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Eat Widely, Vote Wisely: Lessons from a Campaign Against Vote Buying in Uganda*

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Abstract

We study a large-scale intervention designed by civil society organizations to reduce vote buying in Uganda's 2016 elections. We study this intervention in light of a model where incumbents benefit from a first-mover and valence advantage, vote buying and campaigning are complementary, and voter reciprocity increases the effectiveness of vote buying. The intervention undermined reciprocity as well as the valence advantage of incumbents. As a result, challengers not only campaigned more intensively but also bought more votes in treated locations. Consistent with incumbents being first movers in markets for votes and facing more frictions to adjust their strategies than challengers, their response to the intervention was limited. The intervention ultimately failed to reduce vote buying, but led to short-run electoral gains for challengers and increased service delivery in treated locations.

Keywords: Elections, Voting Behavior, Field Experiment, Africa

JEL Classification: C93, D72, O55

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1 Introduction

Elections in many countries are compromised by the provision of cash or goods in exchange for votes (Anderson et al., 2015; Cruz et al., 2020). Politicians use various tactics to buy votes, from giving supporters incentives to turn out to targeting the voters most likely to reciprocate gifts (Nichter, 2008; Finan and Schechter, 2012). Vote-buying practices foster corruption, reduce political accountability, and undermine economic development (Robinson and Verdier, 2013; Khemani, 2015).

Studies have shown that interventions against vote buying can convince some voters to refuse selling their vote and decrease support for vote-buying candidates (Vicente, 2014; Hicken et al., 2017; Schechter and Vasudevan, 2023). Others show that reducing economic vulnerability undermines clientelistic exchanges with incumbents (Bobonis et al., 2022; Frey, 2022; Blattman et al., 2018). There is less evidence on how political actors—parties, candidates, and their brokers—respond to interventions against vote-buying. These interventions might lead politicians to choose a different mix of electoral tactics, including policy campaigning, across treated and nearby control areas. Such strategic responses, which likely differ across incumbents and challengers, might, in turn, shape voting and policy outcomes.

We study a civil society intervention against vote-buying in Uganda, a country ruled by the same party and leader since 1986. While Uganda is considered a "multiparty autocracy" at the presidential level, elections for parliamentary and lower offices are fairly competitive (Tripp, 2010). As in many other countries, vote buying is endemic in Uganda, and incumbents buy most votes (Conroy-Krutz, 2017). The fieldwork we conducted before the intervention confirmed the ubiquity of vote buying and the role that voters' attitudes and candidates' strategic considerations play in maintaining this equilibrium.

Ahead of Uganda's 2016 general elections, we partnered with the largest collective of electoral civil society organizations in the country, the Alliance for Election Campaign Finance Monitoring (ACFIM), and its international partner, the National Democratic Institute (NDI). We studied their village-level intervention, which they designed to undermine vote buying. The intervention and its evaluation were unprecedented in their scale. The villages in our sample (including control) covered around 1.2 million people registered to vote in 2016—6% of the country's polling stations, and 12% of polling stations in the 53 districts we study. Shortly after the elections, we surveyed 28,454 villagers, collecting rich data on people's experience with vote buying. We also collected extensive qualitative accounts from voters and brokers working for candidates on the ground. Finally, we use fine-grained electoral administrative data from the 2016 and 2021 elections, as well as test scores measured after the 2016-2017 academic year to estimate the short- and long-run effects of the intervention on electoral and policy outcomes.

We designed the randomization strategy to measure both supply- and demand-side responses to the intervention, as well as its spatial spillovers. Our pre-intervention fieldwork showed that the typical operating units for local brokers are parishes, including 3 to 10 villages which are mapped onto within-parish polling stations for the purpose of electoral operations. We randomized two-thirds of the 918 parishes in our sample to treat at least one polling station in the parish; one-third had no polling stations treated. Within treated parishes, we then varied treatment saturation by randomizing the share of treated polling stations. Hence, our design gives us three kinds of polling stations: directly treated; "spillover" (untreated polling stations or in treated parishes); and pure control polling stations (in control parishes).

This allows us to estimate average treatment and spillover effects of ACFIM's intervention on voters' attitudes, candidates' vote-buying and campaigning responses, and electoral outcomes.

ACFIM's intervention took place in the 3 weeks before the 2016 election and included: (i) a leaflet drop; (ii) three village meetings organized by local activists to build awareness of the social costs of vote buying; (iii) a village-wide resolution against vote buying; (iv) posters reminding voters about this resolution; and (v) an automated-call reminder on the eve of the election. These activities were designed to generate changes in attitudes and behavior. First and foremost, ACFIM aimed to increase the perceived social costs and sanctioning around vote selling. Another goal was to inform and persuade voters that they faced no obligation to reciprocate gifts with their vote (Finan and Schechter, 2012). Because it was aware of the difficulty of convincing voters not to sell their vote, ACFIM understood the invitation to break reciprocity, captured by the Ugandan adage "eat widely, vote wisely," as a form of second-best. Ultimately, the intervention aimed to encourage voters to refuse vote-buying transactions. In turn, the resolutions, posters, and robocalls signaled to politicians that the electoral playing field had changed, and that their offers to buy votes might no longer be welcomed or honored in that village.

We provide a theoretical framework to understand how the intervention might have affected voters' preferences, candidate strategies, and electoral outcomes. We built this model based on stylized facts from our fieldwork after the intervention had taken place to shed light on impacts and mechanisms that were not in line with some of our initial hypotheses. In this model, candidates choose how much to invest in vote buying and policy campaigning. Both strategies increase the voters' expressive utility of voting for a given candidate. The elasticity of this utility with respect to vote buying depends on voter reciprocity, and the costs of vote buying and campaigning are complementary. Incumbents benefit from a valence advantage (making them more efficient at vote buying and campaigning) and a first-mover advantage (resulting in challengers being deterred from buying votes and campaigning in areas where incumbents have entered). Using this model, we derive comparative statics on vote buying and campaigning by each candidate type, as well as electoral outcomes, with respect to two key parameters that may have been affected by ACFIM's intervention: voter reciprocity and incumbent valence. We also show that these comparative statics depend on the timing of the intervention relative to the vote-buying and campaigning decisions by each candidate type.

We first explore changes in voters' preferences through surveys, field reports filled by ACFIM activists, and brokers' interviews. To begin with, we show that voters report more NGO visits, leaflet distributions, community meetings, and posters against vote-buying in treated villages. These effects—and their absence in spillover villages—suggest that the intervention was well implemented and can help us isolate mechanisms. In line with ACFIM's reports, respondents in treated villages were more aware of the negative consequences of vote buying could have for their village. However, while village resolutions were more likely to be adopted in treated villages, only a third of such resolutions involved an outright commitment to end vote selling. In contrast, two-thirds of the resolutions encouraged voters to "eat widely, but vote wisely"—i.e., to continue to accept gifts but vote for their preferred candidate.

Through the lens of our theoretical model, the intervention could have affected voter attitudes and behavior via two mechanisms. The first is an erosion of voter reciprocity—consistent with the "eat

widely and vote wisely" guideline adopted in many treated villages. We provide evidence of such erosion using both a subjective and an objective, revealed-preference measure of reciprocity. Moreover, treated respondents report increased perceptions of social sanctions for vote-selling, which likely contributed to this drop in reciprocity. The second potential mechanism is a negative valence shock against incumbents. While the intervention was not explicitly designed to discuss the performance of incumbents, as in Schechter and Vasudevan (2023), it could have affected incumbent valence by challenging the acceptability of a practice largely associated with incumbents. Our results indicate that treated voters were less likely to report feeling close to incumbent candidates.

We then explore how these changes affected the strategies of incumbents and challengers. In our model, candidates have two complementary tools at their disposal to attract voters: vote buying and policy campaigning. Empirically, we show that incumbents did not reduce or increase their vote-buying efforts in treated villages. Meanwhile, challengers bought more votes in both treated and spillover villages. In addition, campaigning efforts by challengers (unlike those of incumbents) intensified in high-saturation parishes. Consistent with the theory, the brokers operating for challengers told us that they saw ACFIM's intervention as levelling the electoral playing field and responded by entering new markets previously dominated by better-endowed incumbents. In turn, the brokers operating for incumbents did not warn their candidates about the intervention, as they likely feared resources might be withdrawn from their areas of operation. This highlights some of the frictions that incumbents face in adjusting their electoral strategies in response to this type of intervention.

As a consequence of these dynamics, the intervention led to short-term electoral gains for challengers. In treated areas, vote shares decreased for incumbents and correspondingly rose for challengers running in the 2016 presidential and parliamentary races. In survey data, self-reported votes for incumbents decreased by 0.06 standard deviations (sd) in both treatment and spillover villages, amounting to 0.18 sd in fully treated parishes. These effects closely match those we estimate in the official electoral data, where incumbent support fell by 0.18 sd in fully saturated parishes. The latter effect roughly corresponds to a 3.2 percentage point decline in the incumbent's vote share in the presidential ballot, and a 4.2 percentage point decline in the vote share of incumbent MPs. These effects, however, fail to hold in the long run: we find that the incumbent president's vote share in 2021 is no different in treated polling stations.

The intervention further led to improvements in service delivery as measured by test scores at the end of the first academic year following the 2016 elections. These increased by 0.15 sd in primary schools near treated polling stations. Together with the null effects on long-run electoral outcomes, these results suggest that newly elected incumbents responded to increased electoral competition by exerting more effort and perhaps were able to rebuild a valence advantage in the leadup to the 2021 election.

Overall, these results are consistent with our theoretical predictions under the assumption that the intervention took place after the vote-buying and campaigning decisions were made by incumbents, but before the corresponding decisions were made by challengers. While incumbents, in principle, had as much time to react to the intervention as challengers, there are two plausible and related explanations for this pattern. First, incumbents have much larger and more rigid campaigning structures than challengers, making them less flexible to quickly adjust their electoral strategies. Second, incumbents rely

on their brokers to inform them about changes on the ground that warrant adjusting vote-buying and campaigning decisions. Consistent with our interviews, brokers working with incumbents (unlike those operating for challengers) had incentives to downplay the expected effectiveness of the intervention to prevent resources being allocated away from them. The model also highlights that neither the change in voter reciprocity nor the valence shock against incumbents is large enough to explain the sizable swing in vote shares and, thus, the need to account for strategic responses by challengers. To provide additional support for our interpretation, we rule out several possible alternative explanations, including the possibility that the ACFIM intervention led to more honest reporting of vote-buying, increased the salience of the phenomenon, or deterred electoral fraud.

Related Literature. This paper brings new theory and evidence on the effects of large-scale policies designed to promote fairer electoral competition and to strengthen political accountability. Previous work has shown that large policy experiments can affect political attitudes and electoral behavior (Beaman et al., 2009; Berman et al., 2019; Gerber and Green, 2017). We focus on a phenomenon, vote-buying, which remains widespread, especially as many countries are experiencing democratic backsliding (Guriev and Papaioannou, 2022). We show that informing voters about the costs of vote buying can shift attitudes in the short run, influence the strategies of politicians, and change electoral outcomes.

One of our main contributions is to highlight how incumbent and challengers adapt to a changing electoral playing field. Recent papers have explored the effect of voter information in diverse authoritarian or populist settings such as Bangladesh, Mexico, the Philippines, and Turkey (e.g., Cruz et al., 2020; Ahmed et al., 2024; Akbiyik et al., 2023; Enríquez et al., 2023, 2024). The response of incumbents and opposition movements to this type of intervention is crucial to their long-term impact; yet such responses have remained largely unexplored. We provide novel evidence on how opposition candidates adapt their electoral strategies to shifts in the structural advantages of incumbents.

Recent work has also studied the effectiveness of interventions against vote buying. Vicente (2014) finds that a voter education campaign in São Tomé and Príncipe reduced the influence of gifts on candidate choice, decreased voter turnout, and favored incumbents. Hicken et al. (2017) tackle vote selling as a time-inconsistency problem and show that ex-ante promises can reduce vote selling in the Philippines. Schechter and Vasudevan (2023) show that a radio campaign led to a reduction in the vote share of candidates known to buy votes in India through increased salience of government corruption as an election issue. We differ from these earlier contributions in several ways. First, we treat vote buying as a market equilibrium problem. The experimental design allows us to estimate responses on the supply side (i.e., how information campaigns affect the willingness of voters to sell their vote) and the demand side (i.e., how candidates adjust their electoral tactics in a changing electoral field). Second, we evaluate a campaign that was sufficiently large and visible to trigger responses of this kind. Our sample covered around 1.2 million registered to vote, or 12% of the population in half of the country's districts. Third, we develop a theory of voter attitudinal and behavioral responses to campaigns against vote buying, as well as strategic responses by candidates. This allows us to unpack the mechanisms linking ACFIM's intervention with electoral strategies and voting outcomes.

We also speak to recent work that studies whether policy campaigning can successfully substitute

for vote buying. Fujiwara and Wantchekon (2013) compare clientelistic rallies to policy-related town hall meetings in Benin, and find that the meetings reduced reported vote buying and lowered candidates' vote shares in their stronghold. Similarly, Bowles and Larreguy (2024) show that candidate participation in a debate initiative reduced on-the-ground campaigning in Liberia. In contrast, Bidwell et al. (2020) find that debate showing increases campaign expenditures by candidates, and Cruz et al. (2020) that information about incumbents' campaign promises increases vote buying. In line with Bidwell et al. (2020), our results indicate that buying votes and campaigning on policy issues might act as complements rather than substitutes. More generally, our model clarifies the conditions under which information campaigns might contribute to, or deter clientelistic politics. Consequently, the model provides a unifying framework for this mixed-finding literature, thus constituting a contribution on its own.

Lastly, we build on recent work on the importance of brokers in mediating vote-buying transactions (Finan and Schechter, 2012; Larreguy et al., 2016; Duarte et al., 2024). We highlight that brokers might introduce constraints in the ability of candidates to adjust their vote-buying strategies in response to anti-vote-buying interventions swiftly. Candidates rely on brokers to learn about events on the ground, and brokers might not have incentives to convey information from the field when this would divert resources away from them.

2 The ACFIM Intervention

We partnered with the Alliance for Election Campaign Finance Monitoring (ACFIM), a coalition of 13 civil society organizations, and its international partner, the National Democratic Institute (NDI), to evaluate their intervention against vote buying during the 2016 Ugandan general election. ACFIM relies on its network of local activists to advocate for greater transparency in electoral financing. In this section, we describe the context in which ACFIM's intervention took place, as well as the intervention itself.

2.1 Context

The 2016 elections. ACFIM operates in Uganda, a low-income East African country of about 45 million people. Since 2006, two major political parties and a number of smaller ones have competed in national elections every five years. Despite this, the National Resistance Movement (NRM) and its leader, President Yoweri Museveni, have held power since 1986.[1] Although politics are fairly competitive at the parliamentary level, Uganda is often regarded as a "multiparty autocracy" with endemic voter intimidation and vote buying in national elections (Tripp, 2010).[2]

Our main focus is on the February 2016 general elections. The president was elected in a two-round system, requiring 50% of the vote to be elected in the first round. Two of the eight presidential candidates were considered frontrunners: Museveni and a long-time opposition leader, Kizza Besigye. Members of Parliament (MPs) were elected in single-member constituencies using first-past-the-post voting. A total

[1] Museveni took power through military victory in 1986, under "no party rule." Elections began in 1996, but party competition was restricted. Multiparty competition was first permitted in 2006, and 2016 represents the third multiparty election.

[2] The Ugandan political regime was classified by the Freedom House as "not free" in 2016 (with a score of 36%).

of 1,743 parliamentary candidates ran across 290 constituencies.

Several irregularities occurred during the 2016 electoral period: the leader of the opposition was arrested twice; security checkpoints were set up on major roads; the presence of security forces massively increased (Amnesty International, 2016); the government enforced a four-day social media blackout; and voting materials were delivered late to a large number of polling stations where voters were expected to vote against the NRM (Associated Press, 2016).

On February 20, 2016, Museveni was declared the winner of the presidential election with 60.8% of the vote (against 35.4% for Besigye). Museveni's party, the NRM, also won 200 out of 290 constituency MP seats (69%). Electoral observation missions provided mixed opinions about the fairness and transparency of the election. For example, the EU Observation Mission cited the "intimidating atmosphere for both voters and candidates," and "the orchestrated use of state resources and personnel for campaign purposes" as major obstacles impeding a free and fair election (European Union Election Observation Mission, 2016). We discuss whether electoral fraud can alternatively explain our results in Appendix B.4.

Vote buying in Uganda. Like other countries in Sub-Saharan Africa, Uganda has a high prevalence of vote buying. 85% of Ugandan Afrobarometer respondents reported that politicians "often" or "always" give gifts during campaigns.[3] Vote buying is "ubiquitous" (Democracy Monitoring Group, 2011) and previous studies have described sizable payment amounts—one such study reported that the median vote price in 2011 was 5 times the daily average income (Conroy-Krutz, 2017).

Despite the magnitude of vote buying in Uganda, little is known about how it works in practice. To fill this gap and to explore possible intervention designs, we worked with NDI and ACFIM to conduct focus groups prior to ACFIM's intervention. In addition, we interviewed several elected candidates to gather information about their electoral strategies. Finally, in order to better understand vote-buying operations and the response of candidates to ACFIM's intervention, we conducted a systematic survey of 438 brokers after the election. Here, we briefly summarize the findings from this qualitative work.

The focus groups highlighted the importance of vote buying for winning elections. While participants agreed that some voters may choose to "eat widely but vote wisely"—to accept gifts but then vote for their preferred candidate—they also highlighted the importance of reciprocity: many voters reciprocate gifts with their vote since money "softens people's hearts." These gifts, which consist of either cash or basic consumption goods (such as soap, sugar, or salt), are usually not handed out by the candidates themselves but by brokers who are typically well-known figures in the community—an NDI survey of 185 elected MPs after the intervention reported that *all* MPs had brokers in the 2016 election. Brokers are not only responsible for handing over gifts, but they also identify and target reciprocity-minded voters and make sure that gift recipients vote as instructed. 73% of the brokers we interviewed reported being confident that voters who accept gifts do vote for the candidate they represent. In addition, 30% of brokers reported transporting voters to the polls, and 28% of brokers reported casting the ballot on behalf of gift recipients, who pretend to be illiterate so that the broker can enter the polling booth with them.

To maximize the returns from vote buying, incumbents use sophisticated pyramidal structures, with

[3]The average across all 18 countries in the 2006 round of the Afrobarometer was 70%. In the same survey, 35% of Ugandan respondents said they had themselves been offered incentives to vote in elections (the sample average was 18%).

chiefs at the constituency level, coordinators at the subcounty level, and managers at the parish level, typically overseeing operations across 3 to 10 villages. The latter are ultimately responsible for recruiting and managing village-level brokers. Managers regularly meet with their brokers to coordinate mobilization efforts, and they monitor performance by assessing electoral outcomes in each broker's area of work. In turn, managers, coordinators, and chiefs are accountable to those higher up in the pyramid.

