A Market Equilibrium Approach to Reduce the Incidence of Vote-Buying: Evidence from Uganda*

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Abstract

We estimate the effects of a large-scale, randomized grassroots campaign designed to combat vote-buying in the 2016 election in Uganda. Our design and data collection allow us to estimate how candidates and their brokers respond to the campaign in treatment and spillover areas and how the effects of the campaign vary with local treatment intensity. Contrary to our expectations, the campaign did not reduce the extent to which voters accepted cash and gifts in exchange for their vote. However, it led opposition candidates to increase their vote-buying and policy-campaigning efforts, and it had sizeable effects on electoral outcomes, with opposition candidates benefiting from the campaign at the expense of incumbent candidates. Consistent with these effects, we present evidence that the campaign diminished the effectiveness of vote-buying transactions by shifting local social norms against vote-selling and by convincing some voters to vote in their conscience, regardless of any gifts received.

Keywords: Elections, Voting Behavior, Field Experiment, Africa

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1 Introduction

Democracy in many countries is undermined by widespread vote-buying – the provision of gifts, in cash or in kind, in exchange for votes. Across political regimes, candidates use many tactics to buy votes (Gans-Morse et al., 2014), from giving likely supporters an incentive to turn out (Nichter, 2008), to targeting the people who are most likely to reciprocate the gift with a vote (Finan and Schechter, 2012). Political intermediaries known as brokers target resources to voters and mobilize them around elections, while extracting significant resources for themselves (Camp and Szwarcberg, 2015; Larreguy, 2013; Larreguy et al., 2016; Stokes et al., 2013). Endemic vote-buying practices impede political and economic development by limiting the ability of citizens to hold elected officials accountable (Stokes, 2005), the emergence of credible political platforms (Keefer and Vlaicu, 2008), and public goods provision (Hicken and Simmons, 2008).

Policy experiments designed to combat vote-buying have found that legalistic appeals to resist vote-selling (Vicente, 2014) and behavioral interventions (Hicken et al., 2014) convince some voters to renounce selling their vote, and hurt the electoral performance of vote-buying candidates (Green and Vasudevan, 2016). But do these interventions reduce vote-buying, or merely displace it? For instance, candidates and their brokers could simply buy their votes elsewhere. Voters could also react by choosing to sell their votes to a different candidate, or to accept gifts from multiples candidates but still vote for their preferred candidate. Because the response to campaigns of this kind is complex and potentially involves spillovers, it is important to track the response of voters (the supply side) in both treated and untreated areas, as well as that of vote-buying parties and candidates (the demand side), to interventions designed to combat vote-buying.

This paper investigates these concerns through a large-scale policy experiment in Uganda. President Yoweri Museveni and his National Resistance Movement, or NRM, have held power since 1986 in a system that most analysts classify as a "hegemonic party system," not unlike that of the PRI in late 20th century Mexico (Tripp, 2010; Magaloni et al., 2013). Ahead of Uganda's 2016 general elections, we partnered with a Ugandan Civil Society Organization (CSO), the Alliance for Election Campaign Finance Monitoring (ACFIM), and an international NGO, the National Democratic Institute (NDI), to evaluate the causal effects of what is (to our knowledge) one the largest anti-vote-buying campaigns ever implemented. Historically, vote-buying has been endemic in Uganda (Conroy-Krutz, 2012; ACFIM, 2015). Starting with an experimental sample composed of 918 parishes (or collections of villages) where ACFIM was active, we randomized roughly two thirds to treatment and one third to pure control. Beyond being a local administrative unit, fieldwork prior to the intervention suggests that the parish is also the level at which local political brokers are known to operate. Within treated parishes we randomized the fraction of villages targeted by the campaign, using a *randomized saturation design* where the

level of local treatment intensity is itself randomly determined.[1] We denote villages in treated parishes as treated and spillover villages depending on whether they were assigned to receive the campaign or not, respectively. We denote all villages in control parishes as control villages.

ACFIM's campaign was conducted prior to the 2016 election and included five main elements: (i) a leaflet drop; (ii) three village meetings, organized by local ACFIM activists, to build awareness of and opposition to vote-buying; (iii) the organization of a public village-wide resolution against vote-buying; (iv) the posting of posters reminding voters that selling their vote would harm the community; and (v) an automated-call reminder on the eve of the election. ACFIM activists spread across the country ran their intervention in 1,427 villages. The villages in our experimental sample cover around 1.2 million people registered to vote in the 2016 Ugandan general election across 6% of the country's polling stations, and 12% of polling stations in the districts we study – unparalleled numbers for this type of intervention.

ACFIM aimed to shift local social norms against vote-selling – people's perceptions about how others will behave, what kinds of behavior are considered appropriate, and the social sanctions for violating norms. The leaflet and the first community meeting attempted to create common knowledge about the costs and inappropriateness of vote-buying in terms of future service delivery and politician corruption. The second and third meetings were designed to convince a critical mass of the community to take a coordinated action against vote-selling – the public, village-wide resolution. Posters and automated calls were intended to reinforce the new norm. All these actions sent a public signal about the new norm, including towards candidates and their brokers.

To assess impacts, we use a combination of administrative data, original survey data, and systematically-collected qualitative accounts from the implementation of the intervention. Shortly after the 2016 elections, we surveyed 28,454 villagers, collecting data on people's experience with vote-buying, as well as data on the local prices of goods commonly used for vote-buying purposes in Uganda. We surveyed all treatment and control villages, as well as 1,399 out-of-sample villages in order to increase our power to estimate spillovers of the ACFIM campaign and thus our ability to capture the reactions of voter and candidates or their brokers to the campaign. In addition, we obtained administrative data on electoral results at the polling station level for the two most important ballots conducted in February 2016 (President and Members of Parliament (MP)).

Contrary to our expectations, as well as those of ACFIM and NDI, we see no evidence that the ACFIM campaign significantly reduced the extent to which voters were offered (and accepted) cash or gifts in exchange for their vote. We preregistered our central hypotheses: that cash and gift giving would fall in treated villages but likely increase in "spillover villages" – untreated villages in the same parish. However, survey respondents do not report a lower

[1] This design follows along the lines of Baird et al. (2014). See Section 3.3 for additional details.

prevalence of vote-buying after the ACFIM campaign. For instance, an index of vote-buying offers received in cash and in kind increased by just 0.03 standard deviations in treated villages (not statistically significant). The fraction of survey respondents who reported receiving cash on behalf of any of the Presidential or MP candidates, which is 43% in control villages, increased by 2 percentage points (not statistically significant).

This null average result, however, masks considerable heterogeneity in the response of candidates and political machines to the ACFIM campaign. We see evidence that opposition candidates, but not incumbent candidates, increased their attempts to buy votes on average as a result of the campaign. For example, only 10% of people in control villages reported that representatives of challengers of either the Presidential or Parliamentary races offered them cash for their vote, and this rose, respectively, by 1.8 and 2.3 percentage points in treated and spillover villages. Incumbent politicians, meanwhile, marginally increased their attempts to buy votes only in spillover areas within parishes randomly assigned to a high level of ACFIM presence. Challenger candidates seem to have also increased their campaigning efforts in heavily treated parishes, though this result often falls short of statistical significance and must be interpreted with caution.

We also see some evidence that instead of refusing offers of cash, voters took the money offered by politicians but nonetheless voted for their preferred candidate. On average, the ACFIM campaign reduced the incumbent's vote share in both the Presidential and Parliamentary elections, though this decrease often falls short of statistical significance. In addition, incumbents received significantly less support in parishes where all villages received the campaign. For example, in these intensively treated parishes, incumbent MPs received a lower vote share by approximately 0.2 standard deviations (SDs). Villages in spillover villages within treated parishes experienced a similar decline in incumbent vote shares, reinforcing the effects of the ACFIM campaign on parish-level vote shares.

Consistent with these effects, we see evidence of a change in attitudes and perceived social norms around vote buying, especially in the most intensively treated parishes. For instance, 75% of respondents in control villages thought that other villagers would be angered over vote selling, and 57% said the consequence would be ostracization. These perceived social sanctions rose about 2 percentage points on average with treatment, with slightly larger shifts occurring in the most intensely treated parishes. Treatment also appears to have changed attitudes: respondents in treated villages were slightly more likely to say that vote buying has ill consequences for the village and is unacceptable, again with the largest effects found in intensely treated parishes.

While we anticipated impacts on incumbent vote shares, we did not anticipate the direction, magnitude, or importance of these effects (we preregistered the outcome as secondary, with the potential for the opposite effect). Thus we must take these vote share results with some caution. Nonetheless, the treatment effects and our qualitative data tell a plausible, coherent

story, where candidates and their brokers responded strategically to ACFIM's campaign. When it reached enough villages in the parish, the ACFIM campaign weakened the enforceability and effectiveness of vote-buying by incumbents in particular, who rely more than challengers on vote-buying, and whom voters associated with poor service delivery and corruption in order to recover the money spent on vote-buying during their campaigns – a complaint voters emphasized during the ACFIM meetings. Moreover, challengers took advantage of the campaign by engaging in greater vote-buying and policy campaigning.

Our findings suggest some promising avenues for policy. To begin, anti-vote buying campaigns may do well to encourage voters to take the funds and vote their conscience, rather than refusing gifts. It is unclear, however, how parties will respond in the longer run. The incentives to buy votes (and the resulting corruption and crowding out of public services) may persist. Alternatively parties may find it optimal to shift away from vote buying to other tactics, if vote buying becomes sufficiently ineffective. Understanding the longer term effects of anti-vote buying campaigns is an important area of future research. Another implication of our study is that campaigning may need to be intense enough to be effective, which might mean either increased resources to anti-vote buying, or geographical targeting of these resources. More broadly, these results suggest that more experimentation is needed to identify cost-effective strategies to counter vote-buying.

This paper builds on two previous randomized evaluations of programs designed to combat vote-buying practices. Vicente (2014) finds that a voter education campaign in São Tomé and Príncipe reduced the influence of money offered on voting, decreased voter turnout, and favored the incumbent, in a context where (relative to Uganda) challengers rely more on vote-buying practices. Hicken et al. (2014)'s experiment tackles vote-selling as a time-inconsistency problem, using ex-ante promises to reject vote-buying offers, or to accept them but instead vote their preferred candidate, in the Philippines. Our experiment differs in its scale and visibility, so as to generate incentives for candidates or their brokers to react and voters to coordinate against vote buying.[2] Another experiment recently implemented in India shows that a radio campaign designed to reduce vote-buying decreases the vote share of candidates known to buy votes (Green and Vasudevan, 2016).

We also build on recent experimental work that studies whether campaigning on public goods provision (as opposed to using standard clientelistic strategies) reduces vote-buying. In Benin, Wantchekon (2003) randomly assigned clientelistic messages and broad-based messages (regarding nationwide issues) endorsed by candidates, and found that clientelism was more effective in generating electoral support. Contrary to this finding, Fujiwara and Wantchekon (2013) showed also in Benin that town hall meetings addressing specific policy platforms of

[2] Vicente (2014) treats only 40 enumeration areas (out of 50 that composed the experimental sample). Hicken et al. (2014) treat 600 voters (out of 900 that composed the experimental sample) privately.

broad-based public goods provision (as opposed to traditional vote-buying strategies) reduced self-reported measures of vote-buying, and lowered the vote shares for the candidate if the village represented a political stronghold.

Outside the experimental literature, various papers have studied how vote-buying works in practice.[3] Of particular relevance to this paper is the literature that highlights the role of brokers for the success of vote-buying (Camp and Szwarcberg, 2015; Larreguy, 2013; Larreguy et al., 2016; Stokes et al., 2013). Our intervention builds heavily on this recent work, both in terms of design and analysis. Accounts from interviews with candidates, brokers and voters from our focus groups suggest that vote-buying in Uganda is facilitated by pyramidal structures of brokers that mediate between candidates and voters. We conducted our treatment saturation at the lowest level at which these brokers are organized, monitored and incentivized, namely at the level of parish.

