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. To examine how reduced vulnerability affects citizens' participation in clientelism, we employ two exogenous shocks to vulnerability. First, we designed a randomized control trial to reduce household vulnerability: our development intervention constructed residential water cisterns in drought-prone areas of Brazil. Second, we exploit rainfall shocks. We find that reducing vulnerability significantly decreases requests for private goods from politicians, especially among citizens likely to be in clientelist relationships. Moreover, reducing vulnerability decreases votes for incumbent mayors, who typically have more resources for clientelism.

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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, potentially leading to both reduced political competition as well as the underprovision of public goods and social insurance.[1]

Given such potential consequences, why would citizens participate in clientelism? Of the many factors posited, perhaps none has garnered more attention than poverty. An extensive theoretical literature points to the decreasing marginal utility of consumption as an underlying reason why impoverished citizens may place relatively greater value on private consumption than on political preferences or public goods provision (e.g., Dixit and Londregan 1996, Lizzeri and Persico 2001, Bardhan and Mookherjee 2012). But while this focus on poverty emphasizes the *level* of consumption, it does not pay close attention to the *variability* of consumption. Recognizing the importance of both dimensions of consumption, the present study broadens the focus to vulnerability. As shown by Ligon and Schechter's (2003) theoretical work, economic vulnerability — defined as encompassing both the level and variability of consumption — has important effects on citizens' well-being.

[1] For example, 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).

The pervasiveness of both vulnerability and clientelism across developing countries raises two important, unexplored questions. First, is there a causal link between 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. A key contribution is our use of two exogenous random shocks to vulnerability. First, 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 water cisterns for individual households. Each cistern captures up to 16,000 liters of water from rainfall or water truck deliveries. As our second source of exogenous variation in vulnerability, we also examine municipal-level rainfall shocks. Analyses show that both shocks to vulnerability — water cisterns and rainfall — improve household well-being, as measured by prominent indicators of food insecurity, depression, and self-reported health status.

Another important contribution is our novel, longitudinal survey of impoverished rural households facing water insecurity. This large representative 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 living in municipalities 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.

Our study provides rigorous evidence suggesting a link between vulnerability and citizen requests. Citizens in the cisterns treatment group — that is, those in households randomly assigned to receive water cisterns — are significantly less likely to request private goods from politicians. The intervention reduces their likelihood of making such requests by 3.0 percentage points, a substantial decline of 17 percent. Furthermore, we observe analogous effects for rainfall shocks: a one standard deviation decrease in municipal rainfall increases requests of private goods from politicians by 2.3 percentage points, a substantial increase of 13 percent. Both findings are robust to excluding water requests, which are directly affected by cisterns and rainfall. We also find that the cisterns treatment — a technology increasing long-term water availability — decreases citizens' requests not only during the election campaign, but also during the year after the election.

Additional analyses point towards clientelism as a mechanism explaining these results. We show that effects of the cisterns treatment 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. Similar patterns are observed for rainfall shocks, albeit with less precision: a one standard deviation decrease in rainfall increases requests by 3.5 percentage points among citizens likely to be in clientelist relationships, compared to only 2.0 percentage points among citizens without such rela-

[2]Electronic voting inhibits monitoring of votes in Brazil. Many citizens mitigate this challenge for clientelism during campaigns: they publicly declare support for candidates with whom they have 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.

tionships. Altogether, these results suggest that the observed link between vulnerability and citizen requests is concentrated among citizens likely to be in clientelist relationships.

Given these findings, we investigate whether decreased vulnerability renders citizens less likely to vote for incumbents, who typically have more resources for clientelism. Because our randomized control trial was designed to reduce vulnerability at the household level, we are able to leverage extraordinarily granular data on voting outcomes. Our survey links individual subjects in the cisterns experiment to their specific electronic voting machines in Brazil's 2012 municipal elections. In order to measure electoral responses to the cisterns 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 running for reelection by 10 percentage points. When extending the sample to include municipalities where the incumbent mayor did not vie for reelection (e.g., due to term limits), findings are similar: the cisterns treatment decreases a citizen's probability of voting for the candidate of the incumbent group by 7.6 percentage points. As with requests, electoral effects are concentrated among citizens likely to be in clientelist relationships. While not dispositive, these results are consistent with the argument that reduced vulnerability makes citizens less beholden to incumbent politicians, in that they may be less reliant on clientelist relationships as a risk-coping mechanism.

Our experimental study is theoretically motivated by Anderson, Francois, and Kotwal's (2015) model of clientelism as informal insurance. In their model, clientelist politicians undermine policies for poor and vulnerable households, so that they can facilitate clientelist arrangements. These clientelist arrangements involve informal insurance transfers — more specifically, in contingent exchange for votes, clientelist politicians provide transfers to particular citizens if they experience negative shocks. In the Online Appendix, we extend this model to examine implications when an intervention — such as our water cisterns — reduces vulnerability by providing an independent risk-coping mechanism that affects both the level and variability of consumption. Consistent with

our empirical results, this formalization suggests that reducing citizens' vulnerability decreases citizens' participation in informal insurance arrangements and reduces votes for clientelist politicians. Also in line with our findings, the model predicts that effects are amplified among citizens with established relationships with clientelist politicians.[3]

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. 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 (2018): economic independence frees the poor to express support for opposition candidates. Third, as discussed above, our study is the first to provide experimental evidence consistent with important theoretical implications from Anderson, Francois, and Kotwal (2015) regarding how introducing independent risk-coping mechanisms affects clientelism. Fourth, 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), Calvo and Murillo (2013) and Duarte et al. (2019), which documents how vote buying and clientelism operate through established networks based on reciprocal, partisan, or personal ties. Fifth, unlike nearly all existing quantitative work on clientelism (e.g., Vicente 2014, Hicken et al. 2018), our study provides evidence about the phenomenon during *both* electoral and non-electoral periods. Finally, an innovative feature of our approach

[3]The cisterns intervention examines effects of decreasing vulnerability, which is a function of both the level and variability of consumption (Ligon & Schechter, 2003). We do not test effects of exclusively decreasing variation of water consumption, as cisterns do not lead to a mean-preserving decrease in this variance.

[4]E.g., Finan and Schechter (2012) argue that due to limited contract enforceability, vote buyers target individuals who are more likely to reciprocate, an individual characteristic that is generally difficult to observe.

is that it emphasizes the important role that citizens play in clientelism, a demand-side perspective that is overlooked by most quantitative and theoretical work on the topic.

The article is organized as follows. Section 2 provides contextual information about rural Northeast Brazil. Section 3 describes the cisterns intervention and rainfall shocks. We follow with a description of our data sources in Section 4. Section 5 discusses the experimental design and empirical methodology. Section 6 presents the central empirical results of our study. Findings in Section 7 point to clientelism as a mechanism explaining these results. Section 8 provides evidence that counters alternative explanations involving politicians' responses and credit claiming, as well as citizens' engagement, beliefs, and preferences. Finally, Section 9 concludes with a discussion of findings and their broader implications.

2 Context

This study focuses on Brazil's semi-arid zone, the vast majority of which is located in the country's Northeast region. The zone spans over one million square kilometers (see Figure 1), and its population of over 28 million residents is disproportionately poor and rural.[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 43.9 cm, compared to 139.5 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 ad-

[5]During our study period, the semi-arid region consisted of 1,133 contiguous municipalities in nine states: Alagoas, Bahia, Ceará, Minas Gerais, Paraiba, Pernambuco, Piauí, Rio Grande do Norte, and Sergipe.

[6]Brazil's Institute for Applied Economic Research (IPEA) classified most of the Northeast region as "very vulnerable" in its 2015 Index of Social Vulnerability.

dress their needs is often limited. Health shocks are another major issue, as inadequate healthcare often ranks as the top concern in opinion surveys. 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 for the richest quintile (Barros et al. 2011).

Given their substantial vulnerability to shocks, many Brazilians request assistance from local politicians. Table 1 presents descriptive evidence from our panel survey described below. During the 2012 election year, 21.3 percent of survey respondents asked for private goods from a mayoral or councilor candidate.[7] Moreover, 8.6 percent of respondents made such requests during the following non-election year. Many requests involve life necessities: a third of requests in both years involved health care, and a quarter involved water. Requests also increase amidst adverse shocks: a standard deviation reduction in municipal rainfall increased requests by 3.6 percentage points in 2012 (but not in 2013). Politicians fulfilled approximately half of requests in both years.

Local politicians have considerable discretion and resources to fulfill citizens' requests.[8] They frequently mete out assistance using political criteria, as requests often exceed 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 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. For example, open-list proportional representation (used to elect councilors and

[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 be reelected later. Councilors are municipal legislators elected by open-list proportional representation without term limits. Voting is compulsory in Brazil, with turnout in most elections around 80 percent.

[8] Brazil's government expenditures are among the most decentralized in the world (IMF 2016), with many public services decentralized to the local level. Most municipalities rely primarily on transfers from higher levels of government to finance expenditures (IMF 2016).

federal/state deputies) heightens intra-party competition, thereby promoting particularism over programmatic appeals (Hagopian 1996, Ames 2002). Brazil's highly fragmented party system also undermines programmatic appeals, as it is challenging for some voters to ascertain which of many parties align with their collective interests.[9]

Clientelism in rural Northeast Brazil is facilitated by politicians' ongoing interactions with citizens. In our panel survey, 18.4 percent of respondents had at least monthly conversations with a local politician before the 2012 election campaign began. Moreover, 69.6 percent of respondents reported receiving at least one home visit from representatives of a mayoral candidate during the 2012 campaign. Citizens tend to interact most often with candidates for city council, who often serve as brokers for allied mayoral candidates and fulfill requests on their behalf.[10] These relationships often might 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 on voting behavior within households, as 77.3 percent of respondents report that all family members vote for the same mayoral candidate. While reasons for such interactions are multifaceted, politicians' reach to so many poor, isolated households suggests the presence of an extensive political network, which is typically a prerequisite for clientelism.

Electronic voting undermines clientelist politicians' ability to monitor vote choices in Brazil, so many citizens mitigate this challenge during campaigns by publicly declaring support for candidates with whom they have ongoing clientelist relationships (Nichter 2018). As discussed in the Introduction, declared support enables citizens to 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

[9] In addition, politicians may find clientelism easier than registering fictitious voters or tampering with electoral returns, as 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).

[10] Beyond Brazil, evidence suggests that city councilors also serve as brokers in Argentina and the Philippines (Stokes et al. 2013, Ravanilla, Haim and Hicken 2021).

on their bodies, on their homes, or at rallies. Citizens are more likely to engage in each form of declared support when their municipality experiences rainfall shocks: a one standard deviation decline in rainfall increases overall declarations by 6.6 percentage points (see Table 1).

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 request 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, Medina and Stokes 2007). 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 5.6 percent by challenger candidates. The disparity is even starker during the 2013 post-election year, reaching an order of magnitude: whereas 3.9 percent of respondents had requests fulfilled by politicians in office, only 0.40 percent had requests fulfilled by politicians out of office.

3 Cisterns Intervention and Rainfall Shocks

In order to examine how reduced vulnerability affects clientelism, we employ two sources of exogenous variation in household vulnerability. The first source is a randomized control trial designed to reduce household vulnerability: the construction of residential water cisterns across neighborhood clusters within drought-prone municipalities. Our secondary source is municipal-level rainfall shocks, which also exogenously affect household vulnerability.

3.1 Cisterns Intervention

With regards to cisterns, we employ a prospective randomized control design that provided rainfed water cisterns to specific households in randomly selected neighborhood clusters. Cisterns were developed by our NGO partner *Articulação no Semi-Arido Brasileiro* (ASA, or Brazilian Semi-Arid Articulation)[11] as a strategy to help poor rural households cope with irregular rainfall. Each water cistern consists of an enclosed structure made of reinforced concrete, capable of holding up to 16,000 liters of water. 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.

Cisterns reduce vulnerability by increasing the level and decreasing the variability of water consumption. This technology harvests rainfall from a home's roof, which not only facilitates immediate water consumption, but also enables smoothing between periods with high and low precipitation. Hence, cisterns increase water consumption during good states, as well as providing insurance against bad states. Beyond rain harvesting, cisterns also provide another way to increase the level and decrease the variability of water consumption: they serve as storage vessels when receiving water truck deliveries. This alternative use of cisterns is especially prevalent during droughts; among the cisterns treatment group in our experiment, households in low-rainfall municipalities received over twice as much water from deliveries as households in high-rainfall municipalities.[12] Given their dual usage, cisterns reduce household vulnerability during periods of both high and low rainfall. One implication is that cisterns may not mediate the effect of rainfall shocks on clientelism — unlike, for example, dams' ability to reduce the sensitivity of other outcomes to rainfall shocks in downstream communities (Duflo and Pande 2007).

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

[12] In our detailed survey data on water deliveries in 2012, treated households in the bottom quartile of rainfall received 4,000 liters of water deliveries, versus 1,750 liters for those in the top quartile.

Cisterns cost approximately US$ 1,000 (R$ 1,500 in 2010) each to construct, and were awarded free of charge to eligible households. As described below, we randomized the construction of cisterns by ASA in specified municipalities, beginning in January 2012. Prior to our experiment, ASA had built cisterns in Northeast Brazil since 2003. 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.