By contrast, challenger candidates have some funds and operational structures allowing them to compete with incumbents, but these resources are less substantial and their operational structures are relatively less organized. For example, our broker interviews indicate that, on average, incumbents hire significantly more staff in higher echelons of the pyramid: 45% more managers, 67% more coordinators, and 73% more chiefs.

2.2 Description of the intervention

The ACFIM intervention sought to reduce vote buying. Through leaflets, village meetings, and robocalls, local activists disseminated messages informing voters about the negative consequences of vote buying. A central message of the campaign was that vote buying undermined public service delivery and, therefore, entailed costs for individuals and the community. With this message, the intervention also sought to weaken reciprocity behavior. Politicians offering gifts in exchange for votes would no longer be perceived as generous caretakers, but rather be associated with negative outcomes. Community resolutions aimed to foster a sense that deviating from the collective commitment by selling one's vote would now be socially sanctioned.

Figure 1: ACFIM Leaflet

The intervention took place in the final three weeks before the election, when most vote-buying transactions and policy campaigning usually take place, and involved several phases. First, ACFIM activists delivered leaflets via door-to-door canvassing to all households in treated villages, and also invited households to participate in subsequent village meetings. The leaflets explained the costs of vote

buying in terms of loss of access to public services. The content of the leaflets was approved by the Electoral Commission and was entirely non-partisan. Figure 1 shows a sample leaflet. This leaflet embodies the main messages behind ACFIM's intervention: first, individuals who sell their votes lose their ability to later demand public services from vote-buying politicians; second, community coordination is important to eradicate vote buying.[4]

Following the leaflet distribution, three village meetings were organized by ACFIM. The first meeting introduced the campaign, discussed the leaflet, and gathered participants' views on vote buying. The second meeting provided an avenue for deliberation about how to collectively tackle the vote-buying problem. Finally, during the third meeting, ACFIM activists invited the community to collectively commit to refuse offers of gifts or money in exchange for their votes. In communities that committed to renouncing vote buying, ACFIM activists then placed posters throughout the village indicating the village was now a "no vote-buying village." Finally, on the eve of the election, meeting participants who had provided their phone number received the following message via automated calls:

> *"Hello! This is an important message from ACFIM. We are calling you to ask you not to sell your vote. You might think it is harmless to accept some small money or goods from politicians during election campaigns, but this will affect the future of your whole community. Do you not want good hospitals, good roads, good schools for your children? When you ask for these services after elections, the politician who wins through buying votes will tell you "I bought your vote, therefore do not bother me by asking me for more things." Don't let your community down. Don't let your country down. Don't sell your vote!"*

2.3 Quality of implementation

Logistical delays meant that ACFIM implemented the intervention later than they originally anticipated, but qualitative data from ACFIM notetakers and our own survey data suggest a reasonably high level of treatment compliance and quality of implementation.

ACFIM estimates that the leaflet was received by 41% of the population (67,374 households) across 1,427 targeted villages.[5] Following the leaflet drop, an estimated 62,566 households participated in at least one meeting, which averaged 30 participants. 88% of meetings took place during the three-week period spanning January 30 to February 18, 2016. ACFIM also sent 21,390 posters (15 per village) to treatment villages. Finally, 32,674 automated calls were made on February 17, 2016, to individuals who provided their phone number to ACFIM at one of the meetings. According to administrative data provided by the implementing company, 18,451 (56%) of these calls were answered.

ACFIM's administrative notes indicate that activists implemented the meetings in accordance with the meeting scripts. For the first meeting, note takers report that 73% introduced the campaign and

[4]The leaflets were printed in the local language. 18 different languages were used: Acholi, Alur, Aringa, Ateso, Kumam, Langi, Lubwisi, Luganda, Lugbara, Lusoga, Madi-Moyo, Ngakarimojong, Rufumbira, Rukhozo, Rukiga, Runyankole, Runyoro, and Rutoro.

[5]This percentage is estimated from the following back-of-the-envelope calculation. Based on the 2014 Ugandan census, the average household had 4.7 members and the fraction of the population under 18 (thus ineligible to vote) was 55%. We validated this estimate using our survey, which found that 37 percent of individuals in treatment villages said they received a leaflet.

discussed the leaflet content, while 51% also involved sharing of participants' views about vote buying and selling. The second meeting was a transition meeting designed to provide an avenue for collective deliberation. There is more variation in what note takers indicated, but all meetings are consistent with the intended purpose. For the third meeting, in 65% of cases, the community deliberated on a collective resolution against vote buying, as intended. Lastly, in 70% of the meetings, at least one influential individual (a local official, MP, parliamentary candidate, or broker) was present. In 74% of such cases, these individuals made arguments in favor of vote buying. Such high participation rates by those with a vested interest in vote buying indicate that these individuals were aware of ACFIM's intervention and potentially felt threatened by it.

3 Theoretical Model

3.1 Baseline motivating facts

In this section, we develop a theoretical framework to understand how changes in voters' preferences and candidate strategies triggered by ACFIM's intervention might have affected electoral outcomes. Our modelling choices aim to reflect several stylized facts in our data, some of which we learned after the data collection, and others which are characteristic of regimes that tend to structurally favor incumbents. Here we provide a brief overview of these facts.

To begin with, incumbents buy many more votes and campaign more intensively than challengers. In our survey data, 28% of respondents in the control group report receiving cash from incumbent candidates, while 15% have received cash from *any* challenger candidate. The average cash amount received from incumbents in the control group is 1,055 UGX, as opposed to 347 UGX received from *all* challengers. This difference translates, albeit to a lesser extent, into different levels of campaigning intensity. On average, 54% of respondents report a visit by the incumbent MP in their village, while 43% report visits by the main challenger candidate. As in similar contexts, the striking gap—especially in resource-intensive vote buying—between incumbents and challengers may result from greater access to state resources (Kitschelt and Kselman, 2013) as well as better, safer career prospects which attract the most able political brokers (Bowles et al., 2020; Larreguy et al., 2017). At the same time, these figures indicate that, while at a disadvantage, challengers do have resources and are able to recruit brokers to contest incumbents.

These patterns also reflect strong complementarities between vote-buying and campaigning. Figure 2 shows the correlation between vote-buying and campaigning, as reported by our survey respondents, illustrating these complementarities separately for incumbents and challengers. Section 6 describes our measures of vote buying and campaigning: the former includes indicators of vote buying along the extensive and intensive margins, and the latter includes measures of on-the-ground campaign activities and policy promises. For incumbents and challengers alike, the intensity of vote-buying is positively correlated with the intensity of campaigning activities by each candidate type. Incumbents also benefit from a popularity advantage. Out of the 80% of respondents that report feeling close to any political party, 81% feel close to the incumbent party at the presidential level and 45% at the MP level.

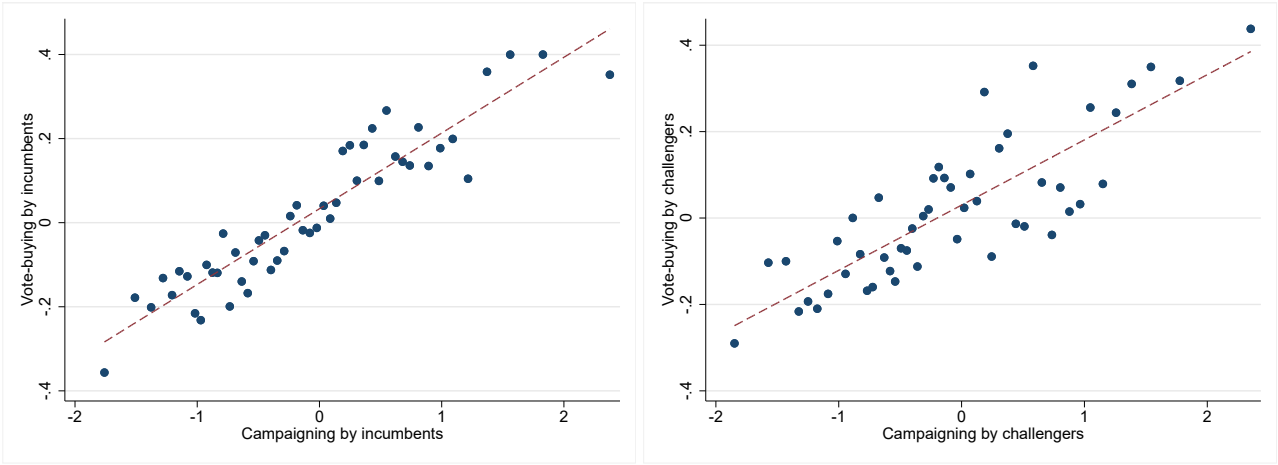


Figure 2: Complementarities between vote-buying and campaigning (control group)

3.2 Setup

The model features an incumbent candidate I, a challenger candidate O, and a continuum of voters of mass one. Each candidate $p \in I, O$ chooses the extent of vote buying, b_p, and campaigning, c_p. Voters cast a ballot for either the incumbent or the challenger: $v \in I, O$.

Payoffs. Voters' preferences are determined by the utility gain from reciprocating vote-buying and by the intensity of vote-buying citep[as in][]Duarteetal2018 and campaigning conducted by each candidate. Specifically, voter i has the following preferences for candidates I and O, respectively:

$$u_I^i = -\sigma^i + \beta c_I + \beta \rho b_I$$

and

$$u_O = c_O + \rho b_O$$

where $\sigma_i \sim U\left[-\frac{1}{2}, \frac{1}{2}\right]$ is a voter-specific ideological bias towards I, ρ is the marginal utility of reciprocating vote buying, and $\beta > 1$ captures the differentially higher valence of incumbents, which makes them more efficient at vote buying and policy campaigning. The higher elasticity of voting with respect to vote buying by incumbents reflects the latter's greater ability to recruit and incentivize the most able political brokers. Moreover, this higher elasticity may also come from greater experience and name recognition (see Bowles and Larreguy, 2024).

We assume that the costs of vote buying and campaigning faced by I are concave; and the cost of vote buying is potentially decreasing in the level of campaigning and vice versa. This creates a potential complementarity between vote buying and campaigning, captured by the parameter ψ:

$$C(b_I, c_I) = \frac{1}{2} b_I^2 + \frac{1}{2} c_I^2 - \psi b_I c_I,$$

where we assume $0 < \psi < 1$. In turn, the cost of vote buying and campaigning faced by both candidates

is identical, except that the cost of vote buying faced by O is increasing in I's effective vote buying, $\beta\rho b_I$:

$$C(b_O, c_O) = \frac{1}{2}b_O^2 + \frac{1}{2}c_O^2 - \psi b_O c_O + \eta(\beta\rho b_I)b_O$$

where $\eta > 0$. The η parameter implies that I enjoys a first-mover advantage over O. This is micro-founded in practice because incumbents set up more extensive and sophisticated voter mobilization structures than challengers can afford. Usually, the incumbent choosing vote-buying and campaigning first and the challenger moving second provides the former with a first-mover advantage. However, in this parsimonious model with linear utility and a uniformly distributed ideological bias (which guarantees tractability and existence), this timing assumption is insufficient to generate the usual incumbent's first-mover advantage. Making the cost of vote buying increasing in the incumbent's vote buying enables us to recover such an advantage. Importantly, this characterization of the first-mover advantage does not preclude the possibility that incumbents might respond to shocks to the market for votes such as ACFIM's intervention, as we explain below.

Timing. The timing of the game is as follows:

1. I decides the extent of vote-buying (b_I) and campaigning (c_I) .
2. O decides the extent of vote-buying (b_O) and campaigning (c_O).
3. Voters cast their vote for their preferred candidate, and payoffs are realized.

3.3 Equilibrium characterization

Solving through backward induction, the weakly undominated strategy of an individual i is to vote for I if and only if:

$$(c_I - c_O) + \rho(b_I - b_O) \geq \sigma^i.$$

Given (c_p, b_p), for $p = I, O$, and integrating over σ^i, I's expected vote share is:

$$\Pi_I(b_I, b_O, c_I, c_O) = \frac{1}{2} + [\beta c_I - c_O + \rho(\beta b_I - b_O)] .$$

The maximization problems are then defined for each candidate as follows. First, for a given (c_I, b_I), O solves:

$$(b_O^*(c_I, b_I), c_O^*(c_I, b_I)) = \max_{(c_O, b_O)} \{1 - \Pi_I(b_I, b_O, c_I, c_O) - C(b_0, c_O)\} .$$

Then, I solves:

$$(b_I^*, c_I^*) = \max_{(c_I, b_I)} \{\Pi_I(b_I, b_O^*(c_I, b_I), c_I, c_O^*(c_I, b_I)) - C(b_I, c_I)\} .$$

Lemma 1. *In a subgame perfect equilibrium in weakly undominated strategies, the optimal levels of vote-buying and campaigning by I are:*

$$b_I^* = \frac{\beta\rho\eta}{(1-\psi)^2} + \frac{\beta(\rho+\psi)}{(1-\psi^2)}$$

and

$$c_I^* = \frac{\beta\rho\eta}{(1-\psi)^2} + \frac{\beta(1+\rho\psi)}{(1-\psi^2)}$$

For the challenger, the optimal levels of vote-buying and campaigning are :

$$b_O^* = \max\left\{\frac{\rho+\psi}{(1-\psi^2)} - \frac{(\beta\rho\eta)^2}{(1-\psi^2)(1-\psi)^2} - \frac{\beta^2\rho\eta(\rho+\psi)}{(1-\psi^2)^2}, 0\right\}$$

and

$$c_O^* = \max\left\{\frac{1+\psi\rho}{(1-\psi^2)} - \frac{(\beta\rho\eta)^2\psi}{(1-\psi^2)(1-\psi)^2} - \frac{\beta^2\rho\eta\psi(\rho+\psi)}{(1-\psi^2)^2}, 0\right\}$$

Proof. All proofs are in Appendix A. □

Interpretation. Figure 3 shows how equilibrium vote buying and campaigning vary as a function of the model parameters. We simulate how b_I^*, c_I^*, b_O^*, and c_O^* evolve when each parameter (ρ, β, ψ and η) is allowed to vary, holding everything else constant. The first intuitive result is that I's vote buying b_I^* is increasing in all parameters. Higher reciprocity and incumbent valence (i.e., higher values of ρ and β) increase the electoral return to vote buying for I. Moreover, the higher the ψ, the stronger the complementarity between b_I^* and c_I^*; and the higher the η, the costlier it is for O to buy votes. Similarly, I's campaigning c_I^* is increasing in all parameters, following the same logic with respect to β and ψ. In turn, the effect of ρ and η on c_I^* operate through its complementarity with b_I^*.

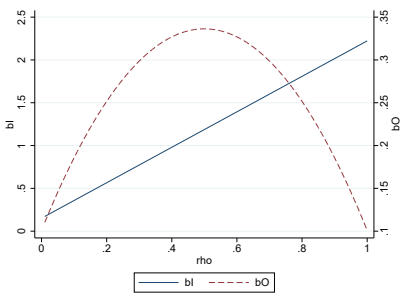
By contrast, Figure 3 indicates much more nuanced effects for challengers. First, a higher β increases the electoral return to I's vote buying, which makes it costlier for O to buy votes. Similarly, a higher η makes it costlier for O to buy votes. Thus, higher values of both parameters reduce challenger vote buying b_O^*. The most interesting result is the non-linear relationship between reciprocity ρ and b_O^*. This non-linearity results from two countervailing forces. On the one hand, ρ increases the electoral return to O's vote-buying. On the other hand, ρ also increases the electoral return to I's vote-buying, which (as with β) makes it costlier for O to buy votes. Which force dominates depends on ρ: for low levels, the former dominates, and for higher levels, the latter. Intuitively, settings with high voter reciprocity tend to benefit incumbents relatively more; this result has broad implications beyond the context of our study. Lastly, we observe a similar pattern and logic for ψ. Due to the complementarity between vote buying and campaigning, we see similar effects for c_O^* in all cases.

3.4 Effects of the anti-vote buying intervention: Comparative statics

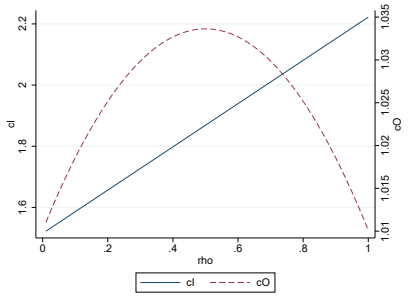
To understand how ACFIM's intervention affected vote buying and campaigning by both candidate types, there are two important aspects to consider: the intervention's timing relative to that of the candidates' decisions, and the intervention's effects on the model's two key parameters β and ρ, i.e., voter reciprocity and incumbent valence. To begin with, the intervention might occur at three different times:

1. At $t = 0$, before I and O choose their level of vote buying and campaigning.

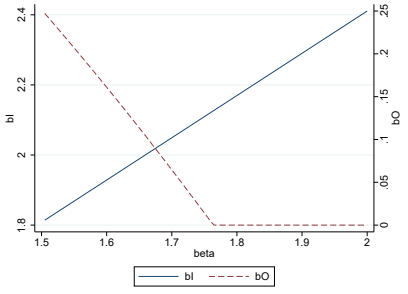
2. Between $t = 1$ and $t = 2$, after (before) I (O) choose their vote buying and campaigning.



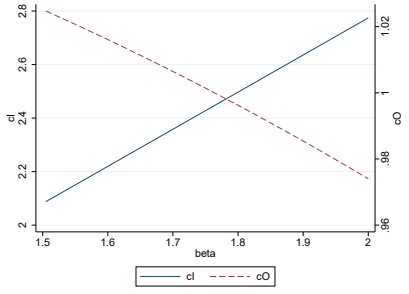
(a) b_I^* and b_O^* by ρ



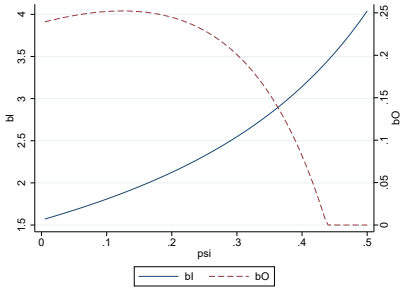
(b) c_I^* and c_O^* by ρ



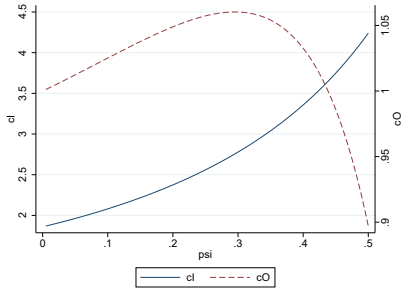
(c) b_I^* and b_O^* by β



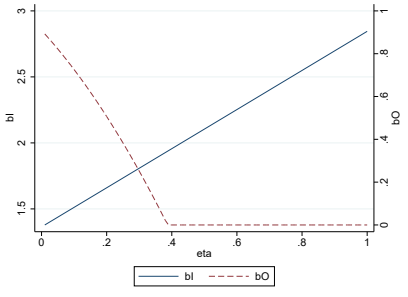
(d) c_I^* and c_O^* by β



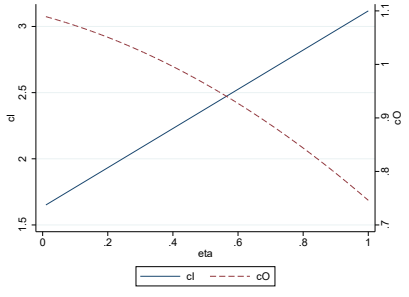
(e) b_I^* and b_O^* by ψ



(f) c_I^* and c_O^* by ψ



(g) b_I^* and b_O^* by η



(h) c_I^* and c_O^* by η

Figure 3: Equilibrium vote-buying and campaigning as a function of parameters

Note: Continuous blue lines correspond to incumbent outcomes, while dashed red lines correspond to challenger outcomes. Simulations are with $\rho = 0.8$, $\beta = 1.5$, $\psi = 0.1$ and $\eta = 0.3$ unless we are plotting how equilibrium outcomes vary when changing the corresponding parameter.