Finally, our experiment addresses vote-buying as a market equilibrium problem. In doing so, it also builds on a new strand of empirical work designed to uncover spillover and general equilibrium effects in experimental settings. A comprehensive review of this literature is beyond the scope of this paper, but a good review can be found in Baird et al. (2014), who provided the conceptual framework for the design used in our experiment. Looking at voter education campaigns more specifically, Fafchamps et al. (2012) find that a voter education in Mozambique has 2 types of spillover effects: the treatment effect is reinforced when targeted individuals are surrounded with other targeted individuals; and non-targeted individuals are also affected when living in close proximity with targeted individuals. Ichino and Schündeln (2012) study the displacement of fraud due to the deployment of observers in the 2008 election in Ghana.

The rest of the paper is organized as follows. We provide relevant background on the 2016 Ugandan general election and vote-buying practices in Section 2. Section 3 describes our experimental design, and Section 4 our data. We present our empirical framework in Section 5. Section 6 presents our main results. Section 7 discusses potential mechanisms and section 8 concludes.

2 Background

2.1 The 2016 Ugandan general election

Uganda holds general elections in February, every five years. The President is elected in a two-round system, requiring at least 50% of the popular vote to be elected in the first round. Mem-

[3] For example, Dekel et al. (2008) provide a model of vote-buying in which vote prices remain low in equilibrium because only the winning party buys votes. Gans-Morse et al. (2014) study how political machines mix across 4 types of clientelist strategies: vote-buying, turnout buying, abstention buying, and double persuasion. Finan and Schechter (2012) show that politicians target their vote-buying offers towards reciprocity-minded individuals. Finan et al. (2016) further argue that brokers exploit their social networks to acquire information about partisanship and reciprocity, which they subsequently use to target voters.

bers of Parliament (MPs) are elected in single-member constituencies using first-past-the-post voting. In addition, voters also elect District Woman Representatives to sit in Parliament. 375 seats were contested during the February 2016 general elections.[4]

The 2016 general elections were held on February 18, in accordance with the electoral calendar. A total of 28,007 polling stations were set up for the election, 6% (1,603) of which were directly treated by the ACFIM campaign. Eight candidates contested the presidential election, among which two were considered frontrunners: the incumbent President, Yoweri Museveni, in office since 1986; and a long-time opposition leader running for the fourth time, Kizza Besigye.[5] Museveni's and Besigye's respective parties, the National Resistance Movement (NRM) and the Forum for Democratic Change (FDC), were also dominant in the campaigns for parliamentary seats and local positions, but these elections additionally involve a large number of independent candidates,[6] as well as candidates from several smaller parties. For the parliamentary election, a total of 1,749 candidates ran for MP positions across the country's 238 constituencies.

Though politics is fairly competitive at the local level, at the national level most analysts consider Uganda a "hegemonic party system" or a "multiparty autocracy" due to suppression of opposition parties and candidates, and the widespread use of patronage and vote-buying by incumbents (Tripp, 2010).[7] For example, several major incidents occurred throughout the 2016 electoral period. First, the leader of the opposition, K. Besigye, was arrested twice in the week leading to the election (Amnesty International, 2015), and subsequently kept under house arrest. Second, checkpoints were set up, and the presence of security forces massively increased throughout the country as the election unfolded (Amnesty International, 2016). Third, the government enforced a four-day social media blackout (The Guardian, 2016). Lastly, voting materials were delivered late to a number of polling stations where voters were expected to vote against Museveni. The alleged goal was to generate long lines in those polling stations in order to ultimately discourage voters from casting their vote (The Guardian, 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 164 out of 238 constituency MP seats (69%), and 86 out of 112 (77%) District Women's Representative

[4]This includes 238 constituency seats, 112 District Woman Representatives, and 25 indirect (reserved) seats. At the same time, voters also elect local leaders. The country is divided into 111 districts, which are themselves divided into counties, subcounties, parishes, and villages. Voters elect a District (or "LC5") Chairman and Councilors, as well as a Subcounty ("LC3") Chairman and Councilors. Village leaders (or "LC1s") are elected through informal processes at the village level, and so are not included in our analysis, and there are no elected positions at the county or parish level (these are governed by LC3 and LC5 councils).

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

[6] Often, these are individuals who lost in the primaries to represent their favored party.

[7] The Ugandan political regime was classified by the Freedom House as "not free" in 2016 (with a score of 36%), and as a "closed anocracy" in 2015 by the Polity IV project (with a score of -1).

positions. Ugandan and international observation missions provided mixed opinions about the fairness and transparency of the election.[8] For example, the EU Observation Mission cited the lack of independence of the Electoral Commission, the excessive use of force against the opposition, the "intimidating atmosphere for both voters and candidates", and "the orchestrated use of state resources and personnel for campaign purposes" as major obstacles against a free and fair election (European Union Election Observation Mission, 2016).

2.2 Vote-Buying in Uganda

Uganda has some of the highest rates of vote-buying in the world. Out of 18 countries with Afrobarometer data, Uganda in 2006 had the second highest reported rate of vote-buying of any country in the sample (after Kenya), with 85% of respondents reporting that politicians "often" or "always" give gifts during political campaigns (Afrobarometer, 2006).[9] The culture of vote-buying in the country has been called "ubiquitous" (Democracy Monitoring Group, 2011), and previous studies have described sizeable payment amounts – one such study reported that the median vote price in 2011 was 5 times the daily average income (Conroy-Krutz, 2012).

Despite the magnitude of vote-buying in Uganda, little is known about how it is undertaken in practice. To fill this gap and to explore possible intervention designs, we conducted (prior to the launch of the ACFIM campaign) and through our partners (NDI and ACFIM) focus groups in 48 locations spread throughout our eventual experimental sampling frame. In addition, we interviewed several elected candidates and active brokers to gather information about their vote-buying operations, and how candidates fund these operations.

The focus groups highlighted the large extent of the vote-buying phenomenon and its importance in enabling candidates to win elections. While focus group participants agreed that some voters may choose to "eat widely but vote wisely," i.e., to take money for their vote but then vote for their preferred candidate, they also highlighted that a large share of voters reciprocate gifts with their actual vote since money "softens people's hearts." Participants also noted that vote-buying addresses short-term needs which are especially salient around elections, when inflation is high.[10]

All participants, candidates and brokers emphasized the importance of brokers for the success of vote buying. An NDI survey of 185 elected MPs after the intervention reflects that all respondents had brokers in the 2016 election – 96% in all villages and 4% only in selected vil-

[8] We discuss allegations of vote fraud in Appendix 2.

[9] The average across all 18 countries in the sample 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%).

[10] Participants also argued they often have very poor information to discern among the best candidates. In addition, elected officials reportedly argue they are not responsible for improving public service delivery. This discourse is often left undisputed since voters are also uninformed about whom they should hold accountable for service delivery.

lages. Brokers are not only responsible for handing over cash or gifts to voters – typically soap, sugar and other more idiosyncratic goods, mostly in the week preceding the election – but they also make sure people who received such gifts turn out on election day. The brokers' ability to mobilize voters is so important for the success of vote-buying that candidates admitted to us that they decide how much to invest in buying votes depending on such ability. To maximize the returns from vote-buying, candidates use sophisticated pyramidal structures, with chiefs at the constituency level, coordinators at the subcounty level (often LC3 chairpersons), and managers at the parish level (often LC3 councilor or LC1 chairpersons) who are the ones ultimately responsible for recruiting and managing village-level brokers.[11]

Brokers have both immediate and long-term financial incentives to deliver voters for the candidates they work for. They are first endowed with a budget to carry out voter mobilization in the village – a fraction of this budget, which comes in the form of cash or gifts, is often retained by the brokers themselves.[12] Importantly, brokers typically receive a bonus for their work based on an evaluation of their performance,[13] and they are able to build a connection with an elected official, as well as to receive the benefits that such a connection entails.

ACFIM conducted a separate survey of Ugandan MPs that indicated that a large majority of costs are borne by individual candidates, not parties. Candidates fund their campaigns using personal resources (savings, property) or sometimes take out loans explicitly for the purpose of campaigning. As a result, we anticipated that parties would be unlikely to respond to the campaign strategically by moving resources across candidates.

3 Experimental Design

3.1 Description of the intervention

ACFIM (along with its 13 local partner organizations) implemented the anti-vote buying campaign in January-February 2016 across 53 Ugandan districts, or about half the country. The design of the campaign was influenced by ACFIM and NDI's past interventions, by a survey of Ugandan MPs (collecting qualitative information on campaign financing), and by the focus groups described above. The campaign sought to reduce the incidence of vote-buying by fos-

[11] Interestingly, some brokers work for candidates that run in different races but belong to different parties. Higher level candidates often also coordinate with lower level candidates that they trust irrespective of their party since these have a fewer resources but much more local presence among both voters and brokers. The NDI survey indicates that 48% of the surveyed MPs shared brokers with other candidates.

[12] While this claim cannot be verified, some focus groups participants argue that brokers keep between two thirds and four fifths of what candidates given them to distribute.

[13] Candidates look closely at the election returns and brokers have to provide a report about the candidate's performance in their area on election day. Agents who did not do well do not even bother to provide such a report.The NDI survey shows that 97% of the surveyed MPs followed their brokers' performance, out of which 66% did so by looking at polling-station results and 21% by requesting brokers to submit reports after the election.

tering a change in local norms as well as collective commitments in the community to not sell any votes. The general goal was to convince participants that selling their vote was not only inappropriate but also costly, since it would undermine the accountability of elected officials and future delivery of public goods to the community. With the adoption of a community-wide resolution on the issue, the campaign sought to improve coordination by fostering a collective commitment at the community level to renounce vote-selling.

The campaign took place during the apex of the electoral period, when most vote-buying transactions take place (i.e., the final weeks leading up to the election), and involved several stages in each selected village. First, all households in treated villages received a leaflet explaining in simple terms the costs and risks of vote-buying to their communities. Leaflet recipients were also invited to participate in subsequent community meetings to discuss the vote-buying issue. The leaflets were delivered via door-to-door canvassing conducted by local ACFIM activists in January 2016. The content of the leaflets was approved by the Electoral Commission and entirely non-partisan. The leaflets contained a cartoon alongside the following message (in the language spoken by the community):[14]

> "You wouldn't sell your future, you wouldn't sell your village's future. So, why sell your vote? Stand together with your village, and don't sell your vote. It is your chance to demand a better future!"

A sample leaflet in English can be found in Figure 1 and shows individuals first receiving money from a candidate for their votes (in the left plot), and then seeing their request for a health center denied on the ground that the candidate had already bought them off (in the right plot). These plots and the caption embody the main messages behind the ACFIM campaign, which were later reemphasized during the complementing components of the intervention. First, individuals who sell their votes are unlikely to later be able to demand public service delivery from the candidates they sold their votes to. Second, community coordination is crucial to fight vote-buying and the associated lack of public service delivery.

Following the leaflet drop, three meetings were organized to discuss vote-buying in the village. The meetings were again facilitated by a local ACFIM activist. The first meeting focused on introducing the campaign, discussing the leaflet and gathering participants' thoughts and experience on vote-buying. The second meeting was designed to provide an avenue for a collective deliberation on vote-buying. Finally, during the third meeting, ACFIM activists invited the community to collectively commit to refuse offers of gifts or money in exchange for votes. ACFIM activists then placed posters through the village indicating the village is a "no vote-buying village."

[14] 18 languages were used as part of the campaign: Acholi, Alur, Aringa, Ateso, Kumam, Langi, Lubwisi, Luganda, Lugbara, Lusoga, Madi-Moyo, Ngakarimojong, Rufumbira, Rukhozo, Rukiga, Runyankole, Runyoro, and Rutoro.

Finally, on the eve of election day, individuals that attended the village meetings and provided their phone number on the attendance sheet received automatized phone calls reminding them about the harm caused by vote buying. The calls included the following message (in the appropriate local language):

> "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 gifts 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!"

3.2 Experimental sample

Our experimental sample included 2,796 eligible villages across 1,603 polling stations within 53 Ugandan districts. The sample villages were spread across 110 parliamentary constituencies and 918 parishes. Eligibility to receive the intervention was tied to the presence of a local ACFIM activist (i.e. one that resided in a nearby location to the village that comprised the local polling station).[15] Throughout the paper, we use "eligible village" to indicate that it was potentially treated. A parish generally consists of 3-10 villages. Parishes with eligible villages can, and usually do, also have ineligible villages, some of which we also sampled for our survey in order to maximize statistical power when looking at spillovers identify the reaction of candidates or their brokers to the intervention, as well as voter coordination around it. Randomization was done among eligible villages so as to preserve internal validity of the design. We provide additional details on sampling and external validity on this procedure in Appendix 1.