3.2 Rainfall Shocks

With regards to our second source of exogenous variation in household vulnerability, we also exploit municipal-level rainfall shocks. The Northeast region of Brazil experienced its worst drought in fifty years during our study period (WMO 2014); all 40 municipalities in our sample experienced negative rainfall shocks of varying intensity in 2012, as did over three-quarters in 2013. In rural Brazil, rainfall variability affects how much water is available for household consumption, as citizens often rely on streams, dams and other surface water sources replenished by precipitation. Moreover, rainfall shocks often affect agricultural productivity and income (Jayachandran 2006; Dell, Jones

and Olken 2014). Given such effects, negative rainfall shocks increase household vulnerability.

An important contribution of our study is that it examines both rainfall shocks and water cisterns — two sources of exogenous variation in household vulnerability. At the outset, two key differences deserve emphasis. First, while rainfall shocks are temporary, cisterns are a technology that increases long-term water availability. And second, rainfall shocks are likely to invoke both demand-side and supply-side responses, as they have broad impacts on municipalities. By contrast, the cisterns intervention was designed to identify changes in citizens' behavior, the central focus of our study. As corroborated in Section 8.1, our cisterns experiment was unlikely to invoke politician responses as it reached a minute share of the population: whereas the municipal population in our sample averaged 49,000 citizens, we built an average of 14 cisterns per municipality.

4 Data

4.1 Study Population and Sample

Our study's population consists of rural households in Brazil's semi-arid zone without reliable access to drinking water. More specifically, households eligible for the study met the following inclusion criteria: (a) they had no piped drinking water or cistern, (b) they had physical space on their property to build a cistern, and (c) their roofs were at least 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. In these municipalities, only households in our study received cisterns from ASA throughout our research period.

4.2 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.[13] This first survey wave — which predated the cisterns 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 cisterns 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.

[13] In total, the baseline survey of household heads obtained information about 2,990 household members.

4.3 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.

To examine the impact of the cisterns 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 the former *Bolsa Família* program). Each section number corresponds to a unique voting machine in a municipality.[14] 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.

For electoral outcomes, the main estimation sample focuses on the 21 sampled municipalities in which the mayor ran for reelection. We also show the robustness of findings using an expanded sample of 39 municipalities for which we can identify the mayoral candidate of the incumbent group.[15] For the expanded sample, the mayoral candidate of the incumbent group met one of the following criteria: (1) was the incumbent mayor;

[14] 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.

[15] In one of our study's 40 municipalities, electoral officials revoked the candidacy of a copartisan of the term-limited incumbent mayor.

(2) was vice-mayor in the incumbent mayor's administration; (3) was a copartisan of the incumbent mayor; or (4) was a member of a party listed in the incumbent mayor's coalition. Across the 21 municipalities in the main estimation sample, we examine electoral results in 909 voting machines located in 190 polling locations (primarily schools), corresponding to a mean of 4.8 machines per location.[16] Given that ballot secrecy requires us to use aggregate vote counts at the voting-machine level, this breadth in the assignment of survey respondents across so many voting machines facilitates our estimation of treatment effects by increasing statistical power. On average, each machine in our sample had 338 registered voters in 2012, of which 260 cast a valid ballot for a mayoral candidate, 19 cast blank or invalid votes, and 59 abstained. Of all votes cast in these machines, incumbent mayoral candidates received an average of 118 votes (45 percent), and challengers received 142 votes (55 percent).

4.4 Rainfall Data

To examine the role of rainfall shocks, we gathered monthly precipitation data at the municipal level for the past quarter century (1986-2013) from the Climate Hazards Group Infrared Precipitation with Station database (Funk et al. 2015). 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.[17]

[16] The expanded sample discussed above includes 1,641 machines in 369 locations.

[17] Following Hidalgo et al. (2010), 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. We then standardize this measure to have mean zero and variance equal to one in each year of the sample.

5 Empirical Methodology

5.1 Research Design

Our sample consists of 1,308 households located across 425 neighborhood clusters. Randomization of water cisterns was performed across these neighborhood clusters (known as *logradouros* in Brazil) within municipalities. More specifically, in October 2011 clusters 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 assigned 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 for our key outcome variables. Our experiment is well-powered; for example, power calculations reveal that in analyses of citizen requests, it can detect a 2.5 percentage point effect at the 5 percent 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).

5.1.1 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.

5.1.2 Attrition

We observe a low level of household attrition across survey rounds. Appendix Table A1 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.[18] 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).

5.2 Empirical Strategy

Our 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.

5.2.1 Well-being

To the extent that the cisterns intervention and rainfall decrease vulnerability, they would also be expected to improve basic measures of household well-being. We thus collect household well-being measures and estimate:

$$y_{hcm} = \alpha_m + \beta_1 \cdot D_{cm} + \epsilon_{hcm}, \tag{1}$$

where y_{hcm} is a well-being 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. Errors ϵ_{hcm} are clustered at the cluster level cm.

[18] One of the primary reasons attrition may have differed across waves is that introductory scripts read aloud by enumerators estimated expected survey durations: 60 minutes for Wave 1, 30 minutes for Wave 2, and 90 minutes for Wave 3.

Similarly, to test whether households' well-being is affected by rainfall shocks, we estimate the bivariate relationship between well-being measures and rainfall shocks. Following the description in Section 4.4, we measure rainfall shocks (Z_m) as the deviation from average municipal rainfall in the year we collected each well-being measure. Given that rainfall is measured at the municipal level, the identification of coefficients in this estimation stems from cross-municipality variation in rainfall shocks. In this case, errors ϵ_{hcm} are clustered at the municipality level m.

5.2.2 Requests for Private Goods

Of central interest is whether the cisterns treatment and rainfall shocks affect individuals' requests for private goods from local politicians. To this end, we next estimate equation (2), 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 since treatment assignment was stratified by municipality. D_{cm} is a dummy indicating whether cluster c in municipality m was assigned to treatment. The coefficient of interest is β_1:

$$y_{ihcmt} = \alpha_m + \gamma_t + \beta_1 \cdot D_{cm} + \epsilon_{ihcmt}. \tag{2}$$

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. We also show estimates of equation (2) separating treatment effect estimates by year, in order to examine differences between electoral and non-electoral periods. We do so by interacting D_{cm} with two different year dummies denoted T_t, where t indexes the 2012 and 2013 survey wave years.

To test if citizens request private goods from politicians in response to rainfall shocks, we examine:

$$y_{ihcmt} = \alpha_m + \gamma_t + \phi_1 \cdot Z_{mt} + \epsilon_{ihcmt}, \tag{3}$$

where Z_{mt} is the deviation from average municipal rainfall (using the measure described in Section 4.4). The term ϕ_1 captures individuals' responses to varying rainfall. The inclusion of municipal fixed effects (α_m) is possible because we have two survey waves. As above, we also analyze effects of rainfall shocks by year. We also show a specification including both D_{cm} and Z_{mt} to examine the effects of both variables. Furthermore, we expand the model to allow for an interaction between the cisterns intervention and rainfall shocks, in order to shed light on whether cisterns affect citizen requests differently during droughts versus rainy periods.

5.2.3 Electoral Outcomes

We now turn to the second motivating question discussed in the Introduction: If vulnerability is a cause of clientelism, what are the electoral consequences for incumbents of reducing vulnerability? To shed light on this question, we employ extraordinarily granular official data: electoral outcomes at the voting-machine level for Brazil's 2012 mayoral elections. This section employs the voting machine as the unit of analysis (given ballot secrecy), and focuses exclusively on the cisterns intervention (as rainfall shocks are only measured at the municipal level).[19]As described above, we link survey respondents to the specific electronic voting machines to which they are assigned by electoral authorities. Our primary specification is as follows:

$$y_{slm} = \alpha_{lm} + \gamma_1 \cdot TV_{slm} + \gamma_2 \cdot EV_{slm} + \gamma_3 \cdot RV_{slm} + \epsilon_{slm}, \tag{4}$$

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

[19]By using official voting data, our electoral analyses sidestep issues of reliability with self-reported data found in numerous contexts (e.g., Katz and Katz 2010; Shachar and Eckstein 2007; Weir 1975).

authorities to vote in that particular machine. Other controls in the regression are EV_{slm}, the overall number of voters in our experimental sample 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 number of voters assigned to a machine.

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 location, citizens are assigned to a specific machine by electoral authorities.[20] 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.[21] Appendix Table A3 shows that neither the number of treated individuals nor the number of treated respondents in a voting machine predict average respondent characteristics such as age, gender, and schooling across machines within each voting location.

As discussed below, analyses show 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 measurement issues: (a) treatment effects for members of treated households who we cannot link to voting machines (e.g., registered voters in sampled households were only interviewed if present during our home visits); (b) spillover effects on neighbors' voting behavior (e.g., due

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

[21] This research design is used to measure spatial (direct and external) treatment effects, as in Miguel and Kremer (2004).

to sharing water with ineligible households); and (c) peer effects on voting behavior by neighbors in the cluster.[22] 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 rescale TV_{slm} and EV_{slm} to incorporate estimates of: (a) how many voting-age members of sampled households we cannot link to machines, (b) how many voters live in other households in the neighborhood cluster (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. Rescaling the TV_{slm} and EV_{slm} regressors addresses upward bias in the magnitude of the estimate of treatment effects on electoral outcomes. This procedure, which is described in Online Appendix D, 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.

For our electoral analysis, inference based on this research design relies on asymptotic approximations that assume a large number of clusters. We thus employ two methods that account for this design. First, we report cluster robust variance estimation (CRVE) standard errors from a model allowing for intracluster correlation at the voting location level. Second, we implement a more conservative approach that allows errors to be correlated across machines and locations within a municipality. This approach employs the wild cluster bootstrap-t procedure (Cameron, Gelbach, and Miller 2008), which addresses the limited number of clusters in our electoral analyses.

6 Results

This section employs the empirical strategy described above to investigate the effects of the cisterns intervention and rainfall shocks.

[22] E.g., studies in several countries find substantial positive peer effects on electoral behavior in voter education campaigns (see Nickerson 2008, Giné and Manzuri 2018, and Fafchamps, Vaz, and Vicente 2020).

6.1 Well-being

In Table 2, we estimate causal effects of the cisterns intervention and rainfall shocks on household well-being. Panel A focuses on cisterns, and Panel B focuses on rainfall.

The first well-being 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). As shown in column 1 of Panel A, the cisterns treatment leads to a reduction in depression of 0.09 units in 2013. This finding is significant at the 5 percent level and equivalent to 0.14 standard deviations in the CES-D 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, the cisterns assignment to treatment leads to an improvement of 0.08 units among treated households (significant at the 5 percent level), representing 0.14 standard deviations on the SRHS 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). Column 3 shows an improvement of similar magnitude (0.08) in this measure, 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 these well-being measures caused by cisterns (significant at the 1 percent level; column 4). By contrast, Appendix Table A4 shows that the cisterns treatment has no significant effect on wealth as measured by the value of owned durable goods, livestock, property and net

liquid savings.[23] Overall, this analysis confirms that the cisterns program improved the well-being of study participants, but had no discernible effect on self-reported wealth.

Next, we consider rainfall shocks in Panel B. Column 1 shows that a one standard deviation decrease in rainfall increases depression by 0.05 units, or about 0.07 standard deviations of the depression scale described above (significant at the 1 percent level). The second column indicates that a one standard deviation decrease in rainfall decreases self-reported health by 0.04 units, or about 0.08 standard deviations on the SHRS scale (significant at the 5 percent level). Column 3 shows that a one standard deviation decrease in rainfall worsens children's food security by 0.05 units, or about 0.05 standard deviations of the Child Food Security Index. The overall index of these three measures shows a 0.06 standard deviation reduction in these well-being measures for each one standard deviation decrease in rainfall (significant at the 1 percent level; column 4). Also indicative of how droughts undermine well-being, negative rainfall shocks are associated with lower household expenditures over the 30 days preceding the survey (significant at the 1 percent level; column 5). A one standard deviation decrease in rainfall reduces household expenditures by R$ 24.74 (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.[24]

Altogether, the evidence in Table 2 suggests that both the cisterns intervention and rainfall shocks significantly affect household well-being.

6.2 Requests for Private Goods

Next, Table 3 estimates causal effects of the cisterns intervention and rainfall shocks on citizen requests for private goods from local politicians. Column 1, which pools data

[23] Although one might expect cisterns to affect property values, rural Brazil exhibits substantial deficiencies in rural property registration as well as high transaction costs. In addition, we estimate that only 10 to 20 percent of households in our sample have formal land titles, based on name matching within each municipality using the federal rural land registry (INCRA's *Sistema Nacional de Cadastro Rural*).

[24] Column 4 is analyzed for rainfall shocks but not the cisterns intervention, because expenditure data were only collected in the 2011 localization survey. The data reflect 2011 Brazilian reais.