3. Between $t = 2$ and $t = 3$, after I and O have chosen their vote buying and campaigning, but before voters cast their vote.

Furthermore, the intervention might result in two different, possibly non-mutually exclusive parameter changes: (i) a drop in voter reciprocity, ρ, and (ii) a drop in β, which we conceptualize as a negative valence shock against the incumbent.[6] We focus on potential shifts in these two parameters as we deem it unlikely that the intervention affected ψ or η. We view the two latter parameters, which only enter the model through the cost functions faced by both types of candidates, as more permanent features of the electoral playing field. Both reciprocity and incumbent valence, on the other hand, might have been affected by the intervention given the messaging described in Section 2.

Case 1: Anti-vote buying intervention at $t = 0$.

Proposition 2. *If the anti-vote buying intervention takes place at $t = 0$,*

$$\frac{\partial b_I^*}{\partial \rho} > 0, \frac{\partial c_I^*}{\partial \rho} > 0, \frac{\partial b_O^*}{\partial \rho} \lesseqgtr 0, \frac{\partial c_O^*}{\partial \rho} \lesseqgtr 0, \frac{\partial \Pi_I}{\partial \rho} > 0$$

$$\frac{\partial b_I^*}{\partial \beta} > 0, \frac{\partial c_I^*}{\partial \beta} > 0, \frac{\partial b_O^*}{\partial \beta} < 0, \frac{\partial c_O^*}{\partial \beta} < 0, \frac{\partial \Pi_I}{\partial \beta} > 0$$

If the intervention takes place at $t = 0$ and reduces ρ, it should unequivocally reduce b_I^*, and due to its complementarity with campaigning, also c_I^*. The effect on b_O^* and c_O^* is ambiguous since there are two opposing forces. On the one hand, we expect a reduction in both b_O^* and c_O^* resulting from the same forces as with I. On the other hand, the reduction in I's effective vote buying may lead to increased vote buying and campaigning by O. Ultimately, the intervention decreases the vote share of the incumbent. In turn, if the intervention takes place at $t = 0$ and reduces β, it unequivocally reduces b_I^* and c_I^*, increases b_O^* and c_O^*, and also reduces the vote share of the incumbent.

Case 2: Anti-vote buying intervention between $t = 1$ and $t = 2$.

Proposition 3. *If the anti-vote buying intervention takes place between $t = 1$ and $t = 2$,*

$$\frac{\partial b_I^*}{\partial \rho} = 0, \frac{\partial c_I^*}{\partial \rho} = 0, \frac{\partial b_O^*}{\partial \rho} \lesseqgtr 0, \frac{\partial c_O^*}{\partial \rho} \lesseqgtr 0, \frac{\partial \Pi_I}{\partial \rho} > 0,$$

where the latter holds as long as $\beta > \sqrt{2}$. Moreover,

$$\frac{\partial b_I^*}{\partial \beta} = 0, \frac{\partial c_I^*}{\partial \beta} = 0, \frac{\partial b_O^*}{\partial \beta} < 0, \frac{\partial c_O^*}{\partial \beta} < 0, \frac{\partial \Pi_I}{\partial \beta} > 0$$

That is, if the anti-vote buying intervention takes place between $t = 1$ and $t = 2$ and reduces ρ, it has no effect on b_I^* and c_I^* since these decisions have already been realized. In turn, the effect on b_O^*

[6] Usually, a valence shock enters in an additively separable way. However, such a parametrization would lead to no meaningful comparative statics given the linear utility and uniformly distributed ideological bias.

and c_O^* is ambiguous since there are two opposing forces, as discussed in the previous case. Ultimately, the intervention has a negative effect on I's vote share as long as the baseline incumbent valence is sufficiently high ($\beta > \sqrt{2}$). Instead, if the intervention takes place between $t = 1$ and $t = 2$ and reduces β, it also has no effect on b_I^* and c_I^*, increases b_O^* and c_O^*, and reduces the incumbent's vote share.

Importantly, under this characterization, there is no need for incumbent vote buying to go down for challenger vote buying to increase. Even if incumbent vote buying is unchanged, a reduction in its effectiveness due to lower ρ or β can motivate challengers to increase their vote buying.

Case 3: Anti-vote buying intervention between $t = 2$ and $t = 3$.

Proposition 4. *If the anti-vote buying intervention takes place between $t = 2$ and $t = 3$,*

$$\frac{\partial b_I^*}{\partial \rho} = 0, \frac{\partial c_I^*}{\partial \rho} = 0, \frac{\partial b_O^*}{\partial \rho} = 0, \frac{\partial c_O^*}{\partial \rho} = 0, \frac{\partial \Pi_I}{\partial \rho} > 0$$

$$\frac{\partial b_I^*}{\partial \beta} = 0, \frac{\partial c_I^*}{\partial \beta} = 0, \frac{\partial b_O^*}{\partial \beta} = 0, \frac{\partial c_O^*}{\partial \beta} = 0, \frac{\partial \Pi_I}{\partial \beta} > 0$$

If the anti-vote buying intervention takes place between $t = 2$ and $t = 3$ and reduces ρ, it has no effect on b_I^*, c_I^*, b_O^* and c_O^* since these decisions have already been realized. However, it has a negative effect on the incumbent's vote share. The comparative statics are identical if the intervention reduces β.

3.5 Discussion

The characterization of these comparative statics as a function of the timing of the intervention serves several purposes. First, this clarifies under which conditions we can empirically distinguish how the intervention's effects on vote-buying and campaigning may be driven by impacts on voter reciprocity and incumbent valence. From Propositions 2, 3, and 4 follows the next corollary:

Corollary 5. *Only when the anti-vote buying intervention takes place at $t = 0$ or between $t = 1$ and $t = 2$, and the model parameters are such that*

$$\frac{\partial b_O^*}{\partial \rho} > 0, \frac{\partial c_O^*}{\partial \rho} > 0, \frac{\partial b_O^*}{\partial \beta} < 0, \frac{\partial c_O^*}{\partial \beta} < 0,$$

is it possible to disentangle whether impacts on voter reciprocity or incumbent valence drive the effect of the intervention.

To illustrate parameter feasibility and provide intuition about the cases highlighted by Corollary 5, Appendix Figure A1 shows for case 2 (intervention between $t = 1$ and $t = 2$) how $\frac{\partial b_O^*}{\partial \rho}$, $\frac{\partial c_O^*}{\partial \rho}$, $\frac{\partial b_O^*}{\partial \beta}$, and $\frac{\partial c_O^*}{\partial \beta}$ change as a function of the model parameters. These additional simulations indicate that for higher values of β, ψ and η, it is not possible to disentangle whether the effects of an anti-vote buying intervention are driven by its impact on voter reciprocity or on incumbent valence. The simulations also highlight that the parameter range in which such disentanglement is possible is quite limited. Unfortunately, our

experimental design does not allow us to identify the extent to which the drop in voter reciprocity and incumbent valence explain the intervention's effects on candidates' strategies and electoral outcomes.

Second, the characterization above helps us understand the effective timing of the intervention relative to the realization of vote-buying and campaigning investments. This is informative because the effectiveness of interventions of this kind may generally depend on their timing. Intuitively, if there are no effects of the intervention on vote buying and campaigning by a given candidate type, the model indicates that the intervention must have taken place after these decisions have been realized.

In reality, it could be that some candidates have a limited ability to revert decisions over vote buying and policy campaigning made before ACFIM's intervention. The implementation of these strategies typically takes place in the few weeks leading to the election. However, incumbents have relatively larger structures for vote-buying, which involve significant organizational challenges and introduce frictions in their ability to revisit past strategic decisions. Moreover, candidates rely on their brokers to inform them about changes on the ground that require an adjustment in vote-buying and campaigning. However, not all brokers might have incentives to communicate their candidates about ACFIM's intervention, or its expected effect, since this might lead to the reallocation of resources away from their area of operation.

Building on the theoretical predictions in this section, we now turn to the empirical exploration of the effects of ACFIM's anti-vote-buying intervention. We first present our experimental design and data before describing the results from the experiment.

4 Experimental Design

We used a randomized saturation design, as conceptualized in Baird et al. (2018). This design involved two levels of randomization. We first randomly allocated parishes to a treatment cell (no treatment, partial saturation, or high saturation), and then randomly assigned polling stations (together with their corresponding villages) to receive treatment within partially-treated parishes.

4.1 Parish-level randomization

Our baseline sample included 918 parishes with some ACFIM presence before the intervention. We randomly assigned these parishes into three roughly equally-sized groups: a pure control group, a partial-saturation treatment group, and a high-saturation group. The parish-level randomization was stratified along baseline measures of ACFIM eligibility (the fraction of voters living in ACFIM-eligible villages within the parish); parish-level voter population; and support for the incumbent political party in the 2011 presidential election. A stratum was defined by the three-way interaction of quartile of ACFIM presence, quartile of the voter population, and quartile of district-level NRM support.

4.2 Village-level randomization

The allocation of treatment status to polling stations and villages was made under a logistical constraint: the ACFIM intervention could only take place in villages where a local ACFIM activist resided or was

well known by the villagers prior to the intervention.[7] In what follows, we refer to these villages as "eligible" villages. "Eligible" polling stations were polling stations serving at least one eligible village.[8] Within the 918 parishes were 2,796 eligible villages served by 1,603 polling stations. Accounting for the variation in the number of voters registered at each polling station, the fraction of eligible voters (registered to vote in eligible polling stations) ranged from 1% to 100%, with an average of 54%. Thus, parishes typically contained several ineligible villages—these, and the polling stations serving only ineligible villages, are formally outside our randomization sample.

The second step of our randomization involved allocating polling stations (and their corresponding villages) to treatment or spillover status within parishes allocated to the partial saturation group, as described in Appendix Figure B1. In parishes assigned to the high saturation group, all eligible polling stations (and their corresponding villages) were assigned to receive the intervention. In parishes assigned to partial saturation parishes, half of the eligible polling stations were allocated to treatment, and villages were then assigned to treatment or spillover status based on the treatment status of their polling station. All eligible villages matched to a treated polling station were assigned to receive the intervention. None of the villages falling under spillover polling stations received the intervention.

To illustrate, consider a parish with 8 polling stations, of which 4 were eligible (i.e., the 4 polling stations served villages where an ACFIM activist was present). If this parish were assigned to the high-saturation group, this would mean that all 4 of the eligible polling stations (together with the villages they served) would be treated. If assigned to partial saturation, then a randomly selected 2 of the 4 eligible polling stations (and their corresponding villages) would be treated. If only one polling station in a parish was eligible for treatment (i.e., if all eligible villages fell under a single polling station), then this polling station was either treated with 50% probability or allocated to control with 50% probability. This design ensured that no polling stations were ever split between treatment and control—a feature required to be sufficiently powered to detect effects of the intervention on voting outcomes.

In addition to standard intent-to-treat estimates, this design allows us to identify spillover effects on the untreated polling stations and their corresponding villages in treated parishes. We estimate spillover effects by comparing spillover villages to villages in control parishes. Furthermore, the design allows us to recover estimates of how the estimates of treatment and spillover effects vary with parish saturation, which we do in Appendices D and E.

Importantly, our design rests on the assumption that spillovers are limited across parishes. We believe this assumption is reasonable because the 918 parishes in our sample were widely spread across 110 parliamentary constituencies and 53 districts, leaving little scope for spatial spillovers between parishes.

[7] Due to cultural issues, it is difficult, in practice, for an individual to conduct this type of intervention in villages where they are perceived as an "outsider." Section 5.4 provides a longer discussion.

[8] We matched eligible villages to a unique polling station. This allocation was based on the official voter register provided by the Electoral Commission of Uganda. When an ACFIM-eligible village could be matched to multiple polling stations because voters from that village were split across multiple stations in the register, the village was assigned to the polling station with the largest number of voters coming from that village.

4.3 Out-of-sample villages

To further increase our power to estimate treatment and spillover effects of the campaign, we also collected data in an additional 1,399 ineligible villages located in the same 918 parishes. Thus, in the final estimation, spillover villages include both untreated eligible villages in partial-saturation parishes and ineligible villages in both partial- and high-saturation parishes. All our specifications include controls for the ineligibility status of these villages, in addition to controlling for the baseline level of ACFIM presence in the parish. We return to these specification details below.

5 Empirical Framework

In this section, we describe our multiple data sources and estimation details, and discuss the internal and external validity of our estimates.

5.1 Data

Administrative Data. We use official electoral results obtained from the Ugandan Electoral Commission at the lowest possible level, the polling station. We use this data for the presidential and the parliamentary elections conducted in February 2016 for 1,585 of the 1,603 (99%) polling stations in our experimental sample.[9] We also use data on turnout and the incumbent's vote share in the previous general election, conducted in 2011, and data on the incumbent president's vote share in the 2021 presidential election. We discuss the reliability of the electoral data in Appendix B.4.

Survey data. We conducted an endline survey of 28,454 registered voters following ACFIM's intervention and the 2016 general election. Survey respondents were randomly sampled from the official voter register in each village, stratifying into four categories by age (above or below the median for Ugandan voters) and gender.[10] All respondents were 18 or over, registered to vote, and lived in the village. In addition to the survey of registered voters, we surveyed one "key informant"—an influential individual—in each village, and we conducted a local market survey to collect prices of goods used for vote buying. The surveys started on March 2, 2016, and ended on July 19, 2016. The entire data collection was conducted by an independent and separate research organization with no connections to ACFIM.

Primary school and test score data. We use official test score data from the Primary Leaving Examinations (PLE) administered by the Uganda National Examinations Board after the 2016-2017 academic year.[11] Performance is indicated as the percentage of candidates who were scored in each PLE grading level, which we aggregated into a single score at the school level. We also use data on the geographic location of all polling stations from the Ugandan Electoral Commission, and administrative and geographic location of all primary schools in Uganda from the Uganda Bureau of Statistics.[12] We match

[9] Due to discrepancies in local names and spellings, we are unable to match 1% of 2016 polling stations in our sample.

[10] The voter register for the 2016 election was available for all but two parishes in our sample. In those cases, we used the voter register from the 2011 election.

[11] See url.

[12] See url.

each polling station to the closest available school with test score data. Out of 3,657 polling stations in 918 parishes, we can assign test scores to 2,052 polling stations in 695 parishes.[13]

Qualitative data. To provide insights into the content of community meetings, we use data collected by ACFIM, including information about attendance and the content of meetings, and descriptions of participants' views. After the intervention, we also conducted structured interviews with a non-representative sample of 438 brokers across 11 districts and 62 parishes. Sampling was restricted to accessible locations and stratified by a measure of vote buying in the village, since we wanted to understand the response of brokers across areas of different vote-buying intensity. 57% of the brokers we interviewed served incumbent candidates while 43% served challengers. We use this data for descriptive purposes only.

5.2 Estimation

Our research design estimates the treatment effects, spillover effects, and saturation effects of ACFIM's intervention. Our baseline, pre-specified equation is the following intent-to-treat (ITT) specification:

$$Y_{ivp} = \alpha_0 + \alpha_1 Treatment_{vp} + \alpha_2 Spillover_{vp} + \alpha_3 ACFIM_{vp} + \alpha_4 ACFIM\ Presence_p + \Omega X_{ivp} + \varepsilon_{ivp} \quad (1)$$

where $Treatment_{vp}$ is an indicator for assignment to the intervention in village v in parish p; $Spillover_{vp}$ indicates that village v is untreated but located in parish p that is treated; $ACFIM_{vp}$ indicates that village v is an eligible village; $ACFIM\ Presence_p$ is the baseline presence of ACFIM activists in the parish (measured as the fraction of voters in parish p that live in an eligible village) and X_{ivp} is a vector of pre-specified individual-level controls from the survey and parish-level controls from the electoral data.[14] In Appendix C, we show that all our results are robust to more flexibly controlling for $ACFIM\ Presence_p$ or including strata fixed effects. The $ACFIM_{vp}$ term is an indicator for being part of our experimental sample, which we include in all specifications since the dataset includes 1,399 out-of-sample villages in treated parishes, as described in Section 4.3. We focus on the treatment and spillover estimates, α_1 and α_2. We use the same specification for regressions conducted using the polling station-level data. In this case, observations are at the level of polling station j within parish p.

To estimate the effects of parish-level treatment saturation, in every table we also report results from the following equation:

$$Y_{ivp} = \gamma_0 + \gamma_1 Saturation_p + \alpha_3 ACFIM_{vp} + \alpha_4 ACFIM\ Presence_p + \Omega X_{ivp} + \varepsilon_{ivp} \quad (2)$$

where $Saturation_p$ is defined as the fraction of voters in parish p that are treated (i.e., the intensity

[13] We are unable to match 603 polling stations and 57 parishes due to a lack of spatial data, mostly at the school level, and an additional 1027 polling stations and 166 parishes due to a lack of test score data. Importantly, the data loss is neither differential across treatment, spillover, and control polling stations nor varies by parish saturation (Table 6).

[14] These controls include, from the survey data, the age, years of education, and marital status of the respondent, whether the household owns any land, the number of adults and children in the household, an index of asset ownership (as defined in Appendix B.3), as well as occupation, ethnicity, and religion dummies. From the electoral data, we include the 2011 turnout, the NRM and FDC vote shares in the 2011 presidential election, the 2011 fraction of the vote received by the winning parliamentary candidate, and the number of registered voters in 2016.

of the treatment at the parish level). As in equation (1), the terms $ACFIM_{vp}$ and $ACFIM\ Presence_p$ account for variation in $Saturation_p$ that comes from the non-randomly assigned baseline ACFIM presence in a parish. γ_1 then measures the average effect of random treatment saturation across treatment and spillover villages. Equation (2) was not pre-specified in our pre-analysis plan. We present estimates from this equation for ease of exposition and because we consider the main effect of treatment saturation to also be of interest. This regression specification assumes a constant effect of saturation on both treated and spillover villages. As the results in Appendix D make clear, and contrary to some of our pre-specified hypotheses, this was the case for most outcomes.[15]

5.3 Addressing multiple outcomes and comparisons

We sought to reduce the risks of false discovery or cherry-picking results in a number of ways. First, we pre-specified our main hypotheses, estimation framework, and outcomes in a pre-analysis plan.[16] Second, we singled out one primary set of outcomes of interest: survey-based reports that candidates gave cash or goods to the respondent or other villagers, which we use to analyze the direct treatment and the spillover effects of ACFIM's intervention on vote buying. In addition, we pre-specified a number of secondary outcomes of particular policy relevance. The pre-specified tests of our main hypotheses with respect to these outcomes are presented in Appendix E. Third, we reduced the number of primary hypotheses to test by combining them into mean effects indexes of all outcomes in that family.[17] Finally, following current best practice, we adjust for multiple testing by reporting for all our coefficients of interest the sharpened two stage False Discovery Rate (FDR) q-values from Anderson (2008). The correction is implemented across all key outcomes in Tables 2 through 5, separately for equations (1) and (2).