3.3 Randomization

The intervention used a randomized saturation design, along the lines of Baird et al. (2014), varying the level of saturation of treatment at the level of a parish. Among the 2,796 eligible villages in 918 parishes, we randomly selected 1,427 villages across 535 parishes for treatment. The remaining 383 parishes were allocated a pure control group with no villages treated. An additional 1,399 villages located in the same 918 parishes were added to the endline survey sample to look for spillovers of the intervention oversampling villages in parishes with a higher treatment saturation.

[15] Due to cultural issues, it is very hard for an individual to conduct this type of intervention in villages where she is perceived as an "outsider". As ACFIM members explained it to us, activists had to be "sons of the soil" for villagers to listen to them.

Because the campaign could only take place in areas where ACFIM activists had a local presence at baseline, the randomized saturation level is defined in terms of eligible villages, where eligible means that the partner had local activists who could work in those villages (note that all our specifications control for the baseline level of partner presence, as described in our pre-analysis plan). The fraction of eligible polling stations in a parish ranged from 3% to 100%, with an average of 48%. Accounting for the variation in the number of voters registered in each station, the fraction of eligible voters ranged from 1% to 100%, with an average of 54%.

In the first step of the randomization, parishes were allocated to one of three cells: a pure control cell (no treatment), a partial-saturation treatment cell (50% of *eligible* villages assigned to treatment), and a high-saturation cell (100% of eligible villages assigned to treatment). To fix ideas, consider a parish with 8 equally sized villages, of which 4 have ACFIM activists. If assigned to 100% treatment, this would mean that all 4 of the *eligible* villages would be treated (equivalent to 50% "true" saturation). If assigned to partial (50%) treatment, then a randomly selected 2 of the 4 eligible villages would be treated (25% true saturation).

This randomization was stratified at the parish level along baseline measures of partner presence (defined in terms of the number of voters covered), parish-level voter population, and support for the incumbent political party in the 2011 presidential election. Specifically, a stratum was defined by the three-way interaction of quartile of partner presence, quartile of voter population, and quartile of district-level NRM support (63 strata in total).

In the second step, eligible villages were assigned to treatment within the partial-saturation parishes. Here, we randomized villages to treatment or control status at the polling station level. All eligible villages in treated polling stations were selected to receive the ACFIM campaign (up to a limit of 3 villages per activist). None of the villages falling under control polling stations were selected to receive the campaign. This creates an integer problem if all eligible villages fall under a single polling station. If only one polling station was eligible for treatment in a parish (i.e. a parish had only a local ACFIM activist), it was either fully treated (with 50% probability) or a full control (with 50% probability). No polling stations were split between treatment and control in order to maximize the usefulness of the official election outcomes.[16]

This design allows us to identify the spillover effect on the non-treated from (potentially) both the responses of those receiving the campaign (social norm coordination) and from changes in candidate or broker behavior, in addition to standard intent-to-treat estimates of direct treatment. Our design also allows us to recover precise estimates of how those estimates vary with

[16]To fix this concept clearly, we can return to our 8 village (4 with ACFIM presence) parish example from before. Imagine first that there are 4 polling stations, each with 2 villages. Then, if that parish was assigned to partial (50%) treatment, there would be no problem (1 eligible, treated polling station, 1 eligible, untreated polling station, and 2 ineligible, untreated polling stations). However, if there were only 2 polling stations (1 with all of the ACFIM villages, 1 with none), then this parish would either be assigned to have either its 1 eligible polling station treated (which is then equivalent to 100% treatment) or its 1 eligible polling station untreated (which is then equivalent to being in the control).

treatment intensity. The spillover effects could differ substantially with local treatment intensity – only if a large number of villagers resist vote-buying, political candidates or their brokers may be forced to change vote-buying tactics, as well as other campaign strategies.

4 Data

4.1 Administrative Data

4.1.1 Overview

We use official electoral results obtained from the Ugandan Electoral Commission at the lowest possible level, the polling station. We use this data for two of the three of the ballots conducted in February 2016 (President and MP) for 1,585 out of the 1,603 (99%) polling stations in our experimental sample.[17] We also use data on turnout and vote share of the corresponding incumbents from the previous general election conducted in 2011, available for 98% of polling stations in our sample. We discuss the integrity of the electoral data in Appendix 2.

4.2 ACFIM administrative notes

We use data collected by the ACFIM partners during the implementation of the three village meetings. Two activists of each ACFIM partner took part in every meeting: one had the role of facilitator and the other one of note taker. The note taker had to fill in basic information about the meeting, which included the start time, end time, and location of each meeting, rough estimates of the number of participants from the village and from outside the village, the presence of influential individuals likely to engage in or mediate vote-buying activities (LC 1, 2, 3, or 5 officials, MPs, candidates or brokers), a range of questions addressing whether the facilitators conducted the meetings as specified during training and in the meeting scripts, the views of the community about the effect of vote buying and possible solutions against it, and activists' perceptions of how likely communities were to vote on a resolution against vote buying and whether they effectively did.

4.3 Survey Data

We conducted an endline survey of 28,454 Ugandan voters in the aftermath of the ACFIM campaign and the general election. The survey started on March 2, 2016, and ended on July 19, 2016, after some of our survey teams encountered administrative delays due to the sensitivity of the information collected. The survey involved three different questionnaires: of registered voters,

[17] Due to discrepancies in local names and spellings, we were unable to match 1% of polling stations in our sample with the official electoral data.

a "key informant" in each village, and a local market survey of the prices of goods commonly used for vote-buying, as well as the prices of goods not subject to vote-buying practices.

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.[18] All respondents were over 18, registered to vote, and living in the village. We also conducted one key informant survey in each sampled village.

5 Empirical Framework

5.1 Estimation

Our baseline equation is the following intent-to-treat (ITT) specification:

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

where $Treatment_{vp}$ is an indicator for assignment to the intervention in village v in parish p; $Spillover_{vp}$ is an indicator that village v is untreated but where there is a village in parish p that is treated; $ACFIM\_Presence_p$ is the baseline level of presence of the implementer in the parish; $ACFIM_{vp}$ is an indicator that village v is an eligible village (as opposed to the 1,399 spillover villages); and X_{ivp} is a vector of individual-level controls from the survey.[19] In addition, in the Appendix we report a modified version of equation (1) that includes strata fixed effects γ_s. 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 how the effects of the ACFIM campaign vary with the level of treatment saturation (at the level of the parish), in every table we report results from the following equation:

$$Y_{ivp} = \gamma_0 + \gamma_1 Saturation_p + \gamma_3 ACFIM\_Presence_p + \gamma_4 ACFIM_{vp} + \Omega X_{ivp} + \varepsilon_{ivp} \quad (2)$$

Where $Saturation_p$ is defined as the fraction of voters in parish p that are being treated (i.e the intensity of the treatment at the parish level). The main coefficient of interest in this equation is γ_1, which measures the average effect of random treatment saturation across treatment and

[18] 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 corresponding to the 2011 election. For villages with fewer than 40 individuals listed in the voter register, we included all individuals, irrespective of age and gender.

[19] 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 3), and a set of occupation, ethnicity and religion dummies. From the electoral data, we include the 2011 turnout, the 2011 fraction of the vote received by the incumbent candidate in the corresponding election, and the number of registered voters in 2016.

spillover villages. Note that equation (2) was not specified in our pre-analysis plan. We present estimates from this equation mainly for ease of exposition, and because we consider the main effect of treatment saturation to also be of interest. Note that this regression specification assumes a constant effect of saturation on both treated and spillover villages – as our results make clear, this is empirically the case for many outcomes. We discuss later why this may be the case.

Finally, to estimate how treatment and spillover effects vary with saturation, we also run the following linear saturation model:

$$\begin{aligned} Y_{ivp} = \beta_0 + \beta_1 Treatment_{vp} + \beta_2 Spillover_{vp} + \beta_3 ACFIM\_Presence_p \\ +\beta_4 Treatment_{vp} \times Saturation_p + \beta_5 Spillover_{vp} \times Saturation_p + \beta_6 ACFIM\_Presence_p \times Treatment_{vp} \\ + \beta_7 ACFIM\_Presence_p \times Spillover_{vp} + \beta_8 ACFIM_{vp} \times Spillover_{vp} + \Omega X_{ivp} + \varepsilon_{ivp} \quad (3) \end{aligned}$$

Estimates from this specification are reported in Table 9A through 9F for our main outcomes of interest. The two main coefficients of interest here are β_4 and β_5, indicating how the treatment and spillovers effects, respectively, change with saturation. The $ACFIM\_Presence_p \times Treatment_{vp}$ and $ACFIM\_Presence_p \times Spillover_{vp}$ terms purge the saturation model of the portion of the variation in saturation that comes from (non-randomly assigned) degree of ACFIM presence, leaving only the randomly assigned portion, giving us causal estimates for the effect of saturation. Note also that the coefficients β_1 and β_2 in this specification are, respectively, simply intercepts for the treatment and spillover groups when parish saturation is zero, and thus do not have a meaningful interpretation.

5.2 Dealing with multiple outcomes and comparisons

We sought to reduce the risks of false discovery or cherry picking results in a number of ways. First, we prespecified our hypotheses, estimation framework, and outcomes in a pre-analysis plan.[20]

Second, we singled out one family of outcomes as primary: survey-based reports that candidates gave cash and in-kind gifts to the respondent or other villagers, where we are interested in both the direct effect of treatment and the spillover effect of the ACFIM campaign. In addition, we pre-specified a number of secondary outcomes to shed light on mechanisms behind our primary results, including measures of the aggregate supply and demand for votes at the village level, policy campaigning, vote shares and turnout, as well as attitudinal outcomes.[21]

Finally, we reduced the number of primary hypotheses to test by combining them into mean effects indexes of all outcomes in that family.[22]

[20] See https://www.socialscienceregistry.org/trials/377, archived on December 18, 2015.
[21] We report experimental results on village inflation in a separate paper.
[22] We take averages of our outcome measures, coded to point in the same direction, akin to the approach by Kling

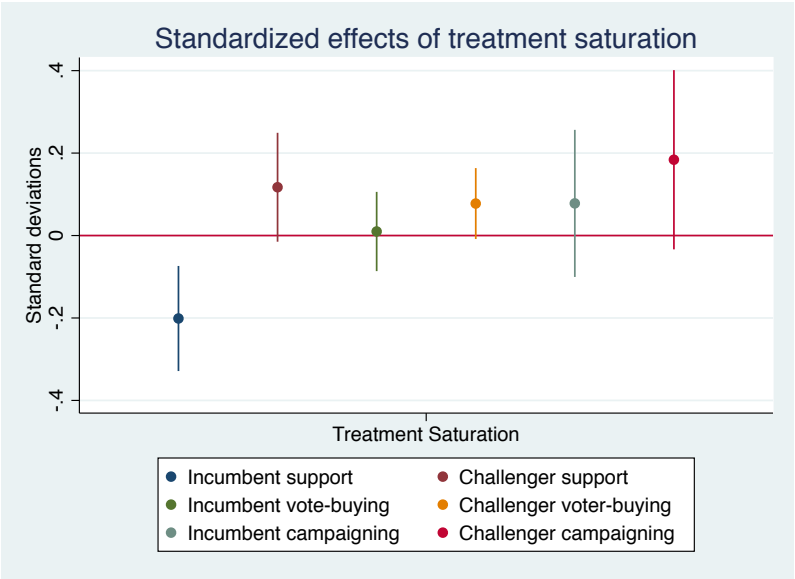

Figure 1: Main treatment effects

5.3 Randomization balance

Treatment is generally balanced along covariates. We present randomization checks in Tables 10A through 10F. We use a range of baseline or time-invariant variables from the voter survey, key informant survey, and official electoral data – these variables are described in Appendix 3. We regress these variables on our two main specifications, namely equations 1 and 2 from section 5, and report all the coefficients from these specifications. Of 99 coefficients (from 66 regressions), only 9 (9%) have a p-value less than 0.1 – almost exactly what should have occurred as a result of chance. Nonetheless, in the remainder of the analysis we show that our main results are robust to controlling for baseline covariates.