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 cisterns intervention reduces the likelihood that citizens request such benefits by 3.0 percentage points (17 percent of the control group mean; significant at the 5 percent level). Column 2 examines rainfall shocks: a one standard deviation decrease in municipal rainfall increases citizen requests by 2.3 percentage points (13 percent of the control group mean; significant at the 5 percent level). Next, column 3 includes both regressors simultaneously and shows that estimated coefficients and precision remain unchanged. In column 4, we add an interaction term between the cisterns intervention and rainfall shocks, and find that the coefficient is small and statistically insignificant. Cisterns' ability to both harvest and store water may explain why the technology similarly reduces citizen requests in both good and bad states. During rainy periods, cisterns enable citizens to harvest and consume water from their rooftops. And during droughts, cisterns enable citizens to store up to 16,000 liters delivered by water trucks, as well as to consume water harvested during earlier rainy periods.

We also investigate differences between the 2012 electoral year and the 2013 post-electoral year. For cisterns, Column 5 shows that similar patterns hold when estimating year-specific effects: the cisterns treatment effect on requests is stable at approximately 3 percentage points (significant at the 10 and 5 percent levels in 2012 and 2013, respectively). We cannot reject the hypothesis that the effect is identical in both years (p-value = 0.91). The fact that this reduction in requests is of the same magnitude outside of the electoral period suggests that the cisterns intervention has longer-term effects, rather than just short-term effects around elections. For rainfall shocks, Column 6 finds a significantly stronger effect on requests in 2012 vs 2013 (-0.042 vs -0.004, respectively). Further investigation reveals this finding is likely an artifact of differing aggregate rainfall conditions; considerably more municipalities experienced substantial negative rainfall shocks in 2012 than in 2013 (see Appendix Figure A1). Column 7 examines an analogous specification with requests for all private goods excluding water as the outcome variable. Find-

ings are robust when focusing on non-water requests, with significance at the 5 and 10 percent level; estimates are mechanically smaller given the exclusion of water requests.[25]

While results so far focus on requests for private goods, they leave open the question of whether individuals substituted requests of private goods for that of public goods. We investigate this issue in column 8, employing an analogous specification with requests for public goods as the outcome variable. We code requests as involving public goods if a respondent asks 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). These types of requests are relatively rare (only 2.7 percent of control group respondents made such requests). We find no evidence that the cisterns treatment or rainfall shocks cause a substitution of requests towards public goods; the estimated coefficients are small and cannot be distinguished from zero. More broadly, these results suggest that both the cisterns treatment and rainfall shocks cause a decrease in requests for private goods from politicians, without considerable substitution towards public good requests.

6.3 Electoral Outcomes

In order to investigate whether reduced vulnerability undermines citizens' support for incumbents, we now follow the empirical strategy in Section 5.2.3 to examine how the cisterns treatment affects incumbent votes in Brazil's 2012 mayoral election. As discussed above, this section employs electoral data at the electronic voting machine level, and focuses exclusively on the cisterns intervention (as rainfall shocks are measured per municipality). Recall that our survey links individual subjects in the cisterns experiment to their specific voting machine. To measure electoral responses to the cisterns treatment, we can thus compare votes across machines — which have distinct, randomly assigned numbers of treated individuals — located in the same polling places.

[25] For completeness, we also employed an instrumental variable approach in which assignment to treatment is used as an instrument for receiving a cistern. As expected, estimated coefficients are amplified in proportion to the degree of compliance, and statistical significance of main results remains unchanged.

Table 4 presents our main results estimating the effect of the cisterns intervention on incumbent votes and other electoral outcomes. Column 1 reports that for every additional respondent assigned to the treatment condition, the incumbent mayor receives 0.10 fewer votes (bootstrap p-value = 0.04). Column 2 expands the sample beyond mayors running for reelection to consider the mayoral candidate of the incumbent group (as defined in Section 4.3). The coefficient remains very close to that of Column 1: for every additional respondent assigned to the treatment condition, the incumbent group receives 0.08 fewer votes (bootstrap p-value = 0.09). Both findings suggest that the cisterns intervention — which exogenously decreased vulnerability — caused a reduction in incumbent votes in the 2012 municipal campaign.

We next investigate whether treatment effects, which suggest a fall in incumbent votes, translate to an increase in votes for mayors' challengers. We employ a specification analogous to column 1, using 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 similar magnitude — but with the opposite sign — as the estimate for incumbent votes. For every additional respondent assigned to the treatment condition, votes for challenger candidates increase by 0.10 (bootstrap p-value = 0.09). We also report treatment effects on voter turnout (column 4), as well as blank and null votes (column 5); both are small and statistically indistinguishable from zero.

7 Clientelism as a Mechanism

Thus far, findings in this study provide substantial evidence that reducing vulnerability causes a decline in citizen requests and votes for incumbents. As discussed in the Introduction, these results are consistent with our extension of Anderson, Francois, and Kotwal's (2015) model of clientelism as informal insurance: a reduction in vulnerability is expected to decrease citizens' participation in clientelism and reduce votes for clientelist politicians. We now conduct additional analyses to test our argument that clientelism is

indeed a key mechanism explaining these results, and then consider alternative explanations in the subsequent section. First, we examine whether the cisterns intervention disproportionately reduces requests for private goods by citizens who are likely to be in clientelist relationships, employing the following model:

$$y_{ihcmt} = \alpha_m + \gamma_t + \beta_1 \cdot D_{cm} + \beta_2 \cdot (D_{cm} \cdot R_{ihcm}) + \beta_3 \cdot R_{ihcm} + \epsilon_{ihcmt}. \quad (5)$$

where R_{ihcm} is a marker that an individual is likely to be in a clientelist relationship, and the other variables remain as previously defined. Secondarily, we also estimate an analogous regression for rainfall shocks, replacing each D_{cm} with Z_{mt} in Equation 5.

The extant literature lacks a well-established marker for whether citizens are likely to be involved in clientelist relationships, so we use a binary proxy. This proxy is defined as one if the respondent conversed at least monthly with a local politician *before* the 2012 electoral campaign began. Such frequent interactions facilitate the face-to-face exchanges between citizens and elites that are a hallmark of ongoing clientelist relationships. While this marker does not definitively identify contingent exchanges, citizens exhibiting such behavior are more likely to be in clientelist relationships than those who do not.[26] The Online Appendix shows shows that this proxy is balanced across treatment and control groups, and does not merely serve as a proxy for vulnerability or other important characteristics.[27]

Consistent with the clientelist mechanism, the first column of Table 5 shows that the cistern treatment's effect on requests is concentrated among citizens with the clientelism marker. The reduction in requests among citizens with frequent interactions with politicians is 10.9 percentage points (significant at the 1 percent level), but is indistinguishable from zero for citizens without such interactions. Column 2 examines heterogeneous ef-

[26] Consistent with clientelist relationships, citizens with the marker are more likely to vote, have all household members vote for the same candidate, receive campaign visits, and declare support.

[27] See Appendix Table A5 and Figure A2. Citizens with the marker have well-being measures, as well as expenditures and wealth per capita, that are statistically indistinguishable from other citizens. Bivariate regression coefficients in Table A6 suggest citizens do not form these relationships in response to recent rainfall shocks.

fects of the negative rainfall shock. A one standard deviation shock increases requests by 3.5 percentage points among citizens with the clientelism marker, compared to only 2.0 percentage points among citizens without the marker. The change in requests among the clientelist subgroup is significant for both cisterns and rainfall; their difference from the non-clientelist subgroup is only significant for the cisterns treatment.[28] Column 3 suggests that these empirical patterns are robust when examining the cisterns treatment and rainfall in the same specification. Next, column 4 shows a specification ignoring requests for water. We observe that the cistern treatment's effects are concentrated among those with the clientelism marker, though the estimated coefficient is mechanically smaller given the exclusion of water requests. By contrast, the effects of rainfall are not significantly greater for citizens with the marker (though the sign follows expectations).

Column 5 examines an analogous specification with an alternative dependent variable: a dummy coded 1 if the individual requested a private good from a local politician *and* that good was received. Among citizens who frequently interact with politicians, the cisterns treatment reduced fulfilled requests by 6.2 percentage points, whereas negative rainfall shocks increased them by 2.7 percentage points (both significant at the 5 percent level). Among citizens without such interactions, both effects are small and insignificant. The difference between subgroups is significant for both cisterns and rainfall (at the 1 and 10 percent level, respectively).

One might be concerned, however, that the clientelism marker could potentially reflect citizens' general engagement with politics, rather than their clientelist relationships with specific politicians. To alleviate this concern, Appendix Table A7 directly controls for measures of citizen engagement and their interactions with the cisterns treatment as well as with rainfall shocks. More specifically, these measures are: (a) whether the respondent is a member of a community association, (b) whether the respondent is the

[28]Our data show that cisterns did not significantly affect requests from family, friends/neighbors, or civil society organizations. For rainfall, effects are insignificant for family and civil society organizations, and significant at only the 10 percent level for friends/neighbors (not shown). These findings are consistent with the clientelism mechanism, as opposed to reduced vulnerability merely decreasing requests more generally.

president of a community association, and (c) whether the respondent voted in the 2008 municipal election. The table reports our main specification from Table 3, showing results described above are stable when controlling for these different community engagement measures separately (columns 1-3) as well as jointly (column 4).

Our electoral analyses also provide evidence consistent with clientelism. First, we emphasize that the cisterns treatment reduced incumbent votes, as predicted if clientelism is a form of informal insurance. Second, as expected if clientelism involves informal insurance transfers, the cisterns treatment had larger effects on incumbent votes than on citizen requests. In the parlance of insurance, votes are "premiums" paid by citizens for risk protection, whereas requests are "claims" made by citizens who experience adverse shocks. Insurance premiums are generally more prevalent than claims; as such, the cisterns treatment — which undercuts this form of informal insurance — reduces votes more than it reduces requests. Third, Appendix Table A8 suggests the cistern treatment's reduction in incumbent votes is also concentrated among citizens likely to be in clientelist relationships. Indeed, the estimated coefficient on treated individuals with the clientelism marker is -0.27 (significant at the 10 percent level), versus only -.05 (and statistically insignificant) for treated individuals without the clientelism marker.

Altogether, these findings point toward clientelism as a key mechanism explaining our results. Reduced vulnerability decreased citizens' requests of local politicians, especially among citizens with a marker that suggests they are likely to be in clientelist relationships. Furthermore, citizens experienced a decrease in *fulfilled* requests. And as predicted, reduced vulnerability not only decreased citizens' participation in clientelism, but also reduced their votes for incumbent mayors (who typically have more resources for clientelism).

8 Alternative Explanations

8.1 Politician Responses

We now consider several possible alternative explanations. First, consider our argument that reduced vulnerability decreased citizens' requests for private goods from local politicians. One might argue that this decline in requests is partially reflective of local politicians changing their clientelist strategies in response to citizens' decreased vulnerability. 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).

With regard to cisterns, it should be emphasized that even though our intervention substantially reduced the vulnerability of recipient households, it represents a low degree of saturation 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 14 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.

Panel A of Appendix Table A9 examines whether any differences can be detected between politicians' actions towards citizens assigned to the treatment versus control groups. With respect to elite targeting strategies, column 1 shows that politicians and their representatives were no more or less likely to visit the homes of treated subjects during the 2012 political campaign. Column 2 suggests that during those politician-initiated campaign 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 3 shows no significant difference in handouts offered by politicians to citizens in the treatment versus control groups. We also inquired

of all respondents whether they had accepted a handout offered to them by a politician in exchange for their vote. Column 4 shows that respondents assigned to the cisterns treatment were not more or less likely than those assigned to the control group to accept a politician's handout. More broadly, we find no evidence that politicians responded differently to citizens depending on their cistern treatment assignment, corroborating our interpretation that our findings reflect citizens' (rather than politicians') responses to the cistern intervention.[29]

With regards to rainfall, Panel B employs symmetric analyses using rainfall shocks as the explanatory variable instead of cisterns assignment. An important caveat: since dependent variables in Appendix Table A9 only inquired about the election year, this examination of rainfall shocks exclusively uses cross-sectional variation and specifications no longer include municipality fixed effects. Column 1 finds no relationship between rainfall shocks and campaign visits to respondents' households. During these politician-initiated visits to households, column 2 finds no relationship between rainfall shocks and the distribution of campaign handouts. By contrast, in municipalities experiencing negative rainfall shocks, respondents were significantly more likely to report that they were offered and accepted campaign handouts from politicians (columns 3 and 4, respectively).

In sum, evidence points against this alternative explanation for the cisterns treatment (which is assigned to specific households), but not for rainfall shocks (which have far broader effects). For cisterns, Panel A finds no evidence that politicians responded differently to citizens depending on their treatment assignment, corroborating our interpretation that our findings reflect citizens' (rather than politicians') responses to this reduction in vulnerability. For rainfall, evidence is consistent with droughts inducing both demand-side and supply-side clientelist responses — highlighting the advantage of our cistern intervention's low saturation within municipalities.

[29] To be clear, we do not claim politicians' strategies would remain unchanged when overall vulnerability in their districts declines. Rather, we argue our intervention was so small that it did not change politicians' strategies. Data are consistent with this argument, which is sufficient to study our key hypotheses.