5.4 Internal and external validity

As described earlier, the presence of a local ACFIM activist in the parishes and villages in our sample was not random. Since the randomization was conducted *within* the sample of parishes and villages with ACFIM activists, this aspect of the design does not affect internal validity. Using equations (1) and (2), Table 1 shows that treatment is not associated with a higher propensity to report vote selling in the previous election in 2011, or to vote for the incumbent presidential party in 2011. The lack of effects on vote buying additionally provides evidence that our results in the next section are not driven by social desirability bias. If the intervention had affected voter's incentives to report vote buying, it would have manifested in their retrospective reporting of vote buying in 2011.

Moreover, Appendix Tables B2 to B7 show that treatment assignment and saturation are balanced along a wide range of pre-determined covariates. We use a range of baseline or time-invariant variables

[15] Appendix Tables D1 through D4 provide additional results on how treatment and spillover effects vary with parish-level saturation for all our main outcomes of interest examined in Tables 2 through 5.

[16] See `https://www.socialscienceregistry.org/trials/965`, archived on December 18, 2015.

[17] We take averages of our outcome measures, coded to point in the same direction, akin to the approach by Kling et al. (2007). Component variables are first standardized, then averaged, then standardized again to have mean zero and unit standard deviation in the control group. We do this first for all variables from the voter survey, and then for all the variables in the key informant survey, and then average the two. This gives the two sources of data equal weight.

Table 1: Balance on Past Vote Buying and Social Desirability Bias

	Vote buying in 2011		Incumbent Vote 2011	
	(1)	(2)	(3)	(4)
Treatment village	-0.002		-0.004	
	(0.010)		(0.008)	
Spillover	-0.003		-0.008	
	(0.012)		(0.009)	
Treatment Saturation		-0.007		-0.019
		(0.018)		(0.013)
R^2	0.07	0.07	0.09	0.09
Control Mean	0.22	0.22	0.84	0.84
Controls	Yes	Yes	Yes	Yes
Observations	28454	28454	21785	21785

Note: Odd-numbered and even-numbered columns report estimates from equations (1) and (2), respectively. The dependent variables measure whether the respondent reported selling their vote in 2011 (columns 1–2) and whether they reported voting for the incumbent president in 2011 (columns 3–4). All regressions control for an ACFIM dummy (in-sample villages) and the parish-level ACFIM presence (see Section 5).
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish.

from the voter survey, key informant survey, and official electoral data (see Appendix B.3 for a detailed description). We regress these variables on our two main specifications, namely equations (1) and (2), and report all the coefficients from these specifications. Of 99 coefficients (from 66 regressions), only 9 (9%) have a p-value less than 0.10—almost exactly what should have occurred as a result of chance. Nonetheless, our main results include and are robust to adding these baseline controls.

However, our design requires a brief discussion of external validity. From the perspective of organizations considering similar interventions, the parishes with a resident ACFIM activist may, in fact, be the policy-relevant sample. The strength of civil society organizations (CSOs) often lies in their local credibility, built over multiple years and sustained through the presence of local members of the larger national CSO. As a result, very few CSOs are willing to launch a campaign in locations where they have never worked before. This was also our experience when we inquired with ACFIM about the possibility of extending the intervention to villages without ACFIM activists. However, it is still worth reporting cross-sectional differences between areas with varying levels of baseline ACFIM presence.

Since we do not survey any parishes with zero ACFIM presence, we cannot compare our sample directly to other parishes. However, we can correlate baseline ACFIM presence with covariates to explore this selection indirectly. For example, ACFIM presence is negatively correlated with the 2011 vote share of the incumbent president—across parishes in the top quartile of ACFIM presence, the incumbent's vote share was approximately 8 percentage points lower (12%) than in the bottom quartile. Similarly, ACFIM presence correlates (slightly) with less prior vote buying: the fraction of respondents who sold their vote in 2011 was 3 percentage points (13%) lower in the top quartile than in the bottom quartile.

6 Results

This section presents our results from the randomized evaluation of ACFIM's intervention. We use the model, in addition to the intervention's stated objectives and our own prior hypotheses, to understand the overall pattern of results, both expected and unexpected.

As intended, the intervention somewhat changed voters' perceptions of the consequences of vote buying and fostered coordination against the practice. We hypothesize that the intervention, by changing attitudes while also increasing perceptions that vote selling would be socially sanctioned, shocked two parameters in the model: voter reciprocity and incumbent valence. Incumbents and challengers responded differently to these shocks. The response of incumbents was limited, while challengers bought more votes and campaigned more intensively in treated areas. This led to short-run electoral gains for challengers, which did not persist until 2021, as well as short-run improvements in service delivery measured via primary school test scores. Throughout this discussion, we rely on the notes of ACFIM activists and interviews with brokers to provide contextual evidence supporting our findings.

6.1 Voters' beliefs and village resolutions

We first assess changes in voters' perceptions of the social consequences of vote buying. A negative change in these perceptions could have weakened voter reciprocity and incumbent valence, since politicians who buy votes would then be associated with negative outcomes for the village. In Table 2, we report the coefficients from equation (1) in odd-numbered columns and those from equation (2) in even-numbered columns. Treated respondents changed their attitude towards vote buying and reported a better understanding of its negative consequences as a result of ACFIM's intervention (columns 1 and 2). While 89% of respondents in control villages already held the belief that vote selling had negative consequences, those believing that vote buying is associated with negative consequences for their village increased by 1.3 percentage points (pp) in treated villages and by 2.8pp in fully treated parishes.

As described in Section 2, the information conveyed to, and discussed with citizens was designed to foster the adoption of a village-wide resolution against vote selling. We explore the intervention's success at promoting the adoption of such resolutions in the remaining columns of Table 2. Specifically, we look at indicators for the adoption of any resolution (columns 3-4), the adoption of a resolution explicitly banning vote buying in the village (columns 5–6), or a resolution inviting citizens to "eat widely and vote wisely," as reported by our survey respondents. People's awareness of these resolutions is the relevant outcome to look at since the effectiveness of resolutions depended on citizens being informed about the new village-level guidelines about vote buying (Appendix Table C1 provides corroborating evidence from the survey of key informants). While treated villages were more likely to adopt a resolution, participants often did not agree to unequivocally condemn vote selling: "eat widely, vote wisely" resolutions were twice more likely to be adopted than "no vote buying" resolutions.

Robustness checks. Appendix C reports robustness checks on Table 2 and all subsequent tables. We show that the results are robust to: more flexibly controlling for the baseline ACFIM presence in the parish, either by including a 4th order polynomial in ACFIM presence or by controlling for deciles of

Table 2: Effectiveness of the Intervention's Messaging

	Negative consequences		Village resolution		"No vote buying" res.		"Eat widely, vote wisely" res.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment village	0.013*		0.082***		0.026***		0.056***	
	(0.007)		(0.005)		(0.003)		(0.005)	
	[0.211]		[0.001]		[0.001]		[0.001]	
Spillover	0.010		0.002		0.002		-0.000	
	(0.008)		(0.004)		(0.002)		(0.004)	
	[0.273]		[0.596]		[0.273]		[0.673]	
Saturation		0.028***		0.099***		0.033***		0.066***
		(0.011)		(0.009)		(0.004)		(0.008)
		[0.019]		[0.001]		[0.001]		[0.001]
R^2	0.04	0.04	0.03	0.02	0.01	0.01	0.02	0.02
Control Mean	0.89	0.89	0.03	0.03	0.01	0.01	0.03	0.03
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28454	28454	28454	28454	28454	28454	28454	28454

Note: Odd-numbered and even-numbered columns report estimates from equations (1) and (2), respectively. The dependent variables measure whether respondents report that: vote buying has negative consequences for the village and/or leads to services not being delivered (columns 1–2), a resolution about the vote buying issue was adopted in the village (columns 3–4), a "no vote-buying" resolution was adopted (columns 5–6), or an "eat widely, vote wisely" resolution was adopted (columns 7–8). Appendix Table C1 presents additional results from the key informant survey. All regressions control for an ACFIM dummy (in-sample villages) and the parish-level ACFIM presence (see Section 5).
* p<0.1, ** p<0.05, *** p<0.01. Robust standard errors clustered by parish are reported in parentheses. Sharpened FDR q-values from Anderson (2008) are reported in brackets.

ACFIM presence; including strata fixed effects, and removing all controls other than baseline ACFIM presence and the eligible village dummy. Our core results are robust to these specification changes.

6.2 Voter reciprocity and incumbent valence

Social sanctions. Treated voters reported greater expectations of social sanctions. This is key to understanding the effects of ACFIM's intervention since social sanctions are an important way in which coordination against reprehensible practices is enforced locally. In Table 3, we look at a measure of perceived social sanctions, namely an indicator for respondents that community members would be angry at vote sellers and/or would seek to ostracize them. While most respondents in the control group (78%) believed this would be the case, this increased by 2-3pp in treated villages and parishes (columns 1–2).

Voter reciprocity. Columns 3–6 of Table 3 examine whether the ACFIM intervention weakened reciprocity (ρ in our model). Given the many villages that adopted the slogan "Eat widely, vote wisely," the intervention may have encouraged some voters to accept gifts from politicians but still vote for their preferred candidate, regardless of the gifts received. We examine both a subjective and an objective, revealed preference of breaking reciprocity.

The subjective measure is constructed from a series of survey questions based on Finan and Schechter

Table 3: Social Sanctions, Voter Reciprocity, and Incumbent Valence

	Social sanctions		Breaking reciprocity				Incumbent valence	
			Subjective		Objective			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment village	0.026***		0.009*		0.013*		-0.047*	
	(0.010)		(0.005)		(0.007)		(0.024)	
	[0.059]		[0.211]		[0.211]		[0.211]	
Spillover	0.006		-0.002		0.009		-0.061**	
	(0.012)		(0.007)		(0.008)		(0.025)	
	[0.596]		[0.596]		[0.312]		[0.082]	
Treatment Saturation		0.029*		0.016*		0.019		-0.083*
		(0.016)		(0.009)		(0.013)		(0.048)
		[0.088]		[0.088]		[0.131]		[0.088]
R^2	0.04	0.04	0.01	0.01	0.06	0.06	0.16	0.15
Control Mean	0.78	0.78	0.04	0.04	0.12	0.12	0.35	0.35
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28454	28454	28454	28454	28454	28454	28454	28454

Note: Odd-numbered and even-numbered columns report estimates from equations (1) and (2), respectively. In columns 1–2, the dependent variable measures whether respondents expect social sanctions against vote sellers. In columns 3–4, the outcome combines several questions about reciprocity behavior; see text for details. In columns 5–6, we look at whether respondents report receiving gift from a candidate but voting for a different candidate. In columns 7–8, the outcome measures whether respondents report feeling close to incumbents in the presidential and parliamentary races. All regressions control for an ACFIM dummy (in-sample villages) and the parish-level ACFIM presence (see Section 5).
* p<0.1, ** p<0.05, *** p<0.01. Robust standard errors clustered by parish are reported in parentheses. Sharpened FDR q-values from Anderson (2008) are reported in brackets.

(2012). First, the following general scenario was presented to survey respondents:

> "*Now I will describe situations about how a candidate might behave and what you would do in response. For each situation, please tell me whether you would vote for the candidate I am describing, whether you would vote for a different candidate, or whether you would decide to not vote at all.*"

The subjective measure in columns 3–4 of Table 2 is then based on the following 3 hypotheticals: (i) you expected a gift from a candidate you like, and you did *not* get it; (ii) you expected a gift from a candidate you do *not* like, and you *did* get it; (iii) you did *not* expect a gift from a candidate you like, and you *did* get it. Our dependent variable is equal to 1 if the respondent would still vote for the candidate in scenario (i), and would vote for a different candidate in scenarios (ii) and (iii). Intuitively, (i) amounts to not punishing preferred candidates who no longer buy votes, (ii) amounts to not reciprocating gifts towards non-preferred candidates, and (iii) amounts to not reciprocating towards (and indeed punishing) preferred candidates who buy votes. We find a positive and significant effect of the intervention on this subjective measure of breaking reciprocity. Only 4% of respondents in the control group report being willing to break reciprocity in this way, and this increases by 1pp and 1.6pp i.e., 22.5 % and 40%) in treated villages and fully treated parishes, respectively (columns 3–4).

We then turn to an objective, revealed preference of breaking reciprocity based on self-reported vote selling behavior and candidate choice. We look at an indicator for respondents reporting they accepted cash from a candidate but voted for a different candidate in a given race. 12% of control respondents report such a pattern of breaking reciprocity. This increases by 1.3pp and 1.9pp (i.e., 11 % and 16%) in treated villages and fully treated parishes, respectively, with the latter effect falling slightly short of statistical significance (columns 5–6). These results provide suggestive evidence that the campaign convinced some voters to "eat widely, and vote wisely"—namely to accept gifts from multiple candidates, without feeling compelled to vote for any given candidate.

These findings are consistent with insights from the broker survey. While there are isolated instances of voters refusing gifts for their vote, or even taking cash from the incumbent and handing it over to a challenger, most accounts indicate that treated voters were likely to take the money but then vote for the candidate that they thought would best represent their views. One broker confided to us that "voters didn't stop accepting gifts, (but) voted for the candidate of their choice" while others mentioned that voters started to "eat widely and vote wisely."

Incumbent valence. Because incumbents accounted for the bulk of vote buying before the 2016 elections, the ACFIM intervention might have inadvertently been interpreted as an anti-incumbent message. The framing of the ntervention was about the pernicious effect of vote-buying practices on public service delivery. Both vote buying and poor service delivery are associated with incumbents. As a result, notwithstanding the intervention's non-partisan tone, the leaflets and meetings might have shifted attitudes towards incumbents in treated villages. In Section 3, we conceptualized this effect as a negative shock to incumbent valence (β). This should have then reduced the incumbents' first-mover advantage, irrespective of whether they reduced their own vote buying and campaigning investments.

Measuring valence is empirically difficult. As a proxy for β, in columns 7–8 of Table 2, we focus on a self-reported measure of ideological proximity to specific candidates, based on the survey question: "do you feel close to any political party?" We then map these preferences onto the party of candidates running in the presidential and the local parliamentary race.[18] Overall, 35% of respondents in the control group report feeling close to the (party of) the incumbent president and the incumbent MP. This figure goes down 4.7pp and 8.3pp in treated villages and fully treated parishes, respectively. Here, we observe a similar decline in valence in spillover villages, as the anti-incumbent message potentially associated with the intervention might have spread through treated parishes via word of mouth.

6.3 Candidate strategies: vote buying and policy campaigning

Next, we analyze how this changing electoral environment affected candidates' strategies along two complementary dimensions: vote buying and policy campaigning. Qualitative accounts from our broker interviews illustrate our quantitative findings. Brokers operating for incumbents argued that they did not perceive these shifts in the electoral playing field, and widely expected that the intervention

[18]Here as in the rest of the paper, we focus on these elections because these two offices are the ones that entail the largest access to public funds, and thus resources invested in vote buying.

would have little effect since voters continued to accept gifts. This could genuinely have been what they expected, or a justification for why they did not recommend that incumbent candidates should reallocate vote-buying and campaigning efforts away from treated areas. On the other hand, brokers serving challengers perceived the intervention as an opportunity since existing vote-buying arrangements tended to benefit incumbents. In total, 30% of the latter brokers we interviewed expected voters would stop reciprocating gifts and start voting for their preferred candidate. 18% of brokers mentioned that challengers started operating in villages where they previously did not operate.

Table 4: Effects on Vote-Buying and Policy Campaigning

	Vote Buying						**Campaigning**			
	All		Incumbents		Challengers		Incumbents		Challengers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment village	0.041*		0.009		0.062***		0.052		0.048	
	(0.024)		(0.025)		(0.024)		(0.040)		(0.041)	
	[0.211]		[0.596]		[0.061]		[0.273]		[0.293]	
Spillover	0.016		-0.014		0.048*		-0.013		-0.037	
	(0.026)		(0.027)		(0.028)		(0.044)		(0.046)	
	[0.590]		[0.596]		[0.211]		[0.596]		[0.482]	
Treatment Saturation		0.055		0.010		0.087*		0.111		0.171**
		(0.043)		(0.045)		(0.045)		(0.077)		(0.079)
		[0.133]		[0.228]		[0.088]		[0.131]		[0.061]
R^2	0.06	0.06	0.05	0.05	0.04	0.04	0.13	0.13	0.11	0.11
Control Mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28454	28454	28454	28454	28454	28454	28454	28454	28454	28454

Note: Odd-numbered and even-numbered columns report estimates from equations (1) and (2), respectively. The dependent variables are an index of vote buying (columns 1–4) and an index of campaigning activities (columns 5–8). The vote-buying index is defined by candidate type and includes 4 variables: whether the respondent received any gift in cash/in kind from the candidate, and the value of gifts received from the candidate in cash/in kind. The campaigning index includes indicators for 5 types of activities (village visits, campaign posters, leaflets, campaigning via phone/SMS, and political merchandise) and the number of policy promises made by each candidate type. All regressions control for an ACFIM dummy (in-sample villages) and the parish-level ACFIM presence (see Section 5).
* p<0.1, ** p<0.05, *** p<0.01. Robust standard errors clustered by parish are reported in parentheses. Sharpened FDR q-values from Anderson (2008) are reported in brackets.

Vote Buying. Table 4 reports effects on vote buying and campaigning. For vote buying, we use a standardized index of 4 variables: whether the survey respondent reported being offered any gift in cash in exchange for votes, the reported amount of cash offered, whether the respondent reported being offered goods, and the value of these goods.[19] Overall, we find a weakly positive on this outcome, implying that

[19]The value of goods and cash offered is winsorized at the 99th percentile. In our survey data, we collected data on all brokers who approached the respondent to give her a gift in exchange for her vote, as well as the identity of the candidates these brokers were working for. A respondent is coded as being offered a gift from a particular candidate if she mentioned this candidate among the individuals the brokers were working for.

contrary to our pre-specified hypothesis, the ACFIM intervention failed to reduce vote buying in treated villages. However, the model suggests this equilibrium result could mask substantial heterogeneity across candidates.