6 Results

Figure 1 shows the main effects of the campaign, in terms of standard deviations of key indices, using the saturation specification in equation 2. The first two effects are the strongest and most robust: the campaign reduced the vote share of incumbents and increased the share of the vote accruing to challengers. The effects on vote-buying are more nuanced – there was no significant effect on incumbent vote-buying, but some evidence for increased challenger vote-buying activities in heavily treated parishes. In addition, there is noisy but sizable evidence for an increase

et al. (2007)). Component variables are first standardized, then averaged, then standardized again to have mean zero and unit standard deviation. 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.

in campaigning, particularly by challengers, in heavily saturated parishes.

Despite having not pre-specified it, we use the saturation specification (equation 2) as a baseline as we explore these effects in more detail below. As an expositional point, we believe that this specification is easily interpretable: it is the effect of total treatment saturation, which is randomly assigned, conditional on initial ACFIM presence (for which we control). However, this specification is only helpful *because* the effects on treated and spillover villages tend to almost always go in the same direction and be of similar magnitudes. If the effects were off-setting, as we had anticipated, then this specification would have masked important heterogeneity. However, this does not seem to be the case here. As we discuss in more detail later, we believe that this is likely true because candidates and brokers noticed the presence of the ACFIM intervention, but out of a lack of precise information or due to logistical returns to scale in vote buying and policy campaigning, tended to affect the entire parish when they changed activities. Likewise, the ensuing changes in perceptions about the candidates affected voters across the parish, not just voters in the targeted villages, but with an intensity that rose with treatment saturation in the parish.

6.1 Compliance and Quality of Implementation

Funding and logistical delays meant that ACFIM implemented the intervention later and more hastily 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 estimated that the leaflet was received by 67,374 households across 1,427 targeted villages, or approximately 41% of the total population in these villages (there were 422,110 registered voters in total across all treatment villages).[23] Following the leaflet drop, an estimated 62,566 households participated in at least one meeting, which averaged 30 participants, and 21,390 posters (15 per village) were sent across all treatment villages. Finally, a total of 32,674 automated calls were made on the eve of the election (i.e. on February 17, 2016 between 5pm and 8pm) to individuals who provided their phone number to ACFIM in one of the previous meetings. 18,451 (56%) of these calls were answered according to administrative data provided by the implementing company.

In general, ACFIM administrative notes suggest that the activists implemented the meetings in accordance with their training and the meeting scripts.[24] The survey data tell a similar story.

[23] This percentage is estimated from a back-of-the-envelope calculation based on the following figures. 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 using our survey, which found that 37 percent of individuals in treatment villages said they received a leaflet.

[24] The note takers indicated that the facilitators followed the script in almost all of the meetings, and that the facilitators succeeded at conveying the purpose of each meeting. Consistent with the goal of the first meeting, when

Table 2 reports control means and treatment effects on various implementation measures, including treatment and spillover effects from equation (1) in odd-numbered columns, and treatment effects from parish-treatment intensity from equation (2) in even-numbered columns. Respondents in treatment villages were 34 percentage points more likely to report an organization with anti-vote buying messages and the presence of leaflets, 29 percentage points more likely to have attended a meeting, and 3 percentage points more likely to have received a call.[25]

The control means in Table 2 are nonzero, suggesting that other civic or political organizations were active, as one would expect, but the absence of any statistically significant effects on spillover villages suggest that the villages were generally not experiencing ACFIM's campaign directly. All this is consistent with ACFIM administrative notes. The meetings were largely conducted with people from the village assigned to treatment – only an average of less that two in thirty individuals were outsiders in the sense that they belonged to another village.[26]

Based on data from ACFIM note-takers, in 70% of the village meetings there was at least one influential individual likely to engage in or mediate vote-buying activities, including local councilors, MPs, candidate, and brokers.[27] In 74% of the cases where at least one of such individuals was present, they reportedly tried to influence the meeting. Such participation rates could indicate that these individuals were well aware of the ACFIM campaign and potentially felt threatened by it.

6.2 Equilibrium outcomes of the ACFIM campaign

ACFIM's campaign urged villagers to refuse to sell their vote to brokers and candidates. This section looks at equilibrium outcomes of the campaign, with a particular focus on the primary prespecified outcome, the incidence of vote-buying transactions. We also examine a natural intermediary outcome, the impact on attitudes and social norms surrounding vote buying. We see no significant evidence that vote buying transactions decreased overall, although there is heterogeneity between types of candidates, with challengers being more likely to engage in vote-buying transactions both in treatment and spillover villages. Consistent with these effects,

asked (not exclusively) about the goals of the first meeting that were conveyed to participants by facilitators, note takers indicated that in 73% of cases was the introduction of the campaign and discussion the leaflet content, and in 51% the sharing of the participants' views about on vote buying and selling. The second meeting was a transition meeting designed to provide an avenue for a collective deliberation on vote-buying. There is more variation in what note takers indicated but all meetings are consistent with the intended purpose. Similarly to the first meeting where its goal was clear, in the third meeting note takers indicated that in 61% of cases the conveyed goal was to deliberate on and arrive to a community resolution against vote buying.

[25] We expected a smaller effect on calls received, since calls were only made to individuals who voluntarily shared their cell phone numbers during the anti-vote-buying meetings organized by ACFIM in the village.

[26] Importantly, the share of outsiders across meetings was constant, which lessens the concern of cumulative effect characteristic of significant spillovers.

[27] If we use a less stringent definition of influential individual likely to engage in or mediate vote-buying activities, in almost all village meetings such an individual was present.

we see only a moderate effect of the campaign on social norms.

6.2.1 Vote buying transactions

Tables 3 and 4 report the effects on self-reported receipts of cash and other gifts in exchange for a vote, measured across all candidates and disaggregated across different types of candidates running for the Presidential and the Parliamentary election. 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. We look at vote-buying measured across all candidates and across two types of candidates: incumbents,[28] and challenger (i.e., non-incumbent) candidates. For the purpose of this categorization, vote tallies are computed at the national level for the presidential election, and at the constituency level for the parliamentary election.[29] We expected that vote buying would fall in treated villages, and potentially rise in spillover villages, with both effects increasing in saturation levels.

Table 3 reports the impacts of treatment on a standardized index of 4 variables capturing the prevalence of vote buying: whether the respondent received any gift in cash, the log of 1 plus the amount of cash received, whether the respondent received any gift in kind, and the log of 1 plus the value of gifts received in kind (disaggregated across the two types of candidates). Coefficient signs are generally in the opposite direction of what we expected, although the magnitudes are generally small and not statistically significant. When pooling all races and candidates, reports of vote buying transactions increase by 0.034 standard deviations in treated villages, and rise by 0.015 standard deviations in spillover villages. We can effectively rule out medium and large effects of the ACFIM campaign on these transactions. The coefficients on the parish saturation interaction term is positive – the opposite of what was expected – and non-significant.

To further explore heterogeneity across candidates, Tables 4A and 4B illustrate campaign effects on the two main components of the index: a dummy variable indicating whether the survey respondent reported receiving cash, summed up across races and candidates (in Table 4A), and the log amount of cash (plus 1 Ush, to avoid dropping zeros) received in exchange for votes (in Table 4B).[30,31] The two tables tell a consistent story. First, across the board there

[28] For incumbent MPs, the identity of incumbents was obtained from a fuzzy match by district, constituency, name and party between the official election results (obtained from the Electoral Commission) and the list of MPs in the 2011-2016 Ugandan Parliament. When no incumbent candidate was found in a particular constituency, incumbent status was defined by party (i.e., if the seat of constituency A had been occupied by party B, the candidate running under the banner of party B was defined as the incumbent, except if party B is "Independent").

[29] These definitions make it unlikely that the coding of challenger candidates corresponding to each race is affected by our treatment.

[30] In our survey data, we collected data on all individuals (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 having received a gift from a particular candidate if she mentioned this candidate among the individuals the brokers were working for.

[31] We do not condition on receiving a positive amount of money in these estimates, so they should not be inter-

is no evidence that the ACFIM campaign reduced the incidence of vote-buying for any type of candidate. Second, there is no evidence that the campaign affected vote-buying by incumbents (column (3) and (4) of both tables), as we cannot reject null average effects, nor that parish-level saturation had a null effect on vote-buying. Third, there is some evidence that the campaign *increased* vote-buying by challenger candidates (columns (5)-(6)). We find that these effects are positive on average across treatment and spillover villages, and increasing with parish-level saturation. The magnitude of these effects is not negligible, with challengers spending 25% more in fully treated parishes than control parishes.

It is noticeable that, when looking at vote-buying by challengers, the significant effect of the campaign is similar in treatment and spillover villages. This suggests the possibility that, when challengers buy votes in a parish, they do so in all villages within the parish, possibly due to logistical returns to scale in vote buying. This would imply that most of the action takes place along the extensive margin, i.e., challengers entering treated parishes and the villages in those parishes. To explore this implication, Tables 4C and D, respectively, show as an outcome whether a candidate operates in a village or parish. These estimates sum across the presidential and MP races, so the variables in question are counts of candidates (note that there can be at most two incumbents (one president and one MP), but many more potential challengers). The strongest results are for challengers (about 0.13 more challengers operating in the parish), though they do not reach conventional levels of significance.

6.2.2 Attitudes and social norms

For a campaign such as ACFIM's to be effective, the campaign must succeed in changing perceived social norms and people's attitudes towards vote buying. Our survey measured a handful of perceived norms and attitudes. We report treatment effects of the campaign on these variables in Table 5. The estimates in this table suggest that a majority of respondents already held the belief that vote-buying had negative consequences and imposed a social cost on their villages, and that the ACFIM campaign increased these attitudes slightly. It also increased the expectation of social sanctions as a result of vote buying, a key way in which social norms are enforced by the community. However, across all of our measures, the changes in beliefs tend to be small, in part because many respondents in control villages already held anti-vote-buying beliefs. In addition, one caveat to this analysis is that our measures are self-reported. Whether individuals condemn or claim to have ostracized vote-sellers might be subject to social desirability bias, particularly in treated villages.

Column (1) in Table 5 reports effects on whether survey respondents thought exchanging money for votes had negative consequences for the village. 89% already agreed with this state-

preted as price effects, but rather as effects on average amount received (including both the intensive and extensive margins).

ment in control villages, and this increased by just 1.3 percentage points in treated villages (significant at the 10% level). This effect is also increasing with parish-level saturation (column (2)). Turning to perceived social sanctions, we asked whether people in the village would be understanding towards, or angry at, the respondent for selling his or her vote (columns (3)-(4)). Similarly, we asked whether a person selling his or her vote would be ostracized by the rest of the village (columns (5)-(6)). Across these columns, treatment increased perceived sanctions on average by roughly 2 percentage (columns (3) and (5)). The corresponding coefficients on parish-level saturation are positive but not statistically significant (columns (4) and (6)).

In addition, we conducted a vignette experiment collecting respondents' perceptions of a hypothetical hard-working man in financial distress selling his vote to provide for his household.[32] 73% of control villagers agreed that this was totally unacceptable. This does not rise significantly in the average treated village, nor with parish-level saturation (columns (7)-(8)). Finally, columns (9)-(10) test whether the campaign affected a measure of self-reported vote-buying – whether the respondent reported to vote for a candidate she also accepted a vote-buying offer from. The campaign had no significant effects on this variable.

6.3 Program impacts on electoral outcomes

6.3.1 Program impacts on candidates' vote shares

In Table 6A and 6B, we report treatment effects on the electoral performance of incumbent and challenger candidates, using self-reports from the voter survey (in Table 6A) as well as administrative data (in Table 6B). The vote shares are z-standardized and results are pooled across the Presidential and Parliamentary races (the results for the individual races are similar). Regressions conducted using the survey data are run at the individual level, while regressions using the electoral data are run at the polling-station level. As above, we report the coefficients from equation (1) in odd-numbered columns and those from equation (2) in even-numbered columns. In both tables, we report outcomes computed across 2 types of candidates: incumbents and non-incumbents (labelled "All challengers").