8.2 Credit Claiming and Political Alignment

We now turn to a possible alternative explanation for our electoral results, which focuses specifically on the cisterns intervention. One might argue 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). For example, 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 the treatment effects on electoral behavior are consistent with this alternative explanation. In Appendix Table A8, column 3 shows that the cisterns treatment does not lead to more votes for mayoral candidates who were members of President Rousseff's left-leaning Workers' Party (*Partido dos Trabalhadores*, or PT). We also find no support for the hypotheses that the treatment induces credit claiming and support for candidates from the president's or governor's coalition (see columns 4-5).[30] Furthermore, column 6 reports the estimated treatment effect on right-leaning candidates, as measured by Power and Rodrigues-Silveira's (2020) party classification. The point estimate is again small and insignificant, suggesting no greater (or lower) support for right-leaning candidates. These results are unsurprising given that the cisterns intervention involved in this study was financed by an international development agency, not by the government. Overall, our findings do not point to credit claiming or misattribution.

[30]Further analyses show that the cisterns treatment has null effects on votes for mayoral candidates with distinct sociodemographic characteristics (e.g., for particular genders, ages, or educational attainment). In addition, treatment effects on requests do not differ between municipalities with and without PT mayors.

8.3 Citizen Beliefs about the Incumbent Mayor

We now turn to another possible alternative explanation for our electoral results, which similarly focuses specifically on the cisterns intervention. The cisterns treatment might affect citizens' beliefs about qualities of the incumbent mayor, thereby influencing their vote choices. Beyond clientelism, citizens may also consider valence issues when casting ballots, such as candidates' competence, honesty, and broadly desired policies. If cistern recipients negatively update their beliefs regarding the incumbent mayor's attributes — for instance, because our non-governmental intervention is viewed as meeting needs that should have been addressed by the municipal government — recipients might shift votes away from the incumbent.

To evaluate this alternative argument, we analyze survey questions in our 2012 wave about citizens' perceptions of their incumbent mayor's honesty and competence. In particular, we inquired whether respondents "strongly agreed," "agreed," "disagreed," or "strongly disagreed" that the incumbent politician was competent and honest, respectively. We construct indicator variables for respondents who agreed or strongly agreed with these characteristics, and estimate the effects of the cisterns treatment on these measures. One caveat is that whereas our electoral analyses employ official data on voting outcomes, these self-reported data for perceptions of the incumbent may suffer from social desirability bias.

Appendix Table A9 estimates treatment effects for these measures. As shown in columns 5-6, the cisterns treatment has no significant effects on citizens' perceptions of their incumbent mayor's competence or honesty. Moreover, the point estimates are positive (albeit imprecisely estimated), suggesting that the intervention did not lead to downward assessments of incumbent mayors. In column 7, we find similar results when examining an analogous question about the perceived competence of the incumbent mayor's

group.[31] Overall, the data point away from the argument that our results stem from effects on citizen beliefs about the incumbent mayor.

8.4 Citizen Preferences

Beyond beliefs about incumbents, another potential alternative explanation for our electoral results is that reduced vulnerability affects other types of citizen preferences — much as the titling literature documents changes to individuals' economic and political preferences (e.g., Di Tella et al. 2007; Field 2007; de Janvry, Gonzalez-Navarro, and Sadoulet 2014). Thus, we test if the cisterns treatment or rainfall shocks affect several economic preferences and behaviors that could arguably drive vote choices. More specifically, we examine five measures obtained through hypothetical or incentivized games in the 2012 and 2013 survey rounds for: risk aversion, altruism, reciprocity, time preferences and contributions in a public good game.[32]

With regards to the cisterns intervention, Panel A of Appendix Table A10 shows that for four measures, treatment effects are statistically indistinguishable from zero. The only exception is a decrease in respondents' time discount rates, which is significant at only the 10 percent level in 2012, and is insignificant in 2013 (columns 7-8). With regards to rainfall shocks, Panel B shows insignificant effects for three of the five measures: risk aversion, altruism, and time preferences. For reciprocity, we observe unstable coefficients, which are significant in 2012 but not 2013 (columns 5-6). For the one survey wave including a public goods game, droughts reduced citizens' contributions, a finding significant at the 10 percent level (column 9). Overall, evidence largely points against this alternative explanation of reduced vulnerability affecting citizens' preferences — especially for the cisterns intervention.

[31] The survey did not ask about the perceived honesty of the incumbent mayor's group.
[32] Online Appendix E describes the construction of these measures.

9 Conclusion

This paper has investigated the relationship between economic vulnerability and citizens' participation in clientelism. We conducted a novel, three-year panel survey of rural households in Northeast Brazil, a drought-prone region where vulnerability is prevalent. Unlike previous research, our representative survey explores various aspects of household vulnerability and measures citizens' interactions with local politicians before, during, and after elections. In order to examine how reduced vulnerability affects clientelism, we employ two exogenous random shocks to vulnerability. First, we use a randomized control trial designed to reduce the vulnerability of sampled households; in this development intervention, we constructed residential water cisterns randomized across 425 neighborhood clusters in 40 municipalities. Second, we exploit municipal-level rainfall shocks, which also exogenously affect household vulnerability.

Our study's investigation of both shocks to vulnerability yields several important findings. With regards to our intervention, the cisterns treatment decreased citizens' requests for private goods from politicians, even when excluding water-related requests. This technology — which increases long-term water availability — decreased citizens' requests not only during the election campaign, but also a full year later. The cisterns treatment disproportionately reduced requests by respondents with a marker suggesting they are likely to be in clientelist relationships. In parallel, we find that negative rainfall shocks increase citizens' requests for private goods from politicians, even when excluding water-related requests. As with cisterns, rainfall's effect on requests is amplified among citizens with the clientelism marker. Because our cisterns intervention was specifically designed to reduce vulnerability at the household level, we are also able to analyze extraordinarily granular data on voting outcomes. We link individual subjects to their specific electronic voting machines, and find that the cisterns treatment significantly decreased votes for incumbent mayors. These electoral effects are magnified for citizens with the clientelism marker. Overall, these findings are consistent with the argument that

reduced vulnerability undermines clientelist relationships and thereby impinges on the electoral performance of incumbents.

More broadly, this study advances the scholarly literature by providing compelling evidence that vulnerability is a key determinant of citizens' participation in clientelism. Unlike most research that focuses on poverty, our analysis of vulnerability underscores that both the level and variability of consumption affect clientelism. Beyond our study's primary contribution of emphasizing the role of this understudied determinant of clientelism, its findings are also relevant for policy efforts to reduce the phenomenon. When considering policy implications, it is important to note that the cisterns intervention focuses on how reducing household vulnerability affects citizens' participation in clientelism. As shown, a key benefit of installing few cisterns per municipality is that it did not influence politicians' strategies. Further research is needed to explore potential effects of scaling up such programs on politicians' behavior in ongoing clientelist relationships, as well as on their use of vote buying and other campaign strategies beyond the focus of the present study.

With respect to citizens, our intervention suggests that reducing vulnerability may provide another potential modality to fight 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. 2018). Our study provides rigorous evidence that improving citizens' livelihoods can undercut their willingness to participate in contingent exchanges. Further research in various contexts 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.

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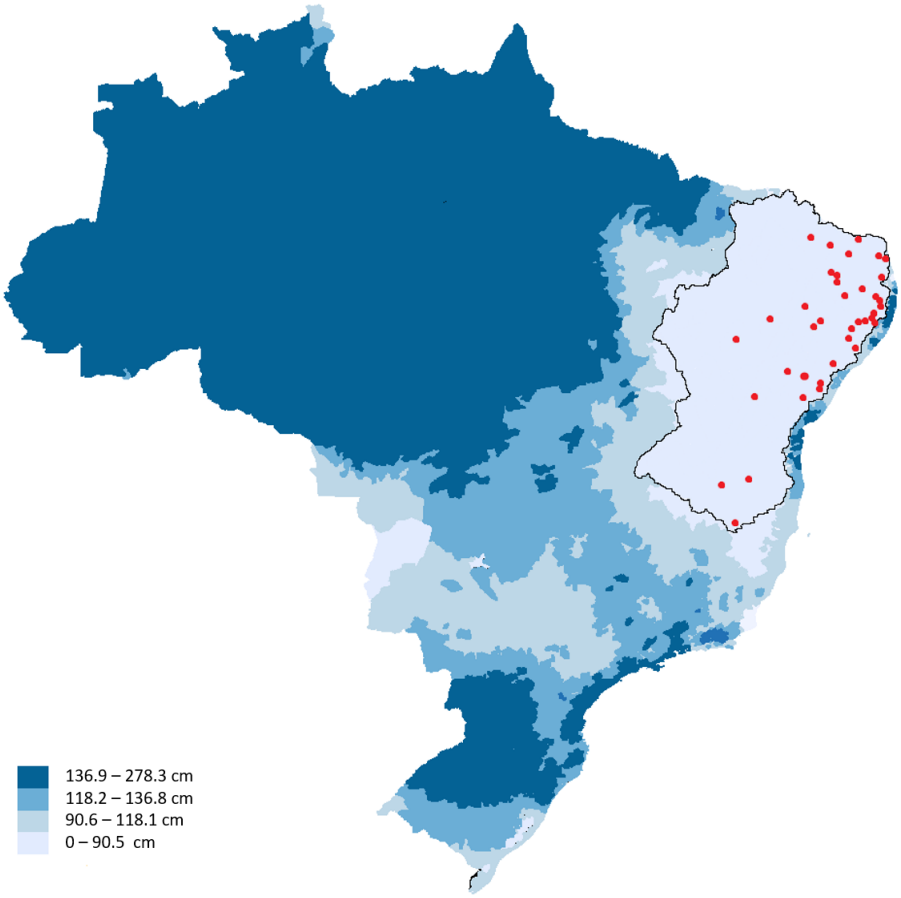
Figures and Tables

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



Notes: During our study period, Brazil's semi-arid region consisted 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

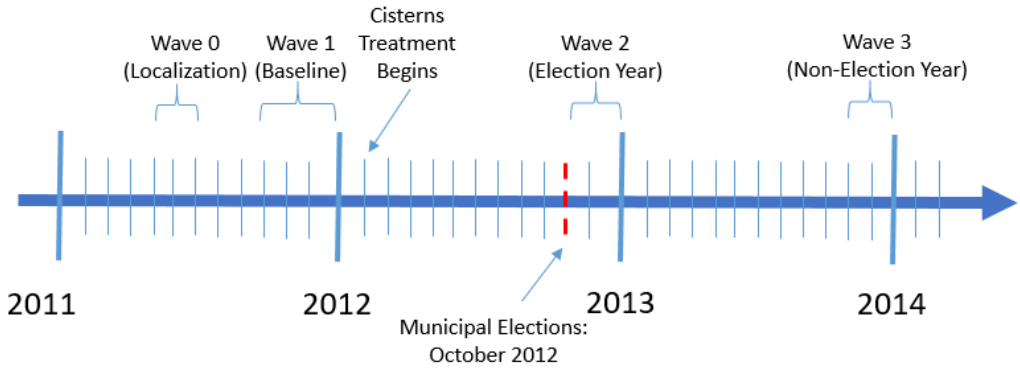


Table 1: Interactions with Politicians

Variable	Mean (1)	Relationship with Rainfall (2)
Request Private Good from Politician, 2012	0.213 [0.409]	-0.036*** (0.010)
Request Private Good from Politician, 2013	0.086 [0.280]	-0.003 (0.007)
Request and Receive Private Good from Politician, 2012	0.124 [0.330]	-0.021*** (0.008)
Request and Receive Private Good from Politician, 2013	0.035 [0.184]	-0.001 (0.005)
Frequent Interactions with Local Politician (Outside of Campaign)	0.184 [0.387]	-0.010 (0.008)
Received Visit from Representatives of Any Mayoral Candidate	0.696 [0.460]	0.015 (0.011)
Voted for Mayor/Councilor of the Same Coalition	0.718 [0.450]	-0.019 (0.015)
All Household Members Voting for the Same Mayoral Candidate	0.773 [0.419]	-0.023** (0.011)
Any Declared Support	0.485 [0.500]	-0.066*** (0.016)
Declared Support on Body (Sticker or Shirt)	0.185 [0.388]	-0.021** (0.009)
Declared Support on House (Flag, Banner, or Painted Wall)	0.387 [0.487]	-0.059*** (0.016)
Declared Support at Rally (Attended and Displayed Paraphernalia)	0.218 [0.413]	-0.036*** (0.010)

Notes: Column 1 presents the mean of each variable and its standard deviation in brackets. Column 2 reports coefficients from regressing each measure on rainfall. Rainfall is measured as the difference between rainfall in January-September of the relevant year (2012 unless otherwise stated) and its historical municipal mean during identical months in 1986-2011, divided by the municipality's historical monthly standard deviation of rainfall (see Section 4.4). Frequent interactions with local politician is coded 1 if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise. Standard errors are clustered at the neighborhood level and reported in parentheses; *10%,** 5%,*** 1% significance levels.