In columns 3–6, we explore this heterogeneity by looking at vote-buying offers disaggregated across incumbents and challengers. As anticipated from the model's predictions (and the results presented thus far) regarding the decrease in reciprocity and incumbent valence, challengers (unlike incumbents) were incentivized to buy more votes. While incumbents did not buy more or fewer votes in response to the intervention, we observe positive and significant effects for challengers. Reports of vote-buying transactions by challengers increase by 0.06 and 0.05 sd in treated and spillover villages, while fully treated parishes experience a significant 0.09 sd increase in reports of vote buying. Appendix E shows additional results on vote-buying outcomes.

Also notable is that the effect of the campaign on vote buying by challengers is similar in treatment and spillover villages. This is consistent with accounts by brokers working for challengers and may indicate the presence of returns to scale in vote buying and campaigning, captured in the model via the complementarity parameter ψ. The fact that challengers also increased their vote-buying efforts in spillover villages suggests that some of the increase in vote buying by these candidates took place along the extensive margin, i.e., in villages and parishes where they previously did not operate.

To further explore heterogeneity across candidates and to better comprehend the magnitudes of estimates in columns 1–4 of Table 4, Appendix Table C2 measures treatment effects on the two main components of the vote buying index: an indicator for whether respondents accepted any gift (in cash or in kind) from incumbents or challengers (columns 1–4), and the monetary value of gifts received from each candidate type (columns 4–8).[20] The results are very similar to those in Table 4. Worth noting, however, is the magnitude of the effects on challengers. Relative to the control group, respondents in treated villages are approximately 10% more likely to receive gifts from challengers (column 3) and the value of gifts received from challengers increases by 68 UGX, or 20% on average (column 7).

Policy Campaigning. We then explore the effect of ACFIM's intervention on policy campaigning. In columns 3–4 of Table 4, we examine an index of policy campaigning conducted by both types of candidates across presidential and parliamentary races. The index includes: five types of on-the-ground activities (displaying political posters in the village, village visits by candidates, campaigning through loudspeakers, SMS or phone calls, distributing leaflets, and distributing political merchandise) as well as the number of promises made by each type of candidate.[21] Appendix Table B1 reports summary statistics for all the non-standardized components of this index.

Again, consistent with the model's predictions regarding a decrease in reciprocity and a negative incumbent valence shock, columns 5–6 indicate that challengers campaigned more actively in response to the ACFIM intervention. For instance, column 6 indicates a 0.17 sd increase in campaigning activities by challengers in fully treated parishes relative to control parishes. While the difference between

[20] We do not condition on being offered a positive amount of money in these estimates, so they should not be interpreted as price effects, but rather as effects on the average amount offered (including both the intensive and extensive margins).

[21] The four most common types of promises are schools, clinics, improved roads, and improved water wells.

challengers and incumbents is less stark than in the case of vote buying, and the estimate on challenger campaigning in treated villages falls short of statistical significance, this increase in campaigning seems slightly larger for challengers than for incumbents. Overall, 50% of the brokers we interviewed reported that they started to campaign on policies after ACFIM's intervention. However, even for challengers, policy campaigning still appeared to be tied to vote buying. As explained by a broker, "we were looking for votes addressing policy issues but at the same time buying votes because nowadays, in Uganda, people are not really used to talking to them without giving them something."

6.4 Effects on voting and policy

Election Results. In Table 5, columns 1–4, we report treatment effects on electoral support for incumbents in the 2016 presidential and parliamentary races. Regressions conducted using the survey data are estimated at the voter level, while regressions using the electoral data are estimated at the polling-station level. Indices of incumbent support are constructed using the survey data in columns 1 and 2, and using the electoral administrative data in columns 3 and 4. All outcomes in columns 1–4 are standardized to have mean zero in the control group, and pooled across the presidential and parliamentary races.

Table 5: Effects on Voting

	Survey Data		Electoral Data			
			2016		2021	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Polling Station	-0.063*		-0.071		-0.006	
	(0.034)		(0.049)		(0.015)	
	[0.211]		[0.273]		[0.596]	
Spillover Polling Station	-0.065*		-0.005		0.014	
	(0.037)		(0.052)		(0.015)	
	[0.211]		[0.673]		[0.434]	
Saturation		-0.184***		-0.171*		0.005
		(0.064)		(0.096)		(0.031)
		[0.015]		[0.088]		[0.228]
R^2	0.09	0.09	0.49	0.49	0.42	0.42
Control Mean	0.00	0.00	0.00	0.00	0.62	0.62
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	27065	27065	3657	3657	3140	3140

Note: Odd-numbered and even-numbered columns report estimates from equations (1) and (2), respectively. The dependent variables are indices of electoral support for incumbents, pooled across presidential and parliamentary races and constructed from the survey data in columns 1–2 and official electoral data in columns 3–6. All regressions control for an ACFIM dummy (in-sample villages) and the parish-level ACFIM presence (see Section 5).
* p<0.1, ** p<0.05, *** p<0.01. Robust standard errors clustered by parish are reported in parentheses. Sharpened FDR q-values from Anderson (2008) are reported in brackets.

As a cautionary note, whether one should put more weight on the (self-reported) survey data or

the administrative data is a priori unclear. The survey data could be subject to social desirability bias. However, if anything, this bias should be directed towards incumbents, and thus is not a source of major concern given our findings. Moreover, since there is some measurement error in the administrative data because of treated polling stations potentially containing some voters from non-treated villages, the survey data may yield more precise estimates of actual voting behavior. Lastly, there were allegations of vote fraud in the 2016 elections. We discuss this possibility in Appendix B.4.

Overall, consistent with our theoretical predictions regarding a negative shock to reciprocity and incumbent valence, the estimates across both data sources suggest that ACFIM's intervention negatively affected the electoral performance of incumbents, to the benefit of their opponents. The survey data yields statistically significant effects in treated and spillover villages as well as in high-saturation parishes relative to control parishes. Incumbent candidates suffered a loss of around 0.06 sd in treated and spillover villages, and of 0.18 sd in fully treated parishes. The latter effect roughly corresponds to a 3.2 percentage point decline in the incumbent's vote share in the presidential ballot, and a 4.2 percentage point decline in the vote share of incumbent MPs. These coefficients are similar in magnitude, but less precisely estimated when using the administrative data.

We use administrative data from the 2021 presidential elections, when no ACFIM intervention took place, to evaluate long-run impacts of the ACFIM intervention on electoral outcomes. We focus on the vote share accruing to the presidential incumbent, since polling-station level results are unavailable for parliamentary races.[22] We find a fairly precise null long-term effect of the ACFIM intervention. We discuss potential explanations for this result below, after introducing results on policy outcomes.

Policy Outcomes. While the intervention increased vote buying, it also increased political competition, and thus, the welfare effects of the ACFIM intervention are theoretically unclear. To shed light on these effects, we examined the impact of the ACFIM campaign on one policy outcome observed after the 2016 elections: standardized average test scores after the 2016-2017 academic year in schools located near the polling stations in our sample. Education spending constitutes a sizeable fraction of local budgets in Uganda. Spending decisions are usually made at the level of subcounties, each ruled by a chairman and a council of parish representatives, under the oversight of higher layers of government. We therefore use test scores as a proxy for local government performance one year after the intervention.

In Table 6, we find that test scores in the vicinity of treated polling stations increased by 0.19-0.21 sd (columns 3 and 5); a fully saturated parish would have experienced a 0.29-0.38 sd increase in test scores (columns 4 and 6). By contrast, the landmark intervention of Andrabi et al. (2017) increased test scores by 0.11 sd using report cards in Pakistan, while Björkman (2004) and Reinikka and Svensson (2005) report a 0.42 sd increase in test scores in Ugandan districts exposed to a large education grant program. Our estimates fall within the range between these previous estimates, which is noteworthy for an intervention that was not specifically targeting educational outcomes, but may have affected policy performance through its effects on electoral accountability.

Our experimental design does not allow us to disentangle between the multiple channels that could explain this increase in test scores, and improved service delivery more broadly. Various forces related to

[22] We could successfully match 3,140 polling stations (85% of polling stations in our sample) with 2021 polling stations.

Table 6: Effects of the ACFIM Intervention on Test Scores

	In sample		Test scores			
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.0435		0.210***		0.186***	
	(0.0300)		(0.0745)		(0.0609)	
Spillover	0.0316		0.0478		0.0396	
	(0.0286)		(0.0828)		(0.0656)	
Saturation		0.0295		0.381***		0.286***
		(0.0528)		(0.130)		(0.107)
R^2	0.094	0.093	0.200	0.201	0.408	0.407
Control for 2015 test scores	No	No	No	No	Yes	Yes
Observations	3441	3441	2052	2052	2052	2052

Note: Odd-numbered and even-numbered columns report estimates from equations (1) and (2), respectively. The dependent variable is the standardized average test scores from the school-level Primary Leaving Examinations (PLE) administered by the Uganda National Examinations Board at the end of the 2016-2017 academic year. All regressions control for an ACFIM dummy (in-sample villages) and the parish-level ACFIM presence (see Section 5). In columns 5–6, we include control for 2015 test scores.
* p<0.1, ** p<0.05, *** p<0.01. Robust standard errors clustered by parish are reported in parentheses.

both selection (better candidates were elected after the ACFIM intervention) and reduced moral hazard (elected candidates exerted higher effort in treated areas) might have been at play, although we deem it unlikely that the intervention changed the identity of elected candidates at the MP and subcounty level. More likely, local officials could have exerted higher effort in response to the threat posed by increased competition from challengers, to the higher incentives to campaign on policy achievements, or both. Finally, these channels could have operated not only at the parliamentary level, but also at lower levels of government (subcounty and parish). Nonetheless, the effects we observe on test scores imply that the intervention, despite having failed to reduce vote buying during the 2016 electoral campaign, improved welfare, at least in the short run. This might explain why we do not observe long-run effects on electoral outcomes. We elaborate on this discussion in the following section.

6.5 Taking stock

Our results show that ACFIM's intervention had meaningful short-run effects on voters' attitudes and perceptions of social sanctions against vote selling, vote buying and campaigning tactics by candidates, electoral outcomes, and service delivery. Challengers attempted to buy more votes and campaigned more actively in villages exposed to the ACFIM intervention, while incumbents did not respond in the same way. These results are largely driven by parishes with high treatment saturation, with little difference between the point estimates for treatment and spillover villages. Consequently, any theoretical explanation for these findings should account for effects of the ACFIM campaign that permeate the entire treated parish, rather than solely treated villages within those parishes.

The model in Section 3 illustrates the forces likely driving these effects. First, the intervention

changed perceptions of vote buying and fostered collective coordination against this practice. Voters expected social sanctions against vote sellers and no longer thought of vote-buying offers as binding contracts, as reflected in the adage "Eat widely, vote wisely" endorsed in many treated villages. Incumbents, who bought most votes before the intervention, were disproportionately hurt by these changes in attitudes, while challengers started to buy more votes and to campaign more intensively in treated areas. This led to short-run electoral gains for challengers and improvements in service delivery measured via test scores. The former likely resulted both from shifts in challengers' strategies and from the voters' higher propensity to not reciprocate gifts. The latter may have resulted from a combination of selection and moral hazard channels affecting both newly elected challengers and incumbents, who may have exerted higher effort in response to the intensified competition in local electoral markets.

Next, we rule out the possibility that alternative explanations can fully account for these results. First, it is possible that the campaign did diminish vote buying, but (contrary to usual expectations about the sign of social desirability biases), induced people to more honestly report vote buying in their villages. This does not seem to be the case. For instance, we found no significant effect of the campaign on self-reported vote buying in the 2011 election (Table 1). Since the 2011 election pre-dated the ACFIM intervention, there should be no relationship between treatment assignment and 2011 vote buying, except through a social desirability or salience channel. In addition, our results on norms about vote buying suggest that the campaign intensified negative feelings about vote buying.

Second, the campaign might have made vote buying more salient in treated communities. However, this explanation cannot fully account for our findings. In the context of our experiment, well-funded incumbents buy significantly more votes than poorly-financed challengers. Had the campaign only increased the salience of vote buying, then we should have observed an increase in reported voted buying by incumbents relative to that of the challengers. However, our findings indicate the opposite.

Third, the campaign may have deterred electoral fraud that otherwise would have favored the incumbents by engaging citizens in the electoral process. This is not a concern for the sequence of results in Tables 2, 3, and 4, but it could potentially affect the results in Table 5. However, Appendix B8 shows that there is no evidence that ACFIM's intervention is associated (either positively or negatively) with the presence of markers for electoral irregularities.

Finally, while the ACFIM intervention failed to reduce vote-buying, the results on a host of outcomes and the theoretical model provide clear guidance on how to obtain increased political competition and improved service delivery while reducing vote-buying. Specifically, vote buying interventions should be conducted well ahead of elections, in order to also induce a reduction in vote buying by incumbents.

7 Conclusion

This paper explores the impacts of a large-scale civil society intervention against vote buying in Uganda in 2016. We found that ACFIM's intervention failed to reduce vote buying but led to short-term electoral gains for challengers, as well as improvements in service delivery measured via test scores. We analyze these findings in light of a theoretical model featuring several key dimensions of our empirical setting:

incumbents have a valence and first-mover advantage, vote buying and campaigning are complementary, and voter reciprocity increases the effectiveness of vote buying. These structural characteristics are broadly applicable to other electoral markets in a variety of contexts.

Despite the intervention's inability to convince voters to refuse gifts from candidates, we provide evidence that it was successful at weakening reciprocity and at reducing the valence and first-mover advantage of incumbents. In response to these shifts in the electoral field, challengers and their brokers, unlike those operating for incumbents, intensified their vote-buying and campaigning efforts in treated areas. As a result, the intervention undermined the electoral performance of incumbents in the short run. Overall, our experimental findings provide strong support for the predictions delivered by theory.

Our results on vote buying run counter to previous experimental evidence on other interventions against vote buying (Hicken et al., 2017; Vicente, 2014). The differences between these earlier findings and ours can be explained by a combination of conceptual and empirical differences. ACFIM's intervention was unprecedented in scale, highly publicized, and thus highly visible to candidates. Local brokers attended the community meetings about vote buying and some adjusted their strategies accordingly. One of our key contributions is to show—theoretically and empirically—how shifts in the supply side of the market of votes may induce complex tactical responses that differ across types of candidates.

Future work should continue to explore how to break down the vote-buying equilibrium. The ACFIM intervention provided an opportunity to study some previously unexplored features of markets for votes. In doing so, we revisited some of our prior assumptions on the functioning of these markets in Uganda. Our findings highlight that one-sided, highly visible interventions may fail to eradicate vote buying if candidates are able to strategically respond to them. In light of this, one interesting avenue for subsequent research may be to introduce variation in the timing of interventions taking place before elections, as this timing dimension may have far-reaching impacts on the types of strategic adjustments made by candidates. Moreover, our findings pave the way for future work exploring how anti-vote buying interventions might lead to greater service delivery, either via selection or greater moral hazard mitigation. The paper ultimately provides new insights on the potential of interventions against vote buying to promote a fairer electoral playing field in various settings, and on the challenges these interventions might face in practice.

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Appendix (For Online Publication)

A Proofs and Simulations

A.1 Equilibrium characterization

The first order conditions for O's maximization problem, defined by equation (3.3) are:

$$\rho \leq b_O - \psi c_O + \eta(\beta\rho b_I) \tag{A1}$$

$$1 \leq c_O - \psi b_O \tag{A2}$$

From (A1) and (A2), it follows that:

$$b_O^*(b_I, c_I) = \max\left\{\frac{\rho+\psi}{(1-\psi^2)} - \frac{\beta\rho\eta}{(1-\psi^2)}b_I, 0\right\} \tag{A3}$$

and

$$c_O^*(b_I, c_I) = \max\left\{\frac{1+\rho\psi}{(1-\psi^2)} - \frac{\beta\rho\psi\eta}{(1-\psi^2)}b_I, \right\} \tag{A4}$$

In turn, assuming an interior solution, the first order conditions for I's maximization problem are:

$$\frac{\beta\rho\eta}{(1-\psi)} + \beta\rho = b_I - \psi c_I \tag{A5}$$

$$\frac{\beta\rho\eta}{(1-\psi)} + \beta = c_I - \psi b_I \tag{A6}$$

Then, from (A5), and (A6), the equilibrium (b_I^*, c_I^*) are:

$$b_I^* = \frac{\beta\rho\eta}{(1-\psi)^2} + \frac{\beta(\rho+\psi)}{(1-\psi^2)} \tag{A7}$$

and

$$c_I^* = \frac{\beta\rho\eta}{(1-\psi)^2} + \frac{\beta(1+\rho\psi)}{(1-\psi^2)} \tag{A8}$$

Then, using (A3), (A4), (A7), and (A8), the equilibrium (b_O^*, c_O^*) are:

$$b_O^* = \max\left\{\frac{\rho+\psi}{(1-\psi^2)} - \frac{(\beta\rho\eta)^2}{(1-\psi^2)(1-\psi)^2} - \frac{\beta^2\rho\eta(\rho+\psi)}{(1-\psi^2)^2}, 0\right\} \tag{A9}$$

and

$$c_O^* = \max\left\{\frac{1+\psi\rho}{(1-\psi^2)} - \frac{(\beta\rho\eta)^2\psi}{(1-\psi^2)(1-\psi)^2} - \frac{\beta^2\rho\eta\psi(\rho+\psi)}{(1-\psi^2)^2}, 0\right\} \tag{A10}$$

A.2 Comparative Statics

Case 1: Anti-vote buying intervention at $t = 0$

The comparative statics with respect to ρ are given by:

$$\frac{\partial b_I^*}{\partial \rho} = \frac{\beta\eta}{(1-\psi)^2} + \frac{\beta}{(1-\psi^2)} > 0,$$
$$\frac{\partial c_I^*}{\partial \rho} = \frac{\beta\eta}{(1-\psi)^2} + \frac{\beta\psi}{(1-\psi^2)} > 0,$$

$$\frac{\partial b_O^*}{\partial \rho} = \frac{1}{(1-\psi^2)} - \frac{2\beta^2\rho\eta^2}{(1-\psi^2)(1-\psi)^2} - \frac{\beta^2\eta(2\rho+\psi)}{(1-\psi^2)^2} \lesseqgtr 0,$$
$$\frac{\partial c_O^*}{\partial \rho} = \frac{\psi}{(1-\psi^2)} - \frac{2\beta^2\rho\eta^2\psi}{(1-\psi^2)(1-\psi)^2} - \frac{\beta^2\eta\psi(2\rho+\psi)}{(1-\psi^2)^2} \lesseqgtr 0,$$

and

$$\frac{\partial \Pi_I}{\partial \rho} = \beta\frac{\partial c_I^*}{\partial \rho} - \frac{\partial c_O^*}{\partial \rho} + (\beta b_I^* - b_O^*) + \rho\left(\beta\frac{\partial b_I^*}{\partial \rho} - \frac{\partial b_O^*}{\partial \rho}\right) > 0,$$

given that it is trivial to see that $\frac{\partial c_I^*}{\partial \rho} > \frac{\partial c_O^*}{\partial \rho}$, $b_I^* > b_O^*$, and $\frac{\partial b_I^*}{\partial \rho} > \frac{\partial b_O^*}{\partial \rho}$, and $\beta > 1$.