As a cautionary note, whether one should put more weight on the self-reported data or the administrative data is a priori unclear. The self-reported 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

[32] The exact phrasing of the vignette experiment was: "Imagine a man who lives with his wife and children in this village. He works hard, but he frequently has trouble maintaining his family economically. During the electoral campaign, a member of a party offered him a certain amount of cash (with the actual amount randomly determined across respondents) so that he would vote for the party. The man accepted the money and voted as he was instructed. In your opinion, was the behavior of this man: completely acceptable, wrong but understandable, or totally unacceptable?"

error in the administrative data (coming from the fact that polling stations to which treated villages correspond had some voters from non-treated villages), self-reported data may yield more precise estimates of actual voting behavior.[33]

Overall, the estimates in Table 6A-6B suggest that the ACFIM campaign led to a decrease in the electoral performance of incumbents in treated villages, which is statistically significant in the survey data (Table 6A, column (1)) but not in the administrative data (Table 6B, column (1)). Across both data sources, there is robust evidence that support for incumbents falls in heavily treated parishes (columns (2)). These estimates suggest two important takeaways. First, the campaign *negatively* affected the electoral performance of incumbents. These results are opposite to those in Vicente (2014), where a comparable anti-vote-buying campaign increased support for incumbent candidates on average. Second, this negative impact is driven by parishes with a high degree of treatment saturation. This could reflect several mechanism being at play, which we discuss later in Section 7.

In the remaining columns of Tables 6A and 6B, we show effects on the electoral performance of challenger candidates. These estimates are, as expected, symmetric to those observed for incumbents. The estimates obtained using the survey data (Table 6A) show a significant increase in support for challengers in both treated and spillover villages (column (3)) and in heavily saturated parishes (column (4). Results obtained using the administrative data are similar, but only statistically significant in column (4).

6.3.2 Program impacts on voter turnout

Table 7 presents treatment effects on voter turnout, measured using administrative data. Turnout is z-standardized and pooled across the Presidential and Parliamentary races in columns (1)-(2), and shown in levels for each race in columns (3)-(6). We do not report results on self-reported turnout given the implausibly high turnout in our survey data (95% for the Presidential election and 93% for the Parliamentary election). Turnout in the administrative data was 67% for the Presidential poll and 69% for the Parliamentary elections.

We see some evidence that the ACFIM campaign moderately increased voter turnout, when pooling across both races – the effect size for treated villages is approximately 0.07 SDs (not statistically significant). Looking at each race individually, the point estimates for the average treatment effect and the average spillover effect are less than one percentage point. Parish-level saturation has a positive effect on turnout, but this effect falls short of statistical significance (columns (2), (4) and (6)).

[33] Lastly, there were allegations of vote fraud, on which the campaign could have an effect. However, we discuss and dismiss this possibility in Appendix 2.

6.4 Program impacts on campaigning

To explore the possibility that candidates substituted to other (non-vote-buying) strategies in response to the ACFIM intervention, in Table 8 we look at whether treatment status, spillover status, and parish-level saturation increased the occurrence of other forms of political campaigning, which was another of our pre-registered secondary outcomes. In our survey data, the three campaigning methods most cited by voters were displaying political posters in the village (cited by 87% of respondents across all ballots and candidates), village visits (62%), and campaigning through loudspeakers, SMS or phone calls (39%). The outcomes in this table are the sum of campaign activities (out of 5 possible activities: visiting the village, putting up posters, distributing leaflets, campaigning via loudspeakers/SMS, and distributing merchandise) conducted by each type of candidate in our sample villages, and averaged across Presidential and Parliamentary races.

Table 8 provides some evidence that some politicians (namely challengers) adapted their campaigning strategies in response to our intervention. For incumbent candidates, campaign effects in treatment villages are positive, but not significant, and smaller in magnitude, while spillover effects are negative and non-significant (column (3)). The coefficient on parish-level saturation is positive but also non-significant (column (4)). Challenger candidates (columns (5)-(6)) seem to increase their campaigning efforts in response to the ACFIM intervention: for example, full treatment saturation leads to a 0.5 SD increase in campaigning effort relative to zero-saturation parishes (column (6)). Overall these estimates suggest that, contrary to our expectations, vote-buying and policy-campaigning strategies are complements rather than substitutes. This is also supported by the positive correlation between the two variables in control parishes.

7 Discussion

Our main results show that the ACFIM campaign had a large effect on vote shares, and some effects on vote-buying and campaigning by candidates. In particular, challenger candidates appear to invest more in policy-campaigning and to engage more in vote-buying. In addition, total turnout rose slightly. Moreover, these results are largely driven by parishes with high treatment saturation, with little difference between the point estimates for treatment and spillover. So, any theoretical explanation behind these findings must account for effects that permeate the entire parish, rather than solely treated villages.

We believe that one of two complementary theoretical explanations can account these results when focusing on the supply side of votes – the voters. First, the treatment shifted voters to reject vote-buying offers or to no longer think about them as binding contracts, and vote for the

candidate they deemed better suited for office. The latter mode of acting is described by the Ugandan adage, "Eat widely, vote wisely," which was adopted as the official resolution in 30% of villages that reported an official resolution. In any case, by convincing voters that they should be free to vote for their preferred candidate, as opposed to the highest-bidding candidate, the ACFIM campaign substantially reduced the advantage of incumbents, who generally have a stronger local presence and can afford to offer more money to voters.

The second alternative is that the campaign served, inadvertently, to coordinate voters on an anti-incumbent message. The framing of the campaign was about the pernicious effect of vote-buying (a practice more commonly used by incumbents) on public-service delivery (a responsibility at which most incumbents are perceived to fail). As a result, notwithstanding the non-partisan language of the campaign, the public meetings on this topic could have shifted local beliefs against incumbents.

Crucially, regardless of the driving force behind the voters' change of attitudes, subsequent to these changes (which favored challengers) candidates as well the brokers working for them responded by shifting their vote-buying and policy-campaigning efforts across parishes. In particular, we observe a larger increase in vote-buying and policy-campaigning by challengers. Importantly, this increase occurs *throughout* the treated parishes (in treated and spillover villages) and rises further with higher treatment saturation. We also see that these effects are largely driven by challenger or their brokers starting operation in villages and parishes where they would have not operated absent the campaign. These effect suggests that there are local returns to scale in vote-buying and policy-campaigning – due to fixed costs of operating in a parish, once a decision to enter the parish is made, candidates and brokers operate in all villages in the parish. Our qualitative accounts indicate that village-level brokers are recruited and managed by higher-level brokers who operate at the parish level, which supports the presence of such fixed costs. This vote-buying and policy-campaigning technology then accounts for the similar magnitude of the treatment effect in treatment and spillover villages: the comparable effects reflect changes in candidate behavior in the parish overall, rather than direct spillovers across villages.

Lastly, we rule out that several alternative explanations can fully account for these results. First, it is possible that the campaign did diminish vote-buying, but, contrary to most expectations about the effect of social desirability bias, induced people to more honestly report vote-buying in their villages, which yields a zero or positive effect on *reported* vote-buying. This does not seem to be the case. For instance, we find no significant effect of the campaign on self-reported vote-buying in the 2011 election (results available upon request). This is likely to be a good test for social desirability bias, since the 2011 election pre-dated the campaign and under random assignment there should be no relationship between treatment and 2011 vote-buying, except through a social desirability or salience channel. In addition, our results on attitudes

about vote-buying suggest that the campaign intensified negative feelings about vote-buying (though only by a small amount).

Second, it is possible that the null results on total vote-buying are due in part to agency problems between candidates and their brokers. Interviews with elected candidates and focus groups indicate that brokers are subject to significant moral hazard and that the combination of imperfect monitoring ability and competition for brokers around elections allows them to extract large rents. Candidates often provide lump sums of cash or other gifts to brokers with which to secure the support of villages under their influence. This is part of a performance contract wherein these brokers are expected to deliver a certain electoral support to the candidates. Brokers who fail to reach these targets lose their position in future elections, as well as their connections with winning candidates. Brokers then do solve a cost-minimization problem to achieve that target and keep the remaining resources for themselves. If brokers responded to the campaign by increasing the fraction of the money that they spent on voters (reducing what they kept for themselves) in an effort to overcome a weakening of the traditional vote-buying arrangements, this would have undone some of the effects of the campaign on vote-buying. However, this should have been particularly true for vote-buying by incumbents (who were facing an actual loss of support), which we do not observe.[34] In addition, importantly, this story does not explain the increase in challenger vote-buying and policy-campaigning.

Third, it is possible that the campaign deterred electoral fraud that otherwise would have favored the incumbents by engaging citizens in the electoral process. As noted in Appendix 2, however, there is no evidence that the campaign was related (positively or negatively) to the presence of markers for electoral irregularities. Although these tests are imperfect, their outcome is also supported by the results that instead focuses on self-reported data as an outcome. Overall, we take this as strong evidence that this cannot be a primary driver of our results.

8 Conclusion

This paper documents the effect of the largest anti-vote-buying campaign ever evaluated – with almost half a million voters treated across nearly 1,500 treated villages in Uganda – on vote-buying, policy-campaigning, and electoral outcomes. We found that the campaign, in spite of its relatively heavy footprint – leaflets, three village meetings, and a village-wide resolution – was not effective at preventing vote-buying, but we provide suggestive evidence that it did free people from traditional vote-buying relationships. As a result, voters were less likely to vote for incumbent candidates and more likely to vote for the main challengers. These effects were large, especially in heavily treated parishes, enough to reverse the position of the average incumbent

[34] Nor do we observe a bimodal distribution of gift-giving, where some brokers give up and others redouble their efforts, which could also occur.

and challenger in parishes with high saturation.

Our results on the prevalence of vote-buying runs counter to previous experimental evidence on such campaigns, as in Hicken et al. (2014) and Vicente (2014), both of whom find that comparatively less intensive interventions had sizable impacts on votes sold. We believe that the differences between our findings and those previously reported in the literature are best explained by the difference in scope and, inadvertently, in message, between our experiment and those cited above. The large scale and high degree of publicity of the ACFIM campaign, as well as the fact that local political brokers attended the community meetings intended to coordinate citizens' efforts against vote-buying, prompted candidates to respond to the ACFIM campaign. In addition, the common local decision to decide to "eat widely, vote wisely" (the public context reduced the ability of the campaign to control the precise message) meant that the effects may have shifted away from changes in vote-buying levels and towards changes in voting decisions *conditional* on the vote-buying offer they accepted.

However, our results indicate that it may be possible to disrupt its effectiveness by unmooring the relationship between vote-buying and voter behavior. In a dynamic game, where candidates seek to use the most cost-effective methods of gaining voter support, this breakdown in voter willingness to honor the vote-buying "contract" should induce candidates to shift towards other methods in future elections.

Thus, in future elections, and during the current tenure of the newly elected officials, we might expect this result to change candidate behavior. In particular, we believe that these results may induce candidates to emphasize and keep promises of future public goods rather than vote-buying, which could have substantial impacts on governance in Uganda. Future research should continue to examine this aspect of candidate message-optimization and its implications for electoral and economic outcomes.

In terms of welfare, as with any intervention around elections, the effects are difficult to estimate and a full accounting is beyond the scope of this paper. Previous research (e.g. Besley et al. (2010)) suggests that increasing the competitiveness of local elections improves the quality of governance. In this sense, since the campaign appeared to relatively advantage challengers, we might expect it to have positive effects. In addition, since total amount received by voters did not fall, it does not appear as though voters had short-term costs in foregone vote-buying offers.

In addition, future work should continue to explore how to break down the vote-buying equilibrium. Our results highlight that one-sided interventions of large scale and visibility are likely to fail to eradicate vote-buying if candidates respond to them. Future work would ideally then target both candidates and voters for treatment. In particular, in addition to tackling vote-selling, as we did in our intervention, there is the need to convince candidates to pledge not to buy votes to then undermine the demand for vote-buying. These efforts are politically sensitive

and thus would need to be taken by a local organization with strong connections to multiple political parties, but could yield important insights about the relative merits of intervening on the demand, as opposed to simply the supply side of the votes' market.

We believe that this paper opens new avenues of research on both vote-buying and on campaigning in low development countries more broadly. This remains a fruitful area for more work, with important policy implications and potential for contributions to our knowledge about voter behavior and governance.