Table 2: Cisterns Treatment, Rainfall Shocks, and Well-Being

	-(CES-D) Scale (2013) (1)	SRHS Index (2013) (2)	Child Food Security Index (2013) (3)	Overall Index (2013) (4)	Total Household Expenditure (2011) (5)
Panel A					
Cisterns Treatment	0.092**	0.075**	0.084	0.126***	
	(0.037)	(0.033)	(0.054)	(0.043)	
Municipality Fixed Effects	Yes	Yes	Yes	Yes	
Panel B					
Rainfall Shock	0.046***	0.039**	0.046*	0.064***	24.736***
	(0.016)	(0.017)	(0.026)	(0.019)	(6.657)
Municipality Fixed Effects	No	No	No	No	No
Observations	1,128	1,052	1,128	1,128	1,281
Mean of Y: Treatment Group	3.377	2.868	-0.277	0.061	364.467
Mean of Y: Control Group	3.289	2.791	-0.340	-0.056	370.877
Standard Deviation of Y: Control Group	0.646	0.535	0.990	0.714	207.744

Notes: Panels A and B show results from separate regressions. Each column reports the coefficient from regressing each vulnerability measure on cisterns treatment with municipality fixed effects (in Panel A), and on rainfall with no fixed effects (in Panel B). Standard errors are clustered at the neighborhood level and reported in parentheses; *10%,** 5%,*** 1% significance levels. Expenditure variables are omitted from Panel A as they are measured in 2011, prior to the cisterns treatment. All other variables are measured in 2013. 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 higher values indicate better food security for children. The Overall Vulnerability Index is the unweighted mean of standardized values of 2013 indices. Cisterns treatment is coded 1 if respondent's household is in a neighborhood cluster selected for treatment; 0 otherwise. Rainfall shock is measured as the difference between rainfall in the months before the survey of the relevant year (January-September for 2013 variables; January-April for 2011 variables) and its historical municipal mean during identical months in prior years (1986-2011 for 2013 variables; 1986-2010 for 2011 variables), divided by the municipality's historical monthly standard deviation of rainfall (see Section 4.4).

Table 3: Citizen Requests, Cisterns Treatment, and Rainfall Shocks

	Request Any Private Good						Request Any Private Good Excluding Water	Request Any Public Good
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β_1: Cisterns Treatment	-0.030**		-0.030**	-0.030**			-0.027**	-0.005
	(0.013)		(0.013)	(0.013)			(0.012)	(0.005)
β_2: Rainfall Shock		-0.023**	-0.023**	-0.021*			-0.015*	-0.007
		(0.010)	(0.010)	(0.011)			(0.009)	(0.004)
β_3: Cisterns Treatment × Rainfall Shock				-0.004				
				(0.012)				
β_4: Cisterns Treatment × 2012					-0.029*			
					(0.017)			
β_5: Cisterns Treatment × 2013					-0.031**			
					(0.016)			
β_6: Rainfall Shock × 2012						-0.042***		
						(0.011)		
β_7: Rainfall Shock × 2013						-0.004		
						(0.013)		
Test of homogeneous effects in 2012 and 2013 (p-value):								
Cisterns Treatment (H_0: $\beta_4 = \beta_5$)					0.910			
Rainfall Shock (H_0: $\beta_6 = \beta_7$)						0.011		
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4,288	4,288	4,288	4,288	4,288	4,288	4,288	4,288
Mean of Y: Treatment Group	0.149	0.149	0.149	0.149	0.149	0.149	0.120	0.020
Mean of Y: Control Group	0.177	0.177	0.177	0.177	0.177	0.177	0.146	0.027

Notes: Outcome variable is coded 1 if respondent reported requesting goods shown in the column header from a local politician in 2012 or 2013; 0 otherwise. Specifications employ pooled data to examine requests in either year. Cisterns treatment is coded 1 if respondent's household is in a neighborhood cluster selected for treatment; 0 otherwise. Rainfall shock is measured as the difference between rainfall in January-September of the relevant year and its historical municipal mean during identical months in 1986-2011, divided by the municipality's historical monthly standard deviation of rainfall (see Section 4.4). Standard errors are clustered at the neighborhood level and reported in parentheses; *10%,** 5%,*** 1% significance levels.

Table 4: Cisterns Treatment and Electoral Outcomes (2012)

	Votes for Incumbent Mayor (1)	Votes for Incumbent Group (2)	Votes for Challenger Candidate (3)	Turnout (4)	Blank and Null Votes (5)
Treated Individuals	-0.101*	-0.076	0.098	-0.009	-0.006
	(0.058)	(0.049)	(0.073)	(0.059)	(0.030)
	[0.041]	[0.093]	[0.087]	[0.853]	[0.864]
Respondents	0.022	0.036	-0.033	-0.001	0.011
	(0.044)	(0.038)	(0.058)	(0.049)	(0.020)
	[0.520]	[0.316]	[0.458]	[0.993]	[0.580]
Control for Registered Voters	Yes	Yes	Yes	Yes	Yes
Location Fixed Effects	Yes	Yes	Yes	Yes	Yes
Rescaled Regressors	Yes	Yes	Yes	Yes	Yes
Observations	909	1,641	909	909	909

Notes: Dependent variables are the number of votes cast as indicated in the column headers. All columns except column 2 use the sample of 21 municipalities in which the incumbent mayor ran for reelection in 2012. Column 2 uses the sample of 39 municipalities where a 2012 mayoral candidate meets one of the following criteria: (1) is the incumbent mayor; (2) is vice-mayor in the incumbent mayor's administration; (3) is a copartisan of the incumbent mayor; or (4) is a member of a party listed in the incumbent mayor's coalition. Standard errors clustered at the voting location are reported in parentheses; *10%,** 5%,*** 1% significance levels. P-values from a wild cluster bootstrap-t procedure, which allows for error terms to be correlated within municipalities, are reported in brackets.

Table 5: Citizen Requests and Heterogeneity by Clientelist Relationship

	Request Any Private Good			Request Any Private Good Excluding Water	Request and Receive Any Private Good
	(1)	(2)	(3)	(4)	(5)
β_1: Cisterns Treatment	-0.012		-0.012	-0.016	0.005
	(0.013)		(0.013)	(0.012)	(0.010)
β_2: Cisterns Treatment × Clientelist Relationship	-0.097***		-0.095***	-0.056*	-0.068***
	(0.034)		(0.034)	(0.032)	(0.025)
β_3: Rainfall Shock		-0.020*	-0.021*	-0.013	-0.007
		(0.011)	(0.011)	(0.009)	(0.009)
β_4: Rainfall Shock × Clientelist Relationship		-0.014	-0.012	-0.006	-0.020*
		(0.016)	(0.016)	(0.015)	(0.012)
β_5: Clientelist Relationship	0.119***	0.071***	0.118***	0.080***	0.075***
	(0.026)	(0.018)	(0.026)	(0.025)	(0.020)
Effect of Cisterns Treatment for Individuals in Clientelist Relationship:					
$\beta_1 + \beta_2$	-0.109***		-0.108***	-0.072**	-0.062**
	(0.032)		(0.032)	(0.031)	(0.024)
Effect of Positive 1 SD Rainfall Shock for Individuals in Clientelist Relationship:					
$\beta_3 + \beta_4$		-0.035**	-0.032**	-0.019	-0.027**
		(0.016)	(0.015)	(0.014)	(0.013)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	4,288	4,288	4,288	4,288	4,284
Mean of Y: Treatment Group	0.149	0.149	0.149	0.120	0.089
Mean of Y: Control Group	0.177	0.177	0.177	0.146	0.094
Mean of Y: Clientelist Relationship in Control Group	0.285	0.285	0.285	0.225	0.169

Notes: Outcome variable is coded 1 if respondent reported: requesting a private good (columns 1-3) /requesting a private good excluding water (column 4) / requesting and receiving a private good (column 5) from a local politician in 2012 or 2013; 0 otherwise. Specifications employ pooled data to examine requests in either year. Cisterns treatment is coded 1 if respondent's household is in a neighborhood cluster selected for treatment; 0 otherwise. Rainfall shock is measured as the difference between rainfall in January-September of the relevant year and its historical municipal mean during identical months in 1986-2011, divided by the municipality's historical monthly standard deviation of rainfall (see Section 4.4). Marker for clientelist relationship is coded 1 if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see Section 7). Standard errors are clustered at the neighborhood level and reported in parentheses; *10%,** 5%,*** 1% significance levels.

Online Appendix (Not for Publication)

Appendix A: Model of Vulnerability and Clientelism

As discussed in the Introduction, our experimental study is theoretically motivated by Anderson, Francois, and Kotwal's (2015) [henceforth AFK] model of clientelism as informal insurance. In their model, clientelist politicians undermine policies for poor and vulnerable households, so that they can facilitate clientelist arrangements. These clientelist arrangements involve informal insurance transfers — more specifically, in contingent exchange for votes, clientelist politicians provide transfers to particular citizens if they experience negative shocks. Clientelist politicians make such arrangements in order to increase the likelihood that they win election, and they provide lower levels of public goods while in office to extract rents that can be partially used for these clientelist transfers. We extend this model to examine implications when an intervention — such as our water cisterns — reduces vulnerability by providing an independent risk-coping mechanism that affects both the level and variability of consumption. Consistent with our empirical results, the model predicts that the exogenous reduction in citizens' vulnerability from an independent risk-coping mechanism causes a decrease in votes traded in exchange for state-contingent clientelist transfers.

The Model

Setup

Each individual l is either a citizen i ($i \in M$) or a politician j ($j \in P$); there are $2n$ citizens and a number of politicians normalized to size 1 in the municipality, where $1 << n$. Each individual has type regarding clientelism (denoted c_l), either $c_l = C$, or $c_l = N$, denoting clientelist and non-clientelist types (respectively). Each agent is thus identified by their political class (M, P) and clientelism type (C, N). Citizens own negligible land or capital and make private good consumption decisions from an exogenous source of state-

contingent income (y_s), where $s \in \{g, b\}$ respectively denote the good and bad states of the world; the latter occurs with probability $\mu \in (0, 1)$. They also enjoy utility from the consumption of a public good (G) provided by the government. Clientelist citizens ($c_i = C$) tend to have stronger relationships with clientelist politicians ($c_j = C$) than do non-clientelist citizens ($c_i = N$); this will be formally specified below.

Officeholders are tasked with providing pro-poor public goods to citizens. There are two coalitions of politicians: incumbents and challengers. The incumbent coalition has access to existing government revenue from federal transfers (T). Following AFK, we assume that all politicians in the incumbent coalition are clientelist types who expend this exogenous revenue stream on public goods (G) and pecuniary rents (R). All politicians enjoy ego rents (E) from office. We also assume that clientelist types — unlike non-clientelist types — extract pecuniary rents R while in office in part to fund clientelist transactions described below. Also following AFK, we assume that when in office, the clientelist coalition's expenditure on public goods ($\tilde{G}$) is strictly lower than the non-clientelist coalition's expenditure on public goods (G). We assume that the challenging coalition is composed of non-clientelist types.[33]

Citizens have additively-separable preferences over the consumption of the private good consumed from state-contingent income (y_s) and from S_i^j, a possible insurance

[33]This assumption is a simplification of a more complex scenario in which challengers could be clientelist or non-clientelist, with their type drawn at random from the pool of potential politicians. In this alternative scenario, clientelist opposition candidates may engage in vote trading with citizens via similar insurance promises. If their types are known to citizens, this complicates the analysis in the model but does not affect the theoretical results. While such modeling assumptions do not precisely match reality, our survey data do suggest that incumbent candidates are indeed more clientelistic than challengers. As discussed in Section 2, control-group respondents were more likely to have received private benefits from incumbents than from challengers during both survey waves. Given that these patterns may reflect incumbents' disproportionate access to resources, we also consider questions in the 2012 wave about citizens' perceptions of both incumbent and challenger candidates in that year's election. In particular, we inquired whether respondents "strongly agreed," "agreed," "disagreed," or "strongly disagreed" that these candidates were competent and honest, respectively. Whereas 83.5 percent of control-group respondents perceived challengers as honest, only 71.1 percent perceived incumbents as honest — a stark difference of 12.4 percentage points (17.4 percent in proportional terms; p-value < 0.001). Analogously, 77.2 percent of control-group respondents perceived challengers as competent, compared to only 67.2 percent for incumbents — a substantial difference of 10 percentage points (14.9 percent in proportional terms; p-value < 0.001).

transfer from clientelist politician j to citizen i, the aforementioned public good, and idiosyncratic preferences for the incumbent coalition:

$$U_{ik}(C_{jk}) = v(\widetilde{G}) + \mu u(y_b + S_i^j) + (1-\mu)u(y_g) + \phi_k \tag{6}$$

where U_{ik} denotes the expected utility outcome corresponding to the coalition in parentheses controlling the municipal government, in this case a clientelist government. Citizens exhibit decreasing marginal utility (and risk aversion) over the consumption of the private good ($u' > 0$, $u'' < 0$) and the public good ($v' > 0$, $v'' < 0$). The ϕ_k term, drawn from distribution $g(\phi_k)$, represents the citizens' idiosyncratic preferences for the incumbent coalition j in municipality k; for instance, a higher quality group of clientelist candidates increases ϕ_k.