Then, focusing on comparative statics with respect to β:

$$\frac{\partial b_I^*}{\partial \beta} = \frac{\rho\eta}{(1-\psi)^2} + \frac{(\rho+\psi)}{(1-\psi^2)} > 0,$$
$$\frac{\partial c_I^*}{\partial \beta} = \frac{\rho\eta}{(1-\psi)^2} + \frac{(1+\rho\psi)}{(1-\psi^2)} > 0,$$

$$\frac{\partial b_O^*}{\partial \beta} = -\frac{2\beta\rho^2\eta^2}{(1-\psi^2)(1-\psi)^2} - \frac{2\beta\rho\eta(\rho+\psi)}{(1-\psi^2)^2} < 0,$$
$$\frac{\partial c_O^*}{\partial \beta} = -\frac{2\beta\rho^2\eta^2\psi}{(1-\psi^2)(1-\psi)^2} - \frac{2\beta\rho\eta\psi(\rho+\psi)}{(1-\psi^2)^2} < 0,$$

and

$$\frac{\partial \Pi_I}{\partial \beta} = c_I^* + \beta\frac{\partial c_I^*}{\partial \beta} - \frac{\partial c_O^*}{\partial \beta} + \rho\left(b_I^* + \beta\frac{\partial b_I^*}{\partial \beta} - \frac{\partial b_O^*}{\partial \beta}\right) > 0,$$

given that it is trivial to see that $\frac{\partial c_I^*}{\partial \beta} > 0$, $\frac{\partial c_O^*}{\partial \beta} < 0$, $\frac{\partial b_I^*}{\partial \beta} > 0$, and $\frac{\partial b_O^*}{\partial \beta} < 0$, and $\beta > 1$.

Case 2: Anti-vote buying intervention between $t = 1$ and $t = 2$

Considering b_I^* and c_I^* as fixed, from (A3) and (A4), the comparative statics for b_O, c_O, and Π_I with respect to ρ are as follows:

$$\frac{\partial b_O^*}{\partial \rho} = \frac{1}{(1-\psi^2)} - \frac{\beta\eta}{(1-\psi^2)}b_I^* \lesseqgtr 0,$$

$$\frac{\partial c_O^*}{\partial \rho} = \frac{\psi}{(1-\psi^2)} - \frac{\beta\psi\eta}{(1-\psi^2)} b_I^* \lesseqgtr 0,$$

and

$$\frac{\partial \Pi_I}{\partial \rho} = -\frac{\partial c_O^*}{\partial \rho} + (\beta b_I^* - b_O^*) - \rho \frac{\partial b_O^*}{\partial \rho} > 0,$$

as long as $\beta > \sqrt{2}$, given what follows. First, note that using (A3), and the expression of $\frac{\partial b_O^*}{\partial \rho}$ and $\frac{\partial c_O^*}{\partial \rho}$,

$$(1-\psi^2)\frac{\partial \Pi_I}{\partial \rho} = -2(\rho+\psi) + \beta b_I^*(1-\psi^2+\eta(2\rho+\psi)),$$

and thus, $\frac{\partial \Pi_I}{\partial \rho} > 0$ as long as

$$\beta b_I^* > 2\frac{\rho+\psi}{(1-\psi^2+\eta(2\rho+\psi))}.$$

From (A7) and using that $\eta(2\rho+\psi) > 0$, it follows that

$$\beta b_I^* > \beta^2 \frac{\rho+\psi}{(1-\psi^2+\eta(2\rho+\psi))},$$

and thus $\frac{\partial \Pi_I}{\partial \rho} > 0$ as long as $\beta > \sqrt{2}$.

Similarly, the comparative statics for b_O, c_O, and Π_I with respect to β are as follows:

$$\frac{\partial b_O^*}{\partial \beta} = -\frac{\rho\eta}{(1-\psi^2)} b_I^* < 0,$$

$$\frac{\partial c_O^*}{\partial \beta} = -\frac{\rho\psi\eta}{(1-\psi^2)} b_I^* < 0.$$

and

$$\frac{\partial \Pi_I}{\partial \beta} = c_I^* - \frac{\partial c_O^*}{\partial \beta} + \rho\left(b_I^* - \frac{\partial b_O^*}{\partial \beta}\right) > 0,$$

given that $\frac{\partial c_O^*}{\partial \beta} < 0$ and $\frac{\partial b_O^*}{\partial \beta} < 0$.

Case 3: Anti-vote buying intervention between $t = 2$ and $t = 3$

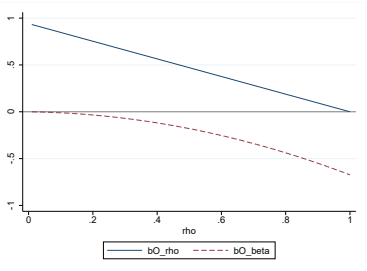
Considering b_I^*, c_I^*, b_O^*, and $c_)^*$ as fixed, the comparative static for Π_I with respect to ρ is as follows:

$$\frac{\partial \Pi_I}{\partial \rho} = \beta b_I^* - b_O^* > 0,$$

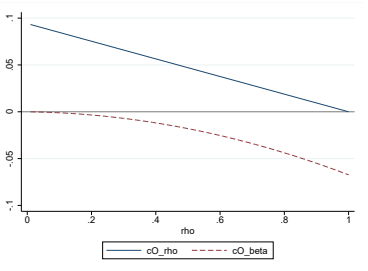
given that $b_I^* > b_O^*$. Similarly, the comparative static for Π_I with respect to β is as follows:

$$\frac{\partial \Pi_I}{\partial \beta} = c_I + \rho b_I > 0.$$

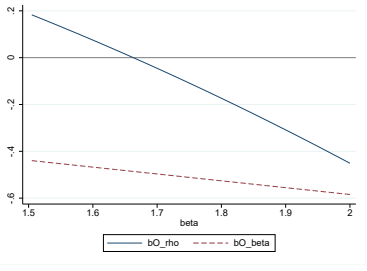
A.3 Additional Simulations



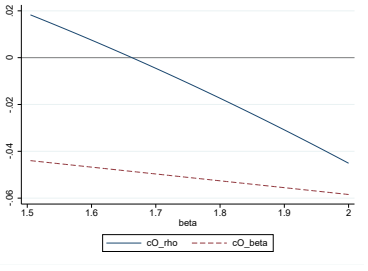
(a) $\frac{\partial b_O^*}{\partial \rho}$ and $\frac{\partial b_O^*}{\partial \beta}$ by ρ



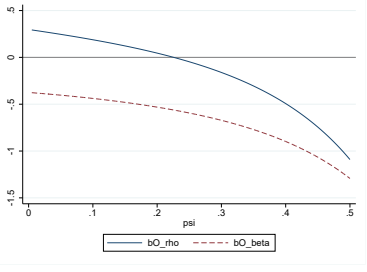
(b) $\frac{\partial c_O^*}{\partial \rho}$ and $\frac{\partial c_O^*}{\partial \beta}$ by ρ



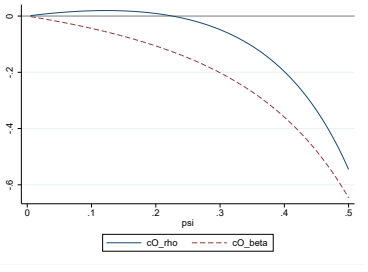
(c) $\frac{\partial b_O^*}{\partial \rho}$ and $\frac{\partial b_O^*}{\partial \beta}$ by β



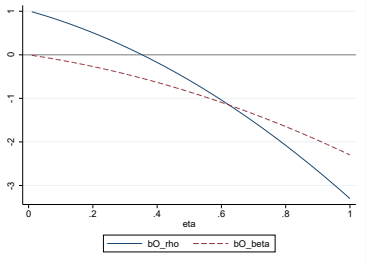
(d) $\frac{\partial c_O^*}{\partial \rho}$ and $\frac{\partial c_O^*}{\partial \beta}$ by β



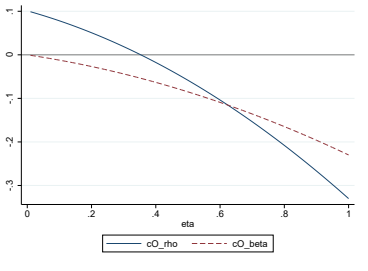
(e) $\frac{\partial b_O^*}{\partial \rho}$ and $\frac{\partial b_O^*}{\partial \beta}$ by ψ



(f) $\frac{\partial c_O^*}{\partial \rho}$ and $\frac{\partial c_O^*}{\partial \beta}$ by ψ



(g) $\frac{\partial b_O^*}{\partial \rho}$ and $\frac{\partial b_O^*}{\partial \beta}$ by η



(h) $\frac{\partial c_O^*}{\partial \rho}$ and $\frac{\partial c_O^*}{\partial \beta}$ by η

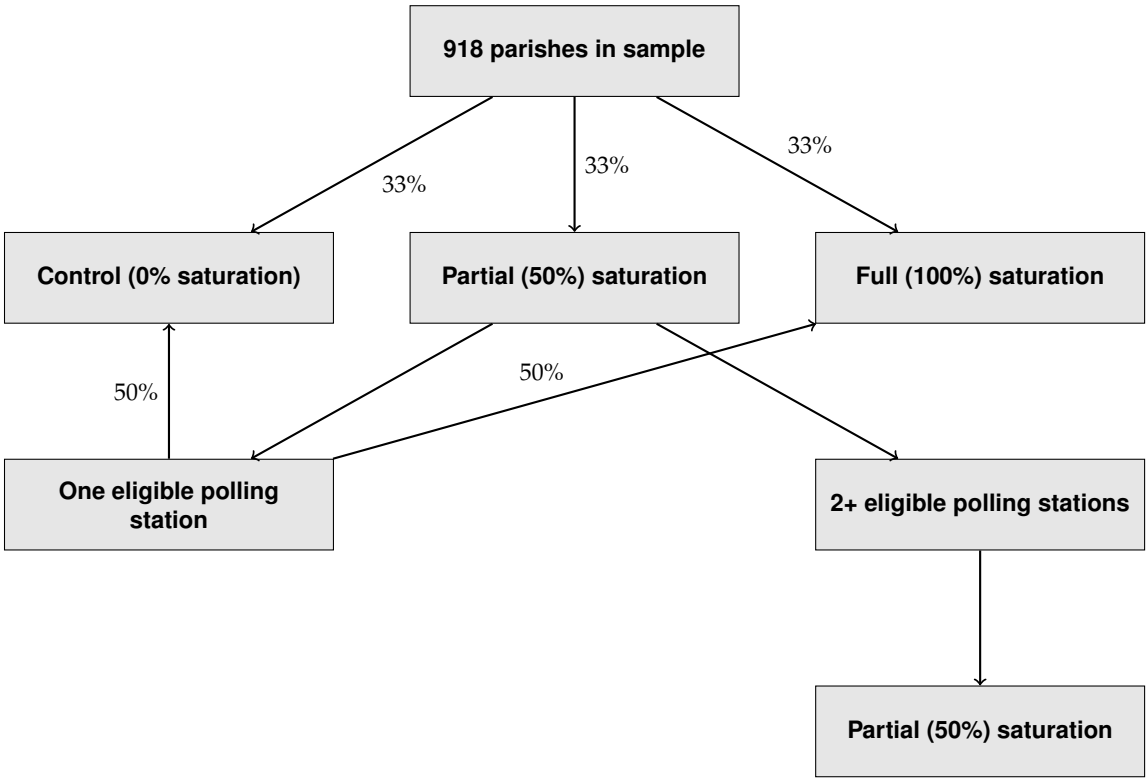
Case 2 comparative statics for b_O^* and c_O^* with respect to the model parameters

Note: Continuous blue lines correspond to comparative statics with respect to ρ, while dashed red lines correspond to comparative statics with respect to β. Simulations are with $\rho = 0.8$, $\beta = 1.5$, $\psi = 0.1$ and $\eta = 0.3$ unless we are plotting how equilibrium outcomes vary when changing the corresponding parameter.

B Randomization Details and Validation Checks

B.1 Randomization design

Figure B1: Overview of the Randomized Saturation Design



B.2 Summary statistics

Table B1: Summary Statistics

Survey Data	Mean	SD	N
Vote buying in 2011	0.217	0.412	28,507
Voted for incumbent president in 2011	0.834	0.372	21,823
Vote-buying has negative consequences for the village	0.895	0.306	28,507
Village adopted resolution at the end of the intervention	0.063	0.243	28,507
Village adopted an *"Eat widely, Vote wisely"* resolution	0.044	0.206	28,507
Village adopted a *"No vote buying"* resolution	0.019	0.135	28,507
Vote buying will be socially sanctioned	0.796	0.403	28,507
Breaking reciprocity, subjective measure	0.051	0.219	28,507
Breaking reciprocity, objective measure	0.124	0.329	28,507
Feeling close to incumbents	0.315	0.465	28,507
Received a gift from incumbents	0.313	0.464	28,507
Received a gift from challengers	0.162	0.369	28,507
Value of gifts from incumbents	1028.34	2573.78	28,507
Value of gifts from challengers	392.32	1417.57	28,507
Visits from incumbents	0.727	0.694	28,507
Posters from incumbents	1.244	0.765	28,507
Incumbents campaigned via loudspeaker, SMS or phone	0.588	0.678	28,507
Incumbents distributed campaign leaflets	0.405	0.633	28,507
Incumbents distributed political merchandise	0.541	0.706	28,507
Number of policy promises from incumbents	2.723	3.11	28,507
Visits from challengers	1.298	0.913	28,507
Posters from challengers	1.794	0.954	28,507
Challengers campaigned via loudspeaker, SMS or phone	0.989	0.93	28,507
Challengers distributed campaign leaflets	0.668	0.895	28,507
Challengers distributed political merchandise	0.445	0.747	28,507
Number of policy promises from challengers	4.66	4.234	28,507
Voted for incumbents in 2016	0.657	0.349	27,112

Electoral Data	Mean	SD	N
Incumbent president vote share, 2011	0.678	0.186	3,641
Incumbent president vote share, 2016	0.614	0.184	3,654
Incumbent president vote share, 2021	0.627	0.224	3,140
Incumbent MP vote share, 2016	0.441	0.246	3,104

B.3 Randomization Balance

We present randomization checks in Appendix Tables B2 through B7. From the voter survey, we use the age, years of education, marital status (an indicator variable for married individuals), land ownership (an indicator for households that own any land), the number of adults and children in the household, an index of asset ownership,[23] variables indicating the individual belongs to one of Uganda's three largest ethnic groups (Ganda, Nkole and Soga), and three indicator variables for reporting beling a Catholic, a Protestant, or a Muslim.

From the key informant survey, we use the years of education and marital status of the respondent, as well as the same four measures of occupational status, ethnicity and religion as above (note that age, land ownership, number of members in the household and assets were not collected in the key informant survey), as well as four indicator variables for whether the key informant is a local chief or elder, a member of a civil society group (a religious, youth, or women's group), a village committee member or a local council member.

Finally, from the official electoral data we use the number of registered voters in 2011, the voter turnout in 2011, the presidential vote shares of the NRM and of the FDC in 2011, the vote share of the winner of the parliamentary vote in 2011 (i.e., the 2011 vote share of the 2016 incumbent MP), and the number of registered voters in 2016.

[23] To construct this index, we simply add up the variables indicating ownership of a TV, radio, motor vehicle, and cell phone four measures of occupational status (indicator variables for individuals working in farming, trade/retail, any high-skill activity, or not actively working). High-skill individuals include artisans or skilled manual workers, clerks and secretaries, supervisors, managers, security providers, mid-level professionals such as teachers, and upper-level professionals. Individuals not actively working include students as well as unemployed, retired, and disabled individuals.

Table B2: Balance on Voter Respondent's Characteristics

	Age		Years Education		Married		Own Land		Adults		Children	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment village	-0.247		0.012		-0.011		-0.002		-0.014		-0.078	
	(0.302)		(0.117)		(0.010)		(0.010)		(0.049)		(0.068)	
Spillover	0.123		-0.120		-0.006		0.002		-0.044		-0.223***	
	(0.338)		(0.146)		(0.011)		(0.011)		(0.057)		(0.075)	
Treatment Saturation		-0.079		-0.004		-0.011		0.008		-0.038		-0.197
		(0.494)		(0.213)		(0.018)		(0.020)		(0.090)		(0.130)
R^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.01
Control Mean	40.09	40.09	5.49	5.49	0.74	0.74	0.87	0.87	3.18	3.18	3.55	3.55
Control SD	15.48	15.48	4.31	4.31	0.44	0.44	0.33	0.33	1.75	1.75	2.36	2.36
Observations	27375	27375	28452	28452	28454	28454	28454	28454	28454	28454	28451	28451

Note: Odd-numbered columns report estimates from equation (1) and even-numbered columns report estimates from equation (2). All dependent variables come from the voter survey data (see text for details).
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table B3: Balance on Voter Respondent's Characteristics (Continues)

	Assets		Farmer		Trade		High Skill		Not Working	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment village	-0.012		0.025		-0.008		-0.005		-0.008	
	(0.032)		(0.016)		(0.008)		(0.006)		(0.005)	
Spillover	0.011		0.015		-0.006		-0.011		-0.002	
	(0.038)		(0.021)		(0.009)		(0.008)		(0.007)	
Treatment Saturation		-0.030		0.028		-0.012		-0.008		-0.009
		(0.059)		(0.033)		(0.014)		(0.012)		(0.009)
R^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Control Mean	1.64	1.64	0.69	0.69	0.09	0.09	0.08	0.08	0.05	0.05
Control SD	0.98	0.98	0.46	0.46	0.28	0.28	0.27	0.27	0.22	0.22
Observations	28454	28454	28453	28453	28453	28453	28453	28453	28453	28453

Note: Odd-numbered columns report estimates from equation (1) and even-numbered columns report estimates from equation (2). All dependent variables come from the voter survey data (see text for details).
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table B4: Balance on Voter Respondent's Characteristics (Continues)

	Ganda		Nkole		Soga		Catholic		Protestant		Muslim	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment village	0.015		-0.011		-0.018		0.035*		-0.022		-0.017	
	(0.014)		(0.014)		(0.015)		(0.019)		(0.018)		(0.012)	
Spillover	0.021		-0.009		-0.029		0.049**		-0.022		-0.028**	
	(0.018)		(0.017)		(0.018)		(0.021)		(0.020)		(0.014)	
Treatment Saturation		0.002		-0.011		-0.028		0.082**		-0.039		-0.043*
		(0.029)		(0.018)		(0.031)		(0.036)		(0.035)		(0.023)
R^2	0.01	0.00	0.03	0.03	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Control Mean	0.07	0.07	0.06	0.06	0.06	0.06	0.42	0.42	0.43	0.43	0.09	0.09
Control SD	0.26	0.26	0.24	0.24	0.24	0.24	0.49	0.49	0.50	0.50	0.28	0.28
Observations	28451	28451	28451	28451	28451	28451	28454	28454	28454	28454	28454	28454