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ONLINE APPENDIX

Appendix 1: Discussion of external validity

The presence of a local ACFIM activist is clearly non-random. Our treatment randomization was within the sample of parishes/villages with local ACFIM activists, so this is *not* a problem for internal validity, but it does require a brief discussion on external validity. The first note on external validity is that, from the perspective of civil society organizations considering similar campaigns, the villages/parishes with pre-existing civil society presence may, in fact, be the policy relevant sample. The strength of CSOs often lies in their local credibility, built over multiple years and sustained through the presence of local members of the larger national CSO, so few CSOs would launch a campaign in villages (or parishes) to which they had never been. However, it is still worth noting the differences.

First, to be in our sample, a parish must contain at least 1 village where a local ACFIM activist works or lives. Since we do not survey any parishes with zero ACFIM presence, we cannot compare our sample directly to other parishes. However, we can correlate the degree of ACFIM presence (i.e. the percent of voters in a given parish who live in villages with ACFIM presence) with covariates to explore this selection indirectly. As we might expect, ACFIM presence is correlated with lower vote-share in 2011 for the incumbent president – a parish with 100% ACFIM presence voted for the incumbent by 7 percentage points fewer, on average, than one with 0% ACFIM presence. In our survey, we asked voters whether they received a gift for their vote in 2011 (the prior election). Again, as we might expect, ACFIM presence is correlated with less prior vote-buying: using the same 100% to 0% comparison, full ACFIM presence is correlated with a 5 percentage lower share of respondents reporting receiving a gift in 2011.

Second, within each parish, we sample every village where an ACFIM activist had the potential to work (whether in the treatment or control). However, in addition, we sampled 1,399 additional villages in the same parishes that were ineligible for treatment, but where we could look at spillovers. Throughout the analysis, we control for a dummy indicating that a village was not part of the experimental sampling frame. As can be seen in the results later, this dummy is usually insignificant, indicating that these villages do not generally differ from the untreated villages that were part of the experimental sample, though in some specifications a small difference appears.

Appendix 2: Electoral data integrity

Opposition leaders in Uganda and international observers challenged the integrity of the voting data in the aftermath of the election (All Africa, 2016; Daily Mail, 2016; Newsweek, 2016). Analysts noted several potentially suspicious patterns. We acknowledge these issues, but believe

that the electoral data can still be useful for our analysis for several reasons. First, we generally obtain similiar results using self-reported voting outcomes from our voter survey and using the official election data. Second, we show in the Appendix that our treatment is uncorrelated with traditional markers of electoral malfeasance (Beber and Scacco, 2012).

Appendix 3: Variables used for Randomization Checks

From the voter survey, we use the age, years of education, marital status (a dummy variable for married individuals), land ownership (a dummy for households that own any land), the number of adults and children in the household, an index of asset ownership,[35] four measures of occupational status (dummy variables for individuals working in farming, trade/retail, any high-skill activity, or not actively working),[36] dummy variables indicating the individual belongs to one of Uganda's three largest ethnic groups (Ganda, Nkole and Soga), and three dummy variables for being 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 dummy 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 valid votes cast in 2011, the voter turnout in 2011, the vote shares of the NRM and of the FDC in 2011, and the number of registered voters in 2016.

[35] To construct this index, we simply add up dummy variables indicating ownership of a TV, radio, motor vehicle, and cell phone

[36] 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 1: Summary Statistics

	Mean	SD	N
Survey data			
Recalls NGO visit in village	.324	.468	27807
Received a leaflet	.172	.377	28060
Recalls meetings took place	.129	.335	27755
Attended meeting	.207	.651	27745
Received a robo-call	.053	.224	28507
Recalls posters	.129	.335	28133
Negative consequences	.895	.306	28507
People angry	.756	.43	28507
Vote sellers ostracized	.579	.494	27732
Vote-buying unacceptable	.744	.437	28501
Any cash received, any candidate	.4	.49	28507
Any cash - incumbents	.331	.578	28507
Any cash - challengers	.111	.321	28507
Cash amount received (USh)	1526.1	4269.3	28507
Cash amount - incumbents	1004.0	2864.7	28507
Cash amount - challengers	697.8	2668.5	28507
Reported vote for incumbent	.657	.349	27112
Campaign activities, all candidates	5.901	4.246	28507
Campaign activities - incumbents	3.504	2.536	28507
Campaign activities - challengers	2.397	2.25	28507
Administrative data			
Registered Voters	574.0	202.9	3659
Turnout 2016 - President	.675	.09	3659
Turnout 2016 - MP	.689	.086	3112
Incumbent vote share 2016 - President	.614	.184	3654
Challengers vote 2016 - President	.386	.184	3654
Incumbent vote share 2016 - MP	.441	.246	3104
Challengers vote share 2016 - MP	.559	.246	3104
Turnout 2011 - President	.601	.103	3641
Incumbent vote share 2011 - President	.678	.186	3641

Table 2: Quality of Implementation

	NGO visit		Received leaflet		Meetings Attended		Received call		Posters	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment village	0.335***		0.338***		0.290***		0.029***		0.189***	
	[0.011]		[0.009]		[0.016]		[0.004]		[0.009]	
Spillover	0.018		0.007		0.002		-0.004		0.003	
	[0.011]		[0.006]		[0.013]		[0.004]		[0.006]	
Treatment Saturation		0.416***		0.433***		0.357***		0.035***		0.250***
		[0.021]		[0.017]		[0.026]		[0.007]		[0.016]
Outside Sampling Frame	-0.018	-0.216***	-0.006	-0.216***	-0.023*	-0.204***	0.005	-0.016***	0.008	-0.110***
	[0.011]	[0.011]	[0.006]	[0.009]	[0.013]	[0.012]	[0.005]	[0.003]	[0.007]	[0.007]
ACFIM Presence	0.022	-0.217***	-0.001	-0.250***	-0.014	-0.221***	0.007	-0.014*	0.025*	-0.119***
	[0.018]	[0.024]	[0.015]	[0.019]	[0.026]	[0.031]	[0.007]	[0.008]	[0.015]	[0.016]
R^2	0.14	0.10	0.20	0.14	0.06	0.04	0.04	0.04	0.09	0.07
Control Mean	0.198	0.198	0.052	0.052	0.113	0.113	0.040	0.040	0.062	0.062
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	27756	27756	28007	28007	27693	27693	28454	28454	28081	28081

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets. All regressions control for an ACFIM dummy (in-sample villages) and the parish-level ACFIM presence. dependent variables in this table are self-reported indicators of program implementation: whether the NGO visited, distributed leaflets, held meetings, conducted robocalls, or posted signs.

Table 3: Treatment Effects on Vote-Buying (Z-Standardized)

	All Candidates		Incumbents		All Challengers	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment village	0.034		0.005		0.059**	
	[0.024]		[0.026]		[0.023]	
Spillover	0.015		-0.011		0.045*	
	[0.025]		[0.029]		[0.027]	
Treatment Saturation		0.052		0.012		0.084*
		[0.044]		[0.047]		[0.043]
Outside Sampling Frame	-0.011	-0.022	-0.000	-0.012	-0.017	-0.022
	[0.025]	[0.018]	[0.028]	[0.019]	[0.026]	[0.016]
ACFIM Presence	-0.006	-0.034	-0.060	-0.067	0.070	0.025
	[0.043]	[0.046]	[0.047]	[0.051]	[0.044]	[0.046]
R^2	0.06	0.06	0.06	0.06	0.04	0.04
Control Mean	-0.008	-0.008	0.006	0.006	-0.024	-0.024
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28454	28454	28454	28454	28454	28454

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets. All regressions control for an ACFIM dummy (in-sample villages) and the parish-level ACFIM presence. These dependent variables are standardized index of the following variables: any cash received, natural log of the amount of cash received, any gift received, and log of the value of any gift received. These dependent variables are restricted to the Presidential and Parliamentary (MP) races.

Table 4: Treatment Effects on Vote-Buying: Cash Received

4A: Any cash received (individual level)

	All Candidates		Incumbents		All Challengers	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment village	0.020		0.002		0.018*	
	[0.019]		[0.015]		[0.009]	
Spillover	0.026		0.003		0.023**	
	[0.021]		[0.017]		[0.010]	
Treatment Saturation		0.049		0.019		0.030*
		[0.034]		[0.026]		[0.017]
Outside Sampling Frame	-0.012	-0.007	0.001	0.001	-0.013	-0.008
	[0.020]	[0.014]	[0.015]	[0.011]	[0.009]	[0.006]
ACFIM Presence	-0.052	-0.077**	-0.051**	-0.061**	-0.001	-0.016
	[0.033]	[0.036]	[0.026]	[0.029]	[0.017]	[0.017]
R^2	0.13	0.13	0.11	0.11	0.06	0.06
Control Mean	0.430	0.430	0.327	0.327	0.102	0.102
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28454	28454	28454	28454	28454	28454

4B: Log cash received (individual level)

	All Candidates		Incumbents		All Challengers	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment village	0.013		-0.008		0.129*	
	[0.096]		[0.089]		[0.076]	
Spillover	0.094		0.019		0.214**	
	[0.109]		[0.101]		[0.087]	
Treatment Saturation		0.104		0.061		0.247*
		[0.162]		[0.149]		[0.140]
Outside Sampling Frame	-0.056	-0.002	0.032	0.045	-0.115	-0.045
	[0.100]	[0.071]	[0.094]	[0.067]	[0.078]	[0.047]
ACFIM Presence	-0.424**	-0.472***	-0.402***	-0.433**	0.020	-0.101
	[0.166]	[0.179]	[0.152]	[0.168]	[0.140]	[0.142]
R^2	0.12	0.12	0.10	0.10	0.07	0.07
Control Mean	2.580	2.580	2.136	2.136	1.117	1.117
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28454	28454	28454	28454	28454	28454

Table 4 (Cont.): Treatment Effects on Vote-Buying: Cash Received

4C: Any cash received (village level)

	All Candidates		Incumbent		All Challengers	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment village	0.023		-0.002		0.042	
	[0.058]		[0.044]		[0.030]	
Spillover	0.086		0.002		0.091***	
	[0.065]		[0.047]		[0.034]	
Treatment Saturation		-0.002		-0.031		0.048
		[0.112]		[0.083]		[0.056]
Outside Sampling Frame	-0.130**	-0.078**	-0.052	-0.046*	-0.082***	-0.041**
	[0.057]	[0.031]	[0.040]	[0.025]	[0.030]	[0.016]
ACFIM Presence	-0.351***	-0.342***	-0.330***	-0.313***	-0.082	-0.101*
	[0.100]	[0.105]	[0.073]	[0.079]	[0.051]	[0.054]
R^2	0.04	0.04	0.04	0.04	0.04	0.03
Control Mean	1.216	1.216	0.919	0.919	0.372	0.372
Observations	4111	4111	4111	4111	4111	4111

4D: Any cash received (parish level)

	All Candidates	Incumbent	All Challengers
	(1)	(2)	(3)
Treatment Saturation	0.159	0.103	0.127
	[0.144]	[0.100]	[0.078]
ACFIM Presence	-0.181	-0.233**	0.077
	[0.136]	[0.097]	[0.076]
R^2	0.03	0.04	0.04
Control Mean	1.627	1.208	0.545
Observations	909	909	909

Table 5: Effects of the Campaign on Attitudes Towards Vote-Buying

	Neg Consequences		Social Cost		Ostracize		Vignette		Vote Choice	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment village	0.013*		0.023**		0.022*		0.004		-0.004	
	[0.007]		[0.011]		[0.012]		[0.011]		[0.011]	
Spillover	0.010		0.015		-0.005		0.018		0.007	
	[0.008]		[0.013]		[0.015]		[0.014]		[0.013]	
Treatment Saturation		0.030***		0.029		0.014		0.025		0.006
		[0.011]		[0.018]		[0.020]		[0.020]		[0.018]
Outside Sampling Frame	-0.012	-0.014**	-0.008	-0.011	-0.004	-0.021**	-0.002	0.007	-0.005	0.002
	[0.008]	[0.006]	[0.013]	[0.009]	[0.014]	[0.010]	[0.014]	[0.009]	[0.011]	[0.008]
ACFIM Presence	-0.019*	-0.035***	0.005	-0.010	-0.003	-0.012	-0.053***	-0.065***	-0.053***	-0.056***
	[0.011]	[0.013]	[0.017]	[0.020]	[0.020]	[0.023]	[0.020]	[0.021]	[0.018]	[0.020]
R^2	0.04	0.04	0.04	0.04	0.07	0.07	0.02	0.02	0.10	0.10
Control Mean	0.888	0.888	0.745	0.745	0.567	0.567	0.733	0.733	0.242	0.242
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28454	28454	28454	28454	27680	27680	28448	28448	28454	28454

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets. All regressions control for an ACFIM dummy (in-sample villages) and the parish-level ACFIM presence. Dependent variabless in this table include (1) an indicator of any negative consequences to participating in vote-buying, (2) an indicator of any social cost, (3) whether others would ostracize vote-buying participants, (4) whether the respondent approved of vote-buying behavior in a vignette survey experiment , and (5) whether the respondent accepted a vote-buying offer.