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

To maintain power, the incumbent coalition must ensure they receive at least n votes in order to win the election. To this end, members of the incumbent coalition divide vote trading responsibilities symmetrically. Each politician has an incentive to free-ride on the vote-trading of his colleagues; to overcome this, they impose sanctions on individuals who renege in their obligations. Following AFK, we assume that a clientelist politician

j receives a punishment X_C imposed by all the other clientelist politicians if he reneges on his promise to citizen i. In contrast, no clientelist insurance agreements take place between opposition candidates and citizens; thus the punishment clientelist politicians impose on each other is greater than the punishment non-clientelist politicians would impose (X_N), or $X_C \geq X_N = 0$. In addition, clientelist citizens can impose non-pecuniary punishments $X \geq 0$ on politicians who renege on the insurance obligation in the case of need; it is equivalent to (and can be interpreted as) the utility loss to the politician from a breakdown of a relationship with a clientelist citizen.

Finally, in addition to the costs or punishments common to all individuals of a particular type, we follow AFK and allow for each politician-citizen pair to share a common idiosyncratic history that generates utility loss (x_i^j) to the politician if he reneges on the promise of an insurance transfer to citizen i in the state of need. This captures (in a reduced-form manner) the loss to the politician of the continuation value of the relationship with the citizen. Consistent with the literature characterizing the structure and value of relationships in social networks (e.g., Jackson and Wolinsky 1996; Johnson and Gilles 2000), most ties tend to be relatively weak and socially distant. We thus assume that the distribution of these relationship values is randomly and independently drawn from a cumulative distribution $F(x_i^j)$ with unimodal and decreasing density.[34]

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

$$max_{S_i^j} \; P_{win|VT}(k)[E + R] - \mu n S_i^j, \tag{7}$$

where $P_{win|VT}(k)$ denotes the probability that the incumbent politicians win reelection under clientelism (i.e., vote trading), subject to the government budget constraint

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

and a set of individual rationality and incentive compatibility constraints. Specifically, insurance transfers between each politician j and citizen i must satisfy the incentive compatibility condition that the value of the transfer should not be greater than the cost to the politician of reneging on the promise, or:

$$S_i^j \leq (X_i + I_i^j X + x_i^j), \tag{8}$$

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

$$P_{win|VT}(k)[E + R] - \mu n S_i^j \geq P_{win|NVT}(k)[E + R]. \tag{9}$$

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

$$U_{ik}(N) = v(G) + \mu u(y_b) + (1 - \mu)u(y_g). \tag{10}$$

Timing

The timing of the model is as follows: (1) Incumbent politicians, and citizens, can make clientelist insurance arrangements. Each arrangement specifies a transfer S_i^j from an incumbent politician to a citizen if in the state of need (i.e., the bad state), in exchange for the citizen's vote. (2) The state is revealed to both parties. (3) Each politician chooses the transfer level if the bad state arises. (4) Elections occur. If the need state occurred

and the transfer received by citizen i is (at least) S_i^j, he casts his vote for the incumbent politician with whom he made a clientelist arrangement. If the bad state occurred and the transfer received is less than S_i^j, he casts his vote against the incumbent politician. Sanctions by other clientelist politicians and citizens are imposed on any reneging incumbent politician. If the bad state does not arise, citizens in clientelist arrangements vote for incumbent politicians as promised.

Characterization of Equilibrium

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

$$x_i^j \geq u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)) - y_b - X_i - I_i^j X. \tag{11}$$

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

$$x_i^j \geq \begin{cases} u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)) - y_b - (X_C + X) \; for \; c_i = C \; and \; c_j = C \\ u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)) - y_b - X_C \; for \; c_i = N \; and \; c_j = C \end{cases}. \tag{12}$$

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

A high value of the incumbent coalition's valence (ϕ_k) makes it less costly for citizens to vote for the clientelist candidates, and their individual rationality easier to satisfy. A high value of the idiosyncratic utility loss (x_i^j) to the politician makes reneging on a promised transfer a more costly action, and hence supports a greater range of incentive compatible transfers from them in return for citizens' votes. When citizens and politi-

cians are in a clientelist relationship ($c_i = C$ and $c_j = C$) (condition (a)), this sustains higher punishments, X, and hence makes higher transfers incentive compatible. Because citizens who do not have a relationship with a clientelist politician cannot punish him, the citizen-induced punishment X term disappears in condition (b), and so only other clientelist politicians can punish (X_C) the reneging politician; this limits the range of incentive compatible transfers to non-clientelist citizens.

Defining the Likelihood of Clientelist Insurance and Transfer Levels

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

$$P_{VT}[k|c_i = C] = 1 - F[u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)) - y_b - (X_C + X)], \qquad (13)$$

and

$$P_{VT}[k|c_i = N] = 1 - F[u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)) - y_b - (X_C)]. \qquad (14)$$

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

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

$$P_{win|VT}[k] = \sigma_{CC,k} P_{VT}[k|c_i = C] + \sigma_{NC,k} P_{VT}[k|c_i = N] \tag{15}$$

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

$$v(\widetilde{G}) + \mu u(y_b + S_i^j) + (1-\mu)u(y_g) + \phi_k \geq v(G) + \mu u(y_b) + (1-\mu)u(y_g). \tag{16}$$

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

$$S_i^{j*} \geq u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)) - y_b. \tag{17}$$

Comparative Statistics

We now examine the implications of the introduction of independent risk-coping mechanisms for citizens, analogous to our cisterns intervention. Specifically, we examine the effects on the likelihood that citizens enter a clientelist insurance arrangement following an improvement in their income levels in the state of need (y_b).

Result 1: An improvement to the citizen's state-contingent income in the bad state (y_b) decreases the probability that both clientelist and non-clientelist citizens will engage in vote trading: $\frac{\partial P_{VT}[k|c_i=C]}{\partial y_b} < 0$ and $\frac{\partial P_{VT}[k|c_i=N]}{\partial y_b} < 0$. The response for clientelist citizens is stronger in absolute terms than that of non-clientelist types.

Result 2: An improvement to the citizen's state-contingent income in the bad state (y_b) decreases votes for incumbent clientelist politicians.

We now clarify why these results provide theoretical motivation for our study. Recall that our treatment examines effects of decreasing vulnerability, which as shown by Ligon and Schechter's (2003) theoretical work, is a function of both the level and variability of consumption. The cisterns intervention not only insures citizens against bad states (by mitigating negative shocks to water consumption during droughts), but also increases consumption in good states (by heightening their ability to harvest rainfall). In the model, an improvement in the citizen's state-contingent income in the good state (y_g) does not affect the probability that clientelist or non-clientelist citizens engage in vote trading ($\frac{\partial P_{VT}[k|c_i=C]}{\partial y_g} = 0$ and $\frac{\partial P_{VT}[k|c_i=N]}{\partial y_g} = 0$), or that they vote for incumbent clientelist politicians. Given Results 1 and 2 above, the cisterns intervention is thus expected to decrease clientelism and incumbent votes, because the insurance value of the transfer decreases with improvements in the bad state.

This model also suggests why the intervention decreases citizens' involvement in clientelism instead of leading to alternative forms of requests. Following Anderson, Francois, and Kotwal (2015), our adaptation assumes that citizens have additively separable utility from consumption of a private good and a public good. The intervention provides an income transfer in the bad state, thus reducing the marginal utility of consumption of the private good. As a direct consequence, a cistern decreases the citizen's expected benefit from participating in a clientelist arrangement — which provides an income transfer from the politicians during the bad state, in contingent exchange for the citizen's vote. This intuition can also be easily derived from a more generalized model with multiple normal goods, so long as the utility function is strictly concave (i.e., with decreasing marginal utility of consumption of each good). In this more general model, citizens consume various private goods (including water) and the intervention is modeled as an in-kind transfer of water. If the in-kind transfer is inframarginal, then the increase in water consumption is equivalent to an income transfer, yielding the result that requests for *all* types of goods decrease — not just requests for water. Even if the in-kind water transfer is extramarginal, the cistern decreases the citizen's benefit from participating in a clientelist

arrangement due to her utility function's strict concavity: a cistern increases consumption of all goods, so the value of a transfer of any good from the politician during the bad state decreases. All in all, our model — as well as the more generalized model with multiple goods — suggests that the cisterns intervention should reduce citizens' involvement in clientelism rather than leading to alternative forms of requests.

Proofs of Propositions

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

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

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

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

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

shown as condition (12) above.

Proof of Result 1:

In the case of clientelist citizens, from equation (13) it is the case that, $\frac{\partial P_{VT}[k|c_i=C]}{\partial y_b} = -f[u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)) - y_b - (X_C + X)][u^{-1'}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)) - 1][u'(y_b)]$. Since $[u^{-1'}(((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b))] = \frac{u'(y_b)}{u'(u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)))}$, and the term in the denominator equals the citizen's marginal utility of consumption in the bad state given the minimum transfer level $\underline{S_i^j}$ that satisfies the citizen's IR constraint ($u'(u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b))) = u'(y_b + \underline{S_i^j})$),

then $\frac{u'(y_b)}{u'(u^{-1}((1/\mu)(\triangle v(\widetilde{G})-\phi_k)+u(y_b)))} > 1$ for any $S_i^{j*} > 0$. Therefore, $\frac{\partial P_{VT}[k|c_i=C]}{\partial y_b} < 0$. Following the same logic for non-clientelist citizens, based on equation (14) it is the case that $\frac{\partial P_{VT}[k|c_i=N]}{\partial y_b} < 0$.

To show that the response for clientelist citizens is stronger in absolute terms than that of non-clientelist types, note that $\frac{\partial P_{VT}[k|c_i=C]}{\partial y_b} < \frac{\partial P_{VT}[k|c_i=N]}{\partial y_b}$ if $f[u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)) - y_b - (X_C + X)] > f[u^{-1}((1/\mu)(\triangle v(\widetilde{G}) - \phi_k) + u(y_b)) - y_b - (X_C)]$. Because $X > 0$, this condition will hold for any unimodal probability distribution $f(x_i^j)$ with decreasing density.

Proof of Result 2:

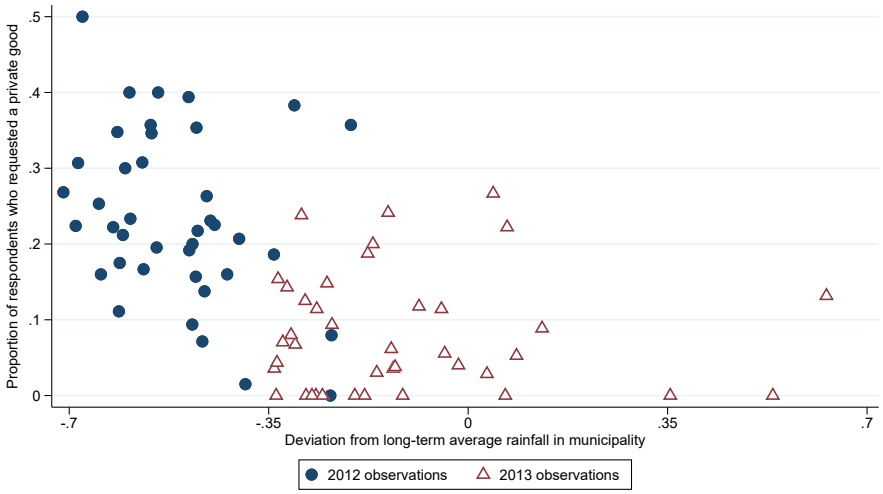
From equation (15), it follows that:

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

and thus $\frac{\partial P_{win|VT}[k]}{\partial y_b} < 0$ given Result 1.

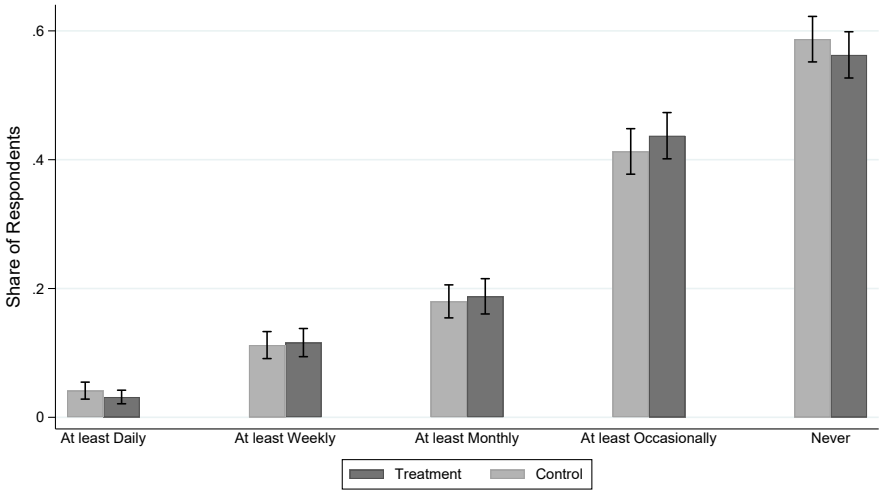
Appendix B: Additional Figures and Tables

Figure A1: Requests for Private Goods and Rainfall Shocks



Notes: Data are collapsed by municipality-year. The rainfall shock for each municipality and year is measured as the difference between rainfall in January-September of the relevant year and its historical municipal mean during identical months in 1986-2011, divided by the municipality's historical monthly standard deviation of rainfall.