Note: Odd-numbered columns report estimates from equation (1) and even-numbered columns report estimates from equation (2). All dependent variables come from the voter survey data (see text for details).
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table B5: Balance on Key Informant Respondent's Characteristics

	Chief or Elder		Civil Society		Village Committee		Local Council	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment village	-0.019		0.009		-0.001		0.026	
	(0.017)		(0.008)		(0.026)		(0.021)	
Spillover	0.023		-0.003		-0.045		0.012	
	(0.023)		(0.008)		(0.031)		(0.025)	
Treatment Saturation		-0.038		0.014		-0.015		0.072*
		(0.031)		(0.011)		(0.047)		(0.039)
R^2	0.01	0.01	0.00	0.00	0.02	0.02	0.01	0.01
Control Mean	0.19	0.19	0.03	0.03	0.43	0.43	0.25	0.25
Control SD	0.39	0.39	0.17	0.17	0.50	0.50	0.43	0.43
Observations	4090	4090	4090	4090	4090	4090	4090	4090

Note: Odd-numbered columns report estimates from equation (1) and even-numbered columns report estimates from equation (2). All dependent variables come from the key informant survey data (see text for details).
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table B6: Balance on Key Informant Respondent's Characteristics (Continues)

	Ganda		Nkole		Soga		Catholic		Protestant		Muslim	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment village	0.006		-0.004		-0.015		0.005		-0.013		-0.018	
	(0.024)		(0.016)		(0.016)		(0.025)		(0.025)		(0.015)	
Spillover	0.031		-0.005		-0.029*		0.029		-0.001		-0.026	
	(0.030)		(0.017)		(0.016)		(0.029)		(0.028)		(0.016)	
Treatment Saturation		-0.003		0.003		-0.021		0.033		-0.023		-0.043
		(0.055)		(0.029)		(0.035)		(0.046)		(0.045)		(0.030)
R^2	0.03	0.02	0.02	0.02	0.01	0.00	0.00	0.00	0.00	0.00	0.01	0.01
Control Mean	0.09	0.09	0.06	0.06	0.06	0.06	0.45	0.45	0.42	0.42	0.09	0.09
Control SD	0.29	0.29	0.24	0.24	0.24	0.24	0.50	0.50	0.49	0.49	0.29	0.29
Observations	4090	4090	4090	4090	4090	4090	4090	4090	4090	4090	4090	4090

Note: Odd-numbered columns report estimates from equation (1) and even-numbered columns report estimates from equation (2). All dependent variables come from the key informant survey data (see text for details).
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table B7: Balance on Pre-determined Electoral Data

	Reg'd Voters 2011		Turnout 2011		NRM Vote 2011		FDC Vote 2011		MP Incumbent Vote 2011		Reg'd Voters 2016	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment Polling Station	-284.303		0.004		-0.010		0.005		-0.019		-2.988	
	(196.161)		(0.008)		(0.014)		(0.013)		(0.015)		(10.425)	
Spillover Polling Station	-374.461		0.000		-0.006		-0.006		-0.001		-14.056	
	(375.616)		(0.009)		(0.017)		(0.015)		(0.016)		(9.117)	
Treatment Saturation		-699.026		0.004		-0.024		0.013		-0.035		4.987
		(458.784)		(0.018)		(0.029)		(0.026)		(0.029)		(17.359)
R^2	0.04	0.04	0.03	0.03	0.04	0.04	0.00	0.00	0.01	0.01	0.04	0.04
Control Mean	3007.687	3007.687	0.600	0.600	0.685	0.685	0.262	0.262	0.554	0.554	575.130	575.130
Control SD	2839.022	2839.022	0.104	0.104	0.182	0.182	0.158	0.158	0.189	0.189	196.410	196.410
Observations	3641	3641	3641	3641	3641	3641	3641	3641	3214	3214	3659	3659

Note: Odd-numbered columns report estimates from equation (1) and even-numbered columns report estimates from equation (2). All dependent variables come from the official electoral data provided by the Ugandan Electoral Commission (see text for details).
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

B.4 Electoral data integrity

Opposition leaders in Uganda and international observers challenged the integrity of the voting data in the aftermath of the election. We acknowledge these issues, but believe that the electoral data can still be useful for our analysis for several reasons.

First, we generally obtain similar results using self-reported voting outcomes from our voter survey and using the official election data. Second, in Table B8, we show that treatment assignment and saturation do not significantly correlate with "suspicious polling stations," defined by being either at least 2 standard deviations above the sample average in both turnout and support for the incumbent president, the incumbent MP, or both. Overall, there is no evidence that treatment and spillover assignment or parish saturation correlate with electoral malfeasance.

Third, we show that our treatment is uncorrelated with traditional markers of electoral malfeasance (Beber and Scacco, 2012). Specifically, Table B9 indicates that treatment and spillover assignment, and parish saturation are uncorrelated with the last digit of the polling station valid votes and votes in favor of incumbents being rounded off to zero or to 5, which is usually associated with electoral fraud (Beber and Scacco, 2012). Only 1 out of 24 coefficients in this table is significant at 10%. Note that the mean of the dependent variable in columns 5, 6, 13 and 14 highlight an abnormal share of polling stations with valid votes and votes in favor of incumbents rounded off to zero for the parliamentary vote. However, this rounding pattern is uncorrelated with treatment and spillover assignment or saturation, which confirms the validity of our estimates using the official election data.

Table B8: Electoral Checks - Abnormal Returns

	Above 2SD, Pres		Above 2SD, MP		Above 2SD, Both	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Polling Station	0.001		0.045		-0.000	
	(0.002)		(0.031)		(0.000)	
Spillover Polling Station	-0.000		0.062**		-0.001	
	(0.002)		(0.030)		(0.001)	
Saturation		0.004		0.090		-0.000
		(0.003)		(0.062)		(0.000)
R^2	0.01	0.01	0.07	0.06	0.00	0.00
Control Mean	0.001	0.001	0.110	0.110	0.001	0.001
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3192	3192	3192	3192	3192	3192

Note: This table tests whether abormal voting returns correlate with treatment status and parish-level treatment saturation. Abnormal returns are defined as returns were both voter turnout and vote tallies for incumbents are 2SD above the mean in the electoral data, for the presidential race (columns 1–2), parliamentary races (columns 3–4), or both races (columns 5–6).
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

Table B9: Electoral Checks - Rounding

	Presidential (Total Valid)				MP (Total Valid)				Presidential (Incumbent Vote)				MP (Incumbent Vote)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	0	0	5	5	0	0	5	5	0	0	5	5	0	0	5	5
Treatment Polling Station	-0.00		-0.02		0.02		0.01		0.01		-0.00		0.03		-0.01	
	(0.02)		(0.02)		(0.03)		(0.02)		(0.02)		(0.02)		(0.03)		(0.02)	
Spillover Polling Station	-0.02*		-0.00		0.05		-0.01		0.00		-0.02		0.05		-0.00	
	(0.01)		(0.01)		(0.03)		(0.01)		(0.01)		(0.01)		(0.03)		(0.01)	
Saturation		-0.00		-0.04		0.05		-0.00		0.02		0.02		0.10		0.02
		(0.02)		(0.03)		(0.06)		(0.02)		(0.02)		(0.02)		(0.06)		(0.02)
R^2	0.00	0.00	0.00	0.00	0.04	0.04	0.01	0.01	0.00	0.00	0.00	0.00	0.04	0.04	0.00	0.00
Control Mean	0.100	0.100	0.105	0.105	0.202	0.202	0.091	0.091	0.100	0.100	0.093	0.093	0.223	0.223	0.080	0.080
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3192	3192	3192	3192	3192	3192	3192	3192	3192	3192	3192	3192	3192	3192	3192	3192

Note: This table tests whether treatment status and parish-level treatment saturation correlate with the likelihood that vote counts were rounded to 0 or 5 in the electoral data. Vote counts are measured as total valid votes in columns 1–8, and valid as votes for incumbents in columns 9–16.
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets.

C Additional Results and Robustness Checks

Table C1: Village Decision (Key Informants)

	No Vote Buying Village		Eat Widely Vote Wisely	
	(1)	(2)	(3)	(4)
Treatment village	0.083***		0.105***	
	(0.010)		(0.011)	
Spillover	0.004		0.006	
	(0.007)		(0.010)	
Treatment Saturation		0.101***		0.141***
		(0.015)		(0.020)
R^2	0.03	0.02	0.04	0.03
Control Mean	0.020	0.020	0.042	0.042
Observations	4195	4195	4195	4195

Note: This table reports results from survey data collected with key informants in every village. Odd-numbered columns report estimates from equation (1) and even-numbered columns report estimates from equation (2), described in section 5. Dependent variables in this table are indicators for: whether the village adopted a common resolution to refuse vote-buying in the village (cols 1–2), and whether the village adopted a common resolution to "eat widely and vote wisely or to accept offers from party representatives, but vote the way we want" (cols. 3–4).
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in parentheses.

Table C2: Value of gifts received from incumbents and challengers

	Any gift received				**Value of gifts received**			
	Incumbents		Challengers		Incumbents		Challengers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment village	-0.005		0.017*		9.055		68.022**	
	(0.012)		(0.010)		(57.080)		(32.324)	
Spillover	0.000		0.025**		-9.221		101.997**	
	(0.014)		(0.011)		(62.401)		(41.657)	
Treatment Saturation		-0.003		0.028		-23.060		96.552*
		(0.020)		(0.019)		(92.671)		(54.522)
R^2	0.10	0.10	0.07	0.07	0.04	0.04	0.04	0.04
Control Mean	0.32	0.32	0.16	0.16	1054.95	1054.95	347.04	347.04
Observations	28454	28454	28454	28454	28454	28454	28454	28454

Note: The dependent variable measures: whether respondents report receiving any gift (in cash or in kind) from candidates of a given type (columns 1–4) and the unconditional value of all gifts received from candidates of a given type (columns 5–8) measured in Uganda Shillings (UGX).
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in parentheses.

Table C3: Robustness Checks on Table 2

	Negative consequences		Village resolution		No vote-buying		Eat widely, vote wisely	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	(a) Baseline estimates							
Treatment village	0.013*		0.082***		0.026***		0.056***	
	(0.007)		(0.005)		(0.003)		(0.005)	
Spillover	0.010		0.002		0.002		-0.000	
	(0.008)		(0.004)		(0.002)		(0.004)	
Saturation		0.028***		0.099***		0.033***		0.066***
		(0.011)		(0.009)		(0.004)		(0.008)
	(b) Controlling for a 4th order polynomial in ACFIM presence							
Treatment village	0.013*		0.082***		0.026***		0.055***	
	(0.007)		(0.005)		(0.003)		(0.005)	
Spillover	0.010		0.003		0.002		0.000	
	(0.008)		(0.004)		(0.002)		(0.004)	
Saturation		0.028***		0.099***		0.033***		0.066***
		(0.011)		(0.009)		(0.004)		(0.008)
	(c) Controlling for deciles of ACFIM presence							
Treatment village	0.012*		0.082***		0.026***		0.055***	
	(0.007)		(0.005)		(0.003)		(0.005)	
Spillover	0.009		0.002		0.002		-0.000	
	(0.008)		(0.004)		(0.002)		(0.004)	
Saturation		0.027**		0.099***		0.034***		0.065***
		(0.011)		(0.009)		(0.004)		(0.008)
	(d) Controlling for strata fixed effects							
Treatment village	0.013**		0.084***		0.027***		0.056***	
	(0.006)		(0.005)		(0.003)		(0.004)	
Spillover	0.013		0.005		0.004*		0.001	
	(0.008)		(0.004)		(0.002)		(0.004)	
Saturation		0.027***		0.102***		0.035***		0.067***
		(0.010)		(0.009)		(0.004)		(0.007)
	(e) Removing controls							
Treatment village	0.013*		0.082***		0.026***		0.056***	
	(0.008)		(0.006)		(0.003)		(0.005)	
Spillover	0.007		-0.000		0.001		-0.001	
	(0.009)		(0.005)		(0.002)		(0.004)	
Saturation		0.031**		0.098***		0.033***		0.066***
		(0.013)		(0.009)		(0.005)		(0.008)
R^2	0.04	0.04	0.03	0.02	0.01	0.01	0.02	0.02
Control Mean	0.89	0.89	0.03	0.03	0.01	0.01	0.03	0.03
Observations	28454	28454	28454	28454	28454	28454	28454	28454

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish. In panel (a), the estimation details as described in the footnote to Table 2. We additionally control for a 4th order polynomial in baseline ACFIM presence (panel b), deciles of ACFIM presence (c), or strata fixed effects (d). In panel (e), we remove all controls other than $ACFIM_{vp}$ and $ACFIM\_Presence_p$ in equations (1) and (2).

Table C4: Robustness Checks on Table 3

	Social sanctions		Breaking reciprocity				Incumbent valence	
			Subjective		Objective			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	(a) Baseline estimates							
Treatment village	0.026***		0.009*		0.013*		-0.047*	
	(0.010)		(0.005)		(0.007)		(0.024)	
Spillover	0.006		-0.002		0.009		-0.061**	
	(0.012)		(0.007)		(0.008)		(0.025)	
Saturation		0.029*		0.016*		0.019		-0.083*
		(0.016)		(0.009)		(0.013)		(0.048)
	(b) Controlling for a 4th order polynomial in ACFIM presence							
Treatment village	0.027***		0.010*		0.013*		-0.046*	
	(0.010)		(0.005)		(0.007)		(0.024)	
Spillover	0.006		-0.002		0.009		-0.062**	
	(0.012)		(0.007)		(0.008)		(0.025)	
Saturation		0.030*		0.017*		0.019		-0.080*
		(0.016)		(0.009)		(0.013)		(0.047)
	(c) Controlling for deciles of ACFIM presence							
Treatment village	0.027***		0.010*		0.013*		-0.045*	
	(0.009)		(0.005)		(0.007)		(0.024)	
Spillover	0.007		-0.002		0.009		-0.058**	
	(0.012)		(0.007)		(0.008)		(0.025)	
Saturation		0.030*		0.017*		0.019		-0.082*
		(0.016)		(0.009)		(0.013)		(0.048)
	(d) Controlling for strata fixed effects							
Treatment village	0.027***		0.009		0.010		-0.047**	
	(0.009)		(0.005)		(0.007)		(0.022)	
Spillover	0.008		-0.004		0.005		-0.064***	
	(0.012)		(0.007)		(0.007)		(0.024)	
Saturation		0.030*		0.016*		0.014		-0.081*
		(0.016)		(0.009)		(0.013)		(0.043)
	(e) Removing controls							
Treatment village	0.030***		0.009*		0.015		-0.056*	
	(0.011)		(0.006)		(0.010)		(0.029)	
Spillover	-0.004		-0.002		0.015		-0.050*	
	(0.014)		(0.007)		(0.010)		(0.030)	
Saturation		0.042**		0.016*		0.026		-0.106*
		(0.020)		(0.010)		(0.019)		(0.055)
R^2	0.04	0.04	0.01	0.01	0.06	0.06	0.16	0.15
Control Mean	0.78	0.78	0.04	0.04	0.12	0.12	0.35	0.35
Observations	28454	28454	28454	28454	28454	28454	28454	28454

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish. In panel (a), the estimation details as described in the footnote to Table 3. We additionally control for a 4th order polynomial in baseline ACFIM presence (panel b), deciles of ACFIM presence (c), or strata fixed effects (d). In panel (e), we remove all controls other than $ACFIM_{vp}$ and $ACFIM\_Presence_p$ in equations (1) and (2).

Table C5: Robustness Checks on Table 4

	Vote buying						Campaigning			
	All		Incumbents		Challengers		Incumbents		Challengers	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	(a) Baseline estimates									
Treatment village	0.041*		0.009		0.062***		0.052		0.048	
	(0.024)		(0.025)		(0.024)		(0.040)		(0.041)	
Spillover	0.016		-0.014		0.048*		-0.013		-0.037	
	(0.026)		(0.027)		(0.028)		(0.044)		(0.046)	
Saturation		0.055		0.010		0.087*		0.111		0.171**
		(0.043)		(0.045)		(0.045)		(0.077)		(0.079)
	(b) Controlling for a 4th order polynomial in ACFIM presence									
Treatment village	0.040*		0.009		0.063***		0.048		0.047	
	(0.024)		(0.025)		(0.024)		(0.040)		(0.041)	
Spillover	0.020		-0.011		0.050*		-0.013		-0.039	
	(0.026)		(0.027)		(0.028)		(0.044)		(0.046)	
Saturation		0.056		0.012		0.087*		0.109		0.168**
		(0.043)		(0.045)		(0.045)		(0.077)		(0.079)
	(c) Controlling for deciles of ACFIM presence									
Treatment village	0.042*		0.012		0.061**		0.050		0.044	
	(0.024)		(0.025)		(0.024)		(0.040)		(0.040)	
Spillover	0.019		-0.010		0.047*		-0.014		-0.042	
	(0.026)		(0.027)		(0.028)		(0.044)		(0.046)	
Saturation		0.060		0.017		0.087*		0.115		0.170**
		(0.043)		(0.045)		(0.045)		(0.076)		(0.079)
	(d) Controlling for strata fixed effects									
Treatment village	0.030		-0.001		0.055**		0.043		0.046	
	(0.023)		(0.024)		(0.024)		(0.036)		(0.038)	
Spillover	0.007		-0.021		0.040		0.009		-0.015	
	(0.025)		(0.027)		(0.026)		(0.041)		(0.043)	
Saturation		0.039		-0.004		0.076*		0.081		0.147**
		(0.041)		(0.042)		(0.045)		(0.068)		(0.074)
	(e) Removing controls									
Treatment village	0.044		0.008		0.070**		0.056		0.067	
	(0.033)		(0.032)		(0.029)		(0.049)		(0.046)	
Spillover	0.041		0.005		0.069**		0.009		-0.018	
	(0.034)		(0.033)		(0.034)		(0.052)		(0.052)	
Saturation		0.062		0.007		0.105*		0.117		0.209**
		(0.061)		(0.058)		(0.056)		(0.092)		(0.090)
R^2	0.06	0.06	0.05	0.05	0.04	0.04	0.13	0.13	0.11	0.11
Control Mean	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Observations	28454	28454	28454	28454	28454	28454	28454	28454	28454	28454

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish. In panel (a), the estimation details as described in the footnote to Table 4. We additionally control for a 4th order polynomial in baseline ACFIM presence (panel b), deciles of ACFIM presence (c), or strata fixed effects (d). In panel (e), we remove all controls other than $ACFIM_{vp}$ and $ACFIM\_Presence_p$ in equations (1) and (2).