Table 6: Index of Treatment Effects on Vote Shares (Z-Standardized)

6A: Electoral Support, Survey Data

	Self-Report (Incumbent)		Self-Report (All Challengers)	
	(1)	(2)	(3)	(4)
Treatment village	-0.087**		0.087**	
	[0.038]		[0.038]	
Spillover	-0.083**		0.083**	
	[0.041]		[0.041]	
Treatment Saturation		-0.236***		0.236***
		[0.071]		[0.071]
Outside Sampling Frame	0.012	0.018	-0.012	-0.018
	[0.033]	[0.019]	[0.033]	[0.019]
ACFIM Presence	-0.234***	-0.108	0.234***	0.108
	[0.067]	[0.073]	[0.067]	[0.073]
R^2	0.01	0.01	0.01	0.01
Control Mean	0.052	0.052	-0.052	-0.052
Controls	Yes	Yes	Yes	Yes
Observations	27065	27065	27065	27065

6B: Electoral Support, Administrative Data

	Electoral (Incumbent)		Electoral (All Challengers)	
	(1)	(2)	(3)	(4)
Treatment Polling Station	-0.112		0.112	
	[0.069]		[0.069]	
Spillover Polling Station	-0.003		0.003	
	[0.082]		[0.082]	
Saturation		-0.271**		0.271**
		[0.132]		[0.132]
Outside Sampling Frame	-0.116***	-0.062**	0.116***	0.062**
	[0.041]	[0.029]	[0.041]	[0.029]
ACFIM Presence	-0.700***	-0.571***	0.700***	0.571***
	[0.130]	[0.145]	[0.130]	[0.145]
R^2	0.03	0.03	0.03	0.03
Control Mean	0.041	0.041	-0.041	-0.041
Controls	Yes	Yes	Yes	Yes
Observations	3657	3657	3657	3657

Table 7: Campaign Effects on Turnout

	Turnout (z-index)		Presidential Election		Parliamentary Election (MP)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Polling Station	0.065		0.005		0.004	
	[0.045]		[0.004]		[0.004]	
Spillover Polling Station	0.017		0.002		-0.000	
	[0.050]		[0.005]		[0.005]	
Treatment Saturation		0.146		0.013		0.011
		[0.097]		[0.008]		[0.009]
Outside Sampling Frame	-0.103***	-0.127***	-0.010***	-0.012***	-0.008**	-0.010***
	[0.038]	[0.029]	[0.004]	[0.003]	[0.004]	[0.003]
ACFIM Presence	-0.076	-0.144	-0.008	-0.014	-0.009	-0.014
	[0.091]	[0.103]	[0.008]	[0.009]	[0.009]	[0.010]
R^2	0.33	0.33	0.31	0.31	0.30	0.30
Control Mean	-0.008	-0.008	0.674	0.674	0.690	0.690
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3659	3659	3659	3659	3112	3112

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets. All regressions control for an ACFIM dummy (in-sample villages) and the parish-level ACFIM presence. The first dependent variable is a standardized index of the following dependent variables: presidential and parliamentary turnout. Turnout is defined as valid votes divided by registered voters.

Table 8: Index of Treatment Effects on Campaigning (Z-Standardized)

	All Candidates		Incumbents		Primary Challenger	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment village	0.228		0.117		0.112	
	[0.194]		[0.104]		[0.112]	
Spillover	-0.186		-0.056		-0.130	
	[0.215]		[0.115]		[0.124]	
Treatment Saturation		0.776**		0.282		0.494**
		[0.386]		[0.200]		[0.227]
Outside Sampling Frame	0.249	-0.080	0.113	-0.015	0.136	-0.065
	[0.177]	[0.100]	[0.100]	[0.060]	[0.097]	[0.050]
ACFIM Presence	-0.682*	-1.130***	-0.371*	-0.535***	-0.311	-0.596***
	[0.368]	[0.369]	[0.191]	[0.195]	[0.211]	[0.214]
R^2	0.11	0.11	0.12	0.12	0.09	0.09
Control Mean	5.759	5.759	3.400	3.400	2.359	2.359
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	28454	28454	28454	28454	28454	28454

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets. All regressions control for an ACFIM dummy (in-sample villages) and the parish-level ACFIM presence. All variables in this table include only Presidential and Parliamentary (MP) races. These dependent variables are the sums of indicators of campaiging activities: visit to the village, posters, leaflets, advertising over loudspeakers, and merchandise.

Table 9A: Interactions on Key Outcomes

	Campaign Implementation (Table 2)					Treatment Effects on Vote-Buying (Index) (Table 3)		
	(1) NGO	(2) Leaflet	(3) Meetings	(4) Call	(5) Posters	(6) All Candidates	(7) Incumbents	(8) Challengers
Treatment village	0.427***	0.370***	0.328***	0.035***	0.203***	0.043	0.021	0.048
	[0.023]	[0.020]	[0.036]	[0.009]	[0.018]	[0.052]	[0.057]	[0.048]
Spillover	0.040**	-0.006	-0.001	-0.007	-0.009	0.010	0.005	0.010
	[0.019]	[0.010]	[0.025]	[0.008]	[0.011]	[0.047]	[0.053]	[0.042]
Treatment*Saturation	0.090	0.059	0.026	0.006	0.077	-0.074	0.020	-0.141
	[0.064]	[0.069]	[0.098]	[0.025]	[0.055]	[0.126]	[0.126]	[0.129]
Spillover*Saturation	0.090	0.071**	-0.031	-0.010	0.052*	0.199	0.168	0.145
	[0.055]	[0.029]	[0.075]	[0.019]	[0.030]	[0.147]	[0.181]	[0.129]
Outside Sampling Frame	-0.015	-0.006	-0.013	0.008	0.007	-0.029	-0.018	-0.029
	[0.012]	[0.006]	[0.012]	[0.005]	[0.007]	[0.026]	[0.030]	[0.022]
ACFIM Presence	0.109***	0.018	0.016	0.010	0.029**	-0.006	-0.039	0.032
	[0.026]	[0.014]	[0.038]	[0.010]	[0.015]	[0.062]	[0.070]	[0.057]
ACFIM Presence*Treatment	-0.258***	-0.114*	-0.094	-0.015	-0.095*	0.042	-0.055	0.126
	[0.065]	[0.067]	[0.106]	[0.026]	[0.055]	[0.132]	[0.138]	[0.131]
ACFIM Presence*Spillover	-0.111**	-0.021	0.019	0.012	-0.008	-0.110	-0.138	-0.033
	[0.050]	[0.029]	[0.073]	[0.019]	[0.026]	[0.121]	[0.154]	[0.101]
R^2	0.14	0.20	0.06	0.04	0.09	0.06	0.06	0.03
Control Mean	0.198	0.052	0.113	0.040	0.062	-0.008	0.006	-0.019
Controls								
Observations	27756	28007	27693	28454	28081	28454	28454	28454

Table 9B: Interactions on Key Outcomes

	Treatment Effects on Vote-Buying (Any Cash Received) (Table 4A)			Treatment Effects on Vote-Buying (Log of Any Cash Received) (Table 4B)		
	(1) All Candidates	(2) Incumbent (Pres. and MP)	(3) Challenger (Pres. and MP)	(4) All Candidates	(5) Incumbent (Pres. and MP)	(6) Challenger (Pres. and MP)
Treatment village	0.003	0.008	0.014	0.157	0.098	0.102
	[0.028]	[0.024]	[0.017]	[0.203]	[0.193]	[0.129]
Spillover	0.023	0.006	0.003	0.085	0.057	0.042
	[0.027]	[0.024]	[0.017]	[0.201]	[0.190]	[0.125]
Treatment*Saturation	0.072	0.040	-0.009	0.119	0.253	-0.076
	[0.060]	[0.051]	[0.037]	[0.420]	[0.399]	[0.275]
Spillover*Saturation	0.139**	0.144**	0.007	0.963*	1.080**	0.065
	[0.067]	[0.062]	[0.051]	[0.523]	[0.478]	[0.375]
Outside Sampling Frame	-0.018	-0.010	-0.010	-0.103	-0.068	-0.080
	[0.014]	[0.013]	[0.009]	[0.109]	[0.106]	[0.065]
ACFIM Presence	-0.013	-0.036	-0.015	-0.343	-0.303	-0.121
	[0.036]	[0.031]	[0.021]	[0.255]	[0.242]	[0.158]
ACFIM Presence*Treatment	-0.094	-0.060	0.014	-0.392	-0.466	0.118
	[0.069]	[0.058]	[0.038]	[0.476]	[0.456]	[0.285]
ACFIM Presence*Spillover	-0.135**	-0.099*	0.034	-0.581	-0.754*	0.229
	[0.061]	[0.055]	[0.041]	[0.458]	[0.425]	[0.300]
R^2	0.11	0.11	0.06	0.12	0.11	0.06
Control Mean	0.409	0.276	0.101	2.410	2.136	0.742
Controls						
Observations	28454	28454	28454	28454	28454	28454

Table 9C: Interactions on Key Outcomes

	Extensive Margin, Village Level (Table 4C)		
	(1) All Candidates	(2) Incumbent (Pres. and MP)	(3) Challenger (Pres. and MP)
Treatment village	0.016	0.086*	0.066
	[0.036]	[0.051]	[0.058]
Spillover	0.028	0.068	0.070
	[0.035]	[0.051]	[0.057]
Treatment*Saturation	0.145*	-0.059	-0.095
	[0.086]	[0.134]	[0.140]
Spillover*Saturation	0.150	0.146	-0.012
	[0.101]	[0.141]	[0.160]
Outside Sampling Frame	-0.040**	-0.040	-0.067***
	[0.016]	[0.026]	[0.026]
ACFIM Presence	-0.094**	-0.102	-0.056
	[0.044]	[0.069]	[0.071]
ACFIM Presence*Treatment	-0.182**	-0.136	0.041
	[0.087]	[0.139]	[0.142]
ACFIM Presence*Spillover	-0.143	-0.232*	0.028
	[0.091]	[0.131]	[0.144]
R^2	0.01	0.02	0.01
Control Mean	0.408	0.691	0.364
Controls			
Observations	4111	4111	4111

Table 9D: Interactions on Key Outcomes

	Attitudes Toward Vote-Buying (Table 5)					Index of Self-Reported Electoral Support (Table 6)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Neg Consequences	Social Cost	Ostracize	Vignette	Vote Choice	Incumbent	Challenger
Treatment village	0.015	0.052**	0.070***	0.006	0.007	0.019	0.008
	[0.015]	[0.023]	[0.026]	[0.024]	[0.023]	[0.065]	[0.062]
Spillover	0.029**	0.020	-0.003	0.002	0.006	-0.008	0.029
	[0.015]	[0.023]	[0.026]	[0.025]	[0.023]	[0.066]	[0.064]
Treatment*Saturation	0.115***	0.006	0.004	0.118**	0.036	-0.416***	0.266*
	[0.036]	[0.049]	[0.062]	[0.055]	[0.041]	[0.158]	[0.155]
Spillover*Saturation	0.039	0.069	0.001	0.038	0.116*	-0.465**	0.407**
	[0.037]	[0.060]	[0.063]	[0.070]	[0.059]	[0.195]	[0.189]
Outside Sampling Frame	-0.018**	-0.011	0.003	-0.002	-0.015	0.052	-0.044
	[0.008]	[0.015]	[0.016]	[0.016]	[0.013]	[0.032]	[0.031]
ACFIM Presence	-0.005	0.031	0.037	-0.059*	-0.052*	-0.015	0.110
	[0.020]	[0.029]	[0.033]	[0.031]	[0.029]	[0.091]	[0.087]
ACFIM Presence*Treatment	-0.110***	-0.064	-0.093	-0.108*	-0.051	0.209	-0.192
	[0.041]	[0.056]	[0.070]	[0.061]	[0.049]	[0.174]	[0.161]
ACFIM Presence*Spillover	-0.065*	-0.055	-0.011	0.009	-0.064	0.173	-0.225
	[0.035]	[0.057]	[0.061]	[0.065]	[0.053]	[0.183]	[0.170]
R^2	0.04	0.04	0.07	0.02	0.09	0.08	0.08
Control Mean	0.888	0.745	0.567	0.733	0.243	0.053	-0.015
Controls							
Observations	28454	28454	27680	28448	28454	27068	27068