Figure A2: Frequency of Conversations before 2012 Election Campaign, by Treatment Status



Notes: Figure shows frequency of interactions with a local politician before the 2012 electoral campaign by treatment assignment. Categories are not mutually exclusive: "at least occasionally" includes respondents in the "at least monthly" category; "at least monthly" includes respondents in the "at least weekly" category; and "at least weekly" includes respondents in the "at least daily" category. Whiskers denote 95% confidence intervals around point estimates.

Table A1: Compliance and Attrition

	Wave 0 2011 Localization (Households) (1)	Wave 1 2012 Baseline (2)	Wave 2 2012 Election Year (3)	Wave 3 2013 Non-election Year (4)
Panel A: Compliance				
Assigned to Cisterns Treatment	615	–	67.45%	90.78%
Assigned to Control	693	–	20.23%	65.30%
Total	1,308			
Panel B: Attrition				
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	-0.019			
Standard Error	(0.017)			

Notes: In Panel A, column 1 reports the number of households in each of the treatment arms. Columns 3 and 4 report the percentage of households in each treatment arm who had a cistern in each of the post-treatment waves (in 2012 and 2013). Panel B shows the percentage of the 1,308 households identified for study participation that were not successfully interviewed in each wave, as well as the correlation of attrition with treatment status.

Table A2: Baseline Characteristics of Treatment and Control Groups

	Treatment Group Mean (1)	Control Group Mean (2)	Difference w/ Municipal FEs (3)	Standard Error of Difference (4)	Observations (5)
Individual Characteristics					
Age	36.587	37.393	-0.345	(0.642)	2,988
Female	0.518	0.535	-0.016	(0.011)	2,990
Current Student	0.139	0.126	0.005	(0.013)	2,972
Years of Education	5.903	5.728	0.006	(0.193)	2,931
P-value of Joint F-test			0.601		
Household Characteristics					
Household Size	4.288	4.221	0.054	(0.119)	1,308
Number of Total Neighbors	17.658	15.959	1.997	(1.377)	1,283
Neighbor has Cistern	0.664	0.598	0.060*	(0.035)	1,237
Bolsa Familia Amount Received	91.954	85.915	4.945	(4.327)	1,290
Total Household Expenditure	367.149	376.861	-6.454	(12.636)	1,281
Total Household Wealth	18,955.478	20,256.436	-1,187.803	(992.416)	1,299
Age of Household Head	43.899	44.840	-0.555	(0.937)	1,307
Household Head Education	5.734	5.830	-0.241	(0.250)	961
Household Head is Female	0.182	0.182	0.007	(0.019)	1,308
Owns House	0.863	0.873	-0.016	(0.021)	1,308
Number of Rooms in House	5.266	5.331	-0.082	(0.079)	1,294
Has Access to Electricity	0.883	0.905	-0.018	(0.018)	1,308
Migrated Recently	0.111	0.107	0.006	(0.017)	1,303
Owns Land	0.483	0.465	-0.004	(0.030)	1,307
Land Size	3.413	3.554	-0.218	(0.684)	1,308
Children in Household 0-6 Months	0.047	0.058	-0.015	(0.013)	1,308
Children in Household 6 Months - 5 Years	0.631	0.612	-0.001	(0.038)	1,308
Household Members 5-64 Years	3.397	3.316	0.099	(0.112)	1,308
Household Members Older than 64 Years	0.213	0.235	-0.029	(0.028)	1,308
Voted in 2008 Municipal Election	0.891	0.865	0.020	(0.019)	1,290
P-value of Joint F-test			0.839		

Notes: Columns 1-2 present the mean of each variable for the treatment and control group, respectively. Column 3 reports differences estimated in an 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: Voting Outcomes: Balance Across Machines in Voters' Characteristics

	Age (1)	Schooling Years (2)	Female Respondents (3)
Treated Individuals	0.102	-0.001	0.001
	(0.063)	(0.016)	(0.002)
Respondents	-0.021	-0.008	-0.002
	(0.051)	(0.016)	(0.002)
Control for Registered Voters	Yes	Yes	Yes
Location Fixed Effects	Yes	Yes	Yes
Missing Outcome Indicator	Yes	Yes	Yes
Rescaled Regressors	Yes	Yes	Yes
Observations	909	909	909
Mean of Y: Overall	38.369	5.644	0.546

Notes: Dependent variables are mean characteristics of study voters linked to a particular voting machine as indicated in the column headers. Standard errors clustered at the voting location site are reported in parentheses; *10%,** 5%,*** 1% significance levels.

Table A4: Wealth and Assignment to Treatment (2013)

	Value of Durables (1)	Value of Livestock (2)	Value of Property (3)	Net Savings (4)	Total Wealth (5)
Cisterns Treatment	-41.2	-110.4	-1,012.8	261.8	-902.6
	(308.8)	(342.5)	(1,120.8)	(195.2)	(1,243.0)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	1,128	1,128	1,128	1,128	1,128
Mean of Y: Treatment Group	3,770.9	2,064.5	24,756.1	-584.9	30,006.5
Mean of Y: Control Group	3,667.0	2,206.5	26,540.7	-776.7	31,637.5

Notes: Each column reports the coefficient from regressing each wealth measure on treatment, with municipality fixed effects. Durables in column 1 are 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 are 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 is defined as the sum of the estimated values of residential property and agricultural plots. Net savings in column 4 are 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 A5: Characteristics of Respondents with and without Clientelist Relationships (2011 and 2012)

	Individuals with Marker of Clientelist Relationship (1)	Individuals without Marker of Clientelist Relationship (2)	Difference w/ Municipal FEs (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	5,894.113	5,641.094	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	3.363	3.323	0.029
			(0.043)
SRHS Index	2.798	2.817	-0.014
			(0.042)
Child Food Security Index	-0.503	-0.607	0.044
			(0.072)
Overall Vulnerability Index	0.048	-0.003	0.032
			(0.042)
Political Activities			
Voted in 2008 Municipality Election	0.916	0.871	0.043**
			(0.019)
Voted for Mayor/Councilor of the Same Coalition	0.732	0.719	0.006
			(0.033)
Entire Household 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 clientelism marker. As discussed in Section 7, 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: Clientelism Marker and Assignment to Treatment

	Clientelist Relationship	
	(1)	(2)
β_1: Cisterns Treatment	-0.002	
	(0.017)	
β_2: Rainfall Shock		-0.010
		(0.008)
Municipality Fixed Effects	Yes	No
Observations	2,667	2,667
Mean of Y: Treatment Group	0.188	0.188
Mean of Y: Control Group	0.180	0.180

Notes: The outcome variable is the marker for clientelist relationships discussed in Section 7. This marker (Clientelist Relationship) is coded one if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise. Cisterns treatment is coded 1 if respondent's household is in a neighborhood cluster selected for treatment; 0 otherwise. Rainfall shock is measured as the difference between rainfall in January-September 2012 and its historical municipal mean during identical months in 1986-2011, divided by the municipality's historical monthly standard deviation of rainfall (see Section 4.4); *10%,** 5%,*** 1% significance levels.

Table A7: Citizen Requests for Private Goods, Controlling for Citizen Engagement (2012 and 2013)

	Request Any Private Good			
	(1)	(2)	(3)	(4)
β_1: Cisterns Treatment	-0.003	-0.013	0.051	0.058
	(0.014)	(0.013)	(0.036)	(0.037)
β_2: Cisterns Treatment × Clientelist Relationship	-0.092***	-0.096***	-0.097***	-0.095***
	(0.034)	(0.034)	(0.034)	(0.034)
β_3: Rainfall Shock	-0.020*	-0.021*	-0.003	-0.003
	(0.010)	(0.011)	(0.020)	(0.020)
β_4: Rainfall Shock × Clientelist Relationship	-0.011	-0.012	-0.014	-0.014
	(0.016)	(0.016)	(0.016)	(0.016)
β_5: Clientelist Relationship	0.115***	0.119***	0.117***	0.115***
	(0.026)	(0.026)	(0.026)	(0.026)
Member of a Community Association	Yes	No	No	Yes
Member of a Community Association × Cisterns Treatment	Yes	No	No	Yes
Member of a Community Association × Rainfall Shock	Yes	No	No	Yes
President of a Community Association	No	Yes	No	Yes
President of a Community Association × Cisterns Treatment	No	Yes	No	Yes
President of a Community Association × Rainfall Shock	No	Yes	No	Yes
Voted in 2008 Municipal Election	No	No	Yes	Yes
Voted in 2008 Municipal Election × Cisterns Treatment	No	No	Yes	Yes
Voted in 2008 Municipal Election × Rainfall Shock	No	No	Yes	Yes
Municipality Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	4,288	4,288	4,231	4,231
Mean of Y: Treatment Group	0.149	0.149	0.149	0.149
Mean of Y: Control Group	0.177	0.177	0.177	0.177
Mean of Y: Clientelist Relationship in Control Group	0.285	0.285	0.286	0.286

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. Cisterns 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 Section 7). Rainfall shock is measured as the difference between rainfall in January-September of the relevant year and its historical municipal mean during identical months in 1986-2011, divided by the municipality's historical monthly standard deviation of rainfall (see Section 4.4). Observations for 2013 employ the subset of individuals for which this marker was measured in 2012. Standard errors clustered at the neighborhood cluster level reported in parentheses; *10%,** 5%,*** 1% significance levels.

Table A8: Cisterns Treatment and Other Electoral Outcomes (2012)

	Votes for Incumbent Mayor		Votes for Candidates From President's Party	Votes for Candidates From President's Coalition	Votes for Candidates From Governor's Coalition	Votes for Right-Leaning Candidates
	(1)	(2)	(3)	(4)	(5)	(6)
Treated Individuals		-1.143*	-0.047*	-0.048	-0.059	-0.079
		(0.655)	(0.026)	(0.064)	(0.061)	(0.062)
		[0.041]	[0.232]	[0.343]	[0.269]	[0.321]
Respondents		0.250	0.033	0.046	0.020	0.072
		(0.493)	(0.022)	(0.051)	(0.047)	(0.047)
		[0.520]	[0.257]	[0.248]	[0.611]	[0.242]
Treated Individuals with Clientelist Relationship	-0.272*					
	(0.146)					
	[0.097]					
Treated Individuals without Clientelist Relationship	-0.052					
	(0.072)					
	[0.491]					
Respondents with Clientelist Relationship	0.047					
	(0.108)					
	[0.663]					
Respondents without Clientelist Relationship	0.011					
	(0.046)					
	[0.800]					
Control for Registered Voters	Yes	Yes	Yes	Yes	Yes	Yes
Location Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Rescaled Regressors	Yes	No	Yes	Yes	Yes	Yes
Observations	909	909	909	909	909	909

Notes: Dependent variables are the number of votes cast as indicated in the column headers. The estimation sample is the 21 municipalities where the incumbent mayor ran for reelection in 2012. Marker for clientelist relationship is coded 1 if a respondent conversed at least monthly with a local politician before the 2012 electoral campaign; 0 otherwise (see Section 7). Rainfall shock is measured as the difference between rainfall in January-September of the relevant year and its historical municipal mean during identical months in 1986-2011, divided by the municipality's historical monthly standard deviation of rainfall (see Section 4.4). Standard errors clustered at the voting location are reported in parentheses; *10%,** 5%,*** 1% significance levels. P-values from a wild cluster bootstrap-t procedure that allows for error terms to be correlated within municipalities are reported in brackets.