Table C6: Robustness Checks on Table 5

	Survey data		Electoral Data			
	(1)	(2)	(3)	(4)	(5)	(6)
	(a) Baseline estimates					
Treatment Polling Station	-0.063*		-0.071		-0.006	
	(0.034)		(0.049)		(0.015)	
Spillover Polling Station	-0.065*		-0.005		0.014	
	(0.037)		(0.052)		(0.015)	
Saturation		-0.184***		-0.171*		0.005
		(0.064)		(0.096)		(0.031)
	(b) Controlling for a 4th order polynomial in ACFIM presence					
Treatment Polling Station	-0.065*		-0.071		-0.006	
	(0.033)		(0.048)		(0.015)	
Spillover Polling Station	-0.065*		-0.003		0.015	
	(0.037)		(0.051)		(0.015)	
Saturation		-0.181***		-0.175*		0.007
		(0.064)		(0.095)		(0.031)
	(c) Controlling for deciles of ACFIM presence					
Treatment Polling Station	-0.061*		-0.072		-0.005	
	(0.033)		(0.048)		(0.015)	
Spillover Polling Station	-0.062*		-0.004		0.015	
	(0.038)		(0.051)		(0.014)	
Saturation		-0.178***		-0.187**		0.012
		(0.065)		(0.095)		(0.030)
	(d) Controlling for strata fixed effects					
Treatment Polling Station	-0.053*		-0.056		-0.004	
	(0.031)		(0.044)		(0.012)	
Spillover Polling Station	-0.052		0.020		0.017	
	(0.035)		(0.047)		(0.013)	
Saturation		-0.163***		-0.156*		0.003
		(0.057)		(0.088)		(0.026)
	(e) Removing controls					
Treatment Polling Station	-0.091**		-0.111		-0.006	
	(0.039)		(0.069)		(0.019)	
Spillover Polling Station	-0.086**		-0.003		0.015	
	(0.042)		(0.082)		(0.023)	
Saturation		-0.242***		-0.270**		-0.001
		(0.074)		(0.132)		(0.041)
R^2	0.09	0.09	0.49	0.49	0.42	0.42
Control Mean	0.00	0.00	0.00	0.00	0.62	0.62
Observations	27065	27065	3657	3657	3140	3140

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish. In panel (a), the estimation details as described in the footnote to Table 5. We additionally control for a 4th order polynomial in baseline ACFIM presence (panel b), deciles of ACFIM presence (c), or strata fixed effects (d). In panel (e), we remove all controls other than $ACFIM_{vp}$ and $ACFIM\_Presence_p$ in equations (1) and (2).

D Additional Results on Saturation Effects

In addition to the results presented in the paper, our randomization design allows us to estimate how treatment and spillover effects vary with parish-level saturation. We run the following specification:

$$\begin{aligned} Y_{ivp} \quad = \quad & \beta_0 + \beta_1 Treatment_{vp} + \beta_2 Spillover_{vp} + \\ & \beta_3 Treatment_{vp} \times Saturation_p + \beta_4 Spillover_{vp} \times Saturation_p + \\ & \beta_5 ACFIM_{vp} + \beta_6 ACFIM\ Presence_p + \beta_7 ACFIM_{vp} \times ACFIM\ Presence_p + \Omega X_{ivp} + \varepsilon_{ivp} \end{aligned} \tag{D1}$$

Here, the two main coefficients of interest here are β_3 and β_4, indicating how the treatment and spillover effects, respectively, change with treatment saturation at the parish level. This equation includes a deviation from pre-specified equation (2) in our pre-analysis plan, which had two additional right-hand side terms ($ACFIM\ Presence_p \times Treatment_{vp}$ and $ACFIM\ Presence_p \times Spillover_{vp}$) but did not include the $ACFIM\ Presence_p \times \beta_6 ACFIM_{vp}$ interaction. The results obtained from both specifications are similar, but equation (D1) above is the correct specification since the previously included terms captured some of the relevant (exogenous) variation and thus should not be included as controls. Note there is no main effect of $Saturation_p$ in this specification since all control parishes have zero saturation by design. β_1 recovers the Treatment on the Uniquely Treated or TUT, which is the intent-to-treat effect of the campaign on a theoretical sole individual offered treatment within a treated parish (Baird et al., 2018). In other words, β_1 measures the direct effect of the campaign measured at the theoretical point of zero saturation. β_1 and β_3 together account for the total treatment effect of the campaign: β_1 captures the TUT while β_3 captures spillovers on the treated.[24]

Appendix Tables D1 through D4 report the estimates from equation (D1) for our main outcomes of interest examined in Tables 2 to 5 in the paper. Three sets of results stand out. First, the interaction of treatment with saturation is positive and significant in column 1 of Table D1, suggesting that shifts in attitudes induced by the ACFIM intervention were more likely to spread throughout the parish when saturation increased. Second, the same interaction is positive in column 4 of Table D3: challengers were more likely to intensify their campaigning efforts at higher levels of parish-level saturation. Third, the intervention also induced a larger short-term electoral losses for incumbents at higher levels of parish saturation (Table D4, columns 1–2), likely as a result of the previous two forces.

[24] β_2 in this specification does not have a meaningful interpretation. Since the corresponding structural parameter is zero by definition, as Baird et al. (2018) explain, a test of $H_0 : \hat{\beta}_2 = 0$ provides a test of the assumption that spillovers are linear.

Table D1: Linear saturation model for Table 2

	Negative consequences	Village resolution	No vote-buying	Eat widely, vote wisely
	(1)	(2)	(3)	(4)
Treatment village	-0.007	0.093***	0.028***	0.065***
	(0.013)	(0.011)	(0.006)	(0.008)
Spillover	0.008	0.001	0.003	-0.002
	(0.013)	(0.007)	(0.003)	(0.006)
Treatment*Saturation	0.042*	-0.024	-0.004	-0.021
	(0.022)	(0.019)	(0.011)	(0.015)
Spillover*Saturation	0.008	0.001	-0.002	0.003
	(0.033)	(0.019)	(0.008)	(0.016)
R^2	0.04	0.03	0.01	0.02
Control Mean	0.89	0.03	0.01	0.03
Observations	28454	28454	28454	28454

Note: This table reports estimates from equation (D1) for the outcomes considered in Table 2.
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in parentheses.

Table D2: Linear saturation model for Table 3

	Social sanctions	Breaking reciprocity		Incumbent valence
		Subjective	Objective	
	(1)	(2)	(3)	(4)
Treatment village	0.044***	0.006	0.015	-0.029
	(0.017)	(0.010)	(0.014)	(0.039)
Spillover	-0.005	-0.011	-0.000	-0.049
	(0.018)	(0.010)	(0.015)	(0.042)
Treatment*Saturation	-0.039	0.006	-0.004	-0.036
	(0.029)	(0.017)	(0.027)	(0.080)
Spillover*Saturation	0.036	0.032	0.030	-0.040
	(0.044)	(0.025)	(0.037)	(0.104)
R^2	0.04	0.01	0.06	0.16
Control Mean	0.78	0.04	0.12	0.35
Observations	28454	28454	28454	28454

Note: This table reports estimates from equation (D1) for the outcomes considered in Table 3.
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in parentheses.

Table D3: Linear saturation model for Table 4

	Vote buying			Campaigning	
	(1) All	(2) Incumbents	(3) Challengers	(4) Incumbents	(5) Challengers
Treatment village	0.050	0.023	0.061	-0.061	-0.137*
	(0.046)	(0.047)	(0.047)	(0.068)	(0.071)
Spillover	0.005	-0.042	0.064	0.006	-0.107
	(0.053)	(0.052)	(0.059)	(0.073)	(0.078)
Treatment*Saturation	-0.019	-0.029	0.002	0.238*	0.389***
	(0.084)	(0.087)	(0.091)	(0.140)	(0.146)
Spillover*Saturation	0.036	0.093	-0.053	-0.050	0.263
	(0.131)	(0.128)	(0.143)	(0.174)	(0.186)
R^2	0.06	0.05	0.04	0.14	0.11
Control Mean	0.00	0.00	0.00	0.00	0.01
Observations	28454	28454	28454	28454	28454

Note: This table reports estimates from equation (D1) for the outcomes considered in Table 4.
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in parentheses.

Table D4: Linear saturation model for Table 5

	Survey data	Electoral Data	
		2016	2021
	(1)	(2)	(3)
Treatment Polling Station	0.059	0.120	-0.002
	(0.056)	(0.081)	(0.025)
Spillover Polling Station	0.018	0.028	0.010
	(0.064)	(0.093)	(0.025)
Treatment*Saturation	-0.255**	-0.418**	-0.011
	(0.112)	(0.171)	(0.059)
Spillover*Saturation	-0.297*	-0.131	0.013
	(0.164)	(0.226)	(0.061)
R^2	0.09	0.49	0.42
Control Mean	0.00	0.00	0.62
Observations	27065	3657	3140

Note: This table reports estimates from equation (D1) for the outcomes considered in Table 5. * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in parentheses.

E Results from Pre-Specified Hypotheses

We report treatment effects on the main hypotheses of our pre-analysis plan in Appendix Tables E1 through Table E5. All tables report estimates from four specifications: equation (1) in column 1, equation (2) in column 2, a modified version of equation (D1) in column 3 that includes interactions between ACFIM presence and the Treatment and Spillover dummies, and equation (D1) in column 4. The specification used in column 3 is the original version of equation (D1) that we included in our pre-analysis plan, but the correct specification should not include these interactions since they capture some of the treatment effects of interest.

Our primary hypotheses stated that *vote buying should fall in treatment villages* (Hypothesis 1) *and rise in spillover villages* (Hypothesis 2). We expected the intensity of these effects to be *increasing in parish saturation levels* (Hypotheses 7 and 8). To test for these hypotheses, the outcome in Table E1 is a preregistered index of self-reported vote buying, knowledge of particular individuals who sold their vote, and perceptions of the frequency of vote buying in the village from the voter survey and the key informant survey. Even if the main coefficients of interest in column 1 have the anticipated sign, we find little evidence in support of these hypotheses: the main effects of treatment and spillover are statistically insignificant and small in magnitude (column 1, Hypotheses 1 and 2). There is also little evidence that treatment effects vary with saturation levels (column 4, Hypotheses 7 and 8).

In Table E2, we show treatment effects for our Hypothesis 3A: *the supply of votes* (i.e., the perceived willingness to sell one's vote) *should fall in treatment villages*. The dependent variable for this hypothesis is an index of the perceived fraction of village residents who would sell their vote at given price points (ranging from 1,000 to 50,000 UGX) and of the perceived acceptability of selling one's vote in the vignette experiment. We find that the (perceived) supply of votes fell in treatment villages (see column 1) and in highly saturated parishes (column 2).

Table E3 shows results for our Hypothesis 3B: *demand for votes may rise or fall in treatment villages.* The dependent variable is an index of total offers received from brokers (accepted or rejected) and of the perceived fraction of village residents who were given a vote-buying offer. Overall, we do not find significant treatment effects supporting this hypothesis, though both the coefficient on saturation (column 2) and the coefficient on the interaction of treatment with saturation (column 4) are positive. This table also provides a test of Hypothesis 5 (demand for votes increases in spillover villages): we find a positive, but statistically insignificant effect on the spillover variable (column 1) and on the interaction between spillover and saturation (column 4).

Finally, Hypotheses 4 and 6 focused on the price of votes, which we argued *may increase or decrease in treatment villages depending on the magnitude of demand and supply shocks* (Hypothesis 4), *and increase in spillover villages* (Hypothesis 6). Tables E4 and E5 present results from these tests, using the two different outcomes we pre-specified: the total amount of cash and goods received by the voter from all brokers in Table E4, and an index of typical amounts offered by candidates from the key informant survey in Table E5. The results from these tables are inconclusive: treatment and spillover effects are positive and statistically insignificant in Table E4, and negative and statistically insignificant in Table E5.

Table E1: ACFIM Campaign Effects, Primary Hypotheses (Overall Vote Buying)

	Index for Hypotheses 1 & 2			
	(1)	(2)	(3)	(4)
Treatment village	-0.030		-0.102	-0.085
	(0.038)		(0.083)	(0.068)
Spillover	0.023		0.040	0.079
	(0.046)		(0.086)	(0.080)
Treatment Saturation		-0.017		
		(0.067)		
Treatment*Saturation			-0.046	0.118
			(0.169)	(0.121)
Spillover*Saturation			0.019	-0.180
			(0.240)	(0.204)
Outside Sampling Frame	-0.060	-0.026	-0.077	-0.145*
	(0.043)	(0.030)	(0.052)	(0.087)
ACFIM Presence	-0.132**	-0.119	-0.180	0.020
	(0.064)	(0.076)	(0.114)	(0.185)
ACFIM Presence*Treatment			0.174	
			(0.199)	
ACFIM Presence*Spillover			-0.041	
			(0.222)	
ACFIM Village*ACFIM Presence				-0.211
				(0.191)
R^2	0.16	0.16	0.16	0.16
Control Mean	0.037	0.037	0.037	0.037
Controls	Yes	Yes	Yes	Yes
Observations	28454	28454	28454	28454

Note: This table reports experimental results for Hypotheses 1 & 2 in our pre-analysis plan: the campaign reduces the equilibrium number of votes sold in treatment villages (H1), and weakly increases the equilibrium number of votes sold in spillover villages (H2). The dependent variable is our pre-specified index of vote buying.
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in parentheses.

Table E2: ACFIM Campaign Effects, Hypothesis 3A (Supply of Votes)

	Index for Hypothesis 3A			
	(1)	(2)	(3)	(4)
Treatment village	-0.059**		-0.028	-0.043
	(0.025)		(0.055)	(0.046)
Spillover	-0.026		0.076	-0.014
	(0.029)		(0.052)	(0.047)
Treatment Saturation		-0.096**		
		(0.043)		
Treatment*Saturation			-0.021	-0.033
			(0.123)	(0.082)
Spillover*Saturation			0.102	-0.043
			(0.141)	(0.115)
Outside Sampling Frame	-0.010	0.010	-0.032	0.036
	(0.029)	(0.022)	(0.033)	(0.055)
ACFIM Presence	0.041	0.094*	0.125*	-0.016
	(0.040)	(0.049)	(0.071)	(0.109)
ACFIM Presence*Treatment			-0.054	
			(0.141)	
ACFIM Presence*Spillover			-0.278**	
			(0.137)	
ACFIM Village*ACFIM Presence				0.102
				(0.114)
R^2	0.03	0.03	0.03	0.03
Control Mean	0.046	0.046	0.046	0.046
Controls	Yes	Yes	Yes	Yes
Observations	28454	28454	28454	28454

Note: This table reports experimental results for Hypothesis 3A in our pre-analysis plan: the campaign reduces the supply of votes in treatment villages. The dependent variable is an index of the perceived fraction of village residents who would sell their vote at given price points and of the perceived acceptability of selling one's vote in the vignette experiment.
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in parentheses.

Table E3: ACFIM Campaign Effects, Hypotheses 3B & 5 (Demand for Votes)

	Index for Hypothesis 3B			
	(1)	(2)	(3)	(4)
Treatment village	-0.002		-0.062	-0.073
	(0.033)		(0.069)	(0.054)
Spillover	0.018		0.023	0.015
	(0.038)		(0.068)	(0.071)
Treatment Saturation		0.038		
		(0.061)		
Treatment*Saturation			0.176	0.151
			(0.127)	(0.101)
Spillover*Saturation			0.050	0.019
			(0.208)	(0.176)
Outside Sampling Frame	-0.046	-0.035*	-0.061*	-0.064
	(0.033)	(0.020)	(0.036)	(0.070)
ACFIM Presence	-0.080	-0.100	-0.123	-0.125
	(0.057)	(0.065)	(0.097)	(0.145)
ACFIM Presence*Treatment			-0.046	
			(0.154)	
ACFIM Presence*Spillover			-0.038	
			(0.189)	
ACFIM Village*ACFIM Presence				-0.015
				(0.146)
R^2	0.19	0.19	0.19	0.19
Control Mean	0.025	0.025	0.025	0.025
Controls	Yes	Yes	Yes	Yes
Observations	28353	28353	28353	28353

Note: This table reports experimental results for Hypothesis 3B in our pre-analysis plan: the campaign affects the demand for votes in treatment villages. The dependent variable is an index capturing offers made by brokers of votes (accepted and rejected).
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in parentheses.

Table E4: ACFIM Campaign Effects, Hypotheses 4 & 6 (Price of Votes)

	Index for Hypothesis 4.1			
	(1)	(2)	(3)	(4)
Treatment village	0.012		0.021	0.026
	(0.020)		(0.044)	(0.037)
Spillover	0.029		0.023	0.024
	(0.023)		(0.042)	(0.041)
Treatment Saturation		0.017		
		(0.033)		
Treatment*Saturation			-0.040	-0.029
			(0.094)	(0.064)
Spillover*Saturation			0.006	0.016
			(0.112)	(0.101)
Outside Sampling Frame	-0.018	-0.004	-0.017	-0.013
	(0.021)	(0.014)	(0.024)	(0.044)
ACFIM Presence	-0.052	-0.059	-0.049	-0.051
	(0.037)	(0.042)	(0.058)	(0.097)
ACFIM Presence*Treatment			0.020	
			(0.107)	
ACFIM Presence*Spillover			0.009	
			(0.108)	
ACFIM Village*ACFIM Presence				0.009
				(0.098)
R^2	0.09	0.09	0.09	0.09
Control Mean	0.003	0.003	0.003	0.003
Controls	Yes	Yes	Yes	Yes
Observations	28454	28454	28454	28454

Note: This table reports experimental results for Hypothesis 4 in our pre-analysis plan: the campaign increases or decreases the price of votes in treatment villages, depending on the relative magnitude of supply and demand shocks. The dependent variable is the sum of all gifts received by the respondent in cash or in kind, by all candidates.
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in parentheses.

Table E5: ACFIM Campaign Effects, Hypotheses 4 & 6 (Price of Votes)

	Index for Hypothesis 4.2			
	(1)	(2)	(3)	(4)
Treatment village	0.002		-0.003	-0.009
	(0.026)		(0.059)	(0.047)
Spillover	-0.041		-0.028	-0.040
	(0.033)		(0.070)	(0.058)
Treatment Saturation		-0.007		
		(0.042)		
Treatment*Saturation			0.130	0.023
			(0.125)	(0.081)
Spillover*Saturation			-0.178	-0.005
			(0.138)	(0.135)
Outside Sampling Frame	0.044	0.013	0.061	0.100
	(0.033)	(0.024)	(0.041)	(0.069)
ACFIM Presence	-0.096**	-0.095*	-0.088	-0.209
	(0.044)	(0.052)	(0.075)	(0.141)
ACFIM Presence*Treatment			-0.101	
			(0.147)	
ACFIM Presence*Spillover			0.081	
			(0.144)	
ACFIM Village*ACFIM Presence				0.140
				(0.148)
R^2	0.07	0.07	0.07	0.07
Control Mean	-0.003	-0.003	-0.003	-0.003
Controls	Yes	Yes	Yes	Yes
Observations	28440	28440	28440	28440

Note: This table reports experimental results for Hypothesis 4 in our pre-analysis plan: the campaign increases or decreases the price of votes in treatment villages, depending on the relative magnitude of supply and demand shocks. The dependent variable is an index of typical gift amounts offered by different candidates in the village.
* p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in parentheses.