Table 9E: Interactions on Key Outcomes

	Index of Official Electoral Support (Table 6)		Turnout (Table 7)		
	(1)	(2)	(3)	(4)	(5)
	Incumbent	Challenger	Turnout Index (Z)	Presidential Turnout	MP Turnout
Treatment Polling Station	-0.038	-0.006	-0.033	-0.003	-0.007
	[0.102]	[0.120]	[0.093]	[0.008]	[0.009]
Spillover Polling Station	0.061	-0.076	-0.001	0.002	0.000
	[0.102]	[0.118]	[0.092]	[0.008]	[0.008]
Treatment*Saturation	-0.932***	0.945***	0.107	0.009	0.012
	[0.231]	[0.321]	[0.253]	[0.022]	[0.024]
Spillover*Saturation	-0.437	0.605*	0.536*	0.051*	0.047*
	[0.277]	[0.364]	[0.313]	[0.030]	[0.027]
Outside Sampling Frame	-0.002	0.009	-0.139***	-0.013***	-0.012***
	[0.031]	[0.037]	[0.040]	[0.004]	[0.004]
ACFIM Presence	-0.187	0.196	-0.144	-0.013	-0.016
	[0.136]	[0.176]	[0.133]	[0.012]	[0.012]
Treatment*ACFIM Presence	0.792***	-0.892***	0.079	0.006	0.010
	[0.246]	[0.331]	[0.271]	[0.024]	[0.025]
Spillover*ACFIM Presence	0.258	-0.543*	-0.337	-0.036	-0.032
	[0.247]	[0.330]	[0.286]	[0.028]	[0.025]
R^2	0.48	0.31	0.33	0.32	0.30
Control Mean	0.029	0.024	-0.013	0.673	0.689
Controls	Yes	Yes	Yes	Yes	Yes
Observations	3663	3663	3665	3665	3235

Table 9F: Interactions on Key Outcomes

	Index of Campaigning (Table 8)		
	(1) All Candidates	(2) Incumbents	(3) Challengers
Treatment village	-0.068	-0.027	-0.092
	[0.092]	[0.085]	[0.097]
Spillover	-0.023	0.016	-0.057
	[0.093]	[0.087]	[0.094]
Treatment*Saturation	0.656***	0.393*	0.759***
	[0.243]	[0.210]	[0.248]
Spillover*Saturation	0.509**	0.112	0.784***
	[0.249]	[0.233]	[0.251]
Outside Sampling Frame	-0.017	0.010	-0.041
	[0.037]	[0.037]	[0.036]
ACFIM Presence	-0.346***	-0.252**	-0.356***
	[0.128]	[0.114]	[0.137]
ACFIM Presence*Treatment	-0.460*	-0.290	-0.517**
	[0.239]	[0.211]	[0.246]
ACFIM Presence*Spillover	-0.382	-0.165	-0.506**
	[0.240]	[0.222]	[0.243]
R^2	0.13	0.14	0.09
Control Mean	-0.014	-0.021	-0.003
Controls			
Observations	28454	28454	28454

Table 10A: Balance Voter Respondent 1

	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		-0.030		-0.096	
	[0.302]		[0.117]		[0.010]		[0.010]		[0.056]		[0.075]	
Spillover	0.123		-0.120		-0.006		0.002		-0.056		-0.248***	
	[0.338]		[0.146]		[0.011]		[0.011]		[0.062]		[0.083]	
Treatment Saturation		-0.079		-0.004		-0.011		0.008		-0.051		-0.240*
		[0.494]		[0.213]		[0.018]		[0.020]		[0.094]		[0.143]
Outside Sampling Frame	-0.843**	-0.650***	0.170	0.083	-0.014	-0.012	-0.001	0.001	-0.005	-0.027	0.102	-0.009
	[0.342]	[0.245]	[0.139]	[0.097]	[0.011]	[0.008]	[0.010]	[0.007]	[0.059]	[0.041]	[0.073]	[0.050]
ACFIM Presence	-1.085**	-1.062**	-0.176	-0.185	-0.022	-0.018	-0.044***	-0.048**	0.317***	0.335***	0.739***	0.829***
	[0.467]	[0.518]	[0.209]	[0.223]	[0.016]	[0.018]	[0.017]	[0.020]	[0.096]	[0.111]	[0.127]	[0.153]
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	40.088	40.088	5.487	5.487	0.741	0.741	0.872	0.872	3.213	3.213	3.605	3.605
Observations	27375	27375	28452	28452	28454	28454	28454	28454	28454	28454	28451	28451

Table 10B: Balance Voter Respondent 2

	Assets		Farmer		Trade		High Skill		Not Working	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment village	-0.006		0.027		-0.010		-0.005		-0.006	
	[0.034]		[0.017]		[0.007]		[0.006]		[0.004]	
Spillover	-0.004		0.010		-0.010		-0.009		0.003	
	[0.039]		[0.020]		[0.008]		[0.007]		[0.006]	
Treatment Saturation		-0.009		0.053		-0.022		-0.013		-0.006
		[0.069]		[0.033]		[0.013]		[0.010]		[0.008]
Outside Sampling Frame	-0.009	-0.008	-0.008	-0.019**	-0.003	-0.004	0.009	0.006	-0.004	0.001
	[0.032]	[0.018]	[0.015]	[0.009]	[0.007]	[0.004]	[0.006]	[0.004]	[0.005]	[0.003]
ACFIM Presence	-0.055	-0.050	-0.011	-0.040	-0.007	0.005	0.014	0.021*	0.010	0.013
	[0.063]	[0.071]	[0.031]	[0.036]	[0.011]	[0.014]	[0.011]	[0.011]	[0.008]	[0.008]
R^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Control Mean	1.638	1.638	0.687	0.687	0.088	0.088	0.078	0.078	0.053	0.053
Observations	28454	28454	28453	28453	28453	28453	28453	28453	28453	28453

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets. All regressions control for the parish-level ACFIM presence (interacted with Treatment and Spillover status in even columns), and an ACFIM dummy.

Table 10C: Balance Voter Respondent 3

	Ganda		Nkole		Soga		Catholic		Protestant		Muslim	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment village	0.021		-0.007		-0.017		0.034*		-0.017		-0.019	
	[0.021]		[0.014]		[0.015]		[0.019]		[0.019]		[0.013]	
Spillover	0.033		-0.012		-0.030**		0.064***		-0.022		-0.040***	
	[0.025]		[0.014]		[0.014]		[0.021]		[0.021]		[0.013]	
Treatment Saturation		0.022		-0.012		-0.020		0.065*		-0.027		-0.038
		[0.048]		[0.024]		[0.034]		[0.036]		[0.038]		[0.027]
Outside Sampling Frame	-0.013	-0.001	0.016*	0.012*	0.026***	0.014**	-0.032*	-0.008	0.003	-0.003	0.024**	0.007
	[0.019]	[0.006]	[0.009]	[0.006]	[0.010]	[0.006]	[0.018]	[0.011]	[0.017]	[0.010]	[0.009]	[0.005]
ACFIM Presence	0.158***	0.148***	-0.116***	-0.110***	0.055**	0.064*	-0.001	-0.032	-0.064*	-0.051	0.074***	0.092***
	[0.043]	[0.042]	[0.025]	[0.028]	[0.027]	[0.034]	[0.033]	[0.038]	[0.033]	[0.036]	[0.019]	[0.027]
R^2	0.02	0.02	0.02	0.02	0.01	0.00	0.00	0.00	0.00	0.00	0.01	0.01
Control Mean	0.075	0.075	0.061	0.061	0.060	0.060	0.423	0.423	0.429	0.429	0.087	0.087
Observations	28451	28451	28451	28451	28451	28451	28454	28454	28454	28454	28454	28454

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets. All regressions control for the parish-level ACFIM presence (interacted with Treatment and Spillover status in even columns), and an ACFIM dummy.

Table 10D: Balance Key Informant 1

	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]
Outside Sampling Frame	-0.028	0.002	-0.002	-0.010	0.055**	0.024	-0.005	-0.016
	[0.021]	[0.013]	[0.008]	[0.007]	[0.027]	[0.016]	[0.025]	[0.016]
ACFIM Presence	0.143***	0.166***	-0.028**	-0.036***	-0.213***	-0.208***	0.152***	0.113***
	[0.028]	[0.035]	[0.011]	[0.013]	[0.042]	[0.049]	[0.034]	[0.039]
R^2	0.01	0.01	0.00	0.00	0.02	0.02	0.01	0.01
Control Mean	0.187	0.187	0.031	0.031	0.430	0.430	0.247	0.247
Observations	4090	4090	4090	4090	4090	4090	4090	4090

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets. All regressions control for the parish-level ACFIM presence (interacted with Treatment and Spillover status in even columns), and an ACFIM dummy.

Table 10E: Balance Key Informant 2

	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]
Outside Sampling Frame	-0.019	0.002	0.011	0.009	0.027**	0.015**	-0.021	-0.005	0.006	0.014	0.004	-0.002
	[0.022]	[0.008]	[0.012]	[0.008]	[0.011]	[0.007]	[0.026]	[0.017]	[0.025]	[0.017]	[0.013]	[0.008]
ACFIM Presence	0.177***	0.181***	-0.115***	-0.117***	0.051*	0.061*	0.011	-0.004	-0.034	-0.021	0.066***	0.088***
	[0.048]	[0.051]	[0.029]	[0.030]	[0.028]	[0.036]	[0.041]	[0.048]	[0.041]	[0.046]	[0.023]	[0.031]
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	0.095	0.095	0.063	0.063	0.063	0.063	0.449	0.449	0.421	0.421	0.091	0.091
Observations	4090	4090	4090	4090	4090	4090	4090	4090	4090	4090	4090	4090

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets. All regressions control for the parish-level ACFIM presence (interacted with Treatment and Spillover status in even columns), and an ACFIM dummy.

Table 10F: 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	-2.555		0.012		-0.011		0.012		-0.019		-3.216	
	[14.153]		[0.009]		[0.015]		[0.013]		[0.015]		[10.404]	
Spillover Polling Station	-6.448		0.005		-0.007		-0.008		-0.001		-14.253	
	[13.730]		[0.009]		[0.017]		[0.015]		[0.016]		[9.097]	
Treatment Saturation		-18.682		0.014		-0.026		0.015		-0.034		4.105
		[26.344]		[0.018]		[0.031]		[0.028]		[0.029]		[17.272]
Outside Sampling Frame	-18.833	-20.776**	-0.017***	-0.020***	-0.021**	-0.018***	0.016*	0.007	-0.020**	-0.011**	-81.538***	-86.651***
	[11.664]	[9.489]	[0.006]	[0.005]	[0.010]	[0.007]	[0.008]	[0.006]	[0.008]	[0.005]	[9.383]	[7.341]
ACFIM Presence	1.120	8.776	-0.082***	-0.089***	-0.149***	-0.138***	0.039*	0.031	-0.077***	-0.061**	-44.869***	-48.285***
	[23.369]	[26.539]	[0.016]	[0.019]	[0.027]	[0.032]	[0.023]	[0.027]	[0.025]	[0.029]	[15.573]	[17.935]
R^2	0.00	0.00	0.03	0.03	0.03	0.04	0.00	0.00	0.01	0.01	0.04	0.04
Control Mean	615.658	615.658	0.605	0.605	0.689	0.689	0.258	0.258	0.554	0.554	575.342	575.342
Observations	2820	2820	2820	2820	2814	2814	2814	2814	3217	3217	3665	3665

Note: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered by parish in brackets. All regressions control for the parish-level ACFIM presence (interacted with Treatment and Spillover status in even columns), and an ACFIM dummy.