Table A9: Politician Responses and Citizen Beliefs (2012)

	Politician Responses				Citizen Beliefs about Incumbent		
	Campaign Visit	Campaign Visit with Handout	Politician Offered Handout	Accepted Politician's Handout	Incumbent Mayor is Honest	Incumbent Mayor is Competent	Incumbent Group is Competent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A							
Cisterns Treatment	0.001	0.009	-0.003	-0.005	0.053	0.027	-0.040
	(0.022)	(0.012)	(0.015)	(0.010)	(0.034)	(0.035)	(0.037)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B							
Rainfall Shock	0.015	-0.011	-0.023***	-0.009**	0.045**	0.035	0.045
	(0.011)	(0.007)	(0.008)	(0.004)	(0.019)	(0.022)	(0.028)
Municipality Fixed Effects	No	No	No	No	No	No	No
Observations	2,680	2,308	1,634	1,624	923	1,083	1,095
Mean of Y: Treatment Group	0.702	0.072	0.077	0.030	0.765	0.708	0.493
Mean of Y: Control Group	0.691	0.067	0.077	0.036	0.711	0.672	0.524

Notes: Dependent variables for columns 1-4 are coded 1 if the respondent answered affirmatively to: receiving a campaign visit to the home by politicians or their representatives in 2012 (col. 1; asked in 2012), receiving a handout during these politician-initiated campaign visits in 2012 (col. 2; asked in 2012), receiving any offer of a handout from a politican in exchange for a vote in 2012 (col. 3; asked retrospectively in 2013 and not limited to campaign visits), and accepted the politican's offer in 2012 if answering affirmatively to the prior question (col. 4; asked retrospectively in 2013); 0 otherwise. For column 5 (6), the dependent variable is coded 1 if the respondent agreed or strongly agreed in the 2012 wave with the statement that the incumbent mayor is honest (competent); 0 otherwise. For column 7, the dependent variable is coded 1 if the respondent indicated (in 2012) that the incumbent group was more competent than the opposition group; 0 otherwise. The survey did not ask about the honesty of the incumbent group. Cisterns treatment is coded 1 if respondent's household is in a neighborhood cluster selected for treatment; 0 otherwise. Rainfall shock is measured as the difference between rainfall in January-September 2012 and its historical municipal mean during identical months in 1986-2011, divided by the municipality's historical monthly standard deviation of rainfall (see Section 4.4). The sample for columns 1-2 comprises respondents who were surveyed in 2012, for columns 3-4 respondents who were surveyed in 2012 and 2013, and for columns 5-7 respondents who were surveyed in 2012 in municipalities in which the incumbent mayor ran for reelection. Standard errors clustered at the neighborhood cluster level reported in parentheses; *10%,** 5%,*** 1% significance levels.

Table A10: Citizens' Preferences and Assignment to Treatment

	Risk Preference		Altriusm		Reciprocity		Discount Rate		Public Goods Contribution
	2012	2013	2012	2013	2012	2013	2012	2013	2013
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A									
Cisterns Treatment	0.015	0.007	-0.030	-0.010	0.003	-0.022	-0.335**	0.012	0.011
	(0.024)	(0.026)	(0.022)	(0.025)	(0.026)	(0.025)	(0.144)	(0.200)	(0.013)
Municipality Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Incentivized Task	No	Yes	No	Yes	No	Yes	No	No	Yes
Panel B									
Rainfall Shock	-0.016	-0.022	-0.007	-0.005	-0.046***	-0.000	-0.123	-0.038	0.014*
	(0.012)	(0.015)	(0.011)	(0.014)	(0.013)	(0.012)	(0.082)	(0.100)	(0.008)
Municipality Fixed Effects	No	No	No	No	No	No	No	No	No
Incentivized Task	No	Yes	No	Yes	No	Yes	No	No	Yes
Observations	2,025	1,622	2,544	1,625	2,540	1,625	2,597	1,605	1,625
Mean of Y: Treatment Group	0.603	0.593	0.135	0.439	0.256	0.365	2.335	4.275	0.557
Mean of Y: Control Group	0.586	0.592	0.167	0.442	0.263	0.381	2.629	4.289	0.553

Notes: Dependent variables are listed in column headers; see Online Appendix E for details of the construction of these measures. Cisterns treatment is coded 1 if respondent's household is in a neighborhood cluster selected for treatment; 0 otherwise. Rainfall shock is measured as the difference between rainfall in January-September of the relevant year and its historical municipal mean during identical months in 1986-2011, divided by the municipality's historical monthly standard deviation of rainfall (see Section 4.4). Observations employ the subset of individuals for which this marker was measured in the 2012 wave. Standard errors clustered at the neighborhood cluster level reported in parentheses; *10%,** 5%,*** 1% significance levels.

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 wave to define this variable:
  - (a) "This year, did you ask a city councilor candidate for help?";
  - (b) [If yes:] "What did you ask for?";
  - (c) "This year, did you ask a mayor candidate for help?";
  - (d) [If yes:] "What did you ask for?"
- Identical questions were asked in 2013, first inquiring about requests of candidates who won the election, and then inquiring about requests of candidates who lost the election.

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)?”

Appendix D: Cisterns Treatment and Electoral Outcomes – Rescaling of Regressors

For analyses of the cistern intervention's effects on electoral outcomes, this appendix discusses the rescaling of regressors as well as procedures to conduct appropriate inference. As emphasized in Section 5.2.3, the statistical significance of findings is robust without any adjustments (see also Table A8). However, the procedure described below improves estimation of the magnitude of treatment effects on electoral outcomes.

Before further discussion, recall that we use extraordinarily granular official data: electoral outcomes at the voting-machine level for Brazil's 2012 mayoral elections. The analysis employs the voting machine as the unit of analysis (given ballot secrecy), and focuses exclusively on the cisterns intervention (as rainfall shocks are only measured at the municipal level). We link survey respondents to the specific electronic voting machines to which they are assigned by electoral authorities. Our primary specification is as follows:

$$y_{slm} = \alpha_{lm} + \gamma_1 \cdot TV_{slm} + \gamma_2 \cdot EV_{slm} + \gamma_3 \cdot RV_{slm} + \epsilon_{slm}, \tag{18}$$

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.[35] Therefore, once

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

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.

Rescaling of Regressors

As mentioned, analyses show 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 measurement issues: (a) treatment effects for members of treated households who we cannot link to voting machines (e.g., 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 households); and (c) peer effects on voting behavior by neighbors in the cluster. 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 rescale TV_{slm} and EV_{slm} to incorporate estimates of: (a) how many voting-age members of sampled households we cannot link to machines, (b) how many voters live in other households in the neighborhood cluster (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. Rescaling the TV_{slm} and EV_{slm} regressors addresses upward bias in the magnitude of the estimate of treatment effects on electoral outcomes.

The following discussion explains the procedure for rescaling the number of treated individuals (TV_{slm}); we follow an analogous procedure to rescale EV_{slm}.

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

$$TV_{slm} = \sum_{c} TV_{cslm}, \tag{19}$$

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:

$$TV_{slm} = \sum_{c} \left[TV_{cslm}^{I,h} + TV_{cslm}^{NI,h} + TV_{cslm}^{NI,h-} \right], \tag{20}$$

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.

To estimate TV_{slm}, we follow this procedure:

(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 the number of other individuals in the interviewed household who are eligible to vote ($TV_{cm}^{NI,h}$) directly from information in the household roster in the baseline survey.

(c) We generate estimates of the number of neighbors who are eligible to vote from responses to two questions in our surveys. The first question (in our localization survey) asked individuals to report the number of neighboring households. 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 the number of additional eligible voters in the neighborhood by using the median of households' responses about

the number of neighboring households $\times$ the median number of household members of voting age.

(d) We estimate the average ratio of total eligible voters per survey respondent across all neighborhood clusters in the sample; the estimated mean ratio is 11.3 for the primary estimation sample of 21 municipalities in which the incumbent mayor ran for reelection (and 9.96 for the expanded sample of 39 municipalities in which a candidate can be assigned to an incumbent group.

(e) Finally, we rescale TV_{slm} and EV_{slm} by multiplying the number of treated and overall number of individuals in our study assigned to vote in each particular machine by this average ratio across all municipalities in our estimation sample.

This procedure improves estimation of the magnitude of treatment effects on electoral outcomes. Estimates are smaller in magnitude, making our inferences about treatment effects on electoral outcomes more conservative. Table 4 reports point estimates from the specification with rescaled regressors of interest, along with cluster robust standard error at the voting location level as well as p-values from the wild cluster bootstrap-t procedure (see column 2). Finally, for purposes of comparability, Table A8 estimates the effects of an additional respondent assigned to the treatment condition on the number of votes for the incumbent mayor — without any rescaling of the regressors. As shown, the statistical significance of findings is robust without any such adjustments.

Appendix E: Measures of Citizens' Preferences

Measures of citizens' preferences employed in Section 8.4 were obtained through unincentivized or incentivized games in the 2012 and 2013 survey waves. These games were designed to capture risk preferences, social (other regarding) preferences, and time preferences, as well as public-mindedness and trust of community members. All tasks in the 2012 wave were played in unincentivized hypothetical scenarios, whereas some tasks in the 2013 wave were probabilistically incentivized.[36] All tasks were completed in the same order by respondents, with the time preferences task first. After all tasks had been completed, participants selected for payment in the 2013 wave were chosen by a random draw and paid in a private setting for one or more decisions. Each task is discussed below.

Risk Preferences

Measures of risk preferences in 2012 are based on a hypothetical gamble-choice task that measures attitudes towards financial risk designed by Eckel and Grossman (2008). The task is a series of choices over money gambles with increasing expected payoffs and risk:

(a) First, respondents are asked to choose between a bag that contains a R$20 bill, and one that contains a R$2 bill and a R$50 bill;

(b) Respondents who preferred the first bag in (a) are then asked to choose between a bag that contains a R$20 bill, and one that contains a R$5 bill and a R$50 bill; and

(c) Respondents who person preferred the first bag in (b) are then asked to choose between a bag that contains a R$20 bill, and one that contains a R$10 bill and a R$50 bill.

[36] Though the unincentivized nature of the games in the 2012 wave may be a limitation, some evidence suggests that choices in incentivized experiments are often in line with choices in hypothetical games (Ben-Ner, Kramer, and Levy 2008). The choices made by individuals in the incentivized risk and trust games in 2013 discussed below correlate with those of the unincentivized games we carried out with survey respondents in 2012.

After completing each choice, the participant would hypothetically put her hand inside one of the bags and take out only one bill, thereby "winning" that money. The risk preferences measure in 2013 is based on an incentivized version of this gamble-choice task with choices over the following money gambles (also with increasing expected payoffs and risk):

(a) First, respondents are asked to choose between a bag that contains a coin representing R$2, and a bag with two coins representing R$0.20 and R$5;

(b) Respondents who preferred the first bag in (a) are then asked to choose between a bag that contains a coin representing R$2, and a bag with two coins representing R$0.50 and R$5; and

(c) Respondents who preferred the first bag in (b) are then asked to choose between a bag that contains a coin representing R$2, and a bag with two coins representing R$1 and R$5;

With respect to incentivization, respondents who chose the bag with one coin in all rounds received R$2. For respondents who chose a bag with two coins in their final round, they drew one of two coins from that bag.

Our measure of risk preferences for each year is a variable indicating an individual's preference in option c (i.e., the riskiest gamble). The findings in Section 8.4 are robust to other indicators of risk preferences based on this task.

Social Preferences

Our measures of altruism and reciprocity are constructed from play in a trust game with anonymous partners (Berg et al. 1995). The game played in 2012 was unincentivized and with a hypothetical anonymous community member; the study participant had the role of player 2. In this game, the hypothetical first mover was given R$5 and had to decide whether to send nothing, R$1, R$2, R$3, R$4, or R$5 to the second mover. Whatever he sent was tripled and the second mover could keep or return as much as she wanted.

The study participant ("second mover") was asked how much she would return if she received R$6, how much if R$9, and how much if R$12.

The measures generated in 2013 are based on a probabilistically incentivized trust game played where the first mover was given R$10 and had to decide whether to send nothing, R$2, R$4, R$6, R$8, or R$10 to the second mover. Whatever he sent was tripled and the second mover could keep or return as much as she wanted. Before finding out how much was sent to him, the second mover was asked how much she would return to the first mover if she received R$6, how much if R$12, how much if R$18, how much if R$24, and how much if R$30. Each player was randomly and anonymously matched to another player in the area; payments were calculated based on the responses just mentioned. Players in one out of ten neighborhoods were randomly selected to be compensated for their plays in the trust game.

Following Finan and Schechter (2012), we measure reciprocity by calculating the average share returned when the individual receives more than half of the first mover's endowment minus the share returned when receiving less than half of the first player's endowment. (We implicitly assume that when the first mover sends at least half, the second mover thinks that she has been treated well. On the other hand, if the first mover sends less than half, then it is assumed that the second mover thinks she has been treated poorly.) In this way, we subtract a measure of altruism in order to have a measure focused on reciprocity. Our reciprocity (i.e., reciprocal individual) variable is an indicator equal to one if the difference in the shares returned to player 1 described above is positive. Accordingly, our measure of altruism (i.e., altruistic individual) is a variable indicating whether the average share returned when the individual receives less than half of the first mover's endowment is positive.

Time Preferences

We used a hypothetical game in both the 2012 and 2013 survey rounds to measure the individual's time discount rate: the implicit interest rate at which an individual would be willing to wait x months to receive a prize of a certain amount instead of receiving a hypothetical prize of R$100 the next day. In the 2012 survey round, the series of questions asked the respondent to state her preference between a R$100 prize tomorrow or a guaranteed prize of R$110, R$150, R$200, R$300, R$500, R$700, or R$1,000 in one month. In the 2013 survey wave, we asked an analogous series of questions for the respondent to state their preference between the prize of R$100 tomorrow and a guaranteed prize three months later of each higher amount just mentioned. We construct implied discount rates for each individual based on their responses to these questions.

Public Goods Contribution

Finally, our measure of public goods contribution is constructed from play in a probabilistically incentivized voluntary contributions game with three anonymous neighbors in the community, in the 2013 survey round. The variable "contribution to public goods" tracks the share of funds the individual contributes to a joint account — rather than keeping it for herself — out of R$5 offered to the player. In this game, funds in the joint account are multiplied by a factor of 2 and then divided equally between the four participants.

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