrapped standard errors in brackets. Marker for clientelist relationship is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see discussion in Section 4.2.1).

Appendix C: Survey Questions for Key Variables

1.- Variable: *Request for Private Goods (asked in 2012 and 2013)*.

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

2.- Variable: *Ask for and Receive Private Good (asked in 2012 and 2013)*.

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

3.- Variable: *Talked Monthly with Politicians Before 2012 Campaign (asked in 2012)*.

- Definition: Respondent reports conversing with a political candidate at least monthly before the 2012 campaign began.

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

4.- Variable: *Declared Support (asked in 2012)*.

- Definition: Respondent publicly declared support for any political candidate during the 2012 election.
- Coded 1 if answered yes to any of the following four questions; 0 otherwise: "Let's talk about this year's political campaign, that is, about the last 3 months before the October election.
  - (a) Did you wear a candidate's sticker or shirt?";
  - (b) [Asked of rally attendees] "In this rally, did you use a flag, shirt or anything else to show your support?";
  - (c) "During the electoral campaign, did you put a flag or a poster on your house?";
  - (d) "Was the wall of your house painted with the name of a candidate